How Does Work from Home Affect Mothers' Labor Supply During Summer School Closures?

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Abstract

Each summer, school closures increase childcare demand and contribute to a seasonal decline in mothers' labor force participation. Access to work from home may mitigate this decline. Using monthly Current Population Survey data on mothers whose youngest child is aged 6 to 12, I find that the decline in employment in July is 2.6 percentage points smaller for mothers with access to work from home than for mothers who work exclusively onsite. The gap emerges in June, peaks in July, narrows by August, and shifts with the timing of state school calendars. The effect comes mainly from labor force exits, with a smaller and less precise contribution from unemployment, and mothers who leave the labor force most often report family care as the reason. I find no effect on hours among employed mothers. No comparable response appears among fathers or women without children, and a pre-COVID placebo from a period when remote work was rare shows no similar summer pattern. More broadly, the findings show that access to flexible work arrangements can shape whether temporary increases in childcare demand lead to interruptions in mothers' employment.

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I. Introduction

Each summer, schools close for 10 to 12 weeks, and the daytime supervision they provide to roughly 54 million children in the United States disappears (NCES, 2024). This increase in childcare demand falls disproportionately on mothers, who provide a larger share of childcare and whose labor supply is more sensitive to constraints on the timing and location of work (Buzard, Gee, & Stoddard, 2024; Wasserman, 2023). Consequently, mothers may respond by reducing hours, taking leave, or leaving employment altogether. Price and Wasserman (2023) document the scale of this adjustment: between May and July, the employment-to-population ratio among prime-age US women falls by 1.1 percentage points while men's rises slightly, and among mothers whose youngest child is aged 6 to 12 the decline reaches 2.3 percentage points. These interruptions contribute to the persistent gender gaps in labor market outcomes that childcare responsibilities generate (Cortés & Pan, 2023; Kleven, Landais, & Søgaard, 2019). Whether mothers can remain employed through these seasonal disruptions depends on how much flexibility their jobs provide (Sherman, 2020).

The rise of work from home (WFH) represents a substantial and persistent increase in workplace flexibility. Before the COVID-19 pandemic, about 5 percent of paid hours were worked from home; the share peaked at around 50 percent during the pandemic and has since stabilized at roughly 20 to 25 percent (Barrero, Bloom, & Davis, 2021). A similar shift occurred across other countries (Aksoy et al., 2022). Previous studies demonstrate that women value the option to work remotely more than men do (Jalota & Ho, 2024; Mas & Pallais, 2017; Wiswall & Zafar, 2018), and WFH can ease the constraints facing workers with caregiving responsibilities by removing the commute and loosening the link between work and a fixed location (Bloom, Dahl, & Rooth, 2024). Yet whether these gains translate into improved labor market attachment remains an open question. Working from home does not eliminate the difficulty of supervising a child while working, and a mother who is at home may face more interruption. However, even if working from home makes it harder to work, it may still allow mothers to work when the alternative is leaving employment altogether. This paper asks whether access to WFH allows mothers to remain employed when summer school

closures raise childcare demand.

Summer school closures provide a particularly useful setting for isolating how workplace flexibility shapes mothers' responses to childcare demands. The timing of summer closures is determined by school calendars rather than by individual mothers' labor supply decisions, providing a predictable source of seasonal variation in childcare demand. Non-summer months provide a benchmark for assessing whether mothers with and without WFH access follow similar labor market patterns when childcare demand is lower. The setting also provides two distinct comparison groups. Women without children are not exposed to school closures, so any summer patterns they display would capture general seasonality in occupations or labor markets rather than responses to childcare constraints. Fathers of similarly aged children experience the same closures in their own households, yet childcare responsibilities within those households generally fall unevenly. Their summer patterns provide a benchmark for household-level disruptions operating through channels other than maternal caregiving.

For the main analysis, I use the Current Population Survey (CPS) Basic Monthly Survey from October 2022 through September 2025, the period in which the CPS consistently measures work from home. I focus on mothers aged 25 to 49 whose youngest child is between 6 and 12 years old. This age range captures mothers whose children are generally in school during the academic year but require additional childcare when schools close during the summer. Because the CPS asks about work from home only among employed respondents, I restrict the sample to mothers who are observed working at least once during the CPS panel. The final main-analysis sample contains 91,996 person-month observations.

I estimate an event study that compares labor supply across calendar months for mothers with and without WFH access, using May as the reference month. The specification includes individual, year-month, and occupation-by-month fixed effects. The identifying assumption is that, absent WFH access, mothers with and without WFH access would have experienced similar seasonal changes in labor supply within occupations. Three patterns support this assumption: differences outside the summer months are small and statistically insignificant, no comparable summer effect

appears among fathers or women without children, and applying the same design to 2010–2019, when WFH was rare, yields no similar effect. I focus on July, the month in which school closures affect the largest share of mothers.

Access to WFH substantially reduces the summer decline in maternal employment. Relative to May, the employment gap between mothers with and without WFH access widens by 2.6 percentage points in July, significant at the 1 percent level. The difference is absent outside the summer months, opens in June, peaks in July, and closes by August. The effect is driven primarily by labor force participation. Mothers with WFH access are 1.6 percentage points less likely to leave the labor force, while the change in unemployment is small and statistically insignificant.

Conditional on employment, the difference in hours between mothers with and without WFH access is small and statistically indistinguishable from zero. Thus, WFH access increases total hours mainly by keeping more mothers employed through the summer, rather than by increasing hours among those who remain employed. Mothers with WFH access are also 3.3 percentage points less likely to report a week-long paid absence in July, with the decline driven almost entirely by vacation.

Several findings support the interpretation that the summer employment effect reflects the value of WFH for managing childcare demands. Neither placebo group shows a July effect that is statistically distinguishable from zero. Fathers of children in the same age range face the same school closures but typically bear less of the resulting childcare; the estimated July effect is -0.04 percentage points. For women without children, who are not exposed to school closures, the corresponding estimate is 0.88 percentage points. The response also tracks state school calendars. In early-closing states the employment gap appears in June, whereas in late-closing states it does not emerge until July. In households with one child, the employment effect is concentrated among mothers whose child is aged 6 to 12 and is smaller for mothers of younger or older children, whose childcare arrangements are less directly tied to the school calendar.

The estimate is robust across alternative samples and specifications. It remains similar when teachers are excluded, individual fixed effects are dropped, alternative reference months are used,

and different clustering schemes are applied. The result is also robust to alternative definitions of WFH access: assigning access based on the first observed month gives an estimate of 2.81 percentage points, while using the first observed non-summer month gives 2.27 percentage points. Propensity score matching on baseline demographics and education, with exact matching on occupation, gives 2.48 percentage points. If the estimate reflected unobserved characteristics of workers in remote-capable occupations rather than remote work itself, a similar pattern should appear before remote work was common. Applying the same design to 2010–2019, with WFH access imputed from a model estimated on the main sample, gives a July estimate near zero.

This paper makes two contributions to the literature. First, it provides evidence that access to WFH reduces the labor supply response to recurring childcare disruptions. Price and Wasserman (2023) establish that summer school closures reduce women's employment and hours and widen the gender gap in labor supply, with the largest declines among mothers of school-age children. Their analysis ends in 2019, before remote work became widely available. I build on this evidence by asking whether flexible work arrangements can mitigate the labor supply disruptions caused by summer school closures. A broader literature documents similar responses to other childcare disruptions, including teacher strikes (Jaume & Willén, 2021), alternative school calendars (Duchini & Van Effenterre, 2020; Graves, 2013), and pandemic school closures (Albanesi & Kim, 2021; Alon, Coskun, Doepke, Koll, & Tertilt, 2022; Amuedo-Dorantes, Marcén, Morales, & Sevilla, 2023; Goldin, 2022), but says relatively little about which job characteristics enable mothers to withstand them. I show that one such characteristic, access to WFH, substantially reduces these employment losses, operating primarily by preserving employment and reducing labor force exits rather than by increasing hours among mothers who remain employed.

Second, the paper extends the literature on flexible work arrangements by showing how WFH affects labor supply in response to a recurring childcare shock. Existing work establishes that WFH raises retention and participation among workers facing barriers to onsite employment, including new mothers (Harrington & Kahn, 2025), workers with disabilities, and those with long commutes (Bloom et al., 2024; Zarate, 2025), and related literature examines how remote work

affects productivity, worker preferences, and time use (Bloom, Liang, Roberts, & Ying, 2015; Choudhury, Foroughi, & Larson, 2021). The paper most closely related to mine is Harrington and Kahn (2025), which uses an instrumental variables approach to ask whether access to WFH affects women's labor force participation after childbirth, using a sample drawn from two professions. This paper differs in two respects. First, the shock is recurring rather than one-time. Summer school closures occur every year on a predictable schedule and affect mothers six to twelve years after childbirth, allowing me to examine how WFH shapes labor supply well beyond the transition into motherhood. Second, I study mothers across the full occupational distribution, which permits comparisons within occupations and captures the broader population of mothers facing this shock rather than a particular professional setting. WFH here functions less as an accommodation to a one-time transition than as a durable job characteristic that helps mothers absorb a recurring childcare constraint.

The remainder of the paper proceeds as follows. Section II describes the data and sample construction, while Section III outlines the empirical strategy. In Section IV, I present the main results and heterogeneity analyses. Section V examines mechanisms, as Section VI reports robustness checks and falsification tests. Finally, Section VII discusses broader impacts and limitations of this analysis.

II. Data

The primary data source is the Current Population Survey (CPS) Basic Monthly Survey, a nationally representative monthly household survey conducted by the U.S. Census Bureau for the Bureau of Labor Statistics.[1] I link respondents across interviews to form a person-by-month panel, so that the same mother is observed in both summer and non-summer months of the same year. The sample period begins in October 2022 because that is when the CPS began measuring telework

[1] I access the data through IPUMS (Flood et al., 2025). A key advantage of the monthly files, relative to the Annual Social and Economic Supplement (ASEC), is that individuals can be followed across months, which is what permits the within-person seasonal comparison used here.

consistently[2] and ends in September 2025, the most recent month available.

The CPS uses a rotating panel design in which households are observed for four months, leave the sample for eight months, and return for four additional months. I link individuals over time using standard CPS identifiers and validate matches based on stable demographic characteristics such as age, sex, and race (Frazis, Robison, & Evans, 2005; Madrian & Lefgren, 2000; Price & Wasserman, 2023). All estimates use CPS sampling weights so that they represent the U.S. population.

The analysis focuses on mothers aged 25 to 49. Restricting the sample to prime-age adults reduces seasonal labor supply variation related to respondents' own school enrollment or retirement, which could otherwise confound the seasonal variation of interest (Price & Wasserman, 2023). I link mothers to their children using CPS family relationship codes and restrict the sample to mothers whose youngest child is between ages 6 and 12. Children in this age range attend school during the academic year but are generally too young to be left unsupervised for extended periods. School is therefore their primary source of daytime supervision, and its removal in summer produces the sharpest increase in childcare demand. Children younger than 6 are typically in childcare arrangements that operate year-round rather than on the school calendar, while adolescents require less direct supervision.

The sample is further restricted to mothers observed employed at least once during their panel, since the telework question is asked only of respondents who are working. The estimates therefore describe mothers with some attachment to the labor market rather than all mothers of school-age children. A mother is coded as having WFH access if she reports working from home at any point during her panel; Section III discusses this measure and its limitations, and Section VI.D reports alternative definitions. The final sample consists of 22,508 mothers and 91,996 person-month

[2]The CPS includes two telework measures. *COVIDTELEW* captures telework due to the COVID-19 pandemic and is available from May 2020 through September 2022. Starting in October 2022, the CPS introduced *TELWRKPAY*, which indicates whether employed respondents teleworked in the last week. I use this latter measure for consistency over the sample period. The variable is asked only of respondents who are currently working, so telework status is unobserved for individuals on leave or out of employment. It also does not distinguish between multiple jobs and records where work was performed during the reference week rather than whether the respondent's schedule is generally flexible.

observations, of which 88,661 are used in estimation after singleton observations are dropped.

The main outcomes are employment status, hours worked, and work absences, capturing both the extensive and intensive margins of labor supply. Employment follows the standard CPS labor force classification. Hours refer to actual hours worked during the reference week, and I also construct an indicator for full-time status, defined as usual weekly hours of 35 or more. An absence is recorded only when an employed respondent was away from work for the entire reference week, so partial-week leave is not observed. For absences, I distinguish between paid and unpaid leave and identify vacation or personal days separately among the reported reasons.

Table 1 reports weighted means by WFH status, for all women aged 25 to 49 and for the estimation sample of mothers whose youngest child is aged 6 to 12. Mothers with WFH access differ substantially from those without. They are more educated, with 66 percent holding a college degree compared with 36 percent among mothers who work exclusively onsite, and are more likely to be married with a spouse present (75 versus 62 percent).[3] They are also about two years older on average, while the number of children is nearly identical across the two groups.

These differences extend to labor market outcomes. Mothers with WFH access are employed in 97 percent of person-months, compared with 92 percent among onsite mothers, and are correspondingly less likely to be out of the labor force. They work about four more hours in the reference week, although the gap falls to two hours when conditioning on positive hours, and they are more likely to work full-time. These differences are present throughout the year (Figure 1). They may reflect selection into remote-capable jobs, the effect of WFH itself, or both, and the empirical design does not attempt to distinguish between these explanations. Instead of comparing average outcomes across the two groups, the empirical strategy compares how outcomes change across calendar months within individuals. Section VI.A presents a propensity-score-matched sample that reduces baseline differences between the groups and produces a July estimate similar to the main result.

[3]Marital status distinguishes mothers who are married with a spouse present from all others, including those separated, divorced, widowed, never married, or married with an absent spouse.

