Bias in the Level, Trend, and Cyclicality of U.S. Unemployment*

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Abstract

Accurate measurement of employment and unemployment is foundational to official labor market statistics, yet these measures are commonly derived from household surveys that suffer from substantial measurement error. To evaluate and correct survey-based measures of unemployment in the United States, we link retrospective reports of unemployment in the Current Population Survey Annual Social and Economic Supplement (CPS ASEC) to administrative tax records that identify the universe of Unemployment Insurance (UI) recipients. We find that 49% of UI recipients in 2010 erroneously reported no unemployment in the same year, with the majority instead reporting full-year employment. Misreporting decreases as UI benefits increase, as 80% of UI recipients in the lowest ventile of benefits reported no unemployment, compared to 30% in the highest ventile. This result suggests that salience plays a central role in reporting accuracy and that non-UI recipients may be even more likely to underreport unemployment. Preliminary estimates suggest that misreporting is procyclical and has roughly doubled from the early 1990s to 2022, raising concerns about accuracy in the levels and trends of current labor market statistics. We develop a method to correct a retrospective unemployment rate constructed from the CPS ASEC for the pronounced underreporting of unemployment that we document, and we additionally account for other possible survey errors. Given the close and stable relationship between the retrospective and official unemployment rates, we argue that these corrections are informative about bias in the official rate.

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1. Introduction

The United States measures unemployment, employment, and labor force participation primarily through household surveys in which respondents report their own labor market activities. The resulting data form the basis of official labor market statistics such as the unemployment rate, the employment-to-population ratio, and the labor force participation rate. These surveys also serve as a source of individual-level data on employment and unemployment for empirical research in labor economics and macroeconomics. A growing body of evidence has documented that household surveys suffer from substantial and increasing measurement error in the reporting of economic activities (e.g., Meyer, Mok, and Sullivan 2015), raising the question of whether survey-based labor market statistics are similarly affected. Because employment, unemployment, and labor force participation are derived from the same underlying survey responses, systematic errors in the reporting of any one labor force status necessarily distorts the others.

The unemployment rate illustrates the stakes most clearly. It is a direct input into foundational macroeconomic relationships such as the Phillips Curve, Okun's Law, and the Beveridge Curve. The Federal Reserve's dual mandate makes it an input into interest rate decisions. Fiscal policy responses such as the extension of Unemployment Insurance (UI) benefits and the design of stimulus packages are calibrated to observed unemployment levels. If the measured unemployment rate is systematically biased, then core macroeconomic relationships could be distorted and policy responses during recessions and recoveries could be improperly calibrated.

Despite this importance, survey-based labor market statistics have largely escaped external validation. GDP is cross-checked against independent income-side and expenditure-side estimates. Inflation is measured by multiple indices (such as the Consumer Price Index and Personal Consumption Expenditures Price Index) that can be compared against one another. Income and poverty statistics have benefited from extensive survey-administrative data linkages that have revealed substantial and growing measurement error (e.g., Meyer, Mok, and Sullivan 2015; Bee and Mitchell 2017; Meyer et al. 2021; Meyer and Wu 2023; Bee et al. 2024). Survey-based measures of employment and unemployment are not typically compared against administrative benchmarks, even as evidence of measurement error in other survey-reported statistics has grown.

This paper provides new evidence on the magnitude and nature of errors in unemployment reports in household surveys and develops methods to correct the unemployment rate for this

documented misreporting, along with other survey errors. We link survey responses from the Current Population Survey Annual Social and Economic Supplement (CPS ASEC) to administrative tax records – specifically Internal Revenue Service (IRS) Form 1099-G, which identifies the universe of UI recipients and serves as an administrative measure of unemployment for a subset of the truly unemployed. We assess the validity of this administrative measure of unemployment by checking whether 1099-G receipt corresponds to genuine declines in earnings experienced by UI recipients using linked W-2 records, consistent with job loss. We focus primarily on 2010, during the peak of the Great Recession and the year with the highest level of UI benefits paid prior to the pandemic. We find that 49% of individuals who received UI in 2010 reported no unemployment in the CPS ASEC in the same year, with the majority instead reporting full-year employment. We also find comparable levels of misreporting in the Survey of Income and Program Participation (SIPP), despite its shorter recall period and design features intended to reduce measurement error. Because the official unemployment rate is derived from the Current Population Survey (CPS) Basic Monthly Survey – which shares the same respondent pool as the CPS ASEC but uses an even shorter reference period – the SIPP results suggest that shortening reference periods alone cannot resolve the errors that we document and that the official unemployment rate is likely subject to these errors.

We find that the underreporting of unemployment varies systematically with the level of UI benefits received. Among UI recipients in the lowest ventile of benefits (approximately $500 in UI), 80% reported no unemployment, compared to 30% in the highest ventile (approximately $25,000). This gradient suggests that salience (or perhaps stigma) plays a central role in reporting accuracy, as individuals with shorter or less financially significant unemployment spells are most likely to fail to report them. This gradient also has implications for the population of unemployed individuals that we cannot observe in administrative data (i.e., those who did not receive UI). If misreporting is most severe among UI recipients with the smallest benefits – for whom unemployment was least financially salient – then non-UI recipients, who received no benefits at all and lack the institutional connection to unemployment that comes with receiving benefits, may be even more likely to be missed.

Examining time series patterns between 1987 and 2022, we find that within-survey inconsistencies between reported UI receipt and reported unemployment have roughly doubled from the early 1990s to 2022, with misreporting lowest during recessions (when unemployment is

widespread and salient) and highest during expansions. The forces underlying this procyclical pattern of misreporting are likely akin to those underlying the benefit gradient: both suggest that unemployment is more accurately reported when it is more salient, whether because of larger individual benefits or because of broader economic conditions. The procyclicality of survey inconsistencies has direct implications for our understanding of business cycle dynamics. If a larger share of the unemployed are missed during expansions than during recessions, then the cyclicality of the true unemployment rate may differ from what published series suggest, with consequences for the estimation of relationships such as the Phillips Curve and Okun's Law that depend on accurate measurement of unemployment fluctuations.

Motivated by the misreporting that we document, the final part of the paper develops methods to correct the unemployment rate. The CPS ASEC allows us to construct a retrospective unemployment rate – the share of the weeks in the labor force spent unemployed – which we show tracks the annual average of the official CPS monthly unemployment rate closely, with a correlation between the two rates of 0.99 and a stable ratio over time. Given this close relationship, we argue that corrections to the retrospective rate are informative about bias in the official rate.

Our simplest correction addresses false negatives – truly unemployed individuals who report no unemployment – by scaling up the count of unemployed individuals using the administrative misreporting rates we estimate and weighting by average reported duration of unemployment. This approach yields transparent bounds on corrections to the official unemployment rate. We additionally develop a more detailed correction that predicts weeks of unreported unemployment from declines in earnings observed in administrative W-2 data, and we validate these predictions against estimated weeks of UI receipt derived from UI benefit amounts, earnings histories, and state-specific benefit rules.

Beyond false negatives, we also account for false positives – truly employed individuals who erroneously report unemployment – using misclassification rates estimated from CPS reinterview data by Poterba and Summers (1986). We also assess the magnitude of errors in reported duration among individuals who correctly report unemployment by comparing their reported weeks unemployed to estimated weeks of UI receipt. False negative corrections are likely to substantially increase the retrospective unemployment rate, while false positive and duration corrections are likely to be small in comparison.

Our analysis contributes to several strands of literature. First, we add to work documenting discrepancies in survey measures of unemployment. Past research has identified conflicts in repeated reports of unemployment for the same individual (Abowd and Zellner 1985; Poterba and Summers 1984, 1986), inconsistencies across rotation groups (Bailar 1975, Solon 1986, Krueger, Mas, and Niu 2017), discrepancies between contemporaneous and retrospective measures (Levine 1993), and misclassification of labor force status inferred from within-survey patterns (Feng and Hu 2013; Shibata 2022; Ahn and Hamilton 2022). A common limitation of these studies is their reliance on within-survey comparisons or statistical assumptions about the misclassification process, and most are restricted to analyzing data from the 1980s and 1990s. The most direct precedent for our approach is Sanchez, Koskinen, and Lewis (2014, 2019), who link administrative unemployment data in Sweden to a survey and find that 10% of the unemployed report no unemployment in the year they were actually unemployed. We find a substantially higher underreporting rate of 49%, although differences in survey design and labor market institutions make direct comparison difficult. Beyond documenting the magnitude of misreporting, our linked data cover more recent years (through 2022) and, to the best of our knowledge, we are the first to document changes in unemployment misreporting over time and to assess their implications for the measured cyclicality of the unemployment rate.

This paper also contributes to a second strand of literature that uses high-quality administrative data to correct for measurement error in surveys and revise our understanding of economic conditions. Recent research has documented that survey error is substantial and growing over time (Meyer, Mok and Sullivan 2015, Meyer, Mittag and Goerge 2022). Several studies have incorporated administrative data to challenge conclusions obtained from survey data alone, including work on extreme poverty (Meyer and Wu 2018; Shantz and Fox 2018; Meyer et al. 2021), the evolution of poverty over time (Corinth, Meyer, and Wu 2022), and the poverty reduction effects of proposed changes to the Child Tax Credit (Corinth et al. 2021). Our findings extend this research agenda to unemployment, demonstrating that one of the most widely used statistics in economics is subject to substantial misreporting and developing methods to correct for this misreporting using linked administrative records.

