

PARENTAL PROXIMITY AND EARNINGS AFTER JOB DISPLACEMENTS*

Patrick Coate [†] Pawel Krolikowski [‡] Mike Zabek [§]

First draft: June 15, 2015

This draft: May 30, 2018

Abstract

Young adults, ages 25 to 35, who live in the same neighborhoods as their parents experience stronger earnings recoveries after a job displacement than those who live farther away. This result is driven by smaller on-impact wage reductions and sharper recoveries in both hours and wages, and is concentrated among workers with children. We find that geographic mobility, different job search durations, housing transfers, and *ex-ante* differences between workers are unlikely explanations. Our findings are consistent with a framework where some workers living near their parents face a better wage-offer distribution, though we find weak evidence of parental network effects.

JEL codes: J61, J64, R23.

Keywords: Parents, family ties, adult children, job loss, neighborhood, transfers, networks

*We would like to thank Dionissi Aliprantis, Joseph Altonji, John Bound, Audra Bowlus, Charlie Brown, Joel Elvery, Bruce Fallick, Darren Grant, Peter Hinrichs, Julie Hotchkiss, Susan Houseman, Fabian Lange, Mark Partridge, Roberto Pinheiro, Bob Schoeni, Jeff Smith, Frank Stafford, Mel Stephens, and Bryan Stuart for helpful conversations. We also thank participants from the PSID's Conference on Intergenerational Transfers and Time Use in Later Life, the PSID and H2D2 Seminar series at the University of Michigan, the 2017 Association for Public Policy and Management meetings, the 2018 Society of Labor Economists meetings, and the 2018 System Applied Microeconomics Conference. Meifeng Yang and Rachel Yang provided excellent research assistance. The collection of data used in this study was partly supported by the National Institutes of Health (NIH) under grant number R01 HD069609 and the National Science Foundation (NSF) under award number 1157698. We acknowledge funding from the National Institute on Aging and National Institute of Child Health and Human Development grants to the Population Studies Center at the University of Michigan (Coate and Krolikowski: T32 AG000221 and R24 HD041028; Zabek: R24 HD041028 and P2CHD041028). The views expressed in this paper are solely the responsibility of the authors and should not be interpreted as reflecting the views of the NIH, the NSF, the National Council on Compensation Insurance, Inc., the Federal Reserve Bank of Cleveland, or of any other person associated with the Federal Reserve System. A previous version of this paper circulated as "Family Ties and Worker Displacement."

[†]National Council for Compensation Insurance, pecoate@gmail.com

[‡]Federal Reserve Bank of Cleveland, Cleveland, OH 44114, pawel.krolikowski@clev.frb.org

[§]University of Michigan, Ann Arbor, MI 48109, zabek@umich.edu

1 Introduction

Americans live very close to their parents. In the Panel Study of Income Dynamics (2017), the median household head, ages 25 to 35, lives just over five miles from their parents and about one-fourth of these people live in the same neighborhoods as their parents.^{1,2} In modeling location choice, economists often explain this proximity to parents with a substantial amenity value of being close to home.³ We find that young adult children also experience labor market benefits when they live close to their parents.

Our focus on the labor market outcomes of young adults is motivated by two literatures. First, Wilson (1987) and Munshi and Rosenzweig (2016) show how parents can provide informal social insurance, and Kaplan (2012) and Dalton (2013) link parental insurance to the labor market outcomes of young adults. Second, Granovetter (1995) and Ioannides and Loury (2004) present evidence that the use of friends and relatives is prevalent and productive during the job search process, potentially improving the match quality between workers and firms. More specifically, Corak and Piraino (2011) and Kramarz and Skans (2014) show how parents can use their professional networks to help ease children’s entry into the workforce.

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

We show that young adults, ages 25 to 35, who lived in their parents’ neighborhoods prior to a displacement event experience a remarkably strong earnings recovery, catching up

¹As in Hellerstein, Kutzbach and Neumark (2015), for example, we use census tracts as a measure of neighborhoods.

²Researchers 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.

³Kennan and Walker (2011), for example, describe young men’s utility gain from living in the state where they grew up as equivalent to a \$20,000 wage increase per year. Other migration models with many locations (Bishop, 2008; Gemici, 2011; Diamond, 2016; Coate, 2017) find similar magnitudes in the US using slightly different combinations of datasets and definitions of home location.

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

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

The earnings results for young workers do not seem to be driven by either selection or differential migration between groups. People who live in the same neighborhoods as their parents tend to earn less, be less educated, and to live in areas with lower employment-to-population ratios. We show that our results are robust to using a propensity score reweighting technique that constructs a sample of people who live farther from their parents, but who are similar in terms of these observable characteristics. In particular, the reweighting procedure controls for the quality of the jobs that both groups lose, since it uses several detailed measures of job quality. We suspect that any differences that remain after the reweighting will lead us to understate the true effect of parental proximity, since workers who have difficulty coping with changes should tend to remain closer to their parents. We also find similar earnings differentials for young workers when we restrict to a subsample of workers who do not migrate after the displacement event.

We present three facts that highlight mechanisms besides parental housing transfers acting as insurance after job losses, a channel that Kaplan (2012) emphasizes. First, we find that the benefit of parental proximity extends to people who live in the same neighborhood as their parents, but who do not coreside with them.⁵ Second, children who live closer to their parents spend similar amounts of time unemployed and work a similar number of hours when they are employed after a displacement. Third, although we do find some support for increases in housing transfers around displacement, these increases are small and they do not appear to move differentially for those living close to and farther away from their parents.

A simple theory shows that our results are consistent with parents facilitating higher-wage job offers for their children and inconsistent with selection based on children’s desire to live at home. Parents could facilitate higher-wage jobs for their children by either finding jobs for them, or supporting them when they accept new jobs. Regarding the former, we find weak evidence that young adults who live closer to their parents are more likely to be employed in their parents’ industries after a job displacement. Regarding the latter, young

⁵We cannot reject the hypothesis that the earnings recovery of workers coresiding with their parents prior to displacement is the same as those living in their parents’ neighborhoods but not coresiding. We show, however, that for workers living outside of the neighborhood – in the same commuting zone and beyond – the benefit dissipates.

adults who have children of their own drive the stronger earnings recoveries among workers closer to their parents; this suggests that grandparents’ help in caring for their grandchildren is important. The theory also shows that a preference of some children to live closer to their parents would lead these children to have weaker wage recoveries after a job displacement.

The rest of the paper proceeds as follows. Section 2 describes our data, describes our sample, and presents our main earnings results using simple averages. Section 3 presents these results with a more sophisticated econometric setup, decomposes them into hours and wages, and presents additional results by age, proximity to parents, and geographic mobility. Section 4 shows that the earnings results are robust to reweighting and including interactions with additional characteristics. Section 5 investigates possible mechanisms related to parental employment networks and the role of housing transfers. Section 6 discusses selection and unobserved heterogeneity in the context of our results and 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

In choosing the appropriate dataset for our analysis, we are confined by three major restrictions: 1) The data need to include inter-generational linkages; 2) the data need to include job-history information to identify worker displacement events and to measure labor earnings; and 3) the data need to provide (preferably many) repeated observations on workers to implement a difference-in-difference approach. To our knowledge, the PSID is one of few datasets that meets all three requirements.⁶ The PSID began in 1968 with an interview of approximately 5,000 families, and follows any new families formed from the original group. We use the 1968 to 2013 waves of the PSID and we employ both the Survey Research Center (SRC) and Survey of Economic Opportunity (SEO) samples with longitudinal weights. Our conclusions, however, are unchanged when we drop the SEO sample and do not use weights. In our analysis, we include observations on people between the ages of 18 and 62 and we restrict our attention to what the PSID refers to as “household heads,” who are at least 16

⁶Other 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 wealth 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.

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

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

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 25 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.⁷

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 their parents at the time of the previous interview.⁸ We repeat this procedure for every base age between 25 and 55 and stack all the samples to create the final dataset.⁹ To track

⁷In the baseline approach we follow the job displacement literature in imposing a positive tenure cutoff, but setting this too high (like six years in Jacobson, LaLonde and Sullivan, 1993, for example) causes small sample sizes in our context.

⁸Because of the genealogical nature of the PSID data, we typically observe the parents of single adults or of one set of parents of a married couple. We treat cases in which we have the location of the husband or wife’s parents symmetrically, although sometimes this means we are using the household head’s parents and sometimes parents-in-law. In some cases, we will observe multiple parents’ locations (typically due to divorce of an original PSID household head); in these cases an adult child is coded as same neighborhood if they live in the same census tract as any parent or in-law.

⁹Note 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

when workers are displaced or not, let the *relative year* be zero in the base age, one in the year after, etc. For example, for the base age 40, the relative year is -8 when workers are 32, zero when workers are 40, and 6 when workers are 46.¹⁰

Table 1 shows the summary statistics for the final sample, which we restrict to observations with non-missing parents' location information.¹¹ Since we are using the PSID's poverty (SEO) oversample, and hence sampling is likely endogenous to our outcome (earnings), we take the suggestion of Solon, Haider and Wooldridge (2015) and use the longitudinal weights provided by the PSID throughout the analysis. The dataset consists of around 50,000 records, with an average of 20 years of observations for each, yielding roughly 1,000,000 person-year observations. The final dataset contains about 1,460 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.¹² Before displacement, the displaced workers are slightly younger, less educated, earn less, and have been with their employer for a 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.

age, etc. In our results, we will cluster standard errors at the worker level to account for these multiple observations. 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 workers who are 25 to 55 in order to insure that they have an attachment to the labor force.

¹⁰Due 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 mitigates this sample selection issue.

¹¹Common 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.

¹²This is consistent with displacements rates in previous work. See, for example, Davis and von Wachter (2011) where it is between three and four percent and Kuhn (2002), where it is between four and five percent.

2.2 Some Preliminary Evidence

Figure 1 plots the average earnings before and after displacements. The top panel in Figure 1 presents the average earnings of workers who were displaced (dashed) and not displaced (solid) averaged over the base ages 25 to 35. All earnings are measured in 2007 dollars (CPI-U-X1). These lines highlight the dramatic earnings consequences of worker displacement. The figure delivers three messages, which have been documented in many prior studies. First, displacement leads to a large initial drop in earnings. Annual earnings drop by around \$10,000, or around 20 percent of pre-displacement earnings.¹³ 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. There is a difference in the level of earnings before displacement between the control and treatment groups. We address these level differences in the empirical exercises that follow.

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

The bottom panel of Figure 1 shows that 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. 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

¹³The earnings question refers to the earnings during the last calendar year. The displacements have been coded to have happened between the previous survey date and the current survey date. Since most PSID interviews happen in April and May, most of our displacements are referring to displacements that happen 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'.

to the displacement event the difference in the earnings of the displaced and non-displaced who live in their parents' neighborhoods is around \$3,000 and the earnings of the displaced 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.

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 propensity score reweighting exercise (Section 4), which 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:

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

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 calendar 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. We pool the -4 dummy and the $+10$ dummy and omit the -2 dummy so all results are relative to two years before the displacement event.¹⁴ As a result, the coefficient δ^k captures

¹⁴We 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

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.¹⁵ The coefficient ζ^k picks up the additional effect of being neighbors with your parents on the earnings outcomes of displaced workers. This approach does not consider how other factors, correlated with living in the same neighborhood as one's parents, might explain the differential impact of displacement on earnings.¹⁶ 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 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).

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.

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

¹⁶We 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.

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

risers 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

We present a series of stylized facts in this section that give some indirect evidence about the mechanisms that lead to stronger earnings recoveries after job displacements among young workers who live closer to their parents. While none of these facts can conclusively identify a particular mechanism, they still are useful in identifying specific properties of the mechanisms involved. In particular, it appears that the mechanism is strongest when children live quite close to their parents, that the mechanism operates for people 25 to 35, but not for people 36 to 55, and that the mechanism operates both for workers who move and for workers who do not move after displacement. We also find evidence that, among those workers who live close to their parents, the mechanism is only operative for those who have children in their household at the time of displacement. In Section 5 we investigate possible mechanisms for our earnings results more directly, and in Section 6 we present a simple theoretical framework to interpret our findings.

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

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.¹⁷ This figure suggests that those living close to their parents, but not in the same neighborhoods, experience similar post-displacement earnings outcomes than those who live farther away. This evidence leans against the idea that parental employment networks, which should operate within the commuting zone, are an important part of the explanation. We present some evidence on parental networks in Section 5.2.

As in Huttunen, Møen and Salvanes (2016) and Cao and Stafford (2017), we document a large impact of job displacement on regional mobility (Appendix Figure 4 shows that switching labor markets increases sharply at the time of displacement).¹⁸ To abstract from mobility, we check that the post-displacement earnings trajectories are not driven by “movers” by restricting the sample to workers who do not change geographic location after the displacement event. For this analysis we take the county as the relevant measure of geography because restricting to young workers who never change tracts after a displacement event reduces the sample sizes dramatically.

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

To estimate the differential impact of parental proximity by whether children are present in the household prior to the displacement event, we estimate the following equation:

$$e_{iat} = \alpha_{ia} + \gamma_t + X_{iat}(\beta^A + \beta^H H_{ia}) + \sum_{k=-4}^{10^+} (D_{iat}^k \delta^k + D_{iat}^k H_{ia} \zeta^k + D_{iat}^k C_{ia} \nu_k + D_{iat}^k H_{ia} C_{ia} \eta_k) + \epsilon_{iat}, \quad (2)$$

which is similar to our baseline equation (1) but with two additions. First, we add terms, $D_{iat}^k C_{ia}$, and the coefficients on these terms, ν_k , capture the additional effect of children’s

¹⁷We 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.

¹⁸See Mincer (1978) for early work on family ties and migration decisions. Molloy, Smith and Wozniak (2011) and Kaplan and Schulhofer-Wohl (2017) provide a more extensive analysis of recent trends in migration, including declining inter-state migration at different geographies.

presence on the post-displacement earnings trajectories of workers. Second, we include the three-way interaction between the displacement dummies, D_{iat}^k , the dummy variable indicating whether worker i was neighbors with their parents in the year prior to age a , H_{ia} , and a dummy variable indicating whether worker i had children present in their household in the year prior to age a , C_{ia} , with associated coefficient η_k . We are most interested in the differential effect of parental proximity on the earnings outcomes of displaced workers by whether they have children in the household or not, which is captured by the difference between $\hat{\delta}_k + \hat{\zeta}_k + \hat{\nu}_k + \hat{\eta}_k$ and $\hat{\delta}_k + \hat{\nu}_k$.

Figure 8 presents evidence that it is those workers in their parents’ neighborhoods that have children in their household that drive our overall earnings results for young workers. In particular, Figure 8 Panel A shows that for those with children, the earnings trajectories around displacement look similar to the overall results. Panel B shows that for workers who live in their parents’ neighborhoods and who do not have children in their household, the earnings recovery looks remarkably similar to those without children who live farther away from their parents. We interpret these findings as evidence that parents can help their adult children by stepping in to provide childcare after a job displacement.

Several additional findings, which we present in Appendix A, suggest that the earnings differences that we observe are driven by having parents nearby. The baseline earnings results are robust to not using PSID sample weights, to including controls for local labor market conditions, to a distance-based measure of proximity (based on latitudes and longitudes of block groups), and are similar for men and women.¹⁹ The results are also similar when we look at workers who are actually co-residing with their parents as opposed to living in the same neighborhoods as their parents (Appendix Figure 9), in the spirit of Kaplan (2012). This finding suggests that the effect of parental proximity is not limited to transfers while coresiding, a topic we discuss further in Section 5. We have also found that the earnings differences for the two groups persist 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). These results suggest that parental proximity has an independent effect on post-displacement earnings from other factors in a worker’s home county.

¹⁹Local 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.

4 Propensity Score Reweighting

In this section, we use propensity score reweighting to choose 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.²⁰ 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 D presents the main results from the reweighting with different sets of characteristics, and the results tend to be quite stable once one controls for basic educational and demographic differences.²¹ 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 re-weight 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 re-weighting three separate groups of workers so

²⁰Since 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 post displacements.

²¹An additional concern is that by controlling for initial wages, we are choosing a comparison group of workers who are less skilled than the group living in their parents' neighborhoods. This would occur, for example, if there was a wage penalty for living close to one's parents, and this wage penalty meant that a worker who earned a given amount in another area would actually earn slightly less if they stayed at home. To address this concern, we present results in Appendix D 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.