III. Empirical Strategy

I examine how mothers' labor supply responds to seasonal changes in childcare demand generated by summer school closures, comparing mothers with and without access to work from home (WFH). To quantify these effects, I estimate the following event-study specification:

$$Y_{imyo} = \sum_{m \neq \text{May}} \beta_m \left(everWFH_i \times \mathbf{1}[\text{month} = m] \right) + \gamma_i + \gamma_{ym} + \gamma_{om} + \varepsilon_{imyo} \tag{1}$$

Y_{imyo} denotes a labor market outcome for mother i in month m, year y, and baseline occupation o. The outcomes include employment, hours worked, and work absences. Mothers are classified as employed, unemployed, or out of the labor force using standard CPS definitions. The specification includes person fixed effects (γ_i), year-month fixed effects (γ_{ym}), and baseline occupation-by-month fixed effects (γ_{om}). Person fixed effects absorb time-invariant differences across mothers, while year-month fixed effects capture aggregate changes over time. Occupation-by-month fixed effects absorb occupation-specific seasonal patterns in labor supply. Standard errors are clustered at the baseline occupation level. Estimates are weighted using CPS survey weights.

The treatment variable, $everWFH_i$, equals one if a mother reports working from home at any point during her CPS panel. This measure captures WFH access as a characteristic of the mother's job rather than remote work in a particular reference week. Because *everWFH* is constant within mothers, its main effect is absorbed by person fixed effects. A concern is that mothers who leave employment in July have fewer opportunities to report working from home, potentially linking treatment status to the outcome. Section VI.D addresses this concern by defining WFH access using only the first observed month or the first observed non-summer month, with both measures producing similar estimates.

Two features of this measure matter for interpretation. First, it groups mothers with different amounts of remote work, so the estimates capture the effect of any WFH access relative to none rather than the effect of a particular arrangement. Second, the CPS records WFH only for the reference week, so a mother with a hybrid schedule who happened to work onsite in every observed

week is classified as onsite. This places some mothers with WFH access in the comparison group and biases the estimated effect toward zero.

The coefficients β_m measure the difference in labor market outcomes between mothers with and without WFH access in month m, relative to May. May is the reference month because it is the last month before the summer school closure period, when childcare constraints are relatively low. July is the month when school closures affect the largest share of mothers, so I focus on the July coefficient in the main results while using the full event-study estimates to examine the timing of the response.

The main assumption is that mothers with and without WFH access would have had similar seasonal patterns in labor supply in the absence of WFH access. The event-study estimates are consistent with this assumption, as coefficients outside the summer period are small and suggest similar seasonal patterns when schools are in session. The occupation-by-month fixed effects also account for seasonal patterns common to mothers within the same occupation. A remaining concern is that mothers in WFH-capable occupations may differ in ways that affect their response to summer childcare needs. For example, mothers who anticipate greater childcare needs may be more likely to choose jobs that offer WFH, in which case the July estimate could reflect differences in preferences for summer availability rather than the flexibility provided by WFH itself. Person fixed effects do not resolve this concern because *everWFH* is fixed within mothers. They remove persistent differences in labor supply between mothers with and without WFH access, but not differences in how the two groups change across months. The evidence against this explanation comes from the pattern of the results. The summer response is absent among women without children and fathers, shifts with the timing of state school calendars, and does not appear when the same design is applied to 2010–2019, when WFH was rare (Section VI.B). Together, these results make it difficult to attribute the estimated summer response to selection into WFH-capable jobs.

IV. Results

This section presents the main results. Mothers with WFH access experience smaller seasonal declines in employment during the summer, and the effect operates primarily by maintaining labor force attachment rather than by adjusting hours worked among those who remain employed.

IV.A Employment

Figure 2 presents event-study estimates of the effect of WFH access on employment across calendar months relative to May. Outside the summer months, the employment coefficients are small and statistically indistinguishable from zero, indicating that mothers with and without WFH access follow similar labor market trajectories when childcare demands are low. The employment gap opens in June, reaches its maximum in July, and closes by August as the school year resumes. Table 2 reports the July estimates for labor force status.

Relative to May, the July employment gap between mothers with and without WFH access widens by 2.6 percentage points, significant at the 1 percent level. The estimate does not measure the overall seasonal decline in maternal employment; it measures how much smaller that decline is for mothers with access to WFH. Figure 1 shows the underlying levels: between May and July, employment falls by 3.9 percentage points among mothers who work exclusively onsite and by 0.7 percentage points among mothers with WFH access.

Figure 3 reports the corresponding estimates for labor force non-participation and unemployment. The employment effect operates primarily through labor force participation. In July, mothers with WFH access are 1.6 percentage points less likely to be out of the labor force, significant at the 5 percent level, while the change in unemployment is smaller and not statistically distinguishable from zero. Together, these changes correspond to the 2.6 percentage point employment difference. Mothers without WFH access therefore appear to respond to the summer childcare shock primarily by leaving the labor force rather than by becoming unemployed. Section V.A examines this response in more detail.

IV.B Placebo Tests

Although the specification controls for occupation-specific seasonality, workers with and without access to WFH may differ systematically within occupations in ways unrelated to summer childcare demands. This section examines two placebo groups for whom the childcare mechanism should be absent. Women without children are not exposed to school closures at all, so any summer employment pattern among them would reflect seasonality in occupations rather than a response to childcare demands. Fathers of children in the same age range are equally exposed to school closures but typically bear a smaller share of childcare responsibilities. Any summer employment pattern among fathers would therefore suggest a broader household or seasonal disruption rather than a maternal response to school closures.

Figure 4 reports estimates for women aged 25 to 49 without children, using the same specification. The July coefficient is 0.88 percentage points, statistically indistinguishable from zero, and the estimates show no consistent seasonal pattern across June, July, and August. Whatever distinguishes jobs that permit remote work, that difference does not generate a summer employment advantage for women who are not caring for school-age children.[4]

Figure 5 repeats the exercise for fathers whose youngest child is aged 6 to 12. The July coefficient is -0.04 percentage points, statistically indistinguishable from zero. Fathers face the same school closures as mothers but show no similar summer employment response. This suggests that the effect for mothers is related to childcare rather than a general summer pattern among parents.

Taken together, the two placebo tests provide evidence against two alternative explanations. The null result for women without children argues against seasonality driven by occupations rather than childcare. If remote-capable occupations simply had different summer labor demand, or if workers in those occupations differed in unobserved ways that affected labor supply in July, a similar pattern should appear among childless women in the same occupations. It does not. The

[4]The two estimates can be combined in a triple-difference specification that stacks mothers whose youngest child is aged 6 to 12 and women without children, and interacts the event study with an indicator for motherhood, allowing each group to have its own person, time, and occupation-by-month fixed effects. The July triple interaction is 1.72 percentage points, indicating that the July WFH-related employment difference for mothers is 1.72 percentage points larger than the corresponding difference among women without children.

null result for fathers argues against household-level summer disruptions that are not specific to maternal caregiving, such as family travel or a spouse's schedule, since fathers face the same school closures within the same households. The remaining pattern is concentrated among mothers, the group that bears the larger share of the childcare demands created by summer school closures.

IV.C Heterogeneity

This section examines how the effect of access to WFH varies across households. The first set of comparisons uses marital status. The second uses age of the youngest child. The third examines whether the effect is smaller when the household has an alternative source of summer childcare, and the fourth uses the mother's education.

Household Structure

Panel A of Table 4 splits the sample by marital status.[5] The July coefficient is larger among married mothers, at 3.5 percentage points, compared with 0.7 percentage points for mothers with no spouse present.[6] The latter subsample is considerably smaller, and the difference between the two estimates is not statistically significant.

One interpretation is that married mothers have a second earner in the household, which makes reducing labor supply or leaving the labor force a feasible response when childcare demands rise. Without WFH access, some married women take that option; WFH access allows them to remain employed instead. Mothers without a spouse present face stronger financial pressure to keep working regardless of childcare constraints and are therefore less likely to leave employment in the first place. For this group, summer childcare demands may instead be absorbed through other margins, such as purchasing childcare, informal care or adjusting hours, leaving less scope for WFH access to affect employment.

[5]Following Price and Wasserman (2023), I distinguish mothers who are married with a spouse present in the household from those who are unmarried, separated, or married with an absent spouse.

[6]Appendix Figure A1 reports the corresponding event study. Appendix Figure A9 presents the corresponding estimates by marital status for fathers. Unlike mothers, fathers show no significant summer employment effect regardless of marital status.

Age of the Youngest Child

Panel B of Table 4 examines heterogeneity by the age of the child in one-child households where no sibling is available to provide informal care, and families do not face the higher childcare needs and costs associated with raising multiple children. For children aged 6 to 12 the July coefficient is 4.12 percentage points, larger than the main estimate and statistically significant at the 5 percent level despite the substantially reduced sample.[7] Restricting the sample to one-child households avoids ambiguity about school exposure from older or younger siblings. The July coefficient is 2.18 percentage points for children aged 0 to 5, and 1.07 percentage points for children aged 13 to 17.

The estimates are largest for children aged 6 to 12, the group for whom school provides a regular source of daytime supervision during the academic year, and for whom the loss of that supervision in summer may create a particularly large change in childcare needs. The smaller estimates for children younger than 6 are consistent with their greater need for childcare throughout the year. While the smaller estimates for children aged 13 to 17 are consistent with their requiring less direct supervision. Although the estimates are imprecise, the pattern is consistent with childcare needs being an important channel through which WFH affects summer employment.[8]

Substitutes for Maternal Childcare

If WFH access matters because it relaxes a childcare constraint, the effect should be larger when households have fewer alternative sources of summer childcare. Panel C of Table 4 reports three comparisons based on the availability of potential caregivers. The first row is the group with an alternative source of childcare available and the second is the group without, so the childcare interpretation predicts a larger effect in the second row.

The clearest difference is whether another adult besides the mother and her spouse is present in the household. Grandparents and other adult relatives may help with childcare during the summer, so WFH access may be less important when another adult is available. The July coefficient is 1.9

[7] Appendix Figure A2 reports the corresponding event study.

[8] Appendix Figure A3 reports the corresponding event study, and Appendix Figure A4 reports July coefficients for each single year of age.

percentage points when another adult is present and is not statistically significant, compared with 3.9 percentage points when no other adult is present, which is significant at the 1 percent level. Thus, the effect is less than half as large when another adult is available, consistent with the idea that WFH matters most when mothers have no one else to help with childcare.

The second asks whether older siblings substitute for maternal care. Teenagers may supervise younger siblings during school closures, reducing the value of the mother's own WFH access. To isolate this channel from other potential caregivers, the sample is restricted to two-parent households with no other adult present. Among households without an older sibling, the July coefficient is 4.4 percentage points, compared with 3.4 percentage points among households where an older sibling is present. The magnitudes are similar, although the estimate is statistically significant only for households without an older sibling.

The third question is whether a husband's ability to work from home allows childcare responsibilities to be shared. This comparison is restricted to mothers whose husbands are present and observed working at least once. The July coefficient is 3.7 percentage points when the husband works exclusively onsite and 2.6 percentage points when he also has WFH access. The 3.7 percentage point estimate is statistically significant at 5 percent level, while the 2.6 percentage point estimate is not. The magnitudes are therefore somewhat smaller when the husband also has WFH access, consistent with the possibility that husbands' WFH access helps share childcare responsibilities.

Across all three comparisons, the estimated effects are larger than the main estimate of 2.6 percentage points when alternative sources of childcare are absent. Although none of the differences is statistically significant and the smaller subsamples produce less precise estimates, the pattern is consistent. Appendix Figures A5, A6, and A7 show the corresponding event studies.

Education

Education provides a check on whether the employment response is specific to WFH access or reflects broader differences between more and less educated mothers. College-educated mothers

are far more likely to hold jobs that allow remote work, so if the summer response were driven by education rather than by WFH itself, it should appear only among college-educated mothers.

Panel D of Table 4 splits the sample by whether the mother holds a college degree, measured at her first interview.[9] College-educated mothers are more likely to hold jobs that allow WFH. The July coefficient is 3.5 percentage points among mothers with a college degree and 1.7 percentage points among those without. Although the estimate is larger for college-educated mothers, the two estimates are not statistically different. The positive estimates in both groups suggest that WFH access helps mothers maintain employment during the summer, regardless of education level.

IV.D Hours

Employment is only a partial measure of labor supply. Mothers may remain employed while working substantially fewer hours, so the extensive margin alone does not capture the full summer response. Table 5 reports estimates of weekly hours and full-time status.

I estimate weekly hours using Poisson pseudo-maximum likelihood (PPML), following Chen and Roth (2024). PPML is well suited to this setting because weekly hours include a substantial number of zero values. These zeros arise both from non-employment and, among employed mothers, from week-long absences. Among employed mothers, the share reporting zero hours rises from roughly 2 percent in May to 9 percent in July. These zeros are economically meaningful rather than a nuisance, since they capture an important part of the summer labor supply response. Dropping them would therefore discard part of the response of interest. PPML allows these observations to remain in the sample and yields coefficients that can be interpreted as proportional changes.

Including non-employed person-months as zeros in row (1), the July coefficient is 0.056, corresponding to a 5.8 percent increase in weekly hours and significant at the 5 percent level. The difference from the conditional estimate reflects the employment effect. Mothers with WFH access have more total weekly hours in July because more of them remain employed. Restricting the sample

[9] Appendix Figure A8 reports the corresponding event study.

to always-employed mothers, the July coefficient is 0.035, corresponding to an approximately 3.6 percent increase, but is not statistically distinguishable from zero. Appendix Figure A11 reports the corresponding event study.

