Disagreeing on DEI:
Investor Responses to Anti-DEI Legislations

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Abstract

Do investors care about corporate Diversity, Equity, and Inclusion (DEI) initiatives? We exploit a natural experiment created by Florida's December 2021 announcement of the Stop WOKE Act, which restricts DEI programs in the private sector. Upon announcement, the equity market value of affected firms on average declines by 40 to 120 basis points compared to unaffected firms. The decline is concentrated in firms with more pro-social investors, while other firms experience a positive announcement effect, highlighting significant heterogeneity across investors. The result flips when in August 2022 a federal court blocks the enforcement of the law, further supporting the interpretation that the response by equity markets is driven by the assessment of corporate DEI initiatives as opposed to concerns about political risk in Florida.

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1. Introduction

By 2021, most U.S. public firms had implemented Diversity, Equity, and Inclusion (DEI) initiatives. Since 2025, however, political pressure - particularly under President Trump's renewed administration - and shifting public opinion have fueled a growing backlash against DEI. In this climate, many firms such as Meta, Walmart, and John Deere have scaled back their DEI efforts, while others, like Costco and Apple, continue to uphold their commitments. These contrasting responses raise fundamental questions: What is the value of DEI for firms, and does this value differ across firms in ways that explain why some cave to political pressure while others resist?

DEI initiatives may create value for firms either for cash-flow reasons - such as improving talent acquisition, innovation, or customer appeal - or because shareholders value them for pro-social reasons (Hart and Zingales, 2017, 2022). Both channels imply that the perceived value of DEI can differ across firms, potentially explaining why some resist political pressure while others do not. Yet, despite widespread adoption, we know little about whether DEI creates value and why its impact might differ across firms, largely because endogeneity concerns make causal inference difficult (Edmans, Flammer, and Glossner (2023)). We address this identification challenge by leveraging the exogeneity of Florida's Stop WOKE Act, one of the earliest anti-DEI legislations in the US that directly targets corporate DEI activities.

On December 15, 2021, Florida governor DeSantis announced the Stop WOKE Act, a legislature measure that would ban employers from requiring employees in Florida participate in activities that promote DEI-related concepts, such as implicit bias regarding race, gender, racism, and privilege. While other states had adopted legislation that similarly banned the teaching of certain DEI concepts, all prior state-level restrictions were limited to public state institutions. The public, including market participants, were therefore surprised to learn that the Florida legislature measure extended such DEI restrictions to private sector employers. The measure, which as expected was adopted by the state legislature, signed into law in May 2022, and became effective on July 1, 2022. Although the law did not ban all DEI initiatives, legal scholars and corporate executives expressed concern that "employers [may] shy away from inclusivity training for fear of violating the Stop WOKE Act."[1]

[1] Sara Margulis, CEO and co-founder of Honeyfund, a Florida-based company (https://news.yahoo.com/florida-stop-woke-act-could-130620653.html).

While the law's enforcement in the private sector was ultimately permanently blocked by a federal judge in June 2024, we exploit the equity market reaction to the initial introduction of the law in December 2021 as an opportunity to understand the value that investors place on corporate DEI initiatives. The introduction of the legislation is plausibly unrelated to the fundamentals of firms headquartered in Florida or with relatively large numbers of employees in Florida, allowing for a causal interpretation of the effect of the legislation on the abnormal returns of affected firms.

However, the proposed legislation could heighten investors' concerns about the Florida state government's increasing willingness to regulate or interfere with firms' operations, introducing political risk. We address this identification challenge by examining the market reaction to the federal court decision that blocked enforcement of part of the law. This decision removed specific restrictions on DEI initiatives for businesses but did not eliminate the broader policy risk firms face in Florida.

We find that around the announcement event in December 2021, firms headquartered in Florida or with 100% of their employees or operations in Florida experience cumulative announcement returns (CAR[-1,+3]) that are between 50 and 120 basis points lower than those of firms not headquartered or without employee or operations in Florida. This effect persists across multiple specifications, different control groups, alternative event windows, and different return models. We also find no evidence of pre-trends or short-term price reversals.

The effect is significantly more pronounced for firms in industries for which DEI is financially material in the assessment of the Sustainability Accounting Standards Board (SASB) relative to firms in other industries. The evidence that the market specifically cares about firms' DEI initiatives as opposed increased policy risk in Florida is further strengthened by a more or less complete reversal of the effect around the court decision to block the enforcement of the Stop WOKE Act. Importantly, the reversal effects are again significantly larger for firms in industries for which DEI is considered essential.

In addition to examining firms for which DEI is considered financially material, we explore the role of investors' prosocial attitudes. Specifically, we infer a firm's institutional investors' prosocial attitudes through their revealed preference of holding firms with high vs. low social (S) scores as part of their portfolio firms' overall ESG scores (e.g., Cao et al. (2023), Pan et al. (2022)). Averaging across a firm's institutional investors' prosocial attitudes, we find that treated firms whose investors exhibit above median prosocial preferences experience significantly larger negative announcement returns than firms whose

investors exhibit below median prosocial preferences. To disentangle whether the effect of investors' pro-social attitudes reflect investors' preferences or their subjective beliefs about economic benefits of corporate DEI initiative, we examine the interaction between investor's prosocial attitudes and DEI materiality. We find that pro-social investors react negatively to DEI restrictions in all industries, but their reaction is stronger when DEI is financially material. This suggests that both pro-social preferences and economic beliefs influence investors' responses to DEI initiatives.

Finally, we provide additional evidence on the important role of heterogeneous investor demand. Specifically, examining investors' portfolio holdings, we find that investors reallocate their holdings away from Florida firms after the Stop WOKE Act, and this allocation is stronger for investors with a stronger pro-social preference. These results highlight heterogeneous investor demand as an important driver of the equity market assessment of DEI initiatives.

Our paper contributes to attempts to uncover the shareholder value of diversity, equity, and inclusion within firms. While several papers have examined the effect of board or team diversity (e.g., , Lu, Naik, and Teo (2024)), establishing the value of diversity has been challenging as policies such as mandating female board representation can have multiple possibly offsetting effects. Firm-wide diversity or corporate DEI initiatives have received less attention, despite ongoing debates on its business case (Mckinsey (2023) vs. Green and Hand (2024)), likely due to limited data and difficulties in quantifying equity and inclusion. Edmans, Flammer, and Glossner (2023) examine employee perceptions of DEI at their workplace, and document positive associations between DEI perceptions among employees and future accounting performance and firm valuation ratios. They also find that perceived DEI is higher in small growth firms and firms with stronger financials, highlighting the difficulty of inferring a causal effect of DEI on firm value. We are able to exploit an unexpected legal ban on corporate DEI initiatives and thereby contribute to determining the value of corporate DEI programs as perceived by equity markets.

While our findings are in line with experimental and field evidence about the existence of prosocial preferences among investors (e.g., Riedl and Smeets (2017), Bonnefon et al. (2022), Heeb et al. (2023), Humphrey et al. (2023)), our results go beyond earlier results by Flammer (2015) who documents a positive announcement return to the surprise adoption of shareholder proposals on CSR and Hartzmark and Sussman (2019) who find a positive capital inflow into mutual funds that experience an unexpected increase in sustainability ratings. In particular, we highlight the importance of investor heterogeneity (Pan et al.

(2022)). Investors might increasingly view DEI initiatives differently either because the nature of such initiatives has evolved or because DEI has been more and more politicized as evidenced by the 99 anti-DEI bills that have been proposed in 33 states since January 2021 (American Physical Society (2023)). At the same time, even pro-DEI investors might increasingly question the value of corporate DEI initiatives if they are perceived as "diversity washing" (Baker et al. (2024)).

This paper also contributes to the literature on the value of CSR/ESG activities for corporations. First, while prior research has focused on ESG/CSR as a whole, or E, S, and G as broad categories, different ESG activities could have distinct, and potentially opposing, effects on firm value (Edmans (2023)). Our paper addresses this limitation by focusing specifically on DEI – a key social issue – responding directly to Edmans' call for more granular ESG research beyond broad categories. Second, while previous studies have provided valuable insights into market reactions to ESG-related news in various contexts (Krüger (2015), Derrien et al. (2022), Serafeim and Yoon (2022), Serafeim and Yoon (2023), He, Kahraman, and Lowry (2023), Daniels et al. (2024) among others), we differentiate ourselves by examining a policy shock. Since this policy is determined exogenously to firms' fundamentals, it helps rule out various alternative explanations at the firm level, including potential endogeneity in firms' public relations.

2. The Stop WOKE Act

The Stop WOKE (Stop Wrongs to Our Kids and Employees) Act, officially known as House Bill 7, was first announced on December 15, 2021, and signed into law by Florida Governor Ron DeSantis in 2022. This law limits how educational institutions and employers can address topics related to race and identity. It bans mandatory trainings or teachings that suggest individuals of a particular race, color, sex, or national origin are inherently racist or should be held responsible for the actions of others in the past. One of the prohibited concepts includes the idea that individuals, based on their race, color, sex, or national origin, should be discriminated against or treated unfairly to promote diversity, equity, or inclusion (DEI).[2]

The rationale behind the Stop WOKE Act is to protect individual rights, as its other common name suggests, the "Individual Freedom Act." Its restriction on businesses focuses on ensuring that workers are not required to participate in trainings or discussions that impose guilt or responsibility based on race, sex,

[2] https://www.flsenate.gov/Session/Bill/2022/148/BillText/Filed/HTML

or national origin. The law emphasizes personal freedom in the workplace, without any references to investor protection or market failures.

The Stop WOKE Act is enforced as an amendment to Section 2 of Florida Statute 760.10, part of the Florida Civil Rights Act of 1992. This statute prohibits discrimination in employment based on race, color, religion, sex, pregnancy, national origin, age, handicap, or marital status. By expanding these provisions, the Stop WOKE Act allows employees to sue employers if certain prohibited concepts are included in mandatory training or instruction. The law applies to Florida employers with 15 or more employees who are employed for 20 or more calendar weeks.

The Stop WOKE Act is expected to affect many companies, as 80% of corporations with diversity training make it mandatory (Dobbin and Kalev, 2018). Legal experts have highlighted its potential to weaken corporate DEI initiatives. Fisher Phillips, a prominent law firm, noted that "although you could still deliver workplace diversity training for your Florida employees, many employers were afraid of unwittingly running afoul of the new parameters."[3] This sentiment is echoed by the CEO of Honeyfund, a Florida-based company, who expressed concern that "employers [may] shy away from inclusivity training for fear of violating the Stop WOKE Act."[4] Even if DEI training is not explicitly banned, the Act could significantly hinder these initiatives by limiting top-level support and follow-through.

The announcement of the Stop WOKE Act marked an unexpected and significant shift in the legal landscape concerning DEI initiatives in business settings. While restrictions on DEI in education, such as the Supreme Court's decision in *Students for Fair Admissions v. Harvard* and earlier state laws like Oklahoma's HB 1775 and Iowa's House File 802, had been debated for years, the Stop WOKE Act was the first formal law to directly target corporate DEI practices. This shift was unexpected, given that just months prior, Florida had primarily focused on limiting DEI in educational settings, with Governor Ron DeSantis and officials publicly criticizing Critical Race Theory (CRT) and banning it from public school classrooms in June 2021. The sudden extension of restrictions to businesses in December 2021 caught many by surprise and garnered immediate national attention.[5] Below is a brief timeline of the Act:

[3] https://www.fisherphillips.com/en/news-insights/floridas-stop-woke-act-struck-down.html

[4] https://news.yahoo.com/florida-stop-woke-act-could-130620653.html

[5] For example, see https://www.washingtonpost.com/politics/2021/12/15/desantis-stop-woke-act-mlk-crt/; https://www.forbes.com/sites/nicholasreimann/2021/12/15/desantis-unveils-stop-woke-act-so-parents-can-sue-over-

Dec 15, 2021: Florida Governor introduces the Stop WOKE Act

Mar 10, 2022: Florida lawmakers pass the Stop WOKE Act into law.

Apr 22, 2022: Florida Governor signs the Act into law, making it official in Florida.

Jul 1, 2022: The law takes effect across Florida.

Aug 18, 2022: A federal judge blocks the law's enforcement on workplace diversity training.

Nov 17, 2022: The federal judge halts the law's enforcement in Florida's higher education system.

Mar 21, 2024: A federal appeals court upholds the earlier ruling on Aug 18, 2022.

Jul 26, 2024: The federal judge permanently blocks the law's workplace diversity training restrictions.

3. Data, sample, and empirical design

3.1. Data and sample

I collect daily stock return data for North American companies from Center for Research in Security Prices (CRSP). From raw daily returns, we calculate abnormal returns using different methods: 1) abnormal return relative to the market return, proxied by the CRSP value-weighted daily return, as in Pan et al. (2022); 2) abnormal return relative to different factor models, namely the Capital Assets Pricing Model (CAPM), the Fama and French 3-factor model, and the Fama and French 5-factor model. As commonly done in the literature, we estimate these factor models for each stock using an estimation window of 252 trading days ending in the two months before the event date (the announcement of the Stop WOKE Act – December 15, 2021), or between Oct. 15, 2020, and Oct. 15, 2021.

We obtain firms' accounting records from Compustat North America. As these allow us to control for firm characteristics before the event (December 15, 2021), we keep accounting records for data dates only in 2020. Compustat also provides information on the state of a firm's headquarters – an important variable for this study. We merge the Compustat and CRSP databases using 6-digit CUSIP as a firm's unique identifier.

