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

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ABSTRACT

Political interference in public pension plans leads to sub-optimal changes in their asset allocation policies. Specifically, plans facing investment restrictions shift from low-cost traditional assets toward higher-fee, lower-performing alternative assets, including hedge funds and real estate. The effects are strongest in Republican-led state plans, where politicians act with greater conviction. While the politicians pushing these restrictions receive increased media attention, states themselves do not appear to benefit, either in the form of higher in-state employment or higher stock returns for in-state firms. My study underscores how political incentives create agency problems that adversely affect public pension plan investment decisions and outcomes.

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1. Introduction

Asset allocation is widely recognized as the primary determinant of investment performance (Brinson, Singer, and Beebower, 1991; Ennis, 2022b; Ibbotson and Brinson, 1987). Specifically, prior literature has documented that approximately 90% of the variability in fund returns can be explained by asset allocation policy (Brinson, Hood, and Beebower, 1986; Ibbotson and Kaplan, 2000). In public pension plans, the board of trustees, which comprises a mix of investment professionals, elected officials, and politicians, is responsible for setting the target asset allocation, while the investment manager strategically implements it within board-approved ranges.[1] Consequently, investment performance is jointly determined by the decisions of the board and the execution of the manager.

However, these decisions are not made unfettered. Public pension plans are sponsored by state and local governments, leaving boards, and thus investment staff, under the formal oversight of political officials (Mitchell and Hsin, 1994; Romano, 1993; Eaton and Nofsinger, 2004; Coronado, Engen, and Knight, 2003). This framework creates opportunities for politicians to interfere with plan operations, notably investment behavior, which may impede the attainment of performance goals. Indeed, prior research provides evidence that political interference affects the specific investments plans select.[2] Less is known about the impact that political interference has on broader asset allocation. The goal of this paper is to provide an answer to that question.

To do so, I use the recent trend of U.S. state officials enacting ESG-related legislation and other policies that limit plans' discretion over their investment choices. The restrictions generally take two forms, both of which constrain public market investment. The first restricts investment

[1] Some plans employ a separate investment board for the role of asset allocation policy (Andonov, Hochberg, and Rauh, 2018).

[2] Bradley, Pantzalis, and Yuan (2016); Lee (2023); Tu, F. Zhang, and H. Zhang (2025); Hochberg and Rauh (2013); Brown, Pollet, Weisbenner (2015); Andonov, Hochberg, and Rauh (2018).

in public securities of specific sectors, most commonly energy (e.g., coal, oil) and firearms. The second restricts business relations with large, traditional asset managers, typically central players in public markets, based on their ESG-related discriminatory practices.[3] These directives work in conjunction with the unique governance backdrop of public pension plans to offer a natural and rich laboratory for exploring whether political interference alters portfolio strategy and outcomes.

Should political interference affect asset allocation? On the one hand, politicians may enact restrictions primarily as grandstanding gestures without genuine intent to alter plan investments.[4] Plan boards may also resist such measures by invoking their fiduciary duty to maximize returns. In other cases, investment constraints, such as product structures, procurement rules, or high transaction costs, complicate compliance. For instance, exiting private or commingled funds is complex, and early sales often occur at discounts. Additionally, plans may already invest in alignment with the restriction, reflecting the partisanship of state officials and meriting no changes to the asset mix. At most, we might expect interference to affect investment within specific public-market assets targeted by these policies, such as energy firms, rather than broader asset allocation.

On the other hand, the restricted investment universe may prompt investment staff to adjust the overall asset mix. Staff are compensated based on performance relative to two key performance benchmarks: (i) the policy return benchmark set by the board, and (ii) the actuarial assumed rate of return (AAR). Falling short of policy benchmarks may raise reputational or career concerns, while AAR shortfalls carry fiscal consequences for stakeholders, such as future tax hikes or cuts to public spending. As a result, investment constraints in a given asset class may lead investment

[3] These asset managers may serve as external managers of pension plan capital, be held directly as equity positions in the plan's portfolio or be indirectly held through another of the plan's investment managers.

[4] Tosi and Warmke (2020) define moral grandstanding as "… the use of moral talk for self-promotion".

staff to shift allocations toward higher-risk alternative products in pursuit of greater returns.[5] Furthermore, public pension plans' size complicates the substitution of managers within a given asset class. When plans are barred from investing with a traditional asset manager, finding a comparable, non-barred manager that can handle such substantial capital is challenging.[6] Notably, the top five asset managers globally, including BlackRock, Vanguard, Fidelity, State Street, and J.P. Morgan, collectively oversee about a quarter of worldwide assets under management, yet many of them are frequently banned under restrictive mandates.[7] Lastly, investment staff may preemptively reduce public-market exposure, shifting toward more opaque private products to lower the risk of future politicization through regulatory pressure.

My primary finding is that political interference impacts asset allocation decisions. Specifically, plans that experience investment restrictions decrease their allocation to traditional assets (e.g., equities, fixed income) by approximately 2.7% and increase their allocations to alternative asset classes. Within individual assets, equity allocation falls by 3.1%, while the percentage of assets allocated to hedge funds and real estate increases by 1.5% and 1.1%, respectively. These shifts are economically meaningful. The reduction to equities, from an average allocation of 52%, translates into roughly a 6% decline in equity exposure. By contrast, the increases in hedge fund and real estate allocations, from average levels of 4.5% and 6.1% respectively, correspond to jumps of about 33% and 18% respectively. To ensure my results are

[5] Mullally, Lu and Ray (2022) show that CIOs are also motivated by compensation and may be indirectly rewarded for greater exposure to alternative assets. They find that higher pay is linked to stronger performance, driven primarily by allocations to these assets.

