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LONG-TERM AND HETEROGENEOUS EFFECTS OF JOB-SEARCH ASSISTANCE

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ABSTRACT

This paper examines long-term and heterogenous treatment effects of an experimental job-search assistance program that operated in Nevada from July through December 2009. Over the eight-year follow-up period, the program increased participants' employment and earnings, reduced short-term UI receipt, reduced loss of homeownership, and increased Federal tax filing and tax receipts. Related to heterogeneity, roughly 40 percent of the sample is estimated to have negative treatment effects on long-term employment which are corroborated by accompanying positive treatment effects on entry into disability insurance. Positive impacts on homeownership appear concentrated among individuals with positive employment treatment effects.

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I. Introduction

Unemployed workers who collect Unemployment Insurance (UI) benefits are typically required to actively search for work while collecting benefits. Over recent decades, policymakers in the United States and other developed countries have established job-search assistance programs to help UI beneficiaries connect to suitable jobs and undergo eligibility reviews to ensure that they are compliant with UI work search requirements (Meyer, 1995; Wandner, 2010; Kahn, 2010; Card *et al.*, 2015; Michaelides and Mueser, 2021). While prior studies have provided experimental evidence on average short-term effects of these interventions (Meyer, 1995; Decker *et al.,* 2000; Klepinger *et al.,* 2002; Black *et al.,* 2003, Michaelides and Mueser, 2018, 2021; Michaelides *et al.,* 2021), identifying the long-term and potentially heterogeneous impacts of these programs is important for understanding how individuals search for jobs and how job-search assistance programs may affect their job-search behavior and individual well-being.

In this paper, we examine the long-term and heterogeneous treatment effects of an experimental job-search assistance program that operated in the state of Nevada from July through December of 2009. The Nevada program required a random sample of UI beneficiaries to attend a one-on-one meeting with program staff at the early stages of their UI spells to undergo an eligibility review, in which they provided information about their job-search activities. These individuals were then offered job counseling during the same meeting, including a skills assessment, development of a work-search plan, resume development assistance, and direct referrals to jobs. To enforce requirements, the Nevada UI agency disqualified from collecting additional UI payments those who did not show up for the required meeting and those identified during the meeting as not conducting an active job search.

The empirical analysis uses program data on the universe of unemployed workers who started collecting UI in Nevada from July through December 2009 (near the depth of the Great Recession), and were eligible for random assignment for participation in the program. These data are merged with administrative tax data from 2001 through 2017, allowing us to measure a wide range of outcomes for treatment and control individuals for up to eight years after program assignment. Third-party reported tax documents are used to measure individual salary employment and income, contractor income, and receipt of UI and social security disability benefits. Third-party reported mortgage interest statements are used to measure homeownership, while filed tax returns are used to measure spousal employment and tax-related outcomes, such as federal income tax return filing, tax payments, and after-tax income. In addition to estimating effects on these outcomes, we use machine learning techniques, specifically causal forests, to assess heterogeneous treatment effects to better understand underlying program mechanisms.

The results indicate positive long-term average treatment effects on wage employment and earnings. The analysis confirms relatively large short-term increases in employment for the treatment group relative to the control group in 2010, the first year after program assignment (Michaelides and Mueser, 2018; 2021). Average employment treatment effects gradually declined over time but remained positive for the entire eight-year follow-up period, indicating persistent employment effects. Consistent with these results, the program led to positive effects on salary earnings through the entire eight-year follow-up. The program also had positive average treatment effects on secondary outcomes for participants and their families. The program reduced receipt of UI benefits in the first post-program year but did not affect receipt of UI in later periods or receipt of disability benefits. Thus, on average, short-term decreases in UI receipt were not offset by longer-term increases in UI receipt or disability insurance receipt among qualified individuals. Further analysis shows that treated individuals were less likely to move out of homeownership, more likely to file a tax return, and paid higher taxes than control individuals.

Using causal forests (Wager and Athey 2018), analyses of heterogeneous treatment effects provide evidence that, while average treatment effects on employment are positive in both the short-term and long-term, some individuals experienced large positive employment treatment effects while others experienced negative effects. More specifically, the largest positive employment effects are among individuals with low employment rates and the largest negative employment effects are among individuals with high employment rates. There is also evidence of heterogenous treatment effects on disability benefit receipt that corroborate the evidence of heterogeneous treatment effects on employment. Disability treatment effects are negatively correlated with employment treatment effects in the long-term, so individuals who experienced small employment declines due to the treatment are also predicted to increase disability receipt. The heterogeneous employment treatment effects are also corroborated by heterogeneous treatment effects on homeownership as the results indicate that positive treatment effects are generally concentrated among individuals with positive employment treatment effects. These results for long-term and heterogenous treatment effects are consistent with a theory of change in which the experimental job search assistance may have reduced fixed costs for job search and may have switched subjective beliefs. Reducing fixed costs would cause some individuals who may not have searched in the absence of the treatment to search, and switching subjective beliefs may cause individuals to reallocate search efforts so that some people who would have used ineffective strategies would use effective strategies, and others who would have used effective strategies would use ineffective strategies.

The current study contributes to the existing literature in multiple ways. First, while prior studies have focused on shorter-term impacts (Meyer, 1995; Decker *et al.,* 2000; Klepinger *et al.,* 2002; Black *et al.,* 2003, Michaelides and Mueser, 2018, 2021; Michaelides *et al.,* 2021), this study provides evidence on the longer-term impacts of job-search assistance on participants' labor market outcomes. Conceptually, job counseling may help participants to obtain high-quality jobs early on and the lessons learned may carry over to subsequent periods, thereby producing persistent effects on employment and earnings. Alternatively, effects may be transitory if the only program impact is to help participants find jobs faster but effects dissipate as control cases begin to obtain jobs in subsequent periods. Another consideration is that participants may feel pressure to find jobs quickly, pushing them to make poor job choices. In this scenario, short-term effects may dissipate (or even be reversed) in subsequent periods, if participants lose their jobs and have trouble obtaining new ones while control cases achieve good job matches after the initial period.

Second, the current study considers outcomes beyond UI durations and employment that provide corroborating evidence for employment impacts and are also policy-relevant themselves. In particular, we consider whether positive employment impacts may improve participants' ability to pay their mortgages and keep their homes or even become homeowners following their unemployment episode. Moreover, positive employment impacts may have implications for tax policy, as participants may be more likely to participate in tax-based benefits, like the Earned Income Tax Credit, and pay more in federal income and payroll taxes.

Lastly, the analysis adds to a growing literature on applying machine learning techniques to analyze treatment effects from randomized controlled trials. By applying these techniques in the current setting, this study considers whether job-search assistance may cause heterogeneous treatment effects on participants' short-term and long-term employment. This analysis of heterogeneous treatment effects provides insights into possible mechanisms through which employment treatment effects could affect receipt of disability benefits and homeownership.

The remainder of this paper is organized as follows. Section II discusses the institutional background of the Nevada program and the data used in the analyses. Section III presents methods and results for analyzing long-term average treatment effects on wage employment and earnings and other outcomes. Section IV presents methods and results for analyzing heterogeneous treatment effects and a conceptual framework to organize thoughts around the results. Section V summarizes and concludes.

II. Background

A. The Nevada Program

Job-search assistance policy in the United States was initiated with the establishment of the Worker Profiling and Reemployment Services (WPRS) program in 1993. Under WPRS, state UI agencies used statistical profiling to identify services-eligible UI beneficiaries[1] who were likely to exhaust their UI entitlements and refer them to public employment offices to receive services (Dickinson *et al.,* 1999). The program's objective was to help participants who had employability issues to find jobs quickly and produce savings for the UI program. WPRS was federally mandated and became operational in all 50 states in 1996, serving about one fifth of eligible beneficiaries (Wandner, 2010). By the early 2000s, policymakers were concerned that the automation of UI systems impeded the ability of state UI agencies to monitor if beneficiaries were actively searching for work as mandated by state UI laws. As a response to these concerns, the U.S. Department of Labor (DOL) established the Reemployment and Eligibility Assessment (REA) program. REA's objective was to reduce UI fraud by requiring beneficiaries who were not served by WPRS to undergo an in-person eligibility review; participants who could not provide evidence of an active

[1] All beneficiaries were considered services-eligible, except those on temporary layoff, those searching for work through union hiring halls, and those active in training programs.

job search during the review were disqualified from collecting additional UI payments (Poe-Yamagara *et al.,* 2012). REA was voluntary so DOL offered annual grants to encourage states to adopt the program. By 2010, REA operated in 33 states (including Nevada), serving about 15 percent of services-eligible beneficiaries; the program expanded to all 50 states by 2015.

Nevada used a different approach to implement REA than the approach recommended by DOL and used in other states. Instead of conducting eligibility reviews of beneficiaries not served by WPRS, Nevada created a new program that required participants to undergo an eligibility review meeting in the first few weeks of their UI spell and, if deemed eligible, to receive job-counseling services during the same meeting. The "enhanced" Nevada REA (hereafter, the Nevada program) operated in public employment offices covering the Las Vegas and Reno metropolitan areas, while WPRS operated in the rest of the state.[2] The program operated as follows. Each week, the Nevada UI agency randomly assigned about 15 percent of services-eligible UI beneficiaries to the program group and the remaining 85 percent to the control group. A letter was sent out to program cases asking them to attend a meeting, typically scheduled in weeks 2-4 of their UI claim, to undergo the review and receive job counseling. The letter informed participants that failure to show up for the meeting would result in loss of benefits. Individuals assigned to the control did not receive such a letter or other program communications, nor were they subject to any program requirements; however, they were subject to the usual UI work search requirements.[3]

Individuals in the treatment group who were not present for the meeting without a reasonable justification were disqualified from collecting additional UI benefits.[4] Participants who attended

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[2] Tabulations of the 2009 American Community Survey show that the two metropolitan areas where REA operated covered about 87 percent of unemployed workers in the state.

[3] State law required UI recipients to actively searching for a job, be available for work, and not reject suitable job offers. UI beneficiaries were responsible for keeping track of their employer contacts in case the UI agency wanted to verify their job search activities.

[4] Participants who had discontinued benefit receipt, received job-search services on their own, or enrolled in training prior to the meeting were excused. Those disqualified could not collect UI benefits until the end of the benefit year

the meeting and were deemed incompliant with work-search requirements were also disqualified. Those who passed the review were offered job counseling during the same meeting, including an individual skills assessment, development of a job-search plan focusing on jobs that were compatible with the participant's skills, assistance in developing a resume and other job application materials, and direct referrals to employers with compatible job vacancies. Services were offered as needed so not all participants received all services. At the end of the meeting, participants were informed that they were not required to receive additional services or attend follow-up meetings, but were expected to continue actively searching for employment.

The Nevada case study is compelling because it was the only state program at the time that required participants to undergo an eligibility review *and* receive job counseling, unlike WPRS and REA programs operating in other states which did not refer nor did they require participants to receive counseling.[5] Our interest in the Nevada program is further motivated by the fact that, in 2015, DOL instructed states to replace their existing programs with interventions that included both the eligibility review and mandatory counseling (U.S. Department of Labor, 2015). To emphasize this policy shift, REA was renamed to Reemployment Services and Eligibility Assessment (RESEA), and in 2018, U.S. Congress appropriated more than $150 million to support implementation of RESEA in all 50 states (U.S. Department of Labor, 2019). As a result, all 50 states have modified their REA programs to include job counseling following the eligibility review and many have discontinued their WPRS programs. Thus, the Nevada program represents the types of interventions operating across the U.S. and evidence on the program's long-term effects can be

associated with their claim (which lasted 365 days from the day the claim was filed). Disqualified participants could submit a new UI claim at the end of the benefit year.

[5] Note that the requirements of the Nevada program are less intensive than those of interventions operating in most European countries, where participants are required to engage in monitoring and counseling activities on a regular basis throughout their unemployment spells (Kahn, 2012; Card *et al.,* 2015).

used to draw more general conclusions about current U.S. job-search assistance policy.

B. A Theory of Change with Long-Term and Heterogeneous Treatment Effects

We discuss a theory of change to illustrate how the experimental job-search assistance could affect individual job search decisions and cause the long-term and heterogeneous treatment effects described in the prior sections. We consider a setting in which an unemployed individual chooses multiple dimensions of effort that affect the probability of finding a job. We assume that there are fixed costs of conducting a job search. We also assume that the individual does not know how productive each dimension of search effort is, and instead must operate based on her own subjective beliefs about the productivity of each dimension of search effort.

In this environment, we consider two potential impacts of job-search assistance. First, we consider that the treatment could reduce fixed costs of job search, which could cause individuals who would not have conducted an active job search in the absence of the treatment to search and become employed. This channel differs from an experimental treatment that would not change the composition of jobseekers and would instead focus on getting jobseekers to find jobs faster than they would have in the absence of treatment.

Second, rather than interpreting the intervention in terms of uniformly increasing search productivity or improving monitoring of an unemployed individual's search activities, we interpret the intervention in terms of changing subjective beliefs. Specifically, job-search assistance may change an individual's subjective beliefs about how different dimensions of search effort influence the probability of getting a job. Intuitively, a case worker may not know how productive each dimension of search effort is and may instead have different subjective beliefs than an unemployed individual. The change in subjective beliefs could cause a reallocation of search efforts from some dimensions to others. If the change in subjective beliefs causes individuals to shift from less effective search strategies to more productive strategies, then the intervention would generate positive employment effects. If the change in beliefs causes other individuals to shift to less productive search strategies, then the intervention may generate negative employment effects.

For example, participants may begin their job search by considering a narrow set of jobs, emphasizing jobs that they feel are compatible with their past work experience and skills. If the social planner's objective is to realize short-term UI savings by connecting participants to jobs as quickly as possible, then the intervention may emphasize conducting a wider search that considers jobs that lie outside the range of options that the participants would consider on their own. This strategy shift may be beneficial for individuals who were previously employed in declining industries and have limited information about jobs in other sectors that may be compatible with their skills. However, this strategy shift may be less productive for individuals with in-demand skills, who are likely to achieve a quality match by focusing their search on their initial set of jobs. It is also possible that participants may feel pressure by the program to find a job quickly, causing some to accept low-quality jobs early in their UI spells instead of seeking a better match.

This conceptual framework highlights that search effort is multi-dimensional and that job-search assistance does not simply increase or decrease search effort but may also reallocate effort into more (or less) effective search strategies. This framework assumes that all individuals experience the same treatment and respond differently to that treatment, rather than assuming that individuals experience different treatments. As a result, while some individuals may experience positive employment impacts because the program caused them to switch to more effective strategies, others may respond to the program by adopting ineffective strategies that are less likely to lead to any employment or may lead to lower quality job matches. Individuals who switch to more effective strategies may experience positive short- and long-term employment effects, while those who switch to ineffective strategies may have initially positive employment effects that decline and become negative as their lower quality job matches dissolve.

