Parental Proximity and Earnings After Job Displacements*

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Abstract

Young adults, ages 25 to 35, who live in the same neighborhoods as their parents experience stronger earnings recoveries after a job displacement than those who live farther away. This result is driven by smaller on-impact wage reductions and sharper recoveries in both hours and wages. We show that geographic mobility, different job search durations, housing transfers, and *ex-ante* differences between individuals are unlikely explanations. Our findings are consistent with a framework where some individuals living near their parents face a better wage-offer distribution, though we find no direct evidence of parental network effects.

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1 Introduction

Americans typically live very close to their parents. In the Panel Study of Income Dynamics (PSID, 2017), the median household head, ages 25 to 35, lives just over five miles from their parents and about one-fifth of these individuals live in the same neighborhoods as their parents.[1,2] In modeling location choice, economists often explain this proximity to parents by referencing the amenity value of being close to home. Kennan and Walker (2011), for example, describe young men's utility gain from living in the state where they grew up as equivalent to a $20,000 wage increase per year.[3] We find that this value of being close to home is partially explained by the labor market benefits that parents afford their young adult children.

Our focus on the labor market is motivated by two literatures. First, Wilson (1987) and Munshi and Rosenzweig (2016) show how parents can provide informal social insurance, and Kaplan (2012) links this insurance to the labor market outcomes of young adults. Second, Granovetter (1995) and Ioannides and Loury (2004) present evidence that the use of friends and relatives is prevalent and productive during the job search process, potentially improving the match quality between workers and firms. More specifically, Corak and Piraino (2011) and Kramarz and Skans (2014) show how parents can use their professional networks to help ease children's entry into the workforce.

To quantify these parental benefits, we focus on displaced workers, individuals who involuntarily lose their stable jobs through no fault of their own (e.g. being laid off).[4] We focus on job displacements because of their plausible exogeneity (von Wachter, Song and Manchester, 2009) and because these events are followed by large and persistent earnings losses, on average. We follow the standard difference-in-difference methodology of Jacobson, LaLonde and Sullivan (1993) to document the earnings losses of displaced workers in the PSID, but we analyze separately these losses by workers who lived in and out of their parents' neighborhoods at the time of displacement.

[1] As in Hellerstein, Kutzbach and Neumark (2015), for example, we use Census tracts as a measure of neighborhoods. For more details see the Census' definition at `https://www.census.gov/geo/reference/gtc/gtc_ct.html`.

[2] 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.

[3] Other migration models with many locations (Bishop, 2008; Gemici, 2011; Coate, 2017) find similar magnitudes in the US using slightly different combinations of datasets and definitions of home location.

[4] See Jacobson, LaLonde and Sullivan (1993) and Stevens (1997). Early literature reviews include Hamermesh (1989), Fallick (1996), and Kletzer (1998). More recent work includes von Wachter, Song and Manchester (2009) and Davis and von Wachter (2011).

We show that young adults, ages 25 to 35, who lived in their parents' neighborhoods prior to a displacement event experience a remarkably strong earnings recovery, catching up to a control group of non-displaced workers five years after the displacement event. Those living farther from their parents experience a large, permanent decline in earnings, similar in magnitude to the losses found by previous researchers. Stronger earnings recoveries for young adults living closer to their parents are driven by smaller on-impact wage reductions and stronger post-displacement recoveries in both hours and wages. We find no benefit from parental proximity after job loss for older workers.

The earnings results for young workers do not seem to be driven by observed differences between workers or different migration responses. We rule out *ex-ante* observable differences using a propensity score reweighting exercise. This exercise creates a group of workers who live farther from their parents, but lose similar jobs and have similar characteristics to those living in their parents' neighborhoods.[5] We also find similar earnings differentials when we restrict to a subsample of workers who do not migrate after the displacement event.

We present three facts that highlight mechanisms besides parental housing transfers acting as insurance after job losses, a channel that Kaplan (2012) emphasizes. First, we find that the benefit of parental proximity extends to people who live in the same census tract as their parents, but not coresiding.[6] Second, hours worked and unemployment durations at the time of displacement do not differ for those living in their parents' neighborhoods and those living farther away. Third, although we do find some support for increases in housing transfers around displacement, these increases are small and they do not appear to move differentially for the two groups.

We find no direct evidence that being nearby causes children to accept jobs from their parents' networks, but a simple theory, combined with our propensity score reweighting, does suggest that children receive better wage offers when they live closer to their parents. Young workers living close to their parents are not employed in their parents' industries at greater rates than workers living farther away, controlling for local industrial composition. A simple framework where some individuals face a better wage-offer distribution near their parents is consistent with our evidence about smaller on-impact wage reductions and faster earnings recoveries. In contrast, the framework suggests that selection due to heterogeneity

[5] We also believe it is unlikely that unobserved heterogeneity can explain the result; if anything, those away from parents should be better able to adapt to new circumstances and be more resilient after displacement.

[6] We cannot reject the hypothesis that the earnings recovery of workers coresiding with their parents prior to displacement is the same as those living in their parents' neighborhoods but not coresiding. We show, however, that for individuals living outside of the tract – in the same commuting zone and beyond – the benefit dissipates.

in the preference for living at home would work against our earnings results. Finally, if our earnings results were driven by people moving away for particularly good wages, then the theory shows that our propensity score reweighting exercise will remove any differences in earnings after displacement.

The rest of the paper proceeds as follows. Section 2 describes our data, describes our sample, and presents our main earnings results using simple averages. Section 3 presents these results with a more sophisticated econometric setup, decomposes them into hours and wages, and presents additional results by age, proximity to parents, and geographic mobility. Section 4 shows that the earnings results are robust to reweighting and interaction methods that account for the sample differences between those displaced near or far from parents. Section 5 investigates possible mechanisms related to parental employment networks and the role of housing transfers. Section 6 discusses selection and unobserved heterogeneity in the context of our results and, based on our empirical estimates, presents some back-of-the-envelope calculations relating to the value of living close to one's parents in the face of job displacement risk. Section 7 discusses broader implications of our work and avenues for future research.

2 Analysis Data and Sample Averages

2.1 Dataset and Sample Construction

In choosing the appropriate dataset for our analysis, we are confined by three major restrictions: 1) The data need to include inter-generational linkages; 2) the data need to include job history information to identify worker displacement events and to measure labor earnings; and 3) the data need to provide (preferably many) repeated observation on individuals to implement a difference-in-difference approach. To our knowledge, the PSID is one of few datasets that meets all three requirements.[7] The PSID began in 1968 with an interview of approximately 5,000 families, and follows any new families formed from the original group. We use the 1968 to 2013 waves of the PSID and we employ both the Survey Research Center (SRC) and Survey of Economic Opportunity (SEO) samples with longitudinal weights. Our conclusions, however, are unchanged when we drop the SEO sample and do not use weights. We include observations on individuals between the ages of 18 and 62 and we restrict our attention to what the PSID refers to as "household heads," persons who are at least 16 years

[7] The National Longitudinal Survey of Youth 1997 also meets these requirements, but the intergenerational aspect of the PSID is much stronger. The PSID collects complete and separate respondent observations for parent and child generations at any time they live in separate households over the entire panel.

old with the most financial responsibility for the family unit. The PSID generally defines this as the male in a husband-wife pair or an unmarried couple who has been co-residing for at least one year. Our results are similar when we include both heads and wives (Appendix A).

Due to the genealogical nature of the PSID we have the location of adult children and their parents in each wave if they choose to respond. At the time of the survey, the PSID also collects information about an individual's labor market experience, including their earnings during the previous calendar year.

Job displacements are determined from a question that asks respondents who have less than a year of tenure with their present employer: "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.[8]

The dataset used for analysis is constructed in the following way. For a given age (the "base age") we include heads that were displaced between the date of their last survey and their current survey and heads that were not displaced. This is the "treatment" and "control" group for this base age. We include heads who were and were not living in the same neighborhoods as their parents at the time of the previous interview.[9] We repeat this procedure for every base age between 25 and 55 and stack all the samples to create the final dataset.[10] To track when workers are displaced or not, let the *relative year* be zero in the base age, one in the year after, etc. For example, for the base age 40, the relative year is -8 when individuals are 32, zero when individuals are 40, and 6 when individuals are 46.[11]

[8]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, LaLonde and Sullivan, 1993, for example) causes small sample sizes in our context.

[9]Because of the genealogical nature of the PSID data, we typically observe the parents of single adults or of one set of parents of a married couple. We treat cases in which we have the location of the husband or wife's parents symmetrically, although sometimes this means we are using the household head's parents and sometimes parents-in-law. In some cases, we will observe multiple parents' locations (typically due to divorce of an original PSID household head); in these cases an adult child is coded as same neighborhood if they live in the same Census tract as any parent or in-law.

[10]Note that individuals may appear more than once in the final dataset because they may be in the control group several times, or in the treatment group at one base age, but in the control group at another base age, etc. In our results, we will cluster standard errors at the individual level to account for these multiple observations. We use earnings information from ages 18 to 62, but displacement events only from ages 25 to 55 to avoid capturing individuals too early in their career and too close to retirement.

[11]Due to the survey design of the PSID, the location of household heads is only observed if they have

Table 1 shows the summary statistics for the final sample, which we restrict to observations with non-missing parents' location information.[12] Since we are using the PSID's poverty (SEO) oversample, and hence sampling is likely endogenous to our outcome (earnings), we take the suggestion of Solon, Haider and Wooldridge (2015) and use the longitudinal weights provided by the PSID throughout the analysis. The dataset consists of around 50,000 records, with an average of 20 years of observations for each, yielding roughly 1,000,000 person-year observations. The final dataset contains about 1,650 displacement events, of which approximately 350 took place while an individual resided in their parents' neighborhoods and approximately 1,300 occurred while an individual was not in their parents' neighborhoods. The average annual displacement probability is around three percent in our sample.[13] Displaced workers are slightly younger, less educated, and have been with their employer for a shorter period of time in relative year -1 than their non-displaced counterparts. They also earn significantly less. Those who live outside of their parents' neighborhoods tend to be older, more educated, and earn significantly more than those who live in their parents' neighborhoods. Around 16 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.

2.2 Some Preliminary Evidence

Figure 1 plots the average earnings before and after displacements. The top panel in Figure 1 presents the average earnings of workers who were displaced (dashed) and not displaced (solid) averaged over the base ages 25 to 35. All earnings are measured in 2007 dollars (CPI-U-X1). These lines highlight the dramatic earnings consequences of worker displacement. The figure delivers three messages, which have been documented in many prior studies. First, displacement leads to a large initial drop in annual earnings of around \$10,000, which is around 20 percent of pre-displacement earnings.[14] Second, while earnings for these displaced

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 significantly mitigates this sample selection issue.

[12] The most common reason we have missing parents' location is that the parents are deceased.

[13] This is consistent with displacements rates in previous work. See, for example, Davis and von Wachter (2011) where it is between three and four percent and Kuhn (2002), where it is between four and five percent.

[14] The earnings question 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

individuals recover, this recovery does not exceed the earnings gains experienced by the control group of non-displaced workers. As a result, although around six years after the displacement event earnings have recovered to their pre-displacement levels, even 10 years after the displacement event the earnings of displaced workers have not caught up with the earnings of non-displaced workers. Finally, although there is a difference in the level of earnings between the average earnings of those in the control group, which we will address in the more thorough empirical exercises that follow, there do not appear to be differences in the trends of earnings prior to the displacement event.

The bottom panel of Figure 1 decomposes the average earnings into those that were in their parents' neighborhoods in relative year -1 (light gray), and those that were not in their parents' neighborhoods (dark gray). Many people in our sample aged 25 to 35 are not in their parents' neighborhoods, so the average earnings of those individuals (displaced or not) is close to the average earnings presented in the top panel of the figure. Before describing the effects of displacement for these two groups, it is worth pointing out that individuals that live in their parents' neighborhoods have significantly lower earnings than people who live farther from their parents, even before the displacement event.

The bottom panel of Figure 1 shows that displaced individuals that were not in the same neighborhoods as their parents see large earnings losses relative to a group of individuals that were not displaced and not in the same neighborhoods. This gap persists over the next 10 years. In stark contrast, those individuals that were in the same neighborhoods as their parents in the year prior to the displacement event, see a much healthier earnings' recovery. Prior to the displacement event the difference in the earnings of the displaced and non-displaced who live in their parents' neighborhoods is around \$3,000 and the earnings of the displaced individuals recover to this pre-displacement difference around six years after the displacement event. The gap in earnings between these displaced workers and the non-displaced group closes entirely within nine years of the displacement event. Appendix Figure 1 presents a similar figure for the natural logarithm of earnings with the same conclusions.

In the next two sections we verify these results with a standard displaced worker specification (Section 3), which controls for, among other things, individual fixed effects, and a propensity score reweighting exercise (Section 4), which controls for observable differences between those in their parent's neighborhood and those farther away. The preliminary results presented in this section are remarkably robust to these more sophisticated methods.

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'.

3 Regression Results

3.1 Earnings Losses by Geographic Proximity to Parents

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

$$e_{iat} = \alpha_{ia} + \gamma_t + X_{iat}(\beta^H + \beta^A H_{ia}) + \sum_{k=-4}^{10^+} \left(D^k_{iat}\delta^k + D^k_{iat} H_{ia}\zeta^k\right) + \epsilon_{iat} \quad (1)$$

Here e_{iat} is the annual earnings of individual i in calendar year t when the base age is a, α_{ia} represent individual-base-age dummies, γ_t control for calender-time fixed effects, X_{iat} control for an age quartic, and H_{ia} is a dummy variable indicating whether individual 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 individual i at time period t and base age a was displaced k periods ago. We pool the -4 dummy and the $+10$ dummy and omit the -2 dummy so all results are relative to two years before the displacement event. As a result, the coefficient δ^k captures the change in earnings for an individual 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.[15] The coefficient ζ^k picks up the additional effect of being neighbors with your parents on the earnings outcomes of displaced workers. This approach does not consider h owother factors, correlated with living in the same neighborhood as one's parents, might explain the differential impact of displacement on earnings. The propensity score reweighting in Section 4 addresses these concerns.

Figure 2 presents the effect of displacement on earnings for workers farther away from their family, $\hat{\delta}_k$, and the effect of displacement for individuals living in the same neighborhoods as their parents, $\hat{\delta}_k + \hat{\zeta}_k$, for workers experiencing a displacement between ages 25 to 35. These results tell the same story as the simple averages presented in Figure 1. At the time of displacement, workers experience large declines in earnings; around \$10,000 for those living in their parents' neighborhoods and around \$15,000 for those living farther away. With the average pre-displacement earnings of these groups being around \$35,000 and \$45,000,

[15] This approach is mostly closely related to the approaches taken by Davis and von Wachter (2011) and Huttunen, Møen and Salvanes (2016). See Krolikowski (2017*a*) for a more thorough discussion of choosing a control group for displaced workers.

respectively, this represents a 30 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 around 25 percent even 10 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 results are similar if one drops observations that have zero annual earnings or if one uses the log of annual earnings on the left hand side in equation (1) as opposed to the level of earnings (Appendix Figures 2 and 3, respectively).

3.2 Employment, Hours, Wages, and Unemployment Duration

Figure 3 presents the results from estimating equation (1) with three different outcomes: an indicator for whether the individual 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. The top panel shows the probability of positive hours last year. This falls during the survey after the displacement event, as some individuals experience an entire year out of work. The graph suggests that displaced individuals, regardless of location, are around 4 percentage points (pp) less likely to have employment in the year after the displacement event than non-displaced individuals. Although the recovery seems a little stronger a few years after the displacement for those living near their parents, it is difficult to tell the two groups apart with the large standard errors. As such, the two groups seem to have similar post-displacement employment outcomes.

The middle panel of Figure 3 shows the results from estimating equation (1) with the hours worked last calendar year as the outcome, where we condition on positive hours. On-impact the reduction in hours for the two groups is similar, around 350 hours (approximately 18 percent of the 2,000 hours prior to displacement). Although those near their parents see a larger fall in hours upon displacement, this difference is not statistically significant. The recovery in hours, however, appears stronger for those living in their parents' neighborhoods. In particular, from two to ten 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.

The bottom panel of Figure 3 shows how hourly earnings, conditional on positive hours, move around displacement. At the time of displacement, those living in their parents' neighborhoods experience a significantly smaller wage reduction (around \$1.50/hr) than those living farther away (around \$4/hr). Moreover, those individuals who lived in the same neighborhoods as their parents at the time of displacement see their wages recover fully, and those individuals that lived farther away see no recovery. In Section 4 we use propensity score reweighting to account for observable differences between the two groups, including pre-displacement wages. The results presented in Figure 3 are qualitatively similar.

Although the intensive and extensive margins plotted in Figure 3 suggest that the two groups of workers were unemployed for similar amounts of time, 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 around 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 very similar. We see these duration results as direct evidence that longer job search is unlikely to be an important explanation for the differing post-displacement earnings outcomes of the two groups.

3.3 Earnings Results by Age, Proximity, and Geographic Mobility

In this section we document three additional facts that are particularly relevant. In Section 5 we investigate possible mechanisms for our earnings results.

Figure 5 shows the results of estimating equation (1) for older workers, ages 36 to 55. As with the earnings outcomes for the young, the earnings losses associated with displacement are large and persistent. This figure, however, suggests that for older workers, living in their parents' neighborhoods does not help post-displacement labor outcomes in the same way that it assists younger workers. If anything, it might be detrimental, but we cannot reject the null hypothesis that the post-displacement earnings effects are the same for the two groups. The discrepancy between the results for the young and the old likely reflects a change in the direction of resource flows, since older adults often live close to their parents in order to care for them in their old age. For example, Chari et al. (2015) estimate the opportunity cost of informal elder care in the US at \$522 billion annually and Lin and Wu (2010) find that among individuals 65 and older who had difficulties with instrumental activities of daily living, about 35% report that a child is a source of informal support. Lin and Rogerson (1995) provide a more general discussion about the "determinants of the distance between elderly parents and their adult children."

Figure 6 shows the results of estimating equation (1), where 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.[16] This figure suggests that those living close to their parents, but not in the same neighborhoods, experience similar post-displacement earnings outcomes than those who live farther away. In fact, the recoveries for the two groups are remarkably similar. We conclude that it is very close proximity that matters.

As in Huttunen, Møen and Salvanes (2016) and Cao and Stafford (2017), we document a large impact of job displacement on regional mobility (Appendix Figure 4 shows that switching labor markets increases sharply at the time of displacement).[17] To abstract from mobility, we check that the post-displacement earnings trajectories are not driven by "movers" by restricting the sample to individuals who do not change geographic location after the displacement event. For this analysis we take the county as the relevant measure of geography because restricting to young individuals who never tracts after a displacement event reduces the sample sizes dramatically.

Figure 7 presents the results with this restricted sample together with the original results from Figure 2. Perhaps not surprisingly, the earnings outcomes of the sample that are restricted to no mobility after a displacement event are almost always worse than for the unrestricted sample. However, the differences between those who, prior to the displacement event, resided close to their parents and farther away are equally pronounced for this restricted sample. Therefore, post-displacement mobility patterns are unlikely to account for our baseline earnings results.

We present several additional findings in Appendix A. The baseline earnings results are robust to not using PSID sample weights, to including controls for local labor market conditions, to a distance-based measure of proximity (based on latitudes and longitudes of block groups), and are similar for men and women.[18] The results are also similar when we

[16]We augment equation (1) by including an interaction of the age quartic with an indicator for whether an individual is in the same commuting zone, but not the same tract, and for whether an individual is in the same tract as their parents. Additionally, we interact the displacement dummies with whether an individual lives in the same commuting zone as their parents and the same tract as their parents. This approach allows for mutually exclusive age quartics for the three different groups (same tract, same commuting zone but not same tract, and different commuting zone) while testing for a distinct effect of displacement for those in the same tract as opposed to just the same commuting zone as their parents.

