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Employer Reallocation During the COVID-19 Pandemic: Validation and Application of a Do-It-Yourself CPS*

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Abstract

Economists have recently begun using independent online surveys to collect national labor market data. Questions remain over the quality of such data. This paper provides an approach to address these concerns. Our case study is the Real-Time Population Survey (RPS), a novel online survey of the US built around the Current Population Survey (CPS). The RPS replicates core components of the CPS, ensuring comparable measures that allow us to weight and rigorously validate our results using a high-quality benchmark. At the same time, special questions in the RPS yield novel information regarding employer reallocation during the COVID-19 pandemic. We document that 26% of pre-pandemic workers were working for a new employer one year into the COVID-19 outbreak in the US, at least double the rate of any previous episode in the past quarter century. Our discussion contains practical suggestions for the design of novel labor market surveys and highlights other promising applications of our methodology.

JEL Codes: C81, C83, E24, J21, J63,

Keywords: Online Survey, Employment, Employer Separations, Employer Reallocation

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1 Introduction

In response to rapidly evolving data demands amid the COVID-19 pandemic, economists have recently begun using independent online surveys to collect national labor market data (Adams-Prassl et al., 2020; Barrero et al., 2021; Belot et al., 2020; Bell and Blanchflower, 2020; Coibion et al., 2020; Foote et al., 2022). The ability of a small group of researchers to design and field a novel national labor market survey represents a potentially important addition to the toolkit of macro and labor economists which could far outlast the pandemic. The primary advantage of independent online surveys is flexibility: surveys can be tailored to address specific research questions by asking novel questions or collecting data at strategic points in time. A second advantage is speed: a single researcher can collect thousands of responses in a few days, which is a faster timeline than existing publicly available surveys. Moreover, because the costly tasks of administering surveys can be outsourced to commercial survey companies in exchange for a relatively modest fee, this approach does not require operating one’s own survey infrastructure and is therefore widely accessible.

However, important questions remain over the utility of labor market data derived from independent online surveys; see Callegaro et al. (2014) for a review. One set of questions concerns the *reliability* of such data; in particular, whether survey samples are representative of the underlying population for variables of interest and whether survey responses contain excessive measurement error. A related set of questions concerns the *comparability* of the data—namely, the extent to which labor market concepts (such as “unemployment” or “earnings”) are comparable to concepts in other widely used surveys.¹

The current paper assesses a novel national online labor market survey designed to speak to the above concerns. Our approach is to use an existing high-quality survey, the Current Population Survey (CPS), as the “scaffolding” for the novel survey, which we label the Real-Time Population Survey (RPS).² By “scaffolding,” we mean that the RPS replicates key portions of the CPS, which ensures that survey concepts are comparable and allows researchers to weight results against a widely used benchmark with a large sample size. At the same time, the RPS omits certain portions of the CPS, leaving room for novel questions.

We begin in Section 2 with a brief overview of how online surveys work. We emphasize prac-

¹Independent online surveys have been used extensively by experimental economists and other social scientists, see e.g. Bursztyn et al. (2014); Kuziemko et al. (2015); Bhargava et al. (2017); Zimmermann (2020). What is new is the adaptation of these surveys to collect national labor market data. In this context, concerns over the representativeness and comparability of the data are particularly acute.

²Scaffolding is a structure that supports a work crew during a construction project. We use this as an analogy for our survey design, which relies on the existing structure of the CPS to construct the RPS.

tical considerations related to survey design, sample selection, and sample weighting based on our experience with the RPS. Section 3 provides more concrete details for the RPS, which ran twice per month from late March through September 2020 and then monthly through June 2021. A crucial feature of the RPS is that it assigns labor market status using the same intricate sequence of questions in the CPS. An example using real-world data demonstrates that even minor deviations from this sequence can have large effects on key estimates. We are in the process of making publicly available a template survey containing this portion of the RPS, which can be used as a starting point for new labor market surveys.³

In Section 4 we use overlapping questions in the RPS and CPS to validate outcomes in the RPS, addressing concerns about the data’s reliability. While we note some discrepancies, RPS time series for labor force participation, employment, unemployment, hours worked, and earnings are similar in both level and trend to the CPS during the pandemic, but were available several weeks before the respective CPS release. In addition, retrospective questions in the RPS about labor market outcomes in February 2020 produce statistics in line with the February 2020 CPS, providing an additional point of validation just before the onset of the pandemic in the US. Together, retrospective and contemporaneous questions allow us to observe individual-level changes over time during the pandemic, which we exploit in our central application.

Bolstered by the above comparisons, Section 5 uses RPS data on employer tenure to provide novel insights on the extent of employer separations and reallocation one year into the COVID-19 pandemic. This information is not available in the CPS because the employer tenure supplement did not run during the first year of the pandemic in the US. We document that, among workers employed in February 2020, 37.2% had separated from their pre-pandemic employer one year later, with 11.0% not employed and 26.2% working for a new employer. While the share transitioning to non-employment was similar to some recent recessions, the share working for a new employer was at least twice as large as in any previous episode in the past quarter century, lending support to the prediction by Barrero et al. (2020) that the COVID-19 pandemic would produce large reallocation effects. We also find notable differences in labor market outcomes by pre-pandemic employer tenure. Among workers who had been with their pre-pandemic employer less than two years, 60.7% had separated one year later, compared with 16.2% of workers who had been with their pre-pandemic employer for at least a decade. At the same time, conditional on a separation occurring, high-tenure workers were less likely to be working for a new employer one year into the pandemic. Collectively, these findings are informative for the calibration and validation of quanti-

³The RPS micro data is available at <https://www.openicpsr.org/openicpsr/project/158081/version/V2/view> and is free to use with appropriate citation.

tative models of the labor market consequences of the pandemic (Gregory et al., 2020) and suggest a particularly important role of match-specific productivity for labor market outcomes during this recession (Menzio and Shi, 2011; Fujita and Moscarini, 2017; Gregory et al., 2021).

The RPS is far from the only national online labor market survey. Several research institutions conduct long-run online panels, which are intended to be nationally representative and collect labor market information. Important examples for the US include the RAND American Life Panel (ALP), the University of Southern California’s Understanding America Study (UAS), and the Federal Reserve Bank of New York’s Survey of Consumer Expectations; examples for other countries include the Longitudinal Internet Studies for the Social Sciences (in the Netherlands) or the German Internet Panel. Two strengths of these types of surveys are their panel component, which allows researchers to track individuals over time, and their probability-based sampling procedures. In contrast to institutional surveys, which require access to a large support infrastructure, independent online surveys like the RPS allow researchers to outsource recruitment and survey collection to a commercial survey company in exchange for a fee. While some of the above institutional surveys do offer independent researchers the opportunity to field their own survey to the institutional panel, the costs are substantially higher. For example, a 10 minute survey with 1,500 respondents costs \$39,500 with UAS and \$42,000 with ALP, versus \$7,500 with the provider we use.⁴ At the same time, during the pandemic the RPS performed at least as well as other institutional online labor market surveys (as judged by comparisons with the CPS).

Even more than its relatively modest cost, the key feature of the RPS is that its design facilitates clean comparison and validation with an existing benchmark. This aspect of the RPS is similar to contemporaneous work by Foote et al. (2022), who ran a repeated online survey from April 2020 through March 2021 that also followed core aspects of the CPS.⁵ Their survey was designed independently from ours, and contains some differences. For example, their survey does not ask about employer tenure, which forms the basis of our analysis in Section 5, or about live-in spouses or partners. They also used a different commercial survey provider to collect responses. However, in weeks that overlapped with the RPS both survey yield similar estimates for several labor market series, which we interpret as additional validation of the scaffolding methodology. Section 6 concludes with a discussion of several other promising applications of this methodology related to better understanding work arrangements (e.g., remote work, non-wage amenities, and “gig work”),

⁴The pricing for UAS and ALP was retrieved on May 3, 2022.

⁵Another related project is Kurmann et al. (2021), who use payroll data from Homebase to shed light on small business dynamics and employment during the pandemic. While their data are administrative, rather than survey-based, our projects are related by an attempt to validate the reliability of new sources of labor market data through the use of external benchmarks.

as well as specific policy topics lacking relevant nation-wide data.

2 A Step-by-Step Guide to Fielding a Novel Online Survey

This section provides a brief overview of how independent online surveys work. Relative to existing guides on survey design and sample collection (see e.g., [Sue and Ritter \(2016\)](#) for a general introduction, and [Callegaro et al. \(2014\)](#) for online panels), we focus on practical step-by-step suggestions for researchers fielding their own online survey. These suggestions are based on experiences and lessons learned throughout the 16 months we spent designing and fielding the RPS.

Some of our discussion centers on the commercial survey company Qualtrics, which is the company that we used to design and administer the RPS. Qualtrics has been widely used by academics as well as by federal agencies for survey pre-testing and evaluation.⁶ However, several other commercial survey companies exist, including Inc-Query, NORC, QuestionPro, and YouGov.

2.1 Designing the Survey Questionnaire

The first step is to design the survey questionnaire, which has two components: a set of questions, and a flow procedure which determines the sequence of questions that are presented to respondents. For an online survey, the questionnaire is built out using survey software (the RPS used software provided by Qualtrics, which is licensed by many academic institutions). This software allows for a variety of question types, such as multiple choice, select-all-that-apply, sliders, or text entry. Among these, text entry requires the most time and effort to answer, which raises the probability of non-response and lowers the total number of questions that can be asked. The survey software also allows for non-trivial flow procedures: for example, the survey can branch (displaying different questions or answer options) based on responses earlier in the survey. Questions or answer options can also be randomized across different respondents, which can be used to run experiments or test aspects of survey design.

Any new survey will face skepticism regarding the reliability of its data: in particular, whether survey samples are representative of the underlying population for variables of interest and whether survey responses contain excessive measurement error. One way to design the survey to address these concerns is to borrow questions from an existing high-quality survey. This helps ensure that questions are well-phrased and allows the data from the new survey to be compared and validated with data from the existing survey. Beyond borrowing a single question, replicating a sequence

⁶See [Yu \(2019\)](#) for an overview of online survey methods and their use for testing at the US Census Bureau and the Bureau of Labor Statistics.

of questions carries additional benefits, both because it provides multiple opportunities for validation and because the ordering of questions can impact how individuals answer them (Krosnick and Presser, 2010). We refer to this as a “scaffolding approach” because the new survey relies on the structure of an existing benchmark survey, allowing the new survey to benefit from the research, testing, and experience of the existing survey. In Section 3.2, we provide additional details on this approach in the specific context of the RPS, which used the CPS as a benchmark.

The scaffolding approach does not require that researchers replicate all of a benchmark survey and therefore allows room for novel questions which are not contained in the benchmark. While novel questions will face the same reliability concerns discussed in the previous paragraph, two considerations can help address them. First, if the scaffolding approach establishes that the novel survey aligns with a benchmark survey along a host of dimensions, concerns over sample selection may be less acute. For example, in Section 4 we establish that the CPS and RPS produce similar statistics for employment, hours worked, earnings, industry composition, and pre-pandemic employer tenure, which could provide some confidence in the novel RPS questions analyzed in Section 5. Second, novel survey questions are particularly powerful when they can be partially validated. For example, Bick et al. (2022) show that novel questions on commuting in the RPS generate a time series of aggregate commuting trips which closely align with workplace mobility metrics from cellphone data. The ability to validate the aggregate time series provides confidence in the quality of the RPS responses, even as the underlying micro data in the RPS allow for several analyses which are not possible with cellphone data. Our analysis of employer separations in Section 5, which is partially validated using transitions to non-employment in the CPS, provides another example.

Flows into and out of online panels make it difficult for independent online surveys to follow individuals over time for longer than a few weeks. This presents a challenge for researchers interested in individual level changes over time. One partial solution is the use of retrospective questions. For example, the RPS asks about labor market outcomes both in the previous week and in February 2020, just before the COVID-19 outbreak in the US, which allows us to study individual-level changes during the pandemic. While a potential concern with retrospective questions is that measurement error may increase as the recall period gets longer, we show in Section 4 that retrospective reports about February 2020 in the RPS align fairly closely with data from the February 2020 CPS. This suggests that measurement error was not excessive in this case, possibly because the time just before the COVID-19 pandemic was particularly salient for many people. More generally, retrospective questions are likely to elicit more accurate information when the past event is easy to recall (Lavrakas, 2008). For example, the Annual Social and Economic Supplement of the CPS, which collects information about income throughout the previous calendar year, is asked in March

to coincide with tax season in the US.

2.2 Setting Sample Criteria and Collecting Survey Responses

After a survey questionnaire has been designed and built out, the next step is to administer the survey. Commercial survey companies like Qualtrics provide access to members of online panels who have agreed to participate in surveys.⁷ The Qualtrics panel is not a random sample of the US population, even if one would condition on access to the internet. (In the 2019 American Community Survey, 94% of individuals aged 18-64 lived in households with internet access.) However, to achieve representativeness along certain dimensions, researchers can set sample quotas for desired groups based on known population shares from existing data sources. These groups can be based on demographics (e.g., sex or age) or other questions (e.g., employment or household income). It is particularly important to set quotas for groups that are underrepresented in the online panel: in our experience, racial minorities and households with high income were often difficult sample quotas to satisfy. In practice, quotas must be sufficiently broad to ensure that fielding does not proceed too slowly. For example, the RPS set quotas for only five age bins: 18-24, 25-34, 35-44, 45-53, 55-64. Finally, researchers can direct Qualtrics to drop surveys which were completed too quickly in an attempt to weed out respondents who did not answer questions carefully.

Researchers communicate their desired sample criteria (total sample size, sample quotas, and the sampling time frame) to a Qualtrics team, who then field the survey according to these criteria. Survey responses can be downloaded from the cloud in real time. Once fielding begins, initial responses are collected rapidly because all quotas remain to be filled. As quotas are filled, collection proceeds more slowly because respondents must now satisfy only the remaining quotas. One way to partly mitigate this issue is to ask the Qualtrics team to anticipate which quotas will be most difficult to satisfy, and attempt to target those quotas first. A second strategy is to “turn off” certain quotas as the fielding nears completion. (For example, if only five percent or so of our sample remained to be collected, and the quota shares in our partial sample were close to our targets, we would often tell Qualtrics to ignore some quotas in order to allow collection to complete more quickly.) Overall, in our experience, Qualtrics was usually able to collect 2,000-3,000 responses that roughly met our sample quotas within two to four days.

⁷Qualtrics does not directly manage a panel of survey recipients. Instead, Qualtrics partners with other companies who actively manage their own panels. Qualtrics then acts as a panel aggregator, distributing surveys across its partner panels to achieve the desired sample size and composition. Collectively, the panels that Qualtrics draws from include about 15 million people in the US. Qualtrics charged us \$5 per valid, completed survey for surveys lasting 10-15 minutes on average, of which respondents received between \$1.50 and \$2.50.

2.3 Constructing Sample Weights

Despite the use of sample quotas, the resulting survey samples may differ from the targeted population in observable characteristics for several reasons. First, sample quotas are rarely matched exactly by the survey company. Second, given limitations in sample size, it is infeasible to match very fine breakdowns (e.g. single year age bins) or combinations of sample quotas (e.g. sex interacted with age and race). Finally, if the survey asks respondents about multiple household members, only the respondent's answers about themselves will count towards sample quotas.

To address these issues, researchers can construct sample weights. There are a variety of methods used to construct weights. We used an iterative proportional fitting (raking) algorithm proposed by [Deming and Stephan \(1940\)](#) because of its intuitive implementation. The algorithm begins with a set of cell shares from the novel and benchmark survey (e.g., the joint distribution of sex and education). It then attempts to set sample weights to minimize the average difference in cell shares between the two surveys. For example, if, compared with the benchmark survey, the novel survey contained a smaller share of females with a bachelor's degree, then the algorithm would increase the sample weight for members of this group so that the weighted cell share was closer to the cell shares from the benchmark survey. At the same time, the algorithm penalizes changes relative to original cell shares in the novel survey, which helps avoid multiple solutions.

3 Sample Selection and Survey Design in the RPS and CPS

This section narrows our focus to the specific case of sample selection and survey design in the RPS. We emphasize areas of overlap with the CPS, which was the benchmark survey used to construct the RPS.

3.1 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 eight months, then 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 a probability-based sample, the RPS derives its sample from the online panel provided by Qualtrics. Sample quotas for the RPS were set to be nationally representative for the US along several broad demographic characteristics: sex, age, race and ethnicity, education,

marital status, number of children in the household, Census region, and household income in 2019.⁸ Panel members are not allowed to take the survey twice in a row, but we are unable to verify whether respondents participate more than once in non-adjacent survey waves. According to Qualtrics staff, very few panel members did so.

From April through September 2020, the RPS typically collected 1,500 to 2,000 responses on the Qualtrics platform in interview waves fielded twice per month. In October 2020, the RPS switched to a monthly frequency with approximately 2,200 respondents. Like the CPS, the RPS also asks respondents to answer the same questions on behalf of spouses or any unmarried partners in the same household (though, unlike the CPS, we do not ask about other household members 15 and older). This additional information expands the number of individual-level observations by about 60 percent at no additional financial cost. In total, the RPS features 73,355 valid observations covering April 2020 through March 2021 (see Table A.1 for the details for each wave).

3.2 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 “scaffolding” approach used to design the RPS involves following as closely as possible CPS 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 whenever practical. In particular, the survey replicates the intricate sequence of questions necessary to assign labor market status (see Appendix A.2-A.3 for additional details). We are in the

⁸More specifically, the targets were based on the following categories: Age: 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's degree or more; Marital status: married or not; Number of children in the household: 0, 1, 2, 3 or more; Census region: Midwest, Northeast, South, West; Annual household income in 2019: <\$50k, \$50k-100k, >\$100k.

process of making publicly available a template questionnaire containing this portion of the RPS within the Qualtrics software, which can be used as a starting point for new labor market surveys.

The RPS also included a suite of questions not contained in the basic CPS on topics including remote work and worker expectations about the future. Our analysis in Section 5 relies on questions about employer tenure and recall, which the CPS did not collect over this time period. To study individual-level changes during the pandemic, the survey asks retrospective questions about February 2020, providing a reference point just prior to the COVID-19 outbreak in the US.

3.2.1 Question Phrasing and Sequencing Matters: An Illustrative Case from the RPS

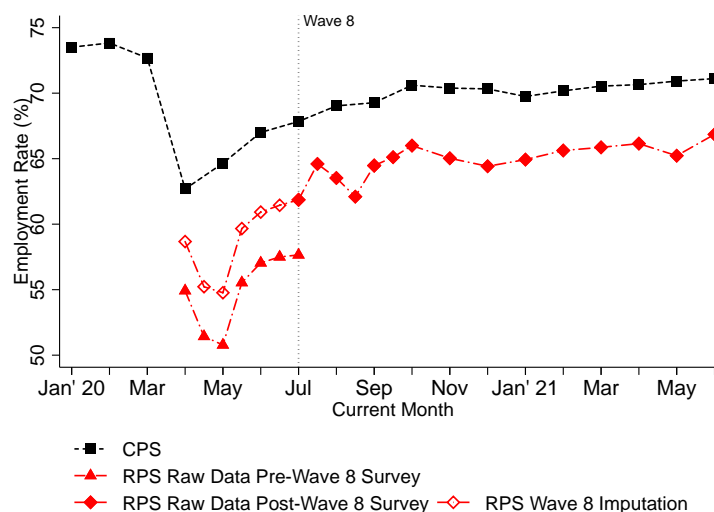
During the early months of the pandemic we made multiple adjustments to the RPS survey (see Appendix A.3 for a complete list with explanations). Here we provide additional details about one adjustment which turned out to be particularly important. The episode illustrates the utility of having a reliable benchmark when evaluating a novel survey.

