Sorting into Job Loss: Heterogeneous Crime and Employment Effects of Mass Layoffs*

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Abstract

Who gets displaced in mass-layoff episodes can be far from random, and selection can generate sizeable heterogeneity in both post-layoff employment and criminal charges. We link Chilean administrative worker-firm records to universe-level public defender data, and compare matched displaced and non-displaced workers using event-study and difference-in-differences designs with a rich set fixed effects. We document that displacement drastically reduces employment and earnings and increases criminal charges in the following periods; on average, employment falls by approximately 18-30 percentage points, while criminal charges increase by approximately 0.3-0.4 percentage points. Crucially, magnitudes vary systematically with the intensity of the mass-layoff: effects are largest when separations occur in lower-intensity layoffs-consistent with negatively selected workers being targeted first-and attenuate monotonically as events approach full layoffs. Selection of more criminally prone individuals is mostly within-firms rather than across firms. A simple model of labor and crime rationalizes these findings. Individuals with a criminal record face two conditions that increase their criminal attitude when displaced: on one hand, they receive fewer and worse job offers; on the other hand, since they already have a record, they are less deterred by an additional criminal record. Taken together, our results caution against treating mass layoffs as uniformly exogenous shocks and suggest that both research designs and policies should account for who is selected into displacement when assessing social costs and targeting support.

Keywords: *Mass Layoff*, *Crime*, *Displacement*, *Unemployment*, *Wages*.
JEL Classification: J63, J64, J65, K42.

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1 Introduction

Job displacement imposes substantial costs on workers and society. Displaced workers experience persistent earnings losses, reduced employment prospects, deteriorating health outcomes, and increased criminal activity. Understanding the magnitude and heterogeneity of these effects is central to designing effective policies that support displaced workers and mitigate social costs. Since the seminal work of Jacobson et al. (1993), mass layoffs have emerged as a key identification strategy in labor economics. This quasi-experimental approach has generated influential estimates of displacement costs and informed both academic research and policy discussions.

An important aspect of empirical estimates based on mass layoffs concerns the degree of selection among workers. Mass layoffs are particularly popular in the literature for their ability to identify internally valid estimates of job loss effects, as the timing and occurrence of these events are typically driven by factors external to individual worker characteristics. However, selection may still affect the magnitude and interpretation of displacement effects. If firms exert discretion over which workers to dismiss during workforce reductions, those selected for displacement may differ systematically from those retained. For instance, negative selection on whom to dismiss may result in more crime-prone individuals being laid off, who could be especially sensitive to job loss and thus exhibit larger displacement effects. Similarly, if different mass layoff intensities target particular types of firms—and consequently, particular types of workers—then the external validity of estimates may vary substantially across threshold definitions. These selection concerns are particularly relevant in settings where firms retain considerable discretion over dismissal decisions and where mass layoffs concentrate in specific firm types.

In this paper, we explore whether the identified effect varies by the size of the mass layoff threshold. There are several issues at play that could affect these internally valid estimates. First, firms may exercise discretion over whom to dismiss even during mass layoffs, leading to within-firm selection where more vulnerable or less productive workers are targeted first. Second, different mass layoff intensities may systematically capture different types of firms and, consequently, a particular type of worker being treated

who could react differently than the average worker, introducing between-firm selection into the estimates. By systematically varying the mass layoff threshold from low to high intensity, we assess what this variation reveals about the relative importance of these selection mechanisms. Specifically, we examine whether observed heterogeneity in displacement effects reflects primarily within-firm selection, between-firm selection, or genuine treatment effect heterogeneity across different displacement contexts.

The degree of selection and treatment effect heterogeneity may vary substantially by institutional context. Estimates from settings where firms can freely choose whom to dismiss may differ considerably from those in countries with stricter regulations, such as last-in-first-out rules that constrain firm discretion during mass layoffs. We focus on Chile, a country with flexible labor legislation, similar to the United States, that grants firms substantial discretion over dismissal decisions. We combine administrative data from multiple sources covering 2005-2015: employment records from the unemployment insurance system, arrest and criminal charge data from the Defensoría Nacional (the public defender agency capturing approximately 95% of criminal cases), and firm-level quarterly employment data to identify mass layoff events. Following recent studies in the literature, we implement a matched difference-in-differences strategy, pairing displaced workers from firms experiencing mass layoffs with similar workers at firms that did not experience mass layoffs in that quarter. We begin by focusing on the standard 30% mass layoff threshold and present event study estimates that examine the evolution of employment, earnings, and criminal charges around the displacement event. Our baseline results show substantial displacement effects: employment falls by 18-30 percentage points, earnings decline significantly, and criminal charges increase by 0.3-0.4 percentage points. These magnitudes are comparable to existing estimates in the displacement literature while extending the analysis to include criminal outcomes.

We document substantial heterogeneity in displacement effects across mass layoff thresholds. The estimated effects on employment, earnings, and criminal charges attenuate systematically as the threshold increases—from 15% to 90%—consistent with negative selection operating at lower-intensity layoffs where firms retain greater discretion. Yet displacement effects on labor outcomes remain statistically significant

even at the 90% threshold, suggesting that while selection mechanisms shape the magnitude of estimated effects, they do not fully account for the costs of job loss. The effects on crime at larger thresholds tend to lose statistical significance and are close to 0.

This pattern reveals that both selection and genuine treatment effect heterogeneity contribute to the observed variation in outcomes. The persistence of effects at extreme thresholds indicates that job displacement imposes real costs even in settings approaching firm-wide shutdowns, while the attenuation across thresholds provides systematic evidence on how within-firm and between-firm selection interact to generate heterogeneity in displacement studies.

A key aspect of our results is that higher thresholds, while reducing the likelihood of within-firm worker selection, may also identify progressively fewer treated firms, creating an important tradeoff for empirical analysis that we document in the paper. We argue that two distinct channels can explain the observed variation across threshold sizes. First, within-firm selection allows employers to dismiss workers in ways that may correlate with their responsiveness to job loss—for instance, targeting individuals more prone to criminal activity, or any other factor that correlates with that characteristic, and who may also be more sensitive to displacement. This selection force attenuates as mass layoff intensity increases and firm discretion becomes more constrained. Second, between-firm selection operates as workers sort into firms with different layoff propensities, and this sorting may also correlate with heterogeneous responses to job loss. Higher mass layoff thresholds are more likely to identify workers from firms approaching complete shutdowns, who may differ in how responsive to displacement this average worker is in terms of criminal activity relative to workers laid off from moderately contracting firms. We develop a simple model where criminal propensity signals worker productivity, rationalizing how these two selection channels generate the observed attenuation pattern. Empirically, we decompose the variation in displacement risk and find that selection operates predominantly within firms rather than between firms, though both channels contribute to the heterogeneity in estimated effects.

This paper contributes to several strands of the literature. First, we contribute to research on the

relationship between unemployment and crime, an area that has expanded substantially in recent years as richer data have become available. Early studies examined this relationship at the local level using aggregate variation, exploiting instruments such as Bartik-type shifters and national-level shocks to demonstrate that unemployment increases crime (Dell et al., 2019; Dix-Carneiro et al., 2018; Raphael and Winter-Ebmer, 2001). More recent work has linked individual-level labor market and criminal trajectories using administrative data from Denmark (Bennett and Ouazad, 2020), Brazil (Britto et al., 2022), Norway (Rege et al., 2019), Washington State (Rose, 2018a), and Colombia (Khanna et al., 2021a). Like these studies, we leverage mass layoffs as a source of identifying variation to establish causal effects of job loss on criminal activity at the individual level. Our contribution extends this literature by systematically examining how displacement effects vary with mass layoff intensity and by explicitly analyzing the selection mechanisms that operate within and between firms. This focus on heterogeneity and selection is particularly relevant in institutional contexts like Chile, where firms retain substantial discretion over dismissal decisions even during large workforce reductions.

We also contribute to the literature that has documented that firms select workers who are laid off and that such selection worsens future labor market prospects (see the seminal work by Gibbons and Katz, 1991).

2 Institutional Background

Across much of Europe, collective redundancies are constrained by legally prescribed selection rules and consultation duties that limit managerial discretion. Classic examples include seniority-based "last-in, first-out" ordering in Sweden's Employment Protection Act (LAS)—recently adjusted but still a key anchor—and Germany's Sozialauswahl, which requires employers to select redundancies using objective social criteria such as tenure, age, dependents, and disability. France similarly obliges employers to define and apply objective criteria (e.g., family situation, length of service, employability, professional skills) and to consult employee representatives when setting the order of dismissals. In many continental systems,

worker councils must also be informed and consulted, and social plans are negotiated where thresholds are met. These rules and processes make the composition of layoff lists more predictable and procedurally constrained, reducing scope for firm-chosen performance or preference-based selection.

By contrast, the U.S. regime is largely at-will: beyond advance-notice requirements under the WARN Act, employers have broad latitude to choose layoff criteria so long as they avoid unlawful discrimination—an environment reinforced by relatively low union coverage (around 10% in 2024). Chile likewise allows termination on "needs of the company," with severance, and (importantly for research design) lacks a dedicated collective-dismissal procedure–again leaving considerable room for employer selection. Brazil is different: before and after the Bolsonaro presidency, mass layoffs require prior union involvement/collective bargaining, giving unions a concrete voice over layoff lists even if formal veto power is not required.

Taken together, designs that use mass layoffs as shocks will generally be less prone to selection in more social-democratic societies with seniority/social-criteria rules or in Brazil where unions must be involved, and more prone to selection in the United States and Chile, where firms retain wider discretion over who is let go.

Chilean Labor Market: Collective bargaining in Chile is relatively limited. Estimates suggest that, as of 2015, coverage ranged between 6.9% and 11.1% of salaried workers, while in Brazil it is about 70%.

Chile's labor market stands out in the Latin American context with relatively low informal employment rates. Informal employment has declined from approximately 30% in 2010 to 25% in 2025, positioning Chile alongside Uruguay as the only countries in the region below 30% informal employment, compared to the regional average of around 50%. This formalization reflects the country's more developed institutional framework and economic structure compared to regional peers.

The Chilean employment protection system presents a unique institutional arrangement among OECD countries. For individual dismissals, the legislation requires 30 days advance written notice specify-

ing reasons, with severance equal to one month's salary per year of service, capped at 11 monthly payments based on the last salary. Most temporary contracts convert to permanent status after six months, ensuring broad coverage under the severance system. However, Chile completely lacks legislation on mass layoffs or collective dismissals, unlike virtually all other OECD countries which impose specific thresholds, notification requirements, consultation periods, and procedural safeguards for large-scale redundancies.