[17]See Mincer (1978) for early work on family ties and migration decisions. Molloy, Smith and Wozniak (2011) and Kaplan and Schulhofer-Wohl (2017) provide a more extensive analysis of recent trends in migration, including declining inter-state migration at different geographies.

[18]Local labor market conditions include county-level employment-to-population ratios and unemployment rates. 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,

look at individuals 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). This finding suggests that the effect of parental proximity is not limited to transfers while coresiding, a topic we discuss further in Section 5. 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 individual was displaced while living in the county that they grew up in. These results suggest that parental proximity has an independent effect on post-displacement earnings from other factors in an individual's home county.

4 Propensity Score Reweighting

4.1 Methodology

Displacements have smaller effects on workers who live closer to their parents. People who live closer to their parents also have less formal education, have lower incomes, and are less likely to be in managerial and professional occupations. To see if observable differences in jobs or observable worker characteristics drive our earnings effects, we use propensity score reweighting to control for these factors. In particular, we use weights so that the observable characteristics of all workers match those of workers who were displaced while living close to their parents. As such, if displaced workers who live farther from their parents are more educated and have jobs with higher wages than displaced workers close to their parents, then the reweighting would address this by emphasizing workers who were away from their parents with less education and in jobs with lower wages. The difference between the various groups in our plots of average (reweighted) earnings around displacement can be interpreted as an effect of the treatment on the treated, where the treatment is being close to one's parents before a job displacement. This links our analysis directly to the literature on propensity score reweighting (Rosenbaum and Rubin, 1983; Hirano, Imbens and Ridder, 2003), but with the slight complication that we examine multiple treatment arms, as in Imbens (2000).[19]

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

where we linearly interpolate between census years. The former data are available from 1969 onwards. Using county-level unemployment rates from the Local Area Unemployment Statistics (LAUS) program delivers a similar conclusion but those data are only available after 1980, substantially reducing our sample.

[19]Unobserved differences could affect our results, but they would have to have two properties. First, they would have to be time varying around a displacement in a relevant way. Second, the numerous variables that we control for would have to be poor proxies for these unobserved differences.

N), $j_{ia} \in \{HD, AD, HN, AD\}$, using the following formula:

$$W_{ia} = \frac{\mathrm{P}(j_{ia} = HD | X_{ia})}{\mathrm{P}(j_{ia} = HD)} \frac{\mathrm{P}(j_{ia})}{\mathrm{P}(j_{ia} | X_{ia})} \tag{2}$$

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

The unconditional probabilities in equation (2) are the proportion of the sample made up by the group, as suggested by Fortin, Lemieux and Firpo (2011). Empirically, we estimate probabilities conditional on X_{ia} using a multinomial logit regression, as suggested by Imbens (2000). The predictors in the multinomial logit regression are a quadratic term and level changes in income, the level of wages, a college dummy and a linear term for the number of years of completed education, dummies for one digit occupations, a linear term for job tenure, a linear term in age, a dummy for gender, and a dummy for race (black or not). The regression is unweighted and all of the controls are the average values of the variables in the three years leading up to the event (ignoring years where they are not observed).

Table 2 shows a validation of the weights using several of the covariates in X as well as some other variables that were not included in the reweighting. It reports the means, standard deviations, 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 Census tract 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 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, and a similar likelihood of having children. We also verify in Appendix B that for non-displaced individuals close to their parents and farther away, average earnings, by relative year, are similar. This serves as further evidence that the reweighting scheme works as intended.[20]

[20] The reweighting exercise is similar to allowing several interaction terms in the regression specification. For comparison we present results from such an interacted model separately in Appendix C. In Appendix D, we implement the same propensity score 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) with similar results.

4.2 Earnings Results Using Propensity Score Reweighting

We begin by showing the effects of reweighting in terms of the simple means that we began with in Section 2.2. Figure 8 shows means of earnings around displacement for people living in the same tract as their parents and reweighted means for people farther away. The two groups have similar earnings before displacement due to the weights, but they also track each other well during the initial decline in relative years zero and one, which was not by design. A few years following the displacement event, the earnings of those who were closer to their parents begin to out-pace the earnings of people who were farther away. In the final years this difference is large, around 10 thousand dollars, and statistically significant at the five percent level.

Figure 9 shows the baseline regression specification (equation 1), but with the new weights.[21] It confirms that the inverse probability reweighting procedure provides similar qualitative results to the main specification. As before, there are substantial drops in earnings following a displacement, though the initial losses are both about $10,000. These results show a more steady divergence between the earnings of those living near their parents and farther away, a divergence that strengthens around five years after the displacement event. People living closer to their parents see no detectable earnings losses in years four through ten, with positive point estimates at longer horizons. People who live farther away from their parents do have detectable earnings losses throughout the post-displacement period. These losses are more modest than the baseline regressions, but they are still substantial, about six thousand dollars, or 10 percent of initial earnings.

The robustness exercises lend credence to the idea that parents play a causal role after job displacement. After controlling for several relevant variables, we find that our baseline results are qualitatively unchanged. The similarity across the many specifications suggests that we have not omitted variables that are important for the recovery process and that the effect of being displaced is similar across people with these different observable characteristics.

5 Investigating Mechanisms

We have documented a robust positive relationship between parental proximity and post-displacement earnings outcomes. In this section we investigate housing transfers and parental

[21] We continue to use clustered standard errors, which address heteroskedasticity induced by the weighting, along with mechanical correlations across observations. Goldschmidt and Schmieder (2015) also use a similar approach in a relatively similar context, making many of the same choices. The main difference with their approach is we use reweighting, while they rely on nearest neighbor matching.

networks to see if they might be responsible for this relationship. After displacements, we find that only a relatively small amount of money is transfered through gifts of in kind housing, and we find no direct evidence that parental networks are leading to jobs post displacement. In Appendix E we investigate whether search intensity and industry switching can account for our baseline earnings results, but find that these mechanisms are also unlikely to explain our baseline findings.

5.1 Housing Transfers

In this section we attempt to measure the cash value of these in kind transfers of housing, and assess the extent that they change around displacements. We find that these transfers are relatively small, that they increase by modest amounts after job displacements, and we find no evidence that they increase by more for people living closer to their parents.

We use two complimentary approaches to measure housing transfers, which may be under reported in surveys like the PSID. The first uses the answer to a question, asked only of households who reported paying no rent, if they received their rent as a gift.[22] Since this question is only asked of people who report paying no rent, it will miss people who pay a below market rent to live with their parents. To address the possibility that some households pay below market rents to live with their parents, we construct an estimate of how much the child saves by living in their parents' household.

We can back out how much a child saves by living with their parents in situations where a child moves in with a parent who is also a respondent in the PSID. In situations where a respondent child moves in with a parent, the PSID classifies the household as two different family units living within a single housing unit. In these situations, the surveyor will assign everyone living in the housing unit to a family unit and then conduct separate interviews, including questions about housing, with each family unit. The information about total housing costs, combined with the composition of the household allows us 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.[23]

[22]For the purposes of this exercise, we include households who report having neither owner nor rented and who then go on to say that they live rent free because of a gift, inheritance, or some other non-work related reason.

[23]In situations where the home is owned, we convert this housing value into a rental value, using the rough conversion factor of 0.0785 (also used in Albouy and Zabek, 2016). We provide a more detailed description

Table 5 reports the proportion of households in our baseline sample of adults ages 25 to 35 who receive housing transfers and the average value of these transfers among households who receive them. We report measures from the survey question about receiving rent as a gift as well as measures of how often children live with their parent, and the implied rent savings, according to our procedure.

Table 5 shows that a relatively small proportion of 25 to 35 year olds receive transfers of rent, and that these transfers are modest relative to both average rents and the earnings losses after a displacement. According to both measures, less than ten percent of the sample is likely to receive a transfer of housing at the date of the survey. Eight percent of the sample live with a parent and around two percent receive all of their rent as a gift. Among households who receive a transfer, the average transfer was around \$4,300 according to the implied savings, and around \$2,500 according to the survey question. Each is smaller than the average rent of around \$6,800 and the estimated earnings losses of around \$15,000 in the year after a displacement.[24]

Figure 10 shows that these measures of housing transfers do seem to spike around displacements, at least according to some measures. The spikes around displacement are relatively small in terms of dollar values, and they do not seem to affect people who initially lived close to their parents any more than people who lived farther away.

The main evidence that housing transfers are important around displacement comes from examining extensive margin changes, shown in Panel A of Figure 10. Around a displacement, households are around four percentage points more likely to receive their rent entirely as a gift. This is quite large, compared to the roughly two percent likelihood in the overall sample. At the same time, it still is quite unusual for a child to receive all of their rent as a gift after a displacement.

Though Panel A shows an effect, Panel B shows that the actual dollar values of the transfers involved are modest. 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. Compared with a \$15,000 decrease in earnings, this is quite small indeed.

Housing transfers appear to be fairly modest, and it does not appear that they increase

of the procedure in Appendix F.

[24]This difference between the value according to the two estimates 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.

dramatically around a displacement. It also does not appear to be the case that they increase by more for people who lived closer to their parents before losing their jobs.[25] This finding casts some doubt on the hypothesis that housing transfers are the primary mechanism driving our results, though we cannot rule out the possibility that other factors could amplify the importance of housing transfers.[26]

5.2 Employment in Parents' Industry

Young adults living close to their parents may have more productive job search experiences and healthier earnings post displacement as a result of family networks, as documented in Kramarz and Skans (2014). The basic idea is that, after job loss, parents may be able to assist their adult children by tapping into their own employment networks to help their adult children to gain employment in their own industry.[27] We can look for direct evidence for this mechanism using PSID data because we have industry codes for all workers, including parents and their adult children. Although we would like to look at the response of being employed in a parents' industry around a displacement event for our two groups of young workers, the PSID data are too thin to pursue this analysis. Instead, we attempt to document that individuals living close to their parents are more likely to be employed in their parents' industries than those living farther away, but find little support for this channel.

Table 3 presents some summary statistics on the probability of individuals' working in their parents' industries. The table reports the fraction of individuals work in the same one-digit industry as their parents. When unemployed, the industry of an individual is the industry they were last employed in. Panel A suggests that, on average, young individuals living in the same tract as their parents are slightly more likely to be working in their parents' industry than those living farther away; this correlation is driven by both unemployed and employed workers. The last two columns of Panel B suggest that older workers who live in their parents' tract are less likely to be employed in their parents' industry, but this is driven

[25]Results for reported transfers of money, presented in Appendix F also 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.

[26]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. We could also be missing frequent movements of people in and out of their parent's homes since we measure housing only at the time of the survey each year.

[27]There is some debate about how referral networks affect workers' wages. For example, Dustmann et al. (2017) find that hires from employment networks raise wages, while Bentolila, Micehacci and Suarez (2010) suggest that networks may reduce wages because they might assign individuals to jobs in which they do not have comparative advantage. Alesina et al. (2015) (p.599) find that "individuals who inherit stronger family ties are less mobile, have lower wages and higher unemployment..."

by employed workers; unemployed older adults who live in their parents' tract are slightly more likely to be employed in their parents' industry. Comparing the two panels reveals that younger adults are more likely to be working in their parents' industry than older adults.

To assess these correlations holding observables constant, we estimate the following equation, where the outcome variable is an indicator of whether the young adult child is employed in the same one-digit industry as their parent:

$$Same\_industry_{it} = \alpha + \beta_1 Sametract_{it} + \gamma X_{it} + \epsilon_{it}$$

The main coefficient of interest, β_1, measures how the probability of being employed in the same one-digit industry as a parent correlates with living in the same tract as them. As controls we include a host of demographic characteristics as well as the share of employment in the parent's industry at the county level from County Business Patterns (CBP) data.

Table 4 presents the results of this analysis by worker age and proximity to parents, where we have pooled the unemployed and employed. The first column reproduces the average difference in the probability of being in the same industry as one's parents between young adults living in their parents' tract and those living farther away that we documented in Table 3. The regression shows that this positive effect of parental proximity on being in the same industry as one's parents is not statistically significant. Columns (2) and (3) add increasing number of controls, including demographic controls, such as age, race, education, and industry employment shares in an individual's county from CBP data, year fixed effects, and individual fixed effects. In the specification that controls for time invariant worker characteristics, the coefficient on living in the same tract as one's parents is positive but not statistically significant. The point estimate suggests that living close to one's parents raises the probability of being employed in their industry by 1.4 pp. The average probability in the young worker sample of being in the industry of one's parents is around 25 percent.

Column (4) of Table 4 shows the same specification estimated using older workers, ages 36 to 55. The effect of same tract on being in the same industry as one's parents is negative for older workers, and statistically significant at the 10 percent level. Column (5) estimates the same equation as in column (3) but uses an indicator for being in the same state (as well as in the same tract) as one's parents to measure parental proximity. The coefficient on this measure of geographic proximity turns out to be positive, and larger in magnitude than the coefficient on the same tract in column (3).

Taken as a whole, these results suggest no robust evidence that young workers living close to their parents are more likely to be employed in their parents' industry.[28]

6 Discussion

In this section, we use a simple model to show that our findings are consistent with some individuals facing a better wage-offer distribution near their parents. Back-of-the-envelope calculations suggest that this better wage-offer distribution, at least after a displacement, implies an annual value of between \$1,000 to \$3,000 for a risk-neutral agent living in the same neighborhood as one's parents.

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.

In this simple setup the average wages of those away are higher than those at home because those moving away need to be compensated for leaving behind the positive benefit, b. 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 earnings on average. Our reweighting exercise removes this selection effect because it only uses individuals living farther away from their parents who have similar pre-displacement wages to those living close to their parents.

It is easy to see that in this environment, without individual heterogeneity, the post-displacement earnings outcomes will be identical for individuals living at home and away because these individuals face exactly the same wage distributions. The next two examples consider cases with individual heterogeneity.

First, suppose that individuals differ in their preference for living at home. In particular, suppose that some individuals ("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 individuals away are more likely to be explorers they will, on average, have better wage outcomes

[28]We also do not find evidence that working in the same industry as one's parents rises during job displacement.

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 individuals 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, an individual 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 individuals away are more likely to be explorers they will, on average, have worse wage outcomes after the displacement event. Therefore, our main empirical finding is successfully explained by differences in the wage offer distribution.[29]

We motivated this paper by estimates of the amenity value of living close to home from Kennan and Walker (2011). In this section we perform some back-of-the-envelope calculations that quantify what fraction of this amenity value can be explained by the benefit of living close to home after a displacement event that we have documented in this paper.

To calculate the latter, we take the difference in post-displacement earnings recoveries for those living close to their parents and those living farther away documented in Section 3. We discount these differences by an annual interest rate of four percent and find that the lifetime benefit (averaging over 35 years) of living close to parents, conditional on a displacement event, is around \$250,000. In our sample, the probability that a young worker experiences displacement is around 20 percent. As such, the expected benefit of living close to home is around \$50,000. This suggests that the benefit of parental proximity after job displacement is associated with an annual value of around \$3,000. If we perform the same calculations with our baseline estimates from Section 4, we obtain a value of \$1,000. These bounds suggest that the benefits of parental proximity after a job displacement account for a non-trivial portion of the \$20,000 per year amenity value that living close to home provides, according to Kennan and Walker (2011). The benefits of being close to home should also

[29]Notice that with a positive moving cost, $c > 0$, even if *all* individuals 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.

apply after other labor market disruptions so our estimates are likely to be a lower bound on the wage benefits of being close to home.

7 Conclusion

Young adults who live in the same neighborhoods as their parents experience stronger earnings recoveries after job displacements than those who live farther away. We find that this result persists even after we apply a reweighting scheme that controls for observable differences between the two groups. A simple theory suggests that the stronger earnings recoveries for those living near their parents can be explained by some workers receiving better wage offers at home, though we do not find any direct evidence of parental network effects. The stronger earnings recoveries are driven by smaller on-impact wage reductions and healthier post-displacement recoveries in both hours and wages. In the year of displacement, however, hours worked and unemployment durations are similar for the two groups, suggesting that longer job search is likely not the explanation. We find that housing transfers, which have been previously emphasized in this context, do increase around displacements, but these increases are modest. Housing transfers do not appear to vary between the two groups.

Our results suggest that workers live close to their parents because this helps them after a negative labor market shock. This phenomenon can explain why people appear so reluctant to move from declining areas (Ganong and Shoag, 2012; Zabek, 2017) and why migration responses have been smaller than expected after several local shocks (Bound and Holzer, 2000; Yagan, 2017). This reluctance to relocate could explain why workers appear to be less mobile in economic downturns (Molloy and Wozniak, 2011) and why immigrants appear to be more mobile than natives (Cadena and Kovak, 2016). Our results could also inform research on the decline in inter-state migration (Molloy, Smith and Wozniak, 2011; Kaplan and Schulhofer-Wohl, 2017), the recent increase in young adults living with their parents (United States Census Bureau, 2016), and the recent rise in leisure among younger men (Aguiar et al., 2017). More directly, parental resources may be an important explanation for the finding that workers place a large premium on living close to their places of origin (Kennan and Walker, 2011; Coate, 2017). Based on our empirical findings, simple calculations suggest that parental proximity after job displacement is associated with an annual value of between $1,000 to $3,000.

More broadly, we think that understanding these family ties can inform governmental assistance programs after job losses and other changes in people's labor market prospects. For example, government programs might be able to economize in cases where family resources substitute for formal insurance, or replicate this support in cases where governments want to encourage geographic mobility. In particular, if governments would like workers to move after job losses, then they might wish to establish programs that substitute for having parents nearby. Even if these programs perfectly crowd out parental investments, they may still be worthwhile since they would facilitate workers' migration decisions.

Going forward, we hope researchers use other data sources to verify our baseline results on parental proximity and post-displacement earnings losses. Ideally, these new data would also facilitate additional analyses that focus on the mechanisms leading to our baseline results. We think that building and estimating a model that incorporates parental location (Kennan and Walker, 2011; Coate, 2017) and matches well the earnings losses of displaced workers (Jarosch, 2015; Krolikowski, 2017*b*) is a particularly fruitful way to proceed.

