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Kevin Rinz and David Wasser

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Unemployment Insurance Generosity and Wage Determination*

Kevin Rinz

Federal Reserve Bank of Cleveland

David Wasser

US Census Bureau

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Abstract

Using public-use data from the Current Population Survey, we estimate the effects of changes in unemployment insurance (UI) generosity on the wages of new hires from unemployment, job changers, and continuously employed workers. We find similar, modestly positive elasticities across all groups of workers. Posted wages respond similarly on average, but differences in distributional effects suggest that changes in wage posting are unlikely to fully explain the effects on realized wages. More generous UI also reduces hiring from unemployment and job-to-job transitions, reduces labor force exit, and increases the hiring of new labor force entrants and labor force non-participants.

Keywords: unemployment insurance, wages, wage posting, labor search
JEL classification: J65, J31, J64

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1 Introduction

Unemployment insurance (UI) enhances workers' ability to trade off time spent not working against post-unemployment job quality. It provides eligible workers with financial support based on past earnings while they look for work, subject to some limitations. The goal is to improve job search outcomes by reducing pressure generated by lost earnings to take a minimally acceptable job quickly.

A great deal of research has considered the degree to which UI leads workers to spend more time searching for work, often finding that more generous UI modestly extends time spent unemployed (e.g., Rothstein, 2011; Schmieder et al., 2012; Kroft and Notowidigdo, 2016; Cohen and Ganong, 2025). Another strand of the literature considers how these individual-level effects play out macroeconomically, potentially having their effect on the overall unemployment rate amplified by changes in firms' behavior (e.g., Hagedorn et al., 2016) or muted by spillover effects on workers not eligible for UI (e.g., Marinescu, 2017) and the support the consumption enabled by UI benefits provides for aggregate demand (e.g., Ganong et al., 2024).

Less research attention has been focused, and less consensus has been reached, on the job quality side of this tradeoff. Though some recent evidence suggests that more generous UI raises wages upon reemployment (Nekoei and Weber, 2017; Dahl and Knepper, 2022), this view is far from universal (Schmieder et al., 2016; Johnston and Mas, 2018; Jager et al., 2020). Relatively little of the work on the macroeconomic or spillover effects of UI has focused on wages, even though empirical and theoretical work suggests that UI's effects may not be limited to UI beneficiaries (Lalive et al., 2015; Doniger and Toohey, 2024). Indeed, the mechanics of some foundational labor search models directly link the value of unemployment with the wages of workers other than new hires from unemployment.[1]

We fill this gap in the literature by estimating the effects of UI on workers most likely to be directly affected and spillovers to other workers within a single cohesive framework. Stated simply, we want to know whose wages are affected by changes in UI generosity and how any effects are realized. Using a simulated instrument approach (in the spirit of, for example, Currie and Gruber, 1996a,b; Cutler and Gruber, 1996), we estimate the effects of UI generosity on the wages of new hires from unemployment who had been laid off from their previous jobs alongside effects on the wages of other workers. Our approach uses all variation

[1] For example, in Mortensen and Pissarides (1994), changes in the value of unemployment affect wages by changing the reservation productivity below which jobs are destroyed; increasing the value of unemployment raises the average wage of continuously employed workers by destroying the lowest-wage jobs. In Burdett and Mortensen (1998), changes in the value of unemployment influence firms' optimal wage posting strategies, which help determine the wages of workers who receive offers while searching on the job.

in UI generosity from 2005–2024, not just large changes due to the creation or cessation of special programs created in response to recessions, and can be executed transparently using public-use data from the Current Population Survey (CPS). This strategy differs from studies that use predicted UI generosity based on an individual's own earnings history (Cullen and Gruber, 2000; East and Kuka, 2015).

For the workers most likely to have been eligible for UI themselves, we find that increased UI generosity raises wages modestly, with an elasticity of 0.13 for the full period we consider. Because the policy response to the COVID-19 pandemic involved much larger changes to UI generosity than had been typical prior to that, as well as multiple other large programs that were potentially relevant to workers' wages, we also estimate effects separately for the pre- and post-pandemic periods, finding a larger elasticity (0.21) for 2005–2019 and a smaller elasticity (0.04) for 2020–2024. The full period and pre-pandemic estimates align closely with other recent high-quality wage estimates based on arguably exogenous, politically driven reductions in UI benefits following the Great Recession (Johnston and Mas, 2018; Dahl and Knepper, 2022), providing some support for the validity of our method.

Among workers not likely to be personally eligible for UI, we find remarkably similar effects on wages. We estimate virtually identical wage elasticities for the full set of new hires from unemployment (regardless of reason for unemployment), new hires from employment, and continuously employed workers as we do for our likely UI-eligible group of new hires. The pattern of point estimates across time periods is also identical for all groups: wages are more responsive to changes in UI generosity prior to the pandemic than they are during and after it. These similarities across worker types are extremely robust to alternative specifications and to estimation within subgroups defined by personal and job characteristics.

The similarity we find in wage effects across worker types suggests that spillover effects or broader changes in the labor market due to increased UI generosity are important for wage determination. In order to contextualize these wage effects and explore potential channels through which changes in UI generosity could be transmitted to the wages of workers not eligible for benefits, we consider the effects of UI on a range of other labor market outcomes.

Beginning with other moments of the wage distribution, we find that wages are more responsive to changes in UI generosity at the bottom of the wage distribution. This pattern is apparent across worker types, and, like our estimates for mean wages, is much more pronounced prior to the pandemic. Turning to labor force transitions, we find that increased UI generosity modestly reduces hiring from unemployment and job-to-job separations. It also more substantially reduces labor force exit and increases both hiring from non-participation and transitions from non-participation to unemployment.

We also consider effects on firms' job posting behavior using job ad data from Lightcast. We find that posted wages increase when UI becomes more generous, and the elasticity is similar to our estimates for realized wages across worker types, though there are some notable differences between other elements of our realized and posted wage estimates. Unlike those for realized wages, elasticities for posted wages do not decline sharply during and after the pandemic. Distributionally, the effects on posted wages are close to zero at the bottom of the distribution and largest in the upper-middle part of the distribution. Our estimates indicate that more generous UI may decrease the job posting rate, but they are at most marginally statistically significant.

Our estimates for new entrants into the labor market, the group of unemployed workers that is probably easiest for employers to identify as not eligible for UI due to their complete lack of employment history, differ from other groups of workers in two potentially interesting ways. First, more generous UI increases the rate at which these workers are hired rather than decreasing it, as it does for other unemployed workers. Second, while average wage effects for new entrants are similar to those of other groups of workers, the pattern of estimates across the distribution more closely resembles the pattern we estimate for posted wages: smaller effects at the bottom and the largest effects in the upper middle. Wage effects for new entrants do not align closely with effects on posted wages for entry-level jobs.

Finally, we consider potential interactions between UI and other large-scale stimulus programs in the context of the pandemic. Across the groups we consider, wage effects decline substantially in 2020–2024, a period when the policy response to the COVID-19 pandemic produced both more dramatic changes in UI generosity and larger non-UI fiscal stimulus than usually seen around a recession. We find that including controls for exposure to Economic Impact Payments (EIPs) and the Paycheck Protection Program (PPP), as well as interactions between those programs and UI generosity, eliminates the decline in the main effect of UI generosity on wages during and after the pandemic, suggesting that the nature of the UI-wage relationship is not dramatically different at higher levels of generosity. However, interactions between UI generosity and EIP exposure are negative, suggesting a potential decline in the sensitivity of wages to UI in the presence of other substantial stimulus programs.

Collectively, our results shed light on how UI affects wages beyond those of workers who receive benefits. The similarity between our realized and posted wage results suggests a meaningful role for wage posting in creating wage spillovers beyond UI recipients. However, our distributional estimates and analysis of new entrants suggest that wage posting probably is not the only mechanism at play.

Realized wage effects are largest at the bottom of the distribution, where posted wage effects are close to zero. This pattern does not hold for new entrants, who are easier to identify as ineligible for UI than other unemployed workers; instead, their realized wage effects more closely track the distributional pattern we see for posted wages. Together, these facts suggest that for some workers, perhaps those whose UI eligibility is more ambiguous to employers, bargaining may play a role in wage determination on top of changes in wage posting. Our analysis of interactions between UI and other pandemic-era programs suggests that some of UI's effect on wages may arise from the role it plays as a stimulus program during recessions. This evidence could be helpful to theoretical modelers interested in UI as they consider where to simplify their models and where to preserve complexity.

The rest of this paper is organized as follows. Section 2 lays out a conceptual framework for thinking about how changes in UI generosity might influence wages. Section 3 describes sources of variation in UI generosity. Section 4 describes our data and methodology. Section 5 reports our results. Section 6 synthesizes and interprets our results and discusses how they fit within the prior empirical and theoretical literature. Section 7 concludes.

2 Labor Market Framework

We consider the relationship between unemployment insurance generosity and wages within a labor search framework. While some details vary across models, this approach, at its core, involves workers who supply labor, accepting jobs with wages that generate value that exceeds the value of unemployment, and profit-maximizing firms that produce using labor as an input, creating vacancies when they want to hire and paying wages out of the surplus produced by matches with workers.

Changes in UI generosity affect workers directly by changing the value of unemployment, to which jobs/wage offers are compared. As the value of unemployment increases, so does the wage required for a job offer to exceed that value. The exact nature of a firm's response to more generous UI benefits will vary according to details of the model, but all else equal, the need to provide higher wages that enable jobs to exceed workers' higher valuation of unemployment will make vacancies less valuable on the margin, reducing the likelihood that firms will create them (e.g. Diamond, 1982; Mortensen, 1982; Pissarides, 1990).

Beyond these direct effects, models differ in terms of how changes in UI generosity affect wages, and for which workers, based on their rules about how workers and firms engage in search. Without detailing all possible variations, some important differences across models include whether workers can search for jobs only while unemployed (e.g., Mortensen and Pissarides, 1994) or if they can engage in on-the-job search (e.g., Burdett and Mortensen, 1998); whether firms post wages or determine wages via a bargaining process with matched workers (i.e., Burdett-Mortensen vs. Diamond-Mortensen-Pissarides); whether worker search is directed based on posted wages or offers arrive randomly; and whether workers and firms can renegotiate wages after they are initially set (e.g., Postel-Vinay and Robin, 2002; Gertler and Trigari, 2009; Gertler et al., 2020). For example, if workers search only while unemployed, wages are determined via bargaining (and search is, by extension, not directed based on posted wages), and there is no wage renegotiation during employment, then an increase in UI generosity should increase wages only for new hires from unemployment, via the direct channel described above (e.g., Marimon and Zilibotti, 1999).

At the other extreme, if workers can engage in directed search, including while on the job (e.g., Menzio and Shi, 2011), wages are posted, and wages can be renegotiated during employment, increased UI generosity could spill over into the wages of workers who make job-to-job transitions (via adjustments to posted wages made in response to unemployed workers' higher reservation wages) or even continuously employed workers (whose wage could be adjusted up to account for the increased value of directed, on-the-job search). Acemoglu and Shimer (1999, 2000) show that these indirect effects can arise through a UI-labor productivity channel when workers are risk averse. In addition, employers' limited ability to distinguish between unemployed workers who are and are not UI-eligible, or to vary wage offers according to that status, may also contribute to wage effects that extend beyond UI recipients.

Given this framework, the empirical investigation below can help inform how the various modeling choices made in the labor search literature align with wage changes observed in the data and what mechanisms may give rise to those changes.

3 Variation in UI Generosity

The amount and duration of regular UI benefits available to a given worker are generally a function of his or her recent earnings history. Supplemental benefits may also be available based on local labor market conditions. In this section, we describe the state and federal UI programs in place during the period we consider and how they created variation in the availability of UI benefits.

3.1 Regular State Programs

States provide some amount of UI benefits to eligible workers regardless of prevailing economic conditions. Subject to limits on total benefits paid, weekly benefits, and benefit duration, individual workers' benefits are determined based on their recent earnings history. States commonly define benefit amounts based on total, average, or peak earnings during some base period, typically the first four of the last five completed calendar quarters. The nature of the relationship between base period earnings and UI benefits varies widely across states and over time, but all states impose some maximum weekly benefit amount. As of July 2024, those weekly maxima ranged from $235 in Mississippi to $1,079 in Washington.

Duration of benefits is generally also a function of earnings, again subject to some maximum duration. The most common duration limit is 26 weeks. Early in the period considered in the paper, 26 weeks was far and away the most common maximum duration, but it has become more common for states to set other limits since 2013. As of July 2024, maximum weeks of UI benefits available ranged from 12 in Arkansas and Florida to 30 in Massachusetts; 36 states capped benefits at exactly 26 weeks.

3.2 Extended Benefits

The Extended Benefits (EB) program is a permanent program that provides additional weeks of benefits based on local labor market conditions to workers who have exhausted their eligibility for regular state benefits. A worker's weekly benefit under the EB program is the same as the weekly benefit they received under their state's regular UI program. The cost of these benefits is generally shared equally between state and federal governments, though the federal government will often assume the full cost of the EB program during recessions.

The EB program "triggers on" in a state when the unemployment rate reaches a certain level. The default is for EB to trigger on when the insured uenmployment rate (that is, the number of workers receiving UI benefits as a percentage of workers covered by the UI system) is at least 5 percent for the previous 13 weeks and is at least 120 percent of the average rate for the same 13-week period in the last two years. When EB is triggered on, workers can receive 13 additional weeks of benefits after exhausting their eligibility under their states' regular programs. States can also choose to provide EB benefits if the insured unemployment rate is at least 6 percent, regardless of how it compares to prior years.

States also have the option to use a trigger for EB that is based on the total unemployment rate (TUR, i.e., the number of unemployed workers as a percentage of labor force participants, regardless of participation in the UI program). When the average TUR over a three-month period is at least 6.5 percent and at least 110 percent of the average TUR for the same three-month period in either of the previous two years, EB provides 13 additional weeks of benefits. States that use TUR triggers must also provide an additional seven weeks of benefits (for a total of 20) through their EB program when the TUR for a three-month period is at least 8.0 percent and at least 110 percent of the rate for the same three-month period in either of the previous two years.

3.3 Emergency Unemployment Compensation

During recessions, Congress consistently creates emergency programs to provide supplemental UI benefits beyond those provided automatically by the EB program. The program that was in effect during and after the Great Recession, from 2008 through 2013, was known as the Emergency Unemployment Compensation (EUC) program. Like EB, it provided additional weeks of benefits in a state when its unemployment rate hit certain thresholds. Those thresholds and the number of weeks of additional benefits they triggered varied over the life of the program, but at its peak, EUC provided additional weeks of benefits in as many as four tiers totaling up to 53 weeks. Between regular state programs, EB, and EUC, unemployed workers in the typical state could receive up to 99 weeks of benefits. The EUC program also paid weekly benefits in the same amount as regular state programs.

3.4 Pandemic Programs

The Coronavirus Aid, Relief, and Economic Security (CARES) Act created three emergency UI programs in response to the unfolding fallout of the pandemic. The Pandemic Emergency Unemployment Compensation (PEUC) program, like its Great Recession analog EUC, provided additional weeks of benefits to workers who had exhausted their eligibility in other programs. The Pandemic Unemployment Assistance (PUA) program provided UI benefits to workers who were not eligible under regular state programs (or EB/PEUC, which serve the same population), including notably self-employed workers and workers with earnings histories too limited to meet eligibility requirements, among others. The Federal Pandemic Unemployment Compensation (FPUC) program provided a $600 weekly supplement to benefits paid under all other UI programs.

