Parental Proximity and Earnings after Job Displacements*

Pawel Krolikowski † Mike Zabek ‡ Patrick Coate §

April 1, 2020

Abstract

The earnings of young adults living in their parents' neighborhoods completely recover after a job displacement, while the earnings of those living farther away permanently decline. Nearby workers appear to benefit from help with childcare. Earnings improvements are larger in states with expensive childcare and among workers in inflexible occupations, and workers' parents do less market work following their child's displacement. Differences in job search durations, transfers of housing services, and geographic mobility are too small to explain the result. Our results are also consistent with workers benefiting from parental employment networks.

JEL codes: J61, J64, R23.

Keywords: Parents, family ties, adult children, job loss, childcare, transfers

*The first two authors made equal contributions. We thank two anonymous referees for very constructive comments. We would also like to thank Dionissi Aliprantis, Joseph Altonji, John Bound, Audra Bowlus, Charlie Brown, Joel Elvery, Bruce Fallick, Olga Gorbachev, Darren Grant, Peter Hinrichs, Julie Hotchkiss, Susan Houseman, Fabian Lange, Mark Partridge, Roberto Pinheiro, Claudia Sahm, Bob Schoeni, Sam Schulhofer-Wohl, Jeff Smith, Frank Stafford, Mel Stephens, Bryan Stuart, Jeremy Tobacman, Erin Troland, and Abigail Wozniak for helpful conversations. We also thank participants from the PSID's Conference on Intergenerational Transfers and Time Use in Later Life, the PSID and H2D2 Seminar series at the U. Michigan, the 2017 APPAM meetings, the 2018 SOLE meetings, the 2018 System Applied Microeconomics Conference, Vanderbilt, and U. Delaware. Meifeng Yang and Rachel Yang provided excellent research assistance. The collection of data used in this study was supported by the National Institutes of Health (NIH) under grant number R01 HD069609 and the National Science Foundation (NSF) under award number 1157698. We acknowledge funding from the National Institute on Aging and National Institute of Child Health and Human Development grants to the Population Studies Center at the University of Michigan (Coate and Krolikowski: T32 AG000221 and R24 HD041028; Zabek: R24 HD041028 and P2CHD041028). The views expressed in this paper are solely the responsibility of the authors and should not be interpreted as reflecting the views of the NIH, the NSF, the National Council on Compensation Insurance, Inc., the Federal Reserve Bank of Cleveland, the Board of Governors of the Federal Reserve System, or of any other person associated with the Federal Reserve System.

†Corresponding author: Federal Reserve Bank of Cleveland, 1455 E 6th St, Cleveland, OH 44114. Tel: +1 216-774-2156. Email: pawel.krolikowski@clev.frb.org

‡Board of Governors of the Federal Reserve System, Washington, DC 20551. Tel: +1 202-452-2950. Email: mike.zabek@frb.gov

§National Council for Compensation Insurance. Email: pecoate@gmail.com

1 Introduction

Why do so many young adults live within a few miles of their parents? In the Panel Study of Income Dynamics (2017), for example, the median 25- to 35-year-old household head lives about five miles from a parent or an in-law.[1] This lack of mobility, often explained by high moving costs, has prompted concerns about workers being stuck in depressed labor markets.[2] But young adults might also benefit from living close to their parents.

We document the labor market benefits of living close to parents by showing that the earnings of young workers who live near their parents recover after a job displacement. In contrast, workers who live farther away experience large, permanent declines in earnings, as in Jacobson, LaLonde, and Sullivan (1993) and Davis and von Wachter (2011). Nearby workers appear to benefit from help with childcare. However, workers do not appear to benefit from searching longer for a new job because they can move back in with parents. Job referral networks might also play a role, though our evidence about them is limited.

Our baseline result is that 25 to 35 year olds who live in the same neighborhoods as their parents see their earnings recover after a job displacement.[3] Those who live farther away suffer large, permanent earnings losses. While job displacements are plausibly exogenous (von Wachter, Song, and Manchester, 2009), children's locations are not randomly assigned, so our results are not necessarily causal. The results do, however, suggest a causal mechanism: They include propensity score reweights that control for detailed characteristics of workers, the jobs they lose, and the places they live. Unobservable differences that remain after reweighting would lead to the opposite pattern—lower earnings after a job displacement—if unobservable differences that lead workers to live closer to their parents also lead to worse outcomes after displacements. For example, workers could live close to their parents because they care for them, because they struggle with change, or because of particularly high moving costs. Workers who live near their parents also are worse off in observable ways; for example,

[1]Researchers find similar patterns using other data sources. For example, Compton and Pollak (2015) use the National Survey of Families and Households to find that most Americans live within 25 miles of their mothers, and Bui and Miller (2015) use the Health and Retirement Survey to show that median older Americans live 18 miles from their mothers.

[2]Cowen (2017), Austin, Glaeser, and Summers (2018), and Chetty, Friedman, Hendren, Jones, and Porter (2018) connect negative economic outcomes to people living in depressed areas. Bishop (2007), Kennan and Walker (2011), Diamond (2016), and Coate (2017) estimate large moving costs, which represent any factors that keep people from realizing the benefits of moving. Additionally, several studies suggest that people are migrating less than previously (Molloy, Smith, and Wozniak, 2011; Kaplan and Schulhofer-Wohl, 2017; Mangum and Coate, 2018). However, Johnson and Schulhofer-Wohl (2019) argue that declining geographic mobility does not explain the poor recent labor market performance of young adults.

[3]We define a neighborhood as a census tract, though we show that our results are not sensitive to this definition by using a measure of geographic distance between parents and children. Much of the literature (e.g. Hellerstein, Kutzbach, and Neumark, 2019) also defines neighborhoods using census tracts.

they are less educated.

We also find several pieces of evidence that convenient childcare from workers' parents drives our baseline result. First, the benefit of nearby parents is concentrated in states where market-based childcare is costly. Second, young workers who lose jobs from inflexible occupations (as defined in Goldin, 2014), in which free and convenient childcare would be most valuable, experience the greatest benefits from nearby parents. Third, workers' nearby parents decrease their hours worked following their child's displacement, and we find evidence that these labor supply decreases are not due to correlated shocks. The decrease in workers' nearby parents' labor supply is consistent with reductions in workers' parents' labor supply after the birth of a first grandchild, as in Rupert and Zanella (2018). Fourth, the effects are concentrated among workers who live in the same neighborhood as their parents, as opposed to the same city. This evidence is consistent with close proximity facilitating irregular childcare (emphasized by Compton and Pollak, 2014) and with workers valuing nearby childcare (Rosenthal and Strange, 2012). Fifth, older workers, who are less likely to have children at home, do not benefit from living closer to their parents, which is also consistent with workers living near their elderly parents to care for them (Lin and Rogerson, 1995; Lin and Wu, 2010; Chari, Engberg, Ray, and Mehrotra, 2015). Finally, our main result is driven by workers who end up having children.

Our results are consistent with local social networks insuring earnings after a job displacement, as in Corak and Piraino (2011), Kramarz and Skans (2014), and Topa and Zenou (2015). We show that workers find jobs in their parents' industries after displacements, but our estimates are statistically insignificant and exhibit a pre-trend.

Our findings suggest that parental proximity provides labor market insurance to young workers, as opposed to it only restricting labor market options and imposing additional burdens. We extend the findings in Compton and Pollak (2014), who document that the availability of childcare from grandmothers increases the labor supply of married women, to the earnings of a mostly-male sample after a job displacement.[4] We show that our results are consistent with a simple model in which parents facilitate better job offers for their children. In the model a preference for living at home will restrict workers' labor market options and lead to the opposite pattern of earnings from our baseline result. Empirically, our baseline result also applies to workers who do not move after a job displacement, so restricted labor market options are an unlikely explanation.

Our results support previous research (Kaplan, 2012; Dalton, 2013; Munshi and Rosen-

[4]Parental help with childcare should help men's earnings after job displacements. Wives increase labor supply after husbands' job losses (Stephens, 2002), which increases men's childcare responsibilities (Raley, Bianchi, and Wang, 2012). Stafford and Sundström (1996) and Spivey (2005) find that men's labor market interruptions are associated with larger earnings penalties.

zweig, 2016) showing that nearby parents provide labor market benefits to young workers, but three pieces of evidence suggest that the results are not because parents facilitate longer job searches by transferring resources or allowing children to move back in with them. First, we find that children who live closer to their parents spend similar amounts of time unemployed after a displacement. Second, we find that children benefit from parental proximity when they live in the same neighborhood as their parents, but outside of their houses. Third, we find only small increases in transfers of money and housing around displacement, and transfers of money and housing are not any larger among workers who lived closer to their parents before a job displacement.

The rest of the paper proceeds as follows. Section 2 describes our data, describes our sample, and presents our main earnings results using averages. Section 3 describes our regression and propensity score reweighting methodologies. Section 4 presents our baseline earnings results, decomposes them into hours and wages, and presents heterogeneous effects of parental proximity. These heterogeneity results emphasize the importance of childcare and suggest that our main result is not driven by geographic mobility or co-residence, and that parental proximity matters for young workers, independent from where they grew up. Section 5 investigates further evidence on mechanisms including workers' parents' labor supply, housing transfers, and parental employment networks. Section 6 discusses selection and unobserved heterogeneity in the context of our results and the economic significance of our empirical estimates. Section 7 discusses broader implications of our work and avenues for future research.

2 Analysis Data and Sample Averages

In this section, we discuss our survey data, sample construction, summary statistics, and some preliminary evidence that nearby parents improve the earnings of young workers after displacement.

2.1 Dataset and Definitions

The PSID is one of few survey datasets that meets all four requirements of our analysis. First, it has granular information about where respondents live through the restricted-use version of the data, including the census tract of each household, which we refer to as a neighborhood. Second, it includes job history information that identifies displacements and measures earnings. Third, it includes repeated observations so we can use a difference-in-difference approach. Finally, it links parents and children due to its genealogical nature. The advantage of the PSID relative to the National Longitudinal Survey of Youth or the Survey of Income and Program Participation is the wealth of information it collects about parents and children over many years and the granularity of the location data.

The PSID also has several strengths over administrative data. For example, our baseline results in Section 4 use propensity score reweighting and benefit from the rich covariates available in the PSID. Administrative datasets often lack many of the variables that we use for the reweighting—most notably wages, education, occupation, and race. In addition, a decomposition of lost earnings into hours and wages is typically not possible with U.S. administrative data, which often include quarterly earnings but not hours.[5] Our findings suggest that this might be an important distinction. One significant limitation of administrative data sources for our application in the U.S. is a limited longitudinal coverage of workers' earnings, which would limit our ability to observe long-term effects on earnings. We discuss details in Appendix A.1.

Our sample contains PSID "household heads" from the PSID's Survey Research Center (SRC) and Survey of Economic Opportunity (SEO) samples. We provide further details about the definition of heads in Appendix A.2.

We define displacement as involuntary job loss among workers with a strong attachment to the labor market. In particular, we classify separations as involuntary if recent job losers respond that they lost their previous job because their plant closed, their employer moved, they were laid off, or they were fired. We impose at least two years of tenure and full-time work status before the displacement. We impose the same restriction for non-displaced workers, with details about the timing in Section 2.2. We provide further details about the survey questions used to determine displacements and the tenure cutoff in Appendix A.3.

2.2 Sample Construction and Summary Statistics

To provide evidence on the effects of displacement by parental proximity and age, we follow Huttunen, Møen, and Salvanes (2018) in constructing a dataset that duplicates records for the same worker based on their displacement status and proximity to parents at different ages. Specifically, we construct the dataset by assigning each worker a displacement status and a proximity to their parents at each age we observe them in our sample, which we refer to as a base age. Displacement status is based on whether the worker lost a job since the last interview. Proximity to parents is based on whether the worker lived in the same neighborhood as their parents in their last interview. The record for a worker at a base age contains all observations of that worker while they are classified as a household head in the PSID and while they are between the ages of 18 and 62. We repeat the procedure for every base age between 25 and 55 and stack all the records to create the final dataset.

[5] Recent exceptions include Kurmann and McEntarfer (2018), Jardim, Solon, and Vigdor (2019), and Lachowska, Mas, and Woodbury (2019) who use administrative data from Washington, where the hours data seem relatively high quality (Lachowska, Mas, and Woodbury, 2018).

A worker's record can appear up to 31 times in our stacked dataset, but a worker's record will only appear in the stacked dataset if it satisfies the tenure and full-time restrictions at that base age. We define the *relative year* as zero in the base age, one in the year after, etc. We provide further discussion about which relatives the PSID surveys, about why parental location may be missing, about workers who never move out of their parents' house, and about workers switching location between the previous and current surveys in Appendix A.4.

Duplicating and stacking workers allows us to more fully exploit the longitudinal dimension of the PSID and to define the control group in a simple way. We exploit the longitudinal dimension of the PSID by capturing multiple potential displacements and multiple proximities to parents around potential displacements for the same worker, increasing our sample size. And we define the control group as workers who are at risk of a displacement at particular ages, which also allows us to define their proximity to parents based on where they live in the previous year. Our definitions of treatment and control also avoid biased estimates of the earnings effects of displacement (Krolikowski, 2018).

We use earnings observations for individuals from the 1968 to 2013 waves of the PSID, and we use displacement events from 1969 to 1997. The latter restriction preserves the interpretation that someone was displaced in the previous year, because the 1968 survey asks about displacements in the last 10 years, and surveys after 1997 ask about the previous 2 years (the PSID is a biannual survey after 1997).

Table 1 shows the summary statistics for the final sample and separately for young and older workers, in which we use longitudinal weights to address possible issues arising from the SEO's unclear sampling frame and from differences in attrition (Brown, 1996; Solon, Haider, and Wooldridge, 2015). We restrict to observations with non-missing parents' location information. The dataset consists of about 45,000 records, with an average of 20 years of observations for each, yielding roughly 900,000 person-year observations. The final dataset contains about 1,400 displacement events, of which 300 took place while a worker resided in their parents' neighborhoods, and approximately 1,100 occurred while a worker was not in their parents' neighborhoods. The average annual displacement probability is about three percent in our sample, which is consistent with Kuhn (2002) and Davis and von Wachter (2011). Before displacement, the displaced workers are slightly younger, less educated, earn less, and have been with their employer for less time than their non-displaced counterparts. About 15 percent of adults live in the same neighborhoods as their parents. We analyze the data separately for younger workers (ages 25 to 35) and older workers (ages 36 to 55); Table 1 presents summary statistics separately for this younger group of workers as well. We define our two age groups as 25 to 35 and 36 to 55 so that we have an equal number of displacement events in both groups (about 700) and to be broadly consistent with the definitions in Kletzer and Fairlie (2003) and Kaplan (2012).

2.3 Preliminary Evidence

Figure 1 provides preliminary evidence that parents insure workers' earnings against the dramatic consequences of a job loss. The top panel in Figure 1 presents the average, real (2007 CPI-U-X1) earnings of workers who were displaced (dashed) and not displaced (solid) when they were between base ages 25 to 35. The bottom panel of Figure 1 separates out average earnings for workers who were in their parents' neighborhoods in relative year -1 (light gray), and those who were not in their parents' neighborhoods (dark gray). Appendix H.2 presents the underlying statistics for all the tables and figures in the main text, including coefficients, standard errors and the number of observations.

The top panel of Figure 1 delivers three messages that many prior studies have documented and summarized, including Jacobson et al. (1993), Fallick (1996), Kletzer (1998), and Davis and von Wachter (2011). First, displacement leads to a large initial drop in earnings of about \$10,000, and we discuss the precise timing of the decline in Appendix A.5. Second, even 10 years after the displacement event, the earnings of displaced workers have not caught up with the earnings of non-displaced workers. Finally, there do not appear to be differences in the trends of earnings prior to the displacement event.

The bottom panel of Figure 1 shows our main finding: the earnings of workers who live in the same tract as their parents are lower, on average, but these workers' earnings appear to recover fully after a job displacement. Displaced workers who were not in the same neighborhoods as their parents see large earnings losses relative to a group of workers who were not displaced and not in the same neighborhoods, and this gap persists over the next 10 years. In stark contrast, those workers who were in the same neighborhoods as their parents in the year prior to the displacement event see a much healthier recovery in earnings. Prior to the displacement event the difference in the earnings of the displaced and non-displaced who live in their parents' neighborhoods is about \$4,000. The earnings of the displaced workers recover to this pre-displacement difference about 6 years after the displacement event. The gap in earnings between these displaced workers and the non-displaced group closes entirely within 9 years of the displacement event. We present similar evidence for the natural logarithm of earnings in Appendix A.6.

3 Empirical Methodology

Our baseline method uses the typical difference-in-difference approach pioneered by Jacobson et al. (1993), together with propensity score reweighting, similar to Couch and Placzek (2010). The typical difference-in-difference approach accounts for time-invariant differences between displaced and not displaced individuals. The propensity score reweighting chooses a comparison group of young adults who live farther from their parents but who lose similar jobs and who have similar characteristics as those who live closer to their parents. This reweighting addresses the possibility that observable differences between workers who live closer to their parents and those who live farther away could lead to the differences in post-displacement earnings.

3.1 Propensity Score Reweighting

Following the literature on propensity score reweighting (Rosenbaum and Rubin, 1983; Hirano, Imbens, and Ridder, 2003), we reweight observations from different groups of young adults so that they have the same characteristics as young adults who are displaced while living closer to their parents. This involves reweighting three separate groups of workers so that each group has similar observable characteristics as the group of displaced workers who live closer to their parents.

We obtain a weight for each person, which is a function of conditional and unconditional probabilities, with details in Appendix B.1.1. We estimate the probabilities conditional on covariates using a multinomial logit regression, as suggested by Imbens (2000). The predictors relate both to the workers and to the jobs that they lose. In terms of the worker, we include a dummy for being college educated, a linear term for the worker's completed schooling, a linear term for the worker's age, a dummy for whether the worker is male, a dummy for whether the worker is African American, and the employment-to-population ratio in the worker's county.[6] In terms of the jobs that workers lose, we include a linear and a square term in earnings, the average year-to-year changes in earnings, wages, a dummy for one-digit PSID occupations, and a linear term for the worker's tenure. In Appendix B.1.2 we discuss the implications of controlling for initial wages. For the logit regression, all variables are measured pre-displacement, the regression is unweighted, and we average several variables for the three years leading up to the event to limit measurement error (ignoring years where the variables are not observed). We discuss robustness to different weighting choices in Section 4.1.

Table 2 shows a validation of the weights for young adults, using several of the covariates used in the reweighting as well as some other variables that were not included in the reweighting. It reports the mean of the variable among different groups of workers and p-values of a Wald test of equality with the group of people who were young and lost their jobs while living in the same neighborhood as their parents. In keeping with our regression analysis, it includes each person separately for each year they were in the sample of people at risk for a displacement. Panel A shows these statistics using the initial PSID person weights,

[6] Employment-to-population ratios were obtained by merging in information from County Business Patterns (CBP) and population information from the National Historical Geographic Information System, where we linearly interpolate between census years. The former data are available from 1969 onwards.

and Panel B uses the propensity score reweights. As intended, the differences across samples disappears in Panel B where each group has similar initial earnings, ages, years of education, gender, tenure, a similar likelihood of having children, and lives in places with similar labor market conditions. To measure labor market conditions, we use county-level unemployment rates from the Local Area Unemployment Statistics (LAUS) program, which are available after 1980. The weights also align the earnings trajectories of non-displaced workers with nearby parents and faraway parents after relative year 0, as shown in Appendix B.1.3. These reweighted means also support our preliminary evidence in Section 2.3 about differential post-displacement recoveries of young workers by proximity to parents.

The reweighting does not control for unobservable difference between the two groups, but we suspect that these are unlikely to change our results for two reasons. First, our results do not appear to be sensitive to either including additional characteristics, or taking characteristics away from our reweighting procedure, as shown in Section 4.1. Second, we suspect that the bias induced by unobservables would go in the other direction: A worker who knows that they are less able to adapt to new circumstances should prefer to live closer to their parents, because moving involves adapting to new circumstances and because parents can act as supports. If this is the case, then selection on unobservables should lead to more severe effects of job displacements among workers who live in their parents' neighborhoods.

3.2 Regression Specification

To control for differences between workers who are displaced and not displaced, we estimate the following difference-in-difference equation:

$$e_{iat} = \alpha_{ia} + \gamma_t + X_{iat}(\beta^A + \beta^H H_{ia}) + \sum_{k=-4}^{6^+} \left(D^k_{iat}\delta^k + D^k_{iat} H_{ia}\zeta^k \right) + \epsilon_{iat}, \tag{1}$$

in which e_{iat} is the annual earnings of worker i in calendar year t when the base age is a, α_{ia} represents a worker-base-age dummy, γ_t controls for calender year fixed effects, the X_{iat} terms control for an age quartic, and H_{ia} is a dummy variable indicating whether worker i was neighbors with their parents in the year prior to age a. This dummy is interacted with the age quartic in X_{iat} to allow for different age-earnings profiles for those living near their parents and farther away, which captures the different counterfactual (non-displaced) earnings trajectories for these two groups that we observe in Figure 1. The variable D^k_{iat} captures whether worker i at time period t and base age a was displaced k periods ago. The -4 dummy includes anybody who is four or more years before a displacement and the 6+ dummy includes anybody who is six or more years after a displacement. We omit the -2 dummy so all results are relative to two years before the displacement event, and we provide more details about this choice in Appendix B.2.

The coefficient δ^k captures the change in earnings for a worker who was displaced k periods ago and was not living in their parents' neighborhoods prior to the displacement relative to other workers who were not neighbors with their parents and were not displaced. The coefficient ζ^k captures the additional earnings effect of being neighbors with your parents.

Our specification (equation 1) includes an individual fixed effect, as in Jacobson et al. (1993), that accounts for our stacking of individual records, described in Section 2.2. The fixed effect in our specification is for each copy of an individual, denoted by α_{ia}, because this fixed effect varies with worker (i) and base age (a). As such, our specification is identical to equation (2) in Jacobson et al. (1993) except that we allow the fixed effect to also account for various copies of an individual in our dataset, and our specification allows the age-earnings profile to vary by proximity to parents. This fixed effect is identified by the omitted displacement year (-2), which differs for every copy of an individual. The fixed effects control for permanent differences in earnings between displaced workers living close to and farther from home at a given base age.

We estimate equation (1) using pooled data from all base ages, whereas Davis and von Wachter (2011) estimate separate event-study regressions for each calendar year and then average the displacement-dummy coefficients. Our approach imposes some mild additional restrictions relative to equation (1) in Davis and von Wachter (2011). In particular, Davis and von Wachter (2011) allow all of their coefficients to vary by calendar year, whereas we restrict our coefficients to be the same for every base age, with the exception of the individual fixed effects, which vary with base age and individual. We also do not include the pre-displacement average earnings of individuals as in Davis and von Wachter (2011) because these are accounted for by our reweighting procedure.

In our baseline results, we cluster standard errors at the worker level to account for repeated observations from the same individuals due to our stacked dataset.

4 Results

In this section, we present three sets of results about parental proximity and the earnings of young workers. First, we document that the earnings of young adults living near their parents completely recover after a job displacement. In contrast, the earnings of those living farther away permanently decline. Second, point estimates suggest that the differential earnings recoveries are due to employment, hours, and wages; however, we find statistically significant differences only for wages. Third, we present hetereogenous treatment effects that suggest that help with childcare might be important, and we rule out stories based on geographic mobility, co-residence, and that parental proximity simply represents other labor market benefits near where young adults grew up.

4.1 Earnings Losses by Geographic Proximity to Parents

Young adults with nearby parents experience a recovery in post-displacement earnings, whereas those with faraway parents see large and permanent declines, as shown in Figure 2. This figure shows the δ_k and ζ_k coefficients from equation (1) with the propensity score weights. There are substantial drops in earnings following a displacement for both groups of young adults, about \$11,000. A steadily increasing difference in earnings emerges in the years after displacement, however, as the group who lives closer to their parents makes up much of the earnings penalty from the displacement. People living closer to their parents see no detectable earnings losses in years 4 and 5. Statistically, the group of workers who were living closer to their parents earns significantly more than the other group 6 years after displacement, at the 5 percent level. These differences do appear to be economically significant as well, with a difference of about \$10,000, or about 30 percent of initial earnings. People who live farther away from their parents have permanent earnings losses, and their earnings are always statistically significantly different from zero after the displacement. We present coefficients 10 years out in Appendix C.1 with the same conclusions.

We use parental deaths as an instrument for parental proximity, in addition to using propensity score reweighting, as a way to address the nonrandom location choice of young adults, with details in Appendix C.2. This approach makes our results much more dramatic, but parental death is a weak instrument. We also reject the null hypothesis of an overidentification test, implying either treatment effect heterogeneity or that the instrument fails an exclusion restriction.

We present results without propensity score reweighting, using a continuous measure of proximity, and for different samples in Appendices C.3 through C.5. None of these make a material difference to our main finding. Our results are stronger when we use the longitudinal weights provided by the PSID instead of our propensity score weights, largely because those living farther away from parents have more to lose than those with nearby parents and so experience larger declines in earnings at the time of displacement (Appendix C.3). Our results are not specific to the definition of census tracts because using a distance-based measure of proximity (based on latitudes and longitudes of block groups) gives similar results (Appendix C.4). Broadening our sample to include household heads and their wives, which reduces the fraction of males in our sample, yields very similar results (Appendix C.5).

We also pursue several robustness exercises to different weighting choices as well as an interacted model in Appendix D. Appendix D.1 presents the main results from the reweighting with different sets of characteristics, and the results tend to be quite stable once one controls for basic educational and demographic differences. In Appendix D.2, we present similar results from a model that allows for several interaction terms in the regression specification, which resembles the reweighting exercise. In Appendix D.3, we present similar results from the same propensity score approach but using only a subset of observations where there is strong common support according to the selection method proposed by Crump, Hotz, Imbens, and Mitnik (2009).

4.2 Employment, Hours, Wages and Unemployment Duration

Point estimates using propensity score reweighting suggest that the differential earnings recoveries among workers living close to parents are due to employment, hours, and wages. However, we find statistically significant differences only for wages. Figure 3 presents the results from estimating equation (1) with three different outcomes: an indicator for whether the person worked positive hours in the previous calendar year, the number of hours worked during the previous calendar year (conditional on positive hours), and earnings per hour.

Young adults with nearby parents experience smaller reductions in employment after displacement and a quicker recovery than those with faraway parents, as shown in Figure 3 panel A. The probability of positive hours last year falls during the survey after the displacement event, as some workers experience an entire year out of work. Displaced workers with nearby parents are about 2 percentage points (pp) less likely to have employment in the year after the displacement, and workers living farther away experience a 5 pp drop in employment. The difference between the two groups is not statistically significant. Employment for those with nearby parents recovers fully six years after displacement, whereas those with faraway parents see a persistent 4-pp decline, but the long-run difference is not quite statistically significant (p-value = 0.08).

Point estimates suggest that young adults with nearby parents see a slightly larger decline in hours than those living farther away and a faster recovery, as shown in Figure 3 Panel B. On impact, the reduction in hours worked last calendar year (conditional on positive hours) is larger for those with nearby parents (about 430 hours) than for those living farther away (about 320 hours), but the difference is not statistically significant. The recovery in hours appears stronger for those living in their parents' neighborhoods. In particular, from two to six+ years after the displacement event, there is a statistically positive increase in the hours of those living in their parents' neighborhoods, whereas those living farther away see their hours plateau. We cannot reject, however, the null hypothesis that the hours recoveries are the same for the two groups.

Individuals with nearby parents experience smaller declines in wages in their new jobs and a faster recovery in the years following displacement than workers with parents farther away, as shown in Figure 3 Panel C. At the time of displacement, those living in their parents' neighborhoods experience a significantly smaller hourly earnings reduction (about \$1/hr) than those living farther away (about $3/hr). Moreover, workers who lived in the same neighborhoods as their parents at the time of displacement see their wages recover fully, and workers who lived farther away see essentially no recovery.

Unemployment duration for the two groups is remarkably similar around displacement, suggesting that longer job searches are unlikely to explain our main finding. The PSID allows us to look directly at the number of weeks a worker spent unemployed in the previous calendar year. Figure 4 presents these results separately for those living close to their parents and those living farther away. Not surprisingly, in the year of displacement, the time spent unemployed rises sharply by about seven weeks, but the increase is remarkably similar for the two groups. Over the next few years, the decline in weeks spent unemployed is also indistinguishable. We see these duration results as evidence that longer job search is unlikely to be an important explanation for the differing post-displacement earnings outcomes of the two groups.

4.3 Heterogeneous Earnings Effects of Parental Proximity

One advantage of living in the same neighborhood as one's parents is convenient help with childcare. Living in the same neighborhood as parents can be especially helpful if one needs childcare unexpectedly (Compton and Pollak, 2014), and if multiple parents commute to work (Rosenthal and Strange, 2012). Childcare could be particularly valuable after a job displacement when workers experience heightened rates of divorce, lower self-rated health, higher rates of depression, and increased social withdrawal (Charles and Stephens, 2004; Burgard, Brand, and House, 2007; Brand, 2015). Childcare from workers' parents could also allow workers to contribute additional hours on a post-displacement job. And additional hours could be particularly informative to employers (Jovanovic, 1979; Menzio, Telyukova, and Visschers, 2016) and lead to the higher post-displacement wages that we observe. For example, Engellandt and Riphahn (2005) document firms screening new employees using temporary contracts that lead employees to work more unpaid overtime.

In this section, we present several heterogeneous earnings effects that suggest that our results are driven by nearby parents providing convenient childcare. First, the benefit of nearby parents is concentrated in states where market-based childcare is costly. Second, young workers who lose jobs from inflexible occupations, in which free and convenient childcare would be most valuable, experience the greatest benefits from nearby parents. Third, the effects for young workers are strongest when children live in the same neighborhood as their parents as opposed to close but farther away. Fourth, older workers, who are less likely to have children at home, do not benefit from living closer to their parents. Finally, our main result is driven by workers who both live close to their parents and who end up having children. We present evidence about workers' parents' labor supply that also supports the interpretation that workers' nearby parents assist with childcare in Section 5.1.

This section also presents evidence that our main result is not driven by geographic mobility or co-residence and evidence that parental proximity matters for young workers independently from where they grew up.

Throughout this section, we often estimate variants of the following equation:

$$\begin{aligned} e_{iat} &= \alpha_{ia} + \gamma_t + X_{iat}(\beta_1 + \beta_2 H_{ia} + \beta_3 C_{ia} + \beta_4 H_{ia} C_{ia}) \\ &\quad + \sum_{k=-4}^{6+} (D^k_{iat}\delta^k_1 + D^k_{iat} H_{ia}\delta^k_2 + D^k_{iat} C_{ia}\delta^k_3 + D^k_{iat} H_{ia} C_{ia}\delta^k_4) + \epsilon_{iat}, \end{aligned} \tag{2}$$

which is similar to our baseline specification (equation 1) but allows the age quartic and the displacement dummies to vary by proximity to parents (H_{ia}), another variable (C_{ia}), and their interaction ($H_{ia}C_{ia}$).

4.3.1 Evidence About Childcare

Displaced workers who live in states where childcare is more expensive benefit more from parental proximity than workers with affordable market-based childcare. We use data on (real) expenditures on childcare among all PSID respondents across all years of the PSID to group workers into states with low and high childcare costs before displacement, demarcated by the median although our results are robust to this cutoff. We include this new dummy variable in equation (2) as the variable C_{ia}. Figure 5 shows the relevant coefficients and highlights that young workers who live in states with expensive childcare see improved earnings recoveries when they live close to their parents. There are no statistically detectable differences in the earnings recoveries of workers who live in states with inexpensive childcare.

