Young Children and Parents' Labor Supply during COVID-19

Authors:

Scott Barkowski, PhD
Assistant Professor
John E. Walker Department of Economics
Clemson University
sbarkow@clemson.edu

Joanne Song McLaughlin, PhD
Assistant Professor
Department of Economics
University at Buffalo
jsmclaug@buffalo.edu

Yinlin Dai
PhD Candidate
John E. Walker Department of Economics
Clemson University
yinlin@g.clemson.edu

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Abstract

We study the COVID-19 pandemic's effects on the labor supply of parents with young children. Using the monthly Current Population Survey, and following a pre-analysis plan, we use three variations of difference-in-differences to compare workers with childcare needs to those without. The first compares parents with young children and those without young children, while the second and third rely on the presence of someone who could provide childcare in the household: a teenager in one and a grandparent in the other. We analyze three outcomes: whether parents were "at work" (not sick, on vacation, or otherwise away from his or her job); whether they were employed; and hours worked. Contrary to expectation, we find the labor supply of parents with young children was not negatively affected by the COVID-19 pandemic. Instead, some evidence suggests they were more likely to be working after the pandemic unfolded. For the outcomes of being at work and employed, our results are not systematically different for men and women, but some findings suggest women with young children worked almost an hour longer per week than those without. These results suggest that factors like employers allowing employees to work at home and informal sources of childcare aided parents in avoiding negative shocks to their labor supply during the pandemic.

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

Introduction

The onset of the COVID-19 pandemic prompted unprecedented policy responses from American state and federal governments, 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. Would the sudden need to provide childcare for children no longer in school or daycare prohibit workers from staying in or finding new jobs? In the immediate aftermath of the closures, several analysts suggested this could be the case (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 in place of more expensive outside options. Indeed, the literature on the effects of childcare costs on labor supply of mothers has typically (though 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 potentially blunting the reliance on formal sources

[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. 2020a; 2020b).

(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 COVID-19 on the labor supply of parents with young children. Following a pre-specified analysis plan[2], we use data from the monthly Current Population Survey (CPS) to implement three variations of a difference-in-differences (DD) research design that compares parents with childcare needs to those without (or lesser needs). In the first research design, our comparison is of parents with young children and 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. Finally, in our 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.

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[2] Our pre-analysis plan was developed before the data for April (the start of our post-period) was available to the public, and 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 (Barkowski, McLaughlin, and Dai 2020).

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 young children were more likely to be working than other types of adults after the onset of the pandemic. These results suggest that factors like the increased flexibility to work at home and the increased availability of informal sources of care dominated the substitution mechanism as the pandemic unfolded. 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.

In post-hoc analysis of sub-group heterogeneity, we find our main results appear to be driven by three groups: white respondents, college graduates, and urban residents. 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 for 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.

This paper contributes to the literature on childcare and, more generally, to that on the effects of COVID-19 on the American labor market by being one of the first papers to study the labor supply response of parents to the pandemic. Three other recent studies also do so, though two of these use empirical approaches that differ greatly from ours and the third investigates a very different population.[3,4] Using state-level variation in school closings to study the general population, Rojas et al. (2020) analyze new unemployment insurance benefit claim 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). 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, somewhat counterintuitively, increased number of

[3] Initial drafts of each of these studies and ours began circulating publicly within a short amount of time. 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 posted on SSRN on June 19th, 2020. Kalenkoski and Pabilonia (2020) was posted on SSRN on July 7th, 2020.

[4] Internationally, Ma, Sun, and Xue (2020) study childcare demand and parents' labor supply in China.

hours worked. However, for both these studies, their results are difficult to interpret because states across the country issued school closure orders due within a matter of days. This leaves little variation through which estimates based on differential timing of closures could be identified.[5] Moreover, their results could be sensitive to differences across states in the way the pandemic progressed and was experienced. In contrast, our approach is based on variation across individuals – including within states – allowing us to obtain more precision in our estimates and avoid concerns over differences in how the pandemic unfolded across states. In their study of self-employed workers, Kalenkoski and Pabilonia (2020) also 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.

Empirical Methods

As specified in our pre-analysis plan (Barkowski, McLaughlin, and Dai 2020), 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

[5] Heggeness (2020) also treats all 2020 data as post-period, including January and February, complicating interpretation further since it is unclear if effects come from behavioral changes occurring after school closures or before.

whether from home or to leave the house (if necessary) to work, activities that might be inhibited by childcare responsibilities. 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 at work 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.

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 be able 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.[6]

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 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.[7]

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

[6] 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.
[7] 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.

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:

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

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 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.[8]

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 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. The second adds state and calendar month (for seasonality) dummies. 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.

[8] 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).

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 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 some effects began to be felt 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.

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. To check whether imbalance by age of young children needing care could be driving our results, for our second and third research designs, in which the samples are limited to adults with young children, we also estimate a version of our model with dummy variables for the age of the youngest child included.

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 is driven by the Census Bureau dropping in-person interviews beginning in March. These in person interviews usually occur for household 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.

In a final post-hoc analysis we examine heterogeneity of effects 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

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

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.

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 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 are living 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.

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 note that it is unusual to specify a pre-analysis plan when using publicly available government surveys, but it is not unprecedented (Neumark 2001). Given the speed with which the pandemic situation unfolded, we were able to develop our analysis plans before post-period data became available. Since we posted our plans publicly on the same day the post-treatment data for April 2020 was made available to the public, our ability to perform specification searches is limited, increasing the credibility of our analysis.

Results

Figures 1 through 3 plot our estimates for our preferred specification of equation (1) that includes full demographic controls using 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.[9] 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

[9] 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.

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.[10] Our unweighted main results in 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=0.052), May (p=0.035), and June (p=0.062). Research design 2 estimates put the increase at about two percentage points for the same months (p=0.013, 0.016, and 0.065), and for March (p=0.020) also, despite that the pandemic response was still at its early stages. Research design 3 returns similar, positive point estimates for April, May, and June, but with much larger standard errors that result in zero-effect null hypotheses not being rejected. 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=0.062).

[10] This expectation was also specified in our pre-analysis plan (Barkowski, McLaughlin, and Dai 2020).

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=0.002 and 0.030), 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=0.045 and 0.011) 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=0.0002 and 0.009), 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=0.014), though in this case the result seems to have been driven equally by men and women.

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. However, we do find some evidence that suggests that, conditional on being at work, women with young children responded to the pandemic by working about a half-hour to hour longer than women without young children, though this is not evident in all research designs.