The CARES Act initially authorized PEUC and PUA through the end of December 2020 and FPUC through the end of July 2020. FPUC was originally allowed to expire as scheduled before being reauthorized at $300 per week in an additional COVID relief package in late December 2020. That package also extended the PEUC and PUA programs. All three programs were further extended for a final time by the American Rescue Plan Act in March 2021. Though they were set to expire at the beginning of September 2021, several (predominantly Republican-led) states withdrew from some or all pandemic UI programs over the course of the summer.

4 Data and Methodology

4.1 Data

4.1.1 UI Rules

Data on state UI benefit rules are collected from the Department of Labor's Employment and Training Administration (ETA). ETA provides snapshots of significant provisions of state laws, such as formulas for calculating benefits, earnings requirements, and the tax base for state UI taxes, as of January and July of each year.[2] We use these rules to calculate benefits available under regular state programs.[3]

ETA also provides "trigger notices" for the EB program, which identify when that program's benefits are available in each state, as well as information about when the various tiers of additional benefits under the EUC program were available in each state between 2008 and 2013. Each of these programs provides additional weeks of coverage without altering benefit amounts, and states' statuses are updated on a weekly basis.[4] To align with the monthly labor market data discussed below, we adopt the implementation of Chodorow-Reich et al. (2019).

Unlike EB and EUC, PEUC and FPUC were not tied to local conditions, applying equally to all workers regardless of location. We apply these rules based on contemporaneous descriptions of the CARES Act, which originally created these programs, and subsequent legislation that extended, reauthorized, or modified

[2]These snapshots, along with other regularly updated reports on state UI rules, can be found here.

[3]We omit Montana and West Virginia from our analysis due to difficulty with reliably calculating benefits over time for these states. For Montana, we were unable to find historical information on the thresholds that determine the claimant's potential benefit duration. For West Virginia, the weekly benefit amount is a function of very narrow bands of base period earnings (each band is approximately $150 wide), which is likely to introduce meaningful measurement error into our approach for assigning benefits.

[4]For part of the time that the EUC program was in place, UI beneficiaries received a $25 supplement to their weekly benefits due to a provision of the American Recovery and Reinvestment Act of 2009. While this supplement applied to benefits paid through the EUC program, it was not itself a feature of that program.

them. For states that withdrew from one or more pandemic UI programs prior to their expiration, we use end-dates reported by Whittaker and Isaacs (2021).

4.1.2 Demographics and Earnings

Our analysis relies on data from the CPS to measure the wages of new hires and to estimate the total dollars of UI benefits available to eligible workers. The CPS has a 4-8-4 panel structure that involves respondents spending four consecutive months in-sample, rotating out of sample for eight months, and then rejoining the sample for four more months (the same four calendar months as the first period in-sample). For each respondent, the CPS collects information on a range of demographic characteristics such as age, sex, race/ethnicity, and educational attainment, as well as monthly information on labor force status, including details of respondents' work (e.g., industry, occupation) or non-work (whether they want a job, duration of unemployment), as applicable. Importantly, the CPS tells us why unemployed workers are unemployed, allowing us to focus on the workers most likely to be eligible for UI benefits themselves (those who have been laid off or had temporary jobs end). Also, linking CPS respondents longitudinally across months of the survey allows us to see whether newly hired workers were previously employed, unemployed, or out of the labor force. Both of these are important advantages of the CPS relative to other data that cannot associate wages with prior labor force status.

The CPS collects information about wages, earnings, and hours in the fourth and eighth months in-sample, the so-called "outgoing rotation groups" (or ORG) that will not be surveyed the following month. For measuring both the wages of new hires and earnings as a determinant of benefits, we focus on usual weekly earnings, adjusted for inflation to 2024 dollars using the CPI-U.

We consider two kinds of new hires, both of which must be employed in an ORG month in order to have their earnings recorded: new hires from unemployment, who were unemployed due to having been laid off and moved from unemployment to employment in the ORG month or one of the two prior months, and new hires from employment, who were continuously employed but report changing employers in the ORG month or at least one of the two prior months.

4.1.3 Job Openings and Posted Wages

In order to analyze how firms respond to changes in UI generosity, we use data from job postings. These data are drawn from the near-universe of online job postings provided by Lightcast. Lightcast data provide the text of online job ads collected from numerous major online job boards since 2010. Lightcast also processes the text of the job ads to provide measures of, among other things, industry, occupation, job title, location, education and experience requirements, and wages, as available. Lightcast data have been used extensively in prior research (e.g., Azar et al., 2020; Acemoglu et al., 2022; Schubert et al., 2025, among others) and align reasonably well in aggregate with measures from the Job Openings and Labor Turnover Survey (JOLTS).[5] Despite some evidence of non-random selection into wage posting (Batra et al., 2023), aggregate wage growth measures based on posted wages align well with measures of realized wage growth from other sources in recent years, and conditioning on employer and job title in regression analysis, as discussed below, should help alleviate many other concerns.

4.1.4 UI Receipt and Household Finances

The Survey of Income and Program Participation (SIPP) provides monthly measures of income received from a variety of sources, including unemployment insurance programs. It also provides annual measures of various types of assets and debts. The SIPP also includes sufficient information on personal and job characteristics to merge in our baseline measure of UI generosity. We use measures of net worth and checking and savings account balances to investigate whether and how changes in UI generosity might affect household finances. We also use the SIPP's monthly employment histories to consider potential relationships between household balance sheets and employment.

4.2 Methodology

In order to analyze the effects of UI generosity, we construct a measure of UI generosity in the spirit of a "simulated instrument": we apply the state (and sometimes federal) rules governing benefit amount and duration that are in place each period to a national sample of workers to estimate the amount of UI available to a typical worker with given characteristics at a given time. This approach overcomes two challenges, the first related to data availability and the second related to identification: we do not observe actual UI receipt in the data we use (and datasets that do contain information on UI receipt are meaningfully limited on other important dimensions), and actual UI generosity/receipt in a place could both cause and be caused by local conditions relevant to wages (in the case of federal recession-response UI expansions, UI availability generally depends explicitly on state unemployment rates).

[5] See Dalton et al. (2021) for discussion.

4.2.1 Calculating UI Generosity

Our measure of UI generosity reflects total dollars of UI benefits available under state rules as applied to consistently employed workers with earnings that are typical given their personal and job characteristics. It reflects both benefit duration and weekly benefit amount. Since individual workers' actual UI receipt is not recorded in our data and difficult for potential employers to observe, we do not attempt to adjust generosity at the individual level based on time spent unemployed. Instead, the generosity measure we assign to each worker can be thought of as reflecting the amount of UI benefits that could potentially be claimed by the average observationally similar worker under the rules in place in their state at a given time.

The relatively short and irregular panel structure of the CPS leaves us without access to the detailed earnings histories that would be required to apply state UI rules to individual respondents exactly as they would be applied by state UI agencies to determine eligibility for UI and benefit amounts. However, even if we could do that, we would not necessarily want to regress a worker's wage at a new job on their own actual UI benefit availability because those two things could be endogenously determined. Workers with high unobserved ability, for example, would tend to have higher UI benefits by virtue of higher past earnings while also being likely to earn higher wages upon reemployment. Instead, we would want to focus on a measure that captures differences in UI generosity that are driven by differences in policy parameters to the greatest extent possible.

Our UI generosity measure is constructed in the spirit of simulated instruments used in a range of literatures (e.g., Currie and Gruber, 1996a,b; Cutler and Gruber, 1996) by applying state UI rules to earnings information for a national sample of workers. Because we lack detailed earnings history information, we simplify application of state UI rules by calculating average weekly earnings within detailed demographic cells, using data from the year prior to the year for which benefits are being calculated, and assume that level of earnings for every week of the base period. This functionally abstracts from extensive margin eligibility differences across states.[6] Our approach is similar to but distinct from other studies that use predicted UI generosity based on an individual's own earnings history (Cullen and Gruber, 2000; East and Kuka, 2015). Applying state rules gives us weekly benefit amounts and number of weeks of benefits available. Our measure of UI generosity is the product of these two values, the total dollars of UI benefits available to someone taking up UI at a given time, adjusted for inflation in the same way as earnings. Rather than assign UI

[6] For analysis of the extensive margin, see, for example, De Souza and Doherty Luduvice (2023); Birinci and See (2024); Chao et al. (2025); Chao (2025); Moore and McQuillan (2025).

generosity based on when individual workers became unemployed, we use the rules in place at the time we observe workers being hired in the CPS.

To capture within-state variation in UI benefit generosity, we estimate earnings and apply UI rules separately within detailed demographic groups constructed by interacting several key personal and employment-related characteristics. The characteristics and categories we use are age (under 25, 25–54, 55+), race/ethnicity (non-Hispanic White, non-Hispanic Black, non-Hispanic other, Hispanic), education (no college degree, college degree), broad industry (goods-producing, services, government), and broad occupation (blue collar, white collar).[7]

Figure 1 shows the density of our UI generosity measure in select years: 2005, when the economy was expanding and no supplemental UI programs were in effect; 2010, the year containing the labor market trough following the Great Recession, when EB and EUC benefits were widely available; and 2020, when pandemic programs made additional weeks of benefits available and dramatically increased weekly benefit amounts for much of the year. Values vary widely within each of these years, though they vary much more widely around higher means in 2010 and especially 2020, when federal programs were supplementing regular benefits.

4.2.2 Baseline Estimation

Our baseline specification regresses the log of real wages of new hires from unemployment on the log of real total UI benefits, controlling for various fixed effects:

$$ln(y_{it}) = \beta ln(UI(i,t)) + \gamma X_{it} + \varepsilon_{it} \tag{1}$$

Here, y_{it} is usual weekly real earnings. $UI(i,t)$ is a function of individual i's personal and employment-related characteristics and location at time t that gives the total available dollars of UI benefits. X_{it} includes state-year fixed effects and fixed effects for the categories of characteristics listed in Section 4.2.1. The coefficient β gives the elasticity of new hire wages with respect to UI generosity. Standard errors are clustered at the state level.

In Equation (1), treatment varies across the detailed demographic groups described in Section 4.2.1 but within state and time (i.e., half years), conditional on worker characteristics. In other words, β is identified

[7] Among associate's degrees, academic degrees are grouped with "bachelor's degrees and higher" in the college degree category, while technical and vocational degrees are included in the no college degree category.

using variation in the effective generosity across groups of workers exposed to the same state-level policy at the same time.[8] Such identification requires two assumptions. First, changes in UI generosity are not correlated with underlying trends in labor market outcomes across demographic groups within the same state. Second, there are no other state-level policies that are correlated with changes in UI generosity that also affect labor market outcomes differentially across our demographic groups.

To consider the effects of UI generosity on the distribution of wages for new hires, we re-estimate Equation 1 via recentered influence function regressions. In this case, the dependent variable is the recentered influence function of the log of usual weekly real earnings, constructed for various key percentiles p of the distribution. Here, β gives the unconditional quantile partial effect of UI generosity across the wage distribution (Firpo et al., 2009), interpreted as the elasticity of unconditional wage percentile p with respect to UI generosity, conditional on the fixed effects in X_{it}.

In some analyses, we use outcome data that contain less granular demographic or geographic information than the CPS. For example, job openings do not differ across race/ethnicity or gender. In these cases, we construct UI generosity by beginning with our baseline measure and then aggregating: we estimate the average generosity within groups defined by the subset of characteristics that are available in the data at hand and use the aggregated generosity measure to estimate a version of Equation 1. Variations on the baseline specification will be discussed in more detail in conjunction with the relevant estimates.

5 Results

5.1 Wages

Our analysis of wages begins with the wages of new hires from unemployment, a group for which we estimate a modest positive relationship that aligns closely with prior work. We go beyond prior work by considering the effects on the wages of workers who were unlikely to receive UI themselves, new hires from employment and continuously employed workers, finding virtually identical effects on the wages of workers in all three groups. Estimates for all groups are reported in Table 1. All specifications include state by year fixed effects, as well as fixed effects for age, race/ethnicity, educational attainment, industry, and occupation. Categories align with those used to define the cells within which we estimate UI generosity, discussed in

[8] Borusyak and Hull (2025) discuss the power advantages of simulated instruments that make use of (some) within-state variation.

Section 4.

5.1.1 New Hires from Unemployment

New hires from unemployment are most likely to have their personal circumstances influenced by changes in UI generosity. The first panel of Table 1 reports estimates for workers newly hired from unemployment after having separated from their prior job in a way that would likely make them eligible for unemployment insurance, such as being laid off or completing a temporary job.[9] In column 1, which includes data for 2005–2024, the elasticity of wages with respect to UI generosity is about 0.13. In column 2, limiting the data to 2005-2019, a period of more conventional changes in UI generosity, increases the elasticity to about 0.21. Strikingly, the elasticity during and after the pandemic, when UI generosity reached and then receded from unprecedented levels, is much smaller than the pre-pandemic or full-period estimates, at just over 0.04 (column 3).[10] These estimates are, if anything, slightly smaller than estimates for new hires from unemployment regardless of the reason for unemployment, reported in the second panel of Table 1.

Despite substantial differences between the estimation strategies used in the two papers, these estimates are remarkably close to those presented by Dahl and Knepper (2022) based on the sudden cuts to UI benefits in North Carolina in 2013. In that case, cuts amounted to a roughly 50 percent reduction in UI generosity and caused starting salaries to fall by 7.2 percent, corresponding to an elasticity of about 0.14.[11] In Appendix Table B1, we report a version of our main wage results based on a specification that includes non-interacted state and year fixed effects (rather than the interacted state-year fixed effects in our baseline specification) to more closely match the Dahl and Knepper (2022) specification (which uses state-year variation and therefore cannot include state-year fixed effects). In that specification, we estimate an elasticity of about 0.14 for all new hires from unemployment for 2005–2019. Limiting our sample to use only the states included in Dahl and Knepper's analyses also produces estimates that closely align with theirs (see Appendix Table B2).

In other words, our methodology matches a recent high-quality estimate from the literature on UI generosity and wages when deployed in a way that uses similar variation. Our methodology also gives us greater

[9]Workers who are unemployed for these reasons must still meet other requirements, such as having sufficient earnings and employment history, in order to be eligible for unemployment insurance.

[10]We return to this in Section 5.6 where we show that including controls for other major pandemic-era stimulus programs and their interactions with UI generosity effectively eliminates the decline in the main effect of UI generosity on wages associated with including the pandemic period in this analysis.

[11]Dahl and Knepper (2022) find that states that undertook more moderate UI reforms saw smaller reductions in starting salaries (about 1.8 percent) alongside declines in UI generosity of about 23 percent, corresponding with a somewhat smaller elasticity of about 0.08.

flexibility to control for time-varying local factors that may affect wages and/or UI generosity (such as state business cycles). When we do, we find that the wages of new hires are somewhat more responsive to UI generosity than prior work suggests. With this baseline established, we extend our analysis to workers who are unlikely to have been eligible for UI and for whom any wage effects of UI generosity would be realized through its influence on the labor market as a whole rather than through its influence on the personal circumstances of the newly hired workers.

5.1.2 New Hires from Employment

The longitudinal structure of the CPS allows us to differentiate between new hires who were previously unemployed (and potentially eligible for UI) and those who were already employed in a different job.[12] We now turn to the effects of UI generosity on the wages of new hires from employment, reported in the third panel of Table 1. These estimates are virtually identical to those in the first panel of the table for likely UI-eligible workers newly hired from unemployment.

Importantly, the similarity between our estimates for new hires from employment and unemployment extends to the pattern of estimates across periods. The elasticity for 2005–2024 is smaller than the elasticity for 2005–2019 because the pandemic-era elasticity is much closer to zero.

