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Employment Protection and Bank Dividends: Evidence From Wrongful Discharge Laws

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ABSTRACT

Using the staggered adoption of the good faith exception to U.S. wrongful discharge laws, we examine the causal effect of employment protection and firing costs on bank dividend payouts. We find stronger employment protection significantly reduces dividends, with good faith adoption lowering payouts by 7% relative to the mean. In line with the operational leverage hypothesis, cross-sectional tests show this effect is concentrated among banks with higher labour costs and greater financial leverage. Overall, our results document how employment conditions influence bank dividend payouts and shape the allocation of corporate value among shareholders and employees.

1 | Introduction

Bank dividend payouts erode equity capital and can affect both bank resilience and broader financial system stability (Acharya et al. 2017; Cziraki et al. 2024; Vallascas et al. 2025). Accordingly, a large literature has examined the determinants of banks' payout policies and how they differ from those of non-financial firms (e.g., Bessler and Nohel 1996; Floyd et al. 2015; Caiazzo et al. 2025). A distinctive feature of banks, compared to non-financial firms, is their reliance on skilled labour and human capital in information-sensitive lending (Philippon and Reshef 2012). Bank employees play a central role in screening and monitoring borrowers (Diamond 1984; Cole et al. 2015), producing soft information (Berger and Udell 1995; Stein 2002) and maintaining lending relationships (Rajan 1992; Petersen and Rajan 1994). At the same time, employees' wages and salaries represent the largest component of banks' non-interest expense (Pinnuck and Lillis 2007; Hall 2016).[1] Consequently, changes in labour adjustment costs have the potential to influence banks' payout decisions through multiple channels. Yet, little is known about how employment protection and firing costs affect bank dividend policies.

In this paper, we examine how stronger employment protection and increased firing costs affect bank dividend payouts. From a theoretical perspective, their effect on banks' payouts is, a priori, ambiguous. On the one hand, banks facing higher employment protection and greater dismissal costs may adopt more conservative payout policies and reduce dividends, as these costs can limit operating flexibility and reduce competitiveness in at least two ways. First, greater employment protection and higher firing costs can limit banks' ability to reduce employment when needed, such as during a financial crisis, making labour more rigid, and earnings more volatile (e.g., Messina and Vallanti 2007; Caggese et al. 2019; Laeven et al. 2023). Second, greater labour adjustment costs reduce the elasticity of substitution between capital and labour, thus constraining banks' ability to adopt new technologies and limiting their ability to remain competitive (e.g., Fung 2006; DeYoung et al. 2007; Mamatzakis et al. 2015). Consequently, these frictions make labour inputs more fixed, which in turn, increases operational leverage, makes earnings more volatile, and discourage the adoption of new technologies (Simintzi et al. 2015; Serfling 2016). We refer to this line of argument as the *operational leverage* channel.

On the other hand, stronger employment protection and increased firing costs may cause banks to increase dividend payouts to deter potential opportunistic rent-seeking behaviour among employees. More specifically, greater employment protection shifts wealth from shareholders to employees, which can encourage possible adverse behaviours, such as shirking or reduced productivity, as well as the leveraging of dismissal protections for private gain (Simintzi et al. 2015; Serfling 2016). For example, empirical research has shown that employees may exploit the higher costs of termination to negotiate higher wages, obtain additional benefits, and avoid disciplinary actions that might otherwise lead to dismissal (e.g., Shapiro and Stiglitz 1984; Kugler and Saint-Paul 2004; Garibaldi and Violante 2005; Autor et al. 2007). In response to the above, banks may increase dividend payouts to minimise the capital available to rent-seeking employees and maximise the value of government guarantees, thereby shifting risk from shareholders to taxpayers (Keeley 1990; Srivastav et al. 2014; Acharya et al. 2017). We refer to this line of reasoning as the *risk-shifting* channel.[2]

Ultimately, these two predictions imply opposing effects of employment protection on banks' payout policies. On the one hand, when employment protection significantly increases labour rigidity and operational leverage, the resulting reduction in operational flexibility and increase in earnings volatility may induce banks to adopt more conservative payout policies to preserve financial flexibility (Simintzi et al. 2015) and to avoid further costly dividend reductions in the future (Healy and Palepu 1988; Bessler and Nohel 1996). On the other hand, when stronger employment protection primarily increases employees' bargaining power without substantially limiting banks' ability to adjust labour in response to adverse shocks, banks may respond through higher payouts, reducing internally available resources and shifting risk from shareholders to taxpayers.[3] Given the a priori ambiguity of these competing predictions, we test this relationship empirically.

To generate plausibly exogenous variation and test these competing predictions, we exploit the quasi-natural experiment arising from the staggered adoption of Wrongful Discharge Laws (WDLs) by U.S. state courts. WDLs have three common law exceptions to the traditional at-will employment doctrine that enables employers to terminate any employee, for any reason, without the risk of legal liability. These legal exceptions are: the good faith exception, the implied contract exception, and the public policy exception. In practice, the good faith exception requires employers to demonstrate just cause for termination and safeguards employees from being dismissed in bad faith, with malice, or as an act of retaliation. In line with Acharya et al. (2014), Serfling (2016), Bai et al. (2020), we focus our analysis on the adoption of the good faith exception, as this provision represents the most significant departure from at-will employment (Kugler and Saint-Paul 2004).

To examine the causal effect of good faith adoption on banks' dividend payouts, we use a sample of 364 unique listed banks over the period 1973–2002. Intuitively, our empirical approach compares the payouts of banks headquartered in states that adopt the good faith exception (treatment group) with banks incorporated in states that do not (control group). However, the staggered nature of good faith adoption gives rise to an empirical issue known as the forbidden comparison problem (Sun and Abraham 2021). Recent research has shown that conventional two-way fixed effects difference-in-differences estimator can yield biased estimates of the treatment effect when treated units are inappropriately used as controls for not-yet-treated units (Sun and Abraham 2021; Callaway and Sant'Anna 2021; Goodman-Bacon 2021; Baker et al. 2022). To address this identification issue, we follow Gormley and Matsa (2011), Gormley et al. (2013), and Duchin et al. (2025) and implement a stacked difference-in-differences design. Specifically for each adoption event, we construct a 9-year event window (−4; +4) and form cohort-specific subsamples of treated and never-treated banks, which we then stack to estimate the average treatment effect. This approach alleviates the forbidden comparison problem and yields a more precise estimate of the true causal effect (Gormley and Matsa 2011; Baker et al. 2022).

Our baseline results show that stronger employment protection and higher firing costs, resulting from the adoption of the good faith exception, have a negative and statistically significant effect on banks' dividend payouts. Specifically, we find the adoption of good faith causes, on average, a 7.0% reduction in dividend payouts, with respect to the pre-treated sample mean. This result holds after controlling for several bank-level characteristics and state-level controls, such as gross domestic product (GDP) growth and political balance, as well as the adoption of adjacent WDLs. These findings are consistent with the *operational leverage* channel and represent an economically significant effect. For instance, as a reference for economic significance, we note that Bai et al. (2020) finds that the adoption of the good faith law increases the capital expenditure of non-financial firms by 6.5%, while Beuselinck et al. (2021) reports that exceptions to the at-will employment doctrine result in non-financial firms increasing their cash holdings by 7.2%, with respect to the sample mean.

To reinforce our baseline results, we conduct a validity test of the *operational leverage* channel and perform a battery of robustness tests to address potential identification concerns. First, we examine whether banks subject to the good faith exception exhibit greater labour rigidity by testing whether employment adjustments become less sensitive to balance-sheet contractions following adoption. Consistent with the *operational leverage* channel, we find that banks exposed to the good faith exception exhibit significantly weaker employment responses to adverse balance-sheet shocks. In additional tests, we also show that earnings volatility increases following good faith adoption, providing further evidence that the *operational leverage* channel underpins, at least in part, the observed decline in banks' dividend payouts. Second, following Serfling (2016) and Shang and Saffar (2023), we estimate dynamic effects around the adoption of the good faith exception and confirm that dividend payouts exhibit no statistically significant pre-treatment differences, validating the parallel-trends assumption and alleviating reverse-causality concerns. Third, following Dang et al. (2021) and Chen et al. (2025), we test whether pre-treatment differences between treated and control bank characteristics determine our main findings. We construct both propensity score-matched and entropy-balanced samples and show that the estimated treatment effect remains robust after controlling for pre-treatment imbalances in bank- and state-level characteristics. Fourth, we implement time-series falsification tests by re-estimating our baseline specification using pseudo shocks dated one and 2 years prior to each state's respective good faith adoption. We find no evidence of anticipatory payout behaviour. Lastly, we address the possibility that contemporaneous regulatory changes confound our treatment estimates. Recent research has shown that changes in competition following the Interstate Banking and Branching Efficiency Act materially reduced banks' dividend payouts (e.g., De Cesari et al. 2024). After controlling for both inter- and intrastate bank deregulation, we demonstrate that our results are unaffected by concurrent changes in competition. Taken together, these additional tests mitigate concerns that our results are driven by violations of parallel trends, pre-treatment differences between treated and control banks, anticipatory behaviour, or concurrent changes in competition.

