Reservation Wage and Unemployment Benefits*

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Abstract

This paper examines how unemployment insurance (UI) generosity affects reservation wages, re-employment wages, and benefit take-up. Using data from the Benefit Accuracy Measurement (BAM) system, we estimate a cross-sectional elasticity of reservation wages with respect to UI benefits of approximately 0.02. During the pandemic, expanded benefits raised reservation wages by 8 to 12 percent, while the composition of UI recipients shifted notably toward younger workers, non-white individuals, and those in leisure and hospitality. CPS data corroborate the modest wage response, showing a 9 percent increase in the wage premium for UI-eligible individuals relative to ineligible ones. At the same time, take-up rose from roughly 30 to 40 percent, driven entirely by the increase in UI generosity. To interpret these patterns, we develop a directed search model with endogenous UI collection. The model highlights the role of the take-up margin in dampening the wage response to more generous benefits. These findings suggest that take-up is a key mechanism through which UI policy operates.

Keywords: Unemployment Benefits, Reservation Wage, CPS, BAM

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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 were expanded dramatically—most notably through the $600 weekly supplement introduced by the CARES Act in March 2020, and later a $300 weekly supplement under the Continued Assistance and American Rescue Plan Acts. These policy changes raise a natural question: how do more generous UI benefits affect the behavior of unemployed workers—specifically, the pass-through to reservation and re-employment wages, and the decision to collect benefits? We answer this question using data from the UI Benefit Accuracy Measurement (BAM) system and the Current Population Survey (CPS), alongside a directed search model with an explicit take-up margin. Our central finding is that the pass-through from benefits to wages is surprisingly weak, while take-up responds strongly to UI generosity—both in the data and in the model.

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 meet earnings and work history requirements. Eligibility rules vary by state, but generally require a minimum level of recent earnings and a qualifying separation, such as a layoff rather than a voluntary quit. Even among eligible individuals, however, not all collect benefits. Take-up depends on a range of factors, including anticipated re-employment, eligibility constraints, administrative or informational barriers, and the perceived value of receiving benefits.[1]

During the COVID-19 period, the level and scope of UI benefits were expanded dramatically through the Coronavirus Aid, Relief, and Economic Security (CARES) Act. The Federal Pandemic Unemployment Compensation (FPUC) program provided an additional $600 per week to all UI recipients from March through July 2020, and a later extension added $300 per week in early 2021. The Pandemic Unemployment Assistance (PUA) program extended UI eligibility to self-employed workers and others not typically covered. These policy changes affected both the generosity of benefits and the composition of workers receiving them, and 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

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

(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 this information with survey responses from claimants. Critically, the BAM survey includes a self-reported measure of each claimant's current reservation wage and their usual hourly wage in the job held prior to unemployment. These data allow us to compare reservation and prior wages among UI recipients, and to examine how this relationship evolves over time—particularly during the pandemic, when UI benefits were temporarily expanded.

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). In both groups, we observe a consistent pattern: reservation wages rise with prior wages but remain below them, and this relationship changes little over time—despite large, temporary increases in UI generosity.

Using the BAM paid claims sample, we use cross-sectional variation in benefit levels to estimate the elasticity of reservation wages with respect to UI benefits. 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. We also use a Blinder-Oaxaca decomposition to examine whether the relationship between benefits and reservation wages varied during periods of elevated UI generosity. The decomposition reveals partially offsetting effects. In 2020, the observed increase in reservation wages is almost entirely explained by compositional shifts among claimants, including a higher share of younger individuals and a lower representation of manufacturing workers.[3] In 2021, benefit expansions are associated with a 7 percent increase in reservation wages relative to the pre-pandemic period, while observable characteristics alone would have predicted a decline of roughly 4 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 focus on the denied claims sample in BAM and compare reservation wages across states with high versus low PUA participation.[4] Treated states are defined as those with above-median PUA claim shares

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

[3] Note that we have limited coverage of the paid claims sample during 2020.

[4] The official Federal guidance (https://www.dol.gov/sites/dolgov/files/ETA/advisories/UIPL/2021/UIPL_16-

during the program period. An event study shows that reservation wages increased more in treated states relative to control states during the PUA period, coinciding with the availability of elevated benefits. As PUA expired, the difference between groups faded. These results suggest that increased access to benefits through PUA led to a moderate rise in reservation wages for workers who were 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 roughly 8 to 12 percent rise in reservation wages across specifications. Assuming a $300 increase on a base weekly benefit of $176 (a 170 percent increase), this range implies an elasticity of reservation wages with respect to benefits between 0.045 and 0.068. 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 offer only a partial view of the unemployed population and cannot distinguish whether changes in the composition of claimants reflect expanded eligibility or changes in claiming behavior. To address this, we turn to the CPS, which offers a broader snapshot of the unemployed, including both UI recipients and non-recipients. These data allow us to proxy for both eligibility and benefit receipt, and show that policy changes during the pandemic were associated with a substantial increase in the UI take-up rate.

We complement the BAM-based findings by using CPS data to compare realized re-employment wages between individuals likely eligible for UI and those who are not. Specifically, we measure the wage that individuals receive immediately upon transiting from unemployment to employment and define the wage premium as the difference in earnings between UI-eligible and ineligible individuals at the time of re-employment. Using a Differences-in-Differences (DiD) design, we estimate that higher UI benefits raised re-employment wages for eligible individuals by 9.1 percent during the COVID period, relative to similar ineligible individuals. This estimate falls within the range of wage increases observed using the denied

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)." In this context, PEUC refers to Pandemic Emergency Unemployment Compensation while EB refers to the Extended Benefit program.

claims sample from BAM.[5]

While the elasticity of wages with respect to UI benefits is modest, take-up responds strongly to benefit generosity. Using the CPS rotation group sample, we estimate a Probit model of UI receipt and show that including benefit levels as an explanatory variable is necessary to explain the observed rise in take-up during the pandemic. Models based only on demographic characteristics cannot explain the sharp increase in benefit receipt. These findings suggest that much of the behavioral response to UI policy occurs along the extensive margin of take-up, rather than through changes in reservation or re-employment wages.

To make sense of these patterns, we develop a parsimonious economic environment with directed search, featuring an endogenous unemployment benefit take-up decision inspired by Auray et al. (2019). Specifically, individuals differ in the cost of filing for unemployment insurance, which makes take-up endogenous. More importantly, the model allows the take-up rate to respond to the generosity of the unemployment insurance system.

We show that the elasticity of wages to changes in UI benefits is muted by the take-up margin: as benefits increase, the pool of UI collectors expands, and the additional collectors consist of individuals with the lowest re-employment wages. A quantitative version of the model demonstrates that incorporating an endogenous take-up decision is essential to replicate our empirical findings.

Our main message—that the relationship between unemployment benefits and wages at the transition to employment is relatively weak—complements findings from the broader literature that emerged following the outset of the COVID-19 pandemic. That literature generally concludes that despite the scale of government intervention, its impact on incentive-related labor market behavior was modest.[6] 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 supplements expired. Michaud (2023) studies UI expansions

[5] This CPS estimate is robust to the inclusion of year fixed effects and controls for demographic characteristics, prior unemployment duration, occupation, and industry.

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

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.

Whereas most of the existing literature emphasizes the effects of UI generosity on job search behavior and employment transitions, we highlight a distinct set of margins: the impact of benefits on reservation and re-employment wages, and the role of endogenous take-up in mediating policy effects.

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 7 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 to 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. Furthermore, 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)). 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 amount of weekly benefits itself as some states set a maximum yearly benefit amount.[7] For the purpose of this paper, we use a uniform 26 weeks maximum duration across all States prior to the Covid relief period.[8]

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 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

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

[8] 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).

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 December 27, 2020, renewed the FPUC program by extending federal unemployment assistance to the tune of $300 per week through March 14, 2021.[9] 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 only available to individuals who applied for and were denied regular unemployment benefits, or who had exhausted their state UI benefits, including PEUC.[10]

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 simply 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

[9] 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.

[10] 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.

the requirement that the individual's unemployment was due to the 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 for 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.[11] 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 to and qualify for compensation under the PUA program.

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

[11]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.

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.[12]

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 of UI recipients in traditional datasets.[13] 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 are 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.

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 de-

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

[13]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.

nied 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.[14] 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 rational 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.[15] As shown below, reservation wages closely track usual hourly wages, albeit at a discount—a pattern consistent with Davis and Krolikowski (2024).

[14]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.

[15]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.

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.

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. We restrict our sample to claimants between 16 and 65 years of age. We Winsorize usual hourly wage and reservation wage at the 1$^{\text{st}}$ and 99$^{\text{th}}$ percentiles. We also Winsorize the weekly benefit amount at the 99$^{\text{th}}$ percentile. All values are deflated to 2021 dollars.

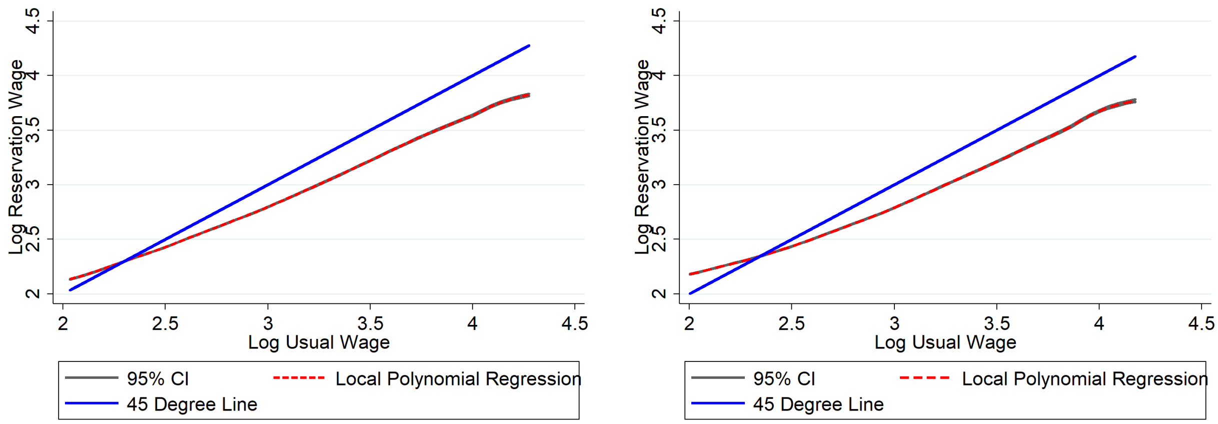
Our analysis uses both the paid claims sample and the denied claims sample. As we alluded to above, the design of the PUA program leads us to focus on the monetary denials subset of the denied claims sample. Table 1 displays summary statistics for the paid and denied claims samples, as well as the subset 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 less representation in the manufacturing industry.[16]

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

[16] Table 9 in Appendix B displays the share of each sample across NAICS 2-digit sectors.

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 percentile. Both variables are deflated to 2021 dollars. Sample sizes are sufficiently large that the 95% confidence intervals are barely visible in each plot.

against the 45 degree line, for the paid claims sample in Panel a and for the denied sample in Panel b.[17] 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.