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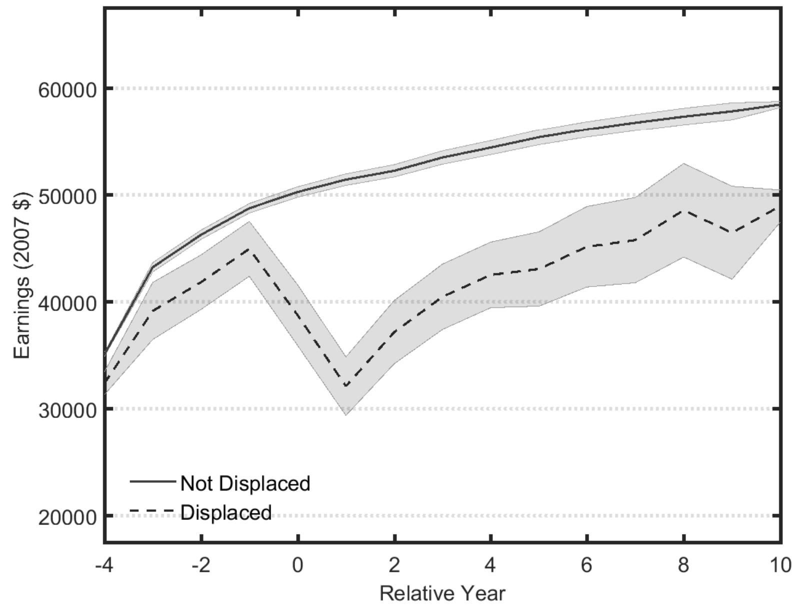
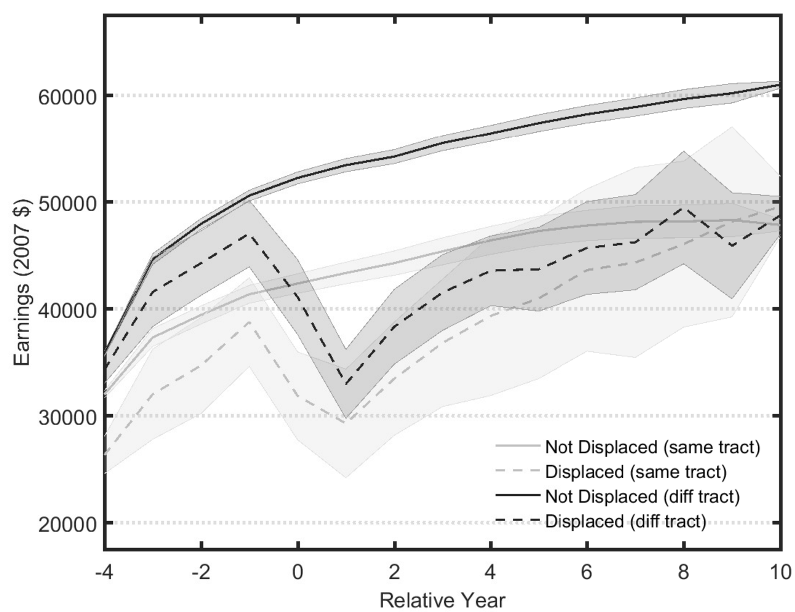


(a) Average Earnings for Young Displaced and Non-Displaced Workers



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

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

Note: Young workers (ages 25 to 35) who live in the parents' neighborhoods experience much stronger earnings recoveries after a displacement event than young workers who are not living in their parents' neighborhoods. Shading represents 95 percent confidence intervals using standard errors clustered by the person in the sample.

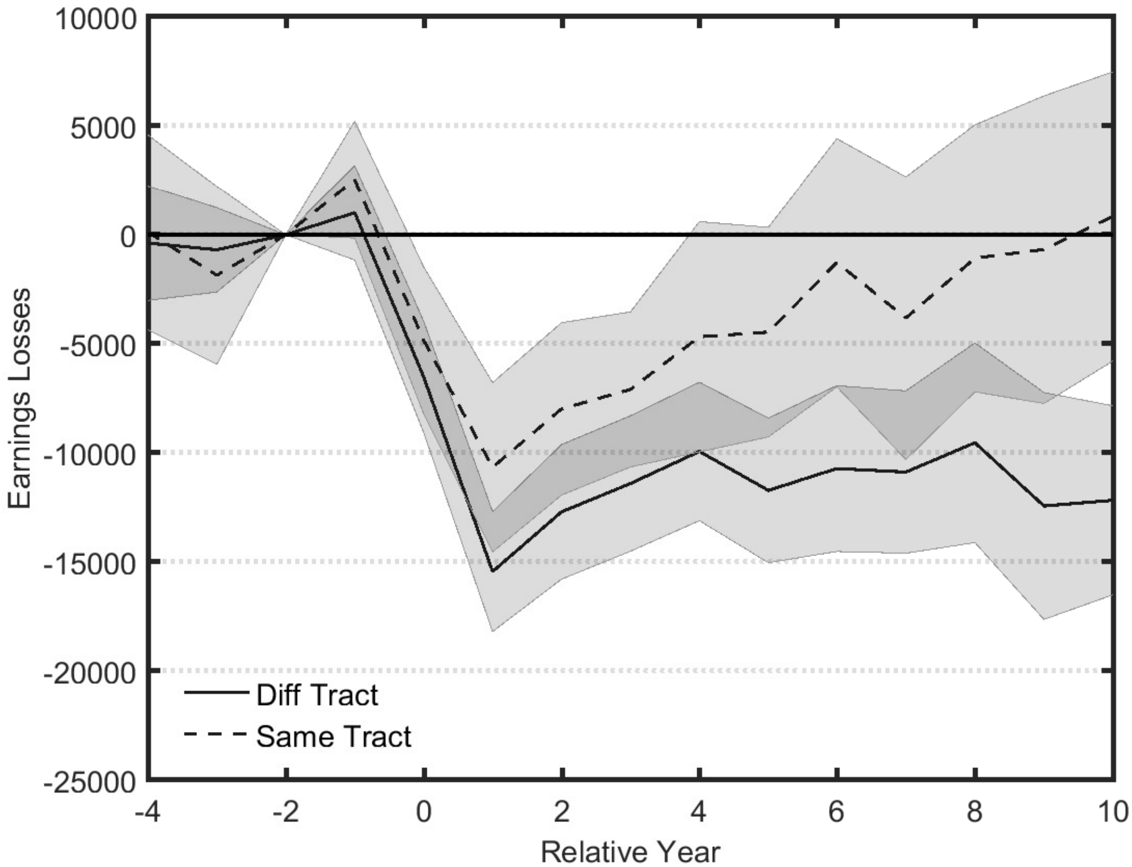
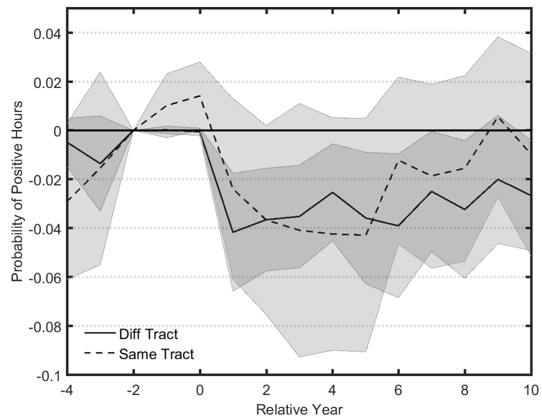
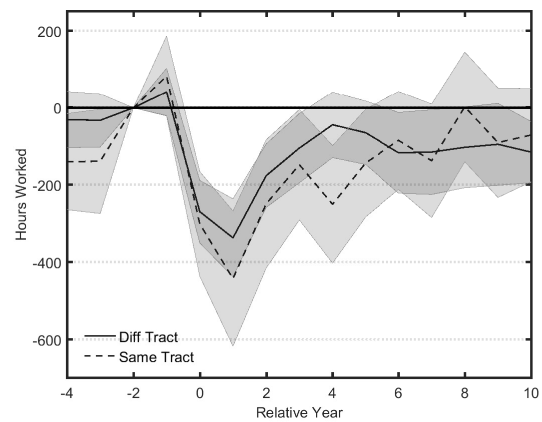


Figure 2: Earnings Losses for Young Displaced Workers

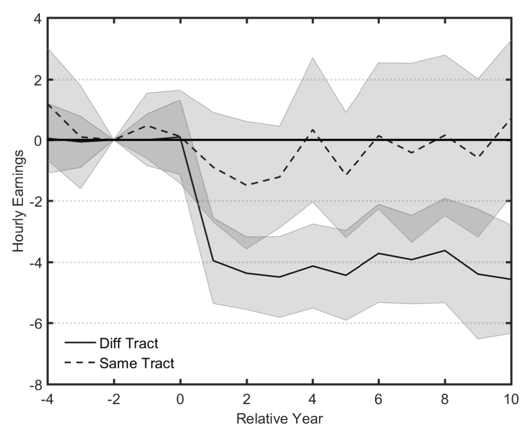
Note: The regression analysis supports the basic intuition in Figure 1: In the medium- and long-run, young workers living in the same neighborhoods with their parents experience a full recovery in earnings after a displacement event. Young workers not living in their parents' neighborhoods experience large and permanent earnings losses, amounting to around 30 percent of their pre-displacement earnings even 10 years after the displacement event. Shading represents 95 percent confidence intervals using standard errors clustered by the person in the sample.



(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: The intensive margin and wages drive the recovery in annual earnings documented in Figure 2. At the time of displacement, wages fall less for individuals living near their parents and hours fall slightly more, although the hours differences are not statistically significant. Shading represents 95 percent confidence intervals using standard errors clustered by the person in the sample.

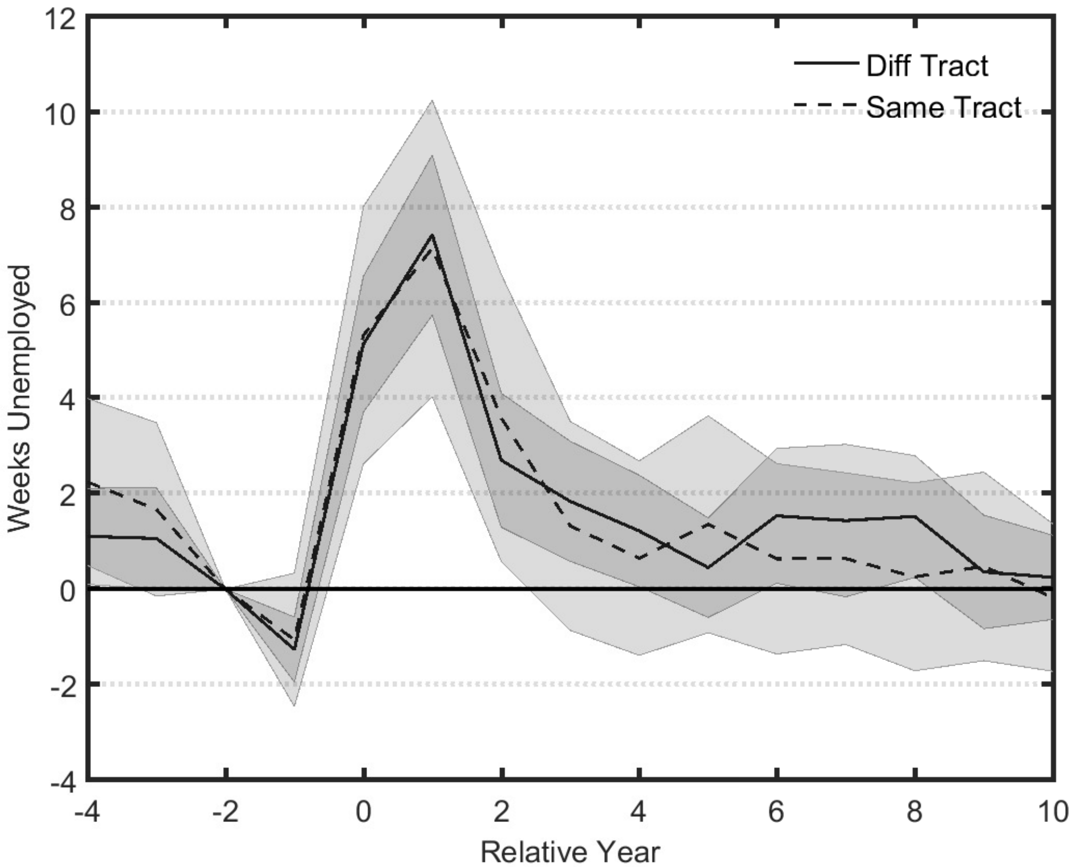


Figure 4: Weeks Spent Unemployed

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 around seven weeks on-impact and a steady decline over the next 10 years. Shading represents 95 percent confidence intervals using standard errors clustered by the person in the sample.

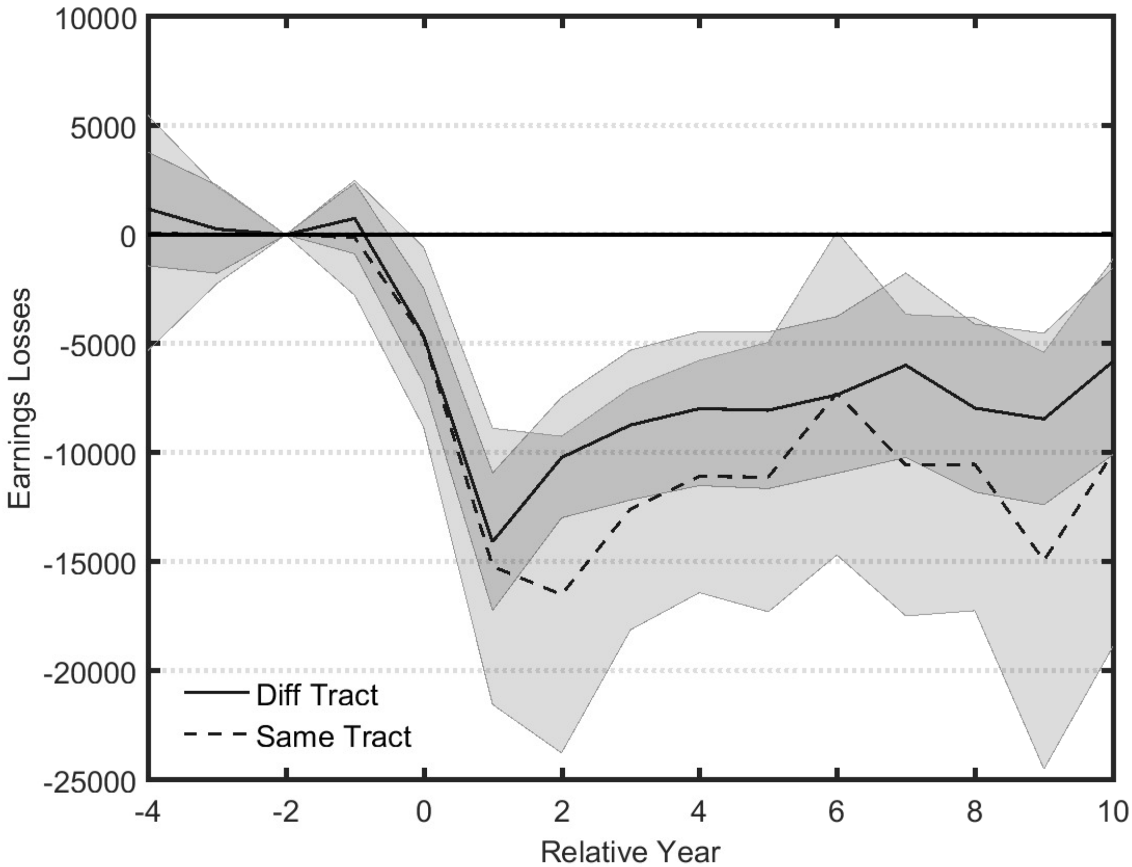


Figure 5: Earnings Losses for Older Displaced Workers

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. If anything, living near parents prior to displacement has a detrimental effect, but these differences are not statistically significant. Shading represents 95 percent confidence intervals using standard errors clustered by the person in the sample.

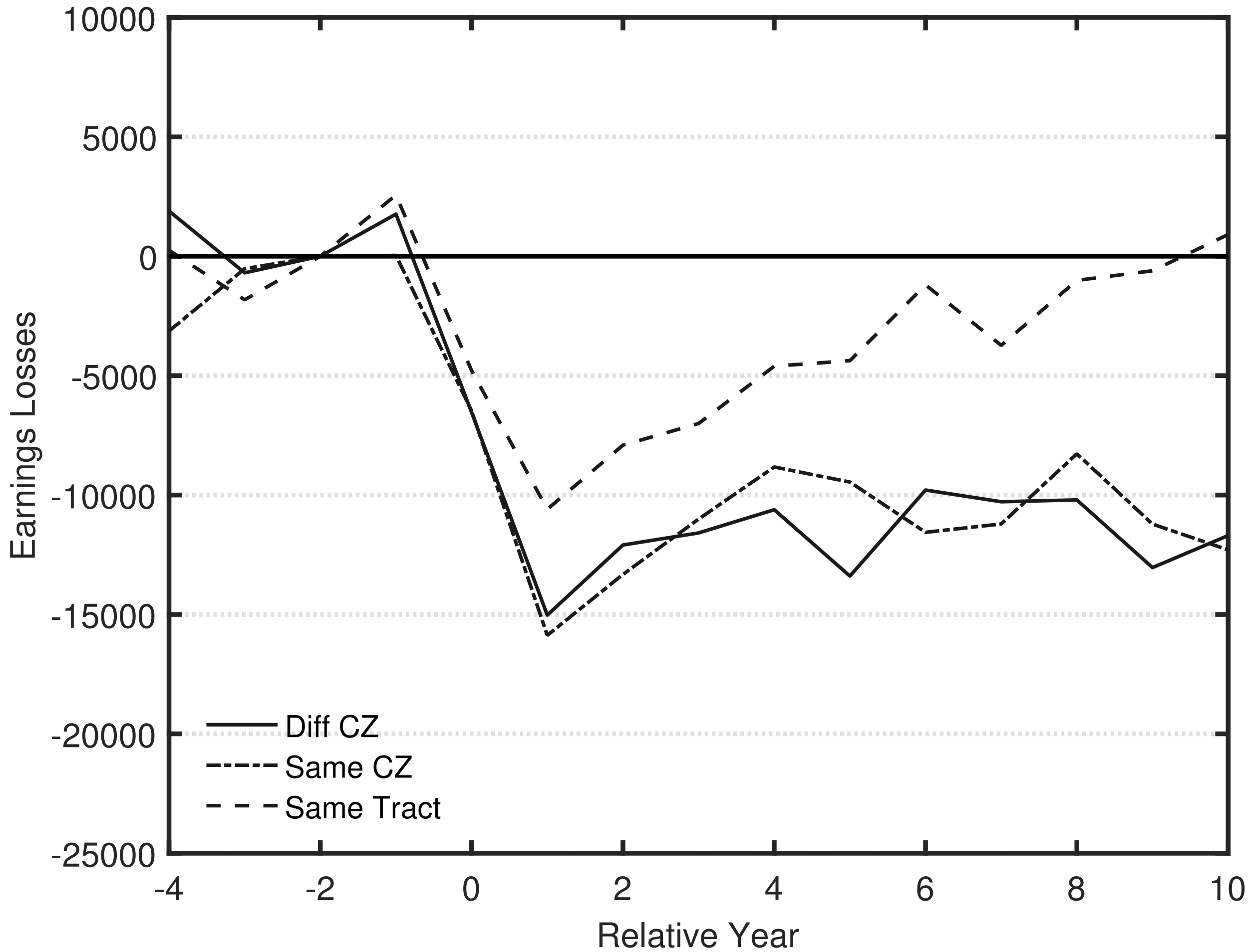


Figure 6: Earnings Losses for Young Workers by Different Proximities to Parents

Note: Those individuals living close to their parents (in the same commuting zone), but not in the same neighborhoods, do not experience significantly better post-displacement earnings outcomes than those who live farther away. The interactions between the displacement dummies and the same commuting zone dummy are not different from zero at the 95 percent confidence level in any relative year. The interactions between the displacement dummies and the same tract dummy are statistically different from the same commuting zone interactions six, nine and ten years after the displacement event.

