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LONG-TERM EFFECTS OF JOB-SEARCH ASSISTANCE: EXPERIMENTAL EVIDENCE USING ADMINISTRATIVE TAX DATA

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Long-Term Effects of Job-Search Assistance: Experimental Evidence Using Administrative Tax Data
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ABSTRACT

This paper uses administrative tax data to examine the long-term effects of an experimental job-search assistance program operating in Nevada in 2009. The program required randomly-selected unemployed workers who had just started collecting unemployment insurance (UI) benefits to undergo an eligibility review and receive personalized job-counseling services. The program led to substantial short-term reductions in UI receipt, and to persistent, long-term increases in employment and earnings. The program also affected participants' family outcomes, including total income, tax filing, tax liability, and home ownership. These findings show that job-search assistance programs may produce substantial long-term effects for participants and their families.

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I. Introduction

Unemployment insurance (UI) programs typically require unemployed workers to actively search for employment while collecting benefits. Over recent decades, job-search assistance programs which are intended to help UI recipients meet these requirements have become common in the United States and other developed countries. These programs typically involve monitoring activities to ensure that UI recipients are conducting an active job-search and provision of job-search assistance services to connect them with available jobs (Meyer, 1995; Wandner, 2010; Card *et al.*, 2010; Kahn, 2012). These requirements are expected to reduce UI recipients' negative incentives (moral hazard) and to improve the effectiveness of their job search. Job-search assistance programs have become a larger part of the menu of government-subsidized employment and training programs, accounting for more than one-half of one percent of the government budget and serving nearly one in every twenty workers in OECD countries (Wandner and Eberts, 2014; Crepon and van den Berg, 2016).

Understanding the long-term effects of job-search assistance programs is important for policymakers and academic researchers.[1] However, because of data limitations and other factors, most prior studies (which we discuss in more detail below) have focused on the programs' short-term impacts on unemployment duration, employment, and earnings, and provide limited evidence on long-term effects. From a policymaker perspective, it is important to consider the long-term impacts of these programs on participants' earnings and tax outcomes when measuring their cost-effectiveness. In particular, it is useful to consider whether these programs create

[1] In addition to the motivation described here, we note that this paper is also relevant for tax policy. In particular, this paper is derived from an original project titled "The Effects of Employment and Earnings on Tax Filing and Tax Liability: Evidence on Short-term and Long-term Effects Using Administrative Tax Data." Since forecasting tax filing and tax liabilities are central components of tax policy, this project aims to understand how changes in employment and earnings affect changes in tax filing and tax liabilities. This project exploits exogenous variation in employment and earnings to estimate the causal effects of changes in employment and earnings on tax filing and tax liabilities.

permanent increases in employment and earnings, and hence, increase state and federal income tax bases. From an academic research perspective, it is important to establish whether job-search assistance helps unemployed workers overcome barriers to reemployment (fixed costs) thereby producing persistent increases in employment and earnings. Alternatively, the programs may have transitory effects on employment, earnings, and income tax bases if their only impact is to help treated individuals to find jobs faster than they would have in the absence of the programs.

This paper examines the long-term effects of an intervention implemented by the state of Nevada in the second half of 2009. The intervention provided UI recipients with personalized job-search assistance at the beginning of their UI spells. The Nevada program examined here is a compelling case study because it used random assignment to determine which UI recipients would be subject to program requirements (treatment group) and which would be exempted from the program (control group). The program required individuals assigned to the treatment group to meet 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. Those who failed to show up for the review and those who were identified during the review as not conducting an active job-search, as required by state UI laws, were disqualified from collecting UI benefits. Those deemed eligible during the review, received personalized job-counseling services during the same meeting, which focused on assessing their occupational skills, helping them develop a job-search plan, providing resume development assistance, and providing direct referrals to local employers with relevant jobs.

The empirical analyses use 2001-2015 administrative tax records for all UI recipients who were subject to random assignment for participation in the program (treatment and control groups) during the study period. The data allow us to construct measures of individual earnings, UI receipt, and other outcomes based on third-party reported tax documents. In addition, we measure tax filing and other family-level outcomes, such as tax liability, total income and home ownership, based on filed tax returns. This information allows us to examine the program's effects on participants' individual and family outcomes for a six-year period after entry into the program.

Results indicate that the treatment had significant, long-term effects on wage employment and earnings. The program increased the employment rates of participants in the year after the intervention by about 8 percent relative to the control group, an effect that declined somewhat over time, but remained in the 4-7 percent range over the subsequent five years. As a result, the program increased participants' earnings in the range of 11 to 14 percent over the entire six-year follow-up period. These persistent, long-term effects suggest that the program increased employment among individuals who would not have found jobs in the absence of the intervention, as opposed to getting individuals who would have ultimately found jobs in the years following the intervention to find them earlier. The program also led to a substantial reduction in UI benefits collected in the two-year period after the intervention, when benefits were still available under the claim associated with assignment into the program. In contrast, there is limited evidence that the program affected self-employment rates or receipt of disability benefits.

Results based on filed tax returns show that, in addition to affecting individual employment and earnings, the program affected family-level outcomes. Treated individuals were more likely to file tax returns and had higher total family income and tax liability relative to the control group. These results are consistent with the program's effects on employment and earnings, but also indicate that the treatment may have had some positive spillover effects on spousal employment and earnings. Additionally, we find that individuals in the control group experienced a steeper decline in home ownership rates in the three-year period following the intervention than did treatment cases. As a result, treated individuals had significantly higher home ownership rates than control individuals following the intervention. Treated individuals may have been less likely to sell their homes or default their mortgages as a result of increased employment and earnings.

The 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 our methodology for estimating program effects and the results of our analyses. Section IV compares our findings with those of previous studies of U.S. job-search assistance programs, and discusses the possible mechanisms behind the effects. Section V summarizes our findings and conclusions.

II. Background and Data

A. The Nevada REA Program

There are two primary job-search assistance programs operating in the United States – the Worker Profiling and Reemployment Services (WPRS) and the Reemployment and Eligibility Assessment (REA) programs. Both programs target unemployed workers who start collecting UI benefits, excluding those on temporary layoff, active in employment or training programs, and attached to union hiring halls. The WPRS program, established in 1993, refers UI recipients to public employment offices to obtain information about available job-search services and referrals to specific services that would aid their job-search (Dickinson *et al.,* 1999; Wandner, 2010). The program's objective is to help participants find jobs quickly and produce savings for the UI program. WPRS is federally-mandated and has been operational in all 50 states since

1996.

REA was established in 2005 by the U.S. Department of Labor (DOL) to conduct in-person eligibility reviews to confirm that UI recipients who are not served by the WPRS program are actively searching for a job, as mandated by state UI laws (Benus *et al.*, 2008). REA's objective is not to provide job-search assistance to UI recipients, but rather to reduce UI fraud by disqualifying those who are not conducting an active job-search (Poe-Yamagata *et al.,* 2012). While WPRS is federally mandated, REA is voluntary, with DOL providing annual grants to encourage states to implement the program. In 2009, which includes the period of our study, all 50 states were implementing WPRS while REA was fully operational in only nine states, including Nevada. Since then, REA has expanded dramatically, and currently 33 states operate the program.

The approach that Nevada used to implement the REA program differed from the approaches of other states that operated both WPRS and REA. Instead of referring UI recipients not served by WPRS to eligibility reviews, Nevada essentially combined WPRS with REA, creating an REA program that required participants to participate in both the eligibility review and job-search services. Specifically, the Nevada REA program required participants to meet with program staff in the first few weeks of their UI spell to undergo an eligibility review and, if deemed eligible, to receive mandatory job-counseling services. The Nevada REA program operated in the workforce regions covering the Las Vegas-Henderson-Paradise and Reno metropolitan areas, while WPRS operated in the rest of the state.[2]

The Nevada REA program operated as follows. Each week, the Nevada UI agency randomly

[2] The two metropolitan areas where REA operated, covered about 87 percent of unemployed workers in the state in 2009 (source: authors' tabulations of the 2009 American Community Survey).

assigned REA-eligible recipients into the treatment or control group.[3] A notification letter was sent out to treatment 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 services. The letter informed participants that failure to show up for the meeting would result in loss of UI benefits. Individuals assigned to the control group 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.

Individuals in the treatment group who did not show up for the meeting without a reasonable justification were immediately disqualified from collecting additional UI benefits.[4] Participants who showed up for the meeting and were deemed to be noncompliant with UI work-search requirements were also disqualified. Those who passed the review, were offered job-counseling services 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. These services were offered as needed, and not all participants received all services. At the end of the meeting, participants were informed that, although they were not required to receive additional services or attend follow-up meetings, they were expected to continue actively searching for employment, as mandated by state UI laws. Poe-Yamagata *et al.* (2012) report that dividing the total funding used to administer REA by the number of treated individuals in 2009, yields a $201 average estimated cost per treated individual.

[3] The proportion of REA-eligible UI recipients assigned to the treatment varied slightly on a weekly basis according to the capacity of public employment offices to provide services in participants. Generally, each week, about 15 percent of eligible recipients were assigned to the treatment and 85 percent to the control group.

[4] Participants who had found employment, participated in job-search or training services, or discontinued benefit receipt were exempted.

