

Bias in the Level, Trend, and Cyclical 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 47.7% of UI recipients in 2010 erroneously reported no unemployment in the same year, most of whom instead reported full-year employment. Underreporting decreases as UI benefits increase, from 80% of UI recipients in the lowest ventile of benefits to 30% in the highest ventile. This suggests that salience plays a central role in reporting accuracy. Combining these results with existing estimates of false positives, we develop a simple correction to a retrospective unemployment rate derived from the CPS ASEC. Given the close and stable relationship between the retrospective and official unemployment rates, we argue that this correction is informative about bias in the official rate. Assuming non-UI recipients underreport unemployment as often as the average UI recipient, our correction raises the 2010 official rate from 9.6% to 14.6%. When we extend the correction to 1987-2024 with public-use data, the corrected rate averages 48-72% above the published rate (depending on the method used), with the size of the corrections having grown since 2010.

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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 errors in the reporting of economic activities (e.g., Bound, Brown, and Mathiowetz 2001; Meyer, Mok, and Sullivan 2015), raising the question of whether survey-based labor market statistics are similarly affected.

The unemployment rate illustrates the stakes especially 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 the unemployment rate 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. Outside of empirical research and government policy, accurate measurement of unemployment plays a critical role in informing business decisions. In a survey conducted by the National Association of Business Economists, 95% of respondent businesses indicated that government data was used to inform their decisions, and of these respondents, employment and unemployment data were rated the most important type of government data to inform business decisions (Hughes-Cromwick and Coronado, 2019). If the measured unemployment rate is systematically biased, then core macroeconomic relationships could be distorted and policy responses during recessions and recoveries could be incorrectly calibrated.

Policymakers and researchers have long raised concerns that official labor market statistics may suffer from bias. In a December 2025 press conference, Federal Reserve Chair Jerome Powell described a “systematic overcount” in payroll job growth (Board of Governors of the Federal Reserve System 2025, pg. 19), suggesting that labor market conditions were weaker than the headline figures implied. Research has also documented the presence of errors in survey-reported labor force status (Poterba and Summers 1986) and estimated a downward bias in the official unemployment rate of an average of 2.1 percentage points between 1996 and 2011 (Feng and Hu 2013). However, these studies have tended to rely on inconsistencies in repeated reports of labor

force participation or structural models rather than direct linkages to administrative data on unemployment.

Other economic statistics are routinely checked against independent sources. GDP is cross-checked using 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). The accuracy of employment measures has been examined in a substantial literature (e.g., Bound and Krueger 1991, Bollinger 1998, Abowd and Stinson 2013, Bollinger et al. 2019, Abraham et al. 2013, Abraham et al. 2021). However, survey-based measures of unemployment have not been compared against administrative measures of truth in the U.S., 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. 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 confirm that the presence of a 1099-G indicates unemployment in the vast majority of cases. We assess the validity of this administrative measure of unemployment by confirming using linked W-2 records that 1099-G receipt corresponds to a genuine decline in earnings consistent with job loss even when unemployment is not reported.

In this version, 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. It is the only year for which estimates using the linked 1099-G data have been disclosed to date, with disclosure for reference years 2011 through 2023 currently underway. We find that 47.7% of individuals who received UI in 2010 reported no unemployment in the CPS ASEC in the same year, with the majority of underreporters instead reporting full-year employment. We also find comparable levels of underreporting in the Survey of Income and Program Participation (SIPP), despite its shorter recall

period and design features intended to reduce recall error, including multiple interviews over a given calendar year. 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 received (approximately \$500 in UI), 80% reported no unemployment, compared to 30% in the highest ventile (approximately \$25,000). This gradient suggests that salience 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 in the public-use CPS ASEC between 1987 and 2024, we find that within-survey inconsistencies between reported UI receipt and reported unemployment have roughly doubled from the early 1990s to 2024, with inconsistencies lowest during recessions and highest during expansions.² The forces underlying this cyclical pattern of misreporting may be similar to the benefit gradient: unemployment may be more accurately reported when it is more salient, whether because of larger individual benefits or because of its rarity in good 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 in surveys during expansions than during recessions, then the cyclicity of the true unemployment rate will differ from what published series suggest.

² Estimates using the linked 1099-G records have been disclosed only for 2010 to date, so we rely on public-use data for the time series. We are extending the linked analyses forward from 2010, which will let us assess how closely within-survey inconsistency rates track underreporting measured against administrative records. However, because the administrative records are only available starting in 2010, the public-use data will remain our only evidence on the pre-2010 trend even after additional years are disclosed.

Motivated by the misreporting that we document, the final part of this paper develops a method to correct the unemployment rate. We focus on a retrospective annual rate constructed from the CPS ASEC, giving the share of aggregate weeks in the labor force spent unemployed. It tracks the annual average of the official CPS Monthly unemployment rate closely, with a correlation of 0.99 and a stable ratio over time. Given this close relationship, we argue that corrections to the annual rate are informative about bias in the official rate, and we apply the percentage point correction to the annual rate to the official rate.

Our correction treats every UI recipient as having experienced some unemployment and scales up non-UI recipients who report unemployment, assuming they underreport as often as the average UI recipient. We regard this assumption as conservative, since non-recipients lack key features of UI recipients that plausibly aid recall, and underreporting exceeds 80% among recipients with the smallest benefits (those closest to non-recipients). We then convert to a retrospective annual rate using average reported durations of unemployment among the unemployed within each group and subtract an estimate of false positives based on estimates from Poterba and Summers (1986). For now, we approximate the required linked-data inputs in the public-use CPS ASEC using our disclosed estimates from 2010 and published Bureau of Economic Analysis (BEA) UI benefit totals, which allow us to construct corrections for every year from 1987 to 2024 (outside of 2020-21). We extrapolate from 2010 in two ways, holding constant either the additive or the proportional gap between the share of UI recipients reporting no unemployment and its survey analog. This methodology can be used to correct the unemployment rate in near real time as new survey data become available and without waiting for new administrative data to be made available, a lag of potentially several years.

The estimated corrections are large. In 2010, the year in which we measure underreporting directly against administrative UI records, our correction raises the annual average of the official unemployment rate from 9.6% to 14.6% (an increase of 52%). From 1987 through 2024, the corrected rate averages 1.48 times the published rate under the additive method and 1.72 times under the proportional method, and it is never less than 1.2 times the published rate under either method. As discussed, the magnitudes for years other than 2010 are preliminary and rest on a public-use implementation described in Section 4. The linked data estimates for 2011 through 2023 that we have submitted for disclosure will allow us to directly examine the accuracy of these

public-use approximations in recent years, including which method better approximates the share of UI recipients reporting no unemployment.

The corrections have also likely grown in recent years. Among years with similar published unemployment rates, the correction under the additive method was 2.3 percentage points in 2000 (when the published rate was 4.0%) compared with 3.9 and 2.7 percentage points in 2022 and 2023 (when the published rate was about 3.6%). Furthermore, under either method (additive or proportional), four of the six largest corrections relative to the published rate outside the pandemic years have occurred since 2018. Over the business cycle, the corrected rate generally rises and falls by more in percentage point terms than the published rate. In percent terms, however, it rises and falls by less than the published rate under the proportional method, while the pattern is mixed under the additive method.

Our analysis contributes to several strands of literature. First, we add to work documenting inconsistencies 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). Most of these studies analyze 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 47.7% in 2010 among a key subset of the unemployed, 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 2024) and, to the best of our knowledge, we are the first to directly document changes in unemployment misreporting over time, although others have used a structural approach to correct the unemployment rate (Feng and Hu 2013).

A separate body of work validates survey reports of employment and earnings against administrative records. Abraham, Haltiwanger, Sandusky, and Spletzer (2013, 2021) reconcile household and establishment measures of employment and compare survey and administrative measures of self-employment. Other work has additionally examined measurement error in

earnings, although this work primarily concerns the accuracy of reported amounts among those who report working.³ This literature has established that survey reports of whether and how much people work diverge systematically from administrative records. Unemployment has not been benchmarked in the same way, in part because identifying it requires not only that a respondent report a period without work but that they also report job search.

This paper also contributes to a literature that uses high-quality administrative data to correct errors in surveys and revise our understanding of economic conditions. Recent research has documented that survey error is substantial and growing over time (e.g., Meyer, Mok and Sullivan 2015, Bollinger et al. 2019, Meyer, Mittag and Goerge 2022). Several studies out of our Comprehensive Income Dataset (CID) Project have incorporated administrative data to challenge conclusions obtained from survey data alone, including work on extreme poverty (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). We 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, and these estimates inherit the errors that we document. The procyclical misreporting that we document and corrections that we estimate further suggest that key relationships in macroeconomics are likely affected by misreporting. Our paper provides the first empirical basis grounded in external administrative data for assessing the potential magnitude of these biases.

³ Abowd and Stinson (2013) model measurement error in annual job earnings by comparing SIPP reports to administrative earnings records, while Bollinger, Hirsch, Hokayem, and Ziliak (2019) document the extent and non-randomness of earnings non-response in the CPS.

The remainder of this paper proceeds as follows. Section 2 describes the data sources and linkage, and provides evidence that receipt of UI on a 1099-G is a reliable indicator of unemployment. Section 3 documents internal discrepancies within surveys, large underreporting of unemployment among UI recipients in the administrative data, and heterogeneity in the misreporting of unemployment. Section 4 develops methods to correct the unemployment rate and presents results. Section 5 concludes.

2. Data

This section describes the survey and administrative data sources used in our analysis and the procedures to link them. We first outline the key survey data and variables, followed by the administrative records we use. We next describe the procedures used to link records and how we adjust for incomplete linkage to preserve the representativeness of the CPS ASEC. Finally, we discuss why receipt of UI on a 1099-G is a reliable indicator of unemployment within the year of receipt, and we discuss analyses that we have conducted or plan to conduct that validate this assumption.

2.1. Survey Data

Our primary source of survey data is the CPS ASEC. In recent years, the CPS ASEC has been 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. The CPS ASEC is sampled from respondents to the CPS Basic Monthly Survey, which is the source of the official unemployment rate, and thus any errors present in the ASEC are likely to be present in the Monthly as well. The distinction between the two surveys and measures of unemployment derived from them is discussed in Section 4.

The CPS ASEC asks respondents whether they were 1) not working for at least one week during the calendar reference year and 2) whether they were actively looking for work or on layoff during that period. We classify individuals meeting both conditions 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 inconsistencies with survey-reported unemployment for a long time series (1987-2023).

As a robustness check and to examine how the length of recall period might affect misreporting, we also use the 2008 panel of the Survey of Income and Program Participation (SIPP), which covers 2008-2013. The SIPP has a reference period of four consecutive months, that requires less than 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 administrative microdata on unemployment come from IRS Form 1099-G, which are available through the Census Bureau for calendar years 2010-2023. 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. Given that UI recipients must be unemployed to be eligible for benefits (except for the case of partial-UI, which we discuss below), we treat UI receipt as an administrative indicator of unemployment for the calendar year.⁴ In some states, Box 1 may also include payments from Paid Family Leave (PFL) or Short-Term Disability Assistance (STDA) programs. However, as we discuss later in this section, these other programs are only in a few states and quite small during our analysis period.

A caveat of the administrative 1099-G records is that they identify only UI recipients and thus a subset of the truly unemployed. Take-up among eligible unemployed workers is incomplete, and many unemployed individuals are ineligible for benefits, including those who left a job voluntarily, were discharged for cause, or self-employed. We therefore cannot directly observe misreporting among a large share of the unemployed, but in Section 4 we discuss how we can use our estimates of misreporting for UI recipients to make inferences regarding the extent of misreporting among non-UI recipients.

⁴ In 2020 and 2021, Pandemic Unemployment Assistance covered workers not otherwise eligible (e.g., the self-employed) and states could waive work-search requirements, so some individuals could qualify for UI without being unemployed per the official definition. Our estimates for these years therefore reflect upper bounds on underreporting.

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 uses person data (such as names, addresses, and dates of birth) to search for a matching record in a reference file containing all transactions recorded against a Social Security Number. PIKs are unique identifiers that enable linkage across our data records that also preserve confidentiality. Approximately 90% of all individuals in the CPS ASEC are assigned a PIK, while over 99% 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 80% of our sample. To address potential bias arising from non-random PIK assignment and whole imputation, we adjust 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. We apply the same restrictions and reweighting to the SIPP sample.

Our disclosed estimates using the linked administrative and survey data currently cover a single reference year, 2010. Specifically, we have disclosed estimates from the 2011 CPS ASEC and the 2008 SIPP panel sample linked to 1099-G records for tax year 2010. We are currently in the process of disclosing linked estimates for reference years 2011 through 2023, and we expect those estimates to be available in the coming months. Until then, any estimates for years other than 2010 come either from public-use data alone or from approximations that combine our disclosed 2010 estimates with public-use data.

2.4. Assessing Whether 1099-Gs Accurately Identify Unemployment

Throughout this paper, we treat non-zero benefits in Box 1 of a 1099-G as indicating some unemployment during the calendar year. UI is designed to compensate for earnings losses following involuntary job loss, so receipt should therefore coincide with an unemployment spell during the year of receipt. However, this conclusion may not hold in a limited number of circumstances. Several events – namely UI fraud, PFL and STDA payments reported in the same field, and partial UI – could cause us to mistakenly identify an individual as unemployed by receipt

of a 1099-G. We separately evaluate each of these concerns and demonstrate that they are unlikely to materially affect our results. We also describe a direct validation test using linked W-2 earnings.

