Applicant Screening and Firm Value: Evidence from Ban-the-Box Laws

Scott Guernsey, Matthew Serfling, and Cheng Yan*

September 1, 2026

ABSTRACT

Firms create value by acquiring workers whose skills complement their productive needs, yet early applicant screening may impede this process by excluding potentially productive candidates before firms observe richer information about their qualifications and fit. We study how this friction affects firm value using staggered Ban-the-Box laws, which delay criminal-history screens until later in the hiring process. Following adoption, affected firms experience higher valuations, especially where an expanded applicant pool should be more consequential. Firms also fill vacancies faster, report fewer labor shortages, and experience fewer early worker separations. These improvements in labor acquisition are followed by higher productivity, profitability, and cash flow, while volatility, default risk, and the implied cost of equity decline. Our findings identify early applicant screening as an important labor-market friction and suggest that reducing screening-related barriers to labor acquisition can increase firm value through both cash-flow and discount-rate channels.

Keywords: Ban-the-Box; Applicant screening; Labor acquisition; Firm value; Cost of equity; Productivity; Labor and corporate finance

JEL Classifications: G30, G32, J23, J63, K31

*Scott Guernsey (sguernsey@utk.edu) and Matthew Serfling (mserflin@utk.edu) are at the Haslam College of Business at the University of Tennessee, and Cheng Yan (cyan@hust.edu.cn) is at Huazhong University of Science and Technology. We are grateful for the helpful comments and suggestions from Matt Henriksson, Eric Kelley, Zhengzhuo (Zyaire) Li, Daniel Weagley, Tracie Woidtke, and Liu Yang, as well as seminar participants at the University of Tennessee.

1 Introduction

Can early-stage applicant screening destroy firm value? Applicant screening is a ubiquitous part of the hiring process, allowing firms to eliminate unsuitable candidates early before undertaking more costly evaluation.[1] By eliminating candidates at this stage, firms can conserve recruiting and managerial resources and focus evaluation on the most promising applicants. Yet relying on coarse early signals may also end consideration prematurely, excluding potentially productive workers before firms observe richer information about their qualifications and fit. Whether early-stage screening facilitates or impedes value creation therefore depends on whether its informational benefits outweigh the value of the productive matches it may prevent. Because firms cannot convert capital, technology, and growth opportunities into output without acquiring workers whose skills and attributes complement their productive needs, this tradeoff can have important consequences for expected cash flows, risk, and ultimately firm value.

The potential cost of early screening operates through firms' ability to acquire labor. An imperfect screen may reduce the firm's effective applicant pool—the set of candidates that survive initial evaluation and receive meaningful consideration for employment. A smaller effective applicant pool can make vacancies harder to fill, increase recruiting costs, constrain firms' ability to pursue valuable opportunities, and reduce the likelihood of forming productive worker-firm matches. Indeed, recruiting intensity and vacancy-filling rates are important margins through which firms adjust their labor inputs (Davis, Faberman, and Haltiwanger, 2013; Gavazza, Mongey, and Violante, 2018). Despite the importance of these margins, little

[1] Applicant screening is a standard feature of hiring among large U.S. employers and commonly includes application review, interviews, verification checks, background investigations, and other job-specific assessments (Levashina and Campion, 2009). Evidence also shows that the design and sequencing of applicant evaluation can affect which candidates advance (Hoffman, Kahn, and Li, 2018; Koren, 2026).

is known about whether the way firms screen applicants affects their ability to acquire labor and create shareholder value.

Identifying this effect is challenging because firms endogenously choose how to evaluate applicants in response to their labor needs and hiring risks. Costly vacancies, applicant volume, legal exposure, and difficulty evaluating worker quality may shape these choices while independently affecting firm outcomes. Applicant evaluation is also difficult to observe systematically, and formal policies may not fully capture how employers make screening decisions in practice. Thus, cross-sectional variation in screening cannot isolate its effect on firm value from the underlying conditions that shape firms' screening choices.

We address this identification challenge using the staggered adoption of private-sector Ban-the-Box (BTB) laws across U.S. states, counties, and cities. These laws were adopted primarily to reduce employment barriers facing individuals with criminal records and, in many cases, to address associated racial disparities in employment (Agan and Starr, 2018; Doleac and Hansen, 2020; Avery and Lu, 2021). BTB laws generally prohibit criminal-history inquiries on the initial application but continue to allow employers to obtain and consider legally relevant criminal-history information later in the hiring process (Agan and Starr, 2018; Raphael, 2021). Thus, rather than removing this information from employer decision-making or requiring firms to hire applicants with criminal records, BTB delays this early screen until employers have an opportunity to evaluate candidates using additional information about their skills, experience, and suitability for a position, as shown in Figure 1.

This institutional setting provides plausibly exogenous variation in firms' effective applicant pools during early-stage evaluation. After BTB adoption, applicants who otherwise would have been eliminated at the outset can remain under consideration until a later stage, although employers can still reject them after conducting a background check. Because early criminal-history screening can remove a substantial share of potential workers from consideration,

delaying this screen can meaningfully expand firms' effective applicant pools.[2] We use this variation to examine whether delaying an early applicant screen improves firms' ability to acquire labor, form more durable employment relationships, and create shareholder value.

Our sample covers private-sector BTB adoptions during 2005–2024. We assign treatment using the earliest applicable mandate covering the jurisdiction of a firm's historical headquarters. We estimate treatment effects using the imputation-based difference-in-differences (DiD) estimator of Borusyak, Jaravel, and Spiess (2024), which accommodates staggered adoption and heterogeneous effects, together with firm, industry-by-year, and Democratic-governor-by-year fixed effects as well as lagged firm- and state-level controls.

We first find that delaying an early applicant screen increases firm value. Following BTB adoption, treated firms experience increases in Tobin's Q, with similar gains in the market value of equity and stock returns in robustness tests. Dynamic event-study estimates reveal no evidence of differential pre-treatment trends, and the valuation gains emerge only after adoption. These findings provide initial evidence that the timing of applicant screening has economically meaningful consequences for shareholder value.

We next examine whether improvements in labor acquisition and retention accompany the increase in firm value. We do not directly observe firms' applicant-screening decisions or the candidates who progress through each stage of the hiring process. We therefore examine several labor-market outcomes that capture firms' ability to acquire and retain workers. We find that treated firms report fewer labor shortages, disclose fewer labor-friction concerns in their 10-K risk factors, fill vacancies more quickly, and have fewer positions that remain persistently unfilled. Early worker separations also decline, including separations during the first and second years of employment. Although these outcomes do not directly reveal

[2]Roughly one-third of U.S. adults have an arrest or conviction record (Avery and Lu, 2021; Raphael, 2021). In addition, Agan and Starr (2018) find that, among employers requesting criminal-history information, applicants with records were approximately 39% less likely to receive callbacks, while Cullen, Dobbie, and Hoffman (2023) find that only 39% of firms are willing to employ individuals with criminal records.

how firms alter screening or identify the marginal applicants affected by BTB, their joint behavior is consistent with the proposed mechanism. In particular, the decline in both vacancy duration and early separations suggests that delayed screening lets firms access a broader set of potentially suitable workers and form more durable employment relationships, rather than merely filling vacancies faster by relaxing hiring standards.

Cross-sectional tests further connect the valuation effects to firms' acquisition of labor. The effects are stronger when an expanded applicant pool should matter more: in tight labor markets and high-crime areas, among firms with stronger demand for workers, and among labor-intensive firms with limited ability to substitute automated capital for labor. In contrast, the effects are weaker when BTB is less likely to alter employer screening, including among firms with greater exposure to occupations in which criminal background checks are legally required or deeply institutionalized. These patterns are difficult to explain with a general local-policy or reputational effect because they scale with both firms' need for labor and the likelihood that BTB alters employer screening.

We then ask whether BTB affects firm value through the cash-flow channel, the discount-rate channel, or both. Consistent with higher expected cash flows, treated firms exhibit higher total factor productivity (TFP), sales per employee, sales relative to assets, profitability, and cash flow, while selling, general, and administrative (SG&A) expenses decline. These improvements are consistent with better labor acquisition allowing firms to operate more efficiently and exploit productive opportunities, rather than with BTB merely reducing vacancy duration or imposing compliance costs. At the same time, treated firms experience lower total and idiosyncratic stock-return volatility, lower expected default frequency, and a lower implied cost of equity (COE), consistent with improved labor acquisition reducing uncertainty about firms' future operating performance. Thus, the increase in firm value appears to reflect both stronger expected cash flows and a lower required return.

We conduct several analyses to address alternative interpretations and threats to identification. Adoption-hazard models indicate that prior firm value and observable state conditions do not predict BTB adoption. The results remain similar after accounting for contemporaneous state-law changes,[3] reweighting firms by their probability of treatment, including Census-region-by-year fixed effects, and applying alternative DiD estimators. Valuation effects are also positive and significant under both Democratic and non-Democratic governors, reducing concerns about political conditions in adopting jurisdictions. The effects increase with mandate strength once controls are included, and the results remain robust to using alternative measures of firms' geographic exposure based on establishment and employee locations, to incorporating intangible capital into Tobin's Q (Peters and Taylor, 2017), and to using alternative valuation measures, including market value of equity and stock returns.

The paper makes two main contributions. First, it identifies firms' ability to acquire labor as a determinant of firm value. Existing labor and finance research shows that institutions governing incumbent workers affect financing, investment, innovation, risk, and valuation (Agrawal and Matsa, 2013; Acharya, Baghai, and Subramanian, 2014; Simintzi, Vig, and Volpin, 2015; Bai, Fairhurst, and Serfling, 2020; Bena, Ortiz-Molina, and Simintzi, 2022; Jeffers, 2024). A complementary literature examines how financial conditions and constraints on worker retention and mobility affect firms' access to labor and corporate outcomes (Brown and Matsa, 2016; Baghai, Silva, Thell, and Vig, 2021; Shen, 2021). We instead study a logically prior margin: the process through which firms identify, evaluate, and acquire future workers. Related evidence shows that labor shortages impair operating performance and induce firms to alter their input mix (Harford, He, and Qiu, 2026). Our results show that frictions operating before an employment relationship begins can affect labor acquisition,

[3]These additional controls include information-environment policies such as anti-SLAPP laws, data-breach notification laws, and the FOIA law index; employment and social policies such as minimum-wage laws, pre-employment credit-check restrictions, paid family and medical leave, paid sick leave, and salary-history bans; and innovation and investment policies such as intellectual-property protections and investor tax credits.

operating performance, risk, and ultimately firm value. These effects operate through both expected cash flows and required returns, indicating that capital markets price not only firms' relationships with incumbent workers but also their ability to acquire future workers.

Second, the paper shows that early-stage applicant screening can itself constitute a hiring friction. Prior research primarily studies BTB as a worker-side policy affecting applicant callbacks, employment, and statistical discrimination (Agan and Starr, 2018; Craigie, 2020; Doleac and Hansen, 2020; Rose, 2021; Shoag and Veuger, 2021; Kaestner and Wang, 2024; Burton and Wasser, 2025). We instead examine whether the timing of an early-stage screen affects firms' ability to acquire labor and create shareholder value. Because we do not observe the characteristics of marginal applicants and hires, our analysis does not identify worker-level or distributional effects, which can be heterogeneous (Agan and Starr, 2018; Doleac and Hansen, 2020). Rather, we focus on the distinct firm-side consequences of applicant screening. Our findings also connect search-and-matching theories of costly evaluation and imperfect information to corporate valuation, showing that firms are affected not only by the information available during hiring, but also by when that information enters the evaluation process (Mortensen and Pissarides, 1994; Huang and Cappelli, 2010; Acharya and Wee, 2020; Cai, Gautier, and Wolthoff, 2025).

2 Institutional Background and Hypotheses

2.1 Applicant Screening and Ban-the-Box Laws

Firms rarely observe all information relevant to an applicant's productivity simultaneously. Hiring instead proceeds through a sequence of increasingly informative stages, allowing employers to allocate recruiting and managerial resources before undertaking more costly assessment (Levashina and Campion, 2009; Hoffman et al., 2018; Koren, 2026). Initial appli-

cation reviews are commonly followed by interviews, employment and education verification, reference checks, criminal-history and other background investigations, credential verification, and other job-specific assessments. Because early stages determine which applicants advance to more intensive evaluation, screening design and sequencing affect which candidates remain under consideration long enough for employers to observe richer information about their qualifications, experience, and potential fit.

Criminal-history screening has historically been one component of this process. Many employers requested criminal-history information on the initial application through a checkbox asking whether an applicant had been arrested or convicted of a crime. Employers could therefore use criminal history to remove applicants before interviews or other substantive evaluation. BTB laws alter this sequence. Rather than prohibiting employers from obtaining or considering criminal-history information, BTB laws generally require employers to delay the inquiry until later in the hiring process, such as after an interview or conditional offer (Agan and Starr, 2018; Raphael, 2021). Employers retain access to legally relevant criminal-history information before making final hiring decisions; the laws primarily change when that information becomes available.

We refer to applicants who survive initial screening and receive meaningful evaluation as the firm's *effective applicant pool*. By delaying an early criminal-history screen, BTB can allow applicants who otherwise would have been eliminated to remain under consideration while employers observe richer information about their skills, experience, references, and fit. Whether this improves hiring outcomes is theoretically ambiguous: delaying an informative screen can increase evaluation costs, whereas delaying an imperfect screen can preserve potentially productive worker-firm matches.

Criminal-history screening can affect a substantial share of potential workers and materially influence whether applicants advance in the hiring process. Roughly one-third of U.S. adults have some form of arrest or conviction record, and approximately 8% have a felony conviction

(Avery and Lu, 2021; Raphael, 2021). In a field experiment, Agan and Starr (2018) find that, among employers requesting criminal-history information, otherwise similar applicants with convictions were approximately 39% less likely to receive callbacks than applicants without convictions. Related evidence indicates that employers' willingness to consider workers with criminal records is highly sensitive to the information available during hiring. Cullen et al. (2023) find that only 39% of businesses on a nationwide staffing platform are willing to work with individuals with criminal records at baseline, but willingness increases substantially when employers receive information about prior performance, face reduced downside risk, or are having difficulty filling jobs. Consistent with these findings, Shoag and Veuger (2021) show that BTB adoption increases employment in high-crime neighborhoods, where workers with criminal records are more concentrated. Together, this evidence suggests that criminal-history information can operate as an economically meaningful early-stage screen and that delaying its consideration can alter the set of applicants receiving substantive evaluation.

BTB laws emerged from broader fair-chance efforts intended primarily to reduce employment barriers facing individuals with criminal histories. The policy rationale is that applicants should have an opportunity to demonstrate their qualifications before employers consider criminal-history information, rather than being excluded at the initial application stage (Agan and Starr, 2018; Craigie, 2020; Avery and Lu, 2021). These efforts also reflect concerns about racial disparities because exposure to the criminal-justice system differs substantially across demographic groups (Agan and Starr, 2018; Doleac and Hansen, 2020). Consistent with these objectives, legislative findings in several adopting jurisdictions emphasize fair consideration of qualified applicants, civil rights, reintegration, and reducing barriers to employment for individuals with records.[4] Thus, the stated objectives of BTB are principally

[4]For example, Illinois's law states that its objectives include giving employers access to a broad pool of qualified applicants, protecting applicants' civil rights, and preventing qualified applicants from being unnecessarily screened out. New Jersey's law emphasizes removing employment obstacles for individuals with criminal records and the role of employment in reducing recidivism, while New York City's law similarly stresses evaluating applicants on their merits before considering criminal history.

worker-side—expanding employment opportunity and promoting fair consideration—rather than altering corporate productivity or shareholder value.

Hawaii enacted the first private-sector BTB law in 1998, and adoption accelerated during the 2010s. By 2021, approximately four-fifths of the U.S. population lived in a jurisdiction covered by some form of fair-chance hiring policy (Avery and Lu, 2021). As of 2026, 15 states, the District of Columbia, and numerous cities and counties have adopted private-sector BTB mandates. Moreover, private-sector BTB laws vary in terms of when criminal-history inquiry becomes permissible, individualized-assessment requirements, records that may be considered, employer-size thresholds, enforcement mechanisms, penalties, and occupational exemptions. Many laws exempt positions in financial services, healthcare, childcare, education, law enforcement, and other occupations that require separate criminal-history screening. Online Appendix OA describes these institutional differences in detail.

2.2 Why Ban-the-Box Can Affect Large Employers

A natural question is why firms used the criminal-history box if delaying that screen could improve labor acquisition and firm performance. Using the box can be privately rational because criminal history is an inexpensive, salient, and easily standardized signal that lets employers quickly reduce large applicant pools and may help address perceived productivity, safety, and negligent-hiring risks (Cullen et al., 2023). The costs of early screening are less visible: firms generally do not observe the productivity of applicants rejected at the initial stage, the vacancies that remain open because qualified candidates were excluded, or the productive opportunities forgone because hiring took longer. Employers may therefore place greater weight on the immediate benefits of a coarse screen than on its diffuse costs.

These incentives are particularly relevant for large employers, whose hiring systems often rely on standardized online applications, applicant-tracking systems, centralized human-

resource procedures, and third-party screening vendors (Hoffman et al., 2018). Once criminal-history inquiries are embedded in these systems, retaining them is administratively simple, whereas changing the screening sequence can require coordinated revisions to application forms, compliance procedures, hiring-manager instructions, and information systems. Employers may also place substantial weight on the perceived risks and potential liability associated with hiring applicants with criminal records, whereas the opportunity cost of prematurely rejecting a potentially productive applicant is less readily observable. As a result, formal hiring protocols typically govern the timing of criminal-history inquiry rather than leaving it to the discretion of individual managers (Lageson, Vuolo, and Uggen, 2015).

A BTB mandate can therefore meaningfully change applicant evaluation by delaying criminal-history inquiry until employers have observed richer applicant information. Whether this improves outcomes remains ambiguous. Delaying an informative screen may raise evaluation costs, whereas delaying an imperfect screen can expand firms' effective applicant pools, increase opportunities for productive worker-firm matches, and facilitate labor acquisition (Hoffman et al., 2018; Koren, 2026).

This argument does not require every covered employer to have used the box before adoption or every position to be affected. Some firms adopted fair-chance practices voluntarily, and many laws exempt occupations subject to independent screening requirements. Our estimates therefore capture the average effect of an externally imposed change in screening timing across affected firms and positions.

2.3 Measuring Exposure to Ban-the-Box Laws

Our empirical design exploits the staggered adoption of private-sector BTB mandates across U.S. states, counties, and cities. Because our mechanism requires a law to alter private employers' applicant-screening practices, we exclude public-sector-only mandates and laws

applying solely to government contractors. We assign each firm-year to the earliest applicable private-sector mandate covering the firm's historical headquarters jurisdiction. A firm becomes treated when a state, county, or city mandate first applies to its headquarters, even if a broader state law is adopted later. Firms outside covered local jurisdictions remain untreated until a subsequent mandate applies.

