Unemployment Insurance, Wage Pass-Through, and Endogenous Take-Up*

Martin Gervais
University of Georgia

Roozbeh Hosseini
University of Georgia

Lawrence Warren
U.S. Census Bureau

June 4, 2026

Abstract

This paper studies how unemployment insurance (UI) generosity affects reservation wages, re-employment wages, and benefit take-up. Using Benefit Accuracy Measurement (BAM) data, we estimate a cross-sectional elasticity of reservation wages with respect to weekly UI benefits of 0.014. Exploiting state variation in Pandemic Unemployment Assistance (PUA) intensity and the timing of federal supplements, we find that expanded benefits during Covid-19 increased reservation wages by 8–12 percent. Over the same period, the UI take-up rate rose from roughly 30 to 40 percent; Probit estimates indicate that higher benefit levels, rather than changes in observables, predict this increase. To interpret these findings, we develop a directed search model with an endogenous filing decision. The model shows that large benefit expansions primarily operate through the extensive margin of take-up, expanding the pool of claimants in ways that dampen the observed pass-through from benefits to wages.

Keywords: Unemployment Benefits, Reservation/Re-Employment Wage, BAM, CPS

*We thank Brant Callaway, Nir Jaimovich, Sean McCrary, Amanda Michaud, Kurt Mitman, Lee Tucker, Gianluca Violante, David Wasser, David Wiczer, and seminar participants at the I-85 conference at Vanderbilt, LAEF in Santa Barbara, SED in Cartagena, the Midwest Macro at Clemson, Miami, Ohio State, the Vienna Macro Workshop, the BSE Summer Forum in Barcelona, the EEA Congress in Barcelona, as well as the Einaudi institute in Rome for helpful comments and suggestions. Any opinions and conclusions expressed herein are those of the authors and do not represent the views of the U.S. Census Bureau. The research in this paper does not use any confidential Census Bureau information.

1 Introduction

This paper examines how unemployment insurance (UI) generosity affects reservation wages, re-employment wages, and benefit take-up. During the Covid-19 pandemic, UI benefits expanded dramatically, most notably through the $600 weekly supplement introduced by the Coronavirus Aid, Relief, and Economic Security (CARES) Act in March 2020 and the subsequent $300 weekly supplement under the Continued Assistance Act and American Rescue Plan. These policy changes raise a natural question: how does increased UI generosity affect unemployed workers, in particular the pass-through from benefits to reservation and re-employment wages and the decision to claim benefits? We answer this question using data from the UI Benefit Accuracy Measurement (BAM) system and the Current Population Survey (CPS), together with a directed search model featuring an explicit take-up margin.

Our central finding is that the pass-through from benefits to wages is modest, while take-up rose sharply alongside UI generosity, in both the data and the model. A key contribution of the paper is to show that these two responses are closely linked through a single mechanism. Claiming UI is itself costly, and workers vary in how heavily those costs weigh on them. Workers who choose not to claim—because the costs outweigh the benefit—face unemployment without UI's financial cushion, so they accept lower-wage jobs to leave unemployment more quickly. When benefits rise sharply, these are the marginal claimants drawn into the program. The pool of claimants therefore grows with workers whose reservation and re-employment wages are relatively low, explaining both the strong take-up response and the muted wage pass-through.

Under normal circumstances, the U.S. UI system provides temporary income support to workers who lose their jobs through no fault of their own and who satisfy state-specific earnings and work history requirements. Eligibility typically requires a qualifying separation, such as a layoff rather than a voluntary quit, and sufficient recent earnings. Even among eligible individuals, however, not all collect benefits. Take-up reflects a range of factors, including expectations about re-employment, eligibility constraints, administrative and informational frictions, and individuals' perceived value of benefits.[1]

During the Covid-19 period, both the level and scope of UI benefits expanded sharply. The Federal Pandemic Unemployment Compensation (FPUC) program provided an additional

[1] These categories summarize responses from the 2018 UI Supplement to the CPS, which asked non-recipients why they did not apply for benefits.

$600 per week from March through July 2020 and an additional $300 per week in early 2021. The Pandemic Unemployment Assistance (PUA) program extended UI eligibility to self-employed workers and others not typically covered by regular UI. These programs raised benefit levels for existing recipients and broadened eligibility to new groups of workers, generating substantial and rapid changes in both the generosity of UI and the composition of potential claimants. These policy shifts form the basis for the variation we exploit in our empirical analysis.

We begin with data from the Department of Labor's Benefit Accuracy Measurement (BAM) system, which audits a nationally representative sample of UI claims in each state.[2] For each selected claim, state agencies verify eligibility, benefit amounts, and separation reasons, and supplement the administrative records with survey responses. Crucially, the BAM survey records both the claimant's current reservation wage and their usual hourly wage in the job held prior to unemployment. These measures allow us to examine the relationship between reservation wages, prior wages, and UI benefits, and to track how this relationship evolved over time, particularly during the pandemic when UI generosity increased sharply.

BAM includes two distinct samples: individuals who received UI benefits (the paid claims sample) and those who applied but were denied (the denied claims sample). Across both groups, a consistent pattern emerges: reservation wages rise with prior wages but remain below them, reflecting a typical job ladder effect in unemployment. Notably, this relationship changes little over time, despite the large and temporary increases in UI generosity during the pandemic.

Using the BAM paid claims sample, we estimate the elasticity of reservation wages with respect to UI benefits using cross-sectional variation in benefit levels. The estimated elasticity is small but positive: a 10 percent increase in benefits is associated with a 0.14 percent increase in the reservation wage. To assess how this relationship evolved during periods of elevated UI generosity, we use a Blinder-Oaxaca decomposition. In 2020, the decline in reservation wages is almost entirely explained by compositional shifts among claimants, in particular a larger share of younger and non-manufacturing workers.[3] Absent these shifts, the expected reservation wage would have increased by roughly 4 percent. In 2021, expanded benefits are associated with a 7 percent increase in reservation wages relative to the pre-

[2]While BAM's sampling procedure is systematic, the provided sampling weights render the sample nationally representative.

[3]Note that BAM offers limited coverage during 2020.

pandemic period, conditional on a fixed composition of claimants. Changes in observable characteristics offset this by about 2 percentage points, yielding a net observed increase of 5 percent.

To further assess the impact of expanded UI access on reservation wages, we exploit variation in state-level participation in the Pandemic Unemployment Assistance (PUA) program, which provided benefits to individuals who were monetarily ineligible for regular UI. Because eligibility for PUA required first being denied traditional UI, we use the denied claims sample in BAM and compare reservation wages across states with relatively high versus low PUA participation.[4] Treated states are those with above-median PUA claim shares. An event-study design shows that reservation wages rose more in treated states during the PUA period, coinciding with elevated benefit levels, and that these differences faded as PUA expired. These results indicate that expanded access to UI benefits through PUA led to a modest increase in reservation wages among workers who were otherwise ineligible for regular UI.

To quantify the elasticity of reservation wages with respect to benefit generosity, we estimate a continuous-treatment Difference-in-Differences model. We exploit quarterly variation in state-level PUA claim intensity and distinguish between periods when only PUA was active and those when both PUA and the $300 FPUC supplement were in effect. The increase in expected benefits during the PUA + FPUC period is associated with a 7.6 to 12.4 percent rise in reservation wages across specifications, as reported in Section 3. Assuming a $300 increase on a base weekly benefit of $176 (a 170 percent increase), these changes imply an elasticity of reservation wages with respect to UI benefit amount of between 0.067 and 0.108.[5] These results are robust to controls for labor market tightness and Covid severity and further confirm that while reservation wages respond positively to benefit generosity, the response is modest in magnitude.

While the BAM data provide detailed information on UI recipients, they capture only individuals who applied for benefits and thus offer a partial view of the unemployed population. To examine labor market transitions and outcomes more broadly, we turn to the CPS, which

[4]The official Federal guidance (https://www.dol.gov/sites/dolgov/files/ETA/advisories/UIPL/2021/UIPL_16-20_Change_5_acc.pdf) for the PUA program states that "To be eligible for PUA, the state must verify that the individual is not eligible for regular UC (or PEUC or EB)."

[5]Appendix B.1 reports a complementary check using CPS rotation data: comparing re-employment wages for UI-eligible vs. ineligible workers yields a 9 percent eligible wage premium during the Covid period, consistent with this range.

includes both UI recipients and non-recipients. Using these data, we identify unemployed workers likely eligible for UI and compare their outcomes—including re-employment wages and benefit take-up—to those of ineligible workers.

While the wage response to UI generosity is modest, the extensive margin moves sharply. In these data, a Probit model of UI receipt shows that higher benefit levels—rather than demographic characteristics—predict the sharp rise in take-up during the pandemic. These results indicate that the principal behavioral response to UI policy operates through the take-up margin rather than through changes in reservation or re-employment wages.

Taken together, the BAM and CPS results reveal a consistent pattern: wage responses to UI generosity are modest, while take-up rose sharply alongside benefit generosity. Explaining why these margins move so differently requires a framework in which claiming UI is itself a decision. We therefore develop a directed search model with an endogenous filing choice inspired by Auray et al. (2019). Workers differ in the cost of filing for benefits, so increases in UI generosity expand the pool of UI collectors while altering its composition. The model allows us to quantify how changes in benefit levels affect both reservation and re-employment wages through this take-up margin.

In this environment, the take-up margin plays a central role in shaping wage pass-through. As UI benefits increase, additional workers choose to file for benefits, and these marginal claimants tend to have relatively low re-employment wages. Their entry raises the take-up rate but puts little upward pressure on wages, thereby dampening the overall elasticity of reservation and re-employment wages with respect to UI generosity. A quantitative version of the model shows that incorporating this endogenous selection margin is essential for matching the empirical patterns observed in BAM and CPS.

Two central empirical regularities emerge from our analysis. First, the pass-through from UI generosity to reservation and re-employment wages is modest, even during the extraordinary benefit expansions of the pandemic period. Prior work has documented similarly small wage elasticities in more stable environments, but these studies rely either on cross-sectional correlations or on policy variation in benefit duration rather than benefit levels; see Krueger and Mueller (2016), Card et al. (2007), Schmieder et al. (2013), and Nekoei and Weber (2017).[6] Our contribution is to show that the muted wage response extends to large,

[6]There is also a macro literature on the effects of changes in the *duration* of UI benefits. For example, Chodorow-Reich et al. (2019) find small effects of duration extensions on aggregate unemployment, while Hagedorn et al. (2025) find larger effects on wages and vacancy creation in a border-county design.

policy-driven changes in the level of benefits: even when replacement rates exceeded 100 percent for large segments of the unemployed, eligibility broadened sharply through PUA, and search requirements were relaxed, wage responses remained modest. This provides a rare stress test of wage-setting mechanisms under extreme UI policy conditions.

Second, we find that UI generosity has a pronounced impact on the extensive margin of benefit claiming. While Blank and Card (1991) document that take-up varies systematically with state replacement rates using pooled state-by-year data, their evidence reflects relatively modest cross-state policy variation and does not exploit large, discrete changes in benefit levels or eligibility rules. In contrast, the Covid-19 expansions provide a large-scale natural experiment in which benefit levels, eligibility criteria, and administrative frictions all shifted sharply. Using BAM and CPS microdata together, we find that higher benefit generosity, rather than changes in worker characteristics, predicts almost all of the surge in take-up during this period.[7] This provides, to our knowledge, the first evidence on the empirical link between large benefit-level changes and UI take-up and highlights the central importance of the extensive margin in large-scale UI expansions.

Importantly, this endogenous composition effect amplifies the extensive-margin response to benefit-level changes while simultaneously dampening wage pass-through. The model therefore unifies the empirical findings and highlights that, without an endogenous take-up margin, the effects of large UI expansions on wages would be overstated.

Our findings are closely related to a growing literature on the labor-market effects of the Covid-19 UI expansions. That literature generally concludes that despite the scale of government intervention, the impact on incentive-related labor market behavior was modest.[8] For example, Petrosky-Nadeau and Valletta (2025) use a standard search model to estimate the benefit level at which individuals are indifferent between accepting a job and remaining unemployed, and find that the 2020 expansions did not deter most workers from taking jobs. Boar and Mongey (2020) reach similar conclusions using a model that allows for job offers to expire and for downward wage mobility. Using proprietary bank account data, Ganong et al. (2024) find only modest increases in job finding rates when the $600 and $300

[7] Related work by Forsythe and Yang (2021) uses survey data to document how UI recipiency evolved during the pandemic, but does not estimate the causal effect of benefit levels on take-up. McQuillan and Moore (2025b) estimate this effect using a regression-kink design, and Moore and McQuillan (2025) examine how UI access affects labor-market outcomes for marginally attached workers.

[8] Some labor market outcomes, such as initial unemployment insurance claims, did respond sharply to the onset of the pandemic.

supplements expired. Michaud (2023) studies UI expansions to low-wage workers and shows that although benefit duration rose substantially for new recipients, standard models over-predict the effect, revealing a quantitative puzzle. While most of this work abstracts from the decision to claim benefits, our empirical analysis shows that take-up responds systematically to expected benefit amounts. This motivates a structural approach in which take-up is endogenous, allowing us to quantify its role in shaping policy effects. In this respect, we complement recent structural work such as Auray et al. (2019), Blasco and Fontaine (2021), and Birinci and See (2023, 2024), which embed endogenous claiming in search models, by bringing reservation wages, re-employment wages, and take-up together in a unified empirical and quantitative analysis disciplined by U.S. pandemic-era data.

The remainder of the paper is organized as follows. Section 2 reviews the structure of the U.S. unemployment insurance system and describes how the CARES Act and related legislation expanded both the level and scope of benefits during the pandemic. Sections 3 and 4 present our empirical evidence using BAM and CPS data, respectively. Section 5 presents a directed search model with endogenous take-up designed to rationalize the observed weak pass-through from benefits to wages and the strong responsiveness of take-up to benefit levels. Section 6 concludes.

2 Overview of the Unemployment Insurance Program

Unemployment Insurance is a joint state-federal program that aims to provide temporary financial assistance to unemployed workers. While each state has specific rules, qualified individuals are typically entitled to a fraction of their earnings over the last four quarters as unemployment benefits, subject to a maximum. Below we review the basic rules governing eligibility for this program, followed by a description of how these rules changed during the Covid era.

2.1 Regular Unemployment Insurance

To be eligible, unemployed individuals must meet two main criteria. First, they need to have lost their job through no fault of their own. As such, job quitters, new entrants into the labor force, and re-entrants into the labor force are not eligible.[9] Second, individuals need to satisfy state-specific work/earnings requirements. We refer to the first criterion as *non-monetary eligibility*, and the second as *monetary eligibility*. Both must be satisfied independently—failure on either front renders a person ineligible.

Claimants must also *maintain* eligibility. First, as is well known, benefits expire after a certain number of weeks. While the duration of benefits available is set at 26 weeks for many states, there are variations that depend on past income and how it is distributed over the previous year, the overall unemployment rate in the state, and the level of weekly benefits itself, as some states set a maximum yearly benefit amount.[10] For the purpose of this paper, we use a uniform 26 weeks maximum duration across all States prior to the Covid relief period.[11]

In addition to duration limits, recipients must also maintain active eligibility by complying with various requirements that can be state-specific. These typically include: filing weekly or biweekly claims; being available for and actively seeking work; reporting any earnings, job offers, or declined offers; attending mandatory job center appointments; registering with the state employment service if required.

2.2 Covid-19 Relief Period

The CARES Act was signed into law on March 27, 2020. The CARES Act expanded unemployment insurance benefits to millions of workers affected by Covid-19 through three main programs: the Federal Pandemic Unemployment Compensation (FPUC) program, the Pandemic Emergency Unemployment Compensation (PEUC) program, and the Pandemic Unemployment Assistance (PUA) program. We briefly describe each program below.

The *Federal Pandemic Unemployment Compensation* (FPUC) program is perhaps the best

[9] Note that since firms' contributions to the program are a function of the likelihood their workers claim benefits, the 'no fault' condition can sometimes be litigious (see Auray et al. (2019), Fuller et al. (2015) and Lachowska et al. (2025)).

[10] For details, see 'Duration of Benefits' in section 3 of the following document: https://oui.doleta.gov/unemploy/pdf/uilawcompar/2022/complete.pdf.

[11] Note that most States still had a 99-week cap extension in place up to January 2014 following the Great Recession. Several states currently offer less than 26 weeks of regular UI benefits: Alabama (14), Arkansas (16), Florida (12), Idaho (21), Iowa (16), Kansas (16), Kentucky (12), Michigan (20), Missouri (20), North Carolina (12), Oklahoma (16) and South Carolina (20). Montana is the only state that offers more than 26 weeks (28).

known. Under the CARES Act, FPUC provided eligible individuals who collected unemployment benefits with an additional $600 per week from inception until July 25, 2020. Efforts were made to extend this program through an executive order allowing an extra $400 of benefits, with $300 funded at the Federal level requiring a $100 match by the State, starting August 1, 2020. The funding from this program, which came from previously appropriated funds, quickly ran out and its total disbursement turned out to be minimal. However, the Covid Relief Bill signed into law on December 27, 2020, renewed the FPUC program by extending federal unemployment assistance to the tune of $300 per week through March 14, 2021.[12] This $300 of extra benefits was further extended through the American Rescue Plan Act (ARPA), signed into law March 11, 2021. While this last extension was scheduled to sunset September 5, 2021, several states chose to end the program as early as June 12, 2021.

The *Pandemic Emergency Unemployment Compensation* (PEUC) program provided up to 13 additional weeks of benefits to individuals who had exhausted their regular unemployment compensation. For most states, this increased the maximum number of weeks of compensation to 39 weeks. This program was initiated by the CARES Act and remained in place until September 5, 2021, though some states elected to end the program as early as June 12, 2021.

The *Pandemic Unemployment Assistance* (PUA) program affected eligibility along several dimensions. At the outset, this program was available to individuals who applied for and were denied regular UI benefits and to workers, such as the self-employed and independent contractors, who were not typically covered by regular UI.[13]

In terms of non-monetary eligibility, PUA applied to individuals who were unemployed, partially unemployed, or unable or unavailable to work because of one of several Covid-19-related reasons, running from having Covid itself to caring for someone with Covid or even having quit a job because of Covid. In a nutshell, to receive PUA compensation, applicants

[12]The Covid Relief Bill of 2020 also created a new program: the Mixed Earner Unemployment Compensation (MEUC) program. The MEUC program provided individuals with both traditional *and* freelance income an additional $100 per week benefit, for a total of $400, if the worker received W2 wages and at least $5,000 in self-employment (such as 1099) income during the latest taxable year. Note that to qualify for MEUC, one must also be an eligible recipient of an unemployment benefit program other than the Pandemic Unemployment Assistance (PUA) program, which we discuss next.

[13]As stated in footnote 4, the official Federal guidance for the PUA program states that "To be eligible for PUA, the state must verify that the individual is not eligible for regular UC (or PEUC or EB)." See https://www.dol.gov/sites/dolgov/files/ETA/advisories/UIPL/2021/UIPL_16-20_Change_5_acc.pdf for details.

needed to provide self-certification that they were partially or fully unemployed, or unable and unavailable to work because of one of many Covid-related circumstances. Unlike other programs, PUA claims could in principle be backdated up to February 2, 2020, provided that an individual met the eligibility requirements to receive PUA as of that date, including the requirement that the individual's unemployment was due to Covid-19 related reasons. For those who qualified, compensation under the PUA program was available for up to 39 weeks. This program went uninterrupted until September 5, 2021, though as with other programs some states elected to end it as early as June 12, 2021.

The PUA program also expanded the notion of qualifying income to include self-employment income, including income of independent contractors and gig workers, and relaxed work requirements for individuals who had not worked long enough to qualify for regular unemployment compensation. Essentially, individuals needed to provide some kind of proof (e.g., pay stubs, income tax return, bank statements, offer letter, etc.) documenting any kind of employment or self-employment that was impacted by Covid-19, or even to document work that would have begun on or after the date when Covid-19 impacted an individual's employment status.

It is worth emphasizing that for many individuals, benefits under the PUA program were significant. First, individuals had to be denied regular UI benefits, and so would normally not have access to any benefits, many because of lack of sufficient work/income history. Second, any individual who qualified under the PUA program was entitled to the minimum Disaster Unemployment Assistance (DUA) weekly benefit amount, which was set to equal 50% of the average weekly payment of unemployment compensation in the state.[14] Third, all individuals who qualified for PUA automatically qualified for FPUC, i.e. received an extra $600 while the program was in effect in 2020 and $300 of extra benefits during the relevant months in 2021. As such, we will argue later that individuals had strong incentives to apply for and be denied regular unemployment benefits in order to apply for and qualify for compensation under the PUA program.

[14]The precise amount of minimum weekly benefit for each state as off the signing of the CARES Act can be found at https://www.dol.gov/sites/dolgov/files/ETA/advisories/UIPL/2019/UIPL_03-20.pdf.

3 Empirical evidence from BAM data

Administered by the U.S. Department of Labor, the purpose of the Benefit Accuracy Measurement (BAM) system is to assess the accuracy of payments and claim decisions for the *regular* Unemployment Insurance program. Specifically, a random subset of claimants and rejected applicants are surveyed and thoroughly examined to determine whether payments were properly administered to claimants or appropriately denied. The administrative data produced by the system consist of two systematic and independent samples of individuals: a paid claims sample of individuals who are 'currently' receiving UI payments; and a denied claims sample of individuals who have 'recently' received disqualifying ineligibility determinations.[15]

A clear advantage of BAM data is that it allows us to study samples of individuals known to be UI benefit recipients or denied claimants. In more commonly used datasets, such as the CPS (see next Section), UI eligibility and collection must be imputed based on limited earnings and labor market history. The fact that the take-up rate of UI benefits is low among the eligible population makes it particularly difficult to study outcomes for UI recipients in traditional datasets.[16] However, BAM data does not allow us to study individuals who did not apply for UI benefits. And since each case investigated by the BAM system provides a single datapoint for a UI recipient or a denied claimant, BAM does not offer a panel dimension. Our ability to measure labor market outcomes within BAM is correspondingly limited.

Below we use BAM data to measure the extent to which individuals' job search behavior responds to changes in UI benefits, exploiting the Covid-19 relief programs documented in the previous section. We highlight the change in the composition of UI collectors during 2020 and 2021 and its influence on the reservation wage. We also estimate a causal effect of UI eligibility and a corresponding elasticity of reservation wages to UI benefits. But first, we document the sampling procedure of BAM data and describe our samples.

[15] See U.S. Department of Labor, Employment and Training Administration (2009) for BAM's Handbook.

[16] In the Survey of Income and Program Participants (SIPP), where unemployment compensation receipt is specifically asked, under-reporting of UI receipt is a known issue: see 2021 and 2022 Data User Notes https://www.census.gov/programs-surveys/sipp/tech-documentation/user-notes/2021-usernotes/volat-unemp-comp-during-covid19-pand.html and https://www.census.gov/programs-surveys/sipp/tech-documentation/user-notes/2022-usernotes/2022-undrestim-unemp-comp-dur-pandmc.html.

3.1 Sampling Procedure and Data Collected

At the outset, it is important to note that the set of paid claims cases is sampled from a "stock" measure, as the population consists of all currently collecting UI claimants in the week. By contrast, the set of denied claims cases is sampled from a "flow" measure of denied initial claims during a specific week. The denied claims sample contains roughly equal proportions of three types of denials: monetary denials (e.g., insufficient earnings), separation denials (e.g., quits), and non-monetary non-separation denials (e.g., unable and/or unavailable to work, not seeking work, etc.).

The sampling of cases, for either paid claims or denied claims, is not proportional to the total population or the unemployed population within each state. Rather, a fixed annual number of cases is set by the Department of Labor for each state and administered roughly uniformly across all weeks during the year.[17] In practice, some variations in sample size from state to state do occur for various reasons. That said, each observation in the BAM dataset comes with a weight equal to the inverse probability of being sampled from the respective state populations (paid claims or each type of denial).

Through the process of investigating each paid claim, the claimant is surveyed about several aspects of their previous job (wage, industry, occupation), job search (reservation wage, searching industry), and unemployment benefits (weekly benefit amount). Each investigation centers on a reference or "key week" for evaluating appropriate payment of the claim. Denied claimants are also surveyed, providing similar information on the claimant's work history, job search activity, and the rationale for denial. The dataset also includes basic demographic characteristics such as age, sex, race, and education.

Our analysis makes extensive use of the concept of *reservation wage*. The reservation wage is elicited during each individual's survey interview, and corresponds to the answer to the question: "What is the lowest rate of pay you will accept for a job?" Interviewers are instructed to express the answer in dollars and cents per hour.[18] As shown below, reservation wages closely track usual hourly wages, albeit at a discount—a pattern consistent with Davis

[17] Currently, the number of paid claims cases for each state is set at 480, except for the 10 smallest UI workload states for which the number of cases is set at 360. For denied claims, the number of cases is uniform across all states and is set to 150 cases for each of the 3 types of denial.

[18] If the reported amount is not in hourly terms, interviewers are instructed to apply a state-specific conversion formula. See U.S. Department of Labor, Employment and Training Administration (2009) for details.

and Krolikowski (2024).

3.2 Samples

The BAM data consist of repeated weekly cross-sections, based on randomly selected survey participants. We use data from January 2014 to June 2022.[19] We restrict our sample to claimants between 16 and 65 years of age. We Winsorize usual hourly wage and reservation wage at the 1st and 99th percentiles. We also Winsorize the weekly benefit amount at the 99th percentile. All values are deflated to 2021 dollars.

Our analysis uses both the paid claims sample and the denied claims sample. As we alluded to above, the design of the PUA program leads us to focus on the monetary denials subset of the denied claims sample. Table 1 displays summary statistics for the paid and denied claims samples, as well as the subset of monetary denials. Denied claimants tend to be slightly younger and less educated, and they typically report lower wages—especially those denied for monetary reasons. This latter sample also has higher representation in the leisure and hospitality industry, though lower representation in the manufacturing industry.[20]

We begin by validating the reservation wage measure. Figure 1 shows the local polynomial regression of the reservation wage ($\ln(\tilde{w})$) on the usual hourly wage ($\ln(w_{usual})$) plotted against the 45 degree line, for the paid claims sample in Panel (a) and for the denied sample in Panel (b).[21] This relationship is evidently very linear. Consistent with the concept that workers fall off the job ladder in unemployment, the reservation wage lies below the usual wage, with the gap increasing for higher-wage workers.

Figure 2 shows how UI benefits vary with past wages, revealing a strong nonlinear pattern relative to usual hourly wages. This pattern arises because UI payments are capped at a relatively low maximum weekly benefit amount, effectively flattening benefits beyond a certain wage threshold.

[19] Many states had 99-week cap extensions in place prior to January 2014. Note that BAM coverage is essentially absent from April through June 2020, when state agencies suspended routine audits to process the surge in pandemic-era claims.

[20] Table 8 in Appendix A displays the share of each sample across NAICS 2-digit sectors.

[21] Table 9 in Appendix A presents results from linear regressions of $\ln(\tilde{w})$ on $\ln(w_{usual})$ with various controls.

