Parental Proximity and Earnings After Job Displacements*

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Abstract

The earnings of young adults who live in the same neighborhoods as their parents completely recover after a job displacement, unlike the earnings of young adults who live farther away, which permanently decline. Nearby workers appear to benefit from help with childcare since grandmothers are less likely to be employed after their child's job displacement and since the earnings benefits are concentrated among young adults who have children. The result also suggests that parental employment networks improve earnings. Differences in job search durations, transfers of housing services, and geographic mobility, however, are too small to explain the result.

JEL codes: J61, J64, R23.

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

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1 Introduction

Why do so many young adults live within a few miles of their parents?[1] This lack of mobility, often explained by high moving costs, has prompted concerns about workers being stuck in depressed labor markets.[2] But young adults might also benefit from living close to their parents.

We document the labor market benefits of living close to parents by showing that the earnings of young workers who live near their parents recover after a job displacement. In contrast, workers who live farther away experience large, permanent declines in earnings, as in Jacobson, LaLonde and Sullivan (1993).[3] Nearby workers appear to benefit from childcare and job referral networks. However, workers do not appear to benefit from searching longer for a new job because they can move back in with parents.

Our baseline result is that 25 to 35 year olds who live in the same neighborhoods as their parents see their earnings recover after a job displacement.[4] Those who live farther away suffer large, permanent earnings losses. While job displacements are plausibly exogenous (von Wachter, Song and Manchester, 2009), children's locations are not randomly assigned, so our results are not necessarily causal. We do, however, find evidence suggestive of a causal mechanism: The results persist after we use a propensity score reweighting procedure to control for detailed characteristics of workers, the jobs they lose, and the places they live.

[1]In the Panel Study of Income Dynamics (2017), for example, the median 25 to 35 year old household head lives about five miles from a parent or an in-law and 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. Additionally, several studies suggest that people are migrating less than previously (Molloy, Smith and Wozniak, 2011; Kaplan and Schulhofer-Wohl, 2017; Mangum and Coate, 2018).

[2]Cowen (2017), Austin, Glaeser and Summers (2018), and Chetty et al. (2018) connect negative economic outcomes to people living in depressed areas. Bishop (2008), Kennan and Walker (2011), Diamond (2016), and Coate (2017) estimate large moving costs, which represent any factors that keep people from realizing the benefits of moving. A closely related literature focuses on elder care as a reason why workers stay close to their parents (Lin and Rogerson, 1995; Lin and Wu, 2010; Chari et al., 2015).

[3]For recent evidence, see Davis and von Wachter (2011) who find that even 20 years after a displacement event, average earnings losses are between 10 and 20 percent of pre-displacement earnings. Early literature reviews include Hamermesh (1989), Fallick (1996), and Kletzer (1998).

[4]As in Hellerstein, Kutzbach and Neumark (2015), for example, we use census tracts as a measure of neighborhoods.

Unobservable differences that remain after reweighting would lead to the opposite pattern – lower earnings after a job displacement – if workers live close to their parents because they care for them, because they struggle with change, or because of particularly high moving costs.[5]

We also find evidence that childcare provided by grandmothers leads to these labor market benefits after displacements. Convenient childcare would be particularly valuable after a job displacement if it helps workers to deal with the higher rates of divorce, declines in physical and psychological well-being, and social withdrawal that accompany a job displacement (Charles and Stephens, 2004; Burgard, Brand and House, 2007; Brand, 2015). Indeed, three pieces of evidence illustrate how grandmothers' help with childcare matters for earnings after a displacement. First, the effects are concentrated among people who live in the same neighborhood as their parents. Second, nearby grandmothers are less likely to work after their adult child's job displacement, consistent with grandmothers devoting more time to childcare. Finally, we find that the earnings recoveries among workers living closer to their parents only apply to workers who have children of their own. Our results extend the findings in Compton and Pollak (2014), who document that the availability of childcare from grandparents increases the labor supply of married women, to the earnings of a mostly-male sample after a job displacement.[6]

Our results also suggest that local social networks help to insure earnings after a job displacement, as in Corak and Piraino (2011), Kramarz and Skans (2014), and Topa and Zenou (2015). However, we find only some evidence that displaced workers find jobs in a parents' industries.

[5]Generally speaking, if workers who live closer to their parents are worse off in unobservable ways, then we would underestimate the benefit of living close to one's parents. In terms of observables these workers are worse off – for example they are less educated and they live in places with worse labor market conditions.

[6]Although previous work has focused on the labor market outcomes of women, the effects on both spouses are likely to be magnified after a job displacement. Wives increase labor supply after husbands' job losses (Stephens, 2002), men contribute more to childcare as their wives time in the labor market and earnings share increases (Raley, Bianchi and Wang, 2012), and men have faced larger wage penalties for labor market interruptions, including those due to childcare (Stafford and Sundström, 1996; Spivey, 2005). The presence of grandparents can therefore help mitigate these wage penalties for men following a job displacement.

Broadly, our results suggest that parental proximity provides labor market insurance to young workers, as opposed to it only restricting labor market options and imposing additional burdens. Indeed, we show that our results are consistent with a simple model where parents facilitate higher-wage job offers for their children. In the model a preference for living at home will restrict workers' labor market options and lead to the opposite pattern of earnings from our baseline result. Empirically, our baseline result also applies to workers who do not move after a job displacement so restricted labor market options are an unlikely explanation. Finally, the additional burden of caring for nearby parents seems to outweigh any benefits for older adult children, as these workers experience no earnings improvements associated with parental proximity.

Our results support previous research (Kaplan, 2012; Dalton, 2013; Munshi and Rosenzweig, 2016) showing that nearby parents provide labor market benefits to young workers, but three pieces of evidence suggest that the results are not because parents facilitate longer job searches by transferring resources or allowing children to move back in with them. First, we find that children who live closer to their parents spend similar amounts of time unemployed and work a similar number of hours after a displacement. Second, we find that children benefit from parental proximity when they live in the same neighborhood as their parents, but outside of their houses. Third, we find only small increases in transfers of money and housing around displacement. And transfers of money and housing are not any larger among workers who lived closer to their parents before a job displacement.

Our findings are particularly relevant for discussions of place-based policies.[7] They suggest that replicating services that parents provide with childcare and job leads could increase worker mobility. They also suggest that providing childcare services could improve earnings after job displacements. Additionally, they highlight the difficulties involved in encouraging workers to move away from their extended families in economically depressed places (discussed in Zabek, 2018) since workers seem to benefit in the labor market when they live close

[7] Glaeser and Gottlieb (2008), Kline and Moretti (2014), Neumark and Simpson (2015), Austin, Glaeser and Summers (2018), and Criscuolo et al. (2019) review place-based policies.

to their families.

The rest of the paper proceeds as follows. Section 2 describes our data, describes our sample, and presents our main earnings results using averages. Section 3 presents these earnings results with our baseline regression, 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 including interactions with additional characteristics. Section 5 investigates possible mechanisms related to childcare, housing transfers, and parental employment networks. Section 6 discusses selection and unobserved heterogeneity in the context of our results and presents some back-of-the-envelope calculations that use our empirical results to calculate the value of living close to one's parents. 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

The dataset for our analysis must contain three elements. First, it must have information about where respondents live alongside job-history information sufficient to identify displacements and to measure earnings. Second, it must include repeated observations so we can use a difference-in-difference approach. Third, it must link parents and children. To our knowledge, the PSID is one of few datasets that meets all of these requirements.[8]

The PSID began in 1968 with an interview of approximately 5,000 households, and follows any new households formed from the original group. Due to the genealogical nature of the PSID we have information about adult children and their parents in each wave if they choose to respond. At the time of the survey, the PSID collects information about people's labor

[8]Other possible sources of publicly available data are the National Longitudinal Survey of Youth (NLSY), or the Survey of Income and Program Participation (SIPP). The advantage of the PSID relative to each of them in this context is the amount of data it collects about parents and children over many years. This is particularly true relative to the SIPP, which only contains information about respondents' birth states.

market experience, including their earnings during the previous calendar year. We use a restricted-use dataset to identify the census tract of each household, which we refer to as their neighborhood.[9]

Our sample contains PSID "household heads" who are between the ages of 18 to 62. The PSID follows Census Bureau procedures from the late 1960s that define a household head as the male member of a heterosexual couple, as long as the couple had been living together for at least one year, and as long as he was not incapacitated. We restrict the sample to the 1968 to 2013 waves of the PSID's Survey Research Center (SRC) and Survey of Economic Opportunity (SEO) samples and we use logitudinal weights for our analyses. Our results are similar when we include both heads and wives and when we perform unweighted analyses that exclude the SEO sample (Appendix A).

Our main results are for job displacements that occur when workers are between 25 and 35 years old, though we also present results for job displacements that occur when workers are between 36 and 55. Job displacements are determined from questions that are asked to employed and nonemployed individuals. Employed individuals who have less than a year of tenure with their present employer (and in some survey years, individuals who started their current job after January 1 of the previous calendar year) are asked: "What happened to the job you had before?" Nonemployed individuals are asked: "What happened to that employer (job)?" (the individual's previous job). The two categories of responses used to identify displacements are "plant closed/employer moved" and "laid off/fired." As is standard in the displaced worker literature, we also impose that workers had at least two years with their employer and were working full-time before the displacement event so that our workers have a strong connection to the labor market. Our results are qualitatively similar with different definitions of attachment.[10]

[9]Since we use detailed geographies coming from administrative records, we performed all of our analyses in a virtual desktop infrastructure (VDI) that includes the main PSID files alongside information about each household's census tract in each year that the PSID has this available. Other researchers can apply for access to these data through the same infrastructure; instructions are on the PSID's website.

[10]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

We construct the analysis dataset 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 either their parents or their in laws at the time of the previous interview.[11] We repeat this procedure for every base age between 25 and 55 and stack all the samples to create the final dataset.[12,13] To track when workers are displaced or not, we set the *relative year* to zero in the base age, one in the year after, etc. For example, for the base age 40, the relative year is -8 when workers are 32, zero when workers are 40, and 6 when workers are 46.[14]

Table 1 shows the summary statistics for the final sample, which we restrict to observations with non-missing parents' location information.[15] Since we are using the PSID's poverty (SEO) oversample, and hence sampling is likely endogenous to our outcome (earn-

sample sizes in our context.

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

[12]Note that workers 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 worker level to account for these multiple observations.

[13]We use an unbalanced panel of data that was collected about workers when they were 18 to 62. Displacements are only included if they are reported in surveys from 1969 to 1997, while respondents are from ages 25 to 55. Our restriction to displacements from 1969 to 1997 is to preserve the interpretation that someone was displaced in the previous year, since the 1968 survey asks about displacements in the last ten years, and surveys after 1997 ask about the previous two years (the PSID is a biannual survey after 1997). We restrict to base ages between 25 to 55 in order to insure that workers have an attachment to the labor force.

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

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

ings), we take the suggestion of Solon, Haider and Wooldridge (2015) and use the longitudinal weights provided by the PSID throughout the analysis, though we also show that our baseline results are robust to omitting them in Appendix A. 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 1,460 displacement events, of which 320 took place while a worker resided in their parents' neighborhoods and approximately 1,140 occurred while a worker was not in their parents' neighborhoods. The average annual displacement probability is around three percent in our sample.[16] Before displacement, the displaced workers are slightly younger, less educated, earn less, and have been with their employer for less time than their non-displaced counterparts. 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. We define our two age groups as 25 to 35 and 36 to 55 so that we have an equal number of displacement events in both groups (around 700) and to be broadly consistent with the definitions in Kletzer and Fairlie (2003) and Kaplan (2012).

2.2 Some Preliminary Evidence

Figure 1 provides the first evidence that parents insure workers' earnings against the dramatic consequences of a job loss. The top panel in Figure 1 presents the average, real (2007 CPI-U-X1) earnings of workers who were displaced (dashed) and not displaced (solid) when they were between base ages 25 to 35. The bottom panel of Figure 1 separates out average earnings for workers who were in their parents' neighborhoods in relative year -1 (light gray), and those who were not in their parents' neighborhoods (dark gray).

The top panel of Figure 1 delivers three messages that many prior studies have documented. First, displacement leads to a large initial drop in earnings. Annual earnings

[16] This is consistent with displacement 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.

drop by around $10,000, or around 20 percent of pre-displacement earnings.[17] Second, while earnings for these displaced workers recover, this recovery does not exceed the earnings gains experienced by the control group of non-displaced workers. Even 10 years after the displacement event, the earnings of displaced workers have not caught up with the earnings of non-displaced workers, despite earnings recovering to their pre-displacement levels after around six years. Finally, there do not appear to be differences in the trends of earnings prior to the displacement event.

The bottom panel of Figure 1 shows our main finding: the earnings of workers who live in the same tract as their parents are lower on average, but these workers' earnings appear to recover after a job displacement. Displaced workers who were not in the same neighborhoods as their parents see large earnings losses relative to a group of workers who were not displaced and not in the same neighborhoods, and this gap persists over the next 10 years. In stark contrast, those workers who were in the same neighborhoods as their parents in the year prior to the displacement event see a much healthier recovery in earnings. Prior to the displacement event the difference in the earnings of the displaced and non-displaced who live in their parents' neighborhoods is around $3,000. The earnings of the displaced workers 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.[18]

In the next two sections we verify these results with a standard displaced worker specification (Section 3), which controls for, among other things, worker fixed effects, and a

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

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

propensity score reweighting exercise (Section 4), which also controls for observable differences between those in their parents' neighborhood and those farther away. The preliminary results presented in this section are robust to these more sophisticated methods.

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:

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

Here e_{iat} is the annual earnings of worker i in calendar year t when the base age is a, α_{ia} represents a worker-base-age dummy, γ_t controls for calender year fixed effects, the X_{iat} terms control for an age quartic, and H_{ia} is a dummy variable indicating whether worker i was neighbors with their parents in the year prior to age a. This dummy is interacted with the age quartic in X_{iat} to allow for different age-earnings profiles for those living near their parents and farther away, which captures the different counterfactual (non-displaced) earnings trajectories for these two groups that we observe in Figure 1. The variable D_{iat}^k captures whether worker i at time period t and base age a was displaced k periods ago. The -4 dummy includes anybody who is four or more years before a displacement and the $+10$ dummy includes anybody who is ten or more years after a displacement. We omit the -2 dummy so all results are relative to two years before the displacement event.[19] As a

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

result, the coefficient δ^k captures the change in earnings for a worker who was displaced k periods ago and was not living in their parents' neighborhoods prior to the displacement relative to other workers who were not neighbors with their parents and were not displaced.[20] 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 how other factors, correlated with living in the same neighborhood as one's parents, might explain the differential impact of displacement on earnings.[21] 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 workers 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, 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

[20]This approach is most closely related to the approaches taken by Davis and von Wachter (2011) and Huttunen, Møen and Salvanes (2016). See Krolikowski (2018) for a more thorough discussion of choosing a control group for displaced workers.

[21]We follow Kletzer and Fairlie (2003), who also focus on young workers, and estimate the earnings model in equation (1) without worker-specific time trends. For young workers there exist relatively few pre-displacement earnings observations so we think that worker-specific trends are unlikely to be well estimated. Also, as we shall see, the post-displacement effect of being in the same neighborhood as one's parents on earnings is gradual and builds over time so it might be incorrectly attributed to worker-specific trends if these were included in the estimating equation. Finally, our specification includes worker-base-age dummies, α_{ia}, which vary within workers by base age and therefore likely already pick up worker-specific trends in earnings.

displacement event, with earnings losses indistinguishable from a full recovery after four years. The difference between the earnings of the two groups is statistically significant at longer horizons. 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 person worked positive hours in the previous calendar year, the number of hours worked during the previous calendar year (conditional on positive hours), and earnings per hour. The top panel shows the probability of positive hours last year. This falls during the survey after the displacement event, as some workers experience an entire year out of work. The graph suggests that displaced workers, regardless of location, are around 4 percentage points (pp) less likely to have employment in the year after the displacement event than non-displaced workers. Although the recovery appears slightly 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. We cannot reject, however, the null hypothesis that the hours recoveries are the same for the two groups.

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, workers who lived in the same neighborhoods as their parents at the time of displacement see their wages recover fully, and workers who lived farther away see 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 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 Heterogeneous Effects of Parental Proximity

This section investigates heterogenous earnings effects based on the age and location of workers who lose their jobs. We find that older workers do not benefit from living closer to their parents, and that the effects for young workers are strongest when children live in the same neighborhood as their parents as opposed to farther away. We also find that differences in post-displacement mobility and local labor market characteristics do not appear to affect our main findings.

Older workers, 36 to 55, who live in their parents neighborhoods do not appear to have any improvements in their earnings after a displacement relative to workers who live farther away, as shown in Figure 5. Older workers who live near parents actually appear to earn even less after a displacement, but we cannot reject the null hypothesis that the post-displacement earnings effects are the same for the two groups. This discrepancy could reflect a change in the direction of resource flows, since older workers are more likely to be caring for elderly parents. For example, Chari et al. (2015) estimate that the annual opportunity cost of informal elder care in the US is $522 billion. Additionally, Lin and Wu (2010) find that a child is a source of informal support for about 35% of people 65 and older who had difficulties with instrumental activities of daily living. Lin and Rogerson (1995) also provide a more general discussion about the determinants of the how far elderly parents live from their adult children.

Returning to young workers, we find that the earnings differences are concentrated among workers who live in the same neighborhood as their parents and are not driven by coresidents. 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.[22] Those living close to their parents, but not in the same neighborhoods, experience similar post-displacement earnings outcomes than those who live farther away. Appendix Figure 9 shows that earnings outcomes look similar when we look at workers who are coresiding with their parents as opposed to living in the same neighborhoods as their parents.[23]

We find that parental proximity matters for young workers independently from where

[22] We augment equation (1) by including an interaction of the age quartic with an indicator for whether a worker is in the same commuting zone, but not the same tract, and for whether a worker is in the same tract as their parents. Additionally, we interact the displacement dummies with whether a worker 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.

[23] Using a distance-based measure of proximity (based on latitudes and longitudes of block groups) also gives similar results (Appendix Figure 7).

they grew up and our earnings findings are not explained by local labor market conditions at the time of displacement. The earnings benefit for those who live closer to their parents persists even if one includes additional interactions of the displacement dummies in equation (1) with whether the worker was displaced while living in the county that they grew up in (Appendix Figure 10). This suggests that close parental proximity has an independent effect on post-displacement earnings from other factors in a worker's home county. Appendix Figure 6 shows that our results are similar when we include controls for local labor market conditions to address the possibility that children may prefer to stay in better labor markets. In fact, we find that children who reside near their parents tend to live in counties with worse local labor market conditions, which tends to increase the earnings losses following displacement (Carrington, 1993).[24]

Geographic mobility is a plausible explanation for the effect, particularly since Appendix Figure 4 documents a large impact of job displacement on regional mobility.[25] We check that the post-displacement earnings trajectories are not driven by "movers" by restricting the sample to workers who remain in the same county for all of the years that we observe them after the displacement event. Figure 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 resided close to and farther away from their parents are equally pronounced for this restricted sample. Therefore, post-displacement mobility patterns are unlikely to account for our baseline earnings results.

[24] 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, 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.

[25] This is similar to the findings in Huttunen, Møen and Salvanes (2016) and Cao and Stafford (2017). See Mincer (1978) for early work on family ties and migration decisions. Molloy, Smith and Wozniak (2011) and Kaplan and Schulhofer-Wohl (2017) also provide a more extensive analysis of recent trends in inter-state migration.

Since young workers who live quite close to their parents drive the recoveries in post-displacement earnings, any mechanisms that lead to these effects would have to be particularly relevant for these workers. This is why we focus on three advantages that young workers have when they live in the same neighborhood as their parents. The first is the availability of childcare, the second is the possibility of moving back home, and the third is improved employment networks. Before turning to these mechanisms in Section 5, we investigate whether the benefit of parental proximity for post-displacement earnings is plausibly causal.

4 Propensity Score Reweighting

In this section, we show that our results are robust to using a propensity score reweighting technique that chooses a comparison group of young adults, aged 25 to 35, who live farther from their parents, but who lose similar jobs and who have similar characteristics as those who live closer to their parents. Workers who live closer to their parents are less educated, earn less, and tend to live in places with lower employment-to-population ratios, so it is possible that these differences lead to differences in post-displacement earnings.[26] Through our reweighting, we emphasize workers who are similar in these dimensions, but who live farther away. This allows for heterogeneous effects of a job displacement on earnings owing to these observable characteristics.

The reweighting does not control for unobservable difference between the two groups, but we suspect that these are unlikely to change our results for two reasons. The first is that our results do not appear to be sensitive to either including additional characteristics, or taking characteristics away from our reweighting procedure. Appendix B (Appendix Figure 11) presents the main results from the reweighting with different sets of characteristics, and the results tend to be quite stable once one controls for basic educational and demographic

[26] Since we use worker fixed effects in our regressions, for these differences to affect our earnings regression results, they would have to have a larger effect on earnings after displacement. For example, this would be the case if workers who lost jobs with lower wages tended to have smaller earnings losses after displacements.

differences.[27] The second is because we suspect that the bias would go in the other direction. A worker who knows that they are less able to adapt to new circumstances should prefer to live closer to their parents, since moving involves adapting to new circumstances and since parents can act as supports. If this is the case, then selection on unobservables should lead to more severe effects of job displacements among workers who live in their parents' neighborhoods.