5.1.3 Continuously Employed Workers

The evolution of labor market and policy conditions over the course of an employment relationship can affect wages even for workers who do not change jobs (Beaudry and DiNardo, 1991). Is UI generosity among the factors that can influence the wages of job-stayers?

The fourth panel of Table 1 reports wage effects for workers we observe employed in four consecutive months (the last of which is an ORG month with wage information recorded) but not changing employers. Since these workers have not been hired recently, we use UI generosity at the time of wage measurement for this analysis. The effects are extremely similar to the effects for both categories of new hires considered above and again point to wages being less responsive to UI generosity during and after the pandemic.

[12] This is an important advantage of these data relative to the Glassdoor salary data used by Dahl and Knepper (2022), which contain no information about the prior employment or labor force status of the people starting the jobs they describe.

5.1.4 Robustness of Elasticity Similarity Across Worker Types

The similarity between the wage elasticities we estimate for new hires from unemployment, new hires from employment, and continuously employed workers is extremely robust to alternative formulations of this analysis. Table 2 compares our baseline estimates for 2005-2024 to estimates using various alternative sets of fixed effects. These fixed effects implement flexible state-by-characteristic trends, one characteristic at a time (columns 2-6); fully interacted characteristic fixed effects (column 7); flexible characteristic trends for all characteristics simultaneously (column 8); and flexible trends within cells defined by fully interacting personal/job characteristics (column 9). Estimates vary somewhat in levels across specifications but are consistently similar across groups of workers within specification. Estimates are also consistently similar across worker types within groups defined by single personal/job characteristics. Appendix Figure B1 shows binned scatter plots of wages and UI generosity, residualized on our baseline fixed effects, across worker types within the categories we use for each personal and job characteristic that figures into our UI generosity calculation. Though slopes do vary across categories of some characteristics (e.g., the slope for prime-age workers is flatter than the slope for older or younger workers), those differences are generally consistent across worker types.

5.1.5 Questions Raised

Our results show that more generous UI increases the wages of the newly hired workers most likely to have been eligible to receive benefits. Our results also suggest that 1) the effects of UI generosity on wages extend to unemployed workers who were unlikely to be eligible to receive benefits, as well as workers changing jobs without becoming unemployed and workers who remain employed without changing jobs, and 2) the effects on wages are of essentially the same magnitude across these groups of workers. This is a striking result and suggests that spillover effects or broader changes in the labor market due to increased UI generosity are important for wage determination, including for workers who are not receiving UI benefits themselves.

How are changes in UI generosity transmitted into wages for workers who are not UI recipients? We now turn to an empirical consideration of various facets of this question. We examine additional outcomes, subgroups, and economic actors to provide additional views of how UI generosity may influence wage setting.

5.2 Distribution of Wages

Could UI generosity have effects on the distribution of wages that differ across types of workers in a way that might clarify the underlying mechanisms at work? Figure 2 plots estimated effects at key points in the wage distribution by type of workers for various periods covering 2005–2024. Estimates are produced using recentered influence function regressions with the same structure as Equation 1 and represent the elasticity of the indicated unconditional wage percentile with respect to UI generosity, controlling for the influence of observable characteristics.

The effects of UI generosity clearly differ across the wage distribution, but they are only slightly different across types of workers. Wages at the bottom of the distribution are more responsive to changes in UI generosity than are wages at the top of the distribution for all worker types. As with the mean regressions reported above, the effects of UI generosity on wages are substantially muted across the distribution since 2020.

Among new hires from unemployment, this pattern of estimates seems consistent with the income support of UI playing a role in wage determination. Typical UI benefits replace some share of base period earnings, subject to weekly maximums that make UI benefits a smaller share of wages as wages increase. The presence of the same pattern for new hires from employment and continuously employed workers, however, suggests that this effect may spill over to other job seekers who cannot receive UI. Interestingly, wages are, if anything, slightly more responsive at the bottom of the distribution and less responsive at the top for new hires from employment and continuously employed workers than they are for new hires from unemployment.

5.3 Labor Supply

Since much of the literature on UI generosity finds that increased generosity extends spells of unemployment (Rothstein, 2011; Farber and Valletta, 2015; Farber et al., 2015; Kroft and Notowidigdo, 2016; Schmieder and von Wachter, 2016; Johnston and Mas, 2018), we next consider how labor supply might react to changes in UI generosity.

5.3.1 Hires

The top panel of Table 3 suggests that the hire rate among workers who are unemployed following a layoff is generally not responsive to changes in UI generosity; over the full period considered, the point estimate has a negative sign but is essentially zero. Since 2020, the sign of the effect on hiring for this group turns positive, but the magnitude remains close to zero. The bottom panel examines hiring of all unemployed workers, regardless of reason for unemployment, and shows slightly more negative and statistically significant estimates for the full and pre-pandemic periods, with implied elasticities of -0.03 and -0.05, respectively. The effect since 2020 is again negative, but it is smaller and not statistically significant. Overall, our estimates suggest that increased UI generosity has, if anything, a slightly negative effect on hiring from unemployment.

5.3.2 Separations and Other Labor Force Transitions

Table 4 shows that increased UI generosity slightly reduces separations over the full period and in the pre-pandemic period (first panel). The effect is small for the full period (the implied elasticity is -0.066) and somewhat larger for the pre-pandemic period (elasticity of -0.19).

Considering different types of separations, the second panel of Table 4 shows decreases in separations to other employment that are consistently small and statistically significant. The 0.001 full-sample semi-elasticity represents only about 6 percent of the baseline rate of employer-to-employer transitions. Estimates for separations to unemployment, reported in the third panel of Table 4, suggest that more generous UI makes workers slightly more likely to move from employment to unemployment over the full period, but this effect is driven by the post-pandemic period; for 2005-2019, the point estimate is negative but not statistically significant. Speculatively, some pandemic-specific factors could explain this pattern of estimates. Early in the pandemic period, UI benefits were available to workers who quit for COVID-19-related reasons, which could show up as increased transitions to unemployment associated with increased UI generosity. Employers also could have increased layoffs when UI benefits became more generous and hoped to rehire workers when conditions improved rather than trying to keep them on payroll. These possibilities merit further investigation, though they are beyond the scope of this paper, and the limited sample size in the CPS during the relatively narrow window in which these mechanisms are relevant makes this difficult in our setting.

Increased UI generosity also has important effects on the participation margin. The fourth panel of Table

4 shows decreased transitions from employment to non-participation during all periods considered, with an implied elasticity for the full period of -0.33. Table 5 shows for 2005-2019 that increased UI generosity also decreases labor force exits from unemployment (with an implied elasticity of -0.42), and increases labor force entrances to both employment (0.22) and unemployment (0.41). This is all broadly consistent with Rothstein (2011). The magnitudes of these effects are large compared to effects on transitions between employment and unemployment. Reduced labor force exit and increased (re-)entry put additional upward pressure on the unemployment rate on top of that created by reduced hiring from unemployment, though workers induced to remain unemployed likely have improved job finding prospects compared to what they would have faced had they exited the labor force (Kudlyak and Lange, 2018).

As an additional check on our wage and separations estimates, we can use our estimated wage elasticity for continuously employed workers and the elasticity implied by our separations estimates to construct an elasticity of labor supply to the firm, in the style of Manning (2003). For all separations in 2005-2019, our estimates imply an elasticity of separations with respect to UI generosity (ε_s) of about -0.19. The elasticity of continuously employed workers' wages with respect to UI (ε_w) for this period is about 0.24. Together, these imply an elasticity of labor supply to the firm, given by $-2\frac{\varepsilon_s}{\varepsilon_w}$, of a little more than 1.5. This is well within the range of recent estimates. It aligns most closely with estimates from Webber (2022) using linked employer-employee data, while it is somewhat smaller than estimates from Bassier et al. (2021), who find elasticities closer to 4 using variation in firm wage policies for estimation.

5.4 Labor Demand

Labor search models predict that increasing the value of unemployment will increase posted wages in order to increase the value of jobs relative to unemployment. As a consequence of this shift, the surplus value to the firm of job matches shrinks, reducing the job posting rate. Our estimates are consistent with these predictions on average, though the effects of UI on posted wages vary over the distribution in potentially informative ways.

5.4.1 Job Postings

First, we consider the effect of changes in UI generosity on the job posting rate. We construct the job posting rate from Lightcast data, which include information on location, industry, occupation, and education requirements.[13] Within each state and month, we construct job posting rates within cells defined by the intersection of sector, blue collar/white collar status, and educational attainment, segmenting job postings according to whether they require a college degree or not. We also aggregate our UI generosity measure to vary on only those dimensions. We use a few different definitions of the job posting rate, including the definition used by JOLTS, and we construct the rate using employment and population estimates from the American Community Survey (ACS). To provide a specific example, the job posting rate JPR_{ioest} in state s at time t within a cell defined by the intersection of sector i, blue/white collar status o, and educational status e is

$$JPR_{ioest} = \frac{Postings_{ioest}}{Emp_{ioest} + Postings_{ioest}}. \tag{2}$$

We also consider two alternative formulations of the job posting rate, one that excludes postings from the denominator and another that replaces the denominator used above with the full population of people associated with a given cell, regardless of employment status.[14]

We then regress the job posting rate on our aggregated UI generosity measure and state-half year, college, sector, and blue collar fixed effects:

$$JPR_{ioest} = \beta_0 + \beta_1 ln\left(\overline{UI}_{ioest}\right) + \delta_i + \gamma_o + \zeta_e + \tau_{st} + \varepsilon_{ioest} \tag{3}$$

where $\overline{UI}_{ioest}$ is the average value of the UI generosity measure used above in the individual-level analysis across all people in a given sector-blue/white collar-education-state-month cell.

As estimates in Table 6 show, increased UI generosity tends to reduce the job posting rate. Though estimates are at best marginally statistically significant, their magnitude is non-negligible. A doubling of UI generosity (a magnitude that is well within the realm of recession-response experience, even before the pandemic) reduces the job posting rate by nearly 1.0 percentage point using the JOLTS definition. Given the 5.1 percent average job posting rate in the analysis sample, this translates into an elasticity of about -0.20. This is somewhat smaller than the elasticity of -0.28 we get by using the log of postings within these same

[13] Postings that are lacking any of this information and cannot be assigned to a cell are excluded from this analysis. State-level estimates of the job opening rate are available from JOLTS, but they do not contain any industry, occupation, or education detail. As such, using these data would require extensive aggregation of our UI generosity measure and would not permit the use of state-time fixed effects.

[14] People not in the labor force are asked about the industry and occupation of their last job if they have worked within the last five years.

cells as the dependent variable, though that estimate is less precise. Alternative definitions of the posting rate give slightly higher elasticities than the JOLTS formulation. These estimates suggest that increases in UI generosity may translate into non-negligible reductions in labor demand.

5.4.2 Posted Wages

We also consider the effects of UI generosity on posted wages. First, we construct aggregate measures of mean and median posted wages within the cells used in our job posting rate analysis and estimate effects on posted wages using Equation 3, with the log of the mean or median posted wage as the dependent variable.[15]

Estimates in Table 7 indicate that posted wages rise when UI becomes more generous. In the aggregate estimates in the top panel, elasticities from unweighted estimates for mean and median posted wages are similar to each other (0.15 and 0.17, respectively) and also similar to the elasticities for realized wages from our CPS analysis in Section 5.1. Estimates that weight cells using the population of associated people yield elasticities that are somewhat smaller but still positive and statistically significant.

The share of job postings that contain wage information has grown dramatically over time. At the beginning of the period covered by our data, about 5-10 percent of postings included wage information; by the end of the period we consider, more than 50 percent of postings included wage information. This suggests substantial scope for selection into wage posting (even at the end of the period), as well as substantial scope for the nature of that selection to change over time.[16] If the types of firms that post wages or the types of jobs for which they post wages respond to changes in UI generosity, wage effects that do not account for potential changes in the composition of the sample could be deceiving.

To address potential concerns about selection into wage posting, we also estimate the effects of UI generosity on posted wages using microdata and a specification that includes employer-job title fixed effects:

$$ln\left(wage_{pst}\right) = \beta_0 + \beta_1 ln\left(\overline{UI(p)}_{st}\right) + \tau_{st} + \xi_{e(p)j,(p)} + \varepsilon_{pst} \tag{4}$$

The UI generosity measure used here is the same as the measure used in the aggregate analysis of posted wages, but it is assigned to individual postings p based on the information they contain about their sector, blue/white collar status, and education requirements (rather than having information from postings aggregated to the level of the UI generosity measure). The employer-job title fixed effects ξ are also assigned

[15] Where only a range of wages is provided, we use the midpoint.

[16] Indeed, Batra et al. (2023) find that wage information missing from Lightcast job postings data is far from missing at random.

based on information contained within each posting, and postings that are not identified with a specific employer or job title are excluded.[17] For this microdata analysis, we switch to more temporally granular state-month fixed effects τ_{st}.[18]

Our microdata estimates, reported in the bottom panel of Table 7, show very similar, statistically significant elasticities of posted wages with respect to UI generosity. Over the full period covered by the Lightcast data (2010–2024), the elasticity is 0.19. Interestingly, there is less drop-off in the responsiveness of posted wages during and after the pandemic than we saw with realized wages: the point estimate declines, but only to 0.13. We can use these wage estimates in combination with our job posting estimates above to construct an own-wage job posting elasticity, which for 2010-2024 is -1.06, suggesting that job postings are roughly negatively unit elastic with respect to wages.

The effects on posted wages also differ from the effects on realized wages in how they vary across the distribution. The effects on posted wages, reported in Table 8 and compared to the effects on realized wages estimated for the same period in Figure 3, peak in the upper-middle part of the distribution rather than at the bottom and remain positive at the top of the distribution.

5.5 New Entrants and Entry-Level Jobs

New entrants are the category of unemployed workers most easily identified as ineligible for unemployment insurance. UI eligibility typically requires meaningful employment over the course of several quarters, experience that will not appear on the resume or job application of someone just entering the labor market. We assess the effects of UI generosity on their wages, as well as on the posted wages of entry-level positions they are likely to consider.

Table 9 shows in its top panel that new entrant wages are similarly responsive to changes in UI generosity as are the wages of other groups of workers. In contrast to workers unemployed due to layoff, increased UI generosity increases the rate at which new entrants are hired, as reported in the middle panel of Table 9. Doubling UI generosity increased the new entrant hire rate by 1.9 percentage points over the full period

[17] These postings are also excluded from the aggregate analysis, since sector and blue/white collar status are assigned based on employer and job title.

[18] In the aggregate analysis, we use state-half year fixed effects both to align with the frequency of updates to our UI generosity measure and to limit the number of parameters being estimated in a context with fewer observations. Here, the number of observations corresponds with the total number of job postings with wage information, so that second consideration is less important. Moreover, Dahl and Knepper (2022) use non-interacted state and quarter fixed effects in their analysis of posted wages. As discussed above, we prefer interacted state-time fixed effects, and the choice of month or quarter for the interacted state-time fixed effects makes no difference to our estimates.

considered and by 2.2 percentage points for 2005–2019, implying elasticities of about 0.11 and 0.13 for those periods, respectively.

Posted wages for entry-level positions, defined using various combinations of experience and education requirements, are much less responsive to UI generosity on average than are realized wages. Under the narrowest definitions used in the bottom panel of Table 9, those explicitly requiring zero years of experience, either without regard to education requirements or requiring no more than a bachelor's degree, wages show essentially no responsiveness to UI generosity on average. As the definition of entry-level expands to incorporate jobs requiring up to one or two years of experience, wage effects grow but remain smaller than effects on posted wages overall.