UI Fraud

Fraudulent UI claims could in principle generate a 1099-G for an individual who was never unemployed. However, only a narrow class of fraud could do so. Our estimates are affected only when a fraudulent payment produces a 1099-G for someone who was never unemployed during the year, such as a claimant working informally while collecting benefits. Fraud by claimants who *were* in fact unemployed, such as those who quit voluntarily and continue searching for work, and overpayments arising from continued claims after reemployment would not generally bias our estimates of misreporting.

The Department of Labor’s Benefit Accuracy Measurement (BAM) program provides a window onto the magnitude of the bias. Each year, the program reinvestigates a random sample of paid weeks in each state’s regular UI program to determine whether the payment was proper and publishes overpayment rates by cause. In 2010, overpayments amounted to 10.6 percent of UI benefit dollars, of which the two causes plausibly reflecting fraud (i.e., inability to verify work search and payments to claimants unable or unavailable to work) accounted for 2.5 percent of total UI dollars. The corresponding figure for 2023 was 4.1 percent of total UI dollars. These shares are likely ceilings for our purposes, since BAM audits weekly payments rather than annual receipt, and a claimant whose work search cannot be verified in one week may have been unemployed in others.

That said, during the COVID-19 pandemic, overpayments of UI rose sharply to roughly 25 percent of UI dollars by the end of 2021, in line with estimates from Isen, Kuka, and Stuart (2026). This possibility is another reason why we do not focus on 2020 and 2021 when implementing our correction for unemployment rates in Section 4.

Paid Family Leave and Short-Term Disability Assistance

A small number of states pay PFL or STDA benefits in Box 1 of the 1099-G, the same field we use to identify UI receipt, which could lead us to treat a non-unemployed individual as unemployed. PFL provides partial wage replacement to workers taking leave to care for a new child or an ill family member. STDA replaces wages for workers temporarily unable to work due

to non-work-related illness, injury, or pregnancy. Four states (CA, NJ, NY, and RI) reported PFL on 1099-Gs before 2020 and five more states (CT, DC, MA, OR, WA) introduced such programs between 2020 and 2023, while STDA appears in Box 1 in three states. Appendix Table A1 provides details about the policies for specific states.

To bound the exposure, we construct the ratio of PFL and STDA dollars to the sum of UI, PFL, and STDA dollars, using aggregate benefit totals from the Department of Labor and state administrative reports (Appendix Figures 1 and 2). California is by far the largest potential source of non-UI dollars, but California reports STDA on 1099-Gs only when it substitutes for UI, so only a subset of its STDA dollars enters Box 1. We are planning to disclose results excluding California and, as a robustness check, all states where Box 1 of 1099-G can potentially include non-UI dollars.

Partial UI and Workshare Programs

Partial UI provides reduced weekly benefits to claimants working part-time hours while searching for full-time work, and every state operates some form of it. Workshare provides reduced benefits to workers whose hours have been formally cut by their employer as an alternative to layoff.⁵ Both programs appear in Box 1, and we refer to them jointly as part-time UI.

Part-time UI violates our assumption that 1099-G receipt indicates some unemployment only for claimants who neither precede nor follow a part-time claim with full UI in the same calendar year. A claimant who transitions between part-time and full UI within a year experiences unemployment in that year, so 1099-G receipt correctly identifies them as unemployed. Appendix Figure 3 plots the share of compensated UI weeks attributable to part-time UI, drawn from the Department of Labor's ETA 9050 and 9051 publications. This share averages 9.1 percent across 2010 through 2023, falling from 10.5 percent in 2010 to 6.5 percent in 2023.⁶ A share of this size therefore cannot account for the underreporting we document.

Earnings Declines Among 1099-G Recipients

⁵ 19 states operated a workshare program in 2010, while 27 states operated a workshare program in 2023.

⁶ The Department of Labor does not publish totals for number of recipients, but this share bounds the share of recipients who collect only part-time UI, provided those recipients do not claim fewer weeks on average than other claimants.

While our focus so far has been on specific mechanical checks, a more direct test of the connection between 1099-Gs and unemployment is whether those receiving a form experience earnings declines consistent with job loss. We measure changes in annual W-2 earnings for 1099-G recipients in the year of receipt relative to the two prior years, conditional on having earnings in at least one of those prior years. Using two prior years guards against comparing against a year already affected by unemployment. While we have not disclosed yet these estimates, our plan is to construct these measures for 2010 (against 2008 and 2009 earnings), 2016 (against 2014 and 2015 earnings), and for 2023 (against 2021 and 2022 earnings).

Our first measure is continuous, the proportional decline between earnings in the year of receipt and the higher of the two prior years:

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

Under the assumption that earnings would have remained constant absent unemployment, this approximates the share of the year spent unemployed. A 50 percent decline corresponds to roughly half a year out of work if an individual obtained similar earnings afterward. We plan to report the median earnings decline to limit the influence of outliers such as individuals with very low prior earnings.

Our second measure is discrete, an indicator for whether the dollar decline exceeds twice total UI benefits received. Twice the benefit amount corresponds to a replacement rate of roughly 50 percent, near the statutory target in most states, so the indicator captures whether earnings losses exceed what UI would be expected to replace. It requires observed benefit amounts and can be computed only for UI recipients.

We plan to conduct two comparisons. The first compares earnings declines of 1099-G recipients who report no unemployment in the survey to those of recipients who report some unemployment in the survey. Similar declines across the two groups would indicate that the difference in survey reports reflects reporting behavior rather than different labor market experiences. The second compares recipients who report no unemployment to non-recipients who report some unemployment, using the continuous measure only. Declines that are similar or larger for the first group would indicate that 1099-G receipt is at least as informative an indicator of unemployment as a survey response.

3. Evidence of Misreporting of Unemployment

This section documents the extent and patterns of unemployment underreporting. We begin by examining internal inconsistencies within the CPS ASEC – cases where respondents report receiving UI yet report no unemployment in the same year – which we measure in public-use data for reference years 1987-2024. We then compare survey responses to administrative 1099-G records to estimate the rate of underreporting directly. As described in Section 2, linked data estimates are currently disclosed only for 2010, and disclosure for 2011 through 2023 is underway. Our estimates exclude individuals whose responses for UI receipt or weeks unemployed were imputed in the CPS ASEC.

3.1. Survey Internal Inconsistencies

We first examine whether reports of UI receipt and unemployment are internally consistent within the responses for a given individual using public-use data. Respondents who reported receiving UI during the reference year should have also reported some unemployment that year – we calculate the share who did not. These are individuals for whom (at least) one of their two survey reports is incorrect, although we cannot say which without administrative data. Figure 1 displays this inconsistency rate for 1987 through 2024.

The share of survey UI recipients who reported no unemployment is high throughout the period, ranging from roughly one quarter to more than one-half. The pattern is procyclical, with inconsistencies lowest during recessions and highest during expansions. In 2010, at 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 above 50 percent.

The procyclicality suggests that these inconsistencies 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. Respondents may also be less likely to report unemployment when fewer other individuals are unemployed because it carries more stigma then. We examine the implications for the measured cyclicity of the unemployment rate in Section 4.

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, and compute the share of these administrative UI recipients who reported no unemployment in the CPS ASEC in the same year. This share can be interpreted as a false negative rate in the reporting of unemployment.

In 2010, 47.7% of administrative UI recipients reported no unemployment in the CPS ASEC. We find a comparable rate in the SIPP for the same year (43%), despite the SIPP's four-month recall period (roughly a third of the CPS ASEC's annual recall period) and typically three interviews in which to report unemployment during the year, as well as other survey features designed to improve reporting.⁷ Recall length alone appears to be a minor determinant of the misreporting we document.⁸ Because the CPS Basic Monthly Survey uses a still shorter reference period (of one week), this finding suggests that the errors we document are very likely present in the official unemployment rate, a point we develop in Section 4.

3.3. Heterogeneity in the Misreporting of Unemployment

We next examine how reporting errors vary by reported labor force status and by the amount of UI benefits received. We focus on calendar year 2010, the year for which we have disclosed results using the linked administrative records and the peak of the Great Recession. UI receipt was also at its highest pre-pandemic level, and maximum benefit durations were extended to 99 weeks.

Variation in Misreporting by Labor Force and Employment Status

Figure 2 presents the distribution of reported labor force status in 2010 among administrative UI recipients who reported no unemployment. 31% of this group reported zero weeks worked and no job search (i.e., being out of the labor force). This is notable given that UI

⁷ The SIPP also attempts to interview each household member aged 15 or older directly, whereas in the CPS one household member usually responds for all members.

⁸ 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. Celhay, Meyer, and Mittag (2024) similarly find no significant relationship between time since last receipt and misreporting of SNAP in the SIPP.

eligibility typically requires active job search. An additional 16% reported working for part of the year (1-49 weeks), with the majority 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 these underreporters reported 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 extends to individuals whose survey responses are grossly inconsistent with their administrative records. Because these individuals report being employed, their responses simultaneously overstate employment and understate unemployment, distorting both statistics.

Variation by UI Benefit Amount

Figure 3 plots the share of 2010 UI recipients in the administrative data who report no unemployment in the CPS ASEC by ventiles of administratively reported UI benefits received, and Figure 4 plots the same share for SIPP respondents. We additionally plot the average annual administrative UI amount in each ventile. For the CPS, we further decompose the misreported share by reported labor force status for successively narrower subsets of those who report no unemployment.⁹

There is a clear inverse relationship between misreporting and benefit receipt amounts. Among UI recipients in the lowest ventile (approximately \$500 in UI benefits received), over 80% report no unemployment. Misreporting declines steadily as benefits received increase. However, it remains substantial (around 30%) even in the highest ventile (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 reporting pattern is somewhat similar in the SIPP, though with a shallower gradient, leading to less extreme misreporting in the lowest ventile of benefit receipt

⁹ Among recipients who report no unemployment, most of those in the lowest ventiles report working at least one week during the year, while most of those in the highest ventiles report zero weeks worked and no job search. Reports of full-year employment are most common at low benefit amounts but persist at higher ones. In the 15th ventile (\$10,300 in UI benefits on 1099-Gs), 16.3% of all recipients report no unemployment and 50–52 weeks of work, as do 6.1% in the top ventile (\$25,200).

(66% in the SIPP versus over 80% in the CPS). The gradient is consistent with salience driving misreporting, since individuals with shorter or less financially significant unemployment spells are most likely to fail to report them.

The benefit gradient also has direct implications for the unemployed population that we cannot observe in administrative data – i.e., 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 mostly 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 plausibly represent a lower bound on misreporting among the broader unemployed population.

4. Correcting the Unemployment Rate

The preceding sections document substantial underreporting of unemployment among UI recipients in the CPS ASEC, along with comparable underreporting in the SIPP despite its shorter recall period. We believe these findings make it likely that the official unemployment rate is biased, even though it is derived from near-contemporaneous survey responses in the CPS Basic Monthly Survey. 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, together with job search and layoff over the previous four weeks. Because CPS ASEC respondents are drawn from the CPS Monthly sample, errors present in one survey are likely present in the other.

In this section, we develop methods to correct an annual unemployment rate constructed from the CPS ASEC for the errors that we document and argue that corrections to it are informative about bias in the official (monthly) rate. We begin by defining that rate, as well as the monthly unemployment rate, and documenting their close relationship with each other. We then present a proposed correction using restricted data and present corrections to the average monthly unemployment rate for 2010. We briefly outline a public-use version of our correction procedure, allowing the unemployment rate to be corrected for misreporting as new survey data are released.

We then present the results of our corrections and their implications for the level, trend, and cyclicity of unemployment.¹⁰

4.1. Constructing an Annual Unemployment Rate from the CPS ASEC

The CPS ASEC asks respondents to report the number of weeks spent unemployed and weeks spent working over the previous calendar year. We denote these quantities as U_{at} and E_{at} . Weeks spent in the labor force is just the sum of weeks working or weeks unemployed, $LF_{at} = U_{at} + E_{at}$. From these responses, we construct a annual unemployment rate, which we define as the share of total weeks in the labor force spent unemployed:

$$UR_{at} = \frac{U_{at}}{U_{at} + E_{at}}.$$

We similarly denote the official unemployment rate in month m and year t as the ratio of the number of individuals unemployed during the week including the 12th of the month (U_{mt}) to the number of individuals in the labor force in that week (LF_{mt}):

$$UR_{mt} = \frac{U_{mt}}{U_{mt} + E_{mt}}.$$

Averaging UR_{mt} over all months in the year should approximate the annual rate because $E[\overline{UR}_{mt}] = UR_{at}$. The measures of the average monthly rate and the annual rate may differ due to sampling and measurement errors, but in practice, the two are closely related. Figures 5 through 7 compare the annual unemployment rate, UR_{at} , to the annual average of the official rate from 1975 to 2023. UR_{at} lies below UR_{mt} and tracks it closely, with a correlation of 0.99. The official rate is about 20 percent higher than the annual rate, consistent with results in Levine (1993), and exceeds the annual rate by an average of one percentage point, with neither the ratio nor the difference exhibiting a secular trend over the fifty year period (though the difference varies procyclically). The close and stable relationship between the retrospective annual and official rate strongly suggests that bias in the annual rate is informative about bias in the official rate. We use that insight to propose corrections to the official rate.

