Young Children and Parents' Labor Supply during COVID-19

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Abstract

We study the effect of childcare needs during COVID-19 and parental labor supply. Following a pre-analysis plan, we implement three variations of an event-study research design comparing workers with and without childcare responsibilities. We find childcare needs did not negatively affect parents' labor supply during the pandemic, with some evidence suggesting parents with childcare requirements were more often working. On the extensive margin, our results are not systematically different for men and women, but some increases are found on the intensive margin only for mothers. We provide evidence suggesting the ability to work remotely helped parents avoid labor supply decreases.

KEY WORDS: Labor supply; COVID-19; Childcare; School closures; Coronavirus

JEL Classifications: I1, J22, H12

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Introduction

The onset of the COVID-19 pandemic prompted unprecedented policy responses from American state and federal governments to implement social distancing, including broad orders for closures of schools and childcare providers. While these closures may have helped reduce disease spread,[1] they induced concerns over their potential impacts on parents' ability to work, particularly for women. Would the sudden need to provide childcare for children no longer in school or daycare prohibit workers from continuing to work or from finding new jobs? In the immediate aftermath of the school and childcare provider closures, several analysts suggested this could be the case (Alon et al. 2020a; 2020b; Bayham and Fenichel 2020; Kahn, Lange, and Wiczer 2020; Dingel, Patterson, and Vavra 2020; Rojas et al. 2020).

At first glance, there is a strong inclination to think school and daycare center closures will lead to parents being unable to work since they raise the cost of childcare. This presumably leads to parents substituting their own time to care for children. Indeed, the literature on the effects of childcare costs on labor supply of mothers has typically, although not always, found that higher costs are associated with lower labor force participation (D. Blau and Currie 2006 and Müller and Wrohlich 2020 provide thorough reviews). The context surrounding the unfolding of the COVID-19 pandemic, however, makes the theoretical prediction less straightforward. Rapid spread of the disease led to schools and childcare providers closing with minimal warning, giving parents little flexibility in finding childcare. At the same time, stay-at-home policies and business closures might have resulted in family or neighbors being available to help provide care. Availability of such informal sources of childcare has been noted in the literature as

[1] There is evidence of school closures inducing some forms of social distancing, but little initial evidence they reduced disease spread (Gupta et al. 2020; Courtemanche et al. 2020b; 2020a).

potentially blunting the reliance on formal sources (Heckman 1974; D. M. Blau and Robins 1988). The extent of such availability in the COVID-19-economy is unclear, though, given social distancing efforts among the population. Finally, as the pandemic unfolded, many employers implemented remote working policies and technologies, allowing parents to work from home much more than in the past. In sum, while there is a straightforward substitution mechanism suggesting COVID-19 school and daycare closures could negatively affect labor supply, the pandemic creates a situation with unique features that could augment or limit the impact of that mechanism.

In this paper, we consider the effects of childcare responsibilities during COVID-19 on the labor supply of parents with young children. Following a pre-specified analysis plan, we use data from the monthly Current Population Survey (CPS) to implement three variations of an event study research design that compares parents with childcare needs to those without (or with lesser needs) to disentangle the effect of childcare responsibilities from the aggregate demand shock the COVID-19 pandemic brought to the labor market. In the first research design, our comparison is of parents with young children to those with no such children. In the second, limiting our sample to those with young children, we compare parents without a teenager in the house to those with one. In the third, we further limit the sample to parents with young children, but no teenager, and use the presence of a grandparent in the house as our basis for comparison. For each of these, we analyze three outcomes of interest: (1) whether a parent was actually working (not sick, on vacation, or otherwise away from his or her job) for an employer during the survey reference week, (2) whether the parent was employed during that week, and (3) the number of hours worked conditional on working. Since the literature on childcare has typically focused on labor supply of mothers, we also perform our analysis separately for men and women.

In our pre-specified analysis, we find that, contrary to expectation, the labor supply of adults was not negatively affected by having young children during the COVID-19 pandemic, a finding that holds for each of our research design variations and outcomes. In contrast, we find some evidence that parents with childcare responsibilities were more likely to be working than those without after the onset of the pandemic, a result that is not systematically different for men and women. Furthermore, some estimates suggest working parents of young children worked more hours per week than those without young children, and that this effect was concentrated among women. This result is consistent with mothers compensating for lost productivity due to childcare demands by working longer hours. Using new questions added to the CPS during the COVID-19 pandemic, we also show that the ability to work remotely was used more often by those with childcare needs, suggesting that employer flexibility with respect to working at home aided parents in avoiding negative impact to their labor supply. These new questions also provide evidence that our results were not due to an increase in labor demand for parents of young children relative to those who are not.

In post-hoc analysis of sub-group heterogeneity, we find our main results appear to be driven by five groups: white respondents, high school drop-outs, college graduates, urban residents, and those whose urban or rural status is not known. Additionally, among the sub-groups, the only statistically significant negative estimates we obtain are for the likelihood of being at work for both single mothers and fathers, and for the number of hours worked (conditional on working) for both single women and black respondents. In each case, however, these significant negative estimates are only found using one research design.

In the context of the literature on childcare, findings like ours are not entirely unprecedented. As we noted above, childcare costs typically have been found to have negative effects on labor supply, but this was especially true in the early studies. In more recent work, findings of little or no impact have become more common (Lundin, Mörk, and Öckert 2008; Fitzpatrick 2010; Havnes and Mogstad 2011; Fitzpatrick 2012). Such results are consistent with general trends of falling labor supply elasticities for women over time (F. D. Blau and Kahn 2007; Heim 2007). Nevertheless, that some of our estimates show a positive effect on labor supply is unusual in this literature, suggesting the COVID-19 pandemic created a unique childcare and work environment for parents.

In addition to contributing to the broad literature on childcare and labor supply, this paper also adds to the literature on the American labor market and COVID-19 as one of the first to study the labor supply of parents early in the pandemic, and the only one to use a publicly-available pre-analysis plan. The pre-specification of our research designs, outcome variables, sample, and regression specifications before the post-period data became publicly available increases the credibility of our results and assures they are not a result of data mining. This paper is also the first to make use of new questions in the CPS about the response to the pandemic to provide context for its findings on labor supply, and important component for understanding how parents responded to the rapidly changing childcare situation.

Finally, we note that our research designs have an important advantage in a pandemic environment: they are not subject to policy endogeneity. An alternative approach of using government policies – such as state-level or school-district-level school-closures –has the drawback that *we know* these policies were adopted in response to the spread of COVID-19. Since the labor market was also impacted by disease spread and other pandemic policies, this policy endogeneity could potentially cause bias. Our use of treatment and control groups based on ages of children, however, is not subject to this critique, and our use of CPS data allows us to provide a long pre-period to evaluate pre-trends between the two groups. Since the nature of the pre-existing differences between our groups is not related to the disease, this provides important evidence on their comparability. On the other hand, COVID-19 policy responses are specific to this outbreak, so pre-period comparison of geographical areas with different policy responses is less informative about how reasonable those comparisons are during the pandemic. Thus, our approach provides a credible alternative to policy-based research designs.

Literature Review

The literature on the effects of childcare responsibilities on the labor supply of American parents during the COVID-19 pandemic is quickly growing. Our article is part of the first wave of papers that began circulating publicly by early July of 2020, which also includes Collins et al. (2021), Heggeness (2020), Kalenkoski and Pabilonia (2020), and Rojas et al. (2020).[2,3] Compared to our study, these others all use empirical approaches that differ greatly from ours and investigate very different populations. Using state-level variation in school closings to study the general population, Rojas et al. (2020) analyze new unemployment insurance benefit claim

[2] Rojas et al. (2020) was distributed by the NBER working paper series on May 11th, 2020. Our pre-analysis plan was posted on OSF Registry two days later. Heggeness (2020) was posted on the website of the Minneapolis Federal Reserve Bank on June 15th, 2020, while our draft was made available on SSRN on June 19th, 2020. Collins et al. (2021) appeared on the website of the journal Gender, Work & Organization on July 2nd, 2020 and Kalenkoski and Pabilonia (2020) was made available on SSRN on July 7th, 2020.

[3] Internationally, studies of childcare and labor supply during the pandemic include Ma, Sun, and Xue (2020) for China, Farré et al. (2020) for Spain, Beauregard et al. (2020) for Canada, and Hanzl and Rehm (2021) for Austria.

filings, while Heggeness (2020) studies unemployment and other labor market outcomes using CPS data. Investigating unincorporated self-employed workers, Kalenkoski and Pabilonia (2020) use CPS data to compare workers across various demographic characteristics, including comparing parents with children to those without (an approach similar to our first research design). Collins et al. (2021) uses CPS data and an individual-level fixed effect strategy to compare the pandemic's effect on married mothers versus fathers.

Rojas et al. (2020) do not find school closures affected filings, but their estimates are imprecise. Similarly, Heggeness (2020) finds no effects for most outcomes, including unemployment, but she does find some evidence of an increased likelihood of one category of employment, being employed but temporarily not at work, and an increase in the number of hours worked by women. The interpretations of results for both of these studies are complicated by the fact that states across the country issued school closure orders within a matter of days. This leaves little variation through which estimates based on differential timing of closures could be identified.[4] More importantly, since they are identified based on policies adopted in response to the pandemic, their results could be affected by the above described policy endogeneity.

In their study of self-employed workers, Kalenkoski and Pabilonia (2020), like us, rely on variation across individuals, reporting results that vary across research design implementations. In contrast with our findings, however, they do obtain some large and statistically significant negative estimates for differences in employment and hours worked between parents and non-parents. Surprisingly, these estimates are concentrated among fathers of children over six years old. Collins et al. (2021) find mothers reduced their hours 4 to 5 times more than fathers in April

[4] Heggeness (2020) also treats all 2020 data as post-period, including January and February, further complicating interpretation.

of 2020. However, their comparison of men and women does not account for gender differences in jobs and industries in which parents work.

Like the initial set of papers, a second wave of research tended to find mixed results. These papers also usually focus on mothers and the comparison of women to men, though in some cases parts of their analyses provide comparisons within gender, which we focus on here. Zamarro and Prados (2021) rely on an internet-based "pulse" survey, finding college-educated women with school-aged children were more likely than those without kids to report having reduced their hours worked, but also find women without college were less likely to be so affected.[5] Amuedo-Dorantes et al. (2020) used school closure policies to compare across states, finding reduced work hours for both men and women, but no effect of school closings on employment or not working the previous week.[6] Russell and Sun (2020) compare responses of women with and without children under 6 years old to state-level child care center closure or capacity limit policies. They find some evidence of an increase in unemployment for women with kids under 5 versus those without, but no effects on hours or labor force participation. Fabrizio, Gomes, and Tavares (2021) closely follow our first research design, though focus on subgroups by education and race. They argue women with kids were less likely to be employed than those without, but do not account for differences in employment for these groups before the pandemic. As our analysis shows, once these previous differences are accounted for, there is little evidence of an effect on women.

[5] They do not report the coefficient estimator covariances needed to determine whether the differences between those with and without young children are statistically significant or not.
[6] Hours losses for women were larger than for men in their main specification, but it is not clear if the difference was statistically significant.

Petts, Carlson, and Pepin (forthcoming) use an online survey taken in late April 2020, and measure levels of hours of childcare lost as the basis for their analysis. They report that mothers of children under 6 who lost more than full-time childcare were less likely to work than mothers who lost less than 10 hours of childcare, and that mothers helping educate their school-aged children were less likely to work than those who were not.[7] In papers that fully focus on comparison of men and women, Collins, Ruppanner, et al. (2021) and Alon et al. (2021) argue childcare responsibilities increased gender gaps. Collins, Ruppanner, et al. (2021) report that the gender gap in labor force participation grew in 2020 versus that in 2019 in states with remote school, whereas in hybrid or in-person states saw less of a gap increase. They do not report whether the difference in change across instruction type was statistically significant. Finally, Alon et al. (2021) argue the employment and hours gender gap in the USA grew more for parents with school-aged children than for those with younger kids or no kids. However, in their analysis that is most similar to our approach in research design 1, their estimates appear to be similar between parents with kids and those without, which would suggest a similar result to ours.[8]

Data

We base our analysis on data from the basic monthly Current Population Survey (CPS), which has important advantages for our question of interest. It is the basis for the government's official labor market statistics, has a large sample size, has a relatively high frequency as a monthly survey, and makes data available to researchers quickly. These features allow us to

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[7] They do not address the concern that work and hours lost to childcare during the pandemic and contributing to kids' educational content are jointly determined outcomes.
[8] They do not provide the estimator covariances needed to determine if the estimate differences across parent types are statistically significant.

provide timely analysis on the performance of the labor market and the impact of school and daycare closure policies during the pandemic.

Our sample, obtained from IPUMS CPS (Flood et al. 2020), includes data for each month from January 2018 through June 2020, and for all non-military, non-student adults ages 21 to 59. Table 1 presents sample averages for our outcomes and selected demographic characteristics for each research design and for both before and after the onset of the pandemic. Since our treatment and control groups are based on differences in children or whether respondents live with their parents, our groups naturally have average differences for some demographic measures. However, as we show below, the groups exhibit parallel trends before the pandemic for our outcomes of interest. Additionally, our preferred specifications of our models include numerous, flexible controls for observable demographic characteristics. Therefore, we do not view the reported differences in some sample averages as being of critical concern for our analysis.

Empirical Methods

We used two separate plans to pre-specify our analyses. Our main analysis was pre-planned in Barkowski, McLaughlin, and Dai (2020a), while a sub-analysis using new COVID-19 variables added to the CPS after the onset of the pandemic was pre-specified in Barkowski, McLaughlin, and Dai (2020b). Both were developed before post-period data for our main analysis, and all the data for the sub-analysis, were available publicly.[9] While it is unusual to pre-

[9] Our pre-analysis plan for our main analysis was posted on the OSF Registry on the same day the U.S. Census Bureau first posted the April data on its website, May 13th, 2020. Our plan for analysis of the new COVID-19 questions was posted on June 8th, 2020. Data for those questions was not made available to the public until November of 2020.

specify analyses when using publicly available government surveys, it is not unprecedented (Neumark 2001). These publicly posted plans limit our ability to perform specification searches, increasing the credibility of our results.

In our main analysis, we focus on three primary labor supply outcomes. The first is a dummy variable indicating whether individuals' employment status is "at work." A worker categorized as at work is employed and actively working. This outcome is related to formal employment but excludes individuals who are employed but not working for reasons such as vacation and illness. This outcome has the advantage of measuring the extent to which individuals were able to perform their job duties whether from home or to leave the house (if necessary) to work, activities that childcare responsibilities might be inhibit. The second outcome is a dummy variable for employment, a more standard measure of labor market activity. Employed individuals are either at work or are temporarily absent from their jobs, so employment is a broader measure of attachment than being at work. Given this, employment smooths out some of the volatility seen in the at work outcome, but may be misleading on the impact of the pandemic for individuals who are using vacation or sick leave to allow them to stay home with children. Finally, we also analyze the number of hours worked during the reference week, conditional on being at work. This allows us to observe whether workers' availability was affected, even if work was not entirely precluded by the need to provide childcare.

An important issue arising with the CPS survey during the pandemic is that the BLS has reported that some respondents may have been misclassified as employed but absent from work instead of unemployed (Bureau of Labor Statistics 2020b). Such misclassification could influence our employment outcome, but our "at work" outcome is not affected. This is an added benefit to our use of this outcome, even if (as we noted above) the primary reason for our interest in this variable was based on the context of our analysis, not considerations of data issues.

We study these outcomes of interest using three variations in specifying treatment and control groups. These depend on the ages of respondents' children (or lack of children) and whether a grandparent also lives in the household (that is, a parent of an adult and grandparent of a young child needing care). In the first variation, which we call research design 1, individuals with children under age 13 ("young children") are the treatment group. The rationale for this cutoff is that such children are less likely to stay home alone while schools are closed, and previous research has suggested the labor supply effect of children ends by the time they are 13 (Angrist and Evans 1998). Respondents without young children are then taken as the control group, since they are less likely to be constrained in supplying labor by the need to provide childcare. Formally, for this part of our analysis we define the dummy variable, *treat*, to differentiate these groups, where *treat* = 1 if a worker's youngest own child is under 13 years old, and *treat* = 0 otherwise.[10]

Our second approach of defining treatment and control groups – research design 2 – narrows the population of study to only those who have a young child. To separate individuals constrained by childcare needs from those who are not, we use the presence of older children. We reason here that older children – teenagers and very young adults – can help provide childcare while schools are closed. Thus, we define the control group for research design 2 (*treat* = 0) as individuals whose oldest own child is 13 to 21 years old. In contrast, the treatment group

[10] Since all relationships are not made clear in CPS data, there might be some cases of own children that are not identified in the data. We address this via a post-hoc robustness check discussed below.

should not have an older child to help provide childcare, implying more childcare restrictions. Therefore, given the definition of the control group, we consider treated individuals (treat = 1) as those whose eldest own child is *not* 13 to 21 years old.[11]

Our third approach, research design 3, further restricts our sample to those with young children but whose oldest children are *not* 13 to 21 years old. That is, the people who fall into both treatment groups for the first two research designs. In this case, we use the presence of a parent of the worker (grandparent of the child needing care) to define the groups. Since a worker without a parent nor an older child to provide childcare for the young child is constrained in his or her ability to work, individuals in this situation form our treatment group. Conversely, those who have parents in their houses, who could provide care for the workers' children, form the control group. Thus, for this part of our study we define *treat* = 1 if a worker does not have a parent living with him or her, and *treat* = 0 otherwise. This approach has an important weakness compared to the first two since the share of the sample with a parent in the house is only about six percent. This results in less precision, but we argue this research design still provides a helpful complement to the other two approaches in our analysis.

