

PARENTAL LABOR SUPPLY AND TIME USE: EVIDENCE AND IMPLICATIONS FROM THE PANDEMIC

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Abstract

The closure of schools to in-person instruction during the COVID-19 pandemic posed a unique shock to parents. This paper re-examines the effect of schooling mode on parental labor supply. The effects are undetectable using a full suite of controls for unobserved heterogeneity, which can be motivated by the failure of more parsimonious models to pass simple placebo tests. Even abstracting from such controls, though, a shift from fully virtual to in **Time-use** implies an increase in hours worked no greater than 2.25 hours per week. **Time-use** data also fails to indicate any significant shift in time **to** in other activities, such as leisure or home production. The paper uses a simple model of parental time allocation and child development to formalize why these estimates are unexpectedly small. Extensions to incorporate telework and nonparental care are then presented. We illustrate how the regression estimates can inform the identification of crucial structural parameters in these more general models.

JEL codes: J21, J22, J48

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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 closure of schools to in-person instruction posed a unique shock to parents. As Goldin (2022) noted, there were significant concerns at the time that the adoption of remote instruction would upend the careers of working parents. And yet, comparisons of labor force outcomes across adults with and without school-age children do not point to a dramatic change in parents' relative working time (Goldin (2022); Furman et al. (2021)).

These initial findings raise potentially interesting questions for our theories of labor supply and time use more generally. How did parents ease the trade-off between market work and child care? Put another way, on what other margins did parents adjust? And, what might these decisions imply about the preferences, technologies, and constraints shaping time allocations?

As a first step toward pursuing these questions, our paper begins by revisiting evidence on the effect of remote instruction on parental labor supply. Following leading work by Garcia and Cowan (2022) and Hansen et al. (2022), we link adults' working time to the *local* schooling mode. As detailed in Section 1, we measure changes in the prevalence of in-person instruction using Parolin and Lee's (2021) estimates of visits to school campuses derived from SafeGraph's mobile phone location data. The ratio of visits during some month in the pandemic era to visits during the same month in the pre-pandemic period is interpreted as the *in-person share of instruction*. These estimates, aggregated to the county or higher geographic unit, can then be combined with observations on individual working time in the Current Population Survey (CPS), which includes comparable geographic identifiers.

These data are applied to estimate the response of individual working time to variation in the in-person share. The potential endogeneity of school policy poses an immediate challenge. For example, one concern is that both school policy and parents' labor supply trend in the same direction as the general public's preference for a return to "normal" activity. Such preferences are not directly, or fully, observed.

As we discuss in Section 2, a common way of addressing this challenge is to leverage variation in working time across adults with and without children (see Garcia and Cowan, 2022; Hansen et al., 2022; Heggeness and Suri, 2021).¹ This approach is grounded in the simple observation that school policy should have a direct effect only on parents of school-age children. Therefore, adults without children can potentially serve as the control group.

¹ Hansen et al. note that this approach is an application of a triple difference in difference, since it exploits variation over time, space (e.g., counties), and parental status.

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The identifying assumption is that parents’ *relative* hours worked—relative, that is, to hours worked of childfree adults—only reflect school policy, controlling for observables.

While this strategy effectively differences out market-wide factors, it potentially neglects systematic differences between parents and childless adults. There is, however, considerable evidence that parents sort into different jobs and firms (Adda et al., 2017; Kleven et al., 2019); hold different political views and public policy preferences (Elder and Greene, 2012; Kerry et al., 2022); and exhibit different degrees of risk aversion in general and in regards to COVID-19 specifically (Görlitz and Tamm, 2020; Elder and Greene, 2021). This heterogeneity may translate into different decisions with respect to economic activity as well as alternative perspectives on the path of school policy, which is accountable to parents. Therefore, we propose in Section 2 to augment the regression with parental status fixed effects intended to capture differences in parents’ circumstances and preferences over time (but common across space) and across space (but fixed over time).

The results presented in Section 3 indicate that the estimated effect of school policy is indeed sensitive to the choice of controls for unobserved heterogeneity. According to the most parsimonious specification, a switch from virtual to in-person instruction implies an increase in hours worked of 0.5 per week. This result is, however, unstable across subsamples; if we restrict attention to the 2020-21 school year, weekly hours rise by two. This instability is resolved by controlling for parental status-by-time effects, revealing a stable coefficient of around two hours per week. Thus, controlling for unobserved heterogeneity in this dimension recovers a larger response. Controlling for spatial heterogeneity, though, has the opposite effect, and dramatically so. After including parental status-by-area effects, the association between in-person shares and hours worked is eliminated.

To assess these competing specifications, Section 3 considers a simple placebo test. This exercise examines, specifically, if differences in pandemic-era school policies across areas may reflect longer-term sources of parental heterogeneity (across space). If so, average *pandemic*-era in-person shares should predict parents’ relative hours worked in the *pre*-pandemic period. We confirm this is so, and, moreover, the size of this correlation is on par with the size of the estimated effects of school policy on pandemic-era data.

While we see this evidence of a null effect as being compelling, estimates derived from more parsimonious models are still instructive. As we discuss in Section 1, it does seem that the predominant source of variation in school policy is spatial. As a result, a null effect does not necessarily rule out an allocative effect of policy but instead indicates there is insufficient variation to tease it out. At the same time, the placebo test results suggest that one is likely to overstate the (true) impact of school policy by neglecting this

form of parental heterogeneity altogether. Accordingly, when abstracting from spatial heterogeneity, one can arguably interpret the estimates as upper bounds on the true effect.

In this spirit, we present a battery of results based on this simpler specification. A few findings stand out. First, labor supply responses are slightly higher among mothers than fathers, but one could not reject equality. The gender gradient is somewhat larger, though, among college graduates; the labor supply of fathers with a college degree is essentially inelastic with respect to in-person share. Second, the muted responses of college graduate fathers may reflect a more intensive use of telework to cope with school closures. Further estimates from the American Time Use Survey (ATUS), presented in Section 4, also point to the importance of telework as a means of sustaining market hours while supervising children. Third, labor supply responses vary little by marital status but do vary notably within the unmarried. Estimates for the latter aggregate two different results: labor supply is relatively responsive among (male and female) lone-adult parents—work hours increase by 4.5 per week when in-person instruction is reinstated—but not among the unmarried in co-residential arrangements with other adults.

Even when this analysis does uncover significant effects, though, they seem rather modest. Consider again a parent whose school district transitions away from a universally virtual format. This change reintroduces over 30 hours of in-person instruction, and yet the estimated increase in labor supply is a small fraction of this, even among groups (such as lone adults) whose labor supply is arguably most exposed to the policy.

In Section 5, we consider a simple model of time allocation to formalize the sense in which these effects are surprisingly small even within lone-parent households. Following Berlinski et al. (2020), a parent in the model values consumption, leisure time, and ~~her~~ child's development. To start, we assume a child's development is a function of two arguments: the parent's supervision and a form of publicly provided supervision, e.g., in-person class time. In addition, a child must be supervised by the parent or school at all times. In this context, a decline in publicly provided supervision leads the parent to substitute ~~her~~ time toward childcare (and away from leisure and market work).

We show that the model predicts counterfactually large labor supply effects. In fact, if the change in school policy is seen as temporary and if the parent can smooth ~~her~~ ^{the} family's consumption in the meantime, then labor supply will drop one for one with in-classroom time. Alternatively, if we assume that the family lives "hand to mouth", labor supply does decline by less. However, for reasonable preference parameters, the model still predicts that half of the change in in-person instruction time would pass through to labor supply. In other words, if 32 hours of in-classroom time is reintroduced, labor supply

would rise by around 16 hours. The empirical estimates in our paper and in other research do not point to effects anywhere near this large.

We go on to illustrate two ways to mitigate the predicted change in hours worked. First, we introduce a new “multi-tasking” technology to capture the idea that teleworking enables parents, to an extent, to carry out multiple tasks at the same time, e.g., working while simultaneously supervising children. Furthermore, we illustrate how to use our regression estimates to identify the structural parameter shaping the scope for multi-tasking. Nevertheless, we observe modest labor supply responses in the data even among groups with relatively little access to telework. Accordingly, we turn to another margin of adjustment: the take-up of *nonparental care*. We extend the child development technology to incorporate this form of care and illustrate how our regression estimates can be used to learn about crucial structural parameters. Specifically, we contend that parental and nonparental care must be strong (gross) substitutes to rationalize the data on hours worked. In this sense, the availability of nonparental care, much of which is provided by other family and friends, is likely to be a critical form of insurance for parents.

The remainder of the paper proceeds as follows. Section 1 discusses school closures in 2020-22 and introduces measures of in-person instruction shares. Section 2 reviews the econometric considerations that guide our regression analysis. Section 3 reports our estimates of the response of labor supply to variation in in-person shares. The hours worked data here are drawn from the CPS. Section 4 extends our empirical analysis to examine the relationship between in-person shares and telework hours, where the latter are taken from the ATUS. Section 5 interprets our results through the lens of simple structural models of time allocation and child development. Section 6 concludes.

1. Data

This section reviews the data on school policies, labor supply, and other controls used in the subsequent regression analysis.

1.1 School policies

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 Forsythe 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. 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 can vary widely across the trackers, which suggests that the different choices of methodology and sampling can meaningfully shape the results (Kurmann and ^{Lalé}Lee, 2022).

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, 2022; Hansen et al., 2022). 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 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 2019.²

Aggregating from the school level, Parolin and Lee present two county-level measures. One is an average of the percent change in visits across schools (in each month). The other is derived by first categorizing a school as “closed” in some month m (and year $y \geq 2020$) if the number of visits to that school at that time is down by at least 50 percent relative to month m in 2019. Each school (in each month) is assigned a one if it is categorized as closed and zero otherwise, and Parolin and Lee report the average over this binary indicator. The complement of this measure—that is, one minus the Parolin and Lee figure—can be interpreted, roughly, as the *in-person instruction share*. The latter has been favored in the related literature and will be our default measure of school policy.

We see several advantages in the Safegraph data. 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 the heterogeneity in learning modes. Within a district, some schools—and, within those schools, some students—may attend on-site

² Parolin and Lee exclude private schools because, for their analysis, they link their school-level estimates to student demographic data available only for public schools.

while others operate predominantly remotely. Other schooling-mode trackers would classify the district according to one of a few coarse, discrete formats, such as “hybrid” or “virtual”; whereas Safegraph’s data implicitly aggregates these modes into a single estimate of the change in on-site activity. In this sense, Safegraph offers both a breadth of coverage and a level of precision that is unique.

Still, mobile phone data are not immune to measurement error. One concern of Safegraph data is that the number of mobile phone pings is not necessarily proportional to the number of students engaged in on-site instruction. Suppose, for instance, that faculty at a primary school are asked to, or prefer to, work in their classrooms when they teach virtual lessons (see Cohen (2020) and Jung (2020)). This policy attenuates the decline in foot traffic even if on-site instruction is prohibited. Clearly, instruction-mode trackers based on published school district policy would not commit this error.

Based on these considerations, our empirical analysis proceeds as follows. We will treat Safegraph—and, specifically, Parolin and Lee’s figures—as our baseline. However, later in Section 3, we contrast Safegraph-based results with estimates derived from two instruction-mode trackers, namely, Burbio and CSDH.

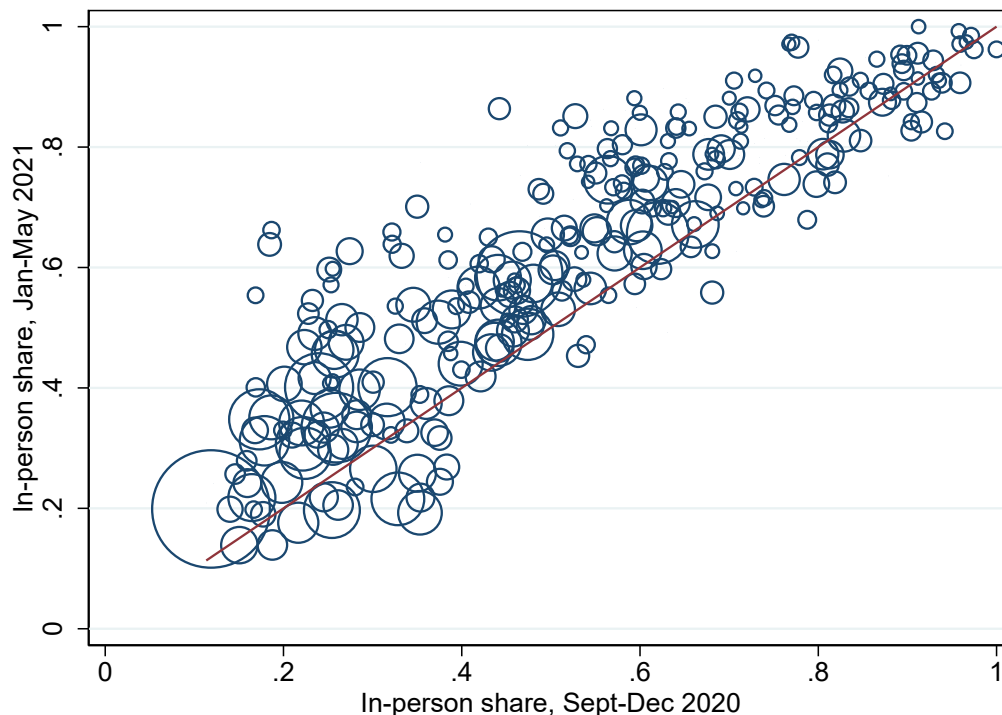
Geographic variation in in-person shares. Although Parolin and Lee’s school-level estimates cover the more than 3,000 U.S. counties, our other data sources do not offer this same breadth and detail. Crucially, the Current Population Survey (CPS), our data source on hours worked, does not disclose school district or universally report the respondent’s county. Over the two (calendar) years 2020-21, the CPS identifies only 280 counties. Although the latter are relatively large, one’s county is not disclosed for 60 percent of (adult) survey respondents. Fortunately, though, the CPS does identify 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. (2022)). First, if a county’s FIPS code is reported in the CPS, we assign its respondents the county in-person share from Parolin and Lee. Second, if a collection of counties is not identified in the CPS but does belong to a disclosed MSA, we aggregate Parolin and Lee’s ~~estimates~~ ^{which} across these counties and assign the mean to CPS respondents in that MSA for ~~whom~~ ^{which} no county is reported. Finally, we aggregate Parolin and Lee’s estimates among 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~~ ^{which} 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 478.³ This strategy maximizes the use of the Parolin and Lee data.

