Barriers to Benefits: Unemployment Insurance Take-Up and Labor Market Effects*

Casey McQuillan
Princeton University

Brendan Moore
Stanford University
(*Job Market Paper*)

January 20, 2026

Abstract

Unemployment insurance (UI) take-up is relatively low in the United States. We implement a large-scale field experiment among 50,000 likely unemployed individuals to study the causes and labor supply implications of incomplete UI take-up. Informational letters increased applications and receipt, with effects concentrated among low-wage workers. Rejection rates among treated applicants also increased: this suggests that the letters primarily reduced learning costs rather than improved eligibility beliefs. Randomized messages aimed at reducing free-rider stigma induced more applications, primarily among high-wage job seekers. Although prior work finds that more generous UI slows job finding, our take-up intervention modestly increased reemployment, as work-search requirements hastened job finding among recipients but also screened out applicants who were unwilling or unable to verify their search. We develop and estimate a structural job search model calibrated to the reduced form-experimental results to quantify these frictions and show that lower search-compliance costs yield the largest welfare gains for unemployed workers.

*We are grateful to our advisors, Isaac Sorkin, Alessandra Voena, Nick Bloom, and Caroline Hoxby, for their feedback, guidance, and support. We also thank Matt Brown, Mihai Codreanu, José Ignacio Cuesta, Mark Duggan, Peter Ganong, Ilyana Kuziemko, Dave Lee, Felipe Lobel, Neale Mahoney, Rob Metcalfe, Petra Persson, Luigi Pistaferri, Brendan Price, Geoff Schnorr, and seminar participants at Stanford, Princeton, and the W.E. Upjohn Institute for Employment Research for their helpful comments and feedback. We are thankful to the Washington State Employment Security Department (ESD), and in particular to Olga Kondratjeva, Matthew Klein, Gustavo Aviles, and Tunde Akingbemi, for their professional support and expertise. Amishi Gupta provided excellent research assistance. Moore acknowledges support from the George P. Shultz Dissertation Support Fund via SIEPR, IRiSS at Stanford University, the Washington Center for Equitable Growth, and the Stanford Center for American Democracy. This experiment was pre-registered on the AEA RCT Registry (AEARCTR-0013608). This research was conducted while both authors were employees for Washington ESD. The authors gratefully acknowledge support for this project from the U.S. Department of Labor (DOL). The opinions, views, and conclusions or recommendations expressed are those of the authors and do not necessarily represent those of ESD, or the broader Government of Washington State. This workforce product was funded by a grant awarded by the U.S. DOL's Employment Training Administration. The product was created by the recipient and does not necessarily reflect the official position of DOL/ETA. DOL/ETA makes no guarantees, warranties, or assurances of any kind, express or implied, with respect to such information, including any information on linked sites and including, but not limited to, accuracy of the information or its completeness, timeliness, usefulness, adequacy, continued availability, or ownership.

Many social insurance and public benefit programs exhibit incomplete take-up, meaning not all eligible individuals claim the benefits for which they are eligible (Currie, 2004). Take-up of unemployment insurance (UI), the primary policy tool for insuring workers after job losses, is puzzlingly low: only about half of eligible jobless workers in the United States apply for and claim benefits (Auray, Fuller, and Lkhagvasuren, 2019; Forsythe, 2021; Lachowska, Sorkin, and Woodbury, 2025). In 2023, the average UI claimant received more than $7,000 during their unemployment spell, implying that non-claimants potentially forfeit substantial sums to which they are legally entitled (U.S. Department of Labor, 2023).

Standard models of unemployment assume that all eligible jobless workers automatically receive UI and treat benefits as a universally available outside option. If, in reality, many forgo benefits, the absence of an outside option may meaningfully alter job search behavior, affecting how quickly workers find reemployment and the types of jobs they accept. However, both the underlying causes of incomplete UI take-up and its consequences for job search remain understudied in the economics literature.

This paper examines incomplete UI take-up both experimentally and theoretically. We ask three main questions: (1) why do some eligible workers not claim UI, (2) which workers are more likely to receive benefits when frictions to claiming are reduced, and (3) what are the implications of increased take-up on job search behavior and worker welfare?

To understand the reasons behind low take-up of UI and its implications for job search behavior, we implement a large-scale, pre-registered information provision field experiment among 50,000 recently unemployed workers in Washington State by targeting letters mailed to potential claimants that provide information about UI benefits. We partnered with the Washington State Employment Security Department (ESD), the state agency that administers UI, to construct a study population of individuals who have not applied for UI but are likely, though not certainly, eligible. Informational letters on agency letterhead described the program's purpose, eligibility criteria, and application instructions. Several messages were cross-randomized on top of this baseline content. Half of the letters included a randomized destigmatizing treatment that emphasized that benefits are earned through work as part of a social insurance system. Half of the letters also randomly included messages aimed at setting more realistic job finding expectations among job seekers, who are often overoptimistic about reemployment prospects (Spinnewijn, 2015), thereby increasing the perceived value of UI benefits. To track the effects of the intervention on take-up and job search, we use Washington's administrative data on UI applications, benefit duration, and hours worked.

We find that informational letters significantly increase UI take-up among our treated sample. Among those who had not applied for UI at the time of mailing, we find that the receipt of informational letters nearly doubles the application rate relative to a status quo control group two months after the letters are sent. Receipt of letters raises UI applications by 1.4 percentage points (85%) relative to the control group. Treatment also raises UI receipt (i.e., whether an applicant is approved for benefits) by 0.4 percentage points (42%) relative to the control group. Treatment effects for application and receipt of benefits persist for the six post-treatment months covered in our data. Treatment is more likely to boost UI receipt among low-wage workers, despite higher baseline take-up among high-wage workers.

Because the letters simultaneously inform workers about UI eligibility and reduce the fixed cost of learning how to apply, we rely on the treatment's effect on the application rejection rate to distinguish between these mechanisms. We develop a partial equilibrium job search model in which the take-up decision depends on a worker's perceived probability of approval and the fixed learning cost of applying for UI. According to the model, a falling rejection rate among applicants suggests that informational letters increase take-up primarily by correcting eligibility misperceptions (i.e., by inducing eligible workers who mistakenly doubted their eligibility to apply). In contrast, a rising rejection rate means that the intervention drew in more ineligible applicants on the margin, implying that reduced learning costs were the primary driver. In the experiment, the rejection rate among treated applicants rises, suggesting that increased take-up operates primarily through reduced learning costs for both eligible but also ineligible applicants.

While reduced learning costs appear to be the primary driver of increased take-up, our experiment also shows a meaningful role for stigma. Specifically, we randomize messages aimed at reducing "free-rider stigma" — that is, guilt associated with claiming UI without visible contribution or demonstrated need (Friedrichsen et al., 2018) — across letters that otherwise contain identical information. The destigmatization treatment raises application rates by 0.4 percentage points ($p = 0.09$), or about one-third of the effect of providing generic information. The effect of destigmatization is negligible for workers from lower-wage jobs but statistically significant among higher-wage individuals. This pattern is consistent with the idea that higher-wage workers are less likely to view themselves as typical beneficiaries of public programs and may feel greater shame or discomfort in claiming UI, even when eligible. To support this interpretation, we field a complementary national online survey of unemployed non-claimants and find that baseline stigmatized attitudes toward UI are more prevalent among higher-wage respondents.

We further show that a cross-randomized message aimed at setting more realistic job finding expectations has no effect on applications. Evidence from our online survey of unemployed non-claimants suggests these messages did not shift workers' beliefs about their own job finding prospects. Without changes in perceived search duration, the relative value of UI benefits remained unchanged, and application behavior was unaffected.

With regard to labor supply, inducing workers to claim UI benefits via a randomized letter does not prolong the job search process. A vast literature estimates a negative effect of UI benefit generosity on reemployment. Cohen and Ganong (2024) perform a meta-analysis and document an average publication-bias corrected elasticity search duration with respect to benefit level of 0.34. We would thus expect an even larger negative effect of UI on search for an even larger increase in workers' UI benefits, such as in our experiment. Instead, we estimate statistically significant, albeit modest, *positive* and significant effects of treatment on stable reemployment in the quarters following job loss. The 95% confidence intervals rule out reductions of 0.25 percentage points in the probability of any reemployment. Those induced to receive UI by the letter (i.e., "complier recipients") exhibit shorter claiming durations than those in the control group; this suggests that easing frictions to claiming does not encourage disproportionately long searchers to apply. We detect no significant effects of receiving a letter on reemployment earnings or hourly wages conditional on working, despite the fact that low-wage workers' outside option shifts from zero to a relatively generous 55% UI benefit replacement rate. Altogether, these labor supply results suggest that the intervention had no negative fiscal externalities.

We argue that the absence of disemployment effects stems from the dual role of UI work search requirements. Many complier applicants in the data ultimately fail to receive benefits because they do not complete the required search verification, yet those who do receive benefits return to work faster than always taker recipients. These patterns suggest that search requirements both exclude a share of induced applicants and accelerate reemployment among those who comply. We rule out alternative institutional explanations for faster job finding, such as increased access to public employment offices or job search assistance, which are not mediating the positive reemployment effects.

To interpret these findings and quantify the underlying drivers of incomplete take-up, we develop and estimate a job search model in which workers decide whether to apply for UI and search for a job in the presence of potential rejection and psychological costs while receiving benefits. The model formalizes the dual role of search requirements by incorporating both a flow utility cost of compliance, which leads some otherwise eligible claimants to opt out of the application process, and differential job offer rates, where claimants subject to requirements receive offers more frequently. Structural estimation suggests that the letters meaningfully reduced worker's fixed learning costs of UI by \$584, or 18%. The imposition of work search requirements is costly to job seekers, at about \$200 per week, but it raises the claimants' job offer arrival rate by 3% relative to nonclaimants.

We use the model to calculate that the outreach experiment generated modest net welfare gains. Specifically, the combined benefits to inframarginal applicants — in other words, applicants who would have applied anyway and who incur fewer learning costs (and, if approved, fewer stigma costs) — and approved marginal applicants exceed the costs borne by marginal applicants who are induced to apply but are denied. In dollar terms, 60% of gross gains accrue to approved marginal applicants, with the remainder accruing to inframarginals. However, many of these gross gains are offset because 30% of treated applicants are induced but rejected. Our baseline estimate suggests that the intervention delivered a total welfare gain to workers of \$59,000, with robustness checks across belief parameterizations yielding gains between \$18,000 and \$490,000, well below the \$1.3 million in UI transfers delivered to compliers. Economic welfare gains fall short of accounting gains because of frictions incurred while on UI and the losses associated with denied applications.

The intervention was cost effective from the perspective of the state workforce agency. Administrative costs, which included printing, postage, and marginal call center staffing, totaled \$41,000. The field experiment's recruitment costs were \$70 per new applicant and \$220 per new recipient, slightly higher than in comparable public benefit outreach efforts (Finkelstein and Notowidigdo, 2019). However, these recruitment costs likely understate the possible welfare gains from a more targeted implementation focused on workers with lower rejection rates.

Lastly, we use the model to simulate a range of counterfactual policies that reduce learning, stigma, and search compliance costs to assess which policy design delivers the largest welfare improvements. Across these simulations, lowering search compliance costs generates the largest welfare gains, as a larger share of the unemployed population is able to comply with less onerous work search requirements that thereby raise the flow value of claiming. The reduction of stigma also delivers substantial welfare improvements, with only slightly smaller gains than the reduction of search costs, to achieve a comparable level of UI receipt. In contrast, the reduction of learning costs generates more modest welfare gains for a given increase in UI take-up, since the reduction in fixed costs delivers a one-time boost for applicants.

Welfare responses to these counterfactuals are not uniformly monotone. While initial reductions in frictions raise welfare by expanding access among eligible workers, more aggressive reductions can lower welfare by disproportionately inducing applications from workers who are ineligible or unable to comply with work search requirements. Because workers differ in eligibility beliefs and compliance costs, marginal reductions in frictions yield increasing rejection rates, leading to administrative processing costs without corresponding benefit receipt. As a result, welfare gains flatten and can reverse once most eligible workers have already claimed UI. This dynamic implies that the welfare consequences of reducing claiming frictions depend not only on how much take-up rises, but also on which workers are induced to apply for benefits.

Contributions to Literature

Our paper contributes to a large empirical literature on the incomplete take-up of public benefits that has found substantial gaps between eligibility and enrollment across a range of programs.[1] Explanations center on informational barriers and eligibility misperceptions, administrative burdens, and stigma surrounding benefit receipt (Bhargava and Manoli, 2015; Finkelstein and Notowidigdo, 2019; Bitler, Cook, Horn, and Seegert, 2022; Giannella, Homonoff, Rino, and Somerville, 2024). As a review by Ko and Moffitt (2024) emphasizes, the lack of take-up often reflects a combination of these frictions as well as rational inattention or a perception that benefits are too small to justify fixed application costs. Two field experiments in Europe highlight these issues for the unemployed in narrow, targeted programs of means-tested assistance (Castell, Gurgand, Imbert, and Tochev, 2025) and wage insurance for older workers (Stephan, van den Berg, and Homrighausen, 2016). Our paper is the first to leverage a field experiment to examine the mechanisms behind the incomplete take-up of UI, which differs fundamentally from other benefit programs. UI is a time-limited social insurance program financed by payroll taxes and requires weekly recertification. These institutional features likely affect how behavioral frictions influence take-up of UI benefits and warrant an analysis that is distinct from studies of long-term means-tested programs.

Incomplete take-up of UI has likewise been documented (Blank and Card, 1991; Anderson and Meyer, 1997; Trenkle, 2023; Kuka and Stuart, 2025), yet far less is known about why eligible workers fail to claim. Existing studies often point to forces outside workers' own decision-making. For example, Lachowska, Sorkin, and Woodbury (2025) show that some firms deter claims by challenging applications, while Kroft (2008) argues that take-up is shaped by local social interactions, with higher community participation spurring more individual claims. Ebenstein and Stange (2010) study the introduction of phone- and Internet-based claiming and do not find effects of lower transaction costs on take-up, while Hertel-Fernandez and Wenger (2013) show in a survey experiment that providing information about eligibility and generosity *reduced* stated interest in claiming. Several macro-labor studies model or document the aggregate implications of incomplete take-up, such as unemployment dynamics or the composition of claimants across the business cycle (Chodorow-Reich and Karabarbounis, 2016; Birinci and See, 2023). Our paper identifies and evaluates the individual-level frictions that influence UI take-up.

We also contribute to a vast literature on the welfare effects of UI policies, studying either its labor supply costs or consumption-smoothing benefits (Katz and Meyer, 1990; Gruber, 1997; Kroft and Notowidigdo, 2016; Johnston and Mas, 2018; Ganong and Noel, 2019; Landais and Spinnewijn, 2021; Karahan, Mitman, and Moore, 2025). This literature focuses almost exclusively on the implications of expanding UI generosity. Jäger, Schoefer, Young, and Zweimüller (2020) finds that more generous UI (holding institutional settings fixed) does not meaningfully pass

[1] Iselin, Mackay, and Unrath (2023) study incomplete take-up of the Earned Income Tax Credit (EITC); Moynihan, Herd, and Harvey (2015) study Medicaid; Deshpande and Li (2019) study Social Security Disability Insurance (SSDI); Hemmeter, Phillips, Safran, and Wilson (2025) study Supplemental Security Income (SSI); and Anders and Rafkin (2022) study the Supplemental Nutrition Assistance Program (SNAP).

through to reemployment wages. In contrast, we examine the employment and welfare effects of easing claiming frictions by inducing UI access on the extensive margin.

The remainder of the paper proceeds as follows. Section 1 describes the institutional setting and data from Washington State. Section 2 outlines how we design the information provision randomized controlled trial. Section 3 documents treatment's reduced-form effect on UI take-up. Section 4 presents a search model that endogenizes the UI take-up decision as a function of various frictions to claiming. Section 5 characterizes the mechanisms that drive increased UI take-up among our experimental sample. Section 6 documents how higher UI take-up induced via the letter impacts labor supply. Section 7 estimates the aforementioned search model calibrated to moments from the field experiment to quantify frictions to UI claiming and policy implications, and section 8 concludes.

1 Institutional Setting and Data

To understand the reasons underlying low UI take-up and its implications for job search, we study the UI system in Washington, a large, demographically and industrially diverse state. We access the state's administrative data on hours worked, UI applications, and other relevant public programs.

1.1 Unemployment Insurance in Washington state

Unemployment benefits partially replace jobless workers' earnings for a limited time while they search for new jobs. In all U.S. states, UI applicants are considered eligible if three conditions are met. First, workers must be "separation eligible," meaning they must be unemployed through no fault of their own. Separation eligibility disallows instances of firings for cause and individual decisions to leave employment. Second, workers must be "monetarily eligible," meaning they must have worked enough in the previous year to qualify for benefits. In order to qualify for UI, Washington requires at least 680 work hours during the base year (i.e., the first four of the last five completed calendar quarters prior to applying), which is equivalent to 17 full-time weeks or about 13 hours per week over a year.[2] Third, workers must actively search for a job each week and verify three search activities with the state workforce agency.[3] Denials for failing to certify work search typically occur when claimants either report insufficient job search activity or fail to

[2]If an applicant did not work at least 680 hours in their base year, ESD assesses eligibility according to the last four completed quarters as an "alternate base year."

[3]Washington's work-search requirement of three activities per week places it in the middle of the distribution across states in terms of stringency. By contrast, its 680-hour monetary eligibility threshold is among the most stringent. McQuillan and Moore (2025a) further discuss external validity for the monetary condition in Washington.

respond to agency inquiry. Washington also imposes a one-week waiting period before benefits begin, during which claimants must search for work without compensation to demonstrate they are actively seeking employment.

Eligible unemployed workers must actively apply with the state workforce agency which administers UI. In Washington, claimants apply with the Employment Security Department. They must apply online or by phone. As part of the application, they provide basic information (e.g., name, date of birth, contact information), work history from the past 18 months (e.g., contact information for past employers, dates of employment), and the nature of their separation. Initial applications often take 30 minutes or less to complete, unless a worker has recent military or federal employment.[4]

Workers can apply for UI benefits at any point during their unemployment spell and are not required to apply immediately after separation. However, approved benefits only begin starting the week the claim is filed — they are not retroactive to the week of separation.

Applicants may backdate their claims by up to two weeks without documenting job search activities. To backdate further, they must demonstrate "good cause" for not applying earlier and provide proof of job search activities in prior weeks.[5] Outreach from ESD, such as an informational mailer, does not qualify as good cause. Applicants who are reemployed when they file a claim and cannot establish good cause for backdating are denied for filing too late.

UI benefits typically replace 50% of a worker's previous weekly wage for up to 26 weeks. Benefits scale positively with previous earnings and are subject to a minimum and maximum cap. At the time of the experiment, the weekly benefit in Washington ranged from $323 to $1,019, making the system among the most generous in the country.

1.2 Administrative Data

Employer-Employee Matched Wage Records

Our main administrative data source is the employee-employer matched wage and hours records. ESD is legally mandated to record earnings for all UI-covered employees on a quarterly basis. Washington is unique because it also collects hours worked.[6] The records include worker- and employer-level identifiers; earnings and hours are therefore observed at each individual worker-

[4]Such applicants may be covered under Unemployment Compensation for Ex-Servicemembers (UCX) or Unemployment Compensation for Federal Employees (UCFE), which are federal programs administered by states. Claims involving military or federal work are more complex because ESD must verify wages and service history with the respective federal agencies before determining eligibility.

[5]According to Wash. Admin. Code § 192-110-095, "good cause" includes circumstances beyond the applicant's control, such as serious illness, natural disaster, or employer-provided misinformation. Once the good cause condition is no longer applicable, the applicant must file their claim within seven days.

[6]Washington is the only state to use hours directly for monetary eligibility. As a result, Lachowska, Mas, and Woodbury (2022) document the high degree of reliability in the hours measure of Washington's administrative data.

firm pair. The availability of hours data confers three main benefits. First, it allows us to analyze our data by hourly wage, rather than just total earnings. Second, it enables direct observation of monetary eligibility.[7] Third, it facilitates a more precise measurement of potential job loss.[8]

Use of the employer-employee matched records incurs three important limitations. First, the records lack reasons for separation or the labor market status of the nonemployed. As a result, separation eligibility is unobservable: layoffs, quits, and firings may look identical in the data. Second, they lack a measure of job search effort; we can therefore only rely on search verification information for the subset of workers receiving UI. Third, although the wage records include employer industry (based on six-digit North American Industry Classification System [NAICS] codes), they lack detailed worker-level information such as demographics and mailing address.

UI Claims

ESD maintains UI claims records for every applicant in Washington dating back to 2005. These records update on a weekly basis. For each claim, we observe the submission date of the initial application, the effective start date, and subsequent compensation records, which indicate whether a claimant received payment in a given week. For paid claims, we observe both the weekly benefit amount in dollars and the number of payments received; this proxies for the duration of UI receipt.

We define a denied (or rejected) initial claim as a UI application submitted without associated compensation. For most denied claims, we observe the reason for rejection as determined by ESD. Because a single claim can be denied for multiple reasons, we cannot assign rejections uniquely to causes. Instead, we report the share of denials citing each reason.[9]

We also access records for UI claimants who receive services from one of Washington's 40 WorkSource offices (listed in Appendix Table D.3). These are the state's public employment offices that provide reemployment services, such as job search assistance, resume creation and development, career-related workshops, and personalized support from staff to connect workers with employers. WorkSource is described in further detail in Appendix D.5.

Claims records also feature a richer set of worker-level characteristics. For each claimant, they include age, sex, race, ethnicity, education, disability status, veteran status, reported date of separation from the employer, and mailing address of the claimant at the time of application.

[7] We likely understate the number of monetarily eligible workers because we are only able to observe in-state working hours and cannot observe out-of-state hours, which still count towards audited monetary eligibility.

[8] Hours are preferable to earnings to define job loss because accrued leave time and fringe benefits are paid out at the end of job spells; this implies that the use of earnings confounds the measured timing of a job loss.

[9] Some share of rejections (i.e., applications without compensation) cannot be matched to the database on reasons for denial. ESD staff have informed the authors that such "unclassified" denials are often due to the fact that applicants file an initial claim but do not respond to follow-up inquiries from ESD for further information.

Department of Licensing (WADOL)

The UI claims records allow us to conduct outreach to previous UI claimants, a group who is more likely to know about the existence of UI and its eligibility rules. To expand the scope of the intervention beyond previous claimants, ESD entered a data sharing agreement with the Washington State Department of Licensing (WADOL), the state agency that issues driver's licenses. WADOL records include the most recent listed mailing address of every Washington resident with a valid license as of March 2024,[10] along with the license holder's full name, last four digits of their Social Security Number (SSN), sex, date of birth, disability status, and veteran status. We merge WADOL records with ESD wage records to obtain mailing addresses for those who lack a mailing address in ESD's internal database because they have never applied for unemployment insurance before. For details on the matching process, see Appendix C.1.

For workers with two addresses on file — one from WADOL and one from the UI claims — we opt to use the driver's license record for outreach for two reasons.[11] First, drivers' license addresses were typically more recent at the time of the experiment and are therefore more reliable. Second, because a key variable in our analysis is a worker's previous interaction with the UI system (Lemieux and MacLeod, 2000), we want to ensure that differences in this measure are not influenced by attrition patterns linked to address sources. Our use of WADOL addresses helps prevent this issue, as UI claims records inherently align with prior UI experience.[12]

Other program benefit records

We also use records from Washington's Paid Family and Medical Leave (PFML) system and the Washington State Department of Labor & Industries (L&I), which oversees distribution of Workers' Compensation. PFML application records are managed by ESD while Workers' Compensation records for those with open claims in May 2024 were procured via a data sharing agreement with L&I. Both datasets are used to more accurately target outreach, as described in the following section.

2 Information Provision Field Experiment

This section describes the field experiment involving informational letters mailed to tens of thousands of workers who likely experienced a UI-eligible job loss but did not claim benefits. Letters advertised the program's purpose, eligibility requirements, and how to apply. The experiment helps uncover the reasons driving low UI take-up and the effects of increasing take-up on job

[10] Roughly 90% of the civilian population aged 16 or older in Washington holds a driver's license. Washington's drivers' licenses (and thus mailing addresses) must be updated at minimum every six years.

[11] In 89% of cases, the ZIP codes of the addresses of such workers matched.

[12] Ultimately, 86% of all addresses in the experimental sample are sourced from WADOL.

search. Appendix C provides further details about the intervention, including treatment arms and sample selection. Appendix D provides more detail on the Washington administrative data.

2.1 Experimental Design

In partnership with the Washington State ESD, we identified a study population of likely eligible non-applicants, randomly assigned them to treatment or pure control groups, and sent targeted letters only to the treated group.[13]

To construct our experimental sample of job losses without an associated UI claim, we use the quarterly wage records to identify individuals with stable employment followed by a sharp reduction in hours in quarter t, referred to as a "potential job loss." Specifically, we restrict to monetarily eligible workers who experienced a 30% or greater drop in hours between quarters $t-1$ and t and who were employed by a single firm during the two quarters preceding job loss.

To avoid outreach to cases where letters would be irrelevant, we further restrict the sample of potential job losses. We naturally exclude those with existing UI claims, since these workers were already receiving benefits. We also exclude cases with concurrent PFML or Workers' Compensation claims, where reduced hours likely reflect reasons other than involuntary job losses. In addition, we exclude workers who earned more than $40 per hour prior to job loss and those from certain high-paying industries who are more likely to self-insure through savings or severance.[14] Lastly, we restrict to workers with mailing addresses in Washington's drivers' license or previous UI application records. Appendix C.1 provides further detail on these restrictions.

We conducted the experiment in two waves, targeting workers who lost their jobs in 2024Q1 and 2024Q2. Within each wave, we randomized the study population into treatment and control groups. Of the 50,199 workers in the experimental sample, 20% were placed in a pure control group that received no letter, while the remaining 80% were assigned to one of several letter treatments.[15] We stratified randomization according to whether a worker had previously received UI in Washington, as we anticipated that prior experience with the UI system could significantly influence outcomes. Figure 1 illustrates the randomization structure and treatment assignment.

All treated individuals received an outreach letter with generic information designed to reduce two key barriers to UI take-up: namely, eligibility misperceptions and learning costs. The

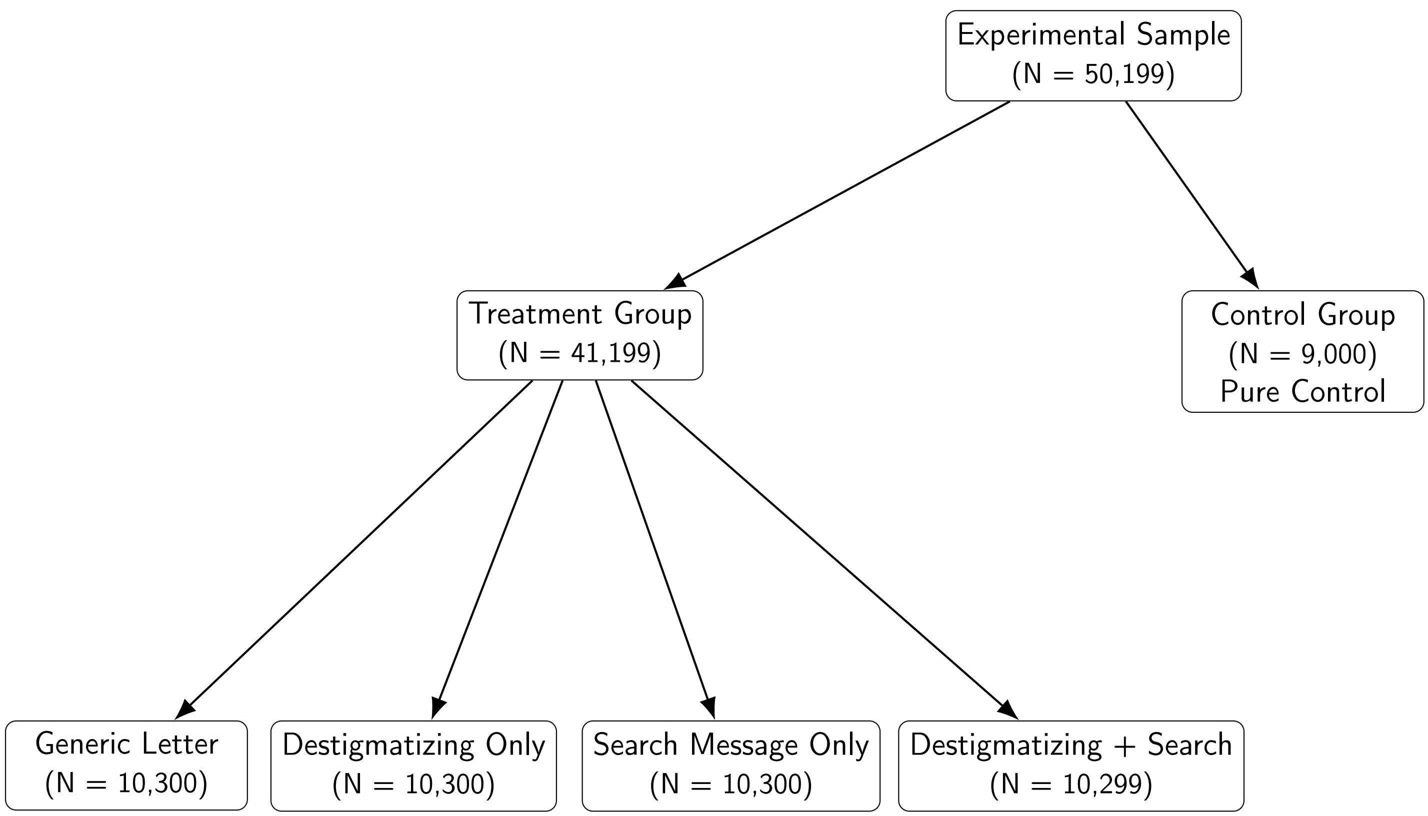
[13]The project was funded through a U.S. Department of Labor grant outlined in UI Program Letter 23-21. The experiment was pre-registered on the AEA RCT Registry (AEARCTR-0013608) and includes a pre-analysis plan.

[14]Excluded industries are NAICS 21 (Mining), 51 (Information), 52 (Finance and Insurance), 55 (Management of Companies), and 61 (Educational Services). Education is excluded due to seasonal employment patterns that often render separations ineligible for UI, as teachers rarely search for work during the summer.

[15]We chose a relatively large treatment group to precisely estimate second-stage labor supply effects, which are identified by comparing compliers in treatment to the number of workers in the pure control group.

letter clarifies who qualifies for benefits: specifically, those who have been laid off through no fault of their own and are actively seeking work. The letter reduces learning costs by highlighting the ease of applying (free of charge, about 30 minutes) and providing the online portal and phone number.[16] The letters were mailed from agency headquarters in Olympia, WA, on May 21 and August 20, 2024, in one large batch per wave, in order to ensure rapid delivery and to provide timely information given the short duration of unemployment spells.

Figure 1: Assignment to Treatments



We drafted the letters, which featured the department logo on both the envelope and letterhead, in collaboration with ESD staff (Figure 2). To refine messaging, we conducted one-on-one interviews with potential UI claimants at a WorkSource office in Seattle, testing different wording and content for clarity and effectiveness. Appendix C.2 provides further details of the mailing procedures and timing.

ESD's customer service division deployed customized phone numbers that were only disclosed to letter recipients and not the general public to track call volume resulting from the outreach letters. The customized phone lines redirected callers to the publicly-accessible main

[16]The letter also lists phone numbers for Spanish language communication and disability accommodations.

customer service line. After verifying their identity, callers seeking to apply for UI were guided through the application process by the attending agent. Appendix C.3 provides further engagement tracking information, including the number of phone calls and undeliverable letters.

Destigmatization Treatment

Individuals randomized to the destigmatization treatment arm received a letter aimed at reducing free-rider stigma, which is defined as the guilt associated with claiming UI without demonstrated need or visible contribution (Friedrichsen, König, and Schmacker, 2018). This treatment arm emphasizes that benefits are based on work history rather than need and highlights that workers contribute to the UI trust fund through employer-side payroll taxes. We included the following statement in these letters: "Benefit amounts are based on your work history — not on financial need. While you worked, your employer paid taxes into the unemployment insurance trust fund for this purpose." The destigmatizing message particularly targets free-rider stigma rather than social stigma: it does not address concerns about being judged negatively by others for receiving UI (Osman and Speer, 2024). Randomizing this message within the generic informational letter allows us to isolate its causal effect on UI take-up.

Search Expectations Treatment

Those randomized to the search expectations treatment received a letter setting expectations about the difficulty of job search and providing information about the typical time it takes to find reemployment. The purpose of this treatment was to correct potential overoptimism about job finding, thereby making UI appear more valuable by comparison. These letters included the following statement: "Searching for work can take a long time and we are here to help you apply for benefits. Statistically, job seekers who have been unemployed for two months typically remain jobless for another three months." The stigma and search treatments were cross-randomized, meaning half of the letters randomly contained each message, and one-quarter contained both.[17] Appendix Figure C.3 shows the saturated letter containing both messages.

2.2 Targeting Potential Job Losses

The target population for our study consists of workers who have not claimed UI benefits but are UI-eligible. However, because we cannot observe incidence of or reason for separation in the administrative hours data, we cannot be sure that all of the workers in our sample are truly eligible. Our experimental sample includes individuals who experienced a reduction in hours

[17] We also test the effect of a cross-randomized "benefit salience" treatment, directing recipients to an ESD web page to estimate their UI benefit level. This treatment makes Washington's relatively generous weekly benefits more salient. In practice, this treatment arm had no impact on applications, and we therefore pool it with the other stigma and search expectation treatments across the remainder of our analysis.

Figure 2: Sample Generic Letter

Employment Security Department
WASHINGTON STATE

May 21, 2024

UEG02
FIRST NAME LAST NAME
ADDRESS 1
ADDRESS 2
CITY STATE ZIP

Dear [FIRST NAME],

If you lost your job through no fault of your own, you might qualify to receive **unemployment benefits**. These benefits provide temporary income while you search for a new job.

It is free to apply. It takes an average of only 30 minutes. The quickest way to apply is online. You can apply as long as you are unemployed and looking for work.

Learn more about applying for benefits:

- For more information, visit esd.wa.gov/learn or call 800-288-5580.
- For information in another language, please contact 800-288-5580.
  Para obtener información en español, comuníquese al 800-288-5580.
- To request an accommodation for a disability, please contact ESDGPUIAccomms@esd.wa.gov or call 844-395-6698.

If you have questions about this letter:
Call 800-288-5580 on weekdays 8 a.m. to 4 p.m. If you have not lost your job, please disregard this letter. This is part of an outreach campaign to workers in Washington state.

Sincerely,
Washington State Employment Security Department

following stable employment but did not claim UI. However, some workers in the sample may not have experienced a job separation (such as those with only a temporary reduction in hours), may have separated for reasons that make them UI-ineligible, or are otherwise unavailable to accept suitable work and are thus ineligible for UI.

To address the issue of non-separators, we exclude from our main analysis workers we label "recall separations" — in other words, those workers who do not actually experience a separation from their employer. These workers recorded more hours at their potential separating firm in quarter $t+1$ than in quarter t and had no employment at any other firm. This pattern suggests unpaid time off or a temporary reduction in hours such as recalls (Nekoei and Weber, 2015; Fujita and Moscarini, 2017) rather than permanent separations. We show that the share of these recall separators is balanced across the treatment and control groups in Appendix Table E.4. While the exclusion of these recalls from the main analysis does not meaningfully amplify the treatment effect, it raises the baseline application rates in both groups by removing false positives from the denominator.

Although our sample likely includes some workers who quit or were fired for cause, thus diluting the treatment effect of the letter on UI-eligible workers, we argue that this is nevertheless a strength rather than a weakness of our design. As we will argue in section 5.1, the moment that allows us to distinguish between misperception of eligibility and learning costs is the UI rejection rate of complier applicants. A positive complier rejection rate due to quit or firing for cause necessitates the existence of some ineligible applications resulting from treatment. Moreover, because the adjudication process screens out those who quit or were fired for cause, only eligible applicants can receive benefits. Our results for UI receipt therefore reflect the true impact of the letters for those who actually lost their jobs. To the extent that the sample includes ineligible individuals, our estimated effects likely understate the true effect on eligible workers.

2.3 Experimental Sample Summary Statistics

To show ex ante balance on observables, Table 1 describes the characteristics of the experimental sample for pure control and treatment groups and provides the p-value for a hypothesis test that the listed characteristic has the same mean across both treatment arms. For ease of comparison, Table 1 only lists balance across these two treatment arms. Appendix Table A.6 lists the mean of economic, industrial, and demographic characteristics for the full experimental sample as well as the p-value for a hypothesis test that the listed characteristic has the same mean across all nine assignment arms (eight treatment arms and the status quo control).

As expected by the process of randomization, our sample exhibits strong balance across key economic, industrial, and demographic characteristics. Of the 26 characteristics listed in Table 1,

Table 1: Summary Statistics by Letter Treatment Status

Variable	Control	Treatment	p-value
Previous Hourly Wage	\$25.63	\$25.69	0.432
Previous Annual Salary	\$46,341	\$46,361	0.917
Base Year Hours Worked	1,804.8	1,803.4	0.739
Ever Received UI	0.581	0.593	0.034
Percent Hours Drop, $t-1$ to t	-65.4%	-64.7%	0.009
Layoff Industry Share			
Retail Trade	0.167	0.166	0.717
Health Care and Social Assistance	0.148	0.144	0.350
Accommodation and Food Services	0.090	0.087	0.288
Manufacturing	0.088	0.094	0.071
Administrative, Support, Waste Management	0.087	0.084	0.356
Construction	0.073	0.081	0.010
Transportation and Warehousing	0.067	0.070	0.208
Professional, Scientific, and Technical	0.060	0.058	0.630
Wholesale Trade	0.057	0.054	0.216
Other Services (except Public Admin)	0.040	0.042	0.315
Public Administration	0.038	0.034	0.094
Arts, Entertainment, and Recreation	0.033	0.030	0.127
Real Estate and Rental and Leasing	0.028	0.028	0.960
Agriculture, Fishing, Forestry	0.025	0.028	0.063
Utilities	0.001	0.002	0.765
Demographics			
Share Female	0.449	0.444	0.333
Age	40.8	41.0	0.175
Share Veteran	0.030	0.029	0.695
Share with Disability	0.037	0.035	0.517
Address in Seattle (proper)	0.078	0.079	0.827
Address in Seattle MSA	0.487	0.481	0.304
N	9,000	41,199	-

Note: Table shows balance across economic, industrial, and demographic characteristics for the status quo control group and the treatment group, which includes any worker who received a letter. The table pools two waves of potential job losses. The rightmost column shows the p-value for a hypothesis test that the listed characteristic has the same mean across both assignment arms. Industry shares may sum to slightly more than one due to rounding. Age is calculated as a worker's age on the date of the first wave mailing.

only three exhibit statistically significant differences between the treatment and control groups, which is well within the range expected by random chance.

The average wage of workers in the experimental sample prior to separation is \$25.68 per hour. These workers earn \$46,357 in the four quarters prior to separation. The typical worker in the experimental sample experiences a significant drop in their hours worked during the quarter of potential job loss, down on average 65% compared to the prior quarter. Assuming smooth employment spells with constant hours, this suggests that the typical worker was laid off at the end of the first month of the quarter in which we identify their potential job loss.[18] This implies the typical subject in our experimental sample has been unemployed for three months when they receive the letter (two months unemployed in quarter t plus a one-month lag for data to be reported to ESD). Given workers with unemployment spells of this duration or longer lack the assets to fully replace lost income from job loss (Gruber, 2001) and UI accounts for 97% of government transfers paid to the unemployed (East and Simon, 2024), studying the UI take-up decisions of this population is of particular interest.

A significant portion of the sample is familiar with the UI system, as 59% received UI in the past. Sample job losses are concentrated in retail trade; health care and social assistance; manufacturing; accommodation and food services; and administrative, support, waste management and remediation services. 44% of the sample is female, the average age is 41 years old, 3% are veterans, and 4% identify having some sort of disability. Roughly half of the experimental sample lives in the Seattle metro area; this reflects the broader population distribution in Washington.

3 Experimental Effects on Take-Up

We estimate statistically significant effects of the information intervention on workers' UI applications and receipt rates. These effects are particularly concentrated among workers laid off from lower-wage jobs. The estimates reflect the intent-to-treat effect of being assigned to receive any type of mailer on both UI applications and benefit receipt.

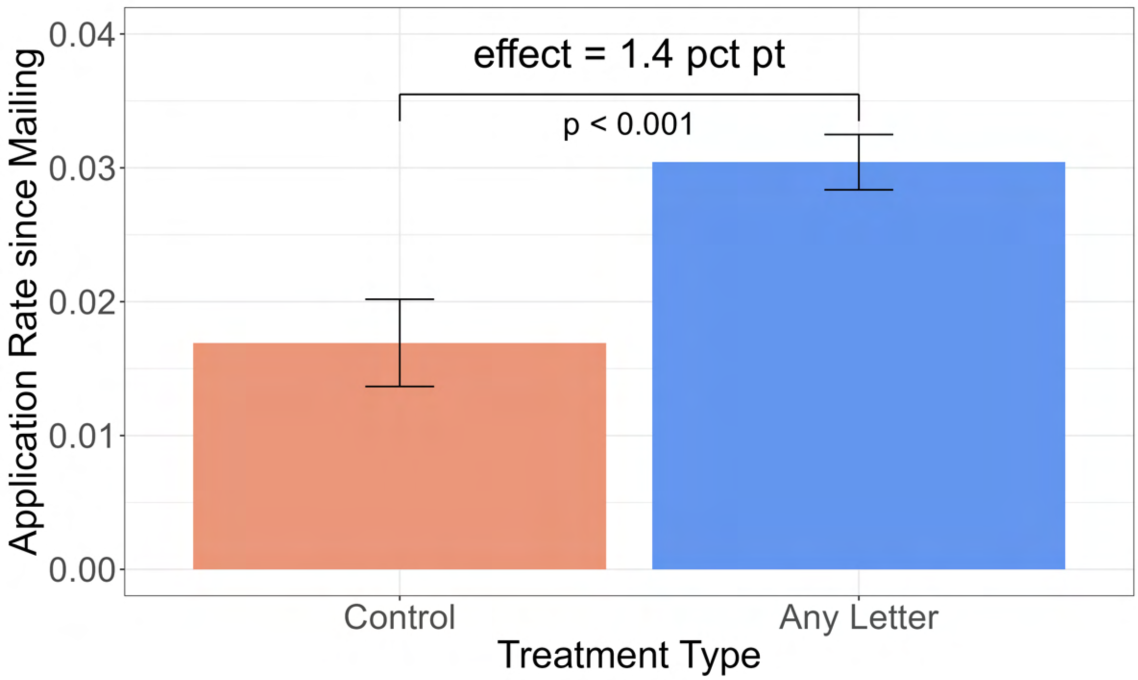
3.1 Overall Effects

Figure 3a presents the overall impact of informational letters on UI application rates. Two months after mailing, treatment significantly increased applications by 1.4 percentage points ($p < 0.001$), reflecting an 82% increase over the control group's baseline application rate of 1.7%.