With our baseline results firmly established, we proceed by exploiting the cross-sectional heterogeneity in our sample and examine the drivers underpinning our main findings. Specifically, we test whether the effect of good faith adoption varies with banks' pre-treatment labour costs, financial leverage, business model, and charter value. If stronger employment protection and higher dismissal costs heighten operational rigidity, then banks with high pre-treatment operational costs and limited financial flexibility should adjust payouts more sharply (Simintzi et al. 2015; Dang et al. 2021). Similarly, banks more reliant on labour-intensive, customer-centred fee-generating activities have higher ex-ante operational leverage (DeYoung and Roland 2001), which may amplify their exposure to the effects of good faith adoption. Consistent with the *operational leverage* channel, we find that dividend reductions are concentrated among treated banks with high pre-treatment labour costs, financial leverage, and fee income, whereas banks with lower operational and financial rigidity exhibit no statistically significant reductions in payouts. By contrast, we find little evidence that the effect of good faith adoption differs meaningfully across banks with high and low charter value. Collectively, these findings suggest that stronger employment protection and higher firing costs increase operational rigidity and earnings volatility, leading banks with greater constraints to adopt more conservative dividend policies.

As a final test, we rule out the possibility that banks respond to stronger employment protection and higher firing costs through alternative risk-shifting channels. Although our main findings are consistent with the *operational leverage* channel, reductions in dividend payouts may simultaneously coincide with increases in asset-side risk-taking aimed at maximising the value of government guarantees (Keeley 1990; Acharya et al. 2017). To rule out this alternative explanation, we re-estimate our baseline specification using two accounting- and two market-based measures of bank risk. We find no significant evidence to suggest that the adoption of the good faith exception increases bank risk-taking.

Our paper makes three important contributions to the literature. First, we contribute to the growing literature on bank payout policy. Given the inherent opacity of banks, dividend payouts play a particularly important role because they convey information about financial strength and interact closely with banks' financial flexibility and risk-taking (Bessler and Nohel 1996; Floyd et al. 2015; Acharya et al. 2017). Accordingly, prior research shows that bank payout decisions are influenced by a range of factors, including competition (e.g., De Cesari et al. 2024), taxation (e.g., DeYoung and Jang 2023), asset and liability structure (e.g., Forti and Schiozer 2015), CEO incentives (e.g., Srivastav et al. 2014), ownership structure (e.g., Duqi et al. 2020), financial health (e.g., Tripathy et al. 2021), and payout externalities (e.g., Acharya et al. 2017). Distinct from this literature, we provide, to the best of our knowledge, the first causal evidence that employment protection and firing costs affect banks' dividend payouts. In doing so, we identify labour market frictions as an important and previously unexplored determinant of bank payout policy.

Second, we contribute to the literature on WDLs by examining the impact of good faith adoption on bank dividend payouts. A growing literature shows that the increase in operating leverage associated with WDL adoption leads non-financial firms to adopt more conservative financial policies. Prior studies document reductions in leverage (Simintzi et al. 2015; Serfling 2016) and investment (Bai et al. 2020), alongside increases in precautionary cash holdings (Beuselinck et al. 2021). More recent evidence further shows that WDL adoption reduces trade credit supply (Li et al. 2023) and constricts corporate diversification (Bu et al. 2025). Most closely related to our paper, Dang et al. (2021) show that non-financial firms increase share repurchases following good faith adoption, consistent with firms using flexible payout policies to mitigate employee rent extraction. In contrast, we show that banks reduce dividends following good faith adoption. This distinction is important because dividends are substantially more persistent and informationally sensitive than repurchases (Bessler and Nohel 1996; Floyd et al. 2015). Our findings therefore suggest that, unlike non-financial firms, banks respond to stronger employment protection through persistent reductions in dividends rather than through flexible share repurchases. Overall, we extend the WDL literature to financial institutions and show that stronger employment protection and increased firing costs negatively affect bank dividend payouts.

Lastly, we contribute to the literature on the conflicts of interest among different stakeholder groups and their influence on banks' corporate financial decisions. While prior research has largely focused on external stakeholders and regulation designed to protect depositors, creditors, or borrowers (e.g., Leung et al. 2019; Gao et al. 2021; Fuster et al. 2023; Chronopoulos et al. 2023), our paper examines how differences in employment protection and firing costs influence the distribution of corporate wealth among banks' shareholders and stakeholders. Specifically, our paper shows that stronger employment protection and higher firing costs, resulting from the adoption of the good faith exception, causes banks to reduce dividend payouts, thus curtailing shareholder wealth extraction and shifting value towards employees. Accordingly, we uncover a new previously unexplored channel through which employment protection shapes the allocation of corporate wealth between banks' shareholders and stakeholders.

2 | Empirical Design

2.1 | Wrongful Discharge Laws

To protect employees from unfair dismissal practices, in the 1970s many U.S. states began recognising legal exceptions to the at-will employment doctrine. These exceptions, or WDLs, gradually evolved into three distinct legal exceptions: the good faith exception, the implied contract exception, and the public policy exception. Among the three, the good faith exception represents the most significant and economically consequential departure from at-will employment (Kugler and Saint-Paul 2004). Specifically, the good faith exception increases the cost of employee termination and reduces workers' unemployment risk by requiring employers to treat employees fairly and in good faith (Autor et al. 2007; Serfling 2016). Under this exception, employers are prohibited from discharging workers out of malice, bad faith, or retaliation. For instance, terminations made to prevent employees from receiving vested pensions, bonuses, or commissions constitute violations of this rule. In its broadest interpretation, the good faith exception imposes a *just cause* standard on dismissal decisions, thereby expanding the circumstances under which employees may pursue legal action against wrongful termination claims (Dang et al. 2021). The implied contract exception, by contrast, protects employees when an employer has implicitly promised continued employment unless there is just cause for dismissal. However, the threat of implied contract lawsuits is regularly mitigated through the inclusion of at-will disclaimers in employee manuals (Autor et al. 2007). Lastly, the public policy exception bars employers from dismissing workers who refuse to violate the law or who act in accordance with public policy. This doctrine reflects the principle that employees should not be penalised for performing actions that serve the public interest, even when such actions conflict with the employer's preferences (Serfling 2016).

In this paper, we follow Acharya et al. (2014), Serfling (2016), and Bai et al. (2020), and focus on the good faith exception to examine how stronger employment protection and higher firing costs affect banks' dividend payouts. Prior research has shown that the adoption of the good faith exception has had wide-ranging effects beyond labour markets and has significantly influenced the financial policies and strategic decisions of non-financial firms. Empirical evidence documents that enhanced employee protection and increased dismissal costs following the adoption of good faith significantly affects non-financial firms' capital structure (e.g., Serfling 2016), cash holdings (e.g., Beuselinck et al. 2021), share repurchases (e.g., Dang et al. 2021), research and innovation (e.g., Acharya et al. 2014; Bena et al. 2022), and tax aggressiveness (e.g., Fairhurst et al. 2020). Despite extensive research on the effects of employment protection laws on non-financial firms, relatively little is known about their implications for financial institutions, particularly banks. With the exception of Bird and Knopf (2009), who show that WDLs are significantly and positively associated with banks' labour input costs, the literature provides limited insight into how the adoption of good faith exceptions influences bank dividend payout policies and the conflict of interest between banks' stakeholders and shareholders. Our paper fills this gap in the literature.

2.2 | Sample and Summary Statistics

To conduct our empirical analysis, we draw from several data sets. First, we collect state-level adoption data of WDLs from Serfling (2016). We follow the literature (e.g., Bai et al. 2020; Karpuz et al. 2023) and build our initial data set for the period spanning 1969 to 2003. We obtain financial data for publicly listed U.S. banks from Compustat and stock market data are obtained from CRSP. We use the Compustat state abbreviation (the variable 'state') to locate each bank's headquarters (Serfling 2016; Bai et al. 2020; Karpuz et al. 2023). To control for local economic activity and political conditions, we supplement our data set with state-level GDP growth from the Bureau of Economic Analysis and data on state-level members in the House of Representatives and Senate from the U.S. Census Bureau.

The staggered adoption of the good faith exception across U.S. states provides plausibly exogenous variation in employment protection which we exploit to identify its causal impact on banks' dividend payouts. Some states, such as California and Massachusetts, recognise all three exceptions while others have adopted one or two, and a few states, such as Florida, Georgia, and Rhode Island, continue to adhere to pure at-will employment with no exceptions. In Figure 1, we show the number of states that have adopted the good faith, implied contract, and public policy exceptions to the traditional employment at-will rule between 1969 and 2003.

To address potential concerns related to forbidden comparisons, we follow Gormley and Matsa (2011), Gormley et al. (2013), and Duchin et al. (2025), and implement a stacked difference-in-differences framework centered on each state's adoption year of the good faith exception. Specifically, we partition the initial data set into cohorts based on each state's adoption year and construct a 9-year event window (−4; +4) around the adoption date[4]. We define all good faith adoptions in a given year as cohort c.[5] Following previous literature (e.g., Acharya et al. 2014; Serfling 2016; Dang et al. 2021), we use headquarters location to identify treated banks. For each cohort, banks headquartered in adopting states are designated as the treatment group, while those in states that never adopt a good faith exception serve as the control group. Until the late 1980s and, in particular, prior to the Riegle-Neal Interstate Banking and Branching Efficiency Act of 1994, significant restrictions on interstate branching were in place, substantially limiting banks' ability to operate across states (Strahan 2003). Therefore, banks' operations were mostly confined to headquarters states, creating de facto state-level banking systems (Morgan 2002). As much of our sample predates the 1994 deregulation, headquarters location is likely to reflect the geography of banks' operations in most of our sample.[6] We retain only banks with non-missing data over the full event window to ensure consistent coverage of pre- and post-treatment variables[7]. We winsorise all continuous variables at the 2nd and 98th percentiles to mitigate the influence of outliers, following Chronopoulos et al. (2023). Our final sample consists of 364 unique banks (25 treated banks and 339 control banks) and 5931 stacked cohort-bank-time observations over the period 1973–2002.

