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Abortion Restrictions and Their Impact on Firm Valuation: Evidence from U.S. Equity Markets

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This study examines whether the Dobbs v. Jackson Women's Health Organization ruling of June 2022, which eliminated the federal constitutional right to abortion in the United States, was priced into the equity valuations of publicly listed U.S. firms. Using a difference-in-differences design over 2019 to 2024, with Tobin's Q as the dependent variable, we test two channels through which the ruling may have affected firm value: (1) whether investors revised their expectations about workforce continuity at firms heavily dependent on women of reproductive age, and (2) whether the ruling's salience around gender prompted investors to discount female-led firms. We find support for the first channel: Firms in abortion-ban states with a high share of reproductive-age female workers experienced a relative valuation decline compared to otherwise similar firms. We find no support for the female perception channel. Results are robust across multiple specifications including entropy balancing, propensity score matching, and exclusion of pandemic-era and sector-specific confounds.

Keywords: Abortion Restrictions, Dobbs v. Jackson Women's Health Organization, Difference-in-Differences, Gender Bias, Maternity Risk

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I. INTRODUCTION

I.A. U.S. ABORTION RIGHTS AND THEIR ECONOMIC IMPLICATIONS

For nearly fifty years, abortion was a federally protected right in the United States. For working-age women, abortion is not an uncommon event. The Centers for Disease Control and Prevention recorded approximately 613,000 legally induced abortions in 2022, representing a rate of 11.2 abortions per 1,000 women aged 15 to 44 (Ramer, 2024). It is estimated that 45 percent of all U.S. pregnancies were unintended in 2011, with around half ending in abortion (Finer and Zolna, 2016). It is evident that access to reproductive healthcare is consequential to hundreds of thousands of women every year. A growing field of research shows that this access shapes women's ability to build careers, invest in their education, become entrepreneurs and receive funding, making abortion access central to women's participation in economic life.

The federally protected abortion right was established in Roe v. Wade (1973), perhaps the most well-known court case in American history. It meant that no state could ban the procedure regardless of its political leaning. States could however restrict access at the margins, and many Republican-controlled ones had done so aggressively through so-called TRAP laws: Targeted Regulation of Abortion Providers, imposing strict requirements on abortion clinics, such as having to meet the standards of full surgical centres. This effectively reduced the number of licensed providers sharply, making it more difficult to have an abortion without formally banning the procedure (Forouzan, 2026). On June 24, 2022, that changed. The Supreme Court issued its decision in Dobbs v. Jackson Women's Health Organization, overturning Roe v. Wade and eliminating the federal constitutional protection entirely. States could now decide themselves whether to impose any abortion restriction. Within a year, roughly half of U.S. states had enacted strong restrictions, with 13 enacting outright bans (Baden and Driver, 2023).

Crucially, the ruling did not arrive without warning. On May 2, 2022, the news outlet Politico published a leaked internal draft of the Supreme Court's majority opinion, which revealed that the Court had voted to overturn Roe v. Wade (Gerstein and Ward, 2022). The leak was unprecedented in the modern history of the Court and generated extraordinary public and media attention weeks before the final ruling on June 24.

The ruling itself landed amid acute macroeconomic turbulence. Inflation peaked at 9.1 percent in June 2022, prompting the Federal Reserve to raise rates aggressively from near zero to above 4 percent by year end. This put broad downward pressure on equity valuations and

sent the S&P 500 down approximately 19 percent over the year (Bureau of Labour Statistics, 2022; S&P Dow Jones Indices, 2026; Federal Reserve Bank, 2023). Simultaneously, the energy price surge induced by the Russia-Ukraine war drove the energy sector returns to nearly 60 percent, creating sharp sectoral divergence across the U.S. economy (S&P Dow Jones Indices, 2026). The preceding years were also far from stable, with COVID-19 causing unprecedented economic contraction in 2020 and uneven sectoral recovery through 2021.

I.B. WHAT EARLIER RESEARCH HAS LEFT UNANSWERED

The ruling created an unexpected shock to the female workforce in the U.S., altering the conditions under which they live, work and make career decisions. It therefore offers a rare opportunity to study how investors price gender-related workforce constraints in equity markets. Firms that are heavily reliant on female labour and situated in a state that enacted an outright ban face a different labour market outlook than they did before: greater workforce uncertainty, higher expected retention costs and potentially lower continuity. Beyond the direct workforce channel, the ruling also represents a highly salient national event centred on gender, which may shape how investors perceive female leadership. Whether investors price either of these shifts into firm valuations is the central question of this thesis.

Our study sits at the intersection of three strands of literature, none of which has tested whether reproductive healthcare policy shocks are priced in equity markets. First, studies on female board quotas and voluntary board gender diversity show that gender-related legislative constraints on leadership, such as a mandatory 40 % female board quota, can be capitalised into firm value (Ahern and Dittmar, 2012; Adams and Ferreira, 2009; Matsa and Miller, 2013). However, these constitute policy changes directed at firms, rather than external shocks. Second, Baker and Wurgler (2006), Pástor and Veronesi (2012), and Smith, Chown and Gaughan (2021) establish that politically salient shocks and gender-related news are priced by sentiment-driven investors, but do not test this in a reproductive health context. Third, Core (2024) and Zandberg (2021) demonstrate that the risk of unintended pregnancy and fertility control have real and meaningful implications for entrepreneurial endeavours, linking them to access to capital, but neither examine the valuations of publicly listed firms nor study the shock following the Dobbs ruling specifically. What remains untested is whether these three pieces connect into an identifiable valuation effect, that is, whether a large, exogenous shift in reproductive health policy is reflected in the equity valuations of firms heavily reliant on female labour or female leadership. We contribute by testing exactly that.

More specifically, we hypothesise that (1) firms with high Female Labour-Share (FLS) situated in ban-states experienced a relative decline in valuation compared to low FLS firms in ban-states following the Dobbs ruling and (2) that firms with female CEOs situated in ban-states experience a relative decline in valuation after the Dobbs ruling compared to similar male-led firms. Since female CEOs in large, listed companies are typically well above childbearing age, the CEO channel is a pure test of investor perception of female leadership in times of salient gender-related news. The FLS channel, by isolating firms dependent on women specifically within the childbearing age (15-44), tests whether abortion restrictions translate into a resource constraint that investors price into firm value. Together, they allow us to test whether and how the risk of unintended pregnancy is priced into the valuations of publicly listed firms.

To test whether each of these channels impact firm valuation, we use data on publicly listed U.S. firms covered in Compustat Capital IQ North America over 2019Q1 to 2024Q4, measuring firm value through Tobin's Q. CEO-level data come from ExecuComp, and Female Labour Share is measured at the industry level using IPUMS Current Population Survey (CPS) microdata. We employ a difference-in-differences design and compute triple interactions as alternative specifications, comparing ban-state and non-ban-state firms before and after the ruling, described in detail in section III. We find support for the hypothesis that high FLS firms in ban states suffered a relative decline in Tobin's Q compared to low FLS firms following the ruling, with significant DiD and triple difference coefficients. The differential is driven specifically by reproductive-age workers. We find no support for the CEO channel; female-led firms show no differential valuation change relative to male-led firms in ban states, confirmed by small and insignificant results. However, understanding the financial consequences of the Dobbs ruling requires disentangling any abortion-policy effect from a particularly noisy macroeconomic environment. Given this, we subject both analyses to a battery of robustness tests - excluding the pandemic years of 2020 and 2021, including state times quarter fixed effects to account for time-varying state-specific trends, and removing financial, energy and utility firms whose valuations were most distorted by the 2022 interest rate and energy price shocks. Propensity score matching and entropy balancing are also applied to ensure treatment and control firms are comparable before the ruling. Finally, the 15-44 age restriction is removed to test whether the effect is specific to the childbearing age. Results are presented in more detail in section IV and discussed in section V.

II. LITERATURE AND THEORY

Firms differ in how much they depend on female labour, both at the executive level and in the broader workforce. Both dimensions have consequences for how investors value firms. A growing body of work focuses on workforce gender composition and shows that industries differ systematically in their dependence on female labour. This creates differences in labour continuity, exposure to workforce disruptions, and retention dynamics. A larger body of literature establishes that female and male CEOs differ in their strategic decisions regarding risk-taking, capital allocation and talent management, and attract different investor reactions in times of heightened attention. These differences can translate into valuation effects through sentiment, stereotyping and gender-related biases. What neither strand has examined is whether an exogenous policy shock – that heightens the attention around female leadership and threatens the labour supply of female-intensive industries – is priced into firm valuation. We aim to address this gap through two channels: The potential resource constraints put on female-intensive workforces and the perception of female leadership.

II.A. FEMALE LABOUR SHARE AND THE RESOURCE CHANNEL

Fundamental Differences Between High and Low FLS Firms

While the literature does not directly compare high and low female-share industries as such, it consistently documents that industry membership and dynamics are primary determinants of financial characteristics, including leverage, capital intensity and cash-flow risk (MacKay and Phillips, 2005). Fama and French (1997) further establish that the cost of equity varies substantially across industries, meaning valuation is not industry-neutral and that consequences of abortion-policy shocks should not be expected to be uniform across sectors. To the extent that observed financial differences between the high and low FLS groups reflect industry structure rather than workforce composition, our empirical design in section III accounts for them. This section however focuses on what the literature does show and on our theoretical channel of interest: gender and geographical distribution, labour continuity, worker retention and flexibility.

Industry data from IPUMS illustrates the highly varying gender distribution across industries, with Manufacturing, Mining, Oil & Gas and Construction exceeding 60 percent male, while Healthcare & Social Services and Education are female dominated. Apart from financial characteristics, high and low FLS industries differ in geographical distribution across

states. Ellison and Glaeser (1997) show that manufacturing industries are highly concentrated geographically, driven by agglomeration economies, natural advantages, and localized spillovers. High Female Labour-Share industries such as Healthcare & Social Services and Education follow the opposite pattern: because they are non-tradable and must be delivered where people live, they distribute broadly with population rather than concentrating in locations with advantageous resources (Hlatshwayo and Spence, 2014). This means that high FLS firms are, by the nature of their industries, more evenly represented across all U.S. states, including those that enacted abortion bans following Dobbs, while low FLS industries such as manufacturing and mining are more geographically clustered.

These industry differences have implications on labour continuity. Goldin (2014) shows that in occupations such as business, finance and law, firms disproportionately reward long, continuous and often inflexible work patterns with strongly variable hours. In other occupations, such as technology, science and health, where employees can more easily substitute for one another and earnings are more linear with hours worked, flexibility is less costly, and gender gaps tend to be smaller. While Goldin's (2014) pattern is job-specific rather than industry-related, we apply the overarching logic to allow for heterogeneity within FLS industries: Sectors organized around in-person service delivery, coordination and rigid scheduling - such as Hospitals and Nursing/Residential Care - are more vulnerable when female employees face constraints impacting their availability. In other FLS industries where tasks are more substitutable and hours easier to reallocate, such as segments of Retail Trade and Insurance, the cost of such disruptions may be smaller. This heterogeneity raises a flag: the valuation impact of abortion restrictions may not be uniform across high FLS firms, but concentrated in those most dependent on continuous, non-substitutable female labour.

The Maternity Risk Channel

The structural differences documented in the preceding section create a specific vulnerability to reproductive health policy shocks. The channel through which this vulnerability operates, and one which is overlooked in literature, is the risk of unintended pregnancy, or "maternity risk": the economic consequences of unintended pregnancy for women's labour market participation and career continuity.