B. Prior Evidence

A number of experimental studies have examined the effects of U.S. job-search assistance programs, including the Nevada REA program examined here. Previous work focusing on the United States has relied on participant data provided by state UI agencies. These data include: (1) UI claims records, which report the number of weeks and UI benefit amounts collected under the UI claim associated with program assignment; and (2) quarterly wage records, which report quarterly earnings from private-sector employers in the program state. Based on available data, previous work has generally focused on whether job-search assistance programs reduced the duration of participants' current UI spells and increased participants' short-term employment and earnings. In most cases, program effects on these outcomes were estimated for up to six quarters after the intervention, and thus evidence on the program's long-term effects is limited.

Meyer (1995) reported that programs targeting UI recipients in South Carolina, New Jersey, and Washington during the 1980s reduced average UI spells, but had substantively small effects on short-term employment and earnings. Studies of programs that operated in the mid-1990s showed that requiring UI recipients to receive job-search assistance services reduced UI duration and benefit amounts collected (Decker *et al.,* 2000; Klepinger *et al.,* 2002; Black *et al.,* 2003). This work provided mixed evidence about whether these programs affected participants' earnings.

Prior studies have also established that the Nevada REA program was effective in improving the short-term outcomes of UI recipients (Michaelides and Mueser, 2017; 2018). These studies used Nevada UI data on all unemployed workers who started collecting UI benefits in the second half of 2009, and who were subject to random assignment for participation in the program.[5]

[5] During that period, the Nevada unemployment rate averaged over 12 percent, peaking at nearly 14 percent in 2010, the highest in the state in 25 years. As a result, UI recipients were eligible for up to 99 weeks of UI benefits: 12-26

Both studies found that the program led to large reductions in UI spells and benefit amounts collected and to substantial increases in employment and earnings. Michaelides and Mueser (2017) compared the results of the Nevada REA program with the results of the WPRS program in Florida and the REA programs in Florida and Idaho. Results showed that all four programs reduced UI duration and increased earnings, but that the Nevada REA program was by far the most effective.

There are also numerous experimental studies of job-search assistance programs operating in many European countries, including Belgium (Cockx *et al.,* 2017), Denmark (Graversen and van Ours, 2008; Maibom *et al.,* 2017), France (Behaghel *et al.*, 2012), Germany (Krug and Stephan, 2013), the Netherlands (Gorter and Kalb, 1996), Sweden (Hägglund, 2011), and the United Kingdom (Dolton and O'Neill, 2002). The European literature is nicely reviewed by Card *et al.* (2010), Kahn (2012), and Crepon and van der Berg (2016). The programs studied by this work have typically involved more intensive requirements than U.S. programs, requiring participants to engage in job-search monitoring and job-counseling activities throughout their UI spells. By comparison, U.S. programs typically require participants to engage in such activities only once, at the beginning of their UI spells, with no requirements thereafter. The European studies have found that job-search assistance programs are often very effective in increasing unemployment exits and reemployment rates. However, lack of data did not allow in most cases examination of effects on individual earnings or tax outcomes.

C. Data

Our study sample consists of all individuals who started collecting UI from July through

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weeks under the regular UI program, 26-53 weeks under the Emergency Unemployment Compensation (EUC) program, and 10-20 weeks under the Extended Benefits (EB) program.

December 2009 and were subject to random assignment for participation in the program. The total sample size is 32,751 individuals, from which 4,673 individuals were assigned to the treatment and the remaining to the control group.[6] The analysis is based on two datasets: (1) Nevada REA program data, provided by the Nevada Department of Employment, Training, and Rehabilitation, and (2) administrative tax data, collected by the United States Internal Revenue Service. The Nevada REA program data provide the following information for individuals in the study sample: treatment status, birth date, gender, five-digit zip code, and quarterly earnings from private-sector employers in Nevada from 2008 through 2011.

Our empirical analyses are based on identifying treatment and control group individuals from the Nevada REA program in the administrative tax data. This identification is done using birth date, gender, zip code, and annual wage earnings in 2009. These variables allow us to match each individual in the Nevada REA sample to a unique individual in the administrative tax data. The matching procedure is as follows. First, we use the administrative tax data to identify all individuals who received UI benefits (i.e., had a 1099G tax form) from Nevada in 2009. For this pool of individuals, we use the tax data to observe their date of birth, gender, zip codes, and annual earnings. Then, we match individuals from the Nevada REA sample to the individuals in the administrative tax data pool based on birth date, gender, zip code, and annual earnings.

Using this process, we are able to match all treatment and control group individuals from the Nevada REA sample to unique individuals in the administrative tax data. To assess the validity of the match, we compare short-term differences in the likelihood of employment (i.e., having

[6] Note that this is the same sample used by Michaelides and Mueser (2018). As reported in that study, 956 (20 percent) of the 4,673 treatment cases did not undergo the review; of these, 34 were disqualified for failure to undergo the review and the remaining 922 were exempted because they had already found a job or they had already received job-search or training services. The remaining 3,717 (80 percent) treatment cases underwent the review; 34 of these were disqualified during the review because they were not conducting an active job search. Our study sample includes all 4,673 treatment cases, regardless of whether they underwent the review, were disqualified, or were exempted. Similarly, our study sample includes all individuals who were assigned to the control group.

positive W-2 wage income) and earnings amounts based on both the Nevada REA program data and the administrative tax data. These results are presented in Appendix Figure 1. Overall, we find that short-term treatment-control differences in earnings from the Nevada REA program data appear to be consistent with differences in the administrative tax data. We conclude that the matching appears valid and proceed with the empirical analyses of the panel data constructed from the administrative tax data.

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 treatment-control differences based on characteristics available in the tax data. As seen, treatment and control cases were generally similar, with few substantive differences, which could be attributable to chance. Moreover, Appendix Table 1 presents results from a regression model that estimates the likelihood of treatment assignment based on pre-treatment covariates (reported in 2008) from the administrative tax data. Overall, the pre-treatment covariates do not consistently predict treatment assignment. As we discuss below, we confirm that the estimated treatment effects are robust to controlling for pre-treatment characteristics and outcomes.

Lastly, we create a panel dataset based on administrative tax records for years 2001 through 2015. The administrative tax records include information reported by third parties, such as W-2 earnings and 1099-MISC tax forms,[7] and information from filed tax returns. Using the third-party-reported information, we measure W-2 employment (whether an individual had W-2 earnings) and W-2 earnings amounts (as reported on Form W-2). We also measure whether

[7] 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. Similarly, IRS Forms 1099-G and 1099-SSA record unemployment insurance and disability payments paid from government agencies to individuals, and these forms are also reported to the IRS by state agencies and the Social Security Administration respectively.

individuals had contractor employment (based on 1099-MISC forms), whether they collected UI benefits (based on 1099-G forms), and whether they collected disability benefits (based on 1099-SSA forms). For each of these income sources, we also observe amounts paid. Additionally, home ownership is observed based on third-party reported mortgage interest statements. Specifically, for each individual who owns a home and has mortgage payment 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. Moreover, using filed tax returns, we measure additional outcomes at the family-level, including whether the individual filed or was claimed on a tax return, spousal earnings, self-employment income, total family income, tax liability, and tax balance due. We note that, in contrast to outcomes based on third-party reported information documents, outcomes based on filed tax returns are self-reported by individuals who file tax returns.

III. Empirical Analyses

A. Regression Specification

Because individuals were randomly assigned to treatment and control groups, we are able to estimate causal effects on participants' outcomes using a standard Ordinary Least Squares (OLS) regression specification. Using y_{it} to denote an outcome of interest for individual i in year t, we estimate treatment effects based on the following regression specification:

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

In this specification, the estimated coefficients β_k capture differences between the treatment and control groups in each year. The analyses focus on differences between the treatment and control groups in the years prior to random assignment into the program, as well as the short-term and long-term differences between the treatment and control groups after random assignment.

Examining whether there were any pre-existing differences between the treatment and control groups provides a check to assess the validity of the random assignment. Treatment-control differences after random assignment estimate the program's average intent-to-treat treatment effects on outcome variables. This regression specification is estimated using panel data on multiple outcome variables, as discussed below.[8]

B. Results

Figure 1 presents the first set of results for W-2 employment outcomes, based on third-party reported tax forms. *W-2 employment* indicates whether the individual had a W-2 form with earnings from an employer, and *W-2 earnings* measures the total amount of W-2 earnings from all employers.[9] Each plot presents the estimated treatment effects for each year and the corresponding 95 percent confidence intervals (corresponding to the left vertical axis). The plots also present the means of the outcome variable in each year for the treatment and control groups (corresponding to the right vertical axis), facilitating a comparison of the treatment effects to the relevant baseline means of the outcome variable.

Results in Figure 1A indicate that the program increased the likelihood of employment in 2010, the first year after program entry, by 5.3 percentage points, compared with a base employment rate of 68.4 percent for the control group. Treatment effects on employment faded slightly in subsequent years, ranging from 3.2 to 4.8 percentage points. The graphical evidence indicates that this fade-out does not appear to be driven by a declining employment rate for the treatment group. Instead, the employment rate for the control group appears to increase slightly more over time relative to the rate for the treatment group, indicating that some individuals in the control group who did not find jobs in the first year after the treatment may have ultimately

[8] Appendix Tables 2A-E present the means of selected outcomes for treatment and control groups, and treatment-control differences in the mean values.
[9] Individuals with no W-2 form are included with values of zero for both employment and earnings amounts.

found jobs in subsequent years. Nevertheless, program effects on wage employment from 2011 through 2015 were positive and statistically significant, and do not appear to be statistically different from the 2010 short-term effect. These results indicate that the program increased employment rates in the entire six-year follow-up period. More specifically, the results indicate that the employment rate for individuals in the control group did not fully catch up to the corresponding measures in the treatment group. Thus, the program appears to have increased employment among individuals who would not have found jobs in the absence of the treatment, as opposed to getting individuals who would have ultimately found jobs to find them earlier.