¹⁰ Given that only estimates for 2010 are currently disclosed, we rely on the public-use correction procedure in the discussion of results. We are awaiting clearance of a broader range of years that we have submitted for disclosure.

4.2. Methods: Correcting the Unemployment Rate Using Linked Administrative Data

We propose calculating corrected unemployment rates:

$$UR_{at}^c = C_t + UR_{at} \text{ and } \overline{UR}_{mt}^c = C_t + \overline{UR}_{mt}$$

Where C_t is a correction factor that we calculate based on restricted-use data. We later calculate a public-use correction factor $\hat{C}_t$ based on a combination of public-use and disclosed restricted-use data from 2010.

For now, we ignore any corrections to the total number of weeks in the labor force, LF_{at} . We focus on correcting the number of weeks unemployed in the year, U_{at} , the numerator of the annual measure. The total number of weeks unemployed is a small component of the total number of weeks in the labor force, and individuals reporting no unemployment largely report working, so corrections to weeks unemployed involve offsetting changes to weeks employed.¹¹

First, note that we can decompose the total number of weeks unemployed, U_{at} , into weeks unemployed from UI recipients and non-recipients: $U_{at} = U_{at}^{UI} + U_{at}^{nUI}$, where the *UI* superscript indicates receipt of UI sometime in the year and *nUI* indicates not receiving UI in the year. Additionally, we can express measures of weeks unemployed as $U_{at} = IU_{at} \cdot WeeksU_{at}$, where IU_{at} is the number of individuals experiencing any unemployment in year t (or the incidence of unemployment), and $WeeksU_{at}$ indicates mean weeks of unemployment among the unemployed. Superscripts indicating UI receipt or non-receipt (i.e., *UI* and *nUI*) can be added to indicate incidence of unemployment and average weeks unemployed among the unemployed for UI recipients and non-UI recipients.

Therefore, the annual unemployment rate can be expressed as:

$$UR_{at} = \frac{IU_{at}^{UI} \cdot WU_{at}^{UI} + IU_{at}^{nUI} \cdot WU_{at}^{nUI}}{LF_{at}}$$

So far, all of our notation has pertained to true values of labor force status. Unfortunately, we do not observe individuals' true labor force status in the CPS ASEC and instead observe potentially mismeasured versions. We use an l superscript to indicate what we observe in linked data – specifically the classification of UI recipients and non-UI recipients (based on administrative linkage to a 1099-G). Specifically, let $IU_{at}^{UI,l}$ denote the number of individuals reporting any unemployment in year t among UI recipients and let $WU_{at}^{UI,l}$ denote mean reported

¹¹ We find that 69.4% of UI recipients who reported no unemployment in the year instead reported working for at least one week--see Figure 2.

weeks of unemployment for those individuals. We similarly define $IU_{at}^{nUI,l}$ as the number of individuals reporting any unemployment in year t among non-UI recipients and $WU_{at}^{nUI,l}$ as their mean reported weeks of unemployment.¹²

Let R_t be the share of UI recipients who report no unemployment during the year. Then, we know from the linked 1099-Gs that $IU_{at} = IU_{at}^{UI,l} \cdot \frac{1}{1-R_t}$. This scales the count of UI recipients reporting unemployment by the inverse of one minus the misreporting rate and is equivalent to treating each UI recipient as being unemployed in the year of UI receipt.

So far, we have corrected the count of the incidence of unemployment for UI recipients, a group that we can directly identify as underreporting in the microdata. We cannot identify underreporting among non-recipients, but it is likely that similar or greater underreporting of unemployment is present among them. A conservative correction (in terms of providing a lower bound on revised unemployment counts) would also assume that $IU_{at} = IU_{at}^{nUI,l} \cdot \frac{1}{1-R_t}$. This assumes that underreporting of unemployment among non-UI recipients is the same as UI recipients.

If lower salience implies more underreporting, as the benefit gradient in Section 3.3 indicates, then this assumption would be a lower bound on the misreporting of non-UI recipients and the resulting correction is conservative. A natural alternative is to set the underreporting rate among non-recipients equal to that among recipients receiving few benefits, those closest to non-recipients. For example, those in the bottom ventile of benefit receipt, among whom over 80% report no unemployment (Figure 3). Let R'_t denote the underreporting rate of unemployment among bottom ventile UI recipients, where $R'_t \geq R_t$.

So far, we have focused on using the failure to report unemployment among UI recipients to provide an estimate of the rate that the true unemployed fail to report unemployment (false negatives). We also need to consider the possibility of false positives, or individuals who report unemployment but are not truly unemployed. The presence of false positives biases the annual rate upward, offsetting downward bias from underreporting. If false positives are quantitatively

¹² Since we have not yet disclosed the values of $WU_{at}^{UI,l}$ and $WU_{at}^{nUI,l}$ from the linked data, we assume for now that $WeeksU_{at}^{UI,l}$ is equal to average reported weeks of unemployment among unemployed survey UI recipients and $WeeksU_{at}^{nUI,l}$ is equal to average reported weeks of unemployed among unemployed survey non-recipients. That said, our other work on survey error also indicates that reporting errors on the extensive margin are larger than those on the intensive margin, and the extensive margin is what our correction addresses directly (Meyer et al. 2026).

important, then correcting for false negatives alone would overstate the upward adjustment to the annual unemployment rate.

False positives cannot be identified in the administrative microdata, and given data limitations, few studies have estimated false positive rates for unemployment. Fortunately, Poterba and Summers (1986) provide an estimate of false positives based on CPS reinterview data in which a subset of CPS Monthly respondents are recontacted a week after their initial interview and asked about their labor market status during the original reference week. Where the two responses conflict, the interviewer attempts to determine which response is correct. Unfortunately, Poterba and Summers relied on 1981 data and recent data like theirs are not collected.

Poterba and Summers calculate false positive rates between 0.54% and 0.64%. To apply these estimates to our correction procedure, we need to know the true distribution of labor force status in the population. This cannot be observed in our data (as we only have access to reported labor force status), so we rely on the reinterview distribution published in Poterba and Summers.¹³ Combining this with their estimated false positive rates, we estimate that the number of individuals misclassified as unemployed was equal to 0.86% of the labor force in 1981.¹⁴ To account for changes in survey misreporting over time, we inflate this estimate by the ratio of our 2010 false negative rate among UI recipients (47.7%) to the false negative rate in Poterba and Summers (15.2%), yielding a false positive correction factor of 2.70%.

Then, the conservative corrected annual unemployment rate is:

$$UR_{at}^c = \frac{(IU_{at}^{UI,l} \cdot WU_{at}^{UI,l} + IU_{at}^{nUI,l} \cdot WU_{at}^{nUI,l}) \cdot \left(\frac{1}{1 - R_t}\right)}{LF_{at}} - 2.70\%$$

We can then apply $C_t = UR_{at}^c - UR_{at}$ to correct the average monthly unemployment rate:

$$\overline{UR}_{mt}^c = C_t + \overline{UR}_{mt}$$

Adding C_t assumes that the misreporting we correct for biases both UR_{at}^c and $\overline{UR}_{mt}$ by the same number of percentage points, which is consistent with the stability of levels between the two rates in Figure 7. If we instead assume that non-recipients misreport as often as UI recipients in the

¹³ See Table II, Panel A of Poterba and Summers (1986). 58.77% of the population is estimated to be employed, 4.85% unemployed, and 36.39% not in the labor force.

¹⁴ To arrive at this estimate, note that PS estimate that 0.54% and 0.64% of employed and not in labor force individuals report unemployment respectively (Table III). Thus, the share of individuals in the labor force incorrectly classified as unemployed is: $\frac{0.54\% \cdot 58.77\% + 0.64\% \cdot 36.39\%}{58.77\% + 4.85\%} = 0.864\%$.

lowest ventile of benefits (80.6% in 2010), then the corrected annual unemployment rate, $UR_{at}^{c'}$, is:

$$UR_{at}^{c'} = \frac{\left(IU_{at}^{UI,l} \cdot WU_{at}^{UI,l} \cdot \frac{1}{1-R_t}\right) + \left(IU_{at}^{nUI,l} \cdot WU_{at}^{nUI,l} \cdot \frac{1}{1-R_t'}\right)}{LF_{at}} - 2.70\%$$

As before, we can then apply $C_t' = UR_{at}^{c'} - UR_{at}$ to arrive at an alternative correction to the average monthly unemployment rate:

$$\overline{UR}_{mt}^{c'} = C_t' + \overline{UR}_{mt}$$

4.3. Results: Correcting the 2010 Unemployment Rate

We first walk through our corrections to the unemployment rate for calendar year 2010, the year for which we have disclosed estimates from the linked data. We start by walking through the corrections to the annual unemployment rate before then applying the implied correction factors to the average monthly unemployment rate. These corrections are presented in Table 1 under 2010.

We begin with an uncorrected annual unemployment rate of 8.3%. In the 2010 CPS ASEC, 24.7 million individuals report any unemployment during the year, and they report an average of 26.1 weeks unemployed. Total weeks in the labor force (LF_{at}) are 7.71 billion. Of those reporting unemployment, 9.3 million are UI recipients ($IU_{at}^{UI,l}$), who report an average of 29.3 weeks unemployed ($WU_{at}^{UI,l}$). The remaining 15.4 million are non-recipients ($IU_{at}^{nUI,l}$), who report an average of 24.3 weeks unemployed ($WU_{at}^{nUI,l}$).

Among UI recipients, 47.7% report no unemployment during the year (R_{2010}). Scaling the count of UI recipients reporting unemployment by $1/(1 - R_{2010}) = 1.91$ raises it to 17.8 million, which is equivalent to treating each UI recipient as unemployed in 2010. Correcting solely for underreporting of unemployment among UI recipients, the annual unemployment rate increases to 11.6%, an increase of 3.3 percentage points (or 39.8%). When we impose the assumption that non-recipients underreport as often as the average UI recipient, we apply the same factor of 1.91 to the 15.4 million non-recipients reporting unemployment, raising their count to 29.5 million. The annual unemployment rate further increases by 4.4 percentage points, to 16.0%. This now constitutes a substantial increase of 92.8% over the uncorrected rate. We subtract off 2.7 percentage points to account for false positives, yielding a corrected annual unemployment rate of

13.3%. This is an increase of 5.0 percentage points over the uncorrected rate (or 60.2%) and gives $C_{2010} = 5.0$ percentage points.

We correct the average monthly unemployment rate by adding C_{2010} to the uncorrected average monthly rate. Starting from an average monthly unemployment rate of 9.6% in 2010, we end up with a corrected rate of 14.6% (an increase of 52.1%), implying a large degree of bias in published rates from survey error.

If we instead assume that non-recipients misreport as often as UI recipients in the lowest ventile of benefits ($R'_{2010} = 80.6\%$), the factor applied to non-UI recipients rises to about 5.2, which raises their count to 79.5 million. The annual unemployment rate is then 29.1% (after subtracting false positives), which is an increase of 20.8 percentage points over the uncorrected value and an increase of 249%. This implies a corrected average monthly unemployment rate of 30.4%.

4.4. Methods: Implementing the Correction with Public-Use Data

While we have shown that the unemployment rate in 2010 suffers from a large degree of bias from false negatives net of false positives, our disclosed results on their own do not shed light on the degree of bias in other years, as well as how bias has changed over time and how it might affect published cyclicalities. For other years, we can – for now – approximate the quantities the linked data would supply, using public-use survey data and published benefit totals.¹⁵

In the years without administrative data, we do not have the quantities R_t , $IU_{at}^{UI,l}$, $IU_{at}^{nUI,l}$, $WU_{at}^{UI,l}$ and $WU_{at}^{nUI,l}$. However, we have survey analogs of these quantities, which we can combine with disclosed quantities from 2010 to approximate what the administrative data corrections would be in other years.

The survey analog of R_t is the share of survey respondents reporting UI receipt who also report no unemployment, which we call the inconsistency rate and denote by R_t^S . To estimate R_t , we can either shift R_t^S by the percentage point difference between R_{2010} and R_{2010}^S (which assumes that the additive gap between R_t and R_t^S is constant over time) or scale R_t^S by $\frac{R_{2010}}{R_{2010}^S}$ (which assumes that the proportional gap between R_t and R_t^S is constant over time). Specifically:

¹⁵ We have submitted corrected unemployment rates from 2011 through 2023 through disclosure and are awaiting these results.

$$\hat{R}_t = R_t^s + (R_{2010} - R_{2010}^s) \text{ or}$$

$$\hat{R}_t = R_t^s \cdot \frac{R_{2010}}{R_{2010}^s}$$

We report corrected unemployment rates under each method. We report estimates of $\hat{R}_t$ under each method, as well as R_t^s in Appendix Table 2. In practice, the first method on average yields lower $\hat{R}_t$, which results in smaller corrections to the unemployment rate. In years in which both methods estimate similar $\hat{R}_t$ (such as 2012), the resulting public-use corrections under each method are then similar. To estimate R_t' , we set it equal to the misreporting rate among UI recipients in the lowest ventile in 2010, $\hat{R}_t' = 80.6\%$. If we used the two methods above, we would arrive at estimates of misreporting of unemployment in the bottom ventile for some years that are likely implausible.