[6] Considering the average restricted plan manages $13.99 billion and the average allocation to equities of 52%, hiring an external manager to handle even just a portion of assets, say 10%, is roughly $727.5 million ($13.99B * .52 * .10).

[7] Data is sourced from a joint study by Thinking Ahead Institute & Pensions & Investments, (2024), *The world's 500 largest asset managers*, available at https://www.thinkingaheadinstitute.org/content/uploads/2024/10/PI-500-2024.pdf.

not driven by concerns about endogeneity, I employ propensity score matching, conditional sample tests, and restricted-window analyses. Regardless of the empirical methodology chosen, the results suggest that plans facing investment restrictions engage in risk-shifting behavior by decreasing their holdings of traditional, public assets and increasing their holdings of alternative, private assets.

My main hypothesis is that these changes stem from politically motivated conflicts of interest. If allocation shifts are politically driven, they should be most pronounced in states where politicians act with greater conviction or have greater control. For instance, prior research shows that Republican officials hold firmer beliefs and stronger partisan identities, which suggests they may enact more binding policies that have greater impacts on allocation decisions (Jost, 2017; Zmigrod, 2020). Consistent with this hypothesis, I find larger changes in allocations for plans (i) headquartered in Republican-leaning states and (ii) operating under politicians who take public, performative actions to advance their agendas. Moreover, state-run plans, where officials have more direct oversight and control, also have larger allocation shifts than city- or county-run plans.

I next examine whether these asset allocation changes impact plans' investment performance. Indeed, I find that performance worsens as a result of these allocation shifts. 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. Plans effectively forgo value in responding to restricted mandates. These results are consistent with Ennis (2020), who finds a negative relation between the level of alternative investment and plan performance.

Fees offer a more direct measure of the costs of political interference. Using transaction-level evidence on what plans buy and sell to comply with these policies, I find that reallocations

from traditional to alternative assets raise fees by up to 5.89%, or approximately $28 million per year. Although the intent may be to preserve a risk-adjusted profile comparable to the pre-divestment portfolio, the fees likely offset or even outweigh any performance gains, indicating a distortion in investment choice and efficiency. This result is consistent with Garrett and Ivanov (2024), who document higher municipal borrowing costs due to restrictive investment policies.

So far, my results indicate that these investment restrictions have adverse impacts on public pension plans. Given these results, it is important to understand what drives politicians to pursue these policies. One possibility is that these restrictions are implemented for self-serving reasons. That is, politicians implement and promote these policies to increase their public visibility, bolster voter support, cultivate relations with like-minded constituencies (e.g., industry and advocacy groups), or secure campaign contributions.[8] Alternatively, while these policies have an adverse impact on public pension plan performance, they may benefit the politicians' states. For instance, forbidding public pensions from integrating ESG criteria in their investments could benefit in-state industries typically affected by such policies (e.g., oil and gas firms, weapons firms). Specifically, such restrictions could bolster in-state employment in these industries or increase affected firms' stock prices.

To examine the first possibility, I examine whether politicians receive increased publicity following the passage of investment restrictions. I document significantly greater media attention among state officials (e.g., Governor, Treasurer), particularly in Republican states. Officials in states with restrictive policies receive about 2 additional articles of coverage per year in well-known news outlets, more than double the average. To test whether these policies have positive

[8] Another possibility is that state officials face lobbyist coercion to advance these measures. For instance, the American Legislative Exchange Council (ALEC) is one of many non-profit organizations that drafts model legislation that states use to craft bills, such as the Energy Discrimination Elimination Act. For detail, see https://alec.org/article/setting-the-record-straight-the-energy-discrimination-elimination-act/.

impacts on states, I examine employment changes in targeted industries and find no discernible improvement in either the energy or firearms manufacturing sectors. I also consider whether restrictions imposed by Republican officials, often framed as protection to the energy sector, channel capital to in-state energy firms and thus increase their stock prices. Using 13F equity holdings data, I find that Republican-led plans do not increase investment in oil and gas firms. I also find no evidence that the introduction of these policies elicits a positive market response (higher stock prices). Combined, the results suggest that lawmakers enact these restrictions to improve their own political prospects rather than to boost the state economy.

Taken together, the results suggest that these policies are a manifestation of an agency problem. Politicians, seeking public visibility and support, impose mandates that improve their own political prospects. The investment staff at these plans have incentives to meet return targets and shield against future regulations. The interaction between the two produces a negative outcome for public funds: reallocation away from traditional, low-cost products and managers to higher-fee, less transparent investments. This shift weakens performance and shows no clear benefits for the state economy.

My study contributes to several strands of 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, via board connections, shareholder activism, and private rent extraction (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 portfolio-wide asset allocation, providing insights into how political interference affects both performance and fee structures. I identify a recent, observable action taken by political actors that shapes plans'

investment behavior: ESG-related investment restrictions. This setting allows me to capture direct interference in investment strategy, moving beyond indirect proxies of political bias.

Second, I contribute to the literature on public pension plan investment behavior. Academic research emphasizes that asset allocation is the primary determinant of investment performance, motivating my focus on allocation decisions (Brinson et al., 1991; Ennis, 2022b; Ibbotson and Brinson, 1987). Prior studies highlight that plans' funding gaps drive asset allocation and risk-taking (Novy-Marx, 2009, 2011; Andonov, Bauer, and Cremers, 2017; Mohan and Zhang, 2014; Pennacchi and Rastad, 2011; Ang, Chen, and Sundaresan, 2013; Brown and Wilcox, 2009). I extend literature by showing that political interference is a more acute and salient impetus for risk-taking, steering plans toward larger private market allocations and weaker performance.