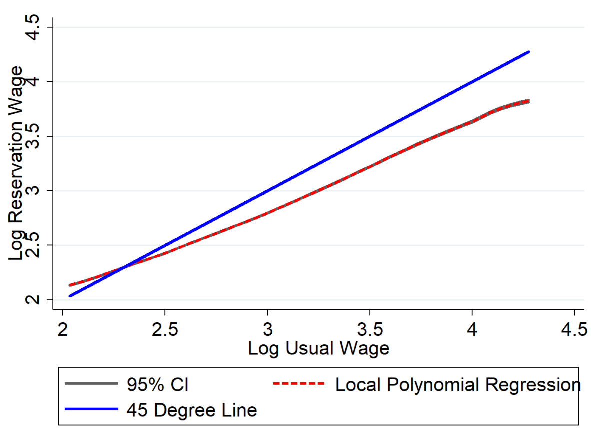
Table 1: Summary Statistics

Sample	Paid Claims	All Denied Claims	Monetary Denials
Avg. Age	41.24	38.23	36.81
Share Female	0.46	0.49	0.48
Share White	0.48	0.44	0.40
Share with College Degree	0.50	0.47	0.35
Share Manufacturing	0.23	0.19	0.15
Share Leisure & Hospitality	0.12	0.12	0.16
Usu. Hrly Wage	\$22.17	\$18.90	\$16.64
Reservation Wage	\$18.56	\$16.17	\$14.67
Weekly Benefit Amt	\$354.75		
Observations	179,230	153,727	42,056

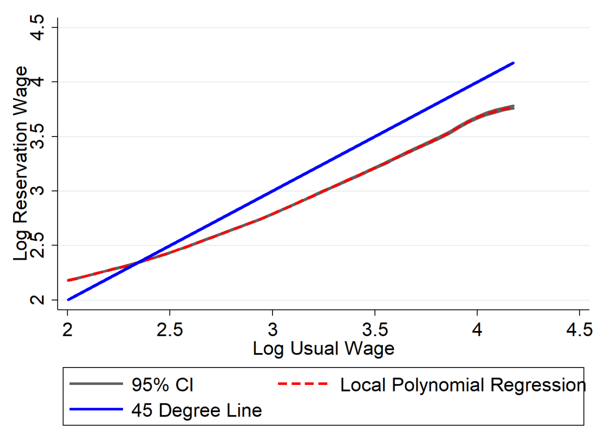
Notes: Share Manufacturing in this table includes NAICS sectors 21: Mining, Quarrying, Oil & Gas Extraction, 23: Construction, and 31-33: Manufacturing. Share Leisure and Hospitality includes NAICS Sectors 71: Arts, Entertainment, & Recreation and 72: Accommodation and Food Services. All dollar amounts are in 2021 dollars. Summary stats are from data spanning from January 2014 to June 2022.

Figure 1: Reservation Wage and Usual Hourly Wage

(a) Paid Claims Sample

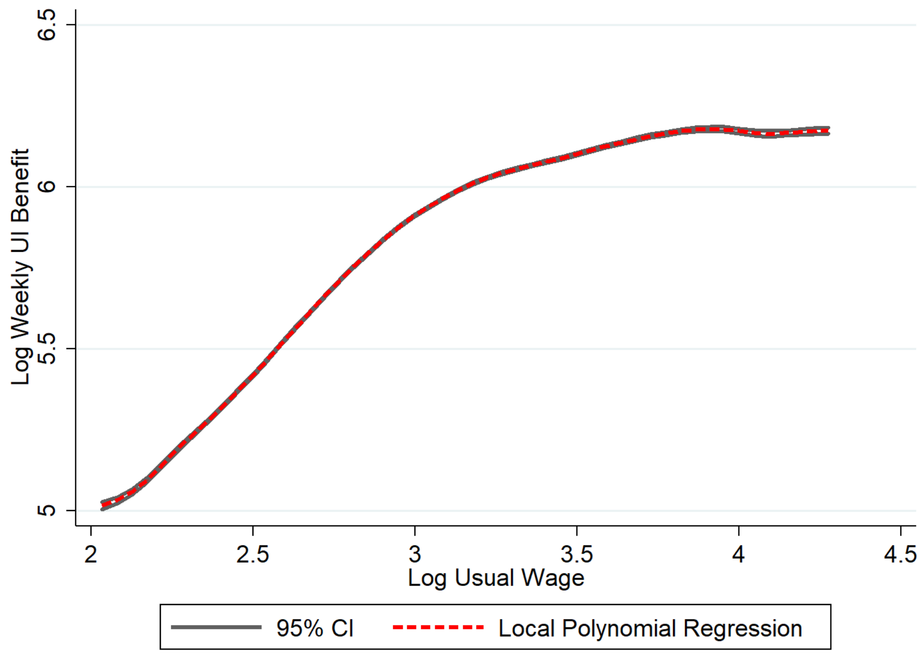


(b) Denied Claims Sample



Notes: Usual wage and reservation wage are Winsorized at the 1st and 99th percentiles. Both variables are deflated to 2021 dollars. Sample sizes are sufficiently large that the 95% confidence intervals are barely visible in each plot.

Figure 2: UI Weekly Benefit and Usual Hourly Wage



Notes: Usual wage Winsorized at the 1st and 99th percentiles. Weekly benefit amounts Winsorized at the 99th percentile. Both variables are deflated to 2021 dollars. Sample sizes are sufficiently large that the 95% confidence intervals are barely visible.

3.3 Reservation Wage and UI Benefits: Evidence from the Paid Claims Sample

In this section, we analyze the paid claims sample to quantify the relationship between the reservation wage and unemployment insurance benefits. We first estimate the reservation wage elasticity with respect to UI benefits and later use a Blinder-Oaxaca decomposition to assess how this relationship changed while the FPUC program was in effect.

To measure the elasticity of the reservation wage with respect to benefits, we regress log reservation wage on the log of weekly UI benefits (including supplemental benefits during FPUC) and various controls using the following specification:

$$\ln(\tilde{w}) = \beta_0 + \beta_1 \ln(Benefit) + \beta_2 \ln(w_{usual}) + \beta_3 X + \varepsilon. \quad (1)$$

Our estimate of the reservation wage elasticity with respect to UI benefits is around 0.014, as shown in Table 2. The first two columns show results for the 2014–2019 period. While there is a strong raw correlation between reservation wage and UI benefits, the coefficient on UI benefits becomes much smaller, though it remains statistically significant, once the usual wage and other controls are added to the regression.[22] The same pattern emerges in the last two columns, which display results for the 2014–2022 period. We note that these estimates are not causal, but we will arrive at a causal estimate using the denied claims sample. [23]

Next, we examine how the relationship between reservation wages and UI benefits evolved during the period when the FPUC program was active. Figure 3(a) shows the trends in reservation wages and usual hourly wages over the sample period, with their ratio displayed in panel (b). At the onset of the Covid-19 lockdowns, there was a sharp decline in both the average reservation wage and the hourly wage of claimants. Both measures gradually recovered through 2020 and 2021, narrowing the gap between them and thus increasing their ratio. As we show below, the decline in the reservation wage was primarily driven by changes in the characteristics of the pool of UI claimants: had the composition of the claimant pool remained constant, the reservation wage would have increased.

Figure 4 illustrates how the pool of claimants changed at the onset of Covid-19, contributing to the decline in the average reservation wage shown in Figure 3(a). For instance, the first row of Figure 4 focuses on age. The left panel (a) shows that while the fraction of unemployed individuals aged 30 and older (based on BLS data) increased, their share among

[22]If benefits and usual hourly wages are highly colinear, it raises concerns about interpreting the magnitude of the coefficient on benefits. Note, however, that benefits are usually a function of earnings, which equal hours worked times wages over a base period, and not just wages. We also run our regression specification using only states where benefits are a function of high quarter earnings (rather than average earnings during a base period) as in Ferraro et al. (2022), which mitigates the colinearity issue. The results, shown in Table 10 in Appendix A, are similar when restricting our sample to states where benefits are only a function of highest quarter earnings.

[23]While research directly estimating the elasticity of reservation wages with respect to UI benefit levels is limited, a related literature studies how unemployment durations respond to benefit generosity. Katz and Meyer (1990) document relatively modest duration responses to changes in benefit levels, and Chetty (2008) shows that much of the observed duration response operates through liquidity effects rather than moral hazard. These duration-based estimates are not directly comparable to our wage elasticities, but they point to generally limited behavioral responses to changes in benefit levels. By contrast, Schmieder et al. (2012) estimate a significantly higher elasticity of unemployment duration to extended unemployment insurance benefits, approximately 0.4, using data from Germany during the Great Recession, where benefit extensions were longer and operated in a different institutional setting. Despite the longer unemployment spells, they find little to no effect on re-employment wages. Using a regression discontinuity design, Chao et al. (2024) find that unemployment insurance eligibility increases quarterly re-employment earnings by approximately 10% for individuals just above the monetary eligibility threshold compared to those just below it.

Table 2: Reservation Wage Elasticity with Respect to UI Benefits

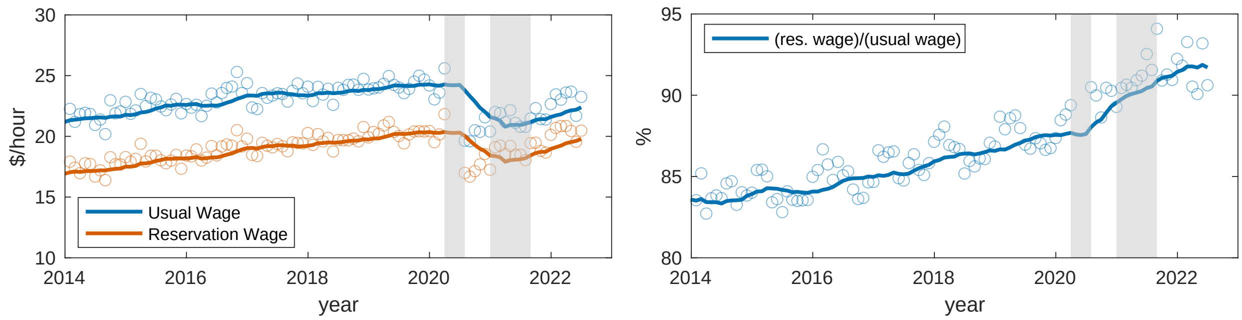
	Jan2014-Dec2019		Jan2014-Jun2022	
	$\ln(\tilde{w})$	$\ln(\tilde{w})$	$\ln(\tilde{w})$	$\ln(\tilde{w})$
$\ln(Benefit)$	0.612***	0.014**	0.365***	0.013*
	(0.019)	(0.006)	(0.019)	(0.007)
$\ln(w_{usual})$		0.757***		0.757***
		(0.017)		(0.019)
U duration		−0.001***		−0.001**
		(0.000)		(0.000)
Age 25-44		0.010***		−0.001
		(0.003)		(0.004)
Age 45-64		0.023***		0.014***
		(0.005)		(0.004)
Age 65+		0.017**		0.003
		(0.008)		(0.016)
Female		−0.019***		−0.015***
		(0.003)		(0.005)
PUA				0.011
				(0.010)
Constant	−0.761***	0.410***	0.621***	0.358***
	(0.112)	(0.053)	(0.115)	(0.074)
Education dummies	No	Yes	No	Yes
Race/Ethnicity dummies	No	Yes	No	Yes
2 dig NAICS dummies	No	Yes	No	Yes
State dummies	No	Yes	No	Yes
Time dummies	No	Yes	No	Yes
Adj. R-squared	0.354	0.798	0.184	0.807
Observations	135,170	132,275	182,862	177,255

Notes: Dependent variable is log of reservation wage. Unemployment duration is measured in weeks. Time dummies are year-month dummies. Standard errors are clustered at the state level. The log of weekly UI benefit includes $600 and $300 dollar supplemental payments during the FPUC program. Robust standard errors, clustered at the state level, are reported in parentheses.

paid claimants decreased significantly—by over 15 percentage points. Meanwhile, the right panel (b) reveals that the reservation wage for younger claimants is typically about 25% lower than for the 30+ age group. Similar patterns are evident in the declining representation of white workers and manufacturing workers and the increasing representation of workers in the leisure and hospitality industry, all of which exerted downward pressure on the average

Figure 3: Reservation Wage and Usual Hourly Wage

(a) Usual & Reservation Wage (b) Reservation/Usual Wage Ratio



Notes: Usual wage and reservation wage are Winsorized at the 1st and 99th percentiles. Both variables are deflated to 2021 dollars. Each circle represents a monthly average observation, with the corresponding line the 12 month moving average. Shaded areas indicate dates over which the FPUC program was in effect.

reservation wage.[24] Changes in the UI collecting population represented by paid claimants in BAM do not necessarily mirror changes in the broader unemployed population. Aside from leisure and hospitality workers in panel (g), the movement in the share of paid claimants is starkly different from the overall unemployed population.

While these figures suggest that the drop in the reservation wage during Covid-19 was largely due to changes in the composition of the pool of UI claimants, we perform a Blinder-Oaxaca decomposition to formally compare the expectation of the reservation wage over the pre-Covid time period to the Covid period during which extra benefits were available. However, the BAM system was essentially suspended from April to June 2020, leaving us with only 1,861 observations while the FPUC program was in place in 2020.[25] Recall that a UI weekly supplement of $600 was in place from April to July in 2020, and an extra $300 was added to UI benefits from January to August in 2021.[26] The decomposition allows us to separate changes in reservation wages into those explained by shifts in claimant characteristics and those explained by changes in how these characteristics translate into reservation wages.

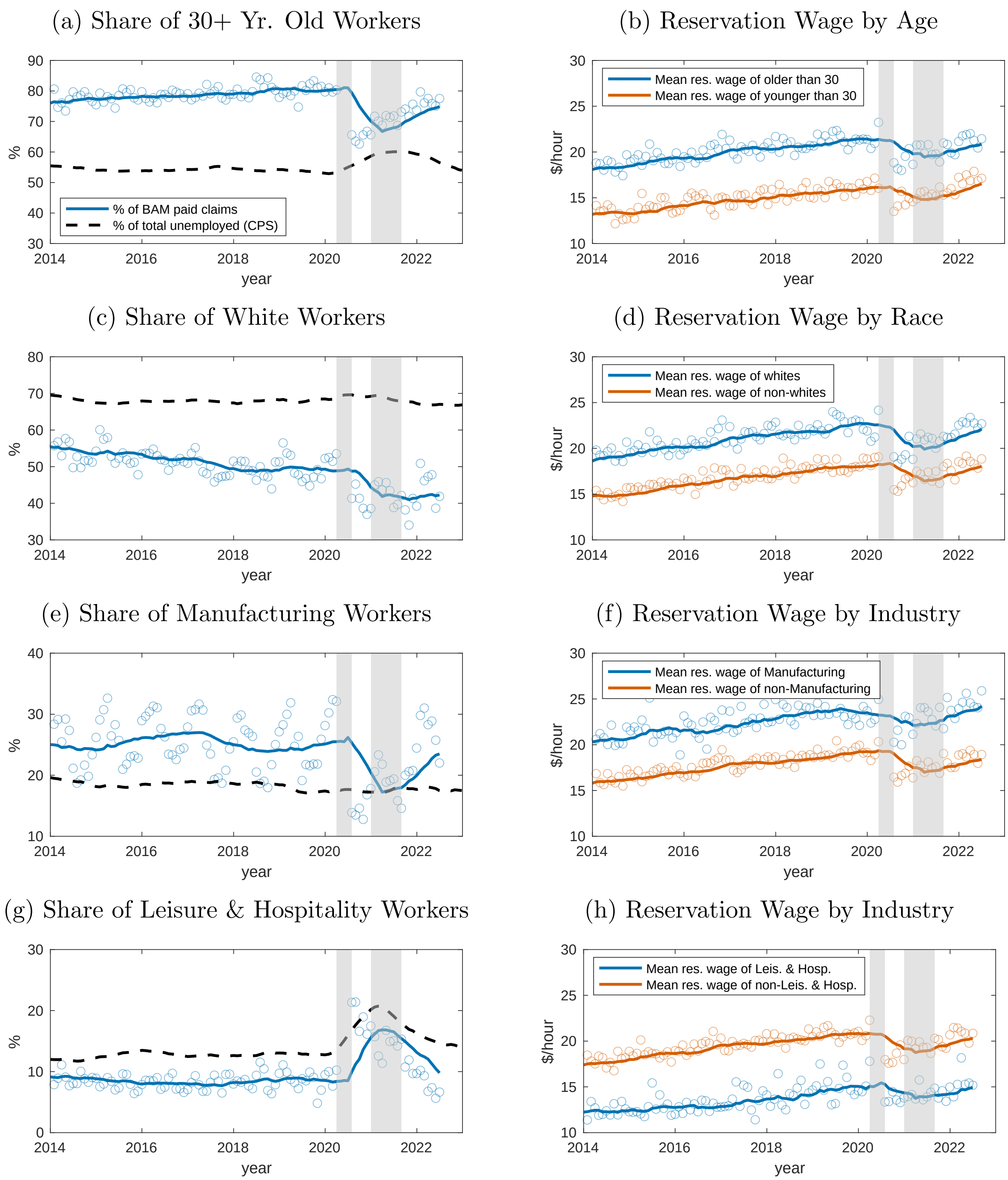
We run a regression of the log of reservation wages on the log of usual hourly wages and the

[24] Composition changes in education and sex are less notable: the share of female claimants rose by about 5 percentage points, while education levels showed little change.

[25] The number of observations used in 2021 is 23,800.

[26] As we emphasized in Section 2, many states chose to end this program prior to the federally mandated sunset date. In the decomposition below, we only include observations during which the FPUC program was active.

Figure 4: Composition changes in the Paid Claims Sample



Notes: Reservation wage Winsorized at the 1st and 99th percentiles, and deflated to 2021 dollars. Each circle represents a monthly average observation. Each line depicts a 12 month moving average. Shaded areas indicate dates over which the FPUC program was in effect.

same covariates as in Table 2 except omitting UI benefits.[27] This regression is run separately for the pre-Covid period, the FPUC period in 2020, and the FPUC period in 2021.[28]

Table 3 shows that the difference in reservation wages between the pre- and post-Covid periods is largely accounted for by changes in the explained component (endowments) in 2020. In other words, shifts in claimant composition are large and actually more than account for the drop, with a smaller coefficients effect partially offsetting them. The small positive change in coefficients suggests that, had the composition of claimants remained constant, reservation wages would have increased by roughly 4%. By contrast, changes in composition pull the reservation wage down by nearly 13%. Recall, however, that coverage during the period of supplemental benefits in 2020 is very limited, which tempers the precision of these estimates.

We repeat this exercise using 2021—during the period when supplemental benefits were available—as the post-Covid sample. Table 4 shows that the difference in the expectation of the reservation wage between samples is small, with a modest increase in the post-Covid period with extended benefits. The decomposition reveals partially offsetting effects between the explained component (endowments) and the unexplained component (coefficients). Specifically, the composition of claimants indicates a decrease in the expected reservation wage of about 2%, but this was offset by a larger change in coefficients: the expanded benefit period in 2021 corresponds to an increase in the expected reservation wage of around 7% when holding observables constant. As discussed above, this relatively small increase in reservation wage is dampened by compositional shifts among UI claimants that persisted into 2021, including a higher share of younger individuals and a continued low representation from the manufacturing sector.

From the paid claims sample, we conclude that there was a substantial composition effect in the reservation wage and usual hourly wage of UI claimants during Covid. This shift in the composition of paid claimants significantly reduced the reservation wage. Once we condition on claimant characteristics, the reservation wage does rise during the periods of elevated benefits, especially in 2021. This composition effect could reflect differences in the

[27] We do not include UI benefits in this specification, as they are part of the policy being evaluated. To the extent that benefits encapsulate otherwise unobservable worker characteristics, such as attachment to the labor force, a specification including benefits minus the FPUC supplements may be warranted. Doing so yields very similar results, as shown in Appendix A Tables 11 and 12.

[28] The controls in these regressions include age bins, sex, 2-digit NAICS codes for the worker's usual job, education dummies, and state dummies.

Table 3: Blinder-Oaxaca Decomposition Pre- and Post-Covid 2020

	Decomposition	95% CI
Pre Covid	2.798***	[2.761,2.835]
2020 Expanded Benefit	2.714***	[2.656,2.771]
Difference	−0.085***	[-0.133,-0.036]
Explained Component (endowments)	−0.127***	[-0.166,-0.088]
Unexplained Component (coefficients)	0.040***	[0.014,0.066]
Interaction	0.002	[-0.005,0.010]

Notes: 2020 Expanded Benefit refers to the time period from March 3rd (week 14) up to August 2nd (week 31) of 2020. Note that BAM data is largely missing from weeks 14 to 26 of 2020. Confidence intervals are calculated from robust standard errors, clustered at the state level.

Table 4: Blinder-Oaxaca Decomposition, Pre- and Post-Covid 2021

	Decomposition	95% CI
Pre Covid	2.798***	[2.761,2.835]
2021 Expanded Benefit	2.851***	[2.805,2.896]
Difference	0.052***	[0.027,0.078]
Explained Component (endowments)	−0.022**	[-0.040,-0.004]
Unexplained Component (coefficients)	0.073***	[0.056,0.090]
Interaction	0.001**	[0.000,0.003]

Notes: 2021 Expanded Benefit refers to the time period from week 1 of 2021 through week 35 (ending Sept 5th) of 2021. FPUC expired federally on September 6th, 2021. Confidence intervals are calculated from robust standard errors, clustered at the state level.

underlying eligible unemployed population or changes in who selects into receiving UI among the eligible unemployed, or both. In Section 4 we turn to the CPS data to distinguish between these possibilities. In particular, we find that higher UI benefits, rather than compositional shifts among the eligible unemployed, predict the increase in take-up rates. This pattern is consistent with benefit-driven selection into UI contributing to the observed changes in reservation wages during Covid. However, these results do not address the causal effect of expanded UI benefits on the reservation wage, leading us to our analysis in the next section using the sample of monetary denials.

3.4 Reservation Wage and UI Benefits: Evidence from the Denied Claims Sample

In this section we leverage the design of the PUA program to shed more light on the reservation wage response to changes in UI benefits. Recall from Section 2 that the PUA program was designed to provide unemployment benefits to individuals who were not eligible for regular UI benefits. Specifically, applicants had to first be denied regular UI benefits in order to qualify for PUA benefits. In other words, the program incentivized individuals with low qualifying income to apply for and be rejected for regular UI benefits. Workers who qualified under the PUA program received a minimum disaster unemployment assistance payment equal to half of the average weekly benefit amount in their state. This minimum weekly benefit amount ranged from \$106 per week in Mississippi to \$267 in Massachusetts.[29] During periods covered by FPUC, recipients would also receive an additional \$300 or \$600 per week.

States varied substantially in their prevalence of PUA claims, and we argue that this variation largely represents idiosyncratic state-level differences in the usage and administration of PUA as a policy.[30] We document that state-level variation in PUA claims as a share of total UI claims appears unrelated to factors such as the state-level unemployment rate or the intensity/severity of the Covid-19 pandemic in the state. We exploit this variation in state-level PUA utilization to estimate the causal effect of increased access to UI for workers who were denied UI benefits for monetary reasons. Workers in this group in states with high PUA utilization had differential access to UI benefits compared to those in states with low utilization, and neither group had access to UI benefits prior to the pandemic.

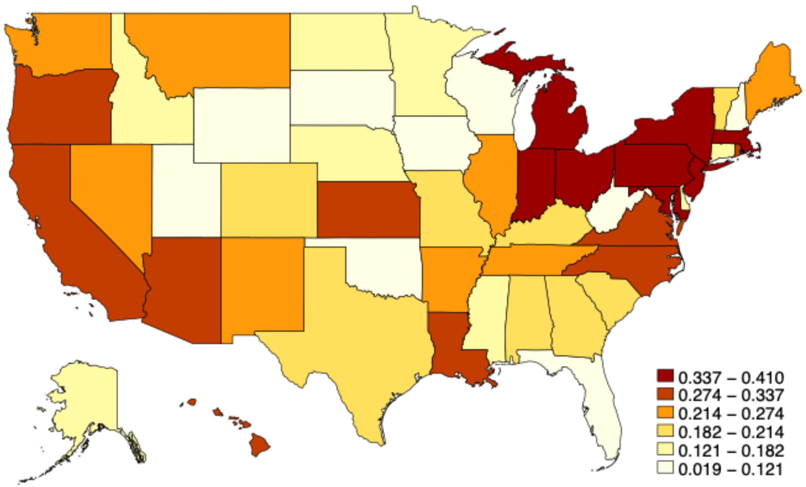
To document the variation in state-level utilization of PUA, we map each state's share of PUA claims as a fraction of total UI claims over the program window. Figure 5 makes clear the substantial cross-state dispersion in state-level PUA claim shares. We consider the share of PUA claimants in a state to be a proxy for expected access to PUA in that state (i.e., the intensity with which the program was used), recognizing that this is not an individual-level eligibility measure but a continuous treatment at the state level.[31]

[29] See https://www.dol.gov/sites/dolgov/files/ETA/advisories/UIPL/2019/UIPL_03-20_Attachment-1_Acc.pdf

[30] See https://www.gao.gov/assets/gao-22-104438.pdf and Navarrete (2024).

[31] The average share of PUA claims as a share of unemployed workers during the PUA period exhibits similar variation, as shown in Figure 23 of Appendix A. We also find similar variation if we exclude continuing claims and measure initial PUA claims over total initial claims, as shown in Figure 18.

Figure 5: PUA Claims as Share of Total UI Claims



Notes: The state-level measure of total PUA claim as a share of total UI claims (regular UI + PUA) for the state during the period where PUA was active.

In our baseline specification, we define the set of treated states as those with a state-level PUA claim share above the median, and the control group as states with a PUA claim share below the median. The outcome of interest is the log of the reported reservation wage of workers. We measure the treatment effect as the difference in expectation of the log of reservation wages, Y, between individuals i in a "treated" state P and those in a "control" state C at time t, following the notation in Callaway and Sant'Anna (2021): Starting from the post-period difference in mean outcomes:

$$\mathbb{E}[Y_{i,t}|i \in P] - \mathbb{E}[Y_{i,t}|i \in C].$$

We are interested in the Average Treatment Effect on the Treated (ATT). Let t indicate time and g indicate the treatment period, then for $t \geq g$:

$$\text{ATT}(t) = \mathbb{E}[Y_{i,t} - Y_{i,g-1}|i \in P] - \mathbb{E}[Y_{i,t} - Y_{i,g-1}|i \in C]$$

Under the standard parallel trends assumption, and conditional on covariates, this becomes:

$$\text{ATT}(t) = \mathbb{E}[Y_{i,t} - Y_{i,g-1}|X, i \in P] - \mathbb{E}[Y_{i,t} - Y_{i,g-1}|X, i \in C]$$

Here, treatment timing does not vary across states because PUA was a federal program rolled out simultaneously in all states[32]. Our controls include the log of usual hourly wage, age bins, sex, race/ethnicity, and NAICS sectors.

Figure 6 displays the coefficients of our difference-in-differences regression. Relative to control states (those with below-median PUA claim shares), our "treated" states exhibited higher reservation wages during the PUA period. The first two quarters after the vertical dashed line correspond to 2020 Q3 and 2020 Q4. The coefficient rises further in 2021 Q1 and 2021 Q2, when the FPUC program provided an additional $300 in weekly payments for those collecting benefits. As the PUA program winds down in the third quarter of 2021, this difference in log reservation wages between treated and control states fades. This event study suggests that for the sample of workers who are monetarily ineligible for regular UI, gaining access to UI via PUA resulted in a small but statistically significant increase in reservation wages.