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Figure 1: In-person shares in 2020-21 school year



Note: This figure plots the average in-person share in September to December 2020 (x-axis) compared to January to May 2021 (y-axis). The size of each circle is proportional to the total enrollment in the geographic area.

Figure 1 illustrates the variation in school policies implied by the Safegraph data. For each of the 478 areas, the figure plots the average in-person instruction share in September-December 2020 along the x-axis and the average share in January-May 2021 along the y-axis. There are a few points to note. First, there are significant differences *across* areas. In each of the two semesters, in-person shares span a wide range from 0.2 to 1. These regional differences are, to some extent, persistent: almost half of the areas lie within 10 percentage points of the 45-degree line, that is, the in-person share shifted by less than $|0.1|$ across semesters. Among the other half of the areas, though, many observations lie well off 45-degree line, which indicates a substantial amount of variation

³ This figure includes 151 MSAs, or subsets of MSA. (The reference to a “subset” reflects that, if a county is reported in the CPS, then it is not included in our construction of MSAs, or MSA-like groups, in the second step.) The remainder comprises data from 47 states where we observed ~~from~~ respondents who do not belong to a disclosed county or MSA. The reason this third step captures data ~~from~~ only 47 states, rather than 51, is that, for a handful of very small states, all survey respondents live in a disclosed county or MSA.

in school policy *within* region. The latter variation generally reflected differences in the timing of reinstating in-person instruction during spring 2021; few regions cut in-person learning in 2021.

What might account for differences in school policies illustrated in Figure 1? We must be especially mindful of any sources of variation that also directly shape the choice of hours worked. Clearly, one possible source is the spread of COVID-19: if the threat of infection and fatality were to recede, for instance, we may see more in-person instruction and an increase in labor supply even if the latter has no causal effect on the former.

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In fact, the link between school policy and COVID-19 case counts is surprisingly modest. The Online Appendix fleshes out the evidence on this point, though it is consistent with what many related papers report (see below). Importantly, this result applies to variation (in school policy and COVID-19 cases) both *within and across* regions. We suspect that, within a region, monthly changes in case counts are only weakly correlated within changes in 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*. Just to its south, Fairfax County (Virginia) announced that, while it anticipated a shorter period of virtual instruction, it would not reinstate on-site instruction until at least the start of November. To the north, in Philadelphia, the school board's initial plan mirrored that of Fairfax. (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, likely to be somewhat insensitive to changes in the state of the pandemic.⁴

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Across regions, the influence of COVID-19 on school policy appears to have been overwhelmed by political forces. In other words, counties with divergent political make-ups set different policies even when faced with comparable rates of cases and fatalities. Partisan affiliation and, more concretely, the degree of popular support for Donald Trump were the most significant predictors of school policy. The strength of teacher unions also helps account for differences in in-person shares.⁵ These factors would seem to reflect long-held local preferences and norms, which in turn may be correlated with labor market

⁴ A commitment of some length enables parents and other stakeholders to plan their schedules around a period of virtual instruction. However, why school boards settled on such apparently long delays is not well documented.

⁵ For results on partisanship and union strength, see DeAngelis and Makridis (2021), Grossman et al. (2021), Hartney and Finger (2021), and Marianno et al. (2022). We review some of these results in the Online Appendix. We also show that the interaction between COVID-19 cases and county attributes (e.g., ~~its~~ political make-up) offers very little additional explanatory power.

activity *independent* of school policy. If so, regional variation in school policy may merely proxy for other employment-relevant factors. 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 aforementioned measures of on-site instruction). Labor supply and worker demographics are taken from the monthly Current Population Survey (CPS). We typically measure labor supply as weekly hours of work in the survey reference week but also report results where the outcome is employment status (in the reference week). 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 Coronavirus Resource Center](#). These data are aggregated up to the monthly frequency and to the local geographic areas described above. We also use [Kaiser Family Foundation](#) measures of government mitigation policies, such as capacity limits on restaurants and bars.

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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. We report results here (and later in the paper) separately for men and women. Table 1 considers two age groups, 21 and over and the more narrow range of 21-59. In addition, the table reports results for parents of school-age children. (The ages of parents are unrestricted, but nearly all fall within the range 21-59.) Finally, tabulations are shown for CPS-reported counties as well as the full sample of local areas described above. As discussed later, our regression sample consists of all areas but restricts attention to ages 21-59. It is instructive, though, to contrast our preferred sample to the alternative groups in Table 1.

A few patterns in the data are particularly noteworthy, if not necessarily unexpected. First, consider the differences across age ranges and parental status for given gender and geographic coverage. The sample of adults of all ages is (naturally) older, has fewer kids in the home, is less racially and ethnically diverse, and works less than the other two. In other words, this subsample is observationally quite different than the “treated” group, namely, the parents of school-age children. By contrast, the full sample of all adults ages 21–59 is very similar to the subsample of parents along nearly all dimensions (with marital status the obvious exception). Next, counties disclosed in the CPS are more urban, more densely populated, and ethnically diverse than the rest of the U.S. (for given age and gender). Finally, well-known differences in male and female labor market

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Table 1: Summary statistics

Variable	Women					
	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.	314,807	201,996	66,131	763,440	482,202	165,914
	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	0.642	0.648
Number of obs.	285,048	191,348	53,742	701,227	462,290	136,596

participation are apparent in the table. In addition, we note that mothers are significantly less likely to be married than fathers. The labor supply of single mothers will be an important topic in the analysis below.

2. Empirical framework

Our aim is to examine the effect of in-person instruction on parental labor supply. In this context, the potential endogeneity of instruction format is the most significant concern for estimation. We first (very briefly) review a difference-in-difference design that can be applied under the assumption that instruction modes are strictly exogenous to the state of the labor market. We then consider alternative strategies that aim to more explicitly address endogeneity. In each case, we leverage the simple, but essential, observation that the effect of school policy depends on the presence of school-age kids in the home; the direct effect on adults without school-age children is zero.

Under strict exogeneity, school policy is independent of other local developments that bear on labor input more generally. Therefore, a straightforward difference-in-difference strategy could be applied to the sub-sample of adults with school-age children. Formally, let h_{iat} be the hours worked of individual i in area a in month t and denote the in-person instruction share by p_{at} . (Recall that p_{at} is measured at the area level, which explains the absence of the i subscript.) The regression equation is then

$$h_{iat} = \psi p_{at} + \boldsymbol{\gamma}' \mathbf{x}_{it} + \chi_a + \tau_t + \varepsilon_{iat}, \quad (1)$$

where \mathbf{x}_{it} is a column vector of additional individual-level controls to be described in the next section (and $\boldsymbol{\gamma}$ is a conformable column vector); χ_a is an area fixed effect; and τ_t is a month fixed effect.⁶ The key parameter in [equation \(1\)](#) is ψ , which measures the parental hours response to a unit difference in the in-person share.

This approach exploits variation in the pace at which different areas reopen to in-person instruction. Importantly, the area fixed effects, χ_a , eliminate regional variation in *average* school policies. Thus, equation (1) is rendered immune to the concern, as raised above, that spatial variation in policies may merely reflect permanent differences in area attributes that also shape patterns in hours worked. The downside of the area fixed effects is suggested by Figure 1: the regional variation that is differenced away is by far more substantial than the temporal variation within areas. In this sense, equation (1) “ties its hands” with respect to the identification of ψ .

At the same time, the assumption of strict exogeneity that underlies equation (1) may be too strong. It is plausible that changes in school policy are correlated with

⁶ We also considered specifications with month-by-Census division effects in place of τ_t , but this added granularity makes hardly any difference.

developments in the local labor market. To the extent that these developments are only partially captured by the controls, the residual in ^{equation} (1) should be interpreted as

$$\varepsilon_{iat} = u_{at} + v_{iat}, \quad (2)$$

where u_{at} represents unobserved factors common to area a at date t and v_{iat} is an idiosyncratic force that satisfies $v_{iat} \perp u_{at}$. To illustrate, the evolution of the general public’s preference for a return to “normalcy” may be only partially captured by observables, which means it is channeled to an extent through u_{at} .⁷ This evolution arguably shapes views over in-person instruction as well as labor supply decisions and consumption patterns (the latter of which have implications for firms’ labor demands).

These concerns guide the empirical strategy adopted in much of the related literature (Garcia and Cowan, 2022; Heggeness and Suri, 2021; Landivar et al., 2021). This approach leverages differences in hours worked across adults with and without children to identify the effect of school policy. The argument is that, even if policy is endogenous to the *overall* state of the labor market, it is arguably (as good as) random with respect to parents’ *relative* labor market experiences.

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 variable equals one if the survey reference person i reports that he/she has children of school age who live in the residence. Equation (1) is then expanded to ^{having}

$$h_{iat} = \alpha \mathbb{k}_{it} + \beta p_{at} + \psi p_{at} \mathbb{k}_{it} + \gamma' \mathbf{x}_{it} + \chi_a + \tau_t + \varepsilon_{iat}, \quad (3)$$

The coefficient β captures the response in hours to school policy that is *common* across all adults. An estimate of $\beta \neq 0$ is rather curious, as school policy should not directly affect the labor supply of adults *without* school-age children. However, $\beta \neq 0$ is a natural outcome if policy is endogenous to broader trends in social and labor market activity, as we have discussed.⁸ The *differential* response of parents’ hours worked to a change in the in-person share is again measured by ψ , which is the main parameter of interest.

Equation (3) can potentially address the endogeneity posed by equation (2). Suppose, again, that ε_{iat} is made up of two components, namely, an idiosyncratic term

⁷ This evolution is almost surely shaped by some interaction of the factors to which we alluded in Section 2—changes in the number of confirmed cases and the area’s political attitudes, for instance—but observables may still fail to give a full account.

⁸ In addition, parental and nonparental hours worked may interact more directly. If the two groups are strong complements, a reduction in parental labor supply leads to reduced demand for nonparental hours—an outcome reflected in the regression as $\beta \neq 0$. Under this assumption, though, school policy should have opposite effects on the wages of the groups. We do not see evidence of this (see the Online Appendix).

v_{iat} and an area-wide factor u_{at} . To eliminate u_{at} , we can difference the terms in equation (3) with respect to their means in area a and time t . Let $\hat{\cdot}$ denote a deviation from an area-by-month mean,⁹ and equation (3) becomes

$$\hat{h}_{iat} = \alpha \hat{\mathbb{k}}_{iat} + \psi p_{at} \hat{\mathbb{k}}_{iat} + \boldsymbol{\gamma}' \hat{\mathbf{x}}_{iat} + \hat{v}_{iat}. \quad (4)$$

This expression eliminates the presumably spurious effect captured by β in equation (3).

The parameter ψ can now be estimated consistently under the assumption that adults with and without children do not have systematically different labor supply preferences or face systematically different labor demands, conditional on observables. Formally, this assumption means that $\hat{v}_{iat} \perp \hat{\mathbb{k}}_{iat}$. Another way to understand this restriction is to note that it eliminates any need to control for unobserved parent-specific heterogeneity. In other words, we may omit parent-level fixed effects from equation (4).

Seen in this light, the restriction has precise testable implications. To formalize these, suppose v_{iat} follows a more general specification,

$$v_{iat} = \zeta_a \mathbb{k}_{it} + \theta_t \mathbb{k}_{it} + \omega_{iat}. \quad (5)$$

While ω_{iat} is assumed to be i.i.d., the first two terms in equation (5) allow that parents and childless adults may be differentially affected by the (unobserved) area-wide factors that underlie u_{at} in equation (2). The month effect, θ_t , captures those parent-relevant factors that are common across areas but vary over time, whereas the area fixed effect, ζ_a , captures mean level differences. Under the null implicit in equation (4), $\theta_t \equiv 0 \equiv \zeta_a$. We relax this restriction in equation (5) but defer a discussion of what specific considerations may underlie θ_t and ζ_a until ^{the} next section.

We now insert equation (5) into equation (3) and difference terms with respect to their area-by-month means, which yields the regression,

$$\hat{h}_{iat} = \psi(p_{at} - p_a) \hat{\mathbb{k}}_{iat} + \zeta_a \hat{\mathbb{k}}_{iat} + \theta_t \hat{\mathbb{k}}_{iat} + \boldsymbol{\gamma}' \hat{\mathbf{x}}_{iat} + \hat{\omega}_{iat}. \quad (6)$$

The in-person share has been expressed here in deviation-from-mean form, where the mean is p_a . The added term $p_a \hat{\mathbb{k}}_{iat}$ is then subsumed into the new regressor $\zeta_a \hat{\mathbb{k}}_{iat}$, as is $\alpha \hat{\mathbb{k}}_{iat}$ from equation (3). Equation (6) can be estimated straightforwardly by OLS.

⁹ The de-meaned value of individual-level variables will have to be indexed by a even if the original value of the variable does not depend directly on the identity of the area. For instance, \mathbb{k}_{it} is not a function of the local area but since the average is taken within area, we use $\hat{\mathbb{k}}_{iat}$ to denote its demeaned value.