For the first seven waves of the RPS, in April - June 2020, the unweighted employment rate in the RPS was below that of the CPS by ten percentage points on average. We eventually suspected that this may be due to a deviation from the CPS questionnaire in the first few questions of the labor market module. In the CPS, the first question in this module is “Last week, did you do any work for pay or profit?” Individuals answering “yes” are classified as employed and at work. Individuals answering “no” are then asked “Last week, did you have a job, either full or part time? Include any job from which you were temporarily absent.” Individuals answering “no” to this question are classified as not employed; those answering “yes” to this question are then asked why they were absent from work, and based on their answer are classified as either “not employed” or “employed and absent.” Through June 2020, the RPS asked these questions slightly differently in an attempt to make the phrasing more natural in an online setting. First, we asked “Last week, did you have a job? Please include any jobs from which you were temporarily absent.” Individuals answering “no” were classified as not employed. Individuals answering “yes” were then asked “Last week, did you work any hours for pay or profit at your job?” Individuals answering “yes” were classified as employed and at work; those answering “no” were then asked the follow-up question from the CPS regarding why they were absent.⁹

In the RPS question sequence above, anyone who answered “no” to the first question regarding whether they had a job would be classified as not employed. Alternatively, in the CPS, anyone

⁹Individuals who reported that someone in the household owns a business and who responded “no” to the first question regarding work in the previous week, were asked the same set of follow-up questions in the CPS and RPS.

Figure 1: The Effect of the Wave 8 Adjustment on the RPS Employment Rate, Age 18-64



Notes: “RPS Raw Data” are the actual raw data, where those respondents with a spouse/partner living in the household and the live-in spouse/partner receive a weight of 0.5 and everyone else a weight of 1 to reflect that the original sampling scheme only included the respondents themselves. In wave 8, we ran two parallel surveys. “RPS Wave 8 Imputation” reflects a backward-imputed series accounting for the differences between the two wave 8 surveys.

who answered “yes” to the first question regarding whether they worked in the past week would be considered employed. This implies that if an individual worked in the past week, but for some reason did not consider that a “job,” they would be classified as employed in the CPS but not in the RPS, which could help explain the lower employment rate in the RPS.

To test this hypothesis, we ran two simultaneous versions of the RPS in wave 8. The two versions were identical except that one used the CPS formulation of the first two labor market questions, and the second continued to use the RPS deviation. The resulting employment rates are displayed in Figure 1. The black squares are the CPS estimates, the red triangles are RPS waves prior to this adjustment, and the red diamonds are RPS waves after this adjustment. Early July 2020 contains two data points for the RPS, since we ran simultaneous surveys in this week. We found that the CPS adjustment increased the employment rate from 57.7% to 61.9%, accounting for two fifths of the discrepancy between the CPS and the original RPS. Based on these results, we adopted the CPS formulation for all subsequent RPS waves. We also backward-imputed RPS estimates prior to wave 8 to reflect this adjustment (the hollow red diamonds).¹⁰

¹⁰Using the parallel wave 8 surveys, we computed the ratio of the employment rate in the adjusted RPS survey to the employment rate in the original RPS survey for a set of demographic groups. We then multiplied demographic-specific employment rates by this ratio for each of the first seven RPS waves, in order to impute what the employment rates would have been if we had used the adjusted survey questionnaire.

We emphasize two lessons from this episode. First, even minor deviations from a benchmark survey can have a sizable impact on estimates. It is therefore important to replicate relevant portions of a benchmark survey as closely as possible. Second, a useful method to assess the impact of a potential survey adjustments is to run two simultaneous waves of the survey which are identical except for a single change.¹¹

3.3 The Impact of Sample Weights in the RPS

The sample quotas in Section 3.1 ensured that the RPS and CPS contained similar distributions of individuals by broad categories of sex, age, race and ethnicity, education, marital status, number of children in the household, Census region, and 2019 household income. We then constructed sample weights that incorporated live-in partners of RPS respondents (which did not count towards sample quotas) and that used finer demographic categories (e.g., our sample quotas used three education categories, while our weighting procedure used five). The results of this weighting are displayed in Figure 2, with the CPS as black squares, the unweighted RPS series as solid red diamonds, and the RPS series with demographic weights as hollow green triangles. The figure shows that demographic weights did not have a large effect on the employment rate in the RPS, and in particular did not help to narrow the remaining gap between the RPS employment rate and the CPS employment rate, though they do noticeably reduce the volatility from one survey wave to the next.

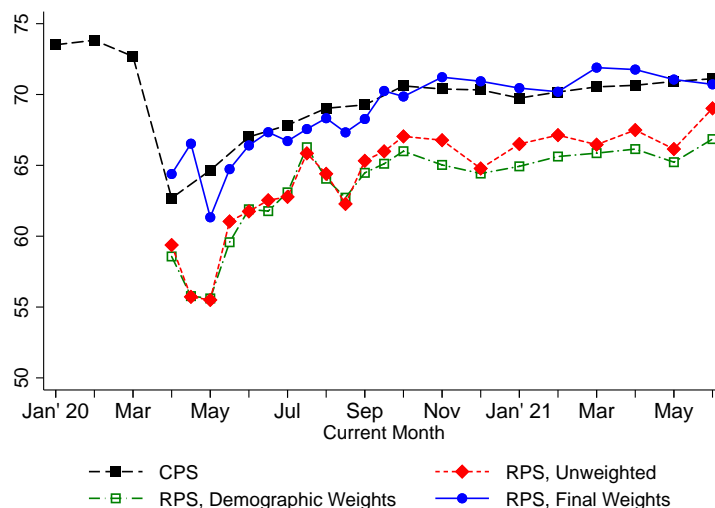
Because we assign labor market status using the same procedure as the CPS, the lower RPS employment rate suggests that RPS respondents are negatively selected based on employment. However, employment in February 2020 was fairly close in the RPS and CPS, implying negative selection only on more recent employment experiences (i.e., higher transition rates to non-employment since February 2020). To account for this pattern, we incorporate two additional categories into the above weighting scheme: the employed-and-at-work status and the layoff status of non-employed workers, both at the time of the most recent past CPS (see Appendix A.4 for details). Intuitively, this weighting scheme ensures that the RPS and CPS contain a similar share of individuals who were either employed-and-at-work or on layoff in the previous month, even while it does not directly weight on any current labor market outcome.¹²

The results of this final weighting procedure are plotted as solid blue circles in Figure 2. On

¹¹ An alternative approach, which does not require running an additional survey, is to randomize which of two versions of a given question are shown to a given respondent. This is straightforward to implement using the survey software.

¹² Another way to ensure that the RPS and CPS produce similar labor market outcomes is to weight on these outcomes directly. For example, Bick et al. (2022) use RPS sample weights which target employment in the contemporaneous CPS month. However, one of the initial goals of the RPS was to provide labor market estimates *ahead of* the CPS, which by construction would rule out using the current CPS to construct sample weights.

Figure 2: The Impact of Sample Weights on Employment in the RPS



Notes: “RPS, Unweighted” are the raw data, where those respondents with a spouse/partner living in the household and the live-in spouse/partner receive a weight of 0.5 and everyone else a weight of 1 to reflect that the original sampling scheme only included the respondents themselves. In addition, this series includes for backward adjustments reflecting survey modifications in waves 8 and 12, for details see [A.3](#).

average, incorporating recent labor market outcomes increases the employment rate by 5.1 percentage points. This reduces the average gap between the CPS and RPS employment rates from 5.7 percentage points with the demographics-only weighting procedure to 0.5 percentage points with the final weighting procedure.

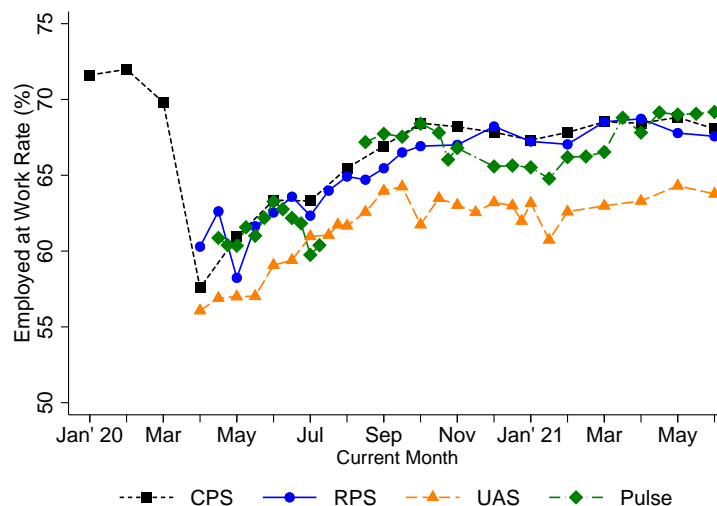
4 Validation of Labor Market Estimates

This section documents the extent to which outcomes in the RPS are consistent with those in the CPS. Although we note some discrepancies, along most dimensions the two surveys closely agree. We view these comparisons with the CPS as validation tests of the RPS: while many variables that we use for sample selection or weighting are of course correlated with current or February 2020 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 this Section by construction.

Figure 3 displays 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. 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?”¹³ Both the CPS and RPS

¹³As 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

Figure 3: Employed and At Work Rate During the COVID-19 Pandemic, Age 18-64



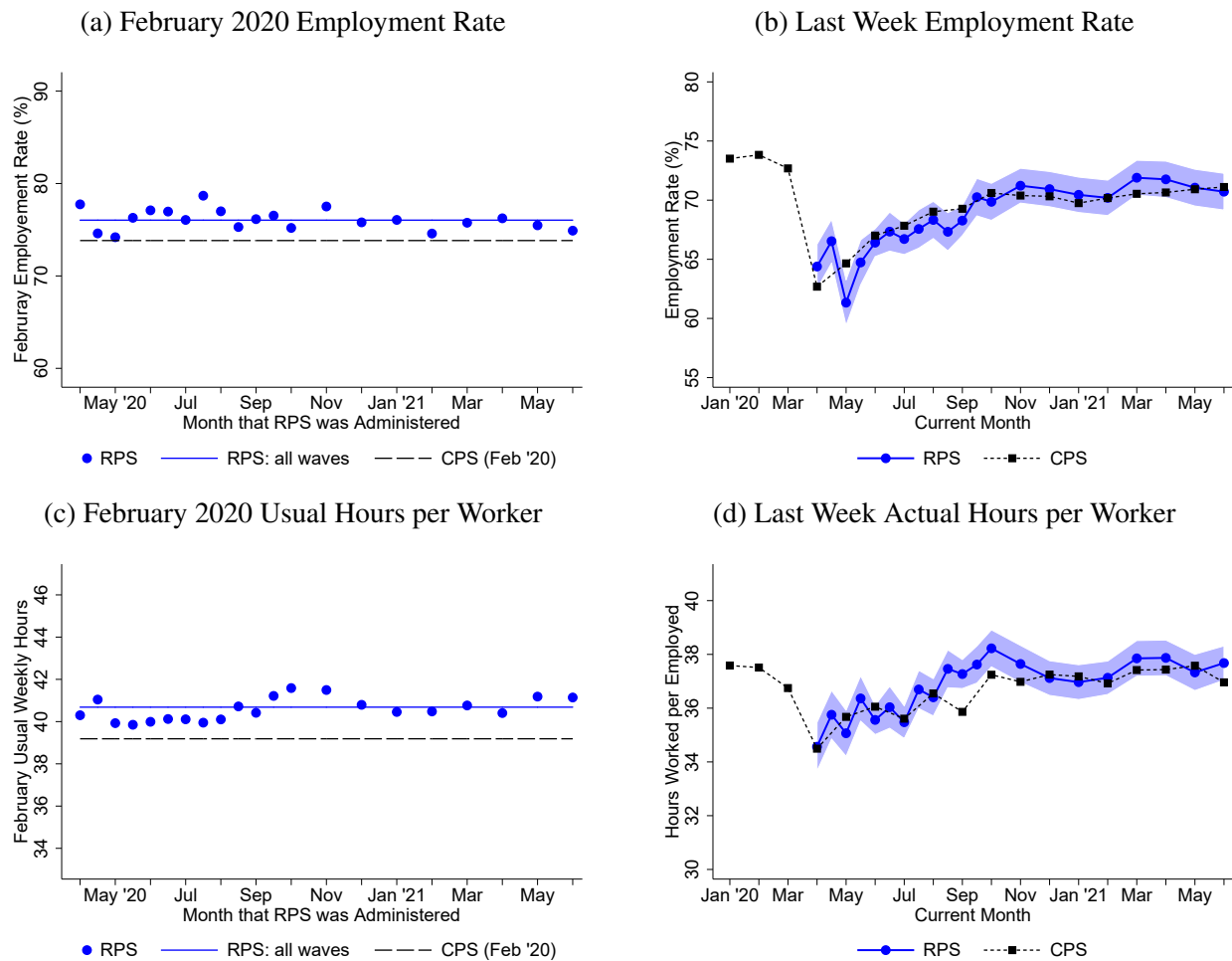
Notes: Figure contains data from the RPS, CPS, Understanding America Survey (UAS), and Census Pulse Survey. The Pulse began collecting data in April 2020 and paused collection from mid-July to mid-August. For each dataset, the sample is all adults age 18-64.

display a sharply lower employed and at work rate in April 2020 relative to the first few months of 2020. The series then increases substantially through June 2020 and then continues to recover more slowly from July 2020 on. The RPS series is more volatile in April and May 2020, when we made early adjustments in survey design and weighting (see Appendix A.3). From June 2020-onwards the RPS series is more consistent and tracks the CPS more closely.

Because this measure of labor input mainly relies on a single question, several other online surveys ask it as well. Figure 3 also displays estimates from the Pulse Survey, a large-scale weekly online survey based on the US Census Bureau's email database, 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 absolute percentage point (pp) deviation relative to the CPS in the sample period is 1.0pp, 1.2pp, and 4.3pp 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. We do not include estimates from Foote et al. (2022) here because their survey covers a different age group, but we compare our results to theirs in Appendix A.5. We find the two surveys produce comparable estimates for most series relative to the respective age-group-specific benchmark in February 2020. The most notable exception is the unemployment rate, for which the RPS and CPS more closely align.

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 4: Employment and Hours Worked in the RPS and CPS, Age 18-64

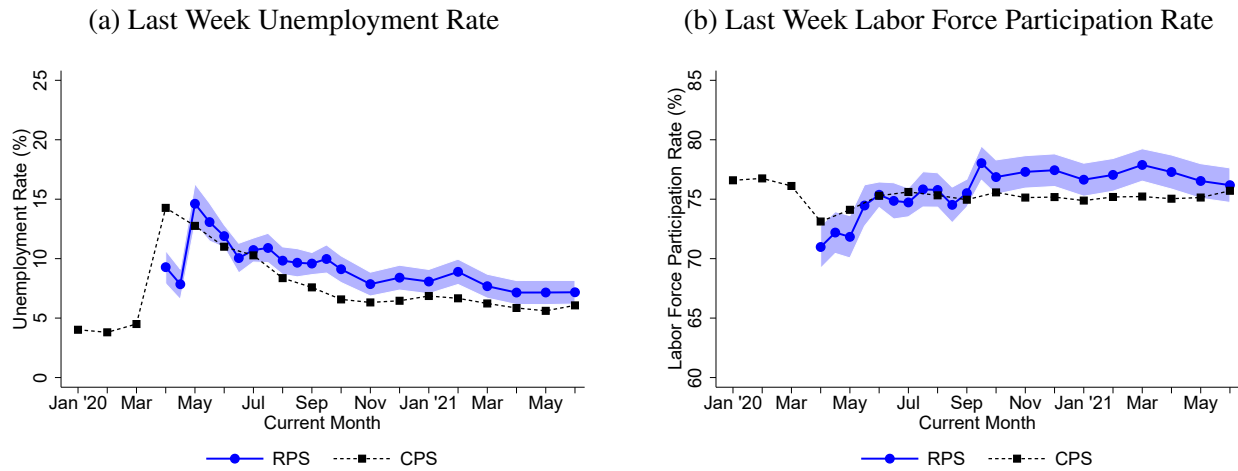


Notes: Panels (a) and (c) display data from the February 2020 CPS and from retrospective questions regarding February 2020 for each wave of the RPS. Panels (b) and (d) display data on outcomes “last week” from May 2020 - June 2021 waves of the RPS and CPS.

The RPS asks respondents whether they were employed in February 2020, the month before the COVID-19 outbreak in the US, and whether they were employed “last week.” Figures 4a and 4b compare these outcomes to their CPS counterparts. Figure 4a shows that the mean February 2020 employment rate across RPS surveys is 76.6%, slightly higher than the 73.8% employment rate in the February 2020 CPS. The standard deviation of February 2020 employment across RPS waves is 1.4%. Figure 4b shows that during the pandemic, employment rates in the RPS and CPS track each other closely from mid May 2020 onward.

Figure 4c displays mean usual weekly hours worked per worker in February 2020 in each RPS wave since April 2020, where the sample is restricted to those who were employed in February 2020. The mean usual work week is 40.6 hours, with a standard deviation of 0.4 hours across sur-

Figure 5: Unemployment and Labor Force Participation in the RPS and CPS, Age 18-64



Notes: Data is from May 2020 - June 2021 waves of the RPS and CPS.

vey waves. This is slightly higher than the 39.2 mean hours per week in the February 2020 CPS. The standard deviation of usual hours in February 2020 between individuals is similar in both surveys (14.1 hours in the RPS vs. 14.0 hours in the CPS). Figure 4d displays mean actual hours worked last week during the pandemic among the employed. In April 2020, the RPS and CPS both report a mean of 34.5 hours. By June 2021, mean hours increased to 37.7 hours in the RPS and 37.0 hours in the CPS.

The unemployment rates for the RPS and CPS are displayed in Figure 5a. The RPS unemployment rate is well below the CPS unemployment rate in April 2020. However, the two series are again more similar from May 2020-onward, with the RPS estimate lying 1.6 percentage points above the CPS on average since then. Both series show a steep decline in unemployment from May to November 2020, a pause in the decline from November 2020 to February 2021, and a resumption of the decline in March 2021. Since the fall of 2020, the higher unemployment rate in the RPS was primarily due to a higher labor force participation rate (see Figure 5b).

