THE SOCIAL VALUE OF TARGETING INTERVENTIONS: EVIDENCE FROM REEMPLOYMENT SERVICES

Susan Athey[+], Zak Campbell[*], Eric Chyn[++], Justine Hastings[§], and Preston White[*]

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PRELIMINARY DRAFT.
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Abstract:

This paper uses novel administrative data from Rhode Island to estimate effects of re-employment services on recipients of Unemployment Insurance (UI) benefits. We study UI claimants who were required to register for basic job-search services at their local career center as part of the Reemployment and Eligibility Assessment (REA) initiative. Our research design relies on the fact that Rhode Island created a comparison group of UI claimants who were not required to participate in REA. We find that this nudge for services reduced weeks spent on UI and total benefits claimed, while having no significant impact on employment or earnings. We use a causal forest algorithm to study treatment effect heterogeneity finding that the average effects (particularly on labor market outcomes) masks important differences in responses across observable characteristics. Using these results, we propose an improved targeting system for REA based on predicted treatment effects. We show that substantial gains in post-services employment and earnings can be attained while reducing weeks on UI and benefiting individuals from all subgroups of the population.

[+]Stanford University and NBER, [*]Brown University and RIIPL, [++] University of Virginia and RIIPL, [§]Brown University, RIIPL, and NBER. We thank the Smith Richardson Foundation and the Laura and John Arnold Foundation for Financial Support.

1 Introduction

Workforce participation is a cornerstone of economic productivity. Recent research has linked joblessness (particularly due to declines in the manufacturing sector) to increases in rates of suicide and crime, declines in two-parent households, and widening political polarization (Wilson 1996; Autor et al. forthcoming; Pierce and Schott 2016; Feler and Senses 2017; Autor et al. 2017). To help unemployed and displaced workers, the U.S. spends tens of billions of dollars annually on unemployment insurance and re-employment services (U.S. CBO, 2012). States often use federal funds to operate career services centers, which provide resources for job search, advice on resume building, and job training.

From 2005 to 2015, the U.S. Department of Labor distributed grants to 21 states as part of the Reemployment and Eligibility Assessment (REA) initiative. The grants required that each state's workforce agency assign a portion of their Unemployment Insurance (UI) beneficiaries to attend one-on-one interviews in person. These interviews would include a review of on-going UI eligibility, provision of labor market information, development of work-search plan and referral to additional career and training services. Individuals selected to be part of the REA initiative received a letter of notification to attend a REA interview and that failure to participate would result in an interruption of their UI benefits. Contracted evaluations of the REA program generally showed that the program reduced on the duration of UI participation and the amount of UI benefits drawn, with mixed evidence on the program's effect on labor market outcomes (Benus et al. 2008; Michaelides et al. 2012; Michaelides and Mueser2018).[1]

[1] Black et al. (2003) find similar impacts on UI duration and benefit amounts in their study of re-employment services provided through the Worker Profiling and Reemployment System (WPRS). They also find that WPRS had substantial impacts on earnings.

This paper uses new machine learning techniques and comprehensive administrative data to measure heterogeneity in treatment effects of re-employment services provided through REA. We study the case of Rhode Island, where authorities selected 23,210 UI claimants for REA program participation from June 2009 through June 2014. During this period, authorities created a comparison group of 25,568 UI claimants who were not required to participate in REA. Our data contain anonymized Rhode Island state administrative records housed in a secure facility at the Rhode Island Innovative Policy Lab (RIIPL). The database contains individual unemployment insurance (UI) claims, re-employment service participation, and earnings records from the UI system. We also have detailed information on an individual's participation in other social programs, educational background, incarceration history, and demographics. Personally identifiable information has been removed from the data and replaced with anonymous identifiers so that approved RIIPL researchers can analyze records associated with an individual while preserving privacy (Hastings et al. 2018).

Our analysis builds on a recent literature that develops new methods for estimating the expected value of treatment effects conditional on observable characteristics of individuals, or conditional average treatment effects (CATEs), in randomized experiments and observational studies. The methods in this literature provide data-driven approaches for flexibly estimating CATEs. Our preferred estimates use Wager and Athey (forthcoming)'s Causal Forest (CF) algorithm, which modifies the popular random forest algorithm to estimate causal effects. CF estimates CATEs as a flexible function of baseline covariates, preserving desirable properties like consistency and the ability to construct valid confidence for the CATE at any realization of the covariates.

Using the CF approach, we find important differences in responses for individuals based on observable characteristics. On average, the receipt of REA services decreased both the number of weeks spent on UI and total amount of UI benefits claimed while there was no corresponding impact on labor market outcomes. These average impacts mask a large range of treatment effects uncovered by the causal forest algorithm. Notably, there are sizable fractions of our sample that have positive treatment effect for short-term employment (51.1 percent) and earnings (46.7 percent). We show that these heterogeneous effects vary with important demographics such as gender, race, ethnicity and measures of skills transferability based on an individual's prior occupation. In particular, whites, males, and those with low skills transferability fall in the range of treatment effects distributions associated with negative outcomes – longer time on UI, lower employment, and lower earnings in the year after an unemployment spell.

Based on these results, we simulate an improved targeting system for re-employment services. Specifically, we examine a scenario where authorities target services to individuals with positive predicted earnings impacts. Relative to the non-targeted (status quo) policy, the targeted approach performs substantially better. Our simulation indicates that the targeted approach would have *decreased* the total number of weeks spent on UI by 25,668 whereas the non-targeted approach only resulted in 23,297 fewer weeks (a reduction of 2,371 weeks). In addition, the targeted treatment would result in an *increase* in post-treatment earnings of $11,218,436 relative to an untargeted *decrease* of $1,134,180.

Overall, this paper contributes to several literatures. First, we build on prior studies of the impact of re-employment services and job search interventions by exploring treatment effect heterogeneity. Prior studies of the REA program only report mean impacts for the full sample of UI claimants (Benus et al. 2008; Poe-Yamagata et al. 2011; Michaelides et al. 2012; Manoli et al.

2018). Similarly, non-experimental or quasi-experimental studies of the Worker Profiling and Reemployment System (WRPS) focus on mean impacts or have limited exploration of heterogeneity (Dickinson et al. 2002; Black et al. 2003; Black et al. 2007).[2] Experimental studies of the impact of job search requirements exclusively study average impacts (Johnson and Klepinger 1994; Meyer 1995; Klepinger et al. 2002; Ashenfelter et al. 2005).

Second, we are one of the first papers to apply machine-learning techniques to shed light on the optimal assignment of publicly provided interventions. Our work is most similar to Davis and Heller (2017a), who use causal forest methods to explore which subgroups are most likely to respond to a summer jobs program. Their sample and size of their program is more limited relative to our setting (N=6,850). Our application lends itself naturally to machine learning; the program is large and prediction could feed quickly into an updated targeting policy with clear objectives of boosting employment while efficiently using UI benefits. Beyond estimating heterogeneity and optimal assignment policies, several recent studies have used machine learning to study related questions about treatment effect heterogeneity. For example, Chernozhukov et al (2018) provide an approach to test hypotheses about estimands such as the average treatment effect for the top 10% of the population, ranked by treatment effects, and they illustrate the approach with an application to the impact of microcredit on economic development.

Across fields in the social sciences, a few authors have used machine learning to design policies based on individual characteristics. Closely related to this paper, Hitsch and Misra (2018) apply causal forest to estimate heterogeneous treatment effects in a catalog mailing experiment, and they evaluate a policy for assigning individuals to treatments based on that policy. Hartford et al (2017) use an approach based on neural networks to estimate treatment effect heterogeneity

[2] Black et al. (2003) explore heterogeneous impacts of the WRPS program along a single dimension.

in a setting with instrumental variables, and apply it to the effect of re-ranking advertisements in internet search. Künzel et al (2017) analyze treatment effect heterogeneity in survey experiments, examining applications such as persuasion about attitudes towards abortion, while Künzel et al (2018) use an approach based on neural networks to revisit heterogeneous treatment effects in voter mobilization. Beyond the problem of treatment heterogeneity, a variety of studies have applied machine learning to study public policy problems.[3]

Finally, our work contributes to a large literature on interventions for disadvantaged workers and workers impacted by trade- or technology- induced job loss. Experimental studies of the National Supported Work demonstration show that intensive job training has large positive benefits for women on social assistance and small, not-statistically-significant impacts for disadvantaged youth (LaLonde 1986; Couch 1992; LaLonde 2003).[4,5] Recent research has linked negative health, family, and community outcomes to trade-induced loss in earnings and employment (Autor et al. forthcoming; Pierce and Schott 2016; Feler and Senses 2017; Autor et al. 2017). We find that REA has larger positive impacts for minorities, women, and those with high skills transferability. We also find that observable measures of disadvantaged status such as having a prior criminal convictions are correlated with having negative treatment effects in the REA program. The patterns of heterogeneity in REA program impact suggest that there is more work to do to understand how workforce training programs can benefit gainful employment for

[3] The prediction policy literature studies problems where the policy-maker's decision can be reduced to a prediction problem; for example, if a patient is likely to die within the year from a heart condition, then they should not receive hip replacement surgery (Kleinberg et al, 2015); see also (Kleinberg et al 2017; Glaeser et al 2016). A literature on estimating average treatment effects in observational studies also employs machine learning methods, for example, to control for potential confounders (Belloni, Chernozhukov, and Hansen 2014; Athey, Imbens and Wager, forthcoming; Chernozhukov et al 2017) or to select instruments (Chernozhukov et al 2015). See Athey (forthcoming) for a discussion of the use of machine learning in economics more broadly.

[4] The NSW program admitted ex-drug addicts, ex-criminal offenders and high school dropouts of both sexes. In addition, the NSW program admitted women on AFDC.

[5] See Stanley et al. (1998) for a summary of studies on the impact of labor market assistance programs in the U.S.

those most in need due to personal circumstances, and due to economic changes resulting from changing trade and technology.

2 Background

2.1 Overview of REA

In 2005, the U.S. Department of Labor (U.S. DOL) began inviting state workforce agencies (SWAs) to apply for the Reemployment Eligibility Assessment grant program (REA). The goals of this initiative were to (1) enhance the rapid reemployment of Unemployment Insurance (UI) claimants; (2) reduce erroneous UI payments; and (3) realize cost savings for the state UI trust funds. SWAs that received funds from the program were required to provide the following services to targeted UI claimants participating in the program:[6]

- Assess UI eligibility and refer claimants to adjudication, if a potential issue is identified;
- Require claimants to report to a state's One-Stop Career Center;
- Provide labor market information specific to the claimant's needs;
- Require registration with the state's job bank;
- Provide an orientation for the services provided at the state's One-Stop Career Center;
- Develop a reemployment plan for the individual, including demonstrated work search activity;
- Refer, as necessary, to other reemployment or training services.

[6] Funds from REA were to be used to administer UI laws, and not to be used toward reemployment services (e.g. resume building or interview preparation).

To provide these REA services, participating states agreed to integrate the funding and services into their existing reemployment services infrastructure.[7] By requiring UI claimants to complete the REA services, the program served as a way to ensure claimants met the UI eligibility requirements and as an entry point for claimants to report and learn about the various services offered in their state's Career Centers. Participation in REA services was mandatory for claimants selected for REA; cases of failure to complete requirements were to be reported to UI authorities for adjudication.

States participating in REA had flexibility to design their state-specific programs, including the format for services and which groups of UI claimants to target.[8] For example, a state could elect to target claimants determined to be less or more likely to exhaust their UI benefits, or a state could choose to require claimants report for additional services or meetings in the Career Center.

To measure the effectiveness of REA services, participating states were required to create a comparison group of claimants who were similar to REA participants (the treatment group). Authorities created the comparison groups in the same week as the treatment group to ensure an even comparison. While randomizing claimants into treatment and comparison group was encouraged by the U.S. DOL, states did not have to conform to a single method of creating the groups.

2.2 REA in Rhode Island

Rhode Island's REA program was in operation from the inception of the U.S. DOL's REA grant program in 2005 until a transition to the Reemployment Services and Eligibility Assessment

[7] Under Wagner-Peyser and Workforce Innovation Act (WIA) funding, states had comprehensive "one-stop" career centers that provided employment and training services.