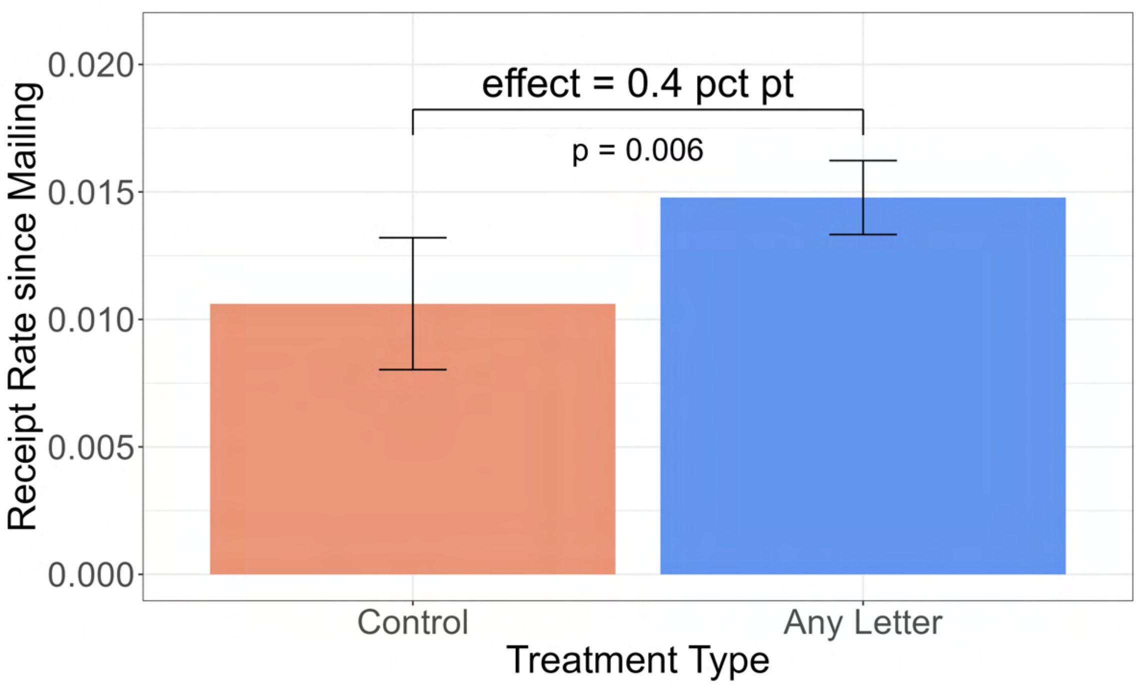
To understand whether this effect reflects a rise in applications from newly induced claimants or simply an acceleration in timing, Figure 4 plots weekly cumulative application rates for treated and control workers relative to the second wave of treatment in August 2024. Application rates track closely in the two weeks prior to mailing, then diverge sharply over the following eight weeks. The gap remains stable for at least the following four months we observe,

[18] In Appendix E.3, we validate the hours drop variable as a reliable proxy for recency of job loss.

Figure 3: Effects of Informational Letters on UI Take-Up



(a) Applications

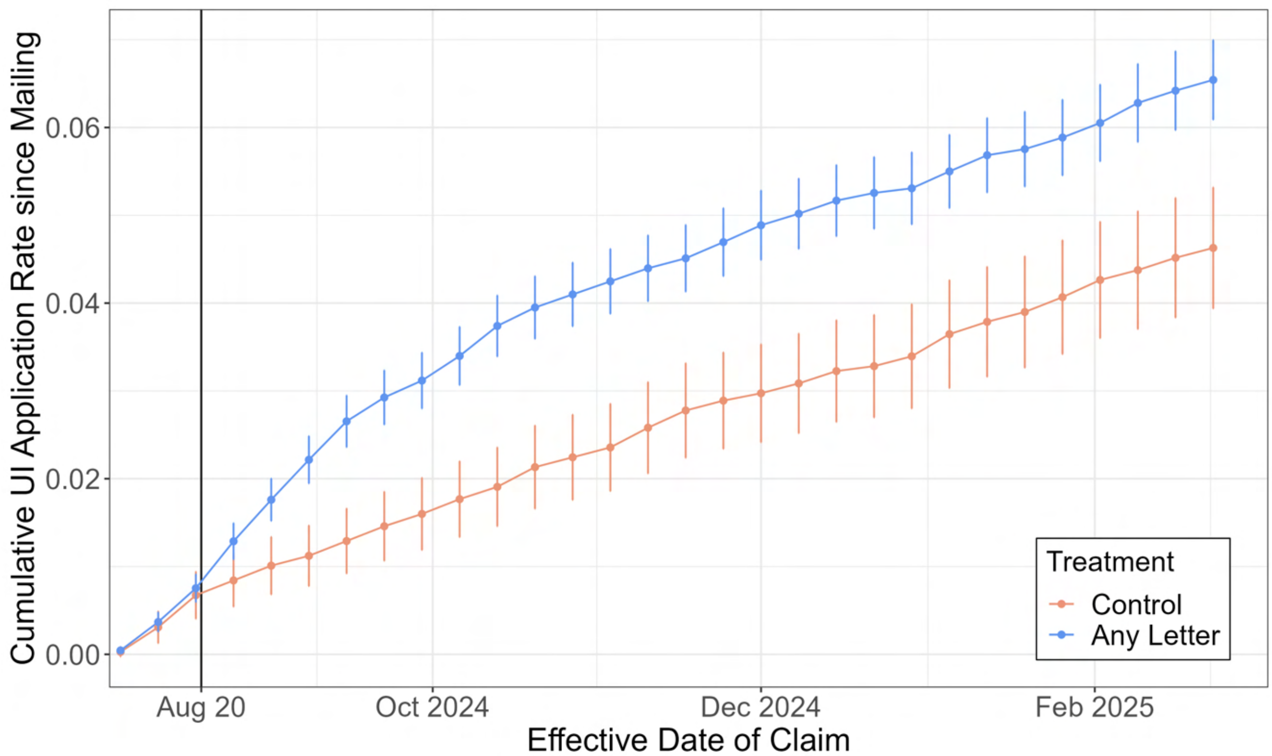


(b) Receipt

Note: Panels represent main effects of intervention on UI applications and receipt. Sample includes potential jobs losses which occur in 2024Q1 and Q2. Claiming outcomes are measured two months after the date of mailing for each wave. Bars represent 95% confidence intervals.

suggesting the intervention induced new applications rather than shifted the timing of eventual applicants.

Figure 4: Dynamic Effect on Applications After Six Months



Note: Graph presents the dynamic effect of letter treatment on UI application six months after mailing a letter. Sample includes potential job losses which occurred in 2024Q2 (second wave), excluding recalls, defined as workers who returned to the same firm with increased hours. Vertical line denotes August 20, 2024, the time of mailing. Bars represent 95% confidence intervals. Dynamic effects of application of wave 1 in A.16a and dynamic effects on receipt are in Figure A.16b.

Figure 3b shows that the treatment also significantly increased UI receipt, indicating that the intervention led to more unemployed workers successfully claiming benefits. Two months after mailing, the treatment increased receipt by 0.4 percentage points ($p = 0.005$) relative to the control group — a substantial 41% increase over the control group's baseline receipt rate of 1.0%.

Our application and receipt results are robust to various alternative measures. First, effects of the letter are robust to the inclusion of all workers in treatment and control groups, including those who ex post appeared not to have separated from their firm despite their sharp drop in hours in quarter t (Appendix Figure A.1).[19] The magnitude of the letter's effect on applications and receipt remains stable across all pre-registered time horizons through six months after mail-

[19]Specifically, the baseline sample excludes "firm stayers" who report more hours at their potential separating employer in quarter $t+1$ than in t and report hours at no other employer.

ing (Appendix Figures A.2 and A.3, Table A.13). Application effects are significant across age groups, genders, veteran status, and disability status (Appendix Figures A.4, A.5, A.6, and A.7).

To contextualize how much the intervention shifted overall UI take-up in our sample, we translate the treatment effect on applications into an implied change in the take-up rate across all recent separations, including those who had already applied before the intervention. Because the experiment excludes workers who claimed UI upon layoff, we add these early claimants back in and extrapolate the treatment effect to the broader experimental sample, including the (small) control group. This yields an estimated increase in the overall UI application rate from 28.0% to 29.0%.

To more accurately measure the true UI take-up rate, we account for the fact that many separations are UI-ineligible (e.g., quits or firings for cause). Using the Current Population Survey (CPS) Nonfilers Supplement, we estimate that 48% of separations in our experimental sample are ineligible for UI, either because of separation type or because the worker was unavailable for work while unemployed. Specifically, we apply the same sample restrictions as in our potential job loss sample (Section 2.1) to a national sample of CPS respondents who were recently non-employed non-claimants with prior work at a single employer. Appendix E.1 contains further details, such as Table E.1, which partitions all UI-eligible and ineligible reasons that recently non-employed non-claimants report.

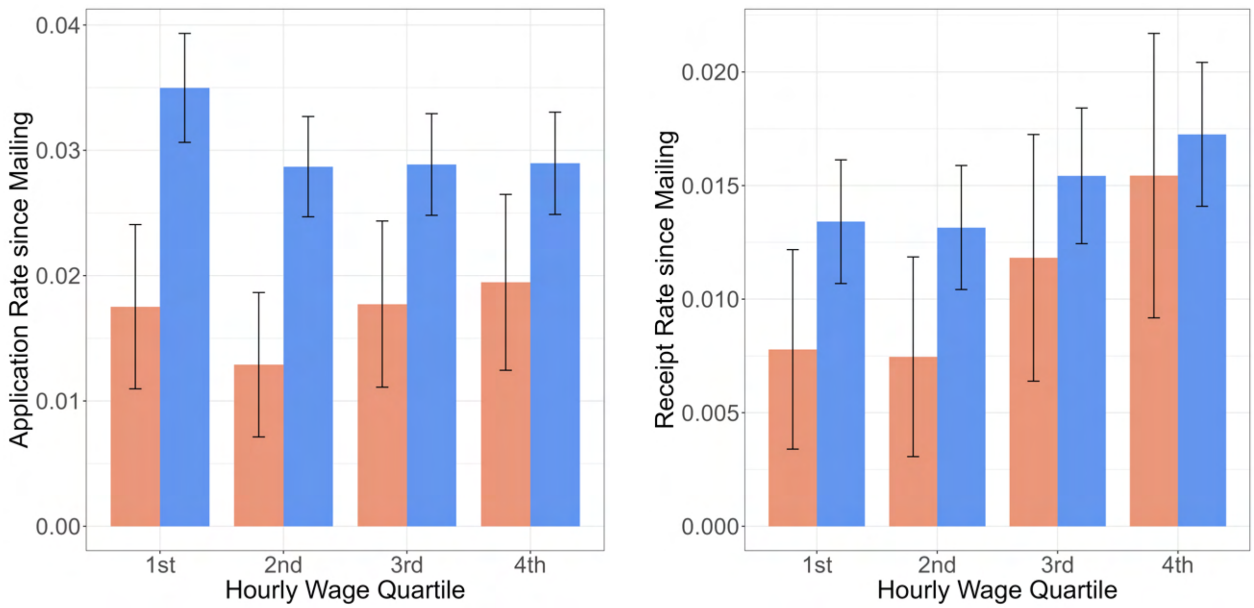
Assuming that 48.2% of separations in our sample are ineligible and that the treatment effect is concentrated entirely among eligible workers, the intervention increased UI take-up among eligible workers from 43% to 45%. This calculation requires an important caveat: it excludes UI-eligible workers whose unemployment spells were too short to be detected by our job loss measure (i.e., those workers who were reemployed at a new firm within the same quarter). We describe calculations in Appendix Table C.4.

3.2 Effects by Previous Hourly Wage

The effect of informational letters on take-up is particularly concentrated among workers laid off from lower-wage jobs. To analyze wage heterogeneity, we divide the sample into quartiles according to a worker's previous hourly wage. The sample median wage is $24.84 per hour. Workers in the bottom quartile earn below $20.38 per hour, while workers in the top quartile earn between $30.71 and $40 per hour.

Figure 5 presents the effects of informational letters on both UI application (panel a) and receipt (panel b) across wage quartiles at the potential separating employer. The figure reveals a clear gradient: treatment effects are largest for workers in the bottom quartile and decline steadily across higher wage groups.

Figure 5: Effects of Informational Letters on UI Take-Up by Previous Hourly Wage



(a) Applications

(b) Receipt

Note: Panels represent main effects of intervention on UI applications and receipt. Red bars represent the control group while blue bars represent the treatment group. Sample includes potential jobs losses which occur in 2024Q1 and Q2. Claiming outcomes are measured two months after the date of mailing for each wave. Bars represent 95% confidence intervals. Prior job median wage in the experimental sample is $28.84/hour.

Application effects are strongest for lower-wage workers. Among bottom-quartile workers, the application rate of treated individuals doubles relative to the control, increasing from 1.75% to 3.5%. In the top wage quartile, the application rate rises more modestly from 1.9% to 2.9%, a 50% increase, though the effect remains statistically significant at the 5% level.

Effects on receipt are even more concentrated among lower-wage workers, further sharpening the treatment effect wage gradient. As shown in Figure 5b, control group workers in the top wage quartile are the most likely to receive UI benefits, while those in the bottom quartile are the least likely to receive them. However, the intervention substantially narrows this disparity. Workers in the bottom quartile experience a 0.6 percentage point increase in receipt on a control mean of 0.8%, which is a statistically significant 72% increase ($p = 0.03$). By contrast, the top quartile sees only a modest increase of 0.2 percentage points on a 1.5% control mean — this effect is not statistically significant. These results underscore the differential impact of informational letters, with the largest gains concentrated among the lowest-wage workers.

Splitting the sample by median wage, as pre-registered, yields similar qualitative conclusions: the letter treatment had both larger absolute and relative effects on applications and receipt for low-wage workers (Figure A.9). Application effects are statistically significant for both high- and low-wage groups, whereas receipt effects are significant only for the low-wage group. A similar pattern emerges when we examine heterogeneity by previous income rather than hourly wage (Appendix Figure A.10) in accordance with our pre-analysis plan, though the gradient is somewhat less pronounced.

3.3 Characteristics of Marginal Applicants and Recipients

To examine the characteristics of the marginal applicant or recipient affected by the intervention, we define the outcome in the treatment or control group to be the average of a specific characteristic among those who apply or receive UI. For example, we compare the workers' previous hourly wage among those who apply or receive UI in the treatment and control groups. Differences in the average characteristics of applicants or recipients in the treatment group relative to the control group reveal how the characteristics of the marginal individual who applies for or receives UI due to the intervention differ from the average applicant or recipient who would enroll absent the intervention. These comparisons are summarized in Table 2.

Complier applicants look similar across most observable characteristics to those who would have applied regardless of treatment (i.e., "always appliers". However, there are several notable differences between always appliers and complier applicants. Compliers earn slightly lower wages on average prior to separation ($25.23 vs. $25.96, $p = 0.32$), are significantly less likely to have ever received UI benefits (64% vs. 75%, $p = 0.02$), and exhibit somewhat larger reductions in hours worked at the separating employer prior to layoff, which suggests longer unemployment durations (a 68% vs. 64% decline, $p = 0.13$). We find no meaningful differences in industry composition or demographics between always appliers and complier applicants.

Patterns among complier recipients mirror those observed for complier applicants, though the magnitudes and statistical significance vary. Complier recipients had lower wages in their previous job than always taker recipients ($26.36 vs. $27.50, $p = 0.22$), but this difference is offset by higher average hours worked prior to layoff; this results in comparable levels of pre-separation income. Similar to complier applicants, complier recipients show signs of longer unemployment durations and experience a larger drop in hours worked (68% vs. 63%, $p = 0.08$). Industry composition is largely similar between always taker and complier recipients, though a greater share of complier recipients were laid off from the health care and social assistance sector (14% vs. 8%, $p = 0.10$). Complier recipients are also less likely to be female (43% vs. 52%, $p = 0.16$).

Table 2: Complier Analysis: Applicants and Recipients

	Applicants			Recipients		
Variable	Always Takers	Compliers	p-value difference	Always Takers	Compliers	p-value difference
Previous Hourly Wage	25.96	25.23	0.32	27.50	26.36	0.22
Previous Annual Salary	$45,476	$44,975	0.77	$48,088	$47,503	0.79
Base Year Hours Worked	1,734.6	1,775.0	0.27	1,727.8	1,801.0	0.08
Ever Received UI	0.75	0.64	0.02	0.73	0.68	0.46
% Hours Drop, $t-1$ to t	-64%	-68%	0.13	-63%	-68%	0.08
Layoff Industry Share						
Retail Trade	0.15	0.18	0.34	0.14	0.15	0.86
Health Care/ Social Asst	0.12	0.13	0.67	0.08	0.14	0.10
Accomm/Food Svcs	0.08	0.10	0.48	0.06	0.10	0.33
Manufacturing	0.09	0.11	0.58	0.10	0.11	0.71
Admin/Waste Mgmt	0.07	0.09	0.51	0.08	0.11	0.50
Construction	0.06	0.08	0.48	0.06	0.08	0.59
Demographics						
Share Female	0.51	0.44	0.20	0.52	0.43	0.16
Age	38.9	40.4	0.30	40.5	42.3	0.33
Share Veteran	0.09	0.06	0.37	0.11	0.07	0.29
Share with Disability	0.14	0.09	0.21	0.11	0.12	0.92
Address Seattle (proper)	0.08	0.08	0.91	0.10	0.08	0.79
Address Seattle MSA	0.46	0.52	0.23	0.48	0.53	0.45
Among UI Recipients						
Weekly Benefit Amount	-	-	-	$532.35	$525.23	0.92
Number Weeks on UI	-	-	-	17.9	13.2	0.29
Total UI Received	-	-	-	$9,541	$7,154	0.38

Note: Table lists the average characteristics for always taker and complier applicants and recipients as well as the p-value difference between them. Not all applicants receive UI, which is why we do not report averages among the subset of UI recipients.

4 A Model of Search with Incomplete Take-Up

This section introduces a partial equilibrium job search model with endogenous take-up to clarify intuition for how easing the barriers to take-up affects an unemployed worker's claiming and search behavior. The model, which extends McCall (1970), provides a guide for interpreting the experimental results by showing how different mechanisms, such as eligibility misperceptions or learning costs, imply distinct predictions for the treatment effect of an informational letter on the UI rejection rate. Extensions and proofs are in Appendix B. Section 7 then applies the model to the data and yields (i) estimates of learning, work-search compliance, and stigma costs, and (ii) a mechanism-level decomposition of the letters' impact on applications and rejections.

4.1 Setup

Let time be discrete. We consider infinitely-lived workers who become unemployed at $t = 0$. Each worker has two exogenous characteristics drawn at the start of unemployment: (i) separation eligibility status $e \in \{0, 1\}$, which indicates whether a worker's separation qualifies them for UI (laid off vs. quit/fired, or QF), and (ii) a search cost type $s \in \{\ell, h\}$ which determines the compliance cost η_s of meeting the UI job search requirement if approved for benefits.

Workers do not observe their true separation eligibility e but form subjective beliefs $\widehat{p}$ about the likelihood of being eligible. Beliefs are drawn from $G_1(\widehat{p})$ if $e = 1$ and $G_0(\widehat{p})$ if $e = 0$, with both defined on $[0, 1]$. Let G_1 first-order stochastically dominate G_0, so eligibles are on average (correctly) more optimistic. Nevertheless, some eligibles underestimate their chances due to pessimism, while some ineligibles overestimate due to optimism.

Similarly, workers do not observe their search cost type s but form beliefs $\widehat{\pi}$ about the probability of being a low-cost type ($s = \ell$). Let $H_\ell(\widehat{\pi})$ and $H_h(\widehat{\pi})$ denote the belief distributions for true low- and high-cost types, with H_ℓ first-order stochastically dominating H_h. Type $s \in \{\ell, h\}$ is revealed only upon applying. If $s = \ell$, the worker complies each period, incurs η_ℓ, and — if $e = 1$ — receives UI. If $s = h$, the worker optimally does not comply with job search verification (even if $e = 1$) and is denied UI (i.e., rejected for "Not Actively Seeking Work", or $NASW$). Thus, the value of trying to claim with a high search cost (V_h^C) is equivalent to the value of simply not claiming (V^N).

At the outset of their spell, the unemployed decide whether to apply for UI by considering the following factors:

1. A worker's perceived likelihood of application approval, $\widehat{p} \in (0, 1]$. This allows for workers to have eligibility misperceptions.

2. A worker's perceived likelihood of being low-search cost, $\widehat{\pi} \in (0, 1]$.

3. A worker's learning cost $\kappa > 0$ that captures the fixed cost of learning about the UI program and how to apply. Workers incur κ upon applying, regardless of whether they receive UI.
4. A worker's recurring stigma cost $\alpha > 0$ that embodies the internalized guilt or shame associated receiving a UI check. Workers only incur stigma cost α if they receive UI benefits.

UI benefits yield payment b per unemployed period. An unemployed worker who does not apply, or who applies and is denied for either reason, receives no UI benefits.

After this initial decision, unemployed workers receive job offers drawn from wage distribution $F(w)$. The arrival rate of offers depends on the workers' efforts. Workers who receive UI by satisfying the UI search requirement receive offers at rate λ_1 (i.e., the probability an offer arrives in a given period), while those who do not receive UI evaluate offers arriving at rate $\lambda_0 < \lambda_1$. Offers cannot be recalled and jobs last forever at constant wage w. Workers maximize the expected sum of earnings (wages or UI) discounted by $\beta \in (0, 1)$.

This setup embeds both the classic McCall trade-off between the cost of waiting too long for a job offer and the cost of accepting too early, as well as the trade-off between the costs of applying for UI (both fixed and recurring) and the cost of forgoing the future flow value of UI benefits while unemployed. This setup also characterizes the dual role of work search requirements: on the one hand, they lead some separation-eligible workers to forgo benefits because of the disutility of compliance, while on the other hand, they induce greater search effort among recipients, which results in higher job offer arrival rates.

Relationship to Experiment. Generic informational letters may increase UI take-up in two ways by providing a potentially bundled treatment: on one hand, they may increase eligible workers' $\widehat{p}$ by informing them of UI's eligibility conditions, and on the other hand, they may decrease fixed learning costs κ for all workers who seek to learn about navigating the UI system. Destigmatizing letters may also decrease α for UI recipients on top of these forces.

4.2 Bellman Equations and Decision Rules

Let V^C represent the value of being unemployed while claiming UI benefits. We model this state explicitly because we observe UI receipt in the data and receipt influences search behavior through not only outside options but also stigma and search requirements. Normalize $\lambda_1 = 1$ to simplify notation (i.e., UI recipients receive an offer every period). A claimant can either accept an offer and receive the present value of wages or receive the benefit amount b net of the recurring costs stigma (α) and search compliance (η_ℓ), plus the discounted expected future value of claiming where they receive wage offers. The Bellman equation for a UI claimant is:

$$V_\ell^C = \max\left\{\frac{w}{1-\beta}, \quad b - \alpha - \eta_\ell + \beta\int V_\ell^C(w')dF(w')\right\}. \tag{1}$$

Workers who do not claim UI — either because they do not apply, are rejected due to $e = 0$, or choose not to meet the search requirement given their η_h — face a similar decision: they either accept an offer or reject and continue in the *non*-claiming state. In this state, offers arrive with probability $\lambda_0 < 1$, and the continuation value does not include UI benefits or stigma and search compliance costs. Let V^N denote the value of unemployment while not claiming:

$$V^N = \lambda_0\left(\max\left\{\frac{w}{1-\beta}, \beta\int V^N(w')dF(w')\right\}\right) + (1-\lambda_0)\beta\int V^N(w')dF(w') \tag{2}$$

Initial Take-Up Decision

At $t = 0$, workers choose to apply if and only if the value of applying exceeds the value of not applying, meaning they will apply if

$$\underbrace{-\kappa + \widehat{p}\left(\widehat{\pi}V_l^C + (1-\widehat{\pi})V_h^C\right) + (1-\widehat{p})V^N}_{\text{value of applying}} > \underbrace{V^N}_{\substack{\text{value of not}\\ \text{applying}}}. \tag{3}$$

Equation (3) can be rearranged to show that the product of one's probability beliefs about being separation-eligible and low-search cost must exceed the ratio of the learning cost and the net value of claiming (proof in Appendix B.3):

$$\widehat{p}\cdot\widehat{\pi} \geq \frac{\kappa}{V_\ell^C - V^N} \tag{4}$$

Appendix Figure B.1 visually illustrates this decision rule.

4.3 Defining Application Rejection Rate

By assumption, separation-ineligible applicants ($e = 0$) are always rejected, and separation-eligible applicants ($e = 1$) are approved only if they are the low-search cost type ($s = \ell$), meaning that they comply with job search requirements. Thus, rejection can occur because of ineligibility at separation or non-compliance with search requirements. For the sake of brevity, we refer to separation-(in)eligibility as (in)eligibility. We observe analogues in our data, as we record rejection due to quit/firing (R_{QF}, $e = 0$) or not actively seeking work (R_{NASW}, $s = h$).

Application Rates

Let $\theta_e = \Pr(e = 1)$ be the share of eligible workers among the unemployed, and $\theta_\ell = \Pr(s =$

$\ell|e=1$) be the share of low-cost types among the eligible.

Applications can come from three types of workers: eligible low-cost types who are always approved (application rate $A_{1\ell}$), eligible high-cost types who do not certify search and are rejected (application rate A_{1h}), and ineligibles who are rejected (application rate A_0). See Appendix B.1 for type-specific application rates expressed in terms of model primitives.

The overall application rate is expressed as the weighted average of these type-specific rates:

$$App = \theta_e\Big(\theta_\ell A_{1\ell} + (1-\theta_\ell)A_{1h}\Big) + (1-\theta_e)A_0 \tag{5}$$

Rejection Rates

The overall rejection rate can be expressed as $1 - \frac{Rec}{App}$, or as

$$Reject = 1 - \frac{\theta_e\theta_\ell A_{1\ell}}{\theta_e\theta_\ell A_{1\ell} + \theta_e(1-\theta_\ell)A_{1h} + (1-\theta_e)A_0}. \tag{6}$$

Type-specific rejection rates, R_{QF}, and R_{NASW}, can be written as shares of all applications:

$$R_{QF} = \frac{(1-\theta_e)A_0}{\theta_e\theta_\ell A_{1\ell} + \theta_e(1-\theta_\ell)A_{1h} + (1-\theta_e)A_0} \qquad R_{NASW} = \frac{\theta_e(1-\theta_\ell)A_{1h}}{\theta_e\theta_\ell A_{1\ell} + \theta_e(1-\theta_\ell)A_{1h} + (1-\theta_e)A_0}$$

4.4 Model Prediction for Rejection Rate Under Belief Updating

The model's assumptions allow us to make the following sharp statement: if additional applications come primarily from eligible low–cost workers updating their beliefs, then rejection shares of both types (QF and $NASW$) must fall. Thus, observing an increase in applications without a decline in the QF or $NASW$ rejection share implies that reduced learning costs κ drawing in ineligibles or high–cost eligibles must be at work. We formalize this below:

Proposition 1: *Suppose an intervention leaves eligibility beliefs among the ineligible ($e=0$) unchanged and leaves beliefs about one's own search-compliance type s unchanged. If the intervention raises UI applications but neither the quit/fired rejection share R_{QF} nor the actively seeking work rejection share R_{NASW} falls, then eligibility belief-updating among low-cost eligible workers cannot be the dominant take-up mechanism; there must also be an increase in applications from ineligible workers or from high-cost eligible types, consistent with a reduction in learning costs κ.*

We provide a proof of this proposition in Appendix B.3. The assumption that the intervention leaves eligibility beliefs of ineligibles unchanged is necessary, but it can be modified to state that the intervention can lower eligibility beliefs among ineligibles (i.e., $G_0'(\widehat{p}^*) \leq G_0(\widehat{p}^*)\ \forall \widehat{p}^* \in (0,1]$) and arrive at the same conclusion. If ineligible workers update their beliefs accurately due to the information provision (i.e., they revise down their beliefs) but the overall rejection rate does not fall, this change must intuitively be offset by a sufficiently large reduction in fixed learning costs for all workers. We consider this a plausible but perhaps stronger assumption.

5 Characterizing the Mechanisms of Incomplete Take-Up

In this section, we quantify the relative importance of eligibility misperceptions, learning costs, and stigma in increasing UI take-up as a result of treatment. We combine a reduced-form effect on rejection rates and model-based reasoning to argue that letters boost UI applications because they reduce learning costs rather than inform qualified workers of their program eligibility. We show that reducing stigmatized attitudes towards UI increases applications, mostly among high-wage workers. Lastly, we find no evidence of overoptimism in job-finding expectations and pessimism in expected benefits.

5.1 Separating Eligibility Misperceptions from Learning Costs

Informational letters provide a bundled treatment by both informing workers about the eligibility criteria for UI and decreasing learning costs of application. This means that we cannot experimentally vary eligibility misperceptions and learning costs separately in our setting.

To clarify their contributions, we leverage proposition 1 from section 4.4, which states that if letters primarily correct eligibility misperceptions, then the rejection rate among applicants should fall, since newly induced applicants would be truly eligible workers who had previously refrained from applying due to mistaken beliefs. Table 3 illustrates that the overall rejection rate rises substantially among complier applicants relative to those who apply regardless of treatment. Among always takers, the overall rejection rate is 37%, while among complier applicants, it jumps to 69%, a statistically significant increase.[20] This suggests that, according to Proposition 1, the intervention may have decreased learning costs more than it induced belief-updating. In order to be certain, however, we must observe how each type of rejection rate (due to quits/firings and not actively seeking work) occurs separately.

Table 3 also breaks down the rejection rate by reason. The share of applications denied for quit/fired for cause (QF) rises modestly from 21% among always takers to 26% among compliers, a statistically insignificant rise; this rules out the possibility that complier applicants were more confident about their separation eligibility. However, the share of applications denied

[20] We calculate the rejection rate of the compliers by assuming the treatment group is composed of always takers and compliers and that the always takers have the same rejection rate as the control group. We calculate 95% confidence intervals using the delta method.

Table 3: Rejection Rates of Always Takers and Compliers

	Rejection Rate	
	Always Takers	Compliers
Main Sample	36%	68%
	[27%, 45%]	[54%, 82%]
Rejection Rate by Reason (as Share of All Claims)		
Quit/Fired for Cause	21%	26%
	[13%, 29%]	[14%, 38%]
Not Actively Seeking Work	5%	18%
	[1%, 9%]	[11%, 25%]
Other	10%	9%
	[4%, 16%]	[0%, 17%]
Unclassified	11%	23%
	[5%, 17%]	[14%, 33%]
By Previous Hourly Wage		
Low-Wage	50%	67%
	[35%, 65%]	[46%, 88%]
Rejection Rate by Reason (as Share of All Low-Wage Claims)		
Quit/Fired for Cause	22%	29%
	[10%, 34%]	[11%, 46%]
Not Actively Seeking Work	9%	18%
	[0%, 17%]	[5%, 30%]
Other	15%	7%
	[5%, 26%]	[-8%, 22%]
Unclassified	17%	20%
	[6%, 29%]	[5%, 36%]
High-Wage	25%	64%
	[14%, 36%]	[46%, 82%]
Rejection Rate by Reason (as Share of All High-Wage Claims)		
Quit/Fired for Cause	20%	23%
	[9%, 30%]	[7%, 39%]
Not Actively Seeking Work	2%	16%
	[-2%, 5%]	[9%, 24%]
Other	5%	8%
	[0%, 11%]	[-1%, 18%]
Unclassified	5%	24%
	[0%, 11%]	[14%, 35%]

Note: Table presents rejection rates for UI applicants in the experimental sample and distinguishes between always takers and compliers. Sample excludes recalls (i.e., workers who returned to the same firm with increased hours). Sample rejection rate is the share of applicants whose claims were denied. Reason-specific rejection rates report the share of all claims denied for each stated reason, again as a fraction of all claims, not as a fraction of denied claims. Some claims are flagged with multiple reasons. "Unclassified" captures denials unable to be mapped to the main categories shown, mostly because applicants did not respond to queries. The bottom panel stratifies applicants by their previous hourly wage relative to sample median.

for not actively seeking work ($NASW$) rises sharply from 5% among the always takers to 18% among the compliers, a statistically significant increase. This indicates that treatment is more likely to induce applicants who are unwilling or unable to meet search requirements and reinforces the interpretation that increased take-up operates primarily through reduced learning costs. Many of these complier applicants may have been deterred once they confronted the higher-than-expected costs of satisfying search requirements.

The disproportionate incidence of search verification denials among complier applicants highlights that the letter lowered learning costs but did not reduce compliance costs associated with maintaining benefit eligibility. As a result, many marginal applicants entered the UI system only to be screened out by frictions that remained unaddressed by the intervention; this emphasizes the importance of program recertification costs that have been documented in settings such as SNAP (Homonoff and Somerville, 2021).

Wage Heterogeneity

Mirroring our earlier results on take-up, the findings on UI application denials exhibit substantial heterogeneity by wage level. Table 3 illustrates that low-wage workers who always apply for UI struggle to obtain benefits, facing a relatively high 50% denial rate. Complier applicants separating from low-wage jobs are denied 67% of the time; this is only a modest increase compared to the denial rate of low-wage always takers. Moreover, the distribution of reasons for rejection is largely similar across the two groups in the low-wage population.

In contrast, high-wage workers who would have applied without treatment are rarely rejected (27%), but those induced to apply by the letter are denied at a rate of 73%. Table 3 reveals that this dramatic reversal in denial rates is explained by differences in the reasons for rejection. High-wage always takers are almost exclusively rejected because they quit or were fired for cause, whereas a large share of high-wage compliers are denied for failing to certify job search.

5.2 The Role of Stigma

This subsection analyzes the added effect of randomized destigmatizing language on UI applications. Figure 6a shows that a basic informational letter without destigmatizing content increases the application rate by 1.2 percentage points relative to the control group ($p < 0.001$), on a base rate of 1.7%. Adding destigmatizing language to the letter further increases the application rate by 0.4 percentage points ($p = 0.09$), about one-third the magnitude of the generic letter treatment effect. These findings indicate that stigma-reducing messages modestly enhance the effect of outreach on UI applications and account for roughly one-quarter of the total observed increase in application rates relative to control. Results from regression specifications reported in Table A.2 confirm these magnitudes. Destigmatization also increases UI receipt, though we are underpowered to detect a significant effect.[21] This suggests that stigma plays a measurable, though partial, role in depressing take-up of UI benefits.

The effect of the destigmatizing message is concentrated among higher-wage workers. As shown in Figure 6b, the addition of stigma-reducing language increases application rates by 0.5 percentage points (pp) for workers above the median wage ($p = 0.07$) relative to a generic letter effect of 0.7 pp, while the corresponding effect for below-median wage workers is only 0.2 pp relative to a generic letter effect of 1.5 pp and statistically insignificant ($p = 0.52$). This heterogeneity is consistent with the idea that higher-wage individuals, who may be less accustomed to receiving public assistance, experience greater internal stigma around UI use.

To provide evidence that destigmatizing treatment from the experiment increased applications due to changes in internalized stigma, we conducted an online survey of 530 likely UI-eligible unemployed non-claimants, recruited nationally via Qualtrics and meant to approximate our experimental sample. Half of the respondents were randomly exposed to the letter's destigmatizing message (i.e., "[b]enefit amounts are based on your work history..."), mirroring the experiment. The destigmatizing message significantly increased the likelihood that respondents viewed UI as similar to car insurance, "which require[s] regular payments so [that] you have a cushion in bad times..." — a framing associated with lower free-rider stigma — by 27% ($p < 0.01$). It also raised the self-reported likelihood of UI application by 28% ($p < 0.05$). See Appendix F.4 for further details.

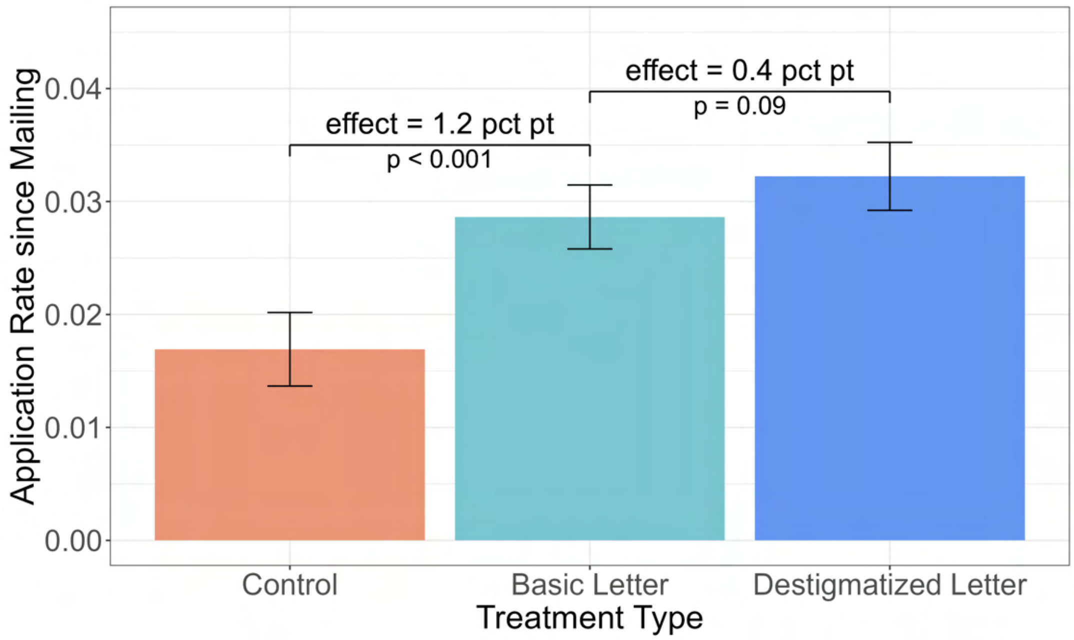
To our knowledge, this is the first setting in which an informational provision field experiment has statistically significantly increased applications for public benefits by reducing stigma. While many encouragement designs have tested stigma-reducing messages without success[22], only Lasky-Fink and Linos (2024) has shown that a destigmatizing message alone can increase engagement with emergency rental assistance; however, its results on applications were modest and not statistically significant at conventional levels.[23] Our finding that higher-wage workers appear more affected by stigma is consistent with evidence from Bitler et al. (2022), who show that higher-income workers hold more stigmatized beliefs about UI benefits.

[21] Appendix Figure A.14 shows that the destigmatizing message raises receipt, particularly for high-wage workers, though the estimates are imprecise.

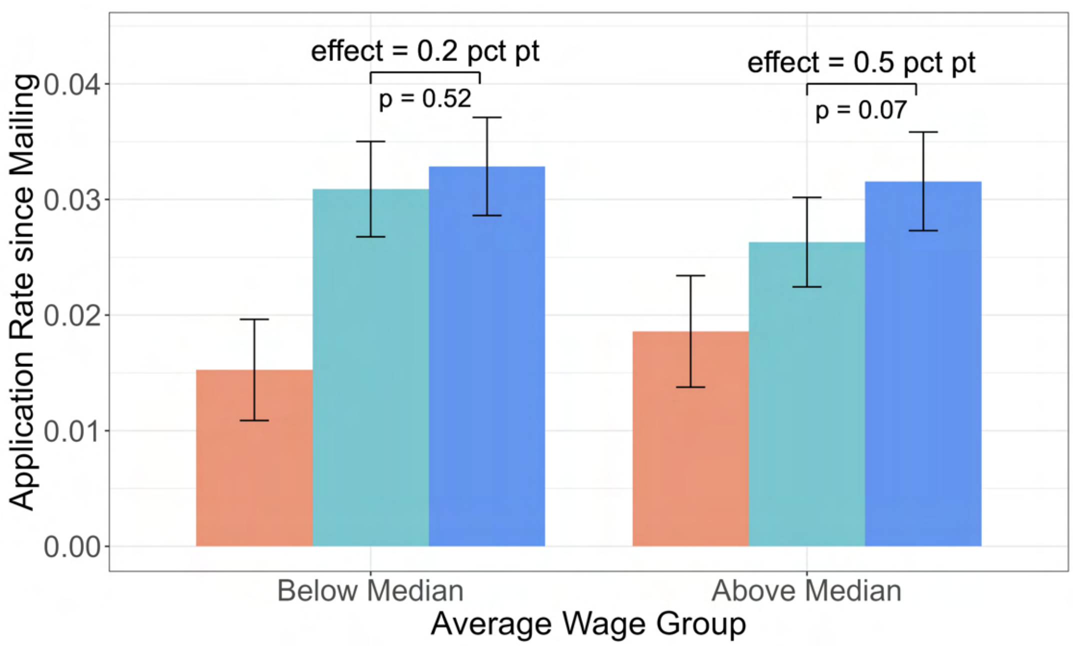
[22] Bhargava and Manoli (2015), Finkelstein and Notowidigdo (2019), and Linos et al. (2022) all experimentally test stigma messages and find no effect on program applications.

[23] Lasky-Fink and Linos (2024) report the randomized "Info + Stigma" treatment as increasing applications for emergency rental assistance by 0.07pp (11%) relative to the application rate of 0.66pp from the "Info Only" group, but this difference was not significant ($p = 0.34$). In contrast, our destigmatizing letter raised applications by 0.4pp (14%) relative to a 2.9pp rate in the generic letter arm ($p = 0.09$).