To implement these research designs, we use the following econometric model:

$$y_{it} = \sum_{j=Jan\ 2018}^{Jan\ 2020} \beta_j treat_i \times \mathbf{1}_j(t) + \beta_{Feb\ 2020} treat_i + \sum_{k=Mar\ 2020}^{June\ 2020} \beta_k treat_i \times \mathbf{1}_k(t) + \alpha' X_{it} + u_{it}. \quad (1)$$

Here *i* and *t* index CPS respondents and month, respectively, and y_{it} represents one of the three outcomes of study discussed above. As already noted above, the treatment group identifier is

[11] Note that this leaves the possibility that an individual with an oldest child who is above 21 and a middle child who is 13 to 21 could be included in the treatment group. We address this via a post-hoc robustness check we discuss below.

represented by $treat_i$, while indicator function $\mathbf{1}_m(t)$ identifies observations for month m. Finally, X_{it} is a column vector of controls, all implemented as sets of dummy variables, α is a column vector of parameters for those controls, and u_{it} is the error term.[12]

We estimate several versions of the above model for each research design and outcome combination. These include weighted and unweighted versions of the model, with the unweighted version representing our preferred approach (as stated in our pre-specification) given its relative ex-ante efficiency. Within the weighted and unweighted categories, we estimate three versions of the model. The first version has minimal controls, with only a set of year-month dummy variables included, while the second adds state dummies.[13] Finally, the last adds dummy variables for gender, age, race, marital status, metro-area status, CPS month-in-sample, veteran status, foreign/domestic nativity, Hispanic ethnicity, education, and disability status. Additionally, to investigate whether the effect of the pandemic response differs by gender, we estimate each of our model variations for both men and women separately, in addition to the combined sample.

The primary coefficients of interest for our analysis are the βs for the months of March 2020 and after. These represent the difference between the treatment group and control group (treat – control) relative to the difference that existed in February 2020, the final month before societal responses to COVID-19 began occurring. In determining our post-period, some judgement was necessary since the national response began occurring in March 2020. Most

[12] All of our regressions were estimated using Stata version 13.1 via the reghdfe command, with standard errors clustered by state (StataCorp 2013; Correia 2016; Bertrand, Duflo, and Mullainathan 2004).

[13] Our pre-analysis plan specifies for the second regression to add calendar-month dummies, but these are redundant given the year-by-month dummies already included.

schools in the country were formally closed by state-level orders the week beginning March 15th, though some districts closed sooner than that. The March CPS survey took place from March 8th through the 14th, so there is reason to think it would miss the full effect of the virus response. This point is underscored by the resulting unemployment rates reported by the Bureau of Labor Statistics (BLS) based on the CPS surveys. For March, the BLS reported a rate of 4.4 percent, almost a one percentage point increase from 3.5 percent in February (Bureau of Labor Statistics 2020a). This suggests that some early effects of the pandemic had begun to appear by the time of the March survey, but they were much smaller compared to the measured impact for April, when the BLS reported a rate of 14.7 percent (Bureau of Labor Statistics 2020b). Accordingly, we consider April to be the beginning of the full post-period of our analysis, but our model measures the effect in March as well, representing the very early effects of the pandemic. Graphs reporting our estimates identify both March and April for clarity.

For all of our models, the underlying identification assumption necessary to identify the labor supply effect on parents is that the labor demand drop that occurred during the pandemic was the same between our treatment and control groups. We provide evidence supporting this assumption in two forms below: we show a long period of similar time trends before the start of the pandemic and provide results from new questions that asked directly during the crisis about how employer demand for workers and respondents' job search efforts were affected.

Post-Hoc Analyses

We perform several post-hoc analyses that are not specified in our pre-analysis plan to provide insight into the character and robustness of our results. To determine if the differences we find between treatment and control groups could be driven by industries or occupations, we estimate an additional version of our model that includes industry and occupation fixed effects.[14] To check whether imbalance by age of young children needing care could be driving our results, we also estimate a version of our model with dummy variables for the age of the youngest child for our second and third research designs, in which the samples are limited to adults with young children.

In addition to adding the above controls, we also perform our analysis using redefined treatment and control groups. In our main analysis, these are based on variables created by IPUMS CPS identifying respondents youngest and eldest own children in the household and their ages. However, the CPS survey does not conclusively identify all relationships between individuals in households, and the use of youngest and eldest children overlooks other children in households of more than two. To address these issues, we redefine the groups on the basis of ages for all children in a household. Hence, for research design 1, the treatment group is those in a household with a child under 13-years-old, while the control is those who are not. In the second design, the groups are defined on whether any child in the household is 13- to 21-years-old. Moreover, the samples for the second and third research designs are limited using these alternative bases for identifying children in the household.

In another robustness check, we examine whether the pandemic's impact on CPS response rates could be influencing our results. The Bureau of Labor statistics has noted that the response rate for respondents of the CPS survey has been dramatically lower since March. In April 2020, the overall response rate was 70 percent, 13 percentage points less than April 2019 and 12 percentage points lower than February 2020 (IPUMS CPS 2020). The fall in response rate

[14] A fixed effect is also included for those that do not have an occupation or industry in the data.

is driven by the Census Bureau dropping in-person interviews beginning in March. These in-person interviews usually occur for households just entering or re-entering the sample (months-in-sample one and five), while households after those points are interviewed by phone. As a result, April response rates were lowest for the first two months after entering or re-entering the sample: 47, 64, 69, and 73 percent for months-in-sample one, two, five, and six, respectively. For those in months-in-sample three, four, seven, or eight, however, April response rates were much closer to normal: 76 percent in months three and seven and 78 percent in four and eight (IPUMS CPS 2020). To check for the effect of the low response rates on our results, we re-estimate the variations of our model using only data for months-in-sample three, four, seven, and eight. Differences in the estimates for this variation of our analysis from our main ones would suggest the low response rates affect our main results.

To examine heterogeneity in our estimates, we also report results obtained by limiting our sample by demographic characteristics, including marital status, race, education level, and setting of residence. To facilitate presentation of these results, we use a standard DD (non-dynamic) version of equation (1) that produces only one post-period coefficient estimate per regression, given by

$$y_{it} = \beta_1 treat_i \times post_t + \beta_2 treat_i + \alpha' X_{it} + u_{it}. \tag{2}$$

Here $post_t$ is a dummy indicating the post-period, April 2020 or later, and other variables are as defined above. Since we are not estimating a separate coefficient in this model for each month, and the March survey potentially only reflects the very early stages of response to the pandemic, we drop data for the month of March to avoid diluting our estimates for the full post-period effects.

Finally, to show how parental labor supply was affected through the summer and into the fall, when some schools began opening again, we also produce estimates of equation (1) using data through November of 2020. Though our pre-analysis plan stated that we would possibly include data beyond June of 2020, we treat these results as post-hoc since we did not specify a definitive end date.

New COVID-19 CPS questions

As part of the May survey, six new questions were added to the CPS questionnaire that are intended to help measure the impact of the COVID-19 pandemic. We use three of the new questions that provide context to our main results. Our analysis of these new variables was fully pre-specified, except for one post-hoc specification we discuss at the bottom of this sub-section. These questions ask the following:

1. "At any time in the LAST 4 WEEKS, did you telework or work at home for pay BECAUSE OF THE CORONAVIRUS PANDEMIC?"
2. "At any time in the LAST 4 WEEKS, were you unable to work because your EMPLOYER CLOSED OR LOST BUSINESS due to the coronavirus pandemic?"
3. "Did the coronavirus pandemic prevent you from looking for work in the LAST 4 WEEKS?"

The first question provides insight as to whether employers differentially provided the two groups work flexibility, or the workers differentially took advantage of it. The second provides some evidence towards whether employer factors, rather than childcare issues, could have influenced the groups differently. Finally, the last question provides additional evidence of whether the groups differed in their ability to look for work, which is relevant since young children presumably could impede parents' search efforts.

Since these are new questions, there is no pre-period data, so we compare each of our treatment and control groups using only May and June post-period data. For each of the three questions, we present regression adjusted group differences ones for our full sample and for men and women separately.[15] We also produce them for our robustness check sample that includes only month-in-sample observations three, four, seven, and eight to examine whether our results were affected by low survey response rates due to the pandemic.[16]

Regression estimates are based on the following model:

$$y_{it} = \beta_{May\ 20} treat_{it} \times \mathbf{1}_{May\ 20}(t) + \beta_{Jun\ 20} treat_{it} \times \mathbf{1}_{Jun\ 20}(t) + \alpha' X_{it} + u_{it}. \quad (3)$$

We include the same set of controls as in our primary analysis and again report standard errors clustered at the state level. The outcomes, y_{it}, are dummy variable versions of the answers to the three questions, where yes if coded as one and no as zero. The primary coefficient estimates of interest from this model are the βs for the months of May and June, which represent the (regression adjusted) differences between the treatment group and control group (treat – control).

The question about teleworking (question number 1, above) was only asked of employed individuals, but in our analysis of this variable we limit the sample to only those who were at work. This is to be consistent with our main analysis of hours worked, where we restrict our sample to those who were at work, not those who were employed. The question about being

[15] As per our pre-specification, we also estimated unadjusted, group sample average differences, but we omit them from the paper to save space. They are available from the authors on request. We present sample averages for each variable and for each treatment and control group for May and June combined in Table 1.

[16] This analysis was pre-specified for these questions even though our use of this sample was not pre-specified for our main analysis. This is due to the data for the new COVID-19 questions being released later than the data for the main analysis. During the delay we recognized that the response rate had been affected and added this sample to the plan, which could not be done for the main analysis since that data was already available.

prevented from looking for work (number 3 above) was only asked of those not in the labor force, and so our sample for that question is limited to those respondents. Only the second question, about being prevented from working by COVID-19, was asked of everyone in our sample.

We perform one post-hoc analysis for these COVID-19 outcomes, focusing on the teleworking question. Teleworking opportunities could be driven by specifics of an individual's job, since not all occupations and industries are equally amenable to remote work. To investigate whether this possibility influences our results, we estimate a model that includes fixed effects for industry and occupation.

Results

Before discussing the details of our estimates, we note here that our use of event-study models for the basis of our analysis tends to obscure an important empirical fact of the pandemic. As the sample averages in Table 1 show, the pandemic created a substantial drop in the likelihood of being at work or employed in our sample, with workers being about seven percentage points less likely to be at work in the post-period as compared to the pre-period. As we discuss our results, the reader should be aware that, at all times, our results are relative to those that have less needs for childcare.

Keeping the above in mind, we turn to Figures 1 through 3, which plot our estimates for our preferred specification of equation (1) that includes full demographic controls based on our full sample. These results are also presented numerically for March 2020 and onward in Table 2, along with estimates for women and men separately.[17] In the plots, the shaded area represents 95 percent confidence intervals and the green dashed and solid lines indicate March and April 2020, respectively. Figures are grouped by outcome, presenting results for all three research designs together.

The figures show that, despite that the treatment and control groups are based on differences in household composition, pre-period trends are reasonably parallel across outcomes and research designs. Most of the small handful of statistically significant (at five percent) differences between groups in the pre-period occur for the at work outcome (Figure 1), where a slight decreasing trend is exhibited in the four months immediately leading up to the pandemic start in research design 1. Given this trend is reversed upon the start of the pandemic, we do not consider it to be an influence on our findings. Hence, we find the pre-trends overall suggest our control groups provide credible comparisons for our treatment subjects.

As we discussed above, our expectation was to find the pandemic caused a negative shock on the labor supply of parents with young children relative to those without.[18] Our unweighted main results in Figures 1 to 3 and Table 2, however, suggest this negative shock did not occur. On the contrary, some estimates suggest parents of young children were more likely to be at work after the onset of the pandemic. Our full sample results for research design 1 on being at work suggest parents of young children were about one percentage point more likely than those without in April (p=0.068), May (p=0.037), and June (p=0.070). Research design 2

[17] Appendix Table 1 presents our results from Table 2 for our full sample along with estimates for pre-period months September 2019 through January 2020. Full results for all analysis variations are available from the authors upon request.
[18] This expectation was included in our pre-analysis plan (Barkowski, McLaughlin, and Dai 2020a).

estimates put the increase at about two percentage points for the same months (p=0.020, 0.035, and 0.12), and for March (p=0.022) also, despite that the pandemic response was still at its early stages. Research design 3 returns similar, positive point estimates for April, and May, and a positive but smaller estimate for June, but with much larger standard errors that result in zero-effect null hypotheses not being rejected for all three months. We find results for employment that are substantially similar to these for being at work, while for hours worked we obtain positive point estimates in most cases but only one instance of an estimate exhibiting a conventional-level of statistical significance: April for research design 2 (10% level, p=0.067).

Our separate estimates for men and women on the outcomes of being at work and employed do not suggest our full sample results are driven by only one gender. As panels B and C show, our estimates for research design 1 appear to be driven by men but those for design 2 predominantly reflect women. In contrast, for the hours worked outcome we find some evidence via research design 1 that women with young children worked more than those without in May and June by more than half-an-hour of work per week (p=0.003 and 0.038), a result not reflected in the estimates for men. However, for the other two research designs we do not obtain any statistically significant effects for either men or women. On net, we find limited evidence that gender response differed, but our results by gender do show clearly that the pooling of men and women does not obscure any large negative labor supply shocks for either gender.

A corresponding set of estimates to those in Table 2, but calculated using sampling weights, is presented in Table 3. Overall, our weighted results largely conform to our unweighted ones, but these estimates tend to be larger in magnitude and often exhibit higher levels of statistical significance. Additionally, the weighted results magnify the partial evidence in Table 2 that hours worked may have increased for some female parents of young children. As Table 3 shows for the hours worked outcome, we obtain estimates for May and June from research design 1 that are positive and statistically significant (p=0.066 and 0.015) for the pooled sample. These estimates suggest parents of young children worked about half-an-hour more per week than parents without such children, conditional on working. The breakdown of these results by men and women shows that they are driven by women, whose estimates are also statistically significant (p=0.0004 and 0.016), while those for men are not (and are, in fact, negative in March through May). The significant estimates for women suggest their weekly number of hours worked exceeded those of women who did not have young children by almost an hour. Additionally, the weighted estimate for April from research design 2 is also positive and significant (p=0.093), though in this case the result seems to have been driven equally by men and women. We note, though, that the hours worked outcome for research design 3 here is also where we find one of our very few statistically significant (p=0.071) *negative* estimates for women for the month of June. This estimate suggests women worked an hour-and-a-half less due to childcare that month.

Taking results from both Tables 2 and 3 into consideration, for the outcomes of being at work and being employed, we do not find evidence that the response to the pandemic was different for men and women. We also do not find evidence overall that hours decreased, with some evidence suggesting that, conditional on being at work, women with young children responded to the pandemic by working about a half-hour to an hour longer than women without young children. This result is not evident in all research designs, though, and is counterbalanced to some degree by the negative estimate we noted above.

In addition to the estimates for our preferred specification that are reported in Tables 2 and 3, we also estimated model variations with fewer controls. Across these variations we obtain similar results to those found in Table 2. Full results are available upon request, but Appendix Table 2 reports estimates from when our model includes minimal controls.

Post-Hoc Analyses Results

Tables 4 through 7, Appendix Tables 3 and 4, and Appendix Figures 1 to 6 present results from our post-hoc analyses of our main labor supply outcomes. The first of these, Table 4, reports estimates from models with additional fixed effects for industry and occupation added, controls which were not pre-specified.[19] Their addition shows whether differences in the composition of the treatment and control groups across industries and occupations could be hiding negative labor supply shocks. Here we find estimates for being at work or employed that are typically smaller than in our preferred specification, particularly for research design 2, which are much smaller and not significant. Our estimates for hours worked, however, are quite similar to our main results, even when broken down by gender. Across the table, the most notable difference from our main results is our estimates for men being employed based on research design 2, which are negative and statistically significant in May and June. These are a stark contrast from the positive and significant estimates produced by research design 1 for the same outcome. Given this inconsistency, and the lack of any other negative and significant estimates, the evidence overall from Table 4 does not suggest childcare needs caused a negative labor supply shock for parents during the COVID-19 crisis. Instead, they show that some of our findings of positive effects come from comparison of treatment and control individuals across

[19] Workers' industries and occupations could be endogenously influenced by the onset of the COVID-19 pandemic, which is why these controls were not included in our pre-analysis plan. Our unexpected results, however, prompted us to consider the influence of industries and occupations. Nevertheless, the threat of endogeneity should be considered when interpreting these results.

industries and occupations.[20] One possible explanation for this is parents of young children may have been working in jobs relatively more sheltered from the impact of the pandemic, resulting in them being less likely to be away from their work.