Finally, our findings have implications for a large body of research in macroeconomics and labor economics that uses survey-based measures of labor force status. Estimates of the Phillips Curve (e.g., Stock and Watson 2021; McLeay and Tenreyro 2020), Okun's Law (e.g., Ball, Leigh,

and Loungani 2017), and the natural rate of unemployment (e.g., Staiger, Stock, and Watson 1997) all rely on the official unemployment rate as a measure of labor market slack. Empirical research in labor economics on topics ranging from job displacement to the returns to education depends on accurately reported employment and unemployment in household surveys. If these survey reports contain systematic errors, the resulting estimates are based on mismeasured inputs. The procyclical misreporting we document suggests that mismeasurement could affect the estimated cyclicality of unemployment, with potential consequences for estimated Phillips Curve slopes and Okun coefficients. Similarly, if the level of unemployment is systematically understated, estimates of the natural rate of unemployment may be biased downward. Our paper provides the first empirical basis, grounded in external administrative data, for assessing the potential magnitude of these biases.

The remainder of this paper proceeds as follows. Section 2 describes the data sources and linkage. Section 3 documents internal discrepancies within surveys and the large discrepancy between survey and administrative measures of unemployment. Section 4 examines heterogeneity in the misreporting of unemployment. Section 5 uses earnings declines among UI recipients to validate receipt of a 1099-G as a measure of unemployment, and Section 6 develops methods to correct the unemployment rate. Section 7 concludes.

2. Data

This section describes the survey and administrative data sources used in our analysis and describes the procedures to link them. We first outline the key surveys and survey variables, followed by describing the administrative records we use. We finally describe the procedures used to link records and how we adjust for missing linkage to ensure the representativeness of the CPS ASEC is preserved.

2.1 Survey Data

Our primary source of survey data is the CPS ASEC, covering reference years 1975-2022. The CPS ASEC is fielded between February and April and collects detailed information on income, program receipt, and labor force status in the previous calendar year. It serves as the official source of income and poverty statistics in the United States and is intended to be

representative of the U.S. civilian non-institutionalized population. Although the CPS ASEC ios not the source of the official unemployment rate, it is drawn from the same sample as the CPS Basic Monthly Survey, which is. The distinction between the two surveys and measures of unemployment derived from them is discussed in Section 6.

The CPS ASEC asks respondents whether they were not working for at least one week during the reference year and whether they were actively looking for work or on layoff during that period. Individuals meeting both conditions are classified as unemployed in the reference year. Respondents also report whether they received UI benefits during the reference year (starting in the 1987 reference year), and if so, the total amount received. We use respondents' self reported UI receipt to document internal survey inconsistencies for a broad range of years (1987-2022).

As a robustness check and to examine how the length of recall period might affect misreporting, we use the 2008 Survey of Income and Program Participation (SIPP) panel, which has a reference period of four consecutive months, roughly one-third the length of the CPS ASEC's annual recall. We construct calendar year indicators for 2010 by combining information from Waves 5-8, using Wave 6, reference month 4 as our base sample.

2.2 Administrative Data

Our primary source of administrative data on unemployment comes from IRS Form 1099-G, which are available for calendar years 2010-2022. State agencies file these forms with the IRS and report the total amount of UI benefits and other government payments received by an individual during the tax year. We use Box 1 of the 1099-G to identify UI recipients and the amount of benefits received and treat UI receipt as an administrative indicator of unemployment within the calendar year.

In some states, Box 1 may also include payments from Paid Family Leave (PFL) or Short-Term Disability Assistance (STDA) programs. Because California's STDA program generates a large volume of non-UI payments on Box 1, we exclude California 1099-Gs from our main analyses. Appendix Section A.1 documents that non-UI benefits in the remaining states comprise a small share of total Box 1 payments.

Because we use UI receipt as an indicator for unemployment, the 1099-G identifies only a subset of the truly unemployed. We cannot observe unemployment among non-recipients in the administrative data. As we discuss in Section 6, the magnitude of misreporting among UI

recipients has implications for the broader population of unemployed individuals who do not receive UI.

We additionally link individuals to IRS Form W-2 wage and salary records for 2008-2022. The W-2 data allow us to measure how earnings change in the year of 1099-G receipt relative to prior years. This serves two purposes. We first validate that 1099-G receipt corresponds to genuine job loss and proxies for the severity of unemployment experienced by UI recipients, and it also allows us to predict the number of weeks unemployed experienced by both UI recipients and non-recipients. These analyses are described in Sections 5 and 6.

2.3 Linking Survey and Administrative Data

We link survey and administrative records using anonymized Protected Identification Keys (PIKs) created by the Census Bureau's Person Identification Validation System (PVS, Wagner and Layne 2014). The PVS maps individuals to PIKs based on Social Security Numbers, names, addresses, and dates of birth. PIKs are unique identifiers that enable linkage across our data records that also preserve confidentiality. Between 2010 and 2022, X-Y% of all CPS ASEC respondents are assigned a PIK, while over Z% of 1099-Gs are assigned a PIK.

We restrict our analysis to individuals in the CPS ASEC with a PIK and whose responses are not whole imputed, representing roughly 80 percent to X percent of our sample between 2010 and 2022. To address potential bias arising from non-random PIK assignment and whole imputation, we reweight sample weights by the inverse of the predicted probability that an individual is PIKed and non-whole imputed, conditional on observable characteristics in the CPS ASEC. We adjust weights at the individual level, as our administrative measure of unemployment is at the individual level.

3. Evidence on Misreporting of Unemployment

This section documents the extent and patterns of unemployment misreporting. We begin by examining internal inconsistencies within the CPS ASEC – cases where respondents report receiving UI yet report no unemployment in the same year. We then compare survey responses to administrative records from IRS Form 1099-G to estimate the rate of underreporting directly. Finally, we discuss how the relationship between administrative misreporting rates and within-

survey inconsistencies can be used to extend our estimates to years prior to 2010, for which administrative data are not available.

3.1 Internal Survey Inconsistencies

We first assess the internal consistency of unemployment reporting by focusing on CPS ASEC respondents who report receiving UI benefits during the reference year. If these reports of UI receipt are accurate, then individuals should also report some unemployment during the same year. We calculate the share of survey-reported UI recipients who report no unemployment in the year they reported receiving UI. This within-survey inconsistency rate is displayed in Figure 1 from 1987-2022.

The share of survey UI recipients who report no unemployment is substantial throughout the period, ranging from roughly one quarter to more than half. The pattern is procyclical, with inconsistencies lowest during recessions and highest during expansions. In 2010, during the peak of the Great Recession, the inconsistency rate is among the lowest in the series. In 2018, during a period of economic expansion, it reaches its highest level prior to the pandemic. Since 2020, the inconsistency rate has remained elevated, exceeding 50 percent.

The procyclicality of these inconsistencies suggests that the errors are systematic rather than random. During recessions, when unemployment is widespread and salient, respondents may be more likely to report it accurately. During expansions, when unemployment spells tend to be shorter and less financially significant, respondents may be less likely to report unemployment, whether due to lower salience, stigma, or both. This procyclical pattern implies that misreporting could affect the measured cyclicality of the unemployment rate, a possibility we examine in Section 6.

3.2 Administrative Evidence on Misreporting

We next bring administrative data to bear on the misreporting of unemployment. We classify an individual as unemployed if they received UI benefits in a given year according to IRS Form 1099-G. We then compute the share of these administrative UI recipients who report no unemployment in the CPS ASEC in the same year. This share can be interpreted as a false negative rate for unemployment.

In 2010, 49% of administrative UI recipients reported no unemployment in the CPS ASEC. As a robustness check, we calculate the same rate in the SIPP for 2010 and find a comparable

magnitude of misreporting (43%), despite the SIPP's recall period of four months (roughly a third of the CPS ASEC's annual recall period) and its survey features designed to improve data quality. This result suggests that recall length alone cannot explain the misreporting we document. International evidence points in the same direction, as Sanchez, Koskinen, and Lewis (2014, 2019) find that in Sweden, 10% of the truly unemployed fail to report unemployment over a one-year recall period, and this share increases only modestly to 16% when the recall period is extended to ten years. We later argue in Section 6 that the small change in misreporting observed in the SIPP, despite a shorter recall period, suggests that the misreporting we document is likely present in the official unemployment rate.

Administrative misreporting rates for subsequent years (2011-2022) are not yet disclosed but will be available in time for the NBER EMRI conference in September 2026. However, the within-survey inconsistency rates presented in the previous section (which do not require administrative data) have increased substantially over this period, suggesting that administrative misreporting rights may have risen as well.

For years in which both measures are available, we can compare administrative misreporting rates directly to the within-survey inconsistency rates. If the two series track each other closely over time, the relationship between them can be used to estimate what the administrative misreporting rate would have been in years prior to 2010, when 1099-G data are not available. This would allow us to assess how misreporting has covaried with the unemployment rate over a long horizon, including across multiple business cycles.

4. Heterogeneity in Misreporting of Unemployment

Having documented substantial misreporting of unemployment among UI recipients, we next examine the characteristics of this misreporting. This section investigates how reporting errors vary by reported labor force status, the magnitude of UI benefits received, and demographic characteristics. Understanding these sources of variation provides insight into the mechanisms underlying misreporting and also informs the correction strategies developed in Section 6.

