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Enhanced Unemployment Insurance Benefits in the United States During COVID-19: Equity and Efficiency

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Enhanced Unemployment Insurance Benefits in the United States During COVID-19: Equity and Efficiency

Abstract

We assess the effects of the historically unprecedented expansion of U.S. unemployment insurance (UI) payments during the COVID-19 pandemic. The adverse economic impacts of the pandemic, notably the pattern of job losses and earnings reductions, were disproportionately borne by lower-income individuals. Focusing on household income as a broad measure of well-being, we document that UI payments reversed the increase in household income inequality that otherwise would have occurred in 2020 and 2021. We also examine the impacts of the $600 increase in weekly UI benefit payments, available during part of 2020, on job search outcomes. We find that despite the very high replacement rate of lost earnings for low-wage individuals, the search disincentive effects of the enhanced UI payments were limited overall and smaller for individuals from lower-income households. These results suggest that the pandemic UI expansions improved equity but had limited consequences for economic efficiency.

Keywords: unemployment insurance, COVID-19, inequality, income support, job search

JEL codes: **D31, J64, J65**

Enhanced Unemployment Insurance Benefits in the United States During COVID-19: Equity and Efficiency

I. Introduction

The COVID-19 pandemic that began in early 2020 prompted widespread curtailment of social and economic activity in the United States aimed at controlling the spread of the virus. The resulting labor market shock was unprecedented in its speed and severity. During the first few months of the pandemic, job losses and unemployment surged to levels not seen since the Great Depression of the 1930s, and labor force participation sagged. The policy response was swift, with the CARES Act passed in late March 2020 providing substantial direct aid for households and individuals affected by the pandemic and ensuing public health responses.

The enhancement of unemployment insurance (UI) benefits was a key element of this expanded public support intended to provide an economic bridge past the pandemic. The UI enhancement was historically unprecedented. Most notably, it included a temporary increase in weekly benefit payments of $600, available from late March through late July 2020. This benefit increase dwarfed any prior increases in UI payments during economic downturns; it more than doubled typical weekly UI payments across the United States (Ganong et al., 2020; Petrosky-Nadeau and Valletta, 2023). After expiration of the $600 benefit followed by partial renewal later in 2020, the benefits enhancement was reinstated at $300, half its prior level, for most of 2021. The increases in weekly benefits were accompanied by substantial expansions of maximum benefit durations and eligibility.

In this paper, we contribute to two existing strands of literature on the impacts of the enhanced pandemic UI benefits. One strand examines the equity or distributional impacts of the UI payments, generally focusing on the degree to which they replaced lost earnings and how this varies by pre-job loss earnings levels (e.g., Ganong et al., 2020; Cortes and Forsythe, 2023a; Larrimore et al., 2022). We extend this line of research by focusing on household income rather than individual earnings. We use microdata from the Current Population Survey Annual Social and Economic Supplement (CPS ASEC). Due to substantial underreporting of UI payments in the CPS ASEC data during the pandemic, we use imputed UI amounts derived from tax records (from Larrimore et al., 2023). We find that UI payments reversed the increase in household income inequality that otherwise would have occurred in 2020 and 2021.

We also examine the impacts of the $600 increase in weekly UI benefit payments on job search outcomes. The very large increase in available UI benefits during the pandemic raised concern about reduced incentives to return to work that could delay the labor market recovery. Such search disincentive or economic efficiency effects have been documented in a broad economic literature (see the overview in Petrosky-Nadeau and Valletta, 2023). They represent a potential adverse consequence of the intended insurance and equity effects of UI benefit payments. Our empirical framework to assess these economic efficiency effects is from Petrosky-Nadeau and Valletta (2023). They use a "difference-in-difference" regression design that exploits the wide variation in the extent to which the UI payments replaced prior earnings, i.e. the "replacement rate." We find that despite the very high replacement rate of lost earnings for low-wage individuals, the search disincentive effects of the enhanced UI payments were limited overall and smaller for individuals from lower-income households. Overall, our results suggest that the pandemic UI expansions improved equity but had limited consequences for economic efficiency.

II. Pandemic UI as Income Support

A. The Pandemic Labor Market Shock and UI Policy Responses

The initial effects of the pandemic on the U.S. labor market were rapid, devastating, and unprecedented. The immediate shutdowns for a wide range of industries pushed millions of individuals out of their jobs, with limited options for alternative employment. Between February and April 2020 the official U.S. unemployment rate rose over 10 percentage points, from 3.5 percent to 14.7 percent, the highest level recorded since the Great Depression of the 1930s. This was accompanied by a sharp drop in labor force participation and widespread misreporting of employment status, with many survey respondents indicating that they were absent from work rather than on layoff. As such, the labor market disruption was even larger than indicated by the increase in the official unemployment rate.[1] Viewed more broadly, the number of individuals employed fell by slightly over 25 million between February and April 2020, representing an employment decline of about 16 percent and suggesting an unemployment rate near 20 percent.