Young workers who lose jobs from inflexible occupations drive our estimates of the benefits from nearby parents. We define inflexible occupations according to Goldin (2014, Table 2). These occupations include business, health, and "other" occupations that are associated with greater time pressure, greater contact with others, more emphasis on establishing and maintaining interpersonal relationships and less independence in determining tasks, and more discretion over projects. We include the pre-displacement flexibility of the job in equation (2) as the variable C_{ia}. Figure 6 shows that young adults who live far away from their parents prior to displacement experience large and persistent earnings losses whether they lose jobs from flexible or inflexible occupations. Young adults with nearby parents, however, benefit most if they lose jobs from inflexible occupations. In particular, four years after displacement, they have statistically significant improvements in earnings relative to those living farther away and to those with nearby parents who lose flexible jobs. In the long run, those losing inflexible jobs see earnings benefits similar to those with nearby parents who lose flexible jobs.

The earnings benefits for those with nearby parents and losing inflexible jobs are large, but we cannot rule out more modest gains. Our point estimates suggest that these workers' earnings are larger by about \$16,500 and \$7,500 four and five years after displacement, respectively. But the standard errors on these estimates are large (about \$7,000) because the number of records is small (see Appendix Table 10). As such, we cannot rule out that the earnings benefits are much more modest and similar to the earnings benefits of these workers in the long run.

Young adults in the same commuting zone as their parents, but not in the same neighborhood, do not see any post-displacement earnings benefits, which further supports the notion that convenient help from workers' parents may be important. Figure 7 shows the results of estimating equation (2), when we look at young workers who are living very close to their parents (same neighborhood), close to their parents (same commuting zone, but not same neighborhood), and farther away from their parents (outside of the commuting zone) at the time of displacement. Those living close to their parents, but not in the same neighborhoods, experience similar post-displacement earnings outcomes than those who live farther away.

One reason why having parents in the same commuting zone, but not the same tract, could be less valuable is because commuting zones are often quite large. Commuting zones are built up by recursively including counties with significant commuting flows going into the already existing commuting zone (Tolbert and Sizer, 1996). So commuting zones can sprawl out widely, and there is no guarantee that any two counties within the commuting zone have flows between them. Using our measure of distance from the population weighted centroid of the census block group where workers and their parents live, we find that workers who live in the same tract as parents are typically within around 1/2 mile, and that workers who live in the same commuting zone, but a different tract, are typically around 8.5 miles away. Young adults living outside of their parents' neighborhood, but still relatively close, experience some of the benefits of parental proximity, as shown in Appendix E.1.

Close proximity would be particularly valuable for irregular childcare and for maintaining continuity in both the child's and the grandparent's routines around the house. Compton and Pollak (2014), for example, argue that workers' parents are particularly useful for irregular tasks that would benefit from close proximity, like picking up a sick child from school on short notice or taking a child to after-school appointments. Furthermore, Rosenthal and Strange (2012) suggest that proximal childcare is particularly valuable because self-employed workers with childcare responsibilities commute roughly 20 percent less than comparable workers.

Older workers, age 36 to 55, who live in their parents' neighborhoods do not appear to have any improvements in their earnings after a displacement relative to workers who live farther away. Because older workers are less likely to have dependent children, these results further support the idea that young adults benefit from the convenience of their parents' childcare. Figure 8 shows the effects of displacement from equation (1) for older workers. Older workers who live near parents actually appear to earn even less after a displacement, but we cannot reject the null hypothesis that the post-displacement earnings effects are the same for the two groups. These results could also reflect a change in the direction of resource flows, since older workers are more likely to be caring for elderly parents (Lin and Rogerson, 1995; Lin and Wu, 2010; Chari et al., 2015).

Improvements in earnings after a displacement are due to faster earnings recoveries among displaced workers who both live close to their parents and who end up having children, as shown in Table 3. This table shows coefficients from equation (2) in which the additional variable C_{ia} captures whether a worker ever lived in a household with children. To increase sample sizes, the specification also pools some of the displacement dummies, similar to Jacobson et al. (1993, Table 2) and includes periods for pre-displacement, a dip, a drop, a recovery, and long-run earnings losses. Young adults who never have children do not experience any earnings benefit from nearby parents, whereas those who end up with children do. The difference between these two groups at the five+ year horizon is large, about $12,500, and statistically significant. Young adults living farther away see large and permanent earnings losses regardless of whether they ever have children.

4.3.2 Ruling Out Some Other Hypotheses

Geographic mobility is a plausible explanation for the effect, particularly because Appendix E.2 documents a large impact of job displacement on regional mobility, which is consistent with Cao and Stafford (2017) and Huttunen et al. (2018). We check that the post-displacement earnings trajectories are not driven by "movers" by restricting the sample to workers who remain in the same county for all of the years that we observe them after the displacement event. Figure 9 presents the results with this restricted sample together with the original results from Figure 2. The differences between those who resided close to and farther away from their parents are similar for this restricted sample, although the difference at six+ is not quite statistically significant (p-value = 0.06). Therefore, post-displacement mobility patterns are unlikely to account for our baseline earnings results.

Our results support previous work by Kaplan (2012) that nearby parents provide labor market insurance to young workers, but our results extend to children who live in the same neighborhood as their parents but outside of their houses. Appendix E.3 shows that earnings outcomes look similar when we look at workers who are co-residing with their parents, as opposed to living in the same neighborhoods as their parents.

We find that parental proximity matters for young workers independently from where they grew up. The earnings benefit for those who live closer to their parents persists even if one includes additional interactions of the displacement dummies in equation (1) with whether the worker was displaced while living in the county that they grew up in. We present more details in Appendix E.4. These results suggest that close parental proximity has an independent effect on post-displacement earnings from other factors in a worker's home county.

5 Further Evidence on Mechanisms

In this section, we investigate three mechanisms that could lead to higher earnings among workers who live near their parents. First, we present results about the labor supply of workers' parents that suggest that displaced workers receive help with childcare from their nearby parents. Second, we find that transfers of housing and of cash are quite small, and little evidence that children spend longer searching for jobs when they live close to parents (as in Kaplan, 2012). Finally, we find some evidence that workers who live near parents move into their parents' industries after a job displacement, but our estimates are noisy and exhibit a pre-trend.

5.1 Help with Childcare

Here we extend evidence in Section 4.3.1 by showing that workers' parents decrease their market work a few years after their child's displacement. Figure 10 shows that workers' mothers who live in the same tract as their children work outside of the home 400 hours less per year (8 hours per week) six or more years after their child's displacement. The change in hours worked among workers' mothers living near their children is statistically different from zero (at the 5 percent level) and from changes in work hours among mothers living in the same commuting zone but a different tract. The change also differs from workers' mothers who live outside of the commuting zone. Workers' fathers also appear to work outside of the home 300 hours less per year (6 hours less per week) six or more years after their child's displacement. But effects for workers' fathers are not statistically significant at the 5 percent level. Appendix F.1 presents results for both parents in terms of intensive (hours) and extensive (employment) changes, but the results are imprecise.

Several pieces of evidence suggest that the labor supply decreases are driven by help with childcare, not by correlated shocks. First, they occur six or more years after the child's displacement, suggesting that they are not due to a local downturn in a particular five-year period. Instead, they seem to coincide with workers' nearby parents approaching the ages of 62 and 65, when they become eligible for pensions and public health insurance. Only 11 percent of workers' mothers are 62 or older at displacement, but 39 percent of mothers are 62 or older six years after the displacement. Workers parents could have increased contact with their grandchildren after a child's displacement and find it particularly attractive to decrease their labor supply at 62, when they can do so with pensions and medical insurance. Second, the decreases in workers' parents' labor supply are only apparent for parents who live in the same neighborhood as their children. Parents of workers who live in the same city but a different neighborhood do not decrease their labor supply despite being exposed to similar labor market conditions. Third, we control for observable changes in local employment conditions by including measures of county level employment rates affecting both the worker and their parent as controls in the specification. Finally, we include fixed effects for the parent's occupation and the industry of the parent interacted by the current year that control for industry-level changes (e.g., as in Bartik, 1991; Goldsmith-Pinkham, Sorkin, and Swift, 2020) affecting workers and their parents. The worker's displacement could affect the parent's choice of occupation and industry, however, so we lag the occupation and industry of the parent.

5.2 Housing Transfers

The cash value of living with parents is small, on average, rises by only a little after a job displacement, and the increase is no larger among workers who lived closer to their parents before a job displacement. Kaplan (2012) emphasizes that housing transfers can help children to earn more after job displacements by allowing them to be more selective about job offers and by facilitating investments in their careers. Our results do not support this view, although our measures of housing transfers do not capture the option value of moving in with parents.

We use two complementary approaches to measure housing transfers. First, we measure who receives all of their rent as a gift. This survey response is only available if people report that they neither own or rent, however, so this approach misses people who pay below-market rents. Second, we back out how much a child saves by living with their parents. When a child lives with a parent, the PSID collects separate housing costs for each family unit, which we can combine with the composition of the household to construct a level of housing consumption, using an OECD equivalence scale. We then ask how much this level of consumption would cost if the family lived separately. This value gives us an amount of rent that the child would have to pay, were they to live alone and have the same level of consumption, and the difference between this hypothetical rent payment and the child's actual rent payment is the rent transfer from their parents. We provide a more detailed description of the procedure in Appendix F.2.1. We present limitations of our approach in Appendix F.2.2.

A relatively small proportion of 25- to 35-year-olds receive transfers of rent, and these transfers are modest relative to both average rents and the earnings losses after a displacement, as shown in Table 4. Our results suggest that less than 10 percent of the sample receives a transfer of housing at the date of the survey: 8 percent of the sample live with a parent, and about 2 percent receive all of their rent as a gift. Among households who receive a transfer, the average transfer was about $4,200 according to the implied savings, and about $2,600, according to the survey question. Each is much smaller than the average rent of about $6,800 and the estimated earnings losses of about $11,000 in the year after a displacement. We discuss reasons for the difference between the two values in Appendix F.2.3.

The regression coefficients plotted in Figure 11 suggest that housing transfers may spike around displacements but that the spikes around displacement are economically small and statistically insignificant. There is no evidence that there are larger increases in housing transfers among people who lived closer to their parents before they lost their jobs. The point estimates in Panel A show that households are about 4 pp more likely to receive all of their rent as a gift in periods around displacements. A 4 pp increase is quite large relative to the 2 percent likelihood in the baseline sample, but it is a small slice of the overall population. Panel B shows the dollar values of the transfers involved; it also suggests that the transfers are modest at best. The implied rent savings estimates are noisy, but we can reject that there is an increase of $500 or more per year coming from a PSID parent. This is at least an order of magnitude smaller than the earnings losses after a displacement.

Results for reported transfers of money, presented in Appendix F.2.4, suggest that children who lived farther from their parents received larger cash transfers after a displacement, and that children who lived closer received no such transfers.

5.3 Employment in Parents' Industries

Young adults living close to their parents may have more productive job search experiences and healthier earnings post displacement as a result of parental employment networks, as documented in Kramarz and Skans (2014). In particular, after a child's job loss, parents might tap into their employment networks to help their adult children to gain employment in their parents' industries. This idea is related to evidence in Granovetter (1995) and Ioannides and Loury (2004) that the use of friends and relatives is prevalent and productive during the job search process. And, Bayer, Ross, and Topa (2008) and Hellerstein et al. (2019) suggest that these referral networks operate at the neighborhood level.

Point estimates suggest that the probability of working in a parent's one-digit industry rises in the years after displacement among workers living in the same neighborhood as their parents, but our results are not statistically significant, and they exhibit a pre-trend, as shown in Figure 12. The estimated specification is similar to equation (1), but the outcome is whether an employed worker's industry is the same as their parents, and we include additional controls for employment industry shares at the county level from the CBP data. The probability of working in a parent's one-digit industry rises in the years after displacement among workers living in the same neighborhood as their parents. The estimated effect is large and imprecise with a 10 pp increase in working in a parent's industry on a base of about 25 percent. The differences are not statistically significant at the 5 percent level (at relative years two, three, and four the p-values are 0.10, 0.16 and 0.28, respectively). The results are difficult to interpret because Figure 12 suggests that workers who live in the same neighborhood as their parents are more likely to work in the same industry as their parents several years before a displacement, violating the parallel-trends assumption. We do not find any differences in working in parents' industries among older workers and among workers who only live in the same commuting zone (Appendix F.3), which is consistent with our findings that older workers and workers living farther away from their parents do not experience post-displacement earnings benefits.

Industry and occupation switching are similar for the two groups around a displacement event so they cannot account for our baseline finding, as explained in Appendix F.4.

6 Discussion

In this section, we use a simple model to show that our results are consistent with some parents improving their children's wage-offer distributions and that selection based on an unobserved preference for home would actually lead to the opposite results. We also investigate selection on ability and conclude that, on average, workers who live closer to their parents tend to be less skilled, and so this is unlikely to drive our results. Back-of-the-envelope calculations suggest that this improved wage-offer distribution is worth about $1,000 per year for a risk-neutral agent at an average risk of displacement, and the earnings benefits of nearby parents are larger than the scarring effects of recessions.

6.1 Interpreting Our Results

To see the implications of heterogeneity in wage-offer distributions and preferences for home, first consider a simple economy where all workers are ex-ante homogeneous. Workers draw wages from two locations, home and away and these wage distributions are identical. Suppose further that there are no moving costs, but that living at home is associated with positive utility payoff, b.

With homogeneous workers, people who move away are paid more because they need to be willing to forgo the utility payoff in their homes. This is one of the reasons why we pursue the propensity score reweighting exercise: even in an environment with ex-ante identical agents, selection (“luck”) means that the earnings losses of those living farther away from their parents may be larger because they had higher pre-displacement earnings. Our reweighting exercise removes this selection effect because it only uses workers living farther away from their parents who have similar pre-displacement wages to those living close to their parents.

Once one controls for differences in workers' initial jobs, however, the post-displacement earnings of homogeneous workers will be identical. In order to match our finding of different post-displacement earnings outcomes, we consider three types of worker heterogeneity.

First, suppose that workers differ in their preference for living at home. In particular, suppose that some workers (''homebodies'') prefer to live close to their parents and receive payoff b, while others (''explorers'') have no preference for living close to parents. Notice that, on average, explorers will have higher wages because they receive no utility from being close to their parents and therefore simply seek the highest wage. Moreover, in equilibrium, those observed away from home are more likely to be explorers than homebodies. As before, the reweighting scheme will address pre-displacement selection on wages, but since workers away are more likely to be explorers, they will, on average, have better wage outcomes after the displacement event. As such, this sort of heterogeneity works against our empirical findings where, after a displacement, those close to their parents tend to have better earnings outcomes than those farther away.

Second, suppose that all workers receive utility b when living near parents, but workers differ in the wage-offer distribution they face. In particular, away from home, homebodies face a wage-offer distribution with mean μ, but at home the mean is $\mu + w_0$, where $w_0 > 0$. Explorers do not have this advantage and face the same distribution at home and away, with mean μ. Notice that, in equilibrium, a worker who is away is more likely to be an explorer because homebodies have a stronger preference for home as a result of the better wage-offer distribution. Also notice that homebodies will, on average, have higher wages due to the wage shifter, w_0. However, note that the expected wage of those at home could be below the expected wage of those away due to the selection on b. As before, the reweighting scheme will address pre-displacement selection on wages, but since workers away are more likely to be explorers they will, on average, have worse wage outcomes after the displacement event. Therefore, our main empirical finding can be explained by differences in the wage-offer distribution.[7]

Third, suppose that all workers receive utility b when living near parents, but workers differ in their unobserved ability and that the return to this ability can be earned in both locations, home and away. In this simple framework, when we compare workers in different locations who have the same wage, as we do with our reweighting approach, the workers

[7] Notice that with a positive moving cost, $c > 0$, even if *all* workers faced a better wage-offer distribution at home, we would get the desired result. This is because people who moved away have to pay the cost, c, to move back home and, as a result, they will on average have worse post-displacement wage outcomes.

living at home will, on average, have higher ability (see Appendix F.5 for a proof). In principle, this selection on worker ability could be driving our empirical results; however, previous literature finds that this simple intuition is not supported by the evidence. In fact, Topel (1986), Bound and Holzer (2000), and Notowidigdo (2019) all find that low-skilled workers are less mobile in response to adverse labor demand conditions so that geographic selection on unobserved ability is unlikely to explain our main findings.

6.2 Economic Significance

We estimate that the value of labor market insurance provided by nearby parents is at least \$1,000 per year. To calculate the value of an improved post-displacement wage-offer distribution, we take an estimate of the earnings differences after displacement, and we modify it to represent an expected value for a worker that has an average lifetime risk of being displaced. We begin with the differences in post-displacement earnings from Section 4.1, and we discount them by an annual interest rate of 4 percent. This simple calculation implies that the lifetime benefit (over a career lasting 35 more years) of living close to parents, conditional on a displacement event, is about \$100,000. In our sample, the probability that a young worker experiences displacement is about 20 percent, so the expected total benefit of living close to home is about \$20,000 for someone at average risk of displacement. This suggests that the benefit of parental proximity after job displacement is associated with an annual value of about \$1,000. Note that this calculation only assumes the benefits of being close to home will apply after a job displacement; if workers received similar benefits after less severe labor market disruptions, our estimates would be a lower bound on the wage benefits of being close to home.

Our results suggest that the benefit of parental proximity to young adults is larger than the scarring effects of recessions. Davis and von Wachter (2011, Figure 4, Panel B) suggest that, six years after displacement and beyond, workers displaced in recessions experience earnings losses that are about \$5,000 larger than the earnings losses experienced by workers displaced in expansions. Our baseline results (Figure 2) suggest that nearby parents increase the post-displacement earnings of their young adults by about \$10,000 six years after the displacement event. This comparison suggests that the effects of nearby parents are economically meaningful.

7 Conclusion

Young adults benefit from stronger earnings recoveries after job displacements when they live in the same neighborhood as their parents. We find evidence that nearby workers' earnings improve because of their parents help with childcare. For example, we find that the effects are stronger in places where childcare is more expensive, among workers who work in less flexible occupations, and among workers who end up having children. And workers' parents decrease their number of hours worked after their child's displacement. Parents could also help by finding jobs for their children, but we find only weak direct evidence of this channel.

Our finding that parental help with childcare is particularly helpful after a job displacement implies that childcare programs have several mostly unexplored benefits. First, they provide earnings benefits to nearby workers. Based on our empirical findings, simple calculations suggest that parental proximity after job displacements is associated with an annual value of about $1,000 and larger than the scarring effects of recessions. Second, they can encourage workers' mobility by allowing workers without nearby parents to experience similar benefits. Even if a childcare program perfectly crowds out parents' efforts, it could function as a place-based policy that encourages workers' mobility.[8] Third, workers' parents could benefit from not having to provide as much childcare.

In the future, we hope that researchers will use complementary data sources and modeling strategies to investigate our finding. Many of our results would not be possible with administrative data, but larger datasets would increase precision and allow more flexibility. For example, administrative data would be helpful in identifying the effects of parental employment networks. Administrative data from a country where the government provides more childcare could also help us to further understand the effects of inexpensive childcare. Another complement would be building and estimating a model that incorporates parental location (Kennan and Walker, 2011; Coate, 2017) and matches the earnings losses of displaced workers (Jarosch, 2015; Krolikowski, 2017).

[8] For recent reviews of place-based policies, see Glaeser and Gottlieb (2008), Kline and Moretti (2014), Neumark and Simpson (2015), and Criscuolo, Ralf, Overman, and Reenen (2019).

References

Albouy, David and Mike Zabek (2016). ‘‘Housing Inequality.’’ doi:10.3386/w21916. NBER Working Paper 21916.

Austin, Benjamin, Edward Glaeser, and Lawrence Summers (2018). ‘‘Jobs for the Heartland: Place-Based Policies in 21st-Century America.’’ *Brookings Papers on Economic Activity*, 49(1), pp. 151--255. Available at https://ideas.repec.org/a/bin/bpeajo/v49y2019i2018-01p151-255.html.

Bartik, Timothy J. (1991). *Who Benefits from State and Local Economic Development Policies?* W.E. Upjohn Institute. URL http://www.jstor.org/stable/j.ctvh4zh1q.

Bayer, Patrick, Stephen L. Ross, and Giorgio Topa (2008). ‘‘Place of Work and Place of Residence: Informal Hiring Networks and Labor Market Outcomes.’’ *Journal of Political Economy*, 116(6), pp. 1150--1196. doi:10.1086/595975.

Bishop, Kelly C (2007). ‘‘A Dynamic Model of Location Choice and Hedonic Valuation.’’ Available at https://are.berkeley.edu/~ligon/ARESeminar/Papers/bishop07.pdf.

Bound, John and Harry J. Holzer (2000). ‘‘Demand Shifts, Population Adjustments, and Labor Market Outcomes during the 1980s.’’ *Journal of Labor Economics*, 18(1), pp. 20--54. doi:10.1086/209949.

Brand, Jennie E. (2015). ‘‘The Far-Reaching Impact of Job Loss and Unemployment.’’ *Annual Review of Sociology*, 41(1), pp. 359--375. doi:10.1146/annurev-soc-071913-043237.

Brown, Charles (1996). ‘‘Notes on the ‘‘SEO’’ or ‘‘Census’’ Component of the Panel Study of Income Dynamics.’’ Available at https://psidonline.isr.umich.edu/Publications/Papers/tsp/1996-03_Notes_on_the_SEO_C_Brown.pdf.

Bui, Quoctrung and Claire Cain Miller (2015). ‘‘The Typical American Lives Only 18 Miles From Mom.’’ NY Times, The Upshot. Available at https://www.nytimes.com/interactive/2015/12/24/upshot/24up-family.html?_r=0.

Burda, Michael C. and Antje Mertens (2001). ‘‘Estimating Wage Losses of Displaced Workers in Germany.’’ *Labour Economics*, 8(1), pp. 15--41. doi:10.1016/S0927-5371(00)00022-1.

Burgard, Sarah A., Jennie E. Brand, and James S. House (2007). ‘‘Toward a Better Estimation of the Effect of Job Loss on Health.’’ *Journal of Health and Social Behavior*, 48(4), pp. 369--384. doi:10.1177/002214650704800403.

Cao, Yipei and Frank P. Stafford (2017). ‘‘Relocation, Intergenerational Transfers and Extended Family Insurance.’’ doi:10.2139/ssrn.2926781. Working Paper.

Chari, Amalavoyal V., John Engberg, Kristin N. Ray, and Ateev Mehrotra (2015). ‘‘The Opportunity Costs of Informal Elder-Care in the United States: New Estimates from the American Time Use Survey.’’ *Health Services Research*, 50(3), pp. 871--882.

doi:10.1111/1475-6773.12238.
Charles, Kerwin Kofi and Melvin Stephens (2004). ‘‘Job Displacement, Disability, and Divorce.’’ *Journal of Labor Economics*, 22(2), pp. 489--522. doi:10.1086/381258.
Chetty, Raj, John N Friedman, Nathaniel Hendren, Maggie R Jones, and Sonya R Porter (2018). ‘‘The Opportunity Atlas: Mapping the Childhood Roots of Social Mobility.’’ Working Paper 25147, National Bureau of Economic Research. doi:10.3386/w25147.
Chetty, Raj, Nathaniel Hendren, Patrick Kline, and Emmanuel Saez (2014). ‘‘Where Is the Land of Opportunity? The Geography of Intergenerational Mobility in the United States.’’ *The Quarterly Journal of Economics*, 129(4), pp. 1553--1623. doi:10.3386/w19843.
Coate, Patrick (2017). ‘‘Parental Influence on Labor Market Outcomes of Young Workers.’’ Available at https://sites.google.com/site/patrickecoate/research.
Compton, Janice and Robert A Pollak (2014). ‘‘Family Proximity, Childcare, and Women’s Labor Force Attachment.’’ *Journal of Urban Economics*, 79, pp. 72--90. doi:10.1016/j.jue.2013.03.007.
Compton, Janice and Robert A. Pollak (2015). ‘‘Proximity and Co-residence of Adult Children and their Parents in the United States: Descriptions and Correlates.’’ *Annals of Economics and Statistics*, Special Issue on the Economic of Gender (117/118), pp. 91--114. doi:10.15609/annaeconstat2009.117-118.91.
Corak, Miles and Patrizio Piraino (2011). ‘‘The Intergenerational Transmission of Employers.’’ *Journal of Labor Economics*, 29(1), pp. 37--68. doi:10.1086/656371.
Couch, Kenneth A. and Dana W. Placzek (2010). ‘‘Earnings Losses of Displaced Workers Revisited.’’ *American Economic Review*, 100(1), pp. 572--89. doi:10.1257/aer.100.1.572.
Cowen, Tyler (2017). *The Complacent Class: The Self-Defeating Quest for the American Dream*. St. Martin’s Press.
Criscuolo, Chiara, Martin Ralf, Henry G. Overman, and John Van Reenen (2019). ‘‘Some Causal Effects of an Industrial Policy.’’ *American Economic Review*, 109(1), pp. 48--85. doi:10.1257/aer.20160034. URL http://www.aeaweb.org/articles?id=10.1257/aer.20160034.
Crump, Richard K., Joseph V. Hotz, Guido W. Imbens, and Oscar A. Mitnik (2009). ‘‘Dealing with Limited Overlap in Estimation of Average Treatment Effects.’’ *Biometrika*, 96(1), pp. 187--199. doi:10.1093/biomet/asn055.
Dalton, Michael (2013). ‘‘Family Transfers in Response to Unemployment.’’ Working Paper.
Davis, Steven J. and Till M. von Wachter (2011). ‘‘Recessions and the Costs of Job Loss.’’ *Brookings Papers on Economic Activity*, Fall(1), pp. 1--72. Available at https://www.brookings.edu/wp-content/uploads/2011/09/2011b_bpea_davis.pdf.
Diamond, Rebecca (2016). ‘‘The Determinants and Welfare Implications of US Workers’ Diverging Location Choices by Skill: 1980-2000.’’ *American Economic Review*, 106(3), pp.

479--524. doi:10.1257/aer.20131706.

DiNardo, John, Nicole M Fortin, and Thomas Lemieux (1996). ‘‘Labor Market Institutions and the Distribution of Wages, 1973-1992: A Semiparametric Approach.’’ *Econometrica*, 64(5), pp. 1001--1044. doi:10.2307/2171954.

Engellandt, Axel and Regina T. Riphahn (2005). ‘‘Temporary Contracts and Employee Effort.’’ *Labour Economics*, 12(3), pp. 281 -- 299. doi:https://doi.org/10.1016/j.labeco.2003.11.006.

Fallick, Bruce C. (1996). ‘‘A Review of the Recent Empirical Literature on Displaced Workers.’’ *Industrial and Labor Relations Review*, 50(1), pp. 5--16. doi:10.1177/001979399605000101.

Flaaen, Aaron, Matthew D. Shapiro, and Isaac Sorkin (2019). ‘‘Reconsidering the Consequences of Worker Displacements: Firm versus Worker Perspective.’’ *American Economic Journal: Macroeconomics*, 11(2), pp. 193--227. doi:10.1257/mac.20170162.

Fortin, Nicole, Thomas Lemieux, and Sergio Firpo (2011). ‘‘Chapter 1 – Decomposition Methods in Economics.’’ In *Handbook of Labor Economics*, volume 4, pp. 1--102. Elsevier B.V. doi:10.1016/S0169-7218(11)00407-2.

Glaeser, Edward and Joshua Gottlieb (2008). ‘‘The Economics of Place-Making Policies.’’ *Brookings Papers on Economic Activity*, 2008(1), pp. 155--253. doi:10.1353/eca.0.0005.

Goldin, Claudia (2014). ‘‘A Grand Gender Convergence: Its Last Chapter.’’ *American Economic Review*, 104(4), pp. 1091--1119. doi:10.1257/aer.104.4.1091.

Goldsmith-Pinkham, Paul, Isaac Sorkin, and Henry Swift (2020). ‘‘Bartik Instruments: What, When, Why, and How.’’ Availabe at `https://sites.google.com/site/isaacsorkin/papers`.

Granovetter, Mark S. (1995). *Getting a Job: A Study of Contacts and Careers*. University of Chicago Press, Chicago.

Havnes, Tarjei and Magne Mogstad (2011). ‘‘Money for Nothing? Universal Child Care and Maternal Employment.’’ *Journal of Public Economics*, 95(11), pp. 1455 -- 1465. doi:10.1016/j.jpubeco.2011.05.016.

Hellerstein, Judith K., Mark J. Kutzbach, and David Neumark (2019). ‘‘Labor Market Networks and Recovery from Mass Layoffs: Evidence from the Great Recession Period.’’ *Journal of Urban Economics*, 113, p. 103,192. doi:10.1016/j.jue.2019.103192.

Hirano, Keisuke, Guido W. Imbens, and Geert Ridder (2003). ‘‘Efficient Estimation of Average Treatment Effects Using the Estimated Propensity Score.’’ *Econometrica*, 71(4), pp. 1161--1189. doi:10.1111/1468-0262.00442.

Huttunen, Kristiina, Jarle Møen, and Kjell G. Salvanes (2018). ‘‘Job Loss and Regional Mobility.’’ *Journal of Labor Economics*, 36(2), pp. 479--509. doi:10.1086/694587.

Imbens, Guido W (2000). ‘‘The Role of the Propensity Score in Estimating Dose-Response

Functions.'' *Biometrika*, 87(3), pp. 706--710. doi:10.1093/biomet/87.3.706.

Ioannides, Yannis M. and Linda Datcher Loury (2004). ''Job Information Networks, Neighborhood Effects, and Inequality.'' *Journal of Economic Literature*, 42(4), pp. 1056--93. doi:10.1257/0022051043004595.

Jacobson, Louis S., Robert J. LaLonde, and Daniel G. Sullivan (1993). ''Earnings Losses of Displaced Workers.'' *American Economic Review*, 83(4), pp. 685--709. Available at `https://www.jstor.org/stable/2117574`.

Jardim, Ekaterina S., Gary Solon, and Jacob L. Vigdor (2019). ''How Prevalent Is Downward Rigidity in Nominal Wages? Evidence from Payroll Records in Washington State.'' doi:10.3386/w25470. NBER Working Paper 25470.

Jarosch, Gregor (2015). ''Searching for Job Security and the Consequences of Job Loss.'' Available at `https://sites.google.com/site/gregorjarosch/research`.

Johnson, Janna E. and Sam Schulhofer-Wohl (2019). ''Changing Patterns of Geographic Mobility and the Labor Market for Young Adults.'' *Journal of Labor Economics*, 37(S1), pp. S199--S241. doi:10.1086/700887.

Jovanovic, Boyan (1979). ''Job Matching and the Theory of Turnover.'' *Journal of Political Economy*, 87(5, Part 1), pp. 972--990. doi:10.1086/260808.

Kaplan, Greg (2012). ''Moving Back Home: Insurance against Labor Market Risk.'' *Journal of Political Economy*, 120(3), pp. 446--512. doi:10.1086/666588.

Kaplan, Greg and Sam Schulhofer-Wohl (2017). ''Understanding the Long-Run Decline in Interstate Migration.'' *International Economic Review*, 58(1), pp. 57--94. doi:10.1111/iere.12209.

Kennan, John and James Walker (2011). ''The Effect of Expected Income on Individual Migration Decisions.'' *Econometrica*, 79(1), pp. 211--251. doi:10.3982/ECTA4657.

Kletzer, Lori G. (1998). ''Job Displacement.'' *Journal of Economic Perspectives*, 12(1), pp. 115--136. doi:10.1257/jep.12.1.115.

Kletzer, Lori G. and Robert W. Fairlie (2003). ''The Long-Term Costs of Job Displacement for Young Adult Workers.'' *Indusrial and Labor Relations Review*, 54(4), pp. 682--698. doi:10.1177/001979390305600408.