We focus on calendar years 2010 and 2018. In 2010, at the peak of the Great Recession, unemployment was more prevalent. UI receipt was at its highest pre-pandemic level, and maximum benefit durations were extended to 99 weeks. In 2018, during an economic expansion, the number of UI recipients was substantially lower and maximum durations were typically set at

26 weeks. Comparing these two years allows us to examine how misreporting differs between periods when unemployment is common and salient versus periods when it is less common. Results for 2018 and the probit analysis described in Section 4.3 are not yet disclosed but will be available in time for NBER EMRI in September 2026.

4.1 Variation in Misreporting by Labor Force and Employment Status

We begin by examining how administrative UI recipients who report no unemployment characterize their labor market status in the CPS ASEC. Figure 2 presents the distribution of reported labor force status among these individuals in 2010.

One-third of UI recipients who report no unemployment report zero weeks worked and no job search (i.e., being out of the labor force). This is notable given that UI eligibility typically requires active job search. An additional 16.4% report working for part of the year (1-49 weeks), with the majority of these reporting 25-49 weeks of work, usually on a full-time basis. These individuals may have experienced job loss at some point during the year but did not classify the resulting period without work as unemployment (due to reporting not looking for work).

The most striking finding is that 53% of UI recipients who report no unemployment report working nearly the full year (50-52 weeks), with the vast majority reporting full-time, full-year employment. These individuals received UI benefits during a year in which they claim to have been employed for essentially the entire period. This pattern implies that misreporting of unemployment is not limited to marginal cases, but rather misreporting extends to individuals whose survey responses are completely inconsistent with their administrative records. Because these individuals report being employed, the misreporting simultaneously overstates employment and understates unemployment, distorting both statistics.

4.2 Variation by UI Benefit Amount

We next examine how misreporting varies with the amount of UI benefits received. Figure 3 plots the share of 2010 UI recipients who report no unemployment against ventiles of UI benefit amounts. We additionally plot the average annual UI amount in each ventile. We further

decompose the misreported share by reported labor force status for successively narrower subsets of those who report no unemployment.

There is a clear inverse relationship between misreporting and benefit levels. Among UI recipients in the lowest ventile (approximately $500 in UI benefits received), over 80% report no unemployment. Misreporting declines steadily as benefits increase. However, it remains substantial (around 30%) even in the highest ventile (corresponding to over $25,000 in UI received). These are individuals who were likely unemployed for much of the year, yet nearly a third failed to report any unemployment.

The gradient is consistent with salience (or stigma) driving misreporting: individuals with shorter or less financially significant unemployment spells are most likely to fail to report them. The composition of misreporters across the benefit distribution supports this interpretation. In the lowest ventiles, the majority of those who report no unemployment report working. In the highest ventiles, the majority report being out of the labor force. However, even among high-benefit recipients, a notable share report full-year employment: 20.5% in the 10th ventile ($5,100 in benefits), 16.3% in the 15th ventile ($10,300), and 6.1% in the top ventile ($25,200).

The benefit gradient has direct implications for the population of unemployed individuals that we cannot observe in administrative data – those who do not receive UI. If misreporting is most severe among UI recipients for whom unemployment was least financially salient, then non-UI recipients are likely even more susceptible to misreporting, as they received no benefits at all and lack the salience of unemployment that comes with applying for and receiving UI. Non-recipients do not file for benefits, do not certify job search activity, and have no financial anchor to their unemployment status in the way of UI benefits. The benefit gradient therefore suggests that our estimates of misreporting among UI recipients represent a lower bound on misreporting among the broader population of unemployed individuals. This is a central consideration in the correction methodology developed in Section 6, where we must make assumptions about the relative misreporting rates of UI recipients and non-recipients.

4.3 Determinants of Misreporting

We next examine how misreporting varies by demographic and socioeconomic characteristics by estimating probit models in which the dependent variable is an indicator for failing to report any unemployment. The models are estimated on the population of UI recipients,

and we report average partial effects. Regressors (survey reported, unless otherwise noted) include race and ethnicity, geographic region, education level, family income relative to the poverty line, age group, family structure, noncitizen status, sex, metropolitan residence, whether survey-reported UI or weeks worked were item imputed, and the amount of administrative UI benefits received.

We estimate two versions of this model. The first defines "true" unemployment status using survey-reported UI receipt. The second defines unemployment using administrative 1099-G records. Comparing average partial effects across the two specifications highlights the value of administrative data for identifying predictors of misreporting. The survey-based model can only identify predictors of within-survey inconsistencies, whereas the administrative model identifies predictors of actual misreporting against an external benchmark. These results are not yet disclosed but will be available for the NBER EMRI conference.

Three comparisons are of particular interest. First, differences in average partial effects between the survey-based and administrative models would demonstrate the extent to which conclusions about the determinants of misreporting depend on whether the benchmark is internal to the survey or external. Second, if demographic characteristics such as race predict misreporting, this would imply that differential underreporting of unemployment could distort measured unemployment rate differences across groups. Third, comparing results between 2010 and 2018 would provide evidence on whether the predictors of misreporting change between recessions and expansions, with implications for the cyclicality of group-specific unemployment rates.

The results in this section indicate that the underreporting of unemployment is neither random nor evenly distributed. It varies systematically with reported labor force status, the magnitude of UI benefits, and potentially, as the probit analyses will assess, with demographic characteristics. These patterns both inform the interpretation of the headline misreporting rates and the design of the correction methodology in Section 6.

5. Using Earnings Drops to Examine Those Misreporting Unemployment

The preceding sections use 1099-G receipt as an administrative indicator of unemployment. The validity of this approach rests on the assumption that receiving UI benefits

reflects a genuine unemployment spell. In this section, we assess this assumption by examining whether 1099-G recipients experience earnings declines consistent with job loss, using linked W-2 earnings data. We define two measures of earnings declines and compare them across groups to test whether the discrepancies between administrative and survey measures of unemployment reflect survey misreporting rather than misclassification in the administrative data. Appendix Section A.1 supplements this analysis by documenting that, after excluding California, non-UI benefits appearing on 1099-G forms are small relative to UI benefits.

5.1 Why Validation is Needed

UI is designed to compensate for earnings losses following no fault involuntary job loss. Receipt of UI should therefore coincide with an unemployment spell during the year of receipt. However, there are limited circumstances in which a 1099-G may not correspond to genuine unemployment. In some states, PFL or STDA payments may appear on Box 1 of a 1099-G. Partial UI, where individuals work part-time while receiving prorated benefits, could result in 1099-G receipt without a clear period of unemployment. Additionally, UI fraud could generate 1099-G records for individuals who were not actually unemployed.

Each of these cases would lead us to erroneously classify an individual as unemployed only if the 1099-G receipt was not accompanied by any unemployment spell elsewhere in the same year. Appendix Section A.1 uses publicly available data to show that these cases are uncommon and cannot account for the systematic misreporting patterns documented in Sections 3 and 4. The earnings drop analysis that follows provides a more direct empirical assessment.

5.2 Measuring Earnings Declines

By design, UI compensates for earnings losses following involuntary job separation. Therefore, receipt of UI should coincide with a loss in earnings and would provide strong evidence of an unemployment spell during the year of receipt.

We measure changes in annual W-2 earnings for 1099-G recipients in the year of receipt relative to earnings in the two prior years, conditional on having earnings in at least one of the two prior years. Using two prior years allows us to more accurately compare against earnings that are unaffected by unemployment (i.e., if an individual's unemployment spell began in the year prior

to UI receipt). We construct two measures of earnings declines for both 2010 (relative to 2008-2009 earnings) and 2018 (relative to 2016-2017 earnings).

Our first measure of earnings declines is continuous. For each individual, we compute the proportional decline in earnings between the year of 1099-G receipt and the higher of the two prior years' earnings:

$$\%\ Earnings\ Drop_{it} = \frac{\max\{Earnings_{i,t-1}, Earnings_{i,t-2}\} - Earnings_{i,t}}{\max\{Earnings_{i,t-1}, Earnings_{i,t-2}\}}$$

Under the assumption that earnings would have remained constant absent unemployment, this measure approximates the share of the year spent unemployed. A 50% decline, for example, would correspond to roughly half a year out of work if the individual resumed comparable earnings after the unemployment spell. We report the median of this measure to limit the influence of extreme values among individuals with very low prior earnings.

Our second measure is discrete and captures the severity of unemployment relative to UI benefits received. For each individual, we construct an indicator equal to one if the dollar earnings decline exceeds twice the total UI benefits received. The threshold of twice the UI amount corresponds to a replacement rate of approximately 50%, or roughly the statutory target replacement rate in most states. The share of individuals for whom this indicator equals one represents the share whose earnings losses exceed what UI would be expected to replace. Because this measure requires observed UI benefit amounts, it can only be computed for UI recipients.

5.3 Validation Comparisons

To assess whether earnings declines validate 1099-G receipt as a measure of unemployment, we conduct two sets of comparisons. Results are not yet disclosed but will be available in time for the NBER EMRI conference in September 2026.

We first compare the earnings drops of 1099-G recipients who report no unemployment to those of 1099-G recipients who report some unemployment, using both the continuous and discrete measures. If the two groups exhibit similar magnitudes of earnings declines, this would indicate that non-reporters experienced comparable job loss. It would also indicate that the difference in

survey reports of unemployment reflects reporting behavior rather than different labor market experiences.