Third, I highlight the impact of governance on performance in the $6 trillion public pension industry, an area that warrants greater attention given its scale and fiscal importance (NASRA, 2023). Prior work shows that public pension governance structures affect portfolio allocation and performance, with downstream implications for government spending and taxpayer contributions (Mitchell and Hsin, 1994; Useem and Mitchell, 2000; Mitchell and Yang, 2005). This study highlights the negative impact of political influence in the investment process. My results indicate that political interventions in this process lead to distortions with long-term repercussions for plan participants (Romano, 1993, 1995; Eaton et al., 2004; Coronado et al., 2003; Nofsinger, 1998).

This paper proceeds as follows. Section 2 describes the data and sample. Section 3 explores the impact of political interference on asset allocation and analyzes heterogeneity in the results. Section 4 examines the consequences of allocation effects on plan performance. Section 5 evaluates the incentives of political interference for state officials. Section 6 presents robustness checks. Section 7 concludes.

2. Data and Sample

My study relies on two primary data 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 information on asset allocation, investment returns, funding, and many other variables. The second source is a hand-collected dataset of state-level ESG-related investment restrictions, drawn from Ropes & Gray LLP.[9] For each state, I gather all laws and policies that impose restrictive mandates on public pension investments. I record key details including the date the policy was introduced, its effective date, scope and subject matter, and other pertinent information. I supplement my two primary data sources with additional variables used throughout the study.

I construct a cross-sectional indicator variable, *Restriction,* set to one for public pension plans subject to at least one investment restriction, either through a state law or a public pension board policy, and zero otherwise. Although board policies are not the same as legislative changes, I include them as instances of political interference for several reasons. First, public pension plans operate under government oversight, allowing political actors to influence board decisions. Second, boards often include political appointees or elected officials (e.g., governors or treasurers) which opens further avenues for politicizing plan management.Third, some public pension plans are housed within the offices of elected officials, meaning their investment operations are not independent from state political leadership.[10]

My main empirical analysis will rely on the use of difference-in-differences (DID) models. For that reason, my main variable of interest is *Post*, an indicator variable set to one if a plan's

[9] Ropes & Gray LLP maintains a webpage dedicated to tracking state-level ESG-related initiatives across the U.S. The webpage can be accessed at https://www.ropesgray.com/en/sites/navigating-state-regulation-of-esg.

[10] In Connecticut and North Carolina, the Treasurer is responsible for investing the assets of the state pension plans.

fiscal year-end occurs after a restriction's effective date. My final sample consists of 228 plans and 5,240 unique plan-fiscal years from 2000 to 2023. All continuous variables are winsorized at the 1% and 99% levels.

Table 1, Panel A, presents distributional statistics of the *Restriction* sample by state and year. In total, 87 plans (38%) are subject to an investment restriction (*Restriction* = 1), with ex-post data available for 84 of them (*Post* = 1). The sample covers sixteen states, with restrictions enacted between 2020 and 2023. I classify restrictions into three types: industry-specific prohibitions, restricted financial company lists ("financial blacklists"), and policy-induced divestments. From 2020 to 2021, five states—including Connecticut, New York, Rhode Island, Maine, and California—implemented industry-specific prohibitions on investments in sectors such as fossil fuels, coal, and weapons. These policies primarily restrict holdings in public equities.[11] In 2021, Texas enacts 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 follow Texas' lead with similar measures. These laws typically constrain relationships with major asset managers central to public securities markets. For example, Oklahoma's initial list includes 13 companies (e.g., BlackRock and State Street)—see Appendix B for the official list.[12] In 2022, six states announce divestments from large, well-known asset managers due to their integration of ESG into investment and proxy voting practices. Such divestments stem from ESG-related revisions to pension board investment or voting policies, or from executive mandates by state officials.

[11] For example, in 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.

[12] Section 4 of Oklahoma's Energy Discrimination Elimination Act of 2022 (House Bill 2034) states that "The Treasurer shall… provide to each state governmental entity a list of financial companies that boycott energy companies…", thereby restricting investment in these companies. The bill is available at https://www.oklegislature.gov/cf_pdf/2021-22%20enr/hb/hb2034%20enr.pdf.

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). 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%).

Plans on average allocate 79.33% of their capital to traditional assets, defined as the percentage of assets allocated to equities, fixed income, and cash. Alternative asset allocation, which includes private equity, hedge funds, and real estate, averages 16.83%, while the residual is allotted cross commodities or miscellaneous alternatives. The average (median) plan allocates 51.73% (53%) of 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 miscellaneous alternatives, and the rest is classified as "other." The highest average asset return is private equity at 11.86%, while cash yields the lowest at 1.99%.

3. Impact of Political Interference on Public Pension Plan Asset Allocation

I begin my analysis by examining the impact of investment restrictions on 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). I observe significant differences in the percentage of assets allocated to traditional assets. Specifically, plans on average offload about 14% of their allocation to traditional assets following the restriction, while alternative allocation increases by about 11%. Equity allocation experiences an approximate 8.3% reduction, while allocation to fixed income declines by about 5.4% and cash experiences no meaningful change. Private equity increases by approximately 6%, real estate by 3%, and hedge funds by 2%. All

differences are statistically significant at the 1% level. These univariate results serve as a baseline, suggesting that plans adjust their investment behavior in a risk-shifting manner when faced with restrictive directives.

Since the analysis in Table 2 does not account for confounding factors or unobserved heterogeneity, I estimate 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, classified as *Traditional* (equity, fixed income, cash) or *Alternative* (hedge funds, real estate, private equity). $Post_{i,t}$ is the key independent variable of interest, set to one for fiscal year ends 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 fiscal year end and plan fixed effects, respectively. Standard errors are double clustered by plan and fiscal year end.

