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The Labor Market Impact of a Pandemic: Validation and Application of a Do-It-Yourself CPS

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The Labor Market Impact of a Pandemic: Validation and Application of a Do-It-Yourself CPS[*]

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Abstract

The Current Population Survey (CPS) is a central source of US labor market data. We show that, for a few thousand dollars, researchers can quickly design and implement their own online survey to supplement the CPS. The survey closely follows core features of the CPS, ensuring that outcomes are conceptually compatible and allowing researchers to weight and validate results using the official CPS. Yet the survey also allows for faster data collection, added flexibility, and novel questions. We show that the survey provided useful estimates of US labor market aggregates several weeks ahead of the CPS during the turbulent start of the Covid-19 recession. We then assess the extent of downward nominal wage rigidity at the onset of the pandemic, finding that wage reductions were widespread, but were more common for job-switchers and recalled workers. We discuss a wide range of additional applications.

Keywords: labor market survey, real-time data, nominal wage rigidity

JEL Classifications: E24, J21, C81

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1 Introduction

For over half a century, a central source of micro data for the US labor market has been the Current Population Survey (CPS). The key characteristics which make the CPS such a valuable data source are (i) a large, representative sample, (ii) a monthly sample frequency, (iii) a rotating panel design, (iv) a large number of variables on demographics, labor market outcomes, and family linkages, and (v) publicly available micro data which is comparable over several decades.

Yet, like all datasets, the CPS has limitations. Considerations of survey length and historical comparability imply that some important questions are asked infrequently or not at all, and individual researchers have little say as to which questions are included. Further, the large scale of the CPS necessitates a substantial amount of time to collect and process results: data is three weeks old on the day it is released, and can be up to eight weeks old before the next release. In normal times this results in only minor inconvenience, but during fast-moving crises such a lag can cause labor market data to be painfully out of date.

This paper introduces a new survey designed to complement the CPS and address these limitations. Essentially, we use the official CPS as the scaffolding for a novel, national online labor market survey, which we refer to as the Real-Time Population Survey (RPS).[1] The RPS leverages the strengths of the CPS by closely following key aspects of the CPS, which ensures outcomes are *conceptually compatible* across surveys. This allows researchers to weight and validate results against a high quality baseline survey with a larger sample size, addressing concerns of sample selection and measurement error. Yet because the RPS can collect data in nearly *real-time*, it can provide labor market estimates ahead of the CPS in time-sensitive situations. The RPS can also collect information which does not appear in the CPS, providing added *flexibility* that allows researchers to address a wider array of research questions. Importantly, our approach is relatively inexpensive and does not require researchers to operate their own survey infrastructure.

We contribute to a rapidly growing literature using innovative labor market data, in particular in the context of the Covid-19 pandemic. For example, Kudlyak and Wolcott (2020), Forsythe et al. (2020), Kurmann et al. (2020), Bartik et al. (2020b), Cajner et al. (2020), and Chetty et al. (2020) analyze data from layoff notifications, vacancy postings, payroll processing, and other sources to complement traditional labor market data. Among many surveys analyzing the labor market effects of Covid-19, the most closely related is contemporaneous work by Foote et al. (2020), who also run a repeated online survey that closely follows the core labor market module in the CPS

[1] Since late April, the RPS is conducted in collaboration with the Federal Reserve Bank of Dallas.

but unlike us do not collect labor market information about spouses/partners who are living in the same household as the survey respondent.[2,3]

Section 2 provides an overview of the RPS, which was conducted twice per month from late March through September and monthly since then. We discuss key features of the RPS design and implementation, highlighting areas of overlap and divergence with the CPS. In particular, we follow questions on demographics and labor market outcomes as outlined in the CPS Interviewing Manual (US Census Bureau, 2015), adopting the same word-for-word phrasing when practical. We use these questions to target a sample that is nationally representative along several dimensions and to construct sample weights to address selection concerns, using the CPS as a benchmark.

Section 3 takes further advantage of overlap between the two surveys to validate RPS outcomes untargeted by our weighting procedure. Retrospective questions in the RPS about earnings, employment, industry, and hours worked in February 2020 produce statistics which are quantitatively in line with the CPS. We then show that the RPS also provided useful and timely estimates of US labor market aggregates during the first several months of the Covid-19 recession. Time series in the RPS for labor force participation, employment, unemployment, hours worked, and earnings are similar in both level and trend to the CPS, but available several weeks earlier and twice as often.

Bolstered by the above comparisons to the CPS, Section 4 demonstrates the utility of novel research questions on wage changes and firm tenure by analyzing the extent of downward nominal wage rigidity at the onset of the Covid-19 recession. Although some supplements of the CPS contain information on wages and firm tenure, we explain why they are not well-suited for this analysis. We find that nominal wage cuts were widespread at the onset of the pandemic, reaching 11-12% of job-stayers in April and May, similar to a finding by Cajner et al. (2020) based on administrative data. From April through October the prevalence of wage cuts was fairly flat for job-stayers, but increased among job-switchers, exceeding 21% for the latter group in October. Workers who were temporarily laid off and then recalled were 50% more likely to experience a wage cut compared to those who were never laid off, suggesting that recalls may be one method of overcoming downward wage rigidity. Among salaried workers, weekly wage cuts became much

[2]The two surveys were initially created without prior knowledge of each other. The first RPS results were made public on April 15, referencing the week of March 29-April 4. Foote et al. (2020) shared a preliminary draft with us on April 24. Their results were first made public on June 24, with an initial reference week of April 5-11.

[3]For other novel survey evidence for the US, see e.g. Adams-Prassl et al. (2020), Barrero et al. (2020), Bartik et al. (2020a), Belot et al. (2020), Bell and Blanchflower (2020), and Coibion et al. (2020). A related development is the launching of the Census Pulse Survey, a large scale online survey collecting information on the impact of the Covid-19 pandemic, which ran from late April-July 2020, and then again from late August-October 2020. None of these datasets allow for the construction of labor market status consistent with the CPS.

less common from April to October, while hourly wage cuts have persisted; these divergent trends emphasize the value of data on hours, which most administrative datasets lack. Collectively, these findings are informative for the calibration and validation of models with nominal wage rigidities, such as Baqaee and Farhi (2020) and Guerrieri et al. (2020).

We view the RPS as a proof of a more general survey concept, which leverages existing large-scale, high-quality surveys like the CPS to enhance the credibility and comparability of novel surveys with specialized research questions. Section 5 discusses many additional promising applications of this approach related to work from home, changes in labor market outcomes around migration, wage growth after job loss, and family and educational background.

2 Survey Design and Sampling in the RPS and CPS

The RPS is a national online labor market survey of indviduals aged 18-64. The survey is designed to mirror the CPS along key dimensions, while also collecting additional information not available in the CPS. We now briefly review central aspects of sampling and design in each survey, pointing out areas of overlap as well as key differences. We analyze data from April through October, for which we have comparable CPS data.

2.1 Sample Selection

The CPS is a monthly survey of roughly 60,000 households, where a household is a residential address. The sample design is a rotating panel: a given household is interviewed for four months in a row, not interviewed for the next 8 months, and then is interviewed again for four more months. Typically, one member of each household reports on behalf of all other members age 15 and over.

While the CPS uses probability-based sampling, the RPS uses a "convenience-based" sampling procedure provided by Qualtrics, a survey company widely used by academics; see Adams-Prassl et al. (2020) and Dietrich et al. (2020) for recent examples. Survey invitations are sent to participants in Qualtrics' online panels, who can choose to take the survey and receive between 30% to 50% of the $5 we pay per completed survey.[4] The online panels are not a random sample of the US population, even if one would condition on the 94% of individuals aged 18-64 living in households with internet access (based on the 2019 American Community Survey). However, Qualtrics allows

[4] Qualtrics acts as a panel aggregator and distributes the survey to their partners' actively managed market research panels. All panel members opt-in to receive survey invitations, and can take any surveys to which they are invited; see Qualtrics (2014) for details. The panels used for the RPS include about 15 million people in the US. Survey responses which are completed too quickly are dropped to avoid concerns that respondents did not answer questions carefully.

researchers to target the survey to desired demographic groups. The RPS sample was selected to be nationally representative along several broad demographic variables: sex, age, race and ethnicity, education, marital status, number of children in the household, Census region, and 2019 annual household income. The RPS typically collected 1,500 to 2,000 responses per survey wave. Appendix C.1 provides additional details and summary statistics.

The above sampling strategy may still yield a sample that deviates from the CPS in observable characteristics for several reasons. First, the targeted moments are never matched exactly. Second, similar to the CPS, we also ask respondents to answer questions about live-in spouses or partners. While this increases our sample by about 60%, spouses and partners are not taken into account by the Qualtrics sampling procedure. Finally, given limitations in sample size, it is infeasible to match very fine breakdowns or combinations of these targets. We address these issues by constructing sample weights using an iterative proportional fitting (raking) algorithm based on Deming and Stephan (1940). To address selection into our sample based on recent labor market history, we also include in our weighting scheme the share of individuals who "work for pay or profit" close to the reference week of the most recent CPS *prior* to the survey. Additional details on weighting can be found in Appendix C.4.

2.2 Survey Design

The CPS is composed of a core module that is asked each month, and a set of supplemental modules that are asked less frequently. The core CPS asks a host of demographic questions, as well as several questions about household members' labor market status. Labor market questions are used to assign individuals to one of five labor market categories: (1) employed/at work, (2) employed/absent, (3) unemployed/on layoff, (4) unemployed/searching, and (5) not in labor force. This assignment requires over a dozen questions with a combination of yes/no or open-ended answer options, and a nontrivial flow/skip process based on previous answers. The core CPS collects additional work-related information, including hours of work, employer type, occupation, and industry. In a household's fourth and eighth interviews, the CPS Outgoing Rotation Group (ORG) supplement asks about workers' usual earnings.

The RPS follows questions on demographics and labor market outcomes in the core CPS and CPS-ORG as outlined in the CPS Interviewing Manual (US Census Bureau, 2015), using the same word-for-word phrasing when practical. In particular, the survey replicates the intricate sequence of questions necessary to assign labor market status. Appendix C.2-C.3 provide additional details on the assignment of labor market status in the RPS.

A key strength of the CPS is a rotating panel, which allows researchers to study changes in labor market outcomes over time; for example, a large literature uses CPS data to discipline models of labor market flows. While the RPS is a one-shot survey, it obtains (quasi-) panel information using retrospective questions. In addition to asking about labor market outcomes last week, as the CPS does, the RPS also asks about outcomes in February 2020 providing a reference point just prior to the Covid-19 shock.

Finally, the RPS also asks a suite of questions not included in the basic CPS. These include questions on work from home, receipt of unemployment insurance, and worker expectations about recall under various hypothetical scenarios. Our analysis of wage changes in Section 4 makes use of information on firm tenure and worker recall, which the CPS does not observe.

3 Validation and Real-Time Labor Market Estimates

This section documents the extent to which outcomes in the RPS are consistent with those in the CPS. First, we compare labor market outcomes in the RPS to counterparts in the CPS for February 2020, the month prior to the onset of the Covid-19 pandemic. Next, we show that the RPS is capable of delivering useful labor market forecasts ahead of the CPS during the first several months of the pandemic.

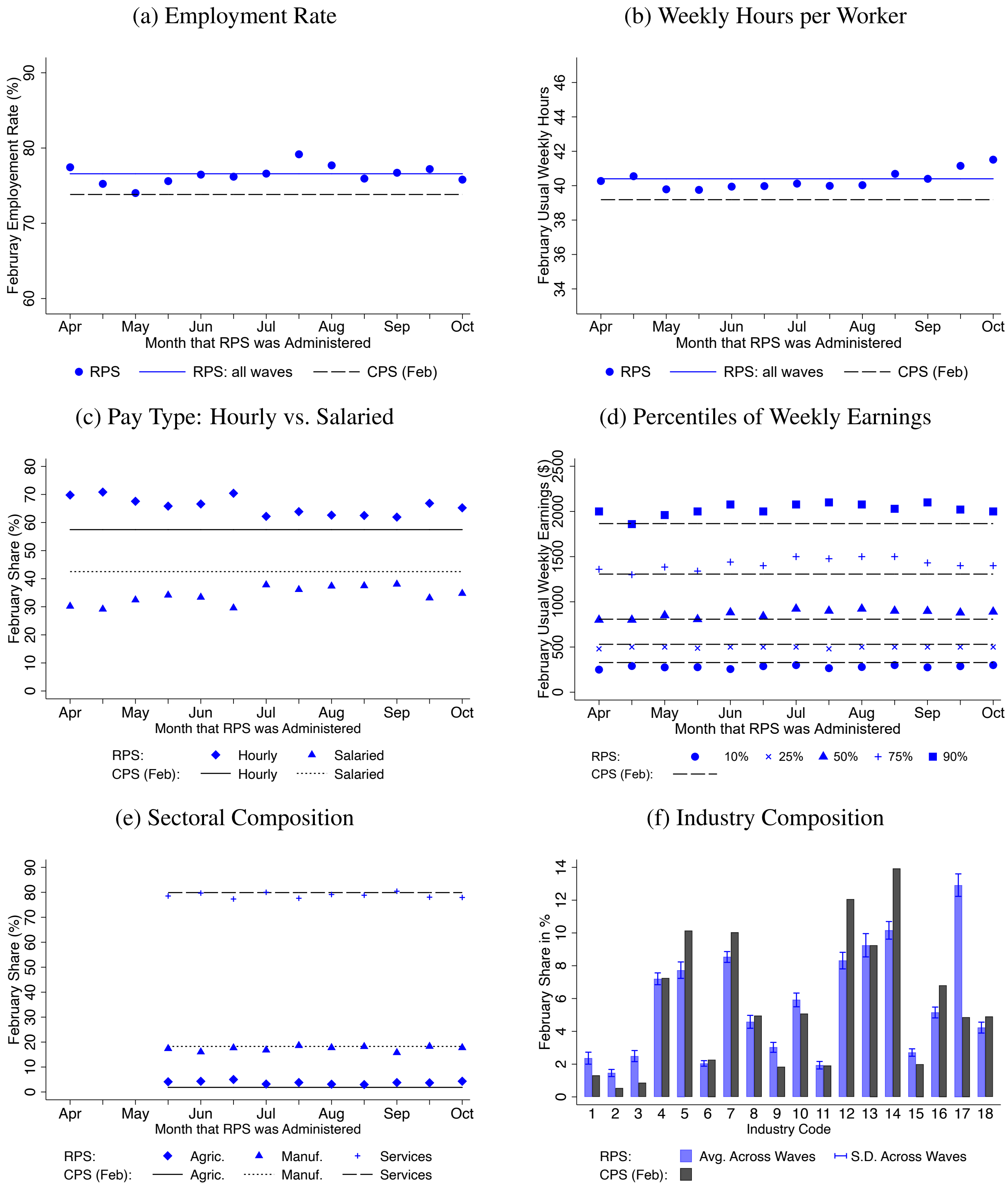
Although we note some discrepancies, along most dimensions the two surveys closely agree. We view these comparisons with the CPS as validation tests of our sample. While many variables that we use for sample selection or weighting are of course correlated with current or February labor market outcomes, we do not explicitly use these moments either for sample selection or weighting. That is, we do not match any moments from Sections 3.1 or 3.2 by construction.

3.1 Pre-Crisis Comparisons to the CPS

The RPS asks respondents whether they were employed in February 2020, the month before the onset of the Covid-19 pandemic. Respondents were also asked about usual hours of work, usual earnings, and industry in February. Figure 1 compares these outcomes to their CPS counterparts.

Figure 1a displays February employment in each RPS wave since April. The mean February employment rate across RPS surveys is 76.6%, slightly higher than the 73.8% employment rate in the February CPS. The standard deviation of February employment across RPS waves is 1.4%.

Figure 1: February Labor Market Outcomes in the RPS and CPS, Age 18-64



Notes: Figure contains data from retrospective questions for February in the RPS, alongside data from the February 2020 CPS. Industries are as follows: 1, Agriculture, forestry, fishing, and hunting, 2, Mining, 3, Utilities, 4, Construction, 5, Manufacturing, 6, Wholesale trade, 7, Retail trade, 8, Transportation and warehousing, 9, Information services, 10, Financial services, 11, Real estate, 12, Professional and business services, 13, Educational services, 14, Health services, 15, Arts, Entertainment, and Recreation, 16, Hotel, Accommodation, and Food Services, 17, Other services, 18, Government (including Armed Forces). The RPS only collected industry results consistently from mid-May onward. In the RPS information on pay type and industry is missing for 2.6% and 0.4% of employed individuals in February, respectively.