In addition to the estimates for our preferred specification that are reported in Table 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 the minimal amount of controls.

Post-Hoc Analyses Results

Tables 4 through 7 and Appendix Table 3 present results from our post-hoc analyses. 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.[11] Their addition shows whether differences in the composition of the treatment and control groups across industries and

[11] 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.

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. None of the results suggests that the COVID-19 crisis caused a negative labor supply shock for parents. Instead, they show that part of our findings of positive effects come from non-uniform distribution of treatment and control individuals across industries and occupations.[12] This suggests that 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. Calculating 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, we find results that are very similar to our main estimates, undermining the possibility that differences in young 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

[12] 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.

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. However, we do obtain a statistically significant, negative estimate for May in research design 1 for men. This suggests at work men with young children worked about half-an-hour less than those without after the pandemic onset. Despite this, the overall impression of these results is not drastically different from our main results, and there is very 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.[13] 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 is 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 are similar to our main results, though they are not statistically significant in nearly all cases due to larger standard errors. Overall, these results do not suggest conclusions different from our main estimates.

[13] 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 final 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. Considering these estimates overall, we find most estimates are positive or not statistically significant. Our results for white respondents, college graduates, and urban residents most closely resemble those from the full-sample, suggesting these groups are important drivers of our main findings. In terms of negative estimates, we find these most often among single respondents. Via research design 1, we estimate they were less likely to be at work by 1.5 percentage points (p=0.031), 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 1.2 hours fewer each week (p=0.088), 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 more than 2 fewer hours per week, conditional on working (p=0.060). 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 after the pandemic onset (p=0.037). Considering our results for sub-groups as a whole, 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.

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. 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 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 other, very recent findings on school closures during the COVID-19 pandemic, as Rojas et al. (2020) finds no effect on unemployment insurance benefit filings while Heggeness (2020) finds no effect on being unemployed. Moreover, Heggeness (2020) also finds some evidence, for both men and 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. For example, 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.[14] 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.

Finally, 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 suggests that employment flexibility is potentially very important for parents, even in normal circumstances.

[14] This includes individuals who 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.

Policies that assist or otherwise encourage work flexibility could, therefore, potentially improve welfare of parents significantly. Research into the potential costs of such flexibility could offer important insights.

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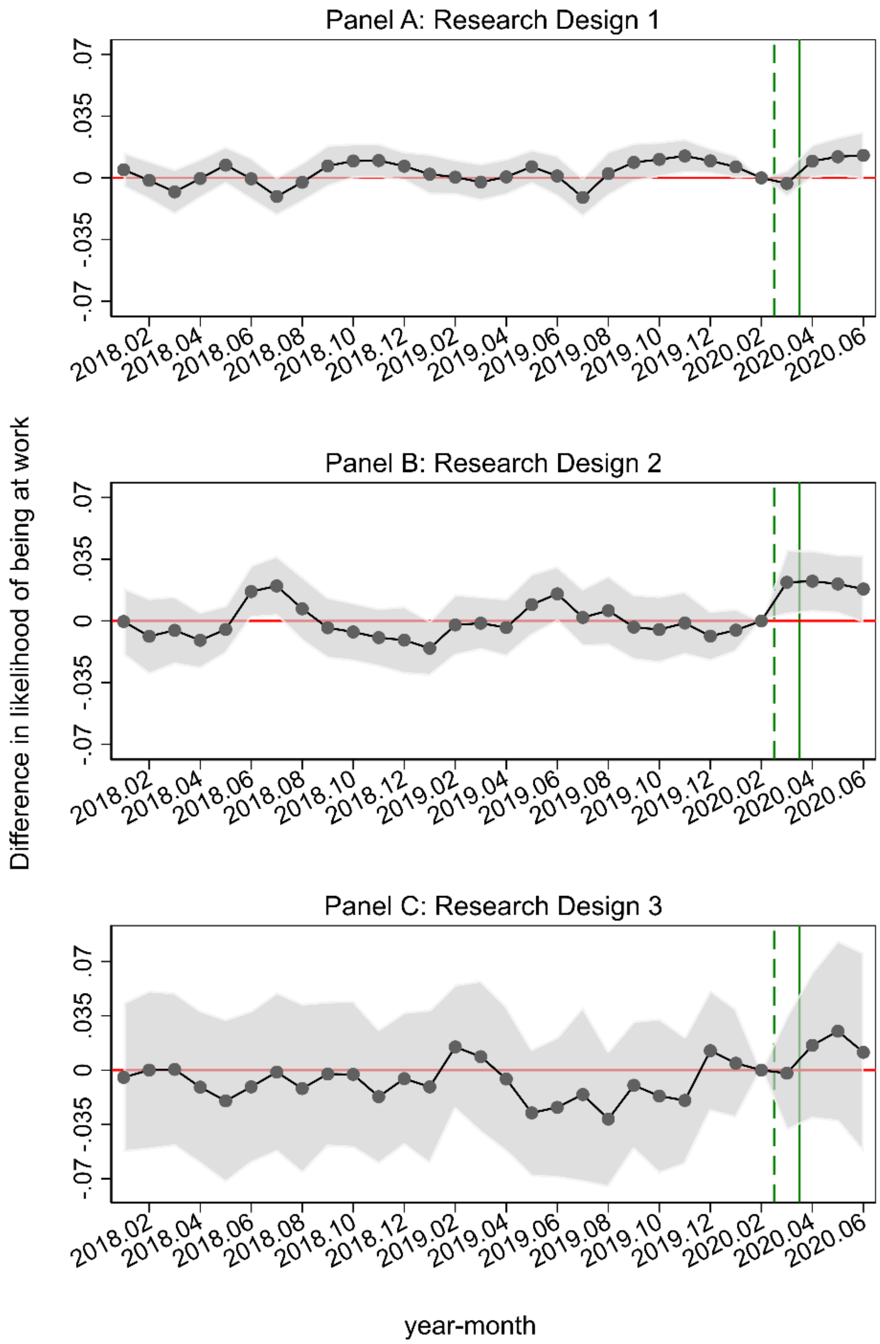
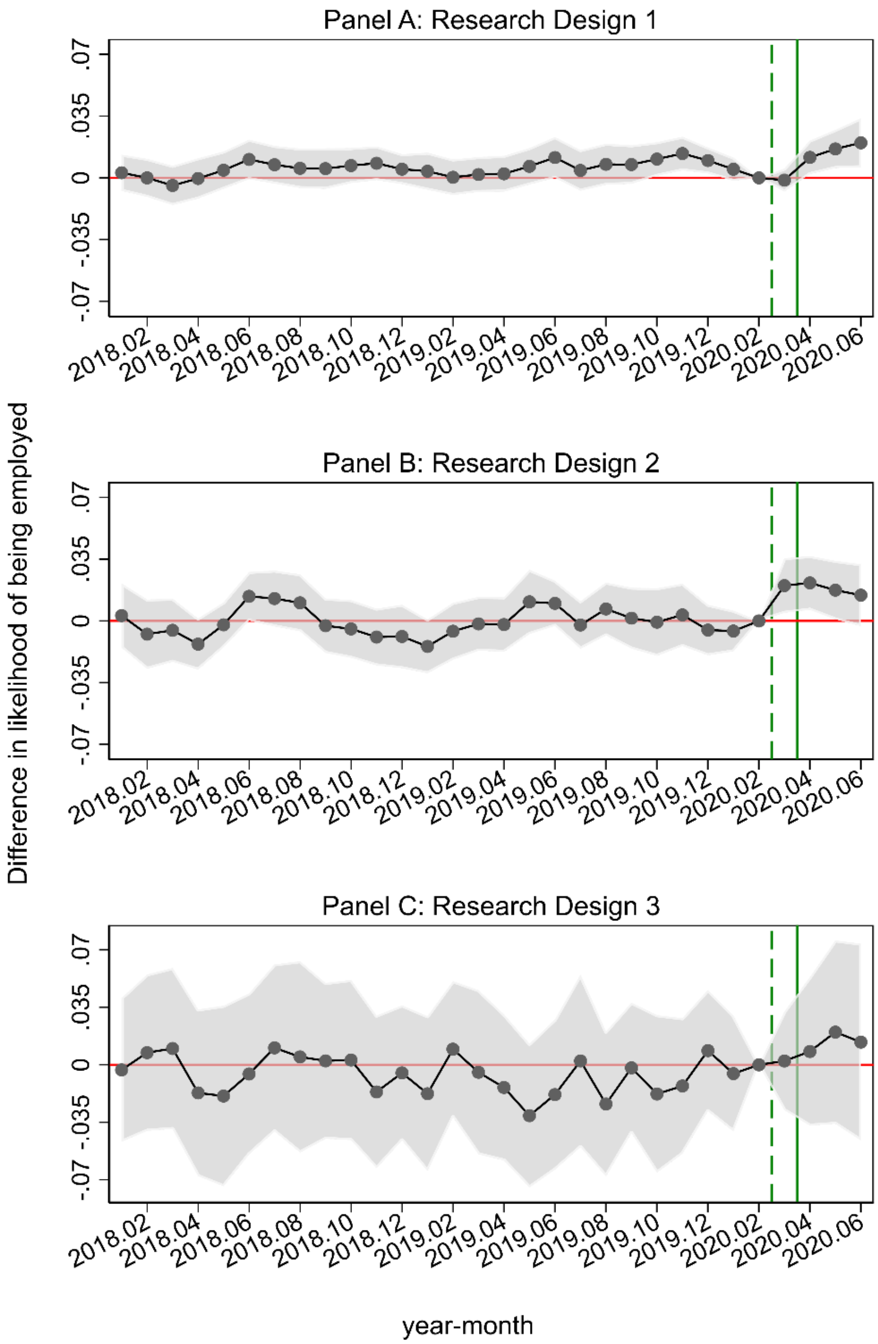
Figure 1: Difference in likelihood of being at work (treated group minus control)