Building on Goldin's (2014) insight that flexibility varies between occupations and that career interruptions are more or less costly for the employer, her findings have direct consequences on family formation decisions. Blau and Kahn (2017) show that women's work

interruptions and shorter hours remain primary drivers of the gender wage gap while accounting for education and experience, with family formation being a central factor affecting working hours and career continuity (Blau and Kahn, 2017; Bertrand, Goldin and Katz, 2010). Bertrand, Goldin and Katz (2010) draw similar evidence from MBA graduates, and while this segment does not equate to the broader female workforce, it establishes a pattern that is nonetheless applicable: constraints related to pregnancy, family and childcare can be economically meaningful for women's labour market participation and career trajectories. These findings have multiple implications. Firstly, it is plausible that investors alter their expectations about female employees' availability and continuity based on assumptions about family formation and caregiving rather than observed competence in hiring decisions. Secondly, if abortion restrictions increase the likelihood of unintended pregnancy among these reproductive-age women, high FLS firms may be penalised not only through immediate labour-supply effects, but also through expected difficulties in retaining and promoting experienced female talent in senior roles.

The closest available empirical evidence that such restrictions produce measurable economic disruptions comes from Zandberg (2021) and Core (2024). Zandberg (2021) uses U.S. census data on women aged 20-40 to study how abortion access affects women's probability of becoming entrepreneurs, the age of which they enter and the subsequent success of the firm. Because it was conducted during the Roe v. Wade era, Zandberg uses the between-state variation in adoption of laws that restricted abortion providers, the TRAP laws, and an index tracking the state-level abortion restrictiveness. The results show that access to reproductive health improves women's entrepreneurial career choices, participation and outcomes. On the contrary, it implies that restrictions regarding fertility control can change participation and women's persistence in top roles. Core (2024) adds to Zandberg's findings. In his study on gender gaps in early-stage innovative entrepreneurship in Italy, it is shown that lower risk of unintended pregnancy is linked to different access to financing and altering performance for innovative firms owned by women. Specifically, it is found that when the risk of unintended pregnancy is mitigated, with access to contraception and abortion rights, female founders increase their investments and equity holdings. The probability of becoming an executive in the venture also rises. These factors in turn increase the attractiveness of the startup to venture capitalists. This highlights that the risk of unintended pregnancy is economically meaningful and can have effects on financing and firm outlook. Hence, it is not simply a private matter (Core, 2024). Although these findings do not prove that investors price the maternity risk explicitly into firm value, they do point toward having an impact on career choices and

outcomes for firms with a high female share or with women in top positions. It is therefore reasonable to investigate whether equity investors after the Dobbs ruling change their risk expectations related to career disruptions and costs associated with childcare when it comes to firms reliant on a largely female workforce.

The next part of the literature review turns to the CEO channel, focusing on how salient and pessimistic gender-related news affect investors' perception of female leadership.

II.B. CEO GENDER AND INVESTOR PERCEPTION

How Female and Male CEOs differ

A recurring finding in the corporate finance literature is that female and male CEOs make different strategic choices, attract different investor reactions and shape their organizations differently. The most consistent finding is that female CEOs are associated with lower financial risk. Faccio, Marchica and Mura (2016) show in a matched-sample analysis that female-led firms have, on average, lower leverage, generate more stable earnings and have a higher chance of survival than otherwise similar firms run by male CEOs. They further state that a transition from a male to a female CEO is often associated with a significant decline in corporate risk-taking. Huang and Kisgen (2013) explain the same pattern but instead of focusing on female cautiousness, they turn to male attributes, arguing that male executives are overconfident in major corporate finance decisions. In their studies, male executives pursue more acquisitions and issue more debt, acting more aggressively from the start. Markets react less favourably to these actions (about 2 % lower stock market announcement returns) than equivalent debt and acquisition decisions made by female executives.

A further layer is added by Flabbi, Macis, Moro and Schivardi (2019) who show that female CEOs are better at identifying productivity signals of female employees, meaning they are better at putting the right women in the right roles and paying them more in line with their actual performance. Interestingly, they find that the impact of female leadership on firm performance increases with the share of female workers - estimating that a female CEO would raise sales per employee by about 3.7% in a firm with an average female workforce share of around 20%. Female-led firms are therefore not only financially distinct from male-led ones but also differ across other dimensions. These structural differences provide a foundation for understanding why investors may take gender into account when valuing a firm. Whether and how they do so is the subject of the following section.

How Investors Price Gender at the Leadership Level

One concrete mechanism through which gender can have a valuation impact is stereotyping: Because leadership is still widely coded as agentic, assertive, and masculine, women may be perceived as a poorer fit for top executive roles even when their actual qualifications are equally strong. The concept of stereotyping is thoroughly documented in studies around the appointments of CEOs. While CEO appointments are not the focus of this thesis, they are highly relevant to our setting since they show how events that attract high attention and are high-salience can activate stereotyping among investors - a mechanism directly applicable to a ruling as prominent as Dobbs v. Jackson.

Early event-study literature interpreting negative market reactions to female CEO appointments reflect precisely this stereotyping, arguing that investors may question women's suitability for leadership because stereotypically female traits are seen as inconsistent with what a CEO is supposed to be (Smith, Chown, and Gaughan, 2021). Market pricing can therefore reflect not only beliefs about firm fundamentals, but also socially embedded expectations about who looks like a credible leader. The picture is however more nuanced: Smith, Chown, and Gaughan (2021) show that the degree to which stereotyping happens depends on the visibility of the CEO appointment. Using the complete list of CEO appointments in U.S. publicly traded firms from 2000 to 2016, they show that female CEO appointments are not met with more negative reactions than male appointments on average. In fact, nearly half of female CEO appointments coincide with positive market reactions, a share that is very similar to that of male appointments. Their key contribution is instead to show that public attention moderates the market response. Female CEO announcements attract substantially more media attention than male ones, and when attention is high, female CEOs receive more negative market reactions than males and male CEOs receive more positive ones than females. When attention is low, however, female CEO appointments receive significantly positive responses. This suggests that market pricing reflects how strongly gender stereotypes are activated by attention and salience. Crucially, the authors also argue that even investors who hold no personal bias may still trade as if female leadership will be penalized, if they expect the broader market to react negatively. Put differently, female-led firms may be discounted not only because some investors stereotype women, but also because many investors anticipate that others will stereotype them and trade accordingly.

The concept of gender-role stereotyping is also seen at the board level. Ranjeeni and Naidu (2024) examine whether investors undervalue firms with female directors. Their

evidence shows that firms with female directors may in fact possess characteristics that should improve intrinsic value and reduce information asymmetry, such as stronger monitoring, better disclosure, and a more informative information environment. Yet markets fail to fully incorporate these benefits in prices, leading female directors to be associated with equity undervaluation. They interpret this as evidence that investors' gender-role stereotypes distort pricing. Crucially, this undervaluation is concentrated in male-dominated industries, which suggests that bias intensifies where female leadership appears less "typical" relative to industry norms. This is particularly relevant to our setting: reviewing ExecuComp data covering the public, large-cap S&P 1500 companies, under 10 % of the firms are female-led, and these firms have the highest representation in manufacturing sectors, which are male-dominated. These are precisely the conditions under which Ranjeeni and Naidu show that bias is most likely to distort pricing. However, the negative association between female leaders and equity valuation becomes less pronounced as societal awareness around the benefits of female directors increases (Ranjeeni and Naidu, 2024), suggesting that country-level norms are influential factors. When women in leadership are more visible, more normalized, and better understood, the scope for stereotype-driven mispricing appears to shrink. In other words, biased pricing is not fixed; it depends on the informational and social environment in which investors interpret gender.

Beyond outright stereotyping, statistical discrimination and homophily create funding and valuation gaps independent of prejudice (Guzman and Kacperczyk, 2019). Studying female-led ventures' access to venture capital, they show that a meaningful funding gap persists even after accounting for differences in initial growth orientation, and they link this residual explicitly to investors' preferences and beliefs about gender. One mechanism is statistical discrimination: when objective signals of quality (prestigious background, strong metrics, etc.) are noisy or incomplete, investors use gender as a shortcut for assessing potential, but this gap narrows when female founders carry especially strong quality signals or when investors are more sophisticated. A second, distinct mechanism is homophily: predominantly male investors may additionally favour founders who resemble themselves, independently of any stereotype about women's fitness for leadership. If female-led firms face greater difficulty obtaining outside finance, as Guzman and Kacperczyk document, investors may interpret that pattern as a signal of weakness even when it partly reflects biased gatekeeping. Although much of this evidence comes from venture capital and board-level settings, the mechanisms translate readily to public equity markets, particularly after a policy shock that simultaneously draws fresh attention to female-led firms. Across all these channels, the recurring theme is that female-led

firms tend to be interpreted through a gendered lens, especially in moments of heightened attention and political salience. The female CEO sample we test may therefore be influenced by sentiment rather than information alone, making it a direct test of investor perception.

How Investors React to Policy Shocks

The gender biases documented in the preceding section were shown to be activated in times of high salience around female leadership. In the setting of the Dobbs ruling, which fuelled extreme media attention, it therefore becomes important to understand how policy shocks trigger investor sentiment.

Scholars' definitions of investor sentiment vary, but in general it involves the tendency among investors to value stocks based on irrational beliefs and non-fundamental conclusions, influenced by mood, and not the stock information at hand. Firms whose valuations are more subjective, such as start-ups and extreme growth stocks, are more exposed to investors valuing them based on sentiment. Fundamental valuation is more uncertain for these kinds of firms and therefore sentiment has a larger pricing effect. More generally, firms that are difficult to arbitrage are more at risk of sentiment-driven valuations (Baker and Wurgler, 2006).

While this thesis does not directly measure investor sentiment (media coverage, search volume etc.), the underlying mechanism of how investors react to news and policy shocks is highly relevant. Pástor and Veronesi (2012) present an asset-pricing model in which government policy affects firms' cash flows. Their model predicts that changes in government policy (and uncertainty about changes) can impact stock prices, with higher uncertainty associated with larger (negative) stock price shifts. In a similar study on spikes around political events, the perceived uncertainty of a firm among investors might have shifted following a policy shock or political event, even before changes in firm fundamentals can be observed (Baker, Bloom and Davis, 2016). It also highlights how investor sentiment impacts valuations through channels such as attention and beliefs, for example revised risk outlooks.

How news is framed in terms of content and tone is also an important factor and can have substantially different effects on markets and valuations (Tetlock, 2007). Tetlock's study specifically highlights how high media pessimism is associated with negative investor sentiment, which can create short-term downward pressure on stock prices without commensurate changes in fundamentals. As sentiment fades and arbitrage gradually corrects mispricing, stock prices gradually mean-revert. Sentiment and market reactions relating specifically to gender may further be amplified by high public attention (Tetlock, 2007). The

Dobbs ruling and preceding May leak generated precisely this kind of attention, stirring up public debate and intense national media coverage. Given the ruling's direct connection to gender, this prolonged and highly salient media cycle plausibly amplified investor sentiment.

II.C. FROM REVISED INVESTOR BELIEFS TO FIRM VALUATION

The preceding sections have established that CEO gender and workforce gender distribution influence investors' expectations and beliefs through two distinct channels - the perception of female leadership and the constraints on labour supply that high female-share workforces can face. Importantly, reproductive health policy shocks can alter both. Ahern and Dittmar (2012), while studying board quotas rather than female labour share, provide direct evidence that such constraints are priced into firm value: Looking at the Norwegian board quota law, passed in 2003 and requiring that 40 % of corporate board members must be women by 2008, they find a significant drop in stock prices at the announcement of the law and a subsequent decline in Tobin's Q. Their interpretation is that a rapid, externally imposed change forced firms to reorganise boards and replace directors with younger and less experienced members. In other words, the disruption of having to rapidly find qualified female directors from a pool not large enough to meet the sudden demand was priced by investors. For our thesis, this is useful because it shows more generally that when firms depend on a specific segment of labour or leadership talent, for example scarce and highly skilled female workers, constraints affecting that segment can be capitalised into market value. It also highlights the chain through which the valuation effect moves, running from a sudden policy shock to revised investor beliefs, to impacted firm valuation. What remains to be established is the second part of this chain; the precise pathway through which revised investor beliefs translate into lower firm valuation as measured by Tobin's Q, defined formally in section III. This section builds that bridge by connecting investor sentiment to capital access, which subsequently influences investment decisions and valuation.