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:

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

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

We can recover the conditional and unconditional probabilities in a semiparametric way using sample averages and logit regressions with flexible functional forms. We estimate the probabilities conditional on X_{ia} using a multinomial logit regression, as suggested by Imbens (2000). The predictors are a linear and a square term in pre-displacement earnings, average year to year changes in pre-displacement earnings, wages, a dummy for one-digit PSID occupations, a dummy for being college educated, the number of years of schooling that the worker has completed, the worker's age, the worker's tenure in the job, the employment-to-population ratio in the worker's county, a dummy for whether the worker is male, and a dummy for whether the worker is African American.²² The unconditional probabilities in equation (3) 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 B that average earnings are similar between the reweighted groups of workers who were not displaced.²³

²²The 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).

²³The reweighting exercise is similar to allowing several interaction terms in the regression specification.

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 9 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 10 shows the baseline regression specification (equation 1) with the new weights.²⁴ 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 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.

For comparison we present results from such an interacted model separately in Appendix C. In Appendix Figure 14, we implement the same propensity score approach, but use only a subset of observations where there is strong common support according to the selection method proposed by Crump et al. (2009) with similar results.

²⁴We 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 Investigating Mechanisms

We investigate housing transfers and parental networks to see if they might be responsible for differences in post-displacement earnings between people who live different distances from their parents. A relatively small amount of money is transferred through gifts of in-kind housing after displacements, and we find no evidence that these transfers are larger for people who lived closer to their parents before displacement. We find that children who live close to their parents are more likely to be employed in the same industry as their parents, controlling for local industrial composition, although the estimates are noisy. Additionally, in Appendix E we investigate whether search intensity and industry switching can account for our baseline earnings results, and we find that these mechanisms are unlikely to explain our baseline findings.

5.1 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 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 missed people who pay below-market rents.²⁵ 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

²⁵More 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.

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.²⁶

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 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.²⁷

The regression coefficients plotted in Figure 11 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

²⁶In 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.

²⁷This 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.

a displacement.²⁸ 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 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.²⁹

5.2 Employment in Parents' Industry

Young adults living close to their parents may have more productive job search experiences and healthier earnings post displacement as a result of family networks, as documented in Kramarz and Skans (2014). The basic idea is that, after job loss, parents may be able to assist their adult children by tapping into their own employment networks to help their adult children to gain employment in their own industry at jobs with favorable wages and wage growth.³⁰ 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.

²⁸Results for reported transfers of money, presented in Appendix F 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.

²⁹Kaplan (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.

³⁰There is some debate about how referral networks affect workers' wages. For example, Dustmann et al. (2017) find that hires from employment networks raise wages, while Bentolila, Micehacci and Suarez (2010) suggest that networks may reduce wages because they might assign 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 12 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.³¹ 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 estimates are noisy and the differences are not statistically significant from each other. 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 17 shows that older workers living close to their parents do not tend to move into their parents’ industries after a displacement event. Appendix Figure 18 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.

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

³¹We 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.

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.³²

Third, suppose that all workers receive utility b when living near parents, but workers differ in their unobserved ability and that the return to this ability can be earned in both locations, home and away. In this simple framework, when we compare workers in different locations who have the same wage, as we do with our reweighting approach, the workers 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 (2013) 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 thought 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.

³²Notice 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.

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 result persists after we apply a reweighting scheme that controls for observable differences between the two groups. Our results are consistent with a simple theory where some parents can facilitate higher wage job offers for children who live nearby. We find some weak evidence that parents find jobs in their industries for their children. Young adults with children of their own drive the stronger earnings recoveries among workers closer to their parents, so grandparents' help with child care might also be important.

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 not any more likely to move in with their parents.

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

An important implication of our finding that young adults experience post-displacement labor market benefits from living close to their parents is that any government program that substitutes for parental proximity will increase children's mobility. Even if a program perfectly crowds out parents' efforts, this program 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 on parental proximity and post-displacement earnings losses. Ideally, these new data would also facilitate additional analyses that focus on the mechanisms leading to our baseline results. We think that building and estimating a model that incorporates parental location (Kennan and Walker, 2011; Coate, 2017) and matches well the earnings losses of displaced workers (Jarosch, 2015; Krolikowski, 2017b) is a particularly fruitful way to proceed.

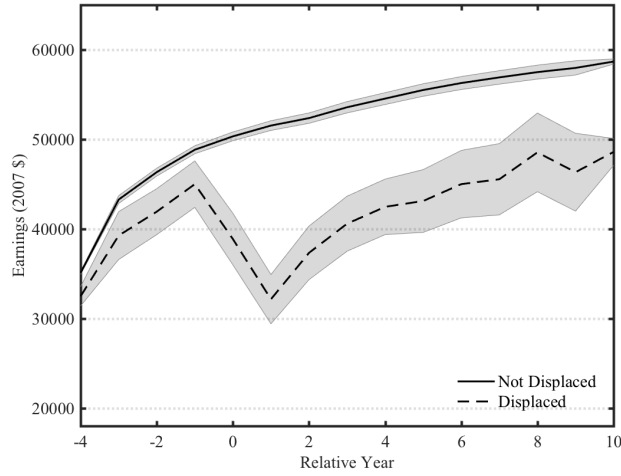
References

- Aguiar, Mark, Mark Bils, Kerwin Kofi Charles, and Erik Hurst.** 2017. “Leisure Luxuries and the Labor Supply of Young Men.” Available at <http://faculty.chicagobooth.edu/erik.hurst/research/>.
- Albouy, David, and Mike Zabek.** 2016. “Housing Inequality.” *NBER Working Paper Series*, 21916.
- Alesina, Alberto, Yann Algan, Pierre Cahuc, and Paolo Giuliano.** 2015. “Family Values and the Regulation of Labor.” *Journal of the European Economic Association*, 14(4): 599–630.
- Bentolila, Samuel, Claudio Micehacci, and Javier Suarez.** 2010. “Social Contact and Occupational Choice.” *Economica*, 77(305): 20–45.
- Bishop, Kelly C.** 2008. “A Dynamic Model of Location Choice and Hedonic Valuation.” *Unpublished, Washington University in St. Louis*. Available at http://law.wustl.edu/Centeris/Events/workfamilyseries/Papers/dynamic_migration.pdf.
- Bound, John, and Harry J. Holzer.** 2000. “Demand Shifts, Population Adjustments, and Labor Market Outcomes during the 1980s.” *Journal of Labor Economics*, 18(1): 20–54.
- Bui, Quoc Trung, and Claire Cain Miller.** 2015. “The Typical American Lives Only 18 Miles From Mom.” *NY Times, The Upshot*. Available at https://www.nytimes.com/interactive/2015/12/24/upshot/24up-family.html?_r=0.
- Burda, Michael C., and Antje Mertens.** 2001. “Estimating wage losses of displaced workers in Germany.” *Labour Economics*, 8(1): 15–41.
- Cadena, Brian C., and Brian K. Kovak.** 2016. “Immigrants Equilibrate Local Labor Markets: Evidence from the Great Recession.” *American Economic Journal: Applied Economics*, 8(1): 257–290.
- Cao, Yipei, and Frank P. Stafford.** 2017. “Relocation, Intergenerational Transfers and Extended Family Insurance.” Available at <https://sites.google.com/a/umich.edu/yipeicao/research>.

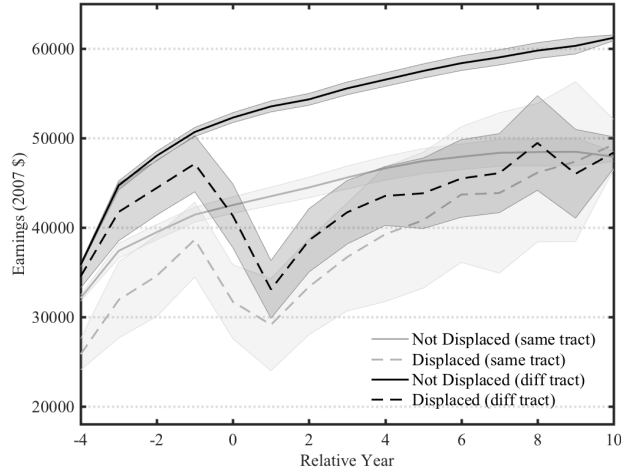
- Chari, Amalavoyal V., John Engberg, Kristin N. Ray, and Ateev Mehrotra.** 2015. "The Opportunity Costs of Informal Elder-Care in the United States: New Estimates from the American Time Use Survey." *Health Services Research*, 50(3): 871–882.
- Coate, Patrick.** 2017. "Parental Influence on Labor Market Outcomes of Young Workers." Available at <https://sites.google.com/site/patrickecoate/research>.
- Compton, Janice, and Robert A. Pollak.** 2015. "Proximity and Co-residence of Adult Children and their Parents in the United States: Descriptions and Correlates." *Annals of Economics and Statistics*, , (117/118): 91–114.
- Corak, Miles, and Patrizio Piraino.** 2011. "The Intergenerational Transmission of Employers." *Journal of Labor Economics*, 29(1): 37–68.
- Crump, Richard K., Joseph V. Hotz, Guido W. Imbens, and Oscar A. Mitnik.** 2009. "Dealing with Limited Overlap in Estimation of Average Treatment Effects." *Biometrika*, 96(1): 187–199.
- Dalton, Michael.** 2013. "Family Transfers in Response to Unemployment." Working Paper.
- Davis, Steven J., and Till M. von Wachter.** 2011. "Recessions and the Costs of Job Loss." *Brookings Papers on Economic Activity*, Fall(1): 1–72.
- Diamond, Rebecca.** 2016. "The Determinants and Welfare Implications of US Workers' Diverging Location Choices by Skill: 1980–2000." *American Economic Review*, 106(3): 479–524.
- DiNardo, John, Nicole M Fortin, and Thomas Lemieux.** 1996. "Labor Market Institutions and the Distribution of Wages, 1973–1992: A Semiparametric Approach." *Econometrica*, 64(5): 1001–1044.
- Dustmann, Christian, Albrecht Glitz, Uta Schönberg, and Herbert Brücker.** 2017. "Factors Determining Callbacks to Job Applications by the Unemployed: An Audit Study." *RSF: The Russell Sage Foundation Journal of the Social Sciences*, 3(3): 168–201.
- Fallick, Bruce C.** 1996. "A Review of the Recent Empirical Literature on Displaced Workers." *Industrial and Labor Relations Review*, 50(1): 5–16.
- Fortin, Nicole, Thomas Lemieux, and Sergio Firpo.** 2011. "Chapter 1 Decomposition Methods in Economics." In *Handbook of Labor Economics*. Vol. 4, 1–102.
- Ganong, Peter, and Daniel Shoag.** 2012. "Why Has Regional Convergence in the U.S. Declined?" *SSRN Electronic Journal*.
- Gemici, Ahu.** 2011. "Family Migration and Labor Market Outcomes." Available at https://sites.google.com/site/ahugemici/gemici_paper3.pdf.
- Goldschmidt, Deborah, and Johannes F. Schmieder.** 2015. "The Rise of Domestic Outsourcing and the Evolution of the German Wage Structure." *Quarterly Journal of Economics* (forthcoming).
- Granovetter, Mark S.** 1995. *Getting a Job: A Study of Contacts and Careers*. Chicago:University of Chicago Press.
- Hamermesh, Daniel S.** 1989. "What Do We Know About Worker Displacement in the U.S.?" *Industrial Relations*, 28(1): 51–59.
- Hellerstein, Judith K., Mark J. Kutzbach, and David Neumark.** 2015. "Labor Market Networks and Recovery from Mass Layoffs: Evidence from the Great Recession Period." NBER Working Paper 21262.

- Hirano, Keisuke, Guido W. Imbens, and Geert Ridder.** 2003. "Efficient Estimation of Average Treatment Effects Using the Estimated Propensity Score." *Econometrica*, 71(4): 1161–1189.
- Huttunen, Kristiina, Jarle Møen, and Kjell G. Salvanes.** 2016. "Job Loss and Regional Mobility." Forthcoming in *Journal of Labor Economics*. Available at <https://sites.google.com/site/krhuttunen/research>.
- Imbens, Guido W.** 2000. "The Role of the Propensity Score in Estimating Dose-Response Functions." *Biometrika*, 87(3): 706–710.
- Ioannides, Yannis M., and Linda Datcher Loury.** 2004. "Job Information Networks, Neighborhood Effects, and Inequality." *Journal of Economic Literature*, 42(4): 1056–93.
- Jacobson, Louis S., Robert J. LaLonde, and Daniel G. Sullivan.** 1993. "Earnings Losses of Displaced Workers." *American Economic Review*, 83(4): 685–709.
- Jarosch, Gregor.** 2015. "Searching for Job Security and the Consequences of Job Loss." Available at <https://sites.google.com/site/gregorjarosch/research>.
- Kaplan, Greg.** 2012. "Moving Back Home: Insurance against Labor Market Risk." *Journal of Political Economy*, 120(3): 446–512.
- Kaplan, Greg, and Sam Schulhofer-Wohl.** 2017. "Understanding the Long-Run Decline in Interstate Migration." *International Economic Review*, 58(1): 57–94.
- Kennan, John, and James Walker.** 2011. "The Effect of Expected Income on Individual Migration Decisions." *Econometrica*, 79(1): 211–251.
- Kletzer, Lori G.** 1998. "Job Displacement." *Journal of Economic Perspectives*, 12(1): 115–136.
- Kletzer, Lori G., and Robert W. Fairlie.** 2003. "The Long-Term Costs of Job Displacement for Young Adult Workers." *Industrial and Labor Relations Review*, 54(4): 682–698.
- Kramarz, Francis, and Oskar Nordström Skans.** 2014. "When Strong Ties Are Strong: Family Networks and Youth Labor Market Entry." *Review of Economic Studies*, 81(3): 1164–1200.
- Krolikowski, Pawel.** 2017a. "Choosing a Control Group for Displaced Workers." *Industrial and Labor Relations Review* (forthcoming).
- Krolikowski, Pawel.** 2017b. "Job Ladders and Earnings of Displaced Workers." *American Economic Journal: Macroeconomics*, 9(2): 1–31.
- Kuhn, Peter J.** 2002. "Summary and Synthesis." In *Losing Work, Moving On: International Perspectives on Worker Displacement*, ed. Peter J. Kuhn, Chapter 1, 1–104. Kalamazoo, MI: W.E. Upjohn Institute for Employment Research.
- Lin, I-Fen, and Hsueh-Sheng Wu.** 2010. "Does Social Support Mediate the Association Between Functional Disability and Depression?" Paper presented at the Population Association of America, Dallas, Texas. Available at <http://paa2010.princeton.edu/papers/101918>.
- Mincer, Jacob.** 1978. "Family Migration Decisions." *Journal of Political Economy*, 86(5): 749–773.
- Molloy, Raven, and Abigail Wozniak.** 2011. "Labor Reallocation over the Business Cycle: New Evidence from Internal Migration." *Journal of Labor Economics*, 29(4): 697–739.

- Molloy, Raven, Christopher L. Smith, and Abigail Wozniak.** 2011. "Internal Migration in the United States." *Journal of Economic Perspectives*, 25(3): 173–196.
- Munshi, Kaivan, and Mark Rosenzweig.** 2016. "Networks and Misallocation: Insurance, Migration, and the Rural-Urban Wage Gap." *American Economic Review*, 106(1): 46–98.
- Notowidigdo, Matthew J.** 2013. "The Incidence of Local Labor Demand Shocks." Available at <http://faculty.wcas.northwestern.edu/noto/>.
- Panel Study of Income Dynamics.** 2017. "Restricted use dataset." *Produced and distributed by the Survey Research Center, Institute for Social Research, University of Michigan*.
- Rosenbaum, Paul R., and Donald B. Rubin.** 1983. "The Central Role of the Propensity Score in Observational Studies for Causal Effects." *Biometrika*, 70(1): 41–55.
- Ruhm, Christopher J.** 1987. "The Economic Consequences of Labor Mobility." *Industrial and Labor Relations Review*, 41(1): 30–42.
- Solon, Gary, Seven J. Haider, and Jeffrey M. Wooldridge.** 2015. "What Are We Weighting For?" *Journal of Human Resources*, 2(50): 301–316.
- Stevens, Ann Huff.** 1997. "Persistent Effects of Job Displacement: The Importance of Multiple Job Losses." *Journal of Labor Economics*, 15(1): 165–188.
- Topel, Robert H.** 1986. "Local Labor Markets." *Journal of Political Economy*, 94(3): S111–S143.
- United States Census Bureau.** 2016. "Historical Living Arrangements of Adults." Available at <https://www.census.gov/data/tables/time-series/demo/families/adults.html>.
- von Wachter, Till, Jae Song, and Joyce Manchester.** 2009. "Long-Term Earnings Losses due to Mass Layoffs During the 1982 Recession: An Analysis Using Longitudinal Administrative Data from 1974 to 2004." Department of Economics, Columbia University.
- Wilson, William Julius.** 1987. *The Truly Disadvantaged: The Inner City, the Underclass, and Public Policy*. Chicago:University of Chicago Press.
- Yagan, Danny.** 2017. "Employment Hysteresis from the Great Recession." Available at <http://eml.berkeley.edu/~yagan/>.
- Zabek, Mike.** 2017. "Local Ties in Spatial Equilibrium." Available at <https://mikezabek.com>.