These adverse effects on the labor market were disproportionately borne by lower-income and otherwise disadvantaged groups. For example, Cortes and Forsythe (2023b) found that he pandemic exacerbated pre-existing inequalities, with substantially larger employment losses in lower-paying occupations and industries. A number of papers also document that disproportionate impact of the pandemic on the economic status of racial/ethnic minorities and women (Bitler et al., 2023; Couch et al., 2020, 2022).

The response to the pandemic's severe and uneven economic shock, via expansion of transfers and social safety net spending, was equally rapid and unprecedented (Bitler et al., 2020,

[1] Bartik et al. (2020) and Cajner et al. (2020) detail the immediate impact of the pandemic on the U.S. labor market. See U.S. BLS (2020) for discussion of the mismeasurement of labor market status and its implications for pandemic effects on the U.S. labor market.

2023; National Academy of Social Insurance, 2023). The Coronavirus Aid, Relief, and Economic Security (CARES) Act, passed quickly and made effective in late March 2020, provided a wide range of near-term income support and stimulus elements aimed at households and businesses. It included direct aid for individuals and households, such as tax rebates, along with loan forgiveness and support provisions for businesses. At $2.2 trillion, it was the largest economic stimulus package ever passed in the United States.

The element of the CARES Act aimed most directly at helping the massive number of job losers was the enhancement and expansion of UI benefits. The act expanded the eligibility for UI benefit receipt, most notably to self-employed and gig workers, along with increasing the maximum duration and weekly amounts payable for UI benefits.

The increase in UI benefit payments was particularly large. At $600 per week, available from late March through July 2020, it represented an historically unprecedented increase in UI benefit amounts. With median UI benefit payments at about $400/week, the extra $600/week more than a doubled typical payment amounts. It was designed to largely replace lost earnings for individuals displaced by the pandemic shutdowns. The uniform amount paid for all earners, however, meant much larger replacement rates for lower earners. As a result, most recipients of UI benefits during this timeframe received weekly UI payments that exceeded their earnings prior to job loss (Ganong et al., 2020; Petrosky-Nadeau and Valletta, 2023).

The emergency UI policies specified in the CARES Act evolved as the pandemic progressed (see Whittaker and Isaacs, 2022, for details). As the pandemic's labor market disturbances continued, an initial extension of 13 weeks via the CARES Act eventually grew to 49 weeks during much of 2021 (through September). This meant 75 weeks of benefit availability when combined with 26 weeks of normal UI benefits, a long duration in historical terms but shorter than the 99 weeks available in the aftermath of the Great Recession of 2007-09 (Valletta, 2014). Due to uncertainty and disagreement about the extent of income support needed during the pandemic, the $600 supplement was not immediately renewed upon expiration at the end of July 2020. Following a brief renewal at a lower level in August 2020, the supplement was renewed in early 2021 but at $300/week, half the prior level. This federal policy was in place until September 2021. However, due to rapidly improving labor market conditions in some states in early to mid-2021, combined with political disagreement about the tradeoffs inherent to UI benefit payments, many states opted out of the program early, beginning in June 2021.

B. Data

We now turn to an assessment of the extent to which the UI policies described above offset the adverse, unequal impacts of the pandemic labor market shock on household income. For our analyses, we use microdata on household income, employment status, and related characteristics from the CPS ASEC, which is the source of official government statistics on income distribution and poverty. The ASEC is administered to monthly CPS respondents primarily in March, with a smaller share of respondents asked to complete it in other months.[2] We use data from the 2020-22 ASEC surveys, which contain detailed information on income sources and employment status in the preceding calendar years of 2019-2021. We adjust all income amounts for inflation using annual values of the PCE price index, relative to 2019 as the base.

[2] The monthly CPS survey is the source of official labor force statistics in the United States. The ASEC is one of a number of supplemental surveys conducted as part of the CPS survey program.