Headquarters location is an imperfect measure because public firms often employ workers across many jurisdictions. In Section 6, we address this concern using employment-weighted exposure measures based on establishment and employee locations and by restricting the sample to geographically concentrated firms. Headquarters location nevertheless determines the firm's primary legal jurisdiction and is widely used in corporate-finance studies of state and local legal changes.

Table 1 reports the signing and effective years of the mandates in our treatment definition, along with one post-sample adoption for completeness. Our baseline analysis dates treatment using the signing year because firms and investors may adjust once legislation is enacted; signing and effective dates occur in the same or adjacent years in most jurisdictions. Our conclusions are unchanged when we use statutory effective dates instead.

2.4 Hypothesis Development

The effect of early-stage applicant screening on firm value is theoretically ambiguous. Early screening can conserve recruiting and managerial resources by allowing firms to reject applicants before undertaking more costly evaluation. If criminal history is sufficiently informative about worker suitability, delaying the inquiry requires firms to evaluate more applicants who will ultimately be rejected, increasing screening costs without materially improving hiring outcomes.

At the same time, an early screen can destroy value if it removes potentially productive applicants before firms observe richer information about their qualifications and worker-firm fit. Criminal history is an imperfect signal of suitability because applicants with records differ substantially in skills, experience, offense characteristics, rehabilitation, and fit for particular jobs. Delaying this screen lets more applicants receive substantive evaluation before firms consider criminal-history information, expanding the firm's effective applicant pool and increasing the likelihood of identifying an acceptable match.

The effect on firm value depends on whether these matching benefits outweigh the additional costs of evaluating a broader applicant pool. If delaying the screen improves access to suitable workers, firms should fill vacancies more quickly and experience fewer labor shortages. Better matches may also be more durable, reducing subsequent separations. Because recruiting and vacancy filling are important margins through which firms adjust labor inputs (Davis et al., 2013; Gavazza et al., 2018), these improvements can allow firms to operate more efficiently and exploit productive opportunities.

Improved labor acquisition can affect valuation through both cash-flow and discount-rate channels. Better and more durable matches can increase productivity, profitability, and expected cash flows. A more reliable labor-acquisition process can also reduce uncertainty about firms' ability to maintain production, satisfy customer demand, and pursue growth opportunities, thereby lowering firm-specific risk and the required return. Thus, delaying early-stage screening increases firm value when the gains from faster hiring, better matching, and lower labor-input risk exceed the additional costs of evaluating more applicants. The model in Online Appendix OB formalizes this tradeoff.

3 Data and Empirical Methodology

3.1 Sample Selection and Data Sources

Our baseline sample consists of U.S. publicly traded firms in the CRSP/Compustat Merged Database from 2005 to 2024. The sample begins in 2005 to provide five years of pre-treatment data before the broader wave of private-sector BTB adoption, which began with Massachusetts in 2010.[5] We exclude firms in the utility (SIC codes 4900–4999), financial (SIC codes 6000–6999), and quasi-public or nonclassifiable industries (SIC codes above 9900). We further require sufficient information to identify firms' historical headquarters and construct the relevant outcome variables, as well as a beginning-of-year stock price of at least $5 to reduce microstructure-related concerns.

Financial-statement data are from Compustat, stock prices and returns from CRSP, analyst forecasts from IBES, and historical headquarters locations from the WRDS SEC Analytics Suite. State-level economic, demographic, political, and policy variables are assembled from publicly available government sources (e.g., the U.S. Bureau of Economic Analysis, Bureau of Labor Statistics, and Department of Labor). We construct measures of hiring difficulty from conference-call transcripts, annual reports, and Revelio Labs job-posting data, while Revelio Labs employment histories measure worker separations. We winsorize continuous firm- and state-level variables at the 1st and 99th percentiles unless otherwise indicated and express all dollar-denominated variables in 2025 dollars. Appendix A provides detailed variable definitions, construction procedures, and data sources.

[5] Hawaii adopted a private-sector BTB law in 1998, but this early adoption affects only seven sample firms and 45 firm-year observations. The next private-sector state adoption occurs in Massachusetts in 2010. We therefore begin the baseline sample in 2005, providing five years of pre-treatment data before the broader wave of adoption begins. In untabulated analyses, our main conclusions hold when we extend the sample back to 1993 and include the Hawaii adoption.

The baseline valuation sample has approximately 37,500 firm-year observations, while other samples vary across specifications because of outcome-specific data requirements and the support requirements of the imputation estimator (Borusyak et al., 2024). Several data sources also have more limited temporal coverage; for example, Revelio Labs job-posting data begin in 2016. Table 2 reports summary statistics for the key variables used throughout the paper.

3.2 Research Design

Our empirical design exploits the staggered adoption of private-sector BTB laws across U.S. states, counties, and cities. As described in Section 2.3, firms become treated upon enactment of the earliest applicable mandate covering their historical headquarters jurisdiction.

To establish notation, consider the conventional two-way fixed-effects specification:

$$y_{i,t} = \alpha_i + \beta_t + \tau D_{i,t} + \gamma X_{i,t-1} + \varepsilon_{i,t}, \tag{1}$$

where $y_{i,t}$ denotes an outcome for firm i in fiscal year t, $D_{i,t}$ equals one following BTB adoption, and $X_{i,t-1}$ contains lagged firm- and state-level controls. Firm and year fixed effects absorb time-invariant firm characteristics and aggregate shocks, respectively. Standard errors are clustered by state.

Because BTB adoption is staggered, conventional two-way fixed-effects estimates can be difficult to interpret when treatment effects vary across cohorts or over time (e.g., Goodman-Bacon, 2021; Baker, Larcker, and Wang, 2022; Borusyak et al., 2024). We therefore use the imputation estimator of Borusyak et al. (2024), which constructs untreated counterfactual outcomes without using already-treated firms as controls and allows treatment effects to evolve over time.

The estimator proceeds in three steps. First, we estimate the untreated outcome process using only firm-years not yet exposed to a BTB law. Second, we use these estimates to impute

the counterfactual outcome each treated firm-year would have experienced absent treatment. Third, we calculate the difference between the realized and imputed outcomes and aggregate these firm-year effects to obtain the average treatment effect on the treated (ATT).

Our baseline specifications include firm, two-digit SIC industry-by-year, and Democratic-governor-by-year fixed effects, which Table 3 introduces sequentially. Industry-by-year fixed effects absorb time-varying industry shocks, while Democratic-governor-by-year fixed effects allow firms in Democratic- and non-Democratic-led states to follow different time paths (i.e., these fixed effects ensure that our estimates do not rely on comparisons generated by contemporaneous differences between Democratic- and non-Democratic-led states). More stringent specifications additionally include lagged firm- and state-level controls. Firm controls include the logarithm of total assets, asset tangibility, cash flow, net book leverage, and the logarithm of firm age. State controls include real GDP per capita and its growth rate, the corporate income-tax rate, unemployment, unemployment-insurance benefits, resident age and educational attainment, the female population share, and medical-marijuana legalization. We report results both with and without these controls because some may themselves respond to treatment.

Identification comes from changes in treated firms' outcomes following BTB adoption relative to untreated observations used to construct their counterfactual outcomes, conditional on the included fixed effects and controls. The key identifying assumption is that, absent adoption, treated firms would have followed the same outcome path as these untreated observations. Two concerns are particularly relevant: BTB adoption may coincide with other economic or policy changes, and headquarters-based treatment may imperfectly capture firms' actual hiring exposure. We assess pre-treatment trends using dynamic event studies and address these and other identification threats in Section 6. Moreover, the stated objectives of BTB laws focus primarily on improving employment opportunities for workers with criminal records, making a direct policy response to firm valuation less likely.

4 Firm Value and Heterogeneous Effects of BTB

We begin by examining whether delaying an important early-stage applicant screen affects firm value. We then examine the dynamics and economic magnitude of these valuation effects and whether they concentrate where improved labor acquisition should matter most.

4.1 Effects on Firm Valuation

Table 3 reports the estimated effects of BTB adoption on firm value using the natural logarithm of Tobin's Q ($Ln(TQ)$) as our primary valuation measure. Section 6 shows that the results are robust to alternative valuation measures and stock returns. The estimated average treatment effects are positive and statistically significant across all specifications. In column 1, which includes firm and industry×year fixed effects, Tobin's Q increases by about 5.5% following BTB adoption. Adding $DemGov$×year fixed effects moderates the estimate to 4.7%. With additional firm- and state-level controls in columns 3 and 4, the estimated effects increase to 7.6% and 8.2%, respectively.

Figure 2 presents dynamic treatment effects using the four specifications from Table 3. In all cases, the estimated pre-treatment coefficients are small and statistically indistinguishable from zero, with no systematic trend preceding BTB adoption. Following adoption, the estimates become positive and statistically significant. The absence of differential pre-treatment trends, together with valuation gains emerging only after adoption, supports the interpretation that the increase in firm value follows the change in applicant-screening rules.

4.2 Discussion of Economic Magnitudes

We find that BTB adoption increases Tobin's Q by 4.7%–8.2%. Because BTB changes the timing of one component of applicant screening rather than directly mandating whom firms

hire, a natural question is whether the affected margin is sufficiently important to generate the observed valuation response. We assess the economic magnitude using external evidence on the scope of criminal-history screening and the importance of labor frictions to firm value.

First, the evidence in Section 2 indicates that criminal-history screening can materially affect which applicants receive meaningful consideration. This margin is particularly relevant among individuals with weaker attachment to employment. Bushway, Cabreros, Paige, Schwam, and Wenger (2022), for example, report that by age 35, 64% of unemployed men have experienced an arrest and 46% have a conviction. Thus, although BTB does not prevent employers from ultimately obtaining criminal-history information, it delays a screen that applies to a potentially important segment of the applicant pool.

Second, prior research suggests that labor-adjustment frictions can account for a meaningful portion of firm value. Merz and Yashiv (2007) argue that when labor is costly to adjust, employment becomes a quasi-fixed input with a positive shadow value that enters firm value alongside physical capital. Their structural estimates imply marginal hiring costs of about two quarters of wage payments. Belo, Gala, Salomao, and Vitorino (2022) also estimate that installed labor accounts for approximately 14%–21% of firm market value and that labor-adjustment costs absorb approximately 2%–6% of annual sales, depending on industry skill intensity. These estimates indicate that labor-adjustment frictions can have economically important consequences for firm value.

Reduced-form evidence provides complementary benchmarks. Shen (2021) exploits exogenous changes in skilled-worker mobility generated by the U.S. green-card allocation system and estimates that labor-mobility frictions account for approximately 6%–7% of firms' market value, with larger effects when adjustment costs are greater. Related evidence from H-1B rationing indicates that constraints on firms' access to skilled workers can reduce market value by approximately 10% (Mayda, Ortega, Peri, Shih, and Sparber, 2023). Although these settings differ from BTB and the estimates are not directly comparable to effects on Tobin's

Q, they demonstrate that labor-market frictions affecting worker access and mobility can substantially affect valuation.

Taken together, these benchmarks suggest that our estimated increase in Tobin's Q is economically plausible. The magnitude does not require BTB to transform firms' entire workforces: criminal-history screening is particularly relevant among workers on the margin of employment, and prior research shows that frictions in acquiring and adjusting labor can account for a meaningful share of corporate value. The following sections examine whether BTB produces the corresponding improvements in labor acquisition and operating outcomes.

4.3 Heterogeneous Effects: Where Labor Acquisition Matters Most

The valuation effects of BTB should vary with both the importance of labor acquisition and the extent to which the law changes firms' applicant-screening practices. We examine four dimensions: local labor-market tightness and crime prevalence, exposure to occupations in which criminal background screening remains required or highly institutionalized, demand for additional labor, and dependence on labor as a productive input.

4.3.1 Labor-Market Tightness and Crime Prevalence

Expanding a firm's effective applicant pool should be more valuable when workers are difficult to obtain. In tight labor markets, removing potentially productive applicants at the initial screening stage can prolong vacancies, constrain production, and increase recruiting and staffing costs. BTB should also have larger effects where criminal-history screening would otherwise exclude more potential workers. Because criminal involvement is more prevalent in high-crime areas, employers in these markets are more likely to encounter applicants whose criminal histories could exclude them before they observe richer information about qualifications and fit.

Columns 1–4 of Table 4 evaluate these predictions using two measures. The first is the lagged unemployment-to-vacancy ratio in the firm's headquarters state. A lower ratio indicates fewer unemployed workers relative to open positions and therefore reflects broad local labor scarcity. The second is the lagged total crime rate in the firm's headquarters state, which proxies for the prevalence of potential workers with criminal histories. The two measures capture distinct dimensions of the mechanism: labor-market tightness indicates when access to additional applicants is more valuable, while crime prevalence indicates when delaying criminal-history screening has greater scope to expand the effective applicant pool.

The evidence is consistent with both predictions. The estimated effect of BTB is larger among firms in tighter labor markets and in areas with higher crime rates, where criminal-history screening is more likely to exclude potential applicants. The differences across subsamples are statistically significant at the 5% level or better in three of the four models.

4.3.2 Exposure to Exemption-Likely Occupations

BTB should have weaker effects where criminal-history screening remains legally required, highly institutionalized, or central to the job, such as in occupations involving healthcare, childcare, education, security, law enforcement, fiduciary responsibilities, vulnerable populations, and other safety-sensitive activities. In these settings, employers have less scope to alter when or how criminal-history information enters the hiring process, so the valuation effects of BTB should be smaller for firms with greater exposure to such occupations.

Columns 5–8 of Table 4 evaluate this prediction using two measures of exemption-likely workforce exposure. The first, *BTB Exempt OES*, classifies occupations as exemption-likely when they involve legal, licensing, fiduciary, safety, or vulnerable-population responsibilities for which criminal background checks are likely to be required or are highly institutionalized, or

when they are explicitly exempt from BTB-style restrictions.[6] Using Occupational Employment Statistics (OES) data, we calculate the employment-weighted share of these occupations within each four-digit NAICS industry and assign this exposure to firms. The second measure, *BTB Exempt Revelio*, applies the same classification to Revelio Labs worker histories and calculates the firm-level share of employees in exemption-likely occupations.[7]

The valuation effect of BTB is substantially larger among firms with low exposure to exemption-likely occupations under both measures. The differences across subsamples are statistically significant at the 5% level or better in all four models. This attenuation where background screening remains legally required or operationally central is consistent with BTB having larger effects when it more meaningfully changes applicant evaluation.

4.3.3 Demand for Additional Labor

The benefits of improved labor acquisition should also depend on firms' demand for workers. Firms with weak product demand or limited expansion opportunities may derive little benefit from evaluating additional applicants, whereas growing firms must add labor to support production and organizational capacity. For these firms, difficulty filling positions can constrain growth and increase the cost of pursuing valuable opportunities.

Columns 1–4 of Table 5 examine this prediction using lagged employment and sales growth. Employment growth captures recent workforce expansion, while sales growth captures increases in output and product demand that may require additional staffing. The valuation effect is substantially larger among firms with high employment and sales growth, with statistically significant differences between high- and low-growth firms in all four models.

[6] The indicator is not intended to establish that every job in an occupation is legally exempt in every jurisdiction. Rather, it identifies occupations in which BTB is likely to bind less because background screening remains legally required, operationally central, or strongly protected by regulation or licensing.

[7] Appendix A details how the occupation-level classification is aggregated to construct both measures.

These results are consistent with improved labor acquisition being more valuable when firms have greater demand for workers.

4.3.4 Dependence on Labor as a Productive Input

Finally, the benefits of improved labor acquisition should depend on the firm's production technology. When labor can be readily replaced with capital or other inputs, an unfilled position may have limited consequences for output. When production depends heavily on human labor and capital is a poor substitute, staffing shortages can disrupt operations, constrain capacity, and increase operating costs.

Columns 5–8 of Table 5 evaluate this prediction using two measures. Lagged SG&A intensity captures reliance on labor-intensive organizational, administrative, sales, and service activities. The substitutability of labor with automated capital ($SLAC$) measure of Bates, Du, and Wang (2025) captures the availability of a technological substitute for labor. The valuation effect is larger among firms with higher SG&A intensity and lower $SLAC$, with differences statistically significant at the 1% level or better in three of the four models. These results are consistent with labor-acquisition frictions having larger effects when firms depend more heavily on labor and have less ability to substitute capital for it.

Taken together, the cross-sectional evidence is consistent with the valuation gains from BTB being concentrated both where improved labor acquisition should be most consequential and where the law has greater scope to alter applicant screening.

5 Labor Acquisition, Operating Performance, and Risk

The previous evidence shows that BTB adoption increases firm value, with larger valuation gains when firms face greater constraints in acquiring labor and depend more heavily on labor as a productive input. We next examine the mechanisms underlying these effects. We

first test whether BTB alleviates hiring frictions and leads to more durable employment relationships. We then examine whether these changes are followed by stronger operating performance and lower risk, providing evidence on the expected-cash-flow and discount-rate channels through which improved labor acquisition can affect firm value.

5.1 Labor Acquisition

We first examine the most direct implication of the proposed mechanism: whether delaying early-stage criminal-history inquiry makes it easier for firms to acquire workers. If BTB expands firms' effective applicant pools by letting employers evaluate applicants with richer information before considering criminal history, treated firms should face fewer labor shortages and fill open positions more quickly.

To test this prediction, we use four complementary measures of firms' difficulty acquiring labor. *LaborShort* is a text-based measure of labor-shortage exposure constructed from earnings conference-call transcripts using the FinBERT approach of Harford et al. (2026). *LaborFriction* measures the intensity of labor-friction language in firms' Item 1A risk-factor disclosures, defined as the number of sentences containing both a labor-related and a friction-related term, scaled by total Item 1A sentences and multiplied by 1,000. Using Revelio Labs job-posting data, we also construct $Ln(DaysToFill)$ as the natural logarithm of the average number of days that a firm's job postings remain open before being filled and *HardToFill* as the share of positions that remain open beyond 90 days.

Columns 1–8 of Table 6 report the results. Across all four outcomes, BTB adoption is followed by a statistically significant decline in firms' difficulty acquiring labor. Compared to their respective sample means, the coefficients indicate that after BTB is adopted, *LaborShort*, *LaborFriction*, *DaysToFill*, and *HardToFill* decline by 19.8%, 5.5%–6.1%, 5.8%–8.1%, and

26.3%–27.4%, respectively.[8] Together, these conceptually distinct measures provide consistent evidence that BTB alleviates firms' labor-acquisition frictions. Figure 3 presents the corresponding dynamic treatment effects.[9] The estimates show no systematic pre-treatment trends, with the improvements emerging only after adoption.

5.2 Worker Separations and Match Durability

Faster vacancy filling need not imply better labor acquisition if firms achieve it by accepting poorer employment matches. Under this explanation, vacancies may close more quickly, but subsequent turnover should increase. We therefore examine whether worker-firm matches formed after BTB adoption become more durable. We use individual employment histories from Revelio Labs to identify employment episodes and measure whether those relationships end shortly after hiring. We combine overlapping positions and positions separated by short administrative gaps into a single continuous episode so that promotions, title changes, and internal transfers are not classified as exits. We treat episodes that remain open at the end of the sample as right-censored and do not classify them as separations.