C. Data

Our study sample consists of the universe of individuals who started collecting UI from July through December 2009 and were subject to random assignment for participation in the program.[6] During this period, the U.S. economy was near the depth of the Great Recession and the Nevada unemployment rate averaged over 12 percent compared with just below 10 percent nationally. UI beneficiaries were entitled to collect up to 99 weeks of benefits.[7] The total sample size is 32,751 individuals, from which 4,673 were assigned to the treatment and the remaining to the control group. While we are not able to observe program compliance in the data used for the current analysis, prior analysis of program records indicates that the program achieved high compliance – eight out of ten participants completed program requirements and received counseling.[8]

Our analyses use two data sources: (1) Nevada program data, provided by the Nevada Department of Employment, Training, and Rehabilitation; and (2) administrative tax data from 2001 through 2017. Our empirical strategy is based on identifying treatment and control individuals from the Nevada data in the administrative tax data using exact date of birth, gender,

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[6] This is the same sample used by Michaelides and Mueser (2018) to examine the program's short-term effects.
[7] UI beneficiaries in Nevada were entitled to collect up to 26 weeks of regular UI benefits. Those who exhausted their regular entitlement could also be entitled to collect up to 53 weeks under the Emergency Unemployment Compensation program and up to 20 weeks under the Extended Benefits program. See Michaelides and Mueser (2021) for a more detailed discussion of UI eligibility rules during the recession.
[8] Michaelides and Mueser (2018) report that 3,717 of the 4,673 program cases (79.5 percent) showed up for the mandatory meeting; 34 were disqualified because of eligibility issues identified during the review and 3,683 passed the review and received counseling. Of the 956 treatment cases who did not show up for the review, 904 were exempted because they had received services on their own, had enrolled in training, or had exited UI prior to the scheduled meeting; the remaining 52 cases were disqualified for failure to meet requirements.

zip code, and annual earnings in 2009. The matching procedure is as follows. First, we use administrative tax data to identify all individuals who received UI benefits (i.e., had a 1099G tax form) from Nevada in 2009. Then, we match individuals from the Nevada sample to individuals in the administrative tax data pool based on exact date of birth, gender, zip code, and annual earnings. Using this process, we matched 4,672 of the 4,673 treatment cases and 27,107 of the 27,153 control cases from the original Nevada sample to unique individuals in the administrative tax data.[9]

Lastly, we create a panel dataset based on administrative tax records for years 2001 through 2017. The administrative tax records include information reported by third parties, such as W-2 and 1099-MISC tax forms,[10] and information from filed tax returns (Form 1040). Using third-party-reported information, we measure salary employment (whether an individual had a W-2 form) and earnings amounts (as reported on W-2 forms). We also measure whether individuals had contractor employment (based on 1099-MISC forms), whether they collected UI benefits (based on 1099-G forms), and whether they collected social security disability benefits (based on 1099-SSA forms). For each of these income sources, we also observe amounts earned.

Additionally, homeownership is measured based on third-party reported mortgage interest statements. Specifically, for each individual who owns a home and has mortgage payments to a lender, a 1098 Mortgage Interest statement is reported by the lender to the IRS for any mortgage interest payment in excess of $600. Using filed tax returns, we measure additional outcomes, including spousal employment, net tax payment, and after-tax income.

[9] The remaining one treatment case and 46 control cases were not matched in the administrative tax data because the Nevada program data did not include sufficient information to perform the matching.

[10] IRS Forms W-2 and 1099-MISC are tax forms that employers are required to submit to individuals and the IRS. The W-2 document records annual wage, salary and tip income paid by the employer to the employee. The 1099-MISC document records annual payments in excess of $600 to non-employees such as independent contractors. IRS Forms 1099-G and 1099-SSA record UI and social security payments paid from government agencies to individuals; these forms are reported to the IRS by state agencies and the Social Security Administration, respectively.

D. Summary Statistics and Weighting

To assess whether there is treatment-control group equivalence in the matched sample, Table 1 presents summary statistics for the treatment and control groups and p-values for tests of differences between the two groups. These summary statistics are based on individual characteristics from administrative tax data and from program data in 2008, the year before the experiment. As seen in the left panel (unweighted sample), summary statistics and associated p-values indicate statistically significant differences between the treatment and control groups in the age distribution, tax filing status, and having UI and social security benefits in 2008. Comparisons show treatment-control balance in the remaining characteristics from the administrative tax data and in education and disability status from the program data.

The observed imbalances raise concerns that estimated treatment effects based on unadjusted treatment-control differences in outcomes could be biased. To adjust for the covariate imbalance, we use inverse probability weighting. We first calculate propensity scores as the fraction treated within cells based on the following discrete characteristics: gender, age group, three-digit ZIP code, industry (two-digit NAICS) code, filing status, education group, and having UI benefits in 2008. Using $c(i)$ to refer to the cell that individual i belongs to, based on these characteristics, we denote this propensity score by $p_i = p(c(i))$. Next, the weights are computed as follows:

$$w_i = \begin{cases} 1/p_i \text{ if } T_i = 1 \\ 1/(1-p_i) \text{ if } T_i = 0 \end{cases}$$

where w_i denotes the weight for individual i and T_i is an indicator equal to 1 if individual i is in the treatment group and 0 otherwise. For individuals in cells where there are only treatment or control individuals (i.e., individuals with p_i = 1 or 0 respectively), the weights are set to zero since there is no common support between the treatment and control observations in these cells. Results indicate that 48 percent of individuals in the control group and 26 percent of individuals in the treatment group with propensity scores of 0 and 1, respectively. These observations lack common support so their weights are set to zero. Lastly, the weights are scaled so the sums match the original treatment and control group sample sizes.[11]

The right panel of Table 1 presents summary statistics for the weighted treatment and control groups. These summary statistics indicate that the weighting adjusts for the observed covariate imbalance as expected. Since inverse probability weights are calculated so that the treatment and control groups have matching distributions for the characteristics used to compute the weights, the p-values for differences in gender, age groups, filing status, and education groups are all 1. Differences in the other covariates do not appear statistically significant so that, overall, covariates appear balanced between the weighted treatment and control groups.

III. Average Treatment Effects

A. Regression Specification

We estimate the causal effects of the program on participants' outcomes using a standard Ordinary Least Squares (OLS) regression specification. Using y_{it} to denote the outcome of interest for individual *i* in year *t*, the regression specification is as follows:

$$y_{it} = \alpha_0 + \sum_k \alpha_k 1(year = k) + \gamma Treat_i + \sum_k \beta_k Treat_i * 1(year = k) + Week_{it}\delta + \varepsilon_{it}$$

We report estimated coefficients, $\gamma + \beta_k$, which capture differences between the treatment and control group in each year, from 2001 through 2017. Parameters for the eight years after program assignment (2010–2017) estimate the program's average intent-to-treat effects. The same

[11] Appendix Figure A presents the distribution of propensity scores for propensity scores for observations with common support, indicating that there are treatment and control observations across the range of propensity scores between 0 and 1.

specification allows us to examine if there were pre-existing treatment-control differences in outcomes, providing a check to assess the validity of random assignment. All regressions include fixed effects for week of random assignment in the vector $Week_{it}$, to account for differences in the proportions of eligible UI recipients assigned to the treatment across weeks and standard errors are clustered on individual.[12] Throughout the paper, all amounts are expressed in 2015 CPI adjusted dollars.

B. Effects on W-2 Employment and Earnings

We begin by estimating program effects on salary employment and earnings. Using third-party W-2 tax forms from 2001 through 2017, we define: *W-2 employment*, which indicates whether the individual had a W-2 form with earnings from an employer, and *W-2 earnings*, which measures the total amount of W-2 earnings from all employers.[13] Figure 1 presents results for the unweighted and weighted samples. To demonstrate the impacts of the common support sample restriction and the use of inverse probability weighting, plots A and B present results using the full unweighted sample and plots E and F present results using the weighting with the common support restriction. Each plot presents treatment effects and 95 percent confidence intervals (corresponding to the left vertical axis) and outcome means for the treatment and control groups (corresponding to the right vertical axis), facilitating a comparison of effects to the relevant outcome means. Plots C and D present outcomes with and without the common support restriction.

[12] We also estimate specifications with additional controls (measured in 2008), where the vector of covariates also includes dummy variables for three-digit ZIP code, two-digit industry (NAICS) code, age bins (< 26, 26-34, 35-44, 45-54, 55-64, > 64), gender, marital status, presence of children, and homeownership. Results are similar between specifications with no additional covariates and specifications with additional covariates, so we present only results based on specifications that control only for week of random assignment.

[13] Individuals with no W-2 form are included with values of zero for both employment and earnings. For 2009, we measure W-2 employment as having more than one W-2 since all participants had at least one job in 2009. Separate analyses (Appendix Figure C1) presents effects of the likelihood of having multiple W-2s across all years.

Results for the full unweighted sample indicate that the program increased W-2 employment and earnings in 2010, the first year after program assignment, and these increases appear persistent in subsequent years. However, the same results indicate some statistically significant treatment-control differences in the years prior to program assignment, which may be partly due to differences in characteristics. Figures 1C and 1D show that restricting the sample to observations with common support in observables in 2008 largely eliminates differences in W-2 outcomes prior to program assignment. This indicates that observations without common support in observables account for much of the pre-program treatment-control differences.

Figures 1E and 1F demonstrate that, after applying inverse probability weighting, treatment-control differences prior to the treatment are not statistically significant. In 2010, the first full year after program assignment, the program increased W-2 employment by 4.2 percentage points as compared with a base employment rate of roughly 70 percent for the control group. Program effects on W-2 employment declined to 3.2 percentage points in 2011 and to 2.8 in 2014 but remained statistically significant. Effects declined further in 2015 and lacked statistical significance. The graphical evidence indicates that this fade-out was driven by a relatively steeper decline in the employment rate for the treatment group after 2014, rather than some control group individuals, who did not find employment early on, finding W-2 employment in subsequent years.[14] Separate analyses (see Appendix Figure C1) indicate that the positive salary employment impacts during the study period were not offset by control cases resorting to contractor

[14] Recent work has focused on whether labor market policies cause displacement effects, occurring because programs may help participants find jobs by displacing individuals who are not served by the program but were also looking for jobs during the same period. Much of the related work concerns job-search assistance programs in Europe, with mixed results – some studies provide direct evidence that displacement may be important (Crepon *et al.*, 2013; Feracci *et al.*, 2010; Gautier *et al.,* 2018), while other studies suggest that the role of displacement is minor (Toohey, 2015; Lise *et al.*, 2004; Martins and Pessoa e Costa, 2014). We acknowledge that we are not able to study displacement effects in our context since we do not have adequate variation to study spillovers between treated and non-treated individuals.

employment in lieu of salary job options.

The weighted results in Figure 1F show that the program increased W-2 earnings in 2010, the year following program assignment by $2,273, compared with a base of $15,055 for the control group. The program had positive and economically meaningful effects in subsequent years, ranging from $1,642 to $3,297; in total, the program increased individual earnings in the entire eight-year follow-up period by $18,879, which represents an 83 percent increase relative to the control group mean (over the eight-year follow up period). Overall, these results are consistent with the view that the Nevada program caused persistent effects on participants' employment and earnings that extend beyond the short-term effects found in studies of similar programs or the same Nevada program studied here.[15]

C. Effects on UI and Disability Benefits

To assess if positive impacts on salary employment and earnings were accompanied by reductions in UI benefit receipt, we estimate treatment effects on the likelihood of collecting UI benefits (has a form 1099-G) and UI benefit amounts collected (amount reported on form 1099-G). Figure 2A shows that the program significantly reduced the likelihood of collecting UI receipt in 2010, the year after program entry, which is consistent with the program's effects on employment. UI receipt declined for both groups in subsequent years and treatment-control differences were small and lacked statistical significance.

Consistent with these results, Figure 2B shows that the program reduced UI benefit amounts collected in 2010 but effects are close to zero in subsequent years. These results suggest that the

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[15] Note that employment and earnings effects for 2008-2011 based on the Nevada program data are similar with effects based on tax records using either the matched unweighted or the matched weighted samples (Appendix Figure B). Employment and earnings information beyond 2011 is not available in the Nevada program data.

program's employment impacts may have occurred mostly for individuals who in the absence of the program would not have returned to the UI program in the future, possibly because they would not have been able to qualify for a new UI claim after exhausting their entitlement on their initial claim. In terms of dollar amounts, the program reduced UI by $808 in 2010; exceeding the $201 reported average program costs (Poe-Yamagata *et al.,* 2012) by more than 3.5 times.

We also consider effects on collecting social security benefits (based on form 1099-SSA), which include receipt of disability insurance. Intuitively, by shifting individuals to more efficient search strategies, the intervention may delay or eliminate entry into disability insurance for qualified individuals. Results in Figure 2C show that the likelihood of entering disability insurance gradually increased following the 2009 unemployment episode for both treatment and control groups, from less than one percent pre-2009 to more than 4 percent in 2017.[16] The trajectory appears to be slightly lower for the treatment than for the control group but differences are not statistically significant. Similarly, Figure 2D shows positive trends in social security benefits collected for both groups but treatment-control differences are close to zero.

D. Effects on Family and Tax Outcomes

Prior research has highlighted the impacts of job loss on mortgage defaults and foreclosures (Tian *et al.,* 2016; Gerardi *et al.,* 2017). Motivated by this prior research and the positive program effects on employment and earnings, we examine effects on homeownership. Figure 3A shows that the program caused positive treatment effects on homeownership that first emerge most prominently in 2011 and persist for the remaining post-experiment period. Given the overall declines in homeownership between 2008 and 2010 for both the treatment and control individuals,

[16] These patterns are consistent with (aggregate) evidence suggesting that expiration of UI benefits is accompanied by a very modest increase in disability insurance take-up (e.g., Mueller *et al.*, 2016)

homeownership treatment effects may be concentrated among individuals who were homeowners in 2008, the year prior to program assignment. As seen in Figure 3B, following the 2009 job loss, homeownership fell rapidly for both treatment and control cases, but the decline was steeper for control cases. Positive treatment effects for homeowners appear as early as 2009 (the year of assignment) and persist through the entire eight-year follow-up. We interpret the long-term effects on homeownership in terms of increased employment causing treated homeowners to be less likely to move out of homeownership following job loss.

Results for individuals who were not homeowners prior to the 2009 job loss (Figure 3C) show positive treatment-control differences in homeownership in the eight-year follow-up although estimates are not statistically different from zero, with partial exception of the 2012 and 2017 estimates (p-value<0.10). This provides some (weak) evidence that the program may have helped some non-homeowners to move into homeownership following the 2009 job loss.

We next consider whether the intervention may have implicitly aided the job-search outcomes of participants' spouses. Results for spousal employment (likelihood that the spouse had W-2 earnings) in Figure 3D shows a small decline for both treatment and control cases in the eight-year period after the intervention. However, there does not appear to be any statistically significant or economically meaningful differences between the treatment and control groups, suggesting that the lessons learned from the intervention did not help (nor encouraged) participants' spouses to conduct a more effective job search.

The effect on tax payment is consistent with the positive impacts on employment and earnings. Figure 3E shows that, following the 2009 unemployment episode, net tax payments (defined as federal income tax liabilities plus federal payroll taxes minus refundable tax credits) increase faster for the program group. As a result, the program increased tax payments each year during the entire eight-year follow-up period. Overall, the program caused participants to pay $4,558 higher taxes than control cases during the eight-year follow-up period.

