SCHOOL CLOSURES, PARENTAL LABOR SUPPLY, AND TIME USE

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Abstract

This paper re-examines the response of parental labor supply to the pandemic-era suspension of in-person instruction. The effect of school closures is undetectable after controlling comprehensively for unobserved heterogeneity. Even excluding such controls, a shift from fully virtual to in-person implies an increase in weekly hours worked of just 2 to 2.5. These estimates are used to inform a simple model of the household in which access to telework and nonparental care mitigate the labor supply impact of school closures. Time use data suggest telework and nonparental care indeed helped some parents balance work and childcare during the pandemic.

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Beginning in March 2020, U.S. schools switched to remote instruction, and many did not reopen for consistent in-person instruction for a year. The suspension of in-person instruction was widely expected to upend working parents' careers (Goldin, 2022). However, initial analyses did not point to a dramatic change in parents' working time (Goldin, 2022; Furman et al., 2021).

Prompted by these findings, we first re-evaluate the impacts of school closures on parental labor supply. We consider a variety of specifications that, taken together, suggest the true response lies within a fairly tight range that includes zero. This raises interesting questions for the economics of time use, which we then examine. How did parents ease the trade-off between market work and childcare? On what margins, beyond labor supply, did they adjust? And what do these decisions imply about the preferences, technologies, and constraints shaping parents' time allocation decisions?

As a first step, we revisit evidence on the effect of remote instruction on parental labor supply. Following leading work by Garcia and Cowan (2024) and Hansen et al. (2024), we link adults' working time to the local schooling mode. As detailed in Section 1, the in-person share of instruction time is based on visits to school campuses as captured by SafeGraph's mobile phone location data (Parolin and Lee, 2021). When aggregated to a county or larger unit, these estimates can be matched to individuals (in that local area) in the Current Population Survey (CPS).

The merged SafeGraph-CPS dataset is the main source for our labor supply analysis. We estimate regressions that relate individual hours of work to the local in-person share of instruction. The potential endogeneity of school policy complicates the interpretation of this estimate, however. For example, parents' labor supply and school policy may be jointly shaped by local institutions and preferences.

While there is no "silver bullet" for this endogeneity problem, we present evidence based on a range of specifications that the causal effect is likely to lie within a reasonably narrow set of estimates. Our most parsimonious model follows the standard practice in this literature, which is to use childless adults as a control group for parents (Garcia and Cowan, 2024; Heggeness and Suri, 2021). This leverages within-area variation in working time across adults with and without children, effectively differencing out area-wide factors. We also consider richer specifications that include fixed effects which interact parental status with time and with local area. These additional fixed effects are motivated by the idea that, insofar as parents and nonparents have different preferences and market opportunities, this heterogeneity may vary across space as well as over time in the pandemic period.

Section 3 presents our main labor supply results. In the most parsimonious specification, a switch from virtual to in-person instruction lifts parents' hours by 0.5 per week relative to those of childless adults. With parental status-by-time effects included, the coefficient jumps to around 2.5 hours per week for mothers and 1.6 for fathers. We trace the source of this difference to what happened in the first half of 2020. We suggest that the original estimate of the effect on parental hours may be depressed because widespread school closures at that time coincided with exceptional labor market turbulence for childless adults.

The introduction of parental status-by-area effects has the opposite impact: it *eliminates* any association between in-person shares and parents' relative hours (that is, hours relative to those of adults with no kids). A null effect may arise if school policies are correlated with long-standing spatial differences in parental working time. However, the addition of more fixed effects also risks saturating the model. To arbitrate this issue, Section 3 conducts a placebo test: are school policies correlated with *pre*-COVID labor market outcomes? Indeed, higher average pandemic-era in-person shares predict higher parents' relative hours worked in the pre-pandemic period, particularly for mothers.

The placebo results suggest that specifications without parent-by-area effects are likely to yield upwardly biased estimates. This is notable since an hours response of around two is modest. The latter may, however, mask larger shifts by some parents. To probe how high such estimates may go, we extend our analysis without parent-by-area effects to various demographic groups.

We highlight three results on the heterogeneity of labor supply responses. First, estimates are similar across levels of educational attainment apart from college-educated fathers, who are essentially unresponsive to the in-person share. Second, among parents of younger school-age children (i.e., with children aged 5-9), hours adjust by as much as three per week. Third, labor supply responses vary little by marital status but do vary within the unmarried. Labor supply is relatively elastic among lone-adult parents—weekly hours rise by as much as four when in-person instruction is reinstated—but unresponsive among the unmarried in co-residential arrangements.

To take stock, we see a labor supply response of 2-4 weekly hours as the upper end of any plausible range of estimates. This figure is a small fraction of the roughly 30 hours of on-site time at reopened schools. This observation suggests that parents must have adjusted time use on other margins so as to both attend to children and supply labor.

To this end, we next report on several results from the American Time Use Survey (ATUS). First, there was in fact little adjustment in leisure, market work, or home production to variation in in-person instruction shares. Second, telework was likely one means by which some parents insulated their schedules from pandemic disruptions. Our estimates suggest that a shift from in-person to virtual school formats led college-educated parents to spend 6 more hours per week working from home while simultaneously looking after their children. We observe no telework response among the noncollege educated, consistent with the observed divide in telework opportunities by education (Mongey et al., 2021). Third, nonparental care was used more intensively in the pandemic period. Respondents over age 60—a group likely to include many grandparents—allocated up to 4 more hours per week to the care of *others'* children when in-person instruction was suspended. This response was observed only among those with no college degree.[1] Because of the small sample size of the ATUS, these estimates are subject to considerable uncertainty. Still, the results point to two promising explanations of the labor supply findings.

In Section 5, we view these results through the lens of simple models of parental time allocation. To start, we consider a baseline with no telework or nonparental care. Following Berlinski et al. (2024), a parent in the model values consumption, leisure time, and child development. In this setting, a child's development is a function of (only) two arguments: the parent's supervision and a form of publicly provided supervision, e.g., in-person class time. In addition, a child must always be supervised by a parent or by school. We show that a decline in publicly provided supervision leads the parent to substitute time toward childcare and yields a reduction in labor supply that is *at least* four times larger than our upwardly biased estimates (see above). In this sense, the regression estimates appear to be remarkably small.

We then amend this baseline setup to illustrate the potential roles for telework and nonparental care. First, we introduce a novel "multi-tasking" technology to capture the idea that teleworking enables parents to carry out, to an extent, multiple tasks at the same time, e.g., working while simultaneously supervising children. The technology is indexed by just a single parameter, and we derive the mapping from this parameter to the labor supply response. Second, noting that

[1] We view grandparents' educational attainment as the best available proxy for that of the parent. Unfortunately, the ATUS does not report the identity or the educational attainment of the parent of the child who received care from the over-age-60 respondent. We return to this issue later in Section 4.

many parents did not have access to a telework opportunity, we next consider a margin of adjustment omitted from the baseline model, namely, nonparental care. We show that our labor supply findings are consistent with parental and nonparental care being strong substitutes in child development (Berlinski et al., 2024). This section concludes by highlighting the broader implications of this substitutability for public policy and cyclical hours dynamics.

Related research. Our paper intersects with several strands of research. First, our analysis of CPS data contributes to the literature on labor supply in the pandemic period. Our placebo test probes for the endogeneity of school policy and frames results from standard, two-way fixed effects models as upwardly biased estimates of the true effect. We see this approach as a complement to some earlier efforts. For instance, Hansen et al. (2024) apply event-study methods to an alternative measure of the in-person share and find support for a causal effect of school policy on (only) married mothers. In our context, the analogue to the pre-trends test—the placebo test—fails. Nevertheless, the failure lends a sharp interpretation to the OLS estimates and enables us to derive from them substantial insight into parental time use.

Consistent with this reading of results, our estimates from more standard specifications tend to exceed those in the broader literature on effect of childcare availability on parental labor supply. For instance, in an analysis of the introduction of public kindergarten, Gelbach (2002) and Cascio (2009) find similar or slightly smaller estimates for unmarried mothers but notably weaker responses of married mothers. The international evidence is more varied, but few if any find larger estimates than we report. Several studies find a comparable impact of longer school instruction for one parental group (i.e., married mothers) but not for others (Contreras and Sepúlveda, 2017;

Padilla-Romo and Cabrera Hernandez, 2019; Berthelon et al., 2023). Null effects have also been reported (Felfe et al., 2016).[2]

Next, our analysis of the ATUS contributes to a growing research agenda on telework. Pabilonia and Vernon (2023) document that take-up of telework increased at the onset of the pandemic, especially for mothers of children under the age of 13. Teleworking parents spent a large share of their day on secondary childcare activities. Atalay (2023) shows that these shifts were more pronounced for college-educated parents (see also Cowan, 2024). Our results echo these findings on the incidence of telework and caregiving during the pandemic. We extend this research by more precisely linking parental time use patterns to local in-person instruction shares.

Finally, we connect pandemic-era research on school closures to economic theory. We show analytically how our regression results inform models of parental investments and adolescent development and illustrate their broader implications for policy interventions and labor market dynamics. The mechanisms that we highlight—most notably, nonparental care—may in turn shed light on earlier empirical analysis of childcare availability (see above). In addition, we offer a means to formalize a new mechanism, telework, that is still used widely (see Barrero et al., 2024). We view our efforts to draw out lessons from the data within simple models as complementary to the estimation of richer models (see Del Boca et al., 2014, and Berlinski et al., 2024).

1. Data

This section introduces our measures of in-person instruction as well as our data sources for labor supply and other variables used in the regression analysis.

[2] A related strand of research documented changes in hours worked in the months immediately after the onset of the pandemic. Some of this research found substantial movements in parental hours (Alon et al., 2020; Heggeness, 2020), whereas others found more muted responses (Lozano- Rojas et al. 2020; Barkowski et al., 2024). Our analysis will span all of 2020-21 and with more of a focus on the period beginning with the fall 2020 to spring 2021 school year.

1.1 In-person instruction

The pandemic prompted almost all school districts to shift toward remote instruction in March 2020. Although many retained this format to start the 2020-21 school year, modes of instruction did begin to diverge then—even across neighboring counties. For instance, the Atlanta district in Fulton County operated strictly remotely, whereas Forsyth County, just 40 miles north, made in-person instruction available to all students (Education Week, 2020).

The variation in school reopening plans spurred the creation of numerous schooling mode trackers, which aim to document the predominant mode of instruction in school districts. A few prominent sources include the American Enterprise Institute's (AEI) Return2Learn database, Burbio's School Reopening Tracker, and the COVID-19 School Data Hub (CSDH). These trackers vary with respect to the breadth of their coverage (e.g., the number of school districts in the sample); level of detail (i.e., grade-level v. district-wide outcomes); and data collection methods (i.e., web scraping v. school- and district-level surveys). The in-person instruction shares do vary across the trackers, which suggests that the different choices of methodology and sampling do shape the results (Kurmann and Lalé, 2023).

Alternatively, some recent research has adopted a more indirect, but also more easily quantifiable, proxy of on-site instruction, namely, the volume of "foot traffic" on school campuses (Garcia and Cowan, 2024; Hansen et al., 2024). The source of the underlying data is SafeGraph, which obtains GPS data from individual mobile phones by pinging certain apps. The location data enable SafeGraph to track the number of visits to over 7 million points of interest (POI) in the U.S. We will draw specifically on Parolin and Lee's (2021) tabulations of SafeGraph data. For each

POI identified as a public school, Parolin and Lee calculate the percent change in visits between year $y \geq 2020$ and month m relative to the same month m in 2019.[3]

Our main measure of school policy from Parolin and Lee is constructed as follows. First, a school is classified as "closed" in some month m (and year $y \geq 2020$) if the number of visits to that school is down by at least 50 percent relative to month m in 2019. Parolin and Lee then calculate the closed share of schools within each county (and month). The complement of this—that is, one minus their figure—can be interpreted, roughly, as the in-person instruction share.

SafeGraph has several advantages. First, it is arguably the most comprehensive source of data in this literature, covering over 100,000 schools and virtually every county during the 2020-21 and 2021-22 school years. In addition, the use of mobile phone data naturally accommodates heterogeneity in learning modes. Within a district, some schools—and, within those schools, some students—may attend on-site while others operate predominantly remotely. Other schooling-mode trackers classify the district according to one of a few coarse, discrete formats, such as "hybrid" or "virtual," whereas SafeGraph's data implicitly aggregate these modes into a single estimate of the change in on-site activity. Thus, SafeGraph provides unique breadth and precision.

The aggregation over foot traffic means, however, that SafeGraph captures both the *provision* of on-site instruction and parents' *take-up* of the in-person option.[4] The take-up decision is endogenous to labor supply: A parent who wants to work is more likely to enroll children in in-person instruction. For this reason, our SafeGraph-based estimates of the hours response to school closures should provide an upper bound. Estimates off CSDH data are subject to the same concern since the latter is derived from enrollment in each instruction mode. By contrast, Burbio documents

[3] Parolin and Lee drop private schools because their analysis uses other student data available only for public schools.
[4] Calarco et al. (2021) report that, in their survey of parents in late 2020, 75 percent of children had at least some access to in-person instruction, but less than 60 percent attended school on-site.

only the availability of on-site instruction. Online Appendix C.2 shows that SafeGraph indeed yields the largest hours responses and Burbio the smallest; results from CSDH lie in between.

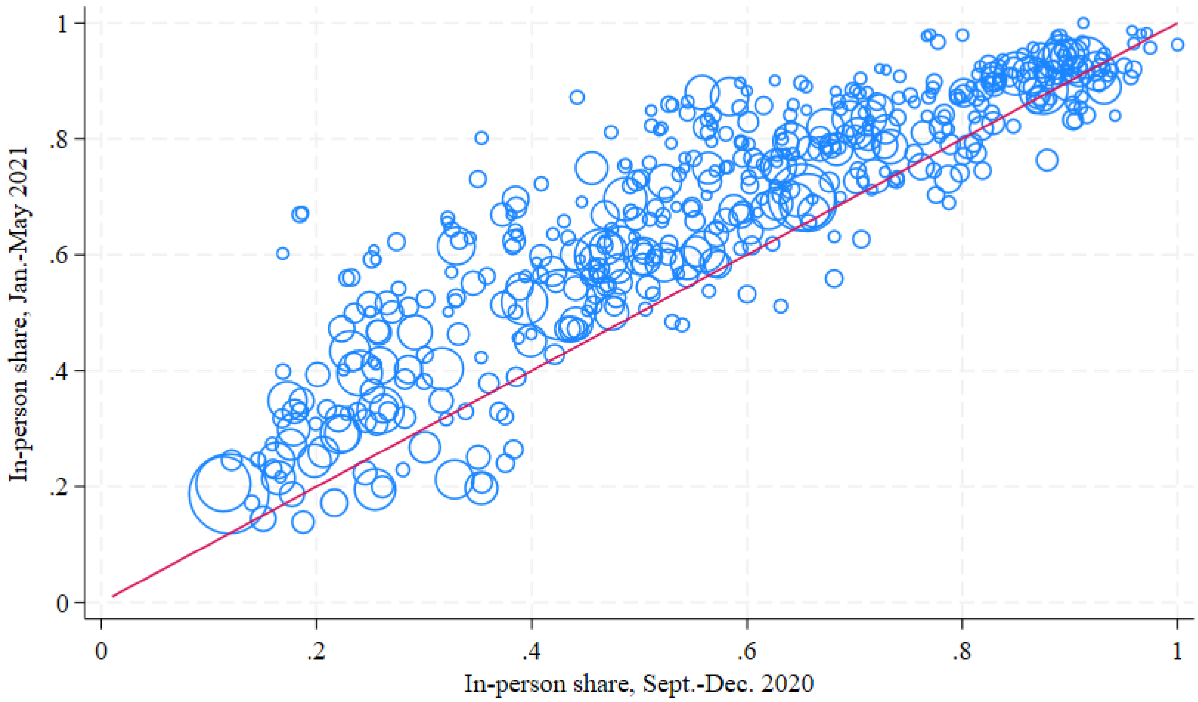
Geographic variation in in-person shares. Although Parolin and Lee's estimates cover the more than 3,000 U.S. counties, other data sources do not offer this same scope. The Current Population Survey, our source on hours worked, neither discloses school districts nor universally reports the respondent's county. Indeed, county is not disclosed for 60 percent of (adult) survey respondents. Fortunately, though, the CPS identifies the metropolitan statistical area (MSA) for almost 60 percent of those with no reported county. A respondent's state is always provided.

In view of these constraints, we apply a three-step method to aggregate SafeGraph data and integrate it into the CPS (see Hansen et al., 2024). First, we assign the county-level in-person share from Parolin and Lee to a survey respondent if the latter's county is one of the 280 identified in the CPS. Second, for respondents who have no county identifier but who belong to a disclosed MSA, we assign the mean in-person share among the non-identified counties in that MSA. Finally, we aggregate Parolin and Lee's estimates among those counties within a state that are not reported in the CPS and do not belong to a reported MSA. The mean among these counties is assigned to CPS respondents in the state for whom no county or MSA identifier is provided. In total, by aggregating within MSA where feasible and within state where necessary, we identify 198 more areas to reach a total of 478.[5] This strategy maximizes the use of the Parolin and Lee data.

Figure 1 illustrates the variation in in-person shares. For each area, the average in-person instruction share in September-December 2020 is shown along the x-axis and the average share in January-May 2021 along the y-axis. The figure shows, first, that there are significant differences

[5] The additional local areas include 151 MSAs, or subsets of MSAs. If a county is reported in the CPS, it is not included in our construction of an MSA-based local area. The remainder of local areas comprises data from 47 states where we observe CPS respondents who do not belong to a disclosed county or MSA. This step captures data from only 47 states because in a handful of very small states, all survey respondents live in a disclosed county or MSA.

Figure 1: In-Person Shares in 2020-21 School Year



Note: This figure plots the average in-person share—from Parolin and Lee's (2021), measures derived from SafeGraph data—in September to December 2020 (x-axis) compared to January to May 2021 (y-axis). The size of each circle is proportional to the population in the geographic area.

across areas. In each of the two semesters, in-person shares span a wide range from 0.2 to 1. Second, these regional differences are, to some extent, persistent: in almost half of the areas, the in-person share shifted by less than 10 percentage points across semesters. In the other half of the areas, there was more substantial variation in instruction format *within* region. The latter variation generally reflected differences in the timing of reinstating in-person instruction in spring 2021.

What might account for the differences in in-person shares illustrated in Figure 1? And are any of these sources of variation likely to shape labor supply? Clearly, one possible source is the spread of COVID-19: if the threat of infection and fatality were to recede, both in-person instruction and labor supply might rise, even if the former has no causal effect on the latter.

In fact, the link between instruction format and COVID-19 case counts is remarkably modest. Online Appendix A lays out the evidence on this point. We suspect that monthly changes in case counts are weakly correlated with changes in that area's policy because the latter had to be set well in advance of implementation. For example, Prince George's County (Maryland) announced in mid-July 2020 that it would not consider a return to in-person instruction before February 2021. Around the same time, Fairfax County (Virginia) announced that it would not reinstate on-site instruction until November. (In each county, COVID-19 cases had been on the decline throughout the summer.) These examples suggest that current school policy was partially predetermined and, therefore, unlikely to react sharply to changes in the state of the pandemic.

Instead, as Online Appendix B illustrates, school policy appears to be shaped by regional political forces. Partisan affiliation and, more concretely, the degree of support for Donald Trump were significant predictors of school policy. The strength of teacher unions also helps account for variation in in-person shares.[6] These factors would seem to reflect long-held local preferences and norms, which in turn may be correlated with labor market activity independent of school policy. We return to this point in Section 2.

1.2 Summary of sample

We draw on several data sources for our main regressions (in addition to the measures of on-site instruction). Labor supply and worker demographics are taken from the monthly Current Population Survey (Flood et al., 2022). We typically measure labor supply as weekly hours of work in the survey reference week (which may be zero) but also report results for employment status. Other variables measure the state of the pandemic and public health policy responses. We draw on county-level data on COVID-19 cases and deaths published weekly by Johns Hopkins

[6] For results on partisanship and union strength, see Grossmann et al. (2021), Hartney and Finger (2021), and Marianno et al. (2022). Online Appendix A reports that the interaction between the latter and COVID-19 cases are statistically significant predictors of instruction format but still account for a very limited share of the variance in in-person shares.

Coronavirus Resource Center (Dong et al., 2020).[7] These data are aggregated up to the monthly frequency and to the local geographic areas described above. We use Kaiser Family Foundation measures of government mitigation policies, such as capacity limits on restaurants and bars.[8]

Table 1 reports means for many of the variables that will be used in our regressions. The averages are presented for several different subgroups of the population, distinguished by sex, age, and location. The top panel collects tabulations for women, whereas the bottom panel refers to men. We also present results for the 280 CPS-reported counties (left-hand side) and the full sample of 478 local areas (right-hand side). Finally, for each sample of locations, the table reports on three groups; adults 21 and over; adults in the narrower range of ages 21-59; and parents of school-age children. (The ages of parents are unrestricted, but nearly all fall within the range 21-59.) As discussed later, our regression sample consists of all areas but restricts attention to ages 21-59. It is instructive to contrast our preferred sample to the alternative groups in Table 1.

A few patterns in the data are noteworthy, if not necessarily unexpected. First, the sample of all adults ages 21 and over has fewer kids in the home, is less racially and ethnically diverse, and works less than the other two groups. In other words, this subsample is observationally quite different from parents of school-age children. By contrast, the sample of adults ages 21-59 is very similar to parents (with marital status the obvious exception). Next, CPS counties are relatively urban, educated, and ethnically diverse and adopted in-person instruction less often. Thus, the use of all local areas captures a broader sample of parents and school policies. Finally, well-known differences in employment and marriage rates between mothers and fathers are evident in the table (Doepke and Tertilt, 2016). The labor supply of single mothers will be of special interest below.

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[7] These data can be found at https://github.com/CSSEGISandData/COVID-19/tree/master/csse_covid_19_data/csse_covid_19_time_series. Accessed August 2, 2023.

[8] These data can be found at https://github.com/KFFData/COVID-19-Data/tree/kff_master/State%20Policy%20Actions. Accessed August 2, 2023.

Table 1: Summary Statistics

	Women					
Variable	CPS Counties			All Local Areas		
	Age ≥ 21	21 − 59	Parents	Age ≥ 21	21 − 59	Parents
Weekly hours	19.221	24.791	23.745	19.255	24.978	24.185
Employment	0.519	0.662	0.645	0.521	0.666	0.655
Age	49.941	39.793	41.122	50.074	39.810	40.650
Kids in home	0.219	0.319	1.000	0.225	0.330	1.000
Bachelor or more	0.405	0.442	0.432	0.376	0.412	0.407
White	0.739	0.717	0.715	0.768	0.743	0.744
Black	0.141	0.152	0.151	0.134	0.145	0.141
Hispanic	0.200	0.233	0.278	0.160	0.192	0.230
Foreign born	0.246	0.255	0.315	0.188	0.203	0.254
Married	0.510	0.514	0.703	0.528	0.534	0.703
Resides in city center	0.342	0.358	0.318	0.286	0.304	0.270
Mo. cases / 100,000	691	686	694	711	706	710
In-person instruction	0.586	0.582	0.590	0.647	0.642	0.650
Number of obs.	314,530	201,720	66,039	762,718	481,485	165,625
	Men					
	CPS Counties			All Local Areas		
	Age ≥ 21	21 − 59	Parents	Age ≥ 21	21 − 59	Parents
Weekly hours	25.951	31.658	35.559	26.106	32.124	36.225
Employment	0.640	0.772	0.845	0.639	0.776	0.851
Age	48.474	39.429	43.800	48.748	39.588	43.363
Kids in home	0.195	0.265	1.000	0.200	0.274	1.000
Bachelor or more	0.386	0.385	0.420	0.351	0.350	0.390
White	0.757	0.735	0.749	0.785	0.764	0.779
Black	0.127	0.137	0.118	0.119	0.129	0.105
Hispanic	0.209	0.242	0.279	0.169	0.201	0.235
Foreign born	0.244	0.255	0.338	0.187	0.204	0.274
Married	0.556	0.500	0.854	0.569	0.516	0.850
Resides in city center	0.345	0.362	0.303	0.286	0.307	0.254
Mo. cases / 100,000	689	685	690	711	707	708
In-person instruction	0.585	0.580	0.591	0.647	0.643	0.649
Number of obs.	282,721	189,026	52,846	695,582	456,655	134,294

Note: "CPS Counties" refers to the sample of counties that are recorded in the Current Population Survey. "Parents" are adults with at least one child between the ages of 5 and 17 in the household. Monthly cases / 100,000 refers to the number of COVID-19 cases in the local area of the respondent in the survey month. In-person instruction refers to the share of schools in a local area open to in-person instruction in the survey month.