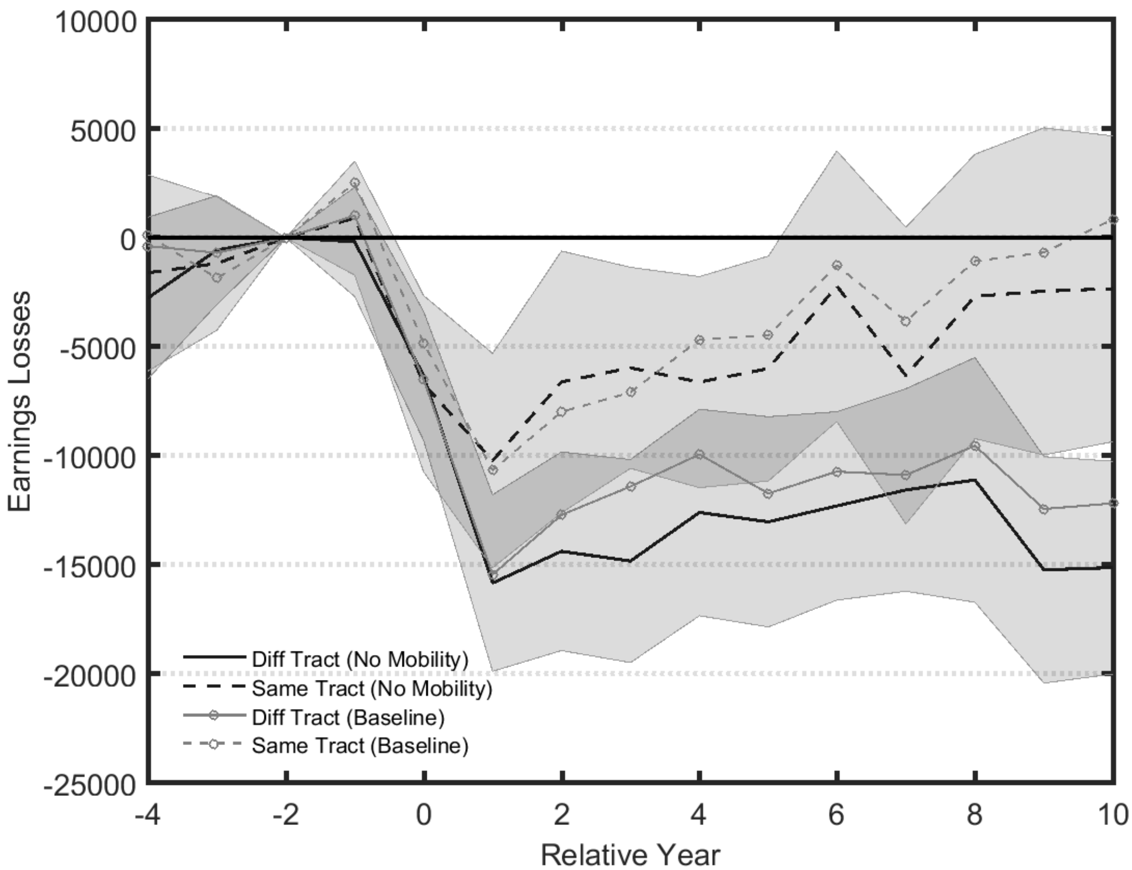


Figure 7: Earnings Losses for Young Workers Who Do Not Move

Note: Restricting the sample to individuals who do not switch counties after the displacement event does not affect the baseline result. In particular, individuals who lived in their parents' neighborhoods prior to the displacement event continue to see healthier earnings recoveries than individuals who lived farther away. At the 10-year horizon the difference between the two groups is virtually the same as in the baseline results. Perhaps not surprisingly, the sample that is restricted to no mobility almost always has worse earnings outcomes (point estimates) than the unrestricted sample. Shading represents 95 percent confidence intervals using standard errors clustered by the person in the sample.

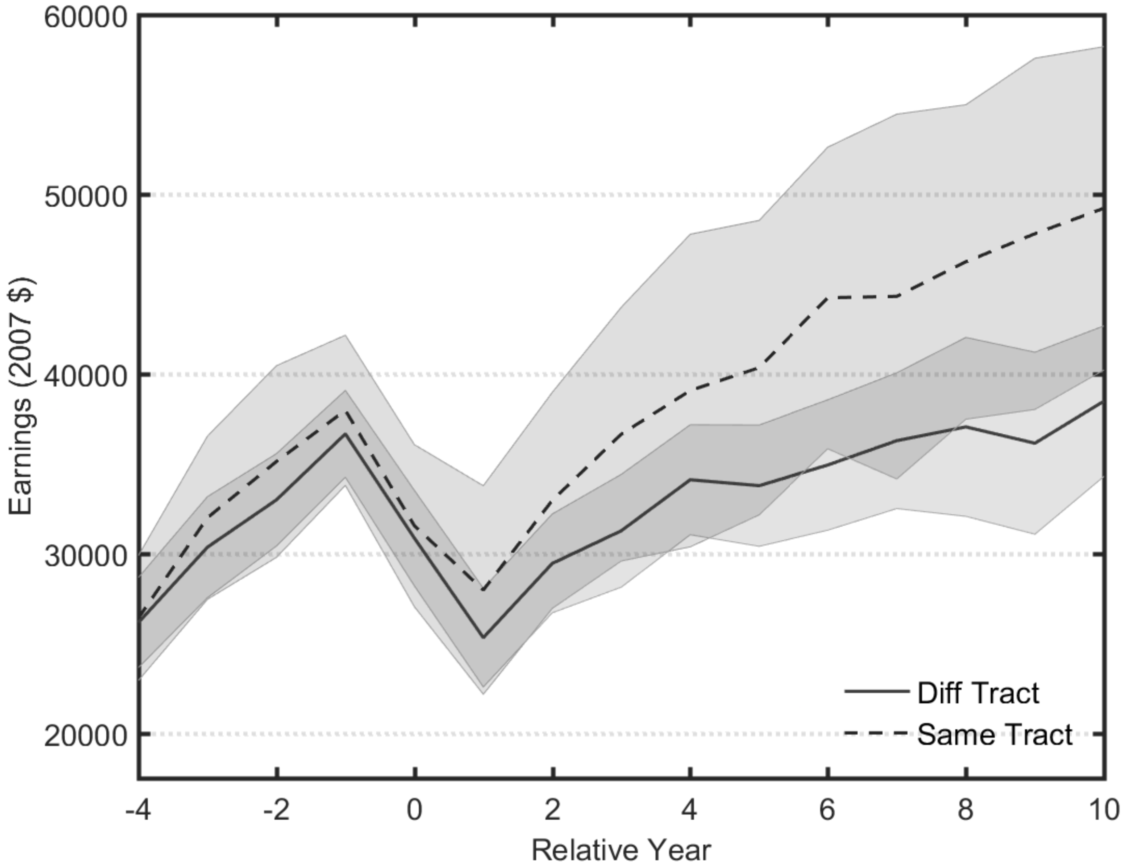


Figure 8: Means After Propensity Score Reweighting

Note: Simple averages, after applying propensity score weights, still suggest that individuals living close to their parents have significantly better post-displacement earnings outcomes than those who live farther away. Plotted are mean earnings in years surrounding a displacement event, using weights based on propensity scores. Shading represents 95 percent confidence intervals using standard errors clustered by the person in the sample.

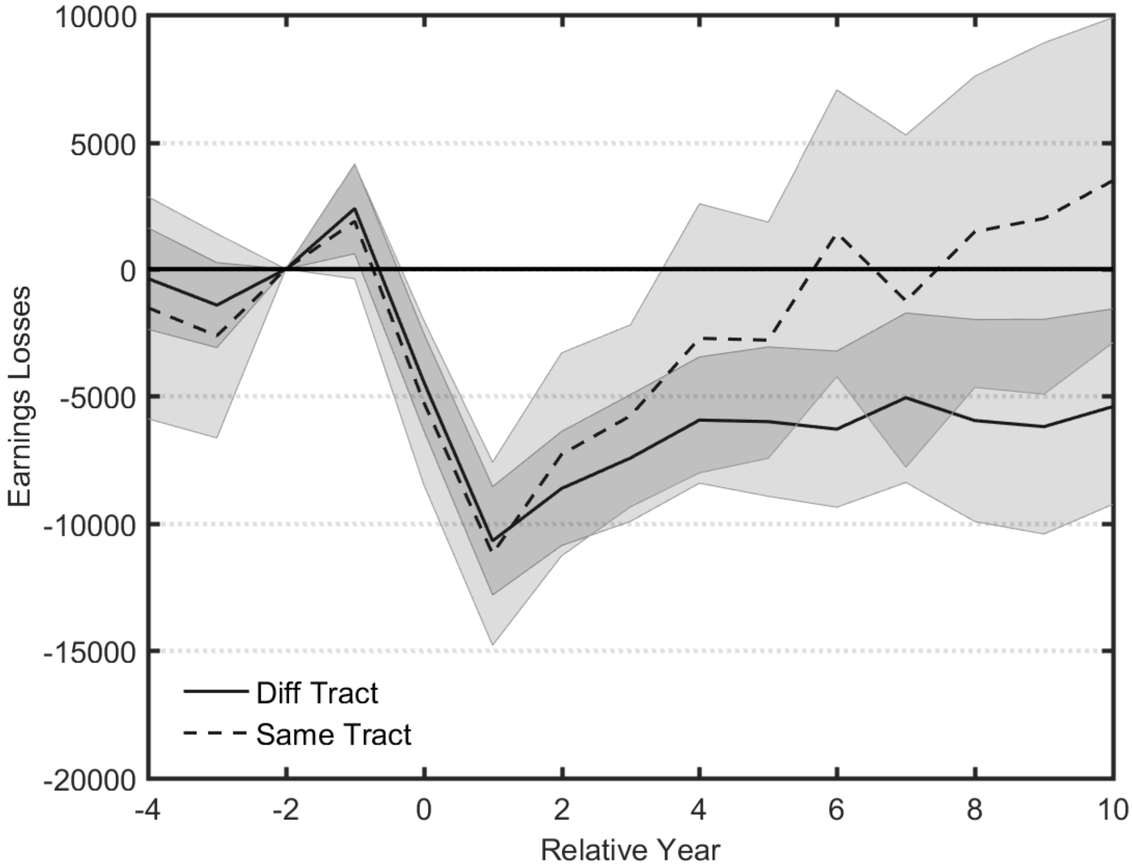
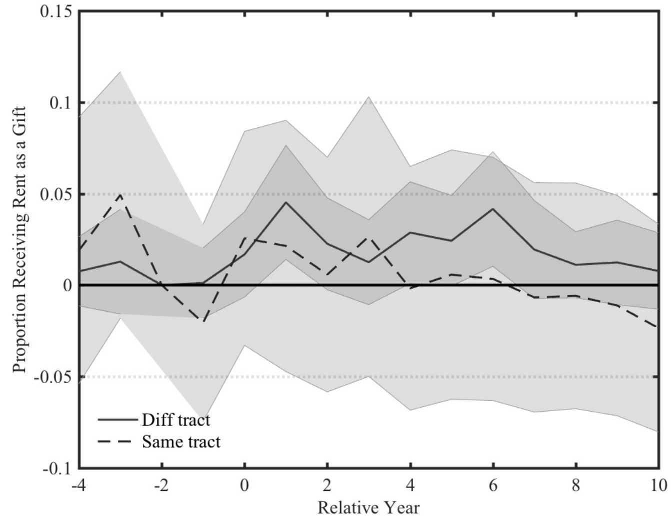
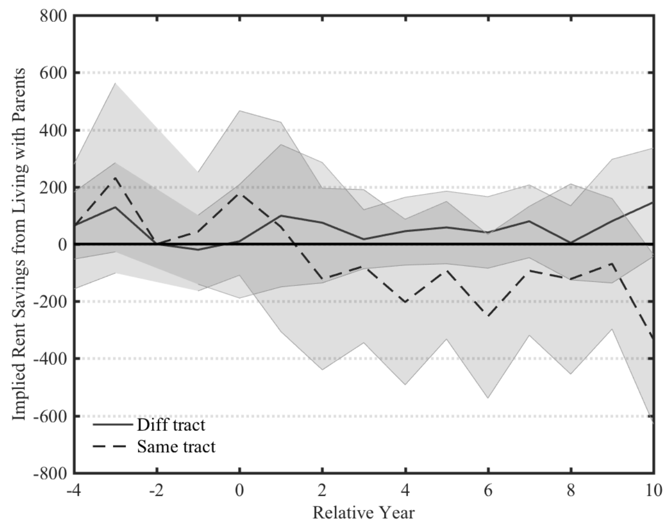


Figure 9: Regressions with Propensity Score Reweighting

Note: Even after controlling for observable differences using propensity score reweighting, young workers living in their parents' neighborhoods at the time of displacement experience healthier earnings recoveries than those living farther away. Although this difference is quantitatively smaller than in Figure 2, it is still statistically significant at longer horizons. Shading represents 95 percent confidence intervals using standard errors clustered by the person in the sample.



(a) Receiving Rent Entirely As a Gift



(b) Implied Rent Savings

Figure 10: Housing Transfers Around Displacements

Note: This plots event studies around displacement, as in Figure 2, for young workers. The dependent variable in the first panel is reporting receiving one's rent as a gift, and in the second panel it is the implied transfer to the family from living with another PSID family, most often parents. There is a noticeable increase in the likelihood of receiving rent as gift, but there is no detectable effect on the dollar value of housing transfers.

Variable	Not Displaced	Displaced	Not Displaced (T)	Displaced (T)	Not Displaced (A)	Displaced (A)
	Panel A: All Workers Age 25 to 55					
Earnings	55,997	48,331	48,332	41,746	57,412	49,906
Age	37.9	36.4	35.5	34.0	38.3	36.9
Years of Schooling	13.6	12.9	13.1	12.5	13.6	13.0
Employer Tenure	10.6	7.4	9.9	7.1	10.7	7.5
Fraction Co-residing	0.08	0.10	0.51	0.52	0	0
Fraction in Parents' Tract	0.16	0.19	1	1	0	0
Number of Children	1.24	1.23	1.26	1.37	1.23	1.20
Fraction Male	0.83	0.84	0.78	0.83	0.84	0.84
Wages ($/hr)	24.70	21.77	21.59	18.68	25.27	22.51
Hours Worked	2,306	2,256	2,273	2,281	2,313	2,250
# of records	44,871	1,468	7,629	320	37,242	1,148
	Panel B: Young Workers Age 25 to 35					
Earnings	48,940	45,128	41,628	36,211	50,756	47,093
Age	29.4	28.9	29.1	28.5	29.4	29.0
Years of Schooling	13.8	13.1	13.1	12.5	13.9	13.3
Employer Tenure	6.5	5.3	6.8	5.5	6.4	5.2
Fraction Co-residing	0.08	0.10	0.39	0.42	0	0
Fraction in Parents' Tract	0.25	0.28	1	1	0	0
Number of Children	1.12	1.13	1.21	1.24	1.10	1.10
Fraction Male	0.82	0.84	0.79	0.83	0.83	0.85
Wages ($/hr)	21.54	20.36	18.65	17.67	22.26	21.25
Hours Worked	2,314	2,237	2,276	2,246	2,323	2,235
# of records	18,074	710	4,034	205	14,040	505

Table 1: Summary Statistics

Note: 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 the base age (relative year −1). 'T' stands for those individuals living in their parents' tract (neighborhood) in relative year −1 and 'A' stands for those individuals away from their parents' neighborhoods. The PSID sample of household heads is composed chiefly of men. We restrict to observations that appear in our baseline sample (equation 1).

	Panel A: PSID Weights				Panel B: Reweighted			
	Same Tract		Different Tract		Same Tract		Different Tract	
Variables	Displaced	Not Displaced	Displaced	Not Displaced	Displaced	Not Displaced	Displaced	Not Displaced
Earnings	35,194	39,668	44,615	48,332	35,194	33,213	33,422	34,955
	(2,132)	(850)	(1,547)	(513)	(2,132)	(692)	(1,155)	(569)
	1.00	0.04	0.00	0.00	1	0.35	0.46	0.91
Average Change in Earnings	2,885	2,245	2,929	3,498	2,885	2,053	3,197	2,460
	(683)	(157)	(469)	(100)	(683)	(166)	(409)	(118)
	1	0.36	0.96	0.37	1	0.23	0.70	0.54
Years of Schooling	12.50	13.10	13.34	13.94	12.50	12.46	12.45	12.53
	(0.21)	(0.09)	(0.12)	(0.05)	(0.21)	(0.09)	(0.12)	(0.07)
	1	0.00	0.00	0.00	1	0.84	0.86	0.89
Employer Tenure	5.38	6.80	5.18	6.43	5.38	5.42	5.28	5.37
	(0.29)	(0.16)	(0.18)	(0.08)	(0.29)	(0.11)	(0.24)	(0.08)
	1	0.00	0.56	0.00	1	0.88	0.80	0.97
Share Manager/Professional	0.20	0.27	0.31	0.41	0.20	0.20	0.18	0.20
	(0.04)	(0.02)	(0.03)	(0.01)	(0.04)	(0.02)	(0.02)	(0.01)
	1	0.11	0.02	0.00	1	0.90	0.69	0.90
Share in Goods Industries	0.53	0.43	0.51	0.34	0.53	0.48	0.51	0.41
	(0.05)	(0.02)	(0.03)	(0.01)	(0.05)	(0.02)	(0.04)	(0.02)
	1	0.06	0.68	0.00	1	0.32	0.73	0.02
U/e Rate in County	7.52	7.16	7.47	6.80	7.52	7.39	7.81	7.44
	(0.34)	(0.15)	(0.23)	(0.09)	(0.34)	(0.17)	(0.36)	(0.19)
	1	0.29	0.91	0.037	1	0.68	0.55	0.85
Age	28.44	29.23	29.06	29.53	28.44	28.11	28.03	28.19
	(0.28)	(0.08)	(0.18)	(0.04)	(0.28)	(0.09)	(0.22)	(0.06)
	1	0.00	0.06	0.00	1	0.25	0.25	0.39
Fraction Male	0.82	0.81	0.86	0.86	0.82	0.81	0.78	0.84
	(0.04)	(0.02)	(0.02)	(0.01)	(0.04)	(0.02)	(0.04)	(0.01)
	1	0.72	0.37	0.41	1	0.83	0.43	0.68
Number of Children	1.29	1.22	1.10	1.12	1.29	1.17	1.23	1.20
	(0.14)	(0.05)	(0.07)	(0.03)	(0.14)	(0.05)	(0.09)	(0.03)
	1	0.64	0.22	0.22	1	0.40	0.72	0.52
# of records	191	3,637	457	12,678	191	3,637	457	12,678

Table 2: Means of Reweighted and Initial Sample

Note: After applying the propensity score weights, the sample of individuals who live in the same tract as their parents and those living farther away look observationally indistinguishable. This table reports means for each group in the initial sample using PSID weights in the first four columns and the propensity score reweights in the last four columns. For each variable, we report the mean, the standard error of that mean, and a p-value of a Wald test that this mean is the same as the value in the first column. Standard errors and p-values adjust for clustering at the individual level. "U/e" stands for "unemployment."

	Unemployed (A)	Unemployed (H)	Working (A)	Working (H)	Pooled (A)	Pooled (H)
Panel A: Young Workers						
$\mathbb{P}[in\ parent's\ industry]$	0.17	0.21	0.25	0.27	0.25	0.27
Panel B: Older Workers						
$\mathbb{P}[in\ parent's\ industry]$	0.061	0.066	0.087	0.052	0.087	0.052

Table 3: Summary Statistics of Sharing Parent's Industry by Parental Proximity and Labor Force Status

Note: Simple averages suggest that young workers living in their parents' neighborhoods are slightly more likely to be working in their parents' industry. For unemployed workers, their recorded industry is the industry of their previous job. Results are based on large sectors but looking at finer levels of disaggregation does not alter the conclusions. Parenthetical (A) stands for "away," i.e. those not in the same tract as their parents at the time of the survey, and (H) stands for "home," i.e. those living in their parents' neighborhoods.