Capital access here refers to three related but distinct financing conditions. The first is the cost of equity: when investors perceive a firm as riskier, they demand a higher expected return to hold its shares, raising the cost of new equity issuance. Baker and Wurgler (2006) document that sentiment-driven shifts in investor risk appetite are directly reflected in the cost of equity across the cross-section of stocks. The second is the cost of debt: lenders and credit markets price perceived default risk and cash-flow volatility into the terms on which firms can borrow, including interest spreads and covenant restrictions. Faulkender and Petersen (2006)

show that firms' access to public debt markets, and the spreads at which they borrow, are sensitive to investors' assessments of firm quality and information asymmetry, conditions that a policy shock affecting perceived talent risk could plausibly worsen. The third channel is investor withdrawal: institutional investors may reduce their holdings in affected firms, shrinking the investor base and reducing the liquidity of the firm's equity. This tightens external financing conditions even without any change in the firm's own reported fundamentals. Guzman and Kacperczyk (2019), reviewed in section II.B, show that female-led firms already face more restricted access to external financing relative to observably comparable male-led firms. If a policy shock further intensifies investor caution around female leadership or female labour dependence, it can tighten all three of these financing channels simultaneously for affected firms. Core (2024) and Zandberg (2021) reinforce this point by demonstrating that reproductive health access has real and measurable effects on the capital available to women-led ventures and on women's persistence in entrepreneurial careers, confirming that the maternity risk channel is economically meaningful rather than merely perceptual.

The second step runs from capital access to Tobin's Q. Since Tobin's Q equals market value divided by asset replacement cost, it compresses whenever expected future cash flows fall or the discount rate applied to those flows rises. A tightening of external financing conditions does both simultaneously: investors demand a higher return to hold equity in firms perceived as riskier, raising the discount rate, while constrained firms cannot fund all positive net present value projects, reducing expected cash flows. Fama (1970) shows that this suppression appears in market valuations well before it shows up in accounting performance, because investors price expected future financing conditions rather than current reported earnings. Tobin's Q is therefore the appropriate dependent variable for our study. It captures forward-looking investor assessments and is sensitive to shifts in perceived financing conditions, even before any change in reported fundamentals.

II.D. CONTRIBUTION AND HYPOTHESES

This study does not build on a benchmark paper. Instead it builds on multiple strands of literature and tries to address the four main gaps that were revealed in the preceding sections. Firstly, Baker and Wurgler (2006), Pástor and Veronesi (2012), and Smith, Chown and Gaughan (2021) establish that politically salient shocks and gender-related news are priced by sentiment-driven investors. Our study extends this by testing it in a reproductive health context. The second gap is methodological: while the distinction between high and low Female Labour

Share industries is implicit in the broader labour economics literature (see MacKay and Phillips, 2005), it is rarely explicitly framed as two distinct groups for the basis of comparison in corporate finance. By constructing a measure of Female Labour Share, dividing the U.S. workforce into two distinct groups that differ meaningfully in gender, this thesis makes that contribution and tests whether it carries valuation consequences. It also allows us to see whether the two groups differ across dimensions previously not observed. The third concerns whether maternity risk is priced into equity markets at all. While Core (2024) and Zandberg (2021) demonstrate that reproductive health access shapes financing outcomes and career persistence for women, neither study examines whether these dynamics are reflected in the market valuations of publicly listed firms. Fourthly, we build on Ahern and Dittmar's (2012), Matsa and Miller's (2013) and Adams and Ferreira's (2009) board studies. While these authors examine the valuation outcomes of board quotas and voluntary board gender diversity, those policies are directly aimed at the boards of firms and do not constitute exogenous policy shocks. Taken together, the full chain from a reproductive health policy shock to investor sentiment to firm valuation has not been studied as a coherent empirical test. Research exists on each individual link - investor sentiment responses to political shocks, gender-based financing constraints and the transmission of capital market frictions to Tobin's Q - but no study has, to our knowledge, connected them. A further contribution of this thesis is the use of the Dobbs v. Jackson ruling as an empirical setting. As the most significant legal shift affecting women's labour market position in recent American history, it offers a high-salience, plausibly exogenous shock that generates geographic variation across states and falls unevenly on firms by both industry labour composition and CEO gender. Despite its scale and the breadth of its economic implications, its consequences for the equity valuations of publicly listed firms differentially exposed to female labour or leadership have not been systematically examined.

These gaps motivate the two following hypotheses:

H1: *Firms operating in High Female Labour-Share industries and situated in ban-states experienced a relative decline in valuation compared to firms in low Female Labour-Share industries following the Dobbs ruling.*

H2: *Firms with a female CEO situated in states that enacted an abortion ban experienced a relative decline in valuation compared to firms with a male CEO following the Dobbs ruling.*

III. RESEARCH DESIGN AND METHODOLOGY

III.A. IDENTIFICATION STRATEGY

The central empirical challenge of this thesis is to isolate the effect of abortion restrictions on firm valuation from other factors that influence it. Our identification strategy rests on the Dobbs v. Jackson Women's Health Organization ruling of June 2022, which we treat as a quasi-natural experiment generating plausibly exogenous variation in abortion-policy restrictiveness across U.S. states. Firms headquartered in states that enacted outright bans constitute the treatment group – divided into one subsample for each test: female-led firms for the CEO test, and high Female Labour Share firms for the workforce-channel. Firms in states that mildly restricted, maintained or expanded access serve as the comparison group, divided into male-led and low Female Labour Share respectively. The treatment group consists of 14 states, whereas the control group consists of 36 states. By the time of this thesis, 13 states have outright bans. Missouri is an edge case, enacting a total ban that was later removed in December 2024, meaning it had a ban throughout our sample period. Hence, we label it as treated.

While a classic event study framework examining abnormal stock returns around the ruling in June 2022 would capture immediate investor reactions, we argue that short-window market responses alone are insufficient for our purposes. Labour-market shocks can have effects that unfold over years because adjustment is slow (Autor, Dorn and Hanson, 2013), implying that court rulings with labour market consequences may take months or years to transmit into firm fundamentals. In the context of Dobbs ruling, even if a ban immediately changes the expected labour supply environment, firms' actual workforce composition, retention rates, and recruitment costs will only shift gradually over subsequent quarters and years as contracts expire, hiring decisions accumulate, and employees make longer-run location, family or career choices. Firm-level financial data is also reported quarterly, further limiting what can be observed in short-windowed studies. We therefore adopt a difference-in-differences (DiD) design over a longer horizon, spanning 2019Q1 to 2024Q4, following the framework outlined in Imbens and Wooldridge (2009). The window of three years before and after the ruling allows us to capture both the immediate repricing and any persistent long-run effects on valuation. The ruling was issued on June 24, 2022, seven days before the start of Q3, and with the assumption that virtually all economic and legal consequences materialized from July 2022 onwards, we treat 2022Q3 as the start of the post-period. The other option would be to treat Q2 as the start of the post-period, since technically both the ruling and May Politico

leak of the draft opinion happened here. However, we deem this suboptimal: The leak created uncertainty, and any market response during that quarter would reflect anticipation rather than a realized policy shock, making it a noisy and theoretically ambiguous treatment date. Setting 2022Q3 as the start of the post period reduces the risk of capturing such anticipatory responses.

The DiD logic is straightforward. We compare the change in firm value between treated firms and control firms before and after the ruling. A key assumption of the difference-in-differences design is the parallel trends assumption (Imbens and Wooldridge, 2009): in the absence of Dobbs ruling, treated firms in ban-states would have followed the same valuation trajectory as control firms in non-ban-states. Under this assumption, any differential post-ruling change in valuation can be attributed to the policy shock. We assess this assumption empirically through a Wald F-test and event study plots that trace the quarterly difference in firm value between ban and non-ban-states relative to 2022Q2 across both channels and present the results in section IV.B. We conduct two separate main analyses, each involving a group specific DiD. The first tests the CEO channel, examining whether female-led firms experienced differential valuation changes relative to male-led firms. The second tests the FLS channel, examining whether firms in industries with a high share of female workers were affected differently by the ruling relative to firms in industries with a low female labour share. Each analysis compares ban-state and non-ban-state firms within each group. These tests are supplemented by triple difference (DDD) specifications following Gruber (1994). The triple difference adds a third interaction that directly tests whether the ban-state valuation effect is significantly larger (negative or positive) for the treated group compared to the control group, netting out any general ban-state trends that affect both. The mechanisms of each specification are described in detail in section III.D.

Across all specifications, state-level controls are included because the ruling's practical impact depended on state policy choices after the Dobbs ruling. Post-Dobbs abortion access became highly polarized across states: many states moved to ban or sharply restrict abortion, while others took steps to protect or expand access. In our sample, treatment status is therefore strongly correlated with state political control, which creates an identification concern: Republican- and Democratic-controlled states differ on a range of characteristics that may independently affect firm outcomes. Prior evidence shows that partisan control is associated with systematic differences in labour-market outcomes across U.S. states, motivating controls for state economic conditions and labour-market tightness (Beland, 2015). We hence control for state-level GDP growth, unemployment rate and political leaning. Finally, trigger laws and subsequent bans came into force at different points following the Dobbs ruling. Missouri and

Arkansas enacted bans on the day of the ruling, while other states such as Tennessee, Texas and North Dakota waited several months, creating staggered treatment timing across states. Sun and Abraham (2021) show that standard two-way fixed effects estimators can produce biased estimates under staggered adoption when treatment effects are heterogeneous across cohorts. Given that all the bans happened within a year, and the assumption that the ruling itself resolved the legal uncertainty, we argue that investors could immediately update their expectations about which states would ban abortions. We nonetheless acknowledge that staggered treatment timing is a limitation of this thesis.

III.B. SAMPLE SELECTION

We retrieved firm accounting and market data from Compustat Capital IQ North America. The standard Compustat filters were applied, retaining only standardized, U.S. dollar-denominated, and consolidated financial statements for industrial firms. Both active and inactive firms were included to avoid survivorship bias, and we also included financial and utility firms for completeness. Investment vehicles such as exchange-traded funds and mutual funds were excluded, as these are not operating businesses but rather entities that hold financial assets, thus complicating the valuation measure. See Appendix A for a comprehensive table of observations lost at each step of the cleaning process. To assign firms to ban or non-ban-states, we collected their headquarters location. This introduces a limitation. Large firms may operate across many states simultaneously, meaning they may be classified as treated although they have most of their operations in non-ban-states. However, within the scope of this study, we rely on the assumption that the HQ reflects firms' primary location of operations. We also collect Compustat data to construct the firm-level control variables used in all regression specifications: leverage, firm size, return on assets (ROA) and cash holdings following Ahern and Dittmar (2012), which are further defined under III.C. State-level controls for unemployment rate and GDP growth were obtained from the Bureau of Labour Statistics (BLS) and the Bureau of Economic Analysis (BEA) respectively, and political leaning was constructed from 2020 presidential election data provided by MIT Election Data and Science Lab. Finally, the North American Industry Classification System (NAICS) codes were collected for each firm to construct the FLS measure. NAICS assigns a five-digit code to all firms operating in North America depending on their industry.

