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

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Abstract

We study how endogenous take-up shapes wage responses among unemployment insurance recipients during the pandemic-era benefit expansions. Benefit Accuracy Measurement data show a sharp shift in the composition of claimants alongside modest increases in reservation wages in states with greater Pandemic Unemployment Assistance utilization: a benefit increase of roughly 170 percent is associated with a 7.6 to 12.3 percent rise in reservation wages, implying an elasticity of 0.067 to 0.108. In the Current Population Survey, take-up rose from about 27 percent before the pandemic to 41 percent in 2020; a prediction model estimated on pre-pandemic data reproduces this rise only when expected benefits are included. Wages thus responded modestly while participation responded sharply. We interpret these facts through a directed search model in which workers differ in the cost of claiming. Higher benefits raise the wages of existing recipients but also draw in high-cost marginal workers, who choose lower wages, so their entry attenuates the increase in recipient-average wages. Quantitatively, endogenous take-up reduces the recipient-average wage response by about 30 percent.

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

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1 Introduction

This paper examines how unemployment insurance (UI) generosity affects reservation wages, re-employment wages, and benefit take-up. Because participation responds to generosity, changes in wages observed among recipients combine the response of existing recipients with changes in claimant composition. During the Covid-19 pandemic, UI benefits expanded dramatically, most notably through the $600 weekly supplement introduced by the Coronavirus Aid, Relief, and Economic Security (CARES) Act in March 2020 and the subsequent $300 weekly supplement under the Continued Assistance Act and American Rescue Plan. These policy changes provide an unusually large setting in which to study the pass-through from benefits to reservation and re-employment wages and the decision to claim benefits. We study these questions using data from the UI Benefit Accuracy Measurement (BAM) system and the Current Population Survey (CPS), together with a directed search model featuring an explicit take-up margin.

Our central finding is that the pass-through from benefits to wages is modest, while take-up rose sharply alongside UI generosity, in both the data and the model. A key contribution of the paper is to show that these two responses are closely linked through a single mechanism. Claiming UI is itself costly, and workers vary in how heavily those costs weigh on them. When benefits rise, some workers with high claiming costs enter the program. At the threshold, benefits net of these costs leave marginal claimants close to indifferent between collecting and not collecting, so they continue to search relatively aggressively and choose lower wages than infra-marginal recipients. The pool of claimants therefore grows with workers whose reservation and re-employment wages are relatively low, explaining both the strong take-up response and the muted wage pass-through.

Under normal circumstances, the U.S. UI system provides temporary income support to workers who lose their jobs through no fault of their own and who satisfy state-specific earnings and work history requirements. Eligibility typically requires a qualifying separation, such as a layoff rather than a voluntary quit, and sufficient recent earnings. Even among eligible individuals, however, not all collect benefits. Take-up reflects a range of factors, including expected benefit amounts, expected unemployment duration, administrative and informational frictions, and other costs individuals associate with claiming benefits.[1]

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

During the Covid-19 period, both the level and scope of UI benefits expanded sharply. The Federal Pandemic Unemployment Compensation (FPUC) program provided an additional \$600 per week from March through July 2020 and an additional \$300 per week in early 2021. The Pandemic Unemployment Assistance (PUA) program extended UI eligibility to self-employed workers and others not typically covered by regular UI. These programs raised benefit levels for existing recipients and broadened eligibility to new groups of workers, generating substantial and rapid changes in both the generosity of UI and the composition of potential claimants. This episode therefore provides an unusually informative setting for our empirical analysis.

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

BAM includes two distinct samples: individuals who received UI benefits (the paid claims sample) and those who applied but were denied (the denied claims sample). Across both groups, a consistent pattern emerges: reservation wages rise with prior wages but remain below them, reflecting a typical job ladder effect in unemployment. The two samples nevertheless describe different populations: paid claims represent current recipients, whereas denied claims contain applicants who did not receive regular UI. Notably, the relationship between reservation and prior wages changes little over time, despite the large and temporary increases in UI generosity during the pandemic.

Using the BAM paid claims sample, we estimate the cross-sectional 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. To assess the role of recipient composition in changes over time, we use a Blinder-Oaxaca decomposition. In 2020, the observed decline in reservation wages is almost entirely explained by compositional shifts among claimants, in particular a larger share of younger and non-

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

manufacturing workers.[3] Holding observed composition fixed, the predicted reservation wage is larger by roughly 4 percent. In 2021, the corresponding predicted increase is 7 percent relative to the pre-pandemic period. Changes in observable characteristics offset this by about 2 percentage points, yielding a net observed increase of 5 percent. These descriptive decompositions show that recipient-average reservation wages are sensitive to who enters the claimant pool.

To further examine reservation wages following the expansion of UI access, we use state-level variation in the utilization of 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 regular UI, we use the denied claims sample in BAM and compare reservation wages across states with relatively high versus low PUA utilization.[4] States with above-median PUA claim shares form the high-utilization group. An event-time comparison shows that reservation wages rose more in high-utilization states during the PUA period, coinciding with elevated benefit levels, and that these differences faded as PUA expired. PUA utilization itself is not randomly assigned, but we interpret this pattern as differential-exposure evidence consistent with a modest positive reservation-wage response to expanded access to UI.

To quantify the relationship between reservation wages and benefit generosity, we estimate a continuous exposure specification. Using quarterly variation in state-level PUA claim intensity, we estimate separate exposure gradients for the PUA-only and PUA-plus-FPUC periods. The principal comparison is the difference between these gradients. Across specifications, this contrast is associated with a 7.6 to 12.3 percent increase in reservation wages, as reported in Section 3. Scaling the contrast by a \$300 increase relative to a \$176 base weekly benefit (a 170 percent increase) yields an implied elasticity of reservation wages with respect to UI benefit amount of 0.067 to 0.108.[5] The estimates are robust to controls for labor market tightness and Covid severity, and their magnitude is modest relative to the policy change.

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

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

[5]Appendix B.1 reports a complementary check using CPS rotation data: the re-employment wage premium for workers classified as UI-eligible rose by 9 percent during the Covid period. Because eligibility is imputed and the populations differ, we view the CPS result as an order-of-magnitude check rather than as an estimate of the same parameter.

BAM covers only workers who applied for UI. We therefore use the CPS to examine eligibility, benefit receipt, and re-employment wages in a broader sample of unemployed workers. We classify unemployed workers by likely UI eligibility, measure take-up among those classified as eligible, and compare re-employment wages across eligibility groups.

In the CPS, the increase in take-up is large relative to the change in wages. We estimate a Probit model of UI receipt using pre-pandemic data and use it to predict take-up during the pandemic. Changes in worker characteristics explain little of the increase. Adding expected benefit amounts, however, allows the model to track the increase closely. Because benefits changed at the same time as eligibility rules and other program features, we interpret this exercise as predictive rather than causal.

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

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

Our analysis confirms that changes in reservation and re-employment wages are modest relative to the increase in UI generosity, even during the extraordinary benefit expansions of the pandemic period. Prior work has documented similarly small wage elasticities in more stable environments, but these studies rely either on cross-sectional correlations or on policy variation in benefit duration rather than benefit levels; see Krueger and Mueller (2016), Card et al. (2007), Schmieder et al. (2013), and Nekoei and Weber (2017).[6] Our contribution is to

[6] There is also a macro literature on the effects of changes in the *duration* of UI benefits. For example,

document that this muted wage response also appears during large changes in benefit levels: even when replacement rates exceeded 100 percent for large segments of the unemployed, eligibility broadened sharply through PUA, and search requirements were relaxed, wage responses remained modest. The pandemic therefore provides evidence on wage setting during an unusually large and broad UI expansion.

We document a pronounced increase in benefit claiming alongside the increase in UI generosity. While Blank and Card (1991) document that take-up varies systematically with state replacement rates using pooled state-by-year data, their evidence reflects relatively modest cross-state policy variation and does not exploit large, discrete changes in benefit levels or eligibility rules. In contrast, the Covid-19 expansions generated large, discrete changes in benefit levels and eligibility rules, together with changes in administrative frictions. Using CPS microdata, we find that expected benefit amounts, rather than changes in worker characteristics, predict almost all of the surge in take-up during this period.[7] Because these program features changed together, we do not interpret this relationship as the causal effect of benefit levels alone. The evidence nonetheless highlights the importance of the extensive margin in large-scale UI expansions.

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

Our findings are closely related to a growing literature on the labor-market effects of the Covid-19 UI expansions. That literature generally concludes that despite the scale of government intervention, the impact on incentive-related labor market behavior was modest.[8] For example, Petrosky-Nadeau and Valletta (2025) use a standard search model to estimate the benefit level at which individuals are indifferent between accepting a job and remaining unemployed, and find that the 2020 expansions did not deter most workers from taking jobs. Boar and Mongey (2020) reach similar conclusions using a model that allows for job

Chodorow-Reich et al. (2019) find small effects of duration extensions on aggregate unemployment, while Hagedorn et al. (2025) find larger effects on wages and vacancy creation in a border-county design.

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

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

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

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

2 Overview of the Unemployment Insurance Program

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

2.1 Regular Unemployment Insurance

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

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

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

2.2 Covid-19 Relief Period

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

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

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

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

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 August 1, 2020. The funding from this program, which came from previously appropriated funds, quickly ran out and its total disbursement turned out to be minimal. However, the Covid Relief Bill signed into law on December 27, 2020, renewed the FPUC program by extending federal unemployment assistance to the tune of $300 per week through March 14, 2021.[12] This $300 of extra benefits was further extended through the American Rescue Plan Act (ARPA), signed into law March 11, 2021. While this last extension was scheduled to sunset September 5, 2021, several states chose to end the program as early as June 12, 2021.

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

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

In terms of non-monetary eligibility, PUA applied to individuals who were unemployed,

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

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

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

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

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

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

3 Empirical evidence from BAM data

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

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

Below we use BAM data to examine how individuals' job-search measures vary with UI benefits during the Covid-19 relief programs documented in the previous section. We highlight the change in the composition of UI recipients during 2020 and 2021 and its relationship with the reservation wage. We then use the denied claims sample to examine whether reservation wages evolved differentially with state PUA utilization and to construct an implied elasticity. But first, we document the BAM sampling procedure and describe our samples.

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

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

3.1 Sampling Procedure and Data Collected

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

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

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

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

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

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

and Krolikowski (2024).

3.2 Samples

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

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

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

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

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

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

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

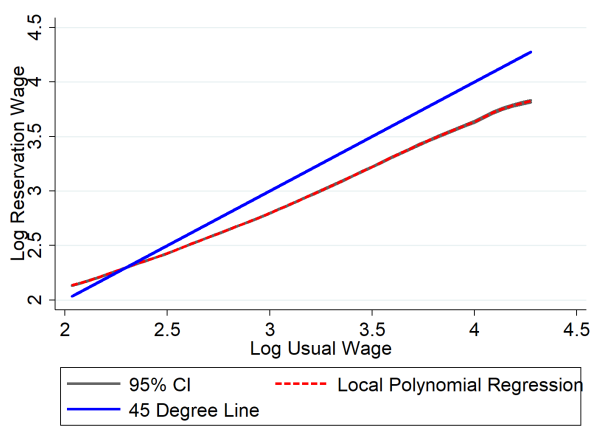
Table 1: Summary Statistics

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

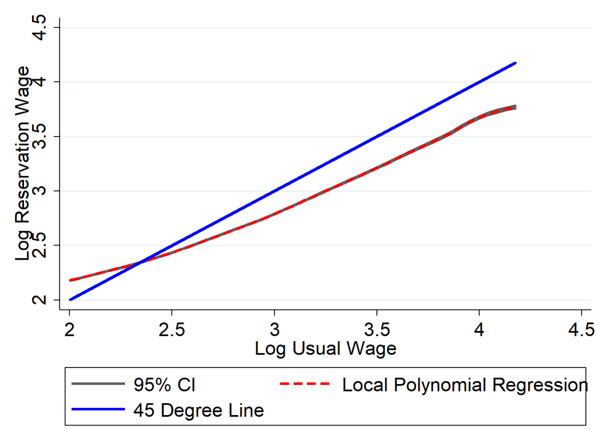
Notes: Share Manufacturing in this table includes NAICS sectors 21: Mining, Quarrying, Oil & Gas Extraction, 23: Construction, and 31-33: Manufacturing. Share Leisure and Hospitality includes NAICS Sectors 71: Arts, Entertainment, & Recreation and 72: Accommodation and Food Services. All dollar amounts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

Figure 1: Reservation Wage and Usual Hourly Wage

(a) Paid Claims Sample

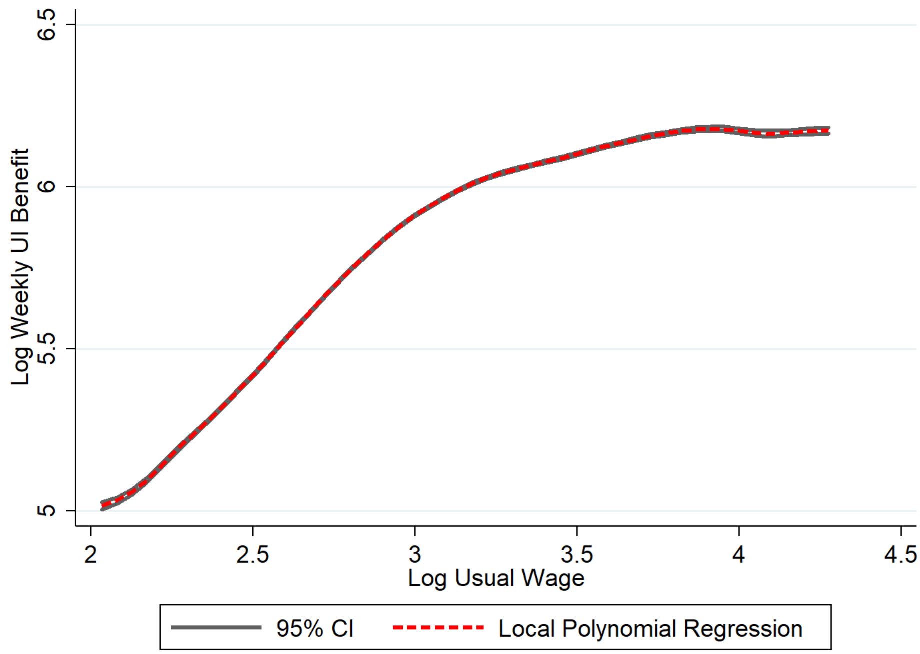


(b) Denied Claims Sample



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

Figure 2: UI Weekly Benefit and Usual Hourly Wage



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

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

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

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

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

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 raw correlation between reservation wage and UI benefits, the coefficient on UI benefits becomes much smaller, though it remains statistically significant, once the usual wage and other controls are added to the regression.[22] The same pattern emerges in the last two columns, which display results for the 2014–2022 period. These estimates are descriptive rather than causal. The denied claims analysis below instead uses cross-state PUA utilization to provide differential-exposure evidence during the pandemic expansion.[23]

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

Figure 4 illustrates how the pool of claimants changed at the onset of Covid-19, contributing to the decline in the average reservation wage shown in Figure 3(a). For instance, the first row of Figure 4 focuses on age. The left panel (a) shows that while the fraction of

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

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

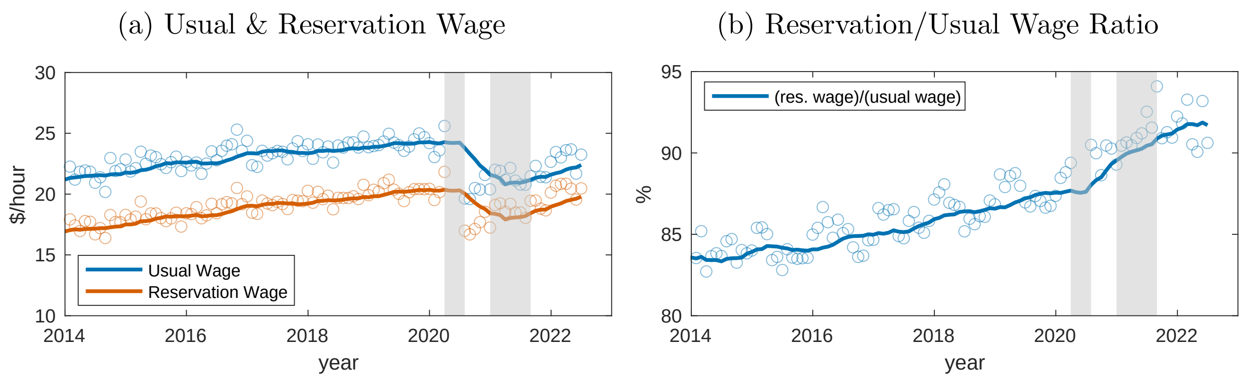
Table 2: Reservation Wage Elasticity with Respect to UI Benefits

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

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

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

Figure 3: Reservation Wage and Usual Hourly Wage



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

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

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

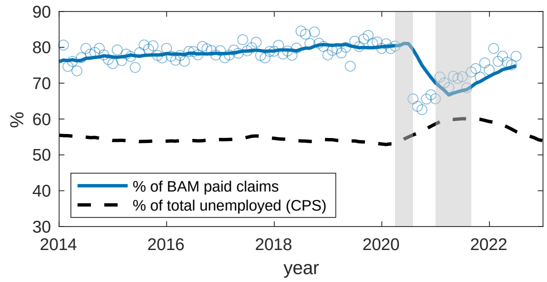
[24]Composition changes in education and sex are less notable: the share of female claimants rose by about 5 percentage points, while education levels showed little change.

