Rhode Island Reemployment Services and Eligibility Assessment (RESEA) Program Evaluation:

A large randomized controlled trial

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1. Executive Summary

The Reemployment Services and Eligibility Assessment (RESEA) program is a federally funded initiative that replaced the REA program in 2015. It is designed to help unemployed individuals receiving Unemployment Insurance (UI) benefits return to work faster while strengthening program integrity. The program requires mandatory participation through meetings with state Department of Labor staff, in which participants receive eligibility assessments and reemployment services including individualized reemployment plans, labor market information, enrollment in Wagner-Peyser Employment Services, and access to other workforce resources.

States and territories are required to implement RESEA interventions and strategies that have "strong causal evidence" according to the CLEAR guidelines established by USDOL and evaluate any strategies that have not been proven to result in substantial benefits to the employment and earnings outcomes for program participants. Since 2019, programs have had the flexibility to use up to 10 percent of their annual RESEA funding to conduct evaluations of their interventions and strategies. Beginning in FY 2023, states were required to use no less than 25 percent of their grant funds for interventions or service delivery strategies that had already been determined to have strong causal evidence for improving program outcomes.

The Clearinghouse for Labor Evaluation and Research website (CLEAR) website includes 30 studies of REA and RESEA. Of those, 19 studies have high causal evidence and of those only 9 find a positive correlation with employment outcomes and 7 have positive correlations with wages/income. In this report, using a high causal evidence design, we evaluate the Rhode Island RESEA program that was in effect from February 9, 2022 to September 27, 2023. During this time, program participants were randomly selected on a weekly basis from the pool of all UI applicants and were required to attend two meetings: one that assessed their eligibility for RESEA and offered job search services and assistance, and a follow-up meeting in which the claimant showed documentation of their job search and could request other services. The study tracks 11,700 claimants in the treatment group and 11,849 in the control group for a total of five complete calendar quarters following their first UI claim. Based on state wage and UI benefit records, we estimate the causal effects of RESEA selection on three pre-registered long-term employment outcomes over this period: wages (total reported annualized income from employment), reemployment (whether the claimant became reemployed), and number of weeks spent on UI. In addition, following CLEAR guidelines, we follow the same methodology to evaluate the effect of RESEA selection on shorter-term variants of these three outcomes: the reported annualized wage in the second complete calendar quarter after the selection date, whether the claimant was reemployed during this quarter, and number of weeks spent on UI in the same benefit year as the claim.

For the preregistered long-term outcomes, we find that RESEA selection results in statistically significant positive impacts on all three outcomes. The estimated effect sizes are $1,153 higher annual wages (a 3.5% increase over the control group average), 1.47 percentage points higher reemployment, and 1.99 fewer weeks spent on UI (an 11.5% decrease compared with the control group average). The short term effects are similar; those selected for RESEA are predicted to have, on average, $1,195 higher annualized wages in the second quarter following selection (a 5.0% increase relative to the control group average), a 1.66 percentage point increase in reemployment during that quarter, and 1.77 fewer weeks on UI during the same benefit year as the claim (an 11.1% decrease compared with the control group average). Our cost estimates suggest that the program produced significant savings that justify the continued funding of the program. In particular, we estimate that the state of Rhode Island paid out about $7.9 million less in UI benefits to claimants randomized during the study period over the same benefit year as their claims. Compared to the program cost of $2.99 million during the study period, this is a significant savings for the state budget.

2. Introduction

Each month, thousands of Rhode Island residents file for unemployment after losing their jobs or having their working hours reduced. The state's unemployment insurance (UI) program provides supplemental wages while these workers seek and find new employment. Reemployment can be challenging for many individuals. Prior studies show that extended periods of unemployment undermine one's ability to meet basic needs and expenses and worsen long term economic and psychological and health outcomes (Abraham et al. 2019; Stauder 2019; McClelland; 2023).

Rhode Island has taken many steps to help un- or under-employed individuals find meaningful, secure work and boost the state economy. The Rhode Island Reemployment Services and Eligibility Assessment (RESEA) program, funded by a grant from the U.S Department of Labor (USDOL) as part of the federal RESEA program, provides access to services such as skills assessment, career counseling, job search assistance, resume building, interview preparation, and referrals to job training programs and other skill-building opportunities to help UI claimants in Rhode Island find new employment. The program, run by the Rhode Island Department of Labor and Transportation (DLT), regularly assesses the eligibility of individuals for continued unemployment benefits, including verification of the requirement that they are actively seeking work.

RESEA programs began in states and territories across the United States in 2015, replacing Reemployment and Eligibility Assessment (REA) programs that placed less of a focus on reemployment services and more emphasis on verifying eligibility for UI payments. The USDOL established RESEA in response to causal evidence from a 2009 randomized controlled trial (RCT) in Nevada that bundling reemployment and eligibility services substantially improved employment outcomes for UI claimants and reduced the amount of UI benefits drawn (Michaelides et al., 2012, Manoli et al., 2018).

In this report, we detail the results from an RCT of Rhode Island's RESEA program from February 9, 2022 to September 27, 2023 (the study period) in which participants (n = 23,549) were tracked for five quarters following their selection into or out of the RESEA program. As of September 2025, none of the reports included in DOL's CLEAR database includes a complete evaluation of RESEA in any state or territory; all of the published studies evaluated REA programs or other related employment assistance programs, most recently in 2015-16 (Klerman et al., 2019). Thus, this evaluation provides valuable insight into the effectiveness of the RESEA program in Rhode Island and can likely inform the effectiveness of other state programs that are designed like Rhode Island's.

The primary research question answered by the RCT is whether RESEA selection in RI increased future earnings, drove reemployment, and decreased the number of weeks spent on UI during a period of roughly five quarters after selection. The main analyses to answer these questions were preregistered at the Open Science Foundation to ensure reported findings are not selection biased.[2]

We estimate that selection into Rhode Island's RESEA program decreased the duration of collecting UI benefits by an average of 1.99 weeks in the 78 weeks following a UI claim (95% CI: [1.75 weeks, 2.24 weeks]). In addition, average wages are estimated to be $1,153 higher on

[2] https://osf.io/c9xe8/

an annualized basis (95% CI: [$166, $2,141]) and reemployment is an estimated 1.47 percentage points higher (95% CI: [0.32, 2.62]) as a result of RESEA selection. While there appears to be some heterogeneity in treatment effects across different demographics, the data indicate a reduction of time spent on UI due to RESEA selection for people across the income spectrum. This represents a meaningful cost savings for the state's UI program while improving claimants' financial stability, particularly for those living paycheck to paycheck, given that UI is not a full wage replacement. Indeed, we estimate that the decrease in the amount of UI benefits paid out to the individuals selected for RESEA during the study period within the same benefit year of their claim was 164% higher than the total expenditures of the RESEA program over the study period, suggesting that the program more than pays for itself.

3. Rhode Island RESEA Program Structure

3.1 Components and Requirements

Rhode Island's RESEA Program is designed for individuals who have recently begun collecting UI to help them identify work opportunities and find employment. The program offers services such as job search techniques, skills assessment, and job matching assistance. It also seeks to help UI claimants improve their resumes, enhance their interviewing skills, find job leads, and understand rights and responsibilities related to UI.

Individuals selected for RESEA in RI during the study period received a notification letter explaining they were required to attend an RESEA counseling session. Starting from March 28, 2022, the program began notifying participants of their requirements through both print letters and emails. If the claimant did not have a valid email address, only a print letter was sent. This notification system has continued through the time of this writing. Meetings with job coaches through RESEA were typically held virtually through BackToWorkRI.com. No virtual same day appointments were provided, but claimants could also attend appointments through in person walk-in services at the local Career Centers, though such appointments were provided first come, first serve and not always available.

Those selected for RESEA were required to make a meeting appointment within 18 days of receiving the selection letter. If the participant did not either make an appointment and meet with a job coach or report a return to work date in this time, their case was referred to adjudication and their benefits may have been suspended. Every Monday afternoon, a report was generated to identify non-compliant individuals. Job coaches updated the Employ RI system, and by Wednesday, non-compliant claims were halted. Participants were notified immediately that they must complete the program by scheduling and attending an appointment to reactivate their UI benefits. Prior to November 29, 2021, participants were only required to schedule an appointment and not required to actually attend; since that date (which is before the beginning of the study period), actual meeting attendance has been required. While UI claimants selected for RESEA were thus mandated to participate in these programs, all of these services were also available to anyone through the "Back to Work Rhode Island" program. The difference is that those selected for RESEA were required to attend.

Once an appointment was scheduled, RESEA participants received a packet via email (or mail for those without email) that outlines the program. The packet included the following information: compliance requirements, information on UI fraud, a list of services and contact information, information about job training opportunities, extensive tips for interviewing, and a job search form in which the claimant keeps track of their job search activities which RIDLT uses for compliance.

During the meeting, RESEA job coaches assessed UI eligibility and discussed the participant's ability to work. They reviewed labor market information, evaluated the participant's skills, and discussed career goals. The session included guidance on using Employ RI for job searches, resume building tailored to job specifications, and often a mock interview. If necessary, particularly if they were laid off from a declining industry, participants were given a skills assessment to help identify work opportunities in sectors with more job openings. Those who were expected to benefit from further training were encouraged to enroll in the Workforce Innovation and Opportunity Act (WIOA) program. Additionally, the program provided referrals to job training programs such as the Real Jobs Rhode Island Program and to social services for low-income individuals in need.

During the study period, a follow-up meeting was mandatory for all claimants selected for RESEA. In this second meeting, the participants were required to submit a 30-day work search verification. However, as of June 24, 2024 (after the study period), this requirement was eliminated. Instead, RESEA participants now complete a one-week job search form during the first appointment. The second appointment is now optional and focuses on exploring training opportunities rather than verifying job searches. As a result, the RESEA program evaluated here does not study the current RESEA program in Rhode Island, but rather the program that was in place during the study period with two required meetings.

3.2 Historical Selection Procedure and Evaluations.

Prior to a temporary pause in RESEA programming due to the COVID-19 pandemic in 2020, RESEA selection was based on an algorithm that was intended to select those “most likely to exhaust” their unemployment benefits. This algorithm was created for RIDLT by the USDOL and preferentially selected claimants for RESEA based on the output of a logistic regression model that took in the number of employers they had in the previous five quarters, the ratio of their highest quarterly wages to their base wages, whether they had dependents, whether they received severance pay, their educational history, and their SOC and NAICS occupation codes as input. Since the program operated in person only, and people were assigned to meetings based on availability of a job counselor at their closest RIDLT office, the algorithm was not followed exactly as adjustments had to be made based on the availability of counselors each week at the various job centers and the number of UI claimants in that geographic area. Specifically, the assignment was imperfect because often too many claimants would be chosen in a geographic area for the corresponding office to handle, due to limited availability of counselor appointments. In such situations, the next claimant located in an area with available appointments on the list would be selected.

An analysis accounting for the non-experimental design of the program during this time period was conducted by Huh et al. (2022). They found that RESEA selection appeared to have a negative relationship with wages and reemployment. However, the authors noted that this could have been driven by differences in unobserved characteristics between those selected for RESEA by the algorithm and those not selected. Given that the algorithm appeared to favor selection of highly educated individuals, the authors speculated that those who were statistically most likely to exhaust their UI benefits were also those who could afford to take longer to conduct their job search and thus be the least likely to benefit from RESEA services. To avoid such issues with unobserved confounding, an RCT was determined to be the best way to more accurately measure the causal effects of the program and meet the USDOL's requirement that RESEA programs must be evaluated in a way that achieves either a high or moderate causal evidence rating, according to CLEAR's standards (Clearinghouse for Labor Evaluation and Research, 2022).

4. Design and methods

4.1 Study design

Starting in January 2022, a randomization algorithm was run every Wednesday to assign select UI claimants in Rhode Island for the RESEA program. Each week, RIDLT produced a list of newly eligible UI applicants and randomly selected around 150 of these claimants for RESEA. All UI claimants who were eligible for RESEA were included in the selection pool and were either assigned to be part of the treatment group or the control group. Claimants were typically eligible for randomization during the week of their first payment. All non-veteran claimants were randomly assigned an integer between 0 and 9,999,999,999, inclusive. This randomness was designed to be uniform: each applicant's social security number was added to the six-digit microsecond timestamp at the time of assignment, and then the digits were written out in reverse to form the seed of a uniform random integer generator in COBOL. All veteran claimants were deterministically assigned 9,999,999,999 because due to regulation, they were always selected for RESEA. Initially, it was intended for the 150 claimants assigned the highest integers each week to be selected for the treatment group. This number was reduced to 135 starting on May 2, 2023 due to small control group sizes resulting from fewer than expected UI applicants. This decision was made before any of the outcome data was examined.