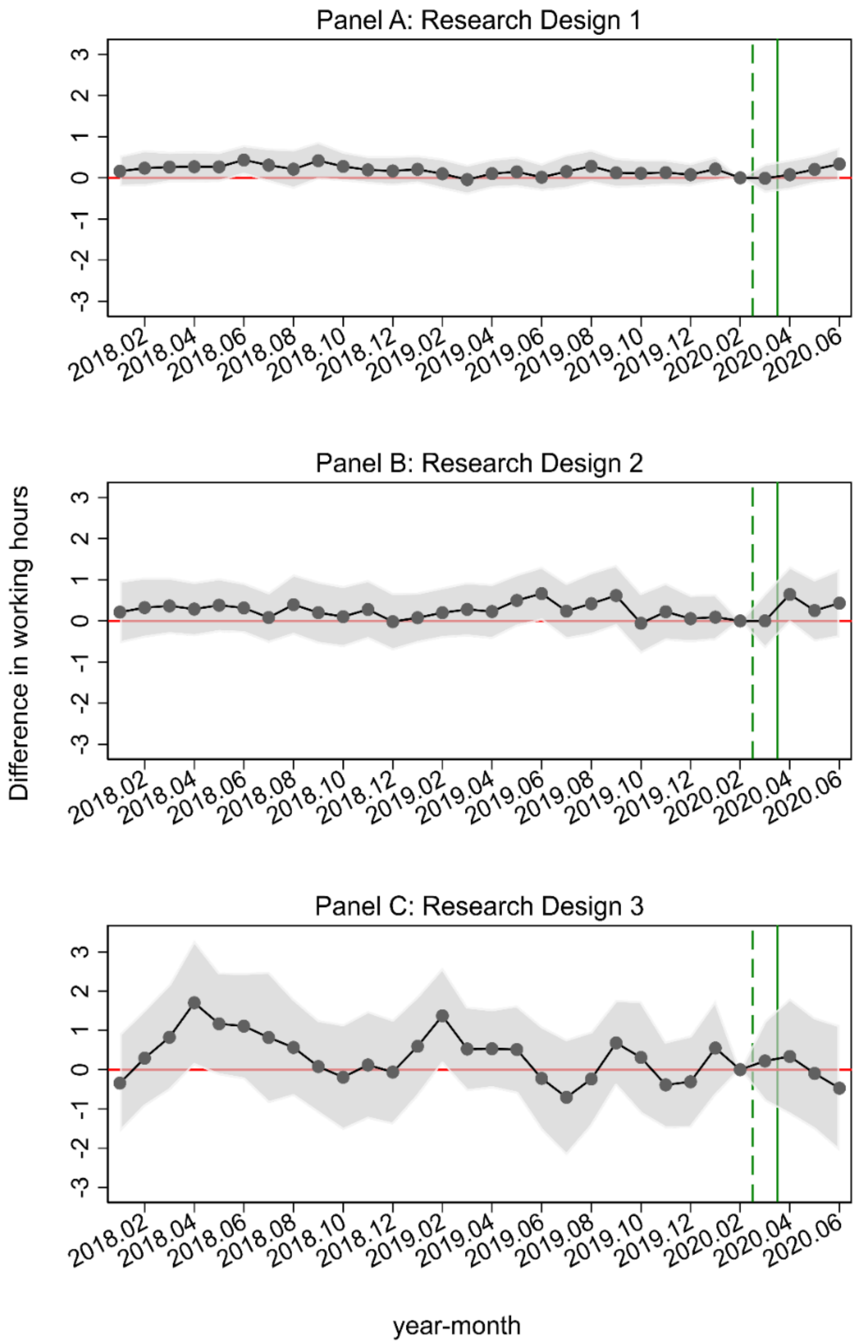
Notes: Shaded area represents 95% confidence intervals based on state-level clustered standard errors. Dashed, green vertical line indicates early pandemic stages (March 2020). Solid, green vertical line represents the start of the post-period (April 2020). Sample is basic monthly CPS for Jan 2018 – 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	0.770	0.764	0.768	0.774	0.773	0.687
	Post	0.704	0.690	0.709	0.690	0.715	0.614
Employed	Pre	0.799	0.787	0.799	0.800	0.804	0.712
	Post	0.749	0.727	0.755	0.733	0.761	0.657
Hours at work	Pre	40.278	40.734	40.204	40.472	40.331	37.835
	Post	39.265	39.601	39.168	39.523	39.269	37.244
Age	Pre	37.513	42.919	36.028	41.455	36.252	32.333
	Post	38.040	42.979	36.608	41.758	36.822	33.140
Age of oldest child	Pre	9.627	21.276	7.284	15.848	7.326	6.588
	Post	9.665	21.329	7.267	15.890	7.297	6.787
Age of youngest child	Pre	5.277	19.376	4.362	7.708	4.353	4.501
	Post	5.327	19.326	4.384	7.775	4.367	4.666
Female	Pre	0.550	0.497	0.549	0.552	0.540	0.699
	Post	0.543	0.500	0.543	0.542	0.534	0.690
White	Pre	0.800	0.799	0.800	0.801	0.807	0.686
	Post	0.803	0.799	0.806	0.794	0.814	0.684
Black	Pre	0.093	0.110	0.090	0.101	0.085	0.164
	Post	0.087	0.107	0.086	0.092	0.081	0.162
Hispanic	Pre	0.186	0.138	0.169	0.232	0.165	0.246
	Post	0.168	0.136	0.152	0.209	0.147	0.240
Married	Pre	0.775	0.475	0.766	0.801	0.792	0.333
	Post	0.805	0.482	0.796	0.830	0.823	0.367
Divorced or separated	Pre	0.082	0.151	0.074	0.102	0.068	0.177
	Post	0.072	0.141	0.067	0.084	0.060	0.177
Single	Pre	0.137	0.355	0.154	0.090	0.135	0.480
	Post	0.119	0.360	0.133	0.082	0.114	0.448
High school dropout	Pre	0.092	0.081	0.076	0.134	0.073	0.115
	Post	0.077	0.071	0.061	0.120	0.060	0.082
High school	Pre	0.251	0.301	0.248	0.259	0.239	0.393
	Post	0.233	0.288	0.228	0.246	0.219	0.379
Some college	Pre	0.261	0.266	0.257	0.270	0.255	0.303
	Post	0.256	0.268	0.251	0.267	0.247	0.321
College	Pre	0.243	0.236	0.255	0.210	0.262	0.138
	Post	0.261	0.247	0.277	0.221	0.284	0.152
In metro	Pre	0.234	0.270	0.240	0.218	0.239	0.255
	Post	0.233	0.270	0.241	0.213	0.240	0.243
In suburb	Pre	0.415	0.386	0.412	0.423	0.412	0.412
	Post	0.421	0.391	0.416	0.433	0.415	0.435
Not in metro	Pre	0.179	0.177	0.176	0.185	0.177	0.174
	Post	0.177	0.173	0.175	0.184	0.175	0.165
Observation Count	Pre	447,551	998,643	325,136	122,415	306,568	18,568
	Post	54,104	125,385	15,044	306,568	36,795	2,265