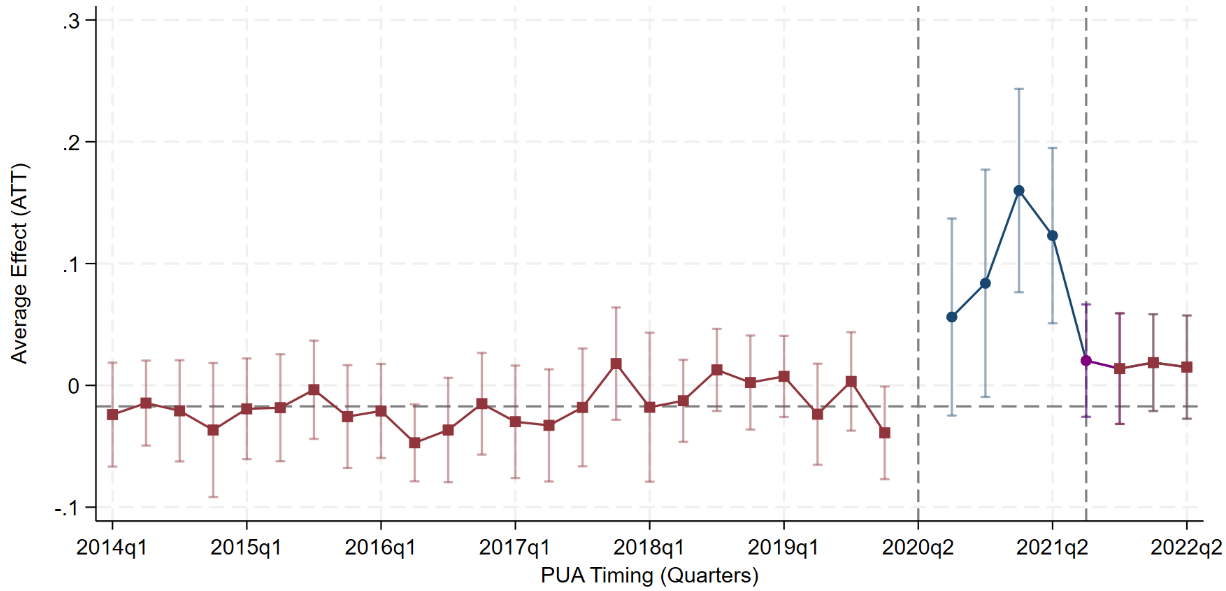
3.5 Threats to Identification and Robustness

Our key assumptions are that treated and control states follow parallel trends, so that, absent differences in PUA exposure, both groups would have exhibited the same trajectory of reservation wages. We also assume that individuals did not anticipate high utilization of PUA in their state prior to the policy. Our design must also satisfy the standard Stable Unit Treatment Value Assumption (SUTVA), meaning that an individual's potential outcomes depend solely on their own treatment status and are unaffected by the treatment assignment of others. Since our treatment and control designation are at the state level, this assumption holds as long as individuals' reservation wage outcomes in a control state are not responsive to outcomes in treatment states[33] We further assume that the treatment was not confounded by other state-level variations coinciding with the timing of PUA. Our event-time plot provides a useful pre-test by showing no clear evidence of differential pre-trends and statistically indistinguishable reservation wages across treated and control states.

[32]As emphasized in Section 2, some states chose to end the PUA program as early as July 2021. We abstract from the staggered phase-out in this specification and will explicitly consider time variation in treatment intensity in Section 3.6. We report estimates using the doubly-robust estimator of Callaway and Sant'Anna (2021) implemented via `csdid`, which in our non-staggered setting reduces to a single treated-control comparison but retains doubly-robust identification and simple covariate reweighting to improve comparability across states.

[33]This should not be conflated with the definition of a local spillover effect, where treatment via expanded UI may affect others' labor market outcomes through mechanisms like search congestion or local equilibrium effects, such as those measured in Doniger and Toohey (2022).

Figure 6: **Reservation Wages and PUA: Monetary Denials Sample**



Notes: Dependent variable is the log of reservation wage. Control group is states with below median level of PUA claim share (.214) during PUA period. Treatment group is states with above median level of PUA claim share during PUA period. Period 0 corresponds to 2020 Quarter 2. The first observation in the treatment period is 2020 Q3. Period 5 corresponds to 2021 Quarter 3 and the end of PUA. Note that some states ended PUA as early as July, while the program ended for all states in September 2021. Confidence intervals are calculated from robust standard errors, clustered at the state level.

Our baseline measure of PUA intensity is the state-level share of PUA claims in total claims (regular UI + PUA) over the period in which PUA is active. This measure incorporates both initial and continuing claims which we believe is a good proxy for perceived access to PUA, because potential applicants observe the stock of PUA recipients rather than only new inflows. A drawback is that continuing claims also embed information about spell duration and local labor market conditions, which could make the measure partly endogenous if it is correlated with search behavior and reservation wages. As a robustness check, we construct an alternative measure based only on initial PUA claims as a share of total initial (UI + PUA) claims. Figure 18 documents the cross-state distribution of this initial-claims intensity, and Figure 19 shows that the corresponding event-time profile is nearly identical to our baseline specification.

We also assess whether our baseline results are sensitive to how we split states into "treated" and "control" groups by PUA intensity. In Appendix A.3, we re-estimate our event-study specification using different quartile cutoffs. First, we treat states in the 50th–75th and 75th–100th percentiles of PUA intensity as separate treatment groups, keeping the bottom half of states as the control group. The resulting event-time profiles in Figure 15 show monotonic treatment effects across these two higher-intensity groups. Second, we use the bottom quartile of PUA intensity as the control group and compare it to the rest of the distribution. Figures 16–17 show that the top half of states remains substantially more affected than the bottom quartile and that the pattern is particularly pronounced for the highest-intensity quartile.

A further concern is that our PUA intensity measure may proxy for other pre-existing state characteristics or cyclical conditions unrelated to UI generosity. We therefore conduct a series of placebo exercises in Appendix A.3.3. First, we implement a placebo timing around the Great Recession, a period with a severe labor market downturn but no policy expanding eligibility to the monetary denial population. Figure 20 shows that, when we shift the event-time window to 2007 Q4, the high-PUA states do not exhibit a positive reservation-wage response. If anything, their reservation wages fall slightly at the onset of the Great Recession, in contrast to the post-2020 pattern.

Second, we reassign "treatment" status based on pre-Covid state characteristics that could plausibly be correlated with both PUA intensity and reservation wages. Using 2019 data from the Quarterly Census of Employment and Wages, we define treatment based on above-median state-level average wages, manufacturing employment shares, and leisure-and-hospitality employment shares prior to the pandemic. As shown in Figure 21, none of these placebo treatments reproduces the persistent increase in reservation wages over the time-period associated with high PUA intensity. Instead, the profiles display transient or negative deviations that more likely reflect pandemic shocks.

Finally, we examine whether our results could be driven by health risks or pandemic-induced layoffs rather than benefit generosity. We stratify states by Covid death rates per capita during the PUA period and by the increase in the unemployment rate from 2019 Q4 to each state's pandemic peak. Figure 22 shows that states with higher Covid mortality or larger unemployment spikes do not exhibit the same sustained reservation-wage increase as high-PUA states. The estimated deviations are modest, often statistically insignificant, and do not align with the timing of PUA and FPUC.

Taken together, these robustness checks support the interpretation that expanding eligibility and benefits through PUA raised reservation wages, and that this effect is not simply capturing other observable state characteristics, cyclical conditions, or pandemic severity. However, since PUA intensity is heterogeneous and our assignment to treatment and control is somewhat arbitrary, the magnitude of these difference-in-differences coefficients is not directly interpretable within our structural model or as an elasticity. We therefore turn next to a slightly modified fixed effects specification with quarterly variation in treatment intensity to recover an elasticity of the reservation wage with respect to benefit generosity.

3.6 Continuous Treatment Specification

We would like to estimate an elasticity of reservation wages with respect to UI benefits. To do so, we consider a continuous treatment variable specification that allows for variation at the intensive margin of PUA claim share. We condense the timing dimension into three distinct treatment regime indicators.[34] The first timing indicator interacts PUA claim shares with a dummy variable for quarters 2020 Q3 and 2020 Q4, representing the regime when PUA benefits were available without the additional benefits from FPUC. The second interacts PUA claim shares with a dummy variable for quarters 2021 Q1 and 2021 Q2, capturing the regime when PUA benefits were available alongside the additional $300 weekly FPUC supplement. The third interacts PUA claim shares with a dummy for 2021 Q3, representing the winding down of both the PUA and FPUC programs. By comparing coefficients across these regimes, we can arrive at an elasticity of reservation wages with respect to UI benefits, *for some constant level of PUA intensity, S*. We show the results of this specification in Appendix A.4. Including the continuous treatment measure in this way allows our estimate to account for variation at the intensive margin of PUA claim share across states. We use the quarterly fraction of state-level PUA claims over total unemployed individuals as our

[34]Following the methodology of Callaway et al. (2024).

treatment variable[35] Accordingly, we consider the following regression specifications:

$$Y_{ist} = \lambda_t \; + \sum_{r \in \mathcal{R}} \alpha_r \big(S_{st} D_{rt} \big) \; + \; \beta' X_{it} \; + \; \varepsilon_{ist}, \tag{2}$$
$$\mathcal{R} = \{\text{PUA, PUA+FPUC, Phase Out}\}.$$

where D_{rt} is an indicator for Regime $\mathcal{R} \in \{\text{PUA, PUA+FPUC, Phase Out}\}$ in quarter t and the variable S_{st} is the quarterly average of the PUA claims as a fraction of the unemployed population for state s in quarter t. Results of this specification appear in column (1) of Table 5. While the PUA coefficient is statistically insignificant, we nevertheless interpret the impact of FPUC on reservation wages as the difference between the coefficient on PUA+FPUC and the coefficient on PUA alone. In this case, the increase in benefits of \$300 per week is associated with a 7.6% rise in reservation wages.

Our specification requires that other variables are not correlated with the intensity of treatment or its timing across states. To address these concerns, we include the following state by quarter variables as controls: the quarterly state-level unemployment rate and the quarterly average of state-level weekly Covid-19 deaths per 100,000 individuals. The second column of Table 5 shows that these results are robust to the introduction of Covid-related state-level controls.[36]

$$Y_{ist} = \lambda_t + \sum_{r \in \mathcal{R}} \alpha_r \big(S_{st} \cdot D_{rt} \big) + \delta Covid_{st} + \gamma Urate_{st} + \beta' X_{it} + \varepsilon_{ist} \tag{3}$$

Results of this specification appear in column (2) of Table 5. As an additional robustness check, we confirm that these results are not coming from unmodeled time variation in the relationship between reservation wages and Covid/economic environment of the state. We do so by interacting our Covid and labor market controls with quarterly dummies, thus

[35] This is to reflect that the share of potential PUA claimants and collectors within a quarter may be closer to the current stock of unemployed in the quarter and less related to the flow of only newly unemployed claimants as would be the case if we used the PUA claim share. We report results using alternative measures of quarterly PUA intensity in Appendix A.4.1. Our alternative specifications include quarterly measures of PUA claims as a share of total (Regular UI + PUA) claims (our baseline measure in Figure 6), PUA claims as a share of denied claims, PUA initial claims as a share of unemployment, and initial PUA claims as a share of total (Regular UI + PUA) initial claims. We arrive at estimates of similar magnitude.

[36] Figures in Appendix A.5 plot the PUA claim share and the respective means of the quarterly average of weekly Covid deaths per 100k in our sample by "treated" and "control" group states. The control states experienced remarkably similar Covid death rates and peak unemployment rates as the treatment states.

controlling for within-quarter, across-state variation in Covid severity and labor market tightness, as follows:

$$Y_{ist} = \lambda_t + \sum_{r \in \mathcal{R}} \alpha_r \big(S_{st} \cdot D_{rt}\big) + \delta_t Covid_{st} * qtr_t + \gamma_t Urate_{st} * qtr_t + \beta' X_{it} + \varepsilon_{ist}, \tag{4}$$

where all these variables are as defined in the previous section. We report the results of this specification in column (3) of Table 5. Similar to our event study and previous specifications, the coefficient on PUA is smaller (the period when only PUA is active), and the coefficient PUA + FPUC is larger and statistically significant.

We next expand the control set further to include time-interacted measures of Oxford stringency, Oxford workplace-closing restrictions, and state-level regular UI monetary-eligibility thresholds in 2020. These observables account for some, but far from all, of the cross-state dispersion in PUA intensity: in a joint state-level balance regression they explain less than half of the variation in baseline PUA utilization. Appendix A.4.2 shows that the main elasticity estimates are essentially unchanged once these additional controls are added. In the fully controlled specification, the elasticity remains about 0.11, and using an alternative average-quarter threshold construction yields nearly identical results.

The magnitude of this last coefficient for α_r during PUA + FPUC more readily lends itself to quantifying the effect of an increase in benefits on reservation wages. In this specification, moving from a PUA claim share of 0 to 1 is associated with an α_r increase in reservation wage. We can interpret the *change* in α_r from the PUA period to the PUA + FPUC period as the effect on the reservation wage from the increase in benefits from FPUC, assuming PUA intensity is fixed at 1 and thus comparing a collector to a non-collector. In our search model, we estimate the increase in the wage premium of UI collectors vs non-collectors going from regular UI benefits to UI benefits plus the FPUC supplement. The analog to this measure in our regression is this difference in α_r from the PUA period to the PUA + FPUC period. Our estimate for the change in the collector reservation wage premium with the FPUC supplemental benefit is an increase of 7.6% to 12.3%.

We can also use this specification to calculate the elasticity of the reservation wage with respect to UI benefits if we hold PUA intensity constant at some average value of PUA intensity, $\bar{S}$. If we assume monetary denied individuals received the minimum disaster unemployment assistance payment, their average weekly benefit in our sample is \$176 and

Table 5: PUA/Total Unemp (State, Qtly)

	$\ln(\tilde{w})$	$\ln(\tilde{w})$	$\ln(\tilde{w})$
PUA	0.023	0.029	−0.008
	(0.039)	(0.035)	(0.037)
PUA + FPUC	0.099***	0.108***	0.116**
	(0.030)	(0.029)	(0.043)
Phase Out	0.014	0.011	−0.015
	(0.042)	(0.041)	(0.037)
Difference	0.076**	0.080**	0.123***
	(0.038)	(0.037)	(0.045)
Elasticity	0.067**	0.070**	0.108***
	(0.033)	(0.032)	(0.039)
Qtly Controls	No	Yes	No
Qtly Controls x Time	No	No	Yes
Adj. R-squared	0.761	0.762	0.763
Observations	33,868	33,868	33,868

Notes: PUA corresponds to the coefficient α_r for 2020 Q3, Q4 since data is not available for 2020 Q2. PUA + FPUC corresponds to α_r for 2021 Q1 and Q2 during which PUA and FPUC were active. Phase Out corresponds to α_r for 2021 Q3, during which some states phased out pandemic-era unemployment programs. All pandemic UI programs ended in September of 2021. Robust standard errors, clustered at the state level, are reported in parentheses.

increases from \$176 to \$476 weekly during the PUA + FPUC period. Our elasticity of reservation wage with respect to unemployment benefit can be calculated from the coefficients for PUA intensity interacted with the regime category, α_{PUA} and $\alpha_{PUA+FPUC}$ moving from the PUA regime to the PUA + FPUC regime, as follows:

$$\varepsilon(\bar{S}) \equiv \frac{\Delta \log \widehat{Y}_{\text{PUA,PUA+FPUC}}(\bar{S})}{\Delta \log B_{\text{PUA,PUA+FPUC}}} = \frac{(\alpha_{\text{PUA+FPUC}} - \alpha_{\text{PUA}})\,\bar{S}}{\log B_{\text{PUA+FPUC}} - \log B_{\text{PUA}}} \tag{5}$$

for a representative PUA intensity level $\bar{S}$, which we take as the mean level of PUA intensity in the PUA + FPUC regime, during 2021 Q1 and 2021 Q2. This calculation, using coefficients from column 3 of Table 5 gives us the following values:

$$\varepsilon(\bar{S}) = \frac{(.116 + .008) * .871}{.995} = .108$$

Using the delta method, we can arrive at a standard error of .039.

Across the three specifications, this suggests an elasticity of reservation wage with respect to UI benefits of between .067 and .108 We arrive at a similar range of values using alternative measures of PUA intensity, reported in Tables 15 and 16 of Appendix A.4.1. Appendix B.1 reports a complementary check using CPS rotation data: comparing re-employment wages for UI-eligible vs. ineligible workers yields a 9 percent eligible wage premium during the Covid period, consistent with the elasticity range above.

4 Empirical evidence from CPS Data

The results from the previous section provide evidence that the pool of UI claimants changed during the Covid-19 relief period. Because of the systematic nature of the sampling procedure underlying BAM data, it is challenging to identify whether this shift reflects relaxed eligibility criteria, a change in the propensity to claim benefits, or both. We show below that rotation data from the CPS can be used to construct a measure of UI eligibility, as well as a measure of UI benefit collection. We use these measures to study how the take-up rate of UI benefits changed in response to modifications to the unemployment insurance system during the Covid-19 relief period.

Before discussing how we construct these measures, we briefly review the structure of the CPS survey, outlined in Table 6. As is well known, individuals who take part of the CPS survey are interviewed 8 times over a 16-month period: data are gathered in the first 4 consecutive months (the first rotation), followed by an 8-month hiatus, and a further 4 months of interviews (the second rotation). While basic labor market data (e.g. labor market status) are collected at all 8 interviews, monthly earnings are only measured for the outgoing rotation, i.e. at interviews 4 and 8.

Meanwhile, retrospective income data are collected from all individuals whose rotation includes the month of March, known as the March supplement or the Annual Social and Economic Supplement (ASEC). For example, an individual interviewed in March 2020 is asked detailed questions about income received during calendar year 2019. We use these retrospective earnings data to impute monetary eligibility and potential benefit amounts

Table 6: Structure of the CPS Survey

D	J	F	M	A	M	J	J	A	S	O	N	D
			1	2	3	4						
		1	2	3	4							
	1	2	3	4								
1	2	3	4									5
			5	6	7	8						
		5	6	7	8							
	5	6	7	8								
5	6	7	8									

Notes: Numbers represent CPS interview number. The top represents the first rotation, the bottom the second rotation. Red numbers represent interviews where current earnings are reported, whereas blue numbers are March ASEC interviews where past income is reported.

for unemployment spells occurring in months proximate to the March interview.[37] We also use the March ASEC to measure whether an individual received any unemployment compensation during the previous calendar year. Combining eligibility information constructed from the first rotation with reported UI income from the subsequent March ASEC in the second rotation, we construct an annual take-up indicator for individuals observed in their first rotation.[38]

4.1 Measuring UI Benefits Eligibility

Regular Unemployment Benefits To identify eligible individuals in CPS data, we first use individuals' employment status. Through a series of questions, civilians are classified as employed, unemployed, or not in the labor force.[39] The BLS distinguishes between two

[37] We use a generalized version of the "UI-Calculator" from Ganong et al. (2020) to impute both eligibility and the amount of benefits an individual would receive given their earnings history and state rules.

[38] Because ASEC UI income covers the full calendar year, some benefits may come from spells not observed in the first rotation. This affects the precision of the take-up measure but should not bias our comparison across years, especially given the large increase in 2020–21.

[39] Individuals are deemed unemployed if they did no work for pay or profit, did not have a job from which they were briefly absent, and answered yes to a question about whether they had been looking for work in

types of unemployed individuals: experienced and new workers. We also use individuals' self-reported reasons for being unemployed, distinguishing among those who lost jobs (due to temporary layoff, involuntary job loss, or the end of a temporary job), those who quit, those re-entering the labor force (re-entrants), and those seeking their first jobs (new entrants). Only experienced unemployed workers who have lost a job can qualify for unemployment benefits: those who quit, re-entrants, and new entrants are ineligible. In addition, any individual who has been unemployed for more than 26 weeks is deemed ineligible. Accordingly, we define experienced unemployed workers who lost a job and have not exhausted benefits as *non-monetary eligible* individuals.

The BLS does not elicit whether individuals meet the monetary eligibility requirements of unemployment insurance. We use information from the March Supplement to simulate filing for unemployment benefits, using a generalized version of the methodology outlined in Ganong et al. (2020).[40] While this methodology gives us a sense of the benefits a qualifying unemployed individual is entitled to, we primarily use the results of this exercise to evaluate monetary eligibility at the extensive margin—that is, whether simulated benefits are strictly positive or zero.[41] Because we only observe annual income in the March Supplement for the previous calendar year, we assume that monetary eligibility applies uniformly to all months surrounding the March interview for each rotation group, as shown in Table 6.

Covid-19 Relief Period As discussed in Section 2, the CARES Act expanded benefit amounts through FPUC, extended the duration of regular unemployment benefits through PEUC, and relaxed eligibility through PUA.

We assume that all individuals whom we deem eligible (more on this below) for unemployment benefits from April 2020 until July 2020 were entitled to an additional \$600 of weekly benefits. Similarly, eligible individuals were entitled to \$300 in extra weekly benefits from

the past four weeks.

[40]State laws are available on a bi-annual basis since 1965 and sporadically since 1940 at https://oui.doleta.gov/unemploy/statelaws.asp#RecentSigProLaws. Since our earnings data pertains to the previous calendar year, we use the rules in place in January of each year.

[41]Since ASEC only elicits total pre-tax wage and salary income for the previous calendar year, we assume, as did Ganong et al. (2020), the most generous distribution of income over the past 4 quarters by first attributing weeks worked to the last quarter (Q4), then the second last quarter (Q3), and so on, until reported weeks worked are exhausted. Since most states use the most recent quarters to determine eligibility and benefits, backloading earnings over the calendar year gives an upper bound to benefits and is the most generous assumption for monetary eligibility. There is no way to test the implications of this assumption using CPS data, though in principle other sources of data could be used to do so.

January 2021 until the state-specific date when the program expired.[42] For states which let the program run its course as scheduled until September 5, we assume that individuals kept receiving the extra $300 through the month of August.

Recall that to be eligible under the Pandemic Unemployment Assistance (PUA) program, an individual who was ineligible for regular benefits needed to certify that they were either unemployed or unable to work because of Covid-related circumstances. To accommodate this notion of non-monetary eligibility under the PUA program, we use two questions that the CPS added in May 2020 asking whether one was 'unable to work due to Covid-19 pandemic', and whether one was 'prevented from looking for work due to Covid-19.' If an unemployed individual answered yes to either of these questions during a particular month, we consider that individual non-monetary eligible for that month during the period the PUA program was in effect. In addition, the PUA program extended potential benefit duration beyond the usual 26 weeks. In practice, enforcing these duration limits was challenging during the relief period, given the volume and complexity of pandemic UI programs and the need for states to rapidly modify legacy systems. Combined with the fact that the CPS records weeks unemployed but not weeks of UI receipt, we treat duration limits as effectively non-binding in our CPS-based eligibility measure and do not attempt to track exhaustion at the individual level during the PUA period.[43] Figure 7 shows the increase in non-monetary eligibility that resulted from this PUA program relative to the regular rules governing non-monetary eligibility.[44] The apparent spike in regular non-monetary eligibility at the onset of Covid reflects a compositional shift in the unemployed stock—a surge in layoffs, who satisfy the standard non-monetary rules—rather than a change in those rules.

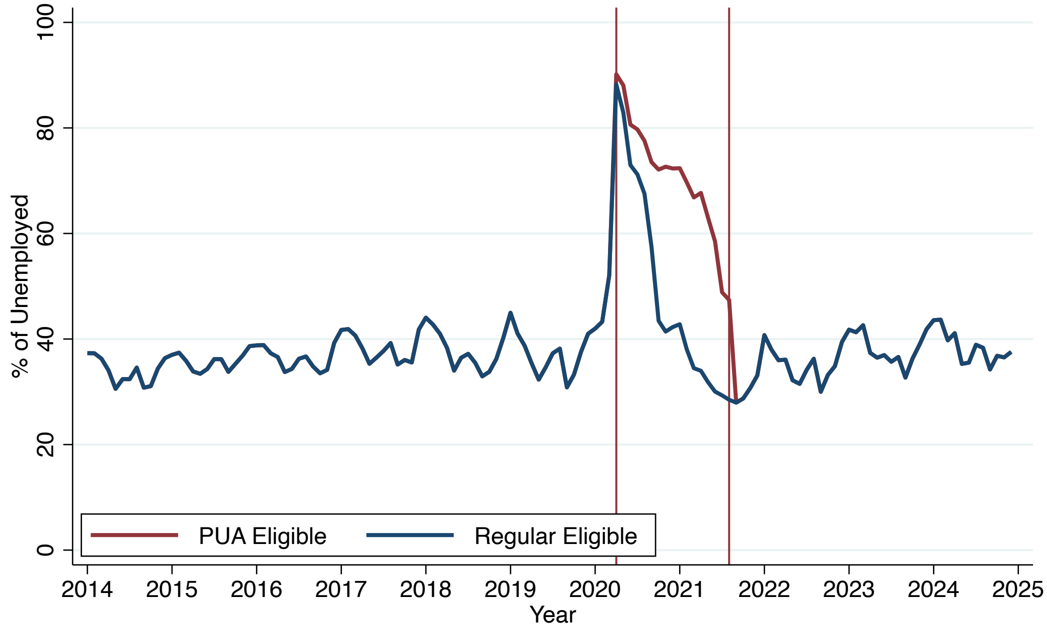
In terms of monetary eligibility, PUA broadened the definition of qualifying income (typically earnings) used to determine the benefit amount to include income of self-employed workers (including gig economy workers and independent contractors). In principle, unem-

[42] At the monthly frequency, we deem the entire month a month of extra benefits if the program ended after the 15th of the month. For example, Iowa ended the program on June 12 so we assume that no extra benefits were extended to unemployed people from that state in June, while we assume that people from Indiana, which ended the program on June 19, received an extra $300 a week throughout the month of June.

[43] For contemporaneous evidence that state UI systems were strained and that determining eligibility and preventing improper payments were major challenges during the pandemic, see the U.S. Government Accountability Office reports GAO-22-104251 (June 2022) at https://www.gao.gov/products/gao-22-104251, and GAO-22-105162 (2022) at https://www.gao.gov/products/gao-22-105162.

[44] As noted before, PUA was in principle payable retroactively to eligible individuals for weeks beginning on or after January 27, 2020. However, few people qualified back to February, and our Covid-related questions only start in the May 2020 CPS.

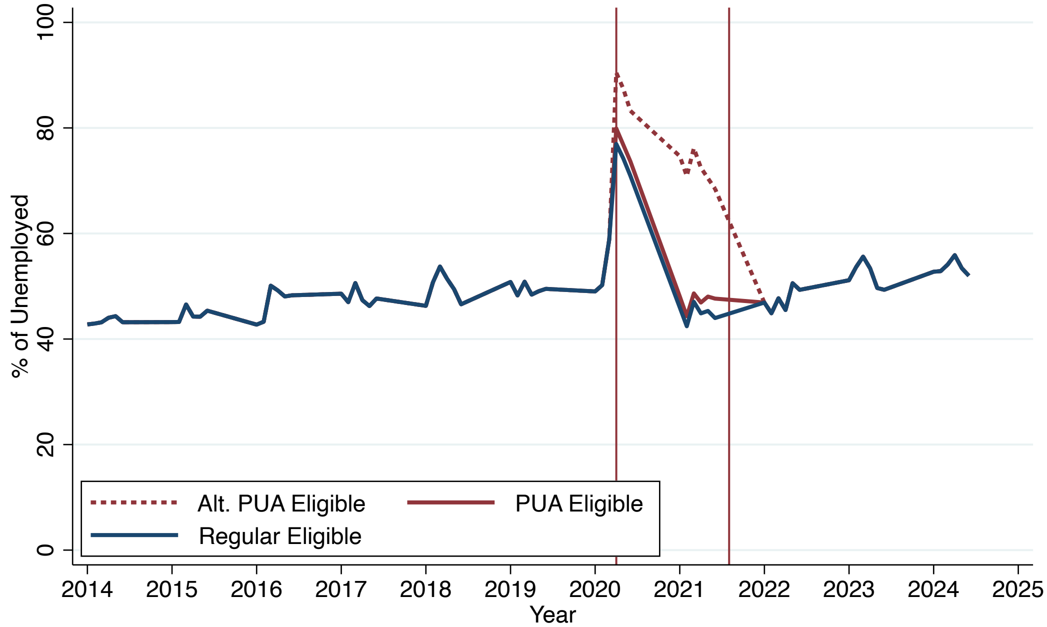
Figure 7: Non-Monetary Eligibility: regular vs PUA



Notes: Regular eligibility refers to the typical rules for non-monetary eligibility. PUA eligibility refers to non-monetary eligibility under the PUA program.

ployed workers had to provide proof (e.g., pay stubs, income tax return, bank statements, offer letter) to document employment or self-employment that was impacted by Covid-19 or to document work that would have begun on or after the date when Covid-19 impacted their employment status. However, providing evidence that some kind of work was interrupted in any way by Covid-19 was essentially sufficient for monetary eligibility, and a minimum unemployment compensation equal to 50 percent of the average payment of regular unemployment compensation in an individual's state (ranging from \$106 in Mississippi to \$267 in Massachusetts) was guaranteed regardless of income. Accordingly, we compute two measures of monetary eligibility. The first measure assumes that state rules for monetary eligibility apply, but using a broader measure of income that includes self-employment income in addition to earnings. The second alternative measure assumes that any positive income (earnings plus self-employment income) in the previous year is sufficient to satisfy monetary eligibility: the idea is that having any income in the previous year shows some degree of attachment to the labor force. Figure 8 shows how PUA measures of monetary eligibility compare to those for regular unemployment benefits.

