Parental Job Loss and Geographic Mobility *

Andrew Joung†
University of Michigan

James Reeves
University of Colorado Denver

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Abstract

Using restricted Census microdata, we examine the role of location and mobility in the intergenerational transmission of parental job loss on children's long-run human capital. Following job loss, children experience a permanent rise in commuting zone outmigration. Without accounting for the location of the job loss, we find that this mobility leads children to worse neighborhoods with insignificant changes to college attendance. However, this masks sharply divergent impacts between families across different labor markets. Exploiting labor market scarring from the Great Recession, we show that children whose parents lost jobs in scarred markets experience greater outmigration, improved neighborhood quality, and increased college attendance.

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†*Job Market Paper*. Email: ajoung@umich.edu

1 Introduction

From 2001 to 2017, roughly 1.5 million U.S. workers annually lost a long-held job with nearly half being parents.[1] Such job losses generate well-documented long-lasting reductions in earnings and employment (Jacobson, LaLonde, and Sullivan 1993; Couch and Placzek 2010; Davis and von Wachter 2011; Hilger 2016). Consequently, most studies on the intergenerational consequences of parental job loss have emphasized the role of family income (Hilger 2016; Oreopoulos, Page, and Stevens 2008; Uguccioni 2021).[2] Far less attention has been given to how geographic mobility among displaced workers affects children, despite consistent evidence of increasing geographic mobility among adult displaced workers (Huttunen, Møen, and Salvanes 2018; Mörk, Sjögren, and Svaleryd 2020; Fackler and Rippe 2017; Dahlberg, Martén, and Öckert 2024; Gathmann, Helm, and Schönberg 2020) and a large literature showing where children live is a first-order determinant of their long-run outcomes (Chetty and Hendren 2018; Chetty, Hendren, and Katz 2016; Bergman et al. 2024; Aloni and Avivi 2025; Chyn 2018; Chyn and Katz 2021; Deryugina, Kawano, and Levitt 2018).

In this paper, we study parental job loss' effect on children's geographic mobility, neighborhood quality, and long-run human capital accumulation. To implement this study, we link administrative and survey data from the U.S. Census Bureau to identify childhood exposure to parental job loss during mass layoff events from 2006 to 2016, track residential histories, and measure early-adult outcomes. Leveraging variation in layoff timing, we compare children of displaced ("job losers") versus non-displaced ("survivors") workers and find significant mobility responses. In the first year after parental job loss, children observe a rise in the probability of leaving their origin commuting zones by 3.2 percentage points, roughly double the baseline mobility rate. These large mobility responses coincide with declines in average neighborhood quality (e.g., poverty rates, educational attainment). Importantly, our event studies detect no significant pre-period differences in neighborhood quality,

[1] Authors' calculation using the Current Population Survey's Displaced Worker Supplement. A long-held job is one which was held for 3 or more year prior to job loss.

[2] A related literature considers the role of the social safety net in mitigating parental job loss through partial income replacement (Aloni and Avivi 2024; Britto, Pinotti, and Sampaio 2022). An exception is Rege, Telle, and Votruba (2011) who emphasize mental distress following job loss. There is also a rich literature studying the propagation of other familial shocks to the next generation, including within-family deaths and health shocks (Adda, Bjorklund, and Holmlund 2011; Persson and Rossin-Slater 2018; Aaskoven, Kjær, and Gyrd-Hansen 2022) and changes to safety net policy (East et al. 2023; Mueller-Smith et al. 2024) - see Almond, Currie, and Duque (2018) for a review.

suggesting that selection into better neighborhoods is unlikely to drive our results.

However, this average effect may mask sharply divergent mobility patterns across families in different labor markets. Families in high-quality location may experience relatively little benefit, or in fact, harms from job loss-induced mobility, as they face limited scope for quality-enhancing moves. In contrast, families in low quality locations face greater potential gains from leaving bad labor markets. We address this by exploiting variation in local labor market scarring following the Great Recession, measured as the change in the commuting zone-level employment-to-population ratio from 2007 to 2016. Over our sample period, the U.S. labor market observed a long post-recovery period following the Great Recession, with some commuting zones stuck in prolonged downturns and others rebounding quickly (Hershbein and Stuart 2022; Yagan 2019; Finkelstein et al. 2023).

Children exposed to job loss in scarred labor markets show markedly different changes in location quality. For each percentage point decline in the employment-to-population ratio between 2007-2016 in their pre-shock commuting zone, job-losing families experience an additional 0.2 percentage point (6.9%) of commuting zone outmigration. Crucially, these moves from scarred origins yield improvements in neighborhood quality. Among job loss-induced movers, we find each percentage point of labor market scarring leads to a 1 percentage point reduction in destination scarring, a 0.8 percentage point (2.6%) increase in the college-educated share, and a 0.1 percentage point (0.9%) decrease in poverty rates. Pre-displacement differences remain small and statistically insignificant across all scarring levels, supporting a causal interpretation of these heterogeneous effects.

Does outmigration from scarred labor markets improve children's long-run outcomes? Following Hilger (2016), we employ a cross-cohort differences-in-differences design exploiting variation in age at exposure to parental job loss and comparing children exposed to parental job loss to a control group of children with parents who "survived" job loss. Consistent with prior work, we find that on average parental job loss produces small, statistically insignificant effects on college attendance. In contrast, we find that childhood exposure to parental job loss in more scarred labor markets raises college attendance from ages 19-22 by an additional 1.2 - 1.6 percentage points for each additional percentage point in our scarring measure.

Taken together, our results show that parental job loss increases children's college attendance in scarred labor markets, where job loss triggers the highest outmigration rates and where families gain the most from leaving. While suggestive of the importance of mobility, these results do not establish a causal link between job loss-induced outmigration and rising college attendance. It is plausible that much of these effects are driven by non-movers in scarred labor markets, who respond, for example, to changing expectations of labor market volatility. To overcome this empirical challenge, we build on the "movers' design" strategy from Chetty and Hendren (2018). We compare children who leave similarly scarred labor markets shortly after parental job loss, but do so at different ages. If selection into mobility is constant by age, then, by comparing early to late movers, we identify the effect of outmigration from a 1 percentage point more scarred labor market. Our estimates imply that, for every 1 percentage point in our measure of labor market scarring, outmigration leads to a 3.1 percentage point rise in college attendance, confirming that mobility and outmigration from scarred labor markets is a key driver for children's long-run human capital accumulation.

Our paper contributes to several literatures. First, our findings speak directly to the literature on the intergenerational consequences of parental job loss. Despite evidence of increased geographic mobility among adult displaced workers (Huttunen, Møen, and Salvanes 2018; Gathmann, Helm, and Schönberg 2020), and extensive evidence that place matters for children's outcomes (Chetty and Hendren 2018; Bergman et al. 2024), prior studies have largely overlooked the role of geographic mobility following parental job loss. Instead, researchers have focused primarily on lost family income and its replacement via the social safety net (Hilger 2016; Aloni and Avivi 2024; Britto, Pinotti, and Sampaio 2022).[3] Our paper fills this gap by providing new evidence on the role of geographic mobility following parental job loss.

This mobility channel may also help explain an ongoing empirical puzzle: evidence on the impacts of parental job loss on children is surprisingly mixed. Several studies on parental job loss have identified negative effects on children (Oreopoulos, Page, and Stevens 2008; Coelli 2011; Uguccioni 2021), while others have found null or weak effects on children (Hilger 2016; Bratberg, Nilsen, and Vaage 2008; Mörk, Sjögren, and Svaleryd 2020). This is despite a large body of theoretical and empirical work emphasizing the positive role of family income in childhood development (Page 2024; Bastian and Michelmore 2018; Becker and Tomes 1986). However, job loss presents both a large shock to family resources and job-specific costs to mobility. By documenting the importance of geographic mobility, and how returns to that mobility vary across place, we demonstrate the

[3]Two minor exceptions are Hilger (2016) and Oreopoulos, Page, and Stevens (2008) who provide estimates on the impact of parental job loss on geographic mobility, but those results do not represent the primary focus for either paper.

limitations to considering parental job loss as solely an income shock.

Our paper also contributes to the literature on the Great Recession and its aftermath. Studies have documented persistent local labor market "scarring" following the Great Recession, with employment-to-population ratios in hard-hit regions remaining depressed for years (Hershbein and Stuart 2022; Charles, Hurst, and Notowidigdo 2016). While a major focus of this literature has been to document how the Great Recession persistently lowered earnings among adult workers (Yagan 2019; Rinz 2022; Rothstein 2023), several studies have also examined its impact on human capital accumulation. While aggregate college enrollment rose during the Great Recession (Barr and Turner 2013; Barr and Turner 2015), other research has found that exposure to the Great Recession led to declines in student achievement, and college enrollment (Shores and Steinberg 2017; Jackson, Wigger, and Xiong 2021; Kirk 2023). Our findings broaden our understanding of how labor market scarring from the Great Recession shaped the mobility patterns and human capital outcomes of children following parental job loss.

Finally, for policymakers, our results highlight that mobility presents an underutilized policy lever. Current responses to job displacement focus almost exclusively on income replacement through unemployment insurance and retraining programs. For families in high-quality neighborhoods, these programs may support families to remain in place and maintain their children's access to opportunity. However, for families in low-quality neighborhoods, policymakers may want to support relocation after job loss by supporting reforms of occupational licensing or noncompete agreements (Johnson and Kleiner 2020; Johnson, Lavetti, and Lipsitz 2025), policies which reduce housing costs (Ganong and Shoag 2017), or direct cash-based relocation assistance.

Our paper proceeds as follows. Section 2 describes our data infrastructure. Section 3 describes our approach to identifying parental job loss and labor market scarring following the Great Recession. Section 4 presents our estimation approach. Section 5 reports our results. Section 6 explores robustness. Section 7 offers a discussion of our paper's implications and concludes the paper.

2 Data Infrastructure

In this paper, we leverage restricted-use microdata from the U.S. Census Bureau's data linkage infrastructure, combining multiple sources of administrative and survey data. All records are linked at the person-level using Protected Identification Keys (PIKs), which are generated by the Census Bureau's Person Identification Validation System (PVS) (Wagner and Layne, 2014).[4] We describe below our approach to identifying parent-child links and constructing population-level residential histories. Additional details on linking procedures and data sources are provided in Appendix B.

2.1 Sample Construction

Identifying Parent-Child Links: We identify all individuals born between 1979 and 2000 using the Census Numident file, which contains the universe of Social Security Number (SSN) applications. To establish child-parent links, we harmonize information across four primary sources: (i) the Census Household Composition Key (CHCK); (ii) the 2000 and 2010 Decennial Censuses; (iii) the American Community Survey (ACS) (2001-2019); and (iv) 1994 IRS Form 1040. These sources capture different types of parent-child linkages, and their combination is crucial to providing the coverage we need to implement our research design. The CHCK provides coverage for births from 1997 onward through SSN applications. Both the Decennial Censuses and ACS contain information on household structure, providing snapshots of co-residing children age$\leq$ 19, with the former providing a population-level snapshot in 2000 and 2010, and the latter providing nationally representative samples from 2001-2019. The 1994 IRS Form 1040 provides us with a snapshot of claimed dependents in 1994, allowing us to extend our sample of birth cohorts further back in time. Since multiple sources can report conflicting parent-child linkages, we prioritize information from SSN applications (CHCK), followed by survey household rosters (Decennial and ACS), and then rely on tax records. By combining multiple data sources, we address gaps in individual datasets—such as missing non-custodial parents in surveys and incomplete birth certificate coverage—and leverage overlapping periods to cross-validate linkages across independent administrative systems, ensuring comprehensive and high-quality parent-child matches throughout our sample period.[5]

One concern when using multiple sources of data is how selection into these sources might vary. This is particularly salient for low-income families, who are less likely to file taxes and appear in the IRS 1040 forms. Fortunately, as we discuss later in Section 3.1, we restrict our focus to workers

[4]PVS uses probabilistic matching to link person-level data to reference files from the SSA's Numident. These files are further supplemented with address records. This enables high-coverage linkages across datasets without exposing SSNs.

[5]Our approach to identifying parent-child links through a union of data series follows the spirit of Finlay, Mueller-Smith, and Street (2023). See Appendix B for additional details.

who are well-attached to the labor market, and largely exclude low-income families in our analysis. Another potential concern is that mass layoff events could affect our ability to identify parent-child linkages—for instance, if job loss causes family separation that disrupts administrative records. This concern is minimal in our setting because our parent-child linkages are primarily established before 2006, when our mass layoff sample period begins. We display coverage in Appendix Figure A1.

Employer-Employee Matched Earnings Records: To identify firms experiencing mass layoff events, and whether workers separate during those events, we use data from the Longitudinal Employer-Household Dynamics (LEHD) program which provides quarterly matched employer-employee earnings records from state unemployment insurance systems. The LEHD covers approximately 95% of U.S. private sector employment (Graham et al. 2022). We were provided a subset of 29 states from 1999 to 2019.[6] We measure quarterly earnings (winsorized at the 99.5$^{\text{th}}$ percentile and inflation-adjusted to 2019 dollars using the CPI-U) and employment status, imputing zero earnings for non-employed residents of covered states.

Tracking Residential History and Geographic Mobility: We construct an annual residence panel (1999–2019) by combining six administrative and survey sources: the 2000 and 2010 Decennial Censuses, the ACS, HUD's Tenant Rental Assistance Certification System (TRACS), the LEHD Composite Person Record (CPR), the LEHD Residence Candidates File (RCF), and the Census Bureau's Master Address File Auxiliary Reference File (MAF-ARF). Similar to our parental-child crosswalk, when different sources provide conflicting information on residence, we impose the following prioritization. The Decennial Censuses and ACS act as our first source of residential information, and provide the addresses used to administer the surveys. We follow this using HUD TRACS which provides administrative data on residence for recipients of rental assistance. We then rely on the CPR and RCF which are non-overlapping sources of residential data from the LEHD. These data sources are derived by aggregating multiple federal administrative sources using an algorithm to identify the "best address." Crucially, this dataset covers all wage workers even over nonemployment spells.[7] Finally, we fill any remaining gaps in residential data using the MAF-ARF, which

[6] Coverage includes: AZ, CA, CO, CT, DE, IA, IN, KS, MA, MD, ME, MT, NE, NJ, NM, NV, ND, OH, OK, OR, PA, SC, SD, TN, TX, UT, VA, WA, and WI (see Appendix Figure A2). Coverage periods vary by state.

[7] For documentation on the Residence Candidates File (RCF), see Graham, Kutzbach, and Sandler (2017). For the Composite Person Record (CPR), authors were provided documentation by the US Bureau Census and confirmed it may be publicly shared; an archived copy is hosted on the author's website at `https://andrewjoung.com/DataRepo/CPR_documentation.pdf`.

serves as the geographic frame for administering Census surveys and decennial censuses.

This integrated approach creates a PIK-by-year panel tracking residential location down to the Census block-level.[8] We aggregate this data to the county-level, and then crosswalk counties to commuting zones.[9]

Measuring Outcomes: For children's educational outcomes and neighborhood characteristics, we leverage both individual-level and aggregate data from the American Community Survey (2005–2019).[10] At the individual-level, we link children to the ACS to measure college attendance during ages 19–22, distinguishing between enrollment at public versus private institutions, applying ACS person weights to ensure nationally representative estimates. At the aggregate-level, we use the ACS to calculate commuting zone characteristics including college-educated share, poverty rates, and median household income. To construct children's neighborhood criminal justice exposure, we use data from the Criminal Justice Administrative Records System (CJARS), constructing standardized z-scores of CZ-level charge and incarceration rates for those ages 19-22, taking the average when both are available.[11]

When reporting impacts on neighborhood quality, we scale our estimated treatment effects by the corresponding mobility response to recover the implied neighborhood quality change for movers. Specifically, we report β/μ as the average neighborhood quality change experienced by job loss-induced movers, where β represents the estimated effect on a neighborhood characteristic and μ represents the effect on outmigration. We calculate the standard errors of the adjusted estimates using the delta method.

This interpretation assumes that parental job loss affects neighborhood quality solely through induced mobility. To eliminate concerns about time-varying location-specific shocks correlated with parental job loss timing, we measure neighborhood characteristics as time-invariant averages over 2006-2016, the period surrounding our mass layoff events. A remaining concern is that job loss affects not only whether families move but also their destination choices. While we document that job loss induces a permanent rise in CZ outmigration, estimates on neighborhood quality can combine the impact on both those induced to move by job loss (compliers) and families who would

[8] A Census block is the smallest geographic unit used by the U.S. Census Bureau for tabulating population data, typically corresponding to a city block bounded by streets in urban areas.

[9] See Appendix B for additional details.

[10] We exclude the experimental 2001-2004 survey waves of the ACS, where sampling was geographically restricted.

[11] This follows a similar approach taken by Finlay, Mueller-Smith, and Street (2023).

have moved regardless (always takers). If job loss causes the latter group of always takers to shift to worse neighborhoods due to diminished financial resources, our estimates would understate the true neighborhood quality gains experienced by families specifically induced to move by job loss. This suggests our estimates should be interpreted as lower bounds on the potential neighborhood improvements available to job loss-induced movers.

3 Quantifying Parental Layoff Exposure and Labor Market Scarring

This section describes our measurement of two key sources of variation for our empirical strategy: exposure to parental job loss through mass layoff events and spatial variation in long-run labor market scarring following the Great Recession.

3.1 Measuring Exposure to Parental Job Loss

Identifying quasi-exogenous job losses is challenging for at least two reasons. First, separations from an employer may reflect voluntary quits or retirements. Second, even if workers observe an involuntary separation, employers also exert discretion in firing. We address this identification problem by focusing on well-attached workers who separate during mass layoff events, following the approach pioneered by Jacobson, LaLonde, and Sullivan (1993).

We define a mass layoff event as a period where a firm's employment declines by at least 30 percent year-over-year and remains persistently below its pre-layoff level for the next three years. We impose standard restrictions on firm size, requirements on employment stability before and after the mass layoff event, and exclude likely mergers, spinoffs, or acquisitions.[12] Among workers at mass layoff firms, we restrict attention to those with at least three years of continuous tenure at the pre-mass layoff employer. We classify these well-attached workers as "job losers" if they separate during the mass layoff event and do not return within four quarters. Our control group consists of "survivors", a sample of similarly well-attached workers at the same firms who maintained continuous employment through the mass layoff event.[13] For workers experiencing multiple job

[12] See Appendix B for more details on our restrictions. Our criteria closely follow the standard approach within the literature. See Appendix Table 2 in Flaaen, Shapiro, and Sorkin (2019) for a comparison of mass layoff event definitions across studies.

[13] Workers who separate but return in less than four quarters are excluded from both treatment and control groups to maintain clean identification.

losses, we define treatment based on their first observed mass layoff separation. While our LEHD data permit identification of mass layoff events from 2002–2018, we restrict our analysis sample to mass layoff events occurring between 2006 and 2016. This restriction ensures we have sufficient coverage for the pre- and post-period, which is a function of data availability and birth cohort coverage. It provides overlap with the Great Recession and its aftermath, allowing us to exploit spatial variation in labor market scarring that we discuss in Section 3.2.

3.1.1 Sample Characteristics

Table 1 reports descriptive statistics for our sample of parents and children. Panel A presents baseline labor market (using the LEHD) and neighborhood characteristics (using the ACS) for parents by displacement status. Prior to the mass layoff event, families with job losing and surviving parents exhibit similar household incomes of approximately \$111,160 and \$118,940, respectively. These relatively high baseline incomes reflect our focus on well-attached workers with stable employment histories. Importantly, displaced and surviving families reside in comparable neighborhoods—a natural consequence of working at the same firms—supporting the validity of survivors as a counterfactual group. While we observe minimal average changes in neighborhood quality following parental job loss, this masks offsetting mobility patterns across labor markets, which we explore in Section 3.2.

3.2 Measuring Labor Market Scarring of the Great Recession

We measure labor market scarring as the percentage point decline in the commuting zone employment-to-population ratio between 2007-2016:

$$\text{Scarring}_c = -100 \times (\text{EPOP}_{c,2016} - \text{EPOP}_{c,2007})$$

where $\text{EPOP}_{c,t}$ denotes the employment-to-population ratio in commuting zone c in year t. Inspired by Yagan (2019), our measure captures the spatial variation in labor market scarring that followed the Great Recession. We construct this measure using publicly available county-level employment data from the Bureau of Labor Statistics' Local Area Unemployment Statistics (LAUS) combined with population data from the National Cancer Institutes' Surveillance Epidemiology and End

Results (SEER) program.[14] We crosswalk these data at the commuting zone-level to construct our commuting zone-level measure of scarring. Our measurement period (2007-2016) spans both the recession and its aftermath. Intuitively, this measure will be positive for commuting zones which did not fully recover to their pre-recessionary EPOP levels by 2016 and zero or negative for commuting zones who returned to baseline or actually improved relative to the pre-recession level.

A central empirical challenge in examining the role of mobility following parental job loss is that families across different neighborhoods face different next-best destinations, leading to different returns to outmigration. Even ignoring concerns around selection, the average impact of job loss-induced mobility on neighborhood quality will report the weighted sum of effects across families. This could be problematic if families experience large divergent effects. For example, families in high-quality neighborhoods will face fewer quality-improving options than families in lower-quality neighborhoods who face many potential destinations that would improve their circumstances. In this setting, economic interpretation of average effects becomes challenging, as the effect across different families may observe sign-flipping. Additional information on the expected returns to outmigration across families is required to recover more easily interpretable effects.[15]

We address this challenge by exploiting spatial variation in labor market recovery following the Great Recession for two principal reasons. First, the Great Recession is a well-studied source of plausibly exogenous spatial variation in local economic conditions. Prior work has increasingly recognized that the effects of recessions extend well beyond the initial shock, and have used similar cross-CZ variation in recession severity and recovery to study employment hysteresis (Yagan 2019; Rinz 2022; Rothstein 2023), human capital investment (Stuart 2022), and mortality (Finkelstein et al. 2023). Our approach follows this literature and leverages plausibly exogenous variation in labor market scarring generated by the Great Recession, allowing us to identify how families respond to persistent differences in local economic conditions.

Second, given our sample ranges from children age 10 (late adolescence) to age 18 (early adulthood), persistent labor market conditions are likely to be particularly relevant. While short-term fluctuations in neighborhood quality may have larger effects on young children during sensitive

[14] LAUS data are available at https://www.bls.gov/lau/; SEER population data at https://seer.cancer.gov/popdata/.