A second set of results from adding controls that were not pre-specified are reported in Appendix Table 3. These estimates come from models that include fixed effects for the age of each parent's youngest own child. This addresses potential concerns that the treatment and control groups could have important differences in child ages that influence their childcare needs. We calculate these estimates only for research designs 2 and 3 since some control group individuals in research design 1 do not live with or have a child, and we find results that are very similar to our main estimates, undermining differences in youngest child ages as a potential explanation for our results.

Table 5 reports results when we redefine the treatment and control groups using the presence of any children of the relevant ages in worker households instead of the IPUMS constructed variables used in our main analysis. In this case, for the outcomes of being at work and being employed, we find that our estimates are smaller in this alternative framework in research design 1, but similar in design 2, and much larger and positive in research design 3. For hours worked, the alternative approach produces estimates that are typically more positive and more likely to be statistically significant. Across all regressions, we obtain two statistically significant, negative estimates, with both occurring for men. One is found for our employment result for June from research design 2 (p=0.096). This mimics our estimate for the same design,

[20] Movement of workers between industries and occupations in response to the pandemic could also be part of the explanation here. Whether such movement occurred would be an interesting question for future research.

outcome, and month in Table 4. The other is for May in research design 1 for hours worked (p=0.023). These negative estimates are not consistent across research designs, as is the case for all of the few instances of negative effects we obtain in this project. So, despite these two negative estimates, the overall impression of these results is not drastically different from our main results, and there is little to suggest a negative labor supply shock response to the pandemic.

Next, we consider whether the reduced response rate to the CPS survey could be influencing our results. To preface this, we note that in our table of sample averages, Table 1, we report both pre-period and post-period sample means.[21] Review of these shows that despite the response rate changes, there is relatively little change across periods, suggesting the types of individuals represented in the survey over time are not changing meaningfully. Nevertheless, we go further and re-estimate equation (1) using individuals with higher values of the month-in-sample variable (as described above), which had higher response rates. These results are found in Table 6 for our full sample. Here we obtain point estimates that, in most cases, are similar to our main results. Differences include smaller, but still positive, estimates from research design 1 for the at work and employment outcomes, and estimates that are nearly all not statistically significant across all designs and outcomes, even when point estimates are similar or larger, due to larger standard errors. Overall, though, like our main results, these estimates do not provide any evidence for a negative labor supply effect.

[21] These are not calculated using sampling weights for two reasons. First, our goal is to compare the various sub-samples themselves for similarity, not the populations from which they are drawn (which the weights are intended to enable). Second, to the extent that response rates *do* affect the sample, it is not clear that the adjustments to the weights made by the Census Bureau for demographic factors are appropriate or accurate given they were not developed for use in periods of viral pandemic when response rates are significantly affected.

Our next post-hoc analysis examines heterogeneity across demographic characteristics via a simpler version of our main model, equation (2). The results of this analysis are presented in Table 7. To provide a point of comparison for estimates based on sample sub-groups, we report estimates of equation (2) using our full-sample in the first-row. These conform to our main results, suggesting being at work or employed increased from about one (research design 1) to two (research design 2) percentage points. In the following rows of the table, the sample for each regression is limited to the sub-group indicated in the far-left cell of each row. We find most estimates across groups are positive or not statistically significant. Results for white respondents, high school non-graduates, college graduates, and residents in urban areas or those where metropolitan status is unknown are significant and positive in some research designs, suggesting these groups are important drivers of our estimates for the main, pooled sample.

In terms of negative estimates, we find only a few, most notably among single respondents. Via research design 1, we estimate they were less likely to be at work by 1.7 percentage points (p=0.015), a result that appears to be driven about equally by men and women. None of the other estimates for single individuals are statistically significant except in the case of hours worked for women, via research design 3. That estimate is significant at the 10 percent level and suggests single women with young children and no grandparent in the household worked 1.3 hours fewer each week (p=0.078) than single women with young children and a grandparent in the house. The only other statistically-significant (at the 10 percent level) negative estimate we obtain is also for hours worked by black respondents via research design 3, an estimate that suggests they worked almost 2 fewer hours per week, conditional on working (p=0.066). This result is contrasted, however, by the positive and significant estimate we obtain for black individuals via research design 2 that suggests they worked 1.2 hours more (p=0.033).

Considering our results for all sub-groups, we find little evidence of negative labor supply effects. The strongest evidence of such effects is found for single individuals, though this result is not robust across specifications.

We present graphical results for our final post-hoc analysis on our labor supply outcomes in Appendix Figures 1 through 6, with numerical results for these plots presented in Appendix Table 4. These results come from extending our sample through November 2020 and estimating a version of equation (1) that allows for the longer post-period. We include results for our full sample and for men and women separately. These findings are similar to our results through June only, though they suggest positive differences in work between groups during the early summer months faded towards zero in the fall. We do not obtain any statistically-significant negative estimate for any month, research design, outcome, or gender in these results, nor do we observe any systematic gender difference.

New COVID-19 CPS Question Results

In Table 8, we report estimates of equation (3) where the newly added survey questions related to the COVID-19 pandemic are used as dependent variables. The first outcome is whether the respondent reported having worked remotely ("teleworked") because of the pandemic, a question that can help us understand the mechanism behind our results. Here we obtain positive estimates in all cases but one, where a positive means parents with childcare responsibilities were more likely to report teleworking. Most point estimates fall in the one to three percentage point range, with our sample in Panel D, which is based on the highest response rate months-in-sample, producing the largest estimates overall. Additionally, and perhaps surprisingly, we find positive and significant estimates for both women and men, depending on the research design. Moreover, as we report in Appendix Table 5, we obtain very similar estimates from a post-hoc specification that included occupation and industry fixed effects. Given that our results for the labor supply outcomes suggested childcare did not reduce how much parents worked, this result offers a possible explanation: that employer flexibility towards remote work helped parents continue working despite their increased responsibilities for childcare. This finding is also consistent with the stylized fact that parents with children were more likely to work at home before the pandemic (Woods 2020).

In addition to providing evidence on the mechanism behind our results, these new questions also help address the fact that the main outcomes we study – being at work or employed and hours worked – are equilibrium outcomes, and reflect the interplay of both the labor supply and demand curves. The next two outcomes allow us to provide some direct measurement on the movements of these underlying curves. The first of these outcomes, asked of all respondents, inquires whether respondents were unable to work because their employers lost business due to COVID-19. This question provides evidence on the nature of labor demand, and helps us address a theoretical possibility that our results could still reflect a reduction in the labor supply curve despite the fact that we do not find a reduction in our labor outcomes. While it is clear that overall labor demand fell in the pandemic, if labor demand for parents with childcare responsibilities rose (that is, shifted rightward in a standard labor supply and demand model) *relative* to that of those without childcare responsibilities, then the labor supply curve could shift to the left (that is, a reduction in labor supply) and still leave the equilibrium quantity of labor unchanged or positive.

In our analysis, negative estimates would be evidence towards the relative labor demand curve of employers shifting rightward for those with childcare needs. We find, however, mostly small and insignificant estimates for being unable to work. Those estimates that are significant are positive in all cases. This suggests that labor demand either shifted equally for those with and without childcare needs, or downward (leftward) for those with childcare needs relatively more than for those without. This result implies our findings of null or positive effects on the equilibrium in labor quantity are not consistent with a reduction in labor supply.

The final outcome is whether respondents claim they are prevented from looking for work by the pandemic. This question represents a direct measure of the labor supply curve for the part of the market that was out of the labor force (since it was only asked of those who were not in the labor force). In this question, positive estimates mean those with childcare responsibilities were more likely to claim they were prevented from looking for work by the pandemic than those without. Thus, a positive estimate implies a reduction in labor supply for those with childcare needs. Unfortunately, for this outcome we obtain results that are contradictory across research designs. In research designs 1 and 2 we find mostly negative estimates, most of which are statistically significant at various levels. In research design 3, however, we find all positive estimates, some of which are significant. Moreover, estimates from research design 3 are very large in magnitude, with the significant ones suggesting those with childcare needs were about 10 percentage points more likely to have been prevented from looking for work. Thus, our estimates for this outcome are not consistent, but suggest some sub-populations may have experienced a relative reduction in labor supply, while others experienced an increase. Combined with the results for the unable to work question, which was asked of many more respondents, we interpret the overall evidence for the full labor market from these direct measures of the labor supply and demand curves as suggesting little change in labor supply *on net* for parents with childcare responsibilities.

Discussion

The COVID-19 pandemic created an extraordinary labor market environment in which social distancing followed by government orders to stay home induced a massive shock to labor demand. As it unfolded, concerns were raised that the closing of schools and daycare centers across the country would compound the labor market shock for parents of young children, who suddenly had to provide childcare for their children, and cause them to reduce their labor supply. The school closings indeed created a severe childcare concern, as Sevilla and Smith (2020) report that families with young children in the UK increased their childcare provision by about 40 hours per week after the pandemic onset. Nevertheless, we find that the concerns about negative labor supply shocks were unfounded, as we fail to find much evidence of a labor supply reduction for parents of young children of either gender. Instead, we find some evidence that they were more likely to work than those without young children or those that had other childcare options in their households. We estimate that parents with young children were about one percentage point more likely to be working than adults without young children in their households after the pandemic began (based on research design 1). Per our CPS data, about 46.7 million adults in the country have young children, so our one-percentage-point estimate corresponds to about 467,000 workers. We also find that among workers with young children in their households, those without a teenager oldest child are more likely to be at work by about two percentage points than those who do have a teen. Again, per our CPS data, the population of parents of young children whose oldest child is not a teen is about 34 million people, which implies our estimate of two-percentage-points corresponds to 680,000 parents nationwide. Taking these together, roughly half a million *more* parents were at work after the COVID-19 pandemic began as compared to those with fewer childcare obligations.

Additionally, we find that men and women did not have systematically different responses to the pandemic for two of our outcome variables, being at work and being employed. While surprising, this is consistent with findings that gender differences in childcare provision narrowed at least slightly during the COVID-19 pandemic (Sevilla and Smith 2020). For our third outcome, the number of hours worked conditional on being at work, we find some evidence that women of young children may have worked nearly an hour more per week in response to the pandemic. While it is surprising parents did not substitute away from hours worked given their sudden additional child care demands, this finding could be rationalized if we consider that children could have reduced the productivity of parents – and women in particular – resulting in them working more hours to complete assigned tasks. This interpretation is also consistent with Gibbs, Mengel, and Siemroth (2021), who show Asian IT workers worked more hours at lower productivity-levels when they had children at home after the onset of the pandemic. Nevertheless, we note here that this finding of increased working hours was not consistent across our three research designs, though it was consistent across our post-hoc robustness checks.

In post-hoc sub-group analysis, the most evidence we found for possible negative effects came from single parents of young children, where we found they were about 1.5 percentage points less likely to be at work than single parents without young children. This effect was present for both men and women, but it was not consistent across research designs. This inconsistency was also found by Kalenkoski and Pabilonia (2020). In specifications that differed in the way they controlled for seasonality, they estimated a large negative effect on single fathers' employment in one specification but none in another. Their estimates for single mothers' employment were not statistically significant.

Our main findings run counter to our pre-stated expectations but are consistent with some studies of the relationship between the cost of childcare and labor supply that have found little evidence of effects (Lundin, Mörk, and Öckert 2008; Fitzpatrick 2010; Havnes and Mogstad 2011; Fitzpatrick 2012). Our results are also broadly consistent with some findings in the recent literature on childcare during the COVID-19 pandemic: Rojas et al. (2020) and Heggeness (2020) find no effect on unemployment, while Amuedo-Dorantes et al. (2020) find no effect on employment, and Russell and Sun (2020) find no effect on labor force participation. Moreover, Heggeness (2020) also finds some evidence for women that hours worked increased during the pandemic.

We argue our findings are suggestive of the importance of employer responses during the pandemic to increase employees' flexibility to work at home, and of the critical role informal sources of childcare play in parents' employment. One of the ways employers increased flexibility during the pandemic was to allow employees to work from home, and we showed that those with childcare needs were more likely to report working remotely during the pandemic. Moreover, Brynjolfsson et al. (2020) found that about half of employees in the USA who were employed before the pandemic were working from home as it unfolded.[22] Other dimensions of flexibility are possible, though, such as allowing workers to complete job tasks during hours outside the typical day schedule. Important insights could be gained by future research, perhaps with time-use surveys or mobility data, into the dimensions of work flexibility during the pandemic.

[22] This includes individuals who were working from home before the pandemic. Brynjolfsson et al. (2020) also report that more than a third of those who commuted to work before the pandemic were working from home after.

Finally, while we argue that labor supply did not fall for parents with childcare needs, this finding does not mean their welfare was not negatively affected by the sudden closure of schools and childcare centers. Instead, the fact that working parents can absorb an additional 40 hours per week of additional childcare in their schedules without a major labor supply shock attests to how important parents' jobs are to them. Policies that assist parents obtain childcare or encourage work flexibility could, therefore, potentially improve the welfare of parents significantly. Research into the potential costs of such policies could offer important insights.

