Remote Work and Women's Labor Supply: The New Gender Division at Home

Isabella Di Filippo*, Bruno Escobar†, and Juan Facal‡

Last updated: December 6, 2025

Abstract

We study how increases in remote work opportunities for men affect their spouses' labor supply. Exploiting variation in the change in work-from-home (WFH) exposure across occupations before and after the COVID-19 pandemic, we find sizable improvements in wives' labor-market outcomes: their probability of annual employment rises by roughly 2.5 percentage points (from a 69% pre-treatment mean), and both hourly wages and weekly earnings increase (by about 5% and 3%, respectively), alongside modest gains in hours and a lower likelihood of part-time work. Evidence from time-use diaries and childcare questionnaires suggests these effects are driven by intrahousehold reallocation of child-caring time: women are less likely to engage in primary childcare activities, while men working at home partially compensate by covering more for their spouse. These results highlight the role of collective household decision-making in shaping the labor market consequences of remote work.

JEL Codes: D13, J13, J22, J16, J31

Keywords: work-from-home, workplace flexibility, labor supply, intra-household allocation, gender gaps, time use, childcare

*Columbia University. Email: **isabella.difilippo@columbia.edu**
†Columbia University. Email: **be2329@columbia.edu**
‡Columbia University. Email: **jmf2285@columbia.edu**

1 Introduction

Over the past century, gender gaps in education, human capital, and labor market outcomes have narrowed substantially in developed countries (Olivetti and Petrongolo, 2016). Nonetheless, a persistent divergence remains in labor market trajectories following parenthood (Kleven et al., 2024). After childbirth, women frequently reduce their labor force participation or exit employment altogether (Kleven, 2022), and those who remain employed often face large earnings penalties relative to men (Bertrand et al., 2010). These gender disparities are largely driven by (i) the unequal division of childcare responsibilities within households, and (ii) the fact that many jobs - including high-paying white collar positions - have historically lacked flexibility options to allow women to balance both their careers and motherhood (Goldin, 2014).

This paper examines whether the expansion of one aspect of workplace flexibility - namely, remote work opportunities - can shift these intra-household dynamics and encourage greater labor force participation among women with children. We focus on U.S. households with young children and combine occupation-level data on remote work vacancies with rich household-level survey data from the ACS and other national datasets. Leveraging quasi-experimental variation in remote work exposure across occupations before and after the pandemic, we estimate whether large-scale changes in remote work availability altered household labor supply decisions, particularly by enabling mothers to remain in or return to the labor force.

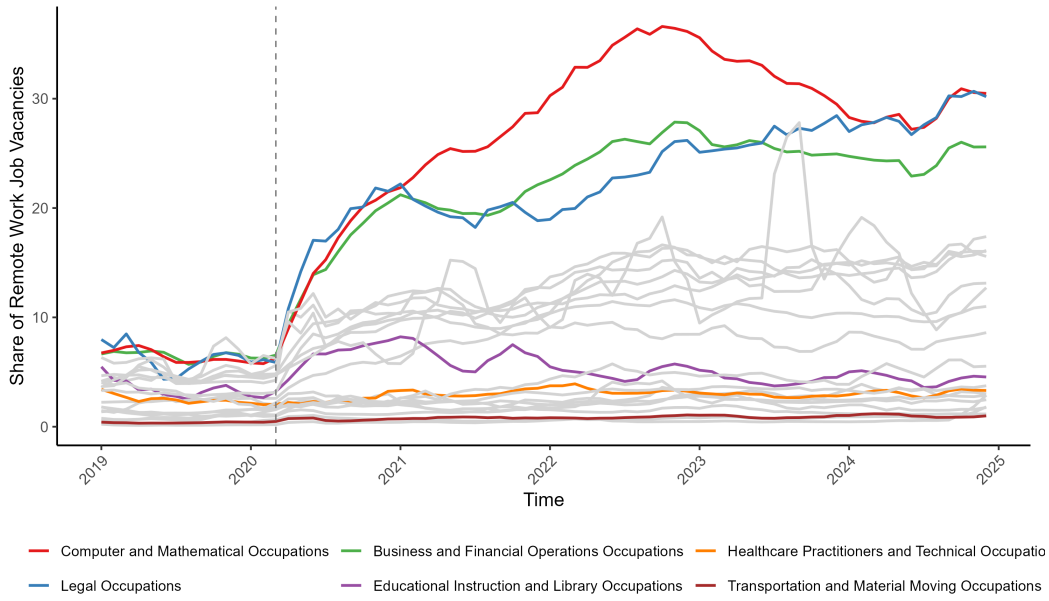
Our motivation builds on the idea, emphasized by Goldin (2014), that the *last chapter* of gender convergence may depend on restructuring work to allow for greater flexibility. Workplace flexibility has been shown to help parents, especially mothers, accommodate both predictable and unexpected caregiving demands (Angelici and Profeta, 2024). Evidence also shows that women and parents of young children place a high value on flexible work arrangements (Mas and Pallais, 2017; Bloom et al., 2022; Aksoy et al., 2022), and are more averse to commuting (Le Barbanchon et al., 2021). While not synonymous with flexibility, remote work is strongly associated with it, particularly in allowing job tasks to be performed from home, thus potentially facilitating a more equitable division of household production.

From the perspective of a collective household model (Chiappori, 1992), increased parental presence at home may ease caregiving constraints and shift bargaining dynamics within couples. Despite this potential, there is limited causal evidence on how remote work affects women's labor supply, particularly via intra-household channels. Our paper fills this gap by documenting how the expansion of remote work, especially for men, affects their spouses' employment and time use.

The backdrop for our analysis is the COVID-19 pandemic, which triggered a rapid and persistent shift in remote work norms. In the U.S., only 7% of full paid workdays were conducted from home in 2019. By June 2023, this share had quadrupled to 28% (Barrero et al., 2023). However, the magnitude of this shift varied widely across occupations: while some sectors (e.g., computer science and professional services) adopted remote work at scale, others (e.g., transport, healthcare, education) largely returned to in-person arrangements. In Figure 1, we illustrate this heterogeneity using data from Hansen et al. (2023) on remote work job postings between 2019 and 2024. We use this variation to study whether and how women's labor market outcomes respond to changes in household exposure to remote work opportunities, shaped by the husband's occupation.

Our empirical strategy compares the labor market outcomes of women with young children whose husbands experienced a large increase in the availability of remote work, measured as the change in the share of job vacancies offering WFH in their occupation, to those whose husbands experienced little or no change. Our main hypothesis is that an increase in the husbands' available WFH options leads to an increase in the wife's engagement in the labor market. This design offers two main advantages. First, the shock plausibly expands the household's set of feasible work–care arrangements, creating opportunities for women to enter or re-enter in the labor market. Second, defining treatment through the husband's occupation avoids a major selection problem: in most cross-sectional surveys, a woman's occupation is only observed if she is employed. Using changes in WFH exposure in the wife's occupation as treatment would condition on labor force participation, precluding identification of extensive margin effects – our primary outcome of interest.