[8] In electing groups of UI claimants to target for REA, states were restricted from including claimants targeted for the Worker Profiling and Reemployment Services program (WPRS), as well as claimants who have a return-to-work date or were part of a hiring hall union.

program (RESEA) in 2015. During a REA session, participants attended an orientation at one of four of Rhode Island's one-stop career centers (called *net*WORKri centers) and received one-on-one counselling for the REA services described in Section 2.1. As part of the services in Rhode Island, participants was required to complete a skills-assessment, complete three work search per week and post a resume on EmployRI, which is Rhode Island's virtual recruitment website. Participants also had to schedule a 30-day follow-up meeting with a counselor, where another one-on-one meeting would check whether the participant completed program requirements and provide further assistance to the job seeker if necessary. Failure to attend one-on-one meetings or complete the program requirements resulted in referral to UI for adjudication.

The U.S. DOL stipulated that states in the REA program must have a method to identify a valid comparison group, and Rhode Island initiated a comparison group in 2009. Between 2009 and 2014, the control group was roughly the same size as the treatment group. After this period, Rhode Island reduced the control group to 20 percent of the REA-eligible pool. Therefore, we will focus our analysis on the period 2009-2014. For full details on selection for REA participation in Rhode Island, see Appendix B.

The description below provide details on the REA selection process and program components in Rhode Island after the comparison group was established:

1. Unemployed persons claim UI benefits. Each week, RIDLT created a list of REA-eligible claimants from the population of all UI claimants who received their first payment that week. Claimants excluded from the REA-eligible list were not contacted.[9]
2. RIDLT split the pool of claimants eligible for REA into treatment and comparison groups. The stated intention was to select them at random. The list of treatment claimants was

[9] See Appendix A for discussion of how the REA-eligible pool of claimants was constructed from the population of all UI claimants.

further split by residential zip-code and distributed accordingly to *net*WORKri centers. The control group claimants were not contacted.

3. Each *net*WORKri center selects claimants from the assigned treatment list up to a capacity constraint. Some centers may not have been able to select all listed claimants. Selected claimants were sent letters notifying them to report to the *net*WORKri center on a scheduled date for REA services.
4. REA participants attended an initial REA meeting, which included an orientation and one-on-one meeting with a REA counselor. Failure to report resulted in referral to the UI authorities for adjudication.
5. REA participants schedule and attend a 30-day follow up meeting in *net*WORKri. If a participant does not show up or fails to completed requirements of the REA program, he or she is referred to UI for adjudication.

3 Data

We use anonymized administrative records from the Rhode Island Innovative Policy Lab (RIIPL) RI 360 database. All personally identifiable information has been removed from the data and replaced with anonymous identifiers. These identifiers allow researchers with approved access to the secure system to join records associated with an individual across data covering different social programs and administrative records (Hastings et al. 2018).

3.1 Unemployment Insurance System Data

We construct the REA study sample for our analysis using data on UI claims from the Rhode Island Department of Labor and Training (RIDLT). From 2005 to 2015, there were 86,293 claims in which the claimant was selected for REA or was assigned to the REA control group. We impose three restrictions on this claims data. First, we remove all claims associated with REA participants

who filed for UI before RIDLT created the REA control group in June 2009. Second, we remove REA participants and control group claims filed after June 2014. After this point, the size of the control group became disproportionately small relative to the REA participant group. Third, we exclude claims for which the claimant was mandated to receive REA services or mandated to be excluded from REA services. Specifically, we exclude claims filed by union hiring hall members, claims filed by members of the Unemployment Compensation for Ex-Service Members program, and claims filed by new entrants to the Federal Emergency Unemployment Compensation program.[10] The final REA study sample for our analysis consists of 39,243 claims; there are 18,914 and 20,329 claims in the REA and comparison groups, respectively.

The UI system records provide several measures that we utilize in the analysis. Many of the demographic variables are provided from claim information including age, race, gender, ethnicity, educational attainment, and number of dependents. The claims data also contains information on labor market history, including the total base period earnings, the number of base period employers, and the Standard Occupational Classification (SOC) code for the claimant's most recent job.[11] We use the SOC code to construct a measure of skills transferability for each claimant.[12] From the payment records, we obtain key outcomes such as the total amount of UI benefits collected, whether the claimant exhausted their regular UI benefits, and the duration on UI.

[10] EUC claimants who transitioned from tier one to tier two benefits during this time frame were also required to receive REA services.

[11] The base period is the time period administrators consider when determining if a claimant has sufficient earnings to be monetarily eligible to receive UI benefits. The regular base period in Rhode Island is the first four of the last five completed quarters. An alternate base period, which is the last five completed quarters, can also be used to meet the earnings requirement (RIDLT 2018).

[12] Intuitively, this measures the similarity of the knowledge required for a particular occupation to the knowledge required for other jobs in the labor market. For detailed information on how we construct this index, please see Appendix Section C.

We also use quarterly earnings data from the UI systems.[13] We construct employment and earnings outcomes in the quarters following UI enrollment as well as measure how many quarters since the claimant last worked in Rhode Island. The earnings data also enable us to identify the primary North American Industry Classification System (NAICS) code for the former employer of a claimant. We use the NAICS code to construct indicators for blue collar, white collar, pink collar and government sector employment.

3.2 Job and Re-employment Services Data

We obtain measures of job and re-employment services based on records for programs funded by Wagner-Peyser and the Workforce Investment Act. The period covered by these records spans 2007-2017. These data include records of core REA services, such as participation in a program orientation, the initial eligibility assessment, and the provision of labor market information. The data also record receipt of other services, such as resume assistance, attending a job or skills workshop, referral to a job posting, and searching for jobs on the online jobs portal, EmployRI. Certain services must be administered in-person, while others can be conducted remotely online on EmployRI. Services conducted on EmployRI are automatically recorded, while Career Center counselors are supposed to record services administered in-person. We use these records to measure whether assignment to REA had an impact on a claimant's likelihood to receive any services and the number of services they received.

3.3 EmployRI Data

We obtain granular information on online job search activities from EmployRI website records. These data contain a measure of self-reported skills and information on the user's activity including job viewings and submitted applications. We observe EmployRI records as far back as

[13] The UI data on earnings contains virtually all employees in Rhode Island, including agricultural and domestic workers.

June 2009 through the present day. All records on EmployRI are created and stored automatically. We use these measures to explore potential mechanisms behind heterogeneous treatment effects.

3.4 Crime Data

We create measures of criminal history, incarceration, arrests, and traffic stops using anonymized records from the Rhode Island Department of Corrections (RIDOC) (covering 1997-2016) and state police agencies (covering 2007-2017). The data on arrests and traffic stops is from the Rhode Island State Police (RISP) and eight municipal police precincts. These nine agencies provide information that covers 44 percent of the RI state population for municipal policing, and all of Rhode Island for state police enforcement.

3.5 Social Services Data

We create measures of enrollment in means-tested social safety-net programs using records from the RI Executive Office of Health and Human Services systems (RIEOHHS). The data include records for participation in TANF, SNAP, Medicaid, General Public Assistance (GPA) and the Child Care Assistant Program (CCAP). The period covered by the data spans 2004 to 2016.

3.6 Disability Insurance Program Data

We create measures of receipt of disability insurance payments received using records from RIEOHHS and RIDLT. The data contain information on three programs: Supplemental Security Income (SSI), Retirement Survivors and Disability Insurance (RSDI) and Temporary Disability Insurance (TDI) programs.[14] These data cover the period from 1997-2016.

3.7 Descriptive Statistics

[14] Workers with temporary illness or physical injury can obtain short-term, partial wage replacement through Rhode Island's Temporary Disability Insurance (TDI) program. The wage replacement rate for TDI is approximately 60 percent, and workers can receive weekly TDI benefits for up to 30 weeks.

Table 1 compares the REA study sample to all successful UI claims over the study timeframe, and the broader population of Rhode Island workers who appear in the UI wage roll records in 2014. Contingent on having positive earnings recorded for all 4 quarters of 2014, the average annual earnings of Rhode Island workers was $53,595 (in 2017$), which is very similar to the most recent BLS estimate for Rhode Island of $53,110 for 2017 (U.S. BLS 2018). UI claimants and those in the REA study sample are more likely to be white, male, and blue-collar than the RI workforce overall. They have 20 to 28 percent lower average RI labor earnings, and are 30 to 75 percent more likely to be enrolled in SNAP and Medicaid. They are less likely to be on permanent disability, as UI can be drawn only if individuals meet a minimum wage earnings level in the year prior to filing.[15] Angell et al. (2018) use the RI 360 database to examine social safety-net and social insurance program use as a function of age. They find that UI programs are used more heavily among those below forty during years where labor earnings are higher, and permanent disability programs are more likely to be used after the age of forty as labor earnings decline.

4 Empirical Strategy

4.1 Identifying the Mean Impact of Assignment to REA

Consider the following model that estimates the mean intention-to-treat impact of REA participation for individual *i* who files for UI in week *t*, and lives in a region covered by REA office *l*:

$$Y_{i,l,t} = \beta_0 + \beta_1 REA_{it} + \Gamma_{l,t} + \varepsilon_{i,l,t} \quad (1)$$

[15] Currently, the standard earnings requirement to receive UI benefits is to have earned at least $12,120 in either the base period or alternate base period (RIDLT 2018).

where $Y_{i,l,t}$ is an outcome, REA_{it} is an indicator for mandatory participation in REA, $\Gamma_{l,t}$ represent fixed effects for the combination of the assignment week and the REA office location, and $\varepsilon_{i,l,t}$ is an error term.

The key assumption to identify β_1 in Equation 1 is that assignment to REA is random after conditioning on the week and office fixed effect – i.e., $cov\left(REA_i, \varepsilon_{i,l,t} \middle| \Gamma_{l,t}\right) = 0$. To investigate the validity of this assumption, we compare pre-claim (baseline) characteristics of individuals in the REA and comparison groups. We statistically test for balance using the model from Equation 1 where the dependent variable is a given baseline characteristic. If REA participation were (conditionally) randomly assigned, we would expect that the coefficient on REA in this model would be statistically indistinguishable from 0. We also test for balance by conducting a joint significance for all baseline characteristics.[16]

Table 2 provides the means for the REA and control groups for baseline characteristics in Columns (1) and (2). For each baseline characteristic, Table 2 reports the *p*-value for the estimated coefficient for the indicator for REA from Equation 1 in Column (3). These results show that we reject the null hypothesis for two out of nine of the baseline measures. In addition, we also reject the null hypothesis in a joint test of the significance of baseline characteristics. These results suggest that assignment to participate in REA was not perfectly randomized. However, the differences between the REA and comparison group characteristics are generally small in magnitude, even for the two out of nine baseline characteristics with significant differences. This suggests that government authorities attempted randomization. For example, individuals selected for REA are only 2.8 percentage points (4 percent) more likely to be white and non-Hispanic

[16] The joint significant test is conducted using all of the controls available to us, a subset of which are shown in Table 2, as well as office and week fixed effects. A full listing of all controls can be found in Appendix Table A.6.

relative to comparison group. Similarly, REA claims are only 1.2 percentage points (1 percent) more likely to have been filed by someone who reported to be a college graduate than comparison group claims.

To address any potential imbalance, we control for observables using the following augmented model to identify mean impacts:

$$Y_i = \beta_0 + \beta_1 REA_i + \beta_2 X_i + \Gamma_{l,t} + \epsilon_i \quad (2)$$

where X_i is a vector of baseline characteristics. In Equation 2, identification of β_1 relies on assuming that assignment to REA is random after conditioning on baseline characteristics and fixed effects for assignment week and office. Formally, we assume $cov(REA_i, \epsilon_i | X_i, \delta_{w(i),o(i)}) = 0$.

We select control variables included in X_i using the machine-learning algorithm Double LASSO (DL) (Belloni et al. 2012; 2014). DL is a regularized regression dimension-reduction approach that selects control variables optimally from a large number of potential variables. In our application, we allow DL to choose controls from a set of baseline characteristics that includes, race, ethnicity, age, gender, earnings, educational attainment, prior conviction, number of dependents, number of quarters since last Rhode Island employment, number of base period employers, prior industry, prior occupation, occupational skills transferability social program participation in the prior year, the maximum number of potential benefit weeks available at the time of filing, and whether the claimant used the regular or alternative base period to meet the UI eligibility requirements.[17] We also include two-way interaction terms for all baseline

[17] See Appendix Table A.6 for more details.

characteristics. The DL algorithm selects controls that predict Y_i and/or REA_i (subject to a penalty function).[18]

4.2 Causal Forest Estimates of Heterogeneous REA Impacts

We further explore treatment effect heterogeneity of the REA program using the Causal Forest (CF) algorithm (Wager and Athey, forthcoming; Athey, Tibshirani, and Wager, forthcoming; see Davis and Heller (2017a) and Hitsch and Misra (2018) for applications). This approach is a supervised machine learning technique that predicts treatment effect heterogeneity based on flexible functions of observable covariates. This method allows us to detect complex patterns of heterogeneous treatment effects that are driven by interactions of more than a single baseline characteristic.