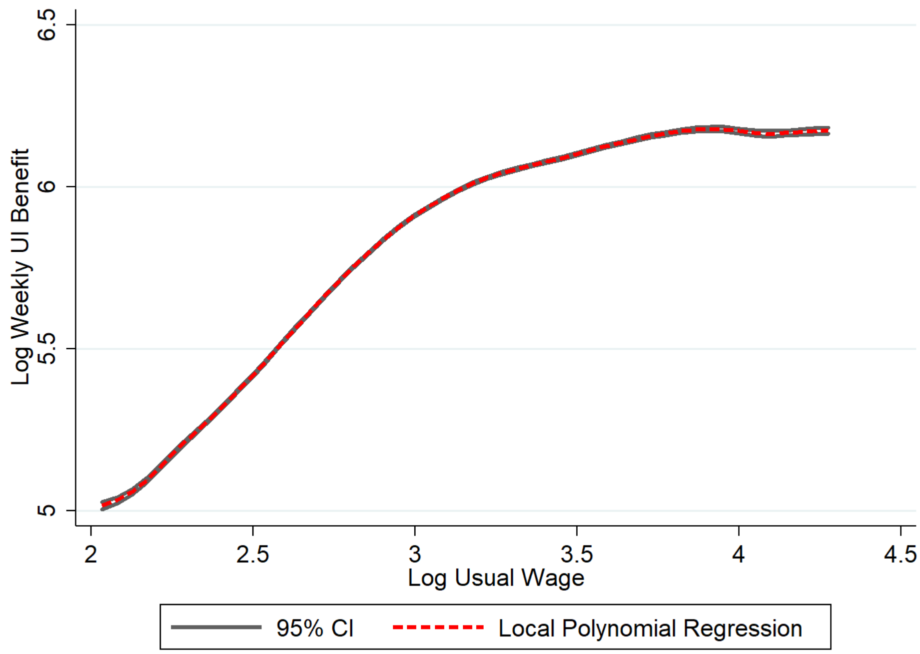
Figure 2 shows how UI benefits vary with past wages, revealing a strong nonlinear pattern relative to usual hourly wages. This pattern arises because UI payments are capped at a relatively low maximum weekly benefit amount, effectively flattening benefits beyond a certain wage threshold.

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.

[17] Table 10 in Appendix B presents results from linear regressions of $\ln(\tilde{w})$ on $\ln(w_{usual})$ with various controls.

Figure 2: UI Weekly Benefit and Usual Hourly Wage



Notes: Usual wage Winsorized at the 1st and 99th percentile. 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.

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:

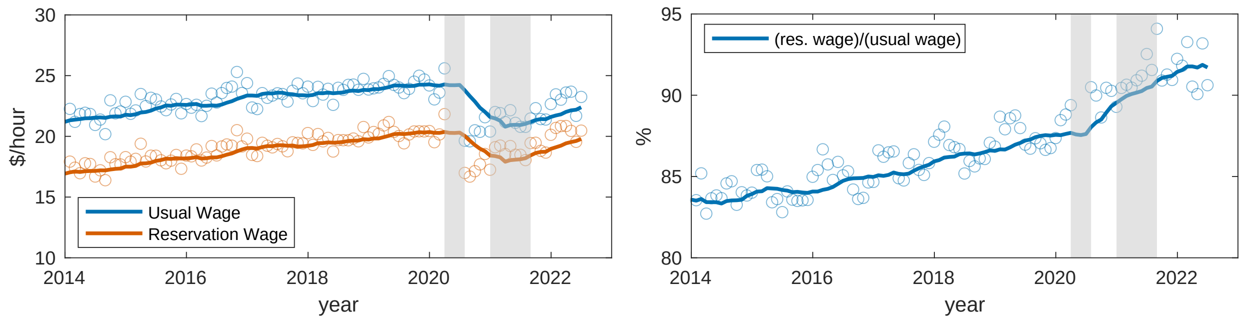
$$\ln(\tilde{w}) = \beta_0 + \beta_1 \ln(Benefit) + \beta_2 \ln(w_{usual}) + \beta_3 X + \varepsilon. \tag{1}$$

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 relationship between reservation wage and UI benefits, the coefficient on UI benefits becomes small and only significant at the 5% level once usual wage and other controls are added to the regression.[18] The same pattern emerges in the last two columns,

[18]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

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 percentile. 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.

which display results for the 2014–2022 period.[19]

Next, we examine how the relationship between reservation wages and UI benefits evolved during the period when the FPUC program was active. Figure 3a 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, contribut-

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 11 in Appendix B, are similar when restricting our sample to states where benefits are only a function of highest quarter earnings.

[19] While research on the elasticity of reservation wages to the generosity of UI benefits is very limited, Katz and Meyer (1990) estimate the elasticity of unemployment duration to UI benefits to range from 0.02 to 0.03, and Chetty (2008) finds elasticities between 0.01 and 0.04, attributing the effects primarily to liquidity rather than moral hazard. 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. 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: Results

	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
Observations	135170	132275	182862	177255

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.

ing to the decline in the average reservation wage shown in Figure 3a. 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 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 reservation wage.[20] Changes in the UI collecting population represented by paid claimants in BAM do not necessarily mirror changes to 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.[21] 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.[22] 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 other covariates, as in Table 2.[23] This regression is run separately for the pre-Covid period, the FPUC period in 2020, and the FPUC period in 2021.[24]

Table 3 shows that the difference in reservation wages between the pre- and post-Covid periods is almost entirely explained by changes in the explained component (endowments) in 2020. In other words, shifts in claimant composition account for nearly all of the observed decline in reservation wages. The small change in coefficients suggests that, had the composition of claimants remained constant, reservation wages would have increased by roughly

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

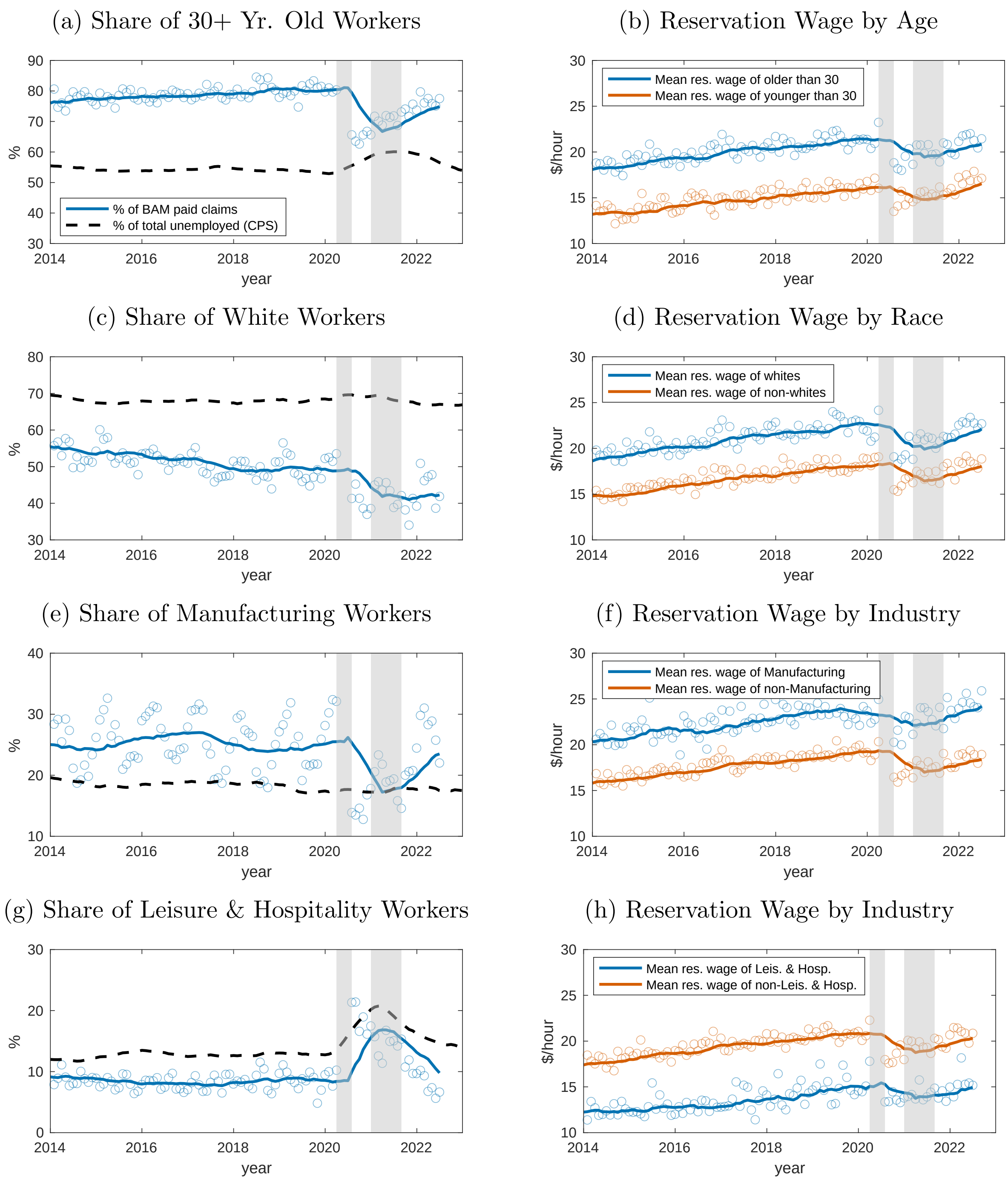
[21] The number of observations used in 2021 is 23,800.

[22] 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.

[23] 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 B Tables 12 and 13.

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

Figure 4: Composition changes in the Paid Claims Sample



(a) Share of 30+ Yr. Old Workers

(b) Reservation Wage by Age

(c) Share of White Workers

(d) Reservation Wage by Race

(e) Share of Manufacturing Workers

(f) Reservation Wage by Industry

(g) Share of Leisure & Hospitality Workers

(h) Reservation Wage by Industry

Notes: Reservation wage Winsorized at the 1st and 99th percentile, 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.

4%. By contrast, changes in composition pull the reservation wage down by nearly 13%. It is important to note, 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.

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

	Decomposition	95% CI
Pre Covid	2.714^{***}	[2.656,2.771]
2020 Expanded Benefit	2.798^{***}	[2.761,2.835]
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.

From the paid claims sample, we conclude that there is 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 underlying eligible unemployed population or changes in who selects into receiving UI among the eligible unemployed. In Section 4 we turn to the CPS data to distinguish between these

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

	Decomposition	95% CI
Pre Covid	2.851***	[2.805,2.896]
2020 Expanded Benefit	2.798***	[2.761,2.835]
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.

possibilities In particular, we demonstrate that higher UI benefits explain the increase in take-up rates, pointing to benefit-driven selection into UI as a key driver of 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 monetary denials sample.

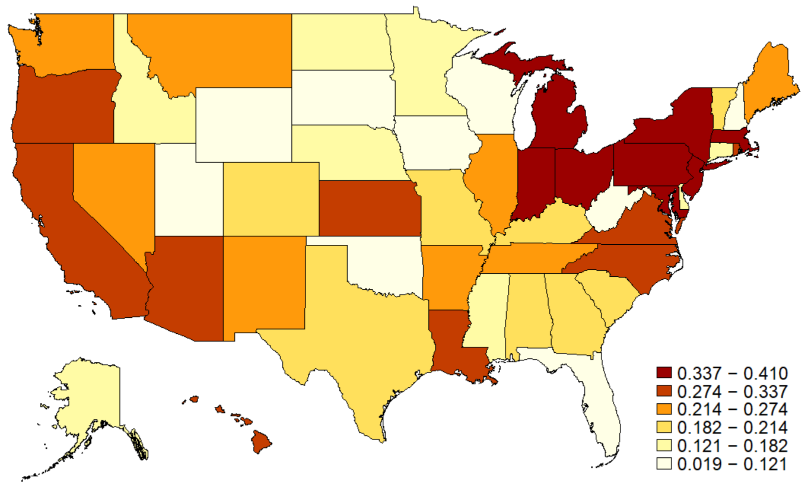
3.4 Reservation Wage and UI Benefits: Evidence from the Denied Claims Sample

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.[25] 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

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

Figure 5: PUA Claims as Share of Total UI Claims



Notes:

variation largely represents idiosyncratic state-level differences in the usage and availability of PUA as a policy.[26] 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. We exploit this variation in state-level PUA utilization to estimate the causal effect of extending UI coverage 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 plot each state's PUA share 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.[27]

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

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

[27]The average share of PUA claims as a share of unemployed workers during the PUA period exhibits similar variation, as shown in Figure 17 of Appendix B.

workers. We measure the treatment effect as the difference in average reservation wages between individuals i in a "treated" state P and those in a "control" state C at time t, following the framework in Callaway and Sant'Anna (2021):

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

We are interested in the Time 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:

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

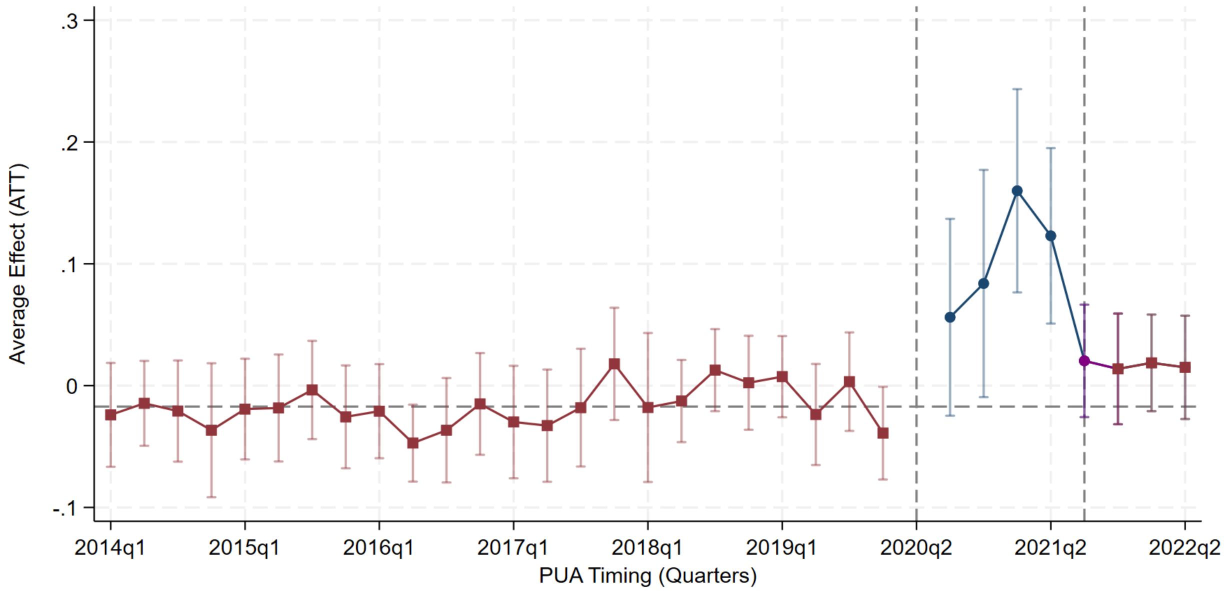
Here, treatment timing does not vary across states because PUA was a federal program rolled out simultaneously in all states.[28] Our controls include the log of usual hourly wage, age bins, sex, race/ethnicity, and NAICS sectors.

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. 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.[29] We further assume that the treatment was not confounded by other state-level variations coinciding with the timing of PUA. Our event study plot provides a useful pre-test by showing that pre-trends are identical for treated and control states. Possible confounding variables are discussed at the end of the section.

[28] As emphasized in Section 2, some states chose to end the PUA program as early as July 2021. While the treatment is considered permanent in this specification, we will explicitly consider time variation in treatment intensity below.

[29] 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 such as search congestion or local equilibrium effects, such as those measured in Doniger and Toohey (2022).

Figure 6: **Monetary Denials and PUA**



Notes: Control group is states with below median level of PUA claim share (.214) during PUA period. Treatment group is states with below 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.

It is important to note that our control group is not entirely untreated, because all states registered at least some PUA claims. To test the robustness of our state selection, we redefine treatment and control groups using various quartile thresholds in Appendix B.3.1. We also present estimates later in this section using a fixed-effects specification to allow for heterogeneity in both the timing and magnitude of treatment at the state level.

Figure 6 displays the coefficients of our event study 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 moderate increase in reservation wages.

While these results suggest that expanding eligibility (and benefits) raises the reservation wage, they are not readily interpretable as an elasticity. To address this, we use a slightly modified specification building on our event study regression, incorporating fixed effects and exploiting quarterly variation in treatment intensity.

3.5 Continuous Treatment Specification

We would like to interpret the results of our difference-in-differences (DiD) specification as an elasticity of reservation wages with respect to an increase in UI benefit level. 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-period indicators.[30] The first timing indicator interacts PUA claim shares with a dummy variable for quarters 2020 Q3 and 2020 Q4, representing the period 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 period 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 periods, we can arrive at an elasticity of reservation wages with respect to UI benefits, *assuming PUA intensity is held constant*. We show the results of this specification in Appendix B.4. Including the continuous treatment measure in this way allows our estimate to account for variation at the intensive margin of PUA claim share. We use the quarterly fraction of state-level PUA claims over total unemployed individuals as our treatment variable. Since we are interested in an estimate of the marginal change in reservation wage from a change in expected benefits, we use this measure of total PUA claimants as a fraction of the unemployed population in the state as a closer measure of the intensity of PUA eligibility.[31] Accordingly, we consider the following regression specifications:

$$Y_{it} = \lambda_t + \alpha_t PUA_{it} * period_t + \beta X_{it} + \varepsilon_{it},$$

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

[31] In Appendix B, we also show results using the PUA claim share as a fraction of total UI claims, as used in the previous subsection, to define treatment/control, as well as PUA claims as a share of the flow of denied claims. We arrive at estimates of similar magnitude.

where PUA_{it} is the quarterly average of the state's PUA claims as a fraction of the unemployed population. 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 an approximately 8% rise in reservation wages.

Our specification requires that other variables are not 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 due to lockdowns. In this case, weaker labor markets could lead to lower reservation wages due to reduced job opportunities, potentially biasing our estimates downward by 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 from expanded UI availability. To address these concerns, we include variables related to both factors 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.[32]

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

$$Y_{it} = \lambda_t + \alpha_t PUA_{it} * qtr_t + \delta_t Covid_{it} * qtr_t + \gamma_t Urate_{it} * qtr_t + \beta X_{it} + \varepsilon_{it},$$

where all these variables are as defined in the previous section. Similar to our event study and previous specifications, the coefficient on PUA is not significant (the period when only PUA is active), and the coefficient PUA + FPUC is significant and around 10%.

[32]Figures in Appendix B.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.

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

	(1) $ln(\tilde{w})$	(2) $ln(\tilde{w})$	(3) $ln(\tilde{w})$
PUA	0.0231	0.0289	-0.00759
	(0.59)	(0.84)	(-0.20)
PUA + FPUC	0.0992**	0.108***	0.116*
	(3.34)	(3.79)	(2.67)
Phase Out	0.0139	0.0107	-0.0146
	(0.33)	(0.26)	(-0.40)
Qtly Controls	No	Yes	No
Qtly Controls x Time	No	No	Yes
N	33868	33868	33868

t statistics in parentheses

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

Notes: PUA corresponds to the coefficient α_t for 2020 Q3, Q4 since data is not available for 2020 Q2. PUA + FPUC corresponds to α_t for 2021 Q1 and Q2 during which PUA and FPUC were active. Phase Out corresponds to α_t for 2021 Q3, during which some states phased out pandemic-era unemployment programs. All pandemic UI programs ended in September of 2021.

The magnitude of this last coefficient for α_t during PUA + FPUC more readily lends itself to a measure of the elasticity of reservation wages to an increase in benefits. Moving from PUA claim share of 0 to 1 is associated with an α_t increase in reservation wage. Since our measure of treatment variable—PUA claims as a share of unemployment—is a very rough proxy for UI eligibility, the coefficient cannot be literally interpreted as moving from 0 to 100% eligibility for UI.[33] We can however interpret the *change* in α_t from the PUA period to the PUA + FPUC period as the effect on the reservation wage from the increase in benefits from FPUC, holding eligibility constant. Across the three specifications, this suggests roughly an increase in reservation wage between 8-12% from a $300 increase in weekly benefit amount. If we assume monetary denied individuals received the minimum disaster unemployment assistance payment, their average weekly benefit in our sample is

[33]We get similar results using PUA claims as a share of denied claims as an alternative measure of PUA "treatment". We show these results in Appendix B.4.1.

\$176. Going from \$176 to \$476 weekly represents a 170% increase. If we use the range of the difference in coefficient α_t and our calculation of 170% increase in benefit, these numbers yield an elasticity of reservation wage with respect to UI benefits of $7.6/170 = .045$ to $11.6/170 = 0.068$.

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. We also corroborate our results from BAM data by examining the earnings of individuals who transition from non-employment to employment—rather than using reservation wages—and analyze how these earnings differ between individuals who are eligible to receive UI benefits and those who are not.

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 their income from calendar year 2019. We use this information to impute monetary eligibility for unemployment benefits should an individual experience a spell of unemployment during the months surrounding the March survey.[34] We also use

[34]We use a generalized version of the "UI-Calculator" from Ganong et al. (2020) to impute both eligibility

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.

the March Supplement to assess whether an individual collected unemployment benefits in the previous year. Combining the information from the first rotation (to assess eligibility) and the second rotation (to assess collection), we construct a measure of the take up rate of unemployment benefits for individuals in their first rotation.

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.[35] The BLS distinguishes between two 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

and the amount of benefits an individual would receive given their earnings history and state rules.

[35] 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 the past four weeks.

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, following a generalized version of the methodology outlined in Ganong et al. (2020).[36] 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.[37] 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 received an additional $600 of weekly benefits. Similarly, eligible individuals received $300 in extra weekly benefits from January 2021 until the State-specific date when the program expired.[38] For States which let the program run its

[36] 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.

[37] 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.

[38] 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.

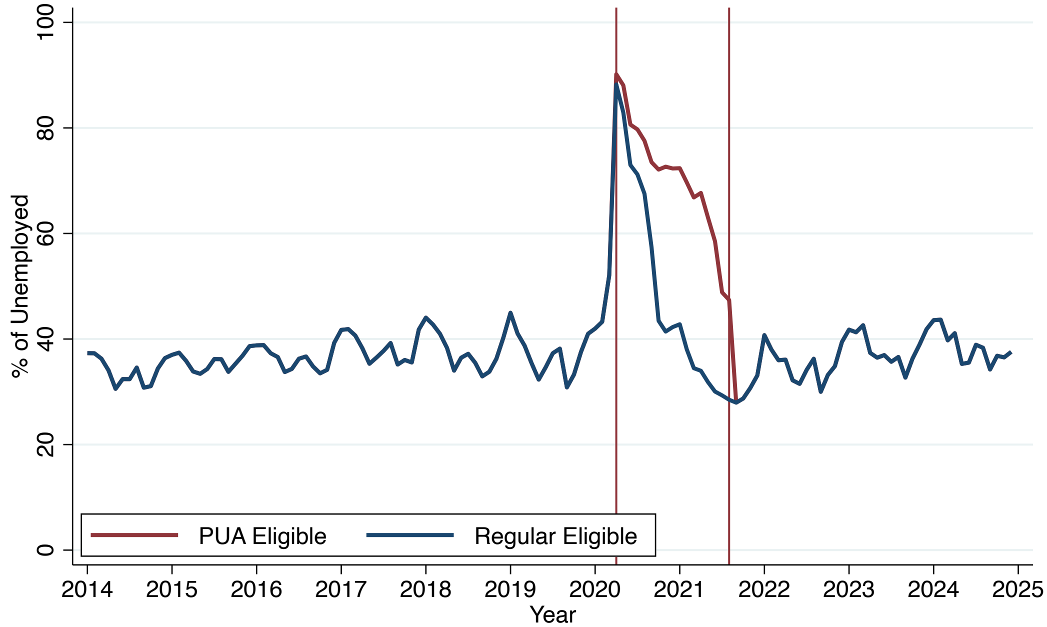
course as scheduled until September 5, we assume that individuals kept receiving the extra \$300 through the month of August.

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. Our measure of non-monetary eligibility uses 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 operating. Also, the PUA extended the duration of eligibility past the usual 26 weeks: we assume that duration became irrelevant during the relief period, though in principle one can only claim PUA (or regular) benefits up to 39 weeks.[39] Figure 7 shows the increase in non-monetary eligibility that resulted from this PUA program relative to the regular rules governing non-monetary eligibility.

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, unemployed 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

[39] 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 refer to the typical rules for non-monetary eligibility. PUA eligibility refers to non-monetary eligibility under the PUA program.

for regular unemployment benefits.