Panel A of Table 3 reports estimates of equation (1) for traditional and alternative allocation. 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%. 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 literature evidence that underfunded plans take more risk (Novy-Marx and Rauh, 2009; 2011; Andonov et al., 2017). In Columns (3) and (4), I include fiscal year end fixed effects to account for market-wide conditions. The *Post* coefficients consistently reveal risk-shifting behavior: *Traditional* declines by 2.7% and the capital is reallocated to

Alternative. These effects are economically meaningful. For instance, a 3% increase in alternative allocation corresponds to an 18% increase 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.

To address selection bias concerns, I employ a propensity score match (PSM) approach. Specifically, I match plans exposed to an investment restriction with unexposed plans in the same FYE of the restriction.[13] I match on plan size, funding ratio, *State*, and fiscal year end. *State* is a variable equal to one for plans administered at the state-level, and equal to zero for local plans. The resulting matched sample contains 1,504 pairs, yielding 3,008 FYEs. I utilize match fixed effects, which absorb pair-specific heterogeneity by relying on within-pair differences.

Panel C of Table 3 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 3.1% reduction in equity allocation, with corresponding increases of 1.5% in hedge funds and 1.1% in real estate.[14]

[13] I match without replacement, using the nearest neighbor approach and enforcing a caliper of 0.05. Results are economically robust to alternate methods.

[14] I re-estimate match sample regressions using other variables in the matching procedure; specifically, I add political lean and fiscal year rather than FYE. I also exclude match fixed effects. The results remain statistically robust.

The equity reduction I observe is consistent with the fact that the investment restrictions target public market investments. Yet, given the discretion CIOs hold in allocation policy, it is not obvious why the response entails shifts across asset classes rather than reallocations within equities. The move into higher-risk private products may reflect efforts to sustain risk-return targets under constrained opportunity sets, or to hedge against future political scrutiny.

The next analyses examine cross-sectional variation in the allocation changes. The purpose is to test whether the largest shifts occur in states where political officials (i) act with greater conviction and (ii) hold more direct control over plans. I measure political conviction using two proxies and political control using one.

First, I measure political conviction using political partisanship, which captures systematic differences between Republican and Democratic officials. Prior research shows Republicans hold firmer convictions and stronger partisan identities, which can yield policies that constituents are more inclined to heed, making them more binding in practice (Jost, 2017; Zmigrod, 2020).[15] To measure partisanship, I identify the political leaning of each state. Political lean is calculated as the average margin difference between how a state votes and how the country votes. [16] I define Republican-leaning (*Red*) states as states with a lean of -10% or more and Democratic-leaning (*Blue*) states as states with a lean of at least 10%. About 27% of plans affected by a restriction are located in a *Blue* state, while about 73% of these plans are *Red.*[17]

[15] Specifically, Republican officials' convictions are rooted in loyalty, authority, and purity, known as 'binding moral foundations', whereas Democrats are more grounded in 'individualizing foundations' such as care and fairness (Haidt & Graham, 2009).

[16] The data is drawn from FiveThirtyEight's partisan lean metric for 2021 (most recent year available). It can be accessed at https://github.com/fivethirtyeight/data/tree/master/partisan-lean.

[17] Appendix B contains a list of states by partisan lean.

Second, I measure political conviction using coalition letters on ESG-related issues. I hand-collect data on recent multi-state coalitions, which take the form of letters signed by state-elected officials, including attorney generals, treasurers, or governors that publicly endorse the initiative outlined in the letter.[18] I argue that more signatures reflect stronger public signals of politicians' conviction on ESG or anti-ESG matters. I create a dummy variable, *LetterSigner,* which is equal to 1 for public pension plans headquartered in states where politicians have signed at least the median number of signatures in the full sample, and 0 otherwise.[19]

Finally, I measure political control by comparing state and local plans, as state officials often exercise more direct oversight of state plans. I construct a dummy variable equal to 1 for state-level plans and 0 otherwise. The group of plans coded as 0s includes county-level and city-level plans for which the state-level legislation is less binding. Of the 87 plans in the *Restriction* sample, 48 are state plans. I expect the main effects to be concentrated among state plans, as they are likely to face more direct oversight and interference from state officials. In contrast, local plans may respond more to city or county governance mandates.[20] I use equation (1) to estimate multivariate regressions within subsamples defined along these three dimensions.

Panel A of Table 4 presents the results of testing Equation (1) within *Red* and *Blue* subsamples. 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%, reallocating the capital to

[18] The information is available at https://www.ropesgray.com/en/sites/navigating-state-regulation-of-esg/multi-state-initiatives.

[19] Results are robust if I utilize the 75th percentile in place of the median cutoff.

[20] 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 through political rhetoric or via anticipation of future investment restrictions at the local level.

alternatives. Blue states do not exhibit a meaningful change in their asset allocation post-restriction. The pronounced shifts in red states suggest that policymakers, shaped by stronger conviction, may implement more binding investment policies that prompt larger allocation effects.

In Panel B of Table 4, I divide the sample based on *LetterSigner*. The findings support my conjecture: plans in states where politicians are more firmly and publicly promoting their stance on ESG are those exhibiting the risk shift in allocation. In the *LetterSigner* subsample, *Traditional* drops by 3.7%, while *Alternative* increases by about 4.2%. I do not observe significant effects in states with low coalition participation (*LetterSigner* = 0).

Panel C of Table 4 reports regression results for state and local plans. The allocation shifts occur exclusively among state plans. This pattern aligns with the political-control conjecture.

Taken together, Table 4 highlights that the changes in asset allocation are concentrated in plans where state officials express stronger political conviction and have more political control.