Notes: Sample and research design definitions described in the notes to Table 1. Calculated without sampling weights. Post-period is period after February 2020. Samples for hours at work and child ages are limited to those at work and those with children.

Table 2: Regression adjusted differences between treatment and control groups, unweighted 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=501,655	n=364,196	n=1,625,683	n=501,655	n=364,196	n=1,231,552	n=382,518	n=277,324
March 2020	-0.00318	0.0219**	-0.00197	-0.00129	0.0200**	0.00233	-0.0113	0.000529	0.222
	(0.00383)	(0.00914)	(0.0184)	(0.00324)	(0.00770)	(0.0149)	(0.174)	(0.333)	(0.511)
April 2020	0.00944*	0.0225**	0.0160	0.0116**	0.0215***	0.00811	0.0769	0.643*	0.337
	(0.00474)	(0.00874)	(0.0236)	(0.00472)	(0.00758)	(0.0225)	(0.181)	(0.337)	(0.736)
May 2020	0.0120**	0.0209**	0.0251	0.0164***	0.0174**	0.0199	0.206	0.251	-0.0971
	(0.00555)	(0.00837)	(0.0291)	(0.00538)	(0.00825)	(0.0278)	(0.172)	(0.375)	(0.710)
June 2020	0.0128*	0.0181*	0.0115	0.0199***	0.0146	0.0138	0.336	0.430	-0.471
	(0.00670)	(0.00958)	(0.0320)	(0.00688)	(0.00874)	(0.0298)	(0.201)	(0.410)	(0.796)
Panel B: Women	n=834,217	n=275,309	n=199,614	n=834,217	n=275,309	n=199,614	n=580,015	n=179,741	n=129,652
March 2020	-0.00489	0.0345***	0.0000861	-0.00336	0.0307**	0.00713	-0.0443	0.140	-0.147
	(0.00499)	(0.0124)	(0.0233)	(0.00406)	(0.0121)	(0.0213)	(0.167)	(0.376)	(0.868)
April 2020	0.00575	0.0407***	0.00326	0.00361	0.0421***	0.000885	0.268	0.752	0.643
	(0.00661)	(0.0120)	(0.0266)	(0.00652)	(0.0113)	(0.0256)	(0.267)	(0.501)	(0.891)
May 2020	0.00440	0.0453***	0.0335	0.00795	0.0417***	0.0384	0.732***	0.652	0.298
	(0.00741)	(0.0137)	(0.0335)	(0.00788)	(0.0137)	(0.0333)	(0.221)	(0.537)	(0.874)
June 2020	0.00520	0.0362***	0.0244	0.0130	0.0320**	0.0403	0.589**	0.330	-1.021
	(0.00918)	(0.0132)	(0.0351)	(0.00994)	(0.0136)	(0.0314)	(0.264)	(0.579)	(0.918)
Panel C: Men	n=791,466	n=226,346	n=164,582	n=791,466	n=226,346	n=164,582	n=651,537	n=202,777	n=147,672
March 2020	0.000903	0.00587	-0.0133	0.00276	0.00641	-0.0152	0.0113	-0.119	0.768
	(0.00583)	(0.0119)	(0.0273)	(0.00455)	(0.00822)	(0.0240)	(0.235)	(0.434)	(0.864)
April 2020	0.0158***	0.000694	0.0485	0.0223***	-0.00298	0.0268	-0.162	0.562	0.374
	(0.00541)	(0.0149)	(0.0397)	(0.00499)	(0.0103)	(0.0362)	(0.259)	(0.375)	(1.262)
May 2020	0.0230***	-0.00936	0.0151	0.0279***	-0.0127	-0.00997	-0.326	-0.0853	-0.227
	(0.00658)	(0.0135)	(0.0444)	(0.00579)	(0.0108)	(0.0430)	(0.246)	(0.481)	(1.207)
June 2020	0.0251***	-0.00482	-0.0217	0.0297***	-0.00741	-0.0386	0.0126	0.473	0.629
	(0.00817)	(0.0148)	(0.0495)	(0.00751)	(0.0119)	(0.0510)	(0.245)	(0.481)	(1.237)

