Evaluation of Impacts of the Reemployment and Eligibility Assessment (REA) Program: Final Report

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No statement in this document should be taken as stating an official interpretation of any federal or state statute or guideline, nor should any statement in this document be taken as stating an official DOL or state policy. Readers desiring information on statutes and guidelines should consult the appropriate legal documents. For the REA program, the pertinent Unemployment Insurance Program Letter is UIPL No. 10-14, "Fiscal Year (FY) 2014 Unemployment Insurance (UI) Reemployment and Eligibility Assessment (REA) Grants," http://wdr.doleta.gov/directives/attach/UIPL/UIPL_10_14.pdf.

This document describes state REA policies and implementation procedures—as inferred from site visits and analysis of administrative data—for the study period 2015 to 2017. Since that time the REA program has been sunsetted and the new Reemployment Services & Eligibility Assessment (RESEA) program has been implemented. Thus, the policies and implementation procedures described here are often no longer in place. Furthermore, in some cases, it is possible that state implementation did not completely align with then current federal REA program guidance. Deliberately, this report makes no comments about any misalignment, instead documenting only Abt's understanding and observation of states' implementation activities.

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Executive Summary

The **Evaluation of the Reemployment and Eligibility Assessment (REA) Program** aimed to estimate the impact of the U.S. Department of Labor's REA program, which supported states to address the reemployment needs of Unemployment Insurance (UI) claimants and to prevent and detect UI improper payments. The evaluation included both an implementation study and an impact study. This document, the evaluation's *Final Report*, presents the results of the impact study.

The REA Program

From 2005 to 2016, the federal government awarded grants to states to operate REA programs—according to the states' own designs, but constrained by federal requirements—as described in Unemployment Insurance Policy Letter (UIPL) 10-14.[1] The intervention was low intensity, at most a few hours of group engagement and a few hours of one-on-one counseling. As such, the REA program generally would not be expected to generate large impacts, and given its low direct cost (usually less than $100 per UI claimant selected for the program), large impacts were not needed for the program to pass the cost-effective test.[2]

The requirements in UIPL No. 10-14 can usefully be thought of as having three parts, each of which might contribute to REA's impact:

1. Corresponding to "***Reemployment***" in the program name, REA funded *assistance* to UI claimants in their search for a new job.

2. Corresponding to "***Eligibility Assessment***" in the program name, REA funded *enforcement* of UI claimants' compliance with ongoing eligibility for the UI program (including that they were able and available for work and actively searching for a job).

3. The assistance and review of ongoing eligibility occurred at one or more in-person REA ***mandatory meeting***(s) with a state case manager—where "mandatory" meant that, subject to the due process protections provided to all UI program participants, noncompliance can result in denial or suspension of benefits.

Within the guidance provided by DOL, states had considerable discretion in how to design and implement their REA program. Using that discretion, states designed and implemented heterogeneous programs (Minzner et al., 2017).

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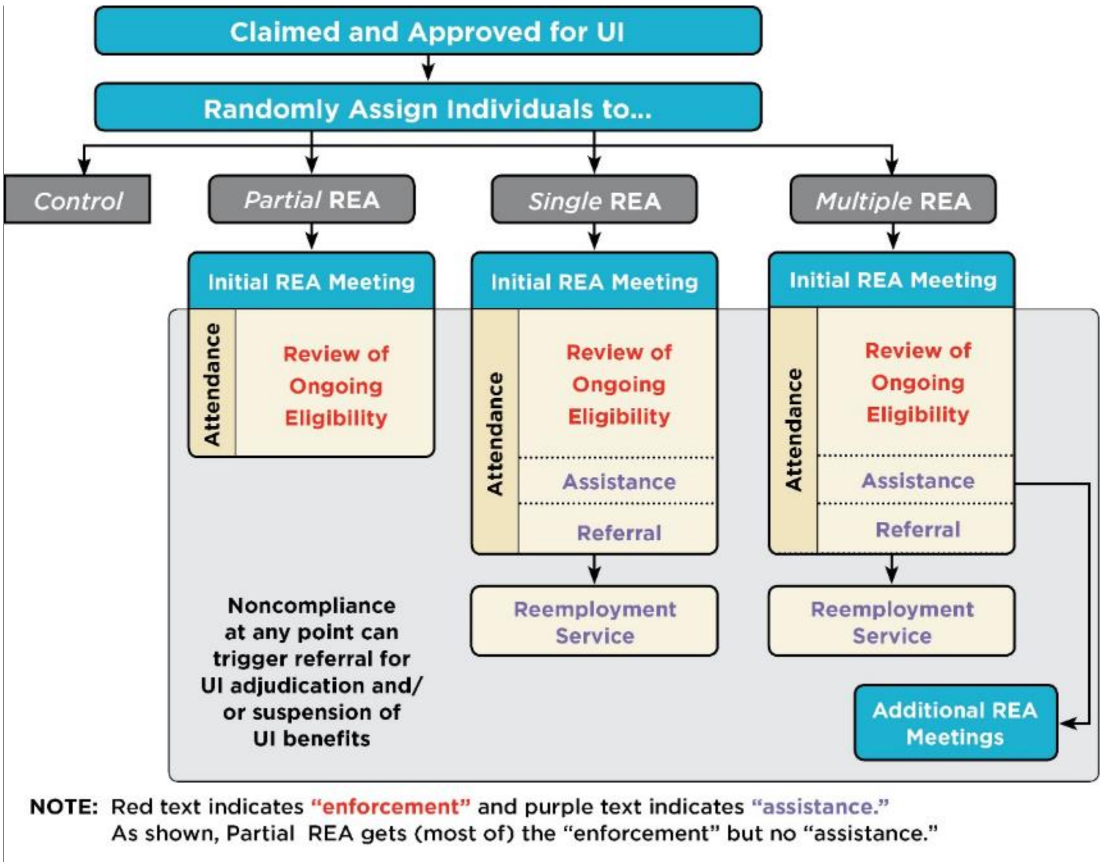
[1] *Fiscal year (FY) 2014 Unemployment Insurance (UI) Reemployment and Eligibility Assessment (REA) grants.* Retrieved from http://www.workforcesecurity.doleta.gov/dmstree/uipl/uipl2k14/uipl_1014.pdf

[2] Note, however, that the direct cost to the REA program is not the appropriate cost for a cost-benefit analysis. A cost-benefit analysis should consider additional costs to other programs (e.g., to the Wagner-Peyser Act program for the provision of some reemployment services) and perhaps the general equilibrium effects (i.e., that some of the increased earnings were because some program non-participant did not get a job).

The Design of the REA Impact Evaluation

The REA Impact Study worked with four participating states—Indiana, New York, Washington, and Wisconsin—to randomly assign nearly 300,000 UI claimants in a multi-arm design (see Exhibit ES-1).

Exhibit ES-1 Multi-Arm Random Assignment Design



Claimants were randomized to one of four treatment conditions, designed as follows:

- ***Control:*** No mandatory REA meeting, and no referral to reemployment services.
- ***Partial:*** Claimant summoned to an abbreviated REA meeting involving no assistance, and not referred for reemployment services.
- ***Single:*** Claimant summoned to one REA meeting, and referred to at least one reemployment service.
- ***Multiple:*** Claimant summoned to one REA meeting, referred to at least one reemployment service, and potentially summoned to one or two additional REA meetings.

For most analyses, we focus on comparing outcomes for claimants experiencing the *Control* condition versus outcomes for those experiencing the *Existing* condition—where *Existing* is whichever condition program model the state implemented in the absence of the evaluation. In Indiana, *Existing* was *Single;* in the other three states, *Existing* was *Multiple*.

This multi-arm random assignment design supports estimation of both the *overall impact* of the REA program and the *relative importance of enforcement versus assistance*.

The REA Implementation Study reported by Minzner et al. (2017) relied on qualitative field work. The REA Impact Study relies solely on state and federal administrative data. There was no claimant survey. States provided data on REA meetings (scheduled and attended), response to noncompliance, and weekly UI benefits claimed and paid—all for the current claim. The federal Office of Child Support Enforcement's National Directory of New Hires (NDNH) provided data on new hires and quarterly information on earnings and UI benefits paid—for several quarters before the claim and several quarters after the claim.

This REA evaluation is among the largest random assignment studies of a social program ever conducted in the United States. In its four participating states, the sample sizes could not have been much larger. Nearly every REA-eligible UI claimant in those states during the study intake period was randomized. Despite this highly inclusive design, the resulting sample sizes are only borderline sufficient to address some of the research questions. This appears to be because—consistent with the low intensity of the intervention—impacts are small. The smaller an impact, the larger the required sample to observe that impact. Because differential impacts are almost always much smaller than overall impacts, samples required for examining *how* a program causes its impacts (for REA, differential impacts of various program designs, differential impacts by claimant characteristics, for example) are approximately an order of magnitude (i.e., 10 times) larger than those required to estimate overall impacts.

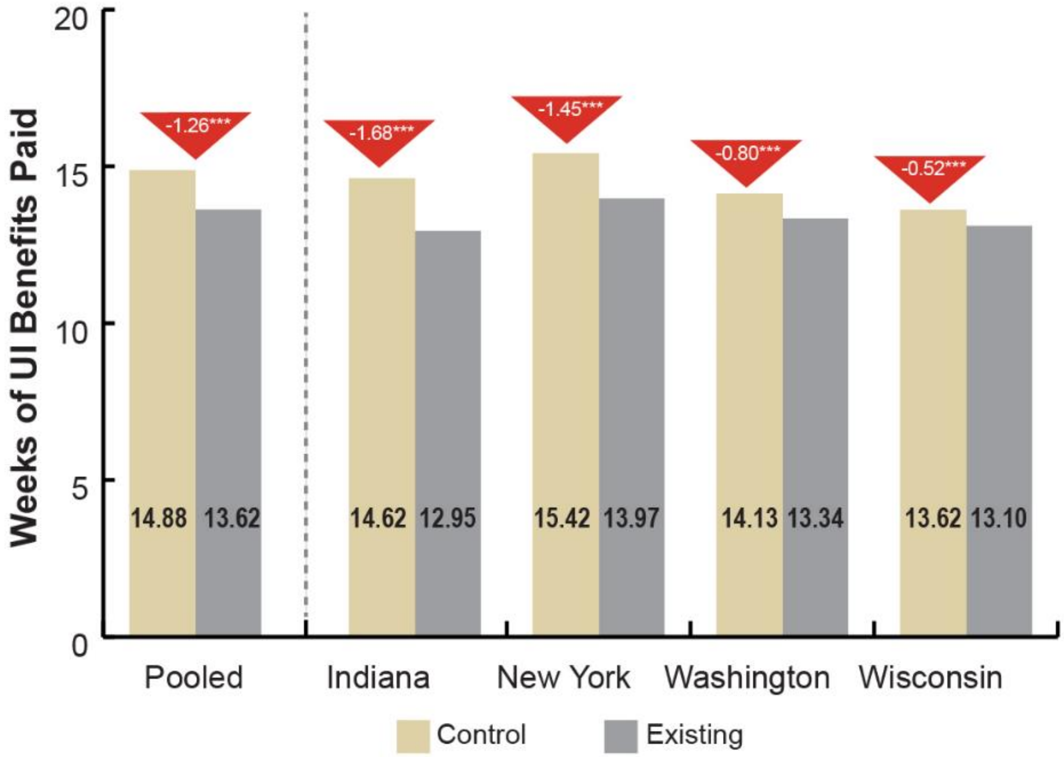
REA's Impact on UI Duration and Earnings

The major study findings are summarized below in red, with brief descriptions and additional context provided.

- **REA cuts duration of UI and benefits paid.**

Pooling the estimates across states, REA—that is, *Existing* vs. *Control*—cuts duration of UI and benefits paid (see Exhibit ES-2 below). This was the study's pre-specified single confirmatory outcome. The findings confirm unequivocally that REA cuts UI duration.

Exhibit ES-2 Impact on Weeks of UI Benefits Paid, by State



Source: Regression-adjusted impact estimates based on state administrative data, Model(s); 'UWSW28ec_zzz', Run Date: 29MAR2019
Note: Statistical significance levels for impacts are based on two-sided tests and flagged with asterisks, as follows: *** < 1 percent, ** < 5 percent, * < 10 percent.

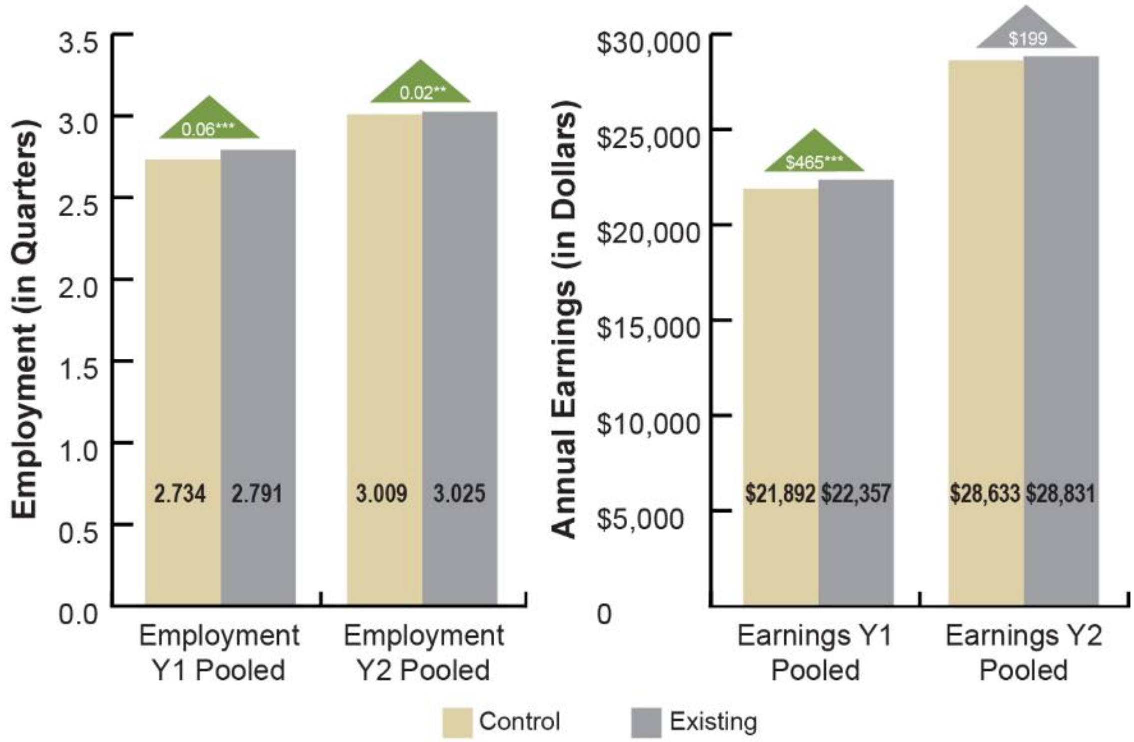
- **REA cuts UI duration, but the estimates vary substantially among the four states, ranging from about one and a half weeks to about half a week.**

In each of the four states, there is clear evidence that REA cuts UI duration, but the true impacts clearly differ among the four states (again see Exhibit ES-2). The estimated impacts in Indiana and New York imply REA cuts UI duration by about one and a half weeks; in contrast, estimated impacts in Washington and Wisconsin imply REA cuts UI duration by half a week. This difference in impacts among states is statistically significant.

- **REA raises short-term employment and earnings. The magnitude is only a small percentage of earnings.**

Pooling estimates across all four states, REA raises employment and earnings in year 1, the four quarters after the initial UI claim (see Exhibit ES-3 below). The impact on earnings is $465, about 2 percent of earnings for claimants not assigned to REA.

Exhibit ES-3 Impacts on Employment and Earnings for Years 1 and 2 (Pooled)



Source: Regression-adjusted impact estimates based on NDNH data, Model(s): EQNY01eczz, EQNY02eczz, EDNY01eczz, EDNY02eczz, Run Date: 10MAY2019
Note: Employment is sum of employment rate over four quarters. Statistical significance levels for impacts are based on two-sided tests and flagged with asterisks, as follows: *** < 1 percent, ** < 5 percent, * < 10 percent.

- **About half of the decrease in UI weeks is due to an increase in employment; the other half is due to more time not receiving UI and not employed.**

Combining the estimated impacts on UI weeks and earnings with information on the level of employment and earnings, it seems plausible to infer that about half of the decline in UI weeks is increased employment; the other half is increased time during which claimants are not receiving UI and are not employed.

- **REA has small positive impacts on employment past the benefit year.**

REA has small impacts on UI receipt and earnings in the longer term (three or more quarters after randomization) (again see Exhibit ES-3 above). Pooling estimates across states, REA has a small impact on employment in year 2, the four quarters after the benefit year. This may be the result of offsetting effects. Perhaps the assistance leads to better job matches, and thereby higher quarterly earnings after reemployment and less return to UI. Conversely, others conjecture that the enforcement would truncate job search, leading to worse job matches—leading to lower quarterly earnings and more return to UI. The evaluation finds mixed results, some significant in each direction, but never large.

How Impacts From the REA Program Vary With Claimant Characteristics

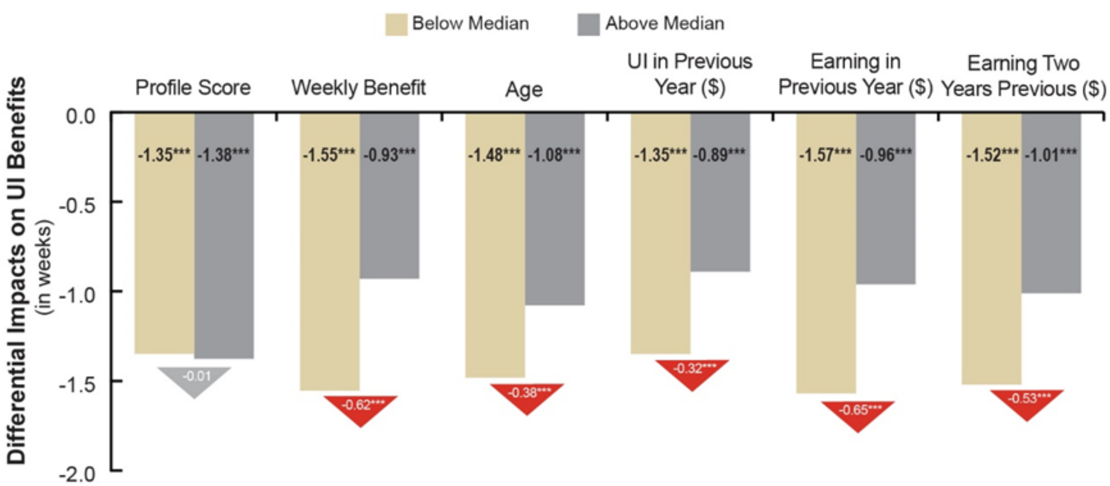
The study's samples are of sufficient size to detect at least larger differential impacts across claimant baseline characteristics.

- **Predicted likelihood of exhaustion does not clearly or strongly relate to the impact of the REA program.**

The Worker Profiling and Reemployment Services (WPRS) statute and the original (pre-2018) Reemployment Services & Eligibility Assessment (RESEA) statute both required targeting those claimants with the greatest probability of exhausting benefits (operationalized as a higher *profile score* that is the result of a statistical model predicting UI exhaustion). In addition, guidance from DOL's Office of Unemployment Insurance noted that many states were using a similar strategy for REA (see UIPL No. 17-13). A state might select UI claimants most likely to exhaust because they might be perceived as the neediest group. Alternatively, a state might select this group because it believed that they would have larger impacts (as would be true with an impact proportional to expected duration).

Differential impact by probability of exhaustion (i.e., profile score) is testable. The evaluation finds a mixed and weak relation between the predicted likelihood of exhaustion and the impact of the REA program (see Exhibit ES-4).

Exhibit ES-4 Differential Impacts of Subgroups on UI Benefits Paid (in weeks), *Existing* vs. *Control* (Pooled)



Source: Regression-adjusted impact estimates based on state administrative data, Model(s): UWSW28ec, Run Date: 29MAR2019
Note: Statistical significance levels for impacts are based on two-sided tests and flagged with asterisks, as follows: *** < 1 percent; ** < 5 percent; * < 10 percent.

- **REA has a larger impact on UI duration for claimants with lower earnings in the year prior to the initial UI claim and in the year before that, as well as for those with lower weekly benefit amounts.**

The evaluation also explored differential impacts with respect to claimant baseline characteristics (including recent individual labor market experience) and aggregate local labor market conditions. Pooling across the states, impacts on UI duration are more than twice as large for claimants with low earnings (below the median in the four quarters prior to the claim) as for those with high earnings (above the median). Similarly, impacts are larger for claimants with UI weekly benefit amount below the median and for younger claimants (again see Exhibit ES-4 above).

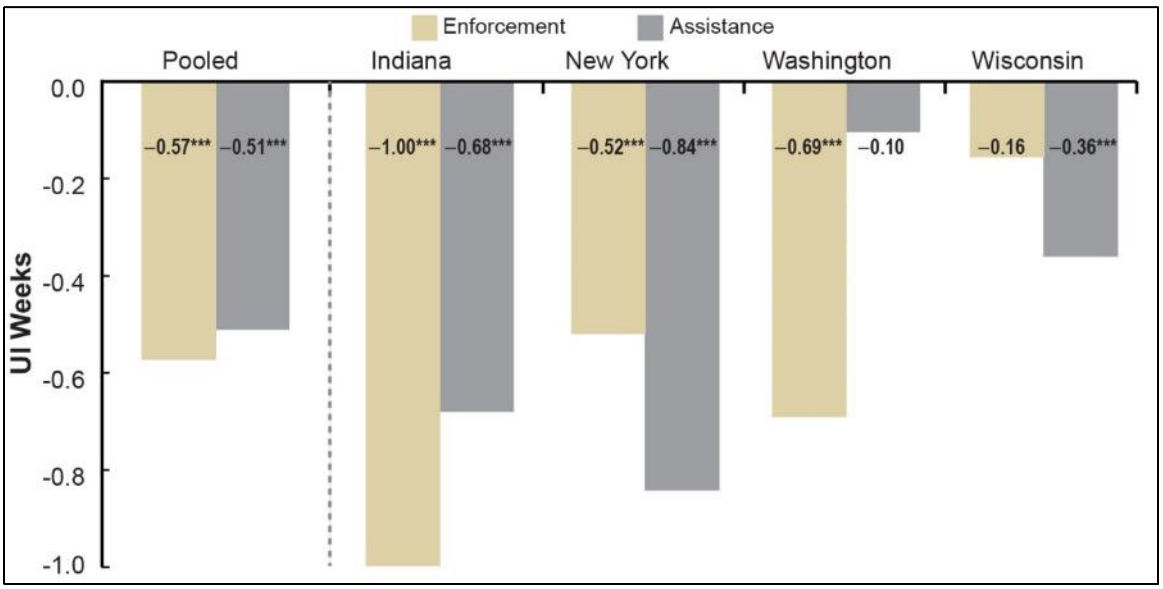
The Pathways Through Which REA Has Impacts

The study's multi-armed random assignment design was specifically intended to improve understanding of the pathways through which REA achieves its impacts. In particular, the *Partial* treatment condition was designed to include all of the enforcement (of the ongoing eligibility requirements, and the procedural requirement to attend the REA meeting), but none of the assistance. It would follow that comparing outcomes for *Partial* vs. *Control* estimates the impact of enforcement, and that comparing outcomes for *Existing* vs. *Partial* estimates the incremental impact of assistance above and beyond enforcement. Such studies of the impact of incremental additions to the program model require very large samples; the REA Impact Study's samples appear to be sufficiently large.

- **Both REA's enforcement and assistance have impacts on UI duration.**

Exploiting this multi-armed random assignment design, there is clear evidence for impacts on UI duration through both enforcement and assistance (see Exhibit ES-5). When interpreted as envisioned when the study design was developed, the results suggest approximately equal impacts of assistance and enforcement. However, considering all the results together (as detailed in the next two findings), a larger impact of enforcement than assistance might be the better interpretation.

Exhibit ES-5 Impact on UI Benefits Paid (in weeks) Through Enforcement vs. Assistance, by State



Source: Regression-adjusted impact estimates based on state administrative data, Model(s): UWSW28ec, Run Date: 29MAR2019

Note: Statistical significance levels for impacts are based on two-sided tests and flagged with asterisks, as follows: *** < 1 percent; ** < 5 percent; * < 10 percent.

- **Little enforcement of job search requirements results from REA meetings, yielding little estimated impact.**

Qualitative field work conducted for the REA Implementation Study suggested that intensively enforcing job search requirements was not a focus of REA meetings. Complementary with that inference, estimated impacts of REA (*Partial* vs. *Control* or *Existing* vs. *Control*) on the detection of ongoing eligibility issues (i.e., being referred for adjudication to enforce compliance with "able and available" status, sufficiently intensive job search, and accepting suitable job offers) are small—a few percentage points. This result suggests that that *Partial* vs. *Control* is probably not primarily estimating the impact of the enforcement of ongoing eligibility requirements.

- **REA's procedural requirement to attend the REA meeting leads to much of the estimated impact.**

Though *Partial* vs. *Control* was originally envisioned as estimating the impact of enforcement of ongoing eligibility requirements, in practice it incorporates two causal pathways—enforcement of UI eligibility and enforcement of the procedural requirement to attend the REA meeting. Further, it appears that enforcement of the procedural requirement explains most of the *Partial* vs. *Control* differential on UI duration and most of the overall impact of REA on UI duration.

Clearly, the stronger the enforcement of ongoing eligibility requirements, the smaller would be the relative importance of the procedural requirement to attend the REA meeting. Nevertheless, whenever noncompliance with the procedural requirement to attend the REA meeting leads to (near) uniform suspension of benefits until compliance, the impact of that policy is likely to be much larger than the impact of enforcement of other ongoing eligibility requirements.

This is true for two reasons. First, ongoing eligibility requirements can only be enforced for those who come to the REA meeting. Meeting rates are so far from universal that it would be hard for this pathway to be important. Second, suspension until compliance with the *procedural requirement to attend the REA meeting* can lead to the loss of many weeks of UI. In contrast, noncompliance with *other ongoing eligibility requirements* appears to lead only to loss of the week of noncompliance, and even that week is available at the end of the claim. The ongoing eligibility requirement could only have a large impact on UI weeks if the maximum lost number of weeks was larger (e.g., if detected noncompliance led either to loss of multiple weeks or to a requirement to demonstrate sufficient search in every week for the balance of the claim).

Furthermore, these results suggest that earlier studies of interventions that required a meeting—whether providing enforcement or assistance—estimated the joint impact of the content of the meeting and the effect of the program's response to the claimant's non-attendance at the meeting. This study's results suggest that in those earlier studies, the impact of the response to non-attendance at the meeting alone was likely often half a week or larger.

Concluding Discussion

The results of the REA Impact Study provide considerable new information about the impacts of the REA program—on various outcomes, on variation in those impacts with claimant characteristics, and on the pathways through which those impacts occur. These results were only possible because of multi-armed random assignment to program variants (with close monitoring for random assignment and treatment fidelity), considerable evaluation technical assistance to the states, very large samples, and a pure administrative-data strategy (i.e., no claimant survey). Each of these factors clearly is reproducible in other studies of REA-like programs and more broadly.

1. Overview

Most social programs providing cash assistance to unemployed workers also include personal contact with a state or local case manager. That personal contact has dual goals. One goal is assistance—improving the worker's job search skills and helping to identify appropriate jobs. The other goal is enforcement—verifying that the worker was initially eligible for cash assistance and is meeting the ongoing requirements for continued receipt, including participating in the assistance. From 2005 to 2016, a participating state's personal contact with Unemployment Insurance (UI) claimants was provided, in part, through the U.S. Department of Labor (DOL)'s Reemployment and Eligibility Assessment (REA) program—whose name underlines its dual goals.

DOL's Chief Evaluation Office (CEO) funded Abt Associates to conduct the **Evaluation of the Reemployment and Eligibility Assessment (REA) Program**. That evaluation worked with four states to randomly assign nearly 300,000 UI claimants to one of four combinations of assistance and enforcement services (four "treatment conditions"). The study specified those treatment conditions to provide insight not only into the overall impact of the REA program as implemented, but also into how the program achieved that impact. In particular, the study aimed to understand the relative importance of assistance and enforcement.

This REA Impact Study was among the largest random assignment studies of a labor market program ever conducted in the United States. Furthermore, the combination of extensive state administrative data on the details of client contact plus long-term follow-up through the National Directory of New Hires (NDNH) gives the study among the richest data of any evaluation of a U.S. labor market program.

Beginning in 2015, DOL began to replace REA with the Reemployment Services & Eligibility Assessment (RESEA) program model.[3] However, the impact estimates reported here for REA still are the most recent estimates for programs aimed at helping UI claimants to leave UI faster and assessing their associated labor market outcomes. An earlier report under this contract described implementation of the REA program in the four states (Minzner et al., 2017).[4] This is this contract's *Final Report*, providing estimates of impact.

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3 In FY2015, DOL's Office of Unemployment Insurance (OUI) released Unemployment Insurance Program Letter (UIPL) No. 13-15, which introduced guidance and funding for a new Reemployment Services and Eligibility Assessment (RESEA) grant program. RESEA was designed to replace REA, and its structure incorporates many elements of the REA program. The REA program as implemented under the study is no longer funded by DOL. States not already participating in this study began implementing RESEA in 2015. The four states participating in the study continued to deliver the REA program as outlined in UIPL No. 10-14 (FY2014 guidance), then transitioned to RESEA once random assignment was complete (approximately April 2016).

4 The *Implementation Report* is available online at https://www.dol.gov/asp/evaluation/completed-studies/REA-Impact-Study-Implementation-Report.pdf. It describes how the four states differed in their approach to providing REA services.

REA Program Chronology

2005—DOL implements the **Reemployment and Eligibility Assessment** (REA) program to accelerate the reconnection of UI claimants to the workforce.

2013 and 2014—DOL contracts with Abt Associates to evaluate the REA program's impact. See UIPL No. 10-14 for a description of REA as of the 2014 grant cycle, as the evaluation got underway.

2015—DOL begins replacing the REA program with the **Reemployment Services and Eligibility Assessment** (RESEA) program. See UIPL No. 13-15 for a description of RESEA.

2016—All state REA programs have been replaced by RESEA. For a list of RESEA grants made in 2016, see "U.S. Labor Department Awards $112M for RESEA Programs" (retrieved February 23, 2017).

This opening chapter briefly describes the REA program (Section 1.1), the goals of the evaluation and the design of the REA Impact Study to achieve those goals (Section 1.2), and the plan for the balance of this *Final Report* (Section 1.3).

1.1 The Reemployment and Eligibility Assessment (REA) Program

REA was a federal program through which participating states implemented strategies to address the reemployment needs of UI claimants and to prevent and detect UI improper payments. With federal government funding of very roughly $100 per claimant selected,[5] each participating state implemented REA according to the state's program design, but subject to federal guidance.

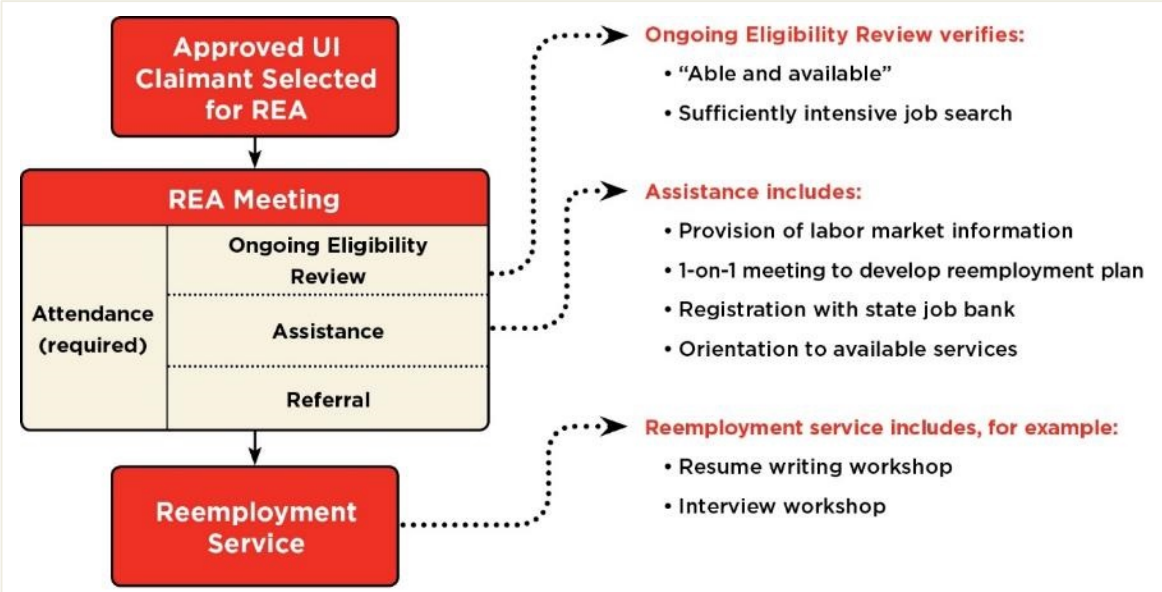
Exhibit 1-1 below depicts the basic program design and structure required of grantees.

[5] Our random assignment methods estimate impact per claimant selected ("intent-to-treat"), not impact per claimant who has an REA meeting (a form of "treatment-on-the-treated"). Analogously, for cost-benefit analysis, the ideal cost estimate would be per claimant selected (not per claimant scheduled, and certainly not per completed REA meeting). Unfortunately, there do not appear to be nationally available data on the number of claimants selected.

Instead, we can approximate REA cost per case selected as the ratio of total REA cost per state (from DOL press releases announcing REA grants and dollar amounts) to the number of UI claimants scheduled (from ETA 9128 data). Implied costs vary moderately across years. Using REA PY2014 (the last full year of the REA program), the relevant documents are https://www.dol.gov/newsroom/releases/eta/eta20141212 and https://oui.doleta.gov/unemploy/DataDownloads.asp. Computing the ratio of total cost across all states to total UI claimants scheduled yields a cost estimate of $87.71 per REA claimant scheduled. Because not every claimant selected is scheduled, costs per case selected would be slightly lower.

The Workforce Investment Act (WIA) Gold Standard Evaluation's cost study provides complementary estimates. Mastri and McCutcheon (2015) report the following unit costs of the components of reemployment services for WIA: resource room visit $16, structured assessment $13, Job Club per person $38, workshop per customer $54, and one-on-one counselor meeting $143. For these purposes, the relevant cost is per person, treating no-shows as not receiving services (e.g., about three-quarters of those selected would have a one-on-one consultation). Costs for workshops and Job Club would have been covered from WIA or Wagner-Peyser Act funds, not from REA funds. Therefore, the WIA estimates are roughly consistent (perhaps slightly higher) than the average cost per case computed from REA sources.

Exhibit 1-1 Overview of the REA Program



States were to select some UI claimants for REA; selected claimants subsequently were required to attend an REA meeting, which involved a UI eligibility review, assistance, and a referral to a reemployment service. Thus, the federal guidance required states to implement a program with both assistance components and enforcement components (including required attendance at an REA meeting).[6] Beyond that, DOL gave states wide latitude in designing and implementing their programs, resulting in considerable cross-state variation that shaped the evaluation and interpretation of the study findings.

1.1.1 REA's "Assistance" Components

DOL's REA program guidelines specified that shortly after UI claimants began to receive benefits, a subset of them were to be selected for and required to attend an initial one-on-one, in-person meeting at an American Job Center (AJC).[7] The first REA meeting consisted of a UI eligibility review (typically including a verification of required work search activities) and job-search-related assistance. This assistance was to include orientation to AJC services, provision of labor market information, development

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6 On the assistance/enforcement conceptualization, see Klerman (2012) and Klerman, Koralek, Miller, and Wen (2012).

7 Regarding selection, DOL's 2014 Unemployment Insurance Program Letter (UIPL) No. 10-14 noted that claimants predicted to be most likely to exhaust their benefits must be served under the state's Worker Profiling and Reemployment Services (WPRS) program. It follows that those most likely to exhaust are not in the sample for this REA study.

UIPL No. 10-14 listed other claimants to be excluded, including those with a definitive return-to-work date. In addition, DOL's guidance included language that seemed to encourage, but not require, selecting those most likely to exhaust who were not already selected for WPRS: "Many states elect to use the WPRS model to select REA participants and the treatment group. Those *claimants who are most likely to exhaust must be referred to WPRS services* and are excluded from the REA program" (p. 9, emphasis added). DOL's 2013 UIPL No. 17-13 made a similar point using slightly different language: "Many states have elected to serve claimants who are at a mid-range in the WPRS model selection. These individuals may benefit from participation in the REA program and are *not likely to need long term and intensive services*" (p. 5, emphasis added).

Beyond that, DOL's guidance allowed states flexibility in determining how to target the REA program to those who were eligible (e.g., whether to target those next hardest to serve or target those with minimal barriers).

of an individual reemployment plan (e.g., resume review, job referrals, ongoing employment coaching, training), and registration with the state's job bank. In addition, during the REA meeting the UI claimant was to be referred to a reemployment service at an AJC, which the claimant was then required to attend. (These reemployment services were funded not by the REA grant, but instead by other workforce funds.)

Federal guidelines also allowed states to require up to two additional REA meetings. Not all states conducted such additional REA meetings. Among states requiring additional REA meetings, rules varied concerning which claimants were scheduled for additional meetings. Such subsequent REA meetings were typically shorter than the first REA meeting, in part because no AJC orientation was provided. However, the additional meetings generally included a second (or third) eligibility review, provision of additional labor market information, and an update to the claimant's individual reemployment plan. Depending on the state, some subsequent REA meetings were in person, and others were on the phone.

1.1.2 REA's "Enforcement" Components

Whether in person or by phone, claimant attendance at REA meetings was mandatory. Federal guidance required state REA programs to refer claimants who failed to attend REA meetings to state UI adjudication.[8] Policies guiding determinations of program noncompliance and denials of benefits varied across states; procedures varied across offices within a state. REA case managers also had some discretion to respond to specific situations, resulting in variation within offices as to whether and when claimants were referred for adjudication.

Possible consequences for program noncompliance were—in increasing severity—no consequence; withholding the claimant's benefits until the claimant complied or for some fixed number of weeks (e.g., four weeks in New York), whichever came first; or withholding benefits until the claimant complied (e.g., until the REA meeting was attended). In almost all cases, the claimant's full period of UI eligibility was retained; that is, the individual could claim any denied benefits through the end of his/her benefit period. Chapter 5 provides additional details on study states' noncompliance policies as implemented.

Terminology: Randomly Assigned Persons

Unless noted otherwise, the text uses *claimant, participant,* and *individual* interchangeably to refer to a person randomly assigned to the study. In particular, "REA participant" refers to a UI claimant who was required by the state, under UI and REA program rules and procedures, to participate in REA. In reality, noncompliance was common (see Chapter 5). Thus, many of the UI claimants referred to as "REA participants" in the report did not actually participate in every required activity and sometimes did not participate in any activity.

[8] UIPL No. 10-14 states the following: "Failure to report or participate in any aspect of the UI REA must result in referral to adjudication of these issues under applicable state law. Claimants who contact the appropriate agency before their UI REA appointment and request to change the scheduled UI REA date or time for good reasons, such as scheduled job interviews, may be accommodated" (p. 7).

1.2 Evaluation Purposes and Design

In 2014, DOL/CEO awarded a contract to Abt for the evaluation of the REA program. Building on previous REA evaluations (Benus, Poe-Yamagata, Wang, & Blas, 2008; Poe-Yamagata et al., 2011; Michaelides, Poe-Yamagata, Benus, & Tirumalasetti, 2012), this REA Impact Study uses a random assignment research design for four purposes:

- *To estimate the impact of the REA program in the short term*—on UI benefits received, on nonmonetary detection actions (i.e., being referred for adjudication for failure to comply with program requirements), and on employment and earnings, where "short term" is defined as the first six months during which a claimant could continuously receive UI benefits;
- *To estimate the impact of the REA program in the longer term*—on UI benefits received, employment, and earnings through the end of the current benefit year (12 months) and beyond;
- *To model how those impacts varied with participant characteristics*; and
- *To understand how various ways the REA program was implemented and operated contributed to its overall impact*—for example, the relative roles of enforcement and assistance, and conducting multiple versus single REA meetings.

To address these study purposes, the evaluation recruited four states—Indiana, New York, Washington, and Wisconsin—to participate in the random assignment study. Exact dates varied across states. Across the states, randomization began in March-April 2015 and continued for approximately one year. States provided data from approximately the start of randomization through approximately six months after the end of randomization.

For more on these four states (their pre-evaluation REA programs and how they implemented the treatment conditions), see Minzner et al. (2017). This *Final Report* deliberately does not repeat the detail provided in that earlier document.

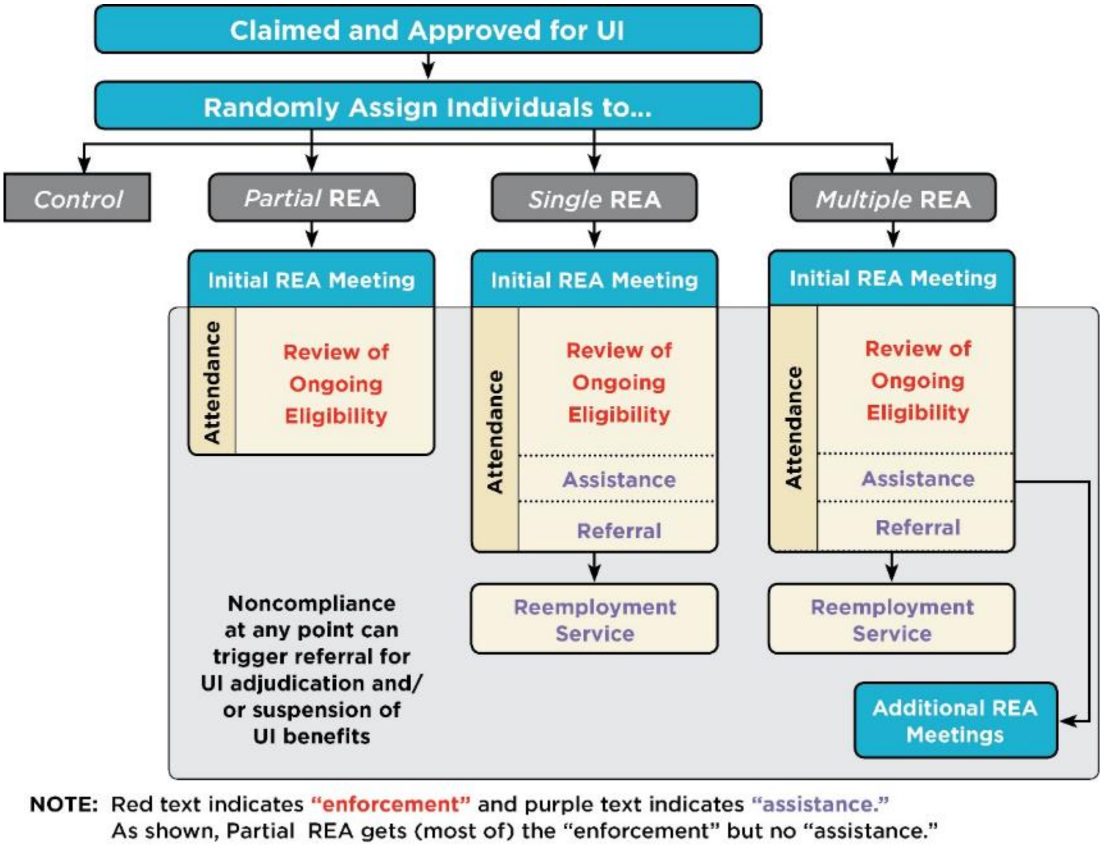
1.2.1 Four Treatment Conditions

Across the four states, UI claimants were randomized into one of four treatment conditions[9] following Abt's multi-arm random assignment design (Exhibit 1-2 below):

- ***Control:*** No mandatory REA meeting, and no referral to reemployment services.
- ***Partial:*** Claimant summoned to an abbreviated REA meeting involving no assistance, and not referred for reemployment services.
- ***Single:*** Claimant summoned to one REA meeting, and referred to at least one reemployment service.
- ***Multiple:*** Claimant summoned to one REA meeting, referred to at least one reemployment service, and potentially summoned to one or two additional REA meetings.