Intuitively, the controls in equation (6) narrow the channel through which school policy acts on hours worked. Average regional variation in parents' relative hours worked—relative, that is, to labor input of childless adults—is now captured by $\zeta_a \hat{\mathbb{K}}_{iat}$. Meanwhile, temporal variation in parents' relative hours worked that is common across areas is picked up by $\theta_t \hat{\mathbb{K}}_{iat}$. Thus, estimation of (6) recovers a significant effect of school policy only to the extent that parents' relative hours worked vary over time with the in-person share in their area. By contrast, equation (4) draws on both the within- and across-area comovement of school policies and parents' relative hours. Notably, the thrust of the approach represented by equation (6)—the reliance on within-area variation—mirrors the strategy that underlies equation (1). The key difference is that (6) allows that changes in school policy may be endogenous changes in the state of the local labor market. Therefore, the identification of ψ in (6) rests on the hours worked response of parents *in excess* of the response in the broader local market.

3. Estimates from the CPS

In this section, we report estimates from the regression models just discussed. To start, we specify our sample and the list of regressors represented by \mathbf{x}_{ict} . We then present estimates based on the full sample of adults, followed by an analysis for separate education and marital status groups.

Sample. Our preferred sample consists of adults aged 21-59. An adult is said to be parent of a school-age child if one of their own children in the home is between the ages of 5 and 17. Households whose only children are *under* age five are excluded to isolate the impact of school-age children on labor supply. The age restriction on adults (21-59) captures 98 percent of parents with school-age children. Thus, this restriction ensures that parents are compared to other adults of comparable age who are more likely to share a roughly similar baseline propensity to work. In the Online Appendix, we show the response of parental labor supply to school policy does appear somewhat larger if we include childless adults over age 59 in the control group, consistent with results in Garcia and Cowan (2022).¹⁰ The difference in the estimates is only notable, though, if we extend the sample well past age 70.

vis-à-vis

¹⁰ Our narrower age range also accounts for other differences vis à vis Garcia and Cowan. For instance, when we split the sample by college attainment, we find little difference in labor supply behavior (see Section 3.2). However, the over-59 group is predominantly made up of workers with no college experience. As a result, Garcia and Cowan find that hours worked of the noncollege group are relatively elastic.

Control variables. There are two distinct groups of regressors in \mathbf{x}_{iat} . The first consists of standard demographic controls and largely mimics the set of variables used by Garcia and Cowan (2022): age (and the square of age); race; marital status; educational attainment; a classification of the geographic area as rural, urban, or suburban; the total number of children (of all ages under 18) in the residence; an indicator for the presence of under-six-year-old children; and indicators of Hispanic heritage, nativity (or, foreign birth), veteran status, and disability.¹¹

The second group of regressors pertain to COVID-19. We track the trajectory of the pandemic with four controls: the cumulative number of cases and deaths as well as the new monthly number of cases and deaths. In addition, we follow Garcia and Cowan (2022) and include (binary) indicators that record if certain nonpharmaceutical mitigation policies are in effect. The policies captured by these indicators are Stay at Home orders; restrictions on “nonessential businesses”; and capacity limits on restaurants and bars. While we include the pandemic-related variables 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 (6)).

A third *potential* group of controls includes information on the respondent’s affiliation with an industry and occupation. There is a case to be made for these controls insofar as the composition of job types could be correlated with areas’ pandemic policies. However, these controls are problematic because they are not reported in the CPS for most nonparticipants.¹² Thus, with these controls, there is a sense in which “ y is regressed on y ”: the absence of an industry (and occupation) affiliation *means* that the agent does not work. Still, the Online Appendix reviews results with these controls and shows that the impact of in-person shares is estimated to be even smaller than reported below.

3.1 Full sample

We now proceed to estimate the standard two-way fixed effects model in equation (3).¹³ Table 2 presents estimates for two outcomes: weekly hours worked and an indicator for employment. Our sample consists of all 478 local areas constructed from county, metro area, and state identifiers in the CPS (see Section 1). Analogous results for the 280 FIPS

¹¹ The only controls here not present in Garcia and Cowan are the indicators for rural-urban-suburban status and for the presence of under-five-year-old children in the home.

¹² Among nonparticipants, industry and occupation are collected only of those in the outgoing rotation groups (which make up one quarter of the sample) who report that they have worked in the past 12 months.

¹³ Estimates of the simpler specification in (1) are reported in the Online Appendix but do not alter the basic conclusions of our analysis.

equation

It's "under-six-year-old children" in the paragraph above where footnote is cited. Should they match?

counties disclosed in the CPS are reported in the Online Appendix, which shows that the estimated effects of school policy are slightly smaller (and less precisely estimated) than those reported below. Estimates differ more across sample periods, as we will see. Therefore, we report results for two periods: the longer one spans all of (calendar years) 2020-21 but for the summer months, whereas the shorter period focuses in on the 2020-21 school year (September 2020 – May 2021). Finally, for each period, we report results separately for men and women.

Table 2: Estimates of Equation (3)

	Weekly hours		Employment	
	All 20-21	School 20-21	All 20-21	School 20-21
Coefficient	Women			
In-person share, β	1.192*** [0.338]	-0.881 [0.595]	0.019** [0.008]	-0.013 [0.014]
In-person \times kids, ψ	0.582* [0.304]	2.118*** [0.590]	0.020*** [0.007]	0.051*** [0.014]
Number of obs.	447,899	228,550	447,899	228,550
Coefficient	Men			
In-person share, β	1.279*** [0.382]	-0.043 [0.649]	0.029*** [0.008]	0.016 [0.014]
In-person \times kids, ψ	0.567* [0.314]	1.454*** [0.588]	-0.010 [0.007]	0.009 [0.012]
Number of obs.	432,856	221,080	432,856	221,080

Note: Each column within each panel is a separate regression. In addition to the coefficients listed in this 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” refers to the entirety of the 2020 and 2021 calendar years, excluding 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 0.1.

Consider first the results for the longer sample period that spans all of 2020-21. Among women, a shift from fully virtual to fully in-person instruction leads to a *market-wide* (β) increase in hours of 1.2 per week. This response reflects in part a movement along the extensive margin: given hours worked of those at work, the nearly two percentage-point increase in the employment rate implies a gain of 0.7 weekly hours, or 60 percent of the overall rise in hours. The hours response among men is similar, but the extensive margin in their case accounts for essentially all of the change.

The presence of a market-wide response in this period suggests that school policy may stand in for broader shifts in the propensity to work. Nevertheless, if equation (3) is otherwise properly specified, the parameter of interest, which measures *parents'* relative labor supply response (ψ), may be consistently estimated. On the extensive margin, the response is again two percentage points among women. The latter implies a gain in hours that slightly exceeds the estimated increase in weekly hours reported in the table, although the difference is statistically insignificant. On balance, then, these results point to a maternal labor supply response on the order of 0.6 hours, which is fully accounted for by a rise in employment. The hours response among fathers is virtually the same, although it reflects, remarkably, a shift along the *intensive* margin. As we shall see, the importance of the intensive margin to fathers is a robust result.

Next, we turn to the 2020-21 school year. These results paint quite a different picture than the longer 2020-21 sample. For starters, the overall hours response among parents is notably higher: a shift from fully virtual to fully open 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 in this period, even if it is not quite as responsive as that of mothers. Once more, though, the margin of hours adjustment differs notably across men and women: the intensive margin dominates for fathers but is essentially unimportant for mothers, for whom changes in employment are more significant. Meanwhile, the market-wide response to policy is now insignificantly different from zero for both parents. This result stands in stark contrast to what we observed in calendar years 2020-21. We have confirmed that this difference across the two periods reflects the influence of the months that preceded the 2020-21 school year (e.g., January – May 2020), not the months that followed (September – December 2021).

The parameter instability evident in Table 2 may reflect model mis-specification. One concern about equation (3) 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 will look as if their labor supply is somewhat insensitive to shifts in school policy that coincided with pandemic-related labor market disruptions.¹⁴ A corollary is that *market-wide* reactions to these disruptions may be large and will be reflected in a significant response to (correlated) changes in school policy. Notably, these concerns are likely less acute later in the pandemic. Thus, the absence of controls for such trends may lead to different estimates of ψ across different periods.

¹⁴ See Adda et al. (2017) on differences in occupational choice across parents and non-parents.

Table 3: Estimates of Equation (6)

	All 20-21	School 20-21	All 20-21	School 20-21
Coefficient	Women			
In-person share, β	0.553 [0.394]	-0.993* [0.601]	1.405*** [0.406]	-0.118 [0.670]
In-person \times kids, ψ	2.360*** [0.634]	2.463*** [0.633]	-0.040 [0.672]	-0.101 [1.129]
Number of obs.	447,899	228,550	447,899	228,550
Coefficient	Men			
In-person share, β	0.898** [0.400]	-0.126 [0.654]	1.490*** [0.427]	0.964 [0.757]
In-person \times kids, ψ	1.888*** [0.645]	1.776*** [0.628]	-0.051 [0.706]	-1.737 [1.194]
Number of obs.	432,856	221,080	432,856	221,080
Month \times parent	Yes	Yes	Yes	Yes
Area \times parent	No	No	Yes	Yes

Note: Each column within each panel is a separate regression. The dependent variable is the number of hours per week worked. See the notes of Table 2 for the other control variables included.

note about p-value?

In view of this concern, we re-estimate the regression with additional controls for parent-specific trends in labor supply. Formally, this step merely requires the introduction of *month-by-parental status* fixed effects ($\theta_t \mathbb{k}_{it}$ in equation (6)), which account for the possibility that aggregate variation in hours worked may systematically differ across parents and childless adults. The first two columns of Table 3 report the results. To conserve space, we present only the response of hours here. In the Online Appendix, we confirm that the extensive margin continues to play an outsized role in women’s labor supply response but matters little for men.

Under this specification, the adjustment of hours to in-person instruction is now remarkably stable across time. Among women, a shift from fully virtual to fully in-person instruction yields an increase in weekly hours of around 2.4—regardless of the sample period. The response among men is somewhat smaller—weekly hours increase by around 1.8—but again is virtually unchanged across sample periods. Thus, as anticipated, the parameter instability in Table 2 reflected the failure to control for broader trends in parental labor supply. With the addition of these controls, the results for all periods are now comparable to the results for the 2020-21 school year in Table 2.

Just as there may be parent-specific *trends* in hours worked, average parental labor supply may also vary across *space*. More precisely, parents’ hours worked *relative* to the hours of childless adults may be higher on average in some local areas than in others. These spatial differences pose a challenge to estimation if they reflect more of a “deep” feature of the local market but are nevertheless correlated with 2020-21 in-person instruction rates. The reasons for any such correlation are perhaps not yet immediate, but it is straightforward to incorporate additional controls for spatial heterogeneity. As previewed in Section 2, we add *area-by-parental status* fixed effects ($\zeta_a \mathbb{1}_{it}$ in equation (6)), which will control for mean differences across areas in relative parental labor supply.

The impact of these controls, shown in the final two columns of the table, is significant: 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 principally off across-area comparisons of parents’ relative hours worked. With additional controls for average regional differences in labor supply, the estimated effects of school policy disappear.

One could fairly question, though, if Table 3 “over-controls” for unobserved heterogeneity. With the addition of area-by-parental status terms, much of the important variation in school policy is now captured by other regressors. In other words, this perspective asserts that differences in average school policy across regions represent plausibly exogenous variation that may be, and indeed should be used to recover the causal effect of in-person instruction.

Fortunately, this claim is at least partially testable. By this view, differences in average policies emerged for reasons unrelated to the “deep” attributes of a region that generally shape parental labor supply. In this case, there is no reason to expect that average policies *in the pandemic* should predict *pre-pandemic* labor supply. This line of argument suggests a simple placebo test: are average in-person instruction shares in 2020-21 correlated with parents’ relative hours worked before 2020?

In fact, this correlation between pandemic-era policy and pre-pandemic hours is evident in the data. Figure 2 illustrates this for a single (and the largest) demographic group, non-Hispanic White people. The x-axis shows the average in-person share in local labor market areas over the *pandemic* period, 2020-21. The y-axis is derived from *pre-pandemic* data on weekly hours in the CPS. Specifically, the y-axis shows the local-area average of parents’ hours less the average of childless adults’ hours over the five years prior to the pandemic, 2015-19. Remarkably, parents’ relative hours worked in the pre-pandemic period are higher in areas where schools chose more in-person instruction in 2020-21.

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, as described in the text (see Section 2). The left panel is based on hours data among non-Hispanic white women ages 21-59; the right panel refers to non-Hispanic white 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 6.404 (s.e. of 1.335), and the line of best fit in the right panel (among men) has slope 2.642 (s.e. of 1.097).

equation?

To make this point more rigorously, we use regression (3) to test if pandemic policy predicts pre-pandemic hours. Hours worked and all individual-level controls are taken from the CPS over 2015-19. The policy term, which was formerly measured by monthly data on 2020-21 in-person shares (p_{at}), is now the area-level *mean* of the latter (and denoted by p_a). Note that since the policy term is fixed over time, the market-wide response to in-person shares, as measured by β , is not separately identified from the area-level fixed effects. Therefore, we will only report the response of parents' *relative* hours, e.g., the coefficient ψ on the term, $p_a \mathbb{1}_{it}$. A significant estimate of ψ indicates that average policies predict parents' relative labor input prior to the pandemic.¹⁵

This placebo test is implemented for men and women and with two variants of mean in-person shares. We first construct the area-level mean p_a based on all of 2020-21

equation or regression?

¹⁵ As in (3), our specification also includes month effects (τ_t), and we have confirmed that the results are virtually unaffected if we insert month-by-parental status effects. Of course, area-by-parental status effects are excluded; the purpose of this exercise is to ask if, in the absence of such controls, we would (spuriously) conclude that there is a link between pre-pandemic hours and pandemic policy.