2. Empirical framework

Our aim is to examine the effect of in-person instruction on parental labor supply. We describe a series of specifications that differ only with respect to their treatment of unobserved heterogeneity.

We first consider the regression specification adopted in much of the related literature (Garcia and Cowan, 2024; Heggeness and Suri, 2021; Collins et al., 2021). This specification allows that the in-person share is endogenous to the state of the labor market but assumes it is (as good as) random with respect to parents' *relative* hours worked (that is, relative to the hours worked of childless adults). Thus, the regression leverages within-area differences in hours worked across adults with and without children.

This approach is formalized as follows. Denote the presence of one's own children in the home in month t by the indicator $\mathbb{k}_{it} = \{0,1\}$. The latter equals one if survey reference person i reports having children of school age in the residence. Next, let p_{at} denote the in-person instruction share in area a.[9] The effect of interest is, specifically, the parental labor supply response to variation in p_{at}. Accordingly, we adopt the estimating equation,

$$h_{iat} = \eta\mathbb{k}_{it} + \delta p_{at} + \psi p_{at}\mathbb{k}_{it} + \mathbf{\Gamma}'\mathbf{X}_{it} + \chi_a + \tau_t + \varepsilon_{iat}, \tag{1}$$

where h_{iat} is labor input of individual i in area a in month t. We generally take h_{iat} to be weekly hours worked, but we also present results where h_{iat} is a binary indicator of employment. The vector $\mathbf{X}_{it}$ captures additional individual-level controls to be described in the next section (and $\mathbf{\Gamma}$ is a conformable vector of coefficients); χ_a is an area fixed effect; and τ_t is a month fixed effect.[10]

[9] In practice, this share varies *within* area, across school districts. Nevertheless, OLS yields consistent estimates so long as p_{at} correctly measures the mean of district-level shares.
[10] We have replaced τ_t with month-by-Census division effects, but this added granularity makes little difference.

The key parameter here is ψ, which measures the parental hours response to a unit difference in the in-person share.

Under certain conditions, ψ can be estimated consistently even if schooling mode is endogenous to local area trends. These trends will be picked up by p_{at} and reflected in δ, which measures the average response of *all* adults. An estimate of $\delta \neq 0$ will emerge if, for instance, schooling mode coincides with a general return to "normalcy", which shapes market-wide labor supply and demand. By contrast, ψ reflects the behavior of *parents' relative* hours of work (that is, relative to that of childless adults). Thus, the identifying assumption behind equation (1) is that any residual factors driving parents' relative hours are uncorrelated with local school closures.

Using a second specification, though, we can partially relax this identifying restriction. Consider the estimating equation,

$$h_{iat} = \delta p_{at} + \psi p_{at} \mathbb{k}_{it} + \zeta_a \mathbb{k}_{it} + \theta_t \mathbb{k}_{it} + \mathbf{\Gamma}' \mathbf{X}_{iat} + \chi_a + \tau_t + \omega_{iat}, \qquad (2)$$

which introduces two new fixed effects that interact with parental status. (These replace and extend the regressor, $\eta \mathbb{k}_{it}$, in equation (1).) The parameter θ_t captures parent-specific factors behind hours worked that are common across areas but vary over time, whereas ζ_a captures fixed cross-area differences in parents' relative hours. These fixed effects allow that the (unobserved) labor supply motives of parents may evolve over time coincident with the "typical" school policy in the U.S. (i.e., θ_t) and/or correlate with the average policy in their local area (i.e., ζ_a). The identifying assumption underlying equation (2) is that changes in these idiosyncratic motives *within* a local area are uncorrelated with changes in *that area's* instruction format.

Thus, the added controls in equation (2) narrow the scope of variation used to identify the coefficient, ψ. Equation (2) recovers a significant effect only to the extent that parents' relative hours co-move with the in-person share in their area.[11] By contrast, equation (1) exploits both the within- and across-area correlation of in-person shares and parents' relative hours. Equation (2) offers potentially more credible identification but at the cost of statistical power.

Extending the approach underlying equation (2) still further, one could insert *individual* level fixed effects. This specification maps the change in in-person shares facing each survey respondent to the change in her hours of work, thereby identifying ψ using only variation in school policies over time. An individual fixed effects regression can be estimated by using the longitudinal dimension of our data. Since respondents in the same area are exposed to the same school policies, though, this specification yields results that are similar to what is implied by the introduction of parent-by-area controls in equation (2).

3. Estimates from the CPS

In this section, we report estimates from the regression models just discussed. After we specify our sample and list of controls, we present our baseline estimates of equations (1) and (2) in Section 3.1. In Section 3.2, we report results by marital status and education.

Sample. Our preferred sample consists of adults aged 21-59. An adult is considered a parent of a school-age child if they have a child between ages 5 and 17 in the home. Households with only children under age five are excluded to isolate the impact of school-age children on labor supply. The age restriction on adults captures 98 percent of parents with school-age children. Online Appendix C.3 considers several variations on this sample. First, we divide households more finely by age of the eldest child and show that our results below largely stem from households with children under age 13. Second, we find that the inclusion of childless adults over age 59 yields

[11] This aspect of equation (2) is shared by a simpler regression that maps hours worked to in-person shares *within the sample of parents.* Since childless adults would be excluded, identification rests entirely on within-area variation in schooling mode. The key difference between these two approaches is that equation (2) allows that changes in in-person shares may be endogenous to changes in the overall state of the local labor market.

larger parental labor supply responses. However, the parental hours response in this context reflects—and is amplified by—a common component in hours shared by *all* adults under age 59.

In addition, our full sample encompasses the broadest geographic coverage possible. We include all 478 local areas constructed from county, metro, and state identifiers in the CPS (see Section 1). Analogous results for the 280 counties disclosed in the CPS are reported in Online Appendix C.5. Estimates based on the latter, more restricted sample are somewhat smaller (and less precisely estimated) than those reported below.

Control variables. There are two distinct groups of regressors in $\mathbf{X}_{iat}$, each of which was advanced in Garcia and Cowan (2024). The first consists of demographic controls: age (and age squared); race; marital status; educational attainment; an indicator for rural, urban, or suburban location; the number of children (of all ages under 18); an indicator for the presence of under-five-year-old children; and indicators of Hispanic heritage, foreign birth, veteran status, and disability.[12]

The second group of regressors tracks the trajectory of the pandemic. These controls are the cumulative number of cases and deaths; the new monthly number of cases and deaths; and indicators for nonpharmaceutical interventions, such as Stay at Home orders. While we include this group for the sake of completeness, our estimates of ψ are essentially invariant to them. The reason is that these controls are common across adults with and without children and, as such, are differenced away in regression models of parents' relative hours worked (see equation (1)).

A third potential group of controls includes respondents' experience in an industry and occupation. Unfortunately, these data are not reported in the CPS for most labor force

[12] The only controls here that are not present in Garcia and Cowan (2024) are the indicators for rural-urban-suburban status and for the presence of under-five-year-old children in the home.

nonparticipants.[13] Nevertheless, Online Appendix C.6 does introduce these controls and confirms that the impact of in-person shares is estimated to be even smaller than reported below.[14]

3.1 Full sample

We proceed to estimate the standard two-way fixed effects model in equation (1), with weekly hours worked as the dependent variable. Online Appendix C.1 reports results for employment. Table 2 presents results for two periods: the longer one spans all of 2020-21 except for the summer months, whereas the shorter period covers the 2020-21 school year (September 2020 – May 2021). Note that the former period featured school closures, whereas the latter was characterized by a staggered reopening to in-person learning. Thus, by separating out the latter period, we can examine if closing and reopening had meaningfully different effects. For each period and each outcome, we also report results separately for men and women. Finally, in view of the arguments in Solon et al. (2015), we report both unweighted estimates and estimates that apply CPS sample weights. While weighting makes little difference on balance, we will highlight the few instances where it does.

Consider first the results for the longer sample period (2020-21). The main parameter of interest is ψ, which measures the response of parents' hours worked to the in-person share. Among women, a shift from fully virtual to fully in-person instruction implies an increase in hours worked of roughly 0.5 per week. The overall hours response among fathers is nearly identical in the unweighted specification, but closer to 0.2 when applying the CPS sample weights. Finally, estimates of δ, which capture the area-wide hours response, are positive and significant, suggesting

[13] Industry and occupation are collected of nonparticipants in the Outgoing Rotation Groups (ORG) who report that they have worked in the past 12 months. The ORGs *as a whole* make up only one quarter of the CPS sample.
[14] Relatedly, we also do not restrict the sample based on industry or occupation affiliation. There is arguably a case to exclude respondents in the education sector since changes in in-person shares might mechanically imply changes in their hours worked (see Hansen et al., 2024). We confirm that this restriction has a negligible impact on our estimates.

Table 2: Estimates of Equation (1)

	All of 2020-21		2020-21 School Year	
	Women:			
In-person share, δ	1.205***	1.130***	-0.853	-0.931
	[0.338]	[0.401]	[0.594]	[0.690]
In-person × kids, ψ	0.582*	0.472	2.113***	2.293***
	[0.304]	[0.326]	[0.590]	[0.629]
Number of obs.	447,899	447,277	228,550	228,225
	Men: Weekly Hours			
In-person share, δ	1.285***	0.856**	-0.009	0.006
	[0.382]	[0.430]	[0.647]	[0.696]
In-person × kids, ψ	0.566*	0.210	1.456**	1.315**
	[0.315]	[0.321]	[0.589]	[0.559]
Number of obs.	432,856	428,244	221,080	218,575
CPS Weights	No	Yes	No	Yes

Note: Each column within each panel is a separate regression. In addition to the coefficients listed in the table, each regression includes the controls described in the main text (see "Control variables"). Standard errors are clustered at the geographic area level. "All 20-21" pools data for all of 2020 and 2021 but for the summer months (June, July, and August). "School 20-21" refers to the period September 2020 to May 2021. *** indicates a p-value less than 0.01; ** a p-value between 0.01 and 0.05; and * a p-value between 0.05 and 0.10.

that in-person shares may pick up broader shifts in the propensity to work.[15]

Next, we turn to the results in Table 2 for the 2020-21 school year. These results paint a different picture than the full 2020-21 sample. First, the overall hours response among parents is notably higher: a shift from fully virtual to fully in-person now implies an increase in mothers' relative labor input of just over two hours per week. Fathers' labor supply also appears to be more elastic, even if it is not quite as responsive as that of mothers. Finally, estimates of δ are no longer significant. We have confirmed that these differences across the two periods reflect the influence of the months that preceded the 2020-21 school year (January – May 2020) and *not* the months that followed (September – December 2021).

[15] Appendix C.1 shows that the estimated changes in hours worked reflects an extensive margin adjustment for mothers and an intensive margin adjustment for fathers.

The parameter instability evident in Table 2 may reflect model mis-specification. One concern about equation (1) is that it omits controls for broader trends in parents' relative labor supply. For instance, if parents' jobs were generally less exposed to the initial turbulence of the pandemic, it would look as if their labor supply is somewhat insensitive to shifts in school policy that coincided with pandemic-related disruptions.[16] A corollary is that initial area-wide reactions to these disruptions will be reflected in a significant response to (correlated) changes in in-person shares. Thus, the absence of controls for such trends may lead to different estimates of ψ across different periods.

Table 3: Estimates of Equation (2)

	Women: All of 2020-2021			
In-person × kids, ψ	2.359***	2.501***	-0.051	0.096
	[0.634]	[0.654]	[0.672]	[0.751]
Number of obs.	447,899	447,277	447,899	447,277
	Women: 2020-21 School Year			
In-person × kids, ψ	2.458***	2.568***	-0.131	-0.440
	[0.633]	[0.668]	[1.127]	[1.248]
Number of obs.	228,550	228,225	228,550	228,225
	Men: All of 2020-21			
In-person × kids, ψ	1.886***	1.708***	-0.051	-0.239
	[0.645]	[0.602]	[0.705]	[0.812]
Number of obs.	432,856	428,244	432,856	428,244
	Men: 2020-21 School Year			
In-person × kids, ψ	1.778***	1.596***	-1.695	-1.129
	[0.629]	[0.590]	[1.191]	[1.344]
Number of obs.	221,080	218,575	221,080	218,575
CPS Weights	No	Yes	No	Yes
Month × parent F.E.	Yes	Yes	Yes	Yes
Area × parent F.E.	No	No	Yes	Yes

Note: Each column within each panel is a separate regression. The dependent variable is the number of hours worked per week. Standard errors are clustered at the geographic area level. "All 20-21" pools data for all of 2020 and 2021 exclusive of June, July, and August. "School 20-21" refers to the period September 2020 to May 2021. *** indicates a p-value less than 0.01; ** a p-value between 0.01 and 0.05; and * a p-value between 0.05 and 0.10.

[16] Lofton et al. (2021) document that, in the first few months of the pandemic, fathers experienced the smallest decline in employment and employed mothers experienced the smallest decline in weekly hours worked.

In view of this concern, we re-run the regression with additional controls for parent-specific trends in labor supply. Formally, these trends are modeled as parental status-by-month fixed effects ($\theta_t \mathbb{k}_{it}$ in equation (2)). The first two columns of Table 3 report the results. (With parent-by-month effects, estimates of δ are now insignificant in the full sample and are omitted here.) Under this specification, the adjustment of parents' hours to in-person instruction is now remarkably stable across time. Among mothers, a shift from fully virtual to fully in-person instruction yields an increase in weekly hours of around 2.4 to 2.6—regardless of the sample period. The response among fathers is somewhat smaller—weekly hours increase by around 1.6 to 1.9, depending on the weighting—but again, is virtually unchanged across sample periods. Thus, as anticipated, the parameter instability in Table 2 reflects the failure to control for broader trends in parental labor supply. With the addition of these controls, the results for all periods are comparable to the results for the 2020-21 school year in Table 2. [17]

Just as there may be parent-specific trends in hours worked, there may be parent-specific factors behind average hours in a given area. These factors drive a wedge between the mean hours of parents and childless adults within an area and may vary across areas. Such spatial differences pose a threat to identification if they are correlated with (average) 2020-21 in-person instruction rates. The reasons for such a correlation are perhaps not immediate (we return to this shortly), but it is easy all the same to add controls for spatial heterogeneity. As previewed in Section 2, these controls take the form of parental status-by-area fixed effects ($\zeta_a \mathbb{k}_{it}$ in equation (2)).

The impact of these controls, shown in the final two columns of Table 3, is considerable: the response of parental labor supply to a change in the in-person share vanishes entirely. These results indicate that, once aggregate time trends are controlled for, the coefficient ψ is identified

[17] Online Appendix C.1 confirms that, in regressions with parent-by-month effects, the extensive margin continues to play an outsized role in women's labor supply response but matters little for men.

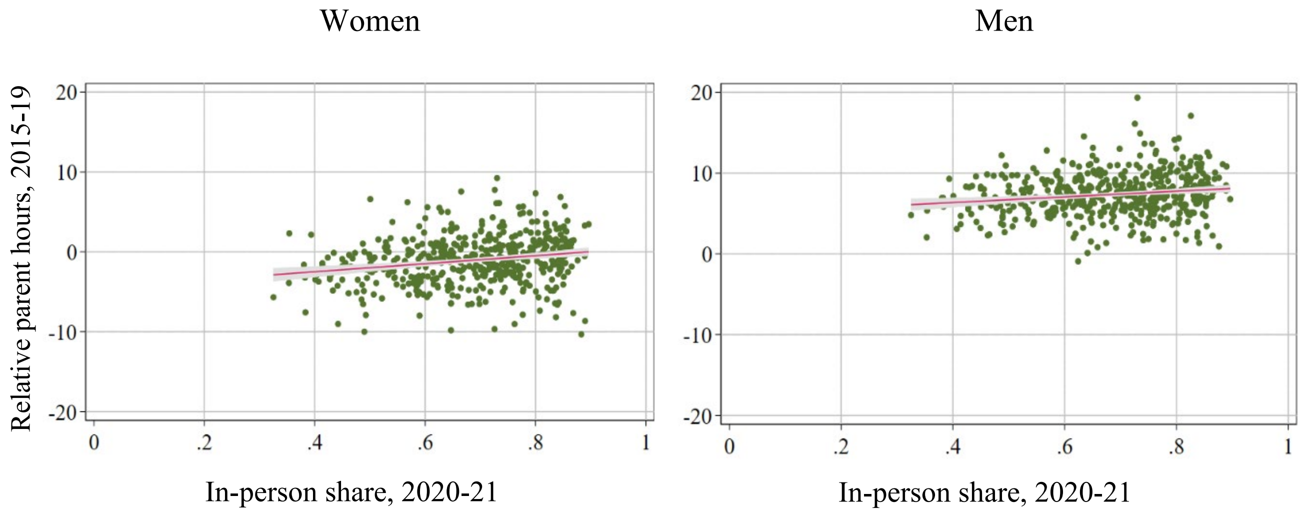
principally off cross-area comparisons of parents' relative hours worked. With additional controls for average regional differences in labor supply, estimated effects of the in-person share disappear.

Online Appendix C.7 shows that the introduction of individual fixed effects has a similar impact as the parent-by-area regressors. Intuitively, each set of controls isolates variation in in-person shares over time within a fixed unit (either a person or area). This variation alone does not identify a statistically significant effect of in-person instruction.

One could question, though, if we have "over-controlled" for unobserved heterogeneity. Even if regional differences in average school policy were exogenous, the addition of the parental status-by-area terms alone could capture much of this variation. To assess the need for these controls, consider a simple placebo test. Suppose in-person shares in 2020-21 are correlated with long-run regional differences in relative parental hours. It follows that average policies in the pandemic should predict *pre*-pandemic labor supply.

In fact, this "pre-trend"—the correlation between the pandemic-era instruction format and pre-pandemic hours—is evident in the raw data. Figure 2 illustrates this point. The x-axis shows the average in-person share in each of our local labor market areas over 2020-21. The y-axis is based on pre-pandemic hours data from the CPS. Specifically, it shows the local-area average of parents' hours less average hours of childless adults over the five years before the pandemic, 2015-19. The left panel reports results for mothers, and the right panel pertains to fathers. Remarkably, parents' relative labor supply in the pre-pandemic period appears to be several hours higher in areas where instruction was largely in-person in 2020-21 than in areas where it was largely remote. To pursue this point further, we apply equation (1) to test if in-person shares in 2020-21 predict pre-pandemic hours. The sample is drawn from the CPS and consists of adults ages 21-59 in the years 2015-19. All individual-level control variables described above are included. The schooling mode, which was formerly measured by monthly data on in-person shares in 2020-21 (p_{at}), is now

Figure 2: Pandemic School Formats and Pre-Pandemic Hours Worked



Note: This figure plots (on the y-axis) the difference in average pre-pandemic weekly hours between parents and childless adults against (on the x-axis) the average in-person share in the pandemic period. Each marker is a local labor market area. The left panel is based on hours data among women ages 21-59; the right panel refers to men in the same age range. The pre-pandemic period spans 2015-19, whereas the pandemic period covers 2020-21. In each period, the summer months (June-August) are excluded. The line of best fit in the left panel (among women) has slope 5.024 (s.e. of 1.106), and the line of best fit in the right panel (among men) has slope 3.504 (s.e. of 0.976). To mitigate sampling error, we drop the seven areas with fewer than 50 mothers or fewer than 50 childless women (left panel) and the nine areas with fewer than 50 fathers or fewer than 50 childless men (right panel).

the area-level *mean* of the latter and denoted by p_a. The regressor of interest is the interaction term, $p_a \mathbb{k}_{it}$. (We do not include p_a as a stand-alone regressor, since it is absorbed by area fixed effects.) A significant coefficient on the interaction means that average on-site shares in 2020-21 pick up general regional differences in parents' relative hours in 2015-19.[18]

The regression analysis corroborates that the pandemic-era schooling mode is strongly related to pre-pandemic *maternal* hours but uncovers a weaker connection to paternal labor supply. These estimates are detailed in Online Appendix B and summarized here. The results are most striking when using an area-level mean p_a based on all of 2020-21 (excluding summer months). In areas that selected full-time in-person instruction, mothers' relative labor input *prior* to the pandemic is estimated to be roughly 3.4 weekly hours higher than in areas with full-time virtual

[18] Results are virtually unaffected if we insert parental status-by-month effects as in equation (2).

instruction. Among fathers, in-person instruction implied around one more hour of work per week, although the latter is not statistically significant. Notably, these figures are comparable to—or even exceed, in the case of mothers—estimates of hours responses in the pandemic period (see Table 3). Alternatively, if we compute mean in-person shares based on 2020-21 school year data, the estimate for mothers falls to about two hours per week but remains strongly significant. The analogue for men lies between 0.6 and 0.8 hours per week but is, again, not significant. In the Appendix, we find the same pattern of results with in-person share measures other than SafeGraph.

To reflect on these results, it is helpful to first consider what, in general, may shape spatial dispersion in (pre-pandemic) parents' labor supply. Market work entails at least two costs that bear especially on parental labor supply and likely vary in the cross section. (Each of these factors is present in the model in Section 5.) The first is the cost of school-age childcare. The second is commute time to work, which reduces, all else equal, time spent with children.

Online Appendix B shows that commute times and school-age childcare costs are correlated with (pandemic-era) in-person shares. This connection runs, in part, through their association with local partisan affiliation. As we noted, in-person shares were highest in areas that heavily supported Donald Trump. At the same time, commutes are longer in metro areas where Trump's vote share was low. Higher childcare costs in anti-Trump areas may partly reflect the burden of higher minimum staff-to-child ratios, suggesting a greater propensity to regulate.

In addition, the Appendix reports on the connection between commute length and childcare costs, on the one hand, and parental labor supply on the other. A statistically significant correlation suggests that any *other* outcome related to commute times and childcare prices, such as the in-person share, will emerge as an apparent contributor to parents' labor supply. We find that longer commutes and higher childcare prices are indeed associated with lower maternal hours worked. By contrast, paternal hours are less sensitive to local childcare costs and essentially uncorrelated with commute times. In qualitative terms, these results echo more careful, causal analyses (see Black et al. (2014) on commute times and Mumford et al. (2020) on childcare prices).[19]

Taking stock of our findings, we conclude with the following observations. The results of the placebo test demonstrate that equation (1) fails to address the endogeneity of schooling mode. As a result, equation (1) likely yields an upwardly biased estimate of its effect. However, the source of this endogeneity is not fully resolved. Among mothers, the connection between hours worked, commute times, childcare prices, and in-person shares suggests that schooling mode stands in for more fundamental forces in the local area. This narrative does not apply neatly to fathers, though.

3.2 Education and marital status

In line with related research, we next ask if parental labor supply responses to virtual instruction differed by marital status and/or educational attainment. The analysis will focus on the response of total weekly hours. Online Appendix C.1 reviews results for employment. The regression model retains parental status-by-time effects but excludes parental status-by-area effects. We confirm that the inclusion of the latter eliminates the statistical significance of the estimates, just as they do in Section 3.1. One might then view the results below as the strongest case that one could present for a role of schooling mode in parental labor supply.