Columns 9–12 of Table 6 report the results. The outcomes include the fraction of employment relationships that end within 12 or 24 months. The results show statistically significant declines in worker separations. Compared to their sample means, the coefficients indicate that after BTB is adopted, *NewSep1Yr* and *NewSep2Yr* decline by 3.6%–5.7% and 1.5%–2.7%, respectively. These early-tenure results are particularly informative because they are difficult to reconcile with faster vacancy filling arising from firms simply accepting poorer employment matches. Under that explanation, early separations should increase; instead, they decline.

[8] The agreement across disclosure-based and job-posting measures is informative because the outcomes are constructed from independent data sources, making it less likely that their common decline reflects disclosure drift or compositional changes within any single data source.

[9] Because the Revelio Labs job-posting data begin in 2016, we use a ±3-year window for the dynamic treatment-effect analyses involving these measures.

Figure 3 also presents the corresponding dynamic treatment effects. Separation rates exhibit no systematic pre-treatment trends and decline following adoption. Together with the vacancy-filling results in Section 5.1, these findings indicate that BTB is associated with both faster labor acquisition and more durable employment relationships.

5.3 Cash-Flow Channel: Productivity and Profitability

If BTB improves firms' ability to acquire and retain productive workers, these labor-market effects should ultimately translate into stronger operating performance. Filling vacancies more quickly can reduce output losses from understaffing, while more durable matches can lower recruiting and replacement costs and allow firms to use labor more productively. We therefore examine whether productivity and profitability increase following BTB adoption.

Table 7 examines the effects of BTB adoption on firm productivity using three outcomes: total factor productivity (TFP); the natural logarithm of sales per employee ($Ln(Sales/Emp)$); and the natural logarithm of the sales-to-assets ratio ($Ln(Sales/Assets)$). We find statistically significant increases, at least at the 10% level, in all three measures after the adoption of BTB laws. In terms of economic significance, the results indicate that TFP, sales-per-employee, and sales-per-dollar-of-assets increase by approximately 5.0%–5.3%, 2.3%–3.2%, and 5.7%–6.3%, respectively.

Table 8 examines whether higher operating cash flows and lower costs accompany stronger labor acquisition and productivity. The outcomes are operating return on assets ($OROA$); cash flow scaled by average assets ($CashFlow$); operating profit margin (OPM); and SG&A expenses scaled by sales ($SG\&A$). We find statistically significant improvements in all four measures after BTB adoption. Because these variables can take negative values, we assess the economic magnitudes of $OROA$, $CashFlow$, and OPM relative to their sample standard deviations. The estimates correspond to increases of 13.5%–14.9%, 12.9%–15.6%, and 8.6%–

9.0%, respectively. Compared to the sample mean of *SG&A*, the results indicate a decline of 7.5%–9.2% after BTB adoption. Figures 4 and 5 present the corresponding dynamic treatment effects. Across outcomes, the estimates show no systematic pre-treatment trends, with operating performance improving following BTB adoption.

Taken together, the operating results are consistent with improved labor acquisition allowing firms to use existing resources more efficiently and generate stronger cash flows. The simultaneous increase in productivity and decline in SG&A intensity are difficult to reconcile with BTB primarily imposing compliance costs or inducing less productive hiring decisions. Although SG&A is too broad to isolate recruiting costs directly, its decline is consistent with lower costs associated with repeated recruiting, onboarding, and vacancy management.

5.4 Discount-Rate Channel: Risk and Implied Cost of Equity

Improved labor acquisition can also affect firm value by reducing uncertainty about future cash flows. Firms that cannot reliably fill positions face greater uncertainty about their ability to maintain production, meet customer demand, and pursue growth opportunities. If BTB makes labor acquisition more reliable, treated firms should therefore experience lower firm-specific risk, financial-distress risk, and required returns.[10]

Table 9 reports estimates for four outcomes: total stock-return volatility (*TotRisk*); idiosyncratic stock-return volatility (*IdioRisk*); the 12-month expected default frequency, *ExpDefault*; and the implied COE, *ImpCOE*, which is the average of four estimates constructed from models based on analyst earnings forecasts. Across all four outcomes, BTB adoption is followed by significantly lower risk and COE. Compared to their sample means, the coefficients indicate that after BTB is adopted, *TotRisk*, *IdioRisk*, *ExpDefault*, and *ImpCOE* decrease by

[10]Consistent with this interpretation, Section 5.1 shows that BTB adoption reduces firms' Item 1A disclosures of labor-related frictions. Because these disclosures reflect management's discussion of staffing constraints and associated operating concerns, their decline provides complementary evidence that firms perceive fewer constraints on obtaining the workers needed for ongoing operations and growth.

1.7%–3.3%, 2.7%–4.9%, 20.8%–30.2%, and 2.9%–3.0%, respectively. Figure 6 presents the corresponding dynamic treatment effects. The pre-treatment coefficients do not exhibit a systematic trend. Following BTB adoption, the risk and implied-COE measures decline.

The consistency across these outcomes strengthens the interpretation of the results. Total and idiosyncratic volatility capture realized equity-market risk, expected default captures financial-distress risk, and implied COE provides a forward-looking measure of required returns. Their common decline, together with the earlier reduction in labor-friction disclosures, suggests a more reliable labor-acquisition process reducing firms' exposure to operating risk.

Overall, the findings trace a coherent path from applicant screening to firm value. Delaying an early-stage screen improves firms' ability to acquire labor and is followed by more durable employment relationships, stronger operating performance, and lower risk. Together, these changes provide evidence of both the expected-cash-flow and discount-rate channels underlying the increase in firm value.

6 Alternative Explanations and Robustness

Our findings suggest that BTB adoption increases firm value through a labor-acquisition mechanism. We next examine the robustness of this interpretation to several potential threats to identification and measurement.

Pre-Trends and Endogenous Adoption: BTB laws may be adopted in jurisdictions where firm value is already improving or where local conditions are changing in ways that independently affect valuation. The dynamic estimates in Figures 2–6 show no systematic differential trends before BTB adoption, providing initial evidence against this concern.

We also examine whether observable firm and state characteristics predict adoption timing using hazard models estimated at the state-year level. States remain in the risk set until adoption, and we lag all explanatory variables by one year. Table OA1 shows that the

state-level average of Tobin's Q does not predict subsequent BTB adoption. More broadly, the economic, political, labor-market, and demographic characteristics in our control set do not significantly predict adoption timing; only $Ln(UnempBen)$ is marginally significant at the 10% level. With roughly 15 state-level adoption events, however, these tests have limited statistical power, so we treat them as one piece of evidence among the broader identification checks in this section. We also find that other state policies related to the information environment, employment and social policy, and innovation and investment do not explain BTB adoption.

Finally, Figure OA1 reports placebo tests in which we randomly reassign BTB adoption years across jurisdictions and reestimate the baseline valuation specifications 1,000 times. The actual treatment effects lie in the extreme upper tails of the placebo distributions, indicating that estimates of similar magnitude rarely arise from randomly assigned treatment timing. Together, the absence of pre-treatment trends, the weak predictive power of observable adoption determinants, and the placebo evidence reduce concerns that the valuation effects reflect endogenous policy timing rather than BTB adoption itself.

Contemporaneous Policy Changes, Observable Treatment Selection, and Regional Shocks: BTB adoption may also coincide with broader policy changes or geographic economic conditions that independently affect firm value. We address this concern in three ways. First, we further control for contemporaneous state policies related to the information environment, employment and social policy, and innovation and investment.[11] Columns 1–2 of Table OA2 show that the valuation effects remain positive and statistically significant after conditioning on these policies. Second, in columns 3–4, we reweight observations using inverse-probability weights based on each firm's estimated likelihood of treatment, thereby reducing observable differences between treated and untreated observations in firm and state characteristics. The resulting estimates remain similar to the baseline effects.

[11] These laws include anti-SLAPP laws, data-breach notification laws, the FOIA law index, minimum-wage laws, pre-employment credit-check restrictions, paid family and medical leave, paid sick leave, salary-history bans, intellectual-property protections, and investor tax credits.

Third, columns 5–6 introduce U.S. Census region-by-year fixed effects, which absorb time-varying shocks common to states within each of the four Census regions. Identification therefore comes from differences in BTB exposure among firms located within the same broad region in the same year. The estimated effects remain positive and statistically significant. Together, these findings indicate that the valuation results are unlikely to be driven by contemporaneous policy changes, observable treatment selection, or broader time-varying regional conditions correlated with BTB adoption.

Alternative Staggered-Adoption Estimators: Our results could also depend on the imputation estimator used in the baseline analysis. We therefore show in Table OA3 that the valuation effects remain positive and statistically significant, at least at the 10% level, when we reestimate the effects using the two-way Mundlak estimator of Wooldridge (2025) and the interaction-weighted estimator of Sun and Abraham (2021).

Treatment Strength, Political Environment, and Voluntary Adoption: We next examine whether the valuation effects vary with the extent to which BTB changes applicant screening and whether they are confined to particular political environments. Table OA4 first shows that the effects are larger for firms subject to stronger, more binding mandates, with the difference between high- and low-strength laws statistically significant once we include firm and state controls (column 2). This dose-response pattern is consistent with greater valuation effects when BTB imposes a larger change in screening practices.

Because BTB adoption may also correlate with broader political characteristics of adopting jurisdictions, we further examine whether the estimated effects depend on the political affiliation of the state governor. If the baseline results primarily reflected unobserved economic, regulatory, or social conditions associated with Democratic-led states rather than the change in applicant screening itself, we would expect the valuation effect to be concentrated in those states. Instead, Table OA4 shows positive and statistically significant effects under both Democratic and non-Democratic governors.

Voluntary adoption provides a complementary test of whether statutory BTB adoption captures a meaningful change in firms' screening practices. Firms that had already delayed criminal-history inquiry before becoming legally required to do so were partially treated before statutory adoption, which should attenuate estimates based on the timing of the legal mandate. We identify 27 such firms; Online Appendix OA discusses prominent corporate fair-chance commitments, including cases in which a firm removed the box before any applicable mandate. We find that excluding them leaves the results intact and produces slightly larger coefficients (untabulated), consistent with voluntary pre-adoption attenuating rather than generating the baseline estimates.

Geographic Measurement of Treatment Exposure: Headquarters location may imperfectly capture the laws governing a firm's actual hiring activity. Publicly traded firms often operate across multiple jurisdictions, so a BTB law covering the headquarters location may apply to only part of the firm's workforce. We address this concern in two complementary ways. First, we replace the headquarters-based treatment indicator with continuous measures of firm-level BTB exposure based on the locations of the firm's establishments and employees. Using Data Axle, we construct an employment-weighted measure of the share of a firm's establishments located in BTB-covered jurisdictions. Using Revelio Labs, we construct a corresponding employment-weighted measure based on employee locations. Table OA5 shows that the estimates remain positive and statistically significant, at least at the 10% level, for both exposure measures.

Second, we examine whether the headquarters-based estimates remain when headquarters location is more likely to reflect the firm's operating exposure. Table OA6 reports these tests. Panel A reestimates the baseline models after excluding firms in geographically dispersed industries, firms with above-median PP&E, firms with an above-median number of employees, and firms operating in an above-median number of states (García and Norli, 2012). Panel B uses Data Axle establishment data and retains firms with at least 50%, 75%, or 95% of their

workers located in the headquarters state. Across these increasingly restrictive samples, the valuation effects remain positive and statistically significant and are generally larger among the most geographically concentrated firms. Together, these tests show that the results persist under workforce-based measures of legal exposure and strengthen when headquarters location more closely reflects firms' actual workforce exposure to BTB.

Alternative Measurement of Firm Value: Finally, we examine whether the valuation results hold under alternative measures of firm value. Conventional Tobin's Q may mismeasure replacement costs because book assets do not fully reflect internally generated intangible capital, a concern that may be especially relevant if improved labor acquisition affects organizational or knowledge capital. We therefore reestimate the baseline specification using *Total Q* from Peters and Taylor (2017), which incorporates both physical and intangible capital in the replacement-cost denominator. Because Bartlett and Partnoy (2020) point out that Tobin's Q can change due to denominator effects rather than changes in value, we additionally use the natural logarithm of the market value of equity. Lastly, we examine annual realized returns and the characteristic-adjusted abnormal returns of Daniel, Grinblatt, Titman, and Wermers (1997), providing complementary measures of changes in shareholder wealth that do not rely on an accounting-based asset denominator. Table OA7 shows positive and statistically significant effects, at least at the 10% level, across *Total Q*, market value of equity, realized returns, and characteristic-adjusted returns.

Overall, these analyses provide little support for the alternative explanations. The findings support the interpretation that the valuation gains reflect changes in the timing of criminal-history inquiry during applicant screening rather than pre-existing trends, endogenous policy selection, treatment mismeasurement, or estimator choice.

7 Conclusion

We examine whether early applicant screening affects firm value. We exploit the staggered adoption of BTB laws, which delay criminal-history inquiry until later in the hiring process, to identify the firm-level consequences of delaying an economically important early-stage applicant screen. We find that BTB adoption increases Tobin's Q, with similar gains in the market value of equity and stock returns. The valuation gains are largest when access to labor is especially valuable and when delaying criminal-history inquiry is most likely to affect applicant evaluation. Consistent with improved labor acquisition, treated firms report fewer labor shortages, fill vacancies more quickly, and experience fewer early worker separations. These improvements in labor acquisition are followed by higher productivity, profitability, and cash flow, together with lower SG&A intensity, volatility, expected default frequency, and implied COE. Overall, the evidence indicates that delaying an early-stage applicant screen can increase firm value through both expected-cash-flow and discount-rate channels.

Our findings identify early applicant screening as an economically important source of labor-acquisition frictions and establish firms' ability to acquire labor as a determinant of firm value. Whereas prior research primarily examines how labor-market institutions affect firms through incumbent workers, we study a logically prior margin: how firms identify, evaluate, and acquire prospective workers. Our evidence shows that frictions arising before an employment relationship begins can affect labor acquisition, operating performance, risk, and shareholder value. Because we do not observe the criminal-history status or demographic characteristics of marginal applicants and hires, our analysis speaks to the firm-side consequences of BTB rather than its distributional effects across workers. More broadly, the findings show that the sequencing of information acquisition inside firms can affect their ability to acquire productive inputs and, ultimately, shareholder value.

References

Acharya, Sushant, and Shu Lin Wee, 2020, Rational inattention in hiring decisions, *American Economic Journal: Macroeconomics* 12, 1–40.

Acharya, Viral V., Ramin P. Baghai, and Krishnamurthy V. Subramanian, 2014, Wrongful discharge laws and innovation, *Review of Financial Studies* 27, 301–346.

Agan, Amanda, and Sonja Starr, 2018, Ban the Box, criminal records, and racial discrimination: A field experiment, *Quarterly Journal of Economics* 133, 191–235.

Agrawal, Ashwini K., and David A. Matsa, 2013, Labor unemployment risk and corporate financing decisions, *Journal of Financial Economics* 108, 449–470.

Ashraf, Musaib, and Jayanthi Sunder, 2023, Can shareholders benefit from consumer protection disclosure mandates? Evidence from data breach disclosure laws, *The Accounting Review* 98, 1–32.

Avery, Beth, and Han Lu, 2021, Ban the Box: U.S. cities, counties, and states adopt fair-chance policies to advance employment opportunities for people with past convictions, Policy guide, National Employment Law Project.

Baghai, Ramin P., Rui C. Silva, Viktor Thell, and Vikrant Vig, 2021, Talent in distressed firms: Investigating the labor costs of financial distress, *Journal of Finance* 76, 2907–2961.

Bai, John (Jianqiu), Douglas Fairhurst, and Matthew Serfling, 2020, Employment protection, investment, and firm growth, *Review of Financial Studies* 33, 644–688.

Baker, Andrew C., David F. Larcker, and Charles C. Y. Wang, 2022, How much should we trust staggered difference-in-differences estimates?, *Journal of Financial Economics* 144, 370–395.

Bartlett, Robert, and Frank Partnoy, 2020, The misuse of Tobin's Q, *Vanderbilt Law Review* 73, 353–424.

Bates, Thomas W., Fangfang Du, and Jessie Jiaxu Wang, 2025, Workplace automation and corporate liquidity policy, *Management Science* 71, 1287–1314.

Belo, Frederico, Vito D. Gala, Juliana Salomao, and Maria Ana Vitorino, 2022, Decomposing firm value, *Journal of Financial Economics* 143, 619–639.

Bena, Jan, Hernán Ortiz-Molina, and Elena Simintzi, 2022, Shielding firm value: Employment protection and process innovation, *Journal of Financial Economics* 146, 637–664.

Bennett, Benjamin, Isil Erel, Léa Stern, and Zexi Wang, 2026, Paid leave pays off: The effects of paid family leave on firm performance, *Review of Finance* 30, 887–919.

Bharath, Sreedhar T., and Tyler Shumway, 2008, Forecasting default with the Merton distance to default model, *Review of Financial Studies* 21, 1339–1369.

Borusyak, Kirill, Xavier Jaravel, and Jann Spiess, 2024, Revisiting event-study designs: Robust and efficient estimation, *Review of Economic Studies* 91, 3253–3285.

Brown, Jennifer, and David A. Matsa, 2016, Boarding a sinking ship? An investigation of job applications to distressed firms, *Journal of Finance* 71, 507–550.

Burton, Anne M., and David N. Wasser, 2025, Revisiting the unintended consequences of Ban the Box, *Journal of Public Economics* 250, 105473.

Bushway, Shawn, Irineo Cabreros, Jessica Welburn Paige, Daniel Schwam, and Jeffrey B. Wenger, 2022, Barred from employment: More than half of unemployed men in their 30s had a criminal history of arrest, *Science Advances* 8, eabj6992.

Cai, Xiaoming, Pieter Gautier, and Ronald Wolthoff, 2025, Search, screening, and sorting, *American Economic Journal: Macroeconomics* 17, 205–236.

Chunyu, Liangrong, Paolo F. Volpin, and Xingchen Zhu, 2026, Do paid sick leave mandates increase productivity?, *Journal of Finance* forthcoming.

Claus, James, and Jacob Thomas, 2001, Equity premia as low as three percent? Evidence from analysts' earnings forecasts for domestic and international stock markets, *Journal of Finance* 56, 1629–1666.

Cordis, Adriana S., Hunghua Pan, Jin Zhang, and Po-Hsuan Hsu, 2025, Freedom of information, regulation enforcement, and corporate pollution, Working Paper, Winthrop University.

Craigie, Terry-Ann, 2020, Ban the Box, convictions, and public employment, *Economic Inquiry* 58, 425–445.

Cullen, Zoë B., Will Dobbie, and Mitchell Hoffman, 2023, Increasing the demand for workers with a criminal record, *Quarterly Journal of Economics* 138, 103–150.

Daniel, Kent, Mark Grinblatt, Sheridan Titman, and Russ Wermers, 1997, Measuring mutual fund performance with characteristic-based benchmarks, *Journal of Finance* 52, 1035–1058.

Davis, Steven J., R. Jason Faberman, and John C. Haltiwanger, 2013, The establishment-level behavior of vacancies and hiring, *Quarterly Journal of Economics* 128, 581–622.