The CPS ASEC separately identifies the amount of regular and supplemental UI benefits received and hence enables us to directly examine their impact on the distribution of household income. One challenge is that a sharp rise in CPS survey non-response during the pandemic intensified measurement challenges for income and employment statistics. In particular, recent work by Larrimore et al. (2023) shows considerable understatements of UI recipiency status and amounts received within the CPS-ASEC relative to administrative data from the IRS, particularly for years within our reference period of 2019-2021. For calendar year 2020, CPS ASEC estimates of total UI recipients and benefits received were respectively about 50% and less than 40% of the amounts reported in administrative data. Misreporting was particularly concentrated toward the lower end of the income distribution. This has important distributional implications. For example, Larrimore et al. report that correcting for underreporting of UI benefits reduces measured poverty by nearly 2 percentage points in 2020, to a record low of 9.6% (versus 10.5% in 2019)

To adjust for this severe understatement of UI benefits in CPS ASEC and other data sources, Larrimore et al. (2023) developed and provide adjusted UI recipiency amounts based on detailed tax records.[3] We use their data and imputation procedure, which entails adjusting UI benefits reported in our CPS ASEC dataset by randomly imputing recipients and amounts received within income centiles to match the distribution of UI benefits reported in their tax data.[4] Our construction of the income measure used as the basis for the imputation procedure

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[3] Available at https://www.davidsplinter.com/.

[4] The imputation procedure is reliable for assessing distributional impacts that reflect variation across but not within the 100 income cells used for the calculations. Our analyses below focus on standard income percentiles (median, 10th and 90th percentile, etc.). This imputation procedure is not appropriate for analyses conducted at the individual level in microdata. For our analyses of individual job search behavior in the next section, we use a different procedure for imputing UI benefits in 2020 (see Section III).

follows Larrimore et al. (2023) in aggregating different sources at the tax-unit level and splitting equally between spouses.[5]

C. Distributional impact of UI benefits in 2020 and 2021

To assess the distributional impact of the UI benefit payments during the pandemic, we use the simple approach of comparing the distribution of real (inflation-adjusted) household income with and without UI benefits (adjusted for underreporting) included in the calculation. We start with a visual representation based on kernel density estimation of the income distribution. Such density plots are essentially smoothed histogram representations of an underlying distribution, where the kernel function and bandwidth used for estimating it determine the exact shape.[6]

Figure 1 shows the impact of UI benefits on the distribution of real household income in 2020. For visual clarity we only display household income ranges of 0 to $100,000; outside this range, UI benefit payments have minimal impact. Comparing the black and grey solid lines shows that the distribution of income shifted out somewhat between 2019 and 2020, indicating increases in overall income after correcting for underreporting of UI benefits. In a stark contrast, the dashed line shows what the distribution of household income would look like if no UI payments had been made in 2020. This counterfactual distribution is shifted sharply to the left

[5] Rothbaum and Bee (2021) analyze non-response bias in the CPS ASEC data collected during the pandemic. They find that lower-income households were less likely to respond, thereby overstating aggregate household income using the survey weights. They develop alternative individual weights, which were used by Larrimore et al. in forming their UI imputation data. Household weights adjusted in a similar manner are not available, so we use the conventional CPS ASEC supplement weights.

[6] Daly and Valletta (2006) discuss kernel density methodology in more detail. We use an Epanechnikov kernel function and 100 bins that are set as identical for the distributions displayed.

and has much more pronounced mass toward the bottom tail, for household incomes below about $30,000.[7]

Figure 1:

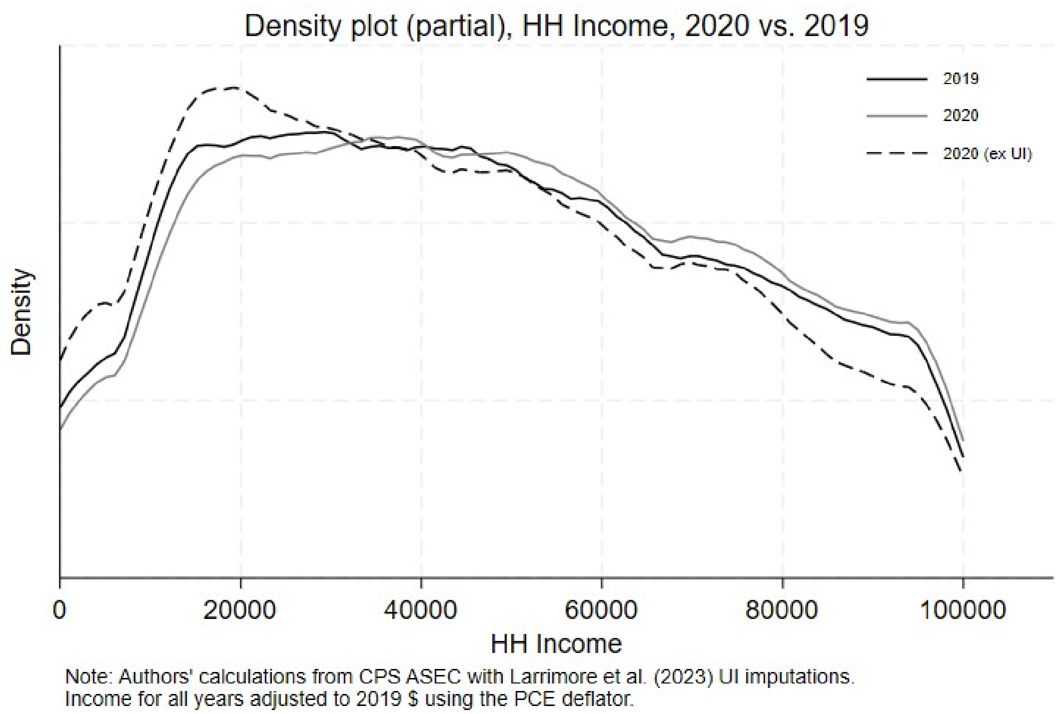
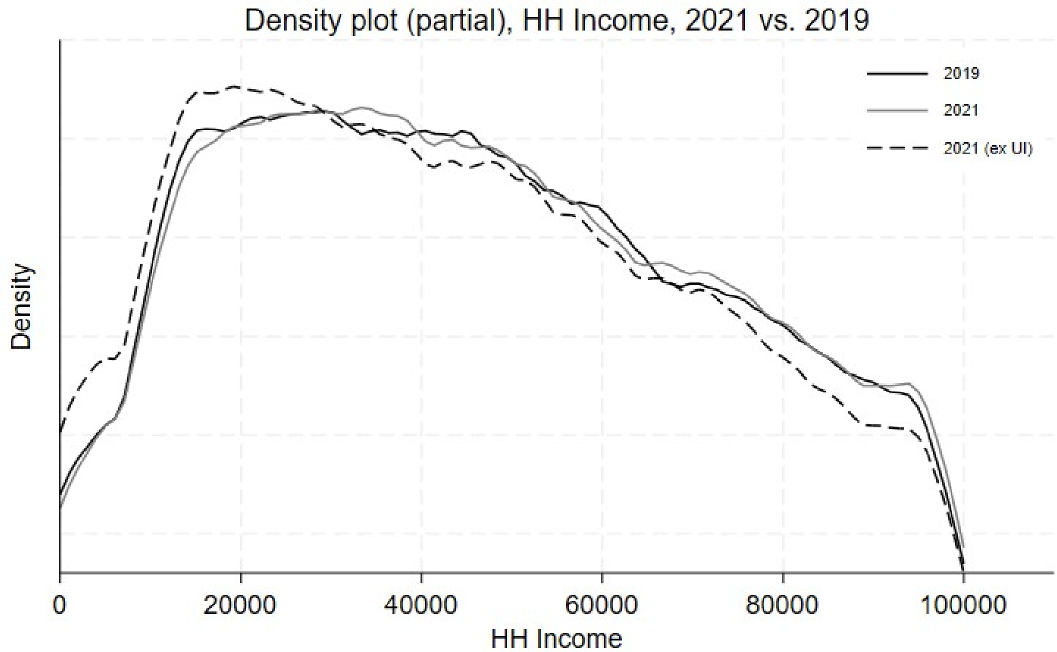


Figure 2 is identical to Figure 1 but provides the income comparison for 2021 rather than 2020. As in Figure 1, the counterfactual distribution with UI payments eliminated in Figure 2 shows a leftward shift and greater mass below the $30,000 level. However, the shift is less pronounced for 2021 in Figure 2 than for 2020 in Figure 1. This smaller distributional effect of UI payments in 2021 reflects: (i) the reduction in the weekly UI supplement, from $600 to $300; (ii) improved labor market conditions that reduced UI recipiency.

[7] It would be interesting and informative to distinguish between payments via regular UI and the expanded pandemic UI programs. Unfortunately, the CPS ASEC data and the tax records used by Larrimore et al. (2023) do not distinguish between different components of UI income.

Figure 2:



Note: Authors' calculations from CPS ASEC with Larrimore et al. (2023) UI imputations.
Income for all years adjusted to 2019 $ using the PCE deflator.

Table 1 provides a quantitative summary of the main distributional impacts of UI benefit payments for the years 2020 and 2021. It lists the median and selected other percentile ratios from the distribution of household income underlying the density plots depicted in Figures 1 and 2. The selected percentiles used for the ratios, the 10^{th}, 25^{th}, 50^{th}, 75^{th}, and 90^{th}, are commonly used to summarize differences in income distributions (see for example Daly and Valletta, 2006).