Next, we collect firms' actual business locations across different US states using InfoGroup data, as used in Pan et al. (2022). For each firm, we collect their number of branches, employee count estimate, and sales volume estimate for each state the firm has some business in, all recorded in 2020. We merge these

critical-race-theory-in-schools/amp/ ; https://www.bloomberg.com/news/articles/2021-12-15/desantis-dangles-personal-lawsuits-over-critical-race-theory

data to Compustat using a fuzzy matching procedure based on firm name and the state of a firm's headquarters address.

In addition, we gather state-level measures to capture location-specific social preference for diversity, equity, and inclusion (DEI), similar to how Pan et al. (2022) captures location-specific aversion to income inequality. Specifically, we collect the minimum wage, tax rate dispersion, and the percentage vote for the Democratic party for each US state, all recorded in 2020.

Finally, we collect investors' holdings of securities using Thomson-Reuters Institutional Holdings (13F) Database. We augment this database to collect investors' location using their addresses listed on their Forms 13F filed with the SEC, using the SEC 13F filing Data Sets.[6]

As we merge across these datasets, we obtain our main sample of 4,172 firms with stock return data and headquarters state information. This sample size is within a reasonable range, lower than the 4,771 U.S. listed domestic companies reported by the World Bank for 2021,[7] but higher than that of prior studies such as Pan et al. (2022) who restricted their sample to firms with available business location data. In our sample, 2,968 firms (71%) are successfully merged with the business location data from Infogroup. Table I provides summary statistics.

3.2. Empirical design

To study the market reaction to the Stop WOKE Act, we calculate each firm's abnormal return around the announcement of the Act, on December 15, 2021. As a common practice in event studies, we focus on the announcement date instead of the enforcement date of the Act, because by the Act's enforcement date, which is seven months after its announcement, there may not be any news to the market anymore.

In our baseline specification, we study each stock's cumulative abnormal return (CAR) between the one trading day before the event and the three trading days after, or CAR[-1,+3]. The choice of the three trading days after the event allows for the possibility that the market needs a few days to react to the new information. This event window is similar to baseline specifications in recent event studies (e.g., Pan et al. (2022), Child et al. (2021)). Brown and Warner (1985) document that multiple-day event studies are

[6] https://www.sec.gov/dera/data/form-13f

[7] https://data.worldbank.org/indicator/CM.MKT.LDOM.NO?locations=US

generally well-specified and if anything, longer event window is more conservative (MacKinlay (1997)). Regardless, we will examine robustness with various shorter event windows.

We then compare the cumulative abnormal return of firms that were affected by the Act (treated firms) to the cumulative abnormal return for other firms in the US (control firms). We follow the literature (e.g., Al-Sabah and Ouimet (2021)), to consider both a firm's headquarters and business location to define the treated firms. However, because restricting our sample to firms with available business location data from Infogroup reduces the sample size by 29% and Infogroup only reports estimated employee counts with unknown precision, we use headquarters information to define treatment in our baseline tests. Nevertheless, we provide robustness analyses with alternative treatment definitions using the business location data.

There are many ways to define control firms, but in our baseline tests, we use a firm's headquarters information to classify control firms to be firms headquartered outside of Florida but within the US, for a few reasons. First, it is simple and easily replicable. Second, the alternative of setting the control firms to firms located in nearby states like Alabama and Georgia may suffer from a positive spillover effect, as investors may expect the new law in Florida to set a precedent for Alabama and Georgia to follow. Similarly, the alternative of setting the control firms to firms located in faraway states like Oregon may suffer from a negative spillover effect, as investors may shift their portfolio from Florida firms to Oregon firms in response to the Stop WOKE Act. Thus, using all firms in all the US states other than Florida as the control group can potentially net out these spillover effects because of their opposite signs. Section 4.2 discusses these spillover effects in detail and the robustness of the results to alternative sets of control firms.

To sum up, and to formally evaluate market reaction to the Stop WOKE Act, we follow Pan et al. (2022)'s event study design to run the following regression:

$$CAR_{i,[-1,+3]} = \alpha + \beta_1 \times Treat_i + \Gamma \times X_i + \epsilon_i \quad (1)$$

Where $Treat_i$ equals one for firms headquartered in Florida in 2020, and zero for other firms; X_i captures a set of firm-level control variables, such as book-to-market ratio and size (log of market capitalization) as in Pan et al. (2022), as well as industry fixed effects. Market capitalization is measured one month before the event date. Book-to-market ratio is based on book equity recorded in Compustat in 2020. Following Pan et al. (2022), we calculate cumulative abnormal return (CAR) in our baseline tests to be the return relative to CRSP value-weighted index return, while performing robustness tests with CAR relative to different factor models, from the CAPM model to the Fama-French 5-factor model.

Following the recent econometric framework of Abadie et al. (2023), standard errors are clustered at the state level, as the treatment (the Stop WOKE Act) is set at the state level. This approach captures cross-sectional dependencies among firms within each state and is more conservative than firm-level clustering.[8] Previously, researchers used a portfolio approach to address potential cross-sectional dependencies, so we provide robustness checks with that method as well (e.g., Schwert (1981), Schipper and Thompson (1983)), though Brown and Warner (1985) argue that such an approach is generally unnecessary with a potentially large reduction in power.

Returns are changes in firm value, so comparing cumulative returns of the treated and control firms is effectively a difference-in-differences (DiD) test. Nonetheless, as Cohn et al. (2022) noted, cross-sectional event studies may fail to control for correlated exposure to confounding events. Thus, among other robustness tests, we follow Cohn et al. (2022)'s recommendation to compare the return spread between the treated and control firms on the event week with the same spread in many pre-event periods, which lets us directly control for firm and time fixed effects.

If investors market-wide value DEI initiatives by an average firm, firms affected by the Stop WOKE Act – a restriction on DEI programs – should experience a negative market reaction, on average, relative to other firms. In that case, the estimate for β_1 will be negative. However, if investors market-wide view DEI initiatives as value-destroying, the estimate for β_1 will be positive.

Underlying the research design is the assumption that the adoption of the Stop WOKE Act is exogenous to firm-level stock returns. Karpoff and Wittry (2018) discuss several factors where a state law change may not be exogenous, namely 1) firms opting out of coverage 2) firms lobbying 3) anticipation and 4) confounding events like other state laws around the same time. The law specified no opt-out conditions at its announcement and firms are unlikely to lobby for a law to restrict their operations, so the first two concerns can be ruled out. Section 4.2 discusses robustness to the remaining concerns, among others.

[8] Abadie et al. (2023) argue that as the number of clusters shrinks (e.g., from firm-level clustering to state-level clustering), the power of the test decreases, i.e., less likely to find significant results. In un-tabulated tests, the results are similar if standard errors are clustered by both firm and state, though clustering by firm is not meaningful as the regression is cross-sectional, with one observation per firm, so there is no clustering per firm to begin with. Similarly, clustering by time is unnecessary because each regression is concerned with the same event window for all firms.

4. Do investors value DEI initiatives?

4.1. Main results

Figure I. illustrates the paper's key discovery: DEI (diversity, equity, and inclusion) initiatives are viewed positively by investors in terms of value. Following the announcement of the Stop WOKE Act, which limits DEI efforts, the impacted companies saw an average drop of 1.232 percentage points in their cumulative abnormal returns during a 5-trading-day window around the announcement, in comparison to unaffected companies. This decrease began to manifest two days prior to the announcement but was most significant on the announcement day and the subsequent three trading days. Many days before the event, by contrast, the returns of the treated and controls firms follow each other very closely, suggesting no pre-trends.

The economic magnitude of the market reaction on the Act's announcement considerable, with the observed reduction in stock prices exceeding the market's response to the unexpected death of a company's CEO, as documented in Jenter, Matveyev, and Roth (2023). In terms of market value, this represents a decline of approximately 65 million US dollars for the average publicly traded company in Florida.

To evaluate the statistical significance of this finding, Table II Panel A shows the baseline regression results, as discussed in equation (1). Without control variables (column 1), the coefficient on the treatment indicator (firms headquartered in Florida) is -1.232, which is statistically significant at the 1% level (t-stats = -6.063). After controlling for industry fixed effects, the coefficient remains large, at -1.084, and statistically significant at the 1% level (t-stats = -10.334). Controlling for book-to-market ratio, size, and past year return, individually and all together, changes neither the statistical significance nor the economic magnitude of the results, with the coefficient remains lower than -0.839, as shown in columns (3) to (5). The Internet Appendix Table 1 shows that the market reaction to the Stop WOKE Act remains similarly negative and significant when we control additionally for other firm characteristics that have been shown to predict returns, such as assets growth (e.g., Cooper, Gulen, and Schill (2008)), profitability (e.g., Fama and French (2006)), and investment (e.g., Hou, Xue, and Zhang (2015)).

Table II Panel B repeats Table II Panel A but controlling for firms' exposure to unobservable return factors by comparing the difference between the treated and control firms' returns during the event week CAR[-1,+3]) with the same difference in the twenty weeks before the event. This specification allows us to

control for firm fixed effects as in a typical DiD analysis, which should capture any fixed differences between the treated and control firms' exposure to any risk factors. Panel B shows that the baseline results remain very similar, with or without controlling for the differential reactions by firms of different book-to-market ratios, sizes, and past one-year returns. The results are similar when we use the returns in the 200 weeks before the event as the benchmark. In addition, using a portfolio approach to account for cross-correlations among sampled firms as in Jaffe (1974), Schwert (1981), Schipper and Thompson (1983), and more recently Eckbo, Nygaard, and Thorburn (2022), we calculate the difference between the return of a portfolio of the treated firms and that of the control firms and find that the difference significantly widens after the event relative to before the event (see Figure II. for a DiD graph with confidence intervals).[9]

These results suggest that the negative market reaction to the Stop WOKE Act was unlikely due to Florida firms having different exposure to common risk factors associated with firm characteristics and industry characteristics, relative to other firms. Nonetheless, we estimate firms' exposure to different risk factors via well-known factor models, to remove the exposure from the calculated cumulative abnormal returns (CARs). Specifically, the Internet Appendix Table 2 shows that the market reaction to the Stop WOKE Act remains negative and significant when we calculate CARs as returns relative to the CAPM model, the Fama-French 3-factor model, and the Fama-French 5-factor model.

Overall, the baseline results suggest that investors collectively value corporate DEI initiatives.

4.2. Robustness

This section discusses various robustness tests and evaluates alternative explanations to the negative market reaction to the Stop WOKE Act, as opposed to the main inference that investors collectively value DEI initiatives in an average firm.

4.2.1. Treatment intensity

In the baseline results, we define treated firms as firms with headquarters in Florida and control firms as other firms in the US. An alternative is to define treatment in a continuous manner, by measuring the

[9] We do not use the portfolio approach as the baseline test because it does not allow for cross-sectional analyses, such as tests for different mechanisms in Section 5. In addition, Kolari and Pynnönen (2010) argue that the portfolio approach "is generally suboptimal due to lower power than other alternatives."

fraction of a firm's businesses or employees that are in Florida and thus exposed to the Stop WOKE Act. In other words, treatment could be defined as treatment intensity, instead of a simple indicator.

The advantage of measuring treatment intensity is that it is closer to the potential enforcement of the Act, which covers employment relationships in Florida. The disadvantage of doing so is that investors may not react to the new law based on the actual location of a firm's employees, but instead on a general impression about the firm's location. In addition, there are two imperfections with the data we can use to measure firms' geographic exposure to Florida. First, using the Infogroup database to capture firms' business locations, as in Pan et al. (2022), our sample size declines by 29%. Second, while Infogroup has data on the actual addresses of a firm's establishments, it can only estimate the number of employees per establishment, the precision of which is hard to verify.

Notwithstanding these caveats, we re-run the baseline tests by replacing the treatment indicator with some measure of treatment intensity. We measure treatment intensity in two ways: the fraction of a firm's branches that are in Florida, and the fraction of a firm's estimated employee count that is attributed to its Florida branches. The first measure is more precise, but the second measure is closer to the potential enforcement of the treatment (the Stop WOKE Act). Thus, we report the results for both.[10]

Table III Panel A shows the results where the treatment intensity is calculated based on employee count estimates. The coefficient on the treatment intensity is -1.466 in column (1), indicating that compared with a firm with no employee in Florida, a firm with 100% of its employees in Florida experienced about 1.5 percentage-point decline in the cumulative abnormal return around the announcement of the Stop WOKE Act. This estimate is statistically significant at the 5% level and becomes significant at the 1% level after controlling for industry fixed effects (column 2). It remains similarly significant after controlling for other firm characteristics (columns 3 to 5).

Table III Panel B shows the results where the treatment intensity is calculated based on a firm's number of branches in Florida. The coefficient on the treatment intensity is -2.497 in column (1), indicating that compared with a firm with no branches in Florida, a firm with 100% of its branches in Florida experienced over 2.4 percentage point decline in the cumulative abnormal return around the announcement of the Stop WOKE Act. This estimate is not only larger than the estimate using employee counts, but also more

[10] Infogroup also estimates establishment-level sales volumes. However, we do not use these estimates to measure treatment intensity because the Stop WOKE Act affects firms by the location of employment, not the location of sales.

statistically significant: it is significant at the 1% level across all the columns. The results are consistent with the measure based on branches capturing treatment intensity better than the measure based on employee count.

Overall, the baseline results are robust to alternative definitions of treatment and treatment intensity.

4.2.2.Spillover effects and the choice of control firms

The Stop WOKE Act in Florida may have a spillover effect on the valuation of firms located in other states. This could happen, if investors anticipate that states with a similar political climate to Florida, i.e., other Republican states, would follow Florida to pass a similar law to restrict DEI initiatives. Under this positive spillover effect, the valuation of firms in other Republican states would decline after the announcement of Florida's Stop WOKE Act as well. Therefore, if we set firms in the other Republican states as the control firms, the estimated effect of the Act on Florida firms' valuation would appear relatively lower.