Fig. 01 shows the actual number of applicants selected for RESEA in each week during the study period, which spans from February 9, 2022 to September 27, 2023, inclusive. The numbers did not exactly match 150 (or 135) each week due to issues such as technical errors and holiday staffing availability for appointments. To preserve the fidelity of our conclusions based on treatment randomization, we exclude from our analysis the 502 eligible claimants on two dates in the study period — June 29, 2022 and August 2, 2023 — on which nobody was randomized due to such issues. We also exclude 27 individuals whose claims were later determined to be fraudulent as well as all 42 veterans (who were always selected for RESEA, as noted above). This leaves a total of 23,549 claimants in our final study cohort. Among these, 11,700 claimants were in the treatment group and 11,849 in the control group.

Basic demographic characteristics of the aggregate treatment and control groups are provided in Table 01. The groups are broadly similar, but we note that this does not necessarily indicate, in itself, that randomization was performed correctly. Since the randomized treatment assignment was performed on a weekly basis, and the number of UI applicants varied substantially across weeks with the number of treated claimants generally fixed across weeks (notwithstanding the issues described in the preceding paragraphs), the aggregate treatment and control groups differ in the relative weights of the populations of UI claimants across different weeks. In other words, we have a blocked experimental design where each randomization week is a block, there is complete randomization within each block, and the block sizes are unequal. Appendix A presents *weekly* balance tests which find that there is no evidence the weekly randomization created unbalanced treatment and control groups within each week.

4.2 Outcomes

We estimate the causal effects of RESEA selection on the following pre-registered long-term employment outcomes:[3] reemployment (whether the claimant became reemployed), wages (total reported income from employment), and number of weeks the recipient spent on UI. We intended to also report the effect on hourly wages, but found that the available data for hours

[3]https://osf.io/c9xe8/

worked was unreliable, with records routinely indicating that individuals worked more than 168 hours per week, and also noting that reported hours are likely not accurate for salaried employees. The wage and reemployment outcomes were computed using RIDLT's wage records. These records include all formal wages employers pay. They are reported by the employers, not the individual employee. The records do not include self-employment income, income that was not reported by an employer, or income earned out of state. Our wage outcome mirrors the computation of base wages performed by RIDLT to determine benefit rates for UI in Rhode Island. It was computed by taking the sum of the two highest quarterly wages for each claimant among the second through fifth complete calendar quarters (inclusive) following the randomization date, multiplied by two for annualization, and ranges from $0 to $630,875. Reemployment is a binary variable defined as 1 if at least two of these quarters had positive (nonzero) reported wages. Weeks on UI were estimated based on credit balances in the 78 weeks following the claim effective date, and range from 0 weeks to 51.1 weeks.[4]

One limitation of the data is the regional job market. Given the small geographic size of Rhode Island, the regional job market is tightly integrated with the neighboring states of Massachusetts and Connecticut. This along with the increasing ability for workers to telework for employers out of state suggest there are likely claimants in the data who appear to not have found work and to have no reported wages, but actually found employment out of state. For example, the 2016-2020 5-Year American Community Survey found that nearly 16% of Rhode Island workers 16 years and older commuted out of state, mainly to Massachusetts (Rhode Island Commuting Patterns). However, the resulting missingness in the wage outcomes of claimants

[4] Each UI recipient has a specific number of credits assigned when they apply for UI. Credits are the number of weeks they can receive UI in the benefit year and are based on the amount worked in the prior year. There was no matching credit balance information for 22 of the claimants in the study cohort, so we excluded them from our analysis of the effect on weeks on UI.

who end up finding work out of state should not affect the directional validity of the causal analysis. This is because randomization ensures that this missingness is balanced between the treatment and control groups (within each block), unless RESEA selection differentially helps or hinders finding employment out of state. We think the latter is unlikely; as a proxy for finding out-of-state employment, we examined whether individuals failed to exhaust their UI benefits in the benefit year of their claim while reporting $0 in wages for all of the five complete quarters after randomization. There were 3,325 claimants falling into this group, including 1,618 individuals (13.7%) in the aggregate control group and 1,707 individuals (14.6%) in the aggregate treatment group. After controlling for randomization week (as discussed below), this difference was not significant ($p = 0.55$). Given the program reports that most (if not all) of the jobs in the Back to Work Rhode Island database that they use are located in Rhode Island, and the lack of statistical significance on this test with our large sample size, we assume that RESEA selection does not differentially assist in finding out-of-state employment, and expect that the reported absolute magnitude of wage and reemployment changes is likely smaller than the truth.

In addition to the three pre-registered outcomes, we also consider short-term variants of these outcomes, in line with the CLEAR guidelines from the U.S. Department of Labor (USDOL) for RESEA intervention effectiveness (Clearinghouse for Labor Evaluation and Research, 2021). Specifically, we consider the reported wage in the second complete calendar quarter after the randomization date, whether this reported wage was nonzero, and weeks on UI in the same benefit year as the claim. We refer to these as the short-term wage, reemployment, and weeks on UI outcomes.

4.3 Primary analysis methodology

Our main analyses are estimates of intent-to-treat (ITT) effects measuring the average causal effect of RESEA *selection* on the three employment outcomes described in the previous section for the population of UI claimants during the study period. This differs from the causal effect of actually completing the RESEA program as designed, because despite the requirement for those selected for RESEA to follow through with scheduling job counseling appointments as a condition to receive their UI benefits, nearly half of those selected for RESEA did not complete the RESEA program (i.e., attend appointments). Appendix B provides some further descriptive statistics and analyses about compliance during the study period.

We note that while our pre-analysis plan includes reporting estimates of the "complier average causal effect" (CACE) on the outcomes of interest in addition to the ITT effect estimates, we ultimately decided not to provide CACE estimates in this report. While the CACE estimates did suggest significant positive effects of RESEA completion on all of our short-term and long-term outcomes, these estimates rely on the randomized RESEA selection being a valid instrumental variable for completion of the program (Imbens and Angrist, 1994). A key assumption of an instrumental variables analysis is the "exclusion restriction" assumption, which suggests in this context that RESEA selection only impacts the outcomes through completing the actual program itself, and that merely receiving a letter notifying selection does not have an effect. However, both anecdotal evidence from RIDLT and previous work (Black et al., 2002) suggests that there is likely a direct causal effect from selection into RESEA, independent of program completion. It is certainly possible that receiving the letter from RESEA about the requirements to attend a meeting to stay compliant and continue to receive benefits may motivate claimants to expedite their job search without attending the required meetings.[5]

[5] At the time of the pre-analysis plan, it was thought that the negative consequences of failing to complete RESEA would prevent substantive non-compliance, making the existence of such a

To properly account for the blocked experimental design in the ITT effect estimates, we first compute the difference between the average outcomes in the treatment and control groups within each week, and then take the weighted average of these differences with weights proportional to block size (the total number of participants randomized each week). Specifically, Y_{ti} is the outcome of interest for subject $i = 1, \ldots, n_t$ in block $t = 1, \ldots, T$ and W_{ti} is the binary treatment indicator for that subject (1 if selected for RESEA, 0 if not selected). Then the point estimate of the ITT effect is

$$\hat{\tau} = \sum_{t=1}^{T} \frac{n_t}{n} \hat{\tau}_t, \quad \hat{\tau}_t = \frac{\sum_{i=1}^{n_t} W_{ti} Y_{ti}}{\sum_{i=1}^{n_t} W_{ti}} - \frac{\sum_{i=1}^{n_t} (1 - W_{ti}) Y_{ti}}{\sum_{i=1}^{n_t} (1 - W_{ti})}. \tag{1}$$

This computation is implemented by the *difference_in_means* function in the *estimatr* package in R (Blair et al., 2025), which also provides associated asymptotic confidence intervals based on the central limit theorem that we report. Since our block sizes are fairly large (at least several hundred), the normal approximations underlying these intervals should be quite reasonable.

We note that our methodology differs from a fixed effects regression approach common in the literature, i.e., estimating the coefficient τ in the regression model

$$Y_{ti} = \alpha_t + \tau W_{ti} + \epsilon_{ti} \tag{2}$$

where α_t is a non-random intercept specific to the randomization week (block). The advantage of our approach is that our estimate is unbiased for the sample average treatment effect for all subjects in our cohort solely under the assumption — satisfied by design — that treatment is

direct effect of selection moot for the purposes of estimating CACE; the main concern distinguishing compliers and non-compliers was thought to be that non-selected individuals would voluntarily sign up for RESEA. However, there were only 5 individuals during our entire study period that were not selected for RESEA but voluntarily completed RESEA services anyway, in contrast to the 3,661 individuals that were selected but did not successfully complete the required services. We emphasize that none of these concerns undermines the fidelity of our ITT effect estimates, which is guaranteed by the randomized selection process.

completely randomized within each block. By contrast, since the proportion of individuals treated varied across blocks, the fixed effects regression estimator only satisfies this property if the true treatment effect is constant across blocks. Otherwise, the fixed effects estimator will estimate a weighted average treatment effect, with subjects in different blocks receiving different weights based on the proportion of treated subjects in their blocks. This issue is discussed in more detail by Gibbons et al. (2019).

5. Results

5.1. Long-term Outcomes

Overall, we find that RESEA selection leads to a significant improvement in all three of the preregistered outcomes of interest. We estimate that RESEA selection leads to a wage outcome that is $1,153 higher annually (95% CI: [$166, $2,141]; $p = 0.022$). Relative to the mean wage outcome of $33,043 in the control group (Table 02), this is a 3.5% increase. RESEA selection is also estimated to cause a 1.47 percentage point increase in reemployment (95% CI: [0.32, 2.62]; $p = 0.013$) and a 1.99 week reduction in the number of weeks on UI (95% CI: [1.75 weeks, 2.24 weeks]; $p < 0.001$). The latter is an 11.5% decrease compared to the mean control group weeks on UI outcome of 17.3 weeks. All three of these results remain statistically significant even after correcting the two-sided p-values for multiple testing using Holm's method to keep the familywise error rate (the probability of making any false rejections) below 5%.

Consistent with our pre-analysis plan, for reemployment, we also report the results on the scale of the odds ratio — that is, the ratio of the odds of reemployment in the treatment group to the odds of reemployment in the control group. Values larger than 1 indicate an increase in reemployment. Our point estimate for the odds ratio is 1.083 (95% CI: [1.018, 1.152]; $p = 0.012$ according to the Cochran-Mantel-Haenszel test). The point estimate was computed as a weighted average of odds ratio estimates within each week with weights proportional to block size, analogous to eq. (1) above.

The effect on weeks on UI appears to be the most substantial of the three outcomes, which is broadly similar to the existing literature (see Klerman et al., 2019 for a comprehensive modern review). While the wage and reemployment effects are more modest in magnitude, as noted previously, our available wage records do not include out-of-state employment, so our estimates for the effects on wages and reemployment are likely attenuated towards zero.

Importantly, the wage effect can plausibly be explained largely by an earlier return to work as implied by the effect on weeks on UI. As a heuristic calculation, we note that spending 1.99 fewer weeks on UI and instead earning the average control group annual wage of \$33,043 during those weeks corresponds to a \$1,265 increase in annual wages assuming 52 weeks in a year. This is quite close to the wage effect estimate of \$1,153; that it is slightly higher is consistent with our prediction that RESEA selection causes a slightly greater reduction in time spent on UI for lower earners than higher earners (Section 5.3). We caution that this is not the only plausible explanation for how RESEA selection causes higher wages; it's certainly possible, for instance, that there is not a one-to-one correspondence between the number of fewer weeks spent on UI and the number of additional weeks working.[6]

In Appendix C.1, we present an alternative analysis of our data that adjusts for several observed covariates: number of dependents, reported biological sex, ethnic code, age, educational attainment, and base wage.This analysis uses the same estimator $\hat{\tau}$ except that the within-block treatment effect estimates $\hat{\tau}_t$ are no longer simple differences in means, but rather

[6]For example, perhaps 1.99 fewer weeks on UI only translates to 1 extra week of work on average, but then this gets offset by a higher weekly rate of pay among selected individuals. Unfortunately, we are unable to investigate such effects due to the granularity of the available data.

adjust linearly for the observed covariates using the methodology of Lin (2013). This adjustment neither introduces any asymptotic bias nor worsens the asymptotic variance of the estimator, though it may introduce finite sample bias that vanishes with sample size. As expected, with this covariate adjustment we obtain very similar point estimates and slightly narrower confidence intervals compared to the main results we have presented above. We do not present these results in the main text as the methodology was not pre-registered, though for future studies we recommend pre-registering such covariate adjustments in this way due to their ability to improve precision at no cost to (asymptotic) bias.

Appendix C.2 presents an alternative analysis of the main outcomes using a fixed effects regression. As discussed above, the unequal treatment fractions across blocks mean the fixed effects estimator is biased for the average treatment effect when there is treatment effect heterogeneity across blocks. In our data, this does not appear to be a large concern; the fixed effects estimator gives very similar results to our main analyses.