We propose two potential reasons why an online survey like the RPS may report a somewhat higher unemployment rate and labor force participation rate than the CPS, despite having similar employment rates. First, 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. This deviation avoids respondents having to type a text response (which we have noted increases response time and lowers response rates) and eliminates the need to manually code responses. However, it is

possible that individuals are more likely to select activities which qualify as “active search” when they are presented with options rather than when they are required to recall and state these activities themselves. Second, [Ahn and Hamilton \(2021\)](#) point out that the unemployment rate among individuals in their final CPS interviews is typically 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 not searching for work. Either effect would bias the overall unemployment rate downwards, which should not be present in a one-shot survey like the RPS. In [Appendix B](#) we compare estimates in the RPS and CPS by interview month and find that panel-induced biases in the CPS could potentially account for a substantial share of the differences between the RPS and CPS. For example, from June 2020 through June 2021, the unemployment rate in CPS first-month interviews was on average 0.59 percentage points higher than the unemployment rate in all interview months, accounting for 39% of the average difference between the RPS and CPS unemployment rates.

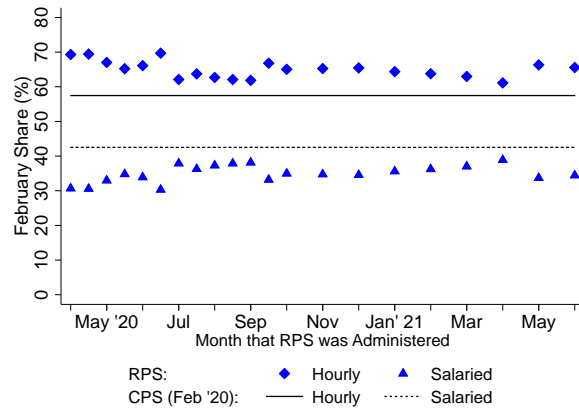
Figure [6a](#) plots the share of workers who were paid hourly in February 2020. Averaging across all RPS survey waves, 65.0% of workers report being paid hourly in February 2020, versus 57.5% in the February 2020 CPS. Figure [6b](#) plots the same statistic during the pandemic. These shares were fairly stable over the sample period: on average, 64.7% of workers were hourly in the RPS compared to 54.6% in the CPS.

Figure [6c](#) displays the distribution of usual weekly earnings in February 2020.¹⁴ 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 log earnings are 2-5 log points larger and the log variance is 6-12 log points lower in the PSID than in the CPS-ASEC, respectively. Figure [6d](#) displays the distribution of usual weekly earnings at the previous week’s main job. Prior to August 2020, the RPS asked workers still working for their February 2020 employer only about earnings changes relative to before the pandemic, which makes it difficult to compare to earnings levels in the CPS. Beginning in August, however, the RPS began

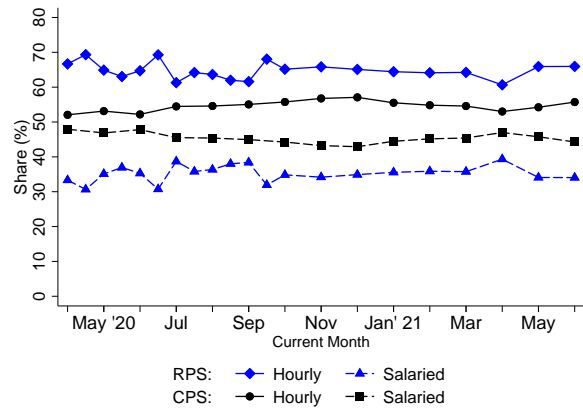
¹⁴To ensure comparability and minimize concerns over measurement error, we restrict both the RPS and CPS samples to individuals with (i) weekly earnings below the CPS topcode of \$2,884.61, (ii) an implied hourly wage exceeding half the federal minimum wage of \$3.62, and (iii) no business in the household (Figure [C.2a](#) in [Appendix C](#) 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 5.7%, 7.4%, and 15.5%.

Figure 6: Pay Type and Usual Weekly Earnings in the RPS and CPS, Age 18-64

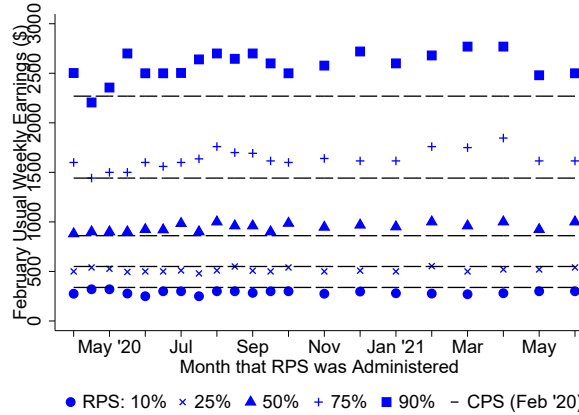
(a) February 2020 Pay Type: Hourly vs. Salaried



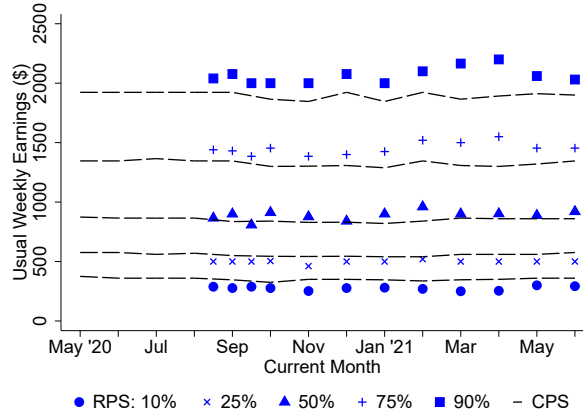
(b) Last Week Pay Type: Hourly vs. Salaried



(c) February 2020 Percentiles of Weekly Earnings



(d) Last Week Percentiles of Weekly Earnings



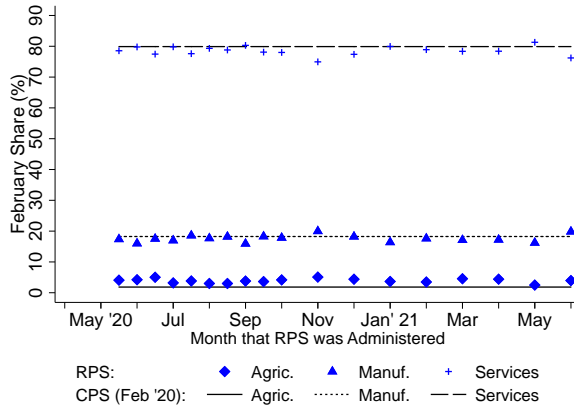
Notes: Panels (a) and (c) display data from the February 2020 CPS and from retrospective questions regarding February 2020 for each wave of the RPS. Panels (b) and (d) display data on outcomes “last week” from May 2020 - June 2021 waves of the RPS and CPS. Earnings, which were only collected consistently in the RPS beginning in August 2020 are the 10th, 25th, 50th, 75th, and 90th percentiles.

asking all workers about their current usual earnings, following the same methodology as in the CPS. As with the retrospective question about February 2020 earnings, the distribution is similar between surveys, with somewhat larger dispersion in the RPS.

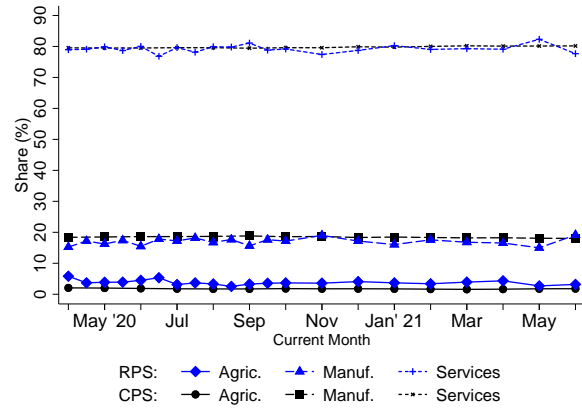
Figure 7a displays the sectoral composition in February 2020 in the RPS and CPS. The two surveys display nearly identical sectoral compositions for February 2020, and the composition hardly varies across RPS waves. Figure 7b displays the sectoral composition during the pandemic; again, we find very similar shares in both datasets. Figure 7c provides a more detailed comparison of the composition across 18 major industries in February 2020. The correlation between RPS and CPS industry shares is 0.79. The industries in which the RPS most undershoots the CPS are professional and business services (8.0% vs. 12.1% in the RPS and CPS, respectively) and health services

Figure 7: Sector and Industry Composition in the RPS and CPS, Age 18-64

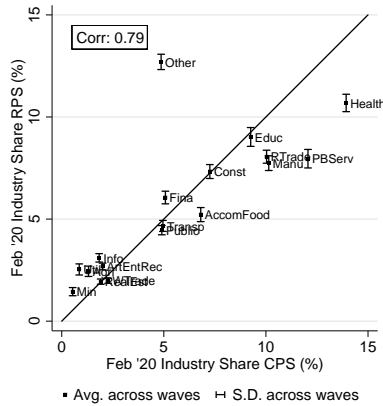
(a) February 2020 Sectoral Composition



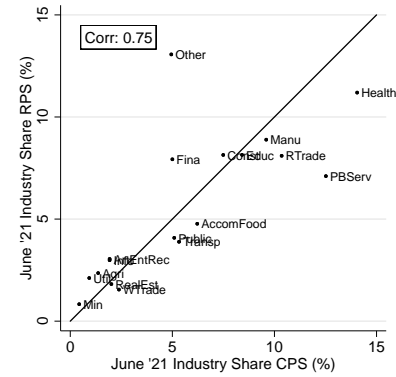
(b) Last Week Sectoral Composition



(c) February 2020 Industry Composition



(d) June 2021 Industry Composition

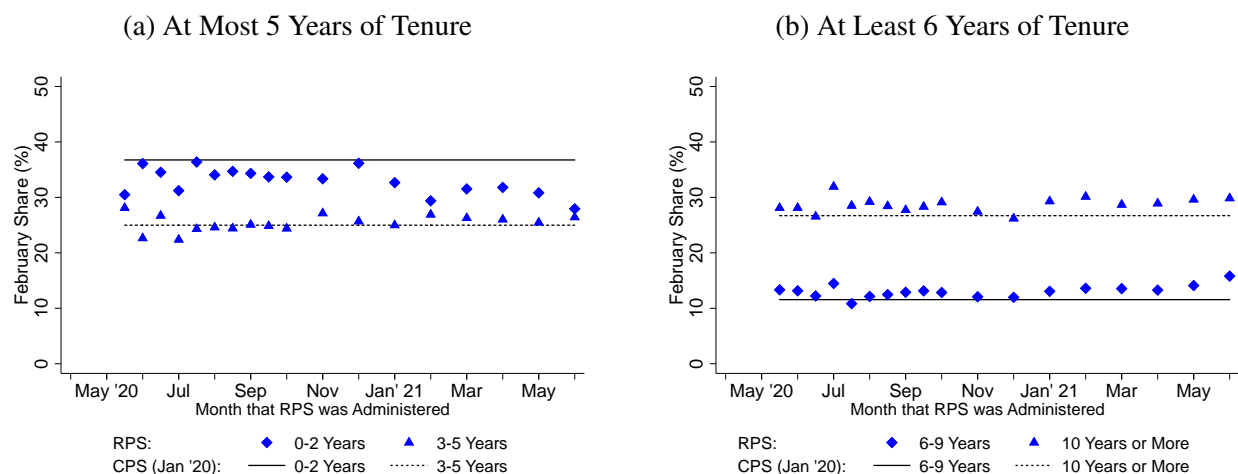


Notes: Panels (a) and (c) display data from the February 2020 CPS and from retrospective questions regarding February 2020 for each wave of the RPS. Panels (b) and (d) display data on outcomes from the June 2021 wave of the RPS and CPS. The RPS only collected information on sector and industry for February 2020 consistently from mid May 2020 onward. Comparisons for sectoral and industry composition between the RPS and CPS for the other months May 2020 - May 2021 are in Appendix C.2.

(10.7% vs. 13.9%); the largest RPS overshoot relative to the CPS is in “other services” (12.7% vs. 4.9%). These disparities may be attributable to some individuals in the service sector not knowing which industry to select, leading to under-counts in particular service industries and an over-count in the “other services” industry. Figure 7d compares industry shares in June 2021, the final month of the RPS. We find a similar correlation of 0.75 between industry shares in the RPS and CPS, again with the largest disparities coming from professional and business services, health services, and other services. (For the other months during our sample see Appendix C.2.)

Figure 8 displays the distribution of employer tenure just before the COVID-19 pandemic in the CPS and RPS. While the CPS does not ask about employer tenure in its core module, it does

Figure 8: Pre-Pandemic Tenure Distribution in the RPS and CPS, Age 18-64



Notes: Figure contains data from retrospective questions for February 2020 starting with the late May 2020 RPS (wave 5, the first time we asked about job tenure), alongside data from the January 2020 tenure supplement to the CPS. In the RPS, information on February 2020 tenure is based on current tenure in the reference week for those holding the same job as in February 2020 and for tenure in February 2020 for anyone else employed in February 2020. The RPS only asks individuals who are neither self-employed nor unpaid family workers about their tenure, and the CPS sample is restricted accordingly. Tenure information is missing for 16.8% of workers in the CPS and for 0.23% in the RPS.

conduct a specialized tenure supplement once every two years. The most recent tenure supplement prior to the RPS sampling period was in January 2020. The RPS collects information about employer tenure in February 2020, which provides a close comparison point. For individuals employed in February 2020 who had separated from this employer by their RPS interview, we ask about employer tenure as of February 2020. For individuals who were still working for the same employer as in February 2020 at the time of their interview, we ask about current employer tenure, and treat this as an estimate of February 2020 tenure. Of course, for later survey months this estimate will be less accurate and will systematically bias RPS results towards lower shares of workers with short tenure.

Overall, Figure 8 shows that the two surveys display a similar distribution of employer tenure just before the pandemic. Due to the bias discussed in the previous paragraph, the RPS share of workers with short tenure declines during the last several months of the survey. However, averaging across RPS waves in all months of 2020, the share of workers with 0-2 years of pre-pandemic tenure was 32.9% in the RPS versus with 36.7% in the CPS; the share with 3-5 years was 25.3% in the RPS versus 25.0% in the CPS; the share with 6-9 years was 13.1% in the RPS versus 11.6% in the CPS; and the share with 10 or more years was 28.7% in the RPS versus 26.7% in the CPS. We conclude that the RPS contains a similar distribution of pre-pandemic employer tenure to the CPS.

Collectively, we view the results in this section 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 with a reference period of “last week.” Second, retrospective questions about February 2020 in the RPS are also broadly consistent with outcomes in the February 2020 CPS. The second result suggests that, at least in the present case, it is feasible to obtain (quasi-) panel information even in a single-shot survey like the RPS. This quasi-panel information will be essential for our analysis of employer separations and reallocation in Section 5.

5 Application: Employer Separations and Reallocation During the COVID-19 Pandemic

The decline in employment in the spring of 2020 represents the largest rate of job loss in the US since the CPS began collecting data in 1940. Yet by March 2021, a year into the pandemic, employment among working age individuals had recovered roughly three quarters of its initial losses. One possibility is that many workers who lost employment early in the pandemic simply returned to their old employers over the course of the year, in which case the labor market disruption caused by the pandemic may have been a violent yet short-lived phenomenon with relatively modest long-run impact. An alternative possibility is that much of the employment recovery was the result of workers finding new employers. In this case, shocks in the initial months of the pandemic may have permanently destroyed a large share of employer-worker matches, many of which may have been highly productive. Because highly productive matches are costly to find (Jovanovic, 1979, 1984; Moscarini, 2005; Menzio and Shi, 2011; Fujita and Moscarini, 2017; Gregory et al., 2020), the economic disruption induced by COVID-19 may have induced persistent reductions in productivity and employment as separated workers are forced to search for new productive and stable matches (Fujita et al., 2020).

To assess these alternative scenarios, this section documents employer separations and related outcomes through the first year of the pandemic March 2021. We document outcomes both overall and conditioning on pre-pandemic employer tenure, which theory suggests is a proxy for the expected productivity of an employer-worker match. Our analysis primarily relies on the RPS because the information in the CPS related to employer tenure is insufficient for our purposes. Recall that the CPS follows individuals for four consecutive months (interviews 1 - 4), then pauses interviews for eight months, and then interviews them for four more consecutive months (interviews 5 - 8). While the CPS does ask about employer changes in interviews 2 - 4 and again in interviews 6 - 8, it does not ask about employer changes during the eight months between interviews 4 and 5, which means the core CPS cannot speak to spells of employer tenure longer than three months at a time.¹⁵

¹⁵An additional limitation of CPS data is that in recent years information on employer changes in interviews 2 - 4 and 6 - 8 is missing for about 9% of workers (Fujita et al., 2021). Yet another limitation is that this question is not

5.1 Aggregate Rates of Employer Separation and Reallocation

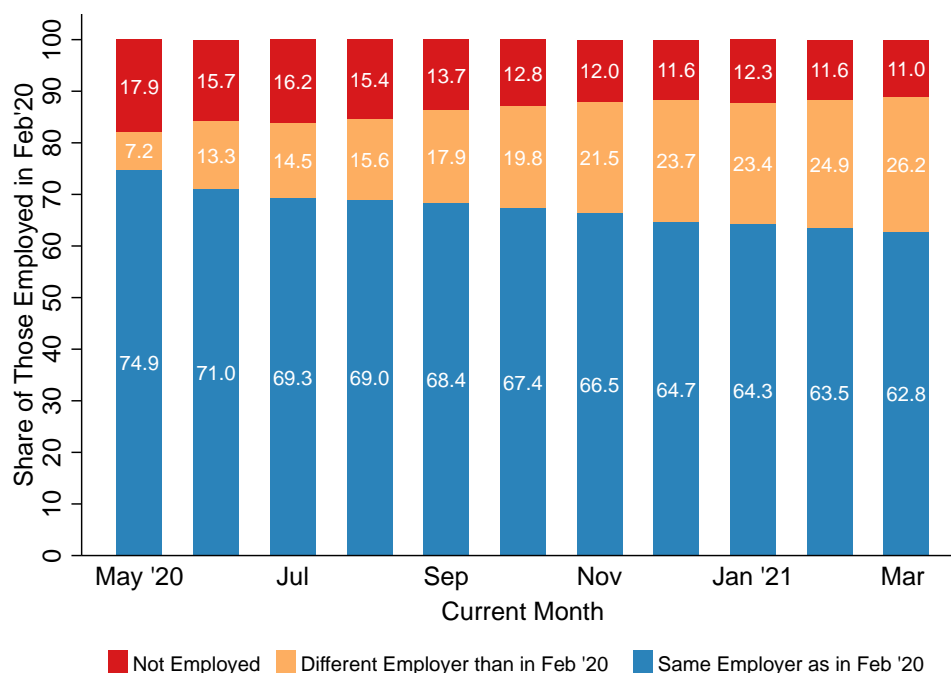
To begin, Figure 9 shows that the disruption caused by the COVID-19 pandemic did indeed result in a large rate of persistent employer-worker separations. Our sample is all individuals who report being employed in February 2020, just before the outbreak of the pandemic in the US. We partition this sample into three main categories: (i) those who are still working for the February 2020 employer, (ii) those working for a new employer, and (iii) those currently not employed. In May 2020, 25.1% of workers had separated from their February 2020 employer, either because they were no longer employed (17.9%) or because they were working for a new employer (7.2%). The separation rate continued to increase in the ensuing months, reaching 37.2% in March 2021, roughly one year out from the onset of the pandemic. At the same time, employment recovered rapidly, driven entirely (in net terms) by a growing share working for a new employer; by March 2021, 26.2% of those employed just before the pandemic were working for a new employer.