In contrast, this positive spillover based on similar political climates is less likely to affect firms located in Democratic states, as these states are less likely to follow Florida in establishing a similar law. If anything, there may be a negative spillover effect. That is, upon the Stop WOKE Act's announcement, investors may rebalance their portfolios away from Florida firms to firms located in Democratic states. Under this negative spillover effect, the valuation of firms in Democratic states would increase after the Act was announced. Thus, if we set the control firms to be firms in Democratic states, the estimated effect of the Act on Florida firms would appear higher.

In the baseline tests, we include firms in all the US states other than Florida as the control firms, so the positive and negative spillover effects above may offset each other. Nonetheless, we perform these tests again, but now replace the set of control firms to either firms in all the other Republican states or firms in all the Democratic states.

Table IV shows the results. Panel A reveals that the estimated treatment effect is -0.738% when the control firms are firms headquartered in the other Republican states in the US. This estimate, after various controls, remains highly statistically significant, at the 1% level (columns 2 to 5). While a decline of over 70 basis points in firm value within a 5-day window is still large, this estimate is lower than the baseline

estimate. The results, thus, are consistent with a positive spillover effect that investors anticipate a higher chance of other Republican states to follow Florida to restrict corporate DEI initiatives in the future.

Table IV Panel B shows the results when we set the control firms to firms in all the US Democratic states. Column (1) indicates that the estimated treatment effect is -1.418, which is statistically significant at the 1% level. The estimate continues to be large and similarly significant after controlling for many firm characteristics and industry fixed effects (columns 2 to 5). Compared with the baseline results, the treatment effect estimated here is larger, consistent with a negative spillover effect: investors reallocate their holdings from Florida firms to firms in Democratic-leaning states.

These results not only suggest that some spillover effects exist, but also show that the baseline results are robust to different sets of control firms based on political distance to the treated firms. How about different sets of control firms based on different geographical distances to Florida? So next, we investigate whether the results change when we set the control firms to firms headquartered in states that are near vs. far away from Florida.

Table IV Panel C shows the results when we set the control firms to firms headquartered in Florida's nearby coastal states, namely Louisiana, Mississippi, and Alabama to the left of Florida, as well as Georgia, South Carolina, and North Carolina to the right of Florida. Since these states are all Republican-leaning, we expect the results to be similar to Panel A, if there are no spillover effects beyond the spillover based on political leaning. We find that the treatment effect in Panel C column (1) is estimated to be -0.728 percentage point, and significant at the 10% level. While the estimate remains negative, it sometimes becomes insignificant after many control variables, consistent with a positive spillover effect to these nearby states (columns 2 to 5).[11]

Table IV Panel D shows that the results become stronger when we set the control firms to firms headquartered in coastal states far away from Florida, like Oregon and Washington. The treatment effect, in this case, is estimated to be -1.799 percentage point, which is statistically significant at the 1% level and remains so after various controls. The larger estimate here is consistent with the negative spillover effect discussed earlier as well, because the coastal states far away from Florida are Democratic leaning.

[11] The statistical significance appears lower than in previous tests, which is understandable because a low number of clusters often lead to high standard error estimates (Abadie et al. (2023)). Since the standard errors are clustered by state, there are only seven clusters (Florida and six nearby states) in this specific test.

Finally, we perform nearest neighbor matching to restrict the control firms to only firms in the same industry with the treated firms, and with a similar size, book-to-market, and past one-year return. This analysis ensures that the results are not driven by the differences in firm characteristics of the treated and control groups. We find that the baseline results are unchanged with this matched control analysis, as shown in the Internet Appendix Table 4.

Overall, the results suggest that spillover effects exist, and the baseline results are robust to many alternative choices for the set of control firms.

4.2.3. Opposite interpretation

While we interpret the negative market reaction to the Act restricting DEI initiatives as evidence for investors' valuing these initiatives, it is possible to interpret it in the opposite way. In particular, if investors had anticipated that Florida would pass a very restrictive law on DEI but realized that the Stop WOKE Act would be less restrictive than expected upon its announcement, then the negative market reaction could be seen instead as investors discounting DEI initiatives. However, since the Stop WOKE Act was the first of its kind in restricting corporate DEI practices, it is hard to imagine that investors could have foreseen much of it. Moreover, if investors had anticipated some stronger version of the law, the anticipation should have been incorporated into stock prices before the event. Yet, controlling for firms' prior year returns does not change the baseline results.

Moreover, if investors value restrictions on DEI efforts, they would react negatively when those restrictions stop. Such a stop did happen on August 18, 2022, when a US District judge declared parts of the Stop WOKE Act relating to workplace diversity training unconstitutional. So, we conduct the same analysis as the baseline test, but replace the event date of December 15, 2021, with August 18, 2022. Table V shows that from the two days before to the five days after August 18, 2022, the treated firms experienced a cumulative abnormal return of between 1.119 and 1.376 percentage points higher than the control firms. The treatment effect is statistically significant at the 1% level after controlling for industry fixed effects and many firm characteristics.. These results imply that the initial reaction to the Stop WOKE Act is reversed when the Act was paused, further supporting the interpretation that investors on average value DEI initiatives.

4.2.4.Potential confounding events

State laws are influenced by their context (Karpoff and Wittry (2018)). The Stop WOKE law was proposed over a year after the COVID-19 pandemic onset and George Floyd protests. While significant, these national events alone cannot explain our results due to our difference-in-differences methodology. States' varying responses to these events are more likely explanatory factors. However, given our focus on stock market reactions, any such policies or responses should have been priced in within a year, making them unlikely to account for our findings.

To explain our results, any confounding event must have affected Florida firms around the time of the Stop WOKE Act's proposal. So, we perform a search for major news about Florida in the five days surrounding the Act's announcement on December 15, 2021. One event was on December 14, 2021, when President Biden approved a disaster declaration for Florida due to the impacts of Hurricane Nicole, which occurred from November 7 to November 30, 2022. This declaration made federal funding available to assist individuals, state, local governments, and certain nonprofit organizations in the recovery process in affected counties. If anything, this was good news for businesses in Florida, so it is unlikely to explain the negative market reaction documented in this paper.[12]

Another potentially confounding event is the surge of COVID cases due to the Omicron variant in December 2021. However, this surge is unlikely to explain the Florida firms' value decline in the week of December 15 because of a few reasons. First, the surge was recorded to be starting at around Christmas time, or more than one week after the Stop WOKE Act's announcement.[13] Second, most news coverage of the surge was released on an even later date, like December 26, 2021.[14] Third, the Omicron surge affected the whole US, not just Florida.[15] Finally, we perform the baseline regressions again, but now specify the event date to be December 26, and found a slightly positive market reaction instead (un-tabulated).

One possibility left is that the Stop WOKE Act's announcement itself contained information not about corporate DEI practices per se. As Section 2 describes, the Act also puts restrictions on DEI teaching at

[12] https://www.whitehouse.gov/briefing-room/statements-releases/2022/12/14/president-joseph-r-biden-jr-approves-florida-disaster-declaration-2/

[13] https://coronavirus.jhu.edu/region/us/florida

[14] https://www.wusf.org/health-news-florida/2021-12-26/florida-breaks-record-for-new-coronavirus-cases-as-positivity-rate-surges

[15] https://coronavirus.jhu.edu/region/united-states

schools and universities. If investors expect these restrictions to harm Florida's future workforce quality, it could explain the decline in Florida firms' value. While it is hard to test this hypothesis directly, Oklahoma's House Bill 1775 provided a placebo test. The Bill was first drafted to require schools to have emergency medical plans for athletic activities, but it was rewritten to ban critical race theory, passing on May 5, 2021. So, we perform the baseline tests again, but now set firms headquartered in Oklahoma as treated firms and May 5, 2021 as the event date. As shown in Internet Appendix Table 3 (Panel A), this yields no consistently significant results. Another similar placebo event is Iowa's House File 802, introduced as House Study Bill 258 on February 25, 2021.[16] The Internet Appendix Table 3 (Panel B) does not show a negative market reaction to this event either, though we advise caution in interpreting these results as they are based on few firms (16 for Iowa and 27 for Oklahoma in our sample).

Further evidence against confounding events comes from cross-sectional tests in Section 5. Firms outside Florida with significant DEI involvement experienced a positive market reaction during the same period, contrasting with the negative reaction of Florida-based firms with significant DEI involvement. This disparity suggests the observed negative effect is specific to Florida's anti-DEI law, rather than broader DEI-related trends, is responsible for the observed market reaction.

Overall, there appeared to be no confounding events that could explain the baseline results.

4.2.5.Different event windows and other robustness checks

The baseline tests focus on cumulative abnormal returns from the one trading day before to the three trading days after the Act's announcement. However, there may be information leakage about the Act many more days before its announcement. Equally likely, there may be underreaction by market participants to the Act, as it may take time to understand the implications of the Act on firm value. Therefore, we perform the baseline regressions again but replacing CAR[-1,+3] by CAR[-3,+3], CAR[-2,+8], or CAR[-1,+3], among others. We find that the main results remain robust to these different event windows. Table 5 in the Internet Appendix presents these results, along with a breakdown of the market reaction on individual trading days before and after the event, following the approach of Serafeim and Yoon (2022). Notably, this analysis reveals the most significant stock price drops on days t-2, t, and t+2 relative to the event.

[16] https://www.legis.iowa.gov/legislation/billTracking/billHistory?billName=HF%20802&ga=89.

Relatedly, an alternative explanation for the baseline results is that investors overreacted to the Act's announcement, creating a short-term value decline for Florida firms. If so, the value decline may be reversed in a longer event window. To test this hypothesis, we plot how cumulative buy-and-hold returns changed for Florida firms vs. control firms over the three months following the Act's announcement.[17] One caveat with this test is that over a longer horizon, many events can happen to the treated and control firms, creating noisy divergences in their paths. Thus, we focus on firms in coastal states near Florida as the control firms, as these states share similar economic and geographic attributes with Florida and thus are likely to share similar trends over a longer horizon. The Internet Appendix Figure 1 shows that the return difference between the treated and control firms remains similar even after three months following the event.

Finally, the baseline results may be explained by something unique about Florida, such as its political climate. Any such Florida-specific risk factor is likely to affect the return of Florida stocks outside of the event window, such as stock returns over the one-year period before the event. The fact that the baseline results hold after controlling for a firm's past year return helps rule out this possibility.

5. Mechanisms and discussions

This section describes and tests the potential mechanisms underlying the investors' valuation of corporate DEI initiatives, as well as discusses nuances in interpreting the main results.

5.1. Main mechanisms

The adverse reaction of the market to the Stop WOKE Act leads us to dismiss the notions that on average investors either disregard diversity, equity, and inclusion (DEI) efforts or see them unfavorably. However, it raises the question of why investors value DEI. Identifying a precise reason is challenging, but we propose two potential explanations linked to financial or non-financial benefits.

From a non-financial standpoint, investors might prioritize DEI because of their pro-social preferences, thus favoring companies with strong DEI commitments, regardless of their financial performance—this is what we term the investor demand channel. On the financial side, investors could see DEI as beneficial for future cash flows because DEI can help companies gain support from customers (e.g., Albuquerque,

[17] Calculating cumulative returns over a long period of time requires compounding daily returns, which makes sense with raw returns but not abnormal returns. Therefore, we study buy-and-hold returns instead of cumulative abnormal returns to study longer-horizon return differences.

Koskinen, and Zhang (2019), Hacamo (2023)), employees (e.g., Choi et al. (2023)), and enhance a company's talent pool and cognitive diversity, which could help improve group decision-making and employee engagement (see Mannix and Neale (2005) for a review).

To test these channels, we explore how the strength of the negative market reaction to the Stop WOKE Act varies in the cross-section. If the financial channel proves significant, industries where DEI is considered financially material will show a more pronounced market response. Under the non-financial (social) channel, firms whose investors exhibit stronger pro-social preferences should experience a more significant response to the Act.

To capture how important DEI is to a firm's cash flows, we follow the Sustainability Accounting Standards Board (SASB) materiality map to identify in which industry DEI is deemed financially material. SASB is a non-profit created to establish sustainability accounting standards that help public corporations disclose material, decision-useful information to investors. Existing academic literature has often used the SASB sustainability map, such as Khan, Serafeim, and Yoon (2016).[18]

As for measuring "investor (social) preference" for a firm, we use a revealed preference approach that prior literature has used to capture investors' preferences based on their holdings (e.g., Cao et al. (2023), Pan et al. (2022)). Specifically, we first calculate the value-weighted average S (social) scores of the stocks each investor holds in 2021 Q3, using 13F filings data and the MSCI ESG ratings – the most widely used ESG ratings. For individual investors, we infer their ownership of a firm by one minus the sum of all 13F filing investors' ownership fractions in the firm. We calculate individual investors' ESG preferences the same way we do with 13F filing investors by simply assuming individual investors as one unique investor. Then we measure the social preference of a firm's investors by the weighted average S score across these investors using their ownership percentage of the firm as the weight. For robustness, we also calculate the same measure for E (environmental) and G (governance) preferences. [19, 20]

[18] We collect the DEI materiality indicator of 77 industries from the SASB website, and then manually match these 77 industries to the 48 Fama-French industries used throughout this paper.

[19] We also use a location-based approach to capture investor preference. The key results are robust to using this measure, as described in the Internet Appendix and the associated Table 6.

[20] Using E and G scores instead of the S score gives weaker or not significant results (un-tabulated), suggesting that investors' preferences on S rather than E or G can explain the market reaction to the Stop WOKE Act.