CEO information was collected from ExecuComp. For each individual, we collect their gender, age and start date in the CEO role, allowing us to compute their tenure. We excluded

all firms with missing values for CEO, age or tenure. The ExecuComp sample is smaller than the full Compustat universe, covering only the S&P1500, which consists of large and established companies. This introduces another limitation: the limited ExecuComp coverage effectively excludes smaller public firms with female CEOs entirely from the analysis. Bertrand and Hallock (2001) find that women in top corporate roles are disproportionately observed in smaller firms, and as noted in the literature review, smaller firms are more sensitive to investor sentiment due to higher uncertainty and more subjective valuations (Baker and Wurgler, 2006). This potentially means that the firms that may be most exposed to gender-related mispricing are underrepresented in the CEO analysis. Within our CEO subsample, there is a 6.8 % female CEO share for the firm-quarter observations (Appendix A). Although small and potentially limiting in terms of statistical power, female CEOs in large corporations are nonetheless of analytical interest due to their high exposure to investor attention.

The Female Labour Share measure was one we needed to construct. To compute this, we collected industry-level employment and gender data from a large survey of individuals in the U.S. workforce. Specifically, we extracted sex, age, employment status and what industry they work in through an industry code. The data was retrieved from IPUMS Annual Social and Economic Supplement (ASEC) to the Current Population Survey (CPS). Approximately 150 to 220 thousand people are surveyed each year, and this data is from March 2022 when 152,732 individuals were surveyed. We have chosen to let this sample represent the entire period of 2019 to 2024. While this can be seen as a limitation, national workforce gender composition is sticky due to deeply embedded structural features of organization and norms (Goldin, 2014). In the survey, a weight is assigned to each individual based on how representable that person is to the entire U.S. workforce. Using each individual's industry code, we computed industry-level gender distribution. We could then map each firm to their respective industry and get a proxy for how large female share every firm has. However, the individual-level industry codes used by IPUMS differ from the standard NAICS framework applied at firm-level, so we had to map these using the crosswalk provided by IPUMS. We chose to map the firms at a three-digit NAICS level and not five-digit. This preserves granularity while not having an abundance of overly specific industries. We dropped all individuals that were not in the labour force, unemployed or had missing industry codes, age, or sex. Roughly 14 % of individuals who were in the labour force, but whose industry code could not be matched to a three-digit NAICS, were also excluded. At this point, we had a sufficiently granular measure of the percentage of female workers in each industry. While this is not an entirely "true" measure of each firm's workforce,

and may be far off for some individual firms, it provides a good enough proxy at the aggregate level, considering the large sample.

III.C. VARIABLE DEFINITION

In accordance with previous literature on gender-related exogenous shocks and valuation, we have chosen Tobin's Q as the main dependent variable to identify an effect on firm value of the legislation change (Ahern and Dittmar, 2012; Adams and Ferreira, 2009). Tobin's Q is calculated as the sum of total assets and market equity less book equity, divided by total assets. Market equity is the quarterly closing stock price times common shares outstanding from Compustat. The variable was Winsorized at the 1st and 99th percentiles to control for outliers. Tobin's Q reflects investor expectations about future cash flows, rather than historic economic performance. It also captures the whole value of the firm by incorporating both equity and debt. This is valuable since an abortion legislation change may affect both equity valuation through investor perception, and debt pricing through cost of capital or altered access to capital, as established in the literature review. The key treatment variable is Female Labour Share (FLS), defined as the share of workers aged 15 to 44 who are female within a firm's NAICS3 industry, and the specific sample construction is outlined in section III.B. The age span of 15 to 44 follows the definition of childbearing age in Kearney, Levine and Pardue (2022). Firms are then split at the median FLS share within their NAICS3 industry into two mutually exclusive and collectively exhaustive groups: High FLS firms, whose industry has an above-median share of reproductive-age female workers, and Low FLS firms, whose industry falls below that median. An alternative comparison using a Male Labour Share sample, defined as firms with an above-median male share regardless of age, was considered but rejected. High FLS and High MLS samples are not mutually exclusive, since a firm can simultaneously have an above-median share of women aged 15 to 44 and an above-median share of males overall, causing some firms to appear in both groups. This overlap complicates joint estimation of fixed effects and causes the DDD coefficient to deviate from the arithmetic difference between group-specific DiD estimates, making interpretation less straightforward. The mutual exclusivity of the low vs. high FLS is theoretically grounded. Low FLS firms constitute a valid counterfactual because their workforce consists either predominantly of males or of women above childbearing age, none of which is directly exposed to abortion restrictions. Women above 44 rarely become pregnant, making them effectively equivalent to male workers from the perspective of Dobbs ruling exposure. Low FLS firms

should therefore experience no differential valuation effect from the ruling, which is precisely what a valid control group requires.

Firm-level controls include leverage, measured as total debt over total assets; firm size, measured as the natural logarithm of total assets; ROA, measured as operating income before depreciation over total assets; and cash holdings, measured as cash and equivalents over total assets. All four controls were Winsorized at the 1st and 99th percentiles to control for outliers. The state-level unemployment control was constructed by aggregating the monthly BLS data to quarterly by simple average. GDP growth was measured as year-over-year growth in real GDP from BEA, base year 2018. Political leaning is defined as whether or not a state is republican by computing the share of votes that went to the republican candidate in the 2020 election, using the MIT data.

III.D. EMPIRICAL MODEL

To identify the effect of the Dobbs ruling on firm value, we estimate the following equation:

$$Tobin's\ Q_{it} = \alpha + \beta(POST_t \times BAN\_STATE_i) + \gamma X_{it} + \delta_i + \lambda_t + \varepsilon_{it}$$

Where i indexes firm and t indexes quarters. $POST_t$ is a binary variable equal to one for quarters from 2022Q3 and onwards and zero otherwise. BAN_STATE_i is a binary indicator equal to one if the firm is headquartered in a state that enacted an abortion ban following Dobbs ruling. The coefficient β is our main parameter of interest, capturing any differential change in Tobin's Q for treated firms after the ruling relative to the control group. This specification is estimated separately on two subsamples within each analysis: high and low FLS, as well as female and male CEO. γX_{it} is a vector for time-varying firm-level controls including leverage, firm size, return on assets, cash holdings, as well as the state-level controls for unemployment, GDP growth and political leaning. δ_i denotes firm fixed effects, which absorb all observed or unobserved firm characteristics that are constant over time and that may affect Tobin's Q. Firm fixed effects ensure that the estimates are not driven by pre-existing, time-invariant differences between firms that have a female CEO or operate in female labour Share industries. λ_t denotes quarter fixed effects, which absorb common macroeconomic shocks affecting all firms in a given quarter such as inflation, federal reserve policy, and broader market trends. Given the inclusion of firm and quarter FE, we report within-R2 rather than adjusted R2, as the latter could inflate with the number of fixed effects. Note that the main effects of $POST_t$ and

BAN_STATE_i are not separately estimable since they are absorbed by the quarter and firm fixed effects respectively. The coefficient of interest, β, is identified purely from how treated firms changed relative to control firms around the ruling. Finally, given the length of the panel and the size of the sample, not all classical OLS assumptions are expected to hold strictly (see Appendix A). For example, heteroskedasticity and residual autocorrelation are natural features of long firm-level panels; we address both by clustering standard errors at the state level, which provides valid inference under arbitrary within-state serial correlation and cross-sectional heteroskedasticity without requiring either to be absent (Bertrand, Duflo and Mullainathan, 2004). As a further check, we re-estimate the main high FLS specification using Driscoll–Kraay standard errors, which are robust to arbitrary spatial and temporal dependence, and confirm that the inference is unchanged.

As an alternative specification, we estimate a triple difference:

$$\begin{aligned} Tobin's\ Q_{it} = {} & \alpha + \beta_1(POST_t \times BAN\_STATE_i) + \beta_2(POST_t \times GROUP_i) \\ & + \beta_3(BAN\_STATE_t \times GROUP_i) + \beta_4(POST_t \times BAN\_STATE_i \times GROUP_i) \\ & + \gamma X_{it} + \delta_i + \lambda_t + \varepsilon_{it} \end{aligned}$$

Where $GROUP_i$ is the third differencing variable taking on High FLS in the FLS analysis, and Female CEO in the CEO analysis. β_1 captures the ban-state DiD effect for the baseline group, and β_4 is the coefficient of interest, capturing whether the DiD effect is significantly different for one subgroup relative to the other. The DDD nets out any general ban-state valuation trend common to both groups, isolating the differential effect attributable to female labour share or female leadership respectively. Controls, fixed effects and standard error clustering follow the baseline DiD specification described above.

In addition to the main specifications, we apply the following robustness tests. First, we re-estimate all models excluding 2020Q1-2021Q4 to remove pandemic-era variation from the pre-period. Second, we exclude firms in financial (NAICS 52), energy (NAICS 21) and utility (NAICS 22) sectors, whose valuations were subject to unusual dynamics coinciding with the post-period. Third, we re-estimate the triple difference specifications with state times quarter fixed effects in place of only quarter FE, to account for state-specific, time-varying trends. Fourth, male- and female-led firms and high and low FLS firms are structurally different on many observable characteristics that affect performance and risk, meaning simple comparisons can be misleading unless we account for these differences. Hence, we apply entropy balancing

following McMullin and Schonberger (2020), which reweights control firm-quarters to achieve exact covariate balance on pre-treatment variables: leverage, ROA, firm size, cash holdings and Tobin's Q. We also apply 1:1 nearest-neighbour propensity score matching on the same pre-treatment variables retaining only matched firm-quarters for estimation. For the FLS channel, a fifth check removes the age definition 15-44 in the high FLS classification, testing whether results are specific to the childbearing-age definition.

IV. EFFECTS OF THE RULING ON FIRM VALUATION

This section presents the results for the two channels through which the Dobbs ruling may have affected firm valuation. We begin with the FLS channel, which utilizes geographic variation in abortion bans across industries with different Female Labour Share. We then turn to the CEO channel, which tests whether female-led firms were differentially valued following the ruling.

