Job Displacement, Crime, and the Value of Unemployment Insurance in the US*

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Abstract

This paper measures the impact of job loss and unemployment insurance on criminal behavior and social welfare. Comparing a sample of workers impacted by mass layoff events across the United States to those who were not laid off, we show that workers who experience layoff suffer large earnings losses and higher criminal legal system contact, particularly felony charging outcomes. We study how changes to unemployment compensation design (replacement rates, benefit duration, and claim access through employer filing environments) mediate these effects. Using a simulated instrument approach, we show that job losers exposed to more generous replacement rates have lower offending and modest changes in employment. We also find that moving to an employer environment with a one-standard-deviation higher post-layoff filing tendency increases successful claiming and reduces offending. Interpreting these estimates through a planner's problem with crime externalities, we show that margins that increase liquidity at separation can improve welfare beyond what has traditionally been measured through standard labor market channels.

Keywords: Unemployment Insurance, Mass Layoffs, Crime
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1 Introduction

Theoretical and empirical work over at least the last half-century has highlighted the important relationships between employment and criminal behavior. Those who face challenges in formal employment, those who are unemployed, working infrequently, or in particularly low-wage jobs, are over-represented in the criminal legal system. The causal relationship between work and crime is likely complex and intertwined. There is significant evidence that a criminal record reduces future employment opportunities and fewer opportunities for stable, legal economic self-sufficiency (via work or income support programs) leads to more criminal behavior. In this paper, we present new evidence that plausibly exogenous job loss increases criminal legal system contact, including felony charging. However, social insurance through unemployment compensation can mitigate these effects. The fact that more generous unemployment compensation increases successful filing and attenuates post-layoff offending is informative regarding the mechanisms linking job loss and crime; together, these findings point to liquidity and short-run income risk as central channels.

This paper overcomes several barriers that have long made answering these questions challenging. Using administrative tax data from the Internal Revenue Service (IRS), we identify more than 2.6 million workers at firms that experience mass layoffs in our sample of 18 states. This tax data not only allows us to identify job losers in mass layoff events, but also their unemployment insurance information returns. We merge this data with court and prison records from the Criminal Justice Administrative Records System (CJARS) to measure potential criminal activity before and after job loss. Using well-established mass layoff designs (difference-in-differences), we credibly identify the impact of job loss on offending. We are also able to present findings across a variety of populations (sex, marital status, race, and others) and outcomes (future earnings, UI filings, number and type of future crimes, future prison entry).

Given our rich data and ample local policy variation, we are able to analyze how unemployment insurance might mediate the job loss and crime relationship. Theory suggests that there may be competing effects of unemployment insurance in this context. By relieving financial strain, unemployment insurance may diminish the incentive to engage in criminal behavior, particularly illicit acts that are financially motivated. However, to the degree unemployment insurance extends the time job losers remain out of work as a result of moral hazard, it may lead to additional criminal behavior that would have been prevented if the offender would have been incapacitated at their job or if they earned relatively more through avoiding labor market scarring due to extended unemployment durations.

To assess the impact of unemployment insurance on job-losers we consider three margins of

unemployment compensation generosity: the replacement rate (how much of a person's wage is paid), the duration of payment (how many weeks of compensation are available to a claimant), and how likely workers are to successfully file a claim (how likely they are to submit paperwork and how likely that paperwork is to be contested by employers).

First, we document that men between the ages of 18 to 45 years old experience substantial increases in criminal activity and incarceration following a job loss associated with a mass layoff event. In this approach, we compare young and middle-aged men who had at least 2 years of employment at a firms with mass layoffs who separate from their employer relative to those who remain employed. While the sample restrictions associated with the research design (e.g. multiple years of continuous stable employment) induces positive selection in the sample making the absolute magnitude of the estimated effects quite low, on a relative basis we observe a 54% increase in the total number of criminal charges, a 101% increase in the number of felony charges, and 200% increase in the likelihood of prison entry during the year of job loss relative to observed outcomes during the preceding 4 years. Similar, although slightly smaller, magnitudes persist up to 4 years after the job loss event. Interestingly, the financial and emotional disruption associated with job loss appears to induce increases in a broad range of offense types, including violent, property, drug, public order, criminal traffic, and driving under the influence crimes.

With the relationship between job loss and crime established, we extend the research design through adding interactions with various levers of unemployment compensation generosity at the time of layoff to the job loss event study. To quantify whether UI might exacerbate or mitigate crime effects, we use a difference-in-differences approach, now interacting job loss with our three measures of policy generosity, relying on changes to state UI policy at the time of layoff and firm features, respectively. To measure changes in replacement rate, we use a simulated instrument approach as in Gruber (1997) and Kuka (2020). To measure duration, we use two approaches. First, we examine a similar simulated instrument approach, estimating how many weeks each state would offer in each year to a fixed sample of job losers. Next, we exploit a sharp 2013 change in federal UI benefit extensions previously explored in the literature.[1] Finally, to measure firm impacts on claim filing, we use an AKM approach, in the spirit of Lachowska et al. (2025). These exercises show robust evidence that UI can help mitigate the estimated criminogenic effect of job losses; however, this policy is not a complete panacea. UI appears most effective at reducing financially motivated crimes, with limited protective effect against crimes of passion. Likewise, UI is most effective in the short-run, when crime risk is greatest, with limited impact after benefits might have

[1]Both exercises related to benefit duration still have yet to be disclosed from the FSRDC environment, and are not further described in this current manuscript.

expired.[2]

Each margin of UI generosity may face different trade-offs relating to impacts on future employment and criminal behavior. In order to make sense of the trade-offs in the design of an optimal unemployment insurance system with multiple manipulable generosity margins and externalities, we develop a model that incorporates the social cost of crime.

This model and our empirical findings suggest that policy levers that raise liquidity at separation especially replacement rates and claim accessibility can generate welfare gains once crime externalities are incorporated into UI design.

2 Data

We use rich administrative data linked through the Census Bureau data linkage infrastructure and accessed through the Federal Statistical Research Data Centers (FSRDCs) to observe individual socio-economic information as well as individual criminal and employment histories for the universe of workers across 18 states. This data allows to identify mass layoff events, unemployment insurance filing, criminal behavior, and a rich set of socio-economic characteristics across states with vastly different, and reforming, unemployment insurance systems.

To measure employment histories we primarily focus on worker W-2 Filings filed between 2005 and 2019. From these tax records we are able to observe an EIN (Employer Identification Number), which we use to track employers. We identify firms that experience mass layoffs as the EINs that have at least 25 employees in the year before layoff and experience at least a 33% reduction in workforce in a given year (that is having 33% or fewer unique W-2s filed in the year of layoff).

Criminal records are measured using CJARS, which compiles criminal justice records from many jurisdictions and agencies. (Finlay & Mueller-Smith, 2021). We restrict all of our analysis to states with sufficient coverage over our analysis period (between 2005 and 2019) to track the relevant outcomes relating to charges (from courts) or prison entry (typically from Correction Departments).

We also gather several demographic characteristics for our sample including information about race, age, sex (all from the numident), number of dependents, marital status, household income (from 1040 filings), and county and state of residence from (Finlay et al., 2023).

As documented in greater detal below, we characterize legal changes to the unemployment

[2]As a robustness exercise, we show changes in work and offending behavior for people in the first years after layoff in states that enacted large changes in their unemployment insurance systems, compared to states that did not. This cross-state difference-in-differences evidence shows that cuts to UI generosity are associated with weaker labor market recovery and higher criminal legal system contact among displaced workers.

insurance using a simulated instrument generated by coding legal changes documented by the Department of Labor based on a fixed sample of workers in the Survey of Income and Program Participation (SIPP).

In Table 1, we show summary statistics for our core analysis sample, people working at firms with mass layoff events. As our analysis will compare workers who separate during these mass layoff and those who do not, column 1 provides details for job-separators and column 2 provides information for those who were not laid off. While there is some evidence of negative selection on the laid-off sample the existence of parallel pre-trends, and the fact that the results are robust to additional sample restriction requriing the two sample to look more similar to each other, a reweighting strategy insisting on mathching samples, and similar results using an alternative identification strategy not relying on job-stayers for estimation provide evidence that the results represent causal impacts.

3 Literature

3.1 Labor Markets and Crime

A large empirical literature documents the substantial and persistent earnings losses following involuntary job separation. Jacobson et al. (1993) use Pennsylvania administrative earnings records to show that workers displaced from long-tenure jobs suffer losses of roughly 25 percent in the years immediately following separation, persisting at around 15 percent even five or more years later; their design compares workers separated during mass layoff events to observationally similar workers at non-contracting firms, mitigating the concern that displacement reflects negative private information. Couch and Placzek (2010) replicate this design using Connecticut administrative data, confirming its external validity, while Davis and von Wachter (2011) document that displacement costs are strongly cyclical, with earnings losses during recessions roughly twice as large as those incurred during expansions. Together, these papers establish that mass layoff events generate large, credibly exogenous income shocks well-suited to identifying the downstream consequences of earnings loss and the value of social insurance in mitigating them.

More recent work extends the mass layoff design to outcomes beyond earnings. Sullivan and von Wachter (2009) show that displacement raises annual mortality rates by 50–100 percent in the years following job loss, while Bhuller et al. (2020) link displacement in Norway to increases in drug-related charges and incarceration, implicating income loss and idleness rather than incapacitation as the primary mechanism. Lachowska et al. (2020) document substantial heterogeneity in post-

layoff outcomes across workers separated from observationally similar firms, motivating the use of firm-level variation we exploit. Building on the mass layoff strategy of Jacobson et al. (1993), we combine it with national administrative crime records and policy variation in UI generosity to separately identify how the income support provided by unemployment insurance shapes both labor market recovery and criminal legal system contact.

A growing body of work estimates the crime effects of job loss directly. Britto et al. (2022) use linked employment and criminal justice records for male workers in Brazil to show that mass layoff events raise the probability of criminal charges by roughly 20 percent, concentrated in economically motivated offenses; UI eligibility substantially offsets these increases while benefits are active, but the protective effect disappears at benefit expiration, directly implicating income support rather than incapacitation as the operative mechanism. Rege et al. (2019) find similar patterns in Norway, with larger effects among workers with weaker labor market attachment. Bennett and Ouazad (2020) find near-zero effects in Denmark, consistent with that country's UI system replacing up to 90 percent of prior earnings and largely insulating workers from the financial distress that drives offending. This literature establishes the core pattern our paper extends to a national U.S. sample: displacement raises crime, income support from UI suppresses it, and the magnitude of both effects is sensitive to the generosity of the insurance environment.

3.2 Social Insurance and Crime

A complementary literature uses natural experiments in other safety net programs to show that the crime-suppressing effect of income support is not unique to UI. Deshpande and Mueller-Smith (2022) study youth who lost SSI benefits at age 18 under the 1996 welfare reform's adult reevaluation, which removed approximately 40 percent of child recipients, and find that SSI removal increases total criminal charges by 20 percent over the following two decades, with financially motivated offenses like theft, burglary, fraud, and forgery, rising by 60 percent and annual incarceration rates increasing commensurately; lost transfer income is not replaced by formal employment, indicating that benefit removal pushes recipients toward illicit activity rather than labor market entry. Mueller-Smith et al. (2023) document that criminal history-based SNAP bans imposed by the same 1996 reform generate large reductions in benefit take-up with spillovers to the spouses and children of affected individuals, suggesting that safety net exclusions impose costs well beyond the directly affected individual. Tuttle (2019) uses a regression discontinuity in Florida's food stamp ban for drug trafficking convictions to show that benefit removal substantially increases recidivism, again concentrated in financially motivated property crimes. Together, these papers establish that income support suppresses financially motivated crime through a channel that operates across program

types and populations. We contribute by applying this logic at national scale to UI – the primary income support available to prime-age workers following job loss – and by separately identifying the contributions of three distinct UI design margins to the program's crime-reducing value.

3.3 UI Research

Unemployment insurance provides liquidity to displaced workers whose savings are insufficient to smooth consumption through a nonemployment spell. The evidence that recipients cannot effectively self-insure is compelling: Gruber (1997) estimates that UI benefits prevent a 22 percent drop in food consumption following job loss; Gruber (2001) documents that one-third of newly unemployed workers cannot replace even 10 percent of their income loss from savings, and half cannot cover more than 5.4 weeks of prior earnings; Chetty (2008) reports that the median recipient holds net liquid wealth of just $252; and Rothstein and Valletta (2017) find that the median worker entering nonemployment holds only $1,350 in liquid financial wealth against monthly earnings exceeding $2,300. These findings establish that the liquidity value of UI is large for most recipients and form the micro-foundation for our welfare analysis of UI design (Chetty, 2008; Gruber, 1997, 2001; Rothstein & Valletta, 2017).

Research on benefit duration finds that extensions modestly lengthen nonemployment spells with limited effects on wages and employment. Studies of the U.S. expansion to 99 weeks during the Great Recession find little effect on job-finding rates and small reductions in labor force participation (Farber et al., 2015; Farber & Valletta, 2015; Rothstein, 2011; Valletta, 2014). German evidence exploiting age-based discontinuities in benefit duration yields similar conclusions (Schmieder & Von Wachter, 2016; Schmieder et al., 2012), with reviews of the broader literature suggesting that a one-month extension lengthens nonemployment by roughly nine additional days in the United States (Card et al., 2007; Card & Levine, 2000; Katz & Meyer, 1990). Wage effects of duration extensions are small and often indistinguishable from zero (Dieterle et al., 2020; Jäger et al., 2020; Lalive, 2007; Nekoei & Weber, 2017; Schmieder & Von Wachter, 2016).

A related literature examines UI eligibility and access. Leung and O'leary (2020) uses a Michigan earnings discontinuity to show that barely-qualifying workers extend their nonemployment spells by roughly 1.7 months relative to those just below the threshold. This paper also develops a model considering the fiscal externalities of the unemployment insurance program, an optimal design question we extend in our own modeling efforts. Lachowska et al. (2025) use an AKM decomposition to document large employer fixed effects in both claiming rates and appeals that are negatively correlated, consistent with employer deterrence of eligible workers; crucially, the dominant source of targeting error in the UI system is not wrongful denial but the failure of eligible

workers to apply, with employer fixed effects accounting for a large share of the income gradient in take-up. Cohen and Schnorr (2024) use randomly assigned examiners in California to show that approving borderline at-fault claimants generates a fiscal externality of only 16 cents per dollar, far below the 67–96 cents associated with expanding benefit amounts or duration. These results suggest that broadening access is comparatively efficient. These findings motivate our analysis of employer-level filing environments as a distinct and underexplored margin of UI design.

Beyond labor market outcomes, UI generates benefits that enter our welfare calculations. Hsu et al. (2018) estimate that UI prevented over a million additional foreclosures during the Great Recession. Barr and Turner (2015) find that an additional 10 weeks of benefits raises college enrollment by roughly 20 percent, and Kuka (2020) documents that a 10 percentage point increase in generosity raises health insurance coverage by 3.3 percentage points. UI also reduces reliance on TANF, SNAP, Social Security, and other programs (Leung & O'leary, 2020; Rothstein & Valletta, 2017). These non-labor effects motivate the extended welfare framework we develop, which augments the standard moral hazard–insurance tradeoff (Baily, 1978; Chetty, 2008) with crime externalities.

Understanding how UI shapes criminal behavior is central to measuring the program's full social value. If employment reduces crime primarily through incapacitation, UI-financed nonemployment should raise offending; but the evidence is more consistent with UI suppressing crime by insulating workers from financial distress. Within the United States looking at individuals who had previous been under the Washington Department of Correction, Rose (2018) uses a regression-kink design at the UI weekly benefit cap in Washington state and finds that a 10 percent increase in benefit amounts reduces offending rates by roughly 5 percent, with effects concentrated in financially motivated crimes. Bennett and Ouazad (2020) find near-zero effects in Denmark, consistent with that country's system already insulating workers from financial distress at the margin. Britto et al. (2022) show in Brazil that UI fully offsets displacement-induced crime increases while benefits are active, with protective effects vanishing at benefit expiration. We extend this literature to a nationally representative sample of U.S. workers, separately identifying the effects of replacement rates, benefit duration, and employer-level filing environments on both labor market recovery and criminal legal system contact.

4 Examining Margins of the Unemployment Insurance System

In this section, we discuss how we use mass layoff events and variation in the unemployment insurance system across states to empirically evaluate how different margins of the UI system

impact engagement with the criminal justice system. Throughout each of these exercises, we use a harmonized empirical specification and vary the differential margin of the UI system we consider. Across three complementary exercises, we examine variation in UI replacement rates, duration, and access.

