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Sara Malik
University of Utah and IZA

MaryJane Rabier
Washington University in St. Louis

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IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

A Word of WARN-ing: Advance Notice of Layoffs and Labor Market Outcomes*

This study investigates the impact of state-level Worker Adjustment and Retraining Notification (WARN) Acts on advance notice and post-displacement labor market outcomes for U.S. workers from 1993 to 2019. State WARN adoption increases the likelihood of receiving 30+ days of notice by about 4 percentage points. Instrumental variables estimates, supplemented with local average response functions to address weak instruments, show that lengthy notice reduces immediate joblessness by 4 to 7 percentage points. The effects are most pronounced for low-skill workers. Longer term outcomes are less robust. The results indicate that enforceable mandates improve short run transitions, particularly for vulnerable workers.

JEL Classification: J63, J83
Keywords: layoffs, advance notice, WARN

Corresponding author:
Sara Malik
University of Utah
201 Presidents' Cir
Salt Lake City, UT 84112
USA
E-mail: sara.malik@eccles.utah.edu

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1 Introduction

Employment loss imposes large pecuniary and non-pecuniary costs on workers and their communities. Displaced workers, especially those affected by plant closures, face annual wage losses of up to 25 percent and elevated risks of physical and mental illness (Jacobson et al., 1993; Brand, 2015). To reduce these costs, the U.S. government enacted the Worker Adjustment and Retraining Notification (WARN) Act of 1988, which requires covered employers to provide 60 days of advance notice before mass layoffs or plant closings. A large empirical literature has examined whether the WARN Act improves post-displacement outcomes, but findings are mixed.[1] These mixed results may reflect the law's narrow coverage, broad exemptions, and weak enforcement, which leave many displaced workers uncovered. Additionally, these studies also contend with the difficulty of evaluating a nationwide policy that lacks a natural control group.

In this paper, we examine whether stronger and more enforceable advance notice mandates improve outcomes for displaced workers by using the staggered implementation of state-level WARN Acts to estimate the effects of mandatory notice on displaced workers in

[1] A large body of research has evaluated whether advance notice improves displaced workers' outcomes, typically using the Displaced Worker Supplement (DWS) to the Current Population Survey. Early studies using the 1984 and 1986 DWS found mixed or modest effects on unemployment and reemployment (Addison and Blackburn, 1997; Ehrenberg and Jakubson, 1988; Swaim and Podgursky, 1990), though these surveys did not distinguish formal from informal notice or record timing. Ruhm (1992), using the improved 1988 DWS, found that written notice reduced joblessness modestly, but emphasized the endogeneity of notice receipt and the limitations of voluntary notice regimes. Later studies similarly questioned the magnitude of benefits, especially when accounting for selection into notice (Nord and Ting, 1991; Addison and Blackburn, 1994). Evidence on earnings effects is limited and generally null (Ehrenberg and Jakubson, 1988; Swaim and Podgursky, 1990), though some subgroup effects have been noted (Ruhm, 1992; Addison et al., 1992). International studies such as Kuhn and Jones (1995) use matched worker-firm data to address endogeneity more directly, but results may not generalize to the U.S. institutional context. Most prior work evaluates the consequences of voluntarily provided notice rather than legal mandates, limiting the ability to identify causal effects of policy.

the United States. These laws expanded coverage beyond the federal baseline and introduced important variation in scope, timing, and enforceability. We use this variation to estimate both the policy's effect on formal notice provision and the effect of receiving lengthy notice on post-displacement outcomes.

Our paper proceeds in three parts. First, we examine whether state-level WARN Acts increased the provision of formal advance notice. We find that adoption of these laws increases the likelihood of receiving written notice by about three percentage points and the likelihood of receiving more than 30 days of notice by about four percentage points. The effects are most pronounced in states where the WARN requirements extend to smaller firms through lower firm-size thresholds, and we find suggestive evidence that states waiving the federal "one-third" workforce exemption (which excludes layoffs affecting fewer than one-third of a site's employees) also see larger increases in lengthy notice. Together, these results indicate that expanding coverage to smaller firms and to smaller layoff events may be an effective way to strengthen the reach of advance notice mandates.

Second, we study the reduced-form relation between advance notice and post-displacement outcomes. We find evidence that receiving greater than or equal to 30 days of notice is associated with significantly lower rates of joblessness, higher re-employment, fewer full-time to part-time transitions, and higher current earnings. These patterns suggest that longer notice may improve short-term and long-term outcomes. However, selection into notice receipt is a concern: employers may systematically provide longer notice to certain groups of workers based on tenure, performance, or expected re-employment prospects, making observed associations difficult to interpret as strictly causal.

To isolate the causal effect of lengthy notice (receiving greater than or equal to 30 days

of notice), we instrument for notice receipt using the staggered adoption of state WARN laws in a 2SLS framework. This approach identifies the impact of lengthy notice among displaced workers whose receipt of notice was induced by the law. We supplement the IV framework with local average response functions (LARF) to address the well-documented tendency of 2SLS estimates to be distorted in the presence of weak instruments and heterogeneous treatment effects (Abadie, 2003), a concern given that our instrument has modest power. Using LARF, we find that lengthy notice reduces short-term joblessness by roughly 4–7 percentage points. While the instrument is modestly powered, 2SLS results remain significant under wild cluster bootstrap and Anderson–Rubin inference. Effects on longer-run outcomes are directionally positive but less precisely estimated.

Third, we also examine treatment heterogeneity by worker characteristics. Benefits are concentrated among groups with weaker bargaining power, particularly nonunion and low skill workers. For low skill workers, we estimate a reduction in joblessness of about 9 percentage points, which is large relative to baseline rates and should be interpreted with caution given the modest number of treated states and limited statistical power. Nonunion workers also experience meaningful gains, though the estimated effect is somewhat smaller than that for low skill workers. For union members, the point estimate is negative and wild cluster bootstrap inference indicates significance, but precision is limited by a union subsample with fewer than 500 displaced workers, so we view this evidence as suggestive. Estimated effects for higher skill workers and for both men and women are smaller in magnitude and not statistically different from zero. Taken together, these patterns suggest that formal advance notice is most valuable for workers who face structural disadvantages in navigating job loss.

This study contributes to the literature on job displacement and labor regulation in

three ways. First, we provide new evidence on the effects of mandated notice using quasi-experimental variation from state law adoption. Second, we document both reduced-form and IV evidence linking lengthy notice to improved short-run outcomes. Third, we identify under what conditions and for whom formal notice is most effective, showing that WARN Acts are particularly beneficial for low-skill workers. These findings offer timely policy insights: expanding coverage and ensuring enforceable mandates can meaningfully improve outcomes for displaced workers in the short run, while effects on longer-term outcomes such as sustained re-employment and earnings are weaker and less precisely estimated.

The rest of the paper proceeds as follows. Section 2 provides institutional background on the federal WARN Act and describes the structure and variation of state-level mandates. Section 3 details the data and sample construction. Section 4 offers descriptive statistics and reduced-form evidence on how state WARN laws affect notice receipt and labor market outcomes. Section 5 presents our main IV results and explores treatment heterogeneity by worker characteristics and legal provisions. Section 6 concludes.

2 Institutional Setting

2.1 The Federal WARN Act

The Federal WARN Act, enacted in 1988 and effective in 1989, requires covered employers to provide workers with 60 days of advance written notice in cases of mass layoffs or plant closings. However, the scope of the law is limited. It applies only to private-sector (non-governmental) firms with at least 100 full-time employees, and only when layoffs affect

at least 50 workers at a single site.[2] In addition, the statute includes broad exemptions, such as for "unforeseeable business circumstances" or natural disasters, and enforcement is decentralized, with no federal agency responsible for oversight. Workers must sue in court to recover damages, which are typically limited to back pay and benefits for the notice period.