Kline, Patrick and Enrico Moretti (2014). ''People, Places and Public Policy: Some Simple Welfare Economics of Local Economic Development Programs.'' *Annual Review of Economics*, 6, pp. 629--62. doi:10.1146/annurev-economics-080213-041024.

Kramarz, Francis and Oskar Nordström Skans (2014). ''When Strong Ties Are Strong: Family Networks and Youth Labor Market Entry.'' *Review of Economic Studies*, 81(3), pp. 1164--1200. doi:10.1093/restud/rdt049.

Krolikowski, Pawel (2017). ''Job Ladders and Earnings of Displaced Workers.'' *American Economic Journal: Macroeconomics*, 9(2), pp. 1--31. doi:10.1257/mac.20140064.

Krolikowski, Pawel (2018). "Choosing a Control Group for Displaced Workers." *Industrial and Labor Relations Review*, 71(5), pp. 1232--1254. doi:10.1177/0019793917743707.

Kuhn, Peter J. (2002). "Summary and Synthesis." In Peter J. Kuhn, editor, *Losing Work, Moving On: International Perspectives on Worker Displacement*, chapter 1, pp. 1--104. W.E. Upjohn Institute for Employment Research, Kalamazoo, MI.

Kurmann, André and Erika McEntarfer (2018). "Downward Nominal Wage Rigidity in the United States: New Evidence from Worker-Firm Linked Data." Available at http://www.andrekurmann.com/.

Lachowska, Marta, Alexandre Mas, and Stephen A. Woodbury (2018). "How Reliable Are Administrative Reports of Paid Work Hours?" Available at http://www.martalachowska.com/research.html.

Lachowska, Marta, Alexandre Mas, and Stephen A. Woodbury (2019). "Sources of Displaced Workers' Long-Term Earnings Losses." doi:10.3386/w24217. NBER Working Paper 24217.

Lin, Ge and Peter A. Rogerson (1995). "Elderly Parents and the Geographic Availability of their Adult Children." *Research on Aging*, 17(3), pp. 303--331. doi:10.1177/0164027595173004.

Lin, I-Fen and Hsueh-Sheng Wu (2010). "Does Social Support Mediate the Association Between Functional Disability and Depression?" Paper presented at the Population Association of America, Dallas, Texas. Available at http://paa2010.princeton.edu/papers/101918.

Mangum, Kyle and Patrick Coate (2018). "Fast Locations and Slowing Labor Mobility." doi:10.2139/ssrn.3175955. Working Paper.

Mazumder, Bhashkar et al. (2016). "Estimating the Intergenerational Elasticity and Rank Association in the United States: Overcoming the Current Limitations of Tax Data." *Research in Labor Economics*, 43, pp. 83--129. doi:10.1108/S0147-912120160000043012.

Menzio, Guido, Irina A. Telyukova, and Ludo Visschers (2016). "Directed Search Over the Life Cycle." *Review of Economic Dynamics*, 19, pp. 38 -- 62. doi:10.1016/j.red.2015.03.002.

Molloy, Raven, Christopher L. Smith, and Abigail Wozniak (2011). "Internal Migration in the United States." *Journal of Economic Perspectives*, 25(3), pp. 173--196. doi:10.1257/jep.25.3.173.

Munshi, Kaivan and Mark Rosenzweig (2016). "Networks and Misallocation: Insurance, Migration, and the Rural-Urban Wage Gap." *American Economic Review*, 106(1), pp. 46--98. doi:10.1257/aer.20131365.

Neumark, David and Helen Simpson (2015). "Chapter 18 - Place-Based Policies." In *Handbook of Regional and Urban Economics*, volume 5, pp. 1197--1287. Elsevier. doi:10.1016/B978-0-444-59531-7.00018-1.

Notowidigdo, Matthew (2019). ‘‘The Incidence of Local Labor Demand Shocks.’’ doi:10.1086/706048. Forthcoming in the *Journal of Labor Economics*.

Panel Study of Income Dynamics (2017). ‘‘Restricted Use Dataset.’’ Produced and distributed by the Survey Research Center, Institute for Social Research, University of Michigan.

Raley, Sara, Suzanne M Bianchi, and Wendy Wang (2012). ‘‘When Do Fathers Care? Mothers’ Economic Contribution and Fathers’ Involvement in Child Care.’’ *American Journal of Sociology*, 117(5), pp. 1422--59. doi:10.1086/663354.

Rosenbaum, Paul R. and Donald B. Rubin (1983). ‘‘The Central Role of the Propensity Score in Observational Studies for Causal Effects.’’ *Biometrika*, 70(1), pp. 41--55. doi:10.1093/biomet/70.1.41.

Rosenthal, Stuart S and William C Strange (2012). ‘‘Female Entrepreneurship, Agglomeration, and a New Spatial Mismatch.’’ *Review of Economics and Statistics*, 94(3), pp. 764--788. doi:10.1162/REST_a_00193.

Rupert, Peter and Giulio Zanella (2018). ‘‘Grandchildren and their Grandparents’ Labor Supply.’’ *Journal of Public Economics*, 159, pp. 89 -- 103. doi:10.1016/j.jpubeco.2017.12.013.

Solon, Gary, Seven J. Haider, and Jeffrey M. Wooldridge (2015). ‘‘What Are We Weighting For?’’ *Journal of Human Resources*, 2(50), pp. 301--316. doi:10.3368/jhr.50.2.301.

Spivey, Christy (2005). ‘‘Time Off At What Price? The Effects of Career Interruptions on Earnings.’’ *Industrial and Labor Relations Review*, 59(1), pp. 119--140. doi:10.1177/001979390505900107.

Stafford, Frank P and Marianne Sundström (1996). ‘‘Time Out For Childcare: Signalling and Earnings Rebound Effects for Men and Women.’’ *Labour*, 10(3), pp. 609--629. doi:10.1111/j.1467-9914.1996.tb00102.x.

Stephens, Melvin (2002). ‘‘Worker Displacement and the Added Worker Effect.’’ *Journal of Labor Economics*, 20(3), pp. 504--537. doi:10.1086/339615.

Stevens, Ann Huff (1997). ‘‘Persistent Effects of Job Displacement: The Importance of Multiple Job Losses.’’ *Journal of Labor Economics*, 15(1), pp. 165--188. doi:10.1086/209851.

Tolbert, Charles M. and Molly Sizer (1996). ‘‘U.S. Commuting Zones and Labor Market Areas: A 1990 Update.’’ ERS Staff Paper # 9614. Available at `https://usa.ipums.org/usa/resources/volii/cmz90.pdf`.

Topa, Giorgio and Yves Zenou (2015). ‘‘Chapter 9 - Neighborhood and Network Effects.’’ In *Handbook of Regional and Urban Economics*, volume 5, pp. 561--624. Elsevier. doi:10.1016/B978-0-444-59517-1.00009-X.

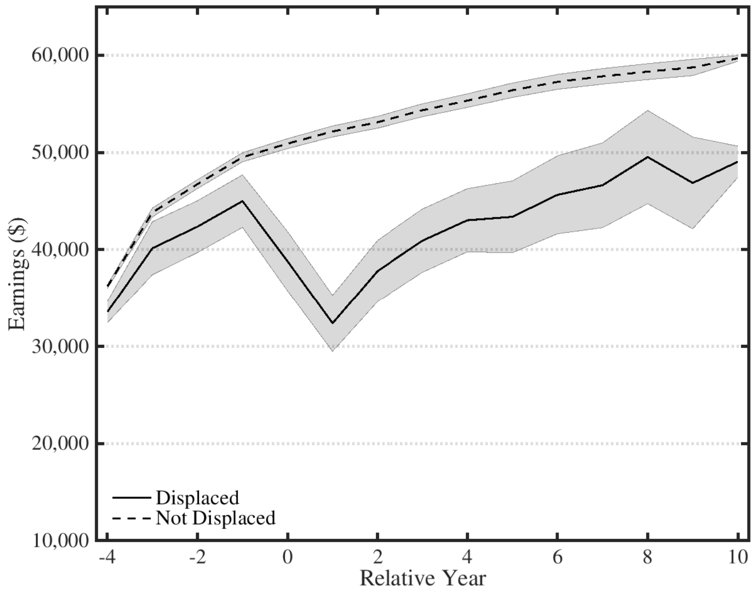
Topel, Robert H. (1986). ‘‘Local Labor Markets.’’ *Journal of Political Economy*, 94(3), pp.

S111--S143. doi:10.1086/261401.

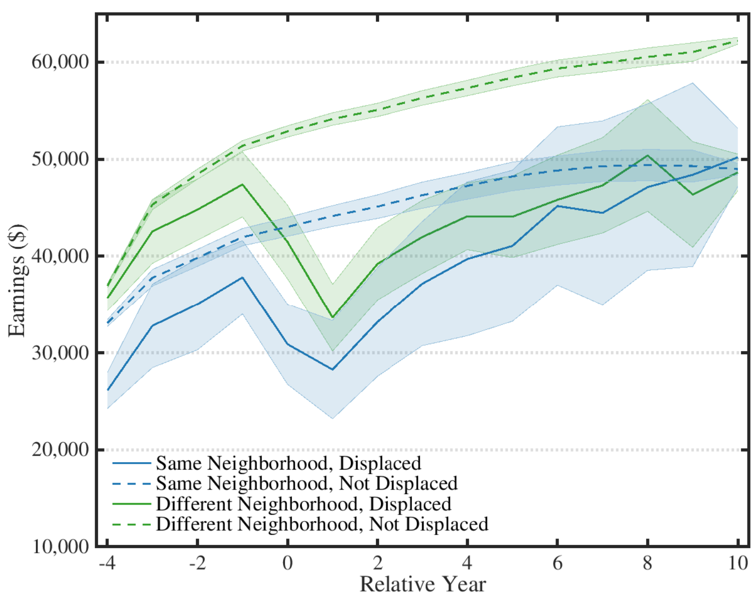
United States Census Bureau (2016). ‘‘Historical Living Arrangements of Adults.’’ Available at
https://www.census.gov/data/tables/time-series/demo/families/adults.html.

Vilhuber, Lars (2018). ‘‘LEHD Infrastructure S2014 files in the FSRDC.’’ Working Papers 18-27, Center for Economic Studies, U.S. Census Bureau. URL
https://ideas.repec.org/p/cen/wpaper/18-27.html.

von Wachter, Till, Jae Song, and Joyce Manchester (2009). ‘‘Long-Term Earnings Losses due to Mass Layoffs During the 1982 Recession: An Analysis Using Longitudinal Administrative Data from 1974 to 2004.’’ Available at
http://www.econ.ucla.edu/tvwachter/papers/mass_layoffs_1982.pdf.



(a) Average Earnings for Displaced and Non-Displaced Workers



(b) Average Earnings for Workers in Their Parents' Neighborhoods and Not

Figure 1: Average Earnings for Young Workers by Proximity to Parents

Note: Young workers who live in their parents' neighborhoods experience stronger earnings recoveries after a job displacement. These figures plot average earnings for displaced and not displaced young workers (aged 25 to 35 in year zero). The shading represents 95 percent confidence intervals, computed by clustering standard errors at the worker level. All of the workers were employed in a job for at least two years. Workers were displaced if they reported that they were no longer in that job because the plant closed, because they were laid off, or because they were fired. The subgroups in Panel B are defined based on how close they lived to their parents before they were at risk of a displacement. The same neighborhood group lives in the same census tract as their parents, while the different neighborhood group lives in a different census tract. See Section 2 for more information on the sample construction, data, and definitions.

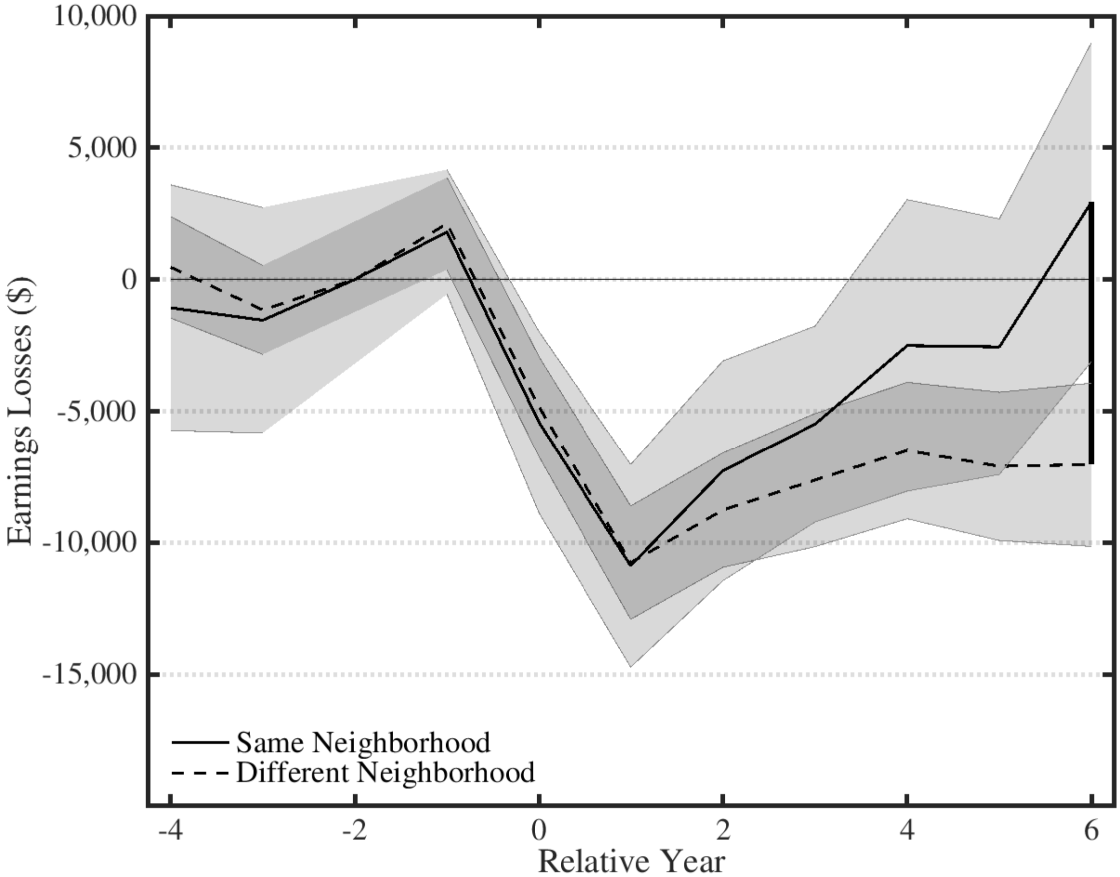
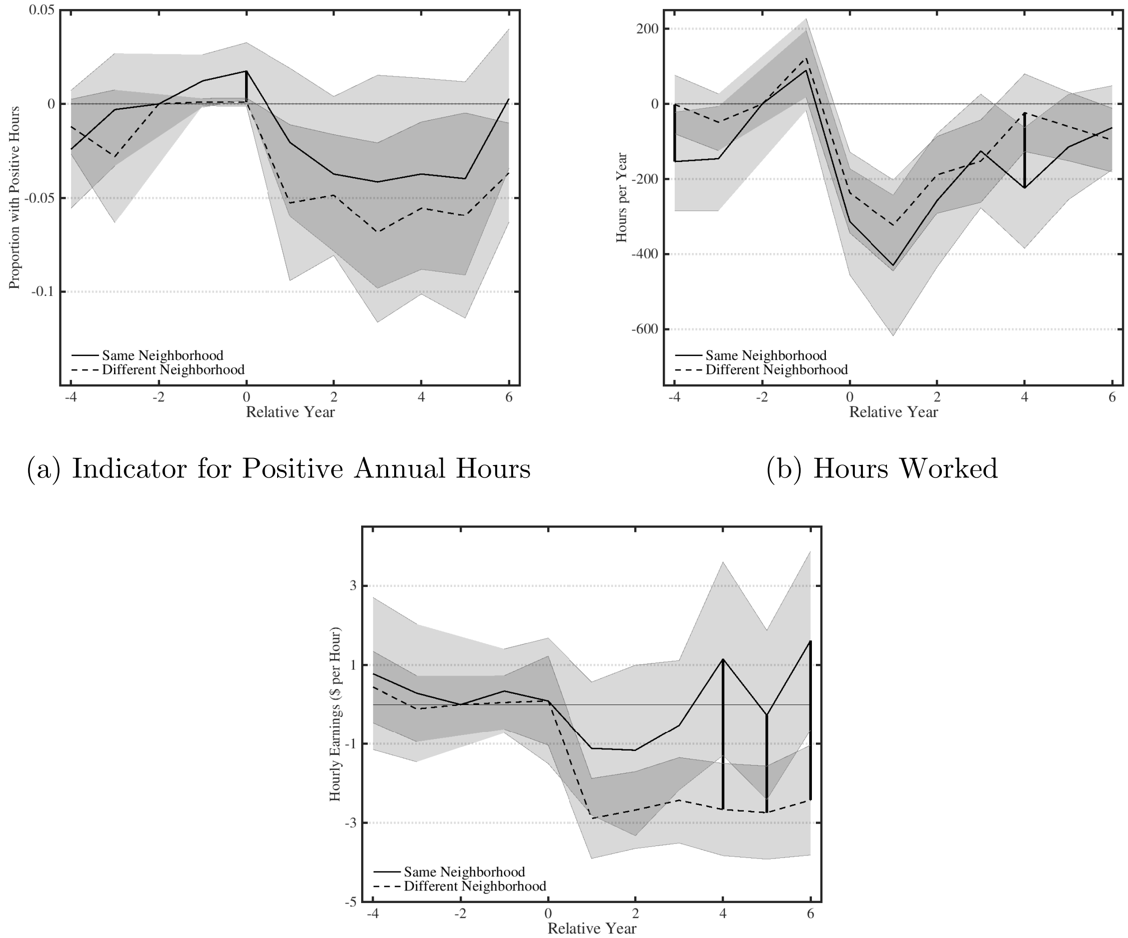


Figure 2: Earnings Losses for Young Displaced Workers

Note: Young workers living in their parents' neighborhoods at the time of displacement experience healthier earnings recoveries than those living farther away, even after controlling for observable differences using propensity score reweighting. The figure plots propensity score weighted regression coefficients from equation (1) describing the impact of a job displacement on the earnings of young workers. The shading represents 95 percent confidence intervals, and any vertical bars connecting the two lines signify that the estimates are statistically significantly different from one another in that year at the 5 percent level. Shading and statistical significance are based on standard errors computed by clustering at the worker level. We designed the weights to make each other group of workers comparable to the group of workers who live in the same neighborhood as their parents before they experience a job displacement. We include characteristics of workers' jobs, of workers' levels of education, of employment-to-population ratios where workers live, and of workers' demographics, including whether they have children or not. See Section 3 for more details about the empirical methodology and Section 4.1 for a discussion of these results.



(a) Indicator for Positive Annual Hours

(b) Hours Worked

(c) Hourly Earnings

Figure 3: Positive Hours, Hours Worked, and Wages for Young Displaced Workers

Note: Employment, hours, and wages drive the stronger recovery in earnings for young adults with nearby parents; however, we find statistically significant differences only for wages. At the time of displacement, wages fall less for workers living near their parents, and hours fall slightly more, although the hours differences are not statistically significant. Employment falls less and recovers more among those with nearby parents. These figures plot regression coefficients from equation (1) describing the impact of a job displacement on measures of labor supply and of wages for groups of young workers, aged 25 to 35 at the time of displacement. The shading represents 95 percent confidence intervals and any vertical bars represent statistically significant differences at the 5 percent level. Standard errors are clustered at the worker level. See Section 3 for more details about the empirical methodology and Section 4.2 for a discussion of these results.

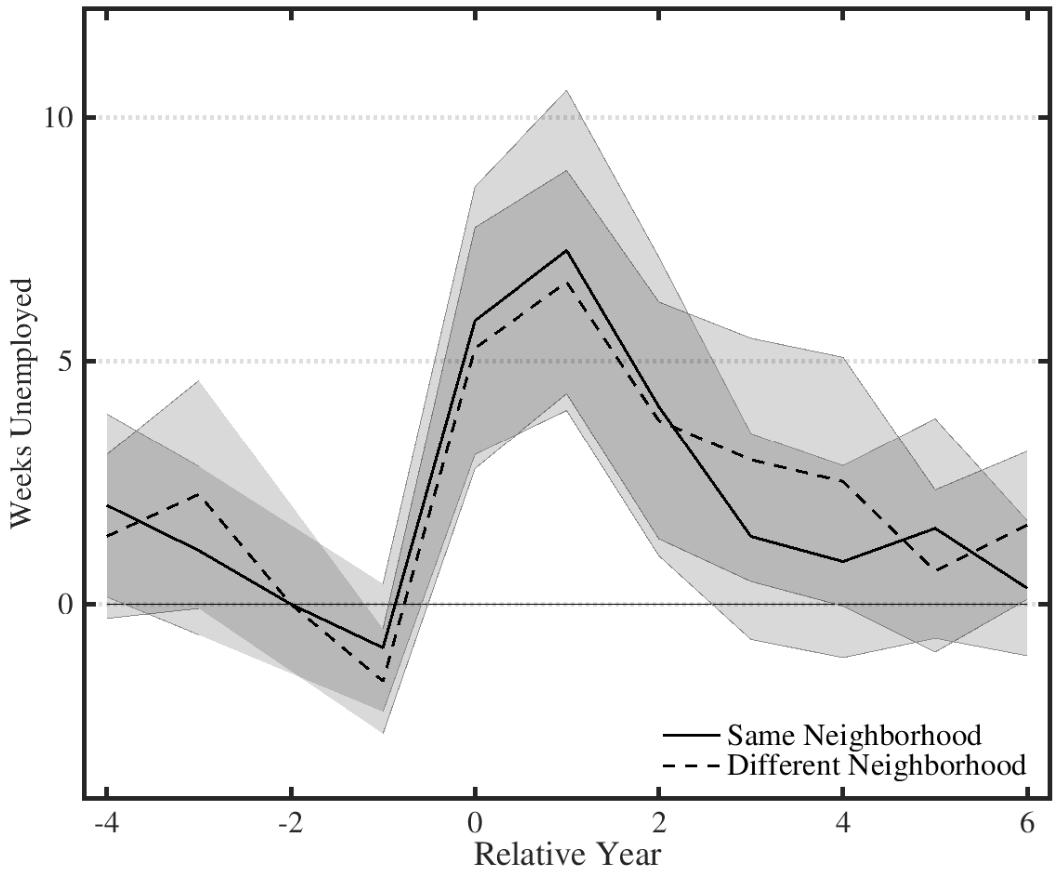


Figure 4: Weeks Spent Unemployed

Note: Younger workers who live in their parents' neighborhoods experience very similar unemployment durations around a displacement event to workers who live farther away. Both groups see an increase of about seven weeks on impact and a steady decline over the next six years. The figure plots regression coefficients from equation (1) describing the impact of a job displacement on the number of weeks that workers were unemployed in each year. The shading represents 95 percent confidence intervals, and any vertical bars connecting the two lines signify that the estimates are statistically significantly different from one another in that year at the 5 percent level. In this figure, the differences are not statistically significant. Shading and statistical significance are based on standard errors computed by clustering at the worker level. See Section 3 for more details about the empirical methodology and Section 4.2 for a discussion of these results.

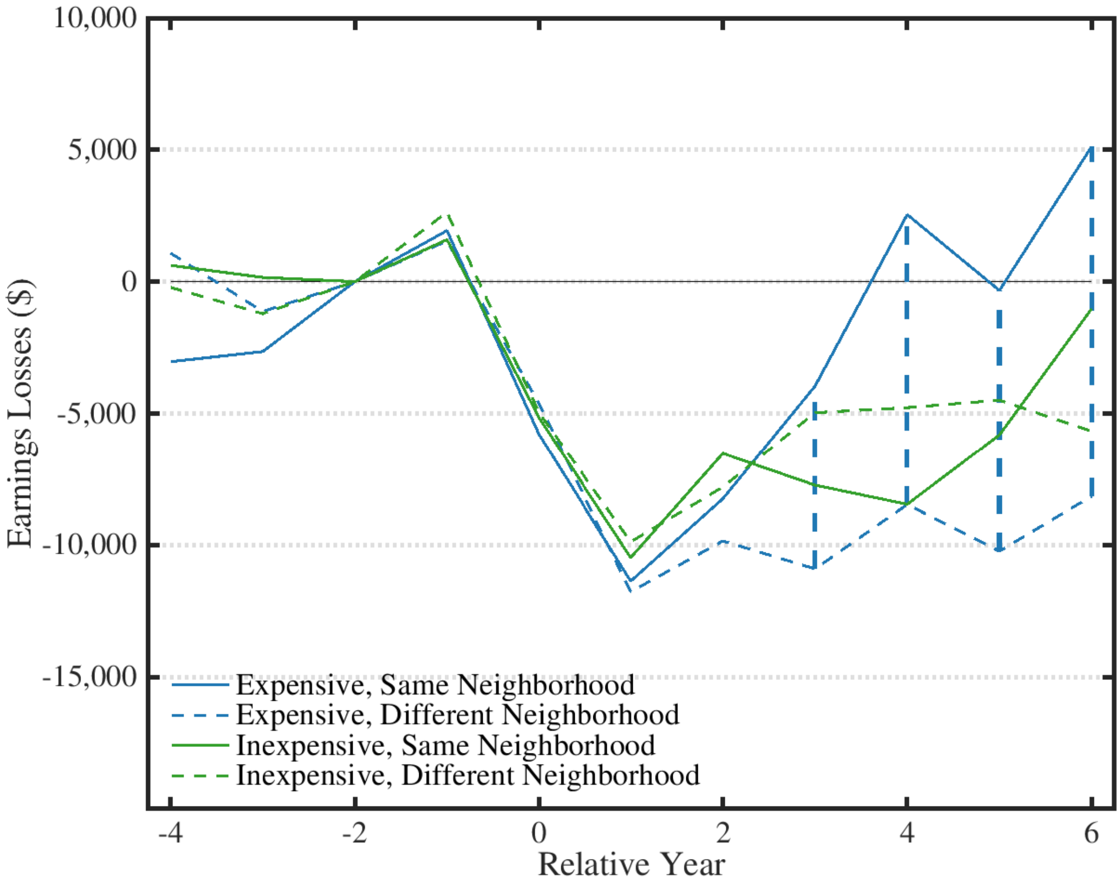


Figure 5: Earnings Losses by Cost of Childcare

Note: Young adults benefit more from nearby parents in states where childcare is more expensive. The figure plots regression coefficients from equation (2) describing the impact of a job displacement on the earnings of displaced workers, in which we allow effects to vary by the cost of childcare. Vertical bars connect two lines of the same color when the estimates are statistically significantly different at the 5 percent level, clustering at the worker level. See Section 4.3 for more details about the empirical methodology and Section 4.3.1 for a discussion of these results.

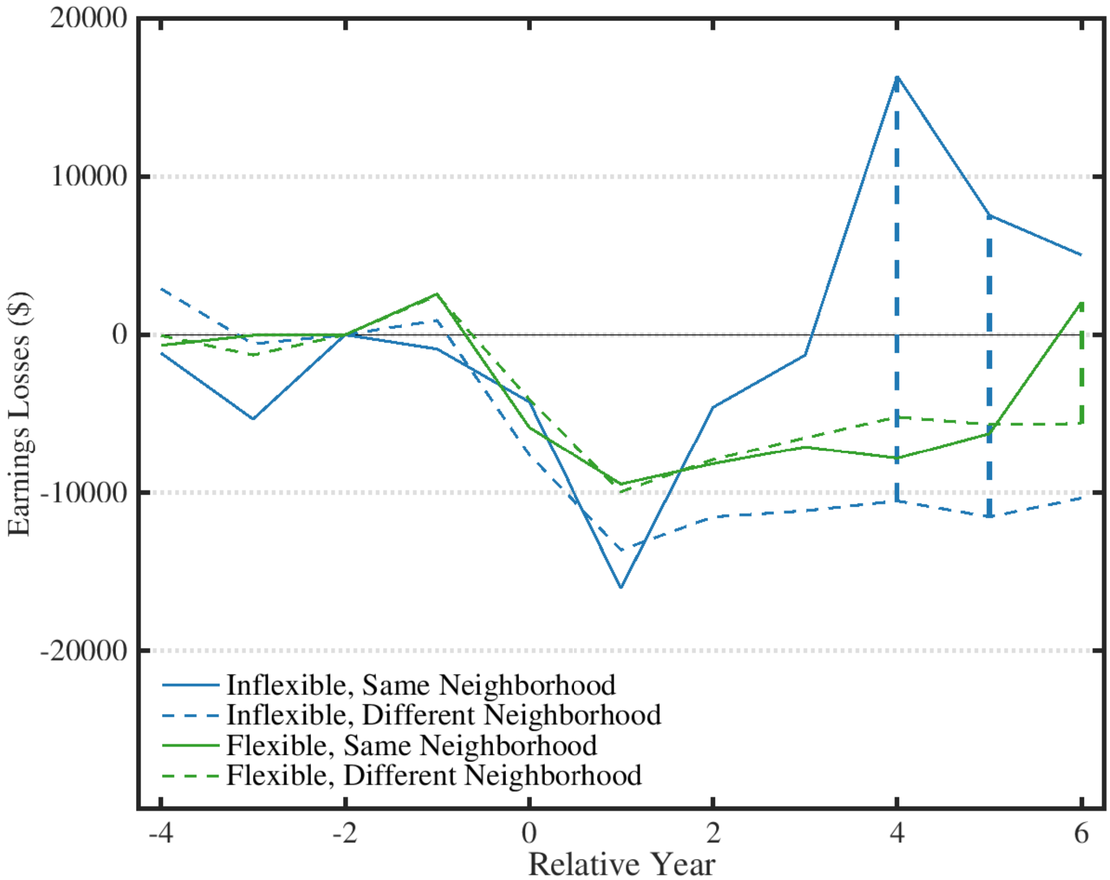


Figure 6: Earnings Losses by Job Flexibility

Note: Young adults who lose jobs in inflexible occupations (as defined in Goldin, 2014) experience larger benefits from nearby parents. The figure plots regression coefficients from equation (2) describing the impact of a job displacement on the earnings of displaced workers, in which we allow effects to vary by flexibility of the lost job. Point estimates suggest that the benefits are large but we cannot rule out more modest gains due to large standard errors. The shading represents 95 percent confidence intervals, and any vertical bars connecting the two lines signify that the estimates are statistically significantly different from one another in that year at the 5 percent level. Shading and statistical significance are based on standard errors computed by clustering at the worker level. See Section 4.3 for more details about the empirical methodology and Section 4.3.1 for a discussion of these results.