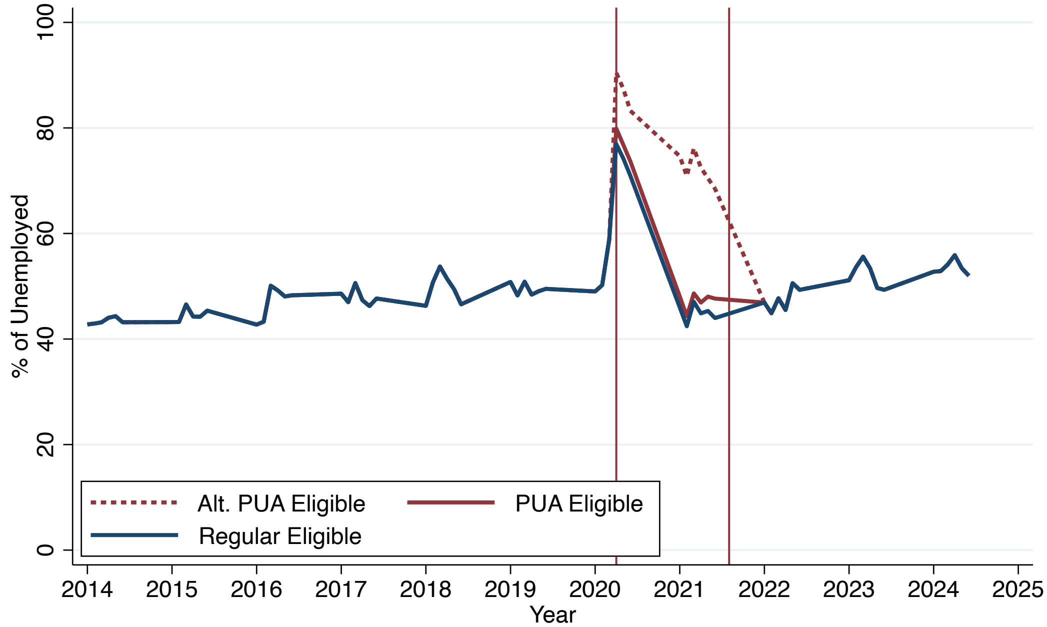
Combining non-monetary and either measure of monetary eligibility yields two series for overall UI eligibility, which are depicted in Figure 9. While there are differences across the two measures during the relief period, both indicate a sizable expansion in eligibility.

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.[40] We use the answer to this question to impute whether an individual collected benefits the

[40]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 8: Monetary Eligibility: regular vs PUA



Notes: Regular eligibility refer 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.

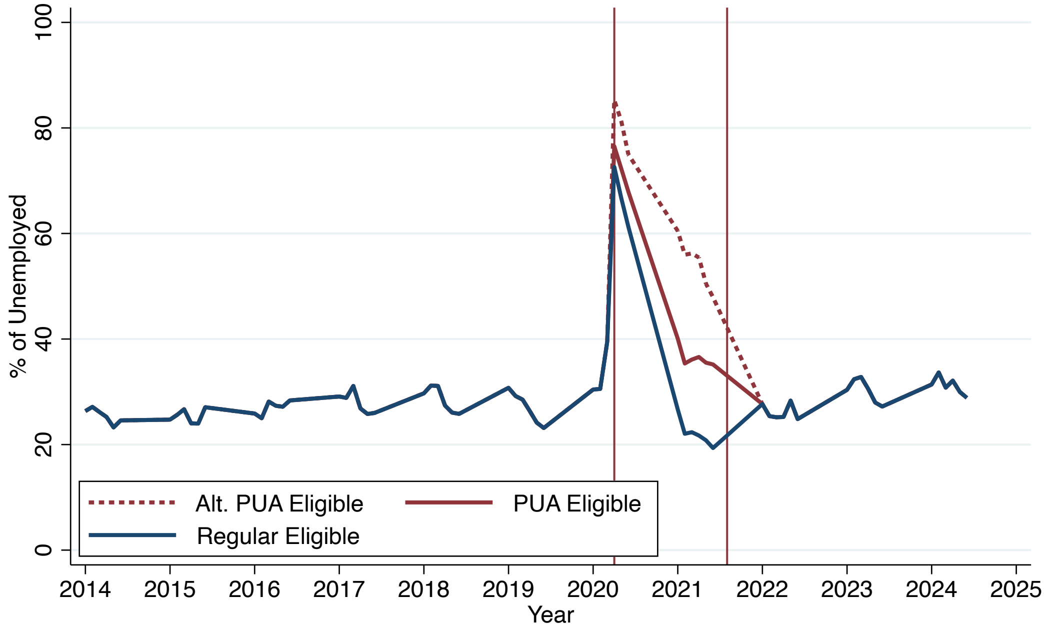
previous year. [41] Evidently, this measure of collection is only available for individuals whose interview rotation spans March: we use the answer to this question in the second rotation to impute collection in the first rotation (see Table 6).

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.[42] 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.

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

[42] 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 9: UI Eligibility: regular vs PUA



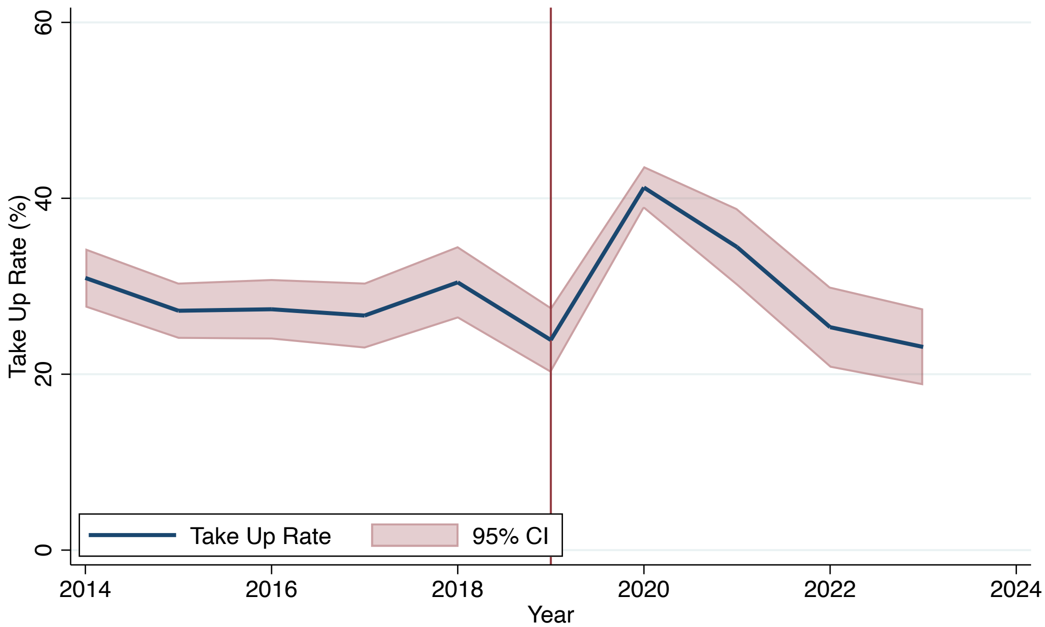
Notes: Regular eligibility refer to the typical rules for eligibility. PUA eligibility refers to eligibility under the PUA program, both assuming that state-rules apply to earnings plus self-employment income for non-monetary eligibility or that any positive past earnings or self-employment income qualifies for monetary eligibility.

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 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. Our baseline specification is:

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

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 Figures 11 and 12, show that the predicted take up rate closely follows the actual take up rate only when benefit amounts,

Figure 10: UI Take up Rate: regular program



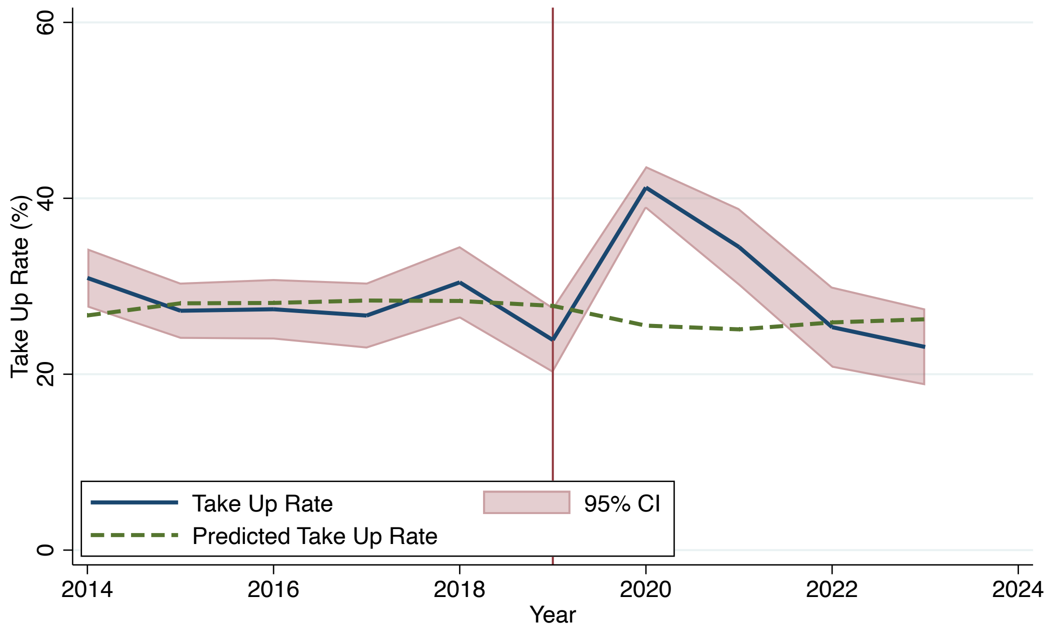
Notes: The take up rate is the fraction of individuals who are eligible to receive regular UI benefits who report having received UI benefits the previous year.

which include the \$600/\$300 supplements when applicaple, are included as a regressor.[43]

We conclude that the decision to file for and claim unemployment benefits is closely linked to the amount one expects to receive once the claim is approved. 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 was not itself the driver of increased take-up. Rather, larger benefits induced selection of workers into claiming and collecting UI. This motivates modeling UI take-up as an endogenous margin that can respond to benefit generosity, as we do in Section 5. We now turn to the next question: to what extent do UI-eligible individuals' reservation wage—and thus the wage they receive upon transiting to employment—react to an increase in benefits?

[43]We also estimate an alternative specification where benefits are measured only as regular benefits—i.e., without PUA. The predicted take up rate looks identical to the specification without benefits shown in Figure 11. See Figure 28 in Appendix C.1.

Figure 11: 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.

4.3 Earnings Following Transition to Employment

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.

These restrictions imply that individuals in our sample must be interviewed in March so we can assess eligibility as discussed above, and they must go through a transition to employment either during 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

Figure 12: 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.

(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 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 for 2020 should be taken with caution.

Fortunately, the Federal Covid Relief Bill (signed into law in December 2020) resurrected the FPUC program, with a $300 a 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.[44] 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 can now 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 who 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; or individuals who quit their last job. In principle, these individuals were not affected by the increase in the generosity of UI benefits in 2020 and 2021. Accordingly, we run the following regression

$$w = \alpha_0 \mathbb{I}(\text{eligible}) + \alpha_1 \mathbb{I}(\text{Covid}) + \boldsymbol{\gamma} \mathbb{I}(\text{eligible})\mathbb{I}(\text{Covid}) + \beta X + \varepsilon,$$

where X includes gender, race, education, and age, as well as indicators for industry and occupation.[45] In this equation, γ measures the change in the *wage premium* that eligible individuals command over ineligible individuals during the Covid relief period relative to pre-Covid.[46]

Figure 13 shows the trend in average weekly wages for eligible and ineligible individuals upon transiting to employment over time. Before the Covid relief period, the distance between wages of eligible and ineligible individuals was stable. During the Covid relief period, wages for eligible individuals rose noticeably, although wages for ineligible individuals also increased in 2020.