As a result of these narrow eligibility criteria and weak enforcement mechanisms, the federal WARN Act covers a small fraction of displacement events. Roughly 98 percent of U.S. establishments have fewer than 100 employees,[3] and prior research has found little evidence that the federal law substantially increased the provision of formal notice (Addison and Blackburn, 1994; US GAO, 2003; McHugh, 1993; Cross, 2003). This limited reach has led policymakers to question the effectiveness of the federal mandate and to explore stronger alternatives at the state level.

2.2 State WARN Acts and Legal Heterogeneity

Between 2003 and 2019, seven states (California, Illinois, Iowa, New Hampshire, New Jersey, New York, and Vermont) adopted their own WARN laws, each mandating some form of advance notice for displaced workers.[4] These laws are generally modeled on the federal WARN Act but differ in important ways that expand coverage and clarify employer obligations. In defining our treatment group, we focus on these states.

[2]The Act sets different thresholds for plant closings and mass layoffs. A plant closing requires notice if 50 or more employees at a single site lose their jobs during any 30-day period. A mass layoff requires notice if (a) 500 or more employees are affected, or (b) 50–499 employees are affected and they constitute at least one-third of the site's active workforce.

[3]See for example, https://www.bls.gov/charts/county-employment-and-wages/establishments-by-size.htm

[4]Delaware enacted its own WARN Act in January 2019. We exclude Delaware from our analysis because nearly all of its post-adoption period overlaps with the COVID-19 pandemic, an idiosyncratic labor market shock during which many states temporarily suspended or waived advance notice requirements. Since it was unclear the extent to which the pandemic was foreseeable, point estimates for Delaware are likely contaminated by pandemic-related noise and not comparable to the other adopting states.

In constructing our control group, we exclude states with pre-existing mandates that predate our analysis window, such as Maine, Hawaii, and Wisconsin, since these cannot inform identification. We also exclude states that merely encourage advance notice without requiring it, such as Massachusetts, Maryland, Michigan, and Minnesota (Ehrenberg and Jakubson, 1988; Ruhm, 1992; Addison and Portugal, 1987; Minnesota Legislature, 2019). Finally, we exclude states with other, related laws, for example Ohio, Connecticut, and Florida, that could confound post-displacement employment outcomes. These laws typically (a) encourage notice to workers, (b) mandate notice to state officials, or (c) apply to plant closing situations outside the scope of the federal or state WARN Acts. Because these provisions are either nonbinding or address different coverage conditions, including them could introduce measurement error and attenuate our estimates.[5]

2.3 Overview of State-Level WARN Acts

Table 1 summarizes the seven state-level WARN Acts adopted between 2003 and 2019, highlighting key features such as enacted date, required notice duration, minimum firm size, whether the state waives the one-third layoff requirement, and whether employees are entitled to a private right of action. While all statutes require formal advance notice for mass layoffs or plant closures, they differ meaningfully in scope and design. Several states, including Iowa and Vermont, apply their mandates to firms with as few as 25 or 50 employees, well below the 100-employee threshold under the federal WARN Act. California, Iowa, and Vermont also waive the federal "one-third" requirement, which exempts layoffs affecting

[5]Appendix Table OA.2 summarizes the excluded states, along with statutory citations and reasons for exclusion. As a robustness check, we reintroduce these states and tabulate the results in Online Appendix Tables OA.5 and OA.7. Including these states in the treatment group yields estimated effects that are very similar in both direction and magnitude, with no meaningful change in our results.

fewer than one-third of the workforce, thereby increasing the number of events that trigger notice requirements. New York mandates the longest notice period at 90 days.

Most states retain the standard remedy of back pay and benefits for the violation period, consistent with the federal statute, but some clarify procedural rules and employee eligibility criteria in ways that could affect enforceability.[6] These statutory differences shape how binding the mandates are in practice and which workers are affected. A more detailed version of Table 1, which includes definitions of qualifying events and employee eligibility thresholds, appears in Table OA.1 in the online appendix. While some state-level variation is modest, these legal changes plausibly expand the population of covered workers and increase the probability that an employer is legally required to give notice. We draw on selected legal features, such as required notice length, firm size threshold, the one-third waiver, and private right of action, to inform our analysis of treatment heterogeneity in later sections.

3 Data and Sample

We use data from the Current Population Survey (CPS), accessed via the Integrated Public Use Microdata Series (IPUMS), and its Displaced Worker Supplement (DWS), which is fielded every other January. The CPS is a nationally representative monthly survey of approximately 60,000 U.S. households conducted by the Bureau of Labor Statistics and the Census Bureau. The DWS collects retrospective information on workers who lost a job in the past three years due to displacement, defined as job loss resulting from a plant or business

[6]Remedies under state WARN Acts are largely uniform: most entitle workers to back pay and benefits for the violation period (capped at 30–60 days), mirroring the federal law. We therefore do not code damages as a separate legal feature, unlike in contexts such as state age discrimination laws where "stronger remedies" are more common (Neumark and Song, 2013; Neumark et al., 2019)

closure, elimination of a position or shift, or insufficient work. The supplement records characteristics of the lost job, post-displacement labor market outcomes, and whether the worker was given advance notice of impending job loss. Following prior work (Farber, 2017; Ruhm, 1992), we rely on the DWS to study displaced workers. Beginning in 1988, the DWS began distinguishing between formal written notice and informal expectations and added categorical data on notice duration, making it well suited for analyzing the role of advance notice in shaping workers' post-displacement trajectories. [7]

Our sample covers displaced workers from the 1993 to 2020 waves of the DWS, although we end our sample period in 2019 to avoid introducing noise from the COVID era. We begin with all 49,269 individuals identified as displaced workers, then apply a series of exclusion criteria to focus on cases where formal notice would plausibly apply. We drop observations with incomplete responses on key variables. We exclude workers in states with pre-existing mandates that predate our analysis window, states that merely recommend advance notice, and displacement events outside WARN-relevant categories such as seasonal jobs. We remove workers who moved after displacement, since the DWS records state of residence at the time of the survey rather than at the time of job loss.[8] We also exclude non-private workers and those in military, household, or niche/non-classifiable industries, as well as occupations that appear fewer than 10 times in the sample. Finally, in most regressions, we drop treated cases where the state retained the federal one-third exemption and the event was not a plant

[7] Unlike other studies using the DWS that compare displaced to non-displaced workers to quantify the overall costs of job loss (Jacobson et al., 1993), our design conditions on displacement. We identify policy effects by comparing displaced workers in states with and without WARN Acts. This allows us to isolate the incremental effects of advance notice laws within the displaced population, rather than the broader consequences of displacement itself.

[8] National data show that interstate migration is relatively rare, even among the unemployed. Between 2001 and 2010, only 3.5 percent of unemployed individuals moved across state lines annually, compared to 2.3 percent of employed individuals and 1.9 percent of those not in the labor force (Molloy et al., 2011).

closing.[9] After all exclusions, the final sample contains 17,086 displaced workers. These counts are summarized in Table 2 Panel A.

Below, we describe how we define our core outcome measures and explanatory variables.

Advance Notice Variables. The DWS asks displaced workers whether they received written notice prior to their job loss and, if so, how far in advance the notice was given. Based on responses to this question, we construct three measures of advance notice. $BinaryNotice$ is an indicator equal to one if the worker received any formal written notice, and zero otherwise. $LengthyNotice$ is an indicator equal to one if the worker received more than 30 days of written notice. Specifically, responses of "1–2 months" or "more than 2 months" are coded as one; responses of "less than 1 month" or "no notice" are coded as zero. Responses of "given notice, but no time period given" are coded as missing for this variable. $OrdinalNotice$ takes values from 1 to 4, increasing with the reported duration of notice: "No notice" = 1; "Less than 1 month" = 2; "1–2 months" = 3; and "More than 2 months" = 4. Responses of "Given notice, but no time period given" are coded as missing for this variable.