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

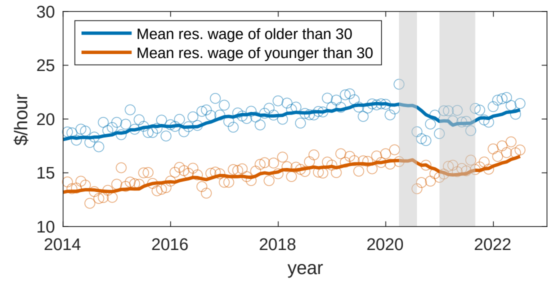
[26]As we emphasized in Section 2, many states chose to end this program prior to the federally mandated sunset date. In the decomposition below, we only include observations during which the FPUC program was active.

Figure 4: Composition changes in the Paid Claims Sample

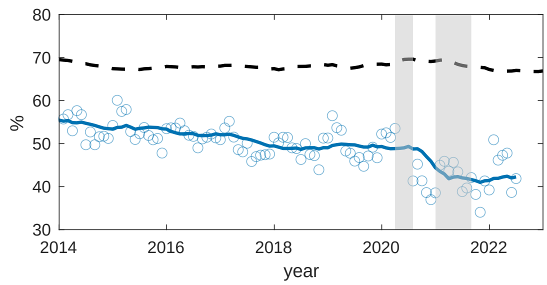
(a) Share of 30+ Yr. Old Workers



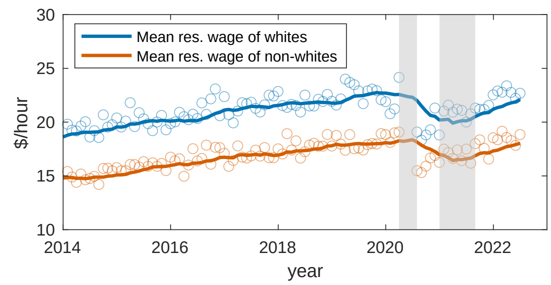
(b) Reservation Wage by Age



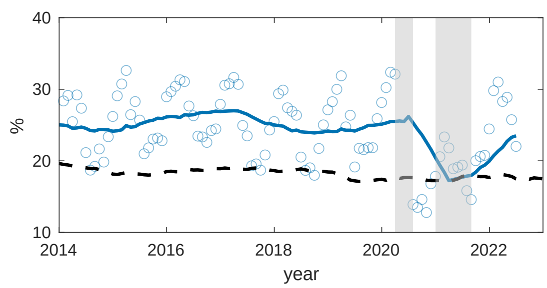
(c) Share of White Workers



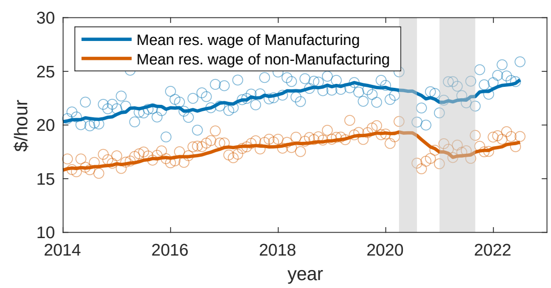
(d) Reservation Wage by Race



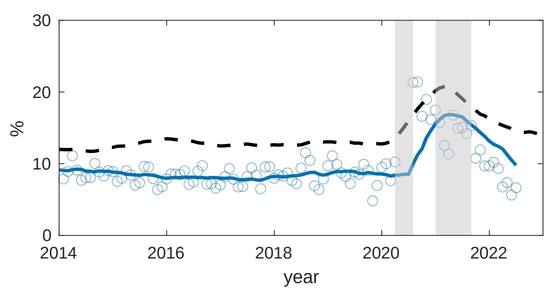
(e) Share of Manufacturing Workers



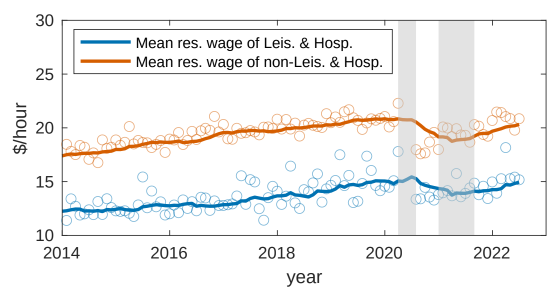
(f) Reservation Wage by Industry



(g) Share of Leisure & Hospitality Workers



(h) Reservation Wage by Industry



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

We run a regression of the log of reservation wages on the log of usual hourly wages and the same covariates as in Table 2 except omitting UI benefits.[27] This regression is run separately for the pre-Covid period, the FPUC period in 2020, and the FPUC period in 2021.[28]

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

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

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

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

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

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

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

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

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

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

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

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, or both. In Section 4 we turn to the CPS data to distinguish between these possibilities. In particular, we find that higher UI benefits, rather than compositional shifts among the eligible unemployed, predict the increase in take-up rates. This pattern is consistent with benefit-driven selection into UI contributing to the observed changes in reservation wages during Covid. The paid claims evidence does not identify the effect of benefit generosity on a fixed population. We next use the monetary-denial sample to examine whether reservation wages evolved differentially with state PUA utilization.

3.4 Reservation Wages and Differential PUA Exposure: Evidence from the Denied Claims Sample

In this section we use the eligibility structure of PUA to examine reservation wages among applicants denied regular UI. Recall from Section 2 that PUA provided benefits to individuals who were not eligible for regular UI. Applicants first had to receive a determination of ineligibility for regular UI before qualifying for PUA, making the BAM denied claims sample a useful population in which to study differential PUA exposure. Workers who qualified for PUA received a minimum disaster unemployment assistance payment equal to half of the average weekly benefit amount in their state. This minimum ranged from $106 per week in Mississippi to $267 in Massachusetts.[29] During periods covered by FPUC, recipients also received an additional $300 or $600 per week.

States varied substantially in their utilization of PUA. This variation reflects a mixture of worker composition, monetary eligibility rules, administrative capacity, program implementation, and local labor-market conditions; it is not randomly assigned.[30] We therefore use state PUA utilization as a measure of differential exposure to the pandemic UI expansion rather than as a pure "treatment." Our analysis asks whether reservation wages among workers denied regular UI for monetary reasons evolved differently in states with greater PUA utilization.

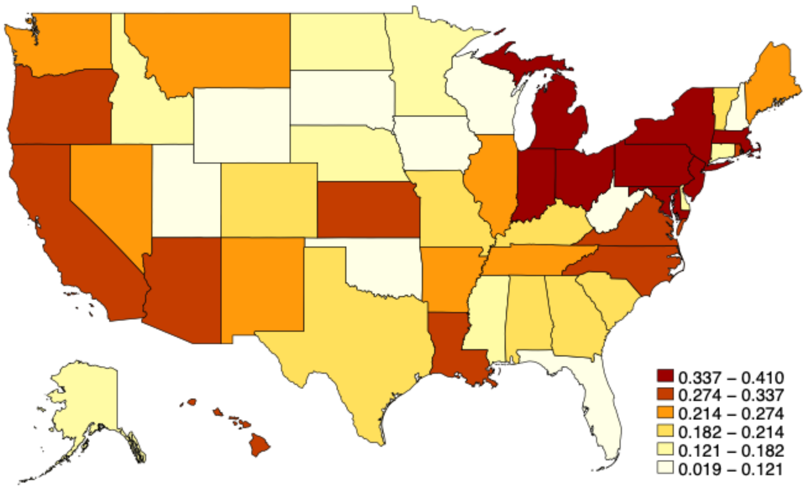
The cross-state variation remains informative because it is not closely related to several prominent observable alternatives. State PUA claim shares display little relationship with unemployment rates or the severity of the Covid-19 pandemic, the event-time results are similar when exposure is measured using initial claims and alternative cutoffs, and placebo groupings based on pre-pandemic state characteristics or pandemic severity do not reproduce the same profile. These exercises motivate the use of PUA utilization as an exposure measure despite its potential endogeneity.

To document differential PUA exposure, we map each state's share of PUA claims in total UI claims over the program window. Figure 5 shows substantial cross-state dispersion in this measure. We use the state PUA claim share as a proxy for expected access to and utilization of PUA. It is a state-level exposure measure rather than an individual-level

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

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

Figure 5: PUA Claims as Share of Total UI Claims



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

eligibility measure, and, because it is based on realized claims, it may reflect both program access and endogenous claiming behavior.[31]

In our baseline specification, states with PUA claim shares above the median form the high-exposure group, while states below the median form the low-exposure comparison group. Throughout, these groups are shorthand for high and low PUA exposure: PUA was available nationally, so the low-exposure group is not an untreated counterfactual.

The outcome is the log reported reservation wage, Y. Let H denote the high-exposure group and L the low-exposure group. Following the event-time notation in Callaway and Sant'Anna (2021), for $t \geq g$ we define the unadjusted difference in changes as

$$\Delta_{HL}(t) = \mathbb{E}[Y_{i,t} - Y_{i,g-1} | i \in H] - \mathbb{E}[Y_{i,t} - Y_{i,g-1} | i \in L].$$

Conditional on covariates, the corresponding event-time differential is

$$\Delta_{HL}(t|X) = \mathbb{E}[Y_{i,t} - Y_{i,g-1} | X, i \in H] - \mathbb{E}[Y_{i,t} - Y_{i,g-1} | X, i \in L].$$

Because PUA was available in every state, these coefficients measure how reservation wages

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

evolved in high-exposure states relative to low-exposure states; they should not be read as an average treatment effect relative to an untreated group. PUA began nationally at the same time, although some states ended the program early.[32] Our controls include the log of usual hourly wage, age bins, sex, race/ethnicity, and NAICS sectors.

Figure 6 displays the adjusted event-time differences. Relative to low-exposure states, high-exposure states exhibit higher reservation wages during the PUA period. The first two quarters after the vertical dashed line correspond to 2020 Q3 and 2020 Q4. The differential rises further in 2021 Q1 and 2021 Q2, when the FPUC program provided an additional $300 in weekly payments to benefit recipients. As PUA winds down in the third quarter of 2021, the reservation-wage difference between the high- and low-exposure groups fades. For workers who were monetarily ineligible for regular UI, this profile is consistent with a modest positive reservation-wage response in states with greater PUA exposure.

3.5 Interpretation and Robustness of the Differential-Exposure Design

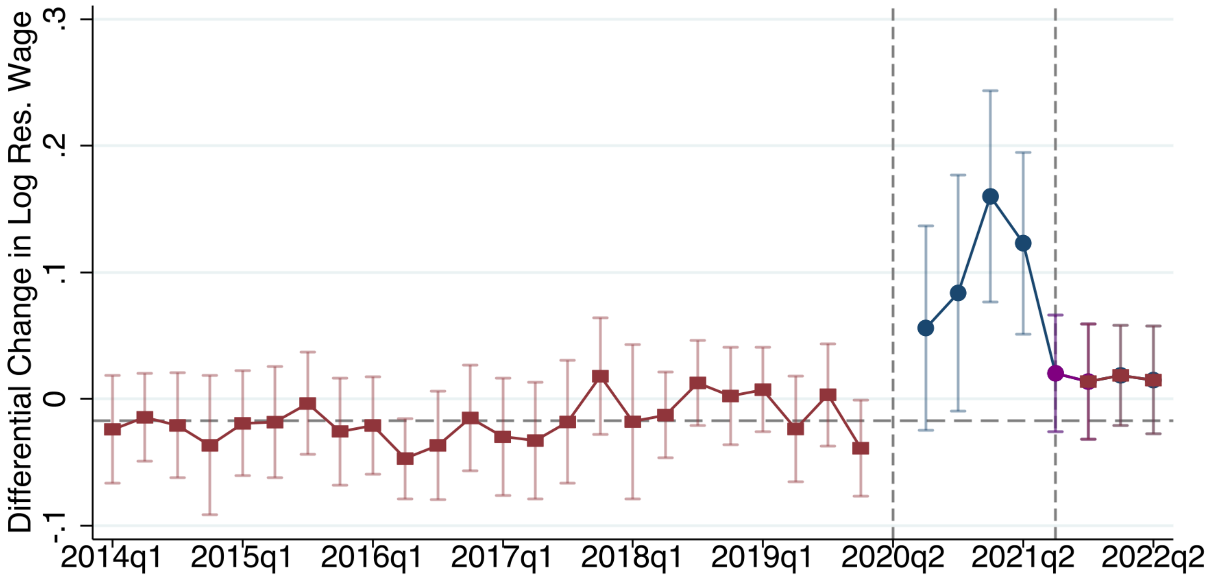
Interpreting the event-time differential as evidence about UI exposure requires that, absent differences in PUA utilization, high- and low-exposure states would have followed similar reservation-wage trends. It also requires that unobserved state factors correlated with PUA utilization did not begin to affect reservation wages differently at the same time. The long pre-period provides a useful diagnostic: the event-time plot shows no clear differential pre-trends and statistically indistinguishable reservation wages across the two groups before PUA.

The design also requires that workers did not adjust reservation wages before the program based on anticipated differences in state PUA utilization. In addition, a state-level version of the Stable Unit Treatment Value Assumption requires that reservation-wage outcomes in low-exposure states are not affected by outcomes in high-exposure states.[33] Throughout

[32]As emphasized in Section 2, some states chose to end the PUA program as early as July 2021. We abstract from the staggered phase-out in this specification and explicitly consider time variation in PUA exposure in Section 3.6. We report estimates using the doubly-robust estimator of Callaway and Sant'Anna (2021) implemented via `csdid`. In our non-staggered setting, it reduces to a single high- versus low-exposure comparison with covariate reweighting.

[33]This should not be conflated with a local spillover effect through mechanisms such as search congestion or local equilibrium effects, as measured in Doniger and Toohey (2022).

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



Notes: Dependent variable is the log reservation wage. There are no observations in 2020q1 and 2020q2. The PUA period starts in 2020q3 and ends in 2021q3, though some states ended PUA as early as July 2021. Confidence intervals are calculated from robust standard errors clustered at the state level.

this discussion, high- and low-exposure refer to relative PUA utilization; neither group is unexposed to the federal program.

Our baseline exposure measure is the state-level share of PUA claims in total claims (regular UI + PUA) over the period in which PUA was active. It incorporates both initial and continuing claims, which may be a useful proxy for perceived access because potential applicants observe the stock of PUA recipients rather than only new inflows. Continuing claims also embed information about spell duration and local labor-market conditions, however, making the measure potentially endogenous to search behavior and reservation wages. We therefore construct an alternative measure based only on initial PUA claims as a share of total initial (UI + PUA) claims. Figure 18 documents its cross-state distribution, and Figure 19 shows an event-time profile nearly identical to the baseline. This comparison reduces the concern that continuing-claim duration mechanically drives the result, although it does not eliminate other sources of endogenous PUA utilization.

We also assess sensitivity to the cutoff used to define high and low exposure. In Appendix A.3, we re-estimate the event-time specification using different quartile comparisons.

We first compare states in the 50th–75th and 75th–100th percentiles of PUA utilization separately with states below the median. Figure 15 shows larger event-time differentials for the higher-utilization quartile. We then use the bottom quartile as the low-exposure comparison group. Figures 16–17 show that the high-exposure differential remains and is most pronounced for the highest-utilization quartile. Throughout, the comparisons are between degrees of PUA exposure rather than between treated and untreated states.

A further concern is that PUA utilization may proxy for pre-existing state characteristics or cyclical conditions unrelated to UI generosity. We therefore conduct a series of placebo exercises in Appendix A.3.3. First, we use a placebo event date around the Great Recession, a severe labor-market downturn with no analogous policy expansion for the monetary-denial population. Figure 20 shows that states later classified as high PUA exposure do not exhibit the same positive reservation-wage differential around this earlier downturn. If anything, their reservation wages fall slightly at its onset, in contrast to the post-2020 profile.

Second, we form placebo high- and low-exposure groups using pre-Covid state characteristics that could be correlated with both PUA utilization and reservation wages. Using 2019 data from the Quarterly Census of Employment and Wages, we split states by average wages, manufacturing employment shares, and leisure-and-hospitality employment shares. As shown in Figure 21, none of these placebo groupings reproduces the persistent reservation-wage differential associated with high PUA utilization.