Doleac, Jennifer L., and Benjamin Hansen, 2020, The unintended consequences of "Ban the Box": Statistical discrimination and employment outcomes when criminal histories are hidden, *Journal of Labor Economics* 38, 321–374.

Easton, Peter D., 2004, PE ratios, PEG ratios, and estimating the implied expected rate of return on equity capital, *The Accounting Review* 79, 73–95.

Ersahin, Nuri, Ruidi Huang, and Naveen Khanna, 2025, How do competition and reputation affect startup and VC behavior?, Working Paper, Southern Methodist University.

García, Diego, and Øyvind Norli, 2012, Geographic dispersion and stock returns, *Journal of Financial Economics* 106, 547–565.

Gavazza, Alessandro, Simon Mongey, and Giovanni L. Violante, 2018, Aggregate recruiting intensity, *American Economic Review* 108, 2088–2127.

Gebhardt, William R., Charles M. C. Lee, and Bhaskaran Swaminathan, 2001, Toward an implied cost of capital, *Journal of Accounting Research* 39, 135–176.

Goodman-Bacon, Andrew, 2021, Difference-in-differences with variation in treatment timing, *Journal of Econometrics* 225, 254–277.

Guernsey, Scott, Matthew Serfling, and Cheng Yan, 2026a, Marijuana legalization and firms' cost of equity, *Journal of Financial and Quantitative Analysis* 61, 1112–1147.

Guernsey, Scott, Matthew Serfling, and Cheng Yan, 2026b, When speaking freely pays: Anti-SLAPP laws and firms' cost of equity, Working Paper, University of Tennessee.

Harford, Jarrad, Qiyang He, and Buhui Qiu, 2026, Firm-level labor shortage exposure, *Review of Financial Studies* forthcoming.

Hoffman, Mitchell, Lisa B. Kahn, and Danielle Li, 2018, Discretion in hiring, *Quarterly Journal of Economics* 133, 765–800.

Holzer, Harry J., Steven Raphael, and Michael A. Stoll, 2006, Perceived criminality, criminal background checks, and the racial hiring practices of employers, *Journal of Law and Economics* 49, 451–480.

Huang, Fali, and Peter Cappelli, 2010, Applicant screening and performance-related outcomes, *American Economic Review* 100, 214–218.

İmrohoroğlu, Ayşe, and Şelale Tüzel, 2014, Firm-level productivity, risk, and return, *Management Science* 60, 2073–2090.

Jeffers, Jessica S., 2024, The impact of restricting labor mobility on corporate investment and entrepreneurship, *Review of Financial Studies* 37, 1–44.

Kaestner, Robert, and Xufei Wang, 2024, Ban-the-Box laws: Fair and effective?, *International Review of Law and Economics* 78, 106192.

Koren, Moran, 2026, The gatekeeper effect: The implications of pre-screening, self-selection, and bias for hiring processes, *Management Science* 72, 426–441.

Lageson, Sarah Esther, Mike Vuolo, and Christopher Uggen, 2015, Legal ambiguity in managerial assessments of criminal records, *Law & Social Inquiry* 40, 175–204.

Levashina, Julia, and Michael A. Campion, 2009, Expected practices in background checking: Review of the human resource management literature, *Employee Responsibilities and Rights Journal* 21, 231–249.

Mayda, Anna Maria, Francesc Ortega, Giovanni Peri, Kevin Shih, and Chad Sparber, 2023, Coping with H-1B shortages: Firm performance and mitigation strategies, *ILR Review* 76, 919–943.

Merz, Monika, and Eran Yashiv, 2007, Labor and the market value of the firm, *American Economic Review* 97, 1419–1431.

Mortensen, Dale T., and Christopher A. Pissarides, 1994, Job creation and job destruction in the theory of unemployment, *Review of Economic Studies* 61, 397–415.

Ohlson, James A., and Beate E. Juettner-Nauroth, 2005, Expected EPS and EPS growth as determinants of value, *Review of Accounting Studies* 10, 349–365.

Papich, Sarah, 2025, Do employer credit check bans increase default?, Working Paper, University of California, Santa Barbara.

Peters, Ryan H., and Lucian A. Taylor, 2017, Intangible capital and the investment-q relation, *Journal of Financial Economics* 123, 251–272.

Raphael, Steven, 2021, The intended and unintended consequences of Ban the Box, *Annual Review of Criminology* 4, 191–207.

Rose, Evan K., 2021, Does Banning the Box help ex-offenders get jobs? Evaluating the effects of a prominent example, *Journal of Labor Economics* 39, 79–113.

Serfling, Matthew, and Wenxuan Sun, 2026, Silencing salary talk: How salary history bans affect corporate investment and performance, Working Paper, University of Tennessee.

Shen, Mo, 2021, Skilled labor mobility and firm value: Evidence from green card allocations, *Review of Financial Studies* 34, 4663–4700.

Shoag, Daniel, and Stan Veuger, 2021, Ban-the-Box measures help high-crime neighborhoods, *Journal of Law and Economics* 64, 85–105.

Simintzi, Elena, Vikrant Vig, and Paolo Volpin, 2015, Labor protection and leverage, *Review of Financial Studies* 28, 561–591.

Suh, Paula, 2023, Intellectual property rights and debt financing, *Review of Financial Studies* 36, 1970–2003.

Sun, Liyang, and Sarah Abraham, 2021, Estimating dynamic treatment effects in event studies with heterogeneous treatment effects, *Journal of Econometrics* 225, 175–199.

Wooldridge, Jeffrey M., 2025, Two-way fixed effects, the two-way Mundlak regression, and difference-in-differences estimators, *Empirical Economics* 69, 2545–2587.

Appendix A: Variable Definitions

This appendix provides variable definitions. Variables not included here are defined in the corresponding table captions. Compustat and CRSP variable names are italicized when appropriate.

%ΔGDPpc: One-year percent change in per capita state-level gross domestic product.

AbReturn: Annual characteristic-adjusted abnormal stock return (Daniel et al., 1997).

Age: State average resident age. Data are from the U.S. Census.

Anti-SLAPP: Indicator variable that equals one if a firm is covered by an anti-SLAPP law enacted by the close of fiscal year t, and zero otherwise, following Guernsey, Serfling, and Yan (2026b).

Assets: Book value of total assets (at), in millions of 2025 dollars.

BTB: Indicator variable that equals one if a firm is covered by a private-sector Ban-the-Box law enacted by the close of fiscal year t, and zero otherwise.

BTB Exempt OES: Employment-weighted share of exemption-likely occupations within a firm's four-digit NAICS industry, based on Occupational Employment Statistics (OES) data. Using ChatGPT, we code an occupation as exemption-likely if its title or occupational classification indicates that the job involves healthcare or patient care, childcare, education, eldercare, social services involving vulnerable populations, law enforcement, security, public safety, aviation or transportation safety, financial fiduciary responsibility, or other positions in which criminal background checks are likely mandated, highly institutionalized, or explicitly exempt from BTB-style restrictions. The occupation-level exemption-likely classification is aggregated to the four-digit NAICS level using 2004 occupational employment weights, measured before the broad adoption of private-sector BTB laws, and the resulting industry-level exposure is assigned to firms.

BTB Exempt Revelio: Firm-level share of employees in exemption-likely occupations, based on Revelio Labs worker histories. The same occupation-level classification is applied to workers' observed roles and aggregated to the firm using the number of employees observed in each role in the full Revelio sample, which maximizes coverage and reduces measurement error from attrition, selection, and changes in Revelio coverage over time.

BTB Strength: Ordinal 0–4 score grading the applicable private-sector BTB law by breadth of employer coverage and procedural stringency (the stage at which the criminal-history inquiry is permitted, whether an individualized assessment is required, and the strength of enforcement remedies), hand-coded from statutory features following Avery and Lu (2021).

CashFlow: Operating cash flow scaled by average total assets $[(ib{+}dp)/((at_t + at_{t-1})/2)]$.

CrimeRate: State-level total number of crimes reported in the Uniform Crime Report per 100,000 population.

DaysToFill: Average number of days a firm's posted vacancies remain open before being filled. Data are from Revelio Labs job-posting data.

DBN: Indicator variable that equals one if a firm is covered by a Data Breach Notification (DBN) law enacted by the close of fiscal year t, and zero otherwise, following Ashraf and Sunder (2023).

DemGov: Indicator variable that equals one if the state governor is from the Democratic Party, and zero otherwise.

EDUC: Fraction of a state's population aged 25 or older holding a bachelor's degree or higher. Data are from the U.S. Census.

EG: Firm employment growth, measured as the one-year change in the natural logarithm of total employees, $Ln(emp_t) - Ln(emp_{t-1})$.

Emp: Total number of employees, in thousands (emp).

ExpDefault: Expected default frequency, computed following Bharath and Shumway (2008) as the probability of default within one year implied by a Merton-style credit model.

Female: Fraction of a state's population that is female. Data are from the U.S. Census.

FirmAge: Number of years since the firm first appeared in Compustat.

FOIA: Freedom of Information Act (FOIA) index value at the end of the year, following Cordis, Pan, Zhang, and Hsu (2025).

GDPpc: Per capita state-level gross domestic product in 2025 dollars. Data are from the U.S. Bureau of Economic Analysis.

HardToFill: Firm-year share of a firm's job positions that remain open beyond 90 days. Data are from Revelio Labs job-posting data.

HQ Employee Share: Share of a firm's employees located in establishments in its headquarters state, computed from establishment-level employment counts.

IdioRisk: Annualized standard deviation of weekly idiosyncratic stock returns, measured as the residuals from a regression of a firm's weekly stock returns on the CRSP value-weighted market portfolio returns over the year.

ImpCOE: Implied cost of equity (COE), defined as the average of the four implied-COE estimates from the models of Gebhardt, Lee, and Swaminathan (2001), Claus and Thomas (2001), Easton (2004), and Ohlson and Juettner-Nauroth (2005).

IPRP: Indicator variable that equals one if a firm is covered by an Intellectual Property Rights Protection (IPRP) law enacted by the close of fiscal year t, and zero otherwise, following Suh (2023).

ITC: Indicator variable that equals one if a firm is covered by an Investor Tax Credit (ITC) law enacted by the close of fiscal year t, and zero otherwise, following Ersahin, Huang, and Khanna (2025).

LaborFriction: Labor-friction sentence intensity, defined as the number of sentences in a firm's 10-K Item 1A risk-factor disclosure that contain both a labor-related term and a friction-related term, scaled by total Item 1A sentences and multiplied by 1,000.

LaborShort: Text-based labor-shortage measure constructed by applying the FinBERT approach to earnings conference-call transcripts (Harford et al., 2026).

Marijuana: Indicator variable that equals one if a firm is headquartered in a state that has legalized medical marijuana by year t, and zero otherwise, following Guernsey, Serfling, and Yan (2026a).

MinWage: State-level minimum wage at the end of the year.

MVE: Market value of equity at the end of the fiscal year ($prcc_f \times csho$), in millions of 2025 dollars.

NBLEV: Net book leverage, defined as the book value of debt ($dltt+dlc$) minus cash and equivalents (che), scaled by the book value of assets (at).

NewSep1Yr: Fraction of employment relationships formed in a given hire year that end within 12 months. Constructed from Revelio Labs individual employment histories only for hire cohorts for which the full 12-month follow-up window is observable.

NewSep2Yr: Fraction of employment relationships formed in a given hire year that end within 24 months. Constructed from Revelio Labs individual employment histories only for hire cohorts for which the full 24-month follow-up window is observable.

OPM: Operating profit margin, defined as operating income before depreciation ($oibdp$) divided by sales ($sale$). The variable is winsorized at the 5th and 95th percentiles.

OROA: Operating return on assets, defined as operating income before depreciation ($oibdp$) divided by average assets (at).

PECCB: Indicator variable that equals one if a firm is covered by a Pre-Employment Credit Check Ban (PECCB) law enacted by the close of fiscal year t, and zero otherwise, following Papich (2025).

PFML: Indicator variable that equals one if a firm is covered by a Paid Family and Medical Leave (PFML) law enacted by the close of fiscal year t, and zero otherwise, following Bennett, Erel, Stern, and Wang (2026).

PP&E: Net property, plant, and equipment ($ppent$), in 2025 dollars.

PSL: Indicator variable that equals one if a firm is covered by a Paid Sick Leave (PSL) law enacted by the close of fiscal year t, and zero otherwise, following Chunyu, Volpin, and Zhu (2026).

Return: Annual realized stock return, computed as the buy-and-hold return over the year.

Sales/Assets: Sales scaled by average total assets $[sale/((at_t + at_{t-1})/2)]$.

Sales/Emp: CPI-deflated sales scaled by the average number of employees $[(sale/cpi)/((emp_t + emp_{t-1})/2)]$. If the average number of employees is less than 50, the variable is set to missing.

SG: Sales growth, measured as the one-year change in the natural logarithm of sales, $Ln(sale_t) - Ln(sale_{t-1})$.

SG&A: Selling, general, and administrative expense ($xsga$) scaled by sales ($sale$).

SHB: Indicator variable that equals one if a firm is covered by a Salary History Ban (SHB) law enacted by the close of fiscal year t, and zero otherwise, following Serfling and Sun (2026).

SLAC: Substitutability of Labor with Automated Capital (Bates et al., 2025). Higher (lower) values indicate a greater (lesser) ability to replace labor with automated capital.

Tangibility: Net property, plant, and equipment scaled by total assets ($ppent/at$).

TaxRate: State-level highest marginal corporate income-tax rate. Data are from the Book of the States.

TFP: Total factor productivity, following İmrohoroğlu and Tüzel (2014).

TotRisk: Annualized standard deviation of weekly stock returns over the year.

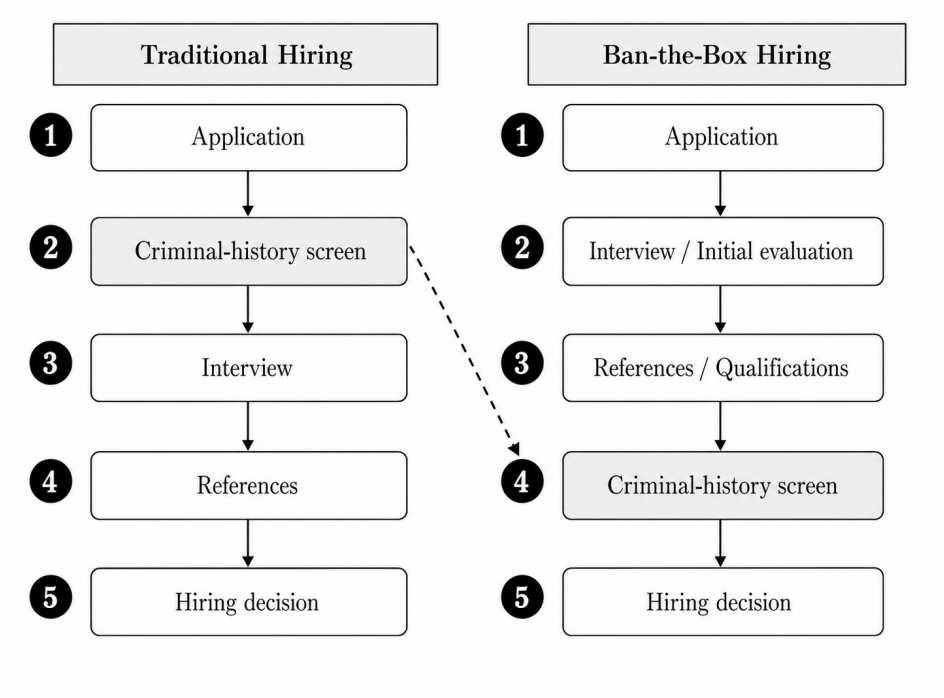
TQ: Tobin's Q, defined as the market value of assets ($prcc_f \times csho + at - ceq$), divided by the book value of assets (at).

UnempBen: State unemployment-insurance benefits, defined as the maximum weekly benefit times the maximum number of weeks. Data are from the U.S. Department of Labor.

UnempRate: State-level unemployment rate, defined as the fraction of the labor force that is unemployed. Data are from the U.S. Bureau of Labor Statistics (BLS).

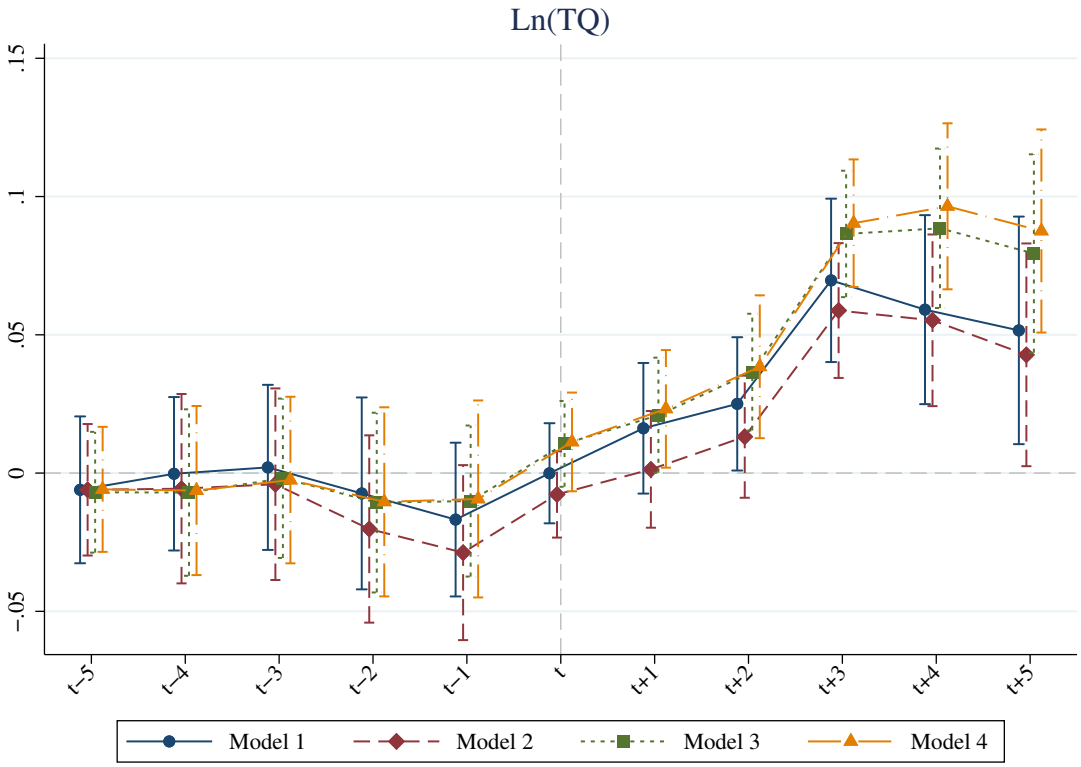
Unemp/Vacancy: State-year ratio of unemployed persons to job vacancies, constructed from BLS state-level unemployment data and state-level vacancy data from the Job Openings and Labor Turnover Survey (JOLTS).