Figure 3F presents results for after-tax income, which is defined as the broad individual income minus net payments (federal income tax liabilities plus federal payroll taxes minus refundable tax credits). Results show that both treatment and control cases experienced declines in after-tax income through year four after program assignment, with after-tax income steadily increasing thereafter. We find positive effects on after-tax individual income in the entire eight-year follow-up period.[17]

IV. Heterogeneous Treatment Effects

A. Causal Forests

We examine heterogeneous treatment effects to better understand mechanisms behind the impacts of the program.[18] We apply machine learning techniques to study heterogeneity in short-term and long-term employment outcomes and correlations in heterogeneous treatment effects

[17] We have also analyzed some additional outcomes using a cross-sectional approach to gain further insights into the average treatment effects. These results are presented in the Appendix. We examine treatment effects on (1) the distribution of average after-tax income over 2010 through 2017, (2) total years of employment from 2010 through 2017, and (3) cumulative employment from 2010 through 2017. Results for the distribution of average after-tax income (Appendix Figure C3) indicate that the effects on after-tax income appear to be driven by decreases in the fraction of individuals in lower income bins (below $20,000) and increases in the fraction of individuals at middle income bins ($30,000 – $50,000) and high-income bins ($70,000 and $90,000). Results for years of employment (Appendix Figure C4) highlight that the treatment may have had both intertemporal and extensive margin impacts. In terms of extensive margin effects, the treatment group has lower fractions of individuals with zero and one years of employment. In terms of intertemporal effects, the treatment group has a higher fraction with eight years of employment while the control group has a slightly higher fraction with seven years of employment. Results for cumulative employment (Appendix Figure C5) show some catching up for the control group relative to the treatment group between 2010 and 2011 and a persistent difference in any employment through 2017.

[18] Aside from heterogeneous treatment effects based on causal forests, we also study heterogeneity along multiple observed dimensions based on simple sample splits based on the following covariates: marital status and children, age and gender, and education. These results, presented in Appendix Figures D through F, show that program effects are not statistically different across subgroups, although there are relatively large effects for married individuals with no children. We note that, in the case of older individuals, we find that the initially large effects on employment fade out in later years, which may be due to older individuals, regardless of their treatment status, withdrawing from the labor force due to retirement.

based on employment, disability, and homeownership outcomes. Analyzing the magnitudes of the heterogeneous treatment effects on short-term and long-term employment outcomes allows us to examine if positive treatment effects are concentrated among particular individuals and there are negative effects for others or if the treatment effects are positive and similar across treated individuals. The correlation between the heterogenous short-term and long-term treatment effects on employment provide insights into which individuals have treatment effects that fade out or emerge later over the longer term. The correlations between heterogenous treatment effects on employment and other outcomes provides insight into whether impacts on other outcomes could be driven by impacts on employment or other factors.

We use causal forests to estimate heterogeneous treatment effects (Wager and Athey 2018). Similar to random forests, causal forests randomly select covariate splitting variables to grow trees and average results across many trees. The averaging across many trees reduces variance and random selection of covariate splitting variables de-correlates the many different trees used in the averaging. Intuitively, if there is only one covariate that is related to heterogeneity, then only trees grown when this covariate is randomly selected will detect the heterogeneity based on this covariate. On the other hand, if the one covariate were always used for splitting, then all of the trees grown based on this covariate would have correlated results and it would not be possible to detect that there is no meaningful heterogeneity when this covariate is not selected. While random forests grow trees based on maximizing an objective function related to variances in outcomes across groups based on observables, causal forests grow trees based on maximizing an objective function related to variances in differences between average outcomes for treatment and control observations within groups based on observables.

Similar to Davis and Heller (2017), our application of the causal forest algorithm is as follows.

Step 1: Start with the full sample N and randomly draw a 20 percent subsample without replacement, ($S_{holdout}$=0.20*N); the remaining 80 percent subsample is the training subsample ($S_{training}$=0.80*N).

Step 2: The training sample is randomly split in half into the splitting subsample which is used to grow trees ($S_{splitting}$) and the estimation subsample which is used to estimate differences in average outcomes for treatment and control observations in each leaf ($S_{estimation}$).

Step 3: Using the splitting subsample, a random subset of covariates is drawn from the full set of spitting covariates. In our application, the full set of splitting covariates consists of eight covariates (age, gender, marital status, presence of kids, 3-digit ZIP code, 2-digit industry, 2008 wages, and 2008 UI receipt). From this set, we randomly select 3 covariates to use in the splitting sequentially. If a covariate is randomly selected, then all values of this covariate are used to consider splits. For example, if age and ZIP code are selected as covariates, then splits are considered based on four age groups <25, 25-34, 35-54,>54 and all 3-digit ZIP codes in the sample. With the randomly selected covariates, trees are grown based on maximizing the following objective function:

$$\Phi = (n_{Tl} - n_{Cl})\tau_l^2 - 2(\frac{var(y_{Tl})}{n_{Tl}} + \frac{var(y_{Cl})}{n_{Cl}})$$

where: n_{Tl} and n_{Cl} refer to the number of treatment and control observations in each leaf *l*; y_{Tl} and y_{Cl} refer to the outcomes for treatment and control observations in each leaf *l*; $\tau_l = \bar{y}_{Tl} - \bar{y}_{Cl}$ denotes the difference in average outcomes for treatment and control observations in leaf *l*; and $var(y_{Tl})$ and $var(y_{Cl})$ refer to the variances of outcomes for treatment and control observations in each leaf *l*.

Step 4: Once the tree is grown using the splitting subsample, it is applied to the estimation sample, and τ_l is computed using the leaves of the tree from the splitting sample. This step makes the causal forest algorithm honest (i.e. avoiding overfitting) since the subsample used to grow the tree is different from the subsample used to estimate differences between treatment and control observations in each group.

Step 5: The estimated treatment effects (τ_l) are then applied to all observations in the full sample that fall in the corresponding leaf defined by the splitting covariates.

Step 6: Steps 1 through 5 are repeated B=10,000 times, and average treatment effect for each individual i is computed based on averaging the predictions, $\tau_i = \frac{1}{B}\sum_{b=1}^{B} \tau_{i,b}$. Using the individual-level treatment effects, we also compute percentile distributions of the treatment effects. We rank individuals based on τ_i, and then for each percentile p, we compute average treatment effects τ^p, by taking the average of the treatment effects across all individuals in the given percentile group. The 95 percent confidence interval is constructed by computing the standard deviation of the mean treatment effects across all individuals in the percentile group and then adding and subtracting 1.96 times the standard deviation.

We apply the causal forest algorithm to multiple outcomes separately, so steps 1 through 6 are repeated 10,000 times for each of the following outcomes: W-2 employment in 2010, W-2 employment in 2017, disability benefit receipt in 2017, and homeownership receipt in 2017.

B. Results

Figures 4 through 8 present the results from the causal forest analysis of heterogeneous treatment effects. For each plot in Figure 4, the horizontal axis illustrates 20 five-percentile bins based on the average treatment effect for each individual using 2010 employment as the outcome variable. Figure 4A illustrates that the fraction treated does not appear to vary systematically across the treatment effects for 2010 employment. This is consistent with random assignment; in particular, it does not appear that assignment to the treatment group was concentrated among individuals with excessively high or low treatment effects. Turning more explicitly to the magnitudes of the 2010 employment treatment effects, Figure 4B plots the average treatment effect within each five-percentile bin. Results indicate that about 90 percent of participants experienced positive employment treatment effects and about 10 percent experienced negative effects. For the highest percentile bins, the treatment effects on 2010 employment are 0.10, which is more than twice as large as the average treatment effect for the whole sample. For the lowest percentile bin, the 2010 treatment effect is negative (-0.025) indicating the treatment actually slightly reduces short-term employment for some individuals. Figure 4C plots average employment rates for each percentile bin, and this plot highlights that the largest positive treatment effects appear concentrated among individuals with lower employment rates, and the negative treatment effects are concentrated among individuals with the highest employment rates.[19]

Figure 5 present plots based on treatment effects on 2017 (long-term) employment outcomes. Similar to Figure 4B, Figure 5A illustrates the magnitudes of the 2017 employment treatment effects. The plot highlights that the treatment effect for the highest percentiles is 0.08, which is significantly larger than the 0.009 average treatment effect for 2017 employment found in our earlier analyses. The plot demonstrates that while the average treatment effect may fade out over the longer-term, about 40 percent of the sample is predicted to have positive treatment effects on long-term employment of 0.02 or larger. On the other hand, the plot also shows that the bottom 30 percent of the sample has negative treatment effects on long-term employment, reaching -0.062

[19] We also studied heterogeneity based on employment using the following regression techniques. First, we used the control sample and regress an indicator for 2010 W-2 employment on pre-experiment covariates. Second, we used this model to predict 2010 employment probabilities for both treatment and control observations. Third, we divided individuals into quartiles based on the predicted employment probabilities. Fourth, we estimated treatment-control differences within each quartile. Consistent with the causal forest results, these results, which are presented in Appendix Figure G, also highlight the largest positive treatment effects for individuals with lower employment probabilities and negative treatment effects for individuals with higher predicted employment probabilities.

for the lowest percentiles.

Consistent with Figure 4C, Figure 5B illustrates that the largest positive treatment effects are generally concentrated among individuals with lower 2017 employment rates, while negative treatment effects are concentrated among individuals with higher 2017 employment rates. Figure 5C presents the relationship between the short-term (2010 employment, x-axis) and long-term (2017 employment, y-axis) treatment effects. This plot highlights a strong positive correlation between the treatment effects on short-term and long-term employment. More specifically, the individuals in the bottom half of short-term employment treatment appear to have negative treatment effects on long-term employment.

Figure 6 presents evidence on heterogeneous treatment effects in 2017 disability receipt and how these effects correlate with treatment effects on 2017 employment. Figure 6A illustrates that treatment effects for disability receipt range from -0.03 to 0.07, so the average treatment effect reported in Figure 2C may mask both increases and decreases in disability receipt. Figure 6B highlights that for individuals in the bottom 30 percent of 2017 employment treatment effects, for whom employment effects are negative, the disability treatment effects are positive. For individuals who experience decreases in long-term employment due to the treatment, the treatment also increases long-term disability receipt. This result makes sense as disability is available primarily to those who cannot work. Furthermore, Figures 6C and 6D present evidence on long-term average treatment effects for individuals with negative and positive treatment effects on 2017 employment. These plots confirm the different patterns in disability receipt across these two subsamples: the treatment increased entry into disability receipt for the sample with negative employment treatment effects (Figure 6C), whereas the treatment decreased entry into disability receipt for the sample with positive employment treatment effects (Figure 6D).

Turning to homeownership, Figure 7A illustrates that treatment effects on homeownership are non-negative for about 70 percent of treatment effects and get as large as 0.068. Figure 7B indicates a positive correlation between long-term treatment effects on employment and homeownership so individuals with larger increases in employment have larger increases in homeownership. The positive correlation between the treatment effects on long-term homeownership and long-term employment corroborates the heterogeneous treatment effects on long-term employment. We focus on this general correlation between the treatment effects as opposed to the specific magnitudes of the treatment effects because the magnitudes may not be perfectly comparable since the causal algorithm is applied using different outcomes when computing the different treatment effects. Figures 7C and 7D present long-term average treatment effects on homeownership for individuals with negative and positive treatment effects on 2017 employment respectively. These plots confirm that the positive impacts on homeownership are generally concentrated among individuals with positive employment treatment effects, and there does not appear to be a persistent decline in homeownership among individuals with negative employment treatment effects.

Overall, based on the results in Figures 4 through 7, we draw the following conclusions from the analysis of heterogeneous treatment effects. First, while average treatment effects are positive in both the short-term and long-term, there is evidence of negative employment treatment effects for some individuals (Figures 4B and 5A). Second, employment treatment effects are negatively correlated with employment rates (Figures 4C and 5B): the largest positive employment treatment effects are among individuals with low employment rates, and the largest negative employment treatment effects are among individuals with high employment rates. Third, there is heterogeneity in treatment effects on disability benefit receipt with positive treatment effects for some individuals and negative treatment effects for others (Figure 6A). Corroborating the negative employment treatment effects for some individuals, the disability treatment effects are negatively correlated with employment treatment effects, so individuals predicted to have decreases (increases) in employment due to the treatment are also predicted to have increases (decreases) in disability receipt (Figure 6B). Fourth, there is heterogeneity in the magnitudes of the homeownership treatment effects (Figure 7A). Individuals with larger positive employment treatment effects may have been less likely to decrease homeownership during the Great Recession (Figure 7D).

V. Conclusion

Our findings show that job-search assistance interventions can produce statistically significant and economically meaningful long-term average treatment effects on individual employment and earnings. These impacts are associated with large short-term reductions in UI benefit receipt, and increases in homeownership, tax filing, after-tax income, and Federal tax receipts. Results show that job-search assistance interventions may have substantial long-term effects on the outcomes of participants, their families, and fiscal policy that extend beyond any short-term employment impacts. Analysis of heterogeneous treatment effects indicates that there is substantial heterogeneity in long-term effects on employment. About 30 percent of participants experienced much larger than average treatment effects on long-term employment, while roughly 40 percent of participants experienced negative long-term employment effects. This heterogeneity is corroborated by analyses of treatment effects on disability benefit receipt and homeownership. Participants with large positive long-term employment effects experienced reductions in long-term disability receipt and increases in homeownership, while negative long-term employment effects are associated with increases in long-term disability insurance receipt.

We provide one possible conceptual framework to reconcile these results. We describe a theory of change in which individuals are unaware of the effectiveness of their job-search efforts, and the job-search assistance intervention reduces the fixed costs of job search and causes individuals to switch search strategies. By reducing fixed costs, the intervention causes some individuals who would not have otherwise searched for jobs to search. By causing individuals to switch job-search strategies, and since individuals are unaware of the effectiveness of their baseline job search, the treatment causes some individuals who would have used ineffective strategies to switch to more effective strategies, and other individuals who would have used effective strategies switch to less effective strategies. Moreover, ineffective strategies may lead to low-quality job matches so that negative treatment effects on employment emerge over the longer term. The long-term and heterogeneous treatment effects indicate that one-time job search assistance can have sustained impacts on employment, but program effectiveness may be improved by targeting job-search assistance to those with low predicted employment or ineffective search strategies.

More broadly, as job-search assistance programs are expanded and replicated, our analysis highlights the importance of having a detailed understanding of individuals' baseline job-search behaviors in the absence of any assistance and a theory of change for how assistance would interact with these baseline search behaviors to produce intended outcomes. In particular, a specific and detailed theory of change could possibly justify targeting assistance to minimize potential negative impacts and possibly provide insights into how assistance services should be designed so that they are most effective for those who receive them. Our results also highlight that, in addition to estimating average treatment effects, future analyses of job-search assistance programs may aim specifically to test hypotheses related to long-term and heterogeneous treatment effects and identify possible negative treatment effects.