[9] In one sense, *Control* is the absence of treatment. Nevertheless, here and in the balance of the document, we refer to *Control* as a "treatment condition." Doing so simplifies the writing.

Exhibit 1-2 Multi-Arm Random Assignment Design



Terminology: Treatment Conditions

- The term *treatment condition* refers to the set of services to which UI claimants might be randomized. In the context of this analysis (unless otherwise noted), the term *treatment condition* includes the three treatment groups (*Partial, Single,* and *Multiple* REA) and also the control group (*Control*), whose members were to receive no eligibility assessment and no referral to reemployment services.
- Throughout this report we italicize the names of treatment conditions (*Control*, *Partial*, *Single,* and *Multiple*) and aggregates of those treatment conditions (*Existing*).

1.2.2 Impact Estimates

This *Final Report* reports intent-to-treat (ITT)[10] estimates of the other treatment conditions relative to the control condition and estimates of differential impact for all other treatment contrasts. In this REA Impact Study design:

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[10] These analyses are ITT in that they give the impact of a UI claimant being *assigned* to a treatment condition, whether the claimant *received* the treatment or not (e.g., due to a no-show or an implementation error on the part of the state REA program).

- To estimate the impact of a single REA meeting versus no REA meeting, we compare outcomes for *Single* versus *Control*.
- To estimate the impact of enforcement, without assistance, we compare outcomes for *Partial* versus *Control*.
- To estimate the impact of assistance, above and beyond enforcement, we compare outcomes for *Single* versus *Partial*.
- To estimate the impact of multiple REA meetings versus a single REA meeting, we compare outcomes for *Multiple* versus *Single*.

The primary analyses compare outcomes for the state's *Existing* REA program (i.e., what the state implements in the absence of the evaluation) versus *Partial* or *Control*. In New York, Washington, and Wisconsin, the *Existing* treatment condition was *Multiple*. The exception was Indiana, where *Existing* was *Single*.

1.3 Plan for This Document

The balance of this *Final Report* proceeds as follows. The next chapter (**Chapter 2**) provides context for the rest of the Impact Study, reviewing the theory of action for REA and REA-like programs and the previous literature on REA and REA-like programs. **Chapter 3** describes the high-level design of the evaluation.

The subsequent two chapters describe the REA programs studied. **Chapter 4** provides a primarily descriptive analysis of attendance at the REA meeting(s) for each study state's existing REA program (i.e., the *Existing* treatment condition) and the detection of eligibility issues for that group. For the state's existing REA program, **Chapter 5** describes each state's response to a claimant's noncompliance with program rules, in particular non-attendance at the REA meeting. These analyses in Chapters 4 and 5 almost exclusively use the state administrative data.

The next two chapters describe the impact of the REA program on outcomes of interest and how that impact varies with individual and local labor market conditions. Using both state administrative data and NDNH data, **Chapter 6** reports estimates of the impact of the treatment conditions on measures of UI benefit receipt—weeks and dollars received in the current UI benefit year and subsequent return to UI (at all and benefit received). **Chapter 7** reports estimates of the impact of the treatment conditions on labor market outcomes—time to reemployment, ongoing employment, and earnings.

The final chapter (**Chapter 8**) summarizes the results, attempts to put them in the context of the broader literature, and considers implications for policy and future research.

Appendices. A separate appendix volume provides additional detail. **Appendix A** develops a formal economic theory of REA-like programs. **Appendix B** provides additional detail on the econometric specification and other estimation issues. **Appendix C** provides additional results corresponding to Chapters 6 and 7.

2. Context

This chapter provides both a conceptual and a comparative context for the evaluation. In the context of the general theory of social insurance programs, Section 2.1 examines REA and REA-like programs as a response to the moral hazard induced by cash assistance—in this case, the UI benefit. Section 2.2 provides a simple logic model for REA and REA-like programs (Appendix A provides a formal economic theory based on neo-classical economic models and dynamic programming). The next two sections survey the existing literature. Section 2.3 concerns studies of REA. Section 2.4 concerns the broader literature on REA-like programs in the United States. That broader review of the literature focuses on the relative importance of assistance and enforcement.

2.1 Social Insurance and Moral Hazard

Income protection for workers who become unemployed through no fault of their own—Unemployment Insurance—is a standard feature of the social safety net of developed countries. The primary purpose of this form of social insurance is to provide some income support during temporary periods of involuntary unemployment. Secondarily, such programs might allow workers to be more selective about accepting job offers, potentially resulting in better skill matches between workers and jobs, and thereby raising overall economic productivity (Blaustein, Cohen, & Haber, 1993). Finally, such programs are an automatic stabilizer for the economy, injecting additional income (and resulting consumption) into it during economic downturns (Chimerine, Black, & Coffey, 1999).

The fundamental challenge in program design is as follows. More generous benefits better cushion the hardship of unemployment and might lead to better job matches (that is, jobs that pay more and last longer). However, more generous benefits might also reduce the incentive to search for a job and accept offers of employment.

The empirical literature is consistent with this concern. Studies show that for most recipients, the intensity of job search is low—only a few hours per week (Krueger & Mueller, 2010; Aguiar, Hurst, & Karabarbounis, 2011). Furthermore, larger UI benefits (per week, relative to pre-unemployment weekly wages) and longer periods of eligibility for UI benefits lead to longer UI spells (Moffitt, 1985; Katz & Meyer, 1990; Card & Levine, 2000; Fredriksson & Holmlund, 2006a, 2006b; Røed, Jensen, & Thoursie, 2002; see Chetty (2008) for a perspective less focused on moral hazard).[11] Evidence for whether more

[11] For the impact of benefit levels on duration of UI receipt in the United States, see Nickell (1997) and Blanchard and Wolfers (2000); for the impact of benefit levels in Europe, see Tatsiramos and van Ours (2011) and Card, Chetty, and Weber (2007).

Many studies have examined the impacts of the maximum UI duration on UI receipt. For earlier U.S. studies of maximum duration impacts, see Moffitt (1985), Meyer (1990), Katz and Meyer (1990), and Card and Levine (2000); for more recent U.S. studies, see Valletta and Kuang (2010), Rothstein (2011), Grubb (2011), Hagedorn, Manovskii, and Mitman (2015), Farber, Rothstein, and Valetta (2015), and Johnston and Mas (2018). For a review of European studies, see Hunt (1995) and Tatsiramos and van Ours (2011). Klerman (2012) and Klerman et al. (2012) review these literatures.

generous UI benefits result in better job matches is mixed.[12] Given that more generous UI benefits lead to longer unemployment spells, there is a tension between program goals that support consumption during involuntary unemployment and encourage sincere and intensive job search.

One way to avoid this tension (i.e., to provide income to the unemployed without discouraging job search) is to both provide income and require intensive job search. Indeed, the theoretical analyses of Fredriksson and Holmlund (2006a, 2006b) and Boone, Fredriksson, and Holmlund (2007) imply that monitoring and penalties are both parts of an optimal UI program.

In practice, job search intensity is not observed. A program can only monitor proxies for the intensity of job search—for example, number of employers contacted, number of interviews—and penalize claimants who do not satisfy the requirement for the proxies. Of course, the proxies are imperfect measures of the true intensity of job search (i.e., perhaps the employers contacted are inappropriate, interviews are sabotaged, suitable job offers are refused).[13]

As we discuss later in Section 2.4, there is evidence that for the United States, more intensive monitoring of job search intensity shortens UI duration and sometimes increases earnings. European evidence is mixed (see the review by Lachowska, Meral, and Woodbury [2016, Section 1.1]).

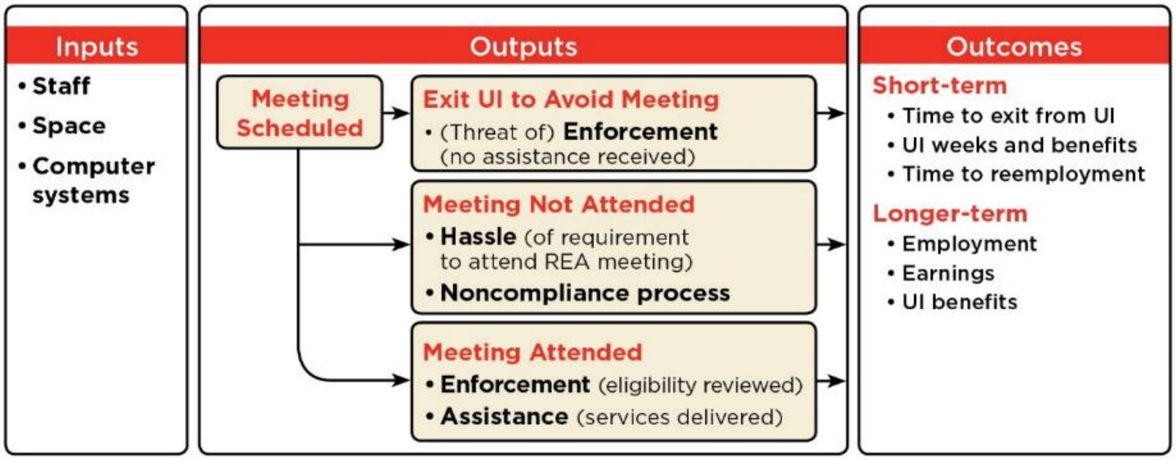
2.2 A Logic Model for REA and REA-Like Programs

Reemployment arises from the interaction of workers, employers, and government assistance—for this document, the REA program. How might assistance and enforcement through REA and REA-like programs influence the outcomes of interest—time on UI (and UI benefits), employment and earnings, and return to UI? Exhibit 2-1 provides a conventional logic model for REA and REA-like programs. As in conventional logic models, the exhibit identifies inputs, outputs, and outcomes. Going beyond a conventional logic model—towards what is sometimes called a "theory of action"—the exhibit also identifies three pathways through which the core activity of the REA program—the REA meeting—could affect outcomes.

[12] On the relation between potential UI duration and subsequent earnings in the United States: Card et al. (2007) find no impact; Schmieder, von Wachter, and Bender (2016) find a negative effect. In the non-U.S. literature, there are several studies of "natural experiments" (i.e., changes in policy). Nekoei and Weber (2017) find a positive effect for Austria; van Ours and Vodopivec (2008) find no effect for Slovena; Lalive (2007) finds no effect for Austria; Lindner and Reizer (2016) find no effect for Hungary.

[13] A complementary approach, not discussed more here, is for benefits to decline over time.

Exhibit 2-1 A Logic Model for the REA Program



As shown in the exhibit:

- the program applies ***inputs***—staff, space, computer systems…
- generating ***outputs***—invitations to the REA meeting; exits from UI by some claimants to avoid the REA meeting; attendance by some, allowing review of their eligibility and provision of assistance; no-shows by some, a referral to the noncompliance process…
- yielding ***outcomes***—in the short term, time to UI exit and time to reemployment; in the long term, UI benefits paid in the benefit year and employment, earnings, and return to UI in future benefit years.

Our analysis generates evidence that describes many components of this logic model. Chapters 4 and 5 report tabulations of outputs for the *Existing* treatment condition. Outputs are not the focus of this study, however. **Rather, the focus of this REA Impact Study is impacts—that is, *outcomes* for one treatment condition relative to outcomes for some other treatment condition, holding all else equal.** Chapters 6 and 7 present estimates of impact.

2.2.1 Outcomes

Some of our analyses in this report treat the REA program as a "black box," meaning we look at the program's outputs and outcomes, but not at *how* the former cause the latter. Specifically, treatment conditions induce impacts on short-term and longer-term outcomes, but the extent to which the outputs implied by the logic model occur and occur differentially across treatment conditions is not considered.

Because we have state administrative data, the REA Impact Study can and does go beyond treating the REA program as a black box. These state administrative data include information about many of the outputs in Exhibit 2-1, including about the first REA meeting—(a) scheduled meetings: at all and when; (b) eligibility issues identified at the meeting and more broadly; and (c) steps of the noncompliance process (e.g., UI payments suspended, nonmonetary issues identified, overpayments identified). As appropriate for the state and the treatment condition, similar information is available for second and third REA meetings.

2.2.2 Causal Pathways

The REA program might induce outcomes through several different pathways. Here we discuss those pathways and highlight the implications for impacts estimated using our rich data. We discuss the assistance pathway first, as it is the most often mentioned by federal, state, and local staff. That order of discussion differs from the order of the boxes in Exhibit 2-1, which depicts the pathway of assistance last, because in practice other pathways operate before (and sometimes lasting until) assistance is provided.

Assistance

Through REA meetings, claimants receive job search assistance (e.g., help creating and implementing an individual reemployment plan), specific referrals to reemployment services (e.g., resume-writing workshops), and an introduction to the broader workforce system and the additional assistance it provides. This is the "reemployment" element in the REA program's name. In discussions with the evaluation team throughout the project, federal, state, and local staff all consistently emphasized this causal pathway—nearly to the exclusion of other causal pathways (see also Minzner et al., 2017).

In discussions with the evaluation team, federal staff described the theory of action as *assistance leading to faster reemployment and therefore faster UI exit and lower UI benefits paid.* Economic theory suggests that assistance might also lead to better job matches and therefore more employment, higher earnings, and less return to UI—and continuing well past initial reemployment. Complementarily, even if the job matches are no better, faster reemployment might lead to higher earnings through greater experience and job tenure[14] and less "scarring" from long periods of unemployment.[15]

Exhibit 2-1 above deliberately emphasizes the *limited conditions under which assistance can be the causal pathway leading to impacts*. Only claimants who attend the REA meeting receive assistance. Even among attendees, any impact of assistance can occur only long enough after the REA meeting for the assistance to have an effect—to lead to more or better job leads and an acceptable job offer, and then for the job to actually start. In order to build on this insight about the timing of any impact of assistance, the empirical analysis in Chapter 4 disaggregates impacts by time since randomization and since the scheduled REA meeting.

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[14] Empirical labor economics (Becker, 1964; Mincer, 1974; Heckman, Lochner, & Todd, 2006; Lemieux, 2006) suggests that greater experience (i.e., at all employers) and greater job tenure (i.e., at this employer) lead to higher earnings, perhaps because of the accumulation of additional "human capital"—both better broad job skills and better skills at this particular job. Note that any differences in experience and tenure induced by REA are likely to be measured in weeks (at most a few months). In contrast, impacts of experience and tenure are usually measured in years. This suggests that any impact of faster or slower reemployment on longer-term earnings is likely to be trivially small.

In contrast, it is plausible that more assistance and a longer job search (supported by UI benefits) would lead to better job matches, higher hourly earnings, and less return to UI. The standard example is someone with specific job skills. Assistance might help that person to locate a job that uses those skills, perhaps at a different type of employer. UI benefits might allow the claimant to wait until a job using the skills can be located and/or becomes available. Note that, implicitly, this last argument is less salient the more flexible the person is on the job search (i.e., take a low-skill job now, then while employed continue looking for a more suitable job).

[15] Such scarring might arise from atrophy of skills or from a perception by potential employers that something is wrong with workers who stay unemployed for a long period of time (e.g., "they lack a strong work ethic" or "other potential employers saw something negative that we are missing").

Direct Effect of Enforcement

The REA meeting is to include a review of the claimant's initial and ongoing eligibility for UI. This is the "eligibility assessment" element in the REA program's name. REA eligibility efforts focus on *ongoing* eligibility issues; that is, once benefits begin, the requirement that UI claimants must certify every week that they remain eligible for benefits by being "able and available" for work, compliant with program requirements, pursuing a sufficiently intensive job search, and have not refused a suitable job offer. REA eligibility efforts might also detect *initial* eligibility issues. Initial eligibility refers to the requirements that must be satisfied in order for claimants to begin receiving UI benefits. Among other criteria, these include having sufficient past earnings (monetary eligibility issues) and having been involuntarily separated from previous employment (separation issues).

Enforcing the ongoing eligibility requirements requires a strategy to verify compliance and to address noncompliance. The REA meeting is potentially a crucial component of such a strategy to assess the "able and available" and sufficiently intensive job search requirements because it provides program staff an opportunity to conduct an eligibility review of a claimant's compliance. More rigorous enforcement of the job search requirement was a crucial component of some of the tests of UI eligibility reviews in the late 1990s and early 2000s, often with an elevated threshold for what proof would satisfy that requirement (Section 2.4 provides a review).

This causal pathway should lead to fewer weeks of UI benefits. Retrospective attempts to verify ongoing eligibility requirements—"able and available," sufficiently intensive job search, and has not refused a suitable job offer—might uncover that noncompliance, leading to a denial of UI for that week. The more often job search activity is verified, the more weeks the claimant might lose. This is the *direct* effect of enforcement on this claimant. In some cases, the loss of UI may lead to faster reemployment. Inasmuch as the job search was cut short, job matches might be worse, possibly leading to lower longer-term earnings and faster return to UI. In addition, REA meetings might detect issues with initial eligibility that may lead to termination of UI benefits and an expectation that benefits already received will be repaid.

As with assistance, this direct and standard effect of enforcement—that is, the detection of ongoing eligibility issues—can only occur for those claimants who attend the REA meeting.

Indirect Effect of Enforcement

Enforcement can also have an *indirect* effect. The claimant's expectation (perhaps incorrect) of increased scrutiny of initial and ongoing eligibility can lead to faster UI exit and reemployment. In particular, this exit might occur before the scheduled REA meeting. Anticipating the burden of complying with the REA program requirements and perhaps the threat of loss of UI benefits stemming from any eligibility issues that might be uncovered at the meeting, some claimants might leave UI before they became noncompliant.

From an a priori perspective, this indirect enforcement pathway seems unlikely to be important. Even if a claimant's plan is to not attend the REA meeting, *there does not appear to be a large incentive to exit UI before the meeting.* Instead, the optimal strategy would appear to be to continue claiming and receiving any UI benefits that the state pays out.[16]

Indirect enforcement can lead to fewer weeks of UI benefits. Again, perhaps the loss of UI would lead to faster reemployment; perhaps the loss of any assistance would lead to slower reemployment and worse job match (including lower short-term earnings and faster return to UI).

Response to Non-Attendance at the REA Meeting

The REA program guidance in UIPL No. 10-14 is explicit that attendance at the REA meeting is "mandatory" (p. 5). We refer to this requirement to attend the REA meeting as "procedural," because the requirement only exists because of the REA program and for claimants selected for that program. Claimants not selected for REA have no such requirement. The REA meeting may help to enforce other ongoing eligibility requirements, but those requirements exist whether or not the claimant is selected for REA. Mandatory or not, not every claimant will attend the scheduled REA meeting. On this point, UIPL No. 10-14 states that non-attendance should refer the claimant to "adjudication…under applicable state law" (p. 7). The results of that adjudication itself might directly lower UI weeks and payments in the short term.[17]

This causal pathway is different from enforcement. This pathway is not related to anything that happens at the REA meeting; instead, it is a response to noncompliance with the procedural requirement merely to attend that meeting. Such a causal pathway will be potentially effective for any program that provides assistance or enforcement through a required meeting.

Two factors imply that the impact of uniform and immediate suspension of UI benefits until compliance in response to non-attendance is potentially huge. First, a large fraction of those claimants scheduled never attend—more than a quarter. Second, in the absence of a requirement to attend an REA meeting, the

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[16] To see this point, note that under current policy, if noncompliance is discovered and the state responds, the claimant receives no benefit for that week; if noncompliance is not discovered or the state does not respond, the full benefit for that week is received. Thus, the noncompliant claimant is no worse off—and may be better off—by claiming than by exiting UI.

Now consider an alternative policy. Suppose insufficiently intensive job search resulted instead in retrospective loss of benefits or limitation on future benefits. In that case, a claimant's decision whether to claim must balance (a) the advantage of payment for this week if either noncompliance is not discovered or the state does not respond *versus* (b) a multiweek suspension of benefits if noncompliance is discovered and the state responds. As the probability of discovery and state response and the size of the state response grow, claiming given willful noncompliance becomes less attractive.

In reality, the consequence is not more than a week, and it is far from certain that noncompliance will be discovered and that the state responds. These are the conditions under which anticipatory exit by the claimant is not optimal. Instead, the optimal behavior would be to continue claiming—even given willful noncompliance.

[17] We discuss the noncompliance process in more detail in Chapter 5. Here we note that finding that a claimant is unable to attend the meeting because the claimant is already employed or otherwise unable or unavailable for work might lead to loss of eligibility for all UI benefits. Alternatively, missing the meeting without a valid reason might lead to loss of benefits for a specific week or set of weeks in the short term; but in the long term, the total number of weeks of eligibility across the 12-month benefit year is unchanged and the claimant can claim the lost weeks later in the year. Few claimants use their entire claim; for most, they receive an appropriate job offer before their UI benefits are exhausted. In that case, the lost weeks are never paid because claimants don't need the weeks later, resulting in lower benefits paid overall.

average number of weeks of UI claimed from when the meeting would have occurred through the end of the benefit year is large: more than nine weeks.[18]

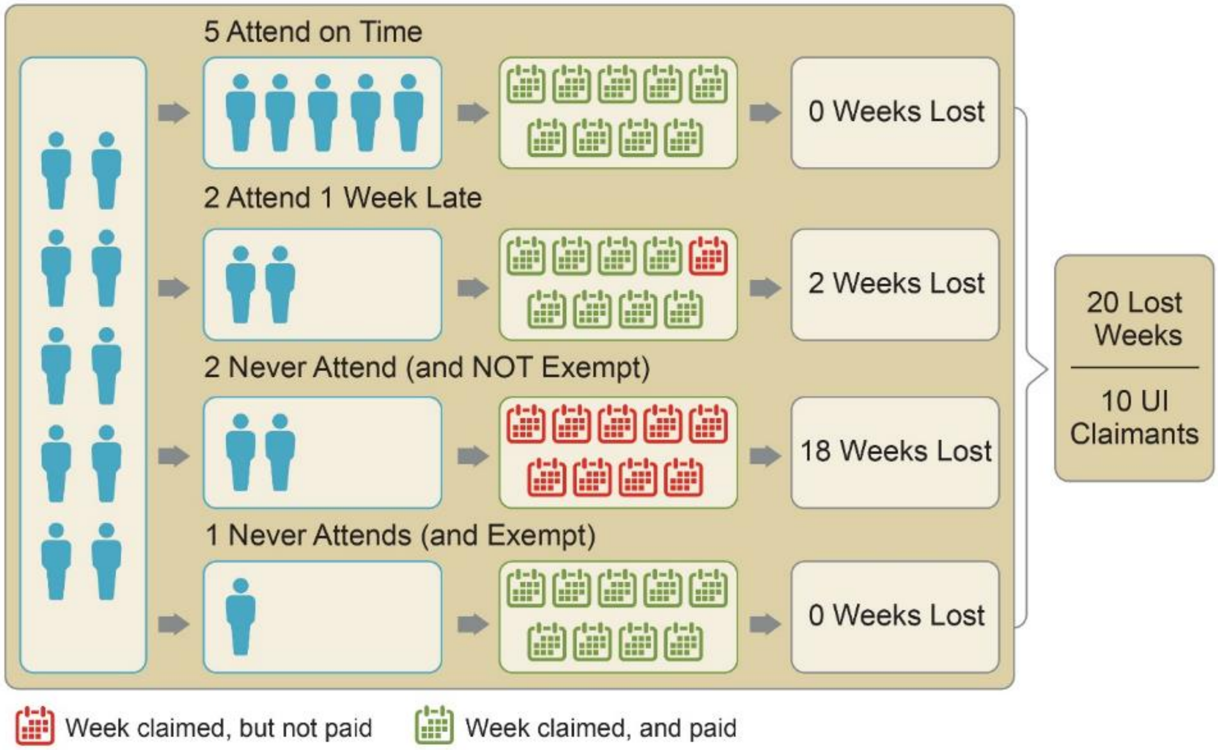
Exhibit 2-2 below reports a simple simulation of the effect on UI weeks of uniform and immediate suspension of benefits until compliance in response to non-attendance. The simulation implicitly assumes that uniform and immediate suspension of UI benefits until compliance would not raise attendance relative to current attendance rates. This is too strong an assumption in reality; some, perhaps considerable, increase in attendance seems likely. From this perspective, the simulation should be viewed as an upper bound.[19] With that crucial caveat, the simulation implies a **potential impact on UI weeks of response to non-attendance at the UI meeting alone of up to two weeks per claimant. That is larger than nearly every *total* estimated impact in the literature.**[20]

[18] If anything, nine weeks is an underestimate. We can use the *Control* condition to approximate weeks of UI if there were no requirement to attend the REA meeting. Tabulations of our data for *Control* find that , the average numbers of weeks of UI benefits from the modal week in which the REA meeting occurred are as follows: 10.4 in Indiana, 14.1 in New York, 12.9 in Washington, and 9.5 in Wisconsin (measured from the last day for which the REA meeting could be self-scheduled). From this perspective the simulation is too low. This consideration at least partially offsets the fact that the simulation is too high because it fails to incorporate induced increased attendance.

[19] Note, however, that increased attendance would have countervailing effects. The additional assistance received itself would be expected to lower UI duration (through faster employment) as would increased enforcement of other ongoing eligibility requirements (through responses to the noncompliance).

[20] The simulation in Exhibit 2-2 assumes a response of uniform and immediate suspension of benefits until compliance to non-attendance at the REA meeting. In practice, state procedures are not rigid: for some claimants, benefits are never suspended or benefits are only suspended with a lag (where every week of lag is a week less of suspended benefits for those claimants who will never comply). Note also that self-scheduling (as in Wisconsin) would push back the date at which the suspension of benefits begins, thereby cutting the total number of weeks lost for those who never comply.

Exhibit 2-2 Simulation of Potential Impact on UI Duration of a Response to Non-Attendance at the REA Meeting of Uniform and Immediate Suspension of Benefits Until Compliance



The simulation in Exhibit 2-2 focuses on impacts on UI weeks. Whether and how such a uniform and immediate suspension of benefits until compliance response to non-attendance at the REA meeting would lead to increased employment and earnings are unclear. The lack of a UI benefit gives claimants a stronger incentive to search intensively for and accept more job offers. This would lead to more employment and (at least in the short term) higher earnings. In addition, the threat (and perhaps imposition) of such a response gives claimants a stronger incentive to attend the REA meeting and receive the job search assistance[21]—which will, in turn, lead to faster reemployment and perhaps better job matches. Such additional job search assistance would also lead to more employment and higher earnings (perhaps even in the longer term).

In practice, response to noncompliance will vary based on existing state laws regarding claimants' rights and due process, administrative processes for adjudication, actual attendance rates, and staff discretion to respond to claimants' unique circumstances. Nonetheless, the simulation in Exhibit 2-2 suggests that regardless of verification of other ongoing eligibility requirements or of assistance, that **even a response less than uniform and immediate suspension of benefits until compliance could explain much of the estimated impact.** This appears to be true for this REA evaluation, for earlier REA evaluations, and for any earlier evaluation of a program with a required in-person meeting (because non-attendance was likely common at those meetings, as well).

[21] As noted, such increased attendance would shrink the impact on UI duration (below that implied by the simulation).

For the REA programs in the four states studied, Chapters 4 through 7 of this report provide evidence on the *actual* impact of each of these causal pathways: assistance, enforcement (of ongoing eligibility requirements), and the procedural requirement to attend the REA meeting.

2.3 Earlier Evaluations of REA

Prior to this REA Impact Study, researchers had evaluated REA programs in six states using random assignment research designs. Many but not all those evaluations found that REA reduces UI duration and total benefits. Exhibit 2-3 below presents the estimates of impact on weeks of UI from those earlier evaluations of the REA program, as discussed in the next paragraphs.[22]

Impacts on Weeks of UI and Required Sample Sizes

For weeks of UI benefit receipt, half the estimates—Florida, Idaho, one of the Minnesota treatments, and Nevada—imply a statistically significant decline. Furthermore, sample size appears to explain much of the pattern of statistical significance. With the exception of Minnesota, the states with statistically significant impacts have sample sizes of more than 18,000 claimants; the states with statistically insignificant impacts have sample sizes of less than 4,000. It thus seems plausible to infer that REA probably had an impact in the latter states but their sample sizes were too small to detect it. A (random effects) meta-analysis would provide formal evidence on this conjecture.

Though some (perhaps most) of the inter-study variability is due to sampling variability (i.e., too-small samples), some of the variability is probably real. That is, there would be (substantial) variability in impact even with infinitely large samples in each site. Simple inspection of Exhibit 2-3 below is consistent with the conjecture that true impacts vary. As noted, the samples in Florida, Idaho, and Nevada are quite large; as a result, the estimates are precise: 95 percent confidence intervals of 0.20 weeks (a fifth of a week) or less. The difference in estimates across the states is much larger than that: 0.40 to 0.80 to 1.80 weeks. Again, a (random effects) meta-analysis would provide formal evidence on this conjecture.

This observation—that the sample sizes in some earlier studies were likely too small to detect plausible impacts—has crucial implications for interpreting the results presented in this *Final Report*. The REA Impact Study deliberately recruited larger states and continued randomization for a year. The resulting sample sizes are quite large, more than 24,000 in each state (see Exhibit 3-2). That said, given estimated impacts, sample sizes required to detect overall impact (i.e., *Existing* vs. *Control*) are large: at least—and probably more than—2,500 claimants per treatment condition, and thus at least 5,000 for a simple treatment/control study.

[22] The exhibit reports the results from the official evaluation reports. Reanalyses of these data have appeared in academic journals (see Michaelides & Mueser, 2018; Manoli, Michaelides, & Patel, 2018). The reanalyses are not included in Exhibit 2-3, but are reviewed in the discussion of Exhibits 2-5, 2-6, and 2-7 later in this section.

Exhibit 2-3 Estimated Impacts on UI Outcomes From Earlier Evaluations of REA Programs

Study	State	N	UI Weeks			Probability of Benefit Exhaustion (ppt)			UI Receipt		
			Effect Estimate		s.d.	Effect Estimate		s.d.	Effect Estimate		s.d.
Benus et al., 2008	Minnesota T1[a]	3,582	−0.73	(0.49)	14.40				$13	(168)	4,977
	Minnesota T2[a]	2,860	−1.16	(0.48)**	12.70				$100	(169)	4,462
	North Dakota adjusted	670	−1.05	(0.74)	9.60				−$136	(207)	2,679
Poe-Yamagata et al., 2011	Florida adjusted	80,531	−0.43	(0.07)***	9.90	−0.034	(0.006)***	0.850	−$101	(20)***	2,838
	Florida unadjusted	80,531	−0.40	(0.05)***	7.10	−0.022	(0.003)***	0.430	−$164	(17)***	2,412
	Idaho adjusted	18,156	−0.45	(0.09)***	6.10	−0.032	(0.009)***	0.610	−$97	(24)***	1,617
	Idaho unadjusted	18,156	−0.50	(0.10)***	6.70	−0.029	(0.009)**	0.610	−$164	(44)***	2,964
	Illinois adjusted	3,112	−0.83	(0.54)	15.10	−0.011	(0.019)	0.530	−$148	(114)	3,180
	Illinois unadjusted	3,112	−0.80	(0.30)**	8.40	−0.011	(0.019)	0.530	−$24	(130)	3,626
Michaelides et al., 2012	Nevada adjusted	32,751	−1.82	(0.12)***	10.90	−0.103	(0.007)***	0.630	−$536	(38)***	3,438
	Nevada unadjusted	32,751	−2.10	(0.10)***	9.00	−0.120	(0.007)***	0.630	−$558	(52)***	4,705
Weighted mean (adjusted estimates)			−1.01	(0.55)*	10.56	−0.035	(0.014)**	0.596	−$165	(145)	2,855
Without Nevada			−0.92	(0.58)	10.68	−0.019	(0.015)	0.589	−$133	(152)	2,704
Weighted mean (unadjusted estimates)			−0.86	(0.19)***	8.17	−0.034	(0.013)***	0.580	−$149	(81)*	3,468
Without Nevada			−0.65	(0.20)***	8.03	−0.019	(0.014)	0.571	−$80	(86)	3,257

Note. Standard errors are reported in parentheses. Impacts on UI weeks are reported for Regular UI benefits, not Emergency Unemployment Compensation or Extended Benefits. "Weighted mean (adjusted estimates)" computed from adjusted estimates for Florida, Idaho, Illinois, Nevada, and North Dakota. Weighting is by the square root of the sample size. "Weighted mean (unadjusted estimates)" computed from unadjusted estimates for Florida, Idaho, Illinois, and Nevada. Weighting is by the square root of the sample size. The three studies listed are random assignment evaluations of the REA program conducted by IMPAQ International. Michaelides et al. (2012) is a follow-up to the Poe-Yamagata et al. (2011) report and examines the state of Nevada in more detail. The reported standard deviations ("s.d.") are inferred based on the standard errors and sample sizes.

[a] Minnesota had a multi-treatment condition study with two treatment groups. Those in T1 received one REA meeting; those in T2 received multiple REA meetings.

***$p < .01$, **$p < .05$, *$p < .10$

Differential impacts (e.g., *Partial* vs. *Control*, *Existing* vs. *Partial*, *Multiple* vs. *Single*) will almost always be much smaller than the overall impact. Because the overall impact (i.e., *Existing* vs. *Control*) is the sum of *Partial* vs. *Control* and *Existing* vs. *Partial*, one of those two differential impacts must be less than half the overall impact. A standard result in statistical power analysis is that detecting an impact half as large requires a sample four times as large. Thus, if a simple treatment/control study requires a sample of 5,000 claimants (2,500 each for treatment and control), an *Existing* vs. *Partial* vs. *Control* study would require at least a sample of 10,000 per treatment condition; that is, a total sample of at least 30,000. A four-armed study such as this one—*Partial* vs. *Control* vs. *Single* vs. *Multiple*—would require a sample of at least 40,000 claimants.

Because in most states in the REA Impact Study, samples were roughly 10,000 claimants per treatment condition, statistical power is likely an issue for some analyses (e.g., assistance versus enforcement, subgroups, employment, and earnings). Put differently, even with our very large sample sizes, we probably fail to detect some impacts that are truly present. Consistent with that analysis, our discussion of results pays careful attention to issues of statistical power.

Returning to the substance, the minimum variance weighted mean of the adjusted estimates is a 1.01 weeks decrease. Note, however, that Nevada’s adjusted impact of 1.82 weeks is clearly an outlier. No other adjusted impact is greater than 1.20 weeks. Excluding Nevada, the weighted mean of the adjusted estimates is 0.92 weeks. Given the evidence just presented that true impacts vary, the simple minimum variance weighted mean is not the ideal summary measure. A more appropriate measure would be the mean across sites from a (random effects) meta-analysis.

Finally, note that the large impact estimates for Nevada were well known when the evaluation team tried to recruit states for this study. At that time, we encouraged states to adopt a program consistent with the Office of Unemployment Insurance’s characterization of the Nevada program—that is, strong integration of UI staff and Wagner-Peyser Act employment services staff. No state expressed any interest in doing so.

Impacts on Other UI-Program Outcomes

In addition to the impact on weeks of UI benefits received, earlier studies considered several other UI-program outcomes, including exhaustion of benefits and amount of benefits received. The studies found similar patterns. The weighted mean impact on the probability of exhausting Regular UI benefits is a decrease of about three and a half percentage points (−0.035). Again, there is considerable variation. The patterns are broadly similar to the pattern for weeks of UI benefits received. In particular, Nevada is again an outlier. Excluding Nevada, the weighted mean impact falls by nearly half, to a decrease of about two percentage points (−0.019).

Similar patterns are also present for the impact on the amount of UI benefits received. Weighted mean impact suggests an overall decline of $165 over the benefit year, again with considerable variation across sites. Excluding Nevada, the weighted mean impact falls to a decline of $133.

Impacts on Employment and Earnings

In the official reports, estimates of impacts on earnings post UI are available only for Florida and Nevada. The results are different across outcomes and states. Both Florida and Nevada had negative and strongly statistically significant impacts on weeks of UI benefits and UI benefits paid, but as shown in Exhibit 2-3 above, the estimate for Nevada was four times the estimate for Florida (1.82 weeks versus 0.43 weeks). Perhaps consistent with that divergence in magnitude of the impact on weeks of UI, there is strong divergence in the impacts on employment and earnings between the two states (see Exhibit 2-4).

Exhibit 2-4 Estimated Impacts on Employment and Earnings From Earlier Evaluations of REA Programs

		Effect Estimate						
Employment (ppt)								
Study	State	Q1	Q2	Q3	Q4	Q5	Q6	Total
Poe-Yamagata et al., 2011	Florida	0.012** (0.005)	0.017** (0.007)	0.022*** (0.006)	0.015** (0.006)			
Michaelides et al., 2012	Nevada	0.074*** (0.008)	0.081*** (0.008)	0.061*** (0.008)	0.059*** (0.008)	0.055*** (0.008)	0.043*** (0.008)	
Earnings ($)								
Study	State	Q1	Q2	Q3	Q4	Q5	Q6	Total
Poe-Yamagata et al., 2011	Florida	$52 (60)	$206*** (50)	$129** (55)	$89 (55)			$476*** (179)
Michaelides et al., 2012	Nevada	$376*** (52)	$498*** (59)	$508*** (68)	$473*** (70)	$386*** (81)	$369*** (75)	$2,611*** (322)

Note. Standard errors are reported in parentheses. Estimates are for each full calendar quarter following the start of a UI claim. The two studies listed are random assignment evaluations of the REA program conducted by IMPAQ International. Michaelides et al. (2012) is a follow-up to the Poe-Yamagata et al. (2011) report and examines the state of Nevada in more detail.
***p < .01, **p < .05, *p < .10

For Florida, impacts on employment, though positive, are small—between one and two percentage points (from a control group mean that ranges across calendar quarters from 35 percent to 42 percent; not shown). There is also evidence of impacts on earnings in Q2 ($206) and Q3 ($129).

In contrast, Nevada had much larger impacts on weeks of UI; and it had large impacts on both employment (five to 10 percentage points from control group means of 40 to 50 percent; not shown) and earnings (over six quarters, $2,611 from a control group mean of $14,361; not shown), suggesting faster employment (and minimal negative effect of truncated job search).

Exhibits 2-3 and 2-4 presented the results from DOL's previous REA evaluation final reports. More recently, Michaelides and Mueser (2017, 2018) conducted reanalyses of data from three REA programs (Florida, Idaho, and Nevada) and of a pre-REA program (also in Florida). Exhibits 2-5 and 2-6 below present their results for UI weeks and employment and earnings outcomes, respectively.

As should be expected, findings from these reanalyses are generally similar to the earlier final reports. Here we note some differences, apparently due to small changes in the analytic samples. Relative to the original analyses conducted by Poe-Yamagata et al. (2011), the Michaelides and Mueser 2017 reanalysis restricted the sample of claimants to those assigned over a slightly shorter time period. The 2018 paper restricted the sample in Nevada to those claimants in the Las Vegas and Reno metro areas.

Exhibit 2-5 Estimated Impacts on UI Outcomes From Reanalysis

Study	State	N	UI Weeks Effect Estimate	
Michaelides & Mueser, 2017	Florida pre-REA	34,945	−0.13	(.08)*
	Florida REA	39,906	−0.47	(.07)***
	Idaho	12,645	−0.43	(.10)***
	Nevada	21,898	−1.71	(.13)***
Michaelides & Mueser, 2018	Nevada	31,793	−1.90	(0.1)***

Note. Standard errors are reported in parentheses. The two studies listed are random assignment evaluations of REA programs. Both include reanalyses of REA programs evaluated by Poe-Yamagata et al. (2011).
***p < .01, **p < .05, *p < .10

Exhibit 2-6 Estimated Impacts on Employment and Earnings From Reanalysis

			Effect Estimate						
Employment (ppt)									
Study	State	N	Q1	Q2	Q3	Q4	Q5	Q6	Total
Michaelides & Mueser, 2017	Florida pre-REA	34,945	.010 (.005)**	.009 (.005)*	.007 (.005)	.011 (.005)**			
	Florida REA	39,906	.011 (.005)***	.017 (.005)***	.022 (.005)***	.018 (.005)***			
	Idaho	12,645	.005 (.011)	.021 (.011)**	.027 (.011)**	.023 (.011)**			
	Nevada	21,898	.066 (.009)***	.076 (.009)***	.059 (.009)***	.058 (.009)***			
Michaelides & Mueser, 2018	Nevada	31,793	.070 (.008)***	.082 (.008)***	.066 (.008)***	.063 (.008)***	.052 (.008)***	.046 (.008)***	
Earnings ($)									
Study	State	N	Q1	Q2	Q3	Q4	Q5	Q6	Total
Michaelides & Mueser, 2017	Florida pre-REA	34,945	$32 (50)	$42 (47)	$3 (50)	$7 (53)			$85 (162)
	Florida REA	39,906	$74 (48)	$110 (47)**	$99 (48)**	$75 (51)			$370 (159)**
	Idaho	12,645	$64 (67)	$131 (61)**	$169 (76)**	$92 (77)			$455 (222)**
	Nevada	21,898	$294 (59)***	$461 (66)***	$502 (76)***	$482 (81)***			$1,740 (232)***
Michaelides & Mueser, 2018	Nevada	31,793	$315 (51)***	$493 (59)***	$542 (68)***	$504 (70)***	$348 (81)***	$404 (75)***	$2,607 (322)***

Note. Standard errors are reported in parentheses. Estimates are for each full calendar quarter following the start of a UI claim. The two studies listed are random assignment evaluations of REA programs. Both include reanalyses of REA programs evaluated by Poe-Yamagata et al. (2011).
***$p < .01$, **$p < .05$, *$p < .10$

Like the previously published evaluation reports, Michaelides and Mueser's reanalyses of REA program data found reductions in weeks of UI for all three states and for Florida's pre-REA program (Exhibit 2-5).