Figure 6: Effects of Destigmatizing Message on Applications



(a) Overall Sample



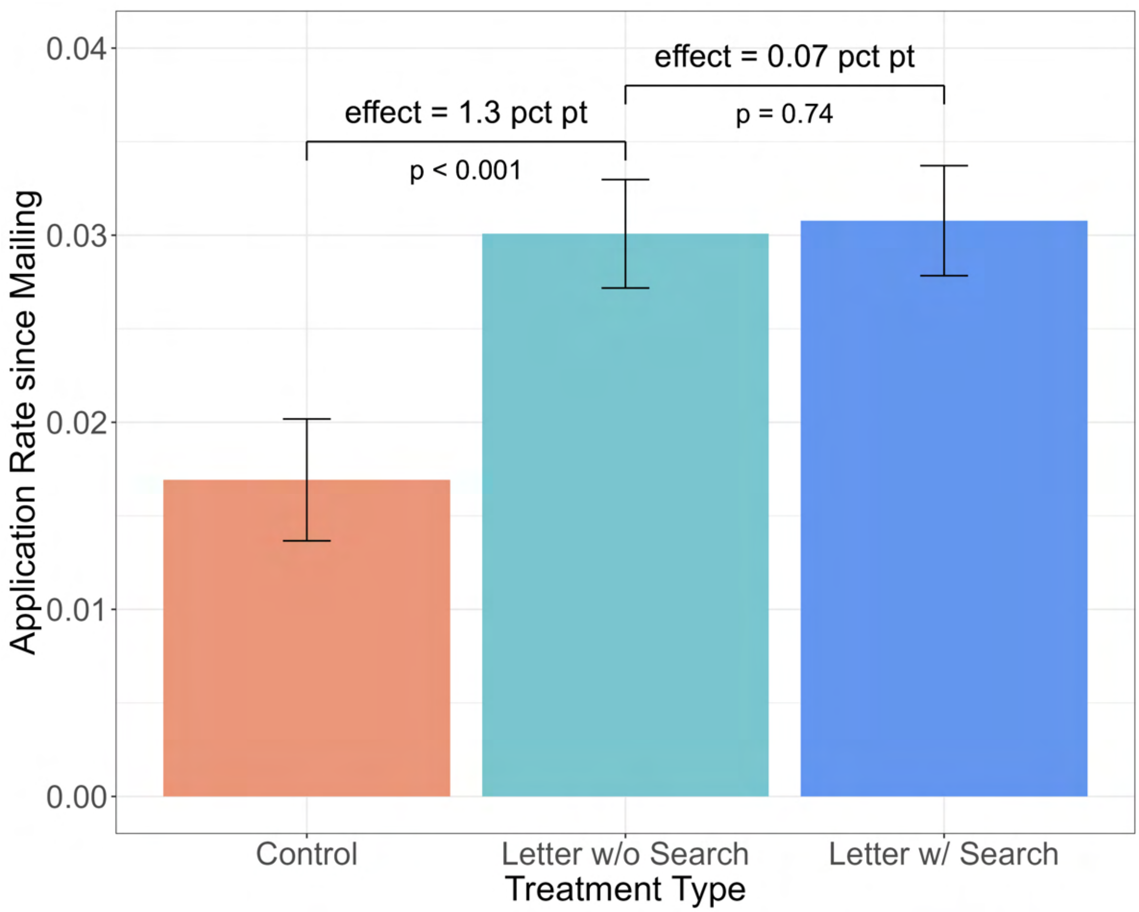
(b) by Previous Hourly Wage

Note: Graphs represent effects of intervention on UI applications according to whether the treatment group's letter included a destigmatizing message. Top panel shows effect on overall sample, bottom panel splits sample by previous hourly wage. The red bar represents the control group, light blue bar represents the treatment group without a destigmatizing message, and dark blue bar represents the treatment group with such a message. Sample includes potential jobs losses which occur in 2024Q1 and Q2. Claiming outcomes are measured two months after the date of mailing for each wave. Bars represent 95% confidence intervals.

5.3 Ruling Out Overoptimism About Job Finding

One potential explanation for low UI take-up is that some unemployed workers may expect to be unemployed for a short period of time, so the fixed cost of claiming exceeds the small expected benefits (Blasco and Fontaine, 2021). However, some non-claimants may be overly optimistic about their job finding prospects (Spinnewijn, 2015) and may therefore delay or forego applying for benefits. If so, providing information that sets more realistic expectations about job finding could increase take-up. As discussed in section 2.1, we test this hypothesis with one experimental arm. The treatment had no effect on UI application rates (0.07 percentage points on a base of 3.0%, $p = 0.74$); this indicates that the message did not change behavior (Figure 7).

Figure 7: Effects of Search Expectations Message on Applications



Note: Graph represent effects of intervention on UI applications according to whether the treatment group's letter included a job finding overoptimism message. The red bar represents the control group, light blue bar represents the treatment group without the job finding overoptimism message, and dark blue bar represents the treatment group with such a message. Sample includes potential jobs losses which occur in 2024Q1 and Q2. Claiming outcomes are measured two months after the date of mailing for each wave. Bars represent 95% confidence intervals. Appendix Figure A.15 shows effect of message on UI receipt.

To explore why this treatment was ineffective, we turn again to the online survey described in section 5.2. Respondents were cross-randomized into receiving the same search-expectations message and asked about their one- and three-month job-finding probabilities.

Appendix Table F.2 shows no effect on these job-finding *beliefs*, suggesting that the message failed to shift subjective search expectations. One possible explanation for this outcome is that workers interpreted the statistic intended to set realistic expectations as a statement about others rather than about themselves. That is, the message may have influenced higher-order beliefs (i.e., what workers think about the typical unemployed person) rather than first-order beliefs about their own job-finding prospects. In any case, it is unsurprising that treatment did not induce changes in UI-claiming behavior since it did not alter perceived job search prospects.

6 Experimental Effects on Labor Supply

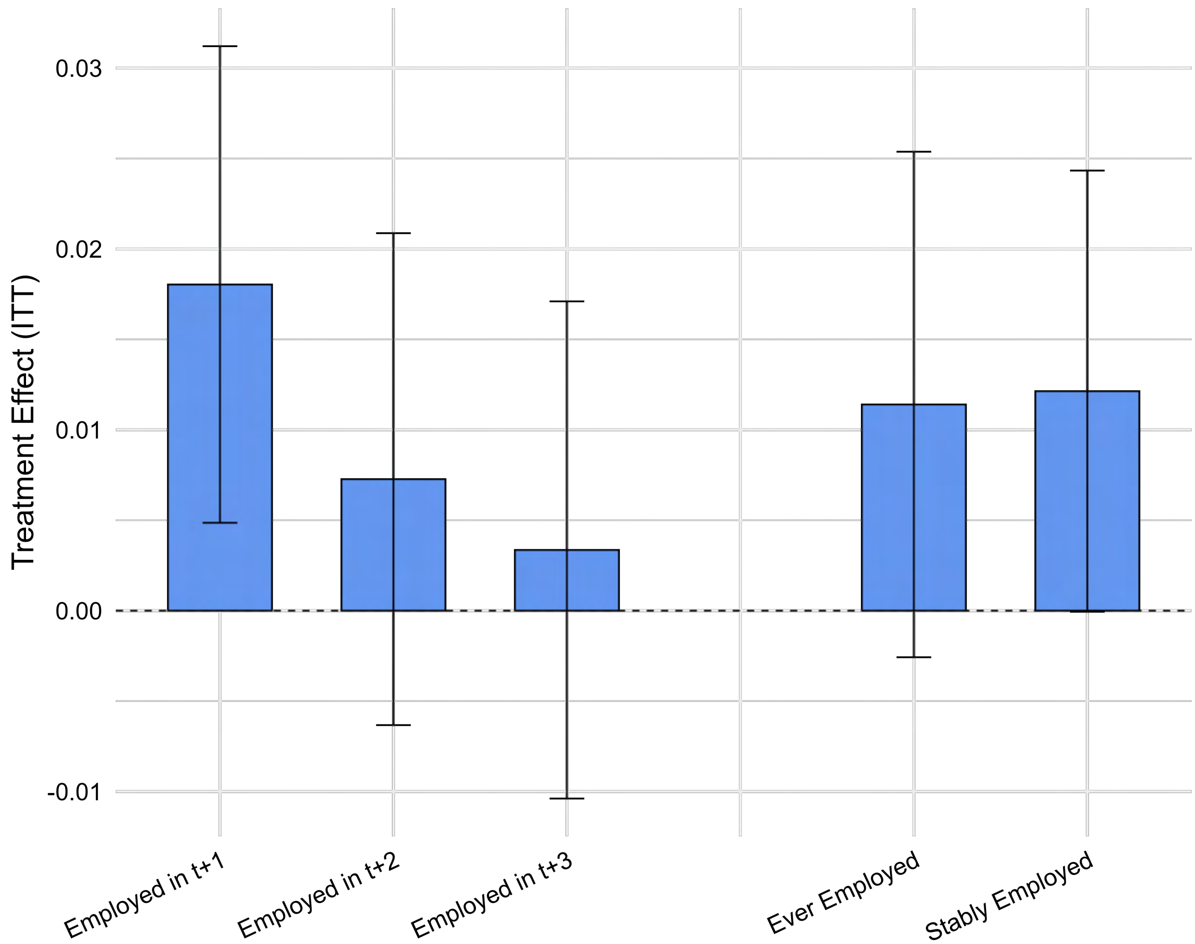
We have established why some workers do not claim UI benefits. We now turn to the implications of increasing take-up on job search behavior. In the job search model from Section 4, the effect of reducing barriers to claiming UI is theoretically ambiguous. On one hand, claiming UI raises the worker's reservation wage, which can prolong unemployment duration. On the other hand, receipt of UI comes with search requirements that may increase job-finding rates. Which force dominates depends on the size of the frictions. We test these predictions empirically and find no evidence that increasing UI take-up via informational outreach prolongs job search. In fact, treated workers are reemployed slightly sooner, and we detect no impact of treatment on reemployment wages.

6.1 Effects on Reemployment

We begin by evaluating the effect of receiving a letter on the likelihood of reemployment in the quarters following job loss. Figure 8 presents intent-to-treat (ITT) estimates of treatment effects on employment in the three quarters immediately following job loss ($t+1$, $t+2$, $t+3$) and indicators for ever being employed or stably employed in any of the three quarters. Point estimates are positive across all measures, suggesting that informational outreach did not slow workers' return to work and may have modestly accelerated it.

Treated individuals are 1.8 percentage points more likely to be employed in the quarter following job loss relative to a control mean of 33.3% ($p = 0.01$). Further, treated workers are 1.3 percentage points more likely to be ever employed post-job loss ($p = 0.06$), and 1.2 percentage points more likely to be stably employed ($p = 0.05$) relative to control means of 48.7% and 25.6%, respectively. While some estimates fall just short of statistical significance, the consistent pattern

Figure 8: Effects of Letters on Re-Employment Outcomes



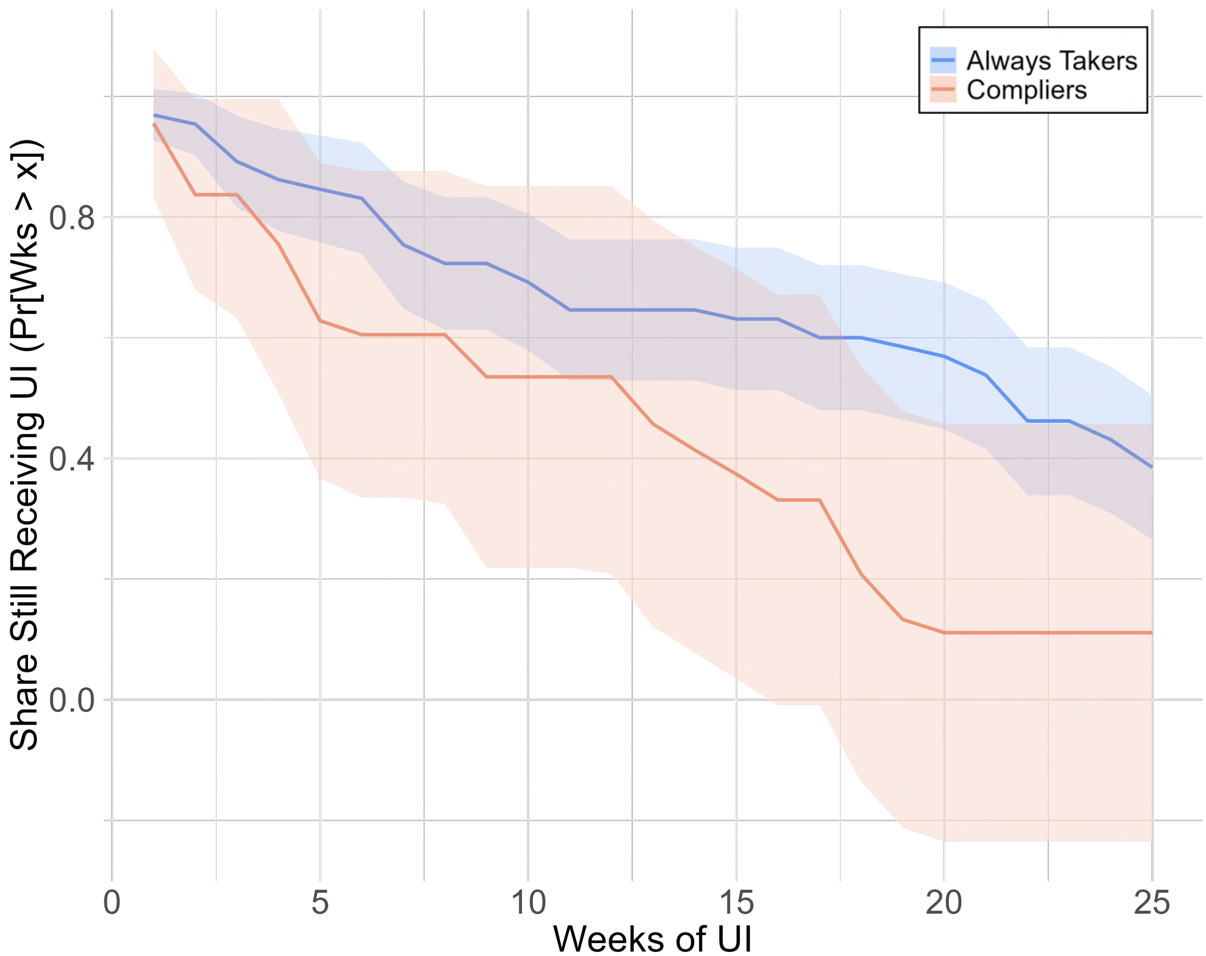
Note: Graph shows intent-to-treat (ITT) estimates on labor supply outcomes in quarters following job loss. Sample of 2024Q1–Q2 Potential Job Losses excluding "false positives," defined as those who recorded more hours at their potential separating firm in quarter $t + 1$ than in t and who had no employment at any other firm in $t + 1$. Bars represent 95% confidence intervals.

of positive effects allows us to rule out with reasonable confidence the possibility that letters delayed reemployment.

These positive results on reemployment are robust to a range of specifications. In Appendix Figure A.17b, we show that the positive treatment effects persist when controlling for pre-treatment characteristics such as age, gender, disability and veteran status, geography, and UI experience. The findings are also robust to using a dependent variable of hours worked after job loss as a continuous measure of employment rather than a binary indicator (Appendix Figure A.18b). Lastly, including recalls (defined in section 2.2) in the analysis sample raises the estimated effects: ITT estimates on subsequent employment increase to roughly 2 percentage points and are consistently significant at the 1% level (Appendix Figure A.17a).

Lastly, Figure 9 shows UI survival curves that illustrate that complier recipients exit the program more quickly than always-taker recipients, with only about 10% of compliers exhausting the maximum 26 weeks of benefits compared to roughly 40% of always takers. This reinforces the earlier reemployment results that treated claimants return to work more quickly rather than prolong their unemployment.

Figure 9: Survival Curve on UI for Always Takers vs. Compliers



Note: *Note*: Graph shows UI duration survival curve for complier (red) and always-taker (blue) recipients by week. The maximum potential benefit duration of UI in Washington is 26 weeks; therefore, the share still receiving UI after 25 weeks is the last point in the support.

IV Estimates: Local Labor Supply Effects of UI Receipt

While intent-to-treat estimates show that informational outreach does not slow reemployment, the key parameter of interest through the lens of our search model is the causal effect of UI receipt on labor supply. In the model, receipt (not application) affects reservation wages and search behavior, so we therefore instrument UI receipt with randomized letter assignment. This two-stage least squares (2SLS) approach estimates the local average treatment effect of UI receipt on reemployment among compliers.

The IV estimates, reported in Appendix Figure A.18a, yield large positive point estimates—for example, receipt is associated with a roughly 1 percentage point increase in employment in the two quarters following job loss, relative to a base of about 0.4. These estimates are imprecise, with wide confidence intervals and a first-stage F-statistic around 7.4, so they should be interpreted with caution. Tests of message variants provide no evidence that letters directly altered inframarginal workers' job search, lending support to the exclusion restriction (Appendix Table A.10). Overall, the IV results are suggestive as they reinforce our ITT evidence, but the magnitudes are too noisy to draw firm conclusions. Full details and robustness checks are reported in the Appendix A.2.1.

Taken together, these findings show that informational outreach aimed at increasing UI take-up does not prolong job search duration. If anything, it modestly accelerates reemployment.

6.2 Effects on Reemployment Earnings

Since our model predicts that UI receipt raises reservation wages, treatment could plausibly increase accepted wages among reemployed workers. However, Appendix Table A.4 reports zero effects of informational outreach on earnings and hourly wages (both point-in-time and cumulative) in all follow-up quarters. Point estimates are small, tightly clustered around zero, and statistically insignificant throughout. For example, two quarters after job loss, the effect on cumulative earnings is -12 dollars (SE = 293) off of a control group mean of $17,374, and the effect on post-separation hourly wage is -0.09 (SE = 0.22) off of a control group mean of $26.80.

6.3 Analysis of Other Forces Driving Labor Supply Effects

Standard job search theory would predict that reducing the fixed cost of applying for UI should prolong unemployment as newly induced claimants raise their reservation wages and become more selective in accepting job offers. However, the search model from section 4 accounts for the fact that the informational outreach bundles access to UI benefits with institutional features that increase search intensity. Our modest positive labor supply effects are consistent with evidence from UI experiments in the 1970s and 1980s whereby randomly provided job search assistance and eligibility enforcement (holding fixed benefits) reduced UI duration and increased reemployment among UI claimants (Meyer, 1995).[24]

[24] We abstract from general equilibrium effects in interpreting these results. As shown in Karahan, Mitman, and Moore (2025), expanding UI can reduce vacancy posting if employers anticipate tighter hiring conditions due to more selective job seekers. However, such equilibrium effects are unlikely to operate in our setting: the outreach

The remainder of this subsection explores additional explanations for the modest positive labor supply effects. We assess whether greater engagement with public employment services among UI claimants plays a role, and we evaluate whether our findings reflect the selected nature of our experimental sample — unemployed workers who have not claimed UI within 2–4 months of separation — whose job search behavior may be less responsive to UI benefits.

Public Employment Offices Do Not Mediate Reemployment Effects

Workers induced to claim UI may have been more likely to access public reemployment services through Washington's WorkSource offices. Such services can accelerate job finding by raising search effort or improving job matching efficiency (Fredriksson and Holmlund, 2006; Schiprowski et al., 2024; Cheung et al., 2025). We can directly test the extent to which our experimental sample engages with WorkSource offices and how it mediates job search. WorkSource services include searching for and applying for jobs, attending job fairs and workshops, creating or updating resumes, and participating in required programs such as Reemployment Services and Eligibility Assessment (RESEA). Appendix D.5 describes this data in further detail.

UI applicants are causally more likely to engage with any WorkSource service as a result of receiving any informational letter from ESD. Appendix Table A.11 shows results from a 2SLS regression that suggest that workers induced to apply for UI benefits are 37% more likely to engage with any WorkSource service and 18% more likely to search for a job while at a WorkSource office. They are 13% more likely to attend a job readiness or career services seminar or to update or create a resume. Reassuringly, the smallest causal effects of the letter are on RESEA activities (only marginally statistically significant), which is intuitive because RESEA conscripts *involuntary* claimants who attend only because they would not receive UI benefits otherwise. Appendix Table A.12 presents the reduced-form effect of the letter on the same WorkSource service outcomes directly and confirms the same finding that randomized treatment increased usage of public employment services.

However, mediation analysis in Table 4 suggests that these services do not mediate the positive effects on reemployment. Panel A illustrates the baseline positive effects on employment, while Panels B, C, and D add covariates for WorkSource service usage to test whether the estimated magnitude of the coefficient on letter treatment shrinks. In all cases, the magnitude of the estimated letter treatment coefficient remains the same when adding covariates for WorkSource usage; this suggests that public employment offices do not mediate the observed effects.

Lack of Disemployment Effects Are Not Driven by Sample Composition of Late Claimants

Lastly, we investigate whether the absence of disemployment effects in our experiment partially

intervention was not widely advertised, meaning firms would have no reason to adjust their vacancy posting behavior in response to the treatment.

Table 4: Mediation Analysis: WorkSource Engagement as a Channel for Employment Effects

	Dependent variable:				
	Employed in $t+1$	Employed in $t+2$	Employed in $t+3$	Ever Employed	Stably Employed
	(1)	(2)	(3)	(4)	(5)
Panel A. Univariate Regression of Employment on Treatment					
Letter Treatment	0.018***	0.007	0.003	0.011	0.012*
	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)
Panel B. Mediation: Regression of Employment on Treatment and Any WorkSource Engagement					
Letter Treatment	0.018***	0.007	0.003	0.011	0.013**
	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)
Any Service	0.034	0.014	0.028	0.156***	−0.104***
	(0.029)	(0.030)	(0.030)	(0.031)	(0.027)
Panel C. Mediation: Regression of Employment on Treatment and WorkSource Job Search Service					
Letter Treatment	0.018***	0.007	0.003	0.011	0.012**
	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)
Job Search Service or Activity	−0.048	0.015	0.058	0.146***	−0.137***
	(0.039)	(0.040)	(0.040)	(0.041)	(0.036)
Panel D. Mediation: Regression of Employment on Treatment and All Individual WorkSource Services					
Letter Treatment	0.018***	0.007	0.003	0.011	0.013**
	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)
Job Search Service or Activity	−0.118**	0.0003	0.058	0.068	−0.117**
	(0.051)	(0.052)	(0.053)	(0.054)	(0.047)
Attend Workshop/Career Services	−0.017	0.066	−0.005	−0.005	−0.007
	(0.067)	(0.069)	(0.070)	(0.071)	(0.062)
Create/Update Resume	0.035	−0.014	0.012	0.017	0.010
	(0.065)	(0.068)	(0.068)	(0.069)	(0.061)
Attend RESEA	0.078	−0.075	−0.123**	0.071	−0.106**
	(0.057)	(0.059)	(0.060)	(0.061)	(0.053)
Other Services	0.108**	0.046	0.062	0.143***	0.003
	(0.050)	(0.052)	(0.052)	(0.053)	(0.046)
Dep. Variable Mean	0.333	0.384	0.408	0.487	0.256
N	32,607	32,607	32,607	32,607	32,607

Note: Each column reports an OLS regression of the specified employment outcome on an indicator for letter treatment, with the inclusion of WorkSource service indicators for mediation analysis. Panel A reports the ITT of the letter without controls. Panel B adds a binary indicator for whether the claimant used any WorkSource service. Panel C includes an indicator for whether the claimant accessed any job-search–related WorkSource service. Panel D includes separate indicators for each WorkSource service category: job search assistance, workshops and career services, resume assistance, RESEA attendance, and other services. WorkSource variables are defined from administrative records described in Appendix D.5. The sample excludes individuals who were recalled to their separating employer. Robust standard errors in parentheses. *p<0.1; **p<0.05; ***p<0.01.

reflects differences in the type of workers induced to claim. Specifically, we test whether late claimants (i.e., unemployed workers who file 2–4 months after separation) are less responsive to UI generosity than claimants who file immediately. To do so, we estimate a regression kink design (RKD) using data from 2011Q3–2019Q1 to measure workers' sensitivity of claim duration to the maximum benefit cap. We define "late claimants" as workers whose initial claim has an effective date in quarters $t+1$ or $t+2$, when the job loss is assigned to quarter t, using the methodology from McQuillan and Moore (2025b) to identify likely UI-eligible job losses. By this definition, 19% of claimants near the benefit kink are late filers, while 81% are early.[25]

Late claimants are substantially less responsive to UI benefit levels compared to early claimants (Appendix Figure A.19). This suggests that the workers induced to claim in our experiment, who resemble late claimants, are less prone to the classic moral hazard response. To gauge how this affects labor supply magnitudes, we apply semi-elasticities from Figure A.19 for the average weekly benefit level ($550) to calculate that UI induces an extra 1.3 weeks of nonemployment for late claimants and 2.6 weeks for early claimants.[26] In the experiment, treated applicants receive UI for 16.3 weeks and control applicants for 17.9 weeks. Reconciling these numbers implies that search requirements in isolation offset about 2.9 weeks of unemployment duration. Although our design does not randomize work search requirements in isolation, this implied 2.9-week effect is remarkably consistent with prior experimental evidence from Washington State. Johnson and Klepinger (1994) show that eliminating the standard requirement of three weekly job search activities lengthened unemployment durations by 3.3 weeks.

This 2.9-week effect of work search requirements is strong enough to offset disemployment effects even if we extrapolate to early claimants who have larger semi-elasticities with respect to UI. In this case, predicted durations in the treated group would be 17.6 weeks, nearly identical to the 17.9 weeks observed in control. Thus, our findings indicate that work search requirements are powerful enough to fully counteract the disemployment effects one would expect if our experimental sample had the same benefit responsiveness as early claimants.

7 Quantitative Analysis of Search Model

This section describes how we estimate the take-up friction parameters in the search model from section 4 using Simulated Method of Moments (SMM) to match experimental application, rejection, and reemployment patterns.

[25] By virtue of claiming in $t+1$ or $t+2$ when we identified the job loss in quarter t, it may not be that this sample actually claimed late but that we did not identify their job loss well. This would also add noise to the RKD for late claimants that makes it seem that this group is less responsive.

[26] Calculation is $\frac{\$550}{100} \times 0.23 = 1.3$ and $\frac{\$550}{100} \times 0.47 = 2.6$.

7.1 Empirical Model

We augment the model described in Section 4 to be suitable for empirical estimation.

Eligibility Beliefs. The search model abstracted from specific functional forms of eligibility beliefs G_1 for separation-eligible workers and G_0 for separation-ineligible workers, aside from stating G_1 first-order stochastically dominates G_0. For empirical implementation, we impose a parametric structure to generate subjective eligibility belief values $\widehat{p}$. For eligibles, we assume $G_1 \sim \text{Beta}(8, 2)$, implying an expected belief $\mathbb{E}[\widehat{p}|e = 1] = 0.80$. For ineligibles, we assume $G_0 \sim \text{Beta}(2, 5)$, implying $\mathbb{E}[\widehat{p}|e = 0] = 0.29$.

These parameterizations capture the assumption that eligible workers are, on average, more confident about their eligibility than ineligible workers. Although the precise Beta parameters are necessarily subjective, our baseline parameterization is consistent with actual UI rejection rates.[27] Our results are robust to alternative values that preserve this ordering while still allowing for underconfidence among eligibles and overconfidence among ineligibles.

Search Cost-Type Beliefs. Similarly, section 4 describes belief distributions H_ℓ and H_h for low- and high-cost search types, with H_ℓ assumed to first-order stochastically dominate H_h. For estimation, we parameterize $H_\ell \sim \text{Beta}(9, 1)$ and $H_h \sim \text{Beta}(1.5, 3)$. Under this parameterization, low-cost types are highly confident in their status ($\mathbb{E}[\widehat{\pi}|s = \ell] = 0.9$, $\text{Var}(\widehat{\pi}|s = \ell) = 0.008$), whereas high-cost types are more uncertain ($\mathbb{E}[\widehat{\pi}|s = \ell] = 0.33$, $\text{Var}(\widehat{\pi}|s = \ell) = 0.04$).

Undeliverable and Unopened Letters. To better fit empirical moments, we must account for the fact that the experimental application rates are mechanically attenuated by imperfect delivery and take-up of the letters themselves. In particular, approximately 5% of mailings were returned as undeliverable to ESD (Appendix Table C.5), and marketing evidence suggests another 20% were likely never opened by recipients. This implies that even if all other frictions were accurately captured by the model, the observed treatment application rates in the experiment would necessarily fall short of those predicted by the model. While our search model in Section 4 abstracts from these delivery frictions, we incorporate them in the empirical implementation.

Wage Distribution. The search model requires a wage offer distribution $F(w)$, which we parameterize using the distribution of hourly wages in Washington in 2023.[28] For estimation, we discretize the distribution into \$1-per-hour bins, starting at the state minimum wage and extending up to \$150 per hour, at which point we truncate the support.

[27] According to the USDOL ETA Nonmonetary Determination Activities Report (ETA-207) from 2024Q4, 53% of separators who applied for UI nationally were determined to be eligible. In our parameterization, the population-wide eligibility belief is 55% ($0.52 \cdot 0.8 + 0.48 \cdot 0.29$).

[28] We use 2023 rather than 2024 data to avoid potential endogeneity with respect to our experiment.

7.2 Estimation

We seek to estimate the vector of parameters: $\Theta := \{\kappa, \Delta\kappa, \alpha, \Delta\alpha, \eta_\ell, \lambda_C, \lambda_N, \theta_\ell\}$. κ is the fixed cost of learning about UI and $\Delta\kappa$ is how much the letter reduced it. Similarly, α is the flow cost of stigma and $\Delta\alpha$ is how much the destigmatizing letter reduced it. η_ℓ is the cost of complying with search requirements, λ_C and λ_N are job offer arrival rates for claimants and nonclaimants, and θ_ℓ is the share of the separation-eligible population that is low-search cost. The moments $\Psi^d = \{App^C, App^G, App^S, R^C_{QF}, R^G_{QF}, R^S_{QF}, R^C_{NASW}, R^G_{NASW}, R^S_{NASW}, Emp^C, Emp^G, Emp^S\}$ are observed in the data from the field experiment, where C,G, and S denote the control, generic, and stigma treatment arms. App denotes application rates, R_{QF} and R_{NASW} denote reason-specific rejection rates (quit/fired and not actively seeking work), and Emp denotes re-employment rates.

A small set of values is calibrated: $X_i := \{\theta_e, \beta, b\}$. We set the weekly discount factor to $\beta = 0.995$; this corresponds to an annualized rate of 0.77 (0.995^{52}), and is broadly consistent with experimentally elicited discount rates in developed countries (Harrison, Lau, and Williams, 2002) as well as market interest rates on credit cards that households use to sustain liquidity during unemployment spells (Braxton, Herkenhoff, and M. Phillips, 2024). We calibrate $b =$ \$323, Washington's minimum weekly benefit during the experiment, and set $\theta_e = 0.52$ (see Appendix Table E.1), thus implying that just over half of workers are separation-eligible and thus draw eligibility beliefs from the more favorable G_1 distribution.

We estimate eight parameters from twelve moment conditions using SMM. The estimator minimizes the weighted distance between model-implied and empirical moments:

$$\hat{\Theta} = \arg\min_{\Theta} \left[\Psi^d - \Psi^s(\Theta)\right]' W \left[\Psi^d - \Psi^s(\Theta)\right] \tag{7}$$

where Ψ^d is the vector of empirical moments, $\Psi^s(\Theta)$ are the corresponding model-simulated moments at parameter vector Θ, and W is a positive definite weighting matrix. We use the optimal weighting matrix given by the inverse of the variance–covariance matrix of the empirical moments, ensuring the efficiency of the estimator (McFadden, 1989). Intuitively, the estimator selects the parameter values that make simulated application, rejection, and employment outcomes resemble those observed in the experiment. Formal identification of each parameter is derived in Appendix B.2.

Because the model has no closed-form solution for $\Psi^s(\Theta)$, we compute simulated moments numerically. We discretize the state space and simulate a large sample of workers under the model for each candidate Θ. Optimization is performed using the Limited-memory Broyden-Fletcher-Goldfarb-Shanno (L-BFGS) algorithm, which is well suited for high-dimensional, nonlinear optimization. Standard errors are obtained by bootstrapping the empirical moments, repeating the estimation procedure across 500 resampled datasets.

7.3 Primitive Estimates

To quantify these frictions, we estimate the structural search model to recover the dollar value of learning costs, stigma costs, and search compliance costs. Table 5 reports these estimates. The model delivers reasonable parameters, with the fixed cost of learning estimated at $3,205 — roughly six times the average weekly benefit value. Letters reduce this fixed learning cost by $377, or about 18%. The stigma cost parameter is estimated at $93 per week, though the destigmatizing message reduces it only marginally ($7). The estimated cost of complying with work search requirements is $202 per week, a large enough estimate to rationalize why high-search cost types choose to not comply with verification. Finally, the calibrated job offer arrival rates imply offers once every 8.5 weeks on average, with claimants facing a 3% faster weekly arrival rate (0.117) than non-claimants (0.114), reflecting the role of search requirements in raising search intensity.

To evaluate model fit, Panel B compares simulated moments with their empirical targets. While not exact, the fit is close across applications, rejections, and reemployment rates. For example, the model accurately predicts reemployment after three quarters and the share of applications rejected due to search requirements, but it underpredicts the share rejected for quits and firings. Importantly, all estimated primitives should be interpreted as local to the post-mailing non-claimant population (i.e., workers who are likely unemployed 2–4 months), since our experiment excludes early claimants who filed immediately upon separation. As a result, the estimated levels of κ and α are likely higher than population-wide values, reflecting the fact that easier-to-reach claimants exited prior to treatment.

Panel C reports untargeted moments and provides additional insight into the model's mechanisms. Generic treatment raises application rates primarily among eligible low-search cost types ($e = 1, s = \ell$), who increase to 67% from 36% in the control. However, the proportional increase is much larger among high-cost eligibles ($e = 1, s = h$) and ineligibles ($e = 0$), whose application rates quadruple, albeit from low baselines. This explains why rejection rates rise sharply among complier applicants relative to always takers: the intervention not only induced more eligible applications, but also drew in applicants disproportionately likely to be screened out. At the same time, the model predicts faster monthly job finding rates for claimants than for non-claimants (0.065 vs. 0.063), consistent with the data.[29] This departure from standard search theory reflects the dual role of work search requirements, which increase reservation wages but simultaneously raise search effort, leaving job finding rates unchanged.

[29] Studies on duration dependence in the U.S. suggest typical monthly job finding rates in the range of 8-15% for workers unemployed between 2–10 months (Kroft, Lange, and Notowidigdo, 2013).

Table 5: Empirical Search Model: Parameter Estimates and Model Fit

A. Parameter Estimates

Description	**Parameter**	**Estimate**
Learning Cost	κ	\$3,205
		(285)
Learning Cost Reduction from Letter	$\Delta\kappa$	\$584
		(122)
Stigma Cost	α	\$93
		(13)
Stigma reduction (destig. letter)	$\Delta\alpha$	\$7
		(5)
Search Compliance Cost	η_ℓ	\$202
		(54)
Job offer arrival claimants	λ_C	0.117
		(0.004)
Job offer arrival non-claimants	λ_N	0.114
		(0.004)
Share low search cost among eligibles	θ_ℓ	0.258
		(0.022)

B. Model Fit

Selected Moment	**Estimated Value**	**Targeted Value**
Control Application Rate	0.051	0.017
Control Rejection | Application: Quit/Fire (QF)	0.015	0.210
Control Rejection | Application: Not Searching ($NASW$)	0.048	0.050
Control Re-Employment (after 3 quarters)	0.452	0.435

C. Implied Behavior (Untargeted)

Quantity	**Control**	**Generic**
Application Rate from eligible low-cost ($e = 1, s = \ell$)	0.356	0.666
Application Rate from eligible high-cost ($e = 1, s = h$)	0.006	0.035
Application Rate from ineligible ($e = 0$)	0.002	0.010
Claimant Job Finding Rate (monthly)	0.065	0.065
Non-Claimant Job Finding Rate (monthly)	0.063	0.063
Reservation Wage (hourly)	\$80.08	\$80.67

Note: The model is solved in discrete time at a weekly frequency. Monthly job finding rates are translated from weekly job finding hazards implied by the model. Full set of targeted moments and estimated values are listed in Appendix Table A.16. Standard errors calculated via bootstrapping, are in parentheses in panel A.

7.4 Welfare Analysis of Experiment

We can evaluate the welfare implications of the experiment by decomposing the 1,277 applicants among the 41,199 treated potential job losses into four groups depending on whether they would have applied absent the letter (inframarginal vs. marginal) and whether they received benefits (approved vs. denied).

i) Inframarginal denied applicants (20% of treated applicants) gain $\Delta\kappa$ each, since they would have applied regardless but save part of the fixed learning cost.

ii) Inframarginal approved applicants (35% of treated applicants) gain $\Delta\kappa$ each for the same reason, and those who receive a destigmatizing letter also benefit from a lower flow stigma cost $\Delta\alpha$ for the average number of weeks on UI for inframarginal claimants.

iii) Marginal denied applicants (30% of treated applicants) lose $(\kappa - \Delta\kappa)$ each, as they are induced to apply but are rejected.

iv) Marginal approved applicants (15% of treated applicants) gain the net value of UI benefits (total amount of UI less the flow costs) minus the fixed application cost $(\kappa - \Delta\kappa)$. Those who received the destigmatizing letter also receive $\Delta\alpha$ each week on UI.[30]

Because utility is linear, we can sum across these four groups to obtain the overall net welfare impact of the letters, which combines gains from reduced learning costs and stigma along with new UI transfers, and is offset by the costs of newly induced unsuccessful applications.

Welfare analysis indicates that the intervention generates positive net gains, as the combined benefits to inframarginal applicants (who incur fewer learning costs and, if approved, stigma costs) and marginal approved applicants exceed the costs borne by marginal denied applicants. Our baseline parameterization suggests that the experiment delivered \$58,717 of net welfare. Gross welfare gains total about \$1.09 million, with roughly 59% (\$637k) coming from marginal approved applicants and the remainder split between inframarginal approved (\$303k) and inframarginal denied (\$147k). These gross gains are largely but not entirely offset by \$1.03 million in welfare losses due to fixed learning costs incurred by induced applicants who never receive benefits.

Table 6 also illustrates that among marginal applicants alone, welfare is negative: although marginal approved applicants gain substantially from UI, they are outnumbered by marginal denied applicants by nearly two to one. Nevertheless, overall net welfare from the experiment remains positive across all belief specifications since the intervention also reduces fixed learning

[30] We cap the number of weeks a worker can draw UI at 26, even if the model suggests longer durations.

Table 6: Field Experiment Welfare Decomposition under Alternative Belief Specifications

		Low-Cost Type ($s = \ell$)		**Eligible Type** ($e = 1$)	
	Baseline	**More Conf.**	**Less Conf.**	**More Conf.**	**Less Conf.**
Belief Distributions Among					
Eligible G_1	Beta(7,3)	Beta(7,3)	Beta(7,3)	Beta(8,2)	Beta(4,2)
Ineligible G_0	Beta(2,5)	Beta(2,5)	Beta(2,5)	Beta(2,5)	Beta(2,5)
Low-search cost H_ℓ	Beta(7,1)	Beta(9,1)	Beta(6,1)	Beta(7,1)	Beta(7,1)
High-search cost H_h	Beta(1.5,3)	Beta(1.5,3)	Beta(1.5,3)	Beta(1.5,3)	Beta(1.5,3)
Primitive Estimate					
κ	\$3,205	\$3,249	\$2,955	\$2,961	\$3,045
$\Delta\kappa$	\$584	\$554	\$430	\$413	\$384
α	\$93	\$100	\$99	\$99	\$16
$\Delta\alpha$	\$7	\$20	\$4	\$1	\$7
Welfare					
Inframarg. Denied (N=252)	\$147,163	\$139,681	\$108,323	\$104,275	\$96,854
Inframarg. Approved (N=449)	\$303,166	\$367,244	\$213,834	\$188,876	\$213,138
Marg. Denied (N=392)	−\$1,028,216	−\$1,057,117	−\$990,501	−\$999,057	−\$1,043,648
Marg. Approved (N=185)	\$636,605	\$751,457	\$740,939	\$723,853	\$1,223,426
Total	\$58,717	\$201,265	\$72,595	\$17,947	\$489,769

Note: Welfare measured in 2024 USD. Negative entries reflect losses from unsuccessful applications. Each column corresponds to an alternative specification of belief distributions for low-search or eligible workers.

costs for the many inframarginals who would have applied regardless and lowers stigma costs for those who would have been approved in any case.

Table 6 shows that we obtain similar qualitative conclusions about welfare as we change the specific parameterization of beliefs. Whether we make low-search cost types more or less confident about their true status ($s = \ell$) and eligible types more or less confident about their true status ($e = 1$) compared to the baseline, net welfare from the experiment is always positive, although magnitudes vary from as low as \$18,000 to as high as \$490,000. Welfare increases when low-search cost types are more confident than their baseline parameterization or when eligible types are less confident. Estimates of κ are always around \$3,000 and $\Delta\kappa$ range from \$400–\$600.

Accounting for printing and postage (\$0.30 and \$0.50 per letter, respectively) for 41,199 mailings and the 0.1 full-time equivalent (FTE) call center staff to handle the additional call volume, the total administrative cost of the intervention was approximately \$40,959.[31] Even allowing for additional fixed management costs to oversee implementation (which we cannot quantify and

[31] We estimate the salary and health benefits of one FTE Washington ESD call center agent at \$80,000 per year.

thus do not account for here), the overall welfare impact of the intervention would likely remain positive. While the outreach experiment was designed to learn about UI claiming and search behavior rather than maximize cost effectiveness, these figures suggest that its economic benefits outweighed its administrative costs.

7.5 Policy Counterfactuals

Finally, the model allows us to study how counterfactual policies affect unemployed workers' UI applications, receipt, and reemployment. The three policies we simulate are reducing (or eliminating) costs of learning κ, stigma α, and work search compliance η_ℓ. In all three cases, reductions in frictions affect not only the level of take-up but also its composition between approved and denied applications. This arises because workers are heterogeneous in eligibility beliefs and compliance costs. Beyond a certain point, further easing of frictions primarily induces applications that do not result in UI receipt yet nonetheless require costly administrative processing. As a result, welfare can be non-monotone in the degree to which frictions are reduced.

Learning Costs. Reducing fixed learning costs κ substantially reshapes application behavior. As shown in Figure 10, applications from truly eligible (low search-cost) workers surge with modest reductions in learning costs and lead to welfare gains. Welfare gains plateau around a 40% reduction once nearly all eligibles have applied, and further reductions primarily attract high-search cost eligibles and inelgibles whose claims are denied, leading to marginal welfare losses. For very large reductions in κ, even those with very pessimistic beliefs about eligibility or compliance choose to apply because there is nothing to lose; this is consistent with the decision rule expressed in equation (4), where $\kappa = 0$ suggests all workers apply.

Lowering learning costs has a negligible effect on reemployment hazards because κ is a fixed entry cost and does not affect post-application dynamics. Eliminating learning costs entirely (Appendix Table A.17) yields a virtually undetectable increase in the three-quarter reemployment rate because more claimants (who have faster reemployment due to job search requirements) enter the system (this is a difference that only appears in the fourth decimal place).

Stigma Costs. By contrast, reducing stigma costs produces a more muted rise in application rates compared to reductions in learning costs, but the induced applicants are more likely to be eligible. As shown in Figure 10, lowering stigma modestly increases the perceived value of applying for UI and encourages more eligible workers to apply while keeping application rates among high-search-cost and ineligible workers very low. When stigma is halved, about 80% of eligibles apply but fewer than 10% of high-cost or ineligible workers do so — this implies a substantial increase in UI receipt (to 85% of eligibles claiming) despite only modest overall application growth (to 16% of all unemployed applying). Because most new applicants qualify for benefits, rejection rates fall and welfare rises steadily. However, as stigma declines further, welfare gains flatten and even slightly decrease: once most eligibles have applied, additional reductions in stigma mainly attract ineligible applicants whose claims are denied and generate net welfare losses on the margin.