5.2. Short-term outcomes

The results for the short-term outcomes are substantively similar to those for the longer-term outcomes. We estimate the effects of RESEA selection on the short-term outcomes using the same methodology as for the main outcomes. The analysis suggests that RESEA selection leads to: (1) short-term wages that are $1,195 higher (95% CI: [$326, $2,064]; p = 0.007) on an annualized basis and (2) a 1.66 percentage point higher rate of short-term reemployment (95% CI: [0.37, 2.96]; p = 0.012). Given that the magnitude of these causal estimates is very similar to those for the corresponding long-term outcomes, this suggests that the full impact of RESEA selection on wages and reemployment appears quickly by the second quarter after randomization, and persists for at least five quarters. Thus, we find no evidence to support concerns that RESEA may nudge claimants to find any employment quickly at the expense of a job that would be more stable and/or higher paying in the medium-to-long term. As noted below in the Future Research section, though, a longer outcome follow-up period than the present study's five quarters would enable us to investigate this hypothesis more fully. This analysis is possible with the existing data pipeline, which includes ongoing collection of the quarterly employment outcomes of the individuals in the study cohort.

We also find that those selected for RESEA spend 1.77 fewer weeks on UI in the benefit year of their claim (95% CI: [1.55 weeks, 1.99 weeks]; $p < 0.001$). As this is a bit smaller than the effect size of 1.99 weeks estimated for the long term weeks on UI outcome, which includes an additional 26 weeks beyond the benefit year, we see some evidence that RESEA selection may also reduce the time spent on UI in the following year, suggesting that it may be helping claimants find employment that lasts longer, thereby reducing "chronic" use of UI benefits. This also could be clarified with the ongoing collection of credit balance information for all UI claimants in Rhode Island.

5.3 Treatment effect heterogeneity

As an additional analysis, we investigate if there are groups that are more or less likely to benefit from RESEA programming. This research question was not part of the pre-analysis plan but was requested to determine if the program could be targeted toward those most likely to benefit from the program. Specifically,we examine differences in estimated treatment effects on the outcomes above across observed individual factors. Such factors could inform RIDLT in the design of a future RESEA selection algorithm to target those most likely to benefit from the program.

5.3.a. *Method.* In an effort to quantify treatment effect heterogeneity in all three of our primary outcomes of interest (wages, reemployment, and weeks on UI), we used "causal forests" (Wager and Athey, 2018) to estimate the conditional average treatment effect (CATE). Causal forests are a variant of the popular random forest algorithm for prediction (Breiman, 2001) designed to target high precision in estimating the CATE. The CATE refers to the average treatment effect for the subgroup of individuals defined by particular values of one or more available covariates. It is often notated by a function $\tau(X)$ where X is a vector of covariates. Here we consider the following covariates, available in the claim data provided by RIDLT: the number of dependents, reported biological sex, ethnic code, age, educational attainment, and base wage. To respect the blocked structure of our experiment, for each primary outcome we fit a separate causal forest to the claimants randomized in each week of the study period. The fitting is performed using the *causal_forest()* function in the *grf()* package in R (Tibshirani et al., 2024) with all categorical covariates represented using one-hot encoding, the default recommended hyperparameters, and the propensity score set equal to the proportion of treated subjects in that week within the study cohort. We use these causal forests to construct final CATE estimates at each of the covariate vectors X observed in the study as follows. Let t be the week in which an individual with covariate vector X appeared. For each week t' = 1,...,T of the study period we have a trained causal forest that gives a prediction $\hat{\tau}_{t'}(X)$ of the treatment effect on the outcome of interest for that individual. When $t' = t$, we use an out-of-bag prediction to prevent overfitting bias. Then we compute the predicted effect given the covariates X by taking a weighted average of all T predictions, with weights proportional to the number of claimants in each week. This mirrors our average effect analysis:

$$\hat{\tau}(X) = \sum_{t=1}^{T} \frac{n_t}{n} \hat{\tau}_t(X).$$

If the true underlying CATE function varies with week, as we might expect due to unobserved differences in the study population across weeks, we can interpret $\hat{\tau}(X)$ as the estimated effect of RESEA selection on an individual with covariate vector X provided they applied for UI in the week of a (uniformly) randomly selected individual from the study cohort.

After repeating the above procedure for all covariate vectors X observed among the study cohort, we can plot the predicted treatment effects versus each covariate individually to understand how the effect of RESEA varies as a function of this covariate.

5.3.b. Findings.

Wages: RESEA is found to have a positive effect on the wage outcome for every subgroup; the predicted effects for the claimants in our study based on their covariates X range from $82 to $2,284. Claimants who were older or had higher base wages tended to have higher predicted wage effects (Fig. 02). These predicted effects rise most rapidly for base wages above $50,000 and ages between 30 and 60. When we compute the wage effect as a percentage of base wage (rather than in raw annual dollars), however, those with higher base wages see a smaller effect (Fig. 03). On the other hand, older claimants still see a larger effect. The latter is consistent with a hypothesis from a RIDLT working group convened before this study was conducted that the RI RESEA program would be most beneficial to claimants having a long history with their single previous employer, though we do not have sufficient backward looking employer data for the claimants available to directly verify this. We observe relatively small heterogeneity in the predicted CATE's in the other covariates studied.

Reemployment: Contrary to the findings for the wage outcome, the estimates indicate that individuals with lower base wages are the ones who experience the greatest gains in reemployment due to RESEA selection. The predicted effect of RESEA selection on reemployment drops sharply from about 2 percentage points for individuals with base wages under $40,000 to just 0.3 percentage points for individuals with base wages over $100,000 (Fig. 04). There are some individuals (though none with base wages under about $70,000) for whom the predicted effects are slightly negative, though the most negative prediction in the dataset is only -0.31 percentage points, while the largest is 2.80 percentage points. Though base wages are fairly highly correlated with education (Fig. 05), we also find that the largest (positive) reemployment effects are predicted for claimants with lower education levels (Fig. 06). The variation in the reemployment effect versus age seems to be less pronounced than the variation versus base wage, though generally the smallest predicted effects are for those around age 30, with noticeably higher predicted effects for both older and younger claimants (Fig. 04).

<u>Weeks on UI:</u> Directionally, the heterogeneity in the predicted effect of RESEA selection on weeks spent on UI appears to be similar to the heterogeneity in the predicted effect on reemployment. That is, the largest predicted effects (greatest decline in weeks spent on UI) are for those with lower base wages and lower reported education levels (Figs. 07 and 08). The smallest effects are for middle aged workers while the effects get somewhat larger for younger and older workers (Fig. 07). However, the magnitude of heterogeneity is fairly small; all individuals have a predicted effect between -2.24 weeks and -1.67 weeks. That is, we predict that all subgroups in the study cohort would see a substantial decline in the weeks spent on UI due to RESEA selection.

<u>Overall:</u> Putting together our findings, the analysis suggests that RESEA selection is differentially helping lower-earning, less-educated workers find employment more quickly, therefore reducing time on UI. The wage effect is most pronounced for older workers, even when computing these effects as a percentage of base wage. While higher-earning and better educated

workers are predicted to have notably smaller reemployment effects, they are still predicted to spend nearly 2 fewer weeks on UI on average due to RESEA selection. Indeed, from fitting additional causal forests with benefit exhaustion (i.e., drawing 26 weeks of UI benefits in the benefit year of the claim) as the (binary) outcome (with the same covariates as above), the estimates suggest that RESEA selection causes a *larger* decrease in exhaustion rates among higher earners and better educated claimants (Figs. 10 and 11). Thus, while RESEA selection may not be strongly nudging such claimants to find (better) employment, it is evidently still encouraging them to find employment more quickly and thus to draw fewer weeks of UI benefits, translating to program cost savings and potential long term benefits in employment stability.

6. Cost Savings

Our findings indicate that the RESEA program benefits both the state and the claimants. For claimants, there is strong causal evidence that the program leads to higher rates of reemployment, higher wages, and fewer weeks spent on UI. For the state, the decrease in time spent on UI corresponds to a considerable cost savings from paying out a smaller amount of UI benefits. We estimate that on average, the individuals selected for RESEA drew $676 less (95% CI: [$563, $789]) in UI benefits during the same benefit year of their claim than they would have if they were not selected for RESEA. This is a 9.9% reduction relative to the control group average of $6,839 in UI benefits drawn during the benefit year of the claim. We computed these estimates by block-size weighted averages as in eq. (1), except with the outcome equal to the product of the weekly UI benefit rate and the number of weeks spent on UI in the benefit year of the claim, and with the block weights proportional to the number of individuals *selected* for RESEA in each week (as opposed to the total number of UI claimants randomized in each week, or to the total number of claimants who actually completed the RESEA services) to get an unbiased estimate of the average effect of RESEA selection on those actually selected. The estimates also control for the number of dependents, sex, age, base wage, and benefit rate using the same methodology as in Appendix C.1 to reduce variance. Multiplying these estimates by the total number of individuals selected for RESEA in the study (n = 11,700) suggests that as a result of the program, RI paid out about $7.9 million less in UI benefits to claimants randomized during the study period over the same benefit year as their claims. This is much higher than the total expenditures of $2.99 million for the RESEA program during the study period.

We reiterate that the present study cannot determine what portion of the effects are attributable to the content of the program meetings versus the mere requirement to attend two meetings. One could imagine that simply being selected for RESEA is enough of a "nudge" to encourage claimants to find work more quickly. Further research could determine whether this is indeed the case by randomly selecting some individuals for RESEA to do only a compliance check-in instead of receiving the full range of services. This would better inform the design of the RESEA program for both program effectiveness for individuals and from the perspective of cost efficiency for the state.

7. Future potential areas of study

Given our findings to date and the limitations of the current study, we recommend several potential avenues for future evaluations:

- *Extend the period of analysis to study the outcomes of the claimants in this study over a longer time period*. Corson and Haimson (1996) found that the treatment effects in the New Jersey Unemployment Insurance Reemployment Demonstration Project largely tapered off after six years, whereas Manoli et al. (2018) find a durable positive effect of the REA program in Nevada on wages and employment over six years, but not on time spent on UI, after the first year. Ongoing data on wages, UI benefit receipts, and employment in Rhode Island for the individuals in the study cohort is readily available to the PIs and so a longer term outcome study is recommended.

- *Examine a more recent cohort*. As claimants have been selected for RESEA through random assignment through the writing of this report, we can continue to monitor the ongoing causal effectiveness of program selection under changing economic conditions and changes to the program. In particular, as the second appointment/job search verification was eliminated in June 2024, after the study period of this report, the analysis we have presented should also be repeated for cohorts after this critical change was made to the program. Klerman et al. (2019) studied REA programs in four states (Indiana, New York, Washington, and Wisconsin) on people's public benefit receipt, employment, and earnings. The authors used a high causal rating RCT to compare public benefit receipt outcomes among unemployment insurance (UI) claimants randomly assigned to have multiple REA meetings vs. a single REA meeting. The authors found that "UI benefits were significantly lower for the multiple REA treatment group than the single REA treatment group." As a result, we recommend RIDLT study the effects of the change to their program using the existing data or (better) employ a new RCT to randomly assign some selected claimants to a second meeting and some to only one meeting to test the efficacy of this second meeting.
- *Disentangle the causal effect of the eligibility verification requirements from the effect of the reemployment services*. What remains unclear is which of the components of RESEA

is driving the reduction in UI benefits and increases in reemployment. Such an evaluation could be performed with a multi-arm randomized trial, similar to Lachowska et al. (2016), where there are two treatment groups: one receiving only eligibility verification requirements and one receiving the full suite of RESEA services.

Finally, a central goal of RESEA administrators is to concentrate the limited staffing resources for the program on those who would be most likely to benefit. Thus, while continued randomization of RESEA selection would create a large amount of causal evidence to continue future evaluations, at the same time, it would be desirable to start preferentially selecting those individuals who we believe would benefit the most based on our present analysis of treatment effect heterogeneity. To balance these competing objectives, future evaluation could employ a tie-breaker design similar to Black et al. (2002), whereby all UI claimants were assigned a "profiling score" based on their characteristics. After designing such a profiling score to be a proxy for the expected benefit from the program (e.g., our CATE estimate from causal forests), one could preferentially select claimants with higher profiling scores for RESEA while still randomizing to some extent to enable high quality causal evaluations of the program. Recent advances in the theory of tie-breaker designs (Li and Owen, 2023; Morrison and Owen, 2024) can inform the details of the selection algorithm in this case.

Finally, in any future evaluations, we recommend that additional data sources besides state wage records (e.g., federal wage data from the IRS, Massachusetts wage records, or claimant surveys as in Benus et al., 2008) be procured to enable more accurate treatment effect estimates.