Figure 1b displays mean usual weekly hours worked in February in each RPS wave since April, where the sample is restricted to those who were employed in February. The mean usual work week is 40.0 hours, with a standard deviation of 0.4 hours across survey waves. This is close the 39.1 mean hours per week in the February CPS. Similarly, the standard deviation of usual hours in February is also somewhat higher in the RPS (13.9 vs. 10.3 hours).

Figures 1c and 1d compare earnings outcomes in the RPS and CPS. Figure 1c displays the share of hourly and salaried workers. Averaging across all RPS survey waves, 65.9% of workers report being paid hourly in February, versus 57.5% in the February CPS. Figure 1d displays the distribution of usual weekly earnings in February.[5] As with hours worked, the earnings distribution is similar between surveys, with somewhat larger dispersion in the RPS. Averaging across all waves, the mean of log earnings in the RPS and CPS is 6.674 and 6.676, respectively; the standard deviation of log earnings is 0.786 and 0.681, respectively. The magnitude of the differences in earnings distributions between the RPS and CPS are comparable to other data sets. For example, Fitzgerald et al. (1998) find that among male household heads mean earnings are 2-5% larger and the log variance is 6-12 log points lower in the PSID than in the CPS-ASEC, respectively.

Figure 1e displays the sectoral composition in February in the RPS and CPS. The RPS only collected information on sector and industry for February consistently from mid May onward. The two surveys display nearly identical sectoral compositions for February, and the composition hardly varies across RPS waves. Figure 1f provides a more detailed comparison of the composition across 18 major industries in February. The correlation between RPS and CPS industry shares is 0.78. The industries in which the RPS most undershoots the CPS are professional and business services (8.3% vs. 12.1%) and health services (10.3% vs. 13.9%); the largest RPS overshoot relative to the CPS is in "other services" (13.0% vs. 4.9%). These disparities may be attributable to some individuals in the service sector not knowing which industry to select. Appendix B.1 shows that the RPS also lines up reasonably well with four broad self-reported industries in the CPS February.

We view these results as validating two central aspects of the RPS design. First, the RPS is broadly consistent with the CPS for several labor market variables of interest. Second, and

[5]To ensure comparability and minimize concerns over measurement error, we restrict both the RPS and CPS samples to individuals with (i) weekly earnings below the CPS topcode of $2,884.61, (ii) an implied hourly wage exceeding half the federal minimum wage of $3.62, and (iii) no business in the household (Figure B.2a in Appendix B shows that the RPS and CPS contain a similar share of households with a business). In the CPS, 32.7% individuals do not report earnings, 3.1% earn less than $3.62 per hour, and 1.1% earn more than $2,884.61. The respective numbers in the RPS are 6.5%, 6.8%, and 15.5%.

crucially, this consistency is obtained despite the fact that respondents are answering *retrospective* questions about labor market outcomes that happened several months in the past. This suggests that it is feasible to include a (quasi-) panel aspect even in a single-shot survey like the RPS. Given the centrality of panel data to modern economic research, this opens the door to a much larger set of potential research questions.

3.2 Real-Time Estimates During the Covid-19 Crisis

Because the CPS is conducted monthly, and requires a three week processing period before results are made public, labor market statistics based on the CPS are perpetually between three and eight weeks out of date. For example, on May 7, the day before the April CPS was published (which found an official unemployment rate of 14.7%) , the latest CPS statistics referred to the week of March 8-14 (which found an unemployment rate of 4.5%). By contrast, one primary benefit of the RPS is the ability to produce high frequency labor market estimates in nearly real-time. Collection of 2,000 valid responses takes 2-3 days, and the results can be processed almost instantly. In this section, we show that the RPS produces labor market estimates that are quantitatively comparable with official CPS measures, but available several weeks earlier and updated twice as often.

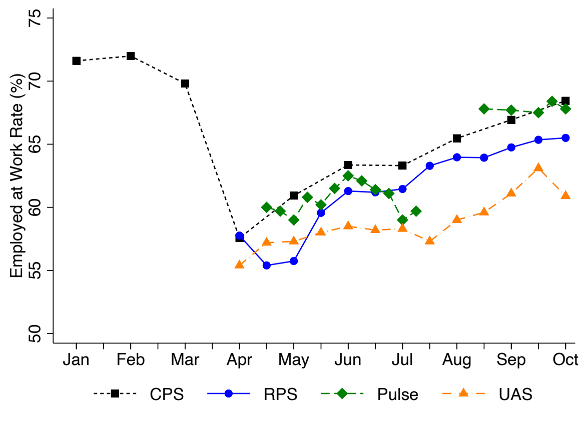
Figure 2 documents RPS and CPS labor market estimates for the US working age population during the first six months of the Covid-19 pandemic. The employed and at work rate, which is the subset of the employed population that excludes individuals who did not work during the reference week for any reason, is displayed in Figure 2a. This is the simplest measure of labor input available in the CPS, because it mostly relies on asking a single yes/no question: “Last week, did you do any work for pay or profit?”[6]

Both the CPS and RPS display a sharply lower employed and at work rate in April relative to the first few months of the year. The series then increases sharply through June, and then continues to recover more slowly from July-on. The level of the RPS tends to be below that of the CPS, with a larger gap in May. Because this measure of labor input mainly relies on a single question, several other online surveys ask it as well. Figure 2a also displays estimates from the Pulse Survey (Pulse), a large-scale weekly online survey run by the US Census Bureau, and the Understanding America Survey (UAS), based on a long run probability-based panel maintained by the Center for Economic and Social Research at the University of Southern California. The average percentage

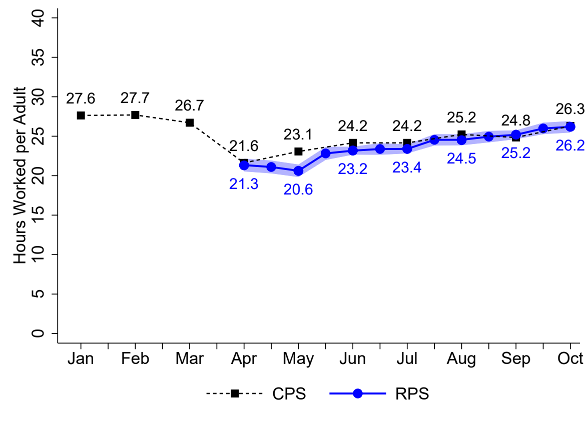
[6] As in the CPS, individuals with a business in the household who answer no to this question are asked whether they did any unpaid work in that business. Those answering yes are then asked again whether they received any payments or profits from the business. Anyone answering yes to this question is categorized as employed at work, as is anyone answering no to this last question but reporting having actually worked at least 15 hours last week.

Figure 2: Labor Market Outcomes in the RPS and CPS, Age 18-64

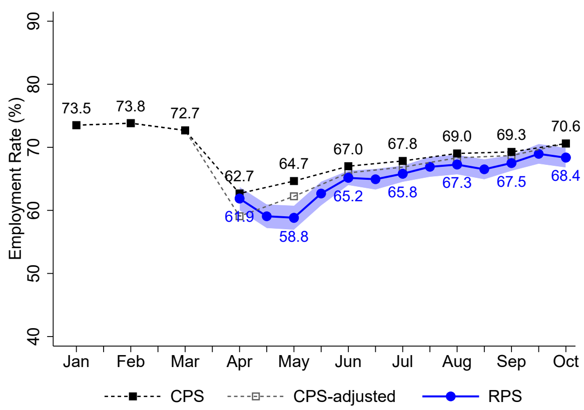
(a) Employed and at Work Rate



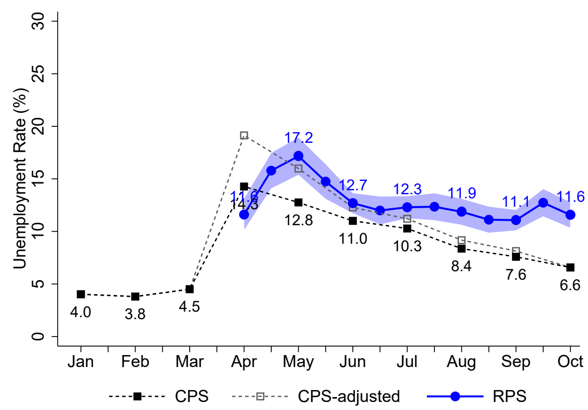
(b) Hours Worked per Working Age Adult



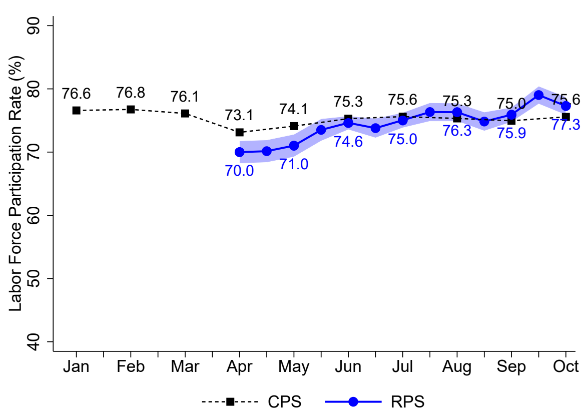
(c) Employment Rate



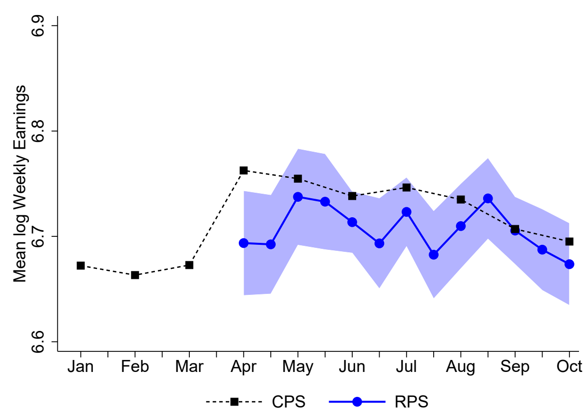
(d) Unemployment Rate



(e) Labor Force Participation Rate



(f) Mean log Weekly Earnings



Notes: Figure contains data from the RPS, 2020 CPS, and for the employed and at work rate from the Census Pulse Survey (which was not conducted between mid-July to mid-August) and the Understanding America Survey (UAS). The adjusted CPS data (in gray) for the employment and unemployment rates reflect BLS estimates that in the April CPS up to 7.5 million individuals who were misclassified as employed but absent, should have been classified as unemployed. For details, see the April FAQ report by the BLS. We quantify the impact of misclassification by constructing the adjusted employment and unemployment rates following the discussion in the FAQ report. Specifically, in April we subtract 7.5 million from the employed and add them to the unemployed. For the figures in the main text, which reflect ages 18-64, we reduce the adjustment (e.g. 7.5 million in the case of April) by 5% to reflect the fact that only 95% of the employed are younger than age 65. We conduct an analogous process for later months. Earnings are the mean of the log of nominal usual weekly earnings. Prior to August, earnings for job-stayers are constructed using information on pre-pandemic earnings and reported earnings changes. We discuss this in detail in Appendix A point (pp) deviation relative to the CPS in the sample period is 2.3pp, 1.7pp, and 5.1pp for the RPS, Pulse, and UAS, respectively. Based on this we conclude that the RPS provides useful estimates of the employed and at work rate relative to other high frequency surveys. Since Foote et al. (2020) cover a different age group, we do not include their time-series. We do conduct a comparison to their results in Appendix C.5 finding that the two surveys produce fairly similar estimates relative to the respective age-group-specific benchmark in February.

Figure 2b displays average hours worked per working age adult. This is also a relatively straightforward measure of labor input, in the sense that it only requires one additional question asked of the employed and at work: “How many hours did you actually work last week?” The RPS and CPS display quite similar levels and trends in average hours worked throughout the sample period, again with the exception of May.

The next three measures of labor market outcomes—employment, unemployment, and labor force participation—are more challenging to assess, as they require many more questions and a nontrivial skip/flow logic. The employment rate is displayed in Figure 2c. In addition to the official CPS rate (in black), the figure also displays an adjusted CPS rate (in gray) reflecting potential misclassification issues at the onset of the pandemic discussed by the BLS in a supplement to their monthly report; please see the figure note for details on how we construct this adjustment. The RPS consistently lies slightly below both the adjusted and unadjusted CPS series, with the exception of April where it lies between the two. However, similar to the employed and at work rate, since June the two series move together fairly closely.

The unemployment rates for the RPS and CPS (official and adjusted) are displayed in Figure 2d. Here the two surveys disagree on the precise timing of the peak in unemployment: the official (adjusted) CPS rate peaks at 14.8% (19.1%) in April, while the RPS rate peaks at 17.2% in May. The decline in unemployment from June-onward is also more rapid in the CPS than the RPS. Given the parallel paths for employment between the RPS and CPS over these months, this disagreement is driven by stronger growth in labor force participation in the RPS (see Figure 2e).

There are several potential reasons why an online survey like the RPS may report a somewhat higher unemployment rate than the CPS. First, employment is slightly lower in the RPS creating a larger pool for potentially unemployed individuals. Second, while both the RPS and CPS ask non-employed workers about their job search activities, respondents report these activities differently in each survey. In the CPS, respondents volunteer search activities orally, and these activities are manually categorized later. In the RPS, we provide these categories as a list which respondents select from. Third, Ahn and Hamilton (2020) point out that the unemployment rate among individuals in their final CPS interviews is often substantially lower than the corresponding rate for the first interview. This could reflect either issues of selection, if unemployed or non-participating individuals are less likely to continue with the CPS, or strategic reporting, if individuals learn over time that they can answer fewer questions if they report being employed. Either effect would bias the overall unemployment rate downwards, which should not be present in a one-shot survey like the RPS.

Finally, Figure 2f displays mean log weekly earnings. Despite large declines in employment and hours worked, in both datasets earnings *increased* at the onset of the pandemic. Cajner et al. (2020) show that this is entirely driven by selection: employment losses were greater among those with lower earnings prior to the pandemic. Earnings in the RPS and CPS are fairly similar from May through October, but display a larger difference in April.

Appendix B.2 shows that the RPS also lines up reasonably well with the prevalence of businesses in the household, pay type, sector, self-reported broad industry, and detailed industry. Overall, these results indicate that an online survey in the style of the RPS is capable of providing useful labor market forecasts well ahead of the CPS. Particularly during fast-moving crises such as the Covid-19 pandemic, this suggests that online surveys like the RPS represent a valuable addition to the toolkit of researchers and policymakers.

4 Application: Nominal Wage Cuts At the Onset of a Pandemic

It is well-known that aggregate nominal wages are relatively acylical compared with output or employment, e.g. Abraham and Haltiwanger (1995). To the extent that acyclical aggregate wages indicate that the marginal cost of labor is relatively unresponsive to business cycle conditions, this has important implications for many macroeconomic models; see e.g. Christiano et al. (2005) for a classic example, and Guerrieri et al. (2020) for an application to the Covid-19 recession. In response to these theoretical motivations, a large and active empirical literature tries to assess the extent to which this is the case; see Elsby and Solon (2019) for a review of the literature and Kurmann and McEntarfer (2018), Grigsby et al. (2020), and Gertler et al. (2020) for recent contributions.

This section uses the RPS to make several contributions to this literature within the context of the Covid-19 recession. The early months of the recession were unusual in several respects, which together make the Covid-19 recession an especially interesting episode. First, the speed of the economic disruption was without modern precedent; one possibility is that usual norms against cutting nominal wages may be less binding in the face of such a salient and widespread economic shock. Alternatively, government transfers to firms and the unemployed were larger than in recent recessions, which may have decreased the bargaining power of firms. Finally, a large majority of layoffs at the start of the recession were classified as temporary, suggesting that after a time many workers could be recalled by their employers.

Wage Cuts Among Job-Stayers and Job-Switchers The literature on wage rigidity draws a distinction between job-stayers and job-switchers. Conceptually, downward wage rigidity among job-stayers may induce larger layoffs following a negative economic shock because firms cannot sufficiently reduce their existing labor expenses. Separately, downward rigidity among new hires may exacerbate the peak and persistence of unemployment following a negative shock because potential hires will be inordinately expensive, e.g. Hall (2005). Because the sources, prevalence, and implications of downward wage rigidity may differ between job-stayers and new job-switchers, it is important to analyze each case separately.