With regard to employment and earnings (Exhibit 2-6), the 2016 and 2018 reanalyses of Nevada's program also reported strongly significant impacts on both outcomes. The reanalysis of Florida's REA program also found impacts of similar magnitude and significance. Over the first four full quarters after the initial UI claim, the reanalysis found that Florida's REA program significantly increased earnings by $370 over Q1 to Q4, somewhat smaller than the $476 impact reported by Poe-Yamagata et al. in 2011. In their 2017 study, Michaelides and Mueser added impacts for employment and earnings in Idaho. Like the reanalysis of Florida's REA program, Idaho's yielded small (about two percentage points) increases in quarterly employment and modest increases in earnings, smaller than those of Nevada's program.

Finally, in a 2018 paper, Manoli, Michaelides, and Patel published another reanalysis of the Nevada REA program data, matching UI records with administrative tax data to analyze long-term impacts on employment and earnings over several years following randomization (Exhibit 2-7). Using a study sample identical to that of the 2011 and 2012 REA impact reports, Manoli et al. report significant impacts that persist six years following program participation, with impacts comparable to those in the first year; smaller impacts on employment over time (from about five percentage points to about three percentage points); and some increase for earnings (from about $1,400 to about $2,000). These long-term impacts suggest the possibility that the short-term, low-intensity REA intervention could have longer-term impacts.

Exhibit 2-7 Estimated Impacts on Employment and Earnings at Long-Term Follow-Up of Nevada REA

			Effect Estimate					
Employment (ppt)[a]								
Study	State	N	Y1	Y2	Y3	Y4	Y5	Y6
Manoli, Michaelides, & Patel (2018)	Nevada	321,751	.051 (.007)***	.045 (.007)***	.041 (.007)***	.033 (.007)***	.029 (.007)***	.034 (.007)***
Earnings ($)[b]								
Study	State	N	Y1	Y2	Y3	Y4	Y5	Y6
Manoli, Michaelides, & Patel (2018)	Nevada	32,751	$1,361 (456)***	$1,413 (456)***	$1,932 (529)***	$1,666 (460)***	$1,656 (464)***	$2,087 (483)***

Note. Standard errors are reported in parentheses.
[a] W-2 employment from tax years 2010-2015.
[b] W-2 earnings from tax years 2010-2015.
****p* < .01, ***p* < .05, **p* < .10

2.4 Earlier Evaluations of U.S. Unemployment Insurance Job Search Assistance Programs

Prior to the REA studies, an earlier generation of random assignment UI studies estimated impacts across a variety of interventions.[23] They showed consistent reductions in UI duration (see Exhibit 2-8 below), but inconsistent impacts on employment and earnings (see Exhibits 2-9 and 2-10 below).

Scanning Exhibit 2-8, most estimated impacts on UI duration are about half a week. Several impacts are well over two weeks (i.e., two of the enforcement interventions and one of the profiling interventions). Impacts for assistance and enforcement together are not clearly larger than for either intervention alone. Better inference would require formal meta-regression.

Exhibit 2-8 Earlier Studies of Enforcement and Assistance Intervention: Impacts on UI Duration

Tested Change	State	Sample Size	Impact (weeks)[a]	Study
Enforcement Interventions (Without Assistance)				
Stronger work test	South Carolina	2,845	−0.55*	Corson et al., 1985
No work search requirements[b]	Washington	5,117	−3.34**	Johnson & Klepinger, 1991
Individualized work search policy	Washington	4,835	0.17	
Report four employer contacts weekly	Maryland	13,223	−0.72**	Klepinger et al., 2002
Two contacts required weekly; no reporting	Maryland	13,168	0.36*	
Report two contacts weekly and both verified	Maryland	13,113	−0.86**	
Standard work test	Washington	3,145	−3.28***	Lachowska et al., 2016
Modified work test (accelerated eligibility review interview)	Washington	2,679	−3.24***	

[23] Only random assignment studies are reviewed. Toohey (2017) provides non-experimental evidence that increasing work search requirements increases work search and probably decreases unemployment. The estimates are not precise enough to detect impacts on employment. Toohey's work is focused on differential effects with the aggregate labor market conditions. Nevertheless, his results suggest that additional, cross-state, non-experimental analyses of the effect of job search requirements might be insightful.

Tested Change	State	Sample Size	Impact (weeks)[a]	Study
Assistance Interventions (Without Enforcement)				
Intensive services	Washington	5,424	−0.47*	Johnson & Klepinger, 1991
Job search assistance	New Jersey	4,801	−0.47**	Corson & Haimson, 1996
Job search assistance, plus training or relocation assistance	New Jersey	6,195	−0.48**	
Structured job search assistance	DC	4,038	−1.13***	Decker et al., 2000
	Florida	6,046	−0.41*	
Individualized job search assistance	DC	4,034	−0.47*	
	Florida	6,021	−0.59**	
Individualized job search assistance, plus training	DC	4,023	−0.61**	
	Florida	6,003	−0.52**	
Profiled by Worker Profiling and Reemployment Services system and referred for early job search assistance	Connecticut	62,029	−0.25**	Dickinson et al., 1999
	Illinois	58,975	−0.41***	
	Kentucky	50,162	−0.21*	
	Maine	15,536	−0.98***	
	New Jersey	220,087	−0.29***	
	South Carolina	50,178	0.02	
Profiled by Worker Profiling and Reemployment Services system and referred for early job search assistance reemployment services	Kentucky	1,981	−2.24***	Black, Smith, Berger, and Noel, 2003
Claimant Employment Program	Nevada	2,832	−2.1***	DOL, 1990
Assistance With Enforcement Interventions				
Stronger work test, plus enhanced placement services	South Carolina	2,660	−0.76**	Corson et al., 1985
Stronger work test, plus job search workshop	South Carolina	3,026	−0.61*	
Report two contacts weekly, plus participate in a 4-day job search workshop	Maryland	13,393	−0.59***	Klepinger et al., 2002

Note. Categorization of interventions based on authors' review.

[a] Weeks of Regular UI benefits received.

[b] Intervention represents a softening of UI enforcement rules, which has the effect of increased UI duration. The sign of the estimated impact was flipped relative to the study's published estimate.

***$p < .01$, **$p < .05$, *$p < .10$

Exhibits 2-9 and 2-10 below show quarterly impacts on labor market outcomes for these earlier studies, adjusted to 2017 dollars. Impacts on employment rarely exceed one to two percentage points; as a whole, these studies rarely found significant impacts on employment. Of the 22 studies that included impacts for the first full quarter after the start of the claim, five studies reported significant impacts. Two of those five impacts were in the "wrong" direction; that is, they reduced employment. Even fewer studies reported significant impacts on earnings in Q1. Only one intervention increased earnings in the first quarter after the start of the claim, a WPRS program implemented in Maine.

Exhibit 2-9 Early Enforcement and Assistance Intervention: Impacts on Employment and Earnings

Tested Change	State	Sample Size	Quarterly Employment Impact				Study
			Q1	Q2	Q3	Q4	
Enforcement Interventions							
No work search requirements	Washington	5,117	−0.02	N/A	N/A	N/A	Johnson & Klepinger, 1991
Individualized work search policy	Washington	4,835	0.02	N/A	N/A	N/A	
Report four employer contacts weekly	Maryland	13,223	0.01	0.01	0.00	0.00	Klepinger et al., 2002
Two contacts required weekly, but no reporting	Maryland	13,168	0.02*	0.01	0.02**	0.01	
Report two contacts weekly and both verified	Maryland	13,113	0.01	0.01	0.01	0.01	
Standard work test	Washington	3,145	0.03*	N/A	N/A	N/A	Lachowska et al., 2016
Modified work test (accelerated eligibility review interview)	Washington	2,679	0.04**	N/A	N/A	N/A	
Assistance with Enforcement Interventions							
Report two contacts weekly, plus participate in a 4-day job search workshop	Maryland	13,393	0.00	−0.01	−0.01	−0.01	Klepinger et al., 2002
Assistance without Enforcement Interventions							
Job search assistance	New Jersey	4,801	0.00	0.02	0.01	0.01	Corson & Haimson, 1996
Job search assistance, plus training or relocation assistance	New Jersey	6,195	0.02	0.00	0.01	0.00	
Profiled by Worker Profiling and Reemployment Services system and referred for early job search assistance reemployment services	Kentucky	1,981	N/A	N/A	N/A	N/A	Black, Smith, Berger, and Noel, 2003
Profiled by Worker Profiling and Reemployment Services system and referred for early job search assistance	Connecticut	62,029	0.01	−0.01	−0.01	0.00	Dickinson et al., 1999
	Illinois	58,975	−0.01*	−0.01**	0.00	0.01	
	Kentucky	50,162	0.00	0.00	−0.01*	0.00	
	Maine	15,536	0.01	0.00	0.01	0.02	
	New Jersey	220,087	−0.02***	−0.01**	−0.02***	−0.02***	
	South Carolina	50,178	−0.01	−0.01	−0.01	−0.02***	
Structured job search assistance	DC	4,038	0.02	0.02	0.01	0.02	Decker et al., 2000
	Florida	6,046	0.01	0.01	0.01	−0.01	
Individualized job search assistance	DC	4,034	0.02	0.01	0.03*	0.03*	
	Florida	6,021	0.01	0.01	0.02	0.02	
Individualized job search assistance, plus training	DC	4,023	0.02	0.00	0.02	0.00	
	Florida	6,003	0.00	0.02*	0.02	0.00	

Note. Categorization of interventions based on authors' review.
***p < .01, **p < .05, *p < .10

Exhibit 2-10 Early Enforcement and Assistance Intervention: Impacts on Earnings (2017 Dollars)

Tested Change	State	Sample Size	Quarterly Earnings Impact				Study
			Q1	Q2	Q3	Q4	
Enforcement Interventions							
No work search requirements	Washington	5,117	−$67	N/A	N/A	N/A	Johnson & Klepinger, 1991
Individualized work search policy	Washington	4,835	$117	N/A	N/A	N/A	
Report four employer contacts weekly	Maryland	13,223	$39	−$31	$26	$56	Klepinger et al., 2002
Two contacts required weekly, but no reporting	Maryland	13,168	$117	$142	$162*	$152*	
Report two contacts weekly and both verified	Maryland	13,113	$29	$38	$28.	$110	
Standard work test	Washington	3,145	$176	N/A	N/A	N/A	Lachowska et al., 2016
Modified work test (accelerated eligibility review interview)	Washington	2,679	$164	N/A	N/A	N/A	
Assistance with Enforcement Interventions							
Report two contacts weekly, plus participate in a 4-day job search workshop	Maryland	13,393	$23	$76	$130	$38	Klepinger et al., 2002
Assistance without Enforcement Interventions							
Job search assistance	New Jersey	4,801	$278	$586	$381	$109	Corson & Haimson, 1996
Job search assistance, plus training or relocation assistance	New Jersey	6,195	$182	$229	$185	$171	
Profiled by Worker Profiling and Reemployment Services system and referred for early job search assistance reemployment services	Kentucky	1,981	$849	$553	$354	$57	Black, Smith, Berger, and Noel, 2003
Profiled by Worker Profiling and Reemployment Services system and referred for early job search assistance	Connecticut	62,029	$59	$8	$105	$13	Dickinson et al., 1999
	Illinois	58,975	$47	$102	$105*	$76	
	Kentucky	50,162	$49	$65	$147*	$4	
	Maine	15,536	$206**	$98	$248*	$274*	
	New Jersey	220,087	$31	$202***	$65	$58	
	South Carolina	50,178	$67	$20	$108	$181	
Structured job search assistance	DC	4,038	$48	$276**	$239**	$439***	Decker et al., 2000
	Florida	6,046	$85	$6	$85	$3	
Individualized job search assistance	DC	4,034	$35	$163	$311	$251**	
	Florida	6,021	$77	$9	$28	$76	
Individualized job search assistance, plus training	DC	4,023	$48	$321**	$384**	$181	
	Florida	6,003	−$52	$43	$30	$109	

Note. Categorization of interventions based on authors' review.
****p* < .01, ***p* < .05, **p* < .001

In general, those analyses that reported significant impacts were able to detect impacts on earnings comparable to those discussed for REA programs above (as seen in Exhibit 2-5 and Exhibit 2-6). For example, Maine's WPRS program consistently increased earnings by about $200 in several quarters of follow-up.

Also, note that statistical power is likely a major issue. Employment in a quarter is a binary outcome, so it is easy to compute required sample sizes, in the absence of covariates. In fact, the impact of covariates is sufficiently small, such that ignoring them does not make a large difference.

Then, making standard assumptions (alpha=80 percent, beta=5 percent, two-tailed test, equal treatment/control split), detecting an impact of two percentage points requires slightly fewer than 10,000 claimants per treatment condition (i.e., 20,000 for a simple treatment/control design). More precisely, sample sizes are about 10,000 for a baseline rate of 50 percent (the worst case) and slightly smaller for 30 percent (which is closer to what is observed). Detecting an impact of one percentage point requires slightly fewer than 40,000 claimants per treatment condition.

Only four studies that included assistance with enforcement have total sample sizes greater than 10,000: Maryland's pre-REA study was about 26,000 for any given contrast, Idaho's REA was about 12,000, Nevada's REA was about 20,000, and Florida's REA and pre-REA studies were both more than 30,000. These are the studies that found impacts on labor market outcomes. The sample sizes for this REA Impact Study are borderline to detect labor market impacts: probably large enough to detect larger impacts (e.g., a two-percentage-point impact on employment in a quarter), but not large enough to detect smaller impacts (e.g., a one-percentage-point impact on employment).

Finally, there is—experimental and non-experimental—evidence on the impact of assistance, from studies of Workforce Investment Act (WIA) Intensive Services.

Using experimental methods (including random assignment) the WIA Adult and Dislocated Worker Program Evaluation estimated the impact of WIA Intensive Services relative to WIA Core Services (Fortson et al., 2017; Mastri, Rotz, and Hanno, 2018). These WIA Intensive Services can usefully be viewed as a form of "assistance" for job seekers that is more individualized and of longer duration than was provided by REA. In the first and second years after randomization, annual impacts on earnings are consistently more than $1,000. Magnitudes and whether the impacts are statistically different from zero are sensitive to details of the specification (e.g., which year is examined, and the source of earnings data; see Mastri, Rotz, and Hanno (2018), Table III.2).

Heinrich, Mueser, Troske, and Benus (2008) present careful (non-experimental) propensity score matching evidence on the impact of WIA Core and Intensive Services versus no WIA services. They find statistically significant impacts starting in Q1, of about $700 for adult men and about $500 for adult women.

To what extent these WIA Intensive Services estimates are applicable for REA and REA-like programs is unclear. That intervention is more intensive than REA. The population is not exclusively UI recipients. Probably most important, participation is voluntary. Rational choice theory would suggest that those likely to benefit most would participate. In contrast, high rates of non-attendance at REA meetings suggest that REA programs serve a very different population. There is some overlap in the populations, but much of REA's either do not attend or attend only because of the threat of loss of UI benefits.

3. The Evaluation

This chapter provides evaluation-specific background. Section 3.1 describes the evaluation's design. Section 3.2 compares how the states implemented the treatment conditions to the ideal as described in Section 1.2.1/Exhibit 1-2. Section 3.3 describes the data used by the evaluation. Finally, Section 3.4 briefly discusses methods, followed by a detailed explanation of how to read the key impact tables that will appear in Chapters 6 and 7.

3.1 Evaluation Goals and Design

Section 1.2 provided a high-level description of the REA Impact Study's goals and four-armed random assignment design. This section provides some additional detail on site selection (i.e., study states), treatment conditions, and sample sizes.

3.1.1 Site Selection

The evaluation approached several states, four of which agreed to participate in the random assignment study: Indiana, New York, Washington, and Wisconsin.

Abt selected these states purposively.[24] In consultation with DOL/CEO, we developed a candidate list of states, which we tried to induce to participate. The candidate list of states focused on (a) larger states, so the study would have sufficient sample to generate reliable state-specific estimates; and (b) better data systems, so the study could proceed using administrative data only. State participation in the evaluation was voluntary.

Since the states were purposively selected and are not a proper sample of the states, the results do not formally generalize to other states or to the nation as a whole.

3.1.2 Timing of Randomization

Exhibit 3-1 gives the timing of randomization for this study and data collection. Across all states, randomization started in March or April 2015 and continued for about a year.

Exhibit 3-1 Study Period by State

	Indiana	New York	Washington	Wisconsin
Week random assignment began	4/4/2015	4/24/2015	4/20/2015	3/28/2015
Week random assignment ended	3/27/2016	3/27/2016	4/17/2016	4/17/2016
End date for state data collection	10/7/2016	11/8/2016	11/12/2016	10/29/2016

3.1.3 Treatment Conditions and Sample Sizes

Across the four states, the study randomized nearly 300,000 UI claimants shortly after their initial UI claim. Not every state implemented every treatment condition in every office. This heterogeneity introduces some subtlety in interpreting the results.

[24] The study's *Implementation Report* (Minzner et al., 2017) discusses the selection process in detail.

For our analytic sample, Exhibit 3-2 gives the reported number of claimants in each treatment condition, by state. These reported numbers are smaller than the total number randomized because of a series of analytic choices.[25]

Exhibit 3-2 Sample Sizes by State and Treatment Condition (*N*=278,641)

Condition	Indiana		New York						Washington		Wisconsin	
			Two-Arm		Four-Arm		Total					
	50,007		100,444		60,702		161,146		43,318		24,170	
Control	15.4%	7,685	18.9%	18,995	24.7%	14,966	21.1%	33,961	22.1%	9,576	33.4%	8,073
Partial	35.7%	17,877			24.7%	14,965	9.3%	14,965	25.3%	10,959	33.3%	8,054
Single	48.9%	24,445			24.8%	15,027	9.3%	15,027	25.9%	11,237		
Multiple			81.1%	81,449	25.9%	15,744	60.3%	97,193	26.7%	11,546	33.3%	8,043

Indiana and Washington implemented randomization in all offices. In Indiana, randomization was to *Control* or *Partial* or *Existing* (in Indiana, *Existing*=*Single*). In Washington, randomization was to all four treatment conditions (*Existing*=*Multiple*).

New York. The situation in New York was more complicated. The state implemented and provided data on four-way randomization (*Control*, *Partial*, *Single*, and *Existing*=*Multiple*) in 10 of 64 offices. In addition, the state provided data from its ongoing (i.e., in the absence of the evaluation) two-way randomization (*Control* and *Existing*=*Multiple*) in its other 54 implementing offices. Exhibit 3-2 provides sample counts for New York as a whole and separately for the two-arm and four-arm offices.

Wisconsin started implementing three-way randomization (*Control*, *Partial*, and *Existing*=*Multiple*) in the 18 of 26 comprehensive AJCs that offered REA. Then, to increase sample size, nearly seven months into the 12 months of randomization, Wisconsin expanded REA and randomization to three more offices (for a total of 21 of 26), randomizing to the same three treatment conditions.

For any given contrast (i.e., comparison of outcomes across two treatment conditions), the subset of states varies:

- All states and all offices contribute to *Existing* vs. *Control* contrasts.
- All states but not all offices contribute to *Existing* vs. *Partial* and to *Partial* vs. *Control* contrasts. In particular, the very large samples from the balance of New York are for offices that did not implement *Partial*, so they do not contribute to contrasts involving *Partial*. As a result, the sample sizes for contrasts involving *Partial* are much smaller than sample sizes for *Existing* vs. *Control*.
- Finally, only 10 New York sites and Washington contribute to *Multiple* vs. *Single* contrasts. Indiana did not implement *Multiple*. Wisconsin and most of New York did not implement *Single*.

[25] We randomized 299,905 claimants, but had to drop 21,264 for various reasons discussed in Appendix B in the appendix volume.

3.2 Implementation of the Treatment Conditions

Section 1.2.1/Exhibit 1-2 provided an idealized view of the four treatment conditions. In practice, states differed in how they implemented the treatment conditions and how close their implementations were to the ideal. This section notes aspects of state implementation that appear to be potentially important for interpreting interstate variation in impact. The study's *Implementation Report* (Minzner et al., 2017) provides more detail.

The implementation of the *Partial* model was to include all of the enforcement, but none of the assistance. The *Existing* model (either *Single* or *Multiple*, depending on the state) was to include all of the assistance, both assistance provided during the REA meeting and referrals to reemployment services after the meeting. In practice, *Partial* included limited assistance in some states (i.e., labor market information, orientation to available services). In addition, requirements for completing services after the REA meeting also varied by state. As a result, the contrast between *Partial* and *Single* is not as straightforward as the original vision, and it varies by state:

- Indiana based *Partial* on its existing Jobs for Hoosiers program. As a result, REA participants assigned to *Partial* were to attend an AJC orientation and complete a one-on-one meeting focused on enforcement but not assistance. In contrast, *Single* participants were to receive reemployment guidance from REA staff and a referral and mandatory follow-up for additional reemployment activities. After the first REA meeting, claimants were to complete two biweekly logs, documenting their job search activities and workshop attendance. This compliance requirement would be expected to increase services received for *Single* far above that of *Partial*, and as a result increased the contrast between the *Partial* and *Single* treatment conditions.
- In New York, *Partial* REA participants were to receive either no one-on-one assistance; if a one-on-one meeting occurred it was only to include a UI eligibility review. In contrast, *Single* and *Multiple* (=*Existing*) participants were to receive an intensive one-on-one assistance session. Lowering the contrast, however, the state allowed most claimants to be given a *recommended* rather than a *required* reemployment service referral. Without the requirement for completion of services or a subsequent meeting to prompt action, most *Single* participants chose not to complete additional reemployment services after the first REA meeting.
- Washington based *Partial* on its preexisting program, the Unemployment Insurance Reemployment Orientation. As a result, *Partial* REA participants received no one-on-one meeting, but they did receive an orientation to the AJC. Increasing the contrast, the orientation to the AJC was more expansive for *Single* and *Multiple* (=*Existing*) participants, emphasizing several additional program components. Also increasing the contrast was that the individual reemployment plan—received by *Single* and *Multiple*=*Existing* participants (but not *Control*)—was begun as part of the initial group orientation and was then further refined during the one-on-one ("deskside") meeting that follows. Finally, there was limited follow-up with *Single* participants to confirm that they completed their required reemployment service referral. Without this follow-up and/or enforcement, very few *Single* participants completed their referrals.
- In Wisconsin, consistent with the ideal, *Partial* REA participants were required to complete an online orientation to workforce services (also required for *Control*) and a 10- to 15-minute one-on-one meeting with an REA case manager to discuss their past work search activities (i.e., enforcement). In contrast, *Multiple* (=*Existing*; Wisconsin did not implement *Single*) participants were to participate in

an orientation to AJC services and key job search strategies lasting two or three hours. During that orientation each claimant would be pulled out for a short one-on-one meeting attended by both an REA case manager and a UI adjudicator. The presence of a UI representative was expected to increase compliance with work search requirements and identify any UI eligibility issues. In practice, those claimants deemed “work ready” were not given an individual reemployment plan or a referral for additional services.

With respect to *Multiple*, states differed in their policies as to which claimants would be scheduled for a subsequent REA meeting. We provide direct evidence on scheduling in Section 4.3; here we note the following:

- Indiana did not implement *Multiple*.
- New York’s *Existing* was *Multiple*, and a large fraction of claimants attending one REA meeting were scheduled for the next one. Thus, the *Existing* vs. *Partial* comparison that we use to estimate the impact of assistance includes state responses to non-attendance at second and third REA meetings.
- Washington’s *Existing* was *Multiple*. State policy was that everyone who both attended the first REA meeting and was still claiming UI should be brought in for subsequent REA meetings. As discussed in Section 5.1, a smaller fraction of claimants attended this first REA meeting than in other states. Apparently as a consequence, in practice, scheduling of and attendance at subsequent REA meetings was lower.
- Wisconsin’s *Existing* was *Multiple*, and the state did not implement *Single*. Staff had considerable discretion as to which claimants were to be brought in for subsequent REA meetings. In practice, few UI claimants received more than one REA meeting. Thus, in practice, Wisconsin’s *Existing* is close to *Single*.

3.3 Data Sources

This study uses administrative data received from the states and multi-state data from the NDNH.

3.3.1 State Data

The four participating states provided information from multiple data systems on claimants randomized into the study, including (a) randomization status; (b) one-time information from the initial UI claim (e.g., basic claimant demographics such as gender, race/ethnicity, age, citizenship, disability status, education, veteran status, occupation, and prior earnings), the profile score,[26] and the benefit award (e.g., weekly amount, maximum weeks); (c) weekly information on claiming UI and dollars of UI benefit paid; and (d) per-incident information on nonmonetary issues raised and adjudication of that claim.

States provided data on UI benefit payments and noncompliance actions (e.g., suspension of benefits) from the start of randomization through approximately 28 weeks (slightly more than six months) after the end of random assignment. Exact information provided varied by state. See Appendix B in the appendix volume for more information on data received and our data processing decisions.

[26] Profile score is a state’s official predicted probability of a claimant exhausting UI benefits, where higher profile scores imply more likely to exhaust. Profile models vary by state.

States did not provide historical (pre-claim) information on claimants' earnings or UI benefit receipt. In addition, because states provided data only through about 28 weeks after the end of random assignment, we do not have state data on UI benefits paid for the full benefit year for most claimants randomized. Instead, this study draws pre-claim information and longer-term follow-up from the NDNH, as discussed next.

Under appropriate data use and security protocols, states provided these data to Abt. On its secure analysis server, Abt merged the multiple datasets by Social Security number (SSN) and then created analysis files stripped of SSNs and other identifiers.

3.3.2 NDNH Data

The study drew long-term follow-up data from the NDNH, operated by the federal Office of Child Support Enforcement. Specifically, de-identified quarterly wage data—with pseudo SSNs and pseudo Federal Employer Identification Numbers (FEINs)—are made available from the NDNH to other federal agencies for approved research purposes only. The NDNH aggregates from quarterly state UI earnings data, augmented with information on federal employment. All NDNH data are administrative, and some types of employment and earnings are not captured through these administrative systems (including self-employment earnings, some types of farm labor, and jobs in the informal sector).

The NDNH data provided the REA Impact Study with three types of information: (a) quarterly data on earnings; (b) quarterly data on UI benefits paid; and (c) dates of new hires as submitted by employers. Subsequent analyses suggest that the new hires data are unreliable for capturing length of time to employment for the REA study sample. When quarterly wage records indicated likely new employment, we could match fewer than half of those records to a corresponding hiring date in the new hires data.[27]

Unlike for the state data, Abt did not have direct access to the NDNH data. Instead, Abt provided the NDNH with a "passthrough file" containing some of the information from the state data (in particular, randomization status and demographics). NDNH provided DOL its data elements and our passthrough file, each with SSN replaced by a common pseudo-SSN. On DOL's secure server, we matched the NDNH data to the passthrough file information using the NDNH-provided common pseudo-SSN. Analysis then occurred on that DOL secure server, accessed by DOL laptops. DOL provided disclosure review of output.

3.4 Methods and Interpretation of the Findings

This section provides guidance on how to read and understand the information conveyed in exhibits in Chapters 6 and 7 reporting random assignment impact estimates and associated discussions. Because this REA Impact Study uses a random assignment design, comparisons of outcomes across treatment conditions give the causal effect; that is, the difference in outcomes between treatments, holding all else equal.

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[27] We received multiple NDNH quarterly extracts for this REA Impact Study, linking them to create individual-level employment spells. We flagged quarters in which a worker received wages from a particular employer for the first time in the study period, and we looked for a hiring date that roughly corresponded to the quarter when those wages were first reported. We found a match for fewer than 40 percent of the wage records. We tried several approaches to broadening the definition of "new hire" using the quarterly wages. In every scenario, however, the match rate to a hiring date was less than 50 percent.

3.4.1 Methods

In practice, we report regression-adjusted impact estimates. Doing so increases the precision of the estimates and corrects for any chance differences between groups. See Appendix B in the appendix volume for more discussion of our statistical methods.

3.4.2 Interpreting the Findings

Most of the exhibits presenting impact analysis results in Chapters 6 and 7 have one of two formats. Exhibits 3-3 and 3-4 below show a sample of each. To save space, states are referred to by their abbreviations.

Primary Outcomes in the Aggregate. Below (Exhibit 3-3) we show a table from Chapter 6 (in Exhibit 6-1) as an example of how we present primary outcomes. The exhibit title provides the outcome being analyzed (in the example, UI Benefits). The unit of measure appears in parentheses (in the example, weeks; other possibilities might be quarters, %, or $). The title also gives the two treatment conditions being contrasted (in the example, *Existing* and *Control*) in italics. The estimated impact is for the more intensive treatment condition (in the example, *Existing*) relative to the less intensive treatment condition (in the example, *Control*).

Exhibit 3-3 (SAMPLE TABLE): Impact on Weeks of UI Benefits Paid (in weeks), *Existing* vs. *Control*

State	1 Control	2 Existing	3 Impact	4 SE	5 Heterogeneity
Pooled	14.882	13.620	−1.262***	0.046	<.01***
IN	14.624	12.945	−1.678***	0.125	<.01***
NY	15.422	13.970	−1.452***	0.061	<.01***
WA	14.133	13.338	−0.795***	0.114	<.01***
WI	13.617	13.100	−0.517***	0.133	<.01***

Source: Regression-adjusted impact estimates based on state administrative data, Model(s): UWSW28eczz, Run Date: 29MAR2019
Note: Statistical significance levels for impacts are based on two-sided tests and flagged with asterisks, as follows: *** < 1 percent; ** < 5 percent; * < 10 percent. The pooled entry in the "Heterogeneity" column is the *p*-value for a test that all of the state impacts are equal; the other entries are *p*-values for a test that this state's impact equals the minimum variance combination of the other states' impacts.

The first row gives the estimate pooled across all states for which we have estimates. The subsequent rows give the estimates for each of the four participating states, in alphabetic order, as shown in the stub.

The first and second columns give the regression-adjusted estimates for the mean outcomes in the two treatment groups.[28]

- **Column 1—**the less intensive treatment condition's regression-adjusted mean outcome (in the example, *Control*). The regression adjustments correct for random variation in baseline covariates between the two groups (and thus differ slightly from the raw means). We compute this regression-adjusted mean as the weighted mean prediction of the model, for this treatment condition, computed across all claimants randomized in this state (regardless of the treatment condition to which they were

[28] The regression-adjusted estimates are computed as weighted averages of the predicted estimates of the model—for the entire sample—switching only the treatment dummy.

randomized). As such, the treatment condition means adjust for random differences in observed baseline characteristics across study members.

- **Column 2—**the more intensive treatment condition's regression-adjusted mean outcome (in the example, *Existing*).
- **Column 3—**the estimated impact; generally the rounded difference between the values in column 2 and column 1. The difference in these two values (more intensive minus less intensive) is the estimated impact of being assigned to the more intensive treatment condition relative to being assigned to the less intensive treatment condition. The estimate is in the units in the title of the exhibit. Thus, for Exhibit 3-3, in the first row ("Pooled"), the value in column 3 implies that relative to *Control*, the *Existing* treatment condition lowered weeks of UI by 1.262 weeks. (A caveat: Due to rounding, the impact listed in this column might differ slightly from the calculated difference between the regression-adjusted means for the control and treatment groups in columns 1 and 2, respectively.)

  This third column also includes asterisks (*) to indicate statistical significance for a two-sided test of equal outcomes (i.e., no differential impact). (We discuss interpretation of the asterisks below.) Due to rounding, asterisks may differ from what would be computed using the values listed in the table to compare the impact/standard error ratio to conventional critical values.
- **Column 4**—the standard error (SE) of the estimated impact. This is a conventional measure of the precision of the estimate. The estimates reported in this document are asymptotically normal. Confidence intervals, computed as the reported impact in column 3 and approximately twice (1.96) this standard error in either direction (above or below the reported impact), contain the true population parameter value in 95 percent of random samples. For example, for the pooled results in sample Exhibit 3-3 above, a 95 percent confidence interval would span (–1.352, –1.172), where −1.352 equals −1.262 minus 1.96 multiplied by 0.046, and −1.172 equals −1.262 plus 1.96 multiplied by 0.046, because 0.046 is the standard error.
- **Column 5**—the results of a test for heterogeneity of impacts across the states. The final column gives the result of a test that the estimated impacts are constant across the states. Estimated impact will always vary across states. Some (often much) of that variation is statistical noise, rather than evidence of variation in true impacts. It is possible to test whether there is more variation than would be expected given the sampling variability in the estimates.

  For the pooled estimate (the first row in such a table), this is a test of the hypothesis that the impacts across all of the states are jointly equal to one another. For the other rows, this is a test of the hypothesis that the impact for this state equals the impact pooled across the other states. These tests provide some insight as to which states deviate from the estimates for the other states. The reported number is the probability that the hypothesis of equal impacts is true. Smaller numbers are stronger evidence against equal impacts. Again, this column also includes asterisks to indicate statistical significance.

Statistical significance is indicated by asterisks as follows:

- ***p* < .01** denoted by three asterisks (***). An impact this large would arise by chance in less than one in 100 hypothetical repetitions of the experiment. In the text, we characterize such results as providing "strong evidence" that the differential impact is not zero.

- **.01 < *p* < .05** denoted by two asterisks (**). An impact this large would arise by chance in only one to five in 100 hypothetical repetitions of the experiment. In the text, we characterize such results as providing "evidence" (with no further modifier) that the differential impact is not zero.
- **.05 < *p* < .10** denoted by one asterisk (*). An impact this large would arise by chance in only five to 10 in 100 hypothetical repetitions of the experiment. In the text, we characterize such results as providing "some evidence" or "weak evidence" that the differential impact is not zero.
- **.10 < *p*** denoted by no asterisks would arise by chance in more than 10 in 100 hypothetical repetitions of the experiment. In the text, we characterize such results as providing "no evidence" of a differential impact.

Analyses of Binary Subgroups. The second exhibit format (Exhibit 3-4) displays results for analyses of binary subgroups (from an excerpt of Exhibit 6-4 in Chapter 6). In most cases, such exhibits will report results for multiple subgroup dimensions in separate panels in the same table (in the example, Some College/No College and Benefit Above/Below Median). The exhibit title provides the outcome measure being analyzed (in the example, UI Benefits Paid), with the unit of that outcome in parentheses (in the example, weeks), and then the two treatment conditions being contrasted (in the example, *Existing* and *Control*) in italics.

Exhibit 3-4 (SAMPLE TABLE): Differential Impacts of Claimant Characteristics on UI Benefits Paid (in weeks), *Existing* vs. *Control*

State	Impact	SE	Impact	SE	Impact	SE
College						
	Some		None		Differential	
Pooled	−0.449***	0.126	−0.898***	0.120	0.434**	0.174
WA	−0.599***	0.170	−0.974***	0.154	0.374	0.229
WI	−0.267	0.187	−0.782***	0.190	0.516*	0.267
Weekly Benefit						
	Benefit Above Median		Benefit Below Median		Differential	
Pooled	−0.929***	0.067	−1.555***	0.064	0.619***	0.093
IN	−1.104***	0.170	−2.283***	0.185	1.179***	0.252
NY	−1.067***	0.090	−1.768***	0.083	0.695***	0.123
WA	−0.677***	0.162	−0.920***	0.161	0.243	0.229
WI	−0.443**	0.190	−0.589***	0.187	0.146	0.267

Source: Regression-adjusted impact estimates based on state administrative data, Model(s): UWSW28ec, Run Date: 29MAR2019
Note: Statistical significance levels for impacts are based on two-sided tests and flagged with asterisks, as follows:
*** < 1 percent; ** < 5 percent; * < 10 percent.

The first row in each panel is for the pooled estimates. The other rows give the estimates for each of the participating states, in alphabetic order, as shown in the stub. In this exhibit format, there are three pairs of columns. The first pair is for the first value of the binary subgroup (in the example in the first panel, Some College); the second pair of columns is for the other value of the binary subgroup (No College/None); and the third column is for the differential impact (the second subgroup relative to the first subgroup; in the example, No College relative to Some College). Within each pair of columns, the first number gives the impact and asterisks (for the hypothesis of no impact in the subgroups or no differential impact across the subgroups), and the second number gives the standard error (SE).

3.4.3 Subgroups Considered in the Analyses

For key outcomes in Chapter 6 (receipt of UI benefits) and Chapter 7 (employment and earnings), we estimated models that allow impacts to vary with claimant characteristics and local labor market conditions. Appendix B, Exhibit B-4 gives exact definitions of the subgroups.

In most cases, those characteristics are binary (e.g., some college/no college). For continuous variables (e.g., weekly UI benefit for individual states), we divide the population in half (i.e., at the median) along that dimension and then compare impacts between the two subgroups (e.g., above and below the median in the state).

This REA Impact Study considers the following for **claimant characteristics**: gender (male/female); age (above/below the median); race (Black/not Black); ethnicity (Hispanic/not Hispanic); education (some college/no college); weekly benefit amount (above/below the median weekly benefit of all study participants in the claimant's state); and profile score (above/below the median profile score of all study participants in the claimant's state).

The analysis considers the following for **claimant labor market experience,** with each subgroup converted to a binary variable, defined as above/below the median of all observations, in the state: UI in the previous year, UI in the two years previous, employment in the previous year, employment two years previous, earnings in the previous year, and earnings for up to two years previous.[29] (The *previous year* is the four calendar quarters before the benefit year; *two years previous* is the four calendar quarters before that.)

Finally, the analysis considers the following for **local labor market characteristics,** again with each subgroup converted to a binary variable, defined as above/below the median of all observations, in the state: county unemployment rate in the quarter before initial UI claim, county unemployment growth rate in previous year, county employment growth rate in previous year, county unemployment rate in previous year, county employment rate in the previous year, state unemployment rate in the month of initial UI claim, state initial UI claims in the month of initial UI claim, and state-covered employment in the month of initial UI claim.

Subgroup analyses with respect to economic conditions should be interpreted with care. Outcomes reported in this *Final Report* occurred during a period of the lowest unemployment rate in decades. Furthermore, within a state, over the year of randomization there is not much variation in local economic conditions. Thus, these analyses should be considered weak tests of how the impact of the REA program (or REA-like programs) would vary with broader variation in economic conditions.

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[29] Some pre-randomization quarters of NDNH data are missing for a variety of reasons. First, unless held for research purposes, NDNH purges data after about two years. Early in the evaluation, we did not submit match requests quickly enough to capture the oldest quarters. Second, we timed NDNH data relative to the start of the benefit year. In some cases (e.g., when someone claimed, became employed, and then became unemployed again), their randomization took place well after the start of a benefit year. When we submitted the match requests for these claimants, the oldest quarters were no longer available. Third, errors in how states submitted earnings data imply that earnings for some quarters were never reported to NDNH. Through special efforts, states recovered some of those missing data. Working with NDNH, we appended those records. Some of the missing quarters were unrecoverable. In our analysis, we coded each of these quarters with a missing data flag, which is included as a regressor.

3.4.4 Statistical Significance Cutoff for Findings Reported in Text

Unless we explicitly note otherwise, the text only discusses results that are statistically significant at least at 5 percent ($p < .05$).

Similarly, unless we explicitly note otherwise, the text only discusses subgroup results where a statistical test clearly indicates different impacts between the two groups. Detecting such differential subgroup impacts usually requires large samples and precise estimates of the pooled impact. Though we consider a large number of subgroups for a large number of outcomes, few show clear evidence of differential impact.

Consistent with the general pattern of few differential subgroup impacts, the discussion of subgroup results for an outcome usually begins by noting that for most impact-subgroup combinations, there is no evidence of differential impact. The discussion then notes patterns of differential impact. We do not discuss every case of differential impact.

3.4.5 Multiple Comparisons Correction

This document reports a very large number of impact estimates with a very large number of corresponding tests for no impact and for heterogeneity of impact (thousands of tests in all). Beyond pre-specifying *weeks of UI benefits received in the data pooled across states (through 28 weeks after randomization)* as the confirmatory outcome, we make no correction for multiple comparisons. It is a near certainty that some of the estimates reported as statistically significant are no different from zero. In particular, estimates with few asterisks and not following a broader pattern should be interpreted with care.

4. REA Meeting Attendance

This chapter primarily describes attendance at REA meetings and how attendance varies across treatment conditions. The discussion at the end of the next chapter and the balance of this document will argue that state responses to non-attendance (discussed in Chapter 5) are crucial for understanding the impact of the REA program on UI weeks and other outcomes. The study's *Implementation Report* (Minzner et al., 2017) provides a detailed discussion of state policies with respect to REA meeting scheduling, content, and attendance.[30]

Chapter 4 Key Findings

With respect to the first REA meeting:

- Three of the states assign a meeting time; Wisconsin requires those selected to self-schedule within five weeks of the notice.
- Fewer than three in five UI claimants scheduled for a first REA meeting attend as scheduled.
- About a third of claimants who do not attend initially do attend later.
- Almost all claimants who attend after not initially attending do so within a few weeks.
- There is some evidence of forward-looking behavior: the longer claimants can expect REA meetings to last, the less likely they are to attend.

For states that implemented *Multiple*, with respect to subsequent REA meetings:

- Rates of assigning claimants to subsequent REA meetings vary widely: highest in New York, intermediate in Washington, and lowest in Wisconsin. (Indiana did not implement *Multiple*.)
- Timing of subsequent REA meetings also varies; sooner after the initial meeting in Washington and Wisconsin (about two weeks), later in New York (about five weeks).

Data. This chapter uses state administrative data on REA meetings: whether and when initially scheduled and when actually attended (if ever), both for the first REA meeting and for later meetings. Our data are limited to those claimants randomized into the study, so everyone in the sample was—at least initially—considered REA-eligible.

Methods. Most of the analyses reported in this chapter are purely descriptive. Causal analyses in Section 4.4 are simple comparisons by treatment arm to which the claimant was randomly assigned (that is, they are not regression adjusted).

Balance of This Chapter. Section 4.1 considers the *scheduling* of the first REA meeting. Section 4.2 discusses *attendance* at that first REA meeting. Section 4.3 considers scheduling and attendance at *subsequent* REA meetings. Section 4.4 addresses how attendance varies, within a state, across treatment conditions. Section 4.5 discusses the results. Examination of state responses to non-attendance at the first REA meeting and identification of ongoing eligibility issues at REA meetings is deferred until Chapter 5. Discussion of how state policies—for scheduling and response to noncompliance—might be related to meeting attendance rates is deferred until the end of Chapter 5.

