Unemployment Insurance and Job Quality: Evidence from the COVID-19 Pandemic

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April 6, 2022

Abstract

The acute economic downturn caused by the COVID-19 pandemic motivated a broad-based expansion of unemployment insurance (UI) policies. While many previous papers have examined UI's effect on the probability of employment, few have studied its impact on employment quality. In this paper, I use data from the Current Population Survey to analyze the impact of UI benefits on reemployment job quality. I create a novel comprehensive dataset of state UI eligibility and benefit schedules to compute estimated weekly UI benefit receipt between 1996 and 2021. I find that the pandemic-era UI supplements had no significant impact on workers' job quality following an unemployment spell. Separate from temporary UI extensions, I find that standard state UI benefits exhibit no significant relationship with reemployment outcomes prior to 2018. However, this association is positive and statistically meaningful in the more recent period that includes COVID-19. While these results present some suggestive evidence that more generous UI benefits coincide with improvements in job quality, the causal effect of UI benefits in less idiosyncratic economic conditions remains ambiguous, and represents a substantive topic of future research.

*Acknowledgements: First and foremost, I'd like to express my deepest gratitude to my advisor, Professor Giuseppe Moscarini, for his guidance, patience, and encouragement throughout this project and my time at Yale. Thank you to my friends for the joint caffeine excursions, late night work sessions, and consistent moral support. Last and most of all, thank you to my parents, whose unwavering love and support over the past year and throughout my life have fundamentally shaped who I am today. All errors are my own.

1 Introduction

Unemployment insurance (UI) is a social welfare policy that has been the center of contentious debate for decades among policymakers, academics, and the general public. Meant to help workers maintain consumption through periods of unemployment, the policy has shown a robust positive relationship with prolonged unemployment spells. However, both the magnitude of these effects and understandings of their underlying causes vary throughout the literature. The primary interpretations of UI's positive relationship with unemployment duration include: 1) UI increases reservation wages and generates moral hazard, reducing job search effort (Rothstein, 2011; Farber and Valletta, 2015; Farber et al., 2015), and 2) UI benefits allow workers to search for better jobs (Acemoglu and Shimer, 1999; Marimon and Zilibotti, 1999; Chetty, 2008; Card et al., 2007). It remains up for debate as to which of these forces outweighs the other.

The COVID-19 pandemic's unprecedented public health environment and resulting steep economic downturn invoked a broad-stroked response from policymakers to support those who lost their jobs. In March 2020, US legislators passed the Coronavirus Aid, Relief, and Economic Stimulus (CARES) Act, which instituted two new programs for unemployed workers. The Federal Pandemic Unemployment Compensation (FPUC) program provided an additional $600 weekly supplement of unemployment benefits on top of standard state benefits, and the Pandemic Unemployment Assistance (PUA) program extended UI eligibility to workers who were traditionally ineligible (e.g., self-employed, independent contractors, gig economy workers) or had lost their job due to specific COVID-19-related reasons. As the FPUC in particular was applied to all UI-eligible unemployed workers regardless of individual characteristics, these policies and their implications have been of significant interest for news media and academic literature. Optimal UI policy (Mitman and Rabinovich, 2021), vacancy postings (Marinescu et al., 2021), and employment outcomes resulting from both adding (Scott et al., 2020) and removing (Holzer et al., 2021) these UI extensions constitute just a small subset of topics addressed in recent studies.

The inherent difficulty of measuring the effect of UI policy on unemployment lies in issues of endogeneity — there may be a variety of unobserved factors that determine both the worker's level of UI and her employment outcomes. Some papers attempt to circumvent these challenges by measuring job search effort directly through time spent (Krueger and Mueller, 2010; Mukoyama et al., 2018). Others instead focus on the effect of UI on worker-job match quality; Farooq et al. (2020) find that UI duration extensions during the Great Recession improved the quality of worker-job matches, measured in terms of the degree of positive assortative matching and mismatch between worker education levels and job skill

requirements.

Few papers, however, use micro-level estimates of unemployment benefits in order to study their effects on the labor market, and given the complexity of state UI policies, this is somewhat unsurprising. Many simply use maximum benefit amounts or maximum duration of benefits by state as a proxy for UI received. One exception includes Ganong et al. (2020); they consolidate 2020 state-level UI eligibility criteria and benefit calculations to develop a UI benefits calculator. The calculator estimates an individual's expected weekly benefits using their four-quarter earnings history. They find that between April and July 2020, pandemic-era UI supplements resulted in over three-fourths of UI-eligible workers receiving statutory replacement rates over 100%, with a median statutory replacement rate of 145%.

Building on this extensive literature, this paper examines the impact of weekly UI benefits on reemployment job quality. Using micro-data on demographics, labor market outcomes, and earnings from the Current Population Survey (CPS) and its Annual Social and Economic Supplements (ASEC), I isolate workers who experience transitions from employment, to unemployment, and back to employment (EUE spells for brevity) during their CPS survey period to study their changes in job quality before and after the unemployment spell. I measure this job quality as the inter-industry and inter-occupation wage differentials that a worker receives by finding employment in a specific industry and occupation.

I extend the UI benefits calculator created by Ganong et al. (2020) to compute estimated weekly benefits for the period 1996–2021. The calculator uses a worker's four quarter earnings history in order to compute their UI benefit amounts; I follow the authors' methodology and simulate four quarter earnings using estimates of weekly earnings and the number of weeks worked. However, the CPS collects weekly earnings data for at most a quarter of the respondents in a given month. Thus, I also outline a process of imputing the missing values for weekly earnings and the number of weeks worked, in order to generate estimates of UI benefit receipt.

I use these estimates of job quality and UI benefits to examine the effect of UI on reemployment quality. I do not find evidence that the $600 (and later $300) weekly UI supplements implemented during COVID-19 had any significant impacts on workers' post-unemployment job quality. When examining standard state UI benefits not including temporary expansions, the results in years prior to COVID diverge from those that include the pandemic. Namely, UI benefits exhibit no statistically meaningful relationship with changes in job quality between 1996 and 2017. On the other hand, reemployment outcomes have a positive and significant association with UI benefits from 2018 onwards. These results suggest that UI benefit amounts share a positive relationship with labor market outcomes following unemployment, though this relationship is substantially caveated with challenges

in identification.

This paper makes two primary contributions to the vast literature around UI: first, to my knowledge, this is the first study to use individual-level estimates of weekly UI benefits at a monthly frequency that span the modern CPS. Second, this study diverges from much of the UI literature, as it focuses on the quality of employment; a large proportion of the literature (as discussed above) has examined the effects of UI on the probability of employment, with a wide range of conclusions. In studying reemployment quality, this paper helps to shed light on other critical dimensions of UI policy.

There are two noteworthy caveats to mention at this stage. First, as is the case in a majority of UI literature, I do not measure actual UI receipt, and to my knowledge, no such data currently exists. Thus, these results represent an intent-to-treat effect, rather than a treatment effect on the treated. However, by determining eligibility through detailed state requirements rather than simply separating job losers from job leavers, this analysis more precisely estimates the intent-to-treat sample. Nevertheless, given the complex and arduous unemployment claims process, it is possible that many unemployed workers do not receive the full benefits they are eligible for. Indeed, Anderson and Meyer (1997) find that 40-50% of eligible unemployed workers actually receive UI, while Rothstein (2011) finds this number increases slightly to over half in 2010.

Second, I measure UI through estimated weekly benefit amounts, but do not take into account the duration of these benefits. UI duration or remaining weeks of UI in the literature has typically been measured as the difference between the maximum weeks of UI in the state and the number of weeks the worker was unemployed, as reported in the CPS. This measure certainly has its own limitations; unemployment durations in the CPS are self-reported, and the line between unemployment and non-participation in the labor force can often be blurry. Even after the redesign of the survey in 1994, misclassification of unemployed individuals as out of the labor force remains an important issue (Rothstein, 2011). Despite these measurement challenges, benefit duration is by no means a trivial consideration for unemployed workers: Krueger and Mueller (2010) find worker behavior varies significantly with benefit duration, as unemployed workers increase job search intensity prior to benefit exhaustion. In the context of the Great Recession, Farooq et al. (2020) find UI duration extensions improved worker-job match quality. As the CARES Act also included a 49-week extension of UI benefits for those who had exhausted standard UI, an analysis utilizing both micro-level UI benefit amounts and UI durations, during the pandemic and more broadly, could constitute an area of potential future research.

The remainder of the paper proceeds as follows: Section 2 provides some background on the institutional features of unemployment insurance, as well as an outline of the temporary

UI expansions exacted in response to COVID-19, and Section 3 describes the data sources used for this project. Section 4 describes the sample of interest for my study, and outlines the methods used to impute missing data and quantify job quality. Section 5 presents the results, and Section 6 concludes.

2 Institutional Background

2.1 Unemployment Insurance Eligibility and Benefits

Unemployment insurance (UI) in the US is a joint state-federal program in which the federal government sets minimum benefits and standards, and states are free to offer further support. As the specifics of UI policy are set by each state, eligibility, benefit durations, and weekly benefit amounts vary among the 50 states and the District of Columbia, and are also subject to change over time. At a minimum, under federal law, workers must have received a minimum level of earnings during a specific reference period, and must not have voluntarily quit their job without good cause in order to qualify for benefits.[1] Most states also require active job search, such as a minimum number of employer contacts or job applications each week. However, monitoring is not particularly strict, as most states rely on postal or phone reports to enforce these requirements (Anderson, 2001). Workers can lose eligibility once they receive a "suitable offer of employment" (including requests that a furloughed worker return to his or her job), even if they reject the offer. Most state benefit formulas replace half of a claimant's average weekly wage up to a weekly maximum, and benefits are available for up to 26 weeks in most states. In August 2019, approximately 1.6 million unemployed workers received UI, with an average weekly benefit amount of $364 (Whittaker and Isaacs, 2019).

The UI system's two primary goals are to provide temporary and partial wage replacement to involuntarily unemployed workers, and to stabilize the economy during recessions. In pursuit of the latter, federal law includes the permanently authorized Extended Benefits (EB) program, which triggers an availability of up to an additional 13 to 20 weeks of standard UI benefits in periods of high unemployment and recession. Congress has also supplemented these permanently authorized programs by enacting temporary UI expansions, doing so nine times since the creation of the UI program in 1935, and most recently in response to the major disruption caused by the COVID-19 pandemic (Whittaker and Isaacs, 2022).