8. Methodological notes for future RESEA evaluations

We conclude this report with some methodological notes that we believe would be useful for future evaluations of RESEA or other social programs relying on a block-randomized experimental design with potentially unequal treatment fractions across blocks, similar to our design. Out of the 13 studies[7] of employment assistance programs rated by USDOL as having "high causal evidence" and readily available on the Internet, we find that six studies used such a design with blocks being weeks and/or offices. For reference, we summarize the methods and main findings of these 13 studies in Table 03. Only two of these studies (Black et al., 2002; Klerman et al., 2019) properly account for this in their analyses. Three of these six studies (Poe-Yamagata et al., 2011; Michaelides et al., 2012; Manoli et al., 2018) fail to directly adjust for the block, instead using regression or matching methods to control for other observed demographic variables. These analyses may be biased even if the treatment effect is constant across blocks, since they fail to adjust for a known treatment confounder. Conversely, by design the block is the *only* treatment confounder. This makes it unnecessary to adjust for any other demographic variables for bias reasons, though as discussed in Section 5.1 and Appendix C.1, covariate adjustment can be performed using the method of Lin (2013) to improve precision. The remaining study (Michaelides and Mueuser, 2016) analyzes the same Nevada REA data as Michaelides et al. (2012) and fits the fixed effects regression in eq. (2), as do two other more recent evaluations in Nevada (Michaelides and Mian, 2021) and Washington (Brigandi et al., 2024) that have not been evaluated by the USDOL as of September 2025 (Table 04). As

[7] There are 19 studies of reemployment services rated on the DOL CLEAR website as having high causal evidence. Three of them (Corson et al. (1985), Behrens (1987), Wisconsin Department of Industry, Labor, and Human Relations (1984)) are not readily available online, while Poe-Yamagata et al. (2011) is listed four times, once for each of the four states studied. Since the analysis methodology is shared for the four states, we consider this as a single study, leaving 19 - 3 - 3 = 13 studies available.

discussed in Section 4.3, this introduces potential biases for estimating the average treatment effect if the true treatment effect and the fraction treated vary across blocks.

To illustrate the importance of adjusting for the blocks, Table 02 compares the average outcomes in the aggregate treatment and control groups. While we observe that the aggregate treatment group spends an average of 2.1 fewer weeks on UI, which is similar enough to our unbiased estimate of 1.99 fewer weeks, we also see that the aggregate treatment and control groups have virtually identical wage and reemployment outcomes, which would naively suggest that RESEA has no effect on these outcomes. This is in contrast to the treatment effect estimates which suggest significant positive effects of RESEA selection. To explain this apparent discrepancy, we note that randomization week appears to, in fact, be a substantial confounder for the wage and reemployment outcomes in our study. This confounding is not adjusted for by a direct comparison of the aggregate treatment and control groups, but is properly accounted for by our block size weighted estimates in eq. (1). Indeed, Fig. 09 shows that in weeks with a higher proportion of treated subjects, the *control* individuals had substantially lower average wage and reemployment outcomes than *control* individuals randomized in weeks with a smaller proportion of treated claimants (i.e., more eligible claimants overall, since the *numbe*r of treated claimants is roughly constant across weeks). Thus, the aggregate control group over-represents individuals in weeks with better wage and reemployment outcomes relative to the aggregate treatment group, perhaps due to the occupational makeup of the individuals or other unobserved factors. Consequently, raw differences between the aggregate treatment and control groups would underestimate the true effect of RESEA selection on these outcomes, consistent with what we observe.

In light of our finding that failing to explicitly adjust for randomization week in this study would have changed the substantive conclusions of our analysis, we highly recommend a re-analysis of the data from the three studies noted above (Poe-Yamagata et al., 2011; Michaelides et al., 2012; Manoli et al., 2018) that did not directly adjust for the treatment blocks using our estimator $\hat{\tau}$ in eq. (1) and the associated confidence intervals. While these studies did all verify that the differences between aggregate treatment and control groups on observed factors tended to be fairly small, the substantial negative findings in the historical analysis of RI RESEA prior to randomization by Huh et al. (2022) suggests the potential for substantial unobserved biases. We also suggest a re-analysis of the studies that used a fixed effects regression with our methodology. While we found minimal differences between the conclusions from the fixed effects estimates in eq. (2) and our main estimates in eq. (1) in the current study (Appendix C.2), we would expect potentially large biases from the fixed effects estimates in settings where the claimants differ substantially across blocks in potentially unobserved ways, inducing significant treatment effect heterogeneity across blocks.

Acknowledgments

We gratefully acknowledge the assistance of numerous employees of the Rhode Island Department of Labor and Transportation (RIDLT), including Megan Swindal, Sarah Bramblett, Robert Kalaskowski, and Sarah Fresch who provided us with much of the information in this report about the details of the RI RESEA program and helped inform the main directions and emphases of this study. Steve Tella, Paul Xu, Edward Huh, Kevin Wilson, Chris Calley, and Nat Rabb provided substantial technical assistance with data processing.

Tables and Figures

Table 01. Unweighted pre-treatment characteristics of the aggregate treatment and control groups in the study cohort. Note that education information was only available for online filers.

	Treatment group (n = 11,700)	**Control group (n = 11,849)**
Proportion female	50.1%	48.3%
Mean base wage	$49,175	$49,748
Mean age	45.2 years	45.2 years
Mean number of dependents	0.44	0.43
Proportion white alone	63.2%	64.7%
Education	3.9% no high school degree 19.3% high school diploma 21.4% some college or Associates 8.8% Bachelors 3.4% Masters 0.4% Doctorate 42.7% missing (phone filer)	4.5% no high school degree 20.8% high school degree 21.5% some college or Associates 8.9% Bachelors 3.2% Masters 0.6% Doctorate 40.5% missing (phone filer)

Table 02. Average outcomes for the aggregate treatment and control groups in the study cohort, along with our unbiased average treatment effect estimates and corresponding 95% confidence intervals. Standard deviations are also given in parentheses for quantitative outcomes. This table illustrates that direct comparisons between the aggregate treatment and control groups can lead to significantly biased conclusions.

Outcome	**Treatment group (n = 11,700)**	**Control group (n = 11,849)**	**Average Treatment Effect Estimate (95% CI)**
Wage	$33,073 (SD: $36,749)	$33,043 (SD: $36,231)	$1,153 (95% CI: [$166, $2,141])
Reemployment	75.2%	75.0%	1.47 pp (95% CI: [0.32 pp, 2.62 pp])
Weeks on UI	15.2 (SD: 9.3)	17.3 (SD: 9.4)	-1.99 weeks (95% CI: [-2.24 weeks, -1.75 weeks])
Short-term wage	$23,776 (SD: $31,948)	$23,897 (SD: $32,293)	$1,195 (95% CI: [$326, $2,064])
Short-term reemployment	61.4%	61.6%	1.66 pp (95% CI: [0.37, 2.96])
Short-term weeks on UI	14.2 (SD: 8.4)	15.9 (SD: 8.3)	-1.77 weeks (95% CI: [-1.99 weeks, -1.55 weeks])

Table 03: A summary of the studies in the DOL CLEAR database rated as having "high causal evidence" for evaluating RESEA (although none of the studies directly study an RESEA program). Rows highlighted in blue correspond to studies that do not appear to be readily available on the Internet.

Reference	Study period	Location(s)	Design	Properly controls for block, if relevant?	Summary of findings
Manoli et al. (2018)	Q3-Q4 2009	NV	Block-randomized with unequal treatment fractions across blocks	**No**: Uses propensity score matching on demographic variables, discards substantial proportion of observations due to poor overlap	Studied REA in NV. -Treatment resulted in higher employment and earnings relative to the control group for each of the six years following the intervention. -Treatment resulted in fewer weeks on UI for the first year after intervention.
Klerman et al. (2019)	2015-2016	IN, NY, WA, WI	Block-randomized with unequal treatment fractions across blocks (IN) Block-randomized with (nearly) equal treatment	Yes (IN): Observations are reweighted based on treatment fraction in a fixed regression. N/A (other states): Block is not a confounder, and a fixed effects regression is used.	Studied REA in four states. -Participants in the multiple REA treatment group spent fewer weeks on UI than the single REA treatment group.

			fractions across blocks (NY, WA, WI)		
Behrens (1987)	March 1985 - May 1986	Hackensack, NJ			
Decker et al. (2000)	1995-1996	Washington, DC and FL	Simple random assignment	N/A: No blocks	Studied the Job Search Assistance Demonstration program. -Treatment reduced UI benefits and increased earnings compared to control group in Washington, D.C., but not in Florida.
Corson et al. (1985)	1983	Charleston, SC			

Black et al. (2003)	1994-1996	KY	Blocked tie-breaker design based on predicted probability of benefit exhaustion, with unequal treatment fractions across blocks	Yes: Uses fixed effects estimator but careful to note interpretation of estimands due to unequal treatment fractions	Studied the Worker Profiling and Reemployment Services program. -Participants in the treatment group had a statistically significant reduction in weeks on UI (2.2 weeks) during the six-quarter follow-up period, compared to the control group but no impact on UI exhaustion or total UI benefits received.
ERP project final report	1984	WI			
Michaelides et al. (2012)	Q3-Q4 (2009)	NV	Block-randomized with unequal treatment fractions across blocks	**No**: Uses a fixed effects regression that only adjusts for demographic and employment variables	Studied REA in NV. -Intervention resulted in a reduction in the average duration and amount of UI benefits receipt, an increase in employment rates, and an increase in earnings compared to control group.
Corson et al. (1989)	1986-1987	NJ	Block-randomized with (nearly) equal treatment fractions across blocks	N/A: Block is not a confounder, reports regression adjusted differences	Studied the New Jersey Unemployment Reemployment Demonstration Project. -Main results are based on surveys, which are viewed as more reliable than wage records. No significant result found in wages.
Anderson et	1986-1987	NJ	Block-	N/A: Block is not a	Studied the New Jersey Unemployment

al. (1991)			randomized with (nearly) equal treatment fractions across blocks	confounder, reports regression adjusted differences	Reemployment Demonstration Project. -Treatment resulted in fewer UI dollars received ($293), and fewer weeks spent on UI)1.6 weeks) compared to control group. -No effects for reemployment, earnings, or weeks worked.
Corson and Haimson (1996)	1986-1987	NJ	Block-randomized with (nearly) equal treatment fractions across blocks	N/A: Block is not a confounder, reports regression adjusted differences	Studied the New Jersey Unemployment Reemployment Demonstration Project. -Treatment resulted in a statistically significant reduction in UI dollars received and weeks spent on UI, compared with the control group. -No effects were found on the probability of working, level of earnings, or weeks worked.
Benus et al. (2008)	2005-2006	ND	Simple randomized design (based on last digit of SSN)	N/A: No blocks	Studied REA in ND. -No statistically significant impacts of REA on UI benefits receipt, employment, or earnings were found as a result of REA treatment.

Poe-Yamagata et al. (2011)		FL, ID, IL, NV	Block-randomized with unequal treatment fraction across blocks (FL, NV). Block-randomized (ID, IL) with unclear treatment fractions.	**No**: Uses a regression analysis that does not control for randomization week	Studied REA in four states. -Treatment resulted in statistically significant reductions in weeks spent on UI, total amount of UI benefits received, and the probability of benefit exhaustion.Treatment also increased probability of employment and earnings over the four follow-up quarters.
Lachowska et al. (2015)	1986-87	WA	Simple randomized design (based on last digit of SSN)	N/A: No blocks	Studied effects of eliminating work-search requirements for collecting UI benefits. -The treatment group with less-stringent work-search requirements was significantly less likely to be employed in the first quarter following their claims, compared with the groups with more-stringent requirements. The group with less stringent work search requirements also received more UI benefit payments for more weeks and exhausted UI benefits at a higher rate during the year following their initial claims.
Lachowska et al. (2016)	1986-87	WA	Simple randomized design (based on last digit of SSN)	N/A: No blocks	Studied effects of eliminating a work test as a requirement for collecting UI benefits. -Extended Lachowska (2015) and found that the treatment groups with more-stringent work search

					requirements were more likely to be employed compared to the less-stringent requirement group in the first year following their claims. More stringent work requirement groups also received UI benefits payments for fewer weeks, exhausted UI benefits at a lower rate, and received fewer conditional payments in the year following their initial UI claim.
Michaelides and Mueser (2016)	Q3-Q4 2009	NV	Block-randomized with unequal treatment fractions across blocks	Only if treatment effect constant across blocks; uses fixed effects regression	Studied REA in NV. -Studied a treatment group who received job matching and work search preparation services, a treatment group with only REA eligibility services and job search activity training, services, and a control group that was only asked to track their job search. Found that the first treatment group had significantly higher reemployment rates and lower UI benefit receipt but similar earnings as compared to the control group.

Table 04: Same as Table 03, but for two recent RESEA evaluation reports that have not been reviewed by USDOL as of the time of writing.