These variables allow us to flexibly measure both the presence and intensity of advance notification.

Joblessness Outcomes. To capture post-displacement employment disruptions, we construct several measures. $Jobless$ is a binary variable equal to one if the respondent reports a spell of unemployment following displacement (including those still unemployed at the time of the survey). $Jobless > 1\ week$ is a stricter binary indicator equal to one if the respondent

[9] In most of our regressions, we exclude workers displaced in states that retain the federal one-third rule, which requires notice only if at least one-third of the workforce is laid off. Because many displacement events in these states fall outside the law's coverage, including them would weaken the estimated effects. By excluding these cases, we focus on situations where state WARN Acts are more likely to apply and influence the provision of advance notice.

was unemployed for more than one week after job loss, filtering out brief administrative gaps between jobs. $WeeksUnemployed$ is a continuous variable capturing the total number of weeks the respondent was unemployed between displacement and re-employment. This question is only asked of workers who report being re-employed at the time of the survey and should be interpreted as conditional on returning to work.[10]

Longer-run Status and Earnings. $CurrentlyEmployed$ is a binary indicator equal to one if the respondent is employed at the time of the DWS survey. $Fulltime\ to\ Parttime$ is a binary indicator equal to one if the worker was employed full-time at the time of displacement but holds a part-time job at the survey date. $CurrentWeeklyEarnings$ is based on reported earnings in the respondent's current job. Because wage responses are top-coded and reported only for a subset of workers, we interpret these results cautiously. Because the Displaced Worker Supplement records outcomes at the time of the survey, these measures reflect labor market status one to three years after displacement, depending on when the job loss occurred within the retrospective window.

All dependent variables are constructed consistently across survey waves and harmonized using IPUMS coding. Summary statistics for all key measures are reported in Table 2 Panel B.

[10] Unemployment duration is top-coded at 168 weeks (3.2 years). We retain these cases but note the potential censoring.

4 Descriptive and Reduced Form Evidence

4.1 Descriptive Statistics

Table 2 Panel B presents summary statistics for three groups: displaced workers in control states (Column 1), displaced workers in treated states (Column 2), and the subset of treated-state workers who report receiving advance notice (Column 3). Workers in treated states are slightly older, have marginally longer tenure (5.50 vs. 5.29 years), and report higher pre-displacement weekly earnings ($886 vs. $652) compared to displaced workers in control states. Those who receive notice have substantially longer tenure (6.81 years), are more likely to have health insurance, and report higher weekly earnings both pre-displacement ($1,026) and post-displacement ($1,004) compared with other treated workers. They also have a lower incidence of full-time to part-time transitions (0.22 vs. 0.24) and higher current earnings.

The observed variation across groups highlights both the relevance of WARN eligibility and the selective nature of notice receipt. These differences reinforce the importance of controlling for observable characteristics and support our use of an instrumental variables approach to isolate causal effects of advance notice.

Table 3 lists the most common occupations and industries represented in our sample. The displaced workers we study come from a wide range of jobs, including managers, sales-persons, truck drivers, cooks, computer specialists, and customer service representatives, as well as industries such as restaurants, computer services, health care, trucking, and retail. This breadth distinguishes our setting from much of the prior displacement literature, which

has often focused on narrower groups of workers, e.g., long-tenure manufacturing workers in Pennsylvania (Jacobson et al., 1993), workers in import-competing sectors such as apparel and textiles (Kletzer, 2001), or case studies of specific plant closings and state-level notice laws (Folbre et al., 1984). By contrast, our data capture displaced workers across a diverse set of occupations and industries, allowing us to evaluate how advance notice laws operate in a broad cross-section of the labor market rather than in a single sector.

4.2 Reduced Form Evidence

Panel A of Table 4 provides descriptive evidence on how advance notice varies across treated and untreated states. Workers in states that adopted WARN Acts report higher levels of notice receipt than those in never-treated states, and these differences widen in the post-adoption period. For instance, the share receiving lengthy notice is 32.3 percent in treated states after adoption, compared with 29.2 percent in the pre-period and 23.1 percent in never-treated states. Similar gaps appear for both the binary and ordinal measures of notice, suggesting that WARN adoption is associated with meaningful increases in the likelihood and length of advance notice.

To estimate the reduced-form effect of state-level WARN Acts on the provision of advance notice, we regress measures of notice receipt on an indicator for whether a state WARN law was in effect at the time of displacement.[11] Formally, we estimate:

[11] Recent work has shown that two-way fixed effects (TWFE) models can produce biased estimates under staggered adoption and heterogeneous treatment effects, particularly when already-treated units are used as controls for later-treated units (Goodman-Bacon, 2021; Sun and Abraham, 2021). This concern is mitigated in our setting due to the limited number of treated states and sparse timing of adoption. A Goodman-Bacon decomposition confirms that 94 percent of the identifying variation comes from comparisons between treated and never-treated states, which are not susceptible to this source of bias. Nevertheless, we rely on IV models for our main causal estimates.

$$\text{NoticeOutcome}_{ist} = \beta \cdot \text{WARNLaw}_{st} + \gamma \cdot X'_{ist} + \delta_s + \delta_t + \delta_o + \epsilon_{ist} \tag{1}$$

where $\text{NoticeOutcome}_{ist}$ is one of three measures: an indicator for receiving any written notice, an indicator for receiving lengthy notice (greater than or equal to 30 days), or an ordinal variable capturing notice length on a 1–4 scale. The variable $WARNLaw_{st}$ equals one if a state-level WARN Act was active in state s during year t. The vector X_{ist} includes worker-level controls (Full-time, Union Member, Years at Employer, Health Insurance, Married, Number of Children, Age, Education, and Pre-displacement Weekly Earnings), as well as state-level controls (State Unemployment Rate, State GDP per capita, and State Employment-to-Population Ratio), all measured in the displacement year. We include state fixed effects (δ_s) to absorb time-invariant regional factors, year fixed effects (δ_t) to capture national shocks, and detailed 3-digit occupation fixed effects (δ_o) to account for occupation-specific separation and notification patterns. Standard errors are clustered at the state level, and p-values are calculated using the wild cluster bootstrap procedure to account for the small number of treated clusters.[12]

Table 4 Panel B shows that the adoption of a state-level WARN Act is associated with a statistically significant increase in the likelihood that displaced workers receive advance notice. WARN adoption raises the probability of receiving any written notice by about 3.1 percentage points ($p < 0.10$) and the probability of receiving more than 30 days' notice by about 4.1 percentage points ($p < 0.05$). The effect on the ordinal measure of notice length is also positive, corresponding to a 0.08-point increase on the 1–4 scale ($p < 0.10$). These

[12] Wild cluster p-values may be incorrect in the presence of heterogeneous cluster size (MacKinnon and Webb, 2017). Because our data share this characteristic, we include conventional standard errors clustered by state for completeness.

findings indicate that state WARN mandates translate into measurable increases in both the incidence and duration of formal advance notice.[13]

To examine the plausibility of parallel trends, we estimate an event study around state WARN adoption using the Sun and Abraham (2021) method.[14] Figure 1 plots estimated effects on the probability of receiving at least 30 days' written notice. Pre-treatment estimates are close to zero, consistent with the parallel trends assumption. Following Roth (2022), however, we note that conventional pre-trend tests often have limited power to detect meaningful violations. Using the pretrends Stata package, we find the test has 80 percent power to detect a linear violation with slope 0.023 percentage points per year, implying potential bias of about 0.12 points over five pre-treatment years—small relative to the post-treatment peak of 1.3 percentage points. At the same time, the estimates are relatively noisy, especially after adoption. This instability reflects the structure of the CPS data: it is a repeated cross-section rather than a panel, we only observe displacement year (not exact dates), and only seven states adopted WARN laws during our sample window. For these reasons, we view the figure primarily as a visual diagnostic of pre-trends rather than a reliable measure of dynamic treatment effects.