[30] That report also includes exhibits similar to some reported here, but for the sample available at that earlier date. The exhibits in this *Final Report* supersede those previous exhibits.

4.1 Scheduling the First REA Meeting

Study states had some flexibility in the timing of random assignment and in scheduling the first REA meeting, though federal guidance indicated that they must contact REA participants no later than the fifth week of the claim (UIPL No. 10-14, p. 6). In practice, states vary as to when randomization occurred relative to the start of the benefit year, and (subsequently) when the first REA meeting was scheduled relative to randomization.[31] Given that many analyses in this document are *relative to randomization*, this is crucial contextual information.

Exhibit 4-1 describes the distribution from the start of the benefit year to randomization—for *Existing*. Specifically, the exhibit gives the 5th, 25th, 50th (the median), 75th, and 95th percentiles. In Washington, the median claimant is randomized two weeks after the start of the benefit year, in New York and Wisconsin three weeks, and in Indiana five weeks.

Exhibit 4-1 Distribution of Weeks from Start of Benefit Year to Random Assignment (for *Existing*)

Percentile	Weeks to Randomization			
	IN	NY	WA	WI
5th	5	1	2	2
25th	5	1	2	2
50th (median)	5	3	2	3
75th	9	3	2	5
95th	28	6	2	17

There is considerable variation within states. Although most claimants are randomized within a few weeks of the beginning of their benefit year, some are randomized much later. For Indiana and Wisconsin, there is a long right tail; that is, the reported values for the 75th percentile imply that for a quarter of the sample, randomization occurs later; for some, much later. This long right tail appears to have several causes. Some claimants perhaps receive benefits for a few weeks and relatively quickly find a job. When they lose that job and return to UI under the earlier claim (i.e., still using the earlier allocation of weeks of benefits), they are randomized. Others may have had a change in status from REA-ineligible to REA-eligible (either a true change in status or new information suggesting that the original determination had been incorrect).[32]

Exhibit 4-2 below describes the distribution from randomization to the initially scheduled REA meeting—for *Existing*.[33] New York and Washington schedule almost everyone two weeks after

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[31] See the *Implementation Report* (Minzner et al., 2017, p. 39) for a summary graphic on policy by state as to the timing of randomization and first REA meeting relative to the start of the benefit year (which typically coincides with the initial claim). New York and Washington have a more condensed process (REA within five weeks) than do Indiana and Wisconsin (REA within seven weeks).

[32] This long right tail has implications for the details of the analysis that follows. We deliberately date pre-claim information relative to the start of the benefit year, not relative to the date of randomization. We do this because otherwise, for long intervals between the start of the benefit year and randomization, the quarter prior to randomization would sometimes be after the start of the benefit year. We would not want a regressor dated after the start of the benefit year.

[33] On *Existing*, see Section 1.2.1. In brief, *Existing* is that state's REA program in the absence of the evaluation. In New York,

randomization; Indiana schedules almost everyone three weeks after randomization. Wisconsin differs because of its self-scheduling system and is discussed separately below. In practice the inter-quartile range is two to four weeks; that is, more than half self-schedule in that period. The extremes are one week (at the 5th percentile) and four weeks (at the 95th percentile).

Exhibit 4-2 Distribution of Weeks from Randomization to Scheduled Meeting (for *Existing*)

Percentile	Weeks to Scheduled Meeting			
	IN	NY	WA	WI
Mean	3.1	2.1	2.2	3.1
5th	2	2	2	1
25th	3	2	2	2
50th (median)	3	2	2	3
75th	3	2	2	4
95th	4	3	3	6

With respect to the interval from randomization to the scheduled date of the first REA meeting, mean duration is 3.1 weeks for Indiana, 2.1 weeks for New York, 2.2 weeks for Washington, and 3.1 weeks for Wisconsin. Median duration is similar: three weeks for Indiana and Wisconsin, and two weeks for New York and Washington.

The Wisconsin column in Exhibit 4-2 is not comparable to the other columns because Wisconsin has self-scheduling. Those selected in Wisconsin are expected to self-schedule within five weeks of receiving the appointment letter. Here we tabulate the date on which the meeting was scheduled by the claimant; claimants who never schedule are excluded from the computation. Even using this definition, more than a quarter of meetings occur at four weeks or later. This is considerably later than in the other three states. On the other side, many claimants in Wisconsin self-schedule in the first or second weeks, so the mean and median intervals are similar to those for Indiana and only about a week later than for New York and Washington.

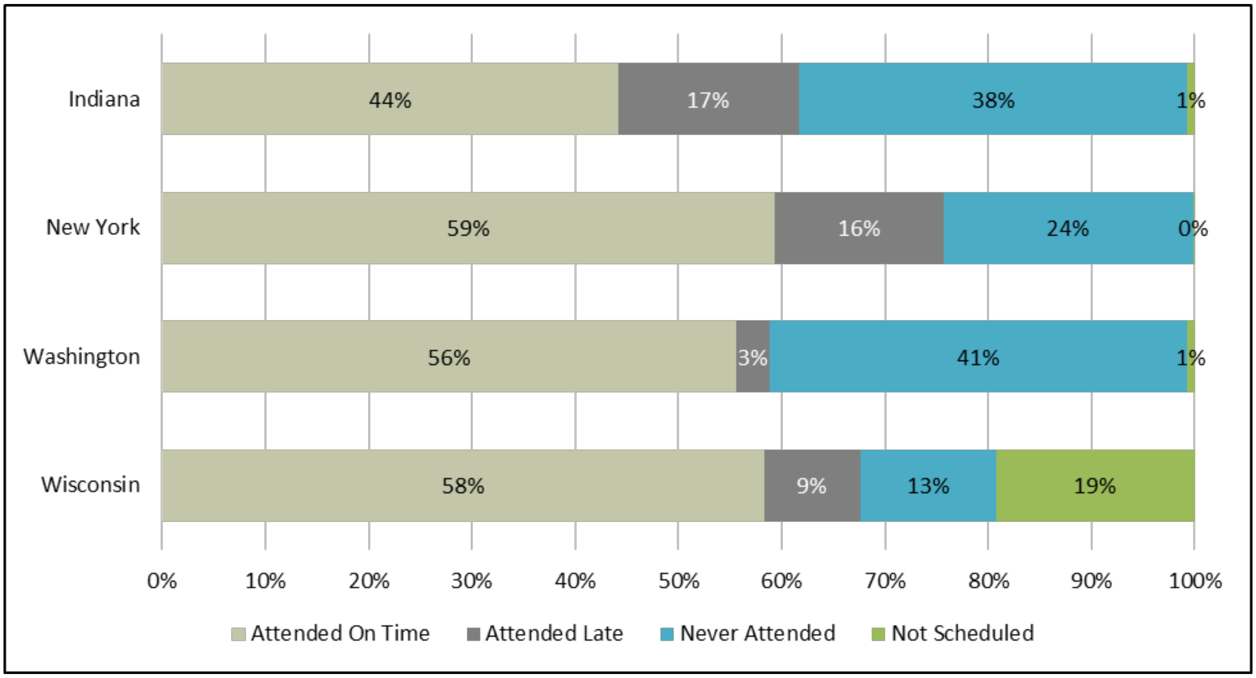
4.2 Attendance at the First REA Meeting

For *Existing*, Exhibit 4-3 below plots attendance at the first REA meeting using the categories “Attended On Time,” “Attended Late,” “Never Attended,” and “Not Scheduled.” **Fewer than three in five claimants attend on time, a sixth of claimants attend late, and the rest never attend.** This implies that only about a third of those who do not attend on time ever attend.[34]

Washington, and Wisconsin, *Existing=Multiple*; in Indiana, *Existing=Single*.

[34] The simulation in Chapter 2 (Exhibit 2-2) assumed a fifth of claimants attended late and a fifth never attended. Relative to those assumptions, Exhibit 4-2 reports more claimants never attended and fewer claimants attended late. However, the simulation assumes that some of those who never attended either were exempted or stopped claiming UI before or shortly after the scheduled REA meeting. Relative to that simulation, this distribution shifts more claimants into the larger impact group. It would therefore imply a larger impact of uniform and immediate suspension of benefits until compliance.

Exhibit 4-3 Attendance at First REA Meeting (for *Existing*)

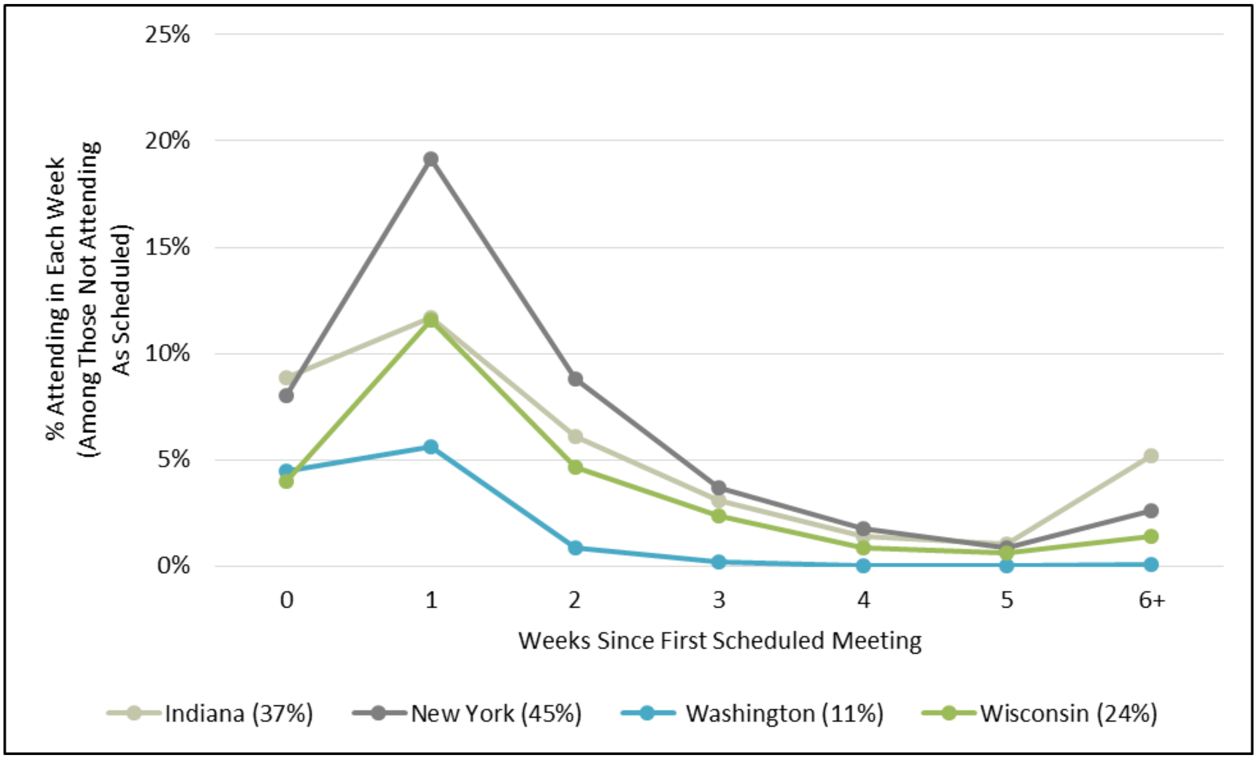


Here we note two deviations from this general pattern. First, **Attended Late is rare in Washington**, only about 3 percent (compare to 9 to 17 percent in the other states). Ever Attended (Attended On Time plus Attended Late) is also lower in Washington than in the other states, 59 percent (compare to 62 to 76 percent in the other states). As discussed in the *Implementation Report* (Minzner et al., 2017), with few exceptions, Washington did not allow for rescheduling of missed REA meetings. Claimants could still come into the office and request services at any time, but the REA meeting was still recorded as missed.

Second, a status of **Not Scheduled is comparatively common in Wisconsin**, 19 percent (compare to well less than 2 percent in the other states). This result is plausibly related to Wisconsin's self-scheduling approach and to the additional, pre-REA-meeting requirement to complete an online orientation before scheduling the REA meeting. In other states, the programs assign a first appointment date and time, usually within the first couple of weeks of the claim (see Exhibits 4-1 and 4-2). In Wisconsin, the selected claimant has two weeks from the mailing of the REA appointment letter to complete an online orientation and assessment, and then three additional weeks to self-schedule and attend the REA meeting. Claimants cannot schedule their meeting if they have not completed the online orientation. Thus, in Wisconsin, Not Scheduled should probably be viewed as a form of Never Attended. Claimants could also reschedule their own appointments as needed. Consistent with this interpretation, Wisconsin also has a lower Attended Late rate, 9 percent (compare to 16 and 17 percent in the other states; we exclude Washington from this comparison because of its weak response to non-attendance). The Ever Attended rate in Wisconsin (68 percent) is between those for Indiana (62 percent) and New York (76 percent), again excluding Washington.

Among those claimants who do not attend on time, Exhibit 4-4 below plots the percentage attending later in that same week and in subsequent weeks. The percentages in parentheses after the state names in the exhibit's legend are the share of claimants ever attending among those claimants who do not attend as initially scheduled.[35] **A sizable fraction attend later in the scheduled week and in the following week. Thereafter, rates of attendance drop sharply**, to less than 2 percent by four weeks out.

Exhibit 4-4 Weeks to Attendance Among Those Who Do Not Attend On Time (for *Existing*)



Note: Percentage in parentheses after the state name is the share of claimants not attending on time who ever attend.

The patterns are not uniform across states. Consistent with Washington's policy on rescheduling, late attendance rates among those claimants who do not attend on time are uniformly low. Some attend later in the week (4 percent), some attend in the following week (6 percent), but thereafter never more than 1 percent. In contrast, the other three states have sharp increases in the following week (12 to 19 percent of those who do not attend on time) and moderate levels in the following weeks (5 to 8 percent).

4.3 Scheduling of and Attendance at Subsequent REA Meetings

This section considers scheduling of and attendance at subsequent (i.e., second and third; not first) REA meetings. The analysis is limited to the three states that implemented *Multiple*: New York (four-arm), Washington, and Wisconsin. For these states, *Multiple* was their *Existing* REA program model. (Indiana did not implement *Multiple*.)

In states implementing the *Multiple* REA treatment condition, not every UI claimant assigned to that treatment condition is required to participate in a subsequent meeting. Those who leave UI soon enough after the first (or second) REA meeting are never scheduled for a second (or third). Beyond that, there is

[35] Specifically, the exhibit displays values for 0 weeks (i.e., attending later in the originally scheduled week), 1 through 5 weeks, and 6 weeks or later (labelled "6+"). The calculations include those who never attend.

cross-state policy variation. In New York, the policy is to schedule all those claimants who attend their first REA meeting for a second meeting.[36] In Washington and Wisconsin, only claimants deemed not work-ready at the first REA meeting are scheduled for a follow-up.

Consistent with those stated policies, **the percentages of claimants scheduled for second and third REA meetings vary widely across the states** (see the left panel of Exhibit 4-5).[37] New York schedules 72 percent of those claimants randomly assigned to *Multiple* to a second REA meeting and 48 percent to a third REA meeting (again, among everyone randomly assigned to *Multiple*). At the other extreme, the corresponding figures for Wisconsin are 27 percent and 4 percent. Washington's rates were between New York's and Wisconsin's, 52 percent and 27 percent.

Exhibit 4-5 Scheduling and Attendance Rates for the Subsequent REA Meetings

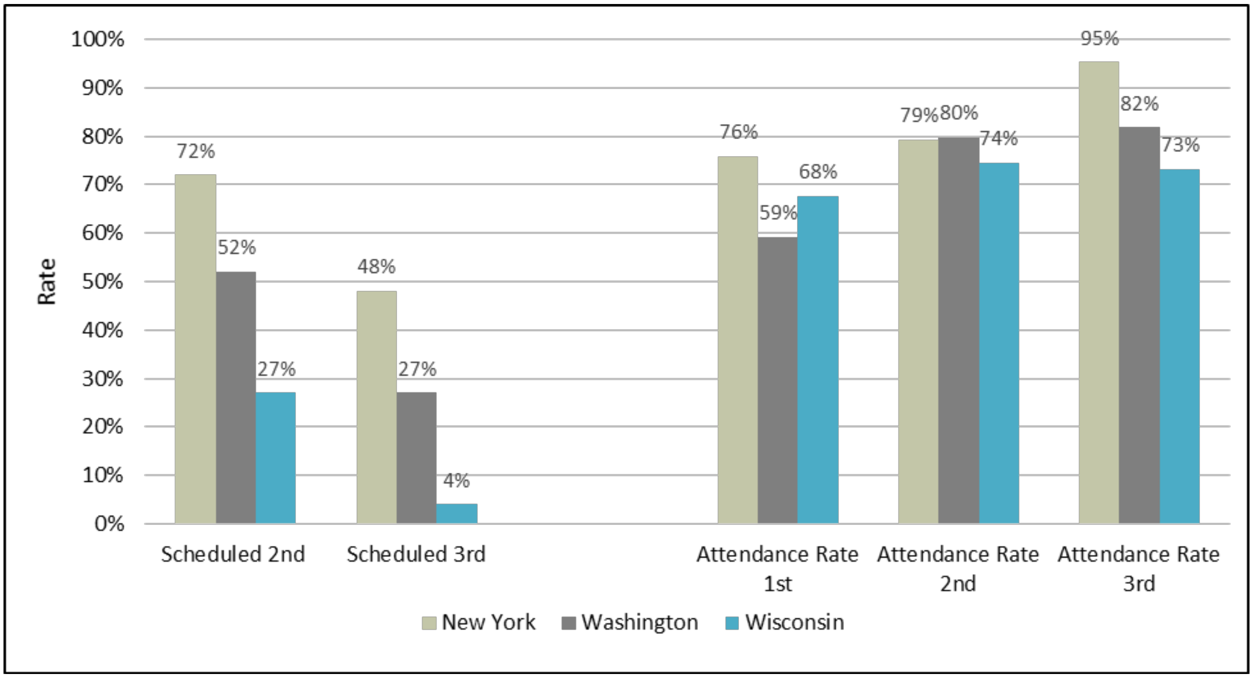


Exhibit 4-5 also plots attendance rates conditional on scheduling.[38] **Attendance rates rise moderately from first to subsequent REA meetings.** In Washington, the increase is particularly large: from only 59 percent at the first REA meeting to 80 percent and 82 percent for the second and third REA meetings, respectively.

[36] If the individual stops claiming, the appointment is cancelled. It is not shown as non-attendance.

[37] We do not plot scheduling rates for the first REA meeting. Those rates are nearly universal (once we view Wisconsin's low scheduling rates in the context of self-scheduling).

[38] For this exhibit, we treat everyone in Wisconsin as scheduled for the first REA meeting.

Exhibit 4-6 describes the distribution of weeks between REA meetings, for those who attend the second/third meeting. In addition, the first row gives the percentage of all claimants randomized to *Multiple* who actually attend second and third meetings. In the other rows, median times from first to second REA meetings are five weeks in New York, three weeks in Washington, and two weeks in Wisconsin. Comparable intervals between the second and third REA meetings are eight weeks in New York, three weeks in Washington, and two weeks in Wisconsin.

Exhibit 4-6 Weeks Between REA Meetings (for *Multiple*)

First to Second REA Meeting				Second to Third REA Meeting			
Percentile	NY	WA	WI	Percentile	NY	WA	WI
% Attending	53%	41%	21%	% Attending	36%	22%	4%
5th	3	3	1	5th	4	3	0
25th	4	3	2	25th	6	3	1
50th (median)	5	3	2	50th	8	3	2
75th	6	4	2	75th	8	4	3
95th	9	5	4	95th	10	5	5

Note: Tabulations are among those claimants who attend the second/third REA meeting.

4.4 Differential Attendance by Projected Length of the First REA Meeting

Exploring how attendance rates vary by treatment condition appears to be informative as to the source of the non-attendance. As the *Implementation Report* noted (see Minzner et al., 2017, Appendix G), in three out of four states (all but Indiana), the appointment letter specifically mentioned the expected length of the REA appointment. In each state, REA appointments for *Single* were longer than appointments for *Partial* (especially in Wisconsin).

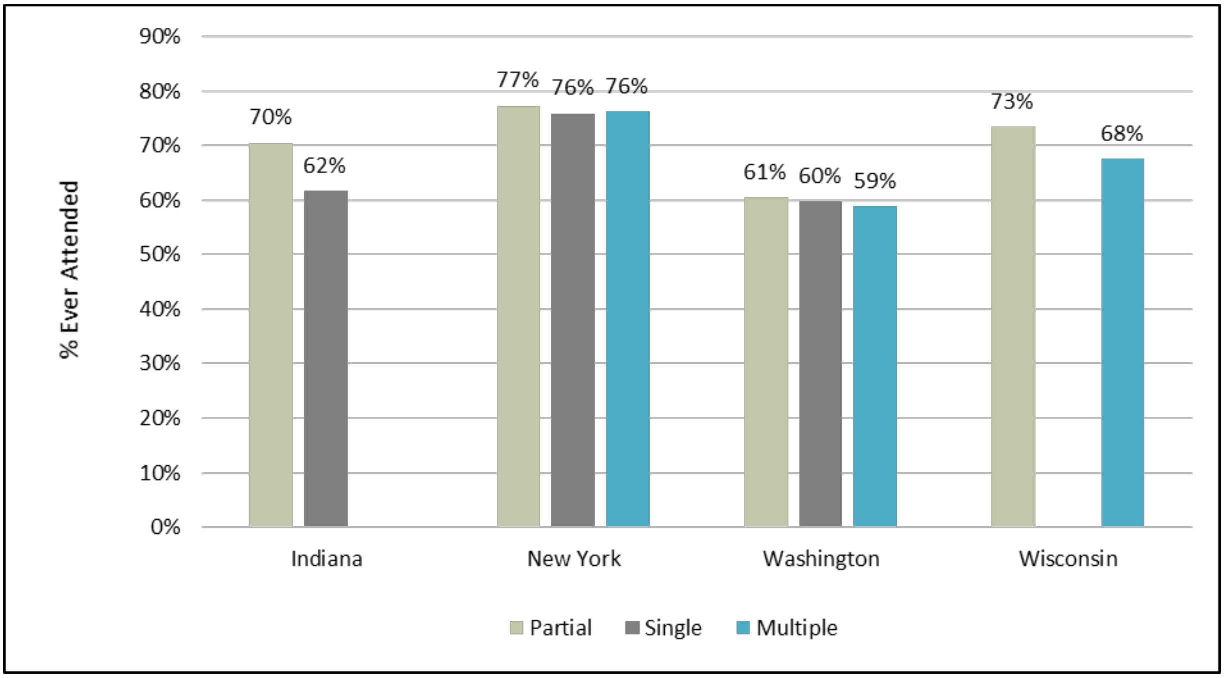
In New York, the appointment letter sent to *Single* and *Multiple* (= *Existing*) claimants indicated their visit might take up to two hours; to *Partial* claimants, up to one hour. In Washington, *Single* and *Multiple* (= *Existing*) appointment letters indicated a meeting of three hours, compared to two hours for *Partial*. In Wisconsin, the difference was more substantial; a *Partial* meeting was expected to take only 15 minutes, but a *Multiple* (=*Existing*) meeting was expected to take three hours, at least for the first REA meeting. Subsequent meetings were shorter in Wisconsin, about 30 minutes. In Indiana, the appointment letter for *Partial* gave no time estimate, but the appointment letter for *Single* gave the expected length for that specific office. In Indiana, the state standard was 45 minutes; for some offices it was longer, for others shorter.

Random assignment ensured that the claimants in each treatment group were otherwise identical. It follows that if claimants did not take the notification about the length of the meeting into consideration when deciding whether to attend (displaying **forward-looking behavior**), we would expect similar attendance rates among *Single* and *Partial* participant groups. Any observed difference in attendance must be caused by some aspect of the treatment condition.

To explore the possible role of forward-looking behavior, in particular whether some claimants understand the notice and choose not to attend, this section considers how attendance varies across treatment conditions. Specifically, Exhibit 4-7 considers attendance ever; Exhibit 4-8 considers attendance as initially scheduled.[39] The results are dramatic.[40]

Attendance rates are clearly related to information on the length of the REA meeting in the appointment letter. In Indiana and Wisconsin, attendance is much lower for the treatment condition that requires more intensive engagement. In Indiana, Ever Attended falls eight percentage points from *Partial* to *Single* (70 to 62 percent; Indiana did not implement *Multiple*). In Wisconsin, Ever Attended falls five percentage points from *Partial* to *Multiple* (73 to 68 percent; Wisconsin did not implement *Single*). In contrast, in New York and Washington, the difference in the length of the meetings is only one hour, and the rates are nearly identical.

Exhibit 4-7 Attendance Ever at First REA Meeting, by Treatment Condition

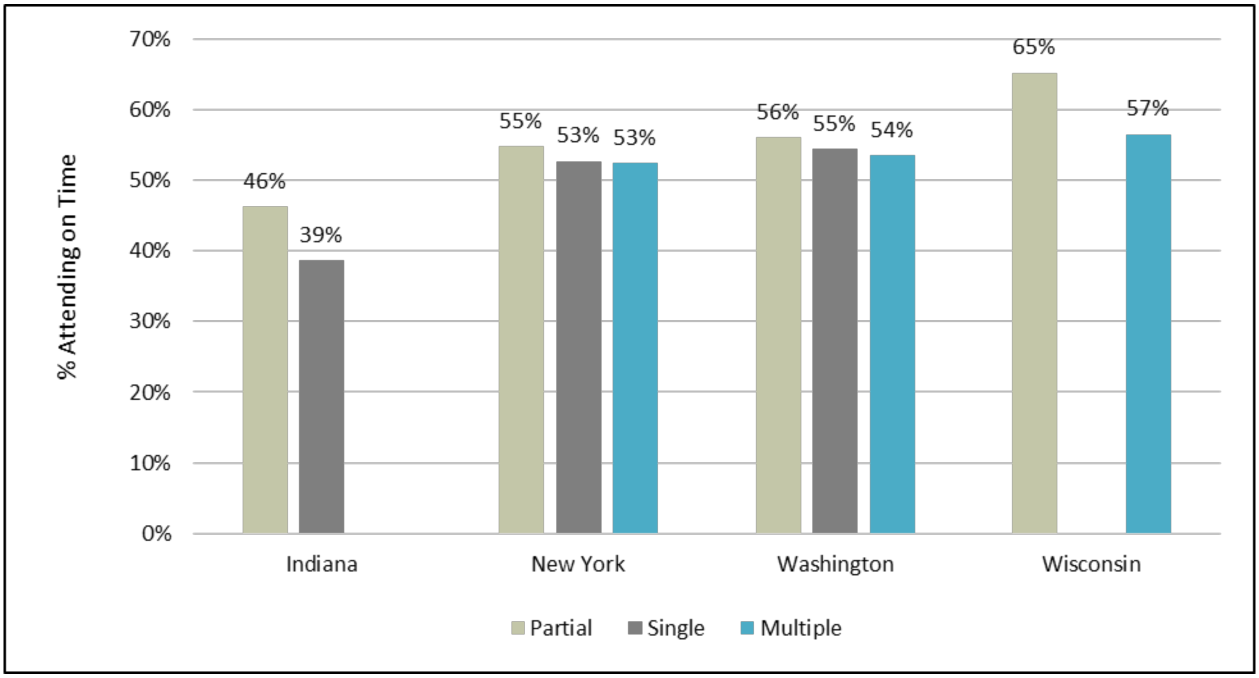


[39] Attendance rates for *Control* are not plotted; *Control* has no attendance requirement. These exhibits implicitly incorporate which states implemented which treatment conditions. New York two-arm implemented only *Control* and *Multiple* (but not *Partial* or *Single*); those observations are excluded from these tabulations. Thus for New York, results are for New York four-arm only. Indiana did not implement *Multiple*. Wisconsin did not implement *Single*.

[40] Throughout this section, estimates are so precise that we do not consider the statistical significance of differences. Any difference of more than 1.4 percentage points is statistically significant at 5 percent ($p < .05$).

Results for attendance as initially scheduled are qualitatively similar to those for Ever Attended. Large differences for Indiana and Wisconsin;[41] minimal differences for New York and Washington.

Exhibit 4-8 Attendance at First REA Meeting as Initially Scheduled, by Treatment Condition



These results provide evidence of forward-looking behavior, in particular that some claimants understand the notice and choose not to attend. Claimants have been randomly assigned to treatment conditions and the first REA meeting has not yet occurred. The only thing that could cause differential attendance rates is some forward-looking response to the signal in the appointment letter about the perceived burden of attending. Given random assignment to treatment conditions, this must be because (at least some) claimants understand that they are required to participate but chose not to. Though Darling et al. (2017) provide evidence consistent with the conjecture that many claimants do not understand the notice, these results imply that some claimants understand the notice well enough that they choose not to attend the REA meeting.

Furthermore, note that this analysis captures the differential rate of attendance only among those claimants who would participate for the lower burden (shorter meeting) but would not for the higher burden (longer meeting). Presumably, many claimants understand the appointment letter and would choose not to attend even the lower-burden meeting. They are not included in this differential. It seems plausible that some substantial part of non-attendance is deliberate.

[41] Given that "date scheduled" means something different with self-scheduling, the Wisconsin rates should be interpreted with care.

4.5 Discussion

A common finding in social programs is that about half of those required to participate in activity will not. Across the four studied states, fewer than three in five claimants selected for REA do not attend as scheduled.[42] About a third of the total will never attend.

At least some portion of claimants may become reemployed and/or stop claiming UI after the first REA meeting is scheduled. The analyses in Section 4.4 imply that at least some of the non-attendance is deliberate, forward-looking behavior by claimants; that is, understanding the notice and making a conscious decision not to attend.

Section 4.2 presented evidence of low rates of initial attendance, with some additional attendance materializing after the missed meeting. Some possible interpretations of those results include problems with notification (i.e., claimants did not receive the appointment letter), that claimants may not understand the appointment letter, and stress associated with job loss that causes them to not prioritize the requirements in the appointment letter. Darling et al. (2017) report promising results of using low-cost, behaviorally informed emails to increase attendance rates, sometime referred to as "nudges" (Thaler & Sunstein, 2009). Such nudges increased attendance rates by 13.8 percentage points. Those results strongly suggest that changing notifications for the REA meeting has the potential to increase attendance.

Nevertheless, the results in Section 4.4 suggest a role for other factors. The higher the perceived burden of attending, the lower the attendance rate. Given random assignment to treatment conditions, this must be because (at least some) claimants understand that they are required to participate but chose not to. Presumably, these claimants are balancing the time required against the potential loss of benefits. This is perhaps most evident in Wisconsin, where the difference in meeting length of *Partial* vs. *Multiple=Existing* is several hours. One might have thought that the response—suspension of UI benefits—would be large enough to induce compliance, or at least that a meeting that was an hour or two longer would not matter.

The next chapter considers responses by states to this non-attendance.

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[42] Darling and colleagues (2017, p. 1) make a similar point:

> *Given the strong incentives to attend REA sessions, we might expect attendance rates to be high. However, many individuals who are selected to participate in this mandatory program do not schedule or attend their REA sessions. For example, in the first three months of the Michigan Works! Southwest REA program, which began operating in January 2015, only 43 percent of claimants who were required to participate in the REA program scheduled their first session. Many other programs experience similar issues. Programs that have been shown to work, like the REA program, often still struggle to attract participants.*

5. State Responses to Non-Attendance at the REA Meeting

This chapter considers state responses to noncompliance with the procedural requirement to attend the REA meeting.

> **Chapter 5 Key Findings**
>
> - Detections of REA-related nonmonetary eligibility issues are common for REA participants.
> - Detections of REA-related nonmonetary eligibility issues vary across treatment conditions consistent with claimants making a conscious decision not to attend the REA meeting.
> - REA does *not* lead to substantially more detection of nonmonetary eligibility issues that are not directly related to REA requirements.
> - Claimed Not Paid status can be viewed as a weak lower bound on the impact of enforcement on UI weeks. In Indiana and New York, Claimed Not Paid implies an impact of enforcement alone of more than a fifth of a week.

The national UI program is authorized under the Social Security Act of 1935, as amended. Benefits are an entitlement based upon past employment; they can only be denied or terminated subject to due process requirements. Consistent with the framework and requirements of federal law, each state develops its own laws and policies governing UI eligibility, including both monetary and nonmonetary requirements. Monetary requirements include criteria related to claimants' work history (earnings). Nonmonetary requirements include additional eligibility criteria related to the nature of the job loss (involuntary and through no fault of the claimant), the ability and availability to seek and start work, and any additional income received (such as pensions).

States determine a claimant's eligibility for UI benefits in three phases. First, *initial eligibility* considers whether a claim has met all the monetary and nonmonetary criteria to qualify for the entitlement. If the initial UI claim is approved, *ongoing eligibility* considers nonmonetary issues related to a claimant continuing to receive benefits. Such ongoing eligibility issues include being "able and available" for work, conducting a sufficiently intensive job search, and not refusing a suitable job offer. Third, those selected for REA, have an additional *procedural requirement* to attend one or more REA meetings.[43]

The REA meeting was to include a review of eligibility, with an understanding that state staff would review and possibly detect issues affecting ongoing eligibility primarily (not initial eligibility) and generally within the category of nonmonetary requirements (not monetary requirements). This was by design; random assignment occurred following approval of an REA participant's initial UI claim, so states would have already assessed monetary and nonmonetary criteria related to initial eligibility. Furthermore, compliance with REA requirements (including attendance at the REA meeting) falls within the category of nonmonetary issues.

This chapter discusses the prevalence and type of eligibility issues that states recorded for REA participants, and the implications of these issues when considering the REA program's impact on the number of UI weeks paid.

[43] This procedural requirement to attend the REA meeting can also be viewed as an additional ongoing eligibility requirement. However, for the exposition that follows it is more useful to view that procedural requirement to attend the REA meeting as separate and different from other ongoing eligibility requirements. Thus, when we use the term "ongoing eligibility requirements," we do *not* mean to include the procedural requirement to attend the REA meeting.

Data. This chapter uses three types of state administrative data:

- **Nonmonetary issues detected and pending, detected and raised, and determinations (including denials)**[44]—States record eligibility issues raised and UI benefit denials in very different ways, and the data we received do not appear to cover all of the possibilities. Cross-state comparisons should be interpreted with care.
- **UI payment information**—Whether a claimant is paid for a week of claimed unemployment.
- **Claiming UI benefits**—To receive benefits, a claimant must make a continuing claim. Claimants whose benefits are suspended—in particular, for non-attendance at the REA meeting—may continue to claim UI benefits, but claimants will usually not be paid.

We refer to those weeks in which a claim is made but benefits are not paid as "Claimed Not Paid." We argue below that Claimed Not Paid is a weak lower bound on the effect of enforcement on weeks of UI; that is, the true effect of enforcement is likely substantially higher than what is suggested by the analysis of weeks of Claimed Not Paid.

Methods. Analyses reported in this chapter are in three modes:

- **Purely descriptive**—(Section 5.1).
- **Causal**—Analyses that build on the random assignment design—comparing outcomes across treatment conditions, within a state—to yield causal inferences with strong internal validity (Sections 5.2 and 5.3). For all these results, formal tests (not reported here) clearly imply that outcomes and impact vary across states.
- **Non-experimental cross-state methods** to make (weaker) causal inferences (Section 5.4)—The number of states is small (only four), whereas the number of differences in program design and implementation is much larger. Under these conditions, cross-state inferences can only be tentative.[45]

Balance of This Chapter. Section 5.1 provides an overview of state processes for documenting eligibility issues and noncompliance. The next two sections consider nonmonetary issues detected and denials: Section 5.2 considers nonmonetary issues detected and resulting denials for failure to comply with REA requirements, in particular to attend one or more REA meetings; Section 5.3 considers nonmonetary issues detected and resulting denials for other reasons. Then Section 5.4 considers impact on weeks of Claimed Not Paid. Finally, Section 5.5 provides an extended discussion of the implications of the findings.

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[44] An eligibility concern might be recorded as a "pending issue," indicating that an administrative process has begun in response to potentially disqualifying information, but that the process is not far enough along for the issue to be formally raised or adjudicated. Typically, pending issues are either resolved or removed, depending on the circumstances and the state.

[45] For a strong critique of such cross-site "narrative syntheses" see Greenberg, Meyer, Michalopoulos, & Wiseman (2003); also Greenberg, Michalopoulos, & Robins (2003) and Greenberg, Meyer, Michalopoulos, & Wiseman (1994). It is our sense that Greenberg's critique is correct.

5.1 Documentation of Eligibility Issues

As eligibility issues arise for UI claimants, states respond according to their own laws, policies, and procedures. We held clarifying calls with each of the study states to better understand the processes related to enforcement of missed REA meetings and the collection of related administrative data.

The REA program incorporates two types of enforcement, either of which can result in an eligibility issue being detected and possibly raised for adjudication. First, the REA meeting includes an eligibility review for enforcement of issues related to claimants' ongoing eligibility ("able and available" for work, sufficiently intensive job search, has not refused a suitable job offer). Second, noncompliance with the procedural requirement to attend the REA meeting should also result in a nonmonetary eligibility issue being formally raised for adjudication. REA was a mandatory program, and DOL's guidance was explicit:

> *Once the state notifies a claimant that s/he has been selected for a UI REA, participation in the UI REA is mandatory. If a claimant fails to report for any UI REA without notifying the state beforehand, the state must refer the issue of the claimant's failure to report to the appropriate UI staff to be adjudicated under state law.* (UIPL No. 10-14, pp. 5-6)

To better understand how states implemented this UIPL language, we asked REA leadership in each of the four study states to describe how they would expect REA line staff to handle a missed REA meeting under a variety of circumstances (e.g., the claimant called to reschedule, the claimant came in the same week or rescheduled for the following week, the claimant never came in, etc.) and to describe the data recorded in each scenario.

In brief, according to those state REA leaders, a missed REA meeting leads to the following:

- **Indiana**—A potential eligibility issue was recorded as a pending issue with an indefinite hold on benefits until the claimant either reschedules or attends (depending on the local office); claimants were notified of the suspension when they try to file their next weekly claim; once the meeting was rescheduled or attended, the previous week(s) of benefits held were adjudicated and a determination made depending on the reason for the missed meeting; after 21 consecutive days, the claimant cannot go back and file for the past weeks.[46] Benefits were also held for failure to complete the follow-up referrals and weekly job logs (for *Single*, but not for *Partial*).
- **New York**—A potential eligibility issue was recorded as a pending issue with a hold on benefits for up to four calendar weeks (in which the individual has to continue claiming, but is not paid) or until the claimant attended the meeting. The hold was lifted once the claimant attends the REA meeting, and the weeks of the missed meeting were adjudicated. If the claimant did not attend the meeting after four weeks of Claimed Not Paid, the hold was lifted regardless. The weeks claimed but not paid would not be adjudicated unless the claimant requested it; if adjudicated, the determination would depend on the reason for the missed meeting.
- **Washington**—An immediate nonmonetary eligibility issue was raised for adjudication, but benefits were not held pending the adjudication; meetings were not rescheduled. If a determination resulted in

46 Prior to the 21 days, a claimant can retroactively claim earlier weeks. That is, if the claimant tries to claim and learns of the suspension for nonattendance, the claimant may attend the meeting and retroactively claim for the previous two weeks.

a denial, *only* the week of the missed meeting was identified as an overpayment and attempts made to recoup the benefits paid; the maximum consequence was non-payment for the week of the missed REA meeting.

- **Wisconsin**—A potential eligibility issue was recorded as a pending issue with an indefinite hold on benefits until the claimant rescheduled the REA meeting in the self-scheduling system. The hold was automatically lifted once the meeting was rescheduled; attendance was not required to lift the hold. The hold on the week(s) of the missed meeting was not lifted or adjudicated unless the claimant requested it. If adjudicated, the determination depended on the claimed reason for the missed meeting. Benefits also were also held for failure to complete an online orientation. This applied to all treatment conditions, including *Control*. This requirement on *Control* was unlike other states. In other states, the only (universal) requirement for *Control* was to file the initial UI claim and then to file the weekly claim.

Study states shared administrative data that included potential eligibility issues detected (including pending issues prior to adjudication), with additional information about determinations. In many cases, the determination included the results of the adjudication (i.e., to allow or deny benefits). However, some issues were lifted or cleared prior to or after formal adjudication, depending on the state. In order to impose some consistency across the data received from the four states, we limited our analysis to issues that were either adjudicated, cleared, or lifted during the first 28 weeks of the initial claim. We received information on pending issues that were not resolved by the end of the study period from some states but not all, so we exclude these from the analysis.

Two countervailing considerations are important in understanding how these policies apply in practice and their likely impact on outcomes. First, UI is an entitlement; weeks of non-payment are not lost, just delayed.[47] Second and on the other side, if benefits are held indefinitely until completion of the REA meeting, then it does not matter that the weeks are not "lost"; unless and until the claimant attends the REA meeting, no additional weeks of benefits are paid.[48]

These stated policies imply that there is considerable interstate variation with respect to how to respond to non-attendance at the REA meeting. Our qualitative field work (reported in Minzner et al., 2017) and the

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[47] In the four states included in the study, the maximum duration was 26 weeks. However, Indiana, Washington, and Wisconsin (but not New York) are "variable duration" states, such that an initial claim with low qualifying wages can be assigned a lower maximum duration. In fact, the overwhelming share of our sample has maximum duration well above 20 weeks. For example, among those assigned to *Existing*, the first quartile (only 25 percent of the cases have shorter maximum duration) is 26 weeks in Indiana, 22 weeks in Washington, and 24 weeks in Indiana (and of course 26 weeks in New York, where everyone is assigned that maximum duration).

Consider a claimant with a maximum duration of 26 weeks who remained unemployed and claimed continuously, but (for example) was denied benefits in week 4 for non-attendance at the REA meeting. Benefits would therefore be paid for weeks 1-3 and 5-27—the full 26 weeks—if the claimant remained unemployed and continued claiming benefits.

[48] Continuing the example from the previous footnote, holding benefits would result in paying for weeks 1-3, but for no subsequent week.