The CF algorithm begins by creating a "tree," or partition of the covariate space. Each element of the partition, described by the region of the covariate space it contains, is referred to as a "leaf." The algorithm creates the partition recursively, splitting the covariate space into two leaves, and then successively splitting each leaf until a stopping criterion (such as minimum number of treated and control observations in each leaf) is reached. Each split is defined by a particular covariate and a cutoff value of that covariate ($W_j = w$), where observations are allocated to either "left" ($W_j \leq w$) and "right" ($W_j > w$) leaves. For each split, the algorithm considers all covariates (or potentially, a random subset of covariates) and all possible split points. The chosen split, among all candidate splits, is the one that maximizes the variance in average

[18] Specifically, Double LASSO entails running two LASSO regressions. First, the outcome is regressed on the set of predictors (without the intervention variable) to identify those predictors that are correlated with the outcome. Second, the intervention variable is regressed on the set of predictors to identify those predictors that are correlated with receiving the intervention. The union of these two sets of predictors are then included in a "Post double LASSO" OLS regression in which the outcome is regressed on the intervention variable and the selected controls (Equation 2). We run double LASSO to select a different set of variables (a different X_i) for each outcome.

treatment effects in the resultant subgroups subject to a penalty for variance.[19] In other words, the algorithm optimizes for treatment effect heterogeneity. Once no more splits can be made in the training sample, the tree is defined. The algorithm then uses a disjoint, randomly selected estimation subsample to estimate treatment effects in each terminal leaf of the tree (that is, the leaves that are not split). Multiple trees comprise a forest; each tree is created by drawing a new training sample used to define the partition of the covariate space using recursive partitioning, as well as a disjoint estimation sample. The estimated treatment effect for each individual is defined as the average treatment effect for that individual across all trees in a forest.

To implement the CF algorithm in our setting, we begin with the full sample and regress a given outcome Y_i on fixed effects for week and office of assignment, and the set of covariates selected by DL, X_i. We use the residuals from this regression as the outcome for the CF algorithm. Next, we randomly assign each observation to one of ten equally sized subsamples. For each of the ten subsamples, we use the other nine subsamples to build causal forests that have 500 trees. Of this forest-building sample (the other nine subsamples), half of the observations will be used in the training sample and half will be used in the estimation sample, with observations being shuffled between these two groups to build each tree. Once we have built a causal forest, we assign each observation in a given subsample (which was held out from the processing of creating the forest) the average predicted treatment effect for that observation from the 500 trees. We repeat this

[19] Specifically, the CF algorithm calculates the value of an objective function in each leaf. Formally, the objective function for leaf k is defined as:

$$O_k = (n_{lk} + n_{ck})(\bar{Y}_{lk} - \bar{Y}_{ck})^2 - 2\left(\frac{\widehat{Var}(Y_{lk})}{n_{lk}} + \frac{\widehat{Var}(Y_{ck})}{n_{ck}}\right).$$

where n_{lk}, n_{ck} are the number of REA and comparison group observations in leaf k, $\bar{Y}_{lk}, \bar{Y}_{ck}$ are the average outcome values for the REA and comparison groups within leaf k, and $\widehat{Var}(Y_{lk}), \widehat{Var}(Y_{ck})$ are the variances of the outcome for the REA and comparison group observations within leaf k. The CF algorithm computes whether a candidate split results in leaves where the sum of the objective function in each leaf exceeds the value for the original leaf. Formally, a split is viable as long as it satisfies the inequality $O_{left} + O_{right} > O_k$. See Davis and Heller (2017b) for additional discussion of how splits are decided.

process ten times, once for each subsample, which results in predicted treatment effects for every observation in the sample.[20]

5 Results

5.1 The Mean Impact of Assignment to REA

Tables 3 reports estimates of the mean impact of REA participation from Equation 2. The top two rows show that REA was effective at nudging individuals to receive reemployment services. The first row reports the impact of REA on the claimant's likelihood to receive any services at their One-Stop Career Services Center. The point estimate is very large: UI claimants in REA were 59.5 percentage points more likely to receive some sort of service. This represents a 536 percent (=0.595/0.111) increase relative to the control group mean. In line with this, we also detect large impacts on the number of REA services received: individuals selected for REA receive 9.367 services versus 1.825 for comparison group claimants.

Table 3 also shows that the REA program had significant impacts on the number of weeks receiving UI and the probability that a claimant exhausted their regular UI benefits. Individuals selected for REA spent 1.367 (6.3 percent) fewer weeks on UI and were 5 percentage points (11 percent) less likely to exhaust their regular UI benefits. These results are similar to the results of the contracted evaluations and other studies of REA, which found estimate on the number of weeks spend on UI between -0.6 and -3.1 weeks (Benus et al. 2008; Poe-Yamagata et al. 2011; Michaelides et al. 2012). Results in four of the six state-level REA were statistically significant.[21]

[20] This process of subsampling follows recommendations from Wager and Athey (forthcoming); in machine learning terminology, it gives us "out of bag" estimates for each observation. This approach ensures that each observation's treatment effect estimate is independent of the observation's realized outcome Y_i, except for any impact the outcome has on the initial regression coefficients in the model used for residualization. This independence can be exploited when we estimate the benefits of using a targeted assignment policy.

[21] Appendix Table A.5 summarizes the results of those studies for comparison.

For labor market outcomes, we find no significant impacts in the in the quarters after REA participation. Table 3 presents results for earnings and employment in quarters 1-2 and 3-6 following initial payment of UI benefits. Point estimates for these outcomes are small in magnitude and not significant. These findings match some of the previous evaluations of REA. Similar to our results, studies of REA in Minnesota and North Dakota found no significant impacts on earnings or employment. At the same time, the REA evaluations in Nevada, Florida and Idaho find significant and positive effects on labor market outcomes (Benus et al. 2008; Michaelides et al. 2012; Michaelides and Mueser 2018). We do find that there is a small but significant, positive impact from REA on whether a claimant switched industries (measured using 2-digit NAICS codes).

Next, Tables 4 provides estimates for impacts on social program and disability insurance participation. We find that REA had no significant impacts on the probability of participating in SNAP, Medicaid or TANF in the 1-2 or 3-6 quarters after a claimant receives their first UI payment. Notably, the point estimates are substantively small with confidence intervals that rule out economically meaningful impacts. In terms of disability insurance programs, we examine enrollment over a two-year horizon given low probability of participation in any given year. Table 4 suggests that REA had positive impacts on enrollment. We find that REA recipients were 17.6 percent (=0.003/0.017) more likely to file for Temporary Disability Insurance (TDI) in the two years following their first UI payment. REA participants were also more likely to receive income from permanent disability insurance (SSI or RSDI) in this period. Despite these impacts, we find no significant impacts on the total government program costs in the one year after entering UI, which includes income from social programs, disability insurance, and the costs associated with incarceration.

Finally, Table 5 provides additional results for the mean impact of REA on law enforcement-related outcomes. Specifically, we test for impacts on the number of traffic stops for speeding and the likelihood of arrest or incarceration in the two years following the claimant's first UI payment. We find no significant impacts of REA. The point estimates are negative, but very close to zero. They suggest that, if anything, REA participants are slightly less likely to have interactions with law enforcement.

As a robustness check for these results, Appendix Table A.3 tests whether REA participants are more likely to move from Rhode Island. This differential attrition would threaten the interpretation of our findings since the analysis relies on state-specific administrative data. We measure an individual as "present in the RI administrative system" for a given month if they have at least one record generated in that month from any one of the government agency systems included in the RI 360 database. Table A3 shows there is no evidence that REA participants are more or less likely to live in Rhode Island in the one or two years after entering the UI system.

5.2 Basic Subgroup Estimates of Heterogeneous REA Impacts

Before exploring treatment effect heterogeneity with Causal Forest, we start by estimating effects for subgroups based on a traditional approach of cutting the sample based on single covariates selected by using economic intuition to guide the choice of characteristics plausibly correlated with treatment effects. For our specific analysis, we divide the data by race (minority versus white and non-Hispanic), sex, age (above and below 40), criminal history (no arrests versus at least one) and skill transferability (low versus high).

Appendix Tables A.1 and A.2 provide estimates from Equation 2 for each subgroup. We find large impacts of the REA program on receipt of any services and on the number of services received across all subgroups. While these estimates all positive and significant, the results suggest

that there is heterogeneity in impacts across subgroups. For example, we can reject the hypothesis that treatment effects on the likelihood of receiving services is the same for minority and white, non-Hispanic claimants. Similarly, we reject the equality of impacts on the number of services receive for females and males.

Next, our analysis reveals some notable heterogeneity in terms of impacts on UI-related outcomes. For example, those under the age of 40 who received REA spend nearly 1.7 fewer weeks on UI and are 6.4 percentage points less likely to exhaust their regular UI benefits. These reductions are much larger in magnitude compared to those 40 and older who spend just 1.1 fewer weeks on UI and are 4.1 percentage points less likely to exhaust their regular UI benefits. A formal test rejects the hypothesis of equal treatment effects for claimants above and below age 40 for both of these UI-program outcomes.

In terms of post-program outcomes, our main finding is that some subgroups appear to experience different treatment effects on earnings in quarters 3-6 after initial payment of UI. Notably, the impact of a REA appears to be different for females and males. While the subgroup-specific point estimates are not statistically significant for females or males, the effects have the opposite sign. We can reject the hypothesis of equal impacts on earnings at the five percent level. Similarly, there is a statistical difference between the point estimates for subgroups based on race. To summarize, these results suggest reemployment services may enhance earnings for some groups and diminish it for others.

5.3 Causal Forest Estimates of Heterogeneous REA Impacts

Table 6 provides summary statistics of the causal forest estimates of observation-specific treatment effects for six outcomes: receipt of services, number of services received, number of weeks spent on UI, likelihood of exhaustion of regular UI benefits, likelihood of employment in quarters 3-6

and earnings in quarters 3-6. For each of these outcomes, Column (1) shows that the mean causal forest estimates of individual treatment effects are similar to the mean impacts reported in Table 3. At the same time, Columns (2) through (6) show report percentiles of the distribution of causal forest estimates. Inspection of these percentiles reveals that there is wide range of individual treatment effects. For example, the interquartile range of the treatment effects on earnings is about $950. This is in line with the subgroup analysis in the prior section, which suggested that REA helps some groups and hinders others.

To illustrate how the causal forest estimated treatment effects vary with individual characteristics, Figures 1-5 provide histograms of the observation-specific treatment effects for four main outcomes: receipt of services, weeks spend on UI, employment in quarters 3-6 and earnings in quarters 3-6. We shade each bar of the histogram according to the fraction of observations in that treatment effect bin who have a given demographic characteristic of interest. These histograms illustrate both the distribution of treatment effects as well as the correlation with certain demographic characteristics of interest.

Panel A of Figure 1 shows there is significant homogeneity in the treatment effect on receipt of services, which is correlated with race and ethnicity. Those observations with the largest magnitude effects are much less likely to be white and non-Hispanic. Similarly, Panels B provides the distribution of treatment effects on weeks receiving UI and shows that those with larger negative treatment effects are much less likely to be white and non-Hispanic. Panels C and D provide some evidence that this reduction in UI duration is also associated with positive treatment effects for employment and earnings; claimants with positive impacts on labor market outcomes are more likely to be non-white or Hispanic. This pattern of impacts is reminiscent of our findings in Appendix Table A.1.

Figures 2-5 conduct a similar visual exercise with gender, age (colored by the proportion of claims filed by individual 40 or older), criminal history and occupational skills-transferability, respectively. Most of the results show that the impact of receiving a REA varies in a monotonic way with background characteristics. Visually, this means that the left and right tails of the distribution of treatment effects are often distinctly different colors. This reflects the fact that claimants at either end of the distribution of treatment effects have the opposite background characteristic. At the same time, there are some non-monotonic relationships. For example, Panel B of Figure 3 shows that individuals with both the largest positive and negative treatment effects are more likely to be over the age of 40. There is also a similar non-uniform pattern of treatment effects on earnings and criminal background. Those with the largest negative and positive effects are less likely to have a prior conviction.