4. Impact of Asset Allocation Decisions on Investment Performance

The results presented so far indicate that plans shift their investments to alternative assets in response to political interference. A natural question is whether, and how, these shifts affect investment performance. I explore the answer to that question in this section. To begin, 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*. All other notations are the same as in Equation (1). *Peer-adjusted return* is calculated by subtracting the average return of all other plans from a plan's investment return during the same fiscal year. $X_{i,t-1}$ is a vector of lagged plan characteristics including natural log of plan size, plan funding ratio, and asset allocations.

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. Standard errors are double clustered by plan and fiscal year. Controls are suppressed for brevity; the full model is reported in the appendix in Table A3.

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 (bps) ex-post, while the performance of alternatives declines by 70 bps. I find that fixed income performance rises by 130 bps (*t*-stat = 3.74) following the restriction. Equity rises by 50 bps, although the effect is not statistically significant. Meanwhile, a 230 bps (*t*-stat = -1.99) drop in hedge fund performance drives the decline in alternative performance. The results suggest that managers' investment choices in response to the restrictions are inefficient. Post-restriction, traditional assets deliver stronger returns, yet pension plans reduce exposure to these assets and shift capital into lower-performing alternatives, such as hedge funds. Collectively, the evidence suggests that investment restrictions compel managers to alter allocation policy in ways that distort portfolio composition.

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, these plans oversee substantial assets,

[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.

averaging $13.6 billion in the full sample. As a result, restriction-driven allocation shifts likely affect a small share of total assets, making performance impacts difficult to detect.[22]

To more directly assess the costs associated with the restrictions, I examine treated plans (*Restriction* = 1) at the transactional level. Appendix Table A4 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 covers the Missouri States Employees' Retirement System. Panel A of Table 5 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]

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. The investment manager reallocated the capital with the goal of generating alpha to maintain the target risk-return profile.[24] While this may enhance gross returns, the accompanying surge in

[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.

[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). However, Aubry (2022) finds no significant improvement in long-term performance despite this profile among alternatives.

fees may offset or exceed potential gains. Moreover, hedge funds demand more time and resources to generate returns, whereas commingled funds offer cost-effective, broad market exposure.

Case B presents the Texas Employees' Retirement System's case, as shown in Panel B. The plan faced a restricted financial company list, released by the TX comptroller in 2022. At the time, TX ERS had exposure to four restricted financial institutions, totaling around $646 million in market value. The exposure was primarily through contracts with external managers. A small portion was held indirectly via restricted public securities in the underlying portfolios of non-restricted external managers. To comply with the legislation, the ERS withdrew capital from BlackRock, a restricted institution, and Pzena, due to its underlying exposure in restricted equities. For underlying exposure via non-restricted managers, the ERS retained relationships so long as the manager divested any restricted securities.

ERS reinvested the divestment of $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 increased 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 of public securities, like BlackRock, known for its predominantly passive products, with actively managed alternative funds. The question remains: why not just move money within the public market to a similar low-cost provider? The rationale may lie in the constraints managers face when replacing their external managers, such as the scale of pension assets, which restricts the set of available providers. Anticipation of further regulation in public markets may also shape these choices. These constraints work in lockstep with the pressure managers face to meet the target return rates despite

a narrowed opportunity set. Thus, the turn to alternatives may aid diversification and return objectives but it exposes plans to higher fees and greater risk.

Case C (Panel C) details the case of TX MRS, which faces the same restricted financial company list as the ERS (Panel B). Upon list release, the 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 a .43% increase in fees, 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 managers' efforts to meet performance goals, while also avoiding contracting costs and moving into less transparent investments that are less vulnerable to future politicization via regulation.

The cases in Table A4 suggest that political interference induces inefficient and costly portfolio transactions. Restrictive policies effectively narrow the investment universe, while required return targets remain unchanged, stimulating plans to shift capital from public markets into more expensive private products with no clear evidence of superior returns (net of fees).

5. Political Incentives and Economic Consequences

The previous sections document the impact that political interference has on both portfolio allocation and performance. Specifically, Section 4 demonstrates that plans subject to political interference adjust their portfolio allocation through a reduction in public markets and a

corresponding increase in private markets. This raises a further question: Do the restrictions impact equity composition? I use Thompson Reuters Institutional Managers (13F) Holdings data to examine whether there are observable shifts in pensions plans' investment in energy and firearms firms, the sectors targeted by the restrictive mandates.[25] I construct custom sector classifications to measure exposure.[26] Within the sample of 13F plans, I estimate the following regression:

$$TargetedSectorExposure_{i,t} = \beta Post_{i,t} + Log(HV)_{i,t-1} + \varphi_t + \delta_i + \varepsilon_{i,t} \qquad (3)$$

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

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) display no meaningful change in exposure to energy stocks. In contrast, column (4) reveals a marginally significant decline in the number of firearms stocks held. Two states in the sample—Connecticut and Rhode Island—enact firearm-related restrictions.

Panel A indicates that the objective of the restriction matters. Plans headquartered in red states face restrictions framed as a protection to the energy sector; therefore, I expect these plans to demonstrate increased investment in energy stocks. Meanwhile, politicians in blue states

[25] 28 public pension plans in my sample appear in Form 13F Filings (see Appendix B for the full list). Plans do not appear in 13F for reasons including: (i) the plan outsources all asset management, (ii) the plan does not meet the $100 million threshold in internally managed 13F-reportable securities.

[26] My energy classification captures firms involved in extraction, refining, distribution, and infrastructure related to fossil fuels. It 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. More details are provided in Appendix B.

implement mandates aimed at restricting investment in the energy and firearms sector, often as part of broader decarbonization or social responsibility goals. I expect these plans to reduce their holdings in energy and firearms stocks.