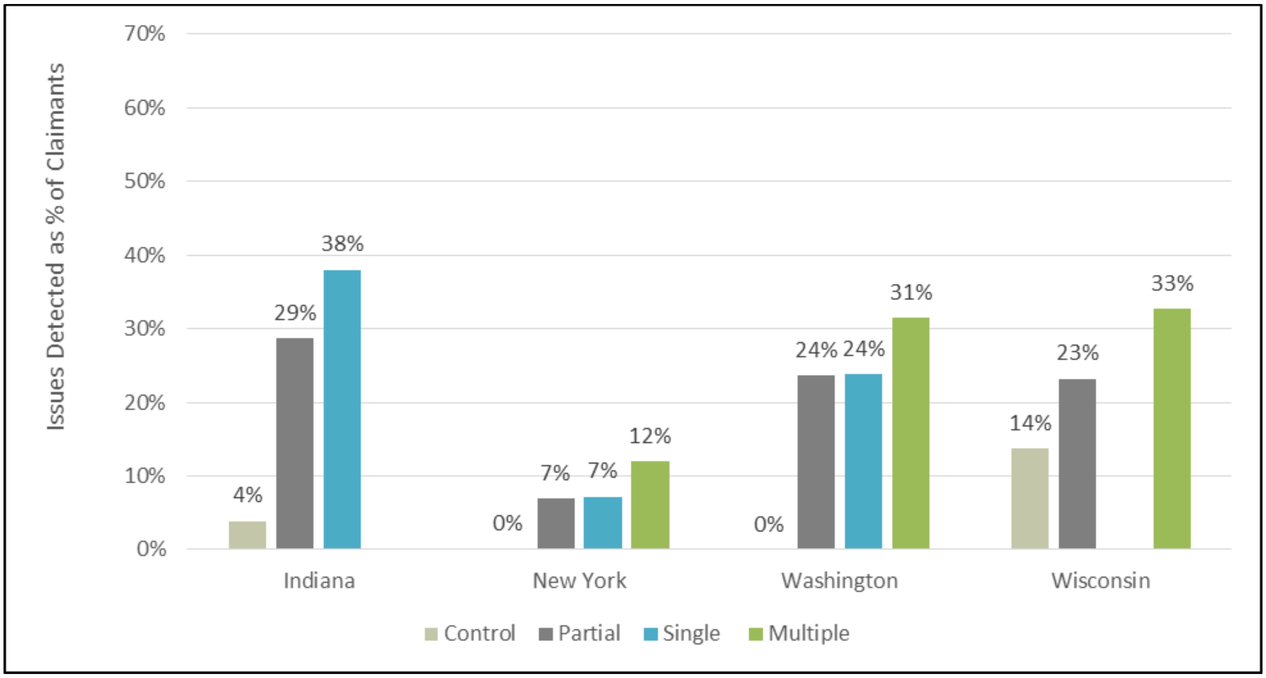
In practice, it appears that the effect of New York's hold for up to four weeks was equivalent to an indefinite hold. Claimants do not appear to have realized that claiming (without being paid) for four weeks will result in resumption of payments in the fifth week—and the potential for collecting the full 26 weeks. Instead, once benefits were suspended, individuals claim for a few weeks—without being paid. Few claim for the fifth week such that benefits resume.

data analysis reported in this section suggest that this interstate variation in stated policy carries over to the practices of REA case managers. Furthermore, this interstate variation in the policies as applied by REA case managers appears to be a plausible explanation for (at least some of) the interstate variation in the impact of REA on weeks of UI (reported in Chapter 6).

5.2 Nonmonetary Issues Detected for Failure to Comply With REA Requirements

With this background on state policy as described by state REA leadership, we present the results of our analyses of state administrative data. For each state,[49] Exhibit 5-1 plots the rates[50] of detection of potential nonmonetary issues related to compliance with REA requirements, as recorded in the state administrative data. The results are stratified by treatment condition. The study's random assignment design means that any differences in outcomes—across treatment conditions, within a state—can be attributed to the treatment condition.[51]

Exhibit 5-1 REA-Related Nonmonetary Issues Detected, by Treatment Condition



[49] Here and throughout this chapter, New York estimates are for four-arm only. Because New York two-arm has only two treatment conditions, it is not possible to do many of the cross-treatment-arm comparisons reported in this chapter.

[50] Here and in all of the analyses of pending and nonmonetary issues and denials in this chapter, the outcome is not the fraction of claimants with nonmonetary issues detected (or denied) of a certain type. Instead, the outcome is nonmonetary issues detected (of a certain type) per claimant. Thus, someone with two REA nonmonetary issues detected would count twice (e.g., someone who does not attend the originally scheduled REA meeting, then attends later, and then does not attend the second REA meeting as originally scheduled would have an outcome of two REA nonmonetary issues detected).

[51] The estimates have sampling variability, but our samples are large. Differences of more than a percentage point are almost certainly not simply statistical noise.

In three of the four states, **REA detections of nonmonetary issues (including pending issues that are ultimately cleared or lifted) are present in about a quarter of all UI claims**. These high rates of REA nonmonetary issues detected are consistent with the high rate of non-attendance at the REA meeting as initially scheduled (see Exhibit 4-3). Note, however, that the nonmonetary issue detection rates are only about three-quarters of the initial non-attendance rates.[52]

Qualitative field work suggests several possible explanations. For many claimants who do not attend as scheduled, a pending issue is recorded but unresolved within the first 28 weeks of the benefit year (and therefore not included in this analysis), sometimes because claimants stopped claiming before due process could be completed. Some of the gap between non-attendance rate and nonmonetary issue detection rate is attendance later in the week of the originally scheduled meeting (i.e., before a hold is imposed); some is good cause (e.g., REA staff discover that the appointment letter went to the wrong address);[53] some is failure of the state to follow through on the requirement to raise a nonmonetary issue for adjudication; and some is a change in circumstances such that the REA meeting is no longer required (e.g., a claimant receives a return-to-work date). Our methods are not informative as to the relative prevalence of these explanations.

The exception is New York, where REA nonmonetary issue detection rates are much lower. Discussions with state staff suggest that the hold induced by a missed REA meeting does not result in a nonmonetary issue being raised until the hold is lifted. Further, missed meetings that are rescheduled and attended within the same week are not recorded as a nonmonetary issue, and so are not adjudicated. Similarly, meetings that are attended much later than originally scheduled (e.g., after four weeks of unpaid claims) do not result in a nonmonetary issue raised unless the claimant requests it. For the purposes of our analysis, this means that most de facto holds in New York have no corresponding nonmonetary issue.

The pattern is different in Wisconsin. In the other three states, REA nonmonetary issues detected for *Control* are trivial. This is as would be expected: If a claimant is not selected for an REA meeting, no REA nonmonetary issue should be raised. However, for 14 percent of *Control* participants in Wisconsin, an REA nonmonetary issue is detected. From *Control* to *Partial*, the rate rises by only nine percentage points. Discussions with REA leadership in Wisconsin suggest that the state imposes a requirement on all UI claimants—including *Control*—to complete an online orientation and assessment. In Wisconsin, failure to satisfy that requirement is the most commonly reported REA-related nonmonetary issue code for *Control*.

[52] Exhibit 4-3 considered *Existing*, so the appropriate comparison is to *Single* in Indiana and *Multiple* in the other states. In Indiana, the initial non-attendance rate (taken as the complement of Attending On Time) was 56 percent, but the nonmonetary issues rate is only 38 percent, for a ratio of 68 percent. In Washington, the corresponding figures are 45 percent, 31 percent, and 70 percent; for Wisconsin, the corresponding figures are 42 percent, 33 percent, and 78 percent.

[53] Exhibit 4-4 plotted rates of attendance among those claimants who did not attend on time. Given that very roughly half of claimants attend on time, halving the plotted rates gives a rough estimate of the prevalence among all claimants randomly assigned to *Existing*. This suggests that attendance later in the week is a few percent of everyone required to attend and attendance in the following week is about 10 percent. Not all of those claimants attending in the following week would have good cause. Instead, some of them would attend in response to the threat of a nonmonetary issue.

Relative to *Single*, the *Multiple* treatment condition requires additional meetings.[54] Thus, there are more opportunities for claimants to miss REA meetings, have benefits suspended, and have an REA-related nonmonetary issue raised for adjudication. Consistent with that pattern, REA nonmonetary issue detection rates are higher for *Multiple* than for *Single* in Washington and in New York (despite the fact that a referral for formal adjudication process is not automatically made after a missed REA meeting).

Wisconsin did not implement *Single*, so we only have REA nonmonetary issue detection rates for *Partial* and *Multiple*. Non-attendance rates at the first REA meeting are higher for *Multiple* than for *Partial* (possibly due to claimant forward-looking behavior). Thus, Wisconsin's higher nonmonetary issue detection rates for *Multiple* relative to *Partial* are likely due to some combination of lower attendance rates at the first REA meeting (anticipating the burden of a possible subsequent meeting) and additional REA nonmonetary issues detected at subsequent REA meetings. Given that Wisconsin sends relatively few claimants assigned to *Multiple* to a second REA meeting and even fewer REA claimants to a third REA meeting (27 and 4 percent, respectively; see Exhibit 4-5), it seems plausible that the difference is mostly due to the lower attendance rate at the first REA meeting (and not mostly state response to non-attendance at subsequent REA meetings).

In summary, **REA nonmonetary issue detection rates vary across treatment conditions in ways consistent with relative REA meeting attendance rates**. In addition, **REA staff record pending or formal nonmonetary issues at both the first and subsequent REA meetings**.

5.3 Nonmonetary Issues Detected for Other Reasons, and Subsequent Denials

Among the goals of the REA program is the "prevention and detection of UI improper payments" (UIPL No. 10-14, p. 1). This is the "eligibility assessment" element in the REA program's name. The literature suggested that enforcement—that is, increased oversight of job search and raising the number of required employer contacts—substantially lowers UI weeks (see the discussion in Section 2.4/Exhibit 2-8). We would therefore expect REA to increase nonmonetary issues detected and raised for adjudication related to ongoing UI eligibility issues other than compliance with REA requirements (e.g., not "able and available" for work, insufficient job search, refusals of suitable job offers).

Indeed, the multi-armed randomization design was specifically intended to distinguish impacts that might be realized through the enforcement pathway versus the assistance pathway. *Partial* was to include all of the enforcement, but none of the assistance. *Single* (and *Multiple*) was to add assistance, allowing us to decompose the total REA program impact into precisely measured impacts of its major program components.[55] (On causal pathways, see Section 2.2.2.)

Exhibits 5-2 through 5-5 plot other non-REA nonmonetary issues detected and denials resulting from those detections. Again, the process of raising and adjudicating issues in New York seems different; we do not discuss New York further in this section.

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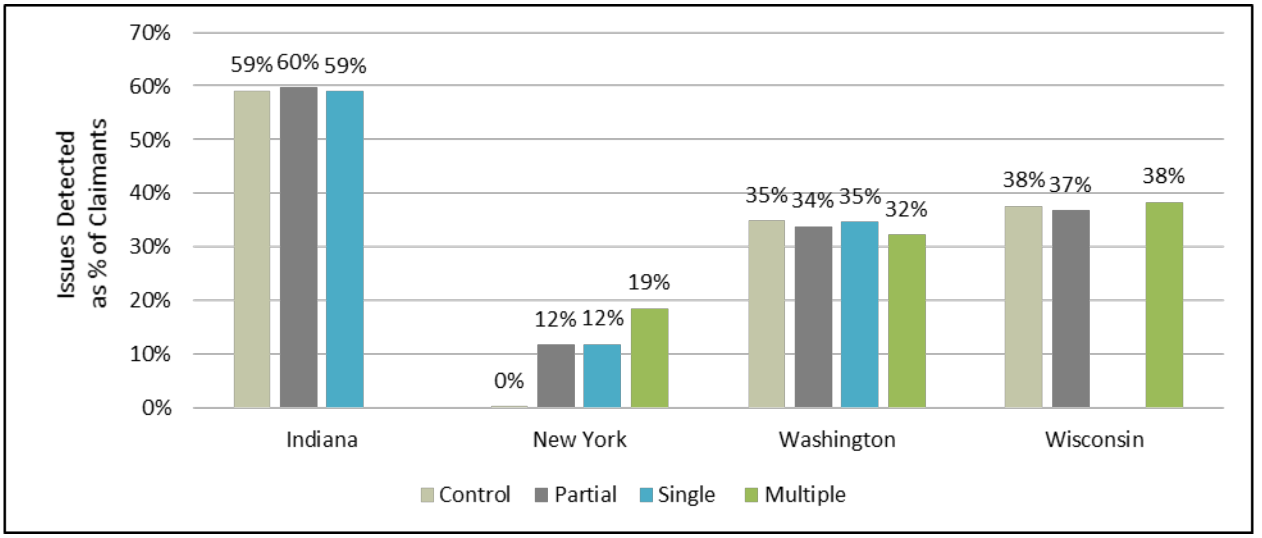
[54] Indiana did not implement *Multiple*, so there are no *Single*/*Multiple* comparisons to discuss.

[55] We will argue for a different interpretation: that much of these impacts may have been of the procedural requirement to attend the REA meeting at which enforcement was to occur—not of the enforcement of other ongoing eligibility requirements.

For those claimants not selected for REA (i.e., *Control*), nonmonetary issues that states classify as related to *initial* eligibility that potentially could affect a claimant's basic entitlement (e.g., a voluntary quit that was discovered after the initial UI claim is approved) or could result in an adjustment of the amount or duration of the claimant's benefits (e.g., additional income the claimant receives after the initial UI claim is approved) are common—a third or more (see Exhibit 5-2).[56]

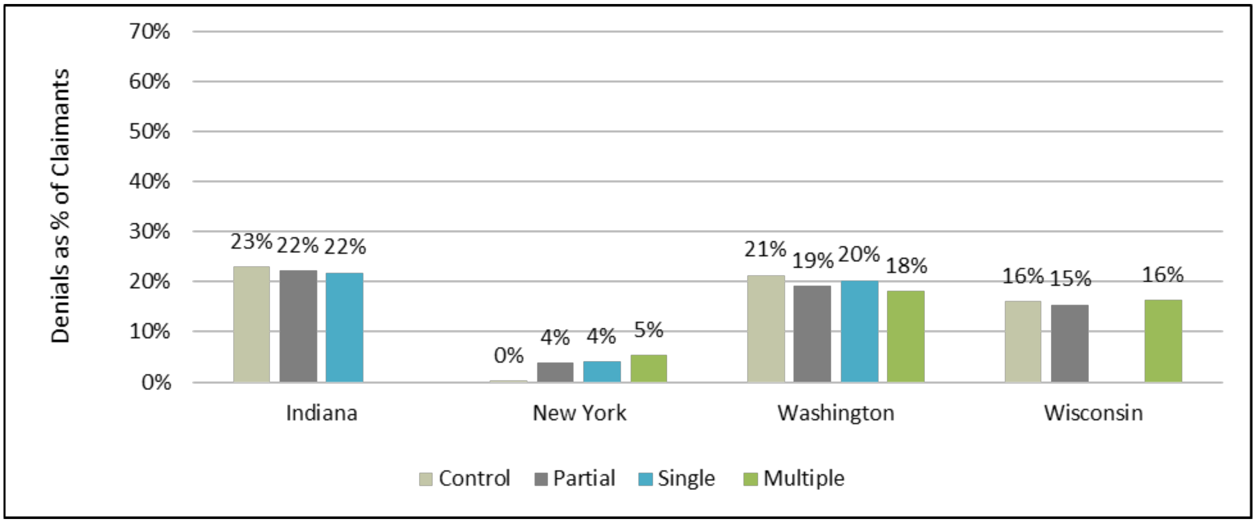
Exhibit 5-2 Nonmonetary Issues Detected Related to Initial Eligibility Issues, by Treatment Condition



Very roughly, half of the issues detected—whether formally raised or pending issues—were later adjudicated as denials (see Exhibit 5-3 below). However, and crucially for this study, REA does not increase the rates at which such initial nonmonetary issues are raised or denied. This is not surprising, as REA was not intended to detect *initial* eligibility issues.

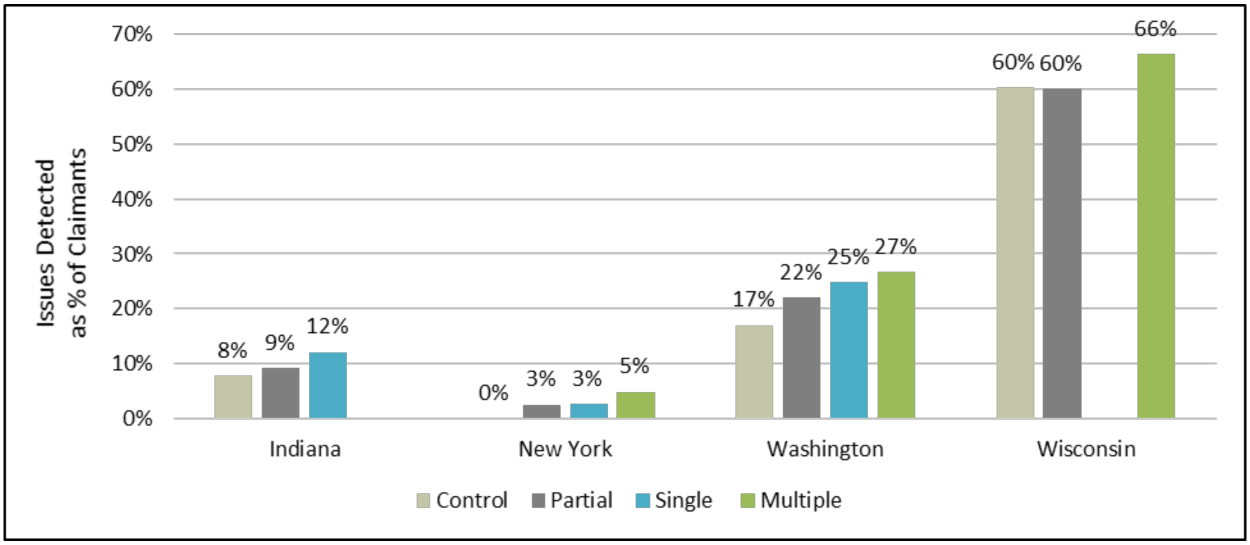
56 Throughout this chapter, we tabulate issues raised and denials per claimant. We make no direct attempt to identify which issues raised and denials are due to REA. We make that inference by comparing rates across treatment conditions.

Exhibit 5-3 Denials Resulting from Nonmonetary Issues Related to Initial Eligibility, by Treatment Condition



The findings are slightly different for *ongoing* eligibility. UI claimants must certify every week that they remain eligible for benefits by being "able and available" for work, compliant with program requirements, pursuing a sufficiently intensive job search, and have not refused a suitable job offer. The REA meeting is not the only way to detect ongoing eligibility issues. Exhibit 5-4 shows that even those claimants not selected for REA (i.e., *Control*) have ongoing eligibility issues detected. Rates of nonmonetary issues detected vary widely across states: highest in Wisconsin (nearly two-thirds), lower in Washington (less than a fifth), and lowest in Indiana (less than a tenth).

Exhibit 5-4 Nonmonetary Issues Detected Related to Ongoing Eligibility Issues, by Treatment Condition

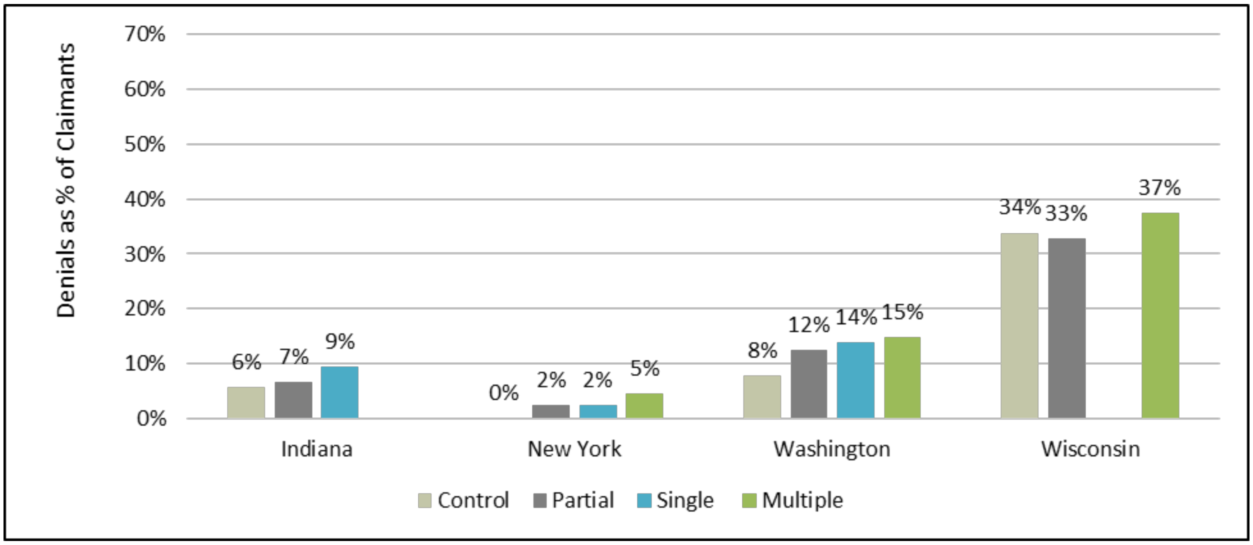


This study's design of *Partial* was specifically intended to include REA's enforcement. Nevertheless, in Exhibit 5-4, *Partial* nonmonetary issue detection rates for ongoing eligibility are barely different from *Control* in Indiana and Wisconsin. In New York, nonmonetary issues detected for ongoing eligibility are only three percentage points more common for *Partial* than for *Control*. In Washington, they are only five percentage points more common. **These differences in rates of detection of ongoing eligibility issues are trivial compared to the difference in rates for REA-related issues in Indiana and Washington** of more than 20 percentage points (see *Single* vs. *Partial* differences in Exhibit 5-1).

Against our initial expectations, Exhibit 5-4 shows that ongoing nonmonetary issue detection rates are moderately higher for *Single* than for *Partial* (three percentage points in Indiana and Washington) and for *Multiple* than for. *Partial* (five percentage points in Washington, six percentage points in Wisconsin). We expect that the more interaction a claimant has with REA staff, the more eligibility issues may potentially be flagged. For Indiana and Wisconsin, Exhibit 4-7 showed that attendance rates at the first REA meeting are lower for *Single* and *Multiple* than for *Partial*. These differential attendance rates may explain some of the differential in ongoing nonmonetary issues detected and in subsequent denials. In Washington and Wisconsin, additional ongoing nonmonetary issues may be detected at the second and third REA meetings. However, because few REA claimants have those later meetings, that pathway is not likely to be important.

Cross-treatment condition patterns for ongoing denials are similar to patterns for ongoing nonmonetary issues detected, but the magnitude of the cross-treatment differentials is much smaller (Exhibit 5-5).

Exhibit 5-5 Denials Resulting From Nonmonetary Issues Related to Ongoing Eligibility, by Treatment Condition



It seems reasonable to summarize the results as follows. Detection of nonmonetary issues (whether pending or raised for adjudication) is common and ongoing throughout the life of a claim; denials are not rare. **REA increases nonmonetary issues detected and subsequently denials of UI resulting from nonmonetary issues**. The more intense the treatment—*Control* is less intensive than *Partial* which is less intensive than *Single* which is less intensive than *Multiple*—the higher the rates. **But the increase in detections and denials due to REA is small**. The largest increase in denials is seven percentage points (for Washington, *Multiple=Existing* vs. *Control*). If each denial resulted in a loss of one week of UI benefits, the decrease in UI would be 0.07 weeks.[57] This is trivial compared to the potential impact of

[57] If the denial leads to a loss of more than a week of UI benefits, then this simple computation is an underestimate. However,

suspension of benefits until compliance attendance at the REA meeting (see Section 5.1 and the simulation in Exhibit 2-2).

5.4 Impact on "Claimed Not Paid"

Claimants whose benefits are suspended—in particular, for noncompliance with REA requirements—may claim UI benefits, but they will usually not be paid. Regardless, a claimant has an incentive to continue to claim. If the suspension is later deemed inappropriate, the individual will be retroactively paid—for weeks in which a claim is filed.

Thus, the difference in weeks claimed and weeks of UI benefits paid is a weak lower bound of the impact on weeks of UI of enforcement of the procedural requirement to attend the REA meeting. That is, the true effect of enforcement of the procedural requirement to attend the REA meeting is likely much larger.[58] We refer to this difference as "Claimed Not Paid." Claimed Not Paid is only a weak lower bound because the claimant has to claim for the week(s) of the missed meeting in order to appear in the analysis. If claimants leave UI to avoid enforcement, they will likely stop claiming right away (we show in Chapter 6 that this behavior appears to be rare). Others claim for several weeks after their benefits are suspended, but then after some period without being paid, they stop claiming. In both of these cases, there are weeks that are not paid because of the enforcement of the procedural requirement to attend the REA meeting for which the individual did not even claim. Thus, those weeks would not be included in "Claimed Not Paid." This leads to "Claimed Not Paid" being a lower bound on the true effect of enforcement.

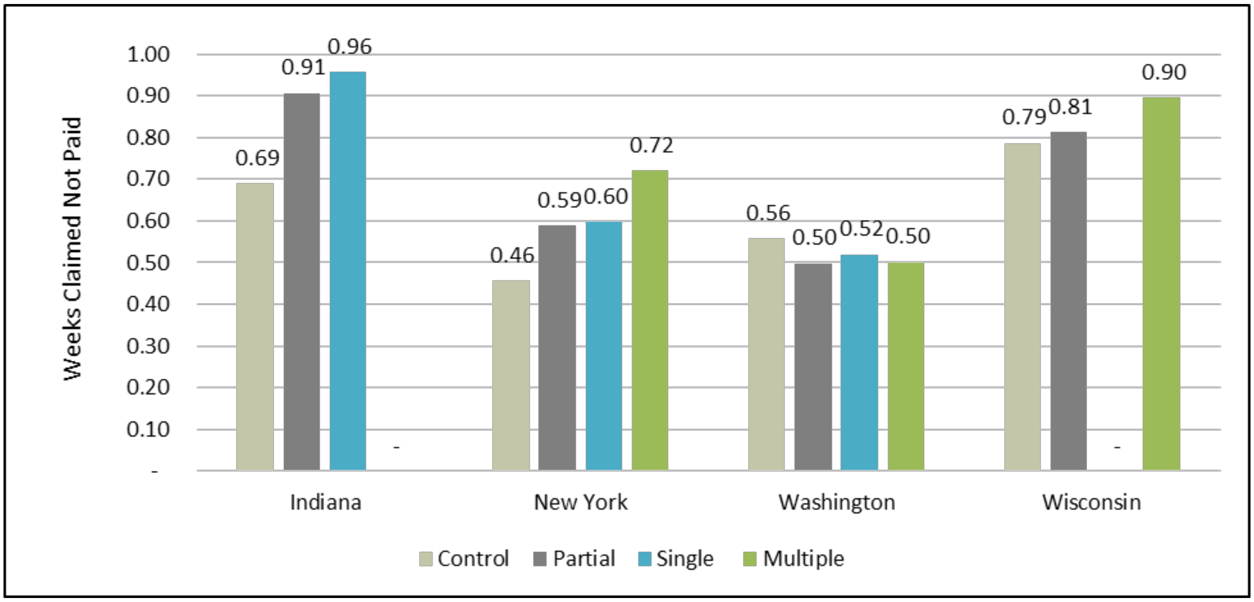
Despite those caveats, Claimed Not Paid appears to be a useful lower bound for the impact of enforcement. Exhibit 5-6 below plots levels of Claimed Not Paid by treatment condition and state.[59] Even for those claimants not selected for REA (i.e., *Control*), **Claimed Not Paid occurs in all states—at rates near or above half a week**.

we heard no discussion that implied that denials due to nonmonetary eligibility issues lead to a loss of more than the week for which the issue is detected. Furthermore, as noted earlier, denied weeks are not lost. Instead, they remain available to be claimed later in the benefit year.

58 Choosing to stop claiming is one pathway. It also appears that in some states, under some circumstances, the computer system will not allow a claimant to claim until some eligibility issue is resolved; then they can retroactively file their claims for the previous weeks. It is unclear to us in which cases that computer system interlock actually leads to no Claimed Not Paid. Inasmuch as it does, Claimed Not Paid is an even weaker bound on the effect of enforcement.

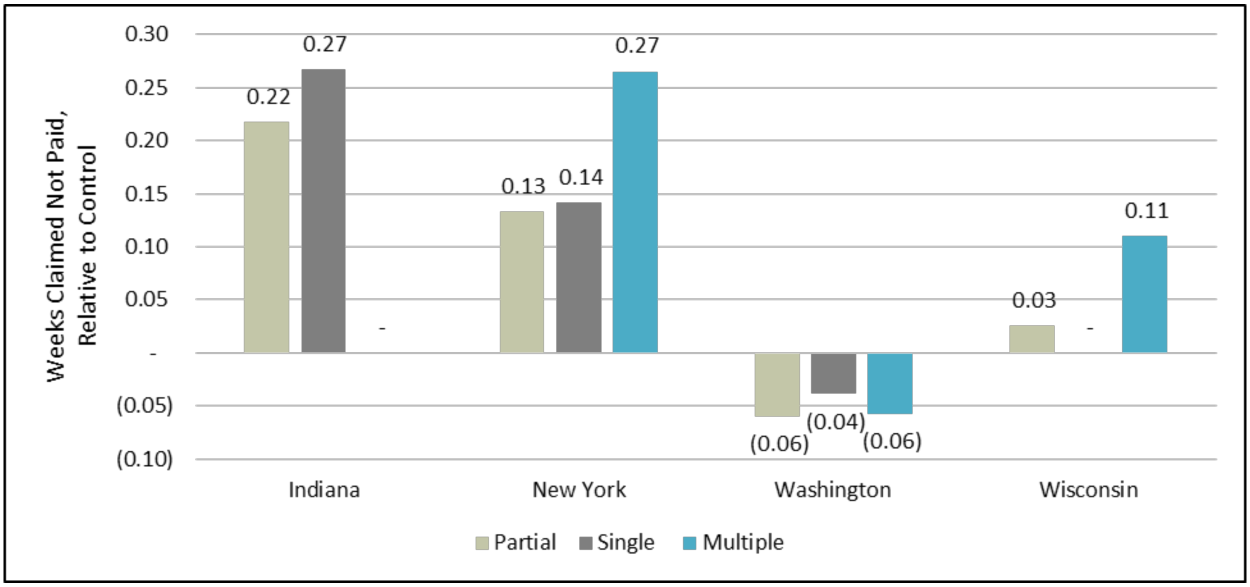
59 Our samples are very large. Standard errors on differences in levels of Claimed Not Paid across treatment conditions imply that differences of one percentage point are clearly statistically significant. The discussion in the text therefore does not consider statistical significance.

Exhibit 5-6 Claimed Not Paid, by Treatment Condition



In all states but Washington, **levels of Claimed Not Paid are higher with REA and increasing with the intensity of REA**. Exhibit 5-7 plots Claimed Not Paid by treatment condition, relative to no REA. Impacts are more than a quarter of a week in Indiana and New York, about half that size in Wisconsin, and negative in Washington.[60]

Exhibit 5-7 Claimed Not Paid, Relative to No REA (*Control*), by Treatment Condition



[60] We would expect a zero impact for Washington. Washington pays benefits for a claim; if benefits are retrospectively deemed to have been paid incorrectly, they are recouped as an overpayment.

We consider possible explanations for this pattern in the next section. Here, even this weak lower bound is a sizable fraction of the 0.5 to 1.5 weeks impact that is standard for interventions for UI claimants (see the discussion in Section 2.3/Exhibit 2-3 and Section 2.4/Exhibit 2-8). The true effect of enforcement is likely much larger—in weeks and also as a share of estimated impacts of such programs.

5.5 Discussion

This chapter has considered state responses to noncompliance. Some of the data on nonmonetary issues detected appear incomplete (in particular, for New York) and the details of what is recorded vary across the states. With those caveats, this chapter's estimates of how REA affects detection and determinations for non-attendance at the REA meeting and for ongoing eligibility requirements are crucial for how we interpret the estimates of impact on UI weeks (in Chapter 6) and employment and earnings (in Chapter 7).

In particular, Section 2.2.2 described three possible causal pathways for this impact: assistance, enforcement, and the procedural requirement to attend the REA meeting. The results in this and the previous chapter provide some insight as to the relative importance of these three possible causal pathways.

The impact of enforcement is mostly driven by the procedural requirement to attend the REA meeting. Consistent with the high rates of non-attendance reported in the previous chapter, rates of detection of nonmonetary issues related to the procedural requirement to attend REA meetings are high and resulting denials are common—about one such issue detected for every three REA claimants. In contrast, though REA has a measurable impact on detection of nonmonetary issues and resulting denials related to ongoing eligibility (unrelated to the procedural requirement to attend the REA meeting), the rate is much lower, less than one such issue detected for every 10 REA claimants. Thus, the impact of *Partial* vs. *Control*—which we interpret as the impact of enforcement—is not so much from the additional enforcement of ongoing eligibility requirements; rather that impact is mostly from the procedural requirement to attend the REA meeting.

In addition, some of the impact of *Existing* vs. *Partial*—which, as the design was originally conceived, was to be interpreted as the impact of assistance—is actually enforcement of the procedural requirement to attend the REA meeting. First, in some states, the longer *Existing* meeting (relative to the shorter *Partial* meeting) leads to lower attendance (at *Existing* relative to *Partial*). This part of the *Existing* vs. *Partial* differential is not an effect of assistance received; rather, it is an effect of enforcement of the procedural requirement to attend the REA meeting applied to the higher non-attendance rate. Second, in states in which *Existing*=*Multiple*, there is additional noncompliance at later meetings. Again, this part of the *Existing* vs. *Partial* differential is not assistance; rather, it is the effect of enforcing the procedural requirement to attend subsequent REA meetings.

Finally, we argued that Claimed Not Paid is a weak lower bound of the impact of enforcement. For Indiana and New York, impacts are greater than a fifth of a week, suggesting the potential for large impacts of enforcement on UI weeks.

This finding of only a small increase in nonmonetary issues detected—*excluding* noncompliance with the procedural requirement to attend the REA meeting—is consistent with our qualitative field work for the *Implementation Report* (Minzner et al., 2017). Discussions with staff and observation of service delivery as part of that field work suggested that staff conducting REA meetings view their primary role as to help claimants find a job quickly. They do not perceive enforcing program rules—for example, checking whether claimants are “able and available” for work, conducting a sufficiently intensive job search, and have not refused a suitable job offer—as their primary role. If they note disqualifying behavior, they are unlikely to report it. Instead, they advise claimants as to how to change their oral and written statements so as to not be recorded as noncompliant. In fact, some states see such advice to clients as an appropriate role for case managers.

What the pre-REA enforcement literature interpreted as an effect of enforcement was likely often primarily the effect of enforcing the requirement to attend the meeting at which the enforcement was to occur (as summarized in Exhibit 2-8). Those studies found that increasing the oversight of job search decreased UI weeks paid. It seems unlikely that those impacts were primarily the direct result of the enforcement. To see this, suppose that every non-REA nonmonetary denial results in loss of benefits for the week in which the eligibility issue is detected, and the increment in the rate of denials due to the REA program is 25 percentage points—which would be more than the largest increment we observed (seven percentage points of *Multiple* vs. *Control* in Washington). Even that increment would be well under half of the 0.5 to 1.5 weeks impacts estimated for pre-REA enforcement programs (see Section 2.4/Exhibit 2-8). The direct impact of such pre-REA enforcement programs could be larger if the denial led to a loss of more than a week of benefits. However, we are unaware of any support for that conjecture, and it is unclear whether such loss of benefits for more than the week of the denial is consistent with UI rules. Instead, in those pre-REA enforcement programs, it seems likely that about half the time, claimants did not attend the meeting at which ongoing eligibility was to be reviewed. States sometimes responded by suspending benefits. Thus, it seems plausible that the estimated impact of such pre-REA enforcement programs was primarily from the suspension of benefits pending compliance with the procedural requirement to attend the program’s meeting.

What the pre-REA assistance literature and the earlier REA literature interpreted as an effect of assistance was likely also primarily the effect of enforcing the requirement to attend the meeting at which the assistance was to occur. These results have similar implications for the assistance literature (also as summarized in Exhibit 2-8) and for studies of the impact of REA (Section 2.3/Exhibit 2-3). Those programs certainly required a meeting. Even moderate suspension of benefits in response to non-attendance at the program’s meeting could explain much of the estimated impact of such programs (about half a week). It thus seems plausible that much of the estimated impact on UI weeks of pre-REA assistance programs and of earlier studies of REA was from the procedural requirement to attend the meeting, not from the assistance itself. Future studies of the impacts of interventions requiring a meeting should carefully explore the extent to which the impact on UI weeks is due to the content of the meeting versus (noncompliance with) the requirement for the meeting.

The overall impact of a state REA program on UI weeks likely varies substantially with the details of the state’s reaction to non-attendance at the REA meeting. Cross-state differences in response to non-attendance at the REA meeting plausibly induce cross-state differences in the impact of REA on UI weeks. These are non-experimental, cross-state observations. Any such observations need to be tentative. We only have four states and their programs and data differ along many dimensions. With that crucial caveat, we use the rough simulation of the effect of the uniform and immediate suspension of benefits until compliance response to non-compliance discussed in Exhibit 2-2 to interpret the likely impact of state policies. (This analysis does not assume or imply that such a response to non-attendance is necessarily desirable, it simply uses the framework to quantify potential impacts and better understand the pattern of results.) That simulation suggested that uniform and immediate suspension of benefits until compliance in response to non-attendance alone (i.e., with no assistance and no increased enforcement of other program requirements) might lower UI weeks by as much as 2.0 weeks.

First, in three of the states the de facto response to non-attendance at the REA meeting is suspension of UI benefits until attendance occurs.[61] In Washington, a formal nonmonetary issue is immediately raised for adjudication, but when the adjudication results in a denial, the claimant only has to repay one week of non-attendance. The meeting is not rescheduled; benefits continue while the adjudication is being processed and while the overpayment is recouped. Returning to our simulation, if no more than a week of benefits is lost, the simulated impact of non-attendance alone drops from 2.0 weeks to 0.4 weeks (because the "Never Attend (and Not Exempt)" category in Exhibit 2-2 drops from 18 weeks lost to two weeks lost). This suggests that we might expect a much smaller impact of REA on UI weeks in Washington than in other states.

Second, in three of the states, a selected claimant is assigned an REA meeting date and time approximately two weeks after randomization. In Wisconsin, a selected claimant has five weeks after randomization to attend the meeting: two weeks to schedule and then three weeks to attend. Successful self-scheduling should eliminate some—perhaps much—of the attendance later in the week or in the following week.

Beyond the switch from scheduling the meeting for the claimant to allowing self-scheduling, as long as the claimant schedules a meeting and completes the online orientation, Wisconsin's five-week self-scheduling window pushes the timing of any response to non-attendance back three weeks (from the two weeks post-randomization when most states schedule the REA meeting to the end of the five-week self-scheduling window). In Wisconsin, a sizable fraction of selected claimants already attend a meeting in the second week after randomization, so a shorter self-scheduling window is probably feasible.

Another difference in implementation in Wisconsin implies an even smaller simulated effect of the impact of the procedural requirement to attend the REA meeting. Unlike the other three states, Wisconsin requires even those claimants assigned to *Control* to complete an orientation. This requirement plausibly lowers the simulated impact of the procedural requirement to attend the REA meeting to 0.7 weeks, about a third of the 2.0 weeks for the full simulation shown in Exhibit 2-2.[62] Inasmuch as the impact of REA is

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[61] In New York, the maximum suspension period was four weeks, but few individuals appeared to continue claiming for four weeks without being paid (instead they just stop claiming or they attend), so de facto benefits are suspended indefinitely.

[62] Suppose that half of the claimants who would never attend the REA meeting (10 percent of all claimants) never even complete the orientation, and that the state suspends benefits promptly such that these claimants on average lose 10 weeks. First consider *Control:* With these assumptions, *Control* loses 1.0 weeks (10 percent of selected claimants lose an average of 10 weeks). Now consider those claimants assigned to REA:

- Suppose self-scheduling cuts the percentage who attend late in half (from 20 to 10 percent: some stop claiming by the end of the self-scheduling period (which is five weeks after randomization), and self-scheduling allows more to attend on time. In total, this group loses 0.1 weeks (= 10% × 1 week).
- The end of the self-scheduling window is about three weeks later than the date of the meeting in other states, so the impact of never attending drops from nine weeks to six weeks. In total, this group loses 0.6 weeks (= 10% × 6 weeks).
- Finally, as for *Control*, the 10 percent who do not even complete the orientation lose 10 weeks. In total, this group loses 1.0 weeks (= 10% × 10 weeks).

Combining the pieces, the simulation implies that a claimant assigned to REA loses 1.7 weeks (= 0.1 for late attendance + 0.6 for non-attendance + 1.0 for not completing the orientation), whereas a claimant assigned to *Control* loses 1.0 weeks for not through the procedural requirement to attend the REA meeting, the simulation suggests that we should expect a much smaller impact of REA on UI weeks in Wisconsin than in other states.

Uniform and immediate suspension of benefits until compliance in response to non-attendance alone could lead to an impact of REA of as much as two weeks. This is more than the *total* impact of REA in almost every study in the earlier literature. The simulation in Exhibit 2.2 already implied an impact of uniform and immediate suspension of benefits until compliance response to non-attendance of two weeks. That simulation did not include the impact of responses to non-attendance at subsequent REA meetings. Non-attendance at subsequent REA meetings also appears to be common (see Exhibit 4-5). The more common are those subsequent REA meetings, the more meetings that will be missed, and the more weeks for which benefits will be suspended (see Section 5.2/Exhibit 5-1). In net, this implies an impact of the procedural requirement to attend the REA meeting that is larger than the simulated impact in Exhibit 2-2.

completing the orientation. The net effect of being assigned to REA is thus 0.7 weeks (= 1.7 weeks for those assigned to REA − 1.0 weeks for those assigned to *Control*). This is about a third of the 2.0 weeks for a conventional REA program with universal and immediate suspension of benefits.

6. Impacts on Receipt of UI Benefits

Returning people to gainful employment and reducing the duration of UI is a primary goal of reemployment programs serving UI claimants. Consistent with that goal, such programs are conventionally evaluated in terms of their impact on weeks of UI paid. That outcome is easily measurable in state UI data and is closely related to drops in benefits paid. This chapter considers the impact of REA on receipt of UI.

Data. This chapter analyzes state administrative data on weekly UI benefits paid[63] (both binary paid/not paid and dollars). The chapter also analyzes NDNH data on quarterly UI benefits paid (dollars).[64]

Methods. Unless otherwise noted, the results presented in this chapter are random assignment estimates of the impact of *Existing* vs. *Control*, generated using our standard estimation methodology (see Appendix B in the appendix volume). When we discuss other contrasts (e.g., *Partial* vs. *Control*), we identify them explicitly. We note explicitly when impacts differ across the four states; otherwise, statistical tests suggest that differences observed across states are likely due to chance. Unless otherwise noted, there is no consistent evidence of differential subgroup impacts.

Chapter 6 Key Findings

- REA unambiguously and substantially cuts UI durations. The pooled impact is about a week.
- Impacts vary—substantially and statistically significantly—across states. Impacts in Indiana and New York are about a week and a half; impacts in Washington and Wisconsin are about half a week.
- Impacts are larger (in absolute value) for claimants who are younger, less educated, have lower weekly benefits, and lower earnings.
- Assistance and enforcement contribute approximately equally to overall impacts and with patterns of timing consistent with the theory of action.
- Evidence is mixed on whether *Multiple* has an incremental impact above and beyond *Single*.
- In the NDNH data, there is evidence of small impacts on UI benefits in the longer term—past Q4.