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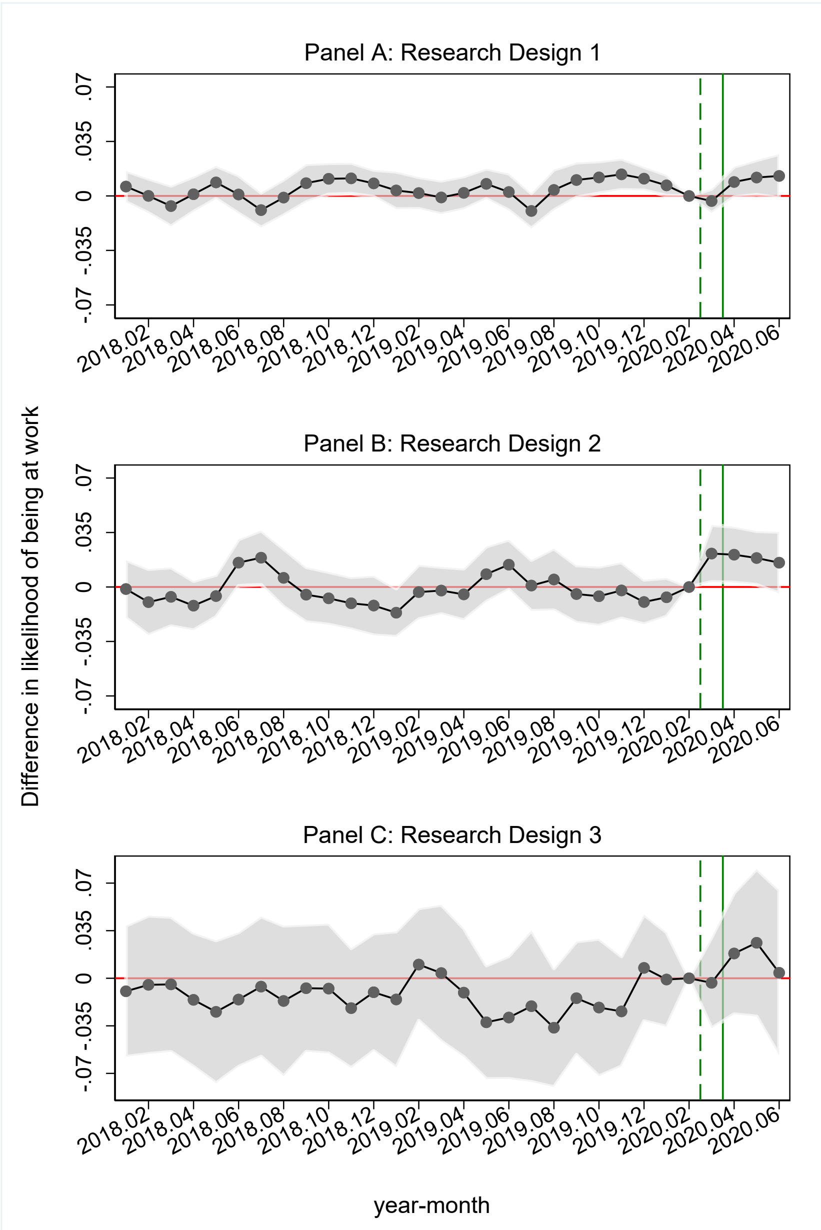
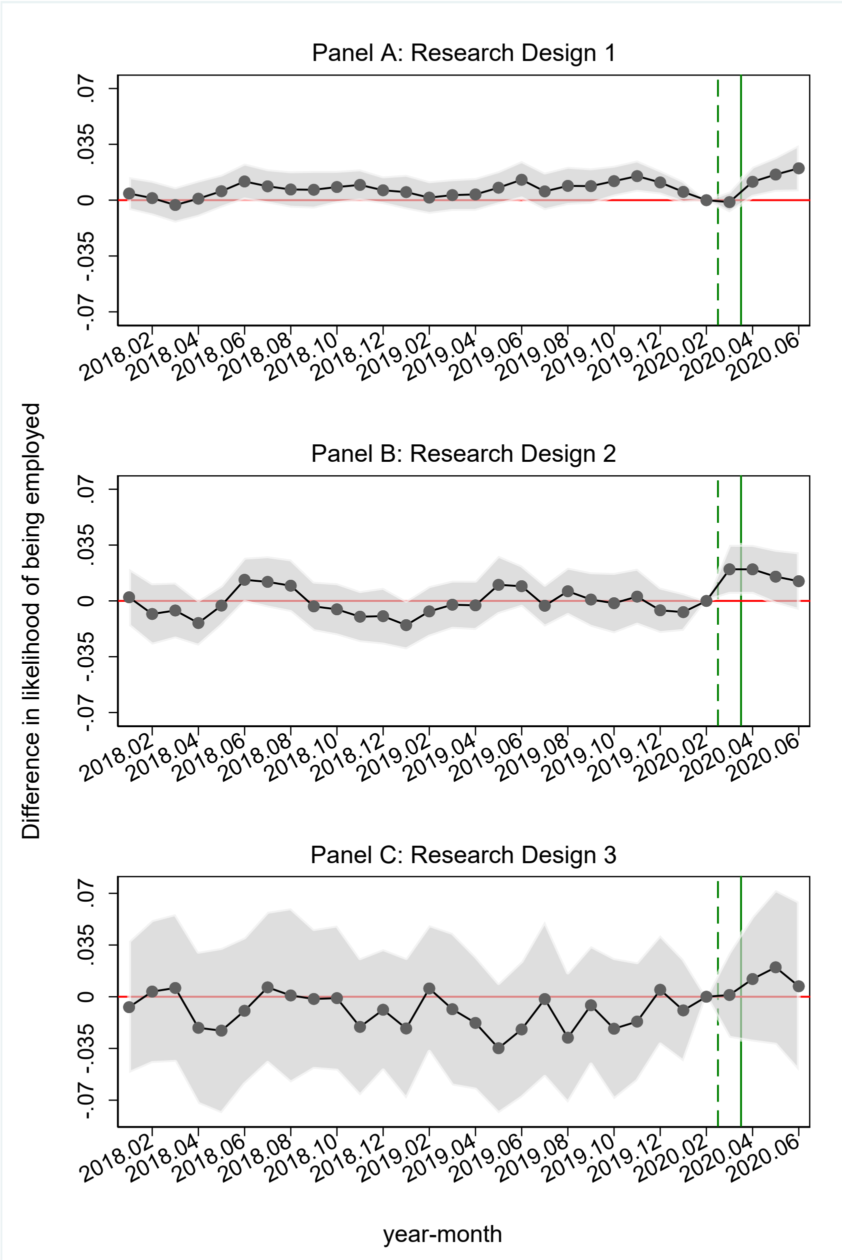
Figure 1: Difference in likelihood of being at work (treated group minus control)



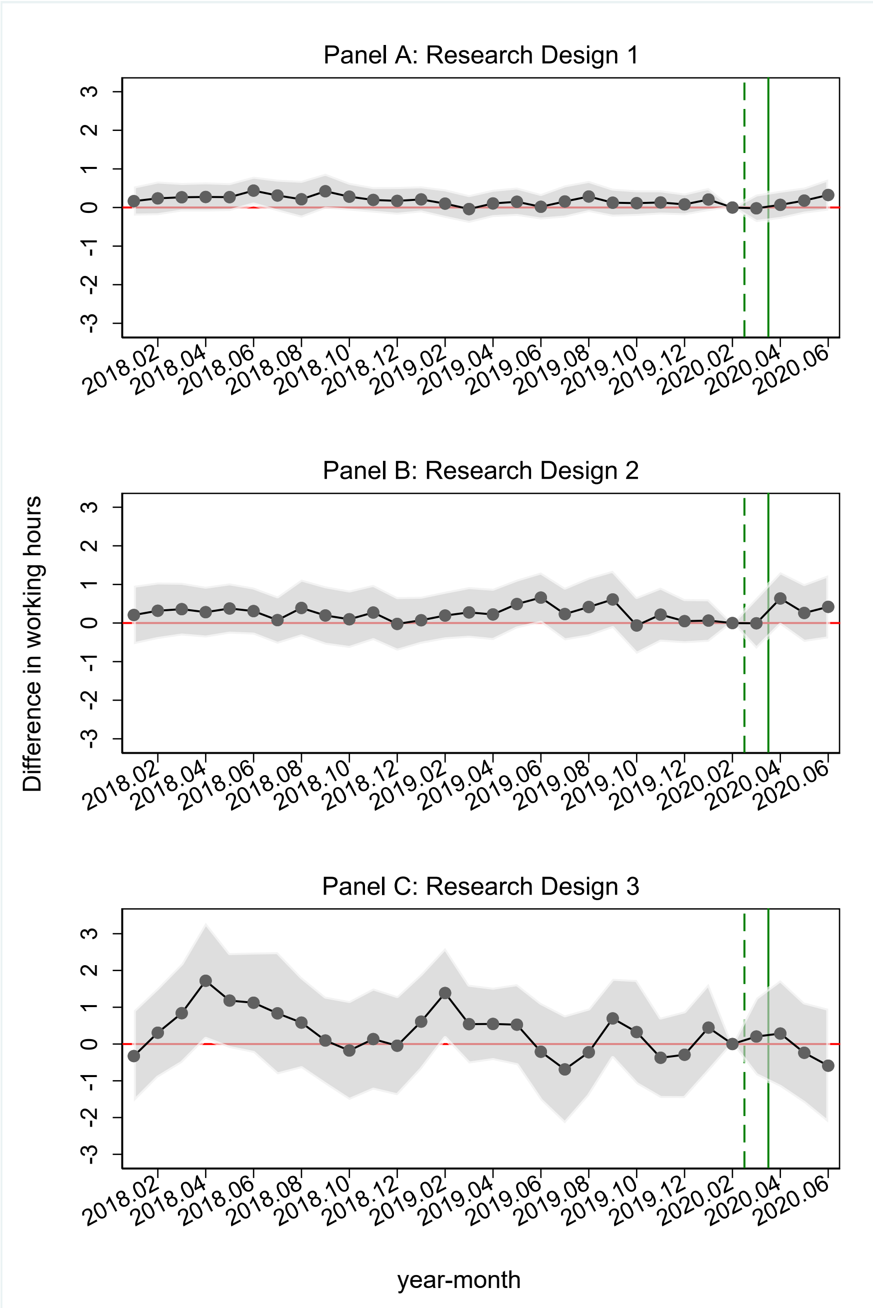
Notes: Shaded area represents 95% confidence intervals based on state-level clustered standard errors. Dashed, green vertical line indicates early pandemic stages (March 2020). Solid, green vertical line represents the start of the post-period (April 2020). Sample is basic monthly CPS for Jan 2018 – June 2020, including non-military, non-student respondents ages 21—59. Estimation performed without sampling weights while including controls for year-month, calendar-month, state, age, race, ethnicity, gender, marital status, education, metropolitan area, month-in-sample, veteran status, foreign birthplace, and disability. “At work” means individuals are employed and actively working. Research design 1 defines treat=1 if a worker’s youngest own child is under 13 years of age, and 0 otherwise. Research design 2 defines treat =1 if a worker’s eldest own child is not 13 to 21 years of age, and 0 otherwise. Research design 3 defines treat =1 if a worker does not have a parent living with him or her, and 0 otherwise.

Figure 2: Difference in likelihood of being employed (treated group minus control)



Notes: Notes to Figure 1 apply, except employed workers include those temporarily absent from their jobs (e.g., sick, vacation).

Figure 3: Difference in hours worked (treated group minus control)



Notes: Notes to Figure 1 apply, except hours worked are conditional on being at work.

Table 1: Selected sample averages

Variables	Period	Research Design 1 Treatment	Research Design 1 Control	Research Design 2 Treatment	Research Design 2 Control	Research Design 3 Treatment	Research Design 3 Control
At work	Pre	0.769	0.764	0.768	0.774	0.773	0.687
	Post	0.702	0.690	0.707	0.690	0.713	0.607
Employed	Pre	0.799	0.787	0.799	0.800	0.804	0.712
	Post	0.747	0.727	0.753	0.732	0.760	0.651
Hours at work	Pre	40.276	40.735	40.202	40.471	40.329	37.839
	Post	39.243	39.611	39.145	39.505	39.244	37.321
Age	Pre	37.511	42.921	36.026	41.455	36.250	32.334
	Post	38.014	43.009	36.583	41.750	36.804	33.108
Age of oldest child	Pre	9.626	21.275	7.284	15.848	7.325	6.595
	Post	9.654	21.311	7.267	15.890	7.290	6.899
Age of youngest child	Pre	5.277	19.375	4.362	7.707	4.353	4.502
	Post	5.316	19.313	4.376	7.770	4.362	4.603
Female	Pre	0.550	0.497	0.549	0.552	0.540	0.699
	Post	0.543	0.499	0.544	0.542	0.535	0.689
White	Pre	0.800	0.799	0.800	0.801	0.807	0.686
	Post	0.801	0.800	0.804	0.794	0.812	0.676
Black	Pre	0.093	0.110	0.090	0.102	0.085	0.164
	Post	0.088	0.107	0.086	0.092	0.082	0.162
Hispanic	Pre	0.187	0.138	0.170	0.232	0.165	0.246
	Post	0.170	0.135	0.154	0.210	0.149	0.243
Married	Pre	0.775	0.475	0.766	0.801	0.792	0.333
	Post	0.803	0.482	0.793	0.829	0.820	0.376
Divorced or separated	Pre	0.082	0.151	0.074	0.102	0.068	0.176
	Post	0.072	0.141	0.067	0.085	0.061	0.173
Single	Pre	0.137	0.355	0.155	0.090	0.135	0.479
	Post	0.121	0.360	0.136	0.081	0.116	0.442
High school dropout	Pre	0.092	0.081	0.076	0.134	0.073	0.115
	Post	0.079	0.070	0.062	0.121	0.061	0.086
High school	Pre	0.251	0.301	0.248	0.259	0.239	0.393
	Post	0.235	0.288	0.230	0.247	0.220	0.382
Some college	Pre	0.261	0.266	0.257	0.269	0.255	0.302
	Post	0.255	0.268	0.251	0.267	0.247	0.320
College	Pre	0.243	0.236	0.255	0.210	0.262	0.138
	Post	0.260	0.247	0.275	0.220	0.283	0.148
COVID-19 related:							
Unable to work due	Post	0.095	0.098	0.094	0.096	0.093	0.103
Teleworked	Post	0.181	0.165	0.187	0.165	0.191	0.127
Prevented looking for work	Post	0.061	0.075	0.062	0.061	0.061	0.063
Observation Count	Pre	447,851	998,343	325,364	122,487	306,760	18,604
	Post	54,589	124,900	39,479	15,110	37,124	2,355

Notes: Sample and research design definitions described in the notes to Figure 1. Calculated without sampling weights. Post-period includes March to June 2020. Samples for hours at work and child ages are limited to those at work and those with children. COVID-19 related variables are available only for May and June 2020.

Table 2: Regression adjusted differences between treatment and control groups

Dependent variable:	At Work			Employed			Hours Worked		
Research Design	*1*	*2*	*3*	*1*	*2*	*3*	*1*	*2*	*3*
Panel A: All	n=1,625,683	n=502,440	n=364,843	n=1,625,683	n=502440	n=364,843	n=1,231,552	n=382,952	n=277,680
March 2020	-0.00365	0.0215**	-0.00333	-0.00120	0.0198**	0.00120	-0.0205	-0.00701	0.204
	(0.00381)	(0.00910)	(0.0167)	(0.00326)	(0.00764)	(0.0144)	(0.177)	(0.329)	(0.523)
April 2020	0.00898*	0.0215**	0.0182	0.0115**	0.0198**	0.0120	0.0690	0.637*	0.285
	(0.00482)	(0.00892)	(0.0222)	(0.00468)	(0.00736)	(0.0211)	(0.181)	(0.340)	(0.722)
May 2020	0.0118**	0.0186**	0.0261	0.0161***	0.0152*	0.0199	0.179	0.261	-0.238
	(0.00552)	(0.00857)	(0.0270)	(0.00539)	(0.00835)	(0.0261)	(0.168)	(0.373)	(0.680)
June 2020	0.0129*	0.0156	0.00404	0.0200***	0.0124	0.00710	0.325	0.419	-0.589
	(0.00697)	(0.00984)	(0.0303)	(0.00720)	(0.00899)	(0.0285)	(0.201)	(0.407)	(0.770)
Panel B: Women	n=834,217	n=275,819	n=200033	n=834,217	n=275,819	n=200033	n=580,015	n=179,982	n=129,848
March 2020	-0.00476	0.0345***	-0.00527	-0.00347	0.0307**	0.00392	-0.0789	0.151	-0.257
	(0.00486)	(0.0123)	(0.0215)	(0.00402)	(0.0119)	(0.0200)	(0.171)	(0.372)	(0.866)
April 2020	0.00572	0.0374***	-0.000477	0.00374	0.0390***	0.00107	0.246	0.748	0.638
	(0.00676)	(0.0116)	(0.0257)	(0.00651)	(0.0110)	(0.0245)	(0.260)	(0.497)	(0.876)
May 2020	0.00440	0.0414***	0.0279	0.00744	0.0379***	0.0329	0.685***	0.697	0.114
	(0.00761)	(0.0136)	(0.0325)	(0.00803)	(0.0137)	(0.0330)	(0.217)	(0.535)	(0.882)
June 2020	0.00550	0.0318**	0.0105	0.0130	0.0278*	0.0290	0.570**	0.353	-1.211
	(0.00954)	(0.0134)	(0.0340)	(0.0103)	(0.0139)	(0.0304)	(0.267)	(0.568)	(0.902)
Panel C: Men	n=791,466	n=226,621	n=164,810	n=791,466	n=226,621	n=164,810	n=651,537	n=202,970	n=147,832
March 2020	0.000561	0.00519	-0.00552	0.00302	0.00624	-0.0115	0.0243	-0.140	0.908
	(0.00581)	(0.0122)	(0.0263)	(0.00465)	(0.00844)	(0.0245)	(0.236)	(0.431)	(0.828)
April 2020	0.0149***	0.00108	0.0629*	0.0221***	-0.00281	0.0377	-0.156	0.562	0.210
	(0.00537)	(0.0155)	(0.0364)	(0.00505)	(0.0108)	(0.0326)	(0.260)	(0.381)	(1.260)
May 2020	0.0227***	-0.00970	0.0281	0.0279***	-0.0129	-0.000401	-0.333	-0.0953	-0.293
	(0.00615)	(0.0136)	(0.0412)	(0.00561)	(0.0106)	(0.0394)	(0.242)	(0.475)	(1.154)
June 2020	0.0251***	-0.00492	-0.0181	0.0300***	-0.00733	-0.0391	0.0120	0.447	0.529
	(0.00824)	(0.0146)	(0.0443)	(0.00754)	(0.0117)	(0.0457)	(0.240)	(0.482)	(1.168)

Notes: Estimates of equation (1). State-level, clustered standard errors reported in parentheses. Statistically significant estimates for two-tailed tests at the one, five, and ten-percent levels are indicated ***, **, and *, respectively. Sample and research designs as described in the notes to Figure 1.