	(1)	(2)	(3)	(4)	(5)
	Young	Young	Young	Older	Same state
Same Tract	0.026	0.0025	0.014	-0.036*	0.007
	(0.022)	(0.022)	(0.016)	(0.021)	(0.016)
Unemployment		-0.029	-0.0045	0.0065	-0.0049
		(0.027)	(0.019)	(0.028)	(0.023)
Same State					0.041*
					(0.022)
Observations	125,765	114,650	114,650	77,379	114,650
R-squared	0.0007	0.11	0.056	0.068	0.060
Demographic controls	NO	YES	YES	YES	YES
Year FEs	NO	YES	YES	YES	YES
Individual FEs	NO	NO	YES	YES	YES

*** p<0.01, ** p<0.05, * p<0.1

Table 4: Same Industry as Parents for Young Workers

Note: After additional controls, young workers (25 to 35 year olds) living in the same tract as their parents are slightly more likely to be employed in their parents' industry than young workers who live farther away, although our estimates are not statistically significant. Demographic controls include, among other things, the industry employment shares in an individual's county. Standard errors adjust for clustering at the individual level.

	Gifted Rent		Implied Savings	
Rent	Proportion	Dollar value	Proportion	Dollar value
\$6,808	0.023	\$2,533	0.080	\$4,267
(94)	(0.002)	(173)	(0.005)	(425)
7,910	18,479	420	18,479	540

Table 5: Measures of Housing Transfers

Note: The first column reports average annual rents for the baseline sample and the following columns report measures of housing transfers. The rows report means, standard errors of those means, and sample sizes. 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 people who live with a parent and the implied annual dollar value that they receive form that parent. Note that the dollar value is only observable when the parent is a PSID respondent themselves.

A Appendix: Additional Robustness Exercises (For Online Publication)

In this section we present several robustness checks to our baseline results in Section 3. The baseline results are remarkably robust to these different specifications, controls, and samples.

Appendix Figure 5 presents our baseline results together with results where PSID weights are not used. The results are similar and tell the same qualitative story.

Appendix Figure 6 presents results where we include controls for local labor market conditions. In particular we use employment numbers by county from County Business Patterns (CBP) and population information from the National Historical Geographic Information System, where we linearly interpolate between census years. These data are available from 1969 onwards. The figure suggests that controlling for county-level employment-to-population ratios does not affect the results. When we use county-level unemployment rates from the Local Area Unemployment Statistics (LAUS) as controls for local labor market conditions we obtain similar results but those data are only available after 1980, substantially reducing our sample.

Although Census tracts are very small areas of geography, 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 individuals 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.

It is possible that our baseline effect differs by gender. In particular, 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. To get at this, we created a dataset that included both heads and wives, since heads in the PSID are very likely to be males. Appendix Figure 8 presents the results from estimating our baseline specification on this pooled sample of wives and heads. We find that the results are very similar.[30]

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 individuals who are actually co-residing with their parents as opposed to living in the same neighborhoods

[30] Estimating the specification separately for men and women suggests that females have a slightly smaller benefit from living at home, but sample sizes are small, and the results are made more difficult to interpret because women, on average, tend to suffer smaller earnings losses following displacement (Ruhm, 1987).

as their parents, in the spirit of Kaplan (2012). Appendix Figure 9 presents the results from this exercise. We cannot reject the null hypothesis that the estimates for the coresiding young adults are different from those sharing the same tract 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 coresiding.

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 individual was displaced while living in the county that that they grew up in. Appendix Figure 10 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 county. These findings suggest that parental proximity has an independent effect on post-displacement earnings from other factors in an individual's home county.

B Appendix: Reweighted Earnings of Non-Displaced Workers (For Online Publication)

Another way to examine the reweighting approach is to plot the earnings trajectories of each control group, suitably reweighted. We do this in Appendix Figure 11 (panel a) by plotting average earnings before and after years where they were at risk of a displacement, according to our definition, but where they did not actually lose a job. Using standard longitudinal weights, there is a large difference in initial earnings, and people who live farther from their parents tend to have steeper earnings trajectories that level off later than those of people who live close to their parents. These differences swamp the size of displacement losses, even for people who lose their jobs.

When we apply the weights, however, the earning dynamics are similar, and statistically indistinguishable except at the very end. Appendix Figure 11 (panel a) shows that the two trends are lined up before the displacement, as shown in the earlier table, which is not surprising. Even from period zero (the simulated displacement) to period ten, when no information appears in the weighting procedure, the earnings trends track each other quite well, although there is a level shift. This implies that matching on initial earnings, education, occupations, gender, and other factors is enough to find workers with similar employment prospects.

Since the reweighting does not perfectly control for worker's expected earnings trajectories, we control for these differences in the regression specification as best we can. In the regression specifications, as before, we include a quartic in workers' ages, interacted with whether they are in the group living close to or far away from their parents. Appendix Figure 11 (panel b) shows these averages after removing the age quartic. Not surprisingly, the differences that remain between the two groups are quite small. Since the reweighted average earnings for non-displaced individuals living farther away from their parents lie above the earnings of those living close to their parents, we suspect that the difference in earnings losses between those in and out of their parents' neighborhoods, if we did not control for an age quartic, would be even larger. This is because those farther away would appear to lose more relative to their healthier counterfactual. In the main text we present what we see as a lower bound on the post-displacement earnings differential.

C Appendix: Including Additional Interactions in the Baseline Regression (For Online Publication)

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:

$$e_{iat} = \alpha_{ia} + \gamma_t + X_{iat}(\beta^A + \beta^H H_{ia} + \beta^C X_{ia}^C) + \sum_{k=-4}^{10+} (D_{it}^k \delta^k + D_{it}^k H_{ia} \zeta^k + D_{it}^k X_{ia}^C \xi^k) + \epsilon_{iat} \quad (3)$$

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 12 shows the coefficient estimates with several different interactions. The light gray lines, reproduced from Figure 2, show the baseline earnings losses for people living in the same tract as parents (dashed line) and people living away from parents (solid line). The darker lines in panels A and B of Appendix Figure 12, show the same coefficient plot if one also allows effects to vary by how remunerative the person's jobs was before 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 12 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 Appendix: Reweighting and Restricting to Observations with Common Support (For Online Publication)

We use the the procedure suggested by Crump et al. (2009) to choose a subsample of people where there is common support across groups for our reweighting analysis. This method chooses a subset of people who were living with their parents (and displaced), but who looked similar, in terms of the jobs they held and their demographics, to people living farther away. We do not find that our results change when we do this.

The procedure developed by Crump et al. (2009) is desirable because it provides a data-dependent way of determining which observations have enough common support to merit inclusion. As with much of the propensity score reweighting literature, the procedure was designed to accurately estimate an average treatment effect where there is a single treated group and another control group. We are estimating, effectively, an effect of treatment on the treated in a case where there are multiple treatment arms, so it does not directly apply in our situation. Nonetheless, it provides a reasonable, objective, guide for determining cutoffs for common support.

Note that the effect that we identify from this exercise could be different from the effect for the entire sample, even if we correctly identify each effect. The effect will be for the group in the more limited sample, not necessarily the full sample. This would be the case if there are heterogeneous effects of being close to ones parents after a job displacement. These different effects could, for example, be driven by particularly close relationships between parents and children in the sample without common support.

Including only people with common support should, however, make this approach more reliable in terms of mean absolute deviations of our parameter estimate from the true parameter for the sample with common support. Intuitively, this is because the procedure limits the range of the weights, making it so that any one observation will have a limited impact on the estimates. This should raise the effective sample size for the parameters, despite the discarding of observations. Crump et al. (2009) explicitly design the procedure to balance these two effects, in a somewhat more simple case than this one.

Another benefit, emphasized by Black and Smith (2004), is that a group with "good support" should be less affected by omitted variable bias, to the extent that it exists. Intuitively, this is because a group of people with very similar observables to those in a control group should also be similar in terms of unobservables. If unobservables are similar, then omitted variable bias should be less pronounced.

We implement the procedure in two steps, following the algorithm suggested by Crump et al. (2009). First we estimate our propensity scores for the full sample, as before. Second, we increase the support cutoff ($\hat{\alpha}$) from zero until the following inequality holds:

$$\frac{1}{\alpha(1-\alpha)} \leq 2\frac{\sum_{i=1}^{N}[1_{\hat{e}(X_i)\{1-\hat{e}(X_i)\}\geq\alpha(1-\alpha)}/\hat{e}(X_i)\{1-\hat{e}(X_i)\}]}{\sum_{i=1}^{N} 1_{\hat{e}(X_i)\{1-\hat{e}(X_i)\}\geq\alpha(1-\alpha)}}$$

Where $\hat{e}(X_i)$ is the estimated propensity score, i indexes (possible) displacement events, and X is our vector of predictors.

The reweighted results using the common support subsample are similar to the reweighted results using the entire sample. We present all the same tables and figures from the analysis in Appendix B for the common support subsample: Appendix Table 1 presents the balance table, Appendix Figure 13 presents the means around displacements, Appendix Figure 14 presents the regression results, and Appendix Figure 15 presents the means of the non-displaced workers.

E Appendix: Search Intensity and Switching Industries (For Online Publication)

In Section 5 we outlined two possible mechanisms: housing transfers and parental employment networks. Here we look at two more: job search intensity and industry switching. We find that neither is a likely explanation for our baseline earnings results.

E.1 Search Intensity

This section outlines some results about how search intensity for young individuals varies with proximity to parents. We are motivated by the idea that parents may provide additional encouragement to their children after the job displacement, which may help with the job search process (Dalton, 2013). Our analysis, however, documents no statistically significant relationship between the search intensity of unemployed young adults and living close to parents.

The search activities data we use here only started in 1988 (as opposed to 1968 for the main analysis) and we stop the analysis in 2013, yielding 18 years of data. This means that the sample used for this exercise will be different from the one in the main text. Nonetheless, unless we think that the relationship between search intensity and parental proximity has changed from the 70s and 80s to the time thereafter, the present analysis should be representative of the entire period. Other than that, we use the same "stacked" version of the data in this analysis as described in Section 2. The search activity questions ask about what methods of job search were used by respondents, e.g. checked with private employment agency, checked with friends or relatives, and placed or answered ads.

Appendix Table 2 presents summary statistics on search intensity for those living in the same tract as their parents and for those living farther away, by labor force status at the time of the interview, for younger (25-35) and older (36-55) adult children. The last two columns of this table suggest that young individuals living close to their parents are perhaps more likely to engage in some form of job activity than those living farther away. A comparison between those two columns should be informed by the fact that those living close to their parents are more likely to be unemployed and unemployed individuals are more likely to search. The latter can be seen in Appendix Table 2 by comparing the search activities of the employed and the unemployed. On the former, the unemployment rate of those living close to their parents is far higher than for those living farther away. Within labor force status, those at home, if anything, appear to search less than those farther away, however when unemployed, they appear to be more likely to check with friends or relatives.

The bottom panel of the table shows that older workers are, on average, less likely to be searching for a new job than young workers, regardless of labor force status. The patterns of search activities by proximity to parents for older adults are similar to younger workers, although the differences between the searching behavior of those close to their parents and farther away is more similar than for younger workers. In particular, older workers who are unemployed at the time of the survey are no more likely to check with their friends or relatives during the search activity than those who live farther away.

In order to go beyond these basic comparisons of means, we estimate the following linear probability model:

$$search_{it} = \alpha + \beta Sametract_{it} + \gamma X_{it} + \epsilon_{it} \tag{4}$$

where $search_{it}$ is a dummy for whether individual i reported any job search activity in period t, $Sametract_{it}$ is a dummy for whether individual i is living in their parents' tract time period t, and X_{it} includes a host of controls.

Table 3 presents the results of this analysis, by labor force status. The first column reproduces the average difference in the probability of searching for a job between unemployed young adults living in the same neighborhoods as their parents and those living farther away from Table 2. Columns (2) and (3) add increasing number of controls, including demographic controls, such as age, race, and education, and year fixed effects. These results rule out large negative effects of parental proximity on young adult search activity and suggest a negative relationship between the two that is not statistically significant. Column (3), for example, suggests that living in the same neighborhood as one's parents reduces the probability of unemployed young adults engaging in search activities by 11 pp (on an average of around 85 percent), but the standard errors are large. Column (4) shows that employed young adults who live in their parents' neighborhoods, conditional on the controls in column (3), are slightly less likely to be engaged in search activities than young adults living away from home, but the point estimate is virtually zero and precisely estimated.

Column (5) pools unemployed and employed young adults and includes individuals fixed effects in addition to the other (time-varying) controls in columns (3) and (4). This approach uses variation in proximity to parents within an individual's observations to identify the effect of parental proximity on search activity as opposed to variation in proximity to parents across individuals. The results of column (5) are also not statistically significant and small. We take these results as not suggesting large differences in job search activity for those living in their parents' neighborhoods and those living farther away.

We are also not able to say much about how search activity changes around a displacement event for those living close to their parents and those living farther away. In particular, when estimating equations like equation (1), but with search activity as an outcome variable, we are unable to reject that the search activity of these two groups move in the same way around a displacement event. Taking these results at face value, we conclude that, although variations in search intensity among young adults living close to and farther away from home could be partially responsible for the markedly differential post-displacement earnings trajectories we observe for these two groups, the effect would likely have to be through something other than higher job finding probabilities for those at home, based on our results on unemployment duration (Section 3.2). Ultimately, a more definitive statement would warrant further research with different data.

E.2 Industry Switching

Previous work, including Jacobson, LaLonde and Sullivan (1993) and Stevens (1997), has documented that industry and occupation switchers experience larger post-displacement earnings losses than individuals who retain a job in their former line of work. We document industry switching around the displacement event for individuals who were in the same tract 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_{it}, that equals one if the individual switches one-digit industry between survey year t and $t+1$.

Appendix Figure 16 presents this 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 16 also suggests that individuals living in the same tract as their parents prior to the displacement event experience markedly sharper increases in their probability of switching industries than individuals living farther away. Based on the prior work cited above, this would predict larger post-displacement earnings losses for individuals 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. In results not shown, occupation switching is similar for the two groups around a displacement event.

F Appendix: Measures of Transfers (For Online Publication)

F.1 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 and smaller houses that have fewer people living in them. To make these comparisons accurate, the scale needs to account for not only crowding that makes a house less desirable, but also returns to scale in household consumption. For example, many parts of a house can be shared without too much loss of utility from either party. For example, two adults living together will generally only require one kitchen and living room, but the same two adults might want to have one kitchen and living room per person, if they lived in different units. The scale also needs to account for the fact that children take up fewer resources than adults.

We use the OECD equivalence scale, given by the equation below, which is among the most commonly used formulations. Mechanically, each adult additional (denoted A) counts for 70 percent of the initial adult, and each child 50 (C, 14 or younger) 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.

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

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 in the larger household. Then it multiplies this amount by the value of the equivalence scale for the child's household to get the amount of total rent this household would have to pay to have this standard of living. The difference between this counter factual rent and the rent that the child actually pays is the implied savings from living with parents.

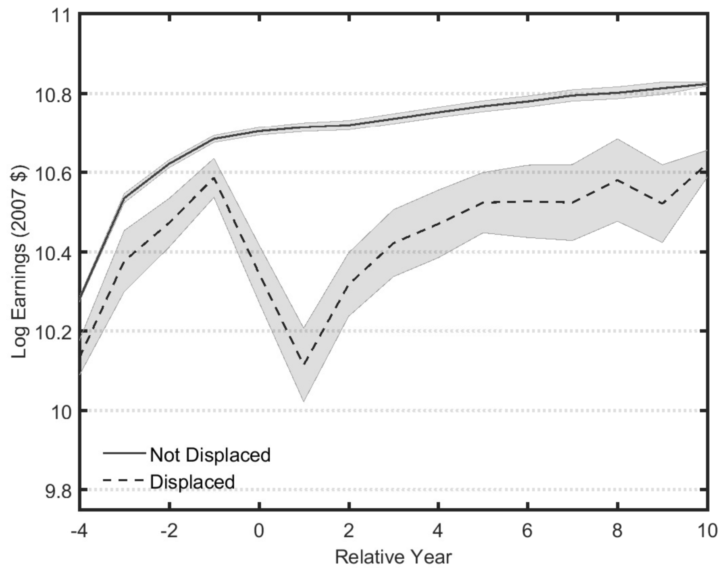
One complication in practice is that parents oftentimes own their houses, which means there is no direct measure of parents' rents. To compute an annual rent equivalent in these settings, we employ a user cost of capital equal to 0.0785 (following Albouy and Zabek (2016)). The user cost gives, essentially, 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 Monetary Transfers from Friends and Relatives

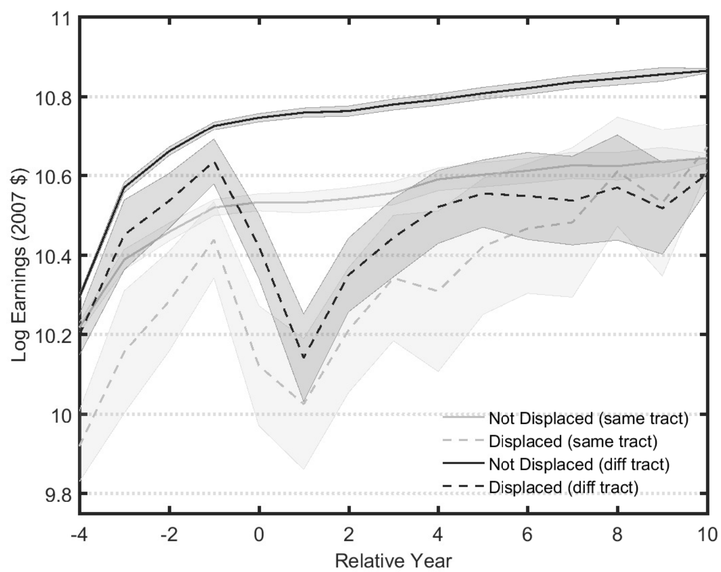
In addition to transfers of housing children can receive monetary transfers from their parents. Appendix Figure 17 shows how these change around displacement, using our main event study specification including fixed effects, an age quartic, and other controls. For these plots we use an annual question 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 a 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.