Panel B of Table 6 investigates whether the impact of the investment restrictions differs by political alignment. I estimate equation (3) on subsamples of Red and Blue-state plans. Columns (1) - (4) reveal that red plans do not significantly alter their portfolio holdings in targeted sectors. This is noteworthy: if politicians implement such policies with intent to promote the energy sector, the results do not support that objective. However, columns (6) - (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 significant difference emerges between their energy or weapons holdings, apart from a relative decline in firearm stocks in blue plans. This finding is somewhat surprising given the conflicting policy stances on energy investment between red and blue states.[27] Combined with the red-plan results in panel B, the evidence implies that politicians predominantly employ these restrictions mainly as political signals and avenues for grandstanding, rather than as binding measures intended to alter equity choice. Next, I explore whether they have quantifiable incentives to do so.

I consider the incentives that politicians possess for implementing or inspiring these investment restrictions. On one hand, they may be motivated by the prospect of gaining public visibility and fostering campaign support, with no pure intention of modifying asset allocation. On the other hand, state officials may intend to bolster aspects of the economy, such as employment or market

[27] This evidence is consistent with Rajgopal, Srivastava, and Zhao (2024), who document no meaningful difference in energy holdings composition between red and blue states, concluding that many political statements on ESG lack tangible follow-through.

valuations of in-state firms. I first examine the former through an analysis of media attention directed at state politicians.

I collect data from prominent news sources using Media Cloud, an open-source news database and count the number of times a politician's name co-occurs with ESG-related or investment restriction-related terms.[28] Politicians are identified using three name variants: full name, nickname with last name, or title with last name) to capture mentions accurately while minimizing false positives. For example, mentions of the current 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 including Governor, Treasurer, Comptroller, Director of Administration, Auditor, and Chief Financial Officer (titles vary by state). These are positions frequently linked to the investment policies in my setting. I estimate a state-level regression:

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

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

[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. See Appendix B for a list of the key words/terms.

matched sample approach.[29] OLS regressions include the sample of states with a restriction (*Restriction* = 1), while the matched sample included a restricted and non-restricted state.

Panel A of Table 7 presents the results of estimating equation (4) for the full sample of treated states. Column (1) shows that politicians garner higher media attention following a restriction. The coefficients on *Post* are uniformly positive, ranging from about 1.36 to 2.91 articles, and are statistically significant in three out of the four specifications. Although restricted-state politicians average just 1.42 related articles per year, the estimates imply a 100% to 200% increase in coverage when they advance such policies. In highly salient cases such as Florida, which reaches 15 related articles in a single year, the same effect still represents a 10% to 20% increase in public visibility.

Panel B explores heterogeneity by dividing the sample on political alignment. Like 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 policy, relative to other red states without such a policy. In contrast, I observe no comparable increase among blue states. This pattern matches that of the findings in Table 4, where the political environment of red states, associated with more assertive, vocal politicians and stringent restrictions leads to tangible allocation changes. Thus, it is not shocking that these politicians garner more media coverage in red states. This evidence indicates that the implementation of these restrictive policies boosts political capital.

Panel C analyzes 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 2 additional articles—persists in Column (4), the estimate loses statistical significance. These

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

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. Overall, Table 7 uncovers a key impetus for political officials in interfering with pension investment policy: attracting media attention. The remaining question is whether these laws deliver any in-state economic benefits, which may represent another objective of lawmakers.

I begin by testing whether the investment restrictions boost state employment levels in the energy and weapons sectors. 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: (i) Fossil Fuel, which includes employment in oil, gas, and coal sectors, and (ii) Weapons, which includes firearms and ammunition manufacturing.[30] For each state-year, I compute the share of total industry employment attributable to each 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

[30] *%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 Appendix B.

fixed effects. Across all columns, I find observe no statistically significant change in fossil fuel or weapon-related employment.

Seeing as red and blue state lawmakers pursue different political goals in crafting these policies, a full-sample analysis likely masks divergent effects. Red state restrictions are positioned as protection to the energy industry from 'wokeness'; accordingly, we may expect stronger employment outcomes in red states. Panel B splits the sample on political lean. The results exhibit no statistically significant changes in employment levels of either state.

Panel C compares post-restriction employment trends between red and blue states. No significant differences are detected. Table 8 explores one potential channel through which restrictions might benefit the state economy. The evidence suggests that these policies unlikely to be passed with the purpose to strengthen industry-specific employment, further supporting the political signaling view. Re-estimating the models on a matched sample yields similar results.

While political interference does not appear to impact the employment of targeted industries, it may be expected to help the very firms within these industries. Specifically, I next test whether the date these restrictive policies are introduced creates a positive market response for firms that are impacted by them. I focus on red-state restrictions only, in that these are the policies in which I would expect there would be potential positive responses from the market. Using Fama French classifications, I identify energy and firearms firms that are headquartered in a state with an investment restriction. If a state's policy is focused on energy, I only assign in-state energy firms for that state, whereas if it is focused on firearms, then I assign firearms firms. I perform the following regression within the sample of in-state firms:

$$CAR_{i,w} = Event_{i,w} + Log(MarketCap)_{i,w} + BrentRet_{i,w} + OPECMeet_{i,w} + Log(OilProd)_{i,w} + \varphi_w + \delta_i + \varepsilon_{i,w} \quad (6)$$