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. Core sample is basic monthly CPS for Jan 2018 – June 2020 and includes non-military, non-student respondents ages 21—59. Each column within a panel represents a separate regression. Estimates measure the difference between treatment and control groups with Feb 2020 as reference period. All regressions are unweighted and include 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; employed workers include those temporarily absent from their jobs (e.g., sick, vacation). The number of hours worked is conditional on being at work. 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.

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=501,655	n=364,196	n=1,625,683	n=501,655	n=364,196	n=1,231,552	n=382,518	n=277,324
March 2020	-0.00474	0.0237*	-0.0101	0.00000688	0.0229**	-0.00366	-0.0788	0.208	0.163
	(0.00505)	(0.0128)	(0.0228)	(0.00453)	(0.0111)	(0.0161)	(0.227)	(0.384)	(0.515)
April 2020	0.0135**	0.0220*	0.0143	0.0139**	0.0216**	0.00748	0.0143	1.033**	0.399
	(0.00548)	(0.0118)	(0.0296)	(0.00603)	(0.0106)	(0.0258)	(0.201)	(0.406)	(0.772)
May 2020	0.0167***	0.0264***	0.0312	0.0195***	0.0228**	0.0208	0.400**	0.223	-0.283
	(0.00520)	(0.00865)	(0.0372)	(0.00530)	(0.00865)	(0.0332)	(0.195)	(0.367)	(0.887)
June 2020	0.0178**	0.0182	0.0208	0.0231***	0.0186*	0.0139	0.616**	0.507	-0.624
	(0.00691)	(0.0114)	(0.0369)	(0.00768)	(0.0101)	(0.0373)	(0.232)	(0.412)	(0.800)
Panel B: Women	n=834,217	n=275,309	n=199,614	n=834,217	n=275,309	n=199,614	n=580,015	n=179,741	n=129,652
March 2020	-0.00824	0.0367*	-0.00973	-0.00525	0.0354*	-0.00239	-0.0621	0.378	0.00713
	(0.00514)	(0.0194)	(0.0287)	(0.00472)	(0.0188)	(0.0216)	(0.194)	(0.440)	(1.034)
April 2020	0.0138**	0.0490***	0.00861	0.0100	0.0498***	0.00683	0.168	1.012*	0.199
	(0.00652)	(0.0140)	(0.0355)	(0.00785)	(0.0138)	(0.0325)	(0.301)	(0.595)	(0.991)
May 2020	0.00937	0.0593***	0.0563	0.0119*	0.0571***	0.0539	0.901***	0.429	-0.135
	(0.00624)	(0.0161)	(0.0407)	(0.00696)	(0.0157)	(0.0381)	(0.221)	(0.513)	(1.072)
June 2020	0.0111	0.0451***	0.0356	0.0173	0.0455***	0.0417	0.802***	0.453	-1.346
	(0.00855)	(0.0164)	(0.0355)	(0.0105)	(0.0154)	(0.0356)	(0.296)	(0.593)	(0.845)
Panel C: Men	n=791,466	n=226,346	n=164,582	n=791,466	n=226,346	n=164,582	n=651,537	n=202,777	n=147,672
March 2020	0.000112	0.00782	-0.0217	0.00634	0.00756	-0.0178	-0.124	0.0321	0.354
	(0.00841)	(0.0142)	(0.0307)	(0.00634)	(0.0111)	(0.0265)	(0.290)	(0.443)	(0.920)
April 2020	0.0155***	-0.00868	0.0373	0.0203***	-0.0106	0.0168	-0.154	1.008**	1.266
	(0.00555)	(0.0206)	(0.0428)	(0.00604)	(0.0149)	(0.0343)	(0.302)	(0.412)	(1.249)
May 2020	0.0272***	-0.0127	0.00169	0.0302***	-0.0177	-0.0239	-0.0949	0.00473	-0.0341
	(0.00706)	(0.0190)	(0.0521)	(0.00631)	(0.0140)	(0.0460)	(0.248)	(0.525)	(1.185)
June 2020	0.0293***	-0.0152	-0.00647	0.0320***	-0.0149	-0.0316	0.389	0.493	0.814
	(0.00802)	(0.0172)	(0.0557)	(0.00772)	(0.0150)	(0.0576)	(0.278)	(0.541)	(1.234)