Balance of This Chapter. Section 6.1 considers the short-term impact of the REA program on weeks of UI and dollars of UI benefits paid (*Existing* vs. *Control*). Section 6.2 considers how impacts vary with claimant baseline characteristics and local labor market conditions. Section 6.3 considers the relative

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63 For both UI benefits and weeks of Claimed Not Paid, state data are analyzed relative to date of randomization, not relative to date of initial UI claim. In general, the difference is a few weeks; for some claimants the difference is much larger.

State administrative data are available for at least 28 weeks after the last claimant was randomized in that state. As a result, the analysis has at least 28 weeks for everyone in the sample and more weeks for decreasing fractions of the sample. For about a quarter of the sample, the state administrative data are available for 52 weeks post-randomization. Unless otherwise noted, state administrative data results refer to the full (28-week post-randomization) sample.

64 NDNH data are analyzed relative to quarter of initial UI claim—termed Q0. Usually, but not always, that quarter is the same as the quarter of randomization. The NDNH provides pre-claim data on UI and earnings. We time quarters relative to the initial claim so that we can be sure that Q(−1) is always before the UI claim.

NDNH data are available for the entire sample for at least two years post-randomization, which would usually be the period Q0 (the quarter of initial claim) through Q8. For smaller fractions of the sample, some data are available for up to three years post-randomization (Q0 through Q12). For very small fractions of the sample, we have data through Q13; these are so few that we report no outcomes for that quarter.

importance of assistance versus enforcement. Section 6.4 considers the incremental impact of *Multiple* vs. *Single*. Section 6.5 considers longer-term impacts. Section 6.6 provides a discussion of these results in the context of theory of and broader literature on REA and REA-like programs.

6.1 Overall Impact of REA

Exhibit 6-1 and 6-2 present results for the study's primary and single confirmatory outcome:[65] the impact of REA (i.e., *Existing* vs. *Control*) on weeks of UI benefits received in the data pooled across all states (through 28 weeks after randomization).[66,67] Considering the pooled estimate, the result is simple and unambiguous: **REA decreases weeks of UI benefits received by more than a week**. The estimated impact of −1.262 weeks is clearly statistically significant ($p < .01$) and precisely estimated (a 95 percent confidence interval covers the range −1.352 to −1.172 weeks; less than a tenth of a week on either side of the estimated impact).

Exhibit 6-1 Impact on Weeks of UI Benefits Paid (in weeks), *Existing* vs. *Control*

State	*Control*	*Existing*	Impact	SE	Heterogeneity
Pooled	14.882	13.620	−1.262***	0.046	<.01***
IN	14.624	12.945	−1.678***	0.125	<.01***
NY	15.422	13.970	−1.452***	0.061	<.01***
WA	14.133	13.338	−0.795***	0.114	<.01***
WI	13.617	13.100	−0.517***	0.133	<.01***

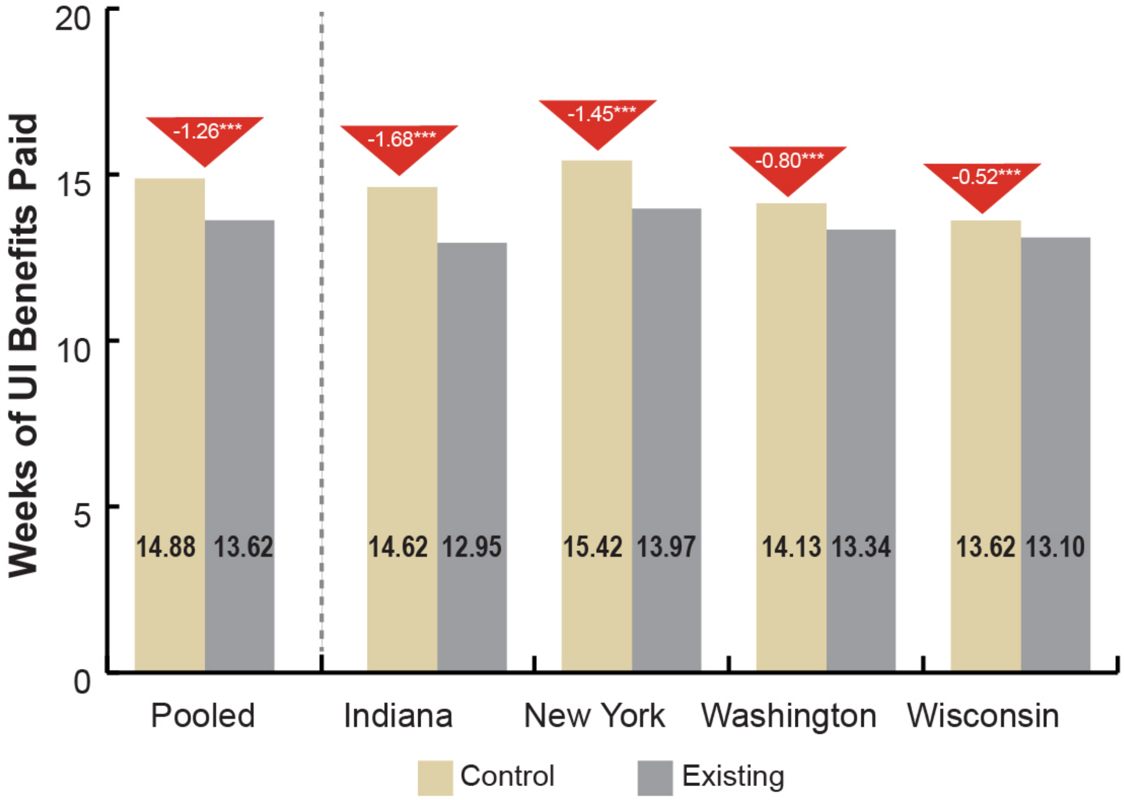
Source: Regression-adjusted impact estimates based on state administrative data, Model(s): UWSW28eczz, Run Date: 29MAR2019
Note: Statistical significance levels for impacts are based on two-sided tests and flagged with asterisks, as follows: *** < 1 percent; ** < 5 percent; * < 10 percent. The pooled entry in the "Heterogeneity" column is the *p*-value for a test that all of the state impacts are equal; the other entries are *p*-values for a test that this state's impact equals the minimum variance combination of the other states' impacts.

65 To address multiple comparisons concerns, the study pre-specified the *Existing* vs. *Control* contrast in weeks of UI benefits paid through 28 weeks post-randomization as the study's single confirmatory outcome; that is the outcome through which we will make a determination of whether the REA program was effective.

66 In the four participating states, benefits can be claimed at any point in the year (i.e., 52 weeks) after the initial claim. Thus, measuring impacts only through 28 weeks post-randomization—roughly 31 weeks post–initial claim—is not ideal. It is, however, the best the evaluation can do for the full sample. States provided data only through 28 weeks following the last claimant randomized. That is, for those claimants randomized early, we have the full 52 weeks of data; for those randomized last, we have only 28 weeks post-randomization. Appendix C in the appendix volume reports a sensitivity analysis to this limitation—comparing estimated impacts through 28 weeks post-randomization to estimated impacts through 52 weeks post-randomization. That sensitivity analysis suggests that results are robust to the limitation to 28 weeks.

67 For help reading this report's impact tables, see the discussion in Section 3.4.

Exhibit 6-2 Impact on Weeks of UI Benefits Paid (in weeks), *Existing* vs. *Control*



Source: Regression-adjusted impact estimates based on state administrative data, Model(s): UWSW28eczz, Run Date: 29MAR2019
Note: Statistical significance levels for impacts are based on two-sided tests and flagged with asterisks, as follows: *** < 1 percent; ** < 5 percent; * < 10 percent.

This pooled estimate hides that **there is considerable heterogeneity—that is, variation in impact—across the states**.[68,69] Estimates for Indiana and New York are about a week and a half; estimates for Washington and Wisconsin are about half a week. A formal test provides strong evidence that true impacts vary across the states (in Exhibit 6-1 see the far right column of the pooled row). This heterogeneity in impacts recurs throughout this report. It has important policy implications, which we consider in the final Chapter 8.

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[68] Heterogeneity is emblematic of the federal-state UI partnership, where federal law allows a wide diversity in the content and procedures in the administration of state UI programs. Furthermore, Wandner (2018, pp. 10-11) that federal guidance has become less salient in recent decades.

[69] One possible explanation for the interstate variation in impacts is interstate variation in the characteristics of UI claimants selected for REA combined with variation in impacts for those characteristics. The next section finds evidence of variation in impact with UI claimant characteristics. This study does not further explore the extent to which interstate variation in the characteristics of claimants selected for UI might explain interstate variation in impacts. That is a subject worthy of further study. Doing so would require careful consideration of the extent to which claimant characteristics are consistently recorded across states. The discussion below suggests that recent earnings are a strong predictor of differential impact. That information should be consistently recorded.

Exhibit 6-1 considers impact on weeks of UI benefits paid. For a cost-benefit analysis, the impact on dollars of benefits paid is a more appropriate measure. The pooled estimate is −$347 (see Appendix Exhibit C-1 in the appendix volume). Percentage impacts will vary inasmuch as impacts vary with the weekly benefit amount (which we explore in Section 6.2). In fact, the percentage impacts for the overall impact of REA (*Existing* vs. *Control*) are slightly smaller for benefits paid than for weeks (7.6 percent versus 8.5 percent for pooled, with a similar pattern across all states; see Appendix Exhibit C-1 for details).[70]

This decline in benefits paid is clearly larger than the direct program cost (i.e., Office of Unemployment Insurance payments to states for REA—which are about $100 per claimant selected). The evaluation did not perform a formal cost-benefit analysis, but this decline in benefits paid is plausibly larger than the full cost of the REA program.[71] To see this, note that the WIA Adult and Dislocated Worker Program Evaluation costed a one-on-one counseling session at $154 and a group workshop at $54 (Mastri & McCutcheon, 2015). Then, even assuming intensive utilization, the implied cost per case selected is only about $300 per claimant[72]—which would be below the UI benefits savings of $347.[73]

As noted, states provided data only through 28 weeks post-randomization. The main results reported here use the full sample and therefore present impacts only through 28 weeks post-randomization. This is potentially problematic. A typical UI claim consists of a maximum benefit of 26 weeks that can be claimed at any point during a 52-week eligibility period. Thus, there could be differential impacts between week 28 post-randomization (usually about week 32 post–initial UI claim) and the end of the benefit year.

70 This is consistent with the finding below of larger impacts for claimants with below-median weekly benefit amounts (see Exhibit 6-3 below).

71 By "full cost" we mean to include both costs borne by DOL for the REA program and also costs borne by DOL for other of its programs. This premise follows from the "global" (all-governments) perspective that is standard in cost-benefit analysis. The benefits accrue to state Unemployment Insurance trust funds. Meanwhile, the cost of the services is paid by a combination of (federal) DOL for REA and WIA and state unemployment agencies for Wagner-Peyser Act services. (The latter funded though Federal Unemployment Tax; most analyses suggest that the true incidence of these taxes is on workers.)

72 The $300 figure is derived using New York *Existing=Multiple*, which is the most intensive REA program studied: 76 percent of those claimants selected attend a first REA meeting, 53 percent a second REA meeting, and 35 percent a third REA meeting—1.64 REA meetings per claimant selected. Suppose that each of the meetings attended had the same cost as a one-on-one meeting lasting an hour; that would yield a cost of $234.52 (= 1.64 meetings × $143 per meeting). Suppose also that every REA claimant attending the first REA meeting then attended a workshop, and every REA claimant attending the second REA meeting then attended a second workshop. That would be 1.29 workshops per claimant selected at a cost of $69.66 (= 1.29 workshops × $54 per workshop). Meetings and workshops total $304.18.

Most state programs are less intensive than NY *Multiple* (i.e., fewer meetings attended), and second and third REA meetings in New York do not last an hour. Plus, some of these claimants would have used some of these services anyway. It follows that the $304.18 is probably an overestimate.

73 Adjusting Jacobson (2006) for inflation, Balducchi and O'Leary (2018) estimate substantially higher costs for Wagner-Peyser Act encounters than do Mastri and McCutcheon (2015) for WIA, $430 for a call-in and Job Search Assistance workshop. Why the two estimates are so different is unclear.

6.2 Differential Impact With Respect to Baseline Characteristics and Local Labor Market Conditions

Having considered overall impacts in the previous section, next we consider how impacts on weeks of UI benefits (through 28 weeks post-randomization) vary with a wide range of claimant characteristics and local labor market conditions (see Appendix B in the appendix volume for the full list and for precise definitions).[74] The analysis considers each subgroup separately. Thus, one would expect and the study finds that highly correlated subgroups show similar results.

Few of the subgroups analyzed show evidence of differential impact; that is, that impacts differ between the two groups (e.g., males versus females). Exhibits in this report present results for which there is clear evidence of differential impact and for selected other subgroups of particular substantive interest.[75] Appendix C presents results for all subgroups, including those for which there is no evidence of differential impact.

There is a large differential impact by weekly benefit amount (see Exhibit 6-3 below). Claimants with a weekly benefit amount below the median for the state have an impact nearly twice that for claimants with a weekly benefit amount above the median for the state (−1.555 weeks versus −0.929 weeks). This result implies that if funds allow serving only some UI claimants, serving those with a smaller weekly benefit amount will yield substantially larger impacts on total UI weeks. Inasmuch as the REA program is perceived as a burden, serving those with smaller weekly benefit amount might be perceived as raising equity issues.

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[74] All of the subgroups are binary. Continuous variables were divided at their (state-specific) median, so half of the sample is in each group.

All of the differential impacts are estimated directly at the state level (treating New York two-arm and New York four-arm as separate states). Specifically, the differential impact is the coefficient on the interaction of the subgroup variable with a treatment dummy variable (for *Partial*, for *Existing*, for *Single*, for *Multiple*). The estimates for the impact within the subgroups are also estimated directly (as the coefficient on the uninteracted treatment dummy, and as the sum of the coefficient on the uninteracted treatment dummy and the coefficient on the treatment dummy interacted with the subgroup variable).

The estimates for New York and for pooled are the minimum variance combinations of the state-specific estimates—*for New York:* New York two-arm and New York four-arm; *for pooled:* Indiana, New York pooled, Washington, and Wisconsin. The relative variances for the impacts being pooled need not be identical across the impacts in the subgroups and the differential impact. As a result, the differential impact sometimes diverges from the difference of the impacts in the subgroups by more than simple rounding error. Nevertheless, each of the reported pooled impacts is the minimum variance combination of the underlying estimated impacts.

[75] In general, we present and discuss subgroup results for which there is evidence for heterogeneity at $p < .05$ for the pooled (across all four states) estimate. We adopt this strategy for two complementary reasons. First, the strategy yields a more focused discussion; the alternative is multi-page exhibits of mostly null results. Second, given the large number of subgroups considered (and the small fraction that appear to show differential impacts), some of the differential impacts are due to chance (the problem of multiple comparisons).

Exhibit 6-3 Differential Impacts of Claimant Characteristics on UI Benefits Paid (in weeks), *Existing* vs. *Control*

State	Impact	SE	Impact	SE	Impact	SE
Age						
	Above Median		Below Median		Differential	
Pooled	−1.077***	0.062	−1.482***	0.069	0.379***	0.094
IN	−1.504***	0.158	−1.957***	0.206	0.453*	0.260
NY	−1.265***	0.086	−1.637***	0.087	0.377***	0.122
WA	−0.707***	0.145	−0.939***	0.186	0.232	0.236
WI	−0.306*	0.175	−0.806***	0.206	0.500*	0.271
College						
	Some		None		Differential	
Pooled	−0.449***	0.126	−0.898***	0.120	0.434**	0.174
WA	−0.599***	0.170	−0.974***	0.154	0.374	0.229
WI	−0.267	0.187	−0.782***	0.190	0.516*	0.267
Weekly Benefit						
	Above Median		Below Median		Differential	
Pooled	−0.929***	0.067	−1.555***	0.064	0.619***	0.093
IN	−1.104***	0.170	−2.283***	0.185	1.179***	0.252
NY	−1.067***	0.090	−1.768***	0.083	0.695***	0.123
WA	−0.677***	0.162	−0.920***	0.161	0.243	0.229
WI	−0.443**	0.190	−0.589***	0.187	0.146	0.267
Profile Score						
	Above Median		Below Median		Differential	
Pooled	−1.376***	0.066	−1.349***	0.075	0.011	0.102
IN	−1.963***	0.182	−1.392***	0.173	−0.570**	0.251
NY	−1.423***	0.079	−1.502***	0.097	0.111	0.127
WA	−0.713***	0.162	−0.884***	0.161	0.171	0.228

Source: Regression-adjusted impact estimates based on state administrative data, Model(s): UWSW28ec, Run Date: 29MAR2019
Note: Statistical significance levels for impacts are based on two-sided tests and flagged with asterisks, as follows: *** < 1 percent; ** < 5 percent; * < 10 percent.

There is some evidence of differential impacts with respect to other individual characteristics (again see Exhibit 6-3). Impacts are larger (in absolute value) for younger (vs. older) claimants and for claimants with no college (vs. at least some college; only available for Washington and Wisconsin). There is no evidence (pooled or consistent across multiple states) for differential impacts by gender, race, or ethnicity.

There is no pooled differential impact by profile score.[76] We note that there is a differential impact of profile score for Indiana; in that state, impacts are larger (in absolute value) for those claimants with profile scores above the median.

From one perspective, that the official profile score does not predict differential impact is somewhat surprising. Programs serving UI claimants have often used the official profile scores to select whom to

[76] This analysis pools over Indiana, New York, and Washington. There is no official profile score for Wisconsin. Instead, Wisconsin decided whom to select for REA using an alternative process. As part of the online orientation and assessment, the state assessed a claimant's need for assistance using a series of questions that included work search, work readiness, career/skill, technology, and employment resources. This information was used to create a score, which in turn was used to determine which claimants were selected for REA. Given that Wisconsin did not use a profile score to select UI claimants for REA, the state is not included in this analysis.

serve. Part of the motivation for using profile scores appears to be a presumption that the program would be more effective—that is, have larger impacts on UI weeks—for claimants with a higher profile score. That result would be expected if impact was proportional to weeks without the program. The estimated differential for Indiana is consistent with the presumption; the estimated impacts for the other two states are not.

From a different perspective, that the official profile score does not predict differential impact is less surprising. First, it is consistent with earlier work on WPRS in Kentucky by Black, Smith, Berger, and Noel (2003). That study had much smaller samples, such that the lack of a relation might have been explained as due to power.

Second and more fundamentally, the profile score models probability of exhaustion, which presumably is related to duration. Higher profile scores are presumably correlated with longer expected duration. If impacts were proportional to duration in the absence of the program, then a profile model would predict differential impact. Such a proportionality is plausible, but hardly necessary. Inasmuch as these results are correct, they suggest that profiling score (i.e., an empirical model for probability of exhaustion) is not a useful strategy for predicting differential impact. If the goal is to identify which REA-eligible claimants would have larger impacts, then one needs to estimate differential impact. Doing so requires large samples. This study has large enough samples to do so for UI duration. The results presented in Chapter 7 suggest that this study's samples are not large enough to identify differential impact for employment or earnings.

There is strong evidence for differential impacts with the claimant's recent labor market experience. Exhibit 6-4 below presents differential results by recent labor market experience as measured from the NDNH. Impacts are larger for those claimants who received UI in the previous year and for more UI benefits received in the previous year. Impacts are also larger for those claimants with less earnings in the previous year and for those with less earnings two years previous.

The results for earnings in the previous year are particularly large. Impacts are about half again as large (in absolute value) for those claimants with earnings below the median relative to those with earnings above the median. The pattern is present and strongly statistically significant in the pooled data and in every state alone except Wisconsin[77].

The finding that impacts are larger for those claimants with earnings in the previous year *below* the median is consistent with the earlier finding that impacts are larger for those with weekly benefit amount below the median. In most states, weekly benefit amount is closely related to previous year earnings; a fixed percentage, truncated above and below. Because earnings are moderately correlated over time, it is also not surprising that impacts are also larger for those with lower earnings two years earlier. Similarly, earnings are on average lower for younger workers, so it is not surprising that impacts are larger for that group.

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[77] The lack of relation for Wisconsin may be due in part to a combination of small overall impact and small sample size (Wisconsin's sample was considerably smaller than the other states'). Both factors make estimating differential impacts harder. In particular, estimating differential impacts requires very large samples.

Exhibit 6-4 Differential Impacts of NDNH Subgroups on UI Benefits (in weeks), *Existing* vs. *Control*

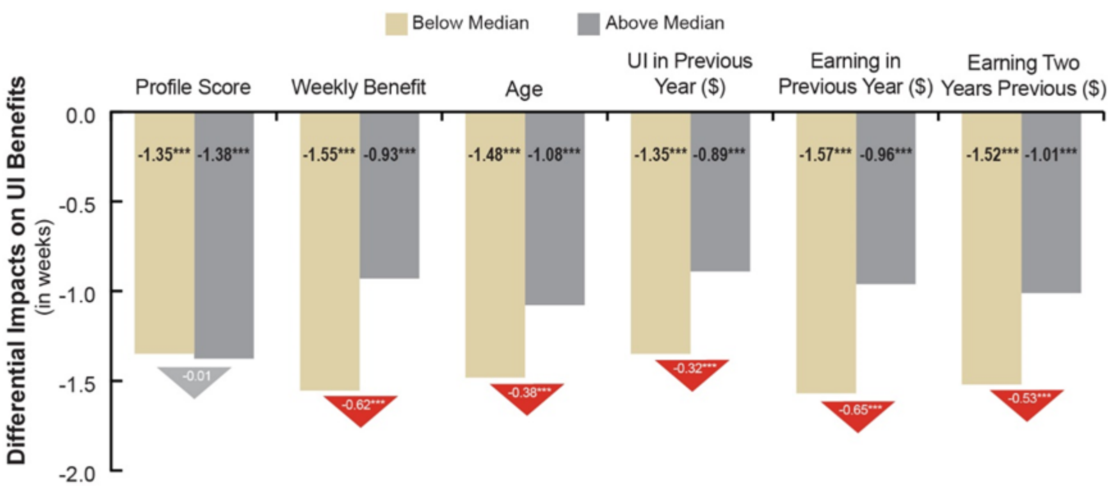
State	Impact	SE	Impact	SE	Impact	SE
UI in Previous Year (binary)						
	No UI		Received UI		Differential	
Pooled	-0.893***	0.099	-1.352***	0.051	0.317***	0.115
IN	−1.256***	0.399	−1.769***	0.125	0.514	0.419
NY	−1.164***	0.141	−1.503***	0.066	0.332**	0.156
WA	−0.634***	0.168	−0.867***	0.145	0.233	0.223
WI	−0.252	0.307	−0.562***	0.144	0.311	0.339
UI in Year Before Previous Year (binary)						
	No UI		Received UI		Differential	
Pooled	-1.031***	0.100	-1.324***	0.051	0.132	0.115
IN	−1.401***	0.341	−1.775***	0.127	0.374	0.365
NY	−1.442***	0.148	−1.450***	0.065	0.003	0.162
WA	−0.706***	0.172	−0.832***	0.144	0.125	0.226
WI	−0.142	0.283	−0.603***	0.147	0.461	0.320
Employment in Previous Year (in quarters)						
	Above Median		Below Median		Differential	
Pooled	-1.214***	0.053	-1.428***	0.088	0.170	0.105
IN	−2.017***	0.164	−1.380***	0.176	−0.638***	0.242
NY	−1.388***	0.068	−1.669***	0.128	0.277*	0.145
WA	−0.617***	0.131	−1.341***	0.210	0.725***	0.249
WI	−0.486***	0.148	−0.630**	0.267	0.144	0.306
Employment in Year Before Previous Year (in quarters)						
	Above Median		Below Median		Differential	
Pooled	-1.199***	0.053	-1.474***	0.089	0.328***	0.105
IN	−1.727***	0.131	−1.715***	0.294	−0.012	0.323
NY	−1.327***	0.070	−1.786***	0.116	0.454***	0.136
WA	−0.695***	0.130	−1.085***	0.218	0.390	0.255
WI	−0.525***	0.156	−0.494**	0.235	−0.032	0.283
Earnings in Previous Year ($)						
	Above Median		Below Median		Differential	
Pooled	-0.957***	0.064	-1.571***	0.064	0.646***	0.091
IN	−1.352***	0.165	−2.102***	0.174	0.750***	0.241
NY	−1.061***	0.084	−1.835***	0.085	0.772***	0.120
WA	−0.498***	0.165	−1.086***	0.152	0.588***	0.225
WI	−0.514***	0.190	−0.520***	0.179	0.006	0.262
Earnings in Year Before Previous Year ($)						
	Above Median		Below Median		Differential	
Pooled	-1.012***	0.064	-1.518***	0.064	0.535***	0.091
IN	−1.303***	0.166	−2.154***	0.173	0.850***	0.242
NY	−1.167***	0.084	−1.728***	0.085	0.560***	0.120
WA	−0.531***	0.165	−1.050***	0.152	0.518**	0.226
WI	−0.483**	0.190	−0.550***	0.180	0.067	0.262

Source: Regression-adjusted impact estimates based on state administrative data, Model(s): UWSW28ec_, Run Date: 29MAR2019
Note: Statistical significance levels for impacts are based on two-sided tests and flagged with asterisks, as follows: *** < 1 percent; ** < 5 percent; * < 10 percent.

We also considered differential impacts by local labor market conditions (see Appendix C in the appendix volume for results).[78] There is no evidence of differential impacts by any local labor market conditions. For Indiana, there is some evidence of larger impacts with better economic conditions: lower county unemployment rate, higher state employment-to-population ratio, higher county employment growth rate, lower county unemployment rate in the previous year, and higher covered employment in the month in which the benefit year began.

Exhibit 6-5 below presents a graphical representation of impacts (pooled only) of claimant characteristics and NDNH subgroups on UI benefits (in weeks), as presented in Exhibit 6-3 and Exhibit 6-4.

Exhibit 6-5 Differential Impacts of Select Claimant Characteristics and NDNH Subgroups on UI Benefits (in weeks), *Existing* vs. *Control,* (Pooled only)



Source: Regression-adjusted impact estimates based on state administrative data, Model(s): UWSW28ec, Run Date: 29MAR2019
Note: Statistical significance levels for impacts are based on two-sided tests and flagged with asterisks, as follows: *** < 1 percent; ** < 5 percent; * < 10 percent.

6.3 Pathways: Assistance vs. Enforcement

Having considered overall impacts and how they vary with claimant characteristics and local labor market conditions, in this section we consider *how* REA achieves its impacts.

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78 How we might expect impacts to vary with local labor market conditions is unclear. Perhaps impacts will be larger in a better labor market; in a weaker labor market there are no jobs to be found. Or perhaps impacts will be larger in a weaker labor market; in a better labor market claimants do not need assistance to find jobs. To provide evidence for these contrasting conjectures, Appendix C reports results of models of differential impacts with respect to local labor market characteristics. Local labor market characteristics we considered include unemployment rate, employment-to-population ratio, and employment growth rate. All local labor market characteristics are imputed for the county of the office to which the claimant was assigned (or for *Control*, would have been assigned) for REA.

Local economic conditions were specified as of randomization (month or calendar quarter, depending on the source). County measures were specified for the county in which the assigned office is located. Thus, variables measured at the state level capture only variation over time; variables measured at the county level capture variation both over time and over place.

The study's multi-armed randomization was specifically designed to estimate the separate impacts of enforcement and assistance. *Partial* was to receive all of the enforcement and none of the assistance; thus, the *Partial* vs. *Control* contrast would provide the impact of enforcement.[79] *Existing* (whether *Single* or *Multiple*) was to receive both enforcement and assistance; thus, the *Existing* vs. *Partial* contrast would give the incremental impact of assistance (above and beyond enforcement).

Accordingly, Exhibits 6-6 and 6-7 below summarize estimates of the separate impact of enforcement and assistance, respectively—inasmuch as the treatment conditions were implemented as designed.[80] **Results imply equal roles for enforcement and for assistance.**[81]

Relative to the pooled estimate for **enforcement** of −0.573 weeks (shown in Exhibit 6-6), there is strong evidence that the impact is larger in Indiana (−0.998) and smaller in Wisconsin (−0.156 and not statistically different from zero). Relative to the pooled estimate for **assistance** of −0.511 weeks (shown in Exhibit 6-7), there is strong evidence that the impact is larger for New York (−0.842) and smaller for Washington (−0.104), and there is weak evidence that the impact is larger for Indiana (−0.680).

Exhibit 6-6 Impact on UI Benefits Paid (in weeks), *Partial* vs. *Control*

State	*Control*	*Partial*	Impact	SE	Heterogeneity
Pooled	14.494	13.922	−0.573***	0.062	.035**
IN	14.624	13.625	−0.998***	0.150	<.01***
NY	15.313	14.793	−0.520***	0.108	.548
WA	14.133	13.442	−0.691***	0.116	.227
WI	13.617	13.461	−0.156	0.134	<.01***

Source: Regression-adjusted impact estimates based on state administrative data, Model(s): UWSW28pczz, Run Date: 29MAR2019
Note: Statistical significance levels for impacts are based on two-sided tests and flagged with asterisks, as follows: *** < 1 percent; ** < 5 percent; * < 10 percent. The pooled entry in the "Heterogeneity" column is the *p*-value for a test that all of the state impacts are equal; the other entries are *p*-values for a test that this state's impact equals the minimum variance combination of the other states' impacts.

79 See the discussion in Chapter 3, which argued that—at least in the four participating states—enforcement is primarily the procedural requirement to attend the REA meeting.

80 These are the results of separate models, so the two components will not sum exactly to the total from Exhibit 6-1. This is especially true in New York. In Exhibit 6-1, the sites with only *Existing* and *Control* are included in the overall estimates, but they are not included in the enforcement and assistance estimates (those sites did not have *Partial*, so the impact of enforcement and assistance cannot be separately estimated).

81 Formally, we cannot reject equal impacts for the pooled estimate. Furthermore, we cannot reject equal impacts for any of the states.

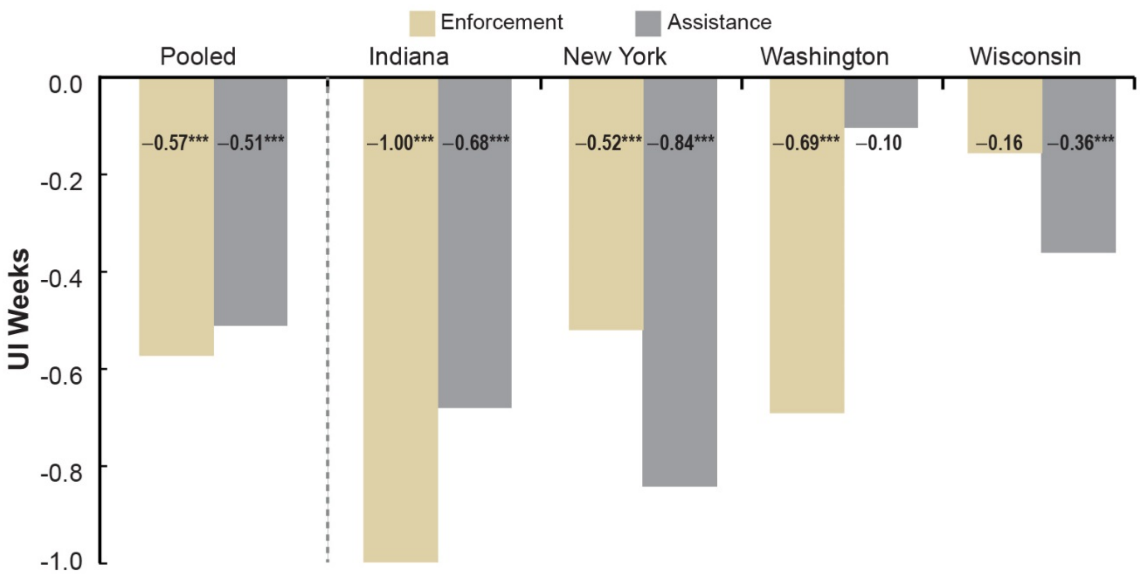
Exhibit 6-7 Impact on UI Benefits Paid (in weeks), *Existing* vs. *Partial*

State	*Partial*	*Existing*	Impact	SE	Heterogeneity
Pooled	13.891	13.379	−0.511***	0.058	<.01***
IN	13.625	12.945	−0.680***	0.117	.097*
NY	14.793	13.951	−0.842***	0.106	<.01***
WA	13.442	13.338	−0.104	0.111	<.01***
WI	13.461	13.100	−0.361***	0.134	.213

Source: Regression-adjusted impact estimates based on state administrative data, Model(s): UWSW28epzz, Run Date: 29MAR2019
Note: Statistical significance levels for impacts are based on two-sided tests and flagged with asterisks, as follows: *** < 1 percent; ** < 5 percent; * < 10 percent. The pooled entry in the "Heterogeneity" column is the *p*-value for a test that all of the state impacts are equal; the other entries are *p*-values for a test that this state's impact equals the minimum variance combination of the other states' impacts.

Exhibit 6-8 provides a graphical presentation of the impacts listed in Exhibit 6-6 (impacts through enforcement) and Exhibit 6-7 (assistance).

Exhibit 6-8 Impact on UI Benefits Paid (in weeks), *Enforcement* vs. *Assistance*



Source: Regression-adjusted impact estimates based on state administrative data, Model(s): UWSW28epzz, Run Date: 29MAR2019
Note: Statistical significance levels for impacts are based on two-sided tests and flagged with asterisks, as follows: *** < 1 percent; ** < 5 percent; * < 10 percent.

The timing of impacts is consistent with this interpretation of equal roles for both pathways—enforcement and assistance—and that the multi-armed random assignment design is capturing those separate pathways.

Exhibit 6-9 below plots impacts on (binary) UI receipt, by state, by week—overall, for enforcement, and for assistance. Timing is relative to randomization; the vertical line in each exhibit panel gives the median timing of the scheduled meeting (for Wisconsin, the last day on which the meeting could be held).[82]

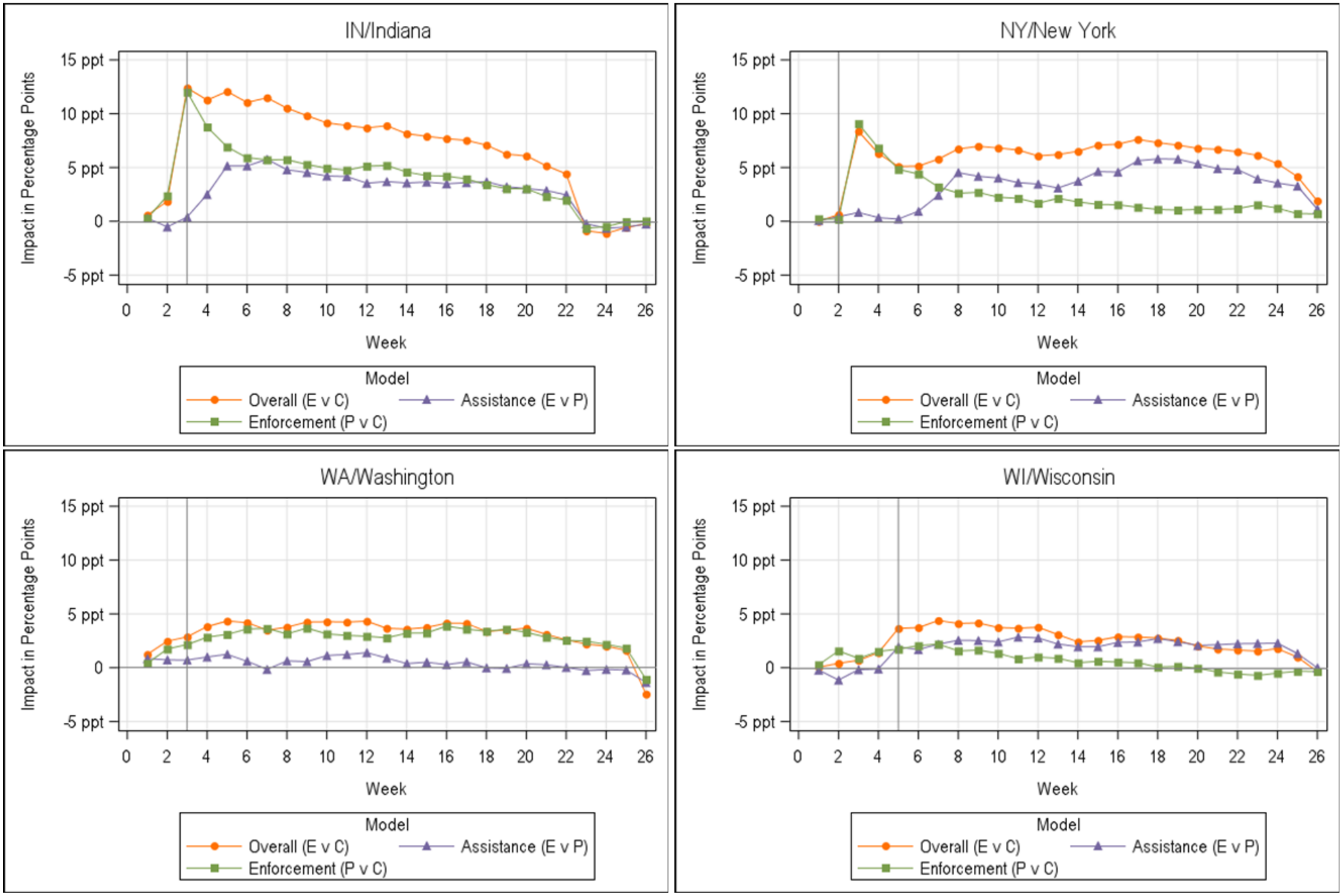
[82] The drops in impact at the very end of the period are likely a timing anomaly. We report results through 28 weeks post-*randomization*. For a claimant eligible for 26 weeks and who claims continuously, benefits would end 26 weeks post–first pay. First pay occurs a week after initial claim; randomization occurs a week after that. Thus, by week 20, even some

The timing of the impact of enforcement and assistance on receipt of UI benefits is consistent with our proposed theory of action (see Section 2.2) and our interpretation of the treatment conditions (see Sections 1.2.1 and 1.2.2).

claimants who were eligible for the full 26 weeks have already used all of their weeks. This would occur earlier for those who were eligible for less than 26 weeks (though, in our data, that group is not large). Once all benefits are used, impacts must be zero.

Furthermore, at the very end of the period the number of remaining claimants is small. This induces additional sampling variability. Finally, inasmuch as the treatment condition itself changes the timing of claiming, that will introduce additional variation. In particular, in the scenario where *Partial* claimants would have claimed continuously but *Existing* claimants leave UI earlier, impacts will drop to zero well before 28 weeks. This likely explains the sharp drop at the very end of the period plotted—past 20 weeks post-randomization.

Exhibit 6-9 Timing of Impacts on UI Benefits Paid: Overall, Assistance, and Enforcement



Specifically, as shown in Exhibit 6-9 above:

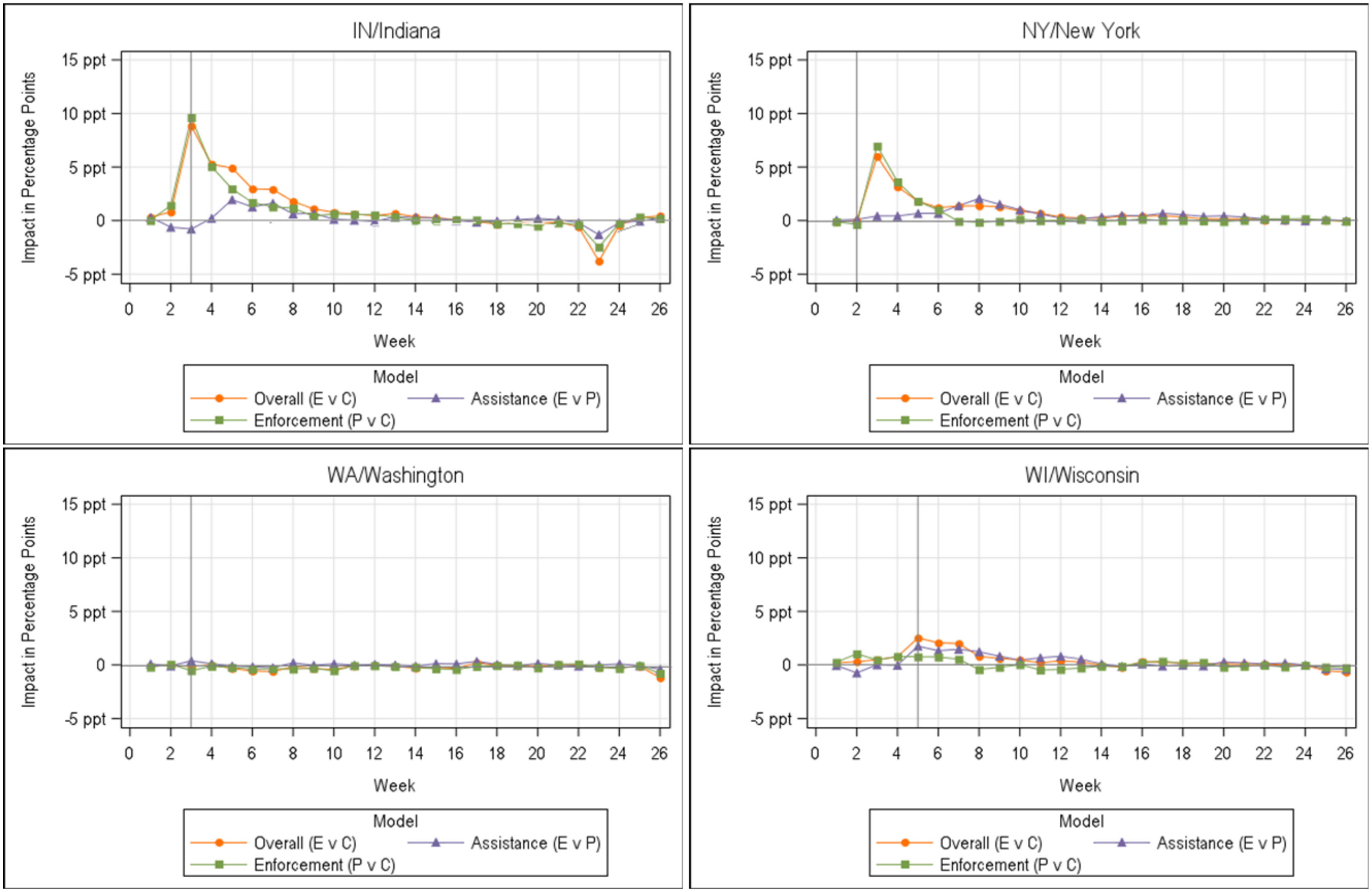
- For enforcement, estimated as *Partial* vs. *Control*:
  - There is little evidence of impact on benefits paid before the scheduled REA meeting. This is consistent with our argument that indirect effect of enforcement leading to claimants' exit before the REA meeting was implausible (see Section 2.2.2, "Indirect Effect of Enforcement").
  - In contrast, there is strong evidence of impact on UI benefits paid starting in the week of and the week after the scheduled REA meeting. This is consistent with these impacts being state response to initial non-attendance and ever non-attendance (more than two-fifths and about a third, respectively) (see the simulation in Exhibit 2-2 and the results presented in Exhibit 4-3).
  - Thereafter, the impact drops sharply. This is consistent with some delayed attendance among those claimants who do not attend the REA meeting when initially scheduled (again see Exhibit 2-2 and Exhibit 4-3).
  - Even many weeks later, the impact of enforcement does not disappear. This is consistent with some claimants never attending the REA meeting. Impacts drop over time because for claimants not selected for REA (i.e., *Control*), increasing shares of them would find a job and leave UI.
- For assistance, estimated as *Existing* vs. *Partial*:
  - There are no impacts until several weeks after the REA meeting. This is consistent with assistance requiring attendance at the meeting and then time after the meeting—to change job search strategies, for the changed job search strategy to yield an appropriate job offer, for the job to start—*before* an impact would appear (see Section 2.2.2, "Assistance"). This is a process that we would expect to take a month or more, with impacts continuing for months after that.