Finally, we examine whether the PUA-utilization profile is reproduced by measures of health risk or pandemic-induced layoffs. We divide states by Covid death rates per capita during the PUA period and by the increase in the unemployment rate from 2019 Q4 to each state's pandemic peak. Figure 22 shows that states with higher Covid mortality or larger unemployment spikes do not exhibit the same sustained reservation-wage differential as high-PUA-utilization states. The estimated deviations are modest, often statistically insignificant, and do not align with the timing of PUA and FPUC.

These exercises show that the PUA-utilization pattern is not reproduced by several prominent measures of pandemic severity, labor-market disruption, or pre-pandemic state composition. They strengthen the interpretation of the estimates as evidence related to differential UI exposure, while leaving open the possibility of other time-varying state factors correlated with PUA utilization. The binary high-/low-exposure comparison also compresses substantial variation in utilization and does not map directly into the structural model or an elasticity. We therefore turn next to a continuous specification that uses quarterly variation in PUA exposure to construct an implied elasticity of reservation wages with respect to benefit generosity.

3.6 Regime-Specific PUA Exposure and Reservation Wages

We would like to estimate an elasticity of reservation wages with respect to UI benefits. This continuous specification asks whether the cross-state relationship between PUA utilization and reservation wages changes when the \$300 FPUC supplement is added. Following Callaway et al. (2024), we estimate separate PUA-exposure gradients for three regimes: PUA without FPUC in 2020 Q3 and Q4, PUA with the \$300 FPUC supplement in 2021 Q1 and Q2, and the phase-out in 2021 Q3. The principal object of interest is the difference between the PUA + FPUC and PUA-only gradients, $\alpha_{\text{PUA+FPUC}} - \alpha_{\text{PUA}}$. By comparing coefficients across these regimes *at a fixed level of PUA intensity, S*, we obtain the change in predicted reservation wages used below to construct an implied elasticity with respect to UI benefits. We report the corresponding condensed-regime results in Appendix A.4. We measure quarterly exposure as state PUA claims divided by the number of unemployed individuals.[34] This specification uses both cross-state and quarterly variation in the intensity of PUA exposure:

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

where D_{rt} indicates regime r in quarter t and S_{st} is the quarterly average of PUA claims as a fraction of the unemployed population in state s. Each α_r is the regime-specific gradient relating PUA utilization to reservation wages. Column (1) of Table 5 reports these gradients. The PUA-only gradient is not statistically different from zero, while the difference $\alpha_{\text{PUA+FPUC}} - \alpha_{\text{PUA}}$ is 0.076.

[34] This denominator is intended to capture the share of potential PUA claimants and collectors in the current stock of unemployed. Appendix A.4.1 reports results using alternative measures of quarterly PUA intensity, including PUA claims as a share of total regular UI and PUA claims, PUA claims as a share of denied claims, PUA initial claims as a share of unemployment, and initial PUA claims as a share of total initial regular UI and PUA claims. These alternatives yield estimates of similar magnitude.

This difference-in-slopes comparison is less sensitive to a permanent cross-state relationship between PUA utilization and reservation wages than either regime-specific coefficient considered separately. Its interpretation nevertheless requires that, absent the supplement, the relationship between PUA utilization and other determinants of reservation wages would have remained stable across the two regimes. The stability condition could fail if state labor-market conditions or Covid severity evolved with PUA utilization across the regimes. We therefore add the quarterly state unemployment rate and the quarterly average of weekly Covid-19 deaths per 100,000 residents. Column (2) of Table 5 shows that the exposure-gradient contrast is similar after including these controls.[35] These results reduce the concern that the exposure-gradient contrast is driven by observed differences in labor-market conditions or Covid severity, although other state-level factors may still contribute to the pattern.

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

Column (2) of Table 5 reports this specification. As an additional check, we interact the Covid and labor-market controls with quarterly indicators, allowing their relationships with reservation wages to vary over time:

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

Column (3) of Table 5 reports this specification. The PUA-only gradient is small, while the PUA + FPUC gradient is larger and statistically significant. Their difference is 0.123. Thus, the differential-exposure pattern remains after allowing the associations with these observed state conditions to vary by quarter.

We next add time-interacted measures of Oxford stringency, Oxford workplace-closing restrictions, and state-level regular UI monetary-eligibility thresholds in 2020. These observables account for some, but far from all, of the cross-state dispersion in PUA intensity: in a joint state-level balance regression they explain less than half of the variation in baseline PUA utilization. Appendix A.4.2 shows that the implied elasticity is 0.115 once these controls are

[35] Appendix A.5 plots PUA utilization, Covid severity, and unemployment for high- and low-PUA-utilization states. The figures retain "Treatment" and "Control" labels for the two exposure groups, but both groups were exposed to PUA.

added, marginally above the upper end of the range reported above. The exposure-gradient contrast remains similar after accounting for these observed state-level factors, although other factors may still contribute.

Table 5: Regime-Specific PUA Exposure Gradients

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

Notes: Each α_r is the regime-specific gradient relating state PUA claims per unemployed person to log reservation wages. "Difference" is $\alpha_{\text{PUA+FPUC}} - \alpha_{\text{PUA}}$. The row labeled "Elasticity" reports the implied elasticity obtained by scaling this gradient difference at the mean PUA intensity by the change in benefit amounts. PUA corresponds to 2020 Q3 and Q4, PUA + FPUC to 2021 Q1 and Q2, and Phase Out to 2021 Q3. All pandemic UI programs ended in September 2021. Robust standard errors, clustered at the state level, are reported in parentheses.

Across the three specifications, the difference between the PUA + FPUC and PUA-only gradients ranges from 0.076 to 0.123. The estimates describe how reservation wages changed in states with different levels of PUA utilization. Moving PUA intensity from zero to one simply scales this state-level relationship and does not compare an individual PUA recipient with a nonrecipient. We use the resulting range to discipline the magnitude of the wage response in the model.

We scale the difference in exposure gradients by the observed change in benefit amounts to obtain an implied elasticity at a representative PUA intensity, $\bar{S}$. Assuming that individuals denied regular UI for monetary reasons received the minimum disaster unemployment assistance payment, average weekly benefits in our sample rose from \$176 to \$476 when the \$300 supplement was available. This calculation translates the state-level reduced-form contrast into a benefit elasticity under the maintained mapping from PUA intensity to expected program exposure.

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

where $\bar{S}$ is the mean PUA intensity in the PUA + FPUC regime during 2021 Q1 and Q2. Using column (3) of Table 5 gives

$$\varepsilon^{\text{imp}}(\bar{S}) = \frac{(0.116 + 0.008) \times 0.871}{0.995} = 0.108.$$

The associated delta-method standard error is 0.039.

Across the three specifications, the implied elasticity ranges from 0.067 to 0.108, with similar estimates using the alternative PUA-intensity measures reported in Tables 15 and 16. The economic magnitude is modest relative to the policy change: a benefit increase of roughly 170% is associated with an estimated reservation-wage increase of about 8–12%, corresponding to an implied elasticity well below one. Appendix B.1 provides a complementary comparison of re-employment wages between workers classified as UI-eligible and ineligible. Because eligibility is imputed and the populations differ, we view the 9% change in the eligible–ineligible wage premium as a robustness check on the magnitude of the estimated elasticity rather than as an estimate of the same parameter.

The BAM analysis therefore provides differential-exposure evidence of a modest reservation-wage response, but it conditions on applicants and cannot show how the broader propensity to claim changed. We next turn to the CPS to measure eligibility and benefit receipt among a wider population of unemployed workers.

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 during the pandemic UI expansion.

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 in the CPS survey are interviewed 8 times over a 16-month period: data are gathered in the first 4 consecutive months (the first rotation), followed by an 8-month hiatus, and a further 4 months of interviews (the second rotation). While basic labor market data (e.g. labor market status) are collected at all 8 interviews, monthly earnings are only measured for the outgoing rotation, i.e. at interviews 4 and 8.

Meanwhile, retrospective income data are collected from all individuals whose rotation includes the month of March, known as the March supplement or the Annual Social and Economic Supplement (ASEC). For example, an individual interviewed in March 2020 is asked detailed questions about income received during calendar year 2019. We use these retrospective earnings data to impute monetary eligibility and potential benefit amounts for unemployment spells occurring in months proximate to the March interview.[36] We also use the March ASEC to measure whether an individual received any unemployment compensation during the previous calendar year. Combining eligibility information constructed from the first rotation with reported UI income from the subsequent March ASEC in the second rotation, we construct an annual take-up indicator for individuals observed in their first rotation.[37]

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.[38] The BLS distinguishes between two

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

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

[38]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

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.

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

The BLS does not elicit whether individuals meet the monetary eligibility requirements of unemployment insurance. We use information from the March Supplement to simulate monetary eligibility and potential benefit amounts, using a generalized version of the methodology outlined in Ganong et al. (2020).[39] While this methodology gives us a sense of the benefits a

the past four weeks.

[39]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.

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.[40] 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.

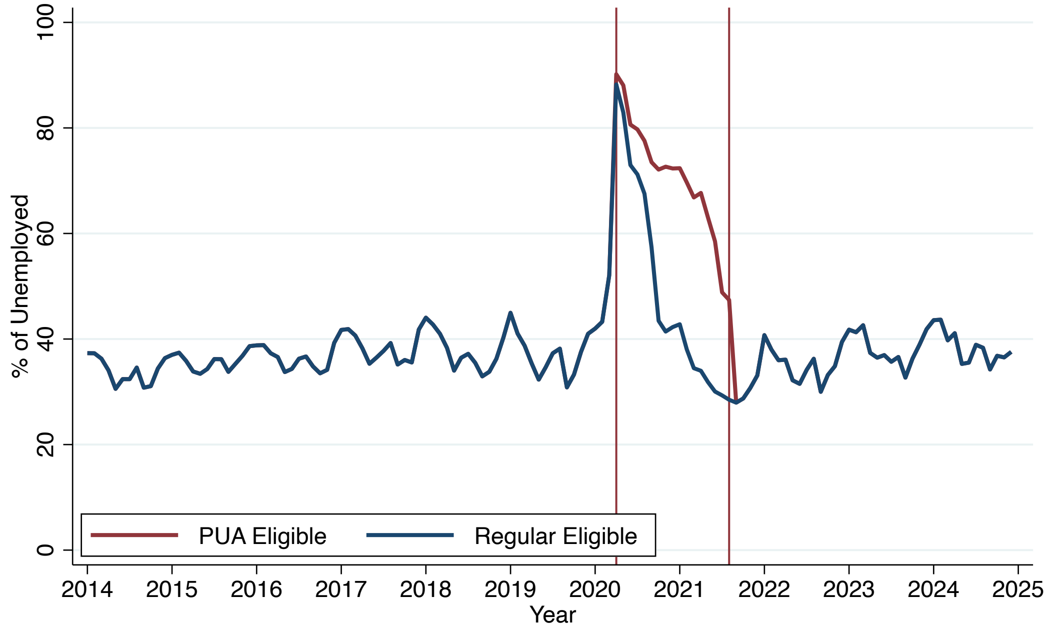
To construct potential benefit amounts, we add \$600 per week from April through July 2020 for all individuals whom we classify as eligible. We similarly add \$300 per week from January 2021 until each state's program expiration date.[41] For states that allowed the program to run until September 5, we include the \$300 supplement through August. These are simulated potential benefit amounts, not measures of actual entitlement or receipt.

Recall that PUA applicants who were ineligible for regular benefits needed to certify that they were unemployed or unable to work because of Covid-related circumstances. To approximate this non-monetary eligibility condition, we use two questions added to the CPS in May 2020 asking whether an individual was unable to work or was prevented from looking for work because of the pandemic. If an unemployed individual answered yes to either question in a given month, we classify that individual as meeting the non-monetary condition during the PUA period. This is an imputed eligibility measure, not confirmation that the individual qualified for or received PUA. The program also extended potential benefit duration beyond the usual 26 weeks. In practice, enforcing these duration limits was challenging during the relief period, given the volume and complexity of pandemic UI programs and the need for states to rapidly modify legacy systems. Combined with the fact that the CPS records weeks unemployed but not weeks of UI receipt, we treat duration limits as effectively non-binding

[40] 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.

[41] At the monthly frequency, we treat the entire month as covered by the supplement if the program ended after the 15th. For example, Iowa ended the program on June 12, so we assign no supplemental benefits in June, while Indiana ended the program on June 19, so we assign the \$300 supplement throughout June.

Figure 7: Non-Monetary Eligibility: regular vs PUA



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

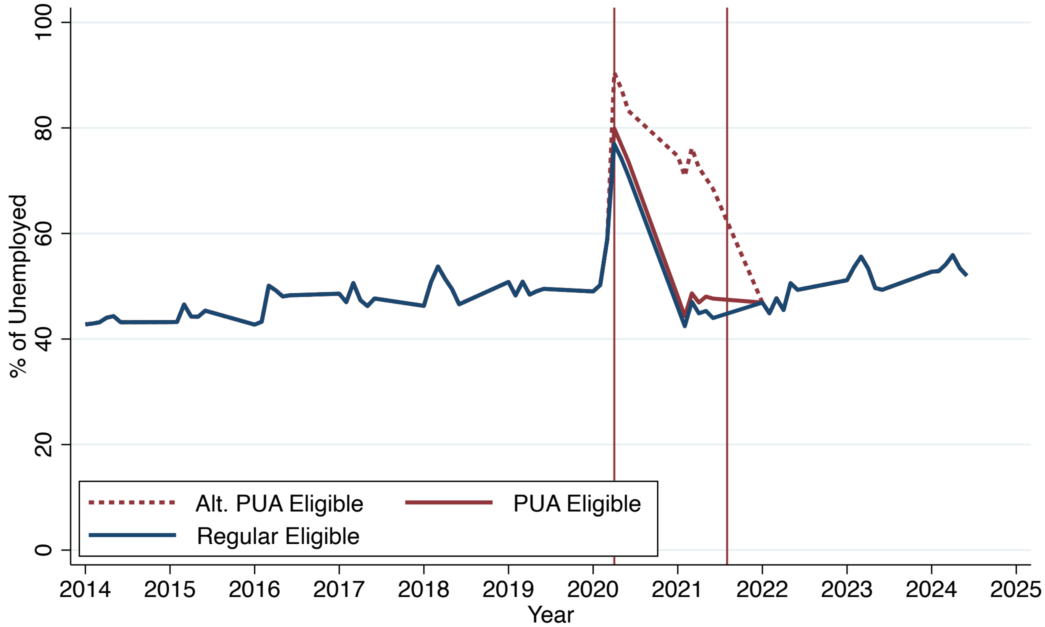
in our CPS-based eligibility measure and do not attempt to track exhaustion at the individual level during the PUA period.[42] Figure 7 shows the increase implied by applying the PUA non-monetary eligibility rules rather than the regular UI rules.[43] The apparent spike in regular non-monetary eligibility at the onset of Covid reflects a compositional shift in the unemployed stock—a surge in laid-off workers, who satisfy the standard non-monetary rules—rather than a change in those rules.

PUA also broadened the forms of prior income relevant to eligibility by covering self-employed workers, including gig workers and independent contractors. Applicants were required to document employment, self-employment, or planned work that was affected by Covid-19. Among applicants who otherwise met the PUA eligibility requirements, weekly

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

[43]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 8: Monetary Eligibility: regular vs PUA

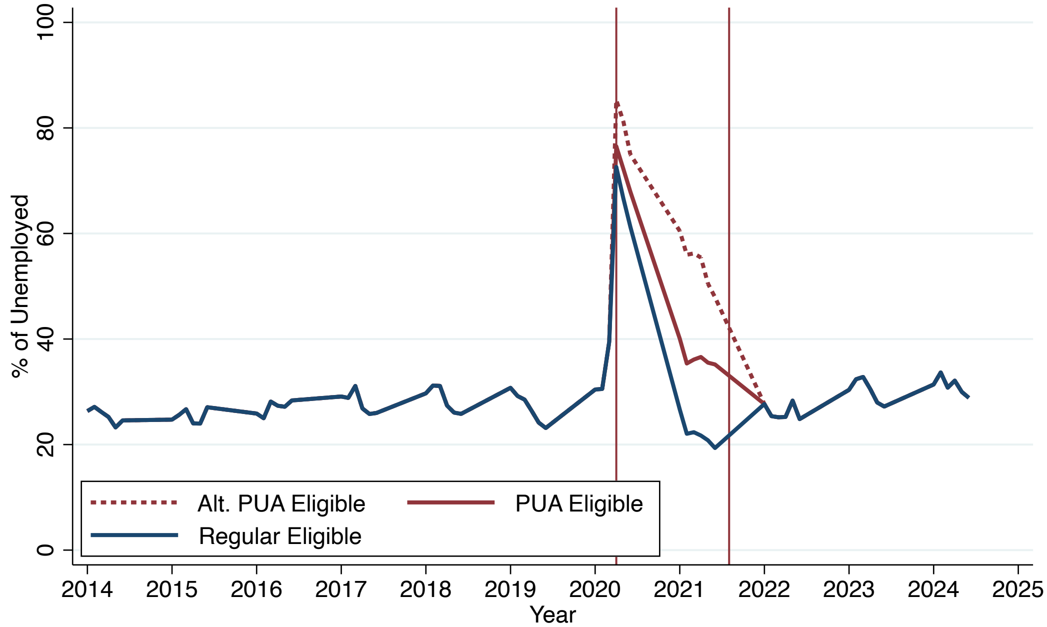


Notes: Regular eligibility refers to the typical rules for monetary eligibility. PUA eligibility refers to monetary eligibility under the PUA program, assuming that state rules apply to earnings plus self-employment income. The alternative measure of monetary eligibility assumes that any positive past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

benefits were subject to a minimum equal to 50% of the state's average regular UI payment, ranging from $106 in Mississippi to $267 in Massachusetts. Because the CPS cannot establish actual PUA eligibility, we construct two proxies for monetary eligibility. The first applies state monetary-eligibility rules to a broader income measure that includes self-employment income as well as earnings. The second, more permissive measure classifies any individual with positive earnings or self-employment income in the previous year as monetarily eligible. Figure 8 compares these constructed PUA measures with the corresponding measure for regular UI.