4.1 Harmonized Empirical Strategy using Mass Layoff Events

The core of our empirical strategy leverages a series of mass layoff events to identify plausibly exogenous job loss events, following Jacobson et al. (1993). Relative to our empirical strategy in Section 5 which uses cross-cohort variation from job losers, we now leverage within-person variation in the timing of job loss and compare the evolution of their outcomes to stayer workers who did not lose their jobs. Formally, we estimate general regressions of the following form:

$$Y_{it} = \sum_k \underbrace{\beta_k 1[E_i = t-k] \times \textit{Job Loss}_i}_{\text{Dynamic effects of job loss}} + \sum_k \underbrace{\beta_k^Z 1[E_i = t-k] \times \textit{Job Loss}_i \times Z_i}_{\text{Dynamic differential effect of UI system}} + \underbrace{\alpha_i + \delta_t}_{\substack{\text{Fixed} \\ \text{effects}}} + \sum_k \underbrace{\theta_k 1[E_i = t-k]}_{\substack{\text{Relative time} \\ \text{fixed effects}}} + \varepsilon_{it} \quad (1)$$

for individual i in year t. E_i are relative time indicators (k indexes relative time), $\textit{Job Loss}_i$ is an indicator for losing a job during a mass layoff event, and Z_i is our measure of unemployment insurance generosity or access. In the below sections, we adopt this general framework and examine the effects of different margins of the UI system by replacing Z_i with measures of generosity and access.

Additionally, we control for individual-level fixed effects α_i and year fixed effects δ_t, along with relative time indicators θ_k. Our coefficients of interest are β_k and β_k^Z, which respectively trace out the direct effects of job loss and the differential effects of the UI system. We normalize both sets of coefficients to two years before the job loss event ($k = -2$). This allows us to compare outcomes in a period not mechanically impacted by our sample definition. Throughout, we cluster our standard errors at the state-level.

The key identifying assumption behind Equation (1) is that the outcomes between job losers and job stayers would have continued to evolve in parallel absent the job loss event. Examining pre-period trends in β_k provides graphical evidence for this assumption. Additionally, to causally interpret β_k^Z, we require that, absent differences in the UI system across state and layoff year, any remaining variation in treatment effects would not be systematically related to our measure of UI

generosity (Olden & Møen, 2022).

Finally, we summarize the dynamic effects of Equation (1) through a semi-parametric version which summarizes the short-run (years 0) and long-run (years 1-4) impacts of job loss and the differential impacts of the unemployment insurance system. We choose short-run as the year of job-loss, as our sample shows evidence of increase UI receipt during this year, but little evidence of increased income in unemployment compensation the following years.

$$
\begin{aligned}
Y_{it} = \beta_0 + & \underbrace{\beta^{SR}[Job\ Loss_i \times Post_{it}^{SR}] + \beta^{LR}[Job\ Loss_i \times Post_{it}^{LR}]}_{\text{Direct job loss effects}} \\
& + \underbrace{\beta_Z^{SR}[Job\ Loss_i \times Post_{it}^{SR} \times Z_i] + \beta_Z^{LR}[Job\ Loss_i \times Post_{it}^{LR} \times Z_i]}_{\text{Differential effect of unemployment insurance system}} \\
& + \underbrace{\alpha_i + \delta_t}_{\substack{\text{Fixed} \\ \text{effects}}} + \sum_k \underbrace{\theta_k 1[E_i = t - k]}_{\substack{\text{Relative time} \\ \text{fixed effects}}} + \varepsilon_{it}
\end{aligned} \tag{2}
$$

We use the coefficients β_Z^{SR} and β_Z^{LR}, which report the average short- and long-run effects of the UI system, to quantify the welfare impacts of alternative UI designs in Section 7.

4.2 Variation in UI Replacement Rates

The first margin of the unemployment insurance system we examine is variation in the replacement rate. To measure the replacement rate, we update and replicate the simulated instrument design used in Kuka (2020), which builds on previous work from Gruber (1997). We first construct a fixed national sample from the SIPP consisting of all individuals observed in the first month of an unemployment spell between 2005 and 2019. Using this invariant sample, we combine each person's pre-unemployment earnings and number of children with contemporaneous state rules to determine UI eligibility and compute the implied weekly UI benefit in every state-year. We then form a simulated replacement rate by dividing each individual's weekly UI benefit by their weekly earnings and aggregate these simulated rates to year x state cells to obtain the average replacement rate RR_{st}.[3] Together, this design isolates policy-driven variation rather than individual heterogeneity. We embed this policy-driven replacement rate variation, RR_{st} into Equation (1) by setting $Z_i = RR_{st}$.

[3]This instrument captures two elements of program generosity: the amount of required pre-employment earnings and the amount of money a claimant receives based on those earnings.

4.2.1 Effects of UI Replacement Rate on Labor and Crime

We first examine the effect of job loss and our simulated replacement rate instrument on labor market outcomes and take-up of unemployment insurance. Figure 1 shows the expected displacement pattern: UI filing rises sharply at layoff while earnings and employment fall, with persistent labor market scarring. The replacement-rate interaction indicates that more generous benefits increase successful UI receipt and partially attenuate earnings losses. Figure 2 shows that job loss increases criminal legal exposure, especially felony outcomes, while higher replacement rates attenuate those increases. Difference-in-differences coefficients summarized in Table 3 are consistent with these event-study patterns.

Figure 1 presents event studies for labor market outcomes around mass layoff events. Each row displays two panels: the left traces the direct effect of job loss and the right displays the differential effect of interacting job loss with the simulated replacement rate instrument. Pre-period trends are flat across all outcomes, supporting the parallel-trends assumption underlying our design. The direct-effect panels show the standard displacement pattern: UI filing rises sharply in the year of layoff (by approximately 7.8 percentage points) while W-2 wages and employment fall substantially and persistently. Earnings losses reach roughly $3,900 in the year of displacement and widen to nearly $7,950 in the subsequent three years, representing short- and long-run declines of approximately 8 and 17 percent of pre-displacement wages, respectively. The interaction panels show that higher simulated replacement rates are very noisily associated with directionally higher UI filing and partial attenuation of earnings losses in the short run, but lower likelihood of filing a W-2, consistent with more generous benefits enabling a more financially stable job search, but some decrease in the extensive margin of attachment.

Figure 2 presents the corresponding event studies for criminal legal system contact. The direct-effect panels show that job loss generates meaningful increases in criminal charges beginning in the year of displacement, with effects concentrated in serious offenses. The probability of any felony charge rises by approximately 0.54 percentage points at displacement, a roughly 46 percent increase relative to the pre-displacement baseline, and new prison entry rates nearly double in the short run. Effects on overall charges and felony charges remain elevated through the post-period, pointing to persistent rather than purely transitional exposure. The interaction panels reveal that higher replacement rates significantly attenuate these increases: the short-run interactions with both the number of charges and the probability of any charge are negative and precisely estimated, with the felony-specific margins exhibiting the strongest attenuation. These patterns are consistent with a liquidity channel in which the income support provided by more generous UI benefits reduces the

acute financial pressure that would otherwise raise incentives for criminal activity in the immediate aftermath of displacement.

Additional Results on Crime Types: Job loss and unemployment insurance may lead to differential offending across crime types. Understanding what types of crime change after layoff or receipt of additional insurance helps us understand whether crime responds to immediate financial distress or to other channels. Figures 3 and 4 display the crime-type breakdown across the extensive and intensive margins, respectively.

The offense-type results indicate that displacement raises criminal exposure broadly and persistently across all three major non-violent categories. On the extensive margin, job loss increases the probability of any property charge by approximately 0.27 percentage points in the short run and 0.18 percentage points in the long run, with nearly identical-magnitude effects on drug charges (0.25 pp and 0.16 pp) and public-order charges (0.26 pp and 0.16 pp). The intensive margin tells a similar story: the number of property charges rises by 0.49 per 100 displaced workers in the short run (0.38 in the long run), drug charges by 0.46 (0.33), and public-order charges by 0.40 (0.27). All of these estimates are precisely estimated and statistically significant at conventional levels. The broad similarity in the magnitude and persistence of effects across offense types is consistent with a general financial-stress channel rather than a mechanism tied narrowly to any one type of criminal activity.

More generous UI benefits provide meaningful protective effects against this broad increase in criminal exposure. In the short run, higher replacement rates significantly attenuate job-loss-induced increases across all three major offense categories on both margins. On the extensive margin, a one standard deviation increase in the simulated replacement rate (approximately 4 percentage points) reduces the short-run increase in property charges by roughly 0.05 percentage points, drug charges by 0.06 percentage points, and public-order charges by 0.06 percentage points; offsets of approximately 19, 23, and 22 percent of the respective direct job-loss effects. The pattern is similar on the intensive margin, where a one standard deviation increase in the replacement rate reduces short-run charge counts by approximately 0.085 per 100 workers for property offenses, 0.11 for drug offenses, and 0.085 for public-order offenses, attenuating the direct effects by 17, 24, and 21 percent respectively. The drug-offense channel shows the strongest and most precisely estimated protective effects on both margins, suggesting that income support from UI may be particularly effective at reducing financially motivated drug-related offending in the immediate aftermath of displacement. Long-run replacement rate interactions are directionally consistent but substantially less precise, reflecting the fading of both the UI income effect and the acute financial stress that

characterizes the period immediately following job loss. Appendix Figures 9 and 10 present a further disaggregation by detailed offense sub-category (e.g., larceny, assault, drug possession, DUI), confirming that the results are broad-based rather than driven by any single charge type, but do seem more common in income related offenses.

4.3 Variation in UI Access Across Employers

The final margin of unemployment insurance generosity we consider is variation in access across employers. When workers are displaced from a firm and claim UI, firms have an opportunity to contest the validity of that claim. To the extent firms vary in the degree to which they contest these claims, this behavior generates variation in ease of access to UI across firms (Lachowska et al., 2025).

Following Lachowska et al. (2025), we quantify the variation in this extensive margin access to UI through estimation of an AKM model (Abowd et al., 1999), where we relate the probability of claiming UI to a vector of firm and individual fixed effects and a series of time-varying controls. Formally, we estimate models of the following form:

$$y_{ijt} = \underbrace{\alpha_i}_{\substack{\text{Worker} \\ \text{effects}}} + \underbrace{\psi_{j(i,t)}}_{\substack{\text{Firm} \\ \text{effects}}} + X'_{it}\beta + \varepsilon_{ijt} \tag{3}$$

for individual i, separating from firm j, in year t.[4] Using a sample of twice-separating workers, we collect the $\psi_{j(i,t)}$ fixed effects from estimation of Equation (3), which represent time-invariant firm-specific variation in access to UI. Firm effects may be larger if, for example, employers are less likely to contest separating workers' UI claims. Below, we use these firm effects to quantify the effects of access to UI on labor market outcomes and criminal justice system involvement.

Appendix Figure 6 describes the distribution of the firm effects. We report the distribution of firm effects for each component of the split-sample IV and the bias-corrected variance, along with the pooled set of firm effects. The observed spread indicates substantial employer-level heterogeneity in claim access, with magnitudes broadly in line with Lachowska et al. (2025).

For firm effects $\psi_{j(i,t)}$ to be interpreted as causal objects, we require the standard exogenous mobility assumption, that is, workers do not sort to firms based on the error term, ε_{ijt}. Such an assumption could be violated if workers sort to (and then separate from) firms where they are more

[4] In the vector X'_{it} we include a series of year fixed effects and a cubic in age. Following Card et al. (2013) we normalize the cubic to be zero at age 40 and omit the linear term to permit identification of the year fixed effects.

likely to be able to claim unemployment insurance, above and beyond what would be predicted by the employer effect alone. Put differently, if there are match effects on claiming between worker i and firm j, this assumption will be violated. Our use of mass layoffs to identify exogenously separating workers limits the potential for this type of sorting to occur.

We provide two empirical exercises that support the validity of the exogenous mobility assumption, following (Lachowska et al., 2025). First, Panel A of Appendix Figure 6 presents a binscatter of the relationship between changes in firm claiming rates and changes in worker claiming rates and shows the expected positive relationship. Panel B replaces changes in firm claiming rates with changes in firm effects and shows a similar positive association.

Having established the validity of the estimated firm effects, we embed this heterogeneity into our dynamic difference-in-differences model from Section 4.1, replacing $Z_i = \psi_{j(i,t)}$.

4.3.1 Effects of UI Access on Labor and Crime

We first examine the effect of job loss and our AKM access measure on labor market outcomes and take-up of unemployment insurance. Figure 5 shows that higher-access firms generate higher post-layoff UI receipt and partial attenuation of earnings losses. Figure 6 shows corresponding attenuation in criminal legal outcomes, especially felony charging outcomes. Difference-in-differences estimates in Table 4 summarize the same pattern, with strongest precision in short-run effects.

A reassuring feature of the AKM results is that the direct effects of job loss in the sample for which we estimate our firm effects are nearly identical to those in the replacement rate analysis, lending confidence that the two exercises are identifying the same underlying displacement shock. In the short run, job loss raises UI filing by approximately 7.4 percentage points (versus 7.8 in the Kuka exercise), lowers W-2 wages by roughly \$3,130, and increases the probability of any felony charge by approximately 0.57 percentage points, essentially matching the felony increase of 0.54 percentage points estimated using the simulated replacement rate instrument. Long-run wage losses of approximately \$7,938 are nearly identical to those in the Kuka specification. The close alignment of direct-effect estimates across the two exercises indicates that the AKM sample of twice-separating workers is representative of the broader mass-layoff population, and that the firm-effects variation we exploit is not confounded by differential selection into the estimation sample.

The AKM firm effects enter with precisely estimated and economically meaningful effects on both labor market and criminal outcomes. On the labor market side, displacement from higher-access firms significantly raises successful UI filing in both the short run ($p < 0.01$) and long run ($p < 0.05$), consistent with the firm-level UI claiming environment directly shaping workers'

ability to access benefits following layoff. On crime, higher firm-level access is associated with significantly lower criminal charges across all primary margins in both the short and long run. In the short run, the interactions with total charges, any charge, felony charges, any felony charge, and prison entry are all negative and significant at $p < 0.01$, with the felony-specific margins exhibiting particularly strong attenuation. The long-run interactions remain negative and statistically significant across all crime margins ($p < 0.05$ or better), suggesting that UI access at the moment of displacement has persistent effects on criminal legal system involvement well beyond the initial benefit-receipt period. The IV estimates in Table 4, which instrument the firm effects to address measurement error, broadly confirm these patterns: short-run attenuation of charges, felony charges, and prison entry remain statistically significant, and the long-run prison entry interaction remains precisely estimated ($p < 0.01$), while some long-run charge outcomes become noisier as expected given the reduced effective sample in the split-sample IV design.

Additional Results on Crime Types: Job loss and employer UI access may differentially affect criminal offending by crime type. Understanding which offense categories are most sensitive to firm-level UI claiming norms helps illuminate whether access to UI operates through the same financial-stress channels suggested by the replacement rate evidence. Figures 7 and 8 display the crime-type breakdown across the extensive and intensive margins, respectively.

The offense-type pattern under the AKM access instrument broadly mirrors the replacement rate findings, with displacement generating statistically significant short-run increases across property, drug, and public-order charges on both margins. On the extensive margin, job loss raises the probability of any property charge by approximately 0.28 percentage points in the short run and 0.18 percentage points in the long run, with nearly identical magnitudes for drug charges (0.28 pp and 0.17 pp) and public-order charges (0.28 pp and 0.18 pp). The intensive-margin estimates are similarly consistent: the number of property charges rises by approximately 0.50 per 100 displaced workers in the short run (0.38 in the long run), drug charges by 0.51 (0.35), and public-order charges by 0.42 (0.30). The striking uniformity of these effects across offense types, now confirmed with a second identification strategy, lends additional credibility to the general financial-stress interpretation.

Firm-level UI access also exerts meaningful protective effects against post-displacement criminal involvement, with attenuation concentrated most clearly in the short run. On the extensive margin, displacement from higher-access firms — those with larger AKM firm effects for UI claiming, is associated with statistically significant short-run reductions in the probability of property charges and drug charges (both at $p < 0.01$), with borderline,significant attenuation of public-order

charges as well. On the intensive margin, the short-run interactions are significant across property, drug, and public-order categories (all at $p < 0.05$), while long-run attenuation of violent and property charges remains precisely estimated (both at $p < 0.05$), suggesting that UI access reduces not only first-time contact but persistent involvement in the criminal legal system. These patterns are broadly consistent with the replacement-rate evidence: the protective effects of UI are strongest in the short run when financial distress is most acute, with some persistence on the intensive margin that points to a role for UI access in interrupting trajectories toward deeper criminal legal system involvement. Appendix Figures 13 and 14 present a detailed sub-category breakdown that confirms these findings are not concentrated in any single offense type, but do seem more common in income related offenses.