Lowering stigma also increases the flow value of UI: it induces longer durations on benefits and slower reemployment among claimants. As the guilt of claiming vanishes, remaining on UI becomes more attractive relative to working. Appendix Table A.17 illustrates this pattern: while baseline claimants have a three-quarter reemployment rate of 45.9%, this falls to 44.9% when stigma is eliminated. Because more workers also become claimants, the aggregate three-quarter reemployment rate declines slightly from 45.3% to 45.2%. Thus, reducing stigma expands access while modestly extending unemployment durations through higher reservation wages.

Search Compliance Costs. Reducing search compliance costs η_ℓ produces dynamics similar to those for stigma, as both represent recurring flow costs that affect the ongoing value of claiming benefits rather than the fixed cost of applying. As shown in Figure 10, smaller reductions in the search compliance costs raises applications (mostly from eligibles) and welfare per capita rises accordingly. Once most eligibles have applied, further reductions in search compliance costs induce high-search cost applicants to apply at disproportionate rates, imposing a cost on the government that processes their denied claims and thus reducing welfare per capita at the margin. However, as work search requirements are waived entirely (i.e. in the limit as η_ℓ approaches zero), all eligible workers can now satisfy job-search verification requirements at no cost. This dynamic produces a corner solution where welfare once again increases, as essentially everyone who is eligible applies and receives UI without the pain of search verification (Appendix Table A.17), and the government does not need to implement costly monitoring of work search.[32]

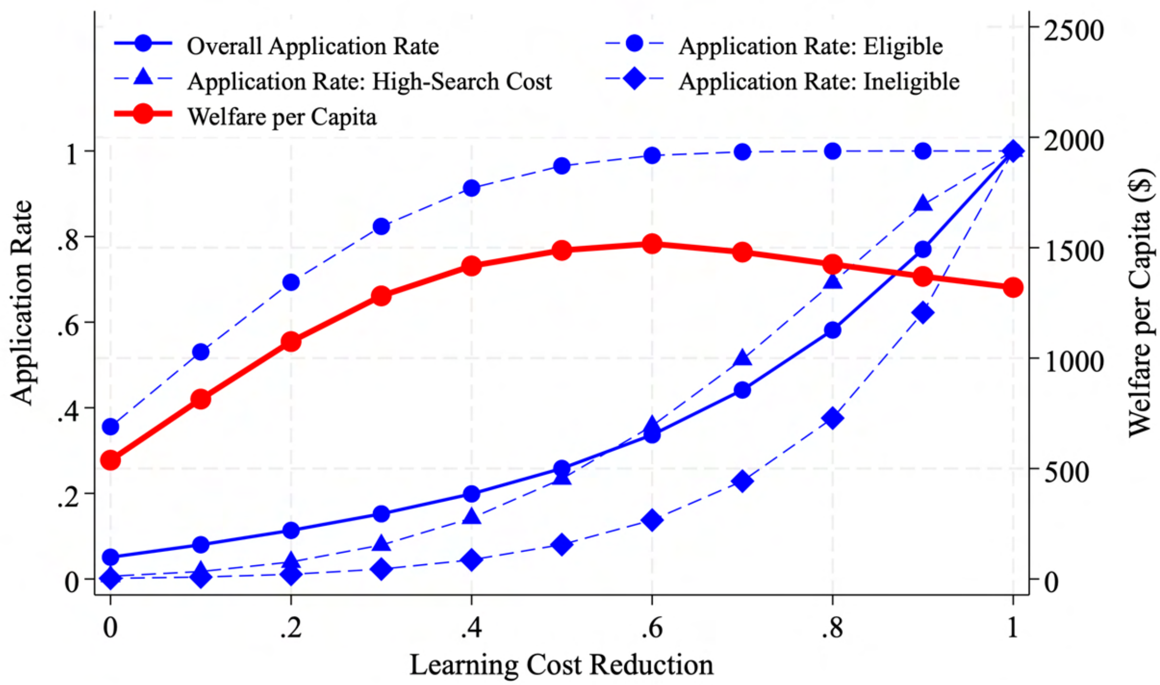
A 50% reduction in search compliance generates greater welfare than a 50% reduction in stigma, since relaxing search requirements directly increases the flow value of UI and makes it more attractive to a broader group of eligible workers who can comply with the search requirements (i.e., a higher θ_ℓ). Although we do not point-identify the search cost for high-cost eligibles (η_h) since these workers are, by definition, unwilling to comply with UI's requirements, the model still implies that many would be induced to apply with a lower η_ℓ because the share of eligibles who are low-search cost (θ_ℓ) and the application threshold in equation (4) jointly determine entry along the margin.[33]

Predictably, relaxing search compliance requirements also lowers reemployment hazards by

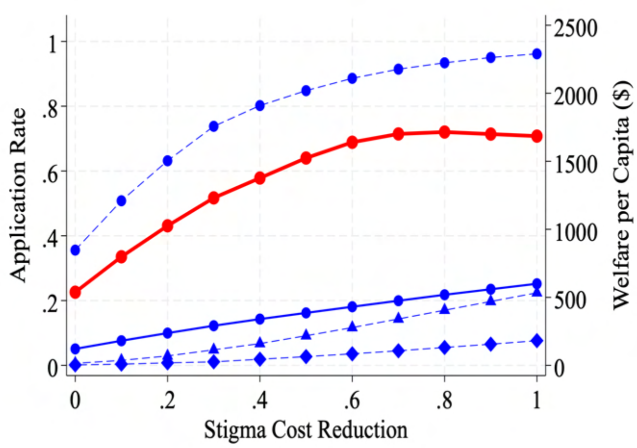
[32]In the baseline version of the empirical exercise, welfare calculations only consider the take-up decision given the frictions and amount of UI benefits to be claimed. A natural extension would consider how welfare is affected by faster or slower labor market re-entry as a result of reduced frictions.

[33]Intuitively, as search compliance becomes less costly, θ_ℓ (the share of eligibles who are low-search cost) rises and the threshold for applying shifts — this induces additional high-cost types whose beliefs lie near the margin.

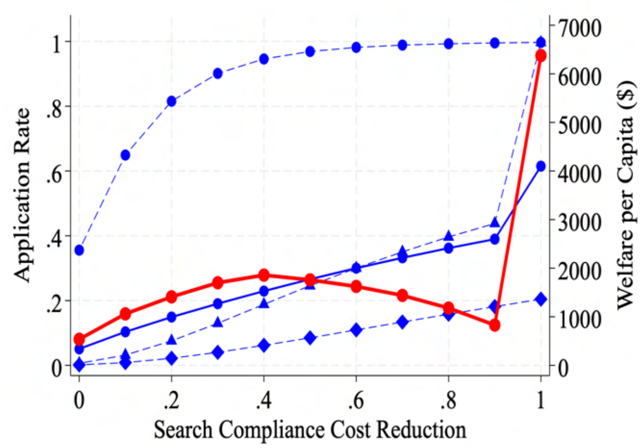
Figure 10: Welfare and Application Rate Counterfactuals Under Friction Reductions



(a) Learning Cost



(b) Stigma Cost



(c) Search Compliance Cost

Note: This figure plots model-implied counterfactuals as each friction to UI take-up (learning costs κ, stigma costs α, and search-compliance costs η_ℓ) is progressively reduced. Blue markers show overall and type-specific application rates (low-cost eligibles, high-cost eligibles and ineligibles), while the red line shows welfare per unemployed worker. High-search cost eligibles are always rejected for UI except in the limit case where $\eta_\ell = 0$ (panel (c)).

reducing the disutility of unemployment on UI. Halving η_ℓ has effects comparable to fully eliminating stigma costs — it modestly lowers job-finding rates and the three-quarter reemployment hazard for claimants from 45.9% to 45.0% (Appendix Table A.17).[34] When search costs are eliminated entirely, claimants' reemployment hazard falls further to 44.9%, though the aggregate effect on overall reemployment remains minimal, declining only from 45.3% to 45.2%.

Welfare-Maximizing Policies. Although all three counterfactual policies expand UI access, they differ in their welfare consequences conditional on the size of that expansion. Figure A.21 plots how the welfare of unemployed workers changes with the target UI receipt rate when the government reduces learning, stigma, or search compliance costs to achieve each level of UI receipt. The baseline receipt rate (with full frictions) in the model is 38%, while the figure traces welfare outcomes as receipt rises from 38% to 100% through reductions in each friction.

Reducing search compliance costs yields the largest welfare gains at nearly every level of UI expansion, followed closely by reductions in stigma costs.[35] Both policies raise the flow value of unemployment by easing recurring frictions that discourage claiming. In contrast, reducing learning costs produces smaller welfare improvements for a given increase in receipt, since more additional applicants are drawn from ineligible or high–search-cost groups whose claims are denied. These dynamics are mirrored in Appendix Figure A.20b, which shows that learning-cost reductions generate the largest surge in applications (and hence the highest rejection rates). In all cases (Figure A.21), welfare is increasing at lower receipt targets but flattens and can decline at high targets once expansion occurs mainly through denied applications.

8 Conclusion

Our findings highlight the important role of fixed and continuing barriers to applications in depressing take-up of UI. By implementing a large-scale, randomized intervention in partnership with a state workforce agency, we show that informational outreach letters significantly increase applications and benefit receipt without negatively affecting job search behavior. Letter treatment effects are concentrated among lower-wage workers, and rejection patterns among applicants suggest that reduced learning costs, rather than improved eligibility beliefs, is the primary mechanism by which take-up increases. A destigmatizing message increases applications primarily among higher-wage workers, suggesting that stigma selectively deters take-up

[34] In practice, some states, while requiring weekly search, allow for biweekly reporting, which may approximate such a policy. See Table 5-15 in `https://oui.doleta.gov/unemploy/pdf/uilawcompar/2023/complete.pdf`.

[35] For levels of UI receipt just short of 100% when easing search requirements, all eligibles have already applied, but the easier search requirements induce so many high-cost types (who still get rejected) to apply that welfare falls markedly, making relaxing search requirements the least desirable policy.

for individuals less accustomed to public benefits.

These results have both theoretical and practical significance. Standard models of unemployment assume frictionless take-up of UI, but our setting demonstrates how incorporating application costs and search requirements changes the predicted effects of UI access expansion. Experimentally, we show that increasing UI access through targeted, low-cost outreach generates private returns for low-wage populations and modest positive labor market consequences.

Lastly, our paper provides important evidence on the role of UI's work search requirements, which are particularly important when considering moving unemployed workers on the extensive margin (McQuillan and Moore, 2025a). In our context, work search requirements both deter many complier applicants from receiving UI and induce faster job search among those who are induced to receive. However, our setting did not have experimental variation in search requirements or information about them, which would be a promising approach for future research. This tradeoff between administrative stringency that screens out applicants and conditions that promote reemployment sits at the core of the UI programs of the United States and warrants careful consideration by policymakers, particularly as Congress has expanded work search requirements for traditional welfare programs.

References

ANDERS, J. AND C. RAFKIN (2022): *The Welfare Effects of Eligibility Expansions: Theory and Evidence from SNAP.*, SSRN.

ANDERSON, P. M. AND B. D. MEYER (1997): "Unemployment insurance takeup rates and the after-tax value of benefits," *The Quarterly Journal of Economics*, 112, 913–937.

AURAY, S., D. L. FULLER, AND D. LKHAGVASUREN (2019): "Unemployment insurance take-up rates in an equilibrium search model," *European Economic Review*, 112, 1–31.

BHARGAVA, S. AND D. MANOLI (2015): "Psychological frictions and the incomplete take-up of social benefits: Evidence from an IRS field experiment," *American Economic Review*, 105, 3489–3529.

BIRINCI, S. AND K. SEE (2023): "Labor market responses to unemployment insurance: The role of heterogeneity," *American Economic Journal: Macroeconomics*, 15, 388–430.

BITLER, M., J. COOK, D. HORN, AND N. SEEGERT (2022): "Incomplete program take-up during a crisis: Evidence from the COVID-19 shock in one US state," *International Tax and Public Finance*, 29, 1373–1394.

BLANK, R. AND D. CARD (1991): "Recent Trends in Insured and Uninsured Unemployment: Is There an Explanation?" *The Quarterly Journal of Economics*, 106.

BLASCO, S. AND F. FONTAINE (2021): "Unemployment duration and the take-up of unemployment insurance," Tech. rep., IZA Discussion Papers.

BRAXTON, J. C., K. HERKENHOFF, AND G. M. PHILLIPS (2024): "Can the unemployed borrow? implications for public insurance," *Journal of Political Economy*, 132, 3025–3076.

CASTELL, L., M. GURGAND, C. IMBERT, AND T. TOCHEV (2025): "Take-up of social benefits: experimental evidence from France," *American Economic Journal: Economic Policy*.

CHEUNG, M., J. EGEBARK, A. FORSLUND, L. LAUN, M. RÖDIN, AND J. VIKSTRÖM (2025): "Does job search assistance reduce unemployment? Evidence on displacement effects and mechanisms," *Journal of Labor Economics*, 43, 47–81.

CHODOROW-REICH, G. AND L. KARABARBOUNIS (2016): "The cyclicality of the opportunity cost of employment," *Journal of Political Economy*, 124, 1563–1618.

COHEN, J. P. AND P. GANONG (2024): "Disemployment effects of unemployment insurance: A meta-analysis," Tech. rep., National Bureau of Economic Research.

CURRIE, J. (2004): "The take up of social benefits," .

DESHPANDE, M. AND Y. LI (2019): "Who is screened out? Application costs and the targeting of disability programs," *American Economic Journal: Economic Policy*, 11, 213–48.

EAST, C. N. AND D. SIMON (2024): "The safety net and job loss: How much insurance do public programs provide?" *Journal of Public Economics*, 238, 105171.

EBENSTEIN, A. AND K. STANGE (2010): "Does inconvenience explain low take-up? Evidence from unemployment insurance," *Journal of Policy Analysis and management*, 29, 111–136.

FINKELSTEIN, A. AND M. J. NOTOWIDIGDO (2019): "Take-up and targeting: Experimental evidence from SNAP," *The Quarterly Journal of Economics*, 134, 1505–1556.

FORSYTHE, E. (2021): "Understanding Unemployment Insurance Recipiency During the Covid-19 Pandemic," .

FREDRIKSSON, P. AND B. HOLMLUND (2006): "Optimal unemployment insurance design: Time limits, monitoring, or workfare?" *International Tax and Public Finance*, 13, 565–585.

FRIEDRICHSEN, J., T. KÖNIG, AND R. SCHMACKER (2018): "Social image concerns and welfare take-up," *Journal of Public Economics*, 168, 174–192.

FUJITA, S. AND G. MOSCARINI (2017): "Recall and unemployment," *American Economic Review*, 107, 3875–3916.

GANONG, P. AND P. NOEL (2019): "Consumer spending during unemployment: Positive and normative implications," *American Economic Review*, 109, 2383–2424.

GIANNELLA, E., T. HOMONOFF, G. RINO, AND J. SOMERVILLE (2024): "Administrative burden and procedural denials: Experimental evidence from SNAP," *American Economic Journal: Economic Policy*, 16, 316–340.

GRUBER, J. (1997): "The consumption smoothing benefits of unemployment insurance," *The American Economic Review*, 87, 192.

——— (2001): "The wealth of the unemployed," *ILR Review*, 55, 79–94.

HARRISON, G. W., M. I. LAU, AND M. B. WILLIAMS (2002): "Estimating individual discount rates in Denmark: A field experiment," *American economic review*, 92, 1606–1617.

HEMMETER, J., J. PHILLIPS, E. SAFRAN, AND N. WILSON (2025): "Communicating program eligibility: A Supplemental Security Income (SSI) field experiment," *American Economic Journal: Economic Policy*, 17, 446–470.

HERTEL-FERNANDEZ, A. AND J. B. WENGER (2013): "Taking up social benefits: A cautionary tale from an unemployment insurance survey experiment," *Available at SSRN 2341885*.

HOMONOFF, T. AND J. SOMERVILLE (2021): "Program recertification costs: Evidence from SNAP," *American Economic Journal: Economic Policy*, 13, 271–298.

ISELIN, J., T. MACKAY, AND M. UNRATH (2023): "Measuring take-up of the California EITC with state administrative data," *Journal of public economics*, 227, 105002.

JÄGER, S., B. SCHOEFER, S. YOUNG, AND J. ZWEIMÜLLER (2020): "Wages and the Value of Nonemployment," *The Quarterly Journal of Economics*, 135, 1905–1963.

JOHNSON, T. R. AND D. H. KLEPINGER (1994): "Experimental evidence on unemployment insurance work-search policies," *Journal of Human Resources*, 695–717.

JOHNSTON, A. C. AND A. MAS (2018): "Potential unemployment insurance duration and labor supply: The individual and market-level response to a benefit cut," *Journal of Political Economy*, 126, 2480–2522.

KARAHAN, F., K. MITMAN, AND B. MOORE (2025): "Micro and Macro Effects of Unemployment Insurance Policies: Evidence from Missouri," *Journal of Political Economy*, 133, 2836–2873.

KATZ, L. F. AND B. D. MEYER (1990): "The impact of the potential duration of unemployment benefits on the duration of unemployment," *Journal of Public Economics*, 41, 45–72.

KO, W. AND R. A. MOFFITT (2024): "Take-up of social benefits," *Handbook of Labor, Human Resources and Population Economics*, 1–42.

KROFT, K. (2008): "Takeup, social multipliers and optimal social insurance," *Journal of Public Economics*, 92, 722–737.

KROFT, K., F. LANGE, AND M. J. NOTOWIDIGDO (2013): "Duration dependence and labor market conditions: Evidence from a field experiment," *The Quarterly Journal of Economics*, 128, 1123–1167.

KROFT, K. AND M. J. NOTOWIDIGDO (2016): "Should unemployment insurance vary with the unemployment rate? Theory and evidence," *The Review of Economic Studies*, 83, 1092–1124.

KUKA, E. AND B. A. STUART (2025): "Racial inequality in unemployment insurance receipt," *Journal of Public Economics*, 247, 105401.

LACHOWSKA, M., A. MAS, AND S. A. WOODBURY (2022): "How reliable are administrative reports of paid work hours?" *Labour Economics*, 75, 102131.

LACHOWSKA, M., I. SORKIN, AND S. A. WOODBURY (2025): "Employers and Unemployment Insurance Take-Up," *American Economic Review*, 115, 2529–2573.

LANDAIS, C. AND J. SPINNEWIJN (2021): "The value of unemployment insurance," *The Review of Economic Studies*, 88, 3041–3085.

LASKY-FINK, J. AND E. LINOS (2024): "Improving delivery of the social safety net: The role of stigma," *Journal of Public Administration Research and Theory*, 34, 270–283.

LEMIEUX, T. AND W. B. MACLEOD (2000): "Supply side hysteresis: the case of the Canadian unemployment insurance system," *Journal of Public Economics*, 78, 139–170.

LINOS, E., A. PROHOFSKY, A. RAMESH, J. ROTHSTEIN, AND M. UNRATH (2022): "Can nudges increase take-up of the EITC? Evidence from multiple field experiments," *American Economic Journal: Economic Policy*, 14, 432–452.

MCCALL, J. J. (1970): "Economics of information and job search," *The Quarterly Journal of Economics*, 84, 113–126.

MCFADDEN, D. (1989): "A method of simulated moments for estimation of discrete response models without numerical integration," *Econometrica: Journal of the Econometric Society*, 995–1026.

MCQUILLAN, C. AND B. MOORE (2025a): "The Benefits of Unemployment Insurance For Marginally Attached Workers," Available at SSRN: https://ssrn.com/abstract=5357413.

——— (2025b): "Incomplete and Endogenous Take-Up of Unemployment Insurance Benefits," Working paper.

MEYER, B. D. (1995): "Lessons from the US Unemployment Insurance Experiments," *Journal of Economic Literature*, 33, 91–131.

MOYNIHAN, D., P. HERD, AND H. HARVEY (2015): "Administrative burden: Learning, psychological, and compliance costs in citizen-state interactions," *Journal of Public Administration Research and Theory*, 25, 43–69.

NEKOEI, A. AND A. WEBER (2015): "Recall expectations and duration dependence," *American Economic Review*, 105, 142–146.

OSMAN, A. AND J. D. SPEER (2024): "Stigma and take-up of labour market assistance: Evidence from two field experiments," *Economica*, 91, 123–141.

Schiprowski, A., J. Schmidtke, J. Schmieder, and S. Trenkle (2024): "The Effects of Unemployment Insurance Caseworkers on Job Search Effort," in *AEA Papers and Proceedings*, American Economic Association 2014 Broadway, Suite 305, Nashville, TN 37203, vol. 114, 567–571.

Spinnewijn, J. (2015): "Unemployed but optimistic: Optimal insurance design with biased beliefs," *Journal of the European Economic Association*, 13, 130–167.

Stephan, G., G. van den Berg, and P. Homrighausen (2016): "Randomizing information on a targeted wage support program for older workers: A field experiment," .

Trenkle, S. (2023): "Unemployment Insurance Receipt, Take-Up and Labor Market Conditions," Tech. rep., Mimeo, IZA.

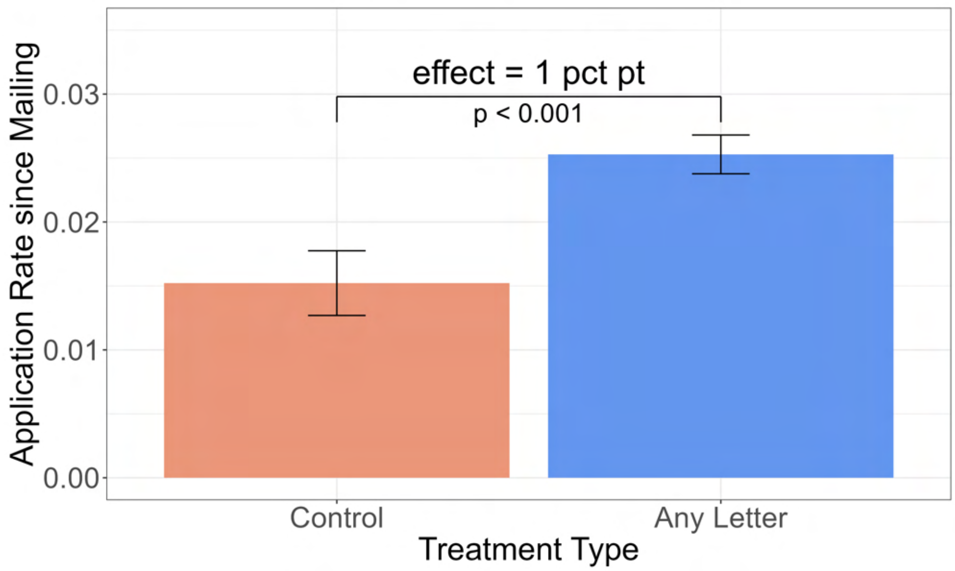
U.S. Department of Labor (2023): "President's Budget FY 2023," https://oui.doleta.gov/unemploy/pdf/prez_budget_23.pdf, office of Unemployment Insurance.

Online Appendix Materials

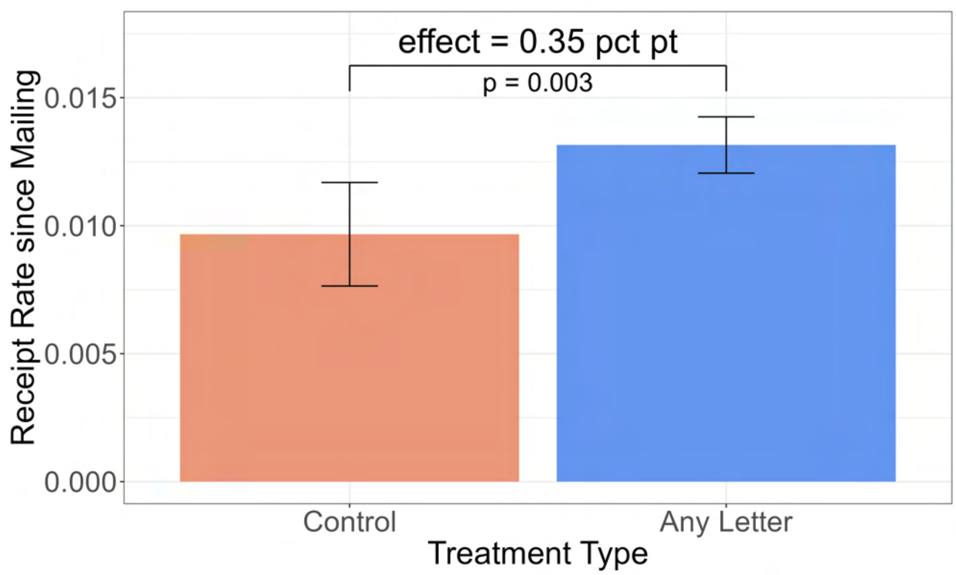
A Additional Figures and Tables

A.1 Take-Up Results

Figure A.1: Effects of Informational Letters on UI Take-Up, Full Experimental Sample



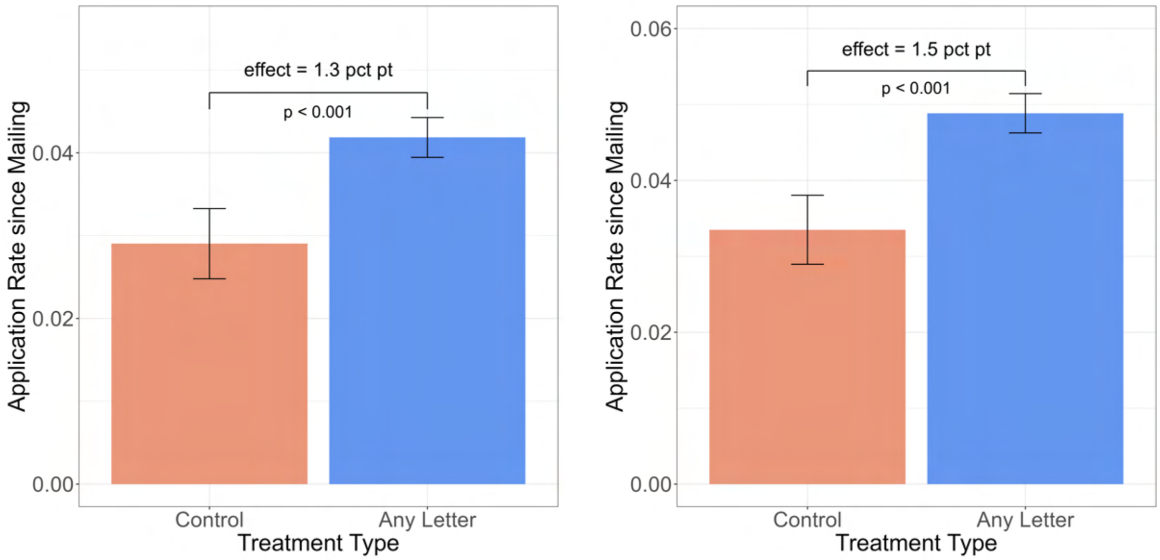
(a) Applications



(b) Receipt

Note: Panels represent main effects of intervention on UI applications and receipt for the full sample of potential jobs losses in 2024Q1 and Q2. Claiming outcomes are measured two months after the date of mailing for each wave. Bars represent 95% confidence intervals.

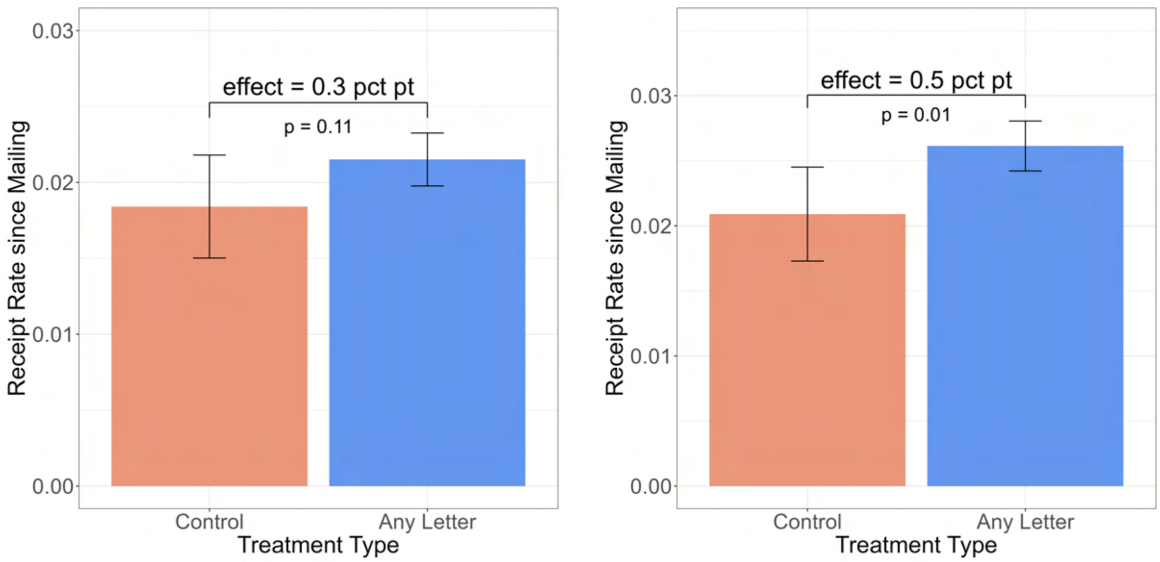
Figure A.2: Effects of Informational Letters on UI Applications at Four and Six Months



(a) Four Months (b) Six Months

Note: Panels represent main effects of intervention on UI applications by measuring outcomes the two other preregistered time horizons, four and six months after the date of mailing for each wave (compared to two months in Figure 3). Red bars represent the control group while blue bars represent the treatment group. Sample includes potential jobs losses which occur in 2024Q1 and Q2 and excludes firm-stayers. Bars represent 95% confidence intervals.

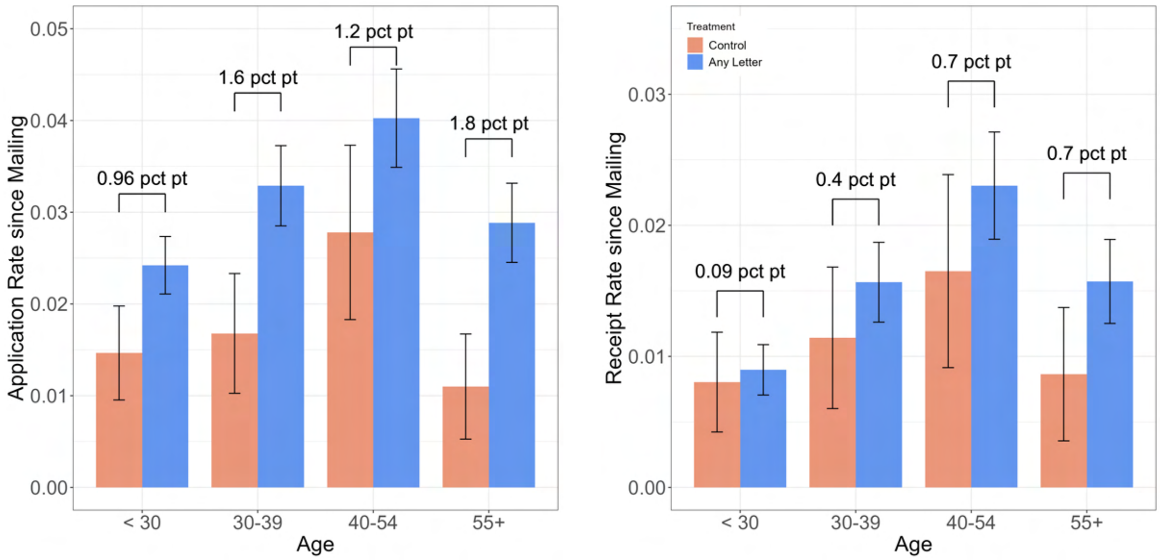
Figure A.3: Effects of Informational Letters on UI Receipt at Four and Six Months



(a) Four Months (b) Six Months

Note: Panels represent main effects of intervention on UI receipt by measuring outcomes the two other preregistered time horizons, four and six months after the date of mailing for each wave (compared to two months in Figure 3). Red bars represent the control group while blue bars represent the treatment group. Sample includes potential jobs losses which occur in 2024Q1 and Q2 and excludes firm-stayers. Bars represent 95% confidence intervals.

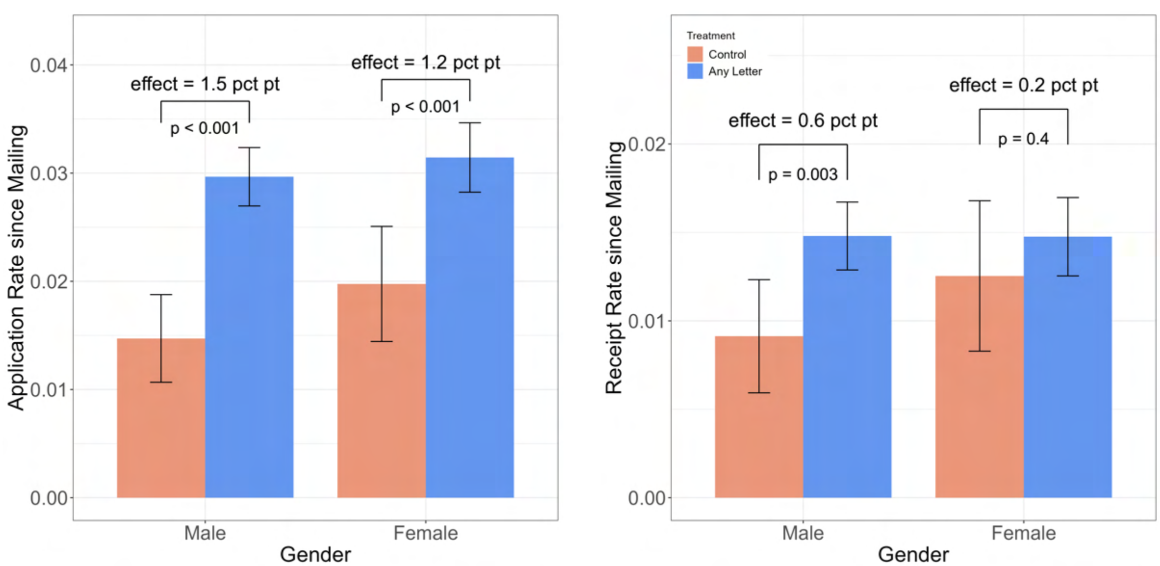
Figure A.4: Effects of Informational Letters on UI Applications and Receipt by Age



(a) Applications (b) Receipt

Note: Panels represent main effects of intervention on UI applications and receipt by age. Sample includes potential jobs losses which occur in 2024Q1 and Q2. Claiming outcomes are measured two months after the date of mailing for each wave. Bars represent 95% confidence intervals.

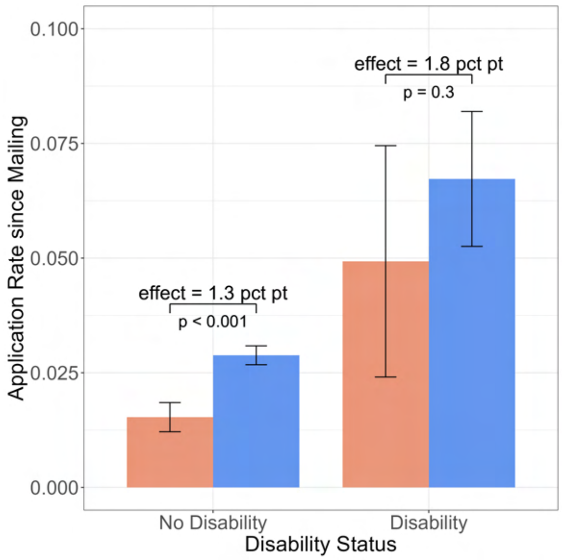
Figure A.5: Effects of Informational Letters on UI Applications and Receipt by Gender



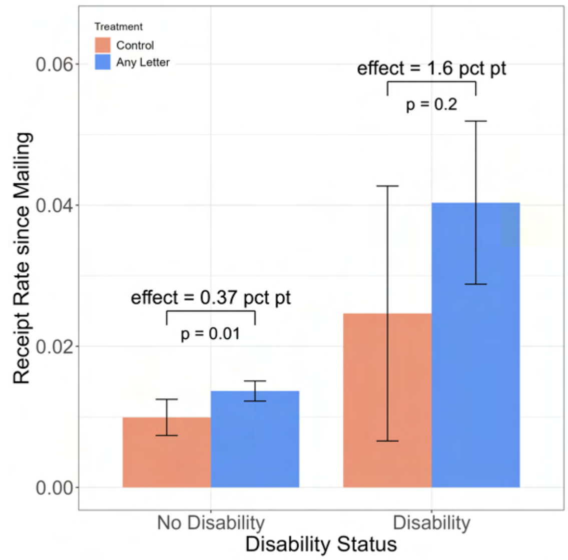
(a) Applications (b) Receipt

Note: Panels represent main effects of intervention on UI applications and receipt by gender. Sample includes potential jobs losses which occur in 2024Q1 and Q2. Claiming outcomes are measured two months after the date of mailing for each wave. Bars represent 95% confidence intervals.

Figure A.6: Effects of Informational Letters on UI Applications and Receipt by Disability Status



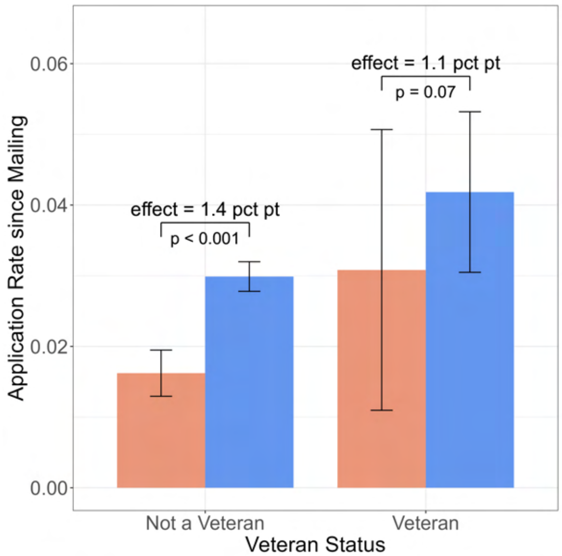
(a) Applications



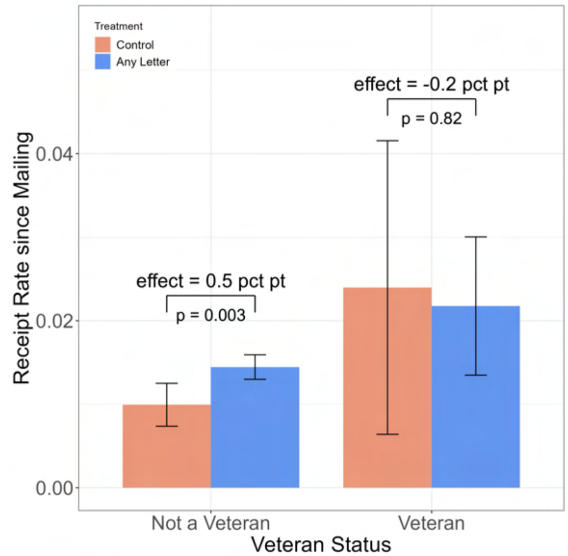
(b) Receipt

Note: Panels represent main effects of intervention on UI applications and receipt by disability status. Sample includes potential jobs losses which occur in 2024Q1 and Q2. Claiming outcomes are measured two months after the date of mailing for each wave. Bars represent 95% confidence intervals.

Figure A.7: Effects of Informational Letters on UI Applications and Receipt by Veteran Status



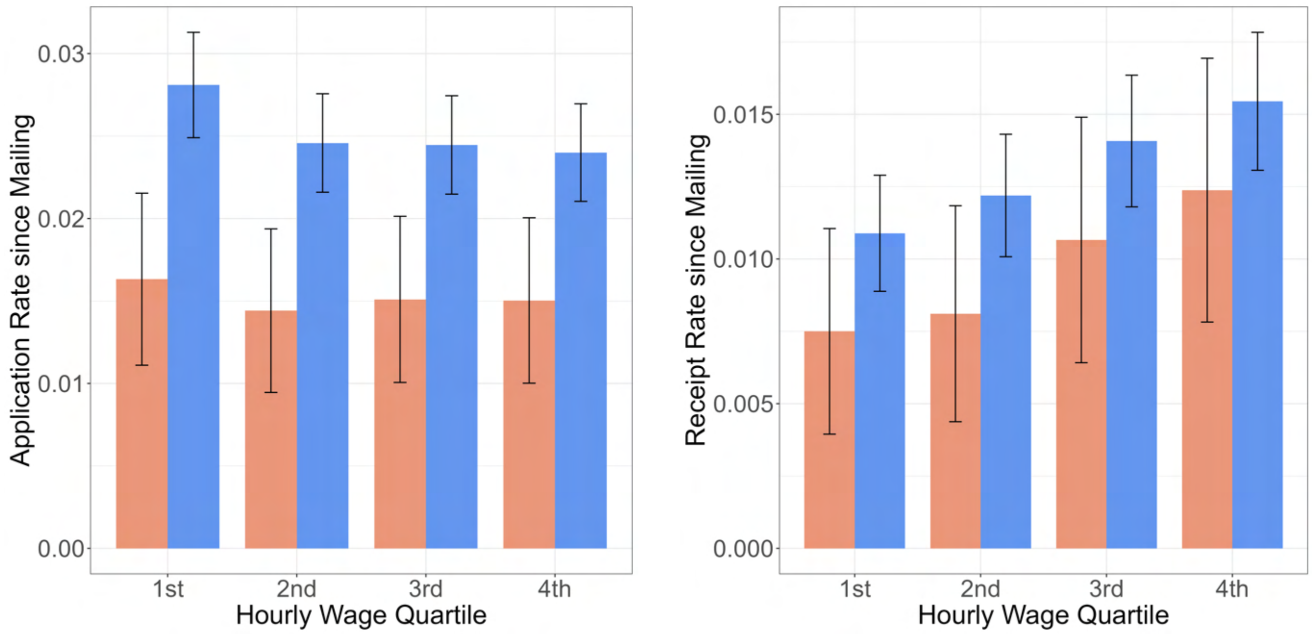
(a) Applications



(b) Receipt

Note: Panels represent main effects of intervention on UI applications and receipt by veteran status. Sample includes potential jobs losses which occur in 2024Q1 and Q2. Claiming outcomes are measured two months after the date of mailing for each wave. Bars represent 95% confidence intervals.