Education. We first divide our sample into a noncollege group—workers with less than a four-year degree—and workers who completed college. We then further split each of these two groups by gender. Results are reported in Table 4.

Consider first the estimates for women in the top panel of the table. Among the noncollege educated, a shift from fully virtual to fully in-person implies an increase in weekly hours of just over two. The response among college graduates is only slightly smaller; the two responses are

[19] For a review of research on childcare prices and maternal labor supply, see Blau and Currie (2006). The divergence between paternal and maternal labor supply responses is a feature of the lifecycle model in Guner et al. (2020).

Table 4: Estimates by Educational Background

	Noncollege		College	
	Women			
In-person × kids, ψ	2.074***	2.374***	1.818*	1.851*
	[0.771]	[0.849]	[1.001]	[1.082]
Number of obs.	266,258	265,968	181,641	181,309
	Men			
In-person × kids, ψ	1.999***	1.790***	1.078	1.152
	[0.751]	[0.666]	[0.863]	[0.973]
Number of obs.	284,723	281,867	148,133	146,377
CPS Weights	No	Yes	No	Yes

Note: Each column within each panel is a separate regression, and the column header reports the regression sample (i.e., "noncollege women"). The period is all of 2020-21 but with summer months excluded. A college (noncollege) graduate is one who did (not) complete a four-year degree. Standard errors are clustered at the geographic area level. *** indicates a p-value less than 0.01; ** a p-value between 0.01 and 0.05; and * a p-value between 0.05 and 0.10.

not statistically distinguishable from one another. Thus, among women, college experience is not a strong predictor of the labor supply response to the in-person share.

The education gradient among men is somewhat more evident. The college educated do not significantly adjust their hours in response to variation in the in-person share. By contrast, the response of noncollege men is similar to that among (noncollege and college-educated) women. A corollary of these results is that male and female labor supply *within* the college group diverged. This point is sharpened if we consider households with two college-educated spouses, as shown in Online Appendix C.8. Mothers in these households raise labor supply by up to one hour more than shown in Table 4, whereas fathers' behavior is in line with the college group as a whole. This imbalance between spouses is evident only in college-educated couples. In households with noncollege-educated parents, spouses' hours responses are almost identical.[20]

[20] These patterns do not seem to reflect intra-household differences in earnings opportunities: a college-educated father is *no* more likely than a noncollege graduate to have higher earnings than his spouse. See Online Appendix C.8.

Table 5: Estimates by Marital Status

	Married		Unmarried		Lone adults	
	Women					
In-person × kids, ψ	2.256***	2.902***	2.591**	2.047**	3.995***	2.973**
	[0.788]	[0.836]	[1.032]	[0.948]	[1.296]	[1.324]
Number of obs.	242,743	242,351	205,156	204,926	60,291	60,282
	Men					
In-person × kids, ψ	1.824***	2.256***	1.657	1.254	4.072*	3.033
	[0.661]	[0.709]	[1.455]	[1.540]	[2.266]	[2.498]
Number of obs.	223,471	219,663	209,385	208,581	55,284	55,275
CPS Weights	No	Yes	No	Yes	No	Yes

Note: Each column within each panel is a separate regression, and the column header reports the regression sample (i.e., "married women"). The period is all of 2020-21 but with summer months excluded. A "lone adult" is a respondent who does not live with any other individual age 18 or over. *** indicates a p-value less than 0.01; ** a p-value between 0.01 and 0.05; and * a p-value between 0.05 and 0.10.

Marital status. We next split the sample by marital status. In addition, within the unmarried, we look at households where the parent is the lone adult. The labor supply response of a single parent is likely to depend on the household's composition. For instance, a parent in a coresidential arrangement with other adults may receive steadier childcare support than a lone-adult parent. This consideration is empirically relevant: almost 60 percent of unmarried mothers live with at least one other adult, which includes unmarried partners, parents, and older children.

Our estimates in Table 5 confirm that household composition mediates the role of marital status. The response of hours worked among all unmarried mothers ranges from 2.0 to 2.6, which is not too different from that of the married sample. However, this estimate masks the difference between mothers with and without other adults in the household. Among lone-adult mothers, hours worked are more responsive: a shift from a virtual to in-person format implies an increase of 3.0 to 4.0 weekly hours. By contrast, the response of unmarried women in co-residential arrangements (not shown) is 1.5 hours and statistically insignificant. The narrative for men is broadly similar although the estimates are less precise (in part because few unmarried men live with their children).

It is instructive to compare results in Table 5 with other research in this area. Garcia and Cowan (2024) adopt a specification very much like equation (1). Our estimates are comparable to, or higher than, theirs save for unmarried men (for whom our ψ is one hour lower). One distinguishing feature of our specification is the use of parental status-by-month effects, which tends to elevate estimates of ψ in the full sample 2020-21. Other differences between our specifications offset one another to some degree. Specifically, the presence of older respondents in Garcia and Cowan's sample elevates their estimates (see Online Appendix C.4), but their restriction to CPS counties and inclusion of industry and occupation controls reduces them (see Online Appendices C.5 and C.6).[21] Hansen et al. (2024) also consider a specification akin to equation (1) but show that their results are robust to event-study methods that abstract from the spatial variation that underlies our placebo test. They uncover a statistically significant effect for married mothers but not for unmarried mothers or married fathers.[22] Thus, we generally find larger estimates of the labor supply response and yet we will argue in Section 5 that even our results are unexpectedly *small*.

4. Estimates from Time Use Data

Our analysis of CPS data suggests that a shift from a virtual to an in-person format was associated with an increase of no more than two to four weekly hours of work. The suspension of on-site instruction, however, removed over 30 hours of school-provided supervision. Thus, the labor supply response suggests that parents must have adjusted to school closures on other margins.

To examine time use adjustments more broadly, we turn to the American Time Use Survey (ATUS) (Flood et al., 2023). Our ATUS sample is selected to conform to the extent possible with

[21] A more complete mapping from Gracia and Cowan's results to our own, including the effects of each of these specification choices, was included in an earlier version of this paper and is available upon request.

[22] The point estimates in Hansen et al. (2024) are not quite comparable to ours because the authors develop their own in-person shares based on SafeGraph. We and Garcia and Cowan use those from Parolin and Lee (2021). In our data, our placebo test fails for women in all demographic groups.

our treatment of the CPS. Therefore, we again restrict attention to individuals ages 21-59 who are childless adults or parents of school-age children. The sample period covers 2020-21 but for the period mid-March to mid-May 2020 during which field work was suspended due to the pandemic.

For each respondent, we observe a minute-by-minute diary of a single day that describes how, where, and with whom they spent their time. However, the days of the week are not uniformly represented: Half come from Saturday or Sunday. We implement a simple reweighting that mimics a uniform sample over days of the week.[23] Alternatively, the oversample of weekend days can be corrected by use of ATUS sample weights. We present results based on both weighting schemes.

Our analysis addresses time allocation across several dimensions. Each respondent's diary entry is assigned a detailed activity code, and we group activities into a few broad categories: work, leisure, home production, childcare, commute time, and sleep. We then estimate how hours spent in each category respond to variation in instruction format. As in Section 3.2, the specification follows equation (1) but with parental status-by-time effects. In addition, we include a fixed effect for each day of the week. Finally, since the data are daily, the point estimates are scaled to express them on a weekly basis and, therefore, comparable to estimates from the CPS.

Remarkably, the reinstatement of in-person instruction has, on the whole, no significant impact on any major category of time use, from work to leisure and home production. These results, which are reported in Online Appendix D, hold for the full sample and when we split the data by college attainment. Given the modest size of our sample, what we take from this exercise is that, whatever are the "true" effects of schooling mode on time allocation, they are not large enough to detect in the ATUS.[24]

[23] To illustrate, if Saturday represents 1/4 of the sample, we apply a weight of 4/7 to all Saturday observations.
[24] The discrepancy between CPS and ATUS results on hours worked is unlikely to reflect systematically different measurements. Research has found substantial agreement between the two sources (Frazis and Stewart, 2004, 2014).

However, there is a sense in which these regressions do not leverage the richness of the ATUS. In addition to the activities undertaken, the ATUS sheds light on how an activity was performed. For instance, while *total* market time might be unresponsive to the closure of in-person instruction, a greater *share* of it may overlap with childcare. Fortunately, the ATUS asks if there was a child in the respondent's care, even if the respondent was engaged in another activity. (We refer to this childcare time as a secondary activity.)[25] Thus, we can observe if parents supervise school-age children while they work at home.

Table 6 reports on the role of working from home as a means of supplying both childcare and market time. To start, the first two columns reiterate that total working hours in the ATUS are insensitive to instruction format. The next two columns report results for *total* hours working at home. Interestingly, this, too, does not respond significantly. However, the fifth and sixth columns show that time spent working at home (as the primary activity) while caring for children (as the secondary activity) is responsive to instruction format, but only among college graduates. After a shift from fully virtual to in-person instruction, college-educated parents reduced time in this activity by 6-7 hours per week. Thus, college graduates continued teleworking after in-person instruction resumed but no longer supervised children while doing so. Online Appendix D shows that this result stems to a large extent from college educated mothers, but standard errors in these subsamples are rather large (which is why we pool men and women in Table 6). The response of the noncollege group is smaller and statistically insignificant, consistent with evidence that this group had fewer telework opportunities (Mongey et al., 2020).

[25] Note that childcare is the only activity that can be recorded as a secondary activity. The ATUS does not ask survey respondents for secondary activities outside of childcare.

In the final columns of Table 6, we report how school closures alter the *total* time spent with children, which includes both primary and secondary childcare.[26] Overall, local school closures led to an increase of 14 to 19 hours per week with one's children, with much of the latter due to the response of the college-educated. This estimate captures adjustments on the work-from-home margin (columns five and six) as well as variation in the extent to which childcare overlapped with *non*-market activities (e.g., leisure and home production).

These results strongly suggest that college educated parents relied, in part, on telework to sustain their hours worked when instruction was virtual. Nevertheless, we would not necessarily infer from Table 6 that the labor supply of the college educated would have fallen 6 weekly hours *but for* telework. The reason is that the noncollege educated in the ATUS also smoothed hours worked but did not rely on telework. Thus, in the absence of telework, the college group would have presumably taken up, at least to some degree, measures adopted by the noncollege group to cope with shifts in instruction format.[27]

We next turn to one of these other possible margins of adjustment: the utilization of nonparental childcare. A survey fielded in late 2020 by Calarco et al. (2021), and analyzed further in Yang et al. (2025), reports specifically on the use of non-center-based, or informal, care, which includes unpaid care by family and friends as well as in-home paid care (e.g., nannies). Sixty percent of surveyed families reported using informal care, which included help supervising children learning at home in fall 2020. By excluding spring 2020, though, the survey likely does miss the disruptions faced by many caregiving arrangements at that time. Even during those initial

[26] These results are not strictly comparable to several others in the table. The reason is that the measured outcomes such as "work" and "work at home" do not capture the time spent in those tasks as secondary activities.
[27] To recover causal effects more credibly, one could try to exploit plausibly exogenous variation in workers' access to telework. However, measures of access are based on occupations (Dingel and Neiman, 2020) and are not easily mapped to nonemployed survey respondents.

Table 6: Work at Home, Childcare, and Instruction Format

	Work		Work at Home		Work at Home + Childcare as Secondary Activity		Childcare, Primary or Secondary Activity	
				All				
In-person × kids, ψ	-0.519	-5.182	-3.124	-2.852	-5.937***	-4.796***	-18.912***	-14.003**
	[4.091]	[5.223]	[3.961]	[4.756]	[1.466]	[1.626]	[4.903]	[5.582]
				Non-College				
In-person × kids, ψ	-2.791	-7.840	1.528	1.192	-1.521	-1.009	-9.683	-2.211
	[7.040]	[7.921]	[4.181]	[4.741]	[2.154]	[2.024]	[8.374]	[9.237]
				College				
In-person × kids, ψ	1.736	0.467	-3.058	-3.901	-7.328***	-6.432**	-25.149***	-25.572***
	[5.867]	[6.024]	[6.892]	[7.875]	[2.620]	[2.801]	[5.053]	[5.265]
ATUS Weights	No	Yes	No	Yes	No	Yes	No	Yes

Note: Each column within each panel is a separate regression. The dependent variable is the implied number of hours per week spent in each activity. The panel title reports the regression sample. There are 6,622 observations in the first panel, 3,371 observations in the second panel, and 3,178 observations in the third panel. Relative to equation (1), we also include fixed effects for days of the week as well as parental status×month controls. Standard errors are clustered at the geographic area level. "Work at home" is the number of work hours carried out in one's own home or another home. "Work at home + childcare" measures the number of hours where "work at home" is the primary activity and "childcare" is the secondary activity. *** indicates a p-value less than 0.01; ** a p-value between 0.01 and 0.05; and * a p-value between 0.05 and 0.10.

Table 7: Time with Others' Children and Local School Formats

	All		Men		Women	
			All			
In-person share, δ	-1.832	-1.753	-2.849	-0.477	-1.597	-2.961
	[1.690]	[2.082]	[1.897]	[2.695]	[2.348]	[2.535]
Number of obs.	4,848	4,848	1,983	1,983	2,787	2,787
			Non-College			
In-person share, δ	-2.177	-4.261*	-0.083	-0.401	-3.249	-6.445**
	[2.157]	[2.287]	[2.837]	[2.626]	[2.790]	[3.105]
Number of obs.	2,945	2,945	1,106	1,106	1,725	1,725
			College			
In-person share, δ	-1.533	1.029	-6.476	-0.681	-0.041	1.957
	[3.019]	[3.734]	[5.448]	[6.511]	[4.396]	[4.491]
Number of obs.	1,817	1,817	765	765	952	952
ATUS Weights	No	Yes	No	Yes	No	Yes

Note: Each column within each panel a separate regression. The dependent variable is the implied number of hours per week spent with other's children. Time spent with other's children includes all time spent with persons under 18 years old outside of market work. The sample includes individuals who are 60 years or older. Standard errors are clustered at the geographic area level. *** indicates a p-value less than 0.01; ** a p-value between 0.01 and 0.05; and * a p-value between 0.05 and 0.10.

months of the pandemic, though, caregiving hours appear to have risen in households where older family members resided (Truskinovsky et al., 2022).

The ATUS also allows us to examine a role for nonparental care, albeit in a more limited form. For each adult aged 60 years or older, we calculate the number of hours spent with children under age 18 who are *not* the respondent's son or daughter. This estimate excludes time spent at work in order to identify unpaid, informal care of the sort that a grandparent or other older relative might provide.

Table 7 reports how these hours of care vary with the in-person share of instruction. Note that since the sample consists of only potential nonparental caregivers, the covariate of interest now is just the in-person share rather than the interaction of the latter with parental status. The identification assumption in this context is that schooling mode did not systematically vary with older respondents' preferences or opportunities for caregiving. Estimates from the ATUS suggest that older respondents' caregiving was responsive to the in-person share. In the full sample, the resumption of in-person instruction implies a reduction of nearly 1.8 hours per week in the time older respondents spend with children, though this estimate is not statistically significant. We obtain larger estimates if we consider those without a college degree: their weekly hours of caregiving fall by up to 4.3 when on-site instruction returns. The response among noncollege women appears to be even larger. One way to interpret these results is to view the grandparent's college experience as a proxy for that of the parent, which suggests that noncollege households relied more on nonparental care.[28] This interpretation is consistent with Kwon (2024), who finds higher parental hours in CPS households where grandparents were present. Moreover, her estimates are largest for households with lesser educated parents.

5. Discussion

We now use a series of time allocation models to guide a discussion of our regression results. We first consider a very simple set-up where a single parent faces a one-for-one tradeoff (in time) between labor supply and childcare. Under a reasonable parameterization, the model implies labor supply responses that far exceed any reported estimate. We then illustrate how telework can relax the work-childcare tradeoff and, therefore, mute the response of hours worked. At the same time, hours worked responses were modest even for the noncollege educated, who were less likely to access telework. This observation leads us to also consider a role for nonparental care, which enables parents to smooth their labor supply and ensure the provision of childcare.

A simple baseline. A single parent maximizes utility over consumption, leisure, and child development subject to two constraints on her time. The first constraint is that the allocation

[28] On the intergenerational correlation of educational attainment, see Kane (1994) and Cameron and Heckman (2001).

of her time across child supervision, leisure activity, and market work must add up to the total time endowment (normalized to 1). The second constraint is that the child is supervised at all times.

To start, we assume there are only two forms of child supervision. There is a publicly provided form of supervision, which the parent takes as given. The notion of publicly provided supervision is a crude description of in-class time, but it arguably captures the dimension of in-person instruction that is most relevant to parental labor supply. We assume that a child who is not in school must be under the parent's supervision. We introduce private nonparental care below.

Formally, the time constraints are as follows. Leisure is denoted by l; time allocated to child supervision by m; and market hours of work by n. Finally, we let g be time spent under publicly provided supervision. The time constraints specify that a parent's allocations add up to 1,

$$l + m + n = 1, \tag{3}$$

and that the child must be under school or parental supervision,

$$g + m = 1. \tag{4}$$

Together, equations (3) and (4) imply $l = g - n$: a decrease in on-site instruction time, g, lowers leisure one for one unless market hours fall.

We assume the parent takes g as given. This rules out substitution from an institution with only virtual instruction to one that is in person. Where this did occur in practice, it appears to have involved a switch from public to private school.[29] For the typical parent, though, the cost of such a switch was likely prohibitive. Therefore, we focus here on other margins of adjustment.

We assume that period utility is given by

$$u(c, l, q) = \alpha \ln c + \beta \ln l + (1 - \alpha - \beta) \ln q, \tag{6}$$

[29] However, much of the 3 percent decline in public school enrollment in Fall 2020 reflected increased homeschooling (Musaddiq et al., 2022; Bacher-Hicks et al., 2024). Our model interprets this as more time under parental care.

where $\alpha, \beta \in (0,1)$. The Cobb-Douglas specification follows Berlinski et al. (2024) and is the form of period utility often used in models of home production. In our context, period utility depends on market consumption, leisure, and a term, q, that indexes child development and is "produced" with both forms of supervision, g and m. Since $m = 1 - g$, though, $q(g, m) = q(g, 1 - g)$: q is pinned down by g, which is taken as given. A more substantive choice problem for q will emerge when we introduce another source of supervision: a form of private nonparental care. Nevertheless, the level of g still shapes labor supply, n, via the time constraint (5).

Time allocations are divisible and, therefore, the model will yield only interior solutions. In practice, though, the hours responses of parents also reflect movements on the extensive margin. In our view, what we sacrifice in realism is worth the insight that it affords. The comparative statics with respect to local changes in g can help reveal fundamental economic forces at play (even if observed shifts in market and on-site time tend to be "lumpy").[30]

Initial comparative statics. The first-order condition for leisure implies

$$l = g - n = \frac{\beta}{\lambda w}, \tag{7}$$

where λ is the marginal utility of consumption and w is the wage. Suppose for now that households can insure consumption to the extent that λ is invariant to g. It follows from equation (7) that market hours move one-for-one with on-site time. Intuitively, the demand for leisure does not change since its price is pinned down by λ and w. Therefore, n must fully offset a shift in g.

The assumption of perfect insurance is of course somewhat stylized, although the surge of government transfers likely did enable households to smooth consumption to a considerable extent

[30] Key features of the model, such as the curvature over leisure, are also likely to bear on the extensive margin. For instance, suppose a worker chooses $n = 0$ or $n = N > 0$ and derives utility $\beta v(g - n)$ where v is concave and β is heterogeneous. The employment *rate* varies inversely with the value of foregone leisure, $v(g) - v(g - N)$ (Mulligan, 2001). A lower g raises this value, and reduces labor supply, to an extent that depends on the curvature of v.

(Wu et al., 2022). Nevertheless, as an alternative, suppose parents live “hand to mouth.” Therefore, consumption must satisfy $c = wn$. It follows that $\lambda = \alpha/c = \alpha/wn$, and equation (7) becomes

$$l = g - n = \frac{\beta}{\alpha} n. \tag{8}$$

A perturbation to g yields a change in hours work equal to

$$\frac{\mathrm{d}n}{\mathrm{d}g} = \frac{1}{1 + \beta/\alpha}. \tag{9}$$

Equation (8) says that β/α is identified by the ratio of leisure to market work time, which can be calculated from data in the American Time Use Survey. We report two figures that bridge different approaches to the measurement of leisure (see Aguiar et al., 2012). First, if all sleep is excluded from leisure, we find that $\beta/\alpha = 1.1$, which implies that an hour more of in-person instruction yields approximately 0.5 more hours of market work. Alternatively, we treat sleep time beyond 6 hours as leisure. This approach elevates β/α and yields $\mathrm{d}n/\mathrm{d}g \approx 1/3$.

This prediction (far) exceeds estimates reported here or elsewhere in the literature. With the reinstatement of 33 hours of on-site instruction (U.S. Department of Education, 2008), equation (9) predicts that a shift from virtual to in-person will lift labor supply by 16 hours per week. By contrast, our OLS estimates suggest a labor supply response between 2-4 hours per week, i.e., at most $\mathrm{d}n/\mathrm{d}g \approx 0.1$ In this sense, our regression estimates are unexpectedly small.[31]

Telework. A key assumption embedded in equation (3) is that parents cannot simultaneously perform market work while they supervise children. However, ATUS data suggest that telework enabled (at least college-educated) parents to provide some childcare even as they continued to work. We illustrate a tractable way to capture this notion of telework in the model.

[31] Alternatively, if some parents will not work in any state of the world, the average “treatment” amounts to an increase in on-site time less than 33 hours. Suppose we discount 33 by 25%, which matches the mean nonemployment rate in Table 1. Still, given a labor supply response of around 3 hours, $\mathrm{d}n/\mathrm{d}g = 3/\left(33 \times (1 - 0.25)\right) = 0.12$.

The new ingredient is a time aggregator function. The idea behind this function is that a parent may supply 8 hours of market work and 2 hours of childcare in under 10 hours. That is, the two activities may, to some degree, be done concurrently. Formally, the time aggregator function maps time engaged in market work, n, and time engaged in childcare, m, into the *total* time that has passed while engaged in one or both activities. The function has the form,

$$t(m,n) = (m^{\rho} + n^{\rho})^{1/\rho}, \tag{10}$$

where $\rho \geq 1$. The time constraint (3) then generalizes to $l + t(m,n) = 1$. Leisure, l, is defined as the absence of any other activity and, therefore, enters the time constraint separably (outside of t). One might also want to allow leisure and childcare time to overlap, consistent with estimates in Table 6. We leave this for future research and focus here on the role of telework.

Equation (10) encompasses two polar cases. The first is $\rho = 1$, which recovers the original time constraint (3), $l + m + n = 1$. This case corresponds to the standard assumption that two activities are perfectly rivalrous—an hour of market work is done to the exclusion of an hour of childcare. The second is the limit where $\rho \to +\infty$, which implies that $t(m,n) \to \max\{m,n\}$. In this case, the two activities are perfectly *non*rivalrous. To illustrate, if $m > n$, an increase in market work can be completed within the time already allocated to childcare. More generally, the activities can be performed concurrently up to (of course) the minimum of the two.

These two polar cases are bridged by a continuum of finite $\rho > 1$. In this interior region, a few properties of equation (10) will be important. First, equation (10) implies $t_m \equiv \partial t/\partial m \in (0,1)$ and, similarly for market work, $t_n \equiv \partial t/\partial n \in (0,1)$. In words, another hour of any activity absorbs less than an hour of new time, because some portion of it is done concurrently with the other activity.[32] Therefore, we say the *time price* of an activity is less than one. Second, the time price of an activity increases in the time allocated to it (i.e., $\mathcal{t}$ is convex) and decreases in the time allocated to the other activity (i.e., $\partial^2 \mathcal{t}/\partial n \partial m = \partial^2 \mathcal{t}/\partial m \partial n < 0$). The intuition is that, if m is large relative to n, a parent can identify many childcare tasks that can be done concurrently with more market work but few work tasks that can be done jointly with more childcare. Therefore, the time price of another hour of work is small, but the price of another hour of care is high.