Figure 1: Timing of Applicant Evaluation Before and After Ban-the-Box



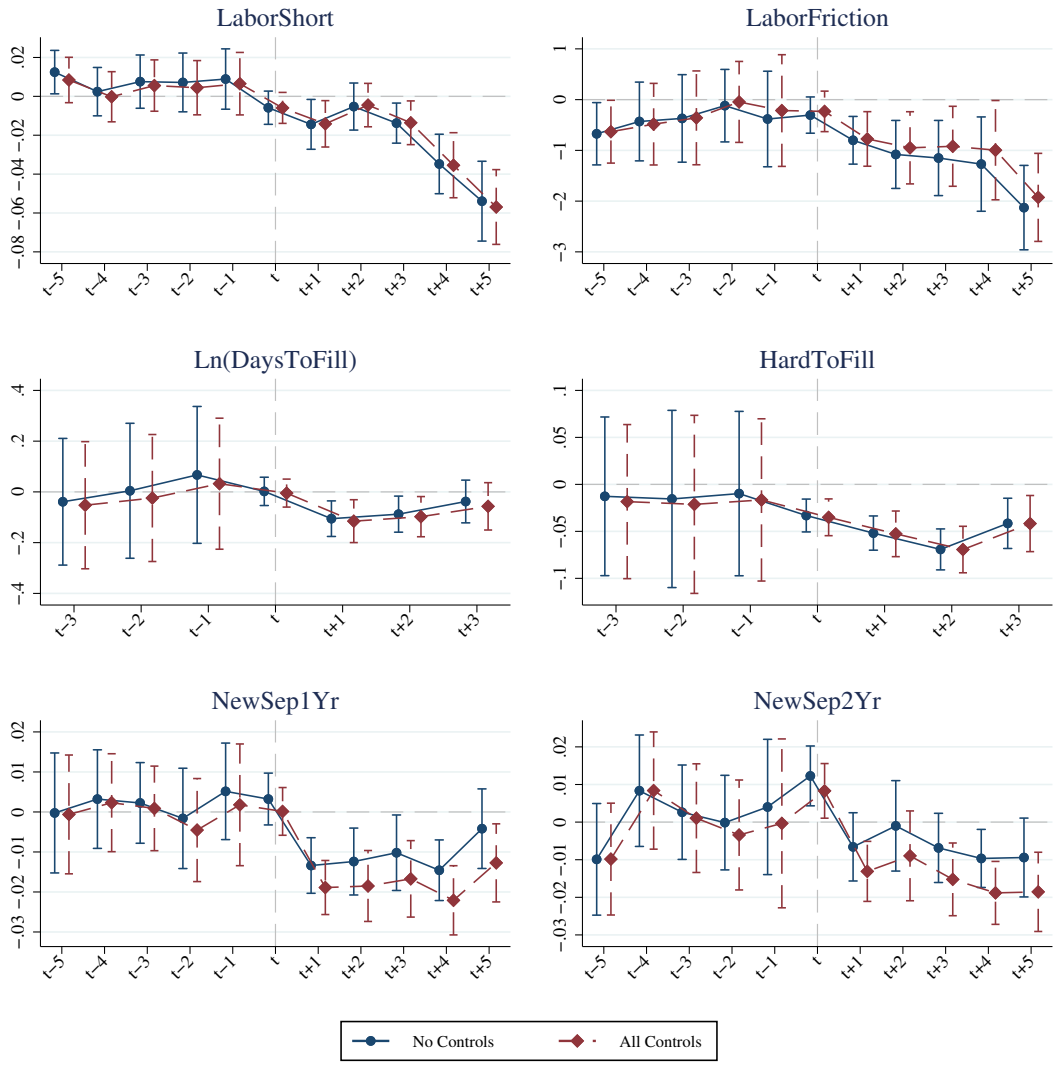
This figure illustrates how BTB laws alter the sequence of applicant evaluation. The laws delay one early-stage applicant screen without generally restricting employers' ultimate access to criminal-history information.

Figure 2: Dynamic Effect of BTB Laws on Firm Value



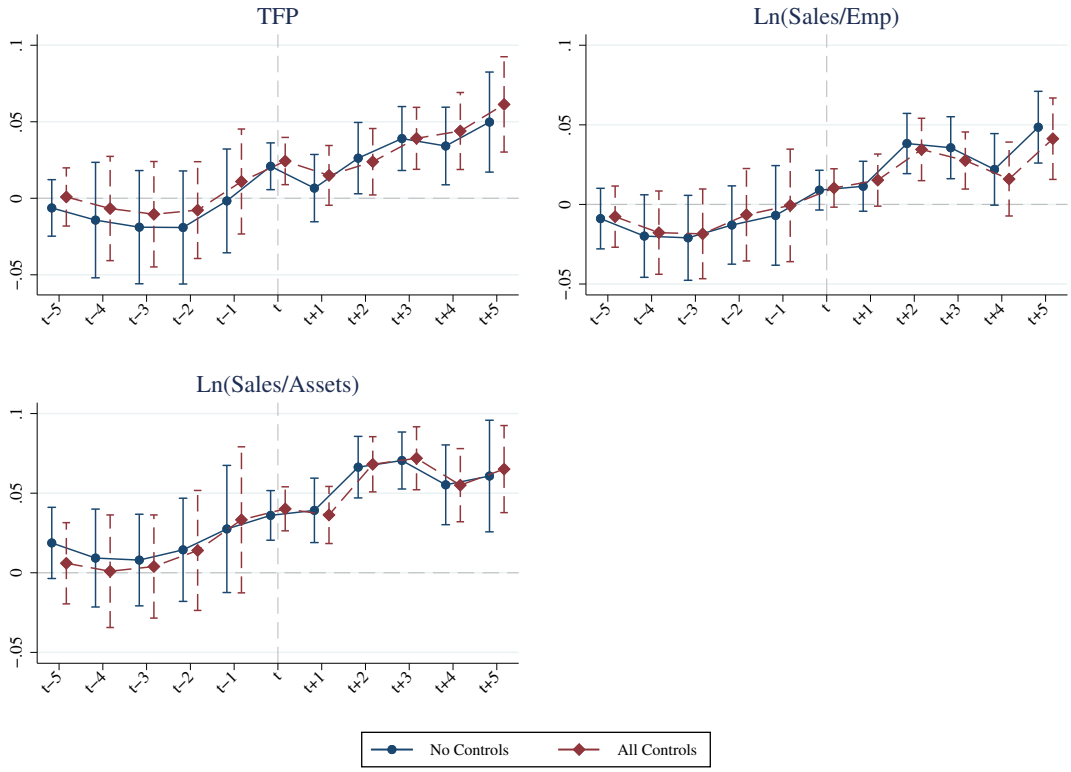
This figure plots the dynamic effect of BTB on firm value ($Ln(TQ)$), estimated using the imputation-based DiD method, and corresponds to the specifications in Table 3. The horizontal axis indicates the number of years relative to BTB adoption in the firm's headquarters jurisdiction, with event time zero corresponding to the year of adoption. Lagged firm control variables include $Ln(Assets)$, *Tangibility*, *CashFlow*, *NBLEV*, and $Ln(FirmAge)$. Lagged state control variables include $Ln(GDPpc)$, $\%\Delta GDPpc$, *TaxRate*, *UnempRate*, *DemGov*, $Ln(UnempBen)$, *Age*, *EDUC*, *Female*, and *Marijuana*. All variables are defined in Appendix A. 95% confidence intervals based on standard errors clustered by state are reported.

Figure 3: Dynamic Effect of BTB Laws on Labor Frictions and Worker Separations



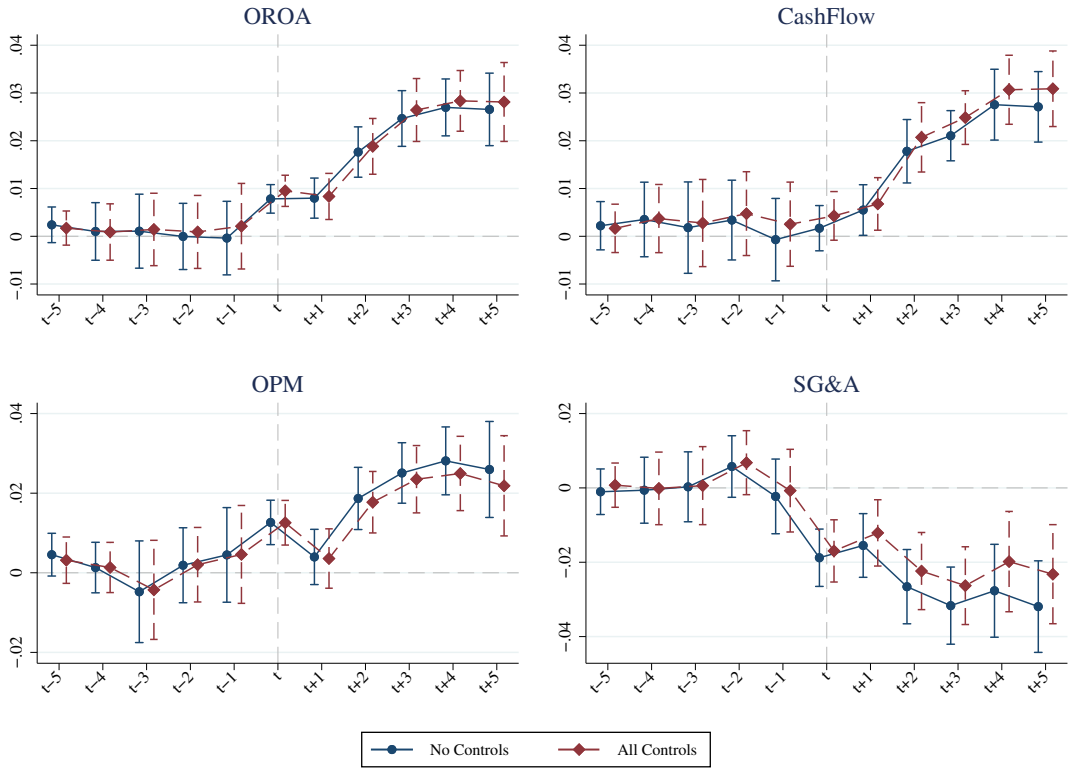
This figure plots the dynamic effect of BTB on hiring-friction outcomes (*LaborShort* and *LaborFriction* over the period 2005 to 2024, and *Ln*(*DaysToFill*) and *HardToFill* over the period 2016 to 2024 due to the availability of Revelio Labs data) and worker-separation outcomes (*NewSep1Yr* and *NewSep2Yr* over the period 2005 to 2024), estimated using the imputation-based DiD method. The horizontal axis indicates the number of years relative to BTB adoption in the firm's headquarters jurisdiction, with event time zero corresponding to the year of adoption. Lagged firm control variables include *Ln*(*Assets*), *Tangibility*, *CashFlow*, *NBLEV*, and *Ln*(*FirmAge*). Lagged state control variables include *Ln*(*GDPpc*), *%ΔGDPpc*, *TaxRate*, *UnempRate*, *DemGov*, *Ln*(*UnempBen*), *Age*, *EDUC*, *Female*, and *Marijuana*. All variables are defined in Appendix A. 95% confidence intervals based on standard errors clustered by state are reported.

Figure 4: Dynamic Effect of BTB Laws on Productivity



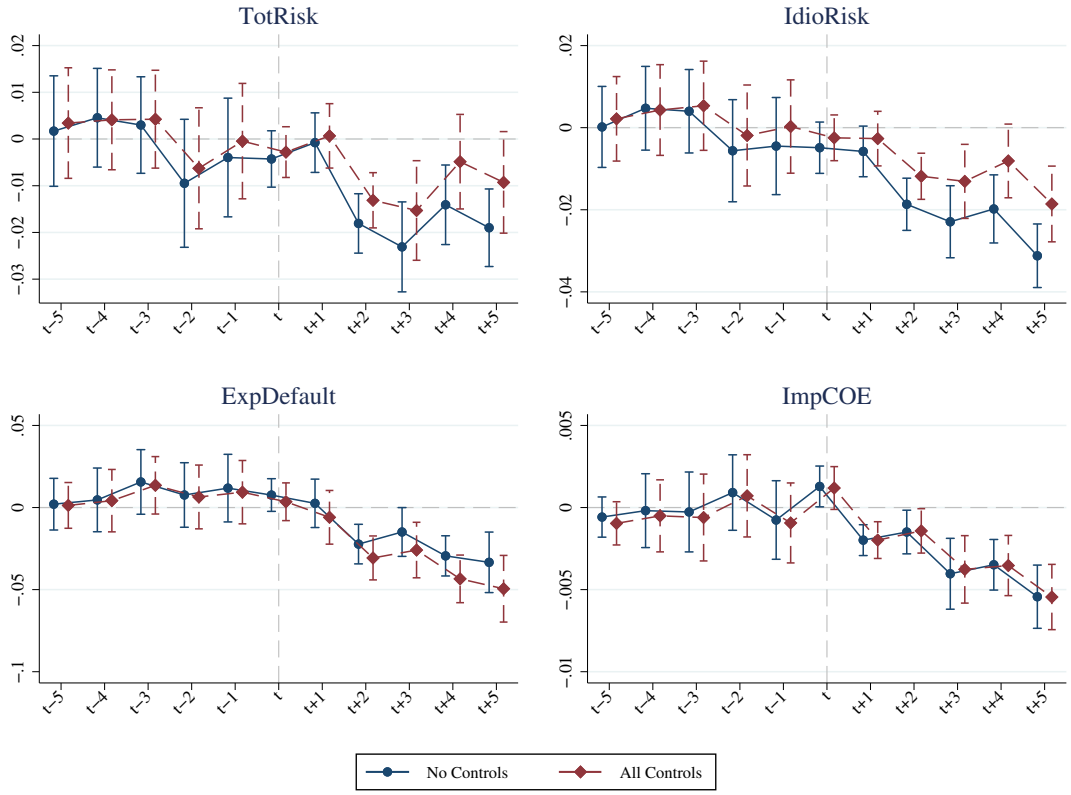
This figure plots the dynamic effect of BTB on productivity outcomes (*TFP*, *Ln*(*Sales*/*Emp*), and *Ln*(*Sales*/*Assets*)) over the period 2005 to 2024, estimated using the imputation-based DiD method. The horizontal axis indicates the number of years relative to BTB adoption in the firm's headquarters jurisdiction, with event time zero corresponding to the year of adoption. Lagged firm control variables include *Ln*(*Assets*), *Tangibility*, *CashFlow*, *NBLEV*, and *Ln*(*FirmAge*). Lagged state control variables include *Ln*(*GDPpc*), *%*Δ*GDPpc*, *TaxRate*, *UnempRate*, *DemGov*, *Ln*(*UnempBen*), *Age*, *EDUC*, *Female*, and *Marijuana*. All variables are defined in Appendix A. 95% confidence intervals based on standard errors clustered by state are reported.

Figure 5: Dynamic Effect of BTB Laws on Profitability and Cost Efficiency



This figure plots the dynamic effect of BTB on profitability and cost-efficiency outcomes (*OROA*, *CashFlow*, *OPM*, and *SG&A*) over the period 2005 to 2024, estimated using the imputation-based DiD method. The horizontal axis indicates the number of years relative to BTB adoption in the firm's headquarters jurisdiction, with event time zero corresponding to the year of adoption. Lagged firm control variables include *Ln*(*Assets*), *Tangibility*, *NBLEV*, and *Ln*(*FirmAge*). Lagged state control variables include *Ln*(*GDPpc*), *%*Δ*GDPpc*, *TaxRate*, *UnempRate*, *DemGov*, *Ln*(*UnempBen*), *Age*, *EDUC*, *Female*, and *Marijuana*. All variables are defined in Appendix A. 95% confidence intervals based on standard errors clustered by state are reported.

Figure 6: Dynamic Effect of BTB Laws on Risk and Implied Cost of Equity



This figure plots the dynamic effect of BTB on firm risk measures and the implied cost of equity (COE) (*TotRisk*, *IdioRisk*, *ExpDefault*, and *ImpCOE*) over the period 2005 to 2024, estimated using the imputation-based DiD method. The horizontal axis indicates the number of years relative to BTB adoption in the firm's headquarters jurisdiction, with event time zero corresponding to the year of adoption. Lagged firm control variables include *Ln*(*Assets*), *Tangibility*, *CashFlow*, *NBLEV*, and *Ln*(*FirmAge*). Lagged state control variables include *Ln*(*GDPpc*), *%*Δ*GDPpc*, *TaxRate*, *UnempRate*, *DemGov*, *Ln*(*UnempBen*), *Age*, *EDUC*, *Female*, and *Marijuana*. All variables are defined in Appendix A. 95% confidence intervals based on standard errors clustered by state are reported.

Table 1: BTB Adoption Dates Used in the Empirical Analysis

This table reports the state-, district-, city-, and county-level BTB private-sector adoption dates that underlie our treatment indicator. Treatment is assigned to a firm-year by the earliest BTB law that applies to the firm's historical headquarters jurisdiction. The treatment year used in our regressions equals the enactment/signing year of that law. Adoption dates are hand-collected from state and local legislative records and cross-referenced against the National Employment Law Project's fair-chance hiring guide (Avery and Lu, 2021) and prior academic studies (Doleac and Hansen, 2020; Burton and Wasser, 2025). Maryland's law (SB 839/HB 994) was enacted by legislative override of a gubernatorial veto on January 30, 2020, and took effect February 29, 2020. Colorado's law took effect September 1, 2019, for employers with 11 or more employees and September 1, 2021, for all employers. One law adopted after the 2024 sample end (marked "outside sample") is listed for completeness and does not enter the treatment indicator.