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.

Figure 9: UI Eligibility: regular vs PUA



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

4.2 Take-up Rate

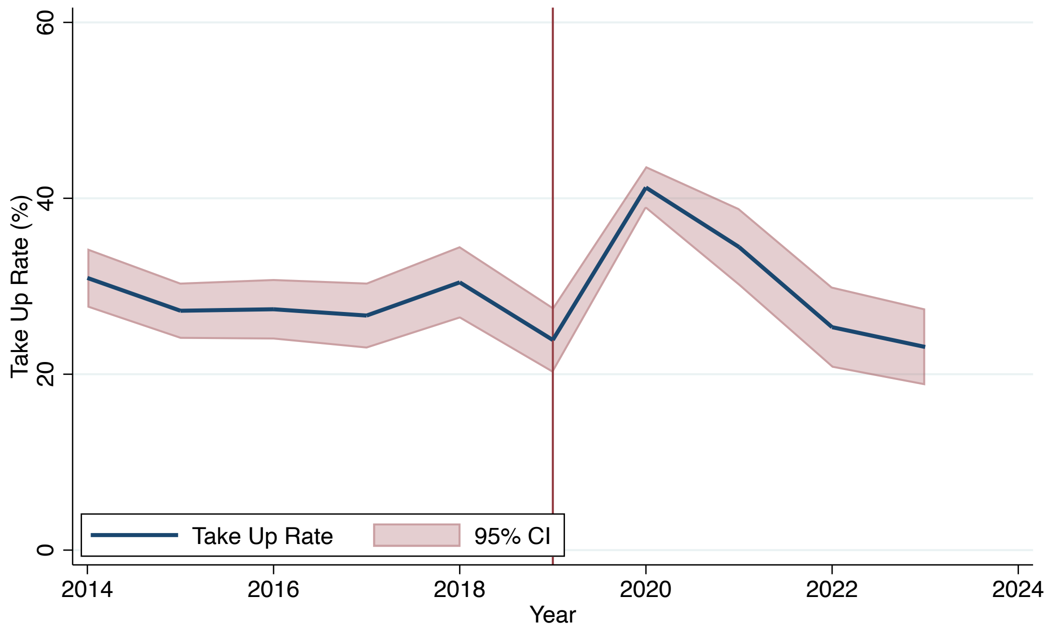
To measure take-up, we identify reported UI receipt among unemployed individuals whom we classify as eligible. The March Supplement asks respondents how much income, if any, they received from unemployment compensation during the previous calendar year.[44] We classify individuals reporting positive unemployment compensation as recipients during the previous year.[45] This measure is available only for individuals whose interview rotation spans March, so we use the report from the second rotation to measure annual receipt for individuals observed 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

[44] The reported amount can include state or federal unemployment compensation, Supplemental Unemployment Benefits (SUB), union unemployment, or strike benefits; the components cannot be separated.

[45] The exact month or months of receipt cannot be identified.

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.

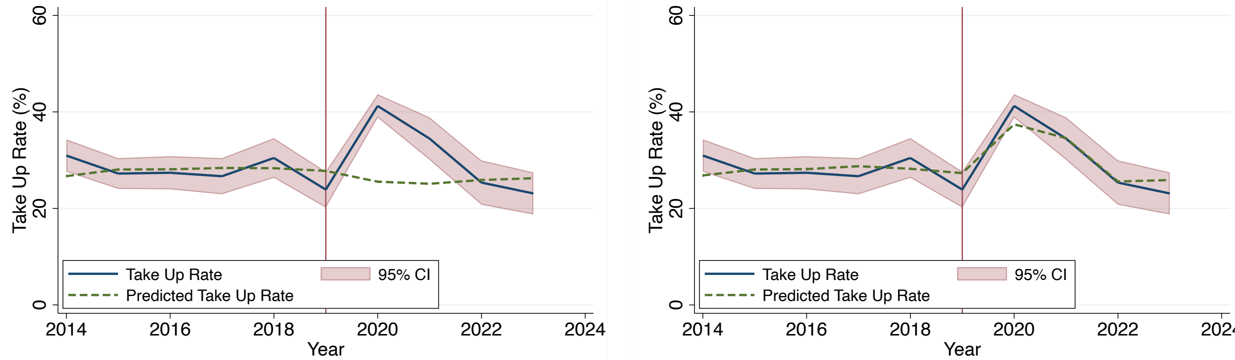
benefits discussed above.[46] The take-up rate increased from around 27% prior to 2020 to 41% in 2020 and 34% in 2021. Surprisingly, the take-up rate among individuals whom we deem eligible under either of our less stringent measures of monetary eligibility is quite similar to that seen in Figure 10: 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.

Many factors could explain the rise in take-up in 2020 and 2021 among individuals classified as eligible for regular UI. Changes in worker characteristics or earnings histories could alter the composition of the eligible population, while features of the expanded UI system summarized by potential benefit amounts could also predict receipt. We estimate a Probit model of the probability that an eligible individual reports UI receipt in normal times (2014–2019) as a function of age, sex, education, race, occupation, industry, state fixed effects, and past earnings, with specifications that include or exclude potential benefit amounts.[47] Our

[46]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.

[47]The Probit is estimated on the same rotation-linked sample used to construct the take-up rate—i.e., individuals classified as eligible in the first rotation and observed in the subsequent March ASEC in the

Figure 11: Actual and Predicted Take-up Rate



(a) Without benefits as a regressor

(b) With benefits as a regressor

Notes: The take-up rate is the fraction of individuals who are eligible to receive regular UI benefits who subsequently report having received UI benefits. The predicted take-up rate uses the coefficient of a Probit regression estimated on 2014–2019 data to predict the take-up rate out of sample for 2020 through 2023. In panel (a), benefits are not included as a regressor; in panel (b), they are.

baseline specification is:

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

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 the two panels of Figure 11, show that the predicted take-up rate closely follows the actual take-up rate only when benefit amounts, which include the \$600/\$300 supplements when applicable, are included as a regressor.[48]

A model estimated on the pre-pandemic period using worker characteristics and earnings alone does not reproduce the increase in take-up. Adding simulated total benefit amounts allows the predicted series to track the rise much more closely. We interpret this exercise as showing that observable changes in the composition of eligible workers are insufficient to

second rotation. The unit of observation is the individual–calendar-year: for each person-year, Collect_i records whether the respondent received any UI income during that calendar year, and all covariates (age, sex, education, race, occupation, industry) are constructed from the same CPS respondent in that year. Past-year earnings and the simulated benefit amount Benefit_i are computed from the earnings history underlying Collect_i.

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

explain the participation surge and that features of the expanded UI system summarized by expected benefits were quantitatively important. Because benefit amounts changed alongside eligibility rules, administrative frictions, work-search requirements, and the stigma or salience of claiming, the exercise does not identify a causal benefit-level effect on take-up.[49] This motivates modeling UI take-up as an endogenous margin that responds to benefit generosity, as we do in Section 5.

Taken together, the evidence documents a pronounced change in claimant composition, a modest change in recipient wage measures, and a large participation response. The next section introduces a directed-search model with endogenous take-up to ask whether these patterns can arise from a common mechanism and to quantify how endogenous entry changes the recipient-average wage response.

5 Economic Environment

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

5.1 Directed Search with Endogenous UI Take-Up

Agents and Markets

There is a continuum of infinitely-lived workers, and a continuum of firms with positive measure. Workers' per-period utility function $v : \mathbb{R} \to \mathbb{R}$ is twice continuously differentiable,

[49] For a complementary regression-kink design that isolates the benefit-level effect on take-up, see McQuillan and Moore (2025b).

strictly increasing, and weakly concave, with derivative $v'(\cdot) \in [\underline{v}', \bar{v}']$, where $0 < \underline{v}' \leq \bar{v}'$. For illustration purposes and to avoid unnecessary algebraic complexity, we assume $v(c) = c$. We relax this assumption in the calibrated version of the model. Both workers and firms have a common discount factor $\beta \in (0, 1)$.

Workers are heterogeneous in the utility cost of filing for and collecting UI benefits, denoted $\varepsilon \in [\underline{\varepsilon}, \bar{\varepsilon}]$, which is distributed according to cumulative distribution function $F(\varepsilon)$ with density $f(\varepsilon)$.[50] If a worker chooses to collect UI benefits, he receives flow consumption b and incurs utility cost ε. If instead the worker chooses not to collect UI benefits, he receives flow consumption d, with $v(b) - \bar{\varepsilon} < v(d) < v(b) - \underline{\varepsilon} < v(b)$.

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

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

Firms

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

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

[50] 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.

where

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

Combining these two equations yields the free entry condition

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

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

Workers

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

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

and therefore

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

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

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

subject to

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

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

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

subject to

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

The envelope condition implies that

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

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

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

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

Decision to Collect UI Benefit

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

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

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

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

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

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

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

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

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

Proposition 1 *The equilibrium has the following properties:*

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

5.1.1 Contribution of Collection Margin to Wage Response

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

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

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

These equations imply that:

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

The average wage of collectors is then given by:

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

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

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

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

The second term reflects the impact of UI benefits on job-finding rates. The sign of this term cannot be determined without additional assumptions. However, if the job-finding rates decline more for workers with the lowest collection costs—those who are most sensitive to changes in b—then an increase in UI benefits tilts the distribution of employed collector types toward those with higher collection costs. These individuals earn lower wages, so this compositional shift dampens the response of average wages. [51]

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 collector types within the employed stock. In particular, the increase in UI benefits brings in new types who previously chose not to collect. These marginal collectors have the highest collection costs within the pool and, therefore, the lowest wages.[52] The addition of this new mass of low-wage workers further dampens the effect of higher UI benefits on average wages.

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

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

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

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

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

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

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

Third, we modify the treatment of collection costs. Rather than treating ε as a fixed individual type, we assume that each unemployed spell begins with a new draw from the distribution of collection costs. Workers who are eligible then decide whether to collect UI, as in the stylized model. This assumption simplifies the state space while preserving the key margin of endogenous take-up.[53]

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,

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

Table 7: Calibration results

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

appears in Appendix D. All other aspects of the model remain unchanged from Section 5.1. In the next section, we describe how we parameterize the model to match key labor market moments.

5.3 Parameterization

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

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 $\gamma = 1.3$.[54] We adopt the estimate $\gamma = 1.3$ from den Haan et al. (2000), which implies a calibrated average job-filling rate of about 80%, consistent with observed monthly hire rates between 2014 and 2019 in BLS data.

The exogenous separation rate is set to $\delta = 0.015$, which yields an unemployment rate of 3.8% in steady state. The vacancy posting cost, $k = 3.02$, is calibrated to generate a job-finding probability of approximately 38% at the monthly frequency, in line with CPS

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

data from 2015 to 2020.[55] The probability that an employed worker has the opportunity to search, λ_e, is set to 0.224—matching the share of employed workers who report actively searching in the SCE Job Search Supplement, as reported by Faberman et al. (2022).

We assume that the utility cost of filing for UI benefits is uniformly distributed: $\varepsilon \sim U[\underline{\varepsilon}, \overline{\varepsilon}]$. The lower bound $\underline{\varepsilon}$ is normalized to zero, and the upper bound $\overline{\varepsilon}$ is set so that the steady-state UI take-up rate matches our baseline estimate of 27% in the CPS for the 2014–2019 period (see Section 4.2).

We assume that all unemployed individuals receive a flow value of $d = 0.2$ from non-employment, capturing home production or other non-market activities. Unemployment benefits are set such that net benefits equal $b - d = 0.4$, implying a gross benefit level of $b = 0.6$. This calibration delivers an average replacement rate of 42.5 percent in the benchmark economy, where the replacement rate is calculated as the ratio of net benefit $b - d$ to average wage of eligible workers. [56] Output of an employed worker is normalized to $y = 1$.

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

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

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

quantitative exercise.

5.4 Quantitative Experiment

Using the calibrated model, we conduct the following experiment. We start the model in steady state. In period 0, we introduce an unexpected separation shock that raises the unemployment rate to 15%. This shock lasts for a single period. To capture the lax eligibility criteria early in the Covid response, we assume that all newly separated workers are eligible for UI benefits. In order to match the observed time path of unemployment and the job-finding rate after March 2020, we assume that the cost of creating a vacancy increases by 50% in period 0 and remains elevated for 12 months. Thereafter, the cost of creating a vacancy gradually declines, returning to its benchmark value by month 24. This path generates a decline in the job-finding rate and a slow recovery of the unemployment rate, in line with the observed post-shock dynamics.

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

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

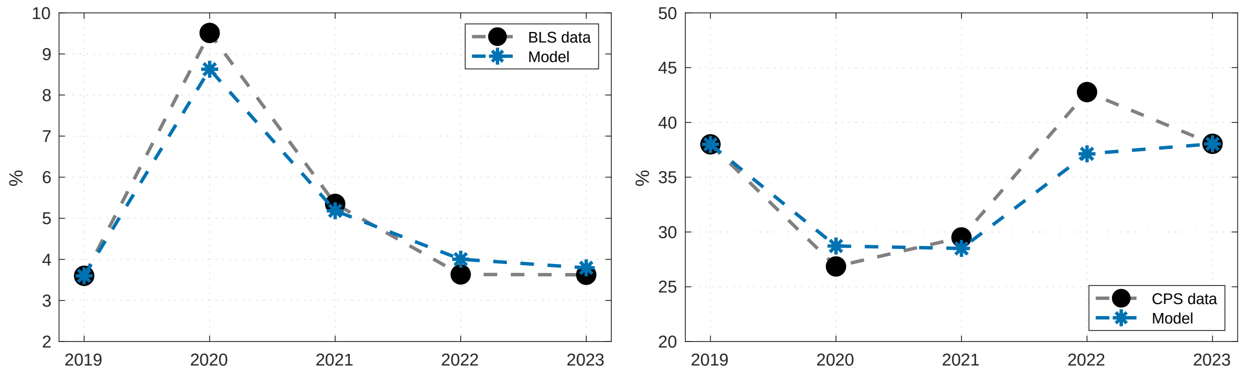
In month 11, the benefit increases again—this time by 80%—and remains at that level for eight months. This adjustment corresponds to the Covid Relief Bill, which raised weekly benefits by $300, i.e., half the amount provided under the CARES Act. After month 18, the benefit returns to its benchmark level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

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

Next, we use the model to quantify the extent to which the endogenous UI take-up margin dampens the response of the reservation wage to an increase in UI benefits. Figure 13(a) plots two transition paths for the UI take-up rate. The blue line shows the take-up rate (averaged over monthly model output) in the benchmark model with endogenous benefit collection.

Figure 12: Unemployment Rate and Job Finding Rate

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



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

The orange line shows the corresponding path in a model where take-up is exogenous. In this alternative specification, the decision to collect UI is fixed: the threshold ε^* is held at its pre-shock value.