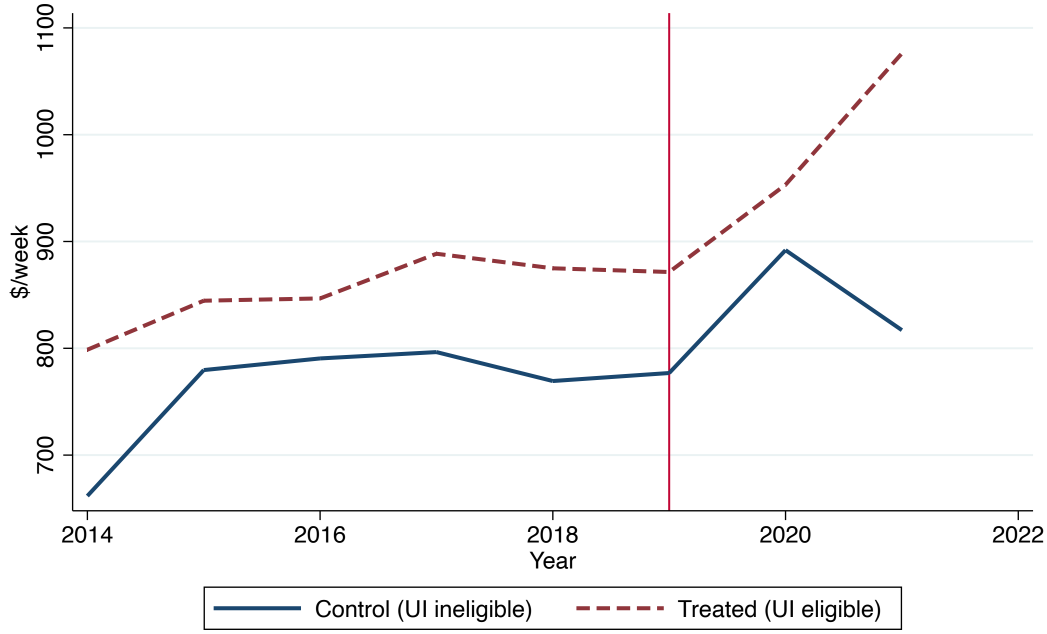
Table 7 shows that the earnings of eligible unemployed individuals, relative to those of ineligible individuals, upon transitioning to employment increased by about \$90 while the

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

[45] We do not include unemployment duration in our baseline specification as durations post-Covid are drastically different from durations pre-Covid, both because many individuals were newly unemployed at the outset of the pandemic while others were unemployed for longer durations than typically seen pre-Covid. Results with unemployment duration nevertheless appear in Appendix C.2.

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

Figure 13: Mean Weekly Wage after Transition to Employment



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

FPUC program supplemented unemployment benefits, though this effect becomes statistically insignificant once year fixed effects are included. If we exclude 2020 (a year with limited data, as discussed above), the wage premium increased by about \$65 in 2021, when the extra benefits were \$300 a week.

Several caveats are in order. First, recall that 2020 was a very unusual year: not only do we have limited data points for that stretch of the Covid relief period, but it is hard to be confident about eligibility measures during that time. And while it would be tempting to conclude that the \$65 change in the wage premium of eligible individuals represents a lower bound because not every eligible person actually files and claims benefits, there may also be people we classify as ineligible but who were in fact collecting benefits. It is notable, however, that the change in the wage premium we estimate from supplemental UI benefits in 2021 in the CPS (about 9.1%) is within the range of estimates found using reservation wages from BAM data—namely, 8 to 12 percent (see Section 3.5).

Our empirical findings can be summarized as follows. The BAM analysis shows that higher expected UI benefits raise reservation wages, but the sensitivity is modest. Evidence from

Table 7: Diff-in-Diff Regression: Eligible Wage Premium pre vs Covid

	Include 2020		Exclude 2020	
	(1)	(2)	(3)	(4)
Wage level				
Wage premium ($)	91.00***	-4.437	73.34**	64.34**
	(33.81)	(41.08)	(30.08)	(26.84)
Log Wage				
Wage premium (log)	0.126***	0.0347	0.112***	0.0911***
	(0.0369)	(0.0434)	(0.0336)	(0.0320)
Year fixed effect	No	Yes	No	Yes
N	7,523	7,523	6,719	6,719

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Data from the CPS. Additional controls in all regressions are: sex, age, education, race, industry and occupation.

the CPS confirms this low wage responsiveness, while also revealing that benefit take-up responds strongly to UI generosity. To interpret these findings, the next section introduces a simple economic environment with directed search, featuring 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 modestly 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, 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 benefit, denoted $\varepsilon \in [\underline{\varepsilon}, \bar{\varepsilon}]$, which is distributed according to cumulative distribution function $F(\varepsilon)$ with density $f(\varepsilon)$.[47] 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 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.

[47] 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.

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

$$w = y - (1-\beta(1-\delta))\frac{k}{q(\theta)}. \tag{2}$$

Recall that $q(\theta)$ is decreasing in θ. Therefore, submarkets with higher market tightness are associated with lower wages. Equation (2) 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 $U(\varepsilon)$ denote the value of being unemployed for workers of type ε. Similarly, let $W(w,\varepsilon)$ be the value of being employed at wage w for workers of type ε. Then

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

and therefore

$$W(w,\varepsilon) = \frac{w+\beta\delta U(\varepsilon)}{1-\beta(1-\delta)} \tag{3}$$

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, who enjoys continuation value $U(\varepsilon)$. The worker chooses the submarket (θ, w) that maximizes this return, taking as given the trade-off highlighted by equation (2):

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

subject to

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

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 (3), we can rewrite this problem as

$$R\left(U(\varepsilon)\right) \equiv \max_{\theta,w} p\left(\theta\right)\left(\frac{w+\beta\delta U(\varepsilon)}{1-\beta\left(1-\delta\right)} - U(\varepsilon)\right) \tag{4}$$

subject to

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

The envelope condition implies that

$$R'\left(U(\varepsilon)\right) = \frac{-p\left(\theta\right)\left(1-\beta\right)}{1-\beta\left(1-\delta\right)}, \tag{5}$$

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 tighter submarkets that offer higher wages but lower probabilities of finding a job.

Decision to Collect UI Benefit

Let $U\left(\varepsilon\right) \equiv \max\left\{U^N, U^C\left(\varepsilon\right)\right\}$ denote the lifetime utility of an unemployed worker with UI benefit collection cost ε. The worker 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:

$$U^N = d + \beta \left\{ U^N + R\left(U^N\right) \right\}. \tag{6}$$

Since ε is constant over the course of an unemployment spell, a worker who decides not to collect benefits at the beginning of an unemployment spell will continue not to collect 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 $U^C(\varepsilon)$, 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:

$$U^C(\varepsilon) = b - \varepsilon + \beta \left\{ U^C(\varepsilon) + R\left(U^C(\varepsilon)\right) \right\}. \tag{7}$$

Equation (7), together with the monotonicity of R (as shown in equation (5)), implies that $U(\varepsilon)$ is monotonically decreasing in ε. This, in turn, implies that there exists a cutoff $\varepsilon^* = b - d$ such that all types with $\varepsilon \leq \varepsilon^*$ choose to collect UI benefits, while those with $\varepsilon > \varepsilon^*$ choose not to collect.

Finally, taking the derivative of equation (7) with respect to b, we obtain:

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

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 following 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:

$$\frac{dw^C}{db} = \underbrace{\frac{\int_0^{\varepsilon^*} x^e(\varepsilon)\frac{dw(\varepsilon)}{db}d\varepsilon}{\int_0^{\varepsilon^*} x^e(\varepsilon)\,d\varepsilon}}_{\substack{\text{direct effect on wages of} \\ \text{existing collectors } (>0)}} + \underbrace{\frac{\int_0^{\varepsilon^*} \frac{dx^e(\varepsilon)}{db}\left(w(\varepsilon) - w^C\right)d\varepsilon}{\int_0^{\varepsilon^*} x^e(\varepsilon)\,d\varepsilon}}_{\substack{\text{effect on JFR among} \\ \text{collectors } (<0)}} + \underbrace{\frac{x^e(\varepsilon^*)\left(w(\varepsilon^*) - w^C\right)}{\int_0^{\varepsilon^*} x^e(\varepsilon)\,d\varepsilon}}_{\substack{\text{change in composition} \\ \text{of collectors } (<0)}} \tag{8}$$

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. This term is always negative. The reason is that job-finding rates decline more for workers with the lowest collection costs—those who are most sensitive to changes in b. As a result, 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.

The third term arises from the shift in the collection threshold. As UI benefits increase, the marginal type ε^* finds it optimal to start collecting. This expands the pool of collectors among employed workers. 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. 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 types. 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 Boostani et al. (2019). 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.[48]

[48] 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 8: 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	3.03	av. job finding rate of 38%
λ_e	prob. of search on the job	0.224	share of employed actively searching
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)

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 A. 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 8 summarizes the calibrated parameters and targets.

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 $\gamma = 1.3$.[49] 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, $\kappa = 3.03$, is calibrated to generate a

[49]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 $\theta = v/a$.

job-finding probability of approximately 38% at the monthly frequency, in line with CPS data from 2015 to 2020.[50] 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 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 not working, which may reflect home production or other non-market activity. The initial unemployment benefit is set to 0.4, so that the benchmark calibration yields an average replacement rate of 45%. Accordingly, the benchmark value of b in the model is 0.6.[51]

Finally, we set the probability of becoming UI eligible, φ, to 0.1, which means that on average individuals become eligible to collect UI benefits after a 10-month work history. We set the probability of UI benefit expiration, ψ, to zero. Since we target a monthly job-finding rate of 38%, only about 10% of unemployed workers remain unemployed for more than six months in steady state. Setting $\psi = 1/6$, as is commonly done, would imply that too many workers lose eligibility before finding a job, thereby muting the impact of UI benefits on reservation wages. Also note that the Pandemic Emergency Unemployment Compensation (PEUC) program extended the duration of unemployment benefits.

5.4 Quantitative Experiment

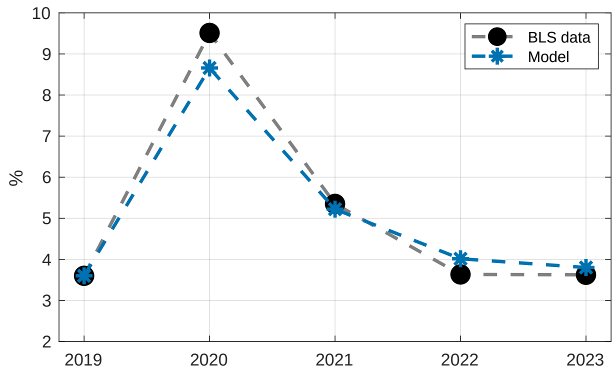
Using the calibrated model, we conduct the following experiment. We start the model in steady state. In period 0, we introduce an unexpected separation shock that raises the unemployment rate to 15%. This shock lasts for one period. To match the observed time path of unemployment and the job-finding 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.

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

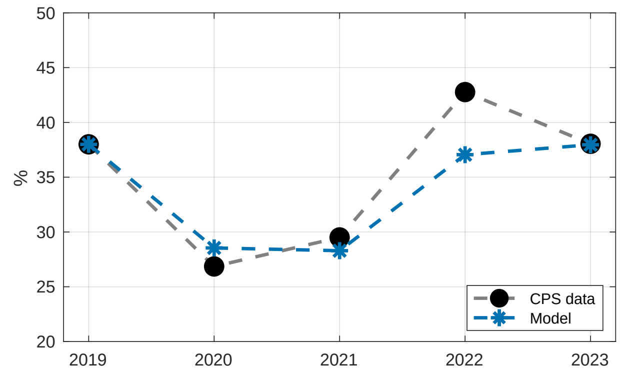
[51] The level of d and the range of the collection cost distribution cannot be chosen independently. We set $d = 0.2$ and calibrate the support of $F(\varepsilon)$ to match the fraction of UI collectors. Choosing a different value of d would require adjusting the distribution of ε to match the same target, but has little effect on the quantitative implications once the moments are matched.

Figure 14: 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 rare 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.