Reference	Study period	Location(s)	Design	Properly controls for block, if relevant?	Summary of findings
Brigandi et al. (2024)	Dec. 2021 - Dec. 2022	WA	Block-randomized with unequal treatment fractions across blocks	Only if treatment effect constant across blocks; uses fixed effects regression	Studied RESEA in WA. -Study evaluated if a scheduling process change would reduce the number of benefit disqualifications. Treatment was found to reduce the number of no-shows and the number of benefit disqualifications that resulted from not making/attending an appointment. The UI claimants who benefitted most were more likely to self-identify as African American/Black and male, were younger, and had lower earnings than those who benefited less.
Michaelides and Mian (2021)	2014-15	NV	Block-randomized with unequal treatment fractions across blocks	Only if treatment effect constant across blocks; uses fixed effects regression	Studied REA in NV. -Found that treatment resulted in an increase in earnings for treated claimants over study follow-up periods ranging from 1.5 to 5 years after random assignment and reduced UI payments.

Figures

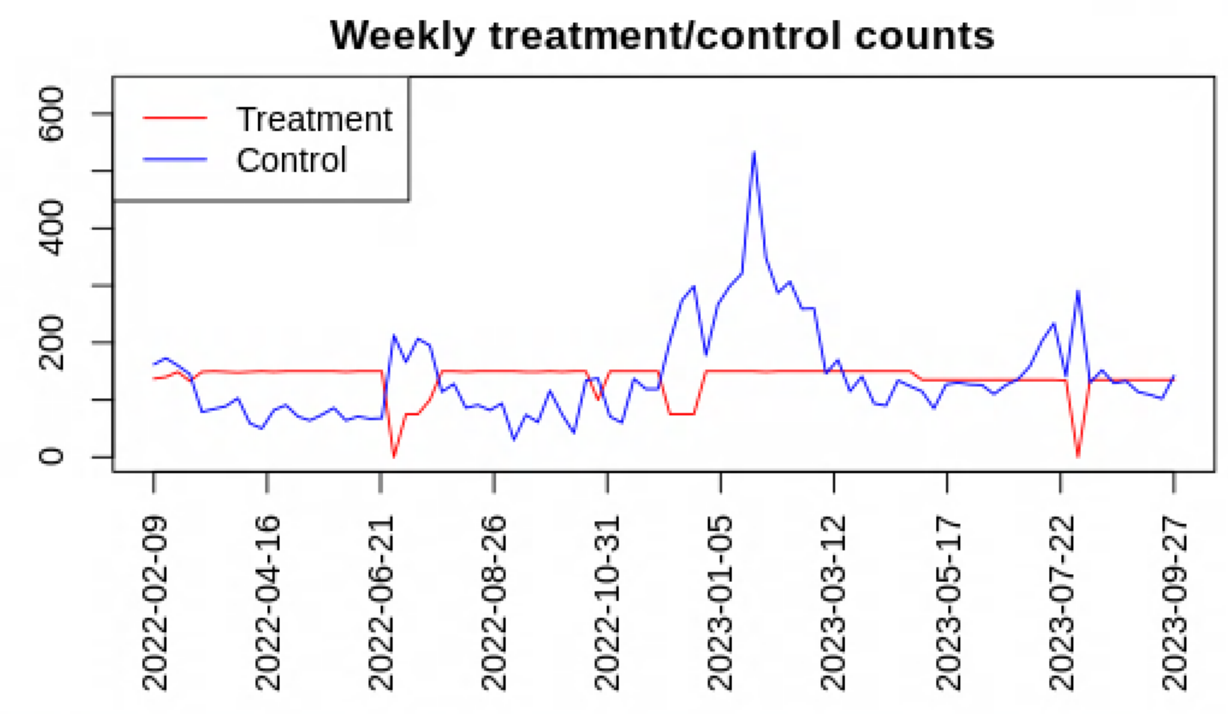

Fig. 01. The number of individuals assigned into the treatment (i.e., selected for RESEA) and control (i.e., not selected for RESEA) groups during each week of the study period. Note that these counts include all claimants (total n = 24,120), including veterans and those whose claims were later determined to be fraudulent.

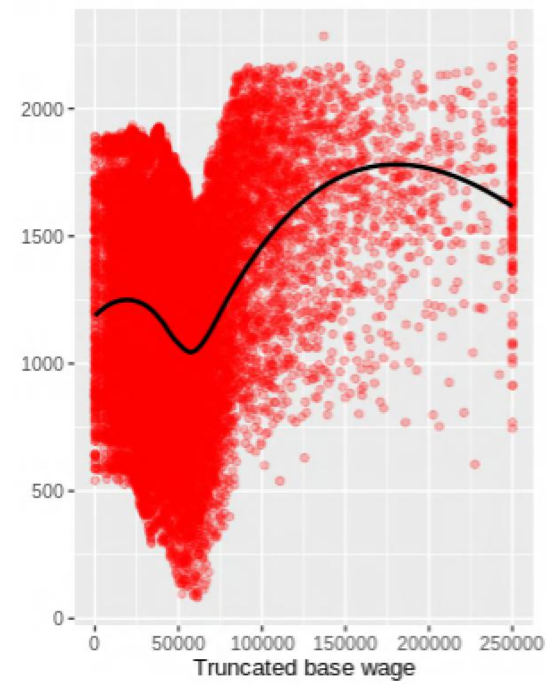

Fig. 02: The predicted effect of RESEA selection on the wage outcome for all individuals in the study period versus their base wage (left) and age (right). The black lines are scatterplot smoothers constructed using locally weighted regression. Note: base wage (resp. age) is truncated to $250,000 (resp. 80 years).

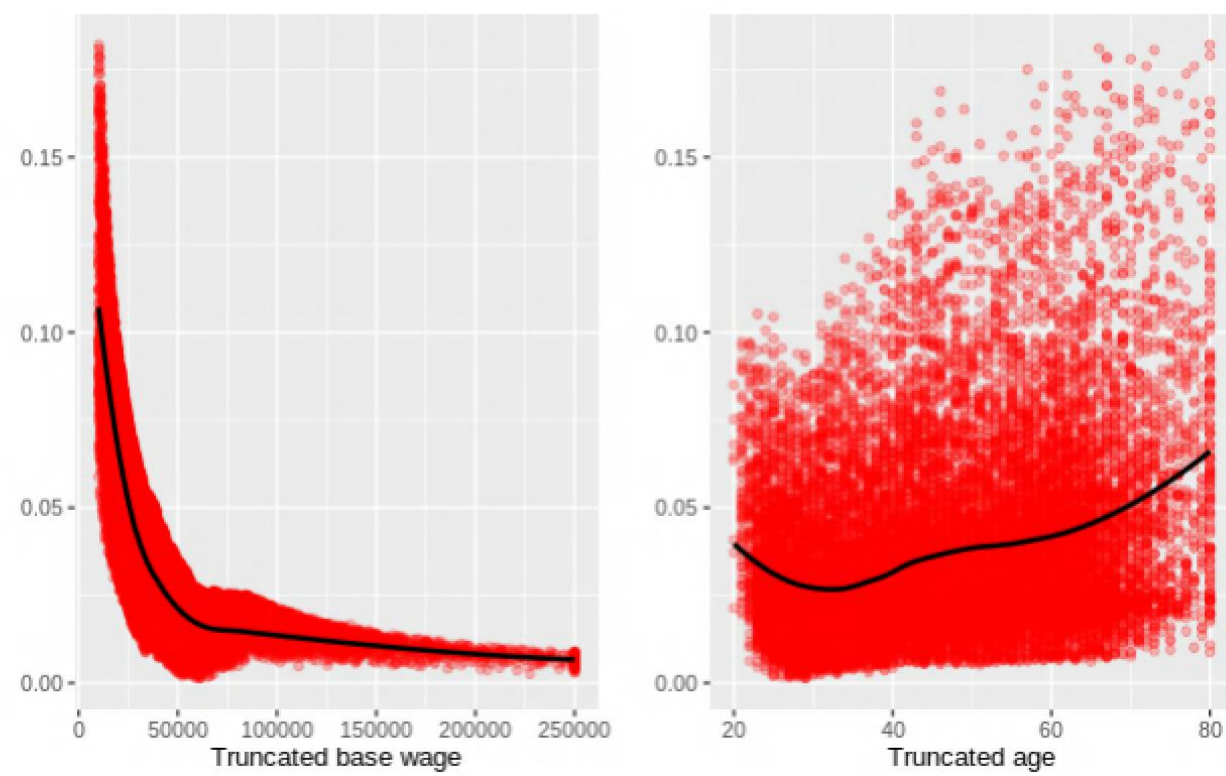

Fig. 03: Same as Fig. 02 but with the vertical axes showing the predicted wage effect as a proportion of truncated base wage. Note we have filtered to only individuals with a base wage of at least $10,000.

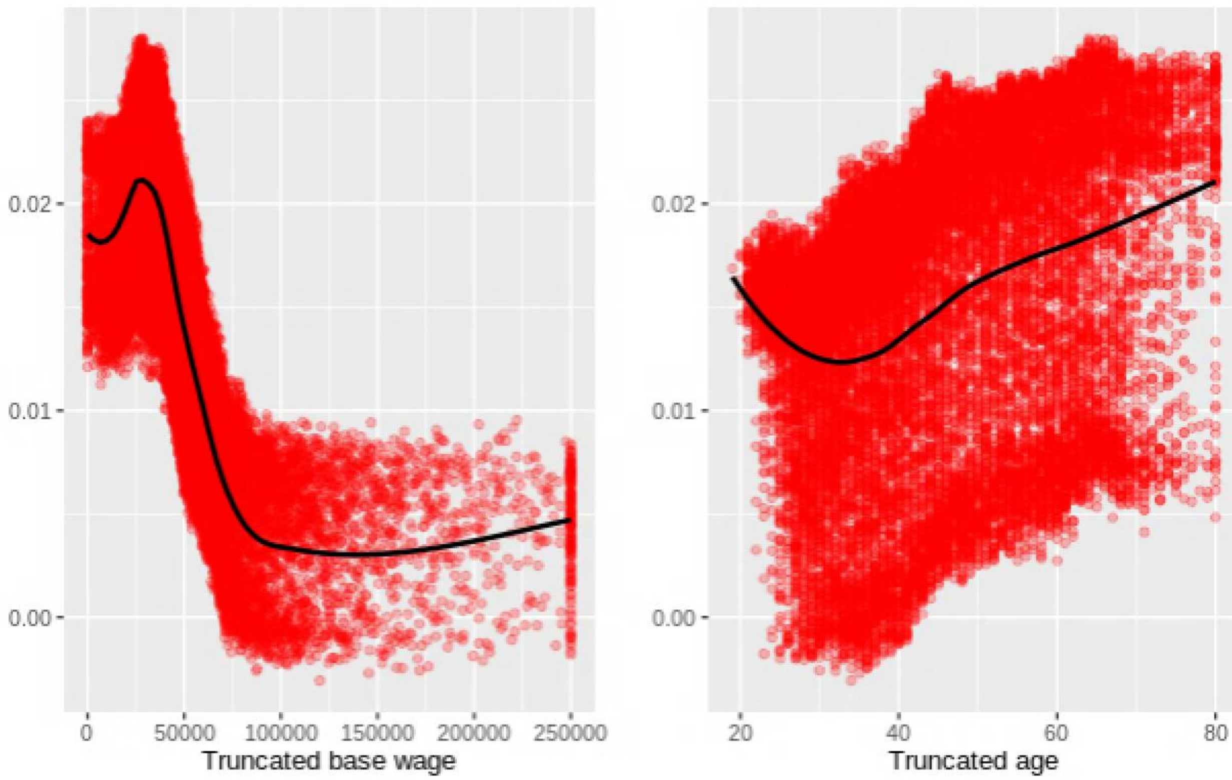

Fig. 04: Same as Fig. 02, but with the vertical axes showing the predicted effect of RESEA selection on the reemployment outcome in percentage points.