5 Robustness

5.1 State-Level Difference-in-Differences

During our analysis period several states have large declines in the generosity of their unemployment insurance systems. We focus on seven of these states: Missouri and South Carolina in 2011, Florida, Georgia, and Michigan in 2012, North Carolina in 2013, and Kansas in 2014. To study these cuts in unemployment insurance, we compare separators after mass layoff events across states that reduced benefits to those that did not in a difference-in-differences and event study framework.

We restrict our analysis sample to workers who lost their job after working for their employer for at least 2 years (so they are very likely eligible for unemployment benefits), and focus on the year of job loss and the following year (the period where workers might be receiving benefits).

First, we show the raw data for key outcomes (wage earnings, employment, UI filing, criminal charges, and prison entry) in states that lowered UI generosity and those that did not in Appendix Figure 17. The raw series show that reform states experience declines in replacement rates and filing, accompanied by weaker labor market outcomes and higher criminal legal system contact after reform.

We estimate the regressions of the form

$$Y_{it} = \sum_k \beta_k 1[E_i = t - k] \times UI\ Reform_i + \sigma_i + \delta_t + \varepsilon_{it}$$

for individual i in year t. E_i are relative time indicators (k indexes relative time), σ and δ are state and year fixed effects. We also summarize the results via $Y_{it} = \beta^{Post} + \sigma_i + \delta_t + \varepsilon_{it}$. This evidence shows that people who lose their jobs after large declines in their state's UI generosity are

less likely to successfully file UI and face higher rates of charges, felony charges, first charges, and prison entry in the short run. Figure 18 displays event studies showing the evolution of replacement rates, unemployment insurance filing, wages, employment (filing any W-2), the number of new court filings a worker receives, whether the worker received any new charge, the number of felony charges, whether the worker received any new felony charge, and whether the worker entered prison. DiD results are summarized in Table 2.

The DiD estimates in Table 2 confirm a clear and consistent pattern of higher criminal legal exposure in reform states. The event studies in Figure 18 formalize this picture. The instrument panel shows that the average reform event lowers the simulated replacement rate by approximately 5 percentage points, with the effect building from roughly 1.4 pp in the first post-reform year to about 5.2 pp by year four, and remaining stable thereafter. In the reduced-form specification, job losers in reform states are more likely to receive any new charge (approximately +2.2 percentage points, $p < 0.01$), accumulate more total charges (+0.043, $p < 0.05$), see increases in felony-specific outcomes (any felony charge +0.67 pp, $p < 0.01$; number of felony charges +0.013, $p < 0.05$), and have higher rates of new prison entry (+0.12 pp, $p < 0.05$). Taken together, the state design provides a transparent estimate of the crime-reducing value of UI generosity, which we view as verification of our more involved mass-layoff exercises that provide variation at the individual level.

Previous research has demonstrated differences across groups in the propensity and ability to successfully claim unemployment compensation. Additionally, some groups may be differentially responsive to benefits based on how dependent they are on W-2 wages or how they experience the stress of job loss. In Appendix Table 5 we display these subgroup results. The heterogeneity results reveal that the crime-increasing effects of UI cuts are broad across demographic groups, though their magnitude varies meaningfully. Younger workers are most sharply affected: among job losers aged 18–24, reform-state exposure raises the probability of any new charge by approximately 2.1 percentage points ($p < 0.01$) and new prison entry by 0.17 percentage points ($p < 0.01$), both larger than the pooled estimates. Single workers also exhibit sizable increases in charges (+1.8 pp, $p < 0.01$) relative to married workers (+0.8 pp, $p < 0.01$), consistent with married individuals having greater household insurance against income shocks. Examining race, White workers show significant and precisely estimated charge increases (+1.8 pp, $p < 0.01$), while Black workers display comparable or larger point estimates for felony charges and prison entry but with wider confidence intervals reflecting the smaller subsample ($N \approx 823{,}000$). Women experience significant increases in any new charge (+1.1 pp, $p < 0.05$) and felony charges ($p < 0.05$), indicating that the crime-cost of UI cuts is not limited to men. Finally, the effects are present across both low- and high-unemployment counties ($p < 0.01$ for any charge in both), with high-

unemployment environments showing a somewhat larger prison-entry response, consistent with weaker outside-option labor demand amplifying the hardship from reduced UI generosity.

5.2 Pre-period earnings restriction and reweighting

We assess the robustness of our main findings across both the replacement rate (Kuka) and AKM access exercises by repeating our full set of event-study and difference-in-differences specifications under two alternative sample restrictions. In each case we verify that estimated effects on labor market outcomes and criminal legal system contact are qualitatively and quantitatively similar to those reported in the main text.

Pre-Period Earnings Restriction: We restrict our sample to workers whose pre-displacement earnings were between $15,000 and $150,000, isolating workers with strong labor force attachment and mitigating the influence of extreme outliers whose UI eligibility or benefit calculation may be atypical. This restriction eliminates workers at the very bottom of the earnings distribution who may have limited UI eligibility and those at the top for whom the replacement rate is not a meaningful financial constraint.

Entropy Weighting: We apply entropy weights to job losers so that their observable pre-displacement characteristics, including earnings, age, industry, and W-2 attachment, align closely with those of job stayers. This reweighting procedure further ensures group comparability and guards against the possibility that estimated treatment effects reflect compositional differences between the displaced and non-displaced populations rather than the causal consequences of job loss and UI generosity.

Appendix Figures 11 and 12 present the reweighted event studies for the replacement rate exercise, paralleling main-text Figures 1 and 2 respectively. Appendix Figures 15 and 16 present the corresponding reweighted event studies for the AKM access exercise, paralleling Figures 5 and 6. Across both instruments and both sample restrictions, the results are qualitatively similar to the main-text findings: job loss generates persistent increases in criminal legal system contact concentrated in felony outcomes and prison entry, while higher replacement rates and greater firm-level UI access attenuate these increases, particularly in the short run. Point estimates under the restricted and reweighted samples are somewhat noisier given reduced sample sizes, but remain directionally consistent and statistically significant on the primary crime margins. The stability of results across these alternative specifications supports the interpretation that our findings reflect genuine causal effects of job loss and UI generosity rather than artifacts of sample composition or outlier influence.

6 Model

Model Setup. We use a canonical model of optimal unemployment insurance (Chetty 2008) adjusted to our setting. We consider an unemployed individual who engages in job search (s) and compensating activities, specifically crime (a). We normalize s to equal the probability of finding a job, denoting the search effort cost by $\phi(s)$; and we normalize a to be the "reward" from the activity, denoting the crime effort cost by $\mu(a)$.

If search is unsuccessful, the agent receives unemployment benefits b. If search is successful, the agent begins working for wage w and pays tax τ used to finance unemployment benefits. Engaging in crime increases consumption in either state of the world, but at a cost of lower earnings if employed by p ("penalty").

With this setup, consumption when unemployed is $c_u = b + a$, and consumption when employed is $c_e = w - \tau + a - p$. The agent's value function is then:

$$J(b) \equiv \max_{s,a} \left[(1-s)\, u(c_u) + s\, u(c_e) - \phi(s) - \mu(a) \right] \tag{4}$$

Planner's Problem. The planner chooses the unemployment benefit system that maximizes the agent's expected utility. The planner policy choices include benefit generosity, b, and a tax rate to fund benefits, τ.

The traditional budget constraint, so that expected taxes collected must be at least as high as expected benefits paid, is captured by: $(1-s)b - s\tau \leq 0$. Importantly, our findings emphasize the need for broader budget considerations since benefits reduce crime. The budgetary gains from reduced crime are threefold. First, they include direct costs, such as damages and policing ($ amount). Second, they include spillovers to other agents, such as crime externalities. We model these two component jointly as a having a cost of θ per unit of crime. Third, they include indirect costs vie the reduction in earnings if employed (affecting tax revenue).

The planner therefore solves:

$$W(b) \equiv \max_{b,\tau} J(b) \quad \text{subject to} \quad (1-s)b - s\tau \leq 0. \tag{5}$$

We assume that search effort s is fixed in order to focus on the implications of our findings for for the optimal design of unemployment insurance (as the results for s are well-established in the literature).

Welfare Gains from More Generous UI. Welfare change from an increase in b is given by:

$$\frac{dW}{db} = (1-s)u' - sv' \frac{d\tau(b)}{db} \tag{6}$$

where

$$\frac{d\tau(b)}{db} = \frac{1-s}{s} + \frac{1}{s}(sp+\theta)\frac{da^*(b)}{db}.$$

It follows that:

$$\frac{dW}{db} = (1-s)(u'-v') - (sp+\theta)\frac{da^*(b)}{db}.$$

The first term is the traditional insurance value term. The second term is the novel portion of the fiscal externality that we estimate, which stems from the effects of crime reduction from more generous benefits.

7 Optimal Program Design With Measured Externalities

To assess the relative efficiency of the three policy levers studied (replacement rate, benefit duration, and claim accessibility) we conduct a budget-neutral exercise from the perspective of a constrained social planner. In this framework, the planner has a fixed budget to allocate toward unemployment compensation and seeks to maximize welfare by identifying which margin delivers the highest return on the marginal dollar spent. Unlike traditional optimal UI calculations that focus primarily on the trade-off between consumption smoothing and moral hazard (reduced search effort), our calculation explicitly incorporates the fiscal externality of crime prevention. As derived in Equation 6, the welfare gain from increased generosity includes a correction term, $(sp+\theta)\frac{da^*(b)}{db}$, which accounts for the reduction in direct costs of crime (damages and policing) and the preservation of future tax revenue by avoiding incarceration-driven earnings penalties.

We operationalize this by comparing the "net cost" of an equivalent expenditure across the three distinct margins: increasing the replacement rate, extending the potential duration of benefits, or increasing the likelihood of successful filing (access). We utilize our empirical estimates of β_Z^{SR} and β_Z^{LR} from Equation 2 to quantify the causal impact of each margin on labor supply and criminal offending. For instance, while extending duration may provide liquidity, it risks mechanically increasing the time out of work, potentially leading to adverse long-run effects if idleness complements criminal activity. Conversely, increasing the replacement rate or reducing administrative friction (access) may alleviate the immediate financial strain that drives offending without necessarily inducing the same magnitude of prolonged unemployment. By weighing the

consumption smoothing benefits against the net fiscal cost—adjusted for the savings generated by the estimated reduction in criminal charges and prison entry—we determine which policy lever minimizes the "fiscal leakage" of the UI system while maximizing social welfare.

[NOTE: THIS SECTION IS INCOMPLETE. WE ARE CURRENTLY WAITING FOR ADDITIONAL RESULTS TO BE DISCLOSED FROM THE FSRDC TO POPULATE THE COST-BENEFIT ANALYSIS AND POLICY COUNTERFACTUALS ALLUDED TO IN THIS SECTION.]

8 Conclusion

Our findings show that job displacement generates meaningful labor market losses and increases criminal legal system contact, including felony charging outcomes. These effects are not only private costs to displaced workers, but also social costs through crime externalities.

Across our policy margins, the evidence points to a consistent role for liquidity at separation. More generous replacement rates and greater employer-level claim accessibility increase successful UI receipt and attenuate post-layoff increases in offending, while partially cushioning earnings losses. In the current draft, duration-based estimates are still being finalized.

Embedding these estimates in an optimal UI framework implies that UI design should account for crime-related fiscal and social externalities in addition to standard consumption-smoothing and search-margin considerations. The central implication is that margins that improve immediate access to resources after displacement can raise welfare beyond traditional labor market channels.

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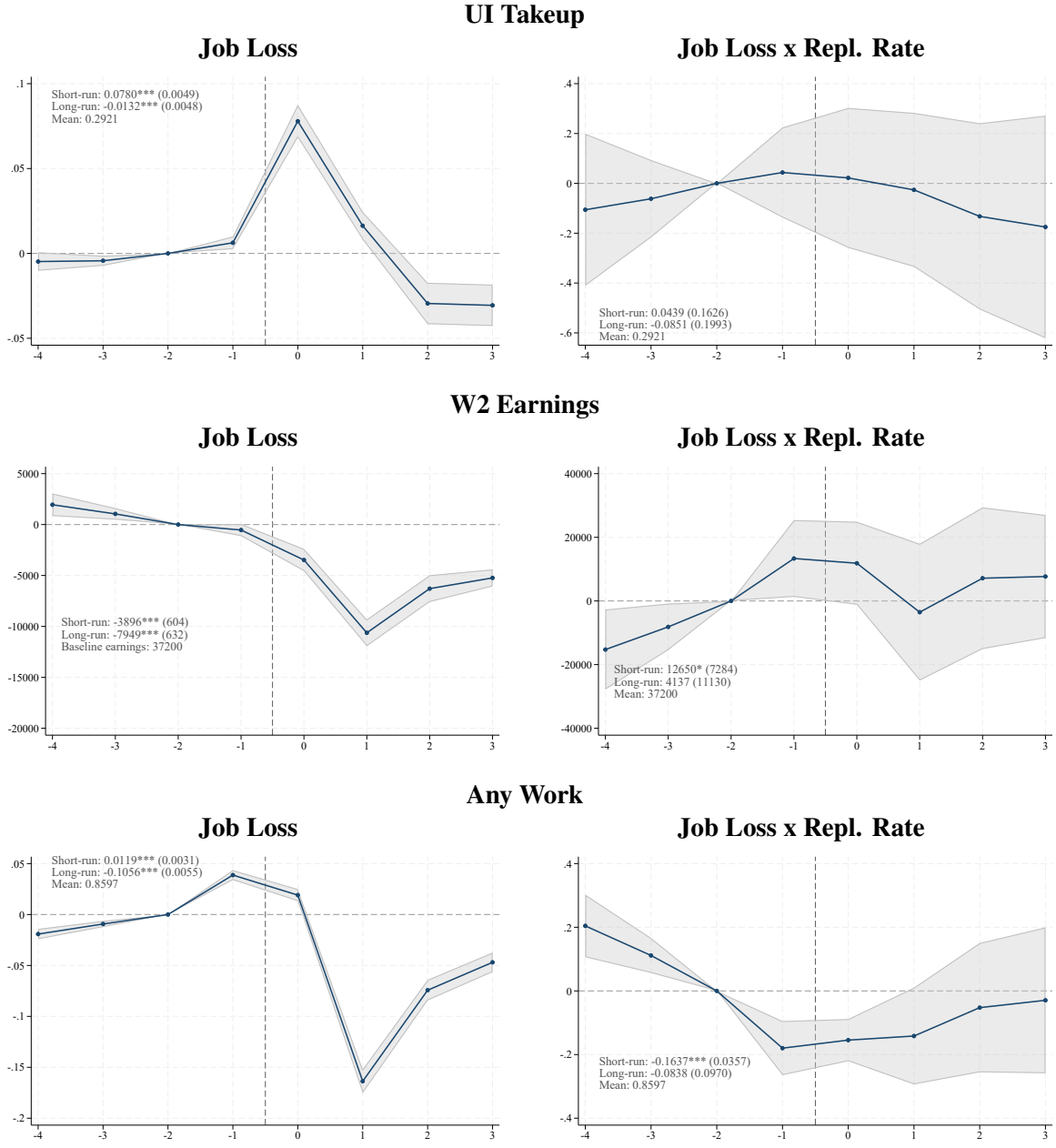
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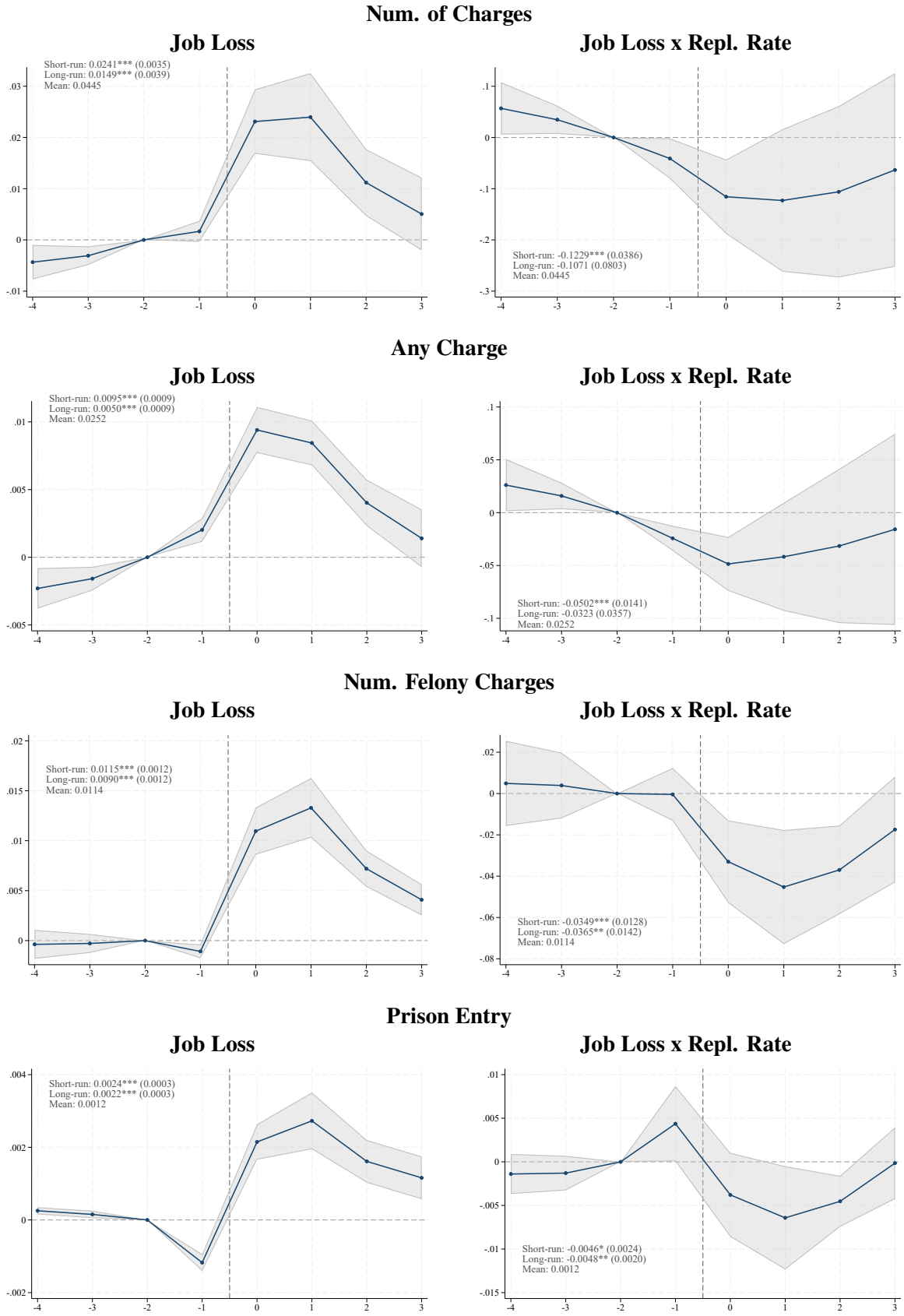
9 Figures

