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Joseph Pickens
U.S. Naval Academy

Aaron Sojourner
W.E. Upjohn Institute for Employment Research, sojourner@upjohn.org

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Authors

Joseph Pickens, *U.S. Naval Academy*
Aaron Sojourner, *W.E. Upjohn Institute for Employment Research*

Upjohn Author(s) ORCID Identifier

https://orcid.org/0000-0001-6839-2512

W.E. UPJOHN INSTITUTE FOR EMPLOYMENT RESEARCH

WORKING PAPER

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Joseph Pickens
United States Naval Academy

Aaron Sojourner
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ABSTRACT

“Just cause” policies aim to discourage the arbitrary firing of employees. Recent efforts at passing such laws in the U.S. have been motivated by deterring discrimination. This paper presents a framework to study the effects of just cause when managers engage in taste-based discrimination. The framework generates predictions on whether just cause will ease achievement and retention of stable employment by exploiting the timing of separations around a probationary period. Since probationary periods are a typical feature of protections, the approach is generalizable. We test predictions using New York City’s 2021 just cause law for fast-food employees. Using a synthetic difference-in-differences design on publicly available data, we do not find results consistent with taste-based discrimination against black, Hispanic, female, or older workers, though lack of enforcement or data issues could be driving the nulls. Further analysis suggests another mechanism: screening discrimination against younger workers.

JEL Codes: J08, J71, K31, J63, M51, H73

Key Words: Just cause, employment protection legislation, taste-based discrimination, screening discrimination, local labor markets, synthetic difference-in-differences

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Introduction

Workers in many industries report and protest decisions by managers that they perceive as unfair to them individually, given the circumstances, or as motivated by managerial racism, sexism, ageism, or another identity-based bias. In a recent nationally representative survey (Schaeffer 2023), 41 percent of black workers and 20 percent of Hispanic workers in the U.S. report having experienced discrimination or unfair treatment by an employer because of their race or ethnicity, while only 8 percent of white workers do. Concern about bias against disfavored worker groups is a key rationale for pushes to move from at-will to just cause standards for employment decisions through public policy and union collective-bargaining agreements. Though preventing the effects of *taste-based discrimination*—occurring when employers have a willingness to pay to avoid interactions with a group they dislike or toward which they have animus—can be a key motivation for worker protections, the economics literature has largely ignored this rationale in studying employment protection legislation (EPL).

What are the effects of just cause regulation on labor markets? Does this regulation have its intended effects? This paper theoretically models and empirically evaluates such regulation, focusing on a 2021 New York City (or, simply New York) law for fast-food-chain employers. "Just causes" for dismissal include firm economic or individual performance issues, and the law removes employer power to dismiss workers for arbitrary, noneconomic, nonperformance reasons. These protections kick in after a 30-day probationary period for newly hired workers. It is plausible that just cause will make it harder to stay employed past this probationary period. Furthermore, in the presence of managerial taste-based discrimination, just cause could enhance retention for disfavored groups, conditional on their being employed for more than 30 days and receiving protection.

Adapting the standard economic model of EPL, we create a simple theoretical framework to evaluate the labor-market effects of just cause regulation under managerial favoritism. In this environment, just cause can enhance fairness by stopping managers from indulging their personal biases at the expense of shareholders and disfavored workers. When favoritism correlates with employee race, gender, or age, it is a model of taste-based discrimination along those lines. We empirically test the model's predictions using the recent New York City fast-food legislation and publicly available data at the county-industry-quarter level. We focus on outcomes by race, ethnicity, gender, and age, as well as for the overall population.

Our main contributions are fourfold. First, in modeling just cause under managerial favoritism, we add to the limited theoretical literature on the interaction between EPL and taste-based discrimination. The model generates predictions on whether just cause will make it easier to *achieve* and *retain* stable employment by exploiting the timing of separations around the probationary period. If the introduction of protections decreases separations before the probationary period (relative to a preprotection environment with no probationary period), we say the legislation makes it easier to *achieve* stable employment. If the protections decrease postprobationary separations for a particular group (relative to another group), we say the legislation makes it relatively easier for this group to *retain* stable employment. Since probationary periods are a typical feature of just cause laws and EPL, our framework is generalizable.

Second, we empirically test the predictions of our model on New York City's just cause law, as well as evaluate other outcomes. Using synthetic difference-in-differences analysis (Arkhangelsky et al. 2021), we compare changes in the treated group to changes in a comparison group of unaffected sectors—either other industries in the same counties or the same industries in other counties. Our *treated group* is a proxy of the *covered worker population*, i.e., workers covered by the legislation. Building off other recent work, this design brings a new approach to literature on the heterogeneous effects of EPL. An important caveat to our analysis is that our proxies for probationary employees and the covered worker population are noisy. Furthermore, the pandemic of COVID-19 and the policy response to it present significant identification challenges. Thus, we view our empirical analysis as suggestive rather than conclusive, and our contribution as providing a framework for similar analysis in the future.

Third, we add to the literature on *screening discrimination*—which occurs when managers tend to hire or retain workers from groups they can more reliably evaluate. Motivated by supplementary results, we run a further empirical analysis of New York City's law to test whether screening discrimination may be at play, although screening discrimination does not feature in our theoretical model.

Last, part of our contribution is that we focus on *recent* EPL in the United States. New York City's just cause law went into effect in 2021, but much of the existing U.S. literature studies legislation from decades ago. A primary reason to study EPL is to learn how similar legislation might impact stakeholders in a similar context. Given that New York fast-food workers birthed the "Fight for $15" movement, which spread around the country, the New York City just cause law could similarly spread geographically or across industries. For example, a recent New York City bill proposes abolishing at-will employment and extending just cause protections beyond fast food to all employees (Shepard 2022), and some are advocating broad expansion (Andrias and Hertel-Fernandez 2021). In studying just cause laws, our analysis can inform policymakers considering these or similar provisions.

Background and policy motivation

This section provides background on the 2021 New York City just cause legislation. We overview the law and provide examples of how discrimination concerns motivated it. In detailing motivation for the law, we simultaneously motivate our investigation of it. Next, we give background on enforcement and compliance. Then, using context on the law, we motivate a separate analysis of screening discrimination. Finally, we give background on relevant COVID-19 labor market and policy changes.

Details on the law

New York City's just cause law—which became effective on July 4th, 2021—extended the existing *Fair Workweek Law* for fast-food workers, which had become effective on November 26, 2017.[1] The 2017 legislation established protections relevant to advanced scheduling, consent to additional hours, premium pay, rest between shifts, and access to hours, but did not govern standards for dismissal or large cuts to workers' hours.

The 2021 law says covered employers could no longer fire nor substantially reduce a worker's hours (by 15 percent or more) without *just cause*. The prior at-will standard allowed an employer to cut hours for or terminate any employee at any time, as long as the decision was not for a specifically illegal reason, such as racial discrimination or taking an unpaid family leave of up to 12 weeks. In contrast, the just cause policy allows termination of a nonprobationary employee only for illegal or dangerous behavior, or for failure to perform job duties. In the latter case, the employer must have offered retraining opportunities and issued multiple warnings over the past year before firing. Furthermore, the employer must provide a written explanation for any firing, hours reduction, or layoff. Such protections do not apply to workers in their first 30 days after hiring. During this *probationary period*, workers are subject to the previous at-will standard. Aside from discharge for individual performance, layoffs for economic reasons must be made in reverse seniority order.

Motivation for the law

Advocates for the law have focused on protecting members of disfavored worker groups against managers' taste-based discrimination. The leading advocacy coalition issued a report that stated, "The lack of legal protections against unfair termination exacerbates mistreatment in an industry overwhelmingly powered by women, immigrants, and people of color. Some of the arbitrary treatment reported by fast-food workers—firing one worker for the same conduct that is tolerated in others—is likely animated by racial and gender

[1] All background information comes from DCWP (2023) unless otherwise stated.

bias. Other workers who have been fired or had their hours slashed suspect they are being punished for speaking up about abuse, wage theft, or hazardous working conditions" (CPD 2019). Jones (2019) summarizes the rationale: "Fast-food workers 'frequently cited favoritism and racial discrimination as sources of unfair treatment from managers.' That bias, they said, could manifest itself in 'frivolous' reasons for a termination, like overly long nails or a seeming reluctance to smile." New York City Council Member Adrienne Adams, cosponsor of the legislation, said, "Essential fast-food workers in New York City have been the victims of arbitrary termination and unfair reduction of hours..." (Escárcega 2021).

Outside of advocacy for this specific law, other recent pushes for EPL have focused on preventing discrimination. For instance, the authors of *Ending At-Will Employment: A Guide for Just Cause Reform* write, "At-will employment . . . leaves workers vulnerable to arbitrary and unfair treatment by managers and supervisors. Workers already likely to experience discrimination or illegal treatment from their employer—for example, black and brown workers, workers with lower levels of formal education, and low-wage workers—are especially vulnerable under at-will employment" (Andrias and Hertel-Fernandez 2021). In a *New York Times* guest opinion piece titled *American Workers Need Better Job Protections*, authors Moshe Z. Marvit and Shaun Richman argue, "Workers may have the right to do their jobs free from sexual harassment and assault, but it has become increasingly clear that employers violate those rights by exploiting the power disparity in the workplace" (Marvitz and Richman 2017). Moreover, pushes for layoff decisions to follow seniority instead of managerial discretion have long been justified by concern that managers will be unfair and that an objective seniority standard reduces the scope for managerial abuse.

Enforcement and compliance

In theory, existing antidiscrimination laws would protect against racial, gender, and age discrimination by managers. However, workers' ability to seek justice in any particular case is freighted with slow, expensive, and high-burden-of-proof processes. In contrast, the New

York City law aims to give fast-food workers speedier, lower-cost access to legal remedies against managers who unfairly dismiss them or cut their hours. Workers can report violations to the New York City Department of Consumer and Worker Protection (DCWP) and request a resolution through binding arbitration. If the arbitrator finds in favor of the worker, the employer *must* reinstate the employee or restore his or her hours, as well as pay the City the cost of arbitration. The employer *may* also be responsible for back pay, cancellation of disciplinary action, and financial penalties to the City (DCWP 2021; DCWP 2023). This arbitration system is on top of a confidential complaint system in place for the 2017 law. DCWP investigates complaints, and, if it finds an employer guilty, the employer may face some of these same consequences. Collectively, the provisions of New York City's just cause law raise the probability that an employee could successfully challenge the employer's decision to terminate that employee or cut his or her hours.[2]

Screening discrimination

Our theoretical model is constructed around the above-discussed motivation for the legislation: preventing managerial taste-based discrimination. Thus, screening discrimination is not featured. However, the law's design may facilitate a screening discrimination mechanism. Indeed, minority or inexperienced workers may initially convey a relatively noisy signal of ability to managers. A 30-day probationary period may provide insufficient time for employers to reliably gauge the ability of all new workers, especially those for whom they have a

[2]Echoing arguments from Pickens and Sojourner (2025), there are a few reasons to believe the New York City just cause law could measurably change employer behavior. First, since the Fair Workweek Law became effective in 2017, the City conducted hundreds of investigations and required millions of dollars in fines for violations affecting thousands of workers; e.g., the city reached a large settlement with Chipotle in 2022 (Shwe 2022). Second, New York's Department of Consumer and Worker Protection (DCWP) regularly conducts educational outreach to workers so that city fast-food workers may be well informed of their rights. Third, workers may be relatively empowered to file complaints. In particular, the complaint and arbitration systems described above can produce enforcement without further effort by the worker, such as acquiring legal representation. Moreover, the law explicitly offers equal rights and protections regardless of immigration status. In general, undocumented workers may perceive themselves in a weaker position and hesitate to file a complaint or use arbitration. Employers could take advantage of this, which could lead to less compliance in the high-immigrant fast-food industry. However, the just cause law's explicit protections of undocumented immigrants and DCWP education campaigns could empower these workers to stand up for their rights and lead to more compliance.

noisy initial signal.

Fast-food managers may hire on perceived ability (at least in part) and have a low bar for retention—i.e., the worker must clearly convey quite poor performance to be fired. A low bar for retention is especially likely in a tight, unskilled labor market, which existed around the time the New York City just cause law became effective. A short, 30-day probationary period may not give enough time for workers to "prove themselves." So the manager's updated view of a worker's ability after 30 days may not differ much from the initial signal. In this case, workers with noisier signals are less likely to be retained, even if their signal mean equals other workers'.

Because this mechanism seems plausible or likely given the short probationary period, we empirically test it alongside our model's predictions. In the literature-review section of this paper, we provide more context by summarizing some of the existing work on screening discrimination.

COVID-19

The New York City fast-food just cause law went into effect during the COVID-19 pandemic, which raises some challenges in empirical evaluation. Differences in COVID-19 exposure and policy responses created different labor-market shocks across locations and sectors. This makes choosing an appropriate control group difficult. Given this and other issues, we view our empirical analysis as suggestive rather than conclusive, and as providing a framework to evaluate similar policies in the future. Here, we provide an overview of relevant, contemporaneous events and policies.

Large labor-market effects of the COVID-19 pandemic started in March 2020. New York City, the initial epicenter of the pandemic, was hit especially hard. Between February and April of that year, the city lost over 20 percent of its employment, almost a million jobs. Furthermore, its employment recovery lagged behind the rest of New York State and the country (NY 2022).

Relative to other locations, New York City was slow to lift a ban on indoor dining (Warerkar 2020) and to reopen other types of establishments like gyms, movie theaters, and bars. Also, many New York establishments started requiring proof of vaccination in the summer of 2021, and a vaccine mandate for all private-sector employees was instituted from December 2021 to November 2022 (Gizzo 2022).

During the pandemic, the federal government expanded unemployment insurance (UI), but UI administration varied significantly across states. In particular, states ended their extended-benefits programs at different times, and some stopped participating in pandemic-related federal unemployment benefits altogether. The extended-benefits program for New York ended in September 2021, which was months after almost every other state ended theirs. Also, New York did not discontinue participating in the federal program until September 2021 (Skinner et al. 2022). We address the potential bias caused by these UI policy differences in a robustness exercise in Appendix E.

Literature review and contribution

This section surveys relevant literature. First, we provide context for our theoretical approach. Then, we discuss the dual labor market, screening discrimination, and heterogeneous-effects literatures.

Theory

In typical employment protection legislation (EPL) theory, a specific environment is constructed to study employment, wage, turnover, or productivity effects of EPL legislation. Our approach follows this tradition, building off the standard EPL model (Boeri 2011; Cahuc et al. 2014). We abstract from several of the standard model's components (like wage bargaining and Mortensen-and-Pissarides-style search and matching) and add the possibility of taste-based managerial discrimination. Despite such discrimination often justifying EPL policies, the economics literature has largely ignored this rationale. An exception is the search model of Holden and Rosén (2014). They show that a sufficiently large fraction of prejudiced employers in the presence of high firing costs can incentivize nonprejudiced firms to discriminate as well.[3]

Dual labor markets

The New York City law introduces a distinction between probationary and nonprobationary employees, which features in our model. In many environments, EPL is modeled as a cost of firing that applies to nonprobationary (or permanent) employees but not to probationary (or temporary) employees. We follow this convention, although we model just cause as a binding law rather than a cost.

Though the dual labor market introduced by the probationary / nonprobationary distinction is uncommon in the United States, it is a feature of many European labor markets. The differing effects of EPL on probationary versus nonprobationary (or temporary versus permanent) employment has a large literature. We give a few relevant examples here. In an investigation of French labor markets in which workers have significant protections after attaining two years of seniority, Cahuc et al. (2019) find a significant rise in the separation rate *before* the two-year threshold and a drop just after. Arnold and Bernstein (2021) study Brazilian EPL, which applies after a three-month probationary period, and find a spike in terminations just before three months.[4]

[3]Other theoretical models on discrimination in the labor market are concerned with explaining economy-wide disparities in wages, employment, and unemployment duration. Though our model has a different objective and is relatively parsimonious, it shares similarities with some of these models. In particular, it relates to models in which employers exercise taste-based discrimination in a *random search* environment; i.e., workers and firms are randomly matched (Black 1995; Rosén 1997; Bowlus and Eckstein 2002; Holden and Rosén 2014). A parallel literature considers taste-based discrimination with *directed search*; i.e., workers choose where to search for a job (Lang and Manove 2003; Lang et al. 2005). Also, having a manager with motives other than profits is similar to the principal-agent model of Ederington et al. (2019), where firm owners are profit-maximizing while managers are gender-discriminating.

[4]Marinescu (2009) and Centeno and Novo (2012) are also closely related. Furthermore, several papers find that job protections induce a substitution of temporary for permanent jobs (Miles 2000; Autor 2003; Kahn 2007; Kahn 2010; Cahuc et al. 2016), which (ceteris paribus) would seem to shift hiring toward temporary jobs.

Screening discrimination

Although we connect our theory to the motivation for the legislation, which was to reduce the impacts of taste-based discrimination, further empirical analysis suggests a possible screening discrimination mechanism. Our analysis adds to the literature on this topic. The most relevant work is Morgan and Várdy (2009)'s study of hiring discrimination in an environment where minorities convey noisier signals of ability than other job candidates. It concludes that worker protection can exacerbate minority underrepresentation by making employers more selective in hiring, which hurts minorities, who in many cases may have more uncertain productivity. Other models with different applications assume managers or employers can better judge one group's productivity compared to another group's (Lundberg and Startz 1983; Cornell and Welch 1996; Ritter and Taylor 2011). Empirically, Pinkston (2006) reports evidence that employers receive noisier productivity signals from black men at market entry than from white men. Also, Benson et al. (2023) derive an empirical test to separate taste-based discrimination, screening discrimination, and complementary productivity using the mean and variance of worker productivity under different manager-worker race pairs (e.g., a black manager and a Hispanic worker). Using data from a major U.S. retailer, they find evidence of screening discrimination across several pairs.