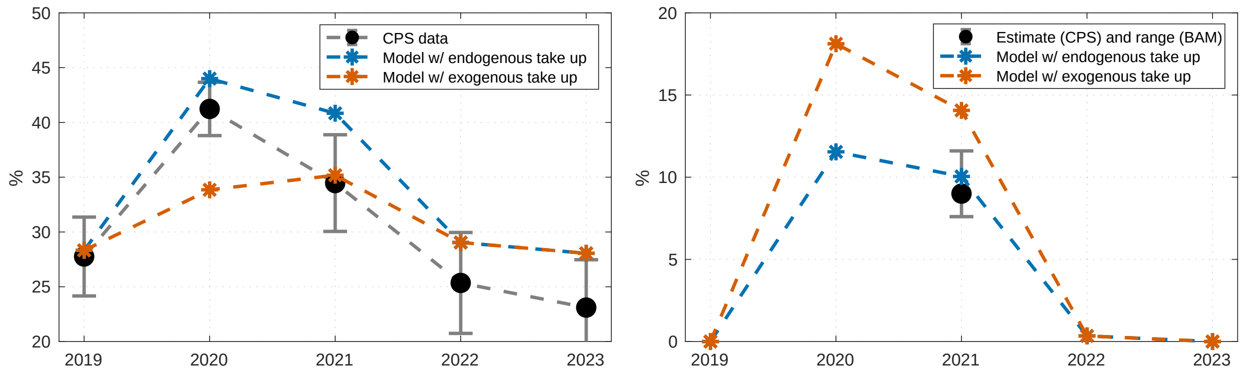
Despite fixed individual collection decisions in the exogenous take-up model, the aggregate take-up rate evolves over the transition due to stock-flow composition effects. In particular, the large initial inflow into unemployment mechanically changes the composition of UI-eligible and ineligible individuals.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

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

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

(a) UI Take-up Rate

(b) Δ Collectors' Wage Premium



Notes: The line 'CPS data' in panel (a) is from Section 4.2. The estimate in panel (b) is from CPS data as shown in Table 18 of Section 4, while the range of estimates is from BAM data as found in Section 3.6. Monthly data are aggregated to annual by simple averaging.

Figure 13(b) plots the change in the collectors' wage premium—defined as the difference between the average log wage of collectors and that of non-collectors—relative to its steady-state (pre-shock) value. The model results are shown for two versions of the environment: one with endogenous take-up and one with a fixed take-up rate. For reference, the figure overlays our CPS-based estimate of the change in the wage premium (9%, from Table 18) and the range implied by BAM data (7.6% to 12.3%, see Section 3.6).

The comparison highlights that the endogenous take-up decision is a central mechanism shaping the pass-through from UI benefits to wages. Higher benefits raise the wages of existing collectors, but two forces attenuate this effect: (i) job-finding rates decline more among the most responsive types, and (ii) the pool of collectors expands to include marginal, high-cost types who search in tighter markets and accept lower-paying jobs. This latter channel—absent when take-up is fixed—is quantitatively important, reducing the wage response by about 30%. The magnitude and direction of this dampening effect align closely with our BAM and CPS evidence, reinforcing the view that much of the labor market response to UI policy changes operates through the participation margin rather than wage setting alone.

6 Conclusion

We use data from BAM and CPS to document modest changes in recipient wage measures alongside a large increase in UI take-up during the pandemic expansions. In BAM, reservation wages rose more in states with greater PUA utilization, producing small implied elasticities across the differential-exposure specifications. Because PUA utilization is not randomly assigned, we interpret these estimates as exposure-gradient evidence rather than as individual treatment effects. The CPS provides a complementary comparison: the eligible–ineligible re-employment wage differential changed modestly, while take-up rose sharply. A prediction model estimated on pre-pandemic data tracks the increase in take-up much more closely when expected benefits are included. Because benefit amounts changed together with eligibility rules and other program features, this exercise establishes their predictive importance rather than the causal effect of benefit levels alone.

To interpret these patterns, we develop a directed search model with endogenous UI take-up. In the model, higher benefits raise the wages of existing collectors but also induce marginal workers with higher claiming costs and lower wages to collect. Their entry attenuates the increase in the recipient-average wage. When calibrated to match pre-pandemic moments, the model reproduces the modest movement in wage measures and the large change in take-up; the endogenous take-up margin reduces the recipient-average wage response by about 30%. This mechanism illustrates why changes in claimant-based wage measures reflect both individual wage responses and changes in the composition of the recipient pool. More broadly, the analysis emphasizes the importance of accounting for participation and recipient composition when evaluating temporary UI expansions.

APPENDIX

A BAM

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

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

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

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

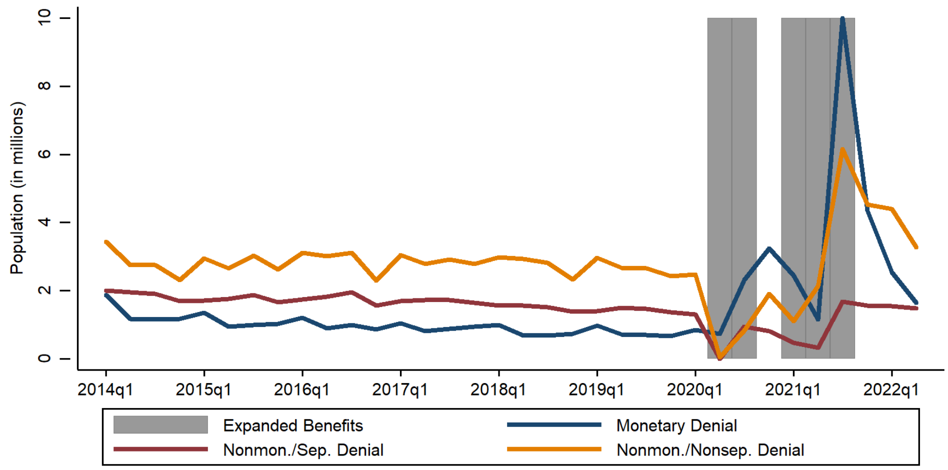
NAICS Sector	Paid Claims	All Denied Claims	Monetary Denials
11: Ag., Forestry, Fish. & Hunt	0.031	0.010	0.015
21: Mining, Quarrying, & Oil & Gas	0.010	0.009	0.004
22: Utilities	0.002	0.002	0.002
23: Construction	0.122	0.079	0.087
31-33: Manufacturing	0.094	0.099	0.057
42: Wholesale Trade	0.041	0.039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

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

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

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

Figure 14: **Denied Claims Population**



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

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

A.2 Oaxaca-Blinder Decomposition

Here we present the results of the Blinder-Oaxaca decomposition when we include unemployment benefits (excluding FPUC supplement amounts) in the regression specification. The reason one may want to include benefit amounts is they are usually a function of earnings history over a prolonged base period. Heterogeneity in benefit amounts may be informative about the attachment of the worker to the labor force that is otherwise unobservable if we only include usual wages and other covariates. We get similar results to our baseline specification which does not include benefits. Tables 11 and 12 show that the results are similar to

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

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

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

the baseline decomposition. In 2021, including regular UI benefits assigns a slightly larger share of the change in reservation wages to observed characteristics. Most of the increase nevertheless remains in the unexplained component (coefficients), while changes in claimant composition (endowments) partially offset it.

Table 10: States with high quarter wage in benefit determination

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

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

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

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

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

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

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

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

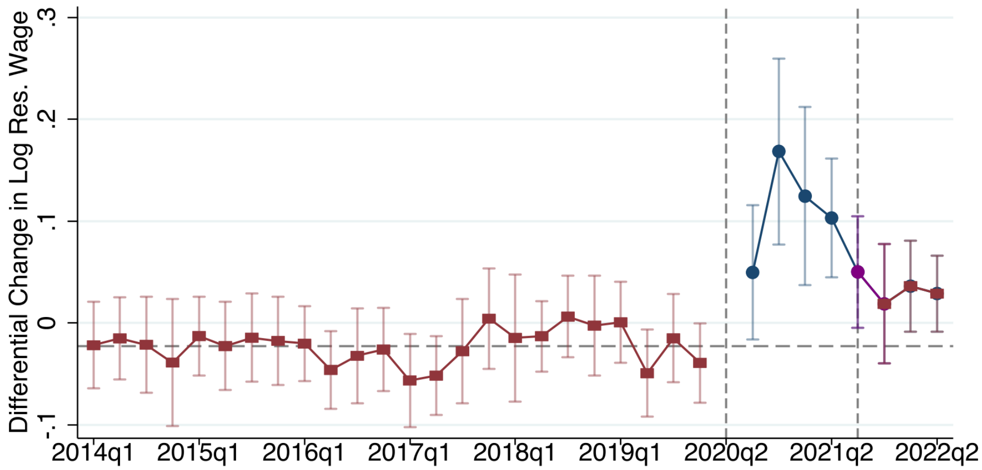
A.3 Binary Differential-Exposure Robustness

A.3.1 Alternative Exposure Cutoffs

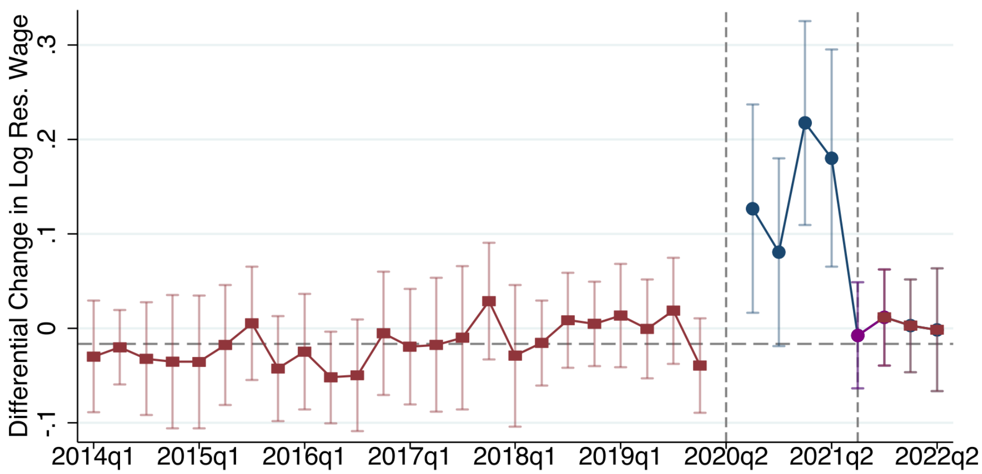
This appendix considers alternative cutoffs for the high- and low-PUA-exposure groups. Throughout, the groups denote higher and lower PUA utilization rather than treated and untreated states. We first divide the baseline high-exposure group into states in the 50th–75th percentile and states in the top quartile of PUA claim share, and estimate a separate event-time differential for each group.

Figures 15(a) and 15(b) compare the third and fourth quartiles separately with the bottom half of states. The event-time differential is larger for the highest-utilization quartile, consistent with an exposure gradient.

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



(a) 3rd quartile as treatment group

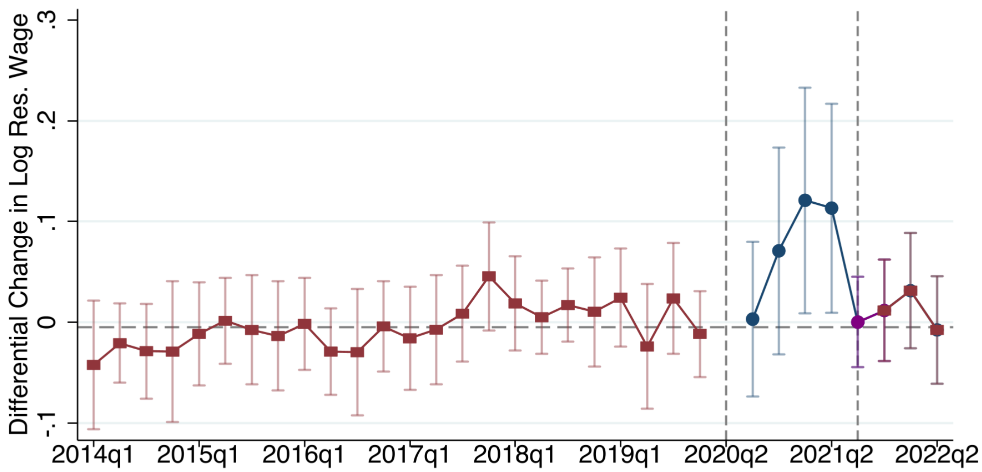


(b) 4th quartile as treatment group

Notes: Dependent variable is the log reservation wage. In both panels the comparison group is states below the median level of PUA claim share (.214) during the PUA period; panel (a) uses states in the 50th–75th percentile (.214–.299) and panel (b) uses states in the 75th–100th percentile (.299–.41). There are no observations in 2020q1 and 2020q2. The PUA period starts in 2020q3 and ends in 2021q3, though some states ended PUA as early as July 2021. Confidence intervals are calculated from robust standard errors clustered at the state level.

We next use the bottom quartile as the low-exposure comparison group. Figure 16 compares the top half of states with this lower-utilization benchmark and yields a profile similar to the baseline. Figures 17(a)–17(c) then compare each of the second through fourth quartiles separately with the bottom quartile. The differential is muted for the second quartile and similar to the baseline for the third and fourth quartiles. This ordering is consistent with the event-time pattern becoming more pronounced as PUA utilization rises.

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



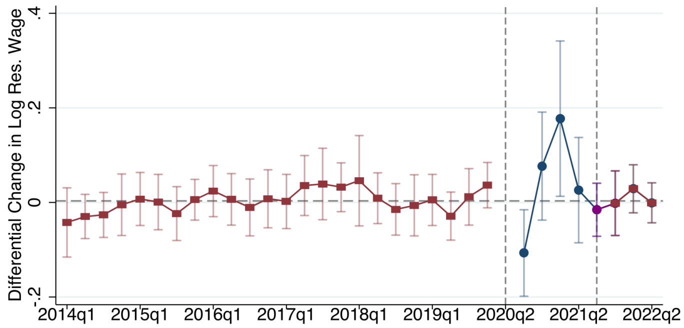
Notes: Dependent variable is the log reservation wage. The comparison group is states in the 0–25th percentile bin by level of PUA claim share (.147) during the PUA period, and the high-exposure group is states above the median level of PUA claim share (.214). There are no observations in 2020q1 and 2020q2. The PUA period starts in 2020q3 and ends in 2021q3, though some states ended PUA as early as July 2021. Confidence intervals are calculated from robust standard errors clustered at the state level.

A.3.2 Alternate PUA Intensity using Initial Claims

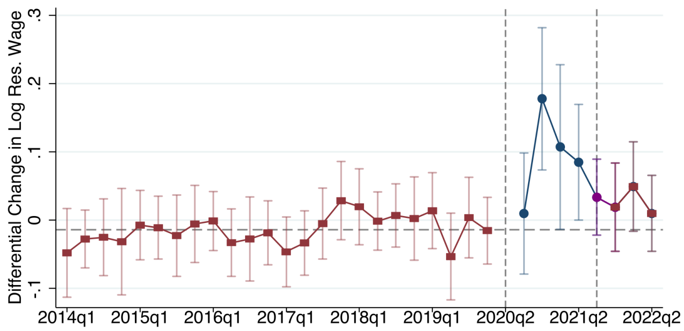
Figure 18 shows the state-level variation of an alternative definition of PUA intensity, the share of initial PUA claims of total (UI + PUA) initial claims.

We re-estimate the baseline high-/low-exposure comparison using the initial-claims measure, splitting states at the median of that measure. Figure 19 shows an event-time profile nearly identical to the baseline figure, indicating that the result is not driven by the duration component embedded in continuing claims.