Table 1
Household income percentiles (median and ratios), with/without UI

	(1) 2019	(2) 2020	(3) 2020 (ex. UI)	(4) 2021	(5) 2021 (ex. UI)
Median (p50)	69030	70057	64980	69179	66608
p90/p10	12.4	11.4	13.9	12.2	14.1
p50/p10	4.3	4.0	4.6	4.1	4.7
p90/p50	2.9	2.9	3.0	2.9	3.0
p75/p25	3.6	3.4	3.8	3.6	3.8

Source: Authors' calculations from CPS ASEC data with Larrimore et al. (2023) UI imputations (years listed are the income reference year, which is the year prior to the corresponding survey year).
Note: Income for all years adjusted to 2019 terms using the PCE deflator.

The comparison of the 2019 and 2020 observed distributions in columns 1 and 2 of Table 1 shows an increase of about 1.5 percent in median real household income and a reduction in inequality (dispersion) of income. The third column shows that the distribution would have been much different had UI payments not been available in 2020: the median value would have been about 7 percent lower, and inequality would have been much higher. The latter effect is almost entirely due to differences in the lower half of the distribution, with a notably larger ratio of median to 10th percentile earnings when UI benefits are excluded.

The comparison of the final two columns to the prior columns shows a similar but less pronounced pattern of UI benefit effects in 2021 compared with 2020. Based on reported income including imputed UI benefits (columns 2 and 4), median real income fell slightly between 2020 and 2021, and bottom-half inequality rose slightly. Absent UI benefits, median real income would have declined materially from 2020, and dispersion in the lower half of the distribution would have increased.

Overall, these results show that expanded UI fully neutralized the impact of the pandemic on the distribution of HH income in the years 2020 and 2021: absent UI benefit payments, inequality in the bottom half of the distribution would have risen substantially. This is consistent with the large poverty-reducing effects of pandemic UI payments reported by Larrimore et al. (2023).

III. Job Search Effects

A. Background

By offsetting lost earnings and reducing the near-term costs of job loss, UI benefit payments make unemployment a less deleterious state for individuals and household members with whom they share income. A long literature in economics discusses the resulting "moral hazard" effect of UI benefit payments on job search effort and job acceptance rates (see Petrosky-Nadeau and Valletta, 2023, for discussion). The $600 UI benefit supplement in 2020 dwarfs any previous benefit enhancement, and it more than doubled typical weekly UI benefit payments (which averaged slightly under $400 across U.S. states prior to the pandemic). Ganong et al. (2020) note that absent supplements, the median UI replacement rate across all eligible workers in the United States hovers slightly below 0.5 (around 50% of prior earnings). They find that the $600 CARES Act UI supplement raised this number substantially, to a median value of 1.34. This implies that a majority of UI recipients were eligible for benefits exceeding their prior job earnings. Such large increases in UI benefit levels could substantially offset job search incentives. We investigate such effects in this section.

B. Data

Our analysis of pandemic labor force transitions relies on matched monthly CPS data, replicating and extending Petrosky-Nadeau and Valletta (2023) (PNV 2023). We link the monthly files to annual earnings sourced from the ASEC, which limits our matched observations to the months of January through July 2020. This timeframe is narrow but enables us to focus on the period when the $600 supplement was in place (April-July), along with the preceding two months of 2020 (February-March), which are used as a pre-treatment comparison period.

Due to the rotating sampling scheme used for the CPS, surveyed households and individuals are in the sample for two separate periods of 4 consecutive months (with an intervening 8-month period spent out of the sample). This enables consecutive month-to-month matching for around 70% of the sample, which we further validate by ensuring that reported demographics do not conflict for matched individuals.[8] We then identify labor market transitions by comparing an individual's labor force status in consecutive months. In this paper, we focus on transitions out of unemployment (U) to employment (E), a subset of the transitions examined by PNV (2023).