Unfortunately, this is challenging to do in the CPS. Workers are asked about their usual earnings at two points in time one year apart, in their fourth and eighth interviews, but the CPS does not observe whether workers have changed employers between those months. Previous papers have imputed job-stayer status using industry and occupation data; see e.g. Daly et al. (2012). A clever partial workaround to this is Elsby et al. (2016), who use firm tenure information in the CPS's Displaced Worker Supplement. However, this is only administered once every two years; the next installment is scheduled for 2022, which will not be relevant for studying wage cuts during 2020.

We overcome these issues using information on earnings and firm tenure in the RPS. Our sample is individuals who worked both in February and the reference month. We define a wage cut as a reduction in wages of at least 10% to focus on clear cases of reductions in pay. For additional details on the measurement of earnings and wages, see Appendix A and C.2.

Figures 3a and 3b display the share of workers who experienced an hourly or weekly wage cut, respectively, from April through October 2020. To increase sample size we pool both surveys within each month. The initial scale of hourly wage cuts among job-stayers is notable: in April, 11.8% of job-stayers reported a cut relative to February. In May we find a similar estimate of 12.0%. This is close to a finding by Cajner et al. (2020) who report just under 12% of job-stayers experienced a reduction in base wages between March and May.[7] The prevalence of hourly wage

[7] Cajner et al. (2020) define base wages as a worker's per-period contract wage; for example, for hourly workers it is

cuts among job-stayers increased slightly over the next several months, reaching 14.3% in October. At the onset of the pandemic, cuts to weekly wages among job-stayers were even more common than to hourly wages, at 17.6%, because hours worked also fell for many workers. In subsequent months, the prevalence of weekly wage cuts among job-stayers declined monotonically because hours per employed worker largely recovered over this period.

Wage cut patterns look very different for job-switchers. In April, at the onset of the pandemic, 12.5% of job-switchers experienced an hourly wage cut, slightly above the rate for job-stayers. However, hourly wage cuts have become more common for job-switchers over time, reaching 21.4% of workers by October. And because job-switchers were twice as likely to experience a reduction in hours worked, the prevalence of weekly wage cuts among job-switchers increased even more sharply, growing from 9.2% in April to 26.2% in October.

In Appendix A, Tables A.2 and A.3 verify that these patterns persist after controlling for a host of observables, alternately using a linear probability model and a logit model. The probability of hourly wage cuts is essentially flat from April to October for job-stayers, but increased sharply over time for job-switchers. In October relative to April, weekly wage cuts were 8.0 percentage points less likely for job stayers, but 17.9 percentage points more likely for job-switchers, all else equal. These results also indicate that wage cuts were more likely among workers who were young, Hispanic, without a bachelors degree, had low household income in 2019, who had young children.

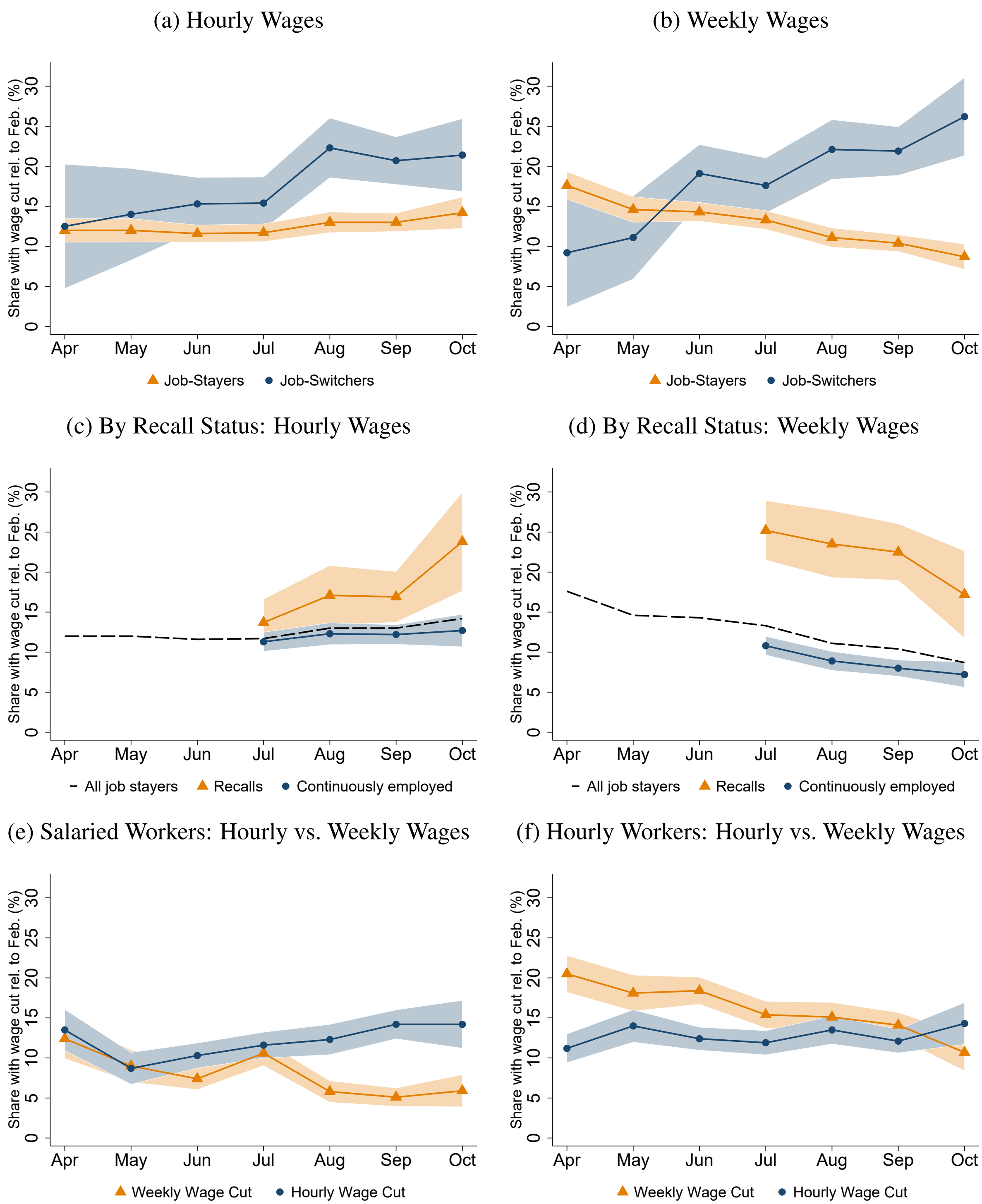
Our finding that wage cuts are more common for job-switchers than job-stayers contrasts with findings for earlier years by Gertler et al. (2020) and Grigsby et al. (2020). The former use SIPP data from 1990-2012, while the latter use administrative data from the payroll company ADP. Both find that, after controlling for worker characteristics, the wages of new hires are similarly cyclical to the wages of job-stayers. In contrast, Table A.2 shows that, in the current recession, job switchers were much more likely to experience wage cuts than job-stayers, and that this difference has steadily increased over time.

Wage Cuts Among Recalled Workers A salient feature of the labor market at the onset of the Covid-19 pandemic was that the vast majority of the unemployed were labeled *temporary layoffs*.[8] Existing work has shown that temporary layoffs are more likely to be recalled by their previous

their hourly wage rate. Their analysis focused only on firms that made 75% of their 2019 wage changes for employees in the months March-May.

[8] In the CPS, the share of unemployed who were on temporary layoff increased from 29% in February 2020 to 88% in April 2020. In contrast, the share of the unemployed on temporary layoff decreased during the Great Recession, from 13% in 2007 and 2008 to 11% in 2009.

Figure 3: Share Experiencing Nominal Wage Cut, Age 18-64



Note: Figure displays the share of workers in the RPS who experienced declines of (a) hourly or (b) weekly wages of at least 10% relative to February 2020. The sample is restricted to those employed both in February and in the current reference week. Job-switchers are those who report working for a different employer than in February. The shaded areas represent 95% confidence intervals.

employers than other unemployed workers; see e.g. Fujita and Moscarini (2017). An important open question, however, is whether workers will be recalled to their previous level of hours and wages. For example, firms may find it easier to cut the wages of workers who have been laid off, or only be able to recall laid off workers if wages are cut.

RPS waves from July-on contained a set of questions designed to identify recalled workers, which the CPS does not observe (see Appendix C.2 for details). Figure 3c displays the prevalence of hourly wage cuts among job-stayers in the RPS, from July-on distinguishing between recalls and workers who were continuously employed since February. In July, 12.8% of recalled workers experienced an hourly wage cut, compared to 11.0% of continuously employed workers. By October the share of recalled workers experiencing an hourly wage cut nearly doubled to 24.2%, compared to a small increase for continuously employed workers. The gap in wage cuts for recalled workers was even larger when considering cuts to weekly wages, indicating that workers are being recalled both to lower hourly wages and to lower hours.

Tables A.4 and A.5 confirm that this relationship remains significant after controlling for a host of other observables. These findings suggest that the combination of temporary layoffs and worker recall may allow firms to overcome downward nominal wage rigidities.

Wage Cuts Among Salaried Workers Administrative data represent an important new tool in the analysis of wage changes over the business cycle. In particular, such data allow researchers to observe worker compensation with minimal measurement error, e.g. Kurmann and McEntarfer (2018), Grigsby et al. (2020) and Cajner et al. (2020). However, one limitation is that administrative data typically report 40 hours per week for salaried workers, independent of how many hours these workers actually work. This prevents the analysis of hourly wages for this group, while the RPS allows us to speak to this issue. For this excercise we restrict the sample to job-stayers.

Figure 3e plots wage cuts for salaried workers, distinguishing between cuts to hourly wages and weekly wages. In April, 12.4% of salaried job-stayers had experienced a weekly wage cut; however, this share declined to 5.9% by October. In contrast, the prevalence of hourly wage cuts among salaried job-stayers increased slightly, from 13.1% in April to 14.2% in October. The trend in cuts to weekly wages and hourly wages differed because hours worked were also changing over this period. Only 7% of salaried job stayers worked more hours in April than in February, but by October the share had increased to 20%. That is, by October many salaried workers had weekly wages at least on par with just prior to the pandemic, but were working more hours to such an extent that their hourly wage had declined. This suggests that, among salaried workers, most cuts to weekly wages were temporary, while cuts to hourly wages persisted through October. To the extent that hourly wages are the most relevant indicator of the marginal cost of labor, these results highlight the importance of information on hours worked for salaried workers.

The patterns for hourly workers look very similar to those for salaried workers, with the exception that weekly wage cuts are about 5 percentage points more likely in all months. This reflects larger hours reductions among hourly workers.

Collectively, the findings throughout this section are informative for the calibration and validation of quantitative models with nominal downward wage rigidities, such as Baqaee and Farhi (2020) and Guerrieri et al. (2020). We also document notable heterogeneity in the propensity of wage cuts, which highlights the unequal impact of the Covid-19 pandemic beyond systematic differences in job losses; see also Adams-Prassl et al. (2020).

5 Concluding Remarks and Additional Applications

This paper evaluates data from the RPS, a national online labor market survey designed to complement the CPS. The central idea underlying the RPS is to use an existing high-quality baseline survey to establish confidence in the representativeness and accuracy of a novel survey that allows for more rapid data collection and specialized research questions. We believe this general approach can be useful in addressing a wide array of research questions beyond those in the current paper. We conclude by discussing a few promising examples.

Another application of RPS data is Bick et al. (2020). In that paper, we exploit RPS information on work from home at two points in time: just prior to the Covid-19 pandemic and in the previous week. This analysis provides key facts to help understand the nature of the labor market disruptions caused by the pandemic, and can serve as inputs into quantitative models of the economic crisis, e.g. Baqaee et al. (2020). Moreover, comparisons between actual work from home, as observed in the RPS, and potential home-based work, e.g. Dingel and Neiman (2020), are important for evaluations of virus containment policies and the long run potential of work from home.

There are many other potential applications of our survey concept beyond the questions contained in the RPS. One example is changes in labor market outcomes around migration. While the CPS-ASEC asks whether an individual has moved between states over the last several years, it does not ask about moves within a state, where the individual was born or spent childhood, and does not allow for comparisons of labor market outcomes pre- and post-migration (for example, the wage gains at migration). An RPS-style survey could ask these additional questions, oversampling movers if desired and then weight the sample to match moments in the CPS-ASEC.

Another potential application is related to the displaced worker supplement (DWS) of the CPS, which has been used widely to study the consequences of job losses; see e.g. Farber (2017). The DWS is only administered once every two years, with the next installment scheduled for 2022. It would be straightforward to implement the DWS as part of the RPS and also add important questions missing in the DWS. For example, from the perspective of search models of the labor market it would be crucial to know the starting date and starting earnings for the most recent job after a job loss (which is not asked in the DWS) in addition to current earnings (which is asked in the DWS).

Finally, RPS-style surveys could provide additional information relevant to individuals' family and educational backgrounds. For example, the CPS does not ask about the family structure during an individual's childhood, such as the education or marital status of parents. Further, the education supplement in the CPS does not ask about college major or student loans. Combining these variables with the standard set of labor market outcomes in the CPS could provide valuable contributions to the study of income inequality.

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Appendix (For Online Publication Only)

A Additional Results: Changes to Earnings and Wages

A.1 Measuring Earnings and Changes in Earnings since February

The RPS collects information on earnings from several questions. Respondents who were employed in February are asked about retrospective usual weekly earnings using the same format as the CPS Outgoing Rotation Group (ORG), see also Appendix C.2. Specifically, respondents are first asked *(A) "What is the easiest way to report your usual earnings for this job, before taxes or other deductions?"*. Next, respondents are asked *(B) "In February, how much did you usually earn per __ before taxes or other deductions? Please include overtime pay, tips, or commissions, if applicable."* In the latter question, the earnings period is based on the respondents' answer to the former question. Respondents who report annual earnings are also asked for how many weeks per year they are paid.