[15] This problem is closely related to the classic problem noted by Heckman and Vytlacil (2007) in the context of unordered choice models and the interpretation of IV estimates.

developmental periods, the impacts of transitory shocks are likely to diminish for older children. As Caucutt and Lochner (2020) illustrate in the context of a life-cycle model, when early and late human capital investments are complementary even relatively large transitory shocks in adolescence may have limited consequences for children's later human capital investments. Our labor market scarring measure identifies commuting zones with sustained economic weakness rather than temporary downturns. Furthermore, as we demonstrate in the next section, this measure serves as a sufficient statistic for multiple dimensions of neighborhood quality beyond labor market strength.[16]

3.2.1 Geographic Patterns of Labor Market Scarring

Panel A of Figure 1 documents employment trajectories during and after the Great Recession among commuting zones with low (below median) and high (above median) labor market scarring. During the Great Recession, the EPOP ratio plummeted similarly across commuting zones, with a permanent gap of roughly 2 percentage points emerging in 2010. The unemployment rate (Panel B) tells a similar story, more than doubling during the recession with a roughly 1 percentage point gap emerging in 2010, though this gap has fell by half by 2013. This persistent gap in labor market recovery exhibited substantial geographic variation in local labor market experiences. Appendix Figure A3 reveals the uneven geographic distribution of labor market scarring across the United States. While some commuting zones rebounded quickly, others experienced persistent employment declines that lasted nearly a decade. The cross-commuting zone interquartile range spans from 0.2 to 2.6 percentage points, indicating substantial variation in recovery trajectories.[17]

How does labor market scarring relate to broader neighborhood characteristics? Table 2 compares neighborhood attributes at the 25$^{\text{th}}$ and 75$^{\text{th}}$ percentiles of the scarring distribution.[18] Scarred labor markets consistently exhibit worse outcomes across multiple dimensions of neighborhood quality on average. Relative to less-scarred areas, heavily scarred commuting zones have higher poverty rates, lower educational attainment, reduced median household income, and greater criminal jus-

[16]Crucially, while other indicators of neighborhood quality might directly measure important local conditions, they may also reflect endogenous sorting patterns that may confound identification. In contrast, our scarring measure leverages the plausibly exogenous spatial variation generated by the Great Recession.

[17]See Appendix Figure A3 for a visual representation of the geographic variation in labor market scarring.

[18]We present results for both the full United States and our 29-state LEHD sample. To reduce noise from outliers, we calculate characteristics using a five-percentile bandwidth around each focal percentile. Appendix Figure A4 presents binscatter plots showing these relationships across the full scarring distribution. Note that these descriptive statistics draw on publicly available data sources; see table notes for details.

tice system involvement. In Appendix Table A1, we present these same comparisons split into an earlier and a later period. These estimates show that while neighborhood characteristics drift over time, differences between high- and low-scarring commuting zones persist, further emphasizing that long-run scarring captures persistent differences in overall neighborhood quality.

Given these stark differences in neighborhood quality across the scarring distribution, do families observe different returns to outmigration by labor market scarring? To examine potential gains from outmigration, we calculate the average change in neighborhood characteristics for families who move between commuting zones using IRS SOI migration data from 2007-2013.[19] For each origin commuting zone, we observe the outmigration share to all destinations. We then weight destination characteristics by these shares to compute the average quality of destination commuting zones for movers. This reveals the average change in neighborhood quality observed by movers from each origin commuting zone.

Figure 2 presents these outmigration premia across multiple neighborhood dimensions. Panel A reveals that families leaving scarred labor markets move to sharply less-scarred destinations. For families leaving labor markets in the upper quartile of scarring, this translates to a 1.2 percentage point decline in scarring, a roughly 82 percent decline in scarring relative to the average commuting zone. In contrast, for families leaving labor markets in the bottom quartile of scarring, this translate to a nearly offsetting 1.3 percentage point rise in scarring, a roughly 83 percent rise in scarring relative to the average commuting zone. Consistent with the correlation structure from Table 2, outmigration from scarred labor markets yields improvements across other neighborhood characteristics: families who leave heavily scarred neighborhoods also move to areas that are wealthier, have smaller criminal justice systems, and have a more educated local population. In contrast, outmigration from non-scarred labor markets yields declines in qualities, or little change. These descriptive patterns suggest that geographic mobility from scarred labor markets could provide an important channel for improving children's economic opportunities—a possibility we test directly in our empirical analysis below.

[19]We restrict to pre-2014 data due to documented quality issues in later years (Olney and Thompson, 2024; DeWaard et al., 2022). Outmigration premia outcomes are calculated using publicly available statistics from Finlay, Muller-Smith, and the CJARS Team (2024), Chetty et al. (2025), and Manson et al. (2024); see table notes for details.

4 Empirical Strategy

In this section, we outline our empirical strategy to estimate the effects of parental job loss. First, we quantify effects on family resources, as well as children's mobility and neighborhood exposure using an event study model that leverages within-person variation over time. Second, we estimate effects on children's long-run human capital outcomes using a cross-cohort design that exploits variation in age at exposure.

4.1 Effects on Family Resources, Children's Mobility, and Neighborhood Quality

Our panel data allows us to observe outcomes at the individual-level both before and after the layoff event through age 18.[20] This allows us to estimate an event study specific with individual-level fixed effects of the following form:

$$Y_{it} = \sum_{\ell=-5,\ell\neq-1}^{10} \beta_\ell \big[\text{Job Loss}_i \times E_{i,\ell}\big] + \theta_i + \phi_t + \mathbf{X}'_{it}\gamma + \varepsilon_{it} \tag{1}$$

where Y_{it} denotes outcomes for child-parent pair i in year t. Job Loss$_i$ equals one if i is exposed to parental job loss, and is zero otherwise. $E_{i,\ell}$ denotes event-time indicators where ℓ indexes years relative to the mass layoff. The model includes child-parent fixed effects θ_i, and year fixed effects ϕ_t.[21] Our individual-level fixed effects flexibly control for any unobservable time-invariant characteristics.[22] We include fixed effects, $\mathbf{X}_{it}$, for pre-mass layoff family characteristics (family income ventile and commuting zone of residence) and employer characteristics (1-digit NAICS code, and pre-mass layoff employer size ventile) all interacted with calendar year indicators to flexibly capture differential time trends along pre-MLE family characteristics, residence, and firm.[23] To address staggered treatment timing, we interact all fixed effects and controls with MLE-year indicators (Cengiz et al. 2019; Deshpande and Li 2019).[24] Our standard errors are clustered at the

[20]For mobility and neighborhood quality outcomes, we track children's residential histories only through age 18, focusing on childhood exposure effects.

[21]Given our data structure, θ_i is analogous to an individual-level fixed effect.

[22]This absorbs firm-specific fixed effects, which crucially controls for pre-MLE firm-level selection.

[23]Similar controls are used by Davis and von Wachter (2011) and Lachowska, Mas, and Woodbury (2020) to control for differential trends in earnings.

[24]Treated units are assigned a year of MLE, or treatment cohort, based on their first MLE where they are classified as a job loser based on our definitions. Control units maybe assigned to multiple treatment cohorts, but crucially are "clean" controls as they have never been classified as a job loser over our sample period.

firm-by-year-of-mass-layoff-level.

Our coefficient of interest, β_ℓ, captures the effect of parental job loss on outcomes ℓ years after the event. Our identifying assumption is parallel trends: absent parental job loss, outcomes would have evolved similarly for children of job losers and survivors. The pre-period coefficients offer a test of this identifying assumption and provide an additional way to quantify any residual time-varying selection between job losers and survivors.

When investigating the role of local labor market scarring and its interaction with parental job loss, we present in Appendix estimates from Eq. (1) where we split our sample by pre-mass layoff event commuting zones above and below the median of our scarring measure. While these subsamples provide a visual representation of the underlying role of labor market scarring, it discards much of the variation in scarring across places and does not directly recover the "dose-response" relationship between labor market scarring and the impact of parental job loss on outcomes. To examine the effect of labor market scarring, we estimate the following triple-difference event study specification:

$$\begin{aligned} Y_{it} = & \sum_{\ell=-5,\ell\neq-1}^{10} \beta_\ell^{\text{scar}} \Big[\underbrace{\text{Job Loss}_i \times E_{i,\ell} \times \text{Scarring}_c}_{\Delta\text{Job Loss Effect for 1 p.p.}\uparrow\text{ in Scarring}_c} \Big] \\ & + \sum_{\ell=-5,\ell\neq-1}^{10} \alpha_\ell \Big[\underbrace{\text{Job Loss}_i \times E_{i,\ell}}_{\text{Baseline Job Loss Effect}} \Big] + \sum_{\ell=-5,\ell\neq-1}^{10} \delta_\ell \Big[\underbrace{\text{Scarring}_c \times E_{i,\ell}}_{\text{Common Scarring Effect}} \Big] \\ & + \theta_i + \phi_t + \mathbf{X}'_{it}\gamma + \varepsilon_{it} \end{aligned} \tag{2}$$

where Scarring_c denotes our scarring measure which we define as the percentage point decline in the employment-to-population ratio from 2007-2016 in i's pre-mass layoff commuting zone c.[25] Our coefficient of interest, β_ℓ^{scar}, reports how the effect of parental job loss varies with each percentage point increase in origin labor market scarring. Our key identifying assumption is that absent differences in labor market scarring, any remaining differences in treatment effects across commuting zones would not be systematically related to our scarring measure (Olden and Møen 2022).

[25] Note that $\text{Job Loss}_i \times \text{Scarring}_c$ is absorbed by θ_i.

4.2 Intergenerational Effects on Children's Human Capital

For children's long-run outcomes, we focus on educational outcomes derived from the ACS. The primary empirical challenge relative to the prior section is that we cannot observe a pre-period, making a standard event study design infeasible. This is because these outcomes are measured during a fixed age window (19-22).[26] We therefore employ a cross-cohort pseudo-DiD design following Hilger (2016):

$$Y_{i,19-22} = \sum_{g \in \substack{\{10\text{–}14,\, 15\text{–}18,\\ 19\text{–}22\}}} \beta_g \left[\text{Job Loss}_i \times \text{Age Group}_{i,g}\right] + \text{Job Loss}_i + \theta_b + \mathbf{Z}_i'\delta + \mathbf{X}_{ib}'\gamma + \varepsilon_i \quad (3)$$

where $Y_{i,19-22}$ denotes the outcome for child-parent pair i measured at ages $19-22$. $\text{AgeGroup}_{i,g}$ indicates the child's age when their parent experienced job loss, with children aged 23+ at exposure serving as the reference group. We include birth-year fixed effects θ_b. $\mathbf{Z}_i$ contains individual and family characteristics that help account for selection in the absence of individual-level fixed effects.[27] $\mathbf{X}_{ib}$ contains pre-mass layoff characteristics (family income ventile, commuting zone, parental employer's 1-digit NAICS and size ventile) that we interact with birth cohort indicators to allow for differential trends across cohorts. To address staggered treatment timing, we interact all fixed effects and controls with MLE-year indicators (Cengiz et al. 2019; Deshpande and Li 2019). Standard errors are clustered at the firm-by-year-of-mass-layoff-level. We apply person weights and include survey wave fixed effects for outcomes derived from the ACS, like college attendance, to account for the survey's sampling design.

Intuitively, this design compares outcomes of children who were exposed to parental job loss at different ages—some young enough that the shock could affect their college decisions (ages 10-22) and others old enough that their college attendance was already determined (ages 23+). Our controls and fixed effects ensure comparisons across children with similar pre-MLE characteristics—including commuting zone, family attributes, and firm characteristics. The older cohorts provide a natural placebo group: since their college outcomes were realized before parental job loss occurred, any

[26] As a simple example, suppose a child is exposed to parental job loss at age 10, and we want to know the impact on college attendance at ages 19-22. Since the child's "pre-period" in calendar time is at ages prior to 10, we mechanically cannot observe a pre-period for college attendance measured at a future age.

[27] These include a cubic in parent's age at exposure and indicators for white/non-white, child's gender, marital status of parents, and whether job loss exposure was through the child's mother or father.

"effect" we estimate for this group reveals residual selection between children of job losers and survivors. Our key identifying assumption is that, absent parental job loss, the outcomes of children whose parents would become job losers and survivors would have evolved in parallel across birth cohorts—that is, any pre-existing differences between these groups would have remained constant across age-at-exposure groups.

To examine the effect of labor market scarring, we estimate the following triple-difference specification:

$$\begin{aligned} Y_{it} = & \sum_g \beta_g^{scar} \left[\text{Job Loss}_i \times \text{Age Group}_{i,g} \times \text{Scarring}_c\right] \\ & + \sum_g \alpha_g \left[\text{Job Loss}_i \times \text{Age Group}_{i,g}\right] + \sum_g \delta_g \left[\text{Age Group}_{i,g} \times \text{Scarring}_c\right] \\ & + \text{Job Loss}_i \times \text{Scarring}_c + \text{Job Loss}_i + \lambda_b + \mathbf{Z}_i' \delta + \mathbf{X}_{ib}' \gamma + \varepsilon_i \end{aligned} \tag{4}$$

where β_g^{scar} captures how the effect of parental job loss at age group g varies with origin labor market scarring. This specification allows us to test whether mobility from scarred labor markets differentially impacts children exposed at different ages. The inclusion of all two-way interactions ensures that β_g^{scar} isolates the differential effect of scarring on the treatment impact, separate from any direct effects of scarring or age-specific selection patterns. We again account for staggered treatment by interacting fixed effects and controls with MLE-year indicators following the approach above and cluster standard errors at the firm-by-year-of-mass-layoff-level.

5 Parental Job Loss Effects Across Labor Markets

In this section, we examine the effect of parental job loss on parental earnings, household mobility, and children's human capital accumulation. We begin with presenting estimates using our standard event-study specification before exploring the intergenerational implications of parental job loss using our cross-cohort design.

5.1 Effects on Parental and Family Earnings

Our estimates of the impact of parental job loss on earnings are shown in Figure 3. Our pre-period estimates find no evidence that families of job losers and survivors exhibit differential earnings trends leading up to the mass layoff event, along with precipitous declines in the post-period both in levels and relative terms. Relative to survivors' earnings trajectories, displaced parents experience a nearly 24% percent decline in their earnings in the year after job loss with relative losses improving to approximately 15% - 18%, and stabilizing around 8-10% (Panel B). Panel C illustrates how some of the persistent earnings losses are due to changes in the extensive margin of employment, with displaced parents having consistently lower employment probabilities. Employment rates fall by 25 percentage points immediately and recover only partially, remaining 7-10 percentage points lower throughout the follow-up period.[28] Our estimates are comparable with prior findings both within and outside the US (Jacobson, LaLonde, and Sullivan 1993; Couch and Placzek 2010; Lachowska, Mas, and Woodbury 2020; Athey et al. 2023). Finally, Panel D examines family-level income responses. We find earnings losses that are similar in magnitude with Panel A, consistent with minimal compensating labor supply responses through "added worker" effects found in more recent studies (Hilger 2016; East and Simon 2024).

5.2 Children's Mobility

While prior work has established that job loss increases adult migration (Huttunen, Møen, and Salvanes, 2018), the implications for children's neighborhood quality remain theoretically ambiguous.[29] Post-job loss liquidity constraints may push families toward lower-cost, lower-quality neighborhoods. Conversely, job loss also means that families no longer face the trade-off between accessing better locations and sacrificing job-specific, or employer-specific capital. Absent these additional mobility costs, families can pursue high-opportunity areas. Given extensive evidence that childhood neighborhood exposure shapes long-run outcomes (Chetty and Hendren, 2018), understanding which force dominates is crucial for evaluating the intergenerational consequences of job loss.

[28] As Lachowska, Mas, and Woodbury (2020) show, displacement also reduces earnings through the destruction of employer-employee match specific effects.

[29] When facing job loss, workers respond through multiple margins including exiting the labor force (Chan and Stevens, 1999; Chan and Huff Stevens, 2001), taking up unemployment insurance (East and Simon, 2024), engaging in criminal activity (Rose, 2018; Britto, Pinotti, and Sampaio, 2022), and adjusting search intensity (Krueger and Mueller, 2010; Faberman et al., 2022).

Pooled Effects: Panel A of Figure 4 presents event study estimates for children's outmigration from pre-mass layoff commuting zone through age 18.[30] The pre-period exhibits modest downward trends, an empirical pattern in line with prior work in the displacement literature (Huttunen, Møen, and Salvanes, 2018) and consistent with strengthening location attachment at the pre-layoff firm. Additionally, the small downward trend suggests that while children of job losers initially exhibit slightly greater mobility than children of survivors, the residual differences between them were converging to zero prior to job loss.[31] Post-displacement, we observe immediate and sustained increases in outmigration. Commuting zone outmigration sharply rises by 3.2 percentage points, a near doubling of the outmigration rate, and stabilizes four years after parental job loss to around 4.3 percentage points.

In Appendix Figure A5, we find similar patterns for county and state-level outmigration. Our results on cross-state outmigration, moreover, highlight that the bulk of our estimated effect on CZ outmigration is driven by out-of-state moves. State-level moves (Panel B) immediately rise by 2.6 percentage points, stabilizing around 3.4 percentage points, implying around 79% of our estimated permanent rise in CZ outmigration involves persistent out-of-state moves. This distinguishes job-loss-induced mobility from temporary relocation, suggesting permanent reallocation rather than transitory adjustment.

Scarring-Dependent Effects: These aggregate patterns obscure that returns to outmigration vary dramatically based on origin labor market scarring (commuting zone-level EPOP decline from 2007-2016), with families in scarred areas having much more to gain from relocation (Section 3.2). Panel B of Figure 4 reveals that families in scarred labor markets exhibit dramatically stronger mobility responses. Each percentage point increase in origin scarring generates an additional 0.2 percentage points of CZ outmigration in the immediate years after parental job loss. Taking the interquartile range from the 25$^{\text{th}}$ to 75$^{\text{th}}$ percentile of the scarring distribution, this implies an increase in outmigration by 0.4 percentage points, or roughly an additional 17% rise in CZ outmigration rates. Unsurprisingly, given our pooled estimates, we again find a small modest downward pre-trend, which

[30] We estimate these outmigration rates using children's residential histories through age 18. As we discussed earlier, this is due to our focus on childhood exposure to changed residential environments.

[31] This pattern likely reflects the fact that displaced workers generally exhibit lower tenure than non-displaced workers-—a consistent finding in the displacement literature. As our three-year tenure restriction takes effect in the pre-period, these pre-existing differences diminish to economically insignificant levels.

converges toward zero prior to job loss.[32,33]

5.3 Neighborhood Quality

Pooled Effects: Our analysis reveals that parental job loss typically leads to deterioration in neighborhood quality. As illustrated in Panel A of Figure 5, children who relocate following a parent's employment loss tend to settle in areas with 0.25 percentage point greater scarring—representing a 20.2% increase in labor market scarring compared to the commuting zone average. Moreover, as demonstrated in Panels C and E, children moving from labor markets with high scarring end up in communities where the proportion of college-educated residents is 0.7 percentage points lower—a 2.2% decline—while poverty rates are 0.5 percentage points elevated—a 3.9% increase. Crucially, our pre-period estimates are neither economically nor statistically significant, suggesting minimal concern that selection into better neighborhoods is driving our results.

Table 3 Columns 1 and 2 present difference-in-differences estimates of neighborhood characteristics for job-losing families relative to survivors. As mentioned in Section 4, we divide our estimates by the corresponding CZ outmigration response to obtain the neighborhood quality change for job-loss-induced movers. On average, children exposed to parental job loss experience deteriorating neighborhood conditions across multiple dimensions following outmigration. The college-educated share in their neighborhoods falls by 0.7 percentage points (a 2.2% decline), while poverty rates increase by 0.5 percentage points (a 3.9% increase). Family neighborhoods also exhibit lower median household incomes and increased criminal justice exposure. Most strikingly, children in job-losing families experience a 0.3 percentage point increase in exposure to labor market scarring indicating that the average family moves toward, not away from, economically distressed areas. These patterns align with a model where liquidity-constrained families prioritize affordability over opportunity, accepting lower neighborhood quality in exchange for reduced housing costs.[34]

Scarring-Dependent Effects: Do job loss-induced moves from scarred origins lead to neighborhood

[32] In Appendix Figure A6 and A7, we show this is particularly pronounced for younger children (ages 10-14), who experience mobility responses three times larger than older adolescents (ages 15-18). This age gradient may partly reflect greater mobility among younger parents, with presumably younger children (Molloy, Smith, and Wozniak 2011).

[33] See Appendix Figure A8 for estimates split by above/below median scarring, and Appendix Figure A9 for baseline estimates α_ℓ from Equation (2).

[34] See Appendix Figure A10 for event studies underlying these tables.

improvements? Figure 5 provides evidence that they do, confirming our descriptive results in Section 3.2. Panel B shows that each percentage point of scarring (commuting zone-level EPOP decline from 2007-2016) leads to destination neighborhoods with 1.0 percentage point less scarring, a roughly one-for-one offset through mobility (relative to pre-shock labor market scarring). This improvement extends beyond labor market conditions toward broader measures of neighborhood quality. Panels D and F document that children from scarred labor markets access neighborhoods with 0.8 percentage points higher college-educated share–a 2.6% increase–and 0.1 percentage points lower poverty rates–a 0.9% decrease. While we observe pre-trends in our event study estimates for destination labor market scarring (Panel B), point estimates are small relative to the main effect, and exhibit an upward trend. For our other measures of neighborhood quality, we observe no statistically significant estimates in the pre-period, suggesting that selection into better neighborhoods does not vary by labor market scarring. Table 3 Columns 3 and 4 report that these improvements span multiple dimensions of neighborhood quality. Children leaving scarred labor markets access neighborhoods with significantly higher pre-kindergarten enrollment rates, higher median household incomes, higher housing costs, and reduced criminal justice exposure.[35,36]

A natural question is whether these effects from labor market scarring stem from differential parental income losses rather than the mobility channel we emphasize. If families in scarred labor markets experienced systematically larger income shocks, this would present a challenge for our interpretation: greater earnings losses should reduce families' ability to finance costly moves and decrease college attendance through credit constraints, working against the positive effects we observe. In Appendix Figure A14, we estimate event studies separately for families in above- and below-median scarred labor markets. The earnings trajectories are remarkably similar. The difference in earnings losses between high- and low-scarring areas is economically small and statistically insignificant throughout our follow-up period. Family income losses show a similar pattern, with no meaningful difference by scarring level.

These results do not contradict prior work establishing that job loss during recessions leads to

[35] The housing cost increases accompanying these moves suggest families are accessing better neighborhoods. For completeness, we also show in Appendix Figures A8 and A11 that these differential migration and neighborhood quality responses persist, in contrast to the earnings effects on adults, when we parsimoniously split the sample by above- and below-median origin scarring.

[36] See Appendix Figure A10 for event studies; Appendix Figure A12 and Appendix Table A3 for unscaled estimates; and Appendix Figure A13 for baseline estimates α_ℓ from Equation (2).

larger earnings losses (Schmieder, Von Wachter, and Heining, 2023; Davis and von Wachter, 2011). In Appendix Figure A15, we present evidence that labor market scarring has little correlation with immediate labor market conditions. In Panel B, we show that initial exposure to the Great Recession (measured as the change in the employment-to-population ratio from 2007-2009) correlates only weakly with our long-run scarring measure, with a correlation coefficient of 0.227. Since our scarring measure captures persistent labor market weakness rather than short-run cyclical conditions studied in prior work, it is unsurprising that we find no heterogeneity in earnings losses by scarring. These results suggest that differences in household resources are unlikely to drive the differences we document by labor market scarring. We provide a more detailed analysis in Appendix C where we focus on mass layoff events between 2007–2009, and compare how the effect of job loss by long-run labor market scarring and initial short-run exposure to the Great Recession.