(save for the summer) and then discuss results for an alternative derived only from the 2020-21 school year. The results are most striking in the former case, especially for mothers. We find that, in areas that selected full-time in-person instruction, mothers' relative working time *prior to* the pandemic was nearly 3.5 weekly hours greater than in areas that selected full-time virtual instruction. Among men, in-person instruction in 2020-21 implied almost 1.5 more weekly hours of work pre-2020. Notably, these figures are of the same order as what we estimated to be the impact of pandemic-era policy when spatial controls are omitted (see Table 3). When we instead compute average in-person shares based on 2020-21 school year data, the coefficients for women and men fall to, respectively, two and just under one but remain statistically significant. All these figures are reported later in Table 5, where we also confirm that other sources of in-person instruction (other than Safegraph) also fail this placebo test.

What might account for this connection between pandemic-era school policy and pre-pandemic labor supply? As a first step, it is helpful to first consider what may account for the general dispersion across space in relative parents' labor supply. In this regard, Black et al. (2014) uncover a notable result. They find that the hours worked of parents, and particularly mothers, is diminished in areas where commute times to work tend to be long. Intuitively, long commute times reduce time for childcare and, therefore, reduce parents' willingness to supply market hours.¹⁶ It is also possible, with a slight leap, to connect commute times to school policy. Longer commutes are more common in denser population areas, which tend to be more liberal politically. This is an important observation because, as we discussed, school policy was strongly shaped by local politics: in-person shares were highest in areas where support for Republicans ran highest. Thus, one possibility is that longer (pre-pandemic) commute times are tied to both lower in-person shares and diminished parental labor supply.

While this argument is intriguing, we find only modest empirical support for it. The Online Appendix does confirm that commute time is a statistically significant predictor of a lower in-person share as well as a higher Democratic share of the vote. Nevertheless, in-person shares are highly dispersed across local areas, and commute times account for relatively little of the overall variation. One way to see this is to introduce an interaction between commute times and parental status into the regression and ask if, conditional on the latter, 2020-21 school policy is still a significant predictor of pre-pandemic parental hours. The answer is, yes: among women, the difference in mothers'

¹⁶ In addition, long commute times to work suggest higher general levels of congestion, which also burdens family-related travel. For instance, a parent must set aside more time to transport a child to after-school events. As a result, there may be less time available for market work.

relative weekly hours between fully in-person and virtual areas falls only from 3.5 to 3.25 (see Online Appendix).

Absent a fuller explanation of the placebo test results, these estimates should be treated with some care. It could be that regressions with a full set of spatial controls (see equation (6)) are over-saturated and, as a result, fail to detect the hours response to in-person shares. At the same time, the placebo test persuades us that more parsimonious specifications (see equation (3)) are just as likely to overstate it because of their failure to address the endogeneity of school policy. For the remainder of this section, we will present results based on both specifications. The two results are likely to bound the true effect.

Sensitivity analysis, I: Policy measures. Our primary indicator of in-person instruction was developed from Safegraph data (Parolin and Lee, 2021). As noted earlier, there were other efforts to track the instruction modes at primary and secondary schools. We now reexamine the impact of in-person instruction through the lens of two trackers independently developed by the COVID-19 School Data Hub, or CSDH, and Burbio.

The CSDH tracker 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.¹⁷ In total, 35 states provided school-level data to CSDH. 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 the reports of instruction mode, categorizing them into one of three groups: in-person, virtual, or hybrid. We then aggregate the school- and, where needed, district-level data to the county level. To this end, we assign a score of one when “in-person” instruction is reported and a score of zero if “virtual” instruction is listed. The on-site portion of “hybrid” instruction is never spelled out in the schools’ reports. Based on responses to the 2021 National Assessment of Educational Progress (NAEP), the most common “hybrid” format involved on-site instruction of two days per week per child. Therefore, we assign a score of 0.4 if a hybrid format is reported in the CSDH data. The enrollment-weighted average score may then be interpreted as the in-person share of instruction in the county.¹⁸

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

¹⁷ 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.

¹⁸ Enrollment by instruction mode is available in CSDH data but for only 15 states.

certain degree of judgment, which can be exercised differentially across states and time. Perhaps the “hybrid” category best illustrates this point.¹⁹ While two days per week is most common, the NAEP data indicate that there is variation even within state as to what “hybrid” encompasses. Pennsylvania exemplifies what we see nationally: half of schools designate a hybrid format to be two days per week but in over a third of schools, a hybrid mode implies a four-day-per-week schedule.

In this context, it is helpful to consider another school-mode tracker that provides a “fresh set of eyes” on the question of how to categorize instruction format. To this end, we turn to Burbio, whose estimates are developed from entirely different sources. Burbio’s network of analysts throughout the U.S. follow an assortment of publicly available data—district websites, local news reports, social media, and more—to track the instruction format of a sample of school districts in their area. Relative to CSDH, Burbio does offer less geographic coverage. In total, Burbio follows districts in just under 600 U.S. counties and, among the latter, tracks the instruction mode for 60 percent of a county’s students.

Despite their differences, CSDH and Burbio share an important feature that distinguishes them from Safegraph. Both CSDH and Burbio document instruction offered by schools, that is, these data measure only *access* to in-person instruction. In many districts, though, a virtual option was made the children of parents. Calarco et al. (2021) report that, in the the children of parents in late 2020, [^]75 percent had at least some access to in-person instruction but [^]less than 60 percent attended school on-site. This incomplete *take-up* of in-person instruction is captured by Safegraph since it tracks foot traffic on school grounds.

The role of take-up in Safegraph is both a “feature” and a “bug”. Relative to CSDH and Burbio, Safegraph aims to measure the share of students’ time that is spent on school campus. By contrast, the availability of in-person instruction, as measured by CSDH and Burbio, does not imply that a parent accepted the “treatment” and enrolled ~~her~~ children in on-site instruction. The presence of incomplete take-up implies that labor supply responses to CSDH and Burbio data will be diluted (relative to the effect of treatment on the treated). However, the role of take-up also poses a challenge who wants to work endogenous to the labor supply decision, and the implied bias is clear: a parent [^]is more likely to enroll ~~her~~ children in in-person instruction ~~if she wants to work~~. For both reasons, estimates off Safegraph data could exceed those off CSDH or Burbio.

¹⁹ The hybrid format is a quantitatively important mode in the CSDH data: the hybrid share of instruction in a state is at least 20 percent and as high as two thirds (North Carolina).

Table 4: Estimates with Alternative Measures of School Formats

Coefficient	CSDH			Burbio		
	Women					
In-person \times kids, ψ	0.681	0.927*	-1.260	0.654	1.074**	0.371
	[0.471]	[0.513]	[0.918]	[0.428]	[0.501]]	[0.724]
Number of obs.	223,262	223,262	223,262	228,550	228,550	228,550
Coefficient	Men					
In-person \times kids, ψ	0.260	0.406	-1.325	0.384	0.610	0.267
	[0.449]	[0.478]	[0.832]	[0.439]	[0.494]	[0.693]
Number of obs.	216,034	216,034	216,034	221,080	221,080	221,080
Month \times parent	No	Yes	Yes	No	Yes	Yes
Area \times parent	No	No	Yes	No	No	Yes

Note: In each regression, the controls are identical to those used in Tables 2 and 3 but for the measurement of the in-person instruction share, which is now drawn from CSDH or Burbio. For each of the latter, one column reports estimates of equation (3), which excludes parent fixed effects. The remainder of the columns include some combination of parent fixed effects and are derived from estimation of equation (6).

note about p-value?

We now proceed to re-estimate the labor supply effects of in-person instruction with CSDH and Burbio measures of school policy. We first re-estimate the standard two-way fixed effects model in (3). We then add controls for unobserved heterogeneity one at a time, first time-by-parental status effects and then area-by-parental status effects. To streamline the discussion, we report results for weekly hours and confine results for employment to the Online Appendix. We also focus in here on the main parameter of interest, namely, the parent's relative hours response, ψ . The sample period is the 2020-21 school year, as these are the months for which CSDH and Burbio are both available. These estimates may be compared to the Safegraph-based results in Tables 2 and 3.

Table 4 indicates that estimates based on CSDH and Burbio are, on balance, remarkably similar. Consider for instance the results for women. In both datasets, estimates of equation (3) imply that a shift from fully virtual to fully open elevates hours by nearly 0.7 per week. With controls for aggregate trends in parental labor supply, the estimated effect of on-site attendance is larger, if not statistically different; a shift to in-person instruction now implies a one hour per week increase in labor supply. As anticipated, both these estimates come in well below the Safegraph-based estimates presented in Tables 2 and 3. The last step is to add controls for regional differences in parental labor supply. As with our results based on Safegraph data, the addition of these controls eliminates any statistical significance observed in the first two specifications (and in fact switches the sign of the coefficient in the case of CSDH).

When we turn to the sample of males, a few results emerge. First, the CSDH and Burbio-based results again largely agree. In addition, the *difference* between the estimates for women and men is comparable to the difference observed in earlier tables based on Safegraph data (see Tables 2 and 3). However, given that CSDH and Burbio-based estimates are lower for women, all the point estimates for men are now quite small and insignificant at conventional levels.

Refers to 'take-up'? It might be clearer to repeat the word instead

The relatively modest effect of school policy in Table 4 harkens back to our discussion of take-up, though the latter is not the only probable source. Perhaps the simplest explanation is measurement error. We hesitate to take a strong stand on this, since there is no means to audit our sources of data. Still, what recommends the foot traffic data in Safegraph is that it naturally aggregates over schools' potentially complicated hybrid instruction formats to yield an estimate of the share of student time on school campus. By contrast, CSDH and Burbio data are more categorical in form; this coarseness invariably injects some noise into the numbers. By this view, we would expect estimates based on CSDH and Burbio to be biased downwards.

In addition, incomplete take-up is likely to drive a wedge between estimates based off Safegraph, on the one hand, and CSDH and Burbio on the other. If school policy were exogenous, one could resolve this tension by leveraging CSDH and Burbio as instruments for the Safegraph measure. This strategy addresses the endogeneity of take-up in the Safegraph measure and, therefore, yields a consistent estimate of the response of labor supply to an exogenous change in the availability of child supervision. In our context, though, policy is unlikely to be exogenous.

In fact, the CSDH and Burbio data also fail the placebo test described above. Results for Safegraph, CSDH, and Burbio data are listed in Table 5. As noted, we report Safegraph-based estimates based on average in-person instruction shares for all of 2020-21 as well as just for the 2020-21 school year. For CSDH and Burbio, we also average in-person shares over the 2020-21 school year.

Consider first the results for women. To interpret these, recall that the idea behind the regression is to ask if differences across areas in 2020-21 in-person shares predict differences in mothers' relative hours in 2015-19 (that is, relative to the hours of childless women in her area). Thus, we read Table 5 to say that, *prior to* the pandemic, mothers' relative labor supply in an area with full-time in-person instruction was 1.5 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, but the two estimates are not statistically distinguishable. Estimates from Burbio show a weaker, but still statistically significant, relationship between pandemic-era policy and pre-pandemic hours worked.

Table 5: Placebo test

	Safegraph		CSDH	Burbio
Coefficient	Women			
In-person \times kids, ψ	3.433*** [0.879]	2.055*** [0.560]	1.553*** [0.428]	1.144** [0.446]
Period of policy	All 20-21	School 20-21	School 20-21	School 20-21
Number of obs.	1,351,083	1,351,083	1,254,179	1,246,747
Coefficient	Men			
In-person \times kids, ψ	1.440* [0.851]	0.874* [0.524]	0.613 [0.413]	0.343 [0.409]
Period of policy	All 20-21	School 20-21	School 20-21	School 20-21
Number of obs.	1,284,357	1,284,357	1,191,245	1,185,249

Note: This table estimates a version of equation (3) on the sample of CPS respondents in the years 2015-19. Relative to equation (3), though, 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 the summer months), whereas “School 20-21” covers only September 2020 – May 2021.

note about p-value?

The results for men generally follow the same pattern observed in pandemic-era data. Specifically, the differences between estimates for women and men are comparable to what we observe in the Safegraph-based results, but point estimates themselves are now quite small and statistically insignificant. This result is not much of a surprise—men’s hours even in the pandemic period were unresponsive to the CSDH and Burbio measures. Indeed, the consistency for men across Tables 4 and 5 is somewhat encouraging.

Sensitivity analysis, II: Sample of parents.

As we have noted, the main regressor of interest is the interaction between the in-person share and the presence of school-age children. Now that we have explored alternatives to our primary measure of the in-person share, we want to consider different ages groups of school-age children.

For these regressions, thus far, a respondent is said to be a parent of school-age children if they have kids between ages 5 and 17. We want to consider alternatives for the upper limit of this range. This exercise is motivated by the possibility that labor supply responses to school policy vary across parents based on their children’s ages. One reason for this is that older children may be able to substitute for adults in the provision of care. For instance, older children are more likely to be entrusted with “self-care” in which case they do not receive any direct adult supervision. Indeed, prior to the pandemic, over a third of high school students were unsupervised after school hours (Afterschool Alliance, 2020). In addition, older children may be asked to look after younger siblings, which suggests that even certain children in primary and junior high school do not require

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full-time parental supervision. However, an important caveat to this argument is that it is derived from observations *prior* to the pandemic and, therefore, before full-day virtual instruction. A parent’s decision-to entrust an older child with self-care for a few hours *after* school does not imply that she will leave the virtual school day unsupervised.

To shed light on this debate, we now re-run the regressions from Table 3 under alternative definitions of “school-age children”. To start, we include ~~a parent in the sample~~ **parents whose** if her eldest child is between the ages of 5 and 13, a choice that is designed to exclude children of high school age. Next, we further **in the sample** the age range to include only parents whose eldest child is between age 5 and 9. Finally, to put these estimates in a fuller context, we re-run the regressions with a sample that includes a parent only if her eldest child is *older* than 13 (e.g., ~~between~~ 14-17.) For the sake of brevity, **parents whose** results in only for total weekly hours and over the period that spans all of calendar years 2020-21 **and 2021**.