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



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

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

Note: Young workers who live in 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.

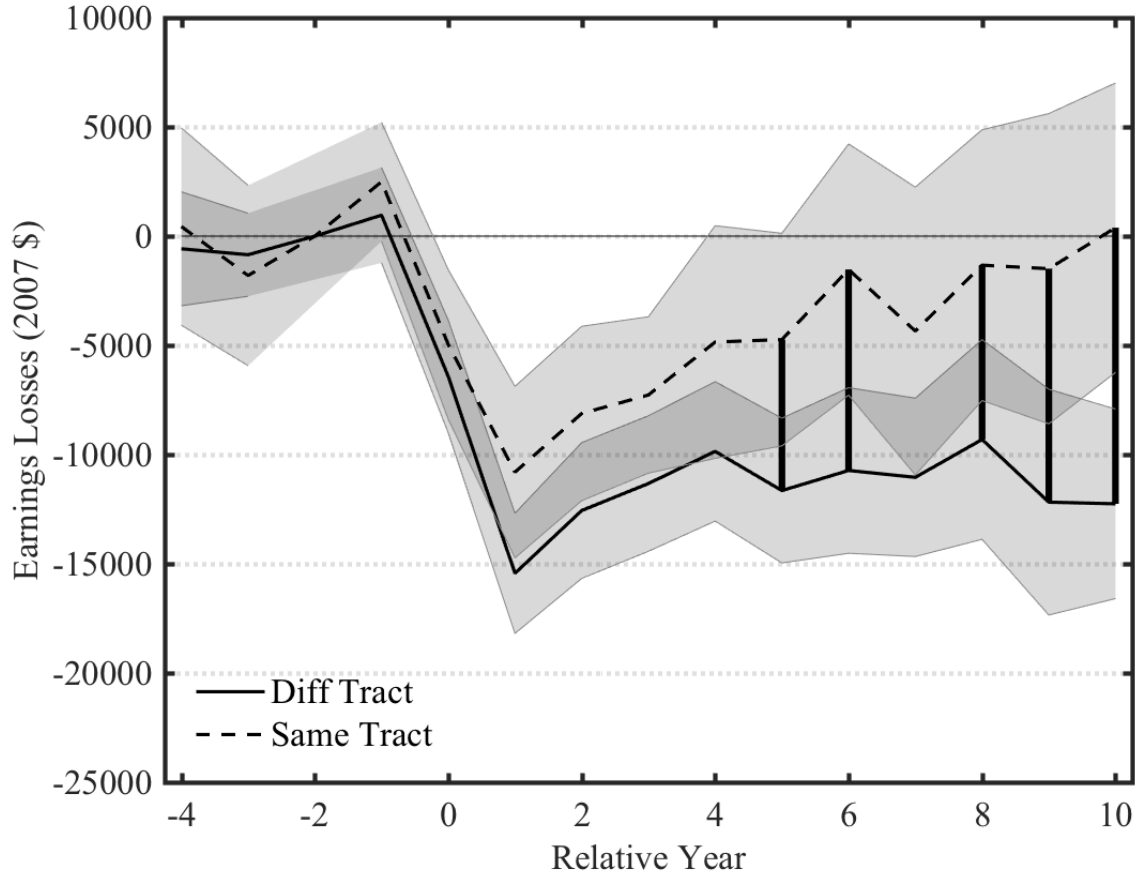
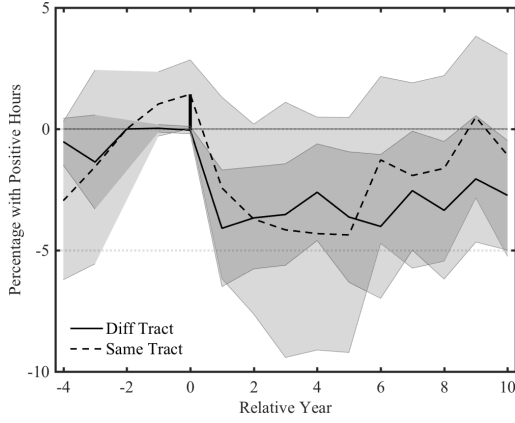
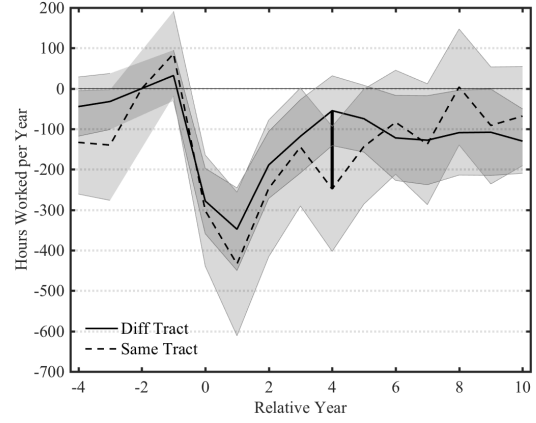


Figure 2: Earnings Losses for Young Displaced Workers

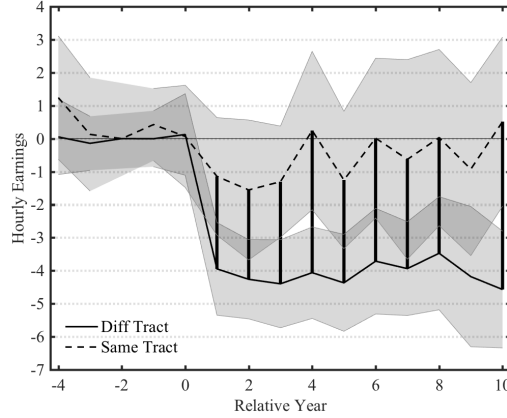
Note: In the medium- and long-run, young workers living in the same neighborhoods with their parents experience a full recovery in earnings after a displacement event. Young workers not living in their parents' neighborhoods experience large and permanent earnings losses, amounting to around 30 percent 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 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 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 are the same as in Figure 1 and Section 2 contains more information on the sample construction, data, and definitions.

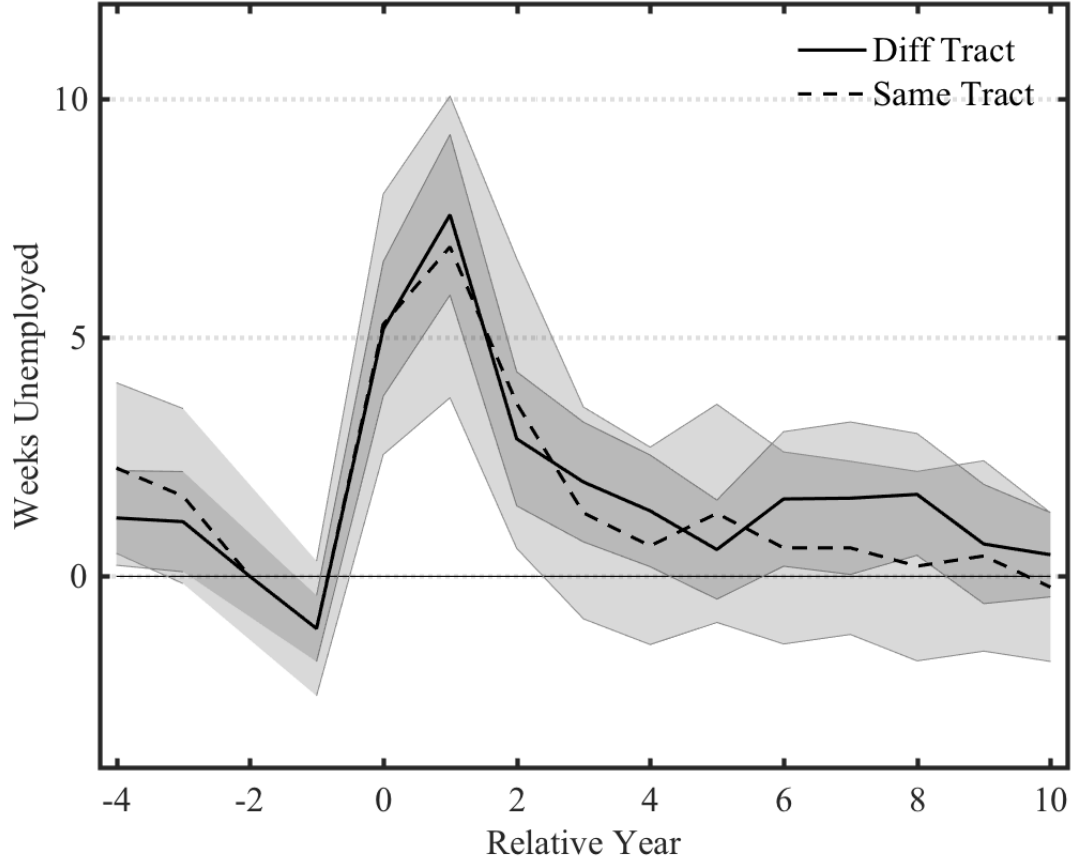


Figure 4: Weeks Spent Unemployed

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 the 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. 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.

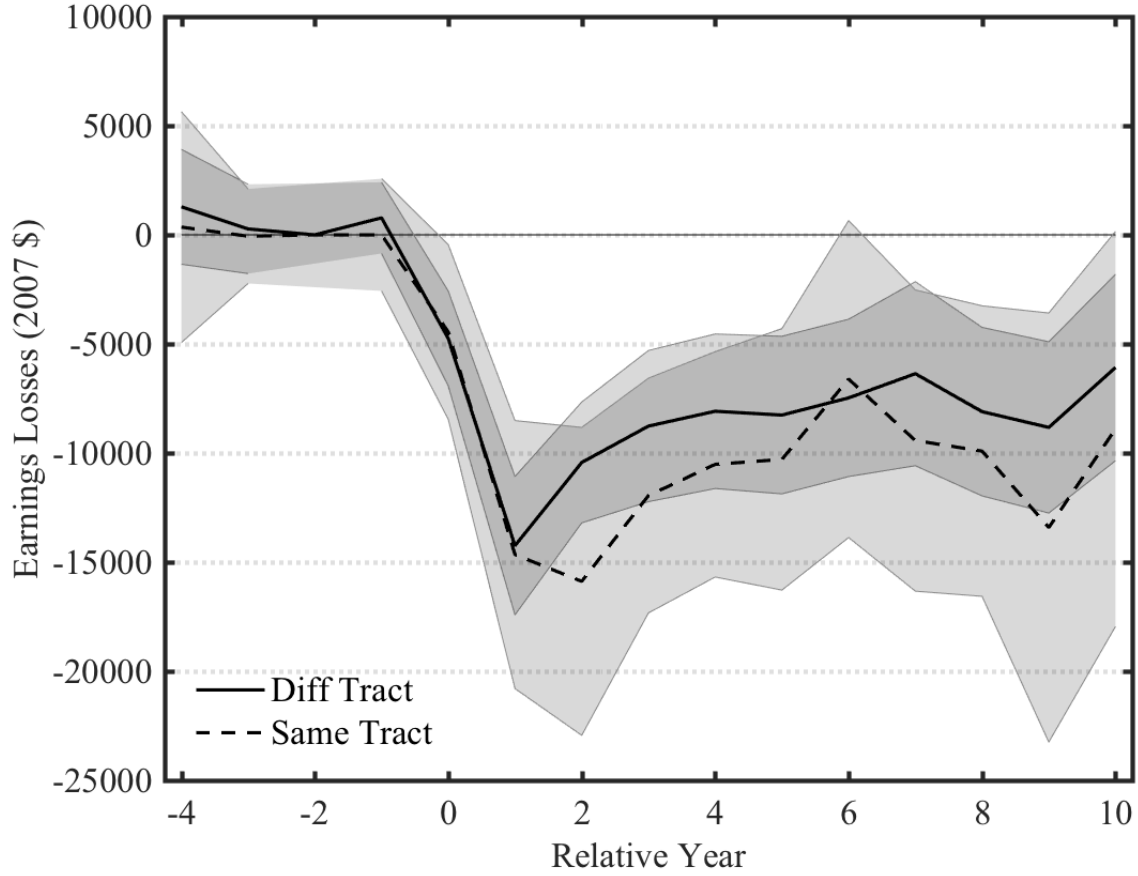


Figure 5: Earnings Losses for Older Displaced Workers

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 the 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. 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.

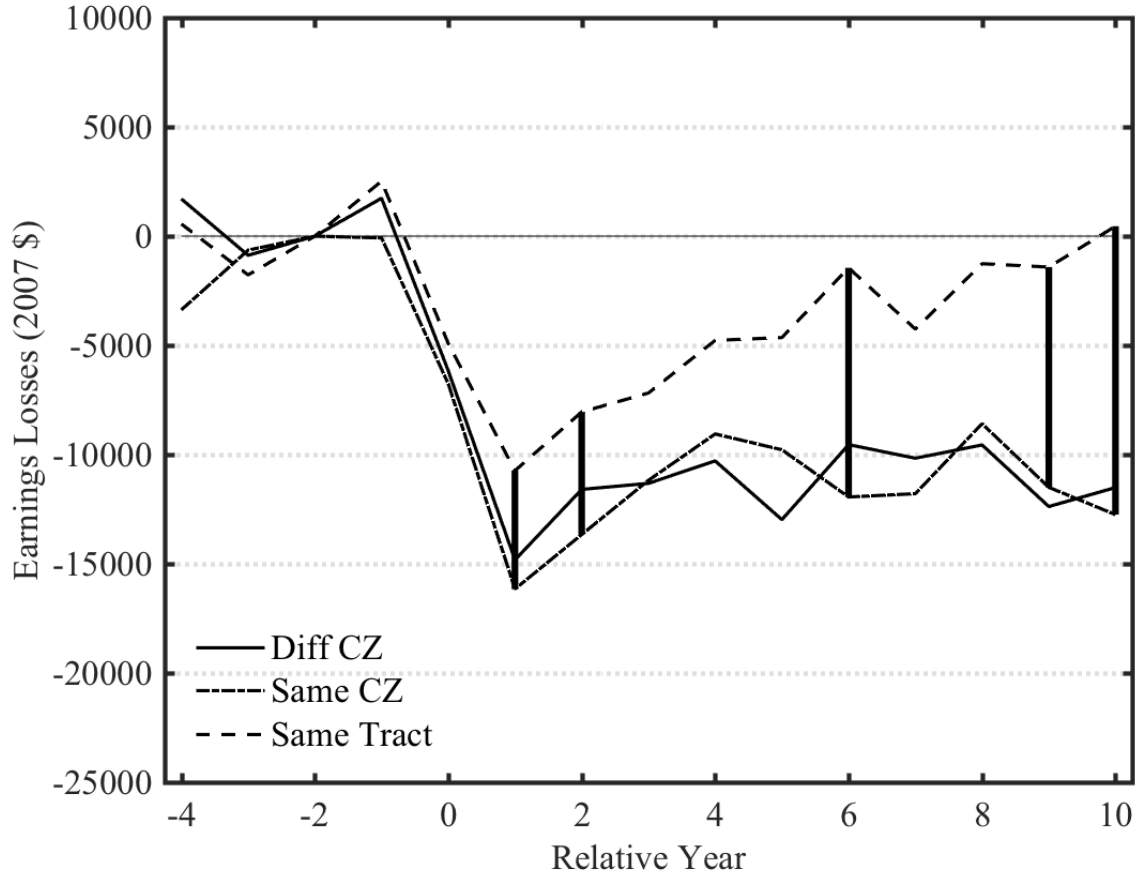


Figure 6: Earnings Losses for Young Workers by Different Proximities to Parents

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. The definitions of displacements follow Figure 1, Section 3.3 describes the specification in more depth, and Section 2 contains more information on the sample construction, data, and definitions.