The social protection framework includes a mandatory unemployment insurance policy with limited coverage and modest benefits, available for both dismissal and voluntary resignation without waiting periods.

Next, we devise a model to better understand how selection may influence labor market and crime outcomes.

3 A Model of Mass Layoffs and Selection

3.1 The Setup

Here, we introduce a simple model of labor supply and crime, where individuals differ in their propensity to commit crimes, which correlates with their productivity. Individuals with a larger criminal propensity tend to be less productive, which may lead to criminal behavior, and thus to a criminal "reputation," which may further reinforce the negative effects of criminal act.

We think of criminal reputation as something broader compared to a criminal record. Workers with a criminal record have a criminal reputation, but there might be individuals who acquire a reputation even without having a proper record (i.e. individuals who are arrested but not incriminated, workers who are boycotting production but are not reported to the authorities, etc.).

Each quarter, individuals choose from three options:

- **Work legally** ($C_t = 0, E_t = 1$): earning a wage $w_t(R_t)$, which depends on their criminal

reputation.

- **Be unemployed and law-abiding** ($C_t = 0, E_t = 0$): potentially receiving unemployment benefits b.
- **Commit crime** ($C_t = 1$): Yields criminal profits π, faces punishment risk pF, and incurs intrinsic cost θ.

Individuals differ based on their **crime resistance** θ, an individual-specific trait, and their **unobserved productivity** η. η is negatively correlated with θ via:

$$\eta = \rho\theta + \varepsilon, \quad \varepsilon \sim N(0, \sigma_\varepsilon^2). \tag{1}$$

Moreover, individuals who work face a **job loss probability** $\lambda(R_t)$ and a **job finding probability** $\gamma(R_t)$ that depend on the worker's criminal reputation, where we will see that $\lambda(1) > \lambda(0)$ and $\gamma(1) < \gamma(0)$ due to employer beliefs.

3.2 The Firm's Decision

If a crime is committed, the individual obtains a reputation with probability p. Firms do not observe θ or η directly but infer productivity from R_t.[1] Firms update their beliefs using Bayes' rule:

$$E[\eta|R_t] = \mu_\eta + \rho(E[\theta|R_t] - \mu_\theta). \tag{2}$$

Since $E[\theta|R_t = 1] < E[\theta|R_t = 0]$, firms set two separate hiring probabilities:

$$\gamma(R_t) = P(\eta \geq \bar{\eta}(R_t)|R_t), \tag{3}$$

[1] As long as R_t is informative about productivity, allowing the firm to learn about the productivity of the worker over time would not alter the main results.

and two separate **wages**:[2]

$$w_t(R_t) = w_0 + \alpha E[\eta|R_t], \quad w_t(1) < w_t(0). \tag{4}$$

We microfound the layoff hazard via a within-firm threshold rule. Each quarter, firm f faces a separation requirement $s_{ft} \in [0, 1]$ ("layoff intensity").

Worker i's index is

$$J_i(R_t, Z_i) \equiv E[\eta_i \mid R_t] - c(Z_i), \tag{5}$$

where Z_i collects firing restrictions, firing costs, tenure/contract terms, or, more generally, firing frictions.

The firm lays off the s_{ft} fraction with the lowest J_i, i.e. all i such that $J_i \leq \kappa_{ft}(s_{ft})$ for a cutoff $\kappa_{ft}(\cdot)$ increasing in s.

Let u_{ift} be the firm's idiosyncratic preference parameter with CDF F_u and define

$$\lambda_i(R_t, s_{ft}, Z_i) \;\equiv\; \Pr\big(J_i(R_t, Z_i) + u_{ift} \leq \kappa_{ft}(s_{ft})\big) \;=\; F_u\big(\kappa_{ft}(s_{ft}) - J_i(R_t, Z_i)\big).$$

3.3 The Worker's Decision

Workers' value functions are, when working

$$V(1, R_t) = w(R_t) + \beta\Big[(1 - \lambda(R_t, s_{ft}, Z_i))\, V(1, R_t) \;+\; \lambda(R_t, s_{ft}, Z_i)\, V(0, R_t)\Big], \tag{6}$$

[2] Low-productivity (often $R_t = 1$ workers generate lower surplus, due to wage compression ($\alpha < 1$) and are thus first to be cut and last to be hired.

and when unemployment,

$$V(0, R_t) = b + \beta\Big[\gamma(R_t)\, V(1, R_t) + (1 - \gamma(R_t)), V(0, R_t)\Big]. \tag{7}$$

When, in addition, workers commit a crime, their value functions become

$$V^{\text{crime}}(1, R_t) = w(R_t) + \pi - pF - \theta$$
$$+\beta\, E_{R_{t+1}\,|\,R_t, C_t=1}\Big[(1 - \lambda(R_t, s_{ft}, Z_i))\, V(1, R_{t+1}) + \lambda(R_t, s_{ft}, Z_i)\, V(0, R_{t+1})\Big], \tag{8}$$

when employed, and

$$V^{\text{crime}}(0, R_t) = b + \pi - pF - \theta$$
$$+\beta\, E_{R_{t+1}\,|\,R_t, C_t=1}\Big[\gamma(R_{t+1})\, V(1, R_{t+1}) + \big(1 - \gamma(R_{t+1})\big)\, V(0, R_{t+1})\Big], \tag{9}$$

when unemployed.

The individual commits a crime if:

$$V_{crime}(E_t, R_t) \geq \max\{V(1, R_t), V(0, R_t)\}. \tag{10}$$

If employed, the condition for committing crimes is:

$$\pi - pF - \theta \geq \beta\Omega_1(R_t), \tag{11}$$

where $\Omega_1(R_t) = (1-\lambda(R_t, s_{ft}, Z_i))\,[V(1, R_t) - EV(1, R_{t+1})]+\lambda(R_t, s_{ft}, Z_i)\,[V(0, R_t) - EV(0, R_{t+1})]$

is the expected loss in utility from getting a criminal reputation.

In words, the expected benefits from committing a crimes need to be larger than the crime resistance *and* the option value of getting a criminal reputation.

A similar condition needs to be met if the individual is unemployed.[3]

In both cases, if $R_t = 1$, and thus $R_{t+1} = 1$, the rule simplifies to $\pi - pF. \geq \theta$, while if $R_t = 0$ it is easy to see that what is on the right hand side of the inequality is larger, and thus the likelihood to commit a crime is smaller. The intuition is simple, workers who do not have a criminal reputation will take into account that committing a crime can potential lead to a criminal reputation, and criminal reputations worsen future labor market aspects.

3.4 Key Economic Insights

From Eq. 5 we have that $J_i(R, Z)$ is strictly decreasing in R and increasing in Z. We also have that the cutoff $\kappa_{ft}(s)$ is strictly increasing in s.

Proposition 1 (Selection and reputation dependence)**.** Assuming that u_{ift} has strictly increasing CDF F_u, for any intensity s and worker covariates Z,

$$\lambda(1, s, Z) \ > \ \lambda(0, s, Z).$$

Hence, a criminal reputation strictly raises the layoff hazard.

Assumption 1 (Regularity for attenuation)**.** The difference $\Delta(\kappa) \equiv F_u(\kappa - J(1, Z)) - F_u(\kappa - J(0, Z))$ is single-peaked in κ (e.g., holds for logit/probit or any location family with monotone likelihood ratios).

[3]The condition is

$$\pi - pF \geq \theta + \beta\Omega_0(R_t) \tag{12}$$

and $\Omega_0(R_t) = \gamma(R_t)\left[V(1, R_t) - EV(1, R_{t+1})\right] + (1 - \gamma(R_t))\left[V(0, R_t) - E\left[V(0, R_{t+1})\right]\right.$.(12)

Corollary 1 (Attenuation with layoff intensity)**.** There is a $\bar{s}_{ft}$ such that, $\forall s_{ft} > \bar{s}_{ft}$

$$\frac{\partial}{\partial s}\Big(\lambda(1, s, Z) - \lambda(0, s, Z)\Big) \; < \; 0.$$

That is, selection on R weakens as s rises (effects converge under high-intensity layoffs).

Proof. By the chain rule, $\frac{\partial}{\partial s}\Delta = \Delta'(\kappa)\,\kappa'(s)$. Since $\kappa'(s) > 0$, $s_{ft} > \bar{s}_{ft}$ places us in the region where $\Delta'(\kappa) < 0$. □

Implications for crime. Since $\lambda(1, s, Z) > \lambda(0, s, Z)$, the continuation value of employment is lower for reputation-holders, tightening the crime condition and amplifying recidivism.

As s increases, the gap $\lambda(1, s, Z) - \lambda(0, s, Z)$ shrinks, attenuating the changes.

4 Data and Descriptive Evidence

We assembled an individual-level dataset from several administrative sources covering the period 2005–2013. The first source is a labor history file covering a 50% sample of the Chilean labor force drawn from the national unemployment insurance system, operated by the Ministry of Labor. This system includes all enrolled workers over 18 years old employed in private-sector salaried jobs, both under open-ended and fixed-term contracts. For each worker i, the dataset reports monthly wages w_{it}, a firm identifier (the RUT), contract type, gender, age, and municipality of residence.[4] In total, we observe wage histories for nearly 3.5 million individuals. Maps in the appendix illustrate the geographic distribution of these workers, showing that they are spread across the country rather than concentrated in the capital, Santiago.

The second source is the criminal-records dataset from the Public Defender's Office (Defensoría Penal Pública, DPP). The DPP provides free legal representation to more than 95% of individuals accused of a crime, whether apprehended in flagrante delicto or charged following an investigation. The

[4] For further details, see Acevedo et al. (2006).

dataset contains the month of the charge, the type of crime—which we later group into broader categories—information on pretrial detention, conviction outcomes, and other procedural details. The third source is the census of all firms in Chile, which we use to compute firm-level employment changes and identify mass layoff events. Specifically, for each firm f in quarter t, we calculate the layoff intensity s_{ft} as the proportional reduction in workforce relative to the previous quarter. These data are available from 2005 to 2013. The labor market and crime datasets were merged using anonymized individual identifiers, while firms were matched to workers via the firm identifier. Because each dataset covers a different time span, our analysis focuses on the years 2008 to 2013.