Figure 1: Evolution of WFH by occupation (2 digits).



Notes: Author's elaboration using WFH job postings data from Hansen et al. (2023). Classification based on SOC 2018 2-digits.

Our first-stage estimates indicate that occupation-level increases in WFH probability after COVID translated into substantial increases in men's realized WFH: take-up rose by about 20 percentage points on average, nearly tripling relative to pre-pandemic levels. In turn, wives' labor-market outcomes improved meaningfully: their probability of annual employment increased by roughly 2.5 percentage points (from a 69% pre-treatment mean), wages rose by about 5%, weekly hours increased by roughly half an hour, and the likelihood of part-time work declined by about 3 percentage points. We also document a 10 percentage-point increase in women's own WFH incidence —approximately a 75% rise relative to the pre -treatment mean. Finally, we find some evidence that the impacts display an inverted U-shaped pattern by children's ages: the largest gains arise for households with children ages 2–5, consistent with childcare constraints being most binding in the preschool years, easing once children enter school, and being especially difficult to offset in infancy.

To validate the credibility of our identification strategy, we conduct a broad set of robustness checks that address potential threats to our findings. We examine the possibility of changes in household composition or selection into treatment following the pandemic, including tests for differential fertility and marriage patterns and reweighting strategies that align pre- and post-COVID covariate distributions. We also assess whether our results are driven

by specific occupation groups through leave-one-out analyses, and evaluate the sensitivity of our estimates to alternative fixed effect structures and demographic controls. Additionally, we explore the robustness of our findings to varying definitions of the post-treatment period and sample restrictions. Across all these exercises, our main results remain consistent, reinforcing the interpretation that increased WFH options for men played a causal role in raising women's labor market engagement.

We then proceed to study how shifts in the household's allocation of labor supply, childcare responsibilities, and household production activities could be explaining our employment effects. To do this, we rely on mainly on two data sources: the time-use diary data from the American Time Use Survey (ATUS) and the topical modules on childcare from the Survey of Income and Program Participation (SIPP). We additionaly have time-use data on commuting for the ACS, as well as a WFH uptake indicator from both ACS and CPS.

Our findings reveal that treated women increase their time spent working, with no significant change in overall household production. However, we observe that women reduce time spent on childcare as a primary activity and husbands timidly seem to compensate for that as both ATUS-based evidence and SIPP topical modules highlight. Households are also less likely to pay for daycare services. We also find evidence that these adjustments in women's employment and the allocation of childcare are not coming at the expense of the children's or women's well-being.

Overall, our findings suggest that increases in men's access to remote work can influence their spouses' labor supply decisions, particularly in households with young children. Our analysis of time-use and childcare patterns offers tentative evidence that the effects may stem from shifts in household responsibilities: women are reducing their primary childcare time and households spend less in daycare services, while husbands modestly seem to provide more primary childcaring and take care of children while women work. Taken together, the results point to the value of analyzing remote work not only as an individual labor market arrangement, but also as a household-level shock, offering new perspective on how WFH access may affect gender gaps in employment.

Related Literature Our main contribution is to the extensive literature on gender inequality in the labor market (Waldfogel, 1998; Anderson et al., 2002; Ejrnæs and Kunze, 2013; Kleven et al., 2024). Goldin (2014) emphasizes that closing gender gaps requires structural

changes in the labor market that enhance flexibility. More recently, Goldin (2022) has argued that the rise of WFH should eventually benefit employment of college-educated mothers. It has been difficult to disentangle net employment effects in the immediate aftermath of the Covid pandemic due to a shortage in the supply of childcare services (Heggeness et al., 2021). By exploiting the long-run effect of Covid on the availability of remote work options, we are among the first papers to find that WFH can lead to closing gender gaps in employment, even in developed country. Our result resembles most closely those by Jalota and Ho (2024), which find in an RCT in India that increased remote work options doubles female labor force participation among data-entry workers.

Second, we contribute to the growing literature on how working arrangements affect labor market dynamics. While existing research has primarily focused on the direct effects of flexible work arrangements on individual outcomes—highlighting their growing value for both workers and firms (Bloom et al., 2015, 2022; Angelici and Profeta, 2024; Choudhury et al., 2024; Mas and Pallais, 2017)—relatively few studies have examined how changes in work arrangements reshape decision-making at the household level. Prior work shows that women and parents place especially high value on flexibility (Mas and Pallais, 2020; Aksoy et al., 2022), yet evidence on intra-household responses remains limited. We contribute to this emerging strand by leveraging a large-scale, quasi-experimental shift in remote work feasibility to study how increased WFH access for men affects household labor supply and home production.[1]

Finally, our paper contributes to the literature on the labor supply effects of parental leave and temporal flexibility policies for parents. Prior work generally finds that such policies have limited, and sometimes even negative, effects on women's long-term labor market outcomes (Kleven et al., 2024; Ginja et al., 2023, 2020; Canaan, 2022). At best, fathers' access to temporal flexibility has been shown to improve maternal postpartum health, with little impact on female labor supply (Persson and Rossin-Slater, 2024). How, then, do we reconcile these findings with our own evidence of sizable and persistent effects from increased remote work? We argue that the scale and perceived permanence of the WFH shock fundamentally differ from traditional parental leave policies. Rather than offering

[1]Closest to our work is Harrington and Kahn (2023), who examine how increases in remote work opportunities, as proxied by women's college attainment, affect mothers' employment and earnings trajectories In contrast, we estimate the spillover effects of men's remote work access on their spouses' labor market behavior and on the intra-household time allocation.

short-term accommodations around childbirth, the pandemic-driven shift toward remote work restructured household production possibilities and gave families a broader set of tools to jointly optimize labor supply and caregiving responsibilities over the longer run.

Outline The remainder of the paper is structured as follows. Section 2 describes our main data sources and outlines the sample restrictions used to construct comparable treatment and control groups. Section 3 presents our empirical strategy and discusses the identification assumptions underlying our analysis. In Section 4, we report the main results on women's labor market outcomes. Section 5 provides a series of robustness checks and placebo tests to validate our identification strategy. In Section 6, we explore time-use and childcare data to shed light on the intra-household market and non-market supply changes that can help to explain the observed effects on the lens of a collective problem. Section 7 concludes.

2 Data

2.1 Data Sources

We draw on several public-use U.S. household surveys for our analysis. Our primary results are based on the American Community Survey (ACS), with complementary evidence from the Current Population Survey (CPS) and the Survey of Income and Program Participation (SIPP). To investigate underlying mechanisms, we use detailed time-use information from the American Time Use Survey (ATUS), with additional evidence from SIPP. To measure changes in WFH exposure at the occupation level, we rely on job postings data on remote and hybrid work arrangements (Hansen et al., 2023), as well as established measures of WFH feasibility by occupation from Dingel and Neiman (2020). We describe each data source in greater detail below.

