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THE IMPACT OF U.S. SCHOOL CLOSURES ON LABOR MARKET OUTCOMES DURING THE COVID-19 PANDEMIC

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ABSTRACT

A substantial fraction of k-12 schools in the United States closed their in-person operations during the COVID-19 pandemic. These closures may have altered the labor supply decisions of parents of affected children due to a need to be at home with children during the school day. In this paper, we examine the impact of school closures on parental labor market outcomes. We test whether COVID-19 school closures have a disproportionate impact on parents of school-age children (ages 5-17 years old). Our results show that both women's and men's work lives were affected by school closures, with both groups seeing a reduction in work hours and the likelihood of working full-time but only women being less likely to work at all. We also find that closures had a corresponding negative effect on the earnings of parents of school-aged children. These effects are concentrated among parents without a college degree and parents working in occupations that do not lend themselves to telework, suggesting that such individuals had a more difficult time adjusting their work lives to school closures.

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1. Introduction

As part of their efforts to curb COVID-19 in the spring of 2020, many state and local governments implemented lockdowns that resulted in the temporary closure of schools and childcare facilities. In the United States, shifts to distance learning began in March 2020 with 27 states recommending that schools temporarily cease in-person operations. By May 2020, all states except Wyoming and Montana recommended school building closures for the remainder of the academic year, affecting at least 50.8 million public school students ("The Coronavirus Spring", 2020). At the beginning of the next academic year in fall 2020, school closures began to be distributed unevenly across the country due to varying decisions made at the state and local level (Parolin and Lee, 2021).[1]

School closures have been controversial: data on almost 200,000 children from 47 states revealed an infection rate of only 0.13 percent among students and 0.24 percent among staff in September 2020 (Oster, 2020). Bravata et al. (2021) used millions of household-week level mobile phone data over the first 46 weeks of 2020 to find that an increase from the 25th percentile to the 75th percentile of the frequency of school visits increased the risk of COVID-19 infection in a household with children by approximately four percent. At the same time, there is evidence that closures carry significant costs in terms of the health and learning outcomes of children (e.g. Azevedo et al., 2020; Kuhfeld et al., 2020; Bailey et al., 2021; Engzell, Frey and Verhagen, 2021; Larsen, Helland and Holt, 2021; Halloran et al., 2021; Fuchs-Schündeln et al., 2021; Goldhaber et al., 2022; Agostinelli et al., 2022). The costs of school closures are also borne by parents. With 40 percent (or 33 million families) of all families having children under 18 years old in 2020 (US Bureau of Labor Statistics, 2021), there is much anecdotal evidence that school closures have affected parental labor supply due to difficulties in balancing work and childcare responsibilities (e.g. Brodeur, 2020; Leonhardt, 2020; Tedeschi, 2020; see also Musaddiq et al., 2021).

Several recent studies have found that women's labor market outcomes were disproportionately harmed by the pandemic relative to men. On one hand, leisure/hospitality and other service industries, which disproportionately employ women, were initially more harmed by the pandemic (e.g. Lee, Park and Shin, 2021; Albanesi and Kim, 2021a). On the other, additional childcare responsibilities owing to closed schools or childcare facilities or parental concerns regarding COVID risk may have exacerbated the gender gap in employment outcomes (Alon et al., 2020; Alon et al., 2021; Heggeness, 2020; Russell and Sun, 2020; Fairlie, Couch and Xu, 2021; Furman, Kearney and Powell, 2021; Barkowski, McLaughlin, and Dai, 2021; and Amuedo-Dorantes et al., 2020). The relative importance of labor supply factors versus demand factors in determining COVID-19 employment outcomes by gender is still not clear.

School closures arguably represent a shock to parental labor supply. Though pandemic conditions obviously contribute to closures while having a direct effect on labor-market outcomes, we posit it that it is unlikely that such conditions on their own would disproportionately affect the outcomes of those with school-age children. Thus, by using

[1] During this time, some parents transferred their children from public to private schools as the latter had more autonomy or flexibility to adhere to COVID-19 health protocols while maintaining in-person operations (Dickler, 2020; Reilly, 2020).

individuals without children in these age ranges as a control group, we may be able to tease out the effects of closures on labor-market outcomes.

To date, information on the impact of school closures on labor market outcomes has been fragmented, mostly covering the early months of the pandemic in spring 2020. We examine the period January 2020 to December 2021, as it covers both early and late periods of the pandemic and the first full academic term in which many schools closed their in-person operations and switched (at least in part) to remote learning. Due to our longer sample period, we can address both lagged school closure effects as well as how effects change over the course of the pandemic. In addition, along with Hansen, Sabia, and Schaller (2022), we make use of aggregated and anonymized mobile phone data from *Safegraph* to measure school closures locally, which likely allows for a more accurate picture of the in-person activity in a school relative to administrative data sources used in earlier studies, which often only classify schools into in-person, hybrid, and remote categories with possible inconsistencies across place and time.

Moreover, we examine a more comprehensive set of outcomes than other studies, including not only the probability of being at work and work hours but also working remotely due to COVID, being out of the labor force due to family/childcare obligations, and notably, earnings. To date, we are not aware of another study that examines how school closures affect the earnings of parents. Lastly, we examine heterogeneity in our results by gender, education level, marital status, race, and occupational classification. This leads to important insights into where school closures had the strongest effects on parents' work lives, which we can then compare to recent evidence on how school closures have affected children's academic achievement.

Our results suggest school closures have affected the labor-market outcomes of both mothers and fathers of school-age children. The reduction in weekly hours worked is a little less than 1 hour for women and a little more than 1 hour for men. Men and women experience a reduction in working full-time (at least 35 hours per week), but only mothers see a drop in their probability of being at work at all (of about 1.5 percentage points). Women are more likely to report being out of the labor force to care for children/family by about the same amount, with no corresponding effect for men. Both men and women with school-age children are significantly more likely to report doing remote work due to COVID with school closures. Thus, although in some ways mothers have been uniquely affected by school closures, the burden of childcare appears to have fallen more equally on mothers and fathers over the full pandemic period than it did in its very early days (see Heggeness, 2020; and Collins et al., 2021). We also measure large economic reductions in weekly earnings among both men and women with school-age children due to school closures, but in the full sample these are somewhat imprecisely estimated. This is likely due in part to earnings only being available for a fraction of our full sample (outgoing rotation groups in the Current Population Survey).

One of our major findings is that effects on any work, full-time work, and work hours are concentrated among women without a college degree rather than women with a degree. The same is true for men. These striking differences by college degree attainment could be due to at least two factors: first, less-educated individuals likely had a harder time arranging a flexible, at-home work schedule than those with more education. This is consistent with the findings of Mongey, Pilossoph, and Weinberg (2020) that 82 percent of individuals with less than college education are in occupations with low ability to work from home. It is also consistent with our findings that college-educated parents see the lion's share of the increase in self-reported remote work when schools close. Second, less educated parents may have not been able to secure options such as private schooling or alternative childcare arrangements to the degree that more educated parents did (Murnane et al., 2018; Musaddiq et al., 2021). For mothers without a college degree, we find a statistically significant (at the 5% level) 16 percent reduction in weekly earnings due to school closures.

2. Related Literature

Several studies have now analyzed how COVID-19 has affected employment outcomes across gender and parental status. Albanesi and Kim (2021a), Fairlie, Couch, and Xu (2021), and Alon et al. (2021) all find that the pandemic has disproportionately reduced the labor-market activity of women with children. In decomposition analyses, these papers reach different conclusions about the relative importance of occupational characteristics, childcare responsibilities, and other factors in explaining this drop. Furman, Kearney and Powell (2021) quantify the effect of parent-specific issues, such as childcare challenges, on the aggregate employment deficit in early 2021 relative to before the pandemic by constructing counterfactual employment and labor force participation rates that assign the mothers of young children the percentage change in employment and labor force participation rates of comparable mothers without young children. They find that the differential job loss among mothers is not a major driver of the overall decline in employment due to 1) demographically similar women without children also having declines in employment over the pandemic, and 2) the small fraction of mothers of young children in the US workforce. Nevertheless, Furman, Kearney and Powell (2021), Heggeness and Suri (2021), and Lofton, Petrosky-Nadeau, and Seitelman (2021) all find that over the pandemic, mothers' employment has declined at least modestly relative to those of women without children as well as fathers.

The literature on school/childcare availability and parental labor supply prior to COVID has generally found positive effects on mothers' employment and work hours (Gelbach, 2002; Baker, Gruber, and Milligan, 2008). Many recent papers have begun to analyze the impact of COVID-19 school and childcare closures on parental labor market outcomes. Several early papers rely on state-by-state variation in COVID restrictions related to childcare and school closures for identification. Russell and Sun (2020) assess the effects of childcare closures and class size restrictions on employment using a triple-differences approach in which being a mother of a child aged 0-5 is interacted with state-level mandates and time (only women are analyzed in their paper). They find that both restriction types increase the unemployment rate of mothers of young children in the short-run, and the impact persists even after states lift the restrictions (through September 2020, the end of their sample window), consistent with a permanent reduction in childcare centers stemming from initial closure mandates.