Table 5 examines how the effect of state WARN Acts on lengthy notice varies with specific legal features. States with smaller firm-size thresholds (less than 75 employees) see the largest and most statistically significant gains in lengthy notice, with an estimated

[13] Results are robust to the Gardner (2022) approach, which uses a weighted estimator to address staggered treatment adoption and varying effects across units and time periods. Results are displayed in the online appendix in Table OA.3.

[14] A traditional event-study approach is not well suited in our setting. Treatment occurs in only seven states, and adoption is staggered over a relatively narrow window, limiting the available variation in event time. Moreover, our data come from repeated cross-sections rather than a panel, and we observe only the year of displacement—not the month or exact timing—making it difficult to construct clean event-time indicators.

increase of 6.8 percentage points ($p < 0.10$), compared to a smaller and non-significant 3.4 percentage point increase in states with larger thresholds (greater than or equal to 75 employees). Laws without a private right of action are associated with an 8.1 percentage point increase in lengthy notice ($p < 0.10$), while those with a private right of action show a smaller, statistically insignificant 2.3 percentage point increase. This counterintuitive pattern may reflect the fact that the no-PRA states in our sample – Iowa, Vermont, and Illinois – have other features that may make the law more binding, such as lower firm-size thresholds (e.g., Iowa and Vermont) and waiving the one-third rule (e.g., Iowa and Vermont), which help make the law more binding on employers. Alternatively, these states may have foregone PRA precisely because they intended to prioritize government enforcement. Eliminating the federal one-third workforce exemption is associated with a 2.9 percentage point increase in lengthy notice, whereas retaining it yields an insignificant 0.7 percentage point effect. Although not all differences are statistically significant, the pattern suggests that WARN provisions are more likely to increase lengthy notice when they apply to smaller firms or adopt other design elements that broaden coverage, and that the one-third waiver may also matter.

4.3 Relationship between Advance Notice and Displaced Worker Outcomes

Panel A of Table 6 provides descriptive evidence on post-displacement outcomes by length of notice. Workers who receive at least 30 days of notice are less likely to experience joblessness (85.2 percent versus 89.3 percent) or joblessness lasting more than a week (77.8

versus 81.7 percent). They are also more likely to be employed at the time of the survey (68.5 versus 64.3 percent), less likely to downgrade from full-time to part-time work (20.3 versus 24.3 percent), and report higher current weekly earnings (log earnings of 6.31 versus 6.09). These differences are statistically significant, though the raw means may reflect selection into notice receipt.

To estimate the reduced-form relationship between lengthy notice and post-displacement outcomes, we regress each outcome on an indicator for whether the worker received more than 30 days of written notice. The estimating equation is:

$$\text{Outcome}_{ist} = \beta \cdot \text{LengthyNotice}_{ist} + \gamma \cdot X'_{ist} + \delta_s + \delta_t + \delta_o + \epsilon_{ist} \tag{2}$$

where $Outcome_{ist}$ is the labor market outcome of interest for worker i in state s and year t, $LengthyNotice_{ist}$ is an indicator for whether the worker received more than 30 days of notice, X'_{ist} is a vector of individual covariates, and δ_s, δ_t, and δ_o are state, year, and occupation fixed effects, respectively. Standard errors are clustered at the state level, and all p-values are calculated using the wild cluster bootstrap procedure to account for the small number of treated clusters.

Table 6 Panel B presents the results. Lengthy notice is associated with a statistically significant reduction in joblessness: 4.8 percentage points for any jobless spell ($p = 0.000$) and 4.9 points for spells lasting more than one week ($p = 0.000$). The coefficient on unemployment duration is negative (–0.56 weeks) but imprecisely estimated ($p = 0.32$). We also find that advance notice is linked to stronger labor force attachment and better job quality: workers receiving lengthy notice are 4 percentage points more likely to be employed at the time of

the survey (p = 0.000), 2 points less likely to transition from full-time to part-time work (p = 0.013), and earn roughly 7.8 percent higher weekly wages (p = 0.000), which corresponds to about $30–35 at the sample mean.

5 Instrumental Variables Results and Heterogeneous Effects

5.1 Identification Strategy

The staggered adoption of state WARN Acts between 2003 and 2019 provides quasi-experimental variation for studying the effects of mandatory advance notice. Because adoption occurred at the state level and was not tied to any particular industry, employer, or worker, we can compare outcomes for otherwise similar workers displaced just before and after enactment. These laws expanded coverage relative to the federal baseline, which has been in place uniformly since 1989, and thereby increased the likelihood of formal notice for some, but not all, workers. We use this policy-induced variation as an instrument in a two-stage least squares framework to estimate the causal effect of receiving lengthy notice.

Tables 4 and 6 motivate our instrumental variables strategy, which uses state-level WARN Act adoption as an instrument for receiving lengthy advance notice. This approach addresses the fact that notice receipt is not randomly assigned and may correlate with unobserved worker quality, firm characteristics, or layoff context.

The exclusion restriction requires that WARN laws affect job outcomes only through their effect on advance notice. While not directly testable, this assumption is supported by

the institutional design of WARN laws and by the reduced-form results in Online Appendix Table OA.6, which show small and inconsistent associations between WARN adoption and post-displacement outcomes. This pattern suggests that the laws generate meaningful variation in notice receipt without systematically influencing outcomes through other channels.

The monotonicity assumption that WARN adoption does not reduce the likelihood of receiving notice for any displaced worker is supported by the first-stage estimates in Table 4 and the upward shift in notice receipt shown in Figure 1. We find no evidence of defiers, and the event-study patterns reveal no differential pre-trends between adopting and non-adopting states, further supporting the validity of our design.

5.2 Instrumental Variables Estimation

To isolate the causal effect of lengthy notice on post-displacement outcomes, we implement a 2SLS regression using state-level WARN Act adoption as an instrument for receiving more than 30 days of notice. Formally, the second stage estimates:

$$\text{Outcome}_{ist} = \beta \cdot \widehat{\text{LengthyNotice}}_{ist} + \gamma \cdot X'_{ist} + \delta_s + \delta_t + \delta_o + \epsilon_{ist} \tag{3}$$

where $Outcome_{ist}$ is the labor market outcome for worker i in state s and year t, $\widehat{LengthyNotice}_{ist}$ is the predicted value from the first-stage regression using $WARNLaw_{st}$ as an instrument, X'_{ist} is a vector of individual covariates, and δ_s, δ_t, and δ_o denote state, year, and occupation fixed effects. This approach identifies a local average treatment effect (LATE) for compliers, displaced workers who received lengthy notice because of WARN adoption.