American Community Survey (ACS). We use public-use microdata from ACS (Ruggles et al., 2025) samples from 2017 to 2023. The ACS provides detailed information on all household members, including the presence and age of children, as well as occupational codes for

both husbands and wives.[2] Critically, the ACS provides information on current weekly employment status and labor force participation, as well as retrospective measures covering the previous 12 months.[3] These include indicators for employment, usual hours worked per week, months worked, pat-time status, and total wage and salary income.[4]

WFH Job Postings Data (Hansen et al., 2023) Our preferred measure of WFH exposure is derived from job postings data on remote and hybrid work arrangements from 2019 to 2024. These data were constructed by a team of researchers in collaboration with Lightcast, using a large language model (LLM) trained to classify job advertisements based on whether the position allows remote work at least one day per week. The resulting measures are available at the month-year–occupation level[5] and at the month-year- industry level.[6] We use this data to build WFH probability changes by occupation, which will be key for our definition of treatment, as we discuss in Subsection 2.3.[7]

American Time Use Survey (ATUS) We use the ATUS (Flood et al., 2023) samples from 2017 to 2024. The ATUS draws its sample from households completing their final CPS interview, ensuring consistency in demographic and labor market variables across surveys. Each ATUS respondent reports detailed time use for a designated 24-hour period, allowing us to examine outcomes such as time spent on childcare, labor and household production. We can also assess the time doing secondary activities, such as the time spent childcaring while doing another activity. These measures are central to investigating the mechanisms underlying the intra-household spillovers we study. Importantly, the ATUS interviews only one respondent per household. By merging the ATUS with the CPS using unique person identifiers, we obtain time-use data for either the wife or the husband, but never both spouses within the same household.

Current Population Survey (CPS) and Survey of Income and Program Participation (SIPP) We replicate our main findings with public-use microdata from the CPS (Flood et al., 2024)

[2]Occupation codes at the 4-digit level are based on the Census Occupation Classification and are already harmonized to the 2010 coding scheme.

[3]This contrasts with the CPS, where annual outcomes refer to the previous calendar year.

[4]Throughout the paper, we define intensive margin outcomes such as wages conditional on employment.

[5]Occupations are classified using the Standard Occupational Classification 2018 (SOC) at 3 digits.

[6]Industry codes are classified using North American Industry Classification System (NAICS) 2022 at 3 digits.

[7]In Section 5, we also use an alternative measure capturing WFH feasibility, that is the teleworkability index à la Dingel and Neiman (2020).

spanning the years 2017–2024, and the 2018-2024 SIPP panel datasets. For the CPS, we integrate data from both the basic monthly files and the Annual Social and Economic Supplement (ASEC), also known as the March files. While weekly measures of employment are available in both the basic monthly and March files, annual employment/labor force participation and income variables are available exclusively in the March files.[8] In the case of the the SIPP, we exploit the 4-year rotational panel survey.[9] Its panel structure allows us to incorporate household fixed effects into our baseline specification to account for time-invariant unobserved heterogeneity. Also, we use SIPP childcare topical modules, which are available for all years in our 2018-2024 SIPP panels, to complement mechanism analysis.

2.2 Sample Restrictions

We restrict our primary analysis, based on the ACS, to dual-parent households in which both the husband and wife reside together and have at least one child of child-rearing age. Specifically, we focus on households with children between the ages of 0 and 8 and exclude households with more than four children. Additionally, we restrict the sample to women aged 20 to 50 and men aged 18 to 64.[10]

In terms of time coverage, we use ACS files spanning 2017 to 2023, the most recent year of data. The files contain both weekly labor force variables — comparable to those in the basic monthly files of CPS — and additional variables such as annual employment, weeks worked, usual weekly hours worked, part-time status and wage income, which refer to the previous 12 months. As a result, the effective data coverage for regressions related to these annual outcomes is 2016 to 2022.[11]

[8] See Appendix B of Kleven (2024) for a detailed discussion of differences between the CPS basic monthly and March CPS files, and between weekly and annual variables. In contrast with ACS, annual variables refer to the *previous calendar year*.

[9] In 2018 the SIPP redesigned the survey that make it difficult to compare the 2014-2017 SIPP panels and post 2018 panels. The 2018-2024 SIPP panels cover information for reference years 2017-2023. Households are interviewed once a year and asked to recall their detailed monthly employment status and income during the previous year. After the year they join the SIPP sample, households are followed and interviewed for the next three years, after which they are dropped from the sample. Each year new households join the sample, so up to 4 different household cohorts populate each year's SIPP sample. Importantly, the 2019 SIPP sample experienced significant logistical difficulties, which led the US Census to prioritize resources on following through with the 2018 sample. As a result, the sample of new households that joined in 2019 was not adequately surveyed and the decision was made to not follow up on them in subsequent surveys.

[10] We adopt standard age restrictions for women, as is common in the literature (Kleven, 2024). We additionally impose age restrictions on men to ensure they are of typical labor-force age.

[11] Note that occupation is measured/reported at the time of the survey, while annual outcomes refer to the previous 12 months. Therefore, in regressions using these outcomes, we implicitly assume that individuals'

2.3 Treated and Control Group Definitions

We use job postings data from Hansen et al. (2023) to construct occupation-level measures of the change in WFH probability before and after the COVID-19 pandemic. Specifically, we compute the difference in the share of hybrid and remote-work job postings between the post-pandemic period (2023–2024) and the pre-pandemic period (2019).[12] Figure A.1 shows the distribution of our WFH exposure measure.

We convert occupation codes from the original SOC 3-digit classification used in the job postings data to the occupation codes used in the ACS. Occupations with a WFH probability change at or above the median are classified as treated, and those below the median as control.[13] These occupation-level measures capture the pandemic-induced shift in WFH feasibility and are merged to our household survey data based on the husband's occupation at the time of the survey.[14]

Appendix Table B.1 lists the 15 most common occupations in the treated and control groups. As expected, occupations that experienced the largest increases in WFH probability after COVID-19 include those in the tech sector, law, and management,such as computer and software engineers, lawyers, and various types of managers. In contrast, occupations with minimal changes in WFH feasibility are concentrated in sales, construction, and retail, as well as in sectors like healthcare (e.g., physicians and surgeons) and education (e.g., teachers), where in-person presence remains a key job requirement. We use both the 2019 Occupational Employment Statistics (OES) data (Table B.2) and 2019 ACS data (Table B.3) to compute characteristics of treated and control occupations. As expected, treated occupations are associated with higher wages, both on average and at any percentile of the distribution, and tend to employ more educated workers with longer schooling and longer work hours. Somewhat surprisingly, women appear slightly more represented in treated occupations in the ACS sample, although the difference is modest (about 3 percentage points) and not economically significant.

occupation at the time of the survey is the same as in the previous 12 months, which may introduce some measurement error.

[12] As an alternative, we also construct a measure based on the change in WFH share between two specific months: July 2023 and July 2019.