In order to estimate $\widehat{IU}_{at}^{UI,l}$, we combine $\hat{R}_t$ with estimates of the number of UI recipients in each year. No public source reports the annual number of distinct UI recipients. We therefore scale the number of survey-reported UI recipients in year t , UI_t^s , by the ratio of total UI benefit dollars paid out in aggregate data (such as BEA totals), $UI \$_t$, to total UI benefit dollars reported in the survey, $UI \$_t^s$. This assumes that the survey misstates recipients and benefit dollars by the same proportion. Then:

$$\widehat{IU}_{at}^{UI,l} = \underbrace{\left(UI_t^s \cdot \frac{UI \$_t}{UI \$_t^s} \right)}_{\text{Estimate of number of UI Recipients}} \cdot (1 - \hat{R}_t)$$

We then obtain the estimate of the number of non-UI recipients reporting any unemployment ($\widehat{IU}_{at}^{UI,l}$) residually by subtracting $\widehat{IU}_{at}^{UI,l}$ from the count of all individuals reporting any unemployment in the survey (IU_{at}^s):

$$\widehat{IU}_{at}^{nUI,l} = IU_{at}^s - \widehat{IU}_{at}^{UI,l}$$

For average weeks unemployed among each group, we use average reported weeks unemployed among each survey reported group (survey UI recipients, survey non-recipients). We denote these as $WU_{at}^{UI,s}$ and $WU_{at}^{nUI,s}$.

Then, we replace the quantities that we cannot observe without restricted data with these estimates to arrive at the conservative public-use corrected annual unemployment rate:

$$UR_{at}^c = \frac{(\widehat{IU}_{at}^{UI,l} \cdot WU_{at}^{UI,s} + \widehat{IU}_{at}^{nUI,l} \cdot WU_{at}^{nUI,s}) \cdot \left(\frac{1}{1 - \widehat{R}_t}\right)}{LF_{at}} - 2.70\%.$$

We can then apply $\hat{C}_t = UR_{at}^c - UR_{at}$ to correct the average monthly rate as follows:

$$\overline{UR}_{mt}^c = \hat{C}_t \cdot \overline{UR}_{mt}$$

Additionally, we can also use our estimates to arrive at a public-use corrected annual unemployment rate that assumes non-recipients underreport unemployment as often as UI recipients in the lowest ventile of benefits:

$$UR_{at}^{c'} = \frac{\left(\widehat{IU}_{at}^{UI,l} \cdot WU_{at}^{UI,s} \cdot \frac{1}{1 - \widehat{R}_t}\right) + \left(\widehat{IU}_{at}^{nUI,l} \cdot WU_{at}^{nUI,s} \cdot \frac{1}{1 - \widehat{R}_t'}\right)}{LF_{at}} - 2.70\%,$$

with the correction factor and corrected average monthly rate defined analogously.

We are currently in the process of disclosing linked estimates for 2011 through 2023, and those years will then use the administrative inputs in place of these approximations. Each approximation becomes testable for 2010 onward once the additional years are disclosed. However, since the 1099-G records are not available before 2010, the public-use approach will remain the only available one for those years. Not only does the public-use approach allow us to extend our corrections back to 1987, but one can also apply it to each CPS ASEC release as it becomes available rather than waiting for administrative records to become available to researchers (which may take several years).

4.5. Results: Correcting the Unemployment Rate for Other Years

Table 1 and Figure 8 present corrections to the published unemployment rate from 1987 to 2024 using the public-use data approximations described in Section 4.4.¹⁶ We first discuss corrections that estimate R_t with the additive method (which assumes that the percentage point difference between R_t and R_t^s is constant over time) and then turn to corrections that estimate R_t

¹⁶ Appendix Table A2 contains all of the parameters that enter into these calculations. We exclude 2020 and 2021 from the results below. In those years, Pandemic Unemployment Assistance extended benefits to individuals outside the CPS definition of unemployment, and job search requirements were widely waived. Improper payments were also substantially elevated. We therefore do not believe it is appropriate to apply our corrections to those years.

with the proportional method. Unless otherwise noted, we assume that non-UI recipients underreport unemployment as often as the average UI recipient.

We find that the unemployment rate is understated in every year under either method. Under the additive method, the correction factor averages 2.7 percentage points, ranging from 1.4 percentage points in 1990 to 5.0 percentage points in 2010. The corrected rate averages 1.48 times the published rate and is never less than 1.25 times as high. In levels, the corrected rate never falls below 5.6% (in 2019), against a published minimum of 3.6% in 2023. If misreporting of unemployment is higher when unemployment is less salient, then the assumption that non-recipients misreport as often as the average UI recipient is likely conservative. The series that instead assumes non-recipients misreport as often as UI recipients in the lowest ventile of benefits raises the rate by an average of 12.6 percentage points (222%), and by at least 8.2 percentage points in every year (Appendix Table A2).

The correction also varies over time. Our estimate of C_t is largest around the Great Recession, peaking at 5.0 percentage points in 2010, and falls to 1.9 percentage points in 2019 as the published rate declines. Comparing years with similar published rates helps to separate this pattern from changes over time. At the end of the 1990s expansion (2000) and the 2010s expansion (2018–2019), the published rate was between 3.7 and 4.0 percent and C_t ranged from 1.9 to 2.3 percentage points. In 2022 and 2023, the published rate was about 3.6 percent, while C_t was 3.9 and 2.7 percentage points. At a similar measured level of unemployment, the implied bias is therefore larger in the 2020s than at the ends of the two preceding expansions. Relative to the published rate, the corrections in 2022 and 2023 are also the largest in the series, at 2.07 and 1.74 times the published rate, and four of the six largest ratios occur from 2018 onward.

Table 1 also reports corrections that estimate R_t with the proportional method, which assumes that the proportional gap between R_t and R_t^S is constant over time. The two methods yield the same $\hat{R}_t$ when the inconsistency rate equals its 2010 value, and the proportional method yields a higher $\hat{R}_t$ whenever the inconsistency rate exceeds that value, as it does in 29 of the 36 years. Under the proportional method, C_t averages 3.7 percentage points, and the corrected rate averages 1.72 times the published rate and is never less than 1.23 times as high. The difference between the additive and proportional methods is largest in the years with the highest inconsistency rates. Under the proportional method, the largest corrections occur in 2022 and 2023 rather than around the Great Recession. In 2022, which has the highest inconsistency rate among the years we correct,

$\hat{R}_t$ is 0.83 under the proportional method and 0.70 under the additive method, and C_t is 10.6 percentage points rather than 3.9. In 2023, C_t is 5.7 percentage points under the proportional method, compared with 3.4 to 4.5 percentage points at the ends of the two preceding expansions.

Finally, we compare how the published and corrected rates change over the business cycle, using periods in which the annual official rate rose (1989–1992, 2000–2003, 2007–2010) and fell (1992–2000, 2003–2007, 2010–2019). Under the additive method, the corrected rate rose by more in percentage point terms than the published rate in each period of rising unemployment and fell by at least as many percentage points in each period of falling unemployment. From 2007 to 2010, for example, the published rate rose by 5.0 percentage points and the corrected rate by 8.0 percentage points. In percent terms, the corrected rate changed by more than the published rate in three of these periods and by less in two, with nearly identical declines from 2010 to 2019. Under the proportional method, the corrected rate changed by a smaller percentage than the published rate in all six periods but by more percentage points in four of them. Conclusions based on percent changes therefore depend on the method used to estimate R_t . The linked estimates for 2011 through 2023, which include the years in which the two methods differ most, will allow us to assess which method (additive or proportional) better approximates R_t .

5. Conclusions

This paper documents substantial and systematic misreporting of unemployment in U.S. household surveys and develops methods using linked survey and administrative data to correct for this misreporting. Linking the CPS ASEC to IRS Form 1099-G data, we find that 47.7% 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 multiple interviews. In public-use CPS ASEC data covering 1987-2024, within-survey inconsistencies between reported UI receipt and reported unemployment have roughly doubled since the early 1990s to 2022, with inconsistencies lowest during recessions and highest during expansions. Because labor force status is determined from the same responses, these errors distort measured employment as well.

Among UI recipients who report no unemployment, 53% report working nearly the full year, usually on a full-time basis. Underreporting also varies systematically by the amount of benefits received, with 80% of recipients in the lowest ventile not reporting, but about 30% in the

highest ventile doing so, a gradient that suggests salience plays a role in reporting accuracy. This gradient also has direct implications for unemployed individuals whom we cannot observe in administrative records. Non-UI recipients rarely have filed a claim, do not certify job search, and they lack the institutional connectedness to unemployment that comes with filing for and receiving UI. This comparison plausibly makes misreporting among non-recipients at least as large as among recipients.

We develop a method to correct unemployment rates constructed from the CPS ASEC. The method treats every administrative UI recipient as having experienced some unemployment and scales up individuals who report unemployment and do not receive UI, assuming (conservatively) that they underreport at the same rate as UI recipients. It then subtracts an estimate of false positives based on misclassification rates from Poterba and Summers (1986). The procedure requires only a few quantities from linked administrative data (such as the number of UI recipients and the share of them reporting no unemployment). Because these can be approximated with public-use data, we are able to implement corrections for every reference year from 1987 through 2024 (outside of 2020 and 2021).

The retrospective annual unemployment rate tracks the official rate closely, with a correlation of 0.99 and a stable difference over time. Corrections to the annual rate are therefore informative about bias in the official rate. In 2010, the year for which linked estimates are disclosed, our correction raises the official unemployment rate from 9.6% to 14.6%. From 1987 to 2024, the corrected rate averages 1.48 times the official rate under our additive extrapolation from 2010 and 1.72 times under our proportional extrapolation, and corrections have likely grown in recent years. Over the business cycle, the corrected rate generally rises and falls by more percentage points than the official rate. In percent terms, however, it rises and falls by less than the official rate under the proportional extrapolation, while the pattern is mixed under the additive extrapolation.

Our results 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. Our findings have broad implications, bearing on a large body of macroeconomic research that takes survey-based measures of unemployment as accurate inputs. Unemployment is systematically understated in every year we examine, and the magnitude of the understatement varies over the business cycle. Therefore,

estimates of the Phillips Curve, Okun's Law, and the natural rate of unemployment rest on an input that is biased in both level and amplitude. 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 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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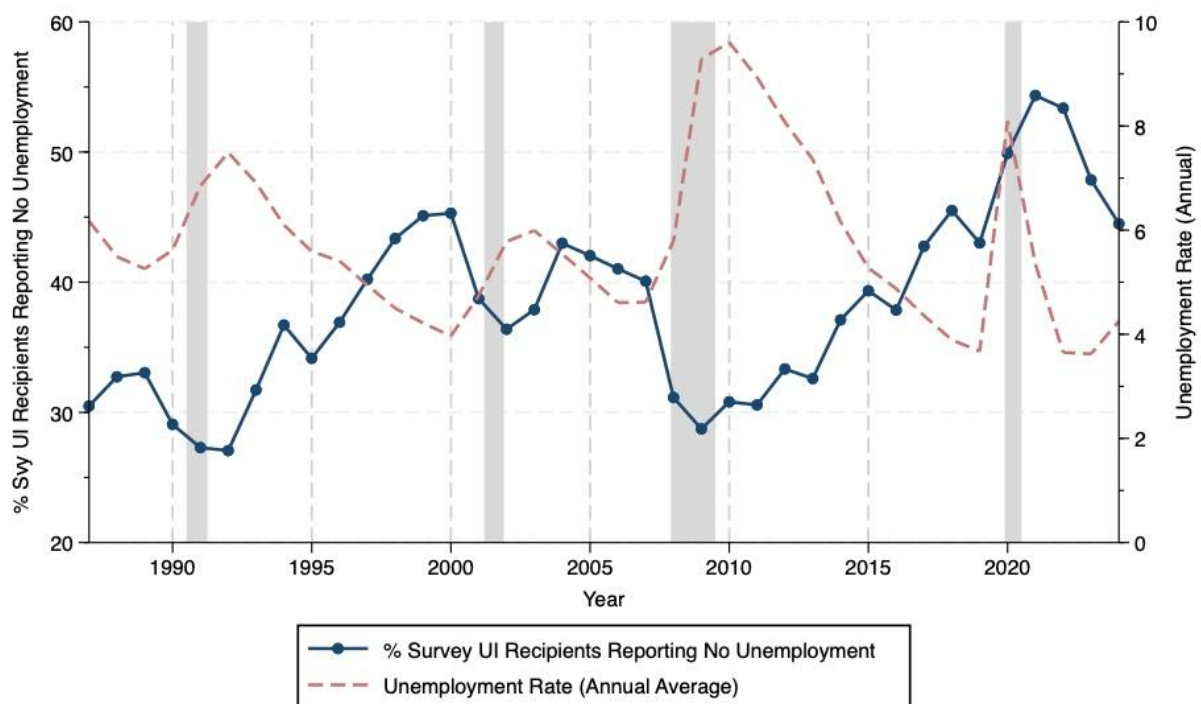
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Figures

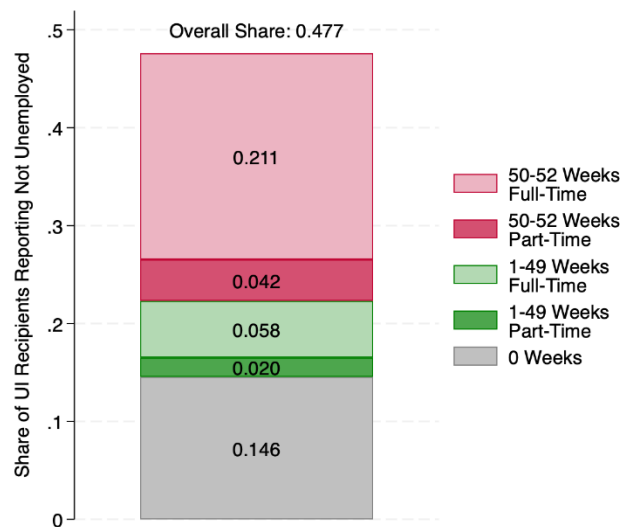
Figure 1. Percentage of Survey UI Recipients Reporting No Unemployment



Source: 1988-2025 CPS ASEC (reference years 1987-2024)

Notes: This graph shows the percentage of survey UI recipients who report no unemployment in the survey, as well as the average monthly 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.