The timing of the impact of enforcement and assistance on Claimed Not Paid—meaning claimants who claim UI but do not receive a benefit due to some shortcoming in their eligibility—is also consistent with our proposed theory of action (again see Section 2.2) and our interpretation of the treatment conditions (again see Sections 1.2.1 and 1.2.2). Analogous to Exhibit 6-9 for impacts on (binary) UI receipt, Exhibit 6-10 below plots impacts on Claimed Not Paid by state, by week—overall, for enforcement, and for assistance.

As in the previous exhibit, in Exhibit 6-10 timing is relative to randomization; the vertical line in each panel of Exhibit 6-10 gives the median timing of the scheduled meeting (for Wisconsin, the last day on which the meeting could be held).

To see the relation to the theory of action and the interpretation of the treatment conditions, recall that Section 5.3 argued that Claimed Not Paid is a lower bound on the impact of the effect of enforcement on weeks of UI.

Exhibit 6-10 Timing of Impacts on Claimed Not Paid: Overall, Assistance, and Enforcement



Specifically, as shown in Exhibit 6-10 above:

- For enforcement, estimated as *Partial* vs. *Control*:
  - There is little evidence of impact on Claimed Not Paid before the scheduled REA meeting. This is consistent with no enforcement before the REA meeting.
  - In contrast, there is strong evidence of impact on Claimed Not Paid starting in the week of and the week after the scheduled REA meeting. This is consistent with these impacts being due to sizable initial non-attendance (again, see the simulation in Exhibit 2-2 and the results presented in Exhibit 4-3). Claimants who do not attend the required REA meeting continue to claim; in many cases, they are not paid.[83]
  - Thereafter, the impact drops sharply. This is consistent with sizable delayed attendance among those claimants who do not attend the REA meeting when initially scheduled (again, see Exhibit 2-2 and Exhibit 4-3). It is also consistent with those who choose not to attend eventually giving up and stopping claiming (before they would otherwise have left UI).
  - Unlike for UI benefits paid, the impact on Claimed Not Paid goes nearly to zero. This appears to be some combination of claimants coming into compliance, payments resuming after maximum weeks of suspension of benefits, and claimants stopping claiming.
- For assistance, estimated as *Existing* vs. *Partial*:
  - There are no impacts on Claimed Not Paid. This is consistent with the hypothesized causal pathway. Enforcement is to occur at the REA meeting. Both *Partial* and *Existing* have an REA meeting. It follows that there should be no incremental effect of assistance (above and beyond enforcement) on Claimed Not Paid.

6.4 Subsequent REA Meetings

Two states—New York and Washington—implemented both the *Single* and *Multiple* treatment condition, allowing the estimation of the differential impact of *Multiple* above and beyond *Single*.[84] Exhibit 6-11 below reports those results. The results differ sharply by state (and there is strong evidence that the impacts are truly different). In Washington, there is no evidence of an incremental impact of multiple

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[83] Details as to the maximum consequence for non-attendance and when that maximum consequence is applied vary across states; see discussion around Exhibit 4-3 and in the *Implementation Report* (Minzner et al., 2017).

Note also that Benus, et al. (2008) report the results of a multi-armed study of Multiple REA (up to four meetings) vs. Single REA vs. Control in Minnesota. Samples are not large: 544 Control, 3,038 for Single and 2,316 for Control. Likely as a result, the regression adjust estimates show find no differential of Multiple relative to Single of weeks claimed and compensated. The point estimate is a decline of 0.4 weeks. There also appear to have been some issues with randomization. The report states (p. 35): “While we believe that our efforts to create matching treatment and control groups have made the groups more comparable, further work is required to eliinate all potential differences between the groups.” Supportive of concern about randomization is the observation that simple differences of means are statistically significant (we would usually expect more precise from regression adjusted estimates).

[84] Wisconsin implemented *Multiple* but not *Single*, so it is not possible to estimate the incremental effect of subsequent meetings.

REA meetings. In contrast, in New York, there is strong evidence that the impact of *Multiple* is greater than the impact of *Single*.

Exhibit 6-11 Impact on UI Benefits Paid (in weeks), *Multiple* vs. *Single*

State	*Single*	*Multiple*	Impact	SE	Heterogeneity
Pooled	13.944	13.656	−0.288***	0.076	<.01***
NY	14.459	13.957	−0.501***	0.106	<.01***
WA	13.393	13.333	−0.060	0.110	<.01***

Source: Regression-adjusted impact estimates based on state administrative data, Model(s): UWSW28mszz, Run Date: 29MAR2019

Note: Statistical significance levels for impacts are based on two-sided tests and flagged with asterisks, as follows: *** < 1 percent; ** < 5 percent; * < 10 percent. The pooled entry in the "Heterogeneity" column is the *p*-value for a test that all of the state impacts are equal; the other entries are *p*-values for a test that this state's impact equals the minimum variance combination of the other states' impacts.

With estimates from only two states, cross-state inferences need to be made with care. Here we note that the larger incremental impact of *Multiple* (vs. *Single*) is consistent with the much more intensive implementation of *Multiple* described in Section 4.3. New York assigns second and third meetings to every claimant still on UI; Washington only assigns second and third meetings to claimants deemed likely to benefit. These diverging policies lead to different rates of scheduled second and third REA meetings, respectively: 72 percent and 48 percent for New York, but only 52 percent and 27 percent for Washington.

Furthermore, the multiple REA meetings explain about a third of the large impact in New York: −0.501 weeks out of the overall (*Existing*=*Multiple* vs. *Control*) impact of −1.452 weeks shown in Exhibit 6-1. In contrast, the difference in impacts for *Single* vs. *Control* between New York (−0.856) and Washington (−0.736) is much smaller and not statistically different from zero.[85] This comparison suggests an important role of subsequent in explaining the large impact of Existing in New York. Note, however, that the largest estimated impact of REA is for Indiana, which implemented only *Single*. So, subsequent meetings can not explain that large impact. There are no consistent patterns of differential subgroup impacts for *Multiple* vs. *Single*.

6.5 Longer-Term Impacts

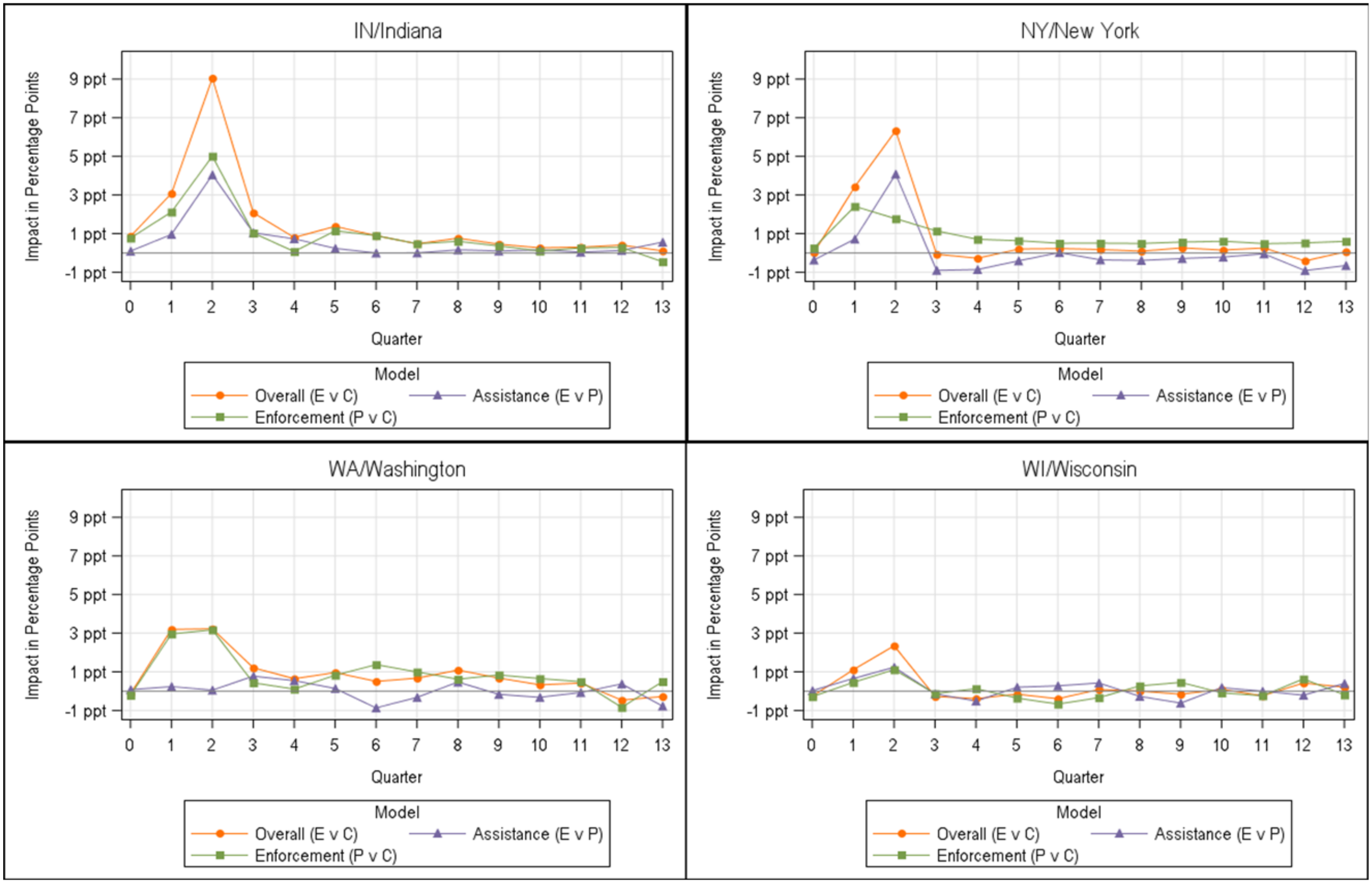
Almost all of the analysis in earlier sections of this chapter has focused on impacts through 28 weeks post-randomization.[86] Using NDNH data, Exhibit 6-12 below plots impacts on (binary) UI receipt, by state, by quarter—overall, for enforcement, and for assistance.[87] Timing is relative to initial UI claim (the initial UI claim occurs in Q0).

85 These estimates are from separate models (not elsewhere reported). The estimate is not identical to the estimate presented for New York in Exhibit 6-1. The estimate in Exhibit 6-1 is based on all New York offices (both New York two-arm and New York four-arm). *Single* was implemented only in some New York offices (New York four-arm). Here we compare *Multiple* vs. *Control* to *Single* vs. *Control* for the same offices (i.e., the New York four-arm offices).

86 Appendix C reports sensitivity analyses of longer-term impacts using state administrative data. That analysis suggests that there is little evidence that estimated impacts change when the follow-up period is extended from 28 to 52 weeks post-randomization. This suggests not a lot of impact past 28 weeks.

87 Results past Q9 should be interpreted with caution. The graphs are based on data for the entire sample through Q9. For Q10 to Q13, data are available for smaller and smaller fractions of the sample. Changes past Q9 are likely due to some combination of changes in sample and statistical noise due to small sample size.

Exhibit 6-12 Timing of Impacts on UI Receipt (binary), by Quarter Since Initial UI claim: Overall, Assistance, and Enforcement (from NDNH)



Very roughly, the 28 weeks post-randomization correspond to Q0 (the calendar quarter of the initial UI claim[88]), Q1 (the first full calendar quarter after the quarter of the initial UI claim), and Q2 (the second full calendar quarter after the quarter of the initial claim[89]).

There is no impact in Q0 (the quarter of randomization). There are, however, large impacts in Q1 and Q2 (the quarters in which a claimant would have received UI if claiming continuously for 26 weeks). There are small and sometimes individually statistically significant impacts thereafter, in Q3 to Q12; that is, **REA cuts UI receipt past the current benefit year**.

Furthermore, those long-term impacts are differential by pathway. There are no impacts for assistance (i.e., *Existing*=*Multiple* vs. *Partial*) past Q2. There are impacts for enforcement (i.e., *Partial* vs. *Control*) as late as Q9, more than two years after the initial UI claim.

Exhibits 6-13 and 6-14 provide detailed results for quarters of UI receipt; that is, the sum of impacts on receipt across two separate periods of four quarters: Q1 to Q4 (approximately the benefit year) and Q5 to Q8 (approximately the year after the benefit year).[90] Over Q1 to Q4, the pooled impact is about a tenth of a quarter. As in the state data on UI weeks, there is strong evidence of heterogeneity; impacts are largest for Indiana (−0.150 quarter), large for New York and Washington (−0.094 and −0.083 quarter), and smaller for Wisconsin (−0.028 quarter).

Exhibit 6-13 Impact on UI Over Q1 to Q4 (in quarters), *Existing* vs. *Control*

State	*Control*	*Existing*	Impact	SE	Heterogeneity
Pooled	1.669	1.576	−0.093***	0.005	<.01***
IN	1.632	1.482	−0.150***	0.013	<.01***
NY	1.691	1.597	−0.094***	0.006	.78
WA	1.724	1.641	−0.083***	0.013	.40
WI	1.549	1.521	−0.028**	0.014	<.01***

Source: Regression-adjusted impact estimates based on NDNH data, Model(s): UQNY01eczz, Run Date: 10MAY2019
Note: Statistical significance levels for impacts are based on two-sided tests and flagged with asterisks, as follows: *** < 1 percent; ** < 5 percent; * < 10 percent. The pooled entry in the "Heterogeneity" column is the *p*-value for a test that all of the state impacts are equal; the other entries are *p*-values for a test that this state's impact equals the minimum variance combination of the other states' impacts.

88 Though this quarter will usually include several pre-claim weeks.

89 Though this quarter will usually include several weeks beyond the 28-week follow-up period.

90 The range of this variable is 0 to 4. For a claimant who claims in none of the four quarters, value of this outcome is 0; for a claimant who claims in all of the quarters, the value is 4. If the fraction claimed in each quarter for *Existing* was 0.1 (that is 10 percentage points) more than for *Control*, then the impact would be 0.4 (=0.1 + 0.1 + 0.1 + 0.1).

For our analysis of receipt of UI, the main value of the NDNH data is the information on longer-term impacts; that is, impacts past Q4. Exhibit 6-14 presents impacts over Q5 to Q8. It provides clear evidence of longer-term impacts, though smaller than over Q1 to Q4: −0.015 quarter over Q5 to Q8 versus −0.093 quarter over Q1 to Q4 as seen in Exhibit 6-13. There is only weak evidence for heterogeneity, so we do not discuss the cross-state patterns.

Exhibit 6-14 Impact on UI Over Q5 to Q8 (in quarters), *Existing* vs. *Control*

State	*Control*	*Existing*	Impact	SE	Heterogeneity
Pooled	0.425	0.410	−0.015***	0.004	.06*
IN	0.260	0.225	−0.035***	0.009	.01**
NY	0.454	0.447	−0.007	0.006	.05*
WA	0.679	0.647	−0.032**	0.013	.15
WI	0.376	0.381	0.005	0.012	.09*

Source: Regression-adjusted impact estimates based on NDNH data, Model(s): UQNY02eczz, Run Date: 10MAY2019
Note: Statistical significance levels for impacts are based on two-sided tests and flagged with asterisks, as follows: *** < 1 percent; ** < 5 percent; * < 10 percent. The pooled entry in the "Heterogeneity" column is the *p*-value for a test that all of the state impacts are equal; the other entries are *p*-values for a test that this state's impact equals the minimum variance combination of the other states' impacts.

Exhibit 6-13 reported overall (*Existing* vs. *Control*) impacts on UI benefits over Q1 to Q4. Appendix C Exhibits C-6 to C-7 in the appendix volume report results by pathway—enforcement versus assistance—over Q1 to Q4. For this period, there is evidence of impacts of both enforcement (*Partial* vs. *Control*) and assistance (*Existing=Multiple vs. Partial*). The effect of enforcement is larger than the effect of assistance (and the difference is statistically significant). This contrasts with the results for UI weeks from state administrative data, where there was not a statistically significant difference between the impacts of assistance and enforcement. The difference may be due to differences in time period (early weeks of UI would fall in Q0 and are therefore not included; weeks after 28 are included in Q2 through Q4) and to units (weeks versus any receipt in a quarter). More study is warranted.

Exhibit 6-14 reported overall (*Existing* vs. *Control*) impacts on UI benefits over Q5 to Q8. Appendix C Exhibits C-8 to C-9 report results by pathway—enforcement versus assistance—over Q5 to Q8. For this period, there is evidence of an impact of enforcement, but not evidence of an impact of assistance (again, the difference is statistically significant). This finding is a little surprising. We might have expected longer-term effects to be due to assistance, on the assumption that the assistance led to more or better employment. NDNH estimates of impact on UI dollars are less precise than for UI quarters.

Overall, there are impacts on receipt of UI over Q1 to Q4, but not over Q5 to Q8. Over Q1 to Q4, there are impacts of both assistance and enforcement, but there is no clear evidence as to which pathway is more important.

6.6 Discussion

For estimating impacts on UI duration, our samples are large enough to support not only precise estimates of the impact of REA, but also insights into how REA has its impacts. This final section summarizes the main points of this chapter.

The main finding: REA modestly lowered short-term UI duration. The theory of job search assistance and the earlier literature are clear: job search assistance in general and REA in particular should lower UI duration and UI benefits paid. However, REA and REA-like programs do not attempt to improve occupational skills or to address deep barriers to work such as mental health issues, substance abuse, domestic violence, and homelessness. Instead, they are low-intensity programs—roughly $100 per claimant—that address job search. We should expect only small impacts. Detecting small impacts requires very large samples. The earlier studies that did not find an impact plausibly did not find an impact because their samples were too small to detect any small impacts that were present.

Consistent with that theory, earlier studies, and our large samples, our estimates imply that REA modestly lowers short-term UI duration in all four states studied. In magnitude, our findings for impacts on UI weeks—the pooled estimate—is 1.262 weeks. Most estimates in the literature are in the range of 0.5 to 1.5 weeks, so this pooled estimate is towards the upper end of those in the literature. The estimate, however, is smaller than the very large estimates for Nevada REA of 1.82 weeks. There is also evidence of smaller impacts over Q5 to Q8.

The magnitude of the impact varies across the four states. Samples in those earlier studies were sufficiently small that the ranging might simply have been due to random variation. That is not true here. We can unambiguously reject that the impact is identical in all four states. Impacts are about a week and a half in Indiana and New York, about half a week in Washington and Wisconsin. This heterogeneity in impacts carries through many of the results in this study. This report's final Chapter 8 considers the implications of this heterogeneity for policy.

The magnitude of the impact varies with claimant characteristics. The Office of Unemployment Insurance's REA guidance (UIPL No. 17-13)[91] had noted: "Many states have elected to serve claimants who are at a mid-range in the WPRS model selection. These individuals may benefit from participation in the REA program and are not likely to need long term and intensive services" (p. 5).

Previous studies have *not* had sufficient samples to explore the "may benefit" statement, and in particular whether there are differential impacts by claimant characteristics. This study *does* have sufficient samples. It finds that impacts are much larger (in absolute value) for claimants with a **lower weekly benefit amount**. Impacts are larger for claimants who are **younger** and **less educated**. There is a differential **profile score** effect for Indiana, but not for New York or Washington.[92] Impacts also are clearly larger for claimants with **weaker recent labor market experience**, in particular earnings in the previous two years. Our subgroup analyses use NDNH data. Those data are primarily based on state UI earnings data collected to administer the UI system (to assess taxes on employers and to determine benefits for claimants).

These results potentially have policy implications. When funds are insufficient to select all claimants for an REA-like program, states might want to select the claimants for whom the impact on weeks of UI will be largest (although there may be equity implications of such a strategy). The results presented here imply that selecting claimants based on the official profile score *would not* consistently advance that goal. In contrast to profile scores, the results presented here imply that selecting claimants with a lower weekly benefit amount, and lower recent earnings *would* advance that goal.

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91 *Fiscal year (FY) 2013 Unemployment Insurance (UI) Reemployment and Eligibility Assessment (REA) grants*. Retrieved from https://wdr.doleta.gov/directives/attach/UIPL/UIPL_17_13_Acc.pdf

92 We do not have an official profile score for Wisconsin.

This study does not find consistent evidence of differential impacts by local economic conditions. The lack of evidence for differential impacts by economic conditions is not surprising. Randomization only spanned a year (March 2015 through April 2016) and it was a period of mildly improving labor market conditions. Given that the models include AJC-specific intercepts, only changes within an AJC would matter. Other approaches with more variation in local economic conditions are likely more appropriate ways to explore these issues.

REA's impacts arise through both enforcement and assistance. As discussed in Section 2.4, pre-REA multi-armed studies appeared to show effects of enforcement but not assistance. The results here are more balanced, suggesting equal importance for both pathways.

How that balance comes to be is not necessarily straightforward, however. Note that the effect of the enforcement pathway appears to be mostly the procedural requirement to attend the REA meeting—not enforcement of ongoing eligibility requirements (e.g., "able and available," sufficiently intensive job search, has not refused a suitable job offer).

Note also that some of the effect of assistance is also properly viewed as the effect of the procedural requirement to attend the REA meeting—not the effect of job search assistance and referrals alone. This is true for two reasons.

First, in some states, the REA appointment letter signals how long the meeting will be; in some of those states, this appears to have led to lower attendance at longer *Single* or *Existing* meetings than at the shorter *Partial* meetings. Because our approach estimates the impact of assistance as *Existing* vs. *Partial*, where attendance was lower for *Single* or *Existing* than for *Partial* we credit the difference in outcomes to assistance. This is not truly an impact of assistance, rather it is lower attendance leading to larger impacts of the state's enforcement of the procedural requirement to attend the REA meeting. Second and in the same direction, in the three states where *Existing*=*Multiple*, there is additional non-attendance at subsequent REA meetings. Again, this is not truly an impact of assistance, rather it is the state's enforcement of the procedural requirement to attend subsequent REA meetings.

Previous studies (Black et al., 2003; Black, Smith, Berger, & Noel, 2007; see review in Filgres & Hansen, 2015) found evidence of a "threat effect"; that is, claimants stop claiming between notification and the date for which the first REA meeting was scheduled. We find little evidence of such a threat effect. This is consistent with the a priori analysis in Section 2.2.2 suggesting that for REA, there is little incentive to leave; instead, the optimal strategy is to claim and collect UI benefits until benefits are suspended.

Evidence is mixed on the impact of subsequent meetings. Finally, the REA program guidance allowed states the option of one REA meeting or up to three REA meetings. We are unaware of any previous research on the impact of this policy choice. For the two states that exercised that option, this study finds a large incremental impact of multiple meetings as implemented in New York (where large fractions of selected claimants are scheduled for subsequent REA meetings), but no such incremental impact of multiple meetings as implemented in Washington (where fewer claimants are scheduled for subsequent REA meetings). Stated policy and analyses of state administrative data (Exhibit 4-5) imply that the *Multiple*/*Single* differential in New York is much larger than in Washington.

Cross-state policy inferences must be tentative, however, especially with only two states. With that crucial caveat, these results suggest a possible incremental impact of requiring more subsequent REA meetings. In Washington, case managers only called in for subsequent meetings claimants whom the case managers viewed as needing additional assistance. These results suggest that case managers' judgement of who is in need of additional assistance may be problematic. Direct multi-armed exploration of this issue seems warranted. Required sample sizes would be large.

7. Impacts on Employment and Earnings

The REA program's assistance is intended to lead to faster reemployment and better job matches (i.e., jobs with higher earnings and that last longer). This chapter reports estimates of the impact of REA on employment and earnings.

Chapter 7 Key Findings

- REA increases employment and earnings in the short term (i.e., Q1-Q4).
- REA also increases employment in the longer term (i.e., past the benefit year), but the impacts are much smaller than over Q1-Q4.
- There is strong evidence of an effect of assistance on employment; there is weak evidence of an effect of enforcement. The estimates are not precise enough to conclude that the impact of assistance is greater than the impact of enforcement.
- There is strong evidence that REA increases job tenure.

Data. This chapter primarily uses NDNH quarterly data on earnings. The analysis codes an individual as employed in a quarter if he/she has positive earnings. The NDNH quarterly earnings data also include FEIN; we use that information to code job tenure in quarters.

Methods. Unless otherwise noted, the results presented in this chapter are random assignment estimates of the impact of *Existing* vs. *Control*, using our standard estimation methodology (see Appendix B in the appendix volume). When we discuss other contrasts (e.g., *Partial* vs. *Control*), they are explicitly identified. We note explicitly when impacts differ across the four states; otherwise, statistical tests suggest that differences observed across states are likely due to chance. Unless otherwise noted, there is no consistent evidence of differential subgroup impacts.

Balance of This Chapter. Section 7.1 considers impacts on employment and earnings. Section 7.2 considers differential subgroup impacts for employment and earnings. Section 7.3 considers impacts on job tenure. Section 7.4 considers impacts on time to reemployment. Section 7.5 provides a discussion of these results in the context of the theory of and broader literature on REA and REA-like programs.

7.1 Impacts on Employment and Earnings

There is strong evidence that **REA increases employment over Q1 to Q4**. Specifically, Exhibit 7-1 below reports the number of quarters employed (i.e., positive earnings recorded in the NDNH) in the first four full quarters after the initial UI claim (so the range is zero to four).[93,94] For pooled results, the impact is about a twentieth of a quarter (slightly less than a week), representing 2 percent of the control group mean. Although there is wide variation across states in the estimated impacts, a formal test provides no evidence of heterogeneity in impacts across the states. Although there is wide variation across states in the estimated impacts, a formal test provides no evidence of heterogeneity in impacts across the states.

[93] The quarter of the initial claim (Q0) is excluded; part of the fourth quarter (Q4) is past the end of the benefit year. Thus, Q1 to Q4 roughly approximates the benefit year, but not exactly. Q0 includes part of the benefit year; Q4 is partially after the end of the benefit year.

[94] For help reading this report's impact tables, see the discussion in Section 3.4.

Exhibit 7-1 Impact on Employment Over Q1 to Q4 (in quarters), *Existing* vs. *Control*

State	*Control*	*Existing*	Impact	SE	Heterogeneity
Pooled	2.734	2.791	0.056***	0.007	.71
IN	2.662	2.740	0.078***	0.019	.22
NY	2.650	2.713	0.063***	0.009	.25
WA	2.978	3.015	0.036**	0.018	.22
WI	2.914	2.938	0.024	0.021	.09*

Source: Regression-adjusted impact estimates based on NDNH data, Model(s): EQNY01eczz, Run Date: 10MAY2019
Note: Statistical significance levels for impacts are based on two-sided tests and flagged with asterisks, as follows: *** < 1 percent; ** < 5 percent; * < 10 percent. The pooled entry in the "Heterogeneity" column is the p-value for a test that all of the state impacts are equal; the other entries are p-values for a test that this state's impact equals the minimum variance combination of the other states' impacts.

Exhibit 7-2 presents the quarter-by-quarter detail. Impacts are largest in Q1 and Q2,[95] about 2 percentage points. Impacts fall rapidly thereafter, below 1 percentage point already in Q3 and at or below half a percentage point by Q6 (see below Exhibit 7-6).

Exhibit 7-2 Impact on Employment (binary), *Existing* vs. *Control*, by Quarters since Claim

State	*Control*	*Existing*	Impact	SE	Heterogeneity
Quarter of Initial Claim					
Pooled	0.871	0.875	0.005***	0.002	0.32
IN	0.812	0.807	-0.006	0.005	0.04**
NY	0.865	0.870	0.006***	0.002	0.34
WA	0.867	0.876	0.009**	0.004	0.28
WI	0.925	0.927	0.002	0.004	0.46
Quarter 1 after Initial Claim					
Pooled	0.587	0.606	0.019***	0.002	0.88
IN	0.572	0.587	0.015**	0.007	0.49
NY	0.564	0.585	0.021***	0.003	0.30
WA	0.663	0.682	0.019***	0.006	0.98
WI	0.621	0.634	0.013*	0.007	0.36
Quarter 2 after Initial Claim					
Pooled	0.674	0.694	0.020***	0.002	0.27
IN	0.639	0.665	0.026***	0.006	0.28
NY	0.647	0.671	0.024***	0.003	0.02**
WA	0.756	0.763	0.007	0.006	0.01**
WI	0.732	0.740	0.009	0.007	0.08*
Quarter 3 after Initial Claim					
Pooled	0.734	0.743	0.009***	0.002	0.59
IN	0.709	0.729	0.020***	0.006	0.05*
NY	0.714	0.724	0.010***	0.003	0.63
WA	0.789	0.793	0.004	0.005	0.28
WI	0.782	0.783	0.001	0.006	0.13
Quarter 4 after Initial Claim					
Pooled	0.741	0.748	0.008***	0.002	0.74
IN	0.742	0.759	0.016***	0.006	0.11
NY	0.725	0.732	0.007**	0.003	0.87

95 For someone claimant for 26 continuous weeks, this is the only quarter for which the individual can receive UI in every week. Q0 will almost always includes some weeks before the claim; Q2 will almost always include some weeks after all benefits have been claimed (if the individual claims continuously).

State	*Control*	*Existing*	Impact	SE	Heterogeneity
WA	0.770	0.776	0.006	0.006	0.71
WI	0.779	0.780	0.001	0.006	0.29

Source: Regression-adjusted impact estimates based on NDNH data, Model(s): EBNQ00eczz, EBNQ01eczz, EBNQ02eczz, EBNQ03eczz, EBNQ04eczz, Run Date: 10MAY2019 22MAY2019

Note: Statistical significance levels for impacts are based on two-sided tests and flagged with asterisks, as follows: *** < 1 percent; ** < 5 percent; * < 10 percent. The pooled entry in the "Heterogeneity" column is the *p*-value for a test that all of the state impacts are equal; the other entries are *p*-values for a test that this state's impact equals the minimum variance combination of the other states' impacts.

There is also evidence that **REA increases employment beyond Q4**. Exhibit 7-3 presents equivalent results for Q5 to Q8. Over that period, the impact is about a fourth of the impact over Q1 to Q4 (0.016 versus 0.056 quarters). Again, there is no evidence of heterogeneity in impacts across the states.

Exhibit 7-3 Impact on Employment Over Q5 to Q8 (in quarters), *Existing* vs. *Control*

State	*Control*	*Existing*	Impact	SE	Heterogeneity
Pooled	3.009	3.025	0.016**	0.007	.84
IN	3.061	3.084	0.023	0.020	.69
NY	2.955	2.978	0.023**	0.009	.22
WA	3.092	3.089	−0.003	0.019	.29
WI	3.122	3.115	−0.007	0.022	.25

Source: Regression-adjusted impact estimates based on NDNH data, Model(s): EQNY02eczz, Run Date: 10MAY2019

Note: Statistical significance levels for impacts are based on two-sided tests and flagged with asterisks, as follows: *** < 1 percent; ** < 5 percent; * < 10 percent. The pooled entry in the "Heterogeneity" column is the p-value for a test that all of the state impacts are equal; the other entries are p-values for a test that this state's impact equals the minimum variance combination of the other states' impacts.

There is strong evidence that **REA's assistance pathway increases employment over Q1 to Q4**, but only weak evidence that enforcement does (see Exhibits 7-4 and 7-5 below). However, the estimates are not precise enough to conclude that the impact of assistance is greater than that of enforcement.

Exhibit 7-4 Impact on Employment Over Q1 to Q4 (in quarters), *Existing* vs. *Partial*

State	*Partial*	*Existing*	Impact	SE	Heterogeneity
Pooled	2.798	2.832	0.034***	0.009	.58
IN	2.679	2.740	0.060***	0.016	.05**
NY	2.665	2.702	0.037**	0.016	.82
WA	3.011	3.015	0.004	0.017	.04**
WI	2.910	2.938	0.028	0.021	.77

Source: Regression-adjusted impact estimates based on NDNH data, Model(s): EQNY01epzz, Run Date: 10MAY2019

Note: Statistical significance levels for impacts are based on two-sided tests and flagged with asterisks, as follows: *** < 1 percent; ** < 5 percent; * < 10 percent. The pooled entry in the "Heterogeneity" column is the *p*-value for a test that all of the state impacts are equal; the other entries are *p*-values for a test that this state's impact equals the minimum variance combination of the other states' impacts.

Exhibit 7-5 Impact on Employment Over Q1 to Q4 (in quarters), *Partial* vs. *Control*

State	*Control*	*Partial*	Impact	SE	Heterogeneity
Pooled	2.796	2.812	0.016*	0.009	.75
IN	2.662	2.679	0.017	0.021	.95
NY	2.651	2.665	0.015	0.016	.91
WA	2.978	3.011	0.033*	0.018	.28
WI	2.914	2.910	-0.005	0.021	.26

Source: Regression-adjusted impact estimates based on NDNH data, Model(s): EQNY01pczz, Run Date: 10MAY2019

Note: Statistical significance levels for impacts are based on two-sided tests and flagged with asterisks, as follows: *** < 1 percent; ** < 5 percent; * < 10 percent. The pooled entry in the "Heterogeneity" column is the *p*-value for a test that all of the state impacts are equal; the other entries are *p*-values for a test that this state's impact equals the minimum variance combination of the other states' impacts.

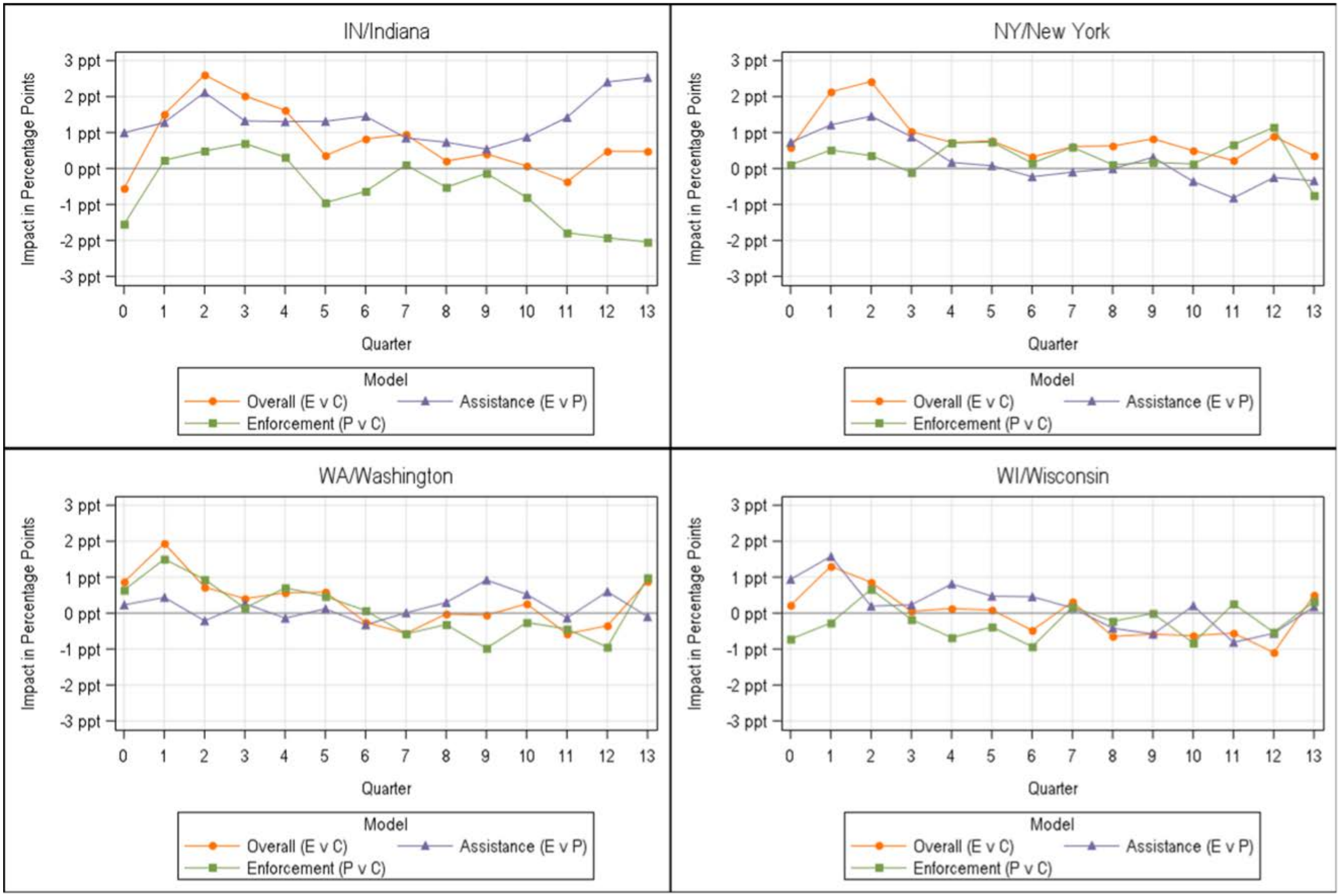
Impacts over Q5 to Q8 are so small that we cannot detect separate effects of either pathway. There is no evidence of differential impact for *Multiple* vs. *Single* in either time period.

Exhibit 7-6 below plots impacts on employment in percentage points, by quarter[96]—overall and separately for assistance and enforcement. Consistent with the results over Q1 to Q4 (see Exhibits 7-4 and 7-5), estimates are consistently larger for assistance, but we cannot rule out that the differences compared to enforcement are due to chance.

[96] In the graphs, the impacts are per quarter, so they have the interpretation of percentage points.

Results past Q9 should be interpreted with caution. The graphs are based on data for the entire sample through Q9. For Q10 to Q13, data are available for smaller and smaller fractions of the sample. Changes past Q9 are likely due to some combination of changes in sample and statistical noise due to small sample sizes.

Exhibit 7-6 Timing of Impacts on Employment (binary): Overall, Assistance, and Enforcement



Patterns of impacts for earnings are similar to those for employment. Exhibit 7-7 presents the results over Q1 to Q4. Relative to the estimates for employment, the estimates for earnings are less precise (the *t*-statistic for employment (pooled) was 8.0, but only 3.9 for earnings[97]). For pooled results, there is strong evidence of an impact over Q1 to Q4 of $465, which is approximately 2 percent of *Control* earnings over that period. There is no evidence of impact over Q5 to Q8.

Exhibit 7-7 Impact on Earnings Over Q1 to Q4 ($), *Existing* vs. *Control*

State	*Control*	*Existing*	Impact	SE	Heterogeneity
Pooled	$21,892	$22,357	$465***	$123	.89
IN	$18,797	$19,259	$461**	$232	.99
NY	$21,971	$22,545	$575***	$181	.41
WA	$28,466	$28,663	$196	$346	.41
WI	$21,932	$22,266	$333	$353	.69

Source: Regression-adjusted impact estimates based on NDNH data, Model(s): EDNY01eczz, Run Date: 10MAY2019
Note: Statistical significance levels for impacts are based on two-sided tests and flagged with asterisks, as follows: *** < 1 percent; ** < 5 percent; * < 10 percent. The pooled entry in the "Heterogeneity" column is the *p*-value for a test that all of the state impacts are equal; the other entries are *p*-values for a test that this state's impact equals the minimum variance combination of the other states' impacts.

This increase in earnings of $465 is clearly greater than direct program costs of about $100 per claimant selected. Though the evaluation did not include a formal cost-benefit analysis, the discussion in Section 6.1 suggested that total government cost per claimant selected is probably less than $300. The increase in earnings of $465 is greater than that.[98]

We can use this estimated impact on earnings to compute a rough estimate of how much of the decline in weeks of UI benefits paid is due to increased employment. If we take Q4 as a proxy for post-claim weekly earnings, then $465 is equivalent to about 0.7 weeks. This is about half of the impact on weeks of UI estimated from state administrative data of about 1.3 weeks (see Exhibit 6-1).[99] In net, it seems plausible to infer that about half of the decline in UI weeks is due to increased employment; the other half appears to be due to increased time during which claimants are not receiving UI and are not employed.

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[97] The *t*-statistic is the ratio of the estimated impact to its standard error. Larger *t*-statistics imply greater certainty that the estimate is different from zero. Conventional cutoffs are 2.56, 1.96, and 1.64 for two-tailed tests at p = .01, .05, and .10, respectively. Both 8.0 and 3.9 are substantially in excess of those cutoffs.

[98] Note that the implicit cost-benefit analysis is from society's perspective. The program costs are borne by the federal government and whoever ultimately pays the taxes to fund its programs. In contrast, the benefits accrue to the UI claimants selected for the program.

[99] The computation underlying this claim is as follows. Pooled Q4 *Control* quarterly earnings are $6,696. The corresponding employment rate (i.e., number of quarters in which any positive earnings were recorded) is about 0.75. So quarterly earnings among those claimants working at all during the quarter are about $8,928 (= $6,696 ÷ 0.75). Assuming that anyone who is employed in the quarter is employed in every week of the quarter, the implied weekly earnings are $687 (= $8,928 per quarter ÷ 13 weeks). At weekly earnings of $687, the benefit year impact on earnings of $481 is equivalent to 0.70 weeks. An impact on employment of 0.70 weeks would be 55 percent of the overall impact on UI weeks shown in Exhibit 6.1of 1.262 weeks.