Table 7 presents the instrumental variables (IV) estimates of the effect of receiving lengthy advance notice on displaced workers' labor market outcomes. The coefficients suggest very large reductions in joblessness, on the order of 72 to 79 percentage points, which are implausibly large given baseline jobless rates of displaced workers. While statistically significant under both clustered standard errors and Anderson–Rubin inference, the magnitudes are difficult to reconcile with descriptive evidence. Effects on other outcomes, including employment status, unemployment duration, transitions to part-time work, and current earnings, are directionally consistent with faster or higher-quality reemployment but are imprecisely estimated. Kleibergen–Paap F-statistics range from 1.5 to 7.4 across specifications, underscoring the modest power of the instrument.[15]

These patterns are consistent with concerns that conventional 2SLS may inflate coefficients in settings with heterogeneous treatment effects and partial compliance (Abadie, 2003; Angrist and Imbens, 1995; Carneiro et al., 2011). In our context, workers differ in skills, union status, and industries that shape both notice receipt and outcomes, while employers vary in size, coverage, and compliance with WARN requirements. Because it is not directly observable which workers are covered or which employers meet and comply with the statutory thresholds in each state, conventional 2SLS can distort estimates by mis-weighting treatment responses. To address this, we turn to the Local Average Response Function (LARF) framework, which directly targets the causal response for compliers and avoids the extrapolation that can distort IV estimates.

[15] Weak instruments may bias the IV estimate towards the OLS estimate and/or artificially inflate the coefficient. For a discussion of the impact of a weak instrument in our setting, please see the Online Appendix Section C.

5.3 Local Average Response Function

LARF uses Abadie (2003)'s weighting function, denoted κ, to reweight the data so that estimation focuses explicitly on compliers, the workers whose probability of receiving notice changes because of WARN adoption. Intuitively, κ upweights individuals at the margin of treatment and downweights those whose notice status is unaffected by the law, thereby recovering the outcome–treatment relationship for the subpopulation directly induced to receive notice. LARF provides a more credible and interpretable measure of policy-relevant effects than 2sls when compliance is partial and treatment effects are heterogeneous (Abadie, 2003; Carneiro et al., 2011). We implement LARF in our setting to obtain estimates that better capture the causal impact of lengthy notice for the workers most directly affected by state WARN mandates. This framework has been applied productively in other policy contexts, including charter schools (Angrist et al., 2013), disability insurance (Maestas et al., 2013), and preschool programs (Kline and Walters, 2016).

Table 8 reports the LARF estimates for our core outcomes. Receiving lengthy notice reduces the probability of experiencing any jobless spell by 5.3 percentage points (wild cluster bootstrap $p = 0.02$) and joblessness lasting more than one week by 6.0 percentage points (wild bootstrap $p = 0.01$), both statistically significant at conventional levels. These magnitudes are consistent with the descriptive evidence and substantially smaller than the inflated coefficients from conventional 2SLS. We also find a positive effect on current employment at the time of the survey (5.6 percentage points), though effects on unemployment duration, part-time downgrades, and current earnings are small and imprecisely estimated. Overall, the results suggest that lengthy advance notice meaningfully reduces immediate

post-displacement joblessness and improves re-employment prospects, while effects on job quality and earnings are weaker. [16]

Table 9 reports LARF estimates of heterogeneous treatment effects on joblessness lasting more than one week. We stratify the sample by worker characteristics to assess whether certain groups benefit disproportionately from receiving lengthy notice. Specifically, we examine heterogeneity by sex (male or female), union status, wage (below or above the median of the weekly earnings distribution), and education, where "low skill" is defined as a high school degree or less and "high skill" as some college or more. The largest reductions appear among groups with limited bargaining power or weaker access to informal information channels. Nonunion workers experience a 6.8 percentage point reduction in joblessness (wild bootstrap $p = 0.00$), and low-skill workers see an even larger gain of 9.2 percentage points (wild bootstrap $p = 0.06$). While the low-skill effect is statistically significant, the magnitude is large relative to baseline jobless rates and should be interpreted cautiously given the modest number of treated states and the limited power of our design. In contrast, the estimated effects for union members, higher-skill workers, and both men and women are smaller in magnitude and statistically insignificant.[17] Overall, these patterns suggest that advance notice is most valuable for workers who face structural disadvantages in navigating job loss.[18]

[16] Separate analyses using the LARF specification show that our main effects are driven primarily by mass layoffs rather than plant closings. We report these results in the Online Appendix: Table OA.8 presents LARF estimates for mass layoffs only, and Table OA.9 presents LARF estimates for plant closings only.

[17] The point estimate for union workers is negative and relatively large, but imprecisely estimated. This likely reflects limited statistical power, as our union sample includes fewer than 500 displaced workers.

[18] Our heterogeneity results align with evidence that advance notice is particularly valuable for workers with weaker bargaining power or limited access to informal job search networks. Ruhm (1992) finds that the benefits of notice are concentrated among more vulnerable groups, including women, nonwhite workers, and household heads in high-unemployment areas. Similarly, Friesen (1997) shows that Canadian "group termination" notice laws reduced unemployment duration following plant closures, a setting disproportionately affecting lower-skill production workers. In contrast, Addison and Portugal (1992) report that

6 Conclusion

We use the staggered adoption of state-level WARN Acts from 2003 to 2019 to study whether stronger advance notice mandates improve outcomes for displaced workers. These laws expanded coverage beyond the federal baseline and increased the probability of receiving more than 30 days of written notice by roughly four percentage points, particularly when applied to smaller firms or when exemptions such as the federal one-third rule were removed.

Instrumental variables estimates, supplemented with local average response functions (LARF), show that lengthy notice reduces the likelihood of immediate post-displacement joblessness by 4–7 percentage points. The benefits are concentrated among workers with less bargaining power, particularly nonunion and low-skill workers. These results are consistent with the idea that timely, formal notice facilitates job search and smooths labor market transitions, especially for those facing structural disadvantages.

The findings suggest that enforceable mandates can meaningfully improve short-run outcomes, especially for vulnerable groups, while effects on longer-term outcomes such as sustained re-employment and earnings are weaker and less precisely estimated. Policy efforts to expand coverage and strengthen enforcement may therefore deliver the greatest benefits to workers most at risk of poor post-displacement outcomes.

formal written notice in the U.S. primarily benefited white-collar workers, with little effect for blue-collar (lower-skill) workers, suggesting that under voluntary or weakly enforced regimes, notice accrues mainly to advantaged groups. Our findings differ in showing that mandated lengthy notice yields the largest gains for nonunion and low-skill workers, consistent with the idea that binding legal requirements expand coverage to workers who historically were least likely to receive notice.