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=501,652	n=364,190	n=1,625,683	n=501,652	n=364,190	n=1,231,552	n=382,514	n=277,317
March 2020	-0.00138	0.00752	-0.00904	0.000434	0.00511	-0.00526	-0.0273	0.0765	0.254
	(0.00334)	(0.00608)	(0.0132)	(0.00197)	(0.00403)	(0.00896)	(0.160)	(0.334)	(0.522)
April 2020	0.00833*	0.00540	0.0227	0.0102***	0.00445	0.0153	0.0676	0.624*	0.664
	(0.00419)	(0.00750)	(0.0139)	(0.00336)	(0.00609)	(0.0133)	(0.171)	(0.356)	(0.752)
May 2020	0.00399	-0.00280	0.0249	0.00786*	-0.00736	0.0213	0.146	0.382	0.482
	(0.00466)	(0.00647)	(0.0201)	(0.00395)	(0.00498)	(0.0164)	(0.168)	(0.390)	(0.705)
June 2020	0.00120	0.000569	0.0103	0.00796*	-0.00382	0.0127	0.308	0.471	-0.232
	(0.00437)	(0.00707)	(0.0158)	(0.00415)	(0.00542)	(0.0130)	(0.184)	(0.391)	(0.792)
Panel B: Women	n=834,214	n=275,294	n=199,596	n=834,214	n=275,294	n=199,596	n=580,010	n=179,724	n=129,632
March 2020	-0.000199	0.0112	-0.0130	0.00152	0.00648	-0.00667	-0.0790	0.222	-0.0844
	(0.00446)	(0.00753)	(0.0168)	(0.00321)	(0.00619)	(0.0133)	(0.159)	(0.377)	(0.830)
April 2020	0.00982	0.0118	0.00608	0.00812	0.0132*	0.00388	0.224	0.717	0.345
	(0.00613)	(0.00902)	(0.0189)	(0.00500)	(0.00702)	(0.0169)	(0.244)	(0.504)	(0.886)
May 2020	0.00187	0.00746	0.000571	0.00574	0.00288	0.00841	0.607***	0.749	0.311
	(0.00597)	(0.00865)	(0.0217)	(0.00579)	(0.00689)	(0.0172)	(0.212)	(0.523)	(0.914)
June 2020	-0.00116	0.0137	-0.0102	0.00682	0.00866	0.00499	0.535**	0.334	-1.365
	(0.00567)	(0.00849)	(0.0193)	(0.00565)	(0.00604)	(0.0157)	(0.255)	(0.555)	(0.968)
Panel C: Men	n=791,465	n=226,337	n=164,571	n=791,465	n=226,337	n=164,571	n=651,536	n=202,765	n=147,659
March 2020	-0.00198	0.00186	-0.00466	-0.000474	0.00260	-0.00621	0.0211	0.0315	0.756
	(0.00462)	(0.00882)	(0.0220)	(0.00266)	(0.00515)	(0.0181)	(0.225)	(0.420)	(0.930)
April 2020	0.00760	-0.00209	0.0553*	0.0132***	-0.00507	0.0320	-0.147	0.537	1.638
	(0.00515)	(0.0129)	(0.0281)	(0.00486)	(0.00826)	(0.0244)	(0.254)	(0.375)	(1.362)
May 2020	0.00726	-0.0145	0.0608	0.0110**	-0.0176**	0.0349	-0.328	0.0187	1.195
	(0.00582)	(0.0121)	(0.0395)	(0.00472)	(0.00819)	(0.0372)	(0.236)	(0.481)	(1.221)
June 2020	0.00592	-0.0138	0.0273	0.00970*	-0.0157*	0.00817	0.0339	0.476	1.763
	(0.00605)	(0.0110)	(0.0335)	(0.00525)	(0.00879)	(0.0329)	(0.227)	(0.463)	(1.305)

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=256,199	n=185,970	n=829,721	n=256,199	n=185,970	n=627,888	n=195,162	n=141,475
March 2020	-0.00867	0.0167	-0.0106	-0.00840	0.0157	-0.0162	0.0897	-0.165	-0.173
	(0.00673)	(0.0144)	(0.0256)	(0.00520)	(0.0127)	(0.0233)	(0.239)	(0.454)	(0.702)
April 2020	0.00245	0.0256	0.0303	0.00250	0.0226*	0.00945	0.00235	0.638	0.0524
	(0.00690)	(0.0153)	(0.0307)	(0.00641)	(0.0135)	(0.0331)	(0.283)	(0.431)	(1.134)
May 2020	0.000975	0.0194	0.0819**	0.00312	0.0121	0.0717**	0.179	0.135	0.222
	(0.00907)	(0.0146)	(0.0348)	(0.00776)	(0.0126)	(0.0355)	(0.281)	(0.480)	(0.923)
June 2020	0.0135	0.0255*	0.0372	0.0160*	0.0104	0.0205	0.261	0.116	0.0925
	(0.00911)	(0.0147)	(0.0336)	(0.00896)	(0.0137)	(0.0331)	(0.300)	(0.656)	(1.026)