4.1 Methodology

Following the literature on propensity score reweighting (Rosenbaum and Rubin, 1983; Hirano, Imbens and Ridder, 2003), we reweight observations from different groups of young adults so that they have the same characteristics as young adults who are displaced while living closer to their parents. This involves reweighting three separate groups of workers so that each group has similar observable characteristics as the group of displaced workers who lived closer to their parents. To accommodate multiple groups, we follow Imbens (2000).

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

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

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

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

same characteristics as this group.

We can recover the conditional and unconditional probabilities in a semiparametric way using sample averages and logit regressions with flexible functional forms. We estimate the probabilities conditional on X_{ia} using a multinomial logit regression, as suggested by Imbens (2000). The predictors relate both to the workers and to the jobs that they lose. In terms of the worker, we include a dummy for being college educated, a linear term for the worker's completed schooling, a linear term for the worker's age, a dummy for whether the worker is male, a dummy for whether the worker is African American, and the employment-to-population ratio in the worker's county. In terms of the jobs that workers lost, we include a linear and a square term in earnings, the average year to year changes in earnings, wages, a dummy for one-digit PSID occupations, and a linear term for the worker's tenure.[28] The unconditional probabilities in equation (2) are the proportion of the sample made up by the group.

Table 2 shows a validation of the weights for young adults using several of the covariates in X as well as some other variables that were not included in the reweighting. It reports the mean of the variable among different groups of workers and p-values of a Wald test of equality with the group of people who were young and lost their jobs while living in the same neighborhood as their parents. In keeping with our regression analysis it includes each person separately for each year they were in the sample of people at risk for a displacement. Panel A shows these statistics using the initial PSID person weights 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 C that average earnings are similar between the reweighted groups of workers who were not displaced (Appendix Figure 12).[29]

[28] All variables are measured pre-displacement. 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).

[29] 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 D (Appendix Figure 13). In Appendix Figure 14, we implement the same propensity score approach, but use only a subset of

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

Figure 9 shows the baseline regression specification (equation 1) with the new weights.[30] It confirms that the inverse probability reweighting procedure produces similar qualitative results as the main specification. The differences between groups are smaller, but they remain both economically and statistically significant. As before, there are substantial drops in earnings following a displacement, though the initial losses are roughly equal and smaller at around \$10,000. A steadily increasing difference in earnings emerges in the years after displacement, however, as the group who lives closer to their parents makes up much of the earnings penalty from the displacement. People living closer to their parents see no detectable earnings losses in years four through ten, and they have essentially zero estimated earnings losses from year six onwards, with some positive point estimates. Statistically, the group of workers who were living closer to their parents earns significantly more than the other group in years six and ten, at the five percent level. These differences do appear to be economically significant as well, with a difference of around \$8,000, or about 10 percent of

observations where there is strong common support according to the selection method proposed by Crump et al. (2009) with similar results.

initial earnings. People who live farther away from their parents have permanent earnings losses, and their earnings are always statistically significantly different from zero after the displacement.

5 Investigating Mechanisms

This section assesses three possible mechanisms that could lead children who live closer to their parents to earn more after job displacements. We investigate grandparent's help with childcare, parents facilitating longer job searches by allowing children to move into their houses, and parents providing job leads in their industries.

First, we find evidence that grandmothers provide labor market insurance to their children by being available for childcare after a job displacement. Second, we assess whether proximity to parents is associated with increased monetary or housing transfers after job loss, which could help adult children smooth consumption and extend their job search, as in Kaplan (2012). Although some children move back in with their parents post displacement, the implied transfers of housing services are quite small and workers who live close to their parents do not appear any more likely to benefit from living with their parents after a displacement. Workers who live near parents also receive fewer explicit cash transfers from their parents (Appendix F.2), search about as intensively (Appendix Table 3), and spend about the same amount of time unemployed as workers who live farther away (Section 3.2). Finally, we find that children who live close to their parents are more likely to be employed in the same industry as their parents after a job displacement, though the estimates are noisy.

[30] 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. The main difference with their approach is we use reweighting, while they rely on nearest neighbor matching.

5.1 Grandmothers and Childcare

One advantage of living in the same neighborhood as one's parents is the availability of childcare from grandparents. Childcare could be particularly valuable after a job displacement when workers experience heightened rates of divorce, lower self-rated health, higher rates of depression, and increased social withdrawal (Charles and Stephens, 2004; Burgard, Brand and House, 2007; Brand, 2015). So this section collects evidence about childcare from grandparents after a job displacement. First, we look at the employment responses of mothers after their child's displacement based on the idea that providing childcare for grandchildren may crowd out their own employment. Second, we test to see how earnings evolve for a sample of workers who live in the same neighborhood as their parents, but who never have children of their own. Each gives suggestive evidence that nearby grandmothers aid their children by caring for grandchildren after a child's job displacement.

We estimate the effect of a child's displacement on mothers' employment using a modified version of our baseline regression specification broken out by three levels of geography, similar to Figure 6. The dependent variable is an indicator for mothers working positive hours in each year.[31] Based on the same logic as the baseline specification, we include controls for worker by base age fixed effects, year fixed effects, and quartic terms in both the workers' and the mothers' ages. Additionally, we include covariates to address the possibility that local economic shocks could lead both the worker and the worker's mother to lose their jobs. These additional controls include local employment to population ratios and a series of indicators for the occupation and the industry that the mother worked in last year.[32]

Mothers who live in the same tract as their displaced children are less likely to be employed after their child's displacement. Figure 10 shows that these mothers are almost 25

[31] As in the rest of the paper, the mother could be the parent of either spouse in the household. In rare cases where we observe both we use values from whomever lives closer. We also cluster our standard errors at the level of these mothers.

[32] The lag is to avoid a mechanical relationship between becoming employed and changing occupations or industries. When we omit these additional controls, the the earnings' recoveries of children in the same tract and the same commuting zone show the same general patterns but the differences are not as large.

percent less likely to be employed seven years after their children have lost their jobs. Mothers who live in the same tract as their children also are significantly less likely to be employed than mothers who live in the same commuting zone as their children, suggesting that the effects are not driven by shocks at the level of local labor markets. Another factor that makes it unlikely that the effects are driven by correlated shocks is that the effects occur several years after children have lost their jobs.[33]

Improvements in earnings after a displacement are due to faster earnings recoveries among displaced workers who both live close to their parents and who end up having children. Figure 11 shows this by plotting coefficients from a specification that interacts the effect of a job displacement with both whether the worker lived near parents and whether the worker ever lived in a household with children as either a head or a wife. Panel A applies to workers who end up having children, and it shows stronger earnings after displacements among workers living closer to parents. But Panel B shows no such differences among workers who never have children.[34]

5.2 Housing Transfers

To see if our results are due to children being able to move in with parents who live nearby, we estimate the cash value of parents allowing children to live with them, and we investigate how these housing transfers change around job displacements. Kaplan (2012), in particular, emphasizes that housing transfers can help children to earn more after job displacements by allowing them to be more selective about job offers, and by facilitating investments in

[33] Appendix Figure 15 performs the same exercise for fathers. It does not show meaningful evidence that fathers who live in the same tract as their children are less likely to be employed after the child's displacement. However, this is somewhat dependent on one's interpretation of a one year spike in employment rates for fathers who live in the same tract as a child two years before the child loses a job.

[34] Since the decision to have children could be affected by a job displacement (Lindo, 2010; Huttunen and Kellokumpu, 2016), Appendix Figure 16 examines if there were differences in fertility among workers who lived in the same tract as their parents after a displacement. This figure shows some evidence that workers who lived farther from their parents were less likely to have children after a job displacement, but no evidence of any effect among workers who lived in the same tract as their parents. This suggests that help with childcare after a job displacement could both be beneficial for the careers of workers with very small children and also that this childcare could lead more of these workers to have children.

their careers. If housing transfers drive our results, we would expect to see increases in the amount of in-kind housing transfers among workers who lived closer to their parents before they lost their jobs.

We use two complementary approaches to measure housing transfers, which may be under-reported in surveys like the PSID. The first records people who report that they receive all of their rent as a gift. The PSID only asks about this if people report that they pay no rent, however, so this approach misses people who pay below-market rents.[35] To address the possibility that some households pay below-market rents to live with their parents, we also construct an estimate of how much the child saves by living in their parents' household.

The second approach backs 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. When a respondent child moves in with a parent, the PSID classifies the household as two different families living within a single housing unit. In these situations, the interviewer 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.[36]

Table 3 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

[35] More precisely, this includes 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.

[36] 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 of the procedure in Appendix F.

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.

According to Table 3, a relatively small proportion of 25 to 35 year olds receive transfers of rent, and these transfers are modest relative to both average rents and the earnings losses after a displacement. According to both measures, less than ten percent of the sample receives 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 much smaller than the average rent of around $6,800 and the estimated earnings losses of around $15,000 in the year after a displacement.[37]

The regression coefficients plotted in Figure 12 suggests that housing transfers may spike around displacements, but that the spikes around displacement are economically small and statistically insignificant. There is no evidence that there are larger increases in housing transfers among people who lived closer to their parents before they lost their jobs. In fact, there is some suggestion that people who live closer see decreases in housing transfers after a displacement.[38] The point estimates in Panel A show that households are around four percent more likely to receive all of their rent as a gift in periods around displacements. A four percent increase is quite large relative to the two percent likelihood in the baseline sample, but it is a small slice of the overall population. Panel B shows the dollar values of the transfers involved; it also suggests that the transfers are modest at best. The implied

[37]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.

[38]Results for reported transfers of money, presented in Appendix F (Appendix Figure 18), 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.

rent savings estimates are noisy, but we can reject that there is an increase of $500 or more per year coming from a PSID parent. This is at least an order of magnitude smaller than the earnings losses after a displacement.[39]

5.3 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).[40] 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 at jobs with favorable wages and wage growth.[41] 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.

Table 4 presents some summary statistics on the probability of workers' working in their parents' industries. The table reports the fraction of workers employed in the same one-digit industry as their parents. We focus on employed individuals because when unemployed, the industry of an individual is the industry they were last employed in.

The table suggests that, on average, young workers living in the same neighborhood as their parents are slightly less likely to be working in their parents' industry than those living farther away. Workers in both groups have around a 25 percent probability of working in their parents' industry.

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

[40] Granovetter (1995) and Ioannides and Loury (2004) also 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. Additionally, work by Bayer, Ross and Topa (2008) and Hellerstein, Kutzbach and Neumark (2015) also find referral networks operating at the neighborhood level.

[41] There is some debate about how referral networks affect workers' wages. For example, Dustmann et al. (2016) find that hires from employment networks raise wages, while Bentolila, Micehacci and Suarez (2010) suggest that networks may reduce wages because they might assign workers 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…"

We estimate a specification that is similar to our baseline regression (equation 1), where the outcome variable is an indicator of whether the young adult child is employed in the same one-digit industry as their parent. To control for local industry composition, we include employment industry shares at the county level from County Business Patterns (CBP) data.

Figure 13 shows the probability of working in a parent's industry rises in the years after displacement and this increase occurs only for workers living in the same neighborhood as their parents, although the results are noisy.[42] The effect is relatively large with a 10pp increase in working in a parent's industry on a base of around 25 percent (Table 4), although the differences are not statistically significant at the five percent level (at relative years two, three, and four the p-values are 0.08, 0.06 and 0.10, respectively). Our results imply that for those who are displaced and living in the same neighborhood as their parents, the probability of working in the same industry as their parents is elevated several years before the displacement event. Appendix Figure 19 shows that older workers living close to their parents do not tend to move into their parents' industries after a displacement event. Appendix Figure 20 shows that workers living in the same commuting zone as their parents do not experience a significant increase in their probability of working in their parent's industry after a displacement event. These results are consistent with our findings that older workers and workers living farther away from their parents do not experience a post-displacement earnings benefit from parental proximity. We think our results suggest that parental employment networks may be operative for young displaced workers when they live in the same neighborhood as their parents, consistent with related work by Bayer, Ross and Topa (2008) and Hellerstein, Kutzbach and Neumark (2015).[43]

[42]We find similar increases when we use three-digit industries, but at longer horizons. We see similar results with two-digit industries as the ones presented here. We do not find any meaningful movement in the probability of working in the same occupation as parents around a displacement event.

[43]Industry switching (Appendix E.2 and Appendix Figure 17) is unlikely to explain our baseline findings.

6 Discussion

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

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

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

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

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

Second, suppose that all workers receive utility b when living near parents, but workers differ in the wage-offer distribution they face. In particular, away from home, homebodies face a wage-offer distribution with mean μ, but at home the mean is $\mu + w_0$, where $w_0 > 0$. Explorers do not have this advantage and face the same distribution at home and away, with mean μ. Notice that, in equilibrium, a worker who is away is more likely to be an explorer because homebodies have a stronger preference for home as a result of the better wage-offer distribution. Also notice that homebodies will, on average, have higher wages due to the wage shifter, w_0. However, note that the expected wage of those at home could be below the expected wage of those away due to the selection on b. As before, the reweighting scheme will address pre-displacement selection on wages, but since workers away are more likely to be explorers they will, on average, have worse wage outcomes after the displacement event. Therefore, our main empirical finding can be explained by differences in the wage-offer distribution.[44]

Third, suppose that all workers receive utility b when living near parents, but workers differ in their unobserved ability and that the return to this ability can be earned in both locations, home and away. In this simple framework, when we compare workers in different

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

locations who have the same wage, as we do with our reweighting approach, the workers living at home will, on average, have higher ability. In principle, this selection on worker ability could be driving our empirical results; however, previous literature finds that this simple intuition is not supported by the evidence. In fact, Topel (1986), Bound and Holzer (2000), and Notowidigdo (2019) all find that low-skilled workers are less mobile in response to adverse labor demand conditions. As such, we think that geographic selection on unobserved ability is unlikely to explain our main findings.

To calculate the value of an improved post-displacement wage-offer distribution in our second though experiment, we take an estimate of the earnings differences after displacement, and we modify it to represent an expected value for a worker that has an average lifetime risk of being displaced. We begin with the differences in post-displacement earnings from Section 4, and we discount them by an annual interest rate of four percent. This simple calculation implies that the lifetime benefit (over a career lasting 35 more years) of living close to parents, conditional on a displacement event, is around \$100,000. In our sample, the probability that a young worker experiences displacement is around 20 percent, so the expected total benefit of living close to home is around \$20,000 for someone at average risk of displacement. This suggests that the benefit of parental proximity after job displacement is associated with an annual value of around \$1,000. Similarly, if we perform the same calculations with our baseline estimates from Section 3, we obtain a value of around \$2,500. Note that this calculation only assumes the benefits of being close to home will apply after a job displacement; if workers received similar benefits after less severe labor market disruptions, our estimates would be a lower bound on the wage benefits of being close to home.

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. This parental insurance persists after we apply a reweighting scheme that controls for observable differences between the two groups. We find evidence that suggests that workers benefit from grandmothers' help with childcare after a job displacement. Another possible cause is parents finding jobs for their children, but we find only some direct evidence that children find jobs in their parents' industries. Broadly, our results are consistent with a simple theory where some parents can facilitate higher wage job offers for children who live nearby.

Longer job searches and children's ability to move in with parents who live nearby do not appear to explain the result. Unemployment durations and changes in hours worked after displacement are similar regardless of how far children live from their parents. Children do tend to move in with their parents after displacements, but the transfers involved are relatively small and involve relatively few workers. Children who lived closer to their parents before they were displaced are also not any more likely to move in with their parents.

Our results suggest that one reason workers live close to their parents is because it offers them insurance against shocks in the labor market, like a job displacement. The presence of parental insurance can explain why people appear so reluctant to move from declining areas (Ganong and Shoag, 2012; Zabek, 2018) and why migration responses have been smaller than expected after several local shocks (Bound and Holzer, 2000; Yagan, 2017; Notowidigdo, 2019). Benefits of living close to parents can 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 also suggest explanations for why inter-state migration has been declining (Molloy, Smith and Wozniak, 2011; Kaplan and Schulhofer-Wohl, 2017) and why young adults are increasingly living with their parents (United States Census Bureau, 2016). More directly, parental resources may be an important explanation for the finding that workers place a large premium on living close to their birthplaces (Kennan and Walker, 2011; Coate, 2017). Based on our empirical findings, simple calculations suggest that parental proximity after job displacements is associated with an annual value of around $1,000.

An important implication of our finding is that any government program that provides similar insurance to parental proximity would increase workers' mobility. Even if a program perfectly crowds out parents' efforts, it still would remove the incentive for workers to locate close to their parents. Without these incentives, younger workers would move to higher-wage jobs, and these movements would increase total output in equilibrium.

Going forward, we hope researchers use other data sources to verify our baseline results. Ideally, these new data would also facilitate additional analyses that focus on the mechanisms leading to our findings. 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) is a particularly fruitful way to proceed.

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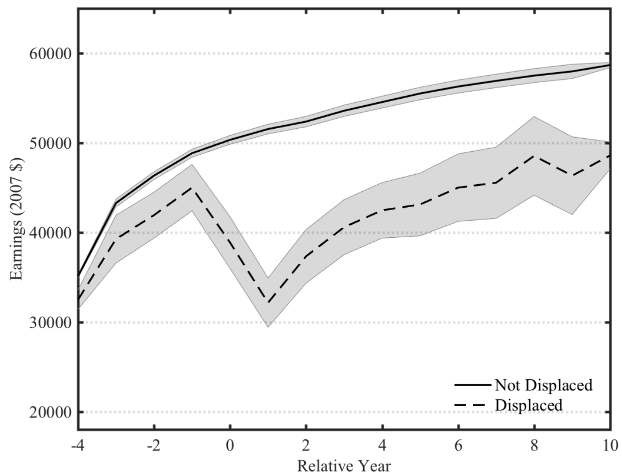
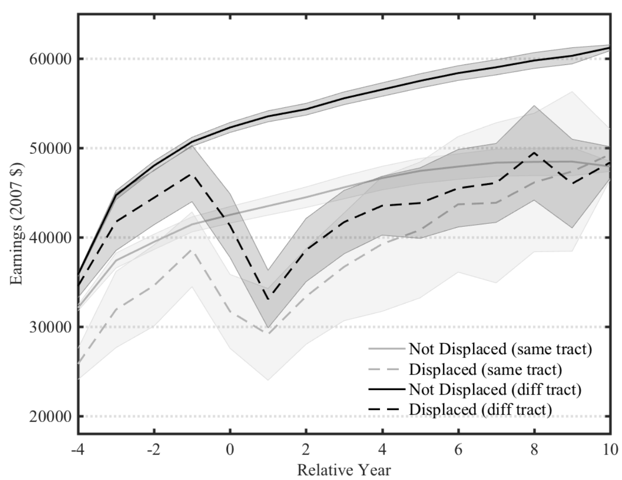