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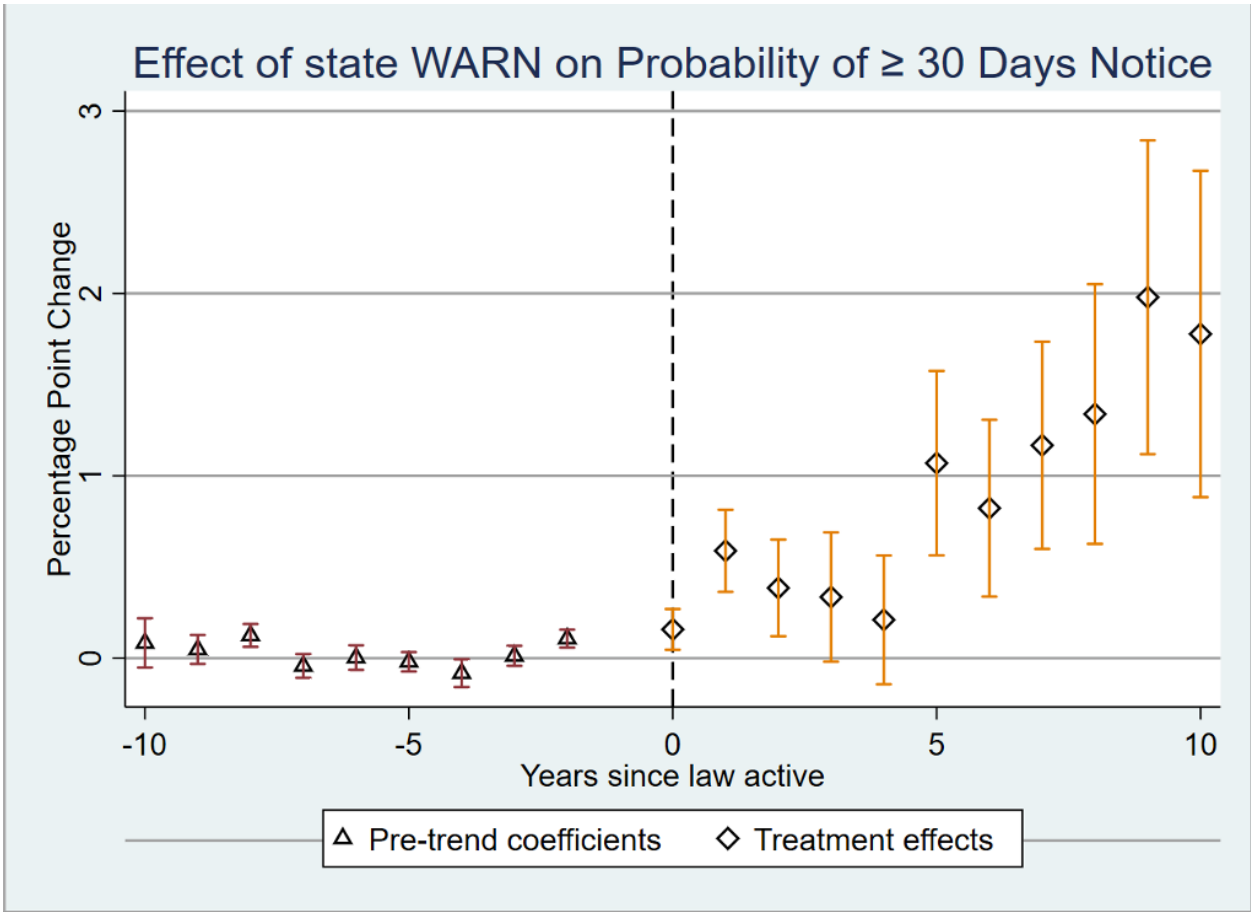

Figure 1

Notes: Data source: CPS Displaced Worker Supplement, 1993–2019. The event-study plot estimates the effect of state WARN laws on the probability of receiving $\geq$ 30 days notice using the Sun and Abraham (2020) method via 'eventstudyinteract'. Triangles (pre-treatment leads, -10 to -2 years) and diamonds (post-treatment lags, 0 to 10 years) show percentage point changes, with 95% confidence intervals. The post-treatment effect peaks at 2 percentage points at year 9, reflecting a lagged impact. Fixed effects (state, year, occupation) and covariates (e.g., full-time status, union membership) are included; standard errors are clustered by state.

Table 1: WARN Act Adoption

Entities	Statute/Citation	Year Passed	Effective Date	Days of Required Notice	Waives One-Third Requirement?	Minimum Firm Size (# of employees)	Private Right to Action
Federal		1988	1989				
California	California Labor Code §§ 1400–1408	2002	1/1/2003	60	Y	75	Y
Illinois	820 ILCS 65	2004	1/1/2005	60	N	75	N
New Jersey	N.J. Stat. Ann. §§ 34:21-1 to 34:21-7; Assembly Bill 4768 (A4768); Senate Bill 3162 (S3162)	2007	12/20/2007	60	N	100	Y
New York	N.Y. Lab. Law §§ 860–860-i	2008	1/1/2009	90	N	50	Y
New Hampshire	N.H. Rev. Stat. Ann. § 275-F:3	2010	1/1/2010	60	N	100	Y
Iowa	Iowa Code Chapter 84C	2010	3/22/2010	30	Y	25	N
Vermont	21 V.S.A. § 411	2015	1/15/2010	30	Y	50	N

Notes: This table summarizes statutory features of the federal WARN Act and the seven state-level WARN Acts adopted between 2003 and 2019. "Days of Required Notice" indicates the minimum period of advance written notice employers must provide before covered layoffs or closures. "Waives One-Third Requirement" refers to whether the state law eliminates the federal provision exempting layoffs that affect fewer than one-third of the workforce. "Minimum Firm Size" is the smallest employer size (measured in number of employees) covered by the statute. "Private Right of Action" indicates whether employees may directly sue for violations under state law. Statutory citations are included for replication and verification.

Table 2: Sample selection and descriptive statistics

Panel A. Displaced Worker Sample Selection: 1993-2019

Filter	Count	Diff
Displaced workers	49,269	
Exclude incomplete responses	45,189	4,080
Exclude observations in states with pre-existing mandates, states that merely recommend notice, and displacement events that fall outside of WARN-relevant categories	31,532	13,659
Exclude workers who moved since displacement	27,317	4,215
Exclude non-private workers and those in military, household, or niche/non-classifiable industries	19,488	7,829
Exclude workers in occupations that appear less than 10 times in sample	18,964	524
Exclude treated cases where no one-third waiver and not a plant closing*	17,086	1,878

Panel B. Displaced worker sample descriptive statistics

	DW Control	DW Treat	DW Treat Rcvd Notice
Number of Observations	15,007	2079	856
Full-time	0.86	0.85	0.89
Union Member	0.06	0.05	0.07
Years at Employer	5.29	5.50	6.81
Health Insurance	0.53	0.52	0.63
Married	0.56	0.52	0.56
Age	39.71	41.07	42.08
Weekly Earnings at Lost Job	$652.29	$885.98	$1,026.83
No. Children	0.51	0.49	0.50
Jobless	0.88	0.91	0.91
Jobless > 1 wk	0.80	0.85	0.85
Weeks Unemployed	12.50	15.25	16.06
Currently Employed	0.67	0.58	0.59
Full-time to Part-time	0.23	0.24	0.22
Weekly Earnings at Current Job	$630.60	$868.39	$1,004.18
Binary Notice	0.36	0.41	1.00
Lengthy Notice	0.24	0.27	0.68
Ordinal Notice	1.72	1.80	2.98
Median Annual Earnings at Lost Job	$25,948.00	$34,320.00	$41,214.94

Notes: Panel A describes sample selection using the Displaced Worker Supplement (DWS), 1993–2019. We restrict to private-sector workers who experienced displacement and provided complete responses on key characteristics. To improve identification, we exclude observations from states with pre-existing mandates or nonbinding recommendations, events unlikely to trigger WARN (e.g., seasonal work or voluntary quits), and workers who moved across states after displacement. We also limit the sample to major occupations and industries where formal notice is most relevant. **Most regressions drop treated cases where the state retained the federal one-third exemption and the event was not a plant closing; Table 4 includes them to assess how one-third waivers affect notice provision.*

Panel B presents descriptive statistics for the final analytic sample of displaced workers from the CPS Displaced Worker Supplement, 1993–2019. All workers experienced involuntary job loss and satisfied inclusion criteria described in Panel A. "DW Control" refers to displaced workers in states that never adopted a state WARN Act during the sample period; "DW Treat" refers to those displaced after state WARN implementation; and "DW Treat Rcvd Notice" is a subset of treated workers who report receiving formal advance notice.

All variables are defined using self-reported DWS responses and harmonized across survey years. See Appendix A for variable definitions.