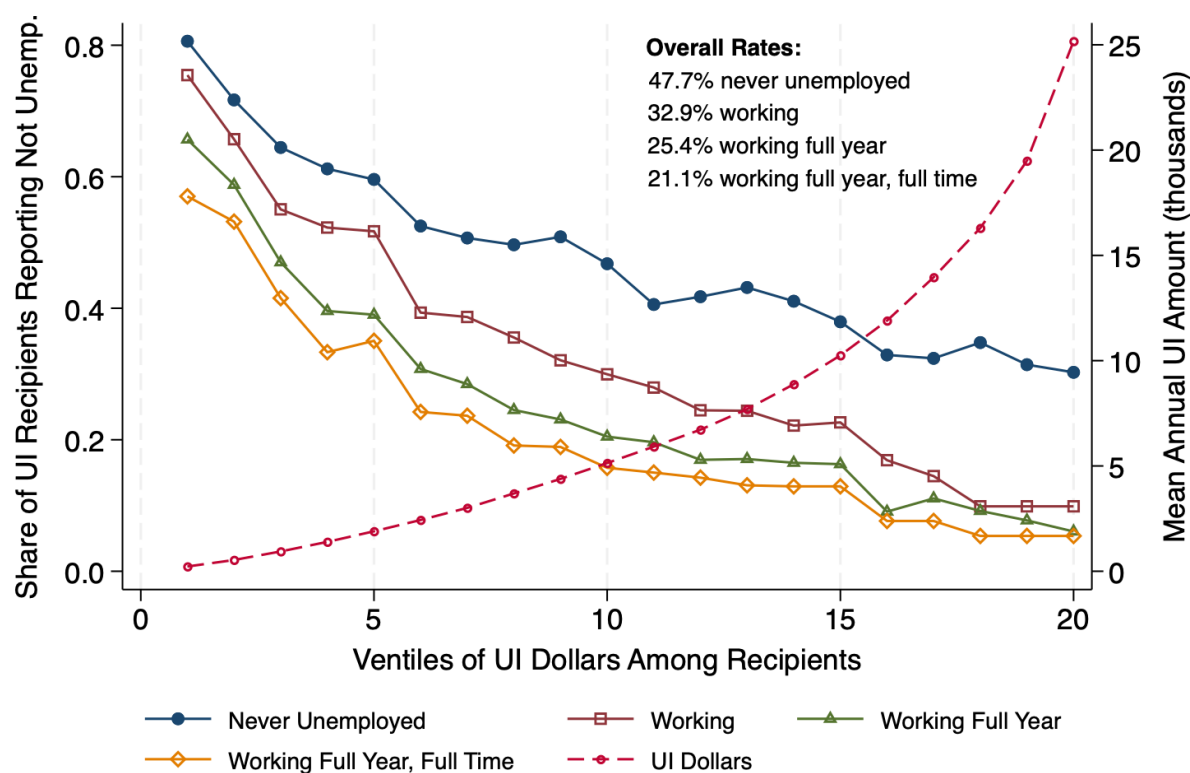
Figure 2. Distribution of Administrative Underreporting of Unemployment by Survey Reported Labor Force and Employment Status, 2010



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.

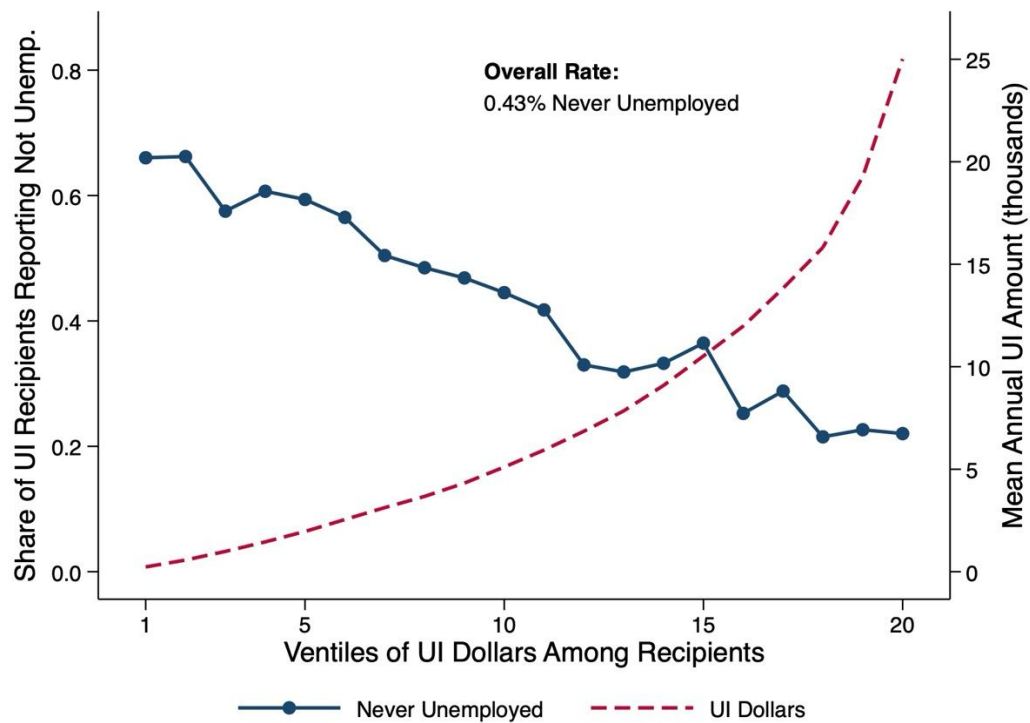
Figure 3. Magnitudes of Unemployment Errors (Split by Employment Status) by UI Amount, CPS ASEC Reference Year 2010



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 in the CPS ASEC 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. Note that the difference between the blue and red series indicates the share of UI recipients within each ventile who report being out of the labor force for the entire year.

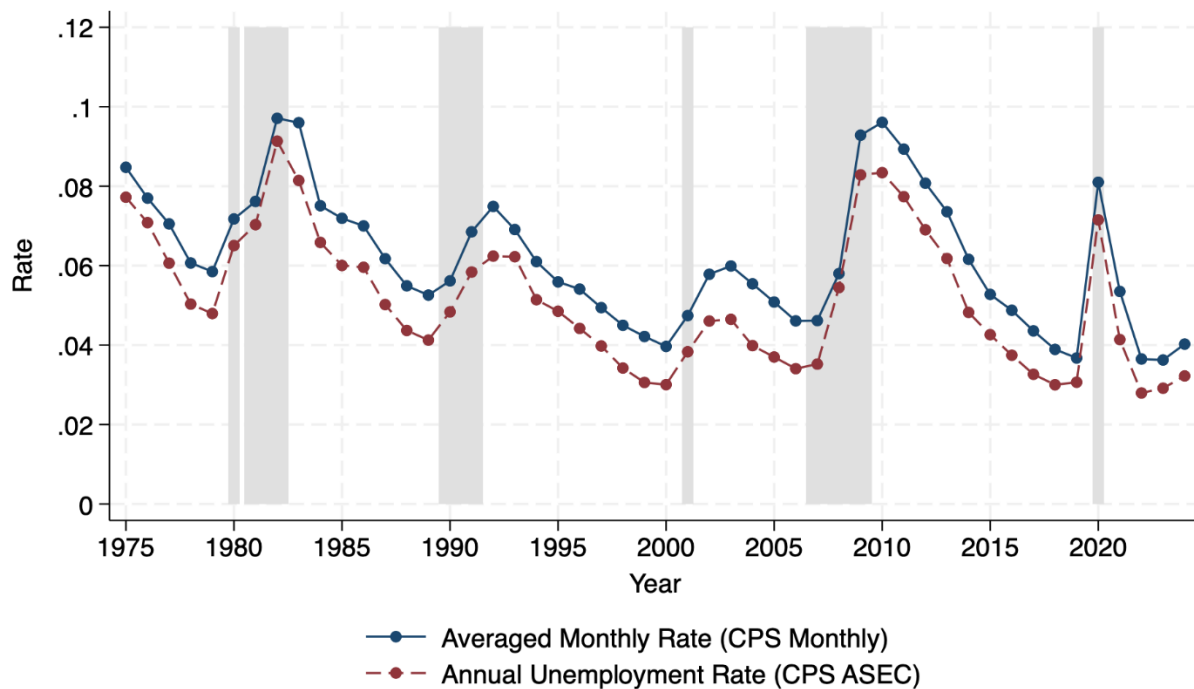
Figure 4. Magnitudes of Unemployment Errors by UI Amount, SIPP 2010



Source: 2008 SIPP Panel (Waves 5-8) and IRS Form 1099-G (Tax year 2010)

Notes: This figure shows the percentage of administrative UI recipients in 2010 who report no unemployment in the SIPP by ventile of administrative UI amount received. This figure also plots the total UI amount within each ventile.

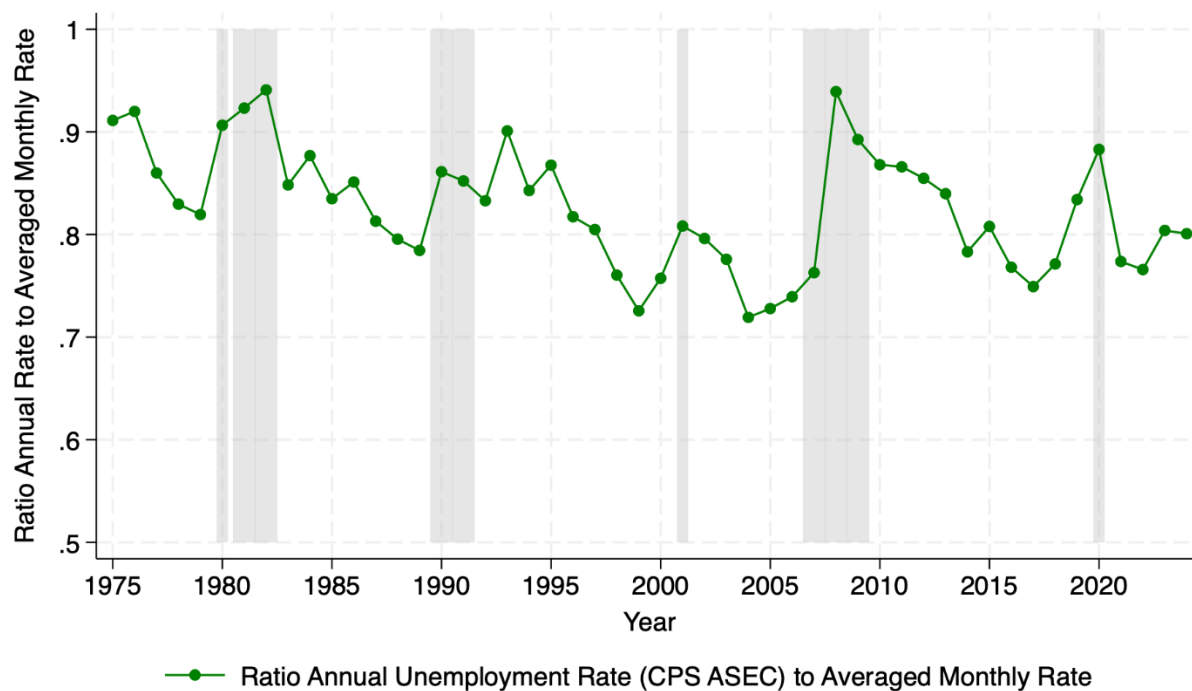
Figure 5. Comparison of ASEC Unemployment Rates and Official Unemployment Rate Over Time, 1975-2023



Source: 1976-2025 CPS ASEC (Reference years 1975-2024)

Notes: This figure displays the average published monthly unemployment rate and the annual unemployment rate (derived from the CPS ASEC) between 1975 and 2024. The annual unemployment rate is defined as the share of reported weeks in the labor force that were unemployed. The grey bars indicate recessions.