Figure A.8: Effects of Informational Letters on UI Take-Up by Previous Hourly Wage, Full Sample

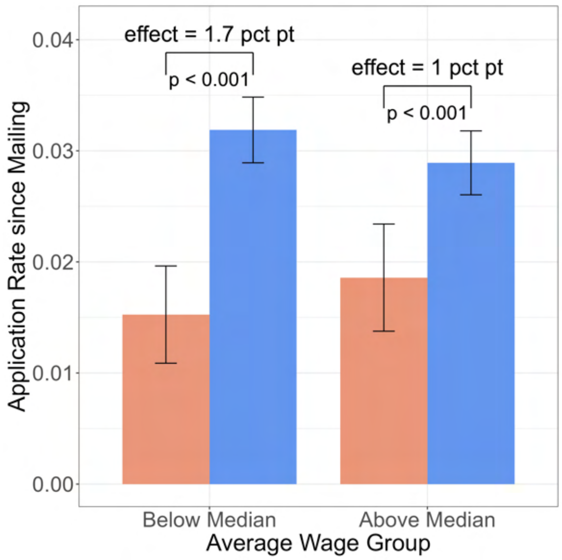


(a) Applications

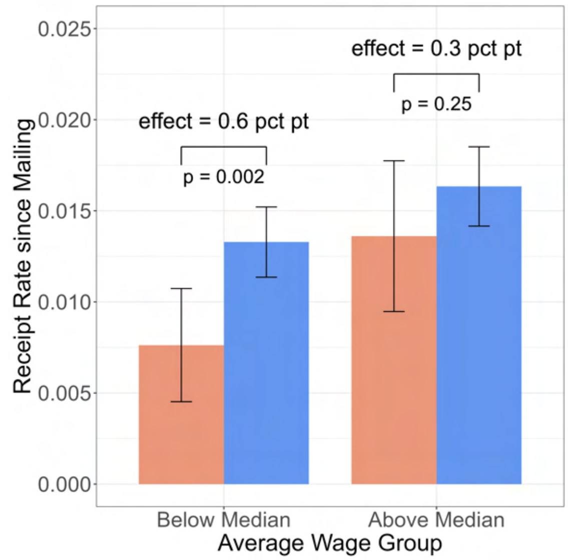
(b) Receipt

Note: Panels represent main effects of intervention on UI applications and receipt. Red bars represent the control group while blue bars represent the treatment group. Sample includes potential jobs losses which occur in 2024Q1 and Q2. Claiming outcomes are measured two months after the date of mailing for each wave. Bars represent 95% confidence intervals. Prior job median wage in the experimental sample is $28.84/hour.

Figure A.9: Effects of Informational Letters on UI Take-Up by Previous Hourly Wage



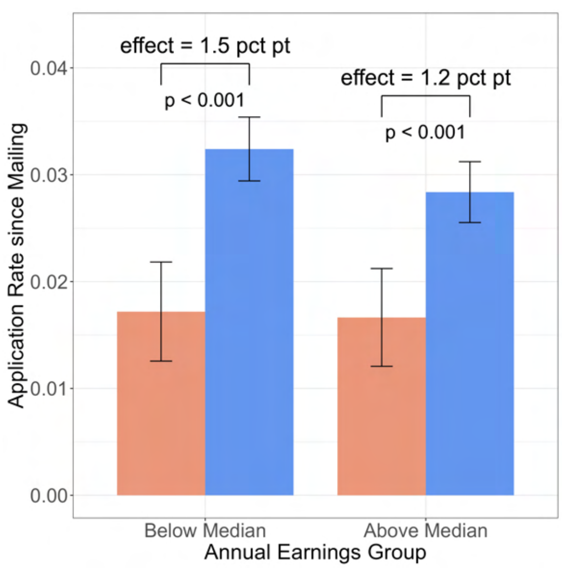
(a) Applications



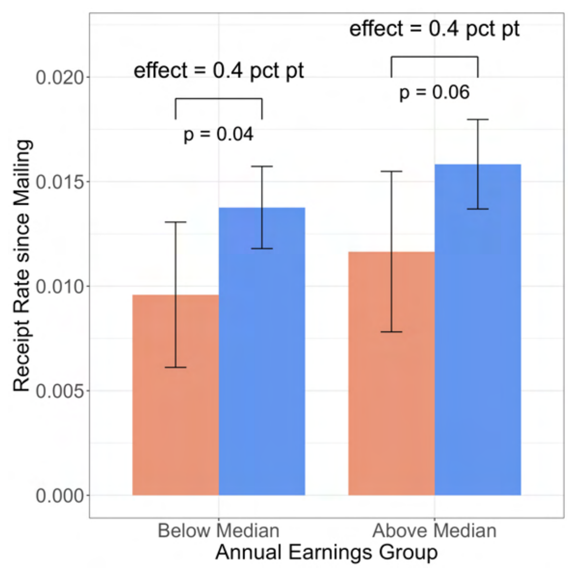
(b) Receipt

Note: Panels represent main effects of intervention on UI applications and receipt by previous wage. Red bars represent the control group while blue bars represent the treatment group. Sample includes potential jobs losses which occur in 2024Q1 and Q2. Claiming outcomes are measured two months after the date of mailing for each wave. Bars represent 95% confidence intervals. Prior job median wage in the experimental sample is $28.84/hour.

Figure A.10: Effects of Informational Letters on UI Take-Up by Previous Annual Earnings



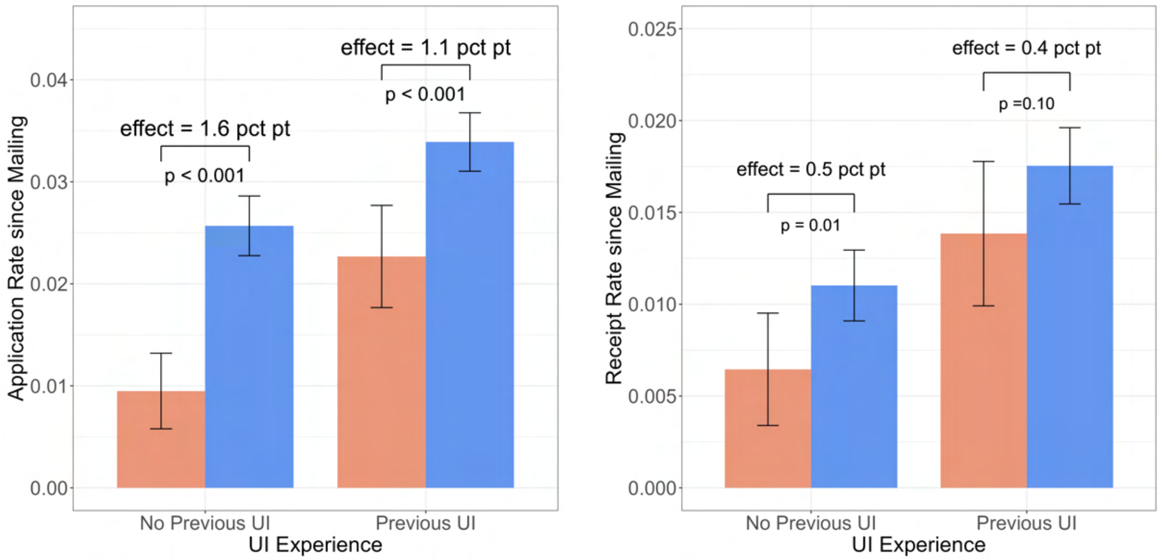
(a) Applications



(b) Receipt

Note: Panels represent main effects of intervention on UI applications and receipt by previous income. Red bars represent the control group while blue bars represent the treatment group. Sample includes potential jobs losses which occur in 2024Q1 and Q2. Claiming outcomes are measured two months after the date of mailing for each wave. Bars represent 95% confidence intervals. Median prior annual earnings in the experimental sample is $44,578.

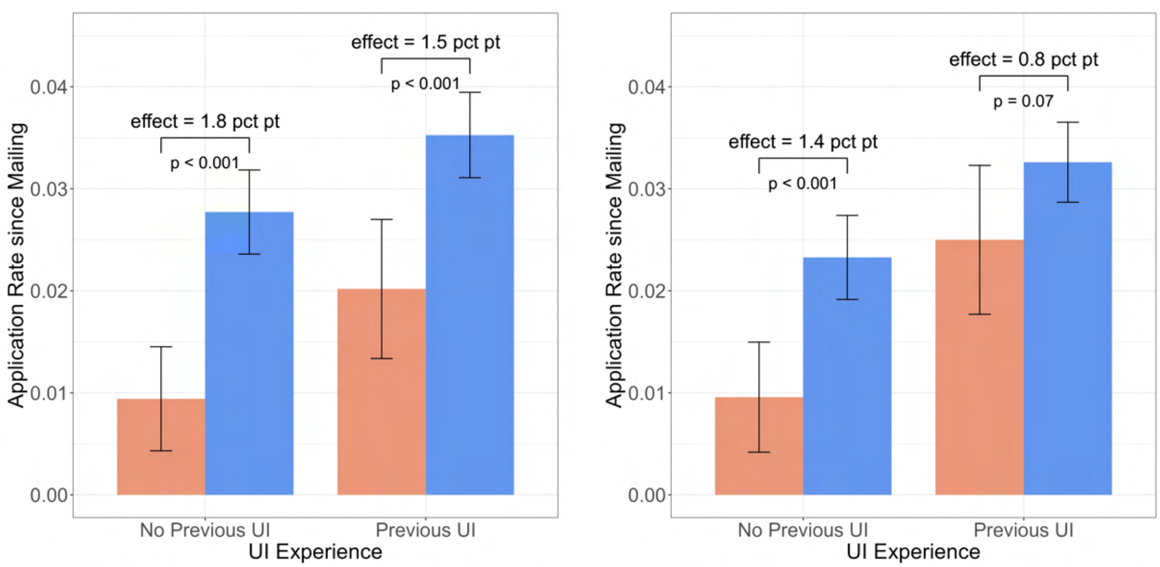
Figure A.11: Effects of Informational Letters on UI Take-Up by Previous UI Experience



(a) Applications (b) Receipt

Note: Panels represent main effects of intervention on UI applications and receipt by previous experience with the UI system. Red bars represent the control group while blue bars represent the treatment group. Sample includes potential jobs losses which occur in 2024Q1 and Q2. Claiming outcomes are measured two months after the date of mailing for each wave. Bars represent 95% confidence intervals.

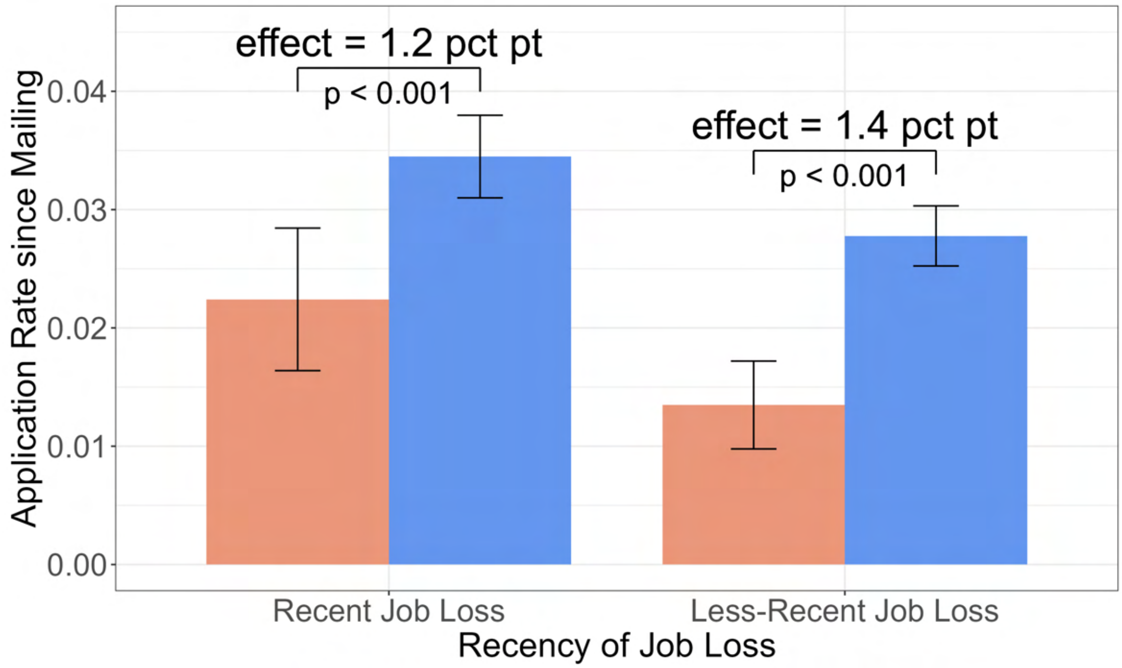
Figure A.12: Effects of Letters on UI Applications by Previous UI Experience and Wage



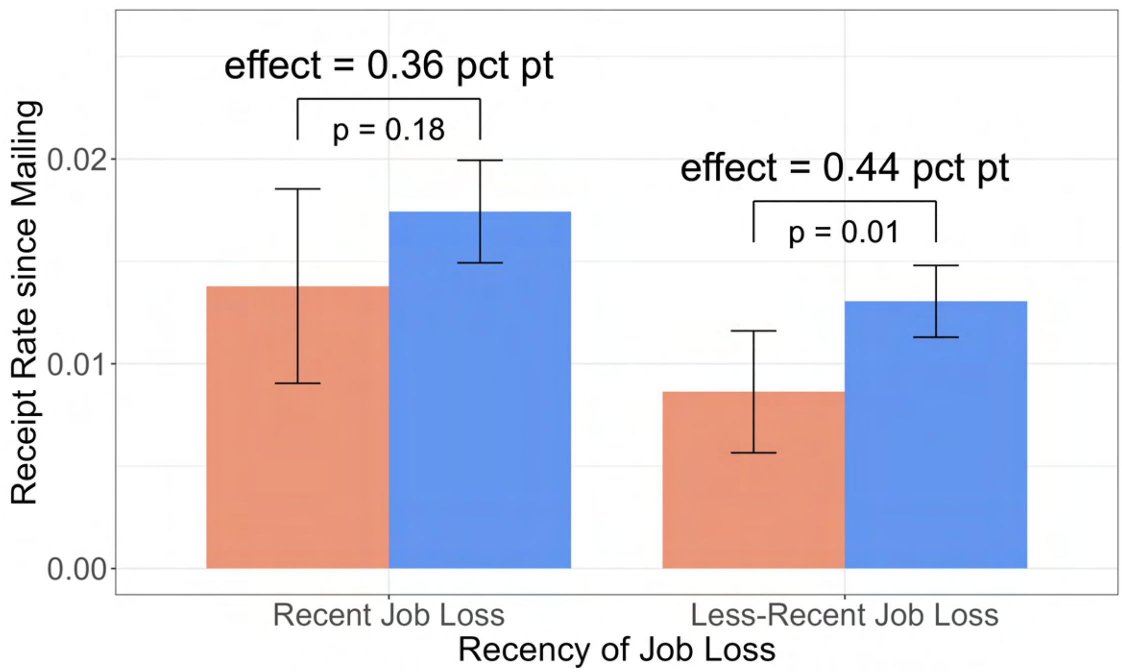
(a) Low Wage (b) High Wage

Note: Panels represent main effects of intervention on UI applications by previous experience with the UI system and whether a worker was laid off from a low- or high-wage job. Red bars represent the control group while blue bars represent the treatment group. Sample includes potential jobs losses which occur in 2024Q1 and Q2. Claiming outcomes are measured two months after the date of mailing for each wave. Bars represent 95% confidence intervals.

Figure A.13: Effects of Letter on Take-Up by Job Loss Recency



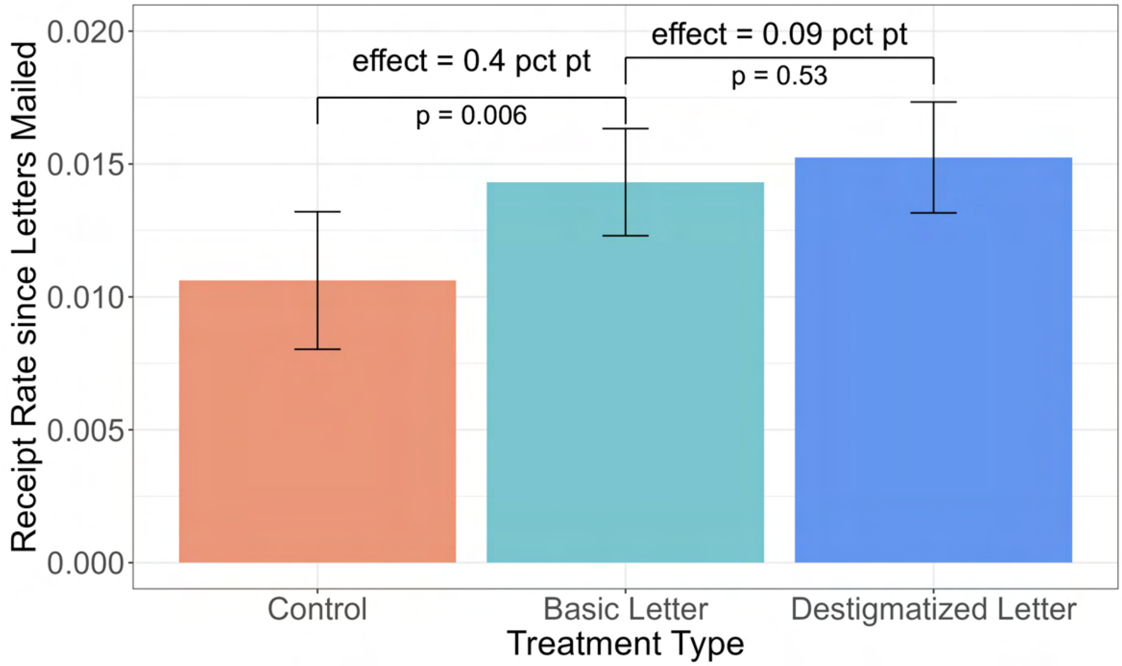
(a) Application



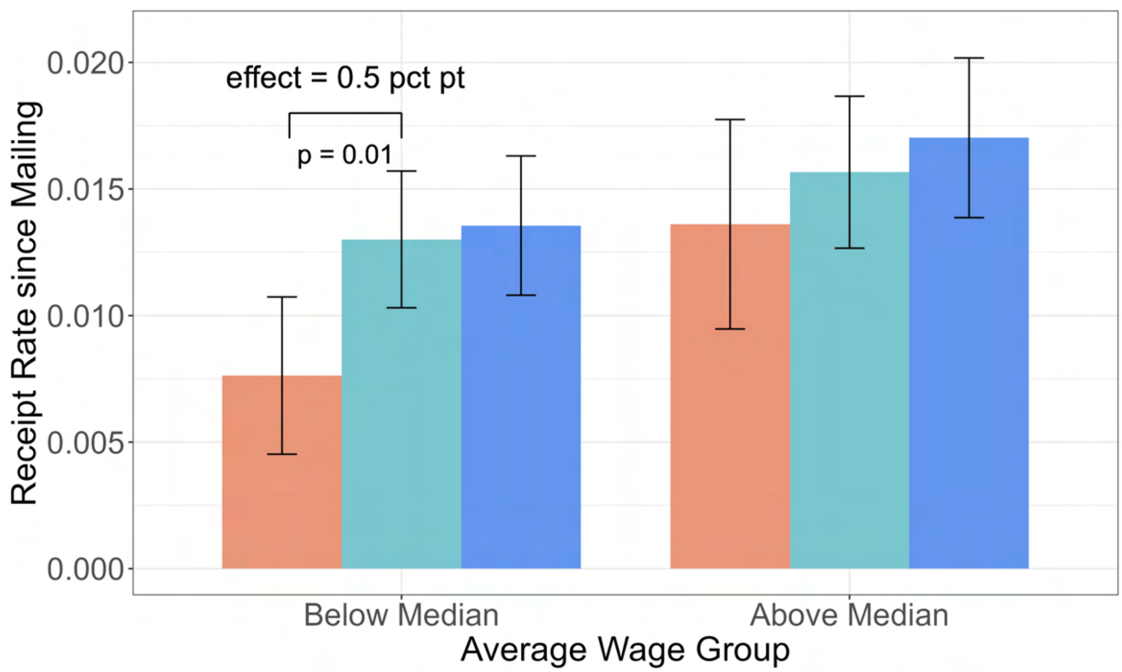
(b) Receipt

Note: Graphs represent effects of intervention on UI applications and receipt according to whether a worker experienced a drop in hours from $t-1$ to t that was smaller or greater than the experimental sample mean of 60%, which proxies for recency of job loss. Sample includes potential jobs losses which occur in 2024Q1 and Q2, excluding "false positive" separators. Claiming outcomes are measured two months after the date of mailing for each wave. Bars represent 95% confidence intervals.

Figure A.14: Effects of Destigmatizing Message on Receipt



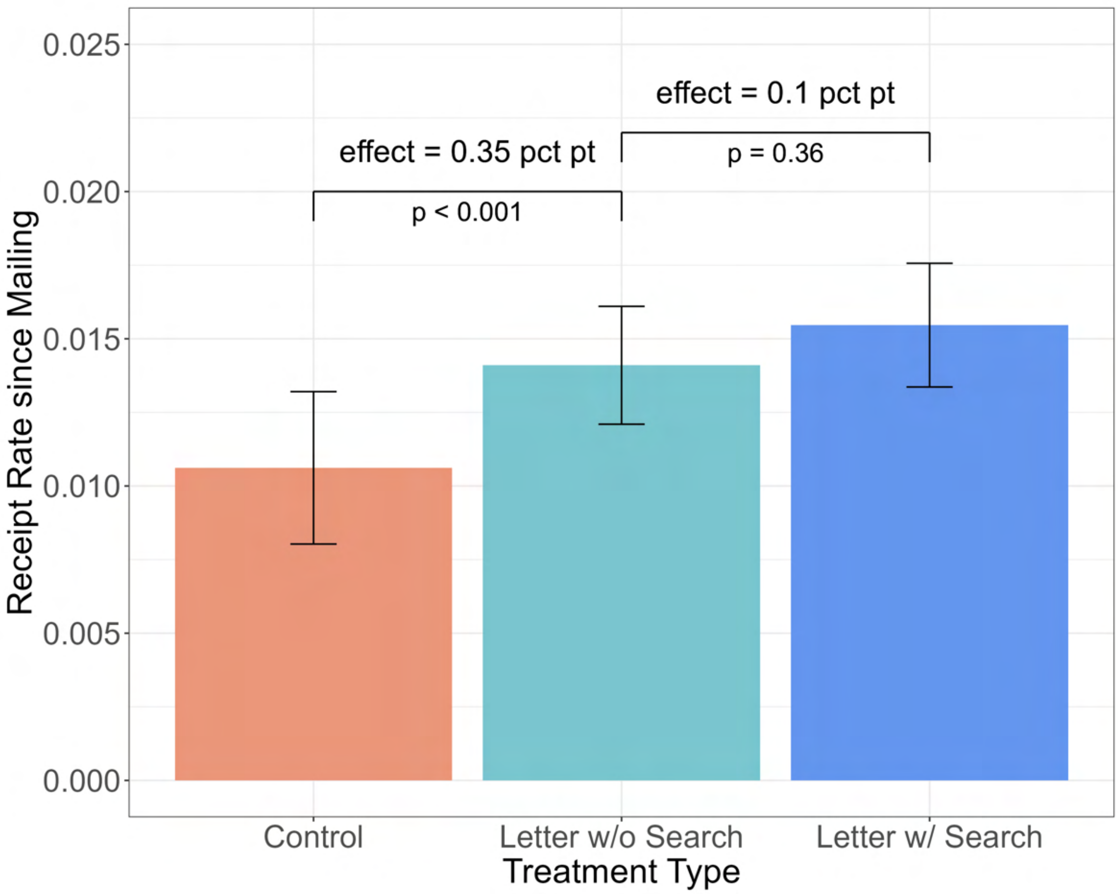
(a) Overall Sample



(b) by Previous Hourly Wage

Note: Graphs represent effects of intervention on UI applications according to whether the treatment group's letter included a destigmatizing message. Top panel shows effect on overall sample, bottom panel splits sample by previous hourly wage. The red bar represents the control group, light blue bar represents the treatment group without a destigmatizing message, and dark blue bar represents the treatment group with such a message. Sample includes potential jobs losses which occur in 2024Q1 and Q2, excluding "false positive" separators. Claiming outcomes are measured two months after the date of mailing for each wave. Bars represent 95% confidence intervals.

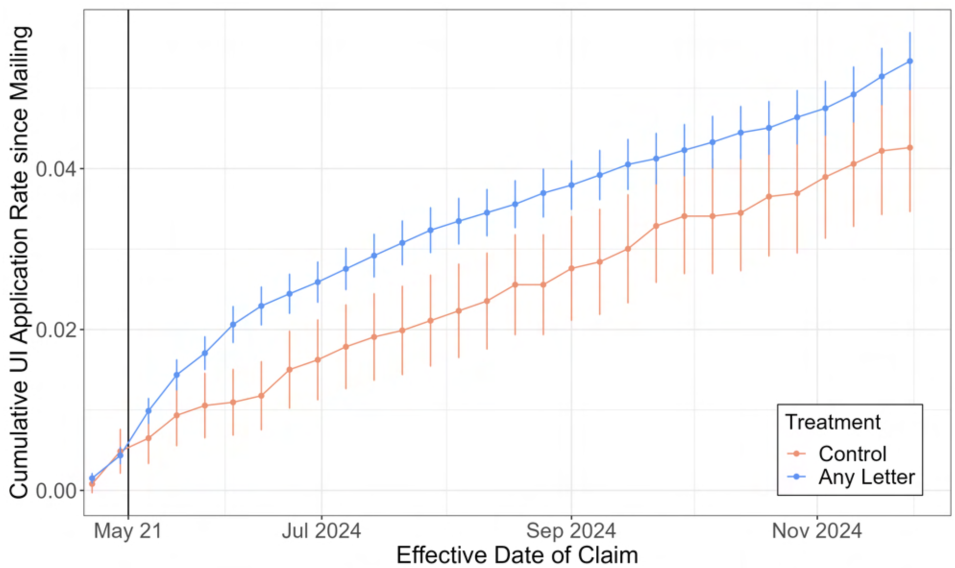
Figure A.15: Effects of Search Expectations Message on Receipt



Note: Graph represents effects of intervention on UI receipt according to whether the treatment group's letter included a message tempering job finding expectation. The red bar represents the control group, light blue bar represents the treatment group without a search expectations message, and dark blue bar represents the treatment group with such a message. Sample includes potential jobs losses which occur in 2024Q1 and Q2, excluding "false positive" separators. Claiming outcomes are measured two months after the date of mailing for each wave. Bars represent 95% confidence intervals. Figure 7 shows effect of message on UI applications.

Figure A.16: Effects of Informational Letters on UI Application and Receipt by Wave

(a) Applications, First Wave



(b) Receipt, Second Wave

Note: .

Table A.1: OLS Specifications for UI Take-Up by Receipt of Any Letter

	Applications		Receipt	
Received Letter	0.014***	0.010***	0.004*	0.003**
	(0.002)	(0.002)	(0.002)	(0.001)
	[0.000]	[0.000]	[0.013]	[0.007]
Intercept	0.017***	0.015***	0.011***	0.010***
	(0.002)	(0.002)	(0.002)	(0.001)
	[0.000]	[0.000]	[0.000]	[0.000]
Dropped Recalls	✓		✓	
R^2	0.0010	0.0006	0.0002	0.0001
N	32,618	50,213	32,618	50,213

Notes: Each column reports estimates from an OLS regression where the dependent variable is UI application (columns 1–2) or receipt (columns 3–4) two months after letters are mailed. Received letter coefficient captures the average effect of receiving the any type of informational letter relative to the pure control group that received no letter. Standard errors are reported in parentheses. p-values are reported in square brackets. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$

Table A.2: OLS Specifications for UI Take-Up by Letter Treatment Arm

	Applications		Receipt	
Generic Letter	0.012***	0.013***	0.0037*	0.0035
	(0.003)	(0.003)	(0.002)	(0.002)
	[0.000]	[0.000]	[0.043]	[0.056]
Destigmatizing Message	0.004		0.001	
	(0.002)		(0.001)	
	[0.075]		[0.519]	
Search Expectations Message		0.0007		0.0014
		(0.001)		(0.001)
		[0.513]		[0.344]
Intercept	0.017***	0.017***	0.011***	0.011***
	(0.002)	(0.002)	(0.002)	(0.002)
	[0.000]	[0.000]	[0.000]	[0.000]
R^2	0.0011	0.0010	0.0002	0.0002
N	32,618	32,618	32,618	32,618

Notes: Each column reports estimates from an OLS regression where the dependent variable is UI application (columns 1–2) or receipt (columns 3–4) two months after letters are mailed. Generic letter coefficient captures the average effect of receiving the generic informational letter relative to the pure control group. The destigmatizing and search expectations message coefficients measure the incremental effect of adding these messages relative to the generic letter alone. Standard errors are reported in parentheses. p-values are reported in square brackets. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$

Table A.3: Rejection Rates of Always Takers and Compliers, Heterogeneity

	Rejection Rate	
	Always Takers	Compliers
Main Sample	36%	68%
	[27%, 45%]	[54%, 82%]
Panel A. By Firm Hours Growth or Contractions *(Leave-One-Out)*		
Growing	51%	57%
	[35%, 67%]	[34%, 80%]
Rejection Rate by Reason (as Share of All Growing-Firm Origin Claims)		
Quit/Fired for Cause	26%	25%
	[12%, 40%]	[5%, 46%]
Not Actively Seeking Work	13%	11%
	[2%, 24%]	[-5%, 27%]
Other	21%	-5%
	[8%, 33%]	[-23%, 13%]
Unclassified	13%	26%
	[2%, 24%]	[9%, 42%]
Shrinking	27%	75%
	[16%, 38%]	[58%, 92%]
Rejection Rate by Reason (as Share of All Shrinking-Firm Origin Claims)		
Quit/Fired for Cause	18%	27%
	[16%, 38%]	[12%, 41%]
Not Actively Seeking Work	0%	22%
	[0%, 0%]	[16%, 29%]
Other	3%	17%
	[-1%, 8%]	[9%, 25%]
Unclassified	10%	22%
	[2%, 17%]	[11%, 34%]
Panel B. By Prior UI Experience		
Previous UI	38%	60%
	[27%, 49%]	[44%, 77%]
Rejection Rate by Reason (as Share of All Previous UI Claims)		
Quit/Fired for Cause	22%	28%
	[13%, 31%]	[13%, 42%]
Not Actively Seeking Work	5%	12%
	[0%, 10%]	[4%, 20%]
Other	9%	6%
	[3%, 16%]	[-4%, 15%]
Unclassified	12%	20%
	[4%, 19%]	[8%, 31%]
Never UI	32%	84%
	[13%, 51%]	[57%, 111%]
Rejection Rate by Reason (as Share of All Never UI Claims)		
Quit/Fired for Cause	16%	25%
	[1%, 31%]	[4%, 46%]
Not Actively Seeking Work	4%	29%
	[-4%, 12%]	[15%, 42%]
Other	12%	13%
	[-1%, 25%]	[-5%, 31%]
Unclassified	8%	32%
	[-3%, 19%]	[15%, 49%]

A.2 Labor Supply Results

A.2.1 IV Estimates: Local Labor Supply Effects of UI Receipt

While intent-to-treat estimates show that informational outreach does not slow reemployment, one may ask whether receiving UI itself affects labor supply. To explore this, we instrument UI receipt with letter treatment assignment and estimate the local average treatment effect (LATE) of UI receipt on employment outcomes.

The results, presented in Figure A.18a, suggest very large effects: for example, IV estimates imply that receiving UI increases the probability of being employed in the two quarters following job loss by more than 1 percentage point on a base of roughly 0.4, an effect size that translates into a 300% relative increase. These estimates are not statistically significant at the 5% level and are accompanied by wide confidence intervals. Given that the first-stage F-statistic is approximately 8, these results must be interpreted with caution.

More fundamentally, the exclusion restriction required for IV identification may be violated. For the LATE to be valid, treatment letters must affect employment outcomes only via their effect on UI receipt. However, this assumption could fail if the letters affect the job search behavior of inframarginal individuals who would have applied for UI regardless of the treatment, but whose beliefs or motivation were nonetheless influenced by the letter content.

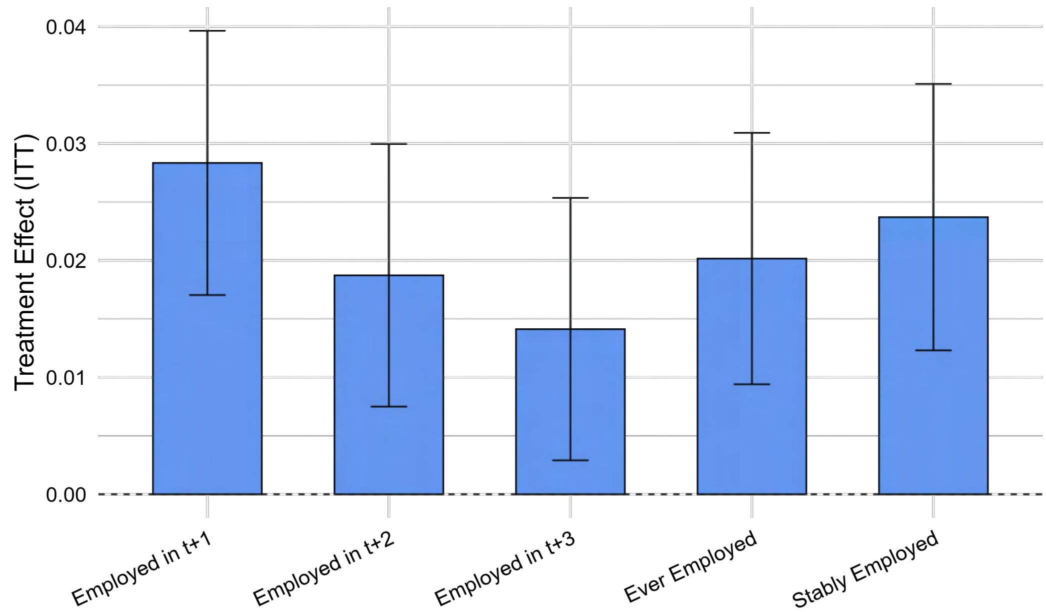
To directly test this concern, Table A.10 reports reduced-form treatment effects of different message variants on reemployment outcomes. Because these messages (e.g., search expectations, benefit salience) had no marginal impact on take-up, any effect on employment would indicate a direct influence on inframarginal behavior – and thus a violation of the exclusion restriction.[36] Across all messages and outcomes, point estimates are very close to zero and statistically insignificant; they do not provide evidence that letter content altered employment among inframarginals.

While these tests support the plausibility of the exclusion restriction, they do not confirm it. We interpret the IV results as suggestive: the large, positive point estimates are consistent with the ITT evidence that UI receipt does not shrink the job search process. However, given the weak first stage and potential residual exclusion violations, we place limited weight on the magnitudes.

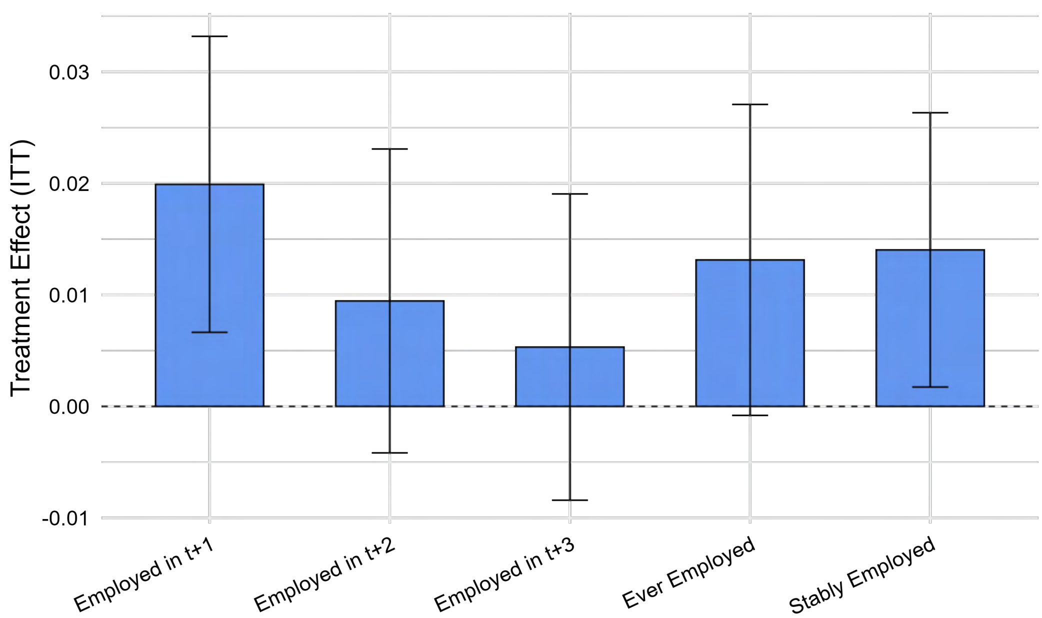
[36] Table A.10 also includes the effects of the destigmatizing treatment on reemployment because, even though there is a positive statistically significant effect on UI applications, the effect on receipt is smaller and not significant. Small receipt effects and large effects on reemployment associated with the destigmatizing message would cast doubt on the exclusion restriction, as some of the labor supply effect would likely operate through inframarginals.

A.2.2 Additional Tables and Figures

Figure A.17: Effects of Letters on Re-Employment Outcomes, Robustness Checks



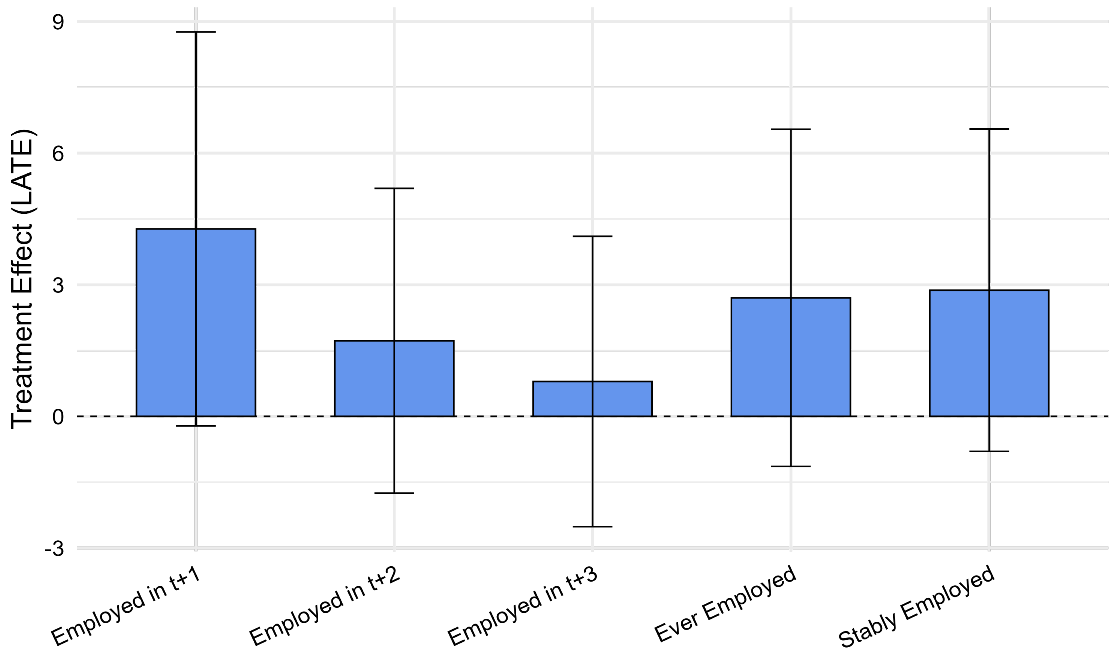
(a) Full Sample, including False Positive Separators



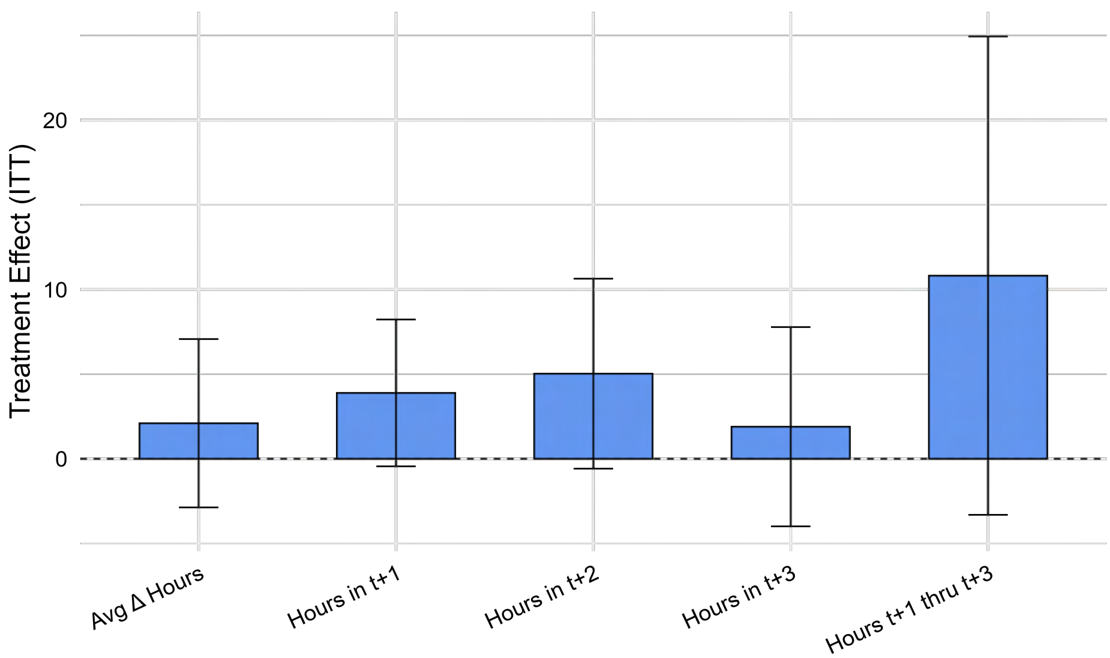
(b) Baseline Sample, Controlling for Pre-Job Loss Characteristics

Note: Graph shows intent-to-treat (ITT) estimates on labor supply outcomes in quarters following job loss. Top panel includes full sample of 2024Q1–Q2 Potential Job Losses, including "false positive" separations. Bottom panel includes baseline sample of 2024Q1–Q2 Potential Job Losses (excluding false positive separations) and shows estimates controlling for gender, age (and its quadratic), veteran status, disability status, an indicator for living in the Seattle MSA, and whether a worker has ever received UI. Bars represent 95% confidence intervals. Main estimates are provided in Figure 8.