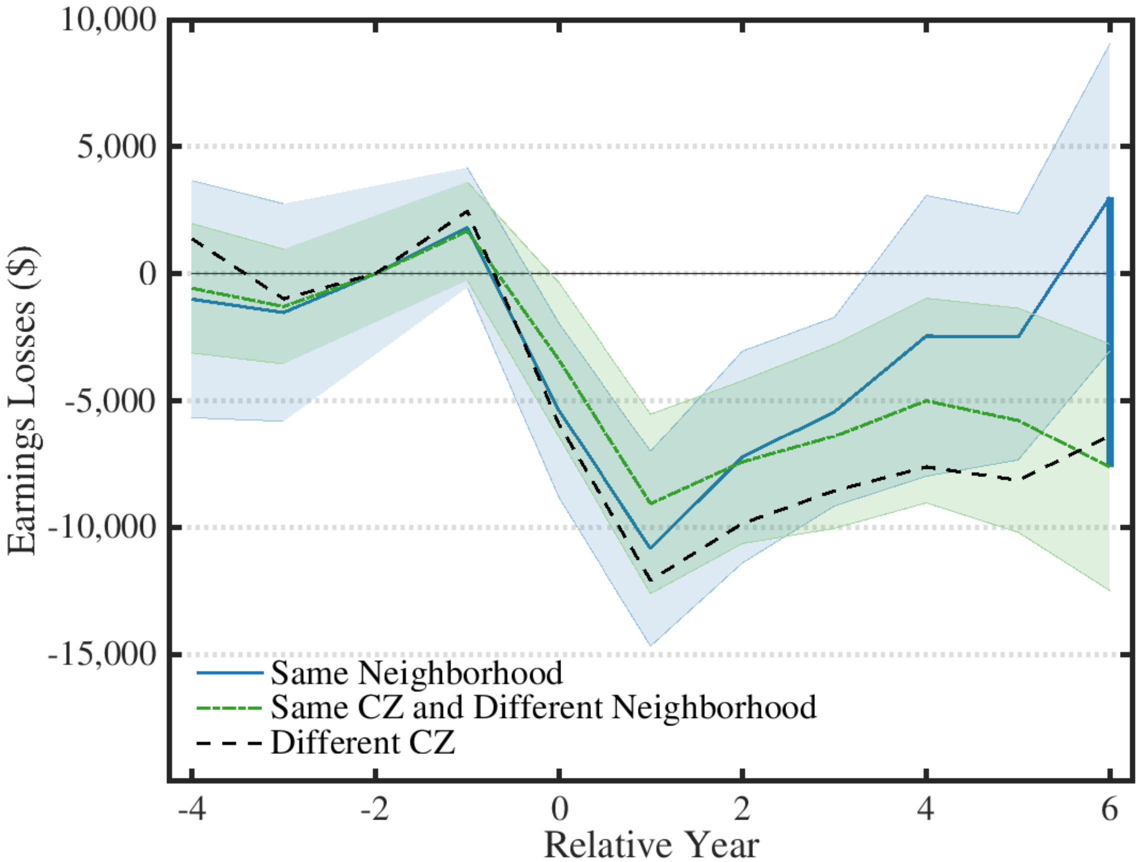


Figure 7: Earnings Losses for Young Displaced Workers by Three Proximities to Parents

Note: Those workers living close to their parents (in the same commuting zone), but not in the same neighborhoods, do not experience significantly better long run post-displacement earnings outcomes than those who live farther away. The figure plots regression coefficients from a specification similar to equation (1) describing the impact of a job displacement on the earnings of three mutually exclusive groups of young workers. The three groups are defined as workers who lived in the same census tract as their parents, those who lived in the same commuting zone but not the same tract, and those who lived in a different commuting zone as their parents. Each group is defined based on the worker's location before the displacement. The figure includes vertical bars that connect the line for workers who live in the same tract with the line for workers who live in the same commuting zone and not the same tract. We include these when the estimates are statistically significantly different from one another at the 5 percent level. Statistical significance is based on standard errors computed by clustering at the worker level. See Section 4.3 for more details about the empirical methodology and Section 4.3.1 for a discussion of these results.

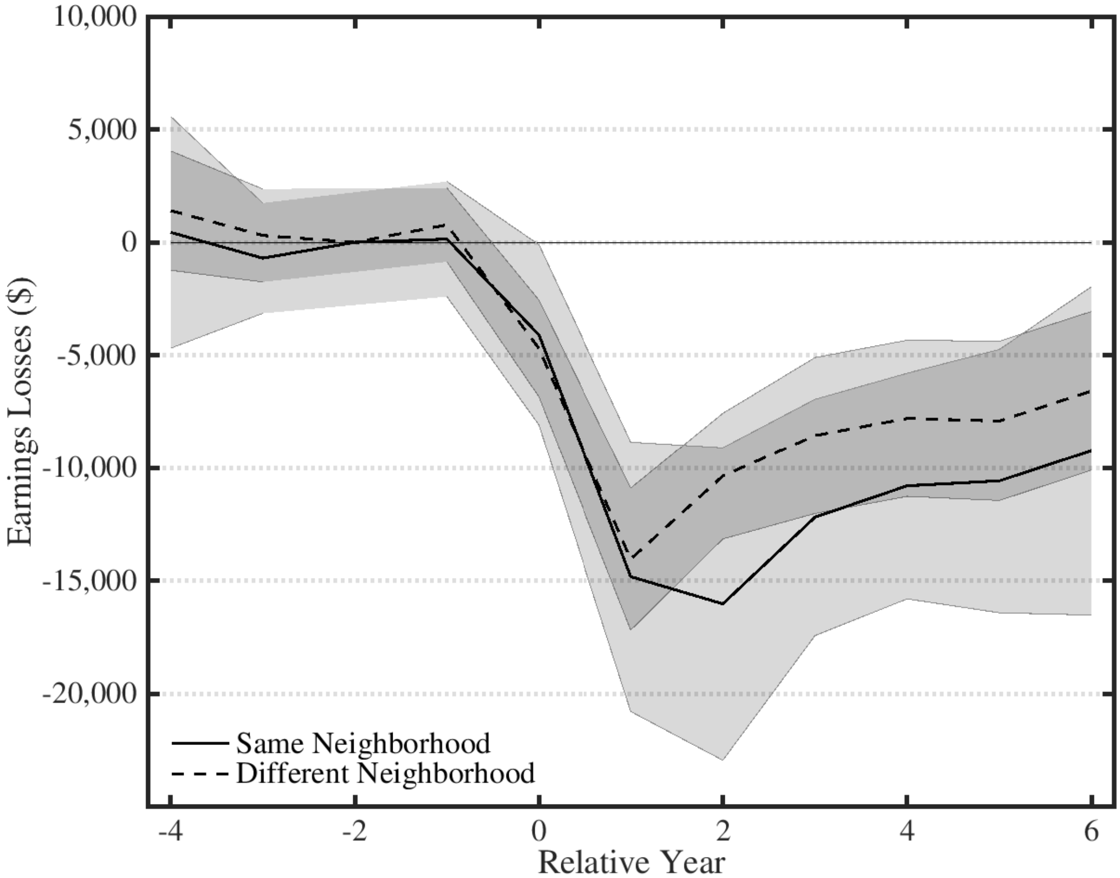


Figure 8: Earnings Losses for Older Displaced Workers

Note: Older workers (ages 36 to 55) who live in their parents' neighborhoods do not experience the same benefit of living close to their parents as young adults. The figure plots regression coefficients from equation (1) describing the impact of a job displacement on the earnings of groups of older workers, aged 36 to 55 at the time of displacement. We use longitudinal weights provided by the PSID. The shading represents 95 percent confidence intervals, and any vertical bars connecting the two lines signify that the estimates are statistically significantly different from one another in that year at the 5 percent level. Shading and statistical significance are based on standard errors computed by clustering at the worker level. See Section 3 for more details about the empirical methodology and Section 4.3.1 for a discussion of these results.

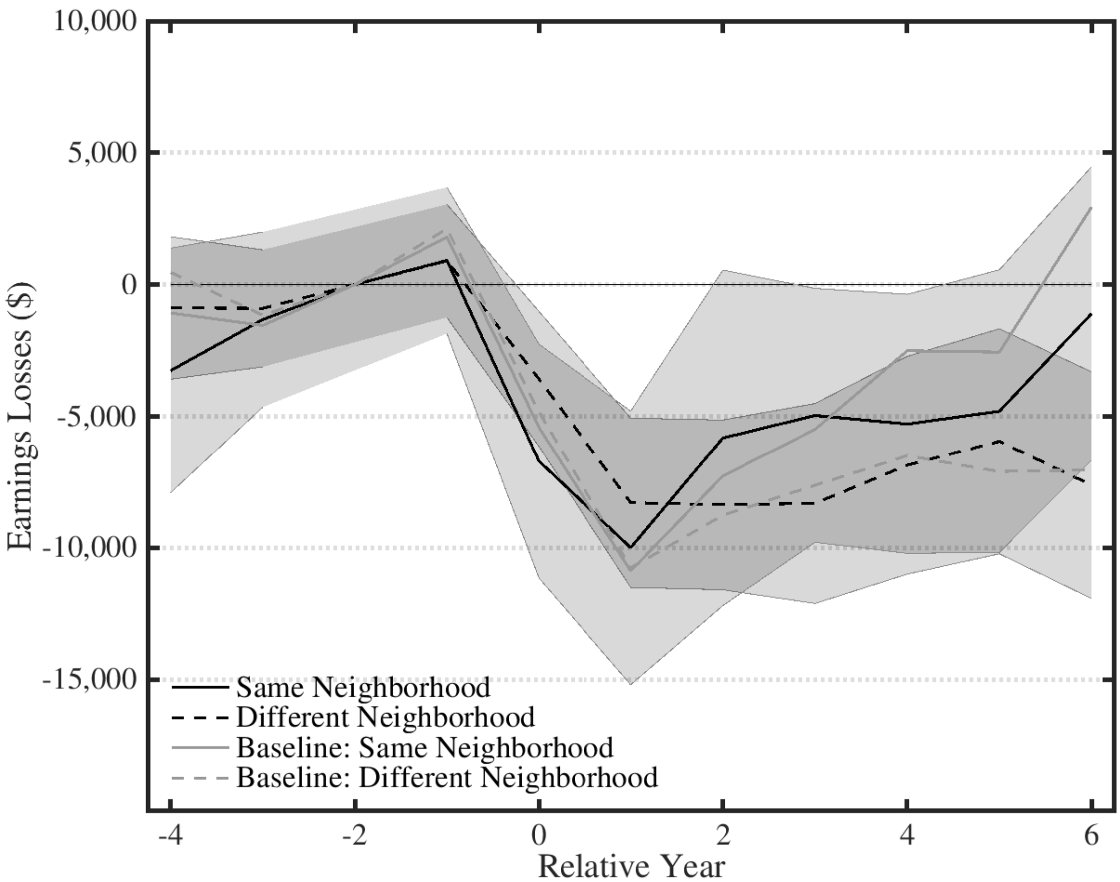
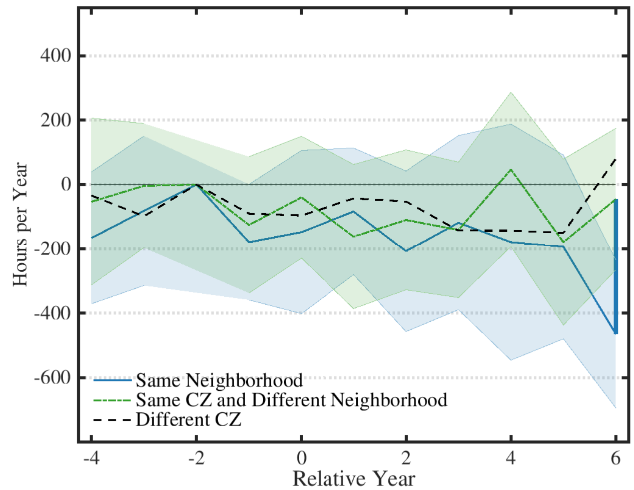
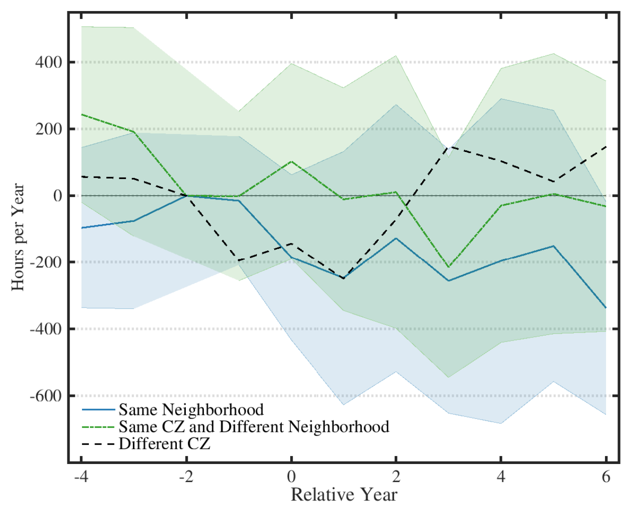


Figure 9: Earnings Losses for Young Displaced Workers Who Do Not Move

Note: Restricting the sample to workers who do not switch counties after the displacement event leads to similar results to Figure 2. The figure plots regression coefficients from equation (1) describing the impact of a job displacement on the earnings of young workers who do not move between counties after a job displacement. The gray lines reproduce our baseline estimates from Figure 2 for comparison. The shading represents 95 percent confidence intervals, and any vertical bars connecting the two lines signify that the estimates are statistically significantly different from one another in that year at the 5 percent level. We only report the vertical bars for significant differences between the regression results with no mobility, since Figure 2 reports them for the whole sample. Shading and statistical significance are based on standard errors computed by clustering at the worker level. See Section 4.3 for more details about the empirical methodology and Section 4.3.2 for a discussion of these results.



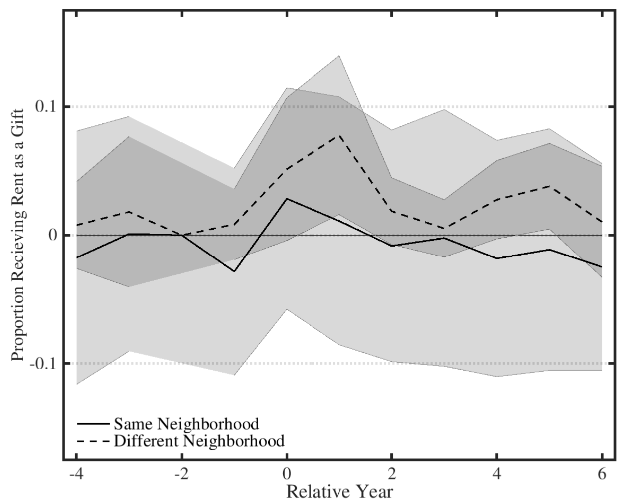
(a) Mothers



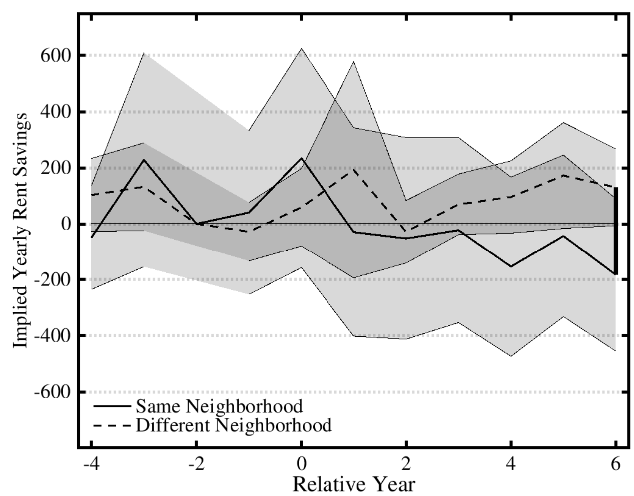
(b) Fathers

Figure 10: Workers' Parents Hours Worked after a Worker's Displacement

Note: Parents of displaced workers who live in the same neighborhood as their children work fewer hours six or more years after their child's displacement. The figure plots regression coefficients from a specification similar to equation (1) showing the impact of a job displacement on the total number of hours worked (including zeros) for closest mother or mother in law and father or father in law. Vertical bars connect the outcome for mothers in the same commuting zone (CZ) and mothers in the same tract when the estimates are statistically significantly different from one another in that year at the 5 percent level. In addition to the standard controls, the specification includes controls for employment to population ratios in the child and the parent's county, fixed effects for the interaction of the parent's industry and the current year, fixed effects for the parent's occupation, and an age quartic for the parent's age interacted by where their child lived before the displacement. The regression includes the worker level reweights and inference is done by clustering at the level of the parent. See Section 5.1 for more details.



(a) Receiving Rent Entirely as a Gift



(b) Implied Rent Savings

Figure 11: Housing Transfers Around Displacements

Note: There is a small, but detectable, increase in housing transfers around displacement, primarily among workers lived outside their parents' neighborhoods before a displacement. These figures plot regression coefficients from equation (1) describing the impact of a job displacement two measures of in-kind transfers of housing to young workers. The measure in Panel A is the proportion of workers who report that they pay no rent and who then volunteer that this is because someone provided it as a gift to them. The measure in Panel B is the estimated dollar value that the worker's family unit saves in rent, based on living with another family unit. The shading represents 95 percent confidence intervals, and any vertical bars connecting the two lines signify that the estimates are statistically significantly different from one another in that year at the 5 percent level. Standard errors are clustered at the worker level. See Section 5.2 for an explanation of each measure and Section 3 for more details about the empirical methodology.

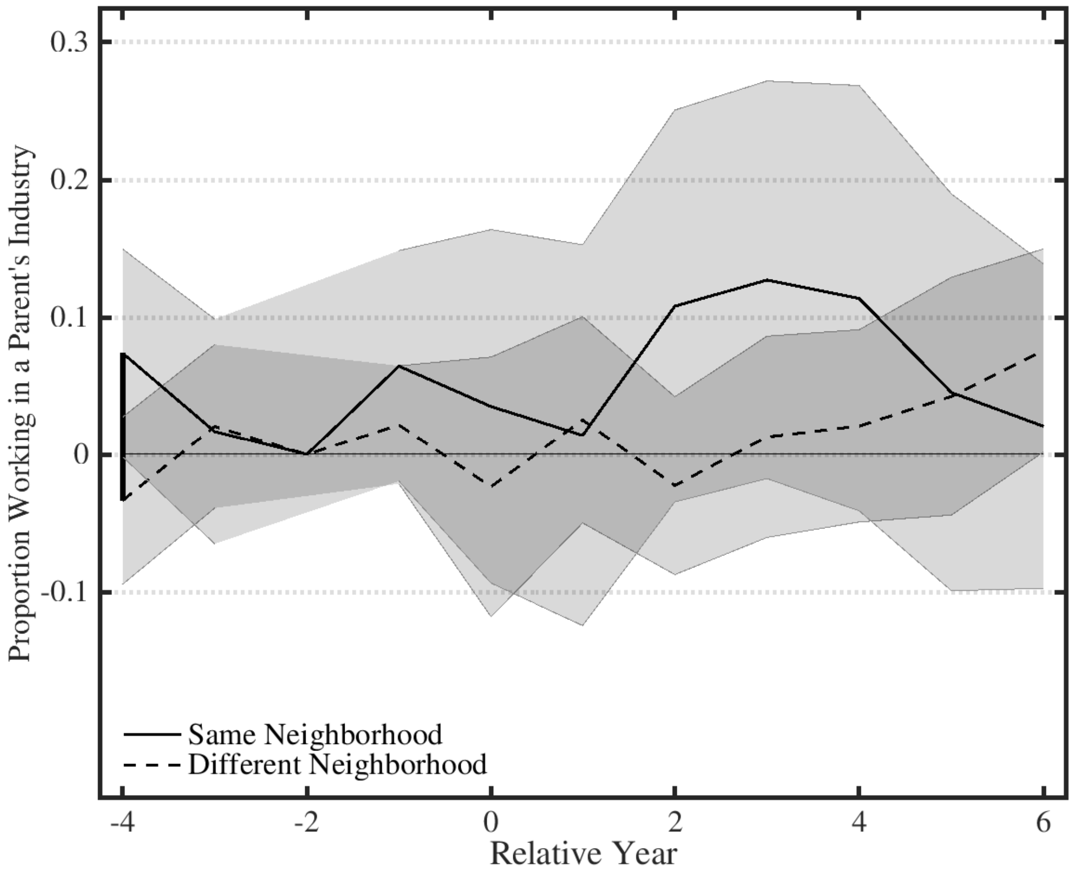


Figure 12: Working in a Parent's Industry

Note: Workers appear to be slightly more likely to work in their parents' industries around a displacement event, though the effect can vary depending on how industries are measured. It does not appear to be due to local industrial composition, however. These figures plot regression coefficients from equation (1) describing the impact of a job displacement on the proportion of workers who work in the same one-digit PSID coded industry as a parent. The shading represents 95 percent confidence intervals, and any vertical bars connecting the two lines signify that the estimates are statistically significantly different from one another in that year at the 5 percent level. Shading and statistical significance are based on standard errors computed by clustering at the worker level. See Sections 3 and 5.3 for more details.

Variable			Same Tract		Different Tract	
	Displaced	Not Displaced	Displaced	Not Displaced	Displaced	Not Displaced
	Panel A: All Workers Age 25 to 55					
Earnings	48,300	56,500	41,300	48,900	50,000	57,900
Age	36.7	38.2	34.1	35.9	37.3	38.6
Years of Schooling	12.9	13.6	12.5	13.2	13.0	13.6
Employer Tenure	7.5	10.7	7.1	10.0	7.6	10.8
Fraction Co-residing	0.11	0.08	0.54	0.53	0.00	0.00
Fraction in Parents' Tract	0.20	0.15	1.00	1.00	0.00	0.00
Mother Employed	0.40	0.40	0.37	0.40	0.41	0.40
Has Children	0.60	0.61	0.65	0.62	0.59	0.60
Fraction Male	0.85	0.84	0.83	0.79	0.85	0.85
Wages ($/hr)	21.8	24.9	18.6	21.8	22.5	25.5
Hours Worked	2,260	2,310	2,280	2,270	2,250	2,310
# of records	1,399	42,929	305	7,193	1,094	35,736
	Panel B: Young Workers Age 25 to 35					
Earnings	45,000	49,600	38,000	42,200	47,400	51,500
Age	29.0	29.5	28.2	29.3	29.2	29.5
Years of Schooling	13.1	13.8	12.5	13.1	13.3	13.9
Employer Tenure	5.3	6.5	5.4	6.8	5.2	6.4
Fraction Co-residing	0.11	0.08	0.45	0.40	0.00	0.00
Fraction in Parents' Tract	0.25	0.20	1.00	1.00	0.00	0.00
Mother Employed	0.49	0.52	0.51	0.50	0.48	0.52
Has Children	0.59	0.59	0.62	0.61	0.58	0.58
Fraction Male	0.86	0.85	0.83	0.81	0.87	0.85
Wages ($/hr)	20.3	21.8	17.2	18.8	21.4	22.5
Hours Worked	2,240	2,320	2,250	2,290	2,240	2,330
# of records	641	16,204	186	3,606	455	12,598

Table 1: Summary Statistics

Note: Workers who were displaced and workers who live in the same tract as their parents are younger, tend to earn less, are more likely to be parents, and tend to have lower socioeconomic status than workers who were not displaced and who lived farther from their parents. This table presents (individual PSID) weighted averages using unbalanced data from the 1968–2013 PSID surveys. Dollar figures are in 2007 dollars using the CPI-U-X1. All variables are measured in the year before a potential displacement (relative year -1). The PSID sample of household heads is composed chiefly of men. We restrict to observations that appear in our baseline sample (equation 1). The definitions of displacements follow Figure 1 and Section 2 contains more information on the sample construction, data, and definitions.

Variable	Same Tract		Different Tract		Same Tract		Different Tract	
	Displaced	Not Displaced	Displaced	Not Displaced	Displaced	Not Displaced	Displaced	Not Displaced
	Panel A: PSID Weights				Panel B: Reweighted			
Earnings	$35,000	$39,800	$44,700	$48,300	$35,000	$33,000	$32,900	$34,700
	[1.00]	[0.03]	[0.00]	[0.00]	[1.00]	[0.33]	[0.40]	[0.90]
Average Change in Earnings	$2,900	$2,300	$2,800	$3,500	$2,900	$2,100	$3,200	$2,500
	[1.00]	[0.34]	[0.90]	[0.44]	[1.00]	[0.24]	[0.79]	[0.57]
Wage	$16.58	$18.29	$20.62	$21.65	$16.58	$15.87	$15.93	$16.36
	[1.00]	[0.06]	[0.00]	[0.00]	[1.00]	[0.42]	[0.53]	[0.82]
Years of Schooling	12.54	13.10	13.32	13.95	12.54	12.48	12.42	12.54
	[1.00]	[0.01]	[0.00]	[0.00]	[1.00]	[0.76]	[0.65]	[0.98]
Share in Goods Industries	0.51	0.43	0.51	0.34	0.51	0.47	0.50	0.40
	[1.00]	[0.10]	[0.91]	[0.00]	[1.00]	[0.40]	[0.87]	[0.03]
Share Manager/Professional	0.20	0.27	0.32	0.41	0.20	0.21	0.19	0.21
	[1.00]	[0.16]	[0.03]	[0.00]	[1.00]	[0.86]	[0.72]	[0.86]
Employer Tenure	5.37	6.78	5.23	6.40	5.37	5.41	5.30	5.34
	[1.00]	[0.00]	[0.69]	[0.00]	[1.00]	[0.90]	[0.86]	[0.93]
Unemp Rate in County	7.46	7.18	7.44	6.81	7.46	7.39	7.79	7.46
	[1.00]	[0.37]	[0.95]	[0.05]	[1.00]	[0.81]	[0.50]	[0.98]
Age	28.18	29.26	29.20	29.52	28.18	28.01	27.95	28.06
	[1.00]	[0.00]	[0.00]	[0.00]	[1.00]	[0.56]	[0.54]	[0.68]
Share with Children	0.63	0.61	0.58	0.58	0.63	0.59	0.65	0.62
	[1.00]	[0.76]	[0.44]	[0.38]	[1.00]	[0.43]	[0.75]	[0.98]
Fraction Male	0.82	0.81	0.86	0.85	0.82	0.81	0.77	0.83
	[1.00]	[0.91]	[0.28]	[0.34]	[1.00]	[0.87]	[0.40]	[0.70]
Number of Records	188	3,624	458	12,642	188	3,624	458	12,642

Table 2: Means Before and After Reweighting

Note: Applying propensity score reweights leads to statistically indistinguishable differences between the samples of workers who do and do not experience displacements as well as workers who live different distances from their parents. This table reports means for each group using PSID weights and the propensity score reweights. We report means and the p-value (in brackets) of a Wald test of equality with the first column -- the mean for workers who were displaced while living near parents. Standard errors and p-values adjust for clustering at the worker level. The number of records refers to the number of person by base age records where we have sufficient earnings data to include the record in the main sample. Missing data for some variables mean that some statistics are based on fewer records, most notably the local unemployment rates. Statistics in this table differ from those in Table 1 because of sample and definitional differences. The sample for Table 1 has slightly fewer records because it restricts to observations with non-missing parents' location information. Additionally, Table 1 uses relative year -1 for all variables, while this table uses an average in relative years -3, -2, and -1 for earnings, changes in earnings, wages, and local unemployment rates to minimize measurement error. For example, average earnings in this table are lower than those in Table 1 because earnings are increasing prior to displacement. The definitions of displacements follow Figure 1, Section 3 describes the reweighting, and Section 2 contains more information on the sample construction, data, and definitions.

	Same Tract with Children	Diff Tract with Children	Same Tract without Children	Diff Tract without Children
Pre Displacement	-844	34	-3,232	482
	(2,189)	(942)	(3,088)	(1,282)
	796	2,056	88	289
Dip	-1,636	-1,569	-3,363	192
	(1,425)	(783)	(1,425)	(2,110)
	321	774	54	140
Drop	-8,638	-9,807	-12,448	-9,588
	(1,946)	(1,072)	(2,233)	(2,309)
	282	711	43	119
Recovery	-3,494	-7,330	-7,283	-4,906
	(2,167)	(1,333)	(2,889)	(2,358)
	255	635	38	102
Five-Year Losses	3,642	-7,339	-9,042	-3,870
	(3,123)	(1,610)	(4,938)	(3,115)
	1,442	3,513	163	347

Table 3: Young Workers With and Without Children

Note: The earnings of young workers who end up having children and live close to their parents recover after a displacement, while those of workers who live farther away, or never have children, are permanently lower. The table reports the regression coefficients from equation (2), along with the standard errors in parentheses and the number of observations in each cell. All of the coefficients come from the same regression specification. Standard errors are clustered at the worker level. See Section 4.3.1 for more details.

	Rent	Gifted Rent		Implied Savings	
	Dollar value	Proportion	Dollar value	Proportion	Dollar value
Value	\$6,800	0.02	\$2,600	0.08	\$4,200
Standard error	(100)	(0.002)	(180)	(0.006)	(440)
N	7,043	16,881	382	16,881	510

Table 4: Measures of Housing Transfers

Note: Less than 10 percent of young workers receive housing transfers, and housing transfers are modest among young workers who receive them. The first column reports average annual rents for the baseline sample and the following columns report measures of housing transfers. Gifted rent reports the proportion of households who report receiving all of their rent as a gift, and the annual value of that gift. Implied savings reports the proportion of young worker families living with workers' parents and the implied annual dollar value of rent that they save due to that arrangement. Note that the dollar value is only observable when the parent is a PSID respondent themselves. The rows report means, standard errors of those means, and sample sizes. We explain each measure of housing transfers in more depth in Section 5.2.

A Appendix: Dataset and Sample Construction (For Online Publication)

A.1 Using Administrative Data

A separate project leveraging administrative data would be helpful in further analyzing our main findings and we believe that it would justify the considerable hurdles to obtaining data linking workers, their employers, and their parents across multiple years. The most obvious U.S. administrative data for earnings and displacements is the Longitudinal Employer-Household Dynamics (LEHD), although these data may lead to some bias in estimates of the earnings losses of displacements (Flaaen, Shapiro, and Sorkin, 2019). We could identify links between workers and their parents alongside alternative measures of earnings using longitudinal data from the Internal Revenue Service (Chetty, Hendren, Kline, and Saez, 2014). Alternatively, we could obtain the parent–child link from the Census or the American Community Survey.

One significant limitation of administrative data sources for our application in the U.S. is a limited longitudinal coverage of workers' earnings, which would limit our ability to observe long-term effects on earnings. The key issue for our case is that researchers typically need to observe parents and children living in the same household at a point when the child is still a minor, usually before the age of 16. Since Chetty et al. (2014) use underlying tax data that are available from 1996 to 2012, they are only able to observe workers' parent matches up to the point where the workers are in their early 30s (Mazumder et al., 2016). Additionally, the earliest links that we know of between workers in the LEHD and a Census survey that allows us to observe their parents is a link to individuals' responses in the 2000 Census long form used in Hellerstein et al. (2019). Since the most recent LEHD files cover earnings up to 2014 (Vilhuber, 2018), we would be limited to earnings observations for workers up to their early 30s.

Suitable administrative data exist in other countries (for example Norway, as in Huttunen et al., 2018), with different institutions and economic dynamics that could allow for a useful comparison. Many Nordic countries, for example, have generous childcare subsidies that can substitute for parental assistance with childcare (Havnes and Mogstad, 2011). Conducting a similar analysis in a country with rich longitudinal administrative datasets that link parents and children would provide important context for what could be driving our results based on these institutional and macroeconomic differences.

A.2 Definitions of Household Heads and Displacement

The PSID follows Census Bureau procedures from the late 1960s that define a household head as the male member of a heterosexual couple, as long as the couple had been living together for at least one year and he was not incapacitated.

A.3 Definitions of Displacements

Job displacements are determined from questions that are asked to employed and nonemployed individuals. Employed individuals who have less than a year of tenure with their present employer (and in some survey years, individuals who started their current job after January 1 of the previous calendar year) are asked: "What happened to the job you had before?" Nonemployed individuals are asked: "What happened to that employer (job)?" (the individual's previous job). The two categories of responses used to identify displacements are "plant closed/employer moved" and "laid off/fired." As is standard in the displaced worker literature, we also impose that workers had at least two years with their employer and were working full time before the displacement event so that our workers have a strong connection to the labor market.

Our results are qualitatively similar with different definitions of attachment. In the baseline approach we follow the job displacement literature in imposing a positive tenure cutoff, but setting this too high (like six years in Jacobson et al., 1993, for example) causes small sample sizes in our context.