where, *i* indexes firm and *w* indexes time in weeks. *CARi,w* represents the cumulative abnormal return (CAR) for firm *i* in week *w*. To calculate CARs, I estimate rolling monthly betas using a standard market model, requiring each month to have at least 10 available observations (Brown and Warner, 1980; MacKinlay, 1997). I lag the betas forward and calculate daily abnormal returns for each firm, where the market return is proxied by the daily value-weighted return from CRSP. I aggregate abnormal returns to the weekly level for each firm-week from 2020 to 2023, the period in which restrictions are introduced in my sample. $Event_{i,w}$ is an indicator variable equal to one for firms headquartered in a state during the week the state introduces an investment restrictive policy, and zero otherwise. $Log(MarketCap)_{i,w}$ is the average firm size in week *w*. To control for contemporaneous oil price movements, I include variables that would impact oil stocks in my analysis. $BrentRet_{i,w}$, represents the return on Brent crude oil, a major global benchmark for oil prices. I calculate returns using weekly spot prices collected from the U.S. Energy Information Administration (EIA). $OPECMeet_{i,w}$ is an indicator variable equal to one if the Organization of the Petroleum Exporting Countries (OPEC) holds a meeting in that week. $Log(OilProd)_{I,w}$ is weekly U.S. Crude oil production collected from the EIA.[31] φ_w denotes week fixed effects. δ_i represents firm fixed effects, included in some models. Continuous measures are winsorized at the 1% and 99% levels. Standard errors are clustered by firms' state headquarters. My final sample contains 143 unique firms from 2020 to 2023. Data is sourced from CRSP and Compustat.

In Table 9, Model (1) includes firm fixed effects and all controls, demonstrating that the introduction of the investment restrictions (*Event*) does not elicit a significant market reaction among in-state energy firms. Brent returns are positively associated with CARs, which is unsurprising for oil firms. OPEC meetings are associated with negative CARs, indicating that the

[31] I alternatively use oil production in OPEC-only regions as a control variable; I find synonymous results.

discussion of these meetings may be taken as negative news for oil investors (e.g., oil production levels). Confirming that inference, oil production is negatively linked to firms' abnormal returns. However, to improve the model, I incorporate week fixed effects in model (2), which absorbs the oil control variables. With a more than doubled adjusted R_2, I continue to document no market response to such political interference. Model (3) implements firm and week fixed effects further corroborating the findings in models (1) and (2).

Table 9 highlights that political interference does not improve market perceptions of in-state firms targeted by these laws. The implication is that politicians are not implementing these policies with the goal of supporting the local economy, despite rhetoric around preventing energy discrimination. Rather, Section 6 indicates that political motives are more likely self-serving.

6. Identification and Robustness

First, I perform 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 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.

I also confront the concern that the economy-wide low equity market returns in 2022 may mechanically reduce equity allocation. While the fiscal year 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.

Lastly, all regressions are re-estimated within the conditional sample of plans facing an investment restriction (*Restricted* = 1), and the results remain statistically robust.

7. Conclusion

In this study, I examine the effect of political interference on asset allocation decisions in public pension plans, and the consequences for investment performance. Specifically, I document that political officials implement ESG-related policies that restrict plans' investments with motives that appear to conflict with the plan's investment performance goals. My main finding is that public pension plans, particularly in states where officials exercise strong conviction and political control, reallocate capital away from traditional, public assets toward higher-cost, less transparent private assets, including hedge funds and real estate. This portfolio reallocation comes at a cost to plan performance. Plans experience lower returns and incur higher fees when making these changes. Moreover, I find no tangible benefits for either in-state firms or in-state employees from the implementation of these restrictions. In contrast, state officials receive increased media attention upon passing these policies. Combined, the evidence suggests that lawmakers, as agents, implement restrictions for their own benefit at the expense of principals: plan members, in-state firms, and employees.

I conclude that political interference produces unintended, negative consequences for public pension plans. Specifically, it distorts asset allocation policy. Together, the findings highlight how the governance structure of public pension plans creates costly opportunities for political interference, underscoring the risks of politicizing public capital. Greater attention to

these structures is warranted to ensure that in the long term, the burdens of incentive misalignment do not fall on the retirees and workers the system is meant to serve and the taxpayers ultimately responsible for paying these costs.

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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 plans per state where *Restriction* = 1, the number of plans 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	Industry Specific
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
Year 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
Year 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	-0.030*	0.033***	-0.031**	0.005	-0.004	0.006	0.015*	0.011**
	(-1.96)	(2.77)	(-2.26)	(0.43)	(-1.48)	(0.96)	(1.84)	(2.53)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Plan FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Match FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,720	2,720	2,720	2,720	2,720	2,720	2,720	2,720
Adj. R-squared	0.7096	0.7064	0.6650	0.5960	0.4056	0.7219	0.5721	0.7416

Table 4. Cross-Sectional Variation by Political Conviction and Control. This table reports the results of multivariate subsample regression on the allocations to Traditional assets, including equity, cash, and fixed income, and Alternative assets, including hedge funds, private equity, and real estate. 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 *LetterSigner*, an indicator set to one if a 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*, set to one for state-level plans. 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 by Political Lean

	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
Year FE	Yes	Yes	Year 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 by Coalition Letter Signers

	LetterSigner = 1			*LetterSigner* = 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
Year FE	Yes	Yes	Year 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 by Political Control Level

	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
Year FE	Yes	Yes	Year 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
Year 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	-0.001	0.001	-0.000	-0.001
	(-0.39)	(0.31)	(-0.86)	(-1.81)
LogHV(t-1)	0.001	0.002	-0.000	-0.000
	(0.30)	(0.48)	(-0.66)	(-0.99)
Year-Quarter FE	Yes	Yes	Yes	Yes
Plan FE	Yes	Yes	Yes	Yes
Observations	735	735	735	735
Adj. R-squared	0.8825	0.5339	0.3627	0.5155