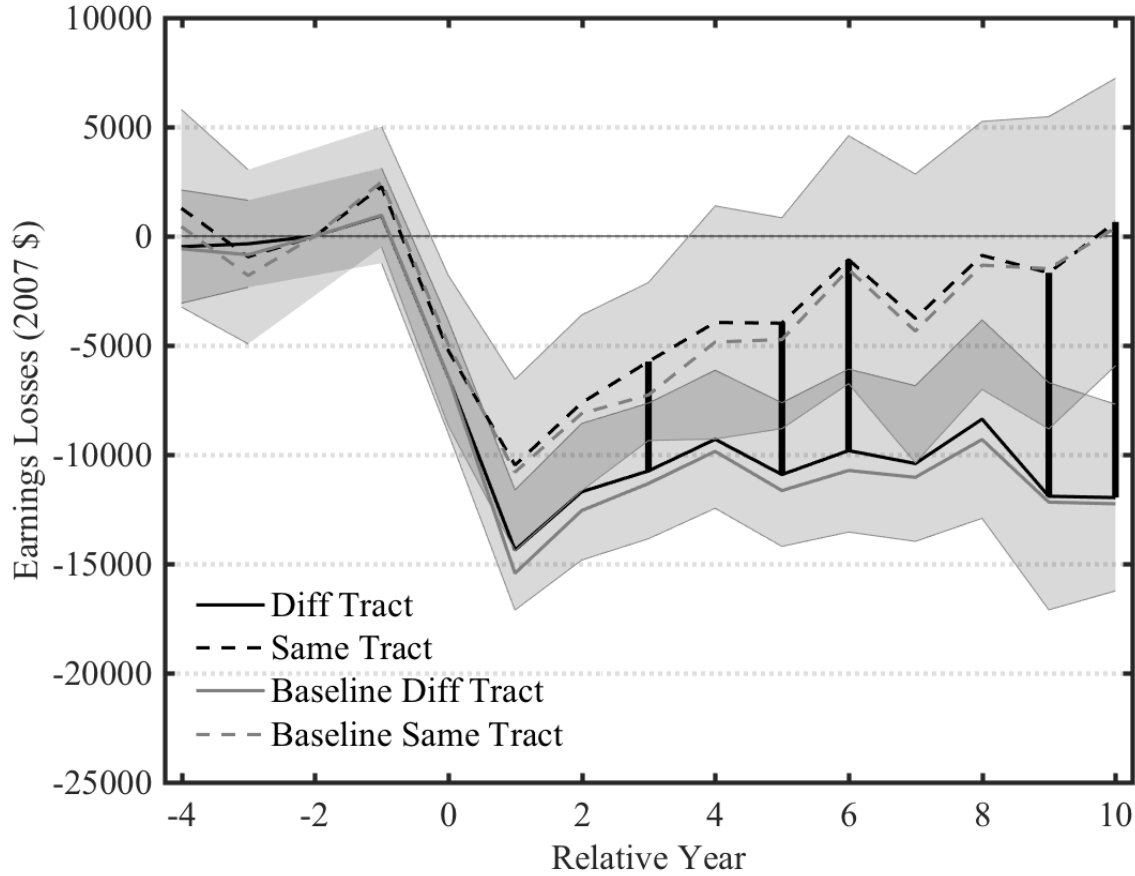
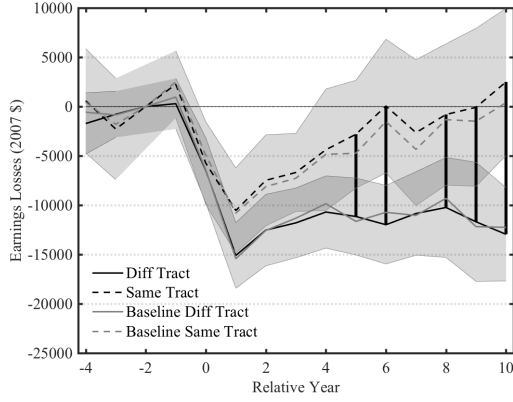
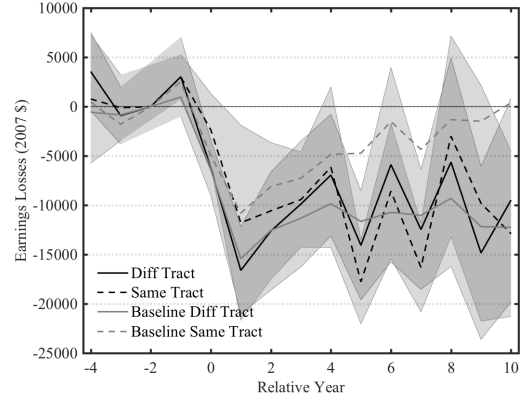


Figure 7: Earnings Losses for Young Workers Who Do Not Move

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 the 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. The definitions of displacements follow Figure 1, and Section 2 contains more information on the sample construction, data, and definitions.



(a) Young Workers With Children



(b) Young Workers Without Children

Figure 8: Earnings Losses for Young 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 with children, and Panel B to those without. All of the coefficients come from the same regression specification. The shading represents 95 percent confidence intervals, and the 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. The definitions of displacements follow Figure 1, and Section 2 contains more information on the sample construction, data, and definitions.

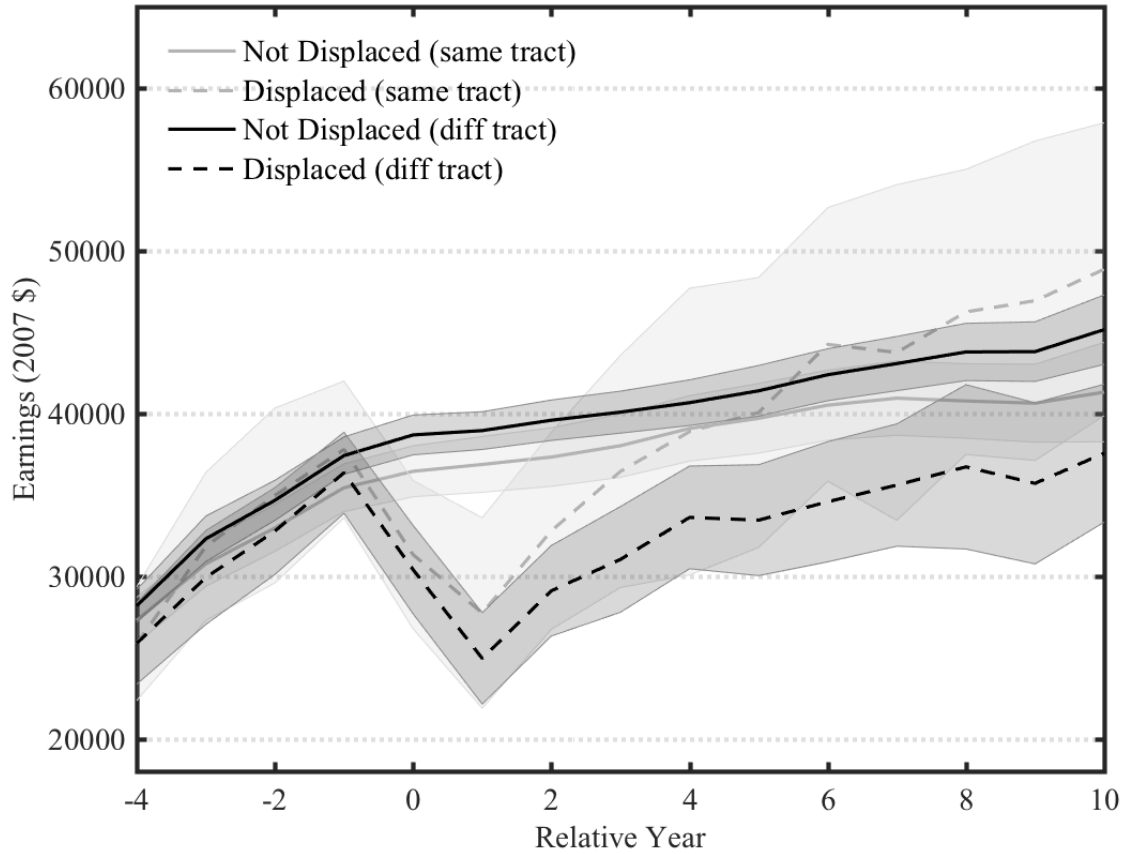


Figure 9: Means After Propensity Score Reweighting

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. 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.

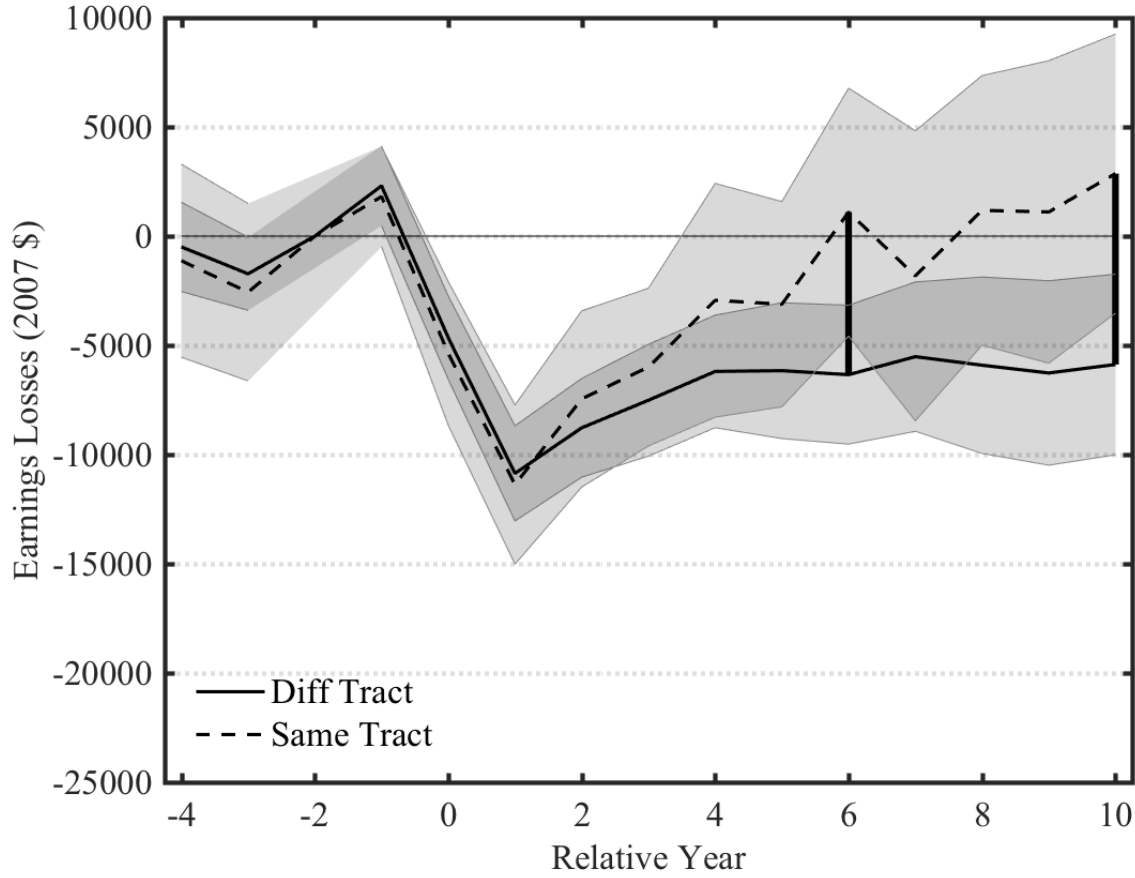
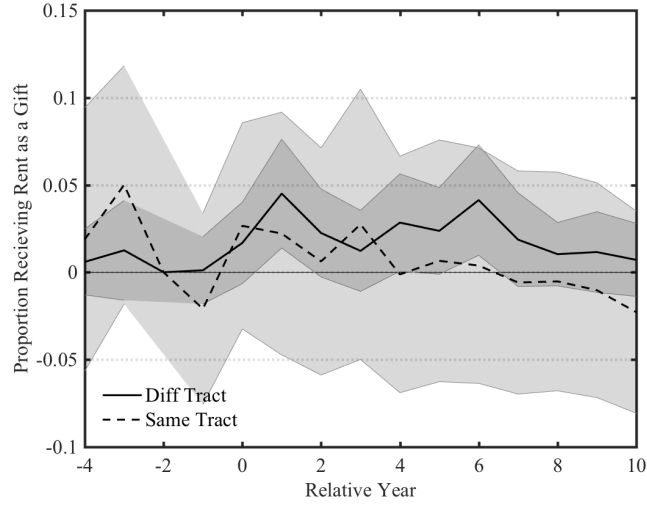
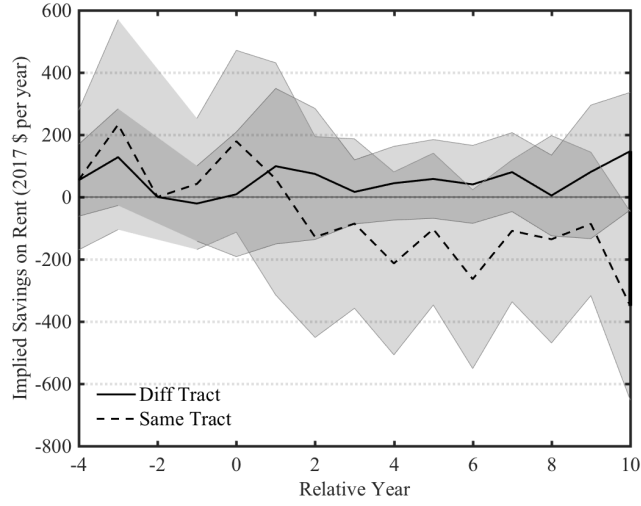


Figure 10: Earnings Losses After Propensity Score Reweighting

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 the 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. 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.



(a) Receiving Rent Entirely as a Gift



(b) Implied Rent Savings

Figure 11: Housing Transfers Around Displacements

Note: There is a small, but detectable, increase in housing transfers around displacement, primarily among workers lived outside their parents' neighborhoods before a displacement. These figures plot regression coefficients from equation (1) describing the impact of a job displacement two measures of in-kind transfers of housing to young workers. The measure in Panel A is the proportion of workers who report that they pay no rent and who then volunteer that this is because someone provided it as a gift to them. The measure in Panel B is the estimated dollar value that the worker's family unit saves in rent, based on living with another family unit. The shading represents 95 percent confidence intervals, and the 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. Each measure is explained in more depth in Section 5.1, the definitions of displacements follow Figure 1, and Section 2 contains more information on the sample construction, data, and definitions.

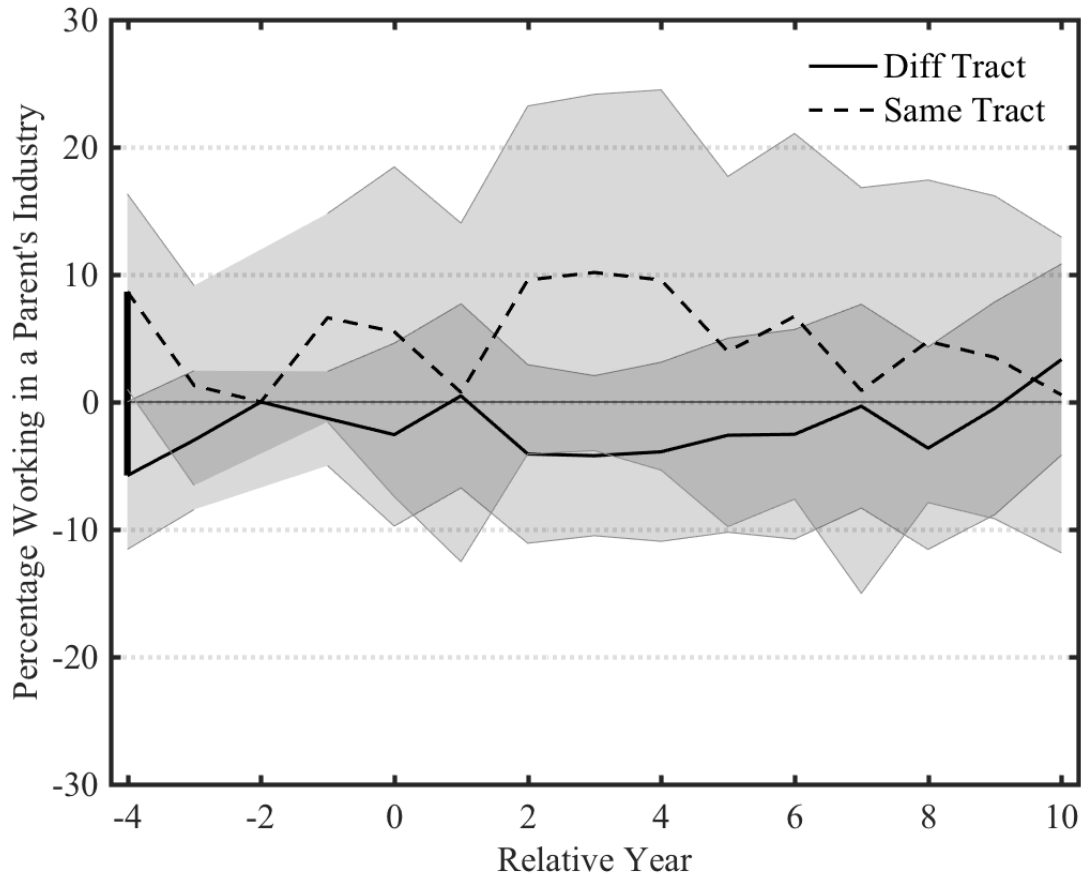


Figure 12: Working in a Parent's Industry

Note: Workers appear to be slightly more likely to work in their parents' industries around a displacement event, though the effect can vary depending on how industries are measured. It does not appear to be due to local industrial composition, however. These figures plot regression coefficients from equation (1) describing the impact of a job displacement on the proportion of workers who work in the same one-digit PSID coded industry as a parent. The shading represents 95 percent confidence intervals, and the 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. The definitions of displacements follow Figure 1, and Section 2 contains more information on the sample construction, data, and definitions.