Table 3: Regression adjusted differences between treatment and control groups, weighted regressions

Dependent variable:	At Work			Employed			Hours Worked		
Research Design	*1*	*2*	*3*	*1*	*2*	*3*	*1*	*2*	*3*
Panel A: All	n=1,625,683	n=502440	n=364,843	n=1,625,683	n=502,440	n=364,843	n=1,231,552	n=382,952	n=277,680
March 2020	-0.00486	0.0232*	-0.00989	0.000205	0.0226**	-0.00321	-0.0937	0.196	0.158
	(0.00491)	(0.0128)	(0.0200)	(0.00454)	(0.0109)	(0.0151)	(0.231)	(0.378)	(0.527)
April 2020	0.0131**	0.0199	0.0210	0.0139**	0.0197*	0.0160	-0.00622	1.029**	0.369
	(0.00565)	(0.0126)	(0.0271)	(0.00601)	(0.0110)	(0.0233)	(0.200)	(0.407)	(0.770)
May 2020	0.0162***	0.0233**	0.0346	0.0188***	0.0201**	0.0242	0.357*	0.233	-0.388
	(0.00507)	(0.00956)	(0.0335)	(0.00526)	(0.00930)	(0.0301)	(0.190)	(0.355)	(0.849)
June 2020	0.0183**	0.0152	0.0138	0.0234***	0.0162	0.00918	0.594**	0.485	-0.708
	(0.00731)	(0.0121)	(0.0351)	(0.00817)	(0.0110)	(0.0360)	(0.235)	(0.411)	(0.769)
Panel B: Women	n=834,217	n=275,8199	n=200033	n=834,217	n=275,819	n=200033	n=580,015	n=179,982	n=129,848
March 2020	-0.00799	0.0365*	-0.0127	-0.00516	0.0349*	-0.00288	-0.102	0.394	-0.134
	(0.00484)	(0.0190)	(0.0257)	(0.00453)	(0.0184)	(0.0199)	(0.201)	(0.436)	(1.040)
April 2020	0.0138**	0.0440***	0.0105	0.0103	0.0453***	0.0135	0.134	1.027*	0.246
	(0.00682)	(0.0135)	(0.0328)	(0.00782)	(0.0135)	(0.0299)	(0.290)	(0.599)	(0.979)
May 2020	0.00904	0.0539***	0.0553	0.0108	0.0520***	0.0544	0.833***	0.493	-0.279
	(0.00649)	(0.0163)	(0.0375)	(0.00736)	(0.0160)	(0.0355)	(0.218)	(0.508)	(1.076)
June 2020	0.0114	0.0396**	0.0252	0.0172	0.0404**	0.0349	0.778**	0.474	-1.536*
	(0.00907)	(0.0170)	(0.0347)	(0.0112)	(0.0164)	(0.0350)	(0.312)	(0.577)	(0.833)
Panel C: Men	n=791,466	n=226,621	n=164,810	n=791,466	n=226,621	n=164,810	n=651,537	n=202,970	n=147,832
March 2020	-0.000544	0.00712	-0.0217	0.00652	0.00774	-0.0141	-0.120	0.00244	0.558
	(0.00823)	(0.0150)	(0.0307)	(0.00660)	(0.0115)	(0.0270)	(0.292)	(0.435)	(0.857)
April 2020	0.0148**	-0.00749	0.0373	0.0200***	-0.00927	0.0280	-0.162	1.003**	1.049
	(0.00553)	(0.0218)	(0.0428)	(0.00620)	(0.0159)	(0.0302)	(0.302)	(0.413)	(1.281)
May 2020	0.0266***	-0.0131	0.00169	0.0299***	-0.0177	-0.0184	-0.113	-0.00937	-0.129
	(0.00647)	(0.0188)	(0.0521)	(0.00608)	(0.0136)	(0.0420)	(0.242)	(0.506)	(1.131)
June 2020	0.0302***	-0.0153	-0.00647	0.0329***	-0.0141	-0.0375	0.370	0.452	0.755
	(0.00824)	(0.0174)	(0.0557)	(0.00793)	(0.0154)	(0.0534)	(0.267)	(0.549)	(1.155)

Notes: Notes to Table 2 apply except sampling weights are used.

Table 4: Regression adjusted differences between treatment and control groups, with industry and occupation controls

Dependent variable:	At Work			Employed			Hours Worked		
Research Design	*1*	*2*	*3*	*1*	*2*	*3*	*1*	*2*	*3*
Panel A: All	n=1,625,683	n=502,437	n=364,837	n=1,625,683	n=502437	n=364,837	n=1,231,552	n=382,948	n=277,673
March 2020	-0.00116	0.00742	-0.00611	0.000754	0.00521	-0.00185	-0.0267	0.0806	0.207
	(0.00335)	(0.00606)	(0.0114)	(0.00197)	(0.00394)	(0.00850)	(0.163)	(0.332)	(0.526)
April 2020	0.00771*	0.00515	0.0274*	0.00985***	0.00424	0.0222*	0.0612	0.635*	0.570
	(0.00412)	(0.00784)	(0.0144)	(0.00333)	(0.00642)	(0.0128)	(0.172)	(0.358)	(0.729)
May 2020	0.00390	-0.00355	0.0302	0.00752*	-0.00795	0.0265	0.112	0.401	0.301
	(0.00457)	(0.00670)	(0.0204)	(0.00387)	(0.00521)	(0.0165)	(0.162)	(0.386)	(0.679)
June 2020	0.00177	0.000135	0.00935	0.00849*	-0.00391	0.0130	0.307	0.464	-0.294
	(0.00448)	(0.00711)	(0.0156)	(0.00429)	(0.00556)	(0.0136)	(0.187)	(0.387)	(0.781)
Panel B: Women	n=834,214	n=275,805	n=200,016	n=834,214	n=275,805	n=200,016	n=580,010	n=179,966	n=129,829
March 2020	0.000646	0.0112	-0.0121	0.00198	0.00646	-0.00396	-0.0944	0.234	-0.263
	(0.00440)	(0.00732)	(0.0150)	(0.00310)	(0.00597)	(0.0124)	(0.164)	(0.377)	(0.848)
April 2020	0.00979	0.0103	0.00694	0.00811	0.0121*	0.00865	0.214	0.721	0.333
	(0.00616)	(0.00889)	(0.0190)	(0.00497)	(0.00706)	(0.0168)	(0.238)	(0.509)	(0.863)
May 2020	0.00250	0.00579	0.00285	0.00568	0.00148	0.0109	0.552**	0.798	0.0975
	(0.00615)	(0.00852)	(0.0224)	(0.00584)	(0.00703)	(0.0177)	(0.207)	(0.526)	(0.924)
June 2020	-0.000251	0.0122	-0.0118	0.00729	0.00774	0.00611	0.525**	0.337	-1.477
	(0.00579)	(0.00822)	(0.0192)	(0.00591)	(0.00613)	(0.0159)	(0.266)	(0.548)	(0.942)
Panel C: Men	n=791,465	n=226,612	n=164,800	n=791,465	n=226,612	n=164,800	n=651,536	n=202,958	n=147,819
March 2020	-0.00243	0.00168	-0.00466	-0.000336	0.00300	-0.00149	0.0375	0.0368	0.886
	(0.00468)	(0.00906)	(0.0220)	(0.00271)	(0.00534)	(0.0170)	(0.226)	(0.416)	(0.885)
April 2020	0.00643	-0.000890	0.0553*	0.0126**	-0.00425	0.0441**	-0.142	0.554	1.495
	(0.00522)	(0.0134)	(0.0281)	(0.00486)	(0.00880)	(0.0218)	(0.256)	(0.377)	(1.345)
May 2020	0.00649	-0.0142	0.0699*	0.0105**	-0.0172**	0.0427	-0.342	0.0235	1.172
	(0.00550)	(0.0124)	(0.0395)	(0.00469)	(0.00834)	(0.0359)	(0.232)	(0.480)	(1.162)
June 2020	0.00633	-0.0131	0.0276	0.0104*	-0.0149*	0.00602	0.0380	0.499	1.695
	(0.00617)	(0.0112)	(0.0325)	(0.00528)	(0.00885)	(0.0316)	(0.222)	(0.470)	(1.263)

Notes: Notes to Table 2 apply except industry and occupation fixed effects are added to the model.

Table 5: Regression adjusted differences between treatment and control groups, alternative group specification

Dependent variable:	At Work			Employed			Hours Worked		
Research Design	*1*	*2*	*3*	*1*	*2*	*3*	*1*	*2*	*3*
Panel A: All	n=1,625,683	n=564,914	n=383,292	n=1,625,683	n=564,914	n=383,292	n=1,231,552	n=423,971	n=288,878
March 2020	-0.00433	0.0208**	0.00752	-0.00199	0.0172**	0.0110	-0.133	0.221	0.0187
	(0.00392)	(0.00911)	(0.0188)	(0.00325)	(0.00776)	(0.0159)	(0.161)	(0.270)	(0.600)
April 2020	-0.000953	0.0265***	0.0757***	0.00232	0.0224***	0.0722***	0.0181	0.749**	1.004
	(0.00433)	(0.00788)	(0.0238)	(0.00485)	(0.00711)	(0.0217)	(0.189)	(0.326)	(0.694)
May 2020	-0.000412	0.0277***	0.0742***	0.00450	0.0266***	0.0614**	-0.00592	0.576*	-0.0872
	(0.00494)	(0.00844)	(0.0270)	(0.00494)	(0.00848)	(0.0252)	(0.171)	(0.303)	(0.657)
June 2020	0.00458	0.000898	0.0473*	0.0123*	0.00253	0.0484**	0.185	0.549	-0.117
	(0.00592)	(0.00935)	(0.0261)	(0.00621)	(0.00850)	(0.0238)	(0.193)	(0.357)	(0.757)
Panel B: Women	n=834,217	n=309,081	n=209,328	n=834,217	n=309,081	n=209,328	n=580,015	n=199,890	n=135,356
March 2020	-0.00465	0.0256**	0.00494	-0.00279	0.0237**	0.0157	-0.129	0.317	-0.443
	(0.00503)	(0.0111)	(0.0237)	(0.00387)	(0.0108)	(0.0194)	(0.161)	(0.343)	(0.966)
April 2020	-0.00162	0.0368***	0.0480*	-0.00261	0.0381***	0.0550**	0.279	0.768*	1.298
	(0.00595)	(0.0102)	(0.0272)	(0.00597)	(0.00999)	(0.0238)	(0.258)	(0.399)	(0.824)
May 2020	-0.00985	0.0533***	0.0640*	-0.00525	0.0548***	0.0628*	0.601***	0.910**	0.157
	(0.00668)	(0.0115)	(0.0352)	(0.00725)	(0.0114)	(0.0334)	(0.220)	(0.385)	(0.765)
June 2020	-0.00230	0.0154	0.0521	0.00611	0.0195	0.0679**	0.511**	0.628	-0.441
	(0.00805)	(0.0130)	(0.0335)	(0.00893)	(0.0129)	(0.0298)	(0.244)	(0.449)	(0.937)
Panel C: Men	n=791,466	n=255,833	n=173,964	n=791,466	n=255,833	n=173,964	n=651,537	n=224,081	n=153,522
March 2020	-0.00197	0.0154	0.0129	0.000457	0.00955	0.00616	-0.142	0.182	0.488
	(0.00606)	(0.0119)	(0.0235)	(0.00508)	(0.00902)	(0.0200)	(0.224)	(0.350)	(0.901)
April 2020	0.00253	0.0150	0.128***	0.0101*	0.00423	0.109***	-0.284	0.773*	1.038
	(0.00559)	(0.0144)	(0.0289)	(0.00576)	(0.0108)	(0.0296)	(0.248)	(0.415)	(1.209)
May 2020	0.0128**	-0.00211	0.105***	0.0177***	-0.00634	0.0771**	-0.599**	0.267	-0.0341
	(0.00556)	(0.0118)	(0.0311)	(0.00520)	(0.0109)	(0.0292)	(0.256)	(0.425)	(0.972)
June 2020	0.0161**	-0.0152	0.0551	0.0212***	-0.0163*	0.0380	-0.195	0.449	0.631
	(0.00745)	(0.0116)	(0.0337)	(0.00688)	(0.00960)	(0.0316)	(0.235)	(0.449)	(0.932)

Notes: Notes to Table 2 apply except treatment and control groups are redefined to account for all children in a household, as described in the text.

Table 6: Regression adjusted differences between treatment and control groups, sample restricted to respondents in 3rd, 4th, 7th, or 8th month in the CPS sample

Dependent variable:	At Work			Employed			Hours Worked		
Research Design	*1*	*2*	*3*	*1*	*2*	*3*	*1*	*2*	*3*
	n=829,721	n=256,599	n=186287	n=829,721	n=256,599	n=186287	n=627,888	n=195,380	n=141,647
March 2020	-0.00864	0.0163	-0.0112	-0.00838	0.0148	-0.0136	0.0641	-0.164	-0.252
	(0.00666)	(0.0143)	(0.0249)	(0.00506)	(0.0126)	(0.0232)	(0.238)	(0.445)	(0.675)
April 2020	0.00229	0.0235	0.0280	0.00275	0.0204*	0.0112	-0.0182	0.635	0.0183
	(0.00692)	(0.0148)	(0.0305)	(0.00641)	(0.0131)	(0.0319)	(0.278)	(0.434)	(1.122)
May 2020	0.00120	0.0168	0.0850**	0.00303	0.00983	0.0727**	0.145	0.164	-0.0426
	(0.00910)	(0.0145)	(0.0347)	(0.00777)	(0.0127)	(0.0341)	(0.276)	(0.481)	(0.890)
June 2020	0.0139	0.0220	0.0357	0.0169*	0.00743	0.0210	0.228	0.115	-0.163
	(0.00927)	(0.0148)	(0.0314)	(0.00900)	(0.0139)	(0.0316)	(0.301)	(0.650)	(0.969)

Notes: Notes to Table 2 apply except the samples are restricted to include only respondents whose month-in-sample is 3, 4, 7, or 8.

Table 7: Sub-group heterogeneity of effects using a standard difference-in-differences model

Dependent variable:	At Work			Employed			Hours Worked		
Research Design	*1*	*2*	*3*	*1*	*2*	*3*	*1*	*2*	*3*
	n=1,578,417	n=487,895	n=354,310	n=1,578,417	n=487,895	n=354,310	n=1,195,624	n=371,883	n=269,608
Full Sample	0.00724*	0.0205***	0.0290	0.00935**	0.0173***	0.0228	0.000503	0.195	-0.565
	(0.00370)	(0.00644)	(0.0176)	(0.00363)	(0.00540)	(0.0186)	(0.126)	(0.205)	(0.485)
Single	-0.0174**	0.0110	-0.0131	-0.00575	0.00581	-0.00314	-0.236	-0.0612	-0.845
	(0.00693)	(0.0155)	(0.0215)	(0.00732)	(0.0159)	(0.0218)	(0.224)	(0.494)	(0.642)
Women only	-0.0192**	0.00783	-0.0197	-0.00914	0.0160	-0.00733	-0.287	0.0514	-1.291*
	(0.00934)	(0.0184)	(0.0251)	(0.00934)	(0.0178)	(0.0244)	(0.208)	(0.644)	(0.717)
Men only	-0.0245*	0.0207	0.00713	-0.0113	-0.0163	0.0148	-0.321	-0.633	0.592
	(0.0134)	(0.0308)	(0.0366)	(0.0140)	(0.0280)	(0.0382)	(0.400)	(0.909)	(1.320)
Married	-0.00134	0.0255***	0.0346*	-0.000203	0.0220***	0.0205	-0.0189	0.253	-0.711
	(0.00460)	(0.00840)	(0.0204)	(0.00438)	(0.00765)	(0.0216)	(0.140)	(0.243)	(0.656)
Women only	0.00345	0.0533***	0.0189	0.00342	0.0483***	0.0152	0.268	0.0824	-0.797
	(0.00633)	(0.0111)	(0.0317)	(0.00624)	(0.0112)	(0.0274)	(0.219)	(0.442)	(1.066)
Men only	-0.00543	-0.00372	0.0471*	-0.00291	-0.00597	0.0218	-0.193	0.343	-0.597
	(0.00499)	(0.0102)	(0.0251)	(0.00423)	(0.00821)	(0.0278)	(0.192)	(0.257)	(1.069)
White	0.00840*	0.0203***	0.0310	0.0110***	0.0167***	0.0244	-0.0417	0.0355	-0.529
	(0.00433)	(0.00645)	(0.0222)	(0.00408)	(0.00607)	(0.0227)	(0.140)	(0.228)	(0.630)
Black	-0.00161	0.0170	-0.00936	0.00116	0.0161	-0.00650	-0.0351	1.239**	-1.937*
	(0.0112)	(0.0264)	(0.0467)	(0.0112)	(0.0218)	(0.0390)	(0.292)	(0.566)	(1.032)
Hispanic	0.00736	0.0271	0.00298	0.00636	0.0184	-0.0222	0.114	0.728**	-0.405
	(0.0108)	(0.0166)	(0.0374)	(0.00960)	(0.0184)	(0.0404)	(0.283)	(0.357)	(0.691)
High school non-grad	-0.0178	0.0666***	0.0416	-0.0140	0.0580***	0.0490	-0.654	-0.540	0.467
	(0.0166)	(0.0193)	(0.0582)	(0.0160)	(0.0162)	(0.0561)	(0.509)	(0.609)	(1.813)
High school grad	-0.00497	0.00606	-0.0109	0.00170	0.00602	-0.00723	0.0871	0.0676	-0.997
	(0.00722)	(0.0129)	(0.0309)	(0.00714)	(0.0128)	(0.0290)	(0.202)	(0.424)	(0.657)
Some college	-0.00700	0.00184	0.00646	0.000112	-0.00796	-0.00263	0.217	0.00622	-0.670
	(0.00889)	(0.0110)	(0.0211)	(0.00890)	(0.00952)	(0.0243)	(0.237)	(0.410)	(0.879)
College grad	0.0231***	0.0123	0.0519	0.0209***	0.0160*	0.0503	0.0696	0.360	-0.463
	(0.00542)	(0.00910)	(0.0324)	(0.00542)	(0.00871)	(0.0343)	(0.214)	(0.313)	(0.725)
Urban resident	0.0117	0.0326**	0.0101	0.0126*	0.0265**	-0.00152	0.291	0.505	-0.780
	(0.00723)	(0.0126)	(0.0375)	(0.00699)	(0.0119)	(0.0362)	(0.257)	(0.532)	(0.838)

Suburban resident	0.00634	0.0192	0.0451**	0.00664	0.0191*	0.0318	0.0261	0.278	-0.264
	(0.00503)	(0.0118)	(0.0213)	(0.00528)	(0.0101)	(0.0233)	(0.194)	(0.294)	(0.652)
Rural resident	-0.00180	0.0114	0.0205	0.00577	-0.00115	0.0297	-0.495	0.365	-1.759
	(0.00804)	(0.0133)	(0.0468)	(0.00876)	(0.0118)	(0.0450)	(0.297)	(0.536)	(1.391)
Metro unknown	0.0116	0.0271*	0.0143	0.0149*	0.0295**	0.0209	-0.0490	-0.613	-0.305
	(0.00860)	(0.0138)	(0.0515)	(0.00858)	(0.0142)	(0.0477)	(0.282)	(0.509)	(1.246)

Notes: Notes for Table 2 apply except estimates are of equation (2), data for the month of March 2020 is excluded from all samples, and each sample is limited the relevant sub-population. For estimates by race we limit to only those respondents that specify one race. Sample sizes at the top of each column are for the full sample.