These properties formalize the sense in which equation (10) yields a motive to "multi-task." Since the time price of market work falls as childcare time rises, the parent has a strong incentive to elevate hours worked, too. This motive to multi-task is strengthened at higher values of ρ. To see this point, note that the time price of another hour of market work relative to childcare is given by $\mathcal{t}_n/\mathcal{t}_m = (m/n)^{-(\rho-1)}$. Thus, at higher values of ρ, a one percent increase in childcare time (all else equal) yields a steeper decline in the relative price of market work.

Consider now the choice of labor supply, n. The first-order condition is

$$l = 1 - \mathcal{t}(m, n) = \frac{\beta}{\lambda w} \cdot \frac{\partial \mathcal{t}}{\partial n}. \tag{11}$$

A decline in on-site time, g, now has two effects. The first is familiar: since parental time must rise, leisure would fall all else equal. To stem the decline in leisure, labor supply is reduced.[33] The second effect is novel: an increase in m also reduces the time price of market work, $\partial \mathcal{t}/\partial n$. This stimulates *more* labor input, mitigating the decline in labor supply due to the former effect.

More formally, under perfect insurance ($\mathrm{d}\lambda = 0$), the comparative static is,

[32] In the limit $\rho \to +\infty$, these derivatives are zero or one. Intuitively, if $m > n$, any market work can be done with current childcare, which implies $\mathcal{t}_n = 0$. Conversely, if m rises, there is no scope to multi-task further, to complete a new childcare task jointly with current market work. Therefore, $\mathcal{t}_m = 1$.

[33] The *extent* to which it is reduced will depend on the shape of $\mathcal{t}$. Thus, even the quantitative effect of this familiar mechanism is different under $\rho > 1$.

$$\frac{\mathrm{d}n}{\mathrm{d}g} = \frac{1-(\rho-1)/\phi(l)}{(n/m)^{\rho}+(\rho-1)/\phi(l)} \cdot \frac{n}{m}, \tag{12}$$

where $\phi(l) \equiv (1-l)/l$ and $m = 1 - g$. When $\rho = 1$, equation (12) collapses to $\mathrm{d}n/\mathrm{d}g = 1$: market work is reduced one for one with a fall in g. Values of $\rho > 1$ can attenuate the decline in labor supply. In fact, there is a unique value of ρ, given by $\rho = 1 + \phi(l)$, that induces *no* change in market time. The term $\phi(l)$ captures the degree of curvature over l in the utility function: if $\phi(l)$ is large, (log) marginal utility of leisure rises steeply with any reduction in l, which prompts the parent to reduce market hours more substantially. For $\mathrm{d}n = 0$, the motive to multi-task, as parameterized by ρ, must be strong enough to match the force of this curvature.

To illustrate the implications of this result, consider the college educated, who relied on telework to sustain labor supply. From the ATUS, leisure for this group constitutes 38 percent of total time allocated to market work, childcare, and leisure.[34] Therefore, the observation $\mathrm{d}n \approx 0$ requires $\rho \approx 2.63$. More generally, we can identify conditions such that $\mathrm{d}n/\mathrm{d}g$ decreases in ρ, which provides a means to match an array of market hours outcomes. See Online Appendix E for a complete characterization.

Nonparental care. Thus far, we have assumed that a child must be supervised by her school or parent. However, changes in labor supply—and along other dimensions of time use—are relatively modest even among workers with little access to telework (i.e., the noncollege educated). One explanation for this is that parents turned to private nonparental care. Note that to zero in on this issue, we will abstract from telework in what follows. Online Appendix E shows that our main insights can be derived in a model that integrates both margins of adjustment.

[34] This is the notion of l within the model. Therefore, we abstract from other margins of time use for this calculation.

The introduction of nonparental care implies a simple, but potentially substantive, change in labor supply. If we denote time in private nonparental care by x, the analogue to equation (4) is

$$g + m + x = 1, \tag{13}$$

which says that a child is supervised by a school, parent, or private third party. The first-order condition for hours worked extends equation (7) to incorporate nonparental care,

$$n = g + x - \frac{\beta}{\lambda w}. \tag{14}$$

Market work now moves one for one with the *sum* of time outside of parental care, $g + x$. Therefore, if private nonparental care (x) rises to offset a decline in publicly provided supervision (g), the labor supply response will be muted.

Each form of supervision is an input into the child's development. A particularly tractable specification for the development "production" function is given by

$$\begin{gathered} q = g^{\gamma} q_{\!,}(m, x)^{1-\gamma}\,, \ \text{ with} \\ q_{\!,}(m, x) = (\mu^{1-\varphi} m^{\varphi} + (1-\mu)^{1-\varphi} x^{\varphi})^{1/\varphi} \end{gathered} \tag{15}$$

and where $\gamma \in (0,1)$ and $\varphi \leq 1$. Equation (15) uses a Cobb-Douglas outer nest to aggregate on-site instruction time (g) and a "bundle" of private care ($q_{\!,}$). The latter inner bundle is a CES aggregate of parental (m) and private nonparental (x) care time. The parameter φ controls the elasticity of substitution between m and x, which is given by $(1-\varphi)^{-1}$. The CES form is a popular specification of development production functions (see, e.g., Cunha et al., 2010) and has been applied in the context of parental and nonparental private care (Berlinksi et al., 2024).[35] The literature offers less guidance on the role of g in q. We opt for a Cobb-Douglas outer nest because

[35] Del Boca et al. (2014) use a Cobb-Douglas aggregator over all inputs but omit on-site time.

it simplifies the analytics of nonparental care (x)—the focus of our discussion—and thereby enables us to draw out lessons for the broader literature on childcare and child development.[36]

The choice of each form of care trades off the value of another hour of care for the child with the price of that care. The price of parental care is the forgone wage, w, whereas nonparental care has price per unit time, p. We assume p is small insofar as $w > p$ to account for the prevalence of informal, unpaid care, such as supervision by friends, grandparents, or older children (Yang et al., 2024). This calibration implies a relatively high opportunity cost of parental care time, m.

We may now consider how parental labor supply, n, responds to a shift in publicly provided supervision, g. The comparative static may be written as

$$\frac{\mathrm{d}n}{\mathrm{d}g} = \frac{z(\xi;\varphi)}{1 + z(\xi;\varphi)}, \tag{16}$$

where $\xi \equiv x/m = x/(1-g-x)$ is nonparental time per hour of parental care and

$$z(\xi;\varphi) \equiv \frac{\left(\frac{\mu\xi}{1-\mu}\right)^{\varphi-1} + (1-\varphi)\xi^{-1} - \varphi}{\left(\frac{\mu\xi}{1-\mu}\right)^{1-\varphi} + (1-\varphi)\xi - \varphi}. \tag{17}$$

Derivations of all results in this section may be found in Online Appendix E.

The comparative static has two important properties. First, for $z > 0$, $\mathrm{d}n/\mathrm{d}g \in (0,1)$: labor supply falls when on-site time is reduced but less than one for one. While other labor supply outcomes are possible, the restriction $z > 0$ is a reasonable one. It obtains for any $\varphi \leq 0$ and, by continuity, over a range of φ to the right of zero. Indeed, equation (17) shows that if $\varphi > 0$, then $z > 0$ unless ξ is sufficiently small (the denominator is negative) or sufficiently large (the numerator is negative). Each of these polar cases conflicts with the ATUS evidence: a very small

[36] The Cobb-Douglas form has the awkward implication that $q \to 0$ as $g \to 0$. However, when paired with log utility, the scale of g has no allocative effect *via its role in* q. Rather, g shapes allocations through the time constraints.

(large) $\xi \equiv x/m$ implies that the marginal value of nonparental (parental) time is so low that m (x) responds far too much to a reduction in g (see Online Appendix E for a fuller discussion).[37]

Second, $\mathrm{d}n/\mathrm{d}g$ declines in φ (for any ξ). Therefore, at higher φ, labor supply falls *less* when on-site time is reduced.[38] Market work is sustained in this context by higher nonparental care. Intuitively, when the opportunity cost of parental time is high ($w > p$), a parent increases x relative to m if the two become more substitutable—that is, if the elasticity of substitution between them is increased. Hence, as φ is raised, a fall in g implies smaller increases in m, which require in turn smaller declines in n.

In light of our regression estimates, we assess the quantitative implications of equation (16) in the case where $\mathrm{d}n/\mathrm{d}g$ is small. We show that, in a neighborhood around $\mathrm{d}n/\mathrm{d}g = 0$, φ is bounded below such that $\varphi > (1 + \xi)^{-1}$. To quantify the latter, we calibrate ξ to capture the initial allocation of childcare among parents "exposed" to school closure. For this purpose, we draw on Blau and Currie's (2006) figures for households where the mother had generally worked, which imply that children were under 1.36 hours of nonparental supervision per hour of parental care.[39] A value of $\xi = 1.36$ yields a *lower bound* of φ equal to 0.424. Thus, the pandemic-era data, as seen through this model, point to significant substitutability between forms of care.[40]

While this exercise aims to highlight the broader implications of our empirical results, one might be wary of generalizing from the pandemic period. For instance, whereas remote instruction posed unique demands in 2020-21, time allocated to childcare in "normal" times is more diffused across academic supervision, extracurricular activities, and other tasks, some of which may require

[37] We also show that a very low $\xi \equiv x/m$, and a very high m, emerge only under the alternative calibration $w < p$.
[38] When we vary φ, we adjust μ to hold fixed the initial value of ξ (and, thereby, n). See Online Appendix E for more.
[39] See primary and secondary arrangements in Blau and Currie's Table 2. The idea behind this approach is that households with employed mothers are arguably most "exposed" to a school closure. If the effect of a closure among them is nearly zero, then the average causal effect of policy will be nearly zero (as it appears to be, empirically).
[40] Note that $\mathrm{d}n/\mathrm{d}g \approx 0$ implies $\mathrm{d}m/\mathrm{d}g \approx 0$. The latter is consistent with estimates for the noncollege group.

more parental inputs (see Ramey and Ramey, 2010). Nevertheless, we see estimates in Berlinski et al. (2024) as broadly supportive of our conclusions. They study a sample of *pre*school children—a population for whom parental time is thought to be particularly crucial—and still find $\varphi = 0.92$ given a similar production function over parental and nonparental care.

The degree of substitutability between forms of care has significant implications for public policy and labor market dynamics. For instance, the price elasticity of demand for nonparental care increases in φ. Therefore, there will be greater take-up of subsidized care if nonparental time is highly substitutable for parental time.[41] Alternatively, consider a temporary increase in aggregate productivity that leads to higher wage offers. The Frisch elasticity of labor supply increases in φ: parents substitute more from childcare to market work if nonparental time is a close substitute for their own. See Online Appendix E for a fuller discussion.

6. Conclusion

This paper has presented new evidence on the response of parental labor supply, and time use more generally, to the closure of schools to on-site instruction. With a full suite of controls for unobserved heterogeneity, we do not detect a labor supply reaction. Even if we omit these controls, the labor supply responses represent a small fraction of the over 30 hours of childcare time "lost" with the suspension of in-person instruction. Time use data show that working from home while supervising children and nonparental private care helped support labor supply during school closures. The paper then integrates telework and nonparental care into a model of parental time allocation and illustrates how our results inform the identification of salient structural parameters.

[41] The federal government makes substantial investments in adolescent care. For instance, the Child Care and Development Fund made available $40 billion of subsidies to families of school-age children (U.S. Dept. of Health and Human Services, 2021).

Our empirical exploration of the roles of telework and nonparental care is limited, however, by the small sample sizes in the ATUS and by the paucity of direct measurements of nonparental care time.[42] We hope our work stimulates further efforts to measure these activities, which will in turn advance research into many related questions. To illustrate, these data would shed light on how shifts in the composition of the household—for instance, a grandparent or an older child moves in—alter the distribution of childcare and, therefore, parental labor supply.

With respect to the theory, we hope future research will push in two directions. One priority is to allow for more residential arrangements (e.g., two parent households, the presence of a grandparent, and so forth). This extension better captures the heterogeneity of care provider arrangements in the data (see Truskinovsky et al., 2022). A second priority is to model the link between on-site time and *specific* child outcomes, such as academic performance (see Jack et al., 2023, and Goldhaber et al., 2023, on test scores). This extension enables one to test if the predicted changes in parental time use patterns are consistent with evidence on academic outcomes.

[42] Surveys by Calarco et al. (2021) and Truskinovsky et al. (2022) are notable exceptions.

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Online Appendix to "School Closures, Parental Labor Supply, and Time Use"

Enghin Atalay, Ryan Kobler, and Ryan Michaels

A. The spread of the pandemic and instruction format

This appendix examines the connection between COVID-19 transmission and the choice of instruction format. Specifically, the outcome of interest is the local area in-person share from Parolin and Lee (2021). Accordingly, the analysis is performed at the area-by-month level. The full set of local areas is used. Throughout, the sample period is the 2020-21 school year. The latter choice eliminates earlier months in 2020 when essentially all schools were closed to in-person instruction and the final months of 2021 when essentially all schools were open. Our regressions include area fixed effects to isolate the within-area co-movement of the in-person share and COVID-19 transmission.[1] The extent of this co-movement may, in turn, vary *across* areas. Therefore, we will also include interactions between certain local (observable) attributes and indicators of the spread of COVID-19.

Table A1 reports our estimates. The initial batch of regressors consists only of monthly COVID-19 cases and fatalities as well as area and month fixed effects. We highlight two results. First, the *within* R-squared of less than 0.04 indicates that these regressors account for a small portion of overall variation in in-person shares. Second, a higher number of cases and fatalities both imply lower in-person shares. Specifically, one more case per 100 area residents is associated with a three percentage point reduction in the in-person share, and one higher fatality per 10,000 residents implies a nearly one percentage point reduction. These estimates are statistically

[1] Appendix B examines the *cross-area* variation in on-site shares.

significant but modest in size. To illustrate, the change in in-person shares associated with a *two* standard deviation movement in cases amounts to just one-fifth of a standard deviation of the shares. A comparable shift in fatalities has a still smaller impact.

Next, we interact cases and fatalities with a vector of local attributes. The latter consists, first, of various demographic controls. A large body of research finds that the pandemic led to sharp increases in mortality among certain groups, especially Hispanics, non-White individuals, and the noncollege educated (see Case and Deaton, 2021). There may be a greater demand for social distancing, including virtual instruction, in areas where such groups are highly represented. Accordingly, the vector of attributes includes each group's share in the local population. Mortality rates may have also risen more in areas of high density.[2] To capture the latter, our list of attributes includes the share of an area's population in a city center.

In addition, our set of local attributes consists of policy-relevant institutions and political attitudes. For example, as noted in the main text, areas with more unionized education sectors returned to in-person instruction later in the 2020-21 school year. Areas that favored the Republican party also generally chose more on-site instruction. We use two indicators of Republican party strength: Donald Trump's share of the area's 2016 presidential vote and the presence of a Republican governor as of January 2020.

Our measures of all attributes other than partisan identity are constructed from pre-pandemic (2015-19) CPS data. The CPS reports on demographics, geographic characteristics (e.g., urban v. rural), and union membership. Donald Trump's 2016 vote share is from the MIT Election Data and Science Lab. The partisan identity of governors in 2020 is from ballotpedia.org.[3]

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[2] See, for instance, Almagro and Orane-Hutchinson (2020). Carozzi et al. (2021) have challenged this claim, though.
[3] On the 2016 vote share, see the "U.S. President 1976-2020" data file at https://electionlab.mit. edu/data. The list of present-day governors is at https://ballotpedia.org/Governor_(state_executive_office). The latter was accessed in December 2022. If the governorship had turned over since 2020, we looked up the governor's party in 2020.

Table A1: The Spread of COVID-19 and Instruction Format

Coefficient		In-person share (SafeGraph)	
Monthly cases / 100		-0.030***	-0.036***
		[0.005]	[0.005]
Monthly deaths / 10,000		-0.009***	-0.009***
		[0.002]	[0.003]
Monthly cases ×	Trump share		0.145***
			[0.040]
	GOP governor		0.042***
			[0.009]
	Teacher union		-0.018
			[0.018]
	Hispanic		0.144***
			[0.023]
	Nonwhite		0.157***
			[0.040]
	Noncollege		-0.050
			[0.044]
	City center		0.040**
			[0.017]
Monthly deaths ×	Trump share		0.056***
			[0.021]
	GOP governor		-0.007*
			[0.004]
	Teacher union		-0.020**
			[0.008]
	Hispanic		-0.024*
			[0.012]
	Nonwhite		-0.042***
			[0.017]
	Noncollege		0.009
			[0.024]
	City center		-0.000
			[0.010]
Number of obs.		4,293	4,293
Within R^2		0.036	0.123

Note: The sample period is the 2020-21 school year, and the unit of analysis is the local area × month. Area fixed effects are included. Other than COVID-19 cases and deaths, regressors are expressed as deviations from the U.S. average. Standard errors are clustered at the geographic area level. *** indicates a p-value less than 0.01; ** a p-value between 0.01 and 0.05; and * a p-value between 0.05 and 0.10.

The second column in Table A1 reports the coefficient estimates for the expanded specification with interaction terms. To aid interpretation, each attribute is expressed as a deviation from the national average. Thus, the coefficient estimates on cases and deaths (first two rows) indicate the response of the in-person share when all attributes are evaluated at their mean. These estimates are negative, statistically significant, and similar to those in the first column. Thus, a positive (negative) coefficient on an interaction term implies that the associated attribute mitigates (amplifies) the decline in in-person instruction associated with higher cases and deaths.

A few results emerge from the table. To start, the introduction of the interaction terms elevates the within R-squared, but the latter remains around just 0.12. Our takeaway from this result is that month to month variation in the state of the pandemic had a limited impact on the evolution of instruction format.

Still, to the extent the spread of COVID-19 shaped the choice of schooling mode, it did so in a heterogeneous manner. The political identity of the local area played a notable role. For instance, in an area where Trump's share was one standard deviation (or, 14.4 percentage points) below the mean, one more confirmed case per 100 residents was associated with a decrease in the in-person share of $-0.036 - 0.144 \times 0.145 = 5.7$ percentage points. By contrast, the in-person share falls just 1.5 percentage points in an area with one-standard-deviation more Trump support. The same contrast applies to higher COVID-19-related fatalities.

Other notable attributes include the nonwhite and Hispanic shares, but their impact is less straightforward. On the one hand, in areas with high nonwhite and Hispanic shares, the in-person share falls *by less* when COVID-19 *cases* rise. Indeed, a one standard deviation shift in either of the latter has nearly the same impact as a similarly scaled shift in the Trump share. On the other hand, when *fatalities* rise, the in-person share falls *by more* in areas with high minority shares.

Finally, the remaining attributes play a less consistently significant role. The presence of a Republican (GOP) governor echoes the effect of the Trump share when interacted with COVID-19 cases but not fatalities. By the same token, the unionized share of teachers and the city-center share of population enter significantly in some interactions but not others. The noncollege share is insignificant throughout.

In sum, the schooling mode in Democratic-leaning areas was more responsive to the pandemic. However, the explanatory power of COVID-19 case and death counts is rather modest.

B. Placebo test results

Table B1 reports the results of the placebo test described near the end of Section 3.1. We relate *pre*-pandemic labor supply to the average in-person share in the pandemic period. We present results based on in-person shares from SafeGraph, CSDH, and Burbio. The latter two are available only for the 2020-21 school year. Therefore, for the sake of comparability, we report results based on SafeGraph estimates over the 2020-21 school year as well as over all of 2020-21.

Since we discussed the SafeGraph-based results in the main text, our comments here pertain mainly to our other two data sources. (Details on the construction of CSDH and Burbio measures are available in Section C later in this appendix.) Estimates based on CSDH data show that mothers' pre-pandemic relative labor supply in an area with full-time in-person instruction in 2020-21 was nearly 1.4 weekly hours higher than in an area with full-time virtual instruction. This result is somewhat smaller than its SafeGraph-based counterpart over the school year. Estimates from Burbio show a weaker, but still statistically significant, positive relationship between pandemic-era in-person shares and pre-pandemic hours. For all three measures, the placebo estimates are comparable in magnitude to those obtained over the 2020-21 sample (see Tables 3 and C3). Estimates for men are smaller and less precisely estimated, as stressed in the main text.

Table B1: Placebo Test

	SafeGraph		CSDH	Burbio
	Women: Unweighted			
In-person × kids, ψ	3.353***	2.007***	1.427***	1.132**
	[0.877]	[0.558]	[0.444]	[0.442]
Number of obs.	1,351,083	1,351,083	1,254,179	1,245,826
	Women: CPS Weights			
In-person × kids, ψ	3.465***	2.102***	1.378***	1.158***
	[0.886]	[0.534]	[0.446]	[0.423]
Number of obs.	1,349,399	1,349,399	1,252,591	1,244,191
	Men Unweighted			
In-person × kids, ψ	1.386	0.855	0.737	0.406
	[0.851]	[0.525]	[0.450]	[0.418]
Number of obs.	1,284,357	1,284,357	1,191,245	1,184,507
	Men: CPS Weights			
In-person × kids, ψ	0.915	0.590	0.397	0.314
	[0.684]	[0.431]	[0.366]	[0.357]
Number of obs.	1,271,021	1,271,021	1,178,876	1,171,853
Period of policy	All 20-21	School 20-21	School 20-21	School 20-21

Note: This table estimates a version of equation (1) on CPS data 2015-19. Relative to equation (1), the policy variable is the pandemic-era mean. The "period of policy" refers to the specific years over which the mean is taken: "All 20-21" includes calendar years 2020 and 2021 (exclusive of June-August), whereas "School 20-21" covers only September 2020 – May 2021. Standard errors are clustered at the geographic area level. *** indicates a p-value less than 0.01; ** a p-value between 0.01 and 0.05; and * a p-value between 0.05 and 0.10.

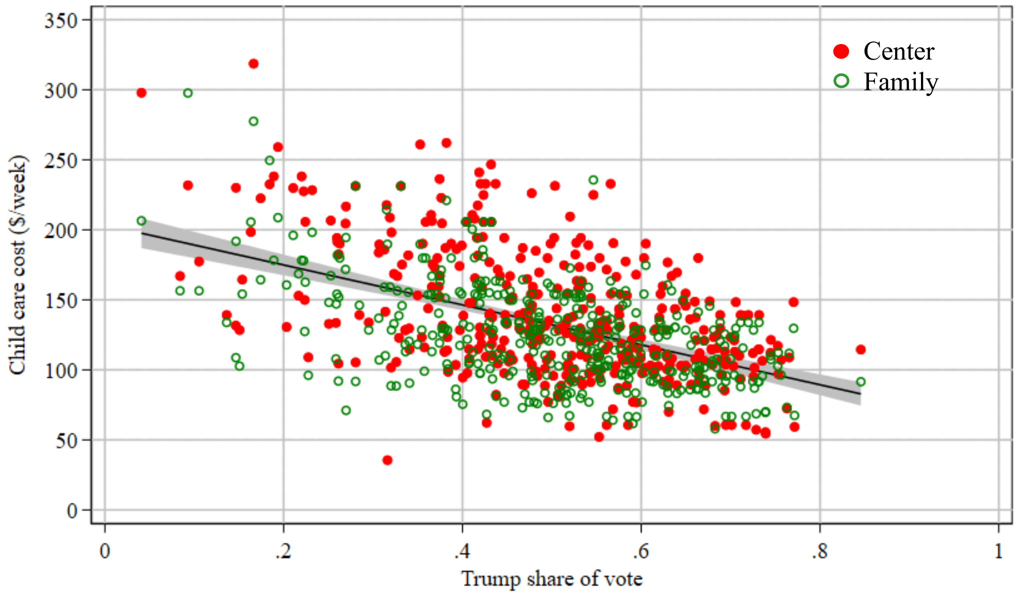
Still, in the SafeGraph-based regressions, the results often fall just short of conventional significance levels and are not statistically distinguishable from estimates over the pandemic years.