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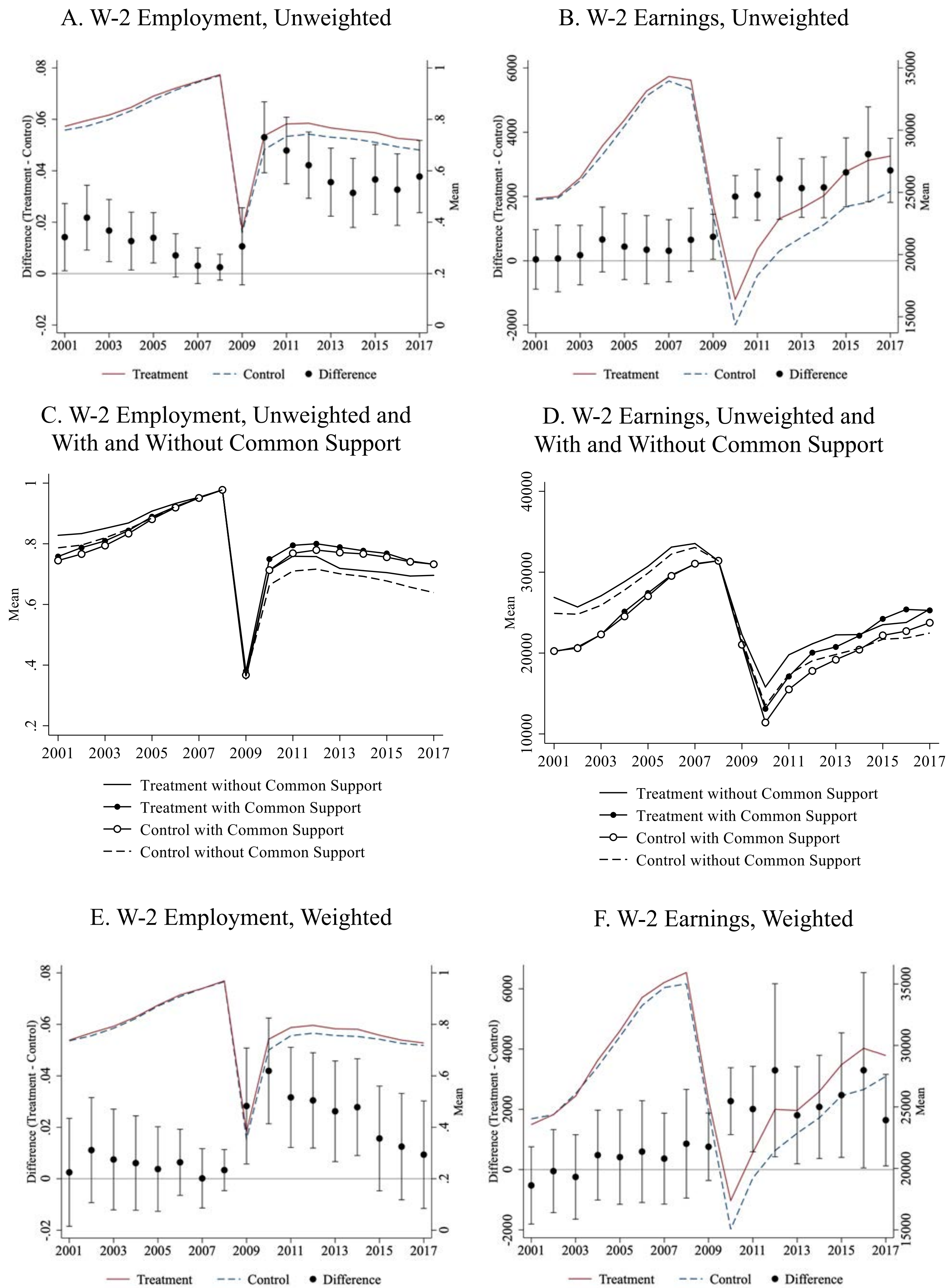
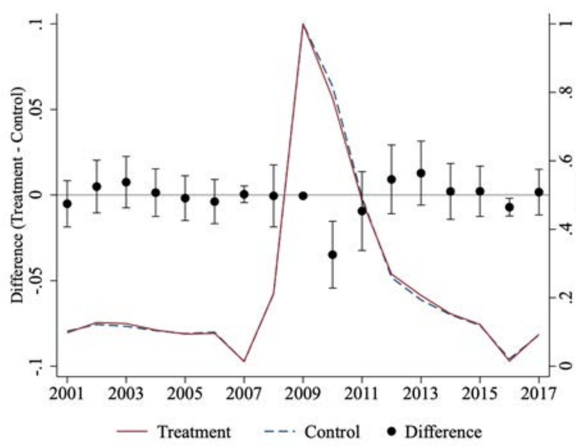
Figure 1. Treatment Effects on W-2 Employment and Earnings



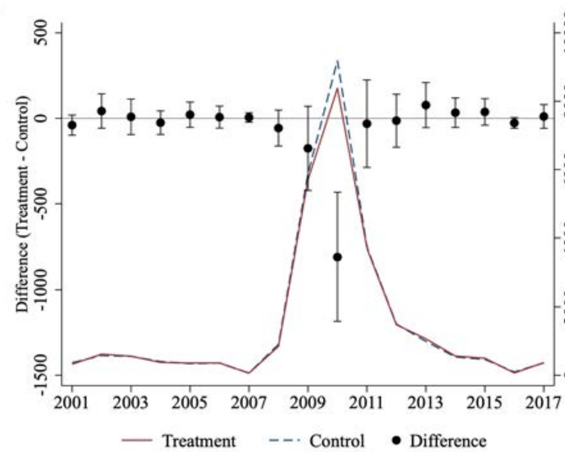
Notes: Black dots represent treatment-control differences in outcomes, with 95 percent confidence intervals shown in vertical bands. Controls include week of random assignment indicators. Standard errors are clustered by individual. Solid and dotted lines present outcome means for the treatment and control groups, respectively. Full results are reported in Appendix Table A.

Figure 2: Treatment Effects on Government Benefit Receipt

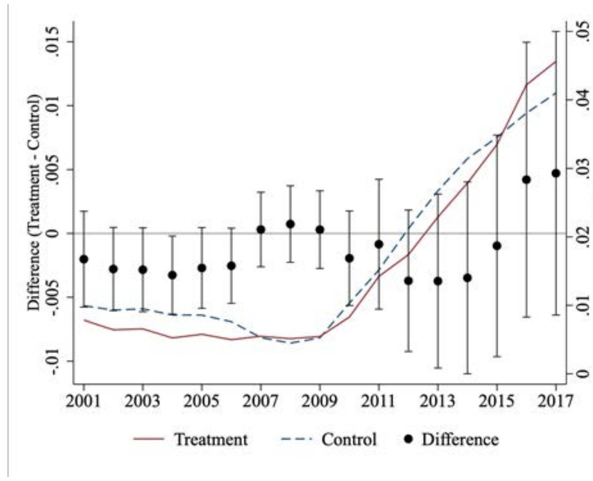
A. Receipt of UI Benefits (Has 1099-G)



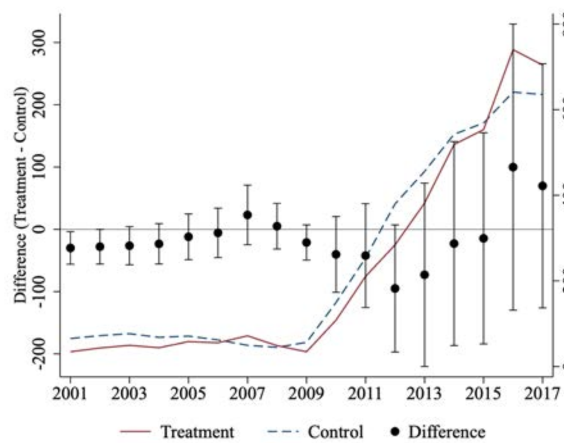
B. UI Benefit Amounts



C. Has Social Security Disability Benefits (Has 1099SSA)

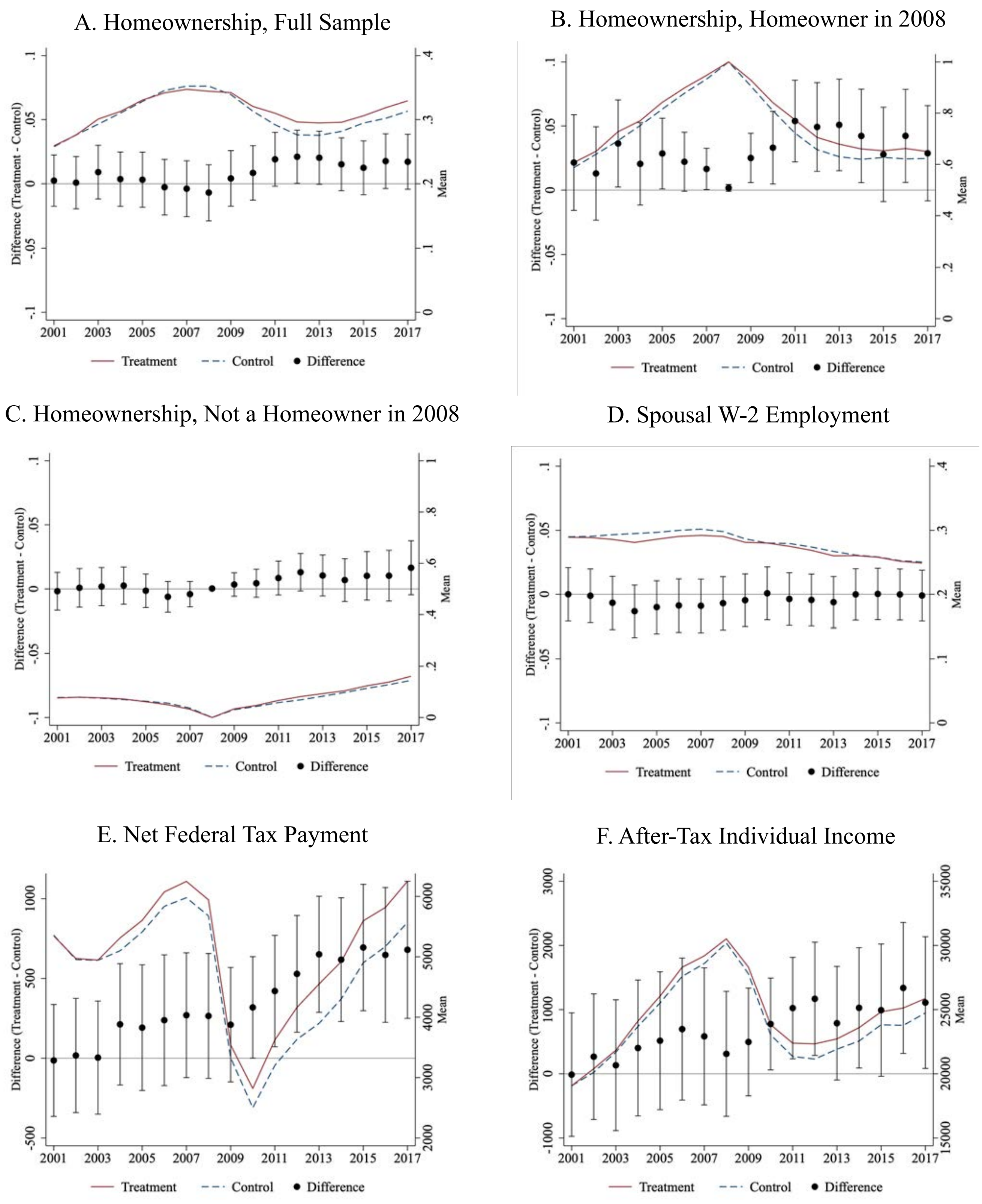


D. Social Security Disability Amounts



Notes: Black dots represent treatment-control differences in outcomes, with 95 percent confidence intervals shown in vertical bands. Controls include week of random assignment. Solid and dotted lines present outcome means for the treatment and control groups, respectively. Full results are reported in Appendix Table B.

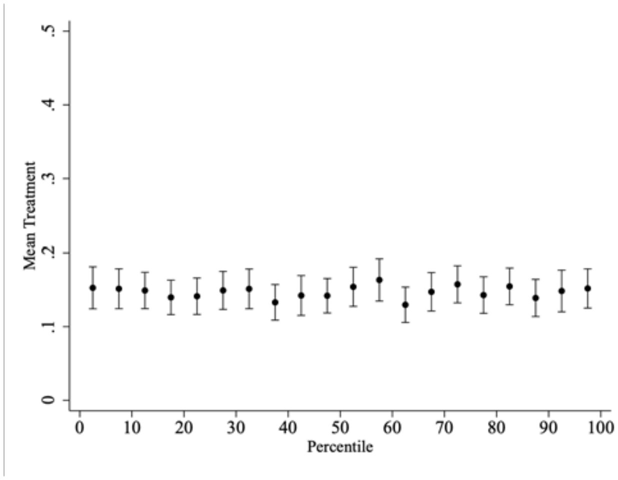
Figure 3: Treatment Effects on Family-Level Outcomes



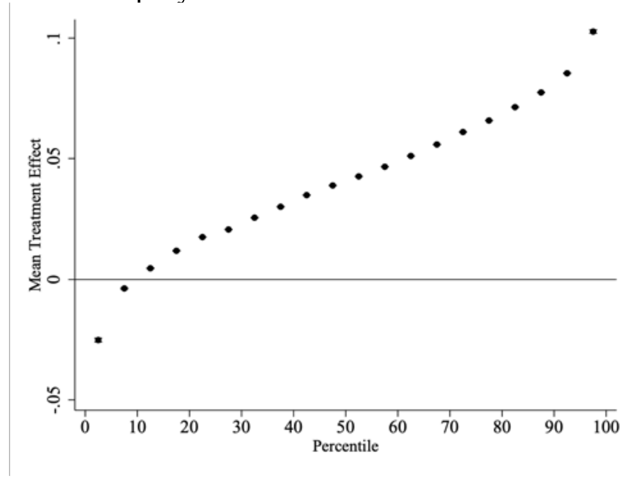
Notes: Black dots represent treatment-control differences in outcomes, with 95 percent confidence intervals shown in vertical bands. Controls include week of random assignment. Solid and dotted lines present outcome means for the treatment and control groups, respectively. Full results are reported in Appendix Table C.