Heterogeneous effects

Our focus on heterogeneous just-cause effects across race, gender, and age categories is part of our contribution. Indeed, work on the labor market effects of U.S. EPL typically focuses on average, rather than heterogeneous, effects. A few papers do focus on heterogeneous effects of at-will employment exceptions (Autor et al. 2006; Pickens 2024), or the intersection between EPL and discrimination (Acemoglu and Angrist 2001; Oyer and Schaefer 2002; Kugler and Saint-Paul 2004; Button 2018; Burn 2018). There is, however, a large international literature on EPL's heterogeneous effects. Our results on younger workers add to a conflicting body of results.[5]

Theory

This section describes our theoretical model and its predictions. The *manager's value* for each worker, determined by a combination of productivity and taste-based discrimination, plays a key role. First, we introduce the baseline environment—i.e., the environment under at-will employment—in which employees are fired if their value to the manager falls below an endogenous threshold. Then, the just cause environment is introduced, in which employees are protected from firing if they are sufficiently productive. Next, just cause's effects are summarized. For full formalization, see the Appendix A. Mathematical notation is omitted below unless useful for description. The law's effects are obtained by comparing the baseline steady state to the just cause steady state.[6]

Baseline environment

Consider a dynamic economy in discrete time with a continuum of identical firms and a continuum of ex ante identical workers. Each firm hires workers to fill a unit mass of positions each period. In a given period, each worker can work for a single firm and supplies labor inelastically. A firm can pay a cost to match with an unemployed worker. This match will start at productivity x, drawn from a distribution with PDF $f(x)$ and positive support on $(0, 1)$. For every subsequent period, the worker experiences a productivity shock with probability $\lambda \in (0, 1)$. Each shock is an independent draw from f. The firm can fire the

[5] Negative effects on young-worker employment are found by the cross-country studies of Skedinger (1995), Heckman and Pagés (2000), Bassanini and Duval (2006), Bertola et al. (2007), and Feldmann (2009); and by the individual-country studies of Kugler et al. (2003), Montenegro et al. (2004), Cahuc et al. (2019), Arnold and Bernstein (2021), and Butschek and Sauermann (2022). However, no significant effects are found by the cross-country studies of Noelke (2011), Avdagic (2015), and Gebel and Giesecke (2016), and the individual-country studies of Blanchard and Landier (2002) and Montenovo and Pickens (2025). This is not a complete list.

[6] Our theoretical model does not consider all provisions of the law. For example, we do not model the intensive employment margin and, thus, do not consider the "hours cut provision" described in the background section.

worker and replace him or her with a new match. Each period, employed workers are paid a wage that is independent of their productivity.

Each firm uses a manager to handle its hiring and firing decisions. We assume the manager has a *taste-based discrimination* value ϵ for each worker and will fire a worker based on $y = x + \epsilon$ instead of x. Call $y = x + \epsilon$ the *manager's value* for a particular worker. In addition to a draw from f, new matches and shocked workers will have an independent and identically distributed (i.i.d.) discrimination value drawn from a distribution with probability distribution function (PDF) π and positive support on $(-1, 1)$. So both new matches and those shocked with probability λ will receive independent draws of x and ϵ from f and π, respectively.

When deciding whether to fire a worker, the risk-neutral manager compares the (lifetime discounted) value of the current match to the expected value of a new match. In particular, it is optimal for the manager to choose a threshold Y in which they will fire a worker if and only if the manager's (current period) value for that worker ($y = x + \epsilon$) is below Y.

The timeline of each period proceeds as follows. First, with probability λ, each worker experiences a shock to x and ϵ. Second, the manager chooses his or her endogenous firing threshold Y and fires workers with $x + \epsilon < Y$. Third, managers hire new workers to replace fired workers. Last, production and wage payment occurs.[7] We denote the steady-state optimal firing threshold in this baseline environment as Y^{base}.

Interpreting ϵ

Before introducing the just cause environment, consider a few points about ϵ, the manager's taste-based discrimination value for a worker. Rather than being shocked and drawn independently, one might think a worker's ϵ value remains consistent throughout an employment relationship. Such a modeling decision would complicate analysis, as the threshold Y would

[7] For a newly hired worker with $x + \epsilon < w$ (where w is the wage), the manager would prefer to fire that worker before production occurs and wages are paid (i.e., between the third and fourth steps in the timeline). However, assuming the firm could not immediately hire a replacement worker, this would not make a difference in our results.

become dependent on ϵ. This could make clear intuition for the results that follow difficult, if not impossible.[8] Since the goal of the model is to provide intuitive, testable predictions, we decided against using consistent ϵ values. However, we do not believe that this will alter the model's qualitative predictions.

One of the model's goals is to generate predictions of just cause's heterogeneous effects. For the moment, suppose the manager is not discriminatory toward workers of a particular group; e.g., the distribution of ϵ is independent of race, gender, and age. Then, one would expect the law to have the same effect across worker groups. Conversely, if the manager does have race-, gender-, or age-specific preferences over workers, one might expect different effects across worker groups. In what follows, we consider the possibility of such discrimination and generate predictions on how the law might affect different groups in this case. We loosely refer to those with relatively high ϵ values as *favored* groups and those with relatively low ϵ values as *disfavored* groups. We assume x and ϵ are uncorrelated, so there are no productivity differences between favored and disfavored groups.

Just cause environment

Now, consider the environment with the just cause law. Assume the law is implemented as a productivity threshold $X_{illegal}$, so that it is illegal to fire a nonprobationary worker, with $x \geq X_{illegal}$. In other words, the manager only has "just cause" to fire a nonprobationary worker when $x < X_{illegal}$. However, any new match (i.e., a probationary worker) can be fired at the end of his or her first full period without just cause. This exception captures the 30-day probation period for new employees in the New York just cause legislation.

The manager will again choose a value threshold Y, below which they prefer to fire workers. Call the steady-state optimal threshold here Y^{jc}. It is illuminating to think about the interaction between manager discrimination and the just cause law graphically. In Figure 1, we plot the law's legal threshold for just cause firing ($x = X_{illegal}$) and the manager's

[8] In particular, the straightforward presentation of results in Figure 2 may not be possible.

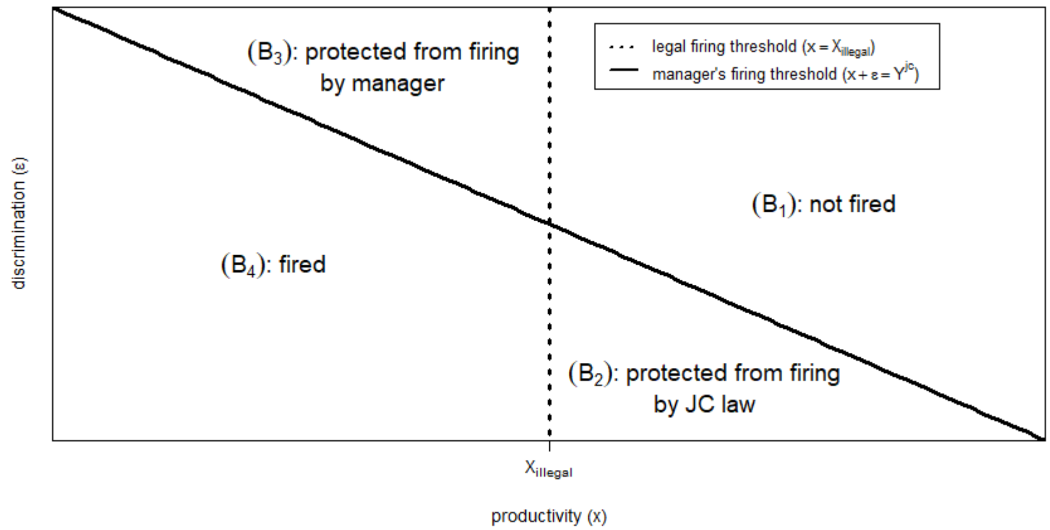


Figure 1: Steady state in JC environment

Notes: This graph shows the interaction between the manager's firing threshold and the legal threshold under just cause in (x, ϵ) space. The manager wants to keep matches above the solid diagonal line and fire matches below it. The legal threshold determines which nonprobationary matches are legally protected from firing. Any to the right of the dotted line are protected. Any to the left are not. This creates four regions labeled in the figure and described in the text.

steady-state optimal firing threshold under just cause $(x + \epsilon = Y^{jc})$ in (x, ϵ) space. The lines split nonprobationary matches into four regions, labeled B_1 through B_4. Any worker in the upper-right region (B_1) is above the manager's value $(x + \epsilon \geq Y^{jc})$ and the legal threshold for just-cause firing $(x \geq X_{illegal})$. The manager does not want to fire that person and, indeed, cannot by law. In the lower right (B_2), the manager wants to terminate the worker, whom he holds in disfavor, but the just cause law bars this because the worker's productivity is sufficiently high. In the upper left (B_3), a manager could legally fire a worker for low productivity, but the manager favors that worker and chooses to protect the worker. In the bottom left (B_4), productivity is low, and the worker falls under the manager's disfavor. The manager wants to fire the worker and has just cause to do so.

For *probationary matches*, the at-will standard still applies, so the manager's threshold is all that matters. The manager keeps those employees who are above it and fires those below.[9] In particular, those types who would have binding protection *after* a probationary period (i.e., in region B_2) would be fired *during* a probationary period. We refer to this

[9]Appendix A shows that the threshold for probationary and nonprobationary employees is the same.

protection of nonprobationary employees in region B_2 as "binding" because the manager wants to fire them but cannot under the just cause law.

Effects of the just cause law

Compare the results of the model with and without just cause. Appendix A contains full proofs and further details. First, the steady-state optimal firing threshold is higher under just cause ($Y^{jc} > Y^{base}$). Intuitively, holding match characteristics fixed, the manager will (weakly) prefer a new worker to an existing worker because they can always fire a new worker, while they cannot always fire an existing worker. This pushes the threshold up, as it incentivizes the firm to fire unprotected workers close to the initial threshold (Y^{base}) and replace them with new workers.[10]

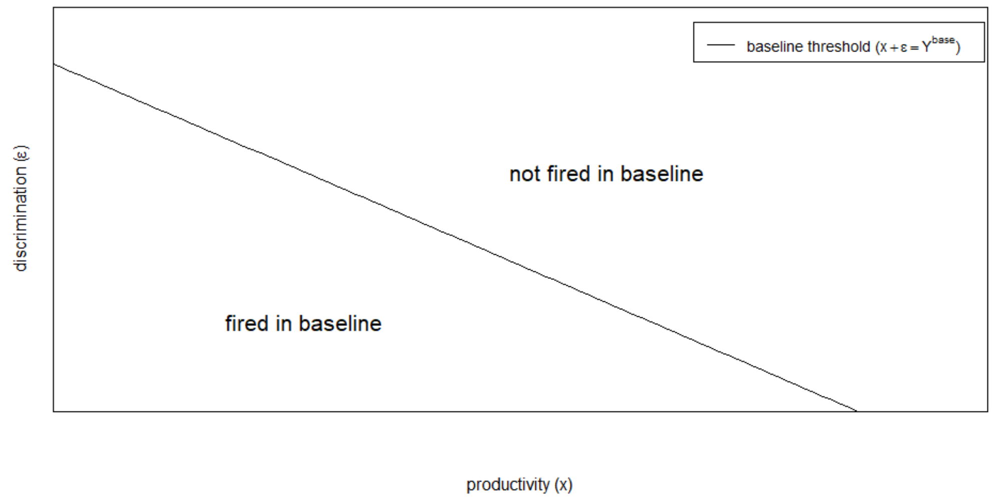
To study probationary versus nonprobationary employment, we split variables into two mutually exclusive categories: stable and nonstable. A *stable employee* lasts more than one period, and a *nonstable employee* lasts only one. A *stable hire* (*stable separation*) is the hiring (separation) of a stable employee; nonstable hires and separations are the respective complements.

To gauge effects of the law, consider Figure 1. The only workers who receive binding protection from the law *after* the probationary period are in region B_2. On average, such types tend to have smaller ϵ values than those who would be fired after the probationary period—those in region B_4. This suggests the law disproportionately protects workers from disfavored groups.[11]

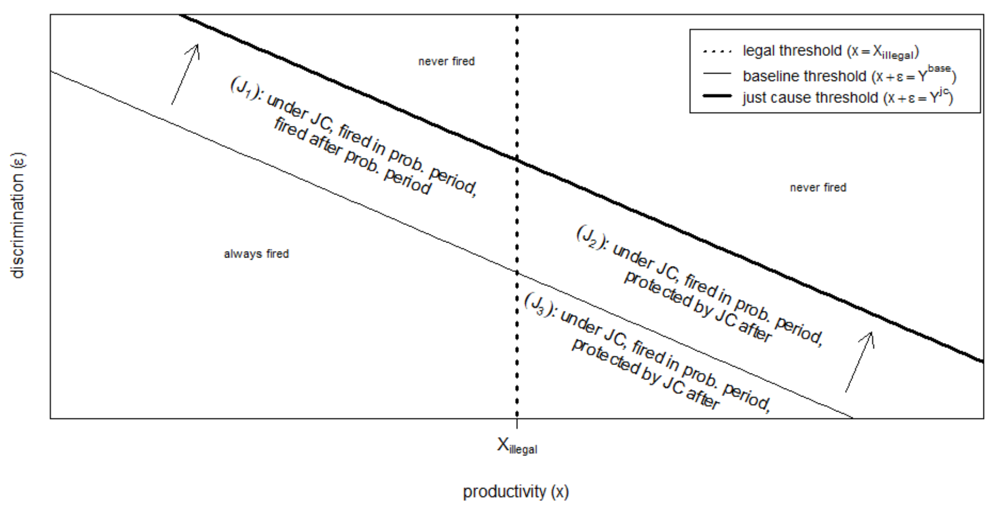
Now, consider the effect of the law as we move from the baseline steady state to the just cause steady state. Figure 2 characterizes the effects of the change. The top panel shows the baseline steady state. The manager fires those below the threshold Y^{base} and not

[10] With this result on thresholds, we show that the separation rate changes ambiguously (see Corollary 1 in Appendix A). On one hand, legal protection prevents the firing of certain workers, which drives separations down. On the other hand, firms raise their standards, which drives separations up. This ambiguous result is notable because, in most other EPL environments, an increase in firing costs decreases turnover (e.g., Bentolila and Bertola 1990; Hopenhayn and Rogerson 1993; Boeri 2011; Cahuc et al. 2014).

[11] Unfortunately, this is not something we can directly test in our empirical analysis, given our data.



(a) Baseline steady state



(b) Just cause steady state

Figure 2: Transition from the baseline to the just cause steady state

Notes: These two figures show the evolution from the baseline steady state to the just cause steady state. In the top figure, the manager fires those below the threshold Y^{base}. The bottom figure shows the just cause steady state. As proven in Appendix A, $Y^{base} < Y^{jc}$, so the manager's threshold under just cause (thicker line) is above that in the baseline environment (thinner line).

In moving from the baseline to the just cause steady state, there are three regions of interest: J_1, J_2, and J_3. The manager will fire all workers below the upper threshold in the probationary period, which applies to all three regions. After the probationary period, those to the right of the dotted line are protected, and those to the left are not. Thus, after the probationary period, workers in region J_1 will be fired, and those in regions J_2 and J_3 will be protected. Workers in regions J_2 and J_3 differ because those in region J_2 are not fired in the baseline, while those in region J_3 are.

those above. The bottom panel shows the just cause steady state. The manager's threshold increases from the thinner line, representing Y^{base}, to the thicker line, representing Y^{jc}, creating three regions of interest: (J_1), those fired during and after the probationary period under JC, but never in the baseline; (J_2), those fired during but not after the probationary period under JC, but never in the baseline; and (J_3), the same as the second, but they *are* fired in the baseline.

We can now share our main predicted effects of the just cause law. Formal analysis and proofs are left to Appendix A, but we provide intuitive explanations here.

Proposition 1 (first main prediction) *Compared to a steady state of the baseline environment, the stable share of hires is lower in a steady state of the just cause environment.*

Intuitively, a greater share of new hires will be fired. Indeed, types in regions J_1 and J_2 are never fired under the baseline but will be fired in the probationary period under just cause. Thus, proportionally fewer hires will become stable employees. The *stable share* of hires—stable hires divided by hires—will decrease.

Proposition 2 (second main prediction) *Compared to a steady state of the baseline environment, the stable separation rate for high ϵ workers (relative to low ϵ workers) is greater in a steady state of the just cause environment.*

Intuitively, we can see that the just cause law will make employment for disfavored groups relatively more secure after the probationary period. While types in J_1 are not fired in the baseline environment, they will be fired after (as well as during) the probationary period under just cause. On the other hand, while types in J_3 were always fired in the baseline, they cannot be fired after the probationary period under just cause (types in J_2 are never fired in the baseline and cannot be fired after the probationary period under just cause). Therefore, since all types in J_1 have a higher ϵ than those in J_3, just cause will necessarily increase the stable separation rate for workers with high ϵ relative to those with low ϵ.

In summary, the two main testable predictions from our model are that just cause will 1) decrease the stable share of hires and 2) increase the stable separation rate for favored workers relative to disfavored workers. The first suggests that just cause will make it harder for new employees to *achieve* stable employment; the second that just cause will make it relatively easier for disfavored groups to *retain* stable employment. As a subsidiary result, just cause has an ambiguous relative effect on stable employment; i.e., countervailing channels imply that either an increase or a decrease in disfavored-group stable employment relative to that of favored groups are both possible.[12] With our main predictions in hand, we turn to testing them empirically.

Empirical design

We aim to test the above theoretical predictions using the introduction of New York's fast-food just cause law. Supplementary and additional analyses look at related outcome variables. This section provides an overview of the empirical design, which is similar across these analyses. First, we introduce the data and variables. Variables are defined to match the corresponding objects in the model as closely as possible. We describe data limitations that make the correspondences imperfect. Table 1 maps between model variables and empirical measures. Second, we give an overview of important empirical details, including caveats.

Data and variables

Empirical analysis uses the Quarterly Workforce Indicators (QWI) data set. The U.S. Census Bureau derives this data set from longitudinal microdata, which enables the reporting of variables beyond employment. The QWI contains hiring and separation levels and can split variables into stable and nonstable versions (discussed below). Quarterly data is at

[12] Similar to the counteracting effects on the overall separation rate mentioned in footnote 10, legal protections have a bigger benefit for disfavored workers, driving their relative stable employment up, while increasing firm standards may drive their relative stable employment down. See Corollary 2 in Appendix A.

the county and four-digit NAICS industry level, and can be separated by gender and age, or by race and ethnicity categories. To measure workers affected by the regulations, we consider *restaurants and other eating places*—NAICS code 7225 and a superset of the fast-food industry—in New York City's five counties, which correspond to the city's five boroughs.

To count as an employee in the QWI, an individual must have "worked" (i.e., had positive earnings at a specific employer in the county-industry) in the given quarter. "Employment" represents the number of such individuals. A "hire" is an employee who worked at an employer in the given quarter, but not the prior quarter. A "separation" worked in the given quarter, but not the following quarter. The hiring (separation) rate equals hires (separations) divided by employment.

Stable variables

We aim to separate probationary and nonprobationary employees as best as possible using available proxies. However, because data are quarterly and the probationary period is 30 days, this presents challenges. To focus on intuition and avoid excessive detail, we describe key factors here and use Appendix B.2 for more context and careful justification of relevant variables.