The results **includes/excludes? Or should one of these be deleted?** are in Table 6. Consider first the estimates in the left panel of the table. (This specification **includes excludes** area-by-parental status controls.) These estimates do offer some support for the claim that parents of younger school-age children were more responsive to school policy. Among parents of children ages 5-9, we find that a shift from fully virtually to fully in-person implies a significant gain in weekly hours of nearly ~~between~~ 2.7 (mothers) and 2.9 (fathers). When we extend the upper limit of this range to 13, the response of fathers’ hours falls notably, whereas the decline among mothers is more muted. Moreover, if we look only at households with ~~only~~ older children (ages 14-17), the hours response of fathers is halved 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. The results on the right panel show, however, that the inclusion of more spatial controls (e.g., area-by-parental status effects) again eliminates the significance of the estimates.

3.2 Education and marital status

Estimates presented thus far are based on the pooled sample of workers. However, there are reasons to suspect that labor supply responses to virtual instruction might have varied across different populations, as distinguished, for instance, by marital status and educational attainment. In this section, we examine the heterogeneity in labor supply responses to the adoption of virtual instruction.

Our analysis here will *not* include the full battery of controls for unobserved heterogeneity highlighted in the prior section. Specifically, we will focus on specifications that condition *only* on time \times parental status effects (as well as observables). The inclusion

Table 6: Estimates with Alternative Definitions of School-Age Children

	Age range of school-age children					
	14–17	5–13	5–9	14–17	5–13	5–9
Coefficient	Women					
In-person share, β	1.313*** [0.398]	0.679* [0.401]	0.703* [0.407]	1.543*** [0.410]	1.366*** [0.410]	1.337*** [0.413]
In-person \times kids, ψ	1.453 [0.960]	2.439*** [0.711]	2.667*** [0.863]	0.041 [1.066]	0.114 [0.757]	-0.504 [0.876]
Number of obs.	326,585	406,570	359,375	326,585	406,570	359,375
Coefficient	Men					
In-person share, β	1.412*** [0.427]	1.000** [0.415]	1.163*** [0.437]	1.563*** [0.437]	1.503** [0.428]	1.530*** [0.431]
In-person \times kids, ψ	1.001 [0.985]	2.057*** [0.686]	2.944*** [0.823]	-0.696 [1.232]	0.017 [0.806]	0.788 [1.011]
Number of obs.	334,121	400,858	363,400	334,121	400,858	363,400
Month \times parent	Yes	Yes	Yes	Yes	Yes	Yes
Area \times parent	No	No	No	Yes	Yes	Yes

Note: This table presents estimates of equation (6) under different definitions of school-age children. The sample that corresponds to each column includes childless adults and parents whose eldest child lies within the age range listed in the column header.

note about p-value?

of the spatial controls (area \times parental status effects) eliminates the statistical significance of the estimates, just as we saw above. One might then view the results below as the strongest case that one could present for a role of school policy in parental labor supply. Estimates based on spatial controls are reported in the Online Appendix.

Marital status. Marital status is often singled out as a critical factor in labor supply decisions in the pandemic, but the *direction* of its effect is not obvious *a priori*. On the one hand, married partners can share the responsibilities of childcare and market work; it is not obvious why *both* parents would exit employment to supervise children. By contrast, a single parent may be the only adult who could supply supervision. Thus, this view states that, even if one worker in a married household reduces labor supply, we would expect labor input among married workers *on who* ge to decline by less (Garcia and Cowan, 2022). On the other hand, ~~if~~ a single parent ^{is} the lone breadwinner in the household, ~~she~~ ^{he} may have a strong incentive to arrange some other form of childcare ~~to enable her~~ ^{to} carry on in the workforce. In other words, this view states that the propensity of a ~~parent~~ ^{in order} to work could exceed that of *any* married individual (Heggeness and Suri, 2021).

This debate over the role of marital status hints at another, related consideration: the broader composition of the household. The time allocation of a single parent is shaped by whether the parent is the *lone adult* in the household. Intuitively, the labor supply of single parents may still be relatively insulated from variation in school policy if other adults in the home are able to supply childcare.

This consideration is likely to be empirically relevant: lone adulthood is not in fact the most common situation among unmarried parents. Consider the household arrangements of unmarried mothers. (We highlight mothers because there are comparatively few unmarried men who live with their children.²⁰) In the CPS, almost 55 percent of unmarried mothers live with at least one other adult in *coresidential* arrangements. Among the latter, the most common arrangement is cohabitation: nearly 45 percent of these unmarried mothers live with a self-identified (unmarried) partner. In addition, roughly one quarter live with an older parent, and another quarter live with an adult child (aged 18 or older).²¹ Notably, in households with married couples, the extended family is present less often. For instance, just under ⁴four percent of households with married couples include a parent of one member of the couple.

Our estimates, as summarized in Table 7, confirm that household composition mediates the role of marital status. For these regressions, we again report results for only for total weekly hours and over the period that spans both calendar years 2020-²¹21. Consider first the results for women. The responses of hours worked are similar across ^{and 2021}married and unmarried samples: a shift from fully virtual to fully in-person implies a gain of 2.25 to 2.5 weekly hours. However, the estimate for the unmarried group masks a significant difference in behavior between mothers with and without other adults in the household. When we restrict attention to lone-adult mothers, hours worked are notably more responsive: a shift from fully virtual to fully in-person now implies an increase of roughly 4.25 weekly hours.

A corollary of this result is that labor supply is relatively *unresponsive* among unmarried mothers who *do* live with other adults. Indeed, the result for the latter group (not shown in Table 7) indicates a statistically insignificant gain of only 1.4 weekly hours when in-person instruction is reinstated. By this measure, coresidential household arrangements facilitate relatively stable employment. It is difficult, though, to pinpoint

²⁰ Just 9 percent of unmarried men live with children, as compared to 22.5 percent of the sample of unmarried women.

²¹ In another ⁵five percent of cases, the other adult is a brother or sister. Note that there may be some overlap among these categories, e.g., an unmarried mother may live with a partner and an older parent.

Table 7: Estimates by Marital Status

	Married	Unmarried	Lone adults
Coefficient	Women		
In-person share, β	0.582 [0.599]	0.569 [0.484]	-0.622 [0.926]
In-person \times kids, ψ	2.248*** [0.787]	2.595** [1.032]	4.279*** [1.177]
Number of obs.	242,743	205,156	67,592
Coefficient	Men		
In-person share, β	0.464 [0.624]	1.372** [0.589]	0.630 [1.034]
In-person \times kids, ψ	1.817*** [0.661]	1.680 [1.455]	4.385*** [2.251]
Number of obs.	223,471	209,385	61,954

Note: This table presents estimates of equation (3) augmented with time \times parental status fixed effects. The column header reports the composition of the sample used in the regression. A “lone adult” is an individual age 18 or over who does not live with any other individual age 18 or over.

note about p-value?

the precise source of this stability. For instance, if we restrict further to a subset of unmarried mothers (who live, i.e., with a parent), the sample size shrinks considerably, and the OLS coefficients are imprecisely estimated.

The narrative for men is broadly similar. For instance, as we saw for women, the results differ little across marital status. Estimates for married and unmarried men do come in below their counterparts for women, but there is enough uncertainty around these coefficients that one could not reject equivalence. Moreover, the insignificant response among unmarried men is not a surprise since comparatively few single men live with children, as noted above. In light of this fact, perhaps the most remarkable result in the table emerges when we restrict to attention to lone-adult men. Even though relatively few unmarried men are lone adults²², we find a sizable labor supply response in this group. Indeed, the response of lone-adult men is almost identical to its counterpart for women.²³

²² Lone adults make up a quarter of unmarried fathers. Among unmarried mothers, this share is 45 percent.

²³ One may note that estimates for married men and women nearly *add up* to the same magnitude as those of lone-adult parents. However, this sum of average individual responses is not, in general, the average *household* response, as we discuss in the Online Appendix. We also report there the results of regressions where the outcome is the total weekly hours worked of both spouses in the household.

Our results in Table 7 may be compared with other analyses of onsite instruction in 2020-21. Our estimates for men match those in Garcia and Cowan (2022) rather closely. The estimates for women in Table 7 are quite a bit higher; indeed, the estimated response of married mothers (2.25 hours per week) is three times as large as these authors report. As we show in the Online Appendix, these differences may be traced to differences in sample—Garcia and Cowan include ages over 59—and regressors. The contrast between our results and Hansen et al. (2022) is even more noticeable. Their results for married mothers are somewhat small and comparable to those in Garcia and Cowan. In addition, ~~though~~, Hansen et al. also report no effect among unmarried mothers or fathers in general, results that diverge from both ours and those in Garcia and Cowan.²⁴ Nevertheless, a more general point emerges from this discussion: the estimates in Table 7 are at, or near, the top end of the range in this literature. Later, though, we will argue that even these results are, in a sense made precise, unexpectedly small (see Section 5).

It is also instructive to put our estimates in the context of the broader parental labor supply literature. Specifically, there is a strand of research that has recognized the childcare role of public education and its potential impact on parental labor supply. One topic that has drawn particular attention is publicly funded kindergarten. Gelbach (2002) finds that the availability of public kindergarten significantly elevates the labor supply of unmarried mothers by 2.7 hours per week.²⁵ The effect among married mothers is also significant but roughly half as large. Cascio (2009) reports comparable, if slightly smaller, estimates for unmarried mothers but finds no statistically significant effect among married mothers. Notably, Cascio argues that these relatively modest effects can be traced in part to the availability of other forms of nonparental care (i.e., private preschool), which supported mothers' labor supply before the start of public kindergarten. Our estimate for unmarried mothers is, remarkably, almost the same as reported by these authors despite the very different sources of variation in childcare availability. By contrast, we find a larger effect among married mothers than reported in this literature.

Education. We next consider the role of education in the labor supply response to school policy. We fit regression (3) to the noncollege group—workers with less than a four-year degree—and, separately, to workers who completed college. We further split each of these two groups by gender. Results are reported in Table 8.

²⁴ The reasons for these differences have been difficult to identify. One possibility is that the in-person shares are different; Hansen et al. produce their own with Safegraph data whereas we use Parolin and Lee (2021).

²⁵ It does not appear that Gelbach (2002) isolates lone-adult mothers.

Table 8: Estimates by Educational Background

	Noncollege	College
Coefficient	Women	
In-person share, β	0.635 [0.555]	0.792 [0.599]
In-person \times kids, ψ	2.071*** [0.771]	1.827* [1.001]
Number of obs.	266,258	181,641
Coefficient	Men	
In-person share, β	0.968** [0.463]	0.755 [0.700]
In-person \times kids, ψ	2.010*** [0.751]	1.072 [0.861]
Number of obs.	284,723	148,133

Note: This table presents estimates of equation (3) augmented with time \times parental status fixed effects. The column header reports the composition of the sample used in the regression. A college graduate is one who completed a four-year degree.

note about p-value?

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 not statistically different. Thus, among women, college experience is not a strong predictor of the labor supply response to school policy.

The education gradient among men is more evident, however. College-educated men do not significantly adjust their hours worked in response to variation in school policy. By contrast, among noncollege men, a shift from fully virtual to fully in-person implies an increase in weekly hours that is very comparable to the response observed among (noncollege and college-educated) women.

Another perspective on these results stresses the difference between men and women *within* the college group. This point is sharpened further if we consider households with college-educated spouses. The hours response among mothers in these households is even larger than among college graduate mothers as a whole. A shift from fully virtual to fully in-person *the response* implies an increase of 3.5 hours per week. By contrast, fathers' labor supply in such households appears to be essentially inelastic; the response of total household hours is hardly any different from what is observed among mothers. This imbalance between spouses is, again, *only evident* among the college educated. In

households where both spouses are noncollege educated, the hours responses are almost identical and sum to about 5 hours per week.²⁶

What may account for these differences in the household division of labor? The extent to which partners in a couple specialize in market work or home care may depend in part on the distribution of earnings within the household. For instance, the father may continue working if his earnings are highest, with his spouse allocating more time to childcare. This scenario is potentially consistent with the results for college graduates. Conversely, if the parents' earnings are similar, they may substitute time toward childcare to a (roughly) equal extent. This scenario is potentially consistent with the results for the noncollege educated. Thus, the pattern in Table 8 may arise if the father's *relative* earnings (within the household) are increasing in his educational attainment.

To pursue this point further, we draw on weekly earnings records from the CPS Outgoing Rotation Groups. The data are taken from 2019 and, therefore, capture the situation facing parents when on-site instruction was suspended. Earnings of the non-employed are set to zero. Interestingly, we find that fathers' relative earnings are nearly independent of educational attainment. Two moments of the data demonstrate this point. First, the share of fathers earning more than their spouses varies only modestly with schooling, falling within the narrow range of 70 to 75 percent depending on college attainment. These figures echo results in Winkler et al. (2005), who used annual earnings from the CPS March Supplement. Second, and more pointedly, the mean difference between a father's and his spouse's weekly earnings amounts to roughly three quarters of the couple's average earnings regardless of the father's schooling.²⁷ These moments reflect the tendency of fathers to have spouses with the same level of schooling; the college premia of the two balances out, leaving roughly the same differential as in noncollege households.²⁸

These findings challenge, but do not rule out, a narrative based on differences in the rates of return to market time. One caveat is that *current* earnings do not necessarily represent the present value of market time. Since returns to experience are somewhat higher among men (see Guvenen et al., 2022), a household may select the father for full-time work even if parents' current earnings are comparable. Alternatively, these findings may point away from this class of explanations altogether. Instead, differences in

²⁶ This figure is based on responses of heads of households and their spouses, which is a subset of the full sample. The hours responses of heads and spouses are somewhat larger than reported in Table 8. Therefore, the household response exceeds the sum of estimates in Table 8.