We then compare earnings declines among 1099-G recipients who report no unemployment to earnings declines among non-UI recipients who report some unemployment. Because the discrete measure requires observed UI benefit amounts, this comparison uses the continuous measure only. If 1099-G recipients who report no unemployment have earnings declines similar to or larger than those of non-recipients reporting unemployment, this would provide strong evidence that 1099-G receipt is at least as reliable a measure of unemployment as a survey response.

6. Implications of Misreporting for the Unemployment Rate

The preceding sections document substantial underreporting of unemployment among UI recipients in the CPS ASEC and comparable misreporting in the SIPP, which uses a shorter recall period. These findings raise the question of whether the official unemployment rate, which is derived from near-contemporaneous survey responses in the CPS Basic Monthly Survey, is similarly affected. Respondents to the CPS Monthly are interviewed during the week containing the 19th of the month and asked about work activities during the week containing the 12th. They are also asked about job search and layoff within the previous four weeks. These responses are combined to determine whether an individual is unemployed, which are then used to calculate the official unemployment rate. Respondents to the CPS ASEC are drawn from the same sample as the CPS Monthly, and the two measures of unemployment from each survey are closely related.

In this section, we develop methods to correct a retrospective unemployment rate constructed from the CPS ASEC and argue that corrections are informative about bias in the official rate. We begin by defining the retrospective rate and documenting its relationship with the official rate. We then present a simple incidence-based correction that does not rely on predicting individual-level misreported unemployment. We also correct for false positives and assess the magnitude of errors in unemployment duration. We then describe a more detailed correction that predicts weeks of unreported unemployment from administrative earnings data. The results of both the incidence-based and more detailed corrections are not yet disclosed but will be available in time for the NBER EMRI conference in September 2026.

6.1 Constructing the Retrospective Unemployment Rate

The CPS ASEC asks respondents to report weeks spent unemployed and weeks spent working over the previous calendar year. From these responses, we construct a retrospective unemployment rate, which we define as the share of total weeks in the labor force spent unemployed. This measure is duration-weighted, giving greater weight to individuals with longer unemployment spells. It is distinct from the official unemployment rate, which captures the incidence of unemployment during a single reference week each month.

Absent any misreporting, the retrospective rate (which we also call the ASEC rate) is a weighted average of the monthly official rate, where the weights reflect the size of the labor force in each month. The 1099-G data capture UI receipt in the same calendar-year reference period as the ASEC, which allows us to use our estimates of misreporting to directly correct the ASEC rate.

Figure 4 plots the retrospective ASEC rate alongside the annual average of the CPS Monthly unemployment rate from 1975 to 2022. The two series track each other closely, with the ASEC rate consistently falling below the official rate. The correlation between them is 0.99 over this period. Figure 5 plots the ratio of the official rate to the ASEC rate over time, with the official rate being on average 20% higher than the ASEC rate and no strong secular trend. Figure 6 additionally plots the percentage point difference between the two rates, with an average difference of one percentage point and similarly showing no secular trend. The high correlation between the two rates implies that corrections to the ASEC rate are informative about the official rate. The stability of the ratio and difference over time implies that corrections estimated from our 2010-2022 linked data can plausibly be applied to earlier years, allowing us to assess how misreporting has affected unemployment rate across the business cycle. We proceed by estimating the total number of weeks unemployed that are unreported (or overreported) due to different sources of survey errors.

6.2 A Simple Incidence-Based Correction to the Retrospective Unemployment Rate

We first develop a simple correction to the unemployment rate that does not rely on predicting individual-level weeks of unreported unemployment. Our more detailed method uses declines in earnings observed in administrative data to predict the number of weeks a truly

unemployed individual reporting no unemployment was unemployed. Recent evidence that earnings are substantially volatile even in the absence of unemployment (Ganong et al. 2025) raises concerns about using earnings drops to predict unemployment duration. The incidence-based approach outlined here avoids this issue by operating on counts of individuals rather than predicting weeks of unemployment from changes in earnings.

The incidence approach proceeds in five steps. First, we use the 1099-G data to divide individuals who report any unemployment in the CPS ASEC into two groups: those who receive UI on the 1099-Gs and those who do not. Second, we scale up the count of UI recipients reporting unemployment by the inverse of the administrative reporting rate among UI recipients. This is equivalent to treating all administrative UI recipients as having experienced some unemployment, regardless of whether they reported it. In 2010, the reporting rate among UI recipients is 51%, yielding a scaling factor of 1.96.

Third, among non-UI recipients who report unemployment, we subtract an estimate of false positives (individuals who experienced no unemployment who report some unemployment in the survey) based on misclassification rates estimated from Poterba and Summers (1986). We discuss the estimation of false positives further in Section 6.5. Fourth, we scale up the remaining non-UI recipients reporting unemployment to account for non-reporters who were truly unemployed. We construct bounds on this scaling factor using reported rates estimated among UI recipients. The lower bound scaling factor is equal to the inverse of the average UI recipient unemployment reporting rate, which assumes that non-recipients misreport at the same rate as the average UI recipient. The upper bound scaling factor is equal to the inverse of the reporting rate among the lowest benefit ventile group, which assumes that non-recipients misreport at the rate of the UI recipients who receive the lowest amount of benefits. The lower and upper bound scaling factors for 2010 are equal to 1.96 and 5.0 respectively. These translate into a doubling of non-UI recipients who report some unemployment at the lower bound to a fivefold increase at the upper bound.

The incidence correction recovers an estimate of the true count of unemployed individuals in each group (UI recipients, non-UI recipients). The final step involves converting from the estimate of the count of unemployed individuals to a duration-weighted retrospective rate (i.e., total weeks of unemployment). To arrive at this correction, we weight each group's corrected count of unemployed by the average reported weeks of unemployment of those who reported unemployment. The key assumption underlying this weighting is that non-reporters within each

group have average spell lengths similar to reporters within the same group. Under this assumption, the incidence-based approach recovers a correction to the duration-weighted rate using survey-reported durations alone, without requiring individual-level predictions from earnings drops.

6.3 A Detailed Correction Using Earnings Drops for False Negatives Among UI Recipients

We additionally develop a more detailed correction to the ASEC unemployment rate that predicts weeks of unreported unemployment from earnings drops observed in administrative W-2 data. This approach operates directly on the duration-weighted rate and estimates unreported weeks unemployed for individual false negatives.

For each individual with W-2 earnings in at least one of the two preceding years, we define the percentage earnings drop in year t as the proportional decrease in that year's earnings relative to the maximum of the two preceding years, expressed as a fraction of the year. We also adjust earnings for growth in average W-2 earnings observed in the universe of tax records to account for secular wage growth.

Under the assumption that counterfactual earnings absent unemployment would equal prior earnings, that reemployment wages approximately equal prior earnings, and that the individual is searching for work while not employed, the percent earnings drop approximates the proportion of year t spent unemployed. Violations of these assumptions include wage growth that deviates from the overall average and earnings volatility unrelated to unemployment (Ganong et al. 2025). We use this measure as the primary predictor of weeks spent unemployed.

We first start by predicting weeks unemployed for UI recipients. Among UI recipients who report some unemployment, we estimate a prediction equation with reported weeks unemployed on the left-hand side and a flexible function of the earnings drop on the right-hand side. Our preferred specification for the flexible function of the earnings drop is a linear spline, with indicators and slope coefficients for negative earnings drops (which allow for the relationship between earnings increases and weeks unemployed to differ from the relationship for earnings decreases). For individuals without prior earnings, we assign the average reported weeks unemployed among reporters who also lack prior earnings.

Implementing the earnings drop prediction equation among UI recipients in all states and years requires confidence that the predicted weeks unemployed are meaningful. To validate this approach, we independently estimate weeks of UI receipt using a second method based on administrative UI benefit amounts, W-2 earnings histories, and state-specific benefit rules. We conduct this approach for states with simple UI benefit calculations. Weeks receiving UI are calculated as total UI benefits received (observed on 1099-Gs) divided by the weekly benefit amount, which is determined by the highest quarter of earnings in a base period and state rules. Because we only observe annual earnings, we bound the high quarter of earnings using two assumptions. The lower bound assumes that earnings are distributed uniformly across quarters, and the upper bound assumes earnings are maximally concentrated subject to monetary eligibility constraints. These bounds translate into bounds on estimated weeks of UI receipt.

We assess the plausibility of these estimates against both internal distributional properties and external benchmarks (Department of Labor data on benefit exhaustion rates and average weekly benefit amounts by state and year).

The primary purpose of this exercise is to validate the earnings drop prediction equation. In states with simple benefit formulas where both estimates are available, we assess whether predicted weeks unemployed from earnings drops and estimated weeks from state rules are closely related at the individual level. A strong relationship would justify applying the earnings drop approach across all states and years without needing to model state-specific benefit rules nationally.

6.4 Correcting for False Negatives Among Non-UI Recipients

Our results document large false negatives among UI recipients, and the previous section outlined methods for predicting weeks unemployed among UI recipients who report no unemployment. Several features of UI receipt suggest that misreporting among non-recipients is at least as large. UI recipients have actively applied for and received benefits, creating a direct connection to their unemployment spell that non-recipients lack. UI recipients face ongoing search requirements as a condition of benefit receipt, reinforcing awareness of their unemployment status. Additionally, we have shown in Section 4 that among UI recipients, misreporting is highest among those with the smallest benefits. Non-recipients, who receive no benefits at all, are a natural extension of this gradient.