G Appendix Figures (For Online Publication)



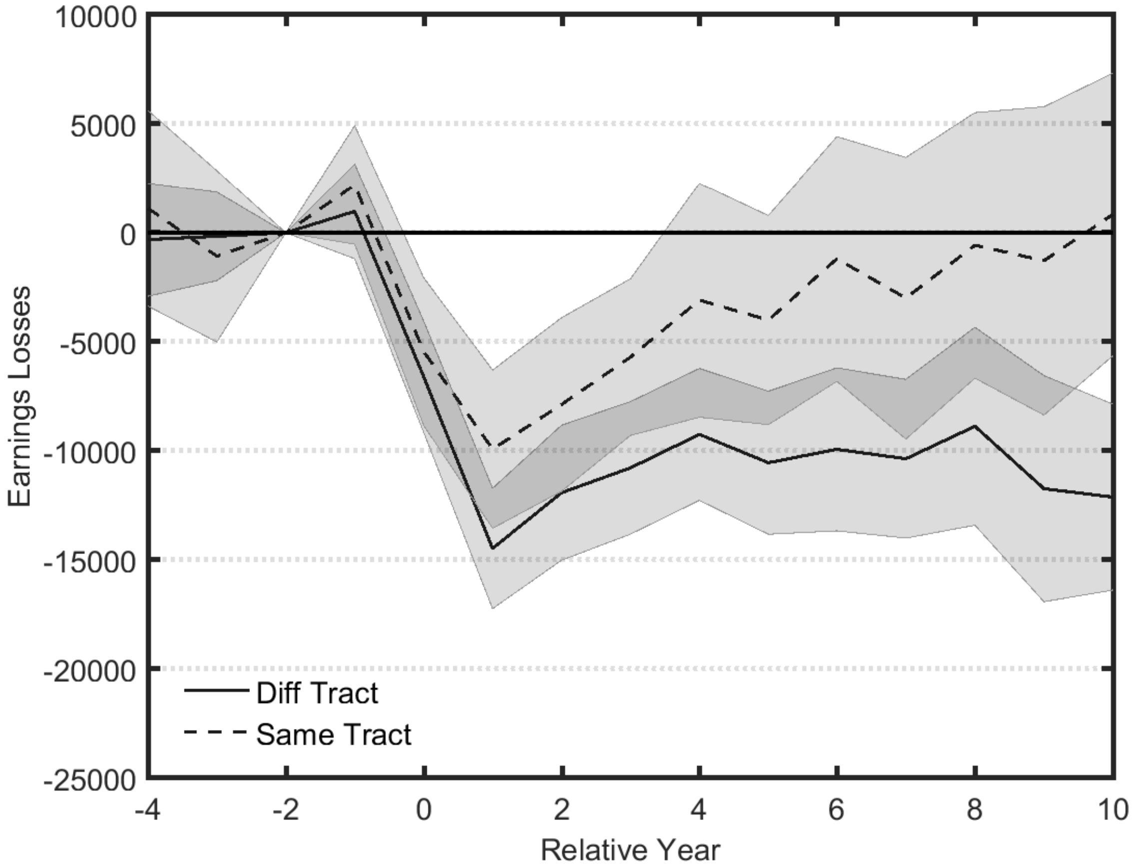
(a) Average Log Earnings for Young Displaced and Non-Displaced Workers



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

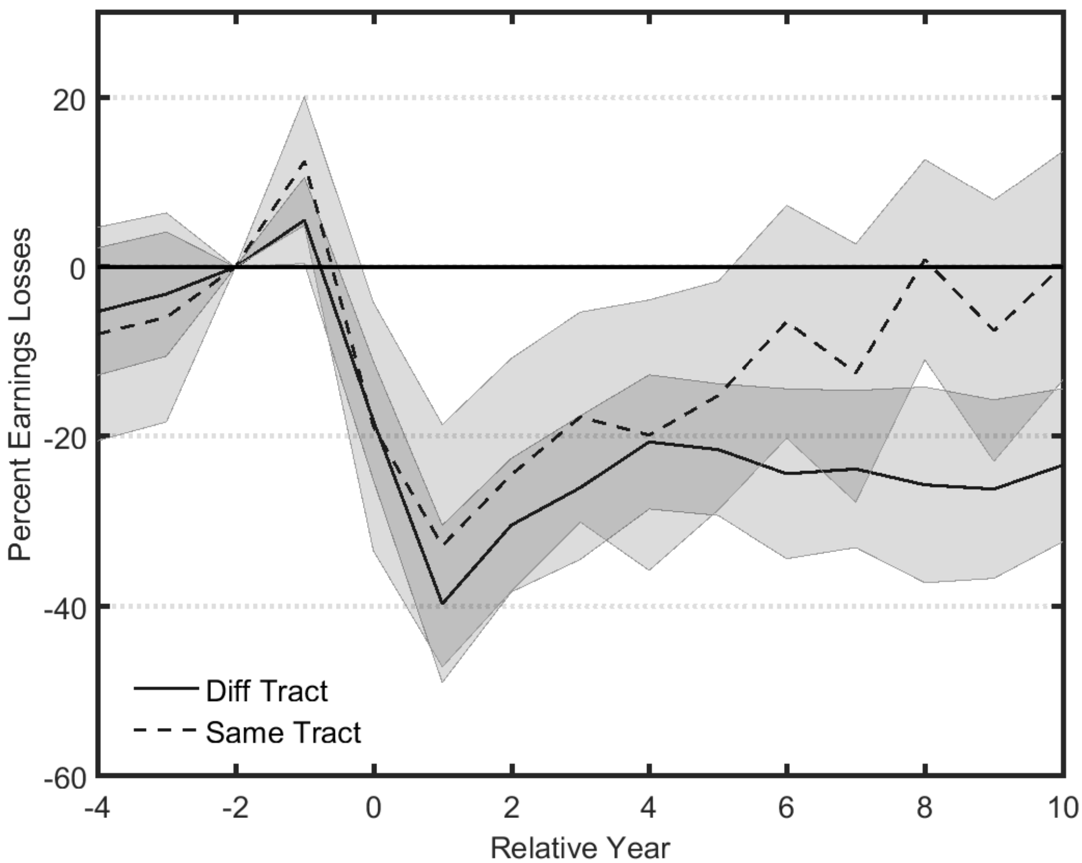
Appendix Figure 1: Average Log Earnings for Young Displaced Workers by Proximity to Parents

Note: Young workers (ages 25 to 35) who live in their parents' neighborhoods experience much stronger earnings recoveries after a displacement event than young workers who are not living in their parents' neighborhoods. This figure depicts log earnings and is analogous to Figure 1 in the main text that depicts earnings in levels.



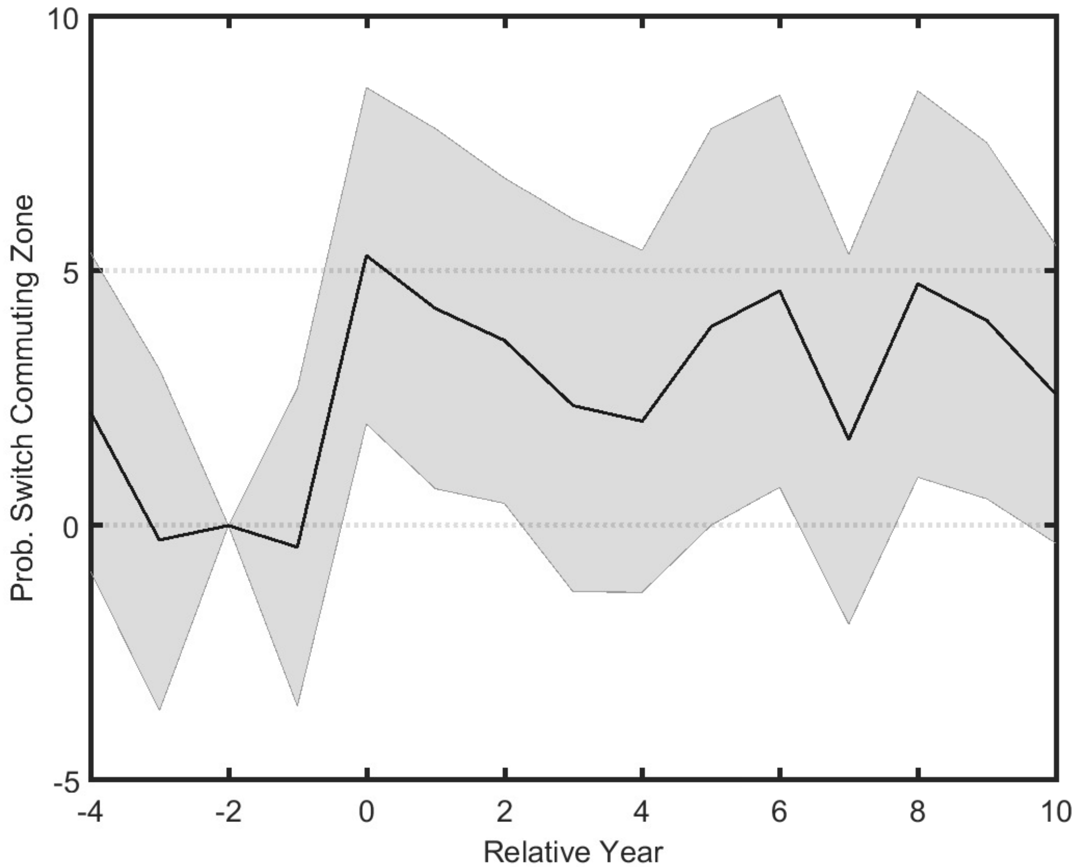
Appendix Figure 2: Earnings Losses for Young Displaced Workers (Excl. Zeroes)

Note: Dropping observations with zero earnings gives the same result as the baseline finding in Figure 2: 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 around 30 percent of their pre-displacement earnings even 10 years after the displacement event.



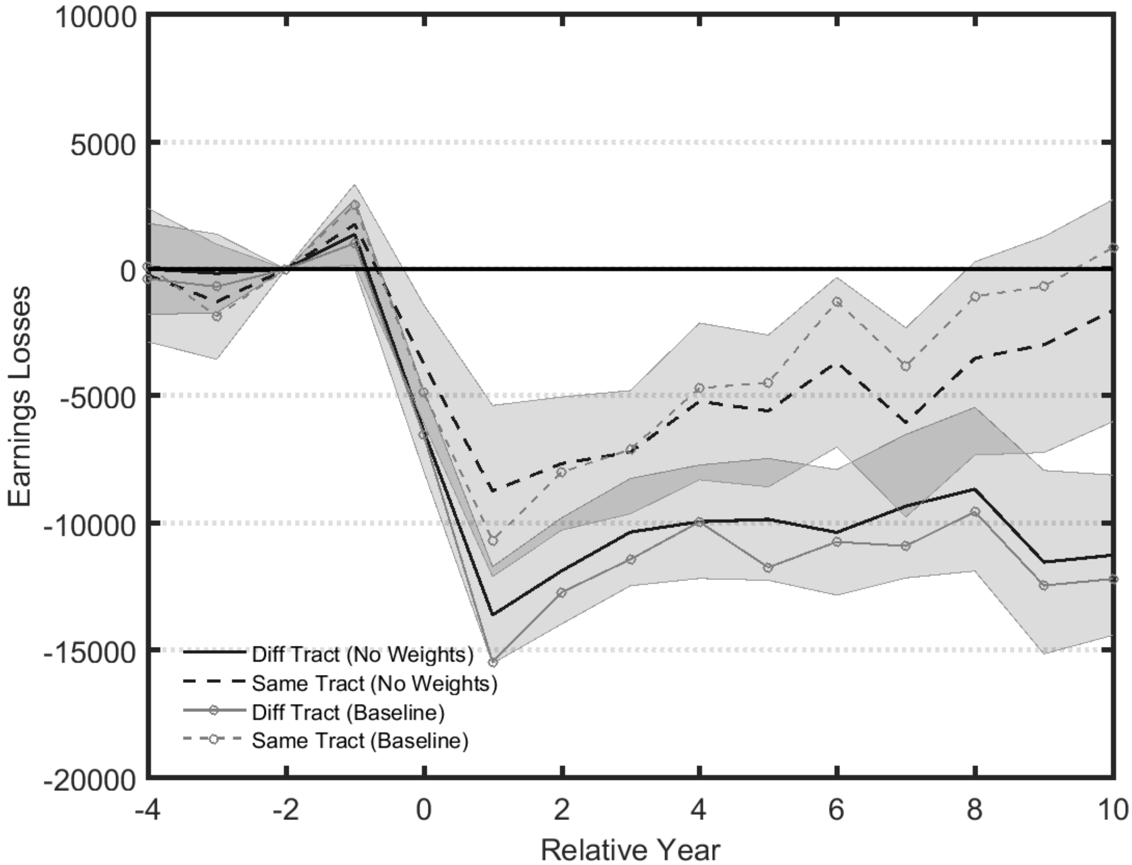
Appendix Figure 3: Percent Earnings Losses for Young Displaced Workers

Note: Using log earnings instead of earnings in levels gives the same result as the baseline finding in Figure 2: 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 almost 30 percent of their pre-displacement earnings even 10 years after the displacement event. To obtain percentage changes we plot $e^{\hat{\delta}^k} - 1$.



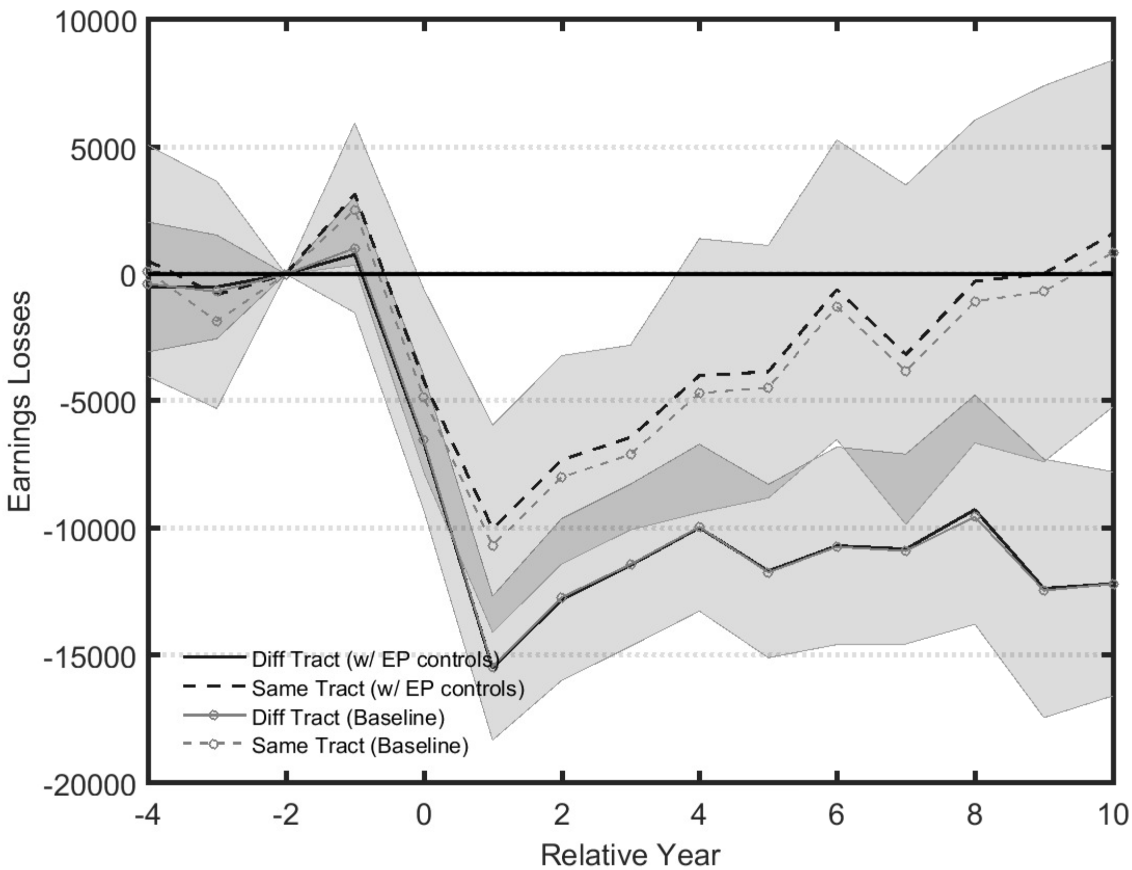
Appendix Figure 4: Probability of Switching Commuting Zones Around Displacement

Note: At the time of displacement, the annual commuting zone switching probability rises by around 5pp. In light of an average annual probability of switching commuting zones at around 5pp, this increase represents a sharp increase in geographic mobility. We use commuting zones as the relevant measure of geography because they most closely resemble the "regional labor markets" that Huttunen, Møen and Salvanes (2016) use with Norwegian data.



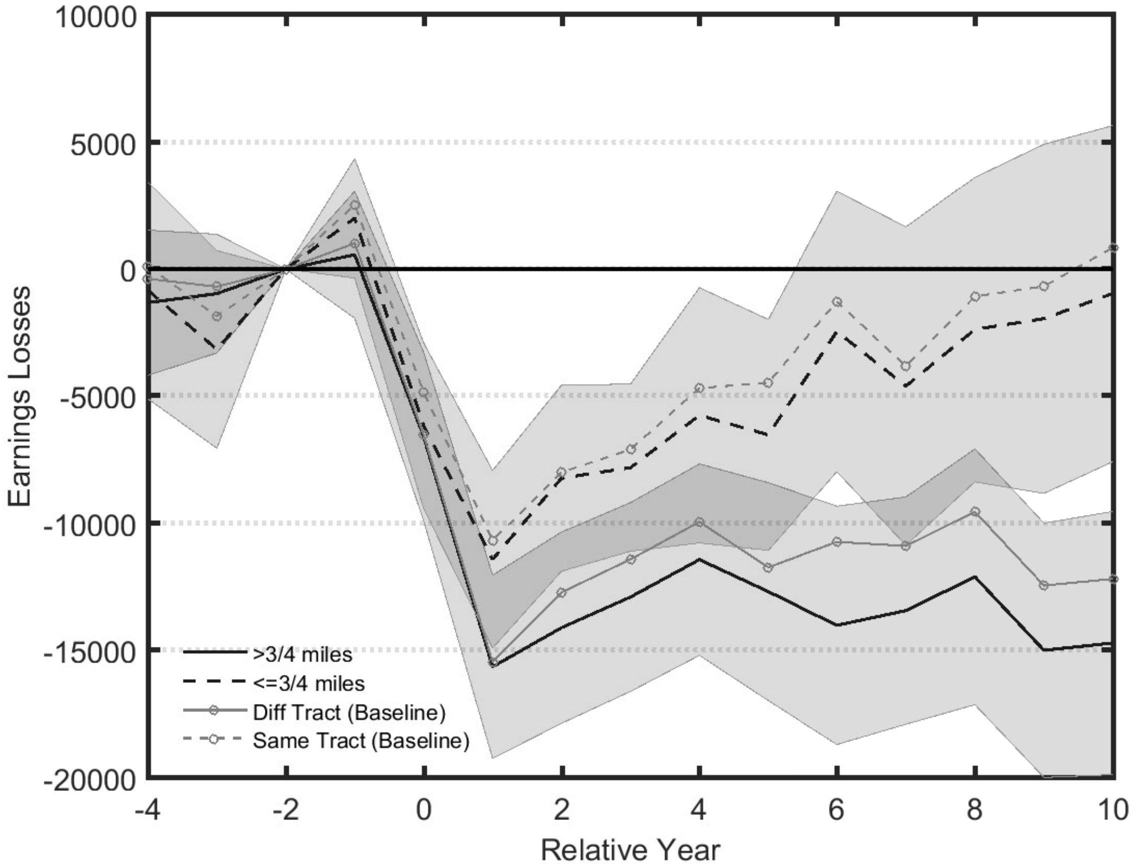
Appendix Figure 5: Earnings Losses for Young Displaced Workers (No PSID Weights)

Note: These are the baseline results from equation (1) when we do not use the PSID longitudinal weights. They tell the same story as the baseline results, although those living near their parents at the time of displacement see slightly less of a benefit.