(a) Average Earnings for Displaced and Non-Displaced Workers



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

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

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

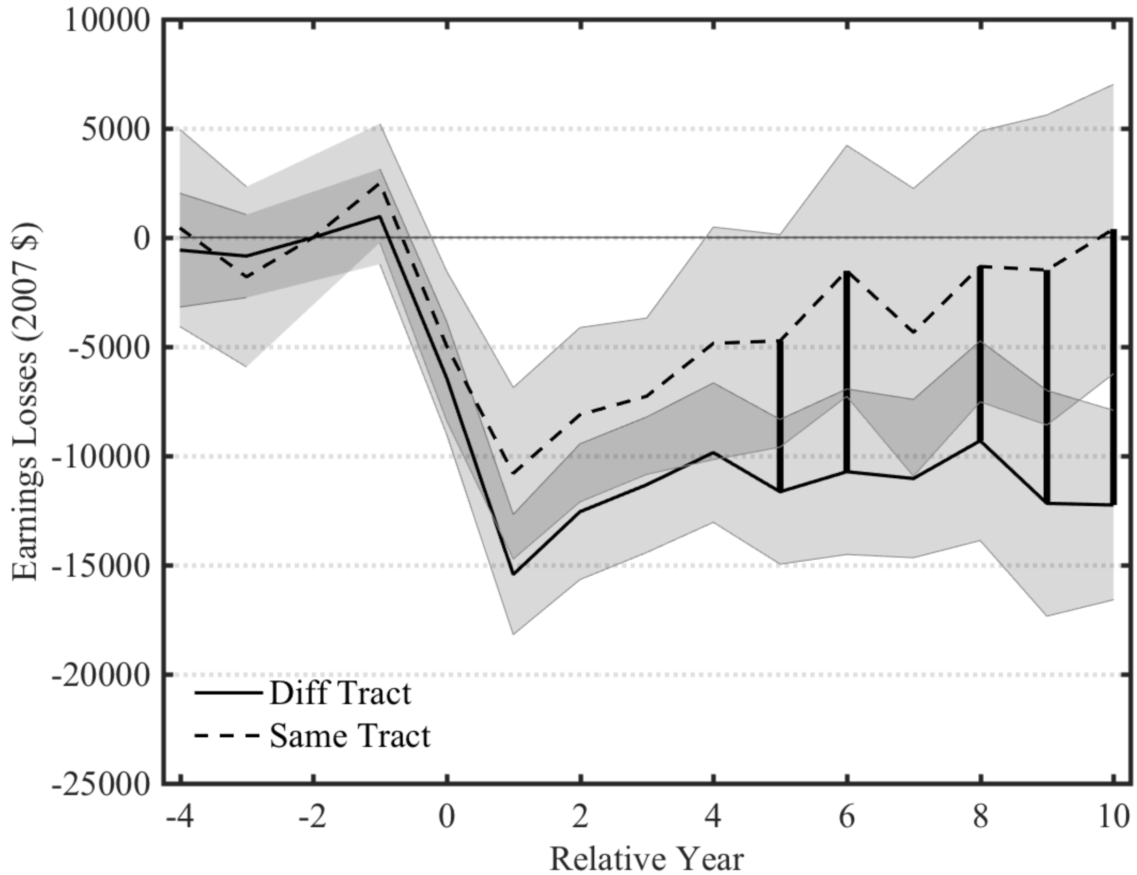
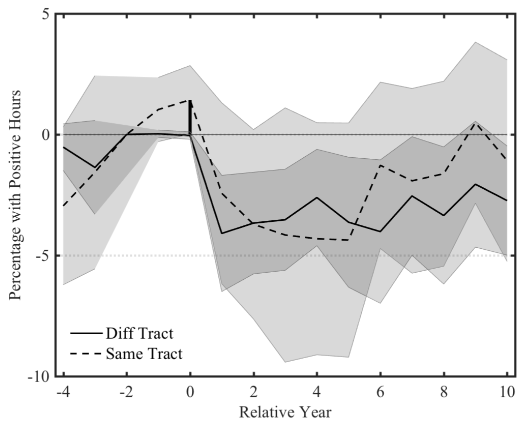
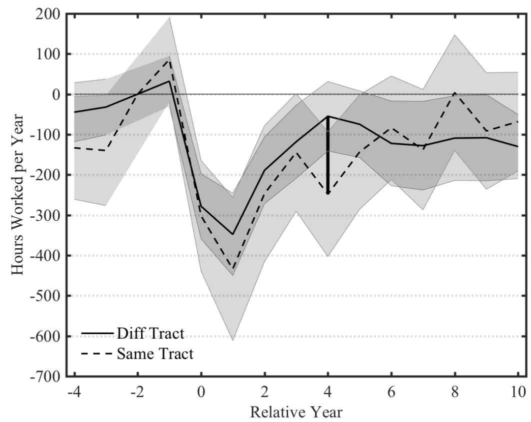


Figure 2: Earnings Losses for Young Displaced Workers

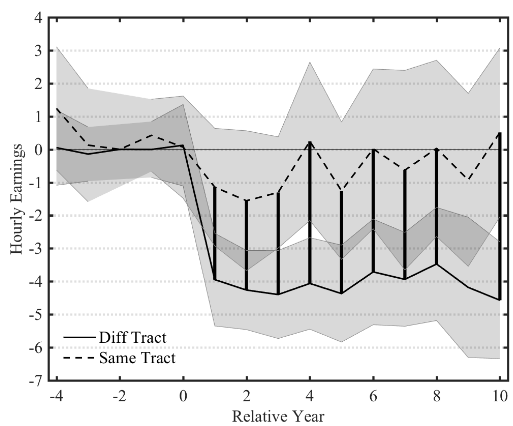
Note: In the medium- and long-run, young workers living in the same neighborhoods with their parents experience a full recovery in earnings after a displacement event. Young workers not living in their parents' neighborhoods experience large and permanent earnings losses, amounting to around 30 percent of their pre-displacement earnings even 10 years after the displacement event. The figure plots regression coefficients from equation (1) describing the impact of a job displacement on the earnings of groups of young workers, aged 25 to 35 at the time of displacement. The shading represents 95 percent confidence intervals and any vertical bars represent statistically significant differences at the five percent level. Standard errors are clustered at the worker level. The definitions of displacements and of whether workers live in the same tract as their parents follow those in Figure 1, and Section 2 contains more information on the sample construction, data, and definitions. The regression controls for worker and year fixed effects as well as a quartic term in each workers' age that we allow to differ between the different groups plotted on the figure.



(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 workers living near their parents and hours fall slightly more, although the hours differences are not statistically significant. These figures plot regression coefficients from equation (1) describing the impact of a job displacement on measures of labor supply and of wages for groups of young workers, aged 25 to 35 at the time of displacement. The shading represents 95 percent confidence intervals and any vertical bars represent statistically significant differences at the five percent level. Standard errors are clustered at the worker level. See Figure 2 for more information about the regression setup.

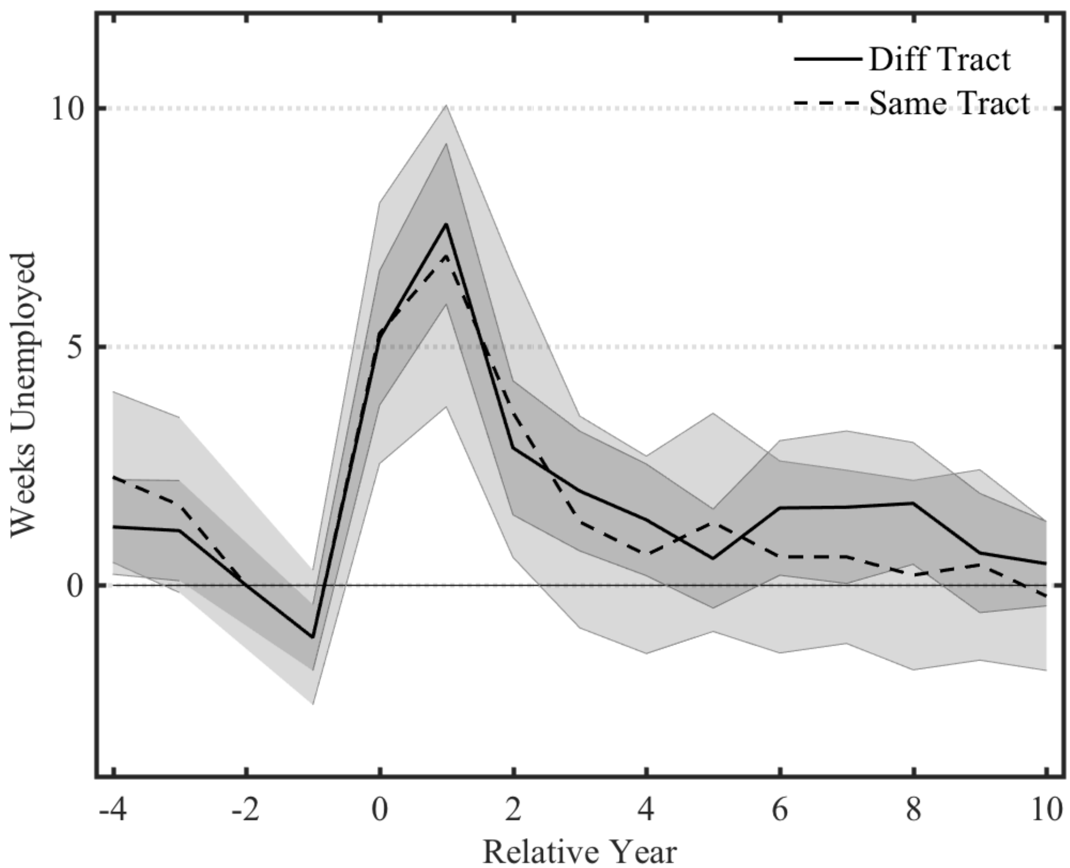


Figure 4: Weeks Spent Unemployed

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. The figure plots regression coefficients from equation (1) describing the impact of a job displacement on the number of weeks that workers were unemployed in each year. The shading represents 95 percent confidence intervals, and any vertical bars connecting the two lines signify that the estimates are statistically significantly different from one another in that year, at the five percent level. In this figure, the differences are not statistically significant. Shading and statistical significance are based on standard errors computed by clustering at the worker level. See Figure 2 for more information about the regression setup.

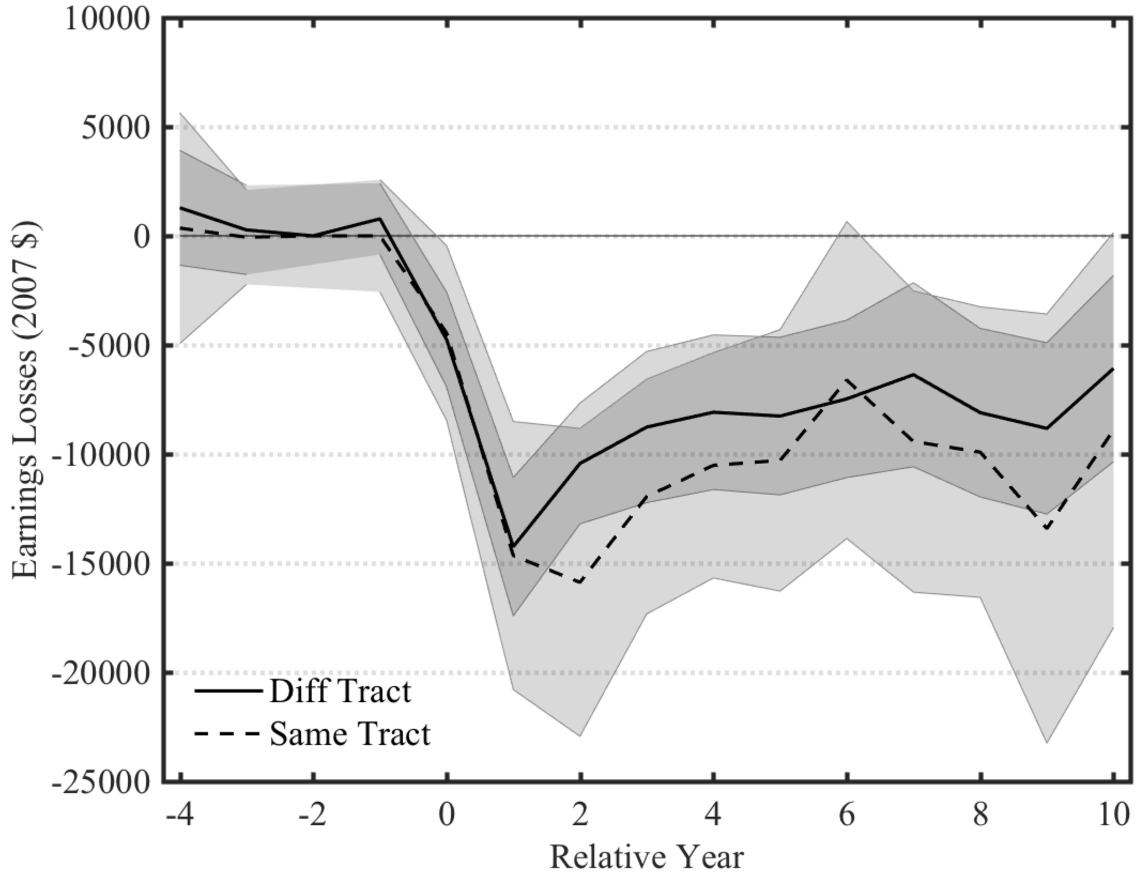


Figure 5: Earnings Losses for Older Displaced Workers

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. The figure plots regression coefficients from equation (1) describing the impact of a job displacement on the earnings of groups of older workers, aged 36 to 55 at the time of displacement. The shading represents 95 percent confidence intervals, and any vertical bars connecting the two lines signify that the estimates are statistically significantly different from one another in that year, at the five percent level. Shading and statistical significance are based on standard errors computed by clustering at the worker level. See Figure 2 for more information about the regression setup.

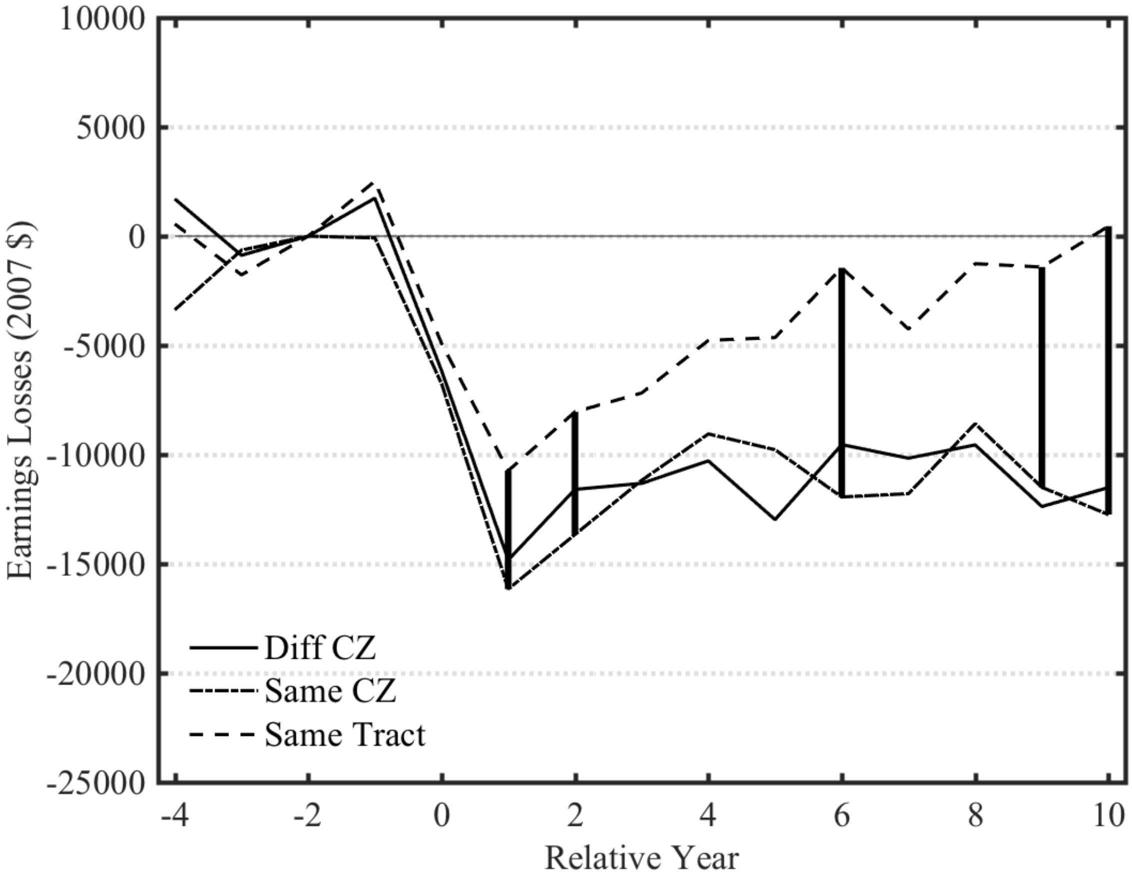


Figure 6: Earnings Losses for Young Displaced Workers by Three Proximities to Parents

Note: Those workers living close to their parents (in the same commuting zone), but not in the same neighborhoods, do not experience significantly better post-displacement earnings outcomes than those who live farther away. The figure plots regression coefficients from a specification similar to equation (1) describing the impact of a job displacement on the earnings of three mutually exclusive groups of young workers. The three groups are defined as workers who lived in the same census tract as their parents, those who lived in the same commuting zone but not the same tract, and those who lived in a different commuting zone as their parents. Each group is defined based on their location two years before the displacement. The figure includes vertical bars that connect the line for workers who live in the same tract with the line for workers who live in the same commuting zone, but not the same tract. We include these when the estimates are statistically significantly different from one another at the five percent level. Statistical significance is based on standard errors computed by clustering at the worker level. See Section 3.3 for more detail on the specification and Figure 2 for more information about the regression setup.

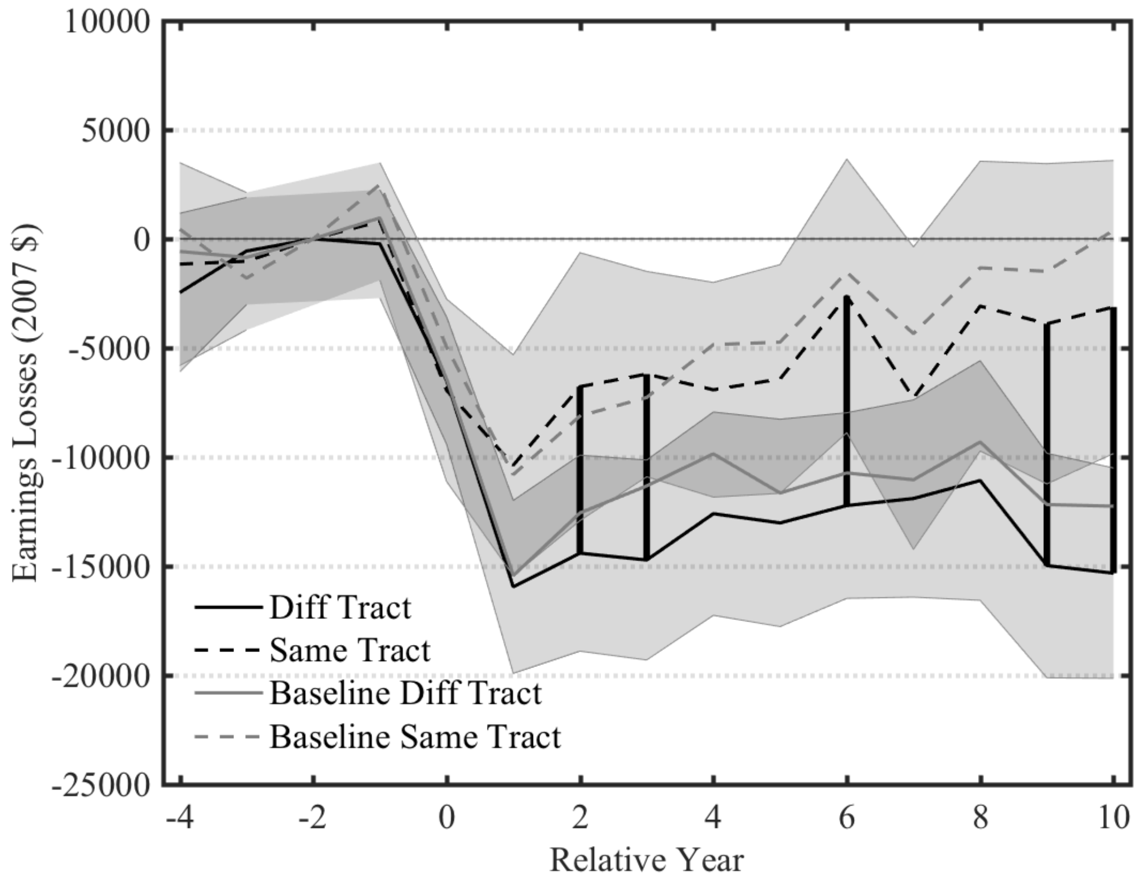


Figure 7: Earnings Losses for Young Displaced Workers Who Do Not Move

Note: Restricting the sample to workers who do not switch counties after the displacement event does not affect the baseline result. Perhaps not surprisingly, the sample almost always has worse earnings outcomes (point estimates) than the unrestricted sample. The figure plots regression coefficients from equation (1) describing the impact of a job displacement on the earnings of young workers who do not move between counties after a job displacement. The shading represents 95 percent confidence intervals, and any vertical bars connecting the two lines signify that the estimates are statistically significantly different from one another in that year, at the five percent level. We only report the vertical bars for significant differences between the regression results with no mobility, since Figure 2 reports them for the whole sample. Shading and statistical significance are based on standard errors computed by clustering at the worker level. See Figure 2 for more information about the regression setup.