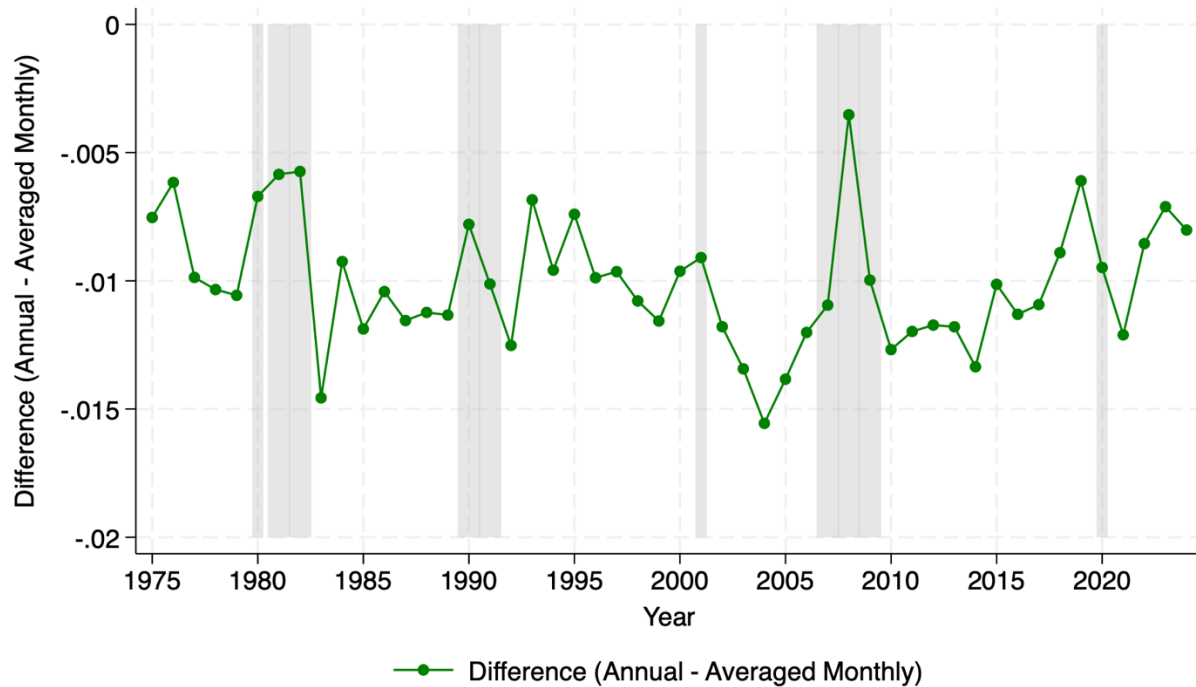
Figure 6. Ratio of Retrospective Unemployment Rate to Annual Average of Official Unemployment Rate, 1975-2024



Source: 1976-2025 CPS ASEC (Reference years 1975-2024)

Notes: This figure displays the ratio of the annual unemployment rate to the average monthly unemployment rate between 1975 and 2024. The average ratio is 0.80 between 1975 and 2024. The grey bars indicate official recessions.

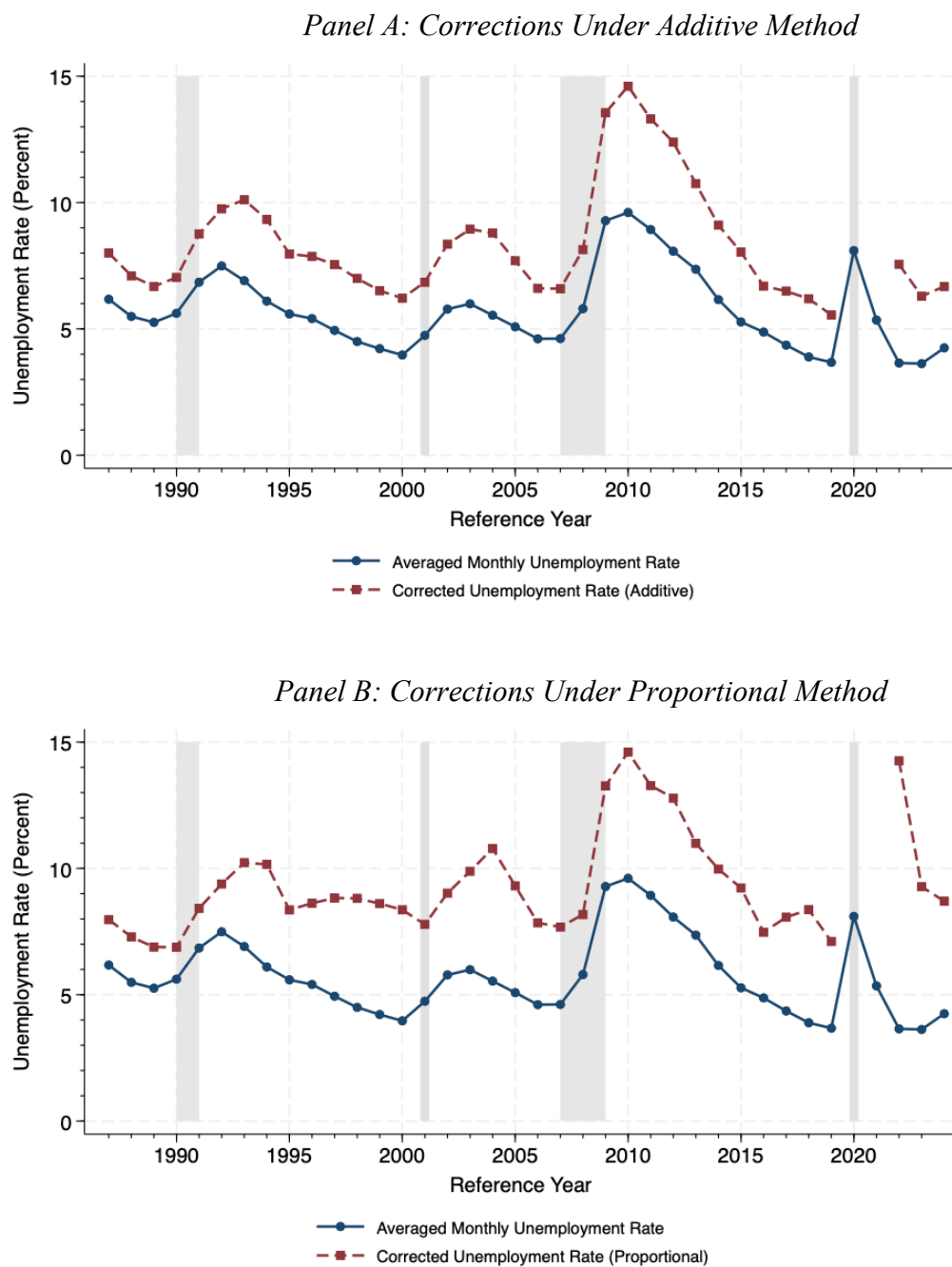
Figure 7. Difference between Retrospective Unemployment Rate and Annual Average of Official Unemployment Rate, 1975-2024.



Source: 1976-2025 CPS ASEC (Reference years 1975-2024)

Notes: This figure displays the difference between the annual unemployment rate and the average monthly unemployment rate between 1975 and 2024. The difference averages -0.01 between 1975 and 2024. The grey bars indicate official recessions.

Figure 8. Uncorrected and Corrected Average Monthly Unemployment Rate, Public-Use Estimates, 1987-2024.



Source: 1988-2025 CPS ASEC (Reference years 1987-2024), IRS Form 1099-G (Calendar Year 2010), BEA

Notes: These figures display the uncorrected average monthly unemployment rate over time, as well as two corrected unemployment rates. Panel A displays the corrected unemployment rate using the additive method to estimate the share of UI recipients reporting no unemployment. Panel B displays the corrected rate using the proportional method. We omit corrected rates in 2020 and 2021 due to how the pandemic affected UI eligibility.

Table 1. Uncorrected and Corrected Unemployment Rates (%), Annual and Average Monthly, 1987-2024.

Year	Annual Rate (1)	Correct for UI Recipients		Additionally Assume Non-Recip. Misreport as Often as Avg. UI Recipients		Additionally Correct for False Positives		Difference, pp (Corrected - Uncorrected)		Adding Diff. to Average Monthly Rate		
		Add.	Prop.	Add.	Prop.	Add.	Prop.	(6) - (1): Add.	(7) - (1): Prop.	Average Monthly Rate	(10) + (8): Add.	(10) + (9): Prop.
		(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
1987	5.0	6.2	6.2	9.6	9.5	6.8	6.8	1.8	1.8	6.2	8.0	8.0
1988	4.4	5.4	5.4	8.7	8.9	6.0	6.2	1.6	1.8	5.5	7.1	7.3
1989	4.1	5.2	5.2	8.2	8.5	5.5	5.7	1.4	1.6	5.3	6.7	6.9
1990	4.8	6.0	6.0	9.0	8.8	6.2	6.1	1.4	1.3	5.6	7.0	6.9
1991	5.8	7.4	7.3	10.4	10.1	7.7	7.4	1.9	1.6	6.8	8.8	8.4
1992	6.2	8.3	8.2	11.2	10.8	8.5	8.1	2.3	1.9	7.5	9.7	9.4
1993	6.2	8.2	8.2	12.1	12.2	9.4	9.5	3.2	3.3	6.9	10.1	10.2
1994	5.1	6.5	6.6	11.1	11.9	8.4	9.2	3.2	4.1	6.1	9.3	10.2
1995	4.9	6.0	6.0	9.9	10.3	7.2	7.6	2.4	2.8	5.6	8.0	8.4
1996	4.4	5.6	5.7	9.6	10.3	6.9	7.6	2.5	3.2	5.4	7.9	8.6
1997	4.0	5.1	5.2	9.3	10.6	6.6	7.9	2.6	3.9	4.9	7.5	8.8
1998	3.4	4.5	4.7	8.6	10.4	5.9	7.7	2.5	4.3	4.5	7.0	8.8
1999	3.1	4.2	4.3	8.1	10.1	5.3	7.4	2.3	4.4	4.2	6.5	8.6
2000	3.0	4.1	4.2	8.0	10.1	5.2	7.4	2.2	4.4	4.0	6.2	8.4
2001	3.8	5.2	5.3	8.6	9.6	5.9	6.9	2.1	3.0	4.7	6.8	7.8
2002	4.6	6.5	6.6	9.9	10.5	7.2	7.8	2.6	3.2	5.8	8.3	9.0
2003	4.6	6.7	6.8	10.3	11.2	7.6	8.5	2.9	3.9	6.0	8.9	9.9
2004	4.0	5.5	5.7	9.9	11.9	7.2	9.2	3.3	5.2	5.5	8.8	10.8
2005	3.7	4.9	5.1	9.0	10.6	6.3	7.9	2.6	4.2	5.1	7.7	9.3
2006	3.4	4.5	4.6	8.1	9.3	5.4	6.6	2.0	3.2	4.6	6.6	7.8
2007	3.5	4.6	4.7	8.2	9.3	5.5	6.6	2.0	3.1	4.6	6.6	7.7
2008	5.4	6.8	6.8	10.5	10.5	7.8	7.8	2.3	2.4	5.8	8.1	8.2
2009	8.3	11.1	11.1	15.3	15.0	12.6	12.3	4.3	4.0	9.3	13.5	13.3
⋮	⋮	⋮	⋮	⋮	⋮	⋮	⋮	⋮	⋮	⋮	⋮	⋮

Table 1 (Continued). Uncorrected and Corrected Unemployment Rates (%), Annual and Average Monthly, 1987-2024.