To test the channels, we thus run the same regressions as in the main tests, but now allow the treatment indicator to interact with each of the following indicators: 1) DEI materiality indicator – equaling one for industries that SASB indicates to have DEI as a material factor, and zero otherwise 2) High investor preference – indicating whether a firm's investors have a higher-than-sample-median social preference. Table VI shows the results corresponding to the two channels, in Panels A and B, respectively.

In Panel A, the coefficient on the interaction between the treatment indicator and the DEI materiality indicator is positive across all columns. While it is not statistically significant without controls, column 5 reveals a significantly positive and economically meaningful effect of 0.639 in the presence of all controls. This result is inconsistent with the financial channel playing a key role in explaining the average decline in firm value after the Stop WOKE Act.

In Panel B, we explore the role of non-financial considerations by interacting the treatment indicator with an indicator for investors with high pro-social preferences. We find that the interaction is negative, economically large and statistically significant at the 1% level. At the same time, the treatment effect is now significantly positive, suggesting important disagreement among investors with and without pro-social preferences. In Panel C, we test the two channels together and find that the coefficient on the interaction between the treatment indicator and the indicator for a high pro-social preference from investors remains negative and statistically significant across all the columns. Meanwhile, the coefficient on the interaction between the treatment indicator and the DEI materiality indicator is small and no longer negative, and statistically insignificant.

The results so far provide more support for the non-financial (investor demand) channel than the financial channel. However, it is possible that more pro-social investors react more strongly to the restriction on DEI initiatives because they have a *subjective* belief that DEI is good for firms' cash flows, regardless of whether DEI is objectively relevant to the firms or not. Nevertheless, if the subjective belief about DEI value was driving investors' reaction to the Stop WOKE Act, such a belief should be stronger if the firm's cash flows are *objectively* related to DEI issues. Following this reasoning, which is similarly argued in Pan et al. (2022), we allow the proxy for investor preferences and the indicator for DEI materiality to interact with each other in affecting the treatment effect.

Table VII shows that the coefficient on the interaction term *Treat x DEI materiality x High investor preference* is highly negative and statistically different from zero. Therefore, the subjective belief

mechanism appears to play a large role. Meanwhile, the coefficient on *Treat x High investor preference* is still negative but no longer so significant, suggesting that the preference channel per se does not matter as much as the belief channel. These findings imply that investors react negatively to the Act mostly because they believe DEI to be important for a firm's cashflows which happen to correlate with their social preference.

Strikingly, in this regression with many interactions, the coefficient on *Treat x DEI materiality* is positive and statistically significant, implying that the market reaction to the Stop WOKE Act is positive for treated firms without a strong presence of pro-social investors. This result highlights the heterogeneity in valuation response to a cut in DEI initiatives, that equity markets value DEI differently for different firms. The contrasting results for various types of firms also rule out alternative explanation related to political risk, because a political risk story would predict a negative market response to the law change, or at best no response, but not a positive response to the perceived increase in political risk.

5.2. Evidence from investors' portfolio rebalancing

Since investors' pro-social preference appears to be the main driver of the negative market reaction to the Stop WOKE Act, we expect investors with a stronger social preference to rebalance more away from stocks affected by the Act. In this section, we provide direct evidence for that hypothesis by examining investors' holdings.

We follow Pan et al. (2022) to design the test and again use the same approach described in the previous section to measure investors' social preference. Like Pan et al., we exclude the largest institutional investors with equity assets under management of more than 250 billion dollars in 2017 constant CPI. Doing so also helps rule out the concern that the aggregate market reaction was driven by the largest investors.

Specifically, we capture each investor' rebalancing activities by the change in the investor's portfolio weight in each stock in constant prices between December 31, 2020 and December 31, 2021 – the two year ends before and after the Stop WOKE Act's announcement:[21]

$$\Delta Stock\ weight_{ij} = Stock\ weight_{ij}^{2021} - Stock\ weight_{ij}^{2020} = \frac{n_{ij}^{2021} p_j^{2020}}{\sum n_{ij}^{2021} p_j^{2020}} - \frac{n_{ij}^{2020} p_j^{2020}}{\sum n_{ij}^{2020} p_j^{2020}}$$

[21] Using year ends follows Pan et al.'s approach. In our setting, doing so also helps avoid any seasonality in heterogeneous investors' quarterly adjustments (for example, due to differential tax rules across different states).

where $n_{ij}^{2021}(n_{ij}^{2020})$ is the number of stock j shares in the portfolio of institutional investor i on December 31, 2021 (2020). We use the end-of-2020 stock prices, p_j^{2020}, to compute the dollar value of portfolio holdings in both 2020 and 2021. Changes in portfolio weights therefore reflect active rebalancing decisions rather than simply changes in stock prices.

We estimate the following model:

$$\Delta Stock\ weight_{ij} = a + b \times Treatment \times Investor\ social\ preference + \gamma_i + \delta_j + \epsilon_{ij}$$

where all variables are defined before, and γ_i capture investor fixed effects and δ_j capture firm fixed effects. The coefficient b will capture the difference in portfolio weight change in treated firms (Florida firms) between investors with stronger and weaker preferences for social issues. For ease of interpretation, we scale the change in stock weight by its mean absolute value in the sample. We double-cluster standard errors by firm headquarter state and investor, as our variable of interest varies in these two dimensions.

By including firm fixed effects, we also control for firm-specific expected change in exposure to political interference or deteriorating business environment due to the Stop WOKE Act. Thus, this specification helps us rule out the alternative interpretation that the negative market reaction was due to investors' concerns over Florida government interfering more with businesses after the Act.

Table VIII Panel A shows the results. Column (1) shows that the interaction between *Treatment indicator* and *Investor social preference* is negative and statistically significant, suggesting that in 2021, investors with a stronger social preference reallocated their portfolio more away from Florida firms. The economic magnitude is large. A one standard deviation higher in social preference is associated with a reduction of the portfolio weight by over 22% relative to the average absolute change in the portfolio weight. Column (2) and (3) shows that the portfolio reallocation based on social preferences is also significant and large for both large and small investors (classified by above or below 1 billion dollars in 2017 constant terms to align with Pan et al.), again ruling out the concern that the aggregate market reaction was due to the preferences of the largest investors. As a placebo, in Panels B and C, we replace the social preference measure with the environmental or governance preference measure and find no similar results.

5.3. Political risk as an alternative interpretation

An alternative view on the negative market reaction to Florida's Stop Woke Act is that investors expect increased political interference from the Florida government, potentially harming firm value. This could

lead to higher compliance and legal costs for Florida businesses, but these costs alone may not justify the 1.8% decline in firm value (about $96 million per firm). Act penalties, capped at $100,000, are modest, and not all firms are likely to violate it. Moreover, if investors' expectations of compliance and legal costs drive the negative market reaction, it should be negative even when a firm's investors do not have a strong pro-social preference. Table 6 Panel C shows the opposite: the coefficient on the Treatment indicator alone is positive. Finally, if investors' expectation of the compliance cost is important, the negative market reaction should be more pronounced for firms that likely face regulatory actions in the absence of DEI initiatives, such as firms with more operations in states with a stronger pro-social attitude. The previous section finds the opposite result. Thus, compliance costs are unlikely to explain the results.

Nonetheless, we seriously consider the political risk explanation by testing whether investors' risk aversion contributes to the reaction. We estimate investors' risk attitudes using a revealed preference approach similar to the way we previously capture investors' social preferences. Specifically, we calculate the negative of the average realized volatility of the stocks each investor holds as our measure of the investor's risk aversion. We use firms' daily stock return volatility in 2020 and investors' holding data on September 30, 2021 – the latest data by the Stop Woke Act's announcement. Assuming risk attitudes are stable, we confirm an autoregressive coefficient of 0.81 for this measure. While this approach does not consider covariances within portfolios, it effectively captures each investor's pre-existing information before making their capital allocation decisions. Unreported tests yield similar results when we infer investor risk aversion using the realized volatility of each investor's portfolio in 2020, which should capture covariances across stocks within the portfolio.

Table IX Panel A presents firm-level results, similar to our baseline. We classify firms into high versus low investor risk aversion by aggregating stockholders' risk aversion based on their holdings. While the coefficient on *Treat x High Investor risk aversion* is negative and significant, suggesting investor risk aversion partially explains the negative market reaction to the Stop Woke Act, the coefficient on *Treat x High Investor social preference* remains negative and significant after controlling for the risk aversion channel. Moreover, in the most saturated regression, the magnitude of the social preference coefficient is larger (1.488% vs. 1.235%), indicating that investor social preference is the more important factor.

Table IX Panel B reports investor-level results, following Table VIII's methodology. The coefficient on *Treatment Indicator x Investor social preference* remains negative and significant, while *Treatment*

Indicator x Investor risk aversion is essentially zero and insignificant. These findings confirm that investor social preference, not risk aversion or aversion to political risk, is the primary driver of the negative market reaction to the Stop Woke Act.

5.4. Other discussions

While the results indicate that investors collectively value DEI initiatives in an average firm, the results do not mean that investors do so for all firms. The Stop WOKE Act provides a novel setting to identify the average treatment effect of restricting corporate DEI practices. Such a setting, however, may not identify the treatment effect across different firms. For example, bigger firms may not be affected by the Act because they are more likely to be geographically diverse, providing a natural hedge to the impact of the Act. If anything, bigger firms may take the Act as a chance to overcome competition, as smaller competitors will be more negatively impacted by the Act and may lose their competitive edge. We test for this hypothesis by evaluating different samples: firms with higher vs. lower than sample median in total assets. Table X shows the results, confirming that the negative reaction to the Stop WOKE Act is stronger among the smaller firms than among the bigger firms. Nonetheless, the market reaction is still negative and economically meaningful for the bigger firms.

Finally, while the Stop WOKE Act affected only one state, the study's implications extend far beyond Florida's borders. First, the observed market reaction represents the collective response of investors nationwide and more, not just those in Florida. Second, Florida's economic significance cannot be overstated; it is the fourth-largest U.S. state by GDP, accounting for over 5% of the nation's GDP, and hosts a diverse array of industries including aviation, hospitality, information technology, finance, energy, retail, and more. This industrial diversity enhances the study's relevance across sectors. Third, the robustness of results when matching Florida firms with similar out-of-state counterparts suggests the findings are not driven by Florida-specific factors. Furthermore, as the first state to enact such legislation, Florida's experience may be indicative of potential market reactions in other states considering similar laws.

6. Conclusion

The evidence presented in this study indicates an overall positive valuation of corporate DEI initiatives, as demonstrated by the significant negative market reaction to Florida's Stop WOKE Act. This legislative

change, which restricted DEI activities in certain firms, resulted in a marked decrease in their stock value, suggesting on average investors value DEI initiatives.

However, further analysis suggests that the average negative market reaction masks important disagreement among investors. Our results suggest that treated firms with more pro-social investors experience significantly negative and economically large returns, while firms with less pro-social investors experience significantly positive, but in magnitude smaller returns. Further evidence from firms for which DEI plays a more material role suggests that the disagreement is even more pronounced for these firms, suggesting the more or less prosocial investors not only have different social preferences but that these differences in preferences might also reflect their divergent beliefs about the value of DEI initiatives.

Our findings highlight the importance of investor disagreement for firms and policymakers. They also offer a potential explanation why different firms respond differently to political pressure to alter or abandon their existing DEI policies.

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Figure I. Abnormal returns around the Stop WOKE Act.

This figure plots the cumulative abnormal returns (CARs, in percentage terms) of firms headquartered in Florida (treated firms) vs. firms headquartered elsewhere in the US (control firms) around the *announcement* of the Stop WOKE Act, which Florida sets to restrict DEI initiatives in the workplace.

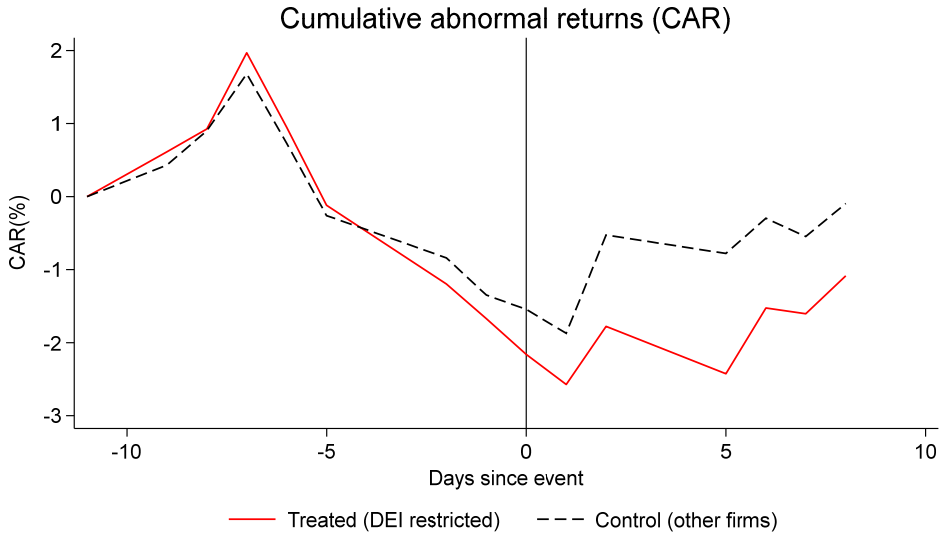


Figure II: DiD graph with a portfolio approach.

This figure plots the difference between the cumulative return of two portfolios, one equally weighting the treated firms' stocks (firms headquartered in Florida) vs. another equally weighting the control firms' stocks (other US firms), around the announcement of the Stop WOKE Act (the event). The graph includes 95%-confidence intervals, computed by robust standard errors, as well. The points and confidence intervals are estimated by regressing the cumulative portfolio return (in percentage terms, benchmarked to zero at the two days before the event) on the interactions between the treatment indicator and time dummies indicating the number of days since the event, while including portfolio fixed effects and time fixed effects. The sample includes 365 days before and after the event day (December 15, 2021). Gaps in the graph correspond to non-trading days. The last point in the graph corresponds to day 6+, so it includes all the trading days more than 6 days beyond the announcement date.