Stable employees are those employed in the given quarter, the previous quarter, and the following quarter; nonstable employees are the complement. We define stable and nonstable versions of hires and separations similarly. A *stable hire* is an employee who worked in a given quarter and the next two quarters, but not the previous quarter. A *stable separation* is one who worked in a given quarter and the previous two quarters, but not the following quarter.[13] Nonstable versions of variables are the respective complements. The stable hiring rate (stable separation rate) is stable hires (stable separations) divided by stable employment.

[13]Note that in empirical measures, three quarters are used to capture stability. If only two quarters were used, these proxies would include workers who did not stay employed past their probationary period. For example, if a worker is hired in the last week of a quarter and separates in the first week of the next quarter, that worker would be considered "stable" despite having been employed for less than the 30-day probationary period. Using three quarters avoids this problem: all workers captured by the proxy are employed past their probationary period, although some workers employed past their probationary period are missed by the proxy.

	First main prediction	*Second main prediction*
prediction	JC will decrease the stable share of hires	JC will increase the stable separations rate for favored workers relative to disfavored worker
intuition	JC will make it harder for new employees to achieve stable employment	JC will make it relatively easier for disfavored groups to retain stable employment
relevant variable	stable hire	stable separation
model measure	worked in the current period and the next period, but not the previous period	worked in the current period and the previous period, but not the next period
empirical measure	worked in the current quarter and the next two quarters, but not the previous quarter	worked in the current quarter and the previous two quarters, but not the next quarter
outcome variable	stable share of hires $= \frac{\text{stable hires}}{\text{all hires}}$	stable separation rate $= \frac{\text{stable separations}}{\text{stable employment}}$

Table 1: Connecting key model and empirical variables

Notes: This table describes the relevant model and empirical measures for the two main theoretical predictions. In empirical measures, two quarters are used to capture stability because a worker can be hired at the end of a quarter or separated at the beginning of a quarter. Further details are given in Appendix B.2.

Finally, the stable share of employment is the fraction of all employment that is stable in a given quarter. The stable shares of hires and separations are defined analogously. Table 1 shows the correspondence between the key outcome variables in the model and the data.

Critical to our objective is computing relative outcomes. To do so, we consider how the law affects the difference in outcome variables between likely favored and disfavored groups. For example, when evaluating whether just cause makes it relatively easier for black workers to retain stable employment, our outcome variable is the difference between the stable separation rates of white and black workers. In addition to a white (non-Hispanic) versus black (non-Hispanic) comparison, we also have favored / disfavored comparisons of white versus Hispanic, men versus women, and "younger" (aged 14–34) versus "older" (aged 35 and older). Appendix B gives details about variables and justifies our group comparisons.

Empirical details

We contrast changing trends in New York fast food against changes in two kinds of control groups: within fast food across other U.S. counties and within New York City across other industries. We refer to analysis with these groups as the *within-industry* model and the *within-location* model, respectively. Having two comparison groups helps gauge robustness.[14]

The within-industry control group is Industry Code 7225 (restaurants and other eating places) in other counties around the country; e.g., 7225 in Los Angeles County, California. The within-location control group is made up of other four-digit industries in a *specific* NYC county; e.g., 3162 (footwear manufacturing) in Queens County, New York. These control groups number 182 and 163, respectively. For details on control-group construction, see Appendix B. The treated group for both approaches—i.e., our proxy of the worker population covered by the legislation—is 7225 for New York City's five counties. We also consider a third, "pooled" model, which combines the control groups from the within-industry and within-location models.

Analysis uses data from 2018 Q1 to 2023 Q2. The just cause law became effective in July 2021, making 2021 Q3 the first postpolicy period. This yields 14 prepolicy quarters and 8 postpolicy quarters. We start in 2018 Q1 to give a sufficiently long prepolicy period. For each empirical model, we use synthetic difference-in-differences (SDID), which combines desirable properties of the synthetic control method and the difference-in-differences estimator (Arkhangelsky et al. 2021). This approach yields a single, average treatment effect.[15]

Some specifications control for minimum wage changes, as such changes may affect outcomes. We construct a quarterly county-level minimum wage data set that spans our period

[14]The two models are susceptible to different kinds of bias. In a study of the earlier New York fast-food Fair Workweek Law, which uses similar methodology, Pickens and Sojourner (2025) point out, "If there were a common shock to NYC industries around the same time as the legislation but unrelated to it, the within-industry estimates would be biased, though the within-location model would be less prone to such bias. If there were a shock common to the U.S. fast-food industry, the within-location estimates would be biased and the within-industry model would be less prone."

[15]Our analysis uses the *synthdid* package (Hirshberg 2019) to implement the SDID estimator. We use the jackknife estimator of standard errors and uniform time weights. Appendix B.6 discusses technical points on the SDID estimator.

of analysis.[16] By the beginning of our analysis, most of the minimum wage increases in NYC were complete. At the end of 2018, the minimum wage jumped from \$13 to \$15 an hour, where it stayed for the remainder of the analysis period. So the model includes minimum wage to capture the influence of changes in control units in the within-industry model.

For the within-industry approach, we report results from the model with and without a minimum wage control. For the within-location model, all industries in New York except the fast-food industry follow the same minimum wage schedule; thus, we can not separate the effect of the laws from that of different minimum wage schedules using the within-location model. We also do not control for the minimum wage in pooled analysis, for a similar reason.[17]

Last, Appendix C provides additional context on the treated and control groups, including details on the synthetic controls from our main empirical tests and descriptive analysis of the treated group before and after the policy went into effect.

Empirical issues

The QWI data have a significant issue for our application: the legislation only applies to a subset of our four-digit proxy industry (New York City's 7225, restaurants and other eating places). In Appendix B.1, we estimate that approximately 70 percent of workers in NYC 7225 are unaffected by the legislation.

On top of this, as we discussed above, we cannot precisely capture outcomes surrounding short-term work, implying another imperfection of the proxy. Keeping these issues in mind, we proceed with the QWI as the best source publicly available. Given these imprecise proxies—which may attenuate estimates toward zero—and the potential confounding

[16]Note that the Arkhangelsky et al. (2021) framework allows for time-varying controls. Appendix B.5 describes construction of the minimum wage data set from Vaghul and Zipperer (2022) and EPI (2024). Additionally, we considered incorporating worker composition controls (e.g., the share of a county industry's employment that is black), as worker composition may affect outcomes. However, outcomes may also affect worker composition, creating an endogeneity concern. For example, the difference in the white-black stable share of hires difference (an outcome considered in Table 4) likely affects the share of black workers employed. In the end, we decided to exclude such controls.

[17]Controlling for the minimum wage in the pooled model slants the synthetic control almost completely towards within-location control units. Thus, we have the same issue as in the within-location model.

effects of COVID-19 discussed in the background section, we view our empirical analysis as suggestive rather than conclusive and our contribution as providing a framework for similar analysis.

In addition to data issues, one may be concerned that the effects of New York's 2017 Fair Workweek Law could bias our estimated effects of the 2021 just cause law. Pickens and Sojourner (2025) study the labor market effects of this initial law and find null overall employment effects. Though these null results lessen concern over possible bias, the analysis in Pickens and Sojourner (2025) is only for the overall labor market. Here, we also consider heterogeneous effects by race, sex, and age.

Results

This section details empirical results. First, we test the two main theoretical predictions. Then, we give highlights from a supplementary analysis in Appendix D of alternative outcomes. Motivated by a result from this supplementary analysis, we then investigate a possible screening-discrimination mechanism. Finally, we summarize a robustness analysis, described more fully in Appendix E.

Considering the empirical issues discussed above, our results *are not* estimates of just cause's average effects. Rather, our analysis should be viewed as suggestive hypothesis tests rather than as delivering unbiased estimates of just cause's effect. While effect estimates are given in the Tables 2, 3, and 4 below, prepolicy averages for outcome variables are given in Table C.1 of Appendix C.

Evaluating main predictions

First main prediction

To evaluate the first main prediction—just cause makes it harder to achieve stable employment—we analyze the stable share of hires for the overall population. Table 2 details the SDID

	population	*Model*			
		within-ind. (no mw cont.) (1)	within-ind. (mw cont.) (2)	within-location (3)	pooled (4)
stable share of hires	overall	-0.0086 (0.0113) [0.0128]	-0.0069 (0.0096) [0.0128]	-0.0198* (0.0119) [0.0125]	-0.0143 (0.0116) [0.0073]

Table 2: Evaluating first main prediction: a decrease in the stable share of hires

Notes: This table reports effect estimates of New York City's just cause (JC) law for our proxy of the covered worker population. We focus on the stable share of hires using the four models described in the empirical design section. For each specification, an estimate is given with the standard error in parentheses and the prepolicy root mean squared prediction error (RMSPE) in brackets. As noted in Table C.1 of Appendix C, the prepolicy average of the outcome variable for the proxy is 0.454. * denotes that a two-sided hypothesis test is statistically significant at the 10 percent level, ** at the 5 percent level, and *** at the 1 percent level.

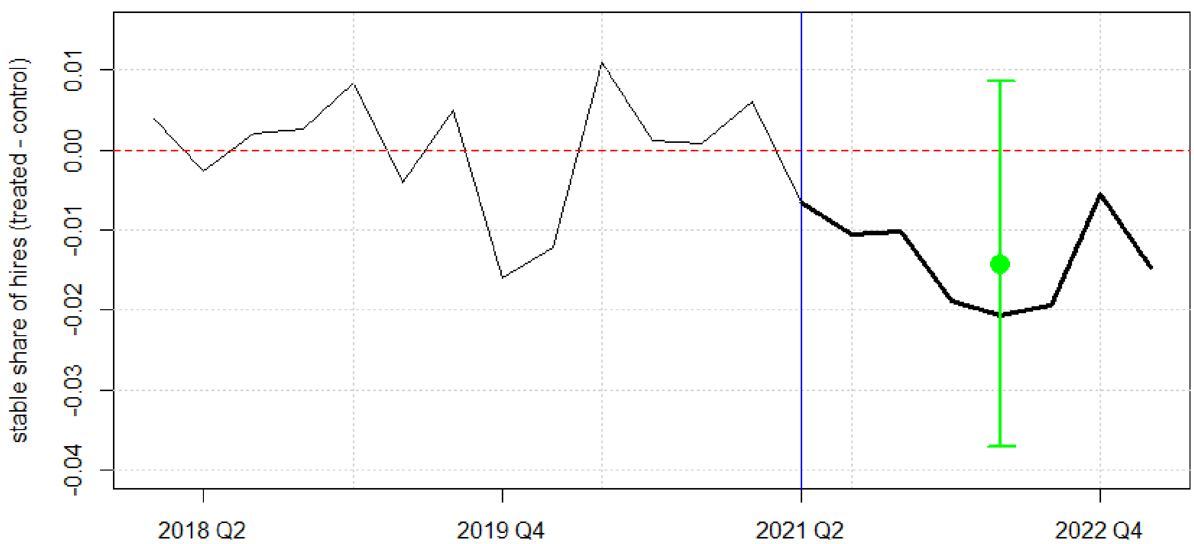


Figure 3: Effect on stable share of hires (pooled model)

Notes: The plot shows the difference between the treated group and the synthetic control over our period of analysis.[a] Data from the prepolicy period is used to choose the synthetic control weights; the line at 2021 Q2 is the last prepolicy period. The closer the difference is to zero in this prepolicy period, the better the synthetic-control fit.

The green point and 95 percent confidence interval illustrate the estimated average treatment effect and its precision. Since we use uniform time weights, this estimate is simply the average postpolicy difference minus the average prepolicy difference. Furthermore, for easier interpretation, we shift the average postpolicy difference to be zero.[b] Thus, the difference in trends has the same units as the average treatment-effect estimate.

[a]In general, our analysis goes until 2023 Q2 instead of 2023 Q1. However, for analysis using the stable share of hires, we have one less quarter of data. This is because the stable share of hires relies on data two quarters into the future, which is not yet available for all counties and industries.

[b]In general, the average prepolicy difference is not zero: SDID allows a gap that is constant in prepolicy periods. See Arkhangelsky et al. (2021) for details.

average-treatment effect estimates, one for each of the four models described in the previous section: 1) the within-industry model *without* a minimum wage control, 2) the within-industry model *with* a minimum wage control, 3) the within-location model, and 4) a model that pools the control groups from the within-industry and within-location models (the "pooled model"). For each specification, there is a treatment-effect estimate followed by the estimate's standard error in parentheses and the prepolicy root-mean-squared prediction error (RMSPE) in brackets. The RMSPE is a standard measure of prepolicy fit for the synthetic-control method and its variants (Abadie 2021). A lower value implies a relatively better fit.

All four models show a negative estimate on the stable share of hires. The pooled model—the best-fitting (lowest RMSPE value) of the four models—yields an estimate of –1.4 (95 percent confidence interval (CI): –3.7 to 0.8). The interpretation of this estimate is that the just cause law decreased the stable share of hires by 1.4 percentage points in our proxy of the covered worker population. Though these four estimates are all in the expected direction, none are statistically significant at the 5 percent level. Thus, we do not find support for the prediction that just cause will make it harder to achieve stable employment on average. Figure 3 plots the difference between the treated group and synthetic control for the pooled model. While the treated group falls below the synthetic control in the postpolicy period, the average treatment effect estimate is not statistically significant at the 5 percent level (as the confidence interval shows). In other words, while a negative postpolicy trend is visible, the effect is not significant.

Second main prediction

Next, we test our second main prediction: just cause makes it easier for disfavored groups to retain stable employment relative to favored groups. To do so, we analyze the effect on the stable separation rate of favored groups relative to that of disfavored groups. As mentioned, in our hypothesis tests that follow, we presume white, male, and younger workers are (on average) favored by managers, while black, Hispanic, female, and older workers are disfavored. For each comparison, our outcomes of interest are the groups' differences in stable separation rates. This design allows us to statistically test the theoretical prediction. For example, just cause should make it relatively easier for Hispanic workers to retain stable employment, so the stable separation rate of white workers should increase relative to that of Hispanic workers, yielding a positive estimate.

Table 3 details results. For the first comparison (white-black), some estimates are positive and some are negative, but all are statistically close to zero. For the fourth comparison (younger-older), all estimates are positive, which is the expected direction, but for the third comparison (male-female), all estimates are negative. These estimates are all statistically insignificant at the 5 percent level. For the second comparison (white-Hispanic), estimates are also positive, and the estimate from the first model is statistically significant at the 5 percent level: 0.4 percentage points (95 percent CI: 0.0 to 0.9). The interpretation of this estimate is that the just cause law increased the stable separation rate for whites by 0.4 percentage points relative to that for Hispanic workers in our proxy of the covered worker population. Two of the other three estimates are significant at the 10 percent level. Figure 4 gives an analogous plot to Figure 3 but for the stable separation rate difference between white and Hispanic workers for the pooled model (the best-fitting model). Despite a positive trend in the treated-control difference beginning a few quarters after the policy became effective, the model's average treatment-effect estimate is not significant at the 5 percent level.

A robust positive estimate would suggest that just cause made it relatively easier for Hispanic workers to retain stable employment (compared to white workers), which would be consistent with taste-based discrimination against Hispanic workers. However, a basic correction for multiple hypothesis testing precludes such a conclusion.

Given that we run four tests for the white-Hispanic comparison, an erroneous statistically significant result (i.e., a Type I error) is more likely. Applying the Bonferroni correction—which requires significance at the 1.25 percent level (5 percent divided by the number of

	comparison	*model*			
		within-ind. (no mw cont.) (1)	within-ind. (mw cont.) (2)	within-location (3)	pooled (4)
stable separation rate	white - black	0.0023 (0.0035) [0.0078]	-0.001 (0.0028) [0.0077]	-0.001 (0.0034) [0.0138]	8e-04 (0.0034) [0.007]
	white - Hispanic	0.0044** (0.0021) [0.004]	0.0035* (0.0021) [0.004]	0.0033 (0.0024) [0.0052]	0.0036* (0.0021) [0.0024]
	male - female	-0.0029 (0.0046) [0.0034]	-0.0028 (0.0041) [0.0034]	-0.0021 (0.0046) [0.0024]	-0.0021 (0.0046) [0.0019]
	age 14-34 - age 35+	0.0048 (0.0045) [0.0069]	0.0056 (0.004) [0.007]	2e-04 (0.0048) [0.0082]	0.0044 (0.0045) [0.0049]

Table 3: Evaluating second main prediction: A relative increase in the stable separation rate for favored groups

Notes: This table has a similar structure to Table 2 but with more rows. Units are the difference in the stable separation rate (i.e., stable separations divided by stable employment) between a favored and disfavored group (e.g., the white-black difference). The prepolicy average stable separation rate for New York City's five counties is given in Table C.1 of Appendix C for each favored and disfavored group. Averages are between 0.146 and 0.216. * denotes that a two-sided hypothesis test is statistically significant at the 10 percent level; ** at the 5 percent level, and *** at the 1 percent level.

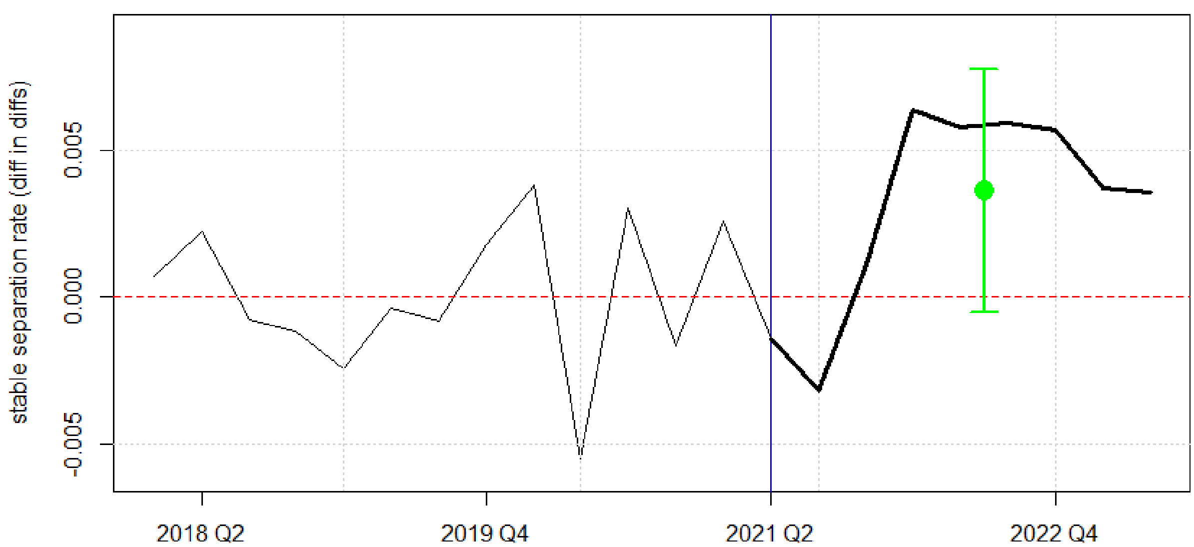


Figure 4: Relative Effect on Stable Separation Rate: White - Hispanic (pooled model)

NOTE: This plot is similar to Figure 3, but the y-axis is now a difference-in-differences: the first difference is between the white and Hispanic stable separation rates; the second difference is between the treated group and the synthetic control.

tests)—none of the four estimates are statistically significant at the appropriate level.