Figure 4. Heterogeneous Treatment Effects: 2010 Employment

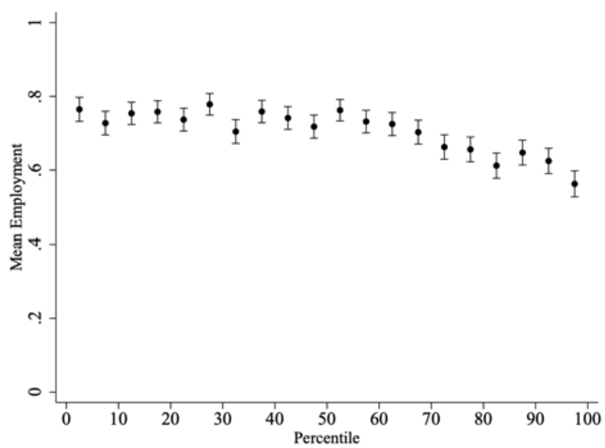
A. Fraction Treated by 2010 Employment Treatment Effect Percentile



B. 2010 Employment Treatment Effect by 2010 Employment Treatment Effect Percentile



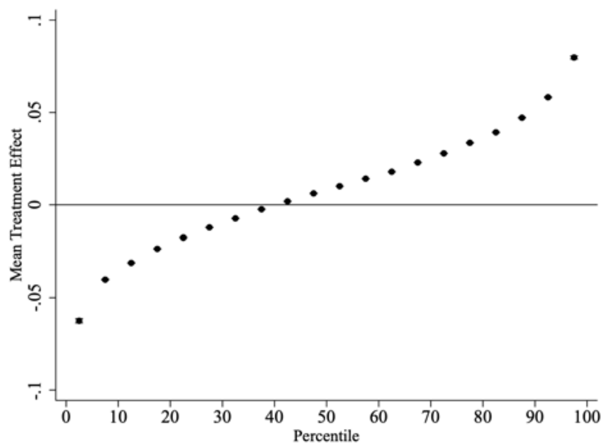
C. 2010 Employment by 2010 Employment Treatment Effect Percentile



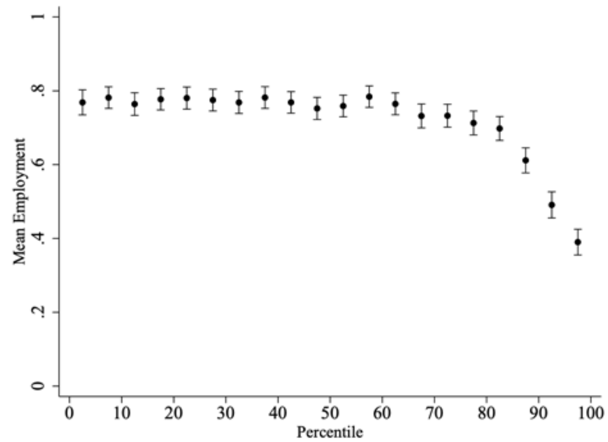
Notes: Each plot illustrates results from estimating heterogeneous treatment effects using causal forests (see Wager and Athey 2018 for the methodology and a similar application in Davis and Heller 2017). After estimating individual-level treatment effects for each individual in the sample, all individuals are sorted into percentile bins based on their individual-level treatment effect. For each plot, black dots illustrate means of the specified outcome variable across all individuals in a given percentile bin. The 95% confidence intervals, illustrated with vertical lines for each mean, are computed by adding and subtracting 1.96 times the standard deviation of the individual-level outcome across all individuals in the percentile bin.

Figure 5. Heterogeneous Treatment Effects: 2017 Employment

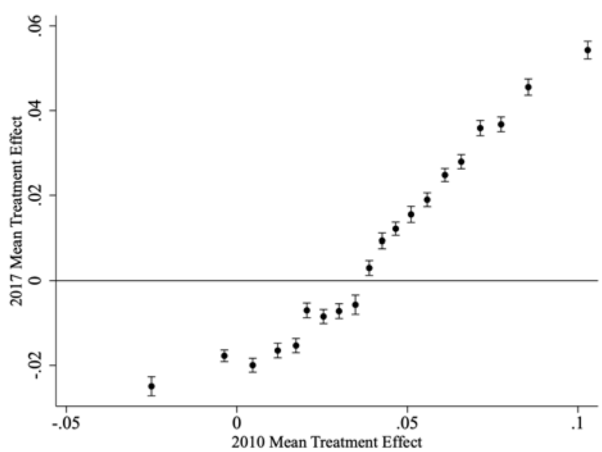
A. 2017 Employment Treatment Effect by 2017 Employment Treatment Effect Percentile



B. 2017 Employment by 2017 Employment Treatment Effect Percentile



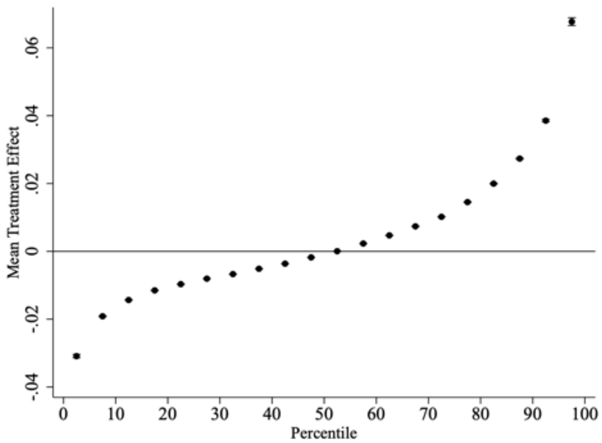
C. 2017 Employment Treatment Effect by 2010 Employment Treatment Effect



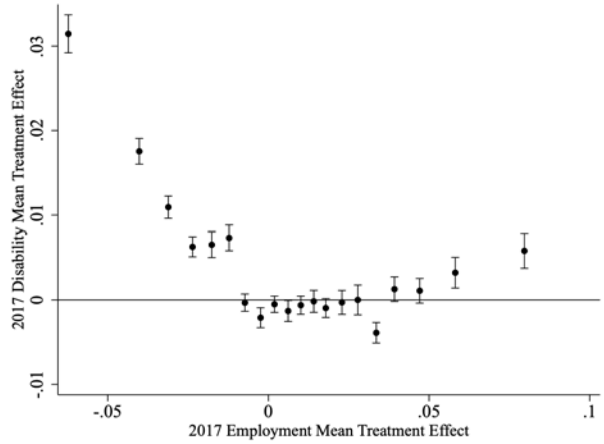
Notes: Each plot illustrates results from estimating heterogeneous treatment effects using causal forests (see Wager and Athey 2018 for the methodology and a similar application in Davis and Heller 2017). After estimating individual-level treatment effects for each individual in the sample, all individuals are sorted into percentile bins based on their individual-level treatment effect. For each plot, black dots illustrate means of the specified outcome variable across all individuals in a given percentile bin. The 95% confidence intervals, illustrated with vertical lines for each mean, are computed by adding and subtracting 1.96 times the standard deviation of the individual-level outcome across all individuals in the percentile bin.

Figure 6. Heterogeneous Treatment Effects: 2017 Disability

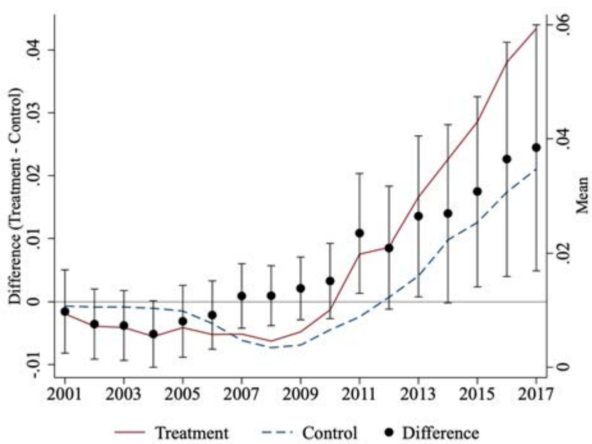
A. 2017 Disability Receipt Treatment Effect by 2017 Disability Receipt Treatment Effect Percentile



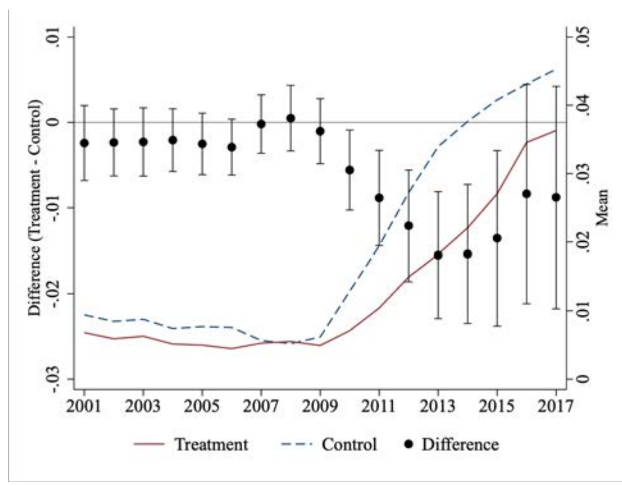
B. 2017 Disability Receipt Treatment Effect by 2017 Employment Treatment Effect



C. Disability Treatment Effects for Individuals with Negative 2017 Employment Treatment Effects



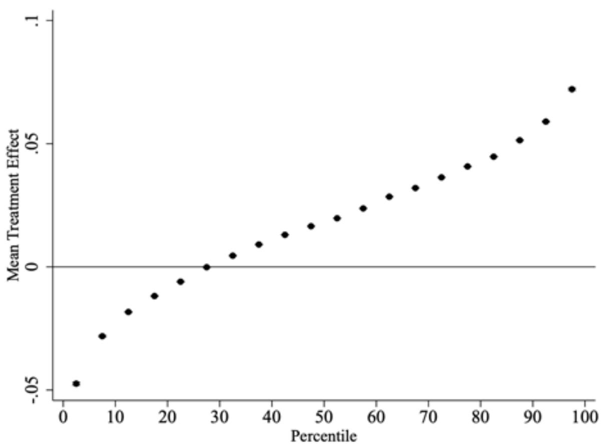
D. Disability Treatment Effects for Individuals with Positive 2017 Employment Treatment Effects



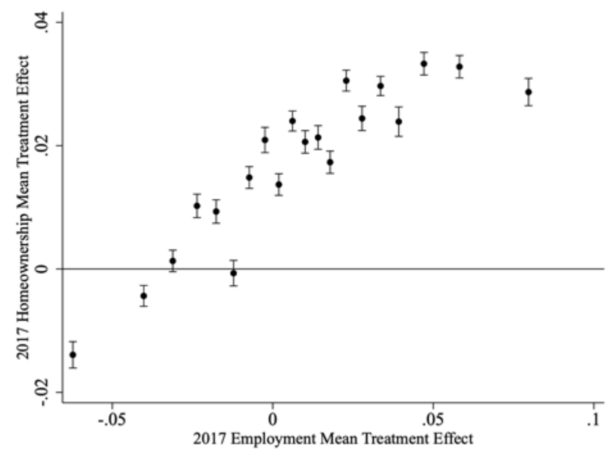
Notes: Each plot illustrates results from estimating heterogeneous treatment effects using causal forests (see Wager and Athey 2018 for the methodology and a similar application in Davis and Heller 2017). After estimating individual-level treatment effects for each individual in the sample, all individuals are sorted into percentile bins based on their individual-level treatment effect. For each plot, black dots illustrate means of the specified outcome variable across all individuals in a given percentile bin. The 95% confidence intervals, illustrated with vertical lines for each mean, are computed by adding and subtracting 1.96 times the standard deviation of the individual-level outcome across all individuals in the percentile bin. Panels C and D are based on splitting the sample using individual-level treatment effects and then estimating the baseline regression specification within each sample.

Figure 7. Heterogeneous Treatment Effects: 2017 Homeownership

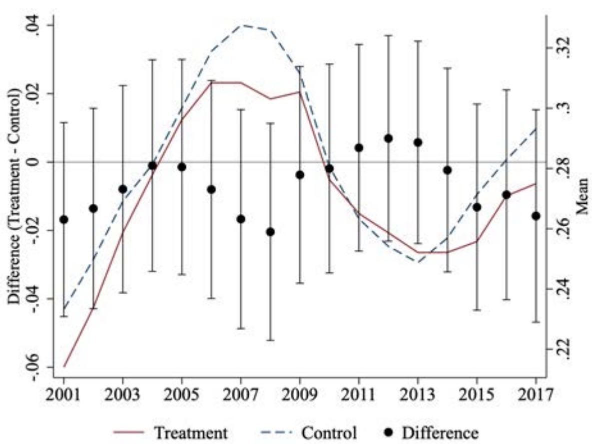
A. 2017 Homeownership Treatment Effect by 2017 Homeownership Treatment Effect Percentile



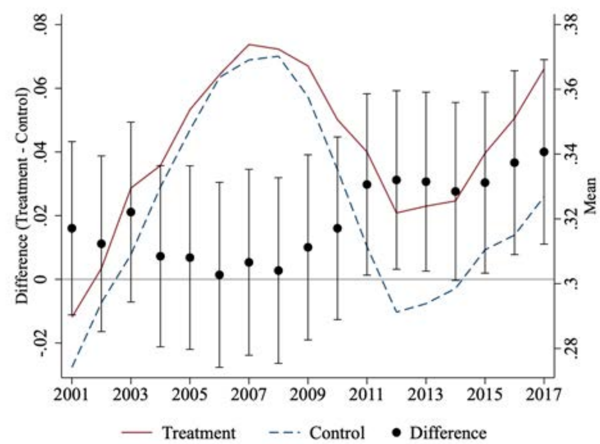
B. 2017 Homeownership Treatment Effect by 2017 Employment Treatment Effect



C. Homeownership Treatment Effects for Individuals with Negative 2017 Employment Treatment Effects



D. Homeownership Treatment Effects for Individuals with Positive 2017 Employment Treatment Effects



Notes: Each plot illustrates results from estimating heterogeneous treatment effects using causal forests (see Wager and Athey 2018 for the methodology and a similar application in Davis and Heller 2017). After estimating individual-level treatment effects for each individual in the sample, all individuals are sorted into percentile bins based on their individual-level treatment effect. For each plot, black dots illustrate means of the specified outcome variable across all individuals in a given percentile bin. The 95% confidence intervals, illustrated with vertical lines for each mean, are computed by adding and subtracting 1.96 times the standard deviation of the individual-level outcome across all individuals in the percentile bin. Panels C and D are based on splitting the sample using individual-level treatment effects and then estimating the baseline regression specification within each sample.