Figure 1: Event Studies: Labor Market Outcomes, Direct Effects & Replacement Rate Instrument



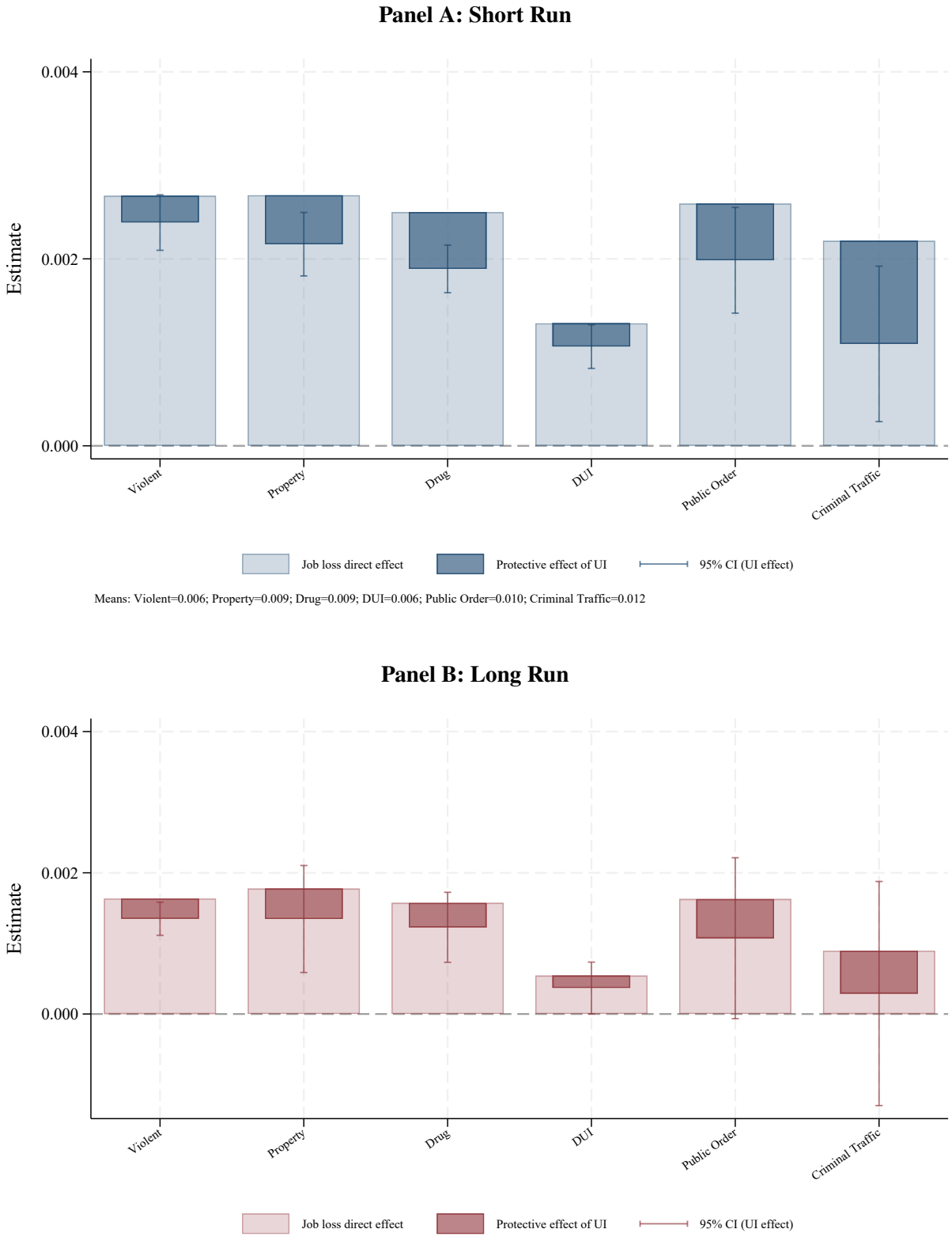
Notes: Each outcome is presented with its own header, followed by the two columns of event study estimates: the main effect of job loss (left) and the interaction with the simulated replacement rate (right). Approved under: CBDRB-FY26-0107.

Figure 2: Event Studies: Crime Outcomes, Direct Effects & Replacement Rate Instrument



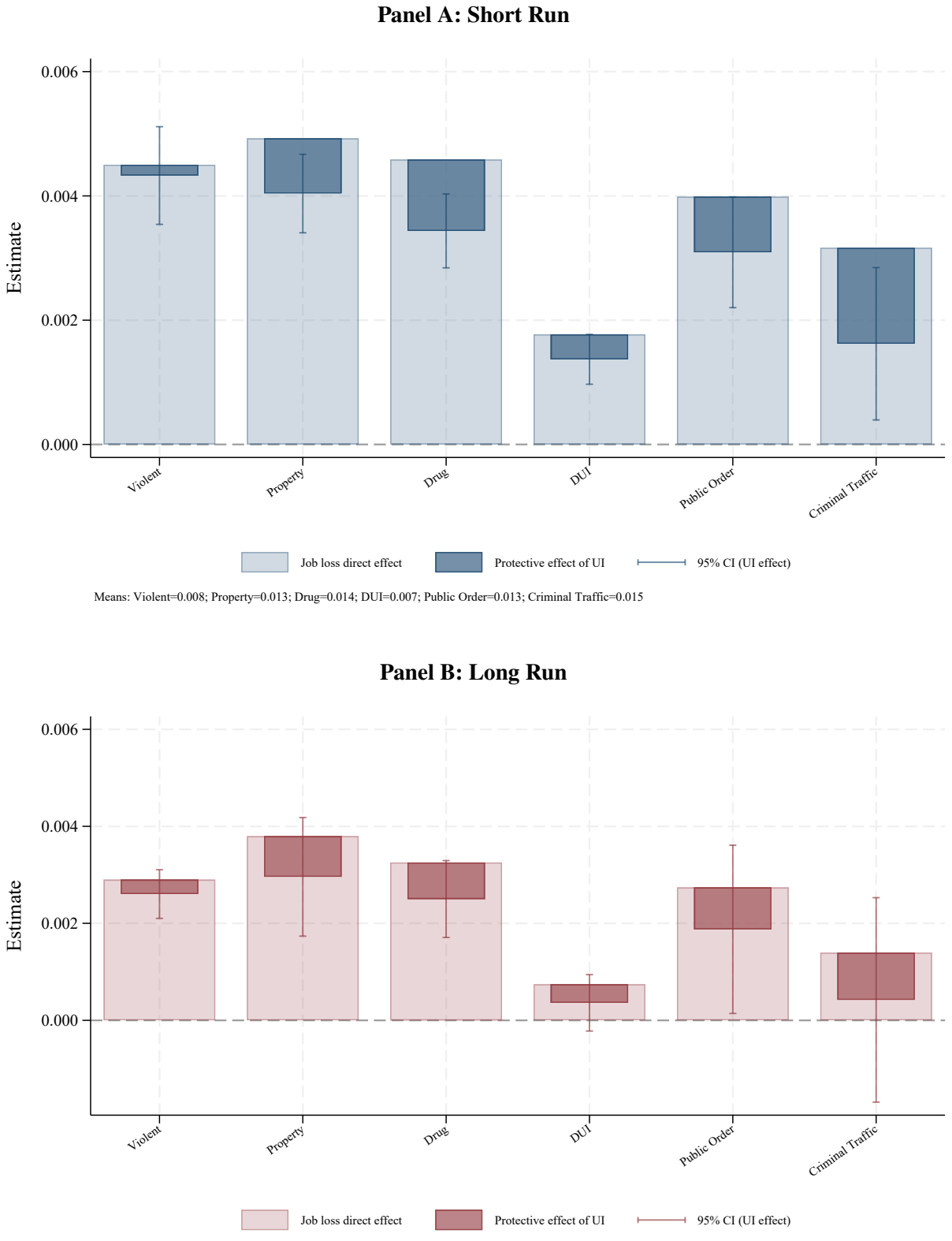
Notes: Each outcome is presented with its own header, followed by the two columns of event study estimates: the main effect of job loss (left) and the interaction with the simulated replacement rate (right). Approved under: CBDRB-FY26-0107.

Figure 3: Crime Type Breakdown, Extensive Margin: Short- and Long-Run Effects, Replacement Rate Instrument



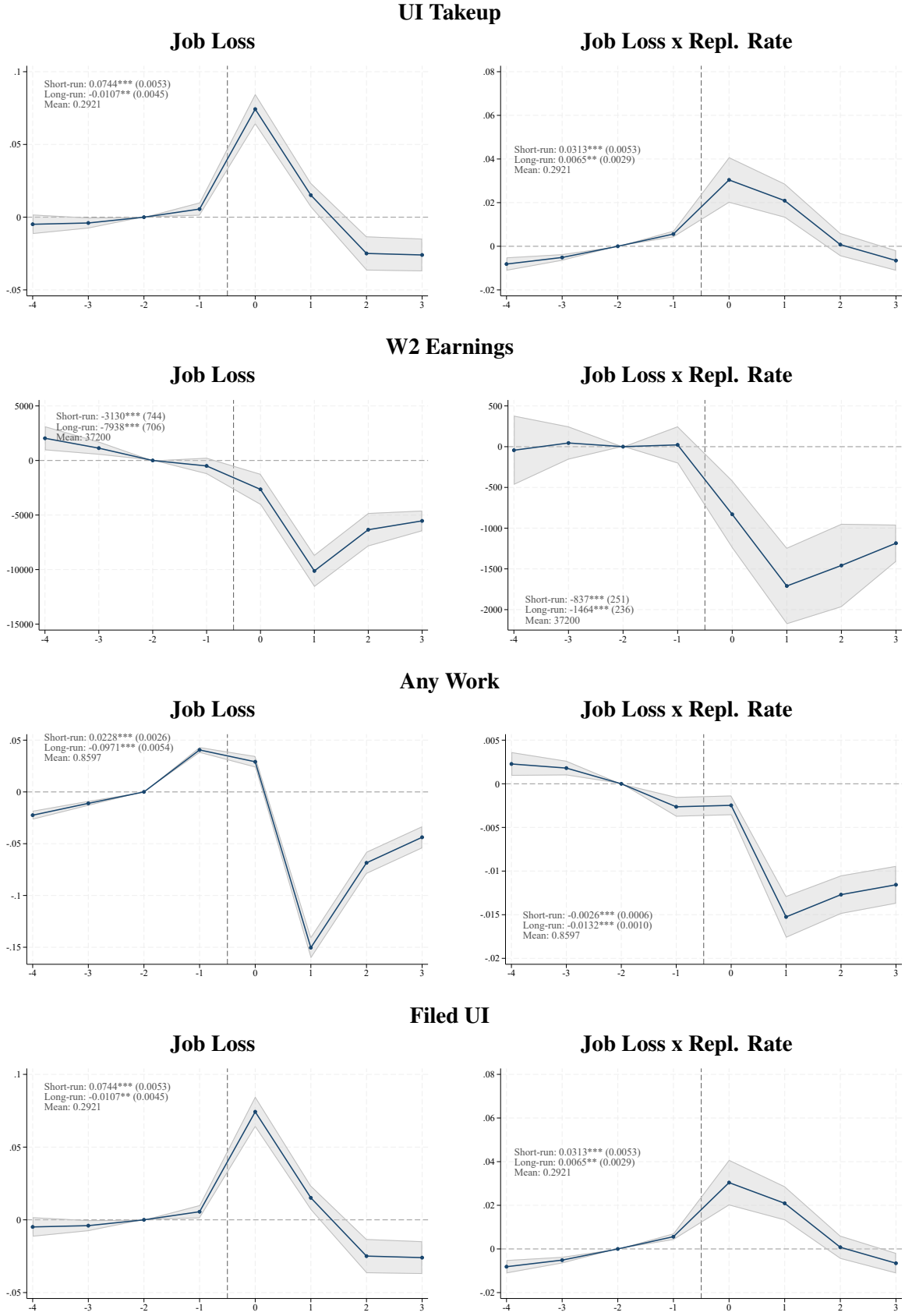
Notes: Figure displays DiD estimates of the effect of job loss and its interaction with the simulated replacement rate instrument on the probability of any charge (extensive margin), broken down by offense type (property, drug, and public order). Panel A shows short-run effects (year of job loss); Panel B shows long-run effects (years 1–4 following job loss). HAC standard errors are clustered at the state level. Approved under: CBDRB-FY26-0107.

Figure 4: Crime Type Breakdown, Intensive Margin: Short- and Long-Run Effects, Replacement Rate Instrument



Notes: Figure displays DiD estimates of the effect of job loss and its interaction with the simulated replacement rate instrument on the number of charges (intensive margin), broken down by offense type (property, drug, and public order). Panel A shows short-run effects (year of job loss); Panel B shows long-run effects (years 1–4 following job loss). HAC standard errors are clustered at the state level. Approved under: CBDRB-FY26-0107.