[13] As a robustness check, we alternatively define treatment based on the median split within the original SOC 2018 3-digit classification.

[14] Ideally, we would match individuals to their pre-pandemic occupation to avoid concerns about endogenous selection into more WFH-intensive occupations on the basis of unobservables.

Using the flexibility indexes from Goldin (2014) - updated with the latest O*NET data - we find little correlation between our treatment definition and these flexibility measures (Figure A.2, Figure A.3), easing concerns about pre-existing occupational differences. As shown in Table B.4, treated occupations are somewhat more WFH-suitable (e.g., less physical proximity, more irregular schedules) but not necessarily more "flexible" in the sense of Goldin's indexes, which are statistically higher for treated occupations.[15] This distinction underscores that while remote work is related to flexibility, the two are not equivalent.

Appendix Table B.5 and Table B.6 present descriptive statistics for baseline labor market outcomes and household-level covariates, respectively. On average, treated households are more likely to have both spouses employed,[16] with both husbands and wives working at higher rates than in control households. As expected, treated households are more likely to have both spouses highly educated and less likely to have attained only a high school degree or less. Treated wives are also slightly older, on average, than their counterparts in the control group. In terms of family composition, treated households tend to have fewer children and younger children on average. Additionally, labor market outcomes for women in the treated group exhibit larger average changes post-COVID compared to women in the control group, though this pattern is purely descriptive. In all regressions, we control for the education levels of both spouses, the wife's age, and the number of children in the household.

3 Empirical Strategy

Baseline Difference-in-Differences We exploit variation in the change in WFH probability across occupations induced by the COVID-19 pandemic. Our identification strategy compares labor market outcomes of women whose husbands were employed in occupations that experienced larger shifts in WFH probability, before and after the pandemic. As explained in the previous section, change in WFH exposure is collapsed at the occupation level and reflects the difference in demand-driven WFH probabilities pre- and post-COVID.

[15] Higher index values indicate lower worker substitutability and greater job flexibility.

[16] For spouse employment and labor force participation measured in the previous week, this pattern is mechanical: the variable equals 1 for all observations because occupation—and hence treatment status—is only recorded for individuals who are currently employed or in the labor force.

Our baseline specification is:

$$y_{ist} = (\beta_1 Post_t + \beta_2 Post_t \times During_t) \times \text{High-}\Delta\text{WFH}_{o(h(i))} + X'_{it}\delta + \gamma_{st} + \theta_{o(h(i))} + \varepsilon_{ist}. \quad (1)$$

where y_{ist} denotes a labor market outcome for woman i, observed in state s and time t.[17] High-ΔWFH$_{o(h(i))}$ is a dummy for treated occupations, whereas $During_t$ and $Post_t$ are time-dummies for the years 2020-2021 and for years 2020 onwards, respectively. The coefficient on the first interaction term, β_1, captures the treatment effect of interest: it equals one for observations after March 2020 in which the husband's occupation $o(h(i))$ belongs to the set of occupations that experienced an above-median increase in the probability of WFH between the pre- and post-pandemic periods. The coefficient on the second interaction termm, β_2, captures the effect only during the covid period, from 2020 to 2021. [18]

The control vector $X_{i,t}$ includes the woman's age, education, and number of children, as well as the husband's education. All regressions include state-by-time fixed effects $\gamma_{s,t}$ to account for local labor market shocks and policy variation, and occupation-of-husband fixed effects $\theta_{o(h(i))}$ to absorb time-invariant differences across husband's occupations.

When leveraging the panel structure of one of our datasets, the SIPP, we estimate a panel version of Equation (1) by including household fixed effects. As a result, we remove any control or fixed effect that is time-invariant, differencing out all unobserved and observed factors that do not vary over time within households and/or individuals.

When conducting heterogeneity analysis by age of children or other relevant categories, we interact the treatment term with the categorical variables of interest, omitting the base category, and similarly interact controls and fixed effects to allow for differential trends across groups.

We also conduct industry-level specifications, where we construct a treatment indicator based on whether the husband's Census 2012 industry ($n(h(i))$) experienced an above-median increase in WFH probability. In these regressions, we replace the occupation-level

[17] Time is defined at the month-year level in CPS, ATUS, and SIPP. In ACS, where month of interview is unavailable, time is defined at the year level.

[18] In the ACS sample, where data are available only at the annual level from 2017 to 2023, we define the post-period as years from 2020 to 2023, due to the absence of month-level timing and in order to have a sufficient sample size to run our regressions. The during period is set for the 2020 and 2021 period. In the ATUS sample, a subsample from CPS, where we have data from 2017 to 2024, we define March 2020 as the starting point of the post-treatment period and March 2022 as the end date of the during period, in order to have sufficient sample size.

treatment term in Equation (1) with the industry-level indicator interacted with the post and during-COVID periods. As with the main specification, we include state-by-time and spouse occupation and/or industry fixed effects, along with the standard set of demographic controls.

Identification Assumption Under a standard parallel trends assumption — namely, that in the absence of the pandemic, the labor market outcomes of women whose husbands worked in occupations that experienced large increases in WFH probability would have followed similar trends to those whose husbands worked in occupations with minimal changes — the coefficient β_1 identifies the causal effect of interest. Specifically, β_1 captures any effect of the husband's exogenous shift in WFH probability, induced by the pandemic, on the wife's labor market outcomes.

Disentangling Spillovers from Matching Effects We categorize the effects on women's labor outcomes into two categories. First, *spillover effects* operating through the husband's increased WFH exposure — for instance, via reallocation of childcare or household production — which can shift the wife's labor supply even holding her own WFH exposure fixed. Second, *correlated direct effects* arising from assortative matching in marriage markets, whereby wives of men in highly WFH-feasible occupations may themselves hold (or seek) WFH-feasible jobs, mechanically correlating the husband's exposure with the wife's own exposure. A key empirical limitation is that most household surveys record occupation only for the employed; the wife's WFH exposure is therefore missing precisely when she is non-employed. This constrains our ability to condition on, or interact with, the wife's occupation in the full sample.[19]

Given these constraints, in Section 4 we adopt complementary strategies to separate and assess the contributions of spillover effects (operating through the husband's WFH exposure) and correlated direct effects (arising from assortative matching) to the overall estimated effect.