Table 1 presents the main summary statistics and Figure 2 plots the average dividend payouts for treated and non-treated banks over event-time. Consistent with prior studies (e.g., De Cesari et al. 2024; Cziraki et al. 2024), the mean dividend payout ratio in our sample, measured as dividends to the book value of

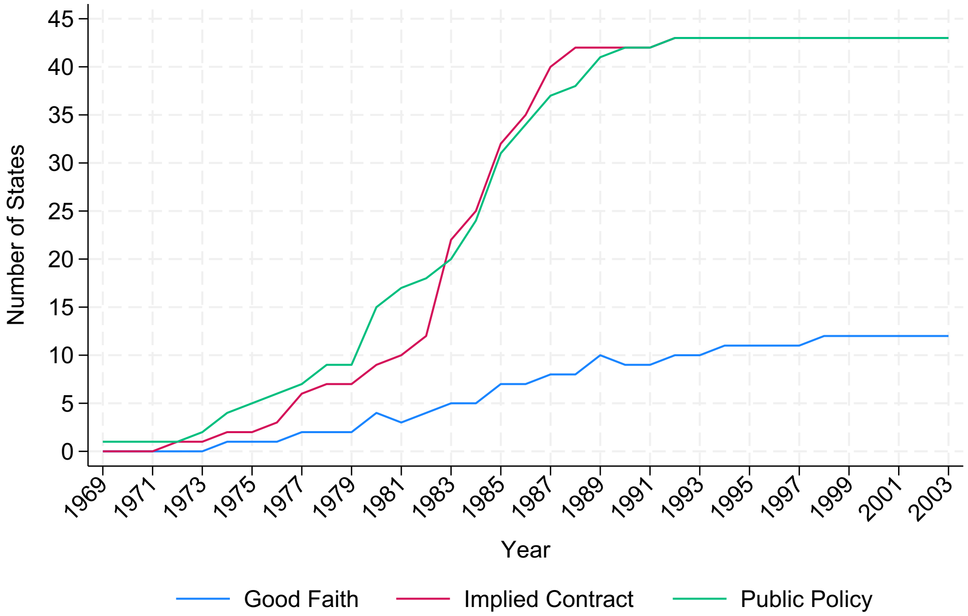

FIGURE 1 | Number of states adopting wrongful discharge laws. *Note:* This figure shows the number of states that have adopted the good faith, implied contract, and public policy exceptions to the traditional employment at-will rule in each year between 1969 and 2003. The exact states and years of adoption are detailed in Internet Table S1. [Color figure can be viewed at wileyonlinelibrary.com]

TABLE 1 | Main sample summary statistics.

Panel A: Dependent variable and policy indicators	**Treated banks**				**Non-treated banks**			
	Mean	**SD**	**5th Perc.**	**95th Perc.**	**Mean**	**SD**	**5th Perc.**	**95th Perc.**
Dividend payout	4.282	1.509	0.749	6.638	4.296	1.759	0.960	7.061
Good faith	0.556	0.498	0.000	1.000	0.000	0.000	0.000	0.000
Implied contract	0.542	0.499	0.000	1.000	0.559	0.497	0.000	1.000
Public policy	0.533	0.500	0.000	1.000	0.596	0.491	0.000	1.000
Panel B: Bank and state controls								
Charter value	1.115	0.067	1.055	1.275	1.170	0.085	1.070	1.336
Bank size	8.028	1.255	6.528	10.112	7.561	1.461	5.184	10.180
Capital	0.055	0.019	0.032	0.094	0.072	0.025	0.041	0.116
Profitability	0.115	0.101	−0.017	0.204	0.119	0.066	0.037	0.184
Loan loss provisions	0.008	0.009	0.001	0.023	0.006	0.006	0.000	0.016
Deposit ratio	0.775	0.063	0.665	0.890	0.760	0.087	0.595	0.879
Cash holdings	0.175	0.082	0.062	0.325	0.131	0.087	0.023	0.296
Retained earnings	0.569	0.246	0.173	0.882	0.547	0.215	0.164	0.871
Dividend payer	0.960	0.196	1.000	1.000	0.965	0.183	1.000	1.000
GDP growth	0.089	0.044	0.010	0.146	0.072	0.035	0.026	0.137
Political balance	0.638	0.162	0.310	0.815	0.602	0.141	0.400	0.881

Note: This table reports the summary statistics for the main variables used in the analysis, presented separately for banks headquartered in states that adopted the good-faith exception (Treated) and those headquartered in states that did not (Non-Treated). The sample includes 364 U.S. banks from 1973 to 2002, for a total of 5272 stacked cohort-bank-time observations. Panel A reports the summary statistics for the main dependent variable and policy indicators. Panel B reports the summary statistics for bank- and state-specific control variables. See Internet Table S2 for variable definitions and construction.

equity, is 4.282 for treated banks and 4.296 for non-treated banks. Figure 2 shows that pre-treatment payout trends are similar between groups; however, following the adoption of the good faith exemption, treated banks exhibit sizeable reductions in their dividend payouts. Importantly, Table 1 shows that the adoption rates of implied contract and public policy exemptions differ only marginally between our treated and control group. This similarity is reassuring, as it suggests that the employee-related conditions of treated and control banks are broadly comparable prior to the adoption of the good faith exemption. Regarding bank and state-level characteristics, treated banks tend to be slightly larger, with lower charter values and capital

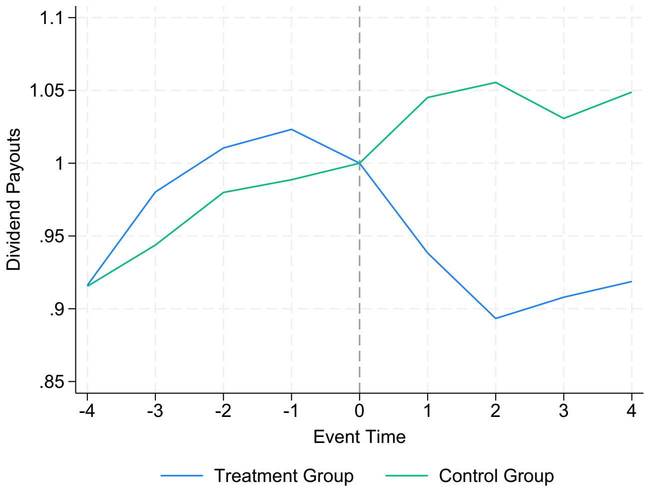

FIGURE 2 | Dividend payouts over event time. *Note:* The figure presents the average dividend payouts over event-time for treated and non-treated banks. Dividend payouts are normalised by their respective means at event time zero. [Color figure can be viewed at wileyonlinelibrary.com]

ratios. They are also located in states that are, on average, more democratic party leaning and exhibit higher GDP growth[8]. Overall, our summary statistics are consistent with the literature.

2.3 | Empirical Specification

To formally examine how good faith adoption affects banks' dividend payouts, we follow Serfling (2016) and Dang et al. (2021) and estimate the following specification:

$$\begin{aligned}\text{Dividend Payout}_{\text{b, s, c, t}} = {} & \beta_1 \text{Good Faith}_{\text{s, c, t}} \\ & + \beta_2 \text{IC}_{\text{s, c, t}} + \beta_3 \text{PP}_{\text{s, c, t}} \\ & + \delta' \text{X}_{\text{b, s, c, t}-1} + \omega_{\text{c, b}} \\ & + \eta_{\text{c, t}} + \varepsilon_{\text{b, s, c, t}}\end{aligned} \tag{1}$$

where subscripts $b, s, c,$ and t denote bank, state, cohort, and time, respectively. The dependent variable, *Dividend Payout*$_{b,s,c,t}$, equals dividends divided by the book value of equity. The key explanatory variable, *Good Faith*$_{s,c,t}$, is an indicator equal to one if a bank's headquarters state, s, has adopted the Good Faith exception by year t, and zero otherwise. The coefficient β_1 thus captures the average effect of Good Faith adoption on banks' dividend payouts. We also include indicators for the coexistence of the implied contract, $IC_{s,c,t}$, and public policy, $PP_{s,c,t}$, exceptions to control for potential confounding effects associated with the adoption of these two laws [9].

Consistent with the literature on bank dividend payouts (e.g., Srivastav et al. 2014; Onali et al. 2016; De Cesari et al. 2024; Cziraki et al. 2024), we include a comprehensive set of bank- and state-level control variables, $X_{b,s,c,t-1}$, which we lag by one time period. These include *Charter Value*, *Bank Size*, *Capital*, *Profitability*, *Loan Loss Provisions*, *Deposit Ratio*, *Cash Holdings*, *Retained Earnings*, and an indicator variable, *Dividend Payer*, equal to one if the bank paid dividends in the previous period, *GDP Growth*, and *Political Balance*. Section 2.4 discusses the economic rationale for the inclusion of the control variables and their expected effects on dividend payout decisions. Lastly, following Gormley and Matsa (2011), we include cohort x banks, $\omega_{c,b}$, and cohort x year, $\eta_{c,t}$, fixed effects to account for cohort-specific time-invariant unobserved bank characteristics and cohort-specific time-varying shocks that may jointly influence both bank dividend payouts and the likelihood of good faith adoption.

All regressions employ heteroskedasticity-robust standard errors clustered at the state level, consistent with the level of variation in the good faith exception. This clustering approach accounts for potential within-state correlations in unobserved shocks over time. In Internet Table S10, we assess the robustness of our results to alternative standard errors, including heteroskedasticity robust, time-clustered, state-time-clustered, and two-way state and time-clustered standard errors. Our findings remain qualitatively unchanged across these different approaches.