Figure 1B illustrates that the program had positive and statistically significant treatment effects on W-2 earnings in the six-year follow-up period. The program increased W-2 earnings in 2010 (the year following program entry) by $2,076, compared with a base of $14,320 for the control group. This effect appears persistent across the subsequent years, ranging from about $2,369 to about $2,800, which represents an 11 to 13 percent improvement over the control group mean.[10] In addition to these outcomes, we also examined whether individuals in the treatment group were more likely to move from their ZIP codes in 2008 (i.e. their ZIP codes at the times of the treatment) following the intervention. We did not find any statistically significant differences between the rates of moving for the treatment and control groups, so it appears that the treatment increased W-2 employment and earnings within individuals' initial local labor markets.

[10] We have also examined W-2 earnings conditional on employment. Appendix Figure 2 presents these results. The results highlight that even beyond increasing employment, the intervention may have increased earnings conditional on being employed. Specifically, average earnings conditional on employment appear to be roughly $1,500 higher for the treatment group relative to the control group. Note that mean earnings conditional on employment for the control group is roughly $30,000. These results suggest that the program may have helped participants find better (higher-paying) jobs or that it may have been particularly effective at getting high-wage individuals to find jobs. Similar to effects on employment, the effects of the intervention on earnings conditional on employment remain persistent for up to six years after the intervention. However, we also acknowledge that these results may provide biased estimates of the program's impacts on conditional earnings because there could be unobserved differences between treatment group individuals who became employed and control group individuals who became employed.

Figure 2 presents program effects in other forms of annual employment and income based on third-party reports. *Contractor employment* indicates whether the individual had a contractor 1099-MISC form. *Receipt of UI benefits* indicates whether the individual collected UI benefits, and *amount of UI benefits* measures the amount of UI benefits collected during the year. *Receipt of disability benefits* indicates whether the individual collected disability benefits, and *broad individual income* is the total amount of individual income based on third-party reports, measured as the sum of earnings, contractor income, UI benefits, and disability benefits.

Figure 2A provides little evidence of substantive treatment-control differences in contractor employment following the intervention. This result indicates that control group individuals did not engage in self-employment activities to compensate for lost W-2 earnings at a higher rate than did the treatment group. Figure 2B highlights that treatment cases were less likely than control cases to have collected UI in 2010 and 2011, the period following the intervention, when regular UI, EUC, and EB benefits under the original claim were still available. Expectedly, UI benefit receipt declined for both groups after this period, possibly because benefit durations had expired for most individuals and many of them did not qualify for subsequent UI claims. Although treatment cases remained less likely than control cases to collect benefits, differences lack statistical significance. Figure 2C shows that the program significantly reduced UI benefit amounts collected from 2009 through 2011, the period when benefits were available under the original claim, although only the 2010 effect was statistically significant. Overall, the program reduced UI benefits collected a total of $818 from 2009 through 2011. The program did not affect UI receipt outcomes in 2012 or later. These results indicate that the program's effect on UI receipt was short-lived and that, in fact, treatment cases were at least as likely as control cases to return to UI in subsequent years. Figure 2D indicates that, while there were increases in the likelihoods of receiving disability benefits for both the treatment and control groups, there were no statistically significant differences in the likelihoods of collecting disability benefits between the groups after the intervention.

The broad individual income variable allows us to examine program effects in the total of all these income measures (see Figure 2E). Results are similar with the W-2 earnings results (Figure 1B), in that we get positive effects in the entire follow-up period. The smallest effect on broad income – and the only one that lacks statistical significance – is observed in 2010, when the substantial positive effect on earnings was partly offset by the large reduction in UI benefit amounts collected. In each year after 2010, the intervention had large and statistically significant effects on broad income, in the $2,000-2,500 range. The magnitude of the effects on broad income in this period largely correspond to the program's effects on W-2 earnings (see Figure 1B).

Our analyses now turn to examining program effects on outcomes based on filed tax returns, so that we can assess whether the program affected outcomes at the family (tax-filing unit) level. Figure 3 presents estimated effects on indicator variables based on filed tax returns: *file tax return* indicates whether the individual filed a tax return or was included on a filed tax return; *spousal W-2 employment* indicates whether the individual had a spouse with positive W-2 income on a filed tax return; *has self-employment income* indicates whether the individual had income from self-employment reported on a filed tax return; and *home ownership* indicates whether an individual or spouse (on the filed tax return) received a 1098 Mortgage Interest statement indicating home ownership.

Figure 3A shows that the treatment group had a slightly higher likelihood of filing a tax return in the years immediately following the intervention, with large and statistically significant effects in 2013, 2014, and 2015. These results suggest that, perhaps due to the program's effect on employment and earnings, treated individuals were more likely to file tax returns than control individuals. Figure 3B shows a noticeable increase over time in treatment-control differences in the likelihood of having a spouse with W-2 earnings. Program effects are not statistically significant, except for 2015, when the 3.7 percentage-point effect is significant at the 10 percent level. These results provide some evidence that the program may have had some positive spillovers on spouses of treated individuals. Figure 3C shows no statistically or economically significant impacts on the likelihood of reporting self-employment income on filed tax returns. Importantly, it does not appear that individuals in the control group were more likely to turn to self-employment to offset reductions in wage earnings relative to the control group.

Figure 3D indicates that the program may have had some positive impacts on home ownership through increased employment and earnings. In particular, after the intervention, home ownership rates declined more sharply for the control group than the treatment group. Intuitively, individuals in the treatment group may have been less likely to sell their homes than individuals in the control group because they were more likely to find employment and hence be able to continue paying their mortgages. This evidence is consistent with recent evidence of impacts of UI on housing markets (see Hsu *et al.*, 2018).

Building on the above analyses, Figure 4 presents results using amounts from filed tax returns, including individual W-2 earnings, spousal W-2 earnings, total family income, tax liability, and the tax balance due. Because outcomes from tax returns are only available for individuals who filed tax returns, for each of these outcomes, we use values based on filed tax returns for tax filers, and we use values based on third-party reported information for non-filers. Specifically, for W-2 earnings, we use earnings amounts reported on IRS Form 1040 for filers and total individual W-2 wages for non-filers; for total family income, we use total income reported on IRS Form 1040 for filers and total broad income for non-filers; for tax liability, we use total tax liability before credits on IRS Form 1040 for filers and total W-2 withholdings for non-filers; for tax balance due, we use the balance due (or refund if negative) on IRS Form 1040 for filers and 0 for non-filers.

Results show that individuals in the treatment group had higher W-2 earnings than individuals in the control group over the entire follow-up period. Spousal earnings also appear to be higher for the treatment group, although effects are significant only in years 5-6 after the intervention and at the 10 percent level. Figure 4C indicates that treated individuals had higher total family income than control individuals following the treatment. In particular, the treatment effects on family income are larger than the treatment effects on individual earnings because of the positive effects on spousal income. Quantitatively, the results indicate that the treatment-control difference in total family income increased from roughly zero in 2009 to roughly $4,400 in 2015. Finally, Figure 4D shows that treatment cases had higher tax liability than control cases in the entire follow-up period, particularly in years 4-6, when effects are statistically significant.

C. Robustness Checks

In some cases, there are differences in outcomes between the treatment and control groups in the period prior to the intervention. For example, treatment cases were more likely than control cases to have positive W-2 earnings from 2002 through 2005 (see Figure 1A). Similarly, there are some treatment-control differences prior to the intervention in receipt of UI benefits, receipt of disability benefits, home ownership, and spousal W-2 earnings. Because of these differences, we verify that estimated effects on each outcome after the intervention (2010 and later) are robust to controlling for individual characteristics and outcomes prior to the intervention (before 2009).

Specifically, for each outcome, we estimated differences between the treatment and control group in each year with no controls for pre-treatment characteristics or pre-treatment outcomes, with controls for pre-treatment characteristics in 2008, and with controls for pre-treatment characteristics and pre-treatment outcomes in 2001-2008. Table 2 presents results for four selected outcomes: W-2 employment, W-2 earnings, broad individual income, and total family income. Results show that estimated effects are all robust to a variety of controls. Similar results are obtained for other individual and household outcomes – results are available upon request. These checks add credibility to interpreting the post-intervention differences between the treatment and control groups as causal effects of the program.