Heggeness (2020) uses state-level variation in the timing of shutdowns in the early part of the pandemic and CPS data from January to May of 2019 and 2020 to estimate the immediate impact of school closures on employment. She finds that working mothers of school-age children coped differently than working fathers: while mothers on average took a full week of leave from formal work in the initial phase of the pandemic, there was no corresponding effect for fathers (though full-time fathers did reduce their hours worked by 0.53 hours per week). Similarly, Collins et al. (2021) find that the gender gap in parental labor force participation grew five percentage points (relative to 2019) in states that offered primarily remote elementary instruction in September 2020 but only one percentage point in states that were primarily in-person or hybrid.

Amuedo-Dorantes et al. (2020) exploit local variation to identify the impact of school closures on employment outcomes in the early months of the pandemic. They calculate a daily school closure index (0 to 1) at the district level from *Education Week*. They do not, however, examine how school closures affect the outcomes of those who are not parents of young children (a control group in our analysis). Their findings suggest that school closures primarily affected the labor supply of mothers and fathers of younger school-age children in two-partnered households through the intensive margin (an 11 and 15 percent decline in the weekly hours worked conditional on working at all by men and women, respectively). The authors also only focus on the early months of the pandemic as their dataset runs from January 2019 through May 2020. Meanwhile, Koppa and West (2021) examine how the decision of whether to *start* the 2020-21 academic year with remote learning affects county-level employment, finding little evidence of a relationship. However, their analysis only considers aggregate employment—for example, neither hours worked nor outcomes of parents specifically are considered.

The paper that most closely resembles our study is that of Hansen, Sabia, and Schaller (2022; hereafter HSS).[2] Those authors also use Current Population Survey (CPS) data to measure labor-market outcomes and *Safegraph* data to measure school closures (or, in their case, the opposite condition of re-opening). However, there are differences in how each paper constructs school closures (re-openings) and in the CPS sample used. We highlight these in the next section. HSS focus on married mothers with school-age children as their treatment group, finding much smaller effects of school closures on other types of women as well as fathers. In our study, however, we find meaningful labor-market effects of school closures for both married and unmarried women and men with school-age children. We also explicitly use parents who only have children younger than age 5 as a placebo group in our analysis (since such children are not directly affected by school closures).

Our results also reveal large differences in the effects of school closures by college degree attainment, while HSS do not find such differences for married women specifically. We augment our analysis by college status by examining how our results vary across industry and occupational characteristics: whether the worker is in a "frontline" industry and whether the worker's occupation has high potential for teleworking. Finally, though several of the labor-market outcomes analyzed across the two studies are similar, ours is the only one to measure the effects of school closures on earnings. We discuss possible reasons for the differences in findings between our two papers below.

[2] Our two papers were developed in parallel. First drafts of each paper were posted online in the same week in January 2022 (unbeknownst to either set of authors beforehand).

3. Data

Our sample includes all individuals age 21 years and over surveyed in the Basic Monthly Current Population Survey (CPS) from January 2020 to December 2021 (Flood et al., 2021). To match individuals to school closures in their area with as much precision as possible, we restrict our sample to the subset of individuals who have non-missing county identifiers (more sparsely populated counties are not identified due to concerns about respondent confidentiality). This is about 40 percent of the full CPS sample over this time period. This is in contrast to HSS, who assign school closures based on MSA or state of residence if county is not available. There is obviously a tradeoff between these choices: closures assigned based on MSA/state will likely contain more error than those based on county, but this also allows for a more representative sample of the entire U.S. population in the analysis. The effects we estimate are local to larger metropolitan areas.

We consider various measures of employment in our analysis. First is the extensive margin of labor supply: whether an individual is "at work," defined as doing any work for pay or profit or working at least fifteen hours without pay in a family business or farm in the previous week. This excludes individuals who are employed but currently absent from work since some may respond to school closures by taking leave. We also consider measures of labor supply that incorporate the intensive margin: whether an individual works "full-time," defined as working at least 35 hours in all jobs in the previous week and "hours worked," defined as the total number of hours worked by the individual in the previous week.[3] Lastly, we examine the individual's self-reported usual weekly earnings (excluding the self-employed).

We also analyze several additional labor-market variables that may be specifically related to COVID and/or school closures. These include whether the individual teleworked or worked from home (for pay) due to COVID in the previous four weeks, whether the individual worked part-time or was absent from work in the previous month due to childcare problems or family/personal obligations, and whether the individual was not in the labor force because they were taking care of house or family.[4]

We utilize the school closures database from Parolin and Lee (2021), which tracks visits to K-12 public schools in 94 percent of school districts spanning 98 percent of counties in the country. This database uses aggregated and anonymized mobile phone data from *Safegraph*. The authors track year-over-year changes in the number of visitors to each individual school or childcare facility in each month relative to the same month in 2019 (the pre-pandemic baseline). Institutions are considered "closed" if there is at least a 50 percent year-over-year decline in the number of in-person visits; we use this same cutoff in our main analyses and use a more stringent cutoff (75 percent reduction) in robustness checks. We use the share of closed institutions in each county in each month to be our measure of the extent of school closures in our analysis (thus, our measure is continuous between zero and one). Data on school closures are available for all CPS respondents for whom county of residence is identified over our full sample period.

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[3] Our definition is based on the definition of "usual full-time work" in the CPS published by the Bureau of Labor Statistics : https://www.bls.gov/cps/definitions.htm#fullparttime.

[4] The question about COVID-induced telework first appeared in the CPS in May 2020. For months prior to that, we code the variable as zero for all individuals.

Our use of the Parolin and Lee (2021) differs from HSS, who compute their measures of school foot traffic directly and benchmark them against January and February 2020. Given that the majority of U.S. Schools are closed in June and July, we do not use those months in our analysis.[5] HSS include summer months in their analysis, which means that their definition of school closures includes both COVID-related as well as break/holiday reductions in school foot traffic. Though each type of closure likely affects parental labor-market decisions, it is not clear that those effects are the same given the much more predictable nature of the latter. Our measure of school closures, though imperfect (due to our inability to rule out every other possible reason for large reductions in school visits), should largely be due to COVID.

As a robustness check, we use another dataset to measure public school closures based on administrative rather than phone traffic data. This is the *Burbio K-12 School Opening Tracker* that covers over 1,200 school districts representing 47 percent of U.S. K-12 student enrollment.[6] The *Burbio* data provides the percentage of public schools in each county fitting various modes of instruction: in-person, virtual (100% online), and hybrid (2-3 days per week in-person). As our measure of school closures with this data, we calculate the percentage of schools that were virtual and hybrid within each county in the second week of each month to match the CPS reference week. *Burbio* has complete information on school closures in all counties identified in the CPS. We use *Safegraph* data in our baseline analyses because of the consistency with which it is collected across location and time; the *Burbio* data is collected from various sources such as school district websites, Facebook pages, local news articles and other publicly available sources that could introduce a higher degree of error in measuring school closures.

We also include a set of COVID-19 related variables as controls in our analysis including the number of confirmed COVID-19 cases and deaths at the county level from the database maintained by the Center for Systems Science and Engineering (CSSE) at Johns Hopkins University (CSSE, 2020), and dummy indicators for state-level COVID-19 policies that include stay-at home orders, non-essential business closures, restaurant limitations, and bar closures from the Kaiser Family Foundation database.[7] [8]

4. Methodology

Our baseline model is shown in Equation (1):

$$Y_{ict} = X_{ict}\alpha + \beta_1(schoolagechild_{ict} \times schoolclosure_{ct}) + \omega_t + \theta_c + \varepsilon_{ict} \qquad (1)$$

Y_{it} refers to the employment outcome for individual *i* in county *c* in month *t*. X_{ict} is a vector containing both individual and county characteristics including an indicator for whether the

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[5] Based on our calculations using Safegraph data, foot traffic declines in June and July by about 70% relative to peak school visits in 2019.
[6] https://cai.burbio.com/school-opening-tracker/
[7] https://github.com/CSSEGISandData/COVID-19/tree/master/csse_covid_19_data/csse_covid_19_time_series
[8] https://github.com/KFFData/COVID-19-Data/tree/kff_master/State%20Policy%20Actions/State%20Social%20Distancing%20Actions

individual has at least one child residing in their household and that the oldest or youngest child is between ages 5 and 17 years old (*schoolagechild*)[9]; the percentage of schools that are closed in the individual's county of residence as described in the previous section (*schoolclosure*); individual demographics (age and its square, number of own children residing in the household, dummies for race and Hispanic ethnicity, foreign born, presence of a disability, marital status, and veteran status); individual industry and occupation dummies[10]; county COVID-19-related variables including the cumulative number of confirmed COVID-19 cases and deaths per 100,000 in each county by the second week of the sample survey month (the reference week of CPS), the number of the additional confirmed cases and deaths per 100,000 in the past month, and state-level policy dummy indicators that include stay-at-home orders, non-essential business closures, restaurant limitations, and bar closures. ω_t represents a month fixed effect and θ_c represents a county fixed effect.

We exploit within-county, over-time variation of school closures for identification of the coefficient of interest, β_1, which tells us how school closures affect the employment outcomes of individuals who live with at least one school-age child relative to those who do not. We acknowledge that school closures may be endogenous with respect to COVID and economic conditions in a particular area. Our identifying assumption is that school closures alone should have a disproportionate effect on parents of school-age children relative to others. In this case, β_1 is the effect of school closures on parental labor-market outcomes.