Analysis of other variables

In Appendix D, we run a supplementary analysis on other variables. Table D.1 reports results on the whole sample for additional outcome variables: employment; the hiring and separation rates; stable employment; the stable hiring and separation rates; and the stable share of employment, hires, and separations. Table D.2 reports results for relative stable employment (in an analogous way to Table 3).

There are two main takeaways from this analysis. First, there are no significant estimates for any of the eight "whole sample" variables. In a plot analogous to Figure 3 but for log employment, Figure D.1a plots the results of the pooled model, the best-fitting model. One can observe a good prepolicy fit, no clear trend, and an average treatment effect statistically close to zero. Second, all four models estimate a large increase in white stable employment relative to black stable employment. Despite large standard errors, this increase is statistically significant at the 5 percent level for the two within-industry models. The within-industry model without the minimum wage control is significant at the 1 percent level, which remains significant after a Bonferroni correction for multiple hypothesis tests. Figure D.1b plots the result of the pooled model. The prepolicy fit is also good, and a positive trend is visible. However, the average treatment effect is insignificant.

Summary

To summarize, we find no statistically significant evidence for either of our two main predictions: 1) that just cause makes it harder to achieve stable employment; or 2) that just cause makes it easier for disfavored groups to retain stable employment relative to favored groups. However, an increase in stable white employment relative to stable black employment motivates a further investigation. Analysis in the next subsection suggests the existence of screening discrimination.

Screening discrimination

In the theory section, we present a framework to predict just cause's effects in an environment with taste-based discrimination from managers. We choose to focus on prejudice because that is part of the New York City just cause law's motivation. While our empirical analysis above finds no evidence of such discrimination, a relative decrease in stable employment for black workers is notable. Though such a decrease is *not* evidence of taste-based discrimination from managers against these workers, it may be a sign of a phenomenon not theoretically motivated above and, thus, missed by our model's predictions.

Perhaps managers take longer to screen black employees than white employees on average. If so, the 30-day probationary period could lead to relatively more terminations of new black employees and relatively less stable black employment. Indeed, for the manager, the just cause law increases the stakes of keeping a new employee past 30 days because that employee then become harder to fire. All else held equal, the manager would be more likely to keep a worker he or she had a "better read" on. So if managers receive a noisier signal about the productivity of black workers than white workers, the just cause law makes them relatively more likely to fire new black workers in the probationary period who would have become stable. The end result would be relatively less stable black employment (which our additional analysis in Appendix D suggests). We refer to this phenomenon as *screening discrimination*—when managers tend to hire or retain workers from groups they can more reliably evaluate (Cornell and Welch 1996).

In addition to or instead of screening discrimination, a more traditional statistical discrimination mechanism may be at play. Suppose managers (on average) have the perception that white workers have a higher mean productivity than black workers. Uncertainty about productivity and the need to decide in the first 30 days could lead managers to fire a relatively higher share of new black employees (which would result in relatively less stable black employment).

To test whether one of these mechanisms is at play, we investigate the following question:

did the just cause law lead to relatively more new employee turnover from certain groups? We do this by looking at relative changes in the stable share of hires. Note that we cannot differentiate between these mechanisms; our results can only suggest that screening or statistical discrimination is occurring. If we find that, for example, the stable share of hires for whites increases relative to that for black workers, this suggests that one of the mechanisms makes achieving stable employment for black workers more difficult.

Table 4 details the results for our four comparisons. For all specifications, the white-black, white-Hispanic, and male-female comparisons yield positive estimates—which we expected—but none are statistically significant at the 5 percent level. This insignificance is perhaps surprising for the white-black comparison, given the large relative increase in white stable employment noted above.

On the other hand, the younger-older comparison yields *negative* estimates across the board. Three of the four estimates are statistically significant at the 5 percent level, including the best-fitting pooled model: –1.9 percentage points (95 percent CI: –3.4 to –0.3). The interpretation of this estimate is that the just cause law decreased the stable share of hires for younger workers by 1.9 percentage points relative to that for older workers in our proxy of the covered worker population. Figure 5 illustrates this result.[18] After applying the Bonferroni correction for multiple hypothesis testing, the second estimate remains statistically significant (at the 1.25 percent level), and the first estimate is very close.

Although not direct evidence, this suggests that one of the above mechanisms is at play against younger workers: perhaps at the end of the 30-day probationary period, managers were (on average) relatively less confident in the productivity of younger workers, which manifested in a smaller share of stable hires. Screening discrimination seems especially plausible: if managers use work experience as a screening tool, they will have less information

[18]While the figure shows a negative trend in the year after passage and the average treatment effect is significantly negative, there is a rebound in the third and fourth quarters of 2022. In these two quarters, the stable share of hires for younger workers increases relative to that for older workers (in the treatment group relative to the synthetic control). A straightforward explanation for this is unclear. We suspect it may have to do with the changing policy environment in NYC during this time (see the background section for some details).

	comparison	*model*			
		within-ind. (no mw cont.) (1)	within-ind. (mw cont.) (2)	within-location (3)	pooled (4)
stable share of hires	white - black	0.0076 (0.0065) [0.006]	0.0104* (0.0061) [0.0059]	0.0134* (0.0069) [0.0103]	0.0098 (0.0066) [0.0066]
	white - Hispanic	0.0026 (0.0028) [0.0039]	0.003 (0.003) [0.0039]	0.0052 (0.0034) [0.0061]	0.0037 (0.0029) [0.0033]
	male - female	0.0039 (0.0028) [0.006]	0.0038 (0.0027) [0.006]	0.0026 (0.0032) [0.0032]	0.0037 (0.003) [0.0025]
	age 14-34 - age 35+	-0.0201** (0.0081) [0.0054]	-0.0197** (0.0079) [0.0054]	-0.008 (0.0086) [0.013]	-0.0186** (0.008) [0.0048]

Table 4: Relative effects on the stable share of hires

Notes: This table has the same structure as Table 3. Units are the percentage point difference in the stable share of hires. The prepolicy average stable share of hires for New York City's five counties is given in Table C.1 of Appendix C for each favored and disfavored group. Averages are between 0.399 and 0.493. * denotes that a two-sided hypothesis test is statistically significant at the 10 percent level, ** at the 5 percent level, and *** at the 1 percent level.

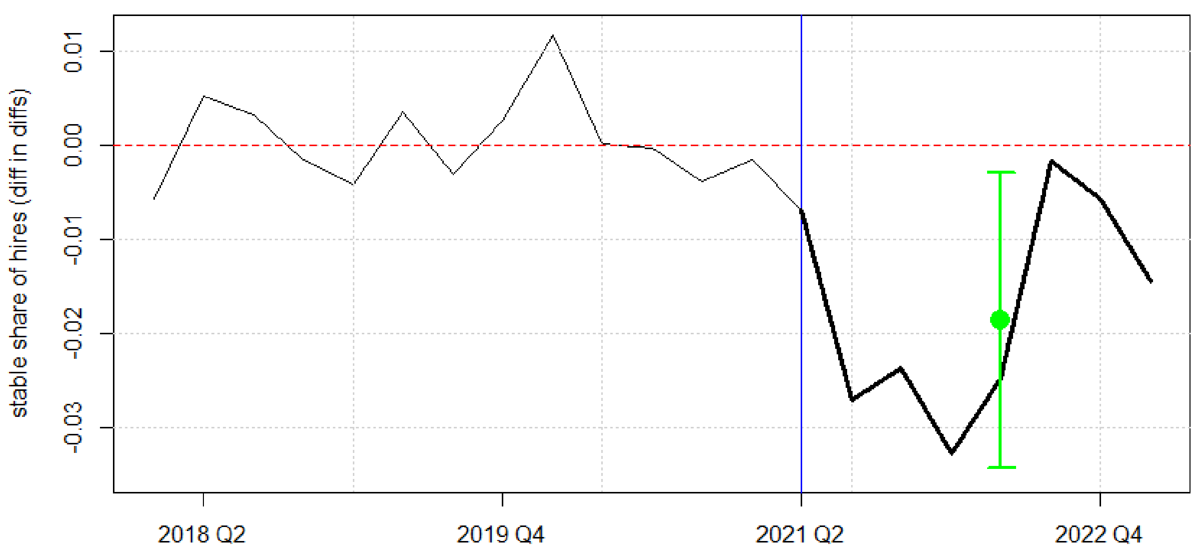


Figure 5: Relative effect on the stable share of hires: younger – older (pooled model)

Notes: The specification shown compares younger (aged 14–34) and older (age 35+) workers. Like Figure 4, the y-axis is a difference-in-differences: the first difference is between the younger and older stable share of hires; the second difference is between the treated group and the synthetic control.

on younger workers who, on average, have a shorter work history.

Robustness

Appendix E assesses the robustness of our empirical results from Tables 2, 3, and 4 – i.e., the evaluation of our two main theoretical predictions and a screening discrimination mechanism. We consider three types of tests.

First, to diminish the effect of COVID-19 on our synthetic control construction, we rerun the analysis, removing the six quarters before the policy took effect (2020 Q1 to 2021 Q2). Second, to account for differing state-level unemployment insurance (UI) policies during the pandemic, we rerun our within-industry models, adding a control for UI replacement rates. The results from these two tests in Tables E.1 and E.2 are broadly consistent with the main analysis.

Third, we construct triple-difference models that consider differences in location, industry, and time simultaneously. A first approach compares the difference between New York City's restaurant and personal care service industries to differences between these industries in other counties (before and after the policy took effect). A second approach compares the difference between New York's and Milwaukee's restaurant industry to the differences between these locations for other industries. We choose the personal care services industry and Milwaukee (i.e., Milwaukee County, Wisconsin) because they are consistently the most heavily weighted control units for at least two of our variables; see Table C.2 in Appendix C.

Table E.3 details the results. For the first approach, standard errors and RMSPE values are notably larger than in the main analysis, and no estimates are statistically significant at the 5 percent level. For the second approach, precision and fit are somewhat better, and results are generally consistent with the main analysis. However, two things are worth noting. First, the stable separation rate *decreases* significantly for white relative to Hispanic workers; this is in contrast to the increase found in Table 3. Second, unlike in the main analysis, a relative decrease in the stable share of hires for younger workers is not significant at the 5 percent level, likely due to lower precision.

While these discrepancies in our results are noteworthy, we view the triple-difference analysis as less credible. Though adding a third difference may reduce bias, it tends to inflate standard errors. Thus, a larger sample is likely needed to detect meaningful effects. In summary, just as in our main analysis, estimates from these triple difference specifications are suggestive rather than conclusive.

Conclusion

Our theory provides testable predictions about the effects of a just cause law in the presence of taste-based discrimination by managers. We find that the law should make it relatively easier for disfavored groups to retain stable employment. To test this theory, we analyze the recent just cause law for the New York City fast-food industry. We find no evidence of this prediction for black, Hispanic, female, or older workers. These null findings are consistent with a few potential interpretations. First, there was no managerial favoritism along these demographic lines, so there was no scope to reduce it. Second, there is favoritism, but enforcement of the law is so weak that it did not change managerial behavior. Third, the magnitude of any effect is smaller than our statistical power to detect it, noting that we have a noisy proxy for the experiences of covered employees. Fourth, bias from COVID-19 and related policies may be hiding important results.[19] Also, we find no empirical support for our prediction that just cause will make it harder to achieve stable employment on average. Similar interpretations exist for this null result.

Although we connect our theory to motivation for the legislation—i.e., to reduce *taste-based discrimination*—further empirical analysis suggests that *screening discrimination* may

[19] Another possibility is that our theoretical model is missing counteracting effects. For example, our model does not consider a hiring decision: managers pay a cost to match with a random unemployed worker; they find out the worker's productivity x and discrimination value ϵ later. Perhaps screening that happens *before* hiring a worker is of first-order concern. In that case, those disfavored workers who are hired may be positively selected for productivity attributes. This may wash away our hypothesized relative effects of just cause for certain group comparisons. We thank Alan Benson for pointing out this possible mechanism.

be at play against younger workers (those under age 35). Indeed, the just cause law introduces a high-stakes decision for the manager at 30 days of tenure. If managers have less confidence in the productivity of younger workers—which seems plausible if they use work experience as a screening tool—we would expect relatively more short-term turnover of these employees. And this is exactly what we observe: it becomes relatively more difficult for younger workers to achieve stable employment.

Though our results are generally robust to different specifications and additional tests, we rely on imperfect proxies for the covered worker population and short-term employment. Furthermore, COVID-19 and policy responses present significant identification challenges. Because of these issues, we think of our empirical analysis as a theory-testing exercise, not a quest to get unbiased estimates of the law's effects. Our primary contributions are theoretical and conceptual; the empirical analysis is illustrative and suggestive rather than conclusive. Future research can improve on our empirical analysis with establishment-level data that better separates covered and uncovered employees, as well as individual employment records with more precise hire and separation dates, reasons for separation, and supplementary information.

Furthermore, our estimates of just cause's effects come from a setting with unusually tight labor markets, and effects from slack labor markets might differ. In tight labor markets, managers may have less discretion in hiring, discipline, and firing, and may engage in less discrimination or favoritism. In theory, the cost of exercising taste-based discrimination is lower when closer substitutes in productivity among the favored groups are available for a disfavored-group worker. This is more likely when an employer gets more applicants per posting (Baert et al. 2015; Challe et al. 2023). Studies in slack markets might find evidence more consistent with our model.

Another avenue for future work is investigating how the New York just cause law affects worker productivity and firm performance. Studying productivity effects is common throughout the employment protection legislation (EPL) literature (e.g.: Hopenhayn and

Rogerson 1993; Ichino and Riphahn 2005; Bastgen and Holzner 2017; Montenovo 2024). Job protection may induce workers to invest in firm-specific skills because they anticipate a long employment spell, but it can also lower the effort of workers because there is less threat of layoff. A theoretical framework with some of the facets of our model—perhaps one that adds firm-specific investment or employee effort—may be well suited to generate predictions on how just cause affects worker productivity and firm performance. In particular, including manager discrimination or a similar idea could help uncover a relationship between discrimination and productivity.

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Appendices

A Details and proofs for model

This appendix is organized as follows: First, notation omitted from the theory section in the body (for brevity) is mentioned. Second, the firm problems in the baseline and just cause environments are developed. Third, results on the effects of just cause are stated, proved, and discussed. Several intermediate results build up to Proposition 1, which formally proves our first main prediction—that just cause will decrease the stable share of hires. The second main prediction—that just cause will decrease the stable separation rate for disfavored workers relative to favored workers—relies on Theorem 1, followed by a graphical argument.

The following notation and details are omitted from the paper's body: the discount rate for firms is γ; the cost of finding a match to a firm is $c > 0$; the common wage paid to all employed workers is $w \in (0, 1)$; the full-time benefit paid to two-shift workers is $b > 0$; the productivity shock distribution PDF (f) is continuous, has accompanying CDF $F(x)$, and has positive support on $(0, 1)$; the discrimination shock distribution PDF (π) is continuous, has accompanying CDF $\Pi(\epsilon)$, and has positive support on $(-1, 1)$; each match is destroyed exogenously at rate $\delta \in [0, 1)$ at the beginning of each period. Although not mentioned in the body, we include exogenous job destruction to match the canonical employment-protection model. However, it is not necessary for the key predictions of the model; i.e., all results would continue to hold if $\delta = 0$. Amending the timing described in the body, we assume exogenous job destruction occurs at the beginning of the period (so the firm will enter each period with all shifts filled). Last, we define g to be the PDF of the distribution of manager values (y):

$$g(y) = \int_{\{x+\epsilon=y\}} f(x)\pi(\epsilon)d(x, \epsilon), \qquad \forall y,$$

where G is the accompanying CDF: $G(y) = \int_{-1}^{y} g(z)dz$. Note that the distribution for y has positive support on $(-1, 2)$ and its PDF g is continuous

A.1 Firm problem in baseline environment

Recall that the manager will choose a threshold Y at which he or she will fire a worker if and only if the worker's $y = x + \epsilon$ value falls below Y. The shift constraint for the manager's problem is

$$H \;=\; \delta + H^{-}G(Y)(1-\delta) + (1-H^{-})\lambda G(Y)(1-\delta) + (1-\lambda)(1-\delta)\int_{\underline{E}(Y)} \phi(x,\epsilon)d(x,\epsilon) \quad \text{(A.1)}$$

, where H is the number of hires, $\underline{E}_x(Y) \equiv \{(x,\epsilon) : x + \epsilon < Y\}$ is the set of manager values less than Y, and $\phi(x,\epsilon)$ is the joint density function detailing the distribution of workers at the firm right before production. Note that we will use $\overline{E}_x(Y)$ to denote the complement of $\underline{E}_x(Y)$; i.e., $\overline{E}_x(Y) \equiv \{(x,\epsilon) : x + \epsilon \geq Y\}$. Also, when we are denoting sets of y as opposed to sets of (x,ϵ), we will use $\underline{E}_y(Y)$ and $\overline{E}_y(Y)$.