2.4 | Control Variables

In this subsection, we discuss the economic rationale for our control variables and their expected effects on banks' dividend payouts.[10] Prior empirical research identifies charter value as a key determinant of bank payouts (e.g., De Cesari et al. 2024; Chronopoulos et al. 2023), although theoretical predictions about its effects are a priori ambiguous. Under the signalling view, banks with higher franchise value may pay higher dividends to convey private information about future performance (Bhattacharya 1979; John and Williams 1985; Miller and Rock 1985). By contrast, risk-shifting theories predict that banks with low charter value have stronger incentives to shift risk from shareholders to debtholders and taxpayers via payouts, as lower expected losses increase incentives to exploit government guarantees (Keeley 1990; Acharya et al. 2017).

Size, capital, and profitability are standard bank-specific characteristics that systematically affect payout decisions (e.g., Abreu and Gulamhussen 2013; Chronopoulos et al. 2023; Güntay et al. 2024). The value of payouts as a disciplinary mechanism is particularly relevant for larger banks, which are typically more complex and opaquer and thus have greater incentives to distribute cash to shareholders to mitigate agency conflicts. Moreover, their better access to external capital enables higher flexibility to sustain payouts (Abreu and Gulamhussen 2013). Similarly, better capitalised banks have greater flexibility in distributing cash relative to weakly capitalised banks, whose incentive to shift risk to debtholders may be significantly reduced by binding regulatory constraints (Onali 2014). In a similar vein, more profitable banks are expected to have higher payouts, as sustained profitability enhances the credibility of dividend commitments and supports dividend smoothing (Lintner 1956).

Loan loss provisions capture banks' forward-looking expectations of credit losses. Lu and Nikolaev (2022) document that provisions significantly affect banks' earnings, capital, and payout decisions, as higher expected losses reduce cash flows and capital buffers. Accordingly, higher provisions are expected to be associated with more conservative payouts. Dividend policies may also depend on banks' funding structure and liquidity positions. Depositors may discipline weaker banks from distributing internal funds to shareholders, while motivating financially healthier banks to signal soundness through higher payouts (Alhalabi et al. 2023). By contrast, the expected effect of cash holdings is ambiguous. While excess liquidity may reflect limited investment opportunities and exacerbate agency conflicts, prompting banks to distribute the excessive cash (Jensen 1986), it may also represent precautionary buffers to reduce future liquidity tensions, implying more conservative policies (Jensen and Meckling 1976).

Retained earnings are included to account for banks' life-cycle stage, as mature banks have accumulated larger buffers of internally generated capital and thus have greater payout capacity (Onali et al. 2016). Dividend payer controls for the persistence of payout behaviours, reflecting banks' tendency to smooth dividends over time (Lintner 1956; Acharya et al. 2017).

State-level GDP growth and Democratic representation in a state's legislature are introduced to control for local economic and political conditions. Dividend policies are sensitive to economic conditions, as downturns increase expected losses and tighten capital constraints (e.g., Acharya et al. 2017; Güntay et al. 2024). We therefore control for GDP growth to mitigate concerns that local economic conditions spuriously drive our results (Bai et al. 2020). Similarly, payout decisions are not neutral to the political context. On the one hand, banks may strategically increase payouts in Democratic political environments to limit exposure to politically motivated lending (Huang and Thakor 2024). On the other hand, greater regulatory scrutiny in these environments may motivate capital retention through lower payouts (Fahlenbrach et al. 2024). These competing channels predict opposite effects of political conditions on dividend payouts.

3 | Main Results

3.1 | Baseline Results

In Panel A of Table 2, we present the results from estimates of Equation (1). Column (1) reports the results for a restricted specification, where we only control for bank controls, cohort-bank- and cohort-time fixed effects. We incrementally add WDLs and state-level controls covariates in columns (2) to (5) to control for local time-varying factors affecting banks' payouts. Our baseline results in column (5) show greater employment protection and higher firing costs via the adoption of the good faith exception have a negative and statistically significant impact on banks' dividend payout policy. Specifically, we find the adoption of good faith causes, on average, a 7.0% (= 0.310/4.409) reduction in banks' dividend payout policy relative to the treated group's pre-treatment mean. These findings are consistent with the *operational leverage* channel, which posits that stronger employment protection reduces banks' operating flexibility, leading them to cut dividends and adopt more conservative payout policies in order to avoid costly future dividend reductions (e.g., Healy and Palepu 1988; Bessler and Nohel 1996).

Our results are consistent with previous papers showing that higher employment protection is associated with more conservative financial policies in non-financial firms (Simintzi et al. 2015; Serfling 2016; Beuselinck et al. 2021). The economic effect of the adoption of WDLs on banks' payout is comparable to those reported by Beuselinck et al. (2021), who find that the adoption of WDLs is associated with an average increase of 7.2% in cash holding by non-financial firms, and Li et al. (2023), who show that firms reduce the supply of trade credit by 6.5%, relative to the sample mean. Our findings, however, contrast with those of Dang et al. (2021), who show that non-financial firms increase share repurchases following the adoption of the good faith exception. A key reason for this divergence is the relevance of labour cost flexibility in banking. Personnel expenses represent the largest component of non-interest costs and, given banks' limited discretion over funding costs, are one of the few margins through which managers can adjust cash flows and profitability (Pinnuck and Lillis 2007; Hall 2016). Persistent increases to employment protection and dismissal costs therefore bind more tightly for banks than for non-financial firms, materially restricting operating flexibility. In addition, bank capital is particularly important for financial stability and therefore attracts closer supervisory oversight, making equity preservation considerably more important (Abreu and Gulamhussen 2013; Onali 2014; Floyd et al. 2015). These operational and regulatory differences help explain why banks reduce payouts following persistent shocks to employment protection and firing costs.[11]

In line with prior studies (e.g., Bai et al. 2020), we find the coefficients on the other two WDLs, *Implied Contract* and *Public Policy*, are small and not statistically significant. The estimated coefficients of other control variables are also mostly in line with previous work on banks' dividend payouts (De Cesari et al. 2024). Specifically, we find that charter value, size, profitability, retained earnings, dividend payer, and GDP per capita are positively related to banks' payouts. In contrast, we find that banks less reliant on deposits have higher payouts.

In Panel B of Table 2, we test the sensitivity of our results to alternative model specification. First, we re-estimate our baseline regression using alternative event windows. For each cohort, we consider three event windows—[−1; +1], [−2; +2], and [−3; +3]—corresponding to one-, two-, and 3-year periods around the adoption of good faith. The results, shown in columns (1) to (3), are consistent with our baseline findings. Second, to mitigate concerns that serial correlation could lead to downward-biased standard errors, we follow Bertrand et al. (2004) and collapse the data into pre- and post-event averages. The results, reported in column (4), corroborate our main findings.

3.2 | Mechanism Validation and Robustness Tests

In this subsection, we conduct a mechanism validity test and a series of robustness tests to further examine our baseline results and address potential identification concerns. First, given that our baseline results are consistent with the *operational leverage* channel, we assess the validity of this mechanism by testing whether banks subject to the good faith exception exhibit weaker employment adjustments during periods of balance-sheet contraction. Second, we examine threats related to violations of the parallel-trends assumption by estimating the

TABLE 2 | The impact of good faith adoption on dividend payouts.

Panel A: Baseline Results	(1)	(2)	(3)	(4)	(5)
Good Faith $_t$	−0.312***	−0.303***	−0.319***	−0.310***	−0.310***
	(0.096)	(0.100)	(0.097)	(0.102)	(0.102)
Implied Contract $_t$			−0.060	−0.061	−0.055
			(0.101)	(0.100)	(0.106)
Public *Policy* $_t$		−0.043		−0.045	−0.035
		(0.128)		(0.128)	(0.126)
Bank Controls					
Charter Value $_{t-1}$	0.501***	0.501***	0.501***	0.501***	0.488***
	(0.087)	(0.087)	(0.087)	(0.087)	(0.087)
Bank Size $_{t-1}$	1.099***	1.100***	1.097***	1.098***	1.097***
	(0.267)	(0.266)	(0.266)	(0.265)	(0.272)
Capital $_{t-1}$	−0.102	−0.102	−0.102	−0.102	−0.094
	(0.081)	(0.081)	(0.081)	(0.081)	(0.080)
Profitability $_{t-1}$	0.112***	0.112***	0.112***	0.112***	0.110***
	(0.037)	(0.037)	(0.037)	(0.037)	(0.037)
Loan Loss Provisions $_{t-1}$	−0.042	−0.042	−0.042	−0.042	−0.036
	(0.057)	(0.057)	(0.057)	(0.057)	(0.055)
Deposit Ratio $_{t-1}$	−0.195***	−0.194***	−0.195***	−0.194***	−0.195***
	(0.070)	(0.070)	(0.070)	(0.070)	(0.072)
Cash Holdings $_{t-1}$	0.050	0.050	0.049	0.049	0.043
	(0.074)	(0.074)	(0.073)	(0.073)	(0.072)
Retained Earnings $_{t-1}$	0.096*	0.097*	0.097*	0.097*	0.093*
	(0.049)	(0.049)	(0.049)	(0.049)	(0.050)
Dividend Payer $_{t-1}$	1.693***	1.692***	1.695***	1.694***	1.694***
	(0.144)	(0.143)	(0.144)	(0.143)	(0.144)
State Controls					
GDP Growth $_{t-1}$					0.070***
					(0.025)
Political Balance $_{t-1}$					0.021
					(0.074)
Cohort-bank Fixed Effects	Yes	Yes	Yes	Yes	Yes
Cohort-time Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	5,272	5,272	5,272	5,272	5,272
Adjusted R^2	0.779	0.779	0.779	0.779	0.779