Taken together, our results establish that parental job loss triggers sustained geographic reallocation, with families in scarred labor markets both more likely to move and more likely to access higher-quality destinations. Similar to work on moves following public housing demolition by Chyn (2018), job loss may force beneficial moves that families would not have otherwise undertaken. For families in distressed labor markets, job displacement may provide greater time for job search, or reduce employment ties that previously constrained mobility, allowing them to capitalize on gains from outmigration. To the extent that neighborhoods are an important input into human capital accumulation, children are likely to benefit from exposure to such environments (Bergman et al. 2024).

5.4 Children's Long-Run Human Capital

Our results documented above suggest that exposure to parental job loss leads children in scarred labor markets to experience improvements in neighborhood quality. A large body of empirical evidence on place-based effects has found that where children live is a first-order determinant of their long-run outcomes (Chetty and Hendren 2018; Chetty, Hendren, and Katz 2016; Bergman et al. 2024; Aloni and Avivi 2025; Chyn 2018; Chyn and Katz 2021; Deryugina, Kawano, and Levitt 2018). This would seem to imply that job loss-induced outmigration from scarred labor markets might improve children's human capital (or conversely hurt those forced from non-scarred labor markets). However, in our setting, children observe these moves alongside the negative shock from parental job loss. Even if children move to better neighborhoods, liquidity constraints or the disruption of social capital following parental job loss may undermine the benefits of improved neighborhood quality.

Pooled Effects: Panel A of Figure 6 presents our cross-cohort difference-in-differences estimates for college attendance at ages 19-22. On average, we find parental job loss has insignificant effects on college attendance. These effects are consistent with Hilger (2016) who also finds limited impacts of parental job loss on college attendance using U.S. federal tax returns.[37] When examining our results separately for public and private college enrollment, we find null effects on private college attendance, and suggestive evidence of a rise in public college attendance though significance is marginal ($p < 0.10$). As we discuss below, this suggestive rise in public college attendance seems to be driven entirely by children exposed to parental job loss in scarred labor markets.

Scarring-Dependent Effects: The modest average effects mask the important role of origin labor market scarring (commuting zone-level EPOP decline from 2007-2016). Panel B of Figure 6 leverages variation in labor market scarring across origin commuting zones to reveal how local economic conditions fundamentally shape the intergenerational transmission of job loss. For children exposed to parental job loss, originating from a labor market with one percentage point higher scarring translates to 1.2-1.6 percentage points higher college attendance.[38] Scaling our estimates by the interquartile range of our scarring measure (see Table 2), we that the implies effects by labor market scarring are magnitudes larger than the pooled effects: children experiencing parental job loss in heavily scarred origins (75$^{\text{th}}$ percentile) see college attendance increase by approximately 2.9-3.9 percentage points relative to those in minimally scarred origins (25$^{\text{th}}$ percentile). These effects are concentrated in public institutions. While private college attendance shows no differential response to origin scarring, public enrollment increases by 1-1.2 percentage points per percentage point of scarring. The absence of private college effects suggests that job loser families may remain financially constrained and cost-sensitive when making post-secondary investment.

We examine whether college enrollment merely reflects differential labor market opportunities.

[37] The 95 percent confidence interval surrounding our estimates contains the estimate from Hilger (2016), even while measuring college attendance over a slightly broader age range, different sample period, and with administrative tax records. Despite these differences in sample and measurement, we conclude there are only mild direct effects of parental job loss on children's college attendance.

[38] See Appendix Figure A16 for baseline estimates α_g from Equation (4).

For example, children exposed to parental job loss in strong labor markets enter the labor force to supplement household income—similar to the spousal labor supply response documented by the "added worker" literature—whereas children exposed to parental job loss in scarred labor markets are less able to enter the labor force and instead enter college. Appendix Figure A17 shows either null or economically insignificant effects from parental job loss on children's earnings or employment at ages 19-22, regardless of labor market scarring.[39] Together, these results rule out a simple substitution story where weak labor markets in scarred origins push children into college by default.[40]

Another potential explanation could be that rising high school graduation rates mechanically lead to rising college attendance rates. While this story would be consistent with improving neighborhood quality, Appendix Figure A18 reports insignificant effects on high school completion, suggesting that these effects are not driven by improvements in high school graduation rates.

6 Leaving Scarred Labor Markets and College Attendance

Our reduced form results document that children of job-losing parents in scarred labor markets experience both increased mobility and improved college attendance. While our mobility response is substantial, the vast majority of families remain in place following parental job loss. These non-movers in scarred labor markets may also increase college attendance through alternative channels, such as viewing education as insurance against local labor market volatility. Given the small share of families induced to move, the observed gains in college attendance are likely to arise from both movers and non-movers, and could conceivably be driven entirely by non-movers. To establish that mobility itself raises college attendance, we adapt the methodology used by Chetty and Hendren (2018) (a movers' design), where we leverage variation in the timing of moves after job loss, comparing children who move by age 14 to those who move at ages 15-18, across origin labor markets with different degrees of scarring.

[39]We estimate job loss reduces children's share of years employed between ages 19-22 by 0.2 percentage points per percentage point of labor market scarring. While statistically significant, this reduction is not economically meaningful, representing roughly on average 3.6 additional days of work.

[40]Labor force participation rates are relatively low among young workers. Between 2000-2019, using the CPS, we estimate the employment rate among 19-22 year high school graduates not attending college is 71%, whereas the prime-age employment rate among 25-54 year old workers is 82%. This highlights that a considerable share of young adults who forgo college do not transition directly into employment, which partially explain why reduced college enrollment does not mechanically translate into higher employment rates.

Formally, we estimate the following:

$$Y_{i,19-22} = \beta^{\text{movers}} \left[\text{Scarring}_c \times \text{YoungMover}_i\right] + \text{YoungMover}_i + \theta_b + \theta_{CZ} + \mathbf{X}_i'\gamma + \varepsilon_i \qquad (5)$$

where YoungMover_i is an indicator for children who moved by age 14. The covariate vector $\mathbf{X}_i$ controls for additional individual (white/nonwhite, gender) and family characteristics (parental marital status, indicator for mother/father laid off, pre-mass layoff family income ventile), pre-mass layoff commuting zone of residence. We also estimate a separate specification including employer's characteristics (1-digit NAICS code, and pre-mass layoff employer size ventile). One major limitation is that this approach dramatically reduces our sample size. As a result, we no longer interact all of our fixed effects and control with year-of-mass-layoff fixed effects or with birth-cohort fixed effects. Standard errors are clustered at the firm-level.

The coefficient β^{movers} identifies the effect of moving from a labor market with one percentage point greater scarring by age 14 rather than later. Since children who move at older ages are still partially treated, this empirical exercise provides us with a lower bound on the effects of moving out of a weak labor market.

The coefficient β^{movers} reports the effect of leaving a commuting zone with an additional percentage point rise in scarring for children who move by age 14 versus those who move at ages 15-18. We focus on children plausibly induced into CZ outmigration by parental job loss, restricting our sample to those who left their origin commuting zone within two years of parental job loss and by age 18. By comparing early and late movers from similarly scarred origins, we leverage variation in exposure to outmigration by labor market scarring: early movers from scarred labor markets observe a longer exposure to improved neighborhoods following outmigration compared to late movers. Our identifying assumption requires that, conditional on origin labor market scarring, selection into mobility does not vary systematically with age at move. This allows us to use late movers as a comparison group to isolate the causal effect of job loss-induced outmigration from scarred labor market. Since children who move at ages 15-18 still receive partial treatment through some exposure to improved locations, our estimates represent a lower bound on the full effect of leaving a scarred labor market early in childhood.

Table 4 reports coefficient estimates from our movers design. In Panel A, Column 1, we report estimates from our preferred sample and specification. We find that children who leave labor markets at ages 10-14 with a one percentage point greater degree of labor market scarring are nearly four percentage points more likely to attend college at ages 19-22 than a child who moved at ages 15-18. In Column 2, we add a set of controls for firm characteristics and continue to find precisely estimated positive impacts on college attendance of outmigration for younger children. In Panel B, we restrict the sample to children who did not move prior to the mass layoff event, reducing concerns about our results reflecting selected families who repeatedly move across neighborhoods. Despite the smaller sample, our results consistently indicate improvements in human capital accumulation.

Our results suggest that movers leaving scarred labor markets observe large benefits from improved neighborhood quality. The 3.1 percentage point effect is nearly two times larger than our reduced-form estimate by origin labor market scarring of 1.6 percentage points for those exposed to parental job loss at ages 10-14. Since our reduced-form estimates pool the effects on movers and non-movers, we perform a back-of-the-envelope calculation using the estimated share of job loss-induced movers by age 18 (4.5 p.p.) from Appendix Figure A6 to back out the implied impact on non-movers in scarred labor markets. Our exercise implies that non-movers in scarred labor market also observe higher college attendance rates: for each additional percentage point of scarring, non-movers exposed to parental job loss at ages 10-14 raise college attendance by 1.4 p.p., which is slightly lower than our reduced-form estimate. These effects among non-movers suggest that parental job loss in economically distressed areas triggers broader behavioral responses beyond the mobility channel we emphasize. For example, non-movers may respond to heightened awareness of local labor market volatility by viewing education as insurance against future economic shocks. An interesting area for future research would be to disentangle these complementary mechanisms.

7 Discussion and Conclusion

Existing work on parental job loss has emphasized the role of lost family income, or its replacement through the social safety net. In a recent review, Page (2024) notes that while the majority of evidence suggests that family income has a positive role on children's long-run outcomes, studies leveraging exposure to parental job loss have produced mixed results.

In this paper, we highlight the importance of channels beyond family income. In particular, we focus our attention on the importance of job loss-induced outmigration. To do so, we use restricted-use Census microdata to construct a rich data environment, allowing us to identify childhood exposure to parental job loss, residential histories, and long-run college attendance. This infrastructure allows us to empirically estimate the effects of parental job loss by comparing the children of job losers and survivors on children's mobility, exposure to high-quality neighborhoods, and early-adult human capital accumulation. A key empirical challenge in our setting is the presence of heterogeneity in the returns to mobility. To address this, we further leverage variation in labor market scarring following the Great Recession.

Our results reveal large, persistent outmigration responses due to parental job loss. In the first year following job loss, children are 130% more likely to leave their initial commuting zone. Consistent with large amounts of spatial heterogeneity, children exposed to job loss experience an additional 0.2 percentage point (6.9%) increase in CZ outmigration for each additional percentage point in labor market scarring. Crucially, while job loss-induced moves lead to neighborhood quality declines on average, families leaving scarred labor markets experience significant improvements: each additional percentage point of origin scarring translates to a 1 percentage point (81.9%) reduction in destination scarring, a 0.6 percentage point (2.6%) increase in the college-educated share, and a 0.1 percentage point (0.9%) decline in poverty rates.

These neighborhood improvements translate to meaningful gains in children's human capital. Using our cross-cohort design, we find that parental job loss has statistically insignificant effects on college attendance on average. However, children in scarred labor markets experience significant increases in college enrollment. These effects occur exclusively at public institutions, a pattern consistent with families leveraging improved neighborhood opportunities while still facing the financial constraints of reduced earnings. While these results find that children exposed to job loss observe a rise in college attendance where outmigration is highest, they do not directly link our mobility channel to rising college attendance. To isolate the causal effect of mobility itself, we employ a movers' design comparing children who leave similarly scarred labor markets at different ages. This analysis reveals even larger effects: moving from a scarred labor market by age 14 increases college attendance by 3.1 percentage points per percentage point of origin scarring.

While we document substantial mobility responses to parental job loss in scarred labor markets, the magnitude of our college attendance effects, combined with the fact that most families do not move, implies that non-movers in scarred areas must also be increasing their educational investments. Using a back of the envelope calculation, non-movers experiencing parental job loss at ages 10-14 increase college attendance by 1.4 percentage points per percentage point of scarring—a response slightly below our reduced-form estimate. Although this does not contradict our core findings, it highlights an important area for future research into the complementary non-mobility channels through which parental job loss in scarred labor markets affects children's human capital formation.

Taken together, our findings emphasize that location matters for determining the optimal policy response. Ignoring location leads policy makers to overlook the divergent effects of parental job loss: while many children may be harmed by job loss-induced mobility away from strong labor markets, but other children may benefit from leaving scarred labor markets. Despite this, narrowly focused on the income channel—whether through unemployment insurance, safety net transfers, or retraining programs designed to speed reemployment. In strong labor markets, these existing income support policies may serve an important function by reducing financial pressures that could force moves which disrupt their children's access to high-quality neighborhoods. In contrast, for families in scarred labor markets, reducing barriers to geographic mobility may offer important relief. Reforms to regulatory barriers, such as reforms to noncompete agreements and occupational licensing requirements have shown promise in increasing mobility (Johnson and Kleiner 2020; Johnson, Lavetti, and Lipsitz 2025), as have policies that reduce housing costs (Ganong and Shoag 2017). Direct relocation assistance could also help families overcome the upfront costs, though such programs are rare in the U.S., and have shown mixed results in Germany for adult workers.[41,42] Our findings suggest these programs could offer a benefit of shielding children from the consequences of parental job loss by supporting access to better neighborhoods.

[41] Caliendo, Künn, and Mahlstedt (2017) find that workers who are induced to move to distant region by relocation assistance observe higher rates of employment and wages. On the other hand, Caliendo, Künn, and Mahlstedt (2022) find that mobility programs also seem to extend unemployment spells.

[42] In the U.S., programs like Trade Adjustment Assistance (TAA) and the Workforce Innovation and Opportunity ACT (WIOA) can offer some financial support for relocation, but are primarily retraining programs (Hyman 2018; Hyman et al. 2025). In 2021, for example, TAA provided relocation allowances to only 66 workers (U.S. Department of Labor 2021). Other forms of relocation assistance have extremely narrow focuses, such as on workers in disaster hit or prone areas, or spousal relocations (Jia et al. 2023).

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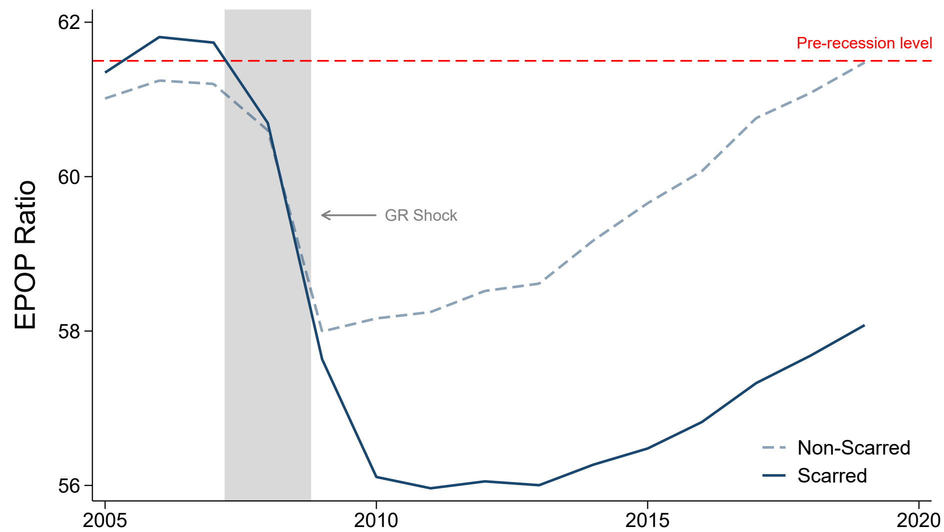
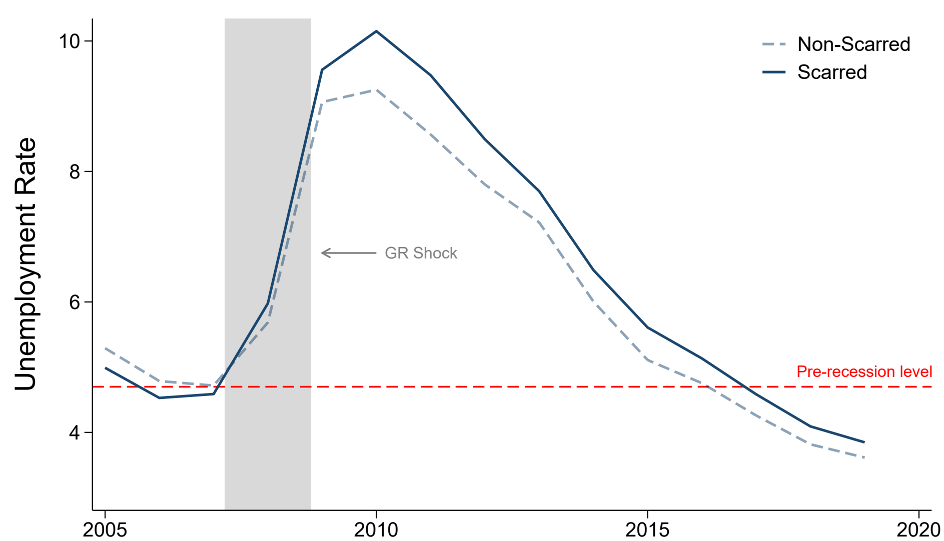
Figure 1: Evolution of Aggregate Measures of U.S. Labor Market Following Great Recession

Panel A: Employment-to-Population Ratio

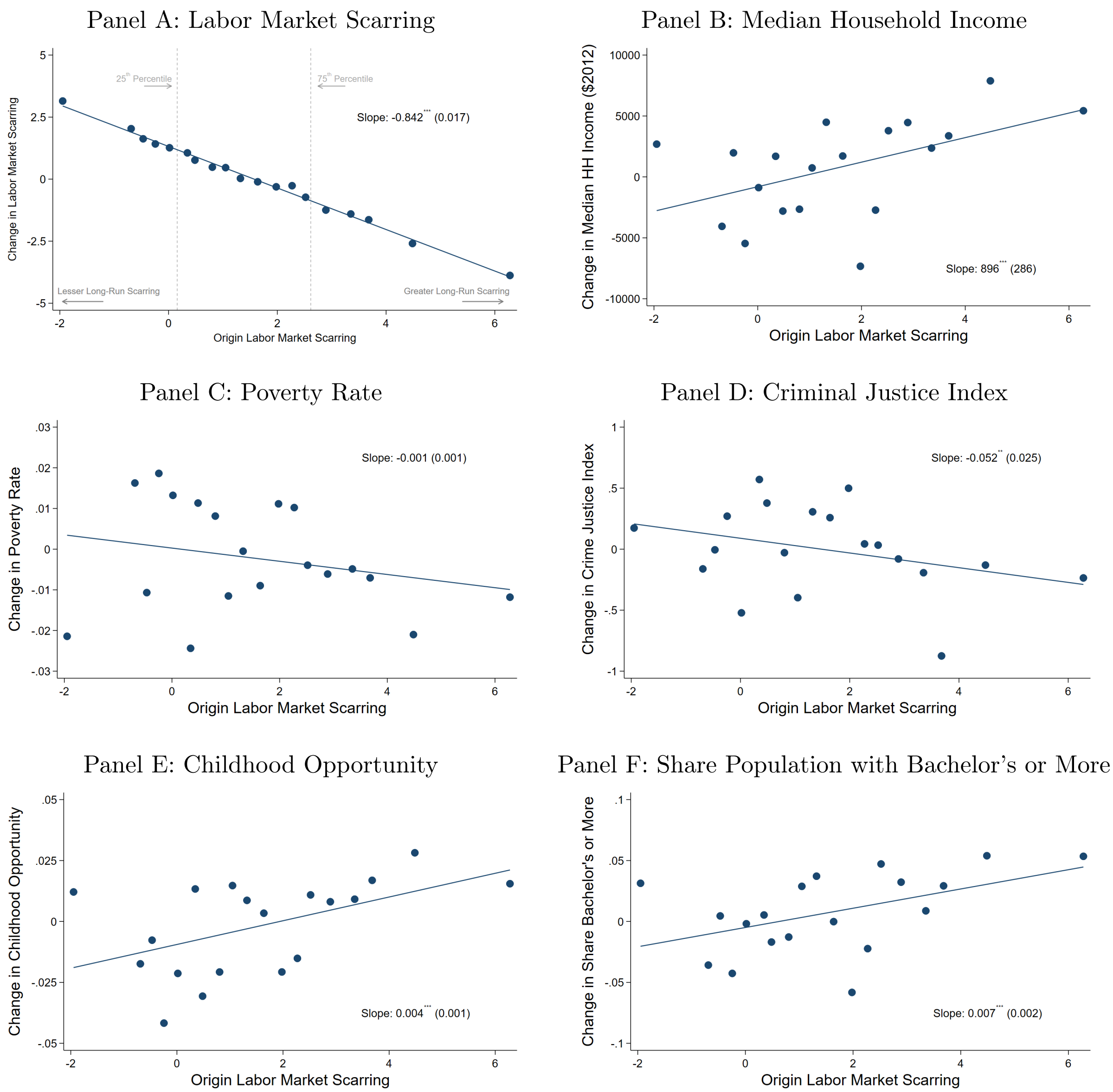


Panel B: Unemployment Rate



Notes: This figure reports time series of aggregate U.S. labor market measures, separately for scarred and non-scarred labor markets. Panel A reports a time series of the average employment-to-population ratio and Panel B reports a time series of the unemployment rate, both split by commuting zones with below and above median labor market scarring as defined in Section 3.2. The shaded region indicates the Great Recession, beginning in December 2007 and ending in June 2009. The red dashed line indicates the average pre-recession level of the series.