Overall, these estimates suggest that in-person shares capture more fundamental forces behind parental labor supply, especially for mothers. To this extent, we expect in-person shares to be related to other (pre-pandemic) attributes of an area that likely shape parental hours. The main text highlights two such outcomes: childcare prices and commute-to-work times.

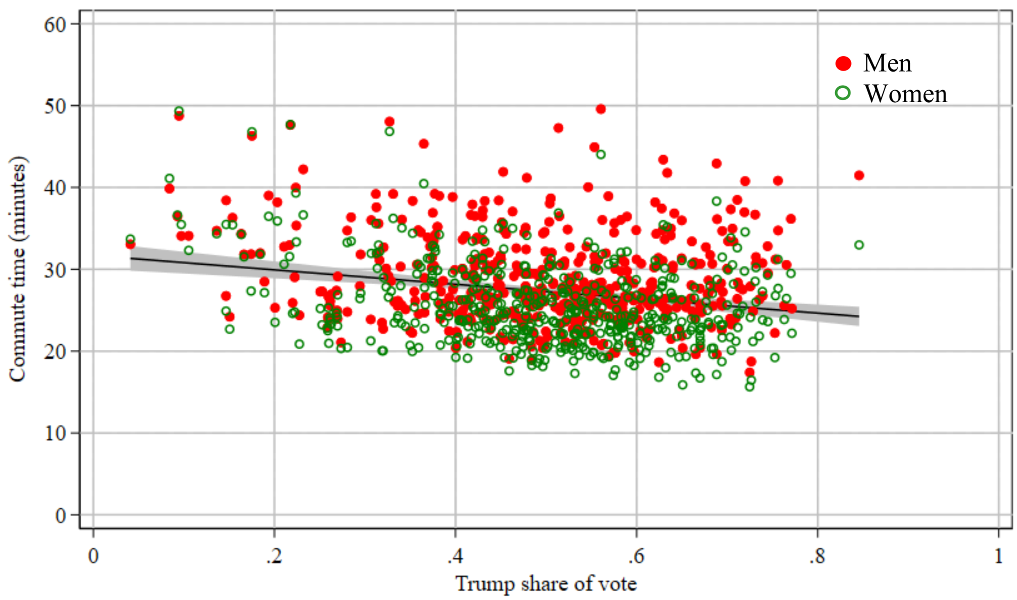
The connection between these two outcomes and in-person shares runs, in part, through their association with local partisan affiliation. Figure B1 shows that lower commute times and childcare prices are each associated with higher support for Donald Trump. (We report our data

Figure B1: Childcare Prices, Commute Times, and Partisan Affiliation

Panel A: Childcare Prices



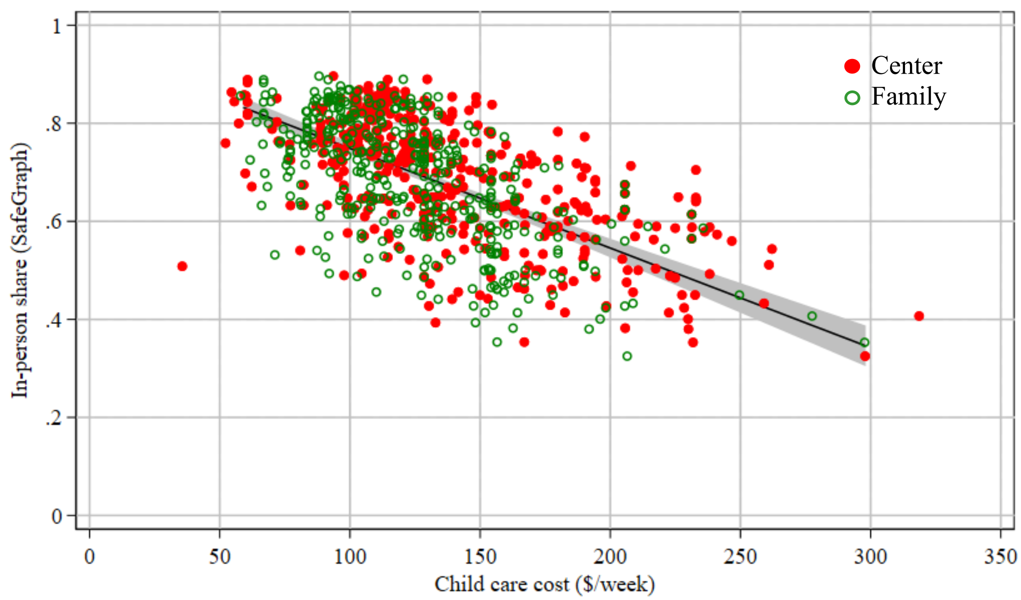
Panel B: Commute Times



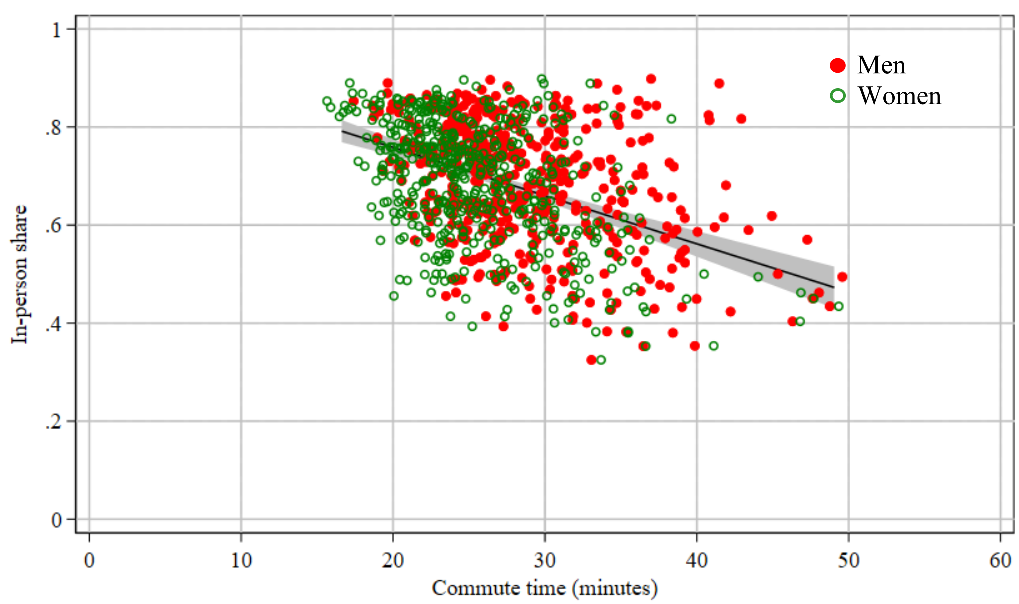
Note: The line of best fit in each panel reflects a regression on the pooled sample, e.g., in Panel A, separate scatter plots are shown by type of childcare center, but the regression line is fit to the average price across both types. Similarly, the regression line in Panel B is fit to the average commute time across men and women. The gray shaded region shows the 95% confidence band. See text for a description of the data sources.

Figure B2: Childcare Prices, Commute Times, and In-person Shares

Panel A: Childcare Prices



Panel B: Commute Times



Note: See notes to Figure B1.

Table B2: Correlates of In-person Instruction

Coefficient	SafeGraph in-person share					
Trump vote share	0.696***			0.581***	0.652***	0.573***
	[0.022]			[0.028]	[0.022]	[0.025]
Childcare price/100		-0.194***		-0.082***		-0.064***
		[0.014]		[0.011]		[0.010]
Commute time/10			-0.102***		-0.056***	-0.042***
			[0.012]		[0.007]	[0.007]
Number of obs.	404	404	404	404	404	404
R^2	0.662	0.374	0.161	0.712	0.708	0.735

Note: See text for description of data sources. The childcare price is expressed in hundreds of dollars, and commute time is expressed in tens of minutes. Standard errors are robust to heteroskedasticity. *** indicates a p-value less than 0.01; ** a p-value between 0.01 and 0.05; and * a p-value between 0.05 and 0.10.

sources momentarily.) This result is significant because the vote share for President Trump is strongly and positively correlated with in-person shares. It follows that lower commute times and childcare prices are likely to predict higher on-site shares. Figure B2 confirms this claim.

Table B2 offers a simple statistical summary of these points. We regress a local area's average in-person share over 2020-21 on up to three variables: Trump's share of the 2016 presidential vote, the average commute time in 2015-19, and average pre-pandemic childcare prices. Donald Trump's share is from the MIT Election Data and Science Lab (see Appendix A). Mean commute times are taken from the Census Bureau's county-level tabulations of the 2015-19 American Community Survey 5-year estimates (Manson et al., 2022). Childcare prices are compiled by the Women's Bureau of the U.S. Department of Labor. The regression sample consists of 404 local areas for which center-based and (smaller-scale) family-based prices are available from the Women's Bureau data. Finally, the regressions are fit to childcare prices averaged across both types of care and commute times averaged over men and women. The figures, which report separate scatter plots by type of care and gender, indicate that little detail is lost if the data are pooled.[4]

The estimates in Table B2 corroborate, and extend, the evidence in Figures B1 and B2. First, childcare prices and commute times are each negatively, and significantly, associated with in-person shares. A price increase of $100/week implies a nearly 20 percentage point lower in-person share (see column 2). In addition, a 10 minute longer commute is associated with a 10 percentage-point lower in-person share (see column 3). However, Trump's support is the most significant predictor of in-person shares among the three regressors. Indeed, when Trump's vote share is added to the regression, the coefficients on commute time and childcare prices are halved. Thus, the connection between these two factors and school policy is mediated, in part, by their association with partisan affiliation. Still, commute time and childcare prices do account for some portion of the variance in in-person shares conditional on Trump's level of support.

To follow up on these results, we examine if the connection between childcare prices and the Trump share reflects a partisan role in childcare regulations. This analysis is done at the *state* level since childcare is regulated by state authorities. For the regression analysis, we zero in on one regulation: the maximum child-staff ratio (see Kimmel, 1998). A higher maximum enables a childcare center to operate with fewer staff and, therefore, at lower cost. We draw on a database of state regulations maintained by the National Center on Early Childhood Quality Assurance. We use the maximum child-staff ratio in 2017, the midpoint of our pre-pandemic sample. Across the states, the maximum varies from 10 to 25 with an interquartile range of 15 to 20.[5]

We find a statistically significant, but quantitatively modest, connection between partisanship, childcare regulations, and childcare prices. An increase in Trump's share of 10

[4] We drop counties if their pre-pandemic childcare prices were imputed based on state-level prices.
[5] These data are available from ICPSR at https://www.icpsr.umich.edu/web/ICPSR/studies/ 37700.

percentage points implies one more child per staff member is permitted under state law. This latter is statistically significant but represents a fairly small share of the variation in child-staff ratios. By the same token, the allowance for one more child per staff member has a limited impact on weekly childcare prices, which fall by 2-3 percent (the higher of the two pertains to center-based rather than family-based care).[6]

As a final exercise, we document how commute times and childcare prices are related to pre-pandemic labor supply. Table B3 reports the results by gender, conditional on the same demographic covariates used in Table B1. These regressions are run on individual-level CPS data in 2015-19 across the 404 local areas for which we have childcare prices.[7] Table B3 shows that commute time and childcare prices are each individually significant correlates of maternal hours worked. Specifically, weekly hours decline by around one per $100 increase in the weekly childcare price and per 10-minute increase in commute time. Among fathers, hours worked are

Table B3: Childcare Prices, Commute Times, and Hours Worked

			Women			
Childcare cost/100	-0.849***		-0.577**	-1.483***		-1.200***
× kids	[0.237]		[0.264]	[0.320]		[0.389]
Commute time/10		-1.129***	-0.915***		-0.975***	-0.506
× kids		[0.331]	[0.300]		[0.281]	[0.328]
Number of obs.	1,069,053	1,069,053	1,069,053	1,067,574	1,067,574	1,067,574
			Men			
Childcare cost/100	-0.354		-0.361	-0.658***		-0.683***
× kids	[0.315]		[0.364]	[0.240]		[0.245]
Commute time/10		-0.076	0.019		-0.203	0.044
× kids		[0.239]	[0.291]		[0.216]	[0.219]
Number of obs.	1,015,075	1,015,075	1,015,075	1,004,028	1,004,028	1,004,028
CPS Weights	No	No	No	Yes	Yes	Yes

Note: The childcare price is the mean of center- and family-based prices and expressed in hundreds of dollars. Commute time is the mean among employed adults ages 21-59 and expressed in tens of minutes. Standard errors are clustered at the geographic area level. *** indicates a p-value less than 0.01; ** a p-value between 0.01 and 0.05; and * a p-value between 0.05 and 0.10.

[6] To conserve space, we only summarize these regression results here. Detailed estimates are available upon request.
[7] We confirmed that the placebo test also fails in this subsample of areas. Indeed, the estimate of ψ (see Table B1) in this subsample is *higher* among fathers (and hardly affected among mothers).

negatively related to childcare costs, but this estimate is statistically significant only in the weighted OLS regression and, even then, is half the size of the estimate for mothers. We find no significant relationship between commute times and paternal hours worked.

C. Sensitivity analysis and additional results from the CPS

This appendix reports additional results on the labor supply response to variation in the in-person share. First, we estimate the role of the extensive margin in total hours adjustment. Second, we report results from a battery of sensitivity tests. Specifically, we examine the implications of alternative measures of the in-person share; the age of the eldest school-age child; the age range of adult respondents; the geographic coverage of the sample; and the use of industry and occupation controls. Third, we return to the issue of unobserved heterogeneity and extend our regression analysis to account for a full set of individual fixed effects. Finally, we rerun our main specification with the *household* as the unit of analysis. This exercise reveals how the household's total hours worked vary with instruction format.

C.1 Extensive margin

Table C1 reports results for employment and echoes several themes observed earlier for hours (see Tables 2 and 3). First, the maternal labor supply response is largely stable across sample periods if parental status-by-month effects are included. Second, the extensive margin accounts for the labor supply response of mothers but not for fathers. Given a 37-hour week among employed mothers, the 5.7 percentage-point gain in the maternal employment rate (see the fourth column of the "All 20-21" panel) implies an increase in weekly hours of 2.1—only slightly smaller than the 2.5 hour per week response reported in Table 3. Third, the introduction of parental-status-by-area effects eliminates the statistical significance of these estimates.

Table C1: Employment Responses Based on SafeGraph In-person Shares

	Women: All of 2020-21					
In-person × kids, ψ	0.020***	0.016**	0.056***	0.057***	-0.005	-0.003
	[0.007]	[0.008]	[0.015]	[0.016]	[0.016]	[0.018]
Number of obs.	447,899	447,277	447,899	447,277	447,89	447,277
	Women: 2020-21 School Year					
In-person × kids, ψ	0.051***	0.053***	0.059***	0.059***	-0.006	-0.006
	[0.014]	[0.015]	[0.015]	[0.016]	[0.027]	[0.029]
Number of obs.	228,550	228,225	228,550	228,225	228,55	228,225
	Men: All of 2020-21					
In-person × kids, ψ	-0.010	-0.016**	0.013	-0.016**	0.000	-0.009
	[0.007]	[0.007]	[0.014]	[0.007]	[0.014]	[0.017]
Number of obs.	432,856	428,244	432,856	428,244	432,85	428,244
	Men: 2020-21 School Year					
In-person × kids, ψ	0.009	0.007	0.012	0.008	-0.012	-0.013
	[0.012]	[0.012]	[0.013]	[0.013]	[0.023]	[0.026]
Number of obs.	221,080	218,575	221,080	218,575	221,08	218,575
CPS Weights	No	Yes	No	Yes	No	Yes
Month × parent F.E.	No	No	Yes	Yes	Yes	Yes
Area × parent F.E.	No	No	No	No	Yes	Yes

Note: "All 20-21" refers to the 2020 and 2021 calendar years save for June-August. "School 20-21" is the period September 2020 to May 2021. Standard errors are clustered at the geographic area level. *** indicates a p-value less than 0.01; ** a p-value between 0.01 and 0.05; and * a p-value between 0.05 and 0.10.

Next, we present employment estimates by educational attainment and marital status in Table C2. (This table is the extensive-margin counterpart to Tables 4 and 5.) The top two panels report results by marital status, whereas the bottom two panels divide the sample into (four-year) college and noncollege graduates. To conserve space, we report results only for the specification used in Section 3.2, which augments equation (1) with parental status-by-month effects.

These results confirm an extensive-margin response among mothers within each marital status and educational attainment category. Consider, for instance, the result for noncollege educated mothers. A shift from virtual to in-person instruction implies an increase in the area's

Table C2: Employment Responses by Marital Status and Education

	By Marital Status					
	Married		Unmarried		Lone adult	
	Women					
In-person × kids, ψ	0.056***	0.064***	0.059**	0.050**	0.104***	0.086***
	[0.018]	[0.020]	[0.023]	[0.022]	[0.028]	[0.030]
Number of obs.	242,743	242,351	205,156	204,926	60,291	60,282
	Men					
In-person × kids, ψ	0.031**	0.036**	-0.002	-0.012	0.067	0.048
	[0.014]	[0.015]	[0.030]	[0.032]	[0.045]	[0.049]
Number of obs.	223,471	219,663	209,385	208,581	55,284	55,275
CPS Weights	No	Yes	No	Yes	No	Yes

	By Education			
	Noncollege		College	
	Women			
In-person × kids, ψ	0.049***	0.055***	0.057***	0.053**
	[0.018]	[0.020]	[0.021]	[0.023]
Number of obs.	266,258	265,968	181,641	181,309
	Men			
In-person × kids, ψ	0.007	0.002	0.010	0.009
	[0.016]	[0.015]	[0.018]	[0.019]
Number of obs.	284,723	281,867	148,133	146,377
CPS Weights	No	No	Yes	Yes

Note: Each column reports an estimate based on equation (1) but where the outcome is an employment indicator and parental status-by-month effects are included. The period is calendar years 2020 and 2021 with the summer months (June-August) omitted. Standard errors are clustered at the geographic area level. *** indicates a p-value less than 0.01; ** a p-value between 0.01 and 0.05; and * a p-value between 0.05 and 0.10.

maternal employment rate of 5-5.5 percentage points. Given an average workweek of 36 hours among noncollege employed mothers, the extensive-margin response accounts for 2 additional weekly hours of work. This portion represents slightly more than 80 percent of the estimated response of (actual) weekly hours reported in the second column of Table 4 (2.4). Other results for women in Table C2 send the same message. By contrast, the extensive margin among fathers is relatively inactive. The one exception is for married fathers, who experience a statistically significant increase in employment upon the return of in-person instruction. This estimate accounts for approximately 70 percent of the response in (actual) weekly hours shown in Table 5.[8]

C.2 Measures of the in-person share

This section provides a more extensive introduction to measures of the in-person share from the COVID-19 School Data Hub (CSDH) and Burbio. We also present further results based on these alternatives to SafeGraph.

CSDH and Burbio data. The CSDH is based primarily on school-level reports of the predominant instruction mode. The reports were usually submitted monthly to state education agencies over the course of the 2020-21 school year.[9] In total, 35 states provided school-level data. In another 11 states where school-level data was unavailable, agencies collected information at the school district-level. The 46 states for which CSDH provides data account for 2,800 of 3,100 U.S. counties and over 90 percent of U.S. student enrollment.

CSDH standardizes reports where needed in order to assign them to one of three instruction modes: in-person, virtual, or hybrid. Clearly, the in-person share of the reported "in-person" ("virtual") format is one (zero). However, the on-site portion of "hybrid" instruction is not specified. As detailed below, we form our own estimate of the latter based on U.S. Department of Education data. A local area's in-person share is then the enrollment-weighted average of in-person shares across schools (and/or school districts).

Although state agency data represent an official record of instruction format, they are not without noise. The categorical nature of the data necessarily involves a certain degree of judgment.

[8] Employed married fathers work 43 hours per week on average. The estimate in Table C2 implies an increase in weekly hours of 1.55, whereas the total weekly hours response (second column in the bottom panel of Table 5) is 2.26.
[9] In 11 states, though, the reports were made to the U.S. Department of Agriculture as part of a program to reach students who were eligible for reduced-price meals but who did not attend school on-site.

For instance, a schedule with two days per week of on-site instruction only for grades K-2 may be understood as a "hybrid" format by one (primary) school but a "virtual" format by another.[10]

Therefore, we next turn to Burbio, whose estimates are developed from entirely different sources. Burbio's analysts follow district websites, local news reports, and social media to track the instruction format of a sample of school districts in their assigned area. The format is categorized as in-person, virtual, or hybrid, but again, the on-site portion of the hybrid format is not given. Relative to CSDH, Burbio offers less geographic coverage: in total, Burbio follows districts in just under 600 U.S. counties.

We impute the in-person portion of the hybrid format in both CSDH and Burbio based on U.S. Department of Education survey data. Specifically, we draw on state-level tabulations of the 2021 National Assessment of Educational Progress (NAEP) Monthly School Survey. The survey was administered in each of the first five months of 2021. The Department's Institute for Education Sciences (IES) published results for 37 states (for which sufficient data was gathered) as well as for the four Census regions. Since the survey was not fielded in 2020, we assume responses for January 2021 applied to earlier months of the school year.[11]

Among schools in each state (or, region) that report a "hybrid" format, the IES calculates the share for which the number of in-person days per week was (a) one to two, (b) three, or (c) four to five. We use these reports to calculate the share of weekly instruction held on-site under a "hybrid" format, where a two-day per week schedule is chosen to represent bin (a) and a four-day schedule represents bin (c). For states that did not participate in the survey, we substitute an estimate based on analogous tabulations for the Census region of the state.

[10] The hybrid format is a quantitatively important mode in the CSDH data: the hybrid share of instruction in each state is at least 20 percent and is as high as two thirds (North Carolina).
[11] These data may be downloaded from https://ies.ed.gov/schoolsurvey/mss-dashboard/.

While we see value in leveraging all the information from IES, we confirmed that simpler treatments of "hybrid" instruction yield similar results. For example, suppose we assign to all areas in all months the same on-site share of weekly instruction under a hybrid format. This share is the national mean in the 2020-21 school year and equal to 0.6, i.e., three days per week. Estimates based on the latter are very similar to results shown below.[12] This conclusion indicates that variation in the overall in-person share is dominated by differences in the take-up of the three basic modes (in-person, hybrid, or virtual) rather than in the in-person scope of hybrid instruction.

Main CSDH and Burbio results. To proceed, we report results on the hours worked response to CSDH and Burbio measures of in-person instruction. We first re-estimate the standard two-way fixed effects model in equation (1) and then add further controls for unobserved heterogeneity. The sample period is the 2020-21 school year, as these are the months for which CSDH is available. The estimates may be compared to SafeGraph-based results in Tables 2 and 3.

A few themes emerge from Table C3. As we saw in the main text, labor supply responses are insignificant (and sometimes of the "wrong" sign) if all controls for unobserved heterogeneity are included. Across the other specifications reported in Table C3, the estimates are uniformly smaller than their counterparts based on SafeGraph data. Nevertheless, maternal labor supply responses based on CSDH and Burbio are often statistically significant (in these specifications), with a range centered around one hour per week. Paternal responses are insignificant, though. Finally, the maternal-paternal differential is somewhat larger in CSDH vis à vis Burbio data. Specifically, the maternal labor supply response exceeds its paternal counterpart by as much as one hour per week in the CSDH data; the gap is at most one quarter of an hour in Burbio data. The relatively large CSDH differential is similar to what we found in SafeGraph data.

[12] For instance, conditional on parental status-by-time effects, the weighted OLS estimate of ψ is 1.143 for women. The analogous result based on this simpler treatment of hybrid instruction is $\psi = 1.125$.