The effects on both realized wages of new entrants and posted wages in entry-level jobs vary across the distribution in ways that differ from both the effects on other groups of workers and effects on posted wages for all positions. Figure 4 shows distributional estimates for entry-level posted wages, new entrant realized wages, and posted wages for all jobs for 2010–2019, to exclude the pandemic period. For entry-level posted wages, effects are smallest at the bottom of the distribution and largest at the top of the distribution, with the 90th percentile estimate being the only one among those shown that is statistically distinguishable from zero. For posted wages for all jobs, the figure shows a pattern of estimates in line with Figure 3 for 2010–2024. The effects on the realized wages of new entrants follow roughly the same distributional pattern as the effects on posted wages for all jobs, a substantially different pattern than that for the effects on posted wages for entry-level jobs.

5.6 Other Pandemic Response Policies

Table 1 shows in column 3 that wages have been less responsive to changes in UI generosity since 2020. Notably, this period includes the unusually robust policy response to the pandemic recession, which could have influenced the relationship between UI and wages in multiple ways. As discussed in Section 3.4, the changes in UI generosity in response to the pandemic were large and involved increases in both benefit amount and benefit duration. It is possible that the relationship between wages and UI is non-linear in ways that lead to smaller estimates for this period.

Policymakers also took steps outside of UI programs in response to the pandemic recession that could have altered the role UI played in wage determination. We focus on two of the most substantial: Economic Impact Payments (EIPs) and the Paycheck Protection Program (PPP).

EIPs were payments issued directly to households on three separate occasions over the course of 2020-2021. In March 2020, the CARES Act authorized payments of up to $1,200 per adult and $500 per child, with the amounts phased out gradually for households earning more than $75,000 for single filers/$150,000 for those married filing jointly. Subsequent legislation authorized additional payments of $600 per adult/$600 per child in December 2020 and $1,400 per adult/$1,400 per child in March 2021, with the same phase-out thresholds as in the CARES Act. Total payments issued across the three rounds reached $814 billion.[19]

PPP provided forgivable loans to small businesses for the purposes of retaining workers while business as usual was disrupted by the pandemic. Loan amounts were based on companies' payroll costs, subject to per-worker and per-loan limits, and businesses could apply for forgiveness based on how the loan proceeds were spent, with expenditures on payroll and other allowable expenses reducing the amount that had to be repaid, subject to certain other conditions. Loans typically covered up to 2.5 times a business's monthly payroll expenses. The program was initially created by the CARES Act and ran, due to various extensions, through March 2021. In total, it made $796.8 billion in loans, of which $762.9 billion was forgiven.[20] Dalton (2021) finds that PPP loans boosted labor demand, increasing both employment and wages.

In order to investigate how these two programs may have affected the relationship between wages and UI generosity, we re-estimate our full-period wage results using a specification that includes controls for exposure to them, as well as interactions between those exposure measures and our UI generosity measure. For EIPs, we calculate total EIP payment amounts per capita, as reported in the CPS's Annual Social and Economic Supplement (ASEC), within the same personal/job characteristic cells we use to calculate our UI generosity measure, for each state in 2020 and 2021.[21] Since PPP issued loans to businesses rather than making payments to individuals, we aggregate loan-level data from the Small Business Administration to

[19] Files detailing EIP payments are available from IRS Statistics of Income here.

[20] See the PandemicOversight.gov PPP dashboard for details.

[21] Though the ASEC did ask about EIP receipt, amounts were ultimately modeled due to potential confusion about which payments should be included and low response rates to these questions. For modeling details, see Bee et al. (2021).

the state-sector level. We estimate

$$
\begin{aligned}
ln\left(y_{it}\right) = & \beta_1 ln(UI(i,t)) + \\
& \beta_2 \left(ln(UI(i,t)) \times EIP(i,t)\right) + \beta_3 EIP(i,t) + \\
& \beta_4 \left(ln(UI(i,t)) \times PPP(i,t)\right) + \beta_5 PPP(i,t) + \\
& \gamma X_{it} + \varepsilon_{it}
\end{aligned} \tag{5}
$$

where $EIP(i,t)$ and $PPP(i,t)$ represent worker i's exposure (in thousands of dollars per person) at time t to EIP payments and PPP loans, respectively, based on their personal and/or job characteristics. These measures are set to zero for years other than 2020-2021.

Incorporating controls for EIPs and PPP and interactions with UI generosity increases the magnitude of the main effect of UI generosity, making it comparable to our pre-pandemic baseline estimate. Table 10 compares estimates of wage effects from our baseline specification with estimates based on Equation 5. Columns 1 and 2 repeat the baseline estimates from Table 1 for reference. Columns 3 and 4 introduce EIP and PPP controls separately, and column 5 includes both. Estimates from column 5 are very similar to pre-pandemic estimates in column 2 across all worker types. Collectively, columns 3 through 5 suggest that controlling for EIPs is primarily responsible for the increased main effect: the increase in the main effect is larger in column 3, where only EIP controls are included, than it is in column 4, where only PPP controls are included, and when both are included (column 5), point estimates on PPP controls fall substantially. This could be because our PPP measure is constrained by the available data to be much coarser than our EIP measure. Coefficients on the EIP controls are also interesting. The estimated main effects for EIPs suggest that larger payments are associated with higher wages, and interactions with UI generosity imply that the wage effects of UI are diminished when EIPs are larger.

Together, these estimates help clarify the difference between the pre- and post-pandemic estimates from our baseline specification. Given that controlling for other significant pandemic-response policies returns the main effect of UI to essentially its pre-pandemic value, it does not seem to be the case that much smaller baseline estimates for the post-pandemic period are due to some non-linearity in the responsiveness of wages to UI generosity. Estimated interactions with the EIP measure may also hint at another mechanism by which increased UI generosity can affect wages: through its role as fiscal stimulus. Increased UI generosity has been a staple of the federal policy response to every recession since 1957 (Council of Economic Advisers and

United States Department of Labor, 2013), but past UI expansions generally have not been accompanied by other spending programs as substantial as EIPs and PPP were. Could the presence of other substantial fiscal stimulus during and after the pandemic recession have crowded out some of the wage benefits ordinarily attributable to the stimulus provided by UI expansions? Further research on this question would be helpful, but is beyond the scope of this paper.

5.7 Secondary Analyses

We also analyze two other mechanisms that could specifically influence the wages of UI-recipient new hires from unemployment: interactions between UI generosity and labor market concentration, and effects of UI generosity on household balance sheets. Ultimately, these analyses do not clearly differentiate UI-recipient new hires from other workers, so we summarize them only briefly here and leave their details to Appendix A.

5.7.1 Interaction with Labor Market Concentration

In more concentrated labor markets, there are fewer employers available to offer a job to a given worker. By increasing the resources available to unemployed workers during their job searches and allowing them to spend more time looking for work, more generous UI could help counteract the negative effects of concentration on wages (e.g., Azar et al., 2022; Rinz, 2022) by giving workers more time to receive job offers, potentially leading to higher wages (Wasser, 2022). When we add a measure of job opening concentration and its interaction with UI generosity to our baseline specification, we find that the main effect of UI remains positive, the main effect of concentration is negative (as expected), and the interaction between the two is positive but small. This pattern of estimates holds across worker types. See Appendix A.1 for details.

5.7.2 Effects on Household Balance Sheets

If increased UI generosity allows UI recipients to build up "excess savings," as some pandemic-era evidence suggests (Ganong et al., 2024), workers could at least partially decouple job search from UI receipt, potentially reducing the estimated wage effects of UI generosity. We use measures of UI receipt, household assets and debts, and employment available in the Survey of Income and Program Participation (SIPP) to investigate this possibility. We find that increased UI generosity increases the value of UI benefits received over various horizons following the beginning of a spell of unemployment, suggesting that our "simulated instrument"-style UI generosity measure would have a "first stage" if used as an actual instrument. However, the estimated effects on asset measures are too imprecise to tell a clear story, and we see little change in employment probabilities over the horizons considered. See Appendix A.2 for details.

5.8 Recap

Our analysis indicates that more generous unemployment insurance increases the wages of not only new hires from unemployment who were previously laid off but also new hires from employment, workers continuously employed in the same job, and new entrants to the labor market. More generous UI reduces the rate at which most unemployed workers are hired, consistent with evidence on the micro effects of UI on job search, but it increases the hiring of new entrants, the unemployed workers most clearly ineligible for UI. Point estimates suggest that the job posting rate declines, but these estimates are relatively imprecise.

Posted wages also rise when UI becomes more generous. The effect on average posted wages is comparable to the effect on average realized wages, but the distributional patterns differ notably. Realized wages respond most strongly to UI generosity at the bottom of the distribution and much less strongly at the top, while there is little effect on posted wages at the bottom of the distribution and the largest effects are found in the top half of the distribution. Posted wages for entry-level jobs show no effect of UI generosity on average. Effects at and below the median are close to zero, while effects in the top half of the distribution are much smaller than the corresponding estimates for overall posted wages. New entrant wage effects more closely follow the distributional pattern we find for all posted wages than the pattern specific to entry-level jobs.

Our baseline estimates suggest that wages for all worker types were less responsive to changes in UI generosity during and after the pandemic, but adding controls for other substantial stimulus programs in effect at the time returns the main effect of UI generosity to essentially its pre-pandemic magnitude for all worker types. EIPs seem to have been especially important, and the estimated interaction between EIP and UI exposure suggests that UI wage effects were smaller where EIP payments were larger. Effects of interactions between UI generosity and labor market concentration are small, and we find no clear evidence of UI effects on household balance sheets.

6 Discussion and Modeling Implications

Our estimated wage effects align closely with the magnitudes of prior estimates from studies of state-level policy changes that lead to substantial reductions in UI generosity. As discussed above, Dahl and Knepper (2022) find that a substantial benefit cut in North Carolina and smaller cuts in other states after the Great Recession reduced starting salaries, implying elasticities similar to ours. Johnston and Mas (2018) use a regression discontinuity design to study the effects on UI recipients in Missouri following a similar benefit cut, again implying a similar elasticity, though their estimate is less precise. That our method, which can be applied to a broader set of workers in a broader set of circumstances, produces estimates of essentially the same magnitude as those prior studies across worker types generalizes those results substantially and raises questions about how such broad wage effects are realized.

Our estimates should also be situated within the macro-oriented UI literature. Much of this literature has focused on how changes in UI generosity affect aggregate employment or unemployment, but a few studies have produced estimates of their effects on wages or earnings. Hagedorn et al. (2016) find that a 1 percent increase in UI generosity (as measured by benefit duration) for one quarter increased the quarterly earnings of continuing employees by 0.0099 log points, using an earnings measure from Quarterly Workforce Indicators (QWI). Also using QWI earnings measures, Hagedorn et al. (2025) find that UI benefit cuts in the US during 2014 reduced earnings for continuing workers (0.02 log points per 1 percent decrease in duration after four quarters) and newly hired workers (0.05 log points) alike, together implying a 0.011 log point decline in aggregate earnings for each percent that UI benefits are reduced. Chodorow-Reich et al. (2019) find no statistically significant change in earnings for either continuing or newly hired workers associated with changes in UI duration.

At a high level, the questions our empirical analysis is intended to address are: 1) whose wages are affected by changes in UI generosity? and 2) how are those effects realized? The answer to the first question turns out to be very simple and somewhat surprising: everyone, in similar measure. The answer to the second is somewhat more nuanced. Our estimates suggest that mechanisms beyond changes in the personal financial circumstances of UI recipients are likely at play, given the breadth of the wage effects we find.

However, they also provide some support for what are often alternative approaches to conceptualizing those mechanisms. For example, the similarity between the average effects of changes in UI generosity on realized wages and posted wages suggests that UI's influence on firms' wage posting strategies likely helps explain the spillover effects on workers who do not themselves receive UI.

But the fact that the effects on realized wages are larger at the bottom of the distribution, where effects on posted wages are relatively small, and smaller at the top, where effects on posted wages are larger, suggests that is unlikely to be the whole story. If, as is often the case, changes in UI generosity are temporary, firms may prefer to bargain over wages at the bottom of the distribution rather than post higher, potentially downwardly rigid wages that may not be profit-maximizing going forward. That bargaining could lead to larger wage effects if the liquidity provided by UI is more valuable in this part of the distribution, and those effects could spill over to UI non-recipients if firms have limited ability to observe workers' UI receipt. Interestingly, new entrants, the unemployed workers who are most clearly not eligible for UI, have wage effects that are fairly consistent in magnitude across the distribution and in line with the average effect on posted wages; this could be consistent with UI wage effects being realized only through a posted wage channel when applicants' UI ineligibility is clear. The increase in the hiring of new entrants that accompanies increased UI generosity could also suggest that firms prefer to avoid dealing with UI-related considerations at the individual level when they can, or alternatively that there are some spillover effects on non-participants from reduced search among the unemployed, as in Marinescu (2017).

There are a range of approaches to modeling search in the labor market, trading off tractability and complexity and allowing changes in parameters to affect decisions made by different agents and on different margins. A model in which wages are set each period for all workers via Nash bargaining (e.g., Pissarides, 2000, Chapter 1) could generate similar changes in wages across worker types due to changes in UI generosity (which influences the bargaining process), but such models involve a greater degree of abstraction from the true search and wage determination processes they approximate. In other cases, the wage determination process precludes similar wage effects across worker types. For example, wages evolve with productivity in Mortensen and Pissarides (1994), all new jobs start with the highest feasible productivity (and pay), and there is no on-the-job search; therefore, changes in UI generosity can only affect the wages of continuing employees and only by raising the productivity threshold below which low-productivity/low-wage jobs are destroyed, changing the average wage of continuously employed workers by changing the composition of jobs. UI generosity can affect the wages of new hires from unemployment and employment via posted wages and on-the-job search in Burdett and Mortensen (1998), but since separations to unemployment are exogenous, there is no mechanism for UI to affect the wages of continuously employed workers.

Across models, the key consequence of changes in UI generosity is to change the value of unemployment relative to the value of employment for both workers and firms. The more valuable is unemployment, the less willing workers will be to accept low wages, or the more willing they will be to risk unemployment in search of high wages if search is directed (Rogerson et al., 2005). Needing to pay higher wages reduces the total surplus available to firms from matches and makes creating vacancies less valuable. Depending on the details of the model, increased UI generosity may also affect outcomes such as offer arrival rates, job destruction, or job-to-job transitions. We do not claim that any particular features of labor search models are essential; all models necessarily abstract from some aspects of the underlying realities they represent, and how they do so depends on the goals of the model.

We believe our results can be useful to modelers interested in unemployment insurance by providing empirical support for labor market and wage dynamics to target as they consider where to simplify their models and where to preserve complexity. We conclude by briefly highlighting three such targets. First, increased UI generosity raises the wages of all types of workers. The elasticity is modest and consistent in magnitude across worker types. Second, transitions from employment to unemployment, sometimes referred to as job destruction, are not very responsive to changes in UI generosity, but job-to-job transitions do decline slightly when UI becomes more generous. The participation margin, which models often omit by assuming workers supply labor inelastically, is much more responsive to changes in UI generosity. Finally, increased UI generosity reduces hiring from unemployment, except for new entrants, the category of unemployed workers most clearly ineligible for UI. We also find that increased UI generosity increases transitions from non-participation to employment, while wage effects for new hires from non-participation are similar to those for other workers, again pointing to the value in considering the participation margin and suggesting some potential preference among firms for hiring UI-ineligible workers when possible or spillovers from changes in search behavior among unemployed workers.[22]

7 Conclusion

Our estimates suggest that increases in UI generosity raise everyone's wages modestly, and wage posting plays a substantial but not exclusive role in those effects. While the effects of UI on personal finances, consumption, and job search behavior are of substantial research and policy interest, our results suggest that the effects of UI on broader matching and wage determination processes could be even more important.