Before putting the separation rates in Figure 9 into historical context, we briefly pause to validate one piece of that figure. While the CPS cannot speak to one-year separation rates during the pandemic, we can use the panel component of the CPS to observe the fraction of February 2020 workers who were also employed one year later, in February 2021. In the CPS, 88.8% of those employed in February 2020 were employed in February 2021, compared to 89.7% in the RPS (this is the sum of the bottom two shades for February 2021 in Figure 9), with a 95% confidence interval of 88.5% to 90.8%. We conclude that the two surveys deliver a consistent message with respect to the transition rate to non-employment for individuals employed just prior to the pandemic.

It is well known that, even in normal times, the US labor market exhibits substantial “churn,” with a large share of workers moving in and out of the labor force and across employers (Farber, 1999). Was the reallocation during the first year of the pandemic documented in Figure 9 unusually large? To answer this question, we compare one-year separation rates in the 2020-2021 RPS to CPS tenure supplements over the previous two decades. We construct separation rates in the CPS as follows, focusing on the year 2020 to fix ideas. CPS respondents whose 5th - 8th CPS interviews took place in January 2020 took part in the 2020 tenure supplement, and were also observed one year earlier, in January 2019, during their 1st - 4th interviews. We can therefore compute the share of individuals employed in January 2019 who one year later were (i) working for the same employer,

asked of individuals who are non-employed in between two employment spells, so employer recalls cannot be observed. Finally, attempts to partially impute employer changes between interviews 4 - 5 using changes in occupation/industry between interviews 4 and 5 are problematic due to three limitations: (i) they will not capture employer moves within industry/occupation, (ii) they may mistakenly assign employer changes if a change in job tasks (e.g. a promotion) is recorded as a change in occupation, and (iii) there is substantial measurement error in industry and occupations, which results in many erroneous changes in these variables (Moscarini and Thomsson, 2007).

Figure 9: Rates of Separation from Pre-Pandemic Employers: May 2020–Mar 2021



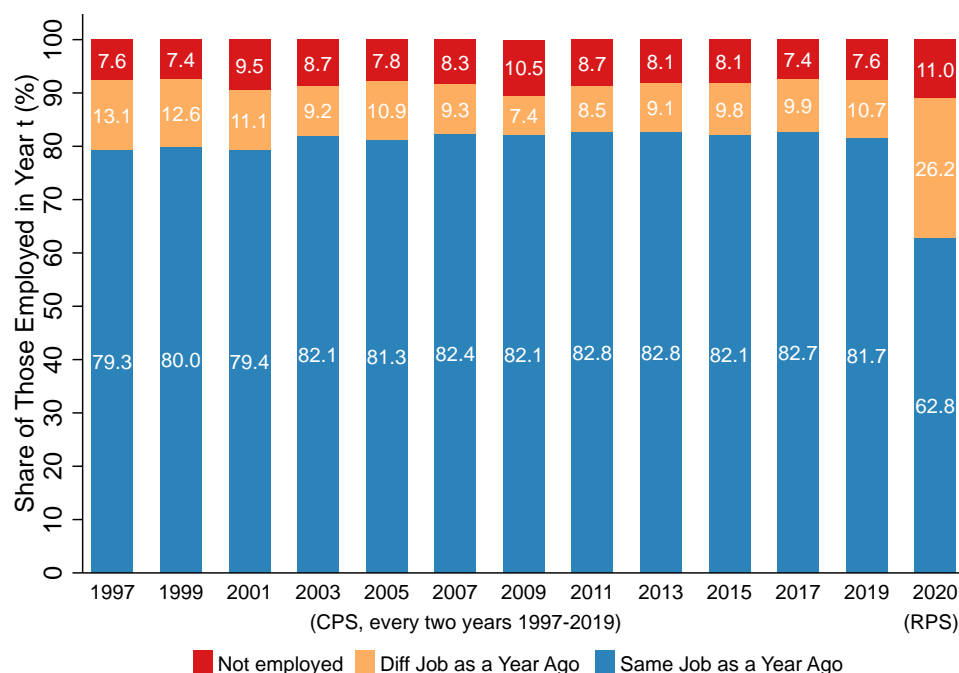
Notes: Figure contains data from the May 2020 - Mar 2021 waves of the RPS. Appendix Figure D.1 extends the data through June 2021. We replace data from February 2021 with the average from January and March 2021 out of concern that some respondents confused February 2020 with February 2021. Sample is everyone employed in February 2020. For employed individuals, we assign “same” versus “different” employer as in February 2020 based on the following question: *When did you start working for this employer (or for yourself if you are self-employed)? If you had any brief interruptions, like a temporary layoff or unpaid leave, please report when you FIRST started working for this employer.* The first answer option is *February 2020 or earlier* followed by each month since then (*March 2020, April 2020, ...*) On average across all waves, 1.2% individuals employed in both February 2020 and the reference week did not answer this question. For those individuals, we impute this status via “missing at random.” Fujita et al. (2021) show that this procedure underestimates the share of employer switches between two subsequent months in the CPS suggesting that our assumption puts a lower bound on the rate of employer switches.

(ii) working for a new employer, or (iii) not employed. Note that because the tenure supplement only runs in even years, and the CPS only tracks individuals for 15 months, these separation rates are only available starting in odd years (e.g., the separation rate for workers employed in 2019, 2017, etc.). Therefore, the January 2022 CPS tenure supplement cannot speak to separation rates during the first year of the pandemic, which began in March 2020 in the US.

Figure 10 displays the results of the above computation for odd years 1997-2019, which make use of CPS tenure supplements for even years 1998-2020, alongside the analogous computation for the RPS (the RPS bars in Figure 10 are equivalent to the March 2021 bar in Figure 9).¹⁶ The figure

¹⁶We start with the 1998 CPS tenure supplement because the 1996 supplement cannot be linked to the 1995 January CPS due to changes in the system for numbering households in the CPS.

Figure 10: One-Year Employer Separation Rates: 1997–2021



Notes: Figure contains data from the 1997-2019 core CPS, the 1998-2020 CPS employer tenure supplement, and the March 2021 wave of the RPS. The sample is all individuals employed in January (or, depending on the year, February) of year t , and who are observed one year later. See text for details on how we construct the CPS statistics. For RPS statistics, see notes to Figure 9. In both data sets, we impute missing “same employer / different employer” statuses via missing-at-random. (On average, this status is missing for 11.8% of individuals in the 1997 - 2019 CPS and 0.9% of individuals in the March 2021 RPS.) [Fujita et al. \(2021\)](#) argue that this procedure underestimates the share of employer switches between two subsequent months in the CPS, suggesting that our assumption puts a lower bound on the rate of employer switches.

makes clear that employer separations were much higher during the pandemic than any other year since at least the late 1990s. Specifically, the average one-year separation rate from 1997-2019 was 18.7%, compared to 37.2% during the pandemic. Notably, the one-year rate of separation-to-non-employment during the pandemic stands out less by historical comparison and is essentially on par with the rate in 2009 during the Great Recession. By contrast, the most striking difference during the pandemic was that the share working for a new employer was 26.2%, twice as large as the next highest rate of 13.1% in 1997.

To summarize, the one-year separation rate in the first year of the COVID-19 pandemic was quite high by recent historical comparisons. Coupled with the rapid recovery in employment that occurred from May 2020-March 2021, this implies that roughly one in four employed individuals just before the pandemic was working for a new employer one year later, which is double the rate of employer reallocation that occurred in the US over the past quarter century.

Figure 11: Separation and Re-employment One Year into COVID-19, by Employer Tenure



5.2 Employer Tenure and Labor Market Outcomes During the Pandemic

Not all employer-worker matches are equally productive. Through the lens of matching models based on Jovanovic (1979, 1984), employers and workers agree to enter into an employment contract before fully knowing the productivity of the match. Over time, both sides learn about the match’s productivity, with unproductive matches tending to dissolve and productive matches tending to endure. This implies that the loss of productive capacity from separations are heterogeneous and that the loss in aggregate productive capacity from a given quantity of separations depends on the average productivity of destroyed matches (Fujita and Moscarini, 2017; Gregory et al., 2020; Fujita et al., 2020).

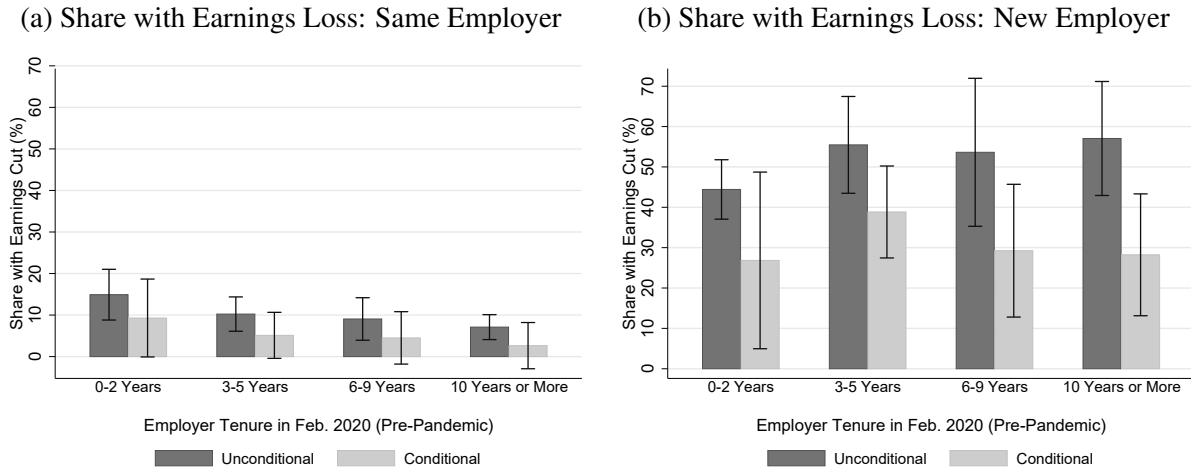
The above argument suggests that employer tenure is a useful proxy for a match’s expected productivity and that looking at the incidence of separations by pre-pandemic employer tenure can shed light on the aggregate loss in productive capacity due to the COVID-19 shock. Analyzing employer separations separately by pre-pandemic tenure requires observing employer tenure at two points: once in the base period, to classify workers by pre-pandemic tenure, and once in the later period, to identify separations. As before, the CPS is not a viable option because it only tracks individuals for up to 15 months and only runs the tenure supplement every two years. We therefore again rely on the RPS to provide this information.

Figure 11a displays the separation rate of workers from their February 2020 employers one year into the pandemic (this includes both workers who are no longer employed and those who are working for a new employer relative to February). Recall from Figure 9 that the overall separation rate among February 2020 workers was 37.2%. Strikingly, Figure 11a shows that this rate was 60.7% for workers who had 0-2 years of tenure before the pandemic (dark gray bars). The separation rate declines monotonically with pre-pandemic tenure, falling to only 16.2% for workers with tenure of 10 years or more. Even the latter rate is quite high, implying that roughly one in six workers with ten years of tenure or more separated from their employers. Nevertheless, the results suggest that the loss in aggregate productive capacity was lower than a hypothetical scenario in which separations were uniform by employer tenure. Figure 11a also shows that a quantitatively similar pattern holds for conditional separation rates after controlling for worker sex, age, education, race, and pre-pandemic industry (light gray bars; for full regression results see Appendix D.2).

The finding that separation rates decline with firm tenure are in line with a large literature studying job displacements induced by firm-level mass layoffs. Davis and von Wachter (2011) find that men with 3-5 years of tenure are about twice as likely to be displaced as men with 6 or more years of tenure. Similarly, in the RPS data 33.0% of workers with 3-5 years of tenure separated from their employer one year into the pandemic, compared with 18.3% of workers with 6 or more years of tenure, a ratio of 1.80.

Although the probability of separation is strongly decreasing in pre-pandemic employer tenure, Figure 11b shows that, conditional on separation, the probability of non-employment is *increasing* in pre-pandemic tenure. That is, low-tenure workers were more likely to separate from their employers during the pandemic, but *conditional on separating* they were also more likely to have matched with a new employer one year into the pandemic. For example, the dark gray bars in Figure 11b show that, conditional on an employer separation, only 24.7% of workers with 0-2 years of pre-pandemic tenure were not employed one year into the pandemic, compared with 44.2% of workers with at least 10 years of pre-pandemic tenure. The light gray bars indicate that a similar pattern holds after controlling for worker characteristics. Abbring et al. (2002) document that both relationships depicted in Figures 11a and 11b also hold for displaced workers in the US. One possible explanation for this pattern is that high-tenure workers remain non-employed for longer because they have higher reservation wages; Hogan (2004) finds that workers' reservation wages are increasing in their previous wage. Another possibility is that this reflects heterogeneity in the job-finding propensity of workers that is correlated with their likelihood of remaining with a given employer (Hall and Kudlyak, 2019; Gregory et al., 2021).

Figure 12: Earnings Losses One Year into COVID-19, by Employer Tenure



Notes: The Figure contains data from the March 2021 waves of the RPS. “Pre-pandemic employer tenure” refers to the worker’s tenure with that employer as of February 2020. For all panels, the sample excludes the self-employed and unpaid family workers. In panel (a), the sample is all employees working for the same employer in February 2020 and March 2021. In panel (b), the sample is all employees working for different employers in February 2020 and March 2021. Earnings losses refers to a decline in earnings of at least 10%. The “unconditional” bars reflect simple transition rates. The “conditional” bars are coefficients from a multivariate regression of transition rates (see columns (1-2) of Table D.3 in Appendix D.2). Whiskers correspond to 95% confidence intervals.

Among pre-pandemic workers in the RPS who were also employed one year into the pandemic, 21.6% experienced an earnings loss of at least 10 percent (compared with 21.7% in the CPS). The probability of experiencing an earnings loss is strongly correlated with pre-pandemic firm tenure and whether a separation occurred. Specifically, Figure 12a shows that, conditional on remaining with their pre-pandemic employer, high-tenure workers were less likely to experience an earnings loss: 14.9% of workers with 0-2 years of pre-pandemic tenure experienced an earnings loss as of March 2021, compared with 7.1% of workers with at least 10 years of tenure (dark gray bars). Figure 12b shows that, unsurprisingly, workers who changed employers were much more likely to experience an earnings loss compared with workers who remained with their pre-pandemic employer. Further, among workers who changed employers during the pandemic the negative relationship between pre-pandemic tenure and earnings losses disappears.¹⁷ These findings, though somewhat imprecise, are in line with past work on displaced workers, which has found that earnings after a displacement tend to decrease with pre-displacement firm tenure (Kletzer, 1989; Neal, 1995; Poletaev and Robinson, 2008).

¹⁷In addition to analyzing the probability of earnings losses, we also investigated whether the magnitude of earnings losses varied systematically with employer tenure. We found that, conditional on an earnings loss of at least 10%, the average loss in earnings was smaller for employer-stayers (-39%) than for employer-switchers (-46%); however, these differences were not statistically significant and we did not observe systematic patterns in mean earnings losses by pre-pandemic employer tenure.

Finally, we leverage novel information in the RPS to assess earnings outcomes for workers who were temporarily separated from their pre-pandemic employer, but then later recalled. A salient feature of the labor market at the onset of the pandemic was that the vast majority of the unemployed were labeled temporary layoffs, which existing research has shown are more likely to be recalled by their previous employers than other unemployed workers (Fujita and Moscarini, 2017). An important open question, however, is whether workers were recalled to their previous level of earnings. For example, firms may find it easier to cut the earnings of workers who have been laid off, or may only be able to recall laid off workers if they are willing to accept lower earnings. Because the CPS does not contain information on worker recalls, we again rely on the RPS for our analysis.¹⁸

Among individuals in the RPS who were employed both in February 2020 and one year later in March 2021, 15.8 percent had experienced an employer recall. Expressed as a percentage of individuals who had experienced either a temporary or permanent separation from their pre-pandemic employer, 34.4% of workers who experienced some kind of employer separation during the first year of the pandemic were recalled by their employer. To assess the relationship between recall and changes in earnings, column (3) of Table D.3 adds a dummy variable for employer recall to the set of controls for worker characteristics and industry. The results show that recalled workers were less likely to experience an earnings loss than workers who transitioned to a new employer, but more likely to experience an earnings loss than workers who continuously worked for their pre-pandemic employer throughout the pandemic. For example, using as a reference group a male 30-49 years old with some college and 0-2 years of pre-pandemic employer tenure, the predicted probability of an earnings loss was 6.8% for a continuously employed worker, $6.8 + 13.4 = 20.2\%$ for a recalled worker, and 26.8% for a worker who switched to a new employer. These results are consistent with past findings that, conditional on an employer separation, recalled workers experience higher wage outcomes at rehiring than workers who switch employers (Katz and Meyer, 1990).

In summary, the RPS data reveal that slightly more than one third of workers separated from their pre-pandemic employer in the first year of the pandemic. While the rate of transition to non-employment was not unprecedented, the rate of reallocation across employers was at least twice as high as in any other time in the past quarter century, consistent with predictions that the COVID-19 pandemic would produce large reallocation effects (Barrero et al., 2020). Separations were most

¹⁸In 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. The CPS asks workers whether they expect to be recalled in the future in order to assign “temporary layoff” status. However, the CPS does not ask whether an employer recall actually occurred. By contrast, the RPS directly asks about employer recall.

common among workers with short employer tenure, suggesting that the average level of match capital destroyed by separations may have been low. Consistent with this notion, over three quarters of short-tenure workers who experienced separations were re-employed one year into the pandemic, and a majority of these workers report earning at least as much as they did one year ago, just before the pandemic. At the same time, roughly one in eight separations that occurred were among matches that had lasted at least a decade, which theory suggests were highly productive matches on average. Collectively, the findings throughout this section are informative for the calibration and validation of quantitative labor market models of the pandemic, especially models which emphasize the importance of heterogeneity in match-specific productivity ([Menzio and Shi, 2011](#); [Fujita and Moscarini, 2017](#); [Gregory et al., 2020, 2021](#); [Goensch et al., 2021](#)).

6 Concluding Remarks and Additional Applications

This paper evaluates data from the RPS, a national online labor market survey fielded at least once per month throughout the first 16 months of the COVID-19 pandemic in the US. To address concerns over data quality and comparability, the RPS replicates key portions of an existing high-quality survey, the CPS, which allows us to weight and validate RPS data using a trusted and widely used benchmark survey. Overlapping questions reveal that the distribution of a host of labor market variables in the RPS are similar to the CPS, both before and during the pandemic, which we take as direct evidence of the reliability of RPS data. At the same time, the RPS allows us to quickly collect novel data not available from other sources. In particular, questions on employer tenure in the RPS reveal that just over one third of US workers experienced an employer separation during the first year of the pandemic. A large majority of separated workers were working for a new employer one year into the pandemic, implying a higher rate of reallocation across employers than at any other point in the past quarter century.