6 Policy Simulations

The causal forest estimates point to significant heterogeneity in the effects of REA receipt. Policy makers could make use of this information to implement a more efficient, "optimized" REA program (or other program) in which only those with desirable intervention impacts receive treatment. One issue to keep in mind is that there are potential tradeoffs associated with any given targeting proposal. The results in Figure 6 and 7 illustrate the tradeoffs in our specific setting. Figure 6 plots binned averages of the estimated treatment effects for earnings (y-axis) and the number of weeks spent on UI (x-axis). It is clear that observations with higher positive impacts on wages also have larger negative impacts on weeks of UI. At the same time, the histograms in Figure 7 show that shows many individuals who have the largest reductions in the duration of UI are also individuals who have large negative treatment effects on earnings (see the blue area in

Panel A). Prioritizing one outcomes when selecting which workers to target will necessarily limit benefits realized for another outcome.

In Figure 8, we plot the predicted outcomes for one possible implementation of optimized REA: participants are those individual who have positive predicted treatment effects on earnings, and those with higher positive predicted treatment effects are prioritized over those with smaller positive treatment effects in order to satisfy capacity constraints. Such a targeted intervention is a policy innovation that is well suited for machine learning algorithms such as causal forest, where predicting outcomes accurately and flexible as a function of observable characteristics is more important than interpreting underlying behavioral causes of outcome heterogeneity. Our simulation indicates that by following this approach to targeting, the impact of REA on cumulative earnings would have been +$11,218,436 compared to non-targeted impact of -$1,134,180. As to be expected from the discussion above, improvements realized along other dimensions are less substantial: the targeted policy would result in -25,668 weeks spent on UI and a -$1,070,240 change in government program expenditures in the subsequent year, compared to realized effects of -23,297 weeks and -$887,845 from non-targeted REA.

Table 7 breaks down how earnings are predicted to change under Targeted REA by key subgroups. Members of both racial groups gain under Targeted REA. Total number of letters sent declines slightly and mostly white & non-Hispanic as negative treatment effects are more likely to occur for that subgroup. Gains from targeting increase the most for white & non-Hispanic, though predicted earnings increases per letter sent for both groups is high. Letters sent to Males decrease, while those sent to females increase, but earnings gains are high for both groups. Gains by Prior Criminal History accrue mainly to non-convicts overall, but this is driven by relative subpopulation sizes. Per letter sent, the average earnings gain from Targeted REA is $676 for those with prior

criminal history, and $737 for those with no prior conviction. Finally, more letters are sent to those with high skills transferability, while letters are reduced for those with low skills transferability. That said, per capita gains are larger at $936 for low skills transferability versus $579 for high skills transferability as the targeting sends letters to the low transferability upper tail in Figure 5.

7 Exploring Job Search Behavior and Treatment Effect Estimates

In Appendix Table A.4, we investigate potential mechanisms for heterogeneous treatment effects using the results from CF and data from EmployRI.[22] We look for regression correlations between treatment effects and activities undertaken after reporting for job services training. It may be the case that estimated treatment effects are positively correlated with different job placement services, or that those who will have a high treatment effect select into using more services.

The cells in columns (A) – (C) and rows (1) – (3) report the coefficients from OLS regressions. Row 1 shows that while the treatment effect on the number of services received is strongly predictive of the treatment effect on other outcomes, but the magnitude of this relationship is very small. For instance, it would require an increase of more than 12 standard deviations in the treatment effect on the number of services to realize a 1 standard deviation increase in the treatment effect on earnings. Rows (A) and (B) estimate impacts using data for online job search behavior, the number of self-reported skills and the number of jobs applied to (on EmployRI) in the year following UI entrance, respectively. We use these as measures of active engagement with job placement services. None of the estimates reported in rows (2) and (3) are economically meaningful. One interpretation of these results is that our measures of job search behavior and

[22] Note that the EmployRI data is available only for a subset of individuals in the REA study sample.

skills suffers from measurement error, thereby leading to attenuation bias. Either way we do not find much evidence linking activities undertaken at the one-stop center to estimated earnings benefits from visiting the center under the REA program.

8 Conclusion

This paper uses new machine learning techniques and comprehensive administrative data to measure heterogeneity in treatment effects of re-employment services provided through REA. We use the Causal Forest (CF) machine learning algorithm to predict individual treatment effects (Wager and Athey, forthcoming). We find sizable fractions of our sample have positive and substantively large treatment effect for short-term employment and earnings. These effects are hidden when considering the mean impacts of the REA intervention (which had no detectable impact on labor market outcomes). We show that the heterogeneity that we detect varies with basic demographics such as gender, race, and measures of skills transferability for an individual's prior occupation. Specifically, whites, males, and individuals with low skill transferability have treatment effects associated with negative outcomes – longer time on UI, lower employment, and lower earnings in the year after an unemployment spell.

We use the CF treatment effect estimates to simulate an improved targeting system for re-employment services. We simulate targeting services to individuals with positive predicted earnings impacts, and show that the targeted approach would have *decreased* the total number of weeks spent on UI by 2,371 *and* led to an *increase* in post-treatment earnings of over $12.3 million within six quarters of claim filing. We show that gains accrue to individuals of all subgroups, as the CF algorithm selects individuals with positive treatment effects in all subgroups and excludes those with negative expected treatment effects. Our results suggest that policy experimentation

coupled with machine learning applied to administrative data may provide inexpensive and accurate ways to improve the efficiency and effectiveness of important social policy programs, and these benefits improve upon the efficiency that can be attained using a simpler approach of selecting individuals based on pre-defined subgroups.

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Table 1: Characteristics of RI Workers, UI Claimants and REA Study Participants

	(1) Population Working in Rhode Island, 2014	(2) Successful UI Claims	(3) REA Study Sample
White	0.790	0.859	0.854
	[293,096]	[210,226]	[36,224]
Male	0.480	0.560	0.566
	[495,788]	[228,182]	[39,243]
Age	40.701	42.782	41.973
	[495,561]	[228,180]	[39,242]
Blue Collar	0.294	0.450	0.383
	[562,161]	[228,176]	[39,242]
White Collar	0.477	0.350	0.389
	[562,161]	[228,176]	[39,242]
Pink Collar	0.135	0.133	0.192
	[562,161]	[228,176]	[39,242]
Government Sector	0.028	0.009	0.005
	[562,161]	[228,176]	[39,242]
Annual RI Earnings	39132.614	28611.097	26258.464
	[562,161]	[228,182]	[39,243]
SNAP in Prior Year	0.107	0.164	0.188
	[561,931]	[228,182]	[39,243]
Medicaid in Prior Year	0.116	0.152	0.178
	[561,931]	[228,182]	[39,243]
Permanant Disability	0.013	0.007	0.008
Income in Prior Year	[561,931]	[228,182]	[39,243]
Any Disability Income	0.017	0.022	0.022
in Prior Year	[561,931]	[228,182]	[39,243]

Note: Column 1 summarizes characterstics of working individuals who appear in the Rhode Island UI earnings data in 2014. Column 2 summarizes characteristics of successful UI claims during our study time frame. Column 3 summarizes characterstics of the REA participant and comparison groups in our study. Sample sizes are shown in brackets below each mean. Earnings are reported in 2015$. For the population working in Rhode Island in 2014, annual earnings is the total of Rhode Island earnings in 2014. For successful UI claimants and the REA study sample, annual earnings is the earnings amount reported as part of the UI claim (for the regular or alternative base period). Race, age, gender and social program participation are taken from the RIIPL RI360 database and anonymized Department of Human Services case records. Earnings and work history information are from anonymized UI claims and earnings records.

Table 2: Balance in Baseline Characteristics

	(1) Comparison Group Mean	(2) REA Participant Mean	(3) p-value of Difference
# of Dependents	0.492	0.504	0.130
White & Non-Hispanic	0.686	0.714	0.000
Age	42.04	41.90	0.476
Male	0.568	0.564	0.355
Profile Score	7135	7125	0.180
Base Period Earnings	26279	26240	0.379
Occupational Skills Transferability	0.603	0.602	0.781
High School Grad	0.852	0.864	0.005
College Grad	0.215	0.216	0.898
Joint p-value		0.000	

Note: Regressions include fixed effects for the week of initial payment and the REA office to which a claimant was assigned. Standard errors are clustered by individual. "Profile score" is a value computed by RIDLT from claim information that measures of how likely a claimant is to exhaust their UI benefits. Earnings are expressed in terms of 2015 dollars. Measures are from anonymized UI claims records and the RIIPL RI360 database.

Table 3: Treatment Effects of REA Participation

	(1) Comparison Group Mean	(2) Treatment Effect
Receipt of services	0.111	0.595***
		(0.004)
# of Services Received	1.825	9.367***
		(0.133)
Weeks of UI Receipt	21.654	-1.367***
		(0.141)
Exhaustion of Regular UI Benefits	0.426	-0.050***
		(0.005)
Employed (=1) in Quarters 1-2	0.704	0.005
		(0.004)
Employed (=1) in Quarters 3-6	0.774	0.001
		(0.004)
Switched 2-digit NAICS Sector	0.256	0.013**
		(0.005)
Earnings in Quarters 1-2	7460.704	101.296
		(78.875)
Earnings in Quarters 3-6	17243.516	-47.385
		(163.545)

Note: Significance reported as *** p<0.001; ** p<0.01; * p<0.05; + p<0.10. Regressions include fixed effects for the week of initial payment and the REA office to which a claimant was assigned, and all controls selected by Double Lasso. Standard errors are clustered by individual. Earnings are expressed in terms of 2015 dollars. Outcome measures are taken from anonymized One Stop Career Center service records and anonymized UI payments and earnings records.

Table 4: Treatment Effects of REA Participation - Social Program Outcomes

	(1) Comparison Group Mean	(2) Treatment Effect
SNAP in Quarters 1-2	0.178	-0.001
		(0.002)
SNAP in Quarters 3-6	0.196	0.001
		(0.003)
Medicaid in Quarters 1-2	0.119	0.002
		(0.002)
Medicaid in Quarters 3-6	0.116	0.001
		(0.002)
TANF in Quarters in 1-2	0.010	0.001
		(0.001)
TANF in Quarters in 3-6	0.015	-0.001
		(0.001)
TDI Receipt, 2 Years	0.017	0.003*
		(0.001)
Permenant Disability Benefit	0.021	0.003+
Receipt, 2 Years		(0.001)
Any Disability Benefit Receipt,	0.036	0.005**
2 Years		(0.002)
Government Program	2255.191	-51.566
Expenditures, 1 Year		(42.299)

Note: Significance reported as *** p<0.001; ** p<0.01; * p<0.05; + p<0.10. Regressions include fixed effects for the week of initial payment and the REA office to which a claimant was assigned, and all controls selected by Double Lasso. Standard errors are clustered by individual. "Permenant Disability Benefit Receipt" measures receipt of SSI or RSDI. "Any Disability Benefit" measures receipt of TDI, SSI, or RSDI. "Government program expenditures" captures payments from SNAP, TANF, Medicaid, GPA, CCAP, SSI, RSDI, and TDI, and is expressed in terms of 2015 dollars. Outcome measures are taken from anonymized case records from the Rhode Island Departments of Human Services, and Labor and Training

Table 5: Treatment Effects of REA Participation - Law Enforcement Outcomes

	(1) Comparison Group Mean	(2) Treatment Effect
Speeding Stops, 2 Years	0.062	-0.002
		(0.003)
Arrested, 2 Years	0.038	-0.001
		(0.002)
Incarcerated, 2 Years	0.020	-0.001
		(0.001)

Note: Significance reported as *** p<0.001; ** p<0.01; * p<0.05; + p<0.10. Regressions include fixed effects for the week of initial payment and the REA office to which a claimant was assigned, and all controls selected by Double Lasso. Standard errors are clustered by individual. Outcome measures are taken from anonymized case records from the Rhode Island Department of Corrections, and Rhode Island law enforcement agencies.