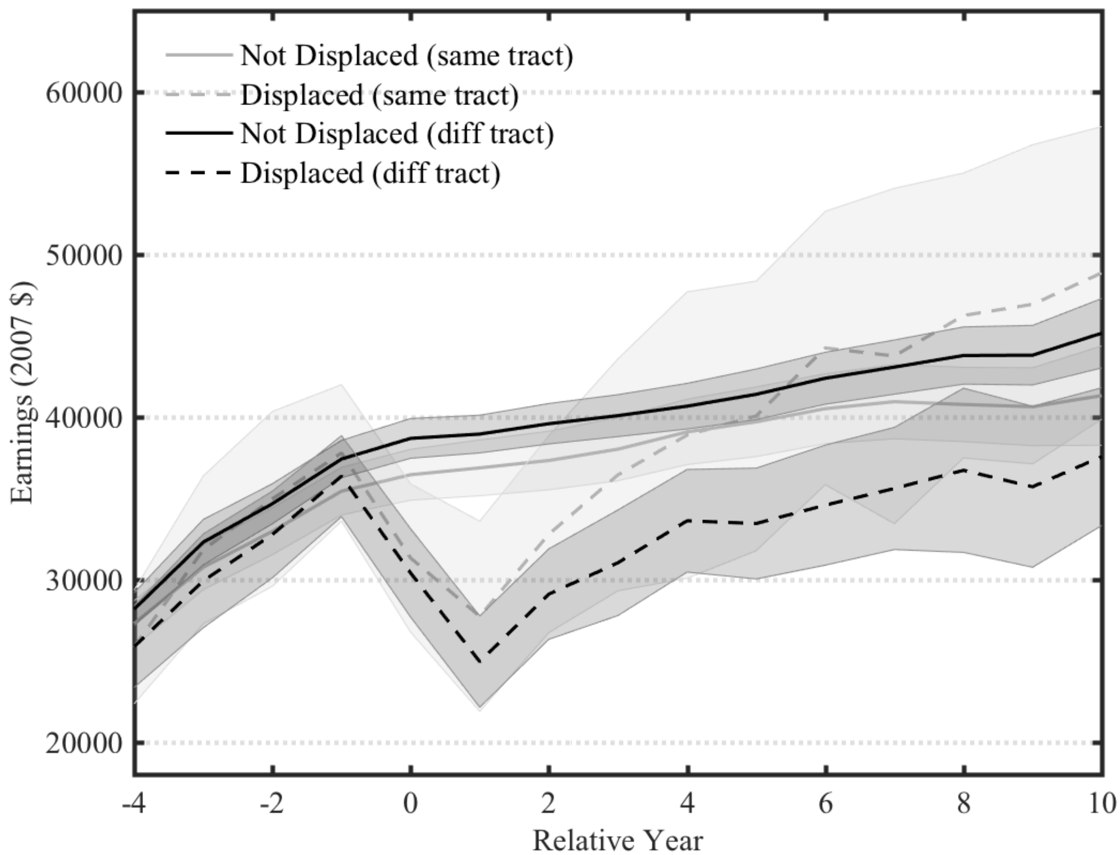


Figure 8: Means After Propensity Score Reweighting

Note: Simple averages, after applying propensity score weights, still suggest that workers living close to their parents have significantly better post-displacement earnings outcomes than those who live farther away. This figures plot propensity score weighted average earnings for displaced and not displaced young workers. The shading represents 95 percent confidence intervals, computed by clustering standard errors at the worker level. We designed the weights to make each other group of workers comparable to the group of workers who live in the same census tract as their parents two years before they experience a job displacement. We include characteristics of workers' jobs, of workers' levels of education, of employment-to-population ratios where workers live, and of workers' demographics, including whether they have children or not. See Section 4 for a description of the reweighting and Figure 2 for more information about the regression setup.

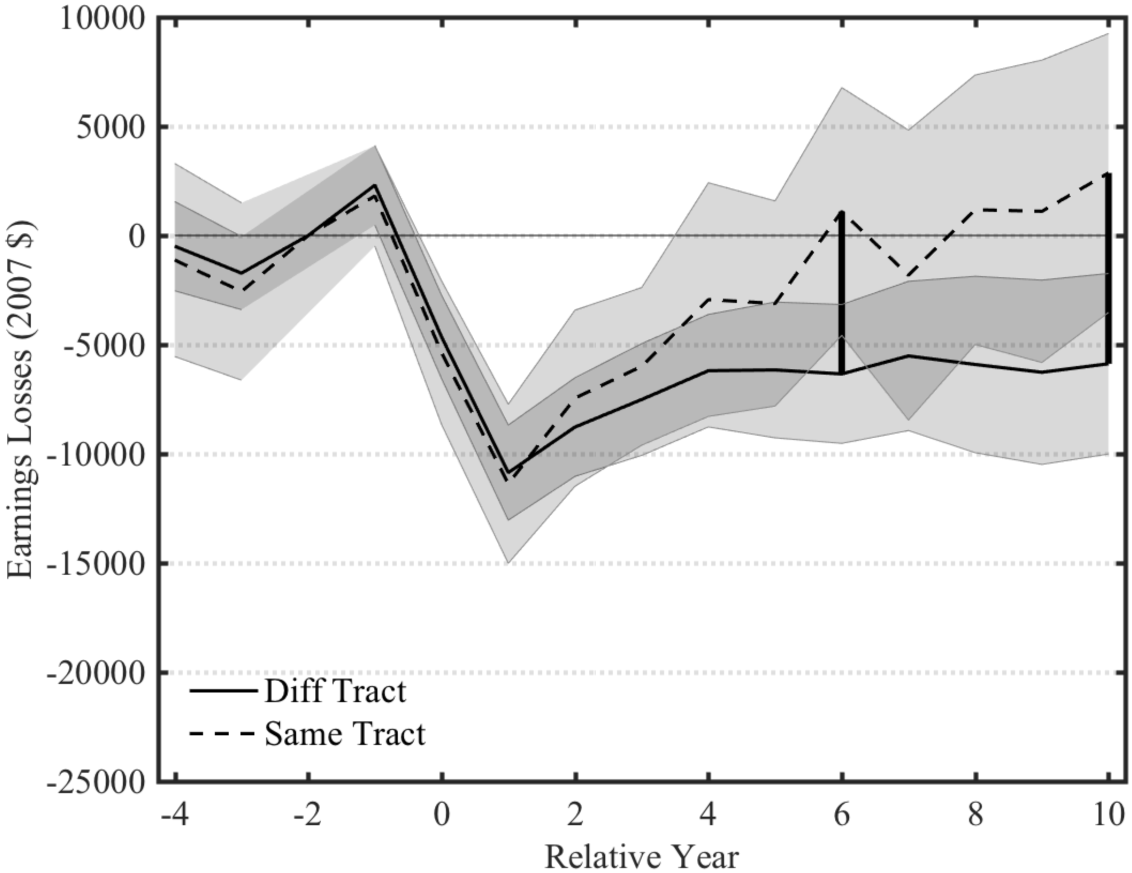


Figure 9: Earnings Losses After Propensity Score Reweighting

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. The figure plots propensity score weighted regression coefficients from equation (1) describing the impact of a job displacement on the earnings of young workers. The shading represents 95 percent confidence intervals, and any vertical bars connecting the two lines signify that the estimates are statistically significantly different from one another in that year, at the five percent level. Shading and statistical significance are based on standard errors computed by clustering at the worker level. We designed the weights to make each other group of workers comparable to the group of workers who live in the same census tract as their parents two years before they experience a job displacement. We include characteristics of workers' jobs, of workers' levels of education, of employment-to-population ratios where workers live, and of workers' demographics, including whether they have children or not. See Figure 2 for more information about the regression setup.

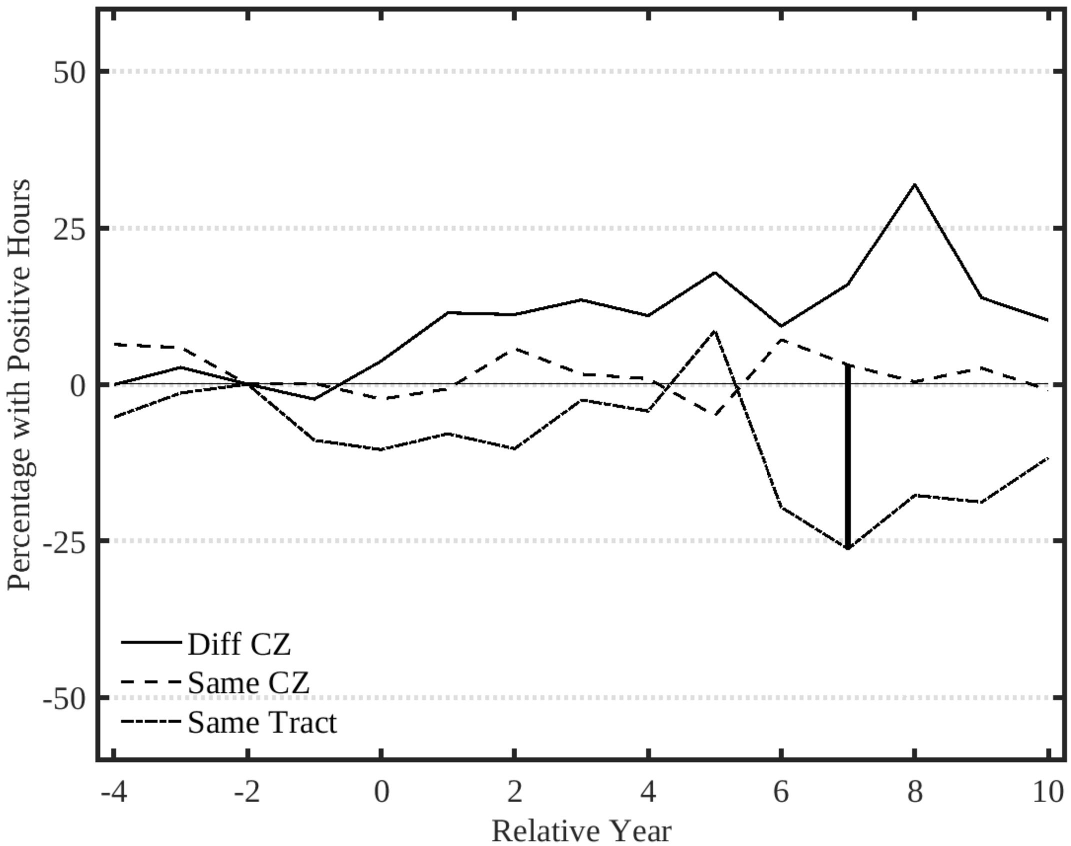
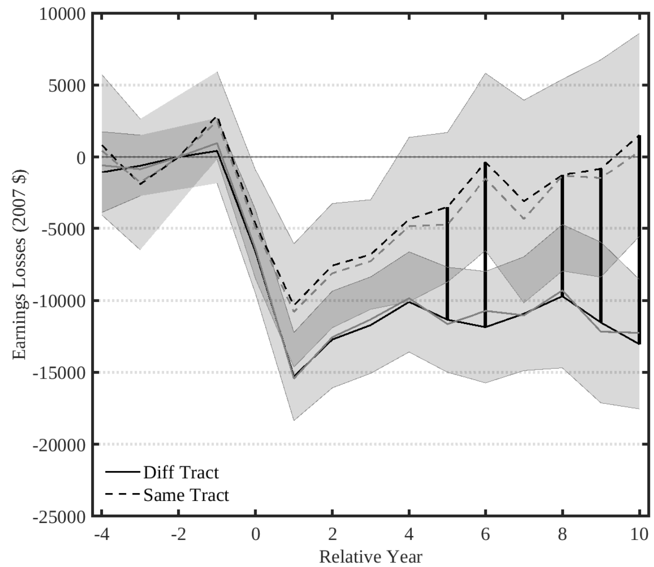
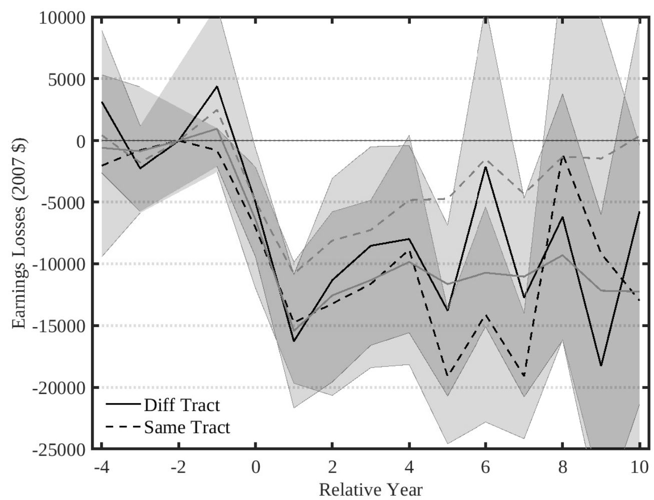


Figure 10: Employment of Mothers after Children's Displacements

Note: Mothers are less likely to work after a child's displacement if they lived in the same tract, but not the same CZ as their displaced child. The figure plots regression coefficients from a specification similar to equation (1) describing the impact of a job displacement on employment of the closest mother or mother in law including controls for employment to population ratios in the child's county and the mother's county as well as lagged fixed effects for the occupation and industry that the mother worked in. Vertical bars connect the outcome for mothers in the same CZ and mothers in the same tract when the estimates are statistically significantly different from one another in that year at the five percent level. Inference is done by clustering at the level of the mother. Figure 6 contains more information about the regression setup.



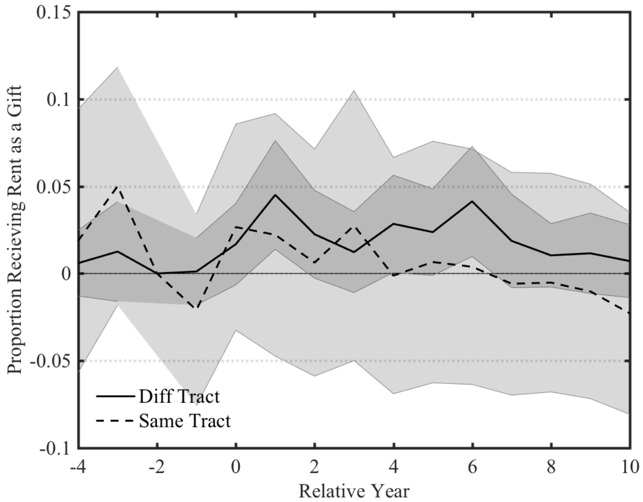
(a) Young Workers Who Have Children



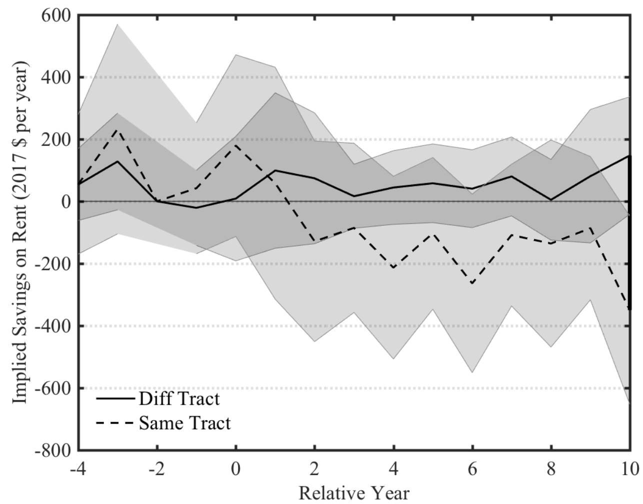
(b) Young Workers Who Never Have Children

Figure 11: Earnings Losses for Young Displaced Workers With and Without Children

Note: The earnings of young workers who have children and live close to their parents recover after a displacement, while those of workers who live farther away, or who do not have children, are permanently lower. The figure plots regression coefficients from a specification similar to equation (1) describing the impact of a job displacement on earnings. Panel A presents the coefficients that apply to workers who are heads or wives in a household with children at some point, and Panel B to those who never are heads or wives in a household with children. All of the coefficients come from the same regression specification. The shading represents 95 percent confidence intervals, and any vertical bars connecting the two lines signify that the estimates are statistically significantly different from one another in that year at the five percent level. Shading and statistical significance are based on standard errors computed by clustering at the worker level. See Figure 2 for more information about the regression setup.



(a) Receiving Rent Entirely as a Gift



(b) Implied Rent Savings

Figure 12: Housing Transfers Around Displacements

Note: There is a small, but detectable, increase in housing transfers around displacement, primarily among workers lived outside their parents' neighborhoods before a displacement. These figures plot regression coefficients from equation (1) describing the impact of a job displacement two measures of in-kind transfers of housing to young workers. The measure in Panel A is the proportion of workers who report that they pay no rent and who then volunteer that this is because someone provided it as a gift to them. The measure in Panel B is the estimated dollar value that the worker's family unit saves in rent, based on living with another family unit. The shading represents 95 percent confidence intervals, and any vertical bars connecting the two lines signify that the estimates are statistically significantly different from one another in that year, at the five percent level. Standard errors are clustered at the worker level. See Section 5.2, for an explanation of each measure and Figure 2 for more information about the regression setup.

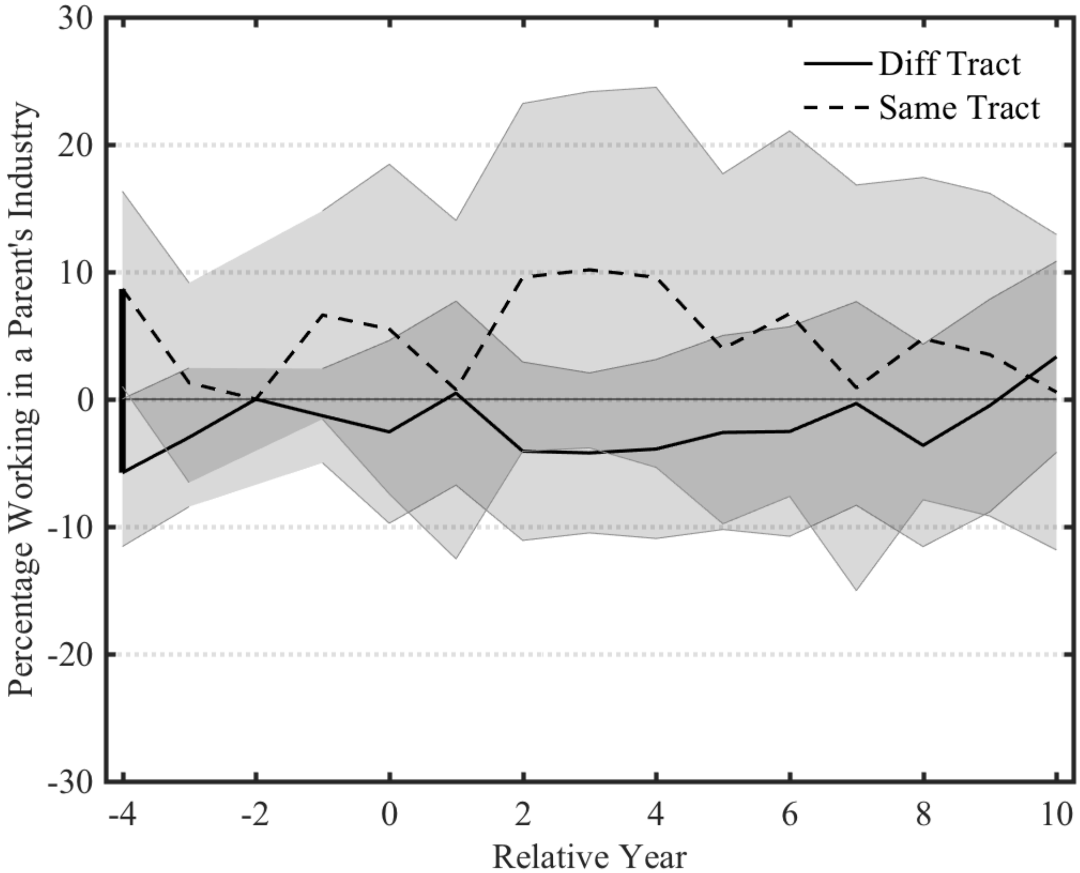


Figure 13: Working in a Parent's Industry

Note: Workers appear to be slightly more likely to work in their parents' industries around a displacement event, though the effect can vary depending on how industries are measured. It does not appear to be due to local industrial composition, however. These figures plot regression coefficients from equation (1) describing the impact of a job displacement on the proportion of workers who work in the same one-digit PSID coded industry as a parent. The shading represents 95 percent confidence intervals, and any vertical bars connecting the two lines signify that the estimates are statistically significantly different from one another in that year, at the five percent level. Shading and statistical significance are based on standard errors computed by clustering at the worker level. See Figure 2 for more information about the regression setup.