4.1 Sample Construction

To implement our event study design, we construct a local panel of nine quarters for both displaced and non-displaced individuals, covering four quarters before the displacement event ($k \in \{-4, -3, -2, -1\}$), the displacement quarter ($k = 0$), and four quarters after ($k \in \{+1, +2, +3, +4\}$). We denote the displacement quarter as q_0 in calendar time. Given our data range (2008–2013), we have sixteen possible treatment quarters q_0 between 2009:Q1 and 2012:Q4. These local panels are then stacked following Cengiz et al. (2019).

We apply several standard worker- and firm-level filters common in the labor economics literature (e.g., Cengiz et al., 2019; Bennett and Ouazad, 2020; Britto et al., 2022). At the worker level, we restrict the sample to male workers, as 83.67% of crimes in our data are committed by men.[5] To ensure labor market attachment, we keep only individuals who worked at least three months at the same firm over the previous two quarters ($q_0 - 2$ and $q_0 - 1$), including the month immediately before the potential displacement quarter q_0.[6] We include both permanent and temporary workers. We also exclude individuals earning less than 100,000 Chilean pesos (approximately USD 200 at the time) in the month prior to q_0; this cutoff is

[5] We focus on men to avoid compositional heterogeneity in crime propensities across gender, though appendix results including female workers yield qualitatively similar employment and earnings effects.

[6] We do not impose stricter attachment requirements because the Chilean labor market is highly dynamic, with frequent job changes. Appendix results using stricter tenure requirements (6+ months) yield similar results but substantially reduce sample size.

slightly above the 2008 minimum wage.[7]

At the firm level, we exclude firms employing fewer than 10 workers in any month of quarter $q_0 - 1$, and those employing fewer than 7 workers in any month of the previous three quarters ($q_0 - 3$ through $q_0 - 1$). This avoids large percentage swings in employment arising from small headcounts. We further drop firms displaying erratic employment patterns, specifically those with quarter-to-quarter increases greater than 70% or decreases greater than 30% in the year preceding q_0. Finally, we exclude firms where more than 40% of workers move to another firm in two consecutive quarters, which likely reflects administrative changes in firm identifiers or firm splits rather than genuine mass layoffs.

After applying these filters, the panel contains 10,088,263 worker-quarter observations, corresponding to 1,249,881 unique individuals i. We classify worker i as displaced (treated) if $D_i = 1$, meaning the individual no longer works for the same firm in quarter q_0 as in quarter $q_0 - 1$. In this dataset, 633,409 individuals (7.24%) are displaced; displaced workers may subsequently become unemployed ($E_{it} = 0$) or find employment at another firm. To avoid dynamic treatment complications arising from multiple displacement events (Sun and Abraham, 2021), we keep only the first displacement event for each individual, so displaced workers appear only once in the dataset.

Similarly, among potential controls, we remove individuals who are ever displaced during the sample period; never-treated workers constitute the preferred control group in difference-in-differences settings with staggered treatment timing (e.g., Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021; Baker et al., 2025). This ensures that the control group consists of workers who remain employed throughout the observation window and never experience displacement at their firm. Nonetheless, in our context—given concerns about selection into firms with different layoff propensities—yet-to-be-treated workers (those who experience displacement in later quarters) are also viable controls. In the appendix, we present results using yet-to-be-treated controls and compare them with our baseline never-treated specification.

After these restrictions, the sample contains 466,464 displaced individuals, representing 8.44%

[7] Law No. 20.279, Article 1.

of the remaining sample. From this group, we focus in our baseline analysis on the subset of workers displaced as a result of their firm downsizing by at least 30% of its workforce, defined as $s_{f,q_0} \geq 0.30$ where s_{f,q_0} represents the proportional workforce reduction at firm f in quarter q_0 relative to quarter $q_0 - 1$. This threshold identifies 51,164 displaced workers. As discussed in Section 6, we subsequently examine how results vary across different threshold definitions $\tau \in \{0, 0.15, 0.30, ..., 0.90\}$ to assess sensitivity to this methodological choice and to understand selection mechanisms operating at different layoff intensities.

4.2 Matching Procedure and Balance

As shown in Table 1, despite applying the filters described above, displaced and non-displaced workers continue to differ along observable characteristics in the unmatched sample. Displaced workers tend to be younger (35.3 years vs. 36.7 years), earn lower wages (log salary 12.80 vs. 13.12), have shorter tenure (19.7 months vs. 30.3 months), and are substantially more likely to be on temporary contracts (51% vs. 18%). Baseline crime rates are similar across groups (5.34 vs. 5.14 per 1,000 workers), though this similarity may mask heterogeneity conditional on other characteristics.

To improve balance and reduce concerns about selection on observables, we perform coarsened exact matching (CEM) on characteristics measured before the treatment quarter q_0.[8] The matching procedure uses the following pre-displacement characteristics measured in quarters $q_0 - 2$ and $q_0 - 1$: age (coarsened into seven bins: 18–25, 26–30, 31–35, 36–40, 41–45, 46–50, 51+), contract type (binary: permanent vs. temporary), wage quintiles in quarters $q_0 - 2$ and $q_0 - 1$ separately (allowing for wage changes), firm size quintile measured in $q_0 - 1$, tenure quintile measured in $q_0 - 1$, two binary indicators for committing any crime in quarters $q_0 - 2$ and $q_0 - 1$ separately, and the average number of crimes committed across quarters $q_0 - 4$ through $q_0 - 1$.

These variables follow those used in related work (e.g., Britto et al., 2022; Bennett and Ouazad,

[8] See Iacus et al. (2012) for documentation of the CEM method. CEM creates coarsened strata based on user-defined bins for each matching variable, then matches treated and control units within the same stratum, reweighting controls to match the treated distribution.

2020), with the addition of detailed crime history measures. As discussed in Section 3, including past criminal behavior is crucial for addressing selection effects and interpreting differences relative to the existing literature, particularly given our theoretical prediction that firms may selectively dismiss workers with higher criminal propensities (captured by θ_i in the model) or criminal reputations ($R_{it} = 1$). The matching procedure retains almost all displaced individuals (49,994 out of 51,164, or 97.7%).

As shown in the matched columns of Table 1, balance between treated and control units substantially improves after matching. In the matched sample, displaced and non-displaced workers have virtually identical characteristics: mean log salary (12.80 vs. 12.77), age (35.30 vs. 35.25 years), tenure (19.65 vs. 19.51 months), temporary contract rates (51% vs. 51%), and baseline crime rates (5.34 vs. 5.44 per 1,000 workers). The remaining small differences are not statistically significant and are substantially smaller than in the unmatched sample. Firm-level characteristics also show improved balance, though some small differences persist. The final matched sample contains 49,994 displaced workers and an equal number of matched controls (49,994), with each local panel spanning nine quarters for a total of 899,946 worker-quarter observations.

Table 1: Summary Statistics: Displaced vs. Non-Displaced Workers in 30% Mass Layoffs

	Displaced	Non–Displaced Unmatched	Non–Displaced Matched
Individual level			
Log Salary	12.80	13.12	12.77
Salary (Chilean Pesos, thousands)	452.63	640.43	439.83
Employment	0.99	1.00	0.99
Mean Crime (per 1000)	5.34	5.14	5.44
Age	35.30	36.73	35.25
Tenure (months)	19.65	30.31	19.51
Temporary Contract	0.51	0.18	0.51
Number Individuals	49,994	4,945,534	49,994
Firm level			
Size: Small (<95)	0.39	0.34	0.40
Size: Medium (95–596)	0.33	0.33	0.33
Size: Big (>596)	0.27	0.33	0.27
Sector: Agriculture	0.06	0.05	0.09
Sector: Industry	0.37	0.32	0.44
Santiago LMA	0.42	0.45	0.34
Year: 2009	0.34	0.21	0.34
Year: 2010	0.21	0.17	0.21
Year: 2011	0.22	0.25	0.22
Year: 2012	0.23	0.36	0.23
Number Firms	987	1,200	880

Notes: Summary statistics for key variables comparing displaced workers ($D_i = 1$) from firms experiencing at least 30% workforce reductions ($s_{f,q_0} \geq 0.30$) with non-displaced workers ($D_i = 0$) before and after coarsened exact matching (CEM). The unmatched sample includes all workers meeting the sample restrictions described in Section 4. The matched sample applies CEM on pre-displacement characteristics (age bins, contract type, wage quintiles in $q_0 - 2$ and $q_0 - 1$, firm size quintile, tenure quintile, crime indicators in $q_0 - 2$ and $q_0 - 1$, and average crime rate in $q_0 - 4$ through $q_0 - 1$). All variables are measured in quarter $q_0 - 1$ (the quarter immediately before potential displacement) unless otherwise noted. Individual-level variables include log salary (natural log of monthly wage in Chilean pesos), salary in levels (thousands of Chilean pesos), employment status (indicator for being employed), mean crime rate (criminal charges per 1,000 workers), age (years), tenure (months at current firm), and temporary contract (indicator). Firm-level variables include size categories based on employment terciles, sector classifications, location (Santiago LMA vs. other regions), and year of potential displacement. Number of individuals refers to unique workers; number of firms refers to unique firm identifiers (RUT).

5 Empirical Strategy & Results with 30% Threshold

We estimate the effect of job displacement by implementing a stacked event study design.[9] For each mass layoff identified in the dataset, we track treated and control individuals over nine quarters: four quarters before the separation event ($k \in \{-4, -3, -2, -1\}$), the quarter of treatment ($k = 0$), and four quarters after ($k \in \{+1, +2, +3, +4\}$). The baseline event study specification takes the following form:

$$Y_{it} = \sum_{k \neq -1} \beta_k \mathbb{1}[k] \cdot D_i + \theta_i + \lambda_t + \gamma_{m(i),s(i)} + \gamma_{m(i),t} + \gamma_{s(i),t} + \epsilon_{it} \tag{13}$$

where Y_{it} is the outcome for individual i in calendar quarter t. We consider three primary outcomes: C_{it}, an indicator equal to one if the individual was charged with any crime in quarter t; E_{it}, employment status measured as the average employment rate across the three months comprising quarter t; and w_{it}, the natural logarithm of average quarterly salary expressed in Chilean pesos. $D_i \in \{0, 1\}$ indicates whether individual i was displaced (treated), and $\mathbb{1}[k]$ denotes an indicator for event time k quarters relative to the displacement event in quarter q_0, with $k = -1$ serving as the reference period. The coefficients β_k capture the dynamic treatment effects of displacement at each event time.