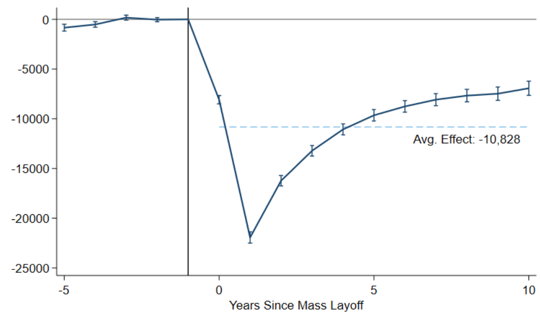
Figure 2: Moving from Scarred Labor Markets Also Improves Neighborhood Quality



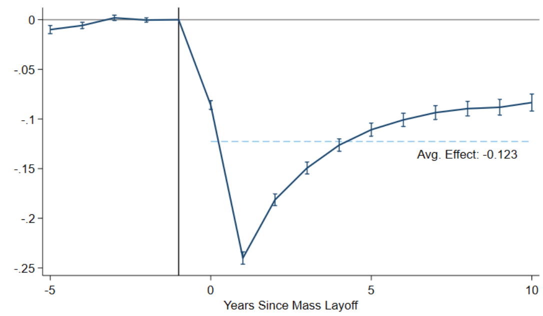
Notes: This figure reports population-weighted binscatters of the outmigration premia against our long-run measure of labor market scarring. The outmigration premia characteristic is listed in the panel title. We calculate the outmigration premia as the difference in neighborhood characteristic between the destination and origin commuting zone, weighted using migration flows from the Internal Revenue Service migration data from 2007 to 2013. Labor market scarring in the origin commuting zone (x-axis) is measured as the percentage point decline in the employment-to-population ratio from 2007 to 2016, as described in Section 3.2. Outmigration premia outcomes are calculated using publicly available statistics from Finlay, Muller-Smith, and the CJARS Team (2024), Chetty et al. (2025), and Manson et al. (2024). Childhood Opportunity is defined as the fraction of children in the top quintile of the household income distribution. Vertical dashed gray lines denote the 25th and 75th percentiles of the population-weighted labor market scarring distribution. Solid lines report lines of best fit. Coefficient estimates report the commuting zone population-weighted relationship between the outmigration premia and the origin labor market scarring measure. Robust standard errors are reported in parentheses. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level.

Figure 3: Parental Job Loss Leads to Lasting Reductions in Parental and Household Earnings

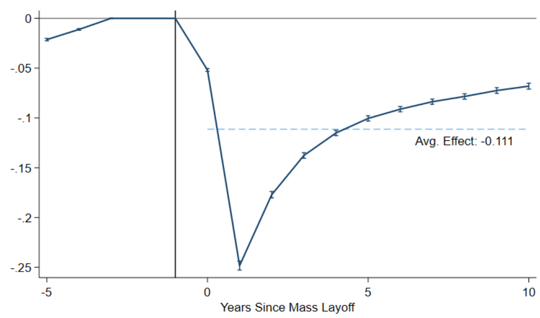
Panel A: Earnings



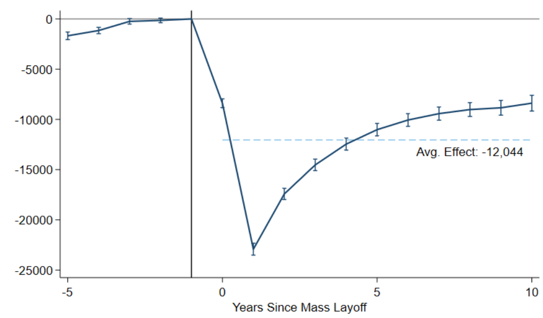
Panel B: Relative Earnings



Panel C: Employment



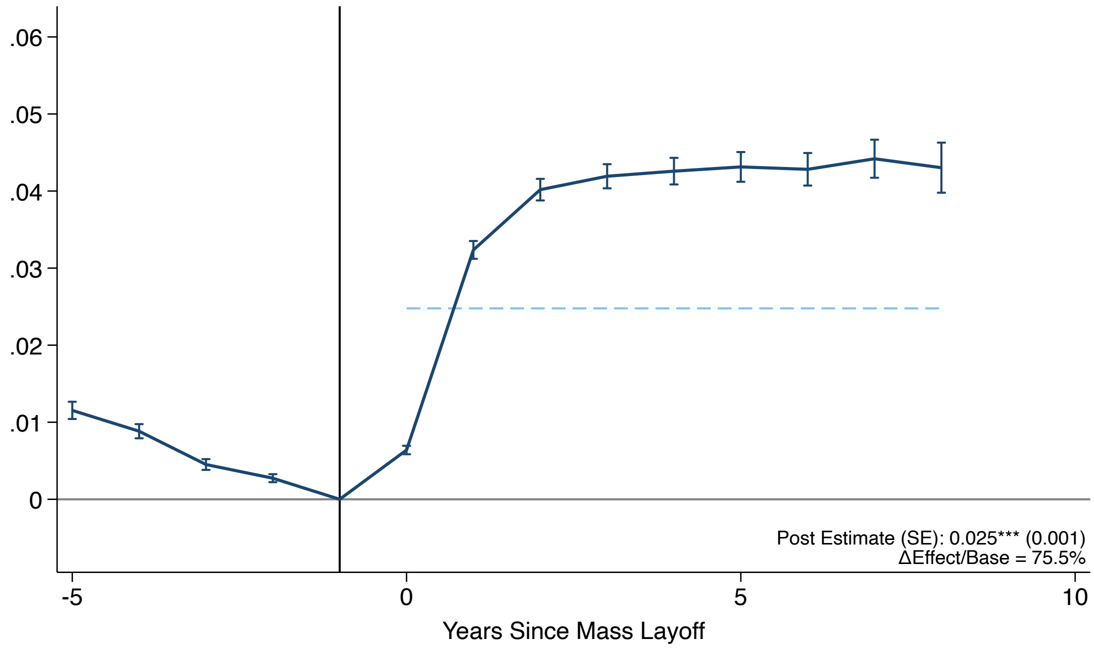
Panel D: Family Income



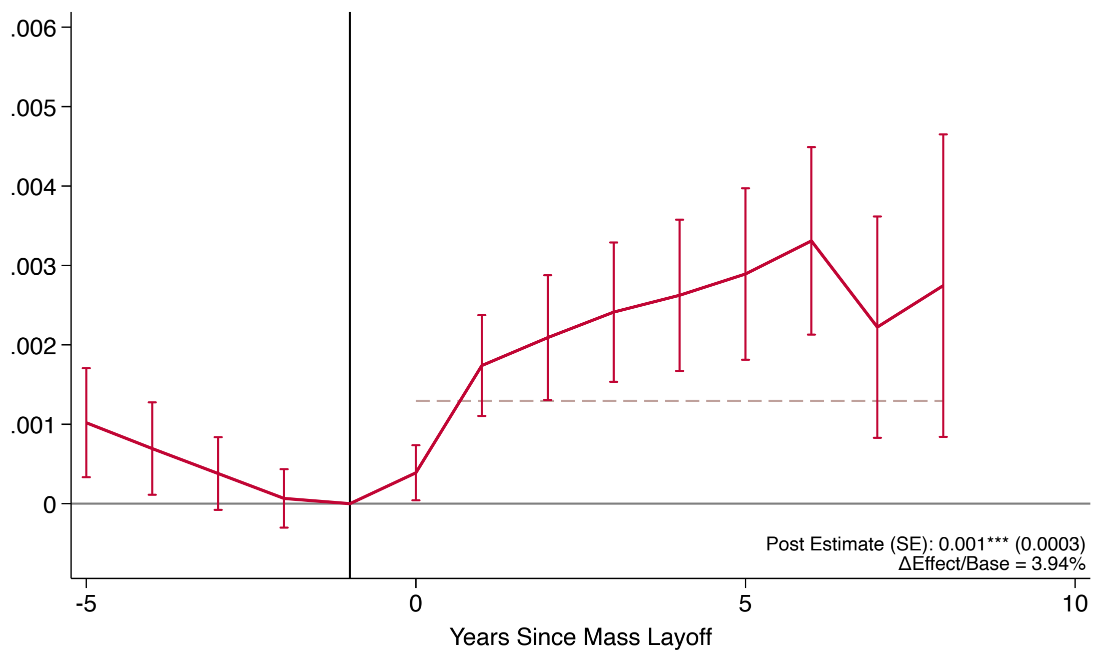
Notes: This figure reports estimates of parental job loss on parental and household earnings and employment. Panel A reports results for earnings, Panel B reports results for earnings relative to the pre-MLE earnings level, Panel C reports results for employment probabilities, and Panel D reports results for family income, defined as the sum of total family earnings. The x-axis corresponds to relative time, years since the mass layoff event. Each point estimate corresponds to the interaction of the treatment indicator and a relative time indicator. Vertical brackets denote 95 percent confidence intervals based on standard errors clustered at the firm by layoff year level. The solid vertical line denotes the year before the layoff. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-P2854-R12604.

Figure 4: Parental Job Loss Leads to a Persistent Rise in Outmigration

Panel A: CZ Outmigration

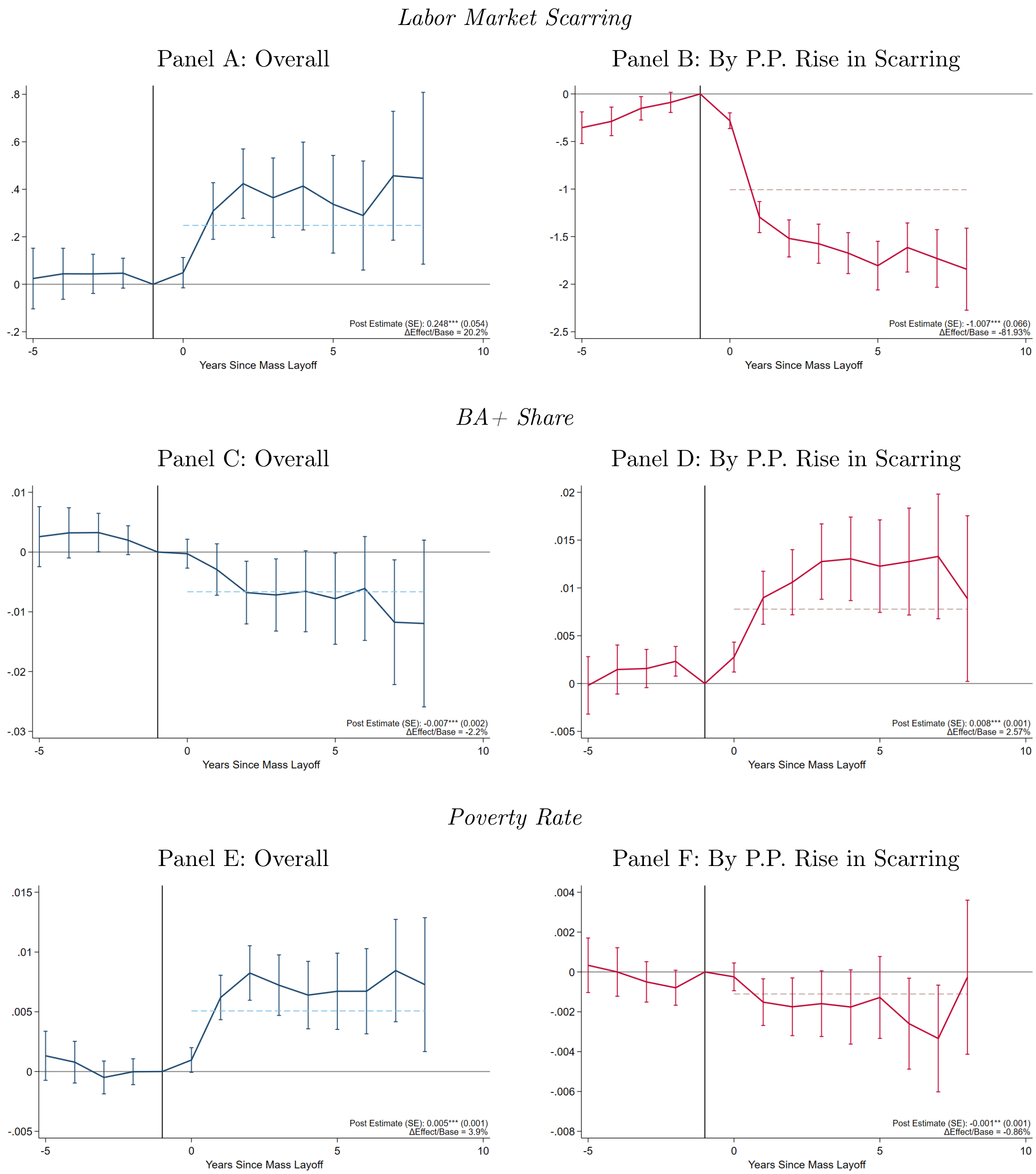


Panel B: CZ Outmigration by Scarring



Notes: This figure reports estimates of parental job loss on household mobility. Panel A reports results for the probability the household leaves their pre-MLE commuting zone and Panel B report the impact on commuting zone outmigration for every percentage point rise in labor market scarring. The x-axis corresponds to relative time, years since the mass layoff event. The sample includes children exposed to parental job loss between the ages of 10 and 18. Horizontal dashed lines indicate the value of the estimate which pools all post-event indicators into a single indicator. Vertical brackets denote 95 percent confidence intervals based on standard errors clustered at the firm by layoff year level. The solid vertical line denotes the year before the layoff. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-P2854-R12604.

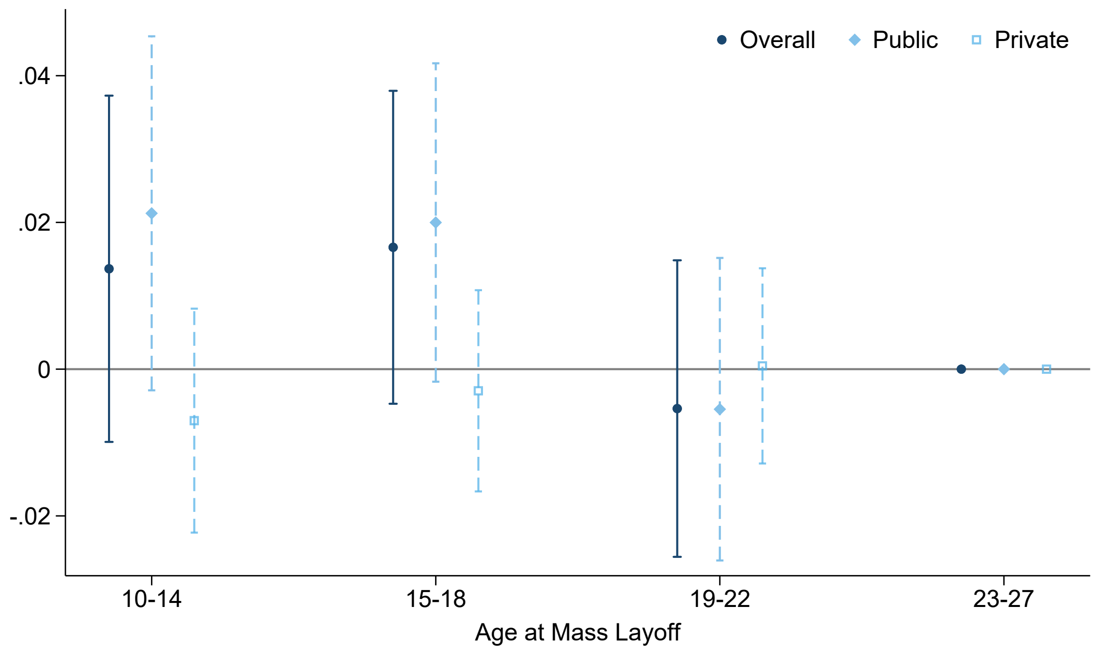
Figure 5: Where Job Loss Occurs Affects Neighborhood Quality



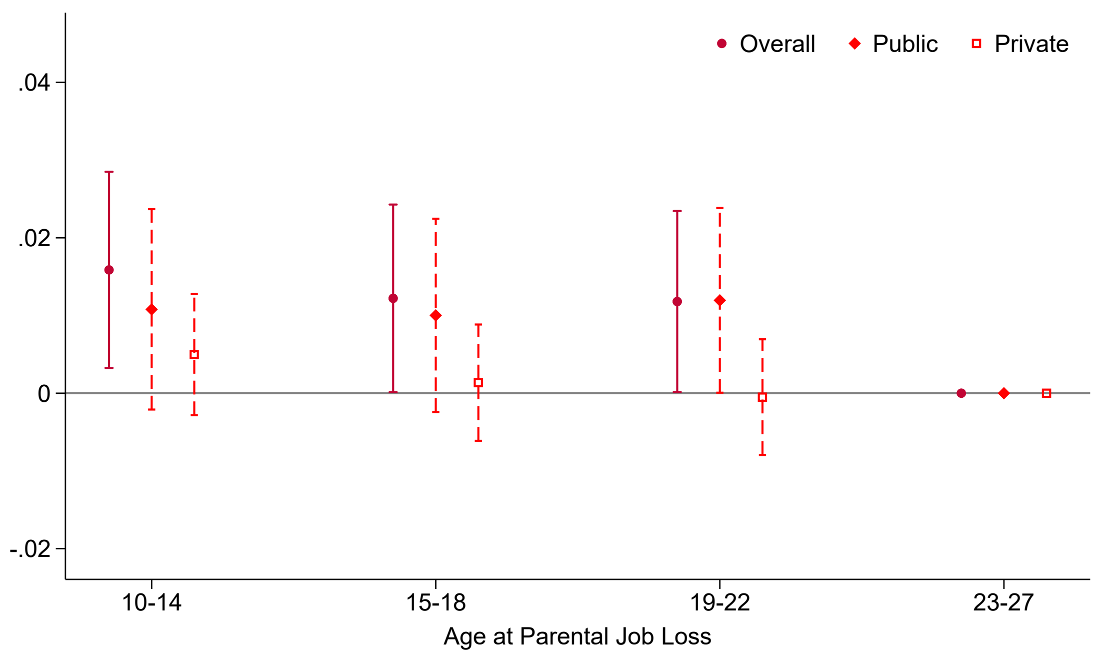
Notes: This figure reports estimates of the interaction of job loss, both ignoring and exploiting long-run Great Recession scarring, on measures of neighborhood quality. Panels A and B reports results for the long-run scarring measure, Panel C and D reports results for the share of the population with a Bachelor's degree or more, and Panel E and F reports results for the poverty rate. Panels A, C, and E report estimates ignoring the role of labor market scarring, focusing on the pooled impact of parental job loss. Each point represents the interaction of a relative time indicator, and an indicator for parental job loss. Panels B, D, and F report estimates exploit variation in labor market scarring, reporting the impact of parental job loss for each percentage point rise in labor market scarring. Each point represents the interaction of a relative time indicator, an indicator for parental job loss, and the scarring measure. The sample includes children exposed to parental job loss between the ages of 10 and 18. Each point reports the estimate scaled by the estimated impact on CZ outmigration, which we discuss in Section 2. Horizontal dashed lines indicate the value of the estimate which pools all post-event indicators into a single indicator. Vertical brackets denote 95 percent confidence intervals based on standard errors clustered at the firm by layoff year level. The solid vertical line denotes the year before the layoff. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-P2854-R12604.

Figure 6: Job Loss in Scarred Labor Markets Increases College Attendance at Age 19-22

Panel A: Overall



Panel B: By Scarring



Notes: This figure reports estimates of the interaction of job loss, both ignoring and exploiting long-run Great Recession scarring, on college attendance at ages 19-22. Panel A report estimates ignoring the role of labor market scarring, focusing on the pooled impact of parental job loss. Each point represents the interaction of a relative time indicator, and an indicator for parental job loss. Panel B report estimates exploiting variation in labor market scarring, reporting the impact of parental job loss for each percentage point rise in labor market scarring. The sample includes children exposed to parental job loss between the ages of 10 and 27. Our reference/placebo group are children exposed to parental job loss at age 23-27. Vertical brackets denote 95 percent confidence intervals based on standard errors clustered at the firm by layoff year level. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-P2854-R12604.

Table 1: Summary Statistics of Layoff Sample

	Layoff		Survivor	
	Pre (1)	Post (2)	Pre (3)	Post (4)
Panel A: Household Resources				
Parental Earnings	81,284	67,999	91,024	87,698
Family Income	111,160	100,021	118,940	118,282
Panel B: Neighborhood Quality				
BA+ Share	0.307	0.308	0.302	0.302
Crime Exposure	−0.166	−0.166	−0.135	-0.134
Homeownership Rate	0.689	0.689	0.694	0.694
Median Monthly Housing Cost	1,116	1,117	1,090	1,093
Poverty Rate	0.129	0.129	0.130	0.129

Notes: This table reports descriptive statistics for our sample of families and children exposed to mass layoff events from 2006-2016. Panel A reports baseline household resources and Panel B reports neighborhood quality. Columns 1 and 3 report descriptive statistics using data in the pre-layoff period and Columns 2 and 4 report descriptive statistics using data from the post-layoff period. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-P2854-R12604.

Table 2: Labor Market Scarring is Correlated with Measures of Neighborhood Quality

	All States			LEHD States		
	Mean	25^{th} Percentile	75^{th} Percentile	Mean	25^{th} Percentile	75^{th} Percentile
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Labor Market Scarring						
$\Delta EPOP_{2007-2016}$	1.519	0.159	2.615	1.307	0.045	2.352
Panel B: Alternative Recession Shock Measures						
$\Delta EPOP_{2007-2009}$	2.689	1.965	2.882	2.491	1.899	2.603
$\Delta UR_{2007-2009}$	4.661	4.229	5.096	4.525	4.104	5.035
Percent Change in Home Prices (2007-2016)	−13.382	−7.079	−16.749	−13.267	−17.069	−16.158
Panel C: Neighborhood Quality						
Childhood Opportunity	0.213	0.239	0.194	0.223	0.245	0.205
Share with Bachelor's or More	0.296	0.329	0.254	0.304	0.313	0.257
Unemployment Rate	0.079	0.077	0.083	0.077	0.073	0.083
Poverty Rate	0.148	0.137	0.154	0.141	0.121	0.150
Median HH Income ($2012)	55,662	60,541	49,889	57,837	60,208	51,092
Criminal Justice Index	−0.008	−0.410	0.088	0.046	0.480	0.146

Notes: This table reports commuting zone characteristics overall and at the 25^{th} and 75^{th} percentile of the long-run labor market scarring measure. The labor market scarring measure is calculated following the procedure described in Section 3.2. Panel A reports the labor market scarring measure, Panel B reports alternative, short-run measures of the Great Recession's impact on labor markets, and Panel C reports neighborhood characteristics. Changes in EPOP and UR are calculated using BLS LAUS, with the former using NCI SEER program population data as well. Changes in home prices are derived from Zillow county-level home price data. In Panel C, except for our criminal justice index and childhood opportunity measure, data is from Manson et al. (2024) pooling 5-Year ACS waves 2008-2012 and 2013-2017. We construct our criminal justice index using Finlay, Muller-Smith, and the CJARS Team (2024). Childhood Opportunity is defined as the fraction of children in the top quintile of the household income distribution and sourced from Chetty et al. (2025). The first three columns report results for using all states and the last three columns report results using the 29 states in our LEHD sample. Columns 2 and 5 report commuting zone characteristics at the 25^{th} percentile of the scarring distribution and Columns 3 and 6 report commuting zone characteristics at the 75^{th} percentile of the scarring distribution. To improve stability, we calculate these characteristics using a bandwidth of five percentile ranks around the focal percentile. All statistics weighted using commuting zone population in 2007.