If for some reason school closures were otherwise correlated with *parental* labor market outcomes specifically, that would jeopardize interpretation of our results. Though we cannot rule this out definitively, we can examine whether school closures affect the labor-market outcomes of parents of only very young children who are not yet old enough to attend public school (age(s) less than 5 years old, which we label *youngchild* below). If school closures had no effect on the labor supply of these parents, it would provide more evidence that any measured effect of closures on parents of school-aged children is causal. We examine this possibility in Equation (2):

$$Y_{ict} = X_{ict}\alpha + \beta_1(schoolagechild_{ict} \times schoolclosure_{ct}) + \beta_2(youngchild_{ict} \times schoolclosure_{ct}) + \omega_t + \theta_c + \varepsilon_{ict} \quad (2)$$

In this equation, our hypothesis is that if the outcome variable is hours worked, for example, $\beta_1 < 0$ and $\beta_2 = 0$. A complicating factor is that school closures are correlated with *childcare facility* closures (which generally serve children too young for public school), so a relationship between school closures and employment outcomes for parents of only young children (age(s) less than 5) might simply reflect that omitted variable bias. We do not control for childcare facility closures in our regressions due to multicollinearity concerns.[11]

[9] Because the CPS only asks about the age of oldest and youngest children, there could be (likely rare) cases in which a middle child is school-aged while resident older and younger siblings are not, in which case we would not be able to identify the family as having a school-age child.
[10] These include a category for no occupation given. HSS do not include industry and occupation controls in their analysis, but they do exclude workers in the k-12 education sector in their main analysis.
[11] Our analysis using Safegraph data on both school and childcare facility closures indicated a correlation between the two of 0.82.

Lastly, we include leads and lags of school closures (in the form of 3-6 month average values from before and after the current month) to see 1) if parental labor market outcomes were different even *before* a particular school closure realization, which might suggest that school closures are merely proxying for something else causing relative changes in parental outcomes, and 2) whether closures have effects on outcomes that persist beyond the current period. In this case, the specification becomes:

$$Y_{ict} = X_{ict}\alpha + \beta_1(schoolagechild_{ict} \times schoolclosure_{ct}) + \beta_2(schoolagechild_{ict} \times schoolclosure\_3\_6\_lead_{ct}) + \beta_3(schoolagechild_{ict} \times schoolclosure\_3\_6\_lag_{ct}) + \omega_t + \theta_c + \varepsilon_{ict} \quad (3)$$

In this equation, X_{ict} now also includes the values of a 3-6 month lead and lag of the school closure variable.

Because other papers in the literature have found different pandemic-related effects on labor-market outcomes for men and women, we estimate Equations (1)-(3) separately by gender. Later in the paper, we split the sample in other ways as well. Standard errors are clustered at the county level throughout our analysis.

5. Main Results

The COVID-19 pandemic has caused job losses for both men and women. Figure 1 shows that the percentage of men and women who were "at work" dipped in the second quarter of 2020, the pandemic's initial peak. Meanwhile, by the first quarter of 2021, at-work rates of all women and women with at least one school-age child (5-17 years old) were still (respectively) 3 and 3.6 percentage points lower than just before the pandemic; at-work rates of all men and men with at least one school-age child were 3.2 and 2.9 percentage points lower, respectively. The larger decline observed in the at-work rates of women with school-age children relative to all women—which is not observed for their male counterparts—is consistent with the hypothesis that school closures have affected the labor market status of working mothers more than fathers. However, this may also be due to other factors, such as the pre-pandemic distribution of occupations among these various groups. This leads us to consider the question of how school/childcare closures affect the employment outcomes of women and men in the regression models outlined in Section 4.

Table 1 displays summary statistics for our sample in September 2020 and September 2021. The average share of schools that were closed in a county declined from 56 percent in September 2020 to 22 percent in September 2021. This partial relaxation of school closures is illustrated in Figure 2 (for April 2020, September 2020, April 2021, and September 2021) and is consistent with the ending of states' stay-at-home orders, non-essential business closures, restaurant limitations, and bar closures over that year. Vaccines were not approved for 12-15 year-olds until May 2021 (Lendon et al., 2021), and for 5-11 year-olds until later in the fall of 2021 (American Hospital Association, 2022), which is likely a major reason why a smaller share of schools remained closed during the first part of the fall 2021 semester.

I. Impacts of School Closures on Hours and Earnings

Our baseline results are contained in Tables 2 (for women) and 3 (for men). The first row of coefficients shows how school closures are associated with the labor-market outcomes of individuals without school-age children. It appears that closures are associated with reductions in any work, hours, and earnings for women even if they do not have school-age children. This could be because closures are associated with other COVID-related restrictions that affect the labor-market opportunities of all women or attitudes regarding COVID risk that affect the labor supply of all women. It is interesting to note, however, that the same pattern does not hold for men: the coefficients of school closure on the labor-market outcomes of men without school-age children are generally small, positive, and not statistically different from zero. Generally speaking, any unobserved drivers of labor-market outcomes that are correlated with school closures will not be problematic to our identification strategy unless they have a differential impact on parents of school-age children. We return to this point below when discussing results pertaining to Equations (2) and (3).

Presence of school-age children without school closure is generally associated with more work and higher earnings for men while the results for women are mixed. Our main effects of interest are those associated with the interaction between our school closure measure and the presence of school-age (5-17 year-old) children in the household, which should capture the labor-supply effects of having school-age children at home instead of in school.

The first three columns in Table 2 (3) show the impact of school closures on whether women (men) are at work. Columns 1-3 display the results from Equations (1)-(3), respectively. In Table 2, the specific effects of school closure for women with school-age children are negative and range between 1.5 and 1.8 percentage points (about a 2.7 to 3.2 percent decrease relative to the pre-pandemic (2019) mean). That is, going from all schools in the county being open to all being closed (taking our closures measure from 0 to 1) would mean that the probability of being at work would fall by about 1.5 percentage points when women have school-age children. For men, the effects are smaller and statistically insignificant.

Regarding the probability of full-time work (Columns 4-6), coefficients on interaction terms between school closure and the presence of school-age children hover right around (negative) 2 percentage points across specifications for women. In this case, we see an even larger reduction for men of about 3.5 percentage points (a 6 percent decrease relative to the pre-pandemic mean). Columns 7-9 display effects on total hours worked, which are about 0.8 hours per week for women (-3.9 percent) and 1 to 1.4 hours per week for men (-3.5 to -4.9 percent). All effects are statistically significant at the 1% level.

The last three columns (10-12) of Tables 2 and 3 show how school closures affect (log) weekly earnings for women and men, respectively.[12] In this case, our sample sizes are greatly reduced because CPS only questions individuals on their earnings in the past week if they are in an outgoing rotation group (that is, if they are in the 4th or 8th month of the survey). Nevertheless, we see reductions in earnings of about 12-15 percent across gender, though these effects are

[12] For those with zero earnings, we assign a value of $1. Alternatively, we transform the data using the inverse hyperbolic sine and find very similar results (available upon request).

somewhat imprecisely estimated (they are significant at the 10% level across specification for women but not for men). Of course, such reductions in earnings could be due to a reduction in the likelihood of work or reduced hours, but they could also be due to other changes in parents' work schedules induced by school closures, such as work-from-home arrangements, which we detail below.

The second column of each triplet described above shows results from estimating Equation (2), which includes an indicator for families with only children younger than 5 years old and its interaction with our school closure variable. When these are added to the regression, effects on our primary interaction of interest (presence of school-age children times school closure) barely change. Furthermore, presence of only young children itself is correlated with less work for mothers but, if anything, more work for fathers. This is expected given similar results in the literature (e.g. Berniell (2021) ; Kleven et al. (2019); Sieppi and Pehkonen (2019)). Lastly, for women, none of the interactions between school closure and presence of only young children are statistically significant. In the case of men, however, 2 of the 4 interactions are significant in a way that mimics the effect of school closures for men with school-age children. Thus, using families with only children too young for public school as a placebo group yields somewhat mixed evidence, though we again note that the significant effects we see (for men) may be due to the fact that childcare facility closures were more likely to occur in areas with closed schools. We do not control for childcare facility closures in our regressions because they are highly correlated with school closures, introducing multicollinearity.

In the third column of each triplet in Tables 2 and 3, we add 3-6 month leads and lags of the school closure variable to examine 1) whether labor-market outcomes begin changing even before a particular realization of the school closure variable, and 2) whether school closure effects persist beyond the current period. We do not find evidence in favor of either of these. Interactions between the lead of closure and presence of school-age children is not statistically significant for any variable for either men or women, and most effects are small in magnitude. This lends confidence to the notion that parental labor-market outcomes are in fact caused by school closures and not due to another co-occurring trend in areas that had a greater degree of closure.