Variable			Same Tract		Different Tract	
	Displaced	Not Displaced	Displaced	Not Displaced	Displaced	Not Displaced
	Panel A: All Workers Age 25 to 55					
Earnings	48,300	56,000	41,600	48,400	49,900	57,400
Age	36.4	37.9	34.0	35.5	37.0	38.3
Years of Schooling	12.9	13.6	12.5	13.1	13.0	13.6
Employer Tenure	7.5	10.6	7.1	9.8	7.5	10.7
Fraction Co-residing	0.10	0.08	0.52	0.52	0.00	0.00
Fraction in Parents' Tract	0.19	0.15	1.00	1.00	0.00	0.00
Mother Employed	0.40	0.40	0.36	0.41	0.41	0.40
Has Children	0.60	0.60	0.64	0.61	0.59	0.60
Fraction Male	0.84	0.83	0.84	0.78	0.84	0.84
Wages ($/hr)	21.7	24.7	18.6	21.6	22.5	25.3
Hours Worked	2,260	2,310	2,280	2,270	2,250	2,310
# of records	1,464	44,732	322	7,548	1,142	37,184
	Panel B: Young Workers Age 25 to 35					
Earnings	45,100	49,000	39,000	41,600	47,100	50,800
Age	28.9	29.4	28.4	29.2	29.1	29.4
Years of Schooling	13.1	13.8	12.5	13.1	13.3	13.9
Employer Tenure	5.3	6.5	5.5	6.7	5.2	6.4
Fraction Co-residing	0.10	0.08	0.42	0.39	0.00	0.00
Fraction in Parents' Tract	0.25	0.20	1.00	1.00	0.00	0.00
Mother Employed	0.49	0.52	0.48	0.50	0.49	0.52
Has Children	0.59	0.58	0.60	0.60	0.58	0.57
Fraction Male	0.84	0.82	0.83	0.79	0.85	0.83
Wages ($/hr)	20.3	21.6	17.6	18.6	21.2	22.3
Hours Worked	2,240	2,310	2,250	2,280	2,240	2,320
# of records	707	17,996	204	4,005	503	13,991

Table 1: Summary Statistics

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

Variable	Same Tract Displaced	Same Tract Not Displaced	Different Tract Displaced	Different Tract Not Displaced	Same Tract Displaced	Same Tract Not Displaced	Different Tract Displaced	Different Tract Not Displaced
	Panel A: PSID Weights				Panel B: Reweighted			
Earnings	$35,000	$39,700	$44,700	$48,300	$35,000	$33,100	$33,100	$34,800
	[1.00]	[0.03]	[0.00]	[0.00]	[1.00]	[0.36]	[0.43]	[0.93]
Average Change in Earnings	$2,900	$2,300	$2,900	$3,500	$2,900	$2,100	$3,300	$2,500
	[1.00]	[0.36]	[0.97]	[0.39]	[1.00]	[0.25]	[0.64]	[0.58]
Years of Schooling	12.50	13.10	13.31	13.94	12.50	12.46	12.37	12.52
	[1.00]	[0.00]	[0.00]	[0.00]	[1.00]	[0.84]	[0.61]	[0.93]
Share in Goods Industries	0.53	0.43	0.51	0.34	0.53	0.48	0.51	0.40
	[1.00]	[0.06]	[0.81]	[0.00]	[1.00]	[0.33]	[0.86]	[0.02]
Share Manager/Professional	0.20	0.27	0.31	0.41	0.20	0.20	0.17	0.20
	[1.00]	[0.11]	[0.03]	[0.00]	[1.00]	[0.92]	[0.54]	[0.91]
Employer Tenure	5.32	6.79	5.16	6.42	5.32	5.39	5.20	5.33
	[1.00]	[0.00]	[0.65]	[0.00]	[1.00]	[0.81]	[0.76]	[0.98]
Unemp Rate in County	7.47	7.17	7.48	6.80	7.47	7.42	7.83	7.45
	[1.00]	[0.34]	[0.98]	[0.04]	[1.00]	[0.89]	[0.45]	[0.96]
Age	28.32	29.26	29.14	29.51	28.32	28.04	27.95	28.09
	[1.00]	[0.00]	[0.02]	[0.00]	[1.00]	[0.34]	[0.32]	[0.43]
Number of Children	1.26	1.21	1.11	1.11	1.26	1.16	1.23	1.18
	[1.00]	[0.72]	[0.32]	[0.26]	[1.00]	[0.42]	[0.83]	[0.56]
Fraction Male	0.82	0.81	0.86	0.85	0.82	0.81	0.77	0.83
	[1.00]	[0.82]	[0.34]	[0.39]	[1.00]	[0.89]	[0.36]	[0.73]
Number of Records	190	3,620	456	12,633	190	3,620	456	12,633

Table 2: Means Before and After Reweighting

Note: After applying the propensity score weights, the sample of workers who live in the same tract as their parents and those living farther away are statistically indistinguishable in terms of many observable characteristics. This table reports means for each group using PSID weights in the first four columns and the propensity score weights in the last four columns. For each variable, we report the mean and a p-value in brackets of a Wald test that this mean is the same as the value in the first column. Standard errors and p-values adjust for clustering at the worker level. The number of records refers to the number of person by age records where we have sufficient earnings observations to include the record in the main sample, which we use for our main results. Missing data for some variables mean that some of the statistics on this table, most notably the local unemployment rates, are computed based on fewer records. The definitions of displacements follow Figure 1, Section 4 describes the reweighting, and Section 2 contains more information on the sample construction, data, and definitions.

	Rent	Gifted Rent		Implied Savings	
	Dollar value	Proportion	Dollar value	Proportion	Dollar value
Value	\$6,800	0.02	\$2,500	0.08	\$4,300
Standard error	(90)	(0.002)	(170)	(0.005)	(430)
N	7,890	18,396	417	18,396	543

Table 3: Measures of Housing Transfers

Note: Less than 10 percent of young workers recieve discounted housing, and the implied amount tends to be modest, and less than the average amount that families spend on rent when they live alone. 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 from that parent. Note that the dollar value is only observable when the parent is a PSID respondent themselves. We explain each measure of housing transfers in more depth in Section 5.2.

	Same Tract	Diff Tract
$\mathbb{P}[in\ parent's\ industry]$	0.25	0.28
Standard error	(0.0018)	(0.0032)
N	89,157	32,371

Table 4: Summary Statistics of Sharing Parent's Industry by Parental Proximity

Note: Simple averages suggest that employed young workers living in their parents' neighborhoods are slightly less likely to be working in their parents' industry. Results are based on large sectors but looking at finer levels of disaggregation does not alter the conclusions. This table reports the proportion of young workers who are employed in their parent's one-digit PSID industries by whether they currently live in the same census tract as their parents. The rows report means, standard errors of those means, and sample sizes.

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

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.[45]

Similar to the exercise in the main text where we break out proximity by same tract, same commuting zone but not same tract, and farther away, we also look at workers who are actually coresiding with their parents as opposed to living in the same neighborhoods 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 neighborhood with their parents. The point estimates suggest that the recovery for these two groups of workers is similar and better than for those who live outside of their parents' neighborhoods at the time of displacement. This finding suggests that the effect of parental proximity is not limited to transfers while 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 worker was displaced while living in the county where they grew up.[46] 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 effect on post-displacement earnings that is independent from other local factors.

[45] 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).

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

B Appendix: Reweighting Based on Other Characteristics (For Online Publication)

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

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

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

To implement each strategy, we estimate our baseline specification, equation 1, with weights, shown in equation 2, using different sets of co-variates.

B.1 Including Parents' Characteristics

For the first exercise, we include all of the same characteristics as in Section 4, the average employment to population ratio in the census tract that the worker's parents live in, and several characteristics of both the mother and the father, entered separately. These characteristics include a dummy for whether the parent is college educated, the parent's total years of schooling, a dummy for whether the parent is currently employed, the parent's age, and the parent's age squared. We also include a dummy for whether the parent was interviewed in any of the three years before the worker's potential displacement. If the parent was not interviewed we set the dummy for whether the parent was interviewed to one and all of the parent's other characteristics to zero. Otherwise, we follow the methodology in Section 4.[47]

The results, shown in Panel A of Appendix Figure 11, are very similar to the propensity score reweighted results in Figure 9. The initial effect of a job displacement is around $11,000 for each group (around a third of initial earnings). The group of workers who live in their parents' neighborhoods earn about the same amount as if they were not displaced after about six years, however, while the other group permanently earns about $7,000 less. Compared with Figure 9, more of the differences between groups are significant at the five percent level, and confidence intervals are slightly smaller.

B.2 Predetermined Characteristics

For the second exercise, we restrict the characteristics to those that are predetermined at the time that the worker decides whether to live in their parents' neighborhood. These characteristics are a dummy for whether the worker is college educated, their total number

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

of years of schooling, their age, a dummy for their gender, a dummy for being African American, and the employment-to-population ratio in the place where they live. Besides the limited set of worker characteristics, we follow the methodology in Section 4. For example, our estimates are based on the same sample of workers.

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

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

One way to see if the propensity score reweighting is finding workers with similar counterfactual outcomes is to simply plot the earnings trajectories of each control group, suitably reweighted. We do this in Appendix Figure 12 by plotting earnings before and after years where workers were at risk of a displacement, according to our definition, but where they did not actually lose a job. Though there still are some differences after we apply the propensity reweights, the reweighted earnings are very similar before the potential displacement, and continue to behave quite similarly afterwards. The two lines are practically indistinguishable after we include the controls from our baseline regression specifications.

Panel A of Appendix Figure 12 shows that the propensity score reweighting procedure removes almost all of the earnings differences between the two groups, and brings them down to the lower level of earnings among workers who were displaced while living in the same neighborhood as their parents. Before the potential displacement, which is the period we use to match workers, the earnings of the two groups are within \$1,000 of one another and their pre-trends are roughly parallel. Even from period zero (the potential displacement) to period ten, a period that we leave out of our logit specification, the earnings trends still track each other quite well. This implies that matching on initial earnings, education, occupations, gender, and other factors is enough to find workers with similar employment prospects. This is very much in contrast to means using the PSID longitudinal weights.

Panel B of Appendix Figure 12 shows that earnings are even more similar between the two groups after we adjusted for an important control in our regression specification (equation 1) – a quartic term in worker age, estimated separately for each group. Taking out the age quartic highlights each worker's good fortune in avoiding a displacement in year zero and makes the lines completely statistically, and economically, indistinguishable. The lines are well within \$1,000 of one another throughout the panel.

D 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 the following equation, which includes all of the terms in our baseline specification, equation 1, as well as some additional terms:

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

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 13 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 neighborhood as parents (dashed line) and people living away from parents (solid line). The darker lines in Panels A and B of Appendix Figure 13, show the same coefficient plot if one also allows effects to vary by how much they earned before the 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 13 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.

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 workers 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 how respondents seared for jobs, e.g. they ask if respondents checked with private employment agency, if they checked with friends or relatives, and if they placed or answered ads.

Appendix Table 2 presents summary statistics on search intensity for those living in the same neighborhood 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 workers 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 people 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 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 worker i reported any job search activity in period t, $Sametract_{it}$ is a dummy for whether worker i is living in their parents' neighborhood in 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 worker 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, further research with different data would be needed to make a more definitive statement.

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 workers who retain a job in their former line of work. We document industry switching around the displacement event for workers who were in the same neighborhood as their parents prior to the displacement event and those who lived farther away. We estimate equation (1) but use as an outcome variable a dummy, D_switch_{iat}, that equals one if the worker i switches one-digit industry between survey year t and $t+1$ at base age a.

Appendix Figure 17 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 17 also suggests that workers living in the same neighborhood as their parents prior to the displacement event experience markedly sharper increases in their probability of switching industries than workers living farther away. Based on the prior work cited above, this would predict larger post-displacement earnings losses for workers living close to parents and would thus work against our baseline findings. As such these results suggest that industry switching cannot explain our baseline findings. In results not shown, occupation switching is similar for the two groups around a displacement event.

F Appendix: Measures of Transfers (For Online Publication)

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

F.1 Calculating Implied Savings on Rent

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

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

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

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

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

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

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

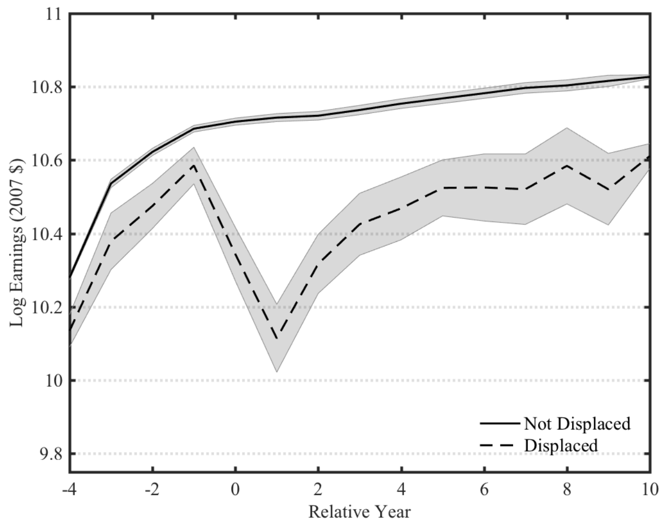
One complication in practice is that parents oftentimes own their houses, which means there is no direct measure of parents' rents. To compute an annual rent equivalent in these settings, we employ a user cost of capital equal to 0.0785 (following Albouy and Zabek (2016)). The user cost gives, essentially,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

F.2 Cash Transfers from Friends and Relatives

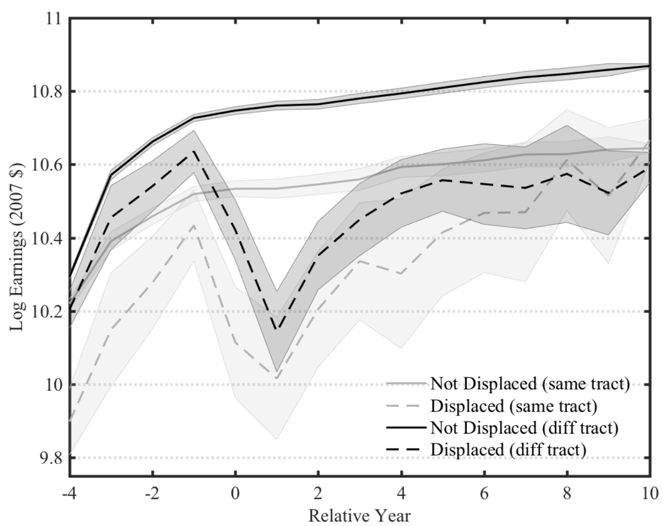
In addition to transfers of housing, children can receive cash transfers from their parents. Appendix Figure 18 shows how these change around displacement, using our main event study specification including fixed effects, an age quartic, and other controls. For these plots we use an annual question in 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 18 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)



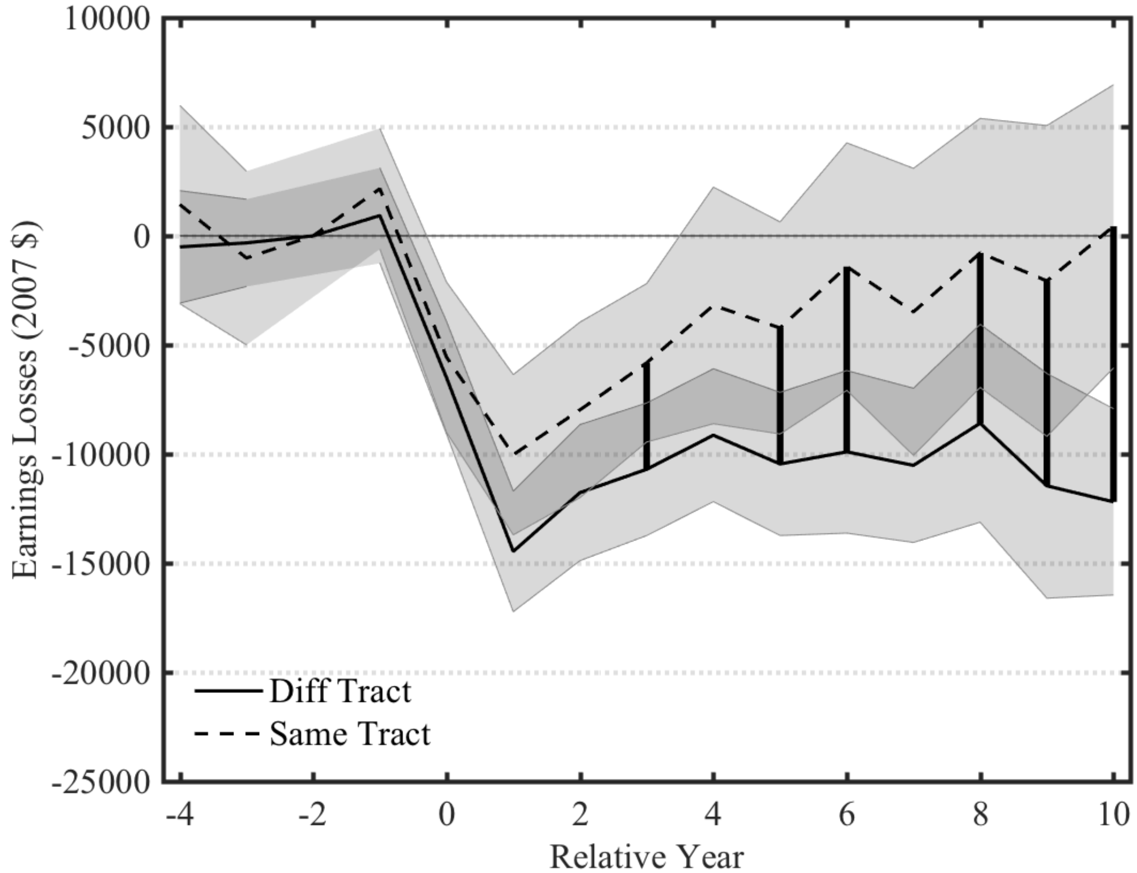
(a) Average Log Earnings for Young Displaced and Non-Displaced Workers



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

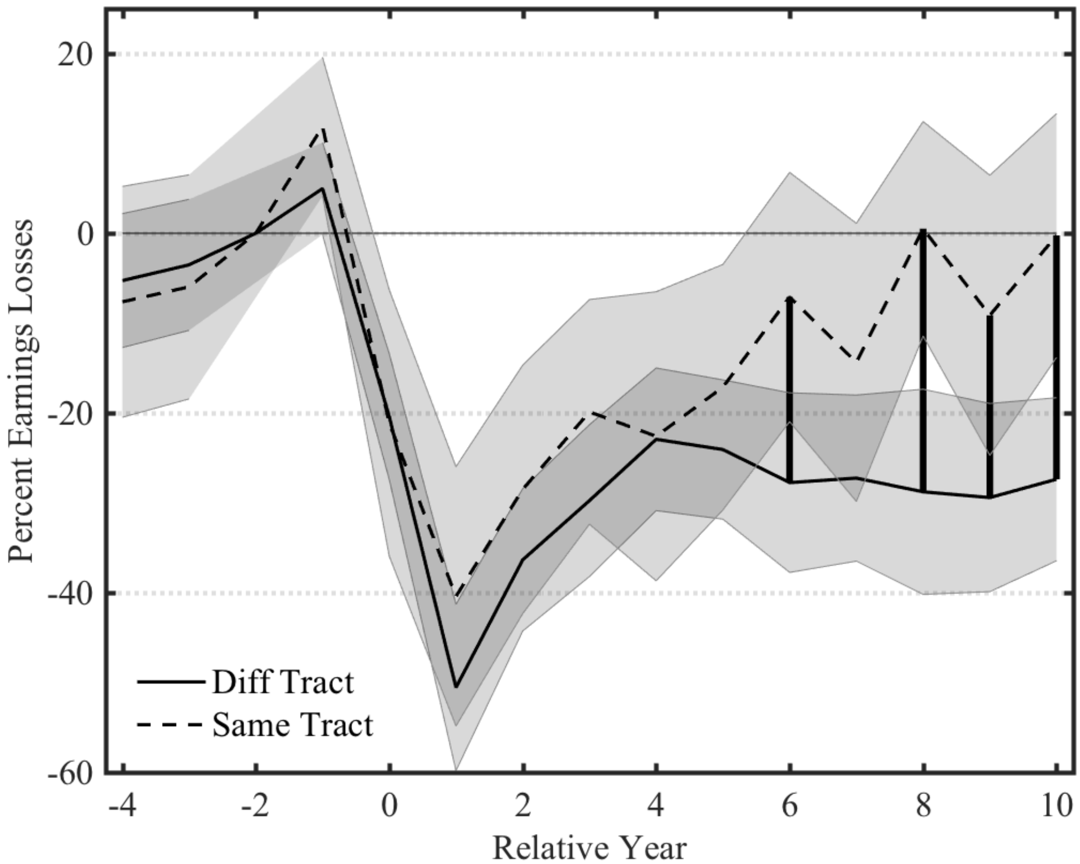
Appendix Figure 1: Average Log Earnings for Young Displaced Workers by Proximity to Parents

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



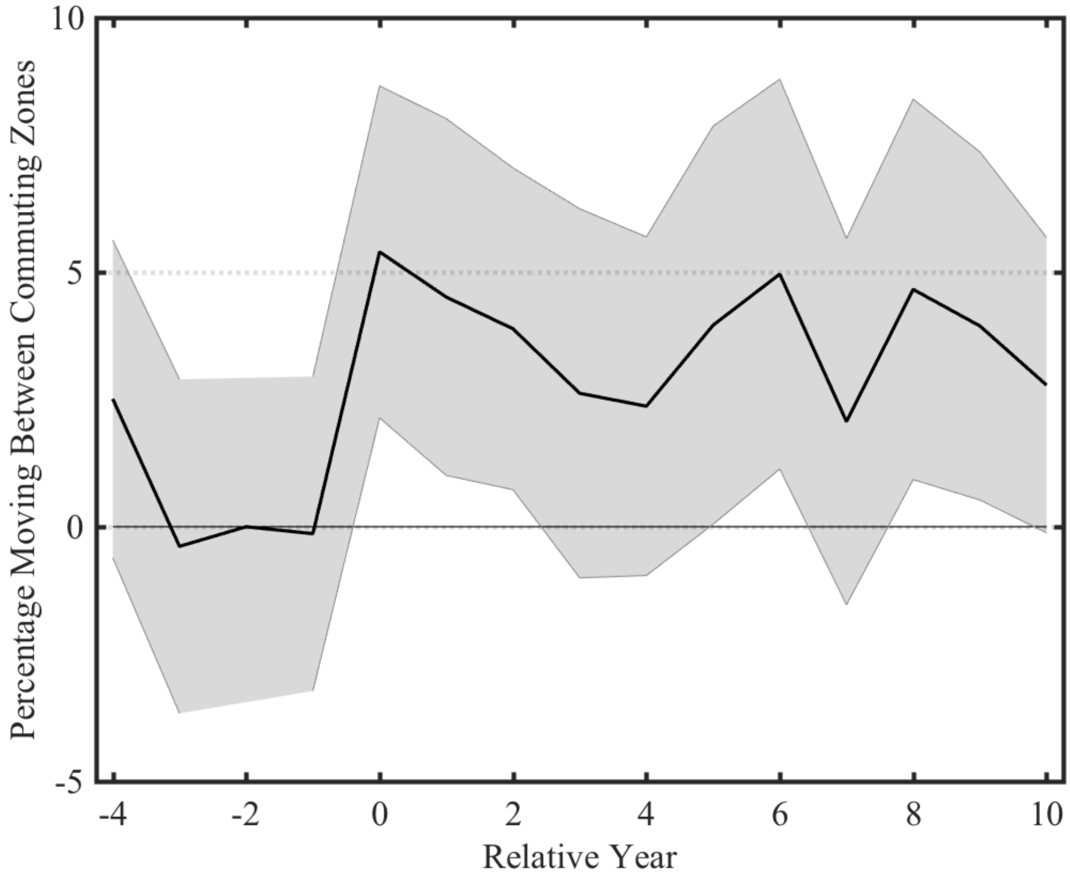
Appendix Figure 2: Earnings Losses for Young Displaced Workers (Excl. Zeroes)

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. Plotted are regression coefficients from equation (1) where we drop years where workers reported zero earnings. The shading represents 95 percent confidence intervals, and any vertical bars connecting the two lines signify that the estimates are statistically significantly different from one another in that year, at the five percent level. We cluster the standard errors at the worker level. Figure 2 contains more information about the specification, definitions, etc.