The scaffolding approach used to design the RPS can be applied to a wide array of research questions beyond those in the current paper. We conclude by discussing several promising examples.

An additional application of RPS data is [Bick et al. \(2022\)](#), which leverages novel questions about commuting and remote work just prior to and during the COVID-19 pandemic. We find that the aggregate commuting patterns in the RPS closely align with high-frequency commuting behavior from cellphone data. At the same time, the RPS micro data contains valuable information on demographics, labor market outcomes, and individual level dynamics which are not available in the cell phone data. This data provides key facts to help understand the nature of the labor market

disruptions caused by the pandemic, including both quantitative models of the short-run economic crisis, e.g. [Baqaee et al. \(2020\)](#), and longer-run effects such as permanent changes in remote work.

Other potential applications could ask questions not contained in the RPS. First, in addition to the ability to work remotely, jobs vary widely in their provision of other non-wage amenities. Amid rising earnings inequality in recent decades, an open question is whether heterogeneity in these amenities exacerbates or dampens the inequality in the overall return to work ([Kaplan and Schulhofer-Wohl, 2018](#)). An online survey built around the CPS could help address this issue by including supplemental questions on access to a workplace office, gym, or company car; flexible versus inflexible hours; quantity of required or actual travel for work; number of paid parental leave, vacation, and sick days; and non-wage compensation such as stock options and employer contributions to pensions. To facilitate historical comparisons, a subset of these questions could be borrowed from the CPS work schedule supplement, which has not been run since 2004, and could be benchmarked against the National Compensation Survey, which is an establishment survey.

Another topic broadly related to better understanding work arrangements is the recent rise in “gig work” for services related to ride-sharing (e.g. Uber, Lyft), food-delivery (e.g. Uber Eats, DoorDash, Instacart), or odd jobs (e.g. TaskRabbit). The CPS has occasionally run a contingent worker supplement, most recently in 2017, which may capture a subset of this work; however, these questions are only directed towards a worker’s main job and therefore do not capture supplemental or occasional gig work ([Abraham et al., 2019](#)). An online survey built around the CPS could include supplemental questions on gig work to help answer questions related to which types of individuals participate in gig work, what share of income is provided by gig work, how much gig work is not captured by the CPS, and to what extent gig work helps insure workers against shocks, such as the loss of a job or an unexpected expense. These findings could complement and be benchmarked against recent analyses of administrative data by, e.g. [Collins et al. \(2019\)](#) and [Abraham et al. \(2021\)](#).

Finally, our approach could also help address specific policy-related questions for which no nation-wide data are available. To give one specific example, the optimal design of taxes and subsidies on renewable energy products, such as electric vehicles and solar panels, will depend on the characteristics of individuals who own these products, especially the incidence by age and income. Questions related to this issue would be straightforward to include in an online survey built around the CPS or ACS.

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Appendix (For Online Publication Only)

A 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.

A.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 use 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 because we did not consistently collect data on (i) spouses and partners living in the household or (ii) employment in February.

Table A.1 provides an overview for each survey. The first column reports the reference weeks (recall that the CPS always references the week containing the twelfth day of the month). The surveys always started fielding on the Monday after the reference week ended, with the exception of wave 17 (January 2021), which started fielding on the Tuesday after the respective reference week ended. The full data collection was completed within two to three days. The only exception is wave 6 (early June 2020). 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 to 2,200 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 is roughly 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 A.3. Each parallel survey aimed for 1,600 final responses. Columns 6 through 8 in Table A.1 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 8 minutes and 29 seconds in wave 19. This increase results from a) an increase in employment (employed individuals are asked more questions) and b) the addition of new questions. With only a few excep-

Table A.1: Overview for Each Survey Wave

| Wave | Dates | | Respondents | Valid Observations | | Survey Duration in Seconds | | |
|------|----------------|-----------------|-------------|--------------------|-----------|----------------------------|--------|------------|
| | Reference Week | Survey Fielding | | Feb' 2020 | Ref. Week | 25th Perc. | Median | 75th Perc. |
| 2020 | | | | | | | | |
| 2 | 04/12-04/18 | 04/22-04/23 | 1541 | 2485 | 2491 | 210 | 299 | 438 |
| 3 | 04/26-05/02 | 05/04-05/06 | 1600 | 2498 | 2501 | 210 | 311 | 466.5 |
| 4 | 05/10-05/16 | 05/18-05/19 | 1558 | 2458 | 2462 | 219 | 307.5 | 433 |
| 5 | 05/24-05/30 | 06/01-06/02 | 1513 | 2414 | 2414 | 237 | 335 | 486 |
| 6 | 06/07-06/13 | 06/14-06/17 | 3927 | 6167 | 6170 | 251 | 357 | 511 |
| 7 | 06/21-06/27 | 06/29-07/01 | 1964 | 3112 | 3113 | 246 | 344.5 | 503 |
| 8 | 07/12-07/18 | 07/20-07/22 | 3117 | 4957 | 4964 | 269 | 385 | 552 |
| 9 | 07/26-08/01 | 08/03-08/04 | 2016 | 3199 | 3202 | 267.5 | 376 | 535 |
| 10 | 08/09-08/15 | 08/17-08/18 | 2110 | 3370 | 3372 | 270 | 381 | 548 |
| 11 | 08/23-08/29 | 08/31-09/01 | 2048 | 3240 | 3244 | 267 | 385 | 562 |
| 12 | 09/06-09/12 | 09/14-09/15 | 3241 | 5168 | 5173 | 273 | 398 | 598 |
| 13 | 09/23-10/03 | 10/05-10/07 | 2024 | 3199 | 3200 | 268 | 398 | 576 |
| 14 | 10/11-10/17 | 10/19-10/20 | 2045 | 3292 | 3294 | 284 | 406 | 604 |
| 15 | 11/08-11/14 | 11/16-11/18 | 2193 | 3625 | 3626 | 294 | 419 | 622 |
| 16 | 12/06-12/11 | 12/14-12/17 | 2244 | 3562 | 3565 | 296 | 427 | 623 |
| 2021 | | | | | | | | |
| 17 | 01/10-01/16 | 01/19-01/21 | 2248 | 3565 | 3569 | 310 | 453.5 | 652.5 |
| 18 | 02/07-02/13 | 02/15-02/16 | 2246 | 3580 | 3581 | 302 | 439.5 | 638 |
| 19 | 03/07-03/13 | 03/15-03/16 | 2247 | 3555 | 3561 | 334 | 509 | 753 |
| 20 | 04/11-04/17 | 04/19-04/20 | 2047 | 3285 | 3285 | 319 | 472 | 703 |
| 21 | 05/09-05/15 | 05/17-05/19 | 2041 | 3261 | 3265 | 318 | 471 | 695 |
| 22 | 06/06-06/12 | 06/14-06/16 | 2044 | 3297 | 3298 | 315 | 467 | 682 |

tions these questions were added at the end of the survey, which helped maintain consistency in the core sequence of questions 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 a 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 A.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 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.

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

| | CPS | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | RPS Wave | | | | 12 | 13 | 14 | 15 | 16 | 17 | 18 | 19 | 20 | 21 | 22 |
|---------------------------------|------|------|------|------|------|------|------|------|------|------|------|----------|------|------|------|------|------|------|------|------|------|------|------|------|------|----|
| <i>Gender</i> | | | | | | | | | | | | | | | | | | | | | | | | | | |
| 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 | 47.7 | 49.1 | 49.0 | 48.9 | 49.0 | 49.0 | 48.9 | 49.0 | 49.0 | 48.9 | 49.1 | |
| 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 | 52.3 | 50.9 | 51.0 | 51.1 | 51.0 | 51.0 | 51.0 | 51.0 | 51.0 | 51.1 | 50.9 | |
| <i>Age</i> | | | | | | | | | | | | | | | | | | | | | | | | | | |
| 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 | 15.4 | 15.7 | 15.5 | 14.9 | 14.9 | 14.8 | 15.9 | 15.7 | | | | |
| 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 | 25.0 | 25.3 | 26.2 | 22.9 | 23.5 | 24.9 | 24.4 | 28.4 | | | | |
| 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 | 22.9 | 21.4 | 24.1 | 22.3 | 23.3 | 22.9 | 24.4 | 25.2 | | | | |
| 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 | 16.3 | 15.7 | 16.0 | 18.1 | 16.2 | 17.5 | 16.1 | 14.7 | | | | |
| 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 | 20.2 | 21.8 | 18.2 | 21.8 | 22.2 | 19.9 | 19.2 | 16.0 | | | | |
| <i>Race/Ethnicity</i> | | | | | | | | | | | | | | | | | | | | | | | | | | |
| 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 | 56.5 | 57.1 | 55.5 | 59.0 | 65.1 | 60.4 | 60.4 | 57.5 | | | | |
| 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 | 13.8 | 13.8 | 13.9 | 12.8 | 11.8 | 13.3 | 12.0 | 12.9 | | | | |
| 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 | 19.4 | 19.3 | 19.0 | 18.5 | 13.3 | 16.0 | 18.1 | 19.4 | | | | |
| 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 | 10.3 | 9.8 | 11.6 | 9.6 | 9.9 | 10.3 | 9.5 | 10.2 | | | | |
| <i>Education</i> | | | | | | | | | | | | | | | | | | | | | | | | | | |
| 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 | 37.5 | 36.8 | 37.3 | 37.1 | 36.7 | 37.3 | 38.5 | 35.5 | | | | |
| 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 | 26.8 | 28.4 | 24.4 | 25.2 | 27.5 | 24.8 | 26.1 | 25.6 | | | | |
| 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 | 35.8 | 34.8 | 38.3 | 37.7 | 35.8 | 37.8 | 35.3 | 38.9 | | | | |
| <i>Marital Status</i> | | | | | | | | | | | | | | | | | | | | | | | | | | |
| 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 | 57.5 | 51.2 | 51.5 | 54.3 | 53.5 | 54.2 | 50.7 | 50.1 | | | | |
| 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 | 42.5 | 48.8 | 48.5 | 45.7 | 46.5 | 45.8 | 49.3 | 49.9 | | | | |
| <i>Number of children</i> | | | | | | | | | | | | | | | | | | | | | | | | | | |
| 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 | 58.3 | 64.4 | 63.7 | 66.0 | 65.1 | 61.6 | 63.5 | 59.6 | | | | |
| 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 | 17.0 | 15.1 | 15.5 | 14.3 | 14.8 | 16.4 | 15.2 | 16.7 | | | | |
| 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 | 17.0 | 13.3 | 13.8 | 13.2 | 13.2 | 14.5 | 14.5 | 15.9 | | | | |
| 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 | 7.6 | 7.1 | 7.0 | 6.5 | 6.8 | 7.4 | 6.8 | 7.8 | | | | |
| <i>Region</i> | | | | | | | | | | | | | | | | | | | | | | | | | | |
| 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 | 18.2 | 21.3 | 19.3 | 21.7 | 20.9 | 19.4 | 20.0 | 18.1 | | | | |
| 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 | 23.0 | 19.2 | 20.0 | 17.9 | 17.9 | 20.4 | 19.4 | 19.8 | | | | |
| 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 | 41.2 | 41.2 | 39.9 | 39.5 | 39.6 | 39.3 | 41.6 | 41.7 | | | | |
| 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 | 17.6 | 18.4 | 20.8 | 20.9 | 21.6 | 21.0 | 18.9 | 20.4 | | | | |
| <i>Household Income in 2019</i> | | | | | | | | | | | | | | | | | | | | | | | | | | |
| \$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 | 32.8 | 35.1 | 32.6 | 32.1 | 32.2 | 32.6 | 35.1 | 32.3 | | | | |
| \$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 | 29.0 | 30.6 | 29.6 | 28.6 | 27.6 | 26.8 | 30.5 | 29.2 | | | | |
| \$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 | 38.2 | 34.2 | 37.8 | 39.3 | 40.3 | 40.7 | 34.5 | 38.5 | | | | |

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.

A.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 Coronavirus on the employment situation.¹⁹ At the end of the section, we also provide details on the industry definitions.

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 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

¹⁹The CPS Interviewing Manual is available on the BLS website, as are the FAQs for [March](#), [April](#), [May](#), [June](#), [July](#), [August](#), [September](#), and [October](#).

work in the last four weeks, and (iv) would be available to work if they had been offered a job last week.

- 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 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 week 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 had 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 (4) 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 asked 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 paid for.

We also ask workers about their current usual earnings. For workers who did not work in February 2020, or who began their current job after February 2020, 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, (v) 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.

A.2.1 Industry Definitions

Industries correspond to the 18 major industries in the NAICS and are defined as follows:

- **Agri:** NAICS = 11-20. Agriculture, Forestry, Fishing and Hunting
- **Mining:** NAICS = 21. Mining, Quarrying, and Oil and Gas Extraction
- **Util:** NAICS = 22. Utilities
- **Const:** NAICS = 23. Construction
- **Manu:** NAICS = 31-33. Manufacturing
- **WTrade:** NAICS = 42. Wholesale Trade
- **RTrade:** NAICS = 44-45. Retail Trade
- **Transp:** NAICS = 48-49. Transportation and Warehousing
- **Info:** NAICS = 51. Information
- **Fina:** NAICS = 52. Finance and Insurance
- **RealEst:** NAICS = 53. Real Estate and Rental and Leasing
- **PBServ:** NAICS = 54-56. Professional, Scientific, and Technical Services and Management of Companies and Enterprises and Administrative and Support and Waste Management and Remediation Services
- **Educ:** NAICS = 61. Educational Services
- **Health:** NAICS = 62. Health Care and Social Assistance
- **ArtEntRec:** NAICS = 71. Arts, Entertainment, and Recreation
- **AccomFood:** NAICS = 72. Accommodation and Food Services
- **Other:** NAICS = 81. Other Services (except Public Administration)
- **Public:** NAICS = 99. Federal, State, and Local Government, excluding state and local schools and hospitals and the U.S. Postal Service (OES Designation)

In waves 2-4, we did not collect industry for those employed in February 2020 but who had a new job in the reference week or were not employed in the reference week. The exception are those who were on layoff in the reference week from their February job. In total, 14.5% of those employed in February 2020 have a missing industry. Starting with the late wave 5 (late May), industry for February 2020 is in principle available for everyone employed in February 2020 with only 0.4% of those not reporting their industry.

A.3 Modifications Across Survey Waves

In waves 3, 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 \(2021\)](#) 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.

A.3.1 Wave 3 Update

Beginning in wave 3, we asked workers whether they were temporarily laid off in two separate places throughout the survey, rather than just once. This resulted in a larger share of people reporting being temporarily laidoff in wave 3 relative to wave 2. Since we did not run a parallel survey that wave with the initial and new structure, we cannot not make any adjustments to wave 2.

A.3.2 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.

A.3.3 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 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 A.2](#)):

1. Employed - at work:

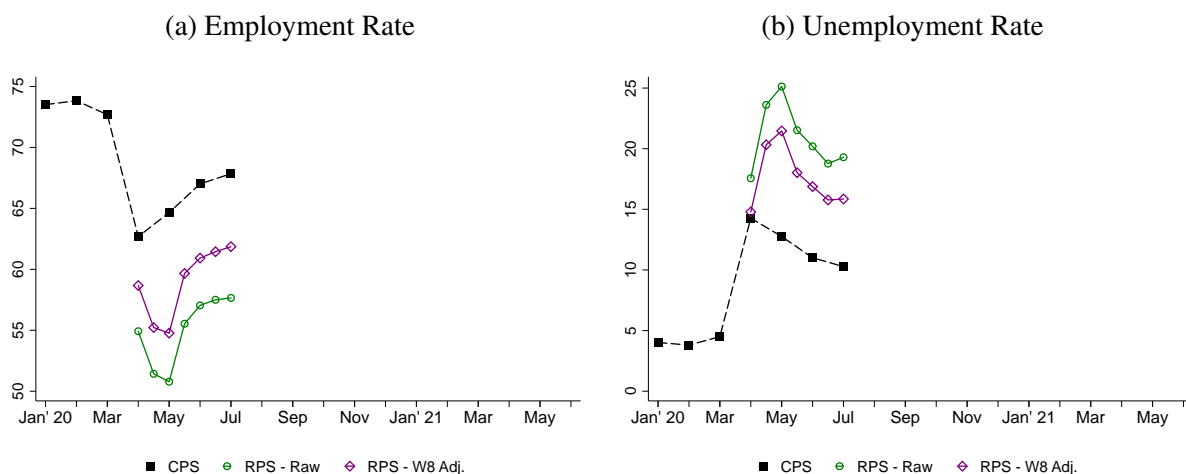
- Respondents who reported that last week they worked for pay or profit
- Respondents who worked at least 15 unpaid hours at a business owned by someone in their household.

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.

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

Figure A.1: The Effect of Wave 8 Adjustment



the two parallel surveys.²⁰ The adjustment factor is then applied to the wave 8 survey with the old methodology and all prior surveys.

Figure A.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.

A.3.4 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

²⁰These 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)

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 [A.3.2](#). 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:

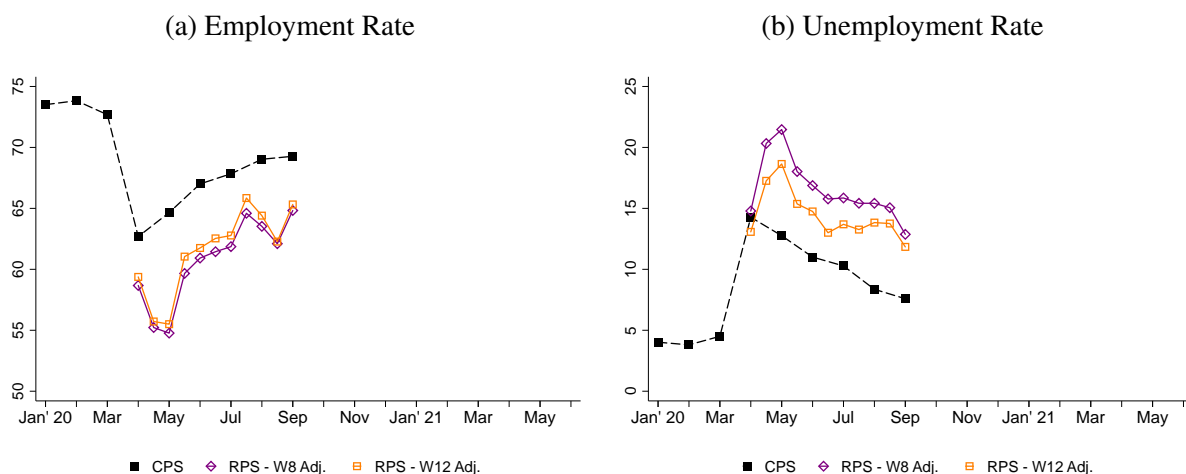
1. No work for pay or profit last week, reported having a job last week, classified as employed absent.
2. No work for pay or profit last week, reported having a job last week, classified NOT as employed absent.
3. Everyone else

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 as an additional adjustment factor in

Figure A.2: The Effect of Wave 12 Adjustment



previous waves for a given realization before applying the actual weighting scheme.