Panel B: Robustness Tests	Window: [−1:+1] (1)	Window: [−2:+2] (2)	Window: [−3:+3] (3)	Two-period Model: Bertrand et al. (2004) (4)
Good Faith $_t$	−0.314***	−0.287***	−0.293***	−0.529**
	(0.072)	(0.090)	(0.098)	(0.232)
All Controls	Yes	Yes	Yes	Yes
Cohort-bank Fixed Effects	Yes	Yes	Yes	Yes
Cohort-time Fixed Effects	Yes	Yes	Yes	No

(Continues)

TABLE 2 | (Continued)

Panel B: Robustness Tests

	Window: [−1:+1] (1)	Window: [−2:+2] (2)	Window: [−3:+3] (3)	Two-period Model: Bertrand et al. (2004) (4)
Observations	1,977	3,295	4,613	1,318
Adjusted R^2	0.828	0.799	0.780	0.734

Note: This table presents estimates of the effect of *Good Faith* on *Dividend Payout* using the stacked Difference-in-Differences model from Equation 1. Panel A reports baseline results using alternative model specifications with a [−4, +4] event window. Panel B presents robustness tests based on different event windows and an alternative two-period model following Bertrand et al. (2004). WDLs controls include indicator variables for *Implied Contract* and *Public Policy* exceptions. Bank controls include *Charter Value*, *Bank Size*, *Capital*, *Profitability*, *Loan Loss Provisions*, *Deposit Ratio*, *Cash Holdings*, *Retained Earnings*, and *Dividend Payer*. State controls include *GDP Growth* and *Political Balance*. Bank- and state-level controls are lagged by one year. All columns include cohort-bank and cohort-time fixed effects. Standard errors are robust to heteroskedasticity and within-state dependence, except for Column (4) in Panel B, and are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. See Internet Table S2 for variable definitions and construction.

dynamic effects of good faith adoption. This allows us to test whether banks in states adopting the good faith exception exhibit payout policies comparable to those headquartered in non-adopting states. Next, we evaluate the sensitivity of our results to pre-treatment differences in bank- and state-level characteristics, potential anticipatory behaviour, and omitted factors associated with shifts in local bank competition. Finally, we implement several supplementary tests to further validate the robustness of our findings.

3.2.1 | *Operational Leverage Validity Test*

To ensure that the *operational leverage* channel underpins our baseline results, we directly test whether the adoption of the good faith exception increases labour rigidity. Prior studies show that stronger employment protection dampens firms' employment responses to adverse shocks (e.g., Messina and Vallanti 2007; Kugler and Pica 2008). If stronger employment protection increases dismissal costs, banks should face greater difficulty adjusting their workforce in response to changing economic conditions. The *operational leverage* channel predicts that the resulting increase in operational rigidity and earnings volatility causes banks to adopt more conservative payout policies in order to preserve financial flexibility and avoid further costly future dividend reductions (e.g., Healy and Palepu 1988; Bessler and Nohel 1996).

To examine the operational leverage channel directly, we exploit the fact that banks typically respond to adverse shocks by reducing lending and contracting balance-sheet size, rather than raising external equity in the short run due to the associated issuance costs (e.g., Peek and Rosengren 2000; Gropp et al. 2019). Accordingly, we use declines in total assets, defined as negative year-to-year changes in total assets, as a proxy for contractions in banks' operations that are likely to require workforce adjustments. To examine the effect of the good faith exception on banks' labour adjustments, we estimate the following specification:

$$\begin{aligned} Employment_{b,s,c,t} = \beta_1\, Good\ Faith_{s,c,t} &+ \beta_2\, Asset\ Decline_{b,s,c,t} \\ &+ \beta_3 (Good\ Faith_{s,c,t} \cdot Asset\ Decline_{b,s,c,t}) \\ &+ \beta_4\, IC_{s,c,t} \\ &+ \beta_5\, PP_{s,c,t} + \delta' X_{b,s,c,t-1} + \omega_{c,b} + \eta_{c,t} \\ &+ \varepsilon_{b,s,c,t} \end{aligned} \tag{2}$$

where subscripts b, s, c, and t denote bank, state, cohort, and time, respectively. The dependent variable $Employment_{b,s,c,t}$ is the annual growth rate of the number of employees ($Employment\ Growth_{b,s,c,t}$), or is a dummy variable equal to one if bank i experiences a reduction in the number of employees in year t, and zero otherwise ($Employment\ Decline_{b,s,c,t}$). $Asset\ Decline_{b,s,c,t}$ is a dummy variable equal to one if bank i's total assets decline from year $t-1$ to t. All other variables are defined as in Equation (1). We include indicators for the coexistence of the implied contract and public policy exceptions, the full set of bank- and state-level controls, and cohort-bank and cohort-time fixed effects. The coefficient of interest is β_3, which captures the differential sensitivity of employment adjustments to asset contractions following the adoption of the good faith exception. If the *operational leverage* channel underpins our baseline results, we expect β_3 to be positive in the $Employment\ Growth_{b,s,c,t}$ specification and negative in the $Employment\ Decline_{b,s,c,t}$ specification, indicating that stronger employment protection and increased firing costs reduces banks' ability to adjust labour in response to balance-sheet contractions.

Table 3 reports the results. Column (1) presents estimates for the employment growth rate, while column (2) reports estimates for the likelihood of an employment decline. In both specifications, the coefficient on $Good\ Faith_{s,c,t}$ is small and statistically insignificant, suggesting that stronger employment protection does not affect banks' employment in the absence of negative shocks. By contrast, the coefficient on $Asset\ Decline_{b,s,c,t}$ is negative in column (1) and positive in column (2), indicating that contractions in banks' operations are associated with lower employment growth and a higher likelihood of employment declines. The coefficients on the interaction terms, however, indicate that these effects are significantly weaker for banks subject to the good faith exception. Overall, these results support the hypothesis that stronger employment protection increases labour rigidity by reducing banks' ability to adjust their workforce in response to balance-sheet shocks.

Furthermore, in Internet Appendix Table 4, we provide an additional test of the *operational leverage* channel by examining whether the adoption of the good faith exception increases earnings volatility. Specifically, we calculate the four-period standard deviation of profitability over the pre- and post-adoption windows and estimate a specification analogous to Equation (1) using two-period event windows. The results

TABLE 3 | The impact of good faith adoption on employment.

	Employment growth (1)	Employment decline (2)
Good Faith $_t$	−0.016	−0.085
	(0.018)	(0.097)
Asset Decline $_t$	−0.056***	0.237***
	(0.007)	(0.035)
Good Faith$_t$ · Asset Decline $_t$	0.042**	−0.251***
	(0.021)	(0.084)
All Controls	Yes	Yes
Cohort-bank Fixed Effects	Yes	Yes
Cohort-time Fixed Effects	Yes	Yes
Observations	4,241	4,241
R-squared	0.311	0.327

Note: This table presents the validation test of the effect of *Good Faith* on employment rigidity. Column (1) and (2) report estimates for *Employment Growth* and Employment Decline, respectively. *Asset Decline* is an indicator variable equal to one if a bank's total assets decline from year $t-1$ to t. WDLs controls include indicator variables for *Implied Contract* and *Public Policy* exceptions. Bank controls include *Charter Value*, *Bank Size*, *Capital*, *Profitability*, *Loan Loss Provisions*, *Deposit Ratio*, *Cash Holdings*, *Retained Earnings*, and *Dividend Payer*. State controls include *GDP Growth* and *Political Balance*. Bank- and state-level controls are lagged by one year. All columns include cohort-bank and cohort-time fixed effects. Standard errors are robust to heteroskedasticity and within-state dependence and are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. See Internet Appendix Table 2 for variable definitions and construction.

indicate that earnings volatility increases following the adoption of the good faith exception. Taken together, these findings provide supporting evidence that the *operational leverage* channel underpins our baseline results.

3.2.2 | Dynamic Treatment Effects

A key identification assumption for the validity of the difference-in-differences analysis is that treatment and control units exhibit parallel trends prior to the good faith adoption. To verify that our findings are not driven by systematic pre-trend differences or reverse causality, we follow Serfling (2016) and Dang et al. (2021) and estimate a dynamic specification of Equation (1). Specifically, we replace the treatment variable, *Good Faith*, with a set of time dummies that take the value of one for treated banks from 3 years before ($GoodFaith_{t-3}$) to 4 years after ($GoodFaith_{t+4}$) the introduction of the good faith exception, and zero otherwise. We expect the coefficients associated with the pre-treated period to be statistically insignificant and those following the adoption to be negative and significant.

Table 4 reports the estimates, and Figure 3 presents the corresponding event-study plot. Consistent with the parallel trends assumption, the pre-adoption coefficients (*Good Faith* $t-3$ and *Good Faith* $t-2$) are small and statistically insignificant across specifications. Furthermore, joint tests of the pre-event coefficients fail to reject the null hypothesis of no differential pre-trends between treated and control banks. Collectively, these results indicate the absence of pre-trends in dividend payout behaviour, alleviating concerns about reverse causality, and demonstrate a persistent negative effect of good faith on banks' dividend payouts for up to 4 years following the adoption.