IV. Discussion

To put the current results in context, we compare them with the results from earlier studies of the Nevada REA program and from experimental studies of other job-search assistance programs in the United States. The estimates from earlier studies are summarized in Table 3. Starting with comparisons to earlier studies of the Nevada REA program (Table 3A), Michaelides and Mueser (2017 and 2018) found that, in the first four quarters after program entry, the program increased earnings by $1,854 and $1,740, respectively. The results in the present study indicate effects on earnings that were higher at $2,076 in 2010 (a period which roughly corresponds to the four-quarter follow-up period of the earlier studies). Michaelides and Mueser (2018) also report earnings effects of $752 in quarters 5 and 6 after entry, which is consistent with our finding that the program increased earnings by $2,127 in 2011 (a period which roughly corresponds to quarters 5-8 after entry of the earlier studies). These comparisons show that the results of the two earlier studies are within the 95 percent confidence intervals of estimates from the present study.[11]

Estimated effects on UI receipt are similar between the current study and earlier studies of the Nevada REA program. Previous studies showed that the Nevada REA program reduced UI benefits collected under the claim associated with random assignment into the program by about $1,145 and $976. Estimates from the current study indicate that effects on UI benefits were lower ($818) based on the total differences in UI benefit amounts from 2009 through 2011.

Tables 3B and 3C presents estimated impacts from other experimental job-search assistance programs that operated in the 1980s and 1990s respectively. Comparing the current estimates to estimates from programs examined by these earlier studies highlights that the Nevada REA program appears to have been more effective. As shown in Table 3B, Meyer (1995) reported that programs targeting UI recipients in South Carolina, New Jersey, and Washington during the 1980s reduced benefit amounts collected by $68 to $150. These programs, however, did not have any impacts on earnings in the year after program entry (all programs) or two to three years after entry (New Jersey program).[12] In percentage terms, the estimated reductions in UI benefit amounts collected for these earlier programs are comparable to those for the Nevada REA program, but earnings effects for the Nevada REA program are substantially larger and more persistent.

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[11] Disparities between the present estimates and the prior estimates may be due to a variety of factors. For example, prior estimates did not account for out of state employment or employment with public employers. Furthermore, some individuals who receive W-2s may not be covered by UI; the current estimates are based on W-2s whereas the prior estimates are based on UI data.

[12] These three demonstration programs were implemented by DOL. The same study reports the results of programs in Nevada and Wisconsin, which were designed and evaluated by state UI agencies. Results show that the Nevada and Wisconsin programs reduced UI duration by 31 and 4 percent, respectively, but provide no analyses of earnings effects. However, the author notes that the "DOL experiments are more carefully designed, implemented and evaluated than the state experiments. Thus, their results should be accepted with greater confidence."

Table 3C summarizes results from programs that operated in the mid-1990s and required UI recipients to receive job-search assistance services (Decker *et al.,* 2000; Klepinger *et al.,* 2002; Black *et al.,* 2003). All four programs examined by this work led to significant reductions in UI benefit amounts collected. But this work found mixed evidence about whether these programs affected participants' earnings. Decker *et al.* (2000) found that the job-search assistance demonstration program in Washington, DC improved earnings by about 10 percent, while the Florida program had no earnings effects. Black et al (2003) found that the Kentucky WPRS program had positive but short-lived effects on earnings and Klepinger *et al.,* (2002) found that the Maryland work-search demonstration program had no effects on earnings. Overall, the effects of the Nevada REA program on UI benefits collected are larger than the effects of programs that operated in the mid-1990s. Also, with the partial exception of the Washington, DC JSA program, the effects of the Nevada REA program on earnings are generally larger and more persistent than the effects of these other programs.

There are a variety of factors that may explain the Nevada REA program's large and persistent effects on employment and earnings, and why the program was more effective than other U.S. job-search assistance programs. First, the Nevada REA program may have involved a more intensive treatment than programs examined by prior experimental studies. Michaelides and Mueser (2018) report that more than 68 percent of Nevada REA participants received job-counseling services, with 56 percent receiving a skills assessment and assistance in developing a job-search plan, and 21 percent receiving a direct job referral during the meeting.[13] The same study reports that other U.S. programs have provided a much lower level of services to participants, with very small proportions receiving individualized job-counseling. In fact, based

[13] Individuals in the control group could, on their own initiative, access these services. Michaelides and Mueser (2018) report that fewer than 10 percent of control cases received job-counseling services.

on comparisons with job-search assistance interventions in Florida and Idaho that did not include job counseling, Michaelides and Mueser (2017) concluded that the Nevada REA's job-counseling services provided direct aid to participants' job-search efforts.[14]

The value of job counseling has also been established by many experimental studies of programs implemented in Europe (e.g, Gorter and Kalb, 1996; Dolton and O'Neill, 2002; Hägglund, 2011; Crepon and van den Berg, 2016). Intuitively, intensive job counseling may improve the long-term outcomes of participants by helping them to develop more effective job-search strategies and obtain sustainable jobs. Job counselling may guide participants who would have otherwise conducted a more general (and less efficient) job search to focus their job search efforts on jobs that are consistent with their skills. Job counseling may also provide direct job referrals which, in some cases, may lead to the immediate reemployment of participants in jobs that are compatible with their skills. Job counseling may also improve participants' basic job-search skills (e.g., use automated job banks, develop professional resumes, and improve interviewing skills), thereby reducing barriers or fixed costs to job-search. Lastly, job counseling may reduce the psychic costs of job search and motivate participants to conduct a more active search, particularly those who in the program's absence may have lacked motivation or may have been discouraged by lack of job options.

Another potential explanation why the Nevada REA program was more effective is that it operated in a different labor market context than the programs examined by other studies. Nevada REA was implemented during the Great Recession, a period when unemployment

[14] Michaelides and Mueser (2017) compared the results of the Nevada REA with the results of REA programs in Florida and Idaho. The REA programs in Florida and Idaho included UI eligibility reviews similar to the Nevada REA, but in contrast to the Nevada REA treatment, the Florida and Idaho REA programs did not include job counseling services. The comparison indicates that the Nevada program had larger short-term effects on UI spells, employment, and earnings than the Florida and Idaho programs, suggesting that the additional job counseling services offered by the Nevada program were a significant factor behind the larger estimated treatment effects.

duration and the potential duration of UI benefits was much higher for Nevada REA participants than it was for participants in previous programs.[15] Thus, individuals in the control group in the Nevada REA program may have been more likely to remain unemployed longer than control group individuals in other programs. This implies that, compared with other programs, effects produced by the early exit of some participants to avoid requirements in the Nevada REA program may have been particularly large compared to similar effects from other programs. However, Michaelides and Mueser (2017; 2018) indicate that much of the effect of the Nevada REA program on UI exits appears to have emerged after participants received job counseling services, rather than the threat or notification of program requirements.

Our findings also shed light on how the effects of job-counseling programs may affect other individual and family outcomes. There is no evidence that the program affected contractor employment and self-employment, indicating that the program did not increase self-employment at the expense of W-2 employment, nor did lack of salary job options pushed control cases to resort to self-employment. We also find no evidence that the program affected receipt of disability benefits, indicating that the reductions in UI receipt were not offset by increases in receipt of disability benefits. Compared with control group individuals, treatment group individuals had higher total family income, were more likely to file a tax return, and had higher total tax liability. The results also indicate positive – but statistically insignificant – effects on spousal earnings, suggesting that the lessons learned from program participation may have been transferred to other family members. A key finding of this study is that treatment group individuals experienced a smaller decline in home ownership in the first three years after entry into the program than did individuals in the control group. As a result, home ownership was

[15] Whereas UI recipients participating in previous job-search assistance programs were typically eligible for up to 26 weeks of benefits, treatment and control cases in the Nevada REA studies were, on average, eligible for about 87 weeks of benefits (23 under regular UI, 46 under EUC, and 18 under EB).

significantly higher for treatment cases throughout the six-year period following the intervention. This finding indicates that the program's treatment effects on salary employment and earnings may have helped participants to avoid selling their homes or defaulting their mortgage debt.

Finally, we note that recent work on the effects of labor market policies have focused on displacement effects. These effects are based on the possibility that programs may help treatment group individuals find jobs and improve their overall outcomes by displacing individuals who were not served by the program but were also looking for jobs during the same period. The empirical analyses presented here do not explicitly account for such displacement effects. Because the experimental sample for the Nevada REA program was small relative to the population of job seekers in Nevada at the time of the program, the estimated program effects may not be affected by such displacement effects. However, if the program were to be implemented on a larger scale, it is possible that, due to displacement effects, the program's actual effects could differ from the estimated effects presented here.

There is mixed empirical evidence on this issue. Some studies provide direct evidence that job-search assistance programs might have substantial displacement effects, particularly among the long-term unemployed youth (Crepon et al., 2013; Feracci et al., 2010; Gautier et al., 2018). Other studies suggest that the modest effects of regulations requiring increased job-search efforts among UI recipients may also be due to displacement effects (Toohey, 2015; Lise et al., 2004). In contrast, Graversen and van Ours (2008) observe that when a relatively small portion of the target population is served by the intervention, it is unlikely that estimated effects are biased due to displacement effects altering the outcomes of those not served by the program. Other studies find that job-search assistance and employment subsidies have substantial positive impacts even when they are provided to a large share of unemployed workers, suggesting the displacement effects are minor (Blundell et al., 2004; De Giorgi, 2005). Further, Martins and Possoa e Costa (2014) conclude that displacement effects are not important and that targeted groups which benefit from job-search assistance might not do so at the expense of other groups.