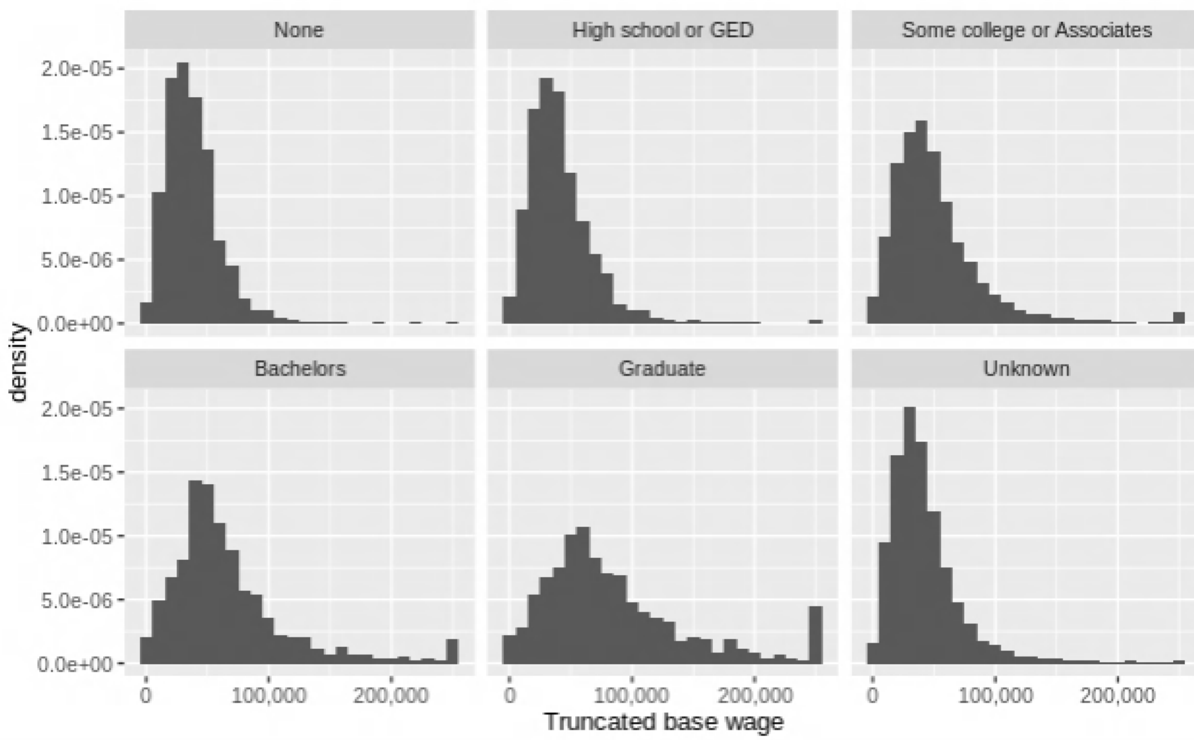

Fig. 05: Density plots showing the distribution of base wage broken down by education level among those in the study cohort

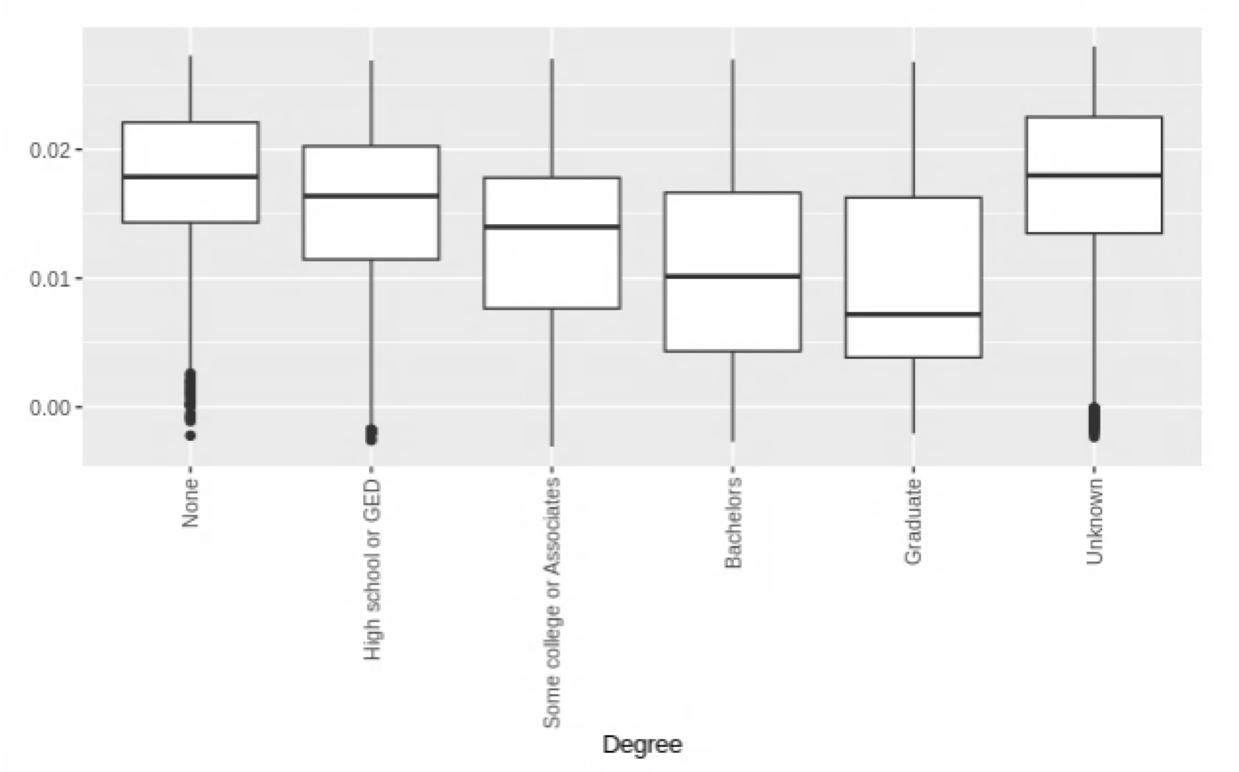

Fig. 06: The distribution of the predicted effects of RESEA selection on the reemployment outcome in percentage points, broken down by education level.

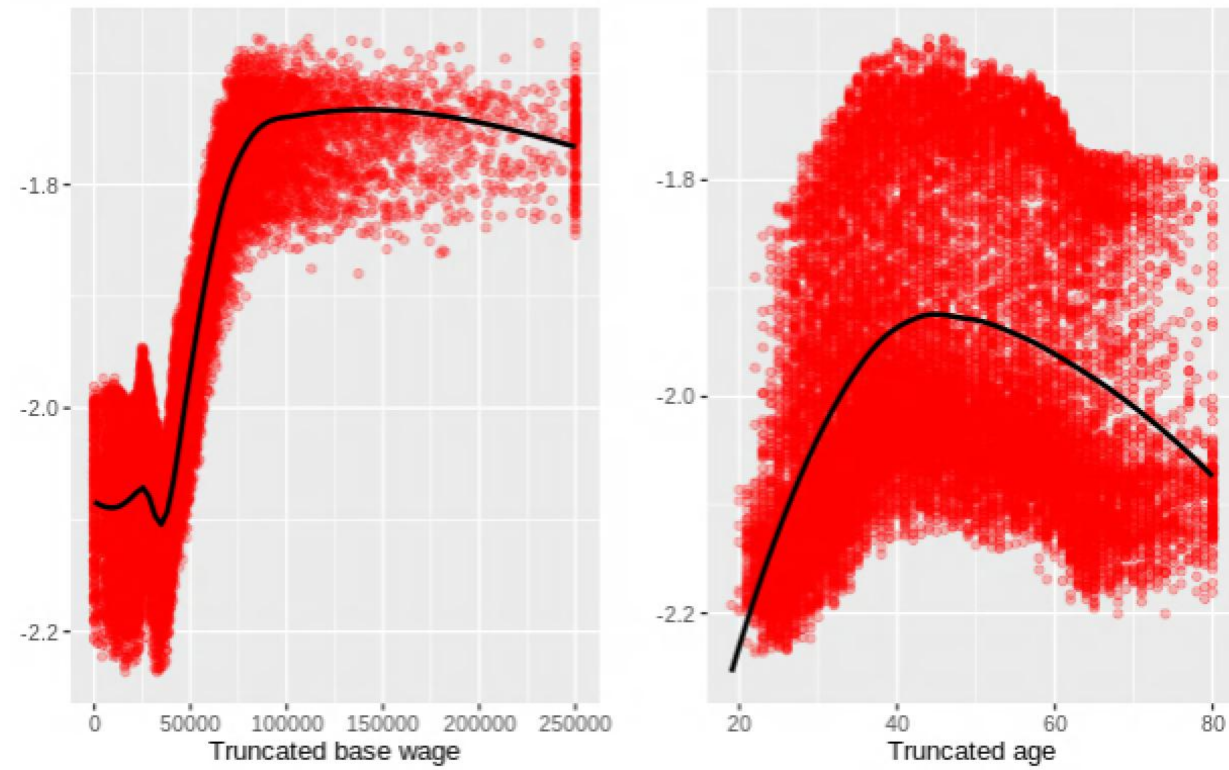

Fig. 07: Same as Fig. 02, but with the vertical axes showing the predicted effect of RESEA selection on the weeks on UI outcome.

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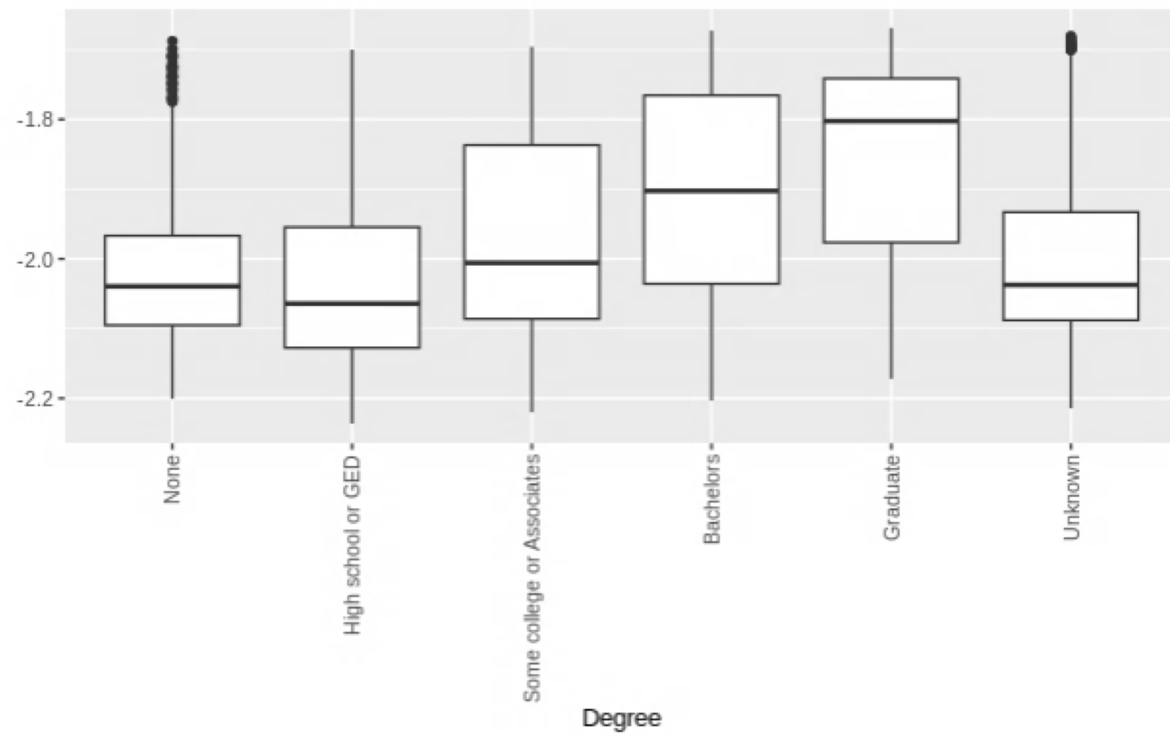

Fig. 08: Same as Fig. 06, but with the vertical axis showing the predicted effect of RESEA selection on the weeks on UI outcome.

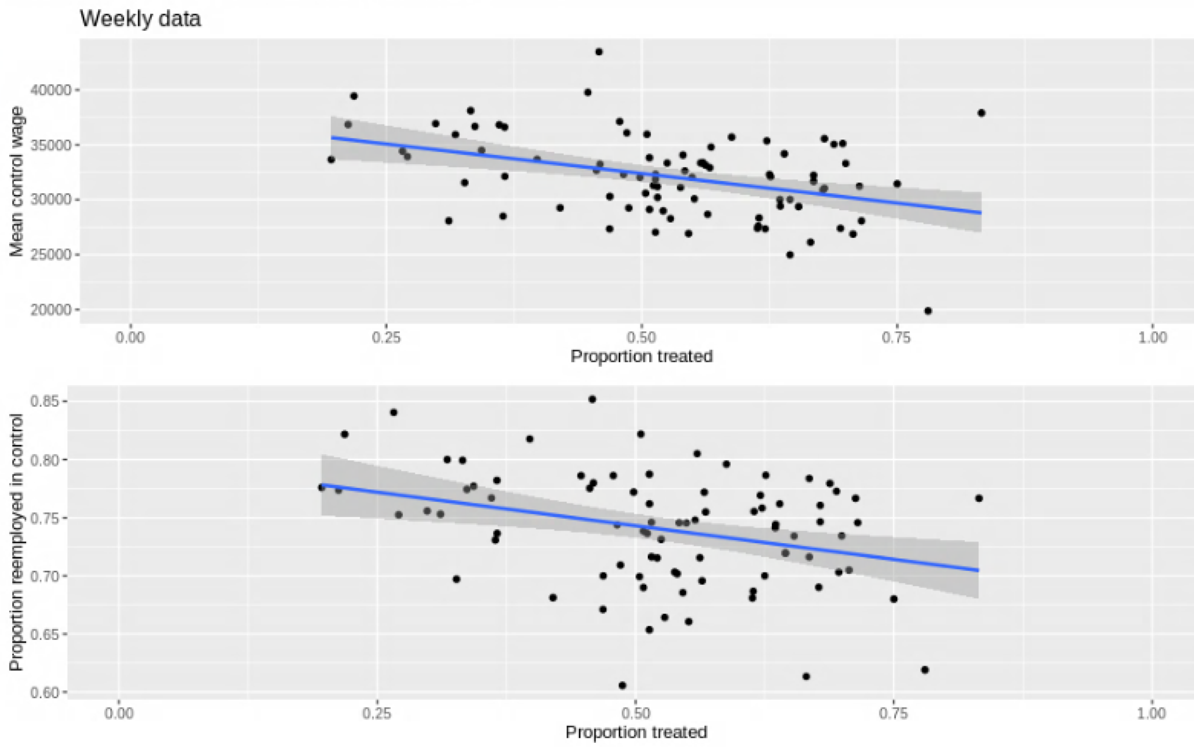

Fig. 09: (Top) The mean wage outcome among the claimants not selected for RESEA (control group) in each week, plotted against the proportion of all claimants selected for RESEA in that week. The blue line is the least squares regression line fit to the scatterplot, with the shaded region around the line showing pointwise 95% confidence intervals for the outcome. (Bottom) The same as the top but with the reemployment proportion on the vertical axis.