A.4 Details about Dataset Construction

Since the PSID follows descendants of original respondents, we typically have information about one set of a couple's two sets of parents. We treat parents symmetrically, so "parents" can mean either the head's parents or the head's parents-in-law. Having information about only one set of parents should induce measurement error that will attenuate differences between people close to and farther from their parents, as some young adults will be close to their parents or in-laws but we will not detect this proximity. We do not suspect that there are large systematic differences between PSID respondent and non-PSID respondent parents, since the PSID started as a probability sample of all families in the US and because of assortative matching of couples.

Due to the survey design of the PSID, the location of household heads is only observed if they have previously moved out of their parents' house. Therefore, adult children who have never moved out of their parents' home are outside the scope of our analysis. The United States Census Bureau (2016, Table AD-1) reports that 50 to 60 percent of 18 to 24 year olds live with their parents (including college students living in dorms during the academic year), but only 10 to 20 percent of 25 to 34 year olds do. Thus, beginning our analysis at age 25 substantially mitigates this sample selection issue.

Common reasons we do not know parents' locations are because the parents are deceased and because they were never interviewed by the PSID. The structure of the PSID means that we are much more likely to see parents who were interviewed by the PSID in later years, since parents of original respondents are not interviewed.

Individuals could switch neighborhoods between their survey date in relative year -1 and their displacement date. This will serve to attenuate our positive treatment effect of parental proximity.

A.5 Timing of the Earnings Decline

The earnings question in the PSID refers to the earnings during the last calendar year. The displacements have been coded to have happened between the previous survey date and the current survey date. Since most PSID interviews happen in April and May, most of our displacements are referring to displacements that happen at the end of the previous calendar year. As such, the earnings on impact, although they fall, may not reflect the entirety of the displacement event, as the earnings from the last calendar year were largely unaffected by the displacement. Rather, in the year following the displacement, the largest reductions may be documented. As such, in the top panel of Figure 1, the declines at year '1' are larger than at year '0'.

A.6 Simple Means with Log Earnings

Appendix Figure 1 presents a similar figure to Figure 1 in the main text, for the natural logarithm of earnings. The conclusions are the same: those living near their parents prior to the displacement event see their earnings recover fully, whereas those living farther away see large and permanent earnings declines.

B Appendix: Empirical Methodology (For Online Publication)

B.1 Propensity Score Reweighting

B.1.1 Propensity Score Reweighting Details

We compute the weights, W_{ia}, for person i at base age a, who is in a group defined by whether they lived close to their parents (H or A) and whether they were displaced (D or N), $j_{ia} \in \{HD, AD, HN, AN\}$, using the following formula:

$$W_{ia} = \frac{\mathrm{P}(j_{ia} = HD|X_{ia})}{\mathrm{P}(j_{ia} = HD)} \frac{\mathrm{P}(j_{ia})}{\mathrm{P}(j_{ia}|X_{ia})} \tag{3}$$

The formula is an application of a typical reweighting scheme (DiNardo, Fortin, and Lemieux, 1996; Fortin, Lemieux, and Firpo, 2011) to multiple groups. The weight is one for the treatment group ($j_{ia} = HD$), since we are reweighting all other observations to have the same characteristics as this group.

We can recover the conditional and unconditional probabilities in a semiparametric way using sample averages and logit regressions with flexible functional forms. We describe the covariates in the logit regression in Section 3.1. The unconditional probabilities in equation (3) are the proportion of the sample made up by the group.

B.1.2 Controlling for Initial Wages

An additional concern is that by controlling for initial wages, we are choosing a comparison group of workers who are less skilled than the group living in their parents' neighborhoods. This would occur, for example, if there was a wage penalty for living close to one's parents, and this wage penalty meant that a worker who earned a given amount in another area would actually earn slightly less if they stayed at home. To address this concern, we present results in Appendix D.1 where we exclude wages from the calculation of our reweights. Instead, we include only education, demographic characteristics, and local employment-to-population ratios. Our findings are similar to the baseline results presented in Section 4.

B.1.3 Comparing Earnings Outcomes after Reweighting

One way to see if the propensity score reweighting is finding workers with similar counterfactual outcomes is to simply plot the earnings trajectories of each control group, suitably reweighted. We do this in Appendix Figure 2 by plotting earnings before and after years where workers were at risk of a displacement, according to our definition, but where they did not actually lose a job. Though there still are some differences after we apply the propensity reweights, the reweighted earnings are very similar before the potential displacement, and continue to behave quite similarly afterwards. The two lines are practically indistinguishable after we include the controls from our baseline regression specifications.

Panel A of Appendix Figure 2 shows that the propensity score reweighting procedure removes almost all of the earnings differences between the two groups and brings them down to the lower level of earnings among workers who were displaced while living in the same neighborhood as their parents. Before the potential displacement, which is the period we use to match workers, the earnings of the two groups are within $1,000 of one another and their pre-trends are roughly parallel. Even from period zero (the potential displacement) to period ten, a period that we leave out of our logit specification, the earnings trends still track each other quite well. This implies that matching on initial earnings, education, occupations, gender, and other factors is enough to find workers with similar employment prospects. This is very much in contrast to means using the PSID longitudinal weights.

Panel B of Appendix Figure 2 shows that earnings are even more similar between the two groups after we adjusted for an important control in our regression specification (equation

1)—a quartic term in worker age, estimated separately for each group. Taking out the age quartic highlights each worker's good fortune in avoiding a displacement in year zero and makes the lines completely statistically, and economically, indistinguishable. The lines are well within $1,000 of one another throughout the panel.

We also show the effects of reweighting in terms of the simple means that we began with in Section 2.3 for displaced and non-displaced workers. Appendix Figure 3 shows reweighted means of earnings around potential displacements for each group of young adults. Each group has similar earnings before the potential displacement, and the groups of workers who do not suffer a displacement have very similar trajectories after, which suggests that the weights emphasize workers with similar counterfactual earnings trajectories in each group. At the time of displacement, workers have similar losses in earnings regardless of whether they live in their parents' neighborhoods, though workers who live closer do earn slightly more on average. The earnings of those who were closer to their parents begin to out-pace the earnings of workers who were farther away after the displacement event, however. In the final years this difference is quite large, at about $10,000.

B.2 Regression Framework

We omit the dummy for two years before the displacement because it is the most recent period that is still before both the displacement and the period that we use to define whether someone is living close to their parents, which is one year before the displacement. Choosing a period before when we define workers' proximity to their parents eliminates any mechanical difference in the baseline level of earnings due to either the displacement, or conditioning on someone living in the same neighborhoods as their parents in that period. This choice also allows us to recover a more precise estimate of our base period, since relatively young workers do not have earnings information going back very far. Kletzer and Fairlie (2003), who also focus on young workers, make a similar choice.

We also follow Kletzer and Fairlie (2003) and estimate the earnings model in equation (1) without worker-specific time trends. For young workers there exist relatively few pre-displacement earnings observations so we think that worker-specific trends are unlikely to be well estimated. Also, as documented in Section 4.1, the post-displacement effect of being in the same neighborhood as one's parents on earnings is gradual and builds over time so it might be incorrectly attributed to worker-specific trends if these were included in the estimating equation. In addition, our specification includes worker-base-age dummies, α_{ia}, which vary within workers by base age and therefore likely already pick up worker-specific trends in earnings. Finally, our reweighting exercise controls for changes in earnings prior to the displacement event, which should eliminate any differences in trend prior to the displacement event.

C Appendix: Additional Robustness Exercises (For Online Publication)

In this section we present several robustness checks to our baseline results in Section 4. The baseline results are remarkably robust to these different specifications, controls, and samples.

C.1 Losses 10 Years Out

We find similar results to our baseline in Figure 2 when we allows 10 post-displacement dummies, as shown in Appendix Figure 4. To increase sample sizes throughout the main text, we pool individuals who are six or more years after displacement.

C.2 Instrumenting for Parental Proximity

Using parental death to instrument for parental proximity strengthens our results (Appendix Figure 5), but parental death is a weak instrument that fails an overidentification test. Very few displacements happen to workers who have lost a parent, since few workers aged 25 to 35 years old have lost a parent. Workers who leave one parent also seem to live closer to their surviving parents—particularly mothers—taking away some of the instrument's negative correlation with parental proximity. We also reject the null hypothesis of an overidentification test using the death of fathers and of mothers as instruments for how far children live from parents. One plausible explanation for why we reject the overidentification test is that levels of morbidity among parents are correlated with the labor market experiences of workers because of factors besides parental proximity. For example, the obesity of a parent could have been passed down to a child and affect the child's labor market outcomes.

C.3 Results Without Propensity Score Reweighting

Appendix Figure 6 presents the effect of displacement on earnings for workers farther away from their family and the effect of displacement for workers living in the same neighborhoods as their parents, for workers experiencing a displacement between ages 25 to 35. But, for these results we use the longitudinal weights provided by the PSID instead of our propensity score weights. These results tell the same story as our baseline results in Figure 2, but workers living farther away have more to lose so they have a larger on-impact dip and the earnings differences between the two groups are more pronounced. At the time of displacement, workers experience large declines in earnings; about \$12,000 for those living in their parents' neighborhoods and about \$16,000 for those living farther away. With the average pre-displacement earnings of these groups being about \$38,000 and \$47,000, respectively, this represents a 35 percent decline in earnings at the time of displacement. The post-displacement recovery, however, is quite different for the two groups. The group living farther away from their parents experiences a small recovery in the short- to medium-run but still has earnings losses of about 25 percent even 6 years after the displacement event. In contrast, the group that was living in the same neighborhoods as their parents prior to the displacement event experiences a steady recovery in the years following the displacement event, with earnings losses indistinguishable from a full recovery after four years. The difference between the earnings of the two groups is statistically significant at longer horizons.

C.4 Using a Continuous Measure of Distance

Although census tracts are very small areas, sometimes residences that are geographically close might be in different tracts. Using latitudes and longitudes of block groups, we have computed as-the-crow-flies distances between parents and their adult children in the PSID. Appendix Figure 7 shows the earnings results when we group workers based on whether they lived within 3/4 of a mile of their parents prior to the displacement event (roughly a 15 minute walk at average walking speeds) or farther away. We see that this distance based measure of proximity yields virtually identical results.

C.5 Results for Heads and Wives

Our main results do not vary by gender. We created a dataset that included both heads and wives, since heads in the PSID are very likely to be males. We used longitudinal weights from the PSID, as opposed to our propensity score weights that were designed to find comparable groups of heads across displacement status and proximity to parents. Appendix Figure 8 presents the results from estimating our baseline specification on this pooled sample of wives and heads, most comparable to our results in Appendix Figure 6 for heads only. We find that the two sets of results are very similar, even though women might be more likely to benefit from parental proximity since they were largely responsible for childcare and household chores during the bulk of our data.

D Appendix: Robustness of Our Reweighting Choice (For Online Publication)

D.1 Reweighting Based on Other Characteristics

In this section, we present two alternative versions of our reweighting procedure that address two possible limitations inherent in reweighting based on observable characteristics. The first concern is that we may miss unobservable differences between people who live different distances from their parents, and that these differences may be driving our results. The second, essentially, is the opposite—that our reweighting could be overfitting the data. The reweighting could be going too far and selecting a group of workers who have less marketable unobservable skills than workers who are living close to their parents, since we emphasize people who have similar paying jobs and who do not have the benefits of living close to their families and friends. It could be that people who live farther form their parents, with jobs of constant observable quality, may be living farther away because they are unable to find comparable jobs nearby.

We address these concerns by repeating the reweighting exercise using two different sets of characteristics and testing if our results are robust to alternative specifications. The broad finding is that our results are quite similar, and sometimes more precisely estimated, when we include different sets of characteristics.

We proceed with two strategies. First, we include as many additional characteristics as possible, including characteristics of parents in this case. Second, we pare down our list of characteristics to those that are predetermined when someone decides whether to live near their parents. Most notably, this omits all characteristics of people's jobs since workers can choose jobs based on their preferences about locations.

To implement each strategy, we estimate our baseline specification, equation (1), with weights, shown in equation (3), using different sets of covariates.

D.1.1 Including Parents' Characteristics

For the first exercise, we include all of the same characteristics as in Section 4, the average employment-to-population ratio in the census tract that the worker's parents live in, and several characteristics of both the mother and the father, entered separately. These characteristics include a dummy for whether the parent is college educated, the parent's total years of schooling, a dummy for whether the parent is currently employed, the parent's age, and the parent's age squared. We also include a dummy for whether the parent was interviewed in any of the three years before the worker's potential displacement. If the parent was not interviewed, we set the dummy for whether the parent was interviewed to one and all of the parent's other characteristics to zero. Otherwise, we follow the methodology in Section 3.[9]

The results, shown in Panel A of Appendix Figure 9, are very similar to the propensity score reweighted results in Figure 2. The initial effect of a job displacement is about \$11,000 for each group (around a third of initial earnings). The group of workers who live in their

[9] Parents' characteristics are averages for the three years before the displacement, which matches the characteristics for the worker. In cases where the parent was interviewed but we still do not have information about the characteristic (primarily item nonresponse) we include the average value of the characteristic for all PSID respondents. Including separate dummy variables for each characteristic of each parent resulted in a log likelihood function that was not concave. Including only observations where we had information for all of the characteristics when a parent was surveyed resulted in very similar results, but a much smaller sample. We include the values for either the relevant parent or the relevant in-law based on which is a PSID sample member. In the rare cases where we have information about both, we average the two values.

parents' neighborhoods earn about the same amount as if they were not displaced after four years, however, while the other group permanently earns about \$7,000 less.

D.1.2 Predetermined Characteristics

For the second exercise, we restrict the characteristics to those that are predetermined at the time that the worker decides whether to live in their parents' neighborhood. These characteristics are a dummy for whether the worker is college educated, their total number of years of schooling, their age, a dummy for their gender, a dummy for being African American, and the employment-to-population ratio in the place where they live. Besides the limited set of worker characteristics, we follow the methodology in Section 3. For example, our estimates are based on the same sample of workers.

The results, shown in Panel B of Appendix Figure 9 are similar to Panel A, and also to Figure 2. The initial effect of a job displacement is also about \$11,000 for each group, though there is some suggestion that it is smaller for the group living closer to their parents. The smaller drop might be because this reweighting results in workers who live closer losing jobs that pay slightly less, on average. Still, the earnings trends are similar to before, with workers who live in their parents' neighborhoods earning about the same amount after four years. The group that lives near their parents earns slightly more in this specification, and the group living farther away earns slightly less, so the differences are slightly larger than in the other reweightings. The estimates are somewhat less precise, but the larger differences between the estimates counteract this in terms of statistical significance.

D.2 Including Additional Interactions in the Baseline Regression

To complement our reweighting approach, we also examined the effects of including interactions with other baseline characteristics, in the same way we separate out the effect of being closer to one's parents. We take another characteristic X_{ia}^C, like the person's earnings before displacement, and interact it with both the age quartic and the displacement dummies.

To be specific, we estimate the following equation, which includes all of the terms in our baseline specification, equation (1), as well as some additional terms:

$$e_{iat} = \alpha_{ia} + \gamma_t + X_{iat}(\beta^A + \beta^H H_{ia} + \beta^C X_{ia}^C) + \sum_{k=-4}^{10+} (D_{it}^k \delta^k + D_{it}^k H_{ia} \zeta^k + D_{it}^k X_{ia}^C \xi^k) + \epsilon_{iat}. \quad (4)$$

The fixed effect, α_{ia} already controls for an effect of X_{ia}^C that is constant across time, but the additional interactions also control for time-varying effects around the displacement. For example, if earnings losses are bigger after layoffs from jobs that pay more, then this would be reflected in negative values of ξ^k for $k > 0$. If this were driving our result that workers who live closer to their parents suffer smaller earnings losses, then including this term would also move the value of ζ^k closer to zero.

As before, the effects of a displacement for different groups are different linear combinations of $\hat{\delta}^k$ and $\hat{\zeta}^k$ terms, and we plot these as a simple way of understanding the impact of these different specifications. We plot $\hat{\delta}^k + \hat{\zeta}^k$ as the effect for people living near their parents and $\hat{\delta}^k$ for people living farther from their parents. Since we are not including the $\hat{\xi}^k$ terms, the effect is for the omitted group where X_{ia}^C is a dummy variable and the value at the mean of X_{ia}^C (since we de-mean X_{ia}^C) when it is a continuous variable. Note that the difference between the two lines is, due to functional form, unchanged regardless of the value of X_{ia}^C. Appendix Figure 10 shows the coefficient estimates with several different interactions. Panels A and B show the coefficients of interest if one allows effects to vary by how much young workers earned before the displacement. In Panel A we include an interaction with a linear earnings control, and in Panel B we include an interaction with a high/low earnings dummy. Controlling for initial incomes generally makes the initial earnings losses much more similar between people at different distances from their parents. Controlling for income, however, does little to the finding that the two paths diverge later on. Panel C of Appendix Figure 10 presents the earnings plots when we include an interaction with a dummy for college education. As with the income interactions, this reduces the difference between the two groups but does not remove the long-run divergence.

D.3 Strong Common Support

Appendix Figure 11, we implement the same propensity score approach as in the baseline approach, but use only a subset of observations where there is strong common support according to the selection method proposed by Crump et al. (2009). The results are similar.

E Close Proximity, Geographic Mobility, Co-residence, and Other Effects of Home

E.1 Close but Not Same Tract

Young adults living outside of their parents' neighborhood, but still relatively close, experience some of the benefits of parental proximity. Workers that live outside of their parents' neighborhood but within the same commuting zone are around 8 miles away from their parents, on average, in our sample. To obtain estimates for individuals who are between same tract and same commuting zone, we define an intermediate level of proximity that is outside of a parents' tract but within four miles of parents. Point estimates in Appendix Figure 12 suggest that this group of workers experiences similar short- and medium-run earnings benefits as those who are in the same neighborhood as their parents, but not the long-run benefits. In particular, there are no statistically significant differences between this group and those in the same tract except at six+. Those individuals living farther away than four miles see the largest earnings losses for each horizon. Our ability to detect differences at finer levels of geography is limited by small sample sizes, and in this particular dimension, a larger sample of workers and parents contained in administrative data would be helpful. We describe such a potential project in Appendix A.1.

E.2 Geographic Mobility after Displacement

Appendix Figure 13 documents a large increase in mobility after displacement. In particular, the probability of switching commuting zones rises by about 5 pp in the year following displacement and remains somewhat elevated for the next six years.

E.3 Co-residence vs. Same Tract

Similar to the exercise in the main text where we break out proximity by same tract, same commuting zone but not same tract, and farther away, we also look at workers who are actually co-residing with their parents as opposed to living in the same neighborhoods as their parents, in the spirit of Kaplan (2012). Appendix Figure 14 presents the results from this exercise. We cannot reject the null hypothesis that the estimates for the co-residing young adults are different from those sharing the same neighborhood with their parents. The point estimates suggest that the recovery for these two groups of workers is similar and better than for those who live outside of their parents' neighborhoods at the time of displacement. This finding suggests that the effect of parental proximity is not limited to transfers while co-residing.

E.4 Accounting for Other Effects of Home

We have also found that the earnings differences for the two groups persists even if one includes additional interactions of the displacement dummies in equation (1) with whether the worker was displaced while living in the county where they grew up.[10] Appendix Figure 15 presents the earnings trajectories for those who are in their parents' neighborhoods and those who are farther away, and not in their home county, after these additional interactions are included in the baseline specification. The results are very similar to the baseline results, and those who are in the county they grew up in at the time of displacement have similar earnings losses to those who are not in their parents' neighborhoods and not in their home

[10] This is based on a retrospective question in the interview.

county. These findings suggest that parental proximity has an effect on post-displacement earnings that is independent from other local factors.

F Supporting Evidence on Mechanisms

F.1 Parents' Intensive and Extensive Margin Labor Supply Responses

Results separating out intensive and extensive labor supply changes for parents are imprecise. Appendix Figure 16 presents intensive (working positive hours) and extensive (number of hours worked if working) margin effects for workers' mothers and fathers. Results are generally imprecise and statistically insignificant. The point estimates do suggest an immediate response on the intensive margin for mothers as well as a six-year delayed effect on the extensive margin. We caution readers not to over-interpret these results, however, given our level of imprecision.

F.2 Measures of Transfers

This section presents the technique that we use to determine how much children were able to save in rent by living with their parents as well as some simple analysis of the PSID's question about cash transfers (help) from friends or relatives.

F.2.1 Calculating Implied Savings on Rent

To calculate the implied amount that a family unit saves on rent, we rely on the OECD equivalence scale and an assumption about the user cost of capital to back out the cost of a dwelling where the family unit could live in the same amount of comfort.

We use an equivalence scale to make comparisons between larger houses that have many people living in them and smaller houses that have fewer people living in them. The OECD equivalence scale is among the most commonly used equivalence scales that accounts for both crowding and returns to scale in household consumption.

$$E(A,C) = 1 + 0.7(A-1) + 0.5C$$

Mechanically, each adult additional (A) counts for 70 percent of the initial adult, and each child (C, 14 or younger) counts for 50 percent of the initial adult. A given value of the scale, $E(A,C)$, implies someone living alone in a house that costs, say, a dollars would be indifferent to living in a house costing $E(A,C) \times a$ dollars if they were to live with $A-1$ other adults and C children.

In a case where a child lives in a house that their parents are renting, it is possible to back out the implied amount the child would have to pay to live alone in a house of a similar quality. Say the child would live in a family unit with A_C adults and C_C children and the parents in a family unit with A_P adults and C_P children. Then, given that the parent's rent is R, the child would have to pay the following to live separately in a house of a similar quality.

$$\frac{R}{E(A_P + A_C, C_P + C_C)} E(A_C, C_C)$$

Intuitively, the formula first converts the rent into a per person level of consumption within the larger household by dividing by the equivalence scale for the larger household. Then the formula multiplies the per person level of consumption by the value of the equivalence scale for the child's household. This give the amount of rent the child's household would have to pay to enjoy the same standard of living. The difference between this counterfactual rent, and the rent that the child actually pays is the implied savings from living with parents.

One complication in practice is that parents oftentimes own their houses, which means there is no direct measure of parents' rents. To compute an annual rent equivalent in these settings, we employ a user cost of capital equal to 0.0785 (following Albouy and Zabek, 2016). The user cost gives, essentially, the implied rental payment that the household pays for using the house for the year, as opposed to renting it out to another family or selling it. It will depend on the depreciation of the house, the interest rate of the mortgage, property tax rates, and any specific tax incentives for home ownership. For simplicity, we set it to a fixed value and we only use it in situations where we need to convert the value of someone's house into a value on the rental market.

F.2.2 Limitations of Our Housing Transfers Analysis

Kaplan (2012) argues that the option value of moving in with parents is important, which could mean that the option value to workers was more valuable than the dollar value of the realized transfers. Another way that this exercise may be understating the value of in-kind transfers of housing from parents is by missing frequent movements of people in and out of their parent's homes, and the additional value coming from this flexibility. Our measures are only based on where people live at the time of the survey each year.

F.2.3 Explaining the Differences between Our Measures of Housing Transfers

The difference between the value according to the two estimates in Table 4 could be for several reasons. The most obvious is because the counterfactual is different between the question and our exercise. The counterfactual in the question is what would be the rent if the respondent's current dwelling were rented, while our question is how much it would cost for the respondent and their family to find similar accommodation. To the extent that dwellings are shared, and the market does not value living with one's parents as much as an OECD scale would suggest, these two estimates should diverge in the direction that we find. It also is possible that children are prone to under-estimating the amount of free rent that they receive.

F.2.4 Cash Transfers from Friends and Relatives

In addition to transfers of housing, children can receive cash transfers from their parents. Appendix Figure 17 shows how these change around displacement, using our main event study specification including fixed effects, an age quartic, and other controls. For these plots, we use an annual question in the PSID that asks how much money a household received from friends and relatives. Much more detailed transfer data exist in two single-year transfer supplements to the PSID. These are of limited use in our context, however, because our estimation strategy is only possible when we have data across many years.

Appendix Figure 17 shows that workers who are living away from their parents appear to receive larger monetary transfers two years after a displacement. This is apparent both for extensive margins (Panel A) and intensive margins (Panel B). Workers who live closer do not appear to receive any more money around a displacement, though this series is noisy and it has very large standard errors. As with housing transfers, the amounts are fairly small; the increase around a displacement is estimated to be about $150 per year, which is about one percent of the estimated earnings losses after a displacement for this group.

F.3 Same Industry for Younger Workers in the Same CZ and Older Workers

We find no effect on working in a parent's industry for older workers and young workers who live in the same commuting zone as their parents but the same neighborhood. Appendix figures 18 and 19 present the relevant point estimates and standard errors.

F.4 Switching Industries and Occupations

Previous work, including Jacobson et al. (1993) and Stevens (1997), has documented that industry and occupation switchers experience larger post-displacement earnings losses than workers who retain a job in their former line of work. We document industry and occupation switching around the displacement event for workers who were in the same neighborhood as their parents prior to the displacement event and those who lived farther away. We estimate equation (1) but use as an outcome variable a dummy, D_switch_{iat}, that equals one if the worker i switches one-digit industry/occupation between survey year t and $t+1$ at base age a.

Appendix Figure 20 presents the probability of switching industries by parental proximity. Both groups of young adults switch industries more frequently around a displacement event than in other periods, consistent with previous work (Burda and Mertens, 2001). This switching rate stays elevated for several years after the displacement event. Appendix Figure 20 also suggests that workers living in the same neighborhood as their parents prior to the displacement event experience markedly sharper increases in their probability of switching industries than workers living farther away. Based on the prior work cited above, this would predict larger post-displacement earnings losses for workers living close to parents and would thus work against our baseline findings. As such, these results suggest that industry switching cannot explain our baseline findings.

Occupation switching is similar for the two groups around a displacement event, as shown in Appendix Figure 21.

F.5 Ability Differences in our Toy Model

Suppose that an individual's wage is a product of their ability, a_i, and the match quality, $q_j > 0$, they draw in a location, so that $w_{ij} = a_i q_j$, in which i indexes individual and $j \in \{h, a\}$ indexes location (home or away). Suppose that, after comparing their match quality draw at home and away, individual 1 (with ability a_1) stays at home and individual 2 (with ability a_2) moves away. These location choices imply that

$$a_1 q_h + b > a_1 q_a \tag{5}$$

and

$$a_2 q_a > a_2 q_h + b \tag{6}$$

Since the two individuals make the same amount, this implies that

$$w_{1h} = w_{2a} \Rightarrow a_1 q_h = a_2 q_a \tag{7}$$

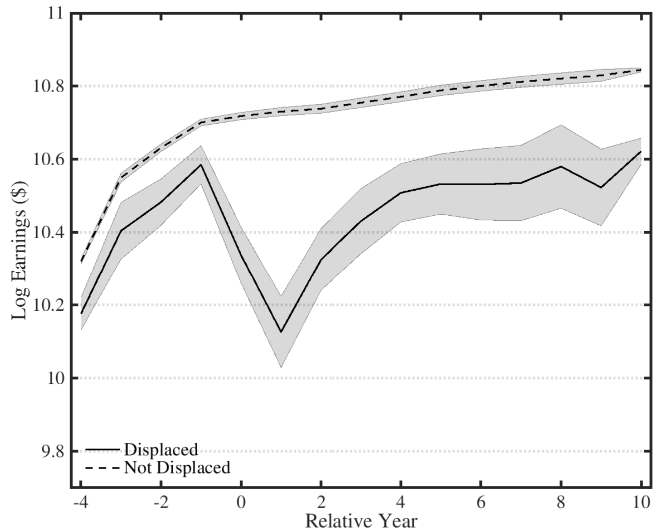
Plugging equation (7) into equation (6) yields

$$\begin{aligned} a_1 q_h &> a_2 q_h + b \\ q_h (a_1 - a_2) &> b \\ a_1 - a_2 &> \frac{b}{q_h} > 0 \end{aligned}$$

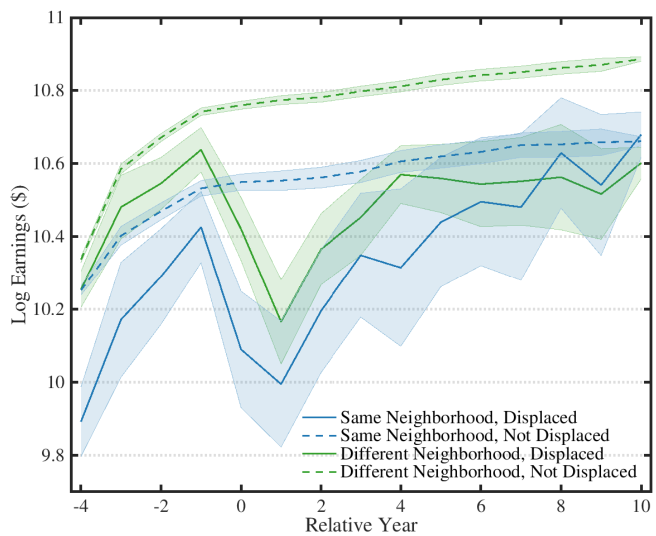
which proves the claim. The same result could be obtained by plugging (7) into equation (5) and rearranging.

The intuition is that, without ability differences, we know that those who move away have higher match quality due to the preference for living at home (b). So, with ability differences, it must be that those at home are more able if they are to make the same amount as those away.

G Appendix Figures (For Online Publication)



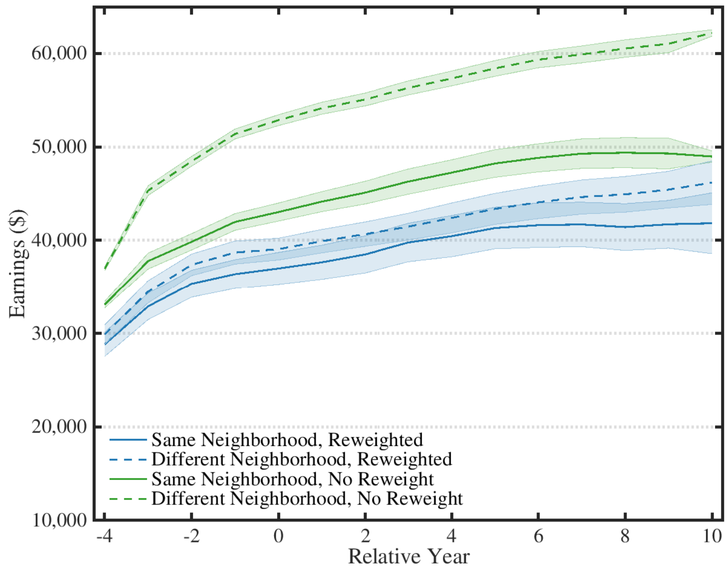
(a) Average Log Earnings for Young Displaced and Non-Displaced Workers



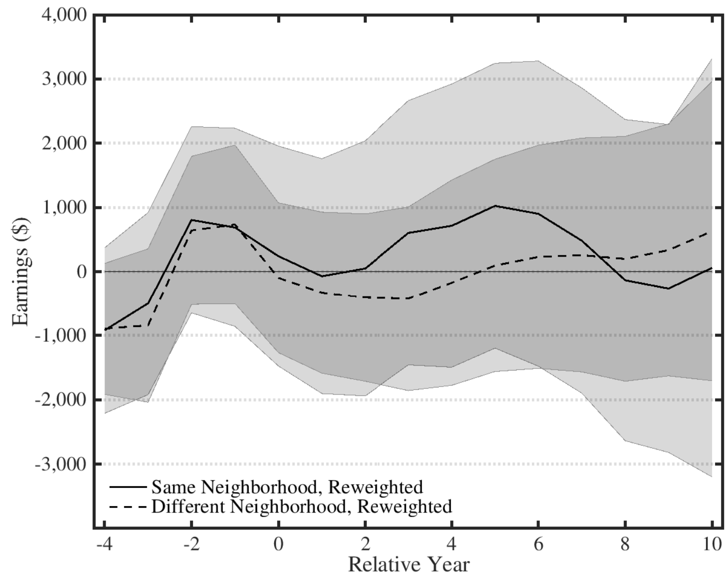
(b) Average Log Earnings for Those In Their Parents' Neighborhoods and Not

Appendix Figure 1: Average Log Earnings for Young Displaced Workers by Proximity to Parents

Note: This figure depicts log earnings and is analogous to Figure 1 in the main text that depicts earnings in levels, including its qualitative results. The shading represents 95 percent confidence intervals based on clustered standard errors, at the worker level. More information on the specification, definitions, etc., is in Figure 1.