Figure A.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 A.1 but continues it through wave 12 (September). For the data points after wave 8 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. 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.

A.4 Weighting

We recommend the interested reader to first read Sections 3.3 and 4 in the main text and the accompanying Appendix A.2, where we introduce the key labor market concepts. In the following, we first provide some more details on our final weighting scheme and afterwards discuss modifications to the survey weighting scheme implemented during the release of our real-time labor market estimates over the period April 2020 through June 2021.

A.4.1 Construction of Weights

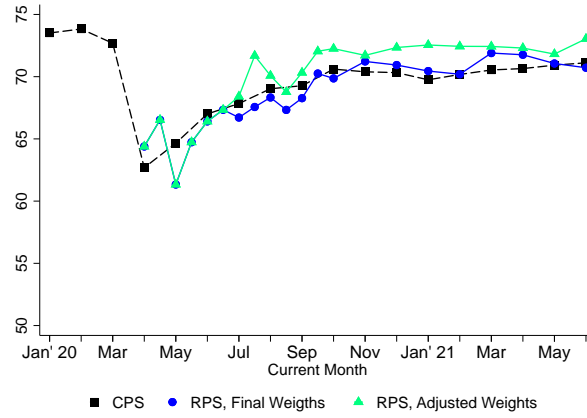
As discussed in Section 3.3 we account for negative selection into our sample based on recent employment history by first also including the employed at work status at the time of the most recent CPS (also interacted with sex, age, race, education, marital status, and region).²¹ To make this negative selection precise, on average in the RPS respondents were 3.9 percentage points less likely employed and at work at the time of the most recent CPS. Second, we also weight on the share of individuals on layoff in the previous month, again interacted with demographics.²² While we do not explicitly ask for whether someone was on layoff at the beginning of the previous months as this would involve multiple questions, we know the start month of the layoff and thus can approximate the share of individuals on layoff in the previous month.²³ It is only an approximation because it misses individuals on layoff last month, who are no longer on layoff this month (because they are either unemployed or non-employed and do not any longer report being on layoff), but in contrast includes individuals whose layoff started after the CPS reference week but in the previous month. Including these two adjustment yields the line “RPS, Final Weights” in Figure 2 and in all subsequent in the main text. This adjustment closes the gap with the CPS as well for employment, unemployment and labor force participation. As we show in Figure 4a, 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

²¹Specifically, 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, 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.

²²We use broad groups for age (18-39, 40-64), race/ethnicity (non-Hispanic White, everyone else), education (at most some college, associate’s degree in college or more), more broadly defined marital status (married + spouse present, never married, other), relationship status (spouse living in the same household, other). We resort to this smaller set of very broadly defined groups to ensure that each cell in the RPS has at least 30 observations.

²³A layoff here is defined as someone who is on layoff, whether temporarily or permanently, and whether they could have returned to work if asked or not. Put differently, these individuals can show up in any of the following labor market states: unemployed on layoff, unemployed looking, or not in the labor force.

Figure A.3: Comparing the Weighting Scheme with the Adjustment Scheme for Waves 8 Onwards for the Employment Rate, Age 18-64



the volatility of the time-series but leaves the level unchanged.

Table A.3 compares the demographic categories (without any interactions) 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 construct a weight. For example, in each survey wave a small number of respondents skipping 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 and share on layoff in the previous CPS in the weighting scheme. To visualize the goodness of fit, we plot in Figures A.4 and A.5 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.

While we consistently collected the information needed to determine the layoff status in the previous month as described above, 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 prior 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

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

| | CPS | RPS Wave | | | | | | | | | | | | | | | | | | | | |
|--|-------|----------|------|------|------|------|------|------|------|------|------|------|------|------|------|------|------|------|------|------|------|------|
| | | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 | 13 | 14 | 15 | 16 | 17 | 18 | 19 | 20 | 21 | 22 |
| Gender | | | | | | | | | | | | | | | | | | | | | | |
| Men | 49.2 | 49.2 | 49.1 | 49.3 | 49.3 | 49.3 | 49.3 | 49.3 | 49.2 | 49.2 | 49.2 | 49.2 | 49.2 | 49.2 | 49.2 | 49.2 | 49.2 | 49.2 | 49.2 | 49.3 | 49.2 | 49.2 |
| Women | 50.8 | 50.8 | 50.9 | 50.7 | 50.7 | 50.7 | 50.7 | 50.7 | 50.8 | 50.8 | 50.8 | 50.8 | 50.8 | 50.8 | 50.8 | 50.8 | 50.8 | 50.8 | 50.8 | 50.7 | 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 | 14.5 | 14.5 | 14.5 | 14.6 | 14.6 | 14.6 | 14.5 | 14.6 | 14.5 | 14.5 | 14.6 | 14.5 | 14.6 |
| 25-34 | 22.7 | 22.8 | 22.8 | 22.7 | 22.7 | 22.8 | 22.8 | 22.7 | 22.7 | 22.7 | 22.7 | 22.7 | 22.7 | 22.7 | 22.8 | 22.7 | 22.7 | 22.7 | 22.7 | 22.7 | 22.7 | 22.7 |
| 35-44 | 20.9 | 20.9 | 20.9 | 20.9 | 20.9 | 20.9 | 21.0 | 20.9 | 21.0 | 21.0 | 21.0 | 21.0 | 21.0 | 21.0 | 21.1 | 21.1 | 21.1 | 21.1 | 21.2 | 21.2 | 21.2 | 21.2 |
| 45-54 | 20.3 | 20.3 | 20.3 | 20.3 | 20.3 | 20.3 | 20.2 | 20.3 | 20.3 | 20.3 | 20.3 | 20.3 | 20.3 | 20.2 | 20.2 | 20.2 | 20.2 | 20.2 | 20.2 | 20.2 | 20.2 | 20.2 |
| 55-64 | 21.4 | 21.4 | 21.4 | 21.4 | 21.5 | 21.4 | 21.4 | 21.5 | 21.5 | 21.5 | 21.5 | 21.4 | 21.4 | 21.4 | 21.4 | 21.4 | 21.4 | 21.4 | 21.4 | 21.4 | 21.4 | 21.3 |
| Race/Ethnicity | | | | | | | | | | | | | | | | | | | | | | |
| Non-hispanic White | 59.1 | 59.1 | 59.1 | 59.1 | 59.1 | 59.1 | 59.1 | 59.0 | 59.0 | 58.9 | 58.9 | 58.9 | 58.9 | 58.9 | 58.8 | 58.7 | 58.7 | 58.8 | 58.8 | 58.8 | 58.8 | 58.8 |
| Non-hispanic Black | 12.7 | 12.7 | 12.7 | 12.7 | 12.7 | 12.7 | 12.7 | 12.7 | 12.7 | 12.7 | 12.7 | 12.7 | 12.7 | 12.7 | 12.7 | 12.8 | 12.7 | 12.8 | 12.7 | 12.7 | 12.7 | 12.7 |
| Hispanic | 18.8 | 18.8 | 18.8 | 18.9 | 18.9 | 18.9 | 18.9 | 18.9 | 18.8 | 18.9 | 18.9 | 18.9 | 19.0 | 19.0 | 19.0 | 19.0 | 19.1 | 19.1 | 19.1 | 19.1 | 19.1 | 19.2 |
| Other | 9.3 | 9.3 | 9.4 | 9.3 | 9.4 | 9.4 | 9.4 | 9.4 | 9.4 | 9.5 | 9.5 | 9.5 | 9.5 | 9.5 | 9.4 | 9.4 | 9.4 | 9.4 | 9.4 | 9.4 | 9.4 | 9.4 |
| Education | | | | | | | | | | | | | | | | | | | | | | |
| No highschool degree | 9.4 | 9.4 | 9.3 | 9.5 | 9.1 | 9.1 | 8.8 | 8.8 | 7.7 | 7.9 | 7.9 | 7.9 | 8.6 | 8.6 | 8.5 | 8.9 | 9.1 | 9.3 | 8.8 | 9.4 | 9.6 | 9.7 |
| Highschool graduate or the equivalent | 26.9 | 26.9 | 26.8 | 26.6 | 27.2 | 27.2 | 26.9 | 26.9 | 27.6 | 27.8 | 27.8 | 27.8 | 28.5 | 28.5 | 28.5 | 28.4 | 28.2 | 27.9 | 27.9 | 27.6 | 27.6 | 27.8 |
| Some college but no degree | 18.0 | 18.0 | 17.9 | 18.0 | 17.8 | 17.8 | 17.9 | 17.9 | 17.8 | 17.8 | 17.8 | 17.8 | 17.6 | 17.6 | 18.1 | 17.8 | 17.4 | 17.5 | 17.4 | 17.3 | 17.5 | 17.4 |
| Associate's degree in college | 10.5 | 10.5 | 10.5 | 10.3 | 10.2 | 10.2 | 10.1 | 10.1 | 10.3 | 10.1 | 10.1 | 10.1 | 9.9 | 9.9 | 10.1 | 10.1 | 10.1 | 10.0 | 10.0 | 10.1 | 10.0 | 10.0 |
| Bachelor's degree | 22.9 | 23.0 | 23.1 | 23.3 | 23.1 | 23.1 | 23.5 | 23.5 | 23.8 | 23.6 | 23.6 | 23.6 | 23.2 | 23.2 | 22.8 | 22.8 | 23.0 | 23.1 | 23.4 | 23.2 | 22.9 | 22.8 |
| Graduate degree | 12.2 | 12.2 | 12.3 | 12.4 | 12.6 | 12.6 | 12.8 | 12.8 | 12.8 | 12.8 | 12.8 | 12.8 | 12.1 | 12.1 | 12.1 | 12.1 | 12.2 | 12.1 | 12.5 | 12.4 | 12.4 | 12.3 |
| Marital Status | | | | | | | | | | | | | | | | | | | | | | |
| Married, spouse present | 50.0 | 50.0 | 50.2 | 50.3 | 50.7 | 50.7 | 50.6 | 50.6 | 50.6 | 50.4 | 50.4 | 50.4 | 49.4 | 49.4 | 49.4 | 49.0 | 49.0 | 49.1 | 49.2 | 48.9 | 49.0 | 48.6 |
| Married, spouse absent | 1.4 | 1.8 | 1.6 | 1.8 | 2.0 | 1.8 | 1.8 | 1.9 | 1.7 | 1.6 | 2.1 | 2.0 | 2.0 | 2.5 | 2.3 | 2.3 | 2.0 | 2.4 | 2.0 | 2.6 | 1.8 | 2.0 |
| Separated | 1.9 | 2.2 | 1.7 | 1.6 | 1.2 | 1.7 | 1.2 | 1.3 | 1.5 | 1.6 | 1.1 | 1.2 | 1.2 | 1.4 | 1.1 | 1.3 | 1.3 | 0.8 | 1.2 | 1.4 | 1.1 | 1.1 |
| Divorced | 9.4 | 9.4 | 9.3 | 9.1 | 9.1 | 9.1 | 9.3 | 9.4 | 9.3 | 9.4 | 9.4 | 9.4 | 9.6 | 9.6 | 9.6 | 9.6 | 9.6 | 9.4 | 9.4 | 9.4 | 9.2 | 9.4 |
| Widowed | 1.7 | 1.1 | 1.8 | 1.5 | 1.5 | 1.2 | 1.6 | 1.5 | 1.5 | 1.6 | 1.5 | 1.6 | 1.7 | 1.1 | 1.7 | 1.5 | 1.6 | 1.8 | 1.8 | 1.0 | 2.1 | 2.0 |
| Never Married | 35.5 | 35.5 | 35.4 | 35.7 | 35.5 | 35.5 | 35.5 | 35.4 | 35.5 | 35.5 | 35.5 | 35.5 | 36.0 | 36.0 | 36.0 | 36.2 | 36.4 | 36.4 | 36.5 | 36.8 | 36.9 | 36.9 |
| Relationship Status | | | | | | | | | | | | | | | | | | | | | | |
| Spouse/Partner living in same household | 58.6 | 58.6 | 58.7 | 58.9 | 59.2 | 59.2 | 58.9 | 58.9 | 59.2 | 58.8 | 58.8 | 58.8 | 58.8 | 58.8 | 58.7 | 58.6 | 58.5 | 58.3 | 58.4 | 58.3 | 58.4 | 58.2 |
| No Spouse/Partner living in same household | 41.4 | 41.4 | 41.3 | 41.1 | 40.8 | 40.8 | 41.1 | 41.1 | 40.8 | 41.2 | 41.2 | 41.2 | 41.2 | 41.2 | 41.3 | 41.4 | 41.5 | 41.7 | 41.6 | 41.7 | 41.6 | 41.8 |
| Number of children | | | | | | | | | | | | | | | | | | | | | | |
| 0 | 68.2 | 68.2 | 68.0 | 68.2 | 68.7 | 68.7 | 68.6 | 68.7 | 68.4 | 68.5 | 68.4 | 68.4 | 68.7 | 68.7 | 68.4 | 68.5 | 68.3 | 68.6 | 68.7 | 68.9 | 68.7 | 69.0 |
| 1 | 13.4 | 13.4 | 13.4 | 13.1 | 13.2 | 13.2 | 13.3 | 13.2 | 13.3 | 13.5 | 13.5 | 13.5 | 13.1 | 13.1 | 13.3 | 13.3 | 13.6 | 13.3 | 13.0 | 13.1 | 13.2 | 13.1 |
| 2 | 12.1 | 12.1 | 12.2 | 12.2 | 11.9 | 11.9 | 11.8 | 11.8 | 12.0 | 11.8 | 11.8 | 11.8 | 11.9 | 11.9 | 11.8 | 11.8 | 11.8 | 11.7 | 12.0 | 11.8 | 11.8 | 11.7 |
| 3+ | 6.3 | 6.3 | 6.3 | 6.4 | 6.2 | 6.2 | 6.3 | 6.3 | 6.3 | 6.3 | 6.3 | 6.3 | 6.3 | 6.3 | 6.4 | 6.4 | 6.4 | 6.5 | 6.3 | 6.2 | 6.3 | 6.2 |
| Region | | | | | | | | | | | | | | | | | | | | | | |
| Midwest | 20.6 | 20.6 | 20.6 | 20.7 | 20.6 | 20.6 | 20.7 | 20.7 | 20.8 | 20.8 | 20.8 | 20.8 | 20.7 | 20.7 | 20.6 | 20.7 | 20.6 | 20.6 | 20.5 | 20.5 | 20.6 | 20.5 |
| Northeast | 17.2 | 17.2 | 17.2 | 17.2 | 17.1 | 17.1 | 17.2 | 17.2 | 17.2 | 17.2 | 17.2 | 17.2 | 17.2 | 17.2 | 17.1 | 17.0 | 17.0 | 16.9 | 16.8 | 16.9 | 16.9 | 16.9 |
| South | 38.1 | 38.1 | 38.1 | 37.9 | 37.9 | 37.9 | 37.9 | 37.9 | 37.7 | 37.7 | 37.7 | 37.7 | 37.8 | 37.8 | 37.9 | 38.0 | 38.2 | 38.3 | 38.3 | 38.4 | 38.3 | 38.3 |
| West | 24.2 | 24.2 | 24.2 | 24.2 | 24.3 | 24.3 | 24.2 | 24.2 | 24.3 | 24.3 | 24.3 | 24.3 | 24.4 | 24.4 | 24.3 | 24.3 | 24.3 | 24.2 | 24.3 | 24.2 | 24.3 | 24.3 |
| Number of Observations | 61932 | 2491 | 2501 | 2462 | 2414 | 6170 | 3113 | 4964 | 3202 | 3372 | 3244 | 5174 | 3200 | 3294 | 3626 | 3565 | 3570 | 3582 | 3561 | 3287 | 3265 | 3298 |

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 and the share of individuals on layoff (as defined in the text) at the time of the last available CPS.

Figure A.4: Weights in CPS vs. RPS for Moments in Raking Procedure for – Waves 2 through 9