Figure 8: Monetary Eligibility: regular vs PUA



Notes: Regular eligibility refers to the typical rules for monetary eligibility. PUA eligibility refers to monetary eligibility under the PUA program, assuming that state rules apply to earnings plus self-employment income. The alternative measure of monetary eligibility assumes that any positive past earnings or self-employment income qualifies.

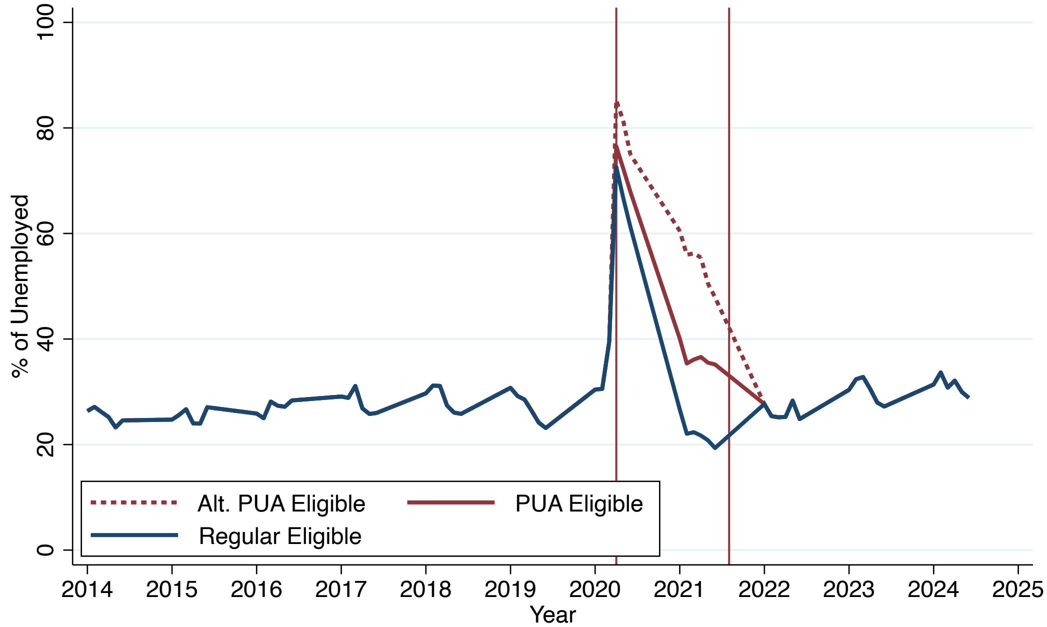
Combining non-monetary and either measure of monetary eligibility yields two series for overall UI eligibility, which are depicted in Figure 9. While there are differences across the two measures during the relief period, both indicate a sizable expansion in eligibility.

4.2 Take-up Rate

To measure the take-up rate, we need a measure of eligible unemployed individuals who successfully filed a claim for unemployment benefits. The BLS offers an indirect way to measure a take-up rate. In the March Supplement, respondents are asked how much income (if any) they received from unemployment compensation during the previous calendar year.[45] We use the answer to this question to impute whether an individual collected benefits the

[45] The amount reported can emanate from state or federal unemployment compensation, but also from Supplemental Unemployment Benefits (SUB), union unemployment, or strike benefits. Each component cannot be identified separately.

Figure 9: UI Eligibility: regular vs PUA



Notes: Regular eligibility refers to the typical rules for eligibility. PUA eligibility refers to eligibility under the PUA program, including non-monetary eligibility (as in Figure 7) and both measures of monetary eligibility (as in Figure 8).

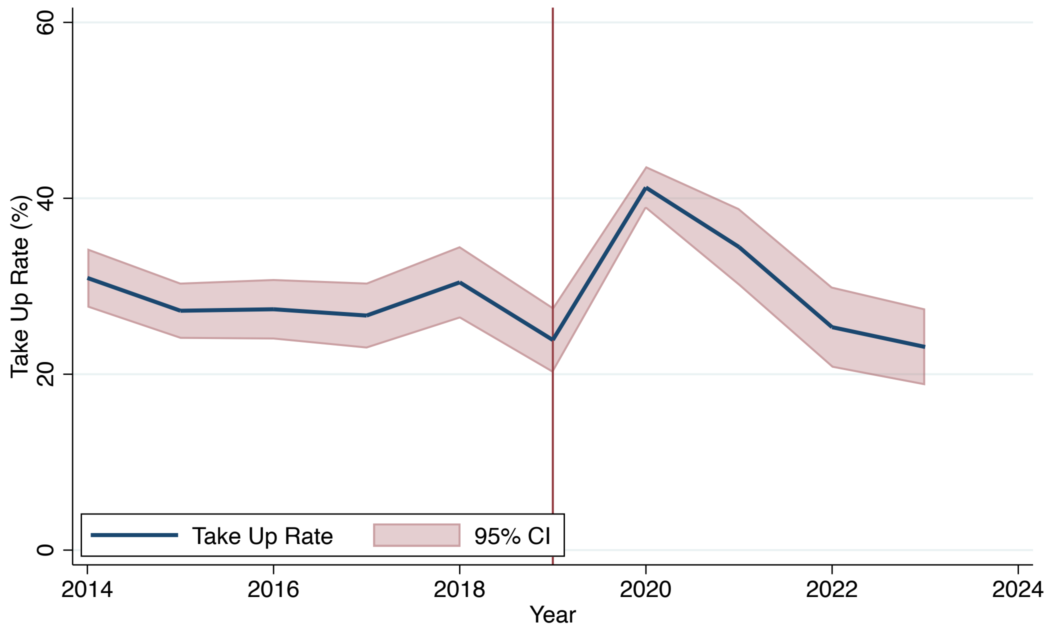
previous year.[46] Evidently, this measure of benefit collection is only available for individuals whose interview rotation spans March: we use the answer to this question in the second rotation to impute collection in the first rotation (see Table 6).

The take-up rate displayed in Figure 10 uses our measure of whether an individual collected UI benefits last year together with our measure of UI eligibility for regular unemployment benefits discussed above.[47] Interestingly, the take-up rate increased from around 27% prior to 2020 to 41% in 2020 and 34% in 2021. Surprisingly, the take-up rate among individuals whom we deem eligible under either of our less stringent measures of monetary eligibility is quite similar to that seen in Figure 10: under our (least stringent) second alternative measure of eligibility, the take-up rate is also 41% in 2020 and only slightly higher (35%) in 2021.

[46] Note that the exact month(s) during which an individual collected benefits cannot be identified.

[47] We report the take-up rate at the annual frequency as our measure of collection refers to the entire previous calendar year, making any monthly variation misleading.

Figure 10: UI Take-up Rate: regular program



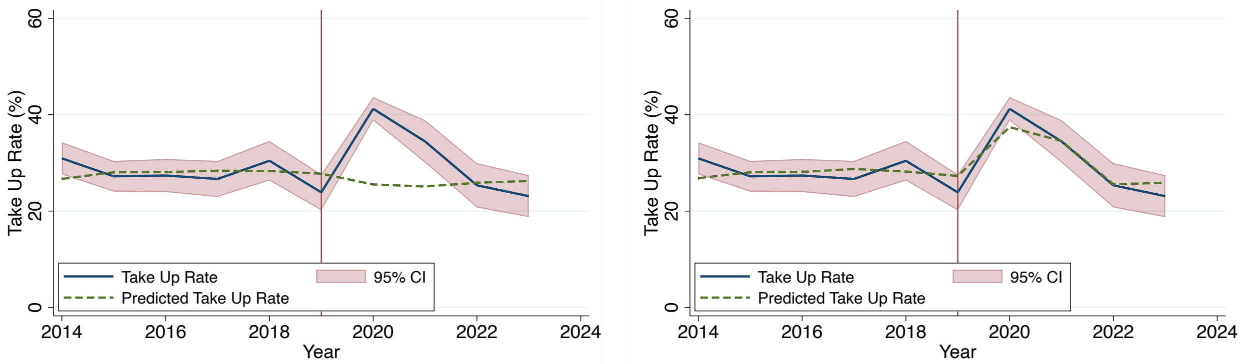
Notes: The take-up rate is the fraction of individuals who are eligible to receive regular UI benefits who report having received UI benefits the previous year.

Many factors could explain the rise in the take-up rate in 2020 and 2021 among individuals who qualify for regular unemployment benefits. As we mentioned in Section 3.3, the set of eligible individuals could have a different composition of characteristics or earnings history that influences propensity to collect benefits, and the increase in benefit amounts could also affect the propensity to claim benefits. We use a Probit regression to estimate the propensity for individuals to claim benefits in normal times (2014–2019) as a function of characteristics (age, sex, education, race, occupation, and industry), state fixed effects, past earnings, and specifications both with and without benefit amounts.[48] Our baseline specification is:

$$\Pr(Collect_i = 1) = \Phi\left(\beta X_i + \theta \log(Earnings_i) + \psi \log(Benefit_i) + \gamma_s + \varepsilon_i\right),$$

[48]The Probit is estimated on the same rotation-linked sample used to construct the take-up rate—i.e., individuals classified as eligible in the first rotation and observed in the subsequent March ASEC in the second rotation. The unit of observation is the individual–calendar-year: for each person-year, Collect_i records whether the respondent received any UI income during that calendar year, and all covariates (age, sex, education, race, occupation, industry) are constructed from the same CPS respondent in that year. Past-year earnings and the simulated benefit amount Benefit_i are computed from the earnings history underlying Collect_i.

Figure 11: Actual and Predicted Take-up Rate



(a) Without benefits as a regressor

(b) With benefits as a regressor

Notes: The take-up rate is the fraction of individuals who are eligible to receive regular UI benefits who subsequently report having received UI benefits. The predicted take-up rate uses the coefficient of a Probit regression estimated on 2014–2019 data to predict the take-up rate out of sample for 2020 and 2021. In panel (a), benefits are not included as a regressor; in panel (b), they are.

where γ_s is a state fixed effect. We use the coefficients from that regression to predict the take-up rate from 2020 through 2023. The results, displayed in the two panels of Figure 11, show that the predicted take-up rate closely follows the actual take-up rate only when benefit amounts, which include the \$600/\$300 supplements when applicable, are included as a regressor.[49]

These results indicate that the decision to file for and claim unemployment benefits tracks the amount one expects to receive once the claim is approved.[50] By contrast, shifts in the composition of eligible workers play a limited role in explaining the 2020–2021 increase. The compositional change in unemployed workers that is evident in the pandemic and reflected in our analysis of BAM is not itself the driver of increased take-up. Rather, larger benefits appear to have drawn additional workers into claiming and collecting UI. This motivates modeling UI take-up as an endogenous margin that responds to benefit generosity, as we do in Section 5.

[49] We also estimate an alternative specification where benefits are measured only as regular benefits—i.e., without FPUC. The predicted take-up rate looks identical to the specification without benefits shown in panel (a) of Figure 11. See Figure 30 in Appendix B.2.

[50] We cannot separately identify the benefit-level channel from other concurrent changes to the UI system during the pandemic—reduced administrative frictions, waived work-search requirements, and changing stigma associated with claiming. For a complementary regression-kink design that isolates the benefit-level effect on take-up, see McQuillan and Moore (2025b).

Our empirical findings can be summarized as follows. The BAM analysis shows that higher expected UI benefits raise reservation wages, with a small implied elasticity. The CPS analysis documents that the Covid-era surge in UI take-up tracks higher benefit levels rather than compositional shifts among the eligible unemployed. To interpret these findings jointly, the next section introduces a simple economic environment with directed search and an endogenous take-up decision. We use this framework to explore how both wages and take-up respond to changes in UI benefits.

5 Economic Environment

The empirical evidence discussed above suggests that individuals transitioning from unemployment are only weakly influenced by the level of unemployment benefits they receive, as reservation wages respond only modestly to benefit amounts. The evidence also highlights the decision to file for unemployment benefits as an important margin of adjustment. To interpret these findings, we describe a parsimonious economic environment with directed search, featuring an endogenous unemployment benefit take-up decision inspired by Auray et al. (2019). We then extend the model to include on-the-job (OTJ) search, and—once calibrated—show it can replicate both the modest wage pass-through from UI benefits and the quantitatively important role of the take-up margin.

5.1 Directed Search with Endogenous UI Take-Up

Agents and Markets

There is a continuum of infinitely-lived workers, and a continuum of firms with positive measure. Workers' per period utility function $v : \mathbb{R} \to \mathbb{R}$ is twice continuously differentiable, strictly increasing, and weakly concave, with derivative $v'(\cdot) \in [\underline{v}', \bar{v}']$, where $0 < \underline{v}' \leq \bar{v}'$. For illustration purposes and to avoid unnecessary algebraic complexity, we assume $v(c) = c$. We relax this assumption in the calibrated version of the model. Both workers and firms have a common discount factor $\beta \in (0, 1)$.

Workers are heterogeneous in the utility cost of filing for and collecting UI benefits, denoted $\varepsilon \in [\underline{\varepsilon}, \bar{\varepsilon}]$, which is distributed according to cumulative distribution function $F(\varepsilon)$ with density $f(\varepsilon)$.[51] If a worker chooses to collect UI benefits, he receives flow consumption b and incurs utility cost ε. If instead the worker chooses not to collect UI benefits, he receives flow consumption d, with $v(b) - \bar{\varepsilon} < v(d) < v(b) - \underline{\varepsilon} < v(b)$.

The labor market is organized as a continuum of submarkets, each indexed by a pair (θ, w), where θ denotes market tightness and w the wage offered to a worker upon matching in that submarket. A worker encounters a vacancy with probability $p(\theta)$, where $p : \mathbb{R}_+ \to [0,1]$ is a twice continuously differentiable, strictly increasing, and strictly concave function satisfying $p(0) = 0$ and $p'(0) < \infty$. Similarly, a vacancy meets a worker with probability $q(\theta)$, where $q : \mathbb{R}_+ \to [0,1]$ is twice continuously differentiable, strictly decreasing, and convex, with $q(\theta) = p(\theta)/\theta$, $q(0) = 1$, and $q'(0) < 0$. In addition, $p(q^{-1}(\cdot))$ is concave. Workers matched in submarket (θ, w) produce output y, earn constant wage w, and are subject to exogenous separation with probability $\delta \in (0,1)$.

Firms choose how many vacancies to create and in which submarkets to locate them. Maintaining a vacancy for one period entails a cost $k > 0$. Both workers and firms take the market tightness and wage (θ, w) in each submarket as given.

Firms

Let $J(w)$ denote the value to a firm of being matched with a worker to whom it pays wage w. To create such a match, a firm must post a vacancy at cost k and faces a probability $q(\theta)$ of filling the vacancy. We assume free entry, which implies that value of a vacancy is zero in equilibrium, hence

$$J(w)\, q(\theta) = k,$$

where

$$J(w) = \frac{y - w}{1 - \beta(1-\delta)}.$$

Combining these two equations yields the free entry condition

$$w = y - (1 - \beta(1-\delta)) \frac{k}{q(\theta)}. \tag{6}$$

[51] As discussed in Section 2, maintaining UI benefit eligibility requires satisfying several conditions throughout an unemployment spell. Our tabulations from the May 2018 CPS Job Search Supplement show that take-up decisions are shaped by a variety of factors, including anticipated re-employment, eligibility constraints, administrative and informational frictions, and the perceived value of benefits. The cost of filing/collecting UI benefits captures these factors in a parsimonious fashion.

Recall that $q(\theta)$ is decreasing in θ. Therefore, submarkets with higher market tightness are associated with lower wages. Equation (6) thus highlights the key trade-off workers face when searching: higher tightness implies a greater chance of meeting a firm but at the cost of a lower wage.

Workers

Employed workers. Let $W(w, \varepsilon)$ be the value of being employed at wage w for workers of type ε. Similarly, let $U(\varepsilon)$ denote the value of being unemployed for workers of type ε. Then

$$W(w, \varepsilon) = w + \beta(\delta U(\varepsilon) + (1 - \delta)W(w, \varepsilon))$$

and therefore

$$W(w, \varepsilon) = \frac{w + \beta\delta U(\varepsilon)}{1 - \beta(1 - \delta)} \tag{7}$$

Unemployed workers. Unemployed workers must choose which submarket to target in their job search. Let $R(U(\varepsilon))$ denote the return to search for an unemployed worker of type ε, with continuation value $U(\varepsilon)$. The worker chooses the submarket (θ, w) that maximizes this return, taking as given the trade-off highlighted by equation (6):

$$R(U(\varepsilon)) \equiv \max_{\theta, w} p(\theta)(W(w, \varepsilon) - U(\varepsilon))$$

subject to

$$w = y - (1 - \beta(1 - \delta)) \frac{k}{q(\theta)}.$$

In other words, the return to search $R(\cdot)$ is simply the highest expected gain a worker can hope to achieve by optimally choosing which submarket to search in. Substituting the expression for $W(w, \varepsilon)$ from equation (7), we can rewrite this problem as

$$R(U(\varepsilon)) \equiv \max_{\theta, w} p(\theta) \left(\frac{w + \beta\delta U(\varepsilon)}{1 - \beta(1 - \delta)} - U(\varepsilon) \right) \tag{8}$$

subject to

$$w = y - (1 - \beta(1 - \delta)) \frac{k}{q(\theta)}.$$

The envelope condition implies that

$$R'\left(U(\varepsilon)\right)=\frac{-p\left(\theta\right)\left(1-\beta\right)}{1-\beta\left(1-\delta\right)}, \tag{9}$$

indicating that the function R is monotonically decreasing in the value of unemployment $U(\varepsilon)$. Moreover, the first order condition—after substituting out w—implies that

$$p'\left(\theta\right)\frac{y-\left(1-\beta\right)U(\varepsilon)}{1-\beta\left(1-\delta\right)}=k.$$

The concavity and monotonicity of the function $p(\cdot)$ imply that the optimal θ is decreasing in the value of unemployment. In other words, workers who enjoy a higher value of unemployment are more selective: they search in slacker submarkets that offer higher wages but lower probabilities of finding a job.

Decision to Collect UI Benefit

Unemployed workers must decide whether to collect UI benefits or not. If the worker chooses not to collect, his lifetime utility is given by U^N, which consists of the utility from consuming d plus the value of being unemployed and searching next period:

$$U^N=d+\beta\left\{U^N+R\left(U^N\right)\right\}. \tag{10}$$

Since ε is constant over the course of an unemployment spell, a worker who decides not to collect benefits at the beginning of the spell will continue not to do so throughout. Also note that the value of unemployment for non-collectors, U^N, does not depend on the level of UI benefit b.

If the worker chooses to collect UI benefits, lifetime utility is given by $U^C\left(\varepsilon\right)$, which consists of the utility from consuming b, incurring utility cost ε, and the continuation value of being unemployed and searching in the next period:

$$U^C\left(\varepsilon\right)=b-\varepsilon+\beta\left\{U^C\left(\varepsilon\right)+R\left(U^C\left(\varepsilon\right)\right)\right\}. \tag{11}$$

Equation (11), together with the monotonicity of R (as shown in equation (9)), implies that $U^C(\varepsilon)$ is monotonically decreasing in ε. This, in turn, implies that there exists a unique cutoff ε^*, such that $U^C(\varepsilon^*) = U^N$. In other words, worker with collection cost ε^* is indifferent between collecting and not collecting. Using Bellman equations (10) and (11) we can find the unique cutoff as $\varepsilon^* = b - d$. All types with $\varepsilon \leq \varepsilon^*$ choose to collect UI benefits, while those with $\varepsilon > \varepsilon^*$ choose not to collect.

Let $U(\varepsilon) \equiv \max\left\{U^N, U^C(\varepsilon)\right\}$ denote the lifetime utility of an unemployed worker with UI benefit collection cost ε. Differentiating equation (11) with respect to b, we obtain:

$$\frac{\partial U(\varepsilon)}{\partial b} = \frac{1}{1 - \beta - \beta R'(U(\varepsilon))}$$

Since $R' < 0$, it follows that $\frac{\partial U(\varepsilon)}{\partial b} > 0$. In other words, the value of unemployment for all collector types (i.e., $\varepsilon \leq \varepsilon^*$) is increasing in the level of unemployment benefits b. These results immediately imply the following proposition:

Proposition 1 *The equilibrium has the following properties:*

1. *Wages are decreasing and market tightness is increasing in the collection cost ε; non-collectors have the lowest wages and highest market tightness.*
2. *The wage of non-collectors is unaffected by the level of UI benefits.*
3. *For each collector type $\varepsilon \leq \varepsilon^* = b - d$, the wage increases as the level of UI benefits increases.*

5.1.1 Contribution of Collection Margin to Wage Response

In this model, the wages of non-collectors are unaffected by changes in the level of unemployment benefits. Therefore, to understand the response of the collectors' wage premium to a change in b, it is sufficient to examine how the average wage of collectors changes with the UI benefit level b.

To compute the average wage of collectors, we first determine the stationary measure of employed workers with collection cost $\varepsilon \leq b - d$. Let $x^e(\varepsilon)$ denote the stationary measure of employed workers with collection cost ε, and let $x^u(\varepsilon)$ denote the stationary measure of unemployed workers of the same type. Let $p(\varepsilon)$ denote the equilibrium job finding probability for type ε. The stationary measures $x^e(\varepsilon)$ and $x^u(\varepsilon)$ must satisfy:

$$\begin{aligned} x^e(\varepsilon) + x^u(\varepsilon) &= f(\varepsilon) \\ (1-\delta)x^e(\epsilon) + p(\varepsilon)x^u(\varepsilon) &= x^e(\epsilon) \\ \delta x^e(\epsilon) + (1-p(\varepsilon))x^u(\varepsilon) &= x^u(\epsilon) \end{aligned}$$

These equations imply that:

$$x^e(\varepsilon) = \frac{p(\varepsilon)}{p(\varepsilon)+\delta} f(\varepsilon).$$

The average wage of collectors is then given by:

$$w^C = \frac{\int_0^{\varepsilon^*} x^e(\varepsilon)\, w(\varepsilon)\, d\varepsilon}{\int_0^{\varepsilon^*} x^e(\varepsilon)\, d\varepsilon}.$$

To examine how the average wage of collectors responds to changes in UI benefits, we differentiate w^C with respect to b:

$$\frac{dw^C}{db} = \underbrace{\frac{\int_0^{\varepsilon^*} x^e(\varepsilon) \frac{dw(\varepsilon)}{db} d\varepsilon}{\int_0^{\varepsilon^*} x^e(\varepsilon)\, d\varepsilon}}_{\substack{\text{direct effect on wages of} \\ \text{existing collectors } (>0)}} + \underbrace{\frac{\int_0^{\varepsilon^*} \frac{dx^e(\varepsilon)}{db} \left(w(\varepsilon) - w^C\right) d\varepsilon}{\int_0^{\varepsilon^*} x^e(\varepsilon)\, d\varepsilon}}_{\substack{\text{effect on JFR among} \\ \text{collectors } (<0)}} + \underbrace{\frac{x^e(\varepsilon^*) \left(w(\varepsilon^*) - w^C\right)}{\int_0^{\varepsilon^*} x^e(\varepsilon)\, d\varepsilon}}_{\substack{\text{change in composition} \\ \text{of collectors } (<0)}} \tag{12}$$

The first term captures the direct impact of UI benefits on wages. As shown above, the wages of all collectors increase in response to a rise in b, so this term is always positive. Its magnitude depends on the parameters of the matching function and the job separation rate.

The second term reflects the impact of UI benefits on job-finding rates. The sign of this term cannot be determined without additional assumptions. However, if the job-finding rates decline more for workers with the lowest collection costs—those who are most sensitive to changes in b, then an increase in UI benefits tilts the distribution of employed collector types toward those with higher collection costs. These individuals earn lower wages, so this compositional shift dampens the response of average wages. [52]

The third term arises from the shift in the collection threshold. As UI benefits increase, the

[52] In Appendix C we provide a sufficient condition to deliver this.

marginal type ε^* finds it optimal to start collecting. This expands the pool of collector types within the employed stock. In particular, the increase in UI benefits brings in new types who previously chose not to collect. These marginal collectors have the highest collection costs within the pool and, therefore, the lowest wages.[53] The addition of this new mass of low-wage workers further dampens the effect of higher UI benefits on average wages.

Note that the first two terms are present even in a model with exogenous collection, i.e., when ε^* is given exogenously. The contribution of endogenous collection arises solely from the third term. To quantify the importance of this effect, we require a version of the model that can be calibrated.

In the next subsection, we extend the model along several dimensions. First, we introduce on-the-job search. In our empirical analysis, we use either the reservation wage or the first wage observed after an unemployment spell. A model with on-the-job search is a natural extension, as it allows us to distinguish between average wages in the economy and the wages of first-time job finders (following unemployment).

This extension, however, introduces several complications when combined with permanent collection-cost heterogeneity. To address this, we assume that collection costs are redrawn from the same distribution each time a worker becomes unemployed. If a worker finds a match and becomes employed, they will draw a new collection cost upon entering unemployment again.

Finally, we relax the assumption of linear utility in order to introduce a meaningful role for unemployment insurance.

5.2 Quantitative Model

The quantitative model extends the framework in Section 5.1 to incorporate additional features that allow a closer alignment with the data. These extensions are minimal but essential: they allow the model to match observed data and simulate the effects of temporary changes in UI policy.

First, we introduce on-the-job (OTJ) search along the lines of Menzio and Shi (2010) and

[53]This link between high collection costs and low wages is an equilibrium property of the model. McQuillan and Moore (2025a) find that informational letters reducing UI application costs disproportionately raise take-up among lower-wage workers, consistent with the model.

Gervais et al. (2022). In the data, we observe wages at the time workers transition from unemployment to employment, not the average wage across employment spells. To reflect this distinction in the model, we allow employed workers to receive offers while on the job. With probability λ_e, an employed worker receives an opportunity to search and, if matched, can transition to a new job.

Second, we allow for stochastic UI eligibility. At the start of every employment spell, workers are not eligible for UI benefits. While employed, individuals become eligible with probability φ each period. If they enter unemployment while eligible, they may lose access to benefits with probability ψ each period, capturing benefit expiration or administrative exit from the program. These eligibility transitions are exogenous and independent of the worker's collection decision.

Third, we modify the treatment of collection costs. Rather than treating ε as a fixed individual type, we assume that each unemployed spell begins with a new draw from the distribution of collection costs. Workers who are eligible then decide whether to collect UI, as in the stylized model. This assumption simplifies the state space while preserving the key margin of endogenous take-up.[54]

Finally, we introduce curvature in the utility function by assuming $v(c) = \log(c)$, which allows UI benefits to have welfare consequences.

A full description of the extended model, including equilibrium conditions and timing, appears in Appendix D. All other aspects of the model remain unchanged from Section 5.1. In the next section, we describe how we parameterize the model to match key labor market moments.

5.3 Parameterization

We now use a parameterized version of the model to quantitatively examine how the generosity of the unemployment insurance system affects individuals' search behavior, with a focus on the wages they receive upon transitioning to employment. Table 7 summarizes the calibrated parameters and targets.

[54] When collection costs are permanent, as in the simple model of section 5.1, this cost becomes a state variable, unnecessarily complicating the firm's problem.