IV.A. DESCRIPTIVE STATISTICS

Table I: Pre-Period Descriptive Statistics

	High FLS	**Low FLS**	**Female CEO**	**Male CEO**
	(1)	(2)	(3)	(4)
N observations	22,584	32,013	1,148	18,652
N firms	2,124	2,923	127	1,491
Tobin's Q (mean)	4.194	5.383	2.490	2.342
Tobin's Q (median)	1.190	1.900	1.563	1.578
Female 15–44 share (mean)	0.288	0.149	0.198	0.199
Female 15–44 share (median)	0.303	0.152	0.169	0.194
Leverage (mean)	0.361	0.393	0.320	0.317
ROA (mean)	-0.027	-0.069	0.026	0.024
Firm size, log assets (mean)	6.900	5.886	8.329	8.358
Cash holdings (mean)	0.165	0.330	0.161	0.143
Female CEO (%)	5.4%	6.0%	100.0%	0.0%
In ban states (%)	21.5%	16.3%	22.3%	21.7%

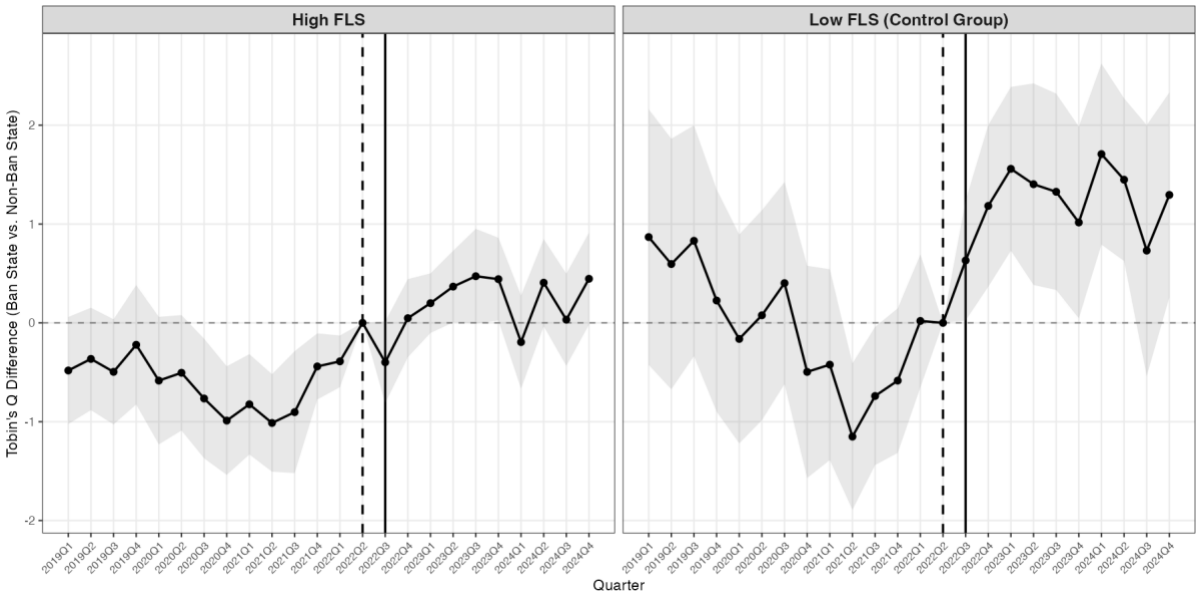
Table I presents pre-period descriptive statistics for the high and low FLS samples as well as the Female and Male CEO samples. For a full descriptive statistics table including quartile values, see Appendix A. Considering the large size of the FLS sample, we expect the mean and median Tobin's Q values to show stark divergence and skewness to the right, likely driven by

a number of high-multiple growth firms. This pattern is observed in the table, with both high (4.194) and low (5.383) FLS firms having a much higher mean than median (1.19 and 1.90). Further, high FLS firms have a lower mean Tobin's Q (4.194) than low FLS firms (5.383), a gap that persists at the median (1.19 vs. 1.90), reflecting the modest valuation of female-intensive industries relative to the broader market. This divergence is not observed to the same degree comparing the CEO subsamples. High FLS firms are slightly less levered and hold less cash, but are on average larger by assets, reflecting fundamental industry differences between the two groups. Mean ROA for the full sample is negative, driven by the large share of small, unprofitable growth firms in high FLS sectors, such as healthcare and technology, rather than deteriorating firm performance. The high FLS subsample shows a higher ban-state representation of 21.5 % compared to 16.3 % for the low FLS group, meaning firms with a large female workforce are headquartered in ban-states to a larger extent. As established in section II.A, female labour-intensive industries such as healthcare, education and social services are non-tradable and distributed with population, while industries with low Female Labour Share, such as manufacturing and mining, tend to agglomerate geographically. The CEO subsamples show positive mean ROA and are similar in size, with log assets of 8.329 and 8.358 for female and male-led firms respectively. Welch t-tests in Appendix A confirm that ban-state and non-ban-states firms differ significantly across most pre-period covariates in both the high and low FLS samples, motivating the entropy balancing and propensity score matching specifications reported in section IV.E.

IV.B. PRE-TRENDS

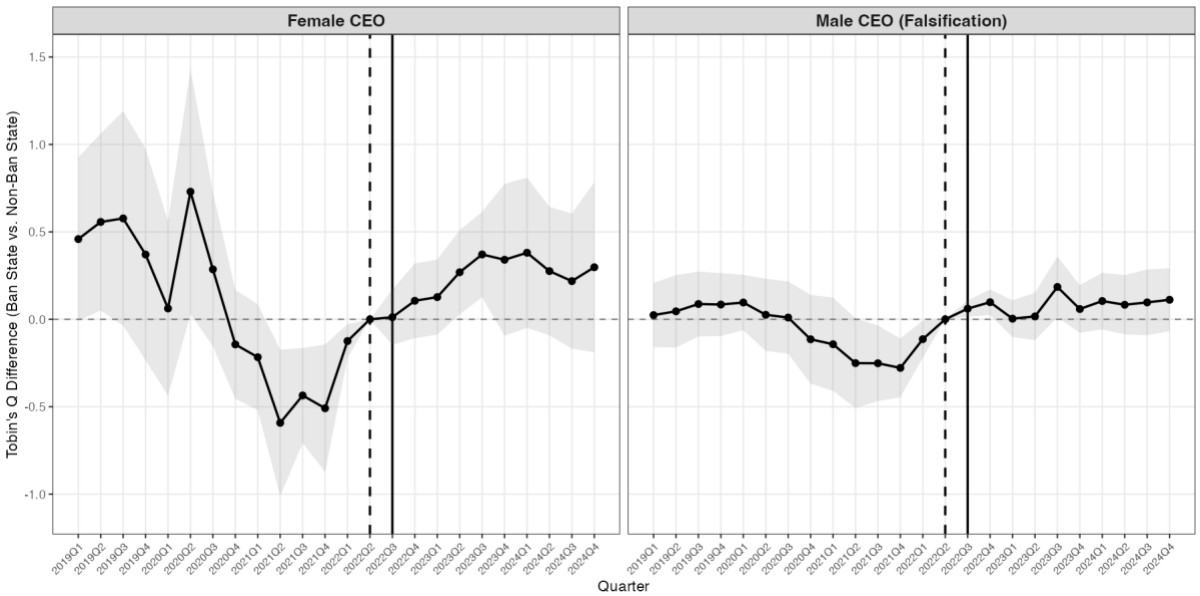
The parallel trends assumption is assessed through a joint Wald F-test and event study plots which trace the quarterly difference in Tobin's Q between ban-state and non-ban-states relative to the base period of 2022Q2. The orange dotted line marks the reference quarter, 2022Q2, where both the leak and ruling happened and is pinned to zero by construction. The blue dotted line marks 2022Q3 - the first quarter of the post-period.

Figure 1: Event Study – High vs. Low FLS



Dependent variable: Tobin's Q (winsorized). Coefficients plot β on Ban State × Event Time. Shaded band = 95% confidence interval. Dashed vertical line = 2022Q2 (reference quarter). Solid vertical line = 2022Q3.

Figure 2: Event Study – Female vs. Male CEO



Dependent variable: Tobin's Q (winsorized). Coefficients plot β on Ban State × Event Time. Shaded band = 95% confidence interval. Dashed vertical line = 2022Q2 (reference quarter). Solid vertical line = 2022Q3

The parallel trends assumption does not hold cleanly across any of the subsamples. The joint Wald F-tests reject the null of jointly zero pre-period coefficients across all four subsamples, confirming that the visual instability in the event study plots reflects pre-period divergence rather than noise. The most plausible driver of this divergence is differential

COVID-19 recovery speeds. All groups experience a visible dip during 2020 and 2021. How fast industries and firms recovered from the pandemic differed, and because the high and low FLS groups, as well as female and male CEO firms, are not evenly distributed across industries, their ban-state and non-ban-state subgroups entered the post-period on different trajectories. This is a consequence of industry composition within each subsample. Female-intensive industries such as healthcare and retail were disproportionately disrupted by lockdowns and reallocation of resources, and their uneven geographic distribution across ban and non-ban-states explains this.

Figure 1 presents the FLS channel event study and Figure 2 the CEO channel. In the High FLS panel, there is a notable dip at 2022Q3, which is not observed in the low FLS panel, consistent with high FLS firms in ban-states experiencing valuation pressure relative to non-ban-state firms following the ruling. The post-2022Q3 coefficients shift upward in both groups, with the shift being more pronounced for the low FLS sample. In the CEO channel, the male CEO panel is the most stable of the four, consistent with its role as a placebo. The female CEO panel is considerably noisier throughout the sample period with large swings, reflecting the small number of female CEO firms in the sample, amplifying idiosyncratic variation.

The violation of parallel trends does not invalidate the analysis, but it does require acknowledging that some portion of the estimated effects may be inflated by these pre-existing differential trends rather than solely reflecting the Dobbs ruling. Results should therefore be understood as descriptive of differential valuation trajectories rather than clean causal estimates, and conclusions are stated in directional rather than definitive terms. Although we cannot fully restore the assumption, we attempt to mitigate the pre-trends with robustness checks in section IV.E: excluding the pandemic period, removing distorted sectors, and applying entropy balancing and propensity score matching. The directional pattern persists across the board.

IV.C. THE FLS CHANNEL

Main Results

Table II reports the main DiD results for the FLS channel, estimated separately for high and low FLS firms. The coefficient of interest is Post x Ban State which captures the change in Tobin's Q for ban-state firms relative to non-ban-state firms following the Dobbs ruling. We

expect a negative and significant coefficient for the High FLS regression, and a small and insignificant coefficient for low FLS, which serves as the benchmark.

Table II: FLS Channel - Difference-in-Differences

	High FLS	**Low FLS**
Post × Ban State	0.723***	1.260***
	(0.171)	*(0.232)*
Median Tobin's Q	1.190	1.900
N	38,548	55,546
Within R^2	0.316	0.308

Notes: *p < 0.10, **p < 0.05, ***p < 0.01. Standard errors in parentheses, clustered by state. Dependent variable: Tobin's Q, winsorized. All specifications include firm fixed effects, quarter fixed effects, and controls (leverage, ROA, log assets, cash holdings, state unemployment rate, GDP growth).

Contrary to expectations, both groups show a positive and statistically significant ($p < 0.01$) ban-state effect. For high FLS firms in ban states, Tobin's Q increased by 0.723 units relative to non-ban-state high FLS firms in the post-period. To contextualise this magnitude, the pre-period median Tobin's Q for high FLS firms is 1.19 (Table I), implying that the estimated coefficient represents an increase of approximately 61 percent relative to the pre-period median. This should be interpreted in light of the broader ban-state tailwind. For low FLS firms, the corresponding estimate is larger at 1.260 units, also significant at the one percent level. Relative to the pre-period median Tobin's Q for low FLS firms of 1.900, this represents an increase of approximately 66 percent. The difference between the coefficients is 0.537, meaning that ban-state firms with low Female Labour Share experienced a larger valuation increase than those with higher female share. This difference is further supported in the DDD.

Alternative Specification

Table III: Triple Difference – FLS Channel

	FLS Channel
Post × Ban State × High FLS	-0.571**
	(0.254)
Median Tobin's Q	1.190
N	94,094
Within R^2	0.309

Notes: *p < 0.10, **p < 0.05, ***p < 0.01. Standard errors in parentheses, clustered by state. Dependent variable: Tobin's Q (winsorized). The sample pools both groups. All specifications include firm FE, quarter FE, and controls (leverage, ROA, log assets, cash holdings, state unemployment rate, GDP growth).