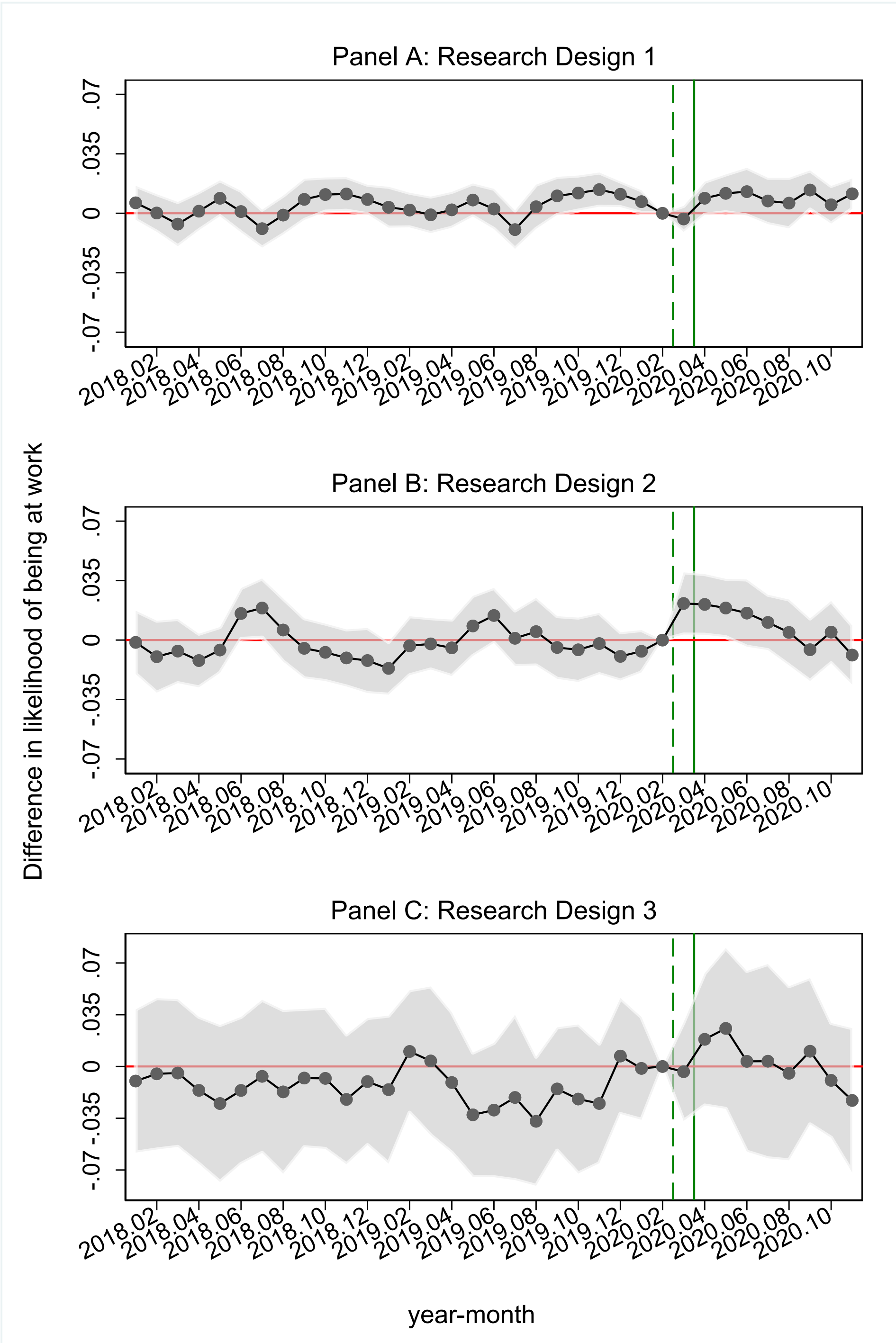
Table 8: Regression adjusted differences between treatment and control groups for COVID-19 questions

Dependent variable:	Teleworked			Unable to Work			Prevented Looking for Work		
Research Design	*1*	*2*	*3*	*1*	*2*	*3*	*1*	*2*	*3*
Panel A: All	n= 59,198	n=18,094	n=13,149	n=86,451	n=25,999	n=18,780	n= 16,928	n=4,843	n=3,389
May 2020	0.0111*	0.0130	0.0299	0.00288	-0.00121	0.0235	-0.0397***	0.00657	0.0439
	(0.00638)	(0.0117)	(0.0273)	(0.00531)	(0.00756)	(0.0236)	(0.0101)	(0.0128)	(0.0482)
June 2020	0.0111**	0.0292**	0.00402	0.00278	0.00680	0.0375***	-0.0354***	-0.0207**	0.110***
	(0.00515)	(0.0121)	(0.0259)	(0.00487)	(0.00786)	(0.0140)	(0.00928)	(0.00970)	(0.0282)
Panel B: Women	n= 27,455	n=8,118	n=5,955	n=44,254	n=14,072	n=10,173	n= 11,277	n= 4,141	n=2,895
May 2020	0.0188**	0.00624	0.0366	-0.00704	-0.00740	0.0455*	-0.0501***	0.000553	0.0278
	(0.00912)	(0.0186)	(0.0357)	(0.00802)	(0.00923)	(0.0246)	(0.0141)	(0.0131)	(0.0451)
June 2020	0.0235**	0.0262	0.0133	-0.00128	0.00565	0.0491***	-0.0421***	-0.0120	0.0914***
	(0.00930)	(0.0180)	(0.0335)	(0.00640)	(0.0108)	(0.0175)	(0.0134)	(0.0103)	(0.0336)
Panel C: Men	n=31,743	n= 9,976	n=7,194	n=42,197	n=11,927	n=8,607	n= 5,651	n=702	n=493
May 2020	0.00556	0.0186	0.0262	0.0128*	0.00544	-0.0167	0.0436*	0.00701	0.160
	(0.00769)	(0.0136)	(0.0413)	(0.00649)	(0.0119)	(0.0381)	(0.0224)	(0.0412)	(0.124)
June 2020	0.00142	0.0311**	-0.00108	0.00597	0.00852	0.0231	0.0282	-0.101**	0.161
	(0.00708)	(0.0136)	(0.0339)	(0.00621)	(0.00941)	(0.0285)	(0.0264)	(0.0468)	(0.0981)
Panel D: MIS=3,4,7,8	n= 32,149	n=9,776	n=7,082	n=47,254	n=14,199	n=10,225	n=9,379	n=2,761	n=1,937
May 2020	0.00232	0.0293*	0.0721*	0.0110	-0.0120	0.0222	-0.0205*	0.00519	0.0275
	(0.00924)	(0.0163)	(0.0361)	(0.00823)	(0.0118)	(0.0314)	(0.0122)	(0.0155)	(0.0585)
June 2020	0.00448	0.0383**	0.0357	-0.00322	0.00582	0.0242	-0.0393***	-0.0113	0.0967**
	(0.00826)	(0.0179)	(0.0299)	(0.00855)	(0.0113)	(0.0201)	(0.0125)	(0.0163)	(0.0362)

Notes: Estimates of equation (3). State-level, clustered standard errors reported in parentheses. Statistically significant estimates for two-tailed tests at the one, five, and ten-percent levels are indicated ***, **, and *, respectively. Research designs definitions and base sample, limited to May and June of 2020, are as described in the notes to Figure 1. "Teleworked" means that the respondent teleworked or worked at home for pay, and the sample is limited to those who were at work. "Unable to work" means that the respondent was not able to work because of the employer closed or lost business due to COVID-19; the question was asked of the full sample. "Prevented looking for work" means that the COVID-19 prevented the respondent from looking for work, and the sample is limited to those who were out of the labor force.

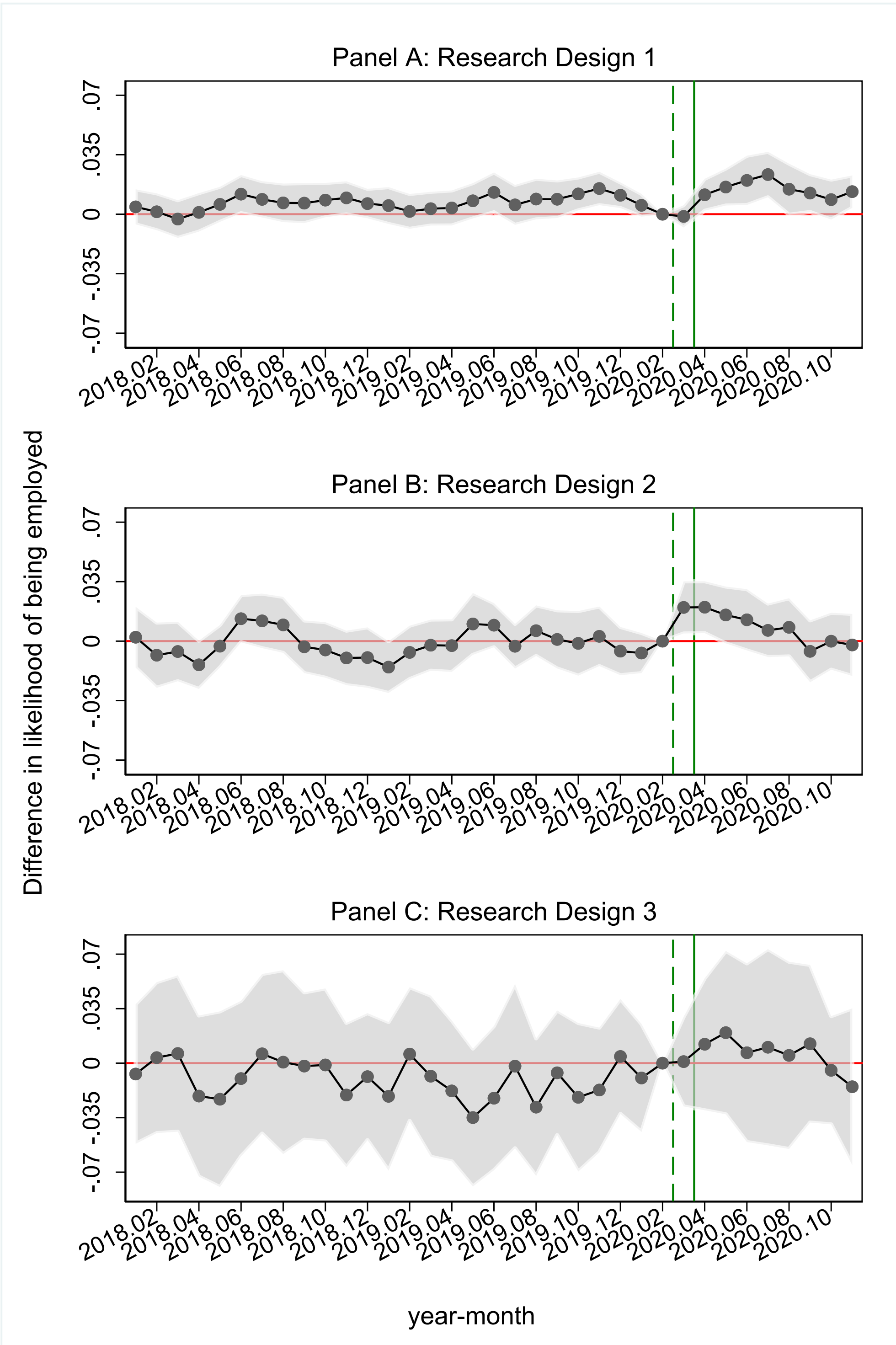
Appendix

Appendix Figure 1: Difference in likelihood of being at work (treated group minus control) through November



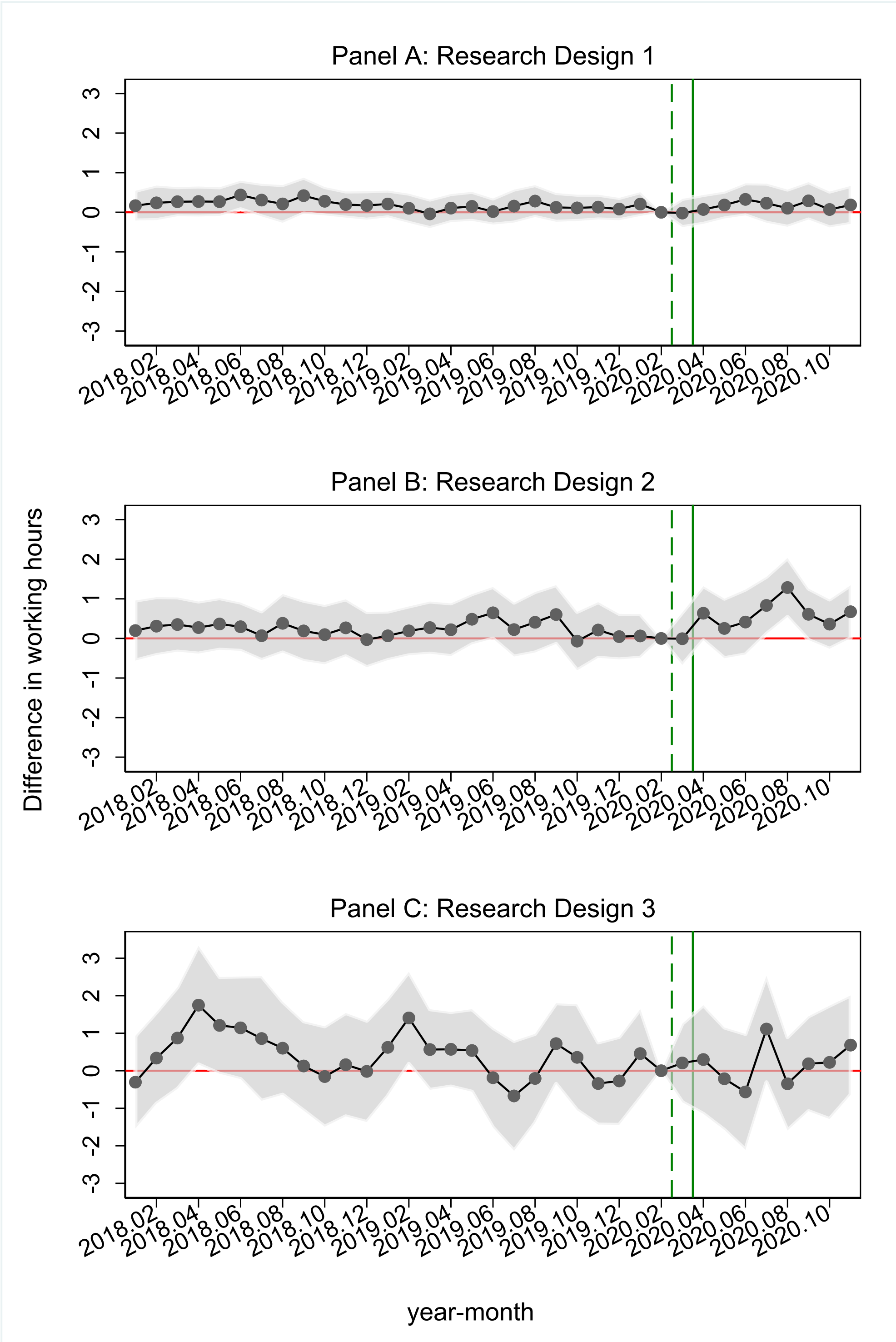
Notes: Notes to Figure 1 apply, except the sample period extends to November 2020.