Figure A.18: Effects of Letters on Re-Employment Outcomes, Robustness Checks



(a) Baseline Sample, IV Estimates

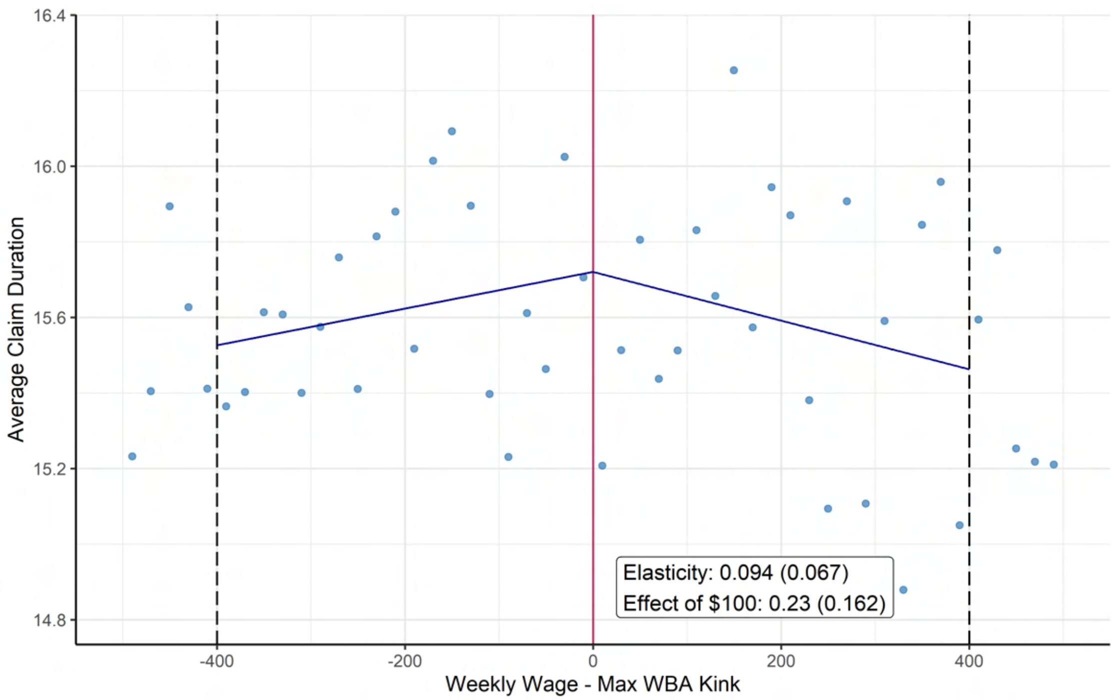


(b) Baseline Sample, Effects on Hours Worked

Note: Graph shows estimates on labor supply outcomes in quarters following job loss. Sample includes 2024Q1–Q2 Potential Job Losses, excluding "false positive" separations. Top panel shows LATE from IV estimation for the effect of UI receipt on subsequent employment. Bottom panel shows ITT estimates for the effect of receiving a letter on subsequent hours worked. Bars represent 95% confidence intervals. Main estimates are provided in Figure 8.

Figure A.19: Claim Duration Effects of Maximum WBA for Early and Late Claimants

(a) Early Claimants (file in quarter before or of job loss)



(b) Late Claimants (file in two quarters after job loss)

Note: Graphs represent effects of intervention on UI applications according to whether the treatment group's letter included a destigmatizing message. Top panel shows effect on overall sample, bottom panel splits sample by previous hourly wage. The red bar represents the control group, light blue bar represents the treatment group without a destigmatizing message, and dark blue bar represents the treatment group with such a message. Sample includes potential jobs losses which occur in 2024Q1 and Q2. Claiming outcomes are measured two months after the date of mailing for each wave. Bars represent 95% confidence intervals.

Table A.4: Effect of Letter Treatment on Earnings and Wages

	Dependent variable:					
	Earnings in		Earnings	Avg Δ in	Hourly Wage	Avg Δ in
	$t+1$	$t+2$	through $t+2$	Earnings	through $t+2$	Hourly Wage
Letter Treatment	-117.44	109.17	-11.75	-40.97	-0.09	-0.02
	(143.35)	(145.18)	(293.09)	(139.64)	(0.22)	(0.20)
Dep. Variable Mean	6,871.6	9,168	17,373.8	-3,376.2	26.80	1.96
Observations	10,856	12,523	9,209	9,209	14,170	14,170

Note: This table reports intent-to-treat estimates of the effect of receiving an informational letter on earnings and hourly wages in the quarters following job loss. Hourly wage is calculated as total earnings divided by total hours in quarters $t+1$ and $t+2$, and is winsorized at the 5% level (2.5% in each tail). Change variables are defined relative to a worker's average earnings or hourly wage in quarters $t-2$ and $t-1$. Standard errors are in parentheses.

Table A.5: Effect of UI Letter Treatment Interacted with Drop in Hours on Employment Status

	Dependent variable:				
	Employed in $t+1$	Employed in $t+2$	Employed in $t+3$	Ever Employed	Stably Employed
	(1)	(2)	(3)	(4)	(5)
Panel A. Baseline Sample					
Letter Treatment	0.039**	0.011	0.027	0.046**	0.015
	(0.020)	(0.021)	(0.021)	(0.021)	(0.019)
Change in Hours	0.546***	0.400***	0.354***	0.460***	0.415***
	(0.024)	(0.025)	(0.026)	(0.026)	(0.023)
Treatment x Change in Hours	0.031	0.006	0.035	0.050*	0.005
	(0.027)	(0.028)	(0.028)	(0.029)	(0.025)
Dep. Variable Mean	0.333	0.384	0.408	0.487	0.256
Observations	32,607	32,607	32,607	32,607	32,607
Panel B. Full Sample					
Letter Treatment	0.040***	0.024	0.035**	0.040***	0.029*
	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)
Change in Hours	0.752***	0.630***	0.558***	0.603***	0.697***
	(0.019)	(0.020)	(0.020)	(0.019)	(0.020)
Treatment x Change in Hours	0.027	0.015	0.039*	0.038*	0.016
	(0.021)	(0.022)	(0.022)	(0.021)	(0.022)
Dep. Variable Mean	0.567	0.587	0.589	0.667	0.485
Observations	50,199	50,199	50,199	50,199	50,199

Note: *p<0.1; **p<0.05; ***p<0.01

Table A.6: Experimental Sample Characteristics

Variable	Mean	SD	p-value
Ever Received UI	0.59	0.49	0.81
Previous Hourly Wage	25.68	6.77	0.13
Previous Annual Salary	46,357	16,514	0.98
Base Year Hours Worked	1,804	357	0.62
Percent Hours Drop, $t-1$ to t	-0.65	0.25	0.23
Layoff Industry			
Retail Trade	0.166	0.372	0.85
Health Care and Social Assistance	0.145	0.352	0.62
Manufacturing	0.093	0.290	0.59
Construction	0.079	0.270	0.07
Administrative, Support, Waste Management	0.084	0.278	0.79
Accommodation and Food Services	0.087	0.282	0.58
Transportation and Warehousing	0.070	0.254	0.69
Professional, Scientific, and Technical	0.058	0.235	0.92
Wholesale Trade	0.055	0.227	0.24
Other Services (except Public Admin)	0.041	0.199	0.56
Agriculture, Fishing, Forestry	0.028	0.164	0.21
Public Administration	0.035	0.183	0.24
Real Estate and Rental and Leasing	0.028	0.165	0.68
Arts, Entertainment, and Recreation	0.031	0.173	0.53
Utilities	0.002	0.039	0.44
Demographics			
Share Female	0.44	0.50	0.05
Age	41.0	15.4	0.49
Share Veteran	0.03	0.17	0.10
Share with Disability	0.04	0.19	0.96
Address in Seattle (proper)	0.08	0.27	0.97
Address in Seattle MSA	0.48	0.50	0.21

Note: Table shows balance across economic, industrial, and demographic characteristics for the experimental sample ($N = 50,199$), which consists of two waves of potential job losses. The rightmost column shows the p-value for a hypothesis test that the listed characteristic has the same mean across all assignment arms. Industry shares may sum to slightly more than 1 due to rounding. Age is calculated as a worker's age on the date of the first wave mailing.

Table A.7: Complier Analysis: Applicants and Recipients

	Always Takers		Compliers		
Variable	Mean	95% CIs	Mean	95% CIs	p-value diff
Previous Hourly Wage	25.96	[24.61, 27.31]	25.23	[24.77, 25.68]	0.32
Previous Annual Salary	$45,476	[42,359, 48,593]	$44,975	[43,829, 46,120]	0.77
Base Year Hours Worked	1,734.6	[1673.1, 1800.1]	1,775.0	[1750.1, 1799.8]	0.27
Ever Received UI	0.75	[0.67, 0.84]	0.64	[0.61, 0.68]	0.02
% Hours Drop, $t-1$ to t	-64%	[-68, -59]	-68%	[-69,-66]	0.13
Layoff Industry Share					
Retail Trade	0.15	[0.08, 0.22]	0.18	[0.16, 0.21]	0.34
Health Care/ Social Asst	0.12	[0.05, 0.18]	0.13	[0.11, 0.16]	0.67
Accomm/Food Svcs	0.08	[0.03, 0.13]	0.10	[0.08, 0.12]	0.48
Manufacturing	0.09	[0.03, 0.14]	0.11	[0.08, 0.13]	0.58
Admin/Waste Mgmt	0.07	[0.02, 0.12]	0.09	[0.07, 0.11]	0.51
Construction	0.06	[0.01, 0.10]	0.08	[0.06, 0.09]	0.48
Demographics					
Share Female	0.51	[0.41, 0.61]	0.44	[0.41, 0.48]	0.20
Age	38.9	[36.4, 41.5]	40.4	[39.4, 41.4]	0.30
Share Veteran	0.09	[0.03, 0.14]	0.06	[0.05, 0.08]	0.37
Share with Disability	0.14	[0.07, 0.20]	0.09	[0.07, 0.11]	0.21
Address Seattle (proper)	0.08	[0.03, 0.13]	0.08	[0.06, 0.09]	0.91
Address Seattle MSA	0.46	[0.36, 0.56]	0.52	[0.49, 0.56]	0.23

Note: Sample is individuals who applied for UI in the 2 months after their initial mailing. Variables reported are the same as in Table 1. First numerical column shows the mean of the always takers (individuals who apply for UI regardless of intervention), while second column shows the corresponding 95% confidence intervals. Third column shows the means for compliers (individuals who apply for UI if and only if they receive the intervention) and the fourth column shows the corresponding 95% confidence intervals. The fifth column reports the p-value of the difference between the compliers and always takers. Standard errors and p-values are computed using the delta method.

Table A.8: Characteristics of Always Taker and Complier Recipients

	Always Takers		Compliers		p-value
Variable	Mean	95% CIs	Mean	95% CIs	diff
Previous Hourly Wage	27.50	[25.84, 29.16]	26.36	[25.69, 27.03]	0.22
Previous Annual Salary	$48,088	[44,181, 51,995]	$47,503	[45,813, 49,192]	0.79
Base Year Hours Worked	1,727.8	[1,655, 1,800]	1,801.0	[1,765, 1,837]	0.08
Ever Received UI	0.73	[0.62, 0.84]	0.68	[0.64, 0.73]	0.46
% Hours Drop, $t-1$ to t	-63%	[-68, -57]	-68%	[-71,-66]	0.08
Layoff Industry Share					
Retail Trade	0.14	[0.06, 0.23]	0.15	[0.12, 0.19]	0.86
Health Care/ Social Asst	0.08	[0.01, 0.15]	0.14	[0.11, 0.18]	0.10
Accomm/Food Svcs	0.06	[0.00, 0.12]	0.10	[0.07, 0.13]	0.33
Manufacturing	0.10	[0.02, 0.17]	0.11	[0.08, 0.14]	0.71
Admin/Waste Mgmt	0.08	[0.01, 0.15]	0.11	[0.07, 0.14]	0.50
Construction	0.06	[0.00, 0.12]	0.08	[0.05, 0.11]	0.59
Demographics					
Share Female	0.52	[0.40, 0.65]	0.43	[0.38, 0.48]	0.16
Age	40.5	[37.1, 43.9]	42.3	[40.9, 43.7]	0.33
Share Veteran	0.11	[0.03, 0.19]	0.07	[0.04, 0.09]	0.29
Share with Disability	0.11	[0.03, 0.19]	0.12	[0.08, 0.15]	0.92
Address Seattle (proper)	0.10	[0.02, 0.17]	0.08	[0.06, 0.11]	0.79
Address Seattle MSA	0.48	[0.35, 0.60]	0.53	[0.48, 0.58]	0.45
UI Recipients					
Weekly Benefit Amount	$532.35	[495.94, 568.76]	$530.23	[514.71, 545.74]	0.92
Number Weeks on UI	15.1	[13.1, 17.1]	14.6	[13.8, 15.5]	0.65

Note: Sample is individuals who applied for and received UI in the 2 months after their initial mailing. Variables reported are the same as in Table 1. First numerical column shows the mean of the always takers (individuals who receive UI regardless of intervention), while second column shows the corresponding 95% confidence intervals. Third column shows the means for compliers (individuals who apply for and receive UI if and only if they receive the intervention) and the fourth column shows the corresponding 95% confidence intervals. The fifth column reports the p-value of the difference between the compliers and always takers. Standard errors and p-values are computed using the delta method.

Table A.9: Employment Rates by Treatment Status and WorkSource Engagement

	Control		Treatment	
	No WorkSource	WorkSource	No WorkSource	WorkSource
Panel A. Any WorkSource Service				
Employed in $t+1$	0.54	0.59	0.57	0.49
Employed in $t+2$	0.57	0.44	0.59	0.46
Employed in $t+3$	0.58	0.32	0.59	0.47
Panel B. Job Search Activities at WorkSource Office				
Employed in $t+1$	0.54	0.55	0.57	0.44
Employed in $t+2$	0.57	0.41	0.59	0.45
Employed in $t+3$	0.58	0.32	0.59	0.48

Table A.10: Exclusion Restriction Tests for Effect of Different Messages on Employment

	Dependent variable:			
	Employed in $t+1$	Employed in $t+2$	Ever Employed	Stably Employed
	(1)	(2)	(3)	(4)
Panel A.				
Destigmatization Treatment	-0.001	0.005	-0.002	0.006
	(0.006)	(0.006)	(0.006)	(0.006)
Panel B.				
Search Message Treatment	0.004	-0.0003	-0.001	0.004
	(0.006)	(0.006)	(0.006)	(0.006)
Panel C.				
Amount Salience Treatment	0.001	0.006	0.004	0.003
	(0.006)	(0.006)	(0.006)	(0.006)
Dep. Variable Mean	0.333	0.384	0.435	0.282
Observations	26,580	26,580	26,580	26,580

Note: Table tests the effect of different randomized messages on employment behavior after job loss, comparing workers who received a letter without a given message (control) to those who received a given message (treated). To focus on one message at a time, each panel runs a regression separately with the single explanatory variable listed. Each column represents a different dependent variable for employment.

Table A.11: Instrumental Variables Estimates of UI Application on WorkSource Engagement

		Specific WorkSource Services				
	Any Service	Job Search	Workshop Career Services	Resume	RESEA	Other
UI Application	0.367***	0.181***	0.131***	0.129***	0.098*	0.174***
	(0.082)	(0.066)	(0.050)	(0.050)	(0.052)	(0.064)
Intercept	-0.002	-0.000	-0.001	-0.001	-0.000	-0.001
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)
Multiple R^2	0.265	0.164	0.056	0.057	0.094	0.145
Control mean (DV)	0.248	0.161	0.058	0.058	0.095	0.139
First-stage F-stat	32.65	32.65	32.65	32.65	32.65	32.65
N	50,213	50,213	50,213	50,213	50,213	50,213

Note: Each column reports an IV regression using the informational letter as an instrument for UI application. The first stage estimates the effect of the letter on the likelihood of applying for UI; the second stage estimates the causal effect of UI application on engagement with WorkSource services. "Job Search Services" include activities such as job searching or applying to jobs and receiving referrals or staff assistance with job applications. "Workshop and Career Services" encompass attendance at job readiness workshops, career guidance, skills assessments, and job fairs. "Resume Services" include creating, updating, or receiving feedback on resumes and cover letters. "RESEA" captures required activities associated with the Reemployment Services and Eligibility Assessment (RESEA) program. "Other Services" includes all remaining service types not falling into the above categories. Table A.12 reports reduced-form effect of letter receipt on WorkSource engagement. Standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.12: Reduced-Form Effects of Letter Receipt on WorkSource Engagement

		Specific WorkSource Services				
	Any Service	Job Search	Workshop Career Services	Resume	RESEA	Other
Letter Treatment	0.004***	0.002**	0.001**	0.001**	0.001*	0.002**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Intercept	0.004	0.002	0.001	0.001	0.001	0.002
	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)
R^2	0.0003	0.0001	0.0001	0.0001	0.0001	0.0001
Control mean (DV)	0.004	0.002	0.001	0.001	0.001	0.002
N	50,213	50,213	50,213	50,213	50,213	50,213

Note: Each column reports an OLS regression of the effect of the informational letter on WorkSource usage. "Job Search Services" include activities such as job searching or applying to jobs and receiving referrals or staff assistance with job applications. "Workshop and Career Services" encompass attendance at job readiness workshops, career guidance, skills assessments, and job fairs. "Resume Services" include creating, updating, or receiving feedback on resumes and cover letters. "RESEA" captures required activities associated with the Reemployment Services and Eligibility Assessment (RESEA) program. "Other Services" includes all remaining service types not falling into the above categories. Table A.11 reports 2SLS effect of UI application on WorkSource engagement instrumented by the letter. Standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

A.3 Pre-Registered Specifications

Table A.13: Pre-Registered Specifications for UI Take-Up, Any Letter

	2 Months		4 Months		6 Months	
	Applications	Receipt	Applications	Receipt	Applications	Receipt
Letter Treatment	0.013	0.004	0.013	0.004	0.016	0.006
	(0.003)	(0.002)	(0.003)	(0.003)	(0.004)	(0.003)
	[0.000]	[0.012]	[0.000]	[0.160]	[0.000]	[0.063]
Previous UI	0.013	0.007	0.017	0.010	0.024	0.016
	(0.003)	(0.002)	(0.004)	(0.003)	(0.004)	(0.003)
	[0.000]	[0.003]	[0.000]	[0.000]	[0.000]	[0.000]
Treatment $\times$ Previous UI	-0.005	-0.001	-0.005	-0.001	-0.0045	-0.0002
	(0.004)	(0.003)	(0.004)	(0.003)	(0.005)	(0.004)
	[0.126]	[0.602]	[0.296]	[0.857]	[0.356]	[0.956]
Intercept	0.008	0.006	0.017	0.011	0.0207	0.014
	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)
	[0.001]	[0.002]	[0.000]	[0.000]	[0.000]	[0.000]
R^2	0.0014	0.0008	0.0018	0.0013	0.0029	0.0026
Control mean (dep. var.)	0.015	0.010	0.027	0.017	0.035	0.023
N	50,213	50,213	50,213	50,213	50,213	50,213

Note: Table shows results from preregistered main equation for impacts of informational letters on application and receipt from specification (1) of pre-analysis plan from author's website. Given randomized treatment is stratified according to whether a worker has previously received UI, the baseline specification interacts treatment with this previous UI experience. Claiming outcomes are measured two, four, and six months after letters are mailed.

Table A.14: Pre-Registered Specifications for UI Take-Up, Letter Treatment Arms

	2 Months	
	Applications	Receipt
Generic Letter	0.011	0.004
	(0.003)	(0.002)
	[0.000]	[0.034]
Destigmatizing Message	-0.003	-0.002
	(0.003)	(0.002)
	[0.330]	[0.288]
Search Expectations Message	-0.004	-0.002
	(0.003)	(0.002)
	[0.194]	[0.377]
Benefit Salience Message	-0.002	-0.002
	(0.003)	(0.002)
	[0.516]	[0376]
Destigmatizing × Search	0.010	0.006
	(0.004)	(0.003)
	[0.017]	[0.069]
Destigmatizing × Benefit	0.003	0.004
	(0.004)	(0.003)
	[0.434]	[0.168]
Benefit × Search	0.005	0.005
	(0.004)	(0.003)
	[0.215]	[0.079]
Destigma × Benefit × Search	-0.011	-0.011
	(0.006)	(0.004)
	[0.060]	[0.013]
Intercept	0.0152	0.0097
	(0.0016)	(0.0011)
	[0.000]	[0.000]
R^2	0.0008	0.0008
Control mean (dep. var.)	0.015	0.010
N	50,213	50,213

Note: Table shows results from pre-registered "long" OLS model for differential impacts of various letter treatment arms on application and receipt from specification (2) of pre-analysis plan from author's website. Claiming outcomes are measured two months after letters are mailed.

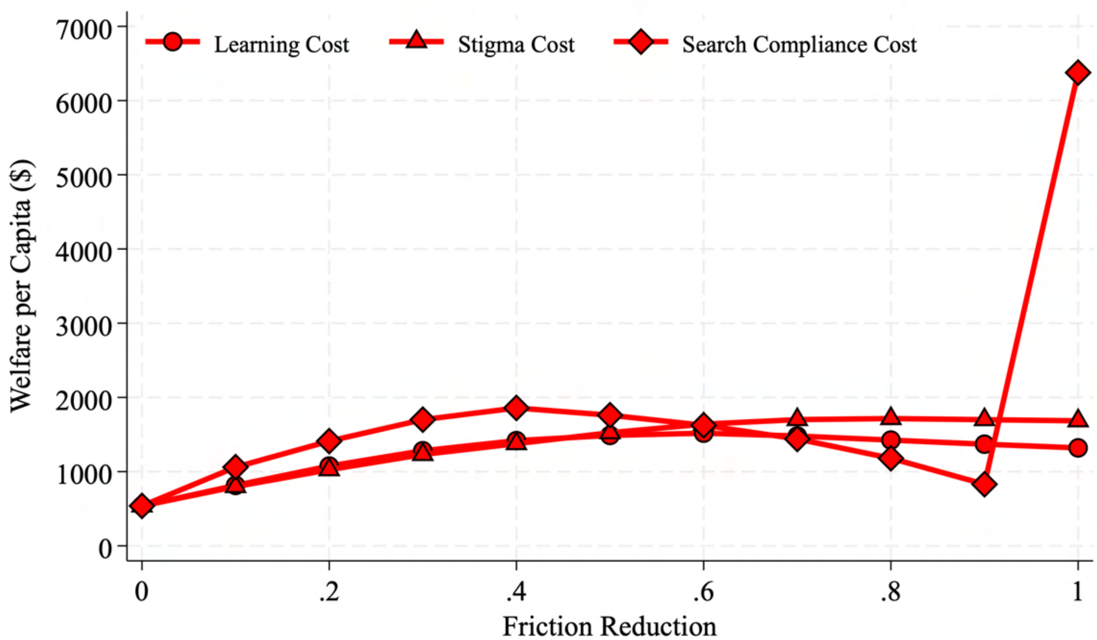
Table A.15: Pre-Registered Specifications for UI Take-Up, Letter Treatment Arms

	Applications			Receipt		
Generic Letter	0.0096***	0.0096***	0.0103***	0.0035*	0.0030*	0.0034*
	(0.003)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
	[0.000]	[0.000]	[0.000]	[0.012]	[0.030]	[0.016]
Destigmatizing Message	0.001			-0.0001		
	(0.001)			(0.001)		
	[0.514]					
Search Expectations Message		0.001			0.0008	
		(0.001)			(0.001)	
		[0.513]			[0.424]	
Benefit Salience Message			-0.0004			0.0002
			(0.001)			(0.001)
			[0.749]			[0.857]
Intercept	0.015	0.015	0.015	0.0096	0.0096	0.0096
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
R^2	0.0006	0.0008	0.0007	0.0001	0.0002	0.0001
Control mean (DV)	0.015	0.015	0.015	0.010	0.010	0.010
N	50,213	50,213	50,213	50,213	50,213	50,213

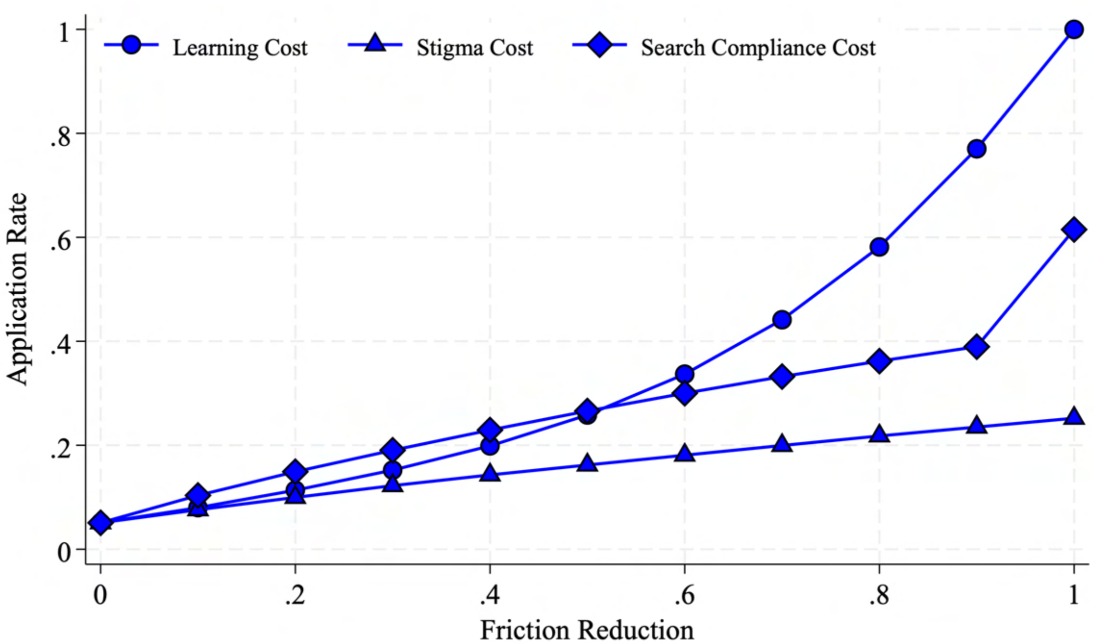
*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$

A.4 Empirical Model and Welfare Results

Figure A.20: Welfare and Application Responses to Reducing UI Access Frictions



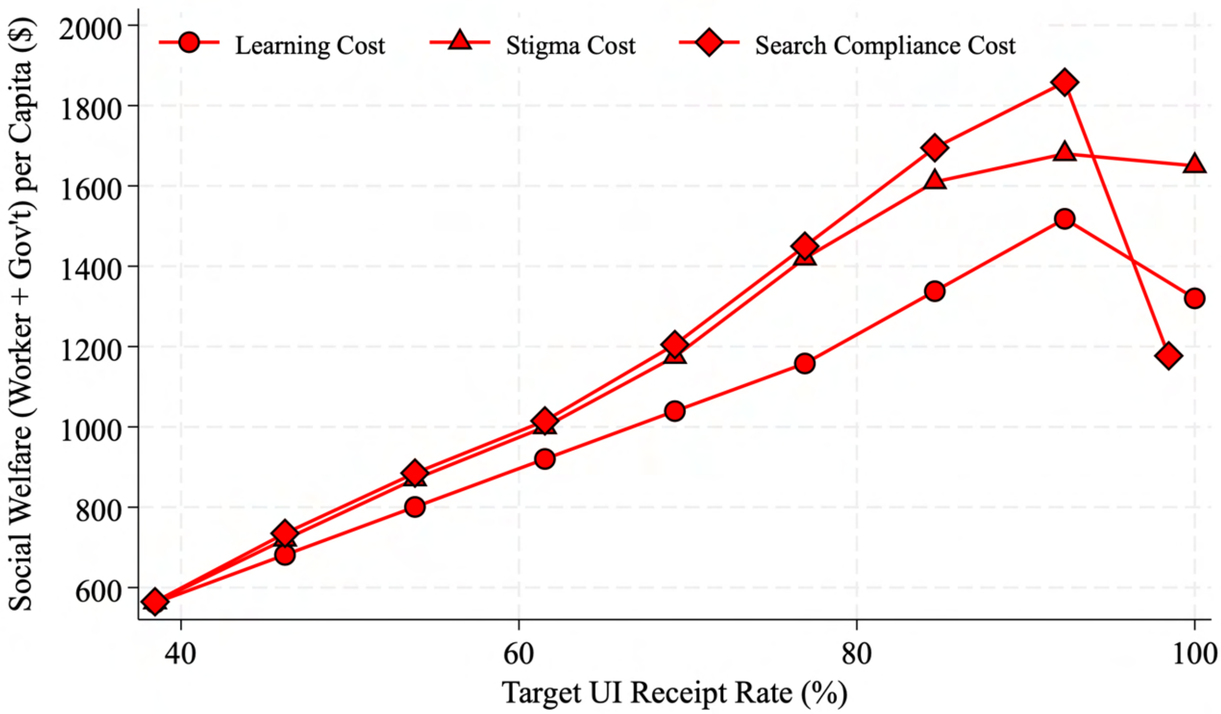
(a) Welfare per Capita ($)



(b) Application Rates

Note: This figure shows how simulated welfare per capita (top panel) and UI application rates (bottom panel) evolve as each friction — learning costs (κ), stigma costs (α), and search compliance costs (η_ℓ) — is gradually reduced from its estimated baseline to zero.

Figure A.21: Welfare Gains from Expanding UI Receipt Through Different Friction Reductions



Note: This figure compares the welfare implications of reducing three distinct frictions — learning costs (κ), stigma costs (α), and search compliance costs (η_ℓ) — to achieve equivalent increases in UI receipt. Each curve plots the welfare per capita implied by the model for target receipt rates between 5% and 10%, obtained via counterfactual simulations.

Table A.16: Model Fit for All Targeted Moments

A. Parameter Estimates		
Description	**Parameter**	**Estimate**
Learning Cost	κ	\$3,205
Learning Cost Reduction from Letter	$\Delta\kappa$	\$584
Stigma Cost	α	\$93
Stigma reduction (destig. letter)	$\Delta\alpha$	\$7
Search Compliance Cost	η_ℓ	\$202
Job offer arrival claimants	λ_1	0.117
Job offer arrival non-claimants	λ_1	0.114
Share low search cost among eligibles	θ_ℓ	0.258
B. Model Fit		
Selected Moment	**Estimated Value**	**Targeted Value**
Control Arm		
Application Rate	0.051	0.017
Rejection $\mid$ Application: Quit/Fire (QF)	0.015	0.210
Rejection $\mid$ Application: Not Searching ($NASW$)	0.048	0.050
Re-Employment (after 3 quarters)	0.452	0.435
Generic Arm		
Application Rate	0.093	0.029
Rejection $\mid$ Application: Quit/Fire (QF)	0.038	0.226
Rejection $\mid$ Application: Not Searching ($NASW$)	0.115	0.093
Re-Employment (after 3 quarters)	0.453	0.445
Destigmatization Arm		
Application Rate	0.109	0.033
Rejection $\mid$ Application: Quit/Fire (QF)	0.051	0.229
Rejection $\mid$ Application: Not Searching ($NASW$)	0.146	0.095
Re-Employment (after 3 quarters)	0.453	0.442

Note: Table shows full set of 12 targeted moments for all three treatment arms for the baseline estimation in Table 5, which shows model fit only for select moments.

Table A.17: Policy Counterfactuals: Reducing Frictions to UI Take-Up

	Baseline	Counterfactual	
Panel A. Reduce Learning Costs		$\kappa \to \frac{\kappa}{2}$	$\kappa \to 0$
Overall receipt rate	0.048	0.129	0.134
Overall application rate	0.051	0.258	1.000
From eligible, low-cost ($A_{1\ell}$)	0.356	0.965	1.000
From eligible, high-cost (A_{1h})	0.006	0.234	1.000
From ineligible (A_0)	0.002	0.080	1.000
Monthly job-finding rate	0.064	0.064	0.064
3-quarter re-employment (all workers)	0.453	0.453	0.453
Claimants	0.459	0.459	0.459
Non-claimants	0.452	0.452	0.452
Panel B. Reduce Stigma Costs		$\alpha \to \frac{\alpha}{2}$	$\alpha \to 0$
Overall receipt rate	0.048	0.108	0.129
Overall application rate	0.051	0.144	0.252
From eligible, low-cost ($A_{1\ell}$)	0.356	0.802	0.962
From eligible, high-cost (A_{1h})	0.006	0.070	0.224
From ineligible (A_0)	0.002	0.020	0.076
Monthly job-finding rate	0.064	0.064	0.063
3-quarter re-employment (all workers)	0.453	0.453	0.452
Claimants	0.459	0.458	0.449
Non-claimants	0.452	0.452	0.452
Panel C. Reduce Search Compliance Costs		$\eta_\ell \to \frac{\eta_\ell}{2}$	$\eta_\ell \to 0$
Overall receipt rate	0.048	0.130	0.134
Overall application rate	0.051	0.266	0.415
From eligible, low-cost ($A_{1\ell}$)	0.356	0.969	0.997
From eligible, high-cost (A_{1h})	0.006	0.246	0.476
From ineligible (A_0)	0.002	0.085	0.204
Monthly job-finding rate	0.064	0.063	0.063
3-quarter re-employment (all workers)	0.453	0.452	0.452
Claimants	0.459	0.450	0.449
Non-claimants	0.452	0.452	0.452

Notes: Baseline corresponds to the model-estimated control environment. Job-finding rate is the claimant-weighted average of monthly job-finding rates. 3-quarter reemployment is the probability of reemployment within 39 weeks. Type-specific entries ($A_{1\ell}, A_{1h}, A_0$) are model-implied application rates by true type.

B Model Appendix

This appendix section exposits a partial equilibrium job search model with endogenous take-up to clarify intuition for how easing the barriers to take-up affects an unemployed worker's search behavior. The model extends McCall (1970).

B.1 Further Model Exposition

Type-specific application rates among eligible low-cost types ($A_{1\ell}$), eligible high-cost types (A_{1h}), and separation ineligibles (A_0) expressed in terms of model primitives are provided below.

Eligible low-cost types (approved):

$$A_{1\ell} = \Pr(\text{Apply}|e = 1, s = \ell) = 1 - H_\ell(\widehat{\pi}^*|e = 1, s = \ell)$$

Eligible high-cost types (do not certify search, denied):

$$A_{1h} = \Pr(\text{Apply}|e = 1, s = h) = 1 - H_h(\widehat{\pi}^*|e = 1, s = h)$$

Separation-ineligibles (denied):

$$A_0 = \Pr(\text{Apply}|e = 0) = 1 - G_0(\widehat{p}^*)$$

B.2 Identifying Model Primitives for Simulated Method of Moments

This section provides an identification map between our unknown parameters and the moments from our data. We are estimating eight unknowns:

$$\underbrace{\lambda_C, \lambda_N}_{\substack{\text{job offer rate} \\ \text{claimants/non}}}, \quad \underbrace{\eta_\ell}_{\substack{\text{compliance cost} \\ \text{of search on UI}}}, \quad \underbrace{\theta_\ell}_{\substack{\text{share low-} \\ \text{search cost}}}, \quad \underbrace{\kappa, \Delta\kappa}_{\substack{\text{level \& change} \\ \text{learning cost}}}, \quad \underbrace{\alpha, \Delta\alpha}_{\substack{\text{level \& change} \\ \text{stigma cost}}}$$

B.2.1 Transforming Cutoff Decision Rules for Identification

To identify our moments, we can write mappings to the cutoff from the decision rule expressed in equation (4) in the paper. In particular, for each treatment arm $a \in \{C, G, S\}$ for the control (C), generic letter (G), and the stigma (S) arm, we can denote the "scaled" learning cost κ as

$$K^C = \frac{\kappa}{V_l^C(\alpha, \eta_\ell) - V^N(\lambda_N)} \qquad K^G = \frac{\kappa - \Delta\kappa}{V_l^C(\alpha, \eta_\ell) - V^N(\lambda_N)} \qquad K^S = \frac{\kappa - \Delta\kappa}{V_l^C(\alpha - \Delta\alpha, \eta_\ell) - V^N(\lambda_N)}$$

For each treatment arm $a \in \{C, G, S\}$, we can express the application rates from the low-search cost eligibles ($e = 1, s = \ell$), high-search cost eligibles ($e = 1, s = h$), and separation ineligibles ($e = 0$):

$$A_{1\ell}^a(K^a) = \int \left[1 - H_\ell\left(\frac{K^a}{p}\right)\right] dG_1(p)$$

$$A^a_{1h}(K^a) = \int \left[1 - H_h\left(\frac{K^a}{p}\right)\right] dG_1(p)$$

$$A^a_0(K^a, \theta_\ell) = \int \left[1 - \Big(\theta_\ell H_\ell + (1-\theta_\ell)H_h\Big)\left(\frac{K^a}{p}\right)\right] dG_0(p)$$

which then allows us to produce overall application rate and reason-specific rejection rates, each weighted by the appropriate shares of eligibility and search-types:

$$App^a(K^a, \theta_\ell) = \underbrace{\theta\theta_\ell A^a_{1\ell}}_{\substack{\text{wgted app rate} \\ \text{elig low cost}}} + \underbrace{\theta(1-\theta_\ell)A^a_{1h}}_{\substack{\text{wgted app rate} \\ \text{elig high cost}}} + \underbrace{(1-\theta)A^a_0}_{\substack{\text{wgted app rate} \\ \text{ineligibles}}}$$

$$R^a_{QF} = \frac{(1-\theta)A^a_0}{App^a} \qquad R^a_{NASW} = \frac{\theta(1-\theta_\ell)A^a_{1h}}{App^a}$$

B.2.2 Identifying All Seven Parameters

1. Share of low-search cost types: θ_ℓ

We can use composition shares to pin down θ_ℓ:

$$S_0 \equiv \widehat{R}^C_{QF} \qquad S_h \equiv \widehat{R}^C_{NASW} \qquad S_l \equiv 1 - S_0 - S_h$$

Let the denominator of the shares be represented as

$$D(\theta_\ell) = \theta\theta_\ell A_{1l} + \theta(1-\theta_\ell)A_{1h} + (1-\theta)A_0$$

Then we can express the shares in terms of the model:

$$S_0 = \frac{(1-\theta)A_0}{D(\theta_\ell)} \qquad S_h = \frac{\theta(1-\theta_\ell)A_{1h}}{D(\theta_\ell)} \qquad S_l = \frac{\theta\theta_\ell A_{1l}}{D(\theta_\ell)}$$

Taking the ratio of the share of high-search cost and low-search types observed in the data:

$$\frac{S_h}{S_\ell} = \frac{\theta(1-\theta_\ell)A_{1h}}{\theta\theta_\ell A_{1l}} = \frac{1-\theta_\ell}{\theta_\ell} \cdot \frac{A_{1h}}{A_{1l}} \iff \theta_\ell = \frac{1}{1 + \frac{S_h}{S_\ell}\frac{A_{1l}}{A_{1h}}}$$

2. Relative reduction in learning cost: $\Delta\kappa/\kappa$

With θ_ℓ fixed, we next turn to how much the informational letter reduces the learning cost via $\Delta\kappa$. For each treatment arm $a \in \{C, G\}$, the threshold mapping is

$$K^C = \frac{\kappa}{V^C_\ell(\alpha, \eta_\ell) - V^N(\lambda_N)} \qquad K^G = \frac{\kappa - \Delta\kappa}{V^C_\ell(\alpha, \eta_\ell) - V^N(\lambda_N)}$$

Notice that the denominators are the same — the only difference is that, in the generic letter arm, the learning cost is reduced by $\Delta\kappa$. Dividing the two expressions gives

$$\frac{K^G}{K^C} = \frac{\kappa - \Delta\kappa}{\kappa} = 1 - \frac{\Delta\kappa}{\kappa}$$

Hence, the relative reduction in learning cost can be identified from the ratio of the two thresholds.

What remains is to link these model thresholds K^a to observables. For each arm, we can invert the observed application composition — specifically, the empirical rejection shares $\widehat{R}^a_{QF}$, $\widehat{R}^a_{NASW}$ — to recover an empirical analogue $\widehat{K}^a$. Plugging these in gives the estimable relation:

$$1 - \frac{\Delta\kappa}{\kappa} = \frac{K^G}{K^C} \approx \frac{\widehat{K}^G}{\widehat{K}^C}.$$

3. Reduction in stigma cost: $\Delta\alpha$

With θ_ℓ already fixed and $\Delta\kappa/\kappa$ pinned down from the previous step, we now turn to the effect of destigmatizing letters on stigma cost $\Delta\alpha$.

For each treatment arm $a \in C, S$, the threshold mapping is:

$$K^C = \frac{\kappa}{V_l^C(\alpha, \eta_\ell) - V^N(\lambda_N)} \qquad K^S = \frac{\kappa - \Delta\kappa}{V_l^C(\alpha - \Delta\alpha, \eta_\ell) - V^N(\lambda_N)}$$

Notice that compared to K^C, the numerator is again reduced by $\Delta\kappa$ (as in the generic letter arm), but now the denominator is also shifted because stigma costs are lowered by $\Delta\alpha$. This implies that:

$$\frac{K^S}{K^C} = \frac{\frac{\kappa - \Delta\kappa}{V_l^C(\alpha - \Delta\alpha, \eta_\ell) - V^N(\lambda_N)}}{\frac{\kappa}{V_l^C(\alpha, \eta_\ell) - V^N(\lambda_N)}}$$

Rearranging,

$$\frac{K^S}{K^C} = \left(1 - \frac{\Delta\kappa}{\kappa}\right) \cdot \frac{V_l^C(\alpha, \eta_\ell) - V^N(\lambda_N)}{V_l^C(\alpha - \Delta\alpha, \eta_\ell) - V^N(\lambda_N)}$$

Hence, the relative shift in stigma cost can be identified by comparing the ratio K^S/K^C (observable via rejection shares in arms S and C) to the learning-cost reduction term $1 - \Delta\kappa/\kappa$ already identified in Step 2.