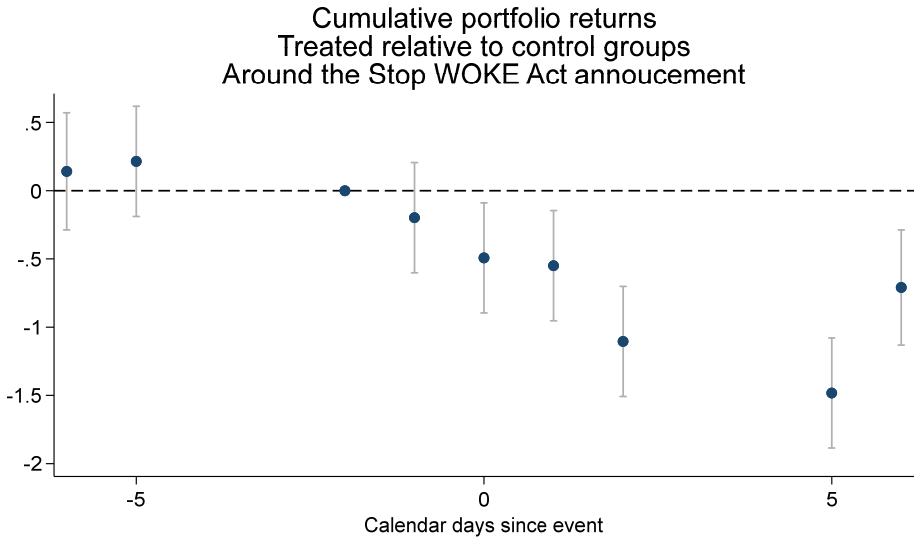


Table I. Summary statistics

This table shows the summary statistics of cumulative abnormal returns (in percentage units) of firms around the event date (the announcement of the Stop WOKE Act on December 15, 2021), along with firm characteristics measured before the event date. Detailed variable descriptions are in the Internet Appendix. Panel A is about the full sample, Panel B is about the sample of Florida-headquartered firms, and Panel C is about the other US firms.

Panel A: Full sample

	N	Mean	SD	Min	Median	Max
CAR[-1,+3] (baseline)	4233	.001	7.513	-51.259	.14	135.525
CAR[-1,+3] (CAPM)	4233	.359	7.681	-49.533	.221	138.345
CAR[-1,+3] (Fama-French 3)	4233	-.279	7.596	-53.435	.027	137.082
CAR[-1,+3] (Fama-French 5)	4233	-.155	7.050	-53.295	0	137.741
Treatment indicator	4232	.048	0.214	0	0	1
Treatment intensity (branches)	2968	.051	0.143	0	0	1
Treatment intensity (employees)	2854	.048	0.157	0	0	1
Book-to-market	4213	.398	0.758	-4.045	.273	28.136
Log (market cap)	4222	14.004	2.085	9.181	13.963	21.649
Past year return	4233	47.792	121.270	-90.229	28.35	2779.944
Assets growth	4032	.219	0.533	-1.52	.088	2.698
Profitability	3865	-.246	1.413	-15.833	.039	.578
Investment	4012	.031	0.056	0	.014	.523
DEI materiality	4048	.416	0.493	0	0	1

Panel B: Florida-headquartered firms

	N	Mean	SD	Min	Median	Max
CAR[-1,+3] (baseline)	204	-1.209	8.115	-44.2	-.436	24.714
CAR[-1,+3] (CAPM)	204	-.701	8.028	-47.222	-.307	27.245
CAR[-1,+3] (Fama-French 3)	204	-1.335	8.221	-53.435	-.225	26.113
CAR[-1,+3] (Fama-French 5)	204	-.615	6.336	-31.186	0	25.137
Book-to-market	202	.443	0.825	-.476	.259	9.119
Log (market cap)	203	13.513	2.117	9.328	13.39	18.959
Past year return	204	48.139	169.519	-79.886	26.41	2217.992
Assets growth	189	.261	0.641	-1.011	.068	2.698
Profitability	182	-.405	1.982	-15.833	.043	.398
Investment	189	.035	0.077	0	.01	.523
DEI materiality	185	.357	0.480	0	0	1

Panel C: Other US firms

	N	Mean	SD	Min	Median	Max
CAR[-1,+3] (baseline)	4029	.063	7.478	-51.259	.173	135.525
CAR[-1,+3] (CAPM)	4029	.413	7.660	-49.533	.254	138.345
CAR[-1,+3] (Fama-French 3)	4029	-.226	7.560	-53.204	.04	137.082
CAR[-1,+3] (Fama-French 5)	4029	-.132	7.084	-53.295	0	137.741
Book-to-market	4011	.395	0.755	-4.045	.274	28.136
Log (market cap)	4019	14.029	2.081	9.181	13.995	21.649
Past year return	4029	47.775	118.334	-90.229	28.477	2779.944
Assets growth	3843	.216	0.527	-1.52	.089	2.698
Profitability	3683	-.238	1.379	-15.833	.039	.578
Investment	3823	.03	0.055	0	.014	.523
DEI materiality	3863	.419	0.493	0	0	1

Table II. Market reaction to the Stop WOKE Act.

Panel A of this table reports the estimates of regressing a firm's cumulative abnormal return (CAR) around the Stop WOKE Act's announcement on a treatment indicator, which equals one for firms headquartered in Florida and zero for other US firms. The CAR is calculated as the cumulative abnormal return relative to the CRSP value-weighted market return, as in Pan et al. (2022), from one trading day before the Act's announcement on December 15, 2021, to three trading days after that announcement. The regressions may include control variables, which include Fama-French 48 industry indicators, book-to-market ratio, log of market capitalization, and stock return over the past one year ending in a month before the event. Detailed variable descriptions are in the Internet Appendix. Panel B reports the estimates of the DiD version of the regressions in Panel A, by stacking 20 additional observations per firm, representing CARs in the 20 pre-event periods with the same length as the event period. This stacked panel allows for the inclusion of firm and time fixed effects, as well as interacting the treatment indicator and other firm characteristics with a Post indicator, equaling one for the event period and zero otherwise. T-statistics (in parentheses) are calculated with standard errors clustered at the state level.

Panel A: Baseline test

	(1)	(2)	(3)	(4)	(5)
Treatment indicator	-1.232***	-1.084***	-1.051***	-0.831***	-0.839***
	(-6.063)	(-10.334)	(-9.668)	(-9.496)	(-9.360)
Book-to-market			0.403***	0.570***	0.584***
			(3.985)	(4.822)	(4.824)
Log (market cap)				0.357***	0.354***
				(8.342)	(8.472)
Past year return					0.001
					(1.180)
_cons	0.063	0.055	-0.107	-5.183***	-5.210***
	(0.308)	(0.486)	(-0.951)	(-8.210)	(-8.263)
Observations	4232	4232	4212	4212	4212
R-squared	0.001	0.055	0.057	0.065	0.066
Industry FE	No	Yes	Yes	Yes	Yes

Panel B: DiD test

	(1)	(2)	(3)	(4)	(5)
Post x Treatment indicator	-1.162***	-1.031***	-0.911***	-0.794***	-0.780***
	(-4.779)	(-8.942)	(-7.474)	(-6.610)	(-6.458)
Post x Book-to-market			0.532***	0.621***	0.596***
			(6.660)	(6.902)	(6.919)
Post x Log (market cap)				0.190***	0.195***
				(4.395)	(4.452)
Post x Past year return					-0.237**
					(-2.400)
_cons	-0.352***	-0.352***	-0.377***	-0.507***	-0.504***
	(-631.297)	(-1332.589)	(-256.484)	(-17.206)	(-16.753)
Observations	88561	88561	87559	87559	87559
R-squared	0.071	0.121	0.125	0.125	0.125
Firm FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Industry*Time FE	No	Yes	Yes	Yes	Yes

t-values are in parentheses
*** p<.01, ** p<.05, * p<.1*

Table III. Market reaction to the Stop WOKE Act – Treatment intensity.

This table reports the estimates of regressing a firm's cumulative abnormal return (CAR) around the Stop WOKE Act's announcement on a treatment intensity measure. In Panel A, the treatment intensity measure is the fraction of a firm's employees that are in Florida in 2020. In Panel B, the treatment intensity measure is the fraction of a firm's branches that are in Florida in 2020. The CAR is calculated as the cumulative abnormal return relative to the CRSP value-weighted market return, as in Pan et al. (2022), from one (trading) day before the Act's announcement on December 15, 2021, to three trading days after that announcement. The regressions may include control variables, which include Fama-French 48 industry indicators, book-to-market ratio, log of market capitalization, and stock return over the past one year ending in a month before the event. Detailed variable descriptions are in the Internet Appendix. T-statistics (in parentheses) are calculated with standard errors clustered at the state level.

Panel A: Treatment intensity = Fraction of employees in Florida

	(1)	(2)	(3)	(4)	(5)
Treatment intensity	-1.466**	-0.708**	-0.719**	-0.516**	-0.522*
	(-2.206)	(-2.493)	(-2.499)	(-2.012)	(-2.008)
Book-to-market			-0.022	0.101	0.085
			(-0.103)	(0.561)	(0.445)
Log (market cap)				0.212***	0.212***
				(3.936)	(3.955)
Past year return					-0.001
					(-0.673)
_cons	0.432	0.392***	0.404**	-2.670***	-2.605***
	(1.544)	(2.891)	(2.104)	(-3.150)	(-2.950)
Observations	2854	2853	2842	2842	2842
R-squared	0.001	0.092	0.093	0.096	0.097
Industry FE	No	Yes	Yes	Yes	Yes

Panel B: Treatment intensity = Fraction of branches in Florida

	(1)	(2)	(3)	(4)	(5)
Treatment intensity	-2.497***	-1.344***	-1.349***	-1.169***	-1.179***
	(-2.999)	(-2.905)	(-2.911)	(-3.442)	(-3.456)
Book-to-market			-0.051	0.070	0.052
			(-0.223)	(0.367)	(0.254)
Log (market cap)				0.207***	0.207***
				(3.913)	(3.957)
Past year return					-0.001
					(-0.923)
_cons	0.592**	0.531***	0.556***	-2.432***	-2.367***
	(2.052)	(4.155)	(3.013)	(-2.953)	(-2.806)
Observations	2968	2967	2955	2955	2955
R-squared	0.003	0.097	0.098	0.101	0.102
Industry FE	No	Yes	Yes	Yes	Yes

t-values are in parentheses
*** p<.01, ** p<.05, * p<.1

Table IV. Potential spillover effects and alternative sets of control groups.

This table reports the estimates of regressing a firm's cumulative abnormal return (CAR) around the Stop WOKE Act's announcement on a treatment indicator, which equals one for firms headquartered in Florida and zero for control firms. The control firms are firms in all other US Republican states in Panel A, firms in all the US Democratic states in Panel B, firms in coastal states near Florida in Panel C, and firms in coastal states far away from Florida in Panel D. The CAR is calculated as the cumulative abnormal return relative to the CRSP value-weighted market return, as in Pan et al. (2022), from one (trading) day before the Act's announcement on December 15, 2021, to three trading days after that announcement. The regressions may include control variables, which include Fama-French 48 industry indicators, book-to-market ratio, log of market capitalization, and stock return over the past one year ending in a month before the event. Detailed variable descriptions are in the Internet Appendix. T-statistics (in parentheses) are calculated with standard errors clustered at the state level.

Panel A: Firms in all other Republican states as control firms

	(1)	(2)	(3)	(4)	(5)
Treatment indicator	-0.738***	-0.822***	-0.788***	-0.575***	-0.575***
	(-4.152)	(-7.735)	(-7.870)	(-3.611)	(-3.664)
Book-to-market			0.241	0.435*	0.442*
			(1.222)	(1.788)	(1.846)
Log (market cap)				0.423***	0.422***
				(4.035)	(4.032)
Past year return					0.000
					(0.321)
_cons	-0.432**	-0.430***	-0.542***	-6.584***	-6.601***
	(-2.429)	(-4.734)	(-3.839)	(-4.206)	(-4.259)
Observations	1281	1279	1269	1269	1269
R-squared	0.002	0.081	0.082	0.099	0.099
Industry FE	No	Yes	Yes	Yes	Yes

Panel B: Firms in all Democratic states as control firms

	(1)	(2)	(3)	(4)	(5)
Treatment indicator	-1.418***	-1.183***	-1.154***	-0.915***	-0.927***
	(-6.497)	(-9.943)	(-9.255)	(-9.008)	(-8.963)
Book-to-market			0.503***	0.685***	0.695***
			(5.501)	(4.863)	(4.769)
Log (market cap)				0.372***	0.368***
				(6.873)	(6.930)
Past year return					0.001
					(1.076)
_cons	0.248	0.229	0.037	-5.246***	-5.256***
	(1.138)	(1.543)	(0.251)	(-6.549)	(-6.604)
Observations	3136	3135	3123	3123	3123
R-squared	0.002	0.054	0.056	0.064	0.065
Industry FE	No	Yes	Yes	Yes	Yes

t-values are in parentheses
*** p<.01, ** p<.05, * p<.1*

Panel C: Firms in nearby states as control firms

	(1)	(2)	(3)	(4)	(5)
Treatment indicator	-0.728*	-0.656***	-0.753***	-0.381	-0.376
	(-2.379)	(-5.391)	(-5.710)	(-1.247)	(-1.209)
Book-to-market			1.297**	1.519***	1.508***
			(3.350)	(5.259)	(5.196)
Log (market cap)				0.500	0.503
				(1.730)	(1.694)
Past year return					-0.001
					(-0.760)
_cons	-0.442	-0.530***	-1.001***	-8.196*	-8.199*
	(-1.442)	(-4.110)	(-3.890)	(-1.997)	(-1.963)
Observations	467	462	459	459	459
R-squared	0.003	0.104	0.119	0.136	0.136
Industry FE	No	Yes	Yes	Yes	Yes

Panel D: Firms in distant states as control firms

	(1)	(2)	(3)	(4)	(5)
Treatment indicator	-1.799***	-1.362***	-1.402***	-0.957**	-0.994**
	(-12.967)	(-17.258)	(-14.279)	(-4.785)	(-4.715)
Book-to-market			0.804**	1.255***	1.309***
			(3.844)	(7.389)	(10.927)
Log (market cap)				0.498**	0.490**
				(4.195)	(4.183)
Past year return					0.003
					(1.533)
_cons	0.629**	0.547***	0.327**	-6.901**	-6.914**
	(4.534)	(9.051)	(4.915)	(-3.909)	(-4.192)
Observations	1028	1027	1023	1023	1023
R-squared	0.007	0.099	0.102	0.116	0.118
Industry FE	No	Yes	Yes	Yes	Yes

t-values are in parentheses
**** p<.01, ** p<.05, * p<.1*

Table V. Market reaction when the Stop WOKE Act was stopped.