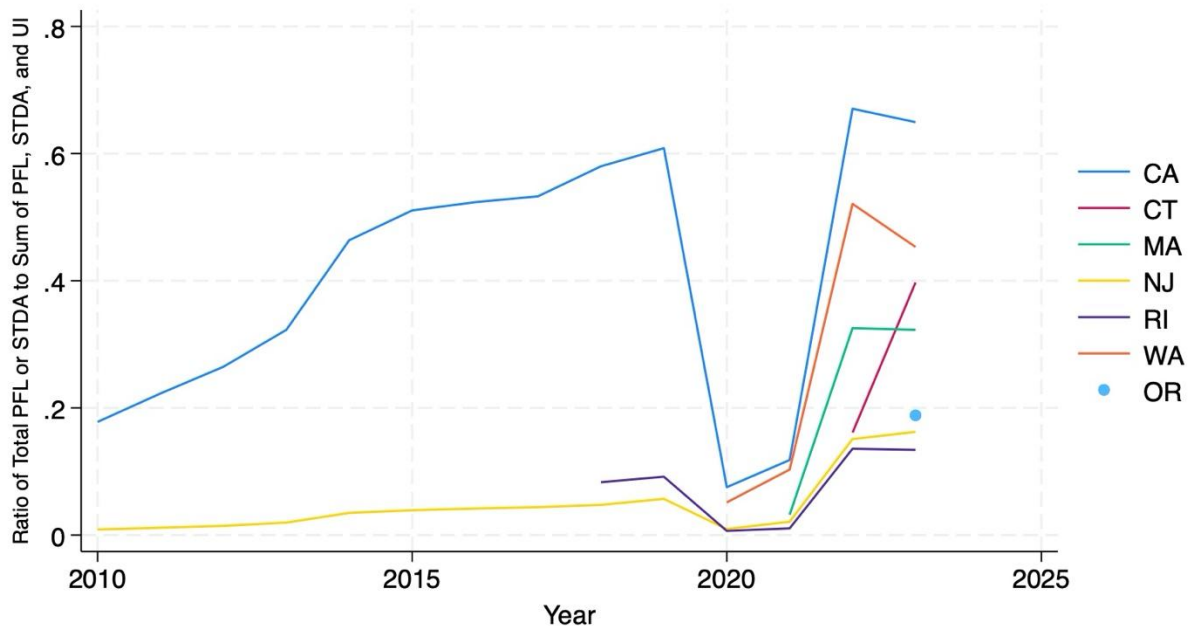
Year	Annual Rate (1)	Correct for UI Recipients		Additionally Assume Non-Recip. Misreport as Often as Avg. UI Recipients		Additionally Correct for False Positives		Difference, pp (Corrected - Uncorrected)		Adding Diff. to Average Monthly Rate		
		Add. (2)	Prop. (3)	Add. (4)	Prop. (5)	Add. (6)	Prop. (7)	(6) - (1): Add. (8)	(7) - (1): Prop. (9)	Average Monthly Rate (10)	(10) + (8): Add. (11)	(10) + (9): Prop. (12)
2010	8.3	11.6	11.6	16.0	16.0	13.3	13.3	5.0	5.0	9.6	14.6	14.6
2011	7.7	10.4	10.4	14.8	14.8	12.1	12.1	4.4	4.3	8.9	13.3	13.3
2012	6.9	9.1	9.2	13.9	14.3	11.2	11.6	4.3	4.7	8.1	12.4	12.8
2013	6.2	7.9	7.9	12.3	12.5	9.6	9.8	3.4	3.6	7.4	10.7	11.0
2014	4.8	5.8	5.9	10.5	11.3	7.8	8.6	2.9	3.8	6.2	9.1	10.0
2015	4.3	5.1	5.2	9.7	10.9	7.0	8.2	2.8	3.9	5.3	8.0	9.2
2016	3.7	4.6	4.7	8.3	9.0	5.6	6.3	1.8	2.6	4.9	6.7	7.5
2017	3.3	4.1	4.2	8.1	9.7	5.4	7.0	2.1	3.7	4.4	6.5	8.1
2018	3.0	3.9	4.0	8.0	10.2	5.3	7.5	2.3	4.5	3.9	6.2	8.4
2019	3.1	3.7	3.8	7.6	9.2	4.9	6.5	1.9	3.4	3.7	5.5	7.1
2020	7.2									8.1		
2021	4.1									5.4		
2022	2.8	3.5	3.6	9.4	16.1	6.7	13.4	3.9	10.6	3.6	7.6	14.2
2023	2.9	3.7	3.9	8.3	11.3	5.6	8.6	2.7	5.6	3.6	6.3	9.3
2024	3.2	3.9	4.0	8.4	10.4	5.6	7.7	2.4	4.4	4.3	6.7	8.7

Data: 1988-2025 CPS ASEC (reference years 1987-2024); IRS Form 1099-G (Tax year 2010); Bureau of Economic Analysis (BEA)

Notes: This table presents uncorrected and corrected versions of the annual unemployment rate and the average monthly unemployment rate from 1987 to 2024, primarily using public-use approximations. We present corrected annual unemployment rates at intermediate points in the correction procedure in Columns (2)-(7). At each intermediate step of the procedure, we present two estimates: one in which the share of UI recipients reporting no unemployment changes percentage point by percentage point with the internal survey inconsistency rate (denoted 'Add.') and one in which we assume that the share changes proportionally with the inconsistency rate. In the last three columns, we present corrections to the average monthly unemployment rate. We correct this rate by adding the percentage point increase in the annual unemployment rate to the uncorrected average monthly rate.

Appendix Figures and Tables

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

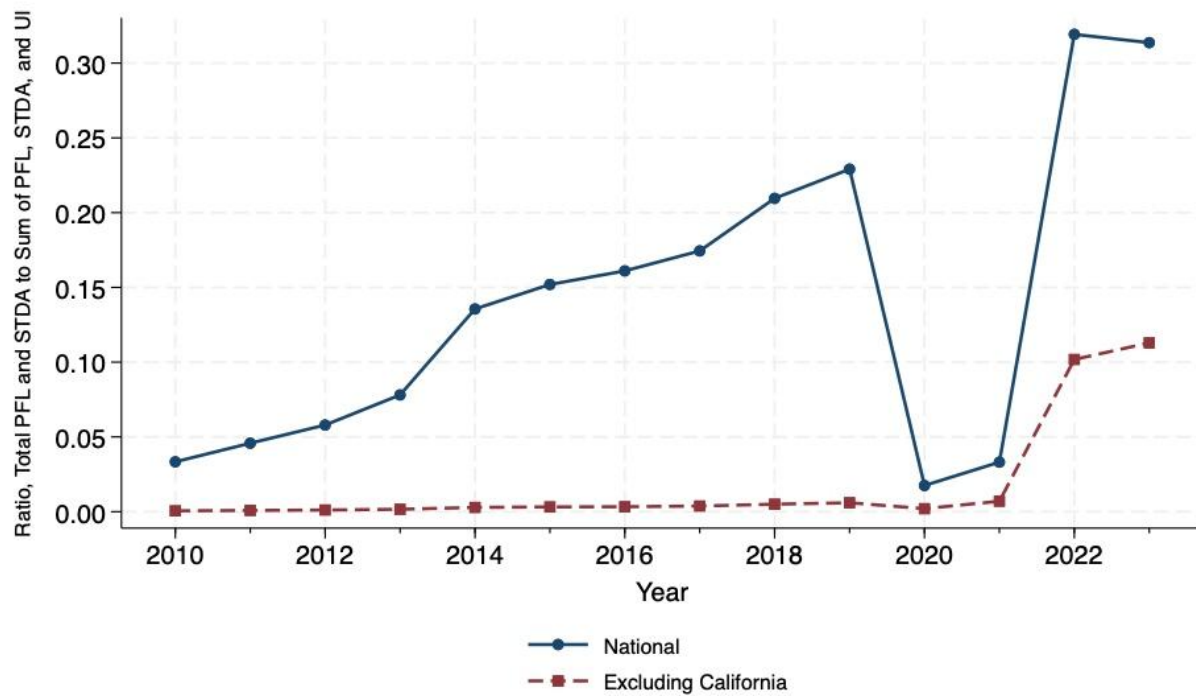


Sources: Department of Labor ETA 2112; State Public Administrative Aggregates
Notes: This graph displays the ratio of total PFL and STDA dollars to total UI dollars over time within each state. PFL benefits in DC and NY are not included due to lack of data.

Source: Department of Labor ETA 2112; State Public Administrative Aggregates

Notes: This graph displays the ratio of total PFL and STDA dollars to the sum of total UI, PFL, and STDA dollars over time within each state offering these programs.

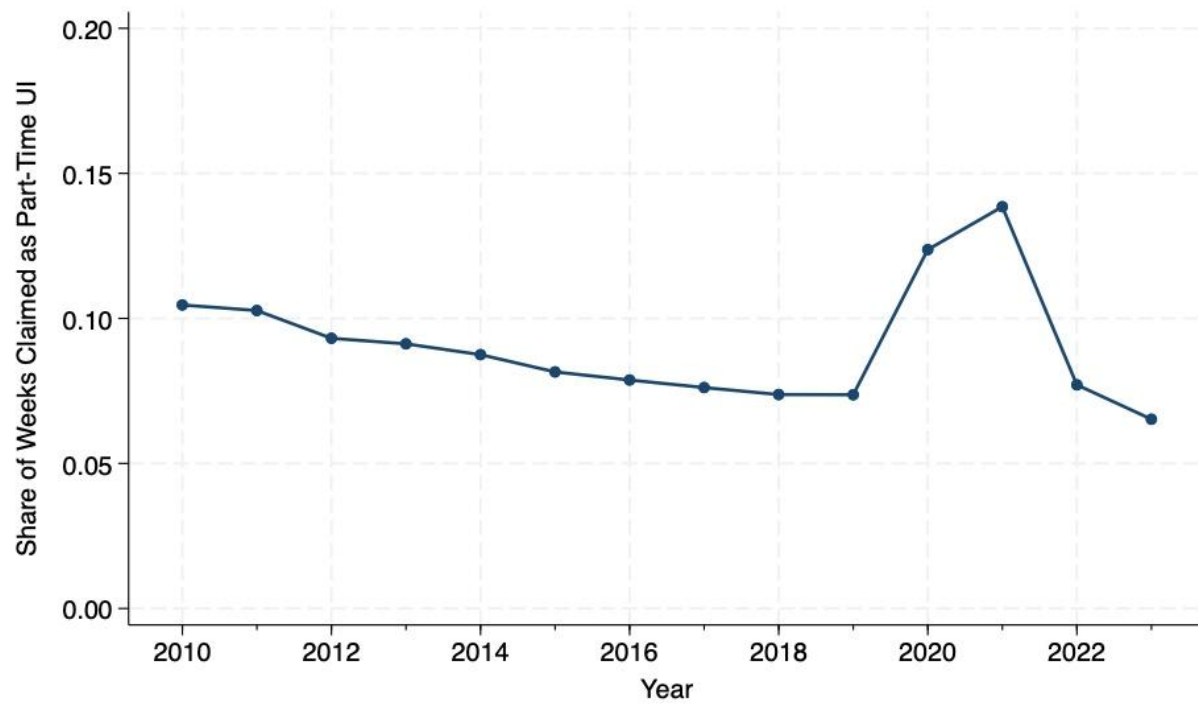
Appendix Figure 2. Ratio of PFL and STDA Dollars to Sum of UI, PFL, and STDA Dollars Nationally and Excluding California, 2010-2023.



Source: Department of Labor ETA 2112; State Public Administrative Aggregates

Notes: This graph displays the ratio of total PFL and STDA dollars to the sum of total UI, PFL, and STDA dollars over time nationally and after excluding California PFL, STDA, and UI. PFL benefits in DC and NY 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 ratio of weeks claimed for part-time UI relative to total weeks claimed for UI between 2010 and 2022.

Table A1. State Paid Family Leave and Short-Term Disability Programs Potentially Reported on Box 1 of IRS Form 1099-G.

State	Program Type	Year Benefits Began	Reporting Rules for Box 1 of Form 1099-G
California	PFL and STDA	PFL: 2004; STDA: 1974	Both PFL and STDA benefits can appear in Box 1. STDA is reported only when it replaces UI for a claimant already receiving UI, in which case the UI work-search requirement is waived.
New Jersey	PFL	2009	PFL benefits are reported in Box 1. New Jersey also operates a temporary disability program, but those benefits are not reported on Form 1099-G.
Rhode Island	PFL	2014	PFL benefits are reported in Box 1.
New York	PFL	2018	PFL benefits are reported on Form 1099-G only when paid through the New York State Insurance Fund. Benefits paid by a private carrier are reported on Form 1099-MISC. Publicly available aggregate benefit totals are unavailable.
District of Columbia	PFL	2020	PFL benefits are reported in Box 1. Publicly available aggregate benefit totals are unavailable.
Washington	Combined	Both: 2020	Only the family leave portion of the combined program is reported in Box 1. Medical leave benefits appear on Form 1099-G only when used as a substitute for UI, in which case the UI work-search requirement is waived. Public aggregates do not separate the two components.
Massachusetts	Combined	Both: 2021	All benefits under the combined program, covering both family and medical leave, are reported in Box 1. Public aggregates do not separate the two components.
Connecticut	PFL	2022	PFL benefits are reported in Box 1.
Oregon	Combined	Both: 2023	Family leave and safe leave benefits are reported in Box 1. Benefits were first paid on September 3, 2023, so 2023 reflects only four months of program operation.

Notes: This table lists states where Paid Family Leave (PFL) or Short-Term Disability Assistance (STDA) benefits can be reported in Box 1 of IRS Form 1099-G. PFL provides partial wage replacement to workers taking leave to care for a new child or an ill family member. STDA provides wage replacement to workers temporarily unable to work because of a non-work related illness. Programs described as combined do not distinguish PFL from STDA in publicly reported aggregates.

Table A2. Inputs to Corrections to the Unemployment Rate, 1987-2024.