²⁷ These estimates do not condition on the mother's schooling. The father's average premium is 68 percent if both parents are college graduates and 88 percent if both have at most a high school degree. By this metric, the rationale for the mother specializing in childcare in college-educated households is weaker still.

²⁸ Nearly 80 percent of married fathers with a college degree have college-educated spouses. Similarly, almost three-quarters of noncollege-educated fathers have spouses with no more than a high school degree.

preferences for and/or norms around childcare may underlie the gender gradient in hours. It is an open question why these differences are present only among college graduates.

Telework. Another potential source of heterogeneity in labor supply stems from differences in job types or occupations. In the pandemic era, one particularly noteworthy dimension of an occupation was its *teleworkability*, e.g., the extent to which a job could be performed at home. Teleworkability can ease the trade-off between market time and childcare by enabling parents to supervise their children during standard working hours.

It is especially fitting to take up teleworkability after exploring the role of educational attainment since the two are highly related. We illustrate this point using a CPS question introduced in May 2020 that asked employed respondents if they worked from home at any point during the survey reference week. These data suggest that a college degree was a crucial pathway to a teleworkable position. For instance, the college-graduate share of teleworking men is 77.5, and the share among teleworking women is only slightly lower (73 percent). Importantly, these estimates are conditioned on selecting into a teleworking position; the results do not necessarily imply that all, or even most, college graduates do in fact telework. Nevertheless, we can confirm that teleworking is more common among the highly educated. For both men and women, just over a third of college graduates teleworked during our sample. Among the noncollege educated, just 6.5 percent of men and 11.5 percent of women report teleworking.

In this context, it is natural to suspect that teleworking opportunities enables college graduates to smooth their labor supply during periods of virtual instruction. However, the prevalence of teleworking on average does not necessarily imply that it was an active margin of *adjustment* to school policy. Parents may derive utility from teleworking for reasons independent of supervising children and will do so regardless of policy. Conversely, parents seeking to supervise children may not view teleworking as enabling the quantity of care they aim to provide, leading them to exit employment.

We can address this debate directly by estimating how the rate of teleworking among men and women varied with on-site instruction. A negative correlation between the two is highly suggestive that teleworking was used to stabilize employment during school closures. Since teleworking is measured only for the employed, though, we are careful not to make the causal claim that a parent would have been out of work without access to teleworking. To establish the latter, one needs more comprehensive measures of access to telework that can be applied to the employed and nonemployed alike.²⁹

²⁹ Even widely accepted classifications such as Dingell and Neiman (2020) are limited in this regard because most labor force nonparticipants in the CPS do not report a recent occupation, which is the basis of the

Proceeding, Table 9 reports results on the connection between teleworking and on-site instruction. The regression here is the same one underlying Table 8 but with the CPS question for teleworking inserted as the dependent variable. Relative to Table 8, the sample period is also slightly shorter since teleworking was not surveyed prior to May 2020. To put the results for teleworking in context, we also present estimates for employment (irrespective of teleworking) over the same sample period.

A few results stand out. To start, consider the college educated. The return of in-person instruction implies a statistically significant increase in employment among women but little if any response from men. These estimates echo the results on hours adjustments in Table 8. While men’s overall working time was static, it does seem that the teleworking share of that time did respond to school policy. Table 9 indicates that a shift from fully virtual to fully in-person implies a reduction in the incidence of teleworking of nearly 5 percentage points. This finding suggests that these fathers took up teleworking as a way of sustaining employment during school closures.

Table 9: Teleworking by Educational Background

	Noncollege		College	
	Working	Telewkg.	Working	Telewkg.
Coefficient	Women			
In-person share, β	-0.005 [0.016]	-0.032*** [0.009]	-0.018 [0.016]	-0.142*** [0.019]
In-person \times kids, ψ	0.049*** [0.018]	0.026*** [0.008]	0.057*** [0.021]	0.028 [0.021]
Number of obs.	204,863	204,863	140,590	140,590
Coefficient	Men			
In-person share, β	0.004 [0.011]	-0.005 [0.007]	0.012 [0.015]	-0.074*** [0.022]
In-person \times kids, ψ	0.009 [0.016]	-0.011 [0.009]	0.009 [0.017]	-0.048** [0.022]
Number of obs.	220,016	220,016	114,575	114,575

Note: This table presents estimates of equation (3) augmented with time \times parental status fixed effects. The column header reports the dependent variable, where “telewkg.” means that the respondent reported working from home for at least some time during the reference week.

note about p-value?

teleworkability indicator. The Online Appendix shows that labor supply responses are quite sensitive to how one treats nonparticipants’ teleworkability.

Teleworking patterns among college-educated women are decidedly different. The reinstatement of in-person instruction did not prompt a decline in teleworking among college educated mothers per se, although it was associated with a substantial decrease in teleworking among female college graduates *as a whole*. The estimated value of β indicates that a shift from fully virtual to fully in-person implies a 14 percentage-point decline (!) in the market-wide incidence of teleworking. The latter finding is a stark reminder that school policy changes may be correlated with a “return to the office” more broadly, highlighting the importance of inferring any impact of policy using the *relative* response of mothers. By the latter metric, teleworking was not a significant driver of the response to school policy, a result that arguably underlies why *overall* working time *was* responsive.

Lastly, consider the results for the noncollege educated. The individual point estimates can be vexing; it appears that noncollege-educated mothers increased, rather than decreased, teleworking after in-person instruction returned. Nevertheless, the broader theme of the estimates is clear: there is no indication here that these parents ramped up teleworking as a means of coping with school closures. Rather, mothers in particular responded to a loss of on-site instruction time by exiting employment altogether.³⁰

4. Estimates from Time Use Data

On balance, our analysis of CPS data suggests that a shift from fully virtual to fully in-person was associated with an increase of no more than two to four hours of work per week. The suspension of on-site instruction, however, had eliminated up to 30 hours or more of school-provided supervision. Thus, the more modest labor supply response would seem to suggest that parents adjusted to school closures on other margins. The CPS results suggest that fathers (if not mothers) may have been able to monitor children while they worked from home. Is there further evidence on this margin of adjustment? Alternatively, did parents substitute from leisure and/or home production toward childcare (and leave market work more or less intact)? The American Time Use Survey (ATUS) enables us to make some headway on these questions (Hofferth et al, 2020).

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Our ATUS sample is selected to conform to the extent possible with our treatment of the CPS. We again restrict attention to individuals ages 21-59 who are childless adults or parents of school-age children. (As above, parents are excluded if all their children are under school age.) The ATUS sample does start slightly later because the pandemic forced

³⁰ The absence of an employment response among noncollege fathers indicates that the significant response of weekly hours in Table 8 reflected a shift along the intensive margin.

the suspension of field work from mid-March to mid-May 2020. In total, we have observations on 6,622 individuals.

For each respondent, we observe a minute-by-minute diary of a single day that describes how, where, and with whom they spent their day. The respondent’s diary entries are assigned one of many detailed activity codes. We group these activities into a few broad categories: work, leisure, home production, childcare, commute time, and sleep.³¹ The daily number of hours spent in each of these categories forms the outcome variable in our initial regressions. To conserve space, though, results for commute time and sleep are placed in the Online Appendix; neither responds to variation in instruction format.

Our first batch of results is reported in Table 10, which presents estimates for the full sample. The specification is identical to equation (3) but augmented with time-by-parental status effects (as in Table 2). Unlike in earlier tables, though, we do not split the sample by gender because of the small sample size. Separate results for men and women are placed in the Online Appendix. Finally, while we observe daily hours in the ATUS, we scale the coefficients to put them on a weekly basis, consistent with CPS estimates.

These estimates reveal little, if any, reallocation of parents’ time use in response to shifts in instruction format. Specifically, the reinstatement of in-person instruction has no significant impact on any of the major categories of time use, from work to leisure, childcare, and home production. Furthermore, several estimates in Table 10 are not just imprecise but also of the “wrong” sign. For example, shift from fully virtual to in-person

Table 10: Time Allocation and Instruction Format

Coefficient	Work	Leisure	Childcare	Home Prod.
In-person share, β	0.439 [4.903]	-5.692 [3.463]	1.198 [0.798]	1.133 [2.383]
In-person \times kids, ψ	-4.575 [5.184]	4.694 [3.423]	-2.070 [2.116]	-0.252 [2.058]
Number of obs.	6,622	6,622	6,622	6,622

Note: Each column is a separate regression. The number of hours per day spent in the headlined activity is the dependent variable. Relative to equation (3), we also include fixed effects for the day of the week and time-by-parental status effects among the regressors.

³¹ Work refers to activities with codes between 50100 and 50199. Leisure refers to activities with codes between 120101 and 129999, between 130100 and 130299, between 140101 and 149999, or between 181201 and 181299. Childcare refers to activities with codes between 30100 and 30399 or between 180301 and 180304. Commuting refers to activities with codes between 180500 and 180599.

is estimated to imply a *reduction* in hours worked and an *increase* in leisure. (The implied reduction in childcare is more intuitive.) What we take from Table 10 is that, whatever ~~are~~ the “true” estimates of school policy, they are not large enough to detect in the ATUS sample of roughly 6,600 respondents. ^{are} Table 10 and online Appendix comes to the same conclusion when we split the sample by college attainment.³²

However, there is an important sense in which Table 10 does not fully leverage the available information in the ATUS. In addition to the activities undertaken, the ATUS records *where* each activity takes place. The ATUS also asks if there was a child in the respondent’s care. Thus, we can observe if a parent is supervising a school-age child while working at home. In this context, work is the *primary* activity, and childcare is referred to as the respondent’s *secondary* activity. Importantly, secondary childcare is *not* captured by the time use data underlying Table 10, which is based only on primary activities.

We use these features of the ATUS to revisit the behavior of telework. The response of telework hours to a change in instruction format may be large enough to detect in a small sample even if the same cannot be said for total work hours. Indeed, the absence of any significant labor supply response may reflect the role of teleworking in enabling parents to sustain their labor supply during school closures. The regressions that estimate telework hours estimate the same specification behind Table 10. Moreover, this regression is virtually identical to that applied to CPS data on telework, although the latter **model** the incidence of telework rather than total telework hours. As a point of comparison, the original labor supply regression results (see above) are also reproduced here.

Is a verb needed here?
If 'model' is the verb,
change to 'models'

Change period to comma

There are two notable results in Table 11. The first is that time spent working at home *while caring for children* is responsive to instruction format among college graduates. After a shift from fully virtual to in-person, time spent in this activity declined by a statistically significant 6.3 hours per week among college graduates, whereas the response of the noncollege educated was small, insignificant, and of the “wrong” sign. Thus, telework may have helped sustain the labor supply of college graduates, if not the noncollege group. A second notable result is that *total* time spent teleworking is nevertheless somewhat inelastic to school policy, even for the college educated. This finding suggests that interest in teleworking extended beyond the return of in-person instruction. Accordingly, by using a measure of total teleworking as in the CPS, we may understate the extent to which teleworking was used as a means of caring for children.

³² The discrepancy between the CPS and ATUS-based results is unlikely to reflect systematically different measurements of hours worked. Earlier analyses generally pointed to substantial agreement between the two sources on hours (Frazis and Stewart, 2004 and 2014).

Table 11: Work at Home, Childcare, and Instruction Format

	Work	Work at home	Work at home + childcare
Coefficient	All		
In-person share, β	0.439 [4.903]	-4.318 [3.009]	1.093 [0.657]
In-person \times kids, ψ	-4.575 [5.184]	-2.585 [4.693]	-4.792*** [1.609]
Number of obs.	6,622	6,622	6,622
Noncollege			
In-person share, β	0.516 [7.004]	0.096 [3.381]	-0.164 [0.386]
In-person \times kids, ψ	-6.647 [7.896]	1.624 [4.705]	-1.069 [1.963]
Number of obs.	3,371	3,371	3,371
College			
In-person share, β	3.113 [5.993]	-5.406 [5.004]	1.176 [1.040]
In-person \times kids, ψ	0.300 [6.051]	-3.662 [7.975]	-6.308** [2.781]
Number of obs.	3,178	3,178	3,178

Note: See Notes to Table 10. “Work at home” is the number of work hours per day spent in one’s own home or another home. “Work at home + childcare” measures the number of hours per day where “work at home” is the primary activity and “childcare” is secondary.

note about p-value?

5. Discussion

In this section, we use simple time allocation models to guide a discussion of our regression results. Our objective is narrowly defined. For the most part, we do not address the heterogeneity in hours responses observed across different sub-samples. Such an effort would require a far richer model, and more intensive numerical analysis, than we have space to pursue here. Instead, to focus this discussion, we zero in on what we see as one of the broader themes of the paper: on balance, the impact of school closures on labor supply appears to have been rather modest. Estimates suggest that a shift from all virtual to all in-person instruction implies a reduction in weekly hours of perhaps two to three (Table 3) and at most four to five (Table 7). Little, if any, related research finds larger

movements than we document. Thus, while there may have been means by which parents (partially) insulated their labor supply—by the aid of a spouse and/or telework—the heterogeneity in hours responses had to be limited since the upper bound of responses was not that high. Meanwhile, we showed that there are credible regression specifications that recover no impact whatsoever (see the placebo tests in Table 5).

Our first aim is to formalize why these results may be seen as unexpectedly small. We demonstrate this point within a simple environment where a single parent who faces a one-for-one tradeoff (in time) between labor supply and childcare. We find that, under any reasonable parameter values, the model implies labor supply responses that far exceed any estimate thus far presented. We do illustrate how a kind of telework technology can relax the work-childcare tradeoff and, therefore, mute the response of hours worked, as suggested by some results in the CPS and ATUS. Nevertheless, the *absence* of telework opportunities still does not generally imply outsized labor supply responses in the data, suggesting that a fuller account of the results is needed.