Variable	Same Tract				Different Tract	
	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
Number of Children	1.24	1.24	1.37	1.25	1.21	1.23
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
Number of Children	1.14	1.11	1.22	1.20	1.11	1.09
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, and tend to have lower socioeconomic status than workers who were not displaced and who lived further from their parents. This table presents (individual PSID) weighted averages using unbalanced data from the 1968-2013 PSID surveys. Dollar figures are in 2007 dollars using the CPI-U-X1. All variables are measured in the year before a potential displacement (relative year -1). The PSID sample of household heads is composed chiefly of men. We restrict to observations that appear in our baseline sample (equation 1). The definitions of displacements follow Figure 1 and Section 2 contains more information on the sample construction, data, and definitions.

Variable	Same Tract		Different Tract		Same Tract		Different Tract	
	Displaced	Not Displaced	Displaced	Not Displaced	Displaced	Not Displaced	Displaced	Not Displaced
	Panel A: PSID Weights				Panel B: Reweighted			
Earnings	\$35,000 [1.00]	\$39,700 [0.03]	\$44,700 [0.00]	\$48,300 [0.00]	\$35,000 [1.00]	\$33,100 [0.36]	\$33,100 [0.43]	\$34,800 [0.93]
Average Change in Earnings	\$2,900 [1.00]	\$2,300 [0.36]	\$2,900 [0.97]	\$3,500 [0.39]	\$2,900 [1.00]	\$2,100 [0.25]	\$3,300 [0.64]	\$2,500 [0.58]
Years of Schooling	12.50 [1.00]	13.10 [0.00]	13.31 [0.00]	13.94 [0.00]	12.50 [1.00]	12.46 [0.84]	12.37 [0.61]	12.52 [0.93]
Share in Goods Industries	0.53 [1.00]	0.43 [0.06]	0.51 [0.81]	0.34 [0.00]	0.53 [1.00]	0.48 [0.33]	0.51 [0.86]	0.40 [0.02]
Share Manager/Professional	0.20 [1.00]	0.27 [0.11]	0.31 [0.03]	0.41 [0.00]	0.20 [1.00]	0.20 [0.92]	0.17 [0.54]	0.20 [0.91]
Employer Tenure	5.32 [1.00]	6.79 [0.00]	5.16 [0.65]	6.42 [0.00]	5.32 [1.00]	5.39 [0.81]	5.20 [0.76]	5.33 [0.98]
Unemp Rate in County	7.47 [1.00]	7.17 [0.34]	7.48 [0.98]	6.80 [0.04]	7.47 [1.00]	7.42 [0.89]	7.83 [0.45]	7.45 [0.96]
Age	28.32 [1.00]	29.26 [0.00]	29.14 [0.02]	29.51 [0.00]	28.32 [1.00]	28.04 [0.34]	27.95 [0.32]	28.09 [0.43]
Number of Children	1.26 [1.00]	1.21 [0.72]	1.11 [0.32]	1.11 [0.26]	1.26 [1.00]	1.16 [0.42]	1.23 [0.83]	1.18 [0.56]
Fraction Male	0.82 [1.00]	0.81 [0.82]	0.86 [0.34]	0.85 [0.39]	0.82 [1.00]	0.81 [0.89]	0.77 [0.36]	0.83 [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 receive 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 5.1.

	Same Tract	Diff Tract
$\mathbb{P}[\textit{in parent's industry}]$	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.³³

Similar to the exercise in the main text where we break out proximity by same tract, same commuting zone but not same tract, and farther away, we also look at workers who are actually co-residing with their parents as opposed to living in the same neighborhoods as

³³Estimating 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).

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.³⁴ 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.

B 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 they 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

³⁴This is based on a retrospective question in the interview.

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.

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

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

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

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

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

As before, the effects of a displacement for different groups are different linear combinations of $\hat{\delta}^k$ and $\hat{\zeta}^k$ terms, and we plot these as a simple way of understanding the impact

of these different specifications. We plot $\hat{\delta}^k + \hat{\zeta}^k$ as the effect for people living near their parents and $\hat{\delta}^k$ for people living farther from their parents. Since we are not including the $\hat{\xi}^k$ terms, the effect is for the omitted group where X_{ia}^C is a dummy variable and the value at the mean of X_{ia}^C (since we de-mean X_{ia}^C) when it is a continuous variable. Note that the difference between the two lines is, due to functional form, unchanged regardless of the value of X_{ia}^C .

Appendix Figure 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.

D 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 further from their parents, with jobs of constant observable quality, may be living further 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 findings are quite similar, and sometimes more precisely estimated, when we include different sets of characteristics.

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

To implement each strategy, we estimate our baseline specification, equation 1, with weights, shown in equation 3, using different sets of co-variates.

D.1 Including Parents’ Characteristics

For the first exercise, we include all of the same characteristics as in Section 4 as well as several characteristics of both the mother and the father, entered separately. These characteristics include a dummy for whether the parent is college educated, a dummy for whether the parent is currently employed, and the parent’s age and 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 4.³⁵

The results, shown in Panel A of Appendix Figure 11, are very similar to the propensity score reweighted results in Figure 10. 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 10, more of the differences between groups are significant at the five percent level, and confidence intervals are slightly smaller.

³⁵Parents’ 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.

D.2 Predetermined Characteristics

For the second exercise, we restrict the characteristics to those that are predetermined at the time that the worker decides whether to live in their parents' neighborhood. These characteristics are a dummy for whether the worker is college educated, their total number of years of schooling, 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 10. 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 re-weightings. The estimates are somewhat less precise, but the larger differences between the estimates counteract this in terms of statistical significance.

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:

$$search_{it} = \alpha + \beta Sametract_{it} + \gamma X_{it} + \epsilon_{it} \quad (5)$$

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 re-

produces 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_{it} , that equals one if the worker switches one-digit industry between survey year t and $t + 1$.

Appendix Figure 15 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 15 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 gives 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.

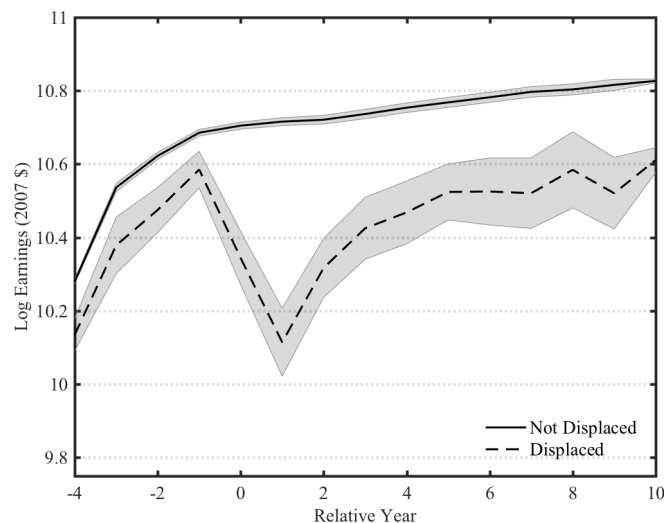
F.2 Cash Transfers from Friends and Relatives

In addition to transfers of housing, children can receive cash transfers from their parents. Appendix Figure 16 shows how these change around displacement, using our main event study specification including fixed effects, an age quartic, and other controls. For these plots

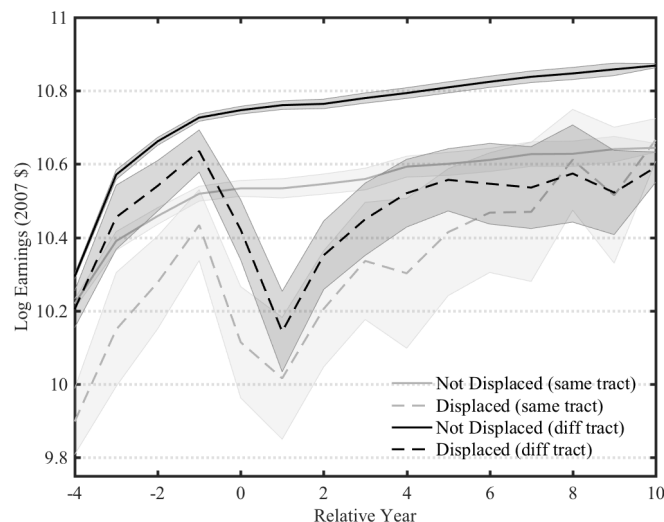
we use an annual question in the PSID that asks how much money a household received from friends and relatives. Much more detailed transfer data exist in two single year transfer supplements to the PSID. These are of limited use in our context, however, because our estimation strategy is only possible when we have a data across many years.

Appendix Figure 16 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)



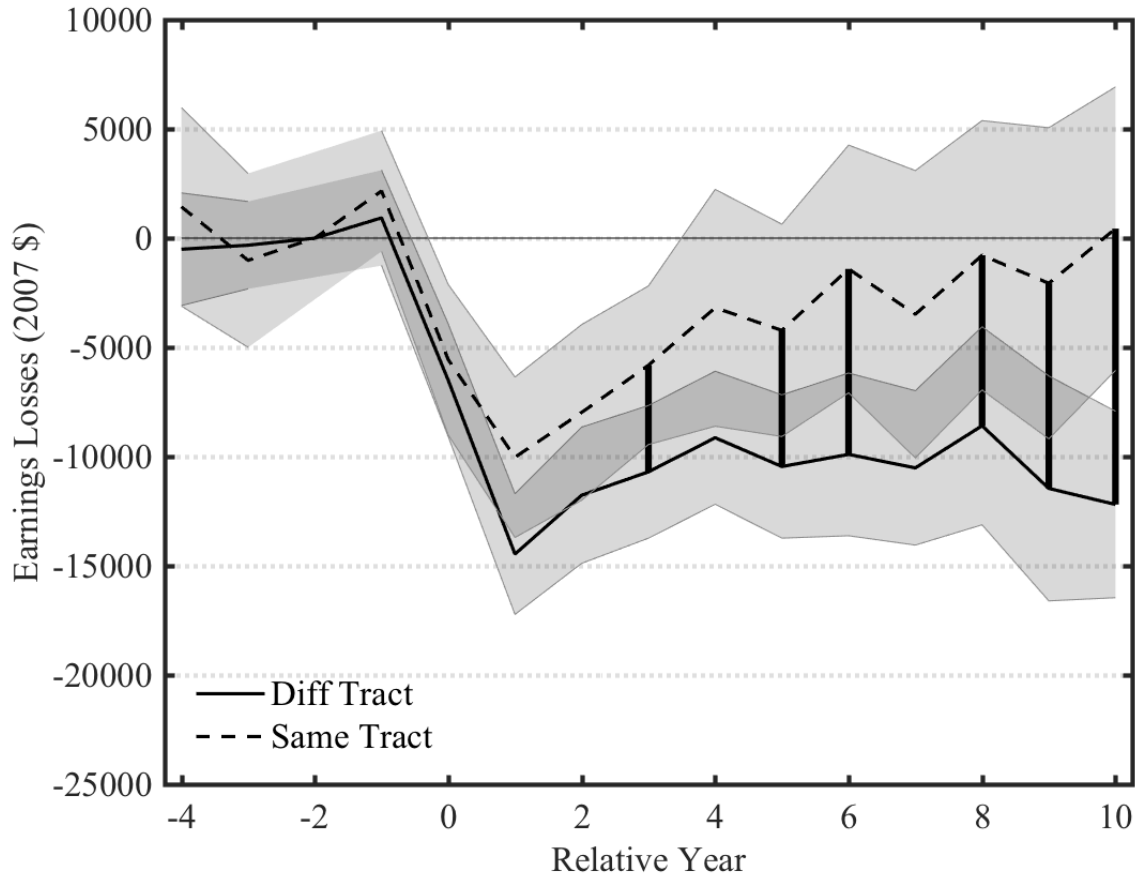
(a) Average Log Earnings for Young Displaced and Non-Displaced Workers



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

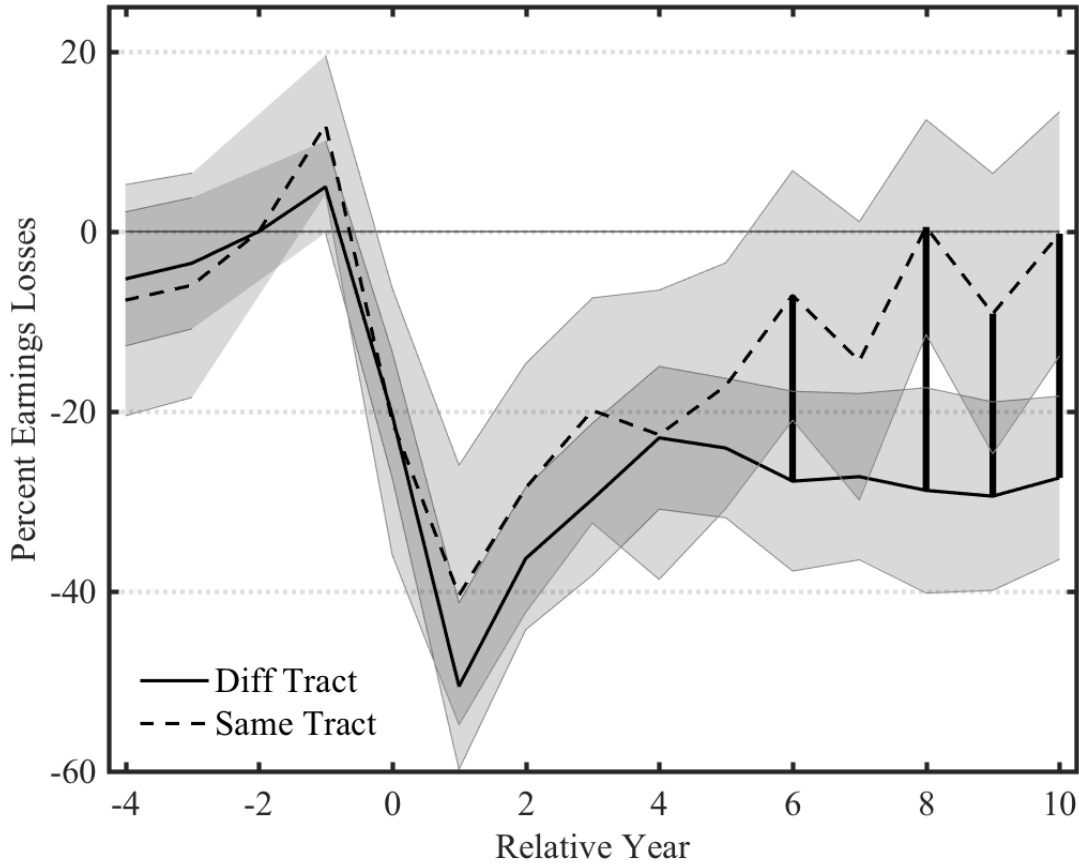
Appendix Figure 1: Average Log Earnings for Young Displaced Workers by Proximity to Parents

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.