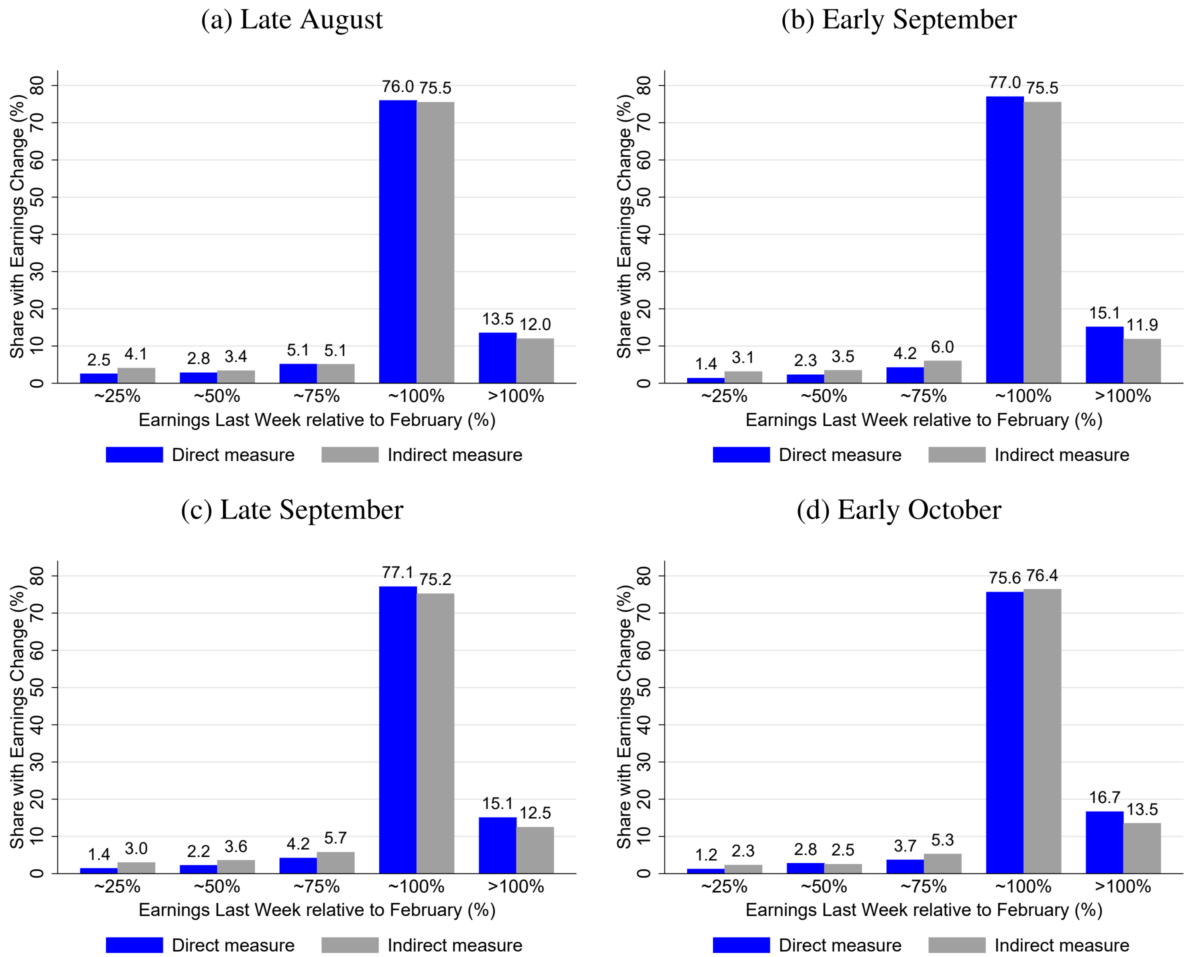
Respondents are then asked about earnings in the previous week. From RPS wave 11 (in August)-onward, if respondents were employed in February, they are first asked to answer the following: *(C) "We want to know how your recent earnings from this job compare to your usual earnings from this job before March. Recently, my usual earnings from this job are..."* with possible options:

1. *more than my usual earnings before March.*
2. *exactly the same as my usual earnings before March.*
3. *slightly less than my usual earnings before March.*
4. *about three quarters (3/4) of my usual earnings before March.*
5. *about half (1/2) of my usual earnings before March.*
6. *about one quarter (1/4) of my usual earnings before March.*

Respondents who select any answer option other than (2) are then asked analogues of questions *(A)* and *(B)* in the above paragraph, except replacing "In February" with "last week"; we will refer to these as questions *(D)* and *(E)*.[9] Prior to wave 11 in August, job-stayers and job-switchers relative

[9] Prior to wave 11, we did not distinguish between *exactly the same* and *slightly less*; instead, we combined these options into "*about the same* as in February."

Figure A.1: Robustness: Alternative Measures of Earnings Changes for Job Stayers, Age 18-64



Note: Figure displays the distribution of changes in weekly earnings relative to February among workers employed both in February and the reference week. The sample is restricted to job-stayers (relative to February), because only these workers are asked both sets of earnings questions. The "direct measure" uses information that directly asks workers how last week's earnings compare to usual weekly earnings in February, with the options of (i) about one quarter (25%), (ii) about half (50%), (iii) about three quarters (75%), (iv) about the same (100%), and (iv) more. The "indirect measure" computes the change in earnings manually by taking the ratio of reported earnings in February and last week. We assign these indirect measures of earnings changes to the $\approx 25\%$, $\approx 50\%$, $\approx 75\%$, $\approx 100\%$, and $> 100\%$ bins if $e'/e \in (0, .375)$, $[.375, .625)$, $[.625, .875)$, $[.875, 1.01), [1.01, \infty)$, respectively. The indirect measure is only available beginning in late August.

to February were asked about earnings in two different ways. Job-stayers were asked *(C)*, but not *(D)* and *(E)*. Job-switchers were asked *(D)* and *(E)*, but not *(C)*.

Since respondents are asked *(C)*, *(D)* and *(E)* in waves 11-on, we can assess the extent to which the two questions are consistent with each other by constructing separate distributions of earnings changes alternately using each question. The results of this exercise are displayed in Figure

A.1. In late August, using question *(C)*, 10.4% of respondents report a decline in weekly earnings, 76.0% report unchanged weekly earnings, and 13.5% report an increase in weekly earnings. Using question *(E)*, the corresponding numbers are 12.6%, 75.5%, and 12.0%. We find similarly modest differences in later waves. We conclude from this exercise that both measures of earnings produce quantitatively comparable estimates of earnings changes relative to February.

A.2 Imputing Earnings

Figure 2f computes the mean of log weekly earnings in the RPS from April through September. As we discuss above, prior to wave 11 in August we do not directly ask about current earnings for job-stayers. However, we do ask about earnings in February and earnings changes since February for job-stayers. We therefore impute current earnings for job-stayers prior to wave 11 in two steps. First, using waves 11-on we compute the mean percentage change in earnings from February to last week for each earnings change bin from question *(C)*. The mean change for each category is as follows. Among those who report in Question *(C)* that earnings increased since February, earnings increased by 4% on average. Among those who report that earnings were about the same, earnings fell by 4% on average. Among those who report that earnings were about 75% relative to February, earnings fell by 19% on average. Among those who report that earnings were about 50% relative to February, earnings fell by 39% on average. Among those who report that earnings were about 25% relative to February, earnings fell by 50% on average. In the second imputation step, we assign this earnings change to each job-stayer prior to wave 11 based on their answer to question *(C)*.

A.3 Measuring Hourly Wages

Section 4 documents the prevalence of cuts to both weekly earnings and hourly wages. Hourly wage cuts are defined as a decrease in hourly wages of at least 10% since February. Hourly wages in February are the ratio of usual weekly earnings and usual weekly hours in February. In the main text, hourly wages last week are the ratio of current weekly earnings to actual hours worked last week. Beginning in late August, in addition to asking about actual hours last week, we also asked about current usual hours. This allows for an alternative measure of hourly wages using usual hours rather than actual hours. Table A.1 compares the prevalence of hourly wage cuts from late August through October using actual or usual hours to construct hourly wages. Both measures deliver similar results, though wage cuts are slightly more common when using usual hours as opposed to actual hours.

Table A.1: Prevalence of Hourly Wage Cuts

	Earnings / actual hours	Earnings / usual hours
Job-switchers: All	21.6	22.5
Job-stayers: All	13.5	12.2
Job-stayers: Non-recall	12.5	11.6
Job-stayers: Recall	19.0	15.8
Job-stayers: Salary	13.9	13.1
Job-stayers: Hourly	13.1	11.6

A.4 Additional Results for Wage Cuts

Tables A.2, A.3, A.4, and A.5 display results from regressions of the probability of a wage cut on various controls. Column 2 of Table A.2 reveals that the propensity of weekly wage cuts decreased from April to September for job-stayers, but increased over the same time period for job-switchers. Column 4 of Table A.2 reveals that the propensity of hourly wage cuts was roughly constant from April to October for job-stayers, but increased over the same time period for job-switchers. Wage cuts were more common among Hispanics, those with less than a Bachelor's degree, and those with lower income in 2019. Column 2 of Table A.3 reveals similar patterns in a logit specification.

Table A.4 reveals that both weekly and hourly wage cuts were more common among workers who were temporarily laid off and then recalled, compared with job-stayers who were continuously employed. Table A.5 reveals similar patterns in a logit specification.

Table A.2: Predictors of a Wage Cut: Linear Probability Model

	Weekly Wages		Hourly Wages	
	(1)	(2)	(3)	(4)
Constant	0.169***	0.179***	0.118***	0.117***
	(0.007)	(0.009)	(0.007)	(0.009)
May X Job Stay		-0.026***		0.001
		(0.009)		(0.009)
Jun X Job Stay		-0.028***		-0.002
		(0.010)		(0.009)
Jul X Job Stay		-0.034***		0.001
		(0.010)		(0.009)
Aug X Job Stay		-0.055***		0.013
		(0.010)		(0.009)
Sep X Job Stay		-0.062***		0.013
		(0.010)		(0.009)
Oct X Job Stay		-0.080***		0.026***
		(0.010)		(0.010)
Apr X Job Switch		-0.115***		-0.005
		(0.034)		(0.034)
May X Job Switch		-0.096***		0.010
		(0.025)		(0.024)
Jun X Job Switch		-0.011		0.023
		(0.020)		(0.020)
Jul X Job Switch		-0.025		0.026
		(0.019)		(0.019)
Aug X Job Switch		0.019		0.094***
		(0.018)		(0.018)
Sep X Job Switch		0.020		0.080***
		(0.016)		(0.016)
Oct X Job Switch		0.064***		0.086***
		(0.016)		(0.016)
Female		0.005		0.008
		(0.005)		(0.005)
Black		0.012		0.001
		(0.008)		(0.008)
Hispanic		0.016**		-0.001
		(0.006)		(0.006)
Bachelors or More		-0.022***		0.000
		(0.005)		(0.005)
HH Income: $0-$50k		0.027***		0.000
		(0.007)		(0.006)
HH Income: $100k +		-0.031***		-0.010*
		(0.006)		(0.006)
Married		-0.011**		-0.005
		(0.005)		(0.005)
Child Under Age 13		0.022***		0.017***
		(0.006)		(0.006)
Age 18-29		0.033***		0.016**
		(0.007)		(0.006)
Age 50-64		-0.013**		-0.007
		(0.006)		(0.006)
Observations	20,392	20,392	20,392	20,392
R^2	0.009	0.029	0.005	0.007

Note: *p<0.1; **p<0.05; ***p<0.01

Table A.3: Predictors of a Wage Cut: Logit Probability Model

	Weekly Wages		Hourly Wages	
	(1)	(2)	(3)	(4)
Constant	0.169***	-1.573***	0.118***	-2.096***
	(0.007)	(0.086)	(0.007)	(0.094)
May X Job Stay		-0.073		0.024
		(0.091)		(0.102)
Jun X Job Stay		-0.152*		0.086
		(0.081)		(0.090)
Jul X Job Stay		-0.194**		0.107
		(0.082)		(0.090)
Aug X Job Stay		-0.432***		0.233**
		(0.090)		(0.093)
Sep X Job Stay		-0.507***		0.175*
		(0.086)		(0.089)
Oct X Job Stay		-0.593***		0.260**
		(0.118)		(0.110)
Apr X Job Switch		-0.979**		-0.108
		(0.434)		(0.384)
May X Job Switch		-0.719**		0.073
		(0.282)		(0.258)
Jun X Job Switch		-0.054		0.350**
		(0.138)		(0.145)
Jul X Job Switch		-0.108		0.199
		(0.138)		(0.149)
Aug X Job Switch		0.161		0.769***
		(0.129)		(0.131)
Sep X Job Switch		0.248**		0.670***
		(0.111)		(0.118)
Oct X Job Switch		0.391***		0.716***
		(0.146)		(0.154)
Female		0.088**		0.007
		(0.042)		(0.042)
Black		0.121*		0.039
		(0.064)		(0.067)
Hispanic		0.220***		0.042
		(0.054)		(0.056)
Bachelors or More		-0.234***		-0.012
		(0.048)		(0.047)
HH Income: $0-$50k		0.125**		0.058
		(0.054)		(0.057)
HH Income: $100k +		-0.326***		-0.079
		(0.054)		(0.053)
Married		-0.102**		-0.036
		(0.049)		(0.049)
Child Under Age 13		0.205***		0.173***
		(0.050)		(0.050)
Age 18-29		-0.033		-0.092
		(0.097)		(0.098)
Age50-64		-0.156		-0.168
		(0.110)		(0.106)
Age		-0.006		-0.005
		(0.005)		(0.004)
Age Sq. / 100		0.041*		0.063***
		(0.023)		(0.022)
Observations	20,392	20,392	20,392	20,392
R^2	0.009		0.005	
Note:				*p<0.1; **p<0.05; ***p<0.01

Table A.4: Predictors of a Wage Cut: Linear Probability Model

	Weekly Wages		Hourly Wages	
	(1)	(2)	(3)	(4)
Constant	0.110***	0.134***	0.108***	0.099***
	(0.006)	(0.010)	(0.007)	(0.011)
Recalled	0.135***	0.114***	0.055***	0.051***
	(0.008)	(0.008)	(0.009)	(0.009)
Aug	-0.019**	-0.020**	0.014	0.013
	(0.008)	(0.008)	(0.009)	(0.009)
Sep	-0.028***	-0.029***	0.013	0.013
	(0.008)	(0.008)	(0.009)	(0.009)
Oct	-0.042***	-0.044***	0.027***	0.027***
	(0.009)	(0.009)	(0.009)	(0.009)
Female		0.010*		-0.001
		(0.006)		(0.007)
Black		-0.004		-0.010
		(0.011)		(0.012)
Hispanic		0.005		-0.012
		(0.008)		(0.009)
Bachelors or More		-0.023***		0.005
		(0.007)		(0.008)
HH Income: $0-$50k		0.015*		0.014
		(0.009)		(0.009)
HH Income: $100k +		-0.031***		0.010
		(0.008)		(0.008)
Married		-0.015**		-0.005
		(0.007)		(0.008)
Child Under Age 13		0.026***		0.023**
		(0.008)		(0.009)
Age 18-29		0.011		0.015
		(0.009)		(0.010)
Age 50-64		-0.011		-0.004
		(0.007)		(0.008)
Observations	10,467	10,467	10,467	10,467
R^2	0.028	0.041	0.004	0.006

Note: *p<0.1; **p<0.05; ***p<0.01

Table A.5: Predictors of a Wage Cut: Logit Model

	Weekly Wages		Hourly Wages	
	(1)	(2)	(3)	(4)
Constant	-1.613***	-1.535***	-1.914***	-1.956***
	(0.087)	(0.109)	(0.097)	(0.117)
Recalled	1.201***	0.977***	0.386***	0.282***
	(0.069)	(0.071)	(0.073)	(0.075)
May	-0.162	-0.108	-0.125	-0.116
	(0.108)	(0.110)	(0.120)	(0.121)
Jun	-0.241**	-0.188*	-0.073	-0.056
	(0.100)	(0.101)	(0.110)	(0.110)
Jul	-0.563***	-0.462***	-0.125	-0.086
	(0.104)	(0.105)	(0.111)	(0.112)
Aug	-0.789***	-0.697***	0.003	0.042
	(0.110)	(0.111)	(0.113)	(0.113)
Sep	-0.872***	-0.786***	-0.058	-0.021
	(0.107)	(0.108)	(0.111)	(0.111)
Oct	-0.926***	-0.855***	0.042	0.072
	(0.134)	(0.135)	(0.128)	(0.128)
Female		0.111**		-0.007
		(0.048)		(0.047)
Black		0.090		0.070
		(0.078)		(0.080)
Hispanic		0.213***		0.024
		(0.063)		(0.065)
Bachelors or More		-0.258***		-0.027
		(0.055)		(0.052)
HH Income: \$0-\$50k		0.115*		0.091
		(0.063)		(0.067)
HH Income: \$100k +		-0.349***		-0.056
		(0.061)		(0.059)
Married		-0.080		-0.019
		(0.056)		(0.055)
Child Under Age 13		0.222***		0.183***
		(0.058)		(0.058)
Age 18-29		0.201***		0.164**
		(0.064)		(0.065)
Age50-64		-0.146**		-0.093
		(0.063)		(0.059)
Observations	16,828	16,828	16,828	16,828
R^2				

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

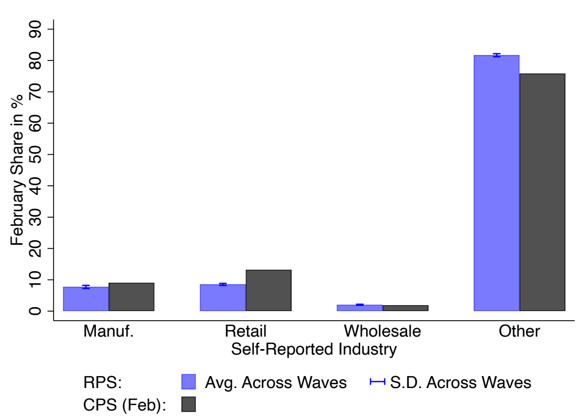
B Additional Validation Results

In the following we present additional validation results, both for February and for "last week", i.e. the reference week in the RPS and CPS, respectively.

B.1 Additional Validation for February

This section presents an additional validation result for February. While the detailed industry of employment in the CPS is not self-reported, the CPS asks about the categories displayed in Figure B.1. Similar to the not-self-reported sector in the CPS, the RPS and CPS closely line up for this measure.