State	Jurisdiction	Level	Signing	Effective	Policy/Law Name
Panel A. State-level private-sector laws					
California	Statewide	State	2017	2018	Assembly Bill 1008 (Fair Chance Act)
Colorado	Statewide	State	2019	2019	House Bill 19-1025
Connecticut	Statewide	State	2016	2017	House Bill 5237
District of Columbia	Districtwide	District	2014	2014	Fair Criminal Record Screening Amendment Act of 2014
Hawaii	Statewide	State	1998	1998	HRS §378-2.5
Illinois	Statewide	State	2014	2015	HB 5701 (JOQAA)
Maine	Statewide	State	2021	2021	Legislative Document 1167
Maryland	Statewide	State	2020	2020	SB 839 / HB 994
Massachusetts	Statewide	State	2010	2010	SB 2583 (Ch. 256) – CORI Reform
Minnesota	Statewide	State	2013	2014	Senate File 523
New Jersey	Statewide	State	2014	2015	AB 1999 / SB 2586 (Opportunity to Compete Act)
New Mexico	Statewide	State	2019	2019	Senate Bill 96
Oregon	Statewide	State	2015	2016	House Bill 3025
Rhode Island	Statewide	State	2013	2014	House Bill 5507
Vermont	Statewide	State	2016	2017	House Bill 261
Washington	Statewide	State	2018	2018	House Bill 1298
Panel B. City- and county-level ordinances when they predate (or coincide with) the state law or no statewide law exists					
California	San Francisco	City	2014	2014	Fair Chance Ordinance
California	Los Angeles	City	2016	2017	Ordinance No. 184652
Florida	Gainesville	City	2022	2022	Ordinance No. 2022-617
Illinois	Chicago	City	2014	2015	Ord. O2014-8347 (Mun. Code §2-160-054)
Iowa	Waterloo	City	2019	2020	Ordinance No. 5522

Continued on next page

State	Jurisdiction	Level	Signing	Effective	Policy/Law Name
Maryland	Baltimore	City	2014	2014	Fair Criminal-Record Screening Practices (Ord. 14-221)
Maryland	Montgomery County	County	2014	2015	Bill No. 36-14
Maryland	Prince George's County	County	2014	2015	CB-96-2014
Missouri	Columbia	City	2014	2014	City Council Ordinance
Missouri	Kansas City	City	2018	2018	Ordinance 180034
Missouri	St. Louis	City	2020	2021	Ordinance 71074
New Jersey	Newark	City	2012	2012	Newark Ord. #12-1630
New York	Buffalo	City	2013	2014	City Ordinance Amendment
New York	Rochester	City	2014	2014	City Ordinance
New York	New York City	City	2015	2015	Fair Chance Act (Local Law)
New York	Westchester County	County	2018	2019	Local Law No. 10913-2018
New York	Suffolk County	County	2020	2020	Local Law No. 14-2020
Oregon	Portland	City	2015	2016	City Ordinance
Pennsylvania	Philadelphia	City	2011	2011	Fair Criminal Record Screening Standards Ordinance (Bill 110111-A)
Pennsylvania	Chester County	County	2025	2025	Ord. No. ORD-2025-03 (outside sample)
Texas	Austin	City	2016	2016	Ordinance No. 20160324-019
Texas	DeSoto	City	2021	2022	Ordinance No. 2231-21
Washington	Seattle	City	2013	2013	Fair Chance Employment Ordinance
Washington	Spokane	City	2017	2018	Ordinance C35564

Table 2: Summary Statistics

This table reports summary statistics for the variables used in our main analyses over the full sample period from 2005 to 2024. Continuous variables are winsorized at their 1st and 99th percentiles unless otherwise indicated, and dollar values are expressed in 2025 dollars. Appendix A provides variable definitions.

	Obs	Mean	SD	P25	P50	P75
TQ	37,525	2.108	1.517	1.194	1.606	2.403
LaborShort	25,660	0.101	0.221	0.000	0.000	0.098
LaborFriction	35,557	22.93	14.70	12.50	20.94	31.25
DaysToFill	11,742	50.52	27.97	33.08	46.31	61.74
HardToFill	11,742	0.186	0.180	0.068	0.146	0.247
NewSep1Yr	31,062	0.331	0.236	0.179	0.286	0.426
NewSep2Yr	30,001	0.515	0.243	0.353	0.500	0.667
TFP	29,294	-0.357	0.565	-0.594	-0.317	-0.053
Sales/Emp	36,162	665.3	880.7	261.4	407.1	671.9
Sales/Assets	37,543	1.053	0.730	0.550	0.875	1.362
OROA	37,590	0.107	0.148	0.071	0.121	0.175
CashFlow	37,600	0.064	0.147	0.040	0.085	0.131
OPM	37,534	0.118	0.233	0.062	0.129	0.213
SG&A	35,554	0.295	0.316	0.117	0.220	0.367
TotRisk	37,614	0.425	0.207	0.280	0.377	0.512
IdioRisk	37,614	0.365	0.184	0.236	0.323	0.444
ExpDefault	31,016	0.106	0.255	0.000	0.000	0.018
ImpCOE	23,850	0.089	0.029	0.070	0.084	0.102
Assets	37,614	7,251	18,020	424.7	1,439	4,952
Tangibility	37,614	0.254	0.232	0.077	0.170	0.364
NBLEV	37,614	0.047	0.347	-0.171	0.085	0.284
FirmAge	37,614	25.00	17.48	11.00	20.00	35.00
GDPpc	37,614	78,883	12,429	70,091	77,667	85,933
%ΔGDPpc	37,614	0.013	0.026	0.000	0.016	0.030
TaxRate	37,614	6.480	2.936	5.500	7.100	8.840
UnempRate	37,614	5.986	2.173	4.400	5.400	7.300
DemGov	37,614	0.475	0.499	0.000	0.000	1.000
UnempBen	37,614	17,149	6,763	14,123	16,197	19,548
Age	37,614	37.58	1.954	36.25	37.53	38.93
EDUC	37,614	0.333	0.060	0.288	0.328	0.371
Female	37,614	0.515	0.008	0.510	0.515	0.520
Marijuana	37,614	0.446	0.497	0.000	0.000	1.000

Table 3: BTB Laws and Firm Value

This table reports results from the imputation-based DiD method examining the effect of BTB laws on firm value over the period 2005 to 2024. The dependent variable in columns 1–4 is the natural logarithm of Tobin's Q (TQ). ATT measures the average treatment effect of BTB adoption. Lagged firm control variables include $Ln(Assets)$, $Tangibility$, $CashFlow$, $NBLEV$, and $Ln(FirmAge)$. Lagged state control variables include $Ln(GDPpc)$, $\%\Delta GDPpc$, $TaxRate$, $UnempRate$, $DemGov$, $Ln(UnempBen)$, Age, $EDUC$, $Female$, and $Marijuana$. All variables are defined in Appendix A. t-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	$Ln(TQ)$			
	(1)	(2)	(3)	(4)
ATT	0.055***	0.047***	0.076***	0.082***
	(3.54)	(3.43)	(5.97)	(6.15)
SIC2 × Year FE	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓
DemGov × Year FE		✓	✓	✓
Firm Controls			✓	✓
State Controls				✓
N of Obs	37,525	37,525	37,525	37,525

Table 4: Heterogeneity by Labor-Market Tightness and Hiring Constraints

This table reports heterogeneous treatment effects from the imputation-based DiD method over the period 2005 to 2024, allowing the ATT to vary with *ex ante* local labor-market tightness, crime prevalence, and exposure to exemption-likely occupations. The dependent variable in columns 1–8 is the natural logarithm of Tobin's Q (TQ). *Unemp/Vacancy* is the lagged unemployment-to-vacancy ratio in the firm's headquarters state. *CrimeRate* is the total crime rate in the firm's headquarters state. *BTB Exempt OES* is an industry-level measure constructed from 2004 Occupational Employment Statistics (OES) data. We first classify occupations as BTB exemption-likely or not. We then compute the employment-weighted share of exemption-likely occupations within each four-digit NAICS industry using OES occupational employment weights and assign the resulting industry-level exposure to firms. *BTB Exempt Revelio* is a firm-level measure constructed from Revelio Labs worker histories. We classify each worker's occupation using the same occupation-based exemption-likely coding and compute the firm-level share of employees in exemption-likely roles. The Revelio measure is constructed using the full available worker sample to maximize coverage. For each split, firms are classified as "Low" or "High" based on the sample median. Lagged firm control variables include *Ln*(*Assets*), *Tangibility*, *CashFlow*, *NBLEV*, and *Ln*(*FirmAge*). Lagged state control variables include *Ln*(*GDPpc*), *%ΔGDPpc*, *TaxRate*, *UnempRate*, *DemGov*, *Ln*(*UnempBen*), *Age*, *EDUC*, *Female*, and *Marijuana*. All other variables are defined in Appendix A. t-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Ln(*TQ*)							
	Split by Unemp/Vacancy		Split by CrimeRate		Split by BTB Exempt OES		Split by BTB Exempt Revelio	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT – Low	0.085***	0.11***	0.049***	0.069***	0.100***	0.13***	0.081***	0.12***
	(4.63)	(6.77)	(3.84)	(4.57)	(4.43)	(6.79)	(4.29)	(5.69)
ATT – High	0.049***	0.059***	0.095***	0.16***	0.0015	0.017	0.0097	0.054***
	(4.13)	(5.59)	(3.35)	(6.86)	(0.10)	(1.01)	(0.43)	(2.94)
Low = High p-val	0.026	0.000	0.153	0.002	0.000	0.000	0.013	0.015
SIC2 × Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
DemGov × Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓		✓
N of Obs	36,115	36,115	35,951	35,951	35,841	35,841	32,824	32,824

Table 5: Heterogeneity by Firm Labor Demand, Labor Intensity, and SLAC

This table reports heterogeneous treatment effects from the imputation-based DiD method over the period 2005 to 2024, allowing the ATT to differ across firms with weaker versus stronger *ex ante* labor demand, labor intensity, and substitutability of labor with automated capital. The dependent variable in columns 1–8 is the natural logarithm of Tobin's Q (TQ). *EG* is a firm's lagged employment growth rate. *SG* is a firm's lagged sales growth rate. *SG&A* is a firm's lagged SG&A expenses scaled by sales. *SLAC* captures a firm's substitutability of labor with automated capital (Bates et al., 2025). For each split, firms are classified as "Low" or "High" based on the sample median. Lagged firm control variables include *Ln*(*Assets*), *Tangibility*, *CashFlow*, *NBLEV*, and *Ln*(*FirmAge*). Lagged state control variables include *Ln*(*GDPpc*), *%ΔGDPpc*, *TaxRate*, *UnempRate*, *DemGov*, *Ln*(*UnempBen*), *Age*, *EDUC*, *Female*, and *Marijuana*. All variables are defined in Appendix A. *t*-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Ln(*TQ*)							
	Split by EG		Split by SG		Split by SG&A		Split by SLAC	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT – Low	0.012	0.031**	0.018	0.037***	0.014	0.020	0.10***	0.12***
	(0.96)	(2.44)	(1.51)	(2.95)	(1.09)	(1.49)	(4.80)	(6.22)
ATT – High	0.100***	0.14***	0.098***	0.13***	0.062**	0.100***	0.0043	0.020
	(6.47)	(8.51)	(5.86)	(7.48)	(2.29)	(4.11)	(0.21)	(0.86)
Low = High *p*-val	0.000	0.000	0.000	0.000	0.103	0.003	0.004	0.001
SIC2 × Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
DemGov × Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓		✓
N of Obs	35,562	35,562	36,601	36,601	34,997	34,997	30,561	30,561

Table 6: BTB Laws, Labor Frictions, and Worker Separations

This table reports results from the imputation-based DiD method examining the effect of BTB laws on labor frictions (*LaborShort* and *LaborFriction* over the period 2005 to 2024, and *Ln*(*DaysToFill*) and *HardToFill* over the period 2016 to 2024 due to the availability of Revelio Labs data), and worker separations (*NewSep1Yr* and *NewSep2Yr* over the period 2005 to 2024). The dependent variables in columns 1–2, 3–4, 5–6, 7–8, 9–10, and 11–12 are *LaborShort*, a text-based labor-shortage measure constructed by applying the FinBERT approach to earnings conference-call transcripts (Harford et al., 2026); *LaborFriction*, the number of Item 1A sentences containing both a labor-related and a friction-related term, scaled by total Item 1A sentences and multiplied by 1,000; *Ln*(*DaysToFill*), the natural logarithm of the average number of days a posted vacancy remains open; *HardToFill*, the share of a firm's positions that remain open beyond 90 days; *NewSep1Yr*, the fraction of employment relationships formed in a given hire year that end within 12 months; and *NewSep2Yr*, the fraction that end within 24 months, respectively. The early-separation measures are constructed only for hire cohorts for which the full follow-up period is observable. *ATT* measures the average treatment effect of BTB adoption. Lagged firm control variables include *Ln*(*Assets*), *Tangibility*, *CashFlow*, *NBLEV*, and *Ln*(*FirmAge*). Lagged state control variables include *Ln*(*GDPpc*), *%ΔGDPpc*, *TaxRate*, *UnempRate*, *DemGov*, *Ln*(*UnempBen*), *Age*, *EDUC*, *Female*, and *Marijuana*. All variables are defined in Appendix A. *t*-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	LaborShort		*LaborFriction*		*Ln*(*DaysToFill*)		*HardToFill*		*NewSep1Yr*		*NewSep2Yr*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
ATT	-0.020***	-0.020***	-1.41***	-1.27***	-0.058**	-0.081***	-0.049***	-0.051***	-0.012***	-0.019***	-0.0075**	-0.014***
	(-4.24)	(-4.86)	(-4.53)	(-3.85)	(-2.51)	(-2.68)	(-5.62)	(-4.69)	(-4.55)	(-6.77)	(-2.20)	(-4.00)
SIC2 × Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
DemGov × Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓		✓		✓		✓
N of Obs	25,300	25,300	35,544	35,544	8,667	8,667	8,738	8,738	31,028	31,028	29,962	29,962

Table 7: BTB Laws and Productivity

This table reports results from the imputation-based DiD method examining the effect of BTB laws on firm productivity over the period 2005 to 2024. The dependent variables in columns 1–2, 3–4, and 5–6 are total factor productivity (*TFP*); the natural logarithm of sales per employee (*Sales/Emp*); and the natural logarithm of sales scaled by average total assets (*Sales/Assets*), respectively. *ATT* measures the average treatment effect of BTB adoption. Lagged firm control variables include *Ln*(*Assets*), *Tangibility*, *CashFlow*, *NBLEV*, and *Ln*(*FirmAge*). Lagged state control variables include *Ln*(*GDPpc*), *%ΔGDPpc*, *TaxRate*, *UnempRate*, *DemGov*, *Ln*(*UnempBen*), *Age*, *EDUC*, *Female*, and *Marijuana*. All variables are defined in Appendix A. *t*-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	TFP		$Ln(\frac{Sales}{Emp})$		$Ln(\frac{Sales}{Assets})$	
	(1)	(2)	(3)	(4)	(5)	(6)
ATT	0.050***	0.053***	0.032**	0.023*	0.057***	0.063***
	(3.22)	(3.61)	(2.49)	(1.66)	(4.52)	(5.47)
SIC2 × Year FE	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓
DemGov × Year FE	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓
N of Obs	28,984	28,984	36,133	36,133	37,537	37,537

Table 8: BTB Laws, Profitability, and Cost Efficiency

This table reports results from the imputation-based DiD method examining the effect of BTB laws on firm profitability and cost efficiency over the period 2005 to 2024. The dependent variables in columns 1–2, 3–4, 5–6, and 7–8 are operating return on assets (*OROA*); cash flow scaled by average total assets (*CashFlow*); operating profit margin (*OPM*); and SG&A expenses scaled by sales (*SG&A*), respectively. *ATT* measures the average treatment effect of BTB adoption. Lagged firm control variables include *Ln*(*Assets*), *Tangibility*, *NBLEV*, and *Ln*(*FirmAge*). Lagged state control variables include *Ln*(*GDPpc*), *%*Δ*GDPpc*, *TaxRate*, *UnempRate*, *DemGov*, *Ln*(*UnempBen*), *Age*, *EDUC*, *Female*, and *Marijuana*. All variables are defined in Appendix A. *t*-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	OROA		*CashFlow*		*OPM*		*SG&A*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT	0.020***	0.022***	0.019***	0.023***	0.021***	0.020***	-0.027***	-0.022***
	(7.18)	(7.32)	(6.59)	(7.56)	(5.16)	(4.58)	(-5.22)	(-4.01)
SIC2 × Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
DemGov × Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓		✓
N of Obs	37,590	37,590	37,600	37,600	37,528	37,528	35,464	35,464

Table 9: BTB Laws, Risk, and Implied Cost of Equity

This table reports results from the imputation-based DiD method examining the effect of BTB laws on firm risk and the implied COE over the period 2005 to 2024. The dependent variables in columns 1–2, 3–4, 5–6, and 7–8 are total stock-return volatility (*TotRisk*); idiosyncratic stock-return volatility (*IdioRisk*); the expected default frequency (*ExpDefault*); and the implied COE (*ImpCOE*), respectively. *ATT* measures the average treatment effect of BTB adoption. Lagged firm control variables include *Ln*(*Assets*), *Tangibility*, *CashFlow*, *NBLEV*, and *Ln*(*FirmAge*). Lagged state control variables include *Ln*(*GDPpc*), *%*Δ*GDPpc*, *TaxRate*, *UnempRate*, *DemGov*, *Ln*(*UnempBen*), *Age*, *EDUC*, *Female*, and *Marijuana*. All variables are defined in Appendix A. *t*-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	TotRisk		*IdioRisk*		*ExpDefault*		*ImpCOE*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT	-0.014***	-0.0074**	-0.018***	-0.0099***	-0.022***	-0.032***	-0.0027***	-0.0026***
	(-4.02)	(-2.09)	(-5.35)	(-3.04)	(-4.01)	(-5.01)	(-5.18)	(-4.24)
SIC2 × Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
DemGov × Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓		✓
N of Obs	37,614	37,614	37,614	37,614	30,415	30,415	23,577	23,577

Applicant Screening and Firm Value: Evidence from Ban-the-Box Laws

Online Appendix

Appendix OA: Detailed Institutional Background

This appendix provides additional institutional detail on Ban-the-Box (BTB) laws. It explains why the criminal background checkbox persisted at listed firms, what BTB laws do and do not restrict, how voluntary corporate adoption affects interpretation of the estimates, and why the laws represent binding policy shocks rather than symbolic regulation.

OA.1 Why the Box Exists and Why Firms Have Not Removed It Voluntarily

The criminal background checkbox is an institutional artifact whose persistence is not obvious. If criminal-history information were optimally used in screening, firms could choose when to acquire it during the hiring process. If it were not valuable, firms could remove it voluntarily. The fact that firms historically asked about criminal history on initial applications, that applicants were screened out before the interview stage on the basis of the checkbox, and that BTB laws required firms to alter hiring procedures suggests that the box represented a friction-laden equilibrium rather than a simple employer optimum.

The checkbox emerged in response to two competing legal pressures. On one side, the Supreme Court's decision in *Griggs v. Duke Power Co.*, 401 U.S. 424 (1971), and subsequent Equal Employment Opportunity Commission (EEOC) guidance exposed employers to disparate-impact liability for screening practices that disproportionately excluded racial minorities. On the other side, negligent-hiring doctrine, including *Ponticas v. K.M.S. Investments*, 331 N.W.2d 907 (Minn. 1983), exposed employers to liability for harms caused by employees whose criminal histories could have been discovered through reasonable screening. The checkbox served both functions: it documented that the employer asked about criminal history while giving the firm a basis to argue that any exclusion was job-related rather than intentionally discriminatory.

In practice, the box is typically a yes/no question about prior convictions, felony convictions, or pending charges. Some applications request additional information about the offense and date. Although many applications state that an affirmative answer does not automatically disqualify the applicant, empirical evidence shows that disclosure substantially reduces callback or interview probabilities (Holzer, Raphael, and Stoll, 2006; Agan and Starr, 2018). The EEOC's 2012 guidance further constrains blanket exclusion by encouraging individualized assessment of the nature of the offense, the time elapsed, and the relationship between the offense and the job.

The checkbox became embedded in large-firm hiring systems. Publicly traded firms commonly use applicant-tracking systems (ATS) in which the criminal-history field is coded as a binary screen. Depending on the position and firm policy, applications with an affirmative response can be routed to manual review or filtered out before the interview stage. BTB laws

force firms to reconfigure this screening sequence by delaying the criminal-history inquiry until later in the hiring funnel.