Table 6: Summary of Predicted Treatment Effects

	(1) Mean	(2) 10th Percentile	(3) 25th Percentile	(4) 50th Percentile	(5) 75th Percentile	(6) 90th Percentile
Receipt of Services	0.556	0.486	0.521	0.558	0.594	0.629
Number of Services Received	8.715	6.313	7.215	8.498	10.034	11.503
Weeks of UI Receipt	-1.262	-2.456	-1.771	-1.205	-0.673	-0.110
Exhaustion of Regular UI	-0.047	-0.090	-0.068	-0.045	-0.025	-0.007
Employed (=1), Q3-Q6	0.001	-0.028	-0.014	0.001	0.015	0.029
Earnings, Q3-Q6	-55.189	-1036.781	-521.122	-49.730	430.478	960.825

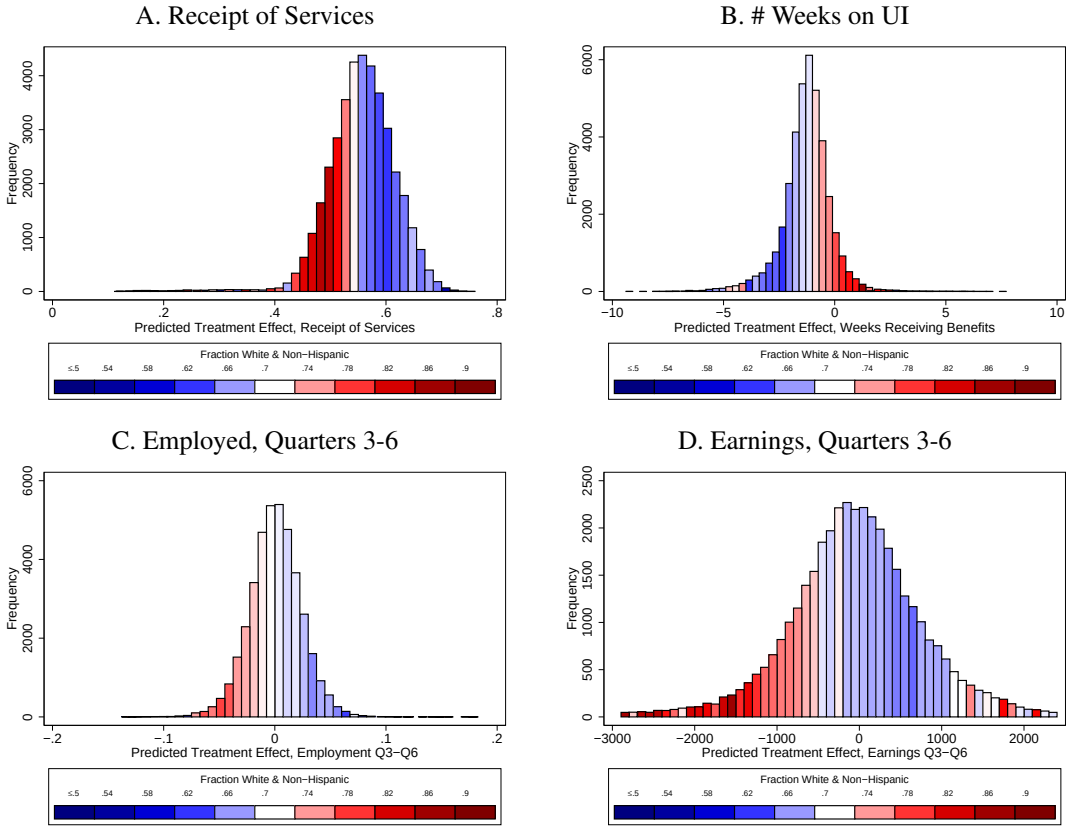
Note: Treatment effect estimates come from causal forests of 500 trees with the following features: an indicator for being white and non-Hispanic, age, gender, indicators for highschool and college graduation, an indicator for a prior conviction, number of dependents, indicators for being on SNAP, Medicaid, SSI and RSDI in the year prior to UI, quarters elapsed since most recent employment, number of employers in base period, indicators for blue/white/pink collar industry of employment, an indicator for employment in government sector, 2-digit SOC occupation code indicators, occupational skills transferability, profile score, an indicator for whether UI eligibility was determined using the regular base period, base period earnings and the maximum potential weeks of UI benefits at time of first payment. See Appendix Table A.6 for more details. The treatment effect on earnings is expressed in 2015$. Outcomes and features come from the RIIPL RI360 database, anonymized case records from the Rhode Island Departments of Corrections, Human Services, and Labor and Training, as well anonymized UI earnings, payments, and claims records.

Table 7: Actual and Simiulated REA Program Impacts by Sugroup

		Number Assigned to Program		Aggregate Impacts (TE) on Q3-Q6 Earnings ($)	
		(1) Actual REA	(2) Targeted REA	(3) Actual REA	(4) Targeted REA
Characteristic	*Subgroup*				
Race & Ethnicity	White & Non-Hispanic	13,228	11,426	-1,291,830	7,827,963
	Non-White or Hispanic	5,323	5,439	157,649	3,390,473
Gender	Male	10,493	8,520	-1,672,257	5,816,978
	Female	8,058	8,345	538,077	5,401,457
Age	40 or Older	10,148	9,393	-375,649	6,794,866
	Under 40	8,403	7,472	-758,531	4,423,570
Criminal History	Prior Conviction	1,461	1,188	-186,678	615,890
	No Prior Convictions	17,090	15,677	-947,502	10,602,546
Skills Transferability	High Transferability	9,149	9,600	665,636	6,221,141
	Low Transferability	9,402	7,265	-1,799,816	4,997,295

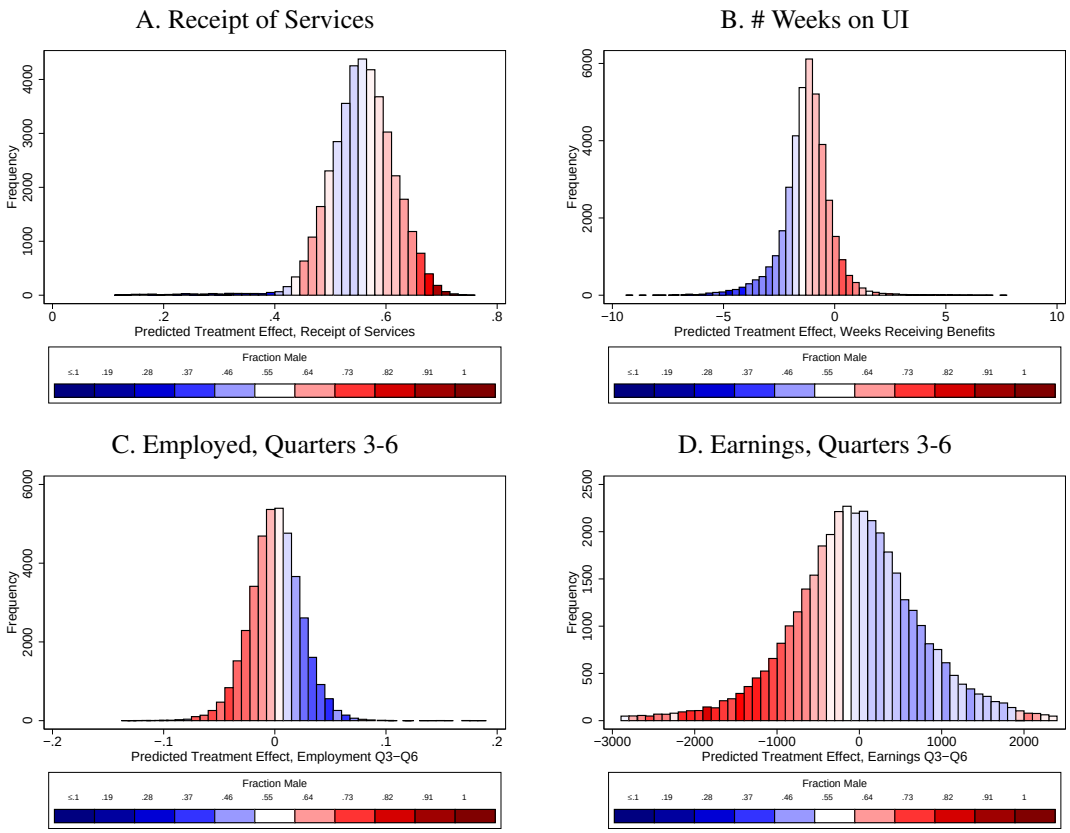
Note: Under "targeted REA", only the individuals whose predicted treatment effect is positive for Q3-Q6 earnings are eligible to be assigned to REA. Additionally, "targeted REA" assumes capacity constraints and thus assigns no more individuals to REA than were actually assigned, so only individuals with the greatest predicted treatment effect for Q3-Q6 earnings are included. Under "Actual REA", 48.2% of claimants (N = 18,551) are assigned to REA; 43.9% (N = 16,865) are assigned to REA under "targeted REA". Earnings impacts are expressed in 2015$. A small number of observations did not have a Q3-Q6 earnings treatment effect estimated for them, and are excluded from this simulation.

Figure 1: Distribution of Predicted Treatment Effects with Race & Ethnicity



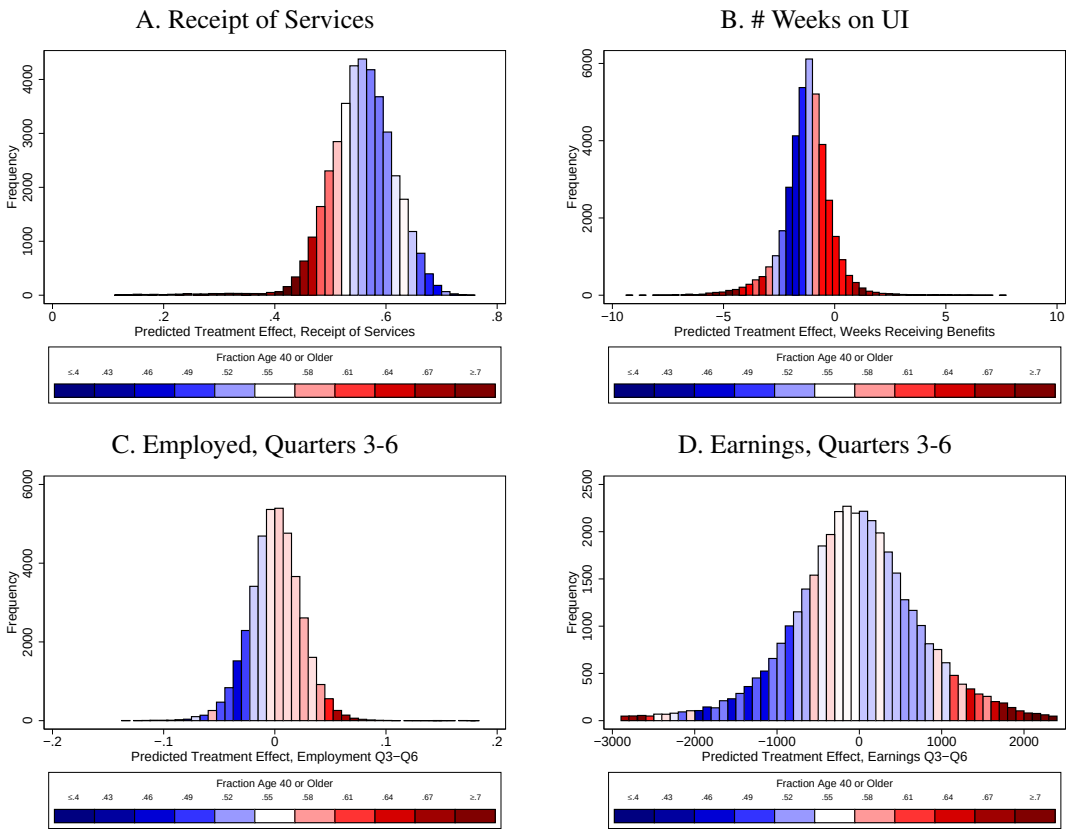
Note: Treatment effect estimates come from causal forests of 500 trees with the following features: an indicator for being white and non-Hispanic, age, gender, indicators for highschool and college graduation, an indicator for a prior conviction, number of dependents, indicators for being on SNAP, Medicaid, SSI and RSDI in the year prior to UI, quarters elapsed since most recent employment, number of employers in base period, indicators for blue/white/pink collar industry of employment, an indicator for employment in government sector, 2-digit SOC occupation code indicators, occupational skills transferability, profile score, an indicator for whether UI eligibility was determined using the regular base period, base period earnings and the maximum potential weeks of UI benefits at time of first payment. See Appendix Table A.6 for more details. Individuals who are missing race or ethnicity information are categorized as "non-white or Hispanic" The treatment effect on earnings is expressed in 2015$. The top and bottom 1% of treatment effects on earnings are omitted from the plot in panel D. Outcomes and features come from the RIIPL RI360 database, anonymized case records from the Rhode Island Departments of Corrections, Human Services, and Labor and Training, as well anonymized UI earnings, payments, and claims records.