[22]See Appendix Table B3 for mean earnings effects for new hires from non-participation and Appendix Figure B2 for distributional effects.

Spillover or macroeconomically driven wage effects of UI generosity have received relatively little attention in the UI literature to date, but such effects are potentially important for interpreting prior research and for policy. The CPS data we use allow for a detailed examination of wage effects across worker types, which reveals that less detailed wage estimates in prior work represent not some weighted average of meaningful effects for more directly exposed workers and zero effects for others, but consistent effects across all types of workers.

This, plus the robustness of the wage effect for likely UI-eligible workers, also informs our understanding of prior estimates showing that more generous UI extends unemployment duration. If more generous UI also raises wages in a way that operates through wage posting and potentially other market-level mechanisms, it seems less likely that the increased unemployment duration is due to a moral hazard effect and more likely that it is a consequence of workers accepting slower hiring in trade for higher wages (and/or firms accepting higher wages in trade for hiring that is less slow than it otherwise would have been).

Understanding how changes in UI generosity affect the labor market is important for policy. Temporary UI expansions have figured prominently in the federal policy response to every US recession since 1957. Recognizing that another strand of the UI literature considers how UI parameters interact with the business cycle, our positive wage effect estimates suggest that UI is enabling workers to make the tradeoff it was designed to facilitate more than it is subsidizing leisure for workers who could have returned to work more quickly. They also raise the question of how raising the wages of different kinds of workers might affect the ability of increased UI generosity to achieve the various goals policymakers have when using it as a recession response policy.

Further investigation of these questions would be an interesting topic for future research. More detailed quantification of the contributions of specific labor market mechanisms to the wage effects identified here would also be valuable. Finally, we hope that the estimates in this paper can provide some empirical guidance to future theoretical modelers interested in UI as they decide where their models should preserve complexity and where they can safely make things simpler.

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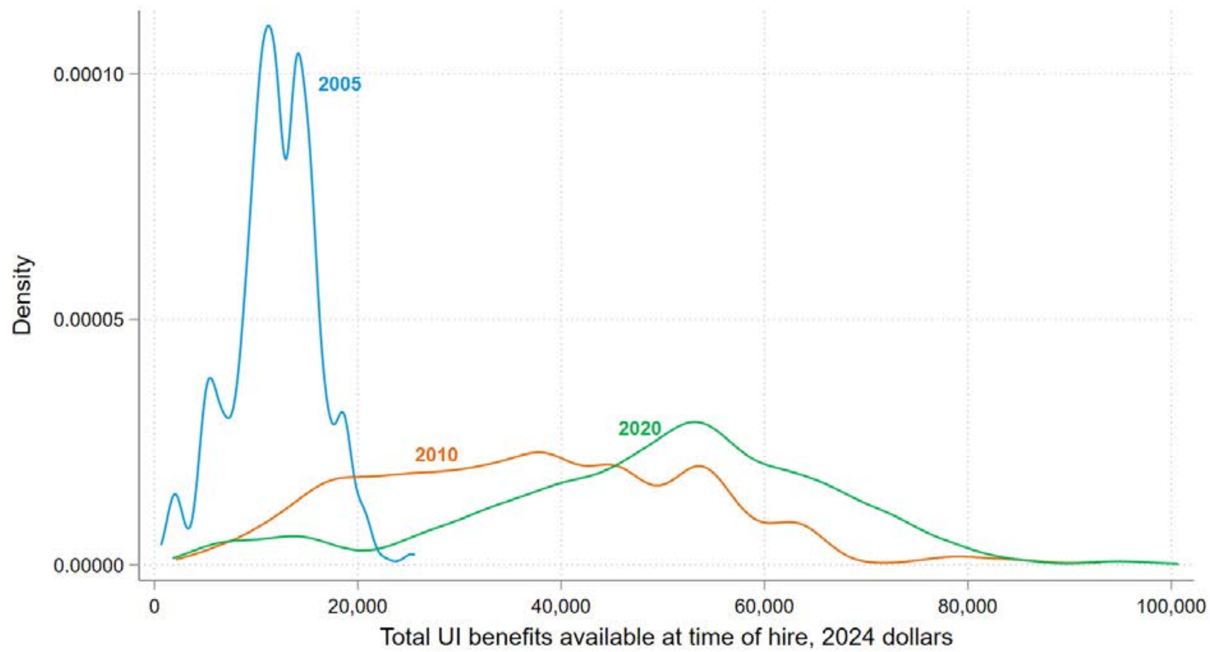
Figures

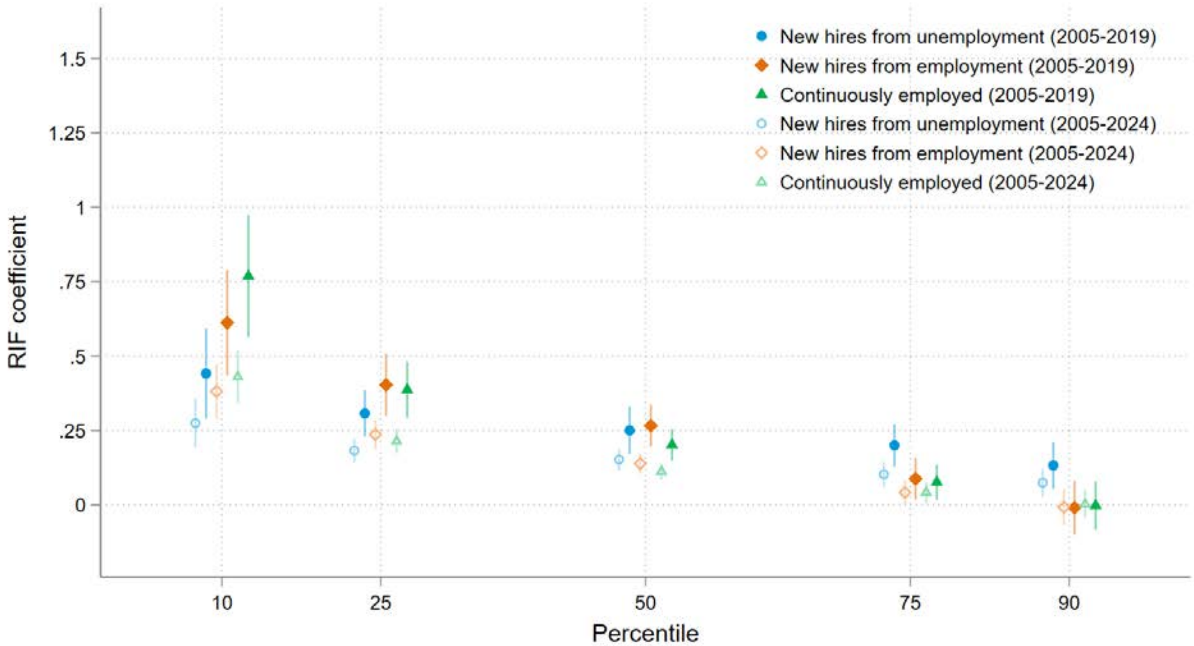
Figure 1: Distribution of total UI benefits available at time of new hires from unemployment, select years



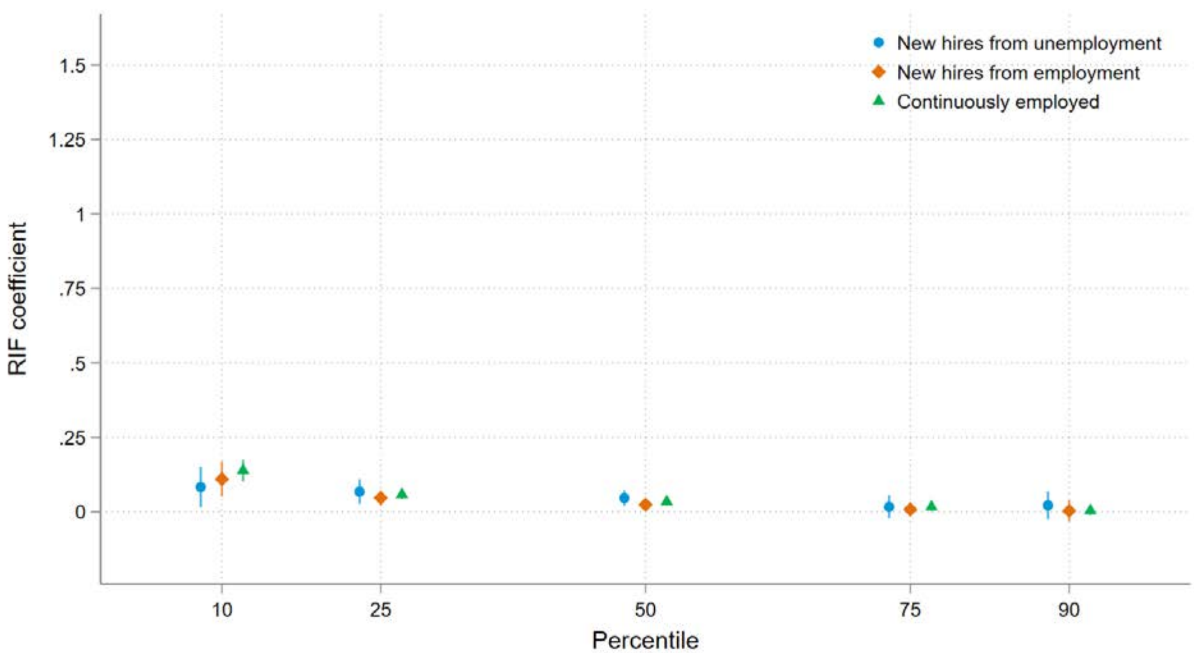
Source: Current Population Survey, Employment and Training Administration Significant Provisions of State UI Laws, and authors' calculations.
Note: Total UI benefits incorporates both weekly benefit amounts and weeks of benefits available and is applied to individual workers as described in Section 4.2.1. Densities are estimated using a Gaussian kernel.

Figure 2: Distributional effects of UI generosity on wages by time period and type of worker

(a) 2005–2019/2024

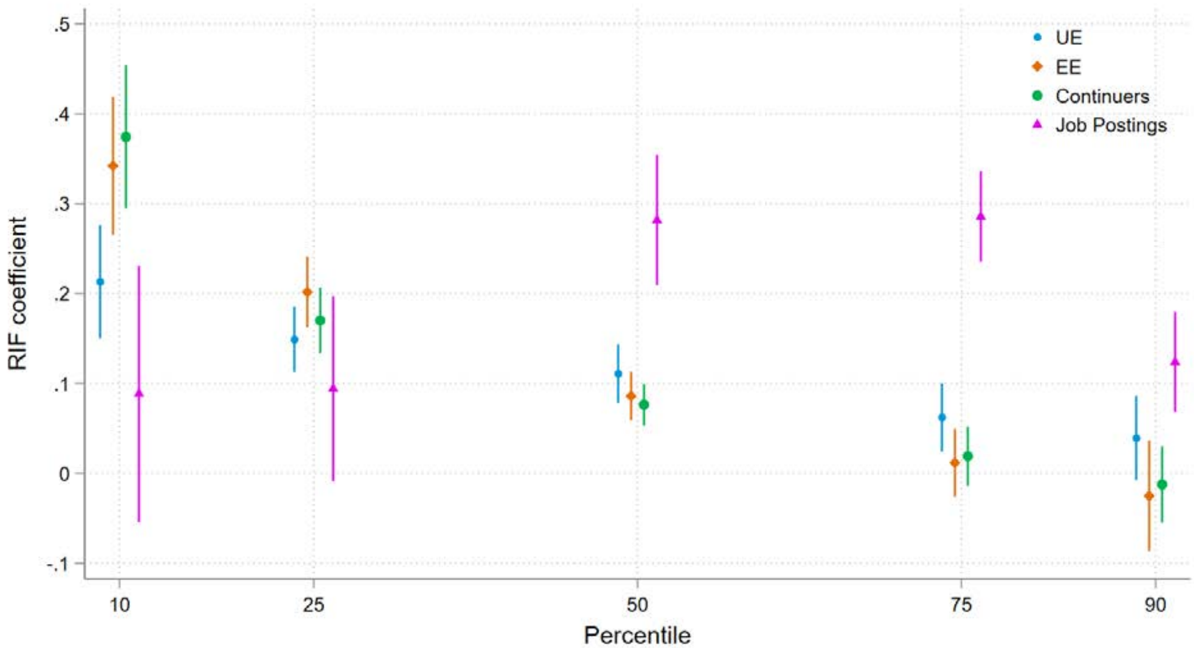


(b) 2020–2024



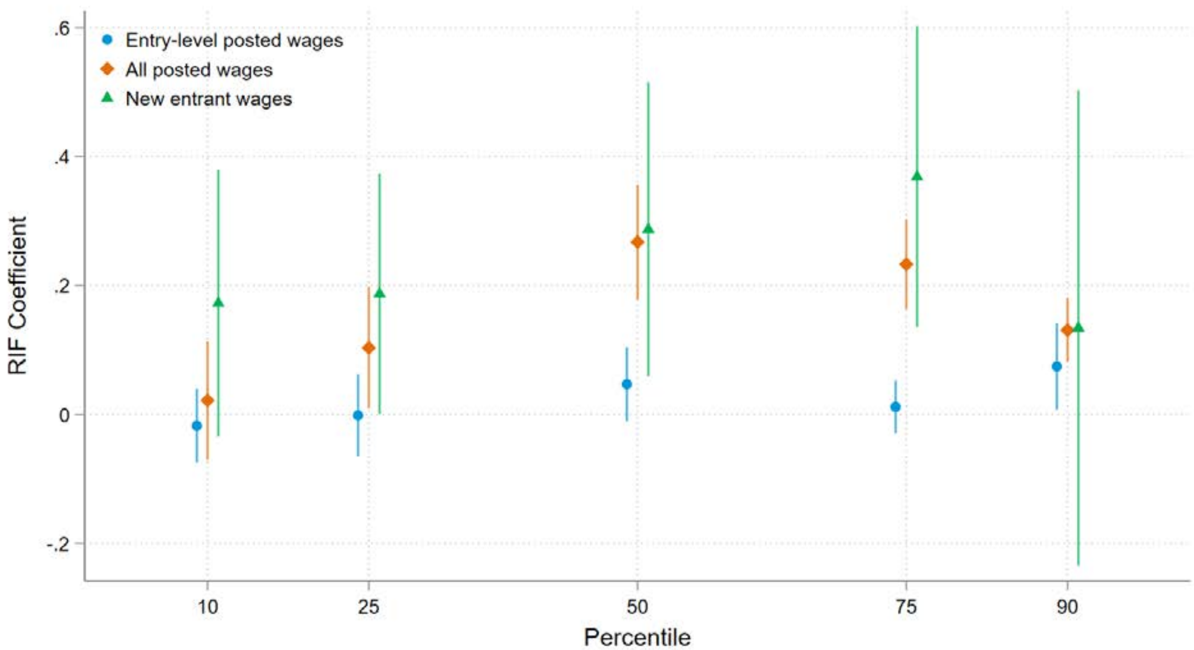
Source: Current Population Survey, Employment and Training Administration Significant Provisions of State UI Laws, and authors' calculations.
Note: Plotted points are coefficients from recentered influence function (RIF) regressions of influence functions constructed for log usual weekly real earnings of new hires from unemployment at the indicated percentiles on log total real UI generosity, as described in Section 4.2.2. Vertical lines represent 95 percent confidence intervals.