The specification includes individual fixed effects θ_i to control for time-invariant worker heterogeneity, and calendar time fixed effects λ_t to account for aggregate macroeconomic shocks and seasonal patterns. We also include labor market area (LMA) by sector fixed effects $\gamma_{m(i),s(i)}$ to control for time-invariant differences across local industry cells, LMA by time fixed effects $\gamma_{m(i),t}$ to flexibly control for local labor market shocks, and sector by time fixed effects $\gamma_{s(i),t}$ to account for sector-specific trends.[10] This rich set of fixed effects ensures that identification comes from comparing displaced workers to non-displaced workers in the same location and sector who experience similar aggregate and local shocks but whose firms did not experience mass layoffs. Standard errors are two-way clustered at the individual and firm levels to account for both serial correlation within individuals over time and correlation among workers at the

[9] Our research design closely follows Britto et al. (2022); Bhalotra et al. (2025); Dube et al. (2025); **?**.

[10] Labor market areas are defined following the commuting zone methodology of [CITATION NEEDED].

same firm.

While the event study design in equation (13) reveals the dynamic evolution of displacement effects across event time, we also estimate a summary measure of the overall impact by collapsing the post-displacement period into a single coefficient. Specifically, we estimate:

$$Y_{it} = \alpha D_i + \beta(D_i \times Post_t) + \theta_i + \lambda_t + \gamma_{m(i),s(i)} + \gamma_{m(i),t} + \gamma_{s(i),t} + \epsilon_{it} \tag{14}$$

where $Post_t \in \{0, 1\}$ is an indicator equal to one for all quarters following the displacement event ($t > q_0$, equivalently $k \geq 0$), and zero for all quarters before the displacement event ($t \leq q_0$, equivalently $k < 0$). The coefficient β captures the average treatment effect across all five post-displacement quarters (including the displacement quarter itself), relative to the four pre-displacement quarters. This collapsed specification facilitates comparisons across different subsamples and varying mass layoff thresholds, serving as our primary measure of displacement impact throughout the analysis.

One of the most fundamental challenges in measuring the causal effect of job displacement is isolating plausibly exogenous job separations. Since the seminal work of Jacobson et al. (1993), the standard methodological approach has been to identify displaced workers as those who separate from firms experiencing mass layoffs, typically defined as employment declines of at least 30 percent. This strategy aims to isolate job losses driven by firm-level shocks rather than individual worker characteristics. By restricting the sample to workers with established labor market attachment who left distressed firms and comparing their trajectories to similar workers who remained employed, this approach creates a quasi-experimental setting that reduces a key source of selection bias. The inclusion of individual fixed effects further controls for time-invariant individual heterogeneity that might otherwise confound the estimates.

The use of 30% as the mass layoff threshold has become common practice in labor economics, employed in studies examining both crime outcomes and other displacement effects (Bennett and Ouazad,

2020; Britto et al., 2022; Rose, 2018b; Khanna et al., 2021b; Rege et al., 2019; Jacobson et al., 1993; Davis and Von Wachter, 2011; von Wachter et al., 2011; Schmutte et al., 2015; Landais et al., 2021; Sullivan and Von Wachter, 2009; Couch and Placzek, 2010; Schwerdt, 2011). We begin our analysis by presenting results using this standard threshold to establish comparability with existing literature. In the following section, we examine how estimates vary across different threshold definitions to assess the sensitivity of our findings to this methodological choice and to shed light on the selection mechanisms operating at different layoff intensities.

Figure 1 presents results from the event study specification in equation (13) for our three main outcomes: crime, employment, and log salary. The right panels display estimated coefficients $\hat{\beta}_k$ with 95% confidence intervals for each event time k. The left panels show mean outcome trajectories for displaced and non-displaced workers across event time, providing visual evidence of parallel trends in the pre-displacement period and treatment effect dynamics in the post-displacement period.

Several patterns emerge from the event study graphs. First, in all cases, displaced and non-displaced workers exhibit nearly identical parallel trajectories in the pre-displacement period, with no statistically significant differences in any outcome. This parallel trends evidence supports the identifying assumption that, absent displacement, the two groups would have continued along similar trajectories. Second, following displacement, the trajectories diverge sharply and immediately, revealing substantial negative effects on employment and earnings accompanied by a corresponding increase in criminal charges.

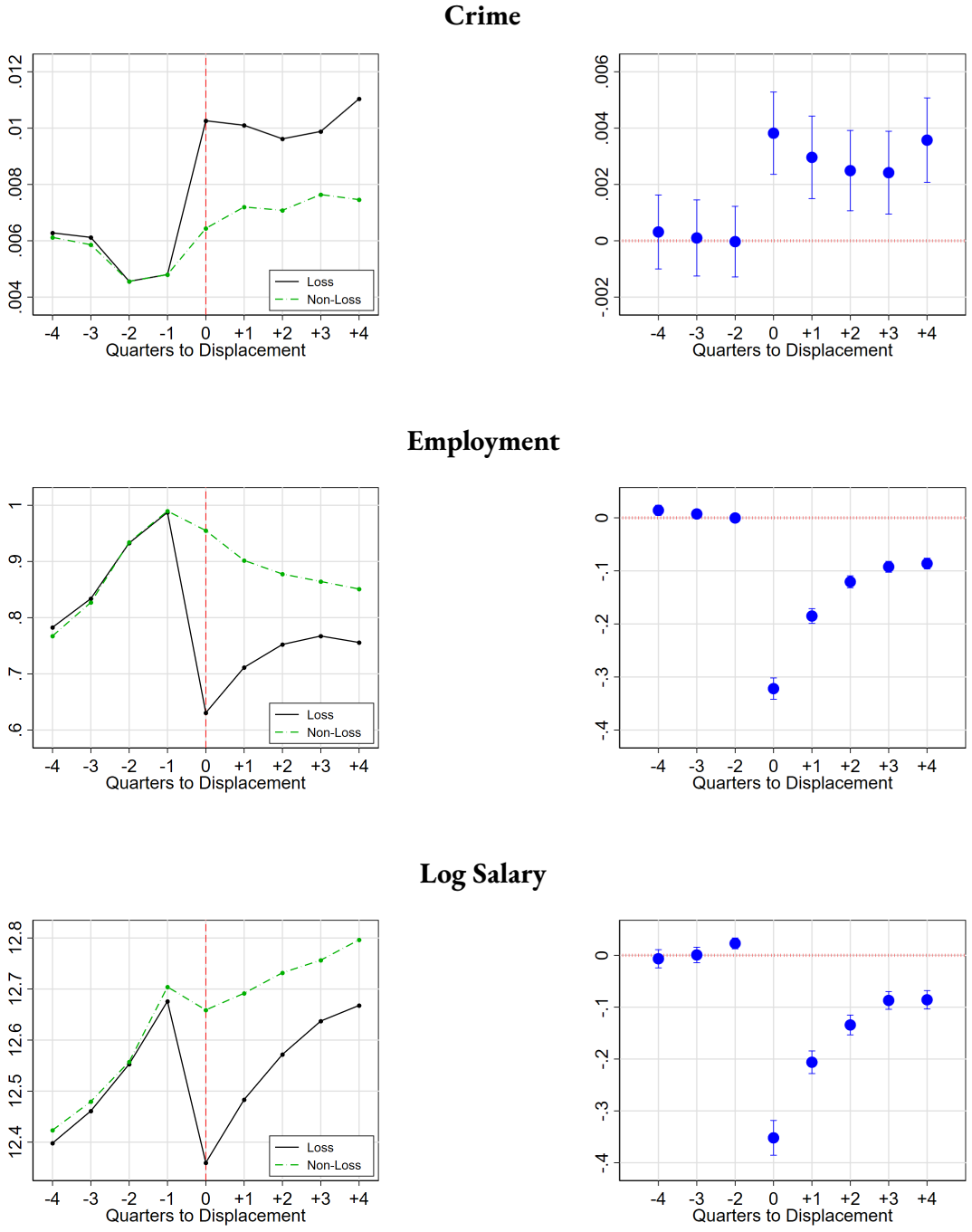
For crime outcomes shown in the top panels, the effect materializes immediately in the displacement quarter ($k = 0$), with criminal charges increasing by approximately 0.4 percentage points, representing an increase of approximately 30% compared to the baseline rate at $k = -1$. The effect persists through the first three post-displacement quarters with some attenuation, declining gradually.

For employment outcomes shown in the middle panels, the effect is immediate and large. Employment falls by approximately 32 percentage points in the displacement quarter and remains depressed throughout the follow-up period. The persistent gap between displaced and non-displaced workers

indicates that displaced workers face substantial and long-lasting barriers to reemployment. Even four quarters after displacement, employment rates have not recovered to pre-displacement levels, consistent with evidence from other displacement studies documenting persistent scarring effects. For log salary outcomes shown in the bottom panels, earnings decline immediately and substantially at displacement, falling by approximately 35% in the displacement quarter. This decline reflects both the extensive margin (workers becoming unemployed) and the intensive margin (workers who remain employed earning lower wages).[11]

[11] In the appendix, we report event study results using a 0% mass layoff threshold, which includes all workforce reductions and reveals slightly larger effects. We also present results using yet-to-be-treated workers as controls rather than never-treated workers, which yields somewhat smaller crime effects, suggesting that anticipation effects or general equilibrium spillovers may attenuate estimates when future layoff victims serve as the comparison group.

Figure 1: Impact of Displacement on Crime, Employment and Salary



Notes: Mean outcomes for displaced and non-displaced workers (left panels) and estimated coefficients $\hat{\beta}_k$ from equation (13) with 95% confidence intervals (right panels). Top row shows crime outcomes (indicator for criminal charges); middle row shows employment outcomes (average employment rate across quarter); bottom row shows log salary outcomes (natural log of average quarterly salary in Chilean pesos). Sample restricted to workers displaced from firms experiencing at least 30% workforce reductions (mass layoff threshold $\tau = 0.30$). Event time $k = -1$ is normalized to zero as the reference period. Standard errors are two-way clustered at the individual and firm levels. Matching method: Coarsened exact matching on pre-displacement characteristics as described in Section 4.