In order to estimate the number of unreported weeks unemployed among non-UI recipients, we estimate a similar prediction equation as for UI recipients. We estimate the prediction equation using earnings drops, as outlined in the previous section. Our sample consists of survey respondents who report unemployment but did not receive UI in the administrative data. This sample more closely resembles the non-recipient false negatives whose weeks unemployed we seek to predict. Finally, we restrict the sample to those with positive W-2 earnings in at least one preceding year and at least one following year, ensuring that earnings drops used to predict weeks reflect a temporary separation from employment rather than permanent labor force non-participation or exit. This strategy assumes that the rate of underreporting among non-UI recipients is comparable to or larger than the rate among UI recipients. Under this assumption, the resulting corrections are lower bounds on the true adjustment.

6.3 Correcting for False Positives

The core result of this paper is the documentation of significant false negatives among UI recipients. The presence of false negatives might bias the retrospective unemployment rate downward. False positives (or individuals who report some unemployment when they actually were not unemployed) might bias the rate upward. If false positives are quantitatively important, correcting only for false negatives would overstate corrections to the retrospective unemployment rate.

Unlike false negatives among UI recipients, false positives cannot be identified in the administrative microdata. We draw on Poterba and Summers (1986), who analyze CPS reinterview data from 1981 to estimate misclassification rates in reported labor force status. The CPS Reinterview Survey recontacts a subset of initial respondents to the CPS Monthly one week after their initial interview and asks about labor market status during the original reference week. In 80% of cases, the reinterview is reconciled, and the interviewer has access to initial responses and, when responses conflict, attempts to determine which response is correct. Poterba and Summers estimate a corrected false positive rate for unemployment of 0.54%.

Incorporating false positive rates estimated by Poterba and Summers requires two assumptions. The first is that the false positive rate estimated from the monthly CPS generalizes to the retrospective ASEC measure. The second assumption assumes that the rate estimated from 1981 data is applicable to subsequent years. There is some reason to question these assumptions.

The Poterba and Summers false negative rate of 15.24% is substantially lower than the 49% rate we estimate in the 2010 CPS ASEC. If false negative rates are higher in the ASEC than in the monthly CPS, false positive rates may be as well. We therefore also consider scaling the Poterba and Summers false positive rate by the ratio of our false negative rate to theirs. If the net adjustment to the retrospective rate is not sensitive to the choice between the baseline and scaled false positive rates, this would suggest that the false positive correction is second-order relative to false negative corrections.

6.4 Assessing Errors in Duration

A separate source of bias could arise if individuals who correctly report being unemployed misreport the number of weeks they were unemployed. If true reporters systematically understate their weeks unemployed, the number of weeks unemployed (the numerator of the ASEC rate) is biased downward even after correcting for false negatives. Systematic overstatement of weeks unemployed would have the opposite effect.

We assess the magnitude of intensive margin errors by comparing reported weeks unemployed among UI recipients who correctly report unemployment to estimated weeks of UI receipt from administrative data, using the same methodology for validating our earnings drop prediction equation. If reported weeks unemployed and estimated weeks receiving UI are closely related (with reported weeks unemployed potentially exceeding weeks receiving UI, as individuals may be unemployed before or after receiving benefits), this would suggest that true reporters are reasonably accurate in their duration reports.

We assume that the magnitude of intensive margin errors among UI recipients reporting unemployment is informative about errors among non-UI recipients reporting unemployment. This is consistent with the maintained assumption throughout this paper that UI recipients represent a lower bound on misreporting. If the validation suggests that intensive margin errors are small, we proceed with false negative and false positive corrections alone. If they are substantial an additional correction is needed.

6.5 Implications for the Cyclicality of Unemployment

The within-survey inconsistency rates documented in Figure 1 are procyclical, with inconsistencies lowest during recessions and highest during expansions. This pattern suggests that

accounting for the underreporting of unemployment would change the measured cyclicality of the unemployment rate. However, the effect of survey error corrections on cyclicality depends on two competing forces. In expansions, inconsistencies are higher, but the total stock of UI recipients should be smaller. In recessions, inconsistencies are lower, but the number of UI recipients should be larger. Whether the corrected unemployment rate is more or less cyclical than the published series depends on which force dominates.

By applying our correction methodology across the years for which we have linked administrative data (2010-2022) and potentially extending to earlier years using the relationship between administrative misreporting rates and within-survey inconsistencies, we can directly assess how the corrected unemployment rate compares to the published series across the business cycle. These results are not yet disclosed but will be available in time for NBER EMRI in September 2026.

7. Conclusion

This paper documents substantial and systematic misreporting of unemployment in U.S. household surveys and develops methods in using linked survey and administrative data to correct for this misreporting. By linking the CPS ASEC to IRS Form 1099-G data, we find that 49% of UI recipients in 2010 reported no unemployment in the same year they received UI, with the majority instead reporting full-year employment. We find comparable levels of misreporting in the SIPP despite its shorter reference period and features designed to reduce misreporting. We also document that within-survey inconsistencies between reported UI receipt and unemployment have roughly doubled from the early 1990s to 2022. Because employment, unemployment, and labor force participation are jointly measured, these errors simultaneously distort employment and labor force participation statistics as well.

The misreporting that we document is not random. Among UI recipients who report no unemployment, 53% report working nearly the full year, usually on a full-time basis. Misreporting varies systematically with the magnitude of UI benefits received, with 80% of recipients in the lowest ventile of benefits reporting no unemployment, compared to about 30% in the highest ventile. This gradient suggests that salience or stigma play a role in reporting accuracy. This gradient also has direct implications for the broader population of unemployed individuals that we cannot observe in administrative records. Non-UI recipients who are unemployed receive no UI

benefits from their unemployment, do not certify job search, and lack institutional connectedness to unemployment that comes with filing for and receiving UI. This plausibly makes misreporting among non-recipients at least as large as among recipients.

We develop methods to correct a retrospective unemployment rate constructed from the CPS ASEC for three sources of misreporting: false negatives, false positives, and errors in reported duration. A simple incidence-based approach scales up the count of unemployed individuals using administrative reporting rates and weighs them by average reported weeks unemployed. A more detailed approach predicts weeks of unreported unemployment from earnings drops observed in W-2 data.

We document that the retrospective unemployment rate constructed from the CPS ASEC tracks the official unemployment rate closely, with a correlation of 0.99 between the two and a stable relationship over time. Given this close relationship, we argue that corrections to the retrospective rate are informative about bias in the official rate. Full numerical results from these corrections will be available in time for the NBER EMRI conference in September 2026.

Our findings have broad implications. They add to a literature that uses linked survey-administrative data to revise our understanding of economic conditions, extending this agenda from income and poverty measurement to labor market statistics derived from surveys. They also bear on a large body of macroeconomic research that takes survey-based measures of unemployment as accurate inputs. If unemployment is systematically understated and misreporting is procyclical, estimates of the Phillips Curve, Okun's Law, and the natural rate of unemployment may be based on a mismeasured input with cyclically varying errors. Beyond these specific implications, our results point to a broader gap in the U.S. statistical system. Unlike GDP, inflation, and income, survey-based measures of employment and unemployment are not typically compared against administrative benchmarks. Closing this gap will require ongoing access to high-quality administrative data on unemployment and employment spells, as well as methods of the kind developed here for integrating these data with existing surveys.

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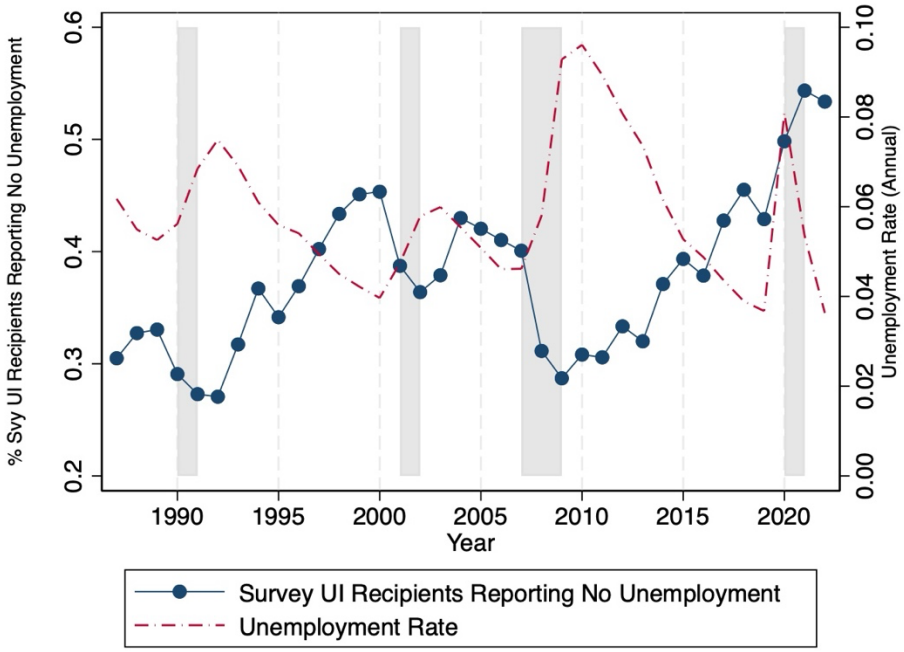
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Figures