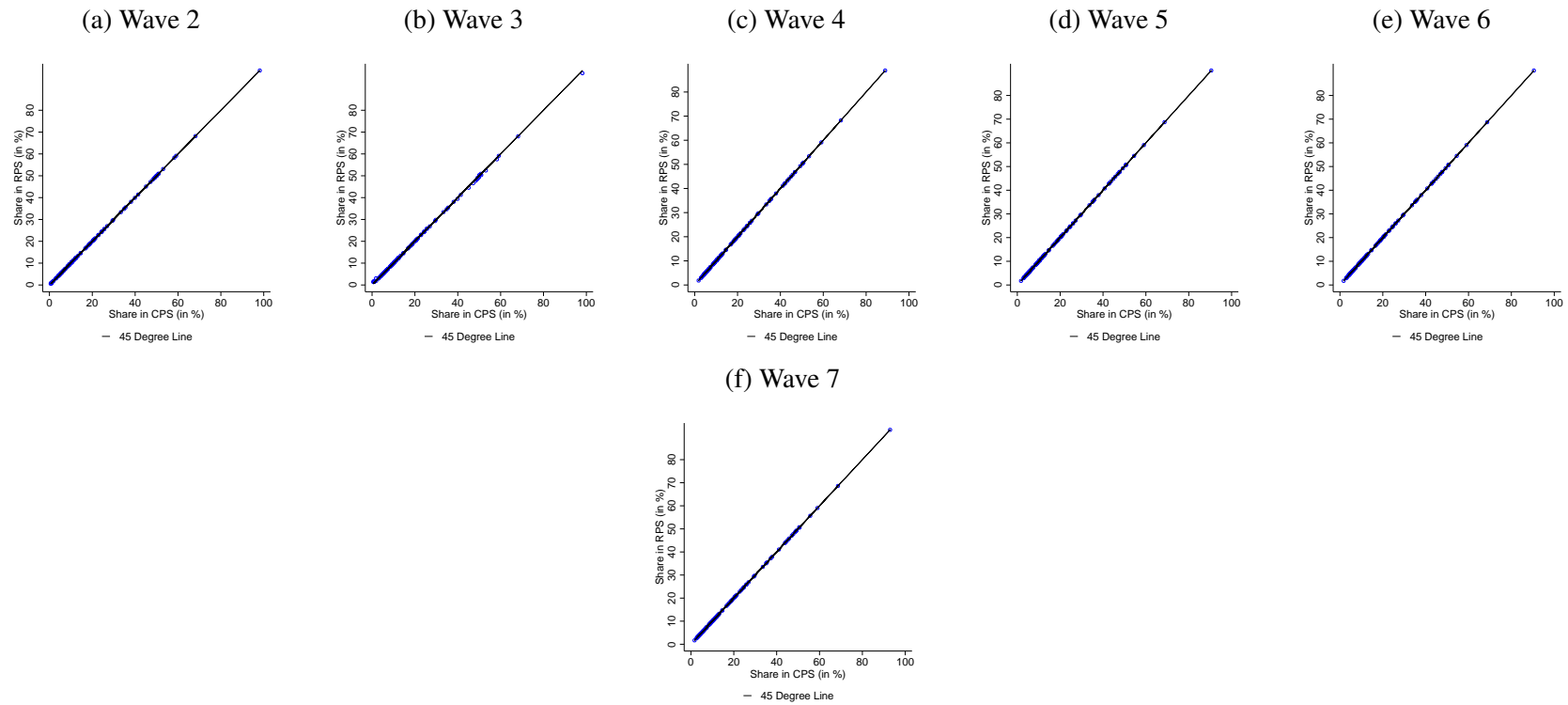
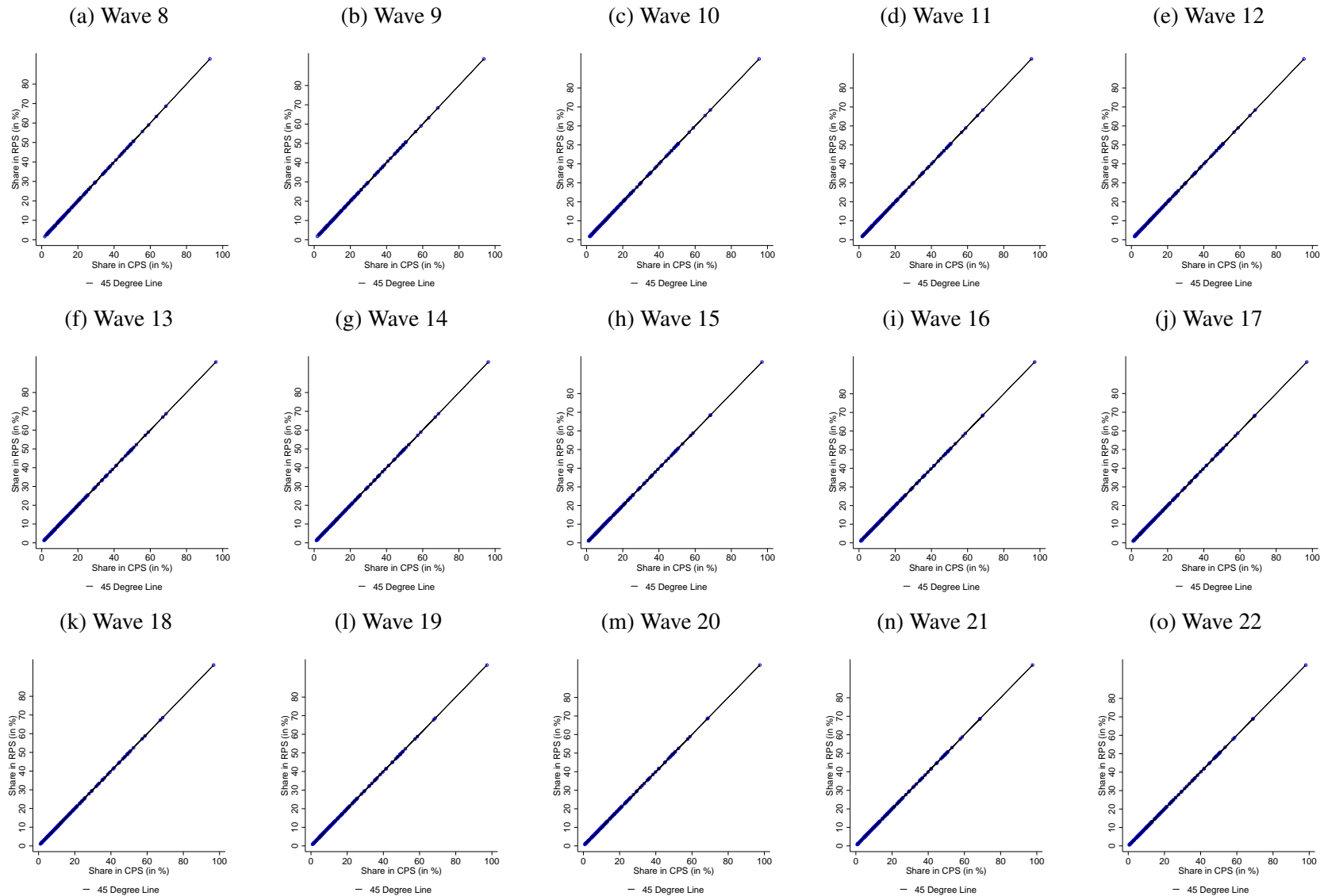


Figure A.5: Weights in CPS vs. RPS for Moments in Raking Procedure for – Waves 8 through 19



at the time of the previous CPS is not included.²⁴ 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 waves prior to wave 8.²⁵ 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 well this procedure works, we can compare our key statistics for waves 8 onwards when using the actual weights including employed at work status at the respective previous CPS and the one with the adjustment factor. Figure A.3 compares the employment rate and unemployment rate when using the actual weights including employed at work status at the respective previous CPS (RPS, Final Weights) and an alternative weighting scheme with the adjustment factor but not using the employed at work status at the respective previous CPS (RPS, Adjusted Weights). Overall the procedure works well, but suggests that our estimates prior to wave 8 for the employment rate would be slightly lower if we would be able to condition on the employed at work status at the previous CPS as well.

A.4.2 Modifications to the Survey Weighting Scheme Over Time

We have occasionally updated our weighting procedure to produce more reliable estimates.

When initially releasing results from our first 3 survey waves, we did not use any weights other than assigning a weight of 0.5 to respondents with a spouse/partner living in the household and to the spouse/partner, with a weight of 1 for everyone else. The motivation behind this weighting scheme was that the initial survey targets already resemble correctly the share of those with and without a spouse present, and without this adjustment the latter group would have been underrepresented.

Starting with wave 4 (early April 2020), we re-weighted the sample to match the share of individuals by household income (at most \$50,000, between \$50,000 and \$150,000, more than \$150,000), age (18-34, 35-49, 50-64), and relationship status (whether spouse/partner lives in the household or not). Starting with wave 14 (early October 2020), we implemented the raking procedure discussed in the previous section but without including the share of individuals on layoff in the previous month. We added the share of individuals on layoff to the weighting scheme starting with wave 20 (March 2020). In each case, we retroactively implemented the newly adopted weighting scheme to prior waves as well. In [Bick and Blandin \(2021\)](#) we publish the results of each new RPS

²⁴These 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.

²⁵Note that we apply this adjustment after the adjustments discussed in Appendix A.3.

release and gather all previous reports on the [project website](#), which allow one to track the estimates released at each point in time.

A.5 Comparison with Foote et al. (2022)

Foote et al. (2022) also ran a repeated online survey that follows the CPS's core labor market module, 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, 2020 referencing the week of March 29-April 4, 2020. Foote et al. (2022) shared a preliminary draft with us on April 24, 2020. Their results, with an initial reference week of April 5-11, 2020, were first made public on June 24, 2020, at which point the RPS had released its first six survey reports. From June 2020 through March 2021 they released weekly labor market estimates for the US.

The YLS covers the population aged 20+, while the RPS covers the population aged 18-64. 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 3. Another difference between the two surveys relates to the weighting scheme. The RPS conditions on employed at work and layoff status in the previous month, whereas the YLS conditioned through November 2020 on employment in February 2020 and from December 2020 onwards on employment, unemployment, and participation status in the previous month. (See the description posted on the Boston Fed's [project website](#).)

In Figure A.6 we draw a comparison between the two surveys for the employment at work rate, employment rate, unemployment rate, and labor force participation rate.²⁶

Figure A.6a shows in the RPS the employed at work rate decreased somewhat less and somewhat more in the YLS than in the respective CPS counterpart. The change in the May 2020 employment at work rate relative to February 2020 in the CPS and YLS are fairly similar to each other, whereas the RPS implied a further decrease in employment relative to April 2020. From then onwards, both surveys track the CPS similarly well although through November and December 2020 the YLS estimates vary quite a bit. This pattern is not visible for the employment rate (see Figure A.6b), suggesting that these are primarily seasonal effects (Thanksgiving, Christmas). From June 2020 onwards, the RPS tends to have a smaller gap with the CPS estimates than the YLS.

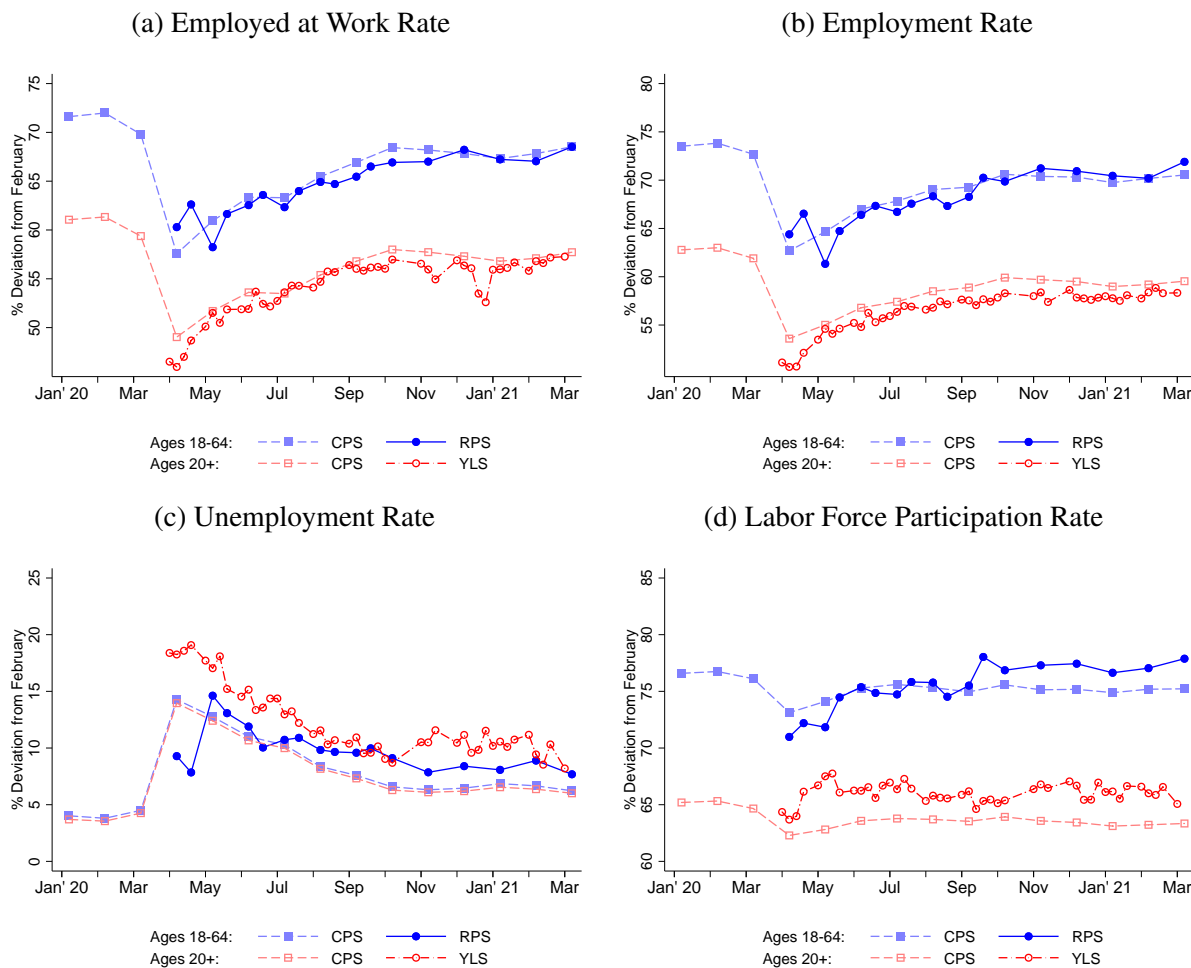
With the exception of April 2020 for the RPS, both the YLS and RPS overpredict the unemployment rate (see Figure A.6c). Mirroring the comparison with the employment rate, the gap with the CPS unemployment rate is larger for the YLS.

Through September 2020, the RPS tracks the labor force participation rate more closely than the YLS. From October onwards the match is similarly close, see Figure A.6d.

²⁶While Foote et al. (2022) also collect data on hours and earnings, they have not yet published the corresponding time-series.

Table A.4 summarizes the absolute percentage deviation from the respective CPS series for each survey. Our assessment is that the two surveys produce fairly comparable estimates of the state of the labor market with the most notable exception being the unemployment rate.²⁷ We interpret this as an additional validation of our methodological approach.

Figure A.6: Labor Market Outcomes in the RPS, YLS and CPS



Notes: Figure contains data from the RPS and YLS, alongside data from the CPS for the respective age coverage.

²⁷For the weighting scheme used for the RPS releases between October 2020 and March 2021, the differences between the YLS and RPS were smaller than in Figure A.6.

Table A.4: Mean Absolute Percentage Point Deviation with the CPS in the CPS Reference Week

| | RPS | YLS |
|---------------------------|-----|-----|
| Employed at Work Rate | 1.0 | 1.5 |
| Employment Rate | 1.2 | 1.0 |
| Unemployment Rate | 1.9 | 3.8 |
| Labor Force Participation | 1.4 | 2.7 |

Notes: Table contains data from the RPS and YLS, alongside data from the CPS for the respective age coverage. For the RPS and YLS only the observations overlapping with the CPS reference week are used. The average is calculated over the time period April 2020 through May 2021.

B CPS and RPS Comparisons by CPS Interview Month

Section 4 in the main text showed that, in most months during the pandemic, the unemployment rate was higher in the RPS than in the CPS. One potential explanation for this pattern begins with a finding by [Ahn and Hamilton \(2021\)](#), who point out that in the CPS the unemployment rate tends to be lower among individuals in their first interviews compared to individuals in their second - eighth interviews. This could reflect either issues of selection, if unemployed or non-labor-force 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 not being unemployed. Either effect would bias the overall unemployment rate downwards, which should not be present in a one-shot survey like the RPS.

Table B.1: Comparing the RPS to CPS By CPS Interview Month, June 2020 - June 2021

| | RPS – CPS | RPS – CPS1 |
|--------------------------------|-----------|------------|
| Labor Force Participation Rate | 1.25 p.p. | 0.36 p.p. |
| Unemployment Rate | 1.51 p.p. | 0.92 p.p. |
| Employment Rate | 0.07 p.p. | −0.30 p.p. |

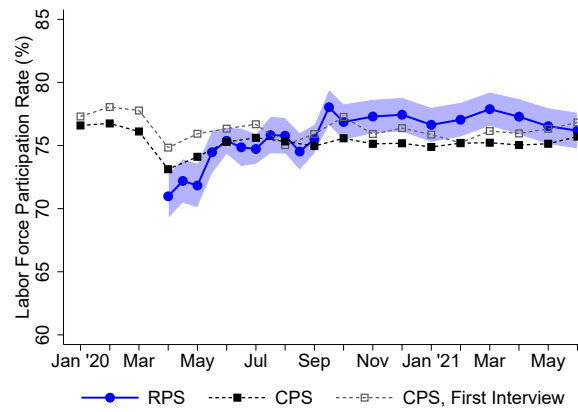
Notes: Table shows average difference in percentage points between labor market estimates in the RPS, CPS, and CPS1. CPS refers to all CPS respondents age 18 - 64 in any interview month. CPS1 refers to only the subset of CPS respondents age 18 - 64 in their first interview. The RPS does not contain a panel component. Average differences are computed between months June 2020 - June 2021.

To assess whether these issues contribute to differences in estimates between the RPS and CPS, Figure B.1 compares aggregate labor market outcomes in the RPS (blue circles), the CPS including all interview months (solid black squares), and the CPS using only first interviews (hollow gray squares, CPS1) between January 2020 and June 2021. Table B.1 summarizes the average differences between the RPS, CPS, and CPS1 series. (The averages are taken between June 2020 and June 2021, excluding the April and May 2020 observations due to high volatility in the RPS series over this time.)

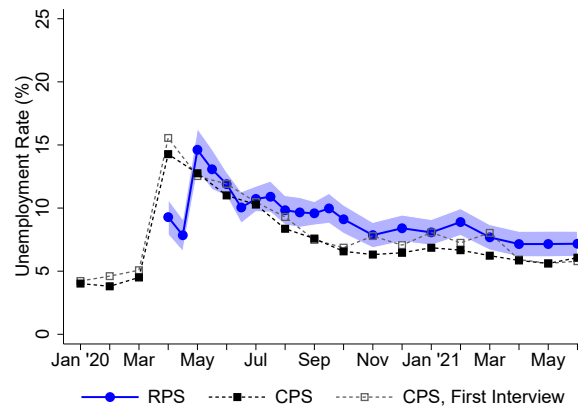
Figure B.1a and the first row of Table B.1 show that the average labor force participation rate in the RPS was closer to the average participation rate among first-interviews in the CPS (CPS1) compared with all interview months (CPS). On average, RPS labor force participation was 0.36

Figure B.1: Comparing CPS Outcomes in All Interviews to First Interviews, Age 18-64

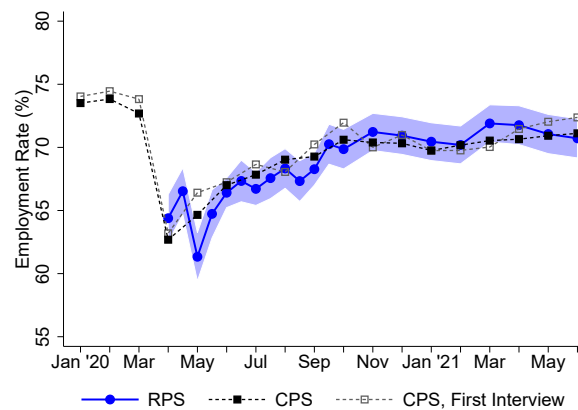
(a) Labor Force Participation Rate



(b) Unemployment Rate



(c) Employment Rate



Notes: Data are from the RPS and CPS, ages 18 - 64. The solid black squares correspond to the full CPS sample; the hollow gray squares correspond to only individuals in the CPS who were in their first interview month. The blue circles correspond to the RPS, and the blue shaded region corresponds to 95 percent confidence intervals for the RPS.

percentage points higher than participation among CPS respondents in their first interview. By contrast, the RPS participation rate was 1.25 percentage points higher than the participation rate among all interview months in the CPS, more than three times larger than the gap between RPS and CPS1. The average unemployment rate in the RPS is also closer to the average rate in the CPS1 than to CPS. On average, unemployment in the RPS was 0.92 percentage points higher than in CPS1, compared to a difference of 1.51 percentage points between the RPS and CPS. Finally, unlike labor force participation and unemployment, average employment in the RPS is closer to the overall CPS than to CPS1. However, these differences are small: on average employment in the CPS was 0.07 percentage points higher than in the CPS, and 0.30 percentage points lower than in CPS1.

Based on these results, we conclude that labor force participation and unemployment in the RPS are closer to the corresponding rates in first-interviews of the CPS (CPS1) than to all interviews of the CPS. While the opposite is true for employment, all three datasets produce very similar estimates for this series. This suggests that a substantial share of the difference in labor force participation and unemployment between the RPS and CPS could be due to either selection or strategic responses in later interviews in the CPS, which do not affect either the RPS or the first interview of the CPS.