Table 1: Summary Statistics

	Unweighted			Weighted		
	Treatment	Control	p-value	Treatment	Control	p-value
Age < 25	0.179	0.177	0.8507	0.204	0.204	1.0000
	(0.383)	(0.382)		(0.403)	(0.403)	
Ages 25-34	0.222	0.222	0.9612	0.230	0.230	1.0000
	(0.416)	(0.415)		(0.421)	(0.421)	
Ages 35-44	0.223	0.229	0.3341	0.234	0.234	1.0000
	(0.416)	(0.420)		(0.423)	(0.423)	
Ages 45-54	0.241	0.207	0.0000	0.226	0.226	1.0000
	(0.428)	(0.405)		(0.418)	(0.418)	
Ages 55-64	0.108	0.120	0.0202	0.092	0.092	1.0000
	(0.311)	(0.325)		(0.289)	(0.289)	
Ages > 64	0.027	0.044	0.0000	0.014	0.014	1.0000
	(0.163)	(0.206)		(0.118)	(0.117)	
Female	0.422	0.433	0.1688	0.399	0.399	1.0000
	(0.494)	(0.496)		(0.490)	(0.490)	
Filed Return	0.940	0.927	0.0013	0.966	0.966	1.0000
	(0.237)	(0.260)		(0.182)	(0.182)	
Filing Status = Single	0.387	0.387	0.9800	0.421	0.421	1.0000
	(0.487)	(0.487)		(0.494)	(0.494)	
Filing Status = Joint	0.371	0.351	0.0073	0.385	0.385	1.0000
	(0.483)	(0.477)		(0.487)	(0.487)	
Filing Status = Head of Household	0.165	0.174	0.1636	0.156	0.156	1.0000
	(0.372)	(0.379)		(0.363)	(0.362)	
Filing Status = Other	0.017	0.016	0.6526	0.004	0.004	1.0000
	(0.129)	(0.126)		(0.060)	(0.060)	
Claimed EITC	0.191	0.203	0.0632	0.182	0.195	0.1179
	(0.393)	(0.402)		(0.386)	(0.396)	
EITC Amount	$427	$458	0.0909	$410	$423	0.5725
	(1111)	(1156)		(1102)	(1109)	
Has Self-Employment Income	0.098	0.097	0.8395	0.100	0.100	0.9730
	(0.297)	(0.295)		(0.300)	(0.300)	
Has Dependents with Age < 25	0.396	0.387	0.2908	0.397	0.403	0.5791
	(0.489)	(0.487)		(0.489)	(0.491)	
Has W-2 Wages	0.974	0.971	0.2688	0.969	0.966	0.4116
	(0.159)	(0.168)		(0.174)	(0.182)	
W-2 Wage Amount	$34,010	$33,329	0.1525	$35,943	$35,025	0.1817
	(31808)	(29710)		(38769)	(30917)	
Has 1099-MISC Income	0.068	0.070	0.5309	0.067	0.064	0.5600
	(0.251)	(0.255)		(0.249)	(0.244)	
1099-MISC Amount	$491	$459	0.4041	$530	$418	0.0311
	(2599)	(2450)		(2792)	(2354)	
Has 1099-G UI Benefits	0.289	0.269	0.0040	0.213	0.213	1.0000
	(0.453)	(0.443)		(0.409)	(0.409)	
UI Amount	$1,233	$1,181	0.2424	$854	$910	0.2820
	(2843)	(2783)		(2399)	(2451)	
Has 1099SSA Disability Benefits	0.005	0.005	0.8620	0.005	0.004	0.6828
	(0.071)	(0.070)		(0.071)	(0.067)	
Disability Benefit Amount	$52	$50	0.8796	$48	$45	0.8351
	(917)	(852)		(937)	(776)	
Has 1099SSA Retirement Benefits	0.030	0.048	0.0000	0.018	0.020	0.4553
	(0.171)	(0.213)		(0.133)	(0.140)	
Retirement Benefit Amount	$416	$671	0.0000	$254	$257	0.9410
	(2639)	(3311)		(2109)	(2059)	
Accomodation & Food Services or Retai	0.155	0.164	0.1322	0.157	0.157	1.0000
	(0.362)	(0.370)		(0.364)	(0.364)	
Agriculture, Construction, Mining or Ma	0.244	0.197	0.0000	0.242	0.242	1.0000
	(0.429)	(0.398)		(0.428)	(0.428)	
Other Industry	0.601	0.639	0.0000	0.600	0.600	1.0000
	(0.490)	(0.480)		(0.490)	(0.490)	
ZIP3 in Las Vegas (889, 891) or Reno (8	0.654	0.780	0.0000	0.802	0.802	1.0000
	(0.476)	(0.415)		(0.398)	(0.398)	
Covariates from Program Data						
No high school diploma	0.164	0.163	0.8285	0.134	0.134	1.0000
	(0.370)	(0.369)		(0.341)	(0.341)	
High School Diploma	0.426	0.435	0.2811	0.489	0.489	1.0000
	(0.495)	(0.496)		(0.500)	(0.500)	
Some College	0.288	0.283	0.5263	0.285	0.285	1.0000
	(0.453)	(0.451)		(0.452)	(0.452)	
College Degree	0.122	0.119	0.6035	0.091	0.091	1.0000
	(0.327)	(0.324)		(0.288)	(0.288)	
Disabled	0.049	0.046	0.3587	0.048	0.045	0.5513
	(0.217)	(0.210)		(0.213)	(0.207)	
Observations (Individuals)	4,672	27,107		4,672	27,107	

Notes: Sample means are reported with standard deviations in parentheses below the corresponding means. The p-value columns refer to tests of equality in the means across treatment and control groups. Summary statistics are based on tax year 2008. Age is age as of January 1, 2008. Amounts are CPI adjusted to 2015 dollars.

APPENDIX FOR ONLINE PUBLICATION ONLY

Additional Analyses Using Cross-Sectional Approach

We examine some additional outcomes using a cross-sectional approach to gain further insights into the average treatment effects. We examine treatment effects on (1) the distribution of average after-tax income over 2010 through 2017, (2) total years of employment from 2010 through 2017, and (3) cumulative employment from 2010 through 2017 using the following cross-sectional regression specification

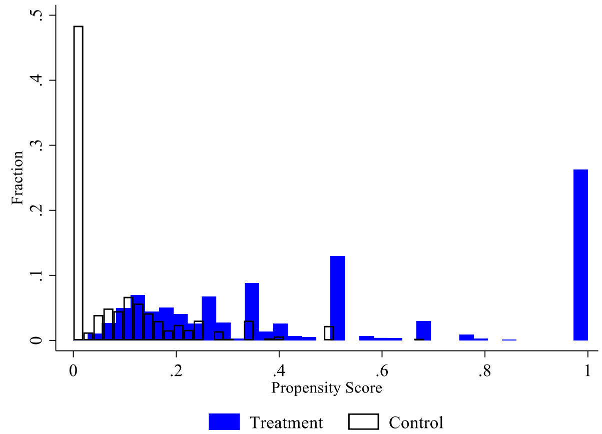
$$y_i = \alpha_0 + \beta Treat_i + Week_i\delta + u_i.$$

For the distribution of average after-tax income, we create indicators for individuals having average annual income over the entire eight-year follow-up period in $10,000 income bins, regress these indicators on the treatment indicator and plot the treatment and control group means and estimated treatment effect for each income bin. For the distribution of total years of employment, we create indicator variables for individuals having different values of years of employment from 2010 through 2017, regress these indicators on the treatment indicator, and plot the control group mean and estimated treatment effect for each number of years of employment. For cumulative employment from 2010 through 2017, we create indicators for ever being employed in 2010, 2010-2011, 2010-2012, 2010-2013, … 2010-2017, regress these indicators on the treatment indicator and then plot the control group mean and estimated treatment effect for each period of employment. These additional employment outcomes provide insights into whether the experimental job search assistance may have affected the timing of job finding (intertemporal treatment effects) or whether individuals found jobs at all (extensive margin treatment effects).

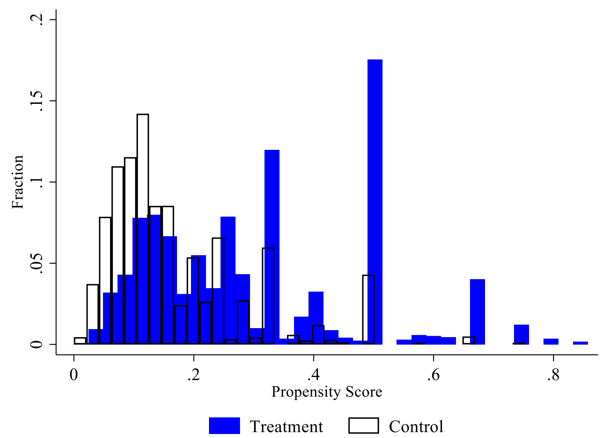
Results for the distribution of average after-tax income (Appendix Figure C3) indicate that the effects on after-tax income appear to be driven by decreases in the fraction of individuals in lower income bins (below \$20,000) and increases in the fraction of individuals at middle income bins (\$30,000 – \$50,000) and high-income bins (\$70,000 and \$90,000). Results for years of employment (Appendix Figure C4) highlight that the treatment may have had both intertemporal and extensive margin impacts. In terms of extensive margin effects, the treatment group has lower fractions of individuals with zero and one years of employment. In terms of intertemporal effects, the treatment group has a higher fraction with eight years of employment while the control group has a slightly higher fraction with seven years of employment. Quantitatively, results in Appendix Table D indicate that the decreases in the fractions at zero (-0.009), one (-0.014), and seven years of employment (-0.013) for the treatment group can account for almost all of the increase in the fraction at eight years of employment (0.038). Thus, extensive margin effects may account for roughly 60 percent ($\frac{0.009+0.014}{0.038}$) of the long-term treatment effect on eight years of employment and intertemporal effects may account for the remaining 35 to 40 percent. The results on cumulative employment (Appendix Figure C5) show some catching up for the control group relative to the treatment group between 2010 and 2011 and a persistent difference in any employment through 2017.

Appendix Figure A. Distributions of Propensity Scores

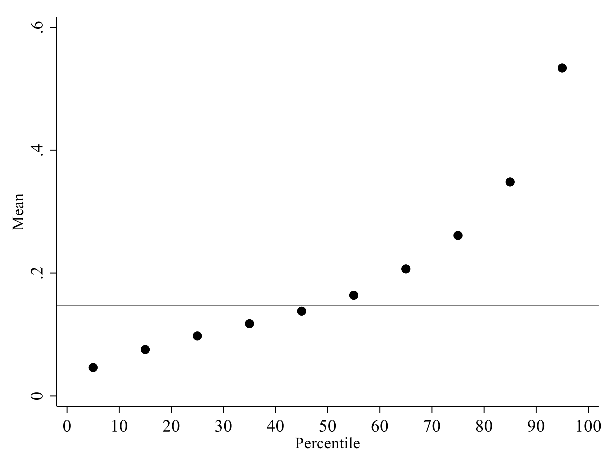
1. Histograms of Propensity Scores by Treatment Status,
Full Sample



2. Histograms of Propensity Scores by Treatment Status,
Sample with Common Support

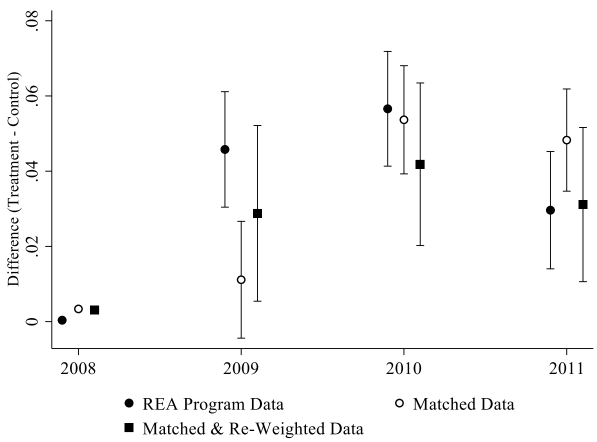


3. Fraction Treated by Propensity Score Bin,
Observations with Common Support

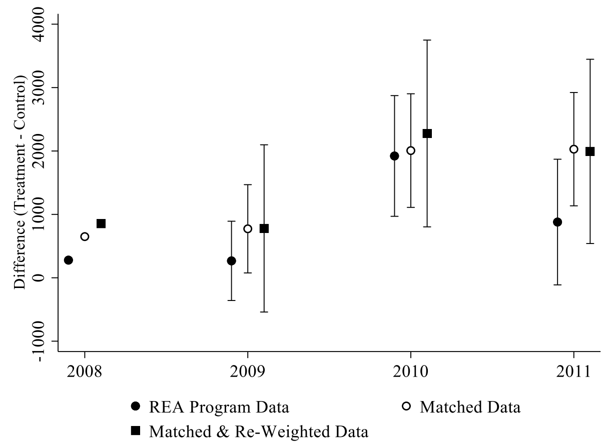


Appendix Figure B: Treatment-Control Differences in Employment and Earnings, REA Program Data vs. Sample Matched to Administrative Tax Data

1. Employment



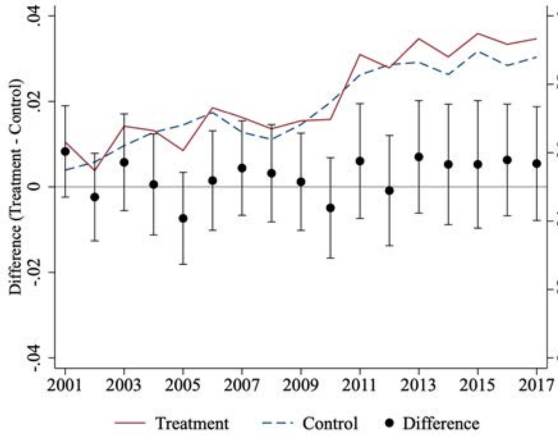
2. Earnings



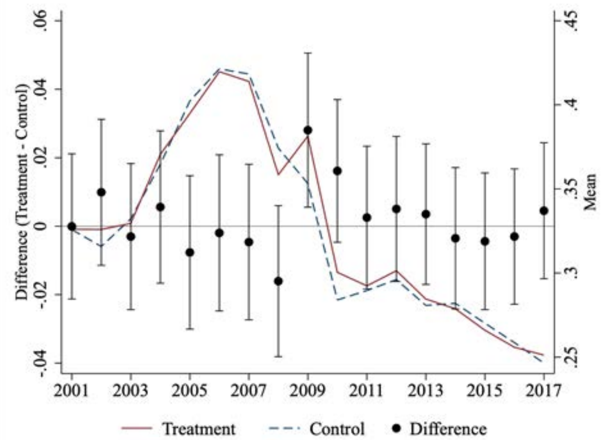
Notes: Black dots represent treatment-control differences based on the Nevada program data, and white squares represent treatment-control differences based on the administrative tax data. For the program data, annual values are based on aggregating quarterly values from the program data to the annual level. The 95 percent confidence intervals are shown in vertical bands. Standard errors are clustered by individual.