Table 7: Calibration results

Parameter	Description	Value	Target
β	discount factor	0.996	annual real return of 5%
δ	exog. job separation	0.015	unemployment rate of 3.8%
k	cost creating vacancy	3.02	av. job finding rate of 38%
λ_e	prob. of search on the job	0.224	share of employed actively searching
y	output of of employed worker	1	normalization
γ	matching function parameter	1.3	den Haan et al. (2000)
d	value of home production	0.2	(see text)
b	consumption of UI collectors	0.6	(see text)
ψ	UI benefit expiration rate	0	(see text)
φ	prob. of becoming UI eligible	0.1	(see text)

The instantaneous utility function is specified as $v(c) = \log(c)$, and the discount factor is set to $\beta = 0.996$, corresponding to a 5% annual interest rate (i.e., $\beta = 1/(1.05^{1/12})$). The matching technology is given by $p(\theta) = \theta(1+\theta^{\gamma})^{-1/\gamma}$, with $\gamma = 1.3$.[55] We adopt the estimate $\gamma = 1.3$ from den Haan et al. (2000), which implies a calibrated average job-filling rate of about 80%, consistent with observed monthly hire rates between 2014 and 2019 in BLS data.

The exogenous separation rate is set to $\delta = 0.015$, which yields an unemployment rate of 3.8% in steady state. The vacancy posting cost, $k = 3.02$, is calibrated to generate a job-finding probability of approximately 38% at the monthly frequency, in line with CPS data from 2015 to 2020.[56] The probability that an employed worker has the opportunity to search, λ_e, is set to 0.224—matching the share of employed workers who report actively searching in the SCE Job Search Supplement, as reported by Faberman et al. (2022).

We assume that the utility cost of filing for UI benefits is uniformly distributed: $\varepsilon \sim U[\underline{\varepsilon}, \overline{\varepsilon}]$. The lower bound $\underline{\varepsilon}$ is normalized to zero, and the upper bound $\overline{\varepsilon}$ is set so that the steady-state UI take-up rate matches our baseline estimate of 28% in the CPS for the 2014–2019 period (see Section 4.2).

We assume that all unemployed individuals receive a flow value of $d = 0.2$ from non-employment, capturing home production or other non-market activities. Unemployment

[55] The underlying matching function, as introduced by den Haan et al. (2000), is $(v^{-\gamma} + a^{-\gamma})^{-1/\gamma}$, where v and a denote the number of vacancies and applicants, respectively, and $\theta = v/a$.

[56] Computed using CPS data and the methodology of Shimer (2005).

benefits are set such that net benefits equal $b - d = 0.4$, implying a gross benefit level of $b = 0.6$. This calibration delivers an average replacement rate of 42.5 percent in the benchmark economy, where the replacement rate is calculated as ratio of net benefit $b - d$ to average wage of eligible workers. [57] Output of an employed worker is normalized to $y = 1$.

Finally, we set the probability of becoming UI-eligible, φ, to 0.1, implying that individuals become eligible to collect unemployment insurance after an average of ten months of employment. We set the probability of benefit expiration, ψ, to zero. There is no straightforward way to discipline this parameter without substantially complicating the model. A common approach in the literature is to set $\psi = 1/6$, which implies an average eligibility duration of six months. However, this assumption generates an exponential distribution of benefit durations, with an implausibly large fraction of unemployed workers losing eligibility before finding a job. This feature is counterfactual and substantially dampens the effect of UI benefits on reservation wages in our model. We therefore adopt the alternative (albeit imperfect) assumption of no benefit expiration, which gives benefit changes the greatest scope to affect reservation wages. Importantly, because we target a monthly job-finding rate of 38 percent, fewer than 9 percent of unemployed workers remain unemployed for more than six months in steady state. Moreover, the Pandemic Emergency Unemployment Compensation (PEUC) program extended benefit durations well beyond six months. In light of these considerations, we view the assumption of no benefit expiration as a reasonable compromise for our quantitative exercise.

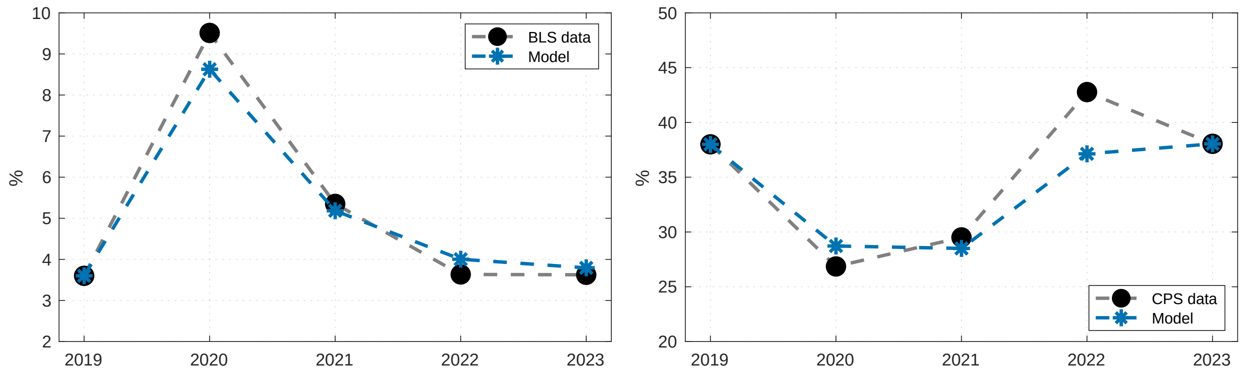
5.4 Quantitative Experiment

Using the calibrated model, we conduct the following experiment. We start the model in steady state. In period 0, we introduce an unexpected separation shock that raises the unemployment rate to 15%. This shock lasts for a single period. To capture the lax eligibility criteria early in the Covid response, we assume that all newly separated workers are eligible for UI benefits. In order to to match the observed time path of unemployment and the job-finding rate after March 2020, we assume that the cost of creating a vacancy increases by 50% in period 0 and remains elevated for 12 months. Thereafter, the cost of creating

[57] The level of d and the support of the benefit collection cost distribution $F(\varepsilon)$ are jointly determined. We fix $d = 0.2$and calibrate the support of $F(\varepsilon)$ to match the observed fraction of UI recipients. Alternative values of d would require a corresponding adjustment of $F(\varepsilon)$ to hit the same target, but have little effect on the quantitative results once the relevant moments are matched.

Figure 12: Unemployment Rate and Job Finding Rate

(a) Unemployment Rate, Model vs Data (b) Job Finding Rate, Model vs Data



Notes: Source for unemployment rate in data is BLS. Source of job finding rate in data is authors' computation based on CPS data using procedures first proposed in Shimer (2005). Monthly data are aggregated to annual by simple averaging.

a vacancy gradually declines, returning to its benchmark value by month 24. This path generates a decline in the job-finding rate and a slow recovery of the unemployment rate, in line with the observed post-shock dynamics.

Figure 12 shows how the model tracks the post-March 2020 evolution of unemployment and job-finding rates. All model and data moments are reported as annual averages, consistent with the frequency of our empirical measurements.

Finally, we introduce the time path of UI benefits. In period 0, the value of the UI benefit—measured as b in excess of home production d—rises by 160%. This matches the average increase in UI benefits between March and July 2020, as documented in Ganong et al. (2020). After six months, the benefit returns to its benchmark level.

In month 11, the benefit increases again—this time by 80%—and remains at that level for eight months. This adjustment corresponds to the Covid Relief Bill, which raised weekly benefits by $300, i.e., half the amount provided under the CARES Act. After month 18, the benefit returns to its benchmark level and remains there permanently.

After period 0, the entire future path of the UI benefit and other parameters is assumed to be known to all agents in the economy.

Next, we use the model to quantify the extent to which the endogenous UI take-up margin dampens the response of the reservation wage to an increase in UI benefits. Figure 13(a) plots two transition paths for the UI take-up rate. The blue line shows the take-up rate (averaged over monthly model output) in the benchmark model with endogenous benefit collection. The orange line shows the corresponding path in a model where take-up is exogenous. In this alternative specification, the decision to collect UI is fixed: the threshold ε^* is held at its pre-shock value.

Despite fixed individual collection decisions in the exogenous take-up model, the aggregate take-up rate evolves over the transition due to stock-flow composition effects. In particular, the large initial inflow into unemployment mechanically changes the composition of UI-eligible and ineligible individuals. The increase in eligibility temporarily raises the share of low-ε UI-eligible workers among the unemployed. Because low-ε types also have lower job-finding rates, this mechanically increases the share of collectors in the unemployed pool. Moreover, higher UI benefits raise the reservation wages of all collectors (i.e., low-ε types), regardless of unemployment duration, further reducing their job-finding rates. In contrast, non-collectors have higher job-finding rates and exit unemployment more quickly, causing the distribution of ε among the unemployed to shift over time toward low-ε collectors. Taken together, these forces generate an initial increase followed by a gradual decline in the share of UI recipients over the transition period, even though the collection threshold ε^* is held fixed exogenously.

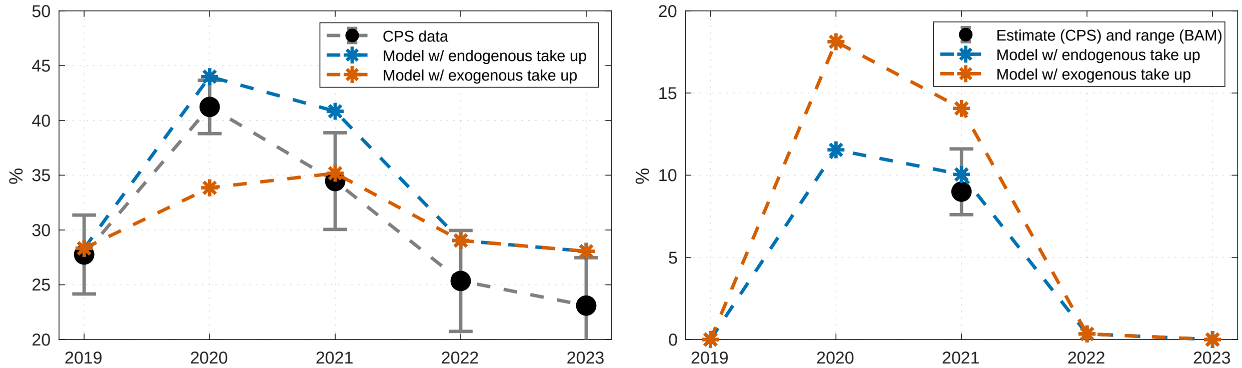
By contrast, in the model with endogenous take-up, the threshold ε^* itself responds to changes in UI generosity, as described in Section 5.1. As shown in the figure, the resulting take-up rate (blue line) closely tracks the empirical path estimated from the CPS.

Figure 13(b) plots the change in the collectors' wage premium—defined as the difference between the average log wage of collectors and that of non-collectors—relative to its steady-state (pre-shock) value. The model results are shown for two versions of the environment: one with endogenous take-up and one with a fixed take-up rate. For reference, the figure overlays our CPS-based estimate of the change in the wage premium (9%, from Table 19) and the range implied by BAM data (7.6% to 12.4%, see Section 3.6).

The comparison highlights that the endogenous take-up decision is a central mechanism shaping the pass-through from UI benefits to wages. Higher benefits raise the wages of existing collectors, but two forces attenuate this effect: (i) job-finding rates decline more among the most responsive types, and (ii) the pool of collectors expands to include marginal, high-cost types who search in tighter markets and accept lower-paying jobs. This latter channel—

Figure 13: UI Take-up Rate and Change in Collectors' Wage Premium

(a) UI Take-up Rate (b) Δ Collectors Wage Premium



Notes: The line 'CPS data' in panel (a) is from Section 4.2. The estimate in panel (b) is from CPS data as shown in Table 19 of Section 4, while the range of estimates is from BAM data as found in Section 3.6. Monthly data are aggregated to annual by simple averaging.

absent when take-up is fixed—is quantitatively important, reducing the wage response by more than half. The magnitude and direction of this dampening effect align closely with our BAM and CPS evidence, reinforcing the view that much of the labor market response to UI policy changes operates through the participation margin rather than wage setting alone.

6 Conclusion

We use data from BAM and CPS to show that while UI generosity has only a modest pass-through to wages, it has a pronounced effect on benefit take-up. In the BAM sample, higher expected benefits raise reservation wages, but the implied elasticity is small. The CPS data corroborate this low wage sensitivity using realized wages after unemployment, and further reveal that UI collection status is highly responsive to benefit levels. Probit models of take-up fit the data well only when benefit generosity is included as a key determinant. Together, these patterns suggest that the extensive margin of UI participation plays a central role in mediating the effects of UI policy—more so than the wage-setting margin.

To interpret these results, we develop a directed search model with endogenous UI take-up. A key innovation is to allow individuals to differ in the cost of applying for benefits, making take-up directly responsive to benefit generosity. When calibrated to match pre-pandemic moments, the model replicates the observed modest wage response and the large policy-driven variation in take-up. The analysis highlights how wage effects alone may understate the behavioral impact of UI expansions, and that benefit receipt decisions themselves are highly sensitive to policy design. These insights are particularly relevant for assessing the incidence and targeting of temporary UI expansions in future downturns.

APPENDIX

A BAM

Table 8 documents the share of claims in each NAICS sector for the paid claims sample, denied claims sample, and the monetary denials subset of the denied claims sample. Cyclical industries like construction and manufacturing have higher representation in the paid claims sample. Lower wage sectors such as administrative, support, waste management and remedial services, and accommodation and food service make up a larger share of the denied claims and monetary denials samples.

Of the denied claims sample in BAM, there are three subsamples: monetary denials, nonmonetary separation denials, and nonmonetary nonseparation denials. During Covid, monetary denials increased substantially. This may reflect the increase in benefit take-up among low-wage individuals. This also may reflect the incentive for those who wouldn't normally qualify for benefits, such as self-employed and gig workers, to claim UI benefits as a prerequisite for making a PUA claim as their ineligibility is only due to insufficient UI-covered earnings and not other disqualifying factors such as availability for work or quit/separation with cause, etc. Figure 14 plots the weighted population of denied claimants based on BAM. The shaded bars represent the periods during which FPUC supplemental benefits were available.

To compare the behavior of the reservation wage variable, we regress the log of the reservation wage on the same set of controls as our regression in Table 2, minus the log of weekly benefit amount since benefits are zero for the denied claims sample. Table 9 reports the coefficients for these regressions on the paid claims sample in the first two columns and for the denied claims sample in the last two columns. The relationship between usual wage and reservation wage is similar for both the paid claims and denied claims population.

Table 8: Share of Claimants by NAICS Sector of Previous Employer

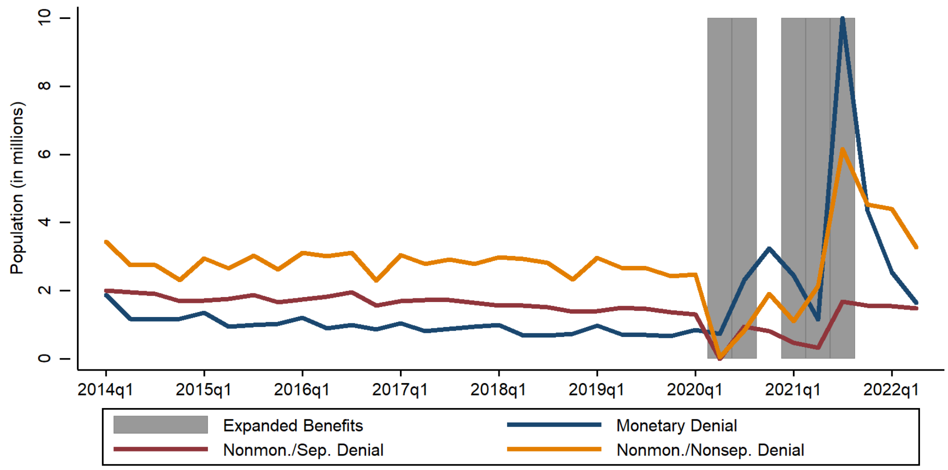
NAICS Sector	Paid Claims	All Denied Claims	Monetary Denials
11: Ag., Forestry, Fish. & Hunt	0.031	0.010	0.015
21: Mining, Quarrying, & Oil & Gas	0.010	0.009	0.004
22: Utilities	0.002	0.002	0.002
23: Construction	0.122	0.079	0.087
31-33: Manufacturing	0.094	0.099	0.057
42: Wholesale Trade	0.041	0.039	0.025
44-45: Retail Trade	0.061	0.078	0.072
48:49: Transport. & Warehousing	0.046	0.046	0.047
51: Information	0.025	0.018	0.013
52: Finance & Insurance	0.028	0.029	0.016
53: Real Estate & Rental & Leasing	0.018	0.015	0.013
54: Prof., Sci., & Technical Serv.	0.058	0.045	0.047
55: Mgmt of Companies & Enterprise	0.007	0.009	0.008
56: Administrative, Support, Waste Mgmt & Remed. Serv.	0.120	0.136	0.184
61: Edu. Services	0.029	0.033	0.023
62: Health Care & Social Asst.	0.098	0.123	0.107
71: Arts, Entertainment & Recreation	0.022	0.014	0.021
72: Accommodation & Food Serv.	0.097	0.108	0.136
81: Other Services	0.031	0.028	0.040
92: Public Administration	0.027	0.031	0.035
Observations	179,230	153,727	42,056

Notes: The Separation Denials sample is a subset of the All Denied Claims sample, which consists of Monetary Denials, Separation Denials, and Nonmonetary and Nonseparation Denials. Data spans from January 2014 to June 2022.

A.1 Collinearity of benefits and wages: High Quarter Earnings only

If benefits and usual hourly wages are highly collinear, then there is concern about interpreting the magnitude of the coefficient on benefits. Note that benefits are usually a function of earnings, which is hours times wages over a base period, and not just wages. We find that while they are positively correlated, usual wages and benefits are not highly correlated and are unlikely to be collinear. As an additional check, we rerun our regression specification only in states where benefits are a function of high quarter earnings (rather than just average earnings during a base period) as in Ferraro et al. (2022). These states are NY,

Figure 14: **Denied Claims Population**



Notes: Each line is the quarterly average of the weekly flow of denied claims by reason. Note that the shaded regions represent the time-periods during which FPUC supplemental benefits were available.

TX, FL, AZ, CA, DC, HI, ID, IA, KS, MD, MI, MN, MS, NE, NV, NM, OK, PA, SC, SD, UT, WI, and WY. Table 10 reports the results of our regression in Table 2 on this subset of states. The coefficient on the log of benefits is slightly smaller, moving from 0.013 to 0.01 in column 4, and no longer significant when we include state and time fixed effects in this smaller sample. It is clear from both regressions that usual wages are far more correlated with reservation wage and the coefficient on the benefit amount is small in magnitude across both samples. The possibility of multicollinearity in our regression motivates the need for alternative specifications to estimate the effect of benefits on the reservation wage, such as our event study approach.

A.2 Oaxaca-Blinder Decomposition

Here we present the results of the Blinder-Oaxaca decomposition when we include unemployment benefits (excluding FPUC supplement amounts) in the regression specification. The reason one may want to include benefit amounts is they are usually a function of earnings history over a prolonged base period. Heterogeneity in benefit amounts may be informative about the attachment of the worker to the labor force that is otherwise unobservable if we only include usual wages and other covariates. We get similar results to our baseline specification which does not include benfits. Table 11 shows that the In 2021, there is a slightly

Table 9: Linear regressions of $\ln(\tilde{w})$ on $\ln(w_{usual})$ for paid claims and denied claims samples

	Paid Claims		Denied Claims	
	$\ln(\tilde{w})$	$\ln(\tilde{w})$	$\ln(\tilde{w})$	$\ln(\tilde{w})$
$\ln(w_{usual})$	0.813***	0.763***	0.818***	0.765***
	(0.018)	(0.020)	(0.024)	(0.022)
U duration		−0.001**		0.000
		(0.000)		(0.000)
Age 25-44		0.001		0.004
		(0.005)		(0.003)
Age 45-64		0.015***		0.004
		(0.004)		(0.005)
Age 65+		0.004		−0.028
		(0.015)		(0.021)
Female		−0.015***		−0.019***
		(0.005)		(0.003)
Constant	0.395***	0.411***	0.368***	0.577***
	(0.059)	(0.062)	(0.059)	(0.077)
Education dummies	No	Yes	No	Yes
Race/Ethnicity dummies	No	Yes	No	Yes
2 dig NAICS dummies	No	Yes	No	Yes
State dummies	No	Yes	No	Yes
Time dummies	No	Yes	No	Yes
Adj. R-squared	0.775	0.807	0.758	0.774
Observations	182,910	177,509	146,106	50,302

Notes: Dependent variable is log of reservation wage. Columns 1 and 2 are regressions on the paid claims sample. Columns 3 and 4 are on the denied claims sample. Note that separation date, and therefore unemployment duration, is only available for the subset of denied claims that were denied due to separation reasons. U duration is in weeks. Time dummies are year-month dummies. Heteroskedasticity-robust standard errors, clustered at the state level, are shown in parentheses.

larger component of the change in reservation wage which can be ascribed to observables. However, the overall results are very similar. In 2021, the increase in reservation wages was due to the unexplained component (coefficients), which was partially dampened or offset by the composition of workers (endowments).

Table 10: States with high quarter wage in benefit determination

	Jan2014-Dec2019		Jan2014-Jun2022	
	$\ln(\tilde{w})$	$\ln(\tilde{w})$	$\ln(\tilde{w})$	$\ln(\tilde{w})$
$\ln(Benefit)$	0.613***	0.008	0.335***	0.010
	(0.029)	(0.008)	(0.017)	(0.011)
$\ln(w_{usual})$		0.753***		0.747***
		(0.024)		(0.027)
U duration		−0.001***		−0.000
		(0.000)		(0.000)
Age 25-44		0.011***		−0.005*
		(0.004)		(0.003)
Age 45-64		0.026***		0.012*
		(0.006)		(0.006)
Age 65+		0.010		−0.016
		(0.012)		(0.024)
Female		−0.016***		−0.012*
		(0.004)		(0.006)
PUA				0.016
				(0.017)
Constant	−0.796***	0.472***	0.782***	0.432***
	(0.180)	(0.075)	(0.107)	(0.108)
Education dummies	No	Yes	No	Yes
Race/Ethnicity dummies	No	Yes	No	Yes
2 dig NAICS dummies	No	Yes	No	Yes
State dummies	No	Yes	No	Yes
Time dummies	No	Yes	No	Yes
Adj. R-squared	0.305	0.789	0.146	0.801
Observations	63,280	61,990	85,371	82,942

Notes: Dependent variable is log of reservation wage. Columns 1 and 2 are regressions on the paid claims sample through December 2019. Columns 3 and 4 are on the paid claims sample through June 2022. This regression is on the subset of states that use high quarter earnings in their benefit determination. These states are: NY, TX, FL, AZ, CA, DC, HI, ID, IA, KS, MD, MI, MN, MS, NE, NV, NM, OK, PA, SC, SD, UT, WI, and WY. U duration is in weeks. Time dummies are year-month dummies. Heteroskedasticity-robust standard errors, clustered at the state level, are shown in parentheses.

Table 11: Blinder-Oaxaca Decomposition Pre and Post Covid 2020

	Decomposition		Percent	
Pre Covid	2.798***	[2.761,2.836]		
2020 Expanded Benefit	2.714***	[2.656,2.773]		
difference	-0.084***	[-0.134,-0.034]		
endowments	-0.127***	[-0.168,-0.087]	151.073***	[97.239,204.906]
coefficients	0.038***	[0.011,0.065]	-44.856*	[-97.000,7.288]
interaction	0.005	[-0.003,0.013]		

Notes: 2020 Expanded Benefit refers to the time period from March 3rd (week 14) up to August 2nd (week 31) of 2020. Note that BAM data is largely missing from weeks 14 to 26 of 2020. Confidence intervals are calculated from robust standard errors, clustered at the state level.

Table 12: Blinder-Oaxaca Decomposition, Pre and Post Covid 2021

	Decomposition		Percent	
Pre Covid	2.798***	[2.761,2.836]		
2021 Expanded Benefit	2.851***	[2.805,2.898]		
difference	0.053***	[0.028,0.078]		
endowments	-0.022**	[-0.040,-0.004]	-41.536	[-91.767,8.695]
coefficients	0.074***	[0.056,0.093]	140.345***	[90.350,190.340]
interaction	0.001	[-0.001,0.002]		

Notes: 2021 Expanded Benefit refers to the time period from week 1 of 2021 through week 35 (ending Sept 5th) of 2021. FPUC expired federally on September 6th, 2021. Confidence intervals are calculated from robust standard errors, clustered at the state level.

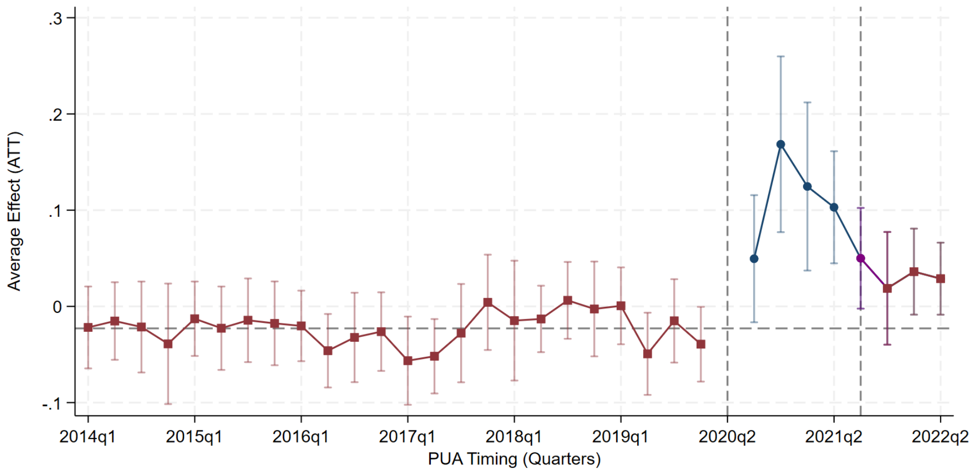
A.3 Diff-in-Diff Robustness

A.3.1 Heterogeneous Treatment Cutoffs

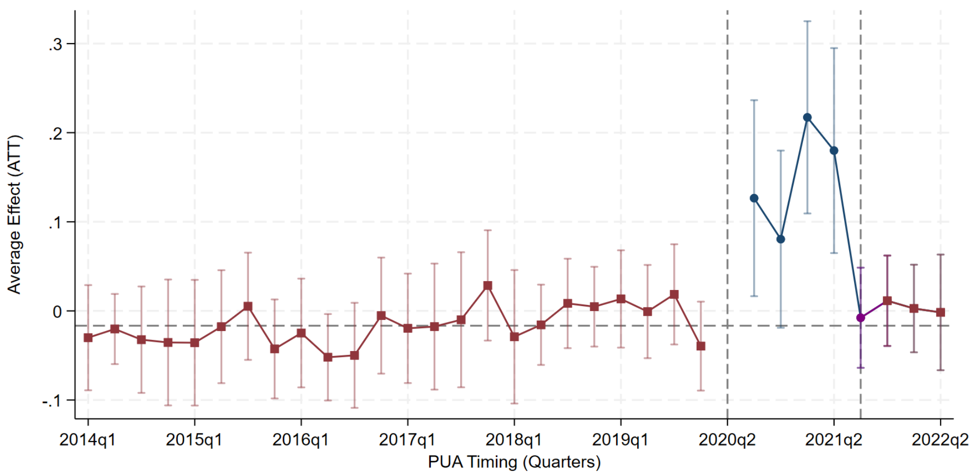
In this section, we consider alternate cutoffs for defining treatment and control states for our analysis. We also look at heterogeneity in treatment intensity by splitting our treatment group into two separate groups – states in the 50-75th percentile of PUA claim share and states in the top quartile of claim share. We then plot the event-time profile of our regression for each quartile as the treatment group separately.

Figures 15(a) and 15(b) present the results of our regression if we use the bottom 50 percent of states as the control group, and look at the 3rd and 4th quartiles of states as the treatment group independently rather than combined together in our baseline.