Estimating the same specification in levels gives a July coefficient of 0.94 hours per week, also statistically indistinguishable from zero. Conditional on employment, mothers with WFH access do not work significantly more hours during the summer than mothers without it. Section V.B examines the week-long absences directly to understand why employed mothers report zero hours during the reference week.

Mothers may also remain employed while shifting from full-time to part-time work during the summer. Appendix Figure A12 reports event-study estimates for full-time employment. Restricting the sample to employed mothers, the July coefficient is 2.34 percentage points, statistically indistinguishable from zero. The estimates are imprecise, but I find no evidence that WFH access affects full-time employment during the summer.

The null result does not necessarily imply that WFH has no effect on hours. Mothers working from home may reduce hours while combining paid work with childcare. Mothers who remain employed onsite face pressures in both directions. They may reduce hours to manage childcare themselves, or work more to cover the cost of paid care. These opposing adjustments may offset one another, leaving no significant difference in observed hours conditional on employment.

Weekly earnings provide a natural extension of the hours analysis, but the CPS observes earnings only for mothers in the outgoing rotation groups. Because most mothers contribute only one earnings observation, person fixed effects cannot be estimated, and the specification cannot compare the same mother across months. Appendix A.I reports these estimates, which are negative and statistically indistinguishable from zero.

V. Mechanisms

Section IV shows that mothers with WFH access are more likely to remain employed during the summer and that the response operates primarily through labor force attachment. This section examines the mechanisms behind this pattern.

V.A Why Mothers Leave the Labor Force

Figure 1 shows weighted employment rates by calendar month for mothers with and without WFH access using data from the complete calendar years 2023 and 2024. Among mothers who work exclusively onsite, employment falls from 93.5 percent in May to 89.5 percent in July, a decline of 3.9 percentage points. Among mothers with WFH access, employment declines by only 0.7 percentage points, from 96.7 to 96.0 percent. The resulting raw difference-in-differences estimate is 3.3 percentage points.

Figure 6 decomposes the May-to-July change in non-employment by the reason respondents report. Among mothers who work exclusively onsite, labor force non-participation rises by 3.04 percentage points, with most of the increase coming from mothers reporting that they are "taking care of house or family," which accounts for 2.21 percentage points. Mothers with WFH access show little change on this margin, at 0.51 percentage points. The difference in exits for family care between the two groups is 1.70 percentage points. Other substantive reasons—schooling, disability, and retirement—show little movement in either group.

The unemployment margin provides a second, but smaller, channel. Among mothers without WFH access, unemployment rises by 0.89 percentage points between May and July, driven largely by temporary layoffs 0.60 percentage points, while unemployment does not change among mothers with WFH access (-0.05 percentage points).

Taken together, the decomposition shows that the summer employment gap is driven mainly by labor force exits, particularly exits for family care. The pattern is consistent with WFH access helping mothers remain employed when childcare demands increase during the summer.

V.B Vacation and Week-Long Absences

Section IV.D showed that the share of employed person-months reporting zero hours rises from roughly 2 percent in May to 9 percent in July. This section examines these absences directly. The CPS records whether respondents were away from work for the entire reference week, whether the absence was paid, and the reported reason. Among employed mothers, paid absences rise from 0.7 percent in May to 6.2 percent in July, while unpaid absences rise from 1.1 to 3.3 percent.

Table 6 reports July estimates of week-long absences, and Appendix Figure A13 reports the corresponding event studies for paid and unpaid absences. Mothers with WFH access are 3.3 percentage points less likely to report a paid absence in July, the difference is significant at the 5 percent level. The unpaid-absence estimate is small and statistically indistinguishable from zero (-0.44 percentage points).

Figure 7 shows that vacation accounts for nearly all of the paid-absence response. The July coefficient is -3.29 percentage points, significant at the 1 percent level. The effect is smaller in June and absent in August. The vacation and paid-absence estimates are nearly identical in the same sample (-3.29 and -3.33), indicating that the paid-absence result is driven by vacation. The pattern suggests that mothers without WFH access respond to school closures by taking leave, while mothers with WFH access are less likely to do so.

Fathers provide a comparison for this interpretation. They face the same school closures and may also value spending time with their children during the summer, but they typically bear a smaller share of childcare responsibilities. Appendix Figure A10 reports estimates from the same specification for fathers whose youngest child is aged 6 to 12. The July coefficient is -0.91 percentage points, statistically indistinguishable from zero. A preference for summer time off would likely affect both parents, whereas a childcare constraint should be stronger for mothers. Consistent with this interpretation, the vacation response is statistically significant for mothers but not for fathers.

The difference in vacation use does not necessarily mean that mothers with WFH access are better able to avoid taking leave. Mothers without WFH access may use vacation to manage

childcare when work cannot be performed remotely, but they may also value the uninterrupted time with their children.

One limitation is that the CPS records an absence only when a respondent is away for the entire reference week. Partial-week leave and informal schedule adjustments are unobserved, so these estimates likely understate the overall use of leave to manage summer childcare.

V.C Timing of School Closures

If the effect is driven by school closures rather than summer conditions more generally, it should follow the school calendar. I classify states by when the academic year typically ends, following Price and Wasserman (2023), and group states with mixed closure timing with the late-closing states.[10]

Figure 8 reports the estimates. In early-closing states, the effect appears in June (2.90 percentage points) and is smaller by July (1.72 percentage points). In late-closing states, the pattern is reversed: the June coefficient is indistinguishable from zero (1.04 percentage points), while July is 4.02 percentage points. The timing of the employment response shifts with the school calendar, consistent with the childcare mechanism and inconsistent with a general summer effect.

VI. Robustness

VI.A Propensity Score Matching

Mothers with WFH access differ from those without it: they are more educated, more likely to be married, and more concentrated in professional and managerial occupations. As a robustness check, I re-estimate the main specification on a propensity-score-matched sample.

I estimate the probability of ever working from home using a logit model based on characteristics measured at the respondent's first interview: education, marital status, number of children, age of

[10] Appendix Table A3 lists the states in each group.

the youngest child, race and ethnicity, state of residence, and entry month. I then perform one-to-one nearest-neighbor matching with exact matching on baseline occupation, so that each mother with WFH access is matched only to a mother in the same occupation.

Table A2 reports covariate balance before and after matching. Differences in marital status, race, and the age composition of children are statistically significant before matching but become statistically indistinguishable from zero after matching. The education gap remains statistically significant, but it declines from 30 to 4 percentage points. Figure 9 reports the event study for the matched sample. The July employment effect is 2.48 percentage points, close to the 2.60 percentage point estimate in the main sample. The coefficients are flat outside the summer months and peak in July. Appendix Figure A14 reports the corresponding estimates for weekly hours in the matched sample.

VI.B Pre-COVID Placebo

The CPS did not ask about remote work before October 2022, so WFH access cannot be measured directly in earlier years. If the summer employment effect reflects the ability to work remotely rather than characteristics of workers in remote-capable occupations, it should not appear in a period when remote work was not widely available.

I estimate a logit model of WFH access using the post-pandemic sample, based on baseline education, marital status, number of children, age of the youngest child, race and ethnicity, and occupation. I then apply the estimated model to mothers observed in the CPS from 2010 to 2019 and impose the same sample restrictions and baseline covariates. Mothers with and without predicted WFH access are matched one-to-one on the predicted probability, and I estimate the baseline specification on the matched sample.[11]

Figure 10 reports the event studies for the two periods side by side. The July coefficient is 0.48

[11]Mothers with predicted WFH access are those with a fitted probability of WFH access above one half. The pre-period sample contains 209,830 person-months. The specification is identical to the main model, including person, year-month, and occupation-by-month fixed effects, May as the reference month, and standard errors clustered at the baseline occupation level.

percentage points, statistically indistinguishable from zero, compared with 2.60 percentage points in the main sample. The absence of a similar summer employment effect before remote work was widely available supports the interpretation that the main result is related to the ability to work remotely.

VI.C Occupational Composition

Because WFH access is closely tied to occupation, a natural concern is that the pooled estimate may be driven by a small number of occupation groups. Figure 11 reports the July coefficient estimated separately for each broad occupation group, with circle size proportional to the number of person-months.

Most groups are close to the pooled estimate of 2.6 percentage points. Education is one of the largest groups and one of the few for which the confidence interval excludes zero. Food preparation and transportation have low rates of observed WFH access and wide confidence intervals.

Panel A of Table 7 reports estimates excluding specific groups. Teachers may have a mechanical summer employment pattern because of school calendar timing. Excluding teachers reduces the July effect to 2.04 percentage points, which remains statistically significant at the 5 percent level. Excluding manual and physical-presence occupations, which are generally less compatible with remote work, increases the estimate to 3.21 percentage points.[12] These exclusions produce estimates in the direction suggested by Figure 11, but the summer employment effect remains present.

VI.D Alternative Specifications and Inference

Panel B of Table 7 reports alternative specifications. Dropping person fixed effects gives a July effect of 3.31 percentage points. The estimate is similar when WFH access is defined using the first observed month (2.81 percentage points) or the first non-summer month (2.27 percentage points),

[12]Manual occupations are defined by `occ2010` major groups covering construction, production, transportation, food preparation, building and grounds maintenance, farming, protective service, and healthcare support. I use major groups rather than the **?** measure because the latter classifies several occupations that became commonly remote after 2020, such as lawyers, as non-teleworkable.

rather than ever reporting WFH during the panel.

The main specification compares July with May, so any May-specific shock enters every coefficient. Panel C re-centres the July effect on broader sets of control months using the same regression. Comparing July with the average of all non-summer months gives 2.58 percentage points. Restricting the comparison to February, October, and November gives 2.17 percentage points, similar to the main estimate and significant at the 5 percent level. The result therefore does not depend on using May as the reference month.

Panel D reports alternative standard error specifications. The July point estimate remains 2.60 percentage points in all estimates. Standard errors range from 0.87 to 0.96, and the July effect remains statistically significant under all alternatives. Thus, the inference is not sensitive to the choice of clustering.

VII. Conclusion

Summer school closures generate a large, predictable, and recurring increase in childcare demand that falls disproportionately on mothers. This paper asks whether access to work from home helps mothers remain employed when that demand rises. Using monthly CPS data from October 2022 through September 2025 and a difference-in-differences design that compares mothers within occupations across calendar months, I find that mothers with WFH access are 2.6 percentage points more likely to be employed in July than mothers who work exclusively onsite. The gap is absent outside the summer, opens in June, peaks in July, and closes by August.

These estimates can be compared with the seasonal decline documented in the prior literature. Price and Wasserman (2023) find that employment among mothers whose youngest child is aged 6 to 12 falls by 2.3 percentage points between May and July, using CPS data from a period before remote work was widely available. Their estimate shows the size of the typical summer employment decline for mothers with children in this age range. Our estimates build on this result by examining whether the decline differs depending on mothers' access to flexible work arrangements. The results

suggest that WFH access is an important source of this variation. The decline is concentrated among mothers who work exclusively onsite and is close to zero among those with WFH access, suggesting that WFH substantially reduces summer employment losses. Because the estimates come from different periods and samples, this comparison is illustrative rather than a precise decomposition of the overall decline.

The effect operates mainly through mothers remaining attached to employment rather than working more hours once employed. Mothers with WFH access are 1.6 percentage points less likely to be out of the labor force in July, while the change in unemployment is smaller and not statistically distinguishable from zero. Conditional on employment, mothers with and without WFH access work similar hours and show no statistically significant difference in full-time employment. Thus, WFH access helps mothers respond to the temporary increase in childcare demand primarily by allowing them to remain employed, rather than by increasing the hours worked by those who remain employed.

Several findings point to childcare as the mechanism. Mothers without WFH access are more likely to leave the labor force in July and primarily report family care as the reason. They are also more likely to take week-long vacation leave, while fathers with children in the same age range show no similar response. Moreover, the employment effect follows the school calendar. It appears earlier in states where schools close earlier and later where schools close later. The effect is also larger when households have fewer potential sources of alternative summer childcare. Finally, among one-child households, the point estimate is largest for mothers whose only child is aged 6 to 12, the age group for whom the loss of school-based daytime supervision is most directly relevant. Taken together, these patterns are consistent with summer childcare demands playing an important role in the employment response.

The results are also robust across several samples and specifications. The effect remains when teachers and manual and physical-presence occupations are excluded. The estimate is also similar under alternative definitions of WFH access, alternative reference months, and alternative standard error specifications. A propensity-score-matched sample produces an estimate close to the main

result, while a pre-COVID placebo shows no comparable summer employment effect in 2010–2019, before remote work was widely available.

Two limitations remain. First, the CPS records an absence only when a respondent is away from work for the entire reference week, so partial-week leave and informal schedule adjustments are unobserved. The data therefore cannot show how mothers with WFH access spend the time they do not take as formal leave. Second, WFH access is not randomly assigned. The design compares mothers within occupations across months, and the results are robust to several alternative specifications and samples. However, the key assumption remains that without WFH access, seasonal employment patterns would have been similar across the two groups.

The findings also have an important distributional dimension. The employment gains from WFH are concentrated among married and college-educated mothers and among mothers in occupations that can be performed remotely. Because these mothers are already more likely to have access to flexible work arrangements, the ability of WFH to reduce seasonal employment losses may not be equally available to all mothers.

More broadly, the findings suggest that WFH can help mothers remain attached to the labor force when childcare constraints temporarily become more binding. If maintaining employment through these periods reduces interruptions in work experience, WFH could have longer-run implications for gender gaps in employment and career outcomes. The findings show that flexible work arrangements can help mothers navigate periods of increased childcare demand without leaving employment.