We also find that the lag of the school closure does not differentially affect the outcomes of parents with school-age children. This is noteworthy given that decisions to quit a job or leave the labor force temporarily (which appears to happen to some degree for mothers) could have long-lasting effects. Nevertheless, in this specification, we do not find evidence that this is a significant issue. Of course, women who leave and re-enter the labor force may do so at different jobs, something we leave to future work given the structure of our data.[13]

On a related note, we can also examine whether school closure effects vary over phase of the pandemic. Perhaps, for example, parents became more adept at adapting their work lives to school closures over time. On the other hand, temporary solutions to school closures (such as taking leave from a job) may not have been available to employees as the pandemic wore on. In

[13] The CPS has a short panel component, where individuals are interviewed for 4 months, out of the survey for 8 months, and then interviewed another 4 months. However, this is likely insufficient to tackle the question of changes in occupation at the individual level.

Appendix Table 1, we divide our sample months into four groups: spring 2020 (January-May 2020, the base group), fall 2020 (August-December 2020), spring 2021 (January-May 2021), and fall 20201 (August-December 2021).[14] We then modify Equation (1) to also include triple interactions between these "semesters" and the product of our school closure measure and an indicator for the presence of school-age children as discussed earlier.

Looking broadly across outcomes, we see little evidence that the effects of school closures changed markedly for parents of school-age children over the course of the pandemic. The main exception is remote work due to COVID, which clearly rose starting in fall 2020 (relative to spring 2020) for both men and women. We note that the power to detect statistically significant differences by semester of the pandemic is limited here, since only variation within each semester in school closures can identify separate effects.

II. Impacts of School Closures on Other Margins of Work

In Table 4, we examine a set of variables that should shed more light on how school closures affect parents. Columns 1 (women) and 4 (men) show how our school closure measure affects whether individuals report working from home due to COVID in the previous month. The first row indicates that in areas with a greater degree of school closure, individuals without school-age children are more likely to work from home. This is likely because school closures are correlated with other COVID restrictions and cultural factors that encouraged a greater degree of COVID risk aversion. However, individuals with school-age children were significantly more likely to report working from home with closed schools than those without school-age children, as indicated by the interaction term effects (row 3). This is in spite of the fact that when schools are completely open, parents of school-age children are somewhat *less* likely to work from home (row 2).

Columns 2 (women) and 5 (men) show how the probability of being absent from work or working part-time specifically because of childcare problems or family/personal obligations is affected by school closures. Curiously, school closures are associated with a *reduction* in this likelihood for mothers with school-age children. We are not sure of the reasons for this phenomenon, but it is interesting to note that the next column shows effects pertaining to the likelihood of not being in the labor force specifically to care for home/family. For women, the effect of school closure on this variable is positive and very similar in magnitude to the absent/part-time reduction. Thus, it may be the case that school closures caused women who were underemployed (absent or part-time) due to childcare responsibilities to exit the labor force entirely when schools closed during the pandemic. In other words, there may have been a shift from the former category to the latter. For men with school-age children, the effects of school closure on these outcomes are small and statistically insignificant.

III. Heterogeneity in Impacts by Education, Marital Status, Race, Occupation, and Industry

We now turn our attention to analyzing how our results differ for various groups. In doing so, we restrict our analysis to estimation of Equation (1) given the robustness of our main results of

[14] Recall that we do not use June and July in our analysis as most U.S. schools are on summer break during those months.

interest across specification in Tables 2 and 3. We present only interaction effects between our school closure measure and presence of school-age child(ren) on a limited number of outcomes. Additional results from these specifications are available in the Online Appendix.[15] Table 5 contains results for women, while Table 6 does the same for men.

A. By Education

First, we examine how parental education mediates our results. Other studies have found a strong relationship between education and labor-market outcomes during the COVID pandemic owing to such differences as the ability to perform work responsibilities from home (e.g. Mongey, Pilossoph, and Weinberg, 2020), propensity to be in occupations designated as "essential," and industry-specific shocks associated with the pandemic and the public health response (e.g. Montenovo et al., 2020). These factors could certainly play a role in how parents respond to school closures specifically. On the one hand, a flexible, at-home work arrangement may allow better educated individuals to maintain their work hours even with children at home since they can adjust their work hours throughout the day (implying that the response in hours worked would be larger for low-educated parents; see Lofton, Petrosky-Nadeau, and Seitelman, 2021). On the other hand, if there is little flexibility in schedule or location in the work arrangements of less-educated individuals, their supply response to school closures might be smaller than that of the high education group.

It has also been shown that shifting children into private schooling during COVID rises with family income (e.g. Musaddiq et al., 2021), which is of course correlated with parental educational attainment. This would imply seeing a more muted response in labor supply to school closures among college graduates, since they would be better able to afford to send kids to private schools during public school closures.

Panel A of Tables 5 and 6 show how school closures affect the employment outcomes of parents with school-age children by college degree status. Considering differences for women first, mothers without a college degree experience a much larger reduction in work and earnings than do mothers with a degree. Women with school-age children but without a degree see a reduction in the probability of being at work of 2.3 percentage points (-3.6 percent), a reduction in the probability of full-time work of 4.0 percentage points (-8.9 percent), and a reduction in overall work hours of almost 1.5 hours per week (-6.5 percent) when schools are closed. On the other hand, effects for mothers with a degree are very small and not statistically different from zero. Women without a degree see a 16 percent reduction in earnings due to school closures (significant at the 5% level). When it comes to remote work, this pattern flips: it is now college-educated women who see a much larger effect (8.1 vs. 1.7 percentage points). This is expected given the profile of jobs by education: having a college degree improves the likelihood of remote work (Brussevich et al., 2022).

Differences by college degree status for men largely mirror those for women (see Table 6, Panel A). In fact, for those with school-age children and no degree, effects on work and hours for men are as large or larger than they are for women, even in the case of the likelihood of being at work

[15] See Online Appendix Table here: https://doi.org/10.3886/E182101V1.

at all (for example, the reduction in hours worked is almost 2.3 hours per week (-6.3 percent)). However, effects on earnings are similar (and not statistically different from zero) across college degree category for men, which was not the case for women. Again, part of the imprecision in the earnings results may be due to the greatly reduced sample size.

Overall, these results are consistent with the notion that individuals with less formal schooling had a more difficult time adjusting their work life to school closures either because of a less flexible schedule, less substitution to private schools, or other factors. Whatever the reason, this is evidence that school closures have disproportionately affected workers with relatively low education levels.

B. By Marital Status

The next dimension we analyze is marital status: married parents with a present spouse may respond differently to school closures by dividing responsibilities differently between labor-market and household production relative to single or cohabitating individuals. It is possible that, for example, married couples are better able to share the time burden of additional childcare when schools close. Conversely, married couples may specialize more across paid work/home production relative to cohabitating couples when schools close.[16] Single mothers are also much more common than single fathers: 21 percent of children under 18 live only with their mother versus four percent who live only with their fathers (Alon et al., 2020). Thus, the burden of school closures on unmarried women may be especially large.

Panel B of Table 5 displays the results for the same set of dependent variables for married women (with spouse present) and unmarried women (or married but absent spouse) separately. We find that both married and unmarried women with school-age children experience reductions in hours and earnings with school closure, though overall effects are somewhat more pronounced among unmarried women (other than with regard to remote work, likely owing to the different occupational profile of these two groups). For men, effects on any work and hours across marital status are even more similar than they are for women, though the effect on earnings is somewhat larger for married men. Once again, it is only married (men) who experience an increase in the likelihood of working from home when schools close.

Overall, we find that married women with school-age children do experience reductions in work and hours with school closures, but their experience is not unique in our sample, which covers people living in larger counties over the full course of the pandemic. Selection into marriage in our more urban/suburban sample may be different than in the general population: for example, 52% of our sample are married with spouse present, but this number is 39% in the full CPS sample.

C. By Race

[16] See Albanesi and Kim (2021b) and Shore (2010) for evidence on risk-sharing in the labor-market behavior of married couples.

Panel C of Tables 5 and 6 contain results for whites and non-whites (People of Color) separately (all individuals who do not report their race as "white" are combined due to small sample sizes among each individual group). In the case of women (Table 5), effects on any work, hours, and earnings are only significant for whites (though effects for People of Color are also negative). Both groups experience a significant increase in the likelihood of working from home due to COVID. For men (Table 6), effects across race are quite similar, with both groups seeing a significant reduction in hours due to school closure. Smaller sample sizes among People of Color make it more difficult to rule out either zero effects or effects that are as large as those for whites.

One potential reason why People of Color may experience smaller effects of school closure on work outcomes than whites is their distribution across industry and occupation relative to whites. In fact, People of Color are much more likely to work in "frontline" industries, which tend to have minimal potential for working from home and which were often designated as "essential" in the early days of the pandemic. Among women, for example, roughly 20% of People of Color work in such industries, while only 15% of white women do (among men, the figures are 13% and 9%, respectively). This leads us to consider how occupation and industry classification mediate our results more generally.

D. By Occupation and Industry Classification

We now turn to investigating how workers' ability to do their job from home change how school closures affect their outcomes. To do so, we rely on an index of occupations: whether there is high potential for telework or not; and an index of industries: whether the industry is "frontline" or not. To designate frontline industries, we follow Rho et al. (2020), who use the same definition as the New York City Comptroller. The six groups in which a particular industry is classified as "frontline" are: (1) Grocery, Convenience, and Drug stores, (2) Public transit, (3) Trucking, Warehouse, and Postal Service, (4) Building Cleaning Service, (5) Health Care, and (6) Childcare and Social Services. 13% of individuals fall into one of these categories. In terms of potential for telework, we follow Dingel and Neiman (2020), who use O*NET occupational surveys to isolate job characteristics that lend themselves to telework. Their index ranges from zero to one; we define an occupation with high telework potential as having a value equal to one (about 25% of the sample); all other occupations are coded as zero. Each of these indices is independently meaningful; the correlation between the two is about -0.03.