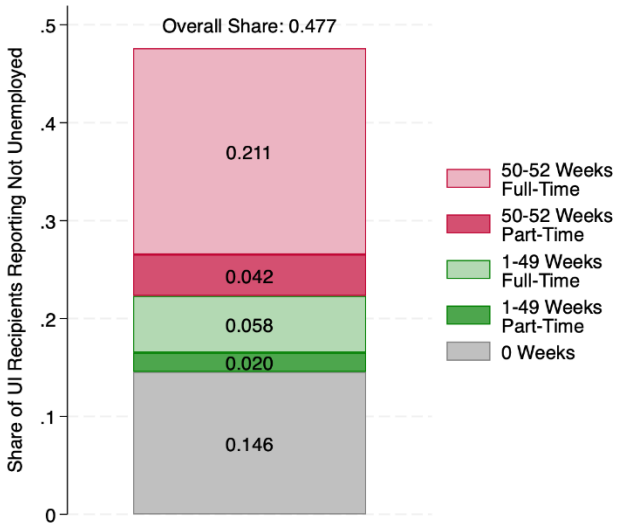
Figure 1. Percentage of Survey UI Recipients Reporting No Unemployment



Source: 1976-2023 CPS ASEC (reference years 1975-2022)

Notes: This graph shows the percentage of survey UI recipients who report no unemployment in the survey and the official unemployment rate over time. An individual is classified as a survey UI recipient if they report receiving UI in the CPS ASEC. An individual is classified as reporting no unemployment in the survey if they report either working for the full year or if they do not report working for the full year, also spending no time looking for work or on layoff while not working. Grey bars indicate official recessions.

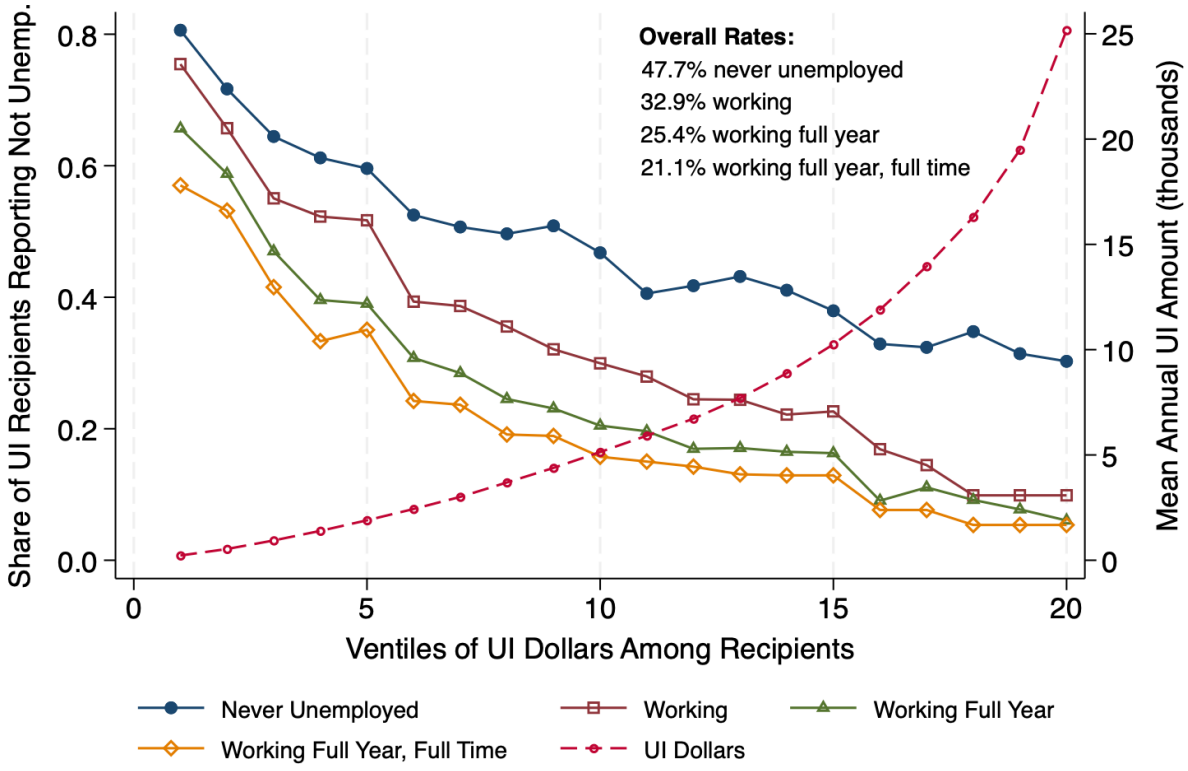
Figure 2. Distribution of Administrative Underreporting of Unemployment by Survey Reported Labor Force and Employment Status, 2010



Source: 2011 CPS ASEC (Reference year 2010) and IRS Form 1099-G (Tax year 2010)

Notes: This figure shows the distribution of admin error rates in 2010 by labor force and weeks worked. Labor force status and weeks worked are obtained via survey responses to questions on usual workforce activities and number of weeks worked in the year. Note that individuals for whom weeks worked or UI receipt were item imputed are excluded from this figure, which is why the total share (47.7%) does not align with our headline result.

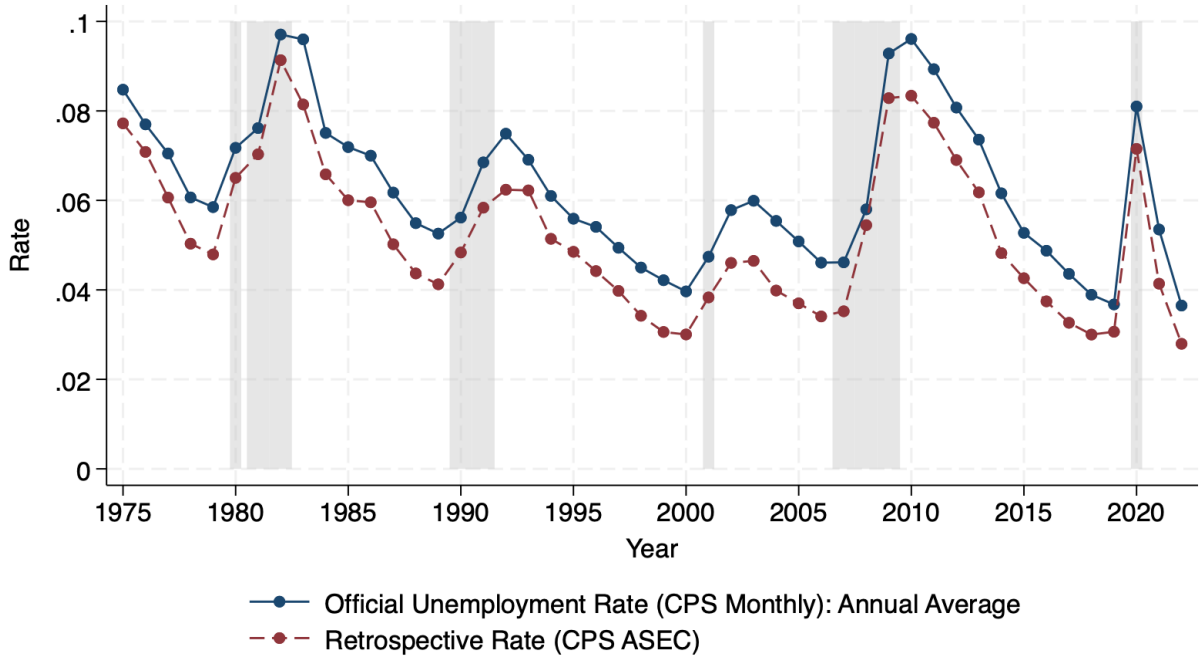
Figure 3. Magnitudes of Unemployment Errors (Split by Employment Status) by UI Amount, 2010



Source: 2011 CPS ASEC (Reference year 2010) and IRS Form 1099-G (Tax year 2010)

Notes: This figure shows the percentage of administrative UI recipients in 2010 who report no unemployment by ventile of administrative UI amount received. This figure also plots the total UI amount within each ventile. Within each ventile, we also show how shares of misreporters vary by successively narrower subsets of reporting no unemployment. Employment status is taken from survey reports. An individual is defined as working for the full year if they respond that they worked for 50-52 weeks in 2010. An individual is defined as working full-time if they report usually working on a full-time basis in 2010.