A well-known concern regarding matched CPS data is the likelihood of spurious transitions in labor force status arising from inconsistent or error-ridden survey responses rather than meaningful changes. Therefore, in addition to examining monthly transitions, we also follow past research by adjusting the data to minimize the incidence of spurious transitions. For this adjustment, we recode individuals identified as having left unemployment one month and

[8] Most of the non-matched observations are from the "outgoing rotation groups" that are exiting the sample for eight months or permanently (one quarter of each monthly sample). In addition, a modest fraction of observations is lost because respondent households that move to different geographic locations are not followed.

returned the next month as having had no transition (Rothstein, 2011; Valletta, 2014; Farber and Valletta, 2015; Farber et al., 2015). If left unaddressed, these errors could bias the estimated effect of UI payments on labor force transitions downward and decrease the precision of our estimates. Applying this modification to the data reduces monthly transitions out of unemployment by about five percentage points (Valletta, 2014). The adjustment results in a sample we term "two-month matches," which comprises matched individuals whom we observe for at least three consecutive months. This reduces the observation count by about one-half relative to the single-match sample and eliminates July 2020 observations (our final month of data, for which we cannot check whether a further transition occurred).

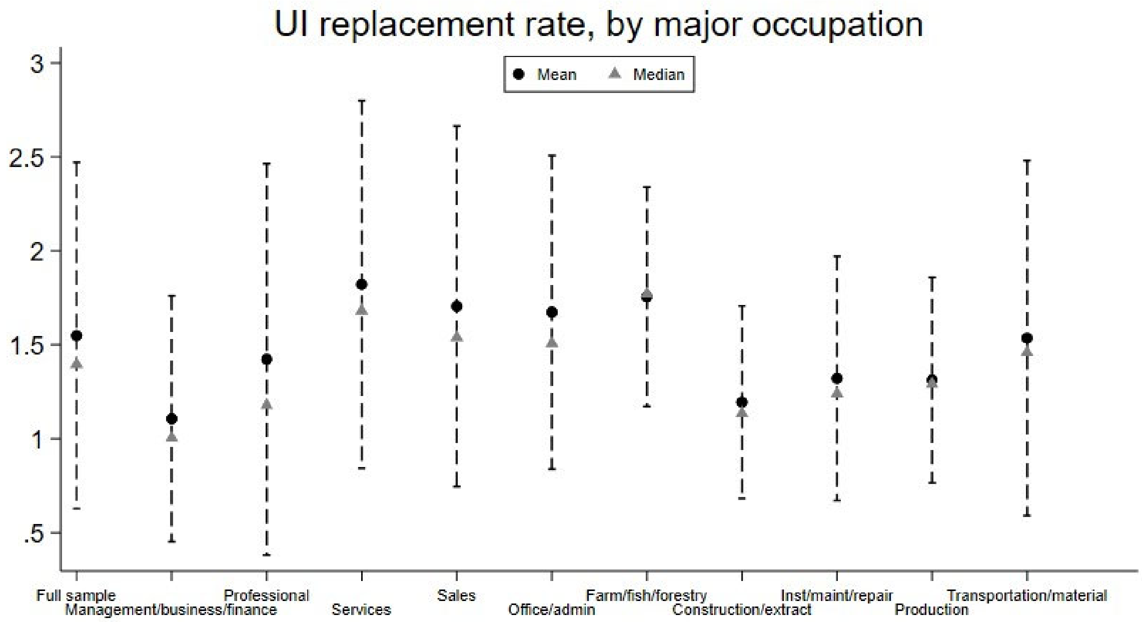
As in PNV (2023), our analysis relies on UI replacement rates—the ratio of weekly UI payments to weekly earnings prior to the job loss resulting in the claim—calculated at the individual level. We rely on the calculator from Ganong et al. (2020) to estimate these replacement rates. The calculator requires individual employment and earnings data from prior quarters, which we obtain from the 2020 ASEC. Because the ASEC does not contain information on the timing of income and employment, we spread annual earnings evenly across all four quarters of 2020 before applying the UI benefits calculator.

One notable feature of the distribution of UI replacement rates is that because normal UI payments generally are determined as a fraction of prior earnings, the uniform $600 supplement increased replacement rates more for individuals with low versus high prior earnings. To quantify this, we divided our sample of unemployed individuals into quintiles based on weekly earnings and calculated their UI replacement rates. The median replacement rate for each group ranged from about 2.5 in the lowest quintile down to about 0.6 in the highest quintile.

This systematic relationship between prior earnings and UI replacement rates raises a concern for our empirical framework. Because early-pandemic job losses were chiefly within low-wage sectors, our estimates based on individual variation in replacement rates may be distorted by sector-specific labor market conditions. PNV (2023) address these concerns in detail and validate their general empirical approach.

As an illustration, Figure 3 (reproduced from PNV) shows the distribution of replacement rates across broad occupation categories. The median and mean replacement rates vary notably across occupations, with higher replacement rates evident in low-wage services occupations. However, replacement rates vary more within than between occupations, as reflected in standard deviation spreads within occupations that generally extend to or beyond the range of means and medians across occupations. This suggests that variation in replacement rates is not closely related to sector-specific effects of the pandemic.