Figure 5: Event Studies: Labor Market Outcomes, Direct Effects & AKM Take-up Instrument



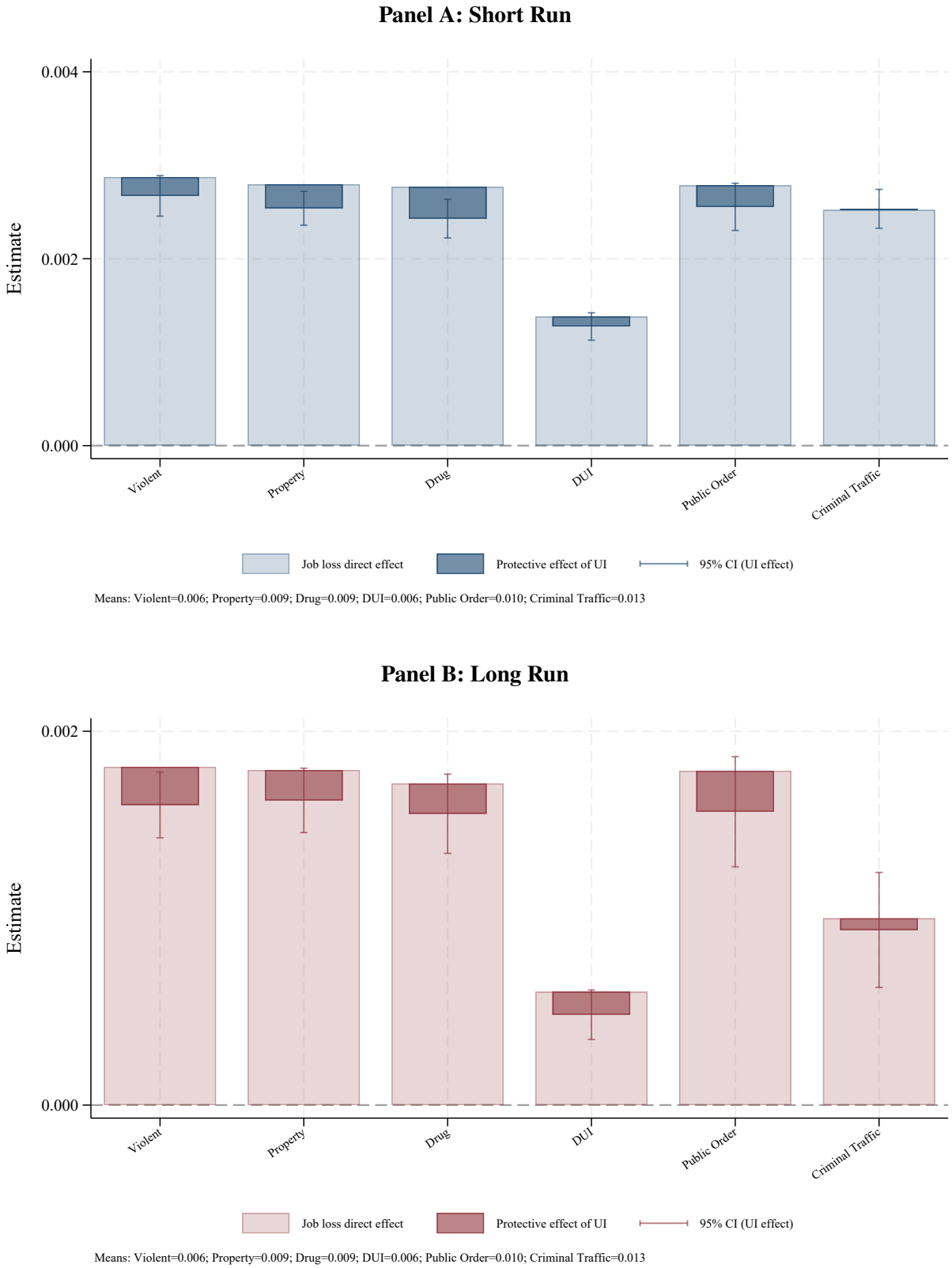
Notes: Each outcome is presented with its own header, followed by the two columns of event study estimates: the main effect of job loss (left) and the interaction with the AKM instrument (right). Approved under: CBDRB-FY26-0107.

Figure 6: Event Studies: Crime Outcomes, Direct Effects & AKM Take-up Instrument



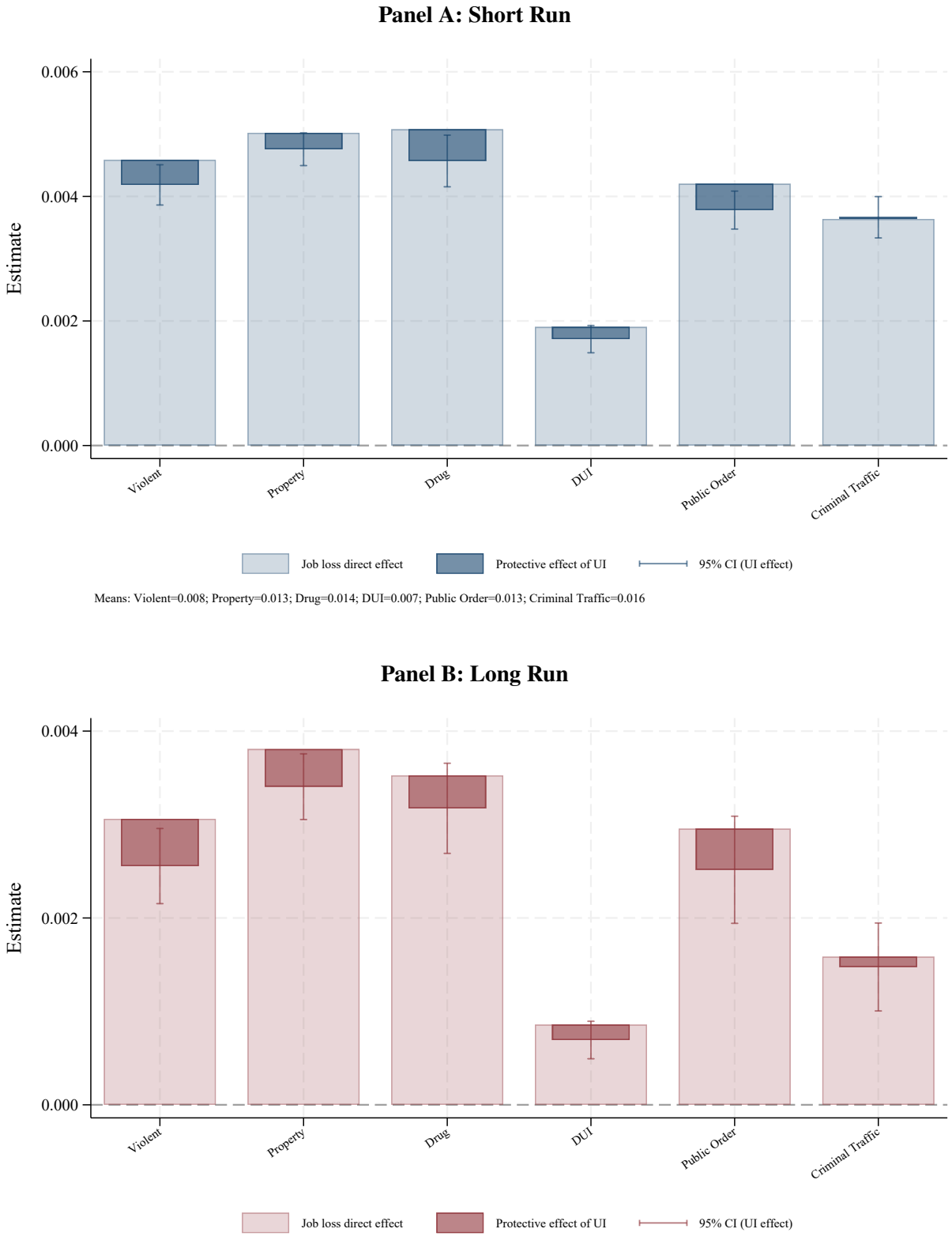
Notes: Each outcome is presented with its own header, followed by the two columns of event study estimates: the main effect of job loss (left) and the interaction with the AKM instrument (right). Approved under: CBDRB-FY26-0107.

Figure 7: Crime Type Breakdown, Extensive Margin: Short- and Long-Run Effects, AKM Access Instrument



Notes: Figure displays DiD estimates of the effect of job loss and its interaction with the AKM firm-level UI access instrument on the probability of any charge (extensive margin), broken down by offense type (property, drug, and public order). Panel A shows short-run effects (year of job loss); Panel B shows long-run effects (years 1–4 following job loss). HAC standard errors are clustered at the state level. Approved under: CBDRB-FY26-0107.

Figure 8: Crime Type Breakdown, Intensive Margin: Short- and Long-Run Effects, AKM Access Instrument



Notes: Figure displays DiD estimates of the effect of job loss and its interaction with the AKM firm-level UI access instrument on the number of charges (intensive margin), broken down by offense type (property, drug, and public order). Panel A shows short-run effects (year of job loss); Panel B shows long-run effects (years 1–4 following job loss). HAC standard errors are clustered at the state level. Approved under: CBDRB-FY26-0107.

Table 1: Summary Statistics

	Job-loss status	
	(1) Firm Separators	(2) Firm Stayers
Age	31.31	33.06
Marital status	1.376	1.477
Binary W-2 share	0.9223	0.9224
White	0.4613	0.5318
Black	0.1269	0.0877
Hispanic	0.2254	0.2078
Other Race	0.1864	0.1727
Children	0.6718	0.9250
W-2 total wages	39,180	54,230
Felony charges	0.0288	0.0122
Any felony charge	0.0170	0.0074
N	1,468,000	1,150,000

Notes: This table displays summary statistics split by job separators and those who remained at firms with mass layoffs. Approved und
0107.

Table 2: State DiD Design

	Outcome:								
	(1) Filed UI	(2) Employment	(3) W-2 Wages	(4) # Charges	(5) Any Charge	(6) # Felony Charges	(7) Any Felony Charge	(8) First Charge	(9) Prison Entry
β^{Post}	0.0319	-0.0090	-2,407	0.0434**	0.0219***	0.0132***	0.0067***	0.0079**	0.0012***
	(0.0596)	(0.0114)	(1,976)	(0.0173)	(0.0068)	(0.0050)	(0.0023)	(0.0032)	(0.0004)

Notes: This table reports reduced-form state-design estimates. Each column is a separate outcome regression with state and year fixed effects. HAC standard errors are clustered at the state level. *** $p < .01$, ** $p < .05$, * $p < .10$

Table 3: Individual Job Loss - Replacement Rate Instrument

	Outcome:							
	(1) UI Takeup	(2) Wages	(3) Employment	(4) # Charges	(5) Any Charge	(6) # Felony Charges	(7) Any Felony Charge	(8) Prison Entry
$\beta^{SR}[Job\ Loss_i \times Post_{it}^{SR}]$	0.07795***	-3,896***	0.01193***	0.02408***	0.00953***	0.01146***	0.00542***	0.00245***
	(0.00488)	(604)	(0.00306)	(0.00352)	(0.00094)	(0.00123)	(0.00064)	(0.00026)
$\beta^{LR}[Job\ Loss_i \times Post_{it}^{LR}]$	-0.01323***	-7,949***	-0.10560***	0.01495***	0.00495***	0.00900***	0.00379***	0.00218***
	(0.00483)	(632)	(0.00548)	(0.00391)	(0.00094)	(0.00121)	(0.00045)	(0.00034)
$\beta_Z^{SR}[Job\ Loss_i \times Post_{it}^{SR} \times Z_i]$	0.04392	12,650*	-0.16370***	-0.12290***	-0.05020***	-0.03493***	-0.01385***	-0.00462*
	(0.16260)	(7,284)	(0.03568)	(0.03863)	(0.01406)	(0.01281)	(0.00502)	(0.00243)
$\beta_Z^{LR}[Job\ Loss_i \times Post_{it}^{LR} \times Z_i]$	-0.08508	4,137	-0.08376	-0.10710	-0.03234	-0.03645**	-0.01033*	-0.00483**
	(0.19930)	(11,130)	(0.09697)	(0.08026)	(0.03567)	(0.01419)	(0.00584)	(0.00202)
Pre-Period Mean	0.29210	37,200	0.85970	0.04451	0.02517	0.01138	0.00681	0.00117
N	46,050,000	46,050,000	46,050,000	27,390,000	27,390,000	27,390,000	27,390,000	36,760,000

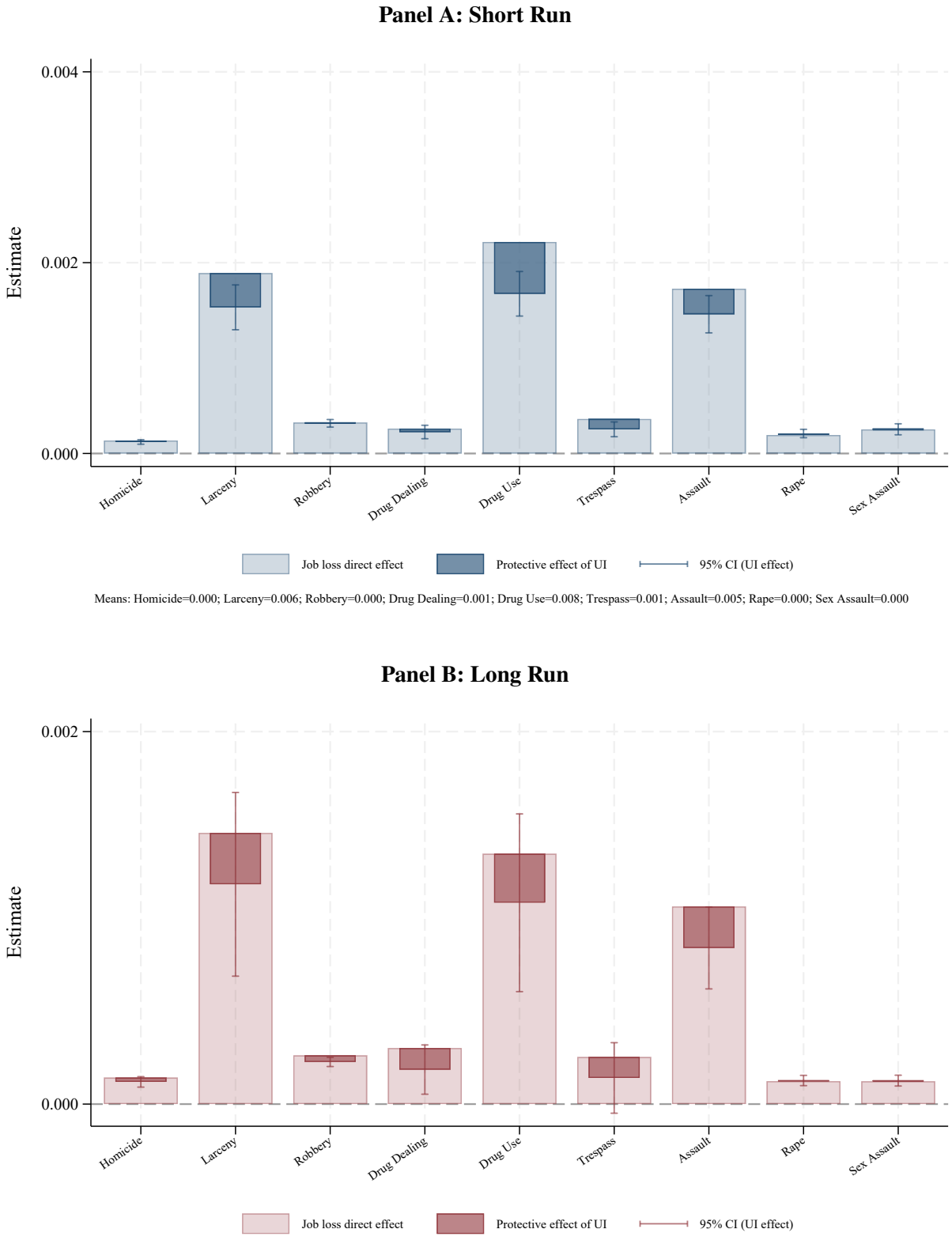
Notes: This table displays estimates from Equation 2. Each column represents the results from a separate regression. HAC standard errors are clustered at the state level. *** $p < .01$, ** $p < .05$, * $p < .10$. Approved under: CBDRB-FY26-0107.