Table 3: Impact of Parental Job Loss on Neighborhood Quality

	Pooled		By Scarring	
	Scaled β	$\Delta\%$	Scaled β	$\Delta\%$
	(1)	(2)	(3)	(4)
Change EPOP	0.248***	20.18%	−1.007***	–
	(0.054)		(0.066)	
BA+ Share	−0.007***	−2.20%	0.008***	2.57%
	(0.002)		(0.001)	
Poverty Rate	0.005***	3.92%	−0.001**	−0.86%
	(0.001)		(0.001)	
Pre-K Share	0.002*	0.55%	0.006***	1.41%
	(0.001)		(0.001)	
Child Poverty	0.007***	3.80%	−0.002**	−0.84%
	(0.001)		(0.001)	
Log Median Monthly Housing Cost	−0.087***	–	0.015***	–
	(0.007)		(0.004)	
Log Median Personal Income	−0.031***	–	0.011***	–
	(0.004)		(0.003)	
Criminal Justice Index (Age 19-22)	0.025	–	−0.059***	–
	(0.019)		(0.012)	
Shock IQR	–		2.456	

Notes: This table reports estimates of parental job loss on children's neighborhood quality up through age 18, both ignoring and exploiting long-run Great Recession scarring. Each cell reports the estimate scaled by the estimated impact on CZ outmigration, which we discuss in 2. We also report the implied percentage change, dividing the estimated scaled coefficient by the average of corresponding outcome. Columns 1 and 2 report estimates ignoring the role of labor market scarring, focusing on the pooled impact of parental job loss. Each point represents the interaction of a relative time indicator, and an indicator for parental job loss. Columns 3 and 4 report estimates exploiting variation in labor market scarring, reporting the impact of parental job loss for each percentage point rise in labor market scarring. At the bottom of the table, we report the interquartile range of the scarring measure, which is the difference between the scarring measure at the 75$^{\text{th}}$ percentile and at the 25$^{\text{th}}$ percentile. Standard errors clustered at the firm by layoff year level are reported in parentheses. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-P2854-R12604.

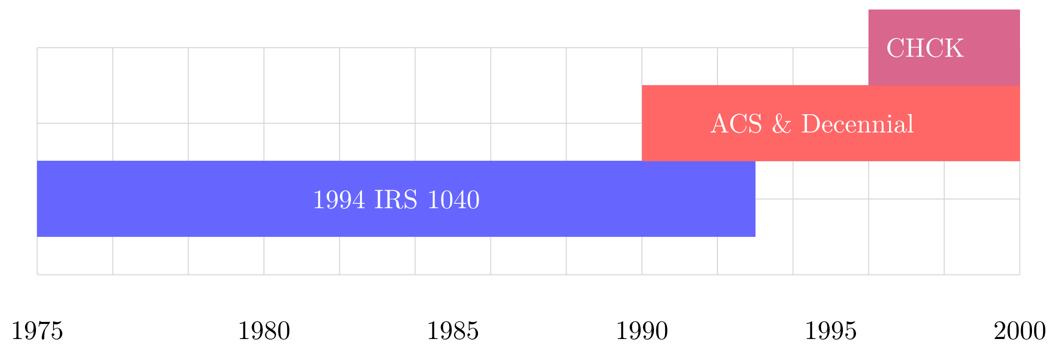
Table 4: Impact of Job Loss-Induced Outmigration on College Attendance

	(1)	(2)
Panel A: Moves ≤ 2 Years Post-MLE		
Moved Age 10–14 × Scarring	0.031**	0.028**
	(0.013)	(0.013)
Observations	1,600	1,600
Panel B: Moves ≤ 2 Years Post-MLE, No Pre-Moves		
Moved Age 10–14 × Scarring	0.032**	0.028*
	(0.016)	(0.017)
Observations	1,200	1,200
Controls		
Baseline Specification	X	X
Firm Controls		X
Shock IQR	2.456	

Notes: This table reports estimates of a movers design leveraging children who move across commuting zones at different ages. The outcome for all point estimates is the probability of college attendance during ages 19-22. Each cell is from a separate regression and corresponds to the coefficient on the interaction listed in the row. In Panel A, the sample is restricted to children of job-losing parents who move across commuting zones within two years after the mass layoff event. Panel B imposes a further restriction of no moves across commuting zones in the three years prior to the job loss event. Column 1 presents the baseline specification and Column 2 adds controls for firm characteristics. Standard errors clustered at the firm-level are reported in parentheses. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-P2854-R12604.

Appendix A Additional Results

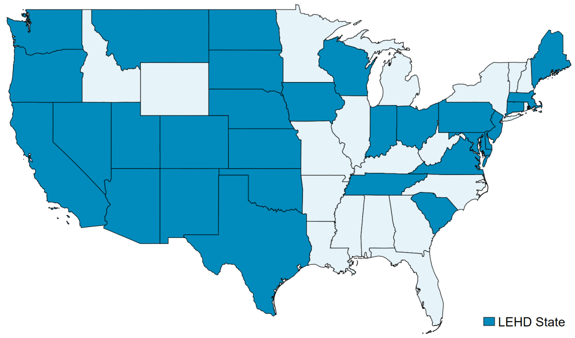
Appendix Figure A1: Parent-Child Data Coverage for Birth Cohorts 1975–2000



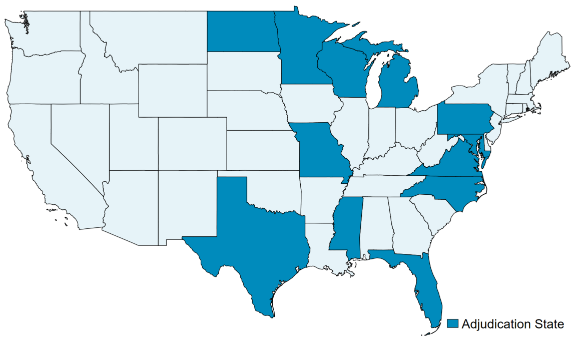
Notes: This figure depicts data coverage identifying parent-child links for our birth cohorts up to 2000. The Census Household Composition Key (CHCK) provides coverage for 1997–2000 births through birth certificates and SSN applications. The ACS (2001–2019) and Decennial Census (2000, 2010) cover 1991–2000 cohorts by identifying co-residing children age $\leq$19. The 1994 IRS Form 1040 captures 1975–1994 cohorts through dependent claims. Overlapping coverage periods (1991–1994, 1997–2000) enable cross-validation of parent-child linkages. Our final sample includes those born between 1979–2000, who were ages 10–27 during the 2006–2016 mass layoff observation period.

Appendix Figure A2: LEHD States

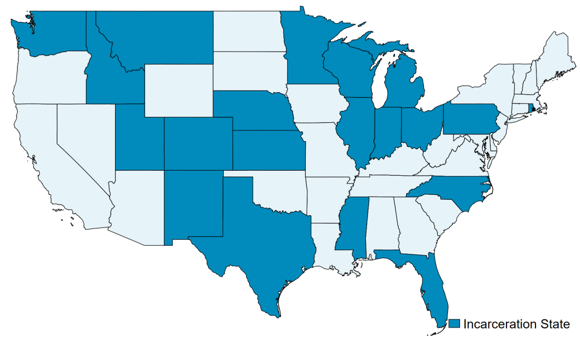
Panel A: LEHD Sample



Panel B: CJARS Adjudication Sample

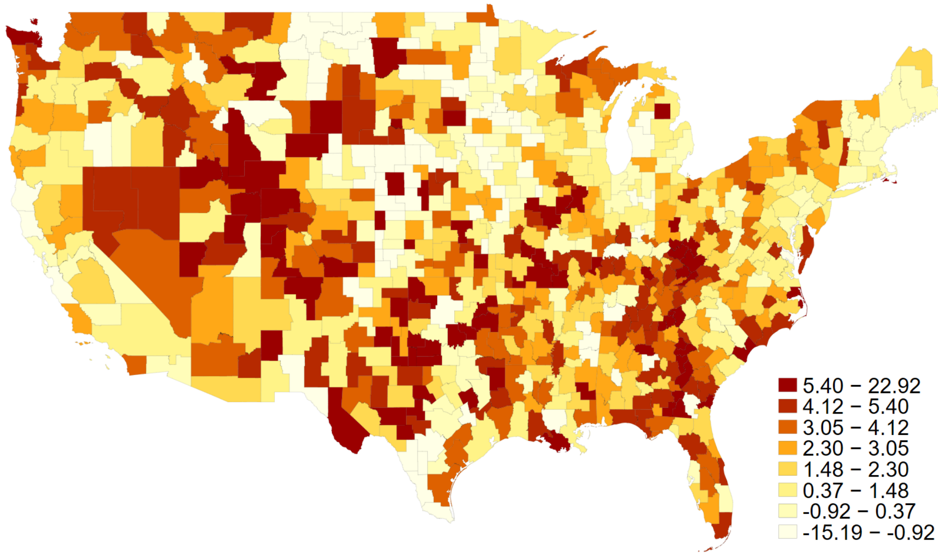


Panel C: CJARS Incarceration Sample



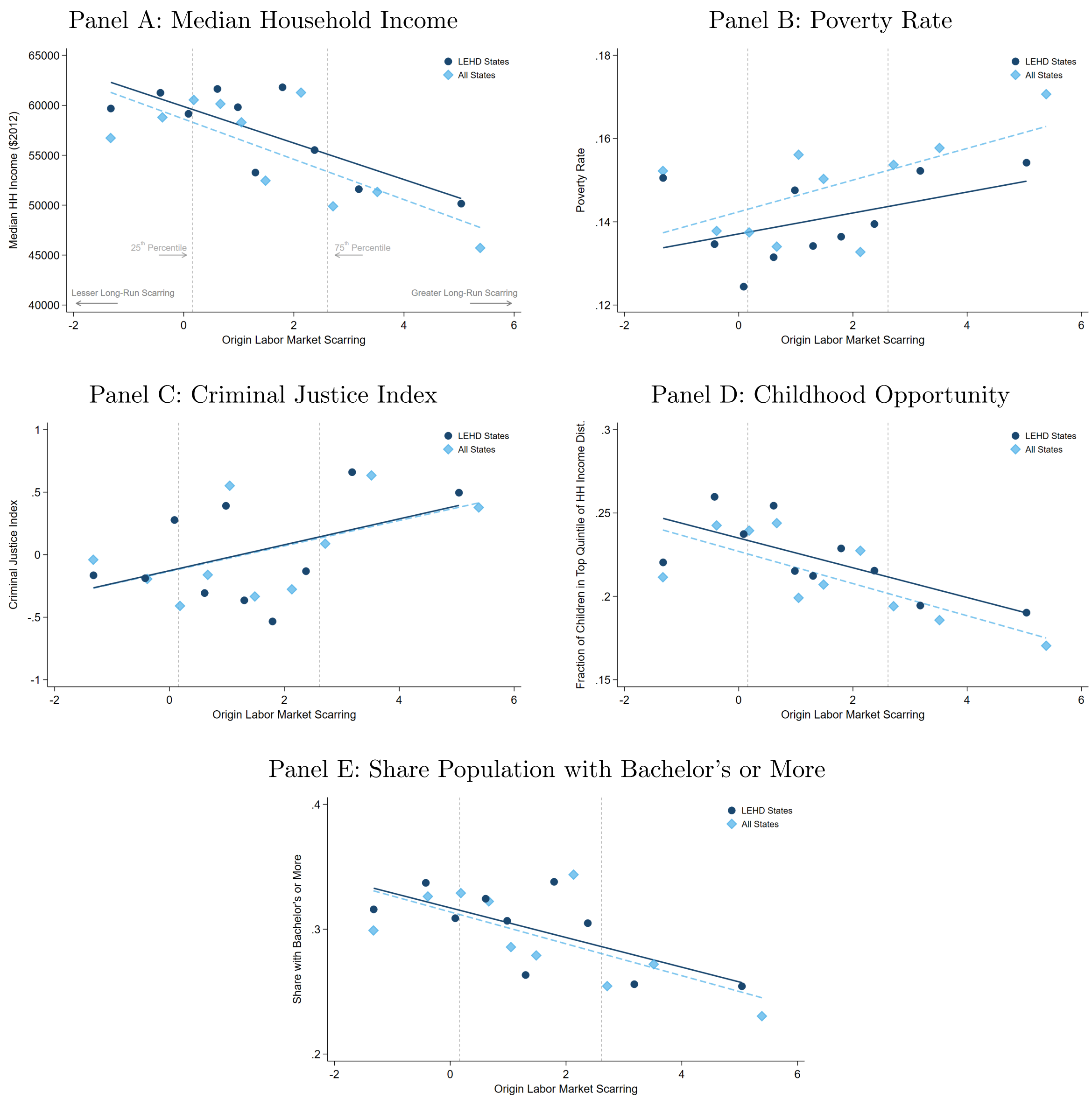
Notes: This figure identifies the 29 states for which we have Longitudinal Employer Household Dynamics (LEHD) data, as well as the states for which we have CJARS coverage. CJARS coverage varies by data source. Adjudication refers to records which allow us to identify charges. Incarceration refers to records which allow us to identify incarceration. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-P2854-R12604.

Appendix Figure A3: Geographic Dispersion of Labor Market Scarring



Notes: This figure reports the geographic distribution of our labor market scarring member at the commuting zone level. Labor market scarring is measured as the percentage point decline in the employment-to-population ratio from 2007 to 2016, as described in Section 3.2. Darker red regions indicate commuting zones with more severe long-run labor market scarring, while lighter shaded regions indicate commuting zones with less severe long-run labor market scarring.

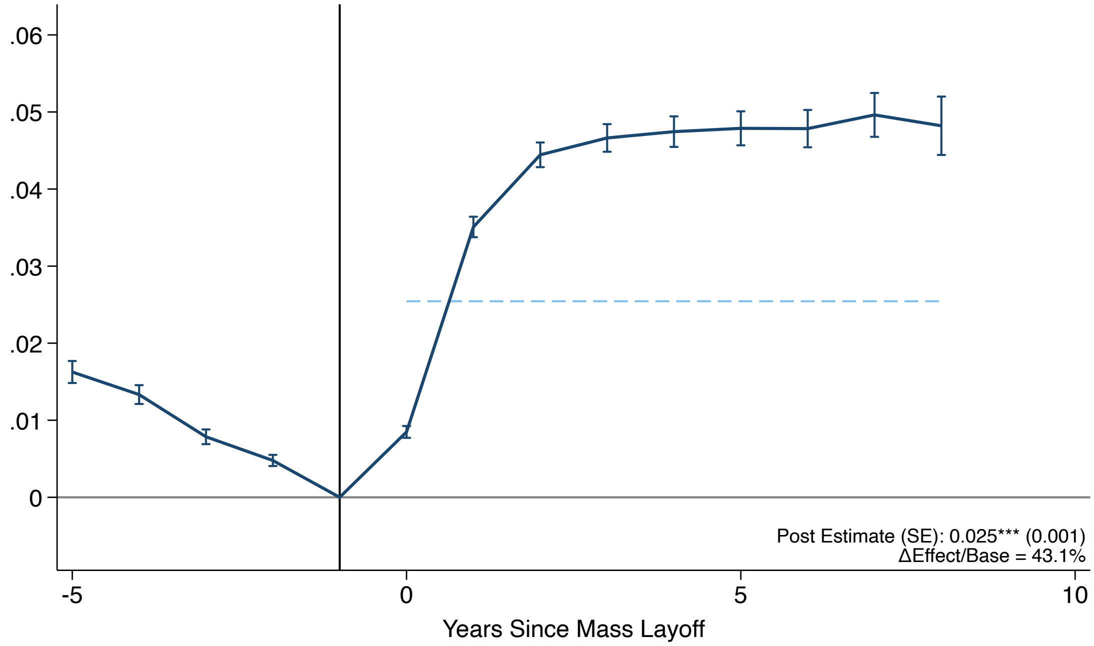
Appendix Figure A4: Relationship Between Labor Market Scarring and Neighborhood Quality



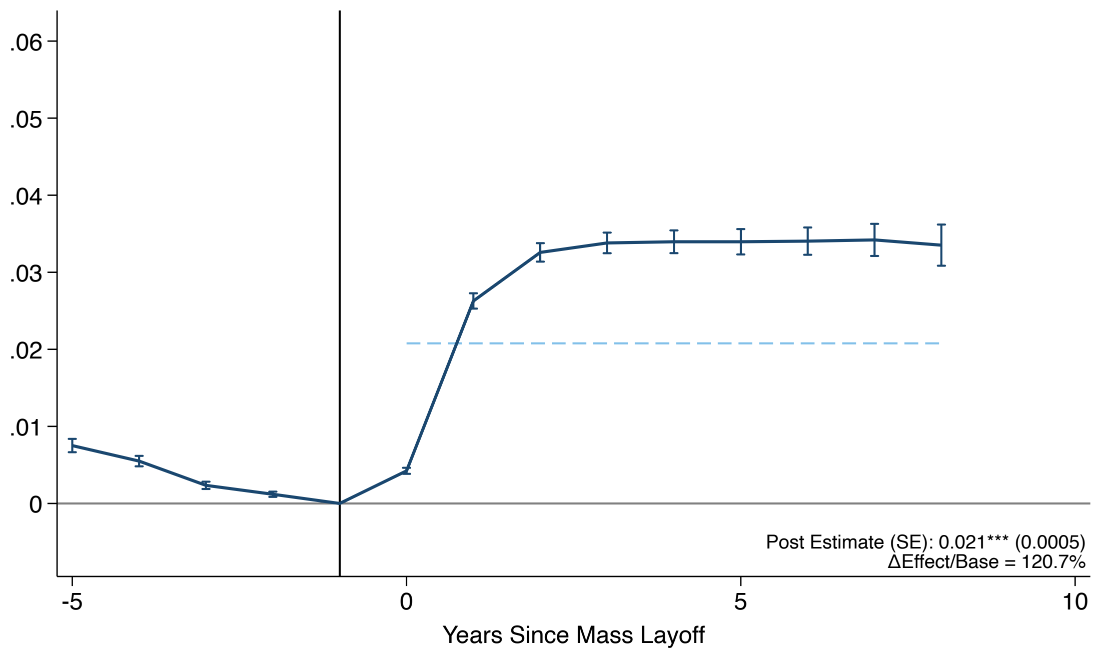
Notes: This figure reports binscatters of measures of neighborhood quality and our measure of labor market scarring at the commuting zone level. The measure of neighborhood quality is listed on the y-axis and in each panel title. The long-run measure of labor market scarring, Section 3.2, is listed on the x-axis. The dark blue circles indicate the estimated relationship using commuting zones from our 29 LEHD states. The light blue diamonds indicate the estimated relationship using all commuting zones in the U.S. Vertical dashed gray lines indicate the 25$^{\text{th}}$ and 75$^{\text{th}}$ percentiles of the scarring distribution. Solid and dashed blue lines indicate lines of best fit. Underlying data and lines of best fit are weighted using commuting zone population.

Appendix Figure A5: County and State Outmigration

Panel A: County Outmigration



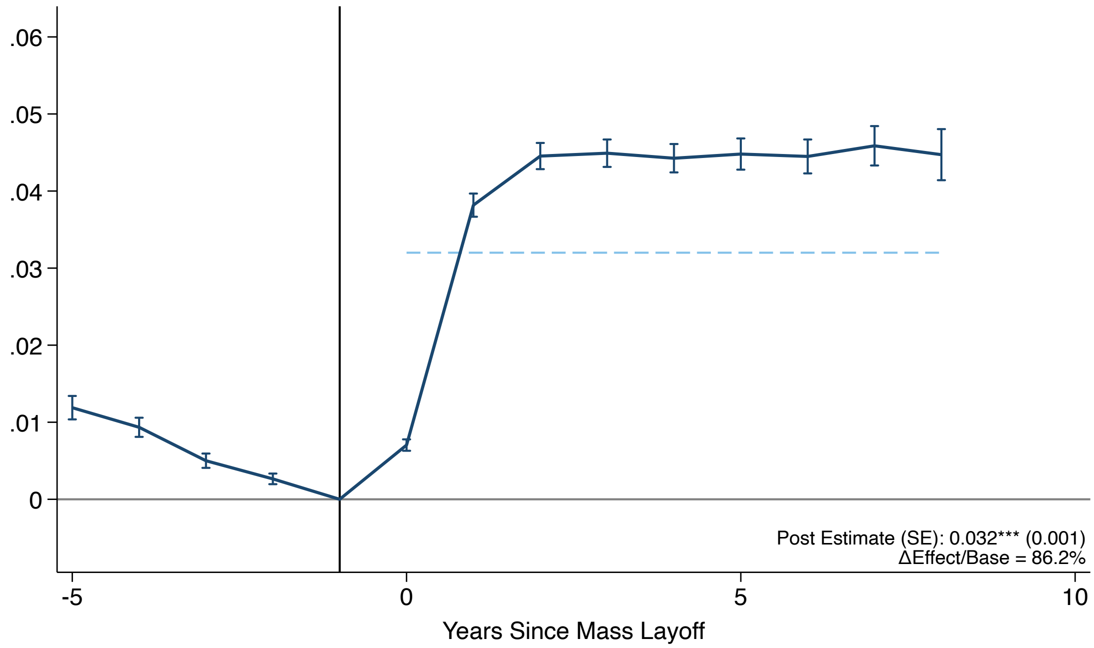
Panel B: State Outmigration



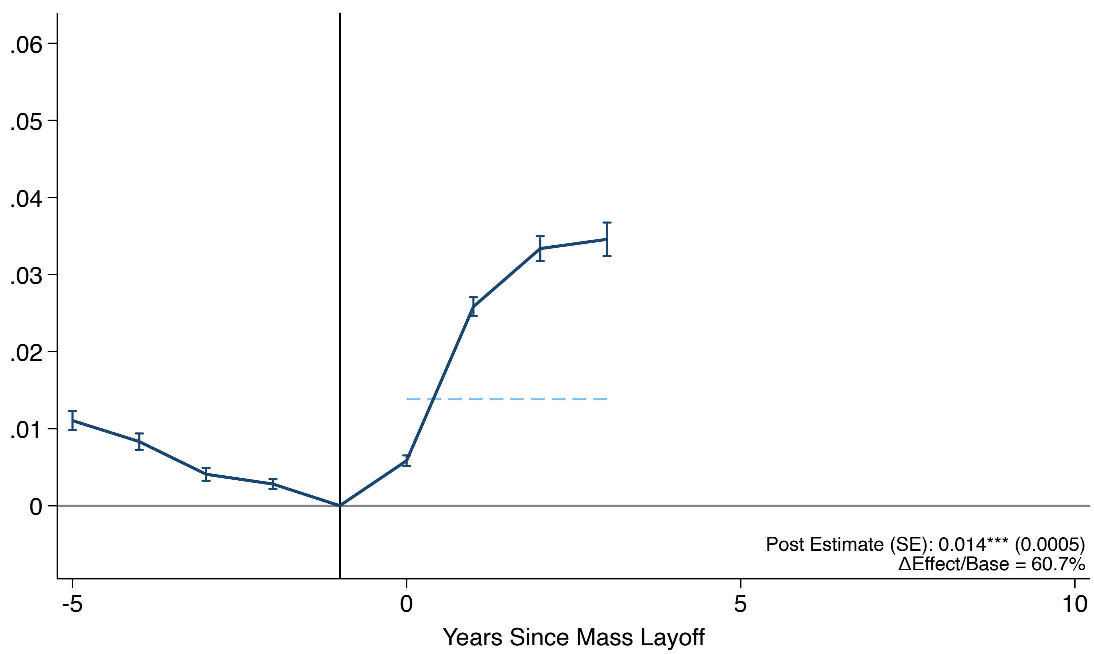
Notes: This figure reports estimates of parental job loss on measures of household mobility. Panel A reports results for the probability the household leaves their pre-MLE county, and Panel B reports results for the probability the households leaves their pre-MLE state. The x-axis corresponds to relative time, years since the mass layoff event. Each point estimate corresponds to the interaction of the treatment indicator and a relative time indicator. The sample includes children exposed to parental job loss between the ages of 10 and 18. Horizontal dashed lines indicate the value of the estimate which pools all post-event indicators into a single indicator. Vertical brackets denote 95 percent confidence intervals based on standard errors clustered at the firm by layoff year level. The solid vertical line denotes the year before the layoff. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-P2854-R12604.