This table repeats Table II, but changes the event date to August 18, 2022, when a US District judge declared parts of the Stop WOKE Act related to workplace diversity training unconstitutional. In particular, it reports the estimates of regressing a firm's cumulative abnormal return (CAR) around the event date on a treatment indicator, which equals one for firms headquartered in Florida and zero for other US firms. The CAR is calculated as the cumulative abnormal return relative to the CRSP value-weighted market return, as in Pan et al. (2022), from the one day before the event date, to three trading days after that date. The regressions may include control variables, which include Fama-French 48 industry indicators, book-to-market ratio, log of market capitalization, and stock return over the past one year ending in a month before the event. Detailed variable descriptions are in the Internet Appendix. T-statistics (in parentheses) are calculated with standard errors clustered at the state level.

	(1)	(2)	(3)	(4)	(5)
Treatment indicator	1.119*	1.561***	1.622***	1.371***	1.376***
	(1.712)	(3.589)	(3.944)	(3.336)	(3.324)
Book-to-market			0.847**	0.657**	0.648**
			(2.634)	(2.304)	(2.303)
Log (market cap)				-0.413***	-0.411***
				(-5.329)	(-5.226)
Past year return					-0.001
					(-0.563)
_cons	-0.956	-0.978**	-1.339***	4.540***	4.559***
	(-1.463)	(-2.533)	(-2.975)	(3.367)	(3.413)
Observations	4101	4101	4082	4082	4082
R-squared	0.001	0.114	0.119	0.128	0.128
Industry FE	No	Yes	Yes	Yes	Yes

t-values are in parentheses
*** p<.01, ** p<.05, * p<.1

Table VI. Testing for different channels

This table reports the estimates of regressing a firm's cumulative abnormal return (CAR) around the Stop WOKE Act's announcement on a treatment indicator, which equals one for firms headquartered in Florida and zero for other US firms, as well as its interaction with different indicators, namely DEI materiality (Panel A), High investor (S) preference (Panel B), and both of them together (Panel C). The CAR is calculated as the cumulative abnormal return relative to the CRSP value-weighted market return, as in Pan et al. (2022), from the one day before the Act's announcement on December 15, 2021, to three (trading) days after that announcement. The regressions may include control variables, which include Fama-French 48 industry indicators, book-to-market ratio, log of market capitalization, and stock return over the past one year ending in a month before the event. Detailed variable descriptions are in the Internet Appendix. T-statistics (in parentheses) are calculated with standard errors clustered at the state level.

Panel A: The financial channel

	(1)	(2)	(3)	(4)	(5)
Treatment indicator	-1.367***	-1.351***	-1.391***	-1.274***	-1.274***
	(-8.357)	(-12.516)	(-12.987)	(-11.685)	(-11.626)
Treat x DEI materiality	0.032	0.305	0.531**	0.654***	0.639***
	(0.087)	(1.243)	(2.216)	(2.869)	(2.803)
DEI materiality	0.931**	0.036	0.070	0.055	0.054
	(2.524)	(0.115)	(0.221)	(0.177)	(0.174)
Book-to-market			0.383**	0.616***	0.632***
			(2.055)	(2.759)	(2.725)
Log (market cap)				0.293***	0.292***
				(5.937)	(5.989)
Past year return					0.001
					(0.590)
_cons	-0.242	0.126	-0.041	-4.252***	-4.280***
	(-1.477)	(0.810)	(-0.234)	(-5.375)	(-5.326)
Observations	4048	4048	4029	4029	4029
R-squared	0.006	0.063	0.064	0.070	0.070
Industry FE	No	Yes	Yes	Yes	Yes

t-values are in parentheses
*** p<.01, ** p<.05, * p<.1

Panel B: The non-financial channel

	(1)	(2)	(3)	(4)	(5)
Treatment indicator	-0.173	0.358**	0.345**	0.358***	0.361***
	(-0.536)	(2.651)	(2.514)	(2.795)	(2.814)
Treat x High Investor S preference	-1.337***	-1.771***	-1.654***	-1.455***	-1.471***
	(-3.879)	(-8.169)	(-7.590)	(-6.463)	(-6.736)
High Investor S preference	-0.708**	-0.835***	-0.860***	-0.566*	-0.585*
	(-2.055)	(-3.030)	(-3.145)	(-1.957)	(-1.946)
Book-to-market			0.303***	0.433***	0.448***
			(2.703)	(3.937)	(3.982)
Log (market cap)				0.277***	0.273***
				(6.768)	(6.811)
Past year return					0.001
					(1.112)
_cons	0.528	0.578***	0.463**	-3.621***	-3.625***
	(1.633)	(2.915)	(2.304)	(-5.167)	(-5.186)
Observations	3962	3962	3947	3947	3947
R-squared	0.004	0.063	0.064	0.069	0.070
Industry FE	No	Yes	Yes	Yes	Yes

t-values are in parentheses
*** p<.01, ** p<.05, * p<.1*

Panel C: Both channels together

	(1)	(2)	(3)	(4)	(5)
Treatment indicator	-0.071	0.329**	0.254*	0.243	0.247*
	(-0.310)	(2.273)	(1.720)	(1.636)	(1.689)
Treat x DEI materiality	-0.288	-0.023	0.211	0.272	0.261
	(-1.018)	(-0.103)	(0.944)	(1.245)	(1.208)
DEI materiality	0.911***	-0.142	-0.118	-0.130	-0.132
	(3.217)	(-0.398)	(-0.328)	(-0.360)	(-0.365)
Treat x High Investor S preference	-1.548***	-1.950***	-1.852***	-1.708***	-1.715***
	(-4.446)	(-8.354)	(-7.908)	(-7.121)	(-7.367)
High Investor S preference	-0.471	-0.602**	-0.620**	-0.396	-0.404
	(-1.354)	(-2.103)	(-2.175)	(-1.328)	(-1.309)
Book-to-market			0.234	0.411**	0.425**
			(1.363)	(2.072)	(2.050)
Log (market cap)				0.218***	0.217***
				(4.930)	(4.939)
Past year return					0.001
					(0.508)
_cons	0.126	0.610**	0.512**	-2.732***	-2.747***
	(0.552)	(2.545)	(2.124)	(-3.453)	(-3.467)
Observations	3819	3819	3804	3804	3804
R-squared	0.007	0.069	0.070	0.073	0.073
Industry FE	No	Yes	Yes	Yes	Yes

t-values are in parentheses
*** p<.01, ** p<.05, * p<.1*

Table VII. Testing for channel interactions

This table reports the estimates of regressing a firm's cumulative abnormal return (CAR) around the Stop WOKE Act's announcement on a treatment indicator, which equals one for firms headquartered in Florida, as well as its interaction with DEI materiality, High investor (S) preference, and their interactions. The CAR is calculated as the cumulative abnormal return relative to the CRSP value-weighted market return, as in Pan et al. (2022), from the one trading day before the Act's announcement on December 15, 2021, to three trading days after that announcement. Control variables include Fama-French 48 industry, book-to-market ratio, log of market capitalization, and stock return over the past one year ending in a month before the event. Detailed variable descriptions are in the Internet Appendix. T-statistics (in parentheses) are calculated with standard errors clustered at the state level.

	(1)	(2)	(3)	(4)	(5)
Treatment indicator	-0.786***	-0.523***	-0.541***	-0.506***	-0.507***
	(-4.201)	(-3.694)	(-3.786)	(-3.552)	(-3.534)
Treat x DEI materiality x High investor pref.	-4.447***	-5.176***	-4.874***	-4.611***	-4.639***
	(-15.611)	(-16.128)	(-15.365)	(-13.942)	(-13.645)
Treat x DEI materiality	2.764***	3.479***	3.506***	3.395***	3.401***
	(8.589)	(13.261)	(13.254)	(12.383)	(12.290)
Treat x High Investor S preference	-0.249	-0.410*	-0.425*	-0.369	-0.370
	(-0.956)	(-1.759)	(-1.836)	(-1.567)	(-1.566)
DEI materiality	1.756***	0.662*	0.690*	0.665*	0.667*
	(5.459)	(1.748)	(1.812)	(1.773)	(1.783)
High Investor S preference	0.251	0.074	0.062	0.264	0.258
	(0.961)	(0.323)	(0.272)	(1.106)	(1.060)
DEI materiality x High investor pref.	-1.762***	-1.635***	-1.647***	-1.616***	-1.626***
	(-6.185)	(-5.914)	(-5.959)	(-5.713)	(-5.660)
Book-to-market			0.233	0.403**	0.420**
			(1.377)	(2.065)	(2.051)
Log (market cap)				0.209***	0.207***
				(4.919)	(4.941)
Past year return					0.001
					(0.623)
_cons	-0.242	0.258	0.158	-2.937***	-2.956***
	(-1.291)	(1.226)	(0.738)	(-3.935)	(-3.932)
Observations	3819	3819	3804	3804	3804
R-squared	0.012	0.074	0.075	0.078	0.078
Industry FE	No	Yes	Yes	Yes	Yes

t-values are in parentheses
*** p<.01, ** p<.05, * p<.1

Table VIII. Investors holding analysis

This table reports the relation between the portfolio rebalancing behavior of institutional investors and their social preference, captured by the average social score of the stocks each investor holds. The dependent variable is the change in portfolio weights using constant prices (in 2020), comparing the number of shares held per firm and investor on December 31, 2020, vs. December 31, 2021. The treatment indicator equals one for firms headquartered in Florida, zero elsewhere in the US. Investor social preference is measured using portfolio holdings and the MSCI social scores of different stocks in 2020. The small (big) investors sample is defined as investors whose equity holdings are valued below (above) 1 billion dollars on December 31, 2020 (adjusted to reflect constant dollars as of December 31, 2017 to align with Pan et al. cutoff). Panel A measures social preference with the social score from MSCI, Panels B and C replace that with the MSCI environmental or governance score. T-statistics (in parentheses) are calculated with standard errors double-clustered at the firm headquarter state level and the investor level.

Panel A: Portfolio allocation based on social preferences

	(1) Full Sample	(2) Small investors	(3) Big investors
Treatment Indicator x Investor S preference	-0.221***	-0.272**	-0.105***
	(-2.695)	(-2.322)	(-3.798)
Observations	571885	270407	301250
R-squared	0.292	0.333	0.172
Firm FE	Yes	Yes	Yes
Investor FE	Yes	Yes	Yes

Panel B: Portfolio allocation based on environmental preferences (placebo)

	(1) Full Sample	(2) Small investors	(3) Big investors
Treatment Indicator x Investor E preference	0.038	0.057	0.009
	(1.058)	(0.916)	(0.706)
Observations	571885	270407	301250
R-squared	0.292	0.333	0.172
Firm FE	Yes	Yes	Yes
Investor FE	Yes	Yes	Yes

Panel C: Portfolio allocation based on governance preferences (placebo)

	(1) Full Sample	(2) Small investors	(3) Big investors
Treatment Indicator x Investor G preference	-0.034	-0.097	0.037**
	(-0.553)	(-0.865)	(2.575)
Observations	571885	270407	301250
R-squared	0.292	0.333	0.172
Firm FE	Yes	Yes	Yes
Investor FE	Yes	Yes	Yes

t-values are in parentheses
*** p<.01, ** p<.05, * p<.1

Table IX. Does risk aversion play a role?

This table explores whether risk aversion explains the market reaction and investor portfolio rebalancing around the Stop Woke Act. We measure investor risk aversion as the negative of the average realized volatility of stocks each investor holds, using firms' daily stock return volatility in 2020 and investor holdings as of September 30, 2021, the latest data before the Act's announcement. In Panel A, we test whether abnormal returns around the Stop Woke Act vary based on a firm's high or low investor risk aversion, aggregating stockholders' risk aversion by holdings for each firm. Regressions may include controls: Fama-French 48 industry indicators, book-to-market ratio, log of market cap, and stock returns for the past year ending a month before the event. Detailed variable descriptions are in the Internet Appendix. T-statistics, in parentheses, are based on standard errors clustered at the state level. Panel B uses a similar setup to Table VIII, examining the investor-level change in portfolio weights as the dependent variable, with investor and firm fixed effects and standard errors double-clustered at the firm headquarters state and investor levels.