The first term of the shift equation's right-hand side represents those jobs that have been exogenously destroyed. The second represents workers hired last period who ended up with y below Y (after their initial value and a possible shock). The third term represents workers not hired last period who received a negative shock. The fourth term is the jobs destroyed without a shock. In a steady-state equilibrium, the choice of the firing threshold Y will be constant over time, so this term can be ignored in that case. Under previous employment ϕ^{-} and the choice Y, the law of motion for ϕ is

$$\phi(x,\epsilon) \;=\; Hf(x)\pi(\epsilon) + \begin{cases} (1-\delta)(1-\lambda)\phi^{-}(x,\epsilon) \;+\; (1-\delta)\lambda f(x)\pi(\epsilon) & \text{if } x+\epsilon \geq Y \\ 0, & \text{if } x+\epsilon < Y. \end{cases} \quad \text{(A.2)}$$

The manager's problem is the following:

$$J(\phi^{-}, H^{-}) \;=\; \max_{Y}\left\{ \int (x+\epsilon)\phi(x,\epsilon)d(x,\epsilon) \;-\; w \;-\; c\cdot H \;+\; \gamma J(\phi, H)\right\}$$

subject to (A.1) and (A.2). We denote the manager's steady-state optimal decision in this environment as Y^b, where 'b' is for "baseline."[1]

A.2 Firm problem in just cause environment

The JC law is implemented in the model as a productivity threshold X_{ill}, in which it is *illegal* to fire a worker with $x \geq X_{ill}$, with one exception: all new matches can be fired at the end of their first full period.[2]

Given this restriction, the manager's problem is different from before. The manager now choose two value thresholds, one for new matches (Y_n) and one for existing matches (Y); the respective steady-state optimal thresholds are denoted as Y_n^{jc} and Y^{jc}. For new matches, the manager can fire a worker with any level of productivity, but for existing matches, the manager can only fire a worker with productivity below $x < X_{ill}$. The new shift equation is now

$$\begin{aligned} H \;=\; \delta \;+\; H^{-}G(Y_n)(1-\delta) \;+\; (1-H^{-})(1-\delta)\lambda\big[G(Y) - P(Y, X_{ill})\big] \\ +\; (1-\delta)(1-\lambda)\int_{\underline{E}_x(Y,X_{ill})} \phi(x,\epsilon)d(x,\epsilon) \end{aligned} \tag{A.3}$$

where

$$P(Y, X_{ill}) \;\equiv\; \int_{E_x^p(Y,X_{ill})} f(x)\phi(\epsilon)d(x,\epsilon)$$

is the fraction of shocked workers for which just cause protection is binding;

$$E_x^p(Y, X_{ill}) \;\equiv\; \{(x,\epsilon) \mid x+\epsilon < Y,\; x \geq X_{ill}\}$$

is the set of matches with binding protection; and

$$\underline{E}_x(Y, X_{ill}) \equiv \{(x,\epsilon) \mid x+\epsilon < Y,\; x < X_{ill}\}$$

[1]Note that in the theory section of the body, we called this variable Y^{base}. We shorten the notation here to declutter the equations that follow.

[2]Note that in the theory section of the body, we called this variable $X_{illegal}$. We shorten the notation to declutter the equations that follow.

is the space of existing matches that will be fired under Y.

Note that those who can legally be fired ($x < X_{ill}$) but whom the manager still wants to keep ($x + \epsilon \geq Y$) will still be kept. In a sense, these workers are "protected" by the manager from a "just" firing. Also, note that we will use $\overline{E}_x(Y, X_{ill})$ to denote the complement of $\underline{E}_x(Y, X_{ill})$; i.e.,

$$\overline{E}_x(Y, X_{ill}) \equiv \{(x, \epsilon) \ : \ x + \epsilon \geq Y \text{ or } x \geq X_{ill}\}.$$

The manager's problem in this setting is:

$$J(\phi^-, H^-) \ = \ \max_{Y, Y_n} \left\{ \int (x + \epsilon)\phi(x, \epsilon) d(x, \epsilon) \ - \ w \ - \ c \cdot H \ + \ \gamma J(\phi, H) \right\}$$

subject to (A.3) and the law of motion[3]

$$\phi(x, \epsilon) = H f(x) \pi(\epsilon) + \begin{cases} (1 - \delta)\big[(1 - \lambda)\phi^-(x, \epsilon) + \lambda f(x) \pi(\epsilon)\big] & \text{if } x + \epsilon \geq Y \\ (1 - H^-)(1 - \delta)(1 - \lambda)\phi^-(x, \epsilon) & \text{if } x + \epsilon < Y, \ x \geq X_{ill} \\ 0 & \text{if } x + \epsilon < Y, \ x < X_{ill}. \end{cases}$$

A.3 Effects of JC

To make the setting interesting, we assume that the law will have a (measurable) effect.

Assumption 1 *In the baseline steady state, there is a positive mass of workers for which the JC law would provide* <u>*binding*</u> *protection. That is,* $P(Y^b, X_{ill}) > 0$.

This assumption will be used in the proof of Theorem 1.

Theorem 1 *In the just cause environment, the steady-state optimal thresholds for new and existing workers coincide* ($Y_n^{jc} = Y^{jc}$). *Also, the steady-state optimal threshold is higher in the environment with just cause than in the baseline environment* ($Y^{jc} > Y^b$).

[3]Note that for the sake of convenience, we write the law of motion in terms of only Y since $Y = Y_n$ in the solution (see Theorem 1).

Proof of Theorem 1: We will first study the baseline environment with manager discrimination (superscript b) and then the JC environment (superscript jc). Both environments will be analyzed in a steady state.

Define Ψ^b_y to be the lifetime discounted value to the manager (right after the hiring phase) of employing a worker with value y:

$$\Psi^b_y \;=\; y - w + \gamma\Big[(1-\delta)(1-\lambda)\Psi^b_y + (1-\delta)\lambda\Xi^b + \Big((1-\delta)\lambda G(Y^b) + \delta\Big)(\Psi^b_n - c)\Big] \quad \text{(A.4)}$$

The three terms in the brackets are the three possible continuation values of employing the worker with value y. If the match is not destroyed and not shocked (first term), the continuation value is the same (Ψ^b_y). If the match is not destroyed but is shocked, and the shock is not below Y^b (second term), then the continuation value is

$$\Xi^b \;\equiv\; \int_{\overline{E}_y(Y^b)} \Psi^b_{y'} g(y')dy'. \quad \text{(A.5)}$$

If this shock is below Y^b or the match is destroyed (third term), then the manager pays a cost c to find a match, and the expected value of that match is

$$\Psi^b_n \;=\; \Pi \;+\; \gamma\Big[(1-\delta)\Xi^b \;+\; \Big((1-\delta)\lambda G(Y^b) + \delta\Big)(\Psi^b_n - c)\Big], \quad \text{(A.6)}$$

where $\Pi \equiv \int_y (y-w)g(y)dy$ is the expected flow profit from a new worker in his or her first period. The optimal threshold Y^b is such that the manager is indifferent as to either 1) keeping a worker with value $y = Y^b$ or 2) firing this worker and replacing the worker with a new one:

$$\Psi^b_{Y^b} \;=\; \Psi^b_n \;-\; c. \quad \text{(A.7)}$$

We now turn to characterizing Y^b. First, we plug (A.4) into (A.5) and use (A.7) to get

$$\Xi^b \;=\; \frac{1}{\Lambda(Y^b)}\Big[\pi(Y^b) \;+\; \gamma(1-G(Y^b))\Big((1-\delta)\lambda G(Y^b) + \delta\Big)\Psi^b_{Y^b}\Big],$$

where $\Lambda(Y) \equiv 1-\gamma+\gamma[\delta+(1-\delta)\lambda G(Y)]$ and $\pi(Y) \equiv \int_{\overline{E}_y(Y)}(y-w)g(y)dy$ for any Y. Plugging this equation for Ξ^b into (A.4) and simplifying, we get

$$\Psi_y^b \;=\; \frac{1}{\Gamma}\left[y-w+\frac{\gamma(1-\delta)\lambda}{\Lambda(Y^b)}\;\pi(Y^b)\right] \;+\; \frac{\gamma[\delta+(1-\delta)\lambda G(Y)]}{\Lambda(Y^b)}\Psi_{Y^b}^b,$$

where $\Gamma \equiv 1-\gamma(1-\delta)(1-\lambda)$. With this, we can solve for $\Psi_{Y^b}^b$:

$$\Psi_{Y^b}^b \;=\; \frac{1}{(1-\gamma)\Gamma}\Big[\Lambda(Y^b)(Y^b-w) \;+\; \gamma(1-\delta)\lambda\pi(Y^b)\Big].$$

Plugging in this equation and (A.6) into (A.7) and simplifying, we get

$$Y^b-w \;=\; \frac{1-\Lambda}{1-\Lambda G(Y^b)}\;\big(\Pi-c\big) \;+\; \frac{\Lambda}{1-\Lambda G(Y^b)}\;\pi(Y^b). \tag{A.8}$$

Next, consider the just cause environment. Recall that the manager can choose two thresholds: one for new workers and one for existing workers (denoted Y_n and Y, respectively, with steady-state optimal values denoted Y_n^{jc} and Y^{jc}, respectively). Define $\Psi_{x,\epsilon}^{jc}$ to be the lifetime discounted value to the manager of employing an existing worker with productivity x and discrimination value ϵ:

$$\begin{aligned}\Psi_{x,\epsilon}^{jc} \;=\; x+\epsilon-w \;+\; \gamma\Big[&(1-\delta)(1-\lambda)\Psi_{x,\epsilon}^{jc}+(1-\delta)\lambda\Xi^{jc}\\ &+\Big((1-\delta)\lambda\big(G(Y^{jc})-P(Y^{jc},X_{ill})\big)+\delta\Big)(\Psi_n^{jc}-c)\Big],\end{aligned} \tag{A.9}$$

where

$$\Xi^{jc} \;\equiv\; \int_{\overline{E}(Y^{jc},X_{ill})}\Psi_{x,\epsilon}^{jc}f(x)\pi(\epsilon)dxd\epsilon. \tag{A.10}$$

Note that the three terms in the brackets are analogous to what they are in (A.4), except that the second and third terms account for some workers being protected by the just cause law. Also, define Ψ_n^{jc} to be the expected value of a new hire:

$$\Psi_n^{jc} \;=\; \Pi \;+\; \gamma\Big[(1-\delta)\Xi^{jc} \;+\; \Big((1-\delta)\lambda\big(G(Y^{jc}) - P(Y^{jc}, X_{ill})\big) + \delta\Big)(\Psi_n^{jc} - c)\Big], \quad \text{(A.11)}$$

The optimal new match firing threshold will be at the point where the manager is indifferent as to keeping the worker versus replacing that worker with a new one: $\Psi^{jc}_{Y_n^{jc}} = \Psi_n^{jc} - c$.[4] Because the continuation value of an existing worker is the same as that of a new worker (holding (x, ϵ) fixed),[5] The optimal existing-match firing threshold will satisfy the same equation:

$$\Psi^{jc}_{Y^{jc}} \;=\; \Psi_n^{jc} \;-\; c. \quad \text{(A.12)}$$

It follows that the optimal firing thresholds for new and existing matches are the same: $Y^{jc} = Y_n^{jc}$. For simplicity's sake, we will refer to them both as Y^{jc}.

We turn to characterizing Y^{jc}. Our strategy for characterizing Y^{jc} is the same as for Y^b in the baseline environment. First, we plug (A.9) into (A.10) and use (A.12) to get

$$\Xi^{jc} \;=\; \frac{1}{\Lambda(Y^{jc}, X_{ill})}\Big[\pi(Y^b) + \gamma\big(1-G(Y^{jc})+P(Y^{jc}, X_{ill})\big)\Big((1-\delta)\lambda\big(G(Y^{jc})-P(Y^{jc}, X_{ill})\big)+\delta\Big)\Psi^b_{Y^b}\Big],$$

where $\Lambda(Y^{jc}, X_{ill}) \equiv 1 - \gamma + \gamma\big[\delta + (1-\delta)\lambda\big(G(Y^{jc}) - P(Y^{jc}, X_{ill})\big)\big]$. Plugging this equation for Ξ^{jc} into (A.9) and simplifying, we get

$$\Psi_y^{jc} \;=\; \frac{1}{\Gamma}\left[y - w + \frac{\gamma(1-\delta)\lambda}{\Lambda(Y^{jc}, X_{ill})}\,\pi(Y^{jc})\right] \;+\; \frac{\gamma[\delta + (1-\delta)\lambda\big(G(Y^{jc}) - P(Y^{jc}, X_{ill})\big)]}{\Lambda(Y^{jc}, X_{ill})}\Psi^{jc}_{Y^{jc}}.$$

[4] Here, we abuse the notation in writing $\Psi^{jc}_{Y_n^{jc}}$ to be any $\Psi^{jc}_{x,\epsilon}$ so that $x + \epsilon = Y_n^{jc}$. Using this notation is okay, because for y values at or above Y^{jc} (which, as we will show, is equal to Y_n^{jc}), the value $\Psi^{jc}_{x,\epsilon}$ only depends on $x + \epsilon$. We cannot abuse notation in this way for $x + \epsilon < Y^{jc}$, because $\Psi^{jc}_{x,\epsilon}$ depends on x and ϵ in that case.

[5] The continuation value for both a new and an existing worker with (x, ϵ) is the term in brackets in (A.9). They are the same because, after the first period, the new worker is protected by the law (just as the existing worker already was).

With this, we can solve for $\Psi^{jc}_{Y^{jc}}$:

$$\Psi^{jc}_{Y^{jc}} \;=\; \frac{1}{(1-\gamma)\Gamma}\Big[\Lambda(Y^{jc})(Y^{jc}-w) \;+\; \gamma(1-\delta)\lambda\pi(Y^{jc})\Big].$$

Plugging in this equation and (A.11) into (A.12) and simplifying, we get

$$\begin{aligned} Y^{jc}-w \;=\; & \frac{1-\Lambda}{1-\Lambda\big(G(Y^{jc})-P(Y^{jc},X_{ill})\big)}\;\Bigg(\Pi - c + \gamma(1-\delta)P(Y^{jc},X_{ill})\Psi^{jc}_{Y^{jc}} \\ & -\;\gamma(1-\delta)\int_{E^p_x(Y^{jc},X_{ill})}\Psi^{jc}_{x,\epsilon}f(x)\phi(\epsilon)d(x,\epsilon)\Bigg) \\ & +\;\frac{\Lambda}{1-\Lambda\big(G(Y^{jc})-P(Y^{jc},X_{ill})\big)}\;\pi(Y^{jc}). \end{aligned} \tag{A.13}$$

This characterization of Y^{jc} is analogous to that of Y^b in (A.8), except that it accounts for some workers being protected by the just cause law. Not only do the two denominators include $P(Y^{jc},X_{ill})$, but the first term includes

$$\gamma(1-\delta)P(Y^{jc},X_{ill})\Psi^{jc}_{Y^{jc}} \;-\; \gamma(1-\delta)\int_{E^p_x(Y^{jc},X_{ill})}\Psi^{jc}_{x,\epsilon}f(x)\phi(\epsilon)d(x,\epsilon). \tag{A.14}$$

Finally, we prove that $Y^{jc} > Y^b$. Assume for sake of contradiction that $Y^{jc} \leq Y^b$. Assumption 1 gives us that $P(Y^b, X_{ill}) > 0$, and since $Y^{jc} \leq Y^b$, it must also be that $P(Y^{jc},X_{ill}) > 0$. The equation in (A.14) is strictly positive because every $(x,\epsilon) \in E^p_x(Y^{jc},X_{ill})$ is such that $x+\epsilon < Y^{jc}$; $\Psi^{jc}_{x,\epsilon}$ is increasing in $x+\epsilon$; and

$$P(Y^{jc},X_{ill}) \;=\; \int_{E^p_x(Y^{jc},X_{ill})} f(x)\phi(\epsilon)d(x,\epsilon) \;>\; 0.$$

Using that (A.14) is positive, it follows from (A.13) that

$$Y^{jc}-w \;>\; \frac{1}{1-\Lambda\big(G(Y^{jc})-P(Y^{jc},X_{ill})\big)}\;\Big((1-\Lambda)(\Pi-c) \;+\; \Lambda\,\pi(Y^{jc})\Big).$$

Further using that $P(Y^{jc}, X_{ill}) > 0$, the right-hand side of this equation is strictly greater than

$$\frac{1}{1-\Lambda G(Y^{jc})}\Big((1-\Lambda)(\Pi - c) \; + \; \Lambda\; \pi(Y^{jc})\Big).$$

Consider the above equation as a function of Y:

$$h(Y) \;\equiv\; \frac{1}{1-\Lambda G(Y)}\Big((1-\Lambda)(\Pi - c) \; + \; \Lambda\; \pi(Y)\Big).$$

From the previous equations, it follows that $Y^{jc} - w > h(Y^{jc})$. Notice that $Y^b - w = h(Y^b)$. Comparing the functions $Y - w$ to $h(Y)$, we see that if $h'(Y) < 1$ for all Y, then the two functions would only have a single crossing point at Y^b. If that were true, every $Y < Y^b$ would be such that $Y - w < h(Y)$, and every $Y > Y^b$ would be such that $Y - w > h(Y)$. In this case, since $Y^{jc} - w > h(Y^{jc})$, it would follow that $Y^{jc} > Y^b$, which is our desired result. We make this assumption here.

Assumption 2 *The derivative of $h(Y)$ is less than 1 for every $Y \in (0, 1)$.*

This assumption seems reasonable, because $h(Y)$ achieves a local maximum at $Y = Y^b$; i.e., $h'(Y^b) < 0$ and $h''(Y^b) = 0$. Indeed, the derivative is

$$h'(Y) \;=\; \frac{\Lambda g(Y)}{[1-\Lambda G(Y)]^2}\Big[(1-\Lambda)(\Pi - c) \; + \; \Lambda\; \pi(Y) \; - \; (1-\Lambda)G(Y)\;(Y - w)\Big].$$

The fact that $h'(Y^b) = 0$ can be seen by substituting the right-hand side of (A.8) in for $Y^b - w$ in the last term. The second derivative is

$$h''(Y) \;=\; -\frac{\Lambda g(Y)}{1-\Lambda G(Y)} + \left[\frac{\Lambda g'(y)}{[1-\Lambda G(Y)]^2} \; + \; \frac{2\Lambda^2 g(Y)^2}{[1-\Lambda G(Y)]^3}\right] \Big[(1-\Lambda)(\Pi - c) \; + \; \Lambda\; \pi(Y) \; - \; (1-\Lambda)G(Y)\;(Y - w)\Big].$$

Substituting the right-hand side of (A.8) in for $Y^b - w$ in the last term, we see that $h''(Y^b) = -\Lambda g(Y^b)/(1 - \Lambda G(Y))$, which is negative.

Though Assumption 2 is sufficient, it may not be necessary. Indeed, a weaker assumption on primitives may also be sufficient. This concludes the proof of Theorem 1. ■

This result on thresholds allows us to compare steady-state turnover before and after the law (note that hires and separations are equal in a steady state). Using the steady-state versions of the shift equations in (A.1) and (A.3), it can be calculated that steady-state hires in the baseline environment are

$$H^b = \frac{\delta + (1-\delta)\lambda G(Y^b)}{1-(1-\delta)(1-\lambda)G(Y^b)},$$

and that after the just cause law they are

$$H^{jc} = \frac{\delta + (1-\delta)\lambda [G(Y^{jc}) - P(Y^{jc}, X_{ill})]}{1-(1-\delta)(1-\lambda)[G(Y^{jc}) - P(Y^{jc}, X_{ill})]}.$$

Note that the change in hires is ambiguous. Indeed, we can decompose the change in hires into two opposing effects. First, the increase from H_0 to

$$\frac{\delta + (1-\delta)\lambda G(Y^{jc})}{1-(1-\delta)(1-\lambda)G(Y^{jc})} \tag{A.15}$$

is the *increasing standards channel*. Ceteris paribus, a greater firing threshold ($Y^{jc} > Y^b$) will leave more shifts to replace through hiring. Second, the change from (A.15) to H_1 is the *protection channel*. Ceteris paribus, the protection of workers with $x \geq X_{ill}$ will decrease the number of firings and, therefore, hires in a steady state.