Figure 15: **PUA Intensity: Event-time Profiles for 3rd and 4th Quartile Treatment Groups**



(a) 3rd quartile as treatment group

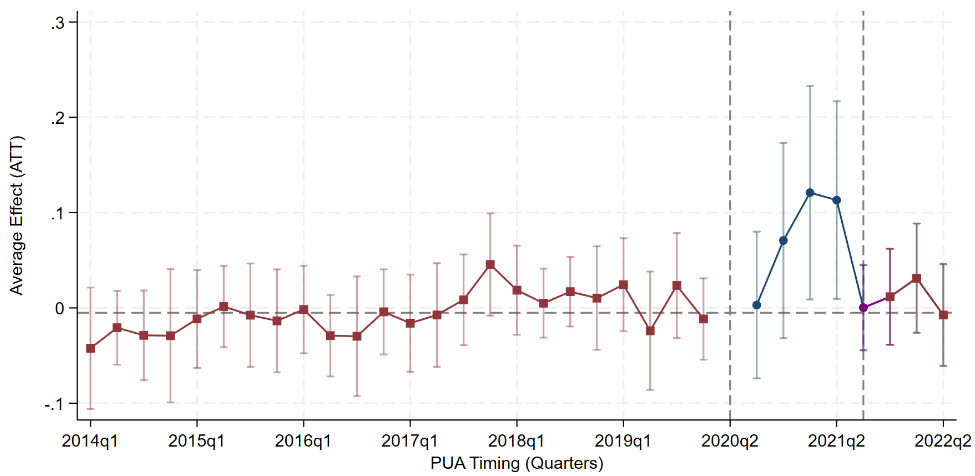


(b) 4th quartile as treatment group

Notes: In both panels, the control group is states below the median level of PUA claim share (.214) during the PUA period. Panel (a) presents results for states in the 50th–75th percentile of PUA claim share (.214–.299). Panel (b) presents results for states in the 75th–100th percentile (.299–.41). Period 0 corresponds to 2020 Q2; the first treatment period is 2020 Q3; and Period 5 corresponds to 2021 Q3 and the end of PUA. Some states ended PUA in July 2021, while the federal program ended in September 2021. Robust standard errors are clustered at the state level.

Now we explore the heterogeneity in treatment intensity by comparing each quartile of PUA claim share states to the bottom quartile, using it as the control group instead of the bottom half. Figure 16 presents the results if only the bottom quartile of states by PUA claim share are used in the control group, and the top two quartiles are the treatment group. Our results are qualitatively robust to using this bottom quartile group as our control group and the top half of states remains in the treatment group. We can also look at each of the 2nd–4th quartiles as treatment groups separately and use the bottom quartile as our control group. As expected, the effect is much more muted for the 2nd quartile, as seen in Figure 17(a). We find similar magnitudes to our baseline event-time plot when we define the 3rd and 4th quartiles as the treatment and the 1st quartile as the control group, as seen in Figures 17(b) and 17(c), respectively.

Figure 16: **PUA Intensity: 1st quartile as Control Group**



Notes: Control group is states in 0-25th percentile bin by level of PUA claim share (.147) during PUA period. Treatment group is states above the median level of PUA claim share (.214). Period 0 corresponds to 2020 Quarter 2. The first observation in the treatment period is 2020 Q3. Period 5 corresponds to 2021 Quarter 3 and the end of PUA. Note that some states ended PUA as early as July, while the program ended for all states in September 2021. Robust standard errors are clustered at the state level.

A.3.2 Alternate PUA Intensity using Initial Claims

Figure 18 shows the state-level variation of an alternative definition of PUA intensity, the share of initial PUA claims of total (UI + PUA) initial claims.

We can run our baseline Differences-in-Differences specification defining "Treated" and "Control" states with this PUA initial claim share measure. We plot the event-time profile of our regression estimates in Figure 19. The plot is nearly identical to our baseline figure.

A.3.3 Placebo Tests

In this section we present the results of several placebo tests to investigate plausible threats to identification in our baseline difference-in-difference specification. A natural concern is that the positive reservation wage effects associated with PUA intensity might instead reflect other state-level conditions or mechanisms unrelated to UI generosity. To evaluate this, we re-estimate our regression specification replacing the timing of PUA events with placebo events where no treatment occurred, and replacing PUA intensity with placebo treatment variables that vary across states but are conceptually unrelated to UI generosity.

To motivate our placebo timing, we may be concerned that high PUA intensity states were more generous in recessions through avenues outside of UI for low income workers, and thus have different cyclical dynamics of reservation wages than the control states, but that are unrelated to PUA. To test this, we implement a placebo timing using the Great Recession as our event time. During the Great Recession, there was a pronounced labor market downturn but no policy was enacted that would have extended eligibility to the monetary denial population, and thus neither group of states was treated. Figure 20 shows that reservation wages for our high PUA states did not increase, but rather decreased slightly at the onset of the Great Recession.

We may also be concerned about state-level characteristics that could coincide with PUA intensity and could affect reservation wages, and run several placebo treatments to show that assigning "treatment" states by varying measures which may confound PUA intensity do not yield the same event-time profile. For instance, worker or industry composition may have influenced the severity of state-level exposure to Covid-induced recessionary shocks and thus have affected reservation wages. We run placebo tests assigning "treatment" status to states with above median average wages, above median manufacturing share of employment, and above median shares of leisure and hospitality employment in 2019.

First, we stratify states using state-level average wages in 2019 as measured in the Quarterly Census of Employment and Wages by the Bureau of Labor Statistics. High wage states differ in skill composition, industry mix, urbanization, and remote-work intensity; if these pre-existing differences were driving our results, the placebo treatment should resemble the PUA pattern. Figure 21(a) shows a pattern that is inconsistent with the PUA effect, with a decline in reservation wage in late 2020 and insignificant increases in 2021.

We stratify states using their 2019 employment shares in Manufacturing (NAICS 31-33) and Leisure and Hospitality (NAICS 71-72) based on QCEW. These industries experienced significant shocks during the pandemic. If sectoral composition were the key driver of our baseline findings, high-manufacturing or high-hospitality states would exhibit patterns similar to the PUA results. Instead, high-manufacturing states displayed a pronounced negative reservation-wage deviation in early 2021, consistent with supply-chain disruptions and weak early-recovery labor demand, while high-hospitality states exhibit an initial positive deviation during the shutdown phase, followed by negative deviations during reopening and a rebound in 2022. These dynamic patterns reflect sector-specific pandemic shocks and are qualitatively different from the smooth, positive increase associated with PUA intensity.

One concern is that reservation wage increases could have reflected labor supply incentives related to health risks and the severity of the pandemic. If states with high PUA intensity also had more severe Covid death rates, higher reservation wages may reflect a risk premium due to workers' unwillingess to risk exposure to the virus. We stratify states by Covid death rates per capita during the PUA period, assigning the top half of states to the treatment group. Figure 22(a) shows that high Covid death rate states did not exhibit an elevated reservation wage in the same manner as PUA intensity. We may also be concerned that PUA intensity was correlated with lockdown-induced layoffs, which may have influenced reservation wages. We run a placebo test assigning treatment to the top half of states by their respective increase in unemployment rates from 2019 Q4 to their peak unemployment rate in the 2020-2021 period. Figure 22(b) shows that states that experienced the highest increases in unemployment saw slightly elevated reservation wages in 2021, but did not see increases in 2020 and these changes remain mostly statistically insignificant and unrelated to the timing of PUA and FPUC.

A.4 Fixed Effect Specification with Condensed Time Dummies

To isolate the impact of increased benefits, we condense the timing in our baseline results in Figure 6 to define the categorical variable *regime=[PUA, PUA + FPUC, Phase Out]* . The first coefficient interacts PUA claim shares with *regime=PUA*, which is true for quarters 2020 Q3 and 2020 Q4, representing the regime when PUA benefits were available without the additional benefits from FPUC. The second interacts PUA claim shares with *regime=PUA + FPUC* for quarters 2021 Q1 and 2021 Q2, capturing the regime where PUA benefits were available alongside the additional \$300 weekly FPUC supplement. The third interacts PUA claim shares with *regime=Phase Out* for 2021 Q3, representing the wind down of both the PUA and FPUC programs. We also show that these results are robust to the introduction of Covid-related state-level controls, specifically the quarterly average of weekly state-level Covid deaths per 100k persons and the quarterly average of the state unemployment rate.

By comparing the coefficients from the pre-FPUC period to the FPUC period interaction terms, we estimate the percentage increase in reservation wages resulting from the additional \$300 in weekly UI benefits, holding access to PUA constant. Assuming the effects of access to PUA remain consistent across both periods, the difference in coefficients can be interpreted as the impact of increasing benefits by \$300 per week.

We first verify that our condensed dummy variables capture the essence of our baseline specification by considering the following specification:

$$Y_{ist} = \lambda_t + \sum_{r \in \{\text{PUA},\, \text{PUA+FPUC},\, \text{PhaseOut}\}} \alpha_r \left(\mathbb{I}_{treat,st} D_{rt} \right) + \beta' X_{it} + \varepsilon_{it},$$

where λ_t is a (quarterly) time dummy, $\mathbb{I}_{treat,st}$ is equal to 1 if the state is a high PUA claims share state, $regime_t$ represents the 3 condensed dummy variables introduced above, and X is a set of controls.[58]

The first column of Table 13 displays the coefficient associated with PUA intensity interacted with each element of *regime*. We present more detailed results of these regressions in the next section, Appendix A.4.1. The interpretation is that the existence of the PUA program alone increases reservation wages by about 6% in high relative to low PUA share states. Similarly, the coefficient on PUA + FPUC suggests that the increase in benefits associated with the FPUC program further increased reservation wages by 5%, for a total of 11% relative to baseline. The second column of Table 13 shows that these results are robust to the introduction of Covid-related state-level controls, specifically the quarterly average of weekly state-level Covid deaths per 100k persons and the quarterly average of the state unemployment rate.

Finally, to confirm that these results are not coming from unmodeled time variation in the relationship between reservation wages and Covid/economic environment of the state. We do so by controlling for within-quarter, across state variation in Covid severity and labor

[58]The controls remain the log of usual hourly wage, age bins, sex, race/ethnicity, and NAICS sectors.

market tightness, as follows:

$$Y_{ist} = \lambda_t + \sum_{r \in \{\text{PUA}, \text{PUA+FPUC}, \text{PhaseOut}\}} \alpha_r \left(\mathbb{I}_{treat,st} D_{rt}\right) + \delta_t Covid_{st} * qtr_t + \gamma_t Urate_{st} * qtr_t + \beta' X_{it} + \varepsilon_{it},$$

where $Covid_{st}$ is the Covid deaths per 100k persons and $Urate_{st}$ is the quarterly average of the state unemployment rate. The estimates, displayed in the third column of Table 13, show that our results are robust to this specification, though the effect of PUA is smaller, both relative to other specifications and our baseline event study.

Table 13: Differences-in-Differences Regression

	$\ln(\tilde{w})$	$\ln(\tilde{w})$	$\ln(\tilde{w})$
PUA	0.062	0.062*	0.030
	(0.039)	(0.034)	(0.034)
PUA + FPUC	0.110**	0.109**	0.101***
	(0.044)	(0.044)	(0.036)
Phase Out	0.013	0.016	0.007
	(0.029)	(0.026)	(0.026)
Qtly Controls	No	Yes	No
Qtly Controls x Time	No	No	Yes
Adj. R-squared	0.761	0.762	0.763
Observations	33,868	33,868	33,868

Notes: PUA corresponds to 2020 Q3, Q4 since data is not available for 2020 Q2. PUA + FPUC corresponds to 2021 Q1 and Q2 during which PUA and FPUC were active. Phase Out corresponds to 2021 Q3, during which some states phased out pandemic-era unemployment programs. All pandemic UI programs ended in September of 2021. Robust standard errors, clustered at the state level, are shown in parentheses.

A.4.1 Fixed Effects Regressions and Quarterly Treatment Variable: Alternate Specifications

We present the results of our regression specification in Table 13 but using quarterly time dummies interacted with our treatment variable ($\mathbb{I}_{treat,it} * qtr_t$) instead of combining quarters.

In our baseline analysis, we use the state-level variation in the share of all Covid-era UI claims that were PUA claims to define our treatment and control group states. For our quarterly specification, we use the PUA claims as a share of the unemployed instead of as

Table 14: Continuous Treatment Specification: Quarterly Coefficients

	$\ln(\tilde{w})$	$\ln(\tilde{w})$	$\ln(\tilde{w})$
2020 Q3	0.067	0.062	0.013
	(0.045)	(0.043)	(0.017)
2020 Q4	0.059	0.061	0.045
	(0.046)	(0.040)	(0.055)
2021 Q1	0.129***	0.121**	0.093**
	(0.048)	(0.048)	(0.041)
2021 Q2	0.094*	0.099*	0.107**
	(0.053)	(0.053)	(0.043)
2021 Q3	0.013	0.016	0.007
	(0.029)	(0.026)	(0.026)
Qtly Controls	No	Yes	No
Qtly Controls x Time	No	No	Yes
Adj. R-squared	0.761	0.762	0.763
Observations	33,868	33,868	33,868

Notes: PUA without FPUC corresponds to 2020 Q3, Q4 since data is not available for 2020 Q2. PUA + FPUC corresponds to 2021 Q1 and Q2 during which PUA and FPUC were active. Phase Out corresponds to 2021 Q3, during which some states phased out pandemic-era unemployment programs. Robust standard errors, clustered at the state level, are shown in parentheses.

a share of total claims. This is to reflect that the share of potential PUA claimants and collectors in a quarter may be better captured by the total unemployed population than by the flow of newly unemployed claimants. Figure 23 shows the state-level variation in this measure when we look at average intensity over the PUA period by state. While there is some difference in levels, the state variation is very similar to our baseline measure of PUA claim shares. Our results are consistent with alternative measures of PUA intensity, as we show in the next two tables. In Table 15, we present the results of our regression specification in Table 5, but using the state-level quarterly average of PUA claims as a share of Total UI claims as the treatment variable. Note that this is the quarterly measure of our treatment and control definition for our baseline difference-in-difference regression represented by the event time profile in Figure 6.

In Table 16, we present the results of our regression specification in Table 5, but using each state's quarterly average share of PUA *initial* claims out of the total population of unemployed in the state as the treatment variable. In our baseline, we use total PUA claims

Table 15: PUA Claims/Total UI Claims

	$\ln(\tilde{w})$	$\ln(\tilde{w})$	$\ln(\tilde{w})$
PUA	0.074	0.079	0.014
	(0.079)	(0.067)	(0.073)
PUA + FPUC	0.178**	0.186***	0.160**
	(0.070)	(0.068)	(0.076)
Phase Out	0.006	0.001	−0.036
	(0.055)	(0.054)	(0.049)
Elasticity	0.050	0.052	0.071*
	(0.034)	(0.034)	(0.040)
Qtly Controls	No	Yes	No
Qtly Controls x Time	No	No	Yes
Adj. R-squared	0.761	0.762	0.763
Observations	33,868	33,868	33,868

Notes: PUA without FPUC corresponds to 2020 Q3, Q4 since data is not available for 2020 Q2. PUA + FPUC corresponds to 2021 Q1 and Q2 during which PUA and FPUC were active. Phase Out corresponds to 2021 Q3, during which some states phased out pandemic-era unemployment programs. Elasticity calculated with $\bar{S} = 0.48$. Robust standard errors, clustered at the state level, are shown in parentheses.

as our measure of intensity, but this could reflect both a high count of unique claiming individuals as well as a longer duration of continuing claims. Using total claims means that the measure is influenced by the duration of continuing claims. This could result in endogeneity as the measure could depend on reservation wages. Our results are robust to using initial PUA claims, which measures the flow of new PUA claimants and ignores the stock of continuing claimants and any concerns about duration.

A.4.2 Expanded State-Level Controls

We further expand the control set in the continuous-treatment specification of Section 3.6 to include the quarterly Oxford stringency index, the quarterly Oxford workplace-closing policy index, and a 2020 measure of the regular UI monetary threshold. We interact each of these controls with quarter indicators, allowing their relationship with reservation wages to vary over time in the same way as our baseline Covid and unemployment controls. The purpose of these additions is to absorb broader cross-state differences in pandemic restrictions and

Table 16: PUA Initial Claims/Total Unemp (State, Qtly)

	$ln(\tilde{w})$	$ln(\tilde{w})$	$ln(\tilde{w})$
PUA	0.690	0.660*	0.447
	(0.484)	(0.381)	(0.318)
PUA + FPUC	3.102***	3.223***	2.843***
	(1.010)	(0.896)	(0.812)
Phase Out	−0.132	−0.198	−0.472
	(0.393)	(0.403)	(0.360)
Elasticity	0.087***	0.093***	0.087***
	(0.026)	(0.027)	(0.023)
Qtly Controls	No	Yes	No
Qtly Controls x Time	No	No	Yes
Adj. R-squared	0.761	0.762	0.763
Observations	33,868	33,868	33,868

Notes: PUA without FPUC corresponds to 2020 Q3, Q4 since data is not available for 2020 Q2. PUA + FPUC corresponds to 2021 Q1 and Q2 during which PUA and FPUC were active. Phase Out corresponds to 2021 Q3, during which some states phased out pandemic-era unemployment programs. Elasticity calculated with $\bar{S} = 0.036$. Robust standard errors, clustered at the state level, are shown in parentheses.

regular UI eligibility stringency that could be correlated with PUA intensity.

Figure 24 summarizes how baseline PUA utilization varies with these controls together with the original Covid severity controls. Panels (c) and (d) show both the raw monetary-threshold measure and the alternative average-quarter threshold construction. Taken together, these panels make clear that some of the cross-state variation in PUA intensity lines up with these policy variables, but the remaining dispersion is substantial. Consistent with that visual impression, the joint state-level balance regression explains less than half of the cross-state variation in baseline PUA intensity, leaving considerable residual dispersion for identification.

Table 17 adds these controls to the continuous-treatment specification reported in Table 5. The first two columns reproduce the baseline progression from the main text, while the third column augments the specification with time-interacted Oxford stringency, workplace-closing, and monetary-threshold controls. The fully controlled column continues to show a positive and statistically significant PUA+FPUC coefficient, while the implied elasticity remains 0.115. The phase-out coefficient remains near zero. Thus, conditioning on this expanded set of state-level policy measures does not overturn the main elasticity estimate.

Table 17: Continuous Treatment Specification with Expanded State-Level Controls

	$\ln(\tilde{w})$	$\ln(\tilde{w})$	$\ln(\tilde{w})$
PUA	0.023	-0.008	0.046*
	(0.039)	(0.037)	(0.026)
PUA + FPUC	0.099***	0.116**	0.116***
	(0.030)	(0.043)	(0.025)
Phase Out	0.014	-0.015	-0.005
	(0.042)	(0.037)	(0.029)
Oxford Stringency x Time	No	No	Yes
Workplace Closing x Time	No	No	Yes
Threshold x Time	No	No	Yes
Qtly Controls x Time	No	Yes	Yes
Difference	0.076**	0.123***	0.070***
	(0.038)	(0.045)	(0.026)
Elasticity	0.067**	0.108***	0.061***
	(0.033)	(0.039)	(0.023)
Adj. R-squared	0.761	0.763	0.762
Observations	33,868	33,868	33,317

Notes: Columns (1) and (2) reproduce the baseline continuous-treatment progression from Table 5. Column (3) additionally includes quarter-interacted Oxford stringency, Oxford workplace-closing, and the 2020 regular UI monetary threshold. PUA without FPUC corresponds to 2020 Q3 and Q4, PUA + FPUC corresponds to 2021 Q1 and Q2, and Phase Out corresponds to 2021 Q3. Elasticity is computed using the mean PUA intensity in the PUA + FPUC regime. Robust standard errors, clustered at the state level, are shown in parentheses.

Finally, Table 18 shows that the same conclusion holds when we replace the raw base-period threshold with the alternative average-quarter threshold construction. The resulting elasticity estimate is nearly identical to the one in Table 17, indicating that the robustness results are not sensitive to how the monetary-threshold control is standardized.

A.5 Covid and Unemployment Controls

As discussed in Section 3.6, our estimates rely on the assumption of parallel trends between the treatment and control states. Additionally, we require that other variables are not

Table 18: Continuous Treatment Specification with Average-Quarter Threshold Controls

	$\ln(\tilde{w})$	$\ln(\tilde{w})$	$\ln(\tilde{w})$
PUA	0.023	-0.008	0.021
	(0.039)	(0.037)	(0.029)
PUA + FPUC	0.099^{***}	0.116^{**}	0.152^{***}
	(0.030)	(0.043)	(0.028)
Phase Out	0.014	-0.015	-0.006
	(0.042)	(0.037)	(0.030)
Oxford Stringency x Time	No	No	Yes
Workplace Closing x Time	No	No	Yes
Avg-Q Threshold x Time	No	No	Yes
Qtly Controls x Time	No	Yes	Yes
Difference	0.076^{**}	0.123^{***}	0.131^{***}
	(0.038)	(0.045)	(0.030)
Elasticity	0.067^{**}	0.108^{***}	0.115^{***}
	(0.033)	(0.039)	(0.026)
Adj. R-squared	0.761	0.763	0.763
Observations	33,868	33,868	33,317

Notes: This table is identical to Table 17 except that the regular UI threshold control uses the average-quarter threshold construction rather than the raw base-period threshold. PUA without FPUC corresponds to 2020 Q3 and Q4, PUA + FPUC corresponds to 2021 Q1 and Q2, and Phase Out corresponds to 2021 Q3. Elasticity is computed using the mean PUA intensity in the PUA + FPUC regime. Robust standard errors, clustered at the state level, are shown in parentheses.

correlated with treatment timing across states. One potential threat to validity arises if PUA claims reflect underlying labor market weakness—such as states with high PUA claims also having disproportionately high unemployment rates. In this case, weaker labor markets could lead to lower reservation wages due to reduced job opportunities, potentially underestimating the disincentive effects of UI on job search behavior.

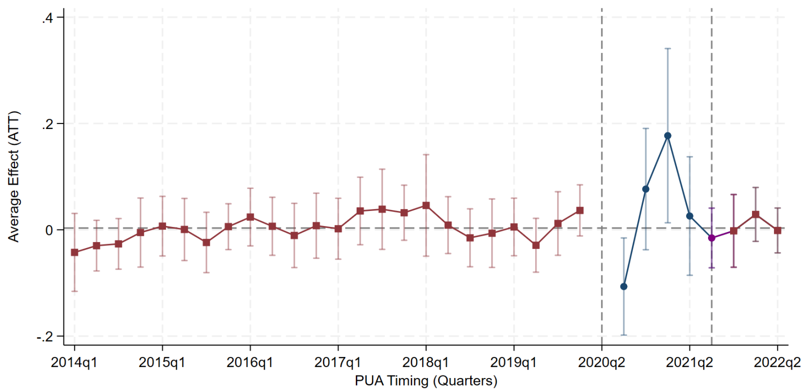
Another possible confounding factor is the severity of Covid-19. If individuals in states with high PUA claims faced greater exposure to Covid-related health risks, the observed increase in reservation wages could reflect a compensating differential required to enter riskier work environments rather than the effect of expanded UI availability.

To address these concerns, we include variables related to both factors as controls: the quarterly state-level unemployment rate and the average weekly Covid-19 deaths per 100,000 individuals. We showed that our measures are robust to including these controls in the regression.

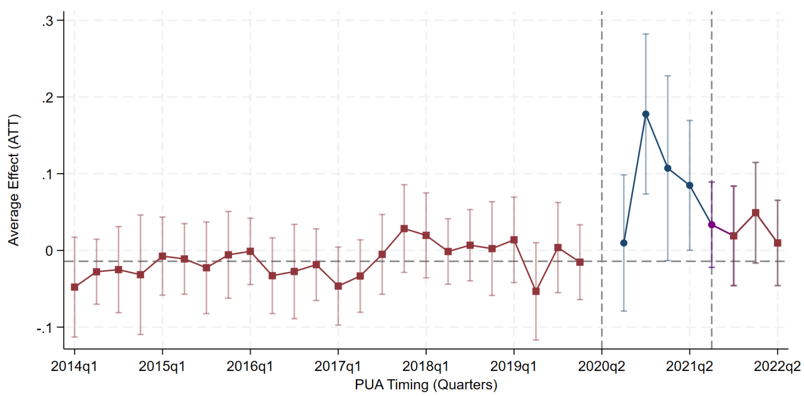
We can also show that our definition of treatment and control states by PUA claim share does indeed split states into high and low PUA utilization, though the treated and control states have remarkably similar time-series in terms of Covid deaths and Unemployment rates at the onset of the pandemic. Figure 25 plots the PUA claims as a share of unemployment in our treatment states and control states at quarterly frequency.

Figures 26 and 27 plot our treatment and control states' respective means of the quarterly average of weekly Covid cases and deaths per 100k in our sample. The control states experienced slightly higher case and death rates than the treatment states, suggesting that increases in reservation wages for the treatment states was unlikely to stem from Covid intensity. Figure 28 plots the treatment and control states' respective means of their quarterly unemployment rate. The peak unemployment rate is similar across both state groups, suggesting that increases in reservation wages for the treatment states is unlikely to have been confounded by differences in labor market tightness across states.

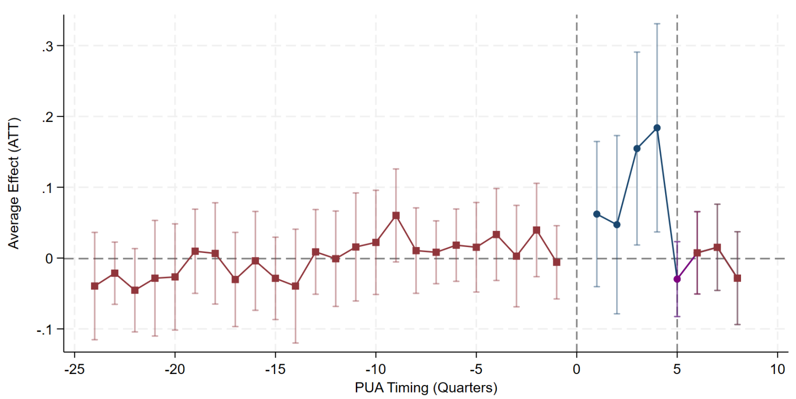
Figure 17: **PUA Intensity: Event-time Profiles with 1st Quartile Control Group**



(a) 1st qtile control, 2nd qtile treatment



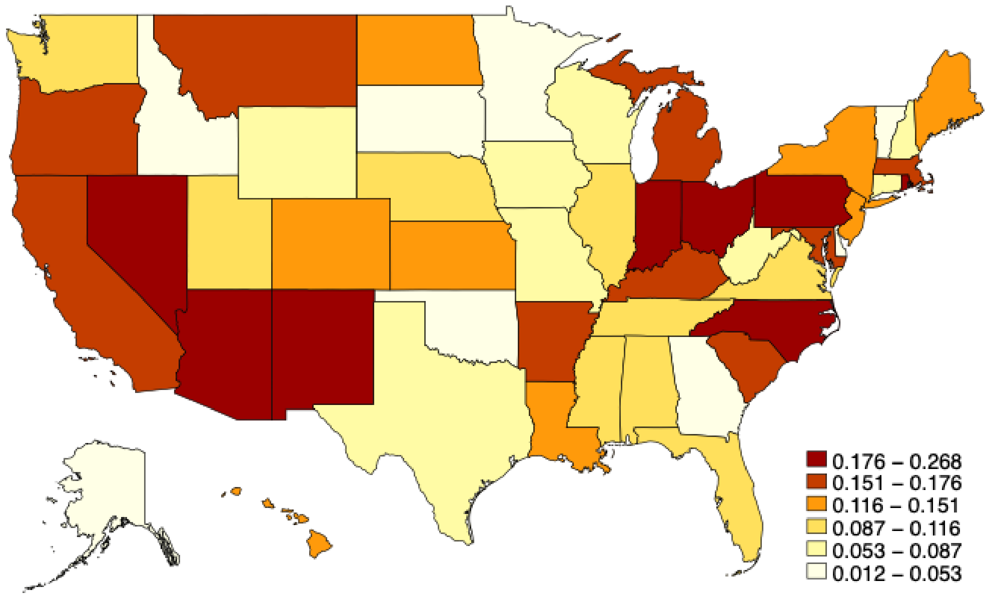
(b) 1st qtile control, 3rd qtile treatment



(c) 1st qtile control, 4th qtile treatment

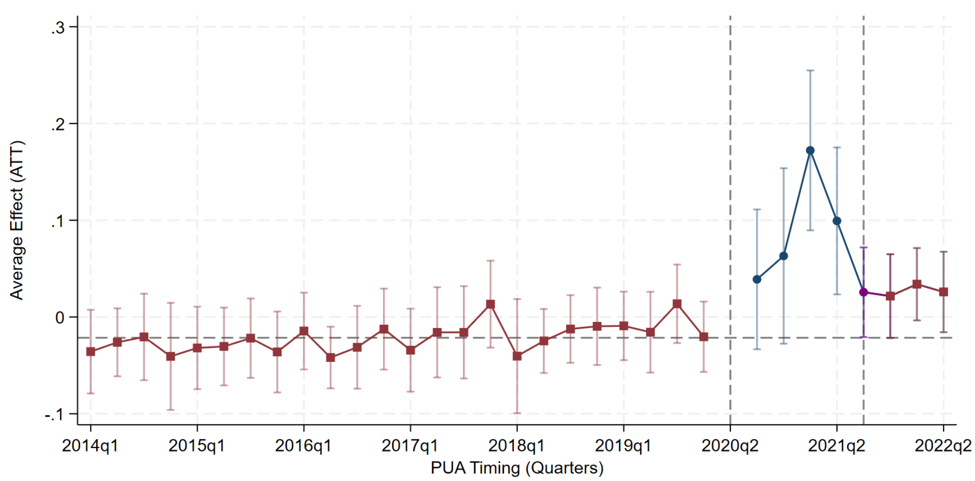
Notes: In all panels, the control group is states in the 0–25th percentile bin by level of PUA claim share (.147) during the PUA period. Panel (a) uses as the treatment group states in the 25th–50th percentile bin (.147–.214). Panel (b) uses states in the 50th–75th percentile bin (.214–.299), and panel (c) uses states in the 75th–100th percentile bin (.299–.41). Period 0 corresponds to 2020 Q2; the first treatment period is 2020 Q3. Period 5 corresponds to 2021 Q3 and the end of PUA. Note that some states ended PUA as early as July, while the program ended for all states in September 2021. Robust standard errors are clustered at the state level.