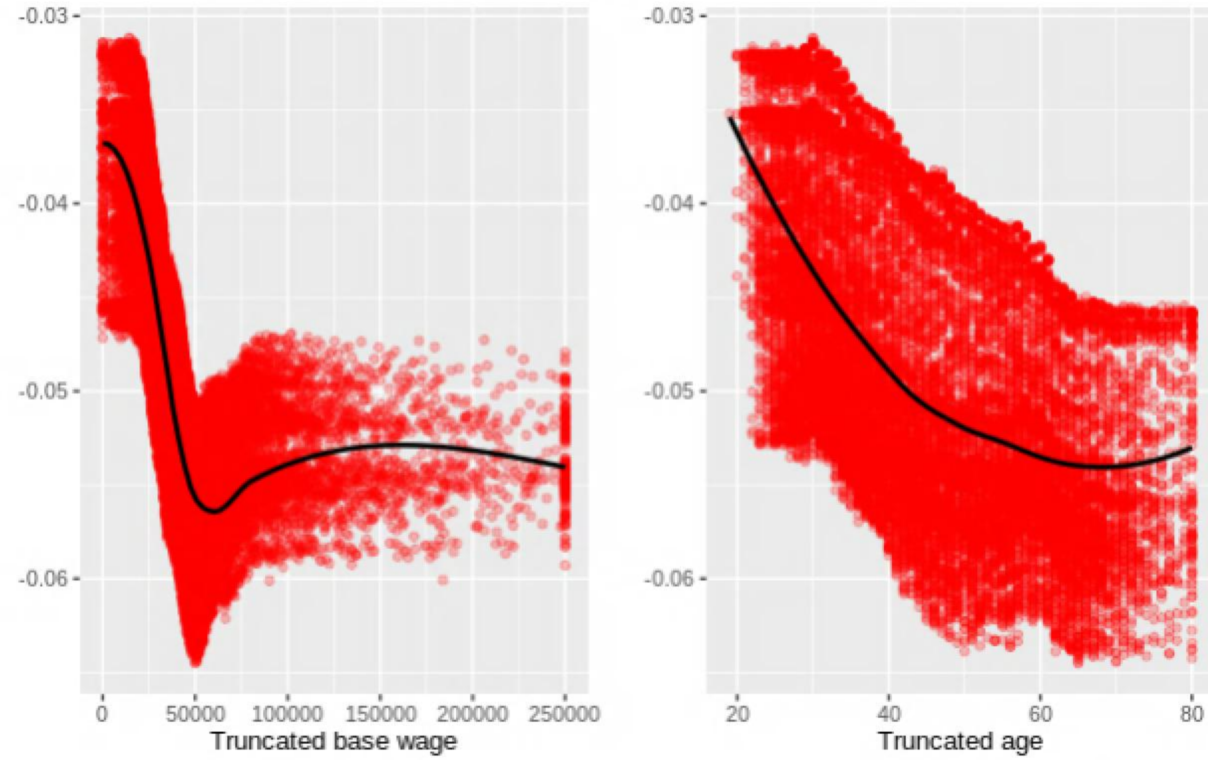

Fig. 10: Same as Fig. 02, but with the vertical axes showing the predicted effect of RESEA selection on the probability of exhaustion, in percentage points.

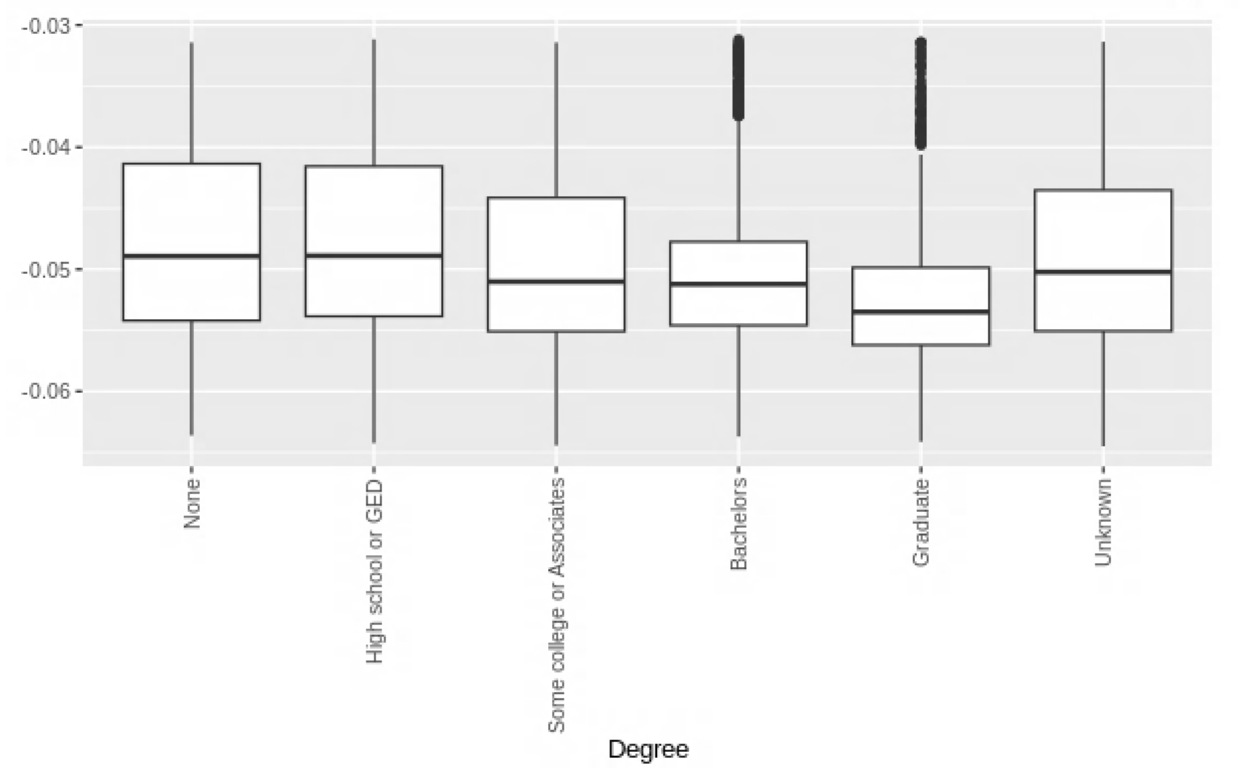

Fig. 11: Same as Fig. 06, but with the vertical axis showing the predicted effect of RESEA selection on the probability of exhaustion, in percentage points.

Appendix

A. Balance tests

To help evaluate whether the randomized RESEA selection algorithm worked properly, we conducted weekly balance tests. Specifically, we conduct a two-sample permutation test on the data from each week to check whether various covariates are balanced between the treatment and control groups among those in the study cohort (i.e., excluding veterans and claimants whose claims were later deemed fraudulent). This test proceeds by first computing a test statistic T, defined as the absolute value of the difference in mean covariate value between the two groups (binary covariates were encoded as 0 or 1). Then we randomly permute the group labels $M = 10{,}000$ times; on each permutation iteration i we compute test statistics T_i on this permuted dataset. Finally, we compute a two-sided p-value based on the number of test statistics T_i larger than the test statistic T from the original dataset using the following formula:

$$p = \frac{1 + \#(T_i > T)}{1 + M}$$

Under the null hypothesis that the control and treatment distributions of the covariate are exchangeable, this p-value stochastically dominates the uniform distribution on [0,1]. In other words, a histogram of the p-values across weeks should be either roughly uniform or tend to have mass concentrated towards the right (i.e., towards 1), under the null hypothesis. In Fig. A1, we observe this is in fact the case for all of the covariates studied in the text.

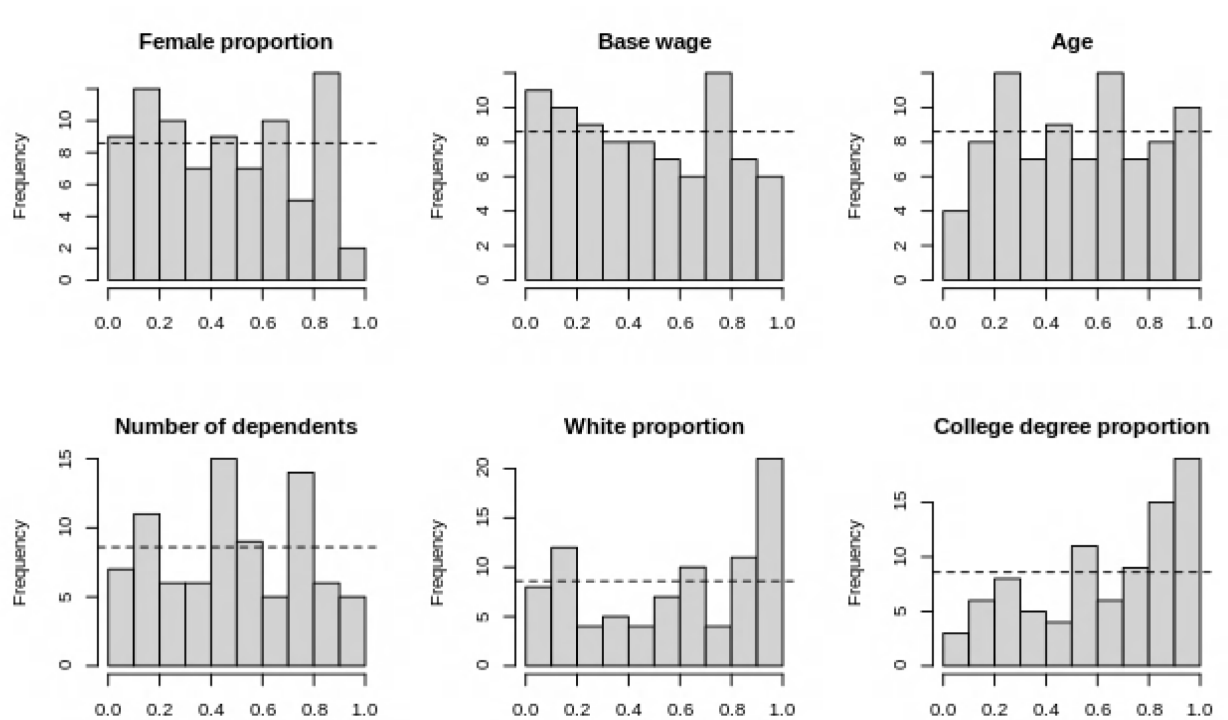

Fig. A1: Histograms of p-values of weekly balance tests across weeks for selected covariates. The bin width is 0.1 and there are a total of 84 weeks, so a truly uniform histogram would have each bar with a height of 8.4, denoted by the horizontal dashed lines.

B. Compliance

Out of the 11,700 individuals who were selected into RESEA during the study period, 6,163 (52.4%) individuals successfully completed the required RESEA meetings, while another 1,876 (16.0%) individuals were deemed exempt from completing all RESEA requirements, typically because they reported a return-to-work date before completing the requirements or were found to be ineligible for UI upon review. The remaining 3,661 individuals (31.1%) did not successfully complete RESEA despite being required to do so. Fig. B1 shows compliance rates (including exempt individuals) were reasonably consistent across weeks during the study period.