Figure 3:



Note: Calculated from the authors' CPS monthly-ASEC match, using the UI benefits calculator from Ganong et al. (2020). Vertical bars extend one standard deviation above and below the mean.

C. Empirical framework

Our empirical analyses rely on a conventional before/after regression design, essentially a differences-in-differences analysis that yields an estimate of the average treatment effect of variation in UI replacement rates. We include imputed UI replacement rates for individuals in our sample using the procedure described in the preceding section. Our specific regressions take the following form, using a logit model for estimation:

$$Pr(Y_{it} = 1) = \delta R_i + (\pi \times R_i \times (Apr\text{-}Jul)) + \gamma_t + \beta X_{i,t-1} + \lambda Z_{s,t-1} \qquad (1)$$

In this equation, the dependent variable Y_{it} is an indicator for whether an individual *i* transitions between unemployment and employment across consecutive months—i.e., whether an unemployed individual finds a job between months *t-1* and *t*. We report results from our one-month and corrected two-month matches (described in the preceding section). The underlying sample contains observations for transitions observed in the months of February through July of 2020, although the two-month matched estimation samples end in June.

The key explanatory variables are the individual's imputed UI replacement rate (R_i) under the CARES Act and its interaction with an indicator for observations corresponding to the months of April through July 2020, when the CARES Act $600 supplement was available. The replacement rate with the $600 supplement included varies across individuals but not over time and hence is not the key source of variation in this equation. Instead, the treatment effect of the $600 CARES supplement is captured by the impact of the replacement rate after the CARES Act was implemented and the supplemental payments were available. This period began in late March 2020, between the March and April CPS reference periods. These effects are estimated by the coefficient (π) on the interaction between the replacement rate R_i and the indicator for the months of April through July (or June for the two-month matched sample). This represents a conventional before/after estimation approach with regression controls, with the months of February and March combined used as the baseline control period.

The regression also includes an extensive set of other controls. The vector $X_{i,t-1}$ consists of individual characteristics observed in the base (pre-transition) month: age (eight categories), education (five categories), race/ethnicity (five categories), gender by marital status, broad occupation (10 categories) and industry (13 categories) of prior employment, and duration to date of the individual's unemployment spell (10 categories, with the final category indicating duration of longer than one year). The model also includes indicators for calendar months (γ_t) and several state/month labor market controls ($Z_{s,t-1}$): cubics in the state unemployment rate and three-month employment growth rate.[9]

We report the parameter estimates from our logit model as average marginal effects. All estimates are weighted by the longitudinal weights that adjust the sample for the characteristics of the sequentially matched observations. The analysis is restricted to individuals with non-zero estimated UI replacement rates under the CARES Act—i.e., individuals who are identified as eligible to receive UI payments based on their prior earnings history. This is a direct implication of our before/after design, since individuals who are not eligible to receive UI payments do not contribute any identifying variation to the estimation.

D. Results

Tables 2 and 3 present the main results of our regression analyses. Table 2 shows the results for the single-match sample, and Table 3 shows results for the double-match sample that corrects for likely reporting error. The tables are constructed identically, with full sample results in the first column and results by tertiles of household income in columns 2-4.

In both tables, the full sample estimates in column 1 show statistically precise negative effects of UI replacement rates on job-finding during the period when the \$600 supplement was available. This is the main coefficient of interest, which represents the effect of variation in UI replacement rates due to the \$600 benefit supplement (the coefficient π from equation 1). The

[9] Unlike the analysis in Petrosky-Nadeau and Valletta (2023), our regression specification does not include state dummies, which cannot be consistently estimated using our sub-samples defined by household income groups. This omission is inconsequential: the full sample results excluding state dummies are nearly identical to those presented in that earlier paper. Our slightly different regression specification also produces small differences in sample counts.

transformed coefficients reported in the tables indicate a 5 to 7 percentage point reduction in job-finding rates for a 100 percent increase in the UI replacement rate. As discussed in PNV (2023), the magnitude of the coefficients implies moderate effects of UI replacement rates on job-finding, with the size of the implied elasticity of job-finding with respect to the replacement rate toward the low end of past estimates of this elasticity from the relevant literature. The larger and more precisely estimated UI effect from the double-match sample in Table 3 compared with Table 2 shows the effects of the measurement adjustment used for the double-match sample. This likely reflects the impact of increased turbulence in labor market transitions and relaxation of normal UI eligibility rules during the early pandemic period. Under these conditions, the estimation is notably more precise when spurious transitions are reduced in the data (Table 3).