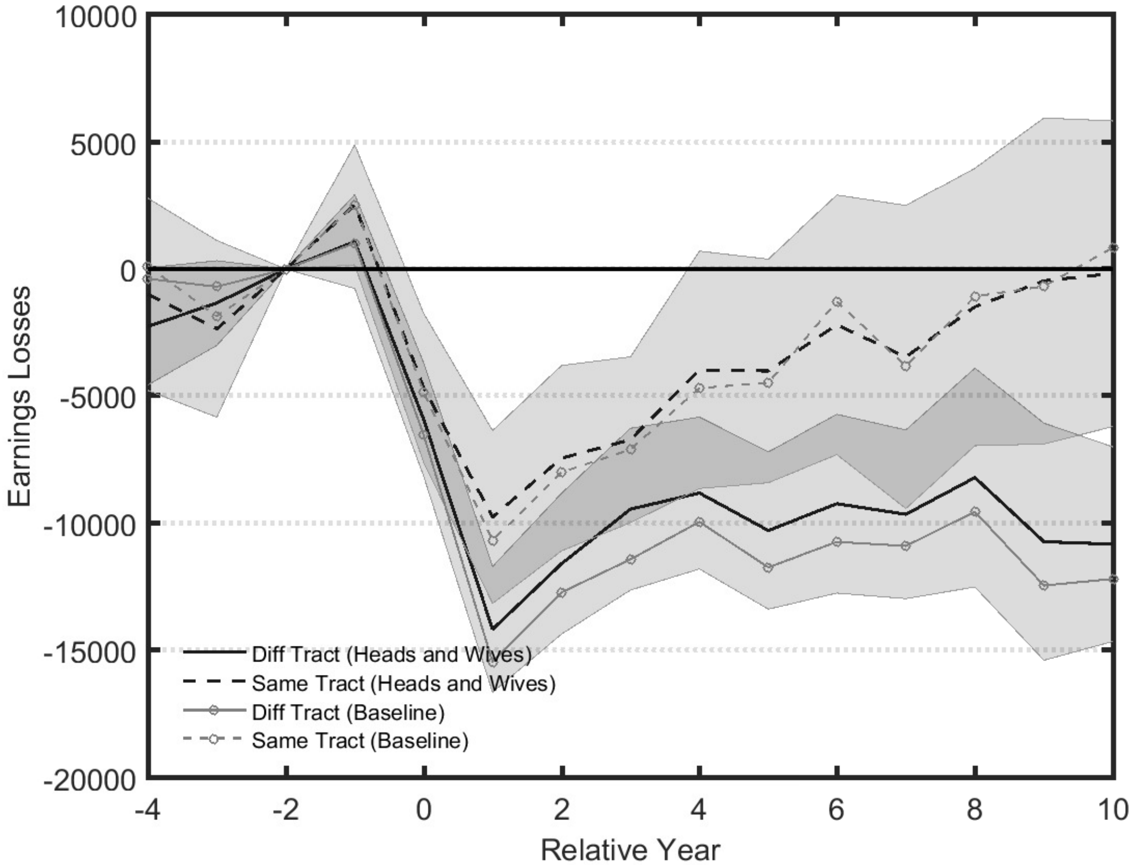
Appendix Figure 6: Earnings Losses for Young Displaced Workers (Controlling for Local Labor Market Conditions)

Note: These results control for employment-to-population ratios at the county level in our baseline equation (1). The results are virtually the same as in the baseline specification. "EP" stands for employment-to-population ratio.



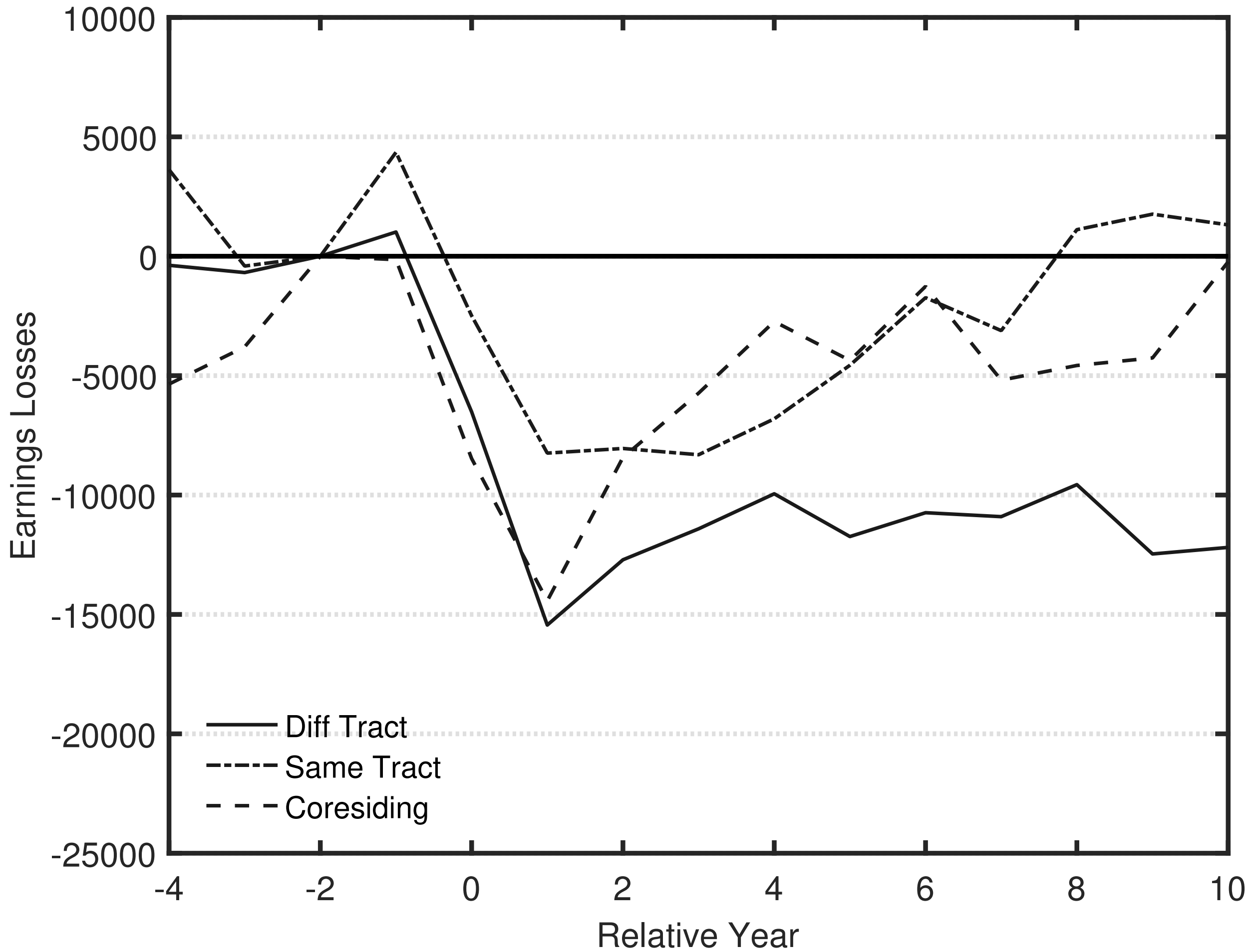
Appendix Figure 7: Earnings Losses for Young Displaced Workers (Using Distance Measures)

Note: These results estimate equation (1) where we define closeness by distance to parents and less than 3/4 miles is close. The results are very similar to the baseline specification. If anything, this approach strengthens the findings slightly.



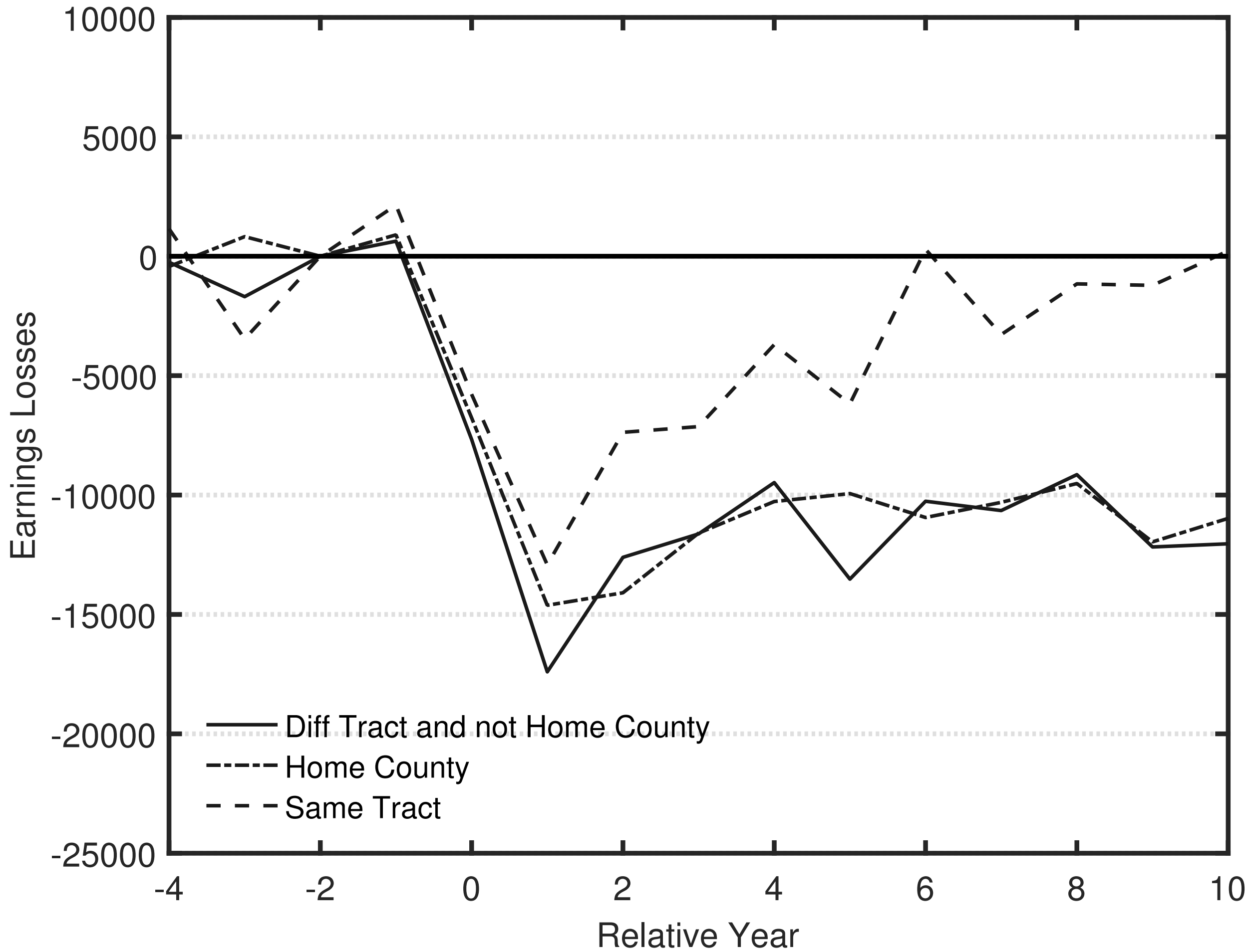
Appendix Figure 8: Earnings Losses for Young Displaced Workers (Heads and Wives)

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.



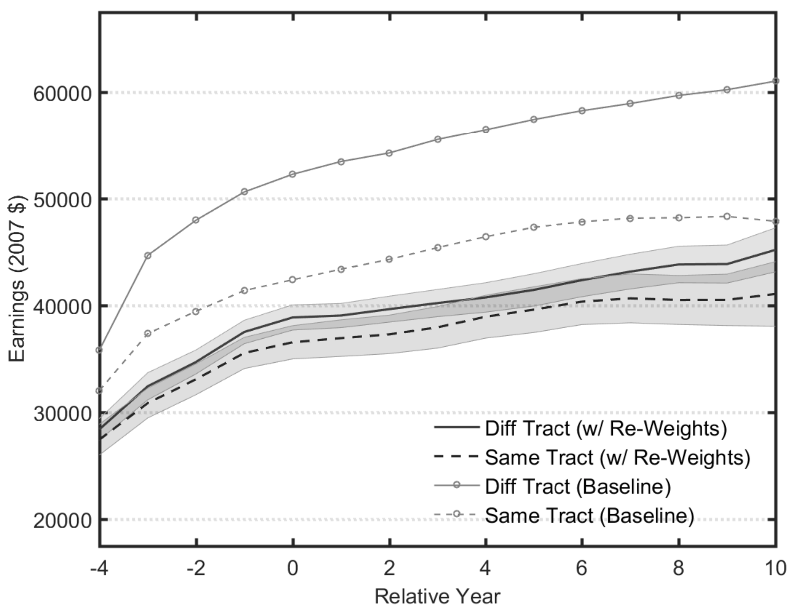
Appendix Figure 9: Earnings Losses for Young Displaced Workers (Same Tract vs. Coresiding)

Note: The post-displacement earnings recoveries look similar for those individuals who actually live in the same house as their parents (coresiding) and those who live in the same tract as their parents but are not coresiding. Both groups appear to do better than those who live outside their parents' neighborhoods.

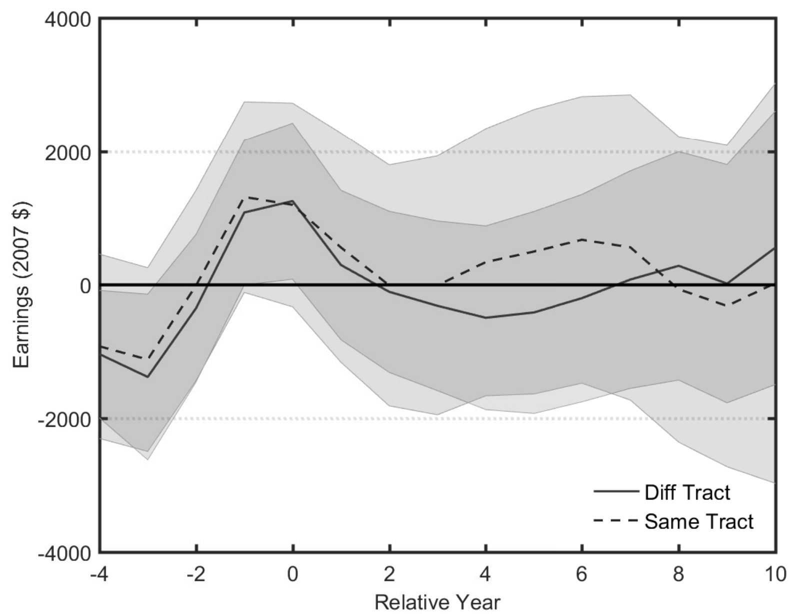


Appendix Figure 10: Earnings Losses for Young Displaced Workers with Home County Interactions

Note: The post-displacement earnings recoveries look similar to our baseline results even after interacting the displacement dummies in equation (1) with whether an individual lived in the county that 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.



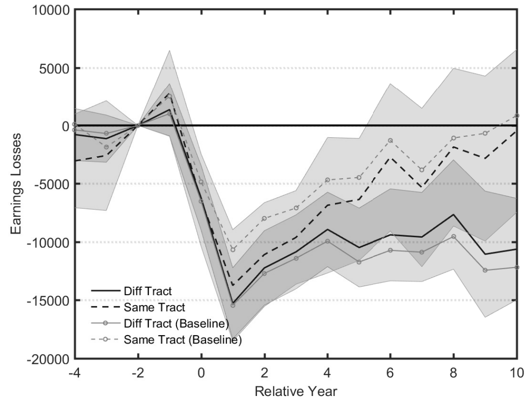
(a) Reweighted Means Unadjusted for Age Quartic



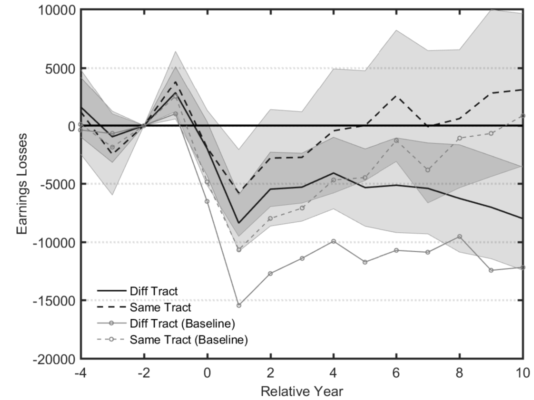
(b) Means After Removing an Age Quartic for Each Group

Appendix Figure 11: Mean Earnings For the Reweighted Control Samples

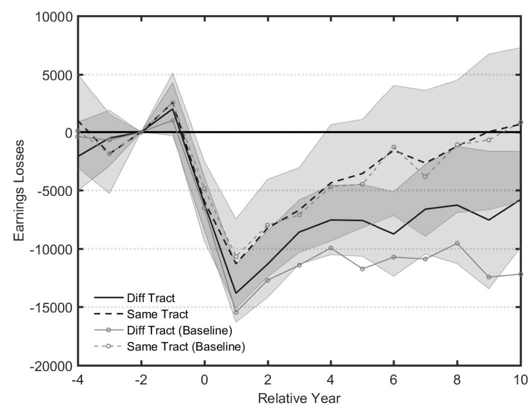
Note: Panel A shows that after applying the propensity score weights, but without adjusting for age quartics, non-displaced workers living close to their parents and farther away in relative year '-1' have similar earnings trajectories, except for a small level shift. Panel B shows the average earnings for the non-displaced after removing an age quartic. Not suprisingly, the differences that remain between the two groups are quite small.