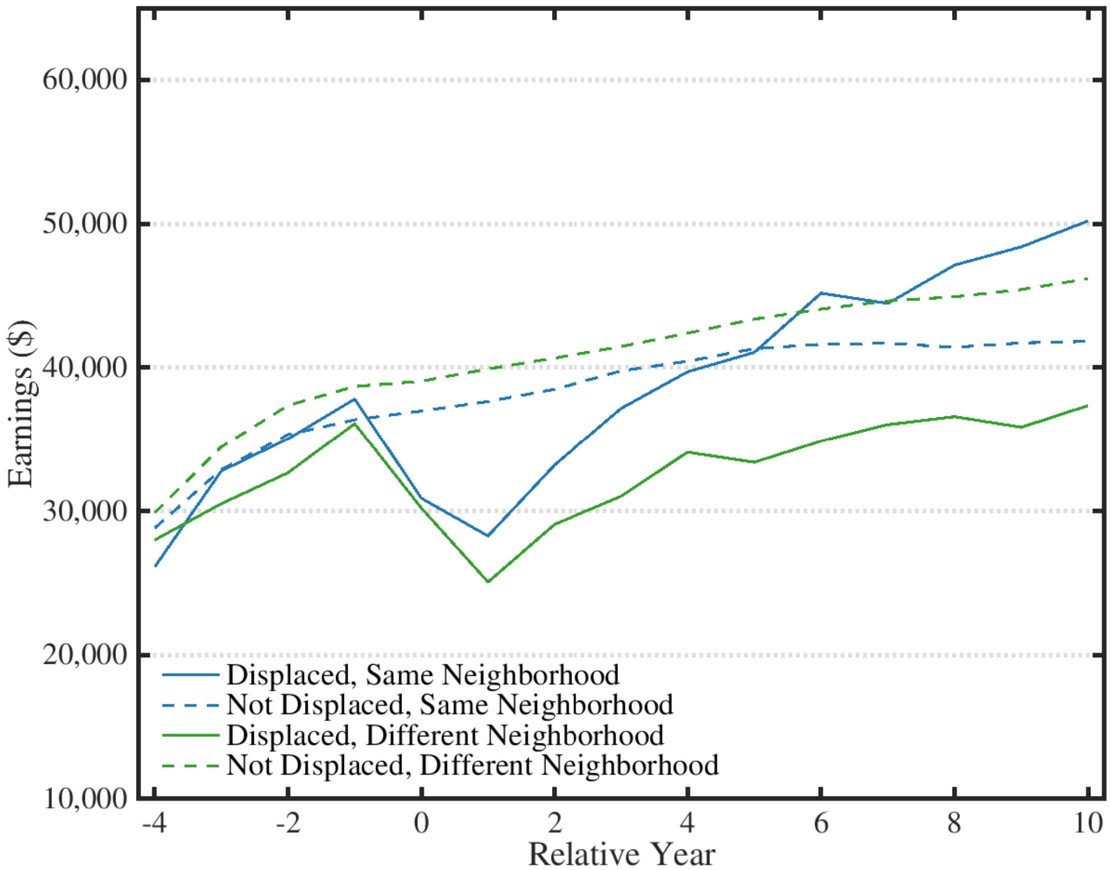
(a) Reweighted Means Without Age Trend



(b) Reweighted Means Including Age Trend

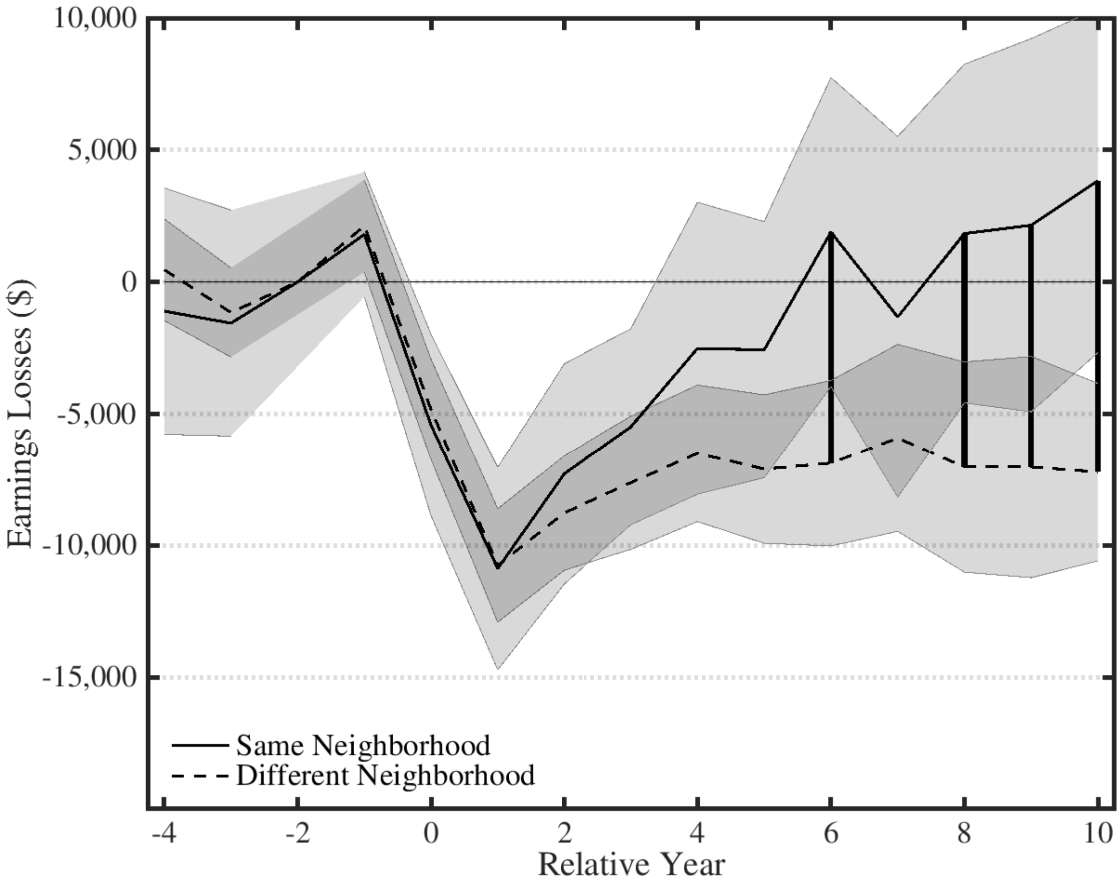
Appendix Figure 2: Mean Earnings for the Reweighted Control Samples

Note: The propensity score reweighting appears to be successful in terms of reweighting each control group so that they are mostly similar to each other. Panel A shows that after applying the propensity score weights, but without adjusting for age quartics, non-displaced workers living close to their parents and farther away in relative year '-1' have similar earnings trajectories, except for a small level shift. Panel B shows the average earnings for the non-displaced after removing an age quartic. Not surprisingly, the differences that remain between the two groups are quite small. See Appendix B.1.3 for a discussion of these results.



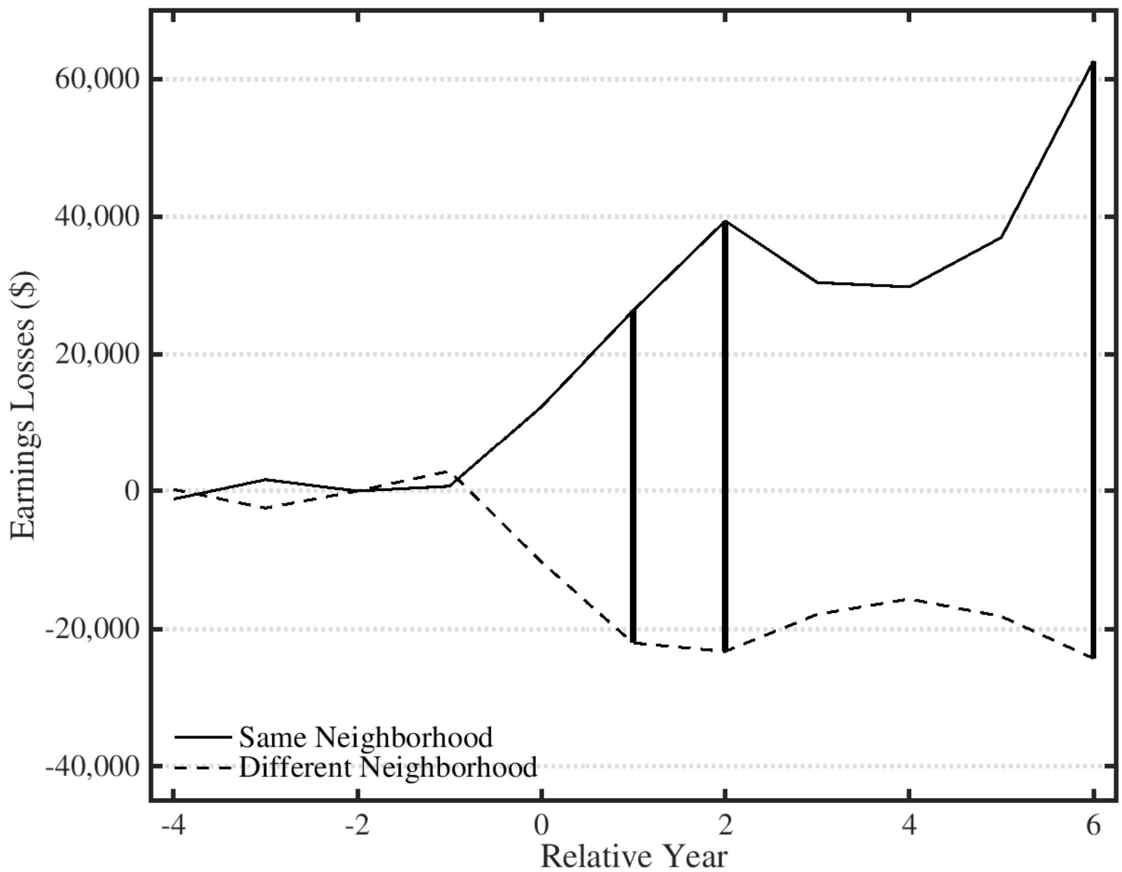
Appendix Figure 3: Means after Propensity Score Reweighting

Note: After applying propensity score weights, simple averages show that workers living near parents earn more than those who live farther away after a displacement. This figures plot propensity score weighted average earnings among workers living different distances from parents who do and do not experience a job displacement. The reweights make each other group comparable to workers who live in the same neighborhood tract as parents before a job displacement in terms of characteristics of workers' jobs, of workers' levels of education, of employment-to-population ratios where workers live, and of workers' demographics, including whether they have children or not. See Section 3.1 for a description of the reweighting. See Appendix B.1.3 for a discussion of these results.



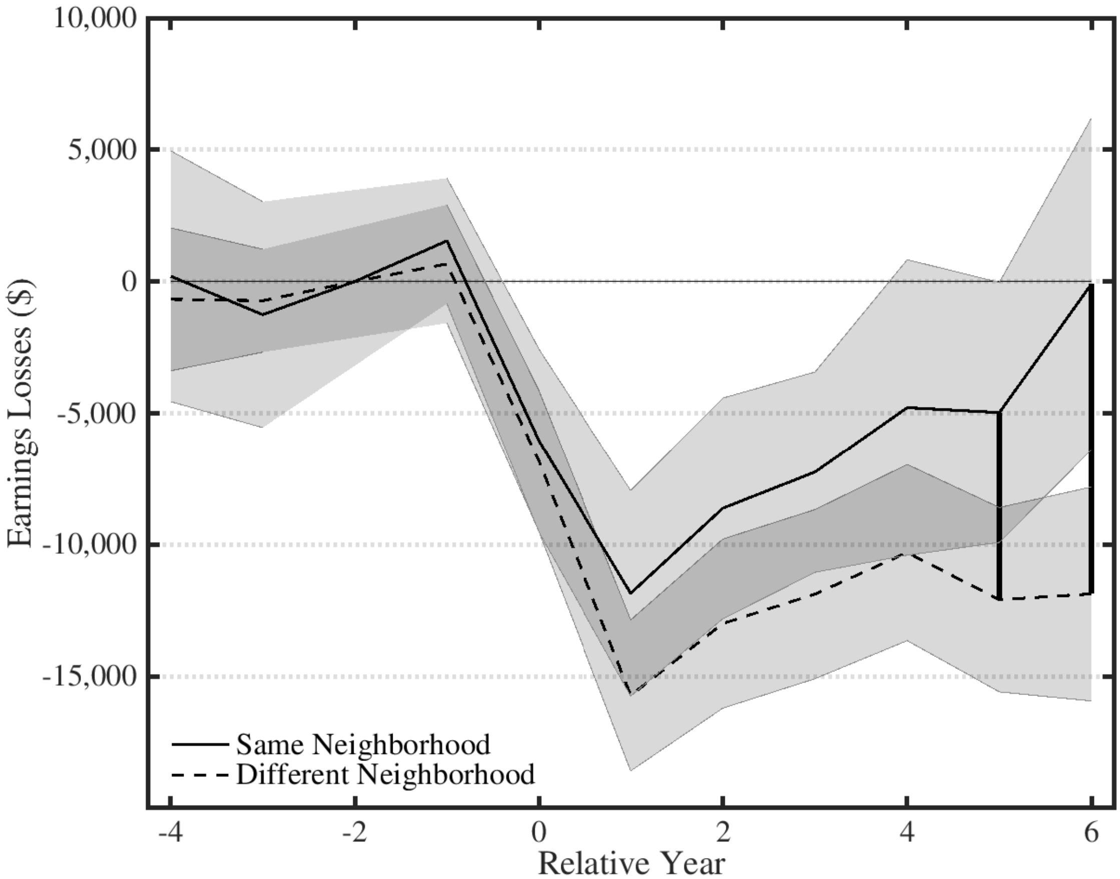
Appendix Figure 4: Earnings Losses 10 Years Out

Note: Young workers living in their parents' neighborhoods at the time of displacement experience healthier earnings recoveries than those living farther away, even after controlling for observable differences using propensity score reweighting. The figure plots propensity score weighted regression coefficients from equation (1) describing the impact of a job displacement on the earnings of young workers. This figure is similar to Figure 2 in the main text but allows for 10 years of coefficients after displacement. The shading represents 95 percent confidence intervals, and any vertical bars connecting the two lines signify that the estimates are statistically significantly different from one another in that year at the 5 percent level. Shading and statistical significance are based on standard errors computed by clustering at the worker level. See Section 3 for more details about the empirical methodology and Section 4.1 for a discussion of these results.



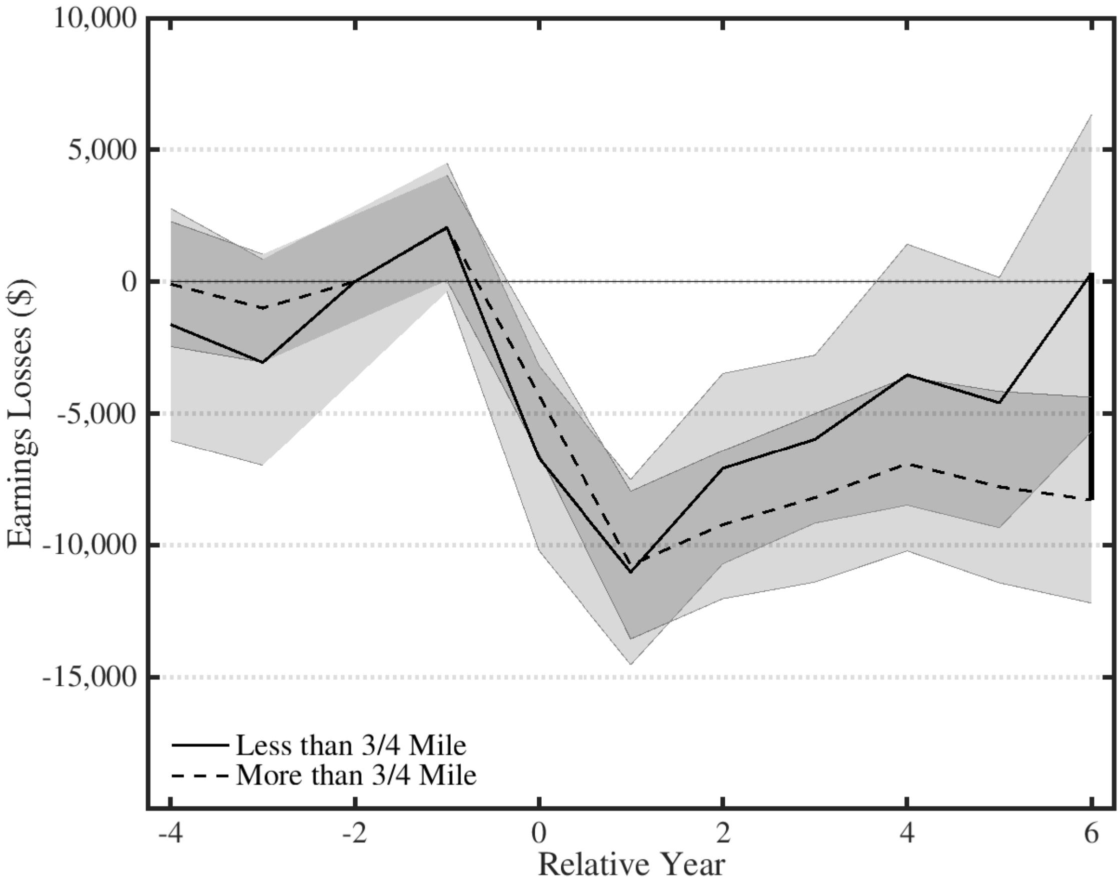
Appendix Figure 5: Instrumenting for Parental Proximity with Parental Death

Note: Using parental death to instrument for parental proximity strengthens our results, but parental death is a weak instrument that fails an overidentification test. This figure is similar to Figure 2 in the main text, except that it uses the death of a parent to instrument for whether workers lived in the same neighborhood as a parent before a job displacement. Vertical bars connecting the two lines signify that the estimates are statistically significantly different from one another in that year at the 5 percent level based on standard errors computed by clustering at the worker level. Please see Appendix C.2 for additional details.



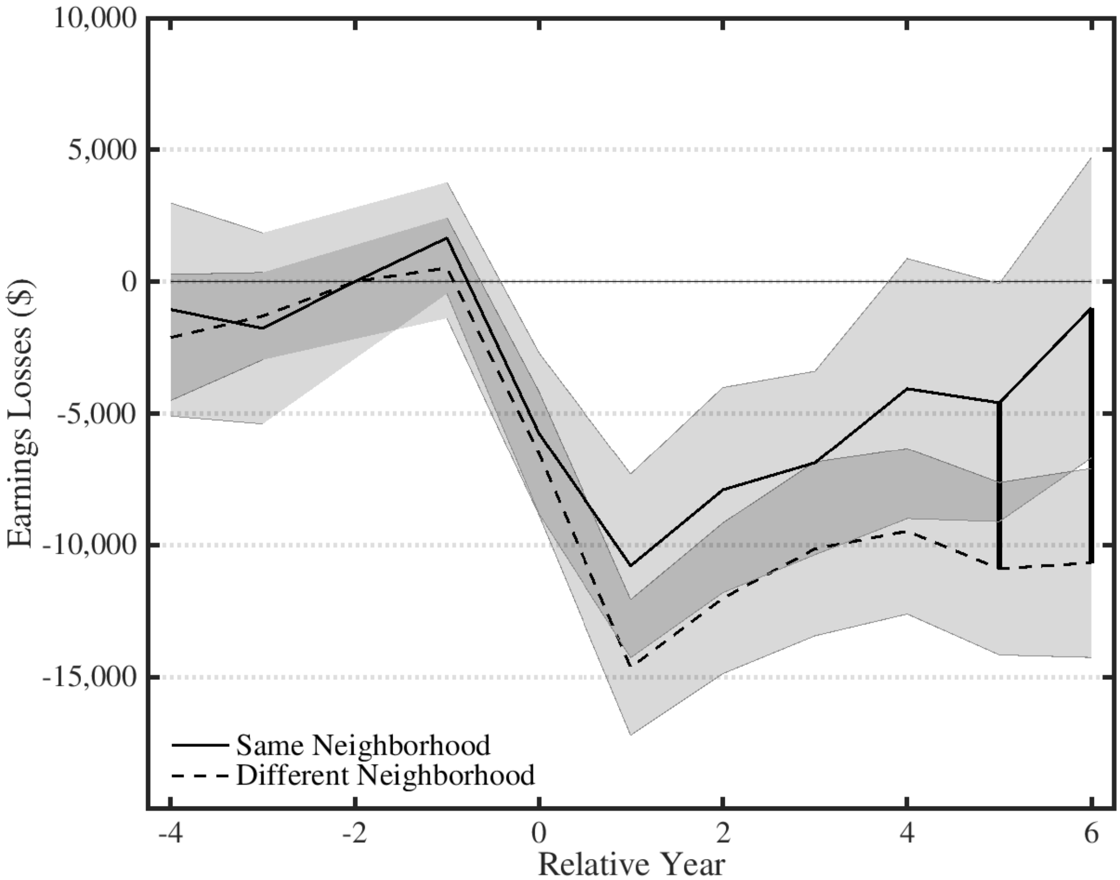
Appendix Figure 6: Earnings Losses Without Propensity Score Reweighting

Note: In the medium- and long-run, young workers living in the same neighborhoods with their parents experience a full recovery in earnings after a displacement event. Young workers not living in their parents' neighborhoods experience large and permanent earnings losses, amounting to about 30 percent of their pre-displacement earnings even six years after the displacement event. The figure plots regression coefficients from equation (1) describing the impact of a job displacement on the earnings of groups of young workers, aged 25 to 35 at the time of displacement, but uses longitudinal weights from the PSID as opposed to propensity score weights. The shading represents 95 percent confidence intervals and any vertical bars represent statistically significant differences at the 5 percent level. Standard errors are clustered at the worker level. The definitions of displacements and of whether workers live in the same tract as their parents follow those in Figure 1, and Section 2 contains more information on the sample construction, data, and definitions. The regression controls for worker and year fixed effects as well as a quartic term in each workers' age that we allow to differ between the different groups plotted on the figure. See Appendix C.3 for details.



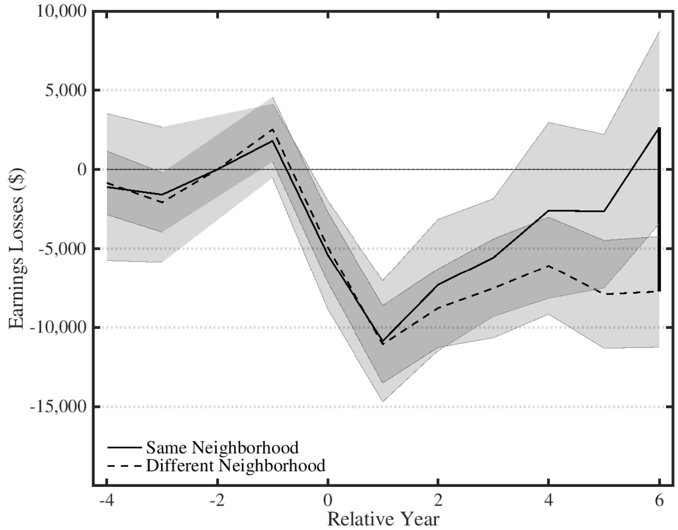
Appendix Figure 7: Earnings Losses for Young Displaced Workers (Using Distance Measures)

Note: These results estimate equation (1) where we define closeness by distance to parents, and where less than 3/4 miles is close. The results are very similar to the baseline specification in Figure 2. The shading represents 95 percent confidence intervals, and any vertical bars connecting the two lines signify that the estimates are statistically significantly different from one another in that year at the 5 percent level. We cluster the standard errors at the worker level. See Section 3 for full details of the specification. See Appendix C.4 for a discussion of these results.

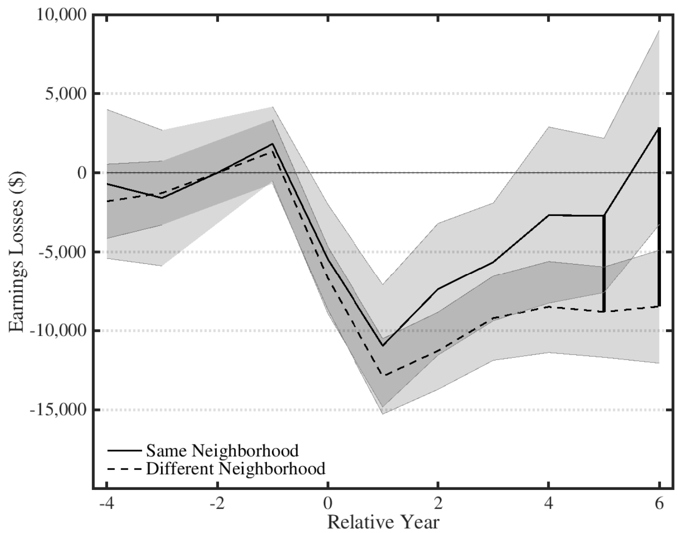


Appendix Figure 8: Earnings Losses for Young Displaced Workers (Heads and Wives)

Note: These results present the coefficients from equation (1) using both heads and wives as opposed to just heads as in our baseline sample. The results are very similar to the baseline results in Appendix Figure 6. The shading represents 95 percent confidence intervals, and any vertical bars connecting the two lines signify that the estimates are statistically significantly different from one another in that year at the 5 percent level. We cluster the standard errors at the worker level. See Section 3 for full details of the specification. See Appendix C.5 for a discussion of these results.



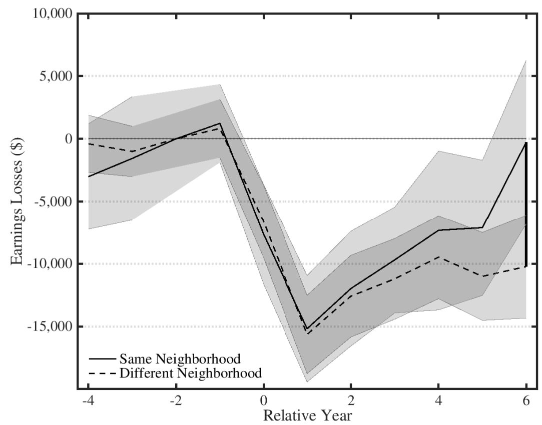
(a) Including Parents' Characteristics



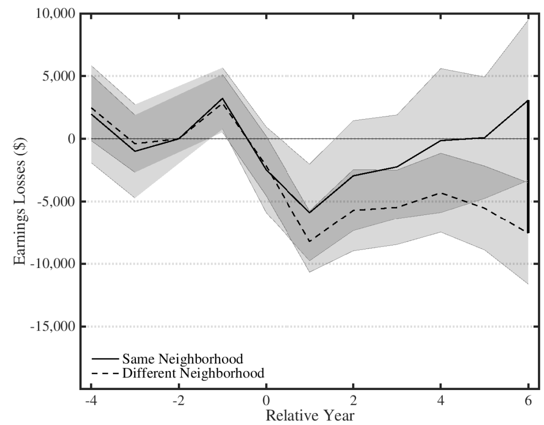
(b) Including Only Predetermined Characteristics

Appendix Figure 9: Reweighted Regressions Based on Alternative Reweighting Specifications

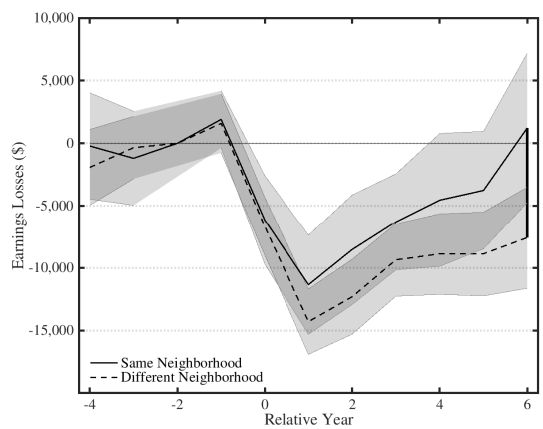
Note: The results from the propensity score reweighting do not appear to be sensitive to including either additional controls for parents' characteristics, or to including only predetermined characteristics, like educational levels, age, and race. The figure plots propensity score weighted regression coefficients from equation (1) describing the impact of a job displacement on the earnings of young workers. The weights in Panel A are calculated to match parents' characteristics between the different samples, in addition to the main characteristics. The weights in Panel B are calculated to only match predetermined characteristics between the sample. See Appendix D.1 for more details on these two types of weights, see Figure 2 for the original specification, and see Section 3 for more information on the reweighting scheme.