Figure B.1: Self-Reported Broad Industry in February



Notes: *Self-reported Industry* 1, Manufacturing, Retail Trade, 3, Wholesale Trade, 4, Missing. In the RPS this information for February is only available from wave 5 (May)-onward.

B.2 Additional Validation for Reference Week

This sections presents additional validation results for "last week", i.e. the reference week in the RPS and CPS, respectively. Figure B.2a shows that in the RPS there are only slighlty more respondents living in a household with somebody owning a business than in the RPS. For all other variables in Figure B.2, the RPS-CPS comparison for the reference week is very similar to February. This is also true for the more detailed industry composition, see Figure B.3.

Figure B.2: Validation for the Reference Week, Age 18-64

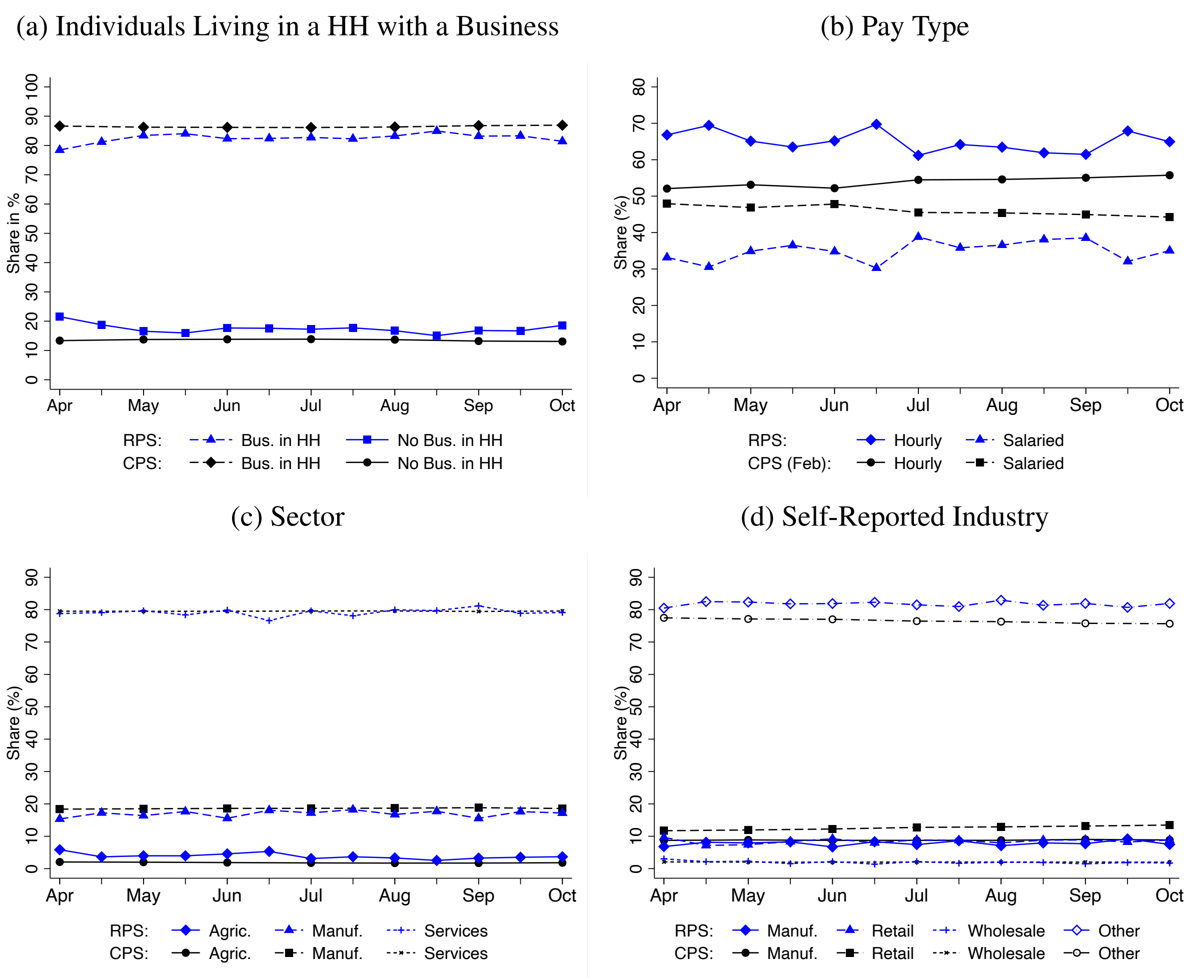
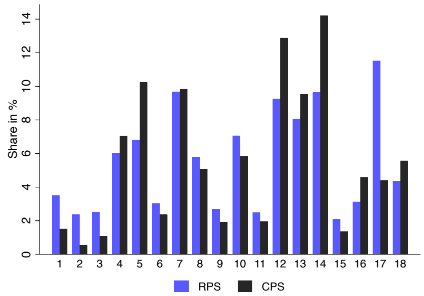
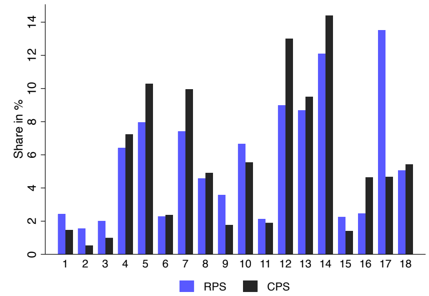


Figure B.3: Industry Composition

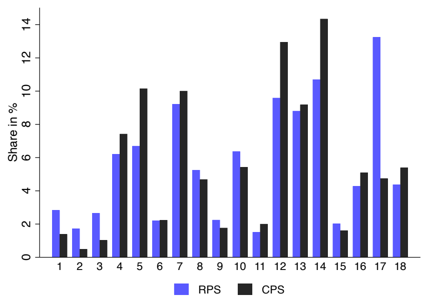
(a) April



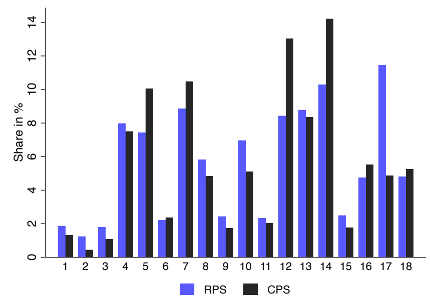
(b) May



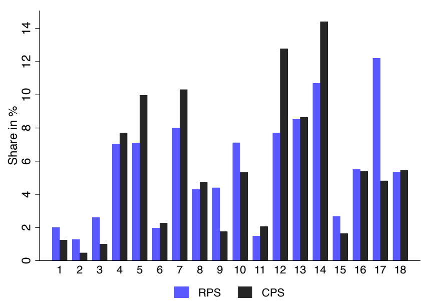
(c) June



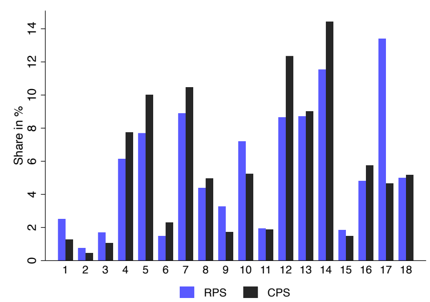
(d) July



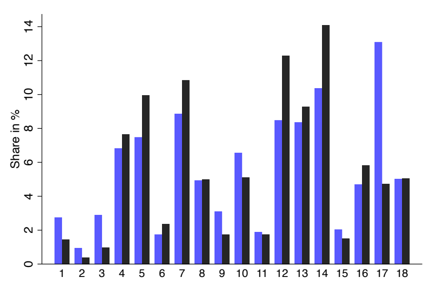
(e) August



(f) September



(g) October



Notes: Figure contains data from retrospective questions in the RPS, alongside data from the February 2020 CPS. Industries are as follows: 1, Agriculture, forestry, fishing, and hunting, 2, Mining, 3, Utilities, 4, Construction, 5, Manufacturing, 6, Wholesale trade, 7, Retail trade, 8, Transportation and warehousing, 9, Information services, 10, Financial services, 11, Real estate, 12, Professional and business services, 13, Educational services, 14, Health services, 15, Arts, Entertainment, and Recreation, 16, Hotel, Accommodation, and Food Services, 17, Other services, 18, Government (including Armed Forces).

C Details on Samples, Variable Construction, and Weighting

All data labeled "RPS" in this paper were collected by an online survey designed by the authors, which we refer to as the Real Time Population Survey (RPS). The survey was constructed using Qualtrics software and administered by Qualtrics.

C.1 Sample Collection and Selection

To be eligible to participate in the study, participants had to reside in the US, be at least age 18 or older, and speak English. In wave 1 and from wave 6 onwards, we imposed an upper age limit of 64. In waves 2 through 5 we also collected data on the age group 65 and older. The results in this paper only pertain to the age group 18-64 and only uses data from wave 2 onwards. We exclude wave 1 (covering the week of March 29 through April 4) from our analysis here because we neither consistently collect information on spouses and partners living in the household, nor on employment in February.

Table C.1 provides an overview for each survey. The first column reports the reference weeks, where weeks including 12th coincide with the CPS reference week in a given month. We started fielding the survey on the Monday after the reference week ended. The full data collection was completed within two to three days. The only exception is wave 6. We ran two parallel surveys in wave 6, one started fielding on Sunday morning, the other started fielding on Monday morning. Fielding was completed for both surveys on Wednesday. We wanted to investigate whether there were systematic differences if fielding started on a weekend day rather than a weekday. For example, it could have been that respondents who are employed are more likely to participate in the survey on a weekend day than on a weekday. However, the two surveys yielded very similar outcomes with regard to all labor market variables.

In general, we aimed for 2,000 final final responses. In waves 2-5 we included the age group 65+. Since we focus on the age group 18-64, the final number of respondents reported here is only about 80% of the originally collected sample. Wave 6 has about twice the size because of the two parallel surveys described above, where each had a sample size of about 2,000 respondents. In waves 8 and 12 we also ran two surveys in parallel to experiment with differences in the survey questionnaire. We will discuss this in detail in Appendix C.3. Each parallel survey aimed for 1,600 final responses. Columns 4 through 6 report statistics on how long it took respondents to take the survey. The median survey completion time was about 5 minutes in wave 2 and 6 minutes and 46 seconds in wave 14. This increase results from a) an increase in employment (employed individ-

Table C.1: Overview for Each Survey Wave (All in 2020)

	Dates			Survey Duration in Seconds		
Wave	Reference Week	Survey Fielding	Respondents	25th Perc.	Median	75th Perc.
2	04/12-04/18	04/22-04/23	1541	210	299	438
3	04/26-05/02	05/04-05/06	1600	210	311	466.5
4	05/10-05/16	05/18-05/19	1558	219	307.5	433
5	05/24-05/30	06/01-06/02	1513	237	335	486
6	06/07-06/13	06/14-06/17	3927	251	357	511
7	06/21-06/27	06/29-07/01	1964	246	344.5	503
8	07/12-07/18	07/20-07/22	3117	269	385	552
9	07/26-08/01	08/03-08/04	2016	267.5	376	535
10	08/09-08/15	08/17-08/18	2110	270	381	548
11	08/23-08/29	08/31-09/01	2048	267	385	562
12	09/06-09/12	09/14-09/15	3241	273	398	598
13	09/23-10/03	10/05-10/07	2024	268	398	576
14	10/11-10/17	10/19-10/20	2045	284	406	604

uals are asked more questions) and b) the addition of new questions. With a few exceptions these questions were added at the end of the survey in order to not alter the flow of the core questionnaire and thus maintain comparability across survey waves.

Qualtrics uses a quota-based sampling approach. At the beginning of the survey respondents are asked a host of demographic questions. For some of these characteristics (sex, education, race and ethnicity, marital status, residing with children, geographic region, and household income in 2019), we provide Qualtrics the corresponding shares from the CPS from January through March 2020, for example the fraction of men and women, or the fraction of those with less high school education or less, some college or an associate degree, bachelor degree or more. Once sufficiently many women have filled out the survey (e.g., in a survey with 2,000 final respondents about 1,000 women), new respondents reporting being female are not able to continue with the survey. The quotas are determined only for one group and not cross-combinations, e.g. only by sex and education separately, but not by the joint distribution of sex and education. Table C.2 shows the breakdown for the different background characteristics we provided to Qualtrics. The first column reports the corresponding fractions in the CPS from January through March 2020, and the subsequent columns the composition for each survey wave. The distribution of household income is taken from the 2020 Release of the CPS ASEC (also often referred to as the March supplement), which reports total household income for the year 2019. Prior to wave 13, the 2020 CPS ASEC was not yet available. We therefore provided Qualtrics with the same distribution from 2019 Release

Table C.2: Targeted Statistics: CPS vs. RPS

		RPS Wave												
	CPS	2	3	4	5	6	7	8	9	10	11	12	13	14
Gender														
Men	49.2	45.9	48.0	47.4	46.3	46.8	47.0	45.9	48.1	47.9	49.1	49.0	48.4	49.0
Women	50.8	54.1	52.0	52.6	53.7	53.2	53.0	54.1	51.9	52.1	50.9	51.0	51.6	51.0
Age														
18-24	14.6	15.2	14.7	14.6	15.6	15.2	15.1	15.4	15.7	16.4	15.3	14.6	14.9	15.1
25-34	22.7	22.2	21.4	19.8	21.6	22.8	23.2	24.1	23.6	23.2	24.5	23.3	24.0	25.1
35-44	20.9	22.4	22.5	23.9	21.3	19.8	21.8	22.1	20.2	21.3	19.4	22.6	23.1	25.9
45-54	20.3	16.5	18.5	18.2	17.3	19.9	17.5	15.9	18.0	17.4	15.6	17.6	16.8	16.1
55-64	21.4	23.6	23.0	23.6	24.2	22.3	22.4	22.5	22.5	21.8	25.2	22.0	21.2	17.8
Race/Ethnicity														
Non-hispanic White	59.2	56.9	58.1	57.1	57.9	59.2	57.7	57.1	60.1	60.3	58.2	59.4	57.7	59.7
Non-hispanic Black	12.7	13.7	12.9	13.7	15.1	13.1	13.2	13.4	12.4	12.7	13.5	12.6	12.9	13.2
Hispanic	18.8	20.3	20.9	19.8	16.5	18.3	19.6	19.5	18.0	17.8	17.8	18.4	19.3	17.4
Other	9.3	9.1	8.1	9.4	10.5	9.4	9.5	10.0	9.5	9.2	10.5	9.6	10.0	9.7
Education														
Highschool or less	36.8	37.1	37.7	37.3	31.0	38.0	38.2	38.9	36.9	37.2	36.5	36.7	37.0	36.4
Some college/Associate's degree	28.4	26.6	27.6	27.6	29.3	25.3	25.4	24.2	27.2	27.0	28.2	25.7	27.4	28.2
Bachelor's or Graduate degree	34.9	36.3	34.7	35.0	39.7	36.7	36.4	37.0	35.8	35.7	35.3	37.6	35.6	35.4
Marital Status														
Married	51.3	50.7	49.7	48.7	51.0	51.0	51.4	52.8	52.3	52.2	51.0	53.7	52.0	55.6
Non-Married	48.7	49.3	50.3	51.3	49.0	49.0	48.6	47.2	47.7	47.8	49.0	46.3	48.0	44.4
Number of children														
0	68.2	66.8	66.2	67.1	67.2	66.8	66.8	66.4	67.1	66.2	68.2	66.3	64.8	60.6
1	13.4	13.8	14.2	13.5	14.0	13.9	14.0	14.2	14.0	14.8	13.3	14.0	15.0	16.3
2	12.0	13.1	13.0	12.7	12.5	12.7	12.5	12.6	12.5	12.4	12.1	13.1	13.5	15.8
3+	6.4	6.3	6.6	6.6	6.3	6.6	6.7	6.8	6.5	6.7	6.4	6.6	6.7	7.3
Region														
Midwest	20.7	18.7	19.3	20.3	19.7	19.7	21.4	20.6	20.2	20.2	17.7	17.7	22.5	20.0
Northeast	17.2	18.4	20.5	19.8	17.4	19.0	18.3	18.6	18.9	19.1	21.2	21.6	19.1	20.6
South	38.0	38.3	39.1	38.1	39.8	38.3	37.7	39.1	39.5	37.7	40.9	40.7	38.4	42.6
West	24.1	24.6	21.0	21.7	23.1	23.0	22.6	21.6	21.4	23.0	20.2	19.9	20.0	16.9
Household Income Last Year														
$0-$50,000	31.7	36.8	38.7	36.3	36.1	36.6	37.8	36.7	34.9	35.2	34.8	34.5	35.3	34.8
$50,000-$100,000	28.7	29.9	33.5	29.6	31.2	30.4	31.7	27.3	28.4	25.6	29.8	29.3	30.2	29.8
$100,000+	39.6	33.3	27.8	34.1	32.7	33.0	30.5	36.0	36.7	39.2	35.4	36.2	34.6	35.4

Note: The statistics from the CPS are for the Basic Interviews from January through March 2020. Household Income is taken from 2020 Release of the CPS ASEC (also often referred to as the March supplement), which reports total household income for the year 2019.

of the CPS ASEC, which reports total household income for the year 2018. Before constructing the earning bins we increased household earnings by 3.5% to account for nominal earnings growth between 2018 and 2019. The corresponding distribution was then 33.8% reporting $0-$50,000 per year, 29% reporting $50,000-$100,000 per year, and the remaining 37.2% reporting more than $100,000 per year.