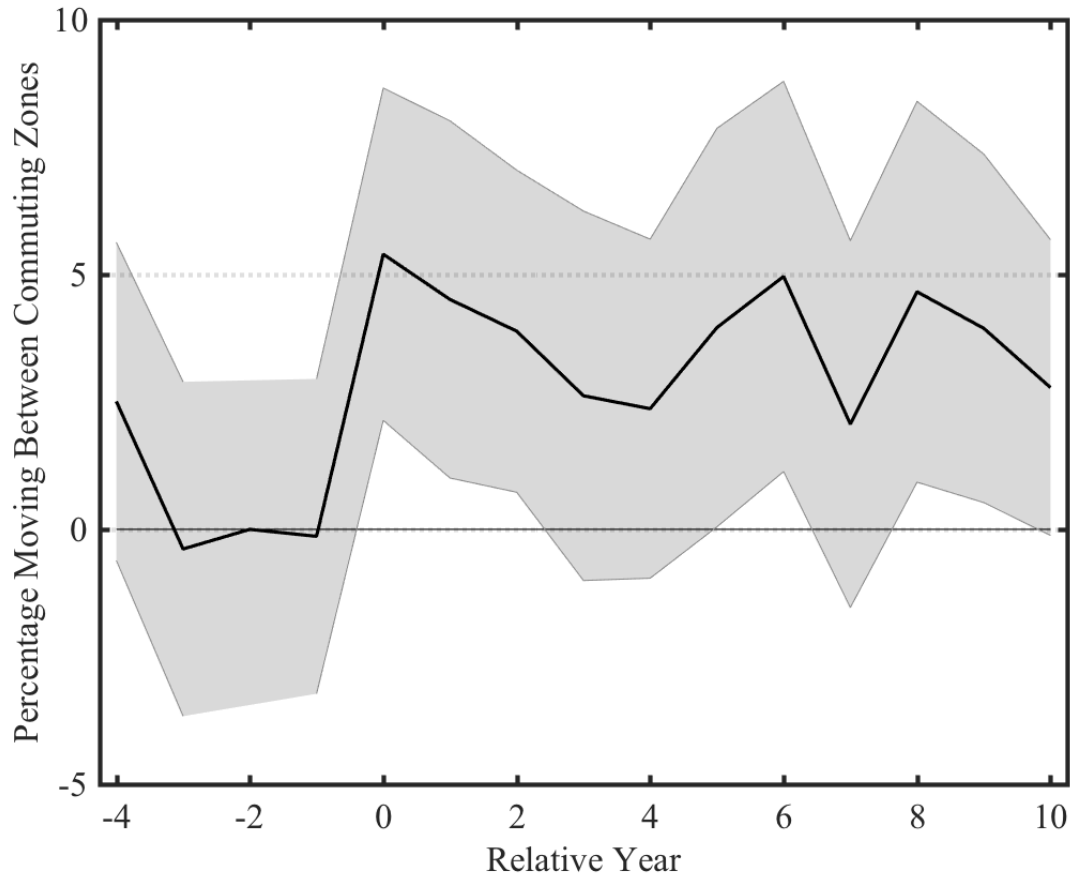
Appendix Figure 2: Earnings Losses for Young Displaced Workers (Excl. Zeroes)

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 the 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.



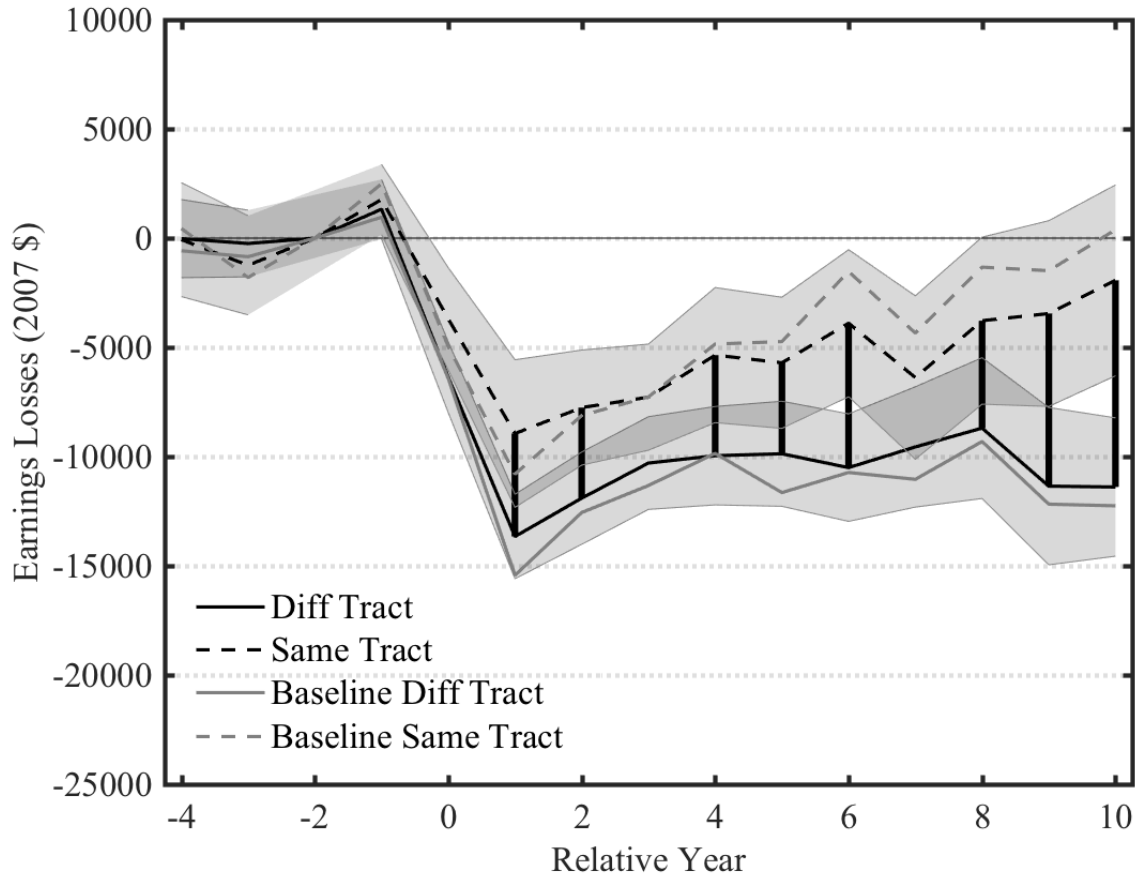
Appendix Figure 3: Percent Earnings Losses for Young Displaced Workers

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 the 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.



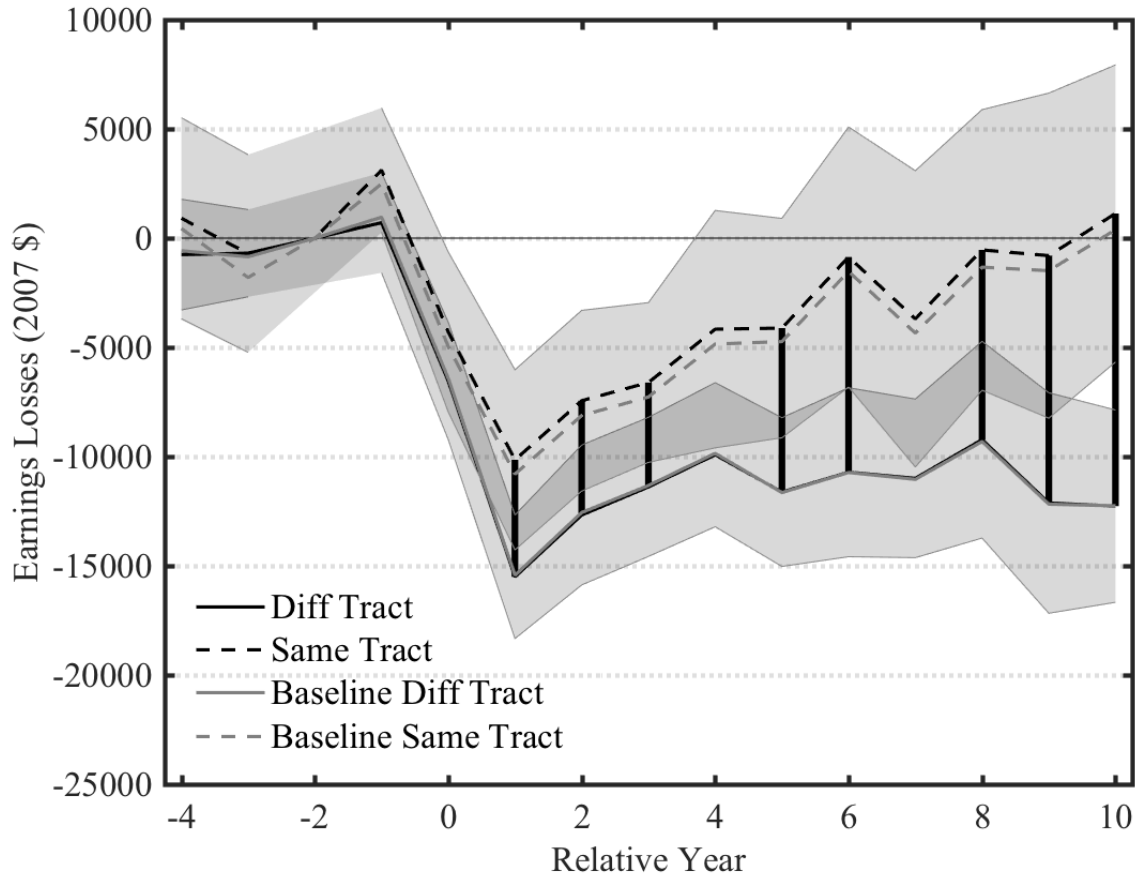
Appendix Figure 4: Probability of Switching Commuting Zones Around Displacement

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 coefficients 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.



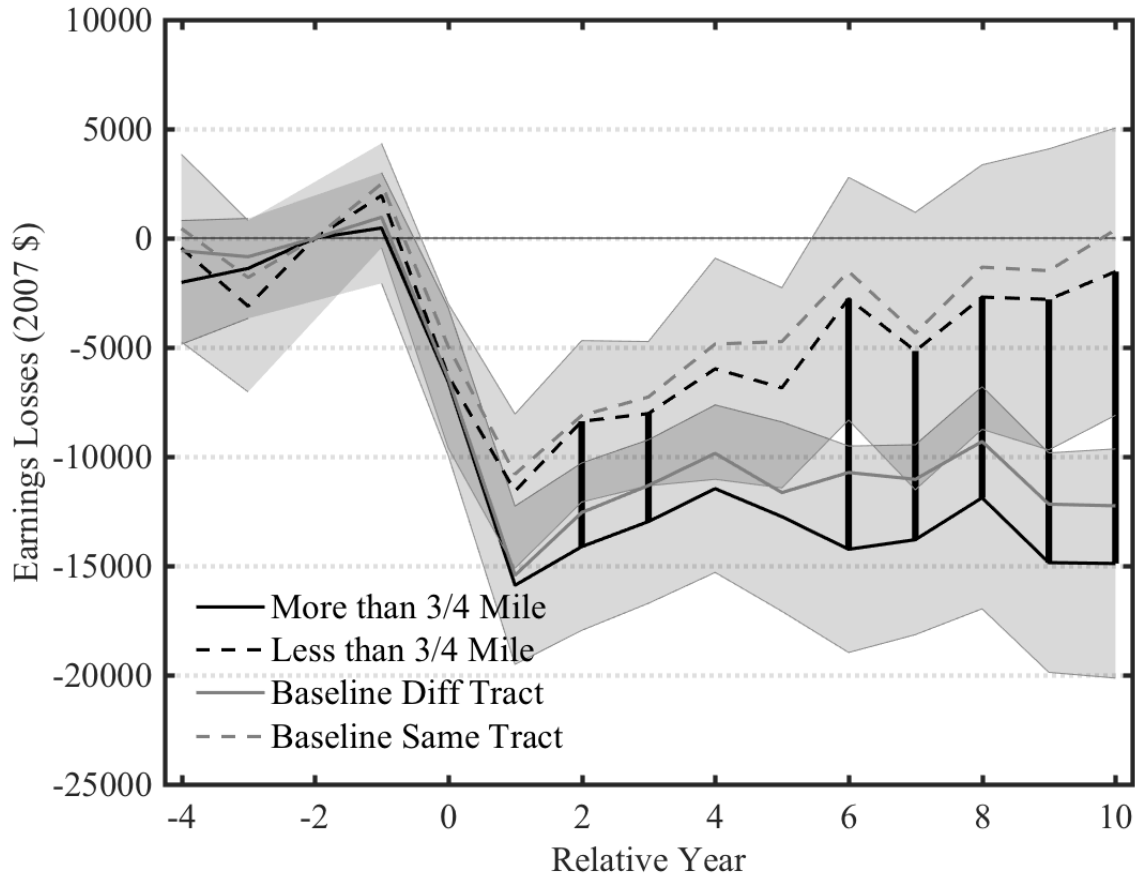
Appendix Figure 5: Earnings Losses for Young Displaced Workers (No PSID Weights)

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 the 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.



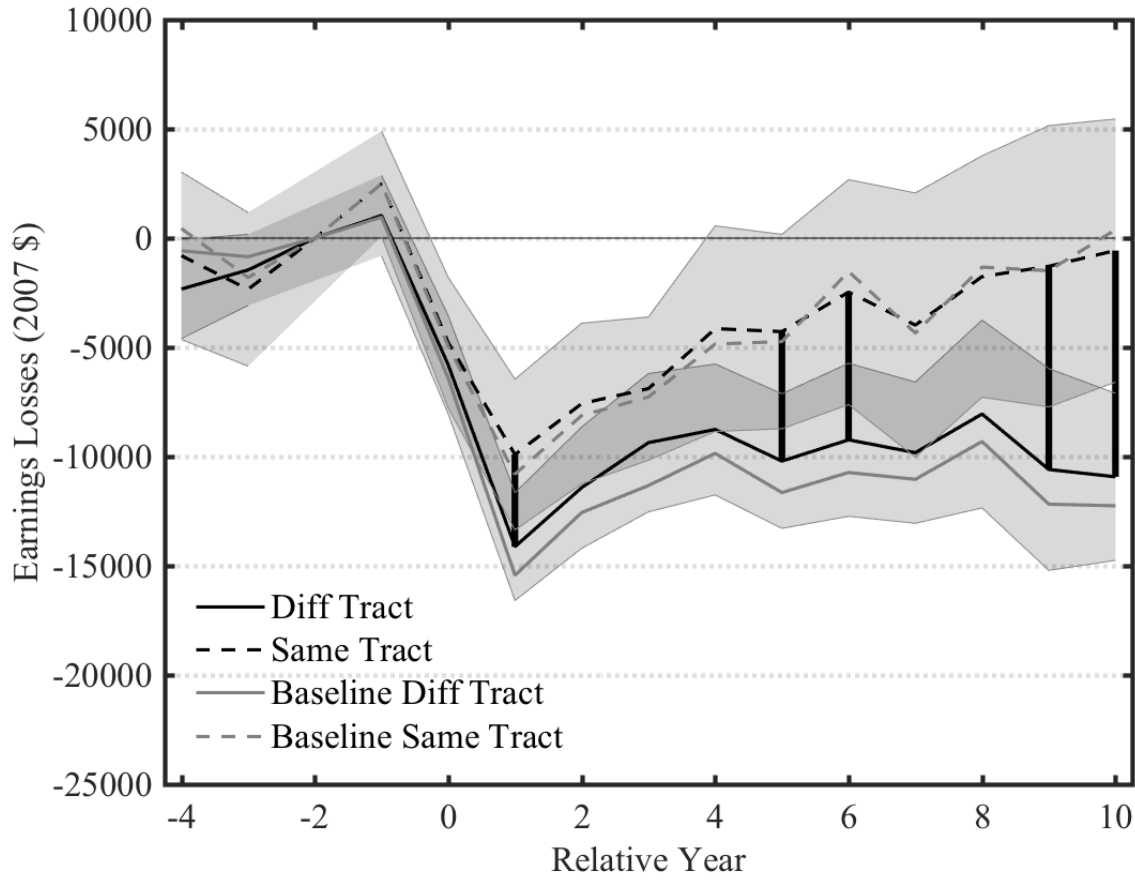
Appendix Figure 6: Earnings Losses for Young Displaced Workers (Controlling for Local Labor Market Conditions)

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 the 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.