3.2.3 | Alternative Control Groups

Second, we conduct two additional tests to examine whether structural differences in bank and state characteristics between treated and control banks could be affecting our results. First, drawing from prior literature (e.g., Serfling 2016; Bai et al. 2020; Dang et al. 2021), we perform propensity score matching (PSM) to identify a group of banks headquartered in non-adopting states comparable to the group of treated banks and re-estimate Equation (1) using the matched sample. Specifically, in the first stage of the PSM, we use pre-treatment observations to estimate a probit model that predicts the ex-ante probability of being treated based on the vector of bank- and state-specific control variables used in the baseline model. We then use the propensity scores and match, with replacement, each treated bank with up to three comparable control banks, imposing a 0.05 caliper.

Second, we use entropy balancing (EB) to further mitigate concerns about covariate imbalance. EB re-weights observations in the control group to achieve a balance in the distribution moments of covariates between treated and control banks (Hainmueller 2012). In contrast to PSM, this technique retains all sample observations and is less sensitive to matching parameters. We perform EB by matching the first and second moments of the covariates included in the baseline analysis, using pre-treatment observations. We then apply the average of the pre-treatment weights in the subsequent analysis. Table 5 reports the estimates of Equation (1) using the PSM-matched sample (column 1) and the EB-re-weighted sample (column 2). Consistent with our baseline results, *Good Faith* remains negative and highly significant, confirming the robustness of our findings.

3.2.4 | Falsification Tests

Next, we address the possibility that dividend payouts in treated banks adjusted prior to the adoption of Good Faith exception, which could bias our estimates if banks anticipated stricter employment protections or if other unobserved factors affected payouts. To examine this, we follow Cornaggia et al. (2015) and Srivastav and Vallascas (2022) and conduct a falsification test by

TABLE 4 | The impact of good faith adoption on dividend payouts : Dynamic treatment effects.

	(1)	(2)
Good Faith $_{t-3}$	−0.014	−0.035
	(0.119)	(0.112)
Good Faith $_{t-2}$	−0.044	−0.095
	(0.096)	(0.109)
Good Faith $_{t}$	−0.077	−0.090
	(0.077)	(0.073)
Good Faith $_{t+1}$	−0.445***	−0.463***
	(0.125)	(0.139)
Good Faith $_{t+2}$	−0.463***	−0.474***
	(0.115)	(0.123)
Good Faith $_{t+3}$	−0.301**	−0.334**
	(0.139)	(0.156)
Good Faith $_{t+4}$	−0.370*	−0.412*
	(0.208)	(0.214)
Bank Controls	Yes	Yes
Implied Contract	No	Yes
Public Policy	No	Yes
State Controls	No	Yes
Cohort-bank Fixed Effects	Yes	Yes
Cohort-time Fixed Effects	Yes	Yes
Pre-trend test	0.78	0.49
Observations	5,272	5,272
Adjusted R^2	0.779	0.778

Note: This table presents estimates of the dynamic effect of *Good Faith* on *Dividend Payout*. $GoodFaith_{t-3}$, $GoodFaith_{t-2}$, $GoodFaith_{t}$, $GoodFaith_{t+1}$, $GoodFaith_{t+2}$, $GoodFaith_{t+3}$, and $GoodFaith_{t+4}$ are time indicators that equal one for treated banks from three years before ($GoodFaith_{t-3}$) to four years after ($GoodFaith_{t+4}$) the adoption of the good faith exception, and zero otherwise. WDLs controls include indicator variables for *Implied Contract* and *Public Policy* exceptions. Bank controls include *Charter Value*, *Bank Size*, *Capital*, *Profitability*, *Loan Loss Provisions*, *Deposit Ratio*, *Cash Holdings*, *Retained Earnings*, and *Dividend Payer*. State controls include *GDP Growth* and *Political Balance*. Bank- and state-level controls are lagged by one year. All columns include cohort-bank and cohort-time fixed effects. Standard errors are robust to heteroskedasticity and within-state dependence and are reported in parentheses. Pre-trend test reports the p-value for a joint hypothesis test that all pre-treatment event-time coefficients are equal to zero. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. See Internet Table S2 for variable definitions and construction.

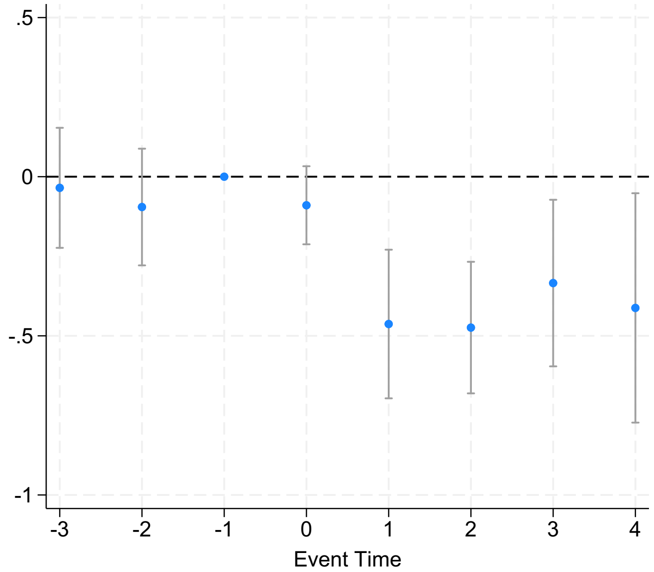

FIGURE 3 | Dynamic treatment effect estimates of good faith adoption on bank payout policy. *Note:* The figure presents the point estimates and corresponding standard errors from the dynamic treatment effect estimation reported in Column (2) of Table 4. The estimation includes all bank controls, state controls, Cohort-bank fixed effects, and Cohort-time fixed effects. Sample banks and variable summary statistics are reported in Table 1. Variable definitions are provided in Internet Table S2. [Color figure can be viewed at wileyonlinelibrary.com]

TABLE 5 | The impact of good faith adoption on dividend payouts : Alternative control groups.

	Propensity score matched Sample (1)	Entropy balance Sample (2)
Good Faith$_t$	−0.299***	−0.253***
	(0.107)	(0.094)
All Controls	Yes	Yes
Cohort-bank Fixed Effects	Yes	Yes
Cohort-time Fixed Effects	Yes	Yes
Observations	1,424	5,272
Adjusted R^2	0.761	0.808

Note: This table presents estimates of the effect of *Good Faith* on *Dividend Payout* using alternative samples. Column (1) reports estimates based on a propensity score-matched sample. Treated banks are matched to up to three non-treated banks using pre-treatment covariates. Column (2) reports estimates based on an entropy-balanced sample constructed by matching the first and second moments of the covariates' distribution. WDLs controls include indicator variables for *Implied Contract* and *Public Policy* exceptions. Bank controls include *Charter Value*, *Bank Size*, *Capital*, *Profitability*, *Loan Loss Provisions*, *Deposit Ratio*, *Cash Holdings*, *Retained Earnings*, and *Dividend Payer*. State controls include *GDP Growth* and *Political Balance*. Bank- and state-level controls are lagged by one year. All columns include cohort-bank and cohort-time fixed effects. Standard errors are robust to heteroskedasticity and within-state dependence and are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels. See Internet Table S2 for variable definitions and construction.

artificially shifting the adoption date one and 2 years earlier, re-estimating our baseline model using these placebo treatment dates. Table 6 reports the results. Across all specifications, the estimated coefficients on these placebo treatments are statistically insignificant, suggesting the absence of anticipatory effects. These findings reinforce the validity of the parallel trends assumption and provide additional evidence that the observed reduction in dividend payouts is determined by the adoption of the good faith exception.

3.2.5 | *Coinciding State Regulation: Intrastate and Interstate Banking Reform*

As a final identification test, we rule out possible coinciding effects attributable to non-employment-related regulations, specifically the relaxation of intrastate and interstate banking restrictions. Before these reforms, banks were subject to significant constraints on their geographical expansion both within and across state borders. From the 1970s to the mid-1990s, however, the U.S. banking system experienced a substantial liberalisation, driven primarily by these intrastate

TABLE 6 | The impact of good faith adoption on dividend payouts : Falsification test.

	Psuedo time shock ($t-1$) **(1)**	**Psuedo ime shock ($t-2$)** **(2)**
Good Faith$_t$	−0.042	0.155
	(0.133)	(0.134)
All Controls	Yes	Yes
Cohort-bank Fixed Effects	Yes	Yes
Cohort-time Fixed Effects	Yes	Yes
Observations	6,336	4,896
Adjusted R^2	0.764	0.762

Note: This table presents estimates from falsification tests. Column (1) reports estimates where *Good Faith* equals one from one year before its adoption date, and 0 otherwise. Column (2) reports estimates where *Good Faith* equals one from two years before its adoption date, and zero otherwise. WDLs controls include indicator variables for *Implied Contract* and *Public Policy* exceptions. Bank controls include *Charter Value*, *Bank Size*, *Capital*, *Profitability*, *Loan Loss Provisions*, *Deposit Ratio*, *Cash Holdings*, *Retained Earnings*, and *Dividend Payer*. State controls include *GDP Growth* and *Political Balance*. Bank- and state-level controls are lagged by one year. All columns include cohort-bank and cohort-time fixed effects. Standard errors are robust to heteroskedasticity and within-state dependence and are reported in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% levels, respectively. See Internet Table S2 for variable definitions and construction.

and interstate deregulatory actions. Intrastate deregulation removed within-state restrictions on branching, reducing the high fragmentation and concentration in local banking markets (Jayaratne and Strahan 1996). Interstate deregulation, in turn, allowed banks to expand their operations across state borders through acquisitions or the establishment of subsidiaries, lowering local monopoly power and further fostering competition (Kerr and Nanda 2010). This deregulation process significantly influenced the structure of banking markets, intensifying competition and reshaping bank behaviours (Jayaratne and Strahan 1998; Rice and Strahan 2010; Kroszner and Strahan 2014; Jiang et al. 2016). In line with this, a recent study by De Cesari et al. (2024) shows that the relaxation of the branching restrictions caused a negative impact on bank payout ratios.