Finally, as before, we connect these model thresholds K^a to observables: inverting the application composition in arms C and S (using $\widehat{R}^a_{QF}$, $\widehat{R}^a_{NASW}$, and optionally $\widehat{App}^a$) gives empirical analogues $\widehat{K}^C$ and $\widehat{K}^S$. Plugging in yields:

$$\frac{\widehat{K}^S}{\widehat{K}^C} \approx \left(1 - \frac{\Delta\kappa}{\kappa}\right) \cdot \frac{V_\ell^C\left(\alpha, \eta_\ell\right) - V^N\left(\lambda_N\right)}{V_\ell^C\left(\alpha - \Delta\alpha, \eta_\ell\right) - V^N\left(\lambda_N\right)}.$$

4. Nonclaimant job offer arrival rate (from reemployment): λ_N

With θ_ℓ fixed and $\widehat{K}^C$ and $\widehat{K}^G$ already backed out, we can compute the share who actually receives

UI (i.e., approved and comply with search requirements) among the unemployed in each arm.

$$q^a \equiv \theta_e \theta_\ell A^a_{1\ell}(K^a) \qquad \text{(UI recipients)}$$

Let T denote the evaluation horizon in terms of weeks (e.g., $T = 39$ for three quarters). Define weekly job-finding hazards as the probability a claimant or nonclaimant in treatment arm a receives an offer above their reservation wage:

$$\phi^a_1 \equiv Pr(w \geq r^a_C)$$

where r^a_C is the reservation wage implied by the claimant Bellman with α_a and η_ℓ (in arms C and G, $\alpha_a = \alpha$; in arm S, $\alpha_a = \alpha - \Delta\alpha$).

$$\phi^a_0(\lambda_N) \equiv \lambda_N \cdot Pr(w \geq r_N)$$

Note that the reservation wage for the nonclaimants is not affected by treatment arm a because they are neither paying a potentially lower learning cost $\kappa - \Delta\kappa$ nor paying a lower stigma cost $\alpha - \Delta\alpha$.

We can then express whether a worker is ever-employed within T weeks in arm a by

$$Emp^a = \underbrace{q^a\left[1-(1-\phi^a_1)^T\right]}_{\substack{\text{UI claimants re-employed}\\ \text{within } T \text{ weeks}}} + \underbrace{(1-q^a)\left[1-(1-\phi_0(\lambda_N))^T\right]}_{\substack{\text{non-claimants re-employed}\\ \text{within } T \text{ weeks}}}. \tag{8}$$

One implication of the model is that the search environment for the control and generic-arm *claimants* are identical, as α and η_ℓ coincide, so $\phi^C_1 = \phi^G_1 \equiv \phi_1$. However, the share of UI claimants across C and G arms is not equal because the generic letter changes fixed learning costs and thus changes take-up.

Using the two observed reemployment moments $\widehat{Emp}^C$, $\widehat{Emp}^G$, together with the known q^C, q^G, equation (8) delivers two equations in the two unknowns (ϕ_1, λ_N). Solving these pins down the non-claimant offer rate λ_N and yields ϕ_1 as a byproduct.

The stigma arm then provides a third moment $\widehat{Emp}^S$ where the claimant hazard changes because α is reduced, increasing the effective value of UI benefits:

$$\phi^S_1 = Pr(w \geq r^S_C(\alpha - \Delta\alpha, \eta_\ell)) \neq \phi_1$$

Together, these three reemployment moments ($\widehat{Emp}^C$, $\widehat{Emp}^G$, $\widehat{Emp}^S$) discipline both sides of the job search process: the baseline offer arrival rate for nonclaimants λ_N and claimant hazards ϕ_1 and ϕ^S_1.

We have now pinned down the job-finding environment, characterizing how incentives differ across claimants and non-claimants. With λ_N in hand, we are now positioned to turn to the levels of stigma and compliance costs, α and η_ℓ, which govern the value of claiming relative to nonclaiming.

5. Stigma level: α

With $\widehat{\pi}$, $\widehat{K}^C$, $\widehat{K}^G$, $\widehat{K}^S$, and $\widehat{\lambda}_0$ in hand, we identify the *level* of stigma α costs. The key identification comes from comparing claimant hazards across the control and stigma arms.

In the control (and generic) arms, the claimant hazard depends on reservation wages formed with stigma level α:

$$\phi_1(\alpha, \eta_\ell) = \Pr\big(w \geq r^C(\alpha, \eta_\ell)\big).$$

In the stigma arm, the hazard is instead

$$\phi_1^S(\alpha - \Delta\alpha, \eta_\ell) = \Pr\big(w \geq r^S(\alpha - \Delta\alpha, \eta_\ell)\big).$$

Using the observed reemployment moment $\widehat{Emp}^S$, together with the known claimant share q^S, recovers $\widehat{\phi}_1^S$. Alongside the control arm analogue $\widehat{\phi}_1$, this delivers two equations linking α (and η_ℓ) to observed hazards.

An overidentifying restriction comes from the ratio of thresholds K^S/K^G. Because both numerators include $(\kappa - \Delta\kappa)$, the difference in K^S versus K^G isolates the denominator shift induced by $\Delta\alpha$. Taken together, these two pieces allow us to discipline the level of stigma cost α, conditional on η_ℓ.

6. Search compliance cost: η_ℓ

With θ_ℓ, $\Delta\kappa/\kappa$, $\Delta\alpha$, λ_N, and α already disciplined, we must also identify the compliance cost of search for low-type eligibles, η_ℓ. This parameter matters only for claimants, since high-search cost types opt not to become non-claimants and face no search requirement.

In the Bellman equation for claimants, η_ℓ enters additively with the stigma cost α:

$$V_\ell^C = (b - \alpha - \eta_\ell) + \beta \mathbb{E}\left[V_\ell^C(w')\right]$$

so higher η_ℓ reduces the flow value of claiming and lowers the reservation wage $r^C(\alpha, \eta_\ell)$.

Empirically, this force shows up most directly in the composition of rejection rates by treatment arm. In particular, a higher η_ℓ implies that more eligible high-cost types ($s = h$) will self-select out of UI, reducing their application rate A_{1h}^a and thereby lowering the share of NASW rejections:

$$\widehat{R}_{NASW}^a = \frac{\theta_e(1-\theta_\ell)A_{1h}^a}{App^a}.$$

Identification of η_ℓ therefore comes from matching the model's predicted R_{NASW}^a to the empirical analogue $\widehat{R}_{NASW}^a$ in each treatment arm $a \in \{C, G, S\}$. Because α has been pinned down, variation in R_{NASW}^a across arms isolates the role of compliance costs.

7. Learning cost: κ Having identified all parameters except κ, the level of learning cost κ follows from the control arm's threshold and the value gap.

After inverting application composition to obtain $\widehat{K}^a$, we get the level of κ directly from the control arm:

$$\kappa = \widehat{K}^C \cdot (V_\ell^C(\alpha, \eta_\ell) - V^N(\lambda_N))$$

Two equivalent expressions for overidentification come from the two treatment arms:

$$\kappa = \Delta\kappa + \widehat{K}^G \cdot (V_\ell^C(\alpha, \eta_\ell) - V^N(\lambda_N))$$

$$\kappa = \frac{\widehat{K}^S\Big(V_l^C(\alpha - \Delta\alpha, \eta_\ell) - V^N(\lambda_N)\Big)}{\Big(1 - \Delta\kappa/\kappa\Big)}$$

B.3 Model Proofs

Proof of Take-Up Decision Rule
A worker perceives the expected value of applying for benefits as

$$V_i^{apply} = -\kappa + \widehat{p}\left(\widehat{\pi}V_\ell^C + (1-\widehat{\pi})V_h^C\right) + (1-\widehat{p})V^N$$

because the worker believes with probability $\widehat{p}$ they are separation eligible and with probability $\widehat{\pi}$ that they are low search cost type η_l. Applying with $e = 1$ and having low search costs means workers enter the claiming state V^C. Otherwise, their application is rejected and they enter the nonclaiming state V^N (we let V_h^C, the value of trying to claim with a high search cost, equal V^N). The worker incurs learning cost κ regardless of approval.

The worker chooses to apply if $V^{apply} > V^N$, meaning they will apply if

$$-\kappa + \widehat{p}\left(\widehat{\pi}V_l^C + (1-\widehat{\pi})V_h^C\right) + (1-\widehat{p})V^N > V^N$$

Subtracting V^N from both sides, collecting common term $\widehat{p}$, and substituting $V_h^C = V^N$:

$$-\kappa + \widehat{p}(\widehat{\pi}V_l^C + (1-\widehat{\pi})V^N - V^N) > 0$$

Moving the learning term to the RHS and simplifying terms:

$$\widehat{p} \cdot \widehat{\pi} \cdot (V_l^C - V^N) > \kappa$$

Lastly, dividing through, we obtain

$$\widehat{p} \cdot \widehat{\pi} \geq \frac{\kappa}{V_\ell^C - V^N} \qquad \blacksquare$$

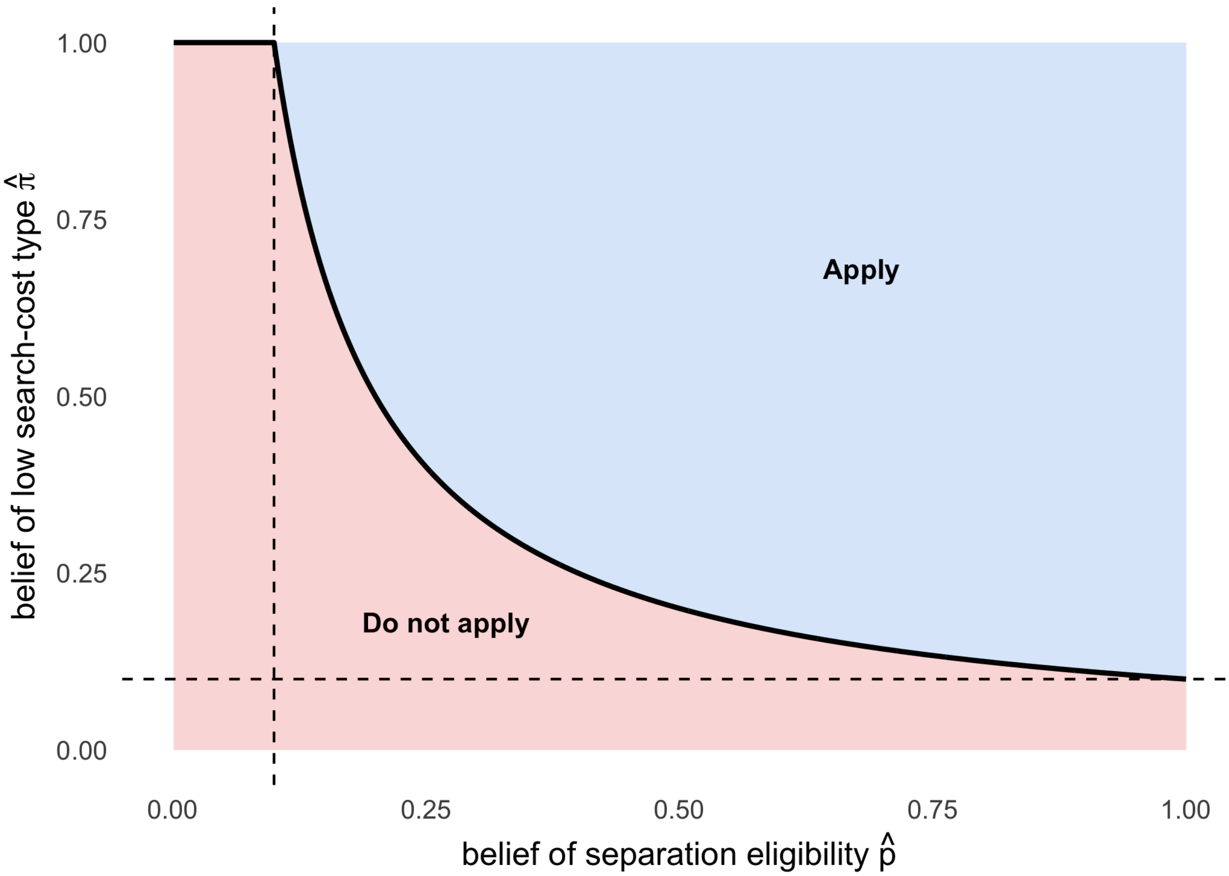
Proof of Proposition 1
Proposition 1: *Suppose an intervention leaves eligibility beliefs among the ineligible ($e = 0$) unchanged and leaves beliefs about one's own search-compliance type s unchanged. If the intervention raises UI applications but neither the quit/fired rejection share R_{QF} nor the actively seeking work rejection share R_{NASW} falls, then eligibility belief-updating among low-cost eligible workers cannot be the dominant take-up mechanism; there must also be an increase in applications from ineligible workers or from high-cost eligible types, consistent with a reduction in learning costs κ.*

Proof: To prove by contradiction: assume the intervention increases total UI applications but neither R_{QF} nor R_{NASW} falls, while belief-updating among low-search cost eligible workers is the only mechanism by which applications rise.

By assumption, beliefs of ineligibles ($e = 0$) are unchanged, so total ineligible applications A_0 is fixed. Beliefs about one's own search–compliance type s are also unchanged, so applications from high–cost eligible workers A_{1h} are fixed.

Under these assumptions, the only change in behavior is that applications among low–cost eligibles $A_{1\ell}$ increase.

Figure B.1: Joint Beliefs of Separation Eligibility and Search Type for Application



Holding A_0 and A_{1h} fixed, consider how these rates vary with $A_{1\ell}$:

$$\frac{\partial R_{QF}}{\partial A_{1\ell}} = -\frac{(1-\theta)\theta\pi A_0}{D^2} < 0, \quad \frac{\partial R_{NASW}}{\partial A_{1\ell}} = -\frac{\theta^2\pi(1-\pi)A_{1h}}{D^2} < 0,$$

where $D = \theta\pi A_{1\ell} + \theta(1-\pi)A_{1h} + (1-\theta)A_0$ is the total number of applications.

Thus, if applications rise only through $A_{1\ell}$, both R_{QF} and R_{NASW} must strictly fall. This contradicts the assumption that applications rise but neither rejection share falls.

Therefore, some other mechanism must also be operative: either an increase in applications from ineligibles ($A_0 \uparrow$) or from high–cost eligibles ($A_{1h} \uparrow$), consistent with a reduction in learning costs κ. ■

C Experiment Details

C.1 Experimental Sample Details

Industry and Wage Restrictions. We conduct the experiment on a sample of workers restricted according to their industry and wage information. Specifically, we exclude workers who earned more than \$40 per hour in the two quarters prior to job loss[37] or who were most-recently employed in one of the following five industries: Mining, Quarrying, and Oil and Gas Extraction (NAICS 21), Information (51), Finance and Insurance (52), Management of Companies and Enterprises (55), and Educational Services (61). The reason for these restriction is that workers in these industries or earning above this threshold are more likely able to self-insure through savings or severance pay and are less frequently the population of concern for policymakers interested in expanding UI take-up.

The educational services sector is characterized by considerable seasonality whereby school employees do not work in the summer months but also do not search for a new job because their employment will restart in the fall. This frequent pattern observed among education employees would render them ineligible for UI, so including the sector in the experiment would disproportionately include many individuals who do not consider themselves eligible for UI. As such, our measured take-up rate (and effects) would be biased downwards given the denominator which seeks to proxy for UI-eligible potential job losses would be artificially high.

Identifying Potential Job Losses. Among this restricted sample, we apply a straightforward "hours-based" rule to detect potential job losses, described in the main paper as "a period of stable employment followed by a significant reduction in hours worked." Specifically, workers must show at least a 30% decrease in hours between quarters $t-1$ and t. We set additional conditions: workers should have worked at least 365 hours in both quarters $t-2$ and $t-1$, but no more than 700 hours per quarter, ensuring that a 30% reduction remains sufficiently large in absolute terms but also below full-time employment levels. [38] We limit our sample to workers with a single employer over the three quarters ($t-2$, $t-1$, and t) to simplify the identification of likely job losses.

Such a definition disallows workers who are laid off at the start of quarter t and find quick reemployment with a new firm within the same quarter. Table C.1 illustrates the hypothetical work history of an individual who separates two weeks into quarter t and finds reemployment at a new firm with two weeks left in the same quarter. Despite the large drop in hours worked indicative of a job loss, such workers are not strong candidates for the experiment because the window of time they would have been eligible for UI has already passed due to their reemployment.

Matching to Address Records. Washington State ESD collects and stores the addresses of all previous unemployment insurance applicants. To expand the scope of the experiment beyond those with previous UI experience, the ESD entered a data sharing agreement with the Washington State Department of Licensing (WADOL), the state agency that issues driver's licenses. The WADOL include the most recent mailing address of every Washington resident with a valid driver's license[39], along with the license holder's full name, last four digits of their Social Security Number (SSN), sex,

[37] Specifically, for job losses occurring in quarter t, we restrict that all workers' hourly wages fall below $(earn_{t-2} + earn_{t-1})/(hours_{t-2} + hours_{t-1})$.

[38] A full-time, full-quarter workload is generally 520 hours (13 weeks $\times$ 40 hours/week), whereas a 30% reduction from 700 hours results in 490 hours.

[39] Roughly 90% of the 16+ civilian population holds a driver's license. Mailing addresses must be updated every six years.

date of birth, disability status, and veteran status.

To merge the wage and WADOL records, we match on individual names within groups according to the last four digits of SSN, which drastically reduces the dimensionality of the string matching. The string matching itself uses the stringdist function in R, matching every name in the wage records with the best corresponding name in the WADOL data based on their Jaro-Winkler score. A match was considered sufficiently accurate if the distance score was less than 0.1. We manually verified matches below this threshold to confirm their reliability, ensuring that only accurate matches were used for merging wage records with driver's license addresses. All workers in the 2024Q1 and 2024Q2 wage records (i.e., workers who lost their jobs in those quarters) matched to the WADOL data at a rate of 74% using this method.

Workers with addresses in the WADOL data were assigned this address for the purposes of mailing informational letters. Workers who remained unmatched to the WADOL data could still qualify for the experimental sample if they had an address on file with ESD due to previously claiming UI benefits. Using internal ESD addresses from previous UI claims expands address coverage in the overall sample of workers (i.e., before job loss and other restrictions) by roughly 25%. For the significant portion of the sample for whom we have addresses from both data sources, we validate the quality of the WADOL address by observing that 89% of the time, the WADOL ZIP code aligns with the ESD-provided ZIP code.

Removing Public Beneficiaries. Lastly, we remove workers who experienced a potential job loss but are otherwise poor candidates for our experimental sample because they have (1) already claimed UI benefits, (2) filed for Washington's publicly-funded Paid Family and Medical Leave (PFML) benefits, or (3) filed for Workers' Compensation benefits. Specifically, we exclude workers who opened a UI claim account in the 365 days prior to a letter's mailing date.[40] We exclude workers who opened a PFML claim with an initial date as early as one month prior to the quarter of their potential job loss and as late as one year following that date.[41] This conservative approach ensures we do not misclassify separations due to PFML-qualifying reasons as UI-eligible job losses instead. Lastly, for the second wave of the experiment, we also exclude workers with an active Workers' Compensation claim as of May 2024. If a worker separated in 2024Q2 for a reason related to workers' compensation (and are thus not UI-eligible), they would likely appear in these records by May 2024.[42]

C.1.1 Estimating Sample UI Take-Up Rate

Table C.3 presents the "waterfall" of how each restriction affects the final experimental sample in each wave. The industry and wage restrictions discard roughly 25% of the monetarily eligible working population of Washington. Among our restricted industries of workers earning less than $40/hour, 2.1% of them experience a potential job loss according to our definition. Using national figures from

[40] Although the maximum potential benefit duration of UI benefits is 26 weeks in Washington during the time of our experiment, workers have 52 weeks to draw their 26 weeks of benefits from the time of their effective date of claim. As such, if a worker already had an open claim at the time they received an informational letter, they would not need to go through the same steps as an initial claimant in order to draw benefits. Hence, we exclude all workers with an open UI claim account at the time of the letter's mailing.

[41] Unlike UI, PFML claimants are allowed to file claims with effective start dates far into the future due to life events such as pregnancy. In our second wave, for example, we excluded all workers who (at the time of mailing) filed a PFML claim with a start date between March 1, 2024 and April 1, 2025.

[42] The workers' compensation restriction is only made in the second wave of the experiment due to a delay in facilitating a data sharing agreement between ESD and the WA Department of Labor & Industries, the agency which maintains the workers' compensation records.

the Current Population Survey, the monthly job loss rate as measured by the fraction of workers transitioning from employed to unemployed as a share of all employed workers hovered around 1% nationally during the time of our experiment. Our measure job loss spans a quarter rather than a month and likely incorporates some transitions out of the labor force (retirements, returning to school, etc.), suggesting our 2.1% is an appropriate magnitude.

Among our sample of potential job losses, we successfully matched a mailing address for 79% of the sample. A substantial share of this group claimed either UI or PFML during the relevant time period that renders them ineligible for the experimental sample. A very small share claimed workers compensation. When we remove PFML and workers' compensation claimants from the denominator of potential job losses to proxy for UI-eligible separations, we calculate a UI take-up rate of 28% across both waves at the time of mailing. This estimate is a conservative lower bound because many potential job losses may include separations for other reasons that are not UI eligible (e.g., quits, fired for cause). As a back-of-the-envelope calculation, assuming half of the experimental sample is UI-ineligible suggests that the pre-intervention UI take-up rate in our sample is 43.8%.

C.2 Mailer Campaign Design and Implementation

The ESD mailed the informational letters on May 21, 2024 and August 20, 2024 from its headquarters in Olympia, WA to each wave of recipients. These dates were selected because tax records used to identify potential job losses are due one month after the end of the quarter. Mailers are sent out three weeks after this due date to allow for late wage record submissions (which could otherwise lead to false positive job loss classifications) and to provide sufficient time for ESD's mail room to print and process tens of thousands of letters. Unlike other information provision experiments that send out tens of thousands of mailers in smaller batches over several weeks (e.g., Finkelstein and Notowidigdo (2019)), the Washington ESD executed an impressive logistical effort by distributing all letters simultaneously. This rapid deployment is particularly critical in the context of unemployment due to the relatively short duration of joblessness spells. The ability to reach recipients without delay maximizes the relevance and impact of the information being provided.

Mailers featured the Washington ESD logo on both the envelope (Figure C.2) and letterhead itself. The research team drafted the content of the letters in collaboration with ESD's Communications, Customer Service, Product, and Legal departments over the course of many months. To ensure deliverability despite address changes, the mailings were sent using the United States Postal Service's Change of Address (COA) service, which automatically reroutes letters to a recipient's new address if a permanent or temporary forwarding request is on file.

In June 2023, the research team conducted one-on-one interviews to test various messages in the letter with potential UI claimants at a WorkSource office in Seattle, WA, shown in Figure C.1. There are 38 WorkSource offices in Washington state that offer employment services for job seekers. Although workers cannot physically fill out an application for UI in-person at a WorkSource office, they are encouraged to apply via the phone or on computer terminals if they are eligible. Input from Washington workers and would-be claimants at WorkSource offices throughout the process of drafting the letters enhanced the efficacy of the final informational mailer.

Figure 2 shows a sample letter for the "generic information" treatment arm. The letter informs individuals that if they lost their job through no fault of their own, they may be entitled to UI benefits and explains what function UI serves. The letter emphasizes the ease of applying to UI and directs likely-eligible individuals (unemployed and searching for work) to apply online or over the phone, and includes additional numbers for Spanish language and disability accommodations.

All workers in the treatment group received letters with the above common bundle of information. Layered on top of this "generic" letter were three cross-randomized treatments that targeted various mechanisms that may drive incomplete take-up. In the order they appear on the letter, the first treatment is a "search" treatment, which provides information about the average amount of time it takes job seekers who have been out of work for two months to find reemployment. The second treatment is a "guilt" treatment, which seeks to reduce a potential claimant's guilt associated with receiving UI benefits by emphasizing benefit amounts are based on one's work history, not on neediness. The treatment also highlights that taxes were paid into the unemployment insurance trust fund while the individual was working. The third treatment is a "benefit salience" treatment which directs readers to an ESD website link where they can estimate their own UI benefit level, which ranged between $323 and $1,079 during the experiment. The "saturated" letter featuring all three messages is presented in Figure C.3.[43] Cross-randomization ensures half of all letter recipients randomly receive each statement and we can separately measure the causal effect of each individual treatment.

C.3 Potential Claimant Engagement with Informational Letters

To track differential engagement with each version of the letter, ESD's customer service division created eight customized phone lines that were only disclosed to letter recipients and not the general public. Thus, we can attribute all call volume to the causal effect of the letter. Figure C.4 plots the time series of the total number of calls to the specialized phone numbers during the 115 business days following the first wave of mailers. The figure shows a considerable spikes in call volume around each instance of batch mailing, including following the reminder letters sent after the second wave. Volume declines considerably following the initial spikes. For example, twenty business days after the first wave of mailing, ESD averaged roughly five calls per day to the lines. By fifty business days post-mailing, call volume essentially fell to zero. In the 115 business days that ESD maintained customized phone lines for the two waves of letters (May 21–October 31), ESD received 705 calls from 405 unique phone numbers, a call rate of just over 1% as a percentage of letters sent.

C.3.1 Queue Time, Abandonment Rate, Talk Time

54 percent of calls were ultimately abandoned by the caller before reaching a customer service agent. The mean (median) time to abandon was 14 (7) minutes. Among all callers (including those who abandoned), the mean (median) time spent in the queue was 20 (10) minutes.[44] Among callers who spoke with a customer service agent, the mean (median) talk time with an agent was 20 (15) minutes. In our qualitative observations during piloting, by studying audio recordings of phone conversations, calls that lasted more than 10 minutes with the agent frequently converted to submitted UI applications.

C.3.2 Demographic Sample of Callers

Table C.6 presents demographic and economic characteristics for the matched sample of callers (i.e. callers who can be identified by matching their phone number against internal ESD databases; see

[43] Individual treatments always appear in this specific order in the letter, rather than a randomized order. This decision was made in consultation with communications experts at ESD to enhance readability of the letter. Thus, if an individual received only the "search" and "benefit salience" treatments, search is always presented before benefit salience.

[44] We define time in the queue as both time spent interfacing with the automated menu as well as time on hold.

further discussion in Appendix D.3). Demographically, callers generally mirrored the larger population of unemployed workers who received letters with the notable exception of age. Those who called were, on average, ten years older than the typical letter recipient. Callers were slightly more likely to be veterans, report having a disability, and reside outside the Seattle metro area. Economically, callers resembled the broader sample of recipients, having only slightly lower wages and earnings in the previous year.

To test the relative effects of the individual treatments on call volume, we compare the call volume to the four lines with a given treatment (e.g., "guilt") to the call volume to the four lines without that treatment.

C.3.3 Letters Returned to Sender (ESD Headquarters)

ESD recorded and scanned all letters that were returned to agency headquarters as undeliverable. The research team manually recorded the number of total letters in the first wave that were returned to ESD and on what day. Ultimately, 5.5% of letters (1,386 of 25,220) that were sent out in the first wave were returned to ESD. We use this number in our calibrated estimates of frictions to claiming in section 7. Of the undeliverable letters, 78% were returned to agency headquarters and were received and processed by ESD within 8 business days of the mailing. See Table C.5 for a temporal breakdown of the undeliverable letters in the first wave.

Table C.1: Example of Hypothetical Hours History Excluded from Experiment

Quarter	$t-2$	$t-1$	t
Employer A Hours	520	520	80
Employer B Hours	0	0	80

Note: Example shows hours trajectory of a job loss that is excluded from our potential job loss sample because the job loss, while occurring in quarter t, has also concluded in quarter t. Thus, by the time the worker receives the letter in $t+1$, they would be employed and thus ineligible for UI.

Table C.2: Industry Take-Up Rate and Median Wages

Industry	Take-Up Rate	Median Hourly Wage
Educational Services	0.194	$35.09
Agriculture, Forestry, Fishing and Hunting	0.316	$16.98
Accommodation and Food Services	0.354	$19.30
Management of Companies and Enterprises	0.372	$73.93
Health Care and Social Assistance	0.402	$26.22
Retail Trade	0.411	$19.57
Utilities	0.421	$48.78
Other Services (except Public Admin.)	0.434	$24.98
Transportation and Warehousing	0.436	$32.52
Professional, Scientific, and Technical Services	0.452	$47.67
Admin., Support, Waste Management Services	0.458	$25.12
Information	0.471	$83.75
Finance and Insurance	0.471	$39.71
Arts, Entertainment, and Recreation	0.480	$22.46
Real Estate and Rental and Leasing	0.495	$28.04
Wholesale Trade	0.502	$32.60
Manufacturing	0.537	$32.57
Construction	0.573	$36.29
Public Administration	0.638	$35.83
Mining, Quarrying, and Oil and Gas Extraction	0.655	$32.13

Note: Lists UI take-up rate from Lachowska et al. (2025) and industry median hourly wage from 2022Q1.

Table C.3: Eligibility Waterfall for Experimental Sample

Sample	Wave 1	Wave 2
All Monetarily Eligible Workers	3,247,791	3,242,207
+ in restricted industries & < $40/hr	2,442,148	2,443,870
+ Potential Job Losses	59,232	45,790
+ Matched to Address Records	45,895	36,808
+ w/o UI Claim	34,180	28,976
+ w/o PFML Claim	29,220	21,277
+ w/o Workers' Compensation Claim	-	20,979

Note: This table describes the sample eligibility waterfall for both waves of the experiment. Wave 1 was conducted on potential job losses in 2024Q1 in May 2024 and wave 2 was conducted on potential job losses in 2024Q2 in August 2024. Restricted industries exclude the following 2-digit NAICS industries: Mining, Quarrying, and Oil and Gas Extraction (21), Information (51), Finance and Insurance (52), Management of Companies and Enterprises (55), and Educational Services (61). The potential job loss sample was matched to address records from both internal ESD records (if the worker had previously applied for UI) or Washington Department of Licensing driver's license records. Workers who had opened a Paid Family and Medical Leave (PFML) claim with the state of Washington 30 days prior to or during the quarter of their potential job separation were excluded from the experimental sample. Workers who had a workers' compensation claim open in May 2024 were excluded from wave 2 of the experiment.

Table C.4: Overall UI Take-Up Effect Calculations

	Wave 1	Wave 2	Both Waves
(1) Assuming Experimental Share Ineligible	48.2%	48.2%	48.2%
(2) Experimental Sample	29,220	20,979	50,199
(3) Claimed UI before mailers	11,715	7,832	19,547
(4) Assumed Eligible in Experimental Sample [= (1) × (2)]	15,136	10,867	26,003
(5) UI-Eligible Job Losses (including claimants) [= (3) + (4)]	26,851	18,699	45,550
(6) Pre-Intervention Take-Up [= (3)/(5)]	43.6%	41.9%	42.9%
(7) Complier Applicants	-	-	703
(8) Post-Intervention Take-Up [= ((3) + (7))/(5)]	-	-	44.5%

Note: Table demonstrates how we calculate UI take-up for the wider population of job losses, including those who claimed UI benefits soon after separation and thus were excluded from the field experiment. (1) comes from Table E.1. (2) comes from bottom line of Table C.3. (3) is the number of UI claimants calculated (and removed) in the researchers' code that constructs the experimental sample. We implicitly assume all UI claimants are eligible by the calculation in row (5). Treatment effects are only calculated for the full experimental sample, not broken down by wave. Application treatment effect is calculated from Table A.13 for the full sample.

Table C.5: Undeliverable Letters from First Wave of Field Experiment

Week Starting	Number Returned in Week	% Cumulative Returned
5/20/24	3	0.2%
5/27/24	708	51.3%
6/03/24	414	81.2%
6/10/24	106	88.8%
6/17/24	64	93.4%
6/24/24	22	95.0%
7/01/24	26	96.9%
7/08/24	11	97.7%
7/15/24	5	98.1%
7/22/24	5	98.4%
7/29/24	7	98.9%
8/05/24	4	99.2%
8/12/24	5	99.6%
8/19/24	6	100.0%
Total	1,386	100.0%

Note: Table presents the week-by-week breakdown of when informational letters from the first wave of the field experiment were returned to and processed by ESD's mailroom in Olympia, WA. There were a total of 25,220 letters sent to potential job losses in the treatment group in the first wave of the experiment, meaning 5.5% of mailed letters were returned as undeliverable.

Table C.6: Demographics and Characteristics of Callers

Variable	Letter Recipients	Matched Callers	p-value difference
Ever Received UI	0.59	0.77	0.000
Previous Hourly Wage	25.69	24.94	0.106
Previous Annual Salary	46,360	44,564	0.107
Base Year Hours Worked	1,804	1,794	0.696
Percent Hours Drop, $t-1$ to t	-0.65	-0.70	0.004
Layoff Industry			
Retail Trade	0.17	0.17	0.894
Health Care and Social Assistance	0.14	0.15	0.655
Manufacturing	0.09	0.10	0.814
Accommodation and Food Services	0.09	0.09	0.893
Construction	0.08	0.06	0.128
Administrative, Support, Waste Management	0.08	0.09	0.773
Demographics			
Share Female	0.45	0.50	0.253
Share Black	-	0.13	-
Share Hispanic	-	0.21	-
Share Asian	-	0.09	-
Four-Year College Degree +	-	0.21	
Age	41.4	52.4	0.000
Share Veteran	0.02	0.06	0.009
Share with Disability	0.03	0.05	0.168
Address in Seattle MSA	0.48	0.47	0.837
N	41,204	255	-

Note: Table shows balance of demographic and economic characteristics for letter recipients in the first wave of the experimental sample and the sample of callers we can identify using their phone number from which they called ESD within 60 business days of mailing. Age is calculated as a worker's age on the date of the first wave mailing. Race and education data are not available for the entire sample of letter recipients.

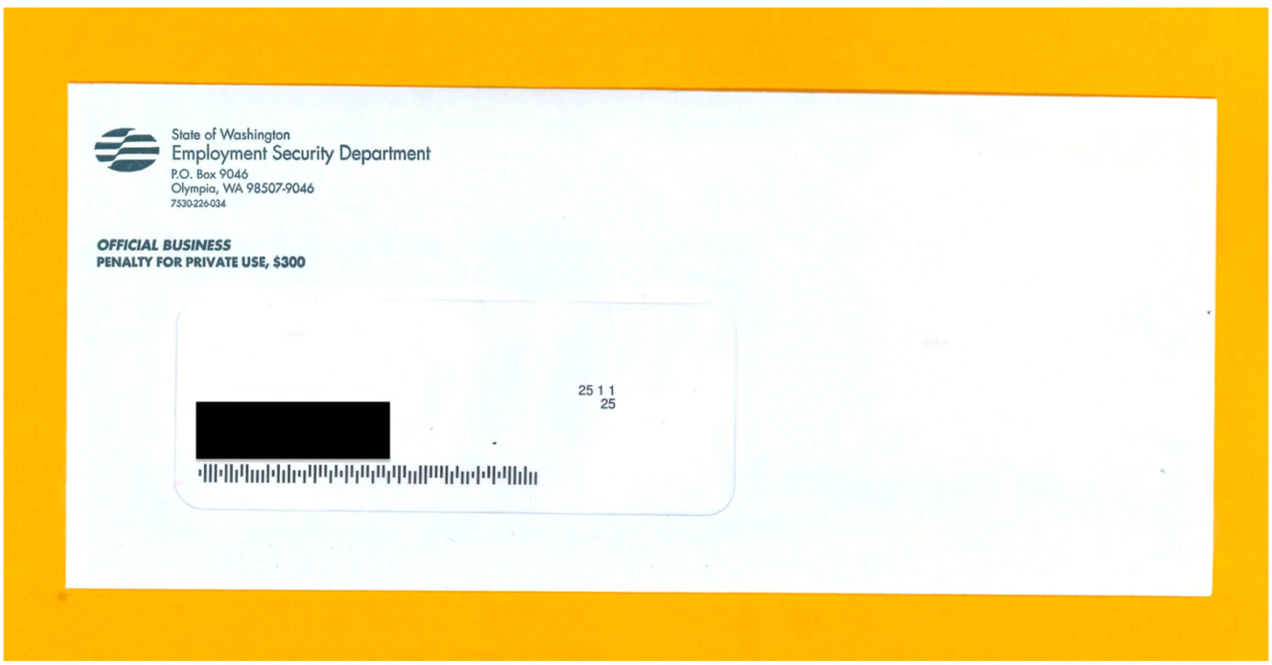
Figure C.1: Washington State WorkSource Office

(a) WorkSource Rainier in Seattle, WA

(b) FrontDesk of WorkSource Office

Note: Pictures show WorkSource Rainier Office, a public employment office in Seattle, WA. WorkSource offices in Washington are a partnership of local and state government agencies, colleges, and nonprofit organizations that offer employment services for businesses and job seeker. There are 38 offices similar to the one shown throughout the state.

Figure C.2: Informational Letter Envelope from Washington ESD



Note: Picture of the envelope with the Washington ESD seal used for the informational letters in the field experiment.

Figure C.3: Sample Saturated Letter

Employment Security Department
WASHINGTON STATE

May 21, 2024

UEG02
FIRST NAME LAST NAME
ADDRESS 1
ADDRESS 2
CITY STATE ZIP

Dear [FIRST NAME],

If you lost your job through no fault of your own, you might qualify to receive **unemployment benefits**. These benefits provide temporary income while you search for a new job.

Searching for work can take a long time and we are here to help you apply for benefits. Statistically, job seekers who have been unemployed for two months typically remain jobless for another three months.

Benefit amounts are based on your work history – not on financial need. While you worked, your employer paid taxes into the unemployment insurance trust fund for this purpose.

You can estimate your own weekly benefit ahead of time. Learn more at esd.wa.gov/unemployment/calculate-your-benefit.

It is free to apply. It takes an average of only 30 minutes. The quickest way to apply is online. You can apply as long as you are unemployed and looking for work.

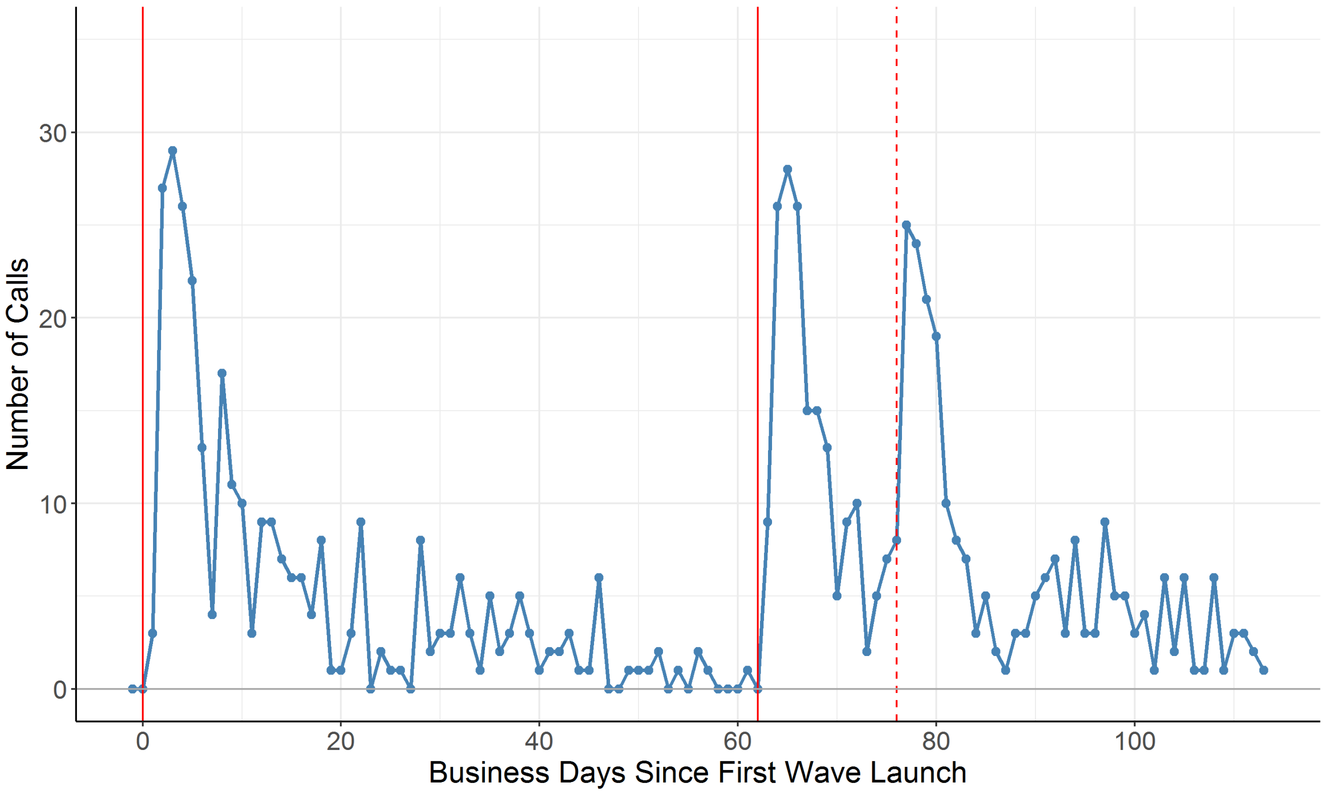
Learn more about applying for benefits:

- For more information, visit esd.wa.gov/learn or call 844-903-0445.
- For information in another language, please contact 844-903-0445.
  Para obtener información en español, comuníquese al 844-903-0445.
- To request an accommodation for a disability, please contact ESDGPUIAccomms@esd.wa.gov or call 844-395-6698.

If you have questions about this letter:
Call 844-903-0445 on weekdays 8 a.m. to 4 p.m. If you have not lost your job, please disregard this letter. This is part of an outreach campaign to workers in Washington state.

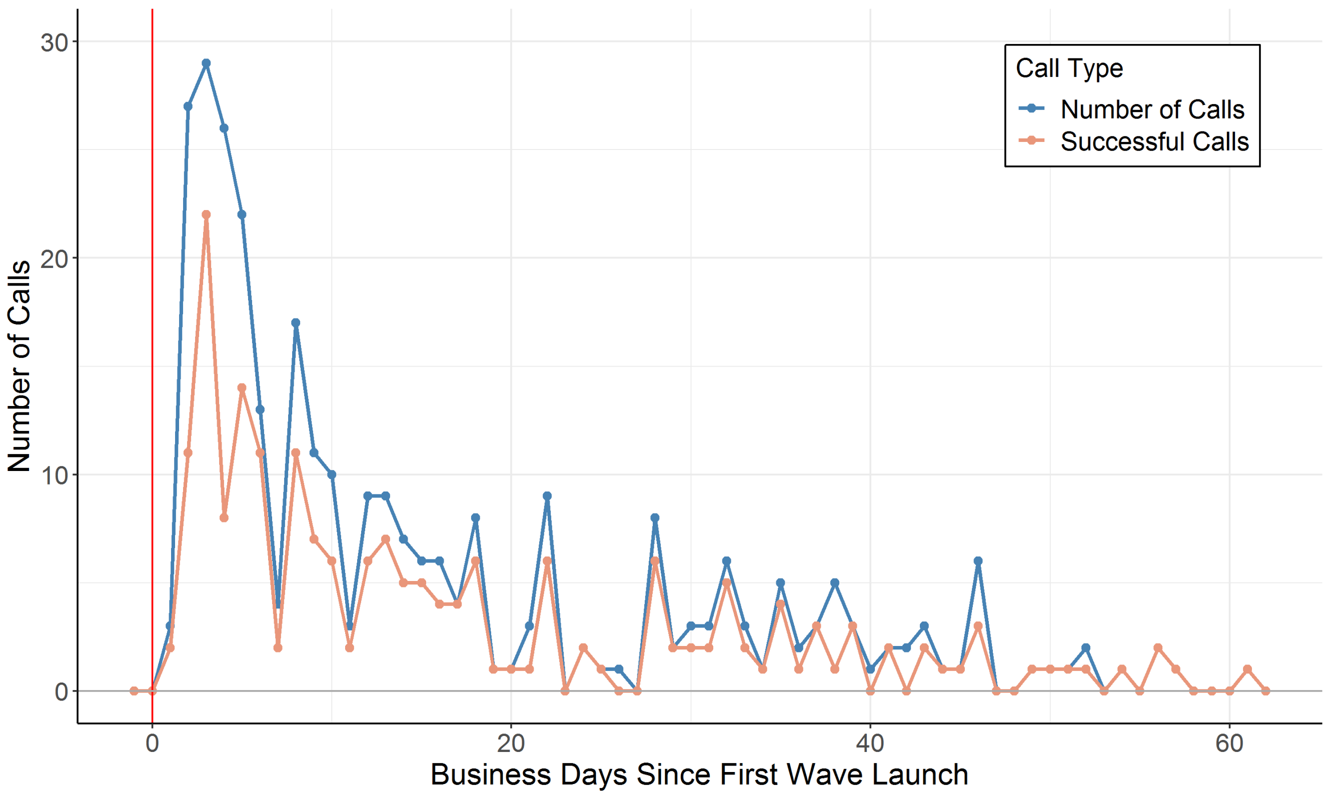
Sincerely,
Washington State Employment Security Department

Figure C.4: Time Series of Calls to Customized Phone Lines



Note: Graph shows time series of number of phone calls to customized phone lines listed on informational mailers sent in the first wave of the experiment. We aggregate call volume across all eight distinct phone numbers corresponding to the eight treatment arms. The time series spans May 20, 2024 through Thursday, October 31, 2024. Call volume includes abandoned calls and calls that reached an agent. Phone lines were open for calls on business days between 9am and 4pm.