Figure 3: Distributional effects of UI generosity on wages, by type, 2010-2024



Source: Current Population Survey, Lightcast, Employment and Training Administration Significant Provisions of State UI Laws, and authors' calculations.
Note: Plotted points are coefficients from recentered influence function (RIF) regressions of influence functions constructed for log usual weekly real earnings of new hires from unemployment at the indicated percentiles on log total real UI generosity, as described in Section 4.2.2. Vertical lines represent 95 percent confidence intervals.

Figure 4: Distributional effects on wages of new entrants and posted wages, 2010–2019



Source: Current Population Survey, Lightcast, Employment and Training Administration Significant Provisions of State UI Laws, and authors' calculations.

Note: Plotted points are coefficients from recentered influence function (RIF) regressions of influence functions constructed for log usual weekly real earnings of new hires from unemployment at the indicated percentiles on log total real UI generosity, as described in Section 4.2.2. Vertical lines represent 95 percent confidence intervals. Entry-level jobs are those that require zero years of experience and have no education requirement.

Tables

Table 1: Effects on log real earnings, by type of worker and time period

	(1) 2005-2024	(2) 2005-2019	(3) 2020-2024
New hires from unemployment, likely UI-eligible			
Log Real Total UI Generosity	0.1298***	0.2139***	0.04341***
	(0.01651)	(0.03118)	(0.01389)
N	65589	53188	12401
R^2	0.2037	0.2041	0.1975
New hires from unemployment, all			
Log Real Total UI Generosity	0.1489***	0.2357***	0.05080***
	(0.01610)	(0.03198)	(0.009432)
N	106710	88084	18626
R^2	0.2397	0.2396	0.2205
New hires from other employment			
Log Real Total UI Generosity	0.1426***	0.2365***	0.04636***
	(0.01730)	(0.03419)	(0.008408)
N	119785	98728	21057
R^2	0.3080	0.3083	0.3024
Continuously employed workers			
Log Real Total UI Generosity	0.1431***	0.2438***	0.05144***
	(0.01571)	(0.03239)	(0.006734)
N	2300748	1869468	431280
R^2	0.3108	0.3138	0.2940

Source: Current Population Survey, Employment and Training Administration Significant Provisions of State UI Laws, and authors' calculations.
Note: Each estimate is from a separate regression. The outcome variable is log real weekly earnings (2024$) as reported in the Current Population Survey. Total UI generosity is the product of the maximum potential weeks of unemployment insurance (UI) and the weekly benefit amount in the worker's state in the month in which they are hired, or the month of wage measurement for continuously employed workers. Weekly benefit amount is constructed by applying state UI rules to average weekly earnings within cells defined by state, year, age, race/ethnicity, college degree, industry of employment, and occupation's blue collar status, as described in Section 4.2.1. Standard errors are clustered at the state level. All regressions include state-year, age, race/ethnicity, college degree, industry, and occupation's blue collar fixed effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Effects of UI generosity on wages, alternative fixed effect specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Wages, new hires from unemployment, likely UI-eligible									
Log Real Total UI Generosity	0.1298***	0.1314***	0.1155***	0.1251***	0.1300***	0.1150***	0.09859***	0.09945***	0.06944***
	(0.01651)	(0.01762)	(0.01702)	(0.01579)	(0.01682)	(0.01553)	(0.01332)	(0.01596)	(0.01101)
N	65589	65583	65215	65562	65589	65308	65584	65589	65207
R^2	0.2037	0.2201	0.2285	0.2304	0.2185	0.2231	0.2188	0.1946	0.2277
Wages, new hires from other employment									
Log Real Total UI Generosity	0.1426***	0.1425***	0.1347***	0.1426***	0.1392***	0.1341***	0.1035***	0.08984***	0.05744***
	(0.01730)	(0.01573)	(0.01724)	(0.01408)	(0.01624)	(0.01691)	(0.01419)	(0.01731)	(0.01258)
N	119785	119785	119586	119785	119785	119717	119785	119785	119488
R^2	0.3080	0.3152	0.3212	0.3216	0.3149	0.3179	0.3220	0.3031	0.3271
Wages, continuously employed workers									
Log Real Total UI Generosity	0.1431***	0.1402***	0.1407***	0.1370***	0.1405***	0.1387***	0.1012***	0.08673***	0.05942***
	(0.01571)	(0.01521)	(0.01599)	(0.01420)	(0.01527)	(0.01571)	(0.01207)	(0.01673)	(0.01165)
N	2300748	2300748	2300748	2300748	2300748	2300748	2300748	2300748	2300726
R^2	0.3108	0.3126	0.3125	0.3127	0.3124	0.3128	0.3225	0.3098	0.3228
State-Year FE	X						X		
College FE	X		X	X	X	X			
Race/Ethnicity FE	X	X		X	X	X			
Age FE	X	X	X		X	X			
Blue Collar FE	X	X	X	X		X			
Industry FE	X	X	X	X	X				
State-Year-College FE		X							
State-Year-Race/Ethnicity FE			X						
State-Year-Age FE				X					
State-Year-Blue Collar FE					X				
State-Year-Industry FE						X			
College-Race/Ethnicity-Age-Blue Collar-Industry FE							X		
State FE								X	X
Year-College FE								X	
Year-Race/Ethnicity FE								X	
Year-Age FE								X	
Year-Blue Collar FE								X	
Year-Industry FE								X	
Year-College-Race/Ethnicity-Age-Blue Collar-Industry FE									X

Source: Current Population Survey, Employment and Training Administration Significant Provisions of State UI Laws, and authors' calculations.
Note: Each estimate is from a separate regression. The outcome variable is log real weekly earnings (2024$) as reported in the Current Population Survey. Total UI generosity is the product of the maximum potential weeks of unemployment insurance (UI) and the weekly benefit amount in the worker's state in the month in which they are hired, or the month of wage measurement for continuously employed workers. Weekly benefit amount is constructed by applying state UI rules to average weekly earnings within cells defined by state, year, age, race/ethnicity, college degree, industry of employment, and occupation's blue collar status, as described in Section 4.2.1. Standard errors are clustered at the state level. ** $p < 0.05$, *** $p < 0.01$

Table 3: Effects on hires from unemployment, by period

	(1) 2005-2024	(2) 2005-2019	(3) 2020-2024
Hires from unemployment, likely UI-eligible			
Log Real Total UI Generosity	-0.002265	-0.006589	0.003275
	(0.004035)	(0.005048)	(0.005138)
N	278543	233637	44906
R^2	0.0629	0.0629	0.0531
Dep. var. mean	0.2613	0.2522	0.3087
New hires from unemployment, all			
Log Real Total UI Generosity	-0.007247**	-0.01126**	-0.003274
	(0.003595)	(0.004382)	(0.004311)
N	490817	417142	73675
R^2	0.0890	0.0892	0.0810
Dep. var. mean	0.2441	0.2368	0.2858

Source: Current Population Survey, Employment and Training Administration Significant Provisions of State UI Laws, and authors' calculations.
Note: Each estimate is from a separate regression. The outcome variable is an indicator for an unemployment-to-employment transition. Total UI generosity is the product of the maximum potential weeks of unemployment insurance (UI) and the weekly benefit amount in the worker's state in each month. Weekly benefit amount is constructed by applying state UI rules to average weekly earnings within cells defined by state, year, age, race/ethnicity, college degree, industry of employment, and occupation's blue collar status, as described in Section 4.2.1. Standard errors are clustered at the state level. All regressions include state-year, age, race/ethnicity, college degree, industry, and occupation's blue collar fixed effects. ** $p < 0.05$, *** $p < 0.01$

Table 4: Effects of unemployment insurance generosity on employment separations, by period

	(1) 2005-2024	(2) 2005-2019	(3) 2020-2024
All separations			
Log Real Total UI Generosity	-0.003826**	-0.01092***	0.003059***
	(0.001859)	(0.003553)	(0.0007752)
N	8545643	6970683	1574960
R^2	0.0591	0.0611	0.0511
Dep. var. mean	0.05808	0.05845	0.05646
Separations to other employment			
Log Real Total UI Generosity	-0.001094***	-0.001561***	-0.0006955**
	(0.0002572)	(0.0004606)	(0.0002602)
N	8545643	6970683	1574960
R^2	0.0026	0.0026	0.0023
Dep. var. mean	0.01852	0.01869	0.01777
Separations to unemployment			
Log Real Total UI Generosity	0.002536***	-0.0005516	0.005415***
	(0.0002799)	(0.0004071)	(0.0002641)
N	8545643	6970683	1574960
R^2	0.0066	0.0064	0.0080
Dep. var. mean	0.01264	0.01271	0.01233
Separations to non-participation			
Log Real Total UI Generosity	-0.005268***	-0.008804**	-0.001661**
	(0.001901)	(0.003526)	(0.0006668)
N	8545643	6970683	1574960
R^2	0.1125	0.1148	0.1026
Dep. var. mean	0.02692	0.02704	0.02637

Source: Current Population Survey, Employment and Training Administration Significant Provisions of State UI Laws, and authors' calculations.
Note: Each estimate is a separate regression. Outcome variables are indicators for transitions from employment to the indicated labor force status. All separations include transitions from one employer to another without a period of unemployment or non-participation. Total UI generosity is the product of the maximum potential weeks of unemployment insurance (UI) and the weekly benefit amount in the worker's state in each month. Weekly benefit amount is constructed by applying state UI rules to average weekly earnings within cells defined by state, year, age, race/ethnicity, college degree, industry of employment, and occupation's blue collar status, as described in Section 4.2.1. Standard errors are clustered at the state level. All regressions include state-year, age, race/ethnicity, college degree, industry, and occupation's blue collar fixed effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Other labor force transitions, 2005–2019

	(1) To employment	(2) To unemployment	(3) To NILF
From employment			
Log Real Total UI Generosity	0.009355**	-0.0005516	-0.008804**
	(0.003512)	(0.0004071)	(0.003526)
N	6970683	6970683	6970683
R^2	0.0860	0.0064	0.1148
Dep. var. mean	0.9602	0.01271	0.02704
From unemployment			
Log Real Total UI Generosity	-0.01126**	0.09815***	-0.08689***
	(0.004382)	(0.01194)	(0.01148)
N	417142	417142	417142
R^2	0.0892	0.1321	0.4437
Dep. var. mean	0.2368	0.5556	0.2077
From NILF			
Log Real Total UI Generosity	0.01546***	0.01449***	-0.02995***
	(0.002226)	(0.002386)	(0.003927)
N	2309462	2309462	2309462
R^2	0.4627	0.1527	0.6311
Dep. var. mean	0.07187	0.03577	0.8924

Source: Current Population Survey, Employment and Training Administration Significant Provisions of State UI Laws, and authors' calculations.
Note: Each estimate is from a separate regression. The outcome variable is an indicator for a labor force transition of the indicated kind. Total UI generosity is the product of the maximum potential weeks of unemployment insurance (UI) and the weekly benefit amount in the worker's state in each month. Weekly benefit amount is constructed by applying state UI rules to average weekly earnings within cells defined by state, year, age, race/ethnicity, college degree, industry of employment, and occupation's blue collar status, as described in Section 4.2.1. All regressions include age, race/ethnicity, college degree, industry, blue collar, and state-year fixed effects. Standard errors are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Effects of UI generosity on job postings, 2010-2024, unweighted

	(1) log(Postings)	(2) Postings/(Emp+Postings)	(3) Postings/Emp	(4) Postings/Pop
Log Real Total UI Generosity	-0.2761	-0.0099*	-0.0151	-0.0107
	(0.1749)	(0.0055)	(0.0117)	(0.0106)
College FE	X	X	X	X
Industry FE	X	X	X	X
Blue Collar FE	X	X	X	X
State-half year FE	X	X	X	X
N	16345	16345	16345	16345
R^2	0.8794	0.6187	0.4448	0.4717
Dep. var. mean	7.7143	0.0506	0.0597	0.0473

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Source: Lightcast, American Community Survey, Current Population Survey, Employment and Training Administration Significant Provisions of State UI Laws, and authors' calculations.
Note: Each estimate is from a separate regression. The outcome variables are measures of the job posting rate constructed within state-year-education-industry-occupation cells. Total UI generosity is the product of the maximum potential weeks of unemployment insurance (UI) and the weekly benefit amount in the worker's state in each month. Weekly benefit amount is constructed by applying state UI rules to average weekly earnings within those same cells. Standard errors are clustered at the state level.

Table 7: Effects of UI generosity on posted wages

Aggregate estimates, 2010-2024

	(1) Mean, no wgt	(2) Mean, pop wgt	(3) Median, no wgt	(4) Median, pop wgt
Log Real Total UI Generosity	0.1494***	0.0844***	0.1687***	0.1206***
	(0.0229)	(0.0241)	(0.0249)	(0.0320)
College FE	X	X	X	X
Industry FE	X	X	X	X
Blue Collar FE	X	X	X	X
State-half year FE	X	X	X	X
N	16261	5301039165	16261	5301039165
R^2	0.6990	0.9158	0.6847	0.9009

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Microdata estimates, by time period

	(1) 2010-2019	(2) 2010-2024	(3) 2010-2013	(4) 2014-2019	(5) 2020-2024
Log Real Total UI Generosity	0.1618***	0.1852***	0.1653***	0.1350***	0.1304***
	(0.0328)	(0.0309)	(0.0321)	(0.0298)	(0.0265)
State-Month FE	X	X	X	X	X
Employer-Job FE	X	X	X	X	X
N	17988398	62514934	3430832	14164383	43170406
R^2	0.8973	0.8903	0.9190	0.9011	0.9085
ymean	10.9841	10.9531	11.0465	10.9687	10.9418

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Source: Lightcast, Current Population Survey, Employment and Training Administration Significant Provisions of State UI Laws, and authors' calculations.
Note: Estimating equation for the top panel is analogous to Equation 3. Wage measures are the log of the mean or median posted wage within state-time-education-industry-occupation cells, as indicated. Regressions are unweighted or weighted according to cell-level population, as indicated. Microdata estimates in the bottom panel are estimated according to Equation 4. Standard errors are clustered at the state level.

Table 8: Effects of UI generosity on the distribution of posted wages, microdata estimates

	(1) 10th pct	(2) 25th pct	(3) 50th pct	(4) 75th pct	(5) 90th pct	(6) 99th pct
ln_totalUIgrp	0.0886	0.0941*	0.2816***	0.2857***	0.1239***	0.0767***
	(0.0709)	(0.0511)	(0.0358)	(0.0251)	(0.0276)	(0.0128)
State-Month FE	X	X	X	X	X	X
Employer-Job FE	X	X	X	X	X	X
N	62514934	62514934	62514934	62514934	62514934	62514934
R^2	0.6333	0.7234	0.8187	0.8345	0.7769	0.8059

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Source: Lightcast, Current Population Survey, Employment and Training Administration Significant Provisions of State UI Laws, and authors' calculations.

Note: Estimates are produced using recentered influence function regression and a specification analogous to Equation 4, estimated for the indicated percentiles. Standard errors are clustered at the state level.