Several institutional frictions explain why firms did not remove the checkbox voluntarily. First, negligent-hiring liability creates a legal incentive to document that the firm inquired about criminal history. Second, ATS architecture often hard-codes the criminal-history field into application forms and screening logic, so removal requires coordinated information-technology changes across many business units and job templates. Third, hiring authority at multistate listed firms is decentralized, making uniform voluntary removal difficult without a corporate-level legal and compliance directive. Fourth, unilateral removal creates a coordination problem: a firm that removes the box before competitors may attract a different applicant pool while retaining negligent-hiring risk. Fifth, federal and state statutes mandate criminal background checks for many regulated jobs, including banking, healthcare, childcare, security, aviation, and law-enforcement-related positions. Finally, national firms often prefer a single application and ATS configuration; before BTB became widespread, that standardization favored retaining the checkbox, whereas after adoption spread, it favored adopting fair-chance procedures nationally.

These frictions imply that BTB laws are meaningful shocks for listed firms. The laws cut through coordination problems, provide legal cover for removing the initial checkbox, and require firms to alter the sequence of applicant screening. The firms most likely to be affected are therefore large employers whose ATS architecture, decentralized hiring practices, and tort exposure had preserved the checkbox.

OA.2 What BTB Laws Do, and What They Do Not Do

BTB laws do not generally prohibit employers from considering criminal history. They regulate the timing and procedural treatment of criminal-history information. The core provision is a delay in when employers may ask about criminal history. Some laws permit inquiry after an initial interview; stricter laws, such as California's Fair Chance Act, Hawaii's statute, and New York City's Fair Chance Act, delay inquiry until after a conditional offer of employment. The economic intensity of the law is increasing in the strictness of this timing rule because a later inquiry requires firms to evaluate more applicants before using criminal history as a screen.

After the permitted point in the hiring process, employers may usually conduct background checks and consider criminal-history information. What changes is the order in which information enters the screening decision. Pre-BTB, an applicant with a record can be excluded at the application stage before the employer observes qualifications, job fit, or evidence of rehabilitation. Post-BTB, that same applicant may reach an interview or conditional-offer stage, where the firm observes additional information before deciding whether the criminal history is job-relevant.

Many jurisdictions also require individualized assessment before adverse action. These assessments commonly consider the nature and gravity of the offense, the time elapsed, the

nature of the job, and evidence of rehabilitation or successful prior employment. Several jurisdictions require written notice before rescinding a conditional offer and provide applicants an opportunity to respond.

BTB laws routinely exempt sensitive positions for which state or federal law independently requires criminal-history screening or disqualification. Common exemptions include law enforcement, security, financial-services positions covered by Section 19 of the Federal Deposit Insurance Act, healthcare, childcare, eldercare, public schools, and positions involving fiduciary responsibility or vulnerable populations. Treatment intensity therefore varies across occupations and industries within treated jurisdictions.

Many BTB statutes also contain employer-size thresholds. New Jersey's statute applies to employers with 15 or more employees, and California's applies to employers with 5 or more. These thresholds are generally not binding for Compustat-listed firms, but they limit extrapolation to small private employers. Enforcement and penalty structures also vary. California enforces its Fair Chance Act through Fair Employment and Housing Act (FEHA) complaint procedures and compensatory remedies, Los Angeles County's 2024 fair-chance ordinance authorizes civil penalties of up to $20,000 per violation, and New York City authorizes penalties up to $250,000 for willful violations.

The key economic point is that timing matters even when information is eventually revealed. The pre-BTB checkbox is a high-volume early-stage filter. BTB changes which applicants reach interviews or conditional offers, allows firms to observe qualifications before criminal history is evaluated, and in many jurisdictions forces a categorical screen to become an individualized assessment. Thus, the law changes the order and context in which information is used, rather than the final information set available to the employer.

OA.3 Voluntary Multistate Adopters

Several large employers adopted fair-chance hiring practices before, or independently of, a private-sector BTB law applying to their headquarters jurisdiction. These cases are important because voluntary adoption attenuates the intent-to-treat contrast: some firms classified as untreated under a law-based treatment definition may already have removed the box.

Walmart. Walmart, the nation's largest private employer, signed the White House Fair Chance Business Pledge in August 2016.[12]

Target. In October 2013, Target announced that it would remove criminal-history questions from its applications nationwide, a change implemented in 2014. In April 2018, Target reached a $3.74 million class settlement in *Carnella Times et al. v. Target Corp.*, agreeing to provide priority hiring opportunities and monetary awards to class members who had been disqualified under Target's prior screening guidelines, retain independent consultants

[12]White House Press Office, "FACT SHEET: White House Announces New Commitments to the Fair Chance Business Pledge," August 16, 2016. https://obamawhitehouse.archives.gov/the-press-office/2016/08/16/fact-sheet-white-house-announces-new-commitments-fair-chance-business

to validate the revised guidelines, and fund nonprofits providing reentry services.[13] Target signed the White House Fair Chance Business Pledge in November 2016.

Bed Bath & Beyond. In April 2014, New York Attorney General Eric T. Schneiderman secured a $125,000 settlement with Bed Bath & Beyond after determining that the retailer automatically disqualified applicants with felony convictions without conducting the individualized assessment required by New York Correction Law Article 23-A. Unlike the other cases in this subsection, this settlement reflects a regulatory enforcement action for noncompliance with an existing individualized-assessment requirement rather than voluntary adoption of fair-chance hiring practices. The settlement included $40,000 in restitution to denied applicants and $15,000 to each of the Center for Employment Opportunities, the Osborne Association, and the Doe Fund.[14]

Koch Industries. Koch Industries, then the second-largest privately held company in the United States with about 60,000 U.S. employees, removed the criminal background checkbox from initial applications in March 2015. General Counsel Mark Holden stated: "As a large United States-based manufacturing company that employs 60,000 American workers, we shouldn't be rejecting people at the very start of the hiring process who may otherwise be capable and qualified, and want an opportunity to work hard."[15]

Starbucks. By 2015, Starbucks no longer inquired about criminal histories on initial U.S. job applications, and it was a founding signatory of the White House Fair Chance Business Pledge in April 2016.[16]

JPMorgan Chase. By 2019, a private-sector BTB law already applied to JPMorgan Chase's headquarters jurisdiction, as New York City's Fair Chance Act had taken effect in 2015. The bank's commitment therefore illustrates voluntary action that extended well beyond the local mandate rather than adoption ahead of any applicable law. On October 21, 2019, JPMorgan Chase announced that it had removed criminal-history questions from job applications nationwide and expanded its second-chance hiring effort. In 2018, the bank hired approximately 2,100 individuals with criminal backgrounds in the United States, roughly 10% of new U.S. hires. JPMorgan Chase pledged more than $7 million in community investment in Chicago, Detroit, New York, Seattle, Nashville, and Wilmington, and created the JPMorgan

[13] *The Washington Post*, "Target to pay $3.74 million to settle claims that it discriminated against black, Latino job applicants," April 5, 2018. The settlement covered approximately 41,500 class members. https://www.washingtonpost.com/news/business/wp/2018/04/05/target-to-pay-3-74-million-to-settle-claims-that-it-discriminated-against-black-latino-job-applicants/

[14] New York State Attorney General, press release, "A.G. Schneiderman Announces Settlement With Major Retailer To End Ban On Hiring Applicants With Criminal Convictions," April 22, 2014. https://ag.ny.gov/press-release/2014/ag-schneiderman-announces-settlement-major-retailer-end-ban-hiring-applicants

[15] *Fortune*, "Koch Industries stops asking job candidates about their criminal records," April 27, 2015. https://fortune.com/2015/04/27/koch-industries-stops-asking-job-candidates-about-their-criminal-records/; National Employment Law Project, "Koch Industries Decision to 'Ban the Box' Signals Broad, Growing Appeal for Fair-Hiring Practices," April 27, 2015. https://www.nelp.org/koch-industries-decision-ban-box-signals-broad-growing-appeal-fair-hiring-practices/

[16] White House Press Office, "FACT SHEET: White House Launches the Fair Chance Business Pledge," April 11, 2016. https://obamawhitehouse.archives.gov/the-press-office/2016/04/11/fact-sheet-white-house-launches-fair-chance-business-pledge

Chase PolicyCenter to support evidence-based fair-chance hiring policy.[17] The bank, regulated under Section 19 of the Federal Deposit Insurance Act, navigated the tension between federal hiring restrictions and its fair-chance commitment by limiting the policy to positions for which Section 19 waivers were not required.

The Fair Chance Business Pledge. The White House Fair Chance Business Pledge, launched on April 11, 2016, asked signatory employers to commit to fair-chance hiring practices including "banning the box," individualized criminal-history review, and partnerships with reentry organizations. The founding signatories included American Airlines, Busboys and Poets, The Coca-Cola Company, Facebook, Georgia-Pacific, Google, Greyston Bakery, The Hershey Company, Johns Hopkins Hospital and Health System, Koch Industries, Libra Group, PepsiCo, Plank Industries, Prudential, Starbucks, Uber, Under Armour, Unilever, and Xerox.[18] By August 2016, the count had grown to 185 employers, including Walmart and Dropbox, and by November 2016 it had grown to more than 300 employers, including Ben & Jerry's, CVS Health, Gap, Intel, Kroger, LinkedIn, Perdue Farms, Target, Tyson Foods, and WeWork, collectively employing more than five million workers.[19]

The voluntary-adoption evidence has two implications for the empirical design. First, these corporate decisions appear to have been driven by Title VII disparate-impact exposure, operational standardization, and corporate-citizenship concerns rather than by anticipated state legislative timing. Second, voluntary adoption, to the extent that it is unrelated to firms' counterfactual outcome trends, attenuates our law-based intent-to-treat estimates toward zero because some control firms may already have adopted fair-chance hiring practices.

OA.4 Federal Policy Benchmarks

Two federal developments provide useful benchmarks. The Fair Chance to Compete for Jobs Act of 2019, enacted as part of the National Defense Authorization Act for Fiscal Year 2020 and effective December 20, 2021, prohibits federal agencies and federal contractors from asking about criminal history before a conditional offer. The Act codifies remedies including

[17] JPMorgan Chase corporate communication, "JPMorgan Chase Joins Second Chance Efforts to Reduce Obstacles to Employment," October 21, 2019. https://www.jpmorganchase.com/newsroom/press-releases/2019/jpmorganchase-joins-second-chance-efforts-to-reduce-obstacles-to-employment; CBS News, "JPMorgan Chase hired more than 2,000 ex-convicts last year," October 22, 2019. https://www.cbsnews.com/news/jpmorgan-chase-hiring-people-with-criminal-records-through-second-chance-hiring-program/

[18] White House Press Office, "FACT SHEET: White House Launches the Fair Chance Business Pledge," April 11, 2016. https://obamawhitehouse.archives.gov/the-press-office/2016/04/11/fact-sheet-white-house-launches-fair-chance-business-pledge

[19] White House Press Office, "FACT SHEET: White House Announces New Commitments to the Fair Chance Business Pledge," August 16, 2016. https://obamawhitehouse.archives.gov/the-press-office/2016/08/16/fact-sheet-white-house-announces-new-commitments-fair-chance-business, and November 30, 2016. https://obamawhitehouse.archives.gov/the-press-office/2016/11/30/fact-sheet-white-house-announces-new-commitments-fair-chance-business

adverse action against federal employees who violate the prohibition and, for contractors, a written warning followed by authority to suspend payment under the contract.[20]

The Fair Hiring in Banking Act, enacted as part of the National Defense Authorization Act for Fiscal Year 2023, became immediately effective on December 23, 2022, substantially narrowing the scope of Section 19 of the Federal Deposit Insurance Act. The FDIC subsequently revised its implementing regulations, which became effective on October 1, 2024. A Section 19 waiver is no longer required if seven or more years have passed since the offense, if five or more years have passed since release from incarceration, or if the offense was committed when the individual was 21 years of age or younger and more than 30 months have elapsed since sentencing.[21] Although the banking reform applies to financial institutions, which our sample excludes, both federal policies illustrate the broader movement from categorical exclusion toward delayed inquiry and individualized assessment.

OA.5 State- and Jurisdiction-Level Enforcement

Published enforcement actions and statutory penalty structures indicate that BTB laws are binding constraints rather than symbolic policy statements. Five large jurisdictions are especially relevant.

California. California enacted its private-sector Fair Chance Act (AB 1008) in 2017, effective January 1, 2018. The law prohibits inquiry into conviction history before a conditional offer and requires individualized assessment before rescinding an offer. The California Civil Rights Department has brought enforcement actions against employers and staffing agencies for premature criminal-history inquiries.

New York City. New York City's Fair Chance Act, effective October 27, 2015, prohibits inquiry until after a conditional offer, incorporates the individualized-assessment factors of New York Correction Law Article 23-A, and authorizes civil penalties up to $250,000 for willful violations. The New York City Commission on Human Rights has actively enforced the statute through settlements and training requirements.

Massachusetts. Massachusetts adopted Criminal Offender Record Information (CORI) reform in 2010, removing the criminal background checkbox from initial employment applications and limiting the scope of background-check disclosures. Later amendments expanded individualized assessment and limited access to sealed records. The state attorney general has brought enforcement actions against employers, including national chains, for premature criminal-history inquiries.

New Jersey. The New Jersey Opportunity to Compete Act, effective March 1, 2015, prohibits covered employers from asking about criminal history during the initial application

[20] National Employment Law Project, "FAQ: Fair Chance to Compete for Jobs Act of 2019." https://www.nelp.org/insights-research/faq-fair-chance-to-compete-for-jobs-act-of-2019/

[21] Ogletree Deakins, "Congress Eases Criminal Offense Restrictions for Employment With Financial Institutions," January 26, 2023. https://ogletree.com/insights-resources/blog-posts/congress-eases-criminal-offense-restrictions-for-employment-with-financial-institutions/

process. The Commissioner of Labor and Workforce Development enforces the law through administrative penalties.

Illinois. Illinois adopted the Job Opportunities for Qualified Applicants Act in 2014, effective January 1, 2015. The statute applies to private employers with 15 or more employees and is enforced by the Illinois Department of Labor. Subsequent amendments to the Illinois Human Rights Act expanded individualized-assessment requirements.

Appendix OB: A Model of Early-Stage Applicant Screening and Firm Value

This appendix presents a model linking the sequencing of applicant screening to firm value. The model emphasizes a tradeoff between screening costs and access to suitable workers. An early-stage screen allows the firm to reject applicants inexpensively, but it may also remove productive applicants before the firm observes richer information about worker-firm fit. Delaying the screen increases evaluation costs but can increase the rate at which vacancies are filled, improve match quality, and reduce uncertainty about future labor input.

OB.1 Setup

Consider a firm with a vacancy. Time is continuous, and future cash flows are discounted at rate $\rho > 0$. While the position remains vacant, the firm incurs a flow cost $\kappa > 0$. This cost captures forgone production, recruiting expenditures, disruption to existing workers, and the loss of growth opportunities that cannot be pursued without the required labor input.

Applicants arrive according to a Poisson process with rate $\lambda > 0$. Each applicant has an underlying worker-firm match value. The firm does not observe this value immediately. Instead, it evaluates applicants under one of two screening regimes:

$$j \in \{E, D\},$$

where E denotes early criminal-history screening (the regime without a BTB law) and D denotes delayed criminal-history screening (the regime under a BTB law).

Under regime j, an arriving applicant produces an acceptable match with probability p_j. Thus, acceptable matches arrive at rate

$$\mu_j = \lambda p_j.$$

Delayed screening allows more applicants to receive fuller evaluation before criminal-history information is considered. If early screening generates false rejections, this increases the probability that an arriving applicant ultimately produces an acceptable match, so that

$$p_D > p_E.$$

If the early screen eliminates only applicants who would ultimately be rejected, however, delaying the screen need not increase p_j and instead primarily raises evaluation costs.

The firm incurs expected screening, evaluation, and compliance costs at flow rate c_j. Delayed screening requires the firm to evaluate a broader set of applicants, so these costs generally satisfy

$$c_D > c_E.$$

Once a position is filled, the match generates expected operating cash flow $\pi_j > 0$ per unit of time. The match ends at Poisson rate $\delta_j > 0$. The parameter δ_j captures both voluntary and involuntary separations and is inversely related to match durability.

Cash flows from the match are also subject to firm-specific labor-input risk. Let σ_j^2 denote the variance of the match-level cash-flow shock. We represent the valuation effect of this risk using a reduced-form risk adjustment $\gamma\sigma_j^2$, where $\gamma \geq 0$ measures the price of firm-specific operating risk. The effective discount rate for cash flows generated by a filled position is therefore

$$r_j = \rho + \gamma\sigma_j^2.$$

The value of a filled position under regime j is

$$H_j = \frac{\pi_j}{\rho + \delta_j + \gamma\sigma_j^2}. \tag{OB-1}$$

Equation (OB-1) captures three possible consequences of improved worker-firm matching. Better matches may generate greater operating cash flow, π_j; they may last longer, implying a lower separation rate, δ_j; and they may make the firm's labor input more reliable, implying lower cash-flow risk, σ_j^2.

OB.2 The Value of a Vacancy

Let V_j denote the value of an unfilled position under screening regime j. While the position is vacant, the firm incurs vacancy cost κ and screening cost c_j. An acceptable match arrives at rate μ_j, at which point the firm transitions from vacancy value V_j to filled-position value H_j. The Bellman equation is

$$\rho V_j = -\kappa - c_j + \mu_j(H_j - V_j). \tag{OB-2}$$

Solving equation (OB-2) gives

$$V_j = \frac{\mu_j H_j - \kappa - c_j}{\rho + \mu_j}. \tag{OB-3}$$

The value of a vacancy is increasing in the arrival rate of acceptable matches and in the value of a filled position. It is decreasing in vacancy costs and screening costs.

The expected duration of a vacancy under regime j is

$$T_j = \frac{1}{\mu_j} = \frac{1}{\lambda p_j}. \tag{OB-4}$$

Thus, delayed screening reduces expected vacancy duration whenever it increases the probability that an arriving applicant becomes an acceptable match:

$$p_D > p_E \quad \Longrightarrow \quad T_D < T_E. \tag{OB-5}$$

This result does not require delayed screening to make the evaluation of an individual applicant faster. Delayed screening may require the firm to spend more time and resources evaluating each applicant. Nevertheless, it can reduce the total duration of a vacancy by lowering the probability that a potentially suitable applicant is prematurely removed from consideration.

OB.3 Delayed Screening and Firm Value

Define the value effect of delayed screening as

$$\Delta V = V_D - V_E. \tag{OB-6}$$

Using equation (OB-3),

$$\Delta V = \frac{\mu_D H_D - \kappa - c_D}{\rho + \mu_D} - \frac{\mu_E H_E - \kappa - c_E}{\rho + \mu_E}. \tag{OB-7}$$

Equation (OB-7) formalizes the central tradeoff. Delayed screening is costly because the firm evaluates more applicants, $c_D > c_E$, but can create value by preventing false early rejections, thereby increasing the arrival rate of acceptable matches, μ_D, and potentially improving subsequent match productivity, durability, and reliability. Firm value increases only when these benefits outweigh the additional evaluation costs.