C.2 Variable Construction

This section details how we use responses to our survey to construct a set of key variables: employment status, layoff status, actual hours worked last week, and usual weekly hours worked. Our procedure for variable construction closely follows the instructions in CPS Interviewing Manual (US Census Bureau, 2015), using the same word-for-word phrasing when practical, as well as the special guidelines provided in the FAQs by the BLS about the impact of the Coronoavirus on the employment situation.[10]

Employment Status. Employment status has four possible values, all referring to last week: (1) employed and at work, (2) employed and absent, (3) unemployed, (4) not in the labor force.

1. Employed - at work:
   - Respondents who reported that last week they worked for pay or profit.
   - Respondents who worked worked at least 15 unpaid hours at a business owned by someone in their household.
2. Employed - absent:
   - Respondents who reported that last week they (i) had a job from which they were temporarily absent (excluding unpaid work), and (ii) were not on temporary or indefinite layoff from a job.
3. Unemployed - on layoff:
   - Respondents who reported that last week they (i) had a job (excluding unpaid work) and did not work positive hours for pay at that job OR did not have a job, (ii) were on *temporary* layoff (or furlough/unpaid leave) from a job, and (iii) could have returned to work last week if they had been recalled. Individuals satisfying these criteria do not have to look actively for work in the last four weeks.
4. Unemployed - looking:
   - Respondents who reported that last week they (i) did not have a job, (ii) were neither on *temporary* nor *indefinite* layoff (or furlough/unpaid leave), (iii) had actively looked for work in the last four weeks, and (iv) would be available to work if they had been offered a job last week.

[10]The CPS Interviewing Manual is available on the BLS website, as are the FAQs for March, April, May, June, July, August, September, and October .

- Respondents who reported that last week they (i) had a job (excluding unpaid work) and did not work positive hours for pay at that job OR did not have a job, (ii) were on *temporary* layoff (or furlough/unpaid leave) from a job, and could have *not* returned to work last week if they had been recalled OR were on *indefinite* layoff (or furlough/unpaid leave), (iii) had actively looked for work in the last four weeks, and (iv) would be available to work if they had been offered a job last week.

5. Not in the labor force

   - Respondents who reported that last week they (i) did not have a job OR were on *temporary* layoff (or furlough/unpaid leave) and could and and could have *not* returned to work last week if they had been recalled OR were on on *indefinite* layoff (or furlough/unpaid leave), and (ii) were either not actively looking for work in the previous four weeks, and/or would not be available to work if they had been offered a job last week.
   - Respondents who worked at a household business and reported that last week they (i) did not work positive hours for pay, and (ii) worked less than 15 unpaid hours.

Layoff Status. Among those on layoff (or furlough/unpaid leave), we distinguish between temporary layoffs (who either have a date to return to work, or who have been given an indication they will be recalled within the next six months) and permanent layoffs (who have not been given an indication they will be recalled within the next six months).

Recall Status. Starting with wave 8 (July), we asked whether those with a job last work experienced a "work interruption" since they started the job or since March 2020 (whichever is more recent), defined as a period lasting at least one full week where they did not work at that job. We classify anyone as a recall who a "work interruption" because of one of the following: (i) a temporary layoff or furlough, (ii) slow work or business conditions, or (iii) other disruptions related to the pandemic.

Actively Looking for Work. The CPS asks individuals to report all of their job search activities, and then categorizes them. These categories can be summarized as follows: (1) contacted employer(s), employment center(s), or friends/relatives about a job; (2) sent out resumes/filled out applications; (3) checked union/professional recommendations; (4) placed or answered ads; (5) looked at ads; (6) attended job training program/courses; (7) nothing. In our survey, we asked respondents to select all activities that apply to them from this list. Anyone who selected at least one of the options (1) through (5) is then classified as *actively* looking for work.

Usual weekly hours worked prior to March. All respondents were asked about their labor market status and hours worked prior to March of this year (2020). This was done in two slightly different ways depending on whether the respondent worked at a job last week that they had started prior to March.

- For respondents who had a job last week, and who reported that they worked at that job in February, we asked "before March, how many hours per week did you usually work at this job?"
- For respondents who either did not have a job last week, or who reported that they were not working at this job in February, we first asked them "In February, which of the following best describes your work experience? (i) work for pay or profit, (ii) unpaid work at a business owned by someone in my household, or (iii) not working." If they selected either (i) or (ii), we asked them "Before March, how many hours per week did you usually work at your job from February?"

Usual weekly hours worked. All respondents who were classified as employed and at work last week were asked and who did not work at this job in February were asked "How many hours did you actually work at your main job?". Beginning in August, we also directly ask those who already had this job in February about their usual hours.

Actual hours worked last week. All respondents who were classified as employed and at work last week were asked "last week, how many hours did you actually work at your job?" Respondents who reported working multiple jobs last week were asked both "last week, how many hours did you actually work at your main job?" and "last week, how many hours did you actually work at your all other jobs?", where main job was defined as the job in which they usually worked the most hours.

Earnings. For individuals who had a job last week, and who began that job prior to March 2020, we asked them to report their usual earnings at that job, where usual was specified to mean prior to March 2020. We followed the CPS in by first asking (i) which period was easiest for respondents to report their usual earnings before taxes or deductions (hourly, daily, weekly, every other week, monthly, or yearly), then asking (ii) how much they usually earned per period. Respondents who report the earnings yearly are also asked how many weeks per year they are are paid for.

We also ask workers about their current usual earnings. For workers who did not work started their job after February, we directly ask about their usual earnings in their new job, and record a wage cut if wages decline by at least 10%. For job-stayers relative to February, we ask how their current usual weekly earnings compare to their usual weekly earnings from February—(i) about 25%, (ii) about 50%, (iii) about 75%, (iv) about the same, (iv) more. Beginning with wave 12 (August), we also directly ask job-stayers about their current usual earnings.

February Employment Status. Since the BLS's full sequence of questions for last week's labor market status is too time consuming to ask for February, we construct an indicator for being employed or not employed in February as follows. In this context, when we refer to someone being on layoff from a job last week it is irrelevant whether that individual expects to be recalled within the next six months or not.

1. Employed in February:
   - Last week was employed
   - On layoff from a job which began in February 2020 or earlier (e.g. 2019), and last time worked at the job in February 2020 or later (e.g. in March 2020)
   - Last week was neither employed nor on layoff, or was employed or on layoff from a job that began after February 2020. Then, for the question "In February of this year (2020), which of the following best describes your [spouses's/partner's] work experience?", answered either of the following:
     (a) "worked for pay or profit"
     (b) "self-employed" or "worked in a business owned by someone else in my household" and reporting to have worked usually at least 15 hours in February.
2. Not Employed in February:
   - On layoff from a job which began in February 2020 or earlier (e.g. 2019), and last time worked at the job was before February 2020 (e.g. in January 2020)
   - Last week was neither employed nor on layoff, or was employed or on layoff from a job that began after February 2020. Then, for the question "In February of this year (2020), which of the following best describes your [spouses's/partner's] work experience?", answered either of the following:
     (a) "not working"
     (b) "self-employed" or "worked in a business owned by someone else in my household" and reporting to have worked usually less than 15 hours in February.

C.3 Modifications Across Survey Waves

In waves 4, 8, and 12 we modified our survey questionnaire in an attempt to get as close as possible to the methodology used by the CPS. In waves 8 and 12, we ran parallel surveys, one with the initial questionnaire and one with the new questionnaire. We use comparisons between these parallel surveys to adjust the weighting scheme for earlier waves to maintain consistency across waves. In Bick and Blandin (2020) we publish the results of each new RPS release and gather all previous reports on the project website. While it would have been ideal to start out without any need for modifications, we view these improvements as a natural process of running a novel survey.

C.3.1 Wave 4 Update

Beginning in wave 4, we followed guidelines in BLS FAQ's related to the impact of the Coronavirus on the employment situation for March and April. Respondents who report having a job but who were absent last week, were given the option to choose "Because of other disruptions related to the Coronavirus Pandemic" as a reason for being absent. Anyone choosing this option was asked the same questions as respondents who said they were laid off from a job. As in the CPS, anyone labeled "laid off" who has not been given a date by their employer to return is asked whether they have been given an indication to be recalled to work within the next six months. In addition to "Yes" and "No", we added the answer option "Unsure because of disruptions related to the Coronavirus Pandemic". Following the BLS guidelines, anyone choosing this option is treated as if they said "Yes". Since we did not run a parallel survey that wave with the initial and new answer options we cannot not make any adjustments to waves 2 and 3.

C.3.2 Wave 8 Update

Starting with wave 8, we changed the order and phrasing of the first questions of our core labor module to better align our survey with the CPS. Initially, the survey asked individuals whether they had a job last week, including any jobs from which they were temporarily absent. Anyone answering yes to this question was asked whether they worked any hours at this job last week. Those not working any hours were asked about the reasons for why they did not work last week. Based on this (in addition to questions about unpaid work in a business owned by someone in the household), we defined employment as follows:

1. Employed - at work:
    - Respondents who reported that last week they (i) had a job (excluding unpaid work), and (ii) worked positive hours for pay at that job.

- Respondents who worked worked at least 15 unpaid hours at a business owned by someone in their household.

2. Employed - absent:

    - Respondents who reported that last week they (i) had a job (excluding unpaid work), (ii) did not work positive hours for pay at that job for any reason, and (iii) were not on temporary or indefinite layoff from a job.

Starting with wave 8, we instead first asked individuals whether they did any work for pay or profit in the previous week using the exact phrasing from the CPS. Respondents who answered no to this question were then asked whether they had a job last week from which they were temporarily absent. Those answering yes to the absent question were asked about the reasons why they were absent. Based on this (in addition to questions about unpaid work in a business owned by someone in the household), we define employment as follows (see also Appendix C.2):

1. Employed - at work:

    - Respondents who reported that last week they worked for pay or profit
    - Respondents who worked worked at least 15 unpaid hours at a business owned by someone in their household.

2. Employed - absent:

    - Respondents who reported that last week they (i) had a job from which they were temporary absent (excluding unpaid work), and (ii) were not on temporary or indefinite layoff from a job.

Based on the comparison with the parallel survey under the old survey methodology, the results from the previous RPS surveys were adjusted for consistency. In particular, we construct this adjustment factor such that the distribution of employment (0 for not working, 1 for being employed independent of whether the respondent was at work last week or absent from work) and joint distribution of employment and other demographic characteristics matches the same distribution between the two parallel surveys.[11] The adjustment factor is then applied to the wave 8 survey with the old methodology and all prior surveys.

[11] These demographics are sex, age (below 40, 40 or older), race (non-Hispanic White, other), education (less than an associate's degree [including some college], associate's degree or more), broad marital status (married + spouse present, other)

Figure C.1: The Effect of Wave 8 Adjustment

(a) Employment Rate (b) Unemployment Rate

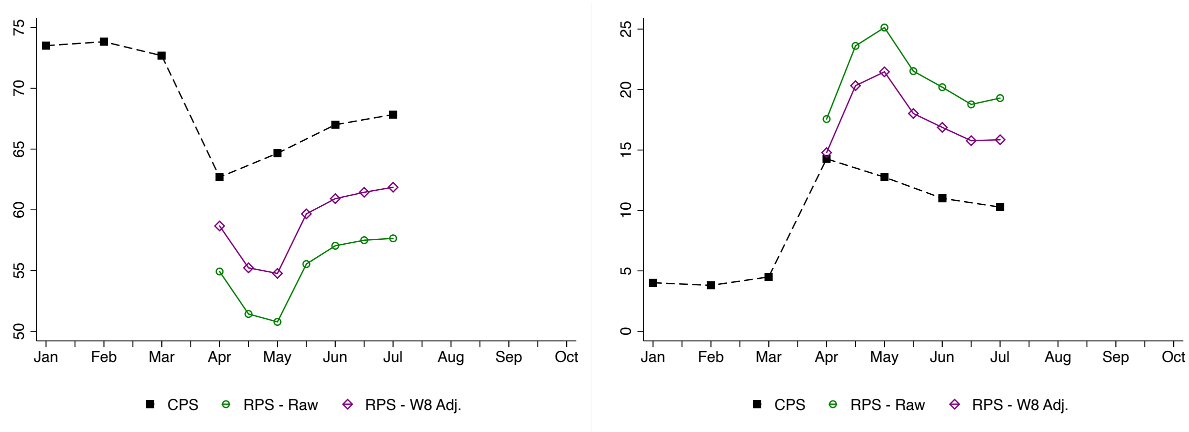


Figure C.1 shows the effect of changing the order of questions and the adjustment for the employment rate and unemployment rate, respectively. The line labeled as "RPS - Raw Data" uses the actual raw data, where those respondents with a spouse/partner living in the household and the spouse/partner receive a weight of 0.5 and everyone else a weight of 1. The reason for this weighting is that the initial survey targets already resemble correctly the share of those with and without a spouse present. The line labeled as "RPS - W8 Adj." shows for wave 8 the corresponding rates from the survey with the new ordering and phrasing of questions and yielded a higher employment/lower unemployment rate than the initial questionnaire. Accordingly, applying the adjustment discussed above in waves prior to July also increased employment/lowered unemployment.

C.3.3 Wave 12 Update

We made two additional modifications to the RPS questionnaire in wave 12. The first modification centered around a question related to employed but absent respondents. Prior to wave 12, respondents were asked "Last week, did you have a job from which you were temporarily absent?" Starting with wave 12, we used an alternative phrasing, matching the CPS: "Last week, did you have a job, either full- or part-time? Include any job from which you were temporarily absent".

Individuals who answered yes to this question are then asked a follow-up question to determine whether they are employed/absent or not employed. The second modification was a change in the answer options to this question.

1. "Vacation/personal days", "Own illness/injury/medical problems", and "Quarantine or self-isolation due to the Coronavirus Pandemic" instead of "Vacation/personal/sick days".

2. "Other family obligations" was added as an answer option.

Individuals choosing the new answer option "Quarantine or self-isolation due to the Coronavirus Pandemic" are classified as employed - absent. Previously, they might have chosen the answer option "Because of other disruptions related to the Coronavirus Pandemic", which would have sent them to the "layoff" path, see Appendix C.3.1. We again evaluated the impact of this modification by running two parallel surveys: one using the old form of both questions, and one using the updated questions. From this comparison we find that, with the new phrasing, more individuals report having a job despite also reporting that they did no work last week. Next, conditional reporting no work but having a job, more individuals were classified as employed and at work (as opposed to being classified as a layoff).

For consistency, we use the difference in labor market outcomes between the two parallel surveys to adjust results from previous RPS surveys. To do so, we construct two variables. The first partitions the sample into three bins:

1. No work for pay or profit last week, reported having a job last week, classified as employed absent.
2. No work for pay or profit last week, reported having a job last week, classified NOT as employed absent.
3. Everyone else

The second variable partitions the sample into two bins:

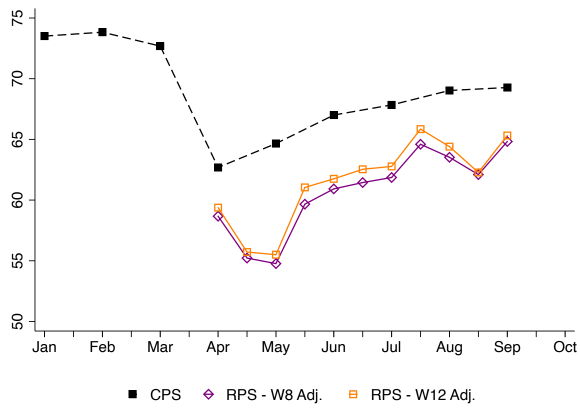
1. Unemployed
2. Everyone else

There are six different combinations of these two variables. For each combination we take the ratio of the share of people from the new and old survey and use it additional adjustment factor in previous waves for a given realization before applying the actual weighting scheme.