Table 4: Individual Job Loss - Firm Uptake AKM Instrument

	Outcome:							
	(1) UI Takeup	(2) Wages	(3) Employment	(4) # Charges	(5) Any Charge	(6) # Felony Charges	(7) Any Felony Charge	(8) Prison Entry
$\beta^{SR}[Job\ Loss_i \times Post_{it}^{SR}]$	0.07439***	-3,130***	0.02278***	0.02560***	0.01030***	0.01190***	0.00567***	0.00249***
	(0.00529)	(744)	(0.00261)	(0.00359)	(0.00106)	(0.00142)	(0.00066)	(0.00024)
$\beta^{LR}[Job\ Loss_i \times Post_{it}^{LR}]$	-0.01073**	-7,938***	-0.09714***	0.01600***	0.00531***	0.00955***	0.00397***	0.00223***
	(0.00453)	(706)	(0.00536)	(0.00430)	(0.00113)	(0.00151)	(0.00050)	(0.00034)
$\beta_Z^{SR}[Job\ Loss_i \times Post_{it}^{SR} \times Z_i]$	0.03126***	-837***	-0.00256***	-0.00184***	-0.00092***	-0.00094***	-0.00059***	-0.00018***
	(0.00535)	(251)	(0.00056)	(0.00044)	(0.00022)	(0.00012)	(0.00007)	(0.00005)
$\beta_Z^{LR}[Job\ Loss_i \times Post_{it}^{LR} \times Z_i]$	0.00648**	-1,464***	-0.01320***	-0.00198**	-0.00066**	-0.00091***	-0.00039***	-0.00016***
	(0.00288)	(236)	(0.00102)	(0.00097)	(0.00032)	(0.00027)	(0.00008)	(0.00005)
Pre-Period Mean	0.29210	37,200	0.85970	0.04451	0.02517	0.01138	0.00681	0.00117
N	37,620,000	37,620,000	37,620,000	22,630,000	22,630,000	22,630,000	22,630,000	30,270,000

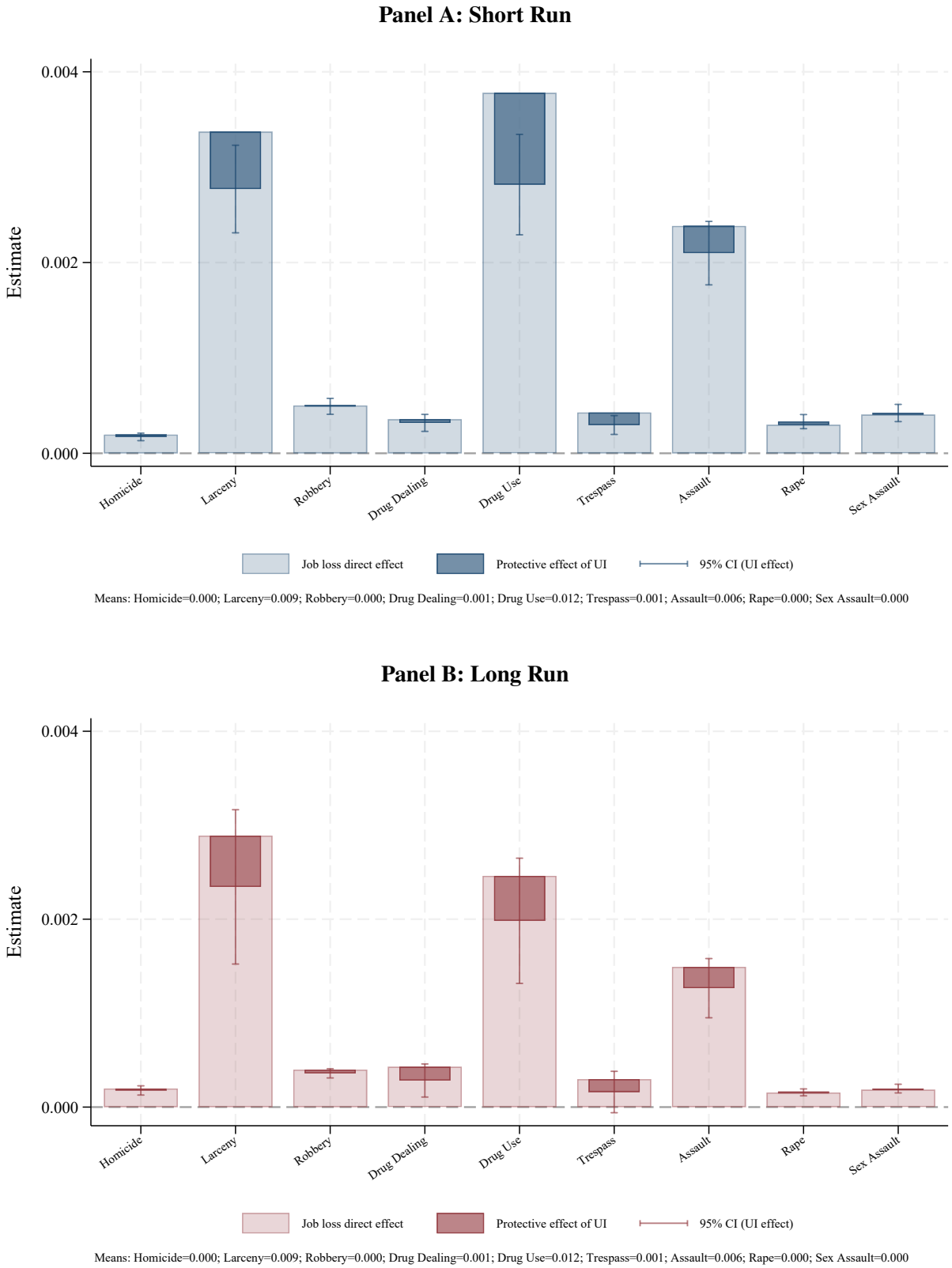
Notes: This table displays estimates from Equation 2. Each column represents the results from a separate regression. HAC standard errors are clustered at the state level. *** $p < .01$, ** $p < .05$, * $p < .10$. Approved under: CBDRB-FY26-0107.

Figure 9: Crime Type Breakdown, Detailed Extensive Margin: Short- and Long-Run Effects, Replacement Rate Instrument



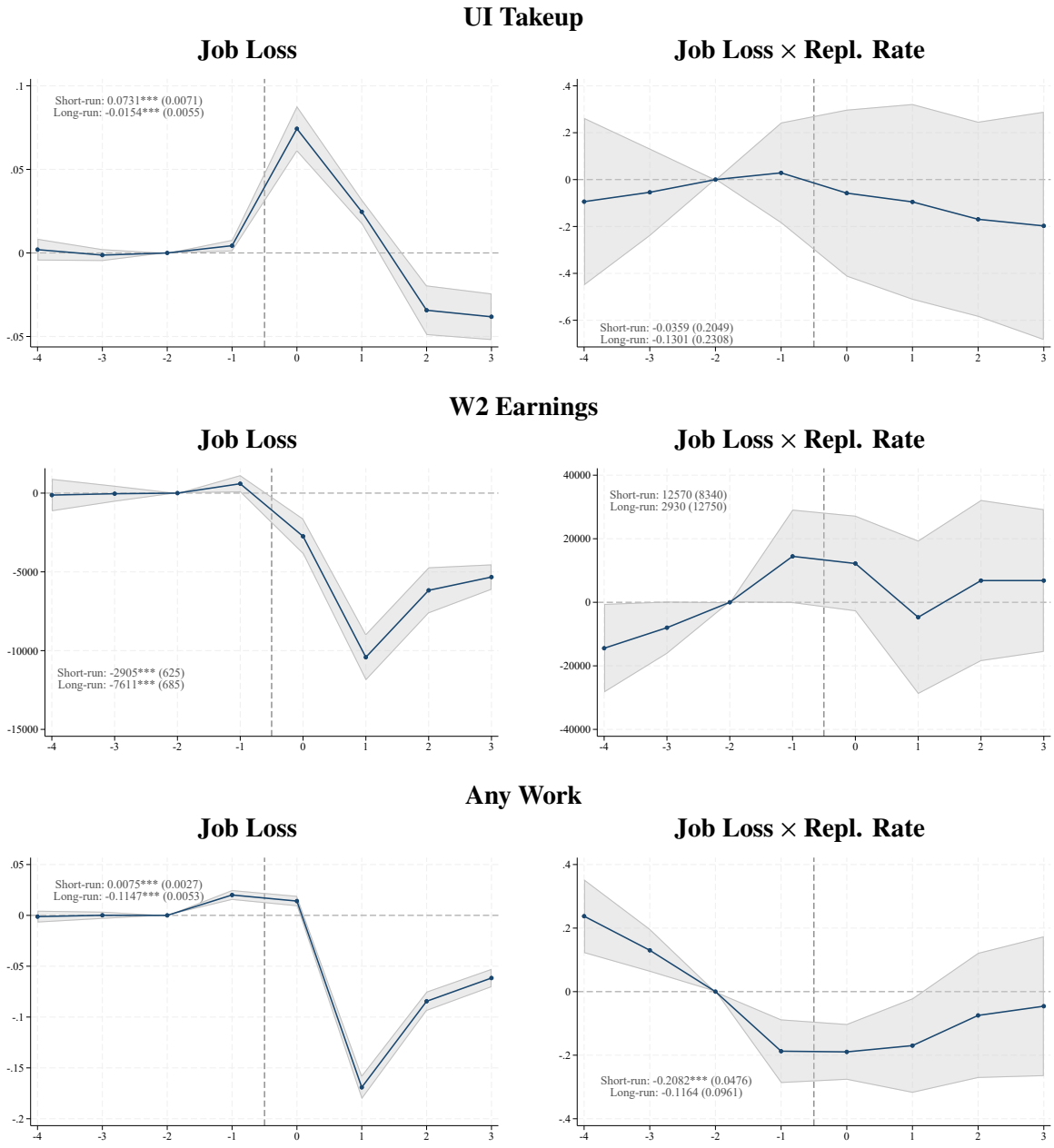
Notes: Figure displays DiD estimates of the effect of job loss and its interaction with the simulated replacement rate instrument on the probability of any charge (extensive margin), broken down by detailed offense sub-category (e.g., larceny, assault, drug possession, DUI). Panel A shows short-run effects (year of job loss); Panel B shows long-run effects (years 1–4 following job loss). HAC standard errors are clustered at the state level. Approved under:

Figure 10: Crime Type Breakdown, Detailed Intensive Margin: Short- and Long-Run Effects, Replacement Rate Instrument



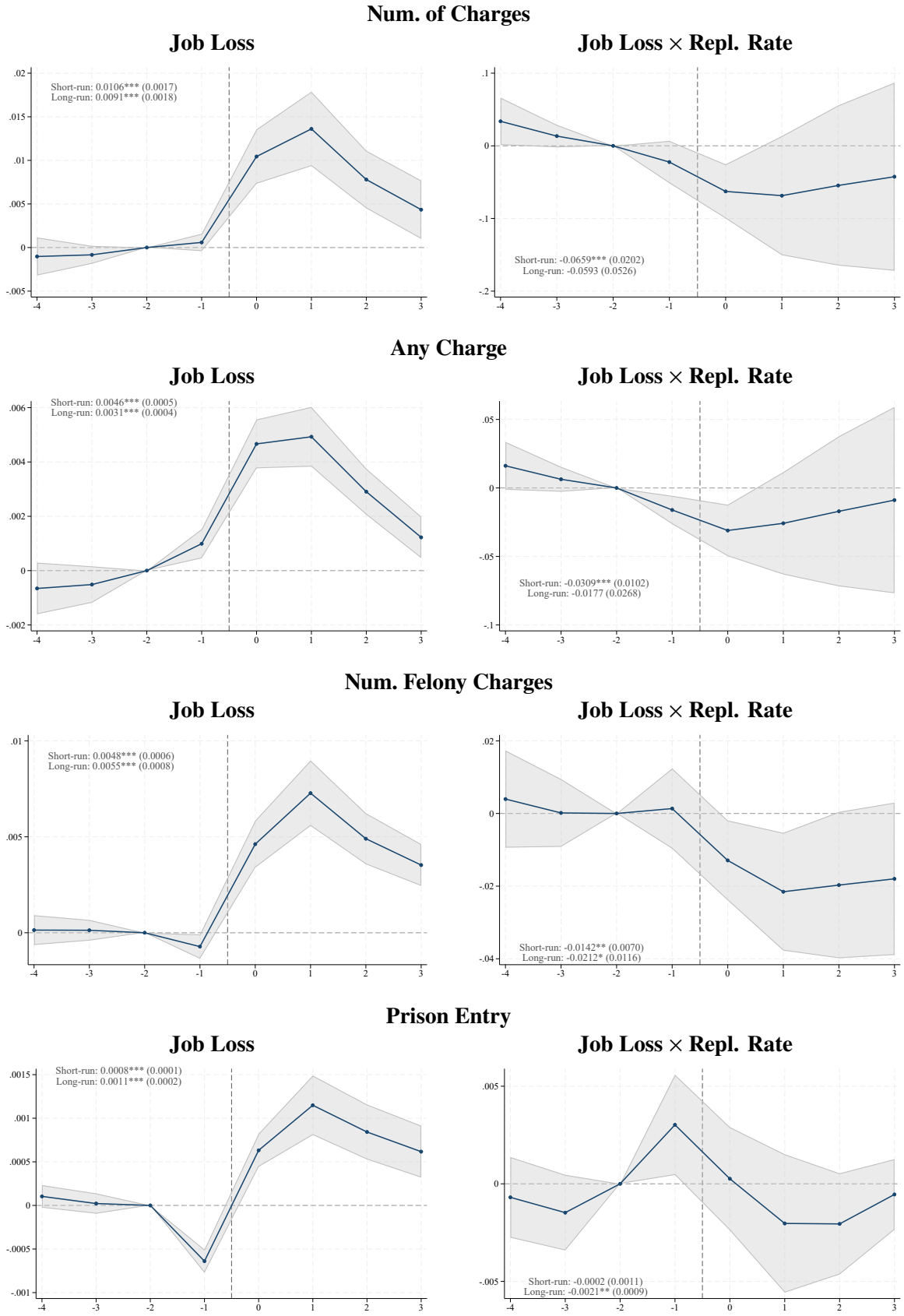
Notes: Figure displays DiD estimates of the effect of job loss and its interaction with the simulated replacement rate instrument on the number of charges (intensive margin), broken down by detailed offense sub-category. Panel A shows short-run effects (year of job loss); Panel B shows long-run effects (years 1–4 following job loss). HAC standard errors are clustered at the state level. Approved under: CBDRB-FY26-0107.

Figure 11: Event Studies: Labor Market Outcomes, Reweighted Sample, Replacement Rate Instrument



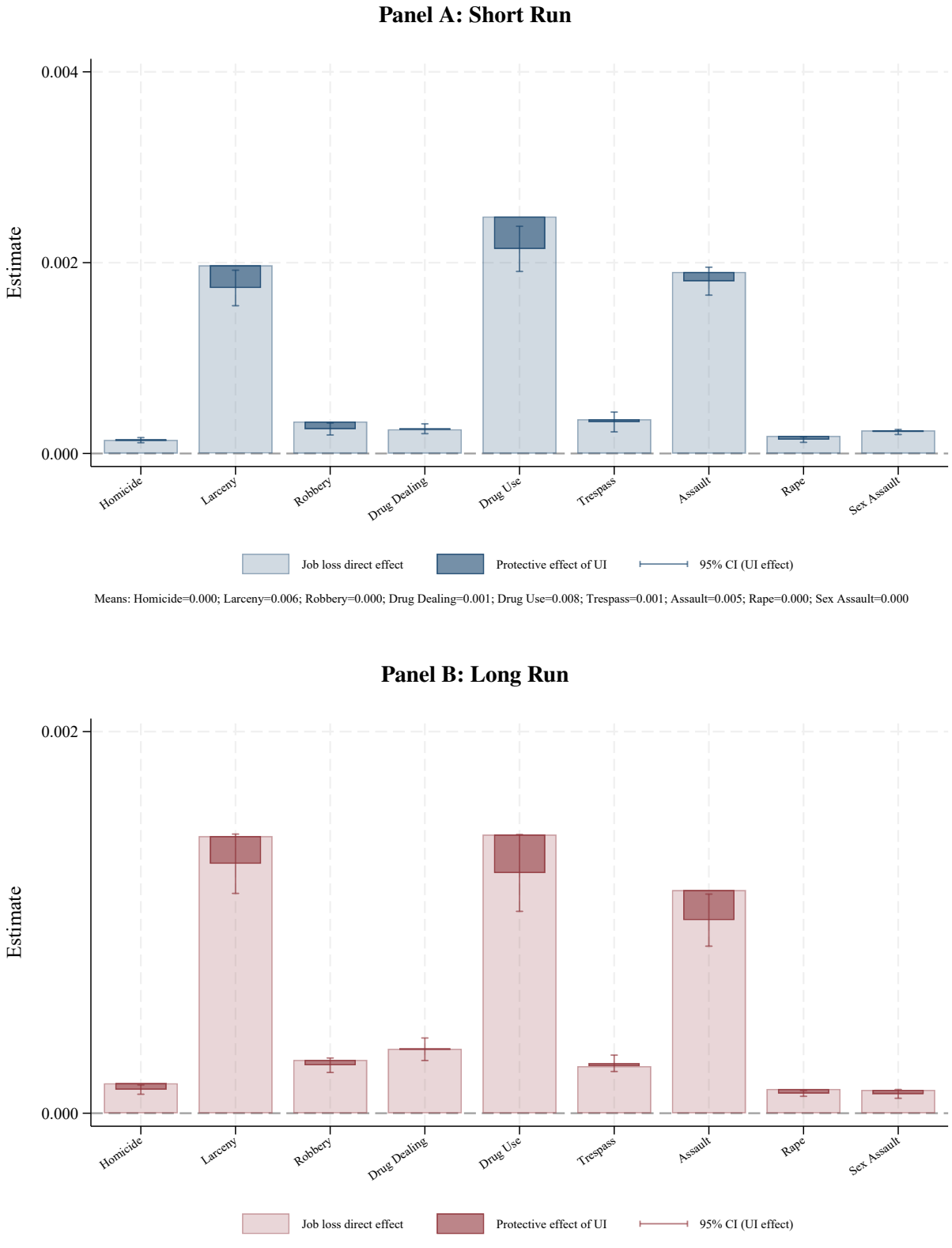
Notes: Replicates Figure 1 on a reweighted sample that balances pre-displacement earnings between job losers and stayers. Each row shows the direct effect of job loss (left) and its interaction with the simulated replacement rate instrument (right). HAC standard errors are clustered at the state level. Approved under: CBDRB-FY26-0107.