Figure 2: Distribution of Predicted Treatment Effects with Gender



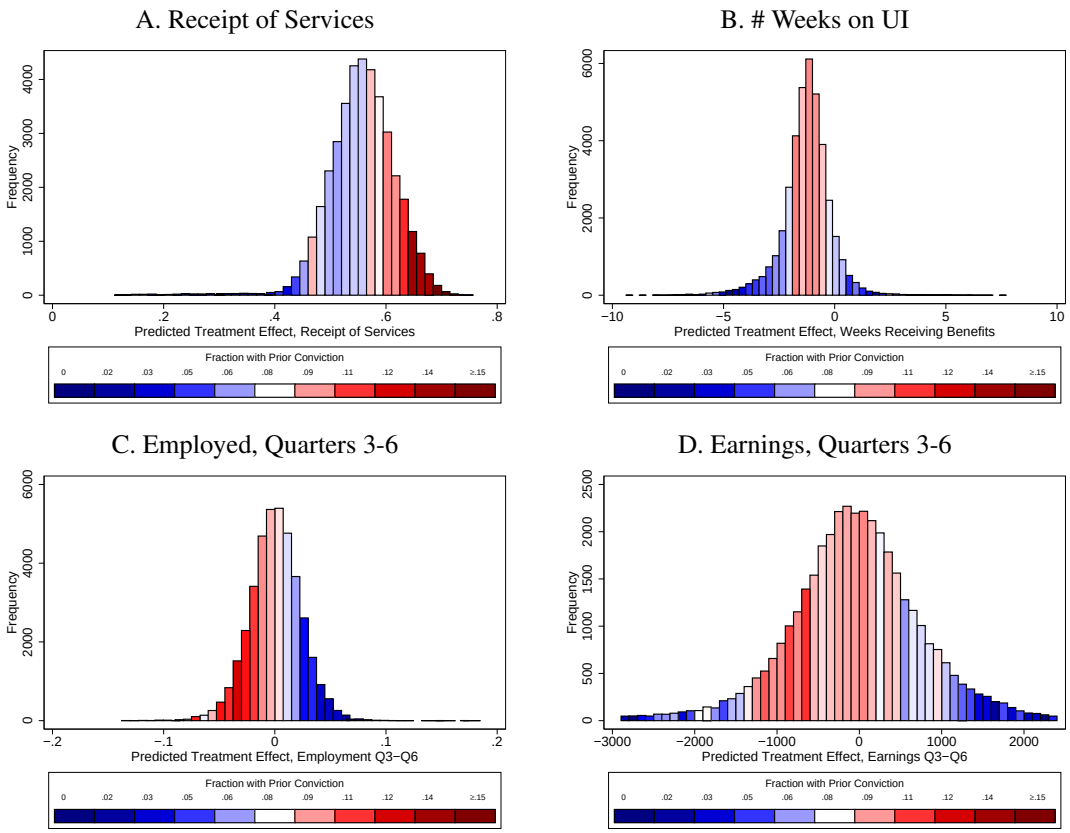
Note: Treatment effect estimates come from causal forests of 500 trees with the following features: an indicator for being white and non-Hispanic, age, gender, indicators for highschool and college graduation, an indicator for a prior conviction, number of dependents, indicators for being on SNAP, Medicaid, SSI and RSDI in the year prior to UI, quarters elapsed since most recent employment, number of employers in base period, indicators for blue/white/pink collar industry of employment, an indicator for employment in government sector, 2-digit SOC occupation code indicators, occupational skills transferability, profile score, an indicator for whether UI eligibility was determined using the regular base period, base period earnings and the maximum potential weeks of UI benefits at time of first payment. See Appendix Table A.6 for more details. The treatment effect on earnings is expressed in 2015$. The top and bottom 1% of treatment effects on earnings are omitted from the plot in panel D. Outcomes and features come from the RIIPL RI360 database, anonymized case records from the Rhode Island Departments of Corrections, Human Services, and Labor and Training, as well anonymized UI earnings, payments, and claims records.

Figure 3: Distribution of Predicted Treatment Effects with Age



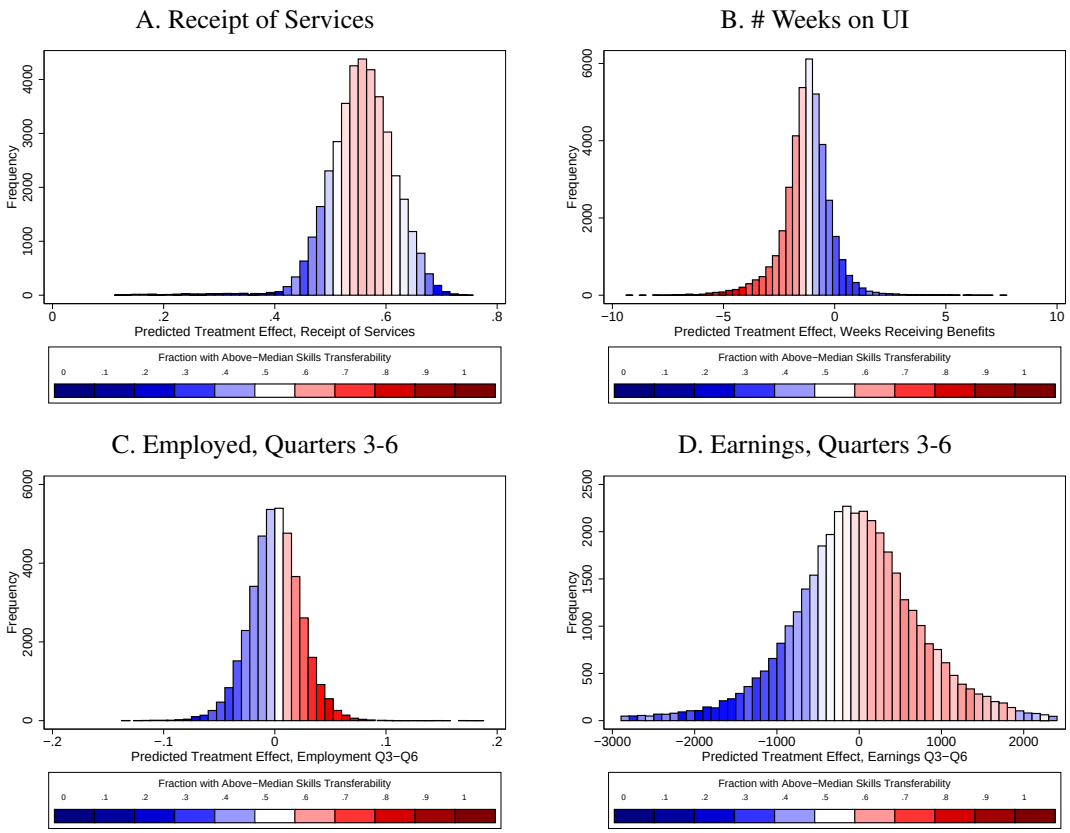
Note: Treatment effect estimates come from causal forests of 500 trees with the following features: an indicator for being white and non-Hispanic, age, gender, indicators for highschool and college graduation, an indicator for a prior conviction, number of dependents, indicators for being on SNAP, Medicaid, SSI and RSDI in the year prior to UI, quarters elapsed since most recent employment, number of employers in base period, indicators for blue/white/pink collar industry of employment, an indicator for employment in government sector, 2-digit SOC occupation code indicators, occupational skills transferability, profile score, an indicator for whether UI eligibility was determined using the regular base period, base period earnings and the maximum potential weeks of UI benefits at time of first payment. See Appendix Table A.6 for more details. The treatment effect on earnings is expressed in 2015$. The top and bottom 1% of treatment effects on earnings are omitted from the plot in panel D. Outcomes and features come from the RIIPL RI360 database, anonymized case records from the Rhode Island Departments of Corrections, Human Services, and Labor and Training, as well anonymized UI earnings, payments, and claims records.

Figure 4: Distribution of Predicted Treatment Effects with Criminal History



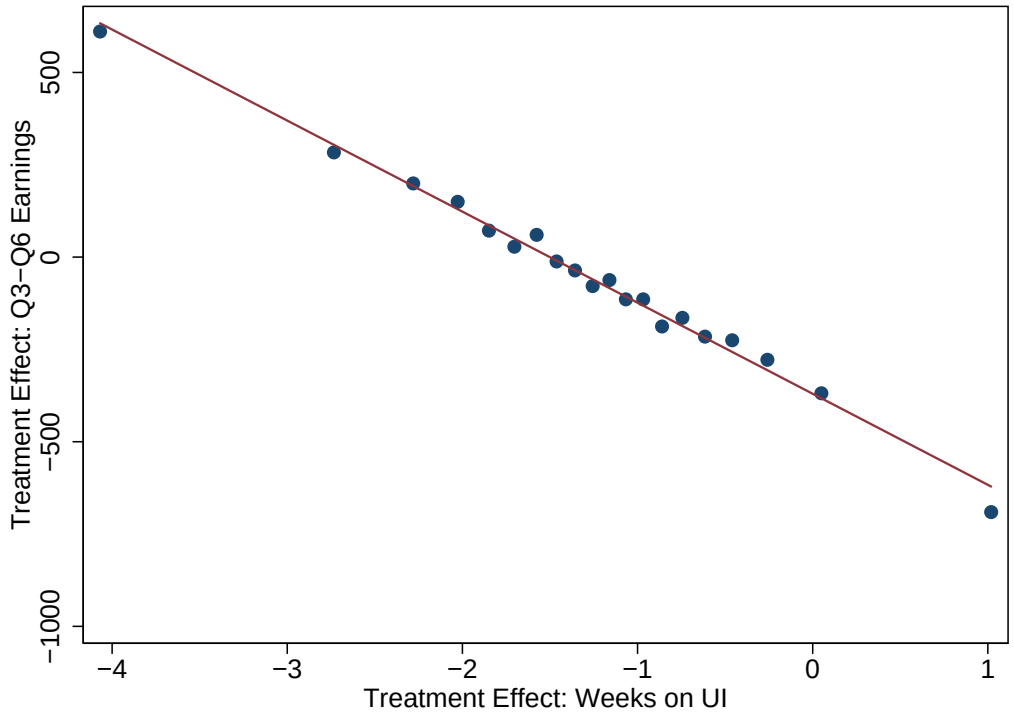
Note: Treatment effect estimates come from causal forests of 500 trees with the following features: an indicator for being white and non-Hispanic, age, gender, indicators for highschool and college graduation, an indicator for a prior conviction, number of dependents, indicators for being on SNAP, Medicaid, SSI and RSDI in the year prior to UI, quarters elapsed since most recent employment, number of employers in base period, indicators for blue/white/pink collar industry of employment, an indicator for employment in government sector, 2-digit SOC occupation code indicators, occupational skills transferability, profile score, an indicator for whether UI eligibility was determined using the regular base period, base period earnings and the maximum potential weeks of UI benefits at time of first payment. See Appendix Table A.6 for more details. The treatment effect on earnings is expressed in 2015$. The top and bottom 1% of treatment effects on earnings are omitted from the plot in panel D. Outcomes and features come from the RIIPL RI360 database, anonymized case records from the Rhode Island Departments of Corrections, Human Services, and Labor and Training, as well anonymized UI earnings, payments, and claims records.