V. Conclusion

Understanding whether job-search assistance creates permanent increases in employment or if the effects are temporary is important for policymakers and academic researchers. This study uses administrative tax data to examine the long-term effects of job-search assistance on employment, earnings, and tax outcomes. The analyses focus on the Nevada REA program which was administered in Nevada in 2009. Examining long-term effects of job-search assistance in this context is compelling for a number of reasons. The Nevada REA program used random assignment to determine whether program-eligible UI recipients would be assigned to the treatment or the control group, provided a wider range of services to participants than most U.S. programs studied to date, and prior evidence has indicated significant short-term effects on participants' individual employment, earnings, and UI receipt.

Our findings indicate that the Nevada REA program led to substantive increases in individual employment and earnings, which were sustained for at least six years after program entry. This suggests that the intensive job-counseling services provided by the program helped individuals who would not have found jobs in the absence of the program to become employed and increase their earnings. Results also show that the program led to substantial reductions in UI benefit amounts collected, with UI savings exceeding program costs by more than four times. The program did not affect receipt disability benefits, suggesting that reductions in UI receipt were not offset by increases in receipt of other types of government assistance. Another key finding is that the program had impacts on participants' household-level outcomes including total income, tax filing, tax liability, and home ownership. Overall, our findings show that job-search assistance programs may produce substantial long-term effects for participants and their families that extend beyond any short-term effects on employment and earnings for the individuals directly receiving services.

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Table 1: Summary Statistics by Treatment Status

	Treatment	Control	p-value
Age < 25	0.179	0.177	0.833
	(0.383)	(0.382)	
Age 26-34	0.222	0.222	0.956
	(0.416)	(0.415)	
Age 35-44	0.223	0.229	0.324
	(0.416)	(0.420)	
Age 45-54	0.241	0.207	0.000
	(0.428)	(0.405)	
Age 55-64	0.108	0.120	0.015
	(0.310)	(0.325)	
Age > 64	0.027	0.044	0.000
	(0.163)	(0.206)	
Female	0.422	0.433	0.172
	(0.494)	(0.496)	
Filed return	0.940	0.927	0.001
	(0.238)	(0.260)	
Single	0.386	0.386	0.968
	(0.487)	(0.487)	
Joint	0.371	0.351	0.007
	(0.483)	(0.477)	
Head of Household	0.165	0.174	0.147
	(0.372)	(0.379)	
Filing, other	0.017	0.016	0.666
	(0.129)	(0.126)	
1(claimed EITC)	0.191	0.203	0.058
	(0.393)	(0.402)	
EITC	$427	$458	0.081
	(1,111)	(1,156)	
1(Self Employed)	0.098	0.097	0.841
	(0.297)	(0.295)	
1(Dependents age < 25)	0.396	0.388	0.301
	(0.489)	(0.487)	

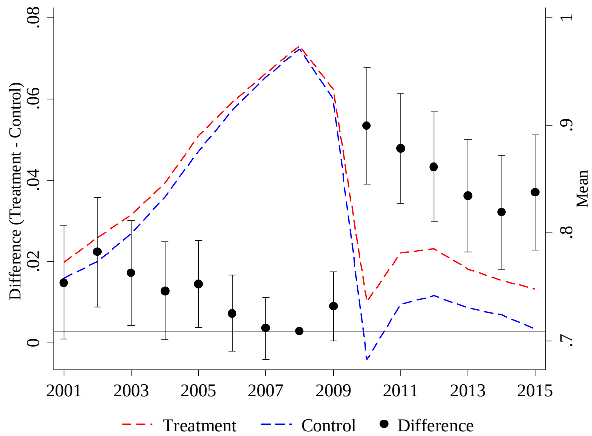
Table 1 (continued): Summary Statistics by Treatment Status

	Treatment	Control	p-value
1(has W-2 wages)	0.974	0.971	0.269
	(0.159)	(0.168)	
W-2 wage amount	$34,037	$33,323	0.152
	(31,804)	(29,711)	
1(has 1099-MISC)	0.068	0.070	0.508
	(0.251)	(0.256)	
1099-MISC amount	$934	$853	0.516
	(7,958)	(7,628)	
1(has 1099-G UI)	0.289	0.269	0.005
	(0.453)	(0.444)	
1099-G UI amount	$1,229	$1,184	0.311
	(2,834)	(2,788)	
1(has 1099-SSA disability)	0.005	0.005	0.860
	(0.071)	(0.070)	
1099-SSA disability amount	$52	$50	0.883
	(916)	(851)	
1(has 1099-SSA retirement)	0.030	0.048	0.000
	(0.170)	(0.213)	
1099-SSA retirement amount	$415	$672	0.000
	(2,632)	(3,313)	
Broad income	$36,668	$36,081	0.250
	(32,513)	(30,367)	
Observations	4,675	27,153	

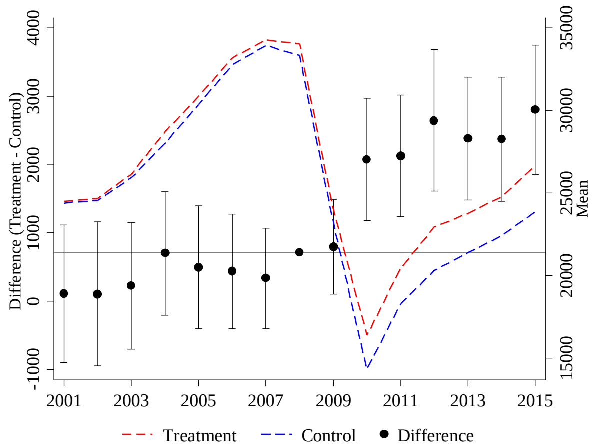
Notes: Sample means are reported with standard deviations in parentheses below the corresponding means. The p-value for a test of equality in the means across treatment and control groups is reported in the right column. Summary statistics are based on tax year 2008. Age is age as of January 1, 2008. Broad income is defined as the sum of W-2 wages, 1099-MISC amount, 1099-G UI amount, and 1099-SSA disability and retirement amount. Dollars are CPI adjusted to 2015 dollars.

Figure 1: Effects on Individual W-2 Employment and Earnings based on Third-Party Tax Forms

A. W-2 Employment

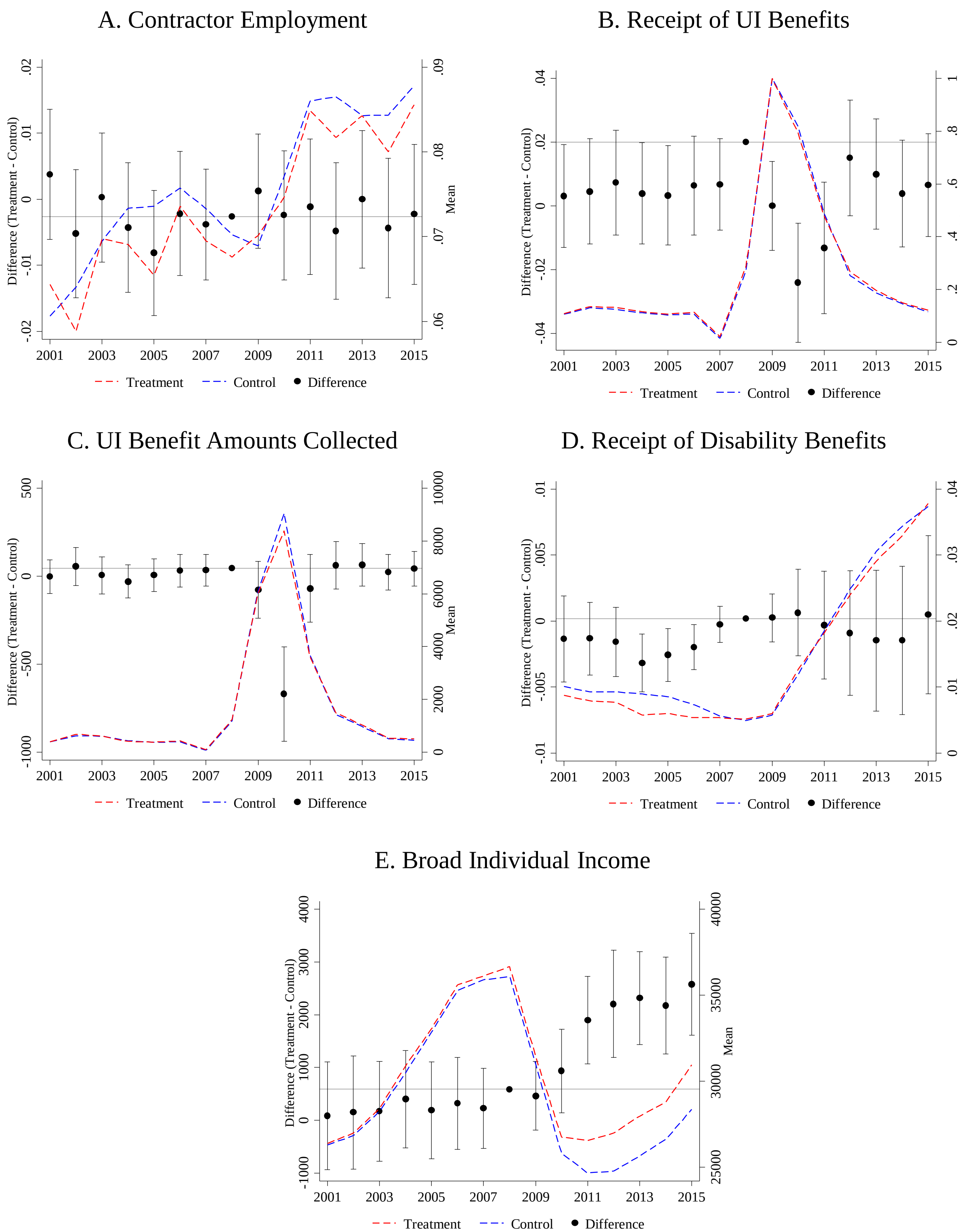


B. W-2 Earnings



Notes: Black dots represent treatment-control differences in outcomes, with 95 percent confidence intervals shown in vertical bands. Standard errors are clustered by individual. Red and blue dotted lines present outcome means for the treatment and control group, respectively.