Panel B: Red and Blue State Subsamples

	Red				*Blue*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Energy%_MV	Energy_Count	Guns%_MV	Guns_Count	Energy%_MV	Energy_Count	Guns%_MV	Guns_Count
Post	0.003	0.004	-0.001	-0.000	-0.001	-0.004**	-0.000	-0.001*
	(0.75)	(0.69)	(-0.72)	(-0.38)	(-1.26)	(-4.50)	(-2.70)	(-3.55)
LogHV(t-1)	0.001	0.004	-0.000	0.000	0.006	-0.014**	-0.001	-0.001
	(0.31)	(1.22)	(-1.06)	(0.11)	(1.34)	(-5.58)	(-1.77)	(-1.57)
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Plan FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	484	484	484	484	249	249	249	249
Adj. R-squared	0.8279	0.4625	0.2563	0.4036	0.9891	0.8499	0.9375	0.8131

Panel C: Red versus Blue

VARIABLES	(1) Energy%_MV	(2) Energy_Count	(3) Guns%_MV	(4) Guns_Count
Post	-0.002	0.003	-0.000	-0.001*
	(-0.95)	(0.49)	(-0.57)	(-2.09)
Post*Blue	0.003	-0.005	-0.000	0.000
	(1.21)	(-0.87)	(-1.04)	(0.55)
LogHV(t-1)	0.001	0.002	-0.000	-0.000
	(0.43)	(0.42)	(-0.75)	(-0.91)
Year-Quarter FE	Yes	Yes	Yes	Yes
Plan FE	Yes	Yes	Yes	Yes
Observations	735	735	735	735
Adj. R-squared	0.8826	0.5343	0.3629	0.5149

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. Market Reaction in Target Firms. This table reports results of multivariate regressions on weekly cumulative abnormal returns (CARs) for firms around the date investment restrictions in their headquartered state are introduced. *Event* is an indicator variable equal to one for firms headquartered in a state during the week that state introduces an investment restrictive policy, and zero otherwise. Standard errors are clustered by the state headquarters of firms. The analysis is performed on in-state (firms in states with investment restrictions) energy and firearms firms in red states from 2020 to 2023.

VARIABLES	(1)	(2)	(3)
Event	-0.009	0.008	0.009
	(-1.35)	(0.71)	(0.78)
Log(MarketCap)	-0.004	0.001***	-0.006
	(-1.81)	(9.85)	(-1.68)
BrentRet	0.057***		
	(4.17)		
OPECMeet	-0.002**		
	(-3.37)		
Log(OilProd)	-0.024*		
	(-2.15)		
Month FE	No	No	No
Week FE	No	Yes	Yes
Firm FE	Yes	No	Yes
Observations	15,054	15,054	15,055
Adj. R-squared	0.0019	0.3266	0.3268

APPENDIX A. Additional Empirical Results.

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).

Panel A: OLS Regressions - Plan Fixed Effects								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Traditional	Alternative	Equity	Fixed Income	Cash	Private Equity	Hedge Fund	Real Estate
Post	-0.036***	0.019**	-0.009	-0.022***	-0.005**	0.011*	-0.001	0.009**
	(-3.49)	(2.15)	(-1.04)	(-3.41)	(-2.04)	(2.01)	(-0.17)	(2.19)
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	569	569	569	569	569	569	569	569
Adj. R-squared	0.8565	0.8816	0.8294	0.7952	0.5066	0.8476	0.806	0.8222
Panel B: OLS Regressions - Plan and FYE Fixed Effects								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Traditional	Alternative	Equity	Fixed Income	Cash	Private Equity	Hedge Fund	Real Estate
Post	-0.017**	0.013	-0.007	-0.010	-0.000	-0.009	0.011**	0.012**
	(-2.02)	(1.31)	(-0.81)	(-0.90)	(-0.05)	(-1.15)	(2.45)	(2.39)
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	569	569	569	569	569	569	569	569
Adj. R-squared	0.8603	0.8812	0.843	0.7955	0.5154	0.8534	0.797	0.8218
Panel C: Matched Sample Analysis								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Traditional	Alternative	Equity	Fixed Income	Cash	Private Equity	Hedge Fund	Real Estate
Post	-0.016**	0.012	-0.006	-0.010	-0.000	-0.009	0.009**	0.012**
	(-2.12)	(1.26)	(-0.69)	(-0.98)	(-0.08)	(-1.04)	(2.10)	(2.15)
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	555	555	555	555	555	555	555	555
Adj. R-squared	0.861	0.8828	0.843	0.8007	0.8234	0.8569	0.8141	0.8186

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							
VARIABLES	(1) Traditional	(2) Alternative	(3) Equity	(4) Fixed Income	(5) Cash	(6) Private Equity	(7) Hedge Fund	(8) Real Estate
Post	-0.038***	0.043***	-0.015	-0.022**	-0.001	-0.000	0.024***	0.020***
	(-3.14)	(3.27)	(-1.33)	(-2.53)	(-0.56)	(-0.06)	(2.83)	(3.71)
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	3,858	3,858	3,858	3,858	3,858	3,858	3,858	3,858
Adj. R-squared	0.7572	0.7479	0.7097	0.5823	0.5522	0.7038	0.5853	0.6914

Table A3. 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 A4. 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%

APPENDIX B: Supplemental Material.

Table B1. 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 B2. Supplementary detail regarding Table 6.

13F Managers (as listed in Thomspon Reuters Holdings)
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 B3. Supplementary detail regarding Table 7.

The keyword group used to scrape media attention 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

Table B4. Supplementary detail regarding Table 8.

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

Example of Restricted Financial Company List: Oklahoma House Bill 2034

TODD Russ
STATE TREASURER

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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)(l). 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)(l)(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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