Thereafter, the cost of creating 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 14 shows how the model fits 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 American Rescue Plan Act, 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 assess how much the endogenous UI take-up margin contributes to dampening the response of the reservation wage to an increase in UI benefits. In Figure 15a, we plot two time paths for the take-up rate. The blue line shows the take-up rate (averaged over monthly model output) for our benchmark model with endogenous UI collection. The orange line shows the same moment for a model in which take-up is not a choice. In this version, the decision to collect is fixed, i.e., the threshold ε^* is fixed at its pre-shock value. Although take-up is exogenous in that model, the overall rate still changes during the transition. This is because the composition of UI-eligible and ineligible individuals—as well as collectors and non-collectors—evolves, even though individuals' collection decisions do not. By contrast, in the model with endogenous collection, the threshold ε^* also responds to changes in UI generosity. As we see, the blue line closely tracks the take-up rate we estimated in the CPS.

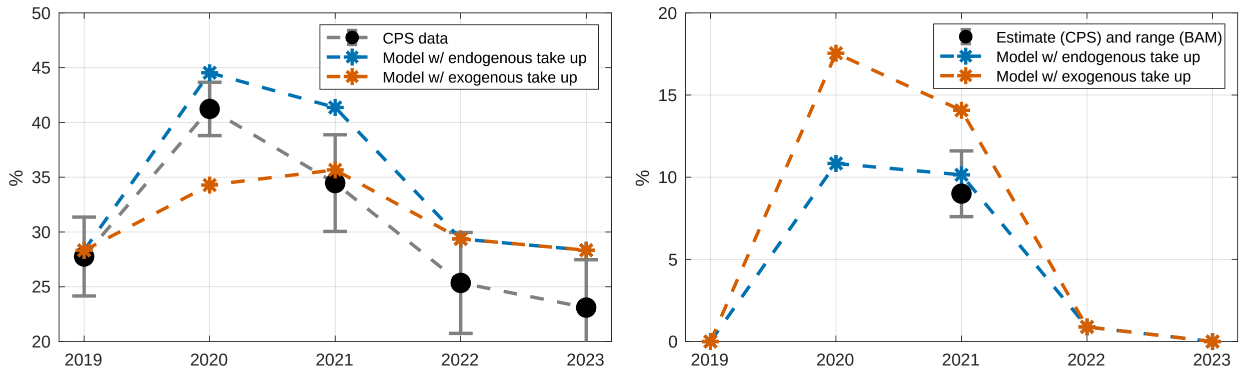
Figure 15b plots the change in the collectors' wage premium—defined as the difference between the average log wage of collectors and that of non-collectors—relative to its steady-state (pre-shock) value. The model results are shown for two versions of the environment: one with endogenous take-up and 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 7) and the range implied by BAM data (7.6% to 11.6%, see Section 3.5).

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—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.

Figure 15: Unemployment Rate and Job Finding Rate

(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 7 of Section 4, while the range of estimates is from BAM data as found in Section 3.5. Monthly data are aggregated to annual by simple averaging.

6 Conclusion

7 Conclusion

Using evidence from BAM and CPS, we find that UI generosity has only a modest pass-through to wages but a strong effect on benefit take-up. BAM results show higher expected benefits raise reservation wages, yet the implied elasticity is small. CPS results confirm this low wage sensitivity using realized wages after unemployment, and show 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 sensitive to benefit generosity. When calibrated to match pre-pandemic moments, the model accounts well for the observed modest wage response and the large policy-driven variation in take-up. The analysis highlights that 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 especially relevant for assessing the incidence and targeting of temporary UI expansions in future downturns.

A Quantitative Model

The setup closely follows the theoretical framework described in Section 5.1, but incorporates several modifications to better connect the model to the data. First, we introduce on-the-job search, following Menzio and Shi (2010) and Boostani et al. (2019), which allows us to match the empirical object we measure—the average wage received at the time of job finding after an unemployment spell. 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 spell.[52] Finally, we distinguish between UI-eligible and UI-ineligible workers and allow unemployment benefits to expire stochastically.

Submarkets

There is a continuum of submarkets indexed by the expected lifetime utility x that firms offer to workers, $x \in X = [\underline{x}, \bar{x}]$, with $\underline{x} < v(d)/\left(1-(1-\delta)\beta\right)$ and $\bar{x} > v(y)/\left(1-(1-\delta)\beta\right)$. 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 their 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 $\lambda_e \in (0, 1]$. 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.

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

$$R^i(V) \equiv \max_{x \in X} p\left(\theta^i(x)\right)(x - V), \quad i \in \{E, I\} \tag{9}$$

[52] If we were to assume collection costs as permanent types, as in the simple model of Section 5.1, the type would become a state variable and greatly complicate the firm's problem.

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^N denote the value of being unemployed and ineligible:

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

Eligible individuals must decide whether to collect or not at the beginning of their unemployment spell. Let $U^E(\varepsilon) \equiv \max\{U^N, U^C(\varepsilon)\}$ denote the lifetime utility of an eligible unemployed worker with UI benefit collection cost ε. If an individual chooses not to collect UI benefits, their lifetime utility is given by U^N, the same value that ineligible individuals receive. 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 spell.[53] To capture the notion that UI benefits expire, let ψ denote the probability that a UI collector loses benefits and thus becomes UI ineligible.

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

$$U^C(\varepsilon) = v(b) - \varepsilon + \beta \left\{ (1-\psi) \left[U^C(\varepsilon) + R^E \left(U^C(\varepsilon) \right) \right] + \psi \left[U^N + R^I \left(U^N \right) \right] \right\}. \tag{11}$$

Finally, let $U^E \equiv \int_{\underline{\varepsilon}}^{\bar{\varepsilon}} U(\varepsilon) \, dF(\varepsilon)$ 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

[53] Note that in general individuals must choose to file for benefits soon after separation from their employer.

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 $J(V)$, 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

$$J^I(V) \equiv \max_{w,V'} \left\{ y - w + \beta(1-\delta)\left[(1-\varphi)\left(1-\lambda_e \tilde{p}^I(V')\right) J^I(V') + \varphi\left(1-\lambda_e \tilde{p}^E(V')\right) J^E(V')\right] \right\} \tag{12}$$

subject to

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

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

$$J^E(V) \equiv \max_{w,V'} \left\{ y - w + \beta(1-\delta)\left(1-\lambda_e \tilde{p}^E(V')\right) J^E(V') \right\} \tag{13}$$

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. If they 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 of 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

$$q\left(\theta^{i}(x)\right) J^{i}(x) \leq k \tag{14}$$

with $\theta^{i}(x) > 0$ whenever $q\left(\theta^{i}(x)\right) J^{i}(x) = k$, 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 (14) 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.

B BAM

Table 9 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. It may also reflect the incentive for those who would not normally

Table 9: Share of Claimants by NAICS Sector of Previous Employer

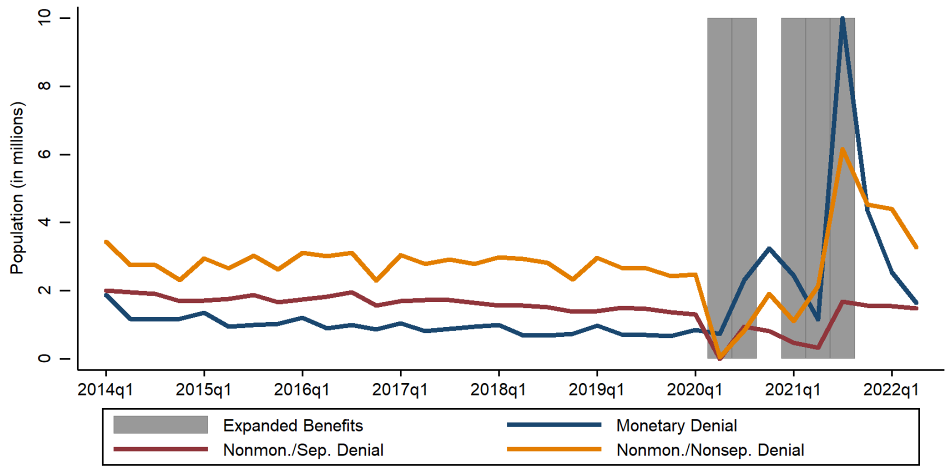
NAICS Sector	Paid Claims	All Denied Claims	Monetary Denials
11: Ag., Forestry, Fish. & Hunt	0.031	0.010	0.015
21: Mining, Quarrying, & Oil & Gas	0.010	0.009	0.004
22: Utilities	0.002	0.002	0.002
23: Construction	0.122	0.079	0.087
31-33: Manufacturing	0.094	0.099	0.057
42: Wholesale Trade	0.041	0.039	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.

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. Figure 16 plots the weighted population of denied claimants in 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 10 reports the coefficients for these regressions on the paid claims sample in the first two columns and for

Figure 16: **Denied Claims Population**



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 samples.

B.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 are 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 average earnings during a base period) as in Ferraro et al. (2022). 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. Table 11 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 spec-

Table 10: 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
Observations	182910	177509	146106	50302

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. Standard errors are clustered at the state level.

ifications to estimate the effect of benefits on the reservation wage, such as our event-study approach.

B.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 to include benefit amounts is that they are usually a function of earnings history over

Table 11: 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
Observations	63280	61990	85371	82942

a prolonged base period. Heterogeneity in benefit amounts may be informative about the attachment of the worker to the labor force otherwise unobservable if we only include usual wages and other covariates. We get similar results to our baseline specification that does not include benefits. Table 12 shows that in 2020 there is little change from the baseline. In 2021, there is a slightly larger component of the change in reservation wage attributable 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 and offset by the composition of workers (endowments).

Table 12: 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***	[0.034,0.134]		
endowments	0.122***	[0.081,0.163]	144.856***	[92.712,197.000]
coefficients	-0.043***	[-0.068,-0.018]	-51.073*	[-104.906,2.761]
interaction	0.005	[-0.003,0.013]		

Table 13: 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***	[-0.078,-0.028]		
endowments	0.021**	[0.003,0.040]	-40.345	[-90.340,9.650]
coefficients	-0.075***	[-0.092,-0.058]	141.536***	[91.305,191.767]
interaction	0.001	[-0.001,0.002]		

B.3 Diff-in-Diff Robustness

B.3.1 Event Study: Alternate Treatment and Control Groups

In this section, we consider alternate cutoffs for defining treatment and control states for our event study 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 study for each quartile separately as the treatment group.

Figure 18 and 19 present the results of our event study using the bottom 50 percent of states as the control group, and the 3rd and 4th quartile of states as the treatment group independently rather than combined in our baseline.