Dynamic Difference-in-Differences To assess pre-trends and the dynamic evolution of treatment effects, we estimate a dynamic version of Equation (1). Specifically, we estimate

[19] See Appendix C for a detailed discussion of these measurement and identification issues and their implications for separating spillovers from direct effects.

the following specification:

$$y_{ist} = \sum_{t \neq 2019} \beta_t \, \text{High-}\Delta\text{WFH}_{o(h(i))} \times D_t + X'_{it}\delta + \gamma_{st} + \theta_{o(h(i))} + \varepsilon_{ist}. \tag{2}$$

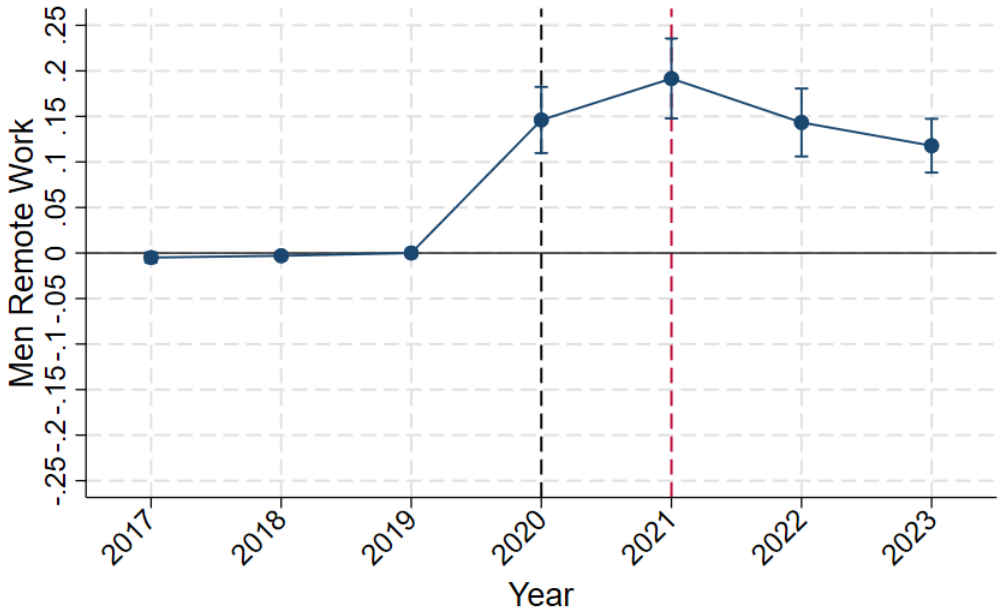
where 2019 serves as the omitted (base) year. The coefficients β_t capture the differential evolution of outcomes for women whose husbands were in occupations with large WFH shifts, relative to those in occupations with smaller or no changes, for each year from 2016 to 2023 (excluding 2019). All notation follows as in Equation (1).

4 Main Results

First-Stage: WFH Shock Exposure and Husbands' WFH uptake Our main identification strategy exploits how occupation-level shocks in women's spouses increased women's labor market participation by plausibly increasing spouse's time availability for household production. Consequently, our first stage analysis consists of verifying that large shocks on occupation-level remote work options available to fathers of young children in our ACS sample led to sizable increase in their uptake of remote work. By and large we can confirm this claim. Figure 2 shows that men in treated occupation increase their uptake of remote work by 20 p.p with respect to the control group and relative to pre-COVID year 2019. Although the effect decreases over time, it clearly does not to return to pre-covid level and is still 10 p.p.in 2023[20]. With a pre-pandemic average WFH rate of 4%, rates for men have tripled well after the pandemic in our sample households. This increase roughly matches the large increases in share of remote work job vacancies for high exposed occupations that we documented in Figure 1.

[20]We use as measure of remote work in ACS the following question: *How did you usually get to work LAST WEEK. Mark (X) ONE box for the method of transportation used for most of the distance.* As discussed in Buckman et al. (2025), this question design yields a measure for the incidence of fully remote work, and aligns with comparable measures from the Survey on Working Arrangements SWAA and other surveys.

Figure 2: Dynamic Effect of Husbands' WFH Shock on Own Remote Work Uptake



Notes: The figure plots coefficients from a dynamic difference-in-differences specification comparing husbands who were in occupations with large WFH shocks to those in occupations with smaller changes. The omitted year is 2019. All regressions include state-by-time and husband-occupation fixed effects, and control for wife's age, number of children, and education levels of both spouses. Confidence intervals are shown at the 95% level and are based on standard errors clustered at the husband-occupation level.

Table 1 reports spouses' WFH uptake estimates from the baseline DiD in (1). On average, husbands' WFH probability increases by about 13 percentage points (pp) in the post-pandemic period, with larger gains concentrated in the COVID years (2020–2021), as shown by the post-Covid and treatment interaction. We also break down WFH uptake by employment status of the husband. The increase is markedly larger for employees — roughly a doubling relative to pre-COVID levels — whereas the self-employed show a smaller but still sizeable rise of about 40 percent from a higher pre-period baseline of 14 percent. In the final column, we examine commuting time (minutes): men in more exposed occupations reduce commuting by about 5 and a half minutes, roughly 18% of the pre-treatment mean. Overall, the increase in WFH uptake among men was as large as the shock in available jobs with remote work options available in their occupations. Although the effects are much smaller by 2023 - which corresponds with more companies increasingly mandating return

to office among their employees - the shocks have had lasting effects, well after Covid.[21] Moreover, these shocks did not immediately increase men's intensive margin of labor supply. In Table B.7 we find that the remote work shock did not increase working hours or wages among men, and if anything, men spend marginally less hours of the week working. From the household's perspective, the shock initially led to a higher and long-term availability of men at households with young children.

Table 1: Effect of Occupation-Level WFH Shocks on Husbands' Remote Work Uptake

	Fully Remote Work			Commuting
	All	Employee	Self-employed	Time To Work
$Exposed_{o(h(i))} \times Post_t$	0.133***	0.139***	0.053***	-5.384***
	(0.002)	(0.002)	(0.011)	(0.179)
$Exposed_{o(h(i))} \times Post_t \times During_t$	0.038***	0.039***	0.017	-1.247***
	(0.003)	(0.003)	(0.013)	(0.205)
Mean pre-treatment	0.044	0.038	0.136	29.688
$\beta_1 + \beta_2 = 0$ (p-val)	0.000	0.000	0.000	0.000
Obs	704,497	659,974	44,491	704,408
R^2	0.17	0.18	0.14	0.06

Notes: Estimates from Eq. (1) using husbands' self-reported telework status as the outcome. The sample is the ACS, 2017–2023. Outcomes refer to the week prior to the interview. We report results for (i) the full sample, (ii) households in which the husband is an employee, and (iii) households in which he is self-employed. The WFH measure is constructed from the commuting-mode question—"How did you usually get to work LAST WEEK? Mark (X) ONE box for the method of transportation used for most of the distance."—and is interpreted as the incidence of fully remote work; see Buckman et al. (2025). All specifications control for the wife's age, education, and number of children, as well as the husband's education. State-by-time and husband-occupation fixed effects are included. Standard errors are clustered at the occupation level.

Effects on Labor Market Outcomes We hypothesize that expanded WFH opportunities for men relax constraints on household production — both temporal and physical — thereby easing wives' participation in the labor market while raising children. Table 2 presents estimates from Eq. (1) and shows that shocks on available remote work options for women's husbands increase women's labour supply both in the extensive and intensive margins.[22] We find that an increase in the probability that a wife is employed of about 2.5 percentage points - increase of roughly 4% relative to a pre-treatment mean of 69%. Weekly hours increase by about 0.5 hours, and accordingly the likelihood of part-time work falls by around

[21] Our ACS captures only fully remote work, but nowadays hybrid options are more prevalent.