Table III reports the triple difference results, which is the preferred test of whether the ban-state valuation effect is significantly larger for high FLS firms relative to low FLS firms. The DDD pools both groups into a single regression and adds a triple interaction term, Post x Ban State x High FLS, with the coefficient capturing the difference between the two groups' DiDs. We expect a negative and significant coefficient. The triple interaction coefficient is negative 0.571 and significant at the five percent level. This means that following the Dobbs ruling, high FLS firms in ban states experienced a relative loss in Tobin's Q of 0.571 compared to low FLS firms in ban states, after accounting for the general ban-state valuation trend common to both groups. To contextualize this, a representative median-sized ban-state firm in a high Female Labour Share industry ($767M assets) experienced approximately $438M less in market value gain relative to a comparable ban-state firm in a low FLS industry, after the Dobbs ruling. These numbers are illustrative, and their high magnitudes will be interpreted in Section V. The negative sign is consistent with Hypothesis 1: firms more dependent on reproductive-age women in their labour force were relatively penalized in ban-states following the ruling, even though both groups experienced an absolute valuation increase driven by the broader ban-state economic environment.

IV.D. THE CEO CHANNEL

Main Results

Table IV: CEO Channel - Female vs. Male CEO

	Female CEO	**Male CEO (Placebo)**
Post × Ban State	0.186	0.131***
	(0.150)	*(0.046)*
Median Tobin's Q	1.563	1.578
N	2,608	35,730
Within R^2	0.169	0.045

Notes: *p < 0.10, **p < 0.05, ***p < 0.01. Standard errors in parentheses, clustered by state. Dependent variable: Tobin's Q, winsorized. All specifications include firm fixed effects, quarter fixed effects, and controls (leverage, ROA, log assets, cash holdings, state unemployment rate, GDP growth).

Table IV reports the main DiD results for the CEO channel, estimated separately for female CEO and male CEO firms. The female CEO coefficient is positive at 0.186 units but statistically insignificant. The male CEO coefficient is also positive at 0.131 units and

significant at the one percent level. The expected pattern in Hypothesis 2 would be a negative and significant female CEO coefficient, alongside a small and insignificant male CEO placebo coefficient. Neither condition is met. The positive and significant male CEO coefficient (0.131) suggests that the ban-state post-Dobbs ruling valuation increase we saw in the FLS channel extends to the broader CEO sample regardless of gender. For a male-led firm with a median Tobin's Q of 1.578, this represents an increase of roughly 8.3 percent. The within-R2 is notably lower for the male CEO regression (0.045 vs. 0.169) indicating that the regressors explain little of the remaining within-firm valuation in Tobin's Q for male CEO firms. The DDD in Table V provides the test of whether the ban-state effect is specifically concentrated among female CEO firms.

Alternative Specification

Table V: Triple Difference – CEO Channel

	CEO Channel
Post × Ban State × Female CEO	-0.050
	(0.224)
Median Tobin's Q	1.563
N	38,338
Within R^2	0.050

Notes: *p < 0.10, **p < 0.05, ***p < 0.01. Standard errors in parentheses, clustered by state. Dependent variable: Tobin's Q (winsorized). The sample pools both groups. All specifications include firm FE, quarter FE, and controls (leverage, ROA, log assets, cash holdings, CEO age, CEO tenure, state unemployment rate, GDP growth).

We expect the triple difference coefficient for the CEO, reported in Table V, to be negative and significant. Although negative 0.050, the coefficient is statistically insignificant. This indicates that the ban-state post-Dobbs ruling effect is not disproportionately concentrated among female CEO firms.

IV.E. ROBUSTNESS

Tables VI through IX report four robustness checks applied identically across the FLS and CEO channels. The checks address the main threats to the baseline specifications: COVID-era confounds, sector-specific valuation shocks coinciding with Dobbs ruling, and potential covariate imbalance between ban-state and non-ban-state firms. For the FLS channel, a fifth check relaxed the age restriction in the FLS definition.

Table VI: FLS Channel – DiD Robustness

	No COVID	No Fin/Ene/Util	Entropy Bal.	PSM	No Age Restr.
	(1)	(2)	(3)	(4)	(5)
Panel A: High FLS					
Post × Ban State	0.417**	0.510**	0.424**	0.301*	0.990***
	(0.176)	*(0.223)*	*(0.171)*	*(0.169)*	*(0.335)*
Median Tobin's Q	1.190	1.190	1.190	1.190	1.190
N	25,743	20,952	24,946	10,484	53,595
Within R^2	0.375	0.316	0.391	0.413	0.295
Panel B: Low FLS					
Post × Ban State	0.884***	1.190***	0.904***	0.155	1.024***
	(0.282)	*(0.253)*	*(0.285)*	*(0.356)*	*(0.286)*
Median Tobin's Q	1.900	1.900	1.900	1.900	1.900
N	37,191	51,032	36,006	11,250	40,499
Within R^2	0.315	0.311	0.342	0.322	0.335

Notes: *p < 0.10, **p < 0.05, ***p < 0.01. Standard errors in parentheses, clustered by state. Dependent variable: Tobin's Q (winsorized). All specifications include firm FE, quarter FE, and controls (leverage, ROA, log assets, cash holdings, state unemployment rate, GDP growth).

Table VII: FLS Channel – DDD Robustness

	No COVID	No Fin/Ene/Util	No Age Restr.	State × Qtr FE
	(1)	(2)	(3)	(4)
Post × Ban State × High FLS	-0.334	-0.580*	-0.110	-0.577**
	(0.299)	*(0.327)*	*(0.549)*	*(0.284)*
N	62,934	71,984	94,094	94,094
Within R^2	0.331	0.309	0.309	0.310

Notes: *p < 0.10, **p < 0.05, ***p < 0.01. Standard errors in parentheses, clustered by state. State × Qtr FE: replaces separate state and quarter fixed effects with fully interacted state × quarter fixed effects.

The high FLS coefficient remains positive and significant across all four main robustness checks. Excluding the COVID period reduces the coefficient from 0.723 to 0.417, and the DDD decreases in magnitude to -0.334 and loses significance. This is consistent with the pre-trend dip visible in the event study in section IV.B, suggesting that the pandemic period moderately inflates the baseline estimate. Removing financial, energy and utility firms yields a coefficient of 0.510, and strengthens the DDD to -0.580, significant at the ten percent level, indicating that the result is not mechanically driven by those sectors. The most demanding

check, the state x quarter fixed effects specification, yields a DDD of -0.577, significant at the five percent level. This specification absorbs state-specific time trends, addressing partisan economy confounds. Entropy balancing and PSM both yield positive and significant DiD coefficients, confirming that the result is not an artifact of covariate imbalance between ban-state and non-ban-state firms. The fifth check, which removes the 15-44 age restriction, yields converging DiD coefficients of 0.990 and 1.024 for high and low FLS respectively, both significant at the one percent level. The DDD collapses to -0.110 with no significance, which was expected. Across all five specifications, the high FLS coefficients remain below the corresponding low FLS coefficients, consistent with the negative differential documented in the main DiD and the DDD result in Tables II and III.

Table VIII: CEO Channel – DiD Robustness

	No COVID	**No Fin/Ene/Util**	**Entropy Bal.**	**PSM**
	(1)	(2)	(3)	(4)
Panel A: Female CEO				
Post × Ban State	0.047	0.293	0.049	-0.029
	(0.123)	*(0.218)*	*(0.120)*	*(0.129)*
Median Tobin's Q	1.563	1.563	1.563	1.563
N	1,811	2,039	1,583	674
Within R^2	0.159	0.180	0.175	0.196
Panel B: Male CEO (Placebo)				
Post × Ban State	0.059	0.108	0.060	0.002
	(0.041)	*(0.069)*	*(0.041)*	*(0.031)*
Median Tobin's Q	1.578	1.578	1.578	1.578
N	23,584	27,523	23,322	9,814
Within R^2	0.041	0.047	0.041	0.064

Notes: *p < 0.10, **p < 0.05, ***p < 0.01. Standard errors in parentheses, clustered by state. Dependent variable: Tobin's Q (winsorized). All specifications include firm FE, quarter FE, and controls (leverage, ROA, log assets, cash holdings, state unemployment rate, GDP growth).

Table IX: CEO channel – DDD Robustness

	No COVID	**No Fin/Ene/Util**	**State×Qtr FE**
	(1)	(2)	(3)
Post × Ban State × Female CEO	0.001	-0.017	-0.056
	(0.154)	*(0.276)*	*(0.223)*
N	25,395	29,562	38,338
Within R^2	0.044	0.053	0.048

Notes: *p < 0.10, **p < 0.05, ***p < 0.01. Standard errors in parentheses, clustered by state. State×Qtr FE: replaces separate state and quarter fixed effects with fully interacted state×quarter fixed effects.

Neither the female nor the male CEO channel shows any effect approaching conventional significance levels across any of the four robustness specifications, and the CEO channel DDD remains negligible across all three checks. This is fully consistent with the null result in our main specification and confirms that it is not an artifact of the baseline sample or specification choices. The male CEO placebo is also consistent with its role. One caveat is that the PSM matched sample retains only 674 female CEO firm-quarters, approximately 26 percent of the full female CEO sample, reflecting the difficulty of finding comparable non-ban-state firms for each ban-state female CEO firm on pre-treatment characteristics. Results from this specification should therefore be interpreted with caution.

V. DISCUSSION

V.A. THE FLS CHANNEL

Interpreting the positive coefficients

The most immediate question raised by Table II is why both the high FLS coefficient (0.723) and the low FLS coefficient (1.260) are positive and significant. There are three layers providing possible explanation, each independently influencing ban-state valuations during the post-period.

The first layer is the differential COVID-19 recovery trajectories of Republican and Democratic states. Following the sharp economic contraction of 2020 and the sectoral disruption of 2021, firm valuations across the board were recovering in the post-period. Republican governors eased pandemic restrictions earlier than Democratic-led states, putting natural upward pressure on Tobin's Q for ban-state firms, independently of Dobbs. This is

consistent with the event study plots, which show that both high and low FLS firms start trending upward from around 2021Q2 into 2022. The event study also reveals a pronounced pre-period dip during 2020 and 2021 for the high FLS group, concentrated in healthcare, retail, and education, the sectors that bore a disproportionate share of pandemic-era disruption. Including these quarters in the baseline moderately inflates the estimated post-period change. Excluding the COVID period reduces the high and low FLS DiD coefficients by roughly 40 %, and the DDD decreases in magnitude and loses significance. This reflects that results are partly driven by COVID recovery dynamics, yet a limitation is that we cannot fully account for these.

The second layer is the 2022 energy price surge. Following Russia's invasion of Ukraine in February 2022, global energy prices rose sharply, with Brent crude oil reaching approximately $128 per barrel in March, levels not seen since 2008 (EIA, 2023). The firms that benefited most - oil and gas producers, primary metals manufacturers, and mining companies - are concentrated in Republican-leaning states such as Texas, Oklahoma, Louisiana and North Dakota (EIA, 2014). These industries are male-dominated, and the states they are situated in strongly coincide with ban-states. This arguably created additional tailwind for low FLS firms in ban states that is unrelated to abortion policy. The gap between the high and low FLS coefficient (0.723 and 1.260) could thus be interpreted as being driven by the energy price surge. To test whether the difference is truly attributable to the Dobbs ruling, we excluded financial, energy, and utility firms in Table VI, column 2. Those firms are particularly sensitive to the interest rate environment and energy price surge of the time (Flannery and James, 1984). The fact that the coefficients decrease in magnitude (0.510 and 1.190), but the gap persists confirms both that the energy price surge is driving part of the baseline results, but also that it is not causing the difference (note that the reduction in the high FLS coefficient is due to exclusion of the finance sector, classified as high FLS). This further strengthens our hypothesis, and the DDD supports this, yielding a negative coefficient of -0.580, significant at ten percent.