Table 3: Top Occupations and Industries

Panel A. Top Occupations

Rank	Occupation	Freq
1	Managers and administrators, n.e.c.	844
2	Salespersons, n.e.c.	674
3	Truck, delivery, and tractor drivers	673
4	Supervisors and proprietors of sales jobs	556
5	Cooks, variously defined	471
6	Computer systems analysts and computer scientists	455
7	Cashiers	452
8	Secretaries	410
9	Assemblers of electrical equipment	384
10	Customer service reps, investigators and adjusters, except insurance	377
	All others	11,911

Panel B. Top Industries

Rank	Industry	Freq
1	Eating and drinking places	1,311
2	Computer and data processing services	620
3	Electrical machinery, equipment, and supplies, n.e.c.	489
4	Health services, n.e.c.	484
5	Business services, n.e.c.	481
6	Trucking service	438
7	Grocery stores	396
8	Insurance	368
9	Department stores	361
10	Hospitals	358
	All others	11,901

Notes: This table identifies the most common occupations and industries in the displaced worker sample.

Table 4: Effect of State WARN Act Adoption on Receipt and Length of Advance Notice

Panel A. Mean Notice Outcomes by Treatment Status and Period			
	Binary Notice	Lengthy Notice	Ordinal Notice
Treat States (Post-period)	0.464	0.323	1.921
Treated States (Pre-period)	0.435	0.292	1.867
Never-Treated States	0.355	0.231	1.691
Panel B. Difference-in-Differences Estimates			
	(1) Binary Notice	(2) Lengthy Notice	(3) Ordinal Notice
Post-Treat	0.031	0.041	0.080
SE clustered by state	(0.017)*	(0.016)**	(0.031)**
Wild cluster bootstrap p-value	[0.07]*	[0.03]**	[0.05]*
Full-time	−0.005	−0.017*	−0.038
	(0.011)	(0.010)	(0.023)
Union	0.133***	0.100***	0.325***
	(0.023)	(0.020)	(0.055)
Years at Employer	0.008***	0.010***	0.027***
	(0.001)	(0.001)	(0.002)
Health Insurance	0.104***	0.079***	0.226***
	(0.008)	(0.008)	(0.017)
Married	0.023***	0.025***	0.065***
	(0.008)	(0.008)	(0.018)
Education	0.006**	0.007***	0.018***
	(0.003)	(0.002)	(0.005)
Age	−0.001***	−0.001**	−0.002***
	(0.000)	(0.000)	(0.001)
Logged (Weekly earnings)	0.018**	0.023***	0.057***
	(0.007)	(0.006)	(0.015)
No. Children	−0.002	0.000	−0.007
	(0.008)	(0.006)	(0.016)
State GPP per Capita	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
State Unemployment Rate	−1.206**	−0.636	−2.361*
	(0.541)	(0.586)	(1.295)
State Employment-to-Population Ratio	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)
Observations	13,922	13,843	13,843
R-squared	0.100	0.114	0.128
Year, State, Occ FE	Y	Y	Y

Notes: Panel A reports mean advance notice outcomes by treatment status and period. Panel B reports difference-in-differences estimates from regressions relating state WARN Act adoption to the likelihood and length of advance notice. The dependent variables are: **(1)** an indicator for receiving any written notice (Binary Notice), **(2)** an indicator for receiving more than 30 days' notice (Lengthy Notice), and **(3)** an ordinal measure of notice length ranging from 1 (no notice) to 4 (more than 60 days). The key regressor, *Post-Treat*, equals 1 if the displacement occurred after state WARN implementation. All models include controls for worker demographics, employment characteristics, and state-level economic conditions, as well as fixed effects for state, year of job loss, and occupation. See Appendix A for additional variable definitions. We report two measures of significance: standard errors clustered at the state level (reported in parentheses) and Wild cluster bootstrap p-values (reported in brackets). *Significance levels:* $p < 0.10^{*}$, $p < 0.05^{**}$, $p < 0.01^{***}$.

Table 5: Effect of State WARN Act Adoption on Receipt and Length of Advance Notice Based on Legal Features

	(1) Lengthy Notice	(2) Lengthy Notice	(3) Lengthy Notice
Large Firm Threshold	0.034		
SE clustered by state	(0.019)*		
Wild cluster bootstrap p-value	[0.15]		
Small Firm Threshold	0.068		
SE clustered by state	(0.020)***		
Wild cluster bootstrap p-value	[0.060]*		
With Private Right of Action		0.026	
SE clustered by state		(0.016)	
Wild cluster bootstrap p-value		[0.18]	
Without Private Right of Action		0.081	
SE clustered by state		(0.022)***	
Wild cluster bootstrap p-value		[0.08]*	
With one third waiver			0.029
SE clustered by state			(0.016)*
Wild cluster bootstrap p-value			[0.254]
Without one third waiver			0.007
SE clustered by state			(0.015)
Wild cluster bootstrap p-value			[0.917]
Observations	13,843	13,843	9,802
Controls	Y	Y	Y
p value of comparison	0.28	0.05*	0.04**
R-squared	0.115	0.115	0.099
Year, State, Occ FE	Y	Y	Y

Notes: Each column reports estimates from a separate regression of lengthy advance notice on treatment indicators interacted with specific legal features of state WARN Acts. The dependent variable is *NOTICE-LEN*, an indicator for whether the worker received more than 30 days of advance notice. All models include controls for worker demographics, employment characteristics, and state-level economic conditions, as well as fixed effects for state, year of job loss, and occupation. Large Firm Threshold and Small Firm Threshold indicate treatment effects in states where the WARN Act applied to firms with ≥ 75 or < 75 employees, respectively. With Private Right of Action and Without Private Right of Action indicate treatment effects in states based on whether the law granted workers the ability to sue for noncompliance. With one third waiver and Without one third waiver capture whether the state WARN law eliminated or preserved the federal exemption for layoffs affecting fewer than one-third of the workforce. *Note:* The one-third rule analysis excludes displacement events associated with plant closings, which are automatically covered under WARN regardless of layoff size. This is why the number of observations is smaller in Model 3. We report two measures of significance: standard errors clustered at the state level (reported in parentheses) and Wild cluster bootstrap p-values (reported in brackets). *Significance levels:* $p < 0.10^{*}$, $p < 0.05^{**}$, $p < 0.01^{***}$.

Table 6: Advanced Notice on Job Displacement Outcomes

Panel A. Mean Outcomes by Length of Notice

	Jobless	Jobless > 1wk	Weeks Unemp.	Currently Employed	FT→PT	Log(Weekly Earnings)
Lengthy Notice						
0	89.30%	81.70%	12.64	64.30%	24.30%	6.09
1	85.20%	77.84%	13.47	68.50%	20.30%	6.31
t-statistic (diff)	7.39***	5.70***	−2.09**	−5.05***	4.30***	−0.21***

Panel B. Regression Estimates of the Effect of Lengthy Notice on Outcomes

	(1) Jobless	(2) Jobless > 1wk	(3) Weeks Unemp.	(4) Re-employed	(5) FT→PT	(6) Log(Weekly Earnings)
Lengthy Notice	−0.048	−0.049	−0.560	0.040	−0.020	0.075
SE clustered by state	(0.008)***	(0.009)***	(0.636)	(0.012)***	(0.011)*	(10.19)***
Wild cluster bootstrap p-value	[0.00]***	[0.00]***	[0.32]	[0.00]***	[0.03]**	[0.00]***
Observations	13,843	13,843	9,847	13,843	7,970	9,122
R-squared	0.061	0.073	0.096	0.119	0.083	0.304
Controls	Y	Y	Y	Y	Y	Y
Year, State, Occ FE	Y	Y	Y	Y	Y	Y

Notes: Panel A reports mean post-displacement labor market outcomes for workers who did and did not receive more than 30 days of formal written notice (Lengthy Notice). Reported t-statistics test for differences in means across groups. Panel B reports regression estimates of the effect of receiving lengthy notice on the same outcomes. Dependent variables are: jobless at the survey date (Col. 1), jobless for more than one week (Col. 2), total weeks unemployed (Col. 3), re-employed at the survey date (Col. 4), transition from full-time to part-time work (Col. 5), and log weekly earnings (Col. 6). All models include controls for worker demographics, employment characteristics, and state-level economic conditions, as well as fixed effects for state, year of job loss, and occupation. We report two measures of significance: standard errors clustered at the state level (reported in parentheses) and Wild cluster bootstrap p-values (reported in brackets).*Significance levels:* $p < 0.10^{*}$, $p < 0.05^{**}$, $p < 0.01^{***}$.