[1] Further details on UI programs, benefits, and eligibility can be found here: `https://crsreports.congress.gov/product/pdf/RL/RL33362`

2.2 COVID-19 Unemployment Insurance Policies

The Coronavirus Aid, Relief, and Economic Stimulus (CARES) Act passed in March 2020 provided significant expansion of UI eligibility and benefits, instituting a number of new programs including: Pandemic Unemployment Assistance (PUA), Federal Pandemic Unemployment Compensation (FPUC), and Pandemic Emergency Unemployment Compensation (PEUC). The PUA program extended UI eligibility to many self-employed and "gig" workers who were traditionally ineligible for unemployment insurance; FPUC augmented standard state benefits by $600 per week for eligible unemployed workers as well as those eligible under PUA; and PEUC extended benefit durations by an additional 49 weeks for those who had exhausted regular unemployment compensation.

The CARES Act programs were initially set to expire on July 31, 2020. Just a week later on August 8, the Trump administration created the Lost Wages Assistance program via executive order, which provided $300 weekly grants to supplement standard UI benefits. The program, however, was administered haphazardly (Whittaker and Isaacs, 2022; Holzer et al., 2021), and it wasn't until the passage of the Continued Assistance Act in December 2020 that the CARES Act programs were officially reauthorized through March 14, 2021, now at a lower amount of $300 per week.

In March 2021, the Biden administration signed the American Rescue Plan, which further extended the PUA, FPUC (at $300 per week), and PEUC programs through September 4, 2021. However, citing work disincentive effects, decreased state unemployment rates, an end to previous barriers of employment (e.g., industry shutdowns, limited childcare facilities), and job openings equivalent to the number of unemployed, 26 states opted out of some or all of the temporary COVID-19 UI programs prior to their official termination date (Whittaker and Isaacs, 2021).

Despite this substantial expansion of UI, many traditional UI eligibility requirements remained in place. Barring those with exceptional circumstances related to COVID-19 (e.g., having a respiratory condition, caring for an elderly relative, etc.), workers who voluntarily quit their job, even for general concerns regarding COVID-19, were ineligible for regular UI benefits and its CARES Act supplements. Furthermore, those that received suitable offers of employment, regardless of whether they accepted the offer, were ineligible.

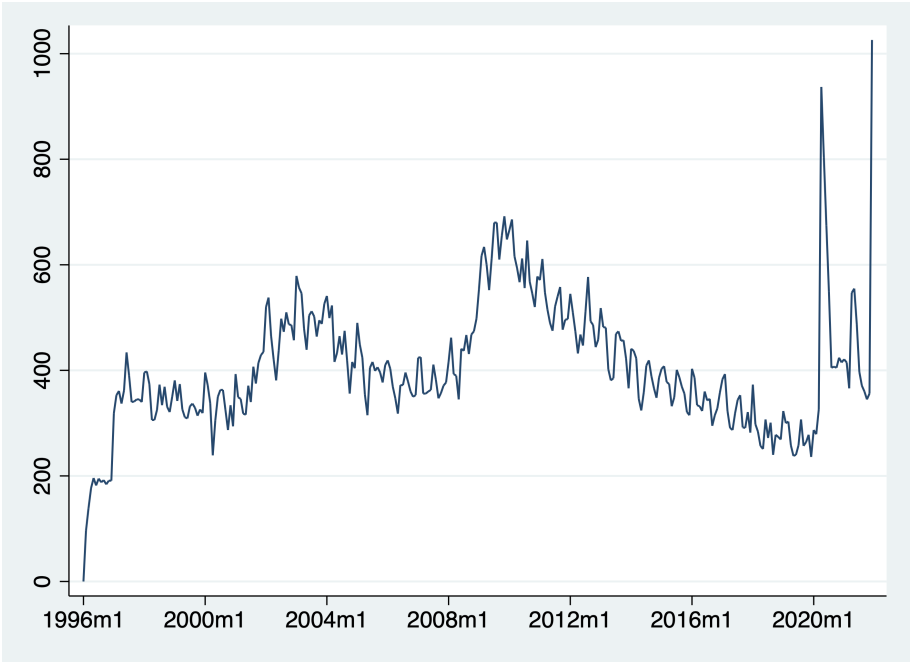
3 Data

3.1 Current Population Survey (CPS)

The data used in this paper is primarily constructed from the Current Population Survey (CPS). The CPS is a comprehensive monthly survey of approximately 60,000 households conducted by the Bureau of the Census for the Bureau of Labor Statistics, and collects a wide range of information about respondents' demographics, labor force status, and earnings. The survey divides the full sample of households into eight "Rotation Groups" in which each household is interviewed for four consecutive months, removed from the sample for eight months, and finally included again for an additional four months. As is well documented in the literature, it is impossible to match households across monthly files between May 1995 and August 1995; I thus focus on post-1995 data. From 1996 onwards, the survey uniquely identifies households and individuals, and thus the panel structure of the survey can be used to follow individuals and their employment trajectories over short periods of time. However, given the limited longitudinal dimension of the CPS (following respondents for a period of 16 months as opposed to decades for some panel surveys), the survey is not primarily intended for longitudinal analysis.

In the case of this paper, which studies spells of employment to unemployment and back to employment (or EUE spells for brevity), the CPS's narrow survey period can be a non-trivial constraint. For example, if a respondent enters the survey while employed, loses his job at some point during the eight months he is surveyed, and fails to find a job prior to the end of his sampling period, we are unable to observe whether he finds a new job, and if so, his reemployment job quality. Figure 1 illustrates the severity of this limitation for the CPS between January 1996 and December 2021. The frequency of these incomplete EUE spells is fairly large (see Table 4 for the number of spells in the final sample), spiking during the COVID-19 pandemic. As the novel pandemic unemployment insurance policies are a key feature of this paper, this substantial reduction in sample size imposes a significant constraint on my analysis. However, to my knowledge, there do not exist other datasets that track respondents for periods longer than the CPS while also maintaining a monthly survey frequency. Other potentially more innovative methods of projecting employment outcomes past the conclusion of respondents' sample periods remains out of the scope of this study.

Figure 1: Number of EUE Spells Lost due to
the Conclusion of the CPS Survey Period, 1996–2022



Source: CPS, January 1996 — December 2021. Incomplete EUE spells (i.e., EU spells), are counted based on the individual's last (i.e., most recent) month in the survey. Thus, as the data begins in 1996, the number of incomplete spells at the beginning of the period is relatively fewer; the analogous opposite reasoning applies to the end of the period in December 2021. I define an employment to unemployment transition (or EU transition) as a month of employment, followed by a month of unemployment, allowing for months out of the labor force, months with missing data, or months with temporary absence or survey response refusal in between.

3.1.1 Matching CPS Files Across Time

A noteworthy aspect of the CPS is its sampling of addresses, rather than individuals or families. As a result, for a given month, attrition from the survey can occur due to: temporary absence (hospitalization, vacation), mortality, or migration (going to college or the military, family- or work-related reasons). In cases where a household prematurely exits the sample, the Census Bureau continues to survey the new respondents that move into the address until the conclusion of its 16-month survey period. In cross-sectional studies that examine questions of aggregate employment, as long as those with missing data due to attrition do not systematically differ from those that do in aggregate (Rothstein (2011) finds this to be the case), this attrition should not pose issues of biased estimates.

However, when measuring individual flows over time, this sample selection can be much more delicate. For example, if an individual enters unemployment during the survey and

migrates or changes her address before finding another job, we are unable to observe her future employment outcomes. Thus, the rates of attrition (from migration especially) across months in the survey are of particular importance for any longitudinal study; the higher these rates, the smaller the usable sample size, and the more likely the remaining sample is biased. Figure 2 shows the rates of migration attrition for the CPS from the start of 1996 to the end of 2021. Three features are worth noting. First, failure rates of matching individuals over time are overall fairly low, remaining less than 3% during most of the sample period. Second, migration attrition between survey months four and five are consistently higher than other months, as one would expect given the eight month hiatus between these survey months. Furthermore, the CPS is required to conduct a personal visit for the fifth-month interview (as is the case for the first-month) and may attempt a telephone interview after this attempt, provided the original household still occupies the sample unit,[2] contributing to the greater failure rate. Lastly, these attrition rates jump in early to mid-2019 and 2020, with failure rates for earlier survey months (1 to 2, 2 to 3, and 3 to 4) rising in mid-2021. Without attributing an explicit cause to these matching failures,[3] this attrition ultimately reduces the potential sample size for my analysis. The fact that these rates are highest during COVID-19 is indicative of how tricky of a shock the pandemic can be to study, despite its attractive economic exogeneity.

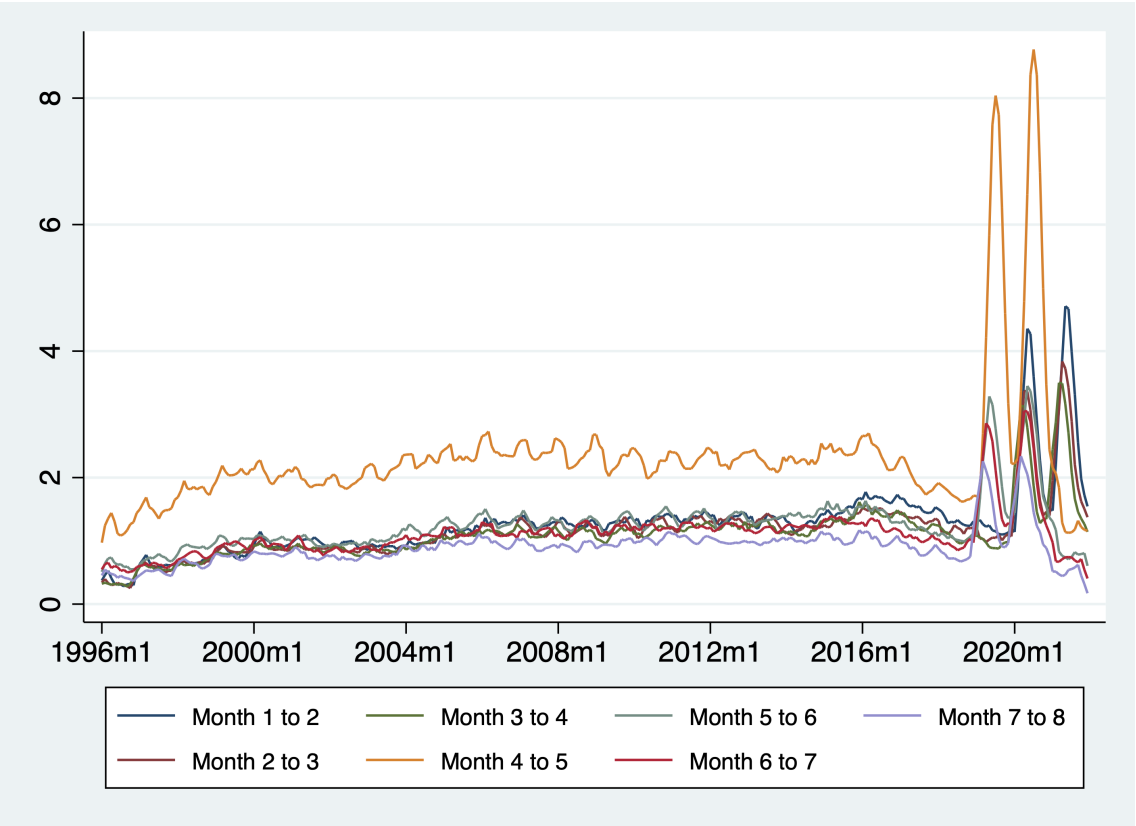
As EUE spells are my primary subject of inquiry, more relevant than instances of migration attrition overall are cases of incomplete EUE spells from migration. I focus on migration as opposed to other forms of attrition (e.g., temporary absence) as it poses similar challenges as the conclusion of the CPS survey period (see Figure 1): we are unable to observe individuals' employment outcomes after they have moved out of the surveyed address. Figure 3 shows these cases from 1996 to 2022. The number of these cases is fairly small but not insignificant, as they constitute almost 10% of the size of the final sample of EUE spells (see Table 4). And as was the case with migration attrition in general, the number of incomplete EUE spells jumps during the pandemic, stemming from a variety of potential reasons. Again, within the scope of this paper, there is little I can do to overcome these data constraints, but one must remain aware of these limitations when interpreting any final estimates.