Table C3: Hours Responses Based on Alternative Measures of School Formats

	Women: CSDH					
In-person × kids, ψ	1.010**	1.419***	-1.174	0.898*	1.143**	-1.036
	[0.485]	[0.548]	[0.942]	[0.511]	[0.567]	[0.983]
Number of obs.	211,156	211,156	211,156	210,851	210,851	210,851
	Women: Burbio					
In-person × kids, ψ	0.609	1.012**	0.380	0.515	0.762	-0.049
	[0.418]	[0.490]	[0.704]	[0.456]	[0.532]	[0.743]
Number of obs.	211,777	211,777	211,777	211,455	211,455	211,455
	Men: CSDH					
In-person × kids, ψ	0.209	0.445	-1.281	-0.130	-0.022	-0.936
	[0.484]	[0.542]	[0.821]	[0.505]	[0.544]	[0.985]
Number of obs.	204,090	204,090	204,090	201,754	201,754	201,754
	Men: Burbio					
In-person × kids, ψ	0.497	0.771	0.696	0.283	0.488	0.952
	[0.438]	[0.494]	[0.733]	[0.448]	[0.489]	[0.800]
Number of obs.	205,039	205,039	205,039	202,631	202,631	202,631
CPS Weights	No	No	No	Yes	Yes	Yes
Month × parent F.E.	No	Yes	Yes	No	Yes	Yes
Area × parent F.E.	No	No	Yes	No	No	Yes

Note: The controls in each column are identical to those used in Tables 2 and 3 but for the measurement of the in-person share, which is now drawn from CSDH or Burbio. For each of the latter, one column reports estimates of equation (1), and the remainder of the columns include some combination of parental status controls in equation (2). *** indicates a p-value less than 0.01; ** a p-value between 0.01 and 0.05; and * a p-value between 0.05 and 0.10.

Next, we consider the extensive-margin response based on CSDH and Burbio data. Estimates are shown in Table C4. As we saw for weekly hours, labor supply responses are smaller than in specifications based on SafeGraph data (see Table C1). Still, we confirm that among mothers, the extensive margin accounts for almost all the increase in hours worked. With one peculiar exception, the extensive margin again appears to be inactive among men. The exception is seen in the far-right column, which is notable in that it is conditioned on parental-status-by-area effects. This is the lone case where the estimate in this specification is even marginally significant.

Table C4: Employment Responses Based on Alternative Measures of School Formats

	Women: CSDH					
In-person × kids, ψ	0.024**	0.034**	-0.024	0.020	0.025*	-0.016
	[0.012]	[0.013]	[0.020]	[0.013]	[0.014]	[0.022]
Number of obs.	211,156	211,156	211,156	210,851	210,851	210,851
	Women: Burbio					
In-person × kids, ψ	0.016	0.026**	0.007	0.014	0.020	0.001
	[0.010]	[0.011]	[0.016]	[0.011]	[0.013]	[0.017]
Number of obs.	211,777	211,777	211,777	211,455	211,455	211,455
	Men: CSDH					
In-person × kids, ψ	-0.001	0.001	-0.014	-0.004	-0.005	-0.002
	[0.010]	[0.011]	[0.016]	[0.010]	[0.011]	[0.018]
Number of obs.	204,090	204,090	204,090	201,754	201,754	201,754
	Men: Burbio					
In-person × kids, ψ	0.005	0.008	0.023	0.005	0.006	0.027*
	[0.009]	[0.010]	[0.014]	[0.009]	[0.010]	[0.016]
Number of obs.	205,039	205,039	205,039	202,631	202,631	202,631
CPS Weights	No	No	No	Yes	Yes	Yes
Month × parent	No	Yes	Yes	No	Yes	Yes
Area × parent F.E.	No	No	Yes	No	No	Yes

Note: The first and third panels are based on in-person shares from CSDH. The second and fourth panels are based on Burbio estimates. Throughout, the sample period is September 2020-May 2021. Standard errors are clustered at the geographic area level. *** indicates a p-value less than 0.01; ** a p-value between 0.01 and 0.05; and * a p-value between 0.05 and 0.10.

C.3 Age range of children

We defined "school age" as ages 5 through 17. However, older children within this range may not require much parental supervision (see Blau and Currie, 2006). This observation leads us to examine if parents' labor supply responses vary based on their children's ages.

We present results for three age ranges. To start, we include parents in the sample only if their eldest child is between ages 5 and 13, which omits children of high school age. Next, we narrow the age range to include only parents whose eldest child is between ages 5 and 9. To put these estimates in context, we also consider a sample that includes parents only if their eldest child is *older* than 13 (but less than 18). Throughout, the sample period is 2020-21 (with summer months

Table C5: Estimates with Alternative Age Groups of School-Age Children

	Ages 5-9		Ages 5−13		Ages 14−17	
			Women			
In-person × kids, ψ	2.664***	2.826***	2.437***	2.694***	1.452	1.440
	[0.862]	[0.945]	[0.711]	[0.761]	[0.959]	[0.980]
Number of obs.	359,375	358,897	406,570	406,025	326,585	326,173
			Men			
In-person × kids, ψ	2.937***	2.707***	2.056***	1.943***	0.994	0.814
	[0.823]	[0.808]	[0.687]	[0.633]	[0.985]	[1.020]
Number of obs.	363,400	359,748	400,858	396,562	334,121	331,495
CPS Weights	No	Yes	No	Yes	No	Yes

Note: Each column's sample includes childless adults and parents whose eldest child's age lies within the range in the column header. The period is calendar years 2020 and 2021 but with June-August omitted. Parental-status-by-month effects are included throughout. *** indicates a p-value less than 0.01; ** a p-value between 0.01 and 0.05; and * a p-value between 0.05 and 0.10.

excluded), and the regression specification is equation (1) augmented with parental status-by-month effects. (The presence of parental status-by-area effects again eliminates the significance of the estimates.) Finally, we use the baseline in-person share estimates from Parolin and Lee (2021).

Results are presented in Table C5. The first two columns show that, for parents of children ages 5-9, a shift from fully virtual to fully in-person implies a gain in weekly hours of between 2.5 and 3 for mothers and fathers. When we extend the upper limit of the age range to 13, the response of fathers' hours falls notably, but the decline among mothers is more muted. Finally, in households with older children (ages 14-17), the hours response of fathers is halved further and is insignificant for both parents. Clearly, the significant response among all parents of children ages 5-17 in Table 3 largely reflects the behavior of parents of younger children.

Our use of Parolin and Lee's baseline measure in Table C5 ensures a certain consistency with earlier specifications, but it only captures average on-site activity across *all grades*. It does not tailor the measurement of in-person shares to each age range considered in Table C5. Fortunately, Parlin and Lee provide additional estimates by broad grade levels. For ages 5-9, we

Table C6: Age-Specific In-person Share Estimates

	Ages 5-9		Ages 5-13		Ages 14-17	
Coefficient	Women					
In-person × kids, ψ	2.831***	3.005***	2.592***	2.800***	1.129	1.119
	[0.888]	[0.977]	[0.709]	[0.764]	[0.853]	[0.886]
Number of obs.	358,146	357,668	405,165	404,620	325,420	325,008
	Men					
In-person × kids, ψ	3.033***	2.712***	2.201***	2.026***	0.953	0.945
	[0.850]	[0.805]	[0.696]	[0.638]	[0.911]	[0.923]
Number of obs.	362,062	358,410	399,384	395,088	332,848	330,226
CPS Weights	No	Yes	No	Yes	No	Yes

Note: In-person shares are constructed for each age group based on Parolin and Lee's data. For further details on the sample and specification, see the Notes to Table C5. Standard errors are clustered at the geographic area level. *** indicates a p-value less than 0.01; ** a p-value between 0.01 and 0.05; and * a p-value between 0.05 and 0.10.

use Parolin and Lee's estimate of the in-person share for elementary schools. For all other grades, Parolin and Lee report a single estimate. We take the latter as the in-person share for both middle schools and high schools. Therefore, we adopt this measure for ages 14-17. Finally, for ages 5-13, we take an enrollment-weighted average of in-person shares in the elementary and non-elementary groups where the weight on the latter is *middle* school enrollment. Data on enrollment are from the National Center for Education Statistics.[13]

These more refined in-person share measures from Parolin and Lee do not alter any of our conclusions. The point estimates, as shown in Table C6, are very similar to results derived from our baseline (and shown in Table C5).

C.4 Age range of adults

This section broadens the age range of adults in our sample. Whereas we had restricted attention to ages 21-59 in the main text, we now admit all adults ages 21 and over into the sample. Table C7 reports the results. The top half of the table pertains to women and the bottom half to

[13] See the Elementary/Secondary Information System (ElSi) at https://nces.ed.gov/ccd/elsi/.

Table C7: Age Range of Adults in Sample

	Women: 21-59					
In-person × kids, ψ	0.582*	2.359***	-0.051	0.472	2.501***	0.096
	[0.304]	[0.634]	[0.672]	[0.326]	[0.654]	[0.751]
Number of obs.	447,899	447,899	447,899	447,277	447,277	447,277
	Women: All Ages					
In-person × kids, ψ	1.971***	2.873***	-0.042	1.876***	3.050***	0.037
	[0.263]	[0.589]	[0.576]	[0.287]	[0.628]	[0.662]
Number of obs.	728,758	728,758	728,758	728,131	728,131	728,131
	Men: 21-59					
In-person × kids, ψ	0.566*	1.886***	-0.051	0.210	1.708***	-0.239
	[0.315]	[0.645]	[0.705]	[0.321]	[0.602]	[0.812]
Number of obs.	432,856	432,856	432,856	428,244	428,244	428,244
	Men: All Ages					
In-person × kids, ψ	1.889***	2.600***	0.428	1.568***	2.500***	0.092
	[0.293]	[0.630]	[0.665]	[0.304]	[0.575]	[0.750]
Number of obs.	671,403	671,403	671,403	666,781	666,781	666,781
CPS Weights	No	No	No	Yes	Yes	Yes
Month × parent	No	Yes	Yes	No	Yes	Yes
Area × parent F.E.	No	No	Yes	No	No	Yes

Note: The first and third panels are based on our baseline sample of ages 21-59. The second and fourth panels are based on a sample of adults ages 21 and over. Columns within each panel are differentiated by the inclusion of parental status-by-month and/or by-area controls. Standard errors are clustered at the geographic area level. *** indicates a p-value less than 0.01; ** a p-value between 0.01 and 0.05; and * a p-value between 0.05 and 0.10.

men. Along columns, we present results for each of the specifications we often consider. Specifically, the first and fourth columns are based on the canonical two-way fixed effects model in equation (1). In the remainder of the columns, parental status-by-time and by-area effects are introduced as in equation (2). Throughout, the dependent variable is weekly hours. Finally, for a point of reference, results from our baseline sample (ages 21-59) are also presented.

The table reveals three results. First, the estimated parental labor supply response based on equation (1) is at least three times larger when adults over age 59 are included in the sample. Second, the difference in estimates across samples is not nearly so large when parental status-by-time effects are included in the specification. Third, when additional spatial controls are added, the parental labor supply response is indistinguishable from zero in each sample.

There is a simple interpretation of these results. Consider first the estimates based on equation (1). This regression compares, in effect, changes in the hours of parents and childless adults with respect to in-person shares. Now suppose that changes in in-person shares are (positively) correlated with changes in economic activity more generally, but over-age-60 labor supply is less elastic with respect to any change in the economic state. Therefore, the hours of a control group that includes older adults will not fully reveal, and control for, the broader forces behind parents' hours. Put another way, with older adults in the sample, OLS does not effectively difference out the common component of hours among under-age-60 adults.

To illustrate this point, consider the exercise reported in Table C8. To equation (1), we now add an interaction between p_{at} and an indicator for under age 60. If the in-person share were strictly exogenous, the latter interaction would be indistinguishable from zero conditional on the interaction between p_{at} and parental status. In fact, the under-age-60 term enters as strongly significant: a shift from fully virtual to fully in-person implies an increase in market work among all under-age-60 respondents of at least three hours per week. In this context, the interaction

Table C8: Separate Controls for Parental Status and Age Range

	Women		Men	
In-person × kids	0.771**	0.575*	0.647**	0.391
	[0.323]	[0.327]	[0.320]	[0.328]
In-person × $\mathbb{I}[21 \leq \text{age} \leq 59]$	2.903***	3.360***	3.544***	3.743***
	[0.355]	[0.334]	[0.323]	[0.350]
Number of obs.	728,758	728,131	671,403	666,781
CPS Weights	No	Yes	No	Yes

Note: The sample includes ages 21 and over. Each column reports an estimation of equation (1) with two added controls: an indicator for ages 21-59, $\mathbb{I}[21 \leq \text{age} \leq 59]$; and an interaction of the latter with the in-person share. Only the coefficient on the interaction is reported above. Standard errors are clustered at the geographic area level. *** indicates a p-value less than 0.01; ** a p-value between 0.01 and 0.05; and * a p-value between 0.05 and 0.10.

between p_{at} and parental status now isolates the response of parental labor supply *relative to* the response among under-age-60 childless adults. This response is conceptually (and quantitatively) similar to what we identify in the sample of ages 21-59.

Next, consider the results in Table C7 based on equation (2), which includes controls for unobserved heterogeneity. These controls reduce the gap between the estimates in the all-ages and ages 21-59 samples. In fact, the introduction of parental status-by-time effects alone eliminates more than half of the gap. One interpretation of this result is as follows. Suppose there is not only a common component of under-age-60 hours but also shifts in labor supply motives that uniquely affect under-age-60 *childless* adults. For instance, these adults may face a large disruption to their labor market opportunities that coincides with the suspension of on-site instruction. This source of variation would confound the estimated effect of in-person shares, but less so in a sample where the control group features over-age-60 respondents (who were not subject to these disruptions). As a result, controls for aggregate fluctuations in hours by parental status, which capture such variation, will have a larger impact in a sample with only under-age-60 respondents.

C.5 Geographic coverage

This section reports results for two different samples differentiated by their geographic coverage. In Table C9, the first and third panels recapitulate our baseline results for the full sample of 478 local areas. The second and fourth panels report results for an alternative sample comprised of only the 280 counties identified in the CPS.

The theme of Table C9 is that the labor supply responses are relatively weak in the latter, smaller subsample. Consider the specification with parental status-by-time controls. The hours response among mothers falls from 2.4 hours per week in the sample with all areas to 1.6 hours per week. Among fathers, the response is somewhat smaller and no longer statistically significant.

Table C9: Geographic Coverage

	Women: All Local Areas					
In-person × kids, ψ	0.582*	2.359***	-0.051	0.472	2.501***	0.096
	[0.304]	[0.634]	[0.672]	[0.326]	[0.654]	[0.751]
Number of obs.	447,899	447,899	447,899	447,899	447,899	447,899
	Women: CPS Counties					
In-person × kids, ψ	0.214	1.555*	0.127	-0.043	1.346	0.097
	[0.390]	[0.935]	[1.019]	[0.414]	[0.914]	[1.094]
Number of obs.	188,204	188,204	188,204	188,204	188,204	188,204
	Men: All Local Areas					
In-person × kids, ψ	0.566*	1.886***	-0.051	0.210	1.708***	-0.239
	[0.315]	[0.645]	[0.705]	[0.321]	[0.602]	[0.812]
Number of obs.	432,856	432,856	432,856	428,244	428,244	428,244
	Men: CPS Counties					
In-person × kids, ψ	0.129	1.089	-0.559	-0.266	0.507	-1.898
	[0.434]	[0.984]	[1.035]	[0.397]	[0.923]	[1.254]
Number of obs.	179,594	179,594	179,594	177,694	177,694	177,694
CPS Weights	No	No	No	Yes	Yes	Yes
Month × parent	No	Yes	Yes	No	Yes	Yes
Area × parent F.E.	No	No	Yes	No	No	Yes

Note: The first and third panels refer to our baseline sample of all local areas. The second and fourth panels are based on the sample of CPS-identified counties. Columns within each panel are differentiated by the inclusion of parental status-by-month and/or by-area controls. Standard errors are clustered at the geographic area level. *** indicates a p-value less than 0.01; ** a p-value between 0.01 and 0.05; and * a p-value between 0.05 and 0.10.

C.6 Industry and occupation controls

Next, we introduce controls for industry and occupation. Specifically, we include indicator variables to span 17 industries, each of which corresponds to a two-digit NAICS sector. We also include indicator variables to span 23 occupations, each of which corresponds to a two-digit SOC code. In the CPS, industry and occupation codes are available for labor force participants.

However, nonparticipants report industry and occupation only if they (i) are in the Outgoing Rotation Groups (ORGs) and (ii) have worked in the last 12 months. These restrictions severely limit the availability of industry and occupation codes (for instance, the ORG sample is just one-quarter of the CPS). Therefore, we introduce another indicator for the absence of an industry and occupation code (see Garcia and Cowan, 2024). Note that if the latter indicator is "on", it *perfectly* predicts the respondent's employment status (i.e., she is out of work).

Table C10 compares our baseline results (in the first and third panels) with estimates of equations (1)-(2) that include industry and occupation controls (in the second and fourth panels). With the added regressors, mothers' labor supply response is now estimated to be indistinguishable from zero. Estimates for fathers are less sensitive to the new controls, but even here the size of the coefficient is reduced by nearly one half if parental status-by-month effects are included.

One reason that the impact of industry and occupation controls varies by gender is that participation is a more active margin among women. Hence, the absence of industry and

Table C10: Industry and Occupation Controls

	Women: Baseline controls					
In-person × kids, ψ	0.582*	2.359***	-0.051	0.472	2.501***	0.096
	[0.304]	[0.634]	[0.672]	[0.326]	[0.654]	[0.751]
Number of obs.	447,899	447,899	447,899	447,277	447,277	447,27
	Women: With Industry and Occupation Controls					
In-person × kids, ψ	-0.168	0.518	0.292	-0.254	0.508	0.412
	[0.202]	[0.358]	[0.438]	[0.223]	[0.373]	[0.494]
Number of obs.	447,881	447,881	447,881	447,259	447,259	447,25
	Men: Baseline Controls					
In-person × kids, ψ	0.566*	1.886***	-0.051	0.210	1.708***	-0.239
	[0.315]	[0.645]	[0.705]	[0.321]	[0.602]	[0.812]
Number of obs.	432,856	432,856	432,856	428,244	428,244	428,24
	Men: With Industry and Occupation Controls					
In-person × kids, ψ	0.574***	1.073***	-0.061	0.430*	1.146***	0.189
	[0.215]	[0.378]	[0.553]	[0.240]	[0.425]	[0.663]
Number of obs.	432,775	432,775	432,775	428,163	428,163	428,16
CPS Weights	No	No	No	Yes	Yes	Yes
Month × parent	No	Yes	Yes	No	Yes	Yes
Area × parent F.E.	No	No	Yes	No	No	Yes

Note: The first and third panels report estimates based on equations (1)-(2). The second and fourth panels add controls for industry and occupation. Standard errors are clustered at the geographic area level. *** indicates a p-value less than 0.01; ** a p-value between 0.01 and 0.05; and * a p-value between 0.05 and 0.10.

occupation codes is a more important predictor of women's hours and leaves little else for in-person shares to account for. To illustrate, we find that nearly 80 percent of nonemployed (zero-hours) women in the Outgoing Rotation Groups do *not* report an industry and occupation, whereas the analogue among men is 65 percent.

C.7 Individual unobserved heterogeneity

In the main text, we found that controls for unobserved heterogeneity could eliminate the estimated effect of the in-person share on parents' relative hours. These specifications allowed for unobserved *group-level* differences. For instance, parental status-by-*area* effects capture regional differences in the propensities of parents to supply labor. Going one step further, we now replace the latter group-level controls with *individual* fixed effects. These fixed effects will still (implicitly) capture variation across parents and childless adults but also permanent differences in hours among adults within parental status.

The introduction of individual fixed effects is feasible because of the longitudinal dimension of the CPS. The sample design calls for each participant to be surveyed for four consecutive months and then, after an eight-month interregnum, for another four consecutive months. This design means that it is possible to measure how an individual's hours change as the in-person share in her area evolves. To this end, though, we modify our baseline sample (see Section 1.2) in two ways. First, whereas our baseline sample began in January 2020, we now admit survey participants if they joined the CPS in the fall of 2019. We observe these participants' hours before the pandemic began and then again, in fall 2020, after restrictions on in-person instruction were imposed. Second, whereas our baseline included any observations through December 2021, we now exclude participants who joined the CPS after May of that year. The reason is that, after the 2020-21 school year, restrictions on in-person instruction were removed. Therefore, if a participant joined the CPS in the summer or fall 2021, she faces no variation in the policy.

The results of estimation are shown in Table C11. In half of the columns, we run the canonical two-way fixed effects estimator that includes only month and individual fixed effects (in addition to the controls included in equation (1)). In the other half, we also include the parental status-by-*month* effects, which were found in the main text to yield larger coefficient estimates. The parental status-by-area effects are removed throughout so we might assess the extent to which individual fixed effects replicate the results based on the former.

The overall message of Table C11 conforms to that in the main text. In the presence of individual fixed effects, there is no statistically significant increase in parents' relative hours when in-person instruction resumes. (In two specifications, there are, somewhat oddly, significant decreases.) We see this result as complementary to the analysis in Section 3.1. Rather than rely on somewhat novel controls (such as parental status-by-area terms), Table C11 is based on a canonical specification. We are reassured that this approach yields the same basic qualitative conclusion.

Table C11: Models with Individual Fixed Effects

	Women			
In-person × kids, ψ	-0.473***	-0.533***	-0.461	-0.380
	[0.178]	[0.201]	[0.442]	[0.503]
Number of obs.	426,000	425,417	426,000	425,417
	Men			
In-person × kids, ψ	0.019	0.041	-0.101	0.319
	[0.220]	[0.260]	[0.507]	[0.632]
Number of obs.	411,220	406,717	411,220	406,717
CPS Weights	No	Yes	No	Yes
Month × parent F.E.	No	No	Yes	Yes

Note: Each column is conditioned on individual fixed effects. The period spans September 2019-December 2021 but with June-August omitted. See text for fuller discussion of sample construction. Standard errors are clustered at the geographic area level. *** indicates a p-value less than 0.01; ** a p-value between 0.01 and 0.05; and * a p-value between 0.05 and 0.10.

C.8 Married couples' hours worked

Thus far, we have analyzed individual labor supply. However, if members of a married couple coordinate their labor supply strategies, their individual responses are not necessarily indicative of the change in the household's total hours. For example, if a mother (father) works less after on-site instruction is suspended, the father (mother) may work more. As a result, if we run regressions separately by gender and add up the estimates, we may overstate the change in total hours among married households.

Therefore, we now estimate the response of a married couple's total hours worked to a shift in instruction format. To ensure that two self-identified married respondents within a family unit are indeed a couple, we require that each report that his/her married partner is "present." This restriction removes 3 percent of married respondents in our original sample. We retain any other married couple in which at least one member is part of our original sample.

Table C12 presents the results. The top panel reports the main estimates of interest, namely, the response of the couple's total hours to a shift from fully virtual to fully in-person. As a point of reference, the bottom two panels present separate estimates for the mothers and fathers who are in both the married couples sample and our original sample.[14] The regression specification for the bottom two panels is identical to that used in Section 3.2, i.e., equation (1) augmented with parental status-by-month effects. In the top panel, we extend this specification to include a full set of demographic controls for *each* member of the couple.

Our takeaways from Table C12 are as follows. First, a shift from fully virtual to in-person implies an increase in couples' total hours of work of between 3.3 and 5 per week (the larger of

[14] Eight percent of members in the married couples sample are not in our original (e.g., ages 21-59) sample. As a result, the number of couples listed in the top panel of the table exceeds the number of married women and married men listed in the bottom panels.