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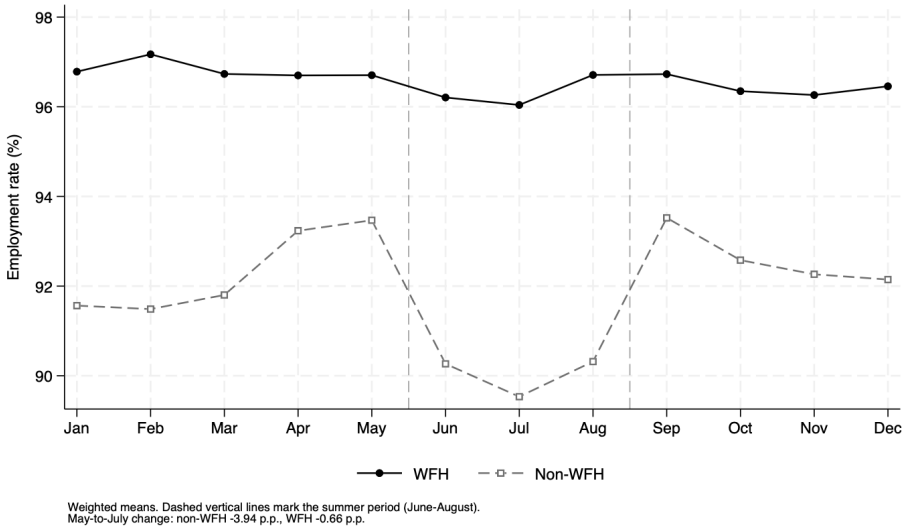
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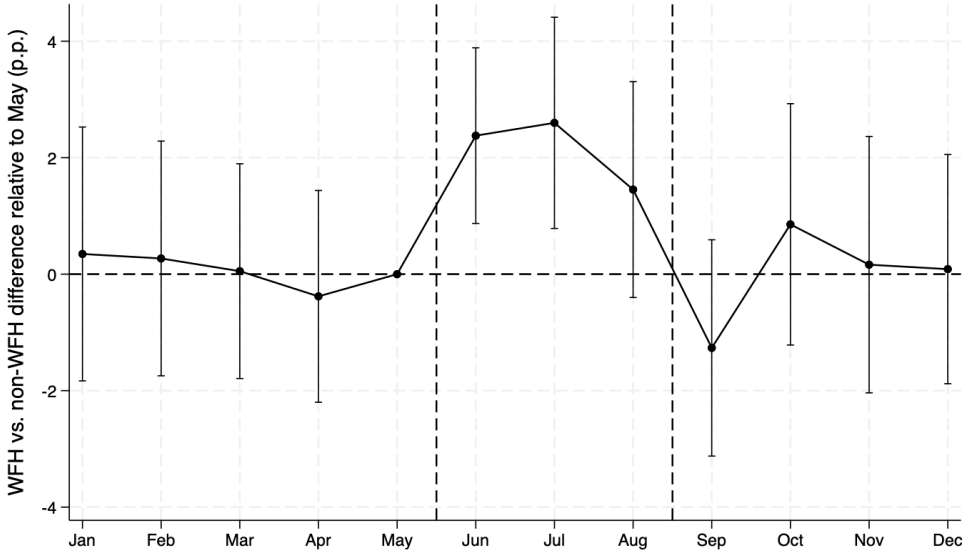
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Figure 1: Raw Employment Rate by Calendar Month



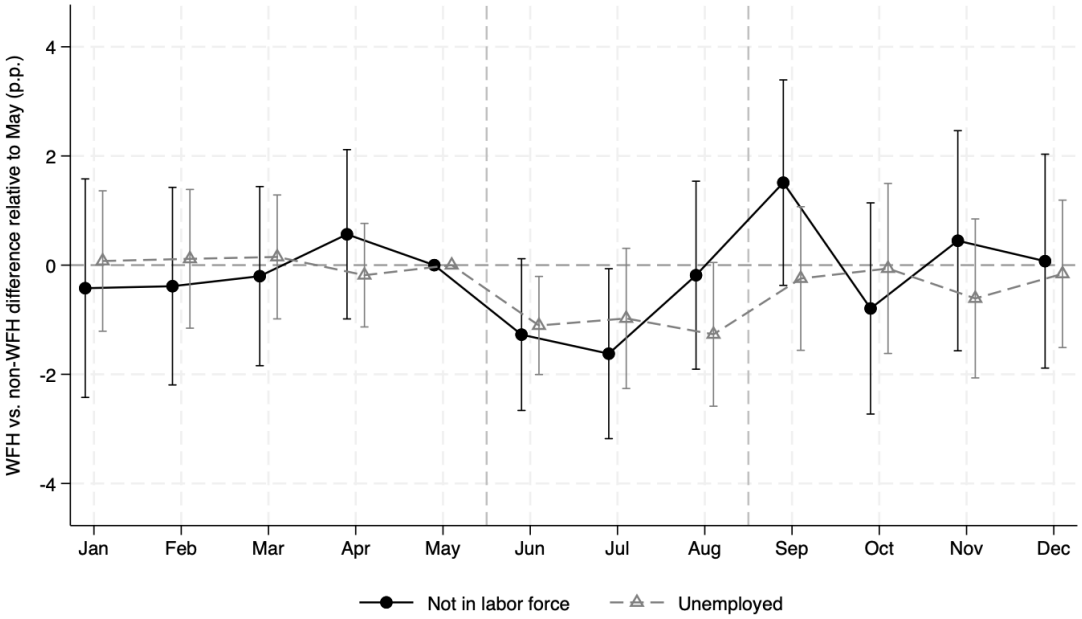
Notes: Weighted mean employment rates by calendar month for mothers with and without WFH access, with no controls or fixed effects. The sample is restricted to the complete calendar years 2023 and 2024 so that each month averages over the same years. Dashed vertical lines denote the summer period (June–August). Between May and July, employment falls by 3.9 percentage points among mothers who work exclusively onsite, compared with 0.7 percentage points among mothers with WFH access.

Figure 2: Effect of WFH Access on Employment



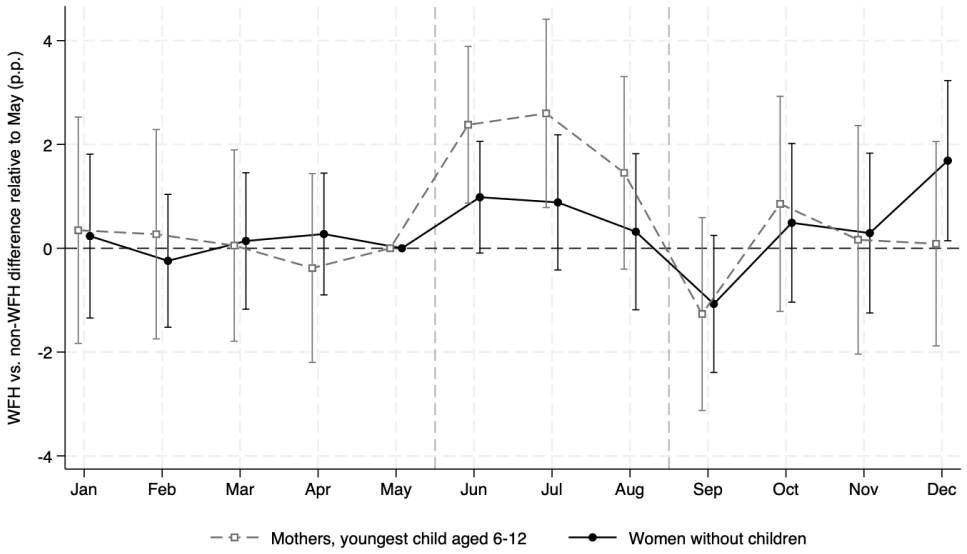
Notes: Event-study estimates of the effect of WFH access on employment, in percentage points relative to May. The sample consists of mothers aged 25–49 whose youngest child is aged 6–12, using CPS data from October 2022 through September 2025. WFH access is defined as having worked from home at any point during the panel. The specification includes person, year-month, and occupation-by-month fixed effects and uses CPS survey weights, with standard errors clustered at the baseline occupation level. Dashed vertical lines mark the summer period (June–August). The July coefficient is 2.60 percentage points. Bars represent 95 percent confidence intervals.

Figure 3: Effect of WFH Access on Non-Participation and Unemployment



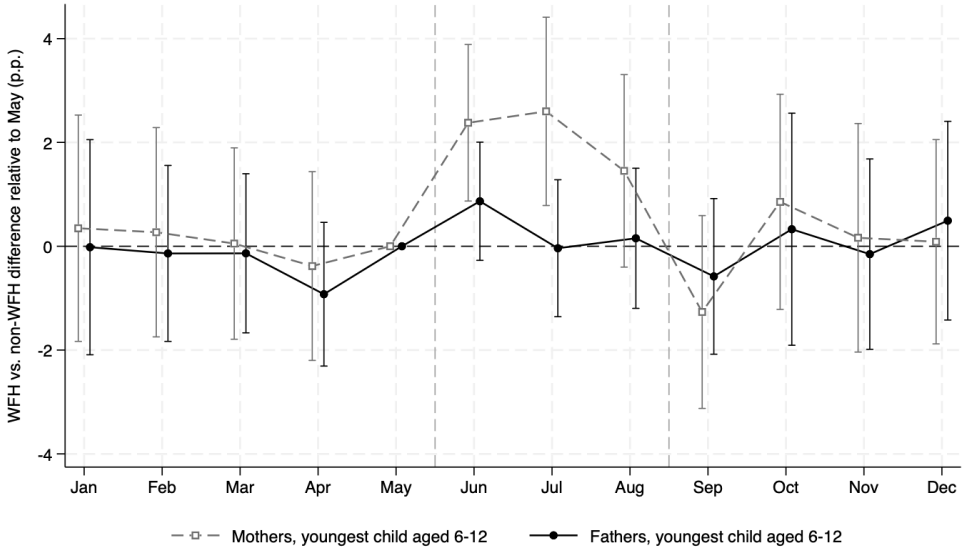
Notes: Event-study estimates of the effect of WFH access on labor force non-participation and unemployment, in percentage points relative to May. Sample, specification, and vertical scale follow Figure 2. Estimates are offset horizontally so the confidence intervals do not overlap. The July coefficients are −1.60 percentage points for non-participation and −0.98 percentage points for unemployment. Bars represent 95 percent confidence intervals.

Figure 4: Placebo: Effect of WFH Access for Women Without Children



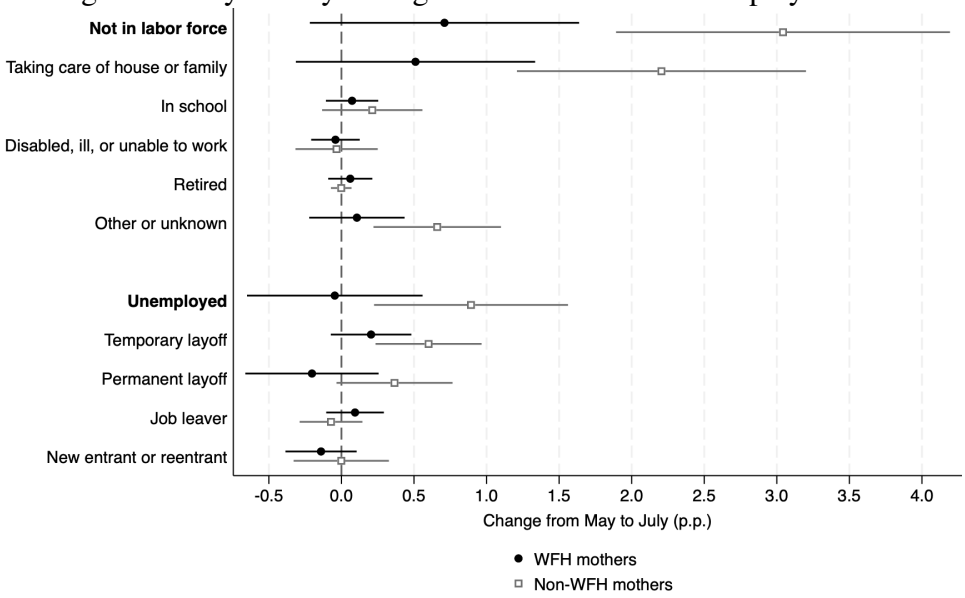
Notes: Event-study estimates of the effect of WFH access on employment for women aged 25–49 without children, shown alongside the main sample of mothers whose youngest child is aged 6–12. Women without children are not exposed to school closures, so any summer pattern among them would reflect occupational seasonality rather than childcare demand. Sample construction and specification follow the baseline model. Bars represent 95 percent confidence intervals.

Figure 5: Placebo: Effect of WFH Access for Fathers



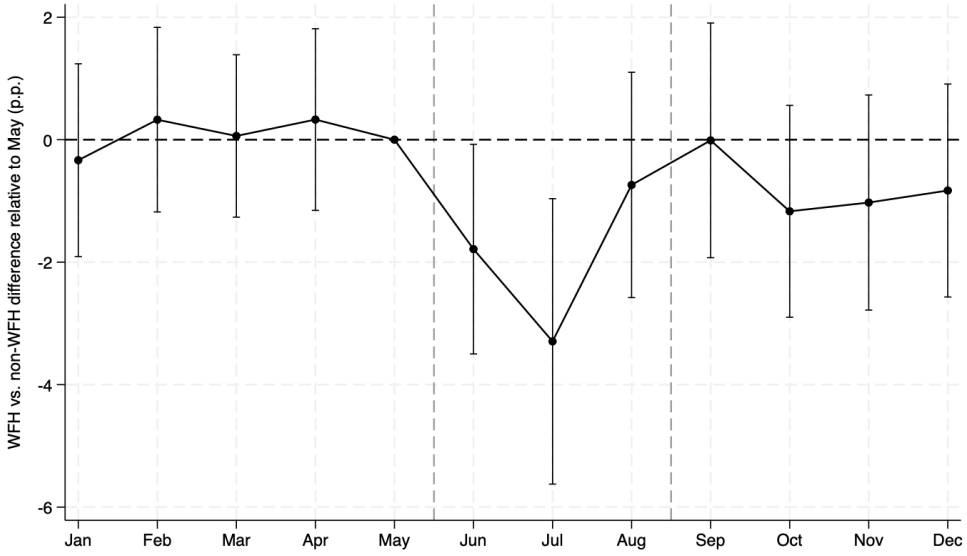
Notes: Event-study estimates of the effect of WFH access on employment for fathers aged 25–49 whose youngest child is aged 6–12, shown alongside the corresponding estimates for mothers. Fathers face the same school closures but typically bear a smaller share of childcare responsibilities. Sample construction and specification are identical across the two groups and follow the baseline model. Bars represent 95 percent confidence intervals.