Our hypothesis is that workers in frontline industries would generally not have the ability to adjust their work schedules (at least on the intensive margin) to school closures relative to those outside of these industries. With regard to teleworking, workers in highly telework-compatible occupations likely have more scope for adjusting their work schedules to school closures relative to others given their ability to work from home. However, it may also be the case that such workers experience *larger* reductions in hours when schools close since other workers may not be able to change when or how much they work unless they find a new job. Thus, it is an empirical question how telework potential interacts with closures in affecting work outcomes.

It is clear from the bottom two panels of Tables 5 and 6 that the negative effects of school closures on work/hours for parents with school-age children are concentrated in 1) occupations in which potential for telework is low, and 2) frontline industries. Effects on earnings for highly telework-compatible occupations are as large or larger as they are for low-telework occupations, however, suggesting that reductions in earnings overall (discussed earlier) may be due in part to the large increases in working from home among the former group. It is difficult to make firm conclusions, however, since none of the individual effects on earnings are statistically different from zero.

6. Sensitivity Analysis

Our analyses thus far have relied on measuring a school closure as a 50 percent year-over-year reduction in phone traffic at that institution using Safegraph data, as recommended in Parolin and Lee (2021). There are two potential issues with this. The first is that the 50 percent cutoff is arbitrary and schools may erroneously appear to be closed by this measure if significant numbers of parents *chose* to remove their children from a particular public school at some point during the pandemic. The second issue is the extent to which such removals are *endogenous* because parents who reduced their time at work also chose to remove their children from school. We view these possibilities as unlikely given that a 50 percent reduction in visits at a school would require an enormous response from many parents simultaneously. Nevertheless, we think it is worthwhile to explore how sensitive our results are to other measures of school closures. We first use the same Safegraph data but with a more stringent 75% cutoff; next, we employ an entirely different dataset that documents school closures from administrative sources as collected by *Burbio*.

Appendix Table 2 contains the results using the alternative 75% cutoff for school closure. These are largely consistent with our baseline results, with effects that are generally somewhat larger than they are in our baseline specifications. This suggests, intuitively, that more stringent closures, in the form of fewer in-person days (for schools that are remote or using a hybrid format), have even stronger effects on labor-market outcomes.

Tables 7 (women) and 8 (men) displays results using *Burbio* rather than *Safegraph* closures. Here the results for the full sample are smaller than they are in our baseline analysis, and statistical significance largely disappears (except with regard to remote work). One possible reason we see a universal reduction in the coefficients with the *Burbio* data is that it measures school closures with a greater degree of error—indeed, the correlation between our primary (Safegraph) school closures measure and the *Burbio* measure is only 0.71. Another is that the *Burbio* data only begin in Fall 2020, so we cannot use the early pandemic months (or the non-pandemic months before that) in this analysis. However, when we restrict the sample to those without a college degree, we observe stronger effects on full-time work and hours that are significant at the 5% level, consistent with the pattern we saw in our *Safegraph* results.

7. Conclusion

We find that school closures over the course of the 2020-21 academic year had a significant effect on the labor-market outcomes of parents with school-age children. Though only women see a reduction in the probability of being "at work" overall, we find that both women and men are less likely to work full-time, and both reduce their hours worked per week in response to these closures. The effects we find are concentrated among individuals without a college degree, likely exacerbating the toll the pandemic has had on lower-income families in the form of student test scores (Goldhaber et al., 2022) and leading to increases in other forms of inequality (e.g., Adams-Prassl et al., 2020; Bonacini, Gallo and Scicchitano, 2020; Andrasfay and Goldman, 2021; Alsan, Chandra and Simon, 2021). This is in line with our finding that individuals in jobs with high telework potential did not experience reductions in work hours when schools were closed, since those with a college degree are much more likely to hold such jobs.[17] Though our results with regard to earnings are less precisely estimated (likely due to smaller sample sizes in CPS), we do find economically meaningful reductions in earnings that are unique to parents of school-age children as school closures rise.

These findings contribute to our understanding of several aspects of how COVID-19 and its fallout have disrupted the lives of working parents. First, the literature on how school and childcare closures has affected economy-wide changes in labor supply have focused on the extensive margin, that is, whether individuals work or are in the labor force (e.g. Albanesi and Kim, 2021a; Furman, Kearney and Powell, 2021). These papers suggest that closures had at most a modest impact on these measures. Our results imply that the intensive margin of labor supply has been affected by school closures; for example, men with school-age children do not see a reduction in the likelihood of being at work but do reduce their hours when school close. Thus, the intensive margin of work appears to be an important dimension for considering how the pandemic has affected labor supply.

Another of our findings that adds to the existing literature on the labor-market effects of COVID is that over our full sample period, reductions in work hours in response to school closures are generally similar for women and men. This is in contrast to evidence from the very early part of the pandemic that the effects were very different across gender (e.g., Heggeness, 2020). With little anticipation of school closures in spring 2020, it appears that women were more likely to take time out of work to care for children who had to stay home. This is consistent with evidence that other kinds of family shocks affect women's labor supply more than men's (e.g., Van Houtven, Coe, and Skira, 2013; Jeon and Pohl, 2017; Saad-Lessler, 2020). With more time to adjust schedules and anticipate closures starting in fall 2020, we find that the additional childcare burden brought on by school closures was more balanced across gender.

During the pandemic, policymakers did not have the luxury of many credible estimates of the benefits and costs of closing schools. Several recent papers suggest that the health and human capital of children are harmed by school closures and that these effects are largest for disadvantaged kids (e.g., Kuhfeld et al., 2020; Engzell, Frey and Verhagen, 2021; Larsen, Helland and Holt, 2021; Halloran et al., 2021; Fuchs-Schündeln et al., 2021). Our results suggest that closures carry costs to families in the form of reduced parental work hours, particularly among less-educated mothers and fathers. These factors should be taken into account as policymakers continue to grapple with reducing disease during future pandemics in ways that are least costly to their constituents.

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[17] 42% of individuals with at least college degree are in occupations with telework potential, as opposed to only 13% of individuals without college degree.