Now we explore the heterogeneity in treatment intensity by comparing each quartile of PUA claim share states to the bottom quartile. We can use the bottom quartile of states

Figure 17: **PUA Claims as Share of Total Unemployment**

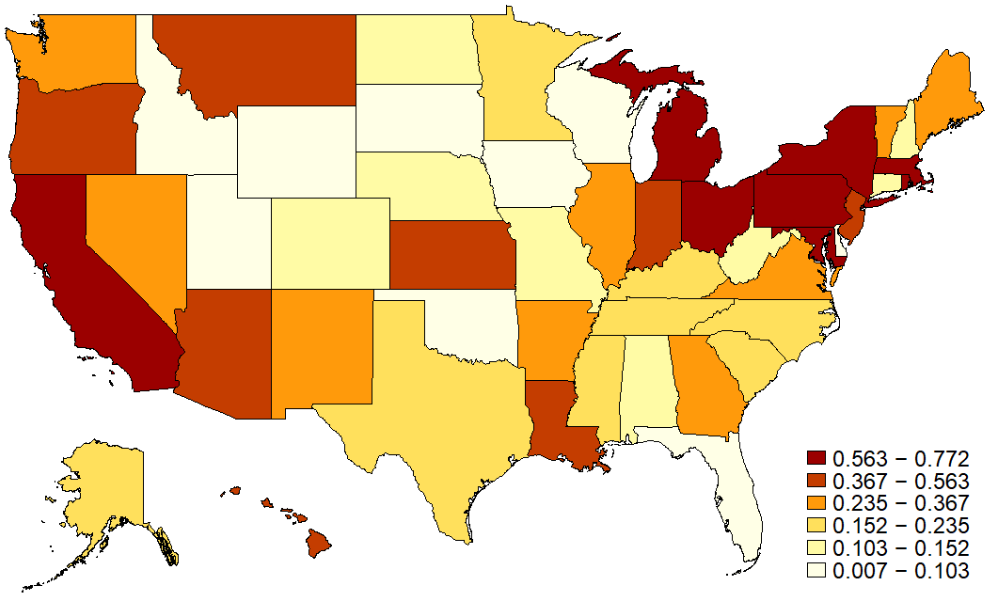
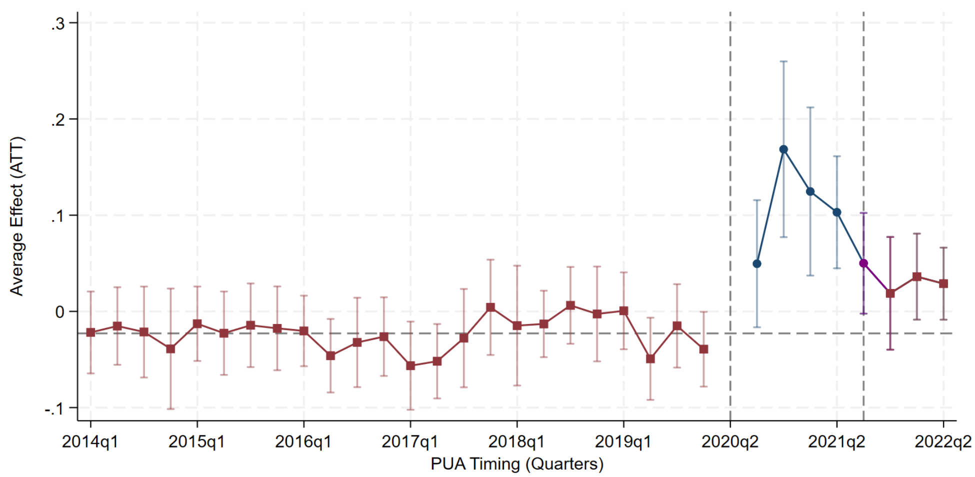


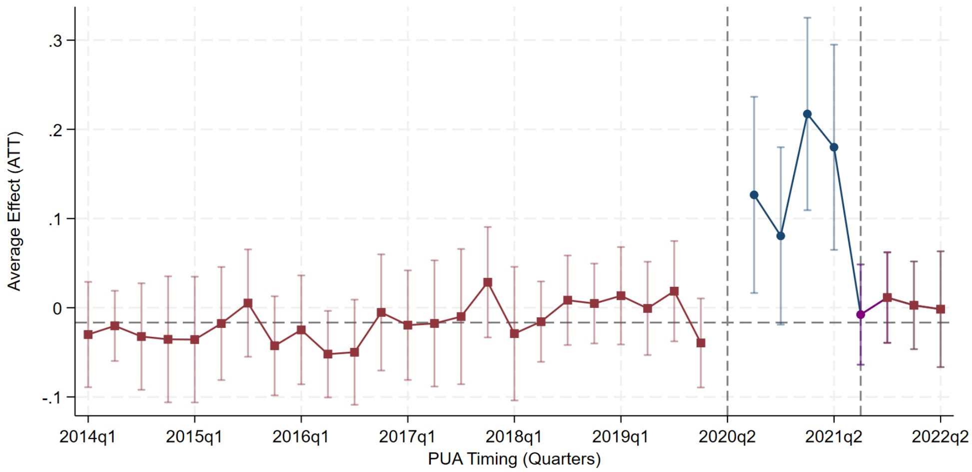
Figure 18: **Monetary Denials and PUA: 3rd Quartile as Treatment Group**



Notes: Control group is states with below median level of PUA claim share (.214) during PUA period. Treatment group is 50th-75th percentile bin of states by level of PUA claim share (.214 − .299). Period 0 corresponds to 2020 Quarter 2. The first observation in the treatment period is 2020 Q3. Period 5 corresponds to 2021 Quarter 3and the end of PUA. Note that some states ended PUA as early as July, while the program ended for all states in September 2021. Standard errors are clustered at the state level.

as the control group instead of the bottom half. Figure 20 presents the results when only the bottom quartile of states by PUA claim share is used in the control group, and the top

Figure 19: **Monetary Denials and PUA: 4th Quartile as Treatment Group**



Notes: Control group is states with below median level of PUA claim share (.214) during PUA period. Treatment group is 75th-100th percentile bin of states by level of PUA claim share (.299 − .41). Period 0 corresponds to 2020 Quarter 2. The first observation in the treatment period is 2020 Q3. Period 5 corresponds to 2021 Quarter 3and the end of PUA. Note that some states ended PUA as early as July, while the program ended for all states in September 2021. Standard errors are clustered at the state level.

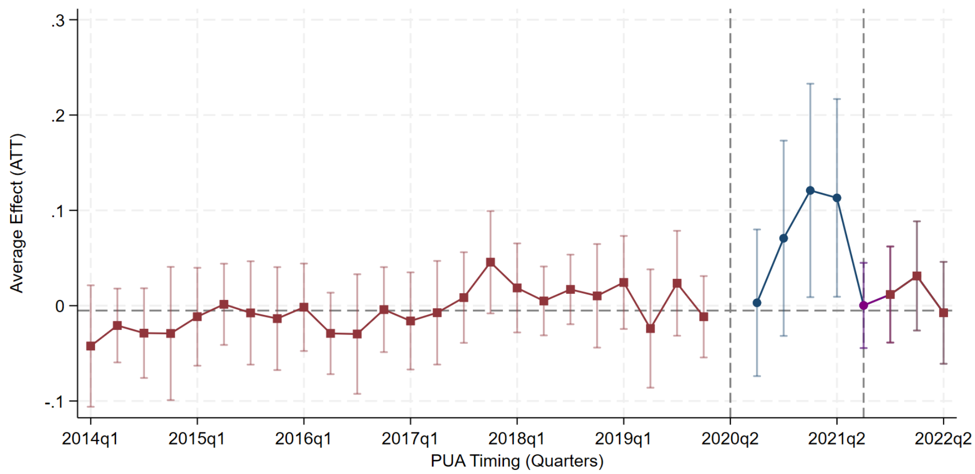
2 quartiles are the treatment group. Our results are qualitatively robust when using this control group compared to our baseline specification where we assign the top half of states to the treatment group.

We can also look at each of the 2–4th 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 21. We find similar magnitudes to our baseline event study when we define the 3rd and 4th quartiles as the treatment and the 1st quartile as the control group, as seen in Figures 22 and 23, respectively.

B.4 Fixed Effect Specification with Condensed Time Dummies

To isolate the impact of increased benefits, we condense the timing in our event study in Figure 6 to define three separate dummy variables. The first interacts PUA claim shares with a dummy variable for quarters 2020 Q3 and 2020 Q4, representing the period 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

Figure 20: **Monetary Denials and PUA: 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 3and the end of PUA. Note that some states ended PUA as early as July, while the program ended for all states in September 2021. Standard errors are clustered at the state level.

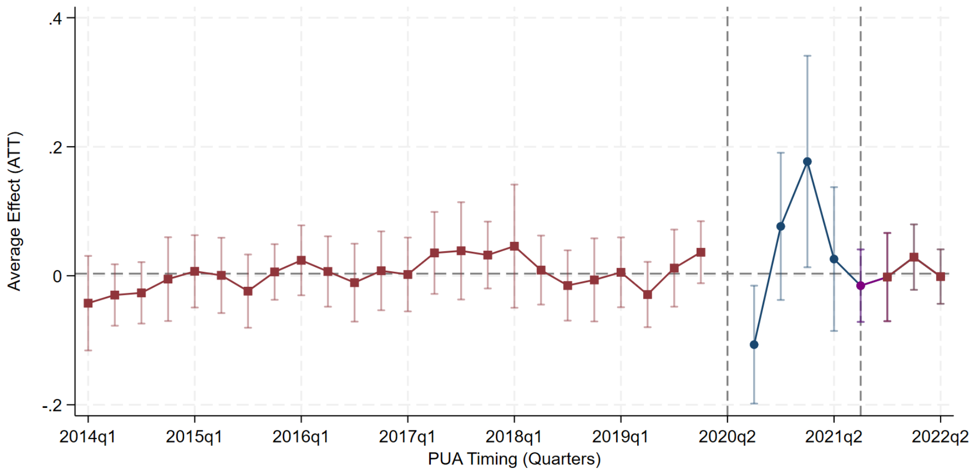
the period when PUA benefits were available alongside the additional $300 weekly FPUC supplement. The third interacts PUA claim shares with a dummy for 2021 Q3, representing the phase-out of both the PUA and FPUC programs. We also show that these results are robust to the introduction of Covid-related state-level controls, specifically the quarterly average of weekly state-level Covid deaths per 100k people 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 event study described earlier, by considering the following specification:

$$Y_{it} = \lambda_t + \alpha_t \mathbb{I}_{treat,it} * period_t + \beta X_{it} + \varepsilon_{it},$$

Figure 21: **Monetary Denials and PUA: 1st Quartile Control 2nd Quartile Treatment**



Notes: Control group is states in 0-25th percentile bin by level of PUA claim share (.147) during PUA period. Treatment group is states in the 25th-50th percentile bin by level of PUA claim share (.147 −.214). Period 0 corresponds to 2020 Quarter 2. The first observation in the treatment period is 2020 Q3. Period 5 corresponds to 2021 Quarter 3and the end of PUA. Note that some states ended PUA as early as July, while the program ended for all states in September 2021. Standard errors are clustered at the state level.

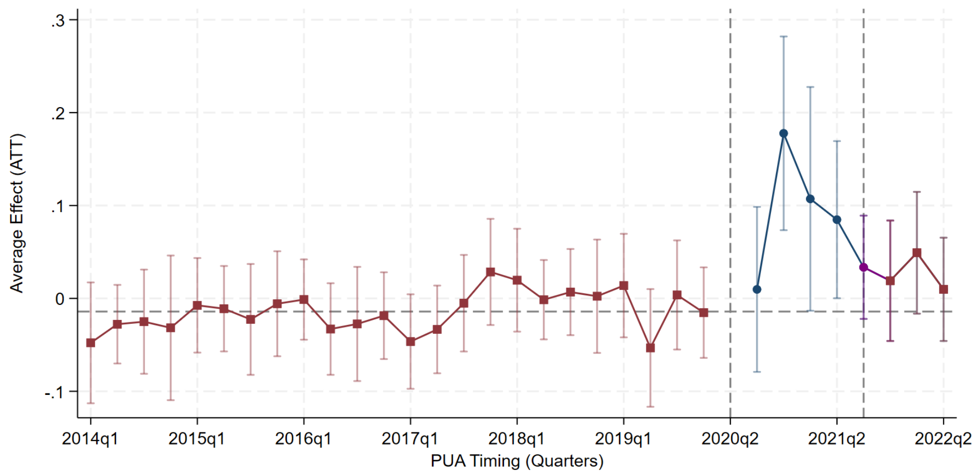
where λ_t is a (quarterly) time dummy, $\mathbb{I}_{treat,it}$ is equal to 1 if the state is a high PUA claims share state, $period_t$ represents the 3 condensed dummy variables introduced above, and X is a set of controls.[54]

The first column of Table 14 displays the coefficient associated with the three condensed dummy variables. More detailed results appear in Appendix B.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 around 5%, for a total of approximately 11%. The second column of Table 14 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 people and the quarterly average of the state unemployment rate.

Finally, we confirm that these results are not driven by unmodeled time variation in the relationship between reservation wages and a state's Covid or economic environment. We

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

Figure 22: **Monetary Denials and PUA: 1st Quartile Control 3rd Quartile Treatment**



Notes: Control group is states in 0-25th percentile bin by level of PUA claim share (.147) during PUA period. Treatment group is states in the 50th-75th percentile bin by level of PUA claim share (.214 – .299). Period 0 corresponds to 2020 Quarter 2. The first observation in the treatment period is 2020 Q3. Period 5 corresponds to 2021 Quarter 3and the end of PUA. Note that some states ended PUA as early as July, while the program ended for all states in September 2021. Standard errors are clustered at the state level.