Figure 6: May-to-July Change in Reasons for Non-Employment



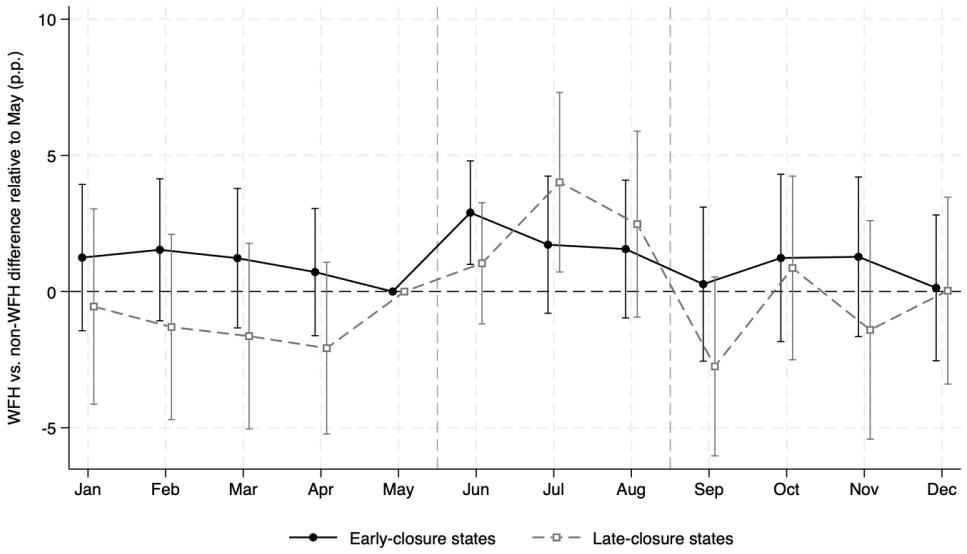
Notes: Change from May to July in the share of person-months in each labor force category, estimated separately for mothers with and without WFH access. Each estimate is the July coefficient from a regression of the category indicator on calendar month dummies with May omitted, weighted by CPS survey weights, with standard errors clustered at the person level. Categories within each block are mutually exclusive and sum to the block total. Bars are 95 percent confidence intervals.

Figure 7: Effect of WFH Access on Vacation Absences



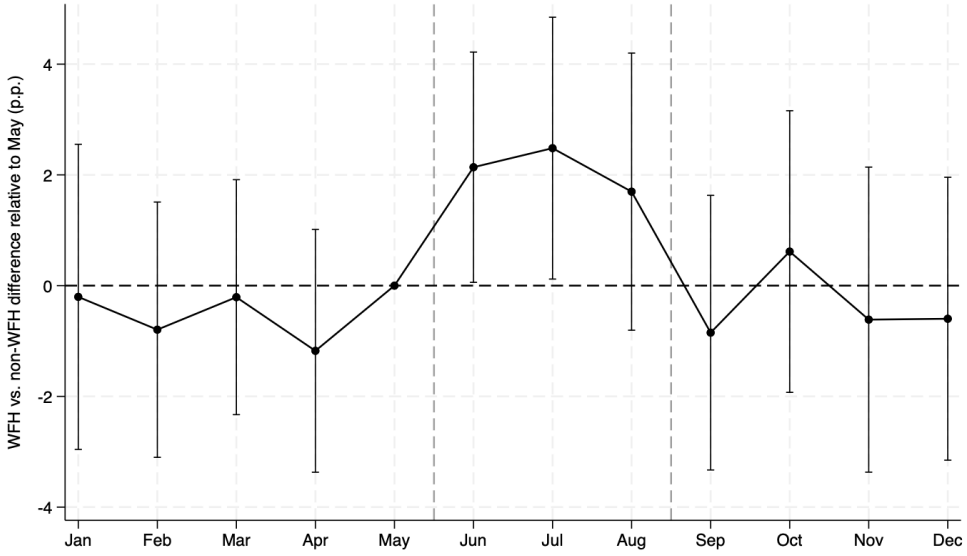
Notes: Event-study estimates of the effect of WFH access on the probability of being absent from work for the entire reference week due to vacation or personal days, in percentage points relative to May. The sample is restricted to employed person-months. The specification follows the baseline model and uses CPS survey weights. Standard errors are clustered at the baseline occupation level. Appendix Figure A10 reports the corresponding estimates for fathers. Bars represent 95 percent confidence intervals.

Figure 8: Effect of WFH Access by Timing of School Closures



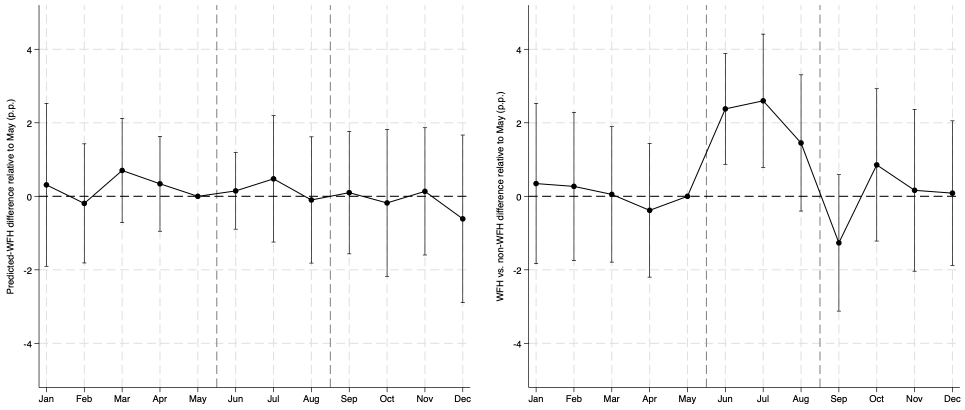
Notes: Event-study estimates of the effect of WFH access on employment, estimated separately for states where the academic year typically ends early and states where it ends late. States with mixed closure timing are grouped with the late-closing states; Appendix Table A3 lists the classification. The sample and specification otherwise follow the baseline model and use CPS survey weights. Bars represent 95 percent confidence intervals.

Figure 9: Effect of WFH Access on Employment: Matched Sample



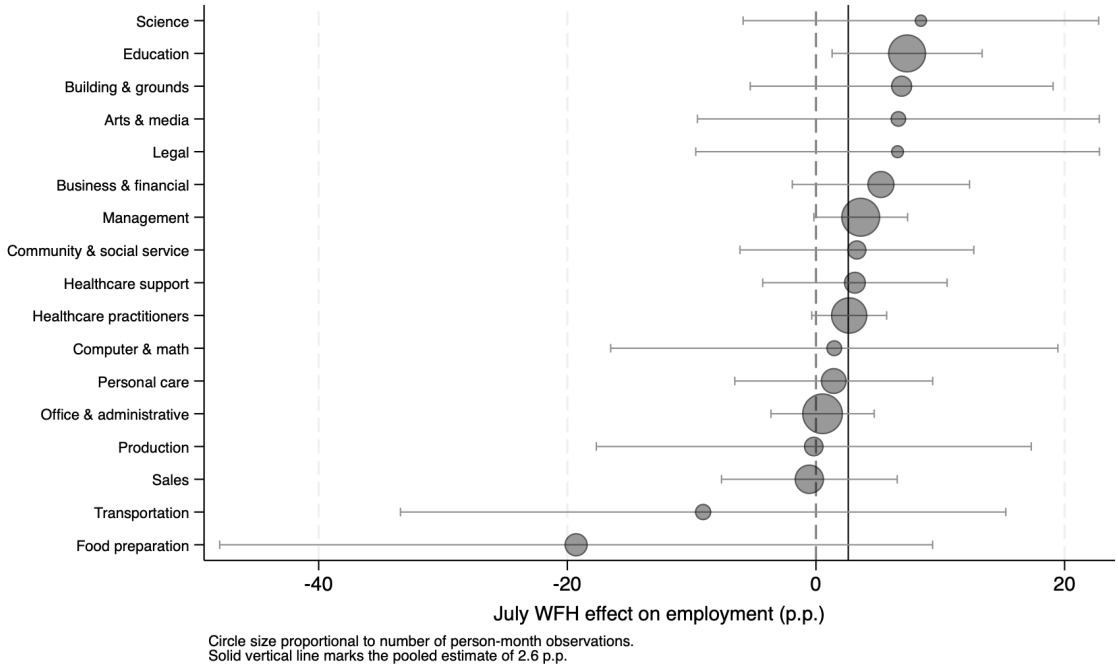
Notes: Event-study estimates of the effect of WFH access on employment, estimated on the propensity-score-matched sample described in Section VI.A. The July coefficient is 2.48 percentage points. Covariate balance before and after matching is reported in Appendix Table A2. The specification otherwise follows the baseline model and uses CPS survey weights. Bars represent 95 percent confidence intervals.

Figure 10: Placebo: Predicted WFH Access Before and After the Pandemic



Notes: Event-study estimates of the employment gap by WFH access, relative to May, estimated separately for the pre-pandemic period (left panel) and the main sample period (right panel). In the pre-pandemic sample, WFH access is assigned using predicted probabilities from a logit model estimated on the main sample, based on baseline education, marital status, number of children, age of the youngest child, race and ethnicity, and occupation; mothers with a fitted probability above one half are assigned to the WFH group. The July coefficient is 0.48 percentage points in the pre-pandemic sample. Both panels use the baseline specification and CPS survey weights. Bars represent 95 percent confidence intervals.

Figure 11: July Employment Effect by Occupation Group



Notes: July coefficients estimated separately by broad occupation group, in percentage points relative to May. Circle size is proportional to the number of person-months in each group. The solid vertical line marks the pooled estimate of 2.6 percentage points. The specification follows the baseline model and uses CPS survey weights. Bars represent 95 percent confidence intervals.

Table 1: Summary Statistics by Work-from-Home Status

	All women		Mothers (6–12)	
	WFH	Onsite	WFH	Onsite
Demographics				
Age	37.22	36.60	40.72	38.89
Married, spouse present	0.59	0.49	0.75	0.62
Number of own children	0.95	0.99	1.93	1.94
College degree or more	0.71	0.39	0.66	0.36
Labor market status				
Employed	0.96	0.91	0.97	0.92
At work	0.93	0.88	0.94	0.88
Absent	0.03	0.04	0.03	0.04
Unemployed	0.01	0.02	0.01	0.02
Not in labor force	0.03	0.06	0.03	0.06
Work intensity				
Hours worked	36.05	32.60	36.06	32.14
Full-time (35+ hours)	0.78	0.72	0.76	0.69
Person-months	161,926	278,930	32,038	59,958

Notes: CPS Basic Monthly Survey, October 2022–September 2025. Cell entries are weighted means. WFH denotes women who report working from home at any point during their CPS panel; onsite denotes those who never do. Columns (3)–(4) are the estimation sample of mothers aged 25–49 whose youngest child is aged 6–12; columns (1)–(2) cover all women aged 25–49 with at least one observed employment status. Employment status and hours refer to the CPS reference week. The regression sample excludes singleton observations, leaving 88,661 person-months. Education is measured at the first interview.

Table 2: July Effects on Labor Force Status

Outcome	July effect	SE	*N*
Employed	2.60^{***}	(0.93)	88,661
Unemployed	−0.98	(0.66)	
Not in labor force	-1.60^{**}	(0.79)	

Notes: Each row reports the coefficient on the *everWFH* × July interaction, relative to May, in percentage points, from a separate estimation of the baseline specification on the main sample of mothers aged 25–49 whose youngest child is aged 6–12. The three categories are mutually exclusive and exhaust the CPS labor force classification. All specifications include person, year-month, and occupation-by-month fixed effects and use CPS survey weights, with standard errors clustered on baseline occupation in parentheses. Corresponding event studies appear in Figures 2 and 3. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 3: July Employment Effect: Placebo Groups

Sample	July effect	SE	*N*
Mothers, youngest child 6–12	2.60***	(0.93)	88,661
Women without children	0.88	(0.66)	197,398
Fathers, youngest child 6–12	−0.04	(0.67)	74,638

Notes: Each row reports the coefficient on the *everWFH* × July interaction for employment, relative to May, in percentage points, from a separate estimation of the baseline specification on the indicated sample. The first row repeats the main estimate from Table 2 for reference. Women without children are not exposed to school closures; fathers face the same closures but generally bear less of the resulting childcare. Sample construction and specification are identical across the three samples. All specifications include person, year-month, and occupation-by-month fixed effects and use CPS survey weights, with standard errors clustered on baseline occupation in parentheses. Corresponding event studies appear in Figures 4 and 5. $^{***}\,p < 0.01$, $^{**}\,p < 0.05$, $^{*}\,p < 0.10$.