(a) Baseline Income as Linear Interaction



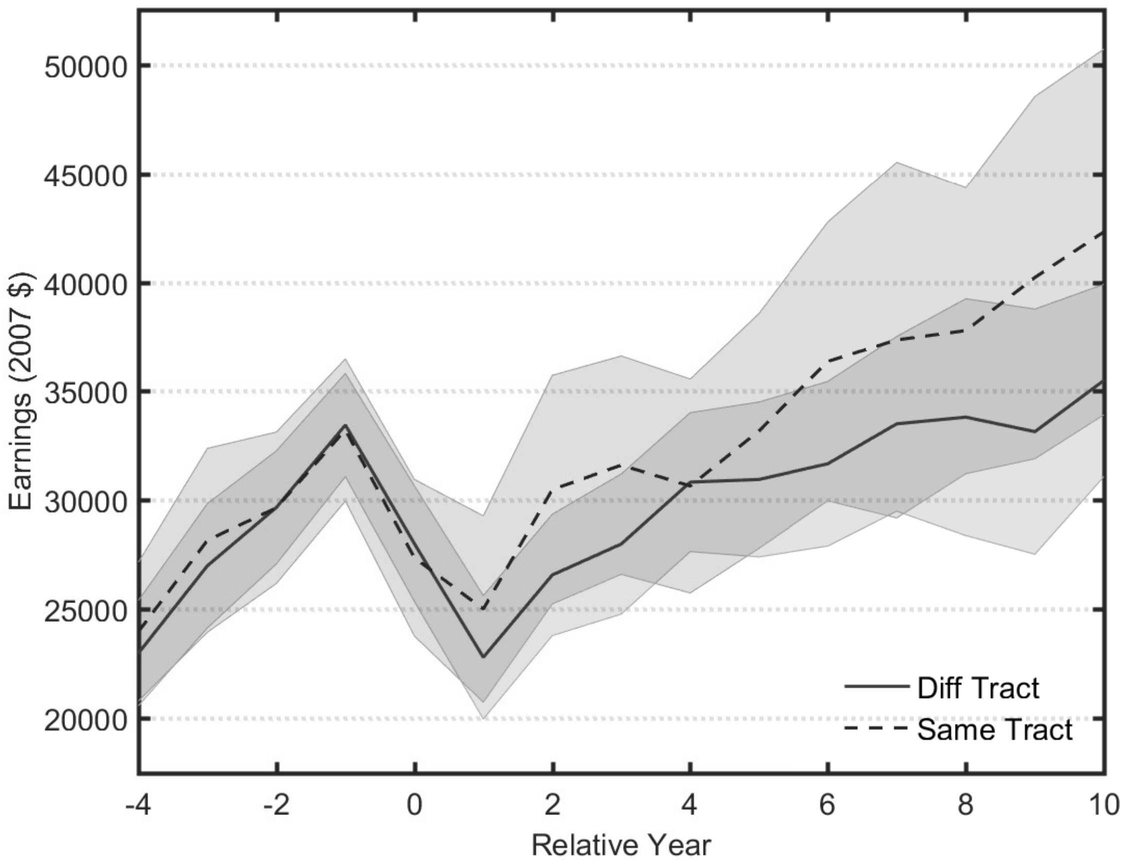
(b) Baseline Income as a Dummy Variable



(c) College Education as a Dummy Variable

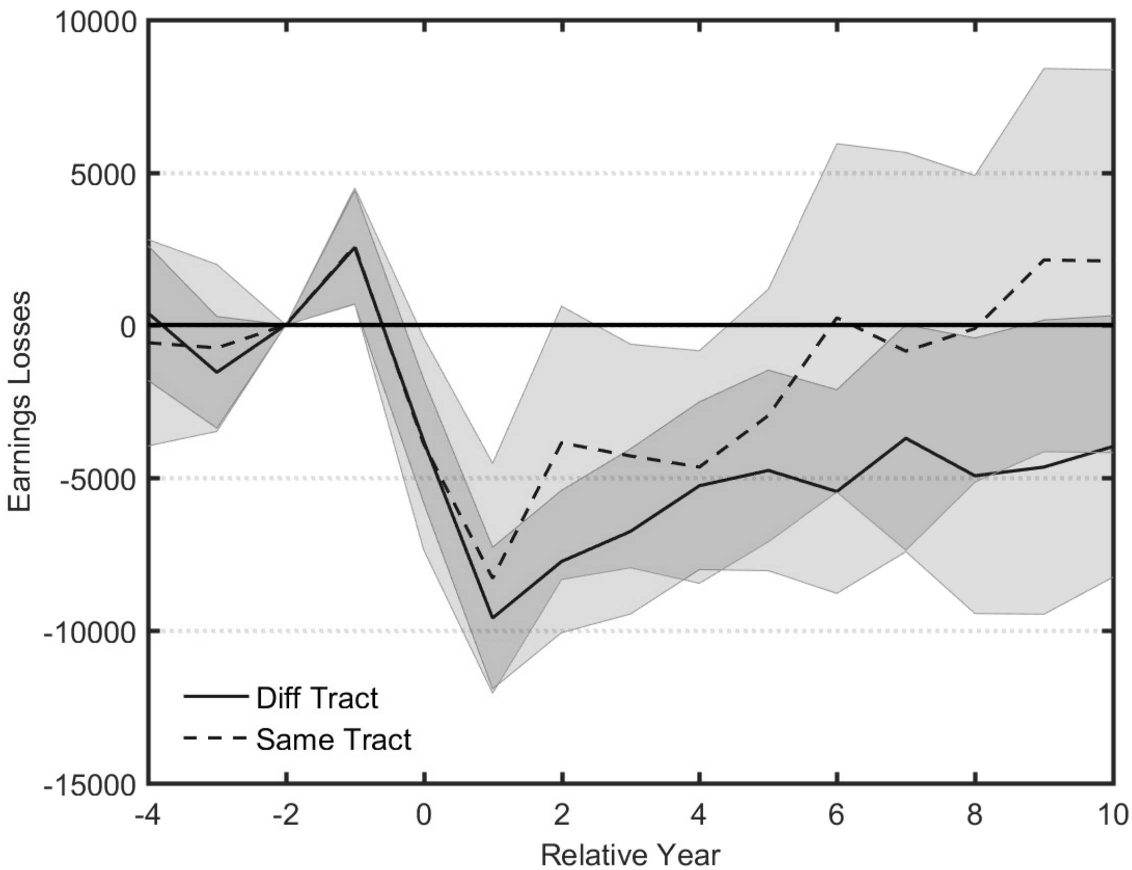
Appendix Figure 12: Including Additional Interactions in the Baseline Specification

Note: Additional interactions with the displacement dummies (equation 3) 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. Interacting with a college education dummy has a similar effect. Lighter lines reproduce the baseline results from Figure 2.



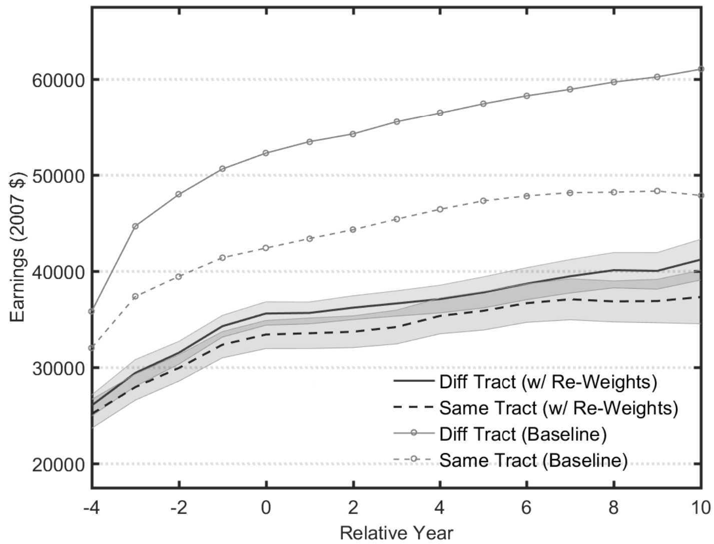
Appendix Figure 13: Means After Propensity Score Reweighting for Sample with Common Support

Note: Simple averages, after applying propensity score weights and using a sample with common support, still suggest that individuals living close to their parents have significantly better post-displacement earnings outcomes than those who live farther away. This figure is analogous to Figure 8 in the main text but with a sample of individuals that satisfy a common support criterion.

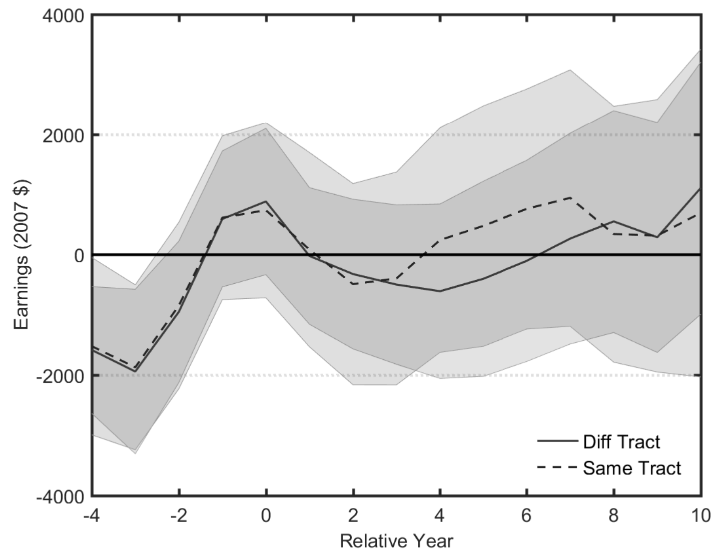


Appendix Figure 14: Regressions with Propensity Score Reweighting for Sample with Common Support

Note: The baseline earnings results survive the propensity score reweighting procedure even after we restrict to a sample of individuals who satisfy a common support criterion. This figure is similar to Figure 9 in the main text but with a sample of individuals that satisfy a common support criterion.



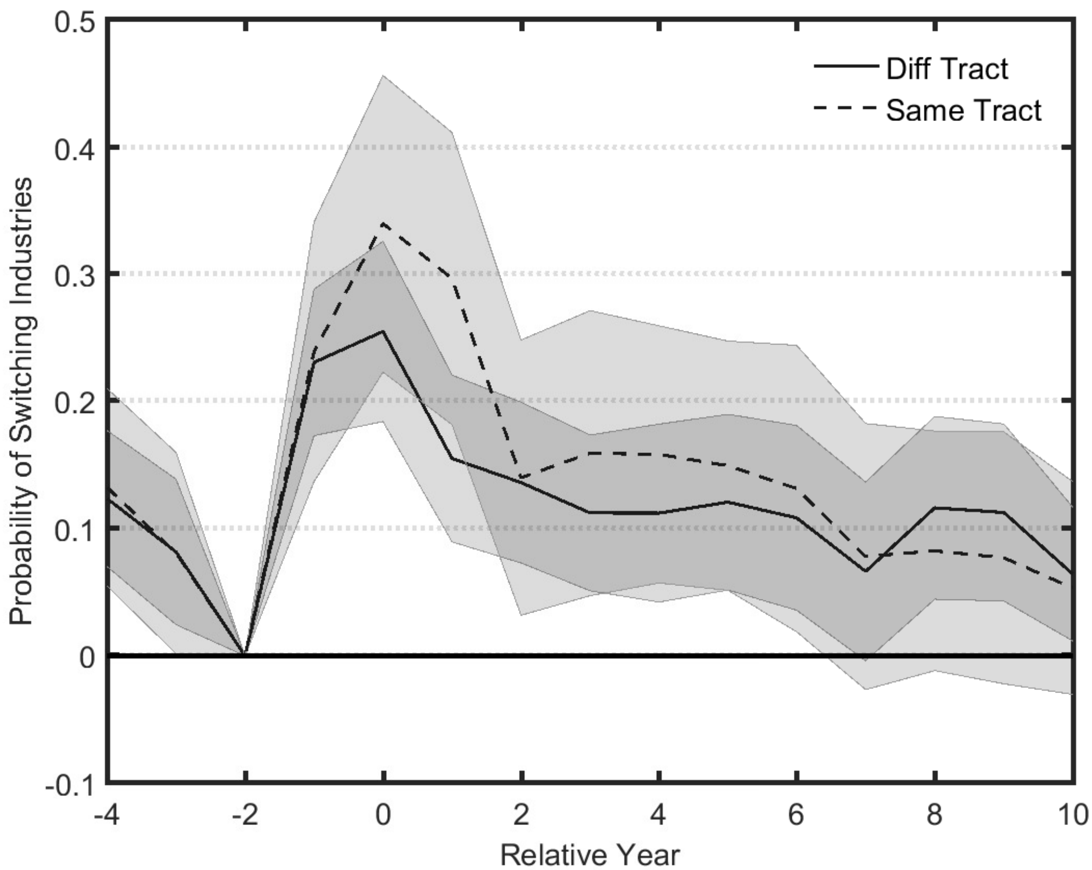
(a) Reweighted Means Unadjusted for Age Quartic



(b) Means After Removing an Age Quartic for Each Group

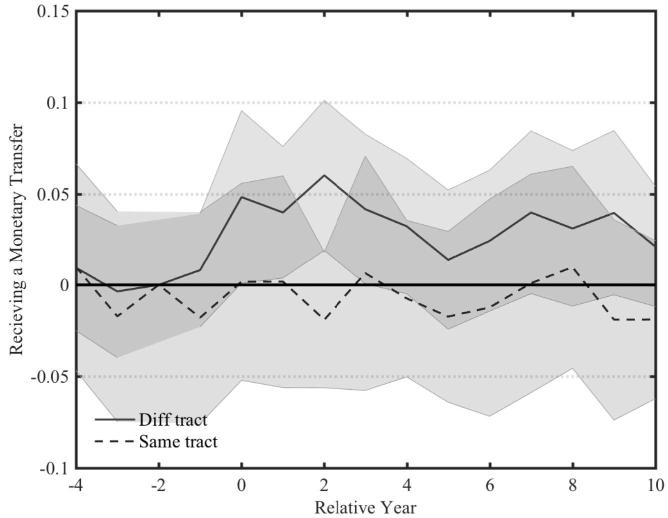
Appendix Figure 15: Mean Earnings For the Reweighted Control Samples Fulfilling the Common Support Condition

Note: Panel A shows that after applying the propensity score weights and restricting to a sample of individuals that satisfy a common support criterion, 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. This figure is similar to Appendix Figure 11 but with a sample of individuals that satisfy a common support criterion.

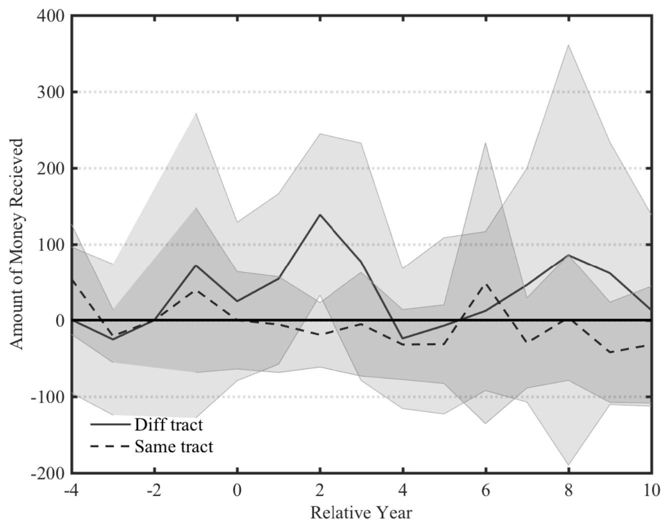


Appendix Figure 16: Probability of Switching Industries

Note: Those who live in the same tract as their parents prior to displacement experience higher industry switching probabilities at the time of job loss than those who live farther away.



(a) Receiving a Monetary Transfer



(b) Total Monetary Transfers

Appendix Figure 17: Monetary Transfers Around Displacements

Note: This plots event studies around displacement, as in Figure 2, for young workers. The dependent variable in the first panel is reporting receiving a transfer of money from friends or relatives, and in the second panel it is the amount of these monetary transfers. There is a noticeable increase in both about two years after a displacement for people who live in a different census tract from their parents, but not for people who live in the same census tract as their parents.

H Appendix Tables (For Online Publication)

	Panel A: PSID Weights				Panel B: Reweighted			
	Same Tract		Different Tract		Same Tract		Different Tract	
Variables	Displaced	Not Displaced	Displaced	Not Displaced	Displaced	Not Displaced	Displaced	Not Displaced
Earnings	30,644	33,044	35,751	36,563	30,644	30,141	30,167	31,840
	(1,660)	(680)	(1,656)	(442)	(1,660)	(651)	(1,136)	(603)
	1.00	0.04	0.00	0.00	1.00	0.35	0.46	0.91
Average Change in Earnings	1,927	1,761	2,699	2,443	1,927	1,783	3,161	2,196
	(516)	(158)	(455)	(102)	(516)	(179)	(446)	(132)
	1.00	0.36	0.96	0.37	1.00	0.23	0.70	0.54
Years of Schooling	11.91	12.51	12.47	12.76	11.91	12.15	12.12	12.20
	(0.17)	(0.08)	(0.12)	(0.06)	(0.17)	(0.09)	(0.13)	(0.08)
	1.00	0.00	0.00	0.00	1.00	0.84	0.86	0.89
Employer Tenure	5.35	5.89	4.62	5.62	5.35	5.11	4.97	5.10
	(0.31)	(0.14)	(0.20)	(0.09)	(0.31)	(0.11)	(0.26)	(0.08)
	1.00	0.00	0.56	0.00	1	0.88	0.80	0.97
Share Manager/Professional	0.12	0.19	0.18	0.21	0.12	0.16	0.13	0.15
	(0.04)	(0.02)	(0.03)	(0.01)	(0.04)	(0.02)	(0.02)	(0.01)
	1.00	0.11	0.02	0.00	1.00	0.90	0.69	0.90
Share in Goods Industries	0.57	0.47	0.53	0.39	0.57	0.50	0.52	0.42
	(0.05)	(0.02)	(0.04)	(0.02)	(0.05)	(0.02)	(0.04)	(0.02)
	1.00	0.06	0.68	0.00	1	0.32	0.73	0.02
U/e Rate in County	7.75	7.37	7.70	7.23	7.75	7.48	7.88	7.60
	(0.38)	(0.19)	(0.33)	(0.16)	(0.38)	(0.19)	(0.43)	(0.22)
	1	0.29	0.91	0.037	1	0.68	0.55	0.85
Age	27.95	28.46	28.22	28.37	27.95	27.74	27.61	27.86
	(0.29)	(0.10)	(0.22)	(0.06)	(0.29)	(0.10)	(0.24)	(0.07)
	1	0.01	0.06	0.00	1	0.25	0.25	0.39
Fraction Male	0.82	0.79	0.82	0.84	0.82	0.81	0.76	0.84
	(0.04)	(0.02)	(0.03)	(0.01)	(0.04)	(0.02)	(0.04)	(0.01)
	1	0.72	0.37	0.41	1	0.83	0.43	0.68
Number of Children	1.32	1.20	1.21	1.16	1.32	1.18	1.28	1.21
	(0.15)	(0.06)	(0.09)	(0.03)	(0.15)	(0.06)	(0.10)	(0.04)
	1.00	0.64	0.22	0.22	1.00	0.41	0.72	0.52
# of records	172	2776	335	7198	172	2776	335	7198

Appendix Table 1: Means of Reweighted and Initial Sample

This reports means for each group in the sample with common support using PSID weights in the first four columns and the propensity score reweights in the last four columns. For each variable, we report the mean, the standard error of that mean, and a p-value of a Wald test that this mean is the same as the value in the first column. Standard errors and p-values adjust for clustering at the individual level. "U/e" stands for "unemployment."

	Unemployed (A)	Unemployed (H)	Working (A)	Working (H)	Pooled (A)	Pooled (H)
Panel A: Young Workers						
$\mathbb{P}[\textit{any search activity}]$	0.88	0.86	0.070	0.068	0.080	0.094
$\mathbb{P}[\textit{checked w/ frnds or rels}]$	0.24	0.32	0.023	0.018	0.028	0.030
$\mathbb{P}[\textit{searched but not w/ frnds or rels}]$	0.62	0.72	0.062	0.061	0.071	0.087
Panel B: Older Workers						
$\mathbb{P}[\textit{any search activity}]$	0.66	0.75	0.039	0.039	0.048	0.059
$\mathbb{P}[\textit{checked w/ frnds or rels}]$	0.21	0.22	0.013	0.015	0.017	0.023
$\mathbb{P}[\textit{searched but not w/ frnds or rels}]$	0.53	0.63	0.036	0.032	0.042	0.055

Appendix Table 2: Summary Statistics of Search Intensity by Proximity to Parents and Labor Force Status

Note: Young workers living in the same neighborhoods as their parents are more likely to engage in search activities than young workers living farther away (pooled results). A similar pattern holds for older workers. Parenthetical (A) stands for "away," i.e. those not in the same neighborhoods as their parents at the time of the survey, and (H) stands for "home," i.e. those living in their parents' neighborhoods.

	(1)	(2)	(3)	(4)	(5)
	Unemployed	Unemployed	Unemployed	Working	Pooled
Same tract	-0.021	-0.075	-0.11	-0.0015	0.004
	(0.083)	(0.12)	(0.14)	(0.009)	(0.014)
Unemployment					0.77***
					(0.057)
Observations	1,751	1,513	1,513	57,527	59,040
R-squared	0.0002	0.037	0.087	0.021	0.14
Demographic controls	NO	YES	YES	YES	YES
Year FEs	NO	NO	YES	YES	YES
Individual FEs	NO	NO	NO	NO	YES

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Appendix Table 3: Any Job Search Activity for Young Workers

Note: Young workers (25 to 35 year olds) living in the same neighborhoods as their parents are no more likely to engage in search activities than young workers who live farther away. This does not depend on employment status. Standard errors adjust for clustering at the individual level.