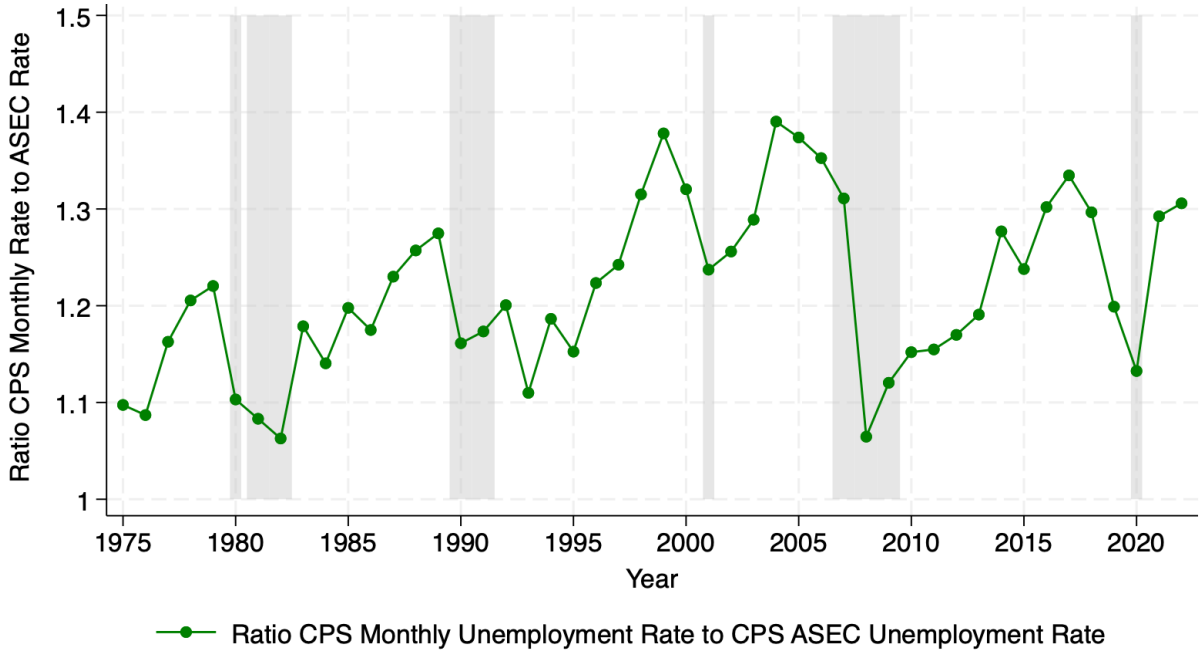
Figure 4. Comparison of the Retrospective Unemployment Rate and Official Unemployment Rate Over Time, 1975-2022.



Source: 1976-2023 CPS ASEC (Reference years 1975-2022)

Notes: This figure displays the retrospective unemployment rate and the official unemployment rate between 1975 and 2022. The retrospective unemployment rate is defined as the percentage of reported weeks in the labor force that were unemployed. The official unemployment rate is the annual average of the monthly unemployment rate within a given year. The correlation between the two series is equal to 0.99. The grey bars indicate recessions.

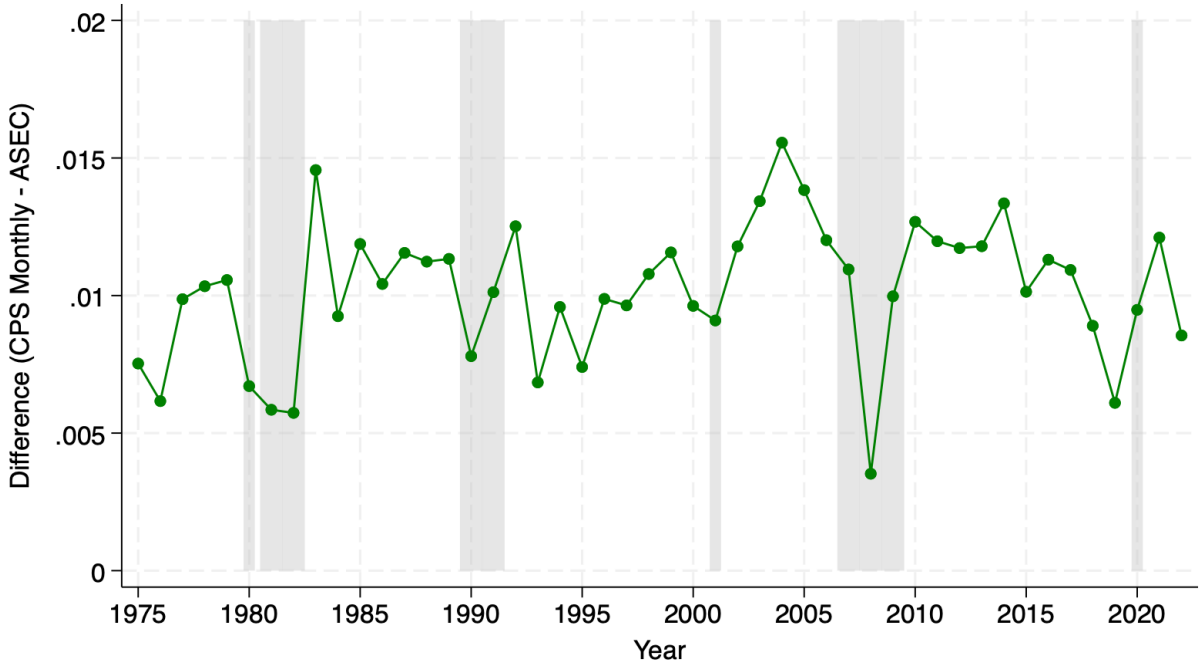
Figure 5. Ratio of Average Official Rate to Retrospective Rate, 1975-2022.



Source: 1976-2023 CPS ASEC (Reference years 1975-2022)

Notes: This figure displays the ratio of the annual average of the official unemployment rate to the CPS ASEC retrospective rate between 1975 and 2022. The retrospective unemployment rate is defined as the percentage of reported weeks in the labor force that were unemployed. The official unemployment rate is the annual average of the monthly unemployment rate within a given year. The average of this series is equal to 1.20. The grey bars indicate recessions.

Figure 6. Difference between Official Rate and Retrospective Rate, 1975-2022.



Source: 1976-2023 CPS ASEC (Reference years 1975-2022)

Notes: This figure displays the difference between the annual average of the official unemployment rate and the retrospective unemployment rate between 1975 and 2022. The retrospective unemployment rate is defined as the percentage of reported weeks in the labor force that were unemployed. The official unemployment rate is the annual average of the monthly unemployment rate within a given year. The average difference is equal to 0.01. The grey bars indicate recessions.

Appendix

Appendix A: Assessing Potential Sources of Misclassification in 1099-G Based Unemployment Measures

Our main analysis identifies those who experienced unemployment in a year by receipt of IRS Form 1099-G. This approach assumes that Box 1 of the 1099-G primarily captures UI benefits paid to unemployed individuals. However, several other programs or scenarios, such as Paid Family Leave (PFL), Short-Term Disability Assistance (STDA), Partial UI, and fraud, can also cause us to mistakenly identify an individual as unemployed by receipt of a 1099-G. This appendix evaluates each of these potential concerns and demonstrates that they are unlikely to materially affect our results.

A.1 Paid Family Leave and Short-Term Disability Assistance

A small number of states operate PFL and STDA programs that can pay out benefits on IRS Form 1099-G in the same box as those used to identify UI benefits. Because we identify the unemployed through non-zero benefits being recorded in Box 1, it is important to verify that these non-UI payments are quantitatively minor. Otherwise, we might mistakenly classify an individual as unemployed who might not actually be unemployed. The possible non-UI programs that can be paid out on Box 1 include Paid Family Leave and Short-Term Disability Assistance.

PFL provides partial wage replacement to workers taking leave to care for a new child or an ill family member. As of 2022, eight states operated state-level PFL programs with benefits appearing on 1099-Gs. STDA replaces wages for workers temporarily unable to work due to non-work-related illness or injury. Only three states – Massachusetts, California, and Washington have the potential to issue STDA benefits on 1099-Gs. In California and Washington, these benefits only appear on 1099-Gs if the individual was previously on UI. If so, the search requirement for UI is waived, and individuals could then receive a 1099-G and truthfully report no unemployment.

However, this is only the case for a subset of those on STDA – it is likely that the vast majority of those on STDA in these two states do not have their benefits appear on 1099-Gs.

Appendix Figure 1 presents the size of total PFL and STDA benefits as a share of UI benefits paid out by states offering these programs for which we have publicly available sources of data. PFL and STDA benefits are extremely small relative to UI benefits paid out in these states, consistently remaining low across multiple states with the exception of 2022 (in Washington) and in California. Because California STDA benefits are extremely large relative to the size of UI in California and these benefits have the potential to appear on 1099-Gs, we exclude California from our main analysis in this paper.

When we exclude California and take the ratio of total non-UI benefits that could appear on 1099-Gs to total UI benefits paid out in Appendix Figure 2, we see that non-UI benefits as a share of UI benefits nationally are extremely low, and increase above 10% in 2022. Given this, we replicate our main set of misreporting results (once disclosed) excluding all PFL/STDA states in 2022. However, these checks serve to validate that it is unlikely that PFL and STDA benefits causing us to misclassify recipients as unemployed are driving our results.

A.2 Partial UI and Workshare

Most states allow partial Unemployment Insurance for individuals whose hours have been cut. Workshare programs also provide prorated UI benefits to groups of employees whose hours are reduced by their employers (as an alternative to laying people off). These represent cases where an individual could receive a 1099-G and truthfully report no unemployment in the same year of receipt.

However, these cases appear to be small. Appendix Figure 3 plots the percent of all UI weeks claimed that were for partial UI or workshare, pulled from U.S. Department of Labor public totals.[2] The percent of all UI weeks claimed that were for partial UI or workshare is typically below 10%, with recessions having slightly higher totals. Given that these shares are small, it is unlikely that the pronounced underreporting that we identify are partial UI recipients. Additionally, if individuals who receive partial UI end up on full UI within the same calendar year, then the error rates that we identify are unbiased by the presence of partial UI.