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=487,236	n=353,762	n=1,578,417	n=487,236	n=353,762	n=1,195,624	n=371,521	n=269,311
Full Sample	0.00881**	0.0217***	0.0255	0.0106***	0.0187***	0.0198	0.0188	0.193	-0.448
	(0.00376)	(0.00629)	(0.0193)	(0.00376)	(0.00535)	(0.0204)	(0.130)	(0.207)	(0.499)
Single	-0.0154**	0.0146	-0.0123	-0.00386	0.0103	-0.00247	-0.216	-0.110	-0.774
	(0.00693)	(0.0158)	(0.0228)	(0.00733)	(0.0161)	(0.0235)	(0.232)	(0.492)	(0.644)
Women only	-0.0164*	0.0137	-0.0188	-0.00650	0.0224	-0.00648	-0.296	-0.0636	-1.258*
	(0.00933)	(0.0190)	(0.0261)	(0.00937)	(0.0183)	(0.0258)	(0.211)	(0.658)	(0.723)
Men only	-0.0243*	0.0198	0.0112	-0.0111	-0.0153	0.0190	-0.263	-0.556	0.664
	(0.0132)	(0.0314)	(0.0371)	(0.0136)	(0.0284)	(0.0393)	(0.410)	(0.900)	(1.354)
Married	0.000327	0.0259***	0.0237	0.00118	0.0225***	0.0107	0.00418	0.257	-0.532
	(0.00465)	(0.00809)	(0.0225)	(0.00450)	(0.00740)	(0.0232)	(0.143)	(0.247)	(0.681)
Women only	0.00504	0.0541***	0.0108	0.00502	0.0490***	0.00373	0.295	0.0922	-0.729
	(0.00639)	(0.0109)	(0.0332)	(0.00638)	(0.0111)	(0.0273)	(0.224)	(0.446)	(1.044)
Men only	-0.00371	-0.00368	0.0315	-0.00181	-0.00568	0.0123	-0.178	0.341	-0.352
	(0.00513)	(0.00973)	(0.0237)	(0.00446)	(0.00782)	(0.0290)	(0.194)	(0.257)	(1.092)
White	0.00924**	0.0208***	0.0254	0.0119***	0.0173***	0.0173	-0.0251	0.0339	-0.412
	(0.00439)	(0.00627)	(0.0230)	(0.00416)	(0.00597)	(0.0237)	(0.143)	(0.231)	(0.634)
Black	0.000354	0.0199	-0.00762	0.00181	0.0199	-0.00727	-0.0142	1.174**	-2.022*
	(0.0113)	(0.0270)	(0.0464)	(0.0115)	(0.0224)	(0.0392)	(0.288)	(0.549)	(1.052)
Hispanic	0.00840	0.0278*	-0.00341	0.00796	0.0197	-0.0324	0.167	0.696**	-0.0297
	(0.0112)	(0.0149)	(0.0405)	(0.0101)	(0.0168)	(0.0439)	(0.306)	(0.346)	(0.662)
High school non-grad	-0.0177	0.0681***	0.0399	-0.0141	0.0602***	0.0442	-0.668	-0.580	1.576
	(0.0164)	(0.0186)	(0.0643)	(0.0155)	(0.0161)	(0.0623)	(0.520)	(0.625)	(2.048)
High school grad	-0.00347	0.00778	-0.0168	0.00292	0.00801	-0.0119	0.154	0.0798	-0.877
	(0.00729)	(0.0134)	(0.0316)	(0.00722)	(0.0131)	(0.0299)	(0.210)	(0.439)	(0.705)
Some college	-0.00504	0.00273	0.00139	0.00220	-0.00705	-0.00590	0.223	0.0125	-0.611
	(0.00915)	(0.0110)	(0.0222)	(0.00920)	(0.00966)	(0.0254)	(0.238)	(0.411)	(0.892)
College grad	0.0242***	0.0130	0.0547*	0.0215***	0.0168*	0.0522	0.0756	0.344	-0.374
	(0.00542)	(0.00912)	(0.0318)	(0.00544)	(0.00871)	(0.0338)	(0.216)	(0.311)	(0.734)
Urban resident	0.0153*	0.0324**	0.0115	0.0152**	0.0269**	0.00173	0.300	0.514	-0.521
	(0.00763)	(0.0128)	(0.0413)	(0.00740)	(0.0124)	(0.0408)	(0.257)	(0.535)	(0.836)

Suburban resident	0.00684	0.0205*	0.0392*	0.00719	0.0207**	0.0234	0.0626	0.262	-0.178
	(0.00503)	(0.0119)	(0.0211)	(0.00532)	(0.0101)	(0.0242)	(0.199)	(0.295)	(0.675)
Rural resident	-0.000546	0.0138	0.0185	0.00698	0.00111	0.0267	-0.491	0.379	-1.795
	(0.00785)	(0.0136)	(0.0483)	(0.00877)	(0.0121)	(0.0464)	(0.299)	(0.539)	(1.418)
Metro unknown	0.0131	0.0281**	0.00724	0.0160*	0.0301**	0.0207	-0.0447	-0.604	-0.177
	(0.00842)	(0.0139)	(0.0527)	(0.00850)	(0.0142)	(0.0478)	(0.284)	(0.508)	(1.282)

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.

Appendix

Appendix Table 1: Regression adjusted differences between treatment and control groups, with 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=364,196	n=1,625,683	n=501,655	n=364,196	n=1,231,552	n=382,518	n=277,324
September 2019	0.00877	-0.00356	-0.00979	0.00753	0.00154	-0.00185	0.122	0.616	0.682
	(0.00548)	(0.00929)	(0.0206)	(0.00542)	(0.00858)	(0.0197)	(0.180)	(0.370)	(0.544)
October 2019	0.0104**	-0.00492	-0.0167	0.0106**	-0.000704	-0.0178	0.110	-0.0584	0.311
	(0.00498)	(0.00944)	(0.0249)	(0.00474)	(0.00951)	(0.0239)	(0.165)	(0.366)	(0.713)
November 2019	0.0124**	-0.00121	-0.0195	0.0138***	0.00341	-0.0129	0.128	0.223	-0.389
	(0.00485)	(0.00894)	(0.0205)	(0.00474)	(0.00880)	(0.0205)	(0.155)	(0.348)	(0.550)
December 2019	0.00964**	-0.00864	0.0125	0.00982**	-0.00519	0.00865	0.0772	0.0538	-0.307
	(0.00367)	(0.00709)	(0.0195)	(0.00367)	(0.00712)	(0.0183)	(0.139)	(0.285)	(0.585)
January 2020	0.00623*	-0.00526	0.00442	0.00487	-0.00575	-0.00539	0.214	0.0905	0.551
	(0.00327)	(0.00607)	(0.0175)	(0.00327)	(0.00573)	(0.0175)	(0.150)	(0.278)	(0.603)
				February 2020 – Reference Period					
March 2020	-0.00318	0.0219**	-0.00197	-0.00129	0.0200**	0.00233	-0.0113	0.000529	0.222
	(0.00383)	(0.00914)	(0.0184)	(0.00324)	(0.00770)	(0.0149)	(0.174)	(0.333)	(0.511)
April 2020	0.00944*	0.0225**	0.0160	0.0116**	0.0215***	0.00811	0.0769	0.643*	0.337
	(0.00474)	(0.00874)	(0.0236)	(0.00472)	(0.00758)	(0.0225)	(0.181)	(0.337)	(0.736)
May 2020	0.0120**	0.0209**	0.0251	0.0164***	0.0174**	0.0199	0.206	0.251	-0.0971
	(0.00555)	(0.00837)	(0.0291)	(0.00538)	(0.00825)	(0.0278)	(0.172)	(0.375)	(0.710)
June 2020	0.0128*	0.0181*	0.0115	0.0199***	0.0146	0.0138	0.336	0.430	-0.471
	(0.00670)	(0.00958)	(0.0320)	(0.00688)	(0.00874)	(0.0298)	(0.201)	(0.410)	(0.796)