Proposition 1 (Firm-value effect)**.** Delayed screening increases the value of an unfilled position if and only if

$$\frac{\mu_D H_D - \kappa - c_D}{\rho + \mu_D} > \frac{\mu_E H_E - \kappa - c_E}{\rho + \mu_E}. \tag{OB-8}$$

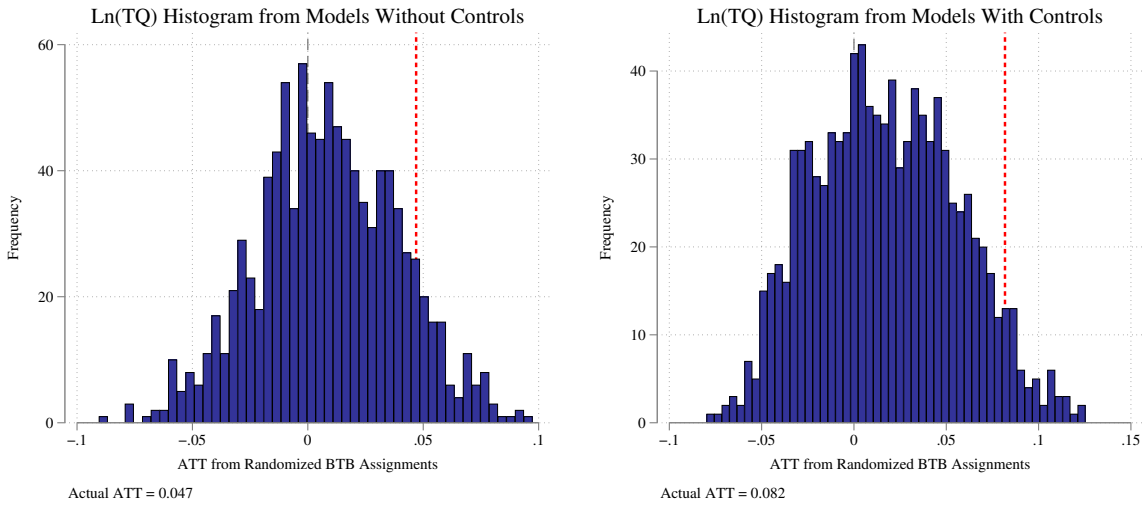
In particular, delayed screening is more likely to increase firm value when it:

1. increases the arrival rate of acceptable matches ($\mu_D > \mu_E$);
2. increases match-level cash flow ($\pi_D > \pi_E$);
3. lowers the separation rate ($\delta_D < \delta_E$);
4. lowers labor-input risk ($\sigma_D^2 < \sigma_E^2$); and
5. imposes only modest additional screening costs, $c_D - c_E$.

Proof. The first statement follows directly from substituting equation (OB-3) into $\Delta V = V_D - V_E$. From equation (OB-1), H_j is increasing in π_j and decreasing in δ_j and σ_j^2. From equation (OB-3), V_j is increasing in H_j and decreasing in c_j. It is also increasing in μ_j whenever $H_j > V_j$, which holds when filling the vacancy creates positive surplus. Therefore, changes in the stated parameters affect ΔV in the indicated directions. □

Overall, the model does not assume that delayed screening is necessarily beneficial. It generates the empirical prediction that if early criminal-history screening excludes some potentially suitable workers, delaying that screen increases the arrival rate of acceptable matches and reduces vacancy duration, leading to higher firm value. However, if delayed screening requires firms to evaluate more applicants and this imposes additional administrative and compliance costs, then delayed screening increases firm value only when improved access to suitable workers and better worker-firm matching outweigh these costs.

Figure OA1: Randomization Inference: Placebo BTB Adoption Years



This figure reports placebo (randomization-inference) tests of the effect of BTB laws on firm value over the period 2005 to 2024, estimated using the imputation-based DiD method. We randomly reassign BTB adoption years across jurisdictions, preserving the overall structure of the staggered experiment but breaking the link between a jurisdiction's true adoption timing and the firms headquartered there. For each of 1,000 placebo draws, we reestimate our baseline firm-value specification, without and with the full set of lagged firm and state controls, and record the coefficient on the placebo BTB indicator. The histograms plot the resulting distributions of placebo coefficients using the natural logarithm of Tobin's Q (TQ), with the left column omitting and the right column including controls. Lagged firm control variables include $Ln(Assets)$, $Tangibility$, $CashFlow$, $NBLEV$, and $Ln(FirmAge)$. Lagged state control variables include $Ln(GDPpc)$, $\%\Delta GDPpc$, $TaxRate$, $UnempRate$, $DemGov$, $Ln(UnempBen)$, Age, $EDUC$, $Female$, and $Marijuana$. All variables are defined in Appendix A.

Table OA1: Timing of BTB Legislation: Duration Model

This table reports results from Cox proportional hazards models over the period 2005 to 2024, where a failure event is defined as the year in which a state passes a private-sector BTB law. All explanatory variables are measured in year $t-1$. Column 1 tests whether adoption is predicted by the state-year average of $StateLn(TQ)$, conditional on the state-level characteristics below. Column 2 examines whether adoption is predicted by a comprehensive set of lagged state-level macroeconomic, political, labor-market, and demographic characteristics; columns 3–5 add, in turn, information-environment, employment and social, and innovation and investment policies, and column 6 includes them jointly. In column 7, all determinants are measured in changes. All variables are defined in Appendix A. z-statistics in parentheses are calculated using jackknife standard errors. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	BTB						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
StateLn(TQ)	1.793						
	(0.84)						
Ln(GDPpc)	-3.261	-2.548	-4.512	-7.099	-3.747	-12.242	4.023
	(-0.54)	(-0.43)	(-0.79)	(-0.22)	(-0.47)	(-0.37)	(0.18)
%ΔGDPpc	28.914	31.131	47.458	29.217	28.796	69.450	3.454
	(0.92)	(1.39)	(0.96)	(0.74)	(1.02)	(0.41)	(0.18)
TaxRate	0.314	0.302	0.354	0.529	0.353	0.531	-0.017
	(0.58)	(0.58)	(0.49)	(0.32)	(0.76)	(0.31)	(-0.01)
UnempRate	0.419	0.272	0.320	0.193	0.216	0.396	-0.648
	(0.67)	(0.57)	(0.22)	(0.39)	(0.37)	(0.26)	(-0.86)
DemGov	0.816	0.804	1.006	1.028	0.007	1.725	0.125
	(0.81)	(0.89)	(0.62)	(0.32)	(0.01)	(0.52)	(0.11)
Ln(UnempBen)	5.276	4.943*	12.634	5.504	6.110	10.957	5.765
	(1.62)	(1.70)	(0.54)	(0.32)	(1.19)	(0.28)	(1.03)
Marijuana	0.679	0.896	0.632	0.273	0.907	0.115	0.023
	(0.45)	(0.82)	(0.26)	(0.06)	(0.61)	(0.03)	(0.00)
Age	-0.269	-0.210	-0.354	-0.636	0.130	-0.632	-0.493
	(-0.64)	(-0.69)	(-0.42)	(-0.26)	(0.27)	(-0.40)	(-0.62)
EDUC	24.273	19.336	31.263	25.214	33.988	48.366	32.892
	(1.20)	(1.04)	(0.67)	(0.25)	(1.04)	(0.25)	(1.41)
Female	10.322	29.487	32.307	87.955	25.230	80.626	-6.879
	(0.16)	(0.48)	(0.26)	(0.23)	(0.39)	(0.95)	(-0.11)
Anti-SLAPP			1.753			1.690	0.454
			(0.63)			(0.34)	(0.29)
DBN			-1.632			-1.639	-0.569
			(-0.61)			(-0.22)	(-0.47)
FOIA			-0.259			-0.155	0.095
			(-0.35)			(-0.11)	(0.46)
MinWage				1.304		1.111	0.742
				(0.19)		(0.16)	(1.14)
PECCB				-1.413		-2.539	1.079
				(-0.17)		(-0.21)	(0.76)
PFML				-0.469		-0.694	0.815
				(-0.15)		(-0.20)	(0.34)
PSL				0.375		1.469	0.652
				(0.13)		(0.30)	(0.38)
SHB				-2.676		-4.117	0.278
				(-0.08)		(-0.12)	(0.01)
IPRP					2.506	0.746	-0.266
					(0.83)	(0.14)	(-0.14)
ITC					-0.358	-0.179	1.007
					(-0.24)	(-0.08)	(0.85)
X Vars in Changes							✓
N of Obs	805	829	829	829	829	829	829
Pseudo R^2	0.488	0.470	0.562	0.562	0.513	0.629	0.353

Table OA2: BTB Laws and Firm Value: Additional Law Controls, Propensity Score Weighting, and Region×Year FEs

This table reports results from the imputation-based DiD method examining the effect of BTB laws on firm value over the period 2005 to 2024. The dependent variable in columns 1–6 is the natural logarithm of Tobin's Q (TQ). ATT measures the average treatment effect of BTB adoption. Columns 1–2 further include lagged controls for related state-level laws and policies: information-environment policies, including anti-SLAPP laws, data-breach notification laws, and the FOIA law index; employment and social policies, including minimum-wage laws, pre-employment credit-check restrictions, paid family and medical leave, paid sick leave, and salary-history bans; and innovation and investment policies, including intellectual-property protections and investor tax credits. Except for the FOIA law index and minimum-wage levels, these additional law controls are measured as indicator variables. Columns 3–4 use the imputation-based DiD method but weight the regressions by the inverse of the probability of treatment. We estimate the weights from a logit regression of the probability that a firm is in a state that will pass a BTB law in the following year, using the same set of firm- and state-level controls as in our main regressions. For this regression, treated firms are those in states that will pass a BTB law in year t, and control firms are those in states that never passed or have not yet passed a BTB law in the year before a legislative event occurs. These weights are then used for all the firm's observations. Columns 5–6 further control for four Census region-by-year fixed effects. Lagged firm control variables include *Ln*(*Assets*), *Tangibility*, *CashFlow*, *NBLEV*, and *Ln*(*FirmAge*). Lagged state control variables include *Ln*(*GDPpc*), *%ΔGDPpc*, *TaxRate*, *UnempRate*, *DemGov*, *Ln*(*UnempBen*), *Age*, *EDUC*, *Female*, and *Marijuana*. All variables are defined in Appendix A. t-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Ln(*TQ*)					
	Additional Law Controls		PS Weighting		Region × Year FE	
	(1)	(2)	(3)	(4)	(5)	(6)
ATT	0.083***	0.11***	0.065***	0.093***	0.060***	0.085***
	(3.41)	(5.23)	(3.77)	(4.53)	(4.06)	(5.68)
Additional Laws	✓	✓				
SIC2 × Year FE	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓
DemGov × Year FE	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓
Region × Year FE					✓	✓
N of Obs	37,525	37,525	34,398	34,398	37,525	37,525

Table OA3: Alternative Difference-in-Differences Estimators

This table reports estimates of the effect of BTB laws on firm value over the period 2005 to 2024, using two heterogeneity-robust DiD estimators as alternatives to the imputation method. Columns 1–2 use the two-way Mundlak estimator of Wooldridge (2025), and columns 3–4 use the interaction-weighted estimator of Sun and Abraham (2021). The dependent variable in columns 1–4 is the natural logarithm of Tobin's Q (TQ). ATT measures the average treatment effect of BTB adoption. Lagged firm control variables include $Ln(Assets)$, $Tangibility$, $CashFlow$, $NBLEV$, and $Ln(FirmAge)$. Lagged state control variables include $Ln(GDPpc)$, $\%\Delta GDPpc$, $TaxRate$, $UnempRate$, $DemGov$, $Ln(UnempBen)$, Age, $EDUC$, $Female$, and $Marijuana$. All variables are defined in Appendix A. t-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	$Ln(TQ)$			
	Wooldridge (2025)		Sun and Abraham (2021)	
	(1)	(2)	(3)	(4)
ATT	0.040**	0.075***	0.031*	0.084***
	(2.30)	(4.11)	(1.81)	(4.66)
SIC2 × Year FE	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓
DemGov × Year FE	✓	✓	✓	✓
Controls		✓		✓
N of Obs	39,381	39,381	39,381	39,381

Table OA4: Heterogeneity by BTB Strength and Political Environment

This table reports heterogeneous treatment effects from the imputation-based DiD method over the period 2005 to 2024, allowing the ATT to vary with the stringency of the applicable BTB law and whether the state has a Democratic Party governor. Firms in adopting jurisdictions are split by *BTB Strength*, an ordinal 0–4 score that grades the applicable BTB law by its breadth of employer coverage and procedural stringency—the stage at which the criminal-history inquiry is permitted, whether an individualized assessment is required, and the strength of enforcement remedies—hand-coded from statutory features following Avery and Lu (2021). A score of 0 indicates that no BTB law yet applies in the jurisdiction. "Low" groups the weak-to-moderate laws (scores 1–2) and "High" the strong-to-strongest laws (scores 3–4). In columns 3–4, "Low" indicates the governor is not from the Democratic Party and "High" indicates that the governor is from the Democratic Party. The dependent variable in columns 1–4 is the natural logarithm of Tobin's Q (TQ). *ATT – Low* and *ATT – High* report the treatment effects for the "Low" and "High" subgroups of each split. Lagged firm control variables include *Ln(Assets)*, *Tangibility*, *CashFlow*, *NBLEV*, and *Ln(FirmAge)*. Lagged state control variables include *Ln(GDPpc)*, *%ΔGDPpc*, *TaxRate*, *UnempRate*, *DemGov*, *Ln(UnempBen)*, *Age*, *EDUC*, *Female*, and *Marijuana*. All variables are defined in Appendix A. *t*-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Ln(TQ)			
	Split by BTB Strength		Split by DemGov	
	(1)	(2)	(3)	(4)
ATT – Low	0.046***	0.068***	0.075***	0.10***
	(3.25)	(5.30)	(3.54)	(5.77)
ATT – High	0.048***	0.096***	0.050***	0.082***
	(3.09)	(5.86)	(3.04)	(5.80)
Low = High *p*-val	0.816	0.013	0.034	0.048
SIC2 × Year FE	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓
DemGov × Year FE	✓	✓	✓	✓
Controls		✓		✓
N of Obs	37,525	37,525	37,525	37,525

Table OA5: Weighted BTB Exposure

This table reports OLS regression results over the period 2005 to 2024 examining the effect of BTB laws on firm value. The dependent variable in columns 1–4 is the natural logarithm of Tobin's Q (TQ). BTB is an indicator equal to one if an establishment or an individual is located in a state, county, or city that has passed a BTB law by year t, and zero otherwise. *BTB-DA* is an employment-weighted average of BTB across all of the firm's establishments in a year. Data are from Data Axle. *BTB-Revelio* is an employment-weighted average of BTB across all of the firm's employees in a year. Data are from Revelio Labs. Lagged firm control variables include *Ln*(*Assets*), *Tangibility*, *CashFlow*, *NBLEV*, and *Ln*(*FirmAge*). Lagged state control variables include *Ln*(*GDPpc*), *%*Δ*GDPpc*, *TaxRate*, *UnempRate*, *DemGov*, *Ln*(*UnempBen*), *Age*, *EDUC*, *Female*, and *Marijuana*. All variables are defined in Appendix A. t-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Ln(*TQ*)			
	(1)	(2)	(3)	(4)
BTB-DA	0.055**	0.079***		
	(2.15)	(3.12)		
BTB-Revelio			0.054*	0.092***
			(1.75)	(3.28)
SIC2 × Year FE	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓
DemGov × Year FE	✓	✓	✓	✓
Controls		✓		✓
N of Obs	31,947	31,947	30,963	30,963
Adj R^2	0.726	0.751	0.723	0.748

Table OA6: Excluding Firms with Dispersed Operations

This table reports results from the imputation-based DiD method on subsamples of firms whose operations are concentrated in their headquarters state over the period 2005 to 2024. The dependent variable in all columns is the natural logarithm of Tobin's Q (TQ). Panel A (Compustat) excludes, in turn, firms in dispersed-operations industries (retail, wholesale, and transportation; two-digit SIC 52–59, 50–51, 40–47), firms with above-median property, plant, and equipment ($PP\&E$), firms with an above-median number of employees (Emp), and firms operating in an above-median number of states, following García and Norli (2012). Panel B (establishment-level data from Data Axle) keeps only firms with at least 50%, 75%, or 95% of their employees in the headquarters state. All variables are defined in Appendix A. t-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Geographic Concentration Using Compustat Data

	$Ln(TQ)$							
	Non-Disp Industries		Low PP&E		Low Emp		# States < Median	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT	0.056***	0.096***	0.066***	0.11***	0.092***	0.11***	0.061***	0.094***
	(3.82)	(6.51)	(3.91)	(5.01)	(4.87)	(5.22)	(3.73)	(5.45)
SIC2 × Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
DemGov × Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓		✓
N of Obs	30,134	30,134	18,024	18,024	17,773	17,773	19,815	19,815

Panel B: Geographic Concentration Using Establishment-Level Data

	$Ln(TQ)$					
	HQ%>50		HQ%>75		HQ%>95	
	(1)	(2)	(3)	(4)	(5)	(6)
ATT	0.11***	0.15***	0.15***	0.18***	0.14***	0.17***
	(5.26)	(7.07)	(4.40)	(5.42)	(3.82)	(4.70)
SIC2 × Year FE	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓
DemGov × Year FE	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓
N of Obs	15,224	15,224	10,026	10,026	6,127	6,127

Table OA7: BTB Laws and Firm Value: Total Q and Stock Returns

This table reports results from the imputation-based DiD method examining the effect of BTB laws on firm value over the period 2005 to 2024. The dependent variables in columns 1–2, 3–4, 5–6, and 7–8 are the natural logarithm of *Total Q* from Peters and Taylor (2017), the natural logarithm of the market value of equity (*MVE*), the annual realized stock return (*Return*), and the annual characteristic-adjusted abnormal stock return, which adjusts for size, book-to-market, and momentum (*AbReturn*), respectively. *ATT* measures the average treatment effect of BTB adoption. Lagged firm control variables include *Ln*(*Assets*), *Tangibility*, *CashFlow*, *NBLEV*, and *Ln*(*FirmAge*). Lagged state control variables include *Ln*(*GDPpc*), *%*Δ*GDPpc*, *TaxRate*, *UnempRate*, *DemGov*, *Ln*(*UnempBen*), *Age*, *EDUC*, *Female*, and *Marijuana*. All variables are defined in Appendix A. *t*-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Ln(*Total Q*)		*Ln*(*MVE*)		*Return*		*AbReturn*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT	0.053*	0.11***	0.17***	0.12***	0.041***	0.070***	0.029***	0.048***
	(1.80)	(3.67)	(6.15)	(5.89)	(4.02)	(5.18)	(2.63)	(3.46)
SIC2 × Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
DemGov × Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓		✓
N of Obs	29,944	29,944	37,548	37,548	37,614	37,614	34,647	34,647