(a) Baseline Earnings as Linear Interaction



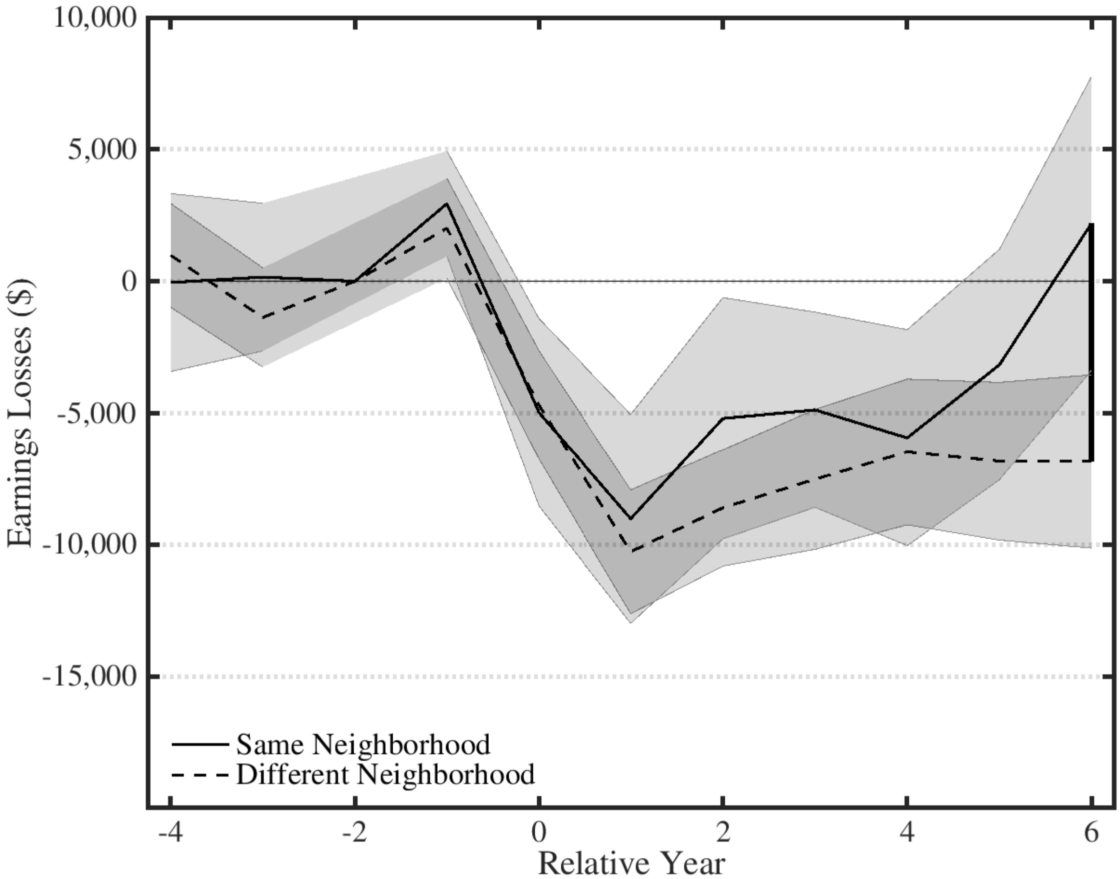
(b) Baseline Earnings as a Dummy



(c) College Education as a Dummy

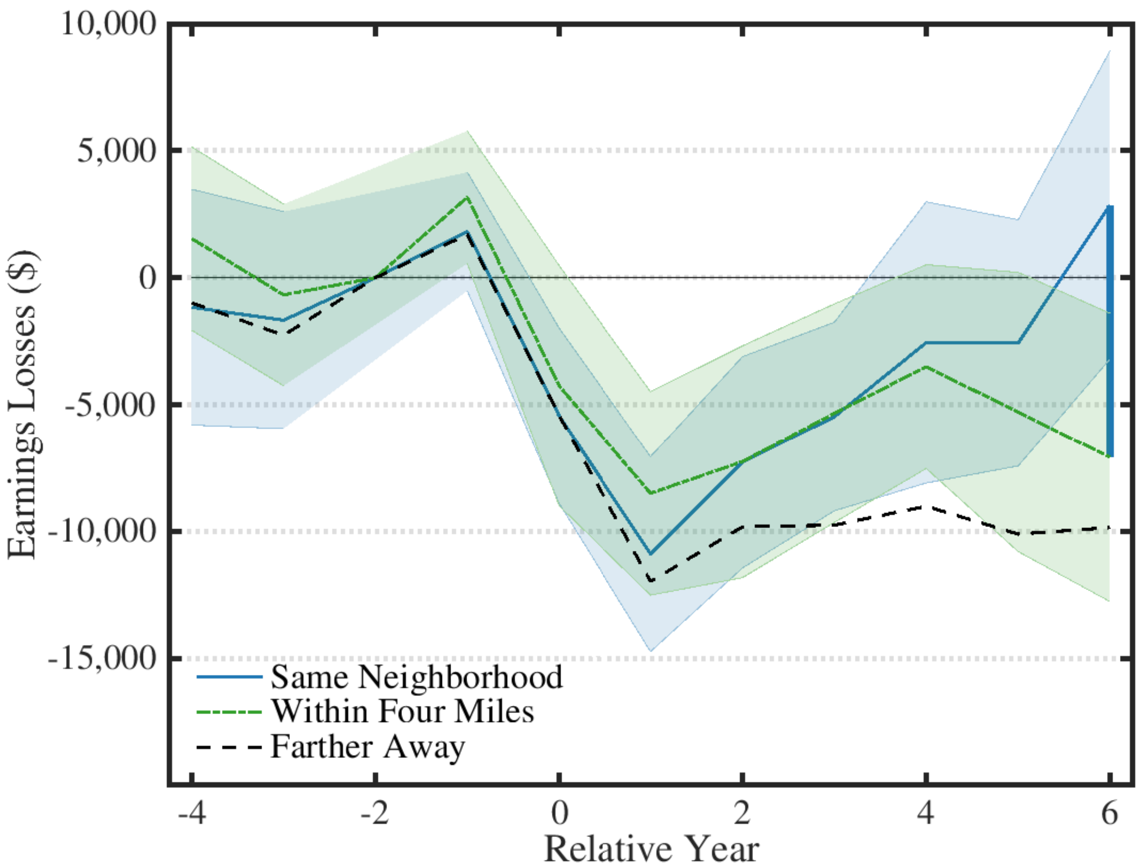
Appendix Figure 10: Including Additional Interactions in the Baseline Specification

Note: Additional interactions with the displacement dummies (equation 4) do not change the effect of parental proximity on the post-displacement earnings outcomes. Although interacting with earnings prior to job loss generally makes the initial earnings losses similar for the two groups, the two paths still diverge later on. Plotted are regression coefficients from estimating equation (4). Panel A shows the coefficients when one includes an additional interaction with a linear term for earnings, Panel B shows the results after including an interaction with a dummy for having above average earnings, and Panel C shoes results after including an interaction with a dummy for being college educated. Appendix D.2 includes more details on the specification.



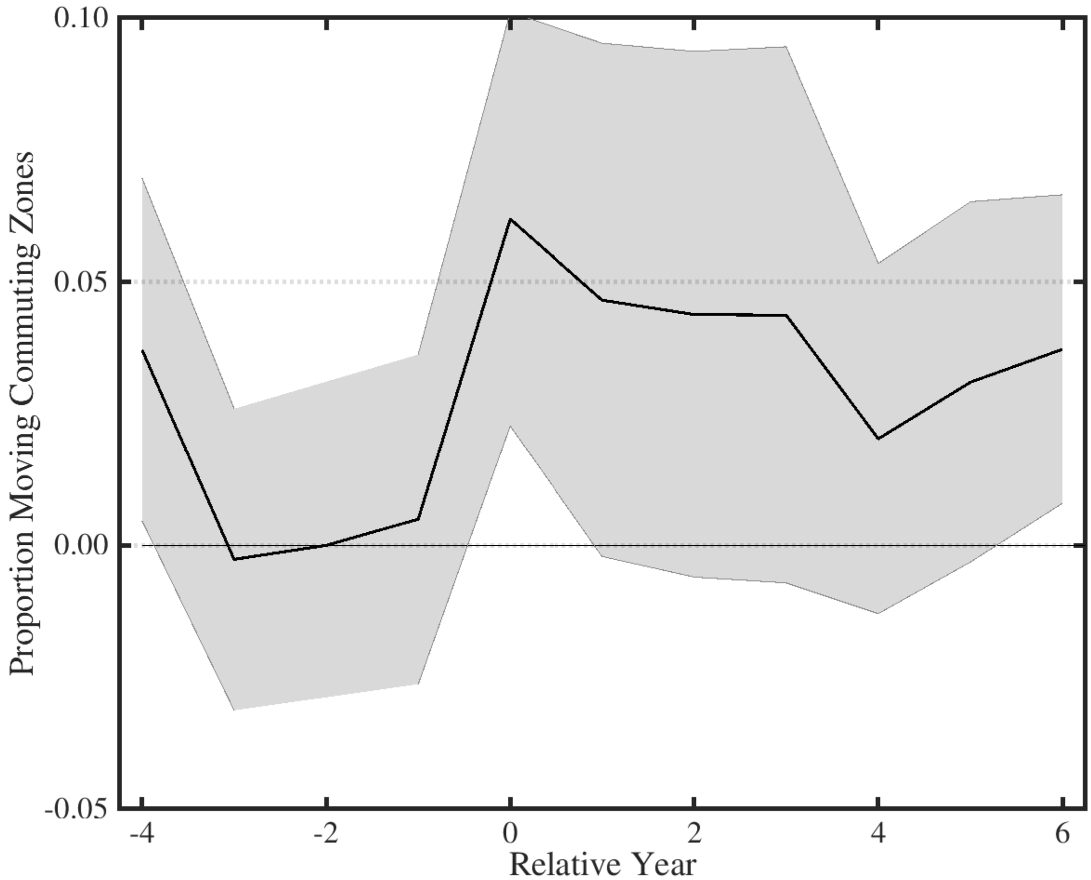
Appendix Figure 11: Reweighting on the Subsample with Common Support

Note: Restricting the sample to a subset of observations with common support gives qualitatively similar results to reweighting the whole sample, though with less precision. This replicates Figure 2 with a sample restricted to observations where there is common support between the group that was displaced at home and the various other reweighting groups. Figure 2 contains more details on the methodology. See Appendix D.3 for a discussion of these results.



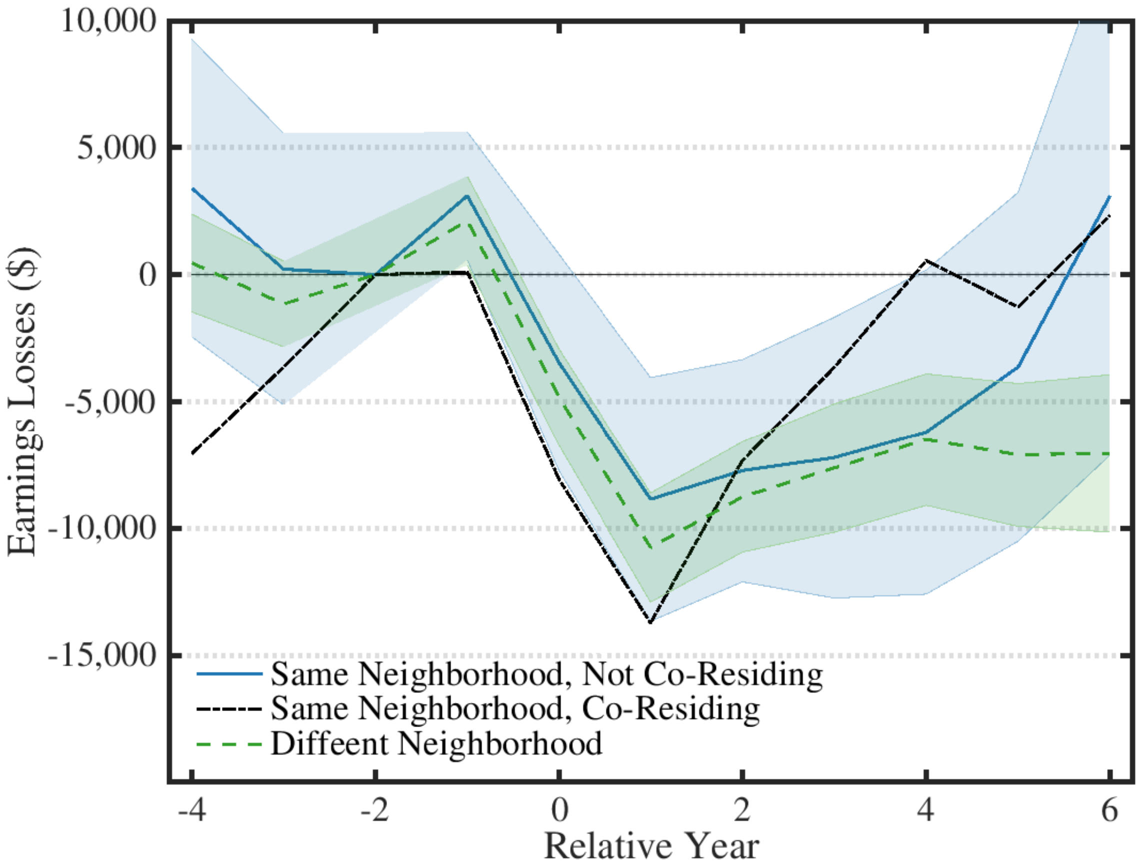
Appendix Figure 12: Earnings Benefits of Living Close but Not Same Tract

Note: Young adults living outside of their parents' neighborhood, but still relatively close (within four miles), experience some of the benefits of parental proximity. Those individuals living farther away than four miles see the largest earnings losses for each horizon. The shading represents 95 percent confidence intervals. Vertical bars between the group within four miles and the group the same tract denote statistical significant differences at the 5 percent level. We cluster the standard errors at the worker level. See Appendix E.1 for more details.



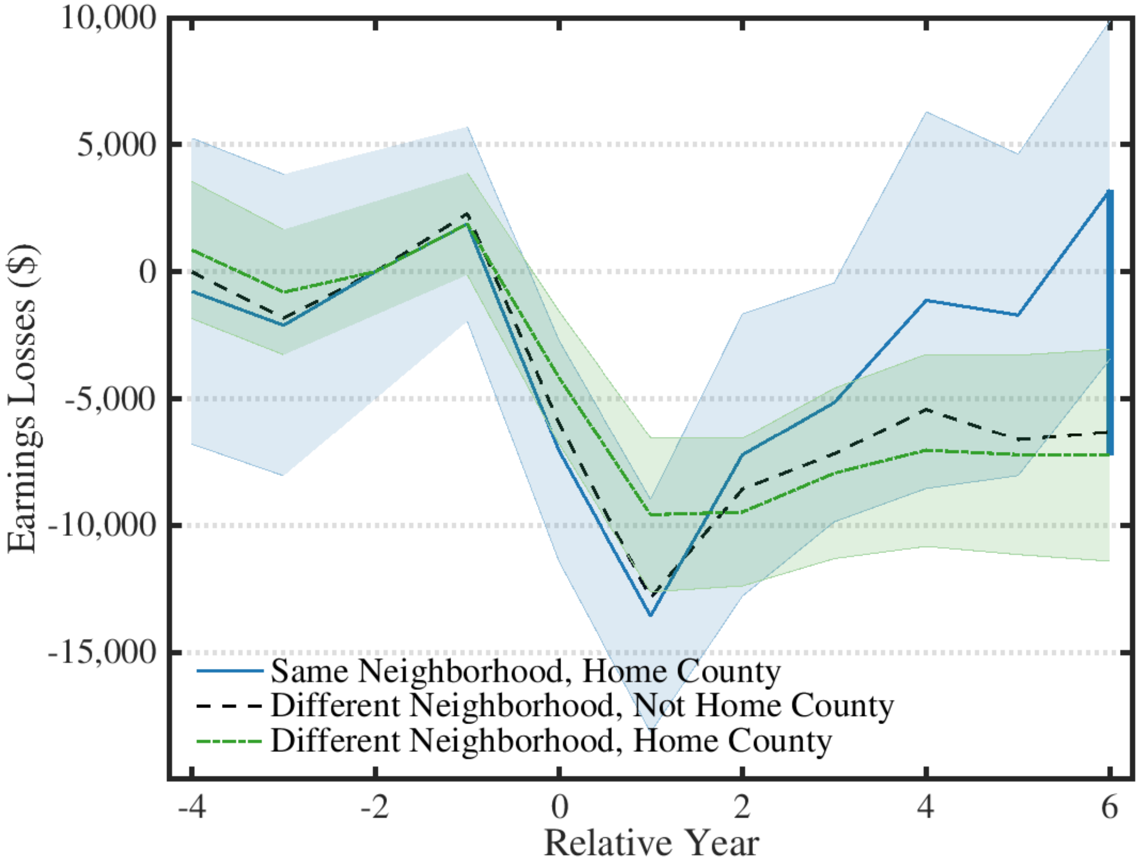
Appendix Figure 13: Probability of Switching Commuting Zones Around Displacement

Note: At the time of displacement, the proportion of workers moving between commuting zones annually rises by about 0.05. Since the baseline is around 0.05, this is a sharp increase. Plotted are regression coefficieints from a linear probability model with a specification very similar to equation (1). The main differences are the outcome, moving between commuting zones in the year in question, and that we pool both groups of workers to increase precision. The shading represents 95 percent confidence intervals based on clustered standard errors, at the worker level. We use commuting zones as the relevant measure of geography because they most closely resemble the ‘‘regional labor markets’’ that Huttunen et al. (2018) use with Norwegian data. Section 3 contains more information about the specification, definitions, etc. See Appendix E.2 for a discussion of these results.



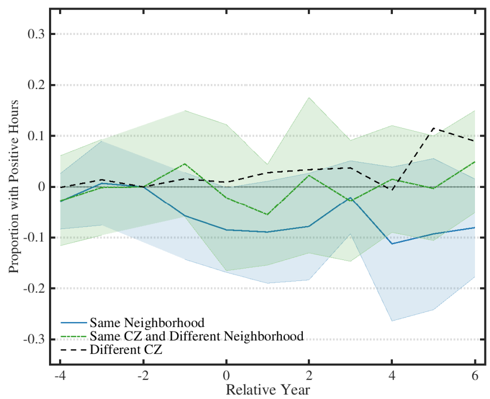
Appendix Figure 14: Earnings Losses for Young Displaced Workers (Same Tract vs. Co-residing)

Note: The post-displacement earnings recoveries look similar for those workers who actually live in the same house as their parents (co-residing) and those who live in the same tract as their parents but are not co-residing. Both groups appear to do better than those who live outside their parents' neighborhoods. The figure plots regression coefficients from equation (2) describing the impact of a job displacement on the earnings of workers who lived in a different census tract from their parents, those who lived in the same census tract but in a different house, and finally those who lived in the same house as their parents. The groups are mutually exclusive. The figure includes vertical bars that connect the line for workers who live in the same tract (not co-residing) with the line for workers who live in a different tract. We include these when the estimates are statistically significantly different from one another at the 5 percent level. We cluster the standard errors at the worker level. See Figure 7 for full details of the specification. See Appendix E.3 for a discussion of these results.

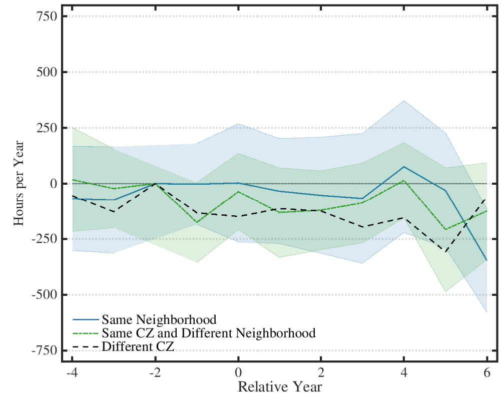


Appendix Figure 15: Earnings Losses for Young Displaced Workers with Home County Interactions

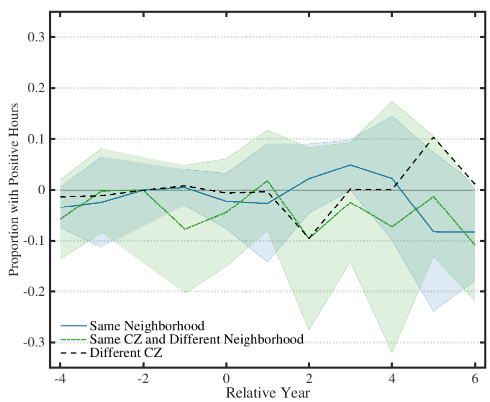
Note: Post-displacement earnings recoveries look similar to our baseline results, even after interacting the displacement dummies in equation (1) with whether a worker lived in the county where they grew up at the time of displacement. The earnings losses for those in their home county look similar to those who are neither in their parents' neighborhoods or their home county. The figure plots regression coefficients from a specification similar to equation (1) describing the impact of a job displacement on the earnings of workers who lived in the same census tract as their parents and their home county, those who lived in the same tract as parents but a different county from where they they grew up, and workers who lived away from their parents but in the county where they grew up. The shading represents 95 percent confidence intervals and the figure includes vertical bars that connect the line for workers who live in the first and third categories when those estimates are distinguishable at the 5 percent level. Standard errors are clustered at the worker level. More information on these specifications is in Appendix E.4.



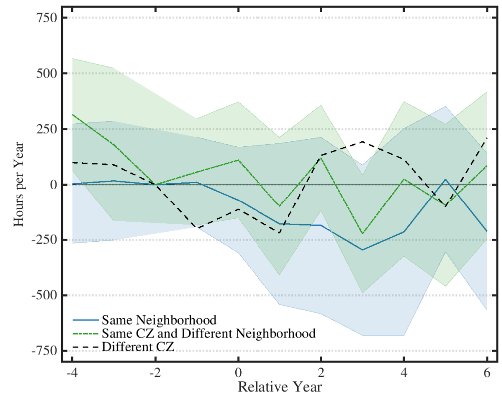
(a) Mothers: Working Positive Hours



(b) Mothers: Number of Hours Worked



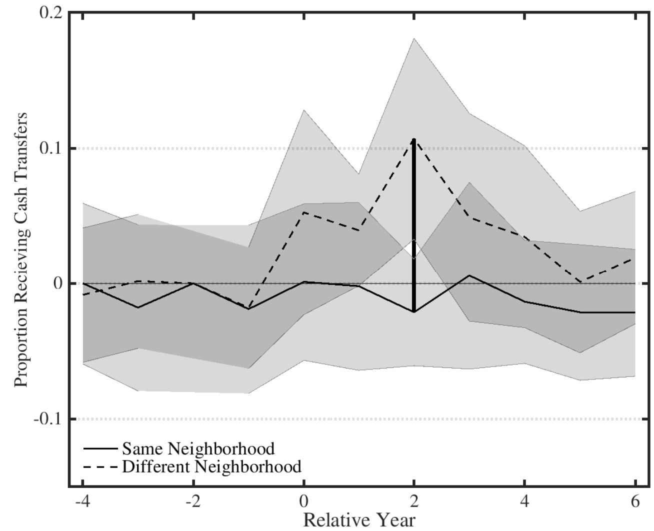
(c) Fathers: Working Positive Hours



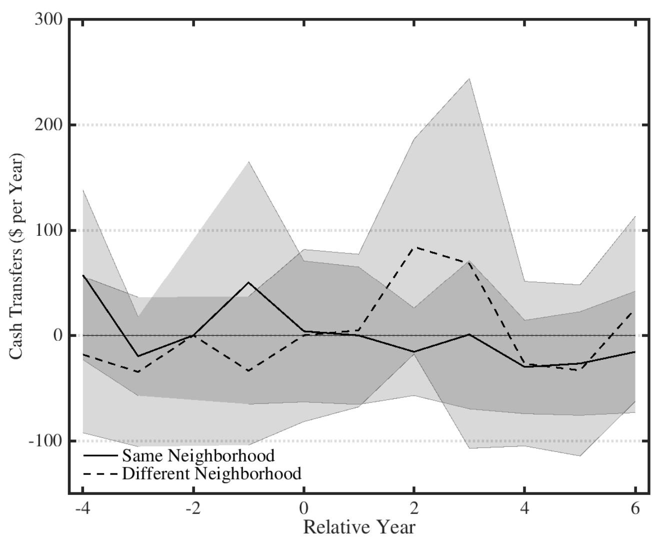
(d) Fathers: Number of Hours Worked

Appendix Figure 16: Workers' Parents' Intensive and Extensive Labor Supply

Note: Intensive and extensive margin labor supply are imprecisely estimated for workers' parents. There is a statistically insignificant suggestion of an extensive margin decrease among mothers immediately after their child's displacement, and another statistically insignificant suggestion of a decrease in intensive margin labor supply six years after their child's displacement. The figure plots regression coefficients from a specification similar to equation (1) describing the impact of a job displacement on employment of the closest parent including controls for employment to population ratios in the child's county and the mother's county as well as lagged fixed effects for the occupation the mother worked in as well as lagged industry (of the mother) by year fixed effects. Vertical bars connect the outcomes for parents in the same CZ as their child and parents in the same neighborhood when the estimates are statistically distinguishable in that year at the 5 percent level. Inference is done by clustering at the level of the parent. The definitions of displacements follow Figure 1, and Section 5.1 contains more information about these coefficients. See Appendix F.1 for a discussion of these results.



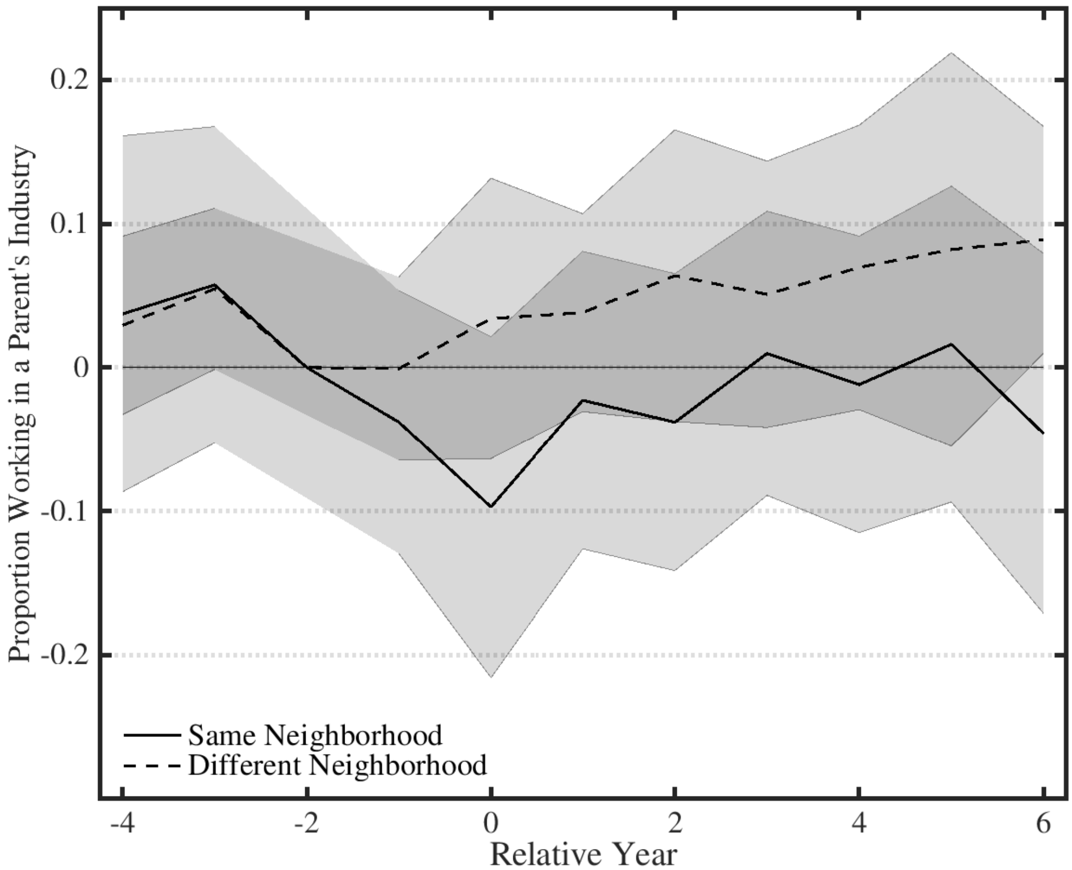
(a) Receiving a Monetary Transfer



(b) Total Monetary Transfers

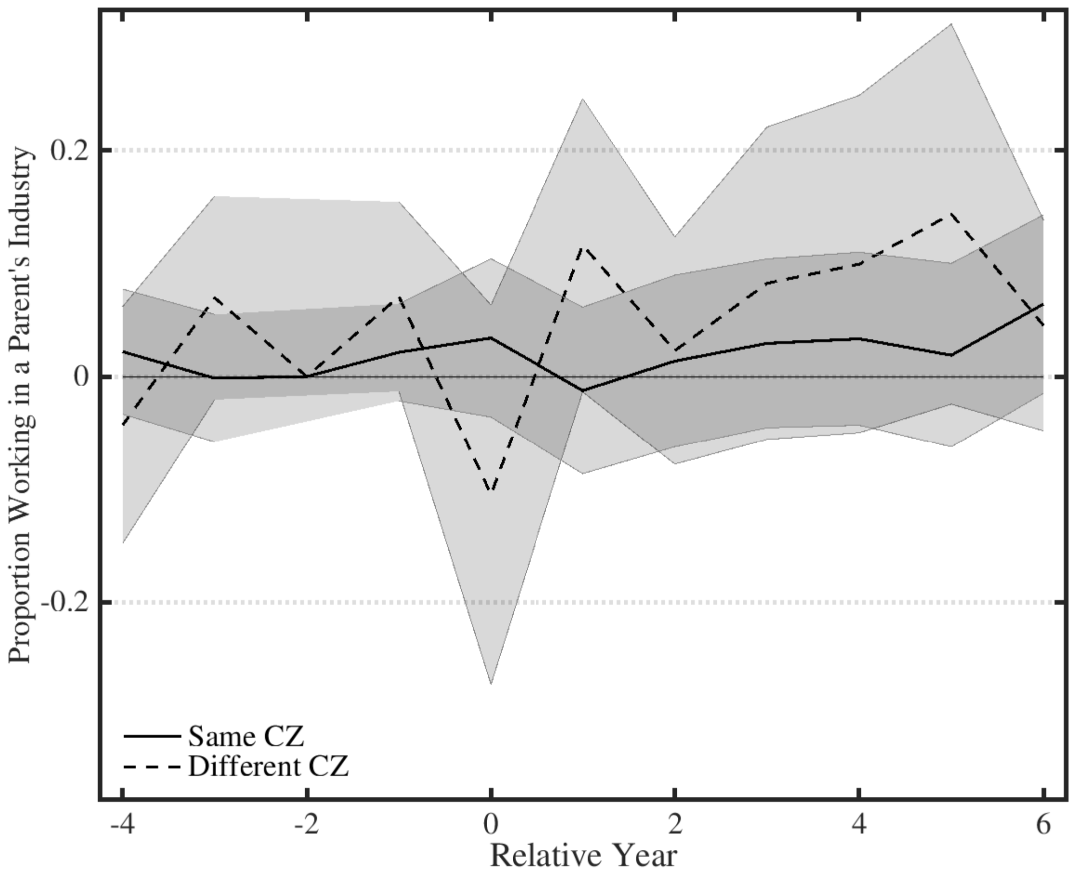
Appendix Figure 17: Monetary Transfers around Displacements

Note: The annual question in the PSID asking about help from friends or relatives gives relatively noisy results around a displacement, but there is some evidence that workers who live farther from their parents are more likely to receive a small amount of monetary help after a displacement. These figures plot regression coefficients from equation (1) describing the impact of a job displacement on monetary transfers. The measure in Panel A is the proportion of workers who report that they received help from friends or family. The measure in Panel B is the reported dollar value per year. The shading represents 95 percent confidence intervals, and any vertical bars connecting the two lines signify that the estimates are statistically significantly different from one another in that year at the 5 percent level. Shading and statistical significance are based on standard errors computed by clustering at the worker level. See Figure 11 and Appendix F.2 for more details on the specification and the data.