Table 2 reports estimates from the collapsed difference-in-differences specification in equation (14), providing summary measures of displacement effects averaged across the post-displacement period. Panel A presents results for our main outcomes, including both log salary and salary in levels (thousands of Chilean pesos). Panel B disaggregates crime effects by offense type. As in Figure 1, the sample includes only workers displaced from firms that reduced their workforce by at least 30% during the displacement quarter. The coefficient $\hat{\beta}$ captures the average difference between displaced and non-displaced workers across post-displacement quarters relative to pre-displacement quarters.

The results in Panel A confirm the patterns evident in the event study graphs and show substantial displacement effects across all outcomes. Overall crime increases by 2.968 percentage points (significant at the 1% level), representing an increase of approximately 55.6% relative to the baseline mean of 5.341 per 1,000 workers among non-displaced individuals in the pre-displacement period. This effect is notably larger than those reported in comparable studies: Rege et al. (2019) find a 20% increase for Norway, Britto et al. (2022) report 23% for Brazil, Bennett and Ouazad (2020) document 32% for Denmark, Khanna et al. (2021b) find 47% for Colombia, and Rose (2018b) estimates between 30% and 50% in the displacement quarter for the United States. The relatively large effect in our Chilean context may reflect the looser Chilean layoff policies described earlier. The labor market consequences are equally substantial. Employment rates fall by 17 percentage points for those who remain employed, earnings decline significantly: log salary decreases by 0.182, corresponding to approximately 16.7% in levels, while salary in levels falls by 8.664 thousand pesos per month. Given the baseline monthly salary of 35.695 thousand pesos among non-displaced workers, this represents a 24.3% reduction in absolute earnings.

Panel B disaggregates crime effects by offense type to examine whether displacement affects different categories of criminal behavior differently. We classify crimes into four categories: property crimes (economically motivated offenses such as theft and burglary), violent crimes (offenses involving violence including aggravated assault, but also robbery), drug-related crimes (possession, trafficking, and drug-related offenses), and traffic crimes (driving violations and traffic-related offenses). The results show that both

property and violent crimes increase substantially and by similar magnitudes, with increases exceeding 60% relative to their respective baseline rates. Property crime increases by 0.750 percentage points from a baseline of 1.200 per 1,000 workers (62.5% increase), while violent crime increases by 1.496 percentage points from a baseline of 2.320 per 1,000 workers (64.5% increase). The similarly large effects on both property and violent crime are consistent with both categories being substantially driven by economic motives. By contrast, drug-related crimes show a small and statistically insignificant increase of 0.055 percentage points from a baseline of 0.130 per 1,000 workers, though the imprecision of this estimate (large standard errors relative to the coefficient) prevents strong conclusions. The muted response of drug crimes, which are less directly tied to immediate economic necessity, provides additional evidence that the criminogenic effects of displacement operate primarily through economic channels. Traffic crimes increase by 0.285 percentage points from a baseline of 0.995 per 1,000 workers (28.6% increase), significant at the 10% level.

Table 2: Impact of Displacement on Crime, Employment, and Salary

	Crime	Employment	Log Salary	Salary
Displaced × After	2.968***	-17.043***	-0.182***	-8.664***
	(0.377)	(0.503)	(0.008)	(0.368)
Mean Non-Displaced Before	5.341	87.981	12.549	35.695
Obs	896843	896843	806883	896843
Adj. R-sq	0.036	0.359	0.641	0.814

	Property	Violent	Drug-Related	Traffic
Displaced × After	0.750***	1.496***	0.055	0.285*
	(0.176)	(0.243)	(0.065)	(0.149)
Mean Non-Displaced Before	1.200	2.320	0.130	0.995
Obs	899892	899892	899892	899892
Adj. R-sq	0.000	0.000	0.000	0.000

Notes: Estimated coefficients $\hat{\beta}$ from equation (14) showing the average effect of displacement in post-displacement quarters ($Post_t = 1$, corresponding to $k \in \{0, +1, +2, +3, +4\}$) relative to pre-displacement quarters ($Post_t = 0$, corresponding to $k \in \{-4, -3, -2, -1\}$). Panel A presents main outcomes: Crime (indicator for any criminal charges, in percentage points), Employment (average employment rate across quarter, in percentage points), Log Salary (natural log of average quarterly salary), and Salary (average quarterly salary in thousands of Chilean pesos). Panel B disaggregates crime by offense type: Property, Violent, Drug-Related, and Traffic. Sample restricted to workers displaced from firms experiencing at least 30% workforce reductions (mass layoff threshold $\tau = 0.30$). Mean Non-Displaced Before reports the average outcome among non-displaced workers in pre-displacement quarters. All specifications include individual fixed effects, calendar time fixed effects, and LMA-by-sector, LMA-by-time, and sector-by-time fixed effects as described in equation (14). Standard errors are two-way clustered at the individual and firm levels. *** p<0.01, ** p<0.05, * p<0.10.

To explore heterogeneity in displacement effects across observable worker characteristics, Table 3 presents estimates from augmented specifications that include triple interaction terms. Specifically, we extend equation (14) to include interactions between the displacement effect and predetermined worker characteristics measured before the displacement event:

$$Y_{it} = \alpha D_i + \beta(D_i \times Post_t) + \sum_j \delta_j (D_i \times Post_t \times X_{ij}) + \theta_i + \lambda_t + \gamma_{m(i),s(i)} + \gamma_{m(i),t} + \gamma_{s(i),t} + \epsilon_{it} \quad (15)$$

where X_{ij} represents characteristic j of worker i measured in the quarter immediately preceding

displacement, and δ_j captures how characteristic j moderates the effect of displacement. We examine five characteristics: age under 30 (young workers), tenure less than 10 months (inexperienced workers), temporary contract status, earnings below the median (low salary), and any criminal charges in the pre-displacement period (prior crime). Columns (1)-(5) include each interaction separately, while column (6) includes all interactions simultaneously to assess robustness to mutual controls.

The estimates reveal substantial heterogeneity in displacement effects across worker characteristics. Young workers (under 30) experience an additional 2.3 percentage point increase in criminal charges (column 1), representing a 106% amplification relative to the baseline effect of 2.2 percentage points for older workers. This effect remains robust (2.0 percentage points) in the fully specified model (column 6). Workers with limited experience (tenure under 10 months) show similarly large amplification, with an additional 2.6 percentage point increase (column 2) that persists in the joint specification (2.3 percentage points, column 6).

Low-salary workers exhibit the largest amplification, with an additional 3.1 percentage point increase (column 4), representing a 143% larger effect than high earners. This effect attenuates to 2.1 percentage points in the fully specified model but remains significant at the 5% level. Temporary contract status shows a significant effect in isolation (1.3 percentage points, column 3) but becomes statistically and economically insignificant in the joint specification (column 6), suggesting it primarily proxies for age and experience rather than exerting an independent effect.

Workers with prior criminal charges show large but imprecise coefficients (10.7 and 6.9 percentage points in columns 5 and 6), suggesting substantial amplification but with standard errors too large to distinguish from zero. The low base rate of pre-displacement crime (0.5% of sample) limits statistical power, though appendix results using larger samples confirm that criminal history significantly moderates displacement effects, consistent with our theoretical predictions.[12]

[12]The model in Section 3 predicts that workers with criminal reputations face lower job-finding probabilities $\gamma(R_t = 1) < \gamma(R_t = 0)$ and higher layoff hazards $\lambda(R_t = 1) > \lambda(R_t = 0)$, making them more sensitive to displacement shocks.

Table 3: Triple Interaction Analysis

	(1)	(2)	(3)	(4)	(5)	(6)
	Age<30	Exper <10 Months	Temp Contr	Low Salary	Crime Before	All
Dis × Post	2.176***	2.134***	2.381***	2.518***	2.813***	1.040**
	(0.414)	(0.397)	(0.478)	(0.390)	(0.307)	(0.435)
Dis × Post × Age<30	2.296***					2.018***
	(0.821)					(0.664)
Dis × Post × Exp<10 Mt		2.616***				2.318***
		(0.866)				(0.746)
Dis × Post × T Contr			1.264*			-0.017
			(0.730)			(0.628)
Dis × Post × Low Sal				3.130**		2.061**
				(1.261)		(1.031)
Dis × Post × Crim					10.676	6.851
					(19.543)	(19.490)
Mean Ctrl Before	5.341	5.341	5.341	5.341	5.341	5.341
Observations	896843	896843	896843	896843	896843	896843
Adj. R-sq	0.036	0.036	0.036	0.036	0.079	0.079

Notes: Estimated coefficients from equation (15) showing how pre-displacement worker characteristics moderate the effect of displacement on criminal charges. The dependent variable is an indicator for criminal charges (percentage points). Displaced × Post represents the baseline effect $\hat{\beta}$ for the reference group (workers without the specified characteristic). Triple interaction terms $\hat{\delta}_j$ capture additional effects for workers with characteristic X_j. All characteristics are measured in the quarter immediately preceding displacement ($q_0 - 1$): Age<30 (workers under 30 years old), Exp<10 Months (tenure less than 10 months), Temp Contract (temporary contract status), Low Salary (earnings below sample median), Crime Before (any criminal charges in pre-displacement period). Columns (1)-(5) include each interaction separately; column (6) includes all interactions simultaneously. All specifications include individual fixed effects, calendar time fixed effects, LMA-by-sector, LMA-by-time, and sector-by-time fixed effects, and main effects for all interaction variables (absorbed by individual fixed effects). Standard errors are two-way clustered at the individual and firm levels. *** p<0.01, ** p<0.05, * p<0.10.