Appendix Figure 2: Difference in likelihood of being employed (treated group minus control) through November



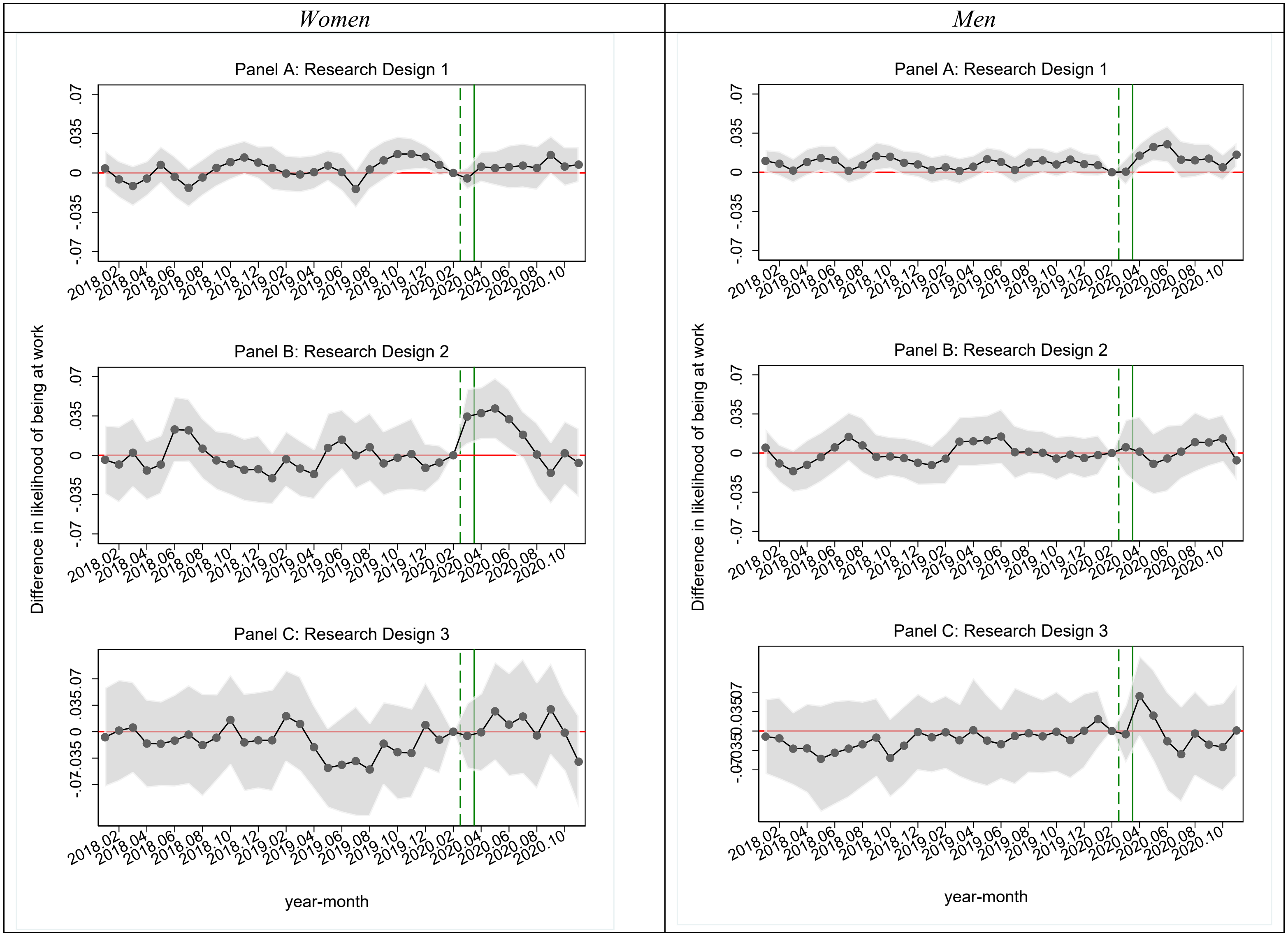
Notes: Notes to Figure 2 apply, except the sample period extends to November 2020.

Appendix Figure 3: Difference in hours worked (treated group minus control) through November



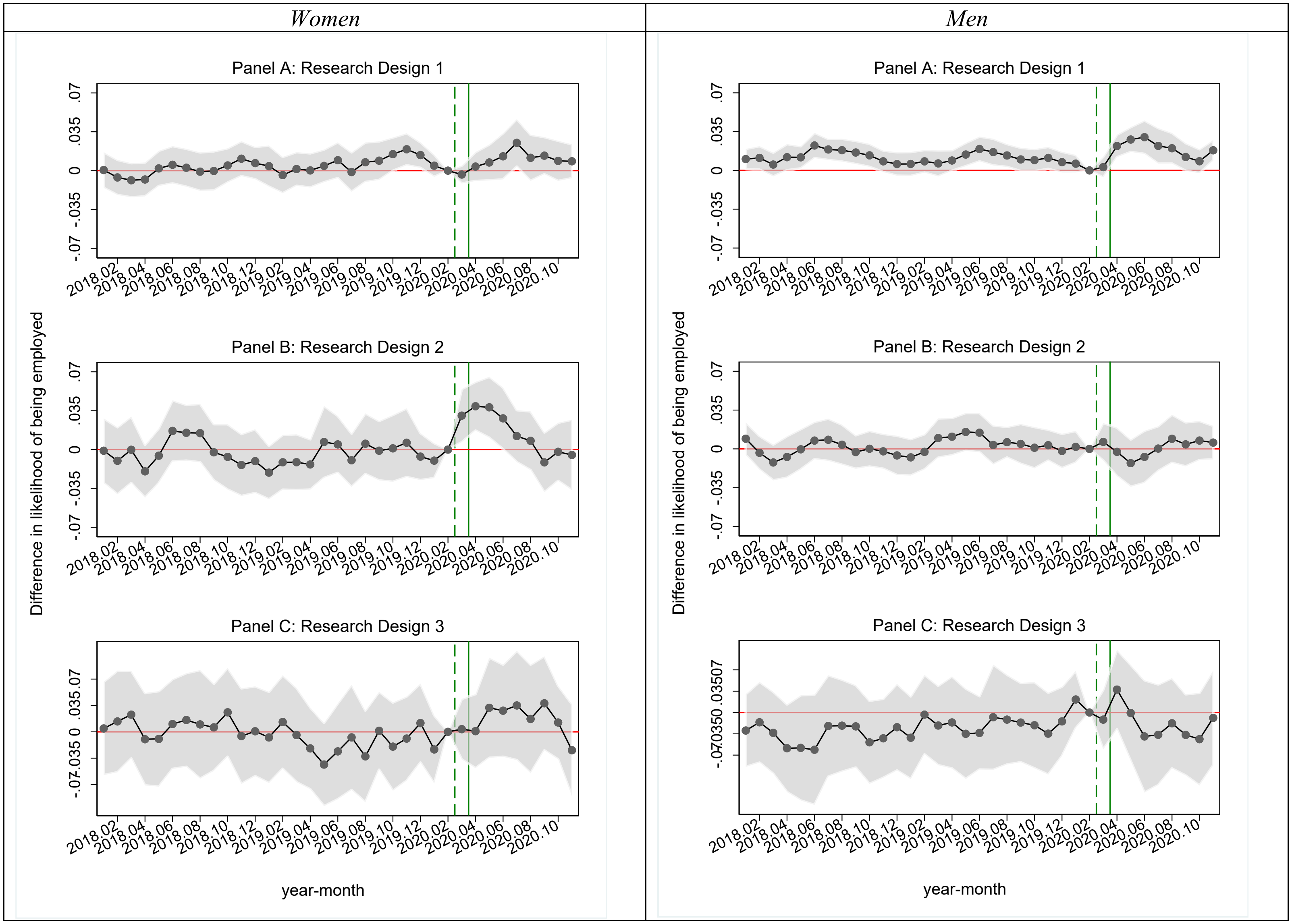
Notes: Notes to Figure 3 apply, except the sample period extends to November 2020.

Appendix Figure 4: Difference in likelihood of being at work (treated group minus control) through November, separately by women and men



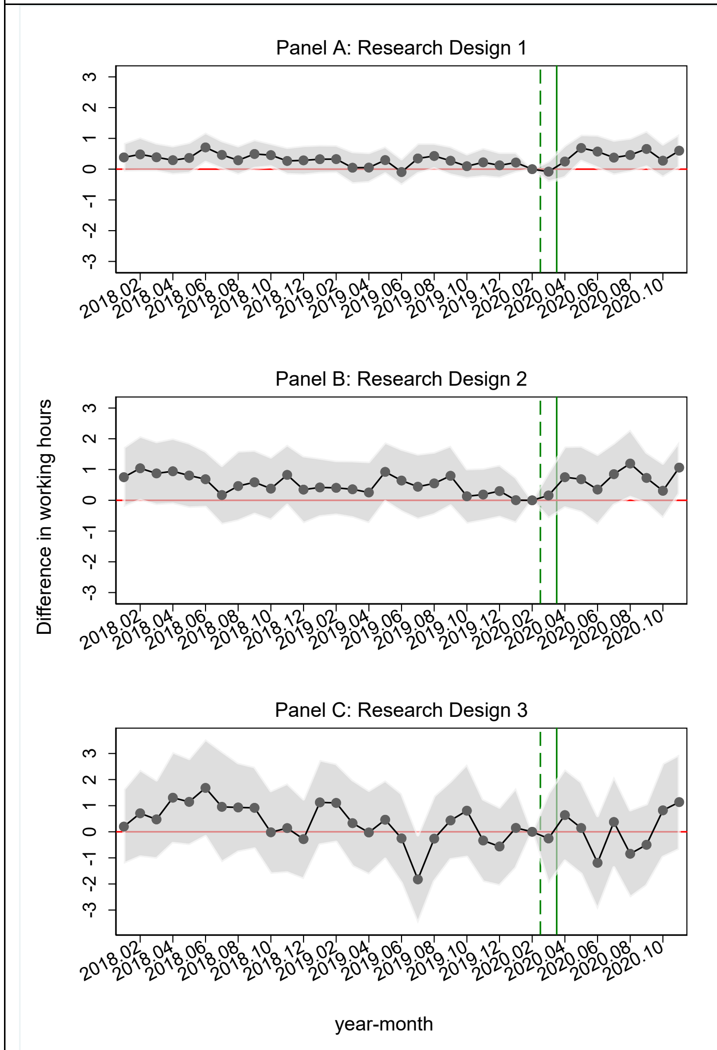
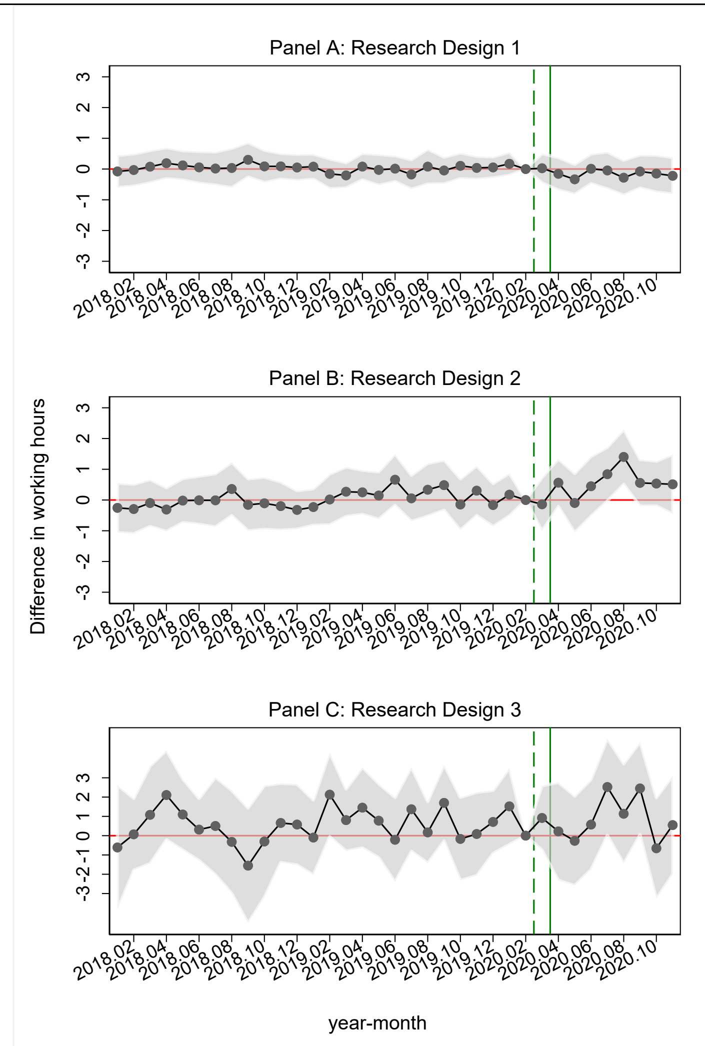
Notes: Notes to Appendix Figure 1 apply, except the analysis was done separtely by gender

Appendix Figure 5: Difference in likelihood of being employed (treated group minus control) through November, separately by women and men



Notes: Notes to Appendix Figure 2 apply, except the analysis was done separtely by gender

Appendix Figure 6: Difference in hours worked (treated group minus control) through November, separately by women and men

Women	*Men*
	

Notes: Notes to Appendix Figure 3 apply, except the analysis was done separtely by gender

Appendix Table 1: Regression adjusted differences between treatment and control groups, with partial pre-period estimates

Dependent variable:	At Work			Employed			Hours Worked		
Research Design	*1*	*2*	*3*	*1*	*2*	*3*	*1*	*2*	*3*
	n=1,625,683	n=501,655	n=364,843	n=1,625,683	n=502440	n=364,843	n=1,231,552	n=382,952	n=277,680
September 2019	0.0102*	-0.00453	-0.0147	0.00882**	0.000854	-0.00574	0.125	0.611	0.697
	(0.00557)	(0.00915)	(0.0207)	(0.00551)	(0.00849)	(0.0199)	(0.181)	(0.370)	(0.536)
October 2019	0.0119**	-0.00592	-0.0215	0.0119**	-0.00142	-0.0216	0.113	-0.0631	0.326
	(0.00511)	(0.00942)	(0.0252)	(0.00480)	(0.00943)	(0.0238)	(0.165)	(0.367)	(0.704)
November 2019	0.0139***	-0.00220	-0.0244	0.0151***	0.00270	-0.0168	0.132	0.218	-0.374
	(0.00499)	(0.00897)	(0.0201)	(0.00488)	(0.00872)	(0.0200)	(0.155)	(0.348)	(0.544)
December 2019	0.0111***	-0.00963	0.00763	0.0111***	-0.00590	0.00472	0.0807	0.0489	-0.292
	(0.00375)	(0.00710)	(0.0195)	(0.00375)	(0.00708)	(0.0184)	(0.140)	(0.284)	(0.588)
January 2020	0.00681**	-0.00661	-0.000917	0.00522	-0.00701	-0.00924	0.207	0.0635	0.446
	(0.00332)	(0.00620)	(0.0173)	(0.00326)	(0.00586)	(0.0173)	(0.149)	(0.274)	(0.593)
				February 2020 – Reference Period					
March 2020	-0.00325	0.0215**	-0.00333	-0.00120	0.0198**	0.00120	-0.0205	-0.00701	0.204
	(0.00381)	(0.00910)	(0.0167)	(0.00326)	(0.00764)	(0.0144)	(0.177)	(0.329)	(0.523)
April 2020	0.00898*	0.0208**	0.0182	0.0115**	0.0198**	0.0120	0.0690	0.637*	0.285
	(0.00482)	(0.00892)	(0.0222)	(0.00468)	(0.00763)	(0.0211)	(0.181)	(0.340)	(0.722)
May 2020	0.0118**	0.0186**	0.0261	0.0161***	0.0152*	0.0199	0.179	0.261	-0.238
	(0.00555)	(0.00857)	(0.0270)	(0.00539)	(0.00835)	(0.0261)	(0.168)	(0.373)	(0.680)
June 2020	0.0129*	0.0156	0.00404	0.0200***	0.0124	0.00710	0.325	0.419	-0.589
	(0.00697)	(0.00984)	(0.0303)	(0.00720)	(0.00899)	(0.0285)	(0.202)	(0.407)	(0.770)

Notes: Notes to Table 2 apply.