Figure 2: Effects on Non-Wage Sources of Income based on Third-Party Tax Forms



Notes: Black dots represent treatment-control differences in outcomes, with 95 percent confidence intervals shown in vertical bands. Standard errors are clustered by individual. Red and blue dotted lines present outcome means for the treatment and control group, respectively.

Figure 3. Effects on Household-Level Outcomes based on Filed Tax Returns

Notes: Black dots represent treatment-control differences in outcomes, with 95 percent confidence intervals shown in vertical bands. Standard errors are clustered by individual. Red and blue dotted lines present outcome means for the treatment and control group, respectively.

Figure 4. Effects on Income based on Filed Tax Returns

Notes: W-2 earnings are earnings reported on the 1040 if filer, W2 earnings if not. Total family income is total income reported on 1040 if filer, broad income (defined above) if not. Tax liability as calculated on the 1040 if filer, W2 withholdings if not. Tax balance due as calculated on the 1040 if filer (negative values are refunds), 0 if did not file. Black dots represent treatment-control differences in outcomes, with 95 percent confidence intervals shown in vertical bands. Standard errors are clustered by individual. Red and blue dotted lines present outcome means for the treatment and control group, respectively.

Table 2: Treatment Effects with and without Controls, Selected Outcomes

	W-2 Employment			W-2 Earnings		
Treat * I(2001)	0.012	0.012		-$604	-$604	
	(0.007)	(0.007)		(514)	(514)	
Treat * I(2002)	0.020	0.020		-$605	-$605	
	(0.007)	(0.007)		(537)	(537)	
Treat * I(2003)	0.014	0.014		-$487	-$487	
	(0.007)	(0.007)		(475)	(475)	
Treat * I(2004)	0.010	0.010		-$14	-$14	
	(0.006)	(0.006)		(461)	(461)	
Treat * I(2005)	0.012	0.012		-$217	-$217	
	(0.005)	(0.005)		(460)	(460)	
Treat * I(2006)	0.004	0.004		-$277	-$277	
	(0.005)	(0.005)		(428)	(428)	
Treat * I(2007)	0.001	0.001		-$379	-$379	
	(0.004)	(0.004)		(376)	(376)	
Treatment	0.003	-0.005	-0.007	$714	$147	-$105
	(0.003)	(0.003)	(0.002)	(499)	(493)	(370)
Treat * I(2009)	0.006	0.006	0.006	$81	$81	$81
	(0.004)	(0.004)	(0.004)	(355)	(355)	(355)
Treat * I(2010)	0.051	0.051	0.051	$1,361	$1,361	$1,361
	(0.007)	(0.007)	(0.007)	(456)	(456)	(456)
Treat * I(2011)	0.045	0.045	0.045	$1,413	$1,413	$1,413
	(0.007)	(0.007)	(0.007)	(456)	(456)	(456)
Treat * I(2012)	0.041	0.041	0.041	$1,932	$1,932	$1,932
	(0.007)	(0.007)	(0.007)	(529)	(529)	(529)
Treat * I(2013)	0.033	0.033	0.033	$1,666	$1,666	$1,666
	(0.007)	(0.007)	(0.007)	(460)	(460)	(460)
Treat * I(2014)	0.029	0.029	0.029	$1,656	$1,656	$1,656
	(0.007)	(0.007)	(0.007)	(464)	(464)	(464)
Treat * I(2015)	0.034	0.034	0.034	$2,087	$2,087	$2,087
	(0.007)	(0.007)	(0.007)	(483)	(483)	(483)
R^2	0.054	0.078	0.144	0.031	0.108	0.276
Observations	477,420	477,420	254,624	477,420	477,420	254,624
Static Controls		X			X	
Pre-Treat Controls			X			X

Table 2 (continued) : Treatment Effects with and without Controls, Selected Outcomes

	Broad Individual Income			Total Family Income		
Treat * I(2001)	-$504	-$504		-$128	-$128	
	(521)	(521)		(942)	(943)	
Treat * I(2002)	-$437	-$437		-$482	-$482	
	(547)	(548)		(930)	(931)	
Treat * I(2003)	-$419	-$419		-$201	-$201	
	(482)	(482)		(897)	(897)	
Treat * I(2004)	-$186	-$186		$591	$591	
	(470)	(471)		(943)	(943)	
Treat * I(2005)	-$400	-$400		$19	$19	
	(469)	(469)		(849)	(849)	
Treat * I(2006)	-$264	-$264		$38	$38	
	(444)	(444)		(791)	(791)	
Treat * I(2007)	-$363	-$363		-$114	-$114	
	(388)	(388)		(694)	(694)	
Treatment	$586	$246	$22	$952	-$123	-$361
	(510)	(501)	(371)	(870)	(810)	(736)
Treat * I(2009)	-$125	-$125	-$125	-$216	-$216	-$216
	(332)	(332)	(332)	(475)	(475)	(475)
Treat * I(2010)	$347	$347	$347	$1,076	$1,076	$1,076
	(404)	(404)	(404)	(959)	(959)	(959)
Treat * I(2011)	$1,309	$1,309	$1,309	$1,526	$1,526	$1,526
	(424)	(424)	(424)	(844)	(844)	(844)
Treat * I(2012)	$1,618	$1,618	$1,618	$1,240	$1,240	$1,240
	(519)	(520)	(520)	(1,079)	(1,080)	(1,080)
Treat * I(2013)	$1,725	$1,725	$1,725	$2,267	$2,267	$2,267
	(449)	(449)	(449)	(1,162)	(1,162)	(1,162)
Treat * I(2014)	$1,588	$1,588	$1,588	$3,139	$3,139	$3,139
	(468)	(468)	(468)	(870)	(870)	(870)
Treat * I(2015)	$1,993	$1,993	$1,993	$3,414	$3,414	$3,414
	(492)	(492)	(492)	(883)	(883)	(883)
R^2	0.015	0.107	0.283	0.008	0.125	0.232
Observations	477,420	477,420	254,624	477,420	477,420	254,624
Static Controls		X			X	
Pre-Treatment Controls			X			X

Notes: The table reports estimated regression coefficients with standard errors cluster on individual in parentheses below the corresponding estimates. Specifications with static controls defined in 2008 include: indicators for age bin (<26, 26-34, 35-44, 45-54,55-64, >64), gender, age bin by gender, 2-digit NAIC, 3-digit zip code, filing status, SSA-1099 receipt for disability or retirement, and 1099-MISC receipt. Specifications with pre-treatment controls defined in 2001-2008 include indicators for 10-year age bins and gender, and, for each year between 2001 and 2008, indicator variables for: SSA-1099 receipt, W-2 employment, spousal W-2 employment, 1099-MISC receipt, 1099-G receipt, filing status (single, joint, head of household, or missing separately), EITC receipt and for $10k increments of W-2 earnings.

Table 3A: Experimental Evidence on the Effectiveness of U.S. Reemployment Programs

	Nevada REA	Florida WPRS	Florida REA	Idaho REA	Nevada REA
Study	Michaelides and Mueser (2017a)	Michaelides and Mueser (2017b)	Michaelides and Mueser (2017b)	Michaelides and Mueser (2017b)	Michaelides and Mueser (2017b)
Study Period	July – Dec. 2009	Aug. – Nov. 2009	Aug. – Nov. 2009	Aug. – Nov. 2009	Aug. – Nov. 2009
Participants	All UI recipients	All UI recipients	All UI recipients	All UI recipients	All UI recipients
Urate (state, national)	12.1%, 9.7%				
Services	Interview, eligibility review, job-counseling	Orientation, referral to services	Eligibility review	Online eligibility review	Interview, eligibility review, job-counseling
Effects on UI					
UI Duration (weeks)	4.40 [-12%]***	-.70 [-2%]**	-1.89 [-5%]***	-1.38 [-4%]***	-3.74 [-10%]***
UI Benefits Collected ($)	-1,145 [-10%]***	-159 [-2%]***	-453 [-5%]***	-290 [-3%]**	-976 [-9%]***
Exhausted Benefits (rate)	-.104 [-15%]***	-.012 [-2%]***	-.036 [-5%]***	-.037 [-5%]***	-.092 [-13%]***
Effects on Earnings					
Quarters 1-4†	+1,854 [+21%]***	85 [+1%]	370 [+4%]	455 [+6%]**	1,740 [+21%]***
Quarters 5-8††	+752 [+13%]***	--	--	--	--
Quarters 9-12†††	--	--	--	--	--

Note: Reported is the estimated treatment effect; in brackets, is the estimated effect as a percentage of the control group mean, where available; ***, **, *= statistically significant at the 1, 5, 10 percent level.

Control group means for UI duration, in program order: 36.5, 40.2, 40.2, 32.0, and 36.7 weeks. Control group means for UI benefits, in program order: $11,119, $9,190, $9,190, $8,336, and $11,188.