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

$$Y_{it} = \lambda_t + \alpha_t \mathbb{I}_{treat,it} * period_t + \delta_t Covid_{it} * qtr_t + \gamma_t Urate_{it} * qtr_t + \beta X_{it} + \varepsilon_{it},$$

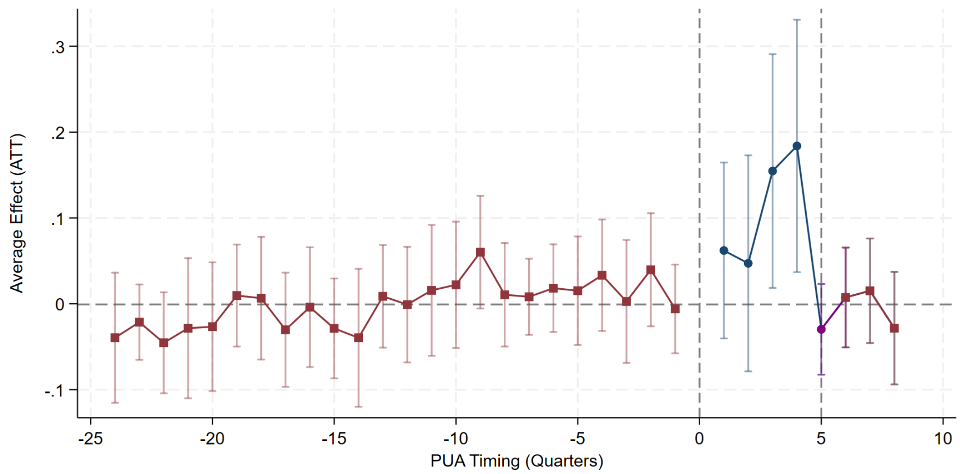
where $Covid_{it}$ is the Covid deaths per 100k people and $Urate_{it}$ is the quarterly average of the state unemployment rate. The estimates, displayed in the third column of Table 14, show that our results are robust to this specification, though the effect of PUA is notably smaller compared with other specifications and the baseline event study.

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

We present the results of our regression specification in Table 14 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

Figure 23: **Monetary Denials and PUA: 1st Quartile Control 4th Quartile Treatment**



Notes: Control group is states in 0-25th percentile bin by level of PUA claim share (.147) during PUA period. Treatment group is states in the 75th-100th percentile bin by level of PUA claim share (.299 – .41). Period 0 corresponds to 2020 Quarter 2. The first observation in the treatment period is 2020 Q3. Period 5 corresponds to 2021 Quarter 3and the end of PUA. Note that some states ended PUA as early as July, while the program ended for all states in September 2021. Standard errors are clustered at the state level.

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 a share of total claims. This choice reflects that the share of potential PUA claimants and collectors in a quarter may be less a function of the flow of newly unemployed claimants than of the total unemployed population. Our results are consistent with alternative measures of PUA intensity, as shown in the next two tables.

In Table 16, 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 event study in Figure 6.

In Table 17, we present the results of our regression specification in Table 5, but using the state's quarterly average of their share of PUA claims out of the total population of *denied* UI claimants in the quarter reported in BAM as the treatment variable.

Table 14: Event Study Regression

	(1) $ln(\tilde{w})$	(2) $ln(\tilde{w})$	(3) $ln(\tilde{w})$
PUA	0.0624	0.0615	0.0299
	(1.58)	(1.80)	(0.87)
PUA + FPUC	0.110*	0.109*	0.101**
	(2.50)	(2.46)	(2.81)
Phase Out	0.0134	0.0159	0.00735
	(0.47)	(0.61)	(0.29)
Qtly Controls	No	Yes	No
Qtly Controls x Time	No	No	Yes
N	33868	33868	33868

t statistics in parentheses

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

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.

B.5 COVID and Unemployment Controls

As discussed in Section 3.5, our estimates rely on the assumption of parallel trends between the treatment and control states. Additionally, we require that other variables are not 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.

Table 15: Event Study Specification: Quarterly Coefficients

	(1)	(2)	(3)
	$ln(\tilde{w})$	$ln(\tilde{w})$	$ln(\tilde{w})$
2020 Q3	0.0674	0.0625	0.0127
	(1.50)	(1.44)	(0.74)
2020 Q4	0.0590	0.0610	0.0446
	(1.27)	(1.52)	(0.82)
2021 Q1	0.129**	0.121*	0.0925*
	(2.71)	(2.54)	(2.24)
2021 Q2	0.0936	0.0993	0.107*
	(1.77)	(1.87)	(2.48)
2021 Q3	0.0134	0.0159	0.00735
	(0.47)	(0.61)	(0.29)
Qtly Controls	No	Yes	No
Qtly Controls x Time	No	No	Yes
N	33868	33868	33868

t statistics in parentheses

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

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.

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 find 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, and that 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 24 plots the PUA claims as a share of unemployment in our treatment states and control states at quarterly frequency.

Table 16: PUA Claims/Total UI Claims

	(1)	(2)	(3)
	$ln(\tilde{w})$	$ln(\tilde{w})$	$ln(\tilde{w})$
PUA	0.0737	0.0791	0.0137
	(0.94)	(1.19)	(0.19)
PUA + FPUC	0.178*	0.186**	0.160*
	(2.55)	(2.74)	(2.11)
Phase Out	0.00554	0.00114	-0.0360
	(0.10)	(0.02)	(-0.74)
Qtly Controls	No	Yes	No
Qtly Controls x Time	No	No	Yes
N	33868	33868	33868

t statistics in parentheses

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

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.

Figures 25 and 26 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 were unlikely to stem from COVID intensity. Figure 27 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 were unlikely to have been confounded by differences in labor market tightness across states.

Table 17: PUA Claims/Denied UI Claims

	(1)	(2)	(3)
	$ln(\tilde{w})$	$ln(\tilde{w})$	$ln(\tilde{w})$
PUA	0.00524	0.00591	0.00241
	(1.55)	(1.64)	(1.82)
PUA + FPUC	0.0619***	0.0673***	0.0792**
	(4.11)	(4.84)	(2.88)
Phase Out	0.00266	-0.000243	-0.00477
	(0.35)	(-0.03)	(-0.42)
Qtly Controls	No	Yes	No
Qtly Controls x Time	No	No	Yes
N	33868	33868	33868

t statistics in parentheses

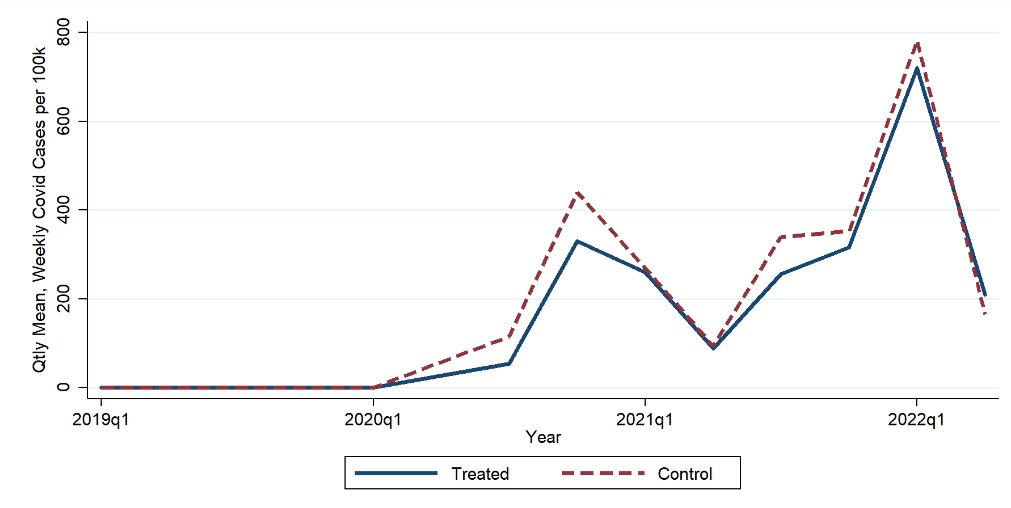
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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.

Figure 24: **PUA Claims/Total Unemployed**

Notes:

Figure 25: Qtly Mean, Weekly Covid Cases per 100k



Notes:

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

Notes:

Figure 27: Quarterly Mean, State Unempemployment Rate

Notes:

C CPS

C.1 Alternative Take Up Probit Specifications

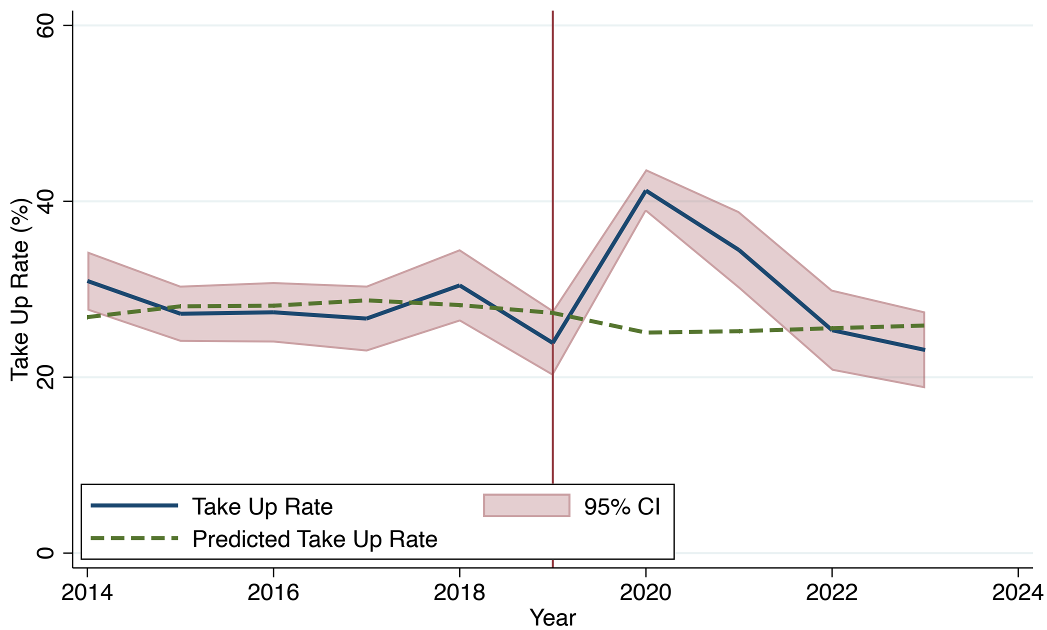
To reinforce the notion that the amount of benefits individuals expect to receive is key to the take up decision, we redo our Probit analysis using regular benefits instead of total benefits, which include the supplemental amounts specified by the FPUC program. The results, displayed in Figures 28, show 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 and 12 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 29 and 30 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.

C.2 Alternative Wage Premia Specifications

Tabel 18 replicates the results from Table 7 by adding individuals who are out of the labor force as well as those who are not eligible because of monetary reasons to the set of ineligible individuals. This table shows that expanding the set of individuals in the control group leaves our main conclusion intact, in the sense that the change in the wage premium remains modest.

Figure 28: 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 29: 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 30: 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 18: Diff-in-Diff Regression: Eligible Wage Premium pre vs Covid

	Include 2020		Exclude 2020	
	(1)	(2)	(3)	(4)
Wage level				
Wage premium ($)	119.3***	41.17	98.10***	26.47
	(36.25)	(43.41)	(33.82)	(41.37)
Log Wage				
Wage premium (log)	0.1630***	0.0873*	0.1480***	0.0764**
	(0.0387)	(0.0473)	(0.0366)	(0.0356)
Year fixed effect	No	Yes	No	Yes
N	3,697	3,697	3,144	3,144

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Data from the CPS. Additional controls in all regressions are: sex, age, education, race, industry, occupation and unemployment duration.

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