To ensure that our results are not biased by coinciding regulatory changes, we augment our baseline model with two dummy variables that capture changes in competitive pressures. Specifically, we include *Intrastate Reform* and *Interstate Reform*, which equal one from the year in which a particular state adopted the relevant deregulation, and 0 otherwise.[12] Table 7 reports the results. While the coefficients of these additional variables are not statistically significant, our main coefficient of interest, *Good Faith*, remains negative and significant. This confirms that our main results are not driven by intrastate and interstate banking deregulation.

3.2.6 | Additional Robustness Tests

In addition to the robustness tests reported in the text, in the Internet Appendix we conduct a series of additional tests to further strengthen our empirical findings. First, in Internet Table S5, we use continuous variables winsorised at the 1st and 99th percentiles and report the same specifications as in Table 2. Second, in Internet Table 6, we estimate our baseline results using traditional staggered difference-in-differences approach. Third, in Internet Table S7, we use an unbalanced sample that includes all banks with at least one observation during the sample period. Fourth, in Internet Table 8, we use both never-treated banks and not-yet-treated banks as the control group. Fifth, in Internet Table S9, we estimate our baseline results using alternative dependent variables, including the change in dividends over year t scaled by dividends at the beginning of the period, dividends-to-deposits, dividends-to-total-loans, and dividends-to-total-assets. Finally, in Internet Table S10, we adopt alternative standard error assumptions. In all cases, we show that our main findings are consistently robust, and therefore we conclude that our main results are not primarily determined by sample selection, the choice of estimator, the type of payout proxy, or the choice of standard errors. Reassuringly, our estimates remain consistent in terms of statistical significance and economic magnitude, suggesting that any potential bias arising from omitted variables or latent local common factors is likely to be low (Oster 2019) and does not pose a serious threat to our identification of good faith adoption on banks' dividend payout policy.

4 | Further Tests

In this section, we conduct a series of additional tests to further examine the cross-sectional variation underlying our baseline results and rule out the existence of possible alternative explanations. First, we perform cross-sectional tests that exploit variation in banks' pre-treatment characteristics to examine whether the effect of good faith adoption on dividend payouts varies with differences in banks' operational rigidity, financial constraints, business model, and charter value. Second, we investigate whether banks respond to stronger employment protection by engaging in alternative forms of risk-shifting through greater asset-side risk-taking. Taken together, these tests provide additional evidence that increases in employment protection and firing costs reduce bank dividend payouts.

4.1 | Cross-Sectional Heterogeneity

Our baseline results show that banks adopt more conservative payout policies and reduce dividends following the adoption of the good faith exception. The good faith exception increases dismissal costs and reduces banks' ability to freely adjust labour inputs, thereby increasing operational leverage and earnings volatility while reducing operational and financial flexibility (Simintzi et al. 2015; Serfling 2016). As a result, the effects of good faith adoption are unlikely to be uniform across banks. In particular, banks with higher labour costs, greater financial leverage, or more labour-intensive business models may face stronger incentives to reduce payouts in order to preserve future financial flexibility. At the same time, banks' charter value and future lending opportunities may also influence their propensity to cut payouts given signalling incentives (Bessler and Nohel 1996). Therefore, we next examine whether the effect of good faith adoption differs across banks' pre-treatment labour costs, financial leverage, business model, and charter value.

TABLE 7 | The impact of good faith adoption on dividend payouts : Coincidental state regulation.

	Dividend payout		
	(1)	**(2)**	**(3)**
Good Faith $_t$	−0.312***	−0.312***	−0.313***
	(0.104)	(0.103)	(0.105)
Intrastate Reform $_t$	−0.016		−0.011
	(0.088)		(0.084)
Interstate Reform $_t$		−0.125	−0.124
		(0.104)	(0.100)
All controls	Yes	Yes	Yes
Cohort-bank fixed effects	Yes	Yes	Yes
Cohort-time fixed effects	Yes	Yes	Yes
Observations	5,272	5,272	5,272
Adjusted R^2	0.779	0.779	0.779

Note: This table presents estimates of the effect of *Good Faith* on *Dividend Payout* after accounting for coinciding regulatory changes. *Intrastate Reform* and *Interstate Reform* are binary variables taking the value one from the adoption year of within-state and cross-state branching deregulation, and zero otherwise. WDLs controls include indicator variables for *Implied Contract* and *Public Policy* exceptions. Bank controls include *Charter Value*, *Bank Size*, *Capital*, *Profitability*, *Loan Loss Provisions*, *Deposit Ratio*, *Cash Holdings*, *Retained Earnings*, and *Dividend Payer*. State controls include *GDP Growth* and *Political Balance*. Bank- and state-level controls are lagged by one year. All columns include cohort-bank and cohort-time fixed effects. Standard errors are robust to heteroskedasticity and within-state dependence and are reported in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% levels, respectively. See Internet Table S2 for variable definitions and construction.

First, banks with higher labour costs are likely to experience larger increases in operational rigidity following good faith adoption because labour represents a greater share of operating expenses. As a result, increases in dismissal costs may have a larger effect on these banks' ability to adjust operating costs in response to adverse shocks, thereby increasing earnings volatility and reducing flexibility (Serfling 2016). Consequently, if the *operational leverage* channel is indeed driving our results, banks burdened by higher ex-ante labour costs are more likely to adopt more conservative payout policies following the adoption of good faith, as they seek to maintain future flexibility and avoid further costly dividend reductions.

Second, we examine heterogeneity across banks' financial leverage. Stronger employment protection increases the expected costs of financial distress by making labour adjustment and restructuring more costly (Simintzi et al. 2015). When combined with high financial leverage, employment rigidity further increases financial inflexibility and exacerbates exposure to adverse shocks, thereby encouraging earnings retention to reduce distress and default risks (DeAngelo and DeAngelo 1990; Chen and Wang 2012). In addition, highly leveraged banks may also be exposed to external disciplining mechanisms. In particular, both market pressure and regulatory constraints may bind more highly leveraged banks (Abreu and Gulamhussen 2013; Floyd et al. 2015), potentially amplifying their payout responses to a regulatory shock such as the adoption of the good faith exception.

Third, we examine whether differences in labour intensity across bank business models affect banks' responses to the adoption of the good faith exception. Fee-oriented banks differ significantly from those that rely more on financial intermediation in terms of cost structure, risk, and performance (DeYoung and Roland 2001; DeYoung and Rice 2004). Fee-generating activities are typically customer-centred and relationship-based. As such, they depend more heavily on skilled labour than lending activities, whose main input in the context of relationship lending is interest expense (DeYoung and Roland 2001). Consequently, banks more reliant on fee-income tend to have more labour-intensive business models and thus higher operational leverage. To the extent that good faith adoption increases labour rigidity, its effect on dividend payouts is likely to be more pronounced for fee-orientated banks.

Finally, we examine whether our baseline findings vary with banks' charter value. Prior research suggests that charter value captures differences in banks' future growth opportunities and incentives to preserve financial flexibility (e.g., Keeley 1990; Floyd et al. 2015; Tripathy et al. 2021; De Cesari et al. 2024). Under asymmetric information, managers may use dividend payouts to convey private information about future cash flows because dividends are costly to adjust (Bhattacharya 1979; John and Williams 1985; Miller and Rock 1985). Banks with higher charter value, which derive a larger portion of firm value from future rents and growth opportunities, may therefore face a trade-off between maintaining payout signals and preserving financial flexibility following increases in labour rigidity (Forti and Schiozer 2015; Tripathy et al. 2021). On the one hand, if preserving internal funds and protecting future profitability dominate signalling concerns, high charter-value banks may be more likely to reduce dividend payouts following the adoption of the good faith exception. On the other hand, if maintaining payout signals about future profitability is sufficiently important, high charter-value banks may instead be less willing to adjust payouts despite the increase in labour rigidity. As a result, the effect of good faith adoption may vary across banks with different levels of bank charter value.

To examine these predictions, we split the sample at the median of each pre-treatment characteristic and re-estimate Equation (1). Table 8 reports the results. The negative effect of *Good Faith* on dividend payouts is concentrated among banks with high pre-treatment labour costs, financial leverage, and fee income, whereas the estimated effects for low-exposure banks are economically small and statistically indistinguishable from zero. These findings suggest that the impact of stronger employment protection is greatest among banks with higher operational and financial rigidity, consistent with the *operational leverage* channel. By contrast, we find little evidence that the effect of good faith adoption differs across banks with high versus low charter value. This finding suggests that the observed decline in dividend payouts is more closely related to increased operational and financial rigidity than to franchise-value or signalling considerations.[13]

4.2 | Ruling Out Alternative Risk-Shifting Channels

Although our main findings are consistent with the *operational leverage* channel, it is possible that banks exposed to stronger

TABLE 8 | Cross-sectional tests.