Figure 5: Distribution of Predicted Treatment Effects with Occupational Skills Transferability



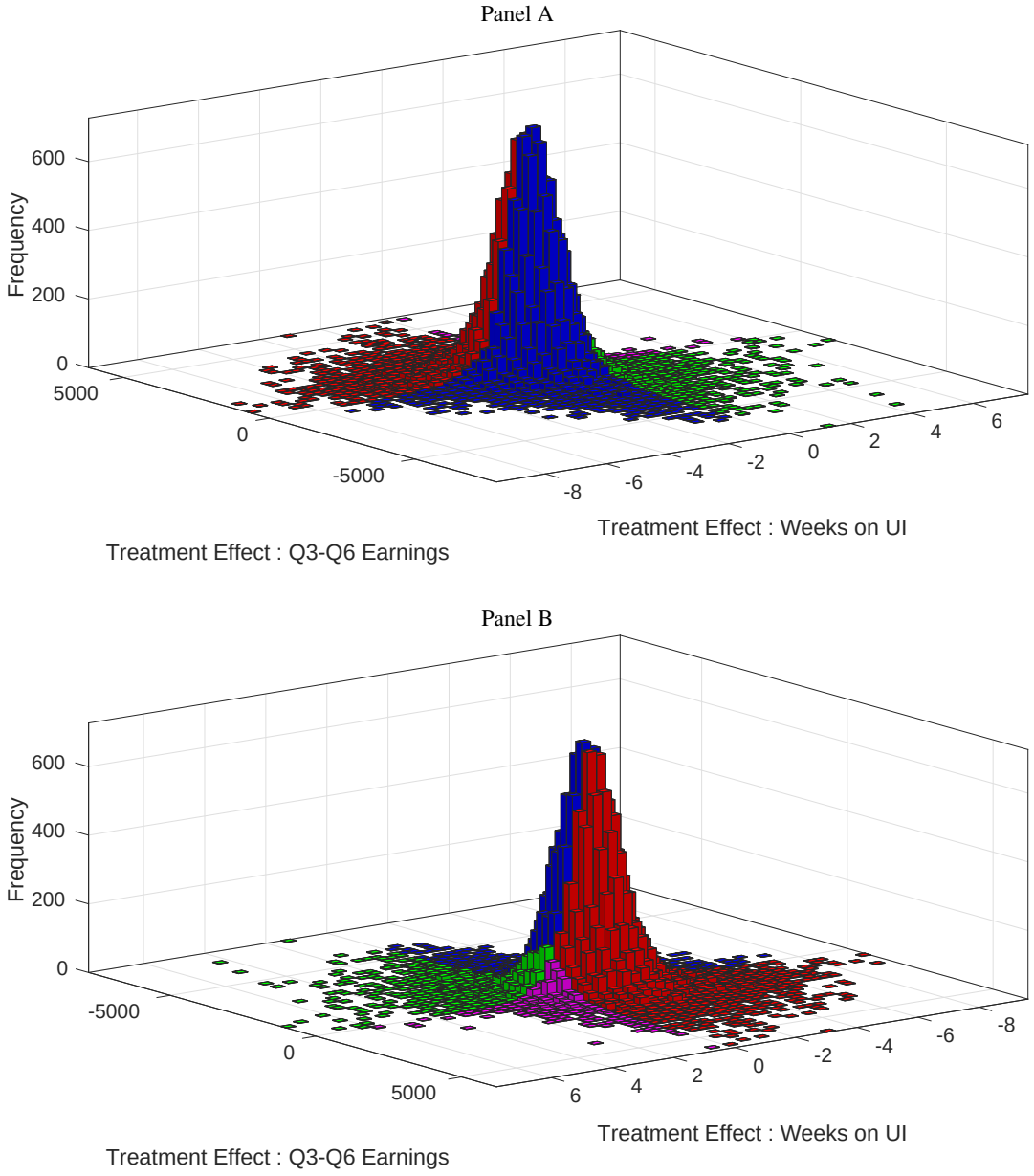
Note: Treatment effect estimates come from causal forests of 500 trees with the following features: an indicator for being white and non-Hispanic, age, gender, indicators for highschool and college graduation, an indicator for a prior conviction, number of dependents, indicators for being on SNAP, Medicaid, SSI and RSDI in the year prior to UI, quarters elapsed since most recent employment, number of employers in base period, indicators for blue/white/pink collar industry of employment, an indicator for employment in government sector, 2-digit SOC occupation code indicators, occupational skills transferability, profile score, an indicator for whether UI eligibility was determined using the regular base period, base period earnings and the maximum potential weeks of UI benefits at time of first payment. See Appendix Table A.6 for more details. The treatment effect on earnings is expressed in 2015$. The top and bottom 1% of treatment effects on earnings are omitted from the plot in panel D. Outcomes and features come from the RIIPL RI360 database, anonymized case records from the Rhode Island Departments of Corrections, Human Services, and Labor and Training, as well anonymized UI earnings, payments, and claims records.

Figure 6: Bin Scatter Plot of Treatment Effects for Weeks on UI Treatment vs. Q3-Q6 Earnings



Note: Treatment effect estimates come from causal forests of 500 trees with the following features: an indicator for being white and non-Hispanic, age, gender, indicators for highschool and college graduation, an indicator for a prior conviction, number of dependents, indicators for being on SNAP, Medicaid, SSI and RSDI in the year prior to UI, quarters elapsed since most recent employment, number of employers in base period, indicators for blue/white/pink collar industry of employment, an indicator for employment in government sector, 2-digit SOC occupation code indicators, occupational skills transferability, profile score, an indicator for whether UI eligibility was determined using the regular base period, base period earnings and the maximum potential weeks of UI benefits at time of first payment. See Appendix Table A.6 for more details. The treatment effect on earnings is expressed in 2015$. Outcomes and features come from the RIIPL RI360 database, anonymized case records from the Rhode Island Departments of Corrections, Human Services, and Labor and Training, as well anonymized UI earnings, payments, and claims records.

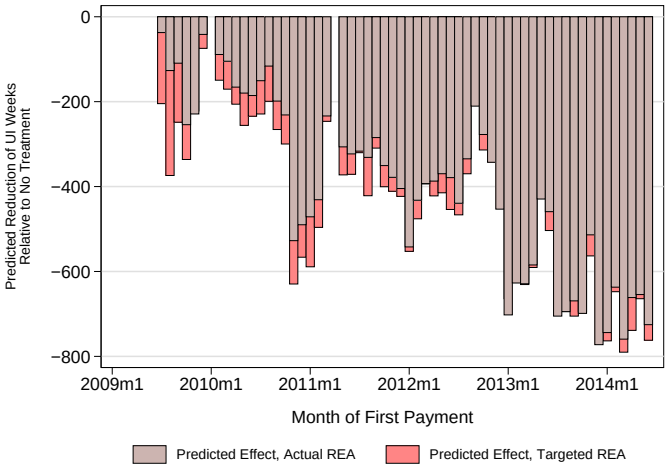
Figure 7: Joint Histogram of Treatment Effects for Q3-Q6 Earnings and Weeks of UI



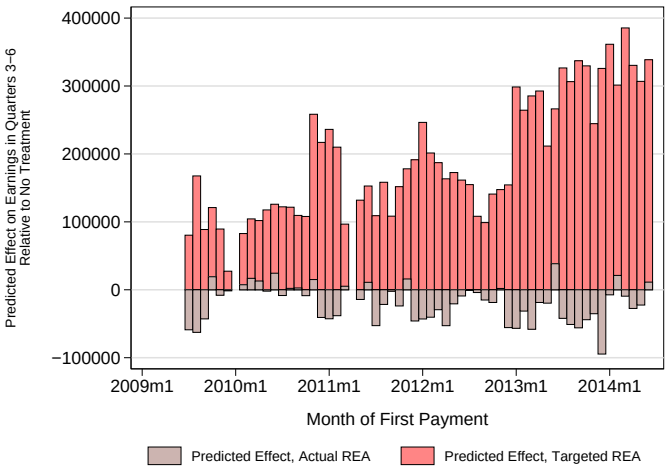
Note: Panels A and B show the same 2-dimensional histogram from different perspectives in the X-Y plane. The histogram is colored according to which quadrant of the X-Y plane it is present in: Blue signifies negative impacts on earnings and weeks of UI, red signifies positive impacts on earnings and negative impacts on weeks of UI, magenta signifies positive impacts on both earnings and weeks of UI, and green signifies negative impacts on earnings and positive impacts on earnings. Treatment effect estimates come from causal forests of 500 trees with the following features: an indicator for being white and non-Hispanic, age, gender, indicators for highschool and college graduation, an indicator for a prior conviction, number of dependents, indicators for being on SNAP, Medicaid, SSI and RSDI in the year prior to UI, quarters elapsed since most recent employment, number of employers in base period, indicators for blue/white/pink collar industry of employment, an indicator for employment in government sector, 2-digit SOC occupation code indicators, occupational skills transferability, profile score, an indicator for whether UI eligibility was determined using the regular base period, base period earnings and the maximum potential weeks of UI benefits at time of first payment. See Appendix Table A.6 for more details. The treatment effect on earnings is expressed in 2015$. Outcomes and features come from the RIIPL RI360 database, anonymized case records from the Rhode Island Departments of Corrections, Human Services, and Labor and Training, as well anonymized UI earnings, payments, and claims records.

Figure 8: Predicted Benefits From Targetted Treatment

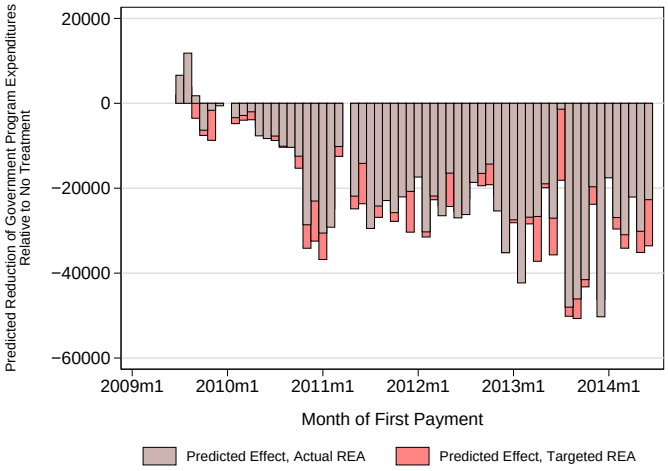
A. # UI Weeks Taken



B. Earnings in Quarters 3-6



C. Government Program Expenditures, 1 Year



Note: Under "targeted REA", only the individuals whose predicted treatment effect is positive for Q3-Q6 earnings are eligible to be assigned to REA. Additionally, "targeted REA" assumes capacity constraints and thus assigns no more individuals to REA than were actually assigned, so only individuals with the greatest predicted treatment effect for Q3-Q6 earnings are included. Under "Actual REA", 48.2% of claimants (N = 18,551) are assigned to REA; 43.9% (N = 16,865) are assigned to REA under "targeted REA". Earnings and government program expenditure impacts are expressed in 2015$. A small number of observations did not have a Q3-Q6 earnings treatment effect estimated for them, and are excluded from this simulation.

Appendix A

Table A.1: Treatment Effects of REA Participation by Observable Characteristics

	(1) Non-White or Hispanic	(2) White & Non-Hispanic	(3) Female	(4) Male	(5) Under 40	(6) 40 Or Older
Receipt of Services	0.619***	0.585***	0.571***	0.615***	0.607***	0.584***
	(0.008)	(0.005)	(0.006)	(0.005)	(0.006)	(0.006)
# Services Received	9.426***	9.298***	9.856***	9.029***	8.604***	10.000***
	(0.247)	(0.162)	(0.221)	(0.169)	(0.171)	(0.201)
Weeks of UI Receipt	-1.628***	-1.203***	-1.625***	-1.132***	-1.684***	-1.111***
	(0.276)	(0.166)	(0.234)	(0.176)	(0.202)	(0.201)
Exhaustion of Regular UI Benefits	-0.067***	-0.044***	-0.056***	-0.045***	-0.064***	-0.041***
	(0.009)	(0.006)	(0.007)	(0.006)	(0.007)	(0.006)
Employed (=1) in Quarters 3-6	0.011	-0.002	0.007	-0.006	0.002	0.000
	(0.008)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)
Earnings in Quarters 3-6	316.451	-240.713	320.322	-311.344	-41.849	-11.542
	(282.879)	(202.654)	(227.423)	(235.016)	(231.206)	(233.853)
Government Program Expenditures, 1 Year	-40.786	-50.753	-102.051	-16.902	-82.574	-14.059
	(96.000)	(47.291)	(74.771)	(50.457)	(72.821)	(49.743)

Note: Significance reported as *** p<0.001; ** p<0.01; * p<0.05; + p<0.10. Regressions include fixed effects for the week of initial payment and the REA office to which a claimant was assigned, and all controls selected by Double Lasso. Standard errors are clustered by individual. Individuals with missing race or ethnicity information are categorized as "non-White or Hispanic". Earnings and government program expenditures are expressed in terms of 2015 dollars. Outcome measures and baseline characteristics are taken from the RIIPL RI360 database, anonymized One Stop Career Center service records, anonymized UI claim, payment and earnings, records, and anonymized case records from the Rhode Island Departments of Corrections and Human Services.