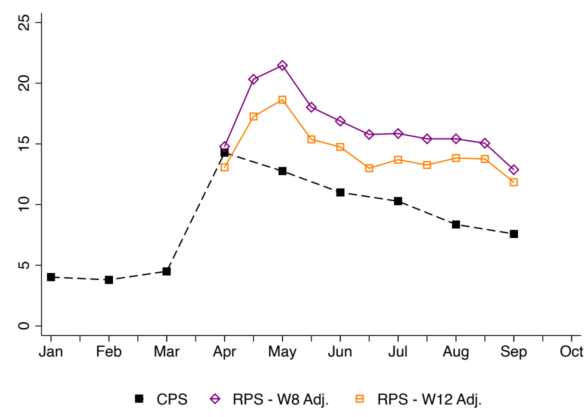
Figure C.2 shows the effect of adjusting the survey questionnaire for the employment rate and unemployment rate, respectively. The line labeled as "RPS - Wave 8 Adj." is the same line as in Figure C.1 but continues it through wave 12 (September). For the data points after wave 8

Figure C.2: The Effect of Wave 12 Adjustment

(a) Employment Rate



(b) Unemployment Rate



the raw data are shown, and respondents with a spouse/partner living in the household and the spouse/partner receive again weight of 0.5 and everyone else a weight of 1. For wave 12, this line only includes the parallel survey with the initial questionnaire. The line labeled as "RPS - Wave 12 Adj." shows for wave 12 the corresponding rates from the survey with the new questionnaire and yielded a higher employment/lower unemployment rate than the initial questionnaire. Accordingly, applying the adjustment discussed above in waves prior to July also increased employment/lowered unemployment.

C.4 Weighting

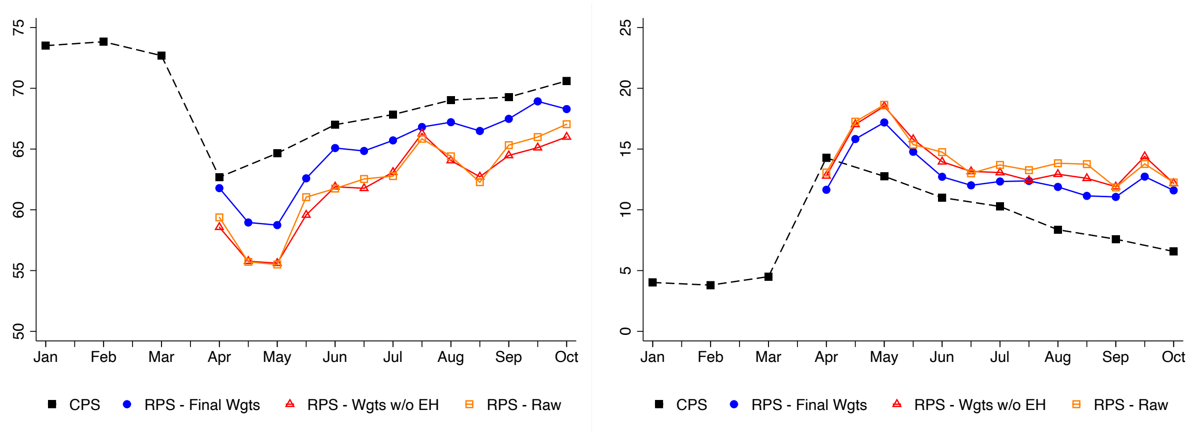
We recommend the interested reader to first read Section 3.2 in the main text and the accompanying Appendix C.2, where we introduce the key labor market concepts.

As described in the Section C.1, we asked Qualtrics administer the survey to a sample of respondents who match the US population along a few broad demographic characteristics: sex, five age bins (18-24, 25-34, 35-44, 45-54, 55-64), race and ethnicity (non-Hispanic White, non-Hispanic Black, Hispanic, other), education (high school or less, some college or associate degree, bachelor degree or more), married or not, number of children in the household (0, 1, 2, 3 or more), three annual household income bins (<\$50k, \$50k-100k, >\$100k) and four census regions. While Qualtrics is able to roughly match these shares, the fit is never exact, and there is fluctuation from wave to wave. Further, our final sample also includes spouses and partners of respondents, but the characteristics of these individuals are not incorporated into Qualtrics' sample design. Finally, given limitations in sample size, the survey company does not attempt to match combinations of these targets.

We address these issues by constructing weights using a raking procedure based on Deming and Stephan (1940). Intuitively, imagine each respondent would only be described by two variables, each with two attributes: age (younger than 40 vs. 40 and older) and education (no college vs. some college or more) and the joint distibution of both characteristics would be different from the distribution in the CPS. In that case, it would be straightforward to manually construct weights for each individual with given attributes such that the weighted joint distribution replicates the CPS. As the number of variables and attributes increases this becomes infeasible because some combination of attributes simply do not exist. The raking algorithm simply tries to find weights to achieve the best match along the desired dimensions. In particular, we match the distribution of targets provided to Qualtrics and finer categories for some variable. Specifically, we use sex, age (18-24, 25-34, ..., 55-64), race/ethnicity (non-Hispanic White, non-Hispanic Black, Hispanic, Other), education (less than high school, high school graduate or equivalent, some college but no degree, associate's degree in college, bachelor's degree, graduate degree), marital status (married + spouse present, divorced, never married, other), relationship status (spouse living in the same household, partner living in the same household, other), number of children (none, 1, 2, 3 or more), and the four major Census regions. We interact those with sex, which ensures that there at least 30 observations in each cell in each wave in the RPS. One could have also considered more combinations of variables but there are some practical limits regarding the convergence of the raking algorithm. Furthermore, we did not include last year's household income in the weighting scheme since going forward for

Figure C.3: The Effect of Weighting

(a) Employment Rate (b) Unemployment Rate



the first 9 months of 2021 one can get only at best a very rough distribution of household incomes in 2020. However, including 2019 household income in the weighting scheme led to virtually the same aggregate labor market statistics as without including it (results not reported here).

Figure C.3 compares the employment and unemployment rate using the raw data (RPS - Raw) and using the weighting scheme above (RPS - Wgts w/o EH).[12] As one can see the extra weighting has only very little effect on the estimates for the employment and unemployment rate.

We also ask respondents about whether they worked for pay or profit in the first full week of the previous month for survey waves covering the CPS reference week or at the start of the current month for survey waves conducted in the end of month. This week either is one week prior to or the same as reference week of the most recently conducted CPS. For most waves, in the raw data the implied employed at work rate falls short by several percentage points (on average 3.7pp) relative to the corresponding CPS number. This suggests that respondents into our survey are negatively selected based on recent employment history. To account for this selection, we also include the employed at work status at the time of the most recent CPS into our weighting scheme (also interacted with sex, age, race, education, marital status and region) which yields the lines "RPS - Final Wgts" in Figure C.3 and was used to construct the estimates in Figures 2 and 2.[13] This adjustment

[12]These not the "true" raw data because they already include adjustments made to reflect changes in the survey questionnaire, which we discuss in Appendix C.3. In fact, the time-series "RPS - Raw" in Figure C.3 correspond to the time-series "RPS - W12 Adj." in Figure C.2. Here we extend it through wave 14 (October). For the data points after wave 12 the raw data are shown, and respondents with a spouse/partner living in the household and the spouse/partner receive a weight of 0.5 and everyone else a weight of 1.

[13]Specifically, we use age (18-24, 25-34, ..., 55-64), race/ethnicity (non-Hispanic White, non-Hispanic Black, Hispanic, Other), education (less than high school, high school graduate or equivalent, some college but no degree,

closes the gap with the CPS as well somewhat reduces the volatility of the time-series.[14]

Table C.3: Detailed Statistics Weighting: CPS vs. RPS

		RPS Wave												
	CPS	2	3	4	5	6	7	8	9	10	11	12	13	14
Gender														
Men	49.2	49.2	49.2	49.2	49.2	49.2	49.2	49.3	49.2	49.2	49.2	49.2	49.2	49.2
Women	50.8	50.8	50.8	50.8	50.8	50.8	50.8	50.7	50.8	50.8	50.8	50.8	50.8	50.8
Age														
18-24	14.6	14.6	14.6	14.6	14.6	14.6	14.6	14.6	14.5	14.6	14.6	14.6	14.6	14.6
25-34	22.7	22.7	22.7	22.7	22.7	22.7	22.7	22.8	22.8	22.8	22.8	22.8	22.8	22.8
35-44	20.9	20.9	20.9	20.9	20.9	20.9	20.9	21.0	21.0	21.0	21.0	21.0	21.0	21.0
45-54	20.3	20.3	20.3	20.3	20.3	20.3	20.3	20.3	20.3	20.2	20.2	20.2	20.2	20.2
55-64	21.4	21.4	21.4	21.4	21.4	21.4	21.4	21.4	21.4	21.4	21.4	21.4	21.4	21.4
Race/Ethnicity														
Non-hispanic White	59.1	59.3	59.3	59.3	59.3	59.3	59.3	59.0	59.1	58.9	58.9	58.9	58.9	58.9
Non-hispanic Black	12.7	12.6	12.6	12.6	12.6	12.6	12.6	12.7	12.7	12.7	12.7	12.7	12.6	12.6
Hispanic	18.8	18.8	18.8	18.8	18.8	18.8	18.8	18.9	18.9	18.9	18.9	18.9	19.0	19.0
Other	9.3	9.3	9.3	9.3	9.3	9.3	9.3	9.4	9.4	9.5	9.5	9.5	9.5	9.5
Education														
No highschool degree	9.4	9.3	9.3	9.3	9.3	9.3	9.3	8.8	7.7	7.9	7.9	7.9	8.6	8.6
Highschool graduate or the equivalent	26.9	27.6	27.6	27.6	27.6	27.6	27.6	26.9	27.6	27.8	27.8	27.8	28.5	28.5
Some college but no degree	18.0	18.1	18.1	18.1	18.1	18.1	18.1	17.9	17.8	17.8	17.8	17.8	17.6	17.6
Associate's degree in college	10.5	10.2	10.2	10.2	10.2	10.2	10.2	10.1	10.3	10.1	10.1	10.1	9.9	9.9
Bachelor's degree	22.9	22.8	22.8	22.8	22.8	22.8	22.8	23.5	23.8	23.6	23.6	23.6	23.2	23.2
Graduate degree	12.2	12.0	12.0	12.0	12.0	12.0	12.0	12.8	12.8	12.8	12.8	12.8	12.1	12.1
Marital Status														
Married, spouse present	50.0	49.5	49.5	49.5	49.5	49.5	49.5	50.6	50.6	50.4	50.4	50.4	49.4	49.4
Married, spouse absent	1.4	1.7	1.6	1.9	2.3	2.0	2.0	1.9	1.7	1.7	2.1	2.0	2.0	2.5
Separated	1.9	2.3	1.8	1.7	1.3	1.9	1.3	1.2	1.5	1.5	1.1	1.2	1.2	1.4
Divorced	9.4	9.6	9.6	9.6	9.6	9.6	9.6	9.3	9.2	9.4	9.4	9.4	9.5	9.5
Widowed	1.7	1.2	1.8	1.6	1.6	1.4	1.9	1.5	1.5	1.6	1.5	1.6	1.8	1.1
Never Married	35.5	35.8	35.8	35.8	35.8	35.8	35.8	35.5	35.5	35.5	35.5	35.5	36.1	36.1
Relationship Status														
Spouse/Partner living in same household	58.6	58.4	58.4	58.4	58.4	58.4	58.4	58.9	59.3	58.9	58.9	58.9	58.8	58.8
No Spouse/Partner living in same household	41.4	41.6	41.6	41.6	41.6	41.6	41.6	41.1	40.7	41.1	41.1	41.1	41.2	41.2
Number of children														
0	68.2	68.4	68.4	68.4	68.4	68.4	68.4	68.6	68.3	68.4	68.4	68.4	68.7	68.7
1	13.4	13.3	13.3	13.3	13.3	13.3	13.3	13.3	13.3	13.5	13.5	13.5	13.1	13.1
2	12.1	12.0	12.0	12.0	12.0	12.0	12.0	11.8	12.1	11.8	11.8	11.8	11.9	11.9
3+	6.3	6.4	6.4	6.4	6.4	6.4	6.4	6.3	6.3	6.3	6.3	6.3	6.3	6.3
Region														
Midwest	20.6	20.6	20.6	20.6	20.6	20.6	20.6	20.7	20.8	20.8	20.8	20.8	20.7	20.7
Northeast	17.2	17.1	17.1	17.1	17.1	17.1	17.1	17.2	17.2	17.2	17.2	17.2	17.2	17.2
South	38.1	38.0	38.0	38.0	38.0	38.0	38.0	37.9	37.7	37.7	37.7	37.7	37.8	37.8
West	24.2	24.2	24.2	24.2	24.2	24.2	24.2	24.2	24.3	24.3	24.3	24.3	24.4	24.4
Number of Observations	61932	2512	2533	2463	2414	6171	3113	4965	3220	3380	3244	5207	3250	3333

Note: Column 1 reports the statistics for the CPS for the Basic Interviews from February 2020. For waves 2 through 7 we use those for weighting. From wave 8 onwards, we always use the last available CPS because we also aim at matching the employed at work rate at the time of the last available CPS.

Table C.3 compares the demographic categories included in the weighting scheme in the CPS and the RPS. Overall, the match is very close. In addition, we report the final number of observations for respondents and spouses/partners if living in the same household for who we could

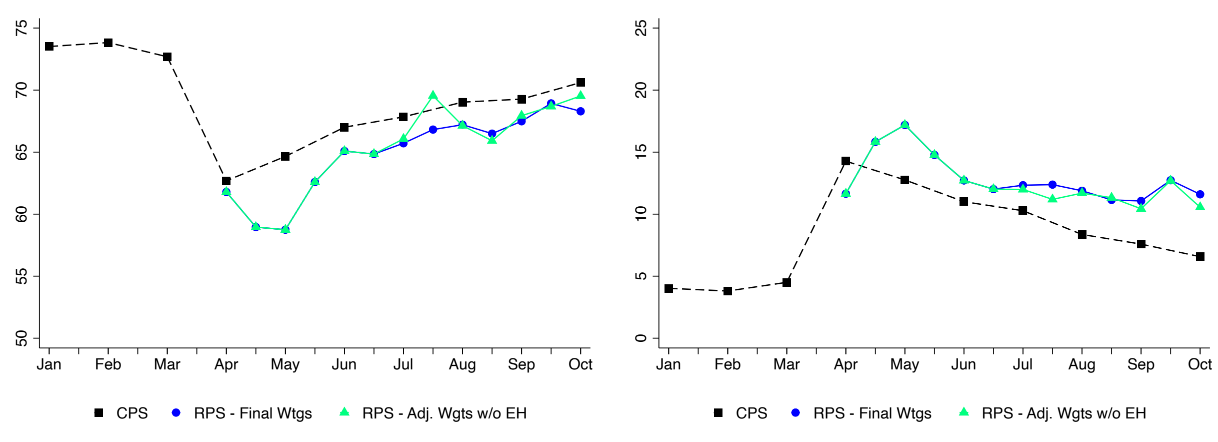
associate's degree in college, bachelor's degree, graduate degree), more broadly defined marital status (married + spouse present, never married, other), relationship status (spouse living in the same household, partner living in the same household, other), and the four major Census regions. We drop the number of children and use a more broadly defined marital status to ensure that each cell in the RPS has at least 30 observations.

[14] As we show in Figure 1a, our sample is not negatively selected based on employment status in February. In the raw data the February employment rate is even closer to the CPS than in the weighted data. Using February employment status rather than the employed at work status at the time of the most recent CPS into our weighting scheme also reduces the volatility of the time-series but leaves the level unchanged.