Next, we can split hires into two mutually exclusive categories: nonstable hires H_n and stable hires H_s (where $H = H_n + H_s$). *Nonstable hires* are those who are fired after one period, and *stable hires* are those who last more than one period. For both the baseline and just cause environments ($i \in \{b, jc\}$), steady-state nonstable and stable hires are split as

$$H_n^i \;=\; H^i[\delta + (1-\delta)G(Y^i)], \qquad H_s^i \;=\; H^i(1-\delta)(1-G(Y^i)). \tag{A.16}$$

We define a *nonstable separation* to be the same as a nonstable hire, and a *stable separation* to be the separation of an employee who lasted more than one period. Since hires and separations are the same in steady state, H^i (i.e., steady-state hires in environment i) also connotes steady-state separations (in environment i). Furthermore, since *nonstable* hires and separations are defined in the same way, steady-state *stable* hires and separations are also the same. Since total, stable, and nonstable hires and separations are the same in a steady state, we use *turnover* to refer to either hires or separations in what follows. With this terminology, we note an above-described corollary for reference in the manuscript's body.

Corollary 1 *Compared to a steady state of the baseline environment, the turnover rate (i.e., both the hiring rate and the separation rate) changes ambiguously in a steady state of the just cause environment.*

Since $Y^{jc} > Y^b$, a greater share of turnover in the just cause environment will be nonstable.

Proposition 1 *(first main prediction) Compared to a steady state of the baseline environment, the stable share of turnover (i.e., both the stable share of hires and the stable share of separations) is lower in a steady state of the just cause environment.*

Proof of Proposition 1 Since the PDFs of x and ϵ (f and π respectively) are both continuous, the PDF of $y = x + \epsilon$ (g) is also continuous. Therefore, $Y^{jc} > Y^b$ implies that $G(Y^{jc}) > G(Y^b)$. Using this and (A.16), we get that the stable share of turnover decreases under just cause. ■

This result on thresholds also helps us show that the stable separation rate increases for favored workers relative to disfavored workers.

Proposition 2 *(second main prediction) Compared to a steady state of the baseline environment, the stable turnover rate (i.e., both the stable hiring rate and the stable separation rate) for high-ϵ workers (relative to low-ϵ workers) is greater in a steady state of the just cause environment.*

Proof of Proposition 2 Consider Figure 2. Since $Y^{jc} > Y^b$, the figure is accurate. In particular, the thick line (the just cause threshold) is above the thin line (the baseline threshold), and the regions J_1, J_2, and J_3 exist and are properly described.

From here, a graphical argument is sufficient. We repeated the argument given in the manuscript's body for the stable separation rate. Since the stable separation rate is equal to the stable hiring rate in a steady state, this is sufficient.

While types in J_1 are not fired in the baseline environment, they will be fired after (as well as during) the probationary period under just cause. On the other hand, while types in J_3 were always fired in the baseline, they cannot be fired after the probationary period under just cause (types in J_2 are never fired in the baseline and cannot be fired after the probationary period under just cause). Therefore, since all types in J_1 have a higher ϵ than those in J_3, just cause will necessarily increase the stable separation rate for workers with high ϵ relative to those with low ϵ. $\blacksquare$

Similar to the counteracting effects on the overall turnover rate, legal protections have a bigger benefit for the stable employment of disfavored workers. Indeed, the low-ϵ types in J_1 are protected by the just cause law after their probationary period, while the high-ϵ types in J_3 are not. This *protection channel* increases the stable employment of low-ϵ workers relative to high-ϵ workers.

On the other hand, increasing firm standards may negate this effect. Indeed, the J_2 region—which may contain many low-ϵ workers, depending on the distribution—will not achieve stable employment post–just cause, despite doing so pre–just cause. The same is true of the J_1 region containing the high-ϵ types. However, without knowledge of the worker distribution, we cannot say whether this *increasing-standards channel* relatively benefits workers with a high or low ϵ. If it relatively benefits high-ϵ workers, this channel may dominate the opposing effect from the protection channel. Thus, the overall effect of just cause on relative stable employment is ambiguous.

Corollary 2 *Compared to a steady state of the baseline environment, the stable employment of high-ϵ workers (relative to low-ϵ workers) changes ambiguously in a steady state of the just cause environment.*

B Empirical appendix

This appendix provides details on several empirical details. Appendix B.1 provides a short background on the QWI and some details on variables that we omitted from the empirical design section of the body. Appendix B.2 gives context and justification for our definitions of stable variables. Appendix B.3 details exclusions from the control group. Appendix B.4 justifies comparisons of worker groups for the relative analysis. Appendix B.5 gives details on the construction of our county-level minimum-wage data set. Finally, Appendix B.6 addresses a few technical points on estimation. Overall, our methods closely follow Pickens and Sojourner (2025).

B.1 QWI background and variables

The QWI provides quarterly state- and county-level labor market statistics by four-digit NAICS industry. It is derived from the Longitudinal Employer-Household Dynamics (LEHD) linked employer-employee microdata. The LEHD combines several data sources, including quarterly reports from state agencies across the country. Other sources relevant to the QWI include quarterly UI data, which provides employment and earnings data at the job level; Quarterly Census of Employment and Wages (QCEW) data, which details a firm's industry, worksite locations, and ownership; Business Dynamics Statistics (BDS) data, which provides age and size information for private-sector firms; and a variety of other sources to provide demographic information. These sources allow QWI data to be split by firm age and size, and by worker age, sex, education, race, and ethnicity. As mentioned in the empirical-design section of the body, the longitudinal structure of the source microdata (LEHD) enables reporting of additional variables beyond employment and earnings, such as hires, separations, and stable versions of variables.

The measure of (total) employment we use is the sum of the counts of people employed in each of the firms in the county-industry at any time during the quarter (i.e., they must

county	estimate of covered workers	*QCEW* 722513 emp.	proxy quality	*QWI* 7225 emp.	proxy quality
Bronx	6,550	7,409	0.884	12,039	0.544
King	13,724	15,525	0.884	36,474	0.376
New York	33,150	37,500	0.884	131,608	0.252
Queens	13,729	15,530	0.884	32,097	0.428
Richmond	2,596	2,937	0.884	6,220	0.417
total	**69,748**	**78,901**	**0.884**	**218,438**	**0.319**

Table B.1: Comparing quality of proxies from QWI and QCEW

Notes: In our analysis, we use two proxies for workers covered by the legislation: primarily the New York City *restaurants and other eating places* industry (7225) from the QWI and in one instance the New York City *limited-service restaurant* industry (NAICS code 722513) from the QCEW. This table compares the quality of these two proxies for each of New York's five counties. The second and fourth columns are the average 2019 quarterly employment in 722513 and 7225, respectively. The first column estimates the number of workers that would be covered under the provision if it had been effective in 2019. These estimates are obtained by multiplying the second column by the nonsupervisory share of 722513 employment; this share is taken from Wolfe et al. (2018), who uses a national estimate of 88.4 percent for 2016. In the third and fifth columns, we define *proxy quality* to be the ratio of our covered worker estimate to the (respective) industry's employment.

have positive earnings at a specific employer in the county-industry-quarter). The U.S. Census Bureau calls this measure *flow employment*; instead of this, the Bureau recommends using *beginning-of-quarter employment* as a measure of total employment (Census 2019). A worker is beginning-of-quarter employed if he or she has positive earnings in the reference quarter and the prior quarter. We use flow employment instead because beginning-of-quarter employment misses all workers who start and end a job within a quarter; we want to capture those workers in our employment measure.

In our analysis, we use two proxies for workers covered by the legislation. Primarily, we use the New York City *restaurants and other eating places* industry (NAICS code 7225) from the QWI. In one instance in the additional analysis of Appendix D, we use New York City's *limited-service restaurant* industry (NAICS code 722513) from the Quarterly Census of Employment and Wages (QCEW). Both proxies contain all affected workers in the fast food industry, but since the latter is a subset of the former, it is a better proxy. Unfortunately, the QCEW only contains employment data, so we cannot use it to evaluate our other outcomes of interest.

Though 722513 includes all New York City workers affected by the regulation, it also includes unaffected workers. Indeed, supervisory jobs are not covered by the regulation, but those employees are included in 722513. Calculations from Wolfe et al. (2018) suggest that more than 10 percent of 722513 New York City workers are unaffected by the legislation.[6]

Since 7225 is a coarser industry classification, this issue worsens in the QWI. Table B.1 demonstrates this problem and compares QCEW and QWI proxy quality for the covered worker population. We define *proxy quality* to be the ratio of estimated covered workers to industry employment (see the table's notes for details). Notice that the QWI's proxy quality is notably lower than the QCEW's. Indeed, based on our estimate, the majority of 7225 employees (68.1 percent) are not covered under the legislation. These shortcomings of the QCEW and QWI will tend to attenuate estimates toward zero, especially for the QWI.

B.2 Stable variables

Here, we provide context for and justify our use of *stable* variables. For completeness, we repeat some details already mentioned in the manuscript. The QWI provides a specific definition of stability that we generally follow. However, our definitions of stable hire and stable separation are slightly different (Census 2019).

Before introducing definitions, we first provide context. In our theoretical model, the increased firing threshold implies that it becomes harder for an employee to make it past one period of employment. Capturing this prediction in the data presents challenges. First, before the law took effect, there was no legal distinction between probationary and nonprobationary employment, so we could not say the law made it harder to move from probationary

[6]In estimating the number of workers covered under the regulation, Wolfe et al. (2018) multiplies New York's 722513 employment by the share of nonsupervisory workers in that industry at the national level. This share, 88.4 percent, is taken from 2016 Current Employment Statistics (CES) data. Also, to be covered, an employee must work in an establishment that "is part of a chain" and "is one of 30 or more establishments nationally, including as part of an integrated enterprise or as separately owned franchises" (DCWP 2023). An unknown fraction of limited-service restaurant employees in New York are not covered because they don't meet these criteria. Unfortunately, the QCEW provides no data to help estimate this fraction. Following Wolfe et al. (2018), we will not adjust for this factor when computing proxy quality.

to nonprobationary employment. Instead, our prediction is that the law increases the probability of a new employee being a *short-term* employee. In the context of the model, we think of one period as 30 days, and so a "short-term worker" is defined as one who works 30 days or less. Given that the QWI provides quarterly aggregate measurements, it does not have the granularity necessary to identify which workers are "short-term." Instead, we proxy short-term employees with *nonstable* employees.

Recall that an *employee* is anyone employed in the given quarter. *Stable employees* are those employed in the given quarter, the previous quarter, and the following quarter; *nonstable employees* are simply the complement. We define stable and nonstable versions of hires and separations similarly: a *stable hire* is an employee who worked in a given quarter and the next two quarters, but not the previous quarter; a *stable separation* is one who worked in a given quarter and the previous two quarters, but not the next quarter. A nonstable hire and a nonstable separation are the respective complements. As mentioned in footnote 13, we use two quarters in empirical measures to capture stability, because a worker can be hired at the end of a quarter or separated at the beginning of a quarter. This ensures that all stable hires and separations in the data are employed longer than 30 days, the length of the probationary period in the just cause law.

Similar to the hiring rate (separation rate), the *stable hiring rate* (*stable separation rate*) is stable hires (stable separations) divided by stable employment. Finally, the *stable share of employment* is the fraction of all employment that is stable in a given quarter; the stable shares of hires and separations are defined analogously.

To evaluate the first main prediction—i.e., that just cause makes it harder to achieve stable employment—we analyze the stable share of hires. A decrease in this share would suggest that the law makes it harder to stay employed past the probationary period. To evaluate the second main prediction—that just cause makes it relatively easier for disfavored groups to retain stable employment—we analyze the stable separation rate of disfavored groups. A decrease in this rate (relative to a corresponding favored group) would suggest that the law makes it relatively easier for disfavored groups to keep their jobs after the probationary period. As mentioned, our empirical variables are defined to correspond to the model variables as closely as possible so that we can evaluate predictions the best we can, but perfect correspondence is impossible given our data. The connection between key model and empirical variables is summarized in Table 1.

B.3 Exclusions from control group

Recall that our two main approaches are the within-industry model, which takes other 7225 (restaurants and other eating places) industries around the country as the control group, and the within-location model, which takes other New York City county-industry pairs as the control group. We make several restrictions on these control groups, and these restrictions closely follow Pickens and Sojourner (2025), with some differences.

For the within-industry approach, the control group is restricted to census-designated "central counties" in metro areas that had more than 1,000,000 people in the 2020 census (Census 2020; Census 2024b). For both the within-industry and the within-location approaches, when evaluating all worker groups together, as in Table 2, we exclude units that have an average total employment of less than 5,000 across our period of analysis (2018 Q1 to 2023 Q2). When comparing *two* worker groups for relative outcomes, as in Table 3, we only exclude units if they fail to meet the 1,000 employee threshold for either group. For example, when comparing white and black workers, a unit must have an average total employment of at least 1,000 for both white and black workers to be included. Also, for all models, we exclude units that have any blank or zero values for variables over the period we consider. Finally, note that all variables are adjusted for seasonality.

B.4 Justifying comparisons

Our relative outcomes consider four favored-disfavored group comparisons: white-black, white-Hispanic, male-female, and younger-older. Note that "white" and "black" are white non-Hispanic and black non-Hispanic (respectively), "younger" workers are under age 35, and "older" workers are age 35 and older.

The decision to consider black, Hispanic, and female workers disfavored by managers follows the motivation for the law discussed in the first two sections (which, among other things, was adopted to prevent discrimination against women and racial minorities).[7] Relevant law assumes older workers to be disfavored: in the U.S., hiring and employment protections are extended to workers aged 40 and older by the Age Discrimination and Employment Act of 1967 (DOL 2024). This suggests that if managers exercise favoritism by age, it is likely, on average, to be against older workers. The age-group breakdown in the QWI is 14–18, 19–21, 22–24, 25–34, 35–44, 45–54, 55–64, and 65 and older. We collapse these into two categories—14–34 and 35 and older—which roughly splits the data in half.

B.5 Minimum wage dataset

During our period of analysis, 2018 Q1 to 2023 Q2, there occurs a significant change in minimum wage policy in states and localities around the country. Some of these changes may affect counties that are donor units for our within-industry model. For this reason, we control for the minimum wage in one of the within-industry specifications.

To this end, we construct a quarterly county-level minimum-wage data set to span our period of analysis. We draw from two sources to account for state and local minimum-wage changes: Vaghul and Zipperer (2022) through the end of 2022 and EPI (2024) for 2023.

[7]However, it is worth noting that the characteristics of managers may be relevant. In particular, a female, black, or Hispanic manager would seem less likely to discriminate against a female, black, or Hispanic worker, respectively. For this reason, we briefly consider publicly available data on food service managers. According to the 2018 American Community Survey, of the 22,315 food service managers in New York City, 67 percent are male, 36 percent are white, 14 percent are black, 26 percent are Hispanic, and 22 percent are Asian; this is compared to the respective national percentages of 52, 61, 18, 9, and 9 (Census 2024a). So the food service managers in New York are disproportionately male and minority relative to the rest of the country. Also, nationally, most food service managers work in the restaurant-and-other-eating-places industry which we use in our empirical analysis: according to a national summary of the occupation from the Bureau of Labor Statistics (BLS) website, 74 percent of such managers are employed in this industry (as of May 2022; the BLS did not do an occupation profile in 2018) (BLS 2023). This suggests that the above breakdown of food service managers in New York City is likely to reflect the breakdown in the New York industry we are studying.

The following is taken directly from Pickens and Sojourner (2025), in which the same strategy is used:[8] "For each quarter, we consider the minimum wage to be its value on the first day of the quarter. To incorporate substate changes into our county-level dataset, we take the population-weighted average of the minimum wage at the beginning of the quarter. In particular, weights are based on city and county-level population estimates from the 2020 Census (Census 2023a; Census 2023b). Thus, weights are fixed over time. As an example, consider the minimum wage changes in Flagstaff, Arizona, that began in 2018. Flagstaff is located in Coconino County and in 2020, it made up 53% of the population in the county. Over our period, 2014 Q1 to 2019 Q4, the minimum wage in Coconino County outside Flagstaff coincided with the Arizona state minimum wage, which underwent annual increases. In 2018, the minimum wage in Flagstaff rose above that in the rest of Coconino County, increasing each year to stay above the Arizona minimum wage. By 2019 Q4, the Flagstaff minimum wage was $12, though the Arizona minimum wage was $11. For 2018 Q1 and after, we take the minimum wage in Coconino County to be 0.53 times the Flagstaff minimum wage plus 0.47 times the Arizona state minimum wage. For example, in 2019 Q4, this was $11.53."

B.6 Technical points on SDID

Recall that we employ the synthetic difference-in-differences (SDID) framework of Arkhangelsky et al. (2021). To do so, we use the *R* package *synthdid* from Hirshberg (2019). Following Pickens and Sojourner (2025), we compute standard errors using the jackknife estimator. The following justification for this decision comes straight from Pickens and Sojourner (2025): "For inference in our SDID framework, we compute standard errors using the jackknife estimator. It computes quicker than other methods and has desirable properties under relatively mild assumptions (Arkhangelsky et al. 2021). The relevant theoretical result of Arkhangelsky et al. (2021) is to prove the estimator is asymptotically normal under assumptions that,

[8] Note that the period of analysis in Pickens and Sojourner (2025) is 2014 Q1 to 2019 Q4, which is different from the one used in this paper.

'are substantially weaker than those used to establish asymptotic normality of comparable methods' (page 4107). Using these assumptions and assuming that the systematic component of the data-generating process is finite, the jackknife estimator yields conservative confidence intervals. Also, assuming that the treatment effect is constant and time weights are predictive enough on the exposed units, the jackknife yields exact confidence intervals. However, because we assume uniform time weights, this result is not established for our main specification. No similar results are established for the alternative estimators."