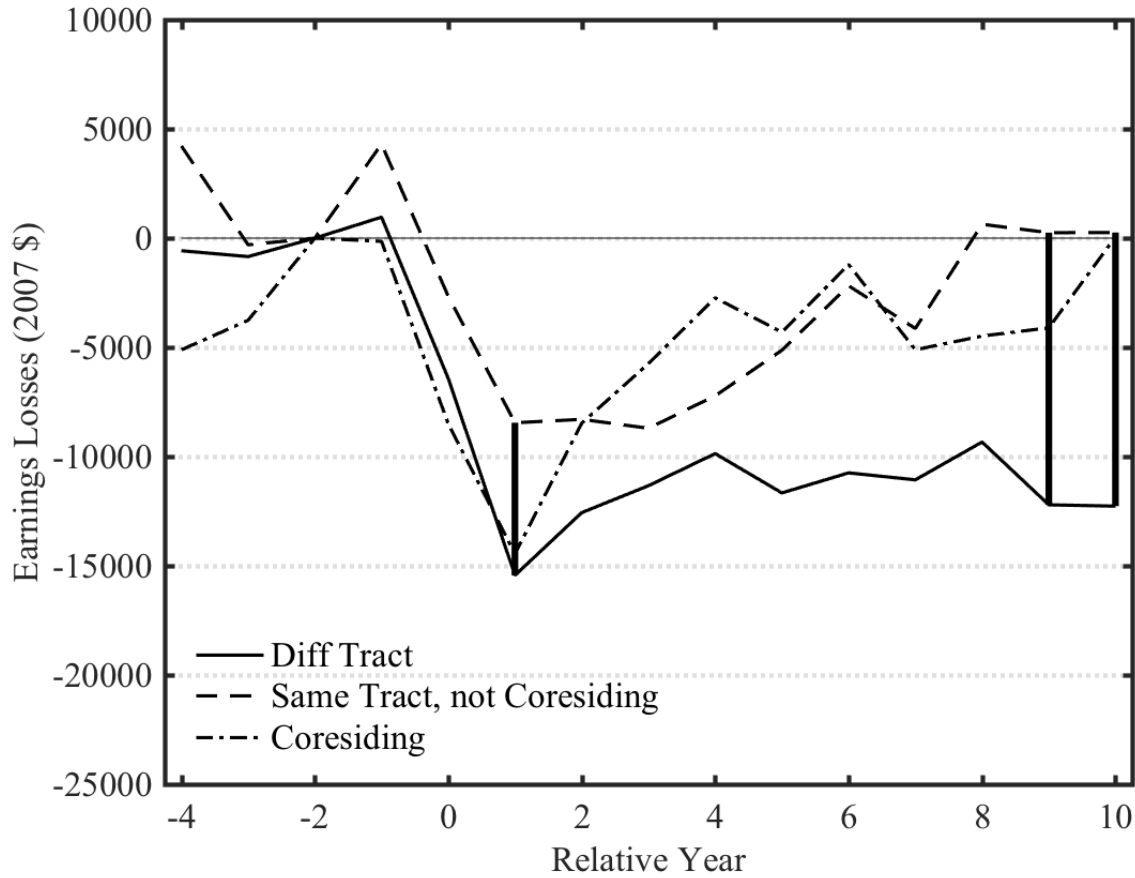
Appendix Figure 7: Earnings Losses for Young Displaced Workers (Using Distance Measures)

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 the 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.



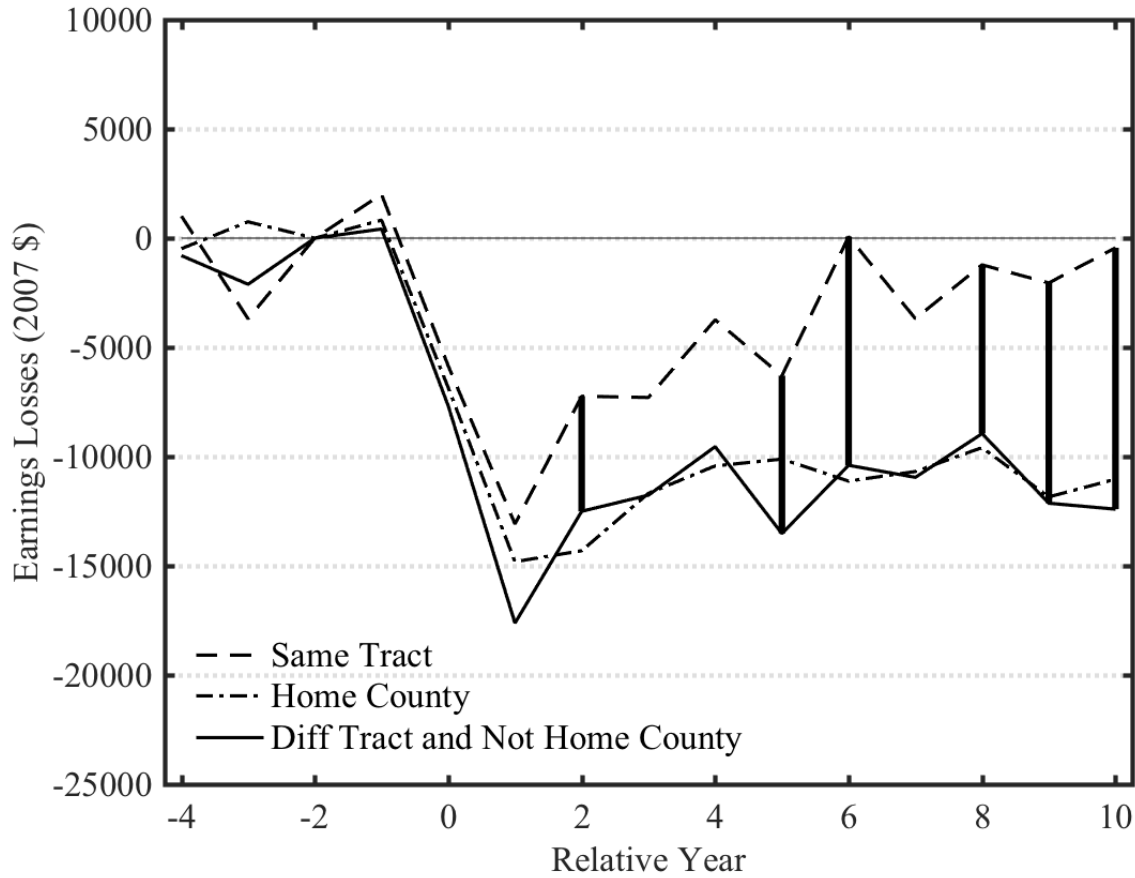
Appendix Figure 8: Earnings Losses for Young Displaced Workers (Heads and Wives)

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 the 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.



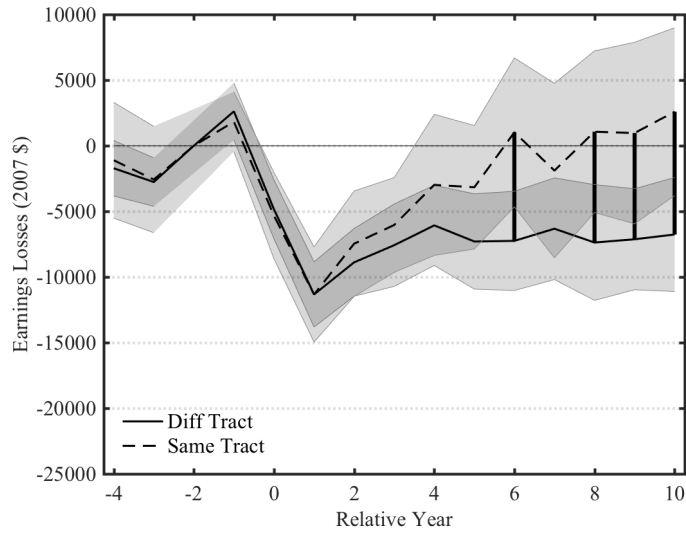
Appendix Figure 9: Earnings Losses for Young Displaced Workers (Same Tract vs. Coresiding)

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.

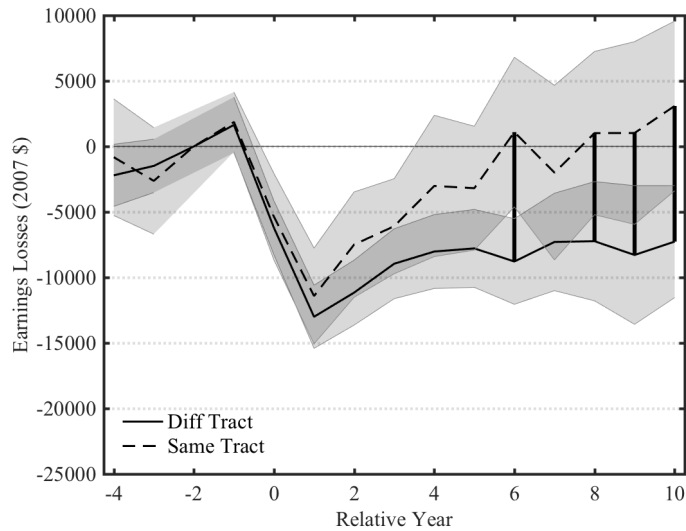


Appendix Figure 10: Earnings Losses for Young Displaced Workers with Home County Interactions

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.



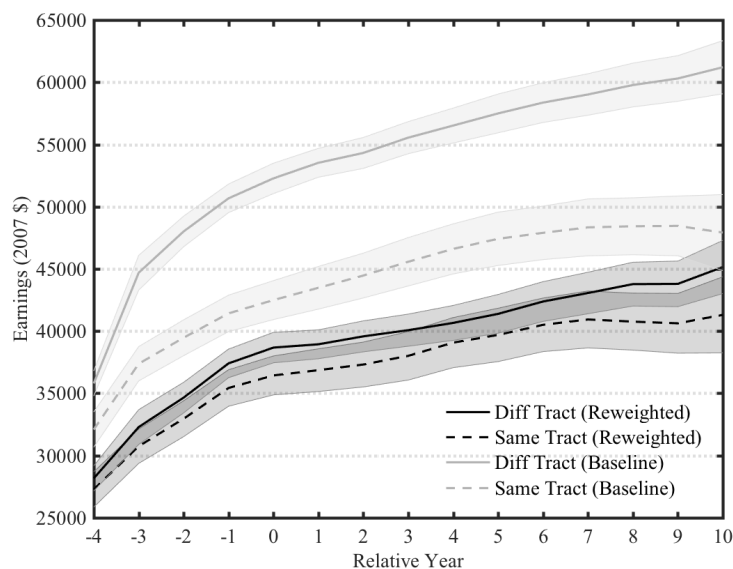
(a) Including Parents' Characteristics



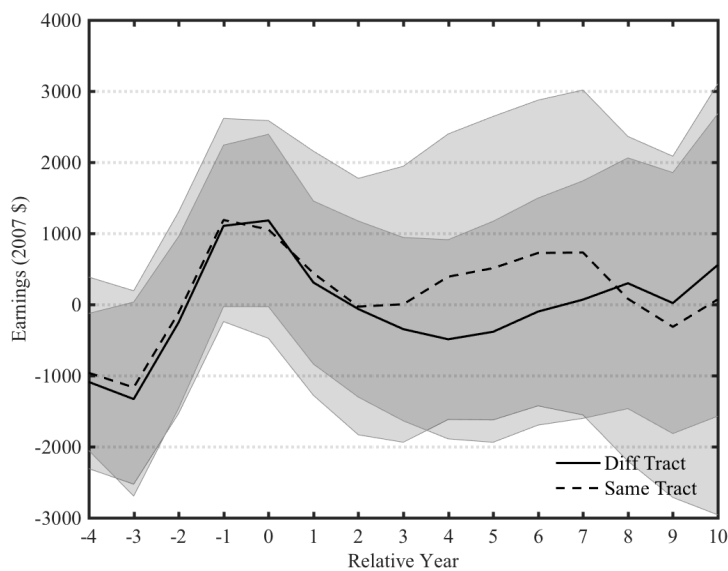
(b) Only Including Predetermined Characteristics

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 D for more details on these two types of weights, see Figure 10 for the original specification, and see Section 4 for more information on the reweighting scheme.



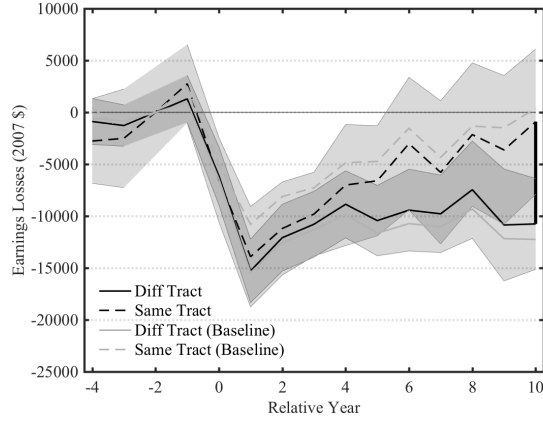
(a) Reweighted Means Without Age Trend



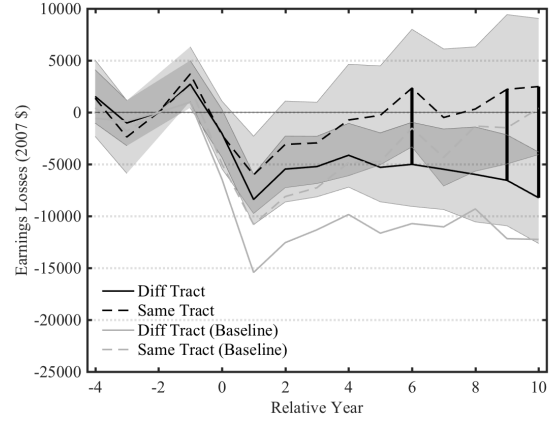
(b) Reweighted Means Including Age Trend

Appendix Figure 12: Mean Earnings For the Reweighted Control Samples

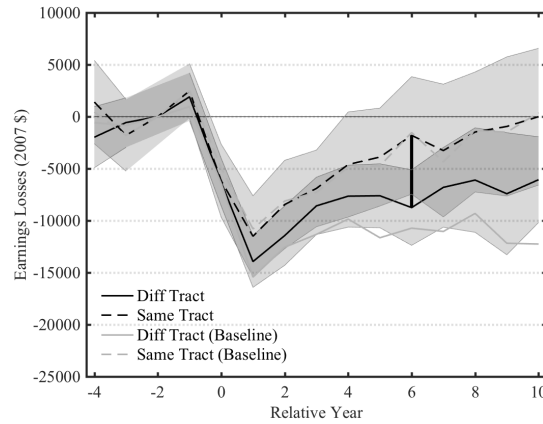
Note: The propensity score reweighting appears to be successful in terms of re-weighting each control group so that they are mostly similar to each other. Panel A shows that after applying the propensity score weights, but without adjusting for age quartics, non-displaced workers living close to their 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



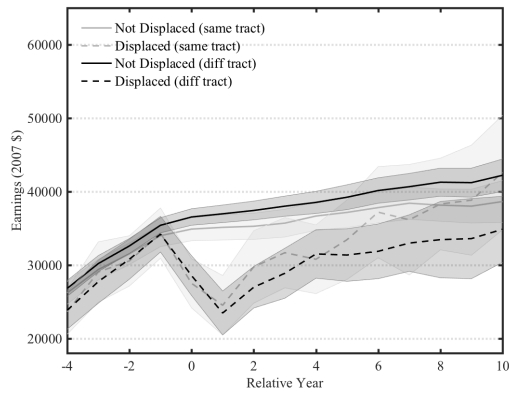
(b) Baseline Income as a Dummy



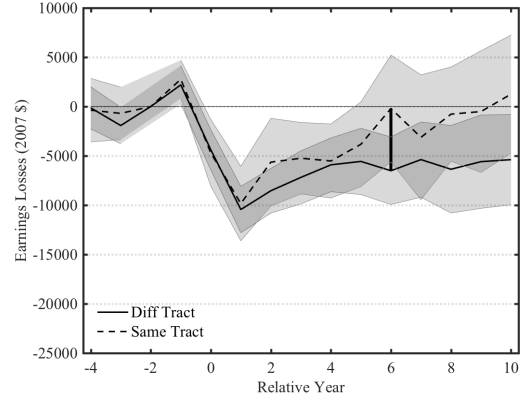
(c) College Education as a Dummy

Appendix Figure 13: Including Additional Interactions in the Baseline Specification