Panel A: Firm-level regressions

	(1)	(2)	(3)	(4)	(5)
Excess return as dependent variable					
Treatment indicator	0.300	1.142***	1.035***	0.990***	1.012***
	(0.715)	(5.982)	(5.145)	(5.135)	(5.580)
Treat x High Investor S preference	-1.341***	-1.774***	-1.656***	-1.471***	-1.488***
	(-4.024)	(-8.075)	(-7.488)	(-6.332)	(-6.625)
Treat x High Investor risk aversion	-0.909***	-1.494***	-1.321***	-1.198***	-1.235***
	(-3.478)	(-6.106)	(-5.189)	(-4.472)	(-4.818)
High Investor S preference	-0.719**	-0.872***	-0.900***	-0.569*	-0.596*
	(-2.156)	(-3.110)	(-3.232)	(-1.922)	(-1.914)
High Investor risk aversion	0.083	0.396**	0.448**	0.006	0.051
	(0.316)	(2.109)	(2.399)	(0.025)	(0.211)
Book-to-market			0.321***	0.429***	0.444***
			(2.962)	(3.972)	(4.022)
Log (market cap)				0.282***	0.273***
				(4.691)	(4.665)
Past year return					0.001
					(1.148)
_cons	0.489	0.396*	0.249	-3.690***	-3.654***
	(1.167)	(1.775)	(1.117)	(-4.242)	(-4.218)
Observations	3961	3961	3946	3946	3946
R-squared	0.004	0.064	0.065	0.070	0.070
Industry FE	No	Yes	Yes	Yes	Yes

Panel B: Investor-level regressions

	(1)	(2)	(3)
Change in portfolio weight as dependent variable	Full Sample	Small	Big
Treatment Indicator x Investor social preference	-0.216***	-0.261**	-0.118***
	(-2.766)	(-2.362)	(-3.778)
Treatment Indicator x Investor risk aversion	-0.019	-0.045	0.037**
	(-0.471)	(-0.667)	(2.578)
Observations	571885	270407	301250
R-squared	0.292	0.333	0.172
Firm FE	Yes	Yes	Yes
Investor FE	Yes	Yes	Yes

t-values are in parentheses
**** p<.01, ** p<.05, * p<.1*

Table X. Subsample results based on firm size.

This table reports the estimates of regressing a firm's cumulative abnormal return (CAR) around the Stop WOKE Act's announcement on a treatment indicator, which equals one for firms headquartered in Florida and zero for other US firms, for two subsamples – low vs. high total assets (Panels A vs. B). The CAR is calculated as the cumulative abnormal return relative to the CRSP value-weighted market return, as in Pan et al. (2022), from the one (trading) day before the Act's announcement on December 15, 2021, to the three trading days after that announcement. The regressions may include control variables: Fama-French 48 industry indicators, book-to-market ratio, log of market capitalization, and stock return over the past one year ending in a month before the event. Detailed variable descriptions are in the Internet Appendix. T-statistics (in parentheses) are calculated with standard errors clustered at the state level.

Panel A: Sample of smaller firms

	(1)	(2)	(3)	(4)	(5)
Treatment indicator	-1.876***	-1.607***	-1.522***	-1.178***	-1.189***
	(-5.563)	(-8.767)	(-8.297)	(-7.740)	(-7.686)
Book-to-market			0.472***	0.734***	0.742***
			(4.604)	(4.691)	(4.538)
Log (market cap)				0.791***	0.776***
				(6.487)	(6.651)
Past year return					0.001
					(0.651)
_cons	-0.079	-0.094	-0.255	-10.385***	-10.248***
	(-0.234)	(-0.427)	(-1.139)	(-6.562)	(-6.654)
Observations	2107	2106	2091	2091	2091
R-squared	0.002	0.045	0.047	0.062	0.062
Industry FE	No	Yes	Yes	Yes	Yes

Panel B: Sample of bigger firms

	(1)	(2)	(3)	(4)	(5)
Treatment indicator	-0.344**	-0.121	-0.129	-0.155	-0.156
	(-2.317)	(-1.302)	(-1.335)	(-1.451)	(-1.471)
Book-to-market			0.090	-0.009	-0.004
			(0.500)	(-0.049)	(-0.018)
Log (market cap)				-0.114	-0.114
				(-1.443)	(-1.401)
Past year return					0.000
					(0.167)
_cons	0.201	0.192**	0.149	1.949	1.912
	(1.351)	(2.669)	(1.380)	(1.521)	(1.382)
Observations	2125	2125	2120	2120	2120
R-squared	0.000	0.162	0.162	0.163	0.163
Industry FE	No	Yes	Yes	Yes	Yes

t-values are in parentheses
*** p<.01, ** p<.05, * p<.1*

INTERNET APPENDIX

This internet appendix accompanies the main paper by providing variable description and various robustness test results.

Variable definitions

Book-to-market: the ratio of a firm's book value of equity to market capitalization. Book equity is taken as the latest record in Compustat in 2020, the year before the event. Market capitalization is recorded as of November 15, 2021 – one month before the event (the announcement of the Stop WOKE Act). Market capitalization is calculated as the number of shares outstanding multiplied by stock price.

Log (market cap): logarithm of the market capitalization of a firm's stocks recorded as of November 15, 2021 – one month before the event (the announcement of the Stop WOKE Act). Market capitalization is calculated as the number of shares outstanding multiplied by stock price.

Past year return: a firm's buy-and-hold stock return over the one-year window ending on November 15, 2021, i.e., one month before the event (the announcement of the Stop WOKE Act).

Investment: capital expenditure in 2020, scaled by lagged total assets

Profitability: earnings before interest, taxes, depreciation, and amortization (EBITDA) in 2020, scaled by lagged total assets.

Asset growth: log change in a firm's total assets in 2020, relative to 2019.

CAR[-1,+3]: cumulative abnormal return (%) relative to the CRSP value-weighted market return, as in Pan et al. (2022), from the one (trading) day before the Act's announcement on December 15, 2021, to the three trading days after that announcement.

CAR[-i,+j]: cumulative abnormal return (%) relative to the CRSP value-weighted market return, as in Pan et al. (2022), from the *i* days before the Act's announcement on December 15, 2021, to the j days after that announcement.

CAR - CAPM: cumulative abnormal return (%) relative to the CRSP value-weighted market return, as in Pan et al. (2022), from the one trading day before the Act's announcement on December 15, 2021, to the three trading days after that announcement. Abnormal return is the raw daily return minus a benchmark return based on the CAPM model estimated using data over the 252-trading-day window ending in one month before the Act's announcement.

CAR – FF3: cumulative abnormal return (%) relative to the CRSP value-weighted market return, as in Pan et al. (2022), from the one trading day before the Act's announcement on December 15, 2021, to the three trading days after that announcement. Abnormal return is the raw daily return minus a benchmark return based on the Fama-French 3-factor model estimated using data over the 252-trading-day window ending in one month before the Act's announcement.

CAR – FF5: cumulative abnormal return (%) relative to the CRSP value-weighted market return, as in Pan et al. (2022), from the one trading day before the Act's announcement on December 15, 2021, to the three trading days after that announcement. Abnormal return is the raw daily return minus a benchmark return based on the Fama-French 5-factor model estimated using data over the 252-trading-day window ending in one month before the Act's announcement.

Treatment indicator: equals one for firms headquartered in Florida and zero for other US firms.

Treatment intensity: the fraction of a firm's employees that are in Florida in 2020, or the fraction of a firm's branches that are in Florida in 2020, depending on the specific table.

DEI materiality: an indicator equaling one for industries where DEI is considered financially material by SASB.

High investor S preference: an indicator equaling one for firms with an above-sample-median investor preference, where investor preference is the weighted average of investors' average S (social) score, with the weights being the ownership fraction of an investor in a firm in 2021 Q3. An investor's S score is the average MSCI S score of the stocks the investors hold in 2021 Q3.

Location-based measure of investors' social preferences

As a robustness, we calculate the weighted average of the social preferences across the states where the firm's U.S. shareholders are located. For a firm's institutional investors, we find their ownership fraction of firms in 2021 Q3 from the Thomson Reuters 13F database, which captures investor holdings of US equities by institutional investors with assets under management of over $100 million. We then augment this data with investor location data from SEC Form 13F Data Sets, by matching the two databases based on investor names.[22] For a firm's individual investors, we follow Pan et al. to infer their ownership faction by one minus the sum of all the firm's institutional investors, and proxy for the individual investors' location by the state of a firm's headquarters, assuming that individual investors often hold local stocks, commonly known as the local bias in the literature.[23]

We do not use this location-based approach for the main tests for two reasons. First, assuming all investors in a state to have the same social preferences may be an oversimplification. Second, our main tests focus on a law change, which is a treatment at the state level, i.e., affecting firms in only one state: Florida. So, if investors exhibit a local bias, i.e., most investors holding Florida firms are based in Florida, then the location-based measure of investors' preferences for those firms will have little variation.

Nonetheless, the finding that the market reaction to the Stop WOKE Act is stronger when a firm's investors have a stronger social preference remains robust to using this location-based measure, as shown in Table 6.

[22] Fuzzy matching performs well. Manual inspection suggests that matching with a match score above 85 appears quite accurate, covering 70% of all the unique US 13F investors in 2021 Q3. When the match score is below 85, instead of dropping the match, we simply proxy the investor's social preference by the US average. Changing the threshold to 80 or 90, or simply dropping matches under the 85 score does not change the main inferences.

[23] Following Pan et al., we remove foreign investors and investors with over 250 billion worth of assets under management in constructing the measure for investor social preference and rescale ownership fractions to sum to one after removing these investors. When the raw sum is greater than one, we drop them from the data (about 1.9% of the sample).

Table 1: Robustness to additional firm level controls

This table reports the estimates of regressing a firm's cumulative abnormal return (CAR) around the Stop WOKE Act's announcement on a treatment indicator, which equals one for firms headquartered in Florida and zero for other US firms. The CAR is calculated as the cumulative abnormal return relative to the CRSP value-weighted market return, as in Pan et al. (2022), from the one trading day before the Act's announcement on December 15, 2021, to the three trading days after that announcement. The regressions may include control variables, which include Fama-French 48 industry indicators, book-to-market ratio, log of market capitalization, stock return over the past one year ending in a month before the event, as well as other firm characteristics that are known to predict returns, including assets growth, profitability (ROA), and investment. Detailed variable descriptions are in the Internet Appendix. T-statistics (in parentheses) are calculated with standard errors clustered at the state level.

	(1)	(2)	(3)	(4)	(5)
Treatment indicator	-0.839***	-0.557***	-0.510***	-0.383***	-0.393***
	(-4.014)	(-4.800)	(-4.226)	(-3.570)	(-3.492)
Assets growth	1.046**	0.605	0.603	0.493	0.510
	(2.649)	(1.542)	(1.533)	(1.269)	(1.261)
Profitability	0.232**	0.485***	0.492***	0.410***	0.412***
	(2.652)	(6.226)	(6.441)	(5.274)	(5.271)
Investment	-8.382***	-5.074	-4.800	-5.601*	-5.686*
	(-3.104)	(-1.638)	(-1.554)	(-1.842)	(-1.865)
Book-to-market			0.368***	0.507***	0.521***
			(3.455)	(4.217)	(4.142)
Log (market cap)				0.278***	0.276***
				(6.175)	(6.206)
Past year return					0.001
					(0.961)
_cons	0.256	0.301*	0.142	-3.799***	-3.835***
	(1.362)	(1.930)	(0.787)	(-5.239)	(-5.176)
Observations	3849	3849	3830	3830	3830
R-squared	0.008	0.072	0.075	0.080	0.080
Industry FE	No	Yes	Yes	Yes	Yes

t-values are in parentheses
*** p<.01, ** p<.05, * p<.1*

Table 2: Robustness to different factor models

This table reports the estimates of regressing a firm's cumulative abnormal return (CAR) around the Stop WOKE Act's announcement on a treatment indicator, which equals one for firms headquartered in Florida and zero for other US firms. The CAR is calculated as the cumulative abnormal return from the one trading day before the Act's announcement on December 15, 2021, to the three trading days after that announcement. Abnormal returns are based on the CAPM model in Panel A, Fama-French 3-factor model in Panel B, and Fama-French 5-factor model in Panel C. In estimating the return models, we winsorize coefficient estimates at the 1st and 99th percentiles to reduce the effect of outliers, especially for firms with a shorter trading history during the estimation window – where the estimation window is 250 trading days ending in the one month before the event date, following Acemoglu et al. (2016). Everything else is similar to the baseline Table II. Detailed variable descriptions are in the Internet Appendix. T-statistics (in parentheses) are calculated with standard errors clustered at the state level.