Independent of drawbacks related to sample size, the main concern regarding EUE spells is the possibility that reemployment outcomes are correlated with instances of migration attrition. It is conceivable that the population more likely not to complete the survey have demographic characteristics, and thus probabilities of EUE transitions, distinct from those

[2]See `https://www.census.gov/programs-surveys/cps/technical-documentation/methodology/collecting-data.html` for more details on the interview process of the CPS.

[3]A whole host of potential causes could contribute — mortality from COVID-19, migration as vaccines were rolled out and the economy began to reopen, among others.

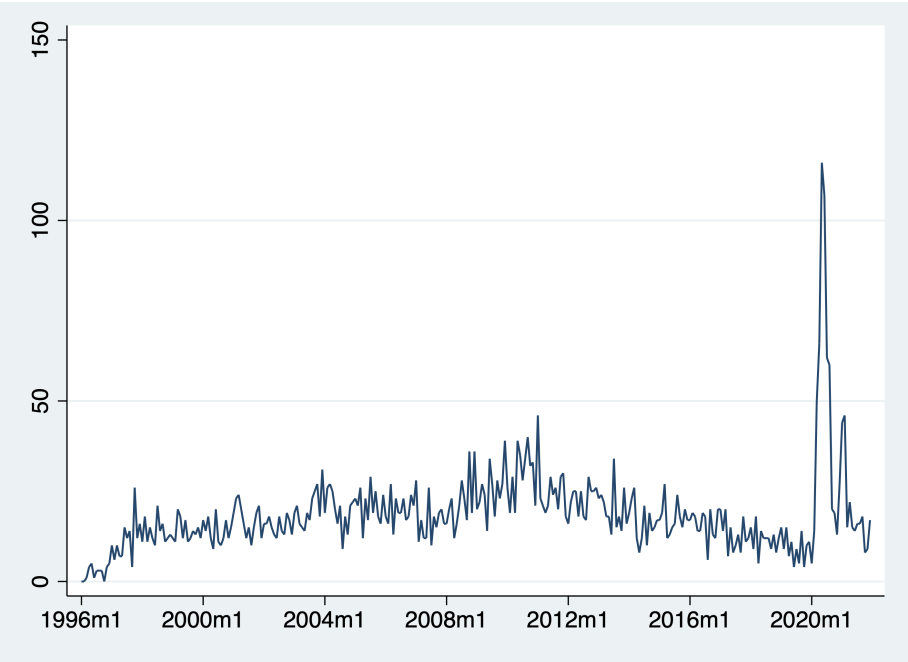
Figure 2: Proportion of Individuals that Fail to Match across Months in Sample (Migration), 1996–2022



To calculate the time series above, for each calendar month, I find the ratio of individuals who have survey data in both survey month i and $i+1$ to the total number of individuals who have data in survey month i. I define someone as "having survey data" in a given month as anyone with an `HUFINAL` code not equal to 214, 216, or 219. These codes correspond to `"UNABLE TO LOCATE"`, `"NO ONE HOME"`, and `"OTHER OCCUPIED - SPECIFY"`, respectively, and constitute my definition of attrition due to migration/a change in address. If a surveyed address experienced migration attrition during the survey, that address remains included in this calculation in later survey months — e.g., if there was migration attrition in survey month 4, and the new individual/household that moved in was present for months 5 and 6 of the survey, I consider this as a successful match across months 5 to 6.

who are present for the entire survey period. For example, young, unmarried, low-income individuals may be more mobile and thus less likely to remain in the surveyed unit for the entirety of their survey period. The relationship between reemployment job quality and attrition, however, is not as immediate. While attrition of low-income workers could include relocating in search of higher paying jobs, higher earning spouses of high-income families may take lower paying jobs in order to relocate along with their partners. Or the opposite may be the case if lower income workers churn between low-quality jobs. Given time constraints, this analysis is not included in this paper, and further exploration of this potential issue

Figure 3: Number of EUE Spells Lost due to Migration Attrition, 1996–2022



Source: CPS, January 1996 — December 2021. Incomplete EUE spells (i.e., EU spells), are counted at the month of migration attrition, which I define as an `HUFINAL` code of 214 – `"UNABLE TO LOCATE"`, 216 – `"NO ONE HOME"`, or 219 – `"OTHER OCCUPIED - SPECIFY"`.

is left to future research. I move forward with the assumption that changes in job quality following an unemployment spell do not correlate to the likelihood of migration attrition.

3.1.2 Changes to Industry and Occupation Coding Over Time

As is discussed in Section 4.3, my definition of job quality prior to and following a worker's unemployment spell centrally relies on the major industry and occupation codings in the monthly CPS. These codings, however, have changed over time, making some historical comparisons difficult or in some cases not possible. Relevant for my time period of interest, major industry and occupation codes in the CPS changed 2003. These changes reflect the evolution of the Standard Occupational Classification (SOC) system and the North American Industrial Classification System (NAICS) (from which the census's occupation and industry classifications, respectively, are derived from) over time. As the composition of detailed and major occupations and industries varied substantially between older and updated classification systems, the Census Bureau provides "crosswalks" to link detailed codes across these systems, and the BLS provides detailed documentation as to how to

implement these crosswalks and revise historical data when possible.[4] Given the complexity of these changes and time constraints, I manually recode the major industry and occupation codes in best attempts to make the data consistent over my entire timeframe (1996–2021). The recoding scheme is outlined in Tables A.1 and A.2.

3.2 Annual Social and Economic Supplements (ASEC)

States use previous four quarter earnings in order to determine eligibility and benefit amounts for unemployment insurance. Following the methodology used by Ganong et al. (2020), I simulate quarterly earnings history (see Section 3.3 for more details) using weekly earnings from the CPS and the number of weeks worked in the past year from the Annual Social and Economic Supplements (ASEC) of the CPS. The ASEC, also referred to as the March supplement of the CPS, is conducted in March each year by the Bureau of the Census for the Bureau of Labor Statistics, and contains basic demographic and labor force information included in the monthly CPS, in addition to data on work experience, income, noncash benefits, health insurance coverage, and migration. Unlike the monthly CPS, questions on employment and income in the ASEC refer to the previous calendar year, particularly useful for purposes of computing UI benefits.

3.2.1 Matching ASEC to Monthly CPS

The ASEC surveys over 75,000 households each year, including all households surveyed in the March monthly CPS as well as an oversampling of individuals from other months of the CPS. In order to match these data with the monthly CPS files, I follow the standard practice of using the ASEC's unique individual identifier `PERIDNUM`. This identifier, however, is included in the CPS only from 2005 and onwards. For years 1998 to 2004,[5] I follow the procedure outlined in the ASEC technical documentation. However, the identifiers provided for these years' ASEC files do not uniquely identify individuals; thus, I also match observations on gender, race, age, and education level, in addition to the documented identifiers, and allow for age and education to increase by one (or one level in the case of education). Even with these additional demographics, however, some duplicates within the ASEC file remained. As these comprised of less than 1% of matches with the monthly CPS files, these observations were dropped (see Section 3.2.1 for further discussion of imputing missing values).

The results of this matching procedure are found in Table **??** below. The table presents the full distribution of individuals in the CPS from 1998 to 2022,[6] and their status in terms of

[4] However, I was unable to find documentation on revising major, rather than detailed, codes.

[5] ASEC files for 1996 and 1997 are not published on the Census website.

[6] I exclude 1996 and 1997 from these results as the 1996 and 1997 ASEC files are not published on the

whether they have ASEC Data (`haswkswork`) and whether they were present in the March monthly CPS (`present_asec`). The two quantities of interest have been highlighted. In principle, we should expect one-third of the CPS sample to have ASEC data, as only rotation groups that begin in December, January, February, and March are surveyed in the March CPS. We see that this proportion is slightly higher than one-third (35.7%). And on the other hand, should the matching be perfect, we would expect that among instances of missing ASEC data, none should arise from respondents present for the March CPS. Table 1 illustrates that such cases are rare. Figures 4a and 4b provide a breakdown of these quantities by year. As expected, the proportion of matches for pre-2005 ASEC data is inferior to that of future years, but much of this seems to stem from a lack of matches in oversampled households, rather than missing those surveyed in March. To note, the prominence of missing values jumps in 2014 because the Census Bureau published two separate versions of the 2014 annual ASEC file, a "traditional" and a "revised" version. I use the traditional version, as the revised file results in poorer match quality (i.e., more missing data).

Table 1: Distribution of Missing ASEC Data and Inclusion in the ASEC Survey

	present_asec								
haswkswork	**0**			**1**			**Total**		
	Freq.	Row %	Col %	Freq.	Row %	Col %	Freq.	Row %	Col %
0	6,289,533	99.4	84.3	38,216	0.6	1.6	6,327,749	100.0	64.3
1	1,167,535	33.3	15.7	2,342,839	66.7	98.4	3,510,374	100.0	35.7
Total	7,457,068	75.8	100.0	2,381,055	24.2	100.0	9,838,123	100.0	100.0

Source: CPS and ASEC, 1998 – 2022. I classify individuals with `HUFINAL` codes: 1, 2, 3, 4, 5, 6, 201, 203, 204, and 205 in March as present for the March monthly CPS, and thus eligible to be surveyed for the ASEC.

These details regarding individuals' missing data and their composition, though ostensibly fastidious, prove to be critical for my analysis, for without ASEC data, I am unable to calculate UI benefits. Understanding the nature of this missing data (completely at random, at random, not at random) helps in selecting the appropriate imputation methods, and preventing a substantial reduction in sample size or, more importantly, a biased final sample.

Census Bureau website and were difficult to locate otherwise (though one could consider using the IPUMS versions of these files instead, which publishes annual ASEC files starting from 1962). As expected, the proportion of missing data is near 100% in both years.