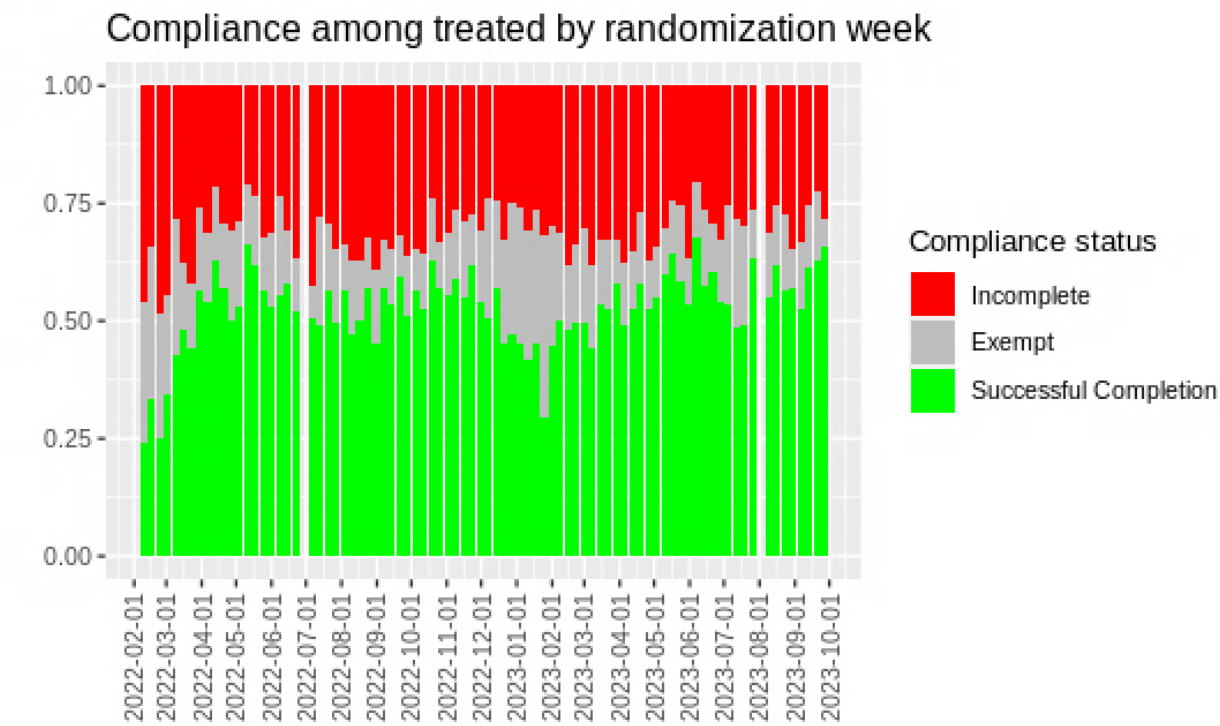

Fig. B1: The proportion of individuals selected for RESEA during each week of the study period who successfully completed the RESEA program (green), were exempt from completing the program (grey), and did not complete all required aspects of the RESEA program (red).

We find several factors among those studied in Section 5.3 on treatment effect heterogeneity to be individually predictive of compliance (again, defined as either successful completion of RESEA requirements or being exempt from them). Most notably, compliance rates increase sharply with age, and also robustly with education level and base wages (Figs. B2, B3). Controlling for all three of these factors in a logistic regression model shows that each of these individual factors remains significantly positively predictive of compliance, keeping the other two variables fixed.

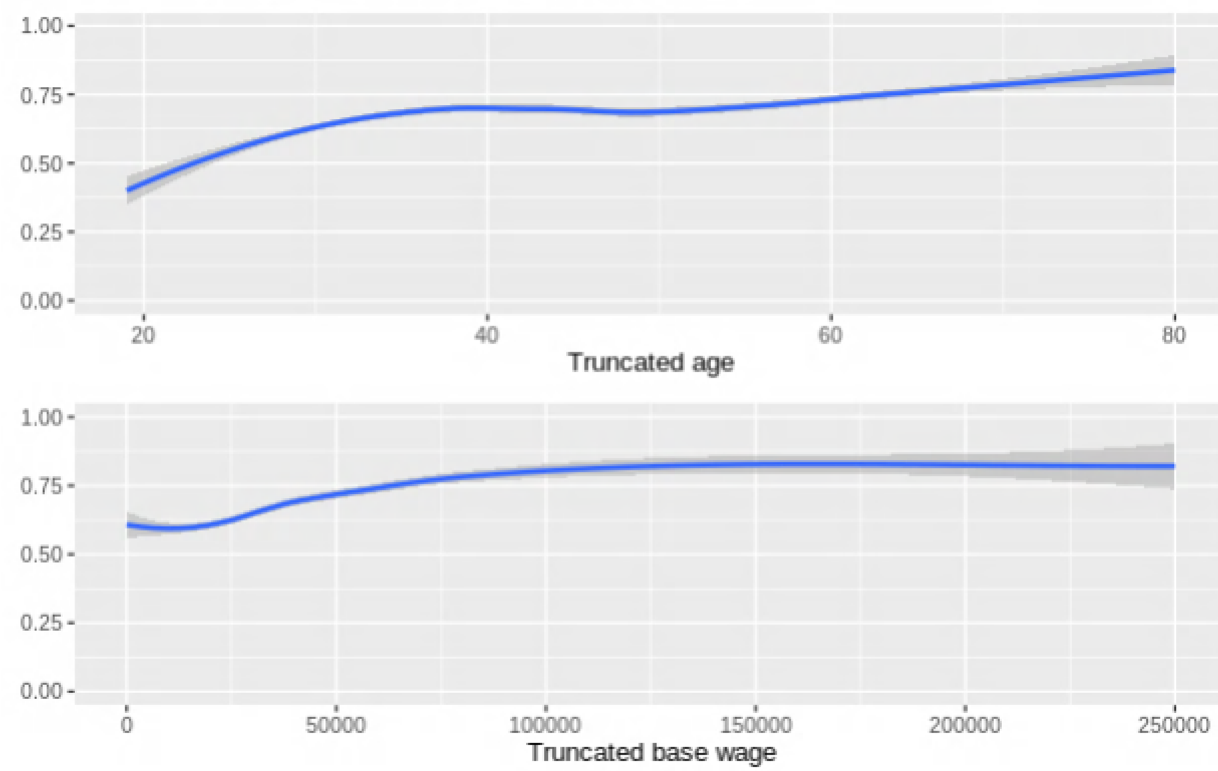

Fig. B2: The smoothed RESEA compliance rates among individuals in the study group selected for RESEA, as a function of age truncated at 80 years (top) and base wage truncated at $250,000 (bottom). The smooths are computed using a locally weighted regression via the loess() function in R with a smoothing window of span = 0.75, and the shaded grey regions around the smoothing lines are pointwise standard errors of the predictions.

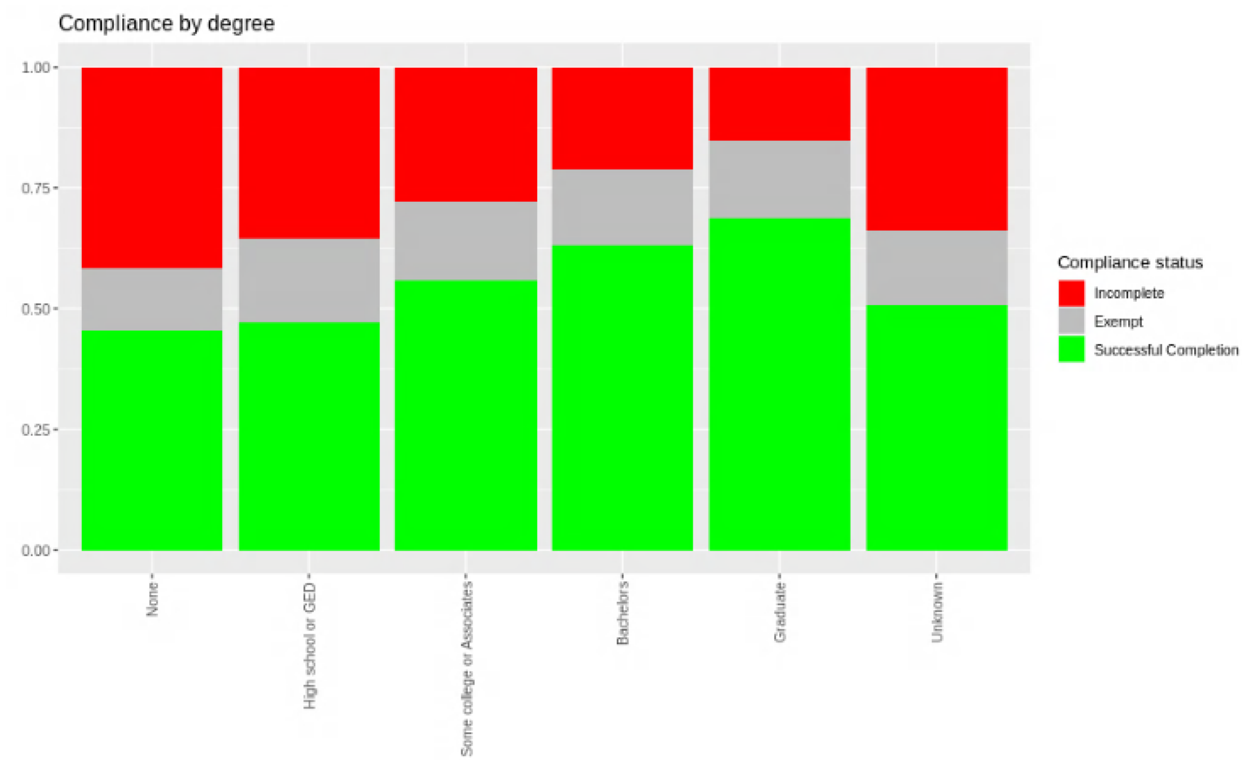

Fig. B3: The compliance rates among individuals in the study group selected for RESEA, as a function of their reported education level. Individuals with "unknown" education level are those who filed their UI claims by phone.

C. Alternative treatment effect estimates

Here, we estimate our main treatment effects of interest using alternative estimators that were not pre-registered.

C.1. Covariate-adjusted estimates

First, we consider an analysis that adjusts for four observed baseline (pre-treatment) covariates: number of dependents, reported biological sex, age, and base wage. We considered also adjusting for ethnic code and educational attainment as in our analysis of heterogeneous effects, but the presence of sparse levels of these factors in some weeks made covariance matrix estimates singular and therefore prevented the computation of confidence intervals.

As described in the main text, the method we propose for covariate adjustment is based on the work of Lin (2013). It involves mean-centering each non-treatment covariate and then fitting linear regressions (via ordinary least squares) to predict the desired response from the treatment indicator, the mean-centered covariates, and all of the two-way interactions between the treatment indicator and each of the mean-centered covariates. We fit such a regression on the data each week, giving asymptotically unbiased estimates $\hat{\tau}_t$ for the average treatment effects within each week $t = 1, \ldots, T$. These estimates can be combined into a single estimate $\hat{\tau}$ for the overall average treatment effect using a block-size weighted average as in our main analysis from eq. (1). Confidence intervals can be constructed using heteroskedasticity-robust standard error estimates; we use the "HC2" estimator of Mackinnon and White (1985). Specifically, we obtain standard error estimates for each of the $\hat{\tau}_t$ using the HC2 estimator. Since it is reasonable to assume the $\hat{\tau}_t$ are independent across $t = 1, \ldots, T$ (as they are based on observations from different UI claims), we can add the squares of the standard error estimates — each multiplied by $(n_t/n)^2$, the square of the fraction of observations in week t — to get an estimate for the variance of $\hat{\tau}$. This can be then used to construct confidence intervals and p-values based on the quantiles of the standard normal distribution.

With these covariate adjustments, the estimated effect of RESEA selection on our wage outcome is $1,137 (95% CI: [$228, $2,047]; $p = 0.014$). We estimate RESEA selection to increase reemployment by 1.48 percentage points (95% CI: [0.32, 2.63]; $p = 0.012$) and decrease our weeks on UI outcome by 1.93 weeks (95% CI: [1.68 weeks, 2.18 weeks]; $p < 0.001$). The point estimates and confidence intervals are nearly identical to our main results, except that the CI for the wage outcome is slightly narrower, which we attribute to the modest predictive power of base wage for the wage outcome ($R^2 = 0.14$ across all individuals in the study cohort).

C.2. Fixed effects regression estimates

We also consider an analysis using the fixed effects regression in eq. (2). Confidence intervals and p-values are again computed using the HC2 standard errors. Based on the fixed effects regression, we estimate that RESEA selection increases the wage outcome by $1,100 (95% CI: [$127, $2,073]; $p = 0.027$), increases reemployment by 1.49 percentage points (95% CI: [0.34, 2.64]; $p = 0.011$), and decreases the weeks on UI outcome by 1.97 weeks (95% CI: [1.72 weeks, 2.22 weeks]; $p < 0.001$). Once again, this is fairly close to the results reported in the main text, though we remind the reader that the fixed effects estimates are, in general, biased for the average treatment effect.

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