Figure 12: Event Studies: Crime Outcomes, Reweighted Sample, Replacement Rate Instrument



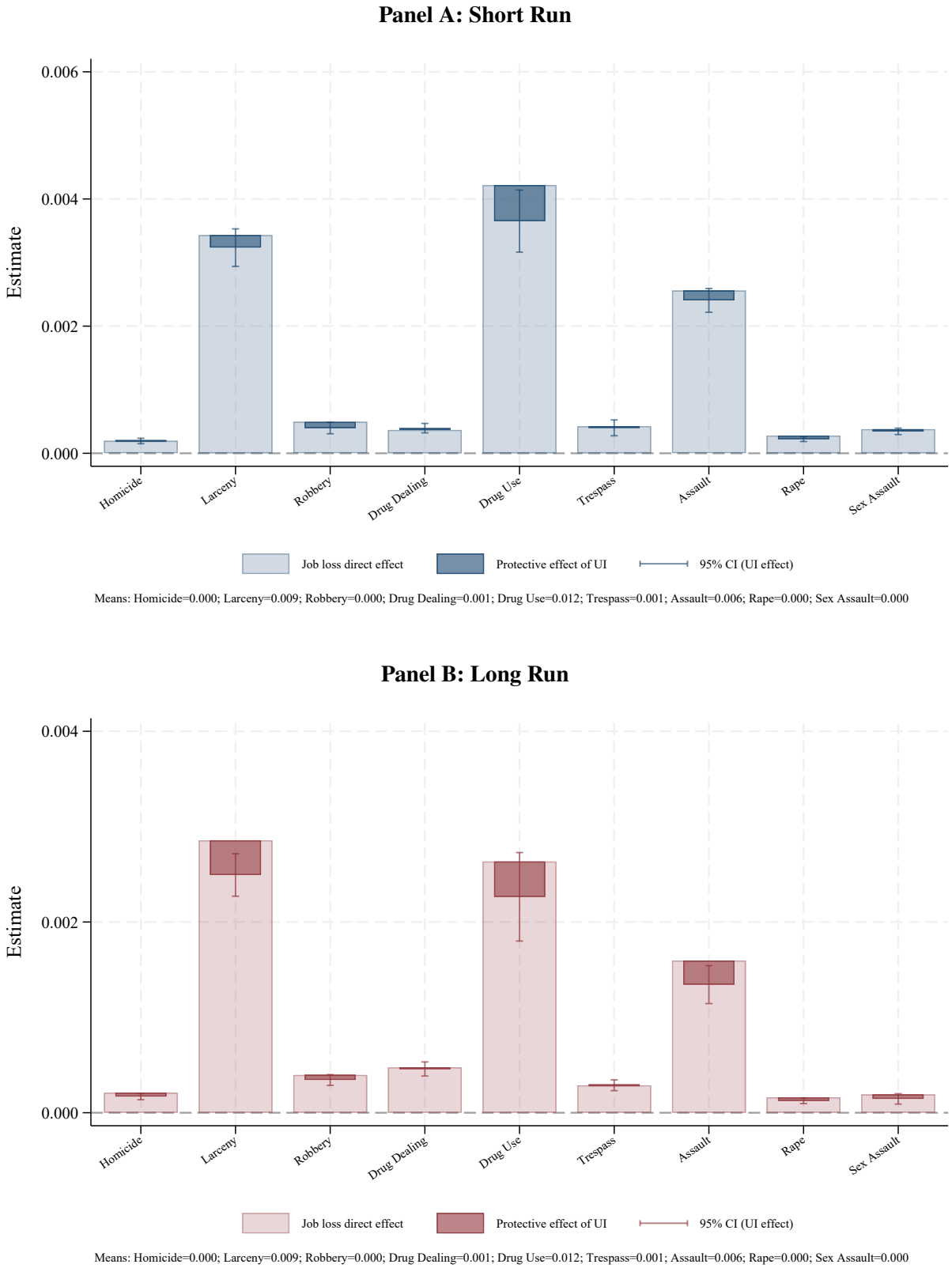
Notes: Replicates Figure 2 on a reweighted sample that balances pre-displacement earnings between job losers and stayers. Each row shows the direct effect of job loss (left) and its interaction with the simulated replacement rate instrument (right). HAC standard errors are clustered at the state level. Approved under: CBDRB-FY26-0107.

Figure 13: Crime Type Breakdown, Detailed Extensive Margin: Short- and Long-Run Effects, AKM Access Instrument



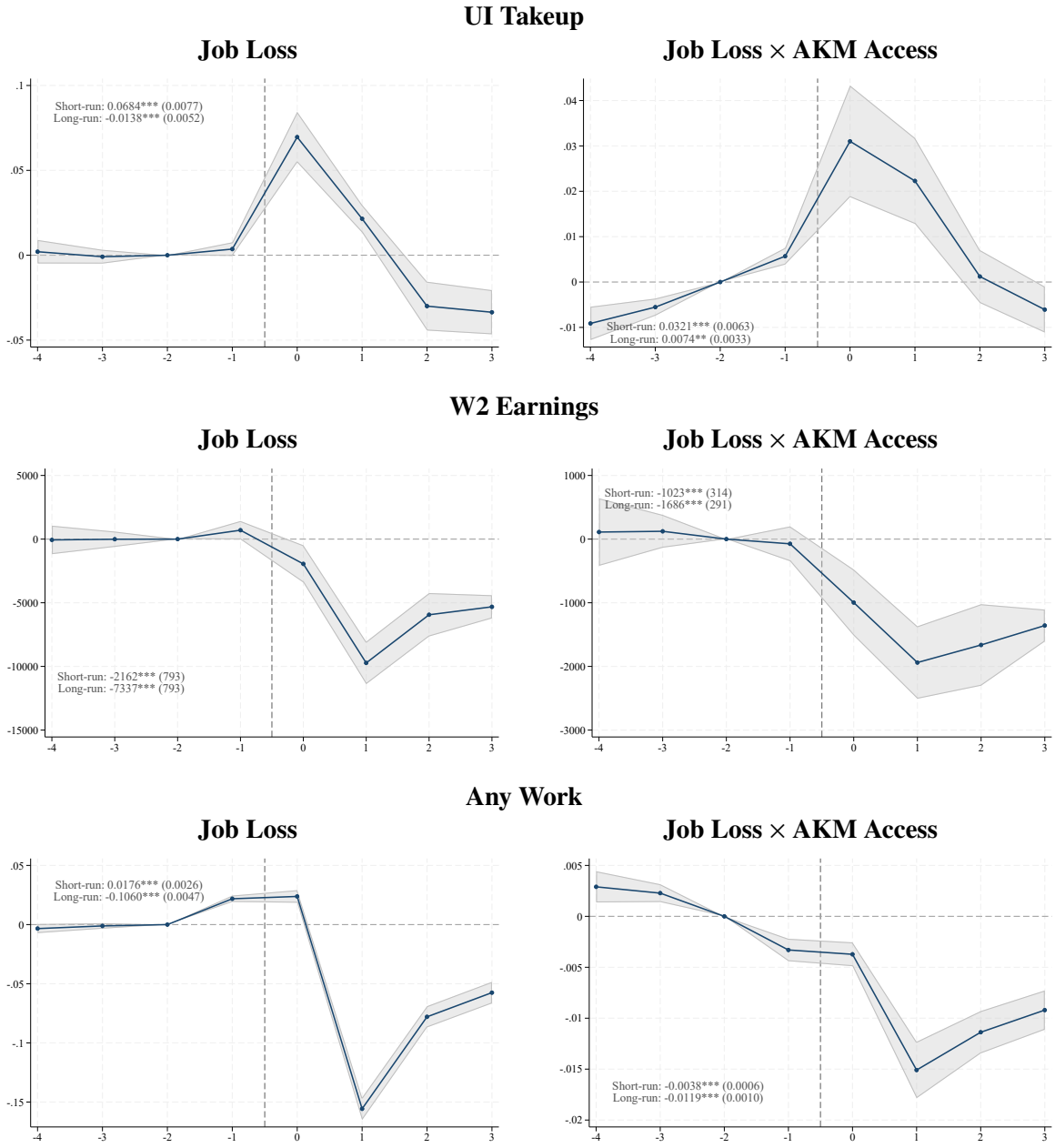
Notes: Figure displays DiD estimates of the effect of job loss and its interaction with the AKM firm-level UI access instrument on the probability of any charge (extensive margin), broken down by detailed offense sub-category. Panel A shows short-run effects (year of job loss); Panel B shows long-run effects (years 1–4 following job loss). HAC standard errors are clustered at the state level. Approved under: CBDRB-FY26-0107.

Figure 14: Crime Type Breakdown, Detailed Intensive Margin: Short- and Long-Run Effects, AKM Access Instrument



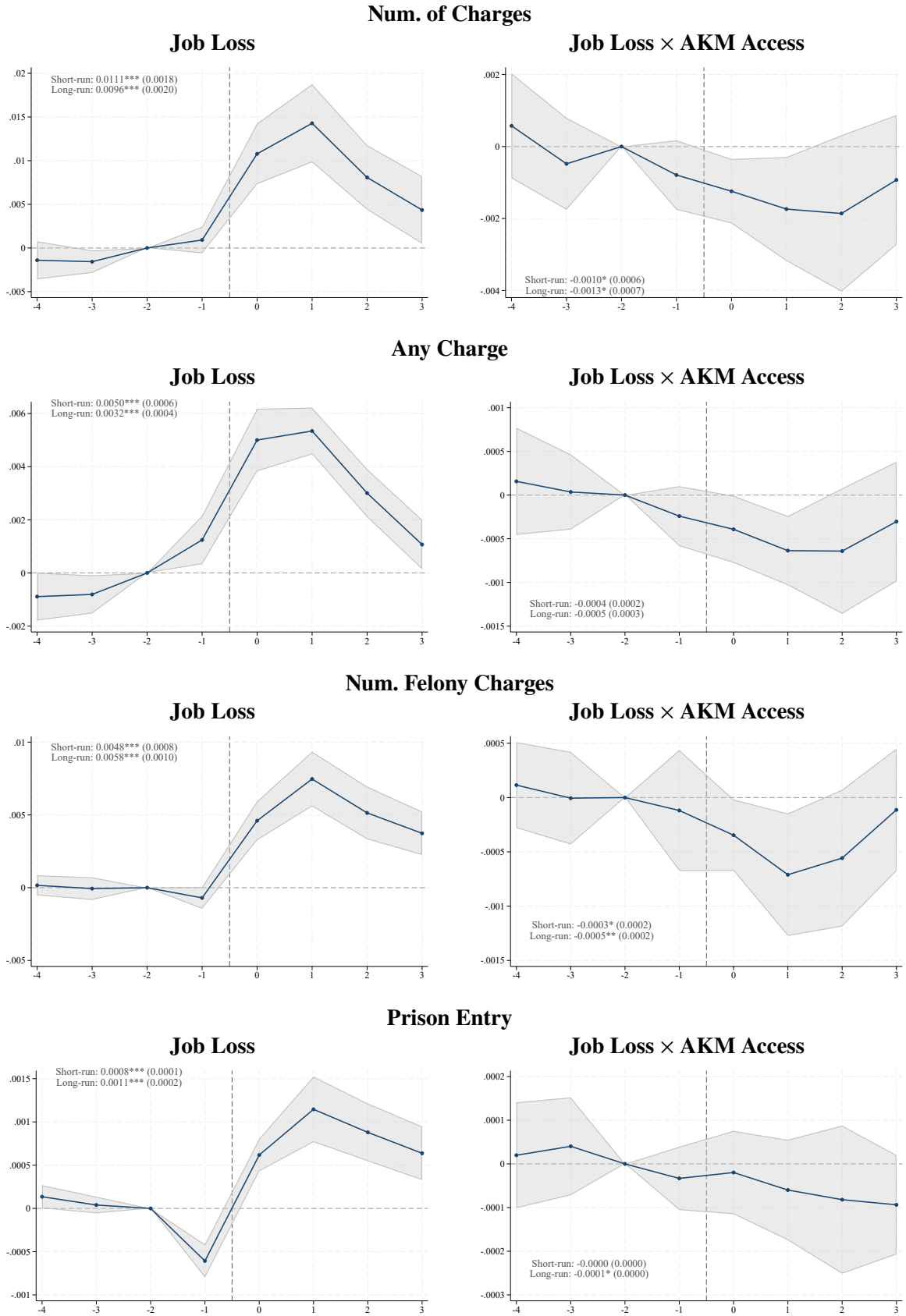
Notes: Figure displays DiD estimates of the effect of job loss and its interaction with the AKM firm-level UI access instrument on the number of charges (intensive margin), broken down by detailed offense sub-category. Panel A shows short-run effects (year of job loss); Panel B shows long-run effects (years 1–4 following job loss). HAC standard errors are clustered at the state level. Approved under: CBDRB-FY26-0107.

Figure 15: Event Studies: Labor Market Outcomes, Reweighted Sample, AKM Access Instrument



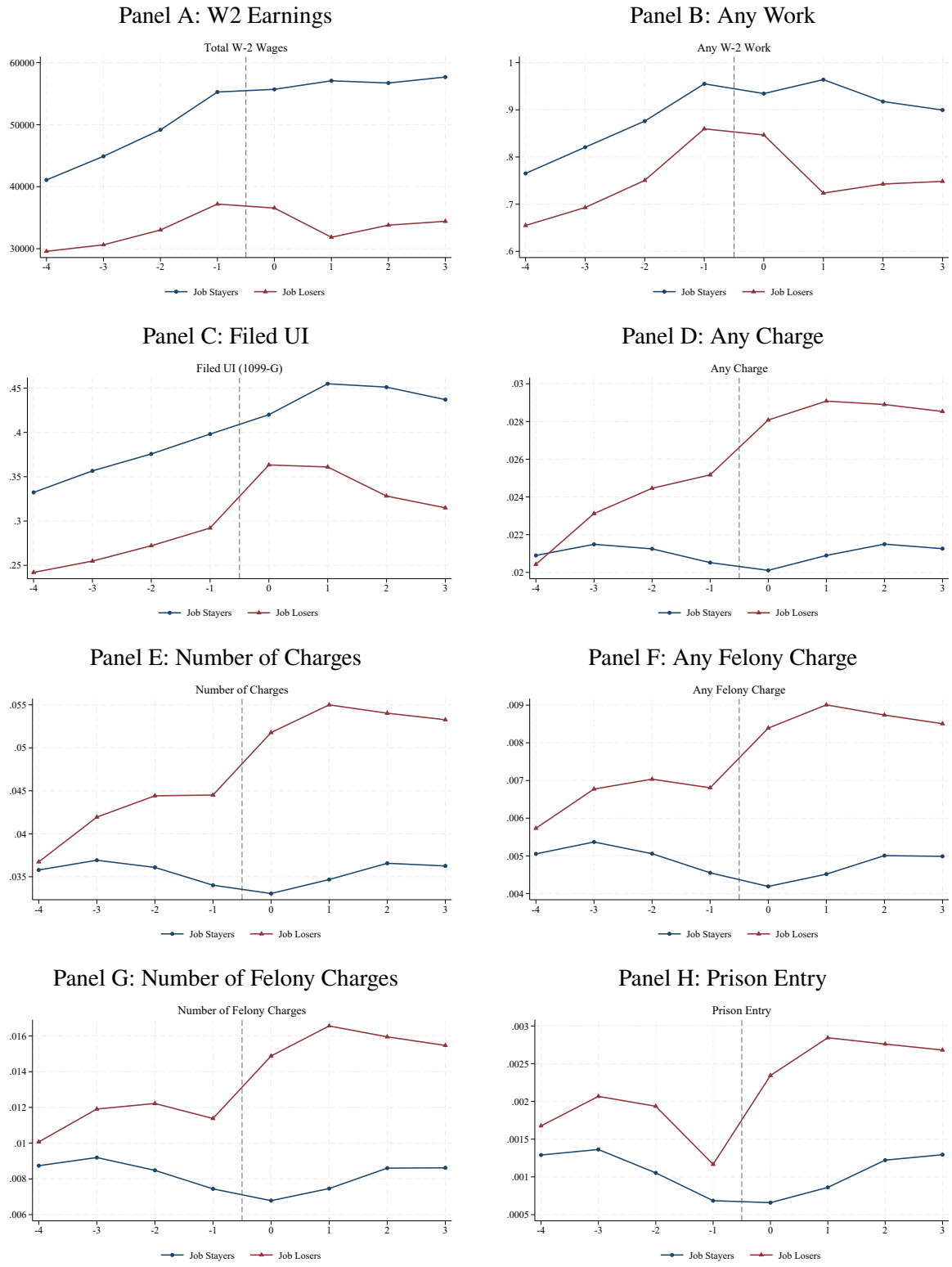
Notes: Replicates Figure 5 on a reweighted sample that balances pre-displacement earnings between job losers and stayers. Each row shows the direct effect of job loss (left) and its interaction with the AKM firm-level UI access instrument (right). HAC standard errors are clustered at the state level. Approved under: CBDRB-FY26-0107.