Figure 4: Missing ASEC Data, 1998–2022

(a) Proportion of Missing ASEC Data

(b) Proportion of Missing ASEC Data in March CPS

The proportion of missing values jumps in 2014 as the Census Bureau published two separate versions of the 2014 annual ASEC file, a "traditional" and a "revised". Here, I use the traditional version – the revised file results in greater instance of missing data.

3.3 Unemployment Insurance Benefits

To calculate unemployment insurance (UI) benefits, I extend the UI benefit calculator developed by Ganong et al. (2020). Following the authors' methodology, I reference the "Significant Provisions of State Unemployment Insurance Laws" documents published semiannually by the Department of Labor (U.S. Department of Labor, 2021b) to construct estimates of UI received. As one of the main contributions of this paper, I consolidate precise state-level eligibility criteria and benefit schedules over a 26-year timeframe. For more details on the calculator and the construction of this dataset, see Appendix A.2.

Most states compute benefits as a percentage of a worker's highest quarter earnings, second-highest quarter earnings, or annual earnings in the four most recent calendar quarters, subject to maximum and minimum benefit amounts. I estimate a worker's earning history using the weekly wage as reported in the monthly CPS and the number of weeks worked in the previous calendar year from the ASEC. Following Ganong et al. (2020), I assume that: 1) workers earned the same weekly wage for all weeks they reported working in the ASEC, 2) the weeks worked were consecutive (i.e., uninterrupted), and 3) all workers worked in the final week of the calendar year prior to the March ASEC in which they were surveyed. This methodology makes the most recent quarter that with the highest earnings.

A noteworthy caveat of UI policy is that the state rules used to determine a worker's eligibility and weekly benefits depend on the location of her workplace, not her residence. As the CPS only includes data on the state of the surveyed unit, in my analysis I simply

assume that all workers work in the same state that they reside in. Future research could explore incorporating commuting data or more innovative methods involving distances to state borders to generate more refined estimates of locations of employment.

3.4 Other Data Sources

I reference three additional datasets to supplement my analysis: 1) COVID-19 caseloads and deaths, 2) state unemployment rates, and 3) starting and ending dates of COVID-19 UI benefit extensions. I use the cumulative and monthly totals of COVID-19 cases and deaths by states published by the CDC (Centers for Disease Control and Prevention, 2021) in order to control for idiosyncratic aspects of the pandemic period that may influence people's job search and job taking behavior.

In less exceptional periods (compared to COVID-19) of economic downturn, state UI policies include automatic triggers that extend UI durations for an additional 13 to 20 weeks during periods of high state unemployment (as mentioned in Section 2.1). I collect these monthly state unemployment rates from the Local Area Unemployment Statistics (LAUS) at the Bureau of Labor Statistics. As worker-firm matching may be impacted by weak economic conditions, state unemployment rates can help lessen identification issues, but also the thresholds that trigger UI extensions vary across states. Therefore, unemployment rates by state can be used to proxy exogenous (at least at the worker level) variation in UI benefits between different states.

Lastly, data on the timing of UI benefit supplements and extensions are gathered from the Congressional Research Service (Whittaker and Isaacs, 2021). CPS surveys are conducted during the calendar week which includes the 19th of the month[7], and questions refer to labor market activities of the prior week (the week that includes the 12th). I use the date of the Friday in the week (Sunday - Saturday) of the 12th to determine whether the pandemic UI extensions were in place during the period in which the survey questions refer.

4 Sampling and Empirical Methodology

4.1 Sampling

As discussed above, the population of interest for this paper consist of workers who experience EUE spells during their CPS survey period. I define an EUE spell as a month of employment, followed by a month of unemployment, and finally bookended by another

[7] https://www.census.gov/programs-surveys/cps/technical-documentation/methodology.html

month of employment. I allow for months out of the labor force, months of temporary absence or refusal of response, and months of missing data[8] to occur at any point between the two months of employment, as long as the respondent also experiences a month of unemployment during this time period. I also include EUE spells that occur over the eight-month hiatus between survey months 4 and 5 (though I distinguish these spells from those that occur within the two four-consecutive-month survey blocks of each individual,[9] given that it is much more challenging to track employment outcomes during the eight month gap of the survey). This methodology also allows for individuals with multiple EUE spells within the CPS survey period.

This definition of EUE spells is subject to the concerns discussed in Holzer et al. (2021): mismeasurement of labor force status may lead to spurious transitions out of unemployment. Holzer et al. (2021) thus implements a recoding procedure also used in Rothstein (2011) and Farber et al. (2015) which considers one-month transitions out of unemployment as spurious. More specifically, U-E-U and U-N-U (N being nonparticipation in the labor force) spells are recoded to be three months of unemployment. Though these concerns are warranted, I move forward with the EUE definition described above; doing otherwise results in a prohibitively small final sample.

The results of isolating EUE spells are outlined in Table 2 below. The monthly CPS from 1996 to the end of 2021 consists of over 43 million observations. From this initial sample, I remove all cases of migration attrition (as described in Section 3.1.1, migration attrition is defined by `HUFINAL` codes `214 - "UNABLE TO LOCATE"`, `216 - "NO ONE HOME"`, and `219 - "OTHER OCCUPIED - SPECIFY"`, and only keep observations that precede the month of attrition. More specifically, if an individual was surveyed for survey months 1 through 3, and moved out of the survey unit in month 4, even if a new resident moves in and provides responses for months 5 through 8, I discard all observations in month 4 onwards.

After isolating individuals that experience at least one complete EUE spell, the sample reduces to approximately 800 thousand observations, less than 2% of the initial sample. This again speaks to the limited longitudinal nature of the CPS.

I then remove workers that were temporarily laid off as well as voluntary job leavers (identified by `PEMLR` code `3 - UNEMPLOYED - ON LAYOFF` and `PRUNTYPE` code `4 - JOB LEAVER`, respectively) from the sample. The distributions of these individuals are presented in Tables 3a and 3b.

[8] e.g., a student returns to school and is not surveyed as part of the household, the individual identifier is entirely absent from the monthly CPS file, etc.

[9] Namely, survey months 1 – 4 and months 5 – 8

Table 2: Construction of Final EUE Sample

	Number of Observations	Observations Remaining	Individuals Remaining	Sample Remaining
Total Observations	43,692,128			100%
Attrition (Moved)	2,069,936	41,622,192	5,461,516	95.26%
Non-Employment at Start or End of Survey Period*	22,115,689	19,506,503	3,698,281	44.65%
Did not experience EUE transition	18,726,915	779,588	117,327	1.78%
Temporary Layoffs	306,260	473,328	71,905	1.08%
Job Leavers	85,160	388,168	59,035	1.08%
Final Sample		388,168	59,035	0.89%

* No-Employment includes unemployment, not in labor force, missing data, and blank responses (refusals, absences, etc.) – results in a dataset that starts and ends with a month of employment.

Table 3

(a) Temporary Layoffs

	Freq.	Percent
Other	71,905	61.29
Temporarily Laid Off	45,422	38.71
Total	117,327	100.00

(b) Job Leavers

	Freq.	Percent
Other	104,110	88.73
Job Leaver	13,217	11.27
Total	117,327	100.00

Source: Respondents in the CPS, January 1996 — December 2021, with at least one EUE spell. Frequencies displayed represent four-month rotations rather than individuals.

Table 3a illustrates that, as expected, a large portion of workers with short EUE spells are temporarily laid off, rather than permanently separated from their employer. Given that the outcome of interest for this study are changes in job quality following an unemployment spell, I remove workers who are temporarily laid off as they frequently return to their previous jobs, implying a change of job quality of zero. Although the high prevalence of temporary layoffs during the pandemic may encourage strong considerations to include these workers, workers on temporary layoffs may have differing incentives and behavior than regular unemployed

workers. It is reasonable to conceive that having the guarantee of eventually returning to his previous job (assuming the company remained in business) substantially influences a worker's job search behavior and response to UI benefits, consequently biasing any analyses of non-temporary unemployed workers. On the other hand, the employment outcomes of temporarily laid off workers, especially during COVID-19, may be of interest — this work is left to future research.

I also remove voluntary job leavers (i.e. quitters) because, as discussed in Section 2, these workers are ineligible for both standard UI benefits as well as the COVID-19 UI supplements. Among the UI extensions exacted during 2020, the PUA expanded UI eligibility to many workers who are traditionally ineligible for UI — self-employed workers, "gig" economy workers, etc. However, I do not account for UI eligibility based on characteristics of prior employment in my analysis; my only method of determining eligibility (apart from the state thresholds incorporated in the UI benefits calculator) is whether or not the worker voluntarily left her last job, both during COVID-19 and periods separate from the pandemic. Though I fail to capture all aspects of the UI expansions enacted in response to COVID-19, I may simultaneously avoid potential challenges related to heterogeneous unemployed populations. For instance, it is plausible that workers who became eligible for UI under the PUA are lower-wage workers and experience high rates of job turnover, churning between low wage sectors. If these workers exhibit substantially lower job stability or are significantly different than UI-eligible workers prior to COVID-19, impetuously including them in analyses of the unemployed prior to and during COVID-19 would be problematic.

This filtering results in a final sample of just less than 60 thousand individuals, less than one percent of the initial dataset. The final dataset consists of 60,528 total EUE spells, and the distribution of the number of EUE spells by individual are shown in Table 4 below. As expected, the number of individuals with more than one EUE spell is small, accounting for less than 3% of the final sample.

Table 4: Distribution of EUE Spells per Individual in the Final Sample

	Freq.	Percent
1	57,563	97.51
2	1,451	2.46
3	21	0.04
Total	59,035	100.00

4.2 Imputation

An important aspect of the final sample of EUE spells used for this analysis is there are no restrictions placed on when the EUE spell takes place within the eight survey months. Separately, as discussed in Section 3.3, data on weekly earnings and the number of weeks worked are necessary to compute weekly UI benefits. Together, these two factors imply that: 1) missing earnings and weeks worked data in the final sample will be prevalent, and 2) properly imputing these missing values will be crucial in order to accurately estimate UI benefits.

4.2.1 Missing Data

Even without any filtering, at most only a quarter of the CPS contains weekly earnings data; questions regarding wages and earnings are asked in only months 4 and 8 of the survey. However, instances of attrition, non-employment, and response refusal result in an availability of earnings data that is substantially lower than 25% in practice. With the added requirement of experiencing at least one EUE spell, the instance of missing earnings data in the final sample is upwards of 80%. While one could instead impose the restriction that workers must be employed in months 4 and/or 8 in order to reduce the prevalence of missing data, this conversely dramatically reduces the size of the final sample. In terms of missing weeks worked data, the final sample does not exhibit as extreme discrepancy with the initial sample as does missing earnings data. Relative to the expected 66% missing rate, just under 60% of the final sample does not have associated weeks worked data.