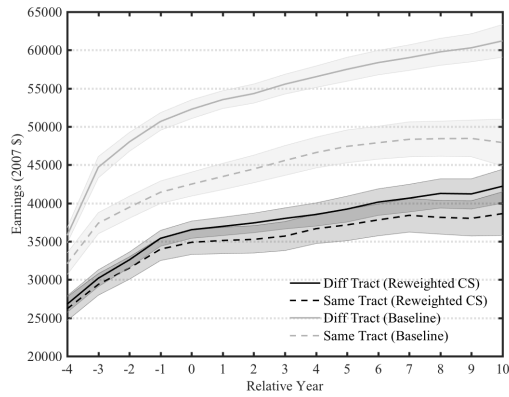
Note: Additional interactions with the displacement dummies (equation 4) do not change the effect of parental proximity on the post-displacement earnings outcomes. Although interacting with earnings prior to job loss generally makes the initial earnings losses similar for the two groups, the two paths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



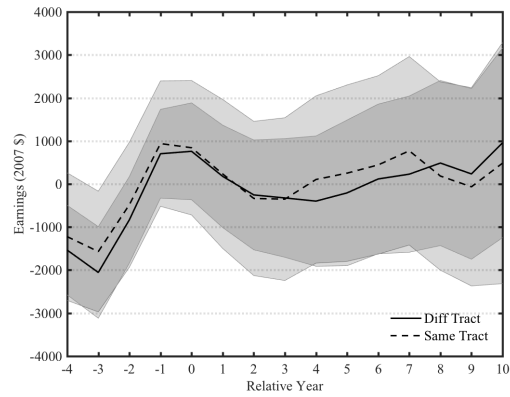
(a) Reweighted Means



(b) Reweighted Regressions



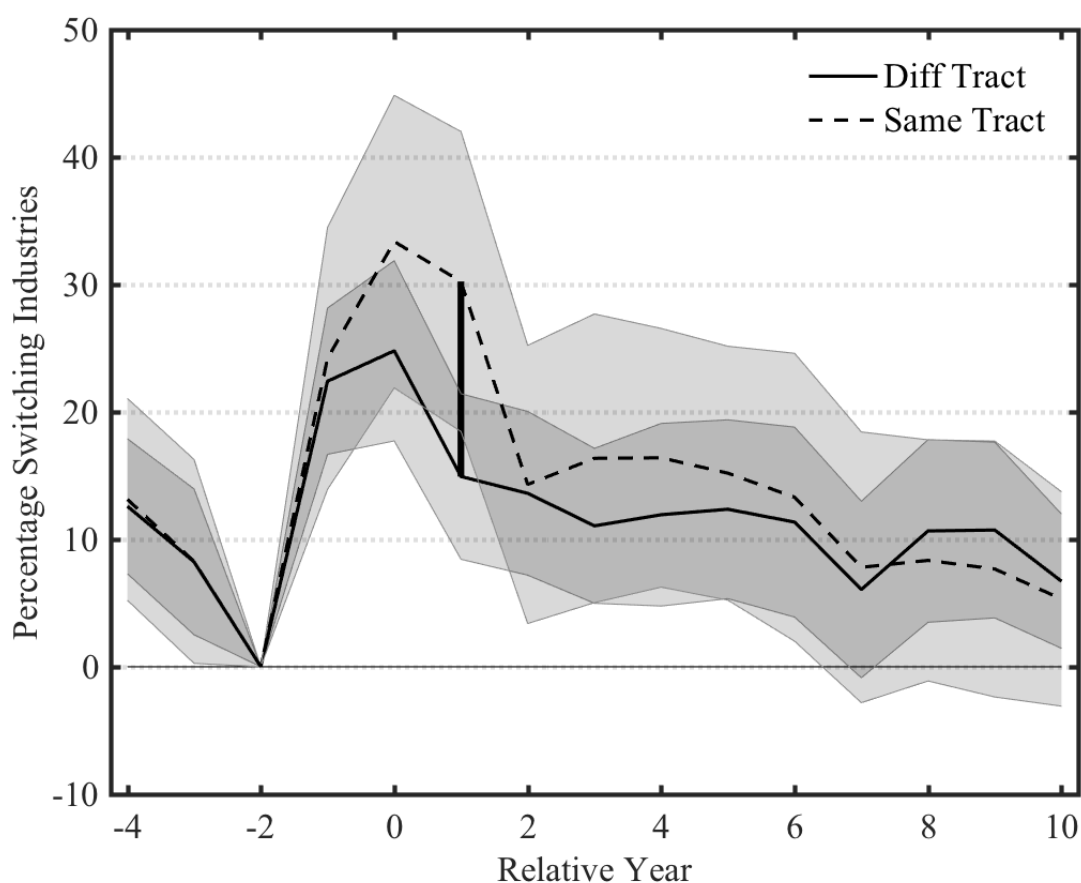
(c) Reweighted Means Without Age Trend



(d) Reweighted Means Including Age Trend

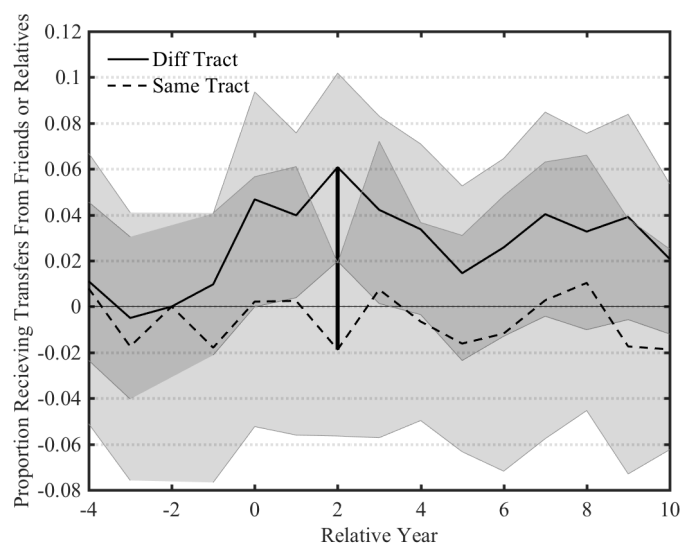
Appendix Figure 14: Reweighting on the Subsample with Common Support

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 Figures 9, 10, and 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 re-weighting groups. Panel A shows propensity score reweighted sample means around a potential displacement, Panel B shows propensity score weighted regression coefficients 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. Figures 9, 10, and 12 contain more details on the methodology.

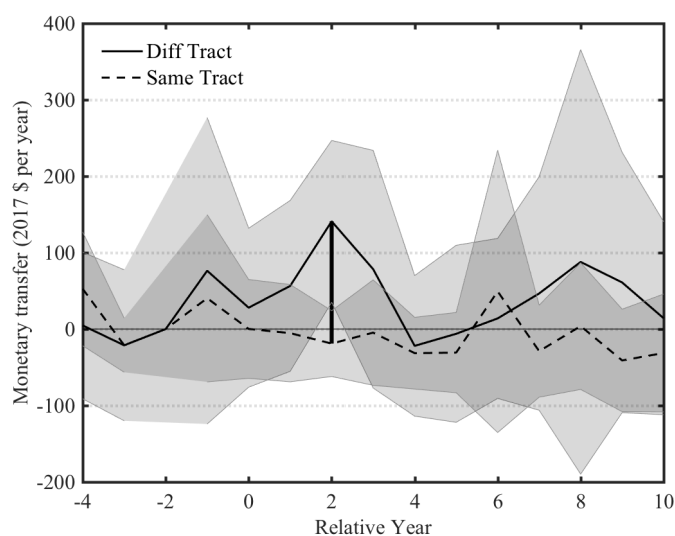


Appendix Figure 15: Probability of Switching Industries

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 the 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.



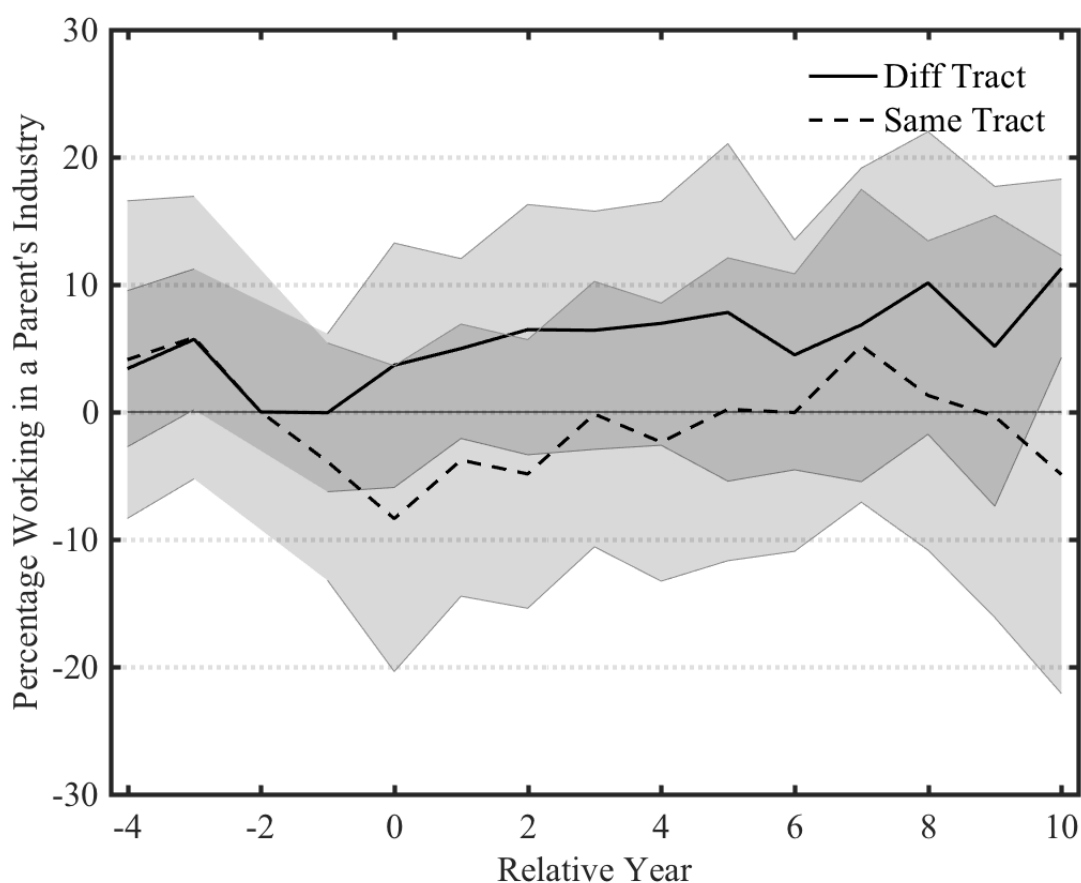
(a) Receiving a Monetary Transfer



(b) Total Monetary Transfers

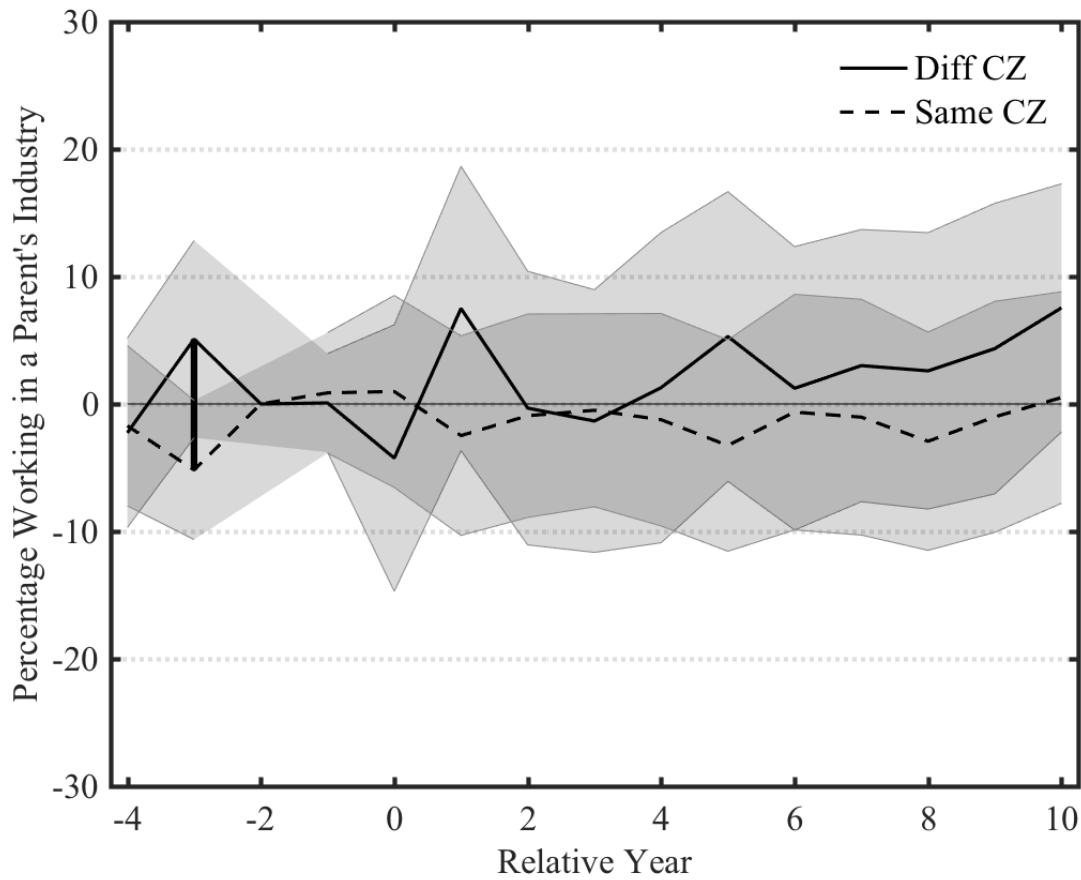
Appendix Figure 16: Monetary Transfers Around Displacements

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 the 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 11 and Appendix F for more details on the specification and the data.



Appendix Figure 17: 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 18: 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 [1.00]	\$36,500 [0.03]	\$38,200 [0.00]	\$41,900 [0.00]	\$31,600 [1.00]	\$31,700 [0.36]	\$31,000 [0.43]	\$32,800 [0.93]
Average Change in Earnings	\$2,100 [1.00]	\$2,100 [0.36]	\$3,000 [0.97]	\$3,000 [0.39]	\$2,100 [1.00]	\$2,000 [0.25]	\$3,400 [0.64]	\$2,400 [0.58]
Years of Schooling	11.96 [1.00]	12.45 [0.00]	12.29 [0.00]	12.72 [0.00]	11.96 [1.00]	12.12 [0.84]	12.01 [0.61]	12.15 [0.93]
Share in Goods Industries	0.55 [1.00]	0.46 [0.06]	0.59 [0.81]	0.39 [0.00]	0.55 [1.00]	0.50 [0.33]	0.54 [0.86]	0.42 [0.02]
Share Manager/Professional	0.13 [1.00]	0.18 [0.11]	0.15 [0.03]	0.21 [0.00]	0.13 [1.00]	0.16 [0.92]	0.11 [0.54]	0.15 [0.91]
Employer Tenure	5.26 [1.00]	6.54 [0.00]	5.13 [0.65]	6.45 [0.00]	5.26 [1.00]	5.29 [0.81]	5.17 [0.76]	5.30 [0.98]
Unemp Rate in County	7.66 [1.00]	7.31 [0.34]	7.87 [0.98]	7.19 [0.04]	7.66 [1.00]	7.50 [0.89]	7.94 [0.45]	7.62 [0.96]
Age	27.97 [1.00]	28.57 [0.00]	28.30 [0.02]	28.53 [0.00]	27.97 [1.00]	27.70 [0.34]	27.60 [0.32]	27.76 [0.43]
Number of Children	1.37 [1.00]	1.25 [0.72]	1.23 [0.32]	1.22 [0.26]	1.37 [1.00]	1.18 [0.42]	1.27 [0.83]	1.22 [0.56]
Fraction Male	0.82 [1.00]	0.80 [0.82]	0.83 [0.34]	0.85 [0.39]	0.82 [1.00]	0.81 [0.89]	0.75 [0.36]	0.83 [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.