Appendix Figure A6: CZ Outmigration, by Age at Exposure

Panel A: Age 10-14



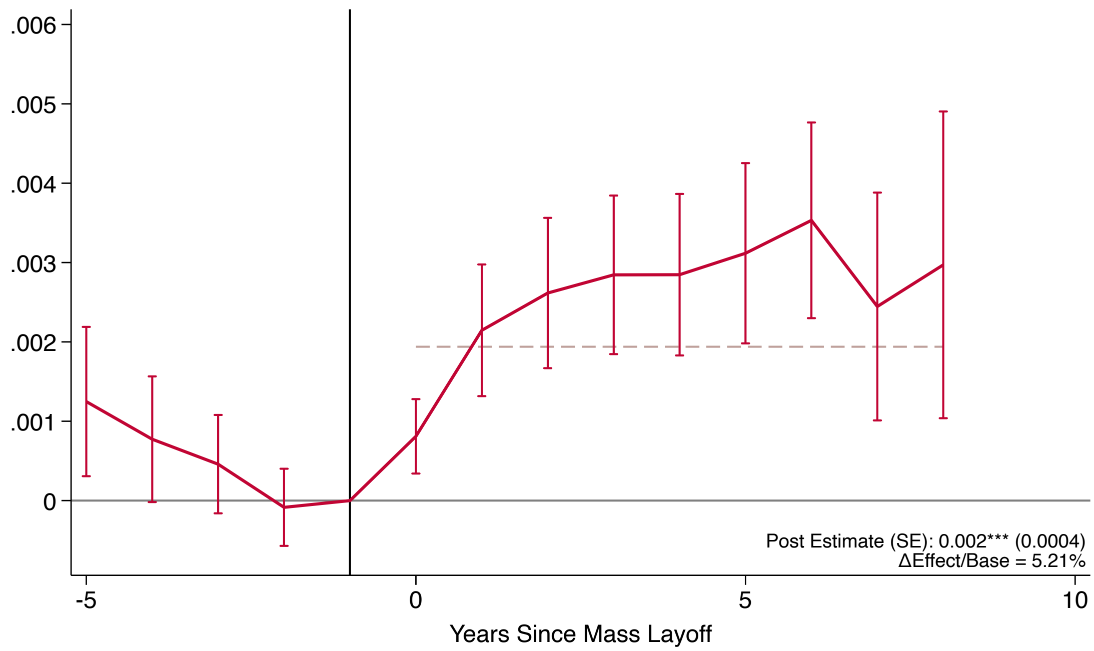
Panel B: Age 15-18



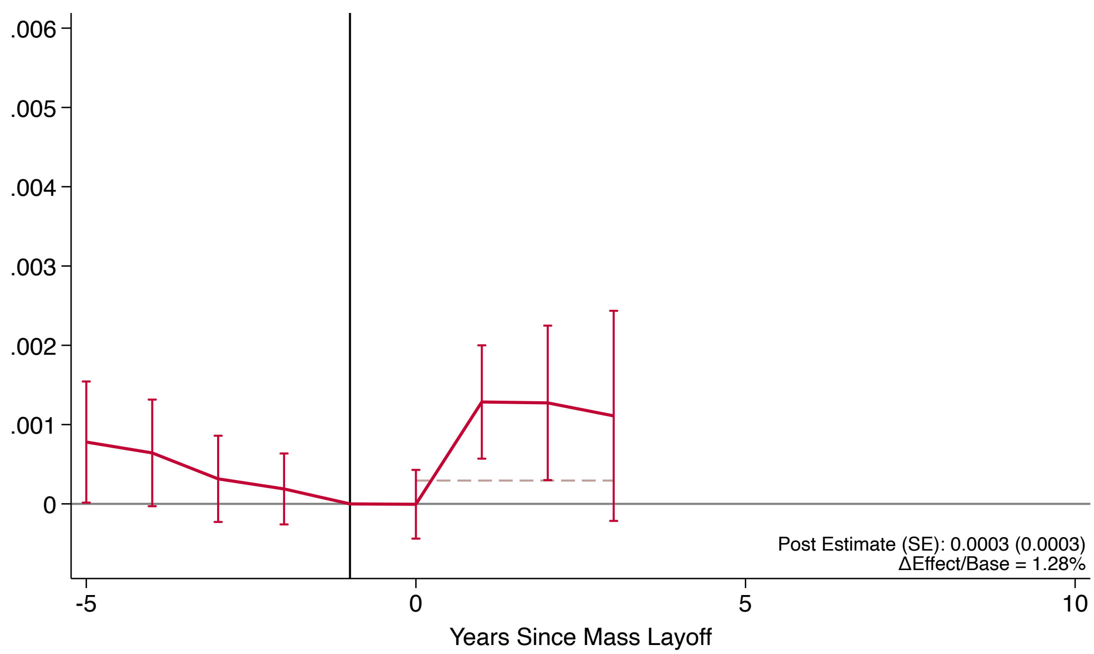
Notes: This figure reports estimates of parental job loss on the probability of the household leaving their pre-MLE commuting zone. Panel A reports results for children ages 10-14 at the time of the layoff and Panel B reports similar results for children ages 15-18. The x-axis corresponds to relative time, years since the mass layoff event. Horizontal dashed lines indicate the value of the estimate which pools all post-event indicators into a single indicator. Vertical brackets denote 95 percent confidence intervals based on standard errors clustered at the firm by layoff year level. The solid vertical line denotes the year before the layoff. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-P2854-R12604.

Appendix Figure A7: CZ Outmigration Per P.P. Rise in Scarring, by Age at Exposure

Panel A: Age 10-14



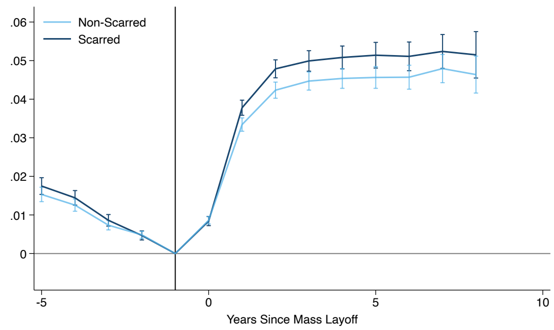
Panel B: Age 15-18



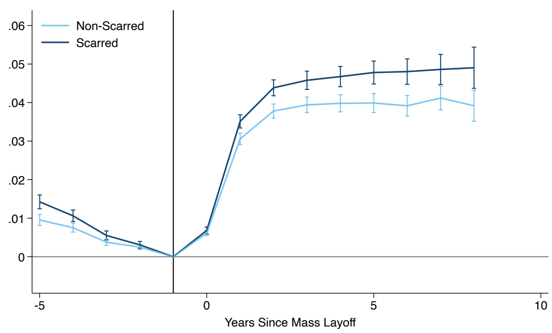
Notes: This figure reports estimates of parental job loss on the probability of the household leaving their pre-MLE commuting zone. Panel A reports results for each percentage point rise in in labor market scarring for children ages 10-14 at the time of the layoff and Panel B reports similar results for children ages 15-18. The x-axis corresponds to relative time, years since the mass layoff event. Horizontal dashed lines indicate the value of the estimate which pools all post-event indicators into a single indicator. Vertical brackets denote 95 percent confidence intervals based on standard errors clustered at the firm by layoff year level. The solid vertical line denotes the year before the layoff. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-P2854-R12604.

Appendix Figure A8: Job Loss Impact on CZ Outmigration by Above/Below Median Scarring

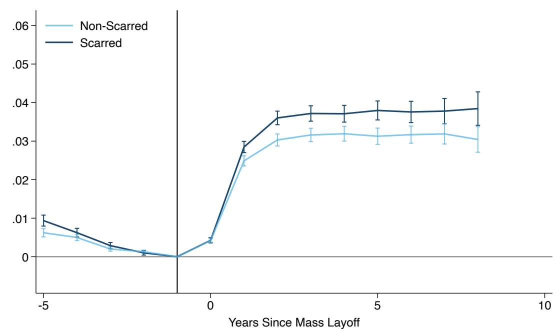
Panel A: CZ Outmigration



Panel B: County Outmigration

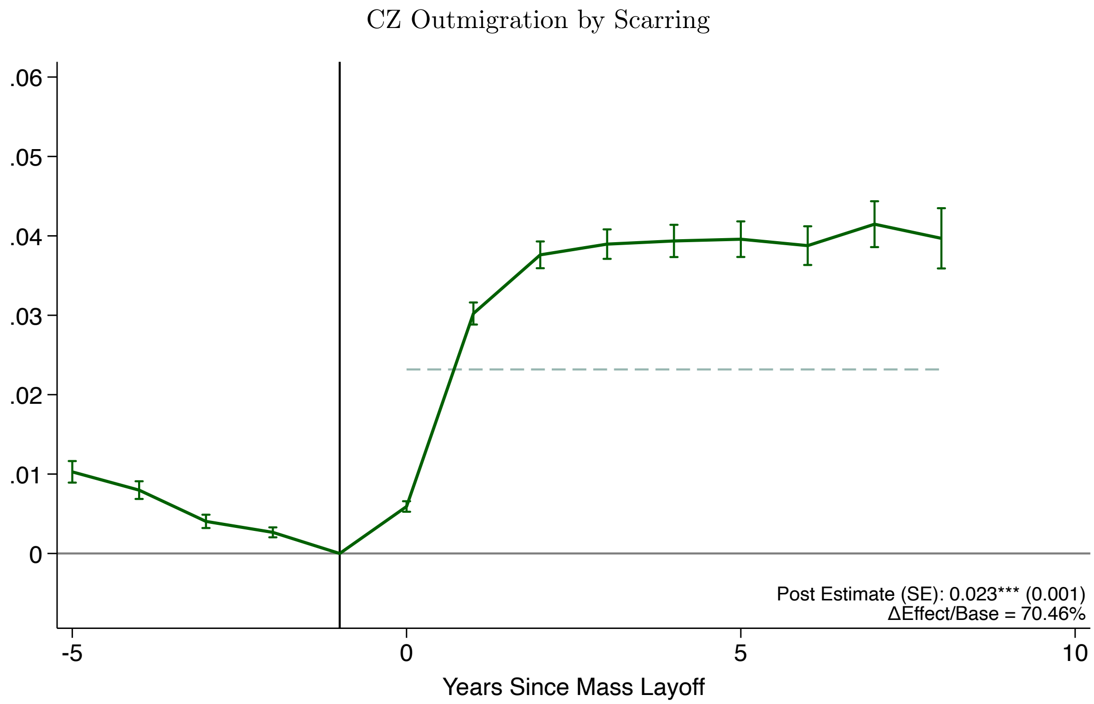


Panel C: State Outmigration



Notes: This figure reports estimates of parental job loss on measures of household mobility, estimated separately for households in scarred (dark blue) and non-scarred (light blue) labor markets, where scarred denotes above-median scarring and non-scarred denotes below-median scarring. The definition of labor market scarring is described in Section 3.2. Panel A reports results for commuting zone outmigration, Panel B reports results for county outmigration, and Panel C reports results for state outmigration. The x-axis corresponds to relative time, years since the mass layoff event. Each point estimate corresponds to the interaction of the treatment indicator and a relative time indicator. Brackets denote 95 percent confidence intervals based on standard errors clustered at the firm by layoff year level. The solid vertical line denotes the year before the layoff. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-P2854-R12604.

Appendix Figure A9: Baseline Job Loss Effect from Triple DiD

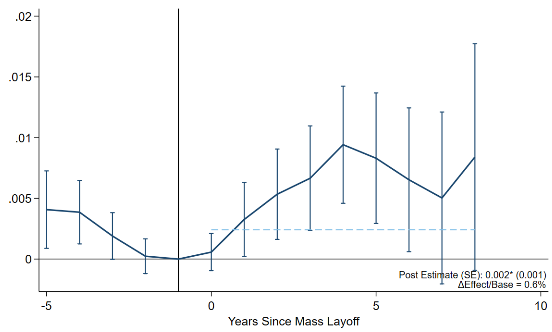


Notes: This figure reports baseline effects of parental job loss on commuting zone outmigration from Equation 2. The x-axis corresponds to relative time, years since the mass layoff event. The sample includes children exposed to parental job loss between the ages of 10 and 18. Horizontal dashed lines indicate the value of the estimate which pools all post-event indicators into a single indicator. Vertical brackets denote 95 percent confidence intervals based on standard errors clustered at the firm by layoff year level. The solid vertical line denotes the year before the layoff. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-P2854-R12604.

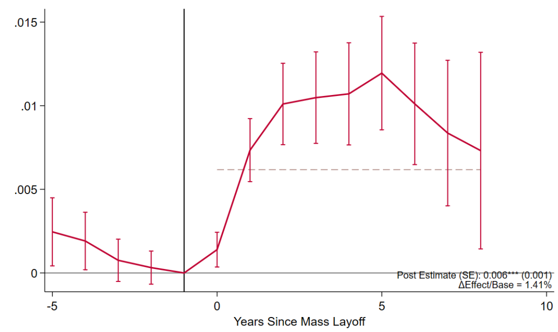
Appendix Figure A10: Job Loss Effect on Neighborhood Quality

Pre-K Share

Panel A: Overall

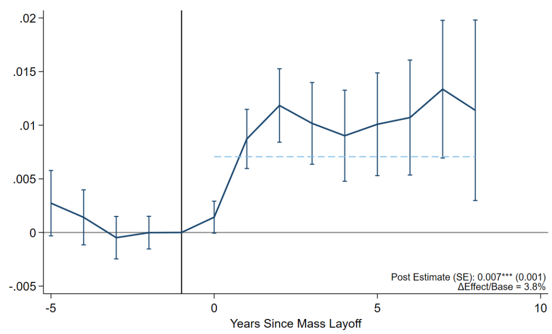


Panel B: By P.P. Rise in Scarring

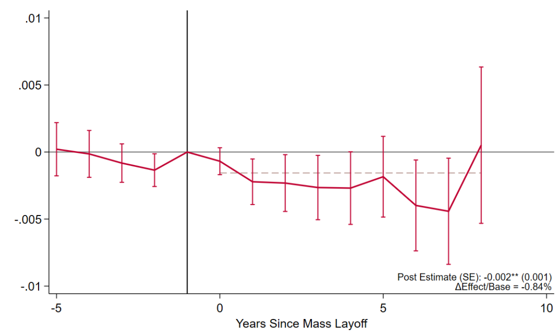


Child Poverty

Panel C: Overall

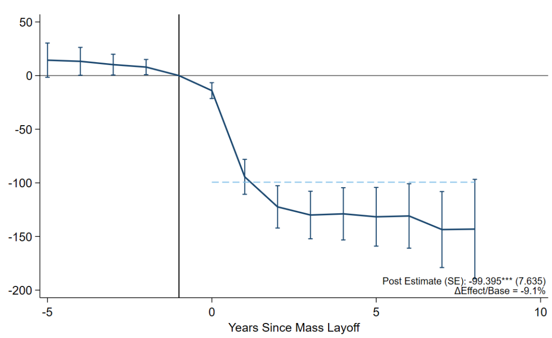


Panel D: By P.P. Rise in Scarring



Log Median Monthly Housing Cost

Panel E: Overall



Panel F: By P.P. Rise in Scarring

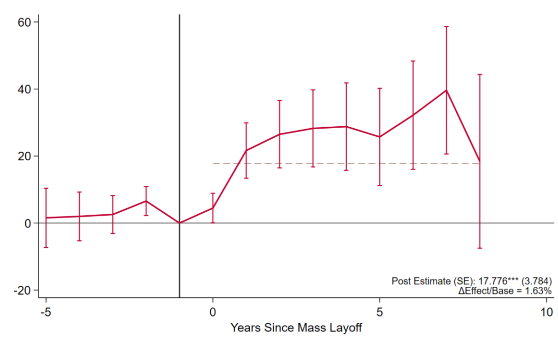
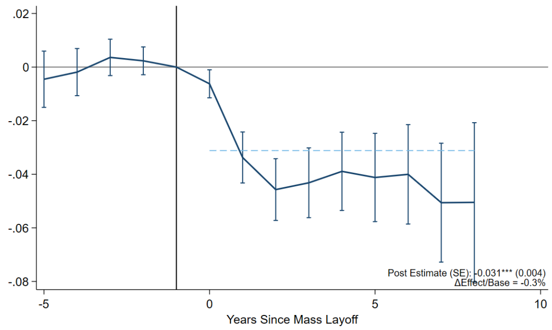


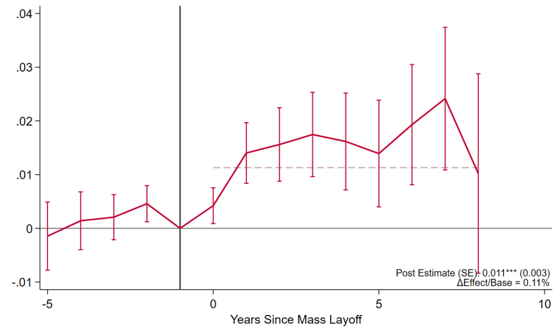
Figure A10: Job Loss Effect on Neighborhood Quality (continued)

Log Median Personal Income

Panel G: Overall

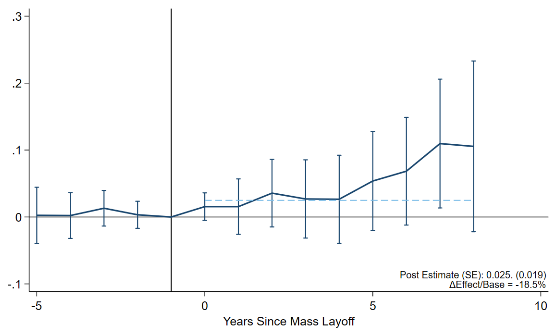


Panel H: By P.P. Rise in Scarring

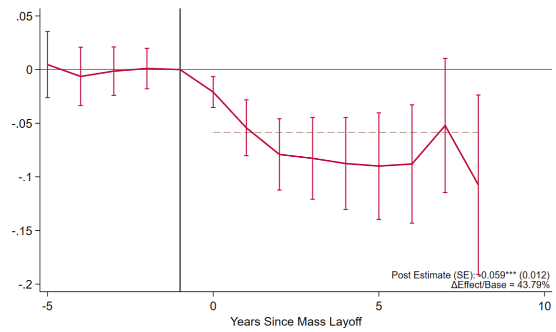


Criminal Justice Index (Age 19-22)

Panel I: Overall



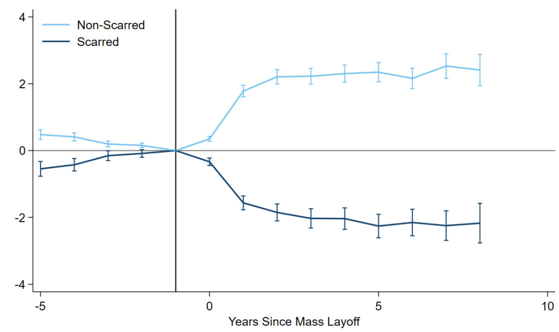
Panel J: By P.P. Rise in Scarring



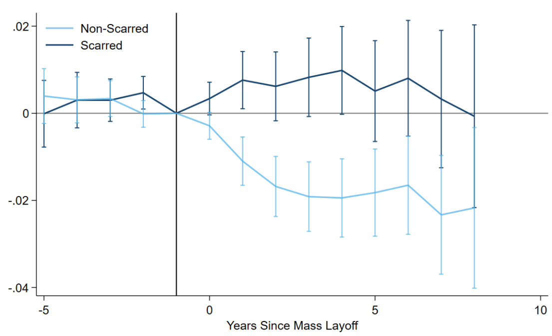
Notes: This figure reports estimates of the interaction of job loss, both ignoring and exploiting long-run Great Recession scarring, on measures of neighborhood quality. Panels A and B reports results for the share of children age 4-5 in Pre-K; Panel C and D reports results for the share of children in poverty; Panel E and F reports results for log median monthly housing cost; Panel G and H reports results for log median personal income; and, Panel I and J reports results for our Criminal Justice Index (Ages 19-22). Panels A, C, E, G, and I report estimates ignoring the role of labor market scarring, focusing on the pooled impact of parental job loss. Each point represents the interaction of a relative time indicator, and an indicator for parental job loss. Panels B, D, F, H, and J report estimates exploit variation in labor market scarring, reporting the impact of parental job loss for each percentage point rise in labor market scarring. Each point represents the interaction of a relative time indicator, an indicator for parental job loss, and the scarring measure. The sample includes children exposed to parental job loss between the ages of 10 and 18. Each point reports the estimate scaled by the estimated impact on CZ outmigration, which we discuss in Section 2. Horizontal dashed lines indicate the value of the estimate which pools all post-event indicators into a single indicator. Vertical brackets denote 95 percent confidence intervals based on standard errors clustered at the firm by layoff year level. The solid vertical line denotes the year before the layoff. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-P2854-R12604.