Appendix Figure C. Additional Outcomes

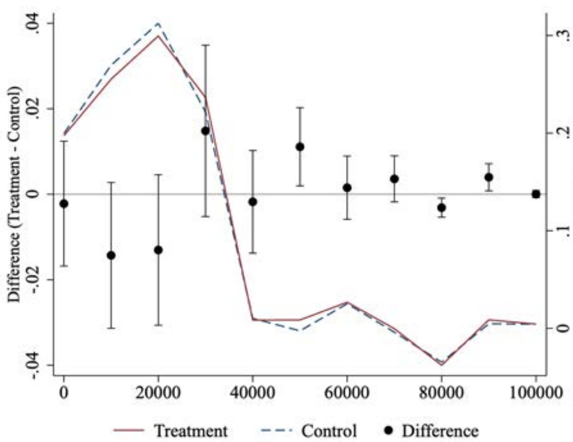
1. Has Contractor Income (Has 1099-MISC)



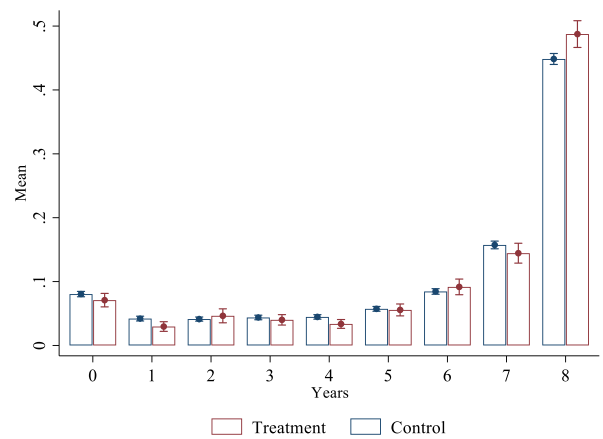
2. Probability of Having Multiple W-2s



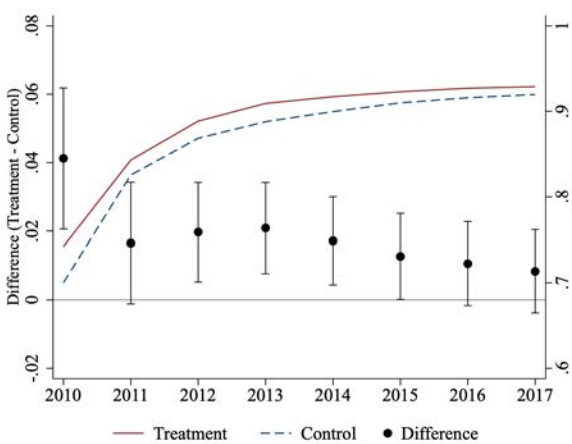
3. Distribution of Average After-Tax Income 2010-2017



4. Years of Employment from 2010-2017



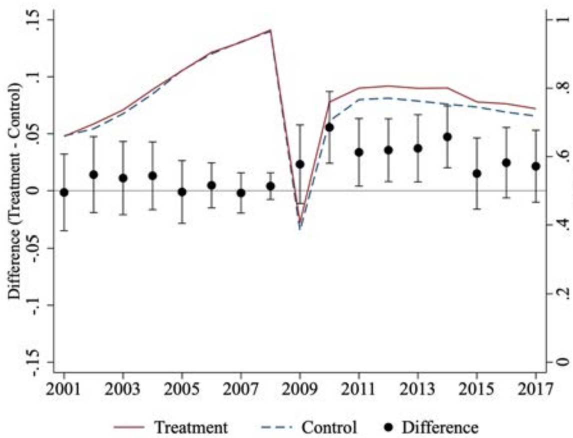
5. Ever Employed



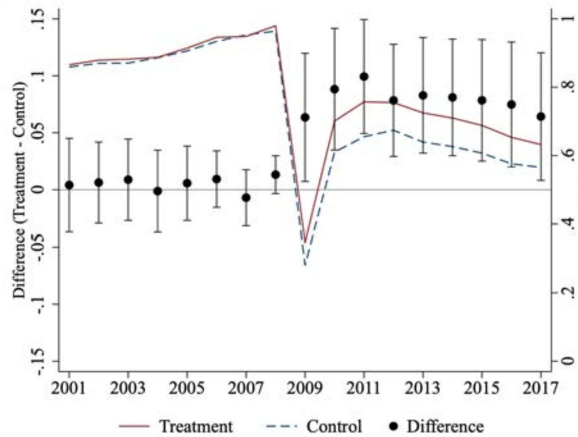
Notes: Black dots represent treatment-control differences in outcomes, with 95 percent confidence intervals shown in vertical bands. Controls include week of random assignment. Solid and dotted lines present outcome means for the treatment and control groups, respectively. Full results are reported in Appendix Table C (has contractor income; probability of having multiple W2s) and Appendix Table D (all other outcomes).

Appendix Figure D: Treatment Effects on W-2 Employment by Marital Status & Children

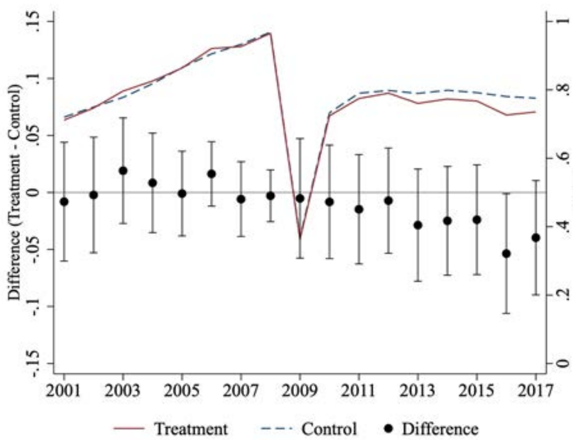
1. Single, No Children



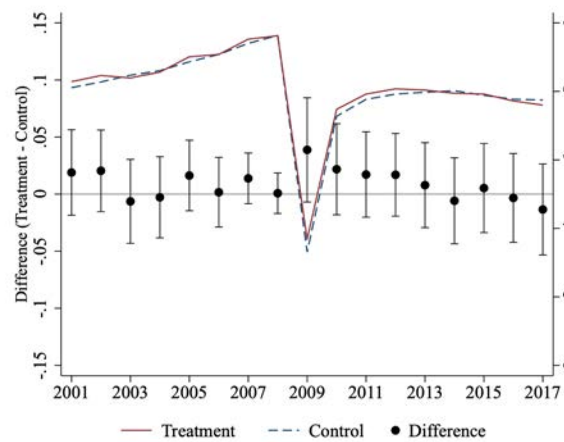
2. Married, No Children



3. Single, With Children



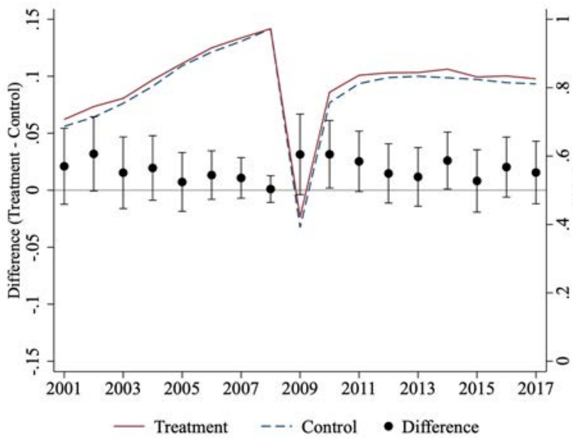
4. Married, With Children



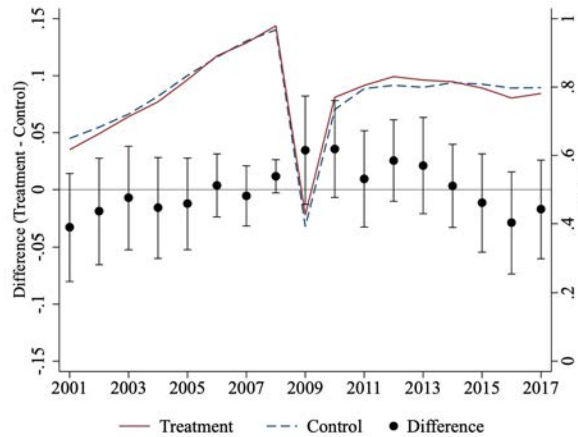
Notes: Black dots represent treatment-control differences in outcomes, with 95 percent confidence intervals shown in vertical bands. Controls include week of random assignment. Solid and dotted lines present outcome means for the treatment and control groups, respectively.

Appendix Figure E: Treatment Effect on W-2 Employment by Age and Gender

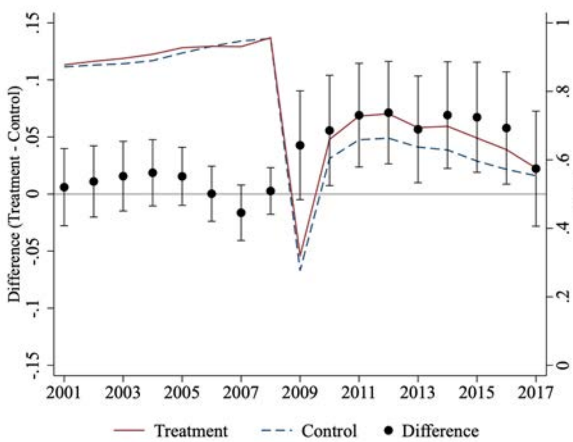
1. Age < 45, Men



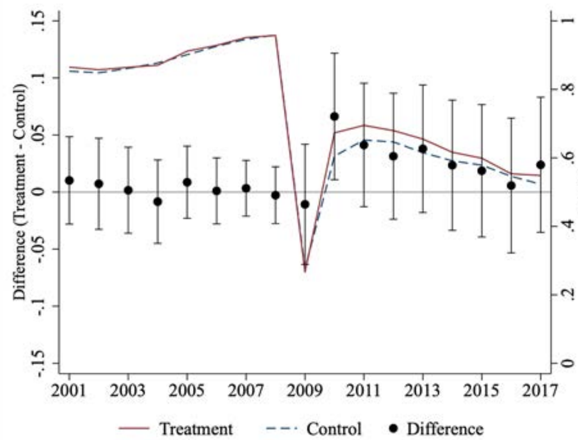
2. Age < 45, Women



3. Age ≥ 45, Men



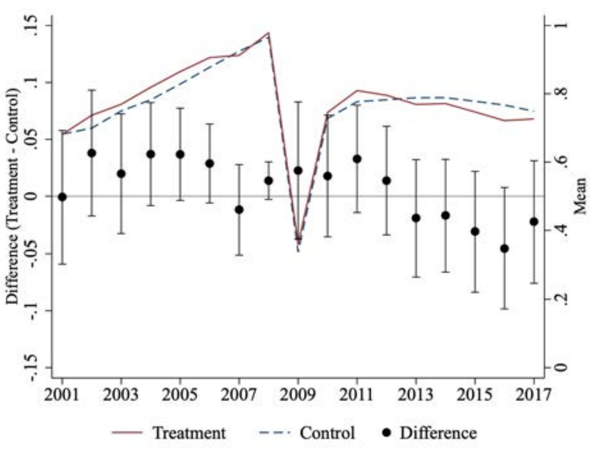
4. Age ≥ 45, Women



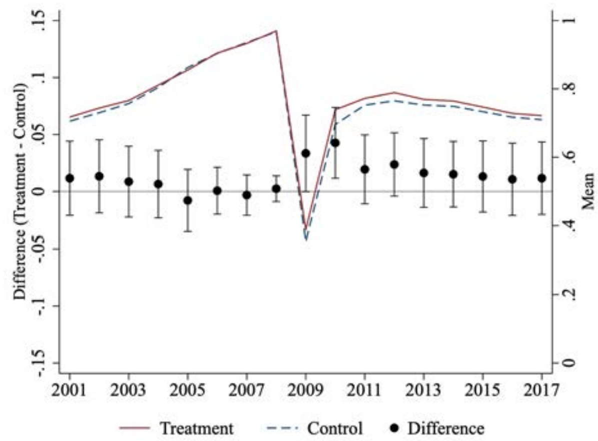
Notes: Black dots represent treatment-control differences in outcomes, with 95 percent confidence intervals shown in vertical bands. Controls include week of random assignment. Solid and dotted lines present outcome means for the treatment and control groups, respectively.

Appendix Figure F: Treatment Effect on W-2 Employment by Education

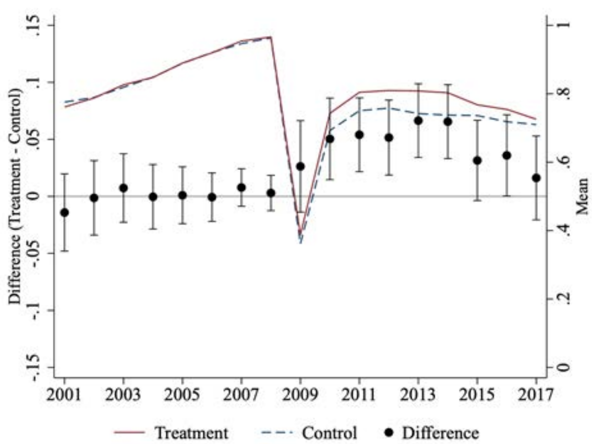
1. No High School Diploma



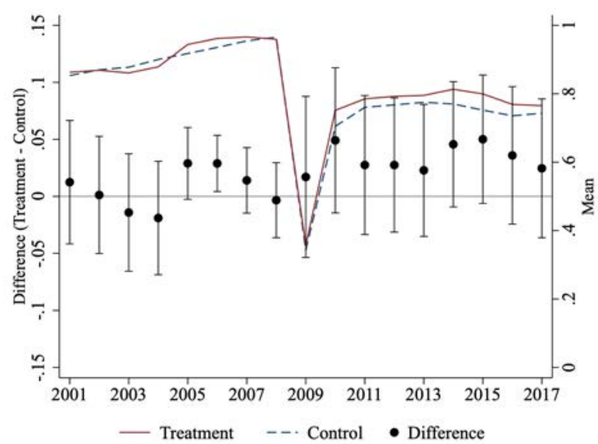
2. High School Diploma



3. Some College



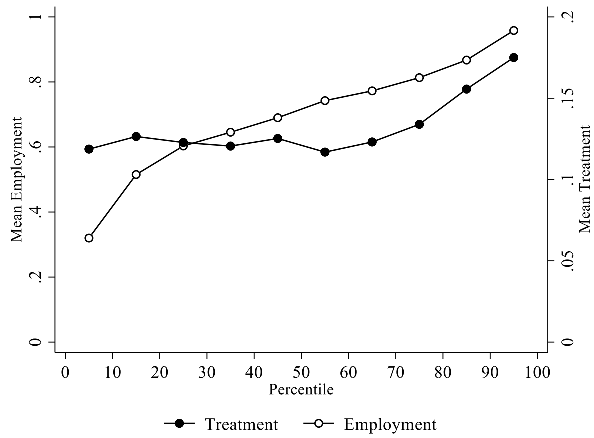
4. College



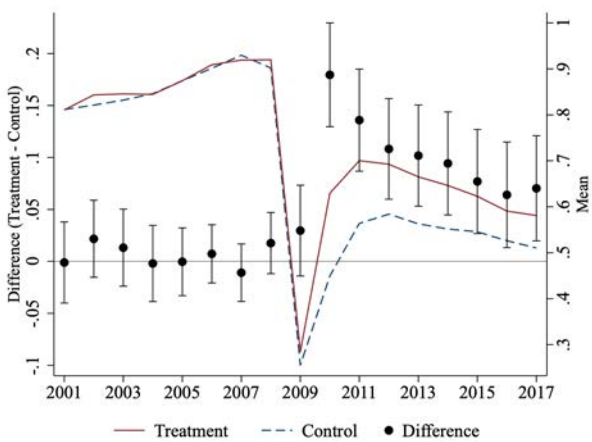
Notes: Black dots represent treatment-control differences in outcomes, with 95 percent confidence intervals shown in vertical bands. Controls include week of random assignment. Solid and dotted lines present outcome means for the treatment and control groups, respectively.