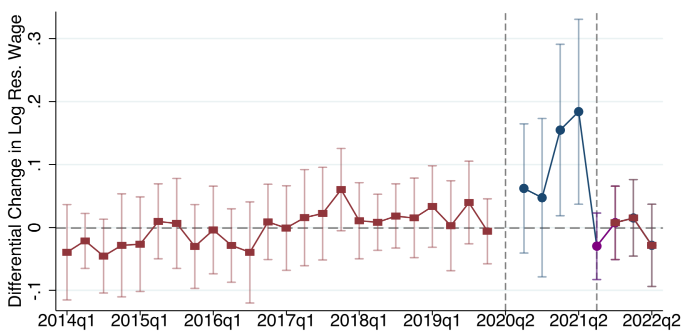
Figure 17: **PUA Intensity: Event-time Profiles with 1st Quartile Control Group**



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



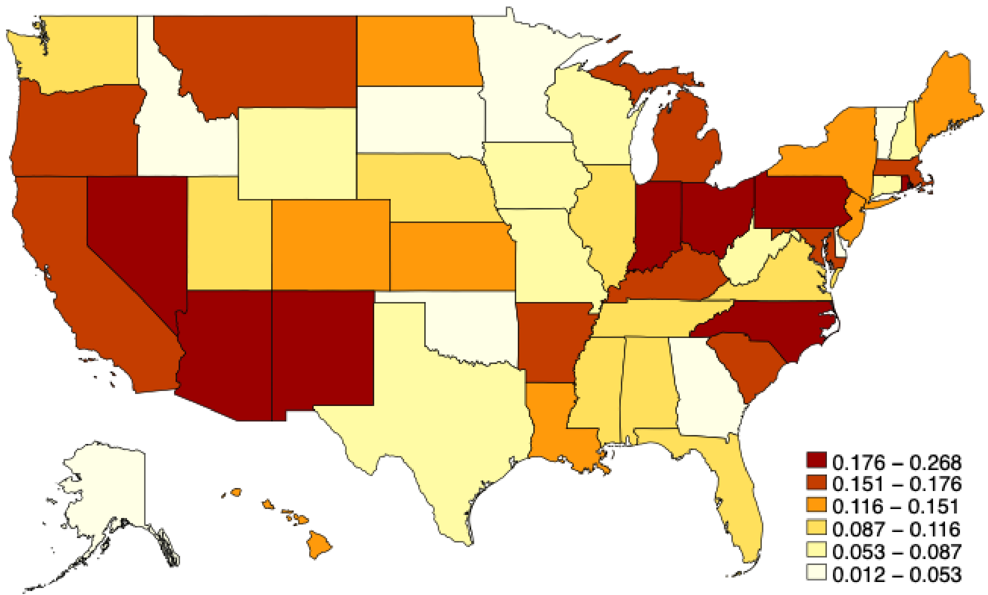
(b) 1st qtile control, 3rd qtile treatment



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

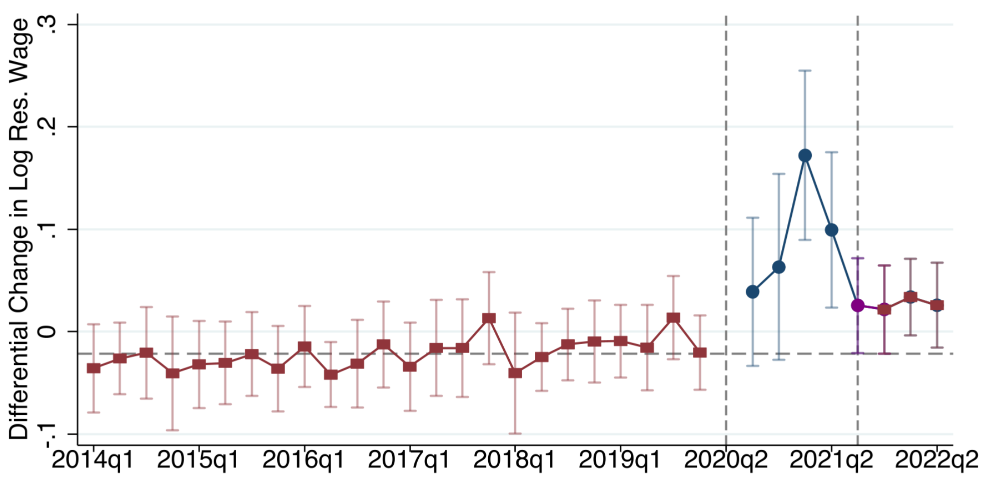
Notes: Dependent variable is the log reservation wage. In all panels the comparison group is states in the 0–25th percentile bin by level of PUA claim share (.147) during the PUA period; panel (a) uses states in the 25th–50th percentile bin (.147–.214), panel (b) uses states in the 50th–75th percentile bin (.214–.299), and panel (c) uses states in the 75th–100th percentile bin (.299–.41). There are no observations in 2020q1 and 2020q2. The PUA period starts in 2020q3 and ends in 2021q3, though some states ended PUA as early as July 2021. Confidence intervals are calculated from robust standard errors clustered at the state level.

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



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

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



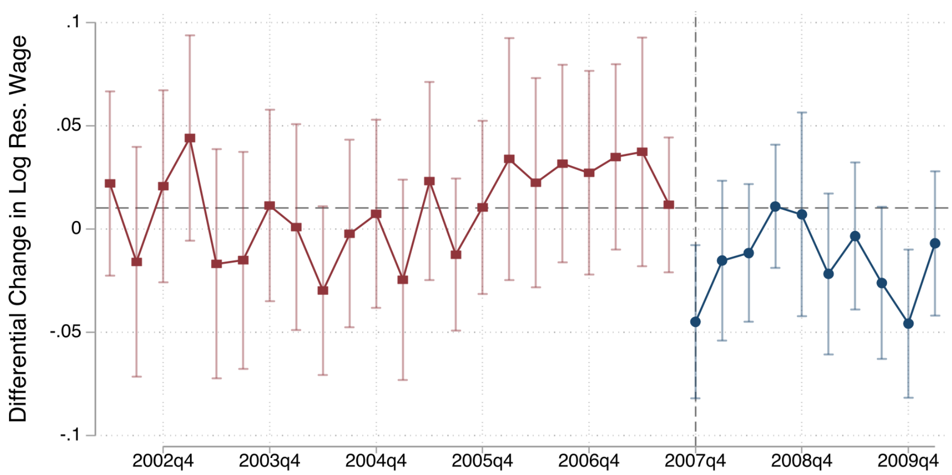
Notes: Dependent variable is the log reservation wage. States are grouped by whether their PUA initial claim share during the PUA period was above or below the median (.116). There are no observations in 2020q1 and 2020q2. The PUA period starts in 2020q3 and ends in 2021q3, though some states ended PUA as early as July 2021. Confidence intervals are calculated from robust standard errors clustered at the state level.

A.3.3 Placebo Tests

This section presents placebo exercises addressing several observable explanations for the baseline differential-exposure pattern. The positive reservation-wage differential associated with PUA utilization may reflect other state-level conditions or mechanisms unrelated to UI generosity. We therefore re-estimate the event-time specification using an earlier downturn as a placebo date and using alternative state characteristics as placebo grouping variables. These exercises test whether selected confounders reproduce the PUA-utilization profile; they do not establish that PUA utilization is exogenous.

The placebo timing addresses the possibility that states with high eventual PUA utilization have different reservation-wage dynamics in severe downturns for reasons unrelated to PUA. We therefore use the onset of the Great Recession as an alternative event date. That recession did not include an analogous eligibility expansion for the monetary-denial population. Figure 20 shows no positive reservation-wage differential for the states later classified as high PUA exposure; their reservation wages instead decline slightly at the onset of the recession.

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



Notes: Dependent variable is the log reservation wage. States are grouped by whether their PUA initial claim share during the later PUA period was above or below the median (.116); neither group faced a comparable eligibility expansion during the Great Recession. The vertical dashed line marks 2007q4, the placebo event date. Confidence intervals are calculated from robust standard errors clustered at the state level.

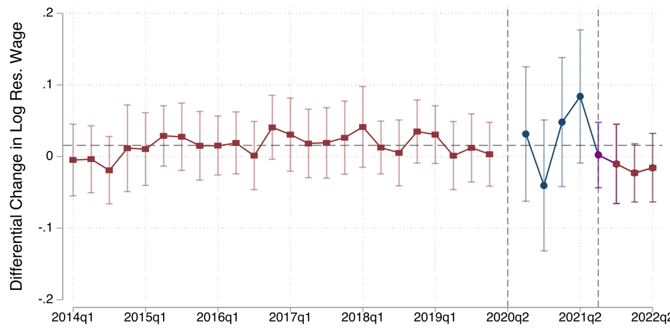
We also consider state characteristics that may be correlated with both PUA utilization and reservation wages. Worker and industry composition, for example, may have affected exposure to Covid-related labor-market shocks. We form placebo high and low groups by splitting states at the median of their 2019 average wages, manufacturing employment shares, and leisure-and-hospitality employment shares.

First, we stratify states using state-level average wages in 2019 as measured in the Quarterly Census of Employment and Wages. High-wage states differ in skill composition, industry mix, urbanization, and remote-work intensity; if these differences accounted for the baseline result, the placebo profile should resemble the PUA-utilization profile. Figure 21(a) instead shows a decline in reservation wages in late 2020 and statistically insignificant increases in 2021.

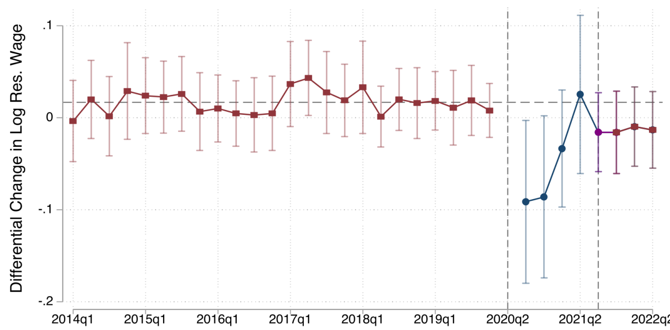
We next stratify states using their 2019 employment shares in Manufacturing (NAICS 31–33) and Leisure and Hospitality (NAICS 71–72) based on QCEW. These industries experienced significant shocks during the pandemic. If sectoral composition accounted for the baseline pattern, high-manufacturing or high-hospitality states would exhibit profiles similar to the PUA-utilization profile. Instead, high-manufacturing states display a negative reservation-wage differential in early 2021, while high-hospitality states display an initial positive differential during the shutdown phase, negative differentials during reopening, and a rebound in 2022. These profiles are qualitatively different from the smoother positive differential associated with PUA utilization.

One concern is that the reservation-wage differential may reflect health risks or the severity of the pandemic. If high-PUA-utilization states also had higher Covid death rates, the differential could capture a risk premium associated with virus exposure. We therefore stratify states by Covid death rates per capita during the PUA period. Figure 22(a) shows that high-mortality states do not exhibit a similar event-time profile. We also stratify states by the increase in unemployment from 2019 Q4 to each state's pandemic peak to assess the role of lockdown-induced layoffs. Figure 22(b) shows slightly higher reservation wages in 2021 among states with the largest unemployment increases, but no increase in 2020; the estimates are mostly statistically insignificant and do not align with the timing of PUA and FPUC.

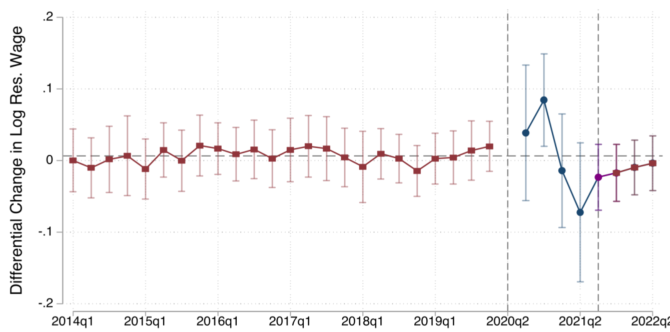
Figure 21: **Placebo Treatments: State Pre-Covid Characteristics**



(a) State average wage



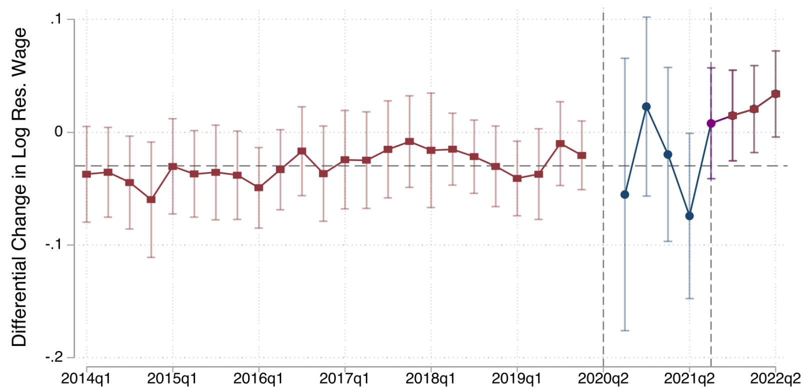
(b) Manufacturing employment share



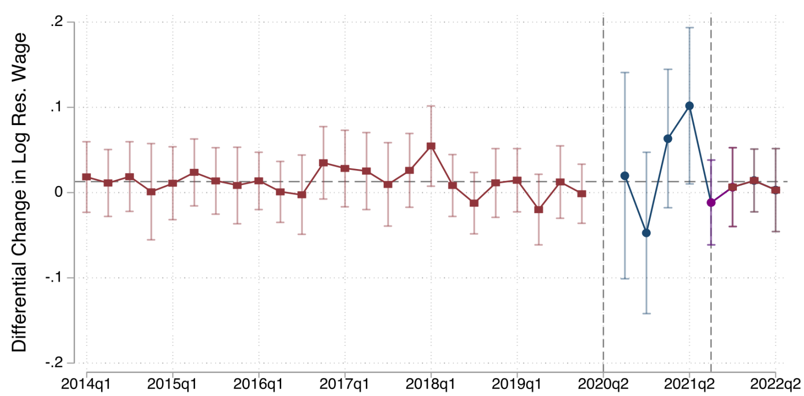
(c) Leisure & hospitality employment share

Notes: Dependent variable is the log reservation wage. Placebo groups split states at the median of a pre-pandemic characteristic measured in 2019 with the Quarterly Census of Employment and Wages (QCEW): average state wages in panel (a), the Manufacturing employment share (NAICS 31–33) in panel (b), and the Leisure and Hospitality employment share (NAICS 71–72) in panel (c). There are no observations in 2020q1 and 2020q2. The PUA period starts in 2020q3 and ends in 2021q3, though some states ended PUA as early as July 2021. Confidence intervals are calculated from robust standard errors clustered at the state level.

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



(a) Covid deaths per 100k



(b) Unemployment rate increase

Notes: Dependent variable is the log reservation wage. Placebo groups split states at the median of a state-level average measured over the PUA period: the Covid death rate per 100k residents in panel (a), and in panel (b) the increase in the unemployment rate relative to 2019 Q4, measured at each state's pandemic peak. There are no observations in 2020q1 and 2020q2. The PUA period starts in 2020q3 and ends in 2021q3, though some states ended PUA as early as July 2021. Confidence intervals are calculated from robust standard errors clustered at the state level.

A.4 Condensed-Regime Binary Exposure Specification

To summarize the timing in our baseline results in Figure 6, we define the categorical variable *regime=[PUA, PUA + FPUC, Phase Out]*. The first coefficient interacts the high-PUA-exposure indicator with *regime=PUA*, which covers 2020 Q3 and 2020 Q4, when PUA benefits were available without the additional FPUC supplement. The second interacts the indicator with *regime=PUA + FPUC* for 2021 Q1 and 2021 Q2, when PUA benefits were available alongside the additional \$300 weekly FPUC supplement. The third interacts the indicator with *regime=Phase Out* for 2021 Q3, during the wind-down of both programs. We also examine sensitivity to Covid-related state-level controls: the quarterly average of weekly Covid deaths per 100,000 residents and the quarterly average of the state unemployment rate.

Comparing the pre-FPUC and FPUC interaction coefficients shows how the high- versus low-exposure reservation-wage differential changed when the additional \$300 weekly supplement was introduced. Because PUA utilization is endogenous and other conditions changed across these periods, the coefficient difference does not isolate the causal effect of the \$300 supplement. It instead provides a concise summary of the differential-exposure pattern shown in the event study.

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

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

where λ_t is a quarterly time dummy, $\mathbb{I}_{high,s}$ equals one if state s is in the high-PUA-exposure group, D_{rt} denotes the three regime indicators introduced above, and X is a set of controls.[57]

The first column of Table 13 reports the interaction coefficient for each regime. We present more detailed results in Appendix A.4.1. During the PUA-only regime, reservation wages rose by about 6% more in high-exposure states than in low-exposure states, relative to the pre-PUA difference. During the PUA + FPUC regime, the corresponding differential is about 11%, or 5 percentage points larger. The second column shows that this pattern is similar after adding the Covid-related state controls.

Finally, we assess whether observable time variation in state Covid severity and labor-market conditions accounts for the differential-exposure pattern. We control for within-quarter, cross-state variation in Covid severity and labor-market tightness as follows:

$$\begin{aligned} Y_{ist} = \lambda_t &+ \sum_{r \in \{\text{PUA}, \text{PUA+FPUC}, \text{PhaseOut}\}} \alpha_r \left(\mathbb{I}_{high,s} D_{rt} \right) \\ &+ \delta_t \left(Covid_{st} \times qtr_t \right) + \gamma_t \left(Urate_{st} \times qtr_t \right) + \beta' X_{it} + \varepsilon_{it}. \end{aligned}$$

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

where $Covid_{st}$ is Covid deaths per 100,000 residents and $Urate_{st}$ is the quarterly average of the state unemployment rate. The estimates in the third column of Table 13 remain positive but are smaller than in the baseline event study. This sensitivity check shows that the profile is not fully accounted for by these observed state conditions; it does not rule out other factors correlated with PUA utilization.

Table 13: Binary Differential-Exposure Regression

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

Notes: Reported coefficients are interactions between the high-PUA-exposure indicator and the indicated regime; low-exposure states form the comparison group. PUA corresponds to 2020 Q3 and Q4 since data are 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 2021. Robust standard errors, clustered at the state level, are shown in parentheses.

A.4.1 Fixed Effects Regressions with a Quarterly Exposure Indicator: Alternate Specifications

We present the results of the regression specification in Table 13, replacing the condensed regimes with quarterly time dummies interacted with the high-PUA-exposure indicator ($\mathbb{I}_{high,s} * qtr_t$).