Figure 18: **PUA Initial Claims as Share of Total Initial UI Claims**



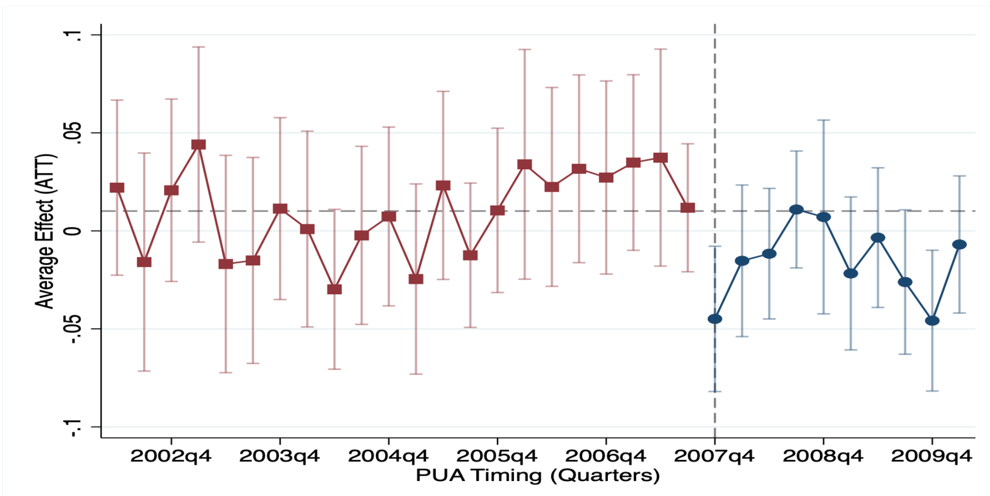
Notes: The state-level measure of total initial PUA claims as a share of total initial UI claims (regular UI + PUA) for the state during the period where PUA was active.

Figure 19: **PUA Intensity by Initial Claims Only**



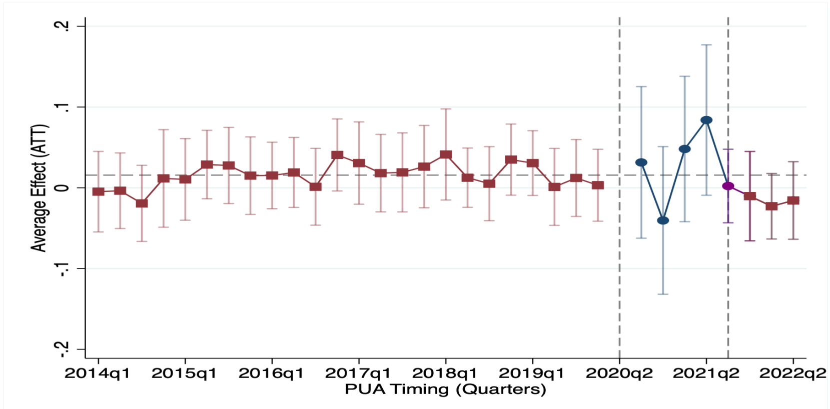
Notes: Dependent variable is the log of reservation wage. Control group is states with below median level of PUA initial claim share (.116) during PUA period. Treatment group is states with above median level of PUA initial claim share during PUA period. Period 0 corresponds to 2020 Quarter 2. The first observation in the treatment period is 2020 Q3. Period 5 corresponds to 2021 Quarter 3 and the end of PUA. Note that some states ended PUA as early as July, while the program ended for all states in September 2021. Robust standard errors are clustered at the state level.

Figure 20: **Placebo Timing: Great Recession**

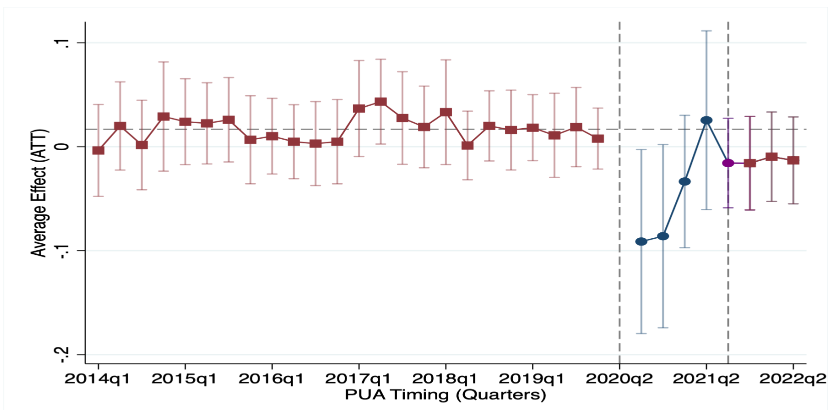


Notes: Dependent variable is the log of reservation wage. Control group is states with below median level of PUA initial claim share (.116) during PUA period. Treatment group is states with above median level of PUA initial claim share during PUA period. Period 0 corresponds to 2007 Quarter 4, the start of the Great Recession. Robust standard errors are clustered at the state level.

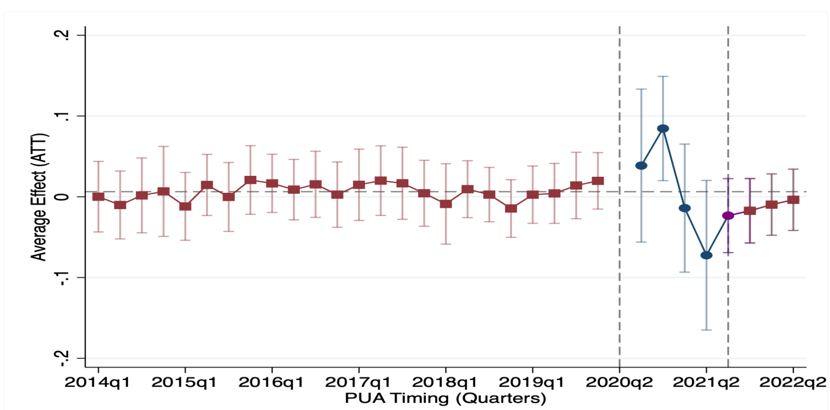
Figure 21: **Placebo Treatments: State Pre-Covid Characteristics**



(a) State average wage



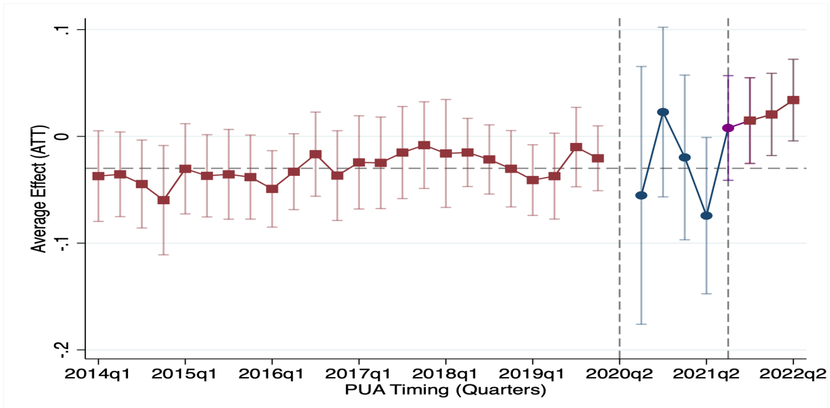
(b) Manufacturing employment share



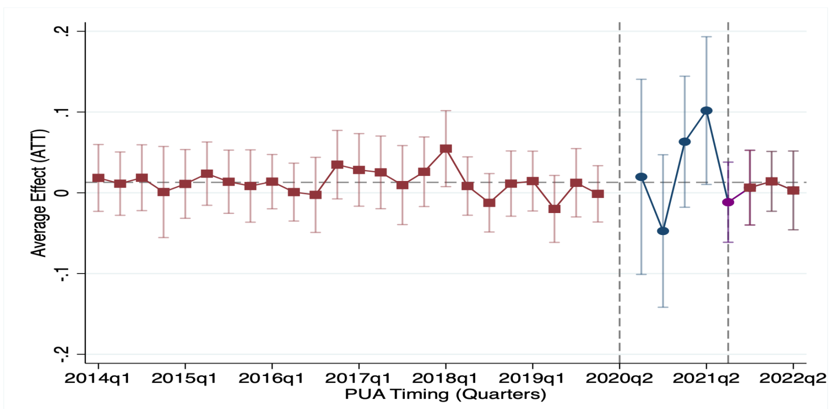
(c) Leisure & hospitality employment share

Notes: In all panels, the dependent variable is the log of the reservation wage. Placebo treatment groups are defined using pre-pandemic state characteristics measured in 2019 with the Quarterly Census of Employment and Wages (QCEW). In panel (a), the control group is states with below-median state-level average wages, and the treatment group is states with above-median wages. In panel (b), the control group is states with below-median employment shares in Manufacturing (NAICS 31–33), and the treatment group is states with above-median Manufacturing shares. In panel (c), the control group is states with below-median employment shares in Leisure and Hospitality (NAICS 71–72), and the treatment group is states with above-median Leisure and Hospitality shares. Period 0 corresponds to 2020 Q2; the first treatment period is 2020 Q3; and

Figure 22: **Placebo Treatments: Covid Deaths and Unemployment**



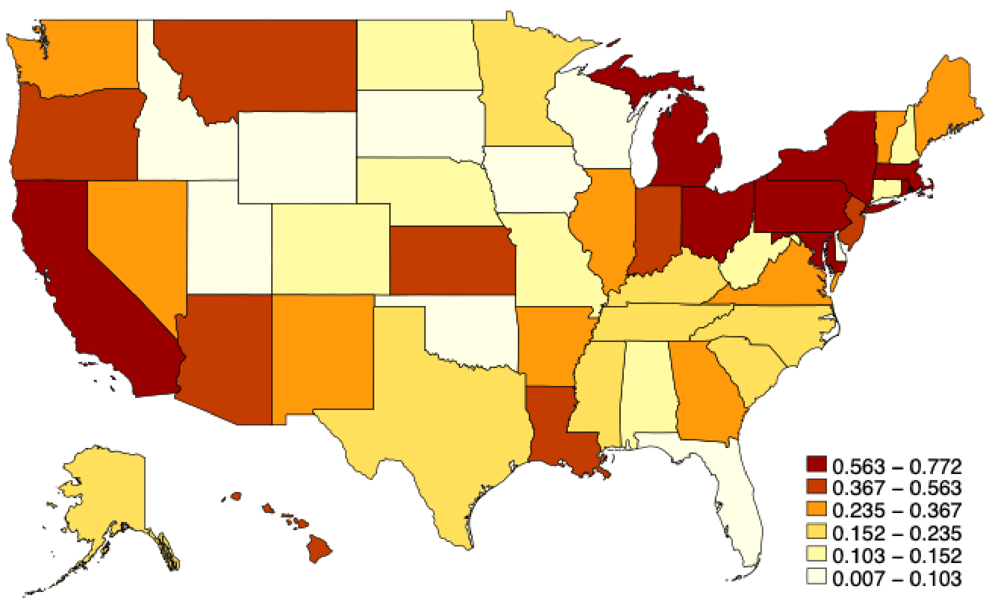
(a) Covid deaths per 100k



(b) Unemployment rate increase

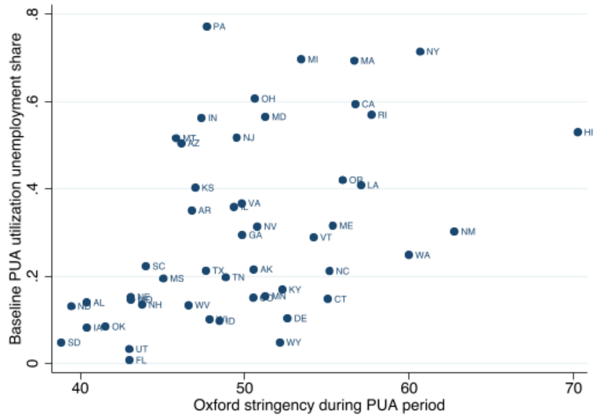
Notes: In all panels, the dependent variable is the log of the reservation wage. Placebo treatment groups are defined using measures of state-level averages during the PUA period. In panel (a), the control group is states with below-median state-level average death rate per 100k residents, and the treatment group is states with above-median death rates. In panel (b), the control group is states with below-median increase in unemployment rate relative to 2019 Q4, and the treatment group is states with above-median increase in unemployment rates relative to 2019 Q4, measured at the peak unemployment rate during the pandemic period. Period 0 corresponds to 2020 Q2; the first treatment period is 2020 Q3; and Period 5 corresponds to 2021 Q3 and the end of PUA. Some states ended PUA as early as July 2021, while the federal program ended in September 2021. Robust standard errors are clustered at the state level.

Figure 23: **PUA Claims as Share of Total Unemployment**

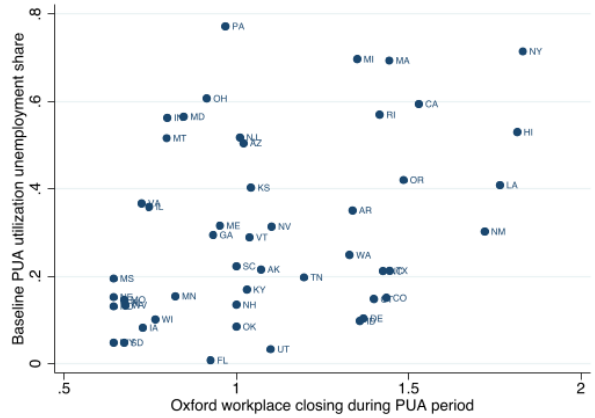


Notes: The state-level average of the weekly measure of total PUA claims in the week as a share of the total unemployed individuals (measured at monthly frequency).

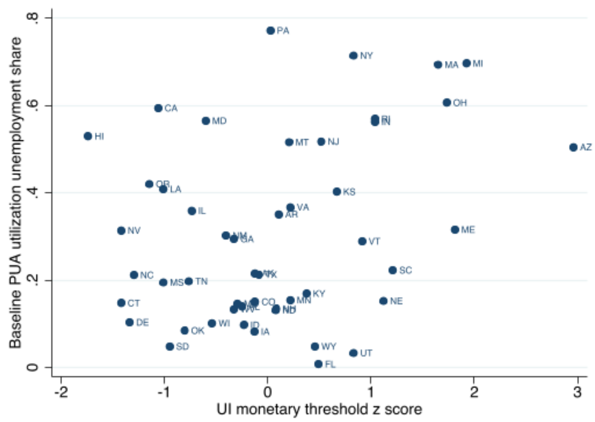
Figure 24: **PUA Intensity and Expanded State-Level Controls**



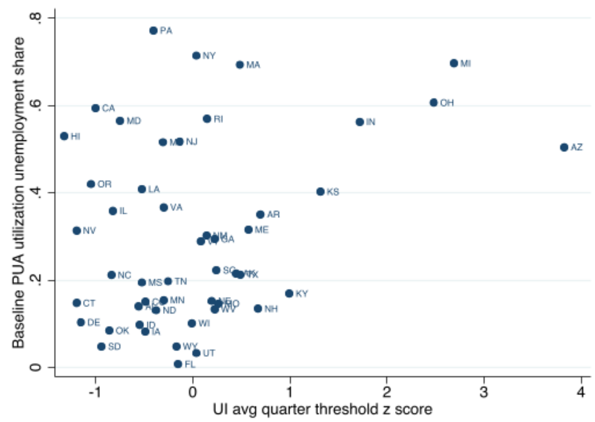
(a) Oxford stringency



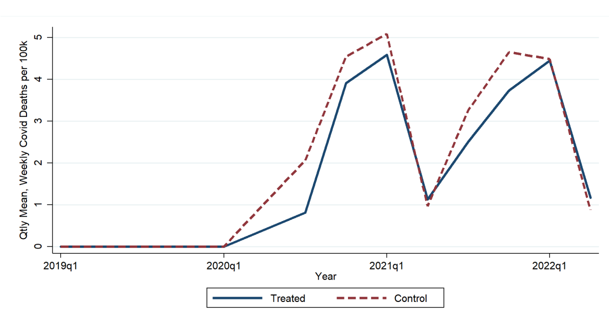
(b) Oxford workplace closing



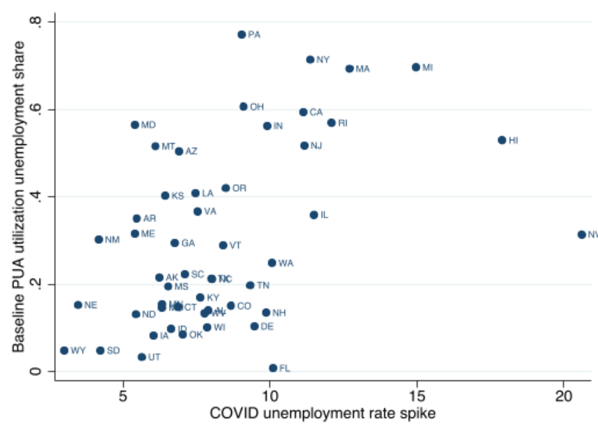
(c) Regular UI monetary threshold



(d) Average-quarter threshold



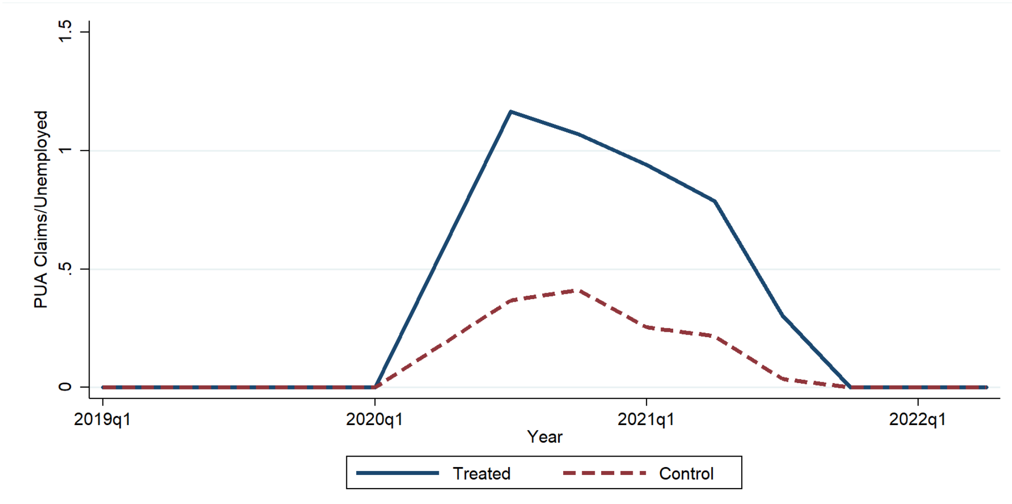
(e) Covid deaths per 100k



(f) Covid unemployment-rate spike

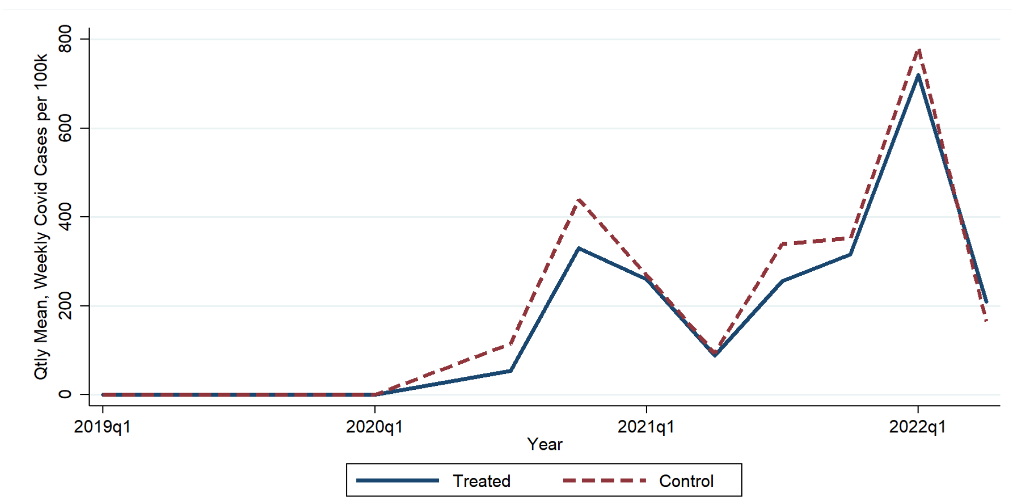
Notes: Panels (a)–(d) and (f) relate state-level baseline PUA utilization to the indicated control variable. The two Oxford panels summarize average policy restrictions over the main PUA period. Panels (c) and (d) show the raw and average-quarter versions of the 2020 regular UI monetary threshold. Panel (e) reproduces the treated-control Covid-death series used elsewhere in the Appendix, while panel (f) reports the cross-state unemployment-spike comparison based on the increase in the unemployment rate from 2019 Q4 to

Figure 25: **PUA Claims/Total Unemployed**



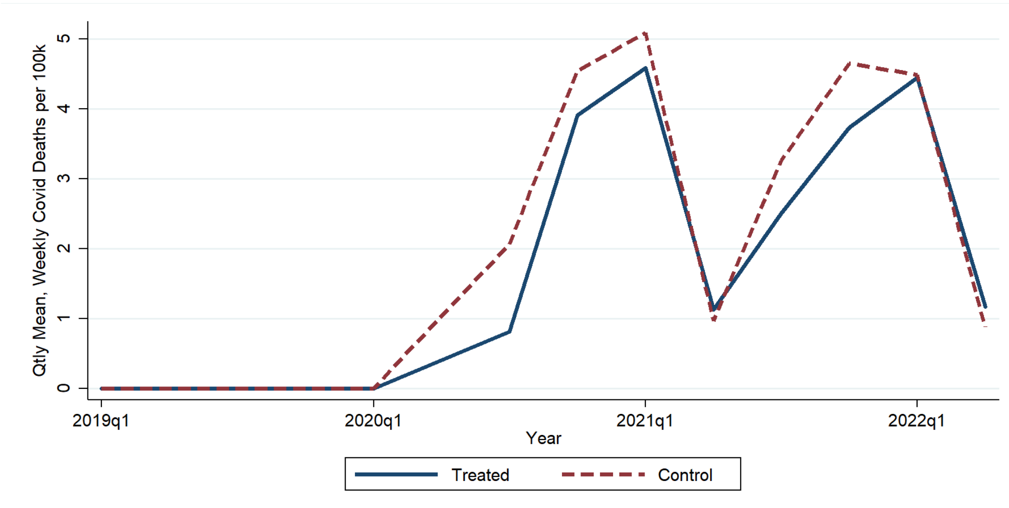
Notes: The mean value across each state group of each state's quarterly average of weekly PUA claims as a fraction of its total unemployed population (measured monthly via CPS). "Treated" states correspond to the top half of states by average PUA claim share and "Control" states correspond to the bottom half of states by their average PUA claim share.

Figure 26: Qtly Mean, Weekly Covid Cases per 100k



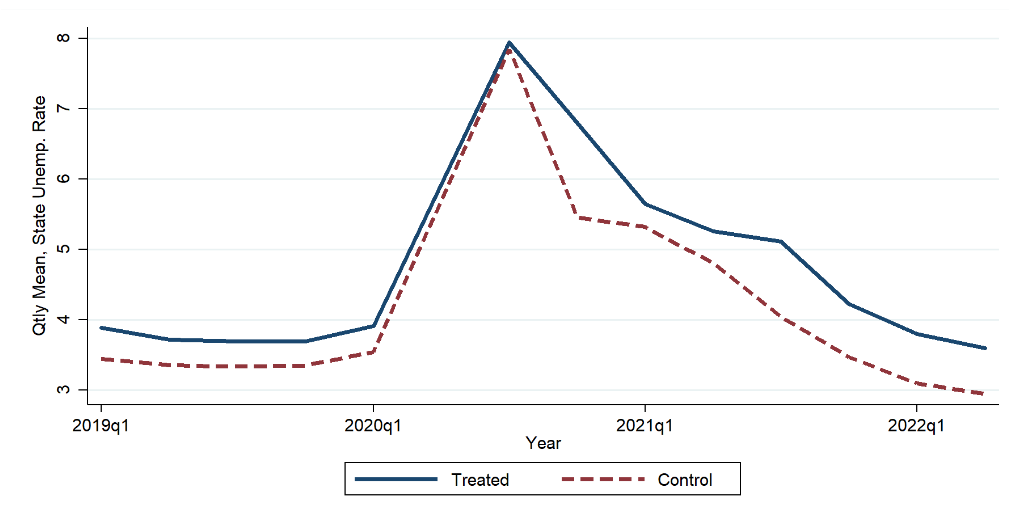
Notes: The mean value across each state group of each state's quarterly average of weekly reported Covid cases per 100,000 persons. "Treated" states correspond to the top half of states by average PUA claim share and "Control" states correspond to the bottom half of states by their average PUA claim share. Covid case data was taken from CDC Aggregate Covid-19 Case & Death Count data, accessed at the following link: CDC Weekly U.S. COVID-19 Cases & Deaths by State

Figure 27: Qtly Mean, Weekly Covid Deaths per 100k



Notes: The mean value across each state group of each state's quarterly average of weekly reported Covid deaths per 100,000 persons. "Treated" states correspond to the top half of states by average PUA claim share and "Control" states correspond to the bottom half of states by their average PUA claim share. Covid death data was taken from CDC Aggregate Covid-19 Case & Death Count data, accessed at the following link: CDC Weekly U.S. COVID-19 Cases & Deaths by State

Figure 28: Quarterly Mean, State Unemployment Rate



Notes: The mean value across each state group of each state's quarterly average monthly unemployment rate. "Treated" states correspond to the top half of states by average PUA claim share and "Control" states correspond to the bottom half of states by their average PUA claim share. The state-level unemployment rate is reported in the Bureau of Labor Statistics Local Area Unemployment Statistics Data, accessed at the following link: https://www.bls.gov/lau/data.htm

B CPS

B.1 Re-Employment Wages of UI-Eligible vs. Ineligible Workers in the CPS

This appendix reports a complementary check of the BAM wage findings using CPS rotation data. Unlike BAM, which is restricted to UI claimants, the CPS allows us to compare re-employment wages between unemployed workers we classify as UI-eligible and those we classify as ineligible. We find a 9 percent increase in the eligible–ineligible wage premium during the Covid period, consistent with the BAM elasticity estimates in Section 3.6.