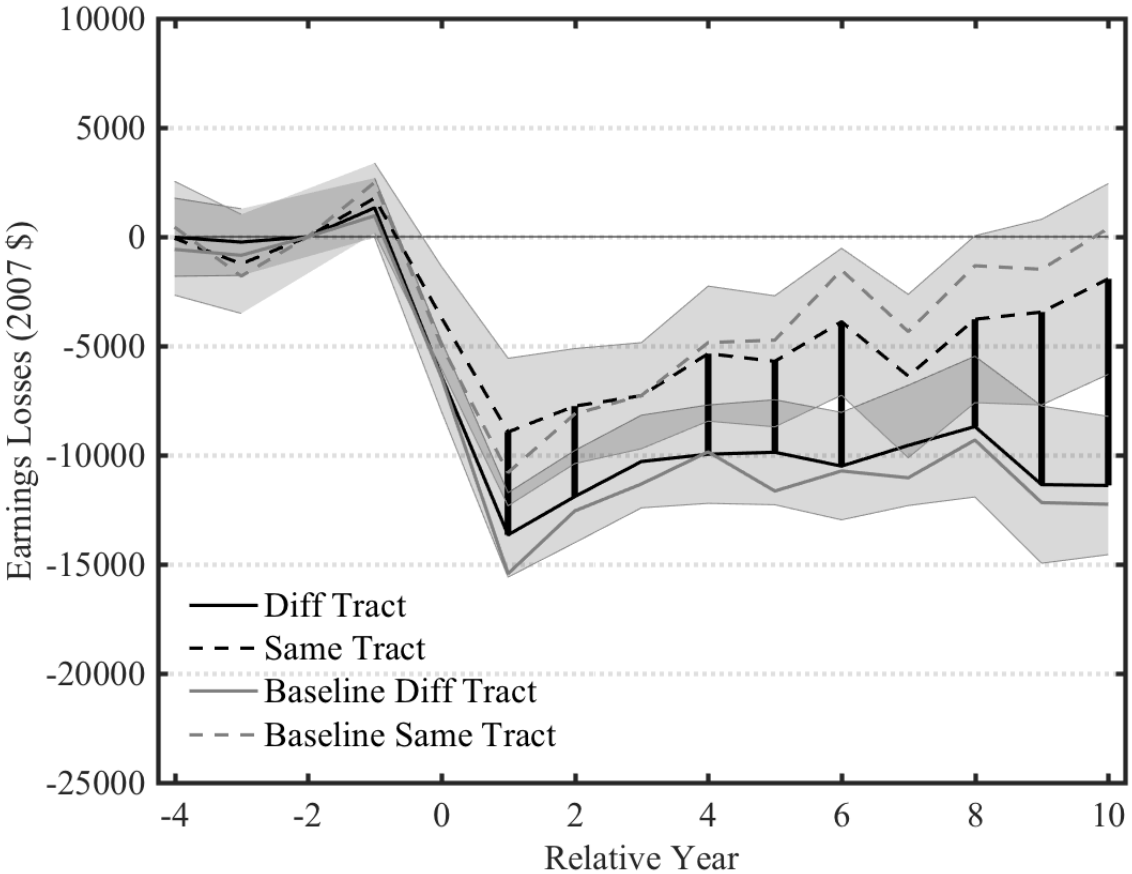
Appendix Figure 3: Percent Earnings Losses for Young Displaced Workers

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. Plotted are regression coefficients from equation (1) with log income on the left hand side. To obtain percentage changes we plot $(e^{\hat{\delta}^k} - 1) \times 100$. The shading represents 95 percent confidence intervals, and any vertical bars connecting the two lines signify that the estimates are statistically significantly different from one another in that year, at the five percent level. We cluster the standard errors at the worker level. Figure 2 contains more information about the specification, definitions, etc.



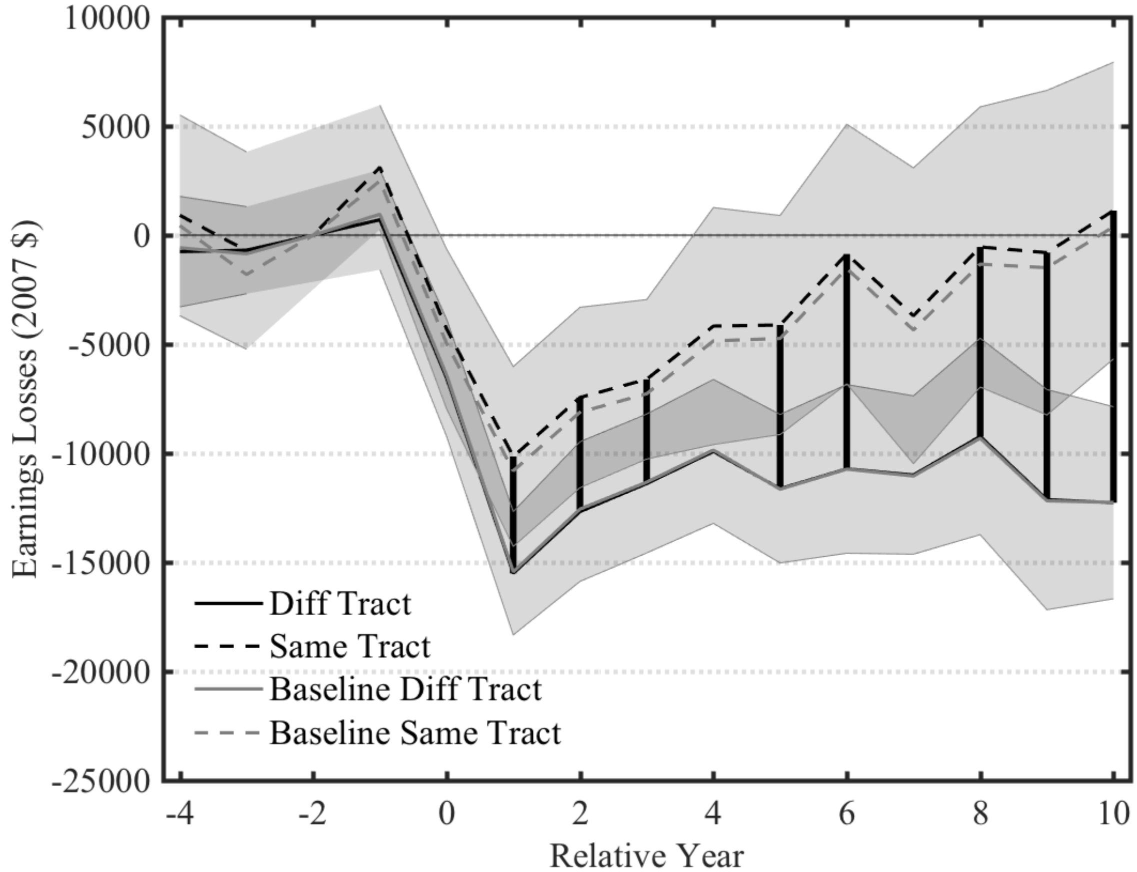
Appendix Figure 4: Probability of Switching Commuting Zones Around Displacement

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. Plotted are regression coefficieints from a linear probability model with a specification very similar to equation (1). The main differences are the outcome, moving between commuting zones in the year in question, and that we pool both groups of workers to increase precision. The shading represents 95 percent confidence intervals based on clustered standard errors, at the worker level. We use commuting zones as the relevant measure of geography because they most closely resemble the "regional labor markets" that Huttunen, Møen and Salvanes (2016) use with Norwegian data. Figure 2 contains more information about the specification, definitions, etc.



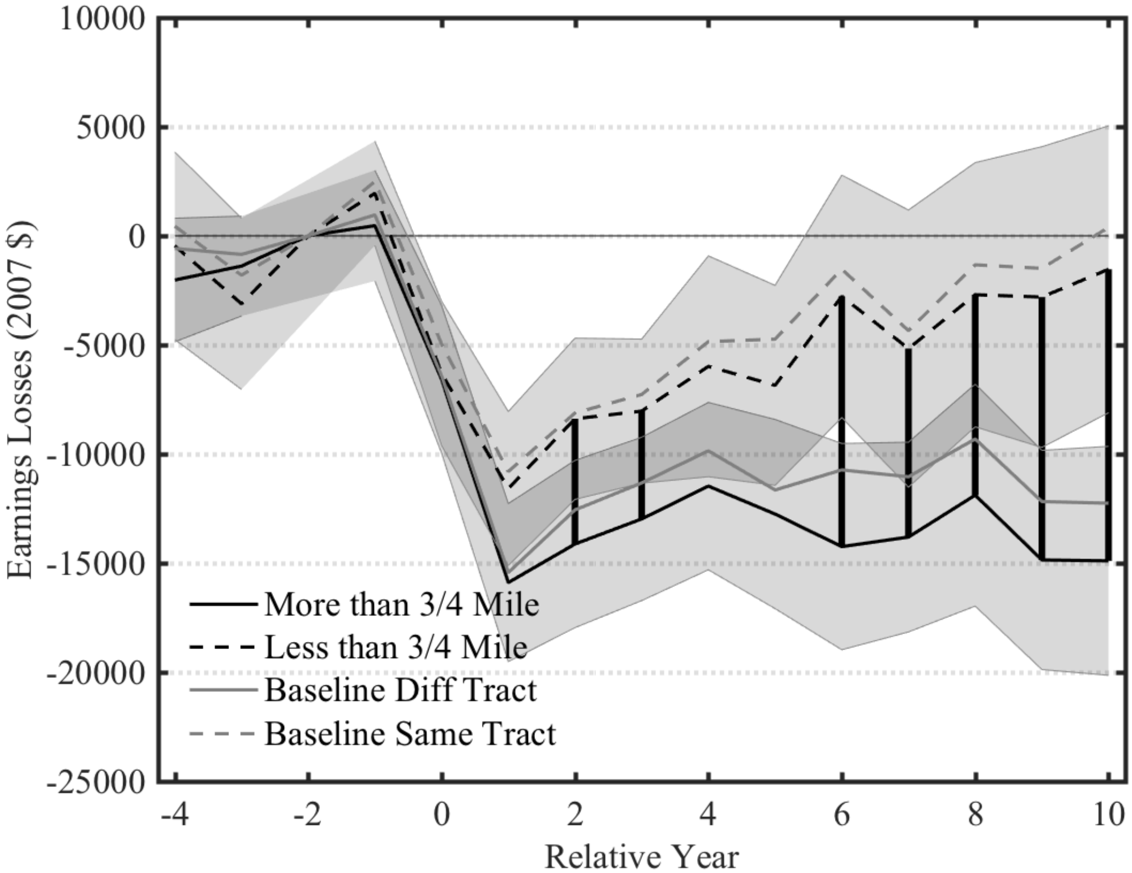
Appendix Figure 5: Earnings Losses for Young Displaced Workers (No PSID Weights)

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 in Figure 2, although those living near their parents at the time of displacement see slightly less of a benefit. The shading represents 95 percent confidence intervals, and any vertical bars connecting the two lines signify that the estimates are statistically significantly different from one another in that year, at the five percent level. We cluster the standard errors at the worker level.



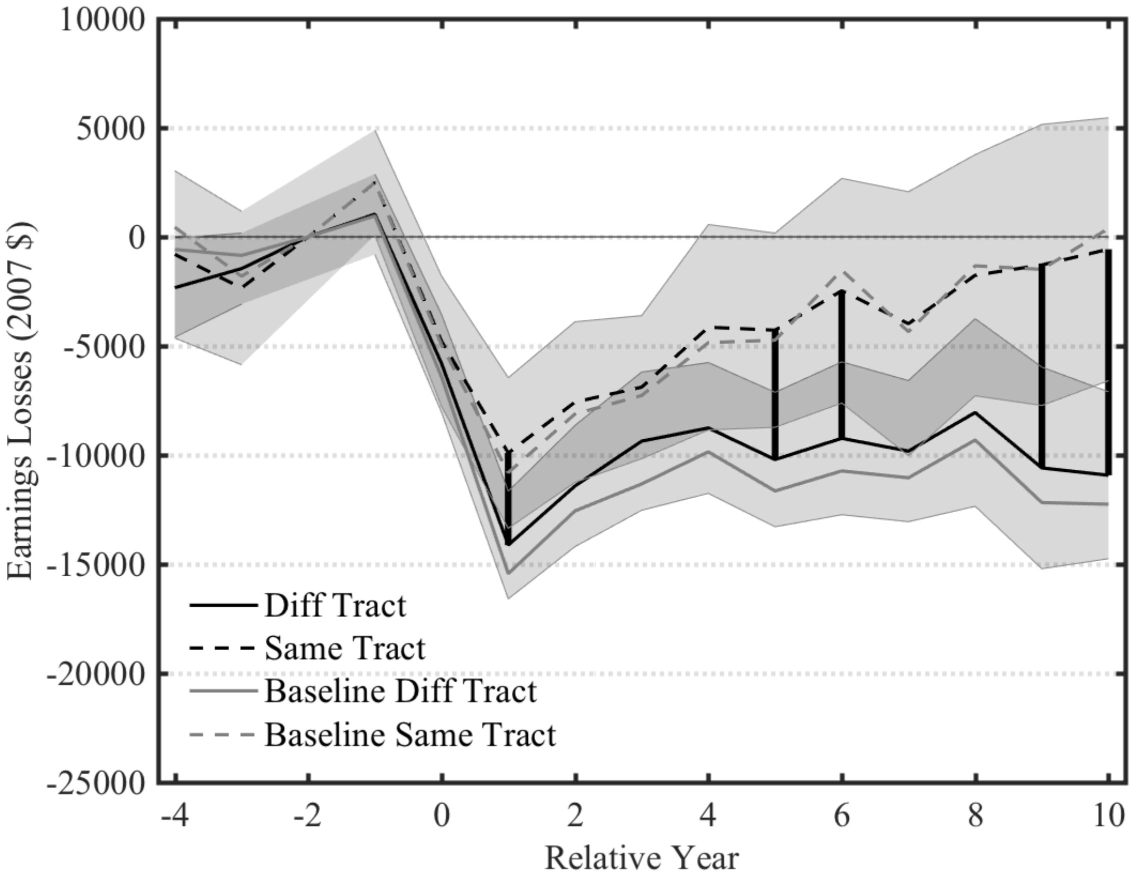
Appendix Figure 6: Earnings Losses for Young Displaced Workers (Controlling for Local Labor Market Conditions)

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. The shading represents 95 percent confidence intervals, and any vertical bars connecting the two lines signify that the estimates are statistically significantly different from one another in that year, at the five percent level. We cluster the standard errors at the worker level. See Appendix A for more details on how we measure local labor market conditions. The specification is very similar to our baseline specification in Figure 2.



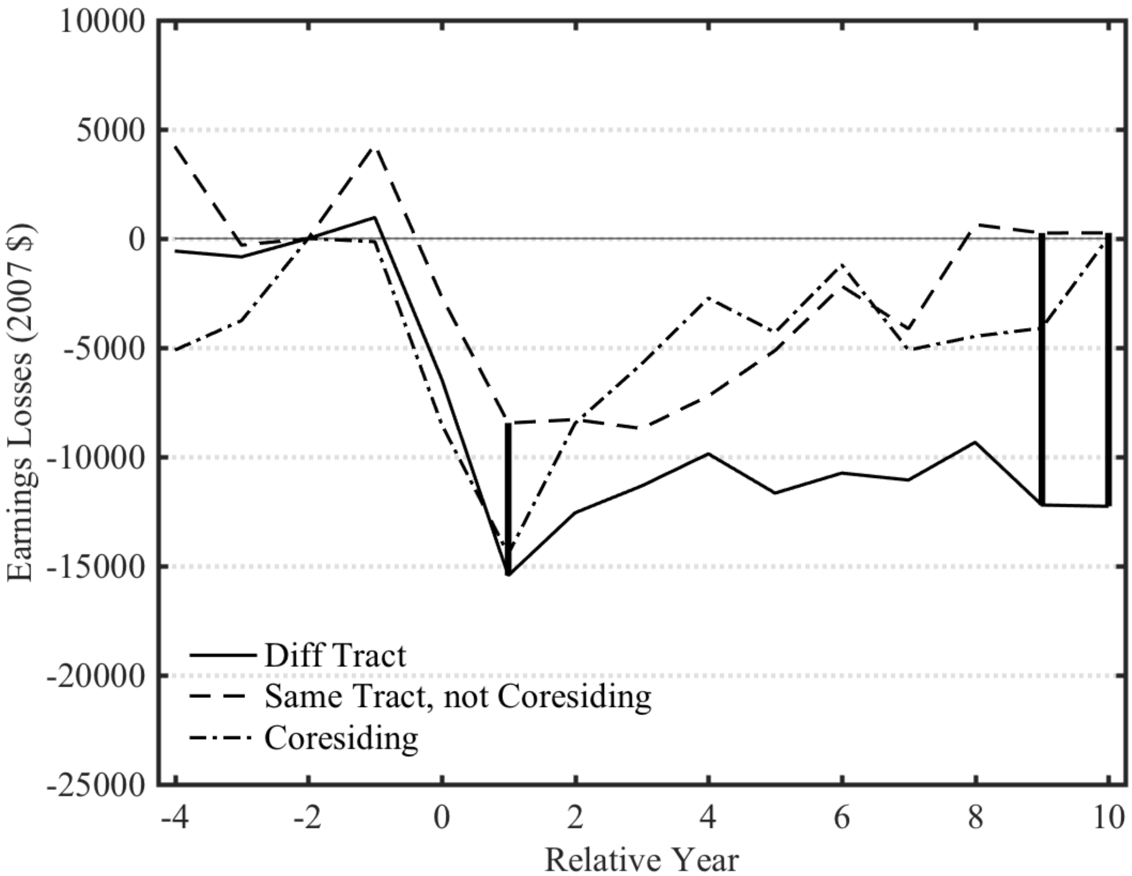
Appendix Figure 7: Earnings Losses for Young Displaced Workers (Using Distance Measures)

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 in Figure 2. If anything, this approach strengthens the findings slightly. The shading represents 95 percent confidence intervals, and any vertical bars connecting the two lines signify that the estimates are statistically significantly different from one another in that year, at the five percent level. We cluster the standard errors at the worker level. See Figure 2 for full details of the specification.