Table 4: July Employment Effect by Household Characteristics

Subsample	July effect	SE	N
Main sample	2.60***	(0.93)	88,661
Panel A. Household structure			
Married, spouse present	3.52***	(1.09)	60,036
No spouse present	0.71	(1.94)	26,523
Panel B. Age of the child, one-child households			
Aged 0–5	2.18	(1.65)	31,290
Aged 6–12	4.12**	(2.02)	26,865
Aged 13–17	1.07	(2.35)	26,846
Panel C. Substitutes for maternal childcare			
Other adult present[a]	1.90	(2.59)	11,734
No other adult present[a]	3.92***	(1.24)	54,752
Older sibling present[b]	3.39	(2.43)	19,842
No sibling aged 13–17[b]	4.37***	(1.36)	34,156
Husband has WFH access[c]	2.64	(2.59)	21,685
Husband onsite[c]	3.75**	(1.69)	41,620
Panel D. Mother's education			
College degree or more	3.50**	(1.45)	46,706
Less than college	1.70	(1.59)	41,336

Notes: Each row reports the coefficient on the interaction between *everWFH* and July, relative to May, in percentage points, from a separate estimation of the baseline specification on the indicated subsample. All specifications include person, year-month, and occupation-by-month fixed effects and use CPS survey weights, with standard errors clustered on baseline occupation in parentheses. The aged 6–12 row of Panel B is the one-child household estimate discussed in Section IV.C. Within Panel C, each pair of rows is a separate comparison. The first row of each pair is the group with the indicated source of alternative childcare available and the second is the group without, so the childcare interpretation predicts a larger effect in the second row. Sample restrictions differ across pairs: [a] married-couple households; [b] married-couple households with no other adult present; [c] married-couple households in which the husband is observed employed at least once. Differences between paired rows are not statistically significant. Education is measured at the first interview. The corresponding event studies appear in Appendix Figures A1–A8. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 5: July Effect of WFH Access on Weekly Hours and Full-Time Status

	July effect	SE	*N*
Panel A. Weekly hours			
All person-months, PPML	0.056**	(0.027)	88,601
Employed only, PPML	0.035	(0.024)	82,235
Employed only, hours	0.935	(0.774)	82,240
Panel B. Full-time status, percentage points			
Employed only	2.34	(2.37)	82,240

Notes: Each row reports the coefficient on the *everWFH* × July interaction, relative to May, from a separate estimation. Units differ across rows and the coefficients are not directly comparable. The two PPML coefficients are proportional changes: 0.056 corresponds to 5.8 percent and 0.035 to 3.6 percent. The levels row reports hours per week and the full-time row reports percentage points. The PPML row labeled all person-months codes non-employed months as zero hours; every other row restricts to employed person-months. Full-time is defined as usual weekly hours of 35 or more. All specifications include person, year-month, and occupation-by-month fixed effects and use CPS survey weights, with standard errors clustered on baseline occupation in parentheses. Corresponding event studies appear in Appendix Figures A11 and A12. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 6: July Effect of WFH Access on Week-Long Absences

Outcome	July effect	SE	N
Panel A. Mothers, youngest child 6–12			
Paid absence	−3.33**	(1.41)	82,240
Unpaid absence	−0.44	(0.87)	82,240
Vacation absence	−3.29***	(1.19)	82,240
Panel B. Fathers, youngest child 6–12			
Vacation absence	−0.91	(1.21)	72,024

Notes: Each row reports the coefficient on the *everWFH* × July interaction, relative to May, in percentage points from a separate estimation of the baseline specification. Absences are defined only for employed respondents absent for the entire reference week; thus, all columns use employed person-months. Vacation absences are a subset of paid absences. All specifications include person, year-month, and occupation-by-month fixed effects, with standard errors clustered at baseline occupation in parentheses. Panels A and B use identical samples and specifications. Corresponding event studies appear in Figure 7 and Appendix Figures A13 and A10. ***, $p < 0.01$, **, $p < 0.05$, *, $p < 0.10$.

Table 7: July Employment Effect: Robustness

	July effect	SE	*N*
Main specification	2.60***	(0.93)	88,661
Panel A. Alternative samples			
Excluding teachers	2.04**	(0.90)	77,008
Excluding manual occupations	3.21***	(0.91)	72,428
Panel B. Alternative specifications			
No person FE	3.31***	(1.16)	88,661
WFH measured at first month	2.81***	(0.90)	88,661
WFH measured at first non-summer month	2.27**	(0.89)	88,661
Panel C. Alternative reference periods			
All non-summer months	2.58***	(0.88)	88,661
Months with no scheduled break	2.17**	(1.01)	88,661
Panel D. Alternative clustering			
State	2.60***	(0.87)	88,661
Occupation and state	2.60***	(0.87)	88,661
Person	2.60***	(0.96)	88,661

Notes: Coefficients in percentage points, relative to May. Except where noted, all rows use the baseline specification with person, year-month, and occupation-by-month fixed effects and standard errors clustered on baseline occupation. Manual occupations are `occ2010` major groups requiring physical presence: construction, production, transportation, food preparation, building and grounds maintenance, farming, protective service, and healthcare support. Panel C rows are linear combinations of the estimated month coefficients from the main regression: all non-summer months averages over January–May and September–December, and months with no scheduled break averages over February, October, and November. Panel D rows estimate the same regression on the same sample as the main specification and differ only in the variance estimator, so the coefficient is 2.60 percentage points throughout. Estimates by occupation group are reported in Figure 11, and the corresponding event study for the teacher exclusion appears in Appendix Figure A15. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Appendix

A.I. Earnings

The CPS reports earnings only for respondents in the outgoing rotation groups, so earnings are observed for a smaller sample of 18,667 person-months. Because most mothers contribute only one earnings observation, person fixed effects cannot be estimated. The specifications in this section therefore include time and occupation-by-month fixed effects but cannot compare the same mother across months.

Table A1 reports the estimates. The July coefficient for log weekly earnings is −0.115, corresponding to an approximately 11 percent decline. A Poisson specification that retains zero earnings gives a similar coefficient of −0.107, corresponding to an approximately 10 percent decline. Neither estimate is statistically significant at the 5 percent level.

These estimates should be interpreted with caution. Section IV.A shows that WFH access keeps some mothers employed during the summer who would otherwise leave employment. As a result, differences in earnings may reflect changes in the composition of employed mothers rather than changes in pay for the same worker. Because the earnings sample does not permit person fixed effects, I do not interpret these estimates as evidence of an effect on pay.

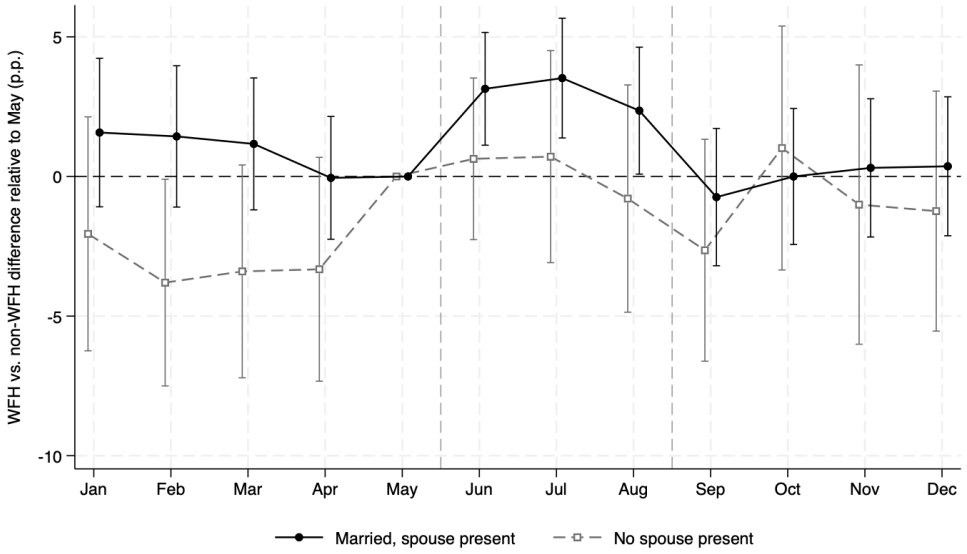
Table A1: July Effect of WFH Access on Weekly Earnings

Specification	July effect	SE	*N*
Log weekly earnings	−0.115*	(0.067)	18,667
Weekly earnings, PPML	−0.107	(0.107)	18,951

Notes: Each row reports the coefficient on the *everWFH* × July interaction, relative to May, from a separate estimation. Both coefficients are proportional changes: −0.115 corresponds to a decline of about 10.9 percent and −0.107 to about 10.1 percent. The log specification drops observations with zero earnings; the PPML specification retains them. Earnings are observed only in the outgoing rotation groups, so the sample is much smaller than the main estimation sample and most mothers contribute a single earnings observation. Person fixed effects are therefore infeasible: both specifications include year-month and occupation-by-month fixed effects only, and do not compare the same mother across months. Standard errors are clustered on baseline occupation in parentheses. $^{***}\,p < 0.01$, $^{**}\,p < 0.05$, $^{*}\,p < 0.10$.

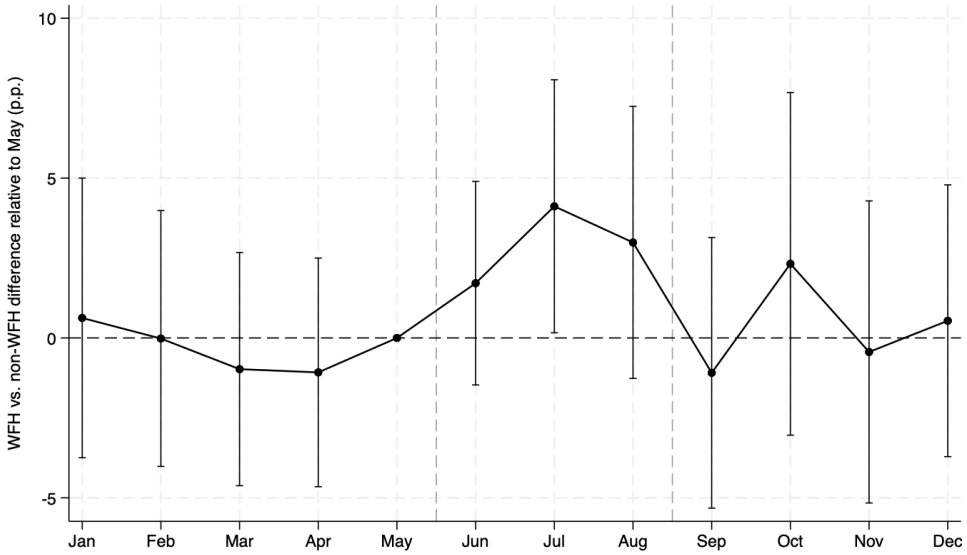
A.II. Heterogeneity

Figure A1: Heterogeneity: Effect of WFH Access by Marital Status



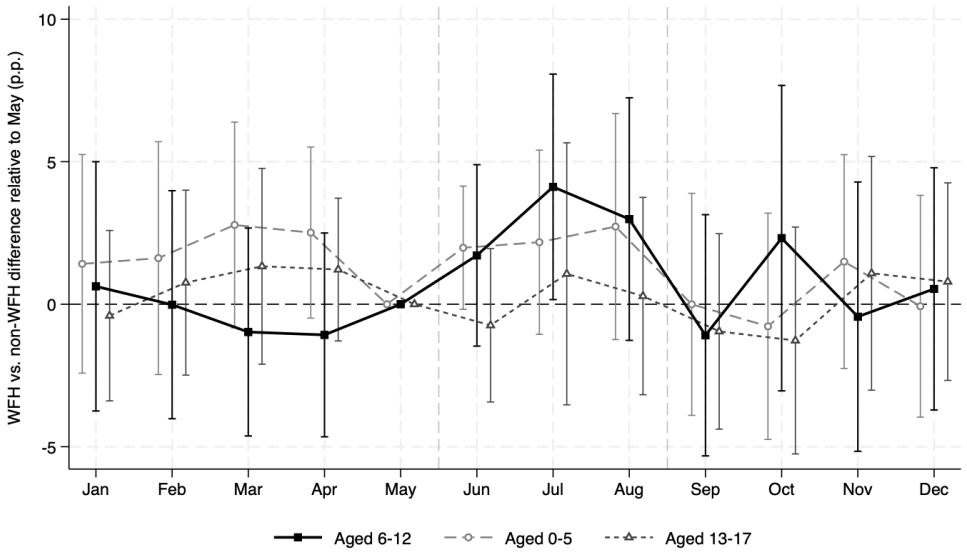
Notes: Event-study estimates of the effect of WFH access on employment, estimated separately for mothers who are married with a spouse present and for mothers with no spouse present. The July coefficients are reported in Panel A of Table 4. Sample and specification follow the baseline model. Bars are 95 percent confidence intervals.