[22] We find similar results if we split occupations by quartiles of the treatment exposure. Effects are concentrated and largely similar among the third and fourth quartiles as shown in Table B.8

3 percentage points.[23]

Table 2: Effect of Husbands' WFH Shock on Wives' Labor Market Outcomes

	Employment	Log Weekly Wage	Log Wage	Usual Weekly Hours Worked	Part-time Status
$Exposed_{o(h(i))} \times Post_t$	0.024***	0.030***	0.052***	0.569***	-0.035***
	(0.005)	(0.010)	(0.012)	(0.117)	(0.006)
$Exposed_{o(h(i))} \times Post_t \times During_t$	-0.009*	-0.015	-0.009	0.073	0.006
	(0.005)	(0.010)	(0.012)	(0.114)	(0.005)
Mean pre-treatment	0.698	6.447	9.956	35.916	0.480
$\beta_1 + \beta_2 = 0$ (p-val)	0.000	0.041	0.000	0.000	0.000
Obs	729,120	514,151	514,151	539,181	729,120
R^2	0.08	0.18	0.15	0.03	0.08

Notes: This table reports estimates from Equation (1) using the 2017–2023 ACS sample of dual-parent households with children of child-rearing age. The dependent variables include annual employment, log weekly wages and annual wages, usual hours worked in the week, and part-time status. All intensive margin outcomes are defined only for employed individuals and are coded as missing for those not working. The treatment indicator is defined at the occupation level based on the change in the probability of WFH between the pre-pandemic period (2019) and the post-pandemic period (2023–2024), as described in Subsection 2.3. All regressions control for the wife's age, education, and number of children, as well as the husband's education. Specifications include state-by-time and husband's occupation fixed effects. Standard errors are clustered at the husband's occupation level.

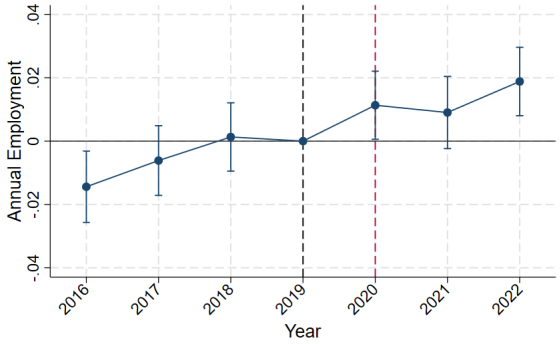
Figure 3 displays the annual coefficients from the dynamic DiD specification described in Equation (2) for our main labor outcomes. Employment exhibits mild pre-trends in 2016–2017, and part-time status exhibit minor pre-trends in 2016. Wage and work hours coefficients are comparatively flat prior to treatment. Overall, evidence mostly supports our parallel trends assumption, and points towards stronger effects in the post-Covid era - after 2022.[24] Moreover, effects are persistent in the post-Covid era which matches the lasting effects on the uptake of remote work found in the first stage analysis.

[23]We also evaluate how our effects would change if we defined treatment at the male spouse's industry occupation-level. Figure A.4 shows time series evolution of industry-level WFH trends between 2017 and 2023, showing largely analogous effects to the occupation-level trends shown in Figure 1. In Table B.9 we present estimates of our baseline specification but using the husband's industry-level WFH exposure as the treatment variable. The results go in the same direction as those based on occupational exposure, but are lower in magnitude and not as significant. This is consistent with our prior that employer industry may not be as precise as occupation to identify available remote work options. Within a given industry or firm - e.g. hospitals or education -, certain jobs are much more likely to be teleworkable than others - e.g. office admin vs health practitioner.

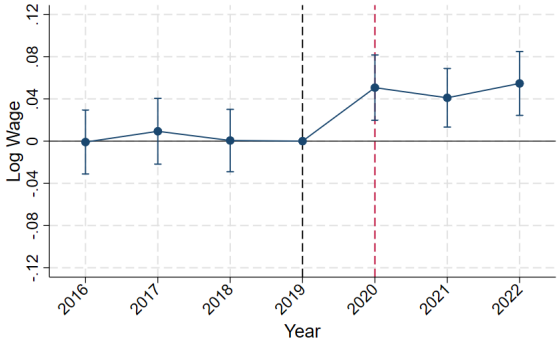
[24]To assess whether these effects are driven by improvements in treated households or declines in control households, Figure A.5 plots adjusted labor market trends separately for treated and control groups. Employment saw a dip for both groups during 2020, but by 2022 the treated group saw large increase in employment with control group going back to pre-pandemic levels. Working hours and wages saw large and highly significant effects only for the treated groups, whereas part-time status saw large decreases only for the treated group albeit with some pre-trends.

Figure 3: Dynamic Effect of Husbands' WFH Shock on Women's Labor Market Outcomes

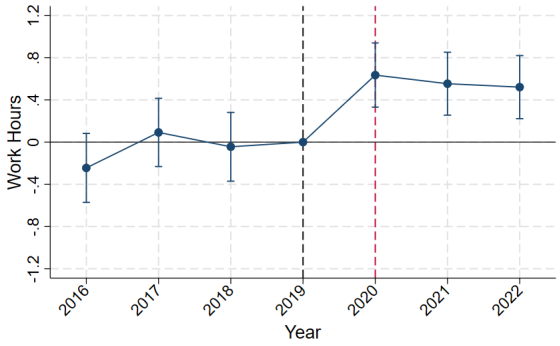
(a) Annual Employment



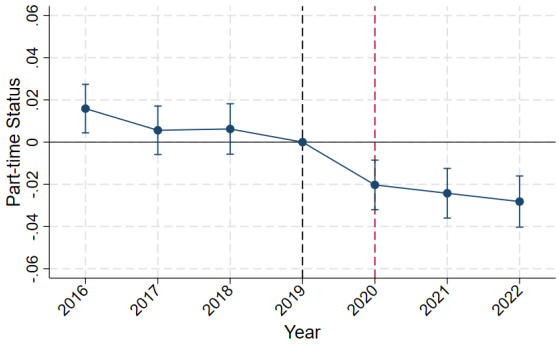
(b) Log Wages



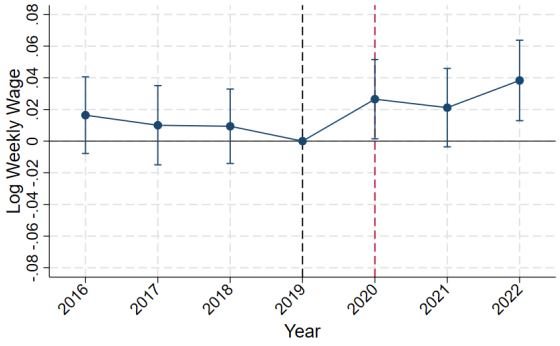
(c) Usual Weekly Work Hours



(d) Part-time Status



(e) Log Weekly Wages



Notes: Each panel plots coefficients from the dynamic DiD specification shown in Equation 2 in the 2017-2023 ACS sample. The omitted year is 2019. All regressions include state-by-time and husband-occupation fixed effects, and control for wife's age, number of children, and education levels of both spouses. Confidence intervals are shown at the 95% level and are based on standard errors clustered at the husband-occupation level.