† = Control group means, in program order: $8,655, $9,045, $9,045, $7,776, and $8,284.

††= Available only for quarters 5 and 6 for the Nevada program. Control group mean: $5,798 (Nevada, quarters 5 and 6 only).

†††= Not available

Table 3B: Experimental Evidence on the Effectiveness of U.S. Reemployment Programs

	Nevada Claimant Placement Program	Wisconsin Eligibility Review Pilot Project	Charleston Claimant Placement and Work Test Demonstration	New Jersey UI Reemployment Demonstration	Washington Alternative Work Search Experiment
Study	Meyer (1995)	Meyer (1995)	Meyer (1995)	Meyer (1995)	Meyer (1995)
Study Period	Feb. 1977 – Mar. 1978	Mar. – Aug. 1983	Feb. – Dec. 1983	July 1986 – June 1987	July 1986 – Aug. 1987
Participants	All UI recipients	All UI recipients	All UI recipients	All UI recipients	All UI recipients
Urate (state, national)	6.2%, 6.9%	10.6%, 9.9%	9.6%, 9.5%	4.5%, 9.5%	7.7%, 6.6%
Services	Interview, eligibility review, job counseling (weekly)	Interview, eligibility review, workshop	Interview, eligibility review, job counseling, workshop	Interview, eligibility review, job counseling	Interview, eligibility review, job counseling, workshop
Effects on UI					
UI Duration (weeks)	-3.90 [-31%]***	-.62 [-4%]	-.76 [-5%]**	-.47 [-3%]**	-.47 [-3%]*
UI Benefits Collected ($)	-318 [-31%] ***	-82 [-4%]	-73 [-5%]**	-150 [-3%]**	-68 [-3%]*
Exhausted Benefits (rate)	--	--	--	--	--
Effects on Earnings					
Quarters 1-4 †	--	--	152 [+3%]	235 [+3%]	-23 [-0%]
Quarters 5-8 ††	--	--	--	279 [+2%]	--
Quarters 9-12 †††	--	--	--	40 [+1%]	--

Note: Reported is the estimated treatment effect; in brackets, is the estimated effect as a percentage of the control group mean, where available; ***, **, *= statistically significant at the 1, 5, 10 percent level.

Control group means for UI duration, in program order: 12.4, 16.4, 15.5, 17.9, and 14.5 weeks. Control group means for UI benefits, in program order: $1,026, $2,158, $1,510, $4,559, and $2,030.

† = Not available for the Nevada and Wisconsin programs. Available only for quarters 1 and 2 for the Charleston program. Control group means – $5,014 (Charleston, quarters 1 and 2 only), $8,836 (New Jersey), and $9,919 (Washington).

†† = Available only for New Jersey program. Control group mean = $11,252.

††† = Available only for New Jersey program (quarters 9 and 10 only). Control group mean = $11,252 (quarters 9 and 10 only).

Table 3C: Experimental Evidence on the Effectiveness of U.S. Reemployment Programs

	Washington, DC JSA Demonstration	Florida JSA Demonstration	Kentucky WPRS	Maryland Work Search Demonstration
Study	Decker *et al.* (2000)	Decker *et al.* (2000)	Black *et al.* (2003)	Klepinger *et al.* (2002)
Study Period	Mar. 1995 – June 1996	Mar. 1995 – June 1996	Oct. 1994 – June 1996	Jan. – Dec. 1994
Participants	UI recipients facing long-term unemployment	UI recipients facing long-term unemployment	UI recipients facing long-term unemployment	All UI recipients
Urate (state, national)	8.4%, 5.6%	5.5%, 5.6%	5.5%, 5.5%	5.2%, 6.1%
Services	Orientation, eligibility review, job-search workshop	Orientation, eligibility review, job-search workshop	Orientation, referral to services	Job-search workshop
Effects on UI				
UI Duration (weeks)	-1.13 [-6%]***	-.41 [-3%]***	-2.23***	-.59 [-5%]**
UI Benefits Collected ($)	-182 [-4%]***	-17 [-1%]*	-143*	-75 [-4%]**
Exhausted Benefits (rate)	-.048 [-8%]***	-.018* [-4%]	-0.024	-.011 [-4%]
Effects on Earnings				
Quarters 1-4†	635 [+10%]**	-6 [-0%]	1,055 [+14%]**	-163 [-2%]
Quarters 5-8††	961 [+12%]**	-313 [-3%]	176 [+3%]	--
Quarters 9-12†††	409 [+10%]**	-275 [-2%]	--	--

Note: Reported is the estimated treatment effect; in brackets, is the estimated effect as a percentage of the control group mean, where available; ***, **, *= statistically significant at the 1, 5, 10 percent level.

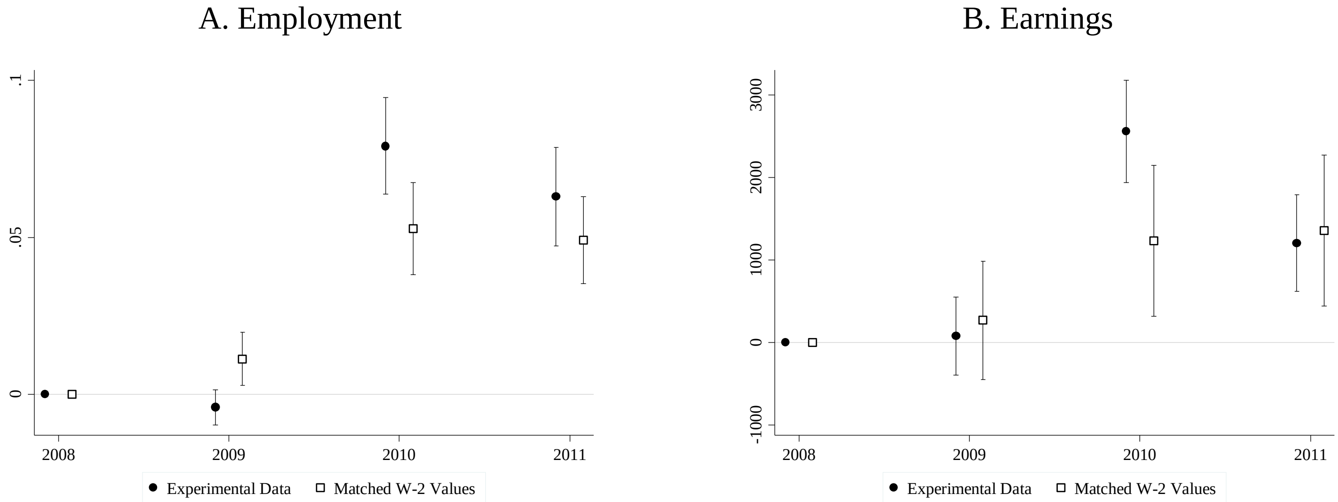
Control group means for UI duration, in program order: 20.1, 15.8, not available, and 11.9 weeks. Control group means for UI benefits, in program order: $4,236, $2,728, not available, and $2,085.

† = Control group means, in program order: $6,318, $9,127, $7,500, and $8,407.

††= Not available for the Maryland program. Available only for quarters 5 and 6 for the Kentucky program. Control group means: $8,148 (Washington, DC); $11,941 (Florida); and $5,100 (Kentucky, quarters 5 and 6 only).

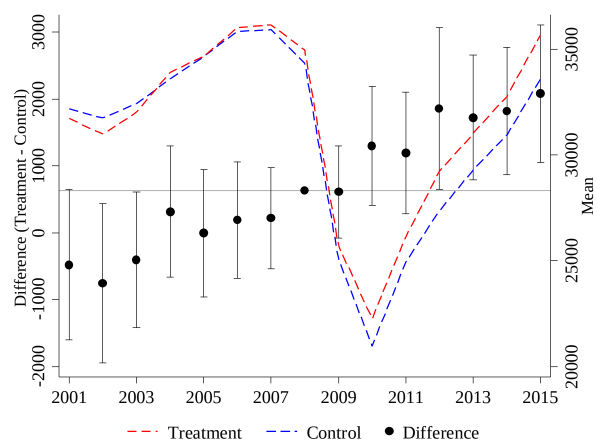
†††= Not available for the Kentucky and Maryland programs. Available only for quarters 10 and 11 for the Washington, DC program. Control group means – $4,255 (Washington, DC, quarters 10 and 11 only); and $11,851 (Florida).

Appendix Figure 1: Treatment-Control Differences in Employment and Earnings, REA Program Data vs. Sample Matched to Administrative Tax Data



Notes: Black dots represent treatment-control differences based on the Nevada REA 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 2: Treatment-Control Differences in Earnings, Conditional on Employment



Notes: Black dots represent treatment-control differences in outcomes, with 95 percent confidence intervals shown in vertical bands. Standard errors are clustered by individual. Red and blue dotted lines present outcome means for the treatment and control group, respectively.