6 Not All Mass-Layoffs Are (Created) Equal

6.1 Heterogeneity by Mass-Layoff Threshold

While the use of mass layoffs as a quasi-natural experiment has become standard in the displacement literature, the choice of layoff threshold is not merely a technical detail—it involves fundamental tradeoffs that may introduce bias into the estimates. First, larger thresholds yield smaller samples: the number of identified displaced workers in our dataset falls from 400,000 under a 15% threshold to 150,000 at 30%, and

to fewer than 25,000 when the threshold exceeds 65% (see Figures in the Appendix). Second, the threshold affects both the composition of displaced workers and the types of firms exposed to those shocks. In layoffs that fall short of complete plant closures—which are more common at lower thresholds—firms may selectively retain their most productive workers, meaning that displaced workers could be negatively selected based on unobservable characteristics. This selective retention may create a compositional difference between displaced and non-displaced workers that undermines the quasi-experimental design. Conversely, while higher thresholds (approaching complete closures) better approximate truly exogenous displacement events by limiting firms' ability to selectively retain workers, they substantially reduce statistical power and may restrict the external validity of estimates to only the most severe displacement episodes.[13]

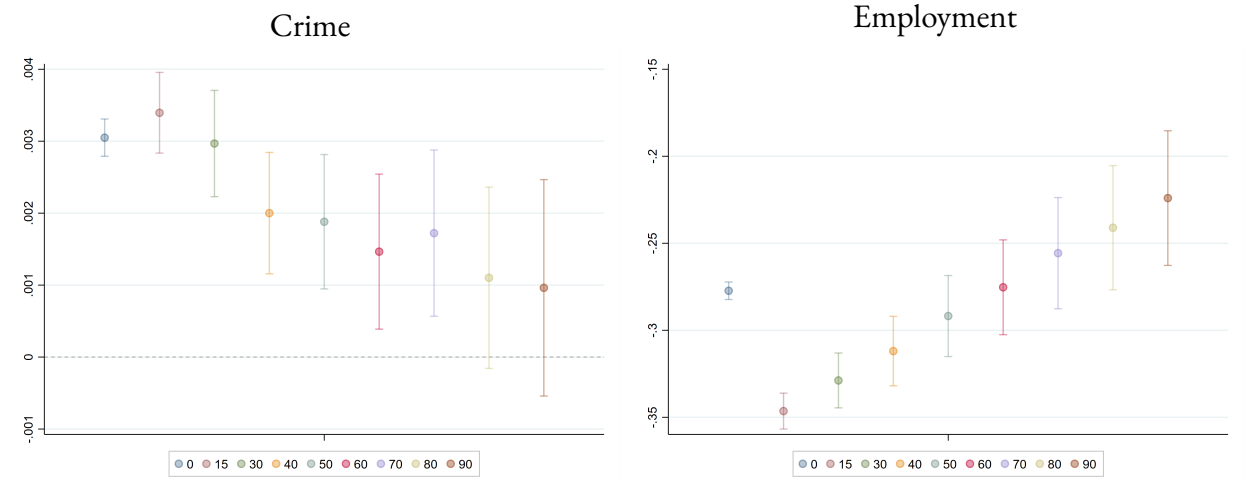
These tradeoffs suggest that the estimated impacts of displacement may vary systematically with the severity of the layoff event, and that mass layoffs defined at different thresholds may capture fundamentally different displacement experiences. This section explores this heterogeneity empirically. If firms selectively dismiss workers based on unobservable characteristics—such as criminal propensity, productivity, or other unobservable factors correlated with them—then displaced and non-displaced workers should differ systematically, and this compositional difference should vary with the layoff threshold τ. The selection mechanism implies a testable prediction: suppose firms prioritize dismissing a fraction θ of workers with high criminal propensity. For thresholds $\tau > \theta$, the marginal worker displaced at higher thresholds should increasingly resemble retained workers. If selection drives the heterogeneity documented above, we expect the estimated effects on employment, earnings, and crime to converge toward zero as τ increases and the treatment and control groups become more compositionally similar. Thus, we compare β_τ across different mass layoff thresholds. More specifically, we test:

[13] In addition, large-scale layoffs, particularly those representing a substantial share of local employment, can affect local labor market conditions by depressing wages and employment opportunities for all workers in the area—not just those directly displaced. This could violate the stable unit treatment value assumption (SUTVA), as the control group of non-displaced workers may themselves be affected by the treatment through general equilibrium effects. We do not consider this identification threat in our setting.

$$Y_{it} = \alpha D_i + \beta_\tau (D_i \times Post_t) + \theta_i + \lambda_t + \gamma_{m(i),s(i)} + \gamma_{m(i),t} + \gamma_{s(i),t} + \epsilon_{it} \quad \text{for all } s_{f,q_0} \geq \tau \quad (16)$$

where s_{f,q_0} denotes the layoff intensity (share of workforce reduced) at firm f in the displacement quarter q_0, and the mass layoff threshold τ ranges from 0% (any layoff, including voluntary quits) to 90%.[14]

Figure 3: Impact of Displacement on Crime and Employment by Mass Layoff Thresholds



Notes: Coefficients $\hat{\beta}_\tau$ from equation (16) with *Crime* (left panel) and *Employment* (right panel) as dependent variables across different mass layoff thresholds $\tau \in \{0, 0.15, 0.30, 0.40, 0.50, 0.60, 0.70, 0.80, 0.90\}$. Each point represents the estimated effect of displacement on the outcome variable for workers displaced from firms experiencing layoffs at or above the specified threshold. Vertical bars represent 95% confidence intervals with standard errors two-way clustered at the individual and firm levels.

Figure 3 reports the $\hat{\beta}_\tau$ coefficients for each threshold, examining both crime and employment outcomes.[15] The results provide strong support for the selection hypothesis - highlighted in the theoretical model -, as the estimated effects attenuate substantially as the threshold increases.

For the crime outcome shown in the left panel, the effect is relatively stable for lower thresholds:

[14] We did not include firm closures to avoid problems with firms appearing as shutting down whereas in reality they changed ID, and we are not able to track such changes. Nevertheless, we remove all firms that see 40% or more of the workforce going to work at another firm.

[15] The results for salary are consistent with those of employment and are in the appendix.

approximately 0.30 percentage points (pp) for 0%, 15%, and 30% thresholds, indicating about a 0.3 percentage point increase in criminal activity following displacement. This represents a 56% increase relative to the baseline crime rate of 0.534% among non-displaced workers. The attenuation begins at the 40% threshold, where the effect drops by approximately one-third to 0.20 pp, corresponding to a 37% increase over baseline. By the 60% threshold, the effect has declined to 0.15 pp—half the magnitude observed at lower thresholds. The effect stabilizes around this level through the 70% and 80% thresholds. At the highest thresholds (80% and 90%), coefficients of approximately 0.11 pp and 0.10 pp remain economically meaningful, representing 21% and 19% increases over baseline, respectively, but are not statistically distinguishable from zero, with confidence intervals spanning approximately $[-0.05, 0.25]$.

For the employment outcome shown in the right panel, we observe a symmetric attenuation pattern with opposite signs. The effect is most negative for low mass layoff thresholds, with $\hat{\beta}_\tau$ approximately -0.28 at the 0% threshold and -0.35 at the 15% threshold, indicating substantial employment losses of 28–35 percentage points. These magnitudes represent 32–40% declines relative to the baseline employment rate of 88% among non-displaced workers. The effect monotonically increases (becomes less negative) as the threshold rises: approximately -0.33 at 30%, -0.32 at 40%, -0.29 at 50%, -0.28 at 60%, -0.26 at 70%, -0.24 at 80%, and -0.18 at 90%. Notably, even at the 90% threshold—where nearly all workers at affected firms are displaced—the employment penalty remains 18 percentage points and highly significant, suggesting genuine treatment effect heterogeneity beyond pure selection effects.

Two patterns emerge as particularly informative for understanding these results. First, crime effects stabilize around the 60% threshold while employment effects continue to attenuate monotonically through the 90% threshold. This divergence suggests that the relationship between crime and employment outcomes weakens at higher layoff intensities. One potential explanation is compositional: at extreme thresholds, displaced workers may include higher-productivity individuals with better outside employment options who experience smaller employment losses but still face similar crime-inducing income shocks. Second, the persistence of statistically and economically significant effects even at the 90% threshold—where

firms have minimal discretion over whom to dismiss—indicates that while selection mechanisms shape effect magnitudes substantially, they do not fully account for the observed displacement effects. Rough calculations suggest that selection mechanisms explain approximately 60–70% of the heterogeneity in crime effects and 45–50% of the heterogeneity in employment effects across thresholds. The next section decomposes these selection mechanisms to understand their relative contributions.

6.2 Sources of Heterogeneity across ML Thresholds: Within and Between Firms' Selection

The attenuation patterns documented in Figure 3 can arise from two distinct selection mechanisms. The first mechanism, which we term within-firm selection, occurs when firms exercise discretion over which workers to dismiss during a mass layoff. Even in large-scale reductions, firms may selectively retain more productive workers, those with greater firm-specific human capital, or those with higher firing costs due to tenure or contract protections. This results in displaced workers being negatively selected on unobservable characteristics such as criminal propensity or productivity within the same firm. Crucially, within-firm selection should attenuate as layoff intensity approaches 100%: when firms must dismiss nearly all workers, discretion becomes severely constrained, and the marginal worker displaced increasingly resembles retained workers.

The second mechanism, which we term between-firm selection, arises when workers sort into firms with different propensities for mass layoffs. If workers with particular characteristics—such as lower productivity, higher criminal propensity, or weaker labor market attachment—systematically work at firms more likely to experience severe layoffs (for example, firms in declining industries, smaller firms, or firms with volatile demand), then different threshold definitions will capture systematically different worker populations even in the absence of within-firm selection. Unlike within-firm selection, between-firm selection may persist or even strengthen at higher thresholds if the most vulnerable firms, defined as those experiencing near-complete workforce reductions, systematically employ different types of workers than

moderately contracting firms.

These mechanisms have distinct implications for interpretation and policy. Within-firm selection suggests that conventional mass layoff studies may overstate average treatment effects by capturing disproportionately vulnerable workers within firms. Between-firm selection implies that external validity concerns extend beyond threshold choice to fundamental differences in which firms and workers are studied. Empirically distinguishing these channels requires examining first, whether observable worker characteristics predict displacement within firms experiencing similar layoff intensities, which would indicate within-firm selection, and second, whether workers employed at firms with different layoff intensities differ systematically even before layoffs occur, which would indicate between-firm selection. We address each mechanism in turn.

6.2.1 Within-Firm Selection Analysis

To assess within-firm selection, we estimate how observable worker characteristics predict displacement among workers at firms experiencing mass layoffs of similar intensity. Specifically, for each threshold τ, we restrict the sample to workers at firms where $s_{f,q_0} \geq \tau$ and estimate:

$$D_i = \beta_0 + X_i'\theta_\tau + \lambda_t + \gamma_{m(i),s(i)} + \gamma_{m(i),t} + \gamma_{s(i),t} + \varepsilon_i \quad \text{for all } s_{f(i),q_0} \geq \tau \tag{17}$$

where $D_i \in \{0, 1\}$ indicates whether worker i was displaced, X_i contains pre-displacement worker characteristics measured in quarter $q_0 - 1$, and θ_τ captures the strength of selection on observables at threshold τ. The analysis is stratified by τ (15%, 30%, 40%, etc.), allowing us to examine how selection evolves with layoff intensity.