Panel A: CAR relative to CAPM model

	(1)	(2)	(3)	(4)	(5)
Treatment indicator	-1.079***	-0.890***	-0.871***	-0.664***	-0.644***
	(-3.628)	(-6.175)	(-5.883)	(-5.128)	(-4.870)
Book-to-market			0.397***	0.554***	0.516***
			(4.324)	(5.477)	(5.018)
Log (market cap)				0.335***	0.344***
				(8.179)	(8.201)
Past year return					-0.004***
					(-3.413)
_cons	0.413	0.404**	0.250	-4.520***	-4.445***
	(1.388)	(2.644)	(1.620)	(-7.319)	(-6.950)
Observations	4232	4232	4212	4212	4212
R-squared	0.001	0.057	0.059	0.066	0.069
Industry FE	No	Yes	Yes	Yes	Yes

t-values are in parentheses
*** p<.01, ** p<.05, * p<.1*

Panel B: CAR relative to Fama-French 3-factor model

	(1)	(2)	(3)	(4)	(5)
Treatment indicator	-1.067***	-0.932***	-0.911***	-0.617***	-0.592***
	(-5.985)	(-8.864)	(-8.397)	(-6.851)	(-6.313)
Book-to-market			0.500***	0.724***	0.677***
			(5.624)	(6.113)	(5.723)
Log (market cap)				0.479***	0.489***
				(10.699)	(10.627)
Past year return					-0.005***
					(-3.848)
_cons	-0.226	-0.232*	-0.430***	-7.236***	-7.143***
	(-1.265)	(-1.892)	(-3.490)	(-10.623)	(-10.073)
Observations	4232	4232	4212	4212	4212
R-squared	0.001	0.039	0.042	0.057	0.062
Industry FE	No	Yes	Yes	Yes	Yes

t-values are in parentheses
*** p<.01, ** p<.05, * p<.1*

Panel C: CAR relative to Fama-French 5-factor model

	(1)	(2)	(3)	(4)	(5)
Treatment indicator	-1.077***	-0.941***	-0.927***	-0.629***	-0.603***
	(-6.588)	(-9.136)	(-8.748)	(-7.059)	(-6.508)
Book-to-market			0.523***	0.751***	0.702***
			(5.532)	(5.817)	(5.447)
Log (market cap)				0.485***	0.496***
				(10.349)	(10.254)
Past year return					-0.005***
					(-4.162)
_cons	-0.215	-0.221*	-0.428***	-7.329***	-7.232***
	(-1.314)	(-1.856)	(-3.535)	(-10.298)	(-9.759)
Observations	4232	4232	4212	4212	4212
R-squared	0.001	0.039	0.042	0.057	0.062
Industry FE	No	Yes	Yes	Yes	Yes

t-values are in parentheses
**** p<.01, ** p<.05, * p<.1*

Table 3: Placebo events

This table reports the estimates of regressing a firm's cumulative abnormal return (CAR) around the passage/release date of two placebo events, Oklahoma's Bill HB 1775 (May 5, 2021) in panel A, and Iowa's House File 802 (February 25, 21), on a treatment indicator, which equals one for firms headquartered in Oklahoma (Panel A) or Iowa (Panel B) and zero for other US firms. The CAR is calculated as the cumulative abnormal return relative to the CRSP value-weighted market return, as in Pan et al. (2022), from the one trading day before the event day to the three trading days after that. The regressions may include control variables, which include Fama-French 48 industry indicators, book-to-market ratio, log of market capitalization, and stock return over the past one year ending in a month before the event. Detailed variable descriptions are in the Internet Appendix. T-statistics (in parentheses) are calculated with standard errors clustered at the state level.

Panel A: Market reaction to Oklahoma's Bill HB 1775

	(1)	(2)	(3)	(4)	(5)
Treatment indicator	4.790***	0.666*	0.563	0.597	0.597
	(6.900)	(1.709)	(1.382)	(1.405)	(1.373)
Book-to-market			0.179	0.411	0.411
			(0.656)	(1.556)	(1.589)
Log (market cap)				0.504***	0.504***
				(3.920)	(3.936)
Past year return					0.000
					(0.018)
_cons	-2.128***	-2.101***	-2.246***	-9.427***	-9.428***
	(-3.065)	(-6.820)	(-6.165)	(-5.978)	(-5.943)
Observations	4097	4097	3972	3972	3972
R-squared	0.002	0.147	0.149	0.160	0.160
Industry FE	No	Yes	Yes	Yes	Yes

t-values are in parentheses
*** p<.01, ** p<.05, * p<.1*

Panel B: Market reaction to Iowa's House File 802

	(1)	(2)	(3)	(4)	(5)
Treatment indicator	1.383***	1.102***	1.147***	1.129***	1.318***
	(11.095)	(3.140)	(2.993)	(2.992)	(3.699)
Book-to-market			-0.073	-0.087	0.058
			(-0.726)	(-0.707)	(0.489)
Log (market cap)				-0.038	-0.061
				(-0.339)	(-0.583)
Past year return					0.014***
					(4.759)
_cons	0.860***	0.861***	0.851***	1.398	0.912
	(6.896)	(8.808)	(8.814)	(0.856)	(0.556)
Observations	4040	4040	3885	3885	3885
R-squared	0.000	0.022	0.023	0.023	0.062
Industry FE	No	Yes	Yes	Yes	Yes

t-values are in parentheses
*** p<.01, ** p<.05, * p<.1*

Table 4: Robustness to a matched control sample

This table repeats Table II but restricts the sample to a matched control sample. Each treated firm is matched with five control firms within the same Fama-French 48 industry using a nearest neighbor matching algorithm based on book-to-market ratio, log of market capitalization, and stock return over the past one year ending in a month before the event. On this matched control sample, we repeat all the specifications in Table II. In particular, the table reports the estimates of regressing a firm's cumulative abnormal return (CAR) around the event date (December 15, 2021) on a treatment indicator, which equals one for firms headquartered in Florida and zero for the matched control firms. The CAR is calculated as the cumulative abnormal return relative to the CRSP value-weighted market return, as in Pan et al. (2022), from the one trading day before the event date, to the three trading days after that date. The regressions may include control variables, which include Fama-French 48 industry indicators, book-to-market ratio, log of market capitalization, and stock return over the past one year ending in a month before the event. Detailed variable descriptions are in the Internet Appendix. T-statistics (in parentheses) are calculated with standard errors clustered at the state level.

	(1)	(2)	(3)	(4)	(5)
Treatment indicator	-1.133***	-1.144***	-1.169***	-1.153***	-1.188***
	(-5.971)	(-5.949)	(-5.958)	(-5.573)	(-5.676)
Book-to-market			0.585*	0.725*	0.772**
			(1.811)	(1.980)	(2.145)
Log (market cap)				0.226	0.216
				(1.299)	(1.270)
Past year return					0.005
					(0.982)
_cons	0.007	0.009	-0.226	-3.353	-3.426
	(0.035)	(0.043)	(-1.117)	(-1.347)	(-1.433)
Observations	1025	1025	1025	1025	1025
R-squared	0.003	0.060	0.061	0.064	0.069
Industry FE	No	Yes	Yes	Yes	Yes

t-values are in parentheses
*** p<.01, ** p<.05, * p<.1*

Table 5: Robustness to different event windows

This table reports the estimates of regressing a firm's cumulative abnormal return (CAR) around the Stop WOKE Act's announcement on a treatment indicator, which equals one for firms headquartered in Florida and zero for other US firms. The CAR is calculated as the cumulative abnormal return relative to the CRSP value-weighted market return, as in Pan et al. (2022). The window to calculate the CAR is from the three trading days before to three trading days after the event for Panel A, from two trading days before to eight trading days after the event for Panel B, from one trading day before to two trading days after for Panel C, and from two trading days before to two days after for panel D. The regressions may include control variables, which include Fama-French 48 industry indicators, book-to-market ratio, log of market capitalization, and stock return over the past one year ending in a month before the event. Panel E shows the breakdown of the market reaction into individual dates within the event window, following the specification in Serafeim and Yoon (2022), which controls for only industry fixed effects (but results are similar with full control set). Detailed variable descriptions are in the Internet Appendix. T-statistics (in parentheses) are calculated with standard errors clustered at the state level.

Panel A: CAR[-3,+3]

	(1)	(2)	(3)	(4)	(5)
Treatment indicator	-1.876***	-1.703***	-1.685***	-1.284***	-1.293***
	(-8.363)	(-10.815)	(-10.564)	(-8.300)	(-8.152)
Book-to-market			0.589**	0.919***	0.903***
			(2.646)	(3.710)	(3.523)
Log (market cap)				0.689***	0.692***
				(8.546)	(8.737)
Past year return					-0.002
					(-1.032)
_cons	-1.497***	-1.505***	-1.745***	-11.551***	-11.512***
	(-6.672)	(-9.071)	(-11.681)	(-9.350)	(-9.194)
Observations	4172	4172	4153	4153	4153
R-squared	0.002	0.047	0.049	0.069	0.069
Industry FE	No	Yes	Yes	Yes	Yes

Panel B: CAR[-2,+8]

	(1)	(2)	(3)	(4)	(5)
Treatment indicator	-1.135***	-1.128***	-0.971***	-0.576***	-0.565***
	(-3.494)	(-5.767)	(-5.093)	(-3.411)	(-3.293)
Book-to-market			0.098	0.423**	0.442**
			(0.592)	(2.345)	(2.358)
Log (market cap)				0.676***	0.673***
				(8.945)	(9.004)
Past year return					0.002
					(0.898)
_cons	0.169	0.168	0.127	-9.502***	-9.549***
	(0.519)	(0.809)	(0.537)	(-7.774)	(-7.710)
Observations	4172	4172	4153	4153	4153
R-squared	0.001	0.035	0.036	0.054	0.054
Industry FE	No	Yes	Yes	Yes	Yes

t-values are in parentheses
*** p<.01, ** p<.05, * p<.1*

Panel C: CAR[-1,+2]

	(1)	(2)	(3)	(4)	(5)
Treatment indicator	-0.899***	-0.768***	-0.772***	-0.582***	-0.568***
	(-5.421)	(-6.263)	(-6.217)	(-4.776)	(-4.672)
Book-to-market			0.421***	0.577***	0.602***
			(4.005)	(5.014)	(5.085)
Log (market cap)				0.326***	0.321***
				(7.770)	(7.898)
Past year return					0.002**
					(2.124)
_cons	0.319*	0.313**	0.145	-4.493***	-4.547***
	(1.923)	(2.547)	(1.138)	(-6.608)	(-6.674)
Observations	4170	4170	4151	4151	4151
R-squared	0.001	0.037	0.040	0.049	0.051
Industry FE	No	Yes	Yes	Yes	Yes

Panel D: CAR[-2,+2]

	(1)	(2)	(3)	(4)	(5)
Treatment indicator	-1.399***	-1.249***	-1.239***	-0.872***	-0.882***
	(-5.509)	(-7.409)	(-7.139)	(-5.806)	(-5.843)
Book-to-market			0.481***	0.783***	0.765***
			(3.395)	(5.467)	(5.163)
Log (market cap)				0.629***	0.632***
				(11.963)	(12.138)
Past year return					-0.002
					(-1.554)
_cons	-0.260	-0.267	-0.458***	-9.418***	-9.376***
	(-1.024)	(-1.575)	(-2.859)	(-11.328)	(-11.186)
Observations	4172	4172	4153	4153	4153
R-squared	0.001	0.044	0.046	0.069	0.070
Industry FE	No	Yes	Yes	Yes	Yes

Panel E: Breakdown by trading day

	(1) t-5, t-3 (weekend)	(2) t-2	(3) t-1	(4) t	(5) t+1	(6) t+2	(7) t+3, t+5 (weekend)
Treatment indicator	-0.080	-0.480***	-0.067	-0.218**	0.104	-0.588***	-0.373***
	(-0.960)	(-4.561)	(-1.101)	(-2.447)	(0.950)	(-4.281)	(-6.754)
_cons	-0.990***	-0.580***	-0.501***	-0.200*	-0.337***	1.351***	-0.249***
	(-13.898)	(-4.826)	(-6.557)	(-1.900)	(-2.893)	(10.667)	(-5.423)
Observations	4172	4172	4170	4169	4169	4168	4167
R-squared	0.058	0.025	0.067	0.052	0.079	0.106	0.037
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

t-values are in parentheses
**** p<.01, ** p<.05, * p<.1*

Table 6: Testing the investor demand channel using a location-based approach.

This table reports the estimates of regressing a firm's cumulative abnormal return (CAR) around the Stop WOKE Act's announcement on a treatment indicator, which equals one for firms headquartered in Florida and zero for other US firms, as well as its interaction with an indicator for High investor preference using a location-based approach. The CAR is calculated as the cumulative abnormal return relative to the CRSP value-weighted market return, as in Pan et al. (2022), from the one trading day before the Act's announcement on December 15, 2021, to the three trading days after that announcement. The regressions may include control variables, which include Fama-French 48 industry indicators, book-to-market ratio, log of market capitalization, and stock return over the past one year ending in a month before the event. Detailed variable descriptions are in the Internet Appendix. T-statistics (in parentheses) are calculated with standard errors clustered at the state level.

	(1)	(2)	(3)	(4)	(5)
Treatment indicator	-1.225***	-1.279***	-1.191***	-0.793***	-0.808***
	(-3.090)	(-4.378)	(-4.084)	(-3.195)	(-3.337)
Treat x High Investor preference	-3.970***	-3.210***	-3.279***	-3.895***	-3.871***
	(-7.457)	(-6.122)	(-6.352)	(-7.993)	(-8.014)
High Investor preference	0.360	-0.285	-0.279	-0.278	-0.279
	(0.677)	(-0.663)	(-0.648)	(-0.724)	(-0.737)
Book-to-market			0.350**	0.653***	0.631***
			(2.059)	(4.455)	(4.090)
Log (market cap)				0.622***	0.625***
				(9.381)	(9.604)
Past year return					-0.002
					(-1.553)
_cons	-0.585	-0.261	-0.410	-9.255***	-9.204***
	(-1.475)	(-0.835)	(-1.265)	(-9.038)	(-8.906)
Observations	3938	3938	3924	3924	3924
R-squared	0.002	0.061	0.062	0.080	0.081
Industry FE	No	Yes	Yes	Yes	Yes

t-values are in parentheses
*** p<.01, ** p<.05, * p<.1*

Figure 1: Returns of treated vs. control firms over a longer event window.

This figure plots the cumulative buy-and-hold returns (in percentage) of firms headquartered in Florida (treated firms) vs. firms headquartered in the nearby coastal states (control firms) from the five days before the announcement of the Stop WOKE Act to the ninety days after the announcement.