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Figures and Tables

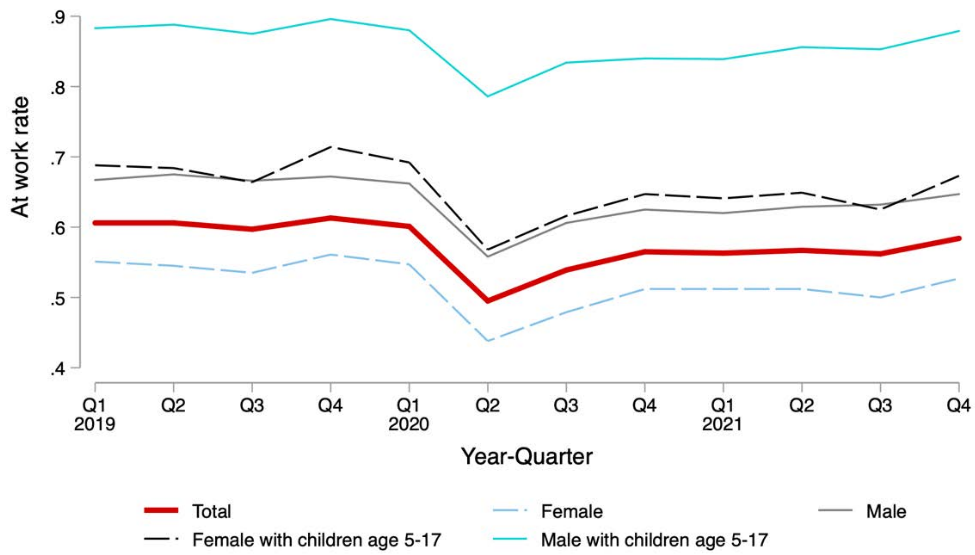

Figure 1. Percentage of individuals “at work,” 2019-Q1 – 2021-Q4

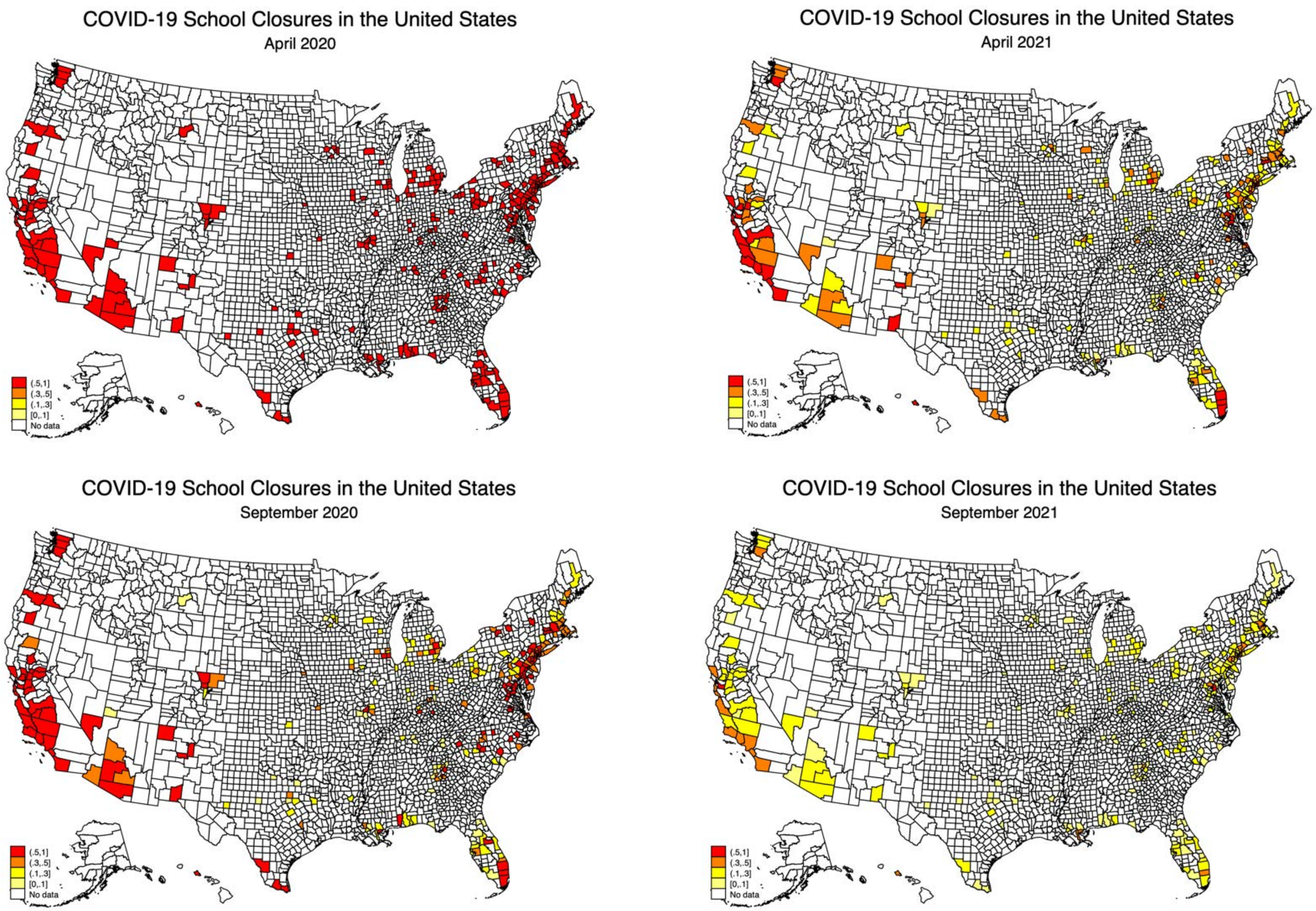

Figure 2. Percentage of school closures in CPS sample with county identifiers according to Parolin and Lee (2021) database, April 2020 & 2021, and September 2020 & 2021

Table 1. Summary Statistics by survey month

VARIABLES	(1) September 2020	(2) September 2021
In labor force	0.64	0.64
At work	0.57	0.59
Absent from work	0.02	0.02
Unemployed	0.05	0.03
Work hours last week	21.30	23.27
Worked Remotely due to COVID	0.16	0.10
Worked Part-time due to childcare/family reasons	0.02	0.02
Not in the labor force due to family reasons	0.06	0.06
Earnings last week (in January 2020 USD)	602.35	648.10
Female (dummy)	0.52	0.52
Age	49.21	49.29
Number of children in household	0.69	0.69
Presence of young children (age <5) only	0.05	0.04
Presence of school-age children (ages 5-17)	0.21	0.21
White race	0.75	0.75
Black race	0.13	0.13
Asian race	0.09	0.09
Other race	0.03	0.03
Married	0.52	0.51
Veteran	0.06	0.06
U.S. born	0.76	0.75
Hispanic ethnicity	0.21	0.20
Presence of disability	0.11	0.11
Less than high school diploma	0.09	0.09
High school diploma	0.27	0.26
Some college	0.25	0.25
College degree	0.25	0.25
Advanced degree	0.15	0.15
Percentage of school facilities closed in county	0.56	0.22
New deaths by 2nd week of the month per 100,000	7.58	7.01
Cumulative deaths by 2nd week of the month per 100,000	78.44	206.42
New cases by 2nd week of the month per 100,000	310.60	1,192.49
Cumulative cases by 2nd week of the month per 100,000	2,118.48	12,325.99
Stay-at-home order	0.37	0.00
Non-essential business closure	0.99	0.00
Restaurant limit	0.89	0.00
Bar Closure	0.76	0.00
Observations	33,668	32,085

(1) All numbers displayed are means weighted with final basic CPS person weights.
(2) The values for new deaths and new cases by the second week of the month is the month-over-month difference with the 14th day of each month as the reference date.
(3) The values for new deaths and new cases by the second week of the month is the month-over-month difference with the 14th day of each month as the reference date.

Table 2. OLS Regressions on “At Work”, “Full-time”, “Hours Worked” and “Log of Weekly Earnings”, Female

VARIABLES	(1) At work	(2) At work	(3) At work	(4) Full-time	(5) Full-time	(6) Full-time	(7) Hours worked	(8) Hours worked	(9) Hours worked	(10) Log of Real Weekly Earnings	(11) Log of Real Weekly Earnings	(12) Log of Real Weekly Earnings
School closure	-0.016***	-0.015***	-0.015***	-0.010	-0.010	-0.009	-0.819***	-0.817***	-0.745***	-0.143**	-0.133*	-0.140**
	(0.005)	(0.005)	(0.005)	(0.008)	(0.008)	(0.008)	(0.265)	(0.261)	(0.264)	(0.068)	(0.068)	(0.070)
Presence of school-age children (5-17)	0.015***	0.003	0.011**	0.003	-0.013***	0.005	0.421***	-0.314**	0.425*	0.047	0.017	0.022
	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.007)	(0.146)	(0.146)	(0.237)	(0.035)	(0.036)	(0.051)
School closure x presence of school-age children	-0.015***	-0.015***	-0.018***	-0.021***	-0.020***	-0.019***	-0.834***	-0.803***	-0.829***	-0.122*	-0.131*	-0.147*
	(0.005)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.250)	(0.258)	(0.269)	(0.071)	(0.073)	(0.080)
Presence of young children only (0-4)		-0.041***			-0.061***			-2.746***			-0.062	
		(0.005)			(0.008)			(0.245)			(0.066)	
School closure x presence of young children only		-0.011			-0.003			0.031			-0.158	
		(0.012)			(0.014)			(0.533)			(0.137)	
Lag closure (past 3-6 months average)			0.001			-0.006			-0.305			0.023
			(0.007)			(0.011)			(0.412)			(0.095)
Lead closure (next 3-6 months average)			-0.011			-0.022			-0.715			-0.170
			(0.011)			(0.014)			(0.528)			(0.120)
Lag closure x presence of school-age children			0.009			0.004			0.140			0.005
			(0.006)			(0.009)			(0.318)			(0.073)
Lead closure x presence of school-age children			0.005			-0.011			-0.145			0.086
			(0.007)			(0.012)			(0.364)			(0.086)
N	348,278	348,278	348,278	348,278	348,278	292,865	348,278	348,278	348,278	90,461	90,461	90,461
R-squared	0.768	0.769	0.768	0.502	0.503	0.502	0.667	0.667	0.667	0.695	0.695	0.695

*** p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses. School closures refer to the share of all schools in each county that had at least 50 percent year-on-year decline in in-person visits.

Table 3. OLS Regressions on "At Work", "Full-time", "Hours Worked" and "Log of Real Weekly Earnings", Male

VARIABLES	(1) At work	(2) At work	(3) At work	(4) Full-time	(5) Full-time	(6) Full-time	(7) Hours worked	(8) Hours worked	(9) Hours worked	(10) Log of Real Weekly Earnings	(11) Log of Real Weekly Earnings	(12) Log of Real Weekly Earnings
School closure	0.002	0.003	0.001	0.006	0.009	0.005	0.015	0.098	-0.091	0.091	0.090	0.089
	(0.006)	(0.006)	(0.006)	(0.009)	(0.009)	(0.009)	(0.303)	(0.307)	(0.312)	(0.086)	(0.088)	(0.094)
Presence of school-age children (5-17)	0.007**	0.007*	0.008	0.024***	0.027***	0.024***	0.978***	1.087***	1.245***	0.117**	0.164***	0.077
	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.007)	(0.191)	(0.218)	(0.284)	(0.051)	(0.058)	(0.068)
School closure x presence of school-age children	-0.007	-0.008	-0.007	-0.034***	-0.037***	-0.034***	-1.290***	-1.374***	-1.042***	-0.118	-0.117	-0.156
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.268)	(0.269)	(0.326)	(0.080)	(0.081)	(0.101)
Presence of young children only (0-4)		0.004			0.028***			0.875***			0.160**	
		(0.005)			(0.008)			(0.303)			(0.081)	
School closure x presence of young children only		-0.008			-0.051***			-1.495***			0.023	
		(0.010)			(0.013)			(0.575)			(0.154)	
Lag closure (past 3-6 months average)			0.008			0.007			0.412			0.044
			(0.009)			(0.012)			(0.479)			(0.115)
Lead closure (next 3-6 months average)			-0.019*			-0.024			-0.602			-0.137
			(0.012)			(0.017)			(0.707)			(0.146)
Lag closure x presence of school-age children			-0.002			0.011			-0.438			0.049
			(0.006)			(0.009)			(0.374)			(0.099)
Lead closure x presence of school-age children			0.000			-0.011			-0.551			0.101
			(0.007)			(0.010)			(0.438)			(0.110)
N	312,703	312,703	312,703	312,703	312,703	312,703	312,703	312,703	312,703	80,932	80,932	80,932
R-squared	0.727	0.727	0.728	0.517	0.517	0.517	0.622	0.622	0.622	0.580	0.580	0.580

*** p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses. School closures refer to the share of all schools in each county that had at least 50 percent year-on-year decline in in-person visits.