The sample consists of all workers who, in each potential treatment quarter, meet initial criteria including salary thresholds, firm size requirements, and labor market attachment conditions. This is

essentially the pre-matching sample used for the event study analysis described in Section 5. All variables in X_i refer to the pre-treatment period and have been described in Section 4, except for the individual fixed effect from the AKM decomposition. This variable, labeled Ind. FE (AKM), represents individual fixed effects obtained from the wage decomposition model of Abowd et al. (1999), which decomposes wages into worker and firm components.[16] To facilitate comparison of coefficients across variables measured on different scales, we standardize all variables in X_i through z-score conversion, such that each has mean zero and standard deviation one.

The results in Table 4 show that the predictive power of observable worker characteristics for displacement declines systematically as mass layoff thresholds increase. For age, at the 15% threshold an increase by one standard deviation (approximately 9.8 years) leads to a decrease in the probability of being displaced by 0.293, and the coefficient is highly significant. By the 90% threshold level, the effect has declined to -0.064, approximately 20% of the initial effect, and is no longer statistically significant. Tenure and temporary contract status also display substantial decreases in coefficient magnitudes. At lower levels of layoff intensity, less experienced workers and those on temporary contracts are systematically more likely to be laid off, whereas at higher threshold levels this relationship becomes much smaller and in some cases reverses. For tenure, the coefficient becomes positive at the 90% threshold, suggesting that in near-complete shutdowns, firms may retain workers with the shortest tenure, perhaps due to lower severance obligations or probationary status.

The coefficient on average crime levels in the four quarters preceding displacement also declines substantially, becoming statistically insignificant at the 60% threshold, with magnitudes at higher thresholds approximately one-third of those at lower thresholds. Similar patterns emerge for the individual fixed

[16] The wage decomposition estimates:

$$\log w_{it} = X_{it}\beta + \alpha_i + \psi_{f(i,t)} + \varepsilon_{it}, \tag{18}$$

where α_i represents the worker fixed effect capturing time-invariant worker heterogeneity including unobserved skills, $\psi_{f(i,t)}$ represents the firm fixed effect capturing firm-specific wage premiums, and ε_{it} is the idiosyncratic error. The individual fixed effects thus capture all unobserved skills, including those related to criminal propensity, that affect wages and productivity. Additional details about this procedure are provided in the appendix.

effect calculated using the AKM methodology, which captures time-invariant productivity differences across workers. At the 15% threshold, one standard deviation increase in this measure reduces displacement probability by 0.203, but by the 60% threshold this effect has declined to -0.039 and is no longer statistically significant.

That key control variables progressively cease to be relevant in explaining displacement as threshold levels increase is further confirmed by the F-statistic, which monotonically decreases as the share of laid-off workers increases. The F-statistic declines from 687.65 at the 15% threshold to 15.78 at the 60% threshold, and further to 2.82 at the 90% threshold. This decline indicates that observable worker characteristics explain progressively less variation in who gets displaced as layoff intensity increases, consistent with firms having less discretion over dismissal decisions when larger shares of the workforce must be eliminated.[17]

Table 4: Within-Firm Selection: Worker Characteristics and Displacement Probability

	Thresholds							
	(1) 15%	(2) 30%	(3) 40%	(4) 50%	(5) 60%	(6) 70%	(7) 80%	(8) 90%
Age	-0.293***	-0.349***	-0.318***	-0.212***	-0.102**	-0.092**	-0.090**	-0.064
	(0.014)	(0.024)	(0.030)	(0.037)	(0.042)	(0.041)	(0.042)	(0.047)
Tenure (months)	-0.171***	-0.158***	-0.128***	-0.094***	-0.057***	-0.016	0.017	0.019*
	(0.006)	(0.011)	(0.015)	(0.017)	(0.018)	(0.014)	(0.012)	(0.010)
Temporary contract	0.103***	0.032***	0.004	0.003	0.016	-0.054***	-0.021	-0.009
	(0.007)	(0.011)	(0.015)	(0.021)	(0.018)	(0.019)	(0.018)	(0.020)
Crime Mean (-4 to -1)	0.021***	0.017***	0.017***	0.017***	0.012**	0.009	0.006	0.005
	(0.002)	(0.003)	(0.005)	(0.006)	(0.006)	(0.006)	(0.005)	(0.004)
Firm Ind. FE (AKM)	-0.203***	-0.242***	-0.217***	-0.122***	-0.039	-0.044	-0.072*	-0.050
	(0.014)	(0.024)	(0.030)	(0.037)	(0.042)	(0.042)	(0.043)	(0.048)
Firm+Trim FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	450,058	175,271	99,155	66,946	47,545	31,271	25,358	21,258
Adj. R-sq	0.13	0.21	0.27	0.32	0.39	0.30	0.19	0.12
Mean Dep. Var.	0.52	0.92	1.34	1.68	2.03	2.74	3.02	3.14
F-stat	687.65	222.58	99.55	41.46	15.78	10.21	3.19	2.82
p-value (F-stat)	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.02

Notes: The dependent variable is an indicator equal to one if the worker was displaced. All regressions include firm and quarter fixed effects. All independent variables are standardized (z-scores) to allow comparison of coefficient magnitudes across variables measured on different scales. The column headers indicate different mass layoff thresholds τ, restricting the sample to workers at firms where $s_{f,q_0} \geq \tau$. Standard errors are two-way clustered at the individual and firm levels.

[17] In the appendix we provide evidence that the decrease in the F-statistic is robust to randomly trimming the dataset to 21,520 observations to maintain comparable sample sizes across different thresholds. Additionally, we show results with crime as the dependent variable, and the patterns are similar to those with displacement as the dependent variable.

These results provide strong evidence for within-firm selection, particularly at lower and moderate layoff thresholds. At the 15% threshold, where firms retain 85% of their workforce, they systematically dismiss younger workers, those with less tenure, those with higher criminal propensities, and those with lower productivity as measured by AKM fixed effects. As layoff intensity increases, these selection patterns attenuate substantially, with most effects declining by 50–80% by the 60% threshold. The near-complete absence of predictive power at the 90% threshold suggests that at extreme layoff intensities, which workers are displaced becomes nearly random with respect to observable and (by extension) unobservable characteristics, consistent with firms having minimal discretion when nearly all workers must be dismissed.

6.2.2 Between-Firm Selection Analysis

A complementary explanation for the declining effect of job loss on crime shown in Figure 3 is that firms conducting layoffs may differ systematically across layoff intensities. To explore this possibility, we focus on period q_0, the quarter when treatment may occur, using the pre-matching sample. For each layoff threshold τ, we retain only workers employed in firms whose layoffs meet that specific cutoff, meaning firms where $s_{f,q_0} \geq \tau$. We then group observations by macro industry sector, Local Labor Market Area (LMA), and calendar quarter.

Next, we construct a comparison group consisting of workers in the same industry–location–period cells but employed in firms that did not lay off that specific share of workers. For example, when using a 30% threshold, the treatment group includes workers at firms that laid off at least 30% of their workforce, while the comparison group includes workers at firms in the same sector, location, and time period that laid off less than 30% of their workforce. For each group, we compute average worker characteristics such as age, tenure, contract type, and prior criminal history. To ensure comparability across groups, we weight the average characteristics of workers in non-layoff firms by the number of workers in layoff firms within each cell. This balancing procedure equalizes the number of observations between workers in firms that meet the layoff threshold and those in firms that do not.

We then estimate the following regression, where the dependent variable is a dummy equal to one if the worker was employed in a firm that laid off at least the specified share of workers:

$$W_i = \beta_0 + X_i'\theta_\tau + \varepsilon_i \quad \text{if } s_{f(i),q_0} \geq \tau \tag{19}$$

where $W_i \in \{0, 1\}$ indicates whether worker i was employed at a firm experiencing a mass layoff at threshold τ. This specification allows us to assess how worker characteristics differ between firms at each layoff intensity and other firms in similar sectors, locations, and time periods. Because the dataset has already been aggregated by industry, location, and period, and because we weight comparison group averages to match treatment group sizes within cells, additional fixed effects are unnecessary. As in the within-firm analysis, all variables are standardized to facilitate comparison across coefficients.

Results reported in Table 5 are broadly consistent with the within-firm analysis, though some differences emerge. The explanatory power of covariates declines as the layoff threshold increases, although this pattern reverses slightly at the highest mass-layoff levels. At the 15% threshold, age shows a positive coefficient of 0.229, indicating that workers at firms experiencing moderate layoffs tend to be older than workers at non-layoff firms in similar sectors and locations. However, this relationship weakens and becomes negative at higher thresholds, reaching -0.206 at the 90% threshold. Tenure and temporary contract status show variable patterns across thresholds, with coefficients losing statistical significance at mid-range thresholds.

Most notably, criminal history—our proxy for criminal propensity—exhibits declining coefficient magnitudes but remains statistically significant across all thresholds. At the 15% threshold, workers at layoff firms have 0.030 standard deviations higher crime rates in the pre-displacement period than workers at non-layoff firms. This coefficient declines moderately to 0.021 at the 60% threshold and 0.015 at the 90% threshold but remains statistically significant throughout. This persistence suggests that between-firm sorting on criminal propensity operates independently of layoff intensity.