Table 7: IV Regression

	Jobless	Jobless > 1 wk	Weeks Unemployed	Currently Employed	Full-time to Part-time	Log(Weekly earnings at current job)
Lengthy Notice (IV)	−0.724	−0.785	5.366	0.398	0.864	−1.690
SE clustered by state	(0.437)	(0.467)	(43.135)	(0.369)	(1.111)	(1.544)
Wild cluster bootstrap p-value	[0.05]**	[0.02]**	[0.39]	[0.28]	[0.21]	[0.07]*
Observations	13,843	13,843	9,847	13,843	7,970	9,122
Kleibergen-Paap F	7.428	7.428	1.452	4.730	3.134	2.208
AR F	3.897	5.171	0.132	0.568	0.104	2.072
AR p	0.056	0.029	0.718	0.456	0.749	0.159
AR chi2	4.123	5.469	0.142	0.595	0.112	2.201
AR chi2 p	0.042	0.019	0.706	0.440	0.737	0.138
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each column reports two-stage least squares (2SLS) estimates of the effect of receiving more than 30 days of written notice (*Lengthy Notice*) on post-displacement labor market outcomes. Lengthy notice is instrumented using state-level WARN Act adoption. Dependent variables include: being jobless at the time of the survey, jobless for more than one week, total weeks unemployed, currently employed, downgraded from full-time to part-time, and current weekly earnings. All models include controls for worker demographics, employment characteristics, and state-level economic conditions, as well as fixed effects for state, year of job loss, and occupation. The Kleibergen-Paap F-statistic reports instrument strength. Anderson-Rubin (AR) test statistics provide robust inference under weak instruments. We report two measures of significance: standard errors clustered at the state level (reported in parentheses) and Wild cluster bootstrap p-values (reported in brackets). *Significance levels:* $p < 0.10^{*}$, $p < 0.05^{**}$, $p < 0.01^{***}$.

Table 8: Local Average Response Function Estimates

	Jobless	Jobless > 1 wk	Weeks Unemployed	Currently Employed	Full-time to Part-time	Log(Weekly earnings at current job)
Lengthy Notice	-0.053	-0.060	0.992	0.056	-0.010	0.074
SE clustered by state	$(0.014)^{***}$	$(0.021)^{***}$	(1.69)	$(0.012)^{***}$	(0.031)	(0.045)
Wild cluster bootstrap p-value	$[0.02]^{**}$	$[0.01]^{**}$	[0.92]	$[0.00]^{***}$	[0.77]	[0.31]
Observations	9,259	9,259	6,608	9,259	5,256	6,074
R-squared	0.063	0.075	0.100	0.121	0.084	0.283
Controls	Y	Y	Y	Y	Y	Y
Fixed Effects	Y	Y	Y	Y	Y	Y
Dep. Var. Mean	0.88	0.80	12.63	0.66	0.24	0.24

Notes: Each column reports local average response function (LARF) estimates of the effect of receiving more than 30 days of written notice (*Lengthy Notice*) on post-displacement labor market outcomes. Lengthy notice is instrumented using state-level WARN Act adoption. Dependent variables include: being jobless at the time of the survey, jobless for more than one week, total weeks unemployed, currently employed, downgraded from full-time to part-time, and current weekly earnings. All models include controls for worker demographics, employment characteristics, and state-level economic conditions, as well as fixed effects for state, year of job loss, and occupation. We report two measures of significance: standard errors clustered at the state level (reported in parentheses) and Wild cluster bootstrap p-values (reported in brackets). *Significance levels:* $p < 0.10^{*}$, $p < 0.05^{**}$, $p < 0.01^{***}$.

Table 9: Heterogeneous Effects of Advance Notice on Joblessness (> 1 week)

	Female	Male	Union	Non-Union	Low Wage	High Wage	Low Skill	High Skill
Lengthy Notice (IV)	−0.074	−0.049	−0.114	−0.068	−0.066	−0.056	−0.092	−0.039
SE clustered by state	(0.051)	(0.029)	(0.072)	(0.023)***	(0.046)	(0.036)	(0.031)***	(0.034)
Wild cluster bootstrap p-value	[0.14]	[0.34]	[0.09]*	[0.00]***	[0.23]	[0.14]	[0.06]*	[0.25]
Observations	4,622	4,637	445	8,814	3,934	4,409	3,943	5,316
R-squared	0.102	0.114	0.486	0.075	0.122	0.096	0.098	0.100
Controls	Y	Y	Y	Y	Y	Y	Y	Y
Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y

Notes: This table presents LARF estimates of the heterogeneous treatment effects of receiving more than 30 days of written notice (*Lengthy Notice*). There are separate regressions run on subsamples defined by sex, union status, wage, skill level, and information environment. The dependent variable is Joblessness (> 1 week) in all columns. All models include controls for worker demographics, employment characteristics, and state-level economic conditions, as well as fixed effects for state, year of job loss, and occupation. We report two measures of significance: standard errors clustered at the state level (reported in parentheses) and Wild cluster bootstrap p-values (reported in brackets). *Significance levels:* $p < 0.10^{*}$, $p < 0.05^{**}$, $p < 0.01^{***}$.

A Variable Definitions

Variable	Definition
Age	the age (in years) of the displaced worker at the time of displacement
Binary Notice	an indicator equal to one if the displaced worker received written formal notice any time before displacement
Currently Employed	a binary indicator equal to one if the respondent is employed at the time of the DWS survey
Current Weekly Earnings	reported earnings in the respondent's current job
Education	the displaced worker's years of schooling measured at the time of the CPS DWS survey
Full-time	an indicator equal to one if the displaced worker was in a full-time position at time of displacement
Full-time to Part-Time	a binary indicator equal to one if the worker was employed full-time at the time of displacement but holds a part-time job at the survey date
Health Insurance	an indicator equal to one if the displaced worker had health insurance through his or her job at the time of displacement
Jobless	an indicator equal to one if the displaced worker experienced any period of joblessness following displacement
Jobless $>$ 1 wk	an indicator equal to one if the displaced worker experienced over one week of joblessness following displacement
Lengthy Notice	an indicator equal to one if the displaced worker received written formal notice at least one month before displacement
Married	an indicator equal to one if the displaced worker was married at the time of the CPS survey
Num. Children	the number of children a worker has at the time of the CPS DWS survey
Ordinal Notice	an ordinal variable that increases in the advance notice period received by a displaced worker
Pre-displacement Weekly Earnings	reported earnings in respondent's lost job in displacement year
Post-Treat	an indicator equal to one when a state-level WARN Act is active in a given state-year
State GDP	State-level GDP per capita (measured the year of displacement)

Variable	Definition
State Employment-to-Population Ratio	ratio of state employment population to total civilian population (measured the year of displacement)
State Unemp. Rate	State-level unemployment rate (measured the year of displacement)
Union Member	an indicator equal to one if the displaced worker was a member of a union or employee organization at the time of displacement
Weeks Unemployed	the number of weeks of unemployment experienced by a displaced worker following displacement
Years at Employer	the number of years a displaced worker worked at his or her employment at the time of displacement