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=501,655	n=364,196	n=1,625,683	n=501,655	n=364,196	n=1,231,552	n=382,518	n=277,324
March 2020	-0.00344	0.0214**	-0.00155	-0.00154	0.0196**	0.00334	0.0135	-0.105	0.0257
	(0.00414)	(0.00926)	(0.0184)	(0.00335)	(0.00781)	(0.0152)	(0.184)	(0.336)	(0.572)
April 2020	0.00996*	0.0229**	0.0203	0.0120**	0.0222**	0.0129	0.181	0.484	0.694
	(0.00518)	(0.00938)	(0.0233)	(0.00502)	(0.00863)	(0.0230)	(0.185)	(0.328)	(0.736)
May 2020	0.0123*	0.0211**	0.0253	0.0168**	0.0179*	0.0207	0.323*	0.0396	-0.0402
	(0.00702)	(0.00929)	(0.0282)	(0.00685)	(0.00950)	(0.0268)	(0.176)	(0.361)	(0.738)
June 2020	0.0105	0.0184	0.0189	0.0177**	0.0154	0.0217	0.426**	0.263	-0.254
	(0.00783)	(0.0114)	(0.0306)	(0.00826)	(0.0110)	(0.0285)	(0.199)	(0.414)	(0.756)
Panel B: Women	n=834,217	n=275,309	n=199,614	n=834,217	n=275,309	n=199,614	n=580,015	n=179,741	n=129,652
March 2020	-0.00568	0.0347**	0.00625	-0.00395	0.0312**	0.0144	-0.0548	0.173	-0.157
	(0.00518)	(0.0131)	(0.0246)	(0.00441)	(0.0126)	(0.0231)	(0.172)	(0.365)	(0.873)
April 2020	0.00328	0.0435***	-0.00406	0.00113	0.0453***	-0.00498	0.298	0.742	0.698
	(0.00668)	(0.0139)	(0.0270)	(0.00673)	(0.0133)	(0.0266)	(0.274)	(0.508)	(0.894)
May 2020	0.00194	0.0492***	0.0209	0.00583	0.0460***	0.0264	0.776***	0.594	0.117
	(0.00863)	(0.0168)	(0.0340)	(0.00914)	(0.0165)	(0.0334)	(0.227)	(0.523)	(0.906)
June 2020	0.000330	0.0416**	0.0173	0.00847	0.0379**	0.0341	0.582**	0.350	-1.129
	(0.0106)	(0.0162)	(0.0359)	(0.0118)	(0.0167)	(0.0316)	(0.263)	(0.576)	(0.892)
Panel C: Men	n=791,466	n=226,346	n=164,582	n=791,466	n=226,346	n=164,582	n=651,537	n=202,777	n=147,672
March 2020	-0.00155	0.00764	-0.0138	0.000254	0.00791	-0.0154	-0.0161	-0.151	0.644
	(0.00608)	(0.0117)	(0.0273)	(0.00453)	(0.00809)	(0.0246)	(0.246)	(0.435)	(0.870)
April 2020	0.0168***	-0.00146	0.0590	0.0233***	-0.00508	0.0383	-0.221	0.501	0.338
	(0.00626)	(0.0152)	(0.0397)	(0.00538)	(0.0104)	(0.0379)	(0.262)	(0.373)	(1.316)
May 2020	0.0216***	-0.0115	0.0191	0.0266***	-0.0147	-0.00504	-0.396	-0.121	-0.130
	(0.00769)	(0.0134)	(0.0446)	(0.00689)	(0.0108)	(0.0436)	(0.255)	(0.500)	(1.214)
June 2020	0.0208**	-0.00811	-0.00803	0.0255***	-0.0102	-0.0236	-0.0498	0.445	0.836
	(0.00906)	(0.0151)	(0.0500)	(0.00833)	(0.0122)	(0.0513)	(0.250)	(0.493)	(1.199)

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=501,655	n=364,196		n=501,655	n=364,196		n=382,518	n=277,324
March 2020	...	0.0220**	0.00114	...	0.0203**	0.00458	...	0.0102	0.248
		(0.00907)	(0.0182)		(0.00771)	(0.0147)		(0.334)	(0.512)
April 2020	...	0.0233**	0.0177	...	0.0224***	0.00947	...	0.654*	0.355
		(0.00904)	(0.0233)		(0.00770)	(0.0221)		(0.335)	(0.739)
May 2020	...	0.0204**	0.0270	...	0.0174**	0.0213	...	0.261	-0.0838
		(0.00850)	(0.0288)		(0.00824)	(0.0274)		(0.373)	(0.709)
June 2020	...	0.0188*	0.0127	...	0.0152*	0.0144	...	0.443	-0.452
		(0.00977)	(0.0316)		(0.00878)	(0.0294)		(0.412)	(0.795)
Panel B: Women		n=275,309	n=199,614		n=275,309	n=199,614		n=179,741	n=129,652
March 2020	...	0.0345***	0.00543	...	0.0312**	0.0113	...	0.166	-0.0726
		(0.0122)	(0.0229)		(0.0121)	(0.0208)		(0.377)	(0.858)
April 2020	...	0.0428***	0.00731	...	0.0443***	0.00452	...	0.801	0.701
		(0.0116)	(0.0266)		(0.0112)	(0.0252)		(0.498)	(0.894)
May 2020	...	0.0450***	0.0371	...	0.0422***	0.0417	...	0.701	0.350
		(0.0131)	(0.0330)		(0.0134)	(0.0325)		(0.542)	(0.873)
June 2020	...	0.0375***	0.0245	...	0.0332**	0.0404	...	0.352	-0.982
		(0.0133)	(0.0340)		(0.0136)	(0.0306)		(0.583)	(0.906)
Panel C: Men		n=226,346	n=164,582		n=226,346	n=164,582		n=202,777	n=147,672
March 2020	...	0.00598	-0.0128	...	0.00653	-0.0149	...	-0.125	0.783
		(0.0119)	(0.0273)		(0.00823)	(0.0240)		(0.434)	(0.866)
April 2020	...	0.000823	0.0483	...	-0.00280	0.0264	...	0.557	0.386
		(0.0149)	(0.0398)		(0.0102)	(0.0363)		(0.376)	(1.260)
May 2020	...	-0.00925	0.0149	...	-0.0125	-0.0105	...	-0.0927	-0.222
		(0.0136)	(0.0444)		(0.0108)	(0.0429)		(0.479)	(1.203)
June 2020	...	-0.00447	-0.0215	...	-0.00707	-0.0390	...	0.464	0.638
		(0.0148)	(0.0495)		(0.0119)	(0.0509)		(0.479)	(1.228)

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 design 1 since some members of the research design 1 control group do not have children or are not living with one.