Figure A2: Heterogeneity: One-Child Households



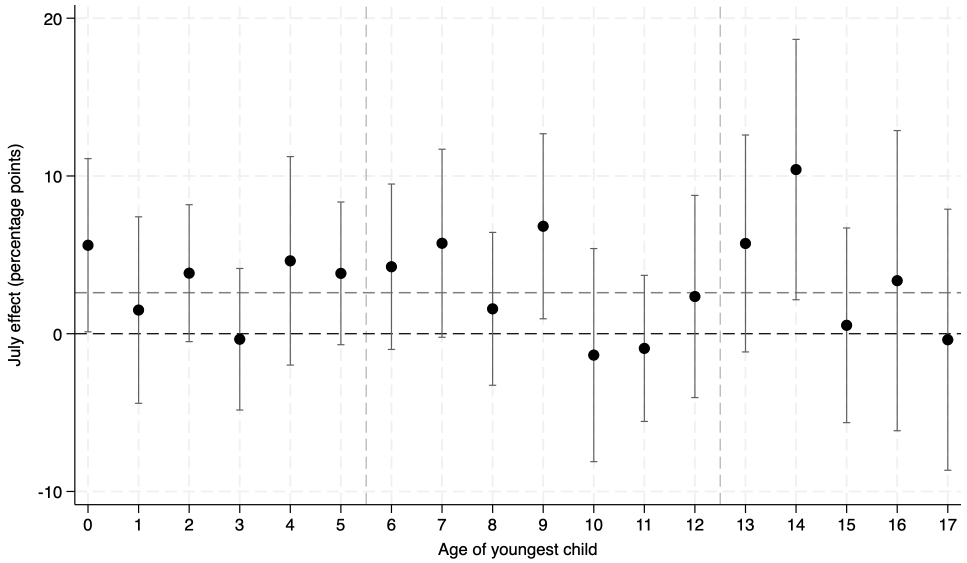
Notes: Event-study estimates of the effect of WFH access on employment, restricted to households with a single child. No sibling of any age is present, and childcare demand does not vary with family size. The July coefficient is reported in Panel B of Table 4. Sample and specification otherwise follow the baseline model. Bars are 95 percent confidence intervals.

Figure A3: Heterogeneity: Effect of WFH Access by Age of Youngest Child



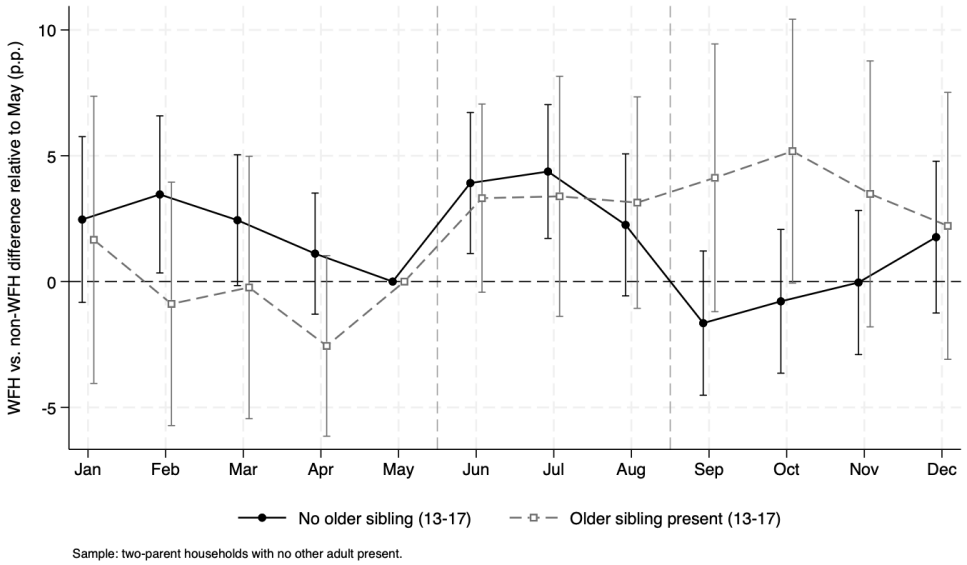
Notes: Event-study estimates of the effect of WFH access on employment, estimated separately by the age of the youngest child in one-child households. The July coefficients are reported in Panel B of Table 4. Sample and specification otherwise follow the baseline model. Bars are 95 percent confidence intervals.

Figure A4: July Employment Effect by Single Year of Age of the Youngest Child



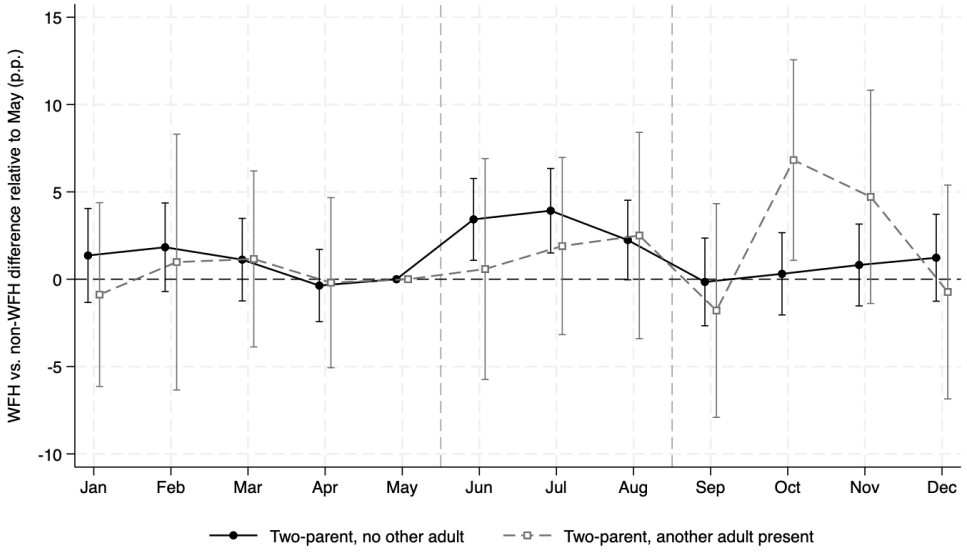
Notes: July coefficients estimated separately for each single year of age of the youngest child, in percentage points relative to May. Each estimate comes from a separate regression on the corresponding subsample, so the samples are considerably smaller than the main sample and the estimates are correspondingly imprecise. The horizontal dashed line marks the main-sample estimate of 2.60 percentage points, and the vertical dashed lines bracket the 6–12 age range used in the main analysis. Specification follows the baseline model. Bars are 95 percent confidence intervals.

Figure A5: Heterogeneity: Presence of an Older Sibling



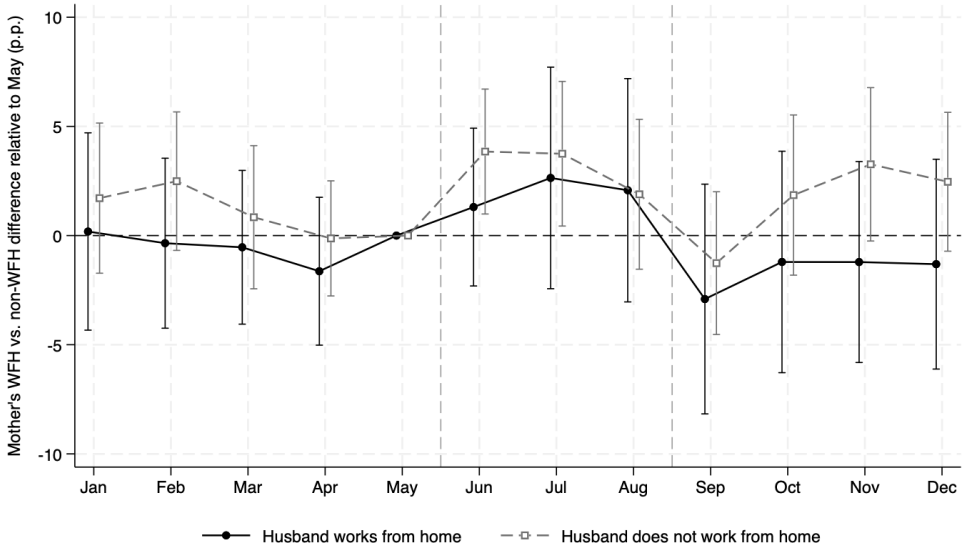
Notes: Event-study estimates of the effect of WFH access on employment, estimated separately for households with and without a sibling aged 13–17. The sample is restricted to married-couple households with no other adult present. The July coefficients are reported in Panel C of Table 4. Specification otherwise follows the baseline model. Bars are 95 percent confidence intervals.

Figure A6: Heterogeneity: Presence of Another Adult in the Household



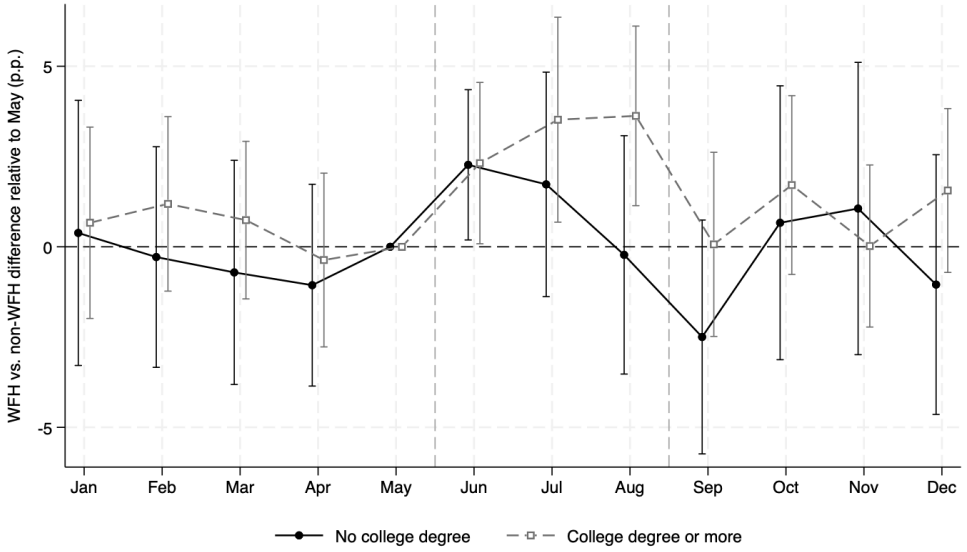
Notes: Event-study estimates of the effect of WFH access on employment, estimated separately by whether an adult other than the mother and her spouse is present in the household. The sample is restricted to married-couple households. The July coefficients are reported in Panel C of Table 4. Specification otherwise follows the baseline model. Bars are 95 percent confidence intervals.

Figure A7: Heterogeneity: Husband's WFH Status



Notes: Event-study estimates of the effect of WFH access on employment, estimated separately by whether the husband works exclusively onsite or also has WFH access. The sample is restricted to married-couple households in which the husband is observed employed at least once. The July coefficients are reported in Panel C of Table 4. Specification otherwise follows the baseline model. Bars are 95 percent confidence intervals.

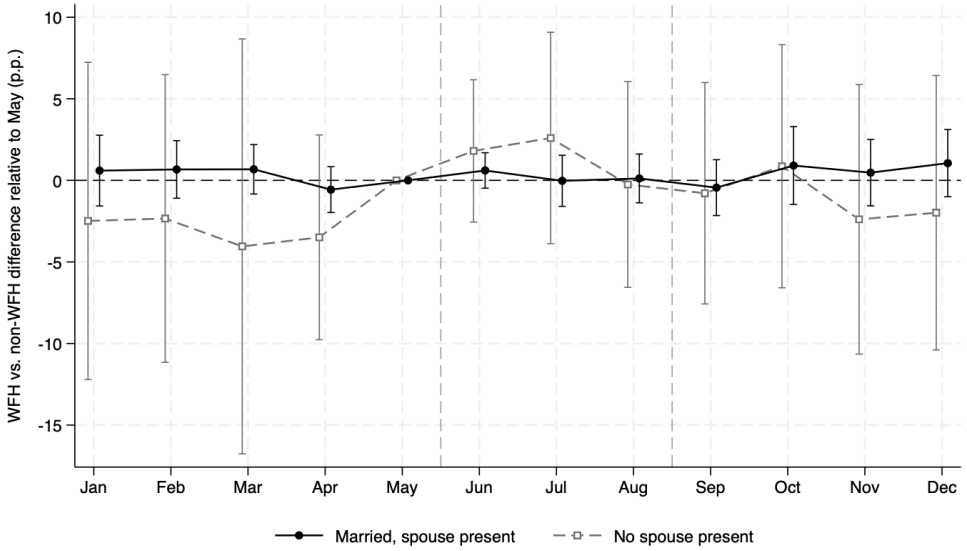
Figure A8: Heterogeneity: Effect of WFH Access by Mother's Education



Notes: Event-study estimates of the effect of WFH access on employment, estimated separately for mothers with and without a college degree, measured at the first interview. The July coefficients are reported in Panel D of Table 4. Specification otherwise follows the baseline model. Bars are 95 percent confidence intervals.

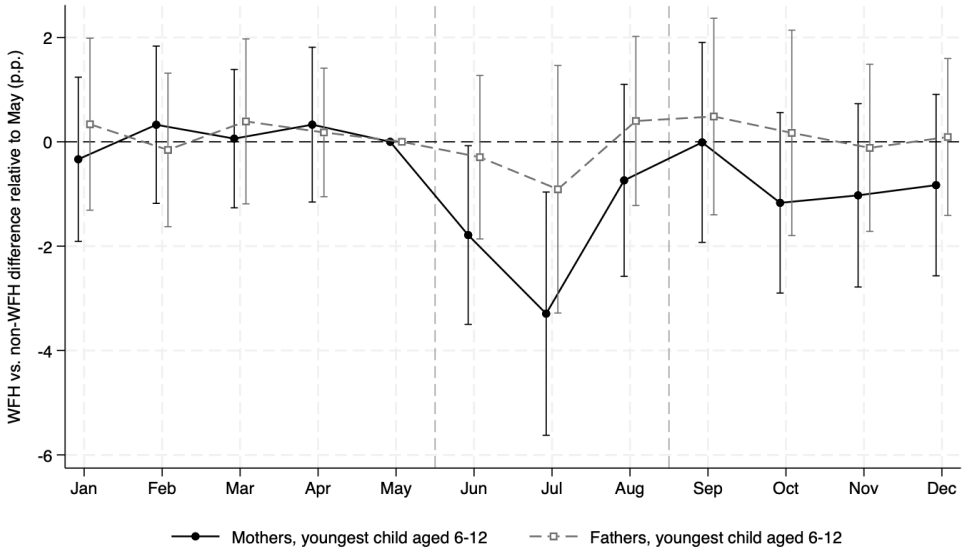
A.III. Fathers

Figure A9: Heterogeneity: Effect of WFH Access by Marital Status, Fathers



Notes: Event-study estimates of the effect of WFH access on employment for fathers, estimated separately by marital status. The corresponding estimates for mothers appear in Figure A1. Specification otherwise follows the baseline model. Bars are 95 percent confidence intervals.