Would just "teleworking" work here?

Based on our initial analysis, we conclude that it is difficult to rationalize the regression results without some appeal to the role of *nonparental care*. The availability of nonparental care enables parents to smooth their labor supply and ensure the provision of childcare. We illustrate how our regression results could then be applied to help identify the salient parameters of a structural model of child development with nonparental care.

The challenge to this narrative is that the data on nonparental care is rather scarce. The ATUS offers a limited glimpse into nonparental care insofar as we observe time spent by older adults (65 years and over) on the care of school-age children, which we interpret as grandparent-provided care. We find that this form of care expands when in-person instruction is unavailable, although the increase of 6.5 hours per week is observed only among older noncollege educated women (see the Online Appendix). More broadly, nonparental care encompasses both informal care by relatives, older children, and family friends as well as paid center-based care. Evidence from a new survey fielded by Calarco et al. (2021) and Yang et al. (2022) points toward the importance of informal care in particular. The share of surveyed families that relied on such care in the pandemic period was on the order of 60 percent (Yang et al., 2022). Moreover, in their written responses, parents noted that this form of support enabled them to work while their children attended school remotely (Calarco et al., 2021). Nevertheless, substantially more data is needed on this form of care, which appears to represent an important kind of insurance for parents. We hope the results below spur additional research in this area.

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

To start, we assume there are only two forms of supervision, or childcare. 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 instruction time, but it arguably captures the dimension of in-person instruction that is most relevant to a study of *parental* labor supply. If the child is not in school, we assume he/she must be under the parent's full-time supervision.³³ We introduce private nonparental care below.

To proceed, it is helpful to make the parent's time constraints explicit. Leisure is denoted by l ; time allocated to child supervision by m ; and market hours of work by n . Finally, we let g be the time spent under publicly provided supervision. The time constraints specify that the parent's allocations add up to 1,

$$l + m + n = 1 \quad (7)$$

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

$$g + m = 1. \quad (8)$$

Together, equations (7) and (8) imply

$$l = g - n. \quad (9)$$

Thus, a decrease in in-class instruction time, g , will reduce leisure all else equal. A crucial choice in the model concerns the extent to which parents will ease labor supply to mitigate the decline in leisure in such a scenario.

Importantly, we assume g is taken as given by the parent. This approach rules out substitution from a school closed to in-person instruction to one that is open to it. Where this did occur, it often took the form of a movement from public to private school. However, the rates of reallocation to private institutions would seem to be far too small to make a material difference to our analysis (see Dee et al., 2021; Musaddiq et al., 2022).

Next, we assume that period utility is given by 2022

$$u(c, l, q) = \frac{(c^\alpha l^\beta q^{1-\alpha-\beta})^{1-\sigma} - 1}{1 - \sigma}, \quad (10)$$

³³ A corollary is that remote school instruction has no supervisory rule. A useful extension of our approach might interpret childcare in terms of the intensity of that form of supervision (rather than in terms of time).

where $\alpha, \beta \in (0,1)$ and $\sigma > 0$. Thus, the utility function has an inner nest that is a Cobb-Douglas aggregate over consumption, leisure, and a term, q , that indexes child development. The Cobb-Douglas form in equation (10) follows Berlinski et al. (2020). The outer nest has an isoelastic form with shape parameter σ .³⁴ To simplify many results in this section, we will set $\sigma = 1$, e.g., utility is logarithmic over the Cobb-Douglas aggregate,

$$u(c, l, q)|_{\sigma=1} = \alpha \ln c + \beta \ln l + (1 - \alpha - \beta) \ln q.$$

Should this have an eq #?

The separability of c , l , and q eases some of the algebra but is unlikely to alter our “bottom line”, a point to which we return shortly.

The function q measures the child’s development and is “produced” with both forms of supervision, g and m . From equation (8), though, we know that g implies m , that is, q is pinned down by g : $q(g, m) = q(g, 1 - g)$. Since g is taken as given by the parent, q acts merely as an intercept in the utility function, and with a logarithmic form of utility ($\sigma = 1$), exogenous shifts in this intercept may be ignored. A more substantive choice problem for q emerges if there is another source of supervision, e.g., a form of private care outside the home. Again, we return to this matter ~~a little~~ later.

In what follows, we will consider the model’s predictions for labor supply. Notably, the model only yields *interior* solutions for n , although the hours responses in the data, among women in particular, were driven by shifts in employment. In our view, what we sacrifice in realism in this regard is worth the tractability that it purchases, especially since our main results are likely to carry over to a model that distinguishes intensive from extensive adjustments. As we will see, the baseline model is instructive insofar as it clarifies the challenge posed by our empirical results: given credible parameter values, the model predicts an hours response to a Keane and Rogerson’s instruction (g) that is several multiples of what we find. Of course, as ~~Rogerson and Keane~~ (2012) stress, the relevant parameters for extensive-margin decisions will be different (for instance, a fixed cost of going to work). However, the latter are not *free* parameters. Rather, their values should be consistent with the quasi-experimental evidence, which generally finds intensive and extensive-margin Frisch elasticities that point to an overall response comparable to what we assume in our calibration (Chetty et al, 2011).

add hyphen: 'intensive- and'

Initial comparative statics. Under the assumption that $\sigma = 1$, and given equation (10), the first order condition for leisure implies

³⁴ The parameter σ serves as the intertemporal elasticity of substitution (IES) for fixed l and m . The IES will be higher if adjustments in l and m are accounted for (see Bilbiie, 2011).

$$g - n = \frac{\beta}{\lambda w}, \quad (11)$$

where λ is the marginal value of wealth. It is instructive to suppose for the moment that the worker can insure against temporary shocks in a life cycle environment. More exactly, she can borrow and lend sufficiently to smooth the marginal utility of consumption. Therefore, the right side of equation (11) is (practically) invariant to g . It follows that n moves *one for one* with a change in g . Intuitively, the demand for leisure does not change since its opportunity cost is fixed given λ and w . Therefore, n must adjust to offset a shift in g , which leaves l unchanged.

This stark result does not obtain under the more general case of $\sigma \neq 1$. However, in the Online Appendix, we argue that, if we are to depart from $\sigma = 1$, the relevant literature strongly suggests $\sigma > 1$. We then show that the latter will in fact *amplify* the labor supply response (relative to the case $\sigma = 1$). The reason is that, under equation (10), l and q are substitutes if $\sigma > 1$, e.g., $\partial^2 u / \partial l \partial q < 0$. If we suppose that q falls when g is lowered, the marginal utility of leisure rises, and the worker further reduces time in market work. However, as we do not have any evidence on these second order properties of $u(\cdot)$, we defer this matter for now and work with the separable case where $\sigma = 1$.³⁵

One might object to our interpretation of equation (11) on the grounds that perfect insurance is unrealistic. We would note, though, that the transfers made available under the CARES Act (and, later, the American Rescue Plan) likely did enable households to smooth their marginal utility to a considerable extent (Wu et al., 2022). Nevertheless, it would be instructive to consider an alternative to the case of perfect insurance.

We now make the opposite assumption, namely, the agent lives “hand to mouth”. Therefore, consumption now must satisfy $c = wn$, where w is the wage rate. It follows that $\lambda = \alpha/c = \alpha/wn$, and equation (11) becomes

$$l = g - n = \frac{\beta}{\alpha} n. \quad (12)$$

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

$$dn = \frac{1}{1 + \beta/\alpha} dg. \quad (13)$$

³⁵ There has been research that sheds light on the question of whether consumption and leisure are complements or substitutes (see the discussion in Bilbiee, 2011). It is unclear, however, how this evidence informs how we calibrate the cross partial derivatives with respect to l and q .

Equation (12) says that β/α is identified by the ratio of leisure to market work time. Based on data from the American Time Use Survey, we then find that $\beta/\alpha = 1.1$. Therefore, an hour more of in-person instruction implies an increase in market work of approximately 0.5 hours.³⁶

This prediction would seem to far exceed any of our empirical estimates (or of estimates elsewhere in the literature). A shift from fully virtual to fully in-person will reinstate, on average, 32.5 hours per week of on-site instruction (NCES, 2008). The result in equation (13) predicts that such a shift should lift labor supply by 16 hours per week. Conversely, the closure of schools to on-site instruction is predicted to dramatically reduce parental hours worked, which is arguably consistent with some predictions at the start of the pandemic (see discussion in Goldin (2022)). By contrast, our OLS estimates suggest a labor supply response no higher than 2-4 hours per week.

Add to ref list

In fact, reasonable alternative calibrations of equation (13) will likely yield even larger predicted changes in market work. To see why, it is straightforward to show that β/α is in fact the Frisch elasticity in this model. If we were to calibrate the latter based on standard estimates of the Frisch elasticity (rather than based on time use data), the pass through from g to n would be larger. For example, consider a Frisch elasticity of 0.7 based on the review in Hall (2009). (Chetty et al. (2011) land on a very similar figure.) It follows that a shift from fully virtual to fully in-person instruction now yields an increase in market work of 19 hours.

Intriguingly, this example illustrates that labor supply is *more* responsive to a change in g when the Frisch elasticity is *lower*. The intuition for this result is in fact straightforward. The Frisch elasticity is inversely related to the curvature of utility over l : labor supply rises less after a wage increase if log marginal utility of leisure rises rapidly as n grows. Now, in our case, we consider a decrease in g , which reduces leisure *directly* (all else equal). Labor supply will fall more if this reduction in leisure implies a large increase in (log) marginal utility. But to say that marginal utility reacts sharply to a shift in l is to say that the Frisch elasticity is small.

Telework. A key assumption embedded in equation (7) is that parents cannot *simultaneously* perform market work while they supervise children. However, there is evidence from the CPS and ATUS suggesting that telework opportunities, at least for

³⁶ This calculation excludes hours of sleep from leisure time. Alternatively, if we treat hours of sleep in excess of some minimum—say, 6 per night—as leisure, then the pass-through rate from g to n falls to 1/3, and the increase in market work implied by a shift to in-person instruction falls from 13 to 10.5.

college graduates, enabled parents to provide some childcare even as they continued to supply labor. We illustrate a tractable way to capture this notion of telework in the model.

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 $8 + 2 = 10$ hours. That is, these two activities may, to some degree, be done concurrently. Formally, the time aggregator function maps units of time engaged in market work, n , and units of time engaged in childcare, m , into the *total* time that has passed while engaged in one or both activities. The function is assumed to have the form,

$$t(m, n) = (m^\rho + n^\rho)^{1/\rho}, \quad (14)$$

where $\rho \geq 1$. The time constraint (7) then generalizes to $l + t(m, n) = 1$. Leisure, l , is implicitly defined here as the *absence* of any other activity and, therefore, enters the time constraint separably (outside of t).

Equation (14) encompasses two polar cases. The first is $\rho = 1$, which recovers the original time constraint (7), $l + m + n = 1$. 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 $\rho \rightarrow +\infty$, which implies that $t(m, n) \rightarrow \max\{m, n\}$. In this case, the two activities are perfectly *nonrivalrous*—an additional hour of market work can be completed concurrently with an hour of childcare.

These two polar cases are bridged by a continuum of (finite) $\rho > 1$. In this region, a few properties of equation (14) will be important. First, (14) 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. Therefore, we say the *time price* of an activity is less than one. Second, the time price of an activity (i) increases in the time allocated to it (e.g., t is convex) and (ii) *decreases* in the time allocated to the *other* activity (e.g., $\partial^2 t / \partial n \partial m = \partial^2 t / \partial m \partial n < 0$). The intuition here is that, if m is large relative to n , a parent can identify many childcare tasks that can be done concurrently with market work but few work tasks that can be done jointly with childcare. Therefore, the time price of another hour of work is relatively small, as conveyed by (ii), but the price of another hour of care is relatively large, as conveyed by (i).

The properties (i) and (ii) formalize the sense in which equation (14) 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, and implement it (to an extent) concurrently with childcare. 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 $\tau_n/\tau_m = (m/n)^{1-\rho}$. At higher values of ρ , the relative price of market work falls more rapidly as m grows relative to n , which means that parents will want n to partially “catch up” to m . In the limit where $\rho \rightarrow +\infty$, the catch-up is complete, e.g., a (momentary) increase in m above n leads an equal increase in n .

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

$$l = 1 - \tau(m, n) = \frac{\beta}{\lambda w} \cdot \frac{\partial \tau}{\partial n}. \quad (15)$$

A decline in publicly provided supervision, g , now has two effects. The first is familiar and relates to the left side of equation (15): $m = 1 - g$ must rise, which would diminish leisure all else equal. To stem the fall in leisure, labor supply is reduced.³⁷ The second effect is novel and captured by the time price of market work, $\partial \tau / \partial n$, on the right of equation (15). An increase in m reduces the latter, e.g., $\partial^2 \tau / \partial n \partial m < 0$. As a result, there is now a motive to supply more, rather than less, work when g falls.

To investigate these points more fully, we inspect the comparative static of n with respect to g . Under perfect insurance ($d\lambda = 0$), we have

$$\frac{dn}{dg} = \frac{1 - (\rho - 1)\phi(l)}{(n/m)^\rho + (\rho - 1)\phi(l)} \cdot \frac{n}{m}, \quad (16)$$

where $\phi(l) \equiv l/(1 - l)$ and $m = 1 - g$. When $\rho = 1$, equation (16) collapses to $dn/dg = 1$: market work declines one for one with a fall in g . Values of $\rho > 1$ can attenuate the decline in labor supply, e.g., $dn/dg \in [0, 1)$. In fact, equation (16) implies that the decline is eliminated entirely if $\rho = 1 + \phi(l)^{-1}$. The term, $\phi(l)$, is the Frisch elasticity of a childless adult (for whom $m \equiv 0$), the inverse of which signals the degree of curvature over l in the utility function. To induce $dn = 0$, the motive to multi-task must be strong enough to “cancel out” this curvature. Although arguably knife-edge, this case is instructive because the regression results (Section 4 in particular) suggest that telework may have fully insulated the labor supply of some parents when in-person instruction was suspended. The technology in equation (14) is flexible enough to reproduce this outcome.