Appendix Table 1: Regression Results, Treatment Likelihood in the Matched Sample

	Treatment Indicator		
	Beta	SE	p-value
Age	-0.0004	0.0001	0.0034
Female	-0.0038	0.0041	0.3548
Wage = 0	-0.0172	0.0130	0.1852
\$0 < Wage <= \$5k	-0.0177	0.0103	0.0850
\$5k < Wage <= \$10k	-0.0170	0.0093	0.0677
\$10k < Wage <= \$15k	-0.0117	0.0090	0.1902
\$15k < Wage <= \$20k	-0.0063	0.0088	0.4721
\$20k < Wage <= \$25k	-0.0051	0.0087	0.5592
\$25k < Wage <= \$30k	-0.0028	0.0086	0.7463
\$30k < Wage <= \$35k	0.0082	0.0090	0.3649
\$35k < Wage <= \$40k	-0.0106	0.0092	0.2476
\$40k < Wage <= \$45k	0.0064	0.0105	0.5429
\$45k < Wage <= \$50k	-0.005	0.011	0.683
\$50k < Wage <= \$55k	-0.004	0.012	0.723
\$55k < Wage <= \$60k	-0.014	0.013	0.302
\$60k < Wage	0.000	0.000	
UI / \$1,000	0.0014	0.0007	0.0546
Disability / \$1,000	0.0011	0.0024	0.6658
NEC / \$1,000	0.0002	0.0003	0.4781
Tax Filer	0.0239	0.0075	0.0014
1(EITC>0)	-0.0098	0.0053	0.0657
R^2		0.00123	
Observations		31,828	

Notes: Reported are estimated parameters, and their standard errors and p-values. Dependent variable is the treatment indicator. Control variables are from administrative tax data in 2008. All dollar values are CPI adjusted to 2015 dollars.

Appendix Table 2A: Treatment-Control Differences in Employment and Earnings

Tax Year	Treatment	Control	Difference	SE	p-Value
		W-2 Employment			
2001	0.773	0.758	0.015	0.007	0.026
2002	0.796	0.774	0.022	0.006	0.001
2003	0.817	0.800	0.017	0.006	0.005
2004	0.847	0.834	0.013	0.006	0.026
2005	0.890	0.876	0.014	0.005	0.004
2006	0.921	0.914	0.007	0.004	0.088
2007	0.948	0.945	0.003	0.004	0.314
2008	0.974	0.971	0.003	0.003	0.269
2009	0.934	0.925	0.009	0.004	0.025
2010	0.736	0.683	0.053	0.007	0.000
2011	0.782	0.734	0.048	0.007	0.000
2012	0.785	0.742	0.043	0.007	0.000
2013	0.767	0.730	0.037	0.007	0.000
2014	0.756	0.724	0.032	0.007	0.000
2015	0.748	0.711	0.037	0.007	0.000
		W-2 Earnings			
2001	24,516	24,407	109	470	0.815
2002	24,669	24,560	109	526	0.836
2003	26,147	25,920	227	469	0.628
2004	28,715	28,015	700	512	0.172
2005	30,853	30,356	497	523	0.342
2006	33,198	32,761	437	540	0.418
2007	34,268	33,934	334	491	0.496
2008	34,037	33,323	714	499	0.152
2009	24,014	23,218	796	355	0.025
2010	16,396	14,320	2,076	333	0.000
2011	20,433	18,306	2,127	402	0.000
2012	22,950	20,304	2,646	644	0.000
2013	23,771	21,391	2,380	464	0.000
2014	24,774	22,405	2,369	481	0.000
2015	26,675	23,875	2,800	545	0.000

Appendix Table 2B: Treatment-Control Differences in Broad and Total Income

Tax Year	Treatment	Control	Difference	SE	p-Value
		Broad Individual Income			
2001	26,398	26,315	83	495	0.868
2002	26,990	26,841	149	553	0.787
2003	28,418	28,251	167	497	0.737
2004	30,909	30,508	401	545	0.463
2005	33,044	32,858	186	553	0.736
2006	35,602	35,280	322	575	0.575
2007	36,122	35,899	223	521	0.668
2008	36,668	36,081	587	510	0.250
2009	31,455	30,994	461	358	0.198
2010	26,755	25,822	933	333	0.005
2011	26,568	24,672	1,896	409	0.000
2012	26,986	24,782	2,204	650	0.001
2013	27,983	25,672	2,311	480	0.000
2014	28,792	26,618	2,174	509	0.000
2015	30,954	28,375	2,579	572	0.000
		Total Family Income			
2001	45,250	44,426	824	718	0.251
2002	44,906	44,436	470	718	0.513
2003	45,824	45,074	750	715	0.294
2004	47,742	46,199	1,543	822	0.061
2005	48,697	47,726	971	760	0.202
2006	50,220	49,231	989	749	0.187
2007	50,740	49,902	838	779	0.282
2008	49,158	48,206	952	870	0.274
2009	41,686	40,951	735	883	0.405
2010	39,466	37,438	2,028	864	0.019
2011	38,937	36,459	2,478	631	0.000
2012	39,330	37,139	2,191	1,087	0.044
2013	41,722	38,504	3,218	1,067	0.003
2014	43,681	39,590	4,091	652	0.000
2015	46,191	41,825	4,366	682	0.000

Appendix Table 2C: Treatment-Control Differences in UI Benefits

Tax Year	Treatment	Control	Difference	SE	p-Value
		UI Benefit Amount			
2001	384	386	-2	24	0.926
2002	678	623	55	37	0.134
2003	604	600	4	35	0.907
2004	404	435	-31	25	0.207
2005	378	373	5	24	0.826
2006	422	391	31	25	0.225
2007	98	65	33	13	0.012
2008	1,229	1,184	45	45	0.315
2009	6,024	6,102	-78	86	0.367
2010	8,390	9,060	-670	127	0.000
2011	3,587	3,657	-70	87	0.421
2012	1,479	1,417	62	56	0.268
2013	1,030	966	64	46	0.163
2014	531	509	22	28	0.427
2015	496	455	41	27	0.130

Appendix Table 2D: Treatment-Control Differences in Spousal W-2 Employment and Earnings

Tax Year	Treatment	Control	Difference	SE	p-Value
		Spousal W-2 Employment			
2001	0.249	0.237	0.012	0.007	0.063
2002	0.250	0.239	0.011	0.007	0.087
2003	0.257	0.248	0.009	0.007	0.194
2004	0.264	0.253	0.011	0.007	0.115
2005	0.275	0.260	0.015	0.007	0.031
2006	0.283	0.268	0.015	0.007	0.039
2007	0.296	0.280	0.016	0.007	0.024
2008	0.299	0.279	0.020	0.007	0.005
2009	0.290	0.270	0.020	0.007	0.005
2010	0.283	0.263	0.020	0.007	0.006
2011	0.286	0.261	0.025	0.007	0.000
2012	0.289	0.262	0.027	0.007	0.000
2013	0.290	0.259	0.031	0.007	0.000
2014	0.285	0.254	0.031	0.007	0.000
2015	0.285	0.251	0.034	0.007	0.000
		Spousal W-2 Earnings			
2001	9,388	9,067	321	336	0.340
2002	9,527	9,127	400	337	0.235
2003	9,874	9,468	406	342	0.235
2004	10,343	9,832	511	350	0.144
2005	10,752	10,270	482	357	0.177
2006	11,174	10,706	468	363	0.198
2007	11,883	11,176	707	373	0.058
2008	11,579	11,038	541	364	0.138
2009	10,786	10,309	477	351	0.174
2010	10,546	9,866	680	347	0.050
2011	10,586	9,893	693	344	0.044
2012	10,585	9,939	646	346	0.062
2013	11,004	9,988	1,016	356	0.004
2014	11,231	10,061	1,170	363	0.001
2015	11,681	10,476	1,205	376	0.001

Appendix Table 2E: Treatment-Control Differences in Tax Filing and Homeownership

Tax Year	Treatment	Control	Difference	SE	p-Value
		File Tax Return			
2001	0.864	0.858	0.006	0.005	0.261
2002	0.874	0.861	0.013	0.005	0.012
2003	0.877	0.863	0.014	0.005	0.006
2004	0.885	0.867	0.018	0.005	0.001
2005	0.899	0.880	0.019	0.005	0.000
2006	0.909	0.895	0.014	0.005	0.002
2007	0.937	0.924	0.013	0.004	0.000
2008	0.940	0.927	0.013	0.004	0.001
2009	0.931	0.913	0.018	0.004	0.000
2010	0.901	0.880	0.021	0.005	0.000
2011	0.878	0.859	0.019	0.005	0.000
2012	0.851	0.829	0.022	0.006	0.000
2013	0.846	0.810	0.036	0.006	0.000
2014	0.822	0.790	0.032	0.006	0.000
2015	0.806	0.766	0.040	0.006	0.000
		Home Ownership			
2001	0.268	0.259	0.009	0.007	0.204
2002	0.282	0.277	0.005	0.007	0.447
2003	0.302	0.290	0.012	0.007	0.101
2004	0.312	0.302	0.010	0.007	0.168
2005	0.326	0.318	0.008	0.007	0.289
2006	0.337	0.332	0.005	0.007	0.505
2007	0.343	0.339	0.004	0.008	0.612
2008	0.339	0.335	0.004	0.007	0.603
2009	0.335	0.320	0.015	0.007	0.042
2010	0.314	0.295	0.019	0.007	0.009
2011	0.299	0.275	0.024	0.007	0.001
2012	0.290	0.263	0.027	0.007	0.000
2013	0.298	0.265	0.033	0.007	0.000
2014	0.297	0.270	0.027	0.007	0.000
2015	0.308	0.280	0.028	0.007	0.000