Appendix Figure A11: Job Loss Impact on Neighborhood Quality by Above/Below Median Scarring

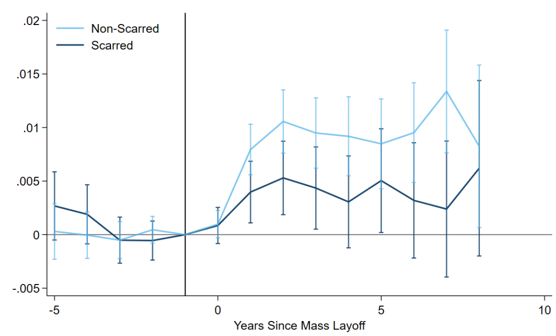
Panel A: Labor Market Scarring



Panel B: BA+ Share

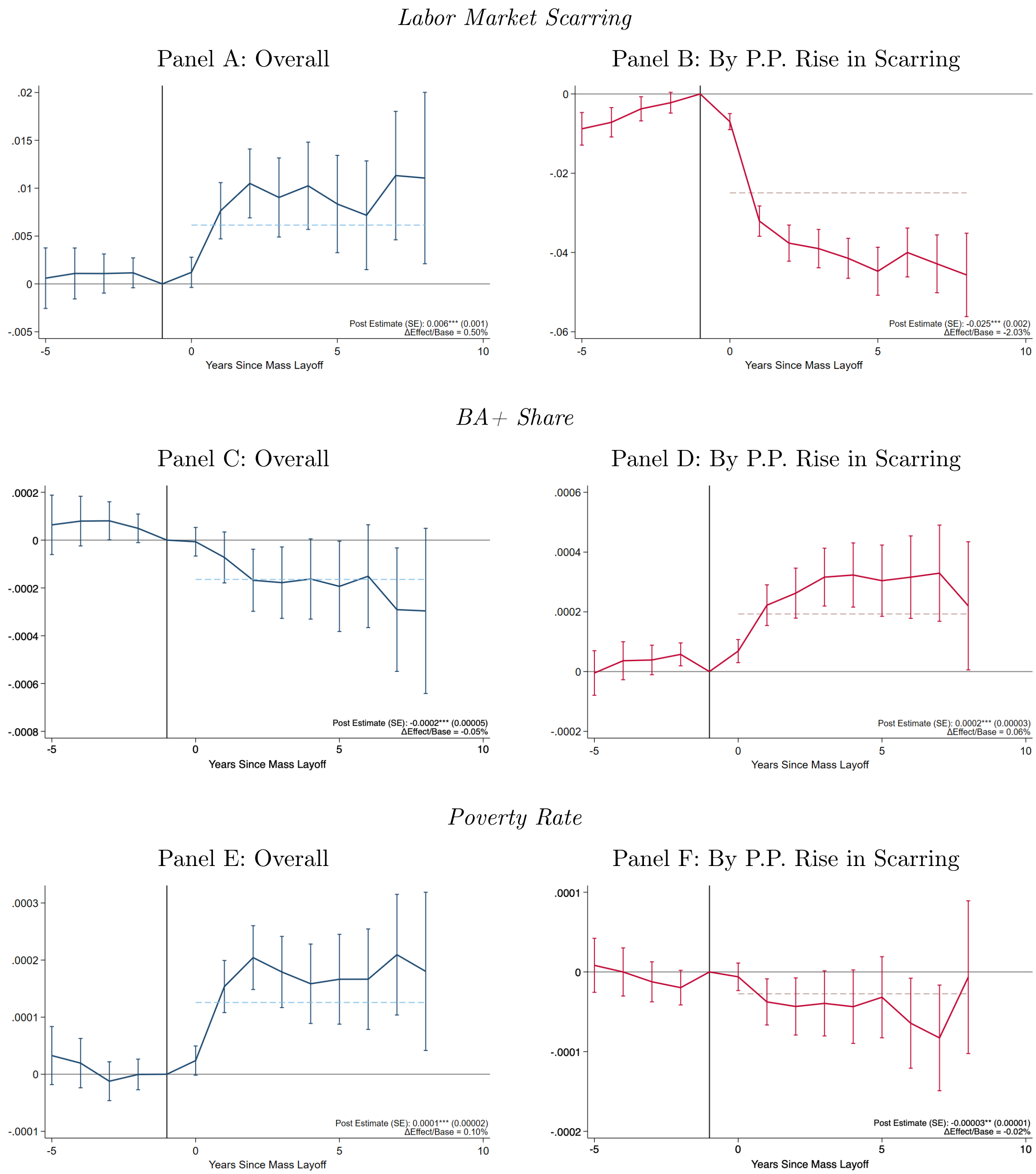


Panel C: Poverty Rate



Notes: This figure reports estimates of parental job loss on measures of neighborhood quality, estimated separately for households in scarred (dark blue) and non-scarred (light blue) labor markets, where scarred denotes above-median scarring and non-scarred denotes below-median scarring. The definition of labor market scarring is described in Section 3.2. Panel A reports results for neighborhood labor market scarring, Panel B reports results for the share of the population with a Bachelor's degree or more, and Panel C reports results for the poverty rate. The x-axis corresponds to relative time, years since the mass layoff event. Each point estimate corresponds to the interaction of the treatment indicator and a relative time indicator. Brackets denote 95 percent confidence intervals based on standard errors clustered at the firm by layoff year level. The solid vertical line denotes the year before the layoff. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-P2854-R12604.

Appendix Figure A12: Job Loss Effect on Neighborhood Quality (Unscaled)

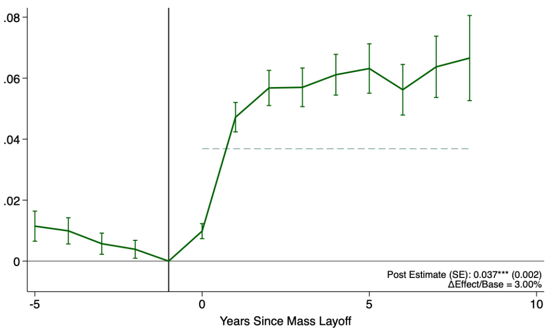


Notes: This figure reports the baseline job loss effects from Equation 2, on measures of neighborhood quality. Panel A reports results for the long-run scarring measure, Panel B reports results for the share of the population with a Bachelor's degree or more, and Panel C reports results for the poverty rate. The sample includes children exposed to parental job loss between the ages of 10 and 18. Unlike our main estimates, these estimates arenot scaled by the estimated impact on CZ outmigration, which we discuss in Section 2. Horizontal dashed lines indicate the value of the estimate which pools all post-event indicators into a single indicator. Vertical brackets denote 95 percent confidence intervals based on standard errors clustered at the firm by layoff year level. The solid vertical line denotes the year before the layoff. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-P2854-R12604.

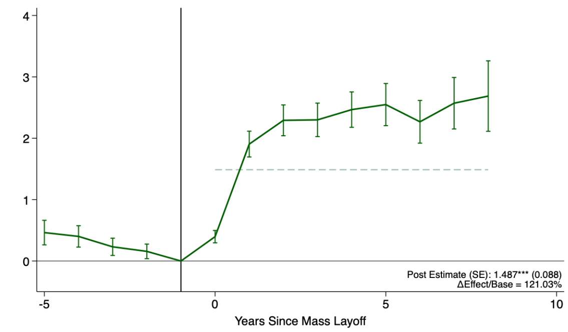
Appendix Figure A13: Baseline Job Loss Effect on Neighborhood Quality from Triple DiD

Labor Market Scarring

Panel A: Raw Estimate

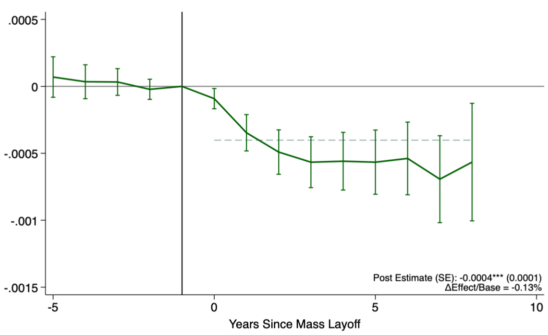


Panel B: Scaled Estimate

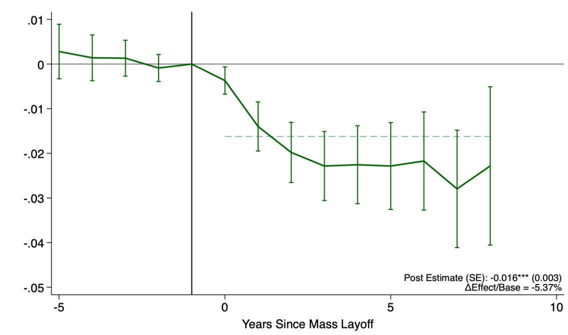


BA+ Share

Panel C: Raw Estimate

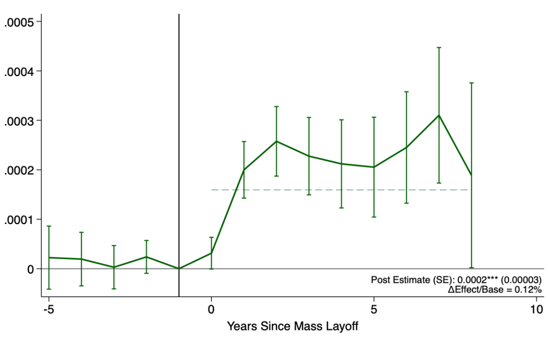


Panel D: Scaled Estimate

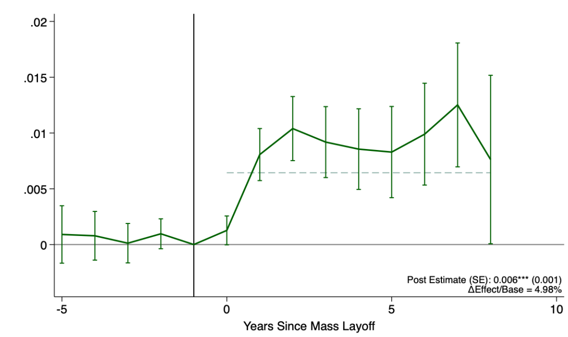


Poverty Rate

Panel E: Raw Estimate



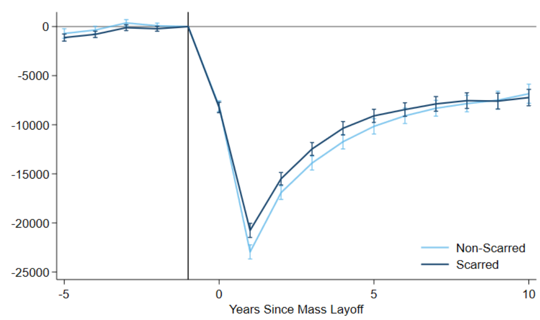
Panel F: Scaled Estimate



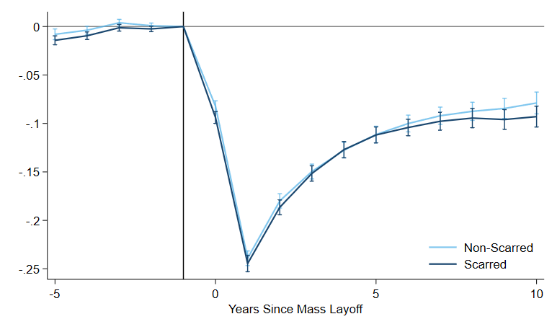
Notes: This figure reports the baseline job loss effects from Equation 2, on measures of neighborhood quality. Panel A reports results for the long-run scarring measure, Panel B reports results for the share of the population with a Bachelor's degree or more, and Panel C reports results for the poverty rate. The sample includes children exposed to parental job loss between the ages of 10 and 18. Each point reports the estimate scaled by the estimated impact on CZ outmigration, which we discuss in Section 2. Horizontal dashed lines indicate the value of the estimate which pools all post-event indicators into a single indicator. Vertical brackets denote 95 percent confidence intervals based on standard errors clustered at the firm by layoff year level. The solid vertical line denotes the year before the layoff. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-P2854-R12604.

Appendix Figure A14: Job Loss Impact on Parental and Household Earnings by Above/Below Median Scarring

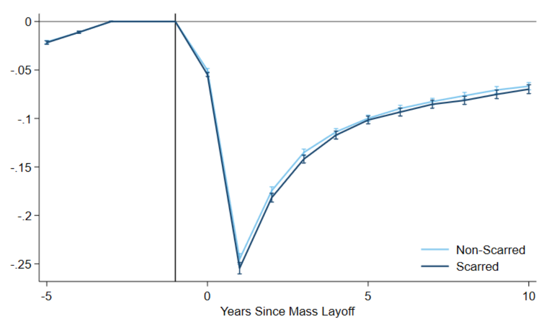
Panel A: Earnings



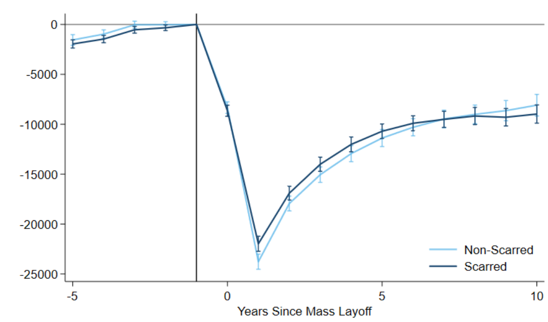
Panel B: Relative Earnings



Panel C: Employment



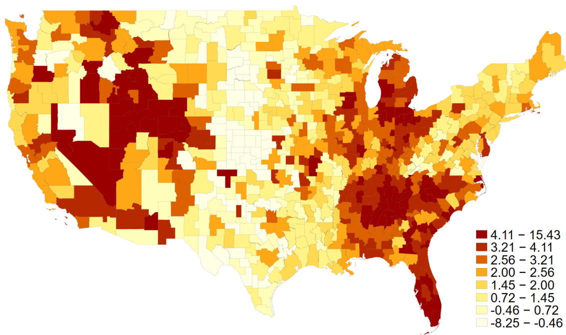
Panel D: Family Income



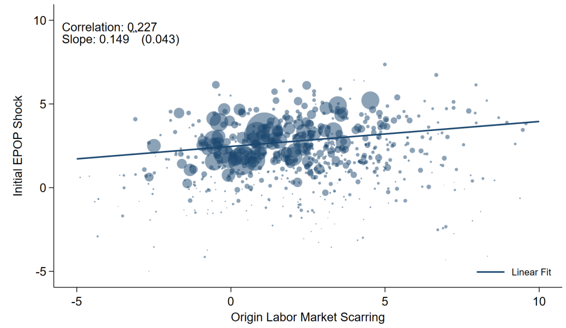
Notes: This figure reports estimates of parental job loss on parental and household earnings and employment, estimated separately for households in scarred (dark blue) and non-scarred (light blue) labor markets, where scarred denotes above-median scarring and non-scarred denotes below-median scarring. The definition of labor market scarring is described in Section 3.2. Panel A reports results for earnings, Panel B reports results for earnings relative to the pre-MLE earnings level, Panel C reports results for employment probabilities, and Panel D reports results for family income, defined as the sum of total family earnings. The x-axis corresponds to relative time, years since the mass layoff event. Each point estimate corresponds to the interaction of the treatment indicator and a relative time indicator. Brackets denote 95 percent confidence intervals based on standard errors clustered at the firm by layoff year level. The solid vertical line denotes the year before the layoff. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-P2854-R12604.

Appendix Figure A15: Correlation Between Alternative Measures of Great Recession Impact and Long-Run Scarring

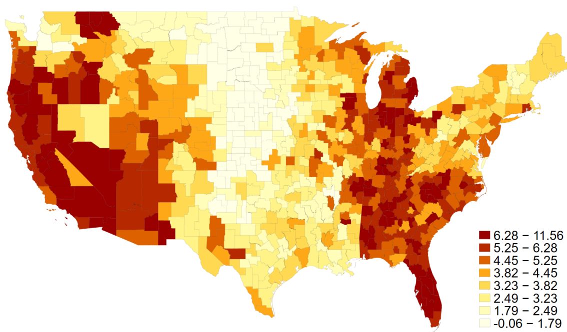
Panel A: Initial EPOP Shock



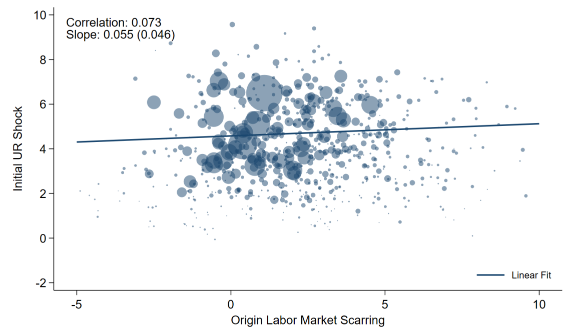
Panel B: Correlation with Long-Run Scarring



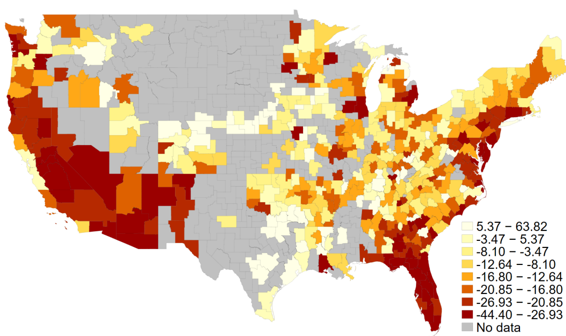
Panel C: Initial UR Shock



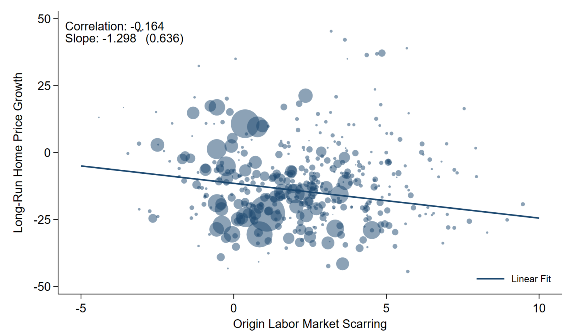
Panel D: Correlation with Long-Run Scarring



Panel E: Long-Run Home Price Shock

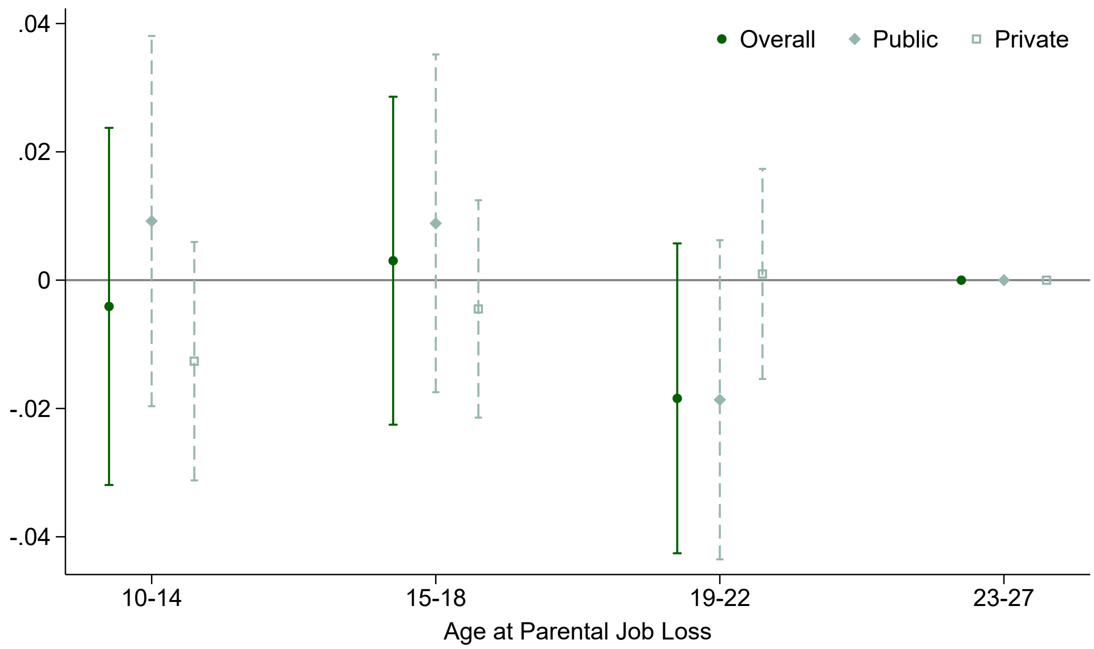


Panel F: Correlation with Long-Run Scarring



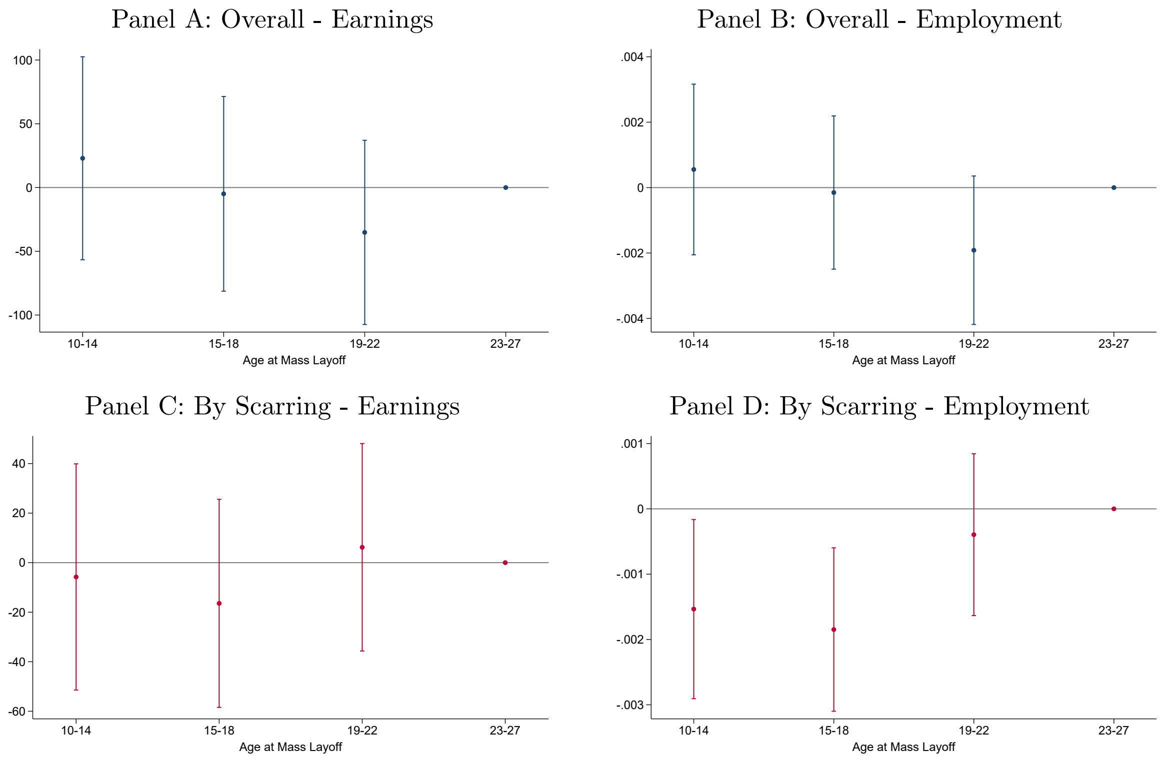
Notes: This figure reports the geographic distribution of several alternative Great Recession "shock" or "scarring" measures, along with their correlation with our long-run scarring measure defined in Section 3.2 at the commuting zone level. The left-hand column reports the geographic distribution of the alternative measure and the right-hand column reports the correlation with our scarring measure, omitting a handful of outlier points for exposition. The alternative measure in the top row is the percentage point decline in the EPOP ration from 2007-2009, the alternative measure in the middle row is the change in the unemployment rate from 2007-2009, and the alternative measure in the bottom row is the percent growth in home prices from 2007-2016, measured using the single-family homes series from Zillow. In the correlation figures, all points are scaled according to commuting zone population. The solid line is the line of best fit, estimated on the underlying data and weighted by commuting zone population. Robust standard errors are reported in parentheses. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level.