The AKM individual fixed effect shows a similar pattern to age, with a coefficient of 0.322 at the 15% threshold that declines and becomes statistically insignificant at middle thresholds before turning negative at the 90% threshold. The F-statistic pattern shows a more rapid decline than in the within-firm analysis, falling from 172 at the 15% threshold to 26 at the 60% threshold, and further to 3 at the 90% threshold. Overall, these findings indicate systematic differences in worker characteristics across firms with different layoff intensities, though the magnitude of between-firm selection appears smaller than within-firm selection as evidenced by the consistently lower F-statistics in Table 5 compared to Table 4.[18]

Table 5: Between-Firm Selection: Worker Characteristics and Firm Layoff Status

	Thresholds							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	15%	30%	40%	50%	60%	70%	80%	90%
Age	0.229***	0.142*	0.032	-0.003	0.024	0.125	-0.167	-0.206**
	(0.072)	(0.078)	(0.076)	(0.100)	(0.092)	(0.094)	(0.104)	(0.103)
Tenure (months)	0.018	-0.056	-0.068	0.044	-0.092	-0.023	0.138	0.084
	(0.047)	(0.044)	(0.047)	(0.071)	(0.079)	(0.056)	(0.084)	(0.070)
Temporary contract	0.050	-0.008	-0.049	0.057	0.003	0.086	0.044	0.040
	(0.047)	(0.049)	(0.045)	(0.070)	(0.055)	(0.053)	(0.079)	(0.061)
Crime Mean (-4 to -1)	0.030***	0.030***	0.031***	0.027***	0.021***	0.021***	0.019***	0.015**
	(0.002)	(0.002)	(0.003)	(0.003)	(0.004)	(0.004)	(0.006)	(0.006)
Firm Ind. FE (AKM)	0.322***	0.187*	0.055	0.055	0.113	0.268*	-0.167	-0.239
	(0.107)	(0.112)	(0.112)	(0.142)	(0.139)	(0.141)	(0.150)	(0.159)
FE	No	No	No	No	No	No	No	No
Obs	884,694	333,509	182,181	117,496	78,352	45,319	33,205	24,775
Adj.R-sq	0.03	0.01	-0.01	-0.02	-0.01	-0.01	-0.02	-0.02
DepVar	0.50	0.50	0.50	0.50	0.50	0.50	0.50	0.50
F-stat	172	86	62	37	26	11	4	3
p-value (F-stat)	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.02

Notes: The dependent variable is an indicator equal to one if the worker was employed at a firm experiencing a mass layoff at threshold τ. The sample is aggregated to the sector-location-quarter level, with observations weighted to ensure equal representation of workers at layoff versus non-layoff firms within each cell. All independent variables are standardized (z-scores). The column headers indicate different mass layoff thresholds, with the treatment group defined as workers at firms where $s_{f,q_0} \geq \tau$ and the control group as workers at firms where $s_{f,q_0} < \tau$ in the same sector, location, and time period. Standard errors are clustered at the cell level.

[18] In the appendix, we document the correlation between mass-layoff thresholds and firm characteristics including size, industry, location, and year, as well as the relationship between previous crime rates and mass-layoff intensity at the firm level.

6.2.3 Relative Magnitude of Selection Mechanisms

Finally, we combine the within- and between-firm models to assess how different layoff thresholds relate to model efficiency and to quantify the relative importance of each selection mechanism. For each model, we compute the Mean Squared Error (MSE), a standard measure of predictive accuracy capturing the average squared difference between observed and predicted values. Lower MSE values indicate better model fit, which in our context means that worker characteristics have less predictive power for displacement outcomes, suggesting weaker selection. We summarize the two regression models (within and between) as:

$$\begin{bmatrix} D_i \\ W_i \end{bmatrix} = \alpha + \theta'_\tau \widetilde{X}_i + \epsilon_i \tag{20}$$

where D_i indicates displaced workers in the within-firm model, W_i indicates workers at mass-layoff firms in the between-firm model, and $\widetilde{X}_i$ denotes standardized pre-displacement characteristics. Under the null hypothesis of no selection, the true parameter vector θ_τ would equal zero, meaning worker characteristics would not predict displacement or firm layoff status. We therefore calculate the MSE under the assumption that the true parameter vector equals zero:

$$MSE(\widehat{\theta}_\tau) = \underbrace{E[(\widehat{\theta}_\tau - E(\widehat{\theta}_\tau))^2]}_{\text{Variance}} + \underbrace{E[(E(\widehat{\theta}_\tau) - 0)^2]}_{\text{Bias}^2} \tag{21}$$

where $\widehat{\theta}_\tau$ represents the estimated coefficients on worker characteristics at threshold τ. The first term captures sampling variability in our estimates, while the second term captures the squared bias introduced by selection, measured as the deviation of expected estimates from the null of zero. Higher MSE indicates that observable characteristics strongly predict outcomes, suggesting stronger selection, while lower MSE indicates weaker selection.

We compute the MSE for each threshold in both models and plot the results in Figure 4. The

patterns mirror and extend those documented in Tables 4 and 5. First, the within-firm MSE is consistently 30–70% lower than the between-firm MSE across all thresholds, indicating that within-firm selection is the dominant mechanism. Observable worker characteristics explain substantially more variation in who gets displaced within firms experiencing layoffs than in which workers are employed at firms with different layoff intensities. This suggests that firm discretion over dismissal decisions, rather than pre-existing sorting of workers across firms, is the primary driver of the selection patterns we observe.

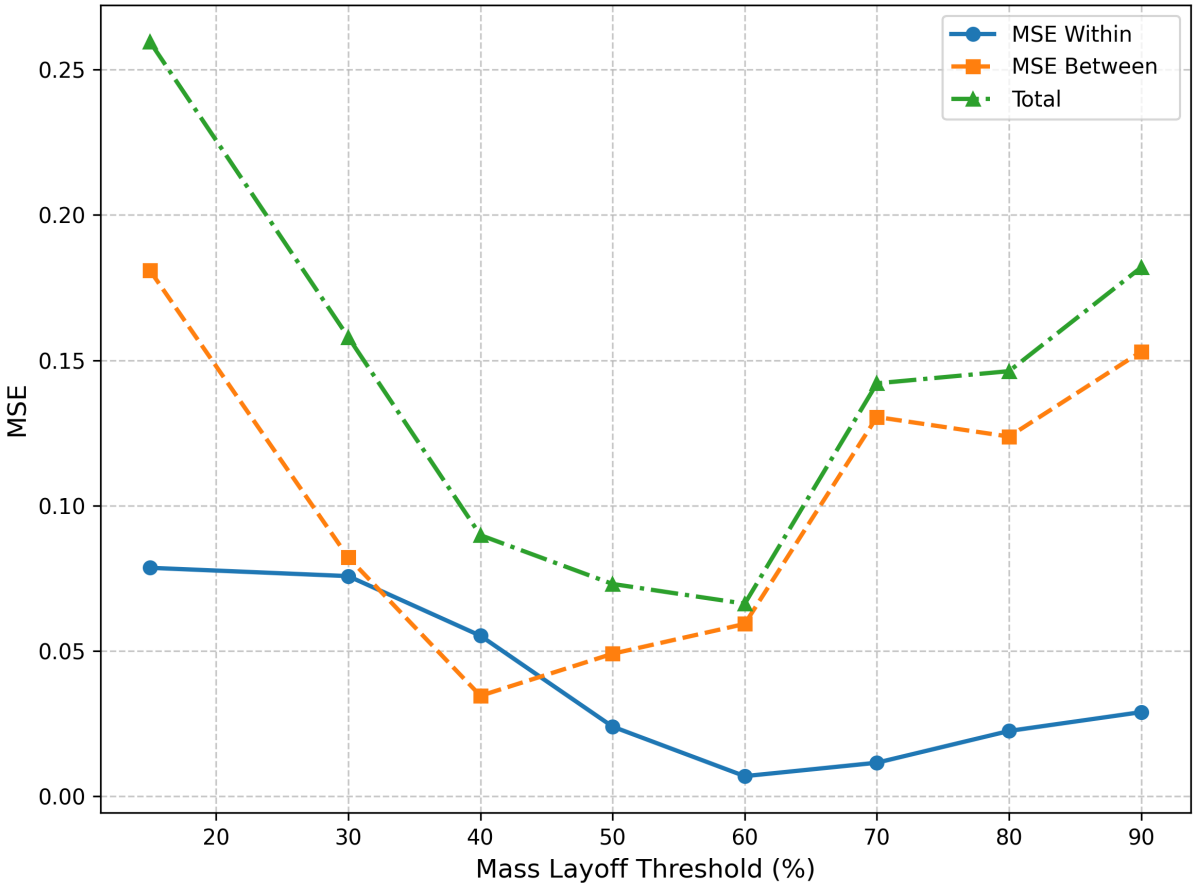
Second, both MSE measures follow a U-shaped pattern, reaching minima around the 60% threshold. At low thresholds (15–30%), high MSE values reflect strong selection: firms exercise substantial discretion over whom to dismiss, making worker characteristics highly predictive of displacement. As thresholds increase to moderate levels (40–60%), selection mechanisms weaken as firms lose discretion, and MSE values decline correspondingly. At high thresholds (80–90%), MSE begins to increase again. This increase likely reflects two factors: small sample sizes at extreme thresholds (fewer than 25,000 workers) increase sampling variability and thus the variance component of MSE, and the nature of selection may differ qualitatively when only the most distressed firms are included, potentially introducing different forms of bias.

Third, the gap between within-firm and between-firm MSE narrows at extreme thresholds, particularly at 90%. At this threshold, within-firm MSE is approximately 0.03 while between-firm MSE is approximately 0.15, a ratio of 1:5, compared to a ratio of approximately 1:2.5 at the 15% threshold. This convergence suggests that as layoffs approach complete shutdowns, firm-level factors such as industry affiliation and geographic location become relatively more important than individual worker characteristics in determining displacement risk. Workers at firms experiencing near-complete layoffs may be more similar to each other within firms but more differentiated across firms, consistent with industry-specific or location-specific shocks driving the most severe displacement episodes.

These patterns collectively suggest that the 60% threshold may optimize the trade-off between selection bias and statistical power in our setting. At this threshold, within-firm selection has substantially

attenuated (F-statistic of 15.78 compared to 687.65 at 15%), sample sizes remain adequate (approximately 47,000 displaced workers), and MSE values reach their minimum for both within and between models.

Figure 4: Mean Squared Error by Mass Layoff Threshold: Within and Between Firm Models



Notes: This figure shows mean squared errors (MSE) computed from within-firm and between-firm selection models across different mass layoff thresholds $\tau \in \{0.15, 0.30, 0.40, 0.50, 0.60, 0.70, 0.80, 0.90\}$. MSE is calculated under the null hypothesis of no selection ($\theta_\tau = 0$) and decomposes into variance and squared bias components as shown in equation (21). The within-firm MSE (blue line) measures predictive accuracy of worker characteristics for displacement within firms experiencing mass layoffs at threshold τ. The between-firm MSE (orange line) measures predictive accuracy for whether workers are employed at firms experiencing mass layoffs at threshold τ. The total MSE (dashed green line) represents the combined selection effects. Lower MSE indicates weaker selection on observables.

7 Discussion: Policy implications

TBD

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