Table A.2: Treatment Effects of REA Participation by Observable Characteristics

	(1) No Prior Criminal History	(2) Prior Criminal History	(3) Low Skills Transferability	(4) High Skills Transferability
Receipt of Services	0.592***	0.660***	0.590***	0.603***
	(0.004)	(0.017)	(0.006)	(0.006)
# Services Received	9.378***	9.442***	8.857***	9.928***
	(0.140)	(0.495)	(0.184)	(0.200)
Weeks of UI Receipt	-1.352***	-1.494**	-1.257***	-1.558***
	(0.148)	(0.538)	(0.197)	(0.208)
Exhaustion of Regular UI Benefits	-0.048***	-0.075***	-0.044***	-0.061***
	(0.005)	(0.022)	(0.007)	(0.007)
Employed (=1) in Quarters 3-6	0.004	-0.032+	-0.007	0.008
	(0.004)	(0.019)	(0.006)	(0.006)
Earnings in Quarters 3-6	-30.062	-334.092	-229.429	216.665
	(172.491)	(599.874)	(255.879)	(214.643)
Government Program Expenditures, 1 Year	-77.352+	193.098	-4.792	-102.797
	(43.105)	(226.055)	(58.922)	(64.230)

Note: Significance reported as *** p<0.001; ** p<0.01; * p<0.05; + p<0.10. Regressions include fixed effects for the week of initial payment and the REA office to which a claimant was assigned, and all controls selected by Double Lasso. Standard errors are clustered by individual. Individuals with missing race or ethnicity information are categorized as "non-White or Hispanic". Earnings and government program expenditures are expressed in terms of 2015 dollars. Outcome measures and baseline characteristics are taken from the RIIPL RI360 database, anonymized One Stop Career Center service records, anonymized UI claim, payment and earnings, records, and anonymized case records from the Rhode Island Departments of Corrections and Human Services.

Table A.3: Treatment Effect of REA on Likelihood to Appear in Rhode Island Administrative System

	Time Span Since Entering UI			
	1 Year		2 Years	
	Comparison Group Mean	Treatment Effect	Comparison Group Mean	Treatment Effect
Present in RI Administrative System For All Months	0.958	0.002	0.943	0.003
		(0.002)		(0.002)
Fraction of Months in RI Administrative System	0.980	0.001	0.971	0.003+
		(0.001)		(0.001)

Note: Significance reported as *** p<0.001; ** p<0.01; * p<0.05; + p<0.10. Regressions include fixed effects for week of initial UI payment. A claimant is considered to be present in the RI administrative system for a given month if at least one record exists for that claimant from that month from at least one of the administrative data sources that comprise RIIPL RI360 database, include UI earnings, DMV, voting and social program participation records.

Table A.4: Regression Correlation Between Number of Services Treatment Effect and Labor Market Outcome Treatment Effects

	Treatment Effect for :			Sample Size
	Earnings, Q3 - Q6	Employment, Q3 - Q6	Weeks on UI	
	(A)	(B)	(C)	(D)
(1) Treatment Effect on Number of Services	71.929***	0.002***	-0.073***	38,448
	(2.211)	(0.000)	(0.003)	
(2) # of Self-Reported Skills	0.058	-0.000	-0.000	12,152
	(0.130)	(0.000)	(0.000)	
(3) # of Jobs Applied To	8.556**	0.000	-0.005	4,464
	(2.905)	(0.000)	(0.004)	

Note: Each cell in columns (A) - (C) and rows (1) - (3) reports the coefficient estimate from a simple OLS regression of the outcome variable listed in the column label on the control variable listed in the row label. In the first row, the only control variable is the causal forest-estimated treatment effect on the number of services received. In the second row, the only control variable is the number of distinct skills reported on EmployRI, the state jobs portal. In the third row, the only control variable is the number of jobs applied to on EmployRI. Significance reported as *** p<0.001; ** p<0.01; * p<0.05; + p<0.10. Standard errors are clustered by individual. A small number of observations did not have treatment effects for Q3-Q6 earnings or number of services received estimated for them, and are excluded from this exercise.

Table A.5: Treatment Effects of REA from Previous Studies

State	Source	Confirmed Randomization	Impact on Weeks of UI	Impact on UI Benefits	Impact on Employment		Impact on Earnings	
Nevada	Michaelides and Mueser, 2018	Yes	-4.4** Weeks	-$1,145**	Quarter 1	0.070**	Quarter 1	$315**
					Quarter 2	0.082**	Quarter 2	$493**
					Quarter 3	0.066**	Quarter 3	$542**
					Quarter 4	0.063**	Quarter 4	$504**
					Quarter 5	0.052**	Quarter 5	$348**
					Quarter 6	0.046**	Quarter 6	$404**
	Michaelides et al., 2012	Yes	-3.13** Weeks	-$873**	Quarter 1	.074**	Quarter 1	$376**
					Quarter 2	.081**	Quarter 2	$498**
					Quarter 3	.061**	Quarter 3	$508**
					Quarter 4	.059**	Quarter 4	$473**
					Quarter 5	.055**	Quarter 5	$386**
					Quarter 6	.043**	Quarter 6	$369**
Minnesota	Benus et al. 2008	Yes	-0.9** Weeks	-$38	Probability of return to work within 6 months	1.1%	Hourly Wage at at first job within 6 months	$0.40
					# Weekly Hours at first job within 6 months	1.5*		
North Dakota	Benus et al. 2008	Yes	1.1 Weeks	-$136	Probability of return to work within 6 months	1.3%	Hourly Wage at at first job within 6 months	-$0.09
					# Weekly Hours at first job within 6 months	-2.9*		
Florida	Poe-Yamagata et al. 2011	No	-1.74*** Weeks	-$395	Quarter 1	0.012**	Quarter 1	$52
					Quarter 2	0.017**	Quarter 2	$206**
					Quarter 3	0.022**	Quarter 3	$129**
					Quarter 4	0.015**	Quarter 4	$89
Idaho	Poe-Yamagata et al. 2011	No	-1.14*** Weeks	-$262***	n/a		n/a	
Illinois	Poe-Yamagata et al. 2011	No	-0.41 Weeks	$99	n/a		n/a	

Note: Significance reported as *** p<0.001; ** p<0.01; * p<0.05; + p<0.10. We include results from regression adjusted estimates from each paper, which include controls for age, gender, race, veteran status, disability status, wages, and weeks of UI eligiblity. The "confirmed randomization" column refers to whether or not the paper confirmed balance on observables between treatment and control groups.

Table A.6: Description of Variables Used in Causal Forests

Variable	Definition
Race and Ethnicity	Indicator for being recorded as both White and non-Hispanic
Age	Age at the time of first payment
Gender	Indicator for being male
Education	Indicators for highschool and college graduation
Prior Conviction	Indicator for having a prior conviction in RI
Number of Dependents	Number of dependents recorded on UI claim
Social Program Participation	Indicators for participation in SNAP, Medicaid, SSI and RSDI in the year prior to UI enrollment
Quarters since RI Employment	Number of quarters since last observed in UI earings data in RI
Number of Employers	Number of employers in base period, as recorded on UI claim
Blue Collar	Indicator for a 2-digit NAICS code of 11, 21, 22, 23, 31, 32, 33, 42, 44, 45, 48, or 49 or a 4-digit NAICS code of 8120
White Collar	Indicator for a 2-digit NAICS code of 51, 52, 53, 54, 55, 56, 61, or 62 or a 4-digit NAICS code of 8140
Pink Collar	Indicator for a 2-digit NAICS code of 71 or 72 or a 4-digit NAICS code of 8130
Government Sector	Indicator for a 2-digit NAICS code of 92
2-digit SOC indicators	Indicators for each of the 23 2-digit SOC occupation codes
Occupational Skills Transferability	Defined elsewhere in this paper. Measures similarity of occupation to other jobs in the labor market.
Profile Score	Score computed by RIDLT as a measure of how likely a claimant is to exhaust their UI benefits
Regular base period	Indicator for UI eligibility calculated using the first four of the most recent five calendar quarters, rather than the most recent four calendar quarters
Base Period Earnings	Total base period earnings in 2015$
Maximum potential benefit weeks	The maximum weeks claimant could have potentially spent on UI

Appendix B: Details on the REA Study Sample

REA was in place in Rhode Island from 2005 until a transition to the Reemployment Services and Eligibility Assessment (RESEA) program in June 2015. The Rhode Island Department of Labor and Training (RIDLT) created a comparison group for REA in Rhode Island in June 2009. The size of the REA letter recipient and comparison groups was comparable until July 2014.[1] We focus on the period from June 2009 through June 2014 for all analysis. Unemployed workers successfully filed 228,182 UI claims during the six years that RIDLT administered the REA program with a comparison group.[2]

RIDLT created the REA letter-recipient and comparison groups after calculating UI profile scores, which measure the likelihood that an unemployed worker will exhaust their benefits.[3] Over the period for our analysis, 51 percent (N =117,407) of approved UI claims were assigned a profile score. Appendix Table B1 compares characteristics of the approved UI claims that do and do not have profile scores. For most baseline characteristics, the differences between the two groups are statistically significant. For example, scored claims are more likely to be filed by male or college-educated workers by 3.5 and 3.0 percentage points, respectively.

48,778 scored and approved UI claims were assigned to the REA selection pool, from which the REA letter recipient or comparison groups were constructed, comprising 42% of all scored and approved UI claims. After excluding certain groups of claims that were mandated to receive or not receive REA services[4], we are left with a sample 39,243 REA letter-receiving or comparison group claims, comprising 33% of all scored and approved claims. This is the sample

[1] After this point, RIDLT expanded the letter recipient group, thereby reducing the comparison group to 20 percent of the sample.
[2] During the period from June 2009 through June 2014, unemployed workers filed a total of 326,116 claims for UI benefits.
[3] RIDLT calculates the profile scores using information provided on UI application forms.
[4] See Section 3 for further details.

of claims used in our analysis. Appendix Table B2 compares recipient baseline characteristics for claims in our study sample to scored and benefit-collecting claims that are not in our study sample (either because they were not assigned to the letter-recipient or comparison groups to begin with, or because they were assigned to one of those groups, but where mandated to be included or excluded from REA services). Statistically significant differences exist between these two groups. Claims selected for the REA selection pool reported .072 fewer dependents, were 1.1 percentage points less likely to be filed by male workers, and had base period earnings that were more than $2,500 lower, on average.

After defining the REA study selection pool, RIDLT assigned claims to the REA letter recipient and comparison groups. We find that there are small, but statistically significant differences between the baseline characteristics of workers in the REA letter recipient and the comparison groups for our analysis.[5]

[5] See Section 4 and Table 2 for further details.

Table B.1: Balance in Baseline Characteristics : Scored vs. Unscored UI Claimants

	(1) Un-Scored Mean	(2) Scored Mean	(3) p-value of Difference
# of Dependents	0.539	0.545	0.029
White & Non-Hispanic	0.692	0.697	0.007
Age	43.45	42.16	0.000
Male	0.542	0.577	0.000
Base Period Earnings	29118	28133	0.199
Occupational Skills Transferability	0.603	0.603	0.092
High School Grad	0.876	0.860	0.000
College Grad	0.191	0.221	0.000
Joint p-value		0.000	

Note: Regressions include fixed effects for the week of initial payment and the REA office to which a claimant was assigned. Earnings are expressed in terms of 2015 dollars. Measures are from anonymized UI claims records and the RIIPL RI360 database.

Table B.2: Balance in Baseline Characteristics : REA Study vs Non-Study UI Claimants

	(1) non-REA Study Mean	(2) REA Study Mean	(3) p-value of Difference
# of Dependents	0.570	0.498	0.000
White & Non-Hispanic	0.695	0.700	0.141
Age	42.32	41.97	0.000
Male	0.577	0.566	0.077
Profile Score	7238	7130	0.000
Base Period Earnings	28941	26258	0.000
Occupational Skills Transferability	0.604	0.602	0.000
High School Grad	0.862	0.858	0.000
College Grad	0.220	0.216	0.000
Joint p-value		0.000	

Note: Regressions include fixed effects for the week of initial payment and the REA office to which a claimant was assigned. Earnings are expressed in terms of 2015 dollars. Measures are from anonymized UI claims records and the RIIPL RI360 database.

Appendix C. Constructing the Skills Transferability Measure

We construct a measure of skill transferability for each individual based on the 3-digit SOC code recorded their UI claim and information from the O*NET Skills, Abilities, and Knowledge data. First, using principal component factor analysis, we describe all skills, abilities, and knowledge categories using 15 orthogonal factors. Next, for each factor and pair of 3-digit occupations, we find the "distance" between occupations along each factor by taking the absolute value of the difference between factor values. We then take an average of the factor distances between each pair of occupations, weighting by the variance explained by the factor.

We transform these values into an index between zero and one by subtracting the minimum value among all pairs (which is zero by construction) and dividing by the maximum value among all pairs; to make these indexes go in an intuitive direction such that higher values mean more similarity, we subtract them from one. Finally, using BLS occupational employment records, we find the weighted average of the indexes for each occupation, weighting by the total employment in an occupation. Intuitively, this measures the similarity of the knowledge required for a particular occupation to the knowledge required for other jobs in the labor market.