The model, as distilled in equation (16), could be used more generally to estimate ρ in other contexts where $dn > 0$. The identification of ρ is particularly straightforward

³⁷ However, the quantitative impact of even this “conventional” channel is different under equation (14). To illustrate, suppose m rises by one, but we wish to prevent any fall in leisure. When $\rho = 1$, n also declines by one. However, for $\rho > 1$, the required decline is shaped by the form of τ and equal to $dn = \tau_m/\tau_n$.

if the data are such that $n/m \geq 1$. In this case, the comparative static monotonically increases in ρ for any observed l , which ensures that there is a $\rho > 1$ that can reproduce any observed dn/dg .³⁸ Although we do not pursue any further quantitative analysis here, this framework could be applied in future research.

Nonparental care. Thus far, we have assumed that a child must be supervised by ~~her~~ school or parent. However, two data points strongly suggest that time in private nonparental care was an important margin of adjustment to school closures. First, labor supply responses are relatively modest even among workers with little access to telework (i.e., the noncollege educated). Second, the stasis in parents' time use more generally—the absence of a clear response of any other activity—is hard to rationalize unless the loss of in-person instruction time was filled by *nonparental* care. Accordingly, we incorporate into the model the choice of time in private nonparental care, denoted here by x . The analogue to equation (8) is then

$$g + m + x = 1, \quad (17)$$

which says that a child is supervised by a school, parent, or private third party.

The revised time constraint implies a simple, but potentially substantive, change in labor supply. The first order condition for hours worked is now,

$$n = g + x - \frac{\beta}{\lambda w}. \quad (18)$$

Market work, n , no longer necessarily moves one for one with in-person instruction time, g . Rather, market work 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.

The remainder of the analysis centers on the determination of x . Together with public and parental supervision, nonparental care is an input into the child's development. A CES aggregator is a common specification for development “production” functions since at least Cunha et al. (2010). Within this class, a reasonably general form is

$$q = (\gamma g^\eta + (1 - \gamma)q(m, x)^\eta)^{1/\eta} \quad \text{with} \quad (19)$$

$$q(m, x) = (\mu m^\psi + (1 - \mu)x^\psi)^{1/\psi},$$

³⁸ Indeed, there a $\rho > 1$ that even corresponds to any given $dn < 0$. As indicated by equation (16), this ρ must be greater than $1 + \phi(l)^{-1}$. See the Online Appendix for more on this point and for a discussion of the role of the initial optimum n/m in the comparative static.

where $(1 - \psi)^{-1}$ is the elasticity of substitution between parental (m) and private nonparental care (x); and $(1 - \eta)^{-1}$ is the elasticity of substitution between on-site instruction time (g) and the “bundle” of private care (q). The form of q borrows from Berlinski et al. (2020). The literature offers less guidance as to how g should enter. Equation (19) adopts a nested form in which public care does not necessarily enter symmetrically with the elements of private care (though symmetry is recovered if $\eta = \psi$).

A tractable special case of equation (19) takes the limit $\eta \rightarrow 0$, which yields

$$q = g^\gamma (\mu m^\psi + (1 - \mu)x^\psi)^{(1-\gamma)/\psi} \quad (20)$$

The Cobb-Douglas outer nest implies that g and $q(m, x)$ are logarithmically separable.³⁹ However, g and (m, x) *do* interact indirectly through the time constraint (17), $m = 1 - g - x$. The analysis in the main text draws on equation (20), as it suffices to illustrate our key points. Results based on equation (19) are presented in the Online Appendix and corroborate our “bottom line”), as articulated below.

The optimal choice of any form of care trades off the value of another hour of time to the child, as implied by equation (20), with the price of that care. The price of parental care is the foregone market wage, w , whereas nonparental care has price per unit time denoted by p . It follows that, at the optimum, the marginal product of parental care *net of* nonparental care is equated to its relative price, $w - p$. Once the time constraint $m = 1 - g - x$ is then imposed, we recover the optimal choice of x . Note that p is “small” here in the sense that $w > p$ to account for the prevalence of informal care, e.g., supervision by friends, neighbors, grandparents or older children (Yang et al., 2022). A formal presentation of the solution is given in the Online Appendix.

We now consider the comparative static, dx/dg . First, we verify in the Online Appendix that $dx/dg < 0$: a decline in publicly provided supervision is offset (at least partially) by a rise in private nonparental care. In addition, the appendix characterizes how the production parameters (ψ, μ) shape this comparative static. The results are clearest if the initial optimum satisfies $x < m$, which imparts a strong motive to ramp up nonparental care rather than accept further parental supervision (after g falls). In this context, a higher elasticity of substitution between nonparental and parental care supports a larger increase in x . There is also a stronger tendency to increase nonparental care if μ is higher. Intuitively, a higher μ attenuates the marginal impact of x for which the parent

³⁹ Del Boca et al (2014) assume q is Cobb-Douglas with respect to all arguments.

compensates by assigning more time to nonparental care.⁴⁰ Thus, in this context, $|dx/dg|$ increases in ψ and μ . In the appendix, we show that data from the National Survey of Early Care and Education (NSECE) supports the initial condition, $x < m$.

Equation (18) translates these results into implications for labor supply. Specifically, the higher take-up of nonparental care induced by higher values of ψ and μ enables parents to mitigate the decline in labor supply after a disruption to on-site instruction. Put another way, the modest *observed* response of hours worked places restrictions on the value of the pair (ψ, μ) . In this sense, our regression estimates can inform the identification of (ψ, μ) .

Figure 3?

To illustrate this point, Figure 2 presents the locus of (ψ, μ) consistent with an estimate of the comparative static and given several alternatives for the initial value of $\xi \equiv x/m$. The range of ξ s is bounded above by one, as suggested earlier. The comparative static assumes that labor supply increases 4 hours per week—the upper bound of our CPS-based estimates (see Table 7)—when 32 hours per week of in-person instruction are reinstated. Therefore, $dn/dg = 1/8$, which implies $dx/dg = dn/dg - 1 = -7/8$ (see equation (18)). Along any fixed- ξ locus, every point (ψ, μ) induces this targeted response. Since an increase in either ψ or μ elevates $|dx/dg|$, a higher μ must be met by a smaller ψ to keep dx/dg fixed. Hence, each fixed- ξ locus is *downward*-sloped. To select a single point on a locus, one would have to target a second moment in addition to dx/dg . Our approach requires fewer inputs but still sheds light on salient structural parameters.⁴¹

The figure indicates that our regression results generally point to $\psi > 0$ (gross substitutes). Indeed, unless one has a strong prior that μ approaches one, values of ψ nearer to one than zero are more likely. Notably, Berlinski et al (2020) estimate $\psi \approx 0.7$ when they fit a generalized form of q in (19) to parents of *preschool* children. Figure 2 suggests that the production technology for school-age children may be similar in this dimension.⁴² This (tentative) conclusion is perhaps unexpected. The thrust of Ramey and Ramey’s (2010) “rug rat race” narrative is that parental time is essential to many child enrichment activities, especially with respect to school-age children. This argument does not imply $\psi < 0$ but does stress the manners in which parental and nonparental inputs

⁴⁰ This result is unambiguous if parental and nonparental inputs are gross complements ($\psi < 0$) but continues to hold for most $\psi > 0$ in our quantitative analysis if x/m is sufficiently small.

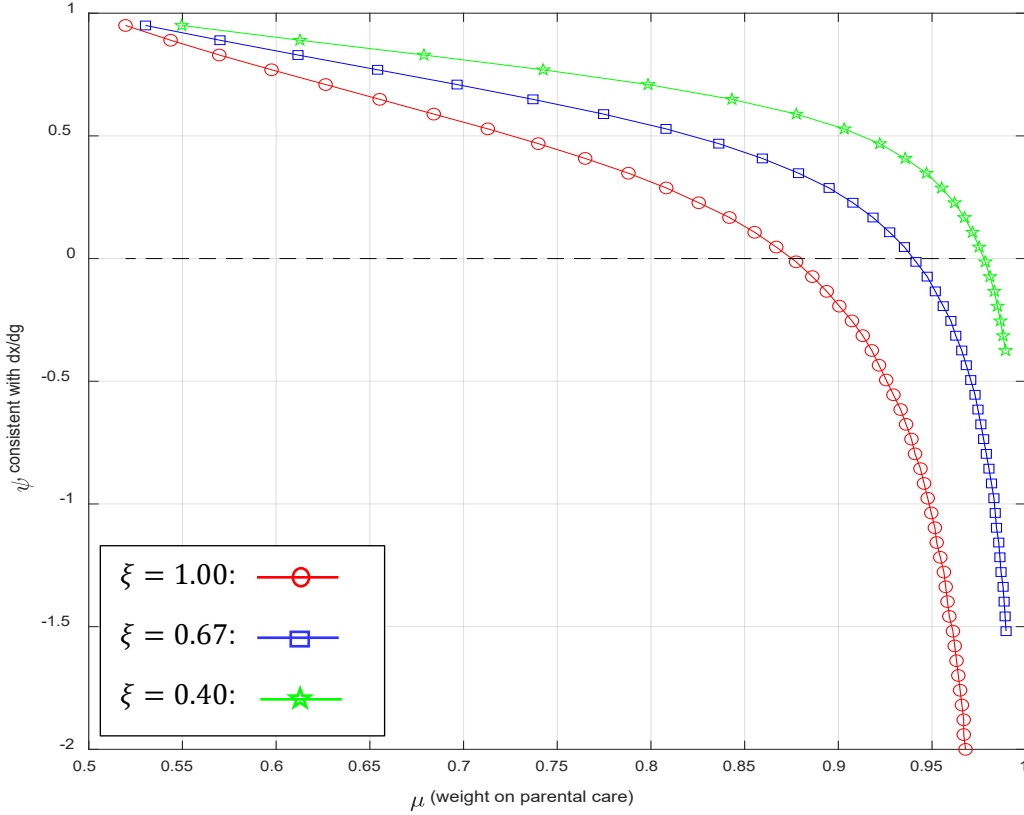
⁴¹ For instance, one might select a pair (ψ, μ) such that the given ξ is optimal when faced with the observed cost of parental care. To implement this strategy, however, one would also have to recover the many other parameters in the first order condition (19) as well as the marginal value of wealth, λ .

⁴² See Hurst (2010) and Sacks and Stevenson (2010) for critiques of Ramey and Ramey’s interpretation. For another insightful discussion of parental time use, see Guryan, Hurst, and Kearney (2008).

Figure 3?

capitalize highlighted

Figure 2: Model parameters consistent with observed labor supply responses



Note: This figure plots the locus of parameters (μ, ψ) consistent with the comparative static, $dn/dg = 1/8$. Each of the three loci is conditioned on a different initial value of $\xi \equiv x/m$. The x-axis starts at one-half because $\mu < 1/2$ is inconsistent with an optimal choice of $\xi \leq 1$. See the Online Appendix for a formal treatment of the latter point.

are imperfect substitutes. While our inferences from Figure 2 may still be consistent with their observations, further research may be needed in this area.⁴³

To close, we highlight that the implications of this discussion for the shape of the development production function are relevant to the study of adolescent development policies. The federal government's role in adolescent care grew substantially with the pandemic-era expansion of the Child Care and Development Fund, which is available for families with children up to high-school age (U.S. Dept. of Health and Human Services, 2021). The \$40 billion of subsidies effectively lowered the price to parents of nonparental care (p in our model). Other, more targeted interventions, including mentoring and

⁴³ There is no analogue to μ in Berlinski et al since they distinguish inputs of mothers from fathers. Still, their estimates suggest (to us) a total parental share among parents' and market inputs just above 0.5. From estimates in Del Boca et al (2014), we infer a total parental share (again, conditioned on parents' and market inputs) of 2/3 to 3/4 (see their Figures 2-3 for school-going ages).

counseling, can be thought of as elevating the productivity of nonparental care (see Cunha et al (2006) for a review). This effect can be captured by generalizing the inner nest of

equation

(16) to $q = (\mu(z_m m)^\psi + (1 - \mu)(z_x x)^\psi)^{1/\psi}$ where z_m and z_x are factor-augmenting productivities. The parameters ψ and μ will shape the comparative statics of each policy and, thus, inform our models of, and predictions for, such interventions.

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 any labor supply response. Even if we omit these controls, though, the labor supply responses represent a small fraction of the more than 30 hours of childcare time “lost” with the suspension of in-person instruction. Across different samples, and given a more streamlined specification, a shift from fully virtual to in-person generally implies an increase in hours worked of around two per week and rarely more than four. The paper uses a simple model of parental time allocation to formalize why these responses are unexpectedly small. Extensions to incorporate telework and nonparental care can help bridge the gap between the model and our regression estimates.

margins of adjustment?

Our exploration of the roles of telework and nonparental care is limited, however, by the availability of data on these margins of adjustments. We saw hints in the ATUS that working from home while supervising children was a potentially important means of coping with school closures. However, the survey’s small sample size poses a challenge, as it is difficult to put these results in context when the response of total work hours is estimated so imprecisely. Evidence on nonparental care in the pandemic era is even scarcer, despite recent efforts by Calarco et al. (2021). Outside of the pandemic, data is available through the NSECE (see Section 5), but its irregular administration and absence of a panel dimension limit its use for the study of higher-frequency adjustments.

We hope that our work stimulates further efforts to measure these activities going forward. While the pandemic era was unprecedented, we suspect it may have highlighted the importance of margins that operate more generally. For example, adjustments to nonparental care can enable households to respond to changes in the demand for market hours. Conversely, shifts in the composition of the household—for instance, an older child moves in—can have implications for parental labor supply.

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