4.2.2 Imputation Model

Thus, in order to impute these missing values, I run the following models for earnings and weeks worked:

$$Y_{ist} = \beta_0 + \beta_1 ind_{ist} + \beta_2 occ_{ist} + \beta_3 d_{it} + \gamma_s + \delta_t + \varepsilon_{ist} \tag{1}$$

where Y_{ist} refers to the log of weekly earnings and weeks worked of individual i in state s at time t, ind_{ist} and occ_{ist} represent variables for workers' industry and occupation, d_{it} is the set of demographic characteristics for worker i,[10] γ_s and δ_t capture state and time fixed effects, respectively, and ε_{ist} is the residual.

I fit both models (weekly earnings and weeks worked) on the full sample of individuals

[10]Specifically, this includes gender, marital status, education, race, age, a quadratic term for age, and the number of dependents (computed as the number of individuals under age 15 in the household).

included in the CPS from 1996 to 2021, removing only cases of attrition (as described in Section 4.1) prior to doing so. My sample of interest consists of individuals that experience at least one EUE spell, which represents a relatively exceptional population — it includes workers who lost their jobs, experienced a particularly brief spell of unemployment, and were able to find a new job without having to relocate. Thus, in order to more comprehensively capture the relationship of earnings and weeks worked with observable worker characteristics, I opt to fit these imputation models on the full dataset rather than the sample of interest. More specifically, through this imputation, I hope to capture, for instance, high paying industries in which workers experience few unemployment spells, not only industries or occupations in which workers have experienced EUE spells.

In terms of imputing earnings, I fit model 1 above using only one of the two potential wage observations per individual, excluding the wage observation in survey month 8 in cases where the worker has two instances of non-missing wage data. It is plausible that there is some selection entangled within whether a worker has one or two wage observations during the eight months she is surveyed; individuals with two wage observations may be more stable in terms of job turnover or housing security, and thus may tend to be higher earning, higher educated, etc. A possibility in fitting the earnings imputation model could be to include individual-level fixed effects to control for any idiosyncratic heterogeneity among workers not captured by their industry, occupation, or demographic characteristics. This methodology could certainly provide more precise estimates of the inter-industry and inter-occupation wage differentials, but these effects will not be identified for those with only one wage observation. And as some selection is involved in the population of workers with two wage observations, this methodology may face similar challenges. Thus, given these nuances of selection, I discard the second non-missing wage observation of each individual. Ultimately, with this imputation, my aim is not to identify the causal impacts of a worker's industry, occupation, and demographics on their earnings (and weeks worked); I am simply looking to identify the relationship between workers' earnings (and weeks worked) and their observable characteristics.

For weeks worked data, it should first be noted that, similar to earnings, it is possible for a given worker to have zero, one, or two distinct weeks worked observations, based on how many times the worker was surveyed for the ASEC. Though one could anticipate similar selection issues here as in the case of earnings, as is discussed in Section 3.2.1, it may be reasonable to assume that ASEC data is much closer to missing at random than is earnings.[11] Thus, when imputing weeks worked, I include both weeks worked observations in cases

[11] That is, assuming that CPS respondents surveyed in different calendar months are not systematically different.

where the worker was included in two separate ASEC surveys, treating these observations as two distinct individuals (i.e., no individual fixed effects). Furthermore, as the data on weeks worked refers to activities in the previous calendar year, I use workers' observable characteristics from the earliest available month in the rotation[12] to estimate the model. By doing so, I capture the observable characteristics closest to the time period referenced in the ASEC survey.

Lastly, I make two final adjustments to the imputed values. First, I follow the conventions set by the CPS and top-code both the weekly earnings and weeks worked data. Any imputed weeks worked greater than 52 are recoded as 52, and any weekly earnings imputed as higher than \$2884.61 are recoded to \$2884.61. Second, as mentioned above, I run a linear model on log earnings in efforts to make the outcome variable closer to normally distributed. However, in order to estimate UI benefits, the benefits calculator requires estimates of earnings in levels rather than logs. Thus, I use Duan's Smearing Estimate (Duan, 1983) to re-transform the log earning estimates back to earnings in levels.

$$\widehat{Y_i} = \exp(\widehat{\ln Y_i}) \cdot \frac{1}{N} \sum_{j=1}^{N} \exp(\varepsilon_j)$$

where ε_j is the residual from fitting model 1 on log weekly earnings.

The results of this imputation are presented in Table 5 below. I compute UI benefits based on earnings and weeks worked data only during months of employment — the original and imputed distributions of earnings and weeks worked in the final sample for months of employment are shown below. The imputation does an imperfect job of predicting earnings and weeks worked, as seen in the distributions as well as their R-squared values: 0.4402 and 0.1088, respectively. In particular, the practically bimodal distribution of the weeks worked data makes it trickier to impute. As seen in Tables 6a and 6b below, in fitting a normally distributed outcome, the standard linear model significantly underestimates the frequency of extreme values (0 and 52 weeks worked).

4.2.3 Cross Validation

To further assess the validity of the imputation, I conduct a cross validation procedure, which can be described as follows:

[12] Survey months 1 — 4 or 5 — 8

Table 5: Imputation Results

Source: Filtered dataset of EUE spells (Table 2). Distributions above are limited only to months of employment

1. Remove cases of migration attrition, and randomly split the full sample (CPS, 1996 — 2021) into 10 subsamples.
2. Fit model 1 on all subsamples except subsample i (nine-tenths of the sample).
3. Using the coefficient estimates from step 2, impute missing earnings (weeks worked) values in subsample i (one-tenth of the sample), using the appropriate top-coding and retransformation adjustments.
4. Compute the root mean-squared error of the imputed values in subsample i.
5. Repeat steps 2 through 4 on all 10 subsamples.
6. Repeat steps 1 through 5 on the final sample of EUE spells.

The results of the cross validation are presented below. Table 7 illustrates the summary statistics for the 10 root mean-squared errors of the weeks worked cross validation in the full sample and the final sample of EUE spells. Given the bimodal distribution (as seen in Table

Table 6: Distribution of Weeks Worked

(a) Original

	Freq.	Percent
missing	162,837	59.16
52	51,784	18.81
some	50,712	18.42
0	9,918	3.60
Total	275,251	100.00

(b) Imputed

	Freq.	Percent
52	2,645	0.96
some	272,606	99.04
Total	275,251	100.00

Source: Filtered dataset of EUE spells (Table 2). Distributions above are limited only to months of employment

5) of the weeks worked data, the prediction of missing values results in an error of approximately 18 weeks in the final sample. In spite of their magnitude, Table 7 demonstrates that these errors are remarkably consistent across the ten subsamples and comparable to those computed in the full sample. The results for weekly earnings are similar, imputing with an average discrepancy of $375 in the final sample, equivalent to approximately 0.75 standard deviations of the earnings distribution (Table 8).

Table 7: Weeks Worked and Weekly Earnings
Cross Validation Results

	Mean	SD	Min	Max	N
Weeks Worked					
Original	12.14	0.01	12.11	12.15	10
Imputed	17.68	0.10	17.53	17.82	10
Earnings					
Original	452.14	0.49	451.26	452.63	10
Imputed	375.00	6.89	360.24	383.19	10

Though it is difficult to simply dismiss these prediction errors as insubstantial, the cross validation illustrates that the imputation performs well in incorporating new data, and that it is fairly reasonable to fit model 1 on the full CPS sample to impute values in the final sample of interest.

Table 8: Distribution of Weekly Earnings in the Final Sample

	Mean	SD	Min	Max	N
prernwa	529.9547	484.6757	0	2884.61	66534

Source: Filtered dataset of EUE spells (Table 2). Distributions above are limited only to months of employment

Ultimately, as mentioned at the start of this section, missing wage and weeks worked data and appropriately imputing these values play a critical role in my analysis. With this imputation procedure, I implicitly move forward under the assumption that UI eligibility and weekly benefit amounts are determined by a worker's industry, occupation, and observable demographic characteristics as of their most recent job. The nontrivial discrepancies between the true and imputed values for earnings and weeks worked demonstrate that various unobserved factors also determine these outcomes. Given the inherent data limitations of the CPS,[13] I proceed with this imputation methodology, and leave more innovative techniques to future research.

4.3 Job Quality

Historically, the literature on UI has primarily focused on how the duration of UI benefits influence the probability of employment; few have explored how they impact the type or quality of employment. In this section, I describe how I define the main outcome of interest for this study: job quality.

To measure job quality, I run the following two regression models on the full CPS dataset:

$$\begin{aligned}\ln(w_{ist}) = \beta_0 + \beta_1 ind_{ist} \times nwages_i + \beta_2 occ_{ist} \times nwages_i \\ + \beta_3 d_{it} \times nwages_i + \gamma_s + \delta_t + \varepsilon_{ist} + \nu_i\end{aligned} \tag{2}$$

$$\begin{aligned}\left(\ln(w_{ist}) - \overline{\ln(w_{is})}\right) = &\beta_1 \left(ind_{ist} - \overline{ind_{is}}\right) + \beta_2 \left(occ_{ist} - \overline{occ_{is}}\right) \\ &+ \beta_3 \left(d_{it} - \overline{d_i}\right) + \delta_t + \left(\varepsilon_{ist} - \overline{\varepsilon_{is}}\right)\end{aligned} \tag{3}$$

where $\ln(w_{ist})$ represents the log wages of individual i in state s in time t, $nwages_i$ is a dummy variable for the number of wage observations of each individual (equal to 0 if they have one wage observation, and 1 if they have two), ind_{ist} and occ_{ist} refer to workers'

[13] Other alternatives tend to publish data at a lower frequency (e.g., annually) and include a much smaller sample.

industry and occupation, d_{it} is the set of demographic characteristics for worker i, γ_s and δ_t capture state and time fixed effects (month and year), respectively, and ε_{ist} is the residual. Finally, ν_i represents unobserved individual-level characteristics.

Model 3 is a fixed effects model that controls for individual-level unobserved heterogeneity. Using Stata's `xtreg, fe` command, this takes the average of all included variables over time t and subsequently de-means the selected outcome and regressors. Notably, this removes the term ν_i, which represents any unobserved individual characteristics.

From these specifications, I define job quality as the sum of the industry and occupation coefficients. This quantity represents the additional log wage that a worker receives by being employed in a specific industry and occupation, relative to the base categories. I plot the coefficients of the industry and occupation variables in Figure 5. Notably, the standard OLS model with interaction terms (Model 2) consistently produces larger estimates of the industry and occupation coefficients (in absolute value).