The SDID algorithm chooses unit and time weights for the synthetic control. We decide to depart from the default specification in Arkhangelsky et al. (2021) and use uniform time weights (in a similar way to Karabarbounis et al. 2022). The following justification is, again, taken directly from Pickens and Sojourner (2025): "Time weights are chosen so that the difference in the time-weighted average of pre- and post-policy values is approximately the same across units, and a penalty term on the time-weight vector is used to ensure uniqueness. Similarly, a penalty term is used when computing the unit weights. However, the regularization term in the penalty (i.e., the default in Arkhangelsky et al. 2021) is much larger for unit weights than for time weights. For time weights, the weight is 10^{-6}, and for unit weights, it is at least 1. In our main specifications, we depart from the default in Arkhangelsky et al. (2021) and give equal weight to all pre-policy periods." Without equal weights, time weights are often strongly biased toward the last prepolicy period or the last few prepolicy periods; it was common for all weight to be put on the last prepolicy period.

C Additional context on treated and control groups

This appendix gives context on treated and control groups. First, we compare prepolicy averages from the treated group and the synthetic controls. Next, we provide detail on synthetic control weights for two of our main tests (which helps motivate the design of our triple-difference analysis in Appendix E). Last, we provide summary statistics for our proxy of the covered worker population (i.e., the "treated group") broken down by worker demographics.

Comparing treated and control groups: The reader may find it useful to see differences between the treated group and the synthetic control in the prepolicy period (i.e., 2018 Q1 to 2021 Q2). Table C.1 reports these differences for the pooled specification of our main tests in Tables 2, 3, and 4. For each test, we report the prepolicy average of the relevant variable. When we are assessing a relative outcome—i.e., comparing two demographic groups as in the last eight rows—we report the averages for both demographic groups, for both the treated group and the synthetic control.

Note also two things: First, the SDID method allows for a gap between the treated group and the synthetic control that is constant in the prepolicy periods. Thus, the prepolicy averages for the treated group and the synthetic control need not be the same. Second, the outcome variable of interest for relative analysis (i.e., with two demographic groups) is the difference between the favored and disfavored groups.

There are two main takeaways. First, the "treated group"—the New York "restaurants and other eating places" industry, our proxy for the covered worker population—has a higher prepolicy average for the stable share of hires and the stable separation rate (not only for the overall population, but for both favored and disfavored groups). Second, white and older workers have a lower stable separation rate and a higher stable share of hires compared to black and younger workers.

Synthetic control weights: Table 2a gives information on the synthetic controls for our empirical test of the first main prediction. In particular, for each of the four empirical models, it reports the five heaviest-weighted units in the synthetic control. The weight corresponds to a fraction of the total weight (i.e., all weights for a synthetic control sum to one). Table 2b does the same for our test of the second main prediction, specifically for the white-Hispanic comparison.

We note a few observations about these weights. First, the weights are spread out across control units. Indeed, the five heaviest weights account for approximately 15–25 percent of the total weight of each synthetic control. Second, the *restaurant and other eating places* industry in Milwaukee County, Wisconsin, appears to have the most similar prepolicy labor market patterns to those in New York City's five counties (at least for the two variables being evaluated here). It has the heaviest weight by at least 2 percentage points for all within-industry specifications listed. Furthermore, it is the only within-industry control to make the top five list in either of the pooled models. Third, the *personal care services* industry in New York City's various counties appears to be the most similar to New York's restaurant industry. That industry makes up three of the five heaviest weights for each of the within-location and pooled models.

The similarity of the restaurant industry in Milwaukee County, Wisconsin, and New York's "personal care services" industry to New York's "restaurant and other eating places" industry motivates their use in a triple-difference exercise (see Appendix E).

Summary statistics for the treated group: Next, we briefly analyze data on our proxy of the covered worker population (i.e., the "treated group") before and after the law became effective—specifically, a quarterly average of the year before the law became effective (2020 Q3 to 2021 Q2) versus the same for the year after the law became effective (2021 Q3 to 2022 Q2).

Table C.3 gives a percentage breakdown of employment, hires, and separations in these two periods by sex and age categories. Note that the age categories are more granular in these tables than in our empirical analysis. Table C.4 does the same for race and ethnicity. The four race and ethnicity categories considered are white (non-Hispanic), black (non-

Hispanic), Hispanic, and other. To start, consider some "static" observations about these tables (i.e., looking at the preperiod only). *Approximately half* of employees, hires, and separations are under age 35, and approximately half are 35 and older. This is why we compare workers above and below this cutoff in our analysis. With that being said, workers under 35, especially women, experience proportionally more turnover than older workers. For example, in the year before the law became effective, women under 35 accounted for 24.8 percent of employment but 29.0 percent of turnover. Also, among races, black workers experience proportionately the most turnover.

Now, consider a few changes from the pre- to the postperiod. First, employment and (especially) turnover shift from workers 35 and older toward the youngest workers (14–24). Indeed, this youngest group's share of employment, hires, and separations increases by 1.1, 3.3, and 3.2 percentage points, respectively. Second, there is a relative increase in white, black, and Hispanic employment (all less than 1 percentage point) at the expense of the "other" category. Third, black workers experience a large relative increase in hires (2.2 percentage points).

Tables C.5 and C.6 are analogous to Tables C.3 and C.4, respectively, but for *stable* employment, hires, and separations. Consider a few observations. First, in comparing these figures to those in Tables C.3 and C.4, the stable share of employment and turnover is generally higher for workers 35 and older than for those under 35. Second, in comparing the pre- and postperiod, stable hires shift from men 35 and older to women under 35. Third, there is a relative increase in white stable employment (1.4 percentage points) and a decrease in black stable employment (0.7 percentage points) from pre- to postperiod. This is despite the fact that black workers experience a large relative increase in stable hires (1.9 percentage points). Fourth, white workers experience a large relative increase in stable separations (2.9 percentage points), while black and Hispanic workers experience a (proportionally) similar decline (2.2 and 1.4 percentage points, respectively).

Finally, the absolute numbers of variables in the treated group before and after just cause became effective are in Table C.7. The large swings in magnitudes are likely the result of adjustments to the COVID-19 pandemic. The above patterns in Tables C.3, C.4, C.5, and C.6 may be the result of COVID-19 having different effects across groups. Our empirical strategy allows us to separate effects of the just cause law from those of COVID-19 as long as COVID-19 has consistent effects across treated and control units.

D Results for additional variables

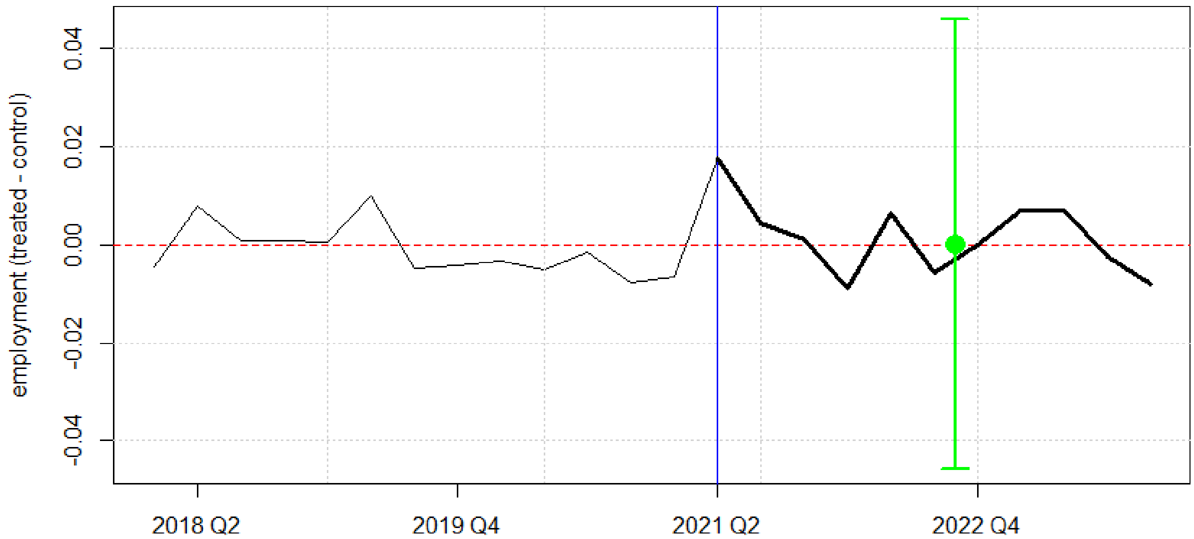
Table D.1 details the results for eight additional whole-sample outcome variables: employment, the hiring rate, the separation rate, stable employment, the stable hiring rate, the stable separation rate, the stable share of employment, and the stable share of hiring. Results for the stable share of hiring (from Table 2) are repeated in the eighth row. Note that data for the first variable, employment, come from the Quarterly Census of Employment and Wages (QCEW), which provides a better proxy of the covered worker population: New York City's limited-service restaurant industry (NAICS code 722513). See Appendix B.1 for details.

Notice that all four employment estimates are statistically insignificant at the 5 percent level. The estimate for the pooled model, the best-fitting model, is 0.1 log points (95 percent CI: –4.6 to 4.6). Figure D.1a plots the difference between the treatment group and synthetic control for the pooled model. One can observe a good prepolicy fit, no clear trend, and an average treatment effect statistically close to zero. Note that QCEW data has two more quarters of data (up to 2023 Q4), so the postpolicy period is two quarters longer.

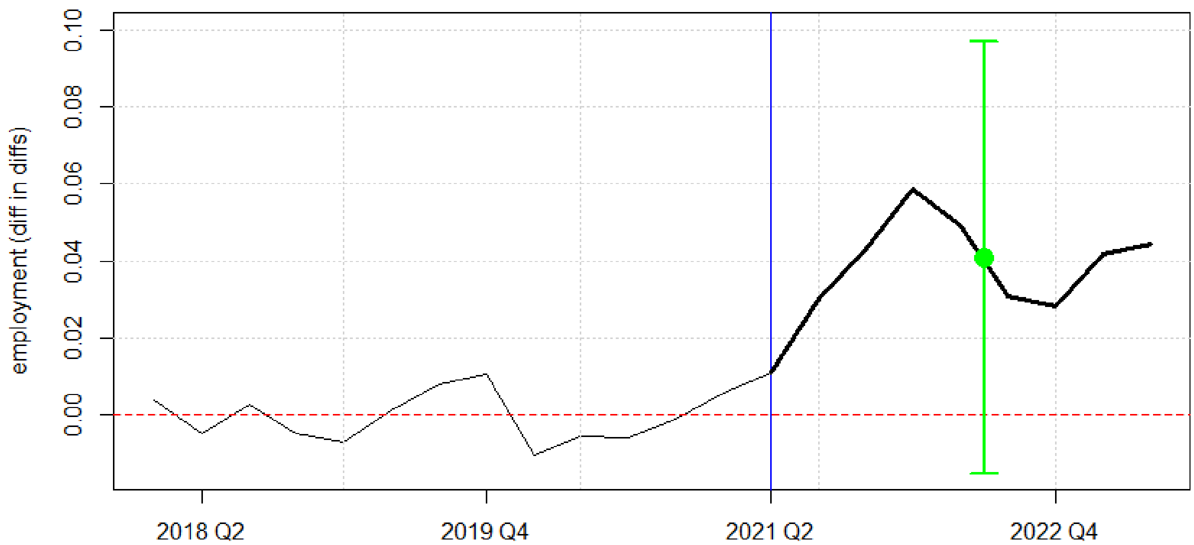
For the other eight variables, all four specifications yield an insignificant estimate (at the 5 percent level). The pooled model consistently fits the best, and one of the within-industry models is consistently the most precise (i.e., has the lowest standard error).

In Appendix A, we show that there are two counteracting forces on the separation rate—an increasing standards channel driving the rate up, and a decreasing standards channel driving the rate down. These counteracting forces yield an overall ambiguous prediction. None of the separation-rate estimates in Table D.1 give any suggestion that one channel dominates the other. Also, the null results on stable employment and the stable share of employment yield no *strong* evidence that just cause makes employment more *secure* for all workers on average.

Next, we analyze relative effects on stable employment; results are in Table D.2. To start, notice that all four models estimate a large increase in white stable employment relative



(a) Effect on employment (QCEW, pooled model)



(b) White versus black stable employment (pooled model)

Figure D.1: Figures from additional analysis

Notes: The top figure corresponds to measuring the effect on employment using the pooled model and QCEW data. Its structure is similar to that in Figure 3, but the y-axis is the difference in log employment between the treated group and the synthetic control. The bottom figure corresponds to measuring the relative effect on the stable employment of white as compared to black workers using the pooled model. The y-axis is a difference-in-differences: the first difference is between white and black log stable employment, and the second difference is between the treated group and synthetic control.

to black stable employment. Despite large standard errors, the average treatment effect is statistically significant (at the 5 percent level) for the two within-industry models. Recall that our theoretical model makes an ambiguous prediction for relative stable employment. This significant result suggests that increasing productivity standards pushed stable employment toward white workers (the assumed favored group), and this dominated any effect of added protection for black workers (the assumed disfavored group).

Figure D.1b shows the treated-control difference. There is a visually good prepolicy fit and a clear positive trend for white stable employment in the postpolicy period. However, the average treatment effect is insignificant (as demonstrated by the confidence interval). For the white/Hispanic difference, all models have *positive* estimates, but none are statistically significant at the 5 percent level. For the male/female and younger/older differences, all models have *negative estimates*, but again, none are statistically significant at the 5 percent level.

E Robustness analysis

The appendix considers robustness tests for our main empirical results. First, to gauge the effects of COVID-19, we rerun analysis when the six quarters of data before the policy became effective are removed. Then, we add a control for unemployment-insurance replacement rates to our within-industry models. Last, we consider two approaches to a triple-difference analysis.

Removing COVID-19 from synthetic controls: The New York City Just Cause Law became effective in July 2021. This coincided with significant policy and labor market changes from the COVID-19 pandemic. These changes vary over location and industry, so they could possibly bias our results. One way to reduce such potential bias is to use only pre-COVID-19 data to construct synthetic controls.

Here, we rerun our main analyses when the six quarters before COVID-19 are removed from the data set; i.e., 2020 Q1 through 2021 Q2 are not used to inform the synthetic control. In particular, we recompute the specifications from Tables 2, 3, and 4, evaluating our two main theoretical predictions and the screening-discrimination mechanism, respectively.

The results are in Table E.1. Estimates are broadly consistent with our main analysis, although some specifications that were insignificantly positive before are now significantly positive, and vice versa.

Adding a UI replacement rate control: As mentioned, significant policy and labor market changes resulted from the response to the COVID-19 pandemic; this is discussed briefly at the end of the background section in the body of the paper. One of those changes was an increase in unemployment insurance (UI) benefits, which differed in administration across states.

To account for changes in UI policy—not just in New York, but across the country—we rerun our main analysis controlling for UI. We do so only for the two within-industry models, since UI policy is constant throughout New York City industries (and, hence, would not be appropriate for the within-location model). The variable we use is a state's quarterly *replacement rate*—state and federal UI benefits as a percentage of the recipient's previous wage, on average, as reported by TCF (2025).

The results are reported in Table E.2. The first two columns restate the results from our main analysis for reference: i.e., from the within-industry model both without and with the minimum wage control. The third column reports results from a specification with only a UI control. The fourth column is for a specification with both a UI and minimum wage control. The within-industry specifications with UI controls yield results consistent with the main analysis, although (again) some specifications that were insignificantly positive before are now significantly positive and vice versa.

Triple-difference models: Our main empirical analysis considers three margins of difference across observations: location, industry, and time. The within-industry model measures differences involving both location and time: it compares New York City's restaurant industry to that in other counties around the country both before and after the policy became effective. The within-location model looks at differences between industry and time: it compares New York's restaurant industry to other New York industries both before and after the policy became effective.

Here, we consider two additional models that delineate all three differences at once. In principle, the third difference could reduce bias. First, we compare the difference between New York City's restaurant industry and another New York City industry to differences between these industries in other counties around the country, both before and after the policy became effective. A natural choice for this other industry is the personal-care services industry, since it appeared to be the most similar New York City industry to the New York restaurant industry (see top panel of Table C.2). We run this triple-difference analysis both with and without a minimum wage control.

Second, we compare the difference between New York's restaurant industry and another location's restaurant industry to differences between these two locations for other industries, both before and after the policy became effective. A natural choice for this other location is Milwaukee County, Wisconsin, since it appeared to be the most similar (other) restaurant industry to New York City's (see bottom panel of Table C.2). We run this triple-difference analysis without a minimum wage control (since all observations are differences between two locations).[9]

Table E.3 details the results. The first two columns consider the first approach, both without and with a minimum wage control, respectively. The third column considers the second approach.

For the first approach, standard errors and RMSPE values are notably larger than in the main analysis. Unsurprisingly, none of the results are statistically significant at the 5 percent level. Standard errors and RMSPE values are also higher on average for the second approach, but not by as much.

Two results from the second approach are worth highlighting. First, the stable separation rate of white workers relative to Hispanic workers decreases significantly at the 5 percent level. This is in contrast to the increase found in Table 3 for all four models, one of which was statistically significant at the 5 percent level. Second, the stable share of hires estimate shows a relative decrease for younger compared to older workers, but the estimate is not statistically significant at the 5 percent level (perhaps due to a higher standard error).

While these discrepancies in results are noteworthy, we view the triple-difference analysis as less credible. Although adding a third difference may reduce bias, it tends to inflate standard errors. Thus, a larger sample is likely needed to detect meaningful effects. In summary, just as in our main analysis, estimates from these triple-difference specifications are suggestive rather than conclusive.

[9]Note that, as in our other specifications, we consider one treated unit for each NYC county. Here, a treated unit is the difference between a NYC county's restaurant industry and that of Milwaukee County; e.g., the difference between restaurant industries in Bronx County and Milwaukee County. The same goes when constructing control units: each industry could have up to five control units. Possible controls (i.e., county-industry pairs) face similar exclusions as detailed in Appendix B.3 for the main analysis.

	population or comparison	*treated group* favored group	*treated group* disfavored group	*synthetic control* favored group	*synthetic control* disfavored group
stable share of hires	overall	0.454	-	0.427	-
stable separation rate	white - black	0.177	0.209	0.155	0.18
	white - Hispanic	0.177	0.179	0.137	0.134
	male - female	0.175	0.186	0.123	0.123
	age 14-34 - age 35+	0.216	0.146	0.179	0.113
stable share of hires	white - black	0.456	0.399	0.429	0.37
	white - Hispanic	0.456	0.447	0.432	0.426
	male - female	0.455	0.453	0.435	0.45
	age 14-34 - age 35+	0.432	0.493	0.373	0.406

Table C.1: Comparing prepolicy averages of treated group and synthetic control

Notes: For each of the nine main variables we test (in Tables 2, 3, and 4), we provide the prepolicy average of favored and disfavored groups for the treated group in the first and second columns, respectively. We do the same for the synthetic control in the pooled specification in the third and fourth columns. The prepolicy period is 2018 Q1 to 2021 Q2. The "treated group" is our proxy of the covered worker population: New York City's "restaurant and other eating places" industry. Note that there are no favored and disfavored groups in the first row since we are testing the overall population, so we put both entries under the respective first columns.