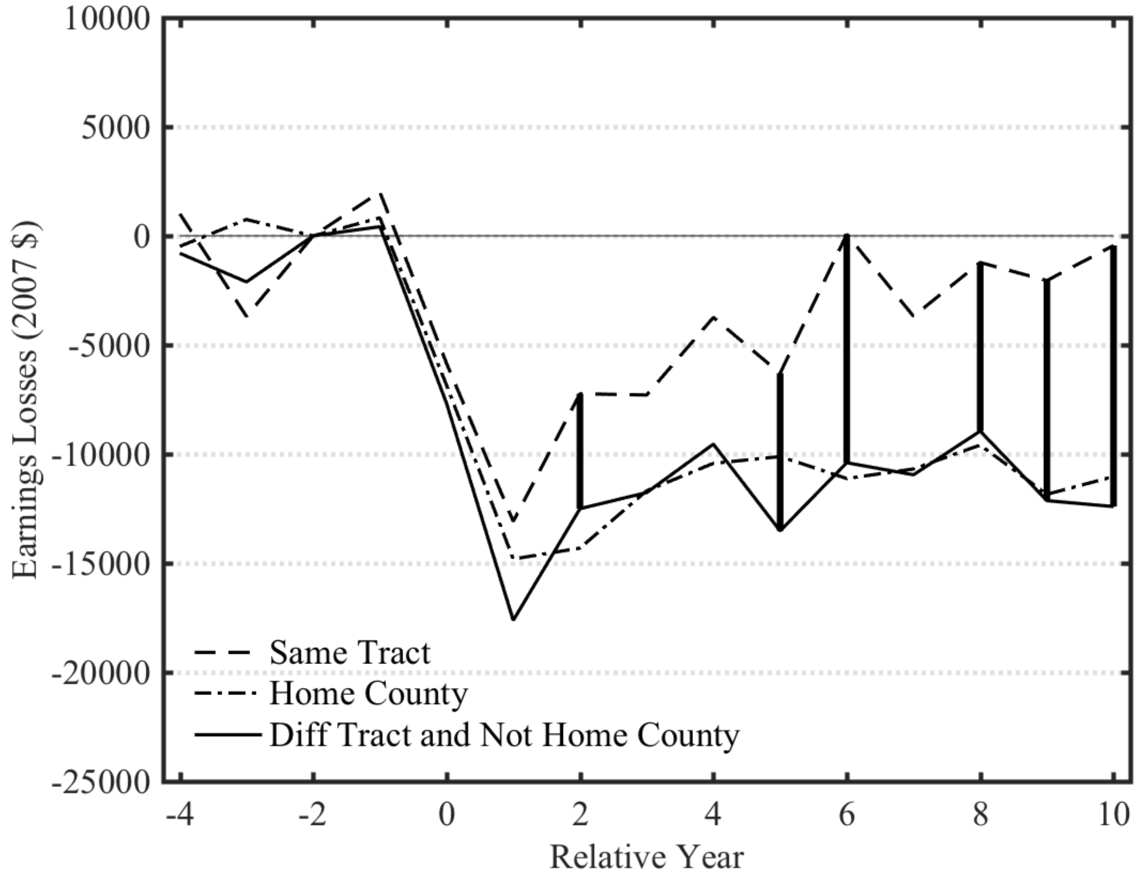
Appendix Figure 8: Earnings Losses for Young Displaced Workers (Heads and Wives)

Note: These results present the coefficients from equation (1) using both heads and wives as opposed to just heads as in our baseline sample. The results are very similar to the baseline results in Figure 2. The shading represents 95 percent confidence intervals, and any vertical bars connecting the two lines signify that the estimates are statistically significantly different from one another in that year, at the five percent level. We cluster the standard errors at the worker level. See Figure 2 for full details of the specification.



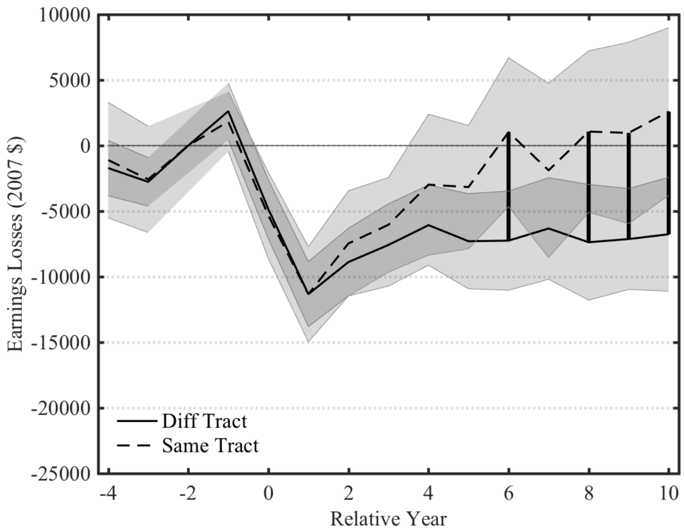
Appendix Figure 9: Earnings Losses for Young Displaced Workers (Same Tract vs. Coresiding)

Note: The post-displacement earnings recoveries look similar for those workers 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. The figure plots regression coefficients from a specification similar to equation (1) describing the impact of a job displacement on the earnings of workers who lived in a different census tract from their parents, those who lived in the same census tract but in a different house, and finally those who lived in the same house as their parents. The groups are mutually exclusive. The figure includes vertical bars that connect the line for workers who live in the same tract (not coresiding) with the line for workers who live in a different tract. We include these when the estimates are statistically significantly different from one another at the five percent level. We cluster the standard errors at the worker level. See Figure 6 for full details of the specification.

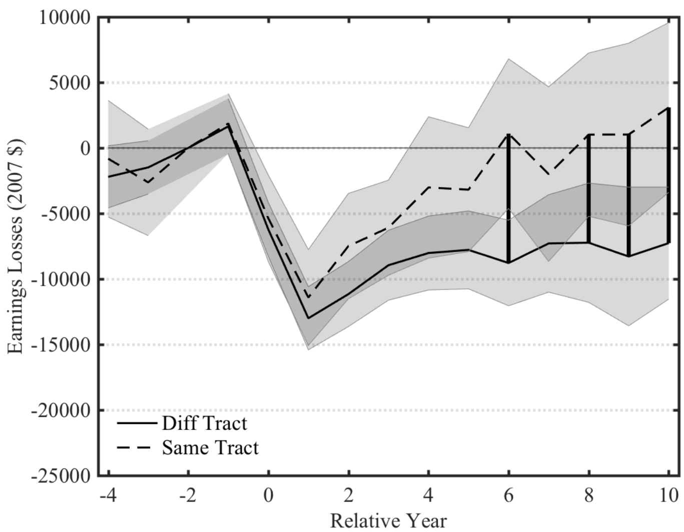


Appendix Figure 10: Earnings Losses for Young Displaced Workers with Home County Interactions

Note: The post-displacement earnings recoveries look similar to our baseline results even after interacting the displacement dummies in equation (1) with whether a worker lived in the county where they grew up at the time of displacement. The earnings losses for those in their home county look similar to those who are neither in their parents' neighborhoods or their home county. The figure plots regression coefficients from a specification similar to equation (1) describing the impact of a job displacement on the earnings of workers who lived in the same census tract as their parents, those who lived in the county they reported that they grew up in, and those who lived away from both their parents and the place that they grew up. For the former two the lines correspond to people where the specified category is true, but the other is not. The figure includes vertical bars that connect the line for workers who live in the same tract with the line for workers who live in a different tract within a county that they did not grow up in. We include these when the estimates are statistically significantly different from one another at the five percent level. We cluster the standard errors at the worker level. More information on these specifications is in Appendix A.



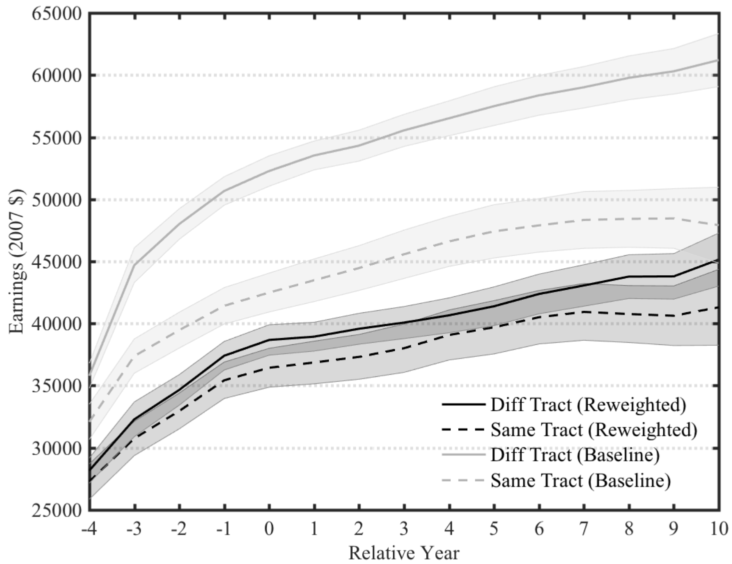
(a) Including Parents' Characteristics



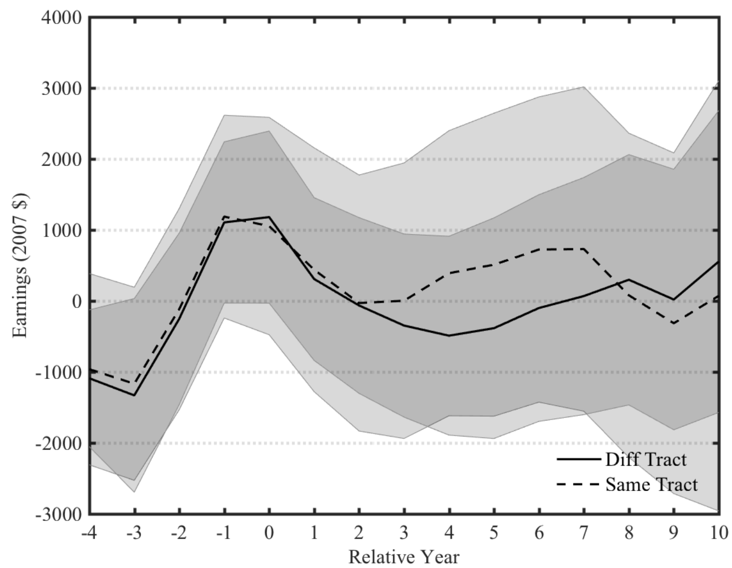
(b) Including Only Predetermined Characteristics

Appendix Figure 11: Reweighted Regressions Based on Alternative Reweighting Specifications

Note: The results from the propensity score reweighting do not appear to be sensitive to including either additional controls for parents' characteristics, or to including only predetermined characteristics, like educational levels, age, and race. The figure plots propensity score weighted regression coefficients from equation (1) describing the impact of a job displacement on the earnings of young workers. The weights in Panel A are calculated to match parents' characteristics between the different samples, in addition to the main characteristics. The weights in Panel B are calculated to only match predetermined characteristics between the sample. See Appendix B for more details on these two types of weights, see Figure 9 for the original specification, and see Section 4 for more information on the reweighting scheme.



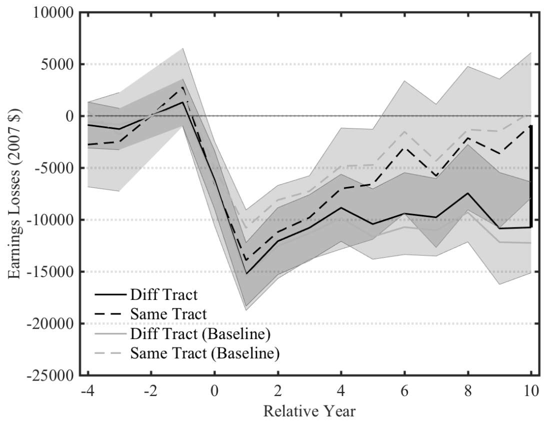
(a) Reweighted Means Without Age Trend



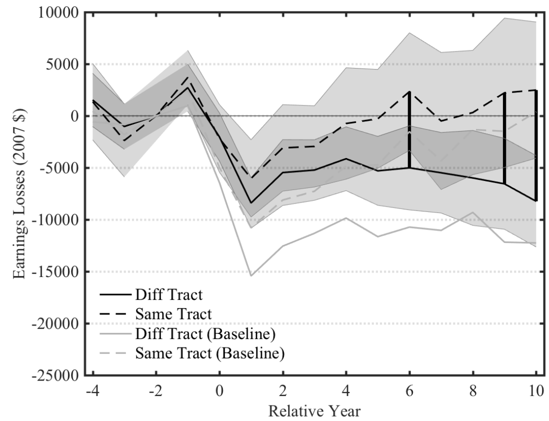
(b) Reweighted Means Including Age Trend

Appendix Figure 12: Mean Earnings For the Reweighted Control Samples

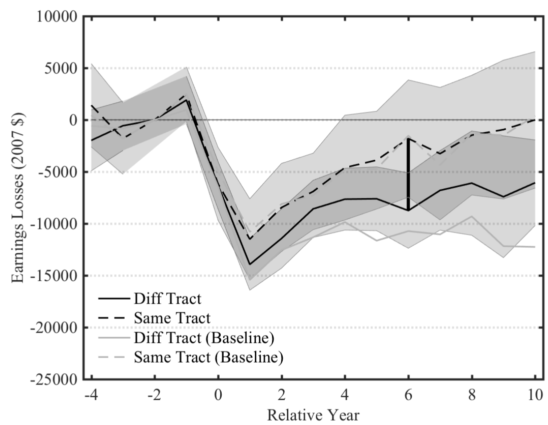
Note: The propensity score reweighting appears to be successful in terms of reweighting each control group so that they are mostly similar to each other. Panel A shows that after applying the propensity score weights, but without adjusting for age quartics, non-displaced workers living close to their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



(a) Baseline Income as Linear Interaction



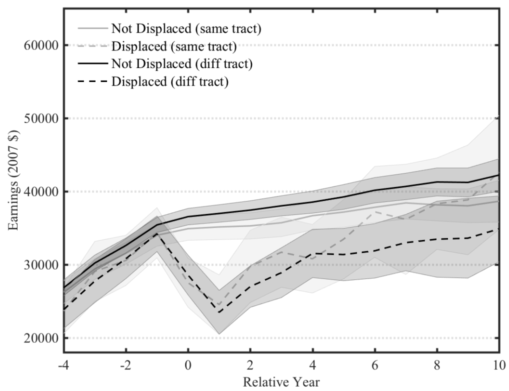
(b) Baseline Income as a Dummy



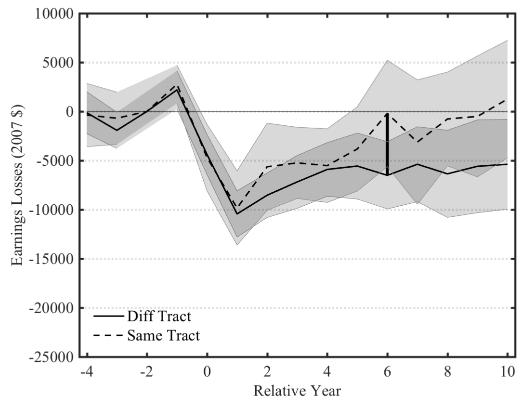
(c) College Education as a Dummy

Appendix Figure 13: Including Additional Interactions in the Baseline Specification

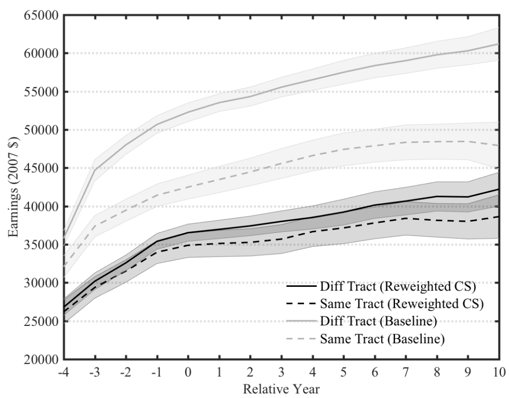
Note: Additional interactions with the displacement dummies (equation 3) do not change the effect of parental proximity on the post-displacement earnings outcomes. Although interacting with earnings prior to job loss generally makes the initial earnings losses similar for the two groups, the two paths still diverge later on. Plotted are regression coefficients from estimating equation (3). Panel A shows the coefficients when one includes an additional interaction with a linear term earnings, Panel B shows the results after including an interaction with a dummy for having above average earnings, and Panel C shoes results after including an interaction with a dummy for being college educated. Lighter lines reproduce the baseline results from Figure 2. Appendix D includes more details on the specification.



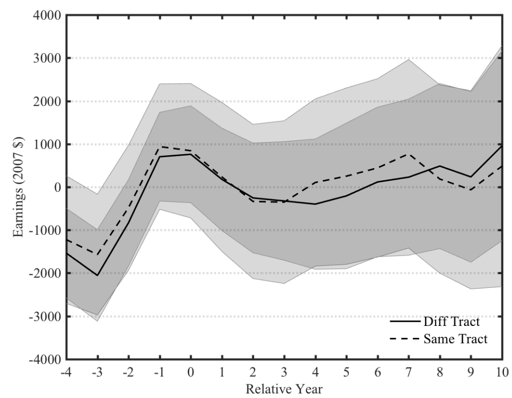
(a) Reweighted Means



(b) Reweighted Regressions



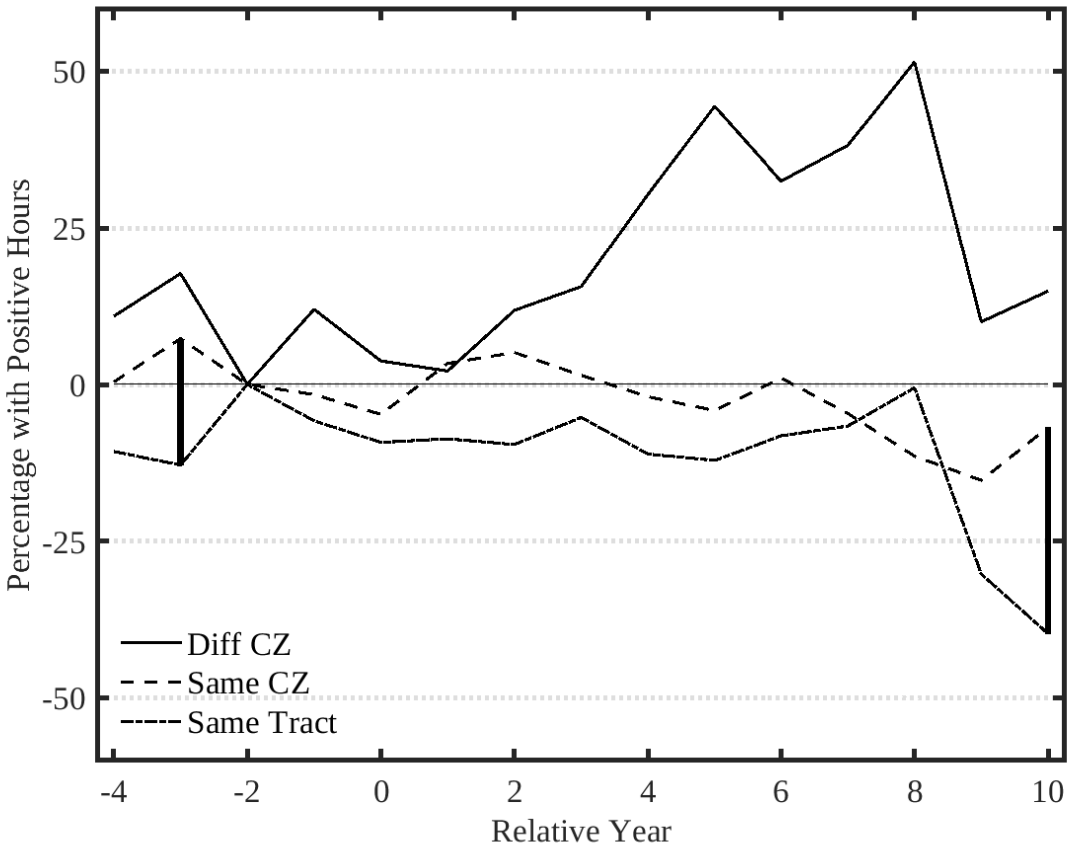
(c) Reweighted Means Without Age Trend



(d) Reweighted Means Including Age Trend

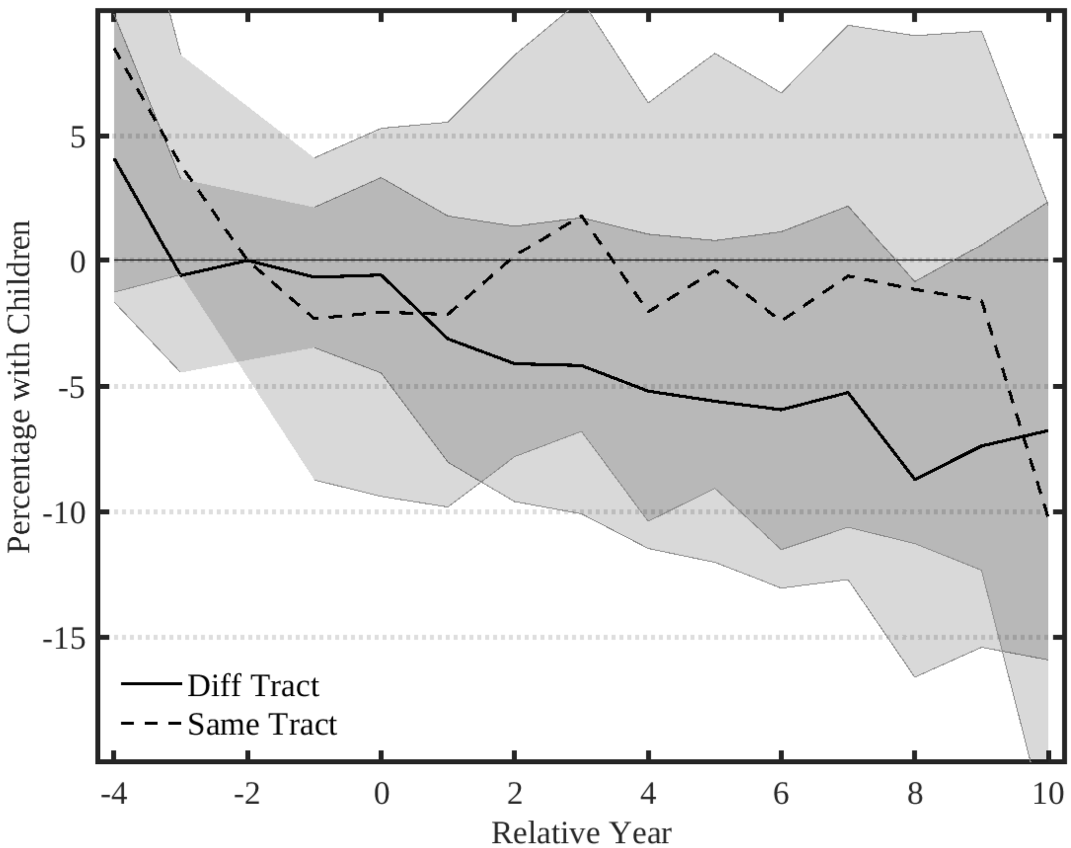
Appendix Figure 14: Reweighting on the Subsample with Common Support

Note: Restricting the sample to a subset of observations with common support gives qualitatively similar results to reweighting the whole sample, though with less precision. These panels replicate Figure 8, Figure 9, and Appendix Figure 12, but with datasets where the sample is restricted to observations where there is common support between the group that was displaced at home and the various other reweighting groups. Panel A shows propensity score reweighted sample means around a potential displacement, Panel B shows propensity score weighted regression coefficieints around a displacement, Panel C shows average earnings among young workers who are not displaced, and Panel D shows average earnings for workers who are not displaced, once a quartic trend in the worker's age is removed. Figure 8, Figure 9, and Appendix Figure 12 contain more details on the methodology.