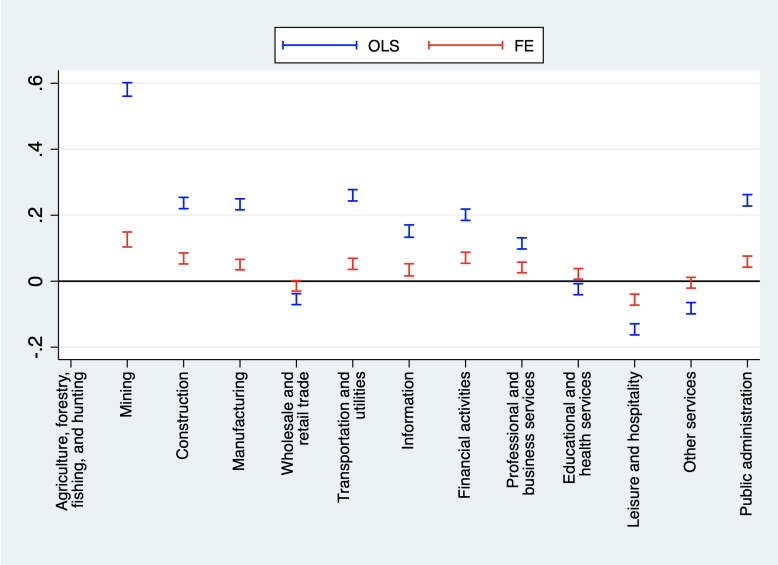
Starting from a mile-high perspective, in a perfectly competitive labor market with no frictions, all workers are paid their marginal products; firms optimize to minimize their costs. Under this framework, classifying workers based on industries and occupations is more a formality than a meaningful distinction. As workers are compensated based on their human capital, they (freely) reallocate between industries and occupations. Under this model, therefore, systematic wage differentials between specific industries or occupations cannot exist. However, in a frictional labor market, with monopolies, labor unions, and costs to change jobs and hire and fire workers, workers may not be compensated at their marginal productivity. It is in this world that composition effects, and systematic industry and occupation wage differentials can arise. Thus, for the question of what effect do UI benefits have on job quality to be coherent in the first place, a model of labor markets with frictions must operate in the background.

To measure these inter-industry and inter-occupation wage differentials, in the ideal scenario, one would collect earnings, industry, and occupation data from a sample of individuals, and fit a model with individual fixed effects (FE). By doing so, the researcher can analyze the true rent or additional profit that stems from working in that particular industry or occupation, independent of any individual-level idiosyncrasies. In principle, one can do the same using the CPS; the results are represented by the red confidence intervals in Figure 5. Although the FE specification precisely captures unobserved individual heterogeneity, it does not account for potential issues of selection.

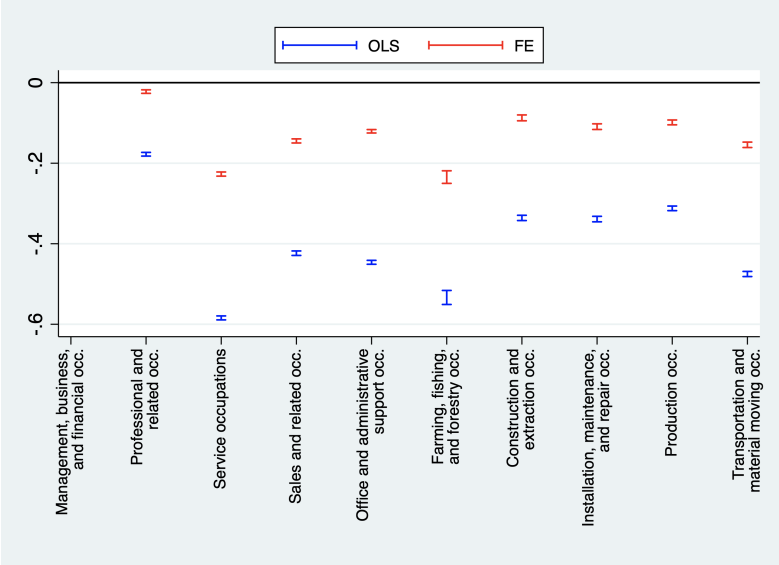
As mentioned in Section 4.2, earnings data is collected in survey months 4 and 8 of the CPS, but there are a variety of reasons for which this data may not be available for one or both months: non-employment, refusal to respond, migration attrition, among others. Therefore,

Figure 5

(a) Industry



(b) Occupation



95% Confidence Intervals are displayed.

the number of wage observations a worker has (zero, one, or two) may be correlated with other unobserved characteristics that determine wages (e.g., single, low-wage workers may relocate more frequently, implying a higher probability of attrition from the CPS). In this case, even the FE model does not fully capture the relationship of industry and occupation with wages.

As a result, I also evaluate a standard OLS model (Model 2) with an interaction term: a dummy variable indicating whether the individual has one or two wage observations. The results of this specification are indicated in blue in Figure 5. Unlike the model with individual fixed effects, this model incorporates individuals with only one wage observation. Nevertheless, this specification has its own drawbacks: though the interaction terms capture the heterogeneity between workers with one versus two wages, the model does not account for all unobserved individual heterogeneity. More specifically, the estimated industry and occupation coefficients in the OLS model represent not only the true wage differential between industries and occupations, but also capture the fact that some workers are compensated more simply because they are better workers (e.g., ability, intelligence, etc.). Indeed, the difference between the FE and OLS coefficients (Figure 5) represents this composition effect.

In short, there are two distinct, yet simultaneous selections at play: 1) what types of people work in each industry and occupation, and 2) what types of people remain in the CPS sample for the entire eight survey months. The delicate selection challenges make it such that neither specification above precisely captures the relationship between wages and industry and occupation; the true effect likely lies somewhere in between. Thus, in the sections below, results using both sets of coefficients are presented.

4.4 Coding Errors in Industry and Occupation in the CPS

As the previous section suggests, major industry and occupation codes in the CPS are central to my outcome variable of interest. As such, it is worth noting some limitations and measurement error in this data that has been documented in the literature.

Prior to 1994, the CPS would ask respondents which industry and occupation they worked in each month. Per Moscarini and Thomsson (2007), this procedure resulted in a 50% error when coders were not informed records from two separate months concerned the same individual, and exhibited a 12% discrepancy even when coders were informed. This motivated an overhaul of the survey process in 1994; rather than re-asking occupation-related questions every month, the survey would simply ask whether respondents worked for the same employer that they reported in the previous month. Doing so reduced occupational mobility from 30% to 3%.

However, following a spell of unemployment, the survey restarts from scratch and asks detailed questions related to worker's industry and occupation. Namely, industry and occupation data are independently coded across unemployment spells, and thus, the probability of occupational change may be inflated substantially. This may raise concerns of nontrivial measurement error in my outcome of interest, but as long as this error is independent of UI benefit receipt, it should not bias my results — indeed, I hold this assumption in the following sections.

These meticulous measurement errors pose less of a concern in cross-sectional analyses that examine industry and occupation shares at a fixed point in time. Yet when examining employment dynamics and transitions of individuals over time, these errors tend to compound. In attempts to minimize the effects of these challenges, I compute UI benefits of each worker based information from their most recent month of employment prior to the unemployment spell. Although my outcome (change in job quality) will inherently be subject to these data limitations, utilizing this methodology for UI benefits (which are computed from predicted earnings and weeks worked, which also derive from industry and occupation data) can hopefully prevent any unnecessary additional complications.

5 Results

To estimate the effect of UI benefits on re-employment job quality, I examine the COVID period (2018-2021) separately from the pre-COVID period (1996-2017). As a baseline, I run the following model on both samples:

$$quality_{ist} = \beta_0 + \beta_1 \ln(ui_{ist}) + \beta_2 prequality_{ist} + \beta_3 d_{it} + \gamma_s + \delta_t + \varepsilon_{ist} \tag{4}$$

where $quality_{ist}$ represents the change in job quality between the two months of employment, measured separately for both sets of coefficients displayed in Figure 5, $\ln(ui_{ist})$ is the log of weekly unemployment benefits, and $prequality_{ist}$ is the quality of the most recent job prior to the unemployment spell.

The results of this initial specification are shown in Table 9 below, for both measures of job quality. It is important to note, however, that the estimates from this reduced form model do not represent causal relationships. Causal identification in this case is considerably challenging, given the high degree of endogeneity; a variety of unobserved worker characteristics (ability, intelligence, etc.) may jointly determine UI benefit receipt and aptitude for re-employment. Even so, this reduced form illustrates that UI benefits did not have a statistically significant relationship with job quality during the pre-COVID period, whereas

the opposite is the case during COVID, even after controlling for monthly COVID cases and deaths. Thus, in attempts to attain clearer identification, I incorporate state unemployment rates during the pre-COVID period in my analysis.

Table 9

	OLS			FE		
	(1) 96-17	(2) 18-21	(3) 18-21	(4) 96-17	(5) 18-21	(6) 18-21
log_ui	0.004	0.064***	0.065***	-0.001	0.017**	0.017**
	(0.006)	(0.018)	(0.018)	(0.002)	(0.007)	(0.007)
job_quality_ols_pre	-0.620***	-0.662***	-0.662***			
	(0.006)	(0.018)	(0.018)			
job_quality_fe_pre				-0.597***	-0.626***	-0.626***
				(0.006)	(0.018)	(0.018)
Observations	55121	4930	4930	55121	4930	4930
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
COVID			Yes			Yes

Note: *** p<0.01, ** p<0.05, * p<0.10

A broader caveat of all the results that follow is that in each of these analyses, I treat UI benefits and measures of job quality as pure data. In other words, I do not account for the fact that both values derive from imputations or other estimations. The fact that many controls (demographics; state, year fixed effects) overlap between these regression models magnifies the measurement error of my outcome and predictor of interest, and thus interpretations of statistical significance should be treated conservatively. To properly account for these larger standard errors, one must utilize more complex econometrics methods (e.g., Delta method, etc.) that are beyond the scope of this paper.

5.1 Pre-COVID — 1996-2017

As discussed in Section 2.1, state UI rules include permanently authorized laws that automatically extend the duration of UI benefits during weak economic conditions. These extensions are triggered by specific state unemployment rate thresholds, and require specific growth

rates of unemployment to remain active.[14] At the individual worker level, the exact levels of these thresholds are relatively arbitrary, as in, whether one worker receives UI duration extensions while others do not depends more heavily on where the worker lives than on any unobserved individual characteristics. In other words, some variation in state unemployment rates, and thus UI receipt, is exogenous, simply due to fluctuations in the business cycle. Thus, the high between-state variation and moderate worker-level exogeneity suggest state unemployment rates may serve as a reasonable instrument for UI benefits. The first-stage regression of this model is shown below.

Table 10 illustrates UI benefits' highly statistically significant relationship with state unemployment rates, meeting the relevance criteria of a strong instrument. Notably however, this relationship is slightly negative, implying a 1 percent increase in state unemployment rates is associated with a 0.2% decrease in UI benefits received. This estimate may be implicitly capturing the composition effects that occur with fluctuations in the business cycle. As UI benefits are often computed as a fraction of a workers' previous wages, if higher state unemployment rates are correlated with larger numbers of lower income workers becoming unemployed, in aggregate, average levels of UI benefit receipt may decrease.