Table 9: Effects of UI generosity on wages and hiring of new entrants and entry-level posted wages

Wages

	(1) 2005-2024	(2) 2005-2019	(3) 2020-2024	(4) 2010-2024	(5) 2010-2019
Log Real Total UI Generosity	0.1352***	0.2538***	-0.0235	0.0970*	0.2216**
	(0.0453)	(0.0811)	(0.0658)	(0.0487)	(0.1010)
Age FE	X	X	X	X	X
Race/Ethnicity FE	X	X	X	X	X
College FE	X	X	X	X	X
Industry FE	X	X	X	X	X
Blue Collar FE	X	X	X	X	X
State-Year FE	X	X	X	X	X
N	5088	4469	619	3597	2978
R^2	0.4028	0.3905	0.4963	0.4206	0.4050

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Hiring

	(1) 2005-2024	(2) 2005-2019	(3) 2020-2024	(4) 2010-2024	(5) 2010-2019
Log Total Real UI Generosity	0.0186***	0.0216***	0.0088	0.0163***	0.0199***
	(0.0036)	(0.0056)	(0.0053)	(0.0034)	(0.0054)
Age FE	X	X	X	X	X
Race/Ethnicity FE	X	X	X	X	X
College FE	X	X	X	X	X
Industry FE	X	X	X	X	X
Blue Collar FE	X	X	X	X	X
State-Year FE	X	X	X	X	X
N	34078	30212	3866	24752	20886
R^2	0.8356	0.8335	0.8516	0.8404	0.8381
Dep. var. mean	0.1676	0.1652	0.1865	0.1595	0.1546

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Entry-level posted wages

	(1) 0 years	(2) 0-1years	(3) 0-2 years	(4) 0 years, <=BA	(5) 0-1 years, <=BA	(6) 0-2 years, <=BA
Log Real Total UI Generosity	0.01365	0.03842*	0.07795***	0.006597	0.04837**	0.08866***
	(0.01907)	(0.01954)	(0.01975)	(0.01647)	(0.02069)	(0.02305)
State-Month FE	X	X	X	X	X	X
Employer-Job FE	X	X	X	X	X	X
N	2573170	10006295	15865096	1019297	4774049	8047298
R^2	0.9255	0.9243	0.9090	0.9235	0.9095	0.9012

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Source: Lightcast, Current Population Survey, Employment and Training Administration Significant Provisions of State UI Laws, and authors' calculations.
Note: Sample for wage and hiring estimates includes workers who reported being new entrants to the labor market when unemployed. The definition of entry-level jobs varies across columns and is based on experience (0 to 2 years) and education (none or up to a bachelor's degree) requirements listed in job ads. Standard errors are clustered at the state level.

Table 10: Wage elasticities with controls for other major pandemic policies

	(1) 2005-2024	(2) 2005-2019	(3) 2005-2024	(4) 2005-2024	(5) 2005-2024
New hires from unemployment, likely UI-eligible					
Log Real Total UI Generosity	0.1298***	0.2139***	0.1908***	0.1445***	0.1904***
	(0.0165)	(0.0312)	(0.0260)	(0.0200)	(0.0260)
Log Real Total UI Generosity X EIP payments per capita ($1000)			-0.0708***		-0.0706***
			(0.0142)		(0.0136)
EIP payments per capita ($1000)			0.7867***		0.7857***
			(0.1571)		(0.1508)
Log Real Total UI Generosity X PPP payments per capita ($1000)				-0.0124**	-0.0000
				(0.0048)	(0.0043)
PPP payments per capita ($1000)				0.1322**	-0.0011
				(0.0512)	(0.0453)
N	65589	53188	65577	65589	65577
R^2	0.2037	0.2041	0.2045	0.2039	0.2045
New hires from other employment					
Log Real Total UI Generosity	0.1426***	0.2365***	0.2288***	0.1609***	0.2290***
	(0.0173)	(0.0342)	(0.0282)	(0.0194)	(0.0282)
Log Real Total UI Generosity X EIP payments per capita ($1000)			-0.1090***		-0.1087***
			(0.0151)		(0.0151)
EIP payments per capita ($1000)			1.1086***		1.1042***
			(0.1616)		(0.1615)
Log Real Total UI Generosity X PPP payments per capita ($1000)				-0.0189***	-0.0005
				(0.0036)	(0.0026)
PPP payments per capita ($1000)				0.1976***	0.0072
				(0.0378)	(0.0275)
N	119785	98728	119763	119785	119763
R^2	0.3080	0.3083	0.3095	0.3082	0.3095
Continuously employed workers					
Log Real Total UI Generosity	0.1431***	0.2438***	0.2350***	0.1636***	0.2351***
	(0.0157)	(0.0324)	(0.0268)	(0.0182)	(0.0269)
Log Real Total UI Generosity X EIP payments per capita ($1000)			-0.1022***		-0.0995***
			(0.0122)		(0.0117)
EIP payments per capita ($1000)			1.0380***		1.0099***
			(0.1290)		(0.1235)
Log Real Total UI Generosity X PPP payments per capita ($1000)				-0.0185***	-0.0023**
				(0.0028)	(0.0011)
PPP payments per capita ($1000)				0.1917***	0.0234*
				(0.0300)	(0.0118)
N	2300748	1869468	2300377	2300748	2300377
R^2	0.3108	0.3138	0.3124	0.3112	0.3124
New hires from unemployment, all					
Log Real Total UI Generosity	0.1489***	0.2357***	0.2140***	0.1598***	0.2129***
	(0.0161)	(0.0320)	(0.0257)	(0.0185)	(0.0256)
Log Real Total UI Generosity X EIP payments per capita ($1000)			-0.0830***		-0.0871***
			(0.0134)		(0.0133)
EIP payments per capita ($1000)			0.9146***		0.9608***
			(0.1491)		(0.1482)
Log Real Total UI Generosity X PPP payments per capita ($1000)				-0.0115***	0.0039
				(0.0040)	(0.0033)
PPP payments per capita ($1000)				0.1186***	-0.0447
				(0.0426)	(0.0347)
N	106710	88084	106684	106710	106684
R^2	0.2397	0.2396	0.2407	0.2398	0.2407

Source: Current Population Survey, Employment and Training Administration Significant Provisions of State UI Laws, Small Business Administration PPP data, and authors' calculations.
Note: Estimates produced based on specification in Equation 5. The outcome variable is log real weekly earnings (2024$) as reported in the Current Population Survey. Total UI generosity is the product of the maximum potential weeks of unemployment insurance (UI) and the weekly benefit amount in the worker's state in the month in which they are hired, or the month of wage measurement for continuously employed workers. Weekly benefit amount is constructed by applying state UI rules to average weekly earnings within cells defined by state, year, age, race/ethnicity, college degree, industry of employment, and occupation's blue collar status, as described in Section 4.2.1. Standard errors are clustered at the state level.

Appendix A Other Analyses

Appendix A.1 Interaction with Labor Market Concentration

There is evidence that a range of policies and institutions (e.g., Azar et al., 2019; Qiu and Sojourner, 2023) mitigate adverse wage consequences for workers of exposure to employer wage-setting power, often referred to as monopsony power and sometimes proxied by measurements of local labor market concentration. Might the implications of changes in UI generosity for wages differ across levels of local labor market concentration and/or types of workers in ways that help illuminate underlying mechanisms of wage transmission?

We define local labor markets as MSA-sector-blue/white collar-education cells and use the concentration of job postings within those cells, as measured using the Herfindahl-Hirschman Index (HHI), as our proxy for employer wage-setting power. We merge this concentration measure with the CPS data (excluding workers who live outside of metropolitan statistical areas) and repeat our analysis of realized wages by worker type using a specification analogous to Equation 1 that also includes the concentration measure and an interaction between concentration and UI generosity. We consider separate specifications with concentration entering in logs and levels.

As shown in Table A1, including the concentration measure has little influence on the estimated direct effect of UI generosity on wages for any type of worker when concentration enters in levels and reduces the main effect somewhat for new hires from unemployment and continuously employed workers when it enters in logs. The correlation between concentration and wages is negative, as one might expect based on the recent labor market concentration literature (for example, Rinz, 2022). The interaction between UI generosity and concentration is typically positive (with the lone exception being the log specification for new hires from employment), suggesting that increased UI generosity produces larger wage gains in more concentrated labor markets, but estimates are typically small and rarely more than marginally statistically significant. For example, the main effect in our log specification implies that doubling UI generosity increases the wages of new hires from unemployment by about 5.8 percent. Increasing log local labor market concentration by one standard deviation, which corresponds to roughly doubling labor market concentration (from a low level, given that analysis is limited to MSAs), increases the wage effect to about 6.5 percent, though the difference is not statistically significant.

Table A1: Effects of UI generosity on wages, concentration interaction

	(1) UE new hires	(2) UE new hires	(3) UE new hires, N>=10	(4) EE new hires	(5) EE new hires	(6) EE new hires, N>=10	(7) Continuers	(8) Continuers	(9) Continuers, N>=10
Log Real Total UI Generosity	0.09771***	0.05829*	0.05824*	0.1110***	0.1035***	0.1028***	0.1094***	0.08041***	0.08152***
	(0.01479)	(0.03300)	(0.03412)	(0.01451)	(0.02539)	(0.02439)	(0.01425)	(0.01777)	(0.01799)
Log Real Total UI Generosity x Log HHI		0.007224	0.007489		0.0007085	0.0008835		0.005215***	0.004839**
		(0.005766)	(0.005929)		(0.003810)	(0.003679)		(0.001859)	(0.001873)
Log HHI		-0.1157*	-0.1166*		-0.06639*	-0.06657*		-0.1053***	-0.1007***
		(0.06079)	(0.06196)		(0.03638)	(0.03501)		(0.01739)	(0.01753)
Age FE	X	X	X	X	X	X	X	X	X
Race/Ethnicity FE	X	X	X	X	X	X	X	X	X
College FE	X	X	X	X	X	X	X	X	X
Industry FE	X	X	X	X	X	X	X	X	X
Blue Collar FE	X	X	X	X	X	X	X	X	X
State-Year FE	X	X	X	X	X	X	X	X	X
N	33668	33668	33542	58395	58395	58129	1163269	1163269	1157157
R^2	0.2069	0.2084	0.2089	0.3240	0.3266	0.3272	0.3202	0.3229	0.3236
UI generosity SD		0.8078	0.8079		0.7852	0.7852		0.7739	0.7737
Log HHI mean		4.9249	4.9128		4.9635	4.9486		5.0210	5.0037
Log HHI SD		0.9857	0.9669		1.0612	1.0397		1.1072	1.0834
Interaction coef * Log HHI SD		0.007121	0.007241		0.0007519	0.0009186		0.005774	0.005242

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

	(1) UE new hires	(2) UE new hires	(3) UE new hires, N>=10	(4) EE new hires	(5) EE new hires	(6) EE new hires, N>=10	(7) Continuers	(8) Continuers	(9) Continuers, N>=10
Log Real Total UI Generosity	0.09771***	0.09324***	0.09493***	0.1110***	0.1069***	0.1075***	0.1094***	0.1058***	0.1054***
	(0.0148)	(0.0155)	(0.0154)	(0.0145)	(0.0147)	(0.0148)	(0.0142)	(0.0141)	(0.0141)
Log Real Total UI Generosity x HHI		0.00001291	0.000009596		0.000009919**	0.000008555		0.000008720***	0.000007515**
		(0.0000)	(0.0000)		(0.0000)	(0.0000)		(0.0000)	(0.0000)
HHI		-0.0001869*	-0.0001547		-0.0001363***	-0.0001223**		-0.0001175***	-0.0001083***
		(0.0001)	(0.0001)		(0.0000)	(0.0001)		(0.0000)	(0.0000)
Age FE	X	X	X	X	X	X	X	X	X
Race/Ethnicity FE	X	X	X	X	X	X	X	X	X
College FE	X	X	X	X	X	X	X	X	X
Industry FE	X	X	X	X	X	X	X	X	X
Blue Collar FE	X	X	X	X	X	X	X	X	X
State-Year FE	X	X	X	X	X	X	X	X	X
N	33668	33668	33542	58395	58395	58129	1163269	1163269	1157157
R^2	0.2069	0.2079	0.2084	0.3240	0.3247	0.3253	0.3202	0.3209	0.3217
UI generosity SD		0.8078	0.8079		0.7852	0.7852		0.7739	0.7737
HHI mean		263.16	248.58		314.69	295.94		354.03	330.88
HHI SD		574.39	497.40		739.60	662.68		836.82	747.68
Interaction coef * HHI SD		0.007417	0.004773		0.007336	0.005669		0.007297	0.005619

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Source: Lightcast, Current Population Survey, Employment and Training Administration Significant Provisions of State UI Laws, and authors' calculations. Note: Analysis excludes workers living outside of metropolitan statistical areas. For the purpose of estimating vacancy concentration, local labor markets are defined as MSA-sector-blue/white collar-education cells; the UI generosity measure is also aggregated to that level. Standard errors are clustered at the state level.

Appendix A.2 Household Finances and Employment

Especially during and after the period of very high UI generosity in response to the COVID-19 pandemic, questions arose in media and policy discussions about whether "excess savings" accumulated during that period were enabling workers to make employment decisions they might otherwise not have made such as changing careers or remaining out of work for extended periods. If workers did, in fact, save a substantial share of the assistance they received early in the pandemic (as some research suggests they did, at least for a time; see, e.g. Ganong et al. 2024), their subsequent employment decisions could be less closely associated with UI generosity, potentially helping to explain the smaller elasticities we find since 2020. We use data from the SIPP to investigate this possibility, focusing on people who we observe transitioning from employment to unemployment during their time in the SIPP.

The SIPP contains the same personal and job characteristics that we use to construct our UI generosity measure in the CPS, making it easy to attach. We consider effects of UI generosity on UI receipt, net worth, and checking and saving account balances over various horizons. Specifically, we estimate

$$y_{it} = \beta_0 + \beta_1 ln\left(UI(i,t)\right) + \gamma X_{it} + \varepsilon_{it} \tag{A1}$$

where variables on the right-hand side are defined as in Equation 1. In the case of net worth and account balance outcomes, which are measured annually in December, X_{it} also includes a fixed effect for the calendar month in which the unemployment spell began, and we focus on measures for the calendar year after the year in which the unemployment spell began (so assets are measured at least 12 months after a worker entered unemployment and we control for time elapsed since entering unemployment). Outcomes y_{it} can take on zero or negative values, so equations are estimated with dependent variables in levels. For UI receipt and employment outcomes, we construct variables that measure amounts of income received and time spent employed over various horizons following the beginning of an unemployment spell.

Table A2 summarizes our results. First, we find that increased UI generosity does increase the amount of UI benefits received over periods ranging from six months to two years following the beginning of an unemployment spell. Due to data collection difficulties during and after the pandemic, the Census Bureau cautions against relying on the SIPP's measure of UI income from 2020 on, so we report estimates for both 2014–2023, which includes pandemic-era policy variation, and 2014–2019, which avoids the pandemic-era data issues. The pre-pandemic estimates imply an elasticity of UI income with respect to our UI generosity measure of about 0.9 over the first six months following the beginning of an unemployment spell. Over longer horizons, elasticities range from about 0.6 to nearly 1. This validates, to some extent, our UI generosity measure: if thought of as a simulated instrument, it appears to have a first stage.

Additional UI income, however, does not appear to translate into improved household finances in these data. Measures of net worth and checking and saving account balances show little response to changes in UI generosity, though estimates are not very precise, especially for net worth. Given that we do not find that UI helps workers accumulate "excess savings," it is unsurprising that we also find no evidence of changes in employment behavior over various longer horizons following the beginning of an unemployment spell.[23]

[23]Though pandemic-era policy discussions provide the motivation for considering this angle, we do not produce estimates specifically for the pandemic period because, as comparing columns 1-4 to 5-8 in Table A2 indicates, the available sample would include only about 400 people, and estimates would likely be too imprecise to be informative.