(1) within-industry model (no mw control)			(2) within-industry model (mw control)		
industry	*county*	*weight*	*industry*	*county*	*weight*
rest. & other eating places	Milwaukee, WI	0.067	rest. & other eating places	Milwaukee, WI	0.067
rest. & other eating places	Suffolk, MA	0.046	rest. & other eating places	Suffolk, MA	0.046
rest. & other eating places	DC	0.045	rest. & other eating places	DC	0.045
rest. & other eating places	Ozaukee, WI	0.044	rest. & other eating places	Ozaukee, WI	0.044
rest. & other eating places	Snohomish, WA	0.039	rest. & other eating places	Snohomish, WA	0.039
(3) within-location model			**(4) pooled model**		
industry	*county*	*weight*	*industry*	*county*	*weight*
personal care services	Queens, NY	0.037	personal care services	Queens, NY	0.033
personal care services	Kings, NY	0.033	personal care services	Kings, NY	0.028
element. & second. schools	Queens, NY	0.028	personal care services	New York, NY	0.021
personal care services	New York, NY	0.026	element. & second. schools	Queens, NY	0.018
school & emp. bus trans.	Kings, NY	0.025	school & emp. bus trans.	Kings, NY	0.018

(a) Test of first main prediction

(1) within-industry model (no mw control)			(2) within-industry model (mw control)		
industry	*county*	*weight*	*industry*	*county*	*weight*
rest. & other eating places	Milwaukee, WI	0.094	rest. & other eating places	Milwaukee, WI	0.094
rest. & other eating places	Suffolk, MA	0.048	rest. & other eating places	Suffolk, MA	0.048
rest. & other eating places	Bucks, PA	0.039	rest. & other eating places	Bucks, PA	0.039
rest. & other eating places	Snohomish, WA	0.036	rest. & other eating places	Snohomish, WA	0.036
rest. & other eating places	DC	0.036	rest. & other eating places	DC	0.036
(3) within-location model			**(4) pooled model**		
industry	*county*	*weight*	*industry*	*county*	*weight*
personal care services	Kings, NY	0.061	personal care services	Kings, NY	0.043
personal care services	Queens, NY	0.051	personal care services	Queens, NY	0.037
drinking places (alc. bev.)	New York, NY	0.047	drinking places (alc. bev.)	New York, NY	0.035
personal care services	New York, NY	0.03	personal care services	New York, NY	0.021
special food services industry	New York, NY	0.017	rest. & other eating places	Milwaukee, WI	0.018

(b) Test of second main prediction (for white-Hispanic comparison)

Table C.2: Heaviest-weighted units in synthetic control

Notes: The top panel reports the five heaviest-weighted units in the synthetic control (SC) for each of the four empirical models when testing our first main prediction: that just cause decreases the stable share of hires. The bottom panel does the same but for the evaluation of our second main prediction for the white-Hispanic comparison: favored groups will have a relative increase in their stable separation rate. Note that for each specification, the weight corresponds to a fraction: while only five weights are listed, all weights sum to one.

		employment		*hires*		*separations*	
		pre	post	pre	post	pre	post
men	14-18	2	2.3	3.2	3.5	2.4	3
	19-21	4.1	4.1	4.8	5.2	5.3	5.5
	22-24	4.2	4.3	4.5	5.3	5	5.3
	25-34	14.3	14.3	14.8	15.1	15.3	15.2
	35-44	11.6	11.1	10.4	9.5	10.2	9.5
	45-54	9.2	8.9	7.7	6.7	7.1	6.5
	55-64	6.1	6.1	5	4.6	4.7	4.4
	65+	3.3	3.5	3.2	3	3.2	3.1
women	14-18	2.9	3.1	4.6	4.6	3.3	4.1
	19-21	5.2	5.4	6.2	7.1	6.8	7.6
	22-24	4.6	4.9	5.1	6	5.7	6.2
	25-34	12.1	12.1	12.6	12.6	13.2	13.1
	35-44	8.3	8	7.5	7	7.4	7
	45-54	5.9	5.7	4.8	4.4	4.6	4.3
	55-64	3.7	3.6	3.1	2.8	3.1	2.8
	65+	2.6	2.7	2.6	2.6	2.6	2.6
totals	men	54.7	54.5	53.7	52.8	53.3	52.4
	women	45.3	45.5	46.3	47.2	46.7	47.6

Table C.3: Sex-age breakdown of variables for NYC rest. ind. before & after JC

Notes: This table provides a breakdown of employment, hires, and separations for the New York City restaurant industry (NAICS code 7225, "restaurants and other eating places") by percentage. There are eight age categories for each sex. For each age-sex combination, there is a prepolicy ("pre") and postpolicy ("post") percentage of the respective totals for each of the three variables (employment, hires, and separations). Each column adds up to 100 percentage points; the bottom two rows tally the total percentages of men and women. The prepolicy percentage is taken from the four quarters before the law became effective (2020 Q3 to 2021 Q2), and the postpolicy percentage is from the four quarters after the law became effective (2021 Q3 to 2022 Q2).

	employment		*hires*		*separations*	
	pre	post	pre	post	pre	post
white (non-Hispanic)	38.2	39.1	40.2	39.6	39	39.8
black (non-Hispanic)	13.5	13.7	14.1	16.3	15.9	16.6
Hispanic	25.4	25.8	25	25.9	25.1	25.5
other	22.9	21.4	20.7	18.2	20	18.1

Table C.4: Race-ethnicity breakdown of variables for New York's restaurant industry, before and after just cause

Notes: This table has an analogous format to Table C.3, but for race and ethnicity. Four categories are considered: white (non-Hispanic), black (non-Hispanic), Hispanic, and other.

		employment		hires		separations	
		pre	post	pre	post	pre	post
men	14-18	1.4	1.6	2.8	3	1.7	2.1
	19-21	3.4	3.3	4.2	4.5	5.4	4.9
	22-24	3.8	3.7	4.2	4.9	5.4	4.9
	25-34	13.9	13.8	14.6	15	15.3	15.2
	35-44	12.4	12.1	11.2	10.3	10.4	10.2
	45-54	10.4	10.3	8.6	7.5	7.2	7
	55-64	6.9	7.2	5.7	5.1	4.8	4.7
	65+	3.4	3.8	3.3	3	2.9	3.1
women	14-18	2	2.2	4	4	2.6	3.2
	19-21	4.4	4.1	5.4	6.1	7.1	6.9
	22-24	4.1	4.1	4.7	5.6	6.2	6
	25-34	11.7	11.6	12.3	12.8	13.4	13.8
	35-44	8.9	8.7	7.9	7.7	7.5	7.6
	45-54	6.6	6.5	5.3	5	4.7	4.8
	55-64	4.1	4.1	3.3	3.1	3.1	3
	65+	2.6	2.8	2.5	2.5	2.4	2.5
totals	men	55.6	55.8	54.6	53.3	53	52.2
	women	44.4	44.2	45.4	46.7	47	47.8

Table C.5: Sex/age breakdown of stable variables for New York's restaurant industry before and after just cause

Notes: This table is analogous to Table C.3, but for stable versions of employment, hires, and separations.

	employment		hires		separations	
	pre	post	pre	post	pre	post
white (non-Hispanic)	37.2	38.6	39.6	39.3	36.5	39.4
black (non-Hispanic)	12.8	12.1	12.2	14.1	16.6	14.4
Hispanic	25.7	26	25.2	25.6	26	24.6
other	24.3	23.3	23	21	20.9	21.6

Table C.6: Race-ethnicity breakdown of stable variables for New York's restaurant industry before and after just cause

Notes: This table is analogous to Table C.4, but for stable versions of employment, hires, and separations.

	total		stable	
	pre	post	pre	post
employment	207,000	292,445	121,187	175,095
hires	61,131	76,435	31,566	35,248
separations	41,509	64,713	15,140	23,926
hiring rate	29.5%	26.1%	26.0%	20.1%
separation rate	20.0%	22.1%	12.5%	13.7%

Table C.7: Variable totals in New York's restaurant industry before and after just cause became effective

Notes: For several variables of interest, this table compares their total value in the New York restaurant industry (7225) the year before (2020 Q3 to 2021 Q2) to the year after (2021 Q3 to 2022 Q2) just cause became effective.

	variable	within-ind. (no mw cont.) (1)	within-ind. (mw cont.) (2)	within-location (3)	pooled (4)
		model			
overall	employment (QCEW)	0.0236 (0.0224) [0.0324]	0.0256 (0.0245) [0.0324]	0.0083 (0.0246) [0.0097]	1e-04 (0.0235) [0.007]
	hiring rate	-0.0015 (0.0039) [0.0146]	-0.0048 (0.0034) [0.0143]	0.0024 (0.0059) [0.0106]	0.0023 (0.0048) [0.0081]
	separation rate	-7e-04 (0.0051) [0.0176]	-0.0026 (0.0046) [0.0177]	0.0071 (0.0084) [0.0136]	0.0048 (0.006) [0.0081]
stable	employment	0.0165 (0.0152) [0.0234]	0.0171 (0.0193) [0.0234]	0.0449 (0.0397) [0.0188]	0.0188 (0.0278) [0.0138]
	hiring rate	-0.0017 (0.0047) [0.0148]	-0.0048 (0.0033) [0.0146]	0.0015 (0.0077) [0.0188]	9e-04 (0.0062) [0.0134]
	separation rate	-0.0032 (0.0141) [0.0257]	-0.0047 (0.0131) [0.0257]	6e-04 (0.0186) [0.0142]	0.0034 (0.016) [0.008]
stable share	employment	0.0044 (0.0045) [0.023]	0.0075* (0.0044) [0.0229]	-0.0048 (0.0095) [0.0134]	-0.0033 (0.0068) [0.0089]
	hires	-0.0086 (0.0123) [0.0128]	-0.0069 (0.0096) [0.0128]	-0.0198* (0.0119) [0.0125]	-0.0143 (0.0116) [0.0073]
	separations	-0.0046 (0.0109) [0.0156]	-0.0044 (0.0104) [0.0157]	-0.0183* (0.012) [0.0133]	-0.0132 (0.0116) [0.0094]

Table D.1: Effect of just cause on other variables

Notes: This table details the results for all nine (whole-sample) outcome variables. * denotes that a two-sided hypothesis test is statistically significant at the 10% level, ** at the 5% level, and *** at the 1% level.

	comparison	*model*			
		within-ind. (no mw cont.) (1)	within-ind. (mw cont.) (2)	within-location (3)	pooled (4)
stable employment	white - black	0.0575** (0.0238) [0.0117]	0.0537** (0.0271) [0.012]	0.0455 (0.0325) [0.0103]	0.0408 (0.0288) [0.0067]
	white - Hispanic	0.0253 (0.0352) [0.0058]	0.0282 (0.036) [0.0058]	0.035 (0.0402) [0.0053]	0.0292 (0.0373) [0.0034]
	male - female	-0.0188 (0.013) [0.0035]	-0.0215* (0.0126) [0.0036]	-0.0241* (0.014) [0.0019]	-0.0233* (0.0135) [0.0014]
	age 14-34 - age 35+	-0.0154 (0.0189) [0.0049]	-0.0179 (0.0205) [0.0049]	-0.0072 (0.0225) [0.0066]	-0.0131 (0.0193) [0.003]

Table D.2: Relative effects on stable employment

Notes: This table has the same structure as Table 3. Units are the log difference in stable employment between groups. For example, the interpretation of the first entry in the first row is that the just cause law decreased the stable employment for white workers by 4.55 log points relative to that for black workers. * denotes that a two-sided hypothesis test is statistically significant at the 10% level, ** at the 5% level, and *** at the 1% level.

	population or comparison	model: within-ind. (no mw cont.) (1)	within-ind. (mw cont.) (2)	within-location (3)	pooled (4)
stable share of hires	overall	-0.0124 (0.0131) [0.0844]	-0.0095 (0.0125) [0.0857]	-0.0046 (0.0154) [0.0851]	-0.0102 (0.014) [0.0875]
stable separation rate	white - black	0.0017 (0.0015) [0.0097]	3e-04 (0.0016) [0.009]	0.0023 (0.0016) [0.0088]	0.002 (0.0015) [0.0097]
	white - Hispanic	0.0037* (0.0019) [0.0056]	0.0037* (0.0021) [0.0056]	0.0028 (0.002) [0.0052]	0.0033* (0.0019) [0.0056]
	male - female	-0.0018 (0.0026) [0.0057]	-0.0017 (0.0027) [0.0056]	-0.0035 (0.0027) [0.0052]	-0.0027 (0.0026) [0.0055]
	age 14-34 - age 35+	0.005 (0.0041) [0.0087]	0.0053 (0.0042) [0.0087]	0.002 (0.0045) [0.0098]	0.005 (0.0041) [0.0085]
stable share of hires	white - black	0.0091 (0.0074) [0.0338]	0.0106 (0.0069) [0.0345]	0.0145* (0.0078) [0.0329]	0.0104 (0.0075) [0.0348]
	white - Hispanic	-0.0027 (0.0029) [0.0102]	-0.002 (0.0033) [0.0104]	-4e-04 (0.0042) [0.0087]	-0.0011 (0.0033) [0.0097]
	male - female	0.0116*** (0.0039) [0.0101]	0.0114*** (0.0037) [0.01]	0.0037 (0.0043) [0.0104]	0.0075* (0.004) [0.0103]
	age 14-34 - age 35+	-0.0219*** (0.0072) [0.0288]	-0.0232*** (0.0072) [0.0292]	-0.0111 (0.0079) [0.0316]	-0.0198*** (0.0072) [0.0292]

Table E.1: Robustness test: dropping the six quarters before just cause became effective

Notes: We redo our main analyses, this time dropping the six quarters before the law became effective (2020 Q1 to 2021 Q2). We do so to gauge the role of policy and labor market changes after COVID-19 and before New York's just cause became effective in July 2021. The three panels correspond to the results in Tables 2, 3, and 4, respectively. * denotes that a two-sided hypothesis test is statistically significant at the 10% level, ** at the 5% level, and *** at the 1% level.

	population or comparison	*model* within-ind. (no controls) (1)	 within-ind. (mw cont.) (2)	 within-ind. (ui cont.) (3)	 within-ind. (both cont.) (4)
stable share of hires	overall	-0.0086 (0.0113) [0.0128]	-0.0069 (0.0096) [0.0128]	-0.0118 (0.0113) [0.00125]	0.003 (0.0115) [0.0131]
stable separation rate	white - black	0.0023 (0.0035) [0.0078]	-0.001 (0.0028) [0.0077]	0.0036 (0.0035) [0.0077]	0.0062* (0.0036) [0.0079]
	white - Hispanic	0.0044** (0.0021) [0.004]	0.0035* (0.0021) [0.004]	0.0053** (0.0021) [0.0039]	0.0049** (0.0021) [0.0039]
	male - female	-0.0029 (0.0046) [0.0034]	-0.0028 (0.0041) [0.0034]	-0.0027 (0.0046) [0.0035]	-0.0039 (0.0046) [0.0036]
	age 14-34 - age 35+	0.0048 (0.0045) [0.0069]	0.0056 (0.004) [0.007]	0.0059 (0.0045) [0.0067]	0.0032 (0.0045) [0.0068]
stable share of hires	white - black	0.0076 (0.0065) [0.006]	0.0104* (0.0061) [0.0059]	0.007 (0.0065) [0.006]	0.003 (0.0065) [0.0063]
	white - Hispanic	0.0026 (0.0028) [0.0039]	0.003 (0.003) [0.0039]	0.0024 (0.0028) [0.0038]	0.0034 (0.0028) [0.0039]
	male - female	0.0039 (0.0028) [0.006]	0.0038 (0.0027) [0.006]	0.0047* (0.0028) [0.0057]	0.0056** (0.0028) [0.0058]
	age 14-34 - age 35+	-0.0201** (0.0081) [0.0054]	-0.0197** (0.0079) [0.0054]	-0.0193** (0.0081) [0.0055]	-0.0223*** (0.0081) [0.0056]

Table E.2: Robustness test: adding unemployment insurance replacement-rate control

Notes: The first two columns repeat the within-industry results from our analyses in the body (i.e., from Tables 2, 3, and 4). The third and fourth columns repeat these specifications, adding a control for state-level unemployment insurance (UI) replacement rates. * denotes that a two-sided hypothesis test is statistically significant at the 10% level, ** at the 5% level, and *** at the 1% level.

	population or comparison	*model*		
		per. care serv. (no controls) (1)	per. care serv. (mw cont.) (2)	Milwaukee County, WI (3)
stable share of hires	overall	-0.0027	-0.0105	-0.0105
		(0.0153)	(0.0142)	(0.0114)
		[0.0055]	[0.0056]	[0.0055]
stable separation rate	white - black	0.0109	0.0088	-0.0031
		(0.0095)	(0.0085)	(0.0038)
		[0.0435]	[0.0435]	[0.008]
	white - Hispanic	-0.0059	-0.008	-0.0052**
		(0.0226)	(0.0267)	(0.0025)
		[0.0452]	[0.0453]	[0.0058]
	male - female	-0.0063	-0.0045	-0.0034
		(0.0105)	(0.01)	(0.0047)
		[0.0169]	[0.0169]	[0.003]
	age 14-34 - age 35+	0.0205	0.0109	0.0115**
		(0.0483)	(0.0468)	(0.0058)
		[0.12]	[0.1195]	[0.0163]
stable share of hires	white - black	0.0122	0.021	-0.001
		(0.0168)	(0.0192)	(0.0077)
		[0.0592]	[0.0589]	[0.0119]
	white - Hispanic	0.0267	0.0242	0.0294***
		(0.0225)	(0.0219)	(0.0055)
		[0.0161]	[0.0159]	[0.0121]
	male - female	0.0026	0.0057	0.0042
		(0.0143)	(0.0137)	(0.0044)
		[0.0224]	[0.0224]	[0.0035]
	age 14-34 - age 35+	0.0142	-0.0235	-0.0169
		(0.0505)	(0.0462)	(0.0135)
		[0.1348]	[0.1232]	[0.0239]

Table E.3: Robustness test: triple-difference models

Notes: The first two columns detail results using the first approach both without and with the minimum wage control, respectively. Here, differences between the ”restaurant and other eating places” industry and the ”personal care” service industry in New York City are compared to that difference in other counties around the country. The third column details results using the second approach when the difference was between the ”restaurant and other eating places” industry in New York City and that same industry in Milwaukee County, and compared that to differences in other industries between these two locations. * denotes that a two-sided hypothesis test is statistically significant at the 10% level, ** at the 5% level, and *** at the 1% level.