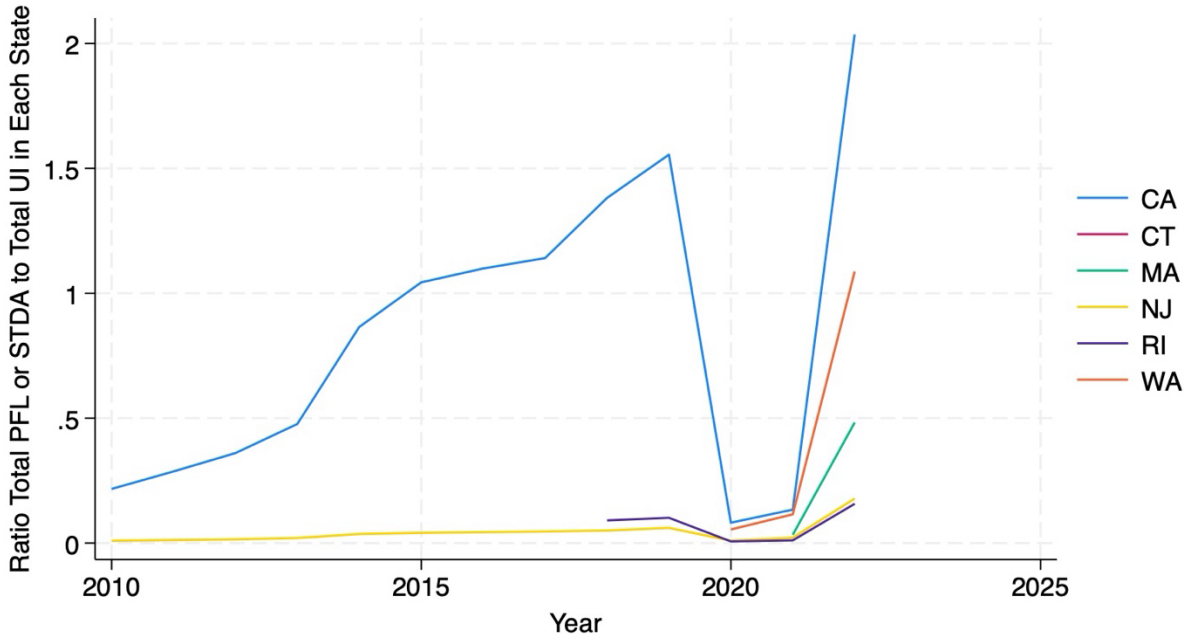
[2] The Department of Labor does not publish totals for number of recipients.

A.3 UI Fraud

Fraudulent UI claims could, in principle, create 1099-G records not associated with genuine unemployment (assuming that the individual committing fraud is not unemployed). However, evidence suggests that this channel is quantitatively small for most years prior to 2020. The U.S. Department of Labor estimated that improper payments accounted for about 2.25 percent of UI benefits in 2010. In 2020, the Government Accountability Office later estimated 11-15% of UI payments going to fraud during the pandemic, largely due to Pandemic Unemployment Assistance (PUA). Additionally, Isen, Kuka and Stuart (2025) estimate that 24% of all UI benefits paid in 2020 being anomalous, with most of these concentrated in PUA. Most of the estimates presented in this paper (or eventually presented post disclosure) are for 2010 and 2018 prior to the pandemic, when fraud has been estimated to be more prevalent. Moreover, fraudulent claimants who are in fact unemployed (for example, an individual quitting their job and looking for a new job while receiving UI despite not being eligible) would not bias our error rates, since their true labor-force status aligns with UI receipt.

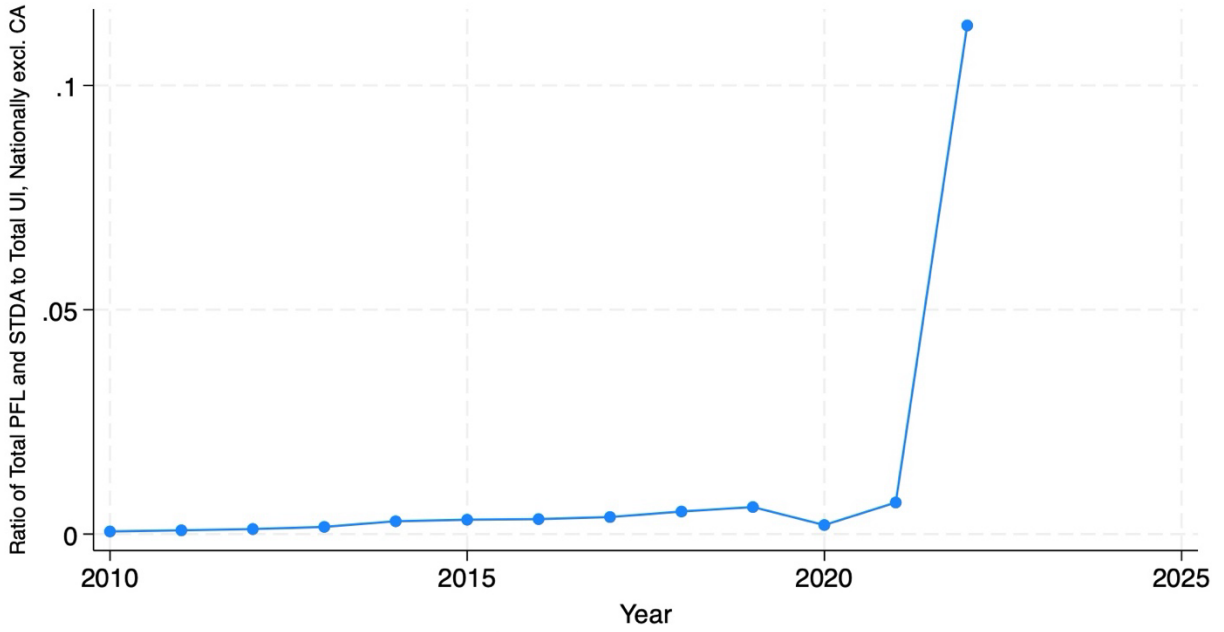
Appendix Figures

Appendix Figure 1. Ratio of PFL and STDA Dollars to UI Dollars Within States Offering Programs, by State



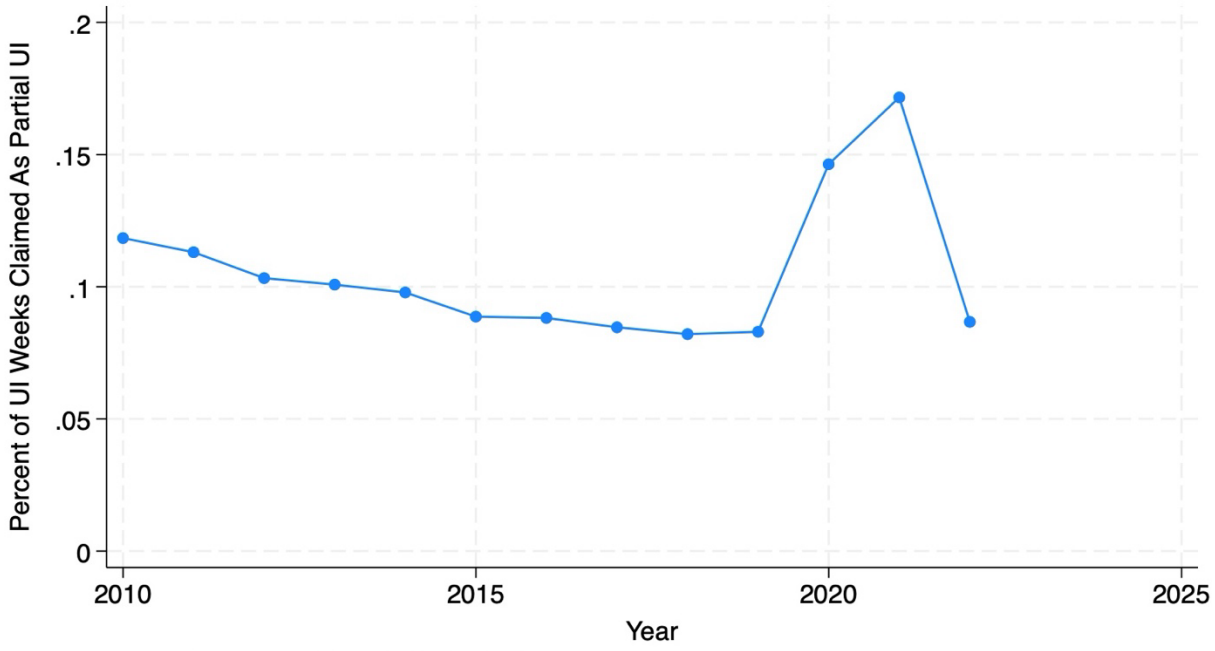
Sources: Department of Labor ETA 2112; State Public Administrative Aggregates
Notes: This graph displays the ratio of total PFL and STDA dollars to total UI dollars over time within each state. PFL benefits in DC, NY and RI (2014-2017) are not included due to lack of data.

Appendix Figure 2. Ratio of PFL and STDA Dollars to UI Dollars Nationally, Excluding California



Sources: Department of Labor ETA 2112; State Public Administrative Aggregates
Notes: This graph displays the ratio of total PFL and STDA dollars to total National UI dollars over time. This graph exclude benefits paid in California. PFL benefits in DC, NY, and Rhode Island (2014-2017) are not included due to lack of data.

Appendix Figure 3. Percent of All UI Weeks Claimed for Partial UI/Workshare Over Time



Source: Department of Labor ETA 9050/9051
Notes: This graph displays the percentage of total UI weeks claimed that were attributable to partial UI.