Figure C.4: Comparing the New Weighting Scheme with the Adjustment Scheme for Waves 8-12

(a) Employment Rate, Age 18-64

(b) Unemployment Rate, Age 18-64



construct a weight. For example, in each survey wave a handful of respondents skip a question. If as a consequence of this, we cannot determine their labor market status, we also do not assign a weight.

As described earlier, we also include the interaction of these variables with gender as well the statistics for the employed at work rate at the previous CPS in the weighting scheme. To visualize the goodnes of fit, we plot in Figures C.5 and C.6 for each wave for all statistics used in the weighting scheme the fraction of individuals with the respective characteristics in the RPS (on the y-axis) and the CPS (on the x-axis). The fact that there are no noticeable deviations from the 45 degree line documents how well the weighting procedure works.

We only started asking about employed-at-work status during the previous CPS from wave 8 onwards. Therefore, we cannot target employed and at work rates in our raking algorithm for waves prio to wave 8. For these early waves, we add an adjustment factor as follows. First, in wave 8 we implement the full raking procedure including employed-at-work status during the previous CPS. Second, again using the data in wave 8, we construct an adjustment factor such that the distribution of employment and joint distribution of other demographic characteristics matches the same distribution from step 1 but this time the employed at work status at the time of the previous CPS is not included.[15] Intuitively, this adjustment assigns more weight to currently employed people with given characteristics to match employment in wave 8 when we also condition on the employed at work status at the previous CPS. We then apply this adjustment factor to all

[15]These demographics are sex, age(below 40, 40 or older), race (non-Hispanic White, other), education (less than an associate's degree [including some college], associate's degree or more), broad marital status (married + spouse present, other) Again, we can use only a restricted set of variables to ensure a sufficient cell size.

Figure C.5: Weights in CPS vs. RPS for Moments in Raking Procedure for – Waves 2 through 7

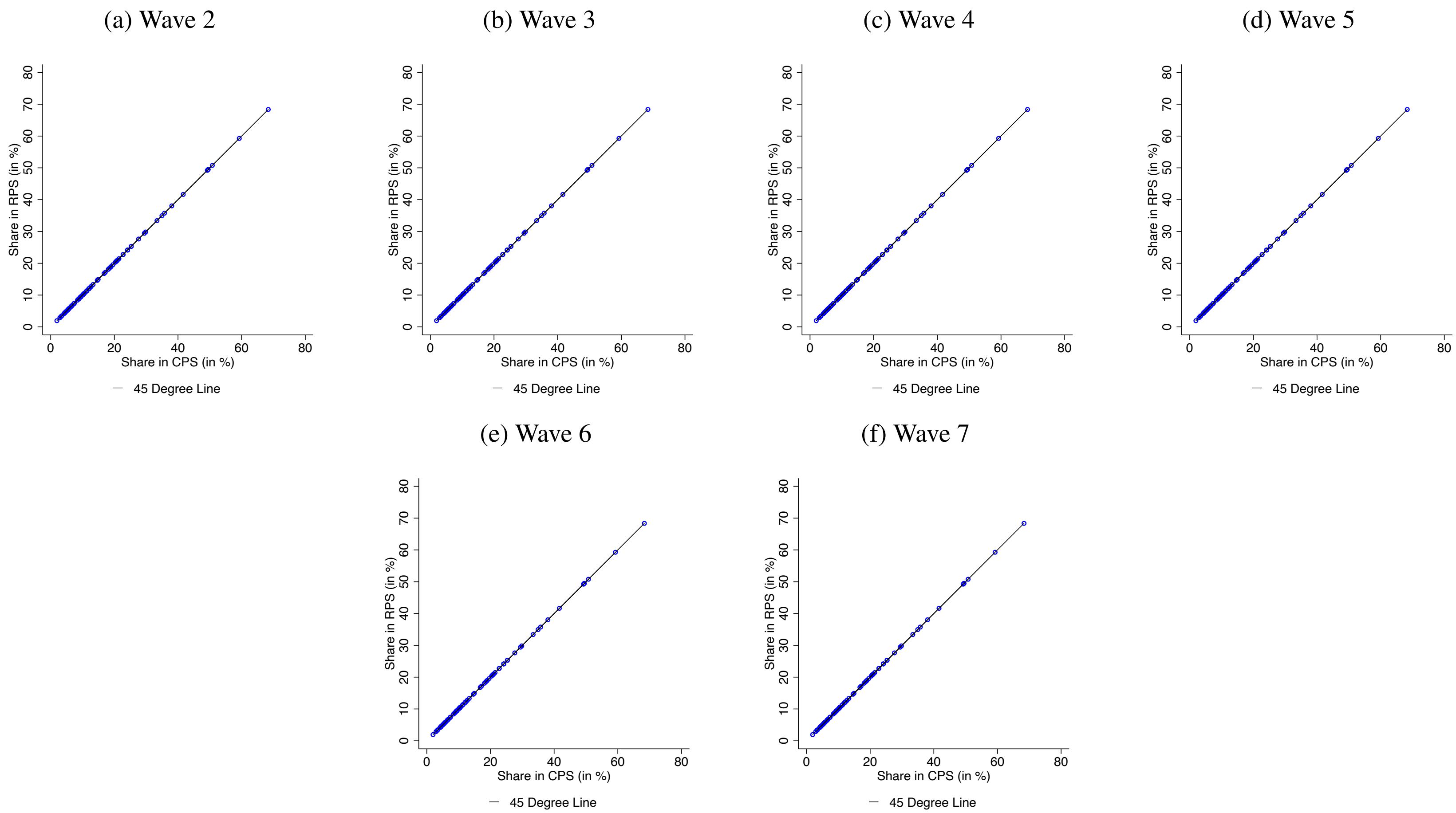
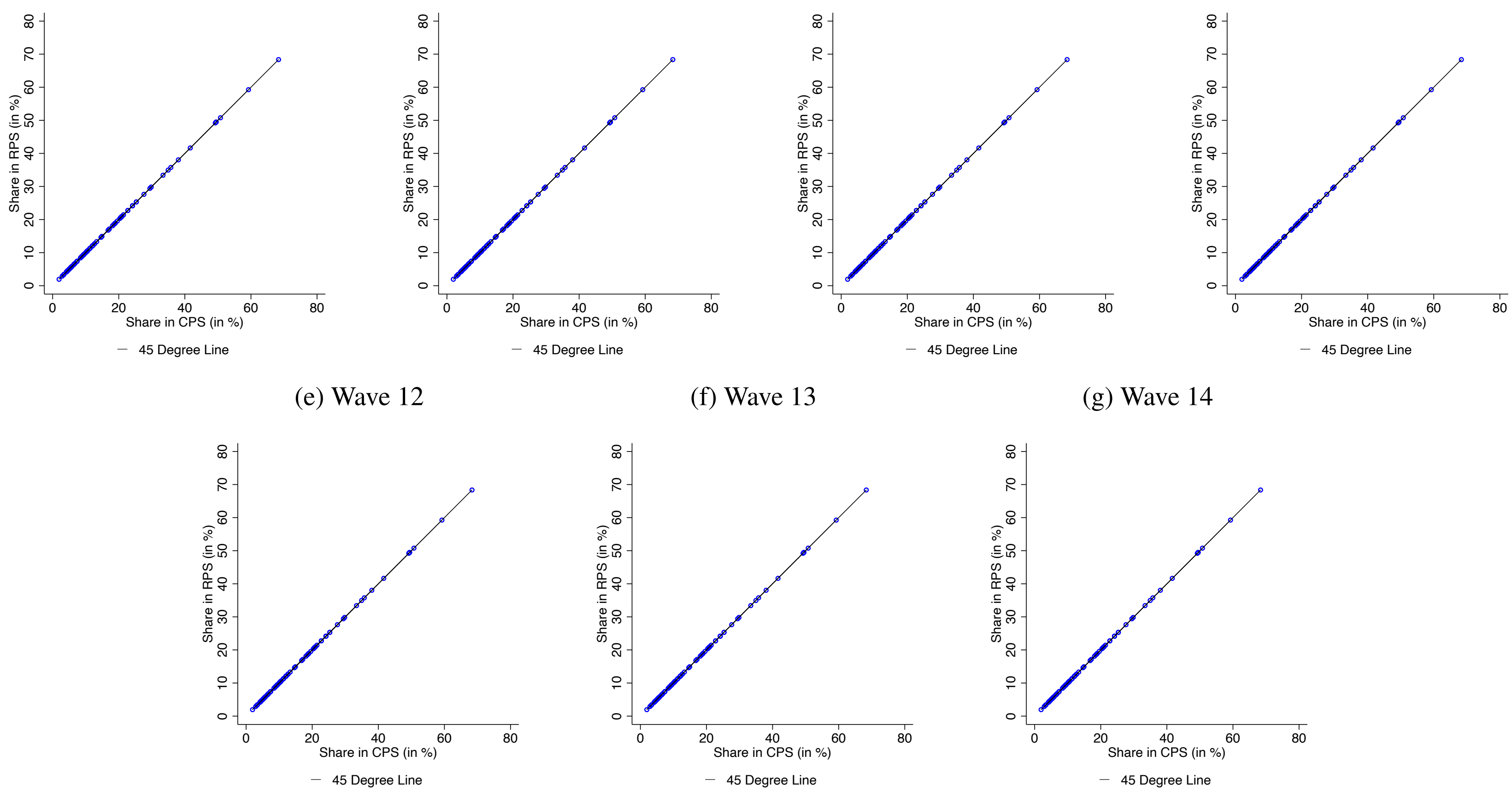


Figure C.6: Weights in CPS vs. RPS for Moments in Raking Procedure for – Waves 2 through 7



(a) Wave 8

(b) Wave 9

(c) Wave 10

(d) Wave 11

(e) Wave 12

(f) Wave 13

(g) Wave 14

waves prior to wave 8.[16] After applying this adjustment factor, we choose weights for waves prior to wave 8 without including employed at work status at the respective previous CPS in the weighting scheme, i.e. we only target demographics. To check how will this procedure works, we can compare our key statistics for waves 8-14 when using the actual weights including employed at work status at the respective previous CPS and the one with the adjustment factor. This procedure works well, see Figure C.4. Here we compare the employment rate and unemployment rate when using the actual weights including employed at work status at the respective previous CPS (RPS - Final Wgts) and an alternative weighting scheme with the adjustment factor but not using the employed at work status at the respective previous CPS (RPS - Adj. Wgts w/o EH). With the exception of the second July wave, which had an above average employed at work rate at the previous CPS, the two weighting schemes closely resemble each other. This suggests that the procedure works well for waves prior to wave 8 as long as selection into the survey based on recent employment was not very different prior to wave 8 than in wave 8.

[16]Note that we apply this adjustment after the adjustments discussed in Appendix C.3.

C.5 Comparison with Foote et al. (2020)

Foote et al. (2020) also run a repeated online survey that closely follows the core labor market module in the CPS, which they refer to as the Yale Labor Survey (YLS). The two surveys were initially created without prior knowledge of each other. The first RPS results were made public on April 15, referencing the week of March 29-April 4. Foote et al. (2020) shared a preliminary draft with us on April 25. Their results were first made public on June 24, with an initial reference week of April 5-11.

The RPS covers the population aged 18-64, while the YLS covers the population aged 20+. Since the level of the employed at work rate varies a lot across age groups, we did not include the YLS in our comparison in Figure 2a. In Figure C.7 we draw a comparison between the two surveys and the CPS not only for the employment at work rate, but also for the employment rate, unemployment rate and labor force participation rate.[17] We display these variables for the CPS for both age groups 18-64 and 20+ as the percentage deviation from the respective February value. We also express the RPS and the YLS time series as deviations from the respective February value in the CPS.

Figure C.7b shows that the employed at work rate in April dropped by 20 percent, independently of the age group. This compares to a decrease of 19.7% in the RPS and to a decrease of 25.1% in the YLS. The change in the May employment rate relative to February in the CPS and YLS are fairly similiar to each other, whereas the RPS implied a further decrease in employment relative to May. From then onwards, both surveys track the CPS similarly well, which is also the case for the employment rate, see Figure C.7a.

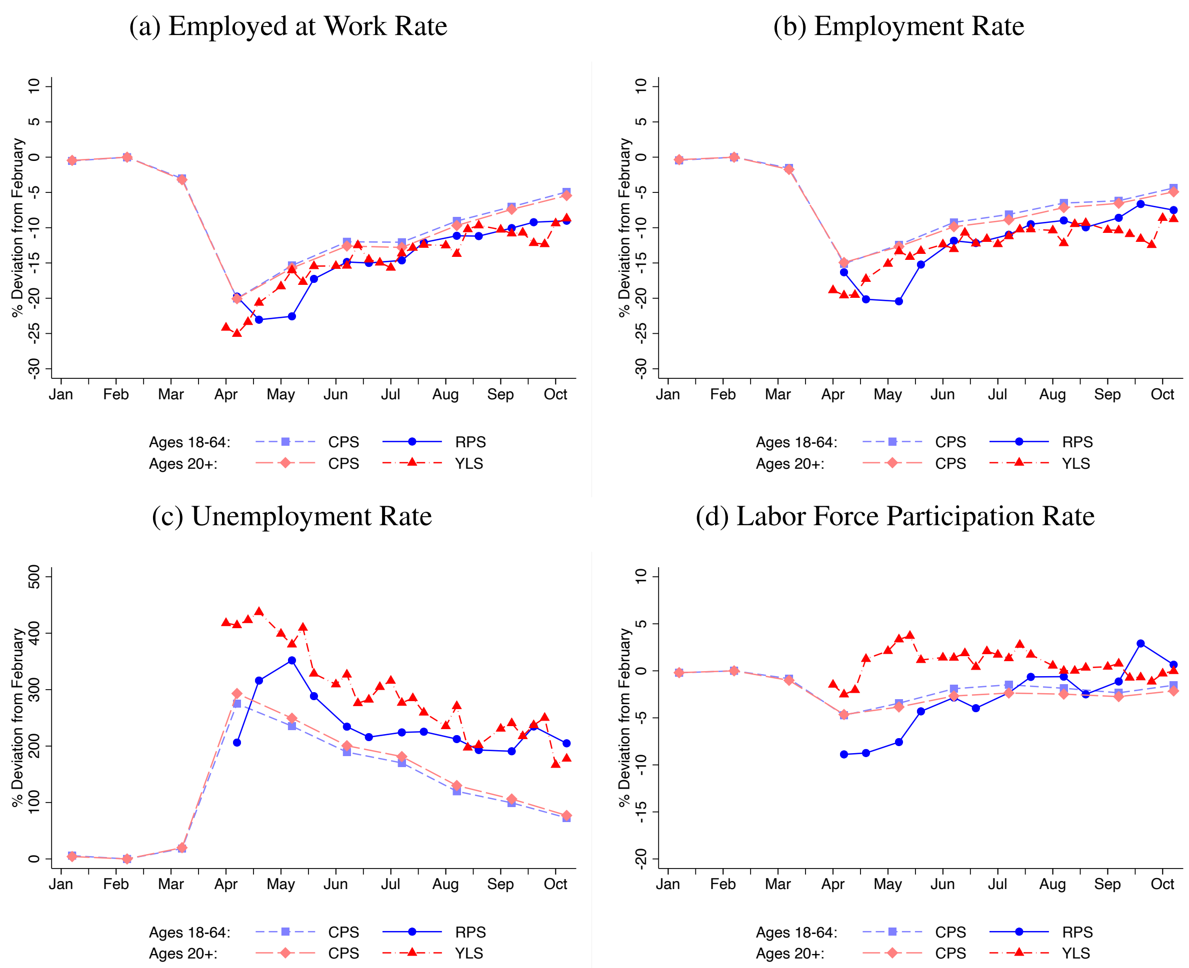
With the exception of the the unemployment rate in April, both the YLS and RPS overpredict the changes relative to February 2020, see Figure C.7c. While the difference between the RPS and CPS is smaller on aver, the difference between the YLS and CPS is more stable over time.

Both surveys show larger increases in the labor force participation rate relative to February than prevailed in the CPS during April and May, see Figure C.7d.

To sum up, our assessment is that the two surveys produce fairly comparable estimates of the state of the labor market.

[17] While Foote et al. (2020) also collect data on hours and earnings, they have not yet published the corresponding time-series.

Figure C.7: Labor Market Outcomes in the RPS, YLS and CPS



Notes: Figure contains data from the RPS and YLS, alongside data from the 2020 CPS. Each data series is shown as percentage deviation from February 2020 in the CPS for the respective age coverage.