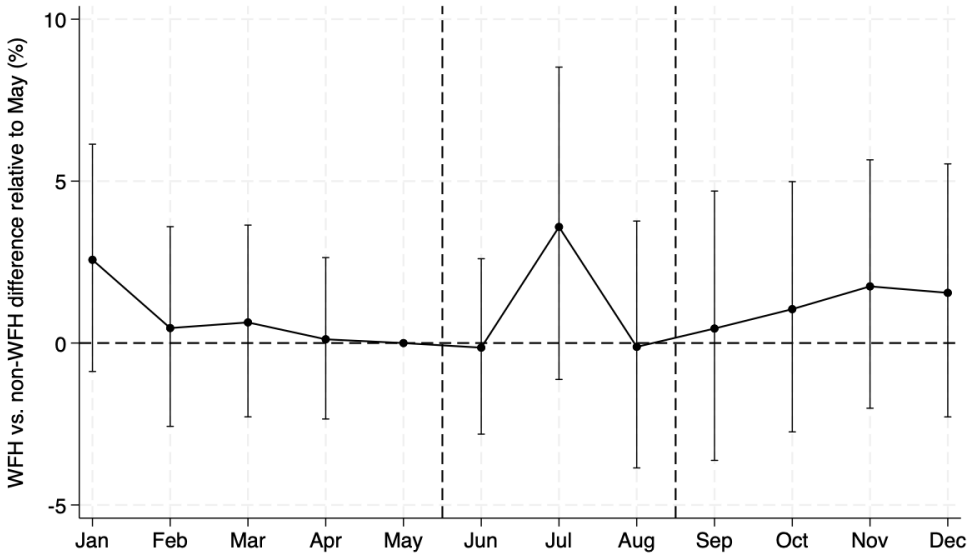
Figure A10: Effect of WFH Access on Vacation Absences: Mothers and Fathers



Notes: Event-study estimates of the effect of WFH access on the probability of being absent from work for the entire reference week for vacation or personal days, in percentage points relative to May. Estimated separately for mothers and fathers whose youngest child is aged 6–12, using identical sample construction and specification. The sample is restricted to employed person-months. The July coefficients are reported in Table 6. Bars are 95 percent confidence intervals.

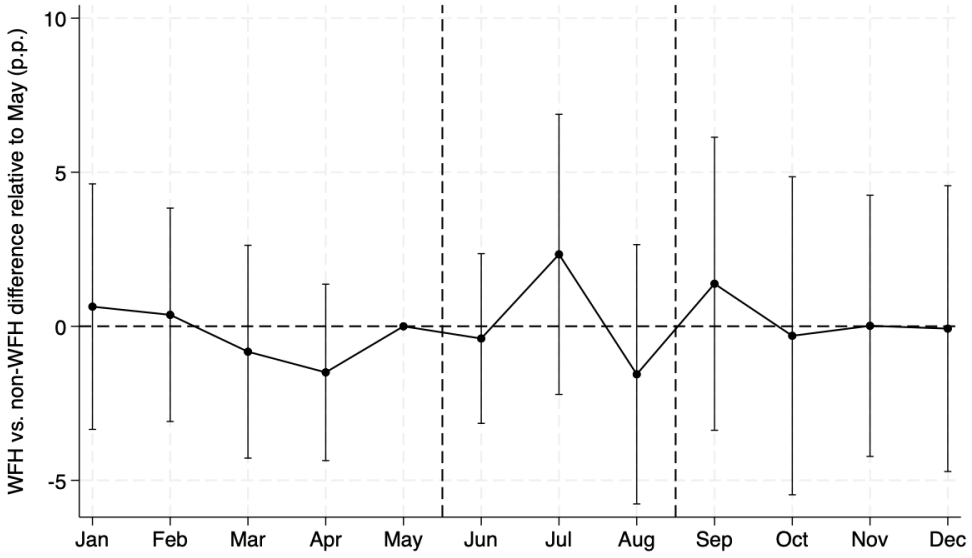
A.IV. Outcomes and Specifications

Figure A11: Effect of WFH Access on Weekly Hours



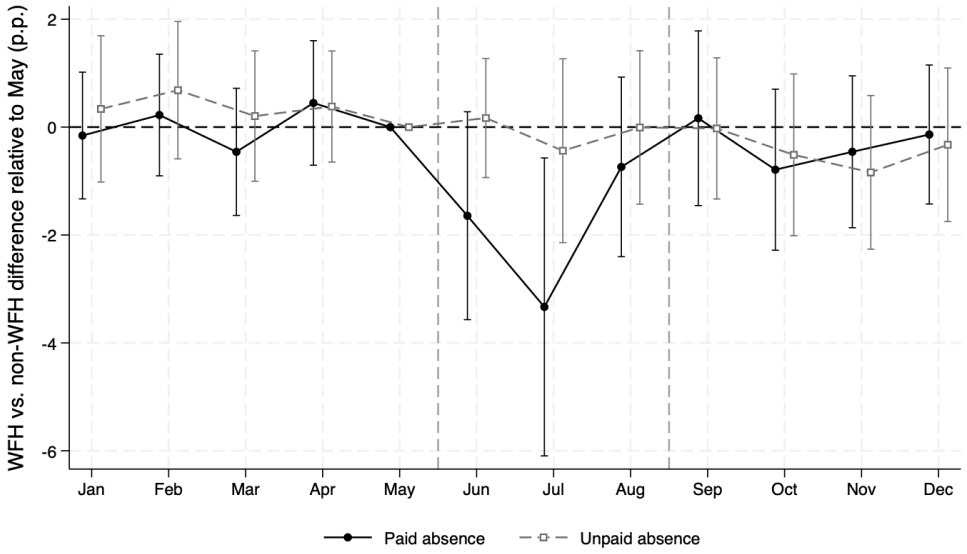
Notes: Event-study estimates of the effect of WFH access on weekly hours worked. Bars are 95 percent confidence intervals. The July coefficient is reported in column (2) of Table 5.

Figure A12: Effect of WFH Access on Full-Time Status



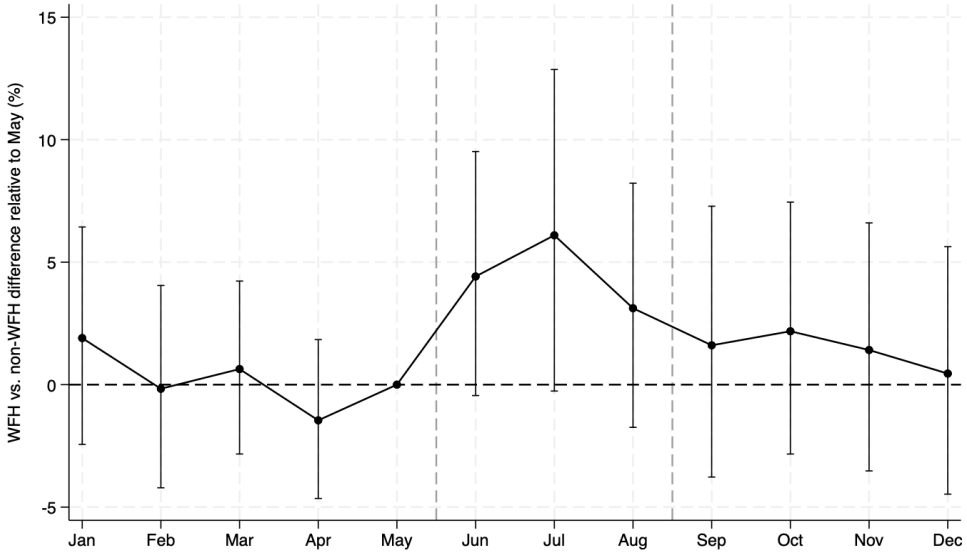
Notes: Event-study estimates of the effect of WFH access on the probability of holding a full-time job, defined as usual weekly hours of 35 or more, in percentage points relative to May. The sample is restricted to employed person-months. The July coefficient is reported in column (4) of Table 5. Specification follows the baseline model. Bars are 95 percent confidence intervals.

Figure A13: Effect of WFH Access on Paid and Unpaid Week-Long Absences



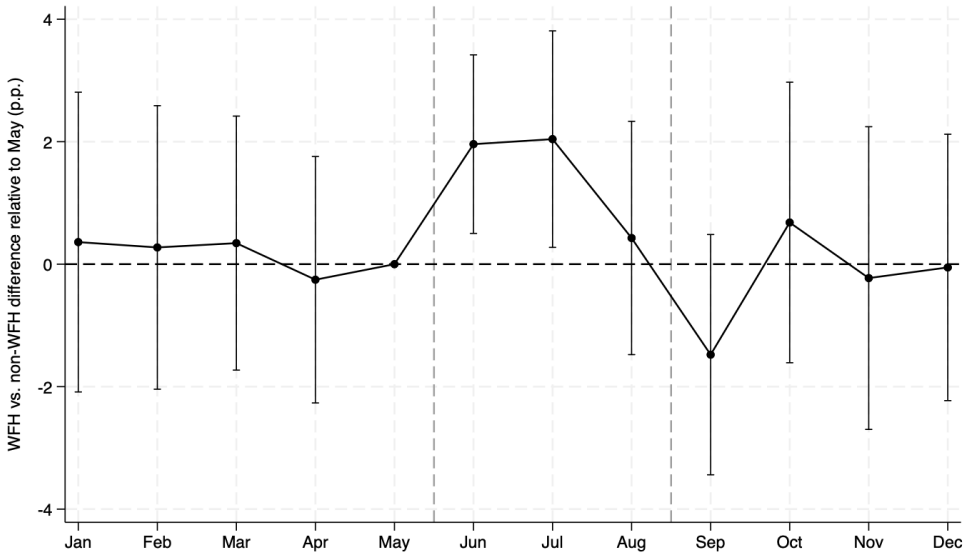
Notes: Event-study estimates of the effect of WFH access on the probability of being absent from work for the entire reference week, shown separately for paid and unpaid absences, in percentage points relative to May. The sample is restricted to employed person-months. The July coefficients are reported in Panel A of Table 6. Specification follows the baseline model. Bars are 95 percent confidence intervals.

Figure A14: Effect of WFH Access on Weekly Hours: Matched Sample



Notes: Event-study estimates of the effect of WFH access on weekly hours, estimated on the propensity-score-matched sample described in Section VI.A. Specification otherwise follows the baseline model. Bars are 95 percent confidence intervals.

Figure A15: Effect of WFH Access on Employment, Excluding Teachers



Notes: Event-study estimates of the effect of WFH access on employment, excluding mothers employed in teaching occupations. The July coefficient is reported in Panel A of Table 7. Sample and specification otherwise follow the baseline model. Bars are 95 percent confidence intervals.

Table A2: Covariate Balance Before and After Matching

	Before matching			After matching		
	WFH	Onsite	p	WFH	Onsite	p
College degree or more	0.655	0.356	0.000	0.556	0.516	0.000
Married, spouse present	0.749	0.620	0.000	0.709	0.697	0.181
White	0.707	0.611	0.000	0.695	0.682	0.142
Age of youngest child	8.529	8.609	0.006	8.577	8.609	0.422
Number of own children	1.942	1.970	0.029	1.979	1.957	0.216
Own children in household, by age						
Age 6	0.237	0.226	0.085	0.233	0.229	0.591
Age 7	0.178	0.174	0.438	0.176	0.173	0.706
Age 8	0.202	0.199	0.599	0.200	0.203	0.686
Age 9	0.218	0.203	0.015	0.213	0.205	0.301
Age 10	0.236	0.218	0.005	0.234	0.227	0.445
Age 11	0.236	0.223	0.046	0.234	0.226	0.363
Age 12	0.205	0.223	0.003	0.219	0.222	0.766

Notes: One observation per mother, with characteristics measured at the first observed interview. The post-matching columns are restricted to matched pairs. Matching is one-to-one nearest neighbor on the propensity score, without replacement, exact on baseline occupation. p-values are from two-sample t-tests. The corresponding event study appears in Figure 9.

Table A3: State Classification by School Closure Timing

Group	States
Early	Alabama, Alaska, Arizona, Arkansas, Colorado, Florida, Georgia, Hawaii, Idaho, Illinois, Indiana, Iowa, Kansas, Kentucky, Louisiana, Minnesota, Mississippi, Missouri, Montana, Nebraska, Nevada, New Mexico, North Dakota, Ohio, Oklahoma, South Carolina, South Dakota, Tennessee, Texas, Utah, West Virginia, Wisconsin, Wyoming
Mixed	California, Connecticut, Delaware, District of Columbia, Maine, Maryland, Michigan, North Carolina, Oregon, Pennsylvania, Vermont, Virginia
Late	Massachusetts, New Hampshire, New Jersey, New York, Rhode Island, Washington

Notes: Classification follows Price and Wasserman (2023) and is based on the share of the May-to-July decline in high school enrollment among 16-year-olds that occurs by June. States are classified as early closing if at least two-thirds of the decline occurs by June, late closing if less than one-third does, and mixed otherwise. The three groups contain 33, 12, and 6 states respectively, including the District of Columbia. In the estimation reported in Figure 8, mixed states are grouped with the late-closing states.