	High (1)	Low (2)
Panel A: Labour cost		
Good Faith $_t$	−0.479**	−0.052
	(0.198)	(0.376)
All Controls	Yes	Yes
Cohort-bank fixed effects	Yes	Yes
Cohort-time fixed effects	Yes	Yes
Observations	2,664	2,608
Adjusted R^2	0.748	0.815
Panel B: Financial leverage		
Good Faith $_t$	−0.407**	−0.067
	(0.157)	(0.111)
All Controls	Yes	Yes
Cohort-bank fixed effects	Yes	Yes
Cohort-time fixed effects	Yes	Yes
Observations	2,664	2,608
Adjusted R^2	0.764	0.805
Panel C: Fee income		
Good Faith $_t$	−0.341**	0.046
	(0.156)	(0.185)
All controls	Yes	Yes
Cohort-bank fixed effects	Yes	Yes
Cohort-time fixed effects	Yes	Yes
Observations	1352	1352
Adjusted R^2	0.714	0.809
Panel D: Charter value		
Good Faith $_t$	−0.317**	−0.271**
	(0.156)	(0.122)
All controls	Yes	Yes
Cohort-bank fixed effects	Yes	Yes
Cohort-time fixed effects	Yes	Yes
Observations	2672	2600
Adjusted R^2	0.812	0.736

Note: This table presents cross-sectional tests of the effect of *Good Faith* on *Dividend Payout*. In each panel, we split the sample at the pre-treatment median of the corresponding variable of interest, distinguishing between banks above (high) and below (low) the median. Panel A is based on *Labour Cost*, Panel B on *Financial Leverage*, Panel C on *Fee Income*, and Panel D on *Growth Opportunities*. Column (1) reports estimates for banks above the median, and Column (2) reports estimates for banks below the median. WDLs controls include indicator variables for *Implied Contract* and *Public Policy* exceptions. Bank controls include *Charter Value*, *Bank Size*, *Capital*, *Profitability*, *Loan Loss Provisions*, *Deposit Ratio*, *Cash Holdings*, *Retained Earnings*, and *Dividend Payer*. State controls include *GDP Growth* and *Political Balance*. Bank- and state-level controls are lagged by one year. All columns include cohort-bank and cohort-time fixed effects. Standard errors are robust to heteroskedasticity and within-state dependence and are reported in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% levels, respectively. See Internet Table S2 for variable definitions and construction.

employment protection and higher firing costs may also engage in risk-shifting through alternative channels. For example, banks seeking to preserve internal capital may shift risk by increasing asset-side risk-taking, thereby maximising the value of government guarantees and allowing shareholders to benefit from the upside of riskier assets while transferring downside risk to taxpayers ((Keeley 1990; Acharya et al. 2017)). Under this alternative interpretation, the observed decline in dividend payouts may reflect earnings retention used to support greater asset-side risk-taking.

To examine the possibility of risk-shifting through alternative channels, we re-estimate Equation (1) using two measures of bank risk and their respective changes. First, following Delis and Kouretas (2011), we proxy for potential increases in asset-side risk-taking using the ratio of risky assets to total assets. This measure captures whether banks respond to stronger employment protection by reallocating balance-sheet exposures toward riskier assets. Second, following Srivastav et al. (2017), we examine broader changes in bank risk-taking using a market-based measure of *Idiosyncratic Risk* constructed from the residual component of daily bank stock returns after controlling for market and banking industry returns.[14] These measures, and their respective changes, provide robust proxies for whether banks respond to stronger employment protection by increasing asset risk and idiosyncratic risk.

Table 9 reports the results. Across all specifications, the coefficients on *Good Faith* are small and statistically insignificant, indicating that the adoption of the good faith exception does not increase either asset-side risk-taking or idiosyncratic risk. Taken together, these findings provide little support for alternative risk-shifting channels and instead reinforce the interpretation that the observed decline in dividend payouts is more consistent with the *operational leverage* channel.

5 | Conclusion

In this paper, we examine how stronger employment protection and higher firing costs affect banks' dividend payouts. Using the staggered adoption of the good faith exception to US WDLs as a plausibly exogenous shock to employment protection, we document that banks significantly reduce their dividend payouts following the introduction of the good faith rule. Our stacked difference-in-differences estimates indicate that the adoption of good faith leads to an economically meaningful decline in dividends of roughly 7%, relative to pre-treatment means. Across a range of empirical tests, we show that our results are robust and not driven by violations of the parallel trends assumption, pre-existing differences between treated and control banks, anticipatory behaviour, or concurrent shifts in local banking competition.

Consistent with the *operational leverage* channel, our cross-sectional analyses reveal that the reduction in dividend payouts is most pronounced among treated banks with high labour costs, high financial leverage, and more labour-intensive business models. Alternatively, we find no evidence to suggest that our findings are driven by differences in banks charter value. Overall, this study underscores the link between employment protection laws and dividend distributions. From a policy perspective, our findings suggest that WDLs, in particular good faith, can generate unintended spillover effects on banks' behaviour and dividend payout policy.

TABLE 9 | The impact of good faith adoption on alternative risk taking channels.

	Risky assets (1)	Δ Risky assets (2)	Idiosyncratic risk (3)	Δ Idiosyncratic risk (4)
Good Faith $_t$	0.008	−0.001	0.003	0.001
	(0.015)	(0.007)	(0.002)	(0.001)
All controls	Yes	Yes	Yes	Yes
Cohort-bank fixed effects	Yes	Yes	Yes	Yes
Cohort-time fixed effects	Yes	Yes	Yes	Yes
Observations	5234	5223	5267	5250
Adjusted R^2	0.764	0.034	0.686	0.198

Note: This table presents the validation test of the effect of *Good Faith* on bank risk. Column (1) and (2) report estimates for *Risky Assets* and Δ *Risky Assets*, respectively. Column (3) and (4) report estimates for *Idiosyncratic Risk* and Δ *Idiosyncratic Risk*, respectively. WDLs controls include indicator variables for *Implied Contract* and *Public Policy* exceptions. Bank controls include *Charter Value*, *Bank Size*, *Capital*, *Profitability*, *Loan Loss Provisions*, *Deposit Ratio*, *Cash Holdings*, *Retained Earnings*, and *Dividend Payer*. State controls include *GDP Growth* and *Political Balance*. Bank- and state-level controls are lagged by one year. All columns include cohort-bank and cohort-time fixed effects. Standard errors are robust to heteroskedasticity and within-state dependence and are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. See Internet Table S2 for variable definitions and construction.

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Conflicts of Interest

The authors declare no conflicts of interest.

Data Availability Statement

Data subject to third-party restrictions. Restrictions apply to the availability of these data, which were used under license for this study.

Endnotes

[1] For example, using the Compustat universe of U.S. banks from 2000 to 2025, the ratio of wages and salaries to total non-interest expense averages 51.7%. The annual cross-sectional average ranges from 45.4% in 2009% to 55.0% in 2021.

[2] An alternative view is that banks may try to weaken employees' continuation values via greater asset risk-taking. However, compared with dividend payouts, increases in asset risk do not directly constrain the internal cash flows available for rent extraction. In addition, employment protection limits employees' exposure to downside risk while preserving upside rents (Lazear 1990). Consequently, adjustments in payout policy are likely to constitute a more immediate and effective mechanism for curbing labour rents than risk-taking through bank assets. In Section 4.2, we estimate our baseline specification using different measures of bank risk as dependent variables. The test provides no evidence to suggest that good faith adoption resulted in risk-shifting via alternative margins.

[3] Regulatory constraints may limit the payout flexibility of capital-constrained banks, without altering the underlying incentive to shelter internal resources. To the extent that capital requirements are met, stronger employment protection may lead banks to distribute cash flows to mitigate rent extraction.

[4] We report the exact date in which WDLs were passed for each state in Internet Table S1.

[5] For example, California and Connecticut adopted the Good Faith exception in 1980. Banks headquartered in these two states represent the treatment group over the event window, whereas those headquartered in never-treated states serve as the control group over the same period.

[6] We acknowledge that some banks, particularly in the later part of the sample, operate across multiple states. As a consequence, assigning treatment based on headquarters may introduce a measurement error in their exposure to employment protection. However, if anything, this misclassification would bias our estimates towards zero and thus work against the significance of our results.

[7] In Internet Table S4, we confirm that our results are robust when using unbalanced sample.

[8] In Section 3.2.2, we address potential concerns that differences in bank- and state-level characteristics influence our main results. Using propensity score matching and entropy balancing, we show that our findings remain robust.

[9] In our stacked DiD design, cohorts and event years are defined around the adoption of the good faith exception. We include IC and PP to control for the adoption of complementary WDL exceptions. However, the estimated coefficients of these variables should not be interpreted causally, as the three WDL exceptions are rarely adopted simultaneously within the same state-year. See Internet Table S1 for the exact adoption dates of the WDL exceptions across states.

[10] We thank one of the anonymous referees for this suggestion.

[11] A potential concern is that banks may substitute dividends with share repurchases, weakening our interpretation that the observed reduction in dividends reflects more conservative financial policies. Because data on repurchases in our dataset are sparsely available, we examine this possibility using FR Y-9C call reports, which are only available for a sub-sample of Bank Holding Companies (BHC) from 1986 onward. The results, which can be made available upon request, indicate that BHCs reduce dividends and total payouts following the adoption of the Good Faith exception, with no corresponding increase in share repurchases.

[12] We obtain the adoption dates of interstate and intrastate deregulation from Beck et al. (2010).

[13] In an additional unreported test (available upon request), we further investigate whether our results vary with banks' capital buffers by splitting the sample into weakly, but not mechanically constrained banks, that is, with a median pre-treatment value of equity-to-total-assets ratio between 4% and 5%, and well capitalised banks. We find that the adoption of the good faith exception only affects dividend payout among well-capitalised banks. Overall, this evidence suggests that the results reported in Panel B of Table 8 are not driven by binding regulatory capital constraints.

[14] Complete variable definitions are provided in Internet Table S1.

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Supporting Information

Additional supporting information can be found online in the Supporting Information section.
Supporting File: eufm70083-sup-0001-Internet_Appendix.pdf.