Underlying these macroeconomic events is a deeper identification challenge. As documented by Béland (2015), Republican and Democratic states differ systematically in labour market conditions and public expenditure patterns that independently affect firm valuation. These differences are persistent and predate the Dobbs ruling, potentially creating valuation effects that are unrelated to abortion policy. As discussed in section III, state-level controls partly address these differences, but when a policy shock aligns almost perfectly with pre-existing partisan geography, disentangling the abortion-policy effect from these structural economic features becomes genuinely challenging. That the DDD remains negative and

significant when state x quarter fixed effects are included, absorbing all state-specific time trends, provides evidence that the differential is not simply a product of this partisan economic divergence.

Apart from the state dimension, ban-state and non-ban-state firms may differ systematically in their pre-treatment characteristics such as size, leverage and profitability, in ways that could confound the post-Dobbs ruling comparison. Entropy balancing and Propensity Score Matching address that concern by reweighting or matching control firms to be more comparable to treated firms on such dimensions (McMullin and Schonberger, 2020; Rosenbaum and Rubin, 1983). Both tests return positive and significant coefficients of 0.424 and 0.301 respectively, and the slight dip in magnitude suggests that pre-existing firm characteristics explain part of the raw effect though not all of it.

Lastly, the magnitude of the coefficients (0.723 and 1.260) likely reflects the presence of high-multiple growth firms, whose Tobin's Q is highly sensitive to investor sentiment shifts during an already volatile market period. The maximum Tobin's Q in the sample reaches 136 even after winsorization at the 1st and 99th percentiles. This implies that the absolute coefficient should be interpreted with caution.

The differential and the DDD

Despite the macroeconomic events causing both high and low FLS firms to gain in absolute terms, the gap between them is negative: The high FLS sample gained 0.723, whereas low FLS gained 1.260, a difference of 0.537 Tobin's Q units. In other words, the firms with the most females in childbearing age experienced a lower valuation increase than firms with a low share of females in childbearing age. This finding is directionally consistent with Hypothesis 1. The triple difference formalises this comparison, yielding a coefficient of −0.571, significant at the five percent level. What does this mean? If investors, following the Dobbs ruling, started believing that firms dependent on reproductive age women will suddenly have higher labour disruption costs and weaker retention and hiring, then those firms would bear a valuation penalty relative to other ban-state firms. The minor discrepancy between the DDD figure and the arithmetic difference between the DiDs reflects the shift to a pooled regression with shared fixed effects and controls and carries no substantive interpretation. Crucially, as Fama (1970) establishes, such forward-looking revisions are reflected in firm valuation before any disruption appears in reported fundamentals. The valuation response is an early-stage signal, not a lagging indicator of actual labour market change. To test whether the

effect is truly driven by the Dobbs ruling and not by other factors, we conducted aforementioned robustness tests.

Robustness and the age-restriction test

Our findings are not incidental. Across all five robustness specifications, the high FLS coefficient is consistently lower than the corresponding low FLS coefficient, yielding a negative difference in every case. Several of these differences are formally tested through the DDD robustness checks in Table VII. However, if there truly is a negative valuation effect for firms highly dependent on females, that effect should theoretically show up specifically among firms with female in *childbearing age,* since those women are the most affected by Dobbs. Hence, we needed to formally test the childbearing age definition by removing the 15-44 age restriction.

When FLS is redefined using the all-age female share, the high FLS DiD coefficient rises to 0.990 and the corresponding all-age low FLS DiD coefficient is 1.024. The arithmetic gap between the two groups in the DiD, 0.571 under the childbearing-age definition, effectively collapses to 0.034 under the all-age definition, and the DDD falls to -0.110 and loses significance. This convergence is precisely what we would expect. Women above 44 are unlikely to become pregnant, making them economically equivalent to male workers from the perspective of abortion-restriction exposure. When these older female workers are folded into the FLS classification both groups contain industries with substantial female labour in a general sense. Though, neither is specifically defined by dependence on workers for whom reproductive health access is directly relevant. The comparison between the two specifications shows that the valuation differential is not a generic female-labour phenomenon but one that is specifically and materially conditioned on reproductive age. This is the most direct evidence in the empirical analysis that the channel operates as theorised.

Does the resource channel hold?

Taken together, the FLS results provide support for the female labour resource channel (H1). The high FLS DiD is robust across all specifications and survives the exclusion of the two most confounding macroeconomic episodes in the sample period. The DDD is negative and directionally consistent with the hypothesis. The age-restriction comparison offers a sharp, mechanistically grounded test that substantially strengthens the interpretation. On balance, the

evidence is consistent with the hypothesis that investors priced a female labour-supply constraint for high FLS firms following the ruling, though this conclusion is built on a pattern of converging results rather than any single decisive test.

V.B. THE CEO CHANNEL

Sample characteristics and investor perception

The CEO channel differs fundamentally from the FLS channel in what it tests. Female CEOs of large, listed firms are typically well above the childbearing age of 44, as defined by Kearney, Levine and Pardue (2022) (the mean age of female CEOs in the ExecuComp sample is 56.6 years), meaning any valuation effect in this channel cannot operate through direct workforce constraints. It is instead a pure test of investor perception: whether markets discount the equity of female-led firms in ban states following a ruling that heightens the salience of gender in the corporate context. The sample reflects these constraints. The female CEO subsample comprises only 2,608 firm-quarters across 213 firms, compared to 35,730 firm-quarters across 1,892 firms in the male CEO group. This asymmetry reflects the underrepresentation of women in CEO positions among large publicly listed firms and potentially weighs negatively on the statistical power for this channel.

The puzzling DiD result

The results in Table IV present a pattern that is the opposite of what Hypothesis 2 would predict. The female CEO coefficient is 0.186 and insignificant, while the male CEO coefficient is 0.131 and significant at the one percent level. One feature of this pattern warrants careful interpretation: A clean test of the female leadership perception channel requires the male CEO result to serve as a placebo: it should be small and insignificant, confirming that any ban-state valuation effect is concentrated among female-led firms. The fact that it is instead positive and significant suggests that the ban-state post-Dobbs ruling valuation trend reflects the general ban-state tailwind. The male CEO sample, by virtue of its much larger size, is simply better positioned to detect this broad trend, whereas the female CEO sample is not. However, we have not tested whether the female CEO sample is sufficiently large to detect a valuation effect if there were any present, leading us to be careful drawing any conclusions from the results.

The DDD and what it does not tell us

The small and insignificant triple difference on the CEO sample contrasts sharply with the FLS channel, and confirms the insignificant female CEO DiD result. It also confirms that whatever ban-state valuation trend exists in the CEO sample, it is distributed equally between female- and male-led firms. This directly contradicts the prediction of Hypothesis 2: if a perception channel were operative, we would expect the ban-state effect to be more negative for female CEO firms, producing a negative and significant triple interaction. The fact that it is not suggests that the Dobbs ruling did not trigger a differential investor response to female leadership, at least not at a magnitude detectable at the firm valuation level.

Robustness

The female CEO null result is robust across all four specifications and the CEO channel DDD remains negligible across its three robustness checks, giving no reason to distrust the null. The one pattern worth noting is that entropy balancing and PSM produce the smallest estimates. This suggests that part of the raw female CEO effect in the baseline reflects pre-existing differences between firm types, though, the PSM matched sample is too small to draw conclusions from. For the male CEO placebo, the baseline significance disappears once COVID and distorted sectors are removed. This is precisely what we would expect if the significant baseline coefficient reflects the broad ban-state macroeconomic tailwind rather than a gender-leadership mechanism.

Does the perception channel hold?

The CEO results do not support the hypothesis that the Dobbs ruling triggered an investor perception discount on female-led firms in ban states. The female CEO coefficient is positive across all specifications and never approaches conventional levels of statistical significance, and the DDD is negligible. The robustness checks also leave the interpretation unchanged: we find no evidence that the Dobbs ruling triggered a differential valuation response for female-led firms in ban states. Before attributing this to the absence of an effect, it is worth considering whether a methodological issue could be driving the insignificance. The specification mirrors the FLS channel which does produce significant results, and the parallel trends violations are no more severe for the female CEO group than for the other subsamples. The sample of 213 firms is concentrated among large-cap, professionally managed companies

where investor behaviour is relatively homogenous, which may itself explain the null: if a perception channel operates, it is more plausibly detected among smaller firms where investor sentiment plays a larger pricing role, as Baker and Wurgler (2006) and Smith, Chown and Gaughan (2021) document. There is therefore no compelling methodological reason to distrust the null result. Future work with more granular data or a longer post-period may find evidence of it, but the present results do not support it.

VI. CONCLUSION

This thesis set out to test whether the Dobbs v. Jackson Women's Health Organization ruling of June 2022 was priced into the equity valuations of U.S. publicly listed firms, operating through two distinct channels: Female Labour Share and female leadership. Using a difference-in-differences design over 2019Q1 to 2024Q4 with Tobin's Q as the dependent variable, we find support for the Female Labour Share (FLS) hypothesis and no support for the female CEO hypothesis.

For the FLS channel, high FLS firms in ban states experienced a relative reduction in valuation compared to low FLS ban-state firms following the ruling, with a triple difference estimate of -0.571, significant at the five percent level. The result holds across multiple robustness specifications. The FLS measure is designed around women aged 15 to 44 specifically. The valuation gap narrows sharply when the childbearing-age restriction is removed from the FLS definition, which we interpret as direct evidence that the differential is specifically tied to reproductive-age female workers rather than female labour more broadly. This segment of workers is most directly affected by abortion restrictions.

For the CEO channel, we find no statistically significant effect: female CEO firms show no differential valuation change relative to male CEO firms in ban states, and the triple difference is negligible at -0.050. Female CEOs in large, listed firms are on average well above childbearing age, and any valuation effect in this channel would be a pure investor perception effect. It would specifically test whether investors discount female leadership in times of high-salience and pessimistic gender-related news. The absence of any detectable effect suggests that, at least among large-cap firms, the Dobbs ruling did not trigger a measurable, sentiment-driven discount on female leadership, a finding that adds nuance to the broader literature on gender bias in equity markets.

VI.A. CONTRIBUTIONS

This study makes several contributions. First, we directly extend Zandberg (2021) and Core's (2024) findings that maternity risk is material for firm success in entrepreneurial settings, by testing whether it is also priced into equity markets. Second, by designing a measure of Female Labour Share in different industries, defined around women aged 15 to 44 specifically, we contribute with a new perspective for understanding industry differences: the division between male- and female-dominated industries. Hence, we contribute to both MacKay and Phillips (2005) findings that industry membership determines financial characteristics, and Goldin's (2014) research of flexibility and work patterns across industries. We also extend sentiment literature (Baker and Wurgler, 2006; Pástor and Veronesi, 2012; Smith, Chown and Gaughan, 2021) by testing whether investors revise their perception of female leadership in times of salient and pessimistic gender-related news, tested through the CEO channel. Lastly, we build on Ahern and Dittmar (2012), Adams and Ferreira (2009) and Matsa and Miller's (2013) board studies. These look at the Tobin's Q impact of policy shocks directly aimed at firms, whereas we examine how an exogenous policy shock affects Tobin's Q. The chain through which we test this, running from policy shock, through investor sentiment, to firm valuation, has not been linked empirically. This ties together the gender and corporate finance literature, the investor sentiment and policy shock literature, and the reproductive health and labour supply literature. To our knowledge, this is the first study to test whether a reproductive health policy shock is priced into the equity valuations of publicly listed firms. We aim to address the current gap in gender-related studies in public equity markets settings.