Year	IU_t^s (millions)	WU_{at}	LF_{at} (millions)	$IU_t^{UI,s}$ (millions)	$UI \$_t$ (billions)	$UI \$_t^s$ (billions)	R_t^s	$\hat{R}_t$		$\bar{IU}_{at}^{UI,l}$ (millions)		$\bar{IU}_{at}^{nUI,l}$ (millions)		$WU_{at}^{UI,l}$	$WU_{at}^{nUI,l}$
								Additive	Proportional	Additive	Proportional	Additive	Proportional		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
1987	17.71	17.0	5,980.7	6.31	14.6	10.4	30.5%	47.4%	47.2%	4.7	4.7	13.1	13.0	17.4	16.8
1988	16.38	16.2	6,084.3	5.61	13.3	9.5	32.7%	49.7%	50.7%	4.0	3.9	12.4	12.5	16.4	16.2
1989	16.56	15.5	6,211.5	5.96	14.4	10.3	33.0%	50.0%	51.2%	4.2	4.1	12.4	12.5	15.8	15.4
1990	18.87	16.1	6,277.0	7.60	18.2	14.2	29.1%	46.0%	45.0%	5.3	5.4	13.6	13.5	16.5	15.9
1991	20.43	18.1	6,326.1	9.17	26.8	21.7	27.3%	44.2%	42.3%	6.3	6.6	14.1	13.9	19.2	17.5
1992	20.68	19.3	6,389.4	9.69	39.6	27.8	27.1%	44.0%	41.9%	7.7	8.0	12.9	12.7	21.5	18.1
1993	19.99	20.2	6,498.6	8.87	34.8	25.8	31.7%	48.6%	49.1%	6.1	6.1	13.8	13.9	22.0	19.5
1994	18.37	18.4	6,560.1	7.71	23.9	20.5	36.7%	53.6%	56.9%	4.2	3.9	14.2	14.5	18.8	18.2
1995	17.47	18.4	6,641.0	7.03	21.7	18.8	34.2%	51.1%	52.9%	4.0	3.8	13.5	13.6	18.0	18.6
1996	16.36	18.3	6,757.5	6.56	22.3	17.6	36.9%	53.8%	57.2%	3.8	3.6	12.5	12.8	18.0	18.3
1997	15.15	18.0	6,846.5	6.03	20.1	15.9	40.2%	57.1%	62.3%	3.3	2.9	11.9	12.3	17.7	18.1
1998	13.51	17.6	6,934.4	5.91	19.7	15.7	43.4%	60.3%	67.2%	2.9	2.4	10.6	11.1	17.4	17.6
1999	12.60	17.1	7,041.9	5.26	20.5	14.6	45.1%	62.0%	69.9%	2.8	2.2	9.8	10.4	17.3	17.0
2000	12.61	17.2	7,236.2	5.22	20.7	14.8	45.3%	62.2%	70.2%	2.8	2.2	9.8	10.4	16.6	17.4
2001	15.33	18.2	7,262.7	7.37	31.9	24.3	38.7%	55.7%	60.0%	4.3	3.9	11.0	11.5	18.4	18.0
2002	16.32	20.7	7,347.1	8.35	53.5	37.9	36.4%	53.3%	56.4%	5.5	5.1	10.8	11.2	22.4	19.9
2003	16.03	21.3	7,360.2	8.22	53.2	36.9	37.9%	54.8%	58.7%	5.3	4.9	10.7	11.1	22.8	20.6
2004	14.71	20.1	7,411.2	6.35	36.4	25.1	43.0%	59.9%	66.6%	3.7	3.1	11.0	11.6	20.1	20.1
2005	14.19	19.6	7,504.5	5.82	31.8	22.3	42.0%	59.0%	65.1%	3.4	2.9	10.8	11.3	19.0	19.8
2006	14.08	18.4	7,619.4	5.23	30.4	20.7	41.0%	58.0%	63.6%	3.2	2.8	10.8	11.3	18.9	18.3
2007	14.71	18.4	7,694.7	5.20	32.7	21.9	40.1%	57.0%	62.1%	3.3	3.0	11.4	11.8	19.2	18.2
2008	20.55	20.6	7,761.7	8.07	51.1	38.9	31.1%	48.1%	48.2%	5.5	5.5	15.0	15.1	21.2	20.4
2009	25.41	25.3	7,747.8	12.88	131.2	98.7	28.7%	45.6%	44.5%	9.3	9.5	16.1	15.9	28.2	23.6
⋮	⋮	⋮	⋮	⋮	⋮	⋮	⋮	⋮	⋮	⋮	⋮	⋮	⋮	⋮	⋮

Table A2 (continued). Inputs to Corrections to the Unemployment Rate, 1987-2024.

Year	IU_t^s (millions)	$Weeks_l$	LF_{at} (millions)	$IU_t^{UI,s}$ (millions)	$UI \$_t$ (billions)	$UI \$_t^s$ (billions)	R_t^s	Additive	Proportional	$\hat{R}_t$		$\bar{U}_{at}^{UI,l}$ (millions)		$\bar{U}_{at}^{nUI,l}$ (millions)		$WeeksU_{at}^{UI}$	$WeeksU_{at}^{nl}$
										Additive	Proportional	Additive	Proportional	Additive	Proportional		
										(1)	(2)	(3)	(4)	(5)	(6)		
2010	24.69	26.1	7,712.7	12.42	138.9	97.2	30.8%	47.7%	47.7%	9.3	9.3	15.4	15.4	29.3	24.3		
2011	23.41	25.6	7,745.1	10.23	107.2	69.6	30.6%	47.5%	47.4%	8.3	8.3	15.1	15.1	27.6	24.7		
2012	22.07	24.4	7,809.2	8.64	83.6	55.0	33.3%	50.3%	51.6%	6.5	6.4	15.5	15.7	26.1	23.9		
2013	20.54	23.7	7,861.3	6.63	62.5	38.8	32.6%	49.5%	50.5%	5.4	5.3	15.1	15.3	24.8	23.3		
2014	17.43	21.8	7,877.1	4.58	35.5	22.7	37.1%	54.0%	57.5%	3.3	3.0	14.1	14.4	20.4	22.1		
2015	16.65	20.5	7,993.9	4.21	32.6	20.6	39.3%	56.3%	61.0%	2.9	2.6	13.7	14.0	19.1	20.7		
2016	15.16	19.9	8,044.6	3.87	32.0	18.7	37.9%	54.8%	58.6%	3.0	2.7	12.2	12.4	19.0	20.0		
2017	14.12	18.8	8,119.5	3.67	30.2	18.2	42.8%	59.7%	66.3%	2.5	2.1	11.7	12.1	18.9	18.8		
2018	12.81	19.2	8,197.6	3.20	27.6	15.5	45.5%	62.4%	70.5%	2.1	1.7	10.7	11.1	20.1	19.1		
2019	13.36	19.2	8,353.1	3.39	27.5	17.8	43.0%	59.9%	66.6%	2.1	1.7	11.3	11.6	18.0	19.4		
2020	25.88	22.5	8,104.6														
2021	15.81	21.5	8,213.8														
2022	12.61	18.5	8,336.6	3.07	23.8	16.3	53.4%	70.3%	82.7%	1.3	0.8	11.3	11.8	19.3	18.4		
2023	13.40	18.4	8,453.5	3.32	33.3	19.7	47.9%	64.8%	74.2%	2.0	1.5	11.4	11.9	19.2	18.3		
2024	14.39	19.3	8,613.8	3.57	36.5	24.7	44.5%	61.4%	68.9%	2.0	1.6	12.4	12.8	19.2	19.3		

Data: 1988-2025 CPS ASEC (reference years 1987-2024); IRS Form 1099-G (Tax year 2010); Bureau of Economic Analysis (BEA)

Notes: This table presents inputs into the public-use correction methodology outlined in Section 4. All items in this table are used to calculate uncorrected and corrected annual unemployment rates from the CPS ASEC.

Table A3. Uncorrected and Corrected Unemployment Rates (%), Annual and Average Monthly, Assuming Non-Recipients Misreport As Often As Lowest Ventile Recipients, 1987-2024.

Year	Annual Rate	Correct for UI Recipients		Additionally Assume Non-Recip. Misreport as Often as Lowest Ventile Recip. In 2010		Additionally Correct for False Positives		Difference, pp (Corrected - Uncorrected)		Adding Diff. to Average Monthly Rate		
		Add.	Prop.	Add.	Prop.	Add.	Prop.	(6) - (1): Add.	(7) - (1): Prop.	Average Monthly Rate	(10) + (8): Add.	(10) + (9): Prop.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
1987	5.0	6.2	6.2	21.5	21.5	18.8	18.8	13.8	13.8	6.2	20.0	19.9
1988	4.4	5.4	5.4	19.1	19.3	16.4	16.6	12.1	12.2	5.5	17.6	17.7
1989	4.1	5.2	5.2	17.9	18.1	15.2	15.4	11.1	11.2	5.3	16.4	16.5
1990	4.8	6.0	6.0	20.4	20.2	17.7	17.5	12.8	12.7	5.6	18.4	18.3
1991	5.8	7.4	7.3	23.6	23.3	20.9	20.6	15.1	14.7	6.8	21.9	21.6
1992	6.2	8.3	8.2	23.6	23.2	20.9	20.5	14.6	14.2	7.5	22.1	21.7
1993	6.2	8.2	8.2	25.4	25.5	22.7	22.8	16.5	16.6	6.9	23.4	23.5
1994	5.1	6.5	6.6	22.9	23.3	20.2	20.6	15.1	15.5	6.1	21.2	21.6
1995	4.9	6.0	6.0	21.7	21.9	19.0	19.2	14.1	14.4	5.6	19.7	19.9
1996	4.4	5.6	5.7	19.7	20.1	17.0	17.4	12.6	13.0	5.4	18.0	18.4
1997	4.0	5.1	5.2	18.1	18.7	15.4	16.0	11.4	12.0	4.9	16.4	16.9
1998	3.4	4.5	4.7	15.7	16.4	13.0	13.7	9.6	10.3	4.5	14.1	14.8
1999	3.1	4.2	4.3	14.0	14.8	11.3	12.1	8.3	9.0	4.2	12.5	13.2
2000	3.0	4.1	4.2	13.9	14.6	11.2	11.9	8.2	8.9	4.0	12.2	12.9
2001	3.8	5.2	5.3	16.6	17.1	13.9	14.4	10.1	10.6	4.7	14.8	15.3
2002	4.6	6.5	6.6	18.7	19.2	16.0	16.5	11.4	11.9	5.8	17.2	17.7
2003	4.6	6.7	6.8	19.1	19.8	16.4	17.1	11.8	12.4	6.0	17.8	18.4
2004	4.0	5.5	5.7	17.9	18.7	15.2	16.0	11.2	12.1	5.5	16.7	17.6
2005	3.7	4.9	5.1	16.7	17.4	14.0	14.7	10.3	11.0	5.1	15.4	16.1
2006	3.4	4.5	4.6	15.4	15.9	12.7	13.2	9.2	9.8	4.6	13.9	14.4
2007	3.5	4.6	4.7	15.8	16.3	13.1	13.6	9.6	10.1	4.6	14.2	14.7
2008	5.4	6.8	6.8	23.2	23.3	20.5	20.6	15.1	15.1	5.8	20.9	20.9
2009	8.3	11.1	11.1	31.5	31.2	28.8	28.5	20.6	20.2	9.3	29.8	29.5
⋮	⋮	⋮	⋮	⋮	⋮	⋮	⋮	⋮	⋮	⋮	⋮	⋮

Table A3 (continued). Uncorrected and Corrected Unemployment Rates (%), Annual and Average Monthly, Assuming Non-Recipients Misreport As Often As Lowest Ventile Recipients, 1987-2024.

Year	Annual Rate	Correct for UI Recipients		Additionally Assume Non-Recip. Misreport as Often as Lowest Ventile Recip. In 2010		Additionally Correct for False Positives		Difference, pp (Corrected - Uncorrected)		Adding Diff. to Average Monthly Rate		
		Add.	Prop.	Add.	Prop.	Add.	Prop.	(6) - (1): Add.	(7) - (1): Prop.	Average Monthly Rate	(10) + (8): Add.	(10) + (9): Prop.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
2010	8.3	11.6	11.6	31.8	31.8	29.1	29.1	20.9	20.9	9.6	30.5	30.5
2011	7.7	10.4	10.4	30.5	30.5	27.8	27.8	20.1	20.1	8.9	29.0	29.0
2012	6.9	9.1	9.2	28.8	29.1	26.1	26.4	19.2	19.5	8.1	27.3	27.6
2013	6.2	7.9	7.9	26.6	26.7	23.9	24.0	17.7	17.8	7.4	25.0	25.2
2014	4.8	5.8	5.9	22.3	22.6	19.6	19.9	14.8	15.1	6.2	20.9	21.3
2015	4.3	5.1	5.2	19.9	20.4	17.2	17.7	13.0	13.4	5.3	18.3	18.7
2016	3.7	4.6	4.7	17.2	17.5	14.5	14.8	10.7	11.1	4.9	15.6	15.9
2017	3.3	4.1	4.2	15.3	15.8	12.6	13.1	9.4	9.8	4.4	13.7	14.2
2018	3.0	3.9	4.0	14.2	14.7	11.5	12.0	8.5	9.0	3.9	12.4	12.9
2019	3.1	3.7	3.8	14.6	15.0	11.9	12.3	8.8	9.2	3.7	12.5	12.9
2020	7.2									8.1		
2021	4.1									5.3		
2022	2.8	3.5	3.6	13.9	14.5	11.2	11.8	8.4	9.0	3.6	12.0	12.6
2023	2.9	3.7	3.9	14.0	14.6	11.3	11.9	8.4	9.0	3.6	12.0	12.6
2024	3.2	3.9	4.0	15.5	15.9	12.8	13.2	9.5	10.0	4.3	13.8	14.2

Data: 1988-2025 CPS ASEC (reference years 1987-2024); IRS Form 1099-G (Tax year 2010); Bureau of Economic Analysis (BEA)

Notes: This table presents uncorrected and corrected versions of the annual unemployment rate and the average monthly unemployment rate from 1987 to 2024, primarily using public-use approximations where we assume that non-recipients misreport as often as the lowest ventile UI recipients in 2010 (80.6%). We present corrected annual unemployment rates at intermediate points in the correction procedure in Columns (2)-(7). At each intermediate step of the procedure, we present two estimates: one in which the share of UI recipients reporting no unemployment changes percentage point by percentage point with the internal survey inconsistency rate (denoted ‘Add.’) and one in which we assume that the share changes proportionally with the inconsistency rate. In the last three columns, we present corrections to the average monthly unemployment rate. We correct this rate by adding the percentage point increase in the annual unemployment rate to the uncorrected average monthly rate.