Table 2
Regression results: UI replacement rates and job finding,
by income tertile, CPS single match

	(1) Full sample	(2) 1st tertile (low)	(3) 2nd tertile (middle)	(4) 3rd tertile (high)
UI rep rate	0.024	-0.023	0.068*	0.043
	(0.019)	(0.037)	(0.038)	(0.034)
UI rep*(Apr-Jul)	-0.047**	-0.004	-0.098**	-0.050
	(0.021)	(0.042)	(0.043)	(0.035)
Observations	5459	1826	1867	1766

Note: Logit coefficient estimates transformed to marginal effects, with standard errors in parentheses. See the text for the full list of controls.
* p<.10, ** p<.05, *** p<.01.

Table 3

Regression results: UI replacement rates and job finding, by income tertile, CPS double match

	(1) Full sample	(2) 1st tertile (low)	(3) 2nd tertile (middle)	(4) 3rd tertile (high)
UI rep rate	0.050**	-0.022	0.097**	0.081**
	(0.022)	(0.039)	(0.043)	(0.041)
UI rep*(Apr-Jul)	-0.072***	-0.011	-0.109**	-0.108**
	(0.026)	(0.052)	(0.048)	(0.044)
Observations	2868	970	941	938

Note: Logit coefficient estimates transformed to marginal effects, with standard errors in parentheses. See the text for the full list of controls.
* p<.10, ** p<.05, *** p<.01.

Of particular note in Tables 2 and 3 is the variation in this estimated effect of UI replacement rates across income tertiles in columns 2-4. For both samples, the estimated effect is indistinguishable from zero for the lowest income tertile. This suggests that job-search behavior by individuals from lower-income households is unaffected by the amount of UI benefits available to them, measured relative to their prior earnings. By contrast, the negative and significant coefficients for individuals from middle-income households in Table 2 and both middle-income and higher-income households in Table 3 indicate that they respond to higher UI replacement rates by reducing their job-search intensity or willingness to accept job offers.

Overall, the job-finding regression results suggest moderate disincentive effects of the higher UI replacement rates arising from the $600 pandemic UI supplement. Also, despite the very large increase in replacement rates for lower-income individuals, the reduction in job-finding associated with higher UI benefit payments was larger for individuals from higher-income households. This latter result may seem surprising, given the well-known finding from Chetty (2008) that UI benefit effects on job search outcomes are larger for liquidity-constrained households, which tend to have low rather than high income. However, the Chetty finding is not consistently reproduced. For example, in their analysis of a 1989 UI benefit increase in New York state, Meyer and Mok (2014) find a larger effect on job search for individuals with higher predicted net assets, which is contrary to Chetty (2008) and broadly consistent with our findings by income group. Moreover, other work has found that the job-search response to variation in UI benefits is more muted when labor market conditions are weak (e.g., Kroft and Notowidigdo, 2016). Labor market conditions during the early pandemic period were especially weak for low-wage services workers, whose employment in many cases was directly precluded by limits on in-person contact. This exceptionally weak labor market for low-wage workers may account for the limited disincentive effects of the $600 UI supplement on their job-search outcomes.

IV. Discussion and Conclusions

The COVID-19 pandemic created a massive negative shock to the U.S. labor market, with the effects largely concentrated on lower-wage workers. We find that enhanced UI benefits during the pandemic eliminated the substantial increase in bottom-half income inequality that otherwise would have occurred, reversing the inequality-increasing effects of the pandemic. These findings reinforce and extend past findings about the poverty-reducing effects of UI benefits during economic crises (Larrimore et al., 2023; Rothstein and Valletta, 2017).

We also find moderate disincentive effects of the historically outsized increase in weekly UI benefits on job-finding outcomes. These search disincentive effects appear larger for individuals from households above the bottom third of the income distribution, despite the much higher replacement rate of pre-layoff earnings due to the UI benefits for lower-income individuals. This finding may reflect the exceptionally weak labor market conditions for low-wage workers early in the pandemic.

Overall, these findings suggest large equity effects from the pandemic UI expansions and limited adverse consequences for economic efficiency. Our findings are potentially relevant for UI policy design. Other authors have assessed the possible efficacy of automatic UI duration extensions during economic downturns, based on analysis of policy responses during prior downturns including the Great Recession of 2007-09 (Chodorow-Reich and Coglianese, 2019; Chodorow-Reich et al., 2022). Our results are informative with respect to the design of policies that include automatic increases in weekly benefits, an issue that may merit further consideration in future research.

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