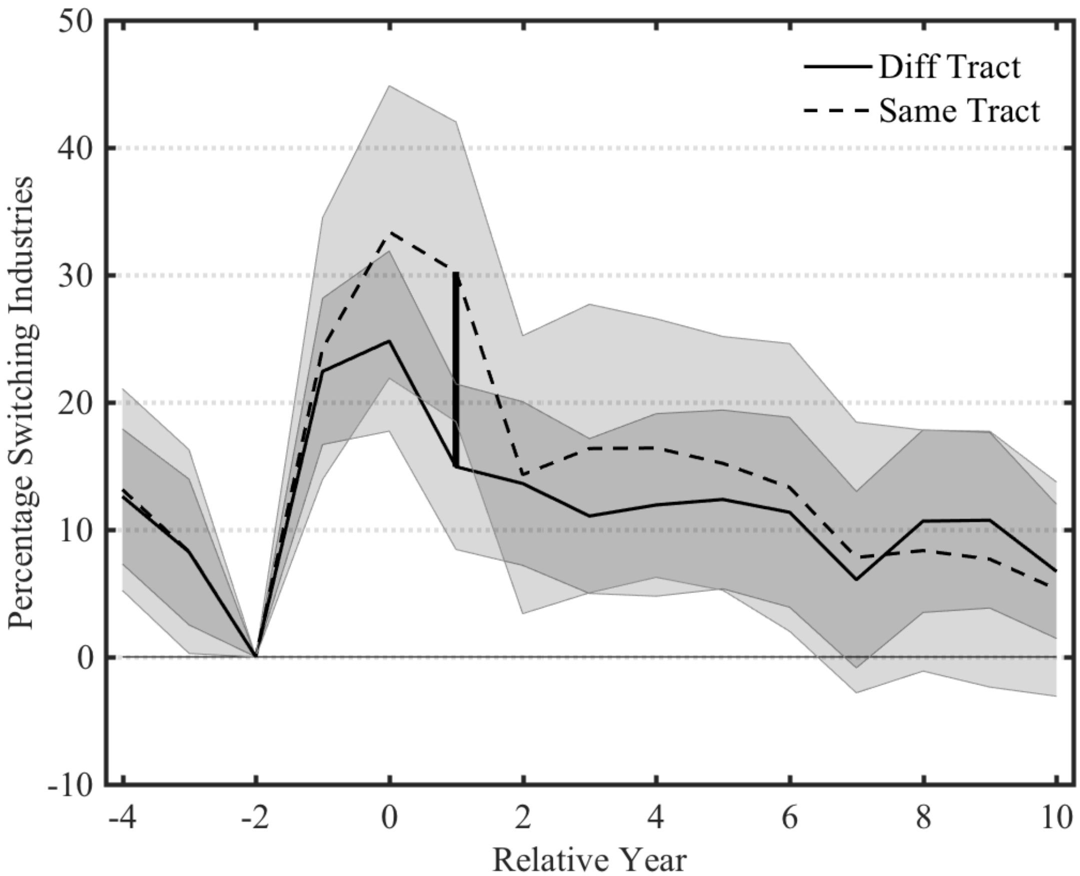
Appendix Figure 15: Employment of Fathers after Children's Displacements

Note: Fathers' employment outcomes are similar before and after a displacement among fathers who live in the same neighborhood and the same commuting zone as their children. However, employment rates among fathers who live outside their children's CZs do appear to be higher after children's displacements. The figure plots regression coefficients from a specification similar to equation (1) describing the impact of a job displacement on employment of the closest father or father in law including controls for employment to population ratios in the child's county and the mother's county as well as lagged fixed effects for the occupation and industry that the mother worked in. Vertical bars connect the outcome for fathers in the same CZ and fathers in the same tract when the estimates are statistically significantly different from one another in that year at the five percent level. Inference is done by clustering at the level of the mother. The definitions of displacements follow Figure 1, and Section 5.1 contains more information about these coefficients.



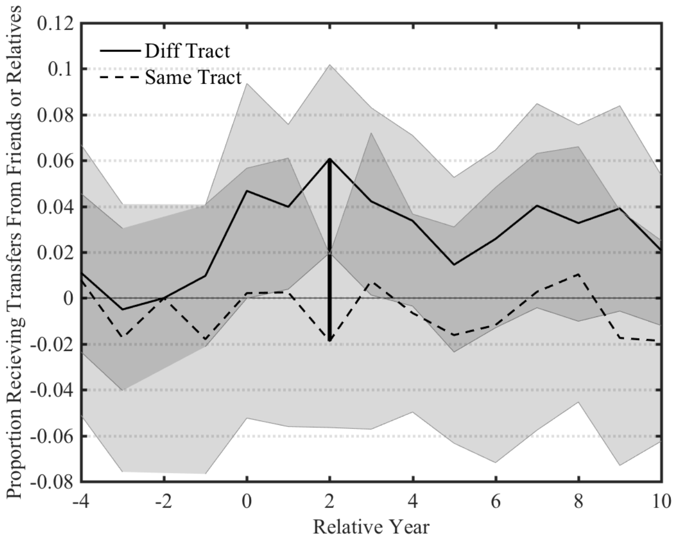
Appendix Figure 16: Having a Child after a Displacement

Note: There are no significant differences in the likelihood that workers have a child post displacement based on how far they live from their parents despite some indications that workers who live father from parents are less likely to have children after a job displacement. The figure plots regression coefficients from a specification similar to equation (1) describing the impact of a job displacement on having a child in the worker's household. The shading represents 95 percent confidence intervals, and any vertical bars connecting the two lines signify that the estimates are statistically significantly different from one another in that year, at the five percent level. We cluster the standard errors at the worker level.

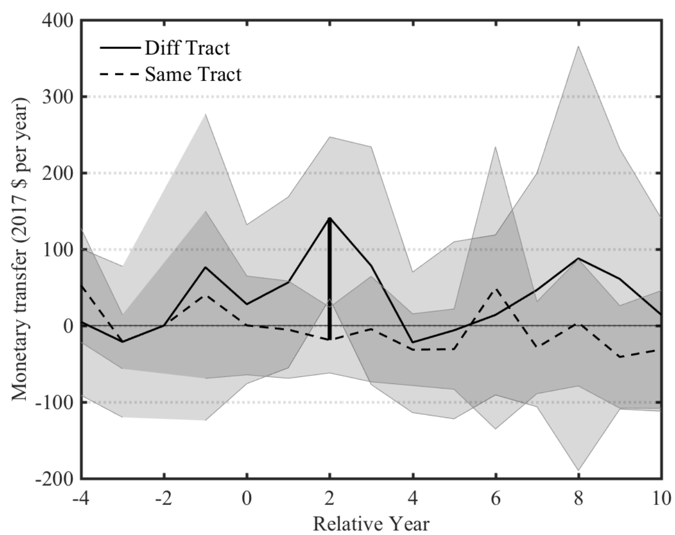


Appendix Figure 17: Probability of Switching Industries

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



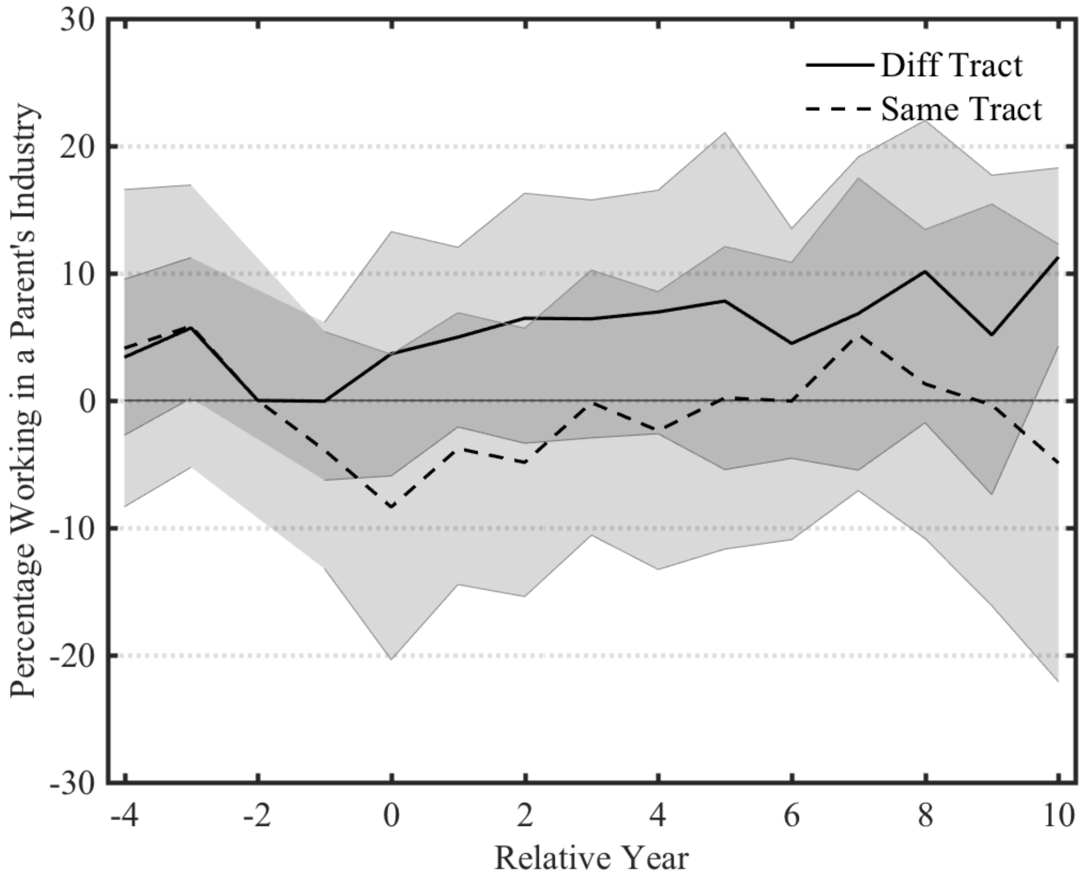
(a) Receiving a Monetary Transfer



(b) Total Monetary Transfers

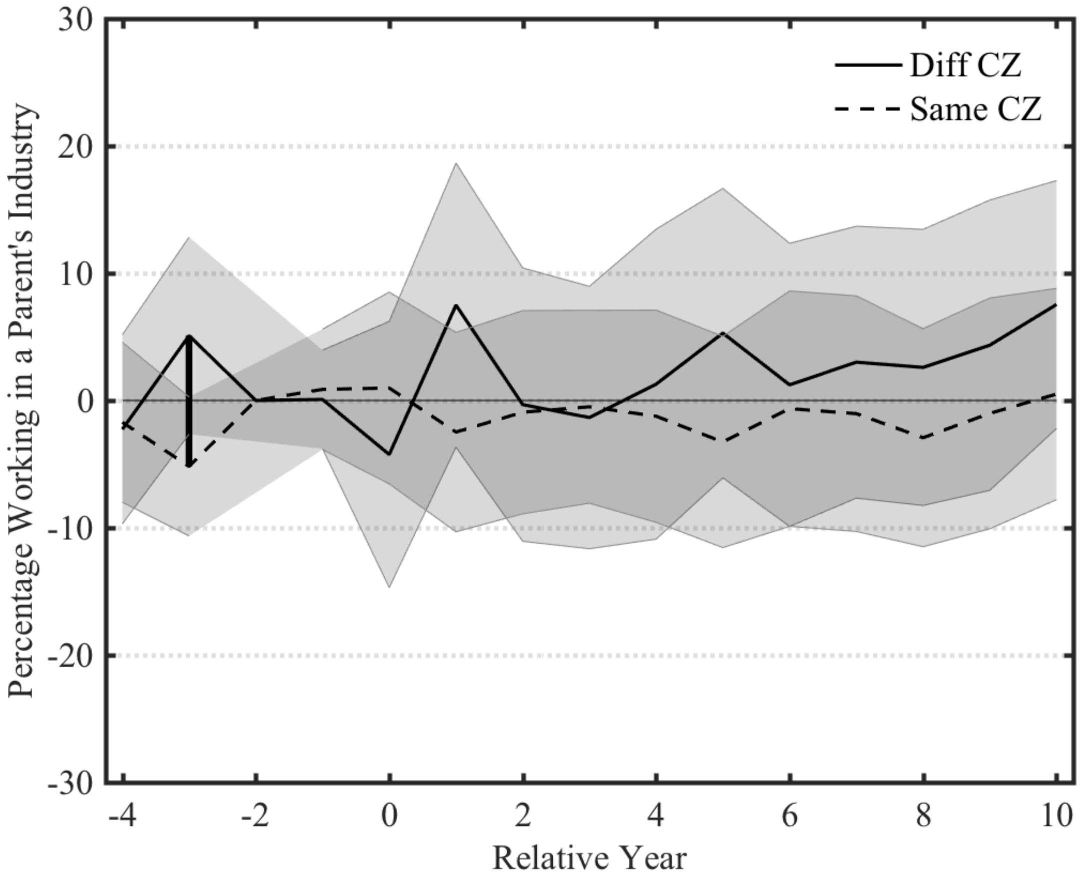
Appendix Figure 18: Monetary Transfers Around Displacements

Note: The annual question in the PSID asking about help from friends or relatives gives relatively noisy results around a displacement, but there is some evidence that workers who live farther from their parents are more likely to receive a small amount of monetary help after a displacement. These figures plot regression coefficients from equation (1) describing the impact of a job displacement on monetary transfers. The measure in Panel A is the proportion of workers who report that they received help from friends or family. The measure in Panel B is the reported dollar value per year. The shading represents 95 percent confidence intervals, and any vertical bars connecting the two lines signify that the estimates are statistically significantly different from one another in that year, at the five percent level. Shading and statistical significance are based on standard errors computed by clustering at the worker level. See Figure 12 and Appendix F for for more details on the specification and the data.



Appendix Figure 19: Working in a Parent's Industry for Older Workers

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



Appendix Figure 20: Working in a Parent's Industry by Commuting Zone

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

H Appendix Tables (For Online Publication)

Variable	Panel A: PSID Weights				Panel B: Reweighted			
	Same Tract		Different Tract		Same Tract		Different Tract	
	Displaced	Not Displaced	Displaced	Not Displaced	Displaced	Not Displaced	Displaced	Not Displaced
Earnings	$31,600	$36,500	$38,200	$41,900	$31,600	$31,700	$31,000	$32,800
	[1.00]	[0.03]	[0.00]	[0.00]	[1.00]	[0.36]	[0.43]	[0.93]
Average Change in Earnings	$2,100	$2,100	$3,000	$3,000	$2,100	$2,000	$3,400	$2,400
	[1.00]	[0.36]	[0.97]	[0.39]	[1.00]	[0.25]	[0.64]	[0.58]
Years of Schooling	11.96	12.45	12.29	12.72	11.96	12.12	12.01	12.15
	[1.00]	[0.00]	[0.00]	[0.00]	[1.00]	[0.84]	[0.61]	[0.93]
Share in Goods Industries	0.55	0.46	0.59	0.39	0.55	0.50	0.54	0.42
	[1.00]	[0.06]	[0.81]	[0.00]	[1.00]	[0.33]	[0.86]	[0.02]
Share Manager/Professional	0.13	0.18	0.15	0.21	0.13	0.16	0.11	0.15
	[1.00]	[0.11]	[0.03]	[0.00]	[1.00]	[0.92]	[0.54]	[0.91]
Employer Tenure	5.26	6.54	5.13	6.45	5.26	5.29	5.17	5.30
	[1.00]	[0.00]	[0.65]	[0.00]	[1.00]	[0.81]	[0.76]	[0.98]
Unemp Rate in County	7.66	7.31	7.87	7.19	7.66	7.50	7.94	7.62
	[1.00]	[0.34]	[0.98]	[0.04]	[1.00]	[0.89]	[0.45]	[0.96]
Age	27.97	28.57	28.30	28.53	27.97	27.70	27.60	27.76
	[1.00]	[0.00]	[0.02]	[0.00]	[1.00]	[0.34]	[0.32]	[0.43]
Number of Children	1.37	1.25	1.23	1.22	1.37	1.18	1.27	1.22
	[1.00]	[0.72]	[0.32]	[0.26]	[1.00]	[0.42]	[0.83]	[0.56]
Fraction Male	0.82	0.80	0.83	0.85	0.82	0.81	0.75	0.83
	[1.00]	[0.82]	[0.34]	[0.39]	[1.00]	[0.89]	[0.36]	[0.73]
Number of Records	177	3,021	351	8,394	177	3,021	351	8,394

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

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

	Unemployed (A)	Unemployed (H)	Working (A)	Working (H)	Pooled (A)	Pooled (H)
Panel A: Young Workers						
$\mathbb{P}[\textit{any search activity}]$	0.86	0.86	0.070	0.068	0.080	0.094
$\mathbb{P}[\textit{checked w/ frnds or rels}]$	0.25	0.32	0.023	0.018	0.028	0.029
$\mathbb{P}[\textit{searched but not w/ frnds or rels}]$	0.62	0.72	0.062	0.061	0.070	0.088
Panel B: Older Workers						
$\mathbb{P}[\textit{any search activity}]$	0.66	0.75	0.039	0.037	0.048	0.058
$\mathbb{P}[\textit{checked w/ frnds or rels}]$	0.22	0.22	0.013	0.015	0.017	0.023
$\mathbb{P}[\textit{searched but not w/ frnds or rels}]$	0.53	0.63	0.035	0.034	0.042	0.054

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	-0.005	-0.059	-0.082	-0.001	0.003
	(0.077)	(0.100)	(0.115)	(0.009)	(0.013)
Unemployment					0.75***
					(0.050)
Observations	1,741	1,509	1,509	57,460	58,969
R-squared	0.000	0.038	0.085	0.020	0.140
Demographic controls	NO	YES	YES	YES	YES
Year FEs	NO	NO	YES	YES	YES
Worker 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 worker level.