Figure C.5: Time Series of All and Successful Phone Calls to Customized Lines



Note: Graph shows time series of total number of calls and successful calls to customized phone lines listed on informational mailers sent in the first wave of the experiment. A successful call is defined as a call that was not abandoned by the caller before reaching a service agent. We aggregate call volume across all eight distinct phone numbers corresponding to the eight treatment arms. The time series spans May 20, 2024 through Thursday, October 31, 2024. Phone lines were open for calls on business days between 9am and 4pm.

D Data Appendix

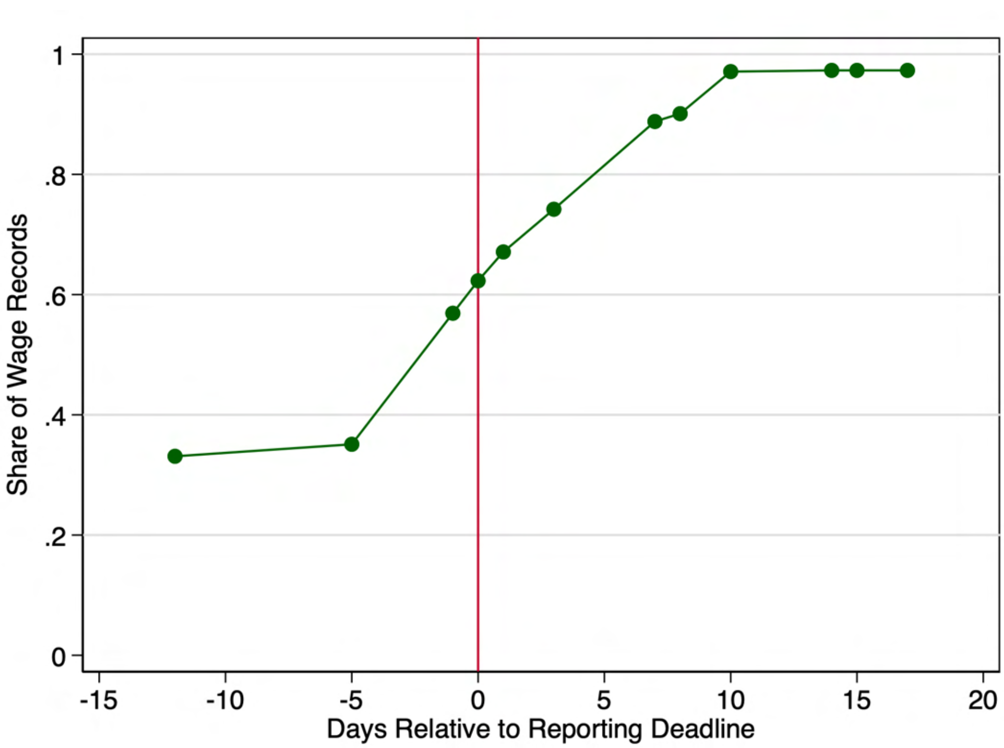
D.1 Near Real-Time Wage Records

One unique aspect to our data and research partnership is the ability to see up-to-date records as they come in. Through ESD, we have access to microdata of the UI claims on a weekly basis. We also observe employer-employee wage records as they are reported each quarter. Firms are required to report their wage records to ESD by the first day of the second month after the quarter ends (i.e., Q1 records are due by May 1st). In practice, some firms report their records early while others report late. Figure D.1 illustrates the speed with which records become available to researchers relative to the due date ("day 0"). Note that the (near) complete set of wage records are not available until 10 calendar days after the reporting deadline (roughly the 11th of the month).

Given the nature of the reporting curve in Figure D.1, we opt to wait to construct our experimental sample until we have the near-complete set of wage records. This is because we seek to avoid misclassifying job transitions as job losses if workers transition from an early-reporting firm to a late-reporting firm. We also seek to maximize our sample size of true job losses.

As a result, each wave of the experiment constructs the experimental sample on the second Monday of the calendar month (May 13 and August 12) and the ESD mailroom sends out the full batch of letters the following Tuesday, May 21 and August 20, 2024, respectively.

Figure D.1: Reporting Curve: Share of Wage Records Reported to ESD Researchers



Note: Graph plots the share of employer-employee wage records which are available to researchers relative to the due date, day 0. Firms are required to report their wage records to ESD by the first day of the second month after the quarter ends.

D.2 Address Records from WA Department of Licensing

Washington State ESD possesses mailing address information for those who have applied for unemployment insurance at some point in their life before. To expand the scope of the experiment beyond those with previous UI experience, the ESD entered a data sharing agreement with the Washington State Department of Licensing (WADOL), the state agency that issues driver's licenses. The WADOL include the most recent mailing address of every Washington resident with a valid driver's license[45], along with the license holder's full name, last four digits of their Social Security Number (SSN), sex, date of birth, disability status, and veteran status. These WADOL records were pulled and shared with ESD in January 2024 as authorized by the agency's Data Governance Committee and federal Driver Privacy Privacy Protection Act (18 U.S.C. §2721).

To merge the wage and WADOL records, researchers match on individual names within groups according to the last four digits of SSN, which drastically reduces the dimensionality of the string matching. The string matching itself uses the stringdist function in R, matching every name in the wage records with the best corresponding name in the WADOL data based on their Jaro-Winkler score. A match was considered sufficiently accurate if the distance score was less than 0.1. Researchers manually verified matches below this threshold to confirm their reliability, ensuring that only accurate matches were used for merging wage records with driver's license addresses.

The authors express gratitude to Madeline Veria-Bogacz for facilitating the data sharing agreement between ESD and WADOL.

D.3 Phone Lines Data

This section provides more detail on the call-in data and the script for customer service agents, which was provided in English and Spanish.

We report raw call-in rates for each study arm, which include repeat callers. The call service at ESD is limited in its ability to determine the identity of the callers, as it only records the phone number of the caller but not any other identifying information. As such, when describing the sample of callers in Appendix Section C.3, we only report economic characteristics for those callers whom we can match to claimant records (and therefore wage records) via the phone number.

Customized Phone Lines and Call Agent Procedure

ESD tracks all calls that it receives to each specific line using Five9 contact center software, which forms the basis for the measure of call-ins to the ESD phone numbers in response to outreach letters. Each distinct version of the letter (8 treatment arms in total) featured a customized phone number for those wanting to apply for UI benefits over the phone. ESD customer service deployed these eight phone numbers specifically for this experiment and did not publicize the lines otherwise. We define a caller as someone who calls in to the appropriate phone number during business hours (9am–4pm) in the 4 months after the mail date. We exclude calls beyond the 4-month window.

Calls to the project-specific phone lines were redirected to the main customer service line that is publicly advertised to anyone who seeks to apply for UI via phone. Calls to the customized phone letter were prioritized in the queue by customer service agents, which translated to slightly shorter hold times. Customer service agents followed their typical procedure when receiving calls from the project lines, which entails asking callers for their name and other identifying information. Callers then report the purpose of their call (i.e., to apply for benefits, ask a question about the letter, etc.).

[45] Roughly 90% of the 16+ civilian population holds a driver's license. Mailing addresses must be updated every six years.

Phone Line Variables
Each instance of a call to the customized phone line is timestamped with the exact date and time. Records show the caller's language preference (English or Spanish) and whether the call was abandoned (i.e., the caller did not speak to an agent). The software records hold times, talk times, and interactive voice response times (i.e., automated voice menu options). For abandoned calls, the software records the length of time until call abandonment. These times collectively sum to a total call time for each caller.

D.4 Reasons for Rejection Data

UI applications are generally denied for one of three broad reasons. First, applicants may not have accrued enough hours in their base period, making them monetarily ineligible. Second, applicants may have separated from their previous employer for reasons not considered to be through no fault of their own—typically because they quit or were fired for cause—rendering them separation ineligible. Third, applicants may fail to meet ongoing requirements, such as being available for and actively seeking work (continuing eligibility). We also document other, less common reasons for application denial in this section. In our experimental sample, we exclude job losses that would be denied due to monetary ineligibility, leaving separation and continuing eligibility as the primary drivers of UI application rejections in our data.

A denial due to separation eligibility occurs when ESD determines that the claimant was separated from their prior job for a disqualifying reason, such as quitting without good cause or being discharged for misconduct. This determination is made through a fact-finding process in which an ESD adjudicator contacts both the claimant and the employer to gather detailed information about the separation. If the claimant or employer fails to respond, ESD makes a decision based on the available evidence. Only separations deemed to be through "no fault of their own" satisfy eligibility criteria for unemployment benefits.

A denial for "Not Actively Seeking Work" occurs when a claimant fails to meet Washington State's weekly job search requirements. In most cases, this happens when the individual files a weekly claim but answers "No" to the question asking if they completed at least three job search activities (see Figure D.2). This response triggers an issue on their claim. If the claimant either fails to respond to a follow-up inquiry or continues to report not searching, their claim is denied. Similar denials can also result from failing a randomized job search review — where the claimant must verify job search activity for specific weeks — or when an agent flags a potential issue during a call or interaction and the claimant doesn't adequately respond.

Some claims are rejected for "unclassified" reasons, meaning a worker filed a claim (applied) for but did not receive UI benefits, and we cannot match these rejections to the database on reasons for denials according to a worker's filing account ID. Such unclassified rejections represent 15% of total UI applications in the experimental sample and 34% of rejections. According to the authors' correspondence with personnel in the IT Services division of ESD, who assembled these datasets, the most common reason for claimants being an "unclassified" denial is being denied due to identity issues carried over from a previous account.

D.5 WorkSource Office Data

As part of its UI system, Washington operates a network of public employment offices known as WorkSource. These offices provide reemployment services to UI recipients who are encouraged, but not required, to participate. Services include job search assistance, resume creation and development, career-related workshops, and personalized support from staff to connect workers with employers.[46]

We link administrative data from WorkSource to our experimental sample to measure which WorkSource services the workers in the treatment and control group use. The datasets are matched on UI claimant IDs, meaning workers must have applied for UI to have a WorkSource dataset record. The WorkSource data feature 190 unique services we manually classify into one of five conceptual buckets: (1) searching/applying for jobs, (2) attending workshops or career services, (3) creating or updating a resumé or interview coaching, (4) attending Reemployment Services and Eligibility Assessment (RESEA), a mandated USDOL-funded program that provides career counseling, or (5) other miscellaneous services.

These five classifications of types of WorkSource services appear in numerous figures throughout the paper, including the mediation analysis of Table 4. The frequency of these five services in the treatment and control group is presented in Appendix Table D.2. Descriptively, roughly 25% of UI applicants in the control group and 30% of applicants in the treatment group engage with some kind of WorkSource service. The most common classification of service sought by UI applicants are activities relating to active job seeking.

Figure D.2: Work Search Certification Question

[46]For further information about WorkSource, visit https://www.worksourcewa.com/.

Table D.1: Incidence Reason for Application Reject in Experimental Sample

	Control	Treatment
Panel A. Main Sample		
Initial Claim Rejected (Never Received Compensation)	36.3%	50.3%
Reason for Denial		
Quit or Fired for Cause	20.6%	23.2%
Did Not Verify Actively Seeking Work	4.9%	10.6%
Identity Issue	3.9%	4.4%
Back Dating Initial Claim Failure	2.9%	3.0%
School Attendance	3.9%	1.7%
Other Failure of Worker Reporting Requirements	1.0%	0.2%
Unclassified	10.8%	16.4%
N (total applications)	102	809
N (total rejections)	37	407
Panel B. Full Sample		
Initial Claim Rejected (Never Received Compensation)	35.8%	46.7%
Reason for Denial		
Quit or Fired for Cause	17.5%	20.4%
Did Not Verify Actively Seeking Work	4.3%	10.0%
Identity Issue	4.4%	4.0%
Back Dating Initial Claim Failure	2.2%	2.8%
School Attendance	2.9%	1.8%
Other Failure of Worker Reporting Requirements	1.5%	0.2%
Unclassified	12.4%	15.9%
N (total applications)	137	1,042
N (total rejections)	49	487

Note: Table lists reasons for rejection among experimental sample who applied for UI benefits two months after mailing. Percentages sum to more than 100% because a claim can be flagged with more than one reason for rejection. "Other Failure of Worker Reporting Requirement" includes: (i) Reporting Requirements for Failure to Schedule/Report, (ii) Reporting Requirements - Did Not Schedule Appointment, (iii) Failure to Respond, and (iv) Reporting Requirements - Weekly Claim. Other reasons for rejection occurred too infrequently to disclose for this sample.

Table D.2: Descriptive Statistics: UI Applications and WorkSource Engagement by Treatment

	Control		Treatment	
	N	% of applicants	*N*	% of applicants
Total in Experiment	9,001	-	41,212	-
Applied for UI	137	100.0%	1,042	100%
Engaged with WorkSource	34	24.8%	308	29.6%
Searched for a Job	22	16.1%	176	16.9%
Attended Workshop/Career Services	8	5.8%	91	8.7%
Build/Update Resume	8	5.8%	90	8.6%
RESEA	13	9.5%	100	9.6%
Other	19	13.9%	159	15.3%

Note: This table summarizes UI applications and subsequent engagement with WorkSource services among individuals in the field experiment two months after letter mailing. Counts (*N*) and the share of UI applicants participating in each activity are shown separately for the control and treatment groups.

Table D.3: WorkSource Offices Information

WorkSource Name	County	ZIP	Open	Walk In
Clarkston Connection	Asotin	99403	✓	✓
Columbia Basin	Benton	99336	✓	✓
Vancouver	Clark	98660	✓	✓
Cowlitz/Wahkiakum	Cowlitz	98626	✓	✓
Clallam County	Clallam	98382	✓	✓
Wenatchee Valley	Douglas	98802	✓	✓
Republic	Ferry	99166	✓	
Central Basin	Grant	98837	✓	✓
Grays Harbor County	Grays Harbor	98520		
Island	Island	98277	✓	✓
Auburn	King	98002	✓	✓
Downtown Seattle	King	98121	✓	✓
North Seattle	King	98103	✓	
Rainier	King	98144	✓	✓
South Seattle	King	98106	✓	✓
Kitsap County	Kitsap	98383	✓	✓
Ellensburg	Kittitas	98926	✓	✓
Goldendale	Klickitat	98620	✓	✓
White Salmon	Klickitat	98672	✓	✓
Lewis County	Lewis	98531	✓	✓
Mason County	Mason	98584	✓	✓
Okanogan	Okanogan	98841	✓	✓
Newport	Pend Oreille	99156	✓	
Long Beach	Pacific	98631	✓	✓
Pierce	Pierce	98405	✓	✓
Pierce at South Hill Mall	Pierce	98373	✓	✓
Skagit	Skagit	98273	✓	✓
Stevenson	Skamania	98648		
Spokane	Spokane	99202	✓	✓
Next Generation Zone	Spokane	99202	✓	✓
Everett	Snohomish	98201	✓	✓
Colville	Stevens	99114	✓	
Thurston County	Thurston	98512	✓	✓
Pullman	Whitman	99163	✓	
Whatcom	Whatcom	98225	✓	✓
Union Gap	Yakima	98903	✓	✓
Valley Mall	Yakima	98903	✓	✓
Sunnyside	Yakima	98944	✓	✓
Walla Walla	Walla Walla	99362	✓	✓

Note: This table lists all WorkSource offices in Washington State, indicating their county, ZIP code, whether they are open, and whether walk-in services were available as of March 2025.

E Potential Job Loss Sample Sensitivity

E.1 Estimating True UI Eligibility of Experimental Sample

Potential Job Losses in Administrative Hours Records Our experimental sample of "potential job losses" without an associated UI claim consists of workers with stable employment at a single firm followed by a sharp reduction in hours.[47] We exclude workers earning more than $40 per hour prior to job loss and those from certain high-paying industries who are more likely to self-insure through savings or severance pay.[48] We know all workers in our experimental sample were monetarily eligible, meaning they accrued a sufficient number of hours in the past year to receive UI. However, we don't know what fraction of the sample lost their job through no fault of their own and are available to work, the other conditions for UI eligibility. We use the Current Population Survey (CPS) Unemployment Insurance Nonfilers Supplement to estimate the share of our experimental sample that was truly UI eligible, which we project to be 52%.

Current Population Survey (CPS) Nonfilers Supplement – The CPS Nonfilers Supplement is a special supplement conducted periodically to gather information about individuals who are eligible for but do not claim UI benefits to better understand why eligible individuals do not file. The Nonfilers Survey was conducted most recently in February and May of 2022, which we pool together at the recommendation of the Census Bureau due to small sample sizes of each individual wave.

The CPS follows a "rotating panel" structure, whereby respondents in a given rotation group are interviewed for four consecutive months, temporarily leave the CPS sample for eight months and then return for four more consecutive months before leaving the CPS altogether. During one of these eight months, the CPS Nonfilers Supplements surveys workers about why they did not claim UI. The supplement samples from unemployed (laid off and searching) and those not in the labor force but who have worked in the last 12 months. The supplement excludes employed respondents or those who have not been in the labor force in the past year.

We replicate our experimental sample from the RCT using the CPS Nonfilers Supplement by further imposing the following restrictions:

- Civilian age 16+
- Did not apply for unemployment insurance since last job
- Previously employed in at least one month of the panel prior to the supplement
- Only held one job for all months while employed
- Reported usual weekly hours consistent with full-time work (680–1,400 hours over 26 weeks)
- Did not work in one of the following industries in last job: mining (NAICS 21), information (51), finance and insurance (52), management of companies (55), and education services (61)

[47]Specifically, workers must be employed in two consecutive quarters preceding potential job loss and the hours drop must be at least 35 percent or more between quarters $t-1$ and t.

[48]The industries dropped include Mining, Quarrying, and Oil and Gas Extraction (NAICS 21), Information (51), Finance and Insurance (52), Management of Companies and Enterprises (55), and Educational Services (61).

The CPS contains detailed labor force status information, including the nature of unemployment for those who are searching and the circumstances for those who report being out of the labor force. We consider a respondent to be “not UI eligible” if they are (i) unemployed and a “job leaver” — suggesting they quit their job voluntarily — or are an “other job loser,” which is distinct from being on layoff and suggests a firing for cause or (ii) not in the labor force because they are in school, ill health, or are tending to family responsibilities.

Table E.1: Partition of Potential Job Losses by Reason for Nonemployment from CPS

	Percent	N
UI Eligible	51.8%	536.3
Unemployed – Laid off	10.1%	104.7
Unemployed – Reentrant	8.2%	84.3
NILF – Available to Work	33.6%	347.2
Not UI Eligible	48.2%	498.7
Unemployed – Other Job Loser (e.g. Fire for Cause)	8.7%	89.7
Unemployed – Quit	8.5%	87.5
NILF – Retired/Disabled	28.5%	294.5
NILF – Unavailable for Work	2.6%	27.0
	100%	1,035

Source: CPS Nonfilers Supplement February and May 2022 waves. *Note*: Table disaggregates by reason for nonemployment the CPS Nonfilers sample which replicates the experimental sample of recently jobless nonclaimants. “NILF – Available to Work” include those out of the labor force and not searching because they believe there is no work available, couldn’t find work, lack necessary schooling, have transportation problems, couldn’t arrange childcare, or other reasons. “NILF – Unavailable to Work” includes those out of the labor force and in school or tending to family responsibilities. “Unemployed – reentrant” describes a job seeker who previously was NILF and is searching again. We assume unemployed respondents identifying as an “other job loser” are fired for cause, rendering them ineligible for UI.

Table E.2: UI Eligibilty by Waterfall of CPS Sample Restrictions

Sample	% UI Eligible
Civilians 16+ who didn't apply for UI	60.5%
" " + with work history in CPS panel	57.1%
" " + only held a single job while working	57.2%
" " + worked full-time while employed	51.3%
" " + worked in restricted industries	51.8%

Source: CPS Nonfilers Supplement February and May 2022 waves. *Note*: Table shows the share of CPS Nonfilers sample classified as UI eligible for each restriction needed to replicate the experimental sample. Working full-time is defined as having usual weekly hours times 26 (weeks) of ≥ 680 but $\leq 1{,}400$ (i.e. $\sim$ 25–55 hours/week). Excluded industries include mining (NAICS 21), information (51), finance/insurance (52), management of companies (55), and education (61).

E.2 Estimating Intervention Effect on Overall Take-Up

Table E.3: Intervention Impact on Sample UI Take-Up Calculation

	Wave 1	Wave 2	Combined
Panel A: Inputs			
Potential job losses (PJL)	29,220	20,979	50,199
UI claimants in sample	11,715	7,832	19,547
PJLs + UI claimants	40,935	28,811	69,746
UI-eligible fraction of PJLs (CPS)	51.8%	51.8%	51.8%
Treatment effect on applications (pp)	1.4	1.4	1.4
Panel B: Outputs			
Overall application rate (baseline)	28.6%	27.2%	28.0%
Overall application rate (post-treatment)	29.6%	28.2%	29.0%
Application rate among eligible workers (baseline)	43.6%	41.9%	42.9%
Application rate among eligible workers (post-treatment)	45.2%	43.5%	44.5%

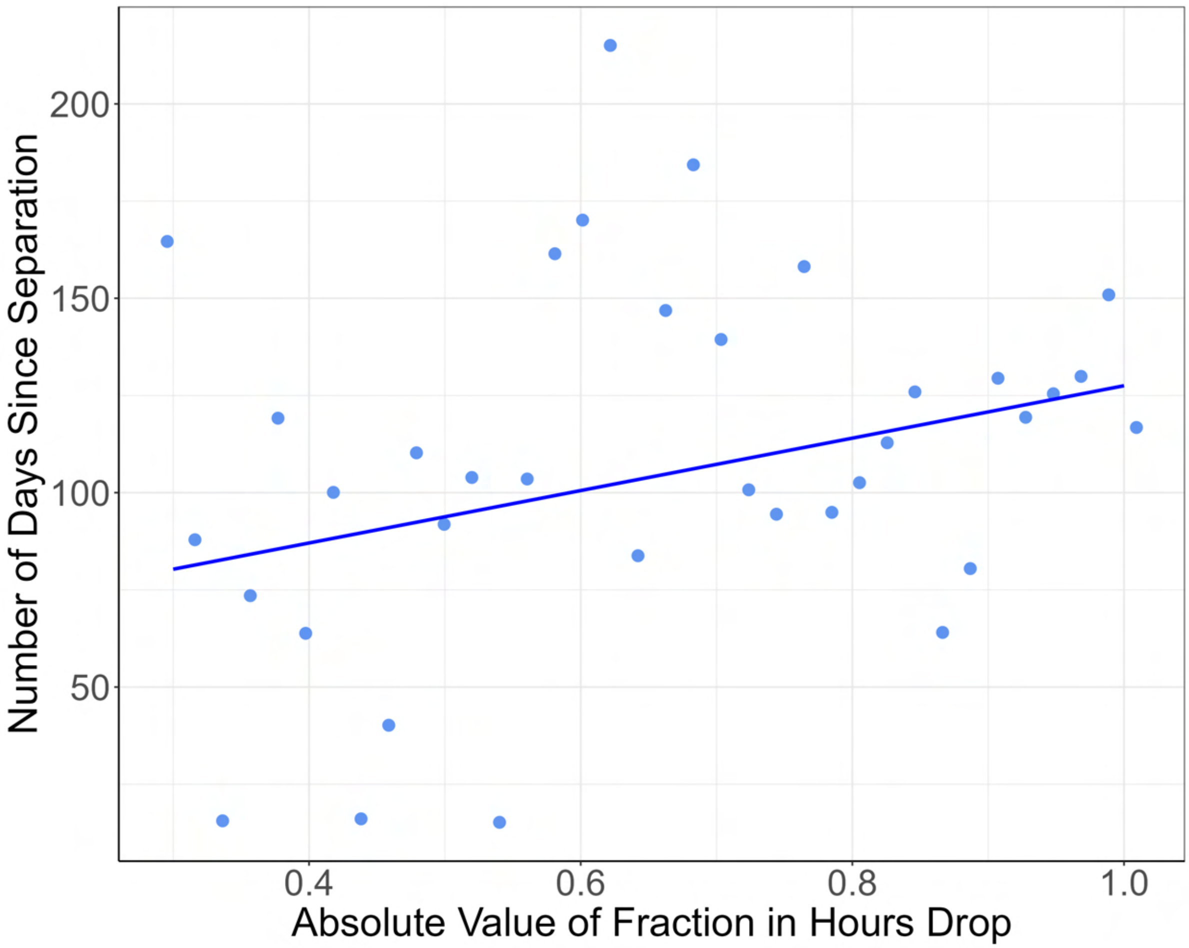
Notes: Table presents the authors' calculations for the UI take-up rate for the first and second waves and the overall experimental sample given the number of potential job losses (PJLs), UI claimants who would have been PJLs had they not claimed, and estimates for the fraction of PJLs who are UI-eligible and application treatment effects. Panel B lists both the overall application rates which do not adjust for estimated UI-eligible fraction as well as the application rate among the eligible population.

E.3 Job Loss Timing Validation

In this section we validate the percentage drop of hours as a reasonable proxy for recency of job loss. The percentage drop in hours variable from quarter $t-1$ to quarter t is defined for all of the workers in the experimental sample using the following formula:

$$Percent\_\Delta\_Hours = \frac{h_t - h_{t-1}}{h_{t-1}}$$

Figure E.1: Validation of Percent Hours Drop Variable



where h_t represents the total number of hours worked in the quarter of potential job loss and h_{t-1} represents the total number of hours worked in the prior quarter. Because the letters were distributed in two waves, the percent hours drop variable is measured between quarter 4 and quarter 1 for workers in the first wave and between quarter 1 and quarter 2 for workers in the second wave.

A 100% drop of hours from quarter $t-1$ to t refers to a complete contraction of hours worked in quarter t (0 hours), while a 0 percent drop reflects no change in hours worked across the two quarters. Potential job losses require a contraction of hours of at least 30% from quarter $t-1$ to t.

For a subset of the experimental sample who submitted a UI claim, we have the reported date of separation from their employer. Using this information, we define a second variable, the number of days since separation, which is defined as the number of days between the reported separation date and the last day of the quarter of potential job loss. This variable directly measures the recency of job loss, with earlier separation dates corresponding to more days between the separation date and the end of the quarter. For example, a wave 1 UI claimant who reported losing her job on February 11, 2024 would have 49 days between her job loss and the end of quarter 1 on March 31, 2024.

Figure E.1 presents a binscatter illustrating a statistically significant positive relationship between the absolute value of the fraction drop in hours and the number of days since separation, validating the idea that the drop in hours variable proxies for recency of job loss. The statistically significant slope of 67.4 implies that an extra 10 percentage point drop in hours from $t-1$ to t suggests an extra 6.74 days have passed between the separation date and the end of the quarter. The hours drop

Table E.4: Summary Statistics by Treatment Status among Recalls (Excluded in Main Analysis)

Variable	Control	Treatment	p-value
Previous Hourly Wage	\$25.76	\$25.95	0.159
Previous Annual Salary	\$46,897.81	\$46,877.30	0.948
Base Year Hours Worked	1,827	1,813	0.045
Ever Received UI	0.617	0.623	0.571
Percent Hours Drop, $t-1$ to t	−54.4%	−53.7%	0.086
Layoff Industry Share			
Retail Trade	0.154	0.154	0.984
Health Care and Social Assistance	0.138	0.139	0.876
Accommodation and Food Services	0.110	0.098	0.030
Manufacturing	0.093	0.087	0.317
Administrative, Support, Waste Management	0.077	0.079	0.762
Construction	0.087	0.094	0.254
Transportation and Warehousing	0.073	0.085	0.023
Professional, Scientific, and Technical	0.046	0.047	0.918
Wholesale Trade	0.043	0.040	0.529
Other Services (except Public Admin)	0.036	0.039	0.371
Public Administration	0.032	0.029	0.390
Arts, Entertainment, and Recreation	0.053	0.046	0.114
Real Estate and Rental and Leasing	0.018	0.020	0.504
Agriculture, Fishing, Forestry	0.039	0.041	0.552
Utilities	0.001	0.001	0.235
Demographics			
Share Female	0.454	0.448	0.587
Age	42.7	42.4	0.494
Share Veteran	0.026	0.031	0.091
Share with Disability	0.029	0.035	0.103
Address in Seattle (proper)	0.071	0.075	0.456
Address in Seattle MSA	0.464	0.474	0.307
N	2,973	14,619	-

Note: Table shows balance across economic, industrial, and demographic characteristics for the control group and the treatment groups we classify as "false positives" or "recalls", defined as those who recorded more hours at their potential separating firm in quarter $t+1$ than in t and who had no employment at any other firm in $t+1$. The table pools two waves of potential job losses. The rightmost column shows the p-value for a hypothesis test that the listed characteristic has the same mean across both assignment arms. Industry shares may sum to slightly more than one due to rounding. Age is calculated as a worker's age on the date of the first wave mailing.

variable ranges from 0.3 to 1 by construction.

If the percentage hours drop variable was perfectly aligned with the days since separation variable, we would expect a slope of around 91, since there are on average 91 days in a quarter. A slope of 67 falls below this benchmark. This weaker-than-expected relationship between the two variables can be influenced by several factors, like misremembering or misstating their separation dates. However, it is also likely the case that workers in the experimental sample worked irregular schedules before complete separation. For example, two workers might see the same drop in hours. However, one could have worked consistently until their date of firing, and the other had intermittent or reduced hours before being separated. Working intermittently means that there are gaps in the work schedule, so to fulfill the same number of hours, the second individual works to a later separation date. In the same vein, shifting towards part-time work for a period in the quarter means the second worker is working fewer hours per day but towards a later separation date to maintain the same hours as the first worker. The later separation date in either one of these scenarios decreases the Number of Days Since Separation and, thus, causes the regression to underpredict the true value of the slope.

F Online Survey Details and Evidence

In this section, we report the survey instrument administered on the Qualtrics platform and additional survey evidence alluded to in the main paper. Section F.1 describes the timeline and recruitment details. The outline of the survey is broadly categorized into four topics in section F.2. Section F.3 describes how representative our survey sample is compared to the experimental sample from the Washington State field experiment. Section F.4 describes main results from the survey supporting conclusions from the main body of the paper.

F.1 Timeline and Recruitment Details

The survey was administered on the Qualtrics online platform. To ensure high-quality responses, Qualtrics filtered out bots and prevented duplicate responses through IP filters. The authors implemented further quality checks by including two attention checks and screening out those who sped through the survey, completing it in less than 5 minutes and 30 seconds. At the beginning of the survey, participants must also confirm that they will answer questions in good faith and to the best of their abilities. Qualtrics recruited participants from a panel drawn from various sources like airline lists, online shopping centers, and targeted customer profiles. Each participant received roughly $7 in payment for a survey whose average (median) length to completion was 11 (14) minutes. Participants were paid with their preferred method of compensation, depending on the channel of their recruitment.

The first page of the survey informed participants that they would be asked questions about their experience as an unemployed worker. Researchers assured participants that privacy would be maintained with minimal risk of confidentiality breach, and provided the contact information for both the research team and an independent IRB representative.[49]

Sampling Restrictions

Our goal is to recruit a sample of UI-eligible unemployed individuals who have not claimed UI. The survey contains a series of screening questions to determine eligibility for the final sample ($N = 530$). The survey requires participants to be of age 18 years or older, be currently unemployed, and have not received UI in their current spell of unemployment.

When asked about the reason for their unemployment, participants who answered "Laid Off," "Temporary Job Ended," "Fired for Cause," "Disagreement with Employer," or "Other" were able to proceed and become part of the sample of analysis. Those who answered "Quit," "Disability/Health Reason," "Retired," or "Family Reasons" were screened out because they would not qualify for UI.

The last question required participants to have worked at least 680 hours in the year prior to unemployment (solicited via asking about usual weekly hours and the number of weeks worked in the past year). All participants who fulfilled the screening criteria were part of the final sample of the survey conducted in November 2024.

[49]Survey is covered under Stanford University Human Subjects Research eProtocol #76995.

F.2 Survey Outline

F.2.1 Nature of Unemployment

Respondents were asked when they last worked at their previous job and several characteristics about their previous employment, including tenure, earnings, and industry. Respondents were asked if they are currently searching for work, and if not, why (the most common answers being illness/medical problems or childcare/family obligations).

F.2.2 UI Knowledge and Awareness

We replicate a question from the Current Population Survey (CPS) Nonfilers Supplement by asking respondents why they did not apply for UI benefits since they became unemployed (respondents could check as many reasons as applicable). Those who reported not applying because they believed they were ineligible were manually classified by their true UI eligibility according to their free-text response to that question.

Respondents were asked if they have ever been on UI previously and how much money per week they believe they would receive if they applied for UI benefits.

Lastly, respondents were asked numerous factual questions about the UI system: they were asked questions testing their knowledge of UI eligibility criteria, benefit payment frequency, replacement rate, and maximum statutory benefit level and potential duration in their state.

F.2.3 Information Treatment Following Outreach Letters

Next, we cross-randomized showing survey participants the messages from the "destigmatization" and "search expectations" treatment arms from the Washington field experiment. Due to cross-randomization, one quarter of respondents saw neither message, one quarter saw both messages, and the remaining half saw one or the other. These messages were shown in isolation on the page with no other information (see Figure F.1), and respondents could not advance to the next page until at least ten seconds had elapsed.

We then elicited from all respondents their attitudes towards UI benefits by asking them to rate on a 5-point Likert scale the extent they believed UI functions like (i) car insurance and (ii) welfare programs (questions presented in Figure F.2. These attitudes are meant to proxy for a sentiment of "free-rider stigma" associated with UI benefits, as respondents may liken to welfare benefits which transfer from those who paid in to those who didn't or may liken it car insurance where all who are covered gain the benefits of filing a claim.

The cross-randomized structure and sequencing of this survey section enable us to isolate the causal effects of message exposure on respondents' attitudes toward UI, their job search expectations, and their self-reported likelihood of applying for benefits, as discussed in subsection F.2.4.

F.2.4 Elicitation of Application Likelihood

In the final part of the survey, we elicit respondents' self-reported likelihood of applying for UI benefits. We directly elicit this likelihood by asking respondents on a 7-point Likert scale how likely they are to apply for UI benefits. We also elicit a "behavioral measure" of UI interest by measuring how long workers look at an informational flyer about UI benefits from the U.S. Department of Labor.

Figure F.1: Cross-Randomized Stigma and Search Expectations Survey Treatments

Did you know

UI benefit amounts are based on your work history – not on financial need. While you worked, your employer paid taxes into the unemployment insurance trust fund for this purpose.

(a) Survey Destigmatization Treatment

Did you know

Searching for work can take a long time and your state unemployment office wants to help you apply for benefits. Statistically, job seekers who have been unemployed for two months typically remain jobless for another three months.

(b) Survey Search Expectations Treatment

Note: Figure shows cross-randomized destigmatization and search expectations treatment following language from the saturated field experiment letter (Figure C.3) shown to online survey respondents. Respondents could not proceed until at least ten seconds had elapsed. One quarter of respondents saw neither message, one quarter saw both messages, and the remaining half saw one or the other.

Figure F.2: Questions Eliciting Stigmatized Attitudes Towards UI Benefits

Please rate the comparison made in the following statement.

	Very similar	Somewhat similar	Neither similar nor dissimilar	Somewhat dissimilar	Very dissimilar
Unemployment insurance is similar to programs like car insurance, which require regular payments so you have a cushion in bad times.	○	○	○	○	○

(a) Comparison of UI to Car Insurance

Please rate the comparison made in the following statement.

	Very similar	Somewhat similar	Neither similar nor dissimilar	Somewhat dissimilar	Very dissimilar
Unemployment insurance is similar to welfare programs like food stamps, which transfer money from haves to have-nots.	○	○	○	○	○

(b) Comparison of UI to Welfare

Note: Figure shows 5-point Likert scale questions asked of all survey respondents to rate how they compare UI benefits. Questions are shown in randomized order.

F.3 Descriptive Statistics and Representativeness

In this section we compare the demographics of the survey sample to both the experimental sample from Washington state and from a national target sample from the CPS Nonfilers Supplement.

Table F.1 compares economic, industrial, and demographic characteristics from the experimental sample (treatment and control) to the survey sample. Compared to the experimental sample, the survey sample is less likely to have received UI, have lower earnings and hourly wages prior to job separation, and have been unemployed longer at the time of treatment or survey. Industrially, the survey sample is overrepresented relative to the WA sample in accommodation and food services and underrepresented in retail trade, administrative/support/waste management, and professional, scientific, and technical services. While the ages of the experimental and survey samples are similar in terms of mean age, the latter skews somewhat more female.

F.4 Core Results from the Survey

Stigma Treatment Affects Stigmatized Views of UI Benefits

The first result from the survey is that respondents who are randomly exposed to the stigma treatment are statistically significantly less likely to hold stigmatized attitudes towards UI benefits. We proxy stigmatized attitudes by the share of respondents who rate UI as "very" or "somewhat" similar to car insurance, "which requires regular payments so you have a cushion in bad times."

Table F.2, columns 1-2, reports that exposure to the destigmatizing treatment displayed in Figure F.1a makes respondents 27% more likely UI to car insurance (11 pp on a base of 40%). This result is highly statistically significant and robust to restricting the survey sample to those who successfully passed attention checks. Columns 5-6 show that this shift in beliefs translates to an increase in self-reported likelihood of applying for benefits, as the reduced form shows that respondents who passed attention checks are 28% more likely to apply for UI (9 pp on 31% base). This survey evidence aligns with the reduced form findings from the field experiment (Figure 6a), in which destigmatizing messages increased application rates, and lends credence to the interpretation that these effects operate through shifts in stigma-related attitudes rather than alternative channels.

Search Treatment Does Not Affect Job Finding Beliefs

Figure 7 shows that the randomized search expectation treatment was unsuccessful at increasing the UI application rates from the treatment group. A natural question then becomes whether setting realistic search expectations is not a relevant mechanism by which UI take-up is increased for this sample or instead whether the search messages were unsuccessful at shifting the job finding expectations of potential job losers in the first place. Table F.2, columns 3-4, finds evidence for the latter.

Table F.2 shows that being randomly exposed to the message displayed in Figure F.1b has no statistical effect on respondents' reported subjective job finding rates, as point estimates of -2 to 2 percentage points are smaller than the standard errors and change signs depending on whether the one-month or three-month horizon is used (a negative sign was the intent of the treatment). Columns 5-6 show that treatment does not increase in self-reported likelihood of applying for benefits, also consistent with Figure 7.

Table F.1: Characteristics of Non-Claimant Job Separations, Administrative vs. Survey Data

Variable	Administrative (WA)	Survey (US)
Ever Received UI	0.59	0.43
	(0.49)	(0.50)
Previous Hourly Wage	25.68	21.28
	(6.77)	(26.29)
Previous Annual Salary	46,357	38,666
	(16,514)	(47,306)
Base Year Hours Worked	1,804	1,813
	(357)	(645)
Quarterly Percent Hours Drop, $t-1$ to t	-0.65	-
	(0.25)	
Number of Months Since Previous Job	-	11.2
		(9.0)
Layoff Industry		
Retail Trade	0.166	0.106
	(0.372)	(0.308)
Health Care and Social Assistance	0.145	0.109
	(0.352)	(0.312)
Manufacturing	0.093	0.083
	(0.290)	(0.276)
Construction	0.079	0.102
	(0.270)	(0.303)
Administrative, Support, Waste Management	0.084	0.006
	(0.278)	(0.075)
Accommodation and Food Services	0.087	0.145
	(0.282)	(0.353)
Transportation and Warehousing	0.070	0.038
	(0.254)	(0.191)
Professional, Scientific, and Technical	0.058	0.015
	(0.235)	(0.122)
Wholesale Trade	0.055	0.008
	(0.227)	(0.087)
Other Services (except Public Admin)	0.041	0.274
	(0.199)	(0.446)
Demographics		
Share Female	0.44	0.60
	(0.50)	(0.49)
Age	41.0	43.2
	(15.4)	(11.4)
N	50,199	530

Note: Table shows balance across economic, industrial, and demographic characteristics for experimental sample of potential job losses (WA administrative data) and national survey sample. Industry shares for survey group do not sum to 1 because table omits industries of respondents that are not included in experimental sample of potential job losses. Administrative data numbers are pulled from Table A.6.

Table F.2: Effects of Stigma and Search Expectation Treatments in Online Survey

	UI similar to car insurance		Subjective JFR		Likely to apply for UI	
			1 Mo.	3 Mo.		
	(1)	(2)	(3)	(4)	(5)	(6)
Stigma Treatment	0.106***	0.103**			0.053	0.086**
	(0.043)	(0.046)			(0.041)	(0.044)
Search Treatment			-0.016	0.026	-0.010	-0.033
			(0.030)	(0.032)	(0.041)	(0.044)
Constant	0.400***	0.419***	0.415***	0.591***	0.321***	0.306***
	(0.030)	(0.033)			(0.029)	(0.031)
Passed Attn Checks		✓	✓	✓		✓
N	530	459	459	459	530	459

Note: Robust standard errors in parentheses. "Similar to car insurance" means respondent said it is similar or very similar. "Likely to Apply for UI" means respondent said they were likely or very likely to apply after completing the survey. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$