Figure 16: Event Studies: Crime Outcomes, Reweighted Sample, AKM Access Instrument



Notes: Replicates Figure 6 on a reweighted sample that balances pre-displacement earnings between job losers and stayers. Each row shows the direct effect of job loss (left) and its interaction with the AKM firm-level UI access instrument (right). HAC standard errors are clustered at the state level. Approved under: CBDRB-FY26-0107.

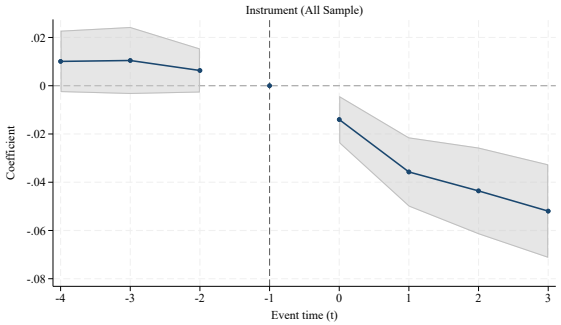
Figure 17: Raw data around mass layoffs for job losers and stayers



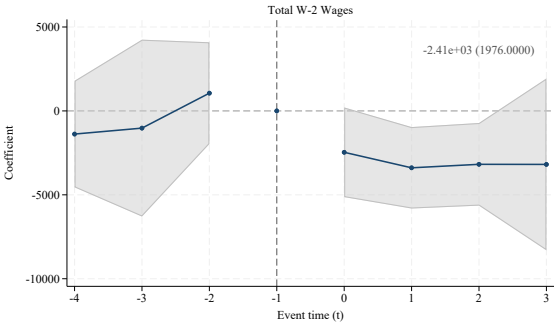
Notes: Each subfigure shows the average evolution of each characteristic across two sets of states (those that enacted a UI reform, in blue, and those that did not, in black). Approved under: CBDRB-FY26-0107.

Figure 18: Event studies around state UI policy reforms

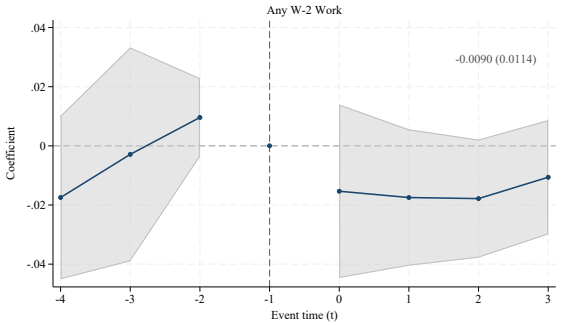
Panel A: UI Replacement Rates



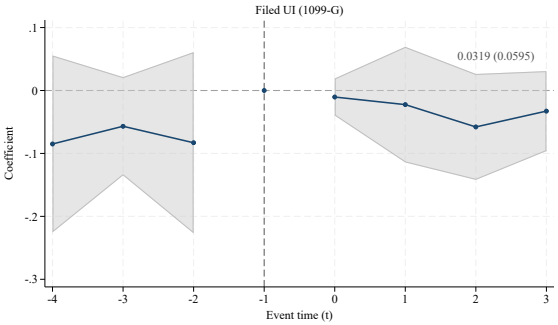
Panel B: W2 Earnings



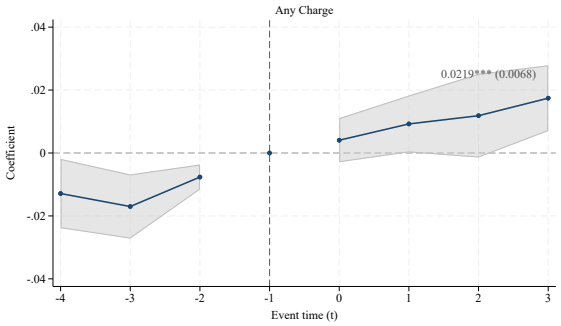
Panel C: Any Work



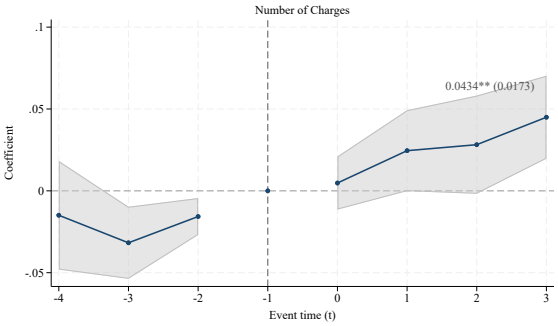
Panel D: Filed UI



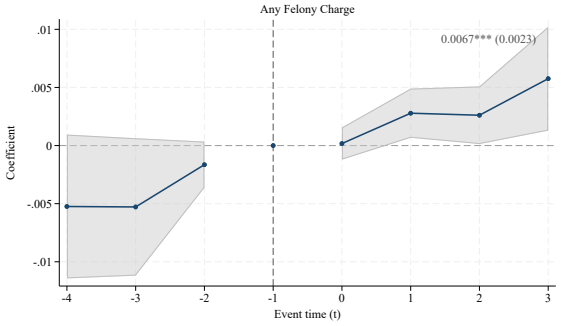
Panel E: Any Charge



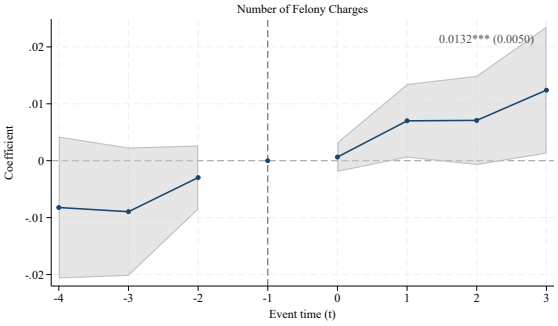
Panel F: Number of Charges



Panel G: Any Felony Charge



Panel H: Number of Felony Charges



Panel I: Prison Entry

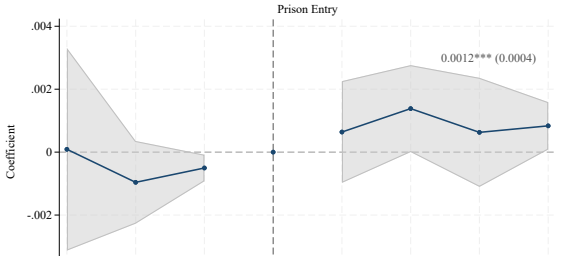


Table 5: State DiD - Heterogeneity

	Outcome:							
	(1) Filed UI	(2) Employment	(3) Wages	(4) # Charges	(5) Any Charge	(6) # Felony Charges	(7) Any Felony Charge	(8) Prison Entry
Panel A: Gender								
Women	0.0406	-0.0080	-1422	0.0200**	0.0107**	0.0026**	0.0014**	0.0001
	(0.0531)	(0.0099)	(1586)	(0.0078)	(0.0043)	(0.0012)	(0.0006)	(0.0001)
	[0.3106]	[0.7502]	[24680]	[0.0401]	[0.0239]	[0.0108]	[0.0062]	[0.0007]
Panel B: Marital Status								
Single	0.0587	-0.0063	-1791	0.0332**	0.0185***	0.0096**	0.0051***	0.0005
	(0.0645)	(0.0141)	(1184)	(0.0154)	(0.0056)	(0.0038)	(0.0019)	(0.0004)
	[0.4288]	[0.8061]	[27030]	[0.1164]	[0.0634]	[0.0348]	[0.0192]	[0.0036]
Married	0.0196	0.0007	-1955	0.0144***	0.0081***	0.0042***	0.0023***	0.0003
	(0.0605)	(0.0113)	(2712)	(0.0031)	(0.0019)	(0.0014)	(0.0008)	(0.0002)
	[0.4747]	[0.8798]	[62310]	[0.0350]	[0.0214]	[0.0098]	[0.0059]	[0.0010]
Panel C: W-2 Earnings Dependency								
Low W-2 Share	-0.0022	-0.0160**	-4244**	0.0396**	0.0195***	0.0108**	0.0053***	0.0005
	(0.0536)	(0.0078)	(1863)	(0.0163)	(0.0065)	(0.0049)	(0.0018)	(0.0006)
	[0.3789]	[0.7168]	[30560]	[0.0902]	[0.0463]	[0.0335]	[0.0177]	[0.0046]
High W-2 Share	0.0472	-0.0042	-2228	0.0357***	0.0187***	0.0106***	0.0055***	0.0009***
	(0.0652)	(0.0124)	(2152)	(0.0137)	(0.0057)	(0.0040)	(0.0019)	(0.0003)
	[0.4438]	[0.8200]	[39730]	[0.0994]	[0.0534]	[0.0321]	[0.0175]	[0.0041]
Panel D: Local Unemployment Rate								
Low Urate	0.1369*	0.0099	-554	0.0509***	0.0262***	0.0180**	0.0101***	0.0003
	(0.0767)	(0.0224)	(3648)	(0.0159)	(0.0074)	(0.0086)	(0.0038)	(0.0017)
	[0.3423]	[0.8114]	[39760]	[0.1101]	[0.0580]	[0.0404]	[0.0218]	[0.0058]
High Urate	0.0277	-0.0076	-1673	0.0495***	0.0223***	0.0133***	0.0064***	0.0012***
	(0.0545)	(0.0165)	(1818)	(0.0151)	(0.0072)	(0.0043)	(0.0022)	(0.0004)
	[0.4649]	[0.7422]	[31400]	[0.1303]	[0.0661]	[0.0430]	[0.0227]	[0.0057]

Table 5: State DiD - Heterogeneity (continued)

	Outcome:							
	(1) Filed UI	(2) Employment	(3) Wages	(4) # Charges	(5) Any Charge	(6) # Felony Charges	(7) Any Felony Charge	(8) Prison Entry
Panel E: Age								
Age 18–24	0.0515	-0.0143	-858	0.0428**	0.0205***	0.0140**	0.0071***	0.0017***
	(0.0490)	(0.0176)	(715)	(0.0194)	(0.0067)	(0.0064)	(0.0026)	(0.0004)
	[0.3058]	[0.6822]	[14030]	[0.1725]	[0.0858]	[0.0572]	[0.0293]	[0.0061]
Age 25–34	0.0236	-0.0079	-2462	0.0488***	0.0241***	0.0149***	0.0075***	0.0010**
	(0.0622)	(0.0125)	(1815)	(0.0155)	(0.0069)	(0.0051)	(0.0024)	(0.0004)
	[0.4177]	[0.8083]	[34930]	[0.1250]	[0.0653]	[0.0445]	[0.0239]	[0.0065]
Age 35–44	0.0268	-0.0010	-1925	0.0269*	0.0155***	0.0097**	0.0050***	0.0008
	(0.0643)	(0.0100)	(2559)	(0.0148)	(0.0058)	(0.0039)	(0.0018)	(0.0006)
	[0.4499]	[0.8185]	[53070]	[0.0762]	[0.0412]	[0.0275]	[0.0155]	[0.0047]
Age 45–54	0.0196	-0.0055	-796	0.0192**	0.0117***	0.0084***	0.0047***	0.0004
	(0.0625)	(0.0100)	(3353)	(0.0078)	(0.0038)	(0.0028)	(0.0015)	(0.0008)
	[0.4517]	[0.8254]	[62570]	[0.0459]	[0.0258]	[0.0158]	[0.0093]	[0.0030]
Panel F: Race								
White	0.0181	-0.0026	-1895	0.0353***	0.0180***	0.0102***	0.0050***	0.0008**
	(0.0480)	(0.0131)	(2307)	(0.0084)	(0.0032)	(0.0033)	(0.0015)	(0.0003)
	[0.4549]	[0.8104]	[43350]	[0.1006]	[0.0515]	[0.0342]	[0.0178]	[0.0047]
Black	0.0474	-0.0247**	-1632	0.0487	0.0337	0.0207	0.0133**	0.0017
	(0.0987)	(0.0123)	(1713)	(0.0663)	(0.0248)	(0.0126)	(0.0061)	(0.0013)
	[0.3675]	[0.6995]	[21570]	[0.2355]	[0.1139]	[0.0895]	[0.0474]	[0.0128]
Hispanic	0.0638	-0.0111	-2316*	0.0617***	0.0290***	0.0148**	0.0063*	0.0026***
	(0.0584)	(0.0083)	(1302)	(0.0157)	(0.0066)	(0.0066)	(0.0033)	(0.0007)
	[0.3383]	[0.7810]	[27460]	[0.1172]	[0.0662]	[0.0409]	[0.0237]	[0.0072]

Notes: This table displays estimates (β^{Post}) from $Y_{it} = \beta^{Post} + \sigma_i + \delta_t + \varepsilon_{it}$. Each cell represents the results from a separate regression. Pre-period group means are shown in brackets. HAC standard errors are clustered at the state level. *** $p < .01$, ** $p < .05$, * $p < .10$. Approved under: CBDRB-FY26-0107.

Table 6: AKM Firm-Effect Validity

Panel A: Variance of Firm-Level UI Access Effects

	Value
Sample A (Main) variance	0.0546
Sample B (IV) variance	0.0589
Bias-corrected variance	0.0069

Panel B: Validity Tests — Firm Effect Predicts UI Take-Up

	Full Sample			Men, Ages 18–45		
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	IV	OLS	OLS	IV
Change in firm effect	1.002***	1.002***	1.011***	1.002***	1.002***	1.004***
	(0.00115)	(0.00115)	(0.01035)	(0.00154)	(0.00154)	(0.01347)
Change in firm effect^2		−0.0009			−0.0019*	
		(0.00076)			(0.00103)	
N (thousands)	2,619	2,619	2,619	1,459	1,459	1,459

Notes: Panel A reports variance statistics for the split-sample AKM firm-effect estimates of UI take-up propensity. The bias-corrected variance is the covariance between the Sample A and Sample B estimates, which corrects for attenuation bias arising from measurement error in a single-sample estimator. Panel B tests whether the estimated firm effects predict observed UI take-up following job loss. The dependent variable is an indicator for filing UI. Columns 1 and 4 regress take-up on the change in firm effect (from pre- to post-displacement employer), controlling for individual fixed effects. Columns 2 and 5 augment the specification with the squared firm effect to test for nonlinearity in the take-up relationship. Columns 3 and 6 instrument for the firm effect using the split-sample IV approach to address classical measurement error in the AKM estimates. Columns 1–3 use the full estimation sample; columns 4–6 restrict to men aged 18–45, the primary subsample in the main DiD analysis. Standard errors in parentheses. *** $p < .01$, * $p < .10$. Approved under: CBDRB-FY26-0107.