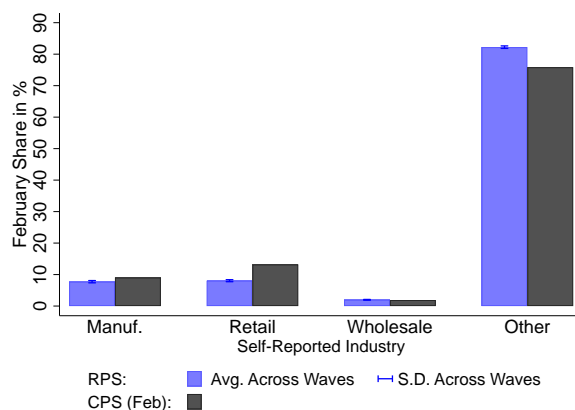
C Additional Validation Results

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.

C.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 C.1. Similar to the not-self-reported sector in the CPS, the RPS and CPS closely line up for this measure.

Figure C.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 consistently available from wave 5 (late-May)-onward.

C.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 C.2a shows that in the RPS there are only slightly more respondents living in a household with somebody owning a business than in the RPS. For all other variables in Figure C.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 Figures C.3 and C.4. For April through September 2020, in Figure C.3 the shares in the RPS are taken from the respective early wave sharing the same reference week as the CPS.

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

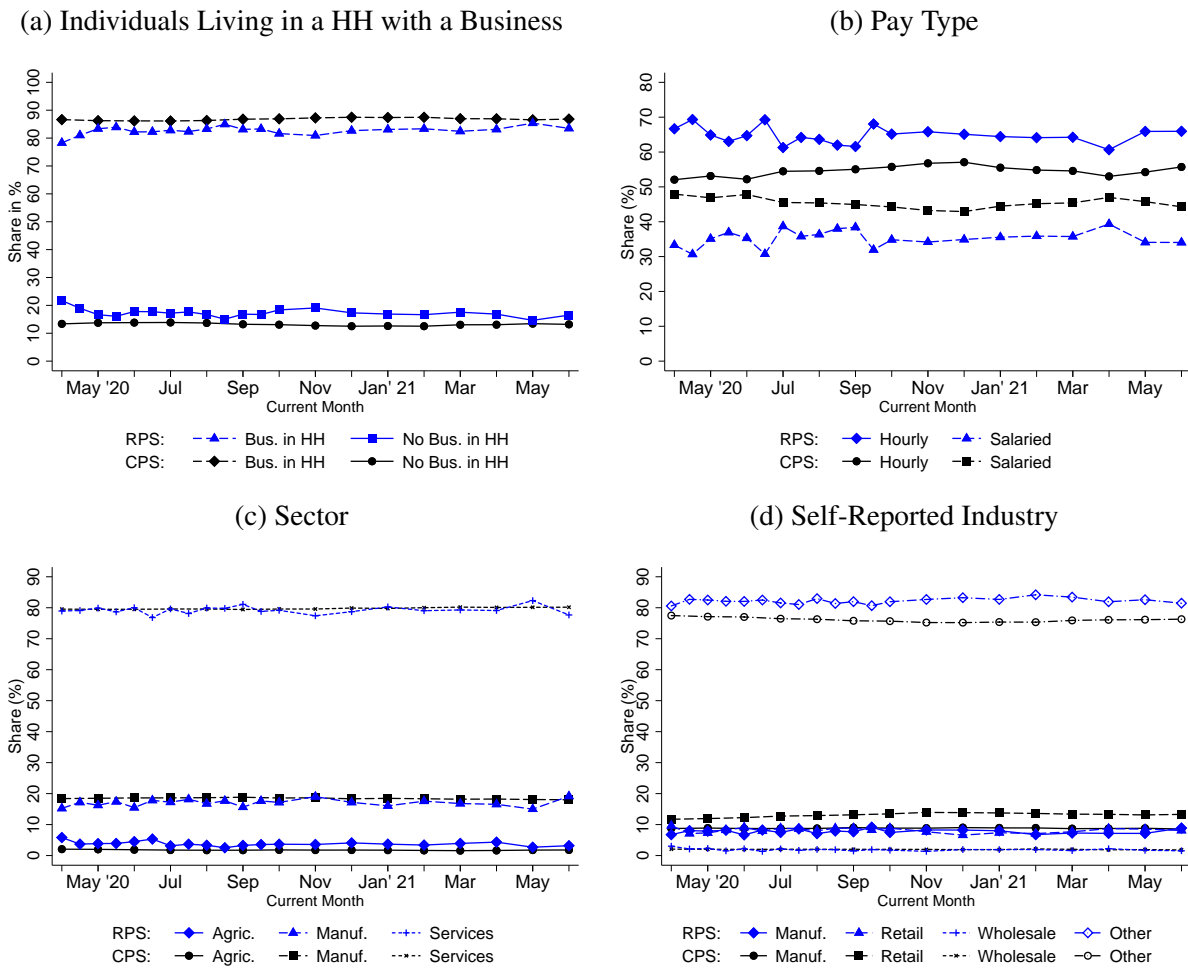


Figure C.3: Industry Composition in 2020

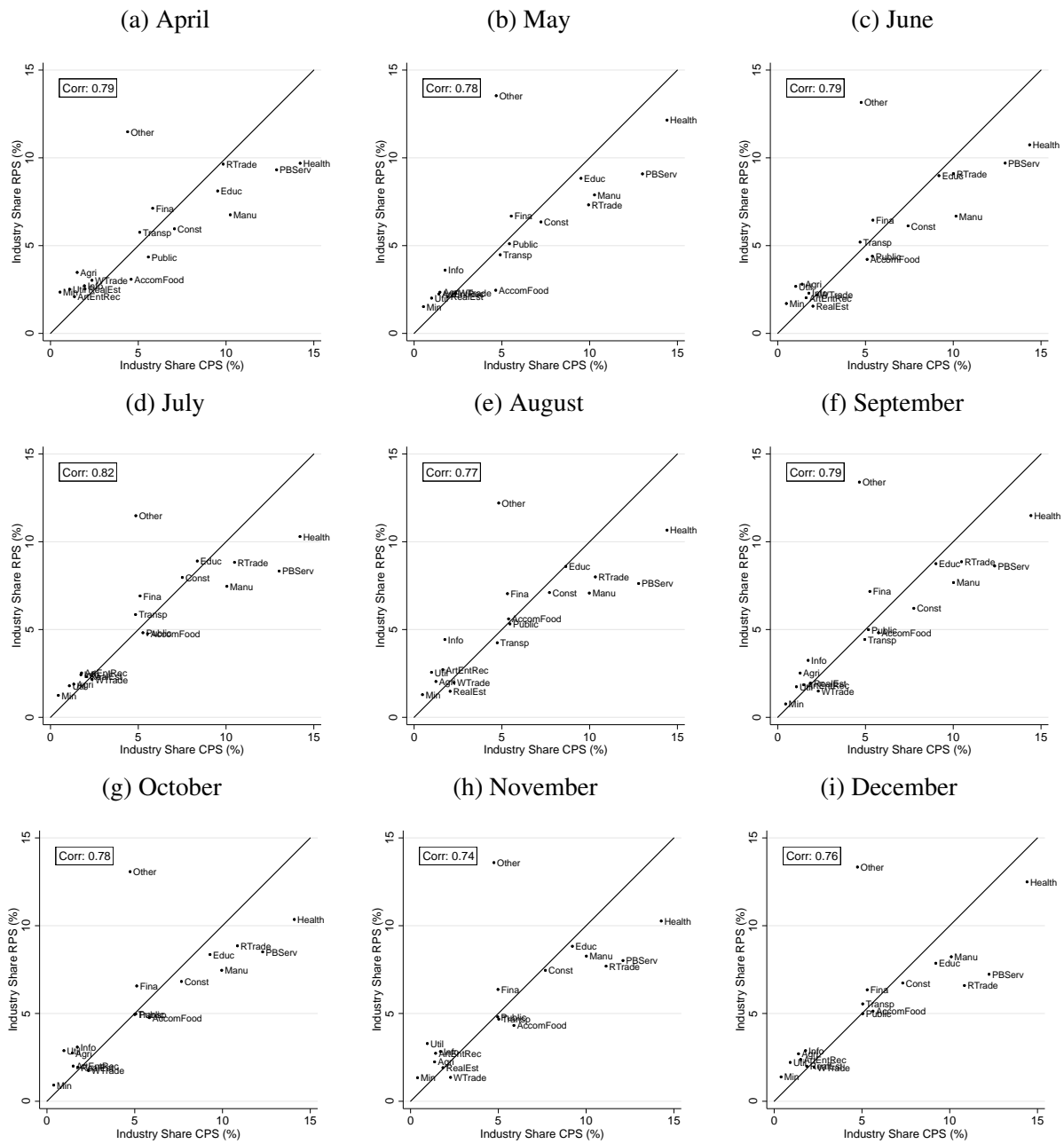
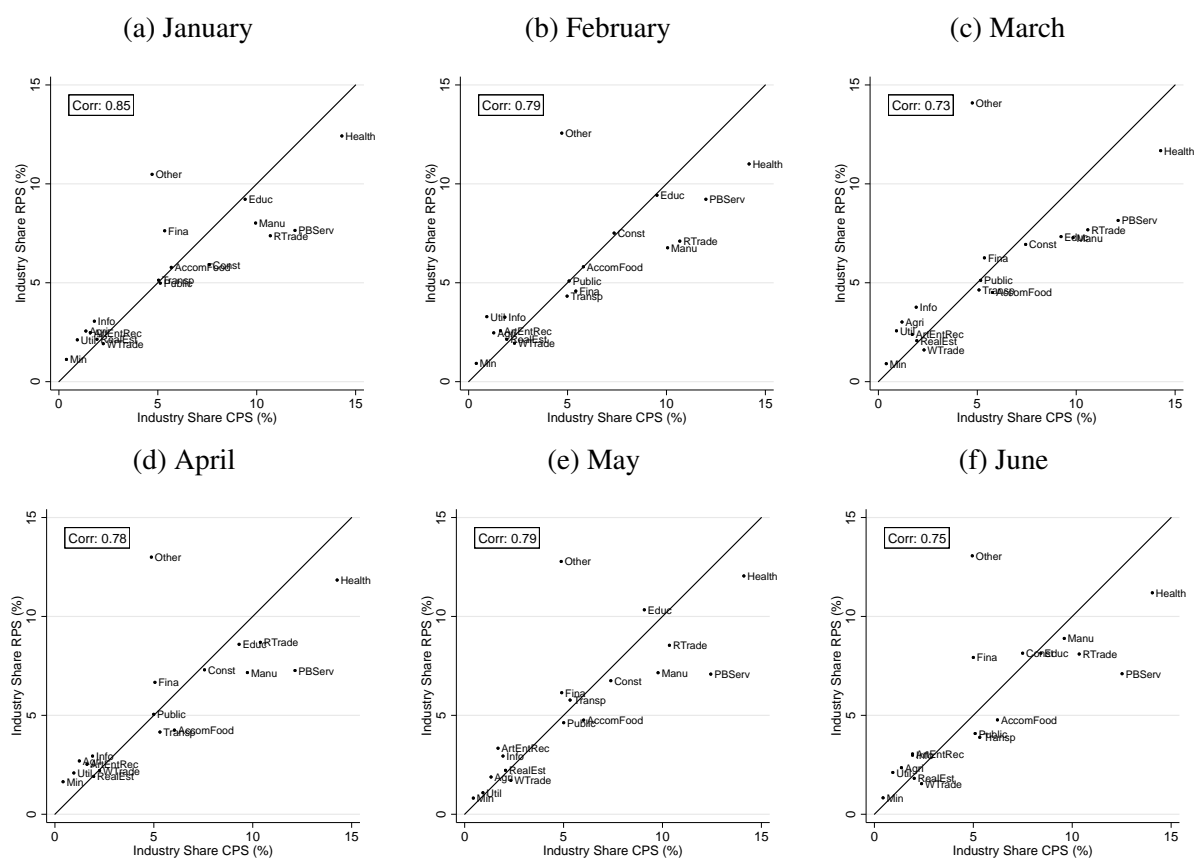


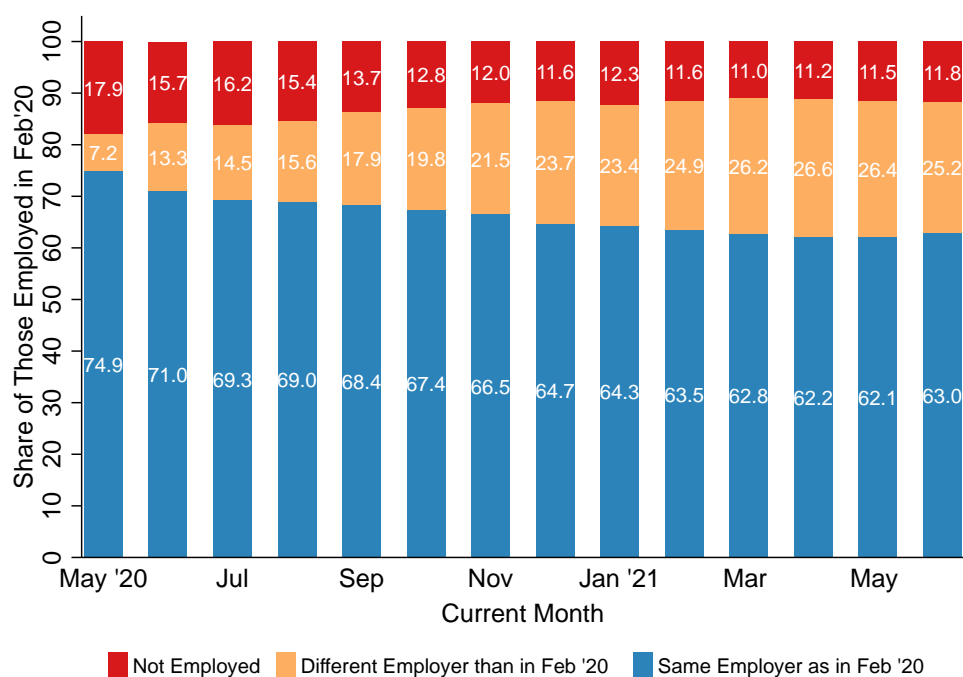
Figure C.4: Industry Composition in 2021



D Additional Results on Employer Tenure and Separations

D.1 Employer Separations Through June 2021

Figure D.1: Rates of Separation from Pre-Pandemic Employers: May 2020–June 2021



Notes: Figure extends data from Figure 9 through June 2021 waves of the RPS. For more details, see figure note of Figure 9.

D.2 Conditional Probabilities of Employer Separation, Non-employment, and Earnings Changes

Table D.1: Conditional Probabilities of Employer Separation

| | |
|--------------------------------|----------------------|
| Constant | 0.457*** (0.046) |
| Employer Tenure: 3-5 Years | -0.245*** (0.024) |
| Employer Tenure: 6-9 Years | -0.290*** (0.031) |
| Employer Tenure: 10+ Years | -0.326*** (0.027) |
| Female | 0.019 (0.019) |
| Age 18-29 | 0.098*** (0.026) |
| Age 50-64 | -0.076*** (0.023) |
| Education: High School or Less | 0.060** (0.025) |
| Education: Bachelors or More | -0.001 (0.023) |
| Black | 0.096*** (0.029) |
| Hispanic | 0.067*** (0.025) |
| Industry Effects | <i>Yes</i> |
| Obs. | 2251 |
| R^2 | 0.21 |

Source: March and April 2021 wave of Real-Time Population Survey, ages 18-64. Estimates the probability of separating from pre-pandemic (February 2020) employer by March 2021, based on a linear probability model. The sample is all February 2020 employees with valid tenure observations, excluding the self-employed and unpaid family workers. The regressions are weighted based on sample weights.

Table D.2: Conditional Probabilities of Non-employment After Employer Separation

| | |
|--------------------------------|---------------------|
| Constant | 0.186** (0.076) |
| Employer Tenure: 3-5 Years | 0.040 (0.037) |
| Employer Tenure: 6-9 Years | 0.035 (0.056) |
| Employer Tenure: 10+ Years | 0.139*** (0.053) |
| Female | 0.041 (0.032) |
| Age 18-29 | 0.065* (0.036) |
| Age 50-64 | 0.181*** (0.046) |
| Education: High School or Less | -0.030 (0.039) |
| Education: Bachelors or More | -0.051 (0.039) |
| Black | 0.047 (0.044) |
| Hispanic | 0.000 (0.038) |
| Industry Effects | <i>Yes</i> |
| Obs. | 908 |
| R^2 | 0.07 |

Source: March 2021 wave of Real-Time Population Survey, ages 18-64. Estimates the probability of being non-employed in March 2021 conditional on having separated from pre-pandemic (February 2020) employer, based on a linear probability model. The sample is all February 2020 employees with valid tenure observations, excluding the self-employed and unpaid family workers, who were no longer working for their pre-pandemic employer. The regressions are weighted based on sample weights.

Table D.3: Conditional Probabilities of Earnings Cut

| | (1) New Employer | (2) Same Employer | (3) Same Employer |
|--------------------------------|------------------------|-------------------------|-------------------------|
| Constant | 0.268** (0.112) | 0.093* (0.048) | 0.068 (0.048) |
| Recalled | — | — | 0.134*** (0.026) |
| Employer Tenure: 3-5 Years | 0.120** (0.058) | −0.042 (0.028) | −0.036 (0.028) |
| Employer Tenure: 6-9 Years | 0.024 (0.084) | −0.048 (0.032) | −0.046 (0.032) |
| Employer Tenure: 10+ Years | 0.014 (0.077) | −0.067** (0.028) | −0.060** (0.028) |
| Female | 0.073 (0.048) | 0.033* (0.019) | 0.032* (0.019) |
| Age 18-29 | 0.077 (0.055) | −0.002 (0.030) | −0.016 (0.030) |
| Age 50-64 | 0.243*** (0.067) | −0.004 (0.021) | −0.001 (0.020) |
| Education: High School or Less | −0.058 (0.056) | 0.043 (0.026) | 0.044* (0.026) |
| Education: Bachelors or More | 0.006 (0.058) | −0.014 (0.022) | −0.011 (0.022) |
| Black | 0.006 (0.065) | −0.031 (0.033) | −0.026 (0.033) |
| Hispanic | −0.057 (0.055) | 0.022 (0.026) | 0.016 (0.026) |
| Industry Effects | <i>Yes</i> | <i>Yes</i> | <i>Yes</i> |
| Obs. | 521 | 1030 | 1030 |
| R^2 | 0.07 | 0.04 | 0.07 |

Source: March 2021 wave of Real-Time Population Survey, ages 18-64. Estimates the probability of experiencing a cut in earnings in March 2021 relative to February 2020. An earnings cut is defined as a decline of at least 10%. The starting sample for each regression is all individuals with non-missing employer tenure information, hourly wages at least half the federal minimum wage, and usual weekly earnings below the CPS topcode value of \$2,885. Within this sample, column (1) contains individuals working for a different employer than in February 2020. Columns (2) and (3) contains individuals still working for their February 2020 employer. All regressions are weighted based on sample weights.