Table C12: Married Couple Hours of Work

	All Married		Married College		Married Noncollege	
	Married Couple					
In-person × kids, ψ	3.328***	5.047***	2.534	3.934**	5.111***	7.300***
	[1.062]	[1.194]	[1.691]	[1.963]	[1.721]	[1.939]
Number of obs.	218,257	218,227	66,360	66,346	99,945	99,938
	Women in Married Couple					
In-person × kids, ψ	1.949**	2.691***	2.465*	2.978*	2.447**	3.437***
	[0.811]	[0.844]	[1.425]	[1.590]	[1.127]	[1.176]
Number of obs.	209,895	209,567	64,691	64,597	95,150	95,043
	Men in Married Couple					
In-person × kids, ψ	1.581**	1.883***	0.877	1.906*	2.520**	3.115***
	[0.690]	[0.708]	[1.000]	[1.117]	[1.114]	[1.044]
Number of obs.	192,975	189,643	60,650	59,694	86,906	85,487
Weighted	No	Yes	No	Yes	No	Yes

Note: A married college (married noncollege) couple is one in which both members are college (noncollege) graduates. The unit of analysis in the top panel is the couple. In the other panels, it is the individual married respondent. The period is calendar years 2020 and 2021 but with June-August omitted. Parental status-by-month effects are included throughout. Standard errors are clustered at the geographic area level. *** indicates a p-value less than 0.01; ** a p-value between 0.01 and 0.05; and * a p-value between 0.05 and 0.10.

the two is based on weighted OLS). This result is in fact similar to the sum of responses for married men and married women, which range from 3.5 to 4.6 (see the bottom two panels). Thus, the individual-level regressions are quite informative about the household response.

Second, the distribution of the hours responses within the household varies by the couple's education. Mothers' labor supply adjustments account for much of the hours response of married, college educated couples. By contrast, in households with two noncollege educated spouses, the labor supply adjustments of mothers and fathers are similar to one another. A corollary of this result is that the education gradient in hours is more noticeable among *married* men. In the weighted OLS results, the difference between noncollege- and college-educated married men is 1.2 hours, which is twice as large as the gap between *all* noncollege- and college-educated men (see Table 4). This fact helps account for the relatively large response in hours among married households with noncollege-educated spouses.

One way of interpreting the role of schooling in Table C12 connects differences in labor supply behavior to differences in earnings opportunities. For instance, responding to a local school closure, a father may continue working if his earnings are highest, with his spouse allocating more time to childcare. Conversely, if parents' earnings are similar, they may take up childcare to a similar extent. Thus, the pattern in Table C12 may arise if a father's relative earnings (within the household) are *increasing* in his schooling, e.g., if a college-educated father is more likely to have earnings exceeding those of his spouse.

In fact, we find no support in the data for this narrative. To pursue this question, we drew on weekly earnings records from the CPS Outgoing Rotation Groups. The data are from 2019 and, therefore, capture the situation facing parents prior to the suspension of on-site instruction. We find, first, that roughly 70 percent of fathers earn more than their spouses regardless of college attainment. These figures echo results in Winkler et al. (2005), who use annual earnings from the CPS March Supplement. In addition, the father's share of couples' earnings is roughly two-thirds regardless, again, of college attainment. These moments reflect the tendency of fathers to have spouses with similar schooling, i.e., one partner's college premium is balanced by the other.[15]

One caveat to this finding is that current earnings may not fully reflect returns on market time. Since returns to experience are somewhat higher for men (see Munasinghe, Reif, and Henriques, 2008), a household may select the father for full-time work even if parents' current earnings are similar. Alternatively, our estimates in Table C12 may point to differences in

[15] Nearly 80 percent of married fathers with a college degree have college-educated spouses. Similarly, roughly three-quarters of noncollege-educated fathers have spouses with no more than a high school degree.

preferences for and/or norms around childcare. It is an open question, though, why these differences pertain only to college graduates.

D. Additional results from the ATUS

This appendix reports additional estimates from the American Time Use Survey (ATUS). To start, we examine each major activity category in Table D1. Specifically, the outcome variable is the total time for which that activity is reported as the *primary* activity. We do not find any significant relationship between local school closures and time allocated to market work, leisure,

Table D1: Instruction Format and Time Use Across Major Activity Categories

	Work	Leisure	Childcare, Primary	Childcare, Primary or Secondary	Home Production	Commute
			All			
In-person × kids, ψ	-5.182	5.038	-1.752	-14.003**	-0.023	-0.666
	[5.223]	[3.378]	[2.046]	[5.582]	[2.066]	[0.655]
Number of obs.	6,622	6,622	6,622	6,622	6,622	6,622
			Men			
In-person × kids, ψ	-6.395	-2.490	-3.211*	-14.528***	2.765	-0.237
	[6.958]	[5.613]	[1.876]	[5.468]	[3.743]	[1.299]
Number of obs.	3,067	3,067	3,067	3,067	3,067	3,067
			Women			
In-person × kids, ψ	-2.428	6.575	-0.743	-15.111**	-0.702	-0.242
	[6.156]	[4.191]	[2.596]	[6.833]	[3.238]	[0.627]
Number of obs.	3,478	3,478	3,478	3,478	3,478	3,478
			Noncollege			
In-person × kids, ψ	-7.840	6.289	-1.530	-2.211	0.805	-1.479
	[7.921]	[5.584]	[3.229]	[9.237]	[3.914]	[1.056]
Number of obs.	3,371	3,371	3,371	3,371	3,371	3,371
			College			
In-person × kids, ψ	0.467	5.256	-3.063	-25.572***	-2.401	0.371
	[6.024]	[4.372]	[1.924]	[5.265]	[2.861]	[0.972]
Number of obs.	3,178	3,178	3,178	3,178	3,178	3,178

Note: Each column by panel is a separate regression, with the implied number of hours per week spent in each activity as the dependent variable. Relative to equation (1), the specification also includes fixed effects for days of the week and parent status×month. Standard errors are clustered at the geographic area level. *** indicates a p-value less than 0.01; ** a p-value between 0.01 and 0.05; and * a p-value between 0.05 and 0.10.

home production, or commuting. Furthermore, we see no significant response in primary childcare among mothers and only a relatively modest adjustment among fathers. However, when we add up time spent in primary *or* secondary childcare, we observe a more substantial response. This result emerges because the reinstatement of in-person instruction reduced time spent in *secondary* childcare by 12 hours per week (compare the third and fourth columns in the top panel). This response was concentrated among college-educated parents, as shown in the bottom panel.

In Table D2, we report on time spent working from home while supervising children. (The response of *total* telework hours remains insignificant.) This margin was more active among women, especially among college-educated women. For instance, the return of on-site instruction reduced time in this activity by 9-10 hours per week among women with college degrees. Responses of college educated men and noncollege educated parents were insignificant.

Table D2: Working From Home While Supervising Children

	All		Men		Women	
			All			
In-person × kids, ψ	-5.937***	-4.796***	-3.866*	-3.289*	-7.265***	-6.472**
	[1.466]	[1.626]	[1.979]	[1.827]	[2.240]	[2.539]
Number of obs.	6,622	6,622	3,067	3,067	3,478	3,478
			Noncollege			
In-person × kids, ψ	-1.521	-1.009	-2.747	-2.599	0.287	0.609
	[2.154]	[2.024]	[3.660]	[3.255]	[3.079]	[2.998]
Number of obs.	3,371	3,371	1,602	1,602	1,677	1,677
			College			
In-person × kids, ψ	-7.328***	-6.432**	-3.231	-3.089	-10.268***	-9.156**
	[2.620]	[2.801]	[3.430]	[3.334]	[3.756]	[3.820]
Number of obs.	3,178	3,178	1,383	1,383	1,711	1,711
ATUS Weights	No	Yes	No	Yes	No	Yes

Note: Each column by panel is a separate regression, with the sample defined by the column header. The dependent variable is the implied number of hours per week where "work at home" is the primary activity and "childcare" is secondary. Relative to equation (1), the specification includes fixed effects for days of the week and parent status×month. Standard errors are clustered at the geographic area level. *** indicates a p-value less than 0.01; ** a p-value between 0.01 and 0.05; and * a p-value between 0.05 and 0.10.

E. Derivations and proofs

In this appendix, we first derive results for the model with nonparental care reported in Section 5. Second, we introduce the telework technology. Thus, we embed both telework and nonparental care within a single framework. We then proceed to present a fuller characterization of the market time decision for a given allocation of childcare time. Finally, we revisit the choice of parental v. nonparental care in this environment and characterize the effect of a change in on-site instruction time, g, on both market and childcare time.

E.1 Nonparental care

To start, we restate the parent's problem,

$$\max_{c,l,m,x} \alpha \ln c + \beta \ln l + (1-\alpha-\beta) \ln q$$

subject to the child development technology,

$$q = g^{\gamma}\left[\left(\mu^{1-\varphi} m^{\varphi} + (1-\mu)^{1-\varphi} x^{\varphi}\right)^{1/\varphi}\right]^{1-\gamma} \tag{E.1}$$

with $\varphi < 1$.[16] The time constraints of the adult and child are, respectively, given by

$$1 = l + m + n, \text{ and}$$

$$1 = g + m + x.$$

The price per unit of time for nonparental care is p, and the wage rate is w. For our purposes, we do not have to specify the full asset allocation problem, so we leave aside other details of the parent's budget set.

The (intra-temporal) first-order conditions may be condensed to two expressions, one for market time, n, and the other for nonparental time, x. The optimal choice of market time, originally reported in the main text, is restated here,

[16] Since this inequality is strict, we can establish that certain mappings are strictly monotone. If we were to allow for the special case of perfect substitutes ($\varphi = 1$), we would simply amend these results to reflect a weak monotonicity.

$$n = g + x - \frac{\beta}{\lambda w}. \tag{E.2}$$

The demand for nonparental time, x, satisfies

$$(1-\alpha-\beta)(1-\gamma)\frac{\nu^{1-\varphi}(1-g-x)^{\varphi-1} - x^{\varphi-1}}{\nu^{1-\varphi}(1-g-x)^{\varphi} + x^{\varphi}} = \lambda(w-p), \tag{E.3}$$

where λ is the marginal utility of consumption and $\nu \equiv \mu/(1-\mu)$.

We can now characterize the response of nonparental time, x, to a change in on-site time, g. The response of market time, n, will follow from equation (E.2). Taking logs of equation (E.3) and totally differentiating with respect to x and g yields

$$\frac{\mathrm{d}x}{\mathrm{d}g} = -\frac{1}{1+z(\xi;\varphi)}, \tag{E.4}$$

where $\xi \equiv x/m = x/(1-g-x)$ is the ratio of time in nonparental care to parental care and

$$z(\xi;\varphi) \equiv \frac{n(\xi)}{d(\xi)} \equiv \frac{(\nu\xi)^{\varphi-1} + (1-\varphi)\xi^{-1} - \varphi}{(\nu\xi)^{1-\varphi} + (1-\varphi)\xi - \varphi}. \tag{E.5}$$

The comparative static (E.4) is unambiguously negative if $w \geq p$, as shown next. Later, we also demonstrate why $\mathrm{d}x/\mathrm{d}g < 0$ may obtain even if $w < p$.

Lemma 1. *If $w \geq p$, nonparental care, x, declines in in-person time,* $\mathrm{d}x/\mathrm{d}g < 0$.

Proof. We must establish that $1 + z > 0$. To start, note from equation (E.3) that an interior solution under $w \geq p$ satisfies $\nu^{1-\varphi}m^{\varphi-1} \geq x^{\varphi-1}$, which implies $\nu\xi \geq 1$. It follows that the denominator in z is positive: $d(\xi) \equiv (\nu\xi)^{1-\varphi} + (1-\varphi)\xi - \varphi \geq (1-\varphi)(1+\xi) > 0$. Hence, $1 + z > 0$ if

$$(\nu\xi)^{\varphi-1} + (\nu\xi)^{1-\varphi} + (1-\varphi)(\xi + \xi^{-1}) - 2\varphi > 0. \tag{E.6}$$

Since $\xi \in \mathbb{R}^{+}$, the term $\xi + \xi^{-1}$ attains a minimum of 2 at $\xi = 1$. Likewise, if we define $\varsigma \equiv (\nu\xi)^{1-\varphi}$, then by the same logic, $\varsigma + \varsigma^{-1}$ is no smaller than 2. Hence, the left side of equation (E.6) has a minimum of $2 + 2(1-\varphi) - 2\varphi = 4(1-\varphi) > 0$. This confirms that $\mathrm{d}x/\mathrm{d}g < 0$. ■

Remark 1: The proof shows that if ξ is sufficiently high such that $\nu\xi \geq 1$, then $z > -1$. We can tighten this result slightly and establish that $z \geq 0$ obtains in one of two cases. First, equation (E.5) implies that $z > 0$ where $0 \geq \varphi$. Second, if $\varphi \in (0,1)$, there is a region of ξ on each side of $\xi = 1/\nu$ where $z \geq 0$. This can be confirmed using the following properties of z: (i) the numerator, $\mathcal{n}(\xi)$, is decreasing and convex in ξ, crossing zero once from *above* at some ξ_n; (ii) the denominator, $d(\xi)$, is increasing and concave in ξ, crossing zero once from *below* at some ξ_d; and (iii) wherever $d(\xi) \leq 0$, we have $(\nu\xi)^{1-\varphi} < \varphi < 1$, which implies $(\nu\xi)^{\varphi-1} > 1/\varphi > 1$ and, therefore, $\mathcal{n}(\xi) > 0$. It follows that $d(\xi)$ reaches zero first (e.g., $\xi_d < \xi_n$), and $z > 0$ in the region $[\xi_d, \xi_n]$. Moreover, since z is strictly positive at $\nu\xi = 1$ (see equation (E.5)), this region must encase the area around $\xi = 1/\nu$. Finally, to the right of ξ_n, we still have $z > -1$ by Lemma 1, ■

To appreciate the empirical content of these results, consider the two scenarios highlighted in Remark 1 that are consistent with $\mathrm{d}x/\mathrm{d}g < 0$. The first is $z > 0$, which means $\mathrm{d}x/\mathrm{d}g \in (-1,0)$: nonparental time declines in on-site time but less than one for one. In other words, nonparental care represents a means of adjusting to school closures but not the only one. This case would appear to agree with the (limited) evidence from the ATUS. The second is $z \in (-1,0)$, which implies $\mathrm{d}x/\mathrm{d}g < -1$. Empirically, this scenario faces a challenge: if x falls by more than g rises, then m must *rise* (since the parent's time constraint implies $0 = \mathrm{d}g + \mathrm{d}m + \mathrm{d}x$). We are hard pressed to find any indication that parental time *rose* when on-site instruction returned.

One scenario on which Lemma 1 is silent is the case where $w < p$. Under this assumption, ξ must be sufficiently low in that $\nu\xi < 1$ (see equation (E.3)). Intuitively, a high price p diminishes demand for nonparental care. Even so, outside of one specific region of the parameter space, the comparative static (E.4) remains negative. For starters, it remains true that $z > 0$ and, therefore, $\mathrm{d}x/\mathrm{d}g < 0$, whenever $0 \geq \varphi$. Next, even if $\varphi \in (0,1)$, we still have $\mathrm{d}x/\mathrm{d}g < 0$ so long as $\xi >$

ξ_d (see Remark 1). For ξ less than ξ_d, the steps of the proof of Lemma 1 can be adapted to show that $z < -1$ and, therefore, $0 < \mathrm{d}x/\mathrm{d}g$.[17] But just as the case with $\mathrm{d}x/\mathrm{d}g < -1$ fell short empirically, this outcome, too, does not find support in the data: our time use results do not point to an increase in nonparental care after on-site instruction resumed.

The comparative static for market time follows from equations (E.2) and (E.4),

$$\frac{\mathrm{d}n}{\mathrm{d}g} = \frac{z(\xi;\varphi)}{1 + z(\xi;\varphi)}. \tag{E.7}$$

The sign of this comparative static is positive—more on-site time supports higher labor supply—unless $z \in (-1,0)$. As we saw above, a $z \in (-1,0)$ has the awkward implication that parental time rises with on-site time. A higher m requires in turn a reduction in labor supply, n. Outside of this singular case, $\mathrm{d}n/\mathrm{d}g > 0$.

We next establish that z, and by extension $\mathrm{d}n/\mathrm{d}g$, is a monotone function of φ given an initial solution ξ. That is, we can characterize the map from φ to z local to an initial optimum. This approach to comparative statics on φ is akin to the "normalization" advocated by La Grandville (1989) and Klump and La Grandville (2000) when one works with CES functions (see Cantore and Levine, 2014, on this point). To perturb φ but hold ξ fixed, the share parameter, $\nu \equiv \mu/(1-\mu)$, is adjusted as needed.

Lemma 2. *For fixed ξ, there is a monotone mapping between φ and the comparative static of market time $\mathrm{d}n/\mathrm{d}g$. The mapping is downward sloping (i.e., $\mathrm{d}n/\mathrm{d}g$ is decreasing in φ) if $w > p$. Conversely, it is upward sloping if $w < p$.*

[17] The intuition is as follows. A smaller g implies a higher m all else equal. But if m and x are gross substitutes, a higher m lowers the marginal value of x, i.e., $\partial^2 \ln q/\partial x \partial m < 0$. The marginal value of m also falls, but the former effect dominates if care is strongly m-intensive (ξ is very small). As a result, x is reduced and $\mathrm{d}x/\mathrm{d}g > 0$. Conversely, if care is strongly x-intensive, the roles of x and m are reversed in this argument, which yields $\mathrm{d}m/\mathrm{d}g > 0$.

Proof. As a first step, we determine how ν must be adjusted so that any initial optimum ξ still holds after φ is perturbed. Recall $\varsigma \equiv (\nu\xi)^{1-\varphi}$ and rewrite the first-order condition (E.3) as

$$\frac{\varsigma - 1}{\varsigma + (x/(1-g-x))} = (1-g-x) \cdot \frac{\lambda(w-p)}{(1-\alpha-\beta)(1-\gamma)}.$$

This expression indicates that to hold x fixed in the face of a shift in φ, it is necessary (and sufficient) to adjust ν such that ς does not change. Therefore, a perturbation $\mathrm{d}\varphi$ requires that

$$\mathrm{d}\nu = \mathrm{d}\varphi \cdot \nu \ln(\xi\nu)/(1-\varphi). \tag{E.8}$$

Now totally differentiate equation (E.5) with respect to ν and φ subject to the restriction (E.8) and given an initial optimum ξ_0 and the associated value of $\varsigma = \varsigma_0$. The comparative static is

$$\left.\frac{\mathrm{d}z}{\mathrm{d}\varphi}\right|_{\xi=\xi_0} = -\frac{(1+\xi_0^{-1})(\varsigma_0 - 1) + (1+\xi_0)(1-\varsigma_0^{-1})}{(\varsigma_0 + (1-\varphi)\xi_0 - \varphi)^2}. \tag{E.9}$$

Finally, equations (E.7) and (E.9) yield

$$\begin{aligned} \left.\frac{\mathrm{d}n}{\mathrm{d}\varphi}\right|_{\xi=\xi_0} &= \big(1 + z(\xi_0;\varphi)\big)^{-2} \left.\frac{\mathrm{d}z}{\mathrm{d}\varphi}\right|_{\xi=\xi_0} \\ &= -\frac{(1+\xi_0^{-1})(\varsigma_0 - 1) + (1+\xi_0)(1-\varsigma_0^{-1})}{\big((1-\varphi)(1+\xi_0^{-1}) + (\varsigma_0 - 1) + (1-\varphi)(1+\xi_0) - (1-\varsigma_0^{-1})\big)^2}. \end{aligned}$$

Equation (E.9) is negative for any $\varsigma_0 > 1$. The latter outcome arises if $w > p$, which confirms that the mapping between φ and n is monotonically decreasing in this case. Conversely, if $w \leq p$, then $\varsigma_0 \leq 1$, and $\mathrm{d}n/\mathrm{d}\varphi \geq 0$. ■

The direction of the effect of φ on $\mathrm{d}n/\mathrm{d}g$ is shown to hinge on the sign of $w - p$. To see why, suppose first that the price of nonparental time is relatively low, $w > p$. In this context, if the two forms of care become more substitutable, a parent takes up nonparental care at a higher rate. This can be seen in equation (E.4): a lower z (due to a higher φ) implies that x is more responsive to g. The adoption of more nonparental care after a fall in g in turn mitigates the decline in market time, i.e., $\mathrm{d}n/\mathrm{d}g$ is smaller. Conversely, suppose $w < p$. By the same logic, a parent now takes up *parental* care at a higher rate if the two forms of care become more substitutable. But if parental care rises more after a fall in g, market time must fall *more*, i.e., $\mathrm{d}n/\mathrm{d}g$ is larger.

To assess these two cases, it is helpful to remember that we have a "target" in mind for $\mathrm{d}n/\mathrm{d}g$: the regression results indicate that this comparative static is positive but not far from zero. From Lemma 2, a small positive $\mathrm{d}n/\mathrm{d}g$ requires a small (indeed, negative) value of φ if $w < p$. But as $\varphi \to -\infty$, one may verify that $z \to \xi^{-1}$ and, therefore, $\mathrm{d}n/\mathrm{d}g \to (1+\xi)^{-1}$ (for given ς). Thus, the comparative static is bounded strictly away from zero (and dependent wholly on the value of ξ).[18] Alternatively, if $w > p$, then $\mathrm{d}n/\mathrm{d}g$ is reduced as φ is raised. Moreover, as $\varphi \to 1$ (for given ς), z must reach zero from above. Therefore, it is possible to attain an arbitrarily small positive value of $\mathrm{d}n/\mathrm{d}g$ for φ sufficiently high. In the main text, we showed quantitatively that this insight substantially narrows the range of feasible φs. Proposition 1 formalizes this argument.

Proposition 1. *For* $\mathrm{d}n/\mathrm{d}g > 0$ *sufficiently near zero, it must be that* $w > p$ *and* $\varphi > (1+\xi)^{-1}$.

Proof. Consider first the special case where $\mathrm{d}n/\mathrm{d}g = 0$. From equations (E.5) and (E.7), this requires $\varphi = \varphi_0 \equiv (\varsigma^{-1} + \xi^{-1})/(1+\xi^{-1})$. Note first that $\varphi_0 < 1$ only if $\varsigma > 1$, which is in turn an optimum only if $w > p$. Next, dropping ς^{-1} from φ_0 yields the sought-after lower bound,

$$\varphi = \frac{\varsigma^{-1} + \xi^{-1}}{1+\xi^{-1}} > \frac{1}{1+\xi}. \tag{E.10}$$

More generally, let the comparative static take the value $\delta/(1+\delta) > 0$ with $\delta \in \mathbb{R}^+$. It follows from equation (E.7) that $z = \delta$, which means φ satisfies

$$\varphi = \frac{\xi^{-1} + \varsigma^{-1} - \delta(\varsigma + \xi)}{1 + \xi^{-1} - \delta(1+\xi)}.$$

[18] Intuitively, if the two forms of care are gross complements, a shift in g always prompts the parent to adjust on *both* margins of care. Therefore, some market time is sacrificed to raise m.

Again, one may show that this respects the fact that $\varphi < 1$ only if $\varsigma > 1 \Leftrightarrow w > p$. Therefore, we proceed under $\varsigma > 1$. Now, to a first order around $\delta = 0$, this solution for φ is given by

$$\varphi = \frac{1}{1+\xi} + \left\{\frac{\varsigma^{-1}}{1+\xi^{-1}} - \frac{(1+\xi)(1-\varsigma^{-1}) + (1+\xi^{-1})(\varsigma - 1)}{(1+\xi^{-1})^2}\delta\right\}.$$

Consider the difference between the two terms inside brackets. The bound in equation (E.10) will still apply if this difference is positive, which will in turn obtain for all δ such that

$$\frac{1}{(\varsigma+\xi)(\varsigma-1)} > \delta. \tag{E.11}$$

In this sense, the bound applies for δ sufficiently small. ■

To illustrate the result in equation (E.11), fix $\xi = 1.3$ (which is approximately what we assume in the main text) and consider $\varphi = 2/3$. Further, suppose $\mu = 4/5$ is consistent with the choice of $\xi = 1.3$ (given $\varphi = 2/3$, wage rate w, price p, and so on). Therefore, $\nu \equiv \mu/(1-\mu) = 4$ and $\varsigma = 1.7325$. Equation (E.11) then requires $\delta < 0.45$, or $\mathrm{d}n/\mathrm{d}g = \delta/(1+\delta) < 0.31$, which is easily satisfied. Importantly, the upper bound on δ *rises* at lower values of μ.