Appendix Figure G: Treatment Effects on W-2 Employment by Predicted 2010 Employment

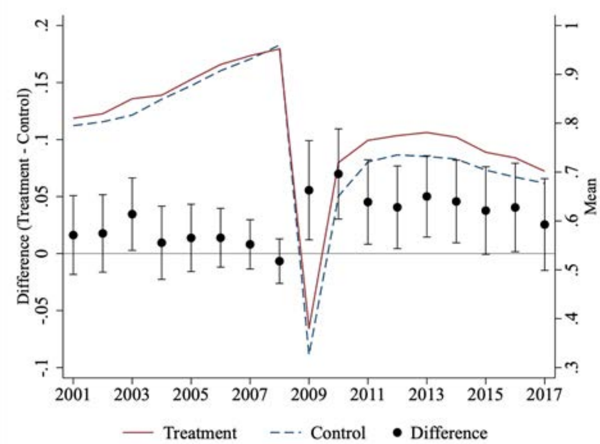
1. Mean Predicted 2010 Employment (left y-axis) and Fraction Treated (right y-axis) by Percentiles of Predicted 2010 Employment (x-axis)



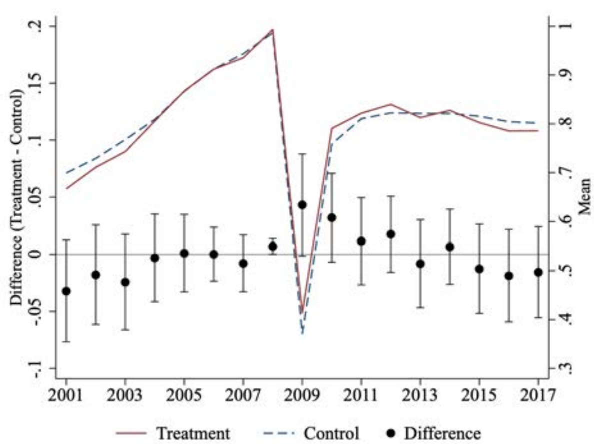
2. Treatment Effects for the Lowest Quartile of Predicted 2010 Employment



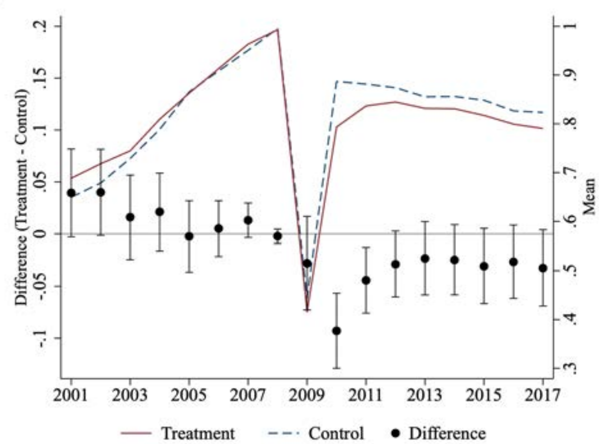
3. Treatment Effects for the 2nd Quartile of Predicted 2010 Employment



4. Treatment Effects for the 3rd Quartile of Predicted 2010 Employment



5. Treatment Effects for the Highest Quartile of Predicted 2010 Employment



Notes: Black dots represent treatment-control differences in outcomes, with 95 percent confidence intervals shown in vertical bands. Controls include week of random assignment. Solid and dotted lines present outcome means for the treatment and control groups, respectively.

Appendix Table A: Treatment Effects on W-2 Employment and W-2 Earnings

	Dependent Variable = W-2 Employment				Dependent Variable = W-2 Earnings Amounts			
	Unweighted		Weighted		Unweighted		Weighted	
	No Controls	With Controls	No Controls	With Controls	No Controls	With Controls	No Controls	With Controls
2001	0.014	0.006	0.002	0.003	$43	-$626	-$523	-$538
	(0.007)	(0.007)	(0.011)	(0.011)	(472)	(458)	(654)	(604)
2002	0.022	0.013	0.011	0.011	$70	-$599	-$49	-$64
	(0.006)	(0.007)	(0.010)	(0.010)	(528)	(519)	(703)	(654)
2003	0.017	0.008	0.007	0.008	$177	-$493	-$242	-$257
	(0.006)	(0.006)	(0.010)	(0.010)	(472)	(457)	(714)	(659)
2004	0.013	0.004	0.006	0.006	$663	-$7	$483	$468
	(0.006)	(0.006)	(0.009)	(0.009)	(515)	(499)	(761)	(704)
2005	0.014	0.005	0.004	0.004	$441	-$228	$415	$400
	(0.005)	(0.005)	(0.008)	(0.008)	(524)	(506)	(798)	(741)
2006	0.007	-0.001	0.006	0.007	$344	-$325	$598	$583
	(0.004)	(0.005)	(0.007)	(0.007)	(542)	(523)	(861)	(809)
2007	0.003	-0.005	0.000	0.000	$311	-$358	$366	$351
	(0.004)	(0.004)	(0.006)	(0.006)	(494)	(480)	(770)	(714)
2008	0.002	-0.006	0.003	0.004	$653	-$16	$860	$845
	(0.003)	(0.003)	(0.004)	(0.004)	(500)	(499)	(920)	(875)
2009	0.011	0.002	0.028	0.029	$746	$77	$759	$744
	(0.008)	(0.008)	(0.011)	(0.011)	(358)	(359)	(571)	(536)
2010	0.053	0.045	0.042	0.042	$1,999	$1,330	$2,273	$2,258
	(0.007)	(0.007)	(0.010)	(0.010)	(334)	(349)	(567)	(563)
2011	0.048	0.039	0.032	0.032	$2,050	$1,381	$2,010	$1,995
	(0.007)	(0.007)	(0.010)	(0.010)	(403)	(418)	(723)	(718)
2012	0.042	0.034	0.030	0.031	$2,556	$1,886	$3,296	$3,281
	(0.007)	(0.006)	(0.009)	(0.009)	(646)	(668)	(1464)	(1455)
2013	0.036	0.027	0.026	0.026	$2,262	$1,593	$1,806	$1,791
	(0.007)	(0.007)	(0.010)	(0.010)	(465)	(477)	(823)	(815)
2014	0.031	0.023	0.028	0.028	$2,283	$1,614	$2,082	$2,067
	(0.007)	(0.007)	(0.010)	(0.009)	(482)	(492)	(874)	(863)
2015	0.037	0.028	0.016	0.016	$2,750	$2,081	$2,473	$2,458
	(0.007)	(0.007)	(0.010)	(0.010)	(547)	(560)	(1054)	(1040)
2016	0.033	0.024	0.012	0.013	$3,314	$2,645	$3,297	$3,282
	(0.007)	(0.007)	(0.011)	(0.010)	(752)	(769)	(1653)	(1642)
2017	0.038	0.029	0.009	0.010	$2,813	$2,144	$1,642	$1,627
	(0.007)	(0.007)	(0.011)	(0.010)	(508)	(507)	(774)	(755)
R-squared	0.0918	0.1283	0.0850	0.1108	0.0249	0.0903	0.0232	0.0833
Observations - Treatment	88,768	88,768	88,768	88,768	88,768	88,768	88,768	88,768
Observations - Control	515,033	515,033	515,033	515,033	515,033	515,033	515,033	515,033

Notes: The table reports estimated regression coefficients on treatment plus treatment*1(year), with standard errors clustered on individual in parentheses below the corresponding estimates. All regressions include week of random assignment indicators. Specifications with controls also include (defined based on 2008 characteristics) indicators for age bin (<25, 25-34, 35-44, 45-54,55-64, >64), gender, age bin by gender, 2-digit NAIC, 3-digit zip code, filing status, SSA-1099 receipt, and 1099-MISC receipt.

Appendix Table B: Treatment Effects on UI & Disability

	Has UI Benefits	UI Benefits Amount	Has Social Security Disability Benefits	Social Security Disability Benefits Amount
2001	-0.005	-$40	-0.002	-$30
	(0.007)	(30)	(0.002)	(13)
2002	0.005	$42	-0.003	-$28
	(0.008)	(51)	(0.002)	(14)
2003	0.008	$9	-0.003	-$26
	(0.008)	(53)	(0.002)	(16)
2004	0.001	-$26	-0.003	-$23
	(0.007)	(35)	(0.002)	(17)
2005	-0.002	$21	-0.003	-$12
	(0.007)	(38)	(0.002)	(19)
2006	-0.004	$7	-0.003	-$6
	(0.007)	(33)	(0.001)	(20)
2007	0.000	$5	0.000	$23
	(0.002)	(14)	(0.001)	(24)
2008	0.000	-$57	0.001	$5
	(0.009)	(54)	(0.002)	(19)
2009	0.000	-$176	0.000	-$21
	(0.000)	(126)	(0.002)	(14)
2010	-0.035	-$808	-0.002	-$40
	(0.010)	(192)	(0.002)	(31)
2011	-0.009	-$32	-0.001	-$42
	(0.012)	(131)	(0.003)	(43)
2012	0.009	-$14	-0.004	-$95
	(0.010)	(79)	(0.003)	(52)
2013	0.013	$77	-0.004	-$73
	(0.010)	(67)	(0.003)	(75)
2014	0.002	$33	-0.003	-$23
	(0.008)	(44)	(0.004)	(84)
2015	0.002	$37	-0.001	-$15
	(0.007)	(40)	(0.004)	(87)
2016	-0.007	-$27	0.004	$100
	(0.003)	(16)	(0.005)	(117)
2017	0.002	$10	0.005	$70
	(0.007)	(35)	(0.006)	(100)
Observations - Treatment	88,768	88,768	88,768	88,768
Observations - Control	515,033	515,033	515,033	515,033

Notes: The table reports estimated regression coefficients on treatment plus treatment*1(year), with standard errors clustered on individual in parentheses below the corresponding estimates. All regressions include week of random assignment indicators.

Appendix Table C: Treatment Effects on Family, Tax, and Other Outcomes

	Has Contractor Income	Homeownership: Entire Sample	Homeownership: Homeowner in 2008	Homeownership: Not a Homeowner in 2008	Spousal Employment	Tax Filing	Net Tax Payment	After-Tax Income	Has Multiple W2s
2001	0.008	0.002	0.020	-0.002	0.000	0.000	-$14	-$13	0.000
	(0.005)	(0.010)	(0.019)	(0.007)	(0.011)	(0.008)	(179)	(491)	(0.011)
2002	-0.002	0.001	0.011	0.001	-0.001	-0.004	$17	$266	0.010
	(0.005)	(0.010)	(0.019)	(0.008)	(0.011)	(0.008)	(183)	(500)	(0.011)
2003	0.006	0.009	0.035	0.002	-0.007	-0.004	$4	$133	-0.003
	(0.006)	(0.011)	(0.017)	(0.008)	(0.011)	(0.008)	(181)	(520)	(0.011)
2004	0.001	0.004	0.019	0.002	-0.013	0.004	$212	$402	0.006
	(0.006)	(0.011)	(0.016)	(0.007)	(0.010)	(0.008)	(194)	(540)	(0.011)
2005	-0.007	0.003	0.027	-0.002	-0.010	0.004	$192	$515	-0.008
	(0.005)	(0.011)	(0.014)	(0.007)	(0.011)	(0.007)	(201)	(548)	(0.011)
2006	0.001	-0.003	0.020	-0.006	-0.009	-0.003	$238	$696	-0.002
	(0.006)	(0.011)	(0.012)	(0.006)	(0.011)	(0.006)	(209)	(564)	(0.012)
2007	0.004	-0.004	0.015	-0.004	-0.009	0.004	$270	$584	-0.005
	(0.006)	(0.011)	(0.008)	(0.005)	(0.011)	(0.005)	(199)	(545)	(0.012)
2008	0.003	-0.007	0.000	0.000	-0.007	0.000	$265	$311	-0.016
	(0.006)	(0.011)	(0.000)	(0.000)	(0.011)	(0.004)	(200)	(497)	(0.011)
2009	0.001	0.004	0.023	0.003	-0.005	0.015	$210	$496	0.028
	(0.006)	(0.011)	(0.010)	(0.005)	(0.010)	(0.004)	(183)	(428)	(0.011)
2010	-0.005	0.008	0.031	0.004	0.001	0.012	$318	$776	0.016
	(0.006)	(0.011)	(0.014)	(0.006)	(0.011)	(0.007)	(162)	(365)	(0.011)
2011	0.006	0.019	0.052	0.008	-0.004	0.017	$421	$1,024	0.003
	(0.007)	(0.011)	(0.016)	(0.007)	(0.010)	(0.007)	(178)	(403)	(0.011)
2012	-0.001	0.021	0.048	0.013	-0.004	0.008	$529	$1,168	0.005
	(0.007)	(0.011)	(0.018)	(0.007)	(0.010)	(0.008)	(187)	(450)	(0.011)
2013	0.007	0.020	0.049	0.010	-0.006	0.027	$651	$788	0.004
	(0.007)	(0.011)	(0.018)	(0.008)	(0.010)	(0.008)	(186)	(451)	(0.010)
2014	0.005	0.015	0.041	0.007	0.000	0.019	$618	$1,028	-0.004
	(0.007)	(0.011)	(0.019)	(0.009)	(0.010)	(0.008)	(198)	(478)	(0.011)
2015	0.005	0.012	0.026	0.010	0.000	0.026	$694	$991	-0.004
	(0.008)	(0.011)	(0.019)	(0.010)	(0.010)	(0.009)	(202)	(526)	(0.010)
2016	0.006	0.018	0.041	0.010	0.000	0.028	$647	$1,338	-0.003
	(0.007)	(0.011)	(0.019)	(0.010)	(0.010)	(0.009)	(216)	(520)	(0.010)
2017	0.005	0.017	0.027	0.016	-0.001	0.019	$680	$1,110	0.005
	(0.007)	(0.011)	(0.019)	(0.011)	(0.010)	(0.010)	(219)	(524)	(0.010)
Observations - Treatment	88,768	88,768	30,569	58,199	88,768	88,768	88,768	88,768	88,768
Observations - Control	515,033	515,033	181,475	333,558	515,033	515,033	515,033	515,033	515,033

Notes: The table reports estimated regression coefficients on treatment plus treatment*1(year), with standard errors clustered on individual in parentheses below the corresponding estimates. All regressions include week of random assignment

Appendix Table D: Cross-Sectional Results

Years of Employment 2010-2017			Ever has W-2 Employment			Average After-Tax Income (2010-2017)		
	Control Group Mean	Treatment Effect		Control Group Mean	Treatment Effect		Control Group Mean	Treatment Effect
Years of Employment = 0	0.074	-0.005	In 2010	0.700	0.041	$0-$10000	0.183	-0.012
		(0.006)			(0.011)			(0.009)
Years of Employment = 1	0.036	-0.010	In 2010-2011	0.826	0.017	$10001-$20000	0.244	-0.011
		(0.004)			(0.009)			(0.010)
Years of Employment = 2	0.035	-0.002	In 2010-2012	0.868	0.020	$20001-$30000	0.254	-0.007
		(0.004)			(0.007)			(0.010)
Years of Employment = 3	0.036	-0.003	In 2010-2013	0.888	0.021	$30001-$40000	0.153	0.015
		(0.005)			(0.007)			(0.009)
Years of Employment = 4	0.035	-0.004	In 2010-2014	0.900	0.017	$40001-$50000	0.072	0.005
		(0.004)			(0.007)			(0.006)
Years of Employment = 5	0.041	-0.002	In 2010-2015	0.910	0.013	$50001-$60000	0.036	0.002
		(0.004)			(0.006)			(0.004)
Years of Employment = 6	0.045	0.004	In 2010-2016	0.916	0.011	$60001-$70000	0.018	0.008
		(0.005)			(0.006)			(0.004)
Years of Employment = 7	0.055	0.001	In 2010-2017	0.920	0.008	$70001-$80000	0.010	0.001
		(0.005)			(0.006)			(0.002)
Years of Employment = 8	0.082	-0.003				$80001-$90000	0.005	-0.001
		(0.006)						(0.001)
						> $90000	0.002	0.002
								(0.001)

Notes: The table reports estimated regression coefficients with standard errors clustered on individual in parentheses below the corresponding estimates. Each row corresponds to estimates from a separate cross-sectional regression in which the dependent variable is regressed on a treatment indicator variable and additional controls. Each regression includes 4672 treatment observations and 27107 control observations. All regressions include week of random assignment indicators.