The binary analysis uses each state's PUA share of Covid-era UI claims to form high- and low-exposure groups. The continuous specification instead measures quarterly PUA exposure as PUA claims per unemployed person, which may better reflect the share of potential

Table 14: Binary Differential-Exposure Specification: Quarterly Coefficients

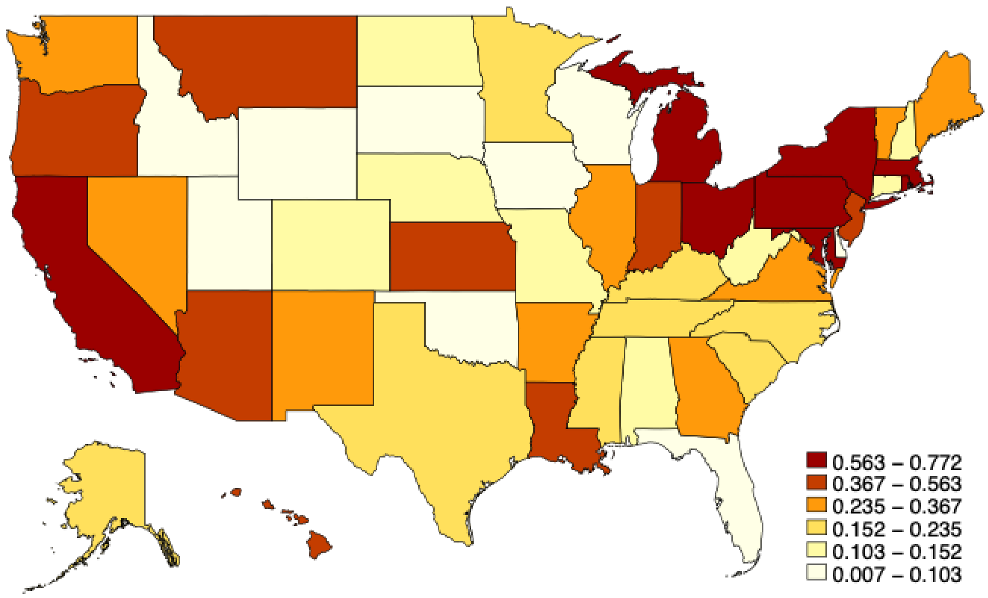
	$\ln(\tilde{w})$	$\ln(\tilde{w})$	$\ln(\tilde{w})$
2020 Q3	0.067	0.062	0.013
	(0.045)	(0.043)	(0.017)
2020 Q4	0.059	0.061	0.045
	(0.046)	(0.040)	(0.055)
2021 Q1	0.129***	0.121**	0.093**
	(0.048)	(0.048)	(0.041)
2021 Q2	0.094*	0.099*	0.107**
	(0.053)	(0.053)	(0.043)
2021 Q3	0.013	0.016	0.007
	(0.029)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

Notes: Reported coefficients are interactions between quarter indicators and the high-PUA-exposure indicator; the omitted comparison is the low-exposure group. PUA without FPUC corresponds to 2020 Q3 and Q4 since data are not available for 2020 Q2. PUA + FPUC corresponds to 2021 Q1 and Q2, during which PUA and FPUC were active. Phase Out corresponds to 2021 Q3, during which some states phased out pandemic-era unemployment programs. Robust standard errors, clustered at the state level, are shown in parentheses.

claimants and collectors in the current unemployed population. Figure 23 shows the cross-state variation in average exposure during the PUA period. Although the levels differ, its geographic pattern is similar to the measure used in the binary comparison. Table 15 re-estimates the regime-specific exposure gradients using quarterly PUA claims as a share of total UI claims. This is the quarterly analog of the measure used to form the high- and low-exposure groups in Figure 6.

Table 16 instead measures exposure using each state's quarterly initial PUA claims as a share of its unemployed population. Total claims combine the number of individuals claiming with the duration of continuing claims, and duration could itself be related to reservation wages. The initial-claims measure removes this duration component and produces similar exposure-gradient estimates. It does not, however, eliminate the broader endogeneity of PUA utilization arising from worker composition, eligibility rules, administration, implementation, and local conditions.

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



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

A.4.2 Expanded State-Level Controls

We further expand the control set in the regime-specific exposure-gradient specification of Section 3.6 to include the quarterly Oxford stringency index, the quarterly Oxford workplace-closing policy index, and a 2020 measure of the regular UI monetary threshold. We interact each control with quarter indicators, allowing its relationship with reservation wages to vary over time. These additions address broader cross-state differences in pandemic restrictions and regular UI eligibility stringency that could be correlated with PUA utilization.

Figure 24 summarizes how baseline PUA utilization varies with these controls and the original Covid-severity measures. Panels (c) and (d) show the raw monetary threshold and the average-quarter construction used in the specification below. Some of the cross-state variation in PUA utilization lines up with these policy variables, but substantial dispersion remains. Consistent with the figure, the joint state-level balance regression explains less than half of the variation in baseline PUA utilization. The remaining variation supports estimation of the exposure gradients, and the balance exercise reduces concern that those gradients are driven by these measured policy differences, although other state-level factors may still contribute.

Table 15: Alternative Exposure Measure: PUA Claims/Total UI Claims

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

Notes: PUA without FPUC corresponds to 2020 Q3 and Q4, PUA + FPUC to 2021 Q1 and Q2, and Phase Out to 2021 Q3. The row labeled "Elasticity" reports an implied elasticity calculated at $\bar{S} = 0.48$. Robust standard errors, clustered at the state level, are shown in parentheses.

Table 17 adds these controls to the exposure-gradient specification in Table 5. The first two columns reproduce columns (1) and (3) of the baseline table, while the third adds the time-interacted Oxford stringency, workplace-closing, and monetary-threshold controls. The PUA + FPUC gradient remains positive and statistically significant, and the implied elasticity is 0.115, marginally above the upper end of the range reported in the main text. The phase-out gradient remains near zero. The stability of the estimates reduces concern that the main difference in exposure gradients is driven by these selected policy measures, although other state-level factors may still contribute.

A.5 Covid and Unemployment Comparisons

As discussed in Section 3.6, interpreting the change in exposure gradients requires that, absent the FPUC supplement, the relationship between PUA utilization and other determinants of reservation wages would have remained stable across regimes. One concern is that PUA claims may reflect underlying labor-market weakness. If states with greater PUA utilization also experienced different changes in unemployment, the estimated gradients could

Table 16: Alternative Exposure Measure: PUA Initial Claims/Total Unemployment

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

Notes: PUA without FPUC corresponds to 2020 Q3 and Q4, PUA + FPUC to 2021 Q1 and Q2, and Phase Out to 2021 Q3. The row labeled "Elasticity" reports an implied elasticity calculated at $\bar{S} = 0.036$. Robust standard errors, clustered at the state level, are shown in parentheses.

partly reflect differences in job opportunities rather than UI exposure.

Covid-19 severity is another possible confounder. If greater PUA utilization coincided with greater health risks, the reservation-wage gradient could partly reflect compensating differentials for entering riskier work environments.

We therefore control for the quarterly state unemployment rate and average weekly Covid-19 deaths per 100,000 residents. The difference in exposure gradients remains similar when these variables are included. This result reduces concern about these two observed explanations, but it does not rule out other factors correlated with PUA utilization.

For a descriptive comparison, we divide states into high- and low-PUA-utilization groups using the same cutoff as in the binary analysis. Figure 25 plots quarterly PUA claims per unemployed person for the two groups. The figures retain "Treatment" and "Control" labels for consistency, but these labels refer to relative exposure; both groups were exposed to PUA.

Figures 26 and 27 plot quarterly Covid cases and deaths for the high- and low-exposure groups. The low-exposure states experienced slightly higher case and death rates. Figure 28 shows that peak unemployment was also similar across the two groups. These comparisons do

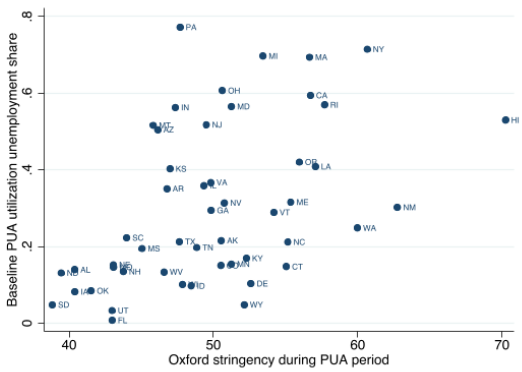
Table 17: PUA Exposure Gradients with Expanded State-Level Controls

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

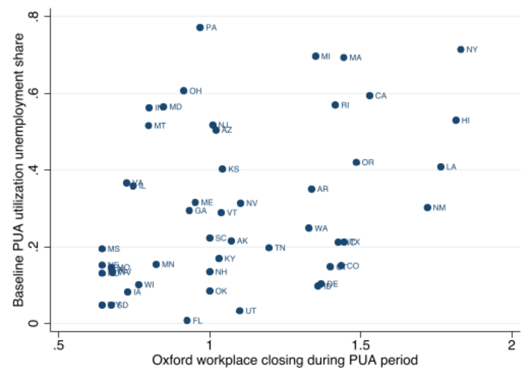
Notes: Columns (1) and (2) reproduce columns (1) and (3) of the baseline exposure-gradient specification in Table 5. Column (3) additionally includes quarter-interacted Oxford stringency, Oxford workplace-closing, and the 2020 regular UI monetary threshold, the last measured using the average-quarter construction. PUA without FPUC corresponds to 2020 Q3 and Q4, PUA + FPUC to 2021 Q1 and Q2, and Phase Out to 2021 Q3. The row labeled "Elasticity" reports the implied elasticity computed at the mean PUA intensity in the PUA + FPUC regime. Robust standard errors, clustered at the state level, are shown in parentheses.

not reproduce the ordering that would explain the positive PUA-exposure gradient through greater Covid severity or markedly weaker labor markets in high-exposure states. They reduce concern that the pattern is driven by these observed differences, although other state-level factors may still contribute.

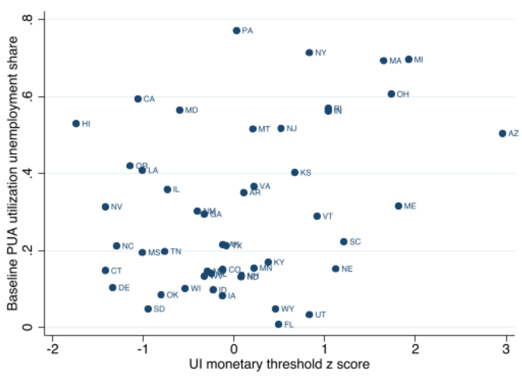
Figure 24: **PUA Intensity and Expanded State-Level Controls**



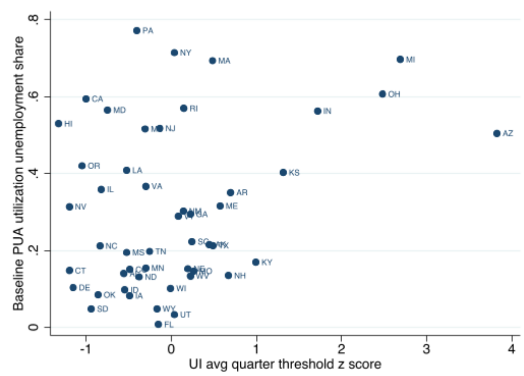
(a) Oxford stringency



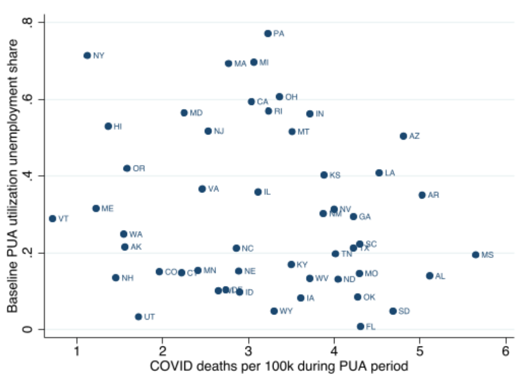
(b) Oxford workplace closing



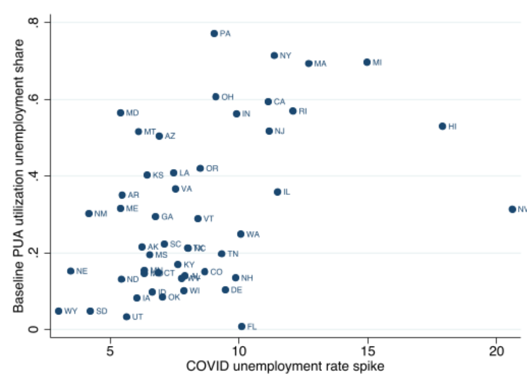
(c) Regular UI monetary threshold



(d) Average-quarter threshold



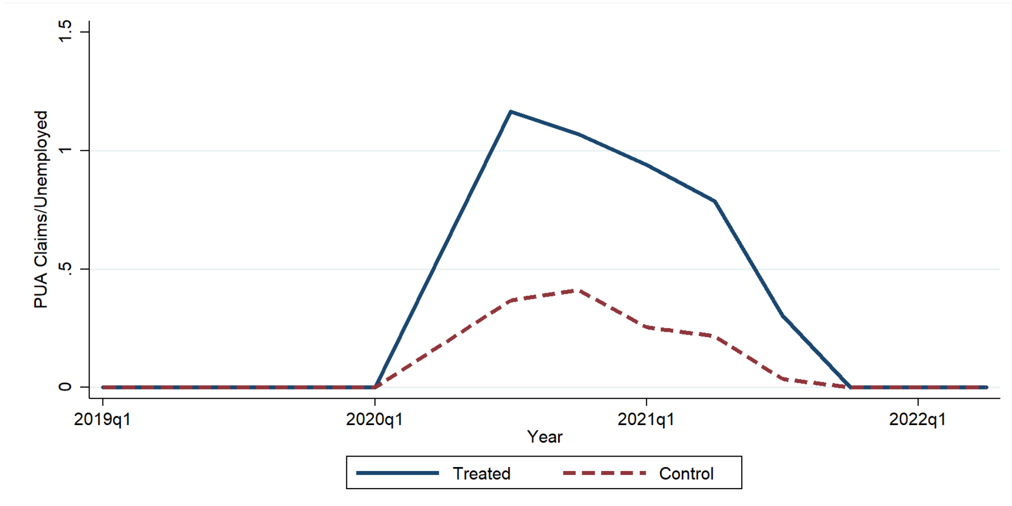
(e) Covid deaths per 100k



(f) Covid unemployment-rate spike

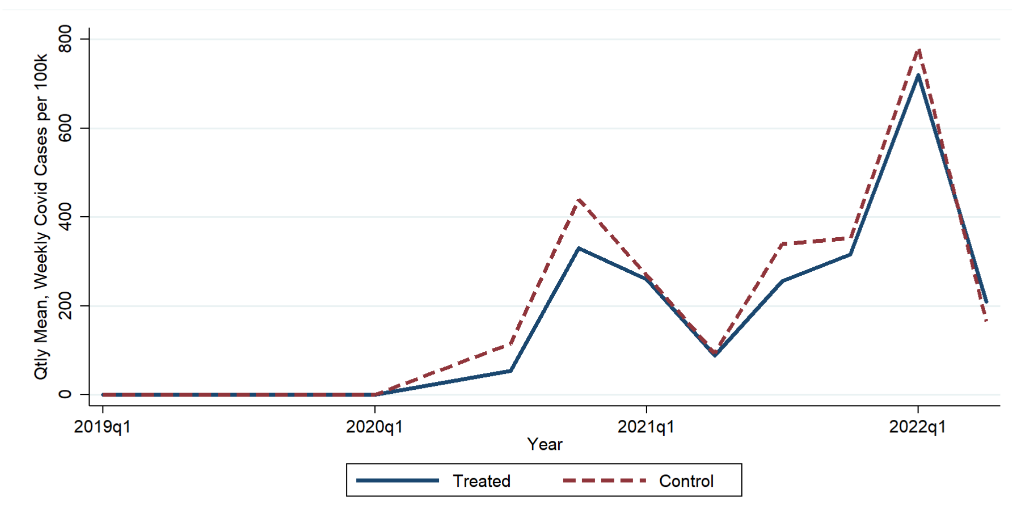
Notes: Each panel relates state-level baseline PUA utilization to the indicated control variable. The two Oxford panels summarize average policy restrictions over the main PUA period. Panels (c) and (d) show the raw and average-quarter versions of the 2020 regular UI monetary threshold. Panel (e) reports the cross-state comparison using average weekly Covid deaths per 100,000 residents over the main PUA period, while panel (f) uses the increase in the unemployment rate from 2019 Q4 to each state's pandemic peak.