The final result of this section demonstrates that the value of φ also mediates the response of market work to variation in other parameters. To illustrate this point, we consider a decrease in the price of nonparental care, p, as would be implied by the subsidies provided in the pandemic period. We will see that the same logic applies if there were an increase in the wage, w. Each of these is interpreted as a temporary change. Therefore, we treat the marginal utility of consumption, λ, as fixed.[19] In light of Proposition 1, we will also assume $w > p \Leftrightarrow \varsigma > 1$ in what follows.

Proposition 2. *For fixed ξ, the absolute size of the response of market time to a (temporary) reduction in the price of nonparental care increases in φ.*

[19] A permanent change would have an income effect that attenuates the increase in labor supply.

Proof. As a first step, we establish that the price elasticity of demand for nonparental care increases in φ. Total differentiation of equation (E.3) yields

$$\left\{(1-\varphi)\frac{\xi\varsigma+1}{\varsigma-1}+\varphi\xi\frac{\varsigma-1}{\varsigma+\xi}\right\}\mathrm{d}\ln x=-\frac{p}{w-p}\mathrm{d}\ln p.$$

Clearly, the left-hand side term is positive for any $\varphi\in[0,1]$ and $\varsigma>1$. Now rearrange this term to see that

$$\Psi(\varphi;\varsigma,\xi)\equiv(1-\varphi)\frac{\xi\varsigma+1}{\varsigma-1}+\varphi\xi\frac{\varsigma-1}{\varsigma+\xi}=\frac{\xi\varsigma+1}{\varsigma-1}-\varphi\varsigma\frac{(1+\xi)^2}{(\varsigma-1)(\varsigma+\xi)}.$$

Thus, Ψ is also positive for any $\varphi<0$ and $\varsigma>1$. Next, we isolate the response of x on the left-hand side,

$$\frac{\mathrm{d}\ln x}{\mathrm{d}\ln p}=-\frac{p}{w-p}\Psi(\varphi;\varsigma,\xi)^{-1}<0, \tag{E.12}$$

and note from above that $\partial\Psi/\partial\varphi<0$ for any $\varsigma>1$. Therefore, a lower price stimulates a bigger increase in nonparental time at higher φ, i.e., when the two forms of care are more substitutable. From equation (E.2), a change in x translates, all else equal, one-for-one into market time, n. Thus, at higher φ, a reduction in p yields a larger increase in n. ■

The analysis of a temporary wage increase follows the same steps as above. Indeed, in equation (E.12), one merely needs to swap the term $-\frac{p}{w-p}<0$ for $\frac{w}{w-p}>0$ to obtain the result.

E.2 Market work and parental care under telework

The telework technology maps time spent in parental care, m, and market work, n, to the total time that has passed, $\mathcal{t}(m,n)=(m^\rho+n^\rho)^{1/\rho}$, where $\rho>1$. The parent's time constraint becomes $l+\mathcal{t}(m,n)=1$. The remainder of the model outlined Section E.1 is unaffected.

The choice of n given m is given by equation (11) in the main text and restated here,

$$1 = \frac{\beta}{\lambda w} \frac{\partial t / \partial n}{1 - t(m,n)} = \frac{\beta}{\lambda w} \frac{(m^\rho + n^\rho)^{1/\rho - 1} n^{\rho - 1}}{1 - (m^\rho + n^\rho)^{1/\rho}}. \tag{E.13}$$

Notably, this result is independent of nonparental care, x. Rather, equation (E.13) implies a solution for market work in terms of only parental care.[20] Hence, we may analyze the association between market and parental time in isolation (in Appendix E.2) and then take up how *non*parental care shapes the choice of parental time (in Appendix E.3).

In particular, this section will develop how ρ shapes the response of market time given a shift in parental time. This analysis has two payoffs. First, to the extent a school policy induces movement in m, we articulate the conditions under which the response of market time is attenuated by a higher ρ. Second, the map between ρ and $\mathrm{d}n/\mathrm{d}m$ suggests a means to *identify* the former based on data on market and parental time alone. More exactly, we show that the co-movement of n and m can be used to inform the choice of ρ even apart from any shift in school policy.

As we have done before, our approach is to evaluate the response of a comparative static given a fixed initial optimum. In this context, where we examine a shift in ρ, another parameter in equation (E.13) must adjust to induce the same choice of n given m. In other words, if ρ were different than in some baseline state, we would infer that another parameter must also be different to rationalize the choice of n (given m). For the purposes here, we treat β as that free parameter.

To proceed, consider again the market time response to a shift in parental care. Since $\rho > 1$ enables one to bundle market tasks with the marginal unit of childcare, it is natural to suspect that, at a *higher* ρ, increases in m induce smaller declines in n, i.e., $\mathrm{d}n/\mathrm{d}m$ increases in ρ. The

[20] That a unique solution for n exists follows from three observations. First, the right-hand side (r.h.s.) of equation (E.13) is zero at $n = 0$. Second, the r.h.s. tends to infinity as $n \to \overline{n}(m) \equiv (1 - m^\rho)^{1/\rho}$. Third, the r.h.s. increases in n over $n \in [0, \overline{n}(m))$. Piecing these points together establishes a single crossing.

next result sheds light on the conditions under which this intuition obtains. To state the result, define $\phi(\mathscr{t}) \equiv \mathscr{t}(m,n)/\big(1-\mathscr{t}(m,n)\big)$ and $\mathscr{m} \equiv m/n$.

Proposition 3. *The comparative static* $\mathrm{d}n/\mathrm{d}m$ *increases in* ρ *if* $[\phi(\mathscr{t}) - \rho + 1] \cdot \ln \mathscr{m} \leq 0$.

Proof. The comparative static $\mathrm{d}n/\mathrm{d}m$ may be expressed as

$$\frac{\mathrm{d}n}{\mathrm{d}m} = -\frac{1}{\mathscr{m}} \cdot \frac{\phi(\mathscr{t}) - (\rho - 1)}{\mathscr{m}^{-\rho}\phi(\mathscr{t}) + (\rho - 1)}. \tag{E.14}$$

The sign of the marginal effect of ρ on $\mathrm{d}n/\mathrm{d}m$ is then found to be,

$$\operatorname{sgn}\left(\frac{\mathrm{d}}{\mathrm{d}\rho}\frac{\mathrm{d}n}{\mathrm{d}m}\right) = \operatorname{sgn}\left\{\frac{\rho-1}{\rho^2}\frac{\delta(\mathscr{m})}{1-\mathscr{t}} + (\mathscr{m}^{\rho}+1) - \big(\phi(\mathscr{t}) - (\rho-1)\big)\ln \mathscr{m}\right\}, \tag{E.15}$$

where $\delta(\mathscr{m}) \equiv (\mathscr{m}^{\rho}+1)\ln(\mathscr{m}^{\rho}+1) - \mathscr{m}^{\rho}\ln \mathscr{m}^{\rho} > 0$. This result implies that, for $\mathrm{d}n/\mathrm{d}m$ to increase in ρ, it is sufficient that $\big(\phi(\mathscr{t}) - (\rho-1)\big) \cdot \ln \mathscr{m} \leq 0$.■

This sufficient condition obtains under one of two scenarios. First, suppose $\mathscr{m} \leq 1$. Then we require $\phi(\mathscr{t}) \geq \rho - 1 \Leftrightarrow \mathscr{t} \geq 1 - 1/\rho$. For some $\mathscr{t}$, the latter holds if ρ is not too large. Thus, this case represents a relatively modest deviation from the baseline model with $\rho = 1$. Consistent with this point, an increase in m is still met by a reduction in n, as happens if $\rho = 1$. However, whereas $\mathrm{d}n/\mathrm{d}m = -1$ at $\rho = 1$, market time falls less than one for one if $1 < \rho < \phi(\mathscr{t}) + 1$.

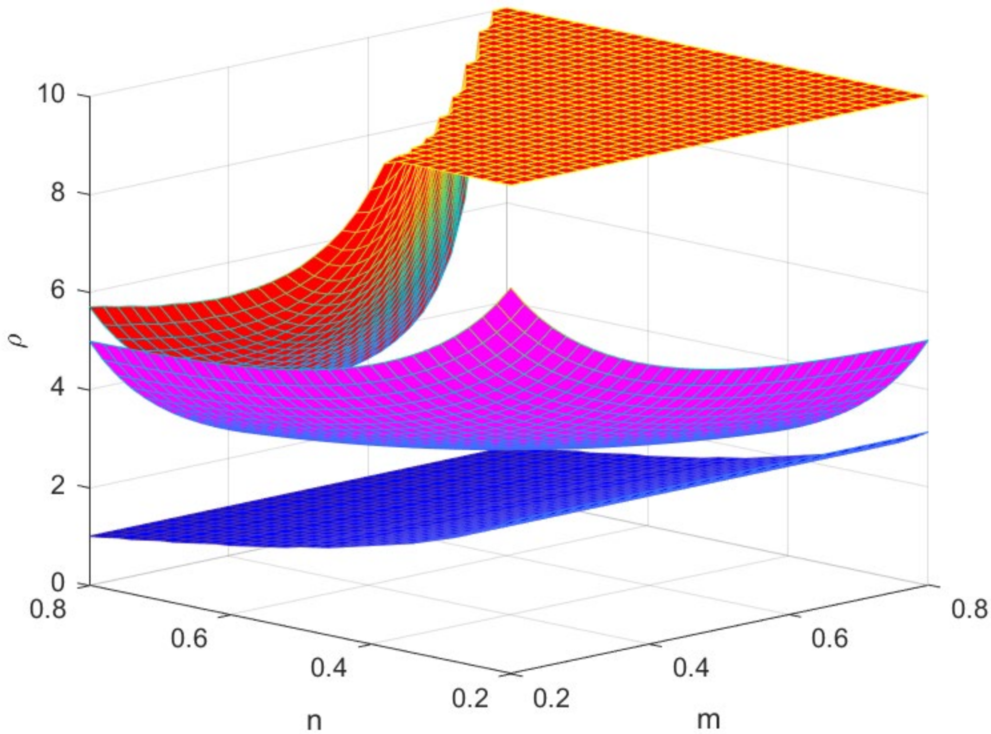
Alternatively, if $\mathscr{m} \geq 1$, then ρ must be sufficiently large in that $\phi(\mathscr{t}) + 1 < \rho$. This is a somewhat peculiar but instructive case. Under this restriction on ρ, equation (E.14) indicates that m and n *rise* together, $\mathrm{d}n/\mathrm{d}m > 0$. With ρ so high, new market work can be bundled to such an extent with the marginal parental care task that it is optimal to elevate n whenever m rises. Now, if ρ were raised further, it would seem this argument applies with greater force: $\mathrm{d}n/\mathrm{d}m$ should rise. This intuition is right if $\mathscr{m} \geq 1$, in which case more parental care time extends m (further) beyond n and thereby creates the necessary scope for many new work tasks to be paired with (new) parental care tasks.[21]

In either scenario, a higher slope, $\mathrm{d}n/\mathrm{d}m$, maps to a higher ρ. This result offers a means to identify ρ even outside of the pandemic period. In general, the observed change in market time per hour change in parental time constitutes a moment that is—seen through the lens of the model—strongly informative of ρ. Estimation of the model's structural parameters (by, for instance, method of simulated moments) lies outside the scope of this paper, but we hope this analysis provides a guidepost for future research.

Finally, as Proposition 3 identifies only a *sufficient* condition, it is also helpful to inspect numerically how $\mathrm{d}n/\mathrm{d}m$ depends on ρ. One perspective on this is shown in Figure E.1. For each pair (m, n), we use equation (E.14) to compute the range of ρ over which $\mathrm{d}n/\mathrm{d}m$ would rise if ρ were raised. The upper plane (in red/orange) shows the upper bound of this range (though it is truncated at $\rho = 10$), and the lower plane (in blue) shows the lower bound. Thus, beginning from any ρ between these two planes, $\mathrm{d}n/\mathrm{d}m$ increases in ρ. As anticipated by Proposition 3, the upper bound is tighter at lower $\mathcal{m} \equiv m/n$, although even here $\mathrm{d}n/\mathrm{d}m$ generally increases in ρ up to at least $\rho \approx 4$. Finally, the monotonicity of $\mathrm{d}n/\mathrm{d}m$ implies that the space between the two planes may be divided into a lower region where $\mathrm{d}n/\mathrm{d}m < 0$ and an upper region where $\mathrm{d}n/\mathrm{d}m \geq 0$. The plane shown in purple in Figure E.1 is the largest ρ consistent with $\mathrm{d}n/\mathrm{d}m < 0$.

[21] This logic need not apply if $\mathcal{m} < 1$. Rather, if m is sufficiently below n, then at higher ρ, any new parental care time is more fully absorbed by time *already* allocated to market work. In other words, the scope for multi-tasking is being "maxed out" in this setting, implying that n rises by less when m is increased.

Figure E.1



Note: Beginning from a ρ between the upper (red/orange) and lower (blue) planes of the figure, $\mathrm{d}n/\mathrm{d}m$ will increase in ρ. The middle plane (purple) is the largest ρ such that $\mathrm{d}n/\mathrm{d}m < 0$. These planes are calculated based on equation (E.14).

E.3 Integrating nonparental care into a model with telework

While the connection between n and m is of interest in its own right, our larger aim is to use the latter to characterize the response of outcomes to a change in in-person time, g. Unlike in the case where $x \equiv 0$ but $\rho > 1$, there is no longer a one-to-one map from m to g. Therefore, the comparative static of n with respect to m reported in equation (E.14) is not sufficient to determine the impact of the policy.[22] Instead, we must pair this result with how m responds to g—the topic to which we now turn.

[22] In addition, and unlike in the case with $\rho = 1$ but $x > 0$ in the main text, there is nontrivial map from m to n.

Given access to a telework technology, the optimality condition that characterizes the allocation of time across modes of childcare becomes,

$$\frac{\nu^{1-\varphi}m^{\varphi-1}-x^{\varphi-1}}{\nu^{1-\varphi}m^{\varphi}+x^{\varphi}}=\frac{\lambda}{(1-\alpha-\beta)(1-\gamma)}\left[w\left(\frac{m}{n}\right)^{\rho-1}-p\right]. \tag{E.16}$$

The left-hand side (l.h.s.) of equation (E.16) reproduces equation (E.3) and is independent of ρ. This expression captures the marginal value of parental care relative to that of nonparental care and is declining in m (owing to the concavity of the utility function). The telework technology does play a role on the right-hand side (r.h.s.), which measures the opportunity cost of parental care time. The latter is increasing in m/n, reflecting a key property of $\mathcal{t}(m,n)$: to add an hour of parental time when m/n is already high requires sacrificing more market time.[23] The shape of the r.h.s. with respect to the *level* of m, however, also depends on how market time responds to higher parental care time according to the first-order condition (E.13). In fact, while it is possible (at high ρ) that n grows with m, it always rises less than one for one, ensuring that m/n is increasing in m.[24] Accordingly, with the l.h.s. declining in m and the r.h.s. increasing in m, one can establish a unique solution for parental-care time.

We now address the comparative static $\mathrm{d}m/\mathrm{d}g$ implied by equation (E.16). Totally differentiating the latter with respect to m and g and using equation (E.13) yields

$$\frac{\mathrm{d}m}{\mathrm{d}g}=-\frac{z(\xi)}{z(\xi)+1+(\rho-1)\Omega(\xi,m,n)}. \tag{E.17}$$

where

$$z(\xi)\equiv\frac{n(\xi)}{d(\xi)}\equiv\frac{(\nu\xi)^{\varphi-1}-\varphi+(1-\varphi)\xi^{-1}}{(\nu\xi)^{1-\varphi}-\varphi+(1-\varphi)\xi}$$

and

[23] More formally, the marginal rate of transformation of market time for a unit of parental time increases in m/n.
[24] To see this, differentiate m/n with respect to m and take account of $\mathrm{d}n/\mathrm{d}m$ in equation (E.14).

$$\Omega(\xi, m, n) \equiv \underbrace{\frac{(\nu\xi)^{1-\varphi} - 1 + (1 - (\nu\xi)^{\varphi-1})\xi}{(\nu\xi)^{1-\varphi} - \varphi + (1-\varphi)\xi}}_{\equiv\omega(\xi)} \cdot \frac{\mathcal{m}^{-\rho} + 1}{\mathcal{m}^{-\rho} + (\rho - 1)/\phi(\mathcal{t})} \cdot \frac{w\mathcal{m}^{\rho-1}}{w\mathcal{m}^{\rho-1} - p},$$

with $\phi(\mathcal{t}) \equiv \mathcal{t}/(1-\mathcal{t})$ and $\mathcal{m} \equiv m/n$. Equation (E.17) has two parts. The first is $z(\xi)$, which pins down the comparative static if $\rho = 1$ (as in Section E.1) The model with a telework technology ($\rho > 1$) adds a second term, $\Omega(\xi, m, n)$.

The manner in which Ω enters equation (E.17) suggests that the response of parental childcare is attenuated under $\rho > 1$. This is indeed the case, as we show next.

Proposition 4. *Given any initial choice of* (m, n, x)*, the absolute size of the response of parental time,* m*, to a change in on-site instruction time,* g*, is lessened if* $\rho > 1$.

Proof. First, we inspect the sign of Ω. Consider its leading term $\omega(\xi)$. In the numerator of $\omega(\xi)$, the expressions $(\nu\xi)^{1-\varphi} - 1$ and $1 - (\nu\xi)^{\varphi-1}$ must have the same sign. Moreover, the sign of $(\nu\xi)^{1-\varphi} - 1$ must agree with that of $w\mathcal{m}^{\rho-1} - p$, which appears on the far right of Ω, because the two are linked via the first-order condition (E.16). Accordingly, the sign of Ω hinges on the sign of the denominator of $\omega(\xi)$, which happens to be $d(\xi)$.

Regarding $d(\xi)$, there are two cases to consider. First, suppose $d(\xi) > 0$, which implies $\Omega > 0$. It also follows that $z(\xi) + 1 > 0$ (see Lemma 1). Therefore, the presence of Ω in equation (E.17) attenuates the absolute size of the response of parental time relative to the $\rho = 1$ model. Conversely, if $d(\xi) < 0$, then each term in equation (E.17) is negative, namely, $z(\xi) < z(\xi) + 1 < 0$ (see Lemma 1) and $\Omega < 0$. Thus, the presence of Ω again adds to the absolute size of the denominator in equation (E.17) and, therefore, reduces the magnitude of the comparative static. ■

Intuitively, the comparative static is attenuated under $\rho > 1$ because the opportunity cost of parental time now increases in m. As m grows (after a fall in g), it displaces more market time, since the marginal childcare task cannot be so easily done concurrently with market work. This property mitigates demand for parental time. In the baseline where $\rho = 1$, parental time also displaces market time but at a constant rate (one for one). Therefore, the marginal cost of parental time is constant (and equal to $w - p$).

At this point, it is helpful to take stock of the results thus far. Equation (E.14) presents the response of market time, m, to a shift in parental time, n. Equation (E.17) then reports the response of parental time to a shift in on-site time, g. The market time response to higher on-site time, $\mathrm{d}n/\mathrm{d}g$, is then the product of these two comparative statics. It follows that $\mathrm{d}n/\mathrm{d}g \approx 0$ emerges if $\mathrm{d}n/\mathrm{d}m \approx 0$ *or* $\mathrm{d}m/\mathrm{d}g \approx 0$. The former case arguably better fits the experience of college graduates, whose adjustments to parental time (see Section 4) appear to have had a limited impact on market time. By contrast, the latter case is a more apt description of the time use data for noncollege graduates: neither their parental nor market time responded greatly. Thus, by integrating remote work and nonparental supervision into a single framework, the model is able to engage the data for each group.[25]

To conclude, we now characterize more generally how φ shapes the comparative static in equation (E.17). To allow for $\mathrm{d}m/\mathrm{d}g \approx 0$ (and, therefore, z arbitrarily close to zero), we restrict attention to the case with $\nu\xi > 1$ (see Proposition 1). We then show that, if $z > 0$, an increase in on-site time, g, reduces parental time but to a lesser extent at higher φ.[26] Moreover, this smaller drop in parental time passes through to higher market time at less than a one-for-one rate so long as ρ satisfies the bound in Proposition 3. In summary, a higher φ attenuates the response of market

[25] This narrative allows that the structural parameters of the model (e.g., ρ and φ) may vary by educational attainment. The reason for this takes us beyond the scope of this paper, but one possibility is that the noncollege group had assembled more extensive nonparental networks (perhaps prior to the pandemic). Thus, while we take the parameters as given, it is possible that they reflect earlier investments in such networks.
[26] Alternatively, if $z \in (-1,0]$, m and g *rise* together, and the increase in parental time is larger at higher φ. In either case, $\mathrm{d}m/\mathrm{d}g$ increases in φ. However, as discussed in Appendix E.1, the data do not favor this scenario.

time to a shift in on-site time. This outcome echoes the result in Section E.1 and thus demonstrates how it may be extended to a more general environment with both telework and nonparental care.

Proposition 5. *Suppose the initial optimum satisfies $\nu\xi > 1$. An increase in φ elevates the response of parental care time, m, to a change in on-site instruction time, g.*

Proof. Consider first how Ω responds to the change in φ. For a given initial optimum, this boils down to evaluating how ω, the leading term in Ω, responds. Totally differentiating ω and applying the mapping (E.8) between φ and ν (to ensure the initial optimum is unchanged),[27] we have that

$$\frac{\mathrm{d}\Omega}{\mathrm{d}\varphi} = \frac{1+\xi}{(\nu\xi)^{1-\varphi} - 1 + (1-\varphi)(1+\xi)} \cdot \Omega > 0. \tag{E.18}$$

The denominator on the right side is $d(\xi)$. We have shown $d(\xi) \lesseqgtr 0 \Rightarrow \Omega \lesseqgtr 0$, that is, the sign of Ω always agrees with the sign of $d(\xi)$. Hence, given $\nu\xi > 1$, equation (E.18) is positive.

Next, totally differentiating equation (E.17) and using the definition of $z(\xi)$ yields

$$\frac{\mathrm{d}}{\mathrm{d}\varphi}\left(\frac{\mathrm{d}m}{\mathrm{d}g}\right) = -\frac{\frac{\mathrm{d}z}{\mathrm{d}\varphi} - \xi^{-1}(\rho-1)\frac{\mathrm{d}\Omega}{\mathrm{d}\varphi}}{[z+1+(\rho-1)\Omega]^2}. \tag{E.19}$$

The comparative static $\mathrm{d}z/\mathrm{d}\varphi$ was reported in equation (E.9) and is repeated here for convenience,

$$\frac{\mathrm{d}z}{\mathrm{d}\varphi} = -\frac{(1-(\nu\xi)^{\varphi-1})(1+\xi) + ((\nu\xi)^{1-\varphi}-1)(1+\xi^{-1})}{[(\nu\xi)^{1-\varphi} + \xi - \varphi(\xi+1)]^2}.$$

Noting that $\mathrm{d}z/\mathrm{d}\varphi < 0$ (since $\nu\xi > 1$) and using $\mathrm{d}\Omega/\mathrm{d}\varphi > 0$, we conclude that the comparative static in equation (E.19) is positive.■

Additional References

[27] We maintain that if φ is perturbed, ν is adjusted to hold $\varsigma \equiv (\nu\xi)^{1-\varphi}$ fixed given $\xi \equiv x/m$. As a result, a new φ leaves the l.h.s. of equation (E.16) unaltered, which confirms there is no inducement to adjust x or $m \equiv 1-g-x$. Moreover, since m is fixed and neither φ nor ν appear in equation (E.13), market time n is unaffected.

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