Appendix Table 2: Regression adjusted differences between treatment and control groups, with minimum controls

Dependent variable:	At Work			Employed			Hours Worked		
Research Design	*1*	*2*	*3*	*1*	*2*	*3*	*1*	*2*	*3*
Panel A: All	n=1,625,683	n=502440	n=364,843	n=1,625,683	n=502440	n=364,843	n=1,231,552	n=382,518	n=277,680
March 2020	-0.00359	0.0209**	-0.00309	-0.00154	0.0209**	0.00206	0.00422	-0.114	0.0286
	(0.00415)	(0.00920)	(0.0168)	(0.00335)	(0.00920)	(0.0148)	(0.186)	(0.332)	(0.582)
April 2020	0.00946*	0.0209**	0.0238	0.0119**	0.0209**	0.0180	0.179	0.490	0.666
	(0.00524)	(0.00946)	(0.0212)	(0.00493)	(0.00946)	(0.0208)	(0.187)	(0.331)	(0.731)
May 2020	0.0119*	0.0184*	0.0273	0.0162**	0.0184*	0.0219	0.297*	0.0568	-0.160
	(0.00693)	(0.00931)	(0.0262)	(0.00679)	(0.00931)	(0.0251)	(0.172)	(0.359)	(0.711)
June 2020	0.0103	0.0153	0.00956	0.0176**	0.0153	0.0133	0.414**	0.261	-0.386
	(0.00693)	(0.0115)	(0.0288)	(0.00857)	(0.0115)	(0.0269)	(0.202)	(0.410)	(0.729)
Panel B: Women	n=834,217	n=275,819	n=200033	n=834,217	n=275,819	n=200033	n=580,015	n=179,982	n=129,848
March 2020	-0.00561	0.0343***	0.00109	-0.00411	0.0307**	0.0144	-0.0902	0.169	-0.262
	(0.00513)	(0.0128)	(0.0228)	(0.00436)	(0.0123)	(0.0218)	(0.176)	(0.360)	(0.873)
April 2020	0.00290	0.0395***	-0.00521	0.000943	0.0414***	-0.00227	0.274	0.730	0.694
	(0.00683)	(0.0133)	(0.0259)	(0.00670)	(0.0129)	(0.0251)	(0.268)	(0.506)	(0.881)
May 2020	0.00136	0.0447***	0.0182	0.00470	0.0416**	0.0239	0.724***	0.634	-0.0668
	(0.00884)	(0.0164)	(0.0334)	(0.00927)	(0.0162)	(0.0334)	(0.223)	(0.521)	(0.914)
June 2020	0.000683	0.0366**	0.00353	0.00791	0.0331*	0.0230	0.558**	0.374	-1.312
	(0.0110)	(0.0161)	(0.0351)	(0.0123)	(0.0166)	(0.0309)	(0.268)	(0.566)	(0.880)
Panel C: Men	n=791,466	n=226,621	n=164,810	n=791,466	n=226,621	n=164,810	n=651,537	n=202,970	n=147,832
March 2020	-0.00202	0.00676	-0.00660	0.000386	0.00755	-0.0123	-0.00338	-0.169	0.794
	(0.00609)	(0.0119)	(0.0266)	(0.00465)	(0.0817)	(0.0254)	(0.247)	(0.433)	(0.833)
April 2020	0.0159**	-0.00134	0.0722*	0.0230***	-0.00516	0.0480	-0.214	0.496	0.201
	(0.00626)	(0.0158)	(0.0366)	(0.00536)	(0.0110)	(0.0345)	(0.264)	(0.378)	(1.310)
May 2020	0.0212***	-0.0123	0.0314	0.0265***	-0.0153	0.00377	-0.409	-0.140	-0.171
	(0.00716)	(0.0134)	(0.0418)	(0.00660)	(0.0105)	(0.0440)	(0.253)	(0.493)	(1.157)
June 2020	0.0206**	-0.00857	-0.00395	0.0257***	-0.0105	-0.0233	-0.0594	0.408	0.811
	(0.00909)	(0.0148)	(0.0454)	(0.00827)	(0.0119)	(0.0464)	(0.245)	(0.493)	(1.131)

Notes: Notes to Table 2 apply except the only controls included are year-month fixed effects.

Appendix Table 3: Regression adjusted differences between treatment and control groups, controlling for the youngest child's age fixed effects

Dependent variable:	At Work			Employed			Hours Worked		
Research Design	*1*	*2*	*3*	*1*	*2*	*3*	*1*	*2*	*3*
Panel A: All		n=502,440	n=364,843		n=502,440	n=364,843		n=382,952	n=277,680
March 2020	...	0.0216**	-0.000194	...	0.0202**	0.00339	...	0.00326	0.230
		(0.00906)	(0.0164)		(0.00768)	(0.0142)		(0.330)	(0.524)
April 2020	...	0.0217**	0.0193	...	0.0209**	0.0128	...	0.650*	0.297
		(0.00934)	(0.0220)		(0.00782)	(0.0207)		(0.337)	(0.726)
May 2020	...	0.0184**	0.0269	...	0.0155*	0.0206	...	0.272	-0.233
		(0.00880)	(0.0269)		(0.00840)	(0.0258)		(0.370)	(0.679)
June 2020	...	0.0166	0.00491	...	0.0133	0.00749	...	0.434	-0.576
		(0.0101)	(0.0299)		(0.00907)	(0.0281)		(0.409)	(0.770)
Panel B: Women		n=275,819	n=200033		n=275,819	n=200033		n=179,982	n=129,848
March 2020	...	0.0348***	0.000635	...	0.0314**	0.00832	...	0.181	-0.186
		(0.0121)	(0.0208)		(0.0120)	(0.0193)		(0.373)	(0.856)
April 2020	...	0.0399***	0.00230	...	0.0416***	0.00357	...	0.800	0.674
		(0.0114)	(0.0257)		(0.0110)	(0.0241)		(0.492)	(0.880)
May 2020	...	0.0417***	0.0290	...	0.0390***	0.0343	...	0.751	0.133
		(0.0131)	(0.0325)		(0.0135)	(0.0326)		(0.540)	(0.883)
June 2020	...	0.0337**	0.00964	...	0.0296**	0.0284	...	0.378	-1.203
		(0.0136)	(0.0330)		(0.0139)	(0.0298)		(0.572)	(0.890)
Panel C: Men		n=226,612	n=164,810		n=226,621	n=164,810		n=202,970	n=147,832
March 2020	...	0.00168	-0.00512	...	0.00638	-0.0113	...	-0.146	0.923
		(0.0906)	(0.0262)		(0.00845)	(0.0245)		(0.431)	(0.832)
April 2020	...	-0.000890	0.0626*	...	-0.00261	0.0373	...	0.557	0.219
		(0.0134)	(0.0364)		(0.0108)	(0.0326)		(0.382)	(1.259)
May 2020	...	-0.0142	0.0279	...	-0.0127	-0.0000990	...	-0.103	-0.290
		(0.0124)	(0.0412)		(0.0106)	(0.0393)		(0.473)	(1.149)
June 2020	...	-0.0131	-0.0179	...	-0.00698	-0.0395	...	0.437	0.540
		(0.0112)	(0.0443)		(0.0116)	(0.0457)		(0.481)	(1.160)

Notes: Notes to Table 2 apply except fixed effects for the age of the youngest child are added to the model. Estimates are not calculated for research desig 1 since some members of the research design 1 control group do not have children or are not living with one.

Appendix Table 4: Regression adjusted differences between treatment and control groups through November 2020

Dependent variable:	At Work			Employed			Hours Worked		
Research Design	*1*	*2*	*3*	*1*	*2*	*3*	*1*	*2*	*3*
All	n=1,866,419	n=575,778	n=418,043	n=1,866,419	n=575,778	n=418,043	n=1,405,610	n=436,386	n=316,427
March 2020	-0.00333	0.0215**	-0.00350	-0.00127	0.0198**	0.00101	-0.0193	-0.00667	0.208
	(0.00381)	(0.00910)	(0.0166)	(0.00327)	(0.00763)	(0.0143)	(0.177)	(0.329)	(0.521)
April 2020	0.00888*	0.0210**	0.0184	0.0114**	0.0200**	0.0122	0.0719	0.637*	0.299
	(0.00482)	(0.00889)	(0.0223)	(0.00467)	(0.00759)	(0.0211)	(0.181)	(0.340)	(0.721)
May 2020	0.0117**	0.0189**	0.0258	0.0160***	0.0154*	0.0196	0.182	0.253	-0.213
	(0.00554)	(0.00854)	(0.0271)	(0.00541)	(0.00832)	(0.0261)	(0.168)	(0.374)	(0.680)
June 2020	0.0127*	0.0159	0.00350	0.0199***	0.0126	0.00670	0.326	0.414	-0.564
	(0.00698)	(0.00985)	(0.0304)	(0.00721)	(0.00897)	(0.0286)	(0.202)	(0.408)	(0.769)
July 2020	0.00727	0.0104	0.00358	0.0233***	0.00635	0.0102	0.229	0.834**	1.111
	(0.00678)	(0.00810)	(0.0327)	(0.00665)	(0.00783)	(0.0314)	(0.246)	(0.359)	(0.706)
August 2020	0.00598	0.00450	-0.00458	0.0147*	0.00814	0.00501	0.103	1.285***	-0.347
	(0.00740)	(0.00964)	(0.0293)	(0.00748)	(0.00852)	(0.0299)	(0.233)	(0.371)	(0.619)
September 2020	0.0137**	-0.00568	0.0103	0.0124**	-0.00599	0.0125	0.289	0.610*	0.187
	(0.00573)	(0.00915)	(0.0246)	(0.00554)	(0.00906)	(0.0252)	(0.234)	(0.323)	(0.629)
October 2020	0.00501	0.00471	-0.00936	0.00859	-0.0000464	-0.00456	0.0704	0.361	0.224
	(0.00551)	(0.00911)	(0.0193)	(0.00584)	(0.00833)	(0.0173)	(0.225)	(0.310)	0.224
November 2020	0.0115**	-0.00884	-0.0229	0.0133***	-0.00223	-0.0151	0.184	0.674**	0.683
	(0.00446)	(0.00864)	(0.0243)	(0.00474)	(0.00908)	(0.0251)	(0.238)	(0.332)	(0.659)
Women	n=957,188	n=315,952	n=418,043	n=957,188	n=315,952	n=229,072	n=661,295	n=204,842	n=147,729
March 2020	-0.00480	0.0344***	-0.00350	-0.00349	0.0306**	0.00356	-0.0800	0.159	-0.255
	(0.00484)	(0.0123)	(0.0166)	(0.00402)	(0.0119)	(0.0200)	(0.171)	(0.373)	(0.865)
April 2020	0.00562	0.0375***	0.0184	0.00367	0.0390***	0.000936	0.248	0.752	0.640
	(0.00675)	(0.0115)	(0.0223)	(0.00650)	(0.0109)	(0.0245)	(0.261)	(0.497)	(0.876)
May 2020	0.00433	0.0416***	0.0258	0.00735	0.0381***	0.0320	0.689***	0.686	0.145
	(0.00763)	(0.0135)	(0.0271)	(0.00805)	(0.0137)	(0.0331)	(0.217)	(0.537)	(0.880)
June 2020	0.00537	0.0321**	0.00350	0.0129	0.0281**	0.0281	0.572**	0.350	-1.189
	(0.00955)	(0.0134)	(0.0304)	(0.0103)	(0.0139)	(0.0305)	(0.267)	(0.569)	(0.898)
July 2020	0.00659	0.0183*	0.00358	0.0250**	0.0121	0.0350	0.375	0.851*	0.376
	(0.00981)	(0.0104)	(0.0327)	(0.0107)	(0.0117)	(0.0361)	(0.282)	(0.502)	(0.871)
August 2020	0.00442	0.000660	-0.00458	0.0115	0.00788	0.0171	0.459*	1.196**	-0.841
	(0.00968)	(0.0141)	(0.0293)	(0.0103)	(0.0132)	(0.0359)	(0.273)	(0.549)	(0.836)

September 2020	0.0160*	-0.0157	0.0103	0.0135	-0.0115	0.0379	0.657**	0.728*	-0.497
	(0.00842)	(0.0138)	(0.0246)	(0.00834)	(0.0134)	(0.0311)	(0.291)	(0.413)	(0.781)
October 2020	0.00574	0.00178	-0.00936	0.00880	-0.00205	0.0124	0.274	0.310	0.822
	(0.00863)	(0.0142)	(0.0193)	(0.00901)	(0.0130)	(0.0244)	(0.269)	(0.446)	(0.896)
November 2020	0.00743	-0.00683	-0.0229	0.00845	-0.00485	-0.0243	0.600**	1.065**	1.138
	(0.00774)	(0.0153)	(0.0243)	(0.00760)	(0.0158)	(0.0304)	(0.270)	(0.423)	(0.907)
Men	n=909,231	n=259,826	n=188,971	n=909,231	n=259,826	n=188,971	n=744,315	n=231,544	n=168,698
March 2020	0.000494	0.00521	-0.00580	0.00296	0.00631	-0.0117	0.0261	-0.139	0.919
	(0.00583)	(0.0122)	(0.0262)	(0.00466)	(0.00845)	(0.0244)	(0.236)	(0.432)	(0.827)
April 2020	0.0148***	0.00121	0.0628*	0.0220***	-0.00270	0.0376	-0.154	0.562	0.224
	(0.00537)	(0.0156)	(0.0364)	(0.00506)	(0.0109)	(0.0326)	(0.261)	(0.384)	(1.264)
May 2020	0.0226***	-0.00963	0.0279	0.0279***	-0.0129	-0.000823	-0.333	-0.0969	-0.267
	(0.00614)	(0.0136)	(0.0412)	(0.00561)	(0.0106)	(0.0394)	(0.242)	(0.478)	(1.154)
June 2020	0.0250***	-0.00481	-0.0185	0.0300***	-0.00723	-0.0393	0.00975	0.448	0.575
	(0.00821)	(0.0147)	(0.0445)	(0.00752)	(0.0117)	(0.0459)	(0.240)	(0.483)	(1.162)
July 2020	0.0112	0.00127	-0.0418	0.0221***	0.000279	-0.0366	-0.0447	0.840*	2.524**
	(0.00837)	(0.0121)	(0.0430)	(0.00801)	(0.0101)	(0.0412)	(0.298)	(0.434)	(1.245)
August 2020	0.0107	0.00975	-0.00452	0.0200***	0.00913	-0.0177	-0.281	1.402***	1.135
	(0.00747)	(0.0133)	(0.0381)	(0.00689)	(0.0109)	(0.0370)	(0.283)	(0.444)	(1.288)
September 2020	0.0123*	0.00952	-0.0245	0.0121*	0.00407	-0.0368	-0.0783	0.558	2.450**
	(0.00665)	(0.0106)	(0.0354)	(0.00623)	(0.00989)	(0.0332)	(0.268)	(0.377)	(1.195)
October 2020	0.00441	0.0130	-0.0290	0.00828	0.00753	-0.0438	-0.147	0.535	-0.654
	(0.00582)	(0.0106)	(0.0395)	(0.00540)	(0.00879)	(0.0379)	(0.299)	(0.361)	(1.303)
November 2020	0.0157***	-0.00652	0.000971	0.0182***	0.00567	-0.00884	-0.219	0.512	0.550
	(0.00540)	(0.00903)	(0.0407)	(0.00464)	(0.00754)	(0.0388)	(0.295)	(0.482)	(1.271)

Notes: Notes to Table 2 apply except the sample period extends to November 2020.

Appendix Table 5: Regression adjusted differences between treatment and control groups for telework outcome, including occupation and industry fixed effects

Dependent variable:	Teleworked		
Research Design	*1*	*2*	*3*
Panel A: All	n= 59,192	n=18,065	n=13,108
May 2020	0.0159***	0.00777	0.0208
	(0.00578)	(0.00963)	(0.0239)
June 2020	0.0170***	0.0256**	0.00359
	(0.00414)	(0.0102)	(0.0220)
Panel B: Women	n=27,412	n=8,050	n=5,867
May 2020	0.0177**	0.00468	0.0121
	(0.00810)	(0.0157)	(0.0306)
June 2020	0.0263***	0.0167	-0.00328
	(0.00836)	(0.0182)	(0.0269)
Panel C: Men	n=31,727	n=9,936	n=7,125
May 2020	0.0135*	0.00697	-0.0125
	(0.00708)	(0.0114)	(0.0333)
June 2020	0.00787	0.0257**	-0.0167
	(0.00594)	(0.0109)	(0.0276)

Notes: Notes to Table 8 apply, except industry and occupation fixed effects have been added to the models.