Table 4. OLS Regressions on "Remote Work due to COVID", "Part-time or Absent due to childcare or family", and "Not in labor force due to family"

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Remote Work due to COVID	Part-time/ Absent due to childcare/ family	Not in labor force due to childcare/ family	Remote Work due to COVID	Part-time/ Absent due to childcare/ family	Not in labor force due to childcare/ family
	Female			Male		
School closure	0.070***	-0.003	-0.005	0.070***	-0.004**	-0.002
	(0.013)	(0.004)	(0.005)	(0.014)	(0.002)	(0.003)
Presence of school-age children (5-17)	-0.012***	0.026***	0.012***	-0.017***	0.003**	0.004**
	(0.003)	(0.003)	(0.004)	(0.004)	(0.001)	(0.002)
School closure x presence of school-age children	0.047***	-0.018***	0.017***	0.059***	-0.003	0.005
	(0.006)	(0.005)	(0.006)	(0.008)	(0.002)	(0.003)
R-squared	0.304	0.076	0.396	0.317	0.018	0.085

*** p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses. N=348,278 for females and N=312,703 for males. Columns 1-3 are for females, and columns 4-6 are for males. School closures refer to the share of all schools in each county that had at least 50 percent year-on-year decline in in-person visits.

Table 5. OLS Regressions on "At Work", "Full-time", "Hours Worked", "Log of Real Weekly Earnings", "Remote Work due to COVID", Female

VARIABLES	(1) At Work	(2) Full-time	(3) Work Hours	(4) Log of Real Weekly Earnings	(5) Remote Work due to COVID	(6) At Work	(7) Full-time	(8) Work Hours	(9) Log of Real Weekly Earnings	(10) Remote Work due to COVID
Panel A	College					Less than College				
School closure x presence of school-age children	-0.006 (0.008)	0.002 (0.013)	-0.034 (0.435)	-0.049 (0.110)	0.081*** (0.009)	-0.024*** (0.007)	-0.040*** (0.008)	-1.459*** (0.285)	-0.162** (0.079)	0.017*** (0.005)
N	143,990	143,990	143,990	37,431	143,990	204,288	204,288	204,288	53,030	204,288
Panel B	Married					Not Married				
School closure x presence of school-age children	-0.015*** (0.005)	-0.023*** (0.009)	-0.789*** (0.285)	-0.150** (0.072)	0.058*** (0.008)	-0.029*** (0.011)	-0.038*** (0.011)	-1.647*** (0.414)	-0.201* (0.113)	0.009 (0.009)
N	175,245	175,245	175,245	45,229	175,245	173,033	173,033	173,033	45,232	173,033
Panel C	White					Non-white				
School closure x presence of school-age children	-0.014** (0.006)	-0.027*** (0.008)	-0.962*** (0.294)	-0.127* (0.072)	0.045*** (0.008)	-0.010 (0.011)	-0.007 (0.014)	-0.303 (0.510)	-0.064 (0.139)	0.057*** (0.010)
N	263,009	263,009	263,009	68,187	263,009	85,269	85,269	85,269	22,274	85,269
Panel D	Teleworkability = 1					Teleworkability < 1				
School closure x presence of school-age children	0.002 (0.009)	-0.004 (0.014)	0.093 (0.451)	-0.125 (0.121)	0.045*** (0.014)	-0.021*** (0.007)	-0.026*** (0.008)	-1.108*** (0.299)	-0.096 (0.087)	0.016*** (0.004)
N	91,363	91,363	91,363	24,515	91,363	256,915	256,915	256,915	65,946	256,915
Panel E	Frontline Industry					Non-frontline Industry				
School closure x presence of school-age children	-0.009 (0.015)	-0.014 (0.018)	-0.337 (0.609)	-0.133 (0.149)	0.016 (0.012)	-0.015** (0.007)	-0.019** (0.008)	-0.820*** (0.309)	-0.123 (0.076)	0.056*** (0.008)
N	54,139	54,139	54,139	14,500	54,139	294,139	294,139	294,139	75,961	294,139

(1) *** p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses. School closures refer to the share of all schools in each county that had at least 50 percent year-on-year decline in in-person visits.
(2) Teleworkability values are based on Dingel and Neiman (2020): https://github.com/jdingel/DingelNeiman-workathome/blob/master/onet_to_BLS_crosswalk/output/onet_teleworkable_blscodes.csv.
(3) Frontline industry classification is based on the classification from Rho et al. (2020): https://cepr.net/a-basic-demographic-profile-of-workers-in-frontline-industries/.

Table 6. OLS Regressions on "At Work", "Full-time", "Hours Worked", "Log of Real Weekly Earnings", "Remote Work due to COVID", Male

VARIABLES	(1) At Work	(2) Full-time	(3) Work Hours	(4) Log of Real Weekly Earnings	(5) Remote Work due to COVID	(6) At Work	(7) Full-time	(8) Work Hours	(9) Log of Real Weekly Earnings	(10) Remote Work due to COVID
Panel A	College					Less than College				
School closure x presence of school-age children	0.011 (0.007)	-0.016 (0.010)	-0.101 (0.383)	-0.148 (0.112)	0.109*** (0.012)	-0.024*** (0.009)	-0.050*** (0.010)	-2.269*** (0.387)	-0.122 (0.132)	0.016*** (0.005)
N	125,587	125,587	125,587	32,467	125,587	187,116	187,116	187,116	48,465	187,116
Panel B	Married					Not Married				
School closure x presence of school-age children	-0.018*** (0.006)	-0.042*** (0.007)	-1.749*** (0.294)	-0.253*** (0.086)	0.063*** (0.009)	-0.030* (0.016)	-0.049*** (0.018)	-1.879** (0.787)	-0.154 (0.222)	-0.029** (0.011)
N	173,312	173,312	173,312	44,720	173,312	139,391	139,391	139,391	36,212	139,391
Panel C	White					Non-white				
School closure x presence of school-age children	-0.006 (0.006)	-0.036*** (0.007)	-1.099*** (0.305)	-0.104 (0.092)	0.056*** (0.008)	-0.007 (0.011)	-0.025 (0.016)	-1.482** (0.607)	-0.148 (0.133)	0.067*** (0.014)
N	242,823	242,823	242,823	62,678	242,823	69,880	69,880	69,880	18,254	69,880
Panel D	Teleworkability = 1					Teleworkability < 1				
School closure x presence of school-age children	0.025*** (0.009)	0.018 (0.014)	0.730 (0.536)	-0.149 (0.149)	0.038*** (0.014)	-0.024*** (0.007)	-0.058*** (0.008)	-2.256*** (0.309)	-0.121 (0.113)	0.032*** (0.006)
N	73,526	73,526	73,526	19,457	73,526	239,177	239,177	239,177	61,475	239,177
Panel E	Frontline Industry					Non-frontline Industry				
School closure x presence of school-age children	0.027* (0.014)	-0.024 (0.022)	0.253 (0.839)	-0.084 (0.201)	0.018 (0.014)	-0.011* (0.006)	-0.034*** (0.007)	-1.452*** (0.289)	-0.125 (0.085)	0.066*** (0.009)
N	29,902	29,902	29,902	8,122	29,902	282,801	282,801	282,801	72,810	282,801

(1) *** p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses. School closures refer to the share of all schools in each county that had at least 50 percent year-on-year decline in in-person visits.
(2) Teleworkability values are based on Dingel and Neiman (2020): https://github.com/jdingel/DingelNeiman-workathome/blob/master/onet_to_BLS_crosswalk/output/onet_teleworkable_blscodes.csv.
(3) Frontline industry classification is based on the classification from Rho et al. (2020): https://cepr.net/a-basic-demographic-profile-of-workers-in-frontline-industries/ .