Furthermore, note that this is likely an overestimate. It seems likely that many claimants who were employed in a quarter were not employed for every week of that quarter. This implies even higher weekly earnings and therefore fewer weeks of employment and a smaller share of the impact on UI weeks that is due to an increase in employment. Conversely, it would imply a larger share of the impact on UI weeks that is due to time not receiving UI but not employed.

Exhibit 7-8 presents the quarter-by-quarter detail. Patterns are similar to those for employment, peaking in Q2 and falling thereafter—both in dollars and as a percent of *Control* earnings (percent of *Control* earnings not shown in exhibit): Q1: $78, 2.0 percent; Q2 $207, 4.0 percent; Q3 $142, 2.2 percent; and Q4: $77, 1.1 percent. Impacts fall sharply thereafter (see below Exhibit 7-9).

Exhibit 7-8 Impact on Earnings by Quarter after Initial Claim ($), *Existing* vs. *Control*

State	*Control*	*Existing*	Impact	SE	Heterogeneity
Quarter of Initial Claim					
Pooled	$5,813	$5,831	$18	$44	0.18
IN	$5,156	$5,180	$24	$72	0.91
NY	$5,877	$5,879	$2	$62	0.71
WA	$8,695	$9,126	$431**	$213	0.05**
WI	$7,160	$6,993	$-167	$172	0.27
Quarter 1 after Initial Claim					
Pooled	$3,694	$3,772	$78**	$35	0.59
IN	$3,203	$3,204	$0	$64	0.14
NY	$3,644	$3,771	$128***	$49	0.14
WA	$5,220	$5,244	$25	$107	0.60
WI	$3,835	$3,975	$140	$145	0.66
Quarter 2 after Initial Claim					
Pooled	$5,221	$5,429	$207***	$38	0.20
IN	$4,498	$4,693	$195***	$73	0.85
NY	$5,280	$5,544	$264***	$52	0.12
WA	$7,339	$7,232	$-108	$155	0.04**
WI	$5,463	$5,620	$157	$98	0.58
Quarter 3 after Initial Claim					
Pooled	$6,323	$6,466	$143***	$44	0.98
IN	$5,285	$5,461	$175**	$79	0.62
NY	$6,404	$6,557	$152**	$74	0.87
WA	$8,155	$8,244	$89	$109	0.59
WI	$6,282	$6,397	$115	$108	0.78
Quarter 4 after Initial Claim					
Pooled	$6,655	$6,732	$77*	$46	0.59
IN	$5,810	$5,900	$90	$81	0.85
NY	$7,027	$7,109	$82	$83	0.95
WA	$7,751	$7,941	$190*	$105	0.23
WI	$6,351	$6,271	$-81	$111	0.12

Source: Regression-adjusted impact estimates based on NDNH data, Model(s): EDNQ01eczz, EDNQ02eczz, EDNQ03eczz, EDNQ04eczz, Run Date: 10MAY2019 22MAY2019

Note: Statistical significance levels for impacts are based on two-sided tests and flagged with asterisks, as follows: *** < 1 percent; ** < 5 percent; * < 10 percent. The pooled entry in the "Heterogeneity" column is the *p*-value for a test that all of the state impacts are equal; the other entries are *p*-values for a test that this state's impact equals the minimum variance combination of the other states' impacts.

Exhibit 7-9 below plots impacts on earnings, by quarter[100]—overall and separately for assistance and enforcement. Consistent with the Q1 to Q4 results (see Exhibits 7-4 and 7-5), estimates are consistently

[100] Results past Q9 should be interpreted with caution. The graphs in Exhibit 7-9 are based on data for the entire sample through Q9. For Q10 to Q13, data are available for smaller and smaller fractions of the sample. Changes past Q9 are likely due to some combination of changes in sample and statistical noise due to small sample sizes.

larger for assistance (detailed results not shown), but we cannot rule out that the differences compared to enforcement are due to chance.

Exhibit 7-9 Timing of Impacts on Earnings ($): Overall, Assistance, and Enforcement

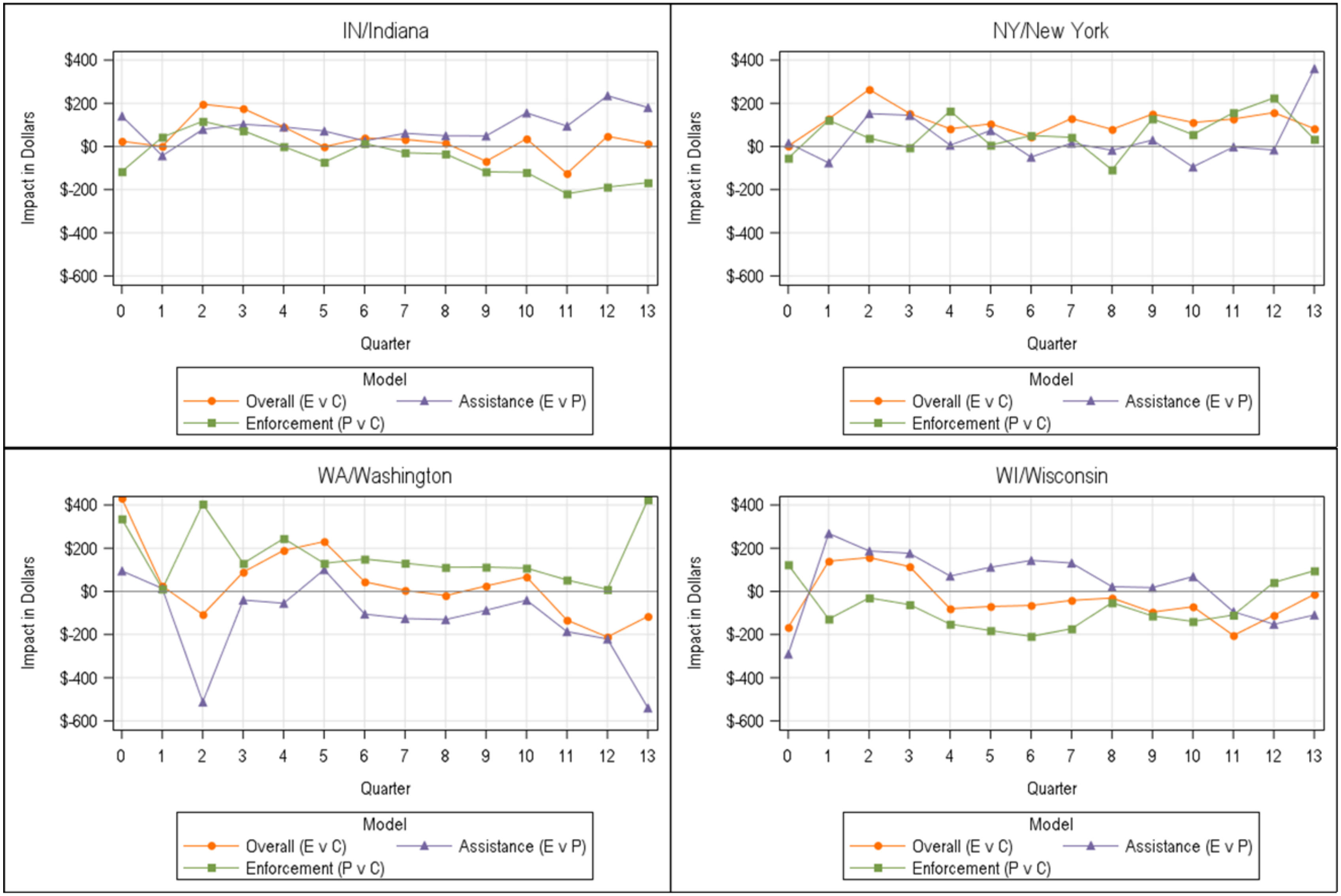
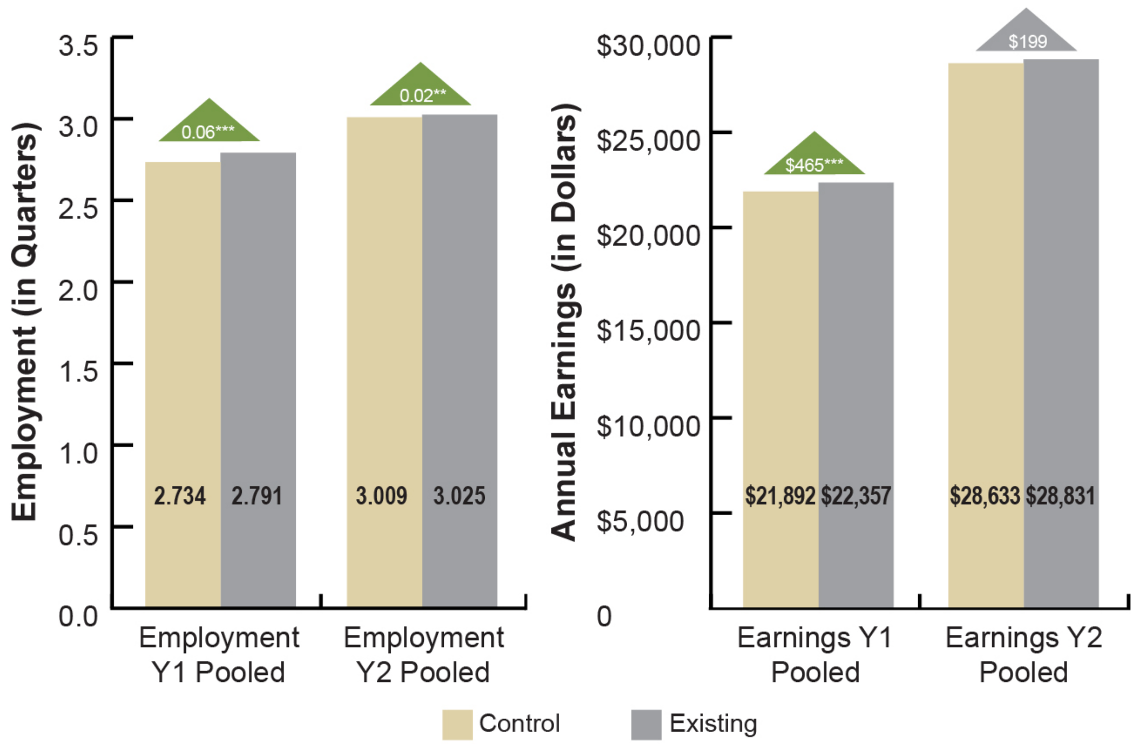


Exhibit 7-10 provides a graphical representation of pooled impacts on employment and earnings for *Existing* vs. *Control* in year 1 (Q1 to Q4) and year 2 (Q5 to Q8) after initial claim.

Exhibit 7-10 Employment and Earnings Impacts for Year 1 and Year 2, *Existing v. Control* (Pooled only)



Source: Regression-adjusted impact estimates based on NDNH data, Model(s): EQNY01eczz, EQNY02eczz, EDNY01eczz, EDNY02eczz, Run Date: 10MAY2019

Note: Statistical significance levels for impacts are based on two-sided tests and flagged with asterisks, as follows: *** < 1 percent; ** < 5 percent; * < 10 percent.

7.2 Differential Subgroup Impacts on Employment and Earnings

To explore differential impacts, we focus on the outcome for which impacts are most precisely estimated: employment. As in Section 6.2, the exhibits present only limited results; more detailed results are presented in Appendix C in the appendix volume.

There is even less evidence of differential impacts for employment over Q1 to Q4 than there was in Chapter 6 for weeks of UI over that same period. There is no evidence of differential impact for gender, ethnicity, education, weekly benefit amount, or official profile score. Impacts are larger for claimants who are older than 38 years old (the median age for the study sample) and are weakly larger for whites (relative to blacks), as shown in Exhibit 7-11 below.

Exhibit 7-11 Differential Impacts of Claimant Characteristics on Employment Over Q1 to Q4 (in quarters), for *Existing* vs. *Control*

State	Impact	SE	Impact	SE	Impact	SE
Gender						
	Female		Male		Differential	
Pooled	0.06***	0.01	0.05***	0.01	0.00	0.01
IN	0.09***	0.03	0.07***	0.02	0.02	0.04
NY	0.06***	0.01	0.07***	0.01	−0.01	0.02
WA	0.07**	0.03	0.02	0.02	0.05	0.04
WI	0.03	0.03	0.02	0.03	0.01	0.04
Age						
	Above Median		Below Median		Differential	
Pooled	0.06***	0.01	0.03**	0.01	0.04**	0.02
IN	0.08***	0.02	0.06	0.04	0.02	0.05
NY	0.07***	0.01	0.03*	0.02	0.05**	0.02
WA	0.04**	0.02	−0.01	0.04	0.05	0.05
WI	0.02	0.02	0.03	0.04	−0.01	0.05
Race						
	Black		Not Black		Differential	
Pooled	0.03	0.02	0.06***	0.01	−0.04*	0.02
IN	0.02	0.04	0.09***	0.02	−0.07	0.05
NY	0.04*	0.02	0.07***	0.01	−0.03	0.03
WA	0.00	0.07	0.04**	0.02	−0.04	0.07
WI	−0.00	0.04	0.03	0.02	−0.03	0.05
Ethnicity						
	Hispanic		Not Hispanic		Differential	
Pooled	0.05***	0.02	0.06***	0.01	−0.01	0.02
IN	0.17*	0.09	0.07***	0.02	0.09	0.09
NY	0.04*	0.02	0.07***	0.01	−0.03	0.03
WA	0.04	0.03	0.04*	0.02	−0.00	0.04
WI	0.11	0.08	0.02	0.02	0.09	0.08
College						
	Some		None		Differential	
Pooled	0.02	0.02	0.04**	0.02	−0.02	0.03
WA	0.04	0.03	0.03	0.02	0.01	0.04
WI	0.00	0.03	0.05	0.03	−0.05	0.04
Weekly Benefit						
	Above Median		Below Median		Differential	
Pooled	0.06***	0.01	0.05***	0.01	0.01	0.01
IN	0.06**	0.03	0.09***	0.03	−0.03	0.04
NY	0.08***	0.01	0.05***	0.01	0.03*	0.02
WA	0.03	0.03	0.05*	0.02	−0.02	0.04
WI	0.02	0.03	0.03	0.03	−0.00	0.04
Profile Score						
	Above Median		Below Median		Differential	
Pooled	0.07***	0.01	0.05***	0.01	0.02	0.02
IN	0.08***	0.03	0.08***	0.03	0.00	0.04
NY	0.07***	0.01	0.05***	0.01	0.02	0.02
WA	0.05*	0.03	0.02	0.02	0.03	0.04

Source: Regression−adjusted impact estimates based on NDNH data, Model(s): EQNY01ec, Run Date: 10MAY2019
Note: Statistical significance levels for impacts are based on two−sided tests and flagged with asterisks, as follows: *** < 1 percent; ** < 5 percent; * < 10 percent.

Exhibit 7-12 reports differential impacts for NDNH subgroups. The only differential NDNH impact in the pooled data is for UI in the previous year. Impacts are larger for claimants who received less UI benefits in the previous year or two years previous (relative to those who received more UI benefits). In contrast to the results for UI weeks shown in Exhibit 6-4, there are no differential impacts by recent employment or earnings.

Exhibit 7-12 Differential Impacts of NDNH Subgroups on Employment Over Q1 to Q4 (in quarters), *Existing* vs. *Control*

State	Impact	SE	Impact	SE	Impact	SE
UI in Previous Year (binary)						
	No UI		Received UI		Differential	
Pooled	0.016	0.015	0.065***	0.008	-0.045***	0.017
IN	−0.029	0.057	0.089***	0.020	−0.118*	0.061
NY	0.009	0.020	0.073***	0.010	−0.064***	0.023
WA	0.012	0.025	0.047**	0.023	−0.035	0.035
WI	0.099**	0.047	0.010	0.023	0.089*	0.053
UI in Year Before Previous Year (binary)						
	No UI		Received UI		Differential	
Pooled	0.036**	0.015	0.061***	0.008	-0.020	0.017
IN	−0.045	0.049	0.097***	0.020	−0.143***	0.053
NY	0.055***	0.021	0.064***	0.010	−0.008	0.024
WA	0.030	0.026	0.038	0.023	−0.008	0.035
WI	0.032	0.044	0.022	0.024	0.010	0.050
Employment in Previous Year (in quarters)						
	Above Median		Below Median		Differential	
Pooled	0.058***	0.008	0.049***	0.014	0.015	0.017
IN	0.114***	0.026	0.036	0.028	0.078**	0.038
NY	0.067***	0.010	0.048**	0.021	0.020	0.023
WA	0.024	0.020	0.074*	0.038	−0.050	0.043
WI	0.017	0.023	0.053	0.049	−0.035	0.054
Employment in Year Before Previous Year (in quarters)						
	Above Median		Below Median		Differential	
Pooled	0.055***	0.008	0.061***	0.015	-0.008	0.017
IN	0.076***	0.021	0.093*	0.048	−0.017	0.052
NY	0.067***	0.011	0.051***	0.019	0.016	0.022
WA	0.019	0.020	0.088**	0.038	−0.069	0.043
WI	0.013	0.024	0.053	0.040	−0.041	0.047
Earnings in Previous Year ($)						
	Above Median		Below Median		Differential	
Pooled	0.065***	0.010	0.047***	0.010	0.017	0.014
IN	0.075***	0.026	0.083***	0.027	−0.008	0.038
NY	0.075***	0.013	0.050***	0.013	0.025	0.019
WA	0.028	0.026	0.043*	0.025	−0.015	0.036
WI	0.047	0.030	0.000	0.029	0.046	0.042
Earnings in Year Before Previous Year ($)						
	Above Median		Below Median		Differential	
Pooled	0.055***	0.010	0.057***	0.010	-0.003	0.014
IN	0.057**	0.027	0.102***	0.027	−0.045	0.038
NY	0.071***	0.013	0.055***	0.013	0.016	0.019
WA	0.031	0.026	0.040	0.024	−0.009	0.036
WI	0.002	0.030	0.045	0.029	−0.043	0.042

Source: Regression-adjusted impact estimates based on NDNH data, Model(s): EQNY01ec_, Run Date: 29MAR2019

Note: Statistical significance levels for impacts are based on two-sided tests and flagged with asterisks, as follows: *** < 1 percent; ** < 5 percent; * < 10 percent.

There is no evidence of differential effects on employment or earnings by any local labor market characteristic. Appendix C in the appendix volume includes detailed results over Q1 to Q4 for employment, the most precisely estimated impact.

7.3 Impacts on Job Tenure

Job tenure is another measure of job match quality. Better matches last longer; poor matches break up quickly. REA hypothesizes two theories of action. First, assistance should lead to better job matches and thus longer job tenure. Second, and in the other direction, enforcement may make claimants more likely to accept a job offer, rather than continue searching. If so, accepting a job offer rather than continuing to search might lead to poorer job matches and thus shorter job tenure.

Exhibit 7-13 below reports estimates of the impact (*Existing* vs. *Control*) on the longest job tenure in quarters through Q8—the latest quarter post-randomization for which we have data on the entire sample.[101] There is strong evidence that **REA increases job tenure**, though the impact is small: about 0.05 quarters, compared to the mean of about 4.70 quarters (or about 1 percent).

Exhibit 7-13 Impact on Job Tenure (in quarters), *Existing* vs. *Control*

State	*Control*	*Existing*	Impact	SE	Heterogeneity
Pooled	4.668	4.715	0.047***	0.012	.80
IN	4.451	4.533	0.082**	0.032	.24
NY	4.590	4.645	0.055***	0.016	.45
WA	5.150	5.180	0.030	0.035	.60
WI	4.814	4.797	−0.017	0.036	.06*

Source: Regression-adjusted impact estimates based on NDNH data, Model(s): JBNQ08eczz, Run Date: 10MAY2019
Note: Statistical significance levels for impacts are based on two-sided tests and flagged with asterisks, as follows: *** < 1 percent; ** < 5 percent; * < 10 percent. The pooled entry in the "Heterogeneity" column is the *p*-value for a test that all of the state impacts are equal; the other entries are *p*-values for a test that this state's impact equals the minimum variance combination of the other states' impacts.

The impact of assistance on job tenure is strongly statistically significant; the impact of enforcement on job tenure is not statistically significant (results shown in Appendix C). The impact of assistance is more than twice as large as the impact of enforcement; nevertheless, the difference in the impacts is not statistically significant.

There is no evidence of differential impact of *Multiple* compared with *Single* or for subgroup impacts (by claimant characteristics or local labor market characteristics).

7.4 Impacts on Time to Reemployment

This section reports estimates of time to reemployment (in weeks) using NDNH data on time to reemployment (from employer-provided data on new hires). These results come with an important

[101] Formally, we code job tenure by identifying the FEIN associated with the highest earnings for a given quarter. For that employer, we count the number of continuous quarters of any earnings back to Q0 (but not earlier). Thus, a claimant with no earnings has tenure equal to 0. A claimant with no earnings in the previous quarter has tenure equal to 1. Note that we do not require that this FEIN have the highest earnings in previous quarters.

methodological caveat: there is overwhelming evidence that reporting of new hires is unreliable.[102] These data issues imply that these results should be interpreted with extreme caution. Consistent with this crucial caveat, our discussion is brief.

Exhibit 7-14 presents strong evidence of an impact on time to reemployment, but the estimated impact is small (−0.274 weeks relative to the *Control* mean of about 22 weeks, or about 1 percent).

Exhibit 7-14 Impact on Weeks to Employment (in weeks), *Existing* vs. *Control*

State	*Control*	*Existing*	Impact	SE	Heterogeneity
Pooled	22.206	21.931	−0.274***	0.043	.52
IN	19.980	19.532	−0.448***	0.126	.14
NY	23.053	22.778	−0.275***	0.054	.99
WA	22.415	22.115	−0.299***	0.115	.81
WI	18.731	18.728	−0.003	0.144	.05**

Source: Regression-adjusted impact estimates based on NDNH data, Model(s): DHNW99eczz, Run Date: 10MAY2019

Note: Statistical significance levels for impacts are based on two-sided tests and flagged with asterisks, as follows: *** < 1 percent; ** < 5 percent; * < 10 percent. The pooled entry in the "Heterogeneity" column is the *p*-value for a test that all of the state impacts are equal; the other entries are *p*-values for a test that this state's impact equals the minimum variance combination of the other states' impacts.

7.5 Discussion

Estimating impacts on employment and earnings appears to require much larger samples than does estimating impacts on UI weeks. Many of the earlier studies that estimate impacts on UI receipt, do not estimate impacts on employment and earnings. Many of the studies that do estimate impacts on employment and earnings appear to be underpowered. This study's samples are large enough to clearly demonstrate that REA improves employment and earnings. However, our samples do not appear to be large enough to be informative about how REA does so. As a result, our discussion here is much shorter than for the previous two chapters.

REA increases employment and earnings in the short-term (i.e., Q1 to Q4). The pooled impact is about one-twentieth of a quarter (less than one week) and slightly more than $500. ZZZ Impacts are largest in Q2, 2 percentage points on employment and $207 on earnings. These estimates are slightly larger than most of the earlier estimates for REA, but much smaller than those for Nevada REA.

REA increases longer-term employment (i.e., Q5 to Q8), but by much less. The impacts on employment in the longer-term are about a quarter of those in the short-term, but statistically significant. There are no statistically significant longer-term impacts on earnings.

There is no strong evidence on differential impacts for groups of claimants, nor on the pathways through which REA affects employment and earnings. The impacts of enforcement (i.e., *Partial* vs. *Control*) are statistically significant; the impact of assistance (i.e., *Existing* vs. *Control*) is not. However, the estimates are so imprecise that statistical tests do not provide evidence that the two pathways have different impacts, so the appropriate inference is no strong evidence of which pathway is more important. Similarly, there are no strong patterns in the subgroups.

[102] See the discussion in Section 3.3.2.

This lack of strong evidence is probably a statistical power issue. Estimating differential impacts and the role of pathways require much, much larger samples than estimating overall impacts. While our samples appear to be large enough to do so for UI duration, they appear not to be large enough to do so for employment or earnings. As a proxy for this, note that the pooled t-statistic on UI duration is 27. In contrast, the pooled t-statistic on first year employment is only 7. Both t-statistics are well above the conventional cutoff of 1.96. They thus both provide clear evidence of impact. Nevertheless, in practice, estimating differential impact and role of pathways appears to benefit from more precisely estimated overall impacts.

REA increases job tenure. However, the impact on job tenure—that is the longest number of successive quarters with a single employer over Q1 to Q8—is small, about a twentieth of a quarter.

NDNH data on timing of new hire unreliable. Comparison of NDNH new hires with new employer IDs in the wage data suggest that the new hire data is missing most new hires. Those data are therefore not useful.

In summary, there is clear evidence that REA improves short-term and—to a much lesser extent—longer term labor market outcomes. However, unlike the results for UI duration, even the REA Impact Study's very large samples do not appear to be large enough to estimate differential impact by subgroups or the role of pathway in achieving the overall impacts.

8. Discussion

Experts and stakeholders emphasize diverse, but largely complementary program goals for REA and REA-like programs. Some view such programs' primary goal as to reduce the UI caseload by "the prevention and detection of UI improper payments" (UIPL No. 10-14, p. 1) and thereby lower UI benefits and the taxes on employers that fund those benefits. For them, strict enforcement is an attractive strategy. In sharp contrast, other experts and stakeholders emphasize that the UI program is an earned right based on past employment, such that terminating benefits (i.e., strict enforcement) is appropriate only for the very most egregious noncompliance. They view REA and REA-like programs' primary goal as to address the "employment needs of UI claimants" (UIPL No. 10-14, p. 4) and thereby to speed return to work. Still others view such programs' primary goal more broadly as to improve job matches and longer-term employment and earnings.

The REA Impact Study aimed to estimate the impact of the REA program for multiple outcomes for UI claimants, how those impacts varied with claimant characteristics, and through what programmatic pathways those impacts were achieved. Towards those goals, the study implemented four-armed randomization of nearly 300,000 UI claimants in four states. This final chapter summarizes the study's findings, puts them in the context of the broader literature, and discusses their implications for policy and future evaluations.

Specifically, the next three sections summarize the findings for the key outcomes—UI duration, employment, and earnings (Section 8.1); how those impacts varied with claimant characteristics (Section 8.2); and how those impacts were achieved—reemployment assistance versus UI eligibility enforcement versus the requirement to attend the REA meeting (Section 8.3). The next two sections consider implications for policy (Section 8.4) and for the focus and design of future studies (Section 8.5). The final section provides concluding thoughts (Section 8.6).

8.1 REA's Impact on UI Duration, Employment, and Earnings

We begin with the overall results.

- **REA cuts duration of UI and benefits paid.**

Pooling the estimates across states, REA—that is, *Existing* vs. *Control*—cuts duration of UI and benefits paid. This was the study's pre-specified single confirmatory outcome. The findings confirm unequivocally that REA cuts UI duration.

- **REA cuts UI duration, but the estimates vary substantially among the four states, ranging from about one and a half weeks to about half a week.**

In each of the four states, there is clear evidence that REA cuts UI duration, but the true impacts clearly differ among the four states. The estimated impacts in Indiana and New York imply REA cuts UI duration by about one and a half weeks; in contrast, estimated impacts in Washington and Wisconsin are about half a week. This difference in impacts among states is statistically significant.

- **REA raises short-term employment and earnings. The magnitude is only a small percentage of earnings.**

Pooling estimates across all four states, REA raises employment and earnings in year 1, the four quarters after the initial UI claim. The impact on earnings is $465, about 2 percent of earnings for claimants not assigned to REA.

- **About half of the decrease in UI weeks due to an increase in employment; the other half is due to more time not receiving UI and not employed.**

Combining the estimated impacts on UI weeks and earnings with information on the level of employment and earnings, it seems plausible to infer that about half of the decline in UI weeks is increased employment; the other half is increased time during which claimants are not receiving UI and are not employed.

- **REA has small positive impacts on employment past the benefit year.**

REA has small impacts on UI receipt and earnings in the longer term (three or more quarters after randomization). Pooling estimates across states, REA has a small impact on employment in year 2 (that is, Q5 to Q8). This may be the result of offsetting effects. Some have postulated that the assistance would create better job matches—leading to higher quarterly earnings after reemployment and less return to UI. Conversely, others conjecture that the enforcement would truncate job search, leading to worse job matches—leading to lower quarterly earnings and more return to UI. The evaluation finds mixed results, some significant in each direction, but never large.

8.2 How Impacts Vary With Claimant Characteristics

The study's samples are of sufficient size to detect at least larger differential impacts across claimant baseline characteristics.

- **Predicted likelihood of exhaustion does not clearly or strongly relate to the impact of the REA program.**

The WPRS statute and the original (pre-2018) Reemployment Services & Eligibility Assessment (RESEA) statute both required targeting those claimants with the greatest probability of exhausting benefits (i.e., those with the highest profile scores). In addition, guidance from DOL's Office of Unemployment Insurance noted that many states were using a similar strategy for REA (see UIPL No. 17-13). A state might select UI claimants most likely to exhaust because they might be perceived as the neediest group. Alternatively, a state might select this group because it believed that they would have larger impacts (as would be true with an impact proportional to expected duration).

Differential impact by probability of exhaustion (i.e., profile score) is testable. The evaluation finds a mixed and weak relation between the predicted likelihood of exhaustion of benefits and the impact of the REA program. It follows that if the goal is to select for REA those UI claimants for whom the program will cause the largest decline in UI weeks, selecting claimants based on their profile score does not advance that goal.

- **REA has a larger impact on claimants with lower earnings in the year prior to the initial UI claim and in the year before that, as well as for those with lower weekly benefit amounts.**

The evaluation also explored differential impacts with respect to claimant baseline characteristics (including recent individual labor market experience) and aggregate local labor market conditions. Pooling across the states, impacts on UI duration are nearly three times larger for claimants with low earnings (below the median in the four quarters prior to the claim) as for those with high earnings (above the median). Similarly, impacts are larger for claimants with UI weekly benefit amount below the median and for younger claimants.

8.3 The Pathways Through Which REA Has Impacts

The study's multi-armed random assignment design was specifically intended to improve understanding of the pathways through which REA achieves its impacts. In particular, the *Partial* treatment condition was designed to include all of the enforcement (of the ongoing eligibility requirements, and the procedural requirement to attend the REA meeting), but none of the assistance. It would follow that comparing outcomes for *Partial* vs. *Control* estimates the impact of enforcement, and that comparing outcomes for *Existing* vs. *Partial* estimates the incremental impact of assistance above and beyond enforcement. Such studies of the impact of incremental additions to the program model require very large samples; the REA Impact Study's samples appear to be sufficiently large.

- **Both REA's enforcement and assistance have impacts on UI duration.**

Exploiting this multi-armed random assignment design, there is clear evidence for impacts on UI duration through both enforcement and assistance. When interpreted as envisioned when the study design was developed, the results suggest approximately equal impacts of assistance and enforcement. However, considering all the results together (as detailed in the next two findings), a larger impact of enforcement than assistance might be the better interpretation.

- **Little enforcement of job search requirements results from REA meetings, yielding little estimated impact.**

Qualitative field work conducted for the REA Implementation Study suggested that intensively enforcing job search requirements was not a focus of REA meetings. Complementary with that inference, estimated impacts of REA (*Partial* vs. *Control* or *Existing* vs. *Control*) on nonmonetary detection actions (i.e., being referred for adjudication to enforce compliance with "able and available," sufficiently intensive job search, and accepting suitable job offer) are small—a few percentage points. This result suggests that *Partial* vs. *Control* is probably not primarily estimating the impact of the enforcement of ongoing eligibility requirements.

- **REA's procedural requirement to attend the REA meeting leads to much of the estimated impact.**

Though *Partial* vs. *Control* was originally envisioned as estimating the impact of enforcement of ongoing eligibility requirements, in practice it incorporates two causal pathways—enforcement of UI eligibility and enforcement of the procedural requirement to attend the REA meeting. Further, it appears that enforcement of the procedural requirement explains most of the *Partial vs. Control* differential on UI duration and most of the overall impact of REA on UI duration.

Clearly, the stronger the enforcement of ongoing eligibility requirements, the smaller would be the relative importance of the procedural requirement to attend the REA meeting. Nevertheless, whenever noncompliance with the procedural requirement to attend the REA meeting leads to (near) uniform suspension of benefits until compliance, the impact of that policy is likely to be much larger than the impact of enforcement of other ongoing eligibility requirements.

This is true for two reasons. First, ongoing eligibility requirements can only be enforced for those who come to the REA meeting. Meeting rates are so far from universal that it would be hard for this pathway to be important. Second, suspension until compliance can lead to the loss of many weeks of UI. However, noncompliance with ongoing eligibility requirements appears to lead only to loss of the week of noncompliance, and even that week is available at the end of the claim. The ongoing eligibility requirement could only have a large impact on UI weeks if the maximum lost number of weeks was larger (e.g., if detected noncompliance led either to loss of multiple weeks or to a requirement to demonstrate sufficient search in every week for the balance of the claim).

Furthermore, these results suggest that earlier studies of interventions that required a meeting—whether providing enforcement or assistance—estimated the joint impact of the content of the meeting and the effect of the response to the claimant's non-attendance at the meeting. In those earlier studies the impact of the response to non-attendance at the meeting alone were likely often half a week or larger.

- **Relative to only a single REA meeting, multiple meetings can lead to substantial drops in UI duration.**

Two states, New York and Washington, randomly assigned claimants to both Single and Multiple. This provides a causal estimate of the impact of an REA program with the possibility of multiple meetings relative to an REA program with only a single meeting. In New York, multiple meetings explain about a third of the total impact; in Washington there is no incremental impact of multiple meetings.

It seems plausible that this difference is, at least in part, related to state program design. In New York nearly everyone still claiming UI is called in for subsequent meetings. In contrast, in Washington only those deemed by caseworkers as likely to benefit are called in. It may be that caseworker judgement here is imperfect; some of those not called in might benefit. Alternatively, the difference might be related to Washington's policy of not suspending benefits until compliance.

The value of caseworker discretion is testable using random assignment methods. Some claimants would be randomly assigned to always be scheduled for a subsequent meeting if still on UI. Other claimants would only be scheduled for a subsequent meeting if a caseworker deemed that claimant likely to benefit.

8.4 Implications for Future Research

This section considers implications of the study findings for future research in three areas: enforcement, assistance, and cost-benefit.

Enforcement. Earlier random assignment evaluation evidence implied that classic enforcement (e.g., requiring more employer contacts; monitoring job search activity closely; and implementing uniform and immediate suspension of benefits until compliance response to noncompliance with these requirement) would have large impacts on UI duration and perhaps on earnings. It is our sense that a strong form of classic enforcement was not implemented in any of this study's four participating states. Instead, REA staff saw their primary goal as to provide assistance. Accordingly, for the states in this study, REA induced relatively few additional ongoing eligibility issues, and those issues seem unlikely to have had a substantial effect on the overall number of UI weeks paid. Though the REA programs in the studied states did not include a strong classic enforcement component, conversations with state UI leadership in other states suggest that some states are doing so. In order to test the impacts of stronger enforcement, those programs might be worthy of rigorous evaluation.

Assistance. It is possible that more intensive assistance would have larger impacts on earnings. The REA evidence is not informative on that conjecture. Rigorous evaluation of a program with more intensive assistance also seems worthwhile.

Cost-Benefit. The study did not include a formal cost-benefit analysis. Both the decrease in UI benefits (about $347 per claimant selected) and the increase in earnings (about $470 per claimant selected) are clearly larger than the cost of the REA program (about $100 per claimant selected). Both figures are also plausibly, but not definitely, larger than the total cost of the program, where the "total cost of the program" includes additional costs incurred through provision of reemployment services by the Wagner-Peyser Act program and other DOL funded programs.

There are likely also some small cost savings from lower public benefit payments—Temporary Assistance for Needy Families (TANF), Supplemental Nutrition Program (SNAP), Medicaid, and other health insurance programs. There are likely some benefits from employee benefits such as paid leave (not captured in earnings).

More careful cost-benefit analyses seem worthwhile. In particular, it seems possible that even if the average REA program passes a cost-benefit test, that some programs—perhaps those with smaller impacts, perhaps those with higher costs—would not. In addition, this cost-benefit discussion ignores general equilibrium issues; that is, perhaps some or even much of the increase in employment and earnings is not a net increase. Instead, perhaps the increased employment and earnings of those assigned to the program came because jobs that would have gone to someone else now go to those selected for the program. Some recent cost-benefit analyses suggest that some programs that appear cost effective when general equilibrium issues are not considered do not appear cost effective once general equilibrium issues are considered (Lise, Seitz, & Smith, 2004; Crepon, Duflo, Gurgand, Rathelot, & Zamora, 2011; Toohey, 2017).

8.5 Implications for Methodology

This section considers implications of the study findings for methodology in four areas: random assignment, data quality, sample sizes, and impact heterogeneity.

Random Assignment. This study has shown that REA-like programs clearly lend themselves to random assignment designs, but formal evaluations are needed. The federal Office of Unemployment Insurance had encouraged states, as part of their ongoing REA programs, to randomly assign which UI claimants would receive REA. Had states done so, it would have allowed ongoing evaluation of impacts. We examined similar efforts in detail when designing random assignment for this evaluation. We concluded that state "random assignment" was not always true random assignment, and the resulting impact estimates were therefore not necessarily informative. Consequently, for this REA Impact Study we actively monitored random assignment and treatment fidelity throughout the evaluation's period of program implementation to ensure sufficient contrast was maintained between the various treatment arms.

As DOL looks forward, at the very least, more intensive evaluation technical assistance would be needed to ensure random assignment efforts produce informative results, and an external evaluator is probably required.

Data Quality. State administrative data on services delivered and responses to non-attendance and other eligibility issues appear to be imperfect. States record the data primarily for operational purposes. Data do not always appear to be of the quality that can support empirical analysis. Furthermore, understanding what the data meant often required considerable interaction with state staff.

Sample Sizes. This study has also shown that impacts are small—in the sense that required sample sizes are greater than ten thousand. Even those sample size estimates assume similar allocations to treatment and control conditions. Designs that assign only a small fraction (e.g., 20 percent) of claimants to the control group would require even larger total sample sizes. Furthermore, sample sizes required to detect impacts on employment and earnings appear to be considerably larger than those required to detect impacts on weeks of UI benefits.

Similarly, very large samples (and multi-armed randomization) are needed to address research questions that explore how a program achieves its impacts: *Comparing two program variants, which one works better? For which claimants—that is, for claimants with which characteristics at the initial UI claim—are impacts larger?*

Together, these considerations suggest that many states do not have enough UI claimants to support state-specific estimates—at least in a single year. This problem of sample sizes will be particularly salient when the labor market is robust, leading to only small flows into UI and to quick reemployment.

Impact Heterogeneity. This study provides clear evidence that REA impacts are truly heterogeneous across states. That is, impacts vary more than can be explained by simple sampling variability. Cross-state analyses suggest an important role for the details of REA meeting scheduling (self-scheduling yields smaller impacts) and response to non-attendance (larger responses yield larger impacts). Many other (unmeasured) factors likely also vary in ways that shift true impact (e.g., the details of the services). Clearly, more research is needed to fully understand the sources of this heterogeneity. That additional analysis likely involves some combination of (a) more three-armed designs (i.e., comparing different active treatment conditions to each other and to control); (b) simple two-armed designs (i.e., treatment versus control) with many more states; (c) and two-armed designs with large enough samples to support exploring how impacts vary with claimant characteristics.

Furthermore, this heterogeneity of impacts implies that careful thought needs to be given to how to best generalize findings from one state to another. Experience to date suggests several possible approaches—varying in their attractiveness and their cost—to extrapolating impact from state to state given heterogeneity in true impacts.

1. Some of the earlier evidence-based literature has deemed it sufficient that a program has been demonstrated effective in some single site, or at least not demonstrated ineffective in this site. This approach seems unattractive. Our results suggest that just because a program has impacts in one state does not mean that it will also have impacts in another state. To some extent the lack of consistency in results is simply sampling variability. However, **our estimates clearly show that there is heterogeneity in REA impacts—above and beyond what can be explained by pure sampling variability**.

2. DOL could take a proper sample of sites/states and construct a pooled estimate. This was the strategy in DOL's WIA and Job Corps evaluations (McConnell et al., 2016; Schochet, Burghardt, & McConnell, 2008). Such estimates from a proper sample of sites/states could then use this pooled estimate to represent sites/states for which there is no evidence, or for which there is not clear evidence that this site differs from the pooled estimate. Empirical Bayes shrinkage methods formalize this intuition (see Klerman, 2017). These ideas are more effective when the number of states is larger. The four states used in this study were not sufficient to apply these ideas.

3. Complementarily, the approach in the teen pregnancy literature has been to require that every site—even those implementing demonstrated-effective programs—run a new random assignment evaluation in every period. As noted above, this is likely to require at the very least substantial evaluation technical assistance and probably an external evaluator.

4. Alternatively, perhaps the heterogeneity in impacts is due—at least in part—to heterogeneity in the programs as implemented. Federal guidance defining REA gave states considerable discretion in designing and implementing their programs. More precisely specifying each program model would be expected to lead to more homogeneous estimates such that DOL could plausibly extrapolate estimated impacts from one state to another.

The underlying technical problem of policy extrapolation in the face of impact heterogeneity is itself an open methodological research question. The results in this *Final Report* provide a start towards data on which to explore such methodological issues, but only a start.

8.6 Concluding Discussion

The results presented in this document provide considerable new information about the impact of the REA program—on various outcomes, on variation in those impacts with claimant characteristics, and on the pathways through which those impacts occur. These results were only possible because of multi-armed random assignment to program variants (with close monitoring for random assignment and treatment fidelity), considerable evaluation technical assistance to the states, very large samples, and a pure administrative data strategy (i.e., with no survey). Each of these factors clearly is reproducible in other studies of REA-like programs and more broadly.

As to causal pathways, the evidence from this study shows large impacts—perhaps three-quarters of a week—from the response to non-attendance at the REA meeting. Denial of benefits for noncompliance with the ongoing eligibility requirements also have the potential to substantially cut weeks of UI benefits. Enforcement of compliance with ongoing eligibility requirements were not tested in this evaluation. Finally, this study's results imply that, above and beyond the effect of enforcement and the requirement to attend the REA meeting, assistance cuts weeks of UI benefits—perhaps by another quarter of a week or more.

The previous paragraph has considered impacts on weeks of UI benefits paid. Impacts on employment and earnings are less precisely estimated, so there is little clear evidence on the relative importance of pathways. Apparently, this is in part because the impacts are smaller and require much larger samples to estimate precisely. The very large samples of this study are only borderline large enough. With that crucial caveat, impacts on earnings seem to imply that only about half of the decrease in weeks of UI benefits paid is due to increase in employment. The other half of the impact appears to be more time spent neither on UI nor employed.

The study provides only limited information on the differential impact of assistance versus enforcement on employment and earnings. Even with about 100,000 observations across *Control*/*Partial*/*Existing* treatment conditions, the estimates are simply not precise enough.

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