Appendix Figure A16: Baseline Job Loss Effect on College Attendance at Age 19-22 from Triple DiD



Notes: This figure reports baseline job loss estimates of the interaction of job loss from Equation 4 on college attendance at ages 19-22. Panel A report estimates ignoring the role of labor market scarring, focusing on the pooled impact of parental job loss. Each point represents the interaction of a relative time indicator, and an indicator for parental job loss. Panel B report estimates exploiting variation in labor market scarring, reporting the impact of parental job loss for each percentage point rise in labor market scarring. The sample includes children exposed to parental job loss between the ages of 10 and 27. Our reference/placebo group are children exposed to parental job loss at age 23-27. Vertical brackets denote 95 percent confidence intervals based on standard errors clustered at the firm by layoff year level. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-P2854-R12604.

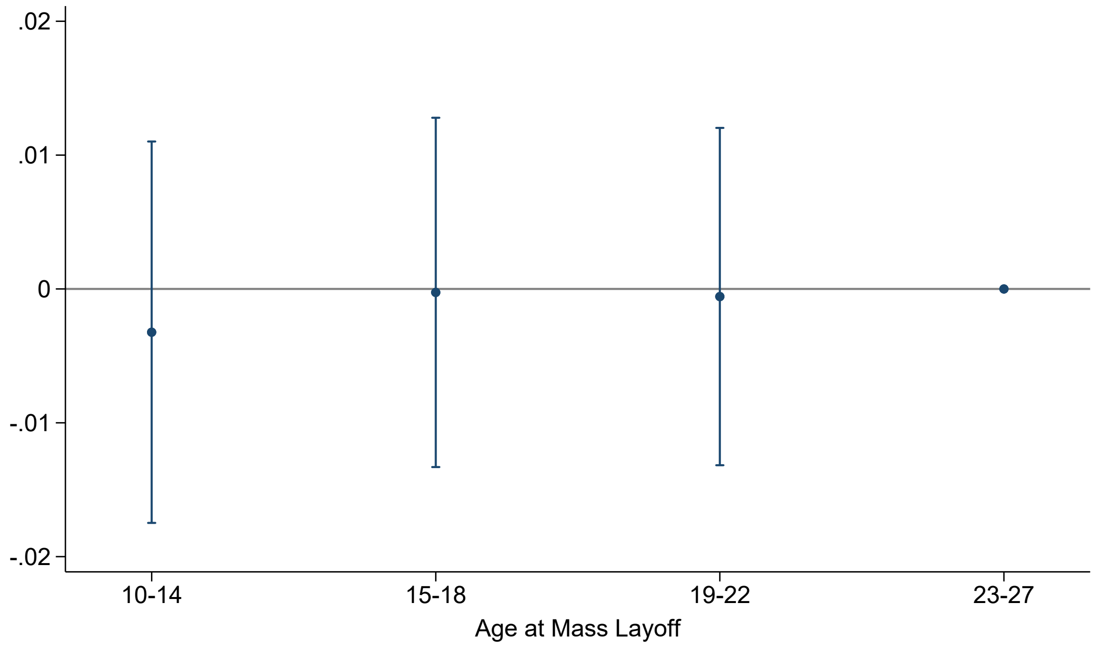
Appendix Figure A17: Impacts of Parental Job Loss on Childhood Labor Market Outcomes, Ages 19-22



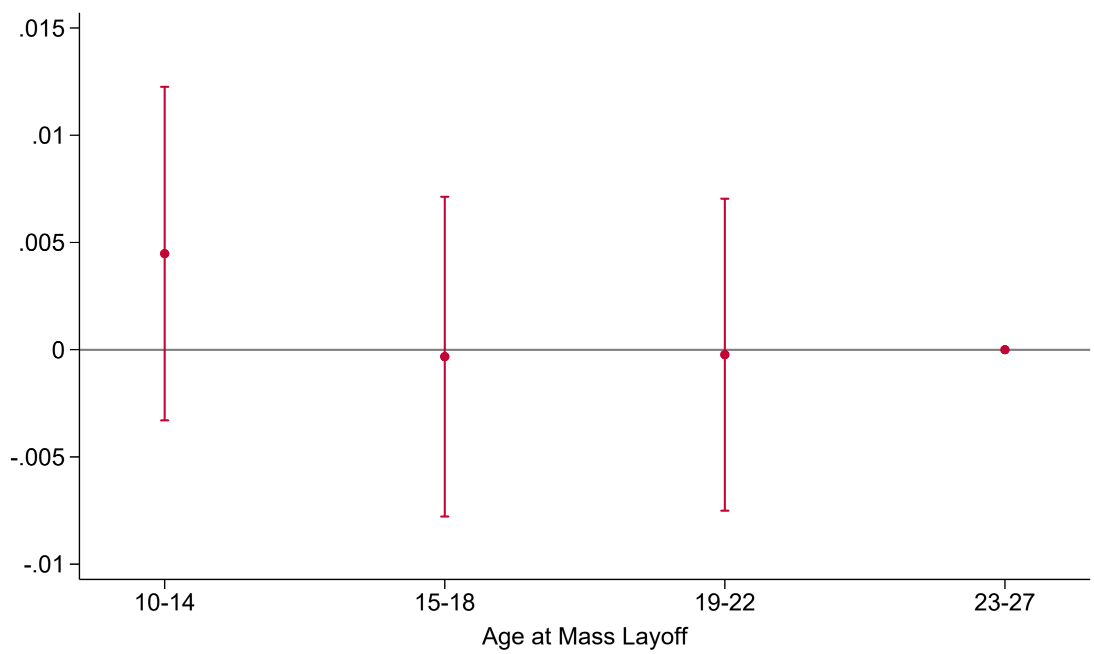
Notes: This figure reports estimates of the interaction of job loss, both ignoring and exploiting long-run Great Recession scarring, on children's labor market outcomes at ages 19-22. Panels A and B report estimates ignoring the role of labor market scarring, focusing on the pooled impact of parental job loss, for earnings and employment, respectively. Panels C and D report estimates exploiting variation in labor market scarring, reporting the impact of parental job loss for each percentage point rise in labor market scarring. The sample includes children exposed to parental job loss between the ages of 10 and 27. Our reference/placebo group are children exposed to parental job loss at age 23-27. Vertical brackets denote 95 percent confidence intervals based on standard errors clustered at the firm by layoff year level. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-P2854-R12604.

Appendix Figure A18: Parental Job Loss on High School Completion, Ages 19-22

Panel A: Overall



Panel B: By Scarring



Notes: This figure reports estimates of the interaction of job loss, both ignoring and exploiting long-run Great Recession scarring, on high school completion at ages 19-22. Panel A report estimates ignoring the role of labor market scarring, focusing on the pooled impact of parental job loss. Each point represents the interaction of a relative time indicator, and an indicator for parental job loss. Panel B report estimates exploiting variation in labor market scarring, reporting the impact of parental job loss for each percentage point rise in labor market scarring. The sample includes children exposed to parental job loss between the ages of 10 and 27. Our reference/placebo group are children exposed to parental job loss at age 23-27. Vertical brackets denote 95 percent confidence intervals based on standard errors clustered at the firm by layoff year level. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-P2854-R12604.

Appendix Table A1: Baseline Impact of Parental Job Loss on Neighborhood Quality

	2008-2012			2013-2017		
	Mean	25th Percentile	75th Percentile	Mean	25th Percentile	75th Percentile
	(1)	(2)	(3)	(4)	(5)	(6)
Neighborhood Quality Measures						
Share with Bachelor's or More	0.284	0.316	0.243	0.307	0.342	0.266
Unemployment Rate	0.093	0.089	0.099	0.066	0.065	0.067
Poverty Rate	0.149	0.138	0.154	0.146	0.137	0.153
Median HH Income ($2012)	55,068	59,842	49,576	56,259	61,244	50,204
Criminal Justice Index	0.138	0.296	0.250	−0.099	−0.480	−0.013

Notes: This table reports commuting zone characteristics overall and at the 25th and 75th percentile of the long-run labor market scarring measure. The labor market scarring measure is calculated following the procedure described in Section 3.2. We report outcomes in Panel C Table 2, excluding Childhood Opportunity which is from Chetty et al. (2025) and not available as a cross-sectional measure. Except for our criminal justice index, data is from Manson et al. (2024) using 5-Year ACS waves 2008-2012 and 2013-2017. We construct our criminal justice index using Finlay, Muller-Smith, and the CJARS Team (2024). The first three columns report results from 2008 to 2012, and the last three columns report results from 2013 to 2017. Columns 2 and 5 report commuting zone characteristics at the 25th percentile of the scarring distribution and Columns 3 and 6 report commuting zone characteristics at the 75th percentile of the scarring distribution. To improve stability, we calculate these characteristics using a bandwidth of five percentile ranks around the focal percentile. All statistics weighted using commuting zone population in 2007.

Appendix Table A2: Impact of Parental Job Loss on Neighborhood Quality (Unscaled)

	Pooled		By Scarring	
	β	$\Delta\%$	β	$\Delta\%$
	(1)	(2)	(3)	(4)
Change EPOP	0.006***	0.50%	-0.025***	–
	(0.001)		(0.002)	
Observations	15,640,000		15,640,000	
BA+ Share	-0.000***	-0.05%	0.0002***	0.06%
	(0.00005)		(0.00003)	
Observations	15,640,000		15,640,000	
Poverty Rate	0.0001***	0.10%	-0.000**	-0.02%
	(0.00002)		(0.00001)	
Observations	15,640,000		15,640,000	
Pre-K Share	0.0001*	0.01%	0.0002***	0.03%
	(0.00004)		(0.00002)	
Observations	15,640,000		15,640,000	
Child Poverty	0.0002***	0.09%	-0.000**	-0.02%
	(0.00003)		(0.00002)	
Observations	15,640,000		15,640,000	
Log Median Monthly Housing Cost	-0.002***	–	0.0004***	–
	(0.0002)		(0.0001)	
Observations	15,640,000		15,640,000	
Log Median Personal Income	-0.001***	–	0.0003***	–
	(0.0001)		(0.0001)	
Observations	15,640,000		15,640,000	
Criminal Justice Index (Age 19-22)	0.001	–	-0.001***	–
	(0.0005)		(0.0003)	
Observations	11,070,000		11,070,000	

Notes: This table reports estimates of parental job loss on children's neighborhood quality up through age 18, both ignoring and exploiting long-run Great Recession scarring. Unlike our main estimates, each cell reports the estimate without being scaled by the estimated impact on CZ outmigration, which we discuss in Section 2. We also report the implied percentage change, dividing the estimated scaled coefficient by the average of corresponding outcome. Columns 1 and 2 report estimates ignoring the role of labor market scarring, focusing on the pooled impact of parental job loss. Each point represents the interaction of a relative time indicator, and an indicator for parental job loss. Columns 3 and 4 report estimates exploiting variation in labor market scarring, reporting the impact of parental job loss for each percentage point rise in labor market scarring. At the bottom of the table, we report the interquartile range of the scarring measure, which is the difference between the scarring measure at the 75^{th} percentile and at the 25^{th} percentile. Standard errors clustered at the firm by layoff year level are reported in parentheses. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-P2854-R12604.

Appendix Table A3: Baseline Impact of Parental Job Loss on Neighborhood Quality

	Raw β (1)	Scaled β (2)
Change EPOP	0.037***	1.487***
	(0.002)	(0.088)
Observations	15,640,000	
BA+ Share	-0.000***	-0.016***
	(0.0001)	(0.003)
Observations	15,640,000	
Poverty Rate	0.0002***	0.006***
	(0.00003)	(0.001)
Observations	15,640,000	
Pre-K Share	-0.000***	-0.005***
	(0.00004)	(0.002)
Observations	15,640,000	
Child Poverty	0.0002***	0.009***
	(0.00004)	(0.002)
Observations	15,640,000	
Log Median Monthly Housing Cost	-0.003***	-0.105***
	(0.0002)	(0.008)
Observations	15,640,000	
Log Median Personal Income	-0.001***	-0.045***
	(0.0001)	(0.005)
Observations	15,640,000	
Criminal Justice Index (Age 19-22)	0.002***	0.095***
	(0.001)	(0.022)
Observations	11,070,000	

Notes: This table reports baseline estimates of parental job loss on children's neighborhood quality up through age 18 from Equation 4. In column (1), we report the "raw" estimate and, in column (2), we report the estimate scaled by the estimated impact on CZ outmigration, which we discuss in Section 2. Standard errors clustered at the firm by layoff year level are reported in parentheses. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-P2854-R12604.

Appendix B Data Appendix

This appendix details the construction of our analysis dataset, including the identification of mass layoff events, the assembly of population-level residence panels, and the linkage of parent-child relationships. All individual-level records are matched using Protected Identification Keys (PIKs) generated by the Census Bureau's Person Identification Validation System (Wagner and Layne 2014).

B.1 Identifying Mass Layoff Events

We identify mass layoff events using the Longitudinal Employer-Household Dynamics (LEHD) program, which provides linked employer-employee records from state Unemployment Insurance (UI) wage records combined with Census business registers. The LEHD microdata consist of quarterly job spells linking worker identifiers to employer identifiers. Our sample covers 29 states from 1999-2019, representing over 95% of wage-and-salary employment in covered states (Graham et al. (2022)).[1]

To define mass layoff events, let $E_{f,t}$ denote employment at firm f in quarter t. To avoid seasonal fluctations in employment, we measure year-over-year employment growth rates:

$$\Delta^{YoY}_{f,t} = \frac{E_{f,t+4} - E_{f,t}}{E_{f,t}}.$$

We classify a mass layoff event as beginning in calendar year t if $\Delta E^{YoY}_{f,t} \leq -0.30$.

To avoid misclassifications of mass layoff events, we follow standard procedures in the literature.[2] First, we restrict to firms with at least 25 employees at the start of the MLE to avoid misclassifying normal turnover in small firms as mass layoffs. Second, we require (a) firms must have positive employment in each of the four quarters prior to the mass layoff; and, (b) employment remain below the pre–layoff level year-over-year over three years. Third, using the LEHD Successor-Predecessor file, we exclude events where $\geq 30\%$ of separating workers transition to the same destination firm, which firm mergers, spinoffs, or acquisitions rather than true layoffs.

After identifying our sample of mass layoff firms, we restrict attention to a sample of workers who

[1] Our data contains earnings data from 29 states: AZ, CA, CO, CT, DE, IN, IA, KS, ME, MD, MA, MT, NE, NV, NJ, NM, ND, OH, OK, OR, PA, SC, SD, TN, TX, UT, VA, WA, and WI.

[2] See Flaaen, Shapiro, and Sorkin (2019) Appendix Table 2 for detailed documentation on approaches used in the displaced workers literature.

worked for 3+ consecutive years at their pre-mass layoff primary employer. Among such workers, "job losers" are those who separated from their pre-mass layoff firm during the mass layoff and do not return for at least four consecutive quarters. We compare these workers to a control group of "survivors" who remained continuously employed during the mass layoff. Workers who separate during the event year and return within four quarters are excluded.

B.2 Annual Residence Panel

We construct person-year residence panels at the census tract level (aggregated to county, CZ, and state for analysis) by hierarchically combining multiple sources:

1. **Decennial Census (2000, 2010)**
2. **American Community Survey (2001-2019)**
3. **HUD Tenant Rental Assistance Certification System (TRACS)**
4. **LEHD Residence Files:**
   - Composite Person Record (1999-2010)
   - Residence Candidates File (2012-2019)
5. **Master Address File-Auxiliary Reference File** (2008-2019)

The Decennial Census offers population-level enumeration in 2000 and 2010. The American Community Survey supplements this with annual samples of the population. HUD TRACS captures addresses for households receiving federal rental assistance. The LEHD residence files are constructed from the Composite Person Record (1999-2010) and Residence Candidates File (2012-2019). Both the Composite Person Record (CPR) and Residence Candidates File (RCF) aggregates multiple federal administrative sources using an algorithim to identify the "best address".[3] Crucially, while our earnings data are restricted to 29 states due to LEHD coverage limitations, these residence files provide national coverage, allowing us to track moves across state boundaries even

[3] For documentation on the Residence Candidates File (RCF), see Graham, Kutzbach, and Sandler (2017). For the Composite Person Record (CPR), authors were provided documentation by US Census and confirmed it may be publicly shared; an archived copy is hosted on the author's website at `https://andrewjoung.com/DataRepo/CPR_documentation.pdf`.

when destination states lack earnings records. The MAF-ARF is a Census Bureau dataset which links all known living quarters with to all associated individuals. While comprehensive, data underlying the MAF-ARF often links individuals to multiple addresses, but provides no means for researchers to distinguish the source of these addresses or randomly assigns one address as the primary residence. As a result, we prioritize other data sources over the MAF-ARF.

B.3 Family Crosswalks

We identify parent-child linkages through a combination of administrative and survey sources. When multiple sources provide conflicting information, we prioritize:

1. **Census Household Composition Key (CHCK)**: Links from SSN applications and birth certificates for cohorts born 1997 onward

2. **Decennial Census (2000, 2010)**: Household rosters for children age ≤ 19

3. **American Community Survey (2001-2019)**: Annual household rosters for children age ≤ 19

4. **IRS Form 1040 (1994)**: Dependent claims for tax year 1994 restricted to dependents age ≤ 19

This hierarchical approach maximizes coverage while maintaining link quality. Given our data coverage and focus on mass layoff events from 2006 to 2016, our final sample includes children born 1979-2000 with at least one identified parent.

Appendix C Labor Market Scarring vs the Initial Shock

Our preferred approach to measuring long-run exposure to the Great Recession is through the percentage point decline in the employment-to-population ratio, which captures long-run variation in local labor market health and sluggish economic recovery. A natural alternative source of cross-commuting zone variation is the initial Great Recession shock. Distinguishing between how families respond to short-run shocks versus long-term scarring has important policy implications. Traditional policy responses to recessions emphasize temporary stabilization measures. However, if families primarily respond to persistent labor market weakness rather than transitory shocks, short-term interventions may prove insufficient.

To illustrate empirically the importance of considering labor market scarring rather than just initial exposure to the Great Recession, we perform two complementary exercises. First, we re-estimate our scarring results, restricting attention to job losses during the Great Recession (2007-2009). This provides a benchmark for our second exercise, where we replace our long-run scarring measure with the initial shock from the Great Recession. Following Yagan (2019), we measure the impact of the initial shock for commuting zone c as:

$$\text{Shock}_c = \text{UR}_{c,2009} - \text{UR}_{c,2007}$$

which is the percentage point increase in the unemployment rate from 2007 to 2009. Appendix Figure A15 demonstrates that long-run scarring bears little relationship to the initial recession shock. In Appendix Figure A15 we display the geographic distribution of the change in EPOP and unemployment rate from 2007 to 2009, measures of the Great Recession's initial shock. Both are only weakly correlated with our long-run scarring measure. Other long-run measures such as the long-run change in home prices also exhibit relatively weak statistical correlations.[4] These descriptive results suggest that it is unlikely for our results by labor market scarring to be driven by initial shock exposure.

Appendix Table C1 Column (1) summarizes the estimates for commuting zone outmigration, labor market scarring and college attendance at ages 19–22 using our long-run scarring measure.

[4] This is similar to results by Finkelstein et al. (2023) showing commuting zones experiencing similar unemployment spikes in 2007-2009 often diverged dramatically in their subsequent recoveries.

While predictably noisier, the empirical pattern is replicated within this subsample: labor market scarring increases commuting zone outmigration, destination labor market strength, and college attendance among children exposed to parental job loss. In Column (2), we report estimates using our measure of initial exposure to the Great Recession shock. In contrast, the unemployment-rate interaction yields coefficients that are smaller or statistically indistinguishable from zero across our outcomes.

These results suggest that the initial Great Recession shock did not differentially impact children of job-losing parents relative to children of survivors, whereas labor market scarring generated substantial heterogeneity in the effects of parental job loss.[5] This contrast highlights our focus on labor market scarring rather than short-run labor demand shocks.

The distinction between initial shocks and long-run scarring is further supported by their weak empirical correlation. As shown in Appendix Figure A15, the correlation between our scarring measure ($\Delta \text{EPOP}_{2007-2016}$) and the initial unemployment shock ($\Delta \text{UR}_{2007-2009}$) is only 0.07, with a slope coefficient of 0.06 that is statistically indistinguishable from zero. This weak relationship indicates that commuting zones experiencing similar initial shocks faced dramatically different long-run recovery trajectories (Finkelstein et al., 2023). The orthogonality of these measures strengthens our interpretation that persistent shifts in labor market scarring–not temporary shocks–drives the heterogeneous effects of parental job loss on children's mobility and human capital accumulation.

Overall these results contribute additional evidence to the importance of distinguishing between short-run business cycle fluctuations and long-run labor market scarring. Alongside countercyclical stabilization, policymakers should also consider policies with a longer time horizon, such as place-based policies to rehabilitate weak labor markets, or mobility assistance programs that help families exit economically depressed areas.

[5]We note that this result does not imply that the initial Great Recession shock had no impact on children's long-run outcomes, just that the initial shock did not have a differential impact on children of job-losing parents.

Appendix Table C1: Impact of Job Loss During the Great Recession by Shock Measure, 2007-2009

	Scarring (1)	Initial Shock (2)
Panel A: Moved CZ		
Post-Job Loss × Shock	0.002***	0.001**
	(0.0004)	(0.0005)
Panel B: Change EPOP		
Post-Job Loss × Shock	−0.903***	−0.017
	(0.084)	(0.060)
Panel C: College Attend.		
Post-Job Loss × Shock × Age 10–14	0.021**	−0.008
	(0.010)	(0.013)
Post-Job Loss × Shock × Age 15–18	0.008	−0.012
	(0.011)	(0.013)
Post-Job Loss × Shock × Age 19–22	0.012	0.002
	(0.010)	(0.013)
Shock IQR	2.456	.867

Notes: This table reports estimates of the interaction of two different shock measures and parental job loss on children's outmigration probabilities, neighborhood quality, and college attendance, restricting to mass layoff events from 2007-2009. Column 1 uses our EPOP measure as the relevant measure of neighborhood scarring and Column 2 uses the short-run (2007-2009) change in the unemployment rate. The outcome is listed in each panel header and the coefficient is listed in each row. Each panel contains estimates from a separate regression. The IQR reports the interquartile range of the listed scarring or shock measure. Standard errors clustered at the firm by layoff year level are reported in parentheses. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-P2854-R12604.