Table 7. OLS Regressions on "At Work", "Full-time", "Hours Worked" and "Log of Real Weekly Earnings", "Remote Work due to COVID", "Part-time or Absent due to childcare or family", and "Not in labor force due to childcare or family", using *Burbio* data, Female

VARIABLES	(1) At Work	(2) Full-time	(3) Work Hours	(4) Log of Real Weekly Earnings	(5) Remote Work due to COVID	(6) Part-time/ Absent due to childcare/ family	(7) Not in labor force due to Family reasons
Panel A				All			
School closure	-0.002	0.005	0.213	-0.089**	0.006	-0.002	-0.000
	(0.003)	(0.005)	(0.188)	(0.040)	(0.004)	(0.003)	(0.004)
Presence of school-age children (5-17)	0.009***	-0.001	0.123	-0.008	-0.008*	0.020***	-0.008
	(0.003)	(0.005)	(0.163)	(0.035)	(0.004)	(0.004)	(0.035)
School closure x presence of school-age children	-0.004	-0.014**	-0.306	-0.032	0.038***	-0.000	0.001
	(0.004)	(0.006)	(0.199)	(0.045)	(0.005)	(0.004)	(0.006)
N	260,380	260,380	260,380	67,048	260,380	260,380	260,380
R-squared	0.778	0.504	0.673	0.703	0.337	0.078	0.399
Panel B				Less than college			
School closure	0.001	0.011*	0.342	-0.071	0.004	-0.003	-0.003
	(0.004)	(0.007)	(0.231)	(0.046)	(0.003)	(0.003)	(0.005)
Presence of school-age children (5-17)	0.002	-0.003	-0.012	0.022	-0.001	0.024***	0.034***
	(0.004)	(0.006)	(0.229)	(0.042)	(0.004)	(0.004)	(0.006)
School closure x presence of school-age children	-0.005	-0.026***	-0.584**	-0.032	0.014**	-0.003	-0.003
	(0.006)	(0.007)	(0.252)	(0.056)	(0.005)	(0.005)	(0.007)
N	152,223	152,223	152,223	39,152	152,223	152,223	152,223
R-squared	0.775	0.497	0.678	0.704	0.237	0.086	0.397

*** p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses. School closures refer to the share of all schools that were reportedly virtual or hybrid (not in-person) in each county. Panel A are the results for all females, and Panel B are for females with less than college degree.

Table 8. OLS Regressions on "At Work", "Full-time", "Hours Worked" and "Log of Real Weekly Earnings", "Remote Work due to COVID", "Part-time or Absent due to childcare or family", and "Not in labor force due to childcare or family", using *Burbio* data, Male

VARIABLES	(1) At Work	(2) Full-time	(3) Work Hours	(4) Log of Real Weekly Earnings	(5) Remote Work due to COVID	(6) Part-time/ Absent due to childcare/ family	(7) Not in labor force due to Family reasons
Panel A				All			
School closure	-0.003	-0.004	-0.348*	0.047	0.002	0.001	0.001
	(0.004)	(0.006)	(0.209)	(0.053)	(0.004)	(0.001)	(0.002)
Presence of school-age children (5-17)	0.003	0.018***	0.566***	0.060	-0.011**	0.002	0.002
	(0.003)	(0.005)	(0.205)	(0.048)	(0.005)	(0.002)	(0.002)
School closure x presence of school-age children	0.002	-0.012**	-0.093	0.090	0.046***	-0.000	0.009***
	(0.004)	(0.005)	(0.205)	(0.060)	(0.008)	(0.002)	(0.002)
N	233,541	233,541	233,541	59,976	233,541	233,541	233,541
R-squared	0.740	0.523	0.630	0.591	0.356	0.022	0.089
Panel B				Less than college			
School closure	0.002	-0.005	-0.100	0.024	0.005	0.002	0.001
	(0.005)	(0.007)	(0.251)	(0.063)	(0.003)	(0.001)	(0.003)
Presence of school-age children (5-17)	0.003	0.017**	0.554**	0.020	-0.006	0.003	-0.000
	(0.005)	(0.006)	(0.254)	(0.058)	(0.003)	(0.002)	(0.003)
School closure x presence of school-age children	-0.008	-0.021***	-0.679**	0.096	0.012**	-0.001	0.012***
	(0.005)	(0.007)	(0.271)	(0.079)	(0.005)	(0.002)	(0.003)
N	139,663	139,663	139,663	35,890	139,663	139,663	139,663
R-squared	0.725	0.518	0.630	0.591	0.214	0.029	0.096

*** p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses. School closures refer to the share of all schools that were reportedly virtual or hybrid (not in-person) in each county. Panel A are the results for all males, and Panel B are for males with less than college degree.

Appendix

Table A1. OLS Regressions on "At Work", "Full-time", "Hours Worked" and "Log of Real Weekly Earnings", "Remote Work due to COVID", "Part-time or Absent due to childcare or family", and "Not in labor force due to childcare or family", with seasonal dummies

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	At Work	Full-time	Work Hours	Log of Real Weekly Earnings	Remote Work due to COVID	Part-time/ Absent due to childcare/ family	Not in labor force due to Family reasons
Panel A				Female			
School closure x presence of school-age children	-0.020***	-0.021***	-0.951***	-0.064	0.024***	-0.024***	0.012*
	(0.007)	(0.007)	(0.300)	(0.089)	(0.005)	(0.005)	(0.006)
School closure x presence of school-age children x Fall 2020	0.009	-0.008	-0.057	-0.131	0.042***	0.015**	0.009
	(0.008)	(0.011)	(0.385)	(0.093)	(0.010)	(0.007)	(0.009)
School closure x presence of school-age children x Spring 2021	0.011	0.011	0.448	-0.020	0.032***	0.009	0.013*
	(0.008)	(0.011)	(0.413)	(0.100)	(0.010)	(0.007)	(0.008)
School closure x presence of school-age children x Fall 2021	0.013	-0.004	-0.363	0.123	0.001	0.019	0.016
	(0.014)	(0.025)	(0.821)	(0.188)	(0.015)	(0.016)	(0.019)
N	348,278	348,278	348,278	90,461	348,278	348,278	348,278
R-squared	0.768	0.502	0.667	0.695	0.304	0.076	0.396
Panel B				Male			
School closure x presence of school-age children	-0.012*	-0.041***	-1.619***	-0.175*	0.030***	-0.005***	0.002
	(0.007)	(0.008)	(0.355)	(0.095)	(0.006)	(0.002)	(0.004)
School closure x presence of school-age children x Fall 2020	0.009	0.004	0.409	0.123	0.057***	0.005	0.006
	(0.009)	(0.012)	(0.471)	(0.132)	(0.013)	(0.004)	(0.005)
School closure x presence of school-age children x Spring 2021	0.013	0.035***	0.667	0.123	0.052***	0.004	0.002
	(0.009)	(0.012)	(0.491)	(0.120)	(0.013)	(0.002)	(0.004)
School closure x presence of school-age children x Fall 2021	0.019	0.044**	-0.252	0.236	0.055**	-0.001	-0.008
	(0.014)	(0.021)	(0.989)	(0.238)	(0.022)	(0.007)	(0.013)
N	312,703	312,703	312,703	80,932	312,703	312,703	312,703
R-squared	0.728	0.517	0.622	0.580	0.317	0.018	0.085

*** p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses. School closures refer to the share of all schools in each county that had at least 50 percent year-on-year decline in in-person visits. Panel A are the results for females, and Panel B are for males.

Table A2. OLS Regressions on "At Work", "Full-time", "Hours Worked" and "Log of Real Weekly Earnings", "Remote Work due to COVID", "Part-time or Absent due to childcare or family", and "Not in labor force due to childcare or family", using 75% closure cutoff

VARIABLES	(1) At Work	(2) Full-time	(3) Work Hours	(4) Log of Real Weekly Earnings	(5) Remote Work due to COVID	(6) Part-time/ Absent due to childcare/ family	(7) Not in labor force due to Family reasons
Panel A				Female			
School closure	-0.027***	-0.015	-1.463***	-0.213**	0.080***	-0.002	0.009
	(0.007)	(0.011)	(0.401)	(0.092)	(0.021)	(0.005)	(0.008)
Presence of school-age children (5-17)	0.013***	-0.001	0.295**	0.020	0.000	0.024***	0.016***
	(0.003)	(0.004)	(0.132)	(0.029)	(0.003)	(0.003)	(0.003)
School closure x presence of school-age children	-0.028***	-0.029***	-1.378***	-0.148	0.044***	-0.032***	0.020**
	(0.009)	(0.010)	(0.436)	(0.123)	(0.009)	(0.007)	(0.009)
N	348,278	348,278	348,278	90,461	348,278	348,278	348,278
R-squared	0.768	0.502	0.667	0.695	0.303	0.076	0.396
Panel B				Male			
School closure	-0.007	0.011	0.219	0.124	0.082***	-0.005**	0.001
	(0.008)	(0.012)	(0.425)	(0.129)	(0.022)	(0.002)	(0.004)
Presence of school-age children (5-17)	0.007**	0.019***	0.796***	0.106**	-0.001	0.003**	0.004***
	(0.003)	(0.004)	(0.171)	(0.042)	(0.003)	(0.001)	(0.002)
School closure x presence of school-age children	-0.017*	-0.059***	-2.199***	-0.233*	0.047***	-0.006**	0.010**
	(0.009)	(0.010)	(0.423)	(0.125)	(0.010)	(0.003)	(0.005)
N	312,703	312,703	312,703	80,932	312,703	312,703	312,703
R-squared	0.728	0.517	0.622	0.580	0.316	0.018	0.085

*** p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses. School closures refer to the share of all schools in each county that had at least 75 percent year-on-year decline in in-person visits. Panel A are the results for females, and Panel B are for males.