Table 10

	(1)
job_quality_ols_pre	0.598***
	(0.00297)
state_unemployment_rate	-0.00247***
	(0.000784)
Constant	3.539***
	(0.0111)
Observations	55,121
R-squared	0.898

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 11 shows the results of the second-stage regression, using the instrumented log UI benefits to estimate job reemployment quality. As was the case in the reduced form

[14] More details on the Extended Benefits (EB) policies can be found here.

model, the results suggest measurement error and mean reversion play a significant role when measuring EUE outcomes — the coefficient for the quality of the worker's last job prior to unemployment is significant even at the 1% level. Consistent with the reduced form model, Table 11 also shows that UI benefits do not exhibit a statistically significant relationship with changes in job quality prior to 2018. This result holds for both measures of job quality (from Model 2 and Model 3), shown in column 1 and column 2 of Table 11, respectively.[15]

Table 11

	(1)	(2)
log_ui	0.789	0.403*
	(0.489)	(0.230)
job_quality_ols_pre	-1.090***	
	(0.293)	
job_quality_fe_pre		-1.250***
		(0.372)
Constant	-3.109*	-1.528*
	(1.725)	(0.808)
Observations	55,121	55,121
R-squared	0.072	
Standard errors in parentheses		
*** p<0.01, ** p<0.05, * p<0.1		

A strong note of caution when interpreting (or over-interpreting) these results is merited: state unemployment rates are likely not a valid instrument in the current framework. States of the business cycle may be correlated with the selection of workers that experience EUE spells; if we are unable to observe the characteristics that differentiate these workers from those that do not undergo EUE transitions, state unemployment rates face similar challenges of endogeneity.

The pre-COVID sample highlights the challenges of causal identification when studying the effects of UI benefits. One could nevertheless conceive of more creative methods to over-

[15] Demographic controls and time and state fixed effects have been removed from the table for space-saving purposes.

come these challenges, e.g., using individual fixed effects to capture unobserved individual heterogeneity,[16] among others. I leave these analyses to prospective future research, and instead exploit exogenous variation in UI benefits during COVID-19.

5.2 COVID Period — 2018-2021

As discussed in Section 2.2, Congress implemented $600, and later $300, weekly UI supplements to support unemployed workers during the steep economic downturn of COVID-19. Importantly, these additional benefits were provided to all UI-eligible workers, regardless of observed or unobserved individual characteristics. I exploit this exogenous variation to examine the effect of UI benefits, and run the following interaction model:

$$\begin{aligned} quality_{ist} =& \beta_0 + \beta_1 \ln(ui_{ist}) + \beta_2 interact600_{it} + \beta_3 interact300_{ist} + \beta_4 prequality_{ist} \\ &+ \beta_5 d_{it} + \beta_6 covid_{ist} + \gamma_s + \delta_t + \varepsilon_{ist} \end{aligned} \tag{5}$$

Where $covid_{ist}$ represent controls for the cumulative number of cases and deaths and the number of new monthly cases and deaths by state. $interact600_{ist}$ is the interaction between a time dummy for when the $600 supplement was in active, and the quantity $\ln(ui_{st}+600)-\ln(ui_{st})$; $interact600_{ist}$ thus represents the incremental impact of the COVID supplements. $interact300_{ist}$ is defined analogously for the $300 supplement. Thus, β_2 and β_3 are the key coefficients of interest, as they capture the effect of an exogenous change in UI on changes in job quality. The results of this specification are displayed in Table 12 below.

Post-2017, measurement error and mean reversion continue to play a significant role, as initial job quality is highly statistically significant. For both measures of job quality, the interaction terms representing the $600 and $300 UI supplements do not indicate a statistically significant relationship with reemployment job quality.

This result in and of itself may suggest that, paradoxically, causal identification of the effects of UI benefits during COVID-19 is more challenging than initially expected. While the $600 and $300 supplements constitute shocks that were independent of unobserved individual worker characteristics, they represent substantially "blunt" instruments; unlike UI extensions from state unemployment, which varies considerably across states and over time, the $600 supplement was enacted in all states at the same time, and also expired across all states simultaneously. Thus, after controlling for state and time fixed effects, the only variation remaining stems from variation in the baseline levels of state UI benefits; workers in states

[16] Although, this approach is not without its own limitations; there are very few CPS respondents that experience more than one EUE spell during the period they are surveyed, so much so that it is questionable as to whether they constitute an unbiased, representative sample.

Table 12

	(1)	(2)
log(ui)	0.0659***	0.0173**
	(0.0185)	(0.0071)
interact_600	0.0125	0.0041
	(0.0144)	(0.0055)
interact_300	0.0232	0.0061
	(0.0202)	(0.0076)
prequality_ols	-0.6622***	
	(0.0178)	
prequality_fe		-0.6264***
		(0.0183)
Observations	4930	4930
COVID	Yes	Yes
State FE	Yes	Yes
Time FE	Yes	Yes

Note: *** p<0.01, ** p<0.05, * p<0.10

with lower average UI experienced a larger increase in replacement rates than those in more UI-generous states. In this sense, the causal identification will be weaker.

Although this caveat is slightly less relevant for the $300 supplements (for which states opted out of the program at different times), the pandemic more broadly involved multiple other pervasive exogenous shocks that operated simultaneously with the UI benefit expansions. Although I control for the cumulative and new monthly cases and deaths from COVID-19 by state, it is highly plausible that the pandemic impacted worker behavior in ways not captured by these measures. The dangerous public health environment may have influenced many workers' propensity to search and find new jobs, and this effect may have been heterogeneous between workers. For instance, workers with large amounts of weekly benefits may be particularly less incentivized to find work given the health risks; therefore, the workers that we have the ability to track may be low-income, low-benefit-receiving workers taking whatever jobs remained available, regardless of the quality relative to their previous job. Similarly, as the COVID-19 pandemic particularly impacted low-wage sectors that involved greater personal contact (restaurants, services, etc.), many low earning workers

were forced to find employment elsewhere. If they did successfully find work, it is therefore likely that it was in a higher paying industry on average, although the worker may receive less pay than he did in his previous job. As the economy began to open back up, if these workers then returned to their previous employers, though they may be earning more by returning to their previous job, if this job is in a lower "ranked" or on average lower paying industry or occupation, my analysis considers this EUE spell as a negative change. In this sense, other cyclical episodes may have been less "special" in comparison to COVID-19, making COVID-19 a paradoxically difficult period for identification.

Nevertheless, the results in Table 12 also illustrate that, unlike in the pre-COVID sample, standard UI benefits during COVID-19 have a positive, statistically significant association with reemployment job quality, suggesting a 1.7% to 6.5% increase in job quality for a 1% increase in UI benefits, where job quality is measured as the average wage differential earned by working in a particular industry and occupation. Although this term similarly faces substantial endogeneity (UI receipt is determined by unobserved characteristics that also correlate with employment outcomes), it is worth noting the divergence in results from the pre-COVID period. Overall, these results highlight that considerable unobserved factors remain in understanding relationship between UI benefits and changes in job quality following a spell of unemployment — future research might explore alternative identification strategies to examine the causal effect of UI benefits on job reemployment quality.

6 Conclusion

In this paper, I contribute to the extensive literature on unemployment insurance and its implications for labor market outcomes, including during the COVID-19 pandemic. Expanding on the methodology developed by Ganong et al. (2020), I create a comprehensive dataset of state-specific UI eligibility rules and benefit schedules. Combining this data with micro-level earnings estimates from the CPS and the ASEC, I estimate weekly UI benefits from 1996 to 2021. To my knowledge, this is the first dataset that covers the entire span of the modern CPS and can impute UI benefit amounts at a monthly frequency based on detailed state UI rules.

I present evidence that the $600 and $300 weekly supplements implemented in 2020 and 2021 had no significant impact on reemployment job quality. Furthermore, I find that standard state UI benefits prior to COVID-19 (1996-2017) exhibited no relationship with job quality following unemployment. Conversely, the results for 2018 onwards demonstrate there is a positive, statistically significant association between UI benefits and job quality, ranging from a 1.7% to a 6.5% increase in reemployment job quality per 1% increase in UI.

The naive interpretation of these results and their corresponding policy implications would argue that standard UI benefits provided beneficial support to unemployed workers only during COVID-19, and the additional emergency supplements enacted during the pandemic were unnecessary. However, as discussed in the previous sections above, there are several key caveats to these results. Most immediate is the fact that certain characteristics that determine a person's UI receipt may also determine their employment outcomes; as many of these characteristics are unobserved, using UI benefits to predict reemployment outcomes suffers from significant endogeneity. The other cautionary note regarding these results is that data limitations and missing data play a substantial role in this study. I use estimated coefficients to compute measures of job quality and am forced to impute missing earnings data in order to calculate estimated UI benefits. However, I do not account for the potentially large measurement errors embedded within these estimates. Thus, the precision of these results may be nontrivially overstated.

On the other hand, however, data limitations in terms of sampling may significantly understate the true causal effect of UI. Given the limited longitudinal dimension of the CPS, we lose a large population of workers who exit the survey either through attrition or the conclusion of their survey period without reentering employment. As a result, we ultimately may not observe the individuals who benefit most from UI: unemployed workers who can now afford to wait longer and search for a better job due to more generous benefits.

These limitations present several avenues for future research. Studies that implement more innovative methods to more precisely impute missing earnings data may improve the precision of the results presented in this paper, or subsequent studies may pursue entirely different identification strategies that more clearly identify exogenous variation in UI while limiting the need for imputing values (e.g., event studies, etc.). Furthermore, studies that explore whether UI benefits have heterogeneous effects across different populations (not limited to demographic categories but also types of unemployment — temporary layoffs, voluntary quits, etc.) may reveal interesting results and implications for policy. While I focus primarily on wages in defining job quality, future research that incorporates other measures of job quality (unionization, part time versus full time, benefits, non-monetary compensation) may capture a more complete picture of the impact of UI. Analogously, studies that incorporate UI durations in addition to UI benefit amounts will capture a large majority in UI variation. Finally, studies that focus on, or to whatever extent possible can overcome, the challenges of incomplete EUE spells should be of key interest for both academics and policymakers.

Overall, this paper utilizes high frequency data on UI benefits to study their role in improving the quality (not only the quantity) of employment. In highlighting both the

identification challenges and the opportunities in analyzing this topic, this paper hopefully helps motivate future research and policy discussions around what optimal UI policy looks like, as well as what the objective function should be.