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



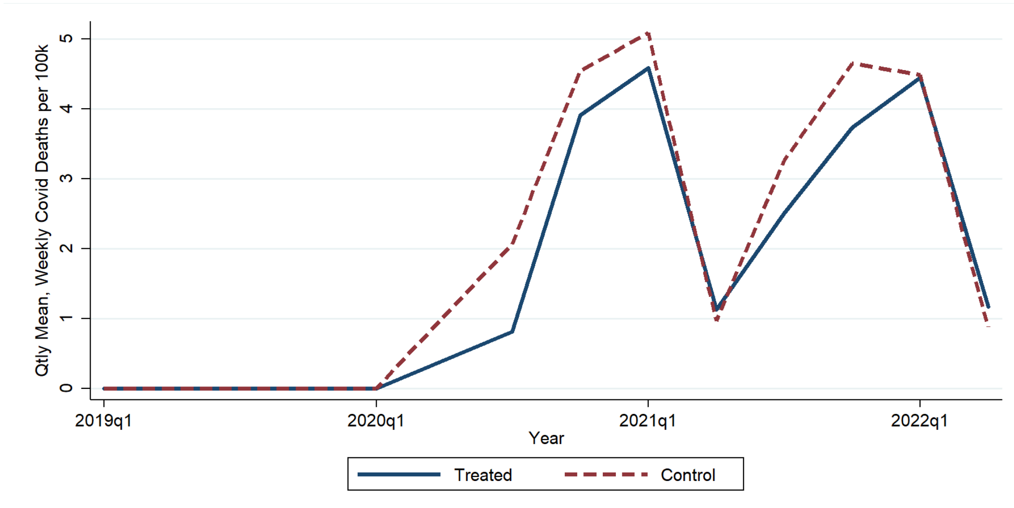
Notes: The mean value across each state group of each state's quarterly average of weekly PUA claims as a fraction of its total unemployed population (measured monthly via CPS). The figure retains "Treated" and "Control" labels as shorthand for the high- and low-PUA-exposure groups; both groups were exposed to PUA.

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



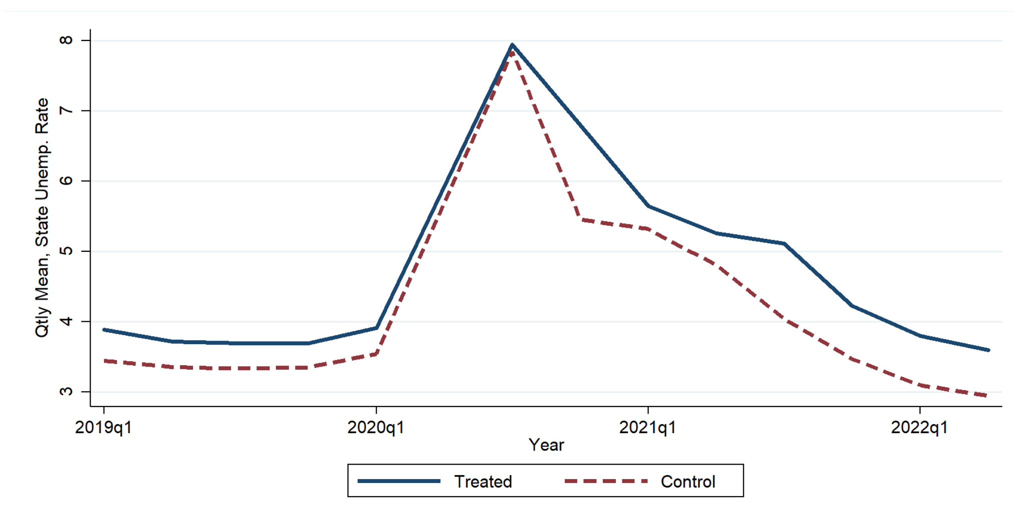
Notes: The mean value across each state group of each state's quarterly average of weekly reported Covid cases per 100,000 persons. The figure retains "Treated" and "Control" labels as shorthand for high- and low-PUA-exposure states; both groups were exposed to PUA. Covid case data were taken from CDC Aggregate Covid-19 Case & Death Count data, accessed at the following link: CDC Weekly U.S. COVID-19 Cases & Deaths by State

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



Notes: The mean value across each state group of each state's quarterly average of weekly reported Covid deaths per 100,000 persons. The figure retains "Treated" and "Control" labels as shorthand for high- and low-PUA-exposure states; both groups were exposed to PUA. Covid death data were taken from CDC Aggregate Covid-19 Case & Death Count data, accessed at the following link: CDC Weekly U.S. COVID-19 Cases & Deaths by State

Figure 28: Quarterly Mean, State Unemployment Rate



Notes: The mean value across each state group of each state's quarterly average monthly unemployment rate. The figure retains "Treated" and "Control" labels as shorthand for high- and low-PUA-exposure states; both groups were exposed to PUA. The state-level unemployment rate is reported in the Bureau of Labor Statistics Local Area Unemployment Statistics Data, accessed at the following link: https://www.bls.gov/lau/data.htm

B CPS

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

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

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

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

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

Fortunately, the Federal Covid Relief Bill (signed into law in December 2020) resurrected the FPUC program, with a \$300 per week supplement, starting the week of December 26, 2020. Accordingly, all individuals who transitioned to employment after January 2021 potentially received the \$300 supplement during the unemployment spell immediately preceding their return to work. Furthermore, through the American Rescue Plan Act (ARPA), this supplement remained in place at least until June 2021: no state ended the program prior to June 2021.[58] Therefore, all transitions to employment that we observe in 2021 and for which we can measure eligibility, as shown in Table 6, potentially occurred after having received unusually high unemployment benefit amounts.

We use a difference-in-differences regression to compare changes in earnings for individuals classified as eligible and ineligible before and during the Covid relief period. From the eligibility methodology outlined above, unemployed individuals whom we classify as ineligible for UI benefits fall into one of several categories: new entrants into the labor market; re-entrants into the labor market; individuals who quit their last job; or individuals who have exhausted benefits.[59] By construction, these workers do not qualify for regular UI under our classification. Because PUA broadened eligibility and the classification is imperfect, however, they should be viewed as a comparison group rather than as a clean untreated group during 2020 and 2021.

Accordingly, we estimate:

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

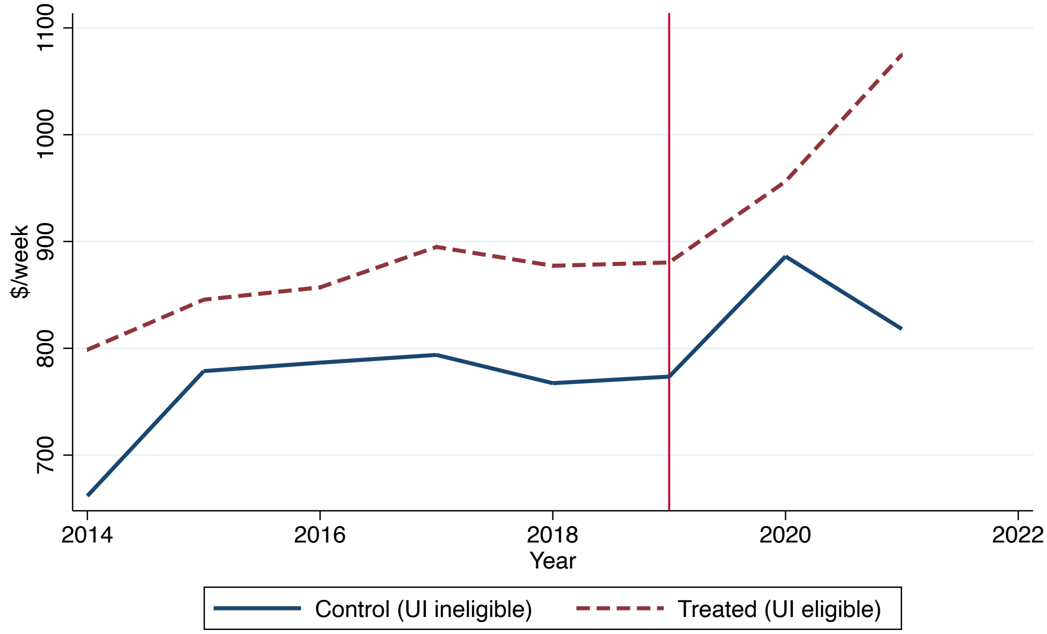
where w is either the level or the log of the weekly wage observed at the transition to employment, $\mathbb{I}(\text{FPUC}_{2020})$ and $\mathbb{I}(\text{FPUC}_{2021})$ are indicators for the Covid relief months in 2020 and 2021, respectively, during which FPUC was in place, and X includes sex, race, education, age, and indicators for industry and occupation.[60] The coefficients γ_{2020} and γ_{2021} capture

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

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

[60] We do not include unemployment duration in our baseline specification because duration may respond to UI generosity and other pandemic conditions. Conditioning on it changes the comparison and may introduce post-policy selection. In addition, durations post-Covid differ sharply from those observed pre-Covid, as many individuals were newly unemployed at the onset of the pandemic while others experienced unusually

Figure 29: Mean Weekly Wage after Transition to Employment



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

the adjusted change in the eligible–ineligible wage differential during the Covid (FPUC) relief periods in 2020 and 2021, respectively, relative to pre-Covid. A causal interpretation would require the strong assumption that, absent the pandemic-era UI expansions, wages for the two groups would have followed parallel trends conditional on X. Because that assumption is difficult to verify in the rapidly changing pandemic labor market, we primarily interpret the coefficients as changes in the wage differential.[61]

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

Table 18 reports the corresponding regression estimates. Columns (1) and (2) use the full 2014–2021 sample and allow the eligible–ineligible wage differential to change separately in

long spells. We therefore treat results that include unemployment duration, reported in Appendix B.3, as a sensitivity check.

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

Table 18: Diff-in-Diff Estimates of the Eligible Wage Premium

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

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

2020 and 2021. For 2020, the point estimates are negative but imprecisely estimated. For 2021, the differential increases by about \$130 without year fixed effects and \$120 with year fixed effects; both estimates are statistically significant.

Because the 2020 transition sample is limited and unusually composed, columns (3) and (4) re-estimate the 2021 comparison after excluding 2020. The estimated increase in the eligible–ineligible wage differential is about \$73 without year fixed effects and \$64 with year fixed effects, and both estimates are statistically significant. We interpret the log-wage estimate in column (4)—a 9.1% increase in the differential in 2021, when the supplement was \$300 per week—as our preferred CPS estimate.

Several caveats are in order. First, 2020 is both data-sparse and conceptually unusual:

many unemployment spells began before the FPUC supplement was in place, and labor market conditions changed extremely rapidly. For these reasons, we place more weight on the 2021 estimates. Second, our eligibility measure is imperfect, especially during the early months of the pandemic: some individuals we classify as ineligible may have qualified under PUA or received benefits, while some eligible individuals may not have filed (as is also the case in normal times). It is nevertheless notable that the roughly 9% increase in the eligible–ineligible wage differential we estimate in the CPS for 2021 is similar to the 7.6–12.3% range implied by our reservation-wage analysis using BAM data (Section 3.6). Because eligibility is imputed and the two exercises cover different populations, we view this similarity as an order-of-magnitude check rather than as an estimate of the same parameter.

B.2 Alternative Take-up Probit Specifications

As a test of the Probit model's predictive fit, we repeat the analysis using regular benefits rather than total benefits, which include the FPUC supplements. Figure 30 shows that worker characteristics, earnings, and regular benefit amounts alone do not reproduce the pandemic increase in take-up; including total simulated benefits improves the model's out-of-sample fit. This comparison is predictive and does not isolate the causal effect of the supplements.

We also replicate Figures 11(a) and 11(b) with unemployment duration included as a predictor. Duration may itself respond to UI generosity and changed sharply during the pandemic, so adding it changes the predictive specification rather than providing a cleaner causal comparison. Figures 31 and 32 show the same qualitative fit pattern: predictions track the observed increase more closely when total simulated benefits are included. This supports the predictive importance of program changes summarized by expected benefits, not a causal interpretation of the benefit-level coefficient.

B.3 Alternative Wage Premia DiD Specifications

Table 19 replicates Table 18 with unemployment duration included as a control. Duration may respond to UI generosity and other pandemic conditions, so conditioning on it changes the comparison and may introduce post-policy selection; we therefore treat this specification as a sensitivity check rather than as an estimate of a direct causal effect. The log-wage

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

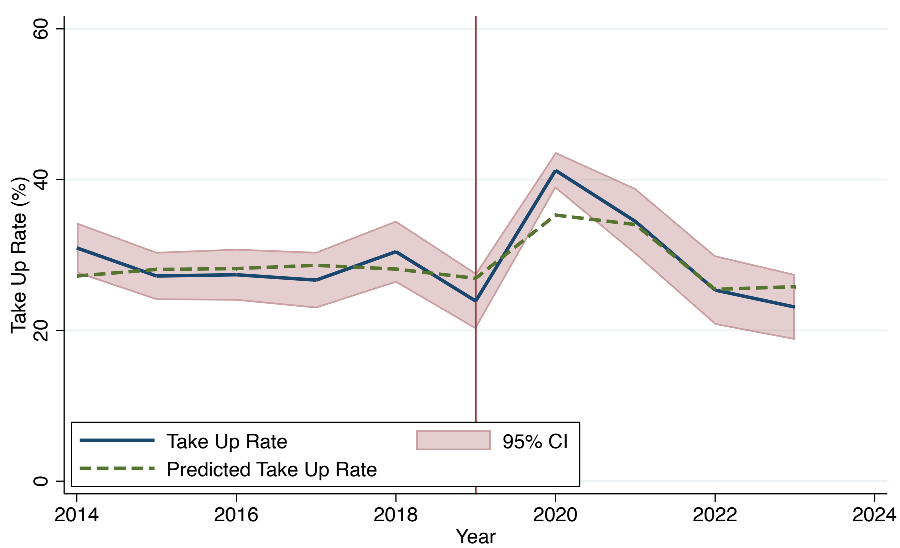
Notes: The take-up rate is the fraction of individuals who are eligible to receive regular UI benefits who report having received UI benefits the previous year. The predicted take-up rate uses the coefficient of a Probit regression to predict the take-up rate out of sample for 2020 through 2023.

estimate in column (4) is a 7.6% increase in the eligible–ineligible wage differential in 2021, slightly below the preferred 9.1% change reported in the main text. Both specifications indicate a modest change in the differential.

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

Notes: The take-up rate is the fraction of individuals who are eligible to receive regular UI benefits who report having received UI benefits the previous year. The predicted take-up rate uses the coefficient of a Probit regression to predict the take-up rate out of sample for 2020 through 2023.

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



Notes: The take-up rate is the fraction of individuals who are eligible to receive regular UI benefits who report having received UI benefits the previous year. The predicted take-up rate uses the coefficient of a Probit regression to predict the take-up rate out of sample for 2020 through 2023.

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

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

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

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

Recall equation (12)

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

where

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

Let's rewrite the second term B as

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

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

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

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

From the first-order condition of the optimal search problem

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

Therefore

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

A sufficient condition for $\frac{\partial\log x^{e}(\varepsilon)}{\partial b}$ to be increasing in ε is that $p(\varepsilon)$, viewed as a function of $U^{C}(\varepsilon)$ through the expression above, be decreasing and concave.

Now recall equation (9)

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

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

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

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

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

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

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

This is sufficient to establish that

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

is increasing in ε.

D Quantitative Model

The setup in this section closely follows the theoretical framework outlined above, but incorporates several modifications to better connect the model to the data. First, we introduce on-the-job search, along the lines of Menzio and Shi (2010) and Gervais et al. (2022). This extension allows us to compute the average wage at the time of job finding following an unemployment spell—an object we measure empirically. Second, to facilitate numerical computation, we assume that unemployed workers draw a new benefit collection cost at the beginning of each unemployment spell. This cost remains fixed for the duration of the spell, but is redrawn (i.i.d.) in the event of a subsequent unemployment spell.[62] Finally, we distinguish between UI-eligible and UI-ineligible workers and allow for unemployment benefits to expire stochastically.

Submarkets

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

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

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

Workers

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

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

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

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

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

Individuals who are eligible must decide whether they collect or not at the beginning of their unemployment spell. Let $U^E(\varepsilon) \equiv \max\left\{U^N, U^C(\varepsilon)\right\}$ denote the lifetime utility of an eligible unemployed worker with UI benefit collection cost ε. Here, U^N is the utility to the worker if he chooses not to collect and $U^C(\varepsilon)$ is the utility if he chooses to collect. Since ε is constant during any unemployment spell, a worker who decides not to collect benefits at the beginning of an unemployment spell will continue to choose not to collect during the entire unemployment spell.[63] To capture the notion that UI benefit eligibility expires, let ψ denote the probability that an unemployed worker loses benefit eligibility and thus becomes UI ineligible. Note that, for collectors this means losing UI benefits (e.g., through expiration or administrative exit), and for non-collectors this means losing the option to collect benefits in the current spell. Therefore, the value of being eligible and not to collect is:

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

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

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

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

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

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

Firms

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

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

subject to

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

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

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

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

subject to

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

Market Tightness

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

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

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

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

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

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