Heterogeneity by Household Demographics. We investigate a handful of standards household demographics to give a sense of what type of couples are driving the positive effects of spousal WFH shocks on women labor supply. In Table B.10, we estimate the baseline regression on separate samples based on the spouses' education levels. We note that the effect of the spousal WFH shock on women's labor supply is the strongest, most significant and the closest to our baseline effects for women that have a college degree. Moreover, the sample of college educated women is the majority of our sample and their point estimate is the closest to our baseline. Regarding husband's education level, we see strong effects on women's labor supply mostly when their spouse has a college degree. This makes sense insofar as this group is more likely to have remote work options and use them effectively. Similarly, wage effects for women are the highest and closest to our overall baseline effects when either spouse has college degree. Thus we argue that we are capturing effects among highly educated couples.[25]

Next, in Table B.11 we perform a similar heterogeneity analysis based on women's age. The effects on labor supply for women in the range of 25 to 40 years old are the most similar to our baseline effects. However, wage effects are significant and high only for ages 30 to 40, whereas extensive margin and working hour effects are the highest for ages 20 to 24 and 25 to 29, respectively.

Finally, we would ideally look at whether intensive margin effects are driven by women that are entering the labor market or that were already working prior to the shock. We do not have a long panel to analyze this directly, but using the wide range of household demographics that are strongly predictive of employment status, we can estimate a model to predict female employment in the pre-Covid sample.[26] Table B.12 shows our results for the ACS sample. The triple interaction coefficient captures the differential effect of a spousal WFH shock on women that our random forest model predicts that would have been in the labor market in the pre-Covid period vis-a-vis. Wage effects are largely the

[25] We also see significant and large effects for the group of women without completed high school education. We note that this has very low employment levels pre-treatment, so any change is more likely to be significant.

[26] Specifically, we estimate the probability that women would have worked in the pre-Covid sample using a random forest classification method and our battery of controls plus the state of residence as inputs in the classification. We use this model to predict the pre-Covid employment status for all women in the sample, including the out-of-the-bag post-Covid sample. The model's predictive power is high: Around 84% of women in the pre-Covid sample that the model classified as being unemployed were indeed employed, and 88% of women classified as employed were in fact employed. We use the classifying variable to estimate heterogeneous effects for women that would have been strongly more likely to be employed in the pre-Covid era.

same for both groups. Following spousal WFH shocks, women more likely to have entered the labor market see a larger. Instead, women already in the labor market saw twice as large decreases in part-time status. Changes in WFH uptake and commuting time and bigger are only significant for women likely to have been in the labor market.

Spousal WFH Shocks and Household Composition Within the universe of two-couple households, we investigate whether differences in household composition can give a sense on how the responsibilities of household allocation shape the effect of spousal WFH shocks on women's labour supply. First, it is sensible to believe that results would differ depending on how young children are within our sample. In Figure A.6, we examine this possibility by estimating heterogeneous effects by age of the youngest child in the household. We estimate our baseline specification on separate subsamples of the ACS by age of the youngest child in the household. Employment and working hours effects are the largest for households with 2-year olds and steadily declines for 3-year olds and upwards. This timing coincides with daycare, pre-K and schooling becoming a more salient option. Part-time effects follow a mild U-shape, which peak for children between 2 to 5 years old. Wage effects are largely the same across for households with children of different ages. Overall, women see the largest gains from spousal WFH shocks when their youngest are aged 2 to 5. This is consistent with childcare constraints being most binding in the preschool years, easing once children enter school, and being especially difficult to offset in infancy.

Second, it is plausible to see women labor supply effects for women with older children or even among couples without children. An increase in spousal location flexibility can enable more work options for the other spouse. We hypothesize that in couples without young children we should see smaller effects of spousal WFH shocks on women's labor supply. We can largely confirm this in Panels A and C of Table B.13 where we look at women labor supply effects for households without any children, or where the youngest child is already an adult, respectively. Effects on employment are at most half the size, and wages and work hours do not change significantly. We do, however, see significant effects among households where the youngest child is roughly a teenager (12 to 17 years old - Panel B) that are fairly similar to our main sample effects. This suggests that eve among older children, childcare responsibilities are still relevant for women when households decide on labor supply.

Effects of Spousal WFH Shocks on Men Our preferred specification uses husbands' changes in their occupation WFH rates following Covid-19 as an exogenous shock that impacts their wives' labor market decisions. However, it could wee be that when their wives' face an increased availability of remote work options, men also respond in their labor supply. While increasing women's availability at home is not bound to change division of home production, it may still increase the intensive margin of men's labor supply. In order to test for any such effects, in Table B.14 we invert the estimation procedure and estimate the effect of female spouses' WFH shocks on their husbands' labor market outcomes in the ACS data. While we do not observe changes in men's work hours, their wages increase by 2%. This suggests men are more capable of taking better paying jobs owing to their wives increased flexibility.

Disentangling Spillovers from Matching Effects As discussed in Section 3, our baseline coefficients combine two channels: the intra-household response generated by men's WFH exposure (*spillover effects*) and the component operating through women's own WFH exposure that is correlated with the former spouse's shock (*correlated direct effects*).

As a first step toward isolating women's own shocks, we augment Equation (1) with women's occupation fixed effects. In this specification, β captures the effect of husbands' exposure to high-WFH occupations, holding constant wives' exposure through their own occupation.[27] Table B.15 indicates economically meaningful spillovers: wives in treated households earn about 5% higher labor earnings, 3% higher wages, work roughly 30 additional minutes per day, and are 2.7 percentage points less likely to be part-time. Second, we estimate the effect of women's WFH shocks on their own labor market outcomes. Table B.16 indicates that women do experience an increase in their intensive margin outcomes.[28]

Neither of the previous strategies allows us, however, to separate *correlated direct* and *spillover* effects on the extensive margin of women's labor supply, nor do they quantify the relative contribution of each channel. To overcome this, we adapt Harrington and Kahn (2023)'s strategy, and leverage women's college major as an ex ante proxy for WFH probability.

[27] By construction, this specification conditions on employed wives, so the estimates pertain to intensive margins only.

[28] Due to survey design, we are unable to estimate extensive-margin own-elasticities. Occupation is only reported for individuals who are currently employed or in the labor force, which makes occupation-based WFH exposure mechanically collinear with participation and employment status. As a result, it is not possible to separately identify the causal effect of own WFH exposure on the decision to enter the labor force or become employed.