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Appendix

A.1 CPS Industry and Occupation Coding

Table A.1: Major Occupation Recoding

1996–2002		2003–2021	
1	Executive, Admin, and Managerial	1	Management, business, and financial
2	Professional Specialty	2	Professional and related
3	Technicians And Related Support		
6	Private Household	3	Service
7	Protective Service		
8	Service, Exc. Protective and Hhld		
4	Sales	4	Sales and related
5	Admin. Support, Incl. Clerical	5	Office and administrative support
13	Farming, Forestry And Fishing	6	Farming, fishing, and forestry
10	Machine Opers, Assemblers and Inspectors	7	Construction and extraction
12	Handlers, equip Cleaners, helpers, laborers	8	Installation, maintenance, and repair
9	Precision Prod., Craft and Repair	9	Production
11	Transportation And Material Moving	10	Transportation and material moving
14	Armed Forces	11	Armed Forces

Source: CPS, January 1996 — December 2021. The table shows my manual recoding of `PRMJOCC1` in the CPS.

Table A.2: Major Industry Recoding

1996–2002		2003–2021	
1	Agriculture	1	Agriculture, Forestry, Fishing, and Hunting
21	Forestry And Fisheries		
2	Mining	2	Mining
3	Construction	3	Construction
4	Manufacturing – Durable Goods	4	Manufacturing
5	Manufacturing – Non-Durable Goods		
9	Wholesale Trade	5	Wholesale and Retail Trade
10	Retail Trade		
6	Transportation	6	Transportation and Utilities
8	Utilities And Sanitary Services		
7	Communications	7	Information
11	Finance, Insurance, And Real Estate	8	Financial Activities
20	Other Professional Services	9	Professional and Business Services
16	Hospitals	10	Educational and Health Services
17	Medical Services, Exc. Hospitals		
18	Educational Services		
15	Entertainment And Recreation Services	11	Leisure and Hospitality
12	Private Households	12	Other Services
13	Business, Auto And Repair Services		
14	Personal Services, Exc. Private Hhlds		
19	Social Services		
22	Public Administration	13	Public Administration
23	Armed Forces	14	Armed Forces

Source: CPS, January 1996 — December 2021. The table shows my manual recoding of `PRMJIND1` in the CPS.

A.2 Unemployment Insurance Benefit Calculator

This section describes in detail the Unemployment Insurance (UI) benefit calculator adapted from Ganong et al. (2020) and the procedure for constructing the eligibility and benefit schedules for each state across time.

To preface any of my own description of the calculator, I highlight that the online appendix to Ganong et al. (2020) includes detailed documentation, minimum working examples in multiple languages, the data describing January 2020 UI eligibility and benefit schedules, and the source code for the calculator itself. The minor adaptations to the calculator and the extension of UI eligibility and benefit schedules to years other than 2020 explained below would not have been possible without the documentation and code provided in this online appendix. For anyone interested in replicating the calculator extensions used in this paper, it would be highly advisable to begin by reviewing the resources published there.

A.2.1 UI Benefit Calculator

The adapted UI benefit calculator used in this paper (just UI benefit calculator henceforth) computes the weekly benefits of a worker without dependents in all 50 states and the District of Columbia from 1996 to 2021. Many states have complex UI benefit rules; in cases where a state provides more than one method to compute benefits, this calculator includes only the first method. Thus, this calculator does not capture all dimensions of a state's UI rules. Data of these state rules are gathered from "Significant Provisions of State Unemployment Insurance Laws" and "Comparison of State UI Laws" documents, published by the Department of Labor (U.S. Department of Labor, 2021b,a). The calculator takes as input: the schedule of quarterly earnings in the past four calendar quarters, the number of weeks worked, the worker's state of residence, the year, and the half-year (either 1 or 2 — state policies are published semiannually in January and July).

The sources of these inputs can vary. As discussed in the body of this paper, I collect data on the number of weeks worked (in the previous calendar year) from the ASEC and impute the missing values. I combine this data with weekly earnings from the monthly CPS (also imputed) to calculate the schedule of quarterly earnings. Following Ganong et al. (2020), I assume respondents worked consecutively at the same weekly wage for all reported weeks worked, and were working in the last week of the calendar year prior to the ASEC, making the most recent quarter the one with the highest earnings. However, quarterly earnings can be computed through other means: Ganong et al. (2020) uses the annual wages reported in the ASEC and uses the number of weeks worked to compute an average weekly wage, without having to impute any missing values. Researchers should use what they feel is most

appropriate for their specific study.

A.2.2 State Thresholds

To compute weekly benefits, the calculator references two datasets, state thresholds and state eligibility. Each state computes benefits using a `rate`, which is the ratio of weekly benefits to a measure of prior earnings (`base_wage`). The weekly benefit amounts (`wba`) are computed as: `wba = rate * base_wage + intercept`, subject to maximum and minimum thresholds. An `intercept` is included in cases where states impose a fixed payment (or reduction) to the benefit.

As described in the appendix of Ganong et al. (2020), the `wage_concept` describes how the `base_wage` is defined.

`wage_concept`	Value of `base_wage`
`annual_wage`	Annual Wages
`hqw`	Wages in the highest quarter
`2hqw`	Wages in the two highest quarters
`2fqw`	Wages in the last two quarters
`ND`	Wages in the two highest quarters + half wages in third highest quarter
`direct_weekly`	Weekly wages
`3hqw`	Wages in the three highest quarters

Note: the `direct_weekly` `wage_concept` was added to the calculator on August 13, 2020 by Ganong and co-authors. The calculator's source code has been updated, but the tables in the online appendix's data dictionary have not yet been updated to reflect this addition.
North Dakota has a `wage_concept` which is not common to any other states and is coded as ND and described in the `wage_concept` table above.

The only difference in `wage_concept` here versus in Ganong et al. (2020) is the addition of a `3hqw` methodology (Washington uses this `wage_concept` in 2004).

Some states also have different benefit schedules based on different levels of prior earnings; an `inc_thresh` variable is included in the data to determine which benefit schedule to use.[17] Colorado and Minnesota use state average annual wages (AAW) in order to set

[17] No state from 1996 to 2021 includes more than one `inc_thresh` in their benefit calculations.

these thresholds each year. I gather data on AAW from the BLS (for further discussion on collecting AAW data, see Section A.2.4).

When creating this dataset, I follow a few other conventions set by the data provided by Ganong and coauthors:

- When a range is provided for the `rate`, in cases where further documentation is not provided to determine which rate is used, I default to the lower limit of the range.
- In cases where a state uses statewide AAW, I use the AAW from the previous calendar year.
- When state average weekly wages (AWW, calculated as the AAW divided by 52) are used:
  - If the document states: AWW in HQ or AWW in 2HQ, I record the `rate` as `hqw` / 13 and `2hqw` / 26, respectively
  - When a specific quarter is not referenced or AWW in BP is written, I default to the `direct_weekly` `wage_concept`

More broadly, when creating this dataset, information from "Comparison of State UI Laws" documents supersedes information from "Significant Provisions of State Unemployment Insurance Laws" documents. While the "Significant Provisions of State Unemployment Insurance Laws" are conveniently formatted to facilitate comparisons across states, the limited space makes it such that the "Comparison of State UI Laws" often contains more comprehensive information.

A.2.3 State Eligibility

To be eligible for UI benefits, unemployed workers must meet certain employment and earnings criteria over a specified "base period." Per the Department of Labor, most states define the base period as the first four of the last five completed calendar quarters, and some states allow for more recent earnings to be used in an alternative base period. Based on the methods I use to simulate past quarterly earnings as described above, I follow Ganong et al. (2020) and define the base period as the calendar year prior to an ASEC survey. This base period corresponds to the standard base period for UI applicants in April, May, or June of the corresponding ASEC year. The various eligibility criteria and their descriptions are listed in the table below.

Minnesota, Montana, Wyoming, and Iowa include statewide AAW as part of their eligibility calculations. Following Ganong et al. (2020), I round thresholds involving AAW to the nearest cent. Maine and Ohio use AWW (like for state thresholds, these are computed as AAW divided by 52); eligibility based on AWW are rounded to the nearest dollar. As in

Variable	Criterion
absolute_base	Minimum total earnings in the base period in dollars
hqw	Minimum total earnings in the base period, expressed as a multiple of high quarter wages
absolute_hqw	Minimum high quarter wage in dollars
wba	Minimum earnings in the base period, expressed as a multiple of worker's weekly benefit amount
num_quarters	Minimum number of quarters with wages
outside_high_q	Minimum earnings outside high quarter in dollars
wba_outside_q	Minimum earnings outside high quarter, expressed as a multiple of the weekly benefit amount
absolute_2nd_high	Minimum earnings in 2nd highest quarter
wba_2hqw	Minimum earnings in two highest quarters as a multiple of the weekly benefit amount
abs_2hqw	Minimum dollar amount in two highest quarters
hqw_2hqw	Minimum in two highest quarters as multiple of high quarter wages

state thresholds, this data is gathered from the BLS, and values from the previous calendar year are used in calculations.

In Rhode Island, Arizona, New Jersey, and Washington, state minimum wages are incorporated in eligibility calculations. Data on minimum wages by state are published by the Department of Labor.

As mentioned in Section A.2.1, this calculator uses only the first method of calculating UI benefits when states list multiple methods. Prior to and including January 2002, the "Significant Provisions of State Unemployment Insurance Laws" documents include footnotes when listing eligibility criteria for some states. From the document itself, it is difficult to discern whether the footnote refers to an additional eligibility requirement or an alternative method of determining eligibility. In these cases, I refer to the corresponding "Comparison of State UI Laws" documents, which often document state rules in more detail, to make final judgments on how to input these rules into my dataset.

A.2.4 State Average Annual Wage Data

In this final subsection, I discuss the state average annual wage (AAW) data published by BLS. The data is released in May of each year, with the exception of years 2003, 2004, and 2011, in which AAW data was published twice (in May and November). To remain consistent, I reference the May files from these years when collecting AAW data.

AAW data by state is available from 2001 onwards. Prior to this, average annual wages are published only by major occupation group, and before 1997, AAW by state is not available at all (only AAW by industry). Thus, for the period 1997 – 2000, I calculate AAW by state manually through the following procedure:

1. In years where total employment and AAW are available for all major occupation groups (1999, 2000), I take the weighted average AAW (by employment) to compute the state AAW for that year.
2. In 1997 and 1998, total employment and AAW are not available for all major occupation groups. For these years,
   (a) I use the total employment and AAW of major occupation groups when available.
   (b) When only AAW is included, I compute the total employment of the major occupation group as the sum of total employment of its sub-occupations.
   (c) In cases when neither total employment nor AAW are populated,
      i. Total employment is computed as the sum of total employment of the sub-occupations.
      ii. AAW is calculated using the employment-weighted AAW of the sub-occupations. If either employment or AAW are missing for a sub-occupation, it is excluded from this weighted average.

As AAW is not published at the state level for years 1995 and 1996, I instead use the 1997 values and adjust these by the inflation rates published by the BLS.