Recall that earnings are only measured for the outgoing rotation. For the purpose of measuring earnings following a transition to employment, we use the first rotation (the first 4 interviews) and the second rotation (the last 4 interviews) independently. In other words, even if we know that an individual transited into employment during their 8-month hiatus period, we do not consider earnings observed in interview 8 as associated with that transition. We exclude these because (1) earnings observed in interview 8 are 8 to 12 months removed from that transition and (2) there may have been more than one labor market transition over that period.

Given this measurement structure, our sample is restricted to individuals who are interviewed in March—so that eligibility can be assessed as discussed above—and who experience a transition to employment during either the first or the second rotation. Looking back at Table 6, respondents whose first (fifth) interview is in March can transit to employment in their second (sixth), third (seventh) or fourth (eighth) interview, and their earnings will be measured in their fourth (eighth) interview. Similarly for respondents whose first interview is in February or January. For respondents whose first interview is in December, since we can only compute eligibility starting with their second (sixth) interview, they can only transit to employment in their third (seventh) or fourth (eighth) interview.

The FPUC program, which supplemented benefit amounts by $600 a week, started in April 2020. Thus, looking back at Table 6, individuals could only transit to employment after having received extra benefits in May or June 2020, leaving a very limited sample. In addition, the number of transitions to employment during those two months was atypically small. For these reasons, results that include 2020 observations should be interpreted cautiously.

Fortunately, the Federal Covid Relief Bill (signed into law in December 2020) resurrected the FPUC program, with a \$300 per week supplement, starting the week of December 26, 2020. Accordingly, all individuals who transitioned to employment after January 2021 potentially received the \$300 supplement during the unemployment spell immediately preceding their return to work. Furthermore, through the American Rescue Plan Act (ARPA), this supplement remained in place at least until June 2021: no state ended the program prior to June 2021.[59] Therefore, all transitions to employment that we observe in 2021 and for which we can measure eligibility, as shown in Table 6, potentially occurred after having received unusually high unemployment benefit amounts.

We use a difference-in-difference regression to estimate the causal effect of UI eligibility on wages by comparing changes in earnings for eligible and ineligible individuals before and during the Covid relief period. From the eligibility methodology outlined above, unemployed individuals whom we classify as ineligible for UI benefits fall into one of several categories: new entrants into the labor market; re-entrants into the labor market; individuals who quit their last job; or individuals who have exhausted benefits.[60] By construction, these workers do not qualify for UI benefits, and in principle should be largely unaffected by the increase in UI generosity in 2020 and 2021.

Accordingly, we estimate:

$$\begin{aligned} w_{it} = {} & \alpha_0\, \mathbb{I}(\text{eligible}_i) + \alpha_1\, \mathbb{I}(\text{FPUC}_{2020,t}) + \alpha_2\, \mathbb{I}(\text{FPUC}_{2021,t}) + \beta X_{it} \\ & + \boldsymbol{\gamma_{2020}}\, \mathbb{I}(\text{eligible}_i)\, \mathbb{I}(\text{FPUC}_{2020,t}) + \boldsymbol{\gamma_{2021}}\, \mathbb{I}(\text{eligible}_i)\, \mathbb{I}(\text{FPUC}_{2021,t}) + \varepsilon_{it}. \end{aligned} \tag{13}$$

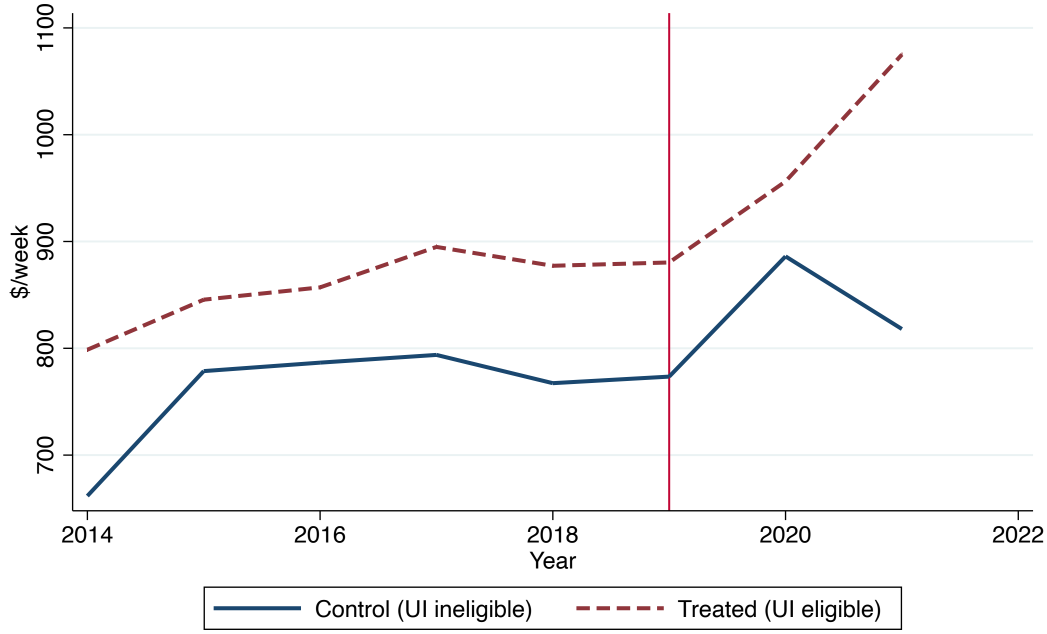
where w is either the level or the log of the weekly wage observed at the transition to employment, $\mathbb{I}(\text{FPUC}_{2020})$ and $\mathbb{I}(\text{FPUC}_{2021})$ are indicators for the Covid relief months in 2020 and 2021, respectively, during which FPUC was in place, and X includes sex, race, education, age, and indicators for industry and occupation.[61] Under the identifying assumption that,

[59] The first states to end the program, Arkansas, Iowa, Mississippi and Missouri, did so as of June 12, 2021.

[60] We use unemployment duration to classify people with more than 26 weeks of unemployment as ineligible during normal times and ignore duration while PUA was active. Individuals whom we initially classify as ineligible but who report having collected UI benefits (as measured in Section 4.2) are reclassified as eligible.

[61] We do not include unemployment duration in our baseline specification because duration is an outcome that UI generosity plausibly affects. Conditioning on duration would therefore induce post-treatment bias and isolate only the direct effect of benefits on wages. In addition, durations post-Covid differ sharply from those observed pre-Covid, as many individuals were newly unemployed at the onset of the pandemic while others experienced unusually long spells. Results that include unemployment duration are nevertheless reported in Appendix B.3.

Figure 29: Mean Weekly Wage after Transition to Employment



Notes: Measure of the average wage of eligible and ineligible unemployed people upon transiting to employment.

absent the pandemic-era UI expansions, wages for eligible and ineligible workers would have followed parallel trends conditional on X, the coefficients γ_{2020} and γ_{2021} measure the change in the *wage premium* that eligible individuals command over ineligible individuals during the Covid (FPUC) relief period in 2020 and 2021, respectively, relative to pre-Covid.[62]

Figure 29 plots average weekly wages for eligible and ineligible individuals upon transiting to employment. Prior to the Covid relief period, the gap between the two series is stable. During the FPUC relief period, wages rise for both groups, but the increase is noticeably more pronounced for eligible workers in 2021.

Table 19 reports the corresponding regression estimates. Columns (1) and (2) use the full 2014-2021 sample. Without year fixed effects, the wage premium for eligible workers during the Covid period increased by about $90, though the effect becomes statistically insignificant once year fixed effects are included. As discussed above, however, 2020 is a peculiar year in which our sample is limited.

[62]Wages are trimmed at the top (5%) and the bottom (5%) to avoid outliers.

Table 19: Diff-in-Diff Estimates of the Eligible Wage Premium

	Regression Equation (13)		Exclude 2020	
	(1)	(2)	(3)	(4)
Wage level				
Eligible × FPUC_{2020}	-131.1	-140.5		
	(141.3)	(142.2)		
Eligible × FPUC_{2021}	129.8***	120.2***	73.34**	64.34**
	(31.62)	(31.51)	(30.08)	(26.84)
Log wage				
Eligible × FPUC_{2020}	-0.133	-0.144		
	(0.114)	(0.116)		
Eligible × FPUC_{2021}	0.0753**	0.0638*	0.112***	0.0911***
	(0.0345)	(0.0340)	(0.0336)	(0.0320)
Year fixed effects	No	Yes	No	Yes
N	7,523	7,523	6,719	6,719

Notes: Each column reports estimates from a separate difference-in-differences regression. Columns (1) and (2) estimate equation (13) with separate Covid/FPUC indicators for 2020 and 2021, without and with year fixed effects, respectively. Columns (3) and (4) re-estimate the baseline specification excluding data from 2020, again without and with year fixed effects. Standard errors are clustered at the state level. Additional controls include sex, age, education, race, industry, and occupation.

To focus on the better-measured 2021 extension of FPUC, columns (3) and (4) re-estimate the specification excluding 2020. In this case, the wage premium for eligible workers during the Covid relief period increases by about \$65 and is statistically significant. We interpret the log-wage estimate in column (4)—a 9.1 percent increase in the eligible-ineligible wage gap in 2021, when the supplement was \$300 per week—as our preferred CPS estimate of the wage response.

Several caveats are in order. First, 2020 is both data-sparse and conceptually unusual: many unemployment spells began before the FPUC supplement was in place, and labor market conditions changed extremely rapidly. For these reasons, we place more weight on the 2021 estimates. Second, our eligibility measure is imperfect, especially during the early months of the pandemic: some individuals we classify as ineligible may in fact have received benefits, while some eligible individuals may not have filed (as is also the case in normal times). It is nevertheless notable that the roughly 9 percent increase in the eligible–ineligible wage premium we estimate in the CPS for 2021 lies within the range implied by our reservation-wage analysis using BAM data—namely, 7.6 to 12.4 percent (Section 3.6).

B.2 Alternative Take-up Probit Specifications

To reinforce the notion that the amount of benefits individuals expect to receive is key to the take-up decision, we redo our Probit analysis using regular benefits instead of total benefits, which include the supplemental amounts specified by the FPUC program. The results, displayed in Figure 30, show that the predicted take-up rate does not follow the actual take-up rate when the benefit amount does not include the extra benefits from the FPUC program during the Covid-19 relief period.

We also replicate Figures 11(a) and 11(b) adding unemployment duration as a control variable. For reasons alluded to in the main text, it is not clear that unemployment duration should be included as a control given the unusual nature of this variable in the post-Covid period. That said, Figures 31 and 32 show that the main results are unaffected by the inclusion of unemployment duration as a control. Our conclusion remains that the decision to file for and claim unemployment benefits is closely linked to the amount one expects to receive should the claim be approved.

B.3 Alternative Wage Premia DiD Specifications

Table 20 replicates the results from Table 19 adding unemployment duration as a control. Because unemployment duration is itself affected by UI generosity, this specification conditions on a post-treatment outcome and therefore isolates the direct effect of benefits on wages. Qualitatively, this addition leaves our main conclusion intact, in the sense that the change in the wage premium remains modest. The log-wage estimate in column (4)—a 7.6 percent increase in the eligible–ineligible wage gap in 2021—is slightly smaller than our preferred estimate of the wage response of 9.1 percent reported in the main text without duration controls.

Figure 30: Actual and predicted Take-up Rate with regular benefits

Notes: The take-up rate is the fraction of individuals who are eligible to receive regular UI benefits who report having received UI benefits the previous year. The predicted take-up rate uses the coefficient of a Probit regression to predict the take-up rate out of sample for 2020 and 2021.

Figure 31: Actual and predicted Take-up Rate without benefits

Notes: The take-up rate is the fraction of individuals who are eligible to receive regular UI benefits who report having received UI benefits the previous year. The predicted take-up rate uses the coefficient of a Probit regression to predict the take-up rate out of sample for 2020 and 2021.

Figure 32: Actual and predicted Take-up Rate with benefits

Notes: The take-up rate is the fraction of individuals who are eligible to receive regular UI benefits who report having received UI benefits the previous year. The predicted take-up rate uses the coefficient of a Probit regression to predict the take-up rate out of sample for 2020 and 2021.

Table 20: Diff-in-Diff Estimates of the Eligible Wage Premium

	Regression Equation (13)		Exclude 2020	
	(1)	(2)	(3)	(4)
Wage level				
Eligible $\times$ FPUC_{2020}	-148.2	-152.0		
	(134.5)	(130.0)		
Eligible $\times$ FPUC_{2021}	106.0***	103.0***	98.10**	26.47
	(32.36)	(32.05)	(33.82)	(41.37)
Log wage				
Eligible $\times$ FPUC_{2020}	-0.156	-0.161		
	(0.109)	(0.110)		
Eligible $\times$ FPUC_{2021}	0.0408	0.0368	0.148***	0.0764**
	(0.0371)	(0.0362)	(0.0366)	(0.0356)
Year fixed effects	No	Yes	No	Yes
N	3,697	3,697	3,144	3,144

Notes: Each column reports estimates from a separate difference-in-differences regression. Columns (1) and (2) estimate equation (13) with separate Covid/FPUC indicators for 2020 and 2021, without and with year fixed effects, respectively. Columns (3) and (4) re-estimate the baseline specification excluding data from 2020, again without and with year fixed effects. Standard errors are clustered at the state level. Additional controls include sex, age, education, race, industry, occupation, and unemployment duration.

C Formal Argument for the Effect of Benefit Increases on Collectors' Wages

Recall equation (12)

$$\frac{dw^C}{db} = \underbrace{\frac{\int_0^{\varepsilon^*} x^e(\varepsilon)\frac{dw(\varepsilon)}{db}d\varepsilon}{\int_0^{\varepsilon^*} x^e(\varepsilon)\,d\varepsilon}}_{A} + \underbrace{\frac{\int_0^{\varepsilon^*} \frac{dx^e(\varepsilon)}{db}\left(w(\varepsilon)-w^C\right)d\varepsilon}{\int_0^{\varepsilon^*} x^e(\varepsilon)\,d\varepsilon}}_{B} + \underbrace{\frac{x^e(\varepsilon^*)\left(w(\varepsilon^*)-w^C\right)}{\int_0^{\varepsilon^*} x^e(\varepsilon)\,d\varepsilon}}_{C}$$

where

$$x^e(\varepsilon) = \frac{p(\varepsilon)f(\varepsilon)}{p(\varepsilon)+\delta}$$

Let's rewrite the second term B as

$$\begin{aligned}
\frac{\int_0^{\varepsilon^*} x^e(\varepsilon)\frac{\partial \log x^e(\varepsilon)}{\partial b}\left(w(\varepsilon)-w^C\right)d\varepsilon}{\int_0^{\varepsilon^*} x^e(\varepsilon)\,d\varepsilon} &= \mathbb{E}\left[\frac{\partial \log x^e(\varepsilon)}{\partial b}\left(w(\varepsilon)-w^C\right)\right] \\
&= \mathbb{E}\left[\frac{\partial \log x^e(\varepsilon)}{\partial b}\right]\mathbb{E}\left[\left(w(\varepsilon)-w^C\right)\right] \\
&\quad + Cov\left[\frac{\partial \log x^e(\varepsilon)}{\partial b}\left(w(\varepsilon)-w^C\right)\right] \\
&= Cov\left[\frac{\partial \log x^e(\varepsilon)}{\partial b}\left(w(\varepsilon)-w^C\right)\right]
\end{aligned}$$

where the $\mathbb{E}[\cdot]$ is expectation with respect to distribution $\frac{x^e(\varepsilon)}{\int_0^{\varepsilon^*} x^e(\varepsilon)d\varepsilon}$, i.e. average over collectors. The term $\mathbb{E}\left[\left(w(\varepsilon)-w^C\right)\right] = 0$ by the definition of w^C. Therefore, the term B is negative if and only if $Cov\left[\frac{\partial \log x^e(\varepsilon)}{\partial b}\left(w(\varepsilon)-w^C\right)\right] < 0$. Since, $w(\varepsilon)-w^C$ is decreasing in ε, it is sufficient if $\frac{\partial \log x^e(\varepsilon)}{\partial b}$ is increasing in ε. In other words we want this term to be less negative for lower ε.

We can now derive an explicit condition for this to hold. Note that

$$\frac{\partial \log x^e(\varepsilon)}{\partial b} = \left(\frac{1}{p(\varepsilon)} + \frac{1}{p(\varepsilon)+\delta}\right)\frac{\partial p(\varepsilon)}{\partial b}$$

From the first-order condition of the optimal search problem

$$p'\left(\theta\left(U^C\left(\varepsilon\right)\right)\right) = k\frac{1-\beta\left(1-\delta\right)}{y-\left(1-\beta\right)U^C\left(\varepsilon\right)}$$

Therefore

$$p\left(\varepsilon\right) \equiv p\left(\theta\left(U^C\left(\varepsilon\right)\right)\right) = p\left(p'^{-1}\left(k\frac{1-\beta\left(1-\delta\right)}{y-\left(1-\beta\right)U^C\left(\varepsilon\right)}\right)\right).$$

A sufficient condition for $\frac{\partial \log x^e(\varepsilon)}{\partial b}$ to be increasing in ε is that the function $p\left(p'^{-1}\left(k\frac{1-\beta(1-\delta)}{y-(1-\beta)U^C(\varepsilon)}\right)\right)$ be decreasing and concave in $U^C\left(\varepsilon\right)$.

Now recall equation (9)

$$R'\left(U^C\left(\varepsilon\right)\right) = -\frac{1-\beta}{1-\beta\left(1-\delta\right)}p\left(\theta\left(U^C\left(\varepsilon\right)\right)\right) < 0$$

Using the Bellman equation of the collectors, equation (11) and the envelope condition (9)

$$\frac{\partial U^C\left(\varepsilon\right)}{\partial b} = \left(\frac{1}{1-\beta}\right)\frac{1}{1+\frac{\beta}{1-\beta(1-\delta)}p\left(\theta\left(U^C\left(\varepsilon\right)\right)\right)} > 0$$

Value of unemployment for collectors is increasing in UI benefit. Moreover, we already know (from FOC of the optimal search problem) that $\theta'\left(U^C\left(\varepsilon\right)\right) < 0$. We also know that value of collection is decreasing in the collection cost ε. Therefore,

$$\frac{\partial^2 U^C\left(\varepsilon\right)}{\partial b \partial \varepsilon} = -\left(\frac{1}{1-\beta}\right)\frac{\frac{\beta}{1-\beta(1-\delta)}p'\left(\theta\left(U^C\left(\varepsilon\right)\right)\right)\theta'\left(U^C\left(\varepsilon\right)\right)}{\left(1+\frac{\beta}{1-\beta(1-\delta)}p\left(\theta\left(U^C\left(\varepsilon\right)\right)\right)\right)^2}\frac{\partial U^C\left(\varepsilon\right)}{\partial \varepsilon}$$

Using these monotonicity conditions and the sign of cross derivative we can show that

$$\frac{\partial^2 p\left(\varepsilon\right)}{\partial b \partial \varepsilon} > 0$$

This is sufficient to establish that

$$\frac{\partial \log x\left(\varepsilon\right)}{\partial b} = \left(\frac{1}{p\left(\varepsilon\right)} + \frac{1}{p\left(\varepsilon\right)+\delta}\right)\frac{\partial p\left(\varepsilon\right)}{\partial b}$$

is increasing in ε.

D Quantitative Model

The setup in this section closely follows the theoretical framework outlined above, but incorporates several modifications to better connect the model to the data. First, we introduce on-the-job search, along the lines of Menzio and Shi (2010) and Gervais et al. (2022). This extension allows us to compute the average wage at the time of job finding following an unemployment spell—an object we measure empirically. Second, to facilitate numerical computation, we assume that unemployed workers draw a new benefit collection cost at the beginning of each unemployment spell. This cost remains fixed for the duration of the spell, but is redrawn (i.i.d.) in the event of a subsequent unemployment spell.[63] Finally, we distinguish between UI-eligible and UI-ineligible workers and allow for unemployment benefits to expire stochastically.

Submarkets

There is a continuum of submarkets indexed by the expected lifetime utility x that the firms offer to the workers, $x \in X = [\underline{x}, \bar{x}]$, with $\underline{x} < v(d) / (1-(1-\delta)\beta)$ and $\bar{x} > v(y) / (1-(1-\delta)\beta)$. Let $\theta(x) \geq 0$ be the market tightness, i.e., the ratio of vacancies created by the firm to the workers looking for a job in submarket x.

Every period an individual who has the opportunity to search decides in which submarket to direct his search. While all individuals who have been unemployed for at least one period have the opportunity to search, employed workers only have the opportunity to search with probability $\lambda_e \in (0, 1]$. Also, during the search stage, a firm chooses how many vacancies to create and where to locate them. The cost of maintaining a vacancy for one period is $k > 0$. Both workers and firms take the market tightness $\theta(x)$ in all submarkets as given.

[63] If we assume collection costs are permanent types, as we do in the simple model of section 5.1, then the type becomes a state variable and greatly complicates the firm's optimal problem.

Workers

Workers are either eligible ($i = E$) or ineligible ($i = I$) to collect UI benefits. The return to search for a worker with eligibility status $i \in \{I, E\}$ is

$$R^i(V) \equiv \max_{x \in X} p\left(\theta^i(x)\right)(x - V), \quad i \in \{E, I\} \tag{14}$$

This search problem results in optimal search strategy $m^i(V)$ and optimal job finding rate $\tilde{p}^i(V) \equiv p(\theta^i(m^i(V)))$. The trade-off that workers face is that submarkets with higher lifetime utility are associated with lower market tightness and therefore lower probability of finding a job.

If an individual is ineligible to collect unemployment benefits at the beginning of an unemployment spell, that individual will remain ineligible for the entire spell. Let U^I denote the value of being unemployed and ineligible:

$$U^I = v(d) + \beta\left(U^I + R^I\left(U^I\right)\right). \tag{15}$$

Individuals who are eligible must decide whether they collect or not at the beginning of their unemployment spell. Let $U^E(\varepsilon) \equiv \max\left\{U^N, U^C(\varepsilon)\right\}$ denote the lifetime utility of an eligible unemployed worker with UI benefit collection cost ε. Here, U^N is the utility to the worker if he chooses not to collect and $U^C(\varepsilon)$ is the utility if he chooses to collect. Since ε is constant during any unemployment spell, a worker who decides not to collect benefits at the beginning of an unemployment spell will continue to choose not to collect during the entire unemployment spell.[64] To capture the notion that UI benefit eligibility expires, let ψ denote the probability that an unemployed worker loses benefit eligibility and thus becomes UI ineligible. Note that, for collectors this means losing UI benefits (e.g., through expiration or administrative exit), and for non-collectors this means losing the option to collect benefits in the current spell. Therefore, the value of being eligible and not to collect is:

$$U^N = v(d) + \beta(1-\psi)\left(U^N + R^E\left(U^N\right)\right) + \beta\psi\left(U^I + R^I\left(U^I\right)\right). \tag{16}$$

Let $U^C(\varepsilon)$ denote the value function of an eligible individual who chooses to collect UI

[64] Note that in general individuals must choose to file for benefits soon after having been separated from their employer.

benefits. His lifetime utility consists of the value of consuming unemployment benefits b, incurring utility cost ε, plus the value of being unemployed (and potentially remaining eligible) and searching for a job next period:

$$U^C(\varepsilon) = v(b) - \varepsilon + \beta(1-\psi)\left(U^C(\varepsilon) + R^E\left(U^C(\varepsilon)\right)\right) + \beta\psi\left(U^I + R^I\left(U^I\right)\right). \qquad (17)$$

Finally, let $U^E \equiv \int_{\underline{\varepsilon}}^{\bar{\varepsilon}} U^E(\varepsilon)\, dF(\varepsilon)$ denote the lifetime utility of an unemployed worker before the realization of UI collection cost ε.

Firms

Once matched with a worker, firms offer contracts to workers. A contract specifies the current wage w and the worker's lifetime utility at the beginning of the next period V'. This future utility will be attained by an implicit sequence of future wages and unemployment benefits, which depend on a worker's future eligibility. We assume that every individual starts an employment spell as UI ineligible. After the first period of employment they become UI eligible with probability φ, reflecting the notion that UI eligibility is a function of past employment history. The firm chooses the contract to maximize expected lifetime profits $J^i(V)$ $(i \in I, E)$, while delivering the lifetime utility previously contracted (promise-keeping constraint). The problem of a firm matched with an ineligible worker with promised lifetime utility V is therefore given by

$$J^I(V) \equiv \max_{w,V'} \left\{ y - w + \beta(1-\delta)\left[(1-\varphi)\left(1-\lambda_e \tilde{p}^I(V')\right) J^I(V') + \varphi\left(1-\lambda_e \tilde{p}^E(V')\right) J^E(V')\right] \right\} \qquad (18)$$

subject to

$$V = v(w) + \beta\left[\delta\left((1-\varphi)U^I + \varphi U^E\right) + (1-\delta)\left(V' + \lambda_e((1-\varphi)R^I(V') + \varphi R^E(V'))\right)\right],$$

where it is assumed that workers eligibility status changes before the separation shock arrives.

Similarly, the problem of a firm matched with an eligible worker with promised lifetime utility V is given by

$$J^E(V) \equiv \max_{w,V'} \left\{ y - w + \beta(1-\delta)\left(1-\lambda_e \tilde{p}^E(V')\right) J^E(V') \right\} \qquad (19)$$

subject to

$$V = v(w) + \beta\left[\delta U^E + (1-\delta)\left(V' + \lambda_e R^E(V')\right)\right].$$

Market Tightness

Every period, a measure of firms choose whether to enter the labor market by opening a vacancy. Should it choose to enter, a firm posts how much lifetime utility it offers (i.e. chooses a submarket x) for all potential applicants to see. Since whether an individual is or would be UI eligible is public knowledge, firms choose whether to target their vacancies at eligible or ineligible individuals. The benefit of creating a type i vacancy, where $i \in \{E, I\}$, in submarket x is the product between the vacancy filling probability $q(\theta^i(x))$ and the value of meeting a worker $J^i(x)$. The cost of creating a vacancy is k. When the benefit of creating a vacancy in submarket x is strictly smaller than the cost, no vacancy is created in that submarket. When the benefit is strictly greater than k, it is optimal to create infinitely many vacancies. Therefore, free entry implies that the expected value of opening a vacancy cannot exceed the cost of creating one. In other words, in any submarket that is visited by a positive number of workers, the market tightness $\theta(x) \geq 0$ must be such that

$$q\left(\theta^i(x)\right) J^i(x) \leq k \tag{20}$$

with $\theta^i(x) > 0$ whenever $q(\theta^i(x))\, J^i(x) = k$, for $i = \{E, I\}$.

While the free entry condition must hold with equality for submarkets which are open in equilibrium (i.e. submarkets in which at least some individuals search), such need not be the case for unvisited submarkets. Following Acemoglu and Shimer (1999) and the subsequent literature, we assume that (20) holds with equality in all submarkets in a relevant range, that is, from the lowest submarket to the submarket where firms would just cover the cost of posting a vacancy with a job filling probability equal to one. Under this assumption, market tightness is a decreasing function of x over the relevant range.

This model shares many properties with the simpler framework presented earlier. In particular, there exists a threshold $\varepsilon^* = v(b) - v(d)$ such that all unemployed workers with $\varepsilon \leq \varepsilon^*$ choose to collect UI, while those with $\varepsilon > \varepsilon^*$ do not. The wage of collectors is monotonically decreasing in ε, with non-collectors earning the lowest wage. Similarly, job-finding rates for collectors are increasing in ε, and non-collectors face the highest job-finding rates.

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