Table A2: Effects of UI generosity on unemployment insurance receipt, employment, and assets

	2014–2023				2014–2019			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	6 months	12 months	18 months	24 months	6 months	12 months	18 months	24 months
Unemployment insurance received over months since beginning of unemployment spell								
Log Real Total UI Generosity	386**	737***	649	1727***	749***	830**	1070*	1686**
	(176)	(267)	(548)	(607)	(261)	(357)	(547)	(683)
N	2754	1601	1104	696	1822	1188	849	597
R^2	0.1969	0.2142	0.2389	0.2653	0.1642	0.1772	0.2168	0.2471
Dep. var. mean	1096	1318	1635	1809	760	972	1218	1502
	Net Worth	Checking	Saving	Checking/Saving	Net Worth	Checking	Saving	Checking/Saving
Value of assets, December of year following beginning of unemployment spell								
Log Real Total UI Generosity	-3686	-478	-732	-1211	-80800*	-5462	-3248	-8710
	(17402)	(753)	(1591)	(1808)	(40121)	(4570)	(4710)	(6860)
N	1562	1562	1562	1562	1157	1157	1157	1157
R^2	0.3003	0.1335	0.1854	0.1637	0.2763	0.1367	0.2059	0.1632
Dep. var. mean	103564	3090	4277	7367	91536	2786	3612	6399
	6 months	12 months	18 months	24 months	6 months	12 months	18 months	24 months
Share of time spent employed over months since beginning of unemployment spell								
Log Real Total UI Generosity	-0.0272	0.0015	0.0148	0.0120	-0.0134	0.0058	-0.0226	0.0163
	(0.0298)	(0.0359)	(0.0350)	(0.0369)	(0.0443)	(0.0552)	(0.0550)	(0.0661)
N	2754	1601	1104	696	1822	1188	849	597
R^2	0.1708	0.1755	0.2217	0.2272	0.1603	0.1590	0.2010	0.2242
Dep. var. mean	0.5160	0.6246	0.6618	0.6805	0.5167	0.6207	0.6605	0.6785

Source: Survey of Income and Program Participation, Current Population Survey, Employment and Training Administration Significant Provisions of State UI Laws, and authors' calculations.
Note: Each estimate comes from separate regressions. All regressions include state-year, age, race/ethnicity, college, industry, and blue collar fixed effects. Asset regressions also include fixed effects for the calendar month in which the unemployment spell began. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix B Additional Tables and Figures

Table B1: Effects on log real earnings, by type of worker and time period

	(1) 2005-2024	(2) 2005-2019	(3) 2005-2013
New hires from unemployment, likely UI-eligible			
Log Real Total UI Generosity	0.09887***	0.1281***	0.04260***
	(0.01620)	(0.02734)	(0.01429)
N	65589	53188	12401
R^2	0.1914	0.1916	0.1870
New hires from unemployment, all			
Log Real Total UI Generosity	0.1116***	0.1408***	0.05069***
	(0.01643)	(0.02904)	(0.009458)
N	106710	88084	18626
R^2	0.2322	0.2316	0.2142
New hires from other employment			
Log Real Total UI Generosity	0.09347***	0.1190***	0.04326***
	(0.01745)	(0.03097)	(0.008031)
N	119785	98728	21057
R^2	0.3016	0.3016	0.2957
Continuously employed workers			
Log Real Total UI Generosity	0.09024***	0.1135***	0.04922***
	(0.01723)	(0.03142)	(0.006506)
N	2300748	1869468	431280
R^2	0.3091	0.3109	0.2935

Source: Current Population Survey, Employment and Training Administration Significant Provisions of State UI Laws, and authors' calculations.
Note: Each column is a separate regression. The outcome variable is log real weekly earnings (2024$) as reported in the Current Population Survey in first outgoing rotation month for continuously employed workers. Total UI generosity is the product of the maximum potential weeks of unemployment insurance (UI) and the weekly benefit amount in the worker's state in the month in which they are hired. Weekly benefit amount is constructed by applying state UI rules to average weekly earnings within cells defined by state, year, age, race/ethnicity, college degree, industry of employment, and occupation's blue collar status, as described in Section 4.2.1. Standard errors are clustered at the state level. All regressions include state, year, age, race/ethnicity, college degree, industry, and occupation's blue collar fixed effects.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B2: Effects of UI generosity, select outcomes, all variation vs. acute changes in generosity

	(1) 2005-19	(2) DK NC	(3) DK Others
UE new hire wages (laid off)	0.2139***	0.1187***	0.1578***
	(0.03118)	(0.03838)	(0.03572)
N	53188	11671	19366
R^2	0.2041	0.2096	0.2000
UE new hire wages (all)	0.2357***	0.1343***	0.1774***
	(0.03198)	(0.04449)	(0.03714)
N	88084	18915	31608
R^2	0.2396	0.2465	0.2406
EE new hire wages	0.2365***	0.1347***	0.1498***
	(0.03419)	(0.03660)	(0.04057)
N	98728	19749	33491
R^2	0.3083	0.3195	0.3178
Continuously employed wages	0.2438***	0.1344***	0.1733***
	(0.03239)	(0.04203)	(0.03001)
N	1869468	388340	654159
R^2	0.3138	0.3215	0.3176
Hires from unemp (laid off)	-0.006589	-0.0009162	-0.01223
	(0.005048)	(0.006058)	(0.007806)
N	233637	53758	90594
R^2	0.0629	0.0521	0.0541
Hires from unemp (all)	-0.01126**	-0.004875	-0.01631**
	(0.004382)	(0.006392)	(0.006767)
N	417142	94953	159238
R^2	0.0892	0.0753	0.0783
Job posting rate	-0.009929*	-0.01021	-0.01242
	(0.005540)	(0.01093)	(0.009011)
N	16345	3392	4232
R^2	0.6187	0.5310	0.5399
Posted wages	0.1494***	0.1544**	0.1563**
	(0.02294)	(0.07067)	(0.06918)
N	16261	3364	4202
R^2	0.6990	0.6727	0.6726

Source: Current Population Survey, Employment and Training Administration Significant Provisions of State UI Laws, Lightcast. and authors' calculations. Note: For wage and hiring outcomes, the baseline specification in column (1) uses data from 2005-2019; for the job posting rate and posted wages, it uses data from 2010-2024. Other columns report estimates based on states and time periods used in Dahl and Knepper (2022), indicated using DK. The NC specification in column (2) includes data for July 2009 through December 2016 from all Midwestern and Southern states except Florida, Georgia, Kansas, Michigan, Missouri, and South Carolina. The Others specification in column (3) includes data from April 2007 through December 2016 for all Midwestern and Southern states except North Carolina.

Table B3: Effects on log real earnings, new hires from non-participation

	(1) 2005-2024	(2) 2005-2019	(3) 2020-2024
Log Real Total UI Generosity	0.1628***	0.2592***	0.0721***
	(0.0169)	(0.0316)	(0.0104)
Age FE	X	X	X
Race/Ethnicity FE	X	X	X
College FE	X	X	X
Industry FE	X	X	X
Blue Collar FE	X	X	X
State-Year FE	X	X	X
N	163566	134682	28884
R^2	0.2412	0.2406	0.2294

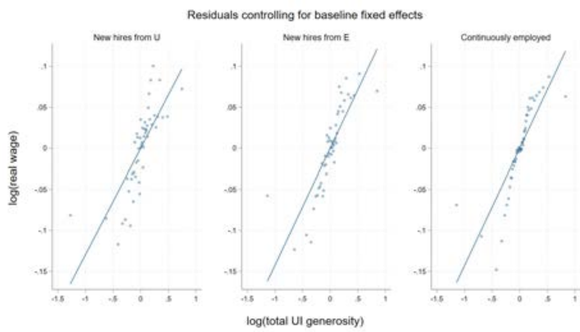
Standard errors in parentheses

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

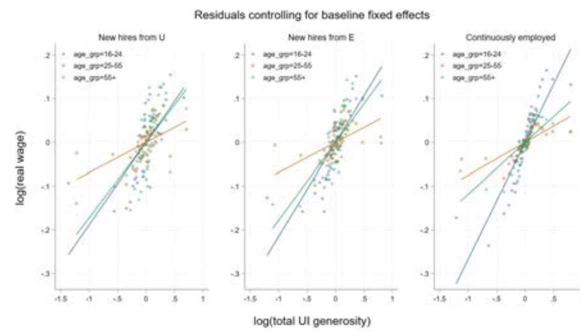
Source: Current Population Survey, Employment and Training Administration Significant Provisions of State UI Laws, and authors' calculations. Note: Sample consists of workers making transitions from nonemployment to employment and remaining employed until their wages are measured in the CPS. The outcome variable is log real weekly earnings (2024$) as reported in the CPS. Total UI generosity is the product of the maximum potential weeks of unemployment insurance (UI) and the weekly benefit amount in the worker's state in the month in which they are hired, or the month of wage measurement for continuously employed workers. Weekly benefit amount is constructed by applying state UI rules to average weekly earnings within cells defined by state, year, age, race/ethnicity, college degree, industry of employment, and occupation's blue collar status, as described in Section 4.2.1. Standard errors are clustered at the state level. All regressions include state-year, age, race/ethnicity, college degree, industry, and occupation's blue collar fixed effects. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Figure B1: Residualized wage-UI relationship across worker types, by personal/job characteristics

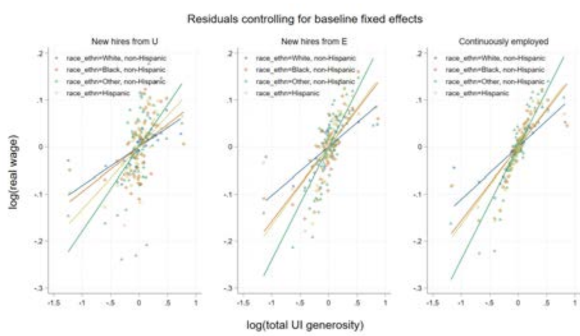
(a) Full Sample



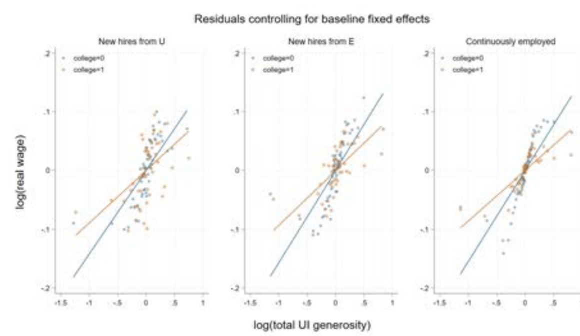
(b) Age



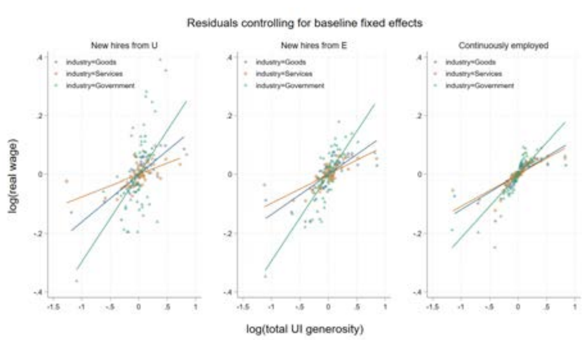
(c) Race/ethnicity



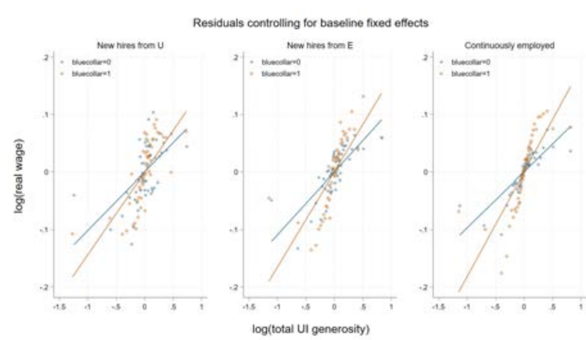
(d) Education



(e) Industrial sector

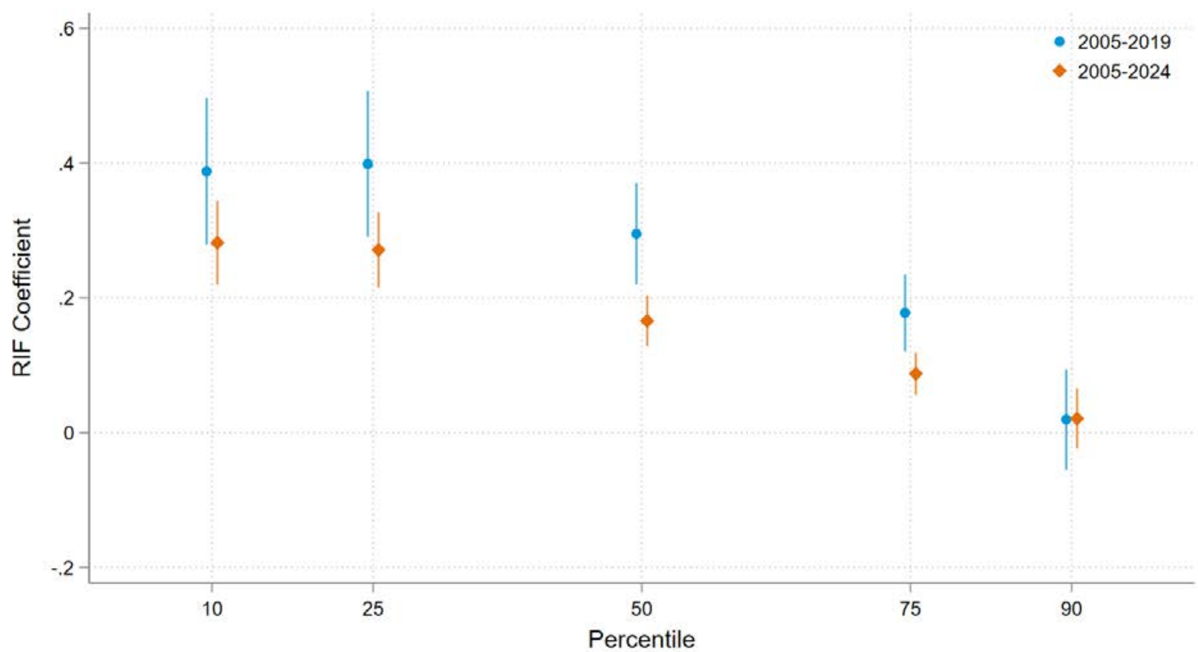


(f) Broad occupation



Source: Current Population Survey, Employment and Training Administration Significant Provisions of State UI Laws, and authors' calculations.
Note: Each panel shows a binned scatter plot of log wages and log UI generosity, each residualized on the fixed effects from Equation 1. Solid lines are based on linear regressions of the wage residuals on the UI generosity residuals.

Figure B2: Distributional effects of UI generosity on wages of new hires from nonparticipation



Source: Current Population Survey, Employment and Training Administration Significant Provisions of State UI Laws, and authors' calculations.
Note: Sample consists of workers making transitions from nonemployment to employment and remaining employed until their wages are measured in the CPS. Plotted points are coefficients from recentered influence function (RIF) regressions of influence functions constructed for log usual weekly real earnings of new hires from unemployment at the indicated percentiles on log total real UI generosity, as described in Section 4.2.2. Vertical lines represent 95 percent confidence intervals.