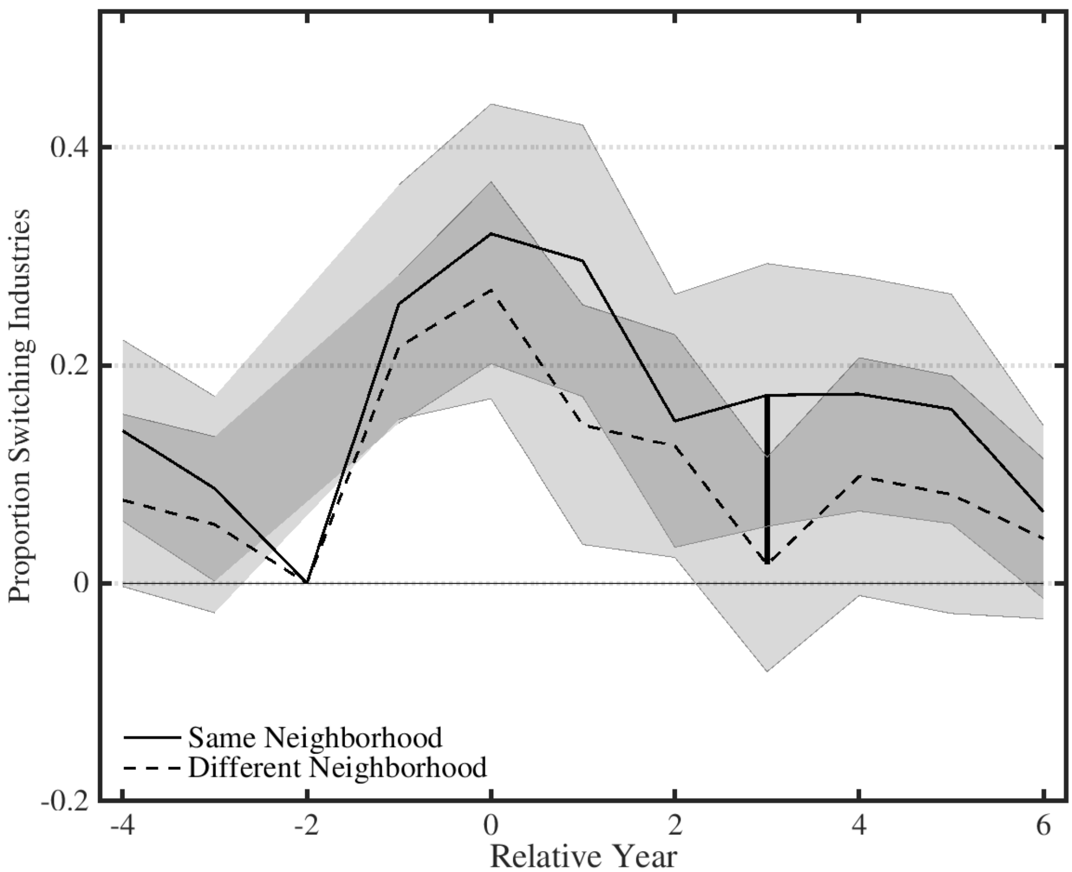
Appendix Figure 18: Working in a Parent's Industry for Older Workers

Note: Older workers who do not live near their parents may be more likely to work in their parents' industries after a displacement event, though the estimates are quite imprecise. These figures plot regression coefficients from equation (1) describing the impact of a job displacement on the proportion of older (36 to 55 year old) workers who work in the same one-digit PSID coded industry as a parent. The shading represents 95 percent confidence intervals, and the lack of vertical bars connecting the two lines signify that none of the estimates are statistically different from one another at the 5 percent level in any years. Shading and statistical significance are based on standard errors computed by clustering at the worker level. See Section 5.3 for a brief discussion of these results.



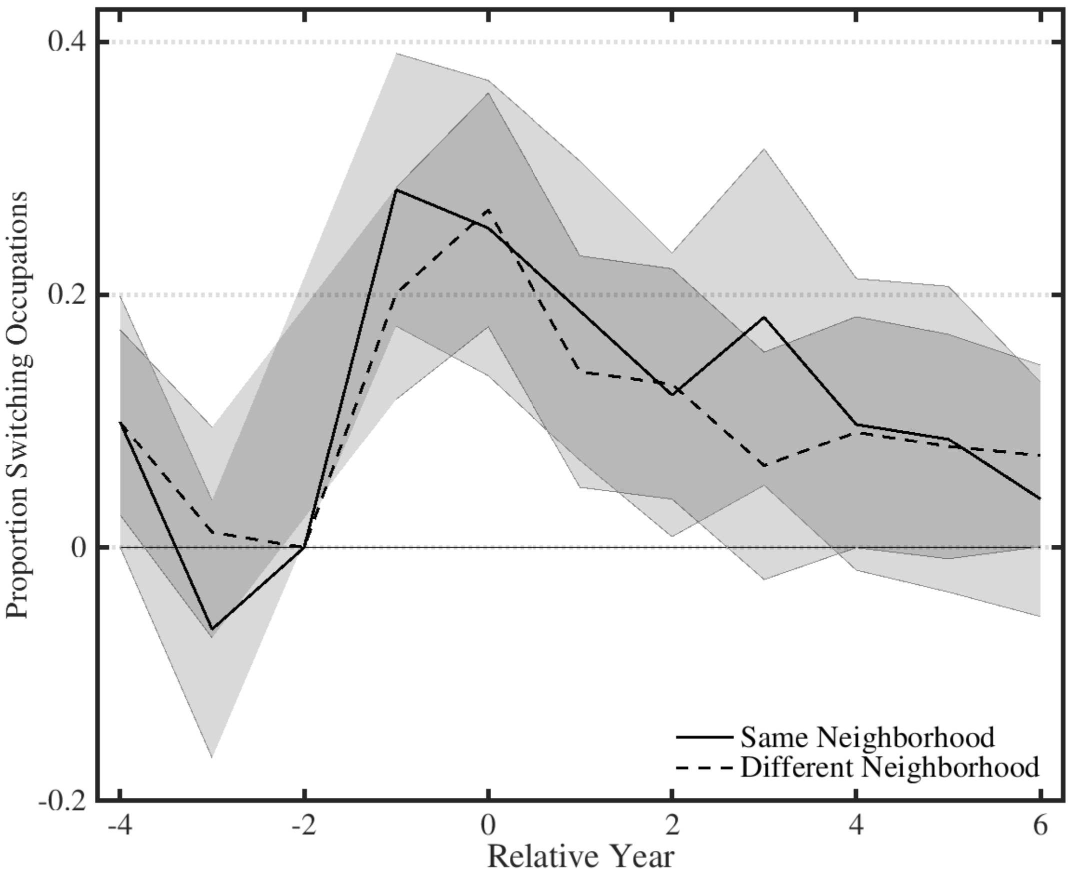
Appendix Figure 19: Working in a Parent's Industry by Commuting Zone

Note: Younger workers who live in the same Commuting Zone as their parents are not any more likely to work in their parents' industries after a displacement, though the estimates are imprecise. These figures plot regression coefficients from equation (1) describing the impact of a job displacement on the proportion of younger workers who work in the same one-digit PSID coded industry as a parent. The shading represents 95 percent confidence intervals, and vertical bars connecting the two lines signify that the estimates are statistically different from one another at the 5 percent level. Shading and statistical significance are based on standard errors computed by clustering at the worker level. Section 5.3 for a brief discussion of these results.



Appendix Figure 20: Probability of Switching Industries

Note: Those who live in the same tract as their parents prior to displacement are more likely to switch industries at the time of job loss than those who live farther away. These figures plot regression coefficients from equation (1) describing the impact of a job displacement on switching one-digit PSID industries. The shading represents 95 percent confidence intervals, and any vertical bars connecting the two lines signify that the estimates are statistically significantly different from one another in that year at the 5 percent level. We cluster the standard errors at the worker level. Section 5.3 for a brief discussion of these results.



Appendix Figure 21: Probability of Switching Occupations

Note: Those who live in the same tract as their parents prior to displacement are just as likely to switch occupations at the time of job loss than those who live farther away. These figures plot regression coefficients from equation (1) describing the impact of a job displacement on switching one-digit PSID occupations. The shading represents 95 percent confidence intervals, and any vertical bars connecting the two lines signify that the estimates are statistically significantly different from one another in that year at the 5 percent level. We cluster the standard errors at the worker level. Section 5.3 for a brief discussion of these results.

H Appendix Tables (For Online Publication)

H.1 Summary Statistics Using Reweights with Common Support

Variable	Panel A: PSID Weights				Panel B: Reweighted			
	Same Tract		Different Tract		Same Tract		Different Tract	
	Displaced	Not Displaced	Displaced	Not Displaced	Displaced	Not Displaced	Displaced	Not Displaced
Earnings	$31,600	$36,500	$38,500	$41,800	$31,600	$31,500	$31,200	$32,500
	[1.00]	[0.03]	[0.00]	[0.00]	[1.00]	[0.33]	[0.40]	[0.90]
Average Change in Earnings	$2,200	$2,000	$2,900	$3,000	$2,200	$2,000	$3,300	$2,400
	[1.00]	[0.34]	[0.90]	[0.44]	[1.00]	[0.24]	[0.79]	[0.57]
Wage	$15.18	$16.93	$18.29	$19.03	$15.18	$15.27	$15.25	$15.50
	[1.00]	[0.06]	[0.00]	[0.00]	[1.00]	[0.42]	[0.53]	[0.82]
Years of Schooling	12.03	12.44	12.33	12.75	12.03	12.13	12.06	12.17
	[1.00]	[0.01]	[0.00]	[0.00]	[1.00]	[0.76]	[0.65]	[0.98]
Share in Goods Industries	0.53	0.46	0.56	0.39	0.53	0.49	0.52	0.42
	[1.00]	[0.10]	[0.91]	[0.00]	[1.00]	[0.40]	[0.87]	[0.03]
Share Manager/Professional	0.14	0.17	0.17	0.21	0.14	0.17	0.14	0.16
	[1.00]	[0.16]	[0.03]	[0.00]	[1.00]	[0.86]	[0.72]	[0.86]
Employer Tenure	5.26	6.53	5.11	6.38	5.26	5.30	5.21	5.30
	[1.00]	[0.00]	[0.69]	[0.00]	[1.00]	[0.90]	[0.86]	[0.93]
Unemp Rate in County	7.65	7.30	7.91	7.21	7.65	7.45	7.94	7.63
	[1.00]	[0.37]	[0.95]	[0.05]	[1.00]	[0.81]	[0.50]	[0.98]
Age	27.76	28.56	28.28	28.50	27.76	27.66	27.52	27.71
	[1.00]	[0.00]	[0.00]	[0.00]	[1.00]	[0.56]	[0.54]	[0.68]
Share with Children	0.65	0.63	0.65	0.63	0.65	0.60	0.67	0.64
	[1.00]	[0.76]	[0.44]	[0.38]	[1.00]	[0.43]	[0.75]	[0.98]
Fraction Male	0.81	0.80	0.84	0.85	0.81	0.80	0.76	0.83
	[1.00]	[0.91]	[0.28]	[0.34]	[1.00]	[0.87]	[0.40]	[0.70]
Number of Records	175	3,019	353	8,413	175	3,019	353	8,413

Appendix Table 1: Means Before and After Reweighting the Sample with Common Support

Note: After applying the propensity score weights on the sample with common support, the sample of workers who live in the same tract as their parents and those living farther away are statistically indistinguishable in terms of many observable characteristics. This table reports means for each group in the sample with common support using PSID weights in the first four columns and the propensity score weights in the last four columns. For each variable, we report the mean and a p-value in brackets of a Wald test that this mean is the same as the value in the first column. See the initial version, Figure 2, for more details on the table specification.

H.2 Underlying Statistics of Figures in the Main Text

	Displaced		Not Displaced	
Pd	Avg	StdEr	Avg	StdEr
-4	33,500	556	36,200	91
-3	40,100	1,370	43,800	227
-2	42,400	1,330	46,700	228
-1	45,000	1,350	49,500	234
0	38,800	1,530	50,900	255
1	32,400	1,450	52,200	282
2	37,800	1,580	53,100	303
3	40,900	1,630	54,300	332
4	43,000	1,630	55,300	349
5	43,400	1,850	56,400	370
6	45,600	2,000	57,300	382
7	46,600	2,180	57,800	399
8	49,500	2,400	58,300	409
9	46,900	2,360	58,700	419
10	49,000	802	59,700	153

(a) Coefficients

	Observations
Not Displaced	340,931
Displaced	12,812

(b) Observations

Appendix Table 2: Underlying Statistics for Figure 1 Panel A

Note: This lists the underlying coefficients, standard errors of those coefficients, and p-values used to calculate the lines denoting statistically significant differences in Figure 1 Panel A. For more details see the notes for that Figure.

	Same Neighborhood				Different Neighborhood			
	Displaced		Not Displaced		Displaced		Not Displaced	
Pd	Avg	StdEr	Avg	StdEr	Avg	StdEr	Avg	StdEr
-4	26,100	927	33,100	175	35,700	655	36,900	105
-3	32,800	2,160	37,800	429	42,600	1,650	45,300	260
-2	35,000	2,360	39,800	439	44,800	1,570	48,400	259
-1	37,800	1,900	42,000	445	47,400	1,670	51,400	267
0	30,900	2,070	43,000	484	41,500	1,900	52,900	291
1	28,300	2,540	44,100	537	33,600	1,730	54,100	322
2	33,200	2,810	45,100	609	39,200	1,870	55,100	344
3	37,100	3,210	46,300	675	42,000	1,880	56,300	376
4	39,700	3,970	47,200	687	44,100	1,720	57,300	397
5	41,100	3,900	48,200	738	44,100	2,100	58,400	420
6	45,200	4,080	48,800	751	45,800	2,300	59,300	436
7	44,500	4,740	49,300	790	47,300	2,460	59,900	454
8	47,100	4,290	49,400	807	50,400	2,880	60,500	466
9	48,400	4,730	49,300	821	46,400	2,720	61,000	478
10	50,200	1,500	49,000	299	48,600	948	62,200	174

(a) Coefficients

	Observations	
	Same	Different
Not Displaced	75,054	265,877
Displaced	3,669	9,143

(b) Observations

Appendix Table 3: Underlying Statistics for Figure 1 Panel B

Note: This lists the underlying coefficients, standard errors of those coefficients, and p-values used to calculate the lines denoting statistically significant differences in Figure 1 Panel B. For more details see the notes for that Figure.

	Same Neighborhood		Different Neighborhood		Difference
	Coef	StdEr	Coef	StdEr	p-value
-4	-1,070	2,330	463	962	0.542
-3	-1,550	2,140	-1,150	843	0.865
-2	0	N/A	0	N/A	N/A
-1	1,800	1,180	2,120	874	0.828
0	-5,430	1,720	-4,830	950	0.764
1	-10,900	1,920	-10,800	1,070	0.957
2	-7,270	2,090	-8,770	1,090	0.524
3	-5,490	1,860	-7,620	1,270	0.343
4	-2,510	2,770	-6,500	1,300	0.191
5	-2,560	2,430	-7,100	1,410	0.105
6	2,950	3,030	-7,040	1,550	0.003

(a) Coefficients

	Observations		Records	
	Same	Different	Same	Different
Not Displaced	75,100	266,000	3,620	12,600
Displaced	3,670	9,140	188	458

(b) Observations

Appendix Table 4: Underlying Statistics for Figure 2

Note: This lists the underlying coefficients, standard errors of those coefficients, and p-values used to calculate the lines denoting statistically significant differences in Figure 2. For more details see the notes for that Figure.

	Same Neighborhood Coef	Same Neighborhood StdEr	Different Neighborhood Coef	Different Neighborhood StdEr	Difference p-value
-4	-0.024	0.016	-0.012	0.007	0.492
-3	-0.003	0.015	-0.028	0.018	0.282
-2	0	N/A	0	N/A	N/A
-1	0.012	0.007	0.001	0.001	0.109
0	0.018	0.008	0.001	0.001	0.029
1	-0.020	0.020	-0.053	0.021	0.263
2	-0.037	0.021	-0.049	0.016	0.664
3	-0.042	0.028	-0.069	0.024	0.465
4	-0.037	0.025	-0.055	0.023	0.595
5	-0.040	0.026	-0.059	0.027	0.598
6	0.003	0.018	-0.037	0.013	0.081

(a) Coefficients

	Observations Same	Observations Different	Records Same	Records Different
Not Displaced	75,100	266,000	3,620	12,600
Displaced	3,670	9,140	188	458

(b) Observations

Appendix Table 5: Underlying Statistics for Figure 3 Panel A

Note: This lists the underlying coefficients, standard errors of those coefficients, and *p*-values used to calculate the lines denoting statistically significant differences in Figure 3 Panel A. For more details see the notes for that Figure.

	Same Neighborhood Coef	StdEr	Different Neighborhood Coef	StdEr	Difference p-value
-4	-153	66	-1	39	0.048
-3	-145	70	-49	38	0.220
-2	0	N/A	0	N/A	N/A
-1	89	53	123	52	0.657
0	-314	71	-236	54	0.382
1	-430	94	-323	61	0.342
2	-257	89	-189	51	0.503
3	-125	76	-152	55	0.771
4	-224	80	-23	52	0.035
5	-114	70	-60	46	0.512
6	-63	56	-96	42	0.637

(a) Coefficients

	Observations		Records	
	Same	Different	Same	Different
Not Displaced	71,900	260,000	3,610	12,600
Displaced	3,460	8,760	188	458

(b) Observations

Appendix Table 6: Underlying Statistics for Figure 3 Panel B

Note: This lists the underlying coefficients, standard errors of those coefficients, and p-values used to calculate the lines denoting statistically significant differences in Figure 3 Panel B. For more details see the notes for that Figure.

	Same Neighborhood Coef	StdEr	Different Neighborhood Coef	StdEr	Difference p-value
-4	0.78	0.96	0.44	0.45	0.750
-3	0.29	0.87	-0.11	0.42	0.680
-2	0	N/A	0	N/A	N/A
-1	0.34	0.53	0.05	0.34	0.646
0	0.09	0.80	0.10	0.56	0.995
1	-1.12	0.84	-2.89	0.51	0.072
2	-1.16	1.08	-2.67	0.48	0.202
3	-0.53	0.82	-2.43	0.54	0.052
4	1.15	1.22	-2.66	0.58	0.005
5	-0.27	1.07	-2.74	0.59	0.043
6	1.62	1.13	-2.42	0.69	0.002

(a) Coefficients

	Observations		Records	
	Same	Different	Same	Different
Not Displaced	71,300	258,000	3,590	12,600
Displaced	3,440	8,720	188	458

(b) Observations

Appendix Table 7: Underlying Statistics for Figure 3 Panel C

Note: This lists the underlying coefficients, standard errors of those coefficients, and p-values used to calculate the lines denoting statistically significant differences in Figure 3 Panel C. For more details see the notes for that Figure.

	Same Neighborhood Coef	StdEr	Different Neighborhood Coef	StdEr	Difference p-value
-4	2.03	0.94	1.40	0.84	0.614
-3	1.10	0.87	2.26	1.17	0.427
-2	0	N/A	0	N/A	N/A
-1	-0.90	0.66	-1.58	0.54	0.420
0	5.83	1.38	5.27	1.24	0.761
1	7.28	1.64	6.63	1.15	0.747
2	4.07	1.54	3.78	1.22	0.883
3	1.39	1.06	2.97	1.25	0.332
4	0.88	0.99	2.52	1.28	0.306
5	1.56	1.13	0.69	0.83	0.535
6	0.33	0.69	1.63	0.76	0.216

(a) Coefficients

	Observations		Records	
	Same	Different	Same	Different
Not Displaced	61,000	219,000	3,520	12,300
Displaced	2,980	7,560	178	444

(b) Observations

Appendix Table 8: Underlying Statistics for Figure 4

Note: This lists the underlying coefficients, standard errors of those coefficients, and p-values used to calculate the lines denoting statistically significant differences in Figure 4. For more details see the notes for that Figure.

	Expensive					Inexpensive				
	Same		Different		Difference	Same		Different		Difference
	Coef	StdEr	Coef	StdEr	p	Coef	StdEr	Coef	StdEr	p
-4	-3,040	3,680	1,080	1,670	0.308	617	2,450	-218	1,080	0.755
-3	-2,650	3,670	-1,130	1,370	0.698	161	1,900	-1,220	1,050	0.526
-2	0	N/A	0	N/A	N/A	0	N/A	0	N/A	N/A
-1	1,930	2,080	1,540	1,550	0.880	1,590	1,220	2,630	960	0.505
0	-5,790	2,660	-4,630	1,240	0.692	-5,160	2,260	-4,970	1,390	0.943
1	-11,400	3,010	-11,800	1,490	0.905	-10,500	2,330	-9,880	1,510	0.830
2	-8,250	2,510	-9,850	1,790	0.603	-6,530	3,420	-7,800	1,320	0.727
3	-3,960	2,370	-10,900	1,900	0.022	-7,730	3,040	-4,970	1,630	0.423
4	2,540	4,370	-8,470	2,000	0.022	-8,460	2,850	-4,780	1,660	0.265
5	-334	3,750	-10,200	2,170	0.022	-5,820	2,870	-4,500	1,810	0.697
6	5,120	4,210	-8,150	2,380	0.006	-1,030	4,460	-5,680	1,990	0.340

(a) Coefficients

	Observations				Records			
	Expensive		Inexpensive		Expensive		Inexpensive	
	Same	Different	Same	Different	Same	Different	Same	Different
Not Displaced	30,600	134,000	44,500	131,000	1,470	6,510	2,160	6,130
Displaced	1,470	4,620	2,200	4,520	71	234	117	224

(b) Observations

Appendix Table 9: Underlying Statistics for Figure 5

Note: This lists the underlying coefficients, standard errors of those coefficients, and p-values used to calculate the lines denoting statistically significant differences in Figure 5. For more details see the notes for that Figure.

	Inflexible Occupation					Flexible Occupation				
	Same		Different		Difference	Same		Different		Difference
	Coef	StdEr	Coef	StdEr	p	Coef	StdEr	Coef	StdEr	p
-4	-1,160	7,470	2,900	2,760	0.609	-668	1,920	-48	954	0.772
-3	-5,330	6,750	-581	1,570	0.493	-33	1,540	-1,280	977	0.494
-2	0	N/A	0	N/A	N/A	0	N/A	0	N/A	N/A
-1	-904	3,960	907	2,210	0.690	2,580	930	2,480	949	0.945
0	-4,240	3,890	-7,590	2,180	0.452	-5,860	1,910	-4,080	1,050	0.416
1	-16,000	4,560	-13,600	2,180	0.633	-9,460	2,070	-9,940	1,220	0.840
2	-4,590	6,310	-11,500	2,860	0.315	-8,160	2,000	-7,900	1,140	0.910
3	-1,280	4,300	-11,200	3,170	0.065	-7,100	2,010	-6,520	1,370	0.813
4	16,300	6,710	-10,500	3,240	0.000	-7,820	2,070	-5,200	1,400	0.294
5	7,540	7,010	-11,500	3,720	0.017	-6,250	1,890	-5,670	1,480	0.810
6	5,030	8,010	-10,400	3,780	0.083	2,060	3,060	-5,610	1,650	0.028

(a) Coefficients

	Observations				Records			
	Inflexible		Flexible		Inflexible		Flexible	
	Same	Different	Same	Different	Same	Different	Same	Different
Not Displaced	14,900	91,300	60,100	175,000	693	4,210	2,930	8,430
Displaced	669	2,320	3,000	6,830	32	113	156	345

(b) Observations

Appendix Table 10: Underlying Statistics for Figure 6

Note: This lists the underlying coefficients, standard errors of those coefficients, and p-values used to calculate the lines denoting statistically significant differences in Figure 6. For more details see the notes for that Figure.

	Same Neighborhood		Same CZ		Different Neighborhood		Same and CZ
	Coef	StdEr	Coef	StdEr	Coef	StdEr	p-value
-4	-1,000	2,330	-574	1,270	1,380	1,390	0.872
-3	-1,530	2,140	-1,290	1,130	-991	1,210	0.922
-2	0	N/A	0	N/A	0	N/A	N/A
-1	1,820	1,180	1,680	971	2,460	1,360	0.927
0	-5,400	1,720	-3,400	1,520	-5,950	1,210	0.386
1	-10,800	1,920	-9,070	1,770	-12,100	1,310	0.499
2	-7,230	2,090	-7,430	1,610	-9,850	1,470	0.939
3	-5,440	1,860	-6,410	1,820	-8,570	1,740	0.710
4	-2,460	2,770	-5,000	2,020	-7,630	1,670	0.457
5	-2,490	2,430	-5,780	2,210	-8,140	1,800	0.318
6	3,010	3,020	-7,630	2,430	-6,400	1,940	0.006

(a) Coefficients

	Observations			Records		
	Same	CZ	Different	Same	CZ	Different
Not Displaced	75,100	106,000	160,000	3,620	5,310	7,330
Displaced	3,670	4,260	4,880	188	221	237

(b) Observations

Appendix Table 11: Underlying Statistics for Figure 7

Note: This lists the underlying coefficients, standard errors of those coefficients, and p-values used to calculate the lines denoting statistically significant differences in Figure 7. For more details see the notes for that Figure.

	Same Neighborhood		Different Neighborhood		Difference
	Coef	StdEr	Coef	StdEr	p-value
-4	443	2,570	1,400	1,320	0.738
-3	-702	1,220	305	1,030	0.525
-2	0	N/A	0	N/A	N/A
-1	145	1,270	775	818	0.677
0	-4,100	2,000	-4,700	1,070	0.790
1	-14,800	2,980	-14,000	1,570	0.818
2	-16,000	3,460	-10,400	1,390	0.130
3	-12,200	2,620	-8,560	1,730	0.248
4	-10,800	2,500	-7,800	1,730	0.329
5	-10,600	2,920	-7,910	1,770	0.436
6	-9,240	3,640	-6,580	1,760	0.511

(a) Coefficients

	Observations		Records	
	Same	Different	Same	Different
Not Displaced	86,800	545,000	3,610	23,300
Displaced	2,890	14,600	119	639

(b) Observations

Appendix Table 12: Underlying Statistics for Figure 8

Note: This lists the underlying coefficients, standard errors of those coefficients, and p-values used to calculate the lines denoting statistically significant differences in Figure 8. For more details see the notes for that Figure.

	Same Neighborhood Coef	StdEr	Different Neighborhood Coef	StdEr	Difference p-value
-4	-3,260	2,320	-886	1,350	0.376
-3	-1,330	1,660	-898	1,110	0.830
-2	0	N/A	0	N/A	N/A
-1	918	1,390	900	1,070	0.992
0	-6,710	2,220	-3,580	1,280	0.225
1	-10,000	2,600	-8,290	1,610	0.577
2	-5,820	3,190	-8,370	1,610	0.475
3	-4,970	2,410	-8,310	1,900	0.276
4	-5,290	2,460	-6,860	2,070	0.625
5	-4,800	2,680	-5,950	2,140	0.738
6	-1,100	2,790	-7,620	2,160	0.064

(a) Coefficients

	Observations		Records	
	Same	Different	Same	Different
Not Displaced	51,600	140,000	2,500	6,990
Displaced	2,240	4,510	114	241

(b) Observations

Appendix Table 13: Underlying Statistics for Figure 9

Note: This lists the underlying coefficients, standard errors of those coefficients, and p-values used to calculate the lines denoting statistically significant differences in Figure 9. For more details see the notes for that Figure.

	Same Neighborhood		Same CZ		Different Neighborhood		Same and CZ
	Coef	StdEr	Coef	StdEr	Coef	StdEr	p-value
-4	-166	103	-53	130	-34	130	0.504
-3	-82	116	-4	97	-98	100	0.608
-2	0	N/A	0	N/A	0	N/A	N/A
-1	-179	90	-125	106	-90	125	0.697
0	-148	127	-40	95	-96	120	0.481
1	-84	99	-162	112	-43	127	0.610
2	-207	125	-110	109	-53	134	0.557
3	-118	135	-141	105	-143	153	0.893
4	-179	183	47	120	-143	171	0.314
5	-194	143	-179	129	-150	187	0.937
6	-465	114	-45	110	80	184	0.006

(a) Coefficients

	Observations			Records		
	Same	CZ	Different	Same	CZ	Different
Not Displaced	16,300	32,000	18,800	1,480	2,800	1,660
Displaced	826	1,170	720	76	109	62

(b) Observations

Appendix Table 14: Underlying Statistics for Figure 10 Panel A

Note: This lists the underlying coefficients, standard errors of those coefficients, and p-values used to calculate the lines denoting statistically significant differences in Figure 10 Panel A. For more details see the notes for that Figure.

	Same Neighborhood		Same CZ		Different Neighborhood		Same and CZ
	Coef	StdEr	Coef	StdEr	Coef	StdEr	p-value
-4	-96	120	243	131	57	134	0.053
-3	-75	132	191	156	51	114	0.163
-2	0	N/A	0	N/A	0	N/A	N/A
-1	-15	97	-1	127	-195	231	0.924
0	-185	124	103	146	-143	189	0.142
1	-247	190	-11	167	-249	161	0.352
2	-127	200	11	204	-71	224	0.619
3	-256	198	-215	165	148	192	0.866
4	-196	243	-29	205	104	226	0.598
5	-151	203	6	210	43	215	0.611
6	-337	159	-32	188	147	208	0.224

(a) Coefficients

	Observations			Records		
	Same	CZ	Different	Same	CZ	Different
Not Displaced	14,800	27,600	18,200	1,220	2,270	1,510
Displaced	592	832	499	54	69	44

(b) Observations

Appendix Table 15: Underlying Statistics for Figure 10 Panel B

Note: This lists the underlying coefficients, standard errors of those coefficients, and p-values used to calculate the lines denoting statistically significant differences in Figure 10 Panel B. For more details see the notes for that Figure.

	Same Neighborhood Coef	Same Neighborhood StdEr	Different Neighborhood Coef	Different Neighborhood StdEr	Difference p-value
-4	-0.018	0.049	0.008	0.017	0.625
-3	0.001	0.046	0.018	0.029	0.751
-2	0	N/A	0	N/A	N/A
-1	-0.028	0.040	0.008	0.014	0.387
0	0.028	0.043	0.051	0.028	0.654
1	0.011	0.048	0.078	0.031	0.245
2	-0.008	0.045	0.019	0.013	0.565
3	-0.002	0.050	0.005	0.011	0.885
4	-0.018	0.046	0.028	0.015	0.347
5	-0.011	0.047	0.038	0.017	0.325
6	-0.025	0.040	0.010	0.022	0.445

(a) Coefficients

	Observations Same	Observations Different	Records Same	Records Different
Not Displaced	72,300	257,000	3,470	12,100
Displaced	3,120	8,210	161	407

(b) Observations

Appendix Table 16: Underlying Statistics for Figure 11 Panel A

Note: This lists the underlying coefficients, standard errors of those coefficients, and p-values used to calculate the lines denoting statistically significant differences in Figure 11 Panel A. For more details see the notes for that Figure.

	Same Neighborhood Coef	StdEr	Different Neighborhood Coef	StdEr	Difference p-value
-4	-49	93	103	65	0.181
-3	228	191	132	78	0.642
-2	0	N/A	0	N/A	N/A
-1	40	146	-28	52	0.660
0	234	196	59	69	0.399
1	-30	186	193	193	0.408
2	-52	180	-29	56	0.900
3	-23	165	69	54	0.596
4	-153	160	96	64	0.150
5	-44	144	172	94	0.214
6	-183	136	130	68	0.040

(a) Coefficients

	Observations		Records	
	Same	Different	Same	Different
Not Displaced	49,700	186,000	2,780	10,300
Displaced	1,910	5,470	119	318

(b) Observations

Appendix Table 17: Underlying Statistics for Figure 11 Panel B

Note: This lists the underlying coefficients, standard errors of those coefficients, and p-values used to calculate the lines denoting statistically significant differences in Figure 11 Panel B. For more details see the notes for that Figure.

	Same Neighborhood Coef	Same Neighborhood StdEr	Different Neighborhood Coef	Different Neighborhood StdEr	Difference p-value
-4	0.074	0.038	-0.034	0.030	0.026
-3	0.017	0.041	0.021	0.030	0.940
-2	0	N/A	0	N/A	N/A
-1	0.065	0.042	0.022	0.021	0.365
0	0.035	0.064	-0.024	0.047	0.467
1	0.014	0.069	0.025	0.038	0.888
2	0.108	0.071	-0.023	0.032	0.098
3	0.127	0.072	0.013	0.037	0.161
4	0.114	0.077	0.021	0.035	0.277
5	0.045	0.072	0.043	0.043	0.973
6	0.021	0.059	0.076	0.037	0.426

(a) Coefficients

	Observations Same	Observations Different	Records Same	Records Different
Not Displaced	41,300	131,000	2,670	7,800
Displaced	1,070	3,090	68	191

(b) Observations

Appendix Table 18: Underlying Statistics for Figure 12

Note: This lists the underlying coefficients, standard errors of those coefficients, and p-values used to calculate the lines denoting statistically significant differences in Figure 12. For more details see the notes for that Figure.