VI.B. LIMITATIONS AND FUTURE RESEARCH

A first limitation concerns the scope of the sample. Both the Compustat and ExecuComp databases cover only publicly listed firms, meaning private firms are entirely excluded from the analysis. As the literature documents, smaller and privately held female-led firms tend to be more sensitive to investor sentiment and face greater financing constraints than their large-cap counterparts. Smaller firms also have the highest female CEO representation. For the CEO channel analysis, it means that the firms that are the most exposed to investor perception may be entirely left out, meaning if there is an effect, our sample is within the wrong scope to detect it. Future work that incorporates private firm data, for instance through registry-

based datasets or private credit records, could test whether the channel is stronger outside the listed universe, where valuation is more subjective and investor sentiment has a larger pricing role.

Apart from looking at the private capital market, the reproductive health channel may operate more visibly in other parts of the capital market that we do not examine: venture capital funding of female-founded firms or analyst price target revisions. Core (2024) and Zandberg (2021) find their strongest results in entrepreneurial and early-stage settings precisely because those contexts are more sensitive to the maternity risk channel. Restricting the analysis to public equity may systematically understate the broader financing consequences of the ruling. Extending the framework to VC funding decisions, debt pricing, or analyst forecasts following Dobbs ruling would provide a more complete picture of how reproductive health policy transmits into capital markets.

Third, while our tests indicate that investors priced a female labour supply constraint, we never observe whether that constraint actually materialised. This study is ultimately about investor expectations: whether markets revised their beliefs about future workforce availability and continuity in high FLS firms located in ban states. What we cannot verify is whether those expectations were correct, whether ban-state firms indeed experienced higher employee turnover, greater difficulty filling roles, or rising compensation costs in the post-Dobbs ruling period. Future research that directly links the ruling to firm-level labour outcomes, such as workforce gender composition, retention rates, or cost data, would allow a cleaner test of the underlying mechanism and establish whether the valuation signal identified here reflects a genuine resource constraint or a sentiment-driven response that ultimately reverted.

Relatedly, the post-period of three years may be too short to fully capture the mechanism we theorise. Labour market adjustments are inherently slow-moving: workforce composition, retention rates and hiring patterns shift gradually as contracts expire, employees make longer-run career and location decisions, and firms revise their recruitment strategies. If investors are pricing a constraint that materialises over five to ten years rather than three, the valuation effect may still be accumulating beyond our sample window. A longer post-period, as more post-Dobbs ruling data becomes available, would allow a more complete assessment of whether and how the ruling ultimately transmits into firm value. A related extension would use high-frequency data around the specific dates of the May 2022 leak and the June ruling to complement the longer-horizon DiD approach with a short-window event study, capturing any immediate investor reaction that our quarterly design may miss.

Finally, this study focuses exclusively on the United States, a setting where the Dobbs ruling shock is deeply entangled with partisan geography. Abortion bans align almost perfectly with Republican-controlled states, which differ from Democratic states along a range of structural economic dimensions. These include regulatory environment, tax burden and labour market conditions, making it genuinely difficult to isolate the abortion-policy effect from these pre-existing differences, even with state-level controls and triple difference specifications. The period in which the ruling happened adds further identification difficulties through macroeconomic instability. Other countries that have undergone similarly significant legal shifts in reproductive health policy, such as restrictions introduced in Poland or Malta or liberalisations in Ireland and Mexico, could offer cleaner identification. In those settings, the policy change is less confounded by pre-existing partisan economic divergence, making it easier to attribute any valuation effect to the reproductive health channel specifically. For example, Ireland legalized abortion in 2018, a more stable period macroeconomically. It also goes the opposite way to the Dobbs ruling, meaning it would test whether expanding reproductive rights is priced into firm valuations, rather than restricting them. Comparative work across such settings would help establish how much of what we find is specific to the U.S. political economy and how much reflects a more general mechanism through which gender-related policy shocks are priced in equity markets.

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APPENDIX A

Table A1: Sample Attrition

Sample Restriction	Firm-quarters	Unique Firms	Notes
Raw Compustat universe	322,902	—	Quarterly, all formats
After standard filters	322,902	15,960	
After US state restriction	271,942	13,578	HQ state identified
After dropping ETFs and funds	166,753	6,964	Investment vehicles
After date restriction 2019Q1–2024Q4	127,949	6,591	Main panel
FLS subsample	114,938	5,953	Non-missing female 15–44 share
High FLS (above-median female 15–44 share)	38,548	2,585	Main FLS regression sample
Low FLS (below-median female 15–44 share)	55,546	3,368	FLS control group
CEO subsample	38,338	1,966	Regression-ready CEO sample
Female CEO subsample	2,608	213	Main CEO regression sample
Male CEO subsample	35,730	1,892	CEO falsification sample

Quarterly Compustat S&P universe, 2018–2025. CEO subsample requires Execucomp match with non-missing CEO gender, age, and tenure.

Table A2: Descriptive statistics

Variable	N	Mean	SD	P25	Median	P75	Min	Max
Panel A: FLS Sample								
Tobin's Q	94,094	4.571	15.593	1.035	1.510	2.765	0.538	136.574
Leverage	94,094	0.377	0.782	0.053	0.212	0.427	0.000	6.614
ROA	94,094	-0.057	0.251	-0.046	0.006	0.027	-1.975	0.130
Firm size (log assets)	94,094	6.327	2.716	4.653	6.643	8.164	-1.891	12.150
Cash holdings	94,094	0.257	0.290	0.041	0.121	0.396	0.000	0.979
Female 15–44 share	94,094	0.206	0.085	0.145	0.194	0.261	0.018	0.515
Ban state	94,094	0.180	0.384	0.000	0.000	0.000	0.000	1.000
Post-Dobbs	94,094	0.420	0.493	0.000	0.000	1.000	0.000	1.000
Unemployment rate	94,094	4.882	2.447	3.500	4.100	5.200	1.700	24.700
GDP growth	94,094	0.066	0.052	0.047	0.064	0.089	-0.156	0.260
Panel B: CEO Sample								
Tobin's Q	38,338	2.314	2.317	1.127	1.558	2.550	0.538	55.441
Leverage	38,338	0.321	0.248	0.133	0.300	0.445	0.000	4.279
ROA	38,338	0.026	0.037	0.012	0.025	0.039	-1.975	0.130
Firm size (log assets)	38,338	8.440	1.697	7.269	8.344	9.575	-1.891	12.150
Cash holdings	38,338	0.141	0.159	0.034	0.084	0.184	0.000	0.978
Female CEO	38,338	0.068	0.252	0.000	0.000	0.000	0.000	1.000
CEO age	38,338	57.956	7.420	54.000	58.000	62.000	28.000	94.000
CEO tenure	38,338	7.623	7.956	2.000	5.000	10.000	0.000	56.000
Ban state	38,338	0.210	0.407	0.000	0.000	0.000	0.000	1.000
Post-Dobbs	38,338	0.408	0.491	0.000	0.000	1.000	0.000	1.000
Unemployment rate	38,338	4.858	2.442	3.500	4.067	5.100	1.700	24.700
GDP growth	38,338	0.065	0.052	0.045	0.064	0.089	-0.156	0.260

Summary statistics for the FLS regression sample (Panel A) and CEO regression sample (Panel B), 2019Q1–2024Q4. All variables winsorized at the 1st and 99th percentiles except indicator variables.

Table A3: Correlation Matrix (FLS Sample)

	Post×Ban	**Leverage**	**ROA**	**Firm Size**	**Cash Hold.**	**Unemp. Rate**	**GDP Growth**
Post×Ban	1.000						
Leverage	−0.031***	1.000					
ROA	0.043***	−0.585***	1.000				
Firm Size	0.046***	−0.386***	0.504***	1.000			
Cash Hold.	−0.083***	0.098***	−0.244***	−0.382***	1.000		
Unemp. Rate	−0.141***	0.014***	−0.017***	−0.018***	0.093***	1.000	
GDP Growth	0.025***	0.013***	−0.003	−0.021***	0.029***	−0.532***	1.000

Pairwise Pearson correlations, lower triangle. *, **, *** significant at 10%, 5%, 1%. High FLS sample, 2019Q1–2024Q4.

Table A4: Variance Inflation Factors

Variable	**VIF**
Leverage	1.23
Firm size (log assets)	1.19
ROA	1.29
Cash holdings	1.01
Unemployment rate	1.04
GDP growth	1.03

VIF on FE-demeaned variables, baseline High FLS DiD. VIF > 10 indicates problematic multicollinearity.

Table A5: Gauss-Markov Assumptions

Assumption	Method	Evidence	Verdict
Linearity	Specification	Linear two-way FE; continuous outcome	Maintained
No multicollinearity	VIF	Max VIF = 1.29	Satisfied
Exogeneity	DDD + event study	Pre-trends flat; DDD isolates within-group variation	Supported
Homoskedasticity	Clustered SEs	State-clustered SEs (50 clusters)	Addressed
No serial correlation	Clustered SEs	Clustering allows arbitrary within-state serial correlation	Addressed
No cross-sectional dep.	State×Quarter FE	State×Quarter FE robustness absorbs state-specific time shocks	Addressed

Two-way FE DiD/DDD. SEs clustered by state (50 clusters).

Table A6: Wald F-Tests for Pre-Trend Coefficients

Sample / Group	Pre-quarters	F-statistic	df	p-value
High FLS	13	3.78	13	< 0.001
Low FLS (Control)	13	6.95	13	< 0.001
Female CEO	13	5.95	13	< 0.001
Male CEO (Falsif.)	13	6.05	13	< 0.001
DDD Triple Interact.	13	108.14	13	< 0.001
High FLS (No COVID)	5	4.39	5	< 0.001
Low FLS, No COVID (placebo)	5	3.47	5	0.004
Female CEO (No COVID)	5	5.21	5	< 0.001
Male CEO, No COVID (placebo)	5	3.34	5	0.005
DDD Triple, No COVID	5	5.59	5	< 0.001

Joint Wald F-test: all pre-period event-time coefficients = 0. Full sample: 13 pre-quarters (2019Q1–2022Q2). No COVID: excludes 2020Q1–2021Q4, leaving 5 pre-quarters (preferred).

Table A7: Pre-Period Balance – Ban vs Non-Ban-States

Variable	Ban State	Non-Ban State	Difference	p-value	Sig.
Panel A: High FLS					
Tobin's Q	2.392	4.687	-2.295	0.000	***
Leverage	0.339	0.367	-0.028	0.010	*
ROA	0.003	-0.035	0.038	0.000	***
Firm size (log assets)	7.220	6.812	0.408	0.000	***
Cash holdings	0.120	0.177	-0.058	0.000	***
Unemployment rate	5.017	5.680	-0.664	0.000	***
GDP growth	0.076	0.063	0.013	0.000	***
Panel B: Low FLS					
Tobin's Q	4.251	5.604	-1.352	0.000	***
Leverage	0.415	0.389	0.027	0.015	*
ROA	-0.034	-0.076	0.042	0.000	***
Firm size (log assets)	6.551	5.757	0.795	0.000	***
Cash holdings	0.181	0.359	-0.178	0.000	***
Unemployment rate	5.062	5.735	-0.673	0.000	***
GDP growth	0.080	0.065	0.015	0.000	***

Welch t-tests, pre-Dobbs period (2019Q1–2022Q2), regression-ready sample. Difference = Ban − Non-Ban mean. p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

APPENDIX B

Generative AI Disclosure

During the course of this thesis, generative AI tools were used as supportive aids throughout the research and writing process. In particular, Claude Sonnet 4.6, developed by Anthropic and accessible via claude.ai, and ChatGPT 5.4, developed by OpenAI and accessible via chatgpt.com, were used on a continuous basis throughout the project. These tools were used for purposes including coding support, language and grammar revision, idea generation and feedback on structure and formulation. All substantive academic judgement, analysis, interpretations and final editorial decisions remain the responsibility of the authors.