For each major, we compute pre- and post-pandemic changes in WFH uptake and define High-ΔWFH$_{e(i)}$ as above the median.[29] We then estimate:

$$\begin{aligned} y_{ist} = \beta_1 \big(Post_t \times \text{High-}\Delta\text{WFH}_{o(h(i))}\big) + \beta_2 \big(Post_t \times \text{High-}\Delta\text{WFH}_{e(i)}\big) \\ + \beta_3 \big(Post_t \times \text{High-}\Delta\text{WFH}_{o(h(i))} \times \text{High-}\Delta\text{WFH}_{e(i)}\big) \\ + X_{it}'\delta + \gamma_{st} + \theta_{o(h(i))} + \varepsilon_{ist}. \end{aligned} \tag{3}$$

In this setup, β_1 identifies intra-household *spillovers* for women who are ex ante unlikely to WFH (low-exposed majors), β_2 captures women's direct exposure holding husbands unexposed, and β_3 captures the incremental effect when both channels are turned on, disentangling the *correlated direct effects* of women.

Table B.17 shows a strong result of our paper: *spillovers effects* do matter and can explain part of the increase in the labor supply of women. Treated households with women with low-exposed majors experience an increase in employment of 1 p.p (about 1.5% of the pre-period mean), a 2.3 p.p reduction in part-time likelihood, and increase their working time by 20 minutes. There are no effects on wages or earnings. Among high-exposed majors, the joint effect of the *spillover effects* and the assortative ones imply a 4.4 p.p. employment increase, a 5.3 pp reduction of part-time and more than an additional hour of work. Together, these results suggest that *spillover effect* accounts for approximately one-quarter of the total effect (or two-fifths for part-time) on the labor supply outcomes, with the remainder operating through women's *correlated direct* effects due to assortative matching. However, the *spillover effects* cannot account for the increase in wages. The treatment gains in wages and earnings are statistically entirely explained by the *correlated direct effects* due to assortative matching.

5 Robustness and Validation

Group Composition and Selection We conduct a series of robustness checks to assess whether our main results are driven by changes in group composition between treated and control households before and after the pandemic. Given the cross-sectional nature of the ACS, one potential concern is that our findings may not reflect intra-household adjustments—for example, a woman increasing her labor supply in response to her husband's

[29] Women without a reported major form a separate bin.

WFH shift—but instead reflect changes in assortative matching. Specifically, it is possible that, post-COVID, men in high–WFH-shock occupations are more likely to be partnered with women who are already more attached to the labor force.

We address this concern by examining changes in assortative matching over time. Figure A.7 in the Appendix provides suggestive evidence that changes in spousal composition are unlikely to be driving our main results. While there is clear evidence of assortative matching, i.e., households in which the husband is in the highest quartile of WFH exposure are more likely to have wives in similarly high-exposure occupations than households in which the husban is in the first quartile, this pattern appears stable over time. Specifically, the distribution of wives' exposure conditional on their husbands' treatment quartile does not shift meaningfully between the pre- and post-COVID periods. As discussed in Subsection 2.3, occupational WFH exposure is strongly correlated with wives' income and labor force attachment. The stability of the matching pattern across periods is therefore reassuring and suggests that selection into treatment via assortative matching is not biasing our estimates. In addition, we examine potential changes in key demographic outcomes - namely, marriage, divorce, and fertility. Table B.18 reports estimates from specification (1), where the dependent variable is an indicator for whether the woman got married, divorced, or had a child within the 12 months preceding the interview date. Across all outcomes, we find no statistically significant effects of the husband's WFH shock, suggesting no differential selection into or out of marriage, nor into fertility, between treated and control groups following the pandemic.

Another potential concern is that the composition of households with young children may have shifted after the pandemic. If couples choosing to have children post-COVID differ systematically in income, education, or labor-market attachment, our estimates could partly reflect selection rather than causal effects. To assess this, Figure A.8 plots changes in the probability of having a child and in the number of children across quartiles of husbands' WFH exposure, before and after COVID. The figure shows that households in the lower two quartiles of husbands' exposure became slightly more likely to have a child and to have more children than those in the upper quartiles, but the differences are very economically small. Consistently, Table B.19 reports regression results from Equation (1) showing no meaningful effects on fertility outcomes, and column (3) of Table B.18 indicates no

statistically significant change in the probability of having a child in the past 12 months.

To address concerns about endogenous changes in group composition over time, we implement reweighting strategies that adjust the post-COVID sample to match the distribution of pre-COVID covariates. We estimate a single logit model for the probability of an observation belonging to the pre-COVID period, using spouse occupation, wife's age and education, and state as predictors.[30] We construct DiNardo et al. (1995) (DFL) weights as $p/(1-p)$ and inverse-propensity score (IPW) weights as $1/p$, where p is the predicted probability of being in the pre-COVID period.[31] As shown in Table B.20, the reweighted estimates remain consistent with our baseline results, alleviating concerns that shifting sample composition drives our findings.

Treatment Definition Robustness A potential concern is that our baseline treatment definition — based on changes in the average within-occupation WFH job postings rate between the post-covid period in 2023 and 2024 and the pre-covid period in 2019 at the Census 2010 4-digit occupation level — may be driving the results. To address this, we examine the robustness of our findings to a range of alternative treatment definitions in Table B.21 presents.

The estimates reported in Panel B confirm that smoothing the share of remote work job vacancies using a three-month moving average does not affect the treatment assignment, and henceforth, the resulting coefficients. Similarly, the results in Panels C, E, F, and G, which use alternative classification schemes and different measures of WFH change, yield estimates that are closely aligned with those obtained under the baseline treatment.

As an alternative measure of remote work potential, we incorporate the teleworkability index developed by Dingel and Neiman (2020) in Panel D and H, which classifies occu-

[30] The predicted probabilities are obtained from the following specification:

$$\Pr(\text{Pre-COVID}) = f_s + f_{o(H)} + f_e + f_a + \varepsilon,$$

where f_s, $f_{o(H)}$, f_e, and f_a denote state, husband's occupation, education, and age fixed effects, respectively. We then plot the predicted probabilities for pre- and post-COVID observations to verify common support (see Figure A.9).

[31] Both sets of weights are normalized to sum to one and scaled by ACS sampling weights.

pations based on their suitability for being performed from home.[32][33] In contrast to our primary measure based on changes in job postings, this index captures the inherent feasibility of telework by occupation, rather than shifts in employer demand for remote work arrangements.[34] The panels yield nearly indistinguishable estimates from those based on the benchmark treatment.

Taken together, these findings indicate that our results are not sensitive to how treatment is defined, and that the estimated effects are robust across a range of reasonable alternatives.

Specification and Sample Robustness We test in several ways the robustness of our main women employment results to minor changes in the specification and sampling choices. First, we test the robustness to the choice of fixed effects. By design, the correct specification should include at least an after Covid-19 dummy and a treatment dummy. In our baseline specification, we generalize these to year-by-state fixed effects and spouse occupation fixed effects to gain more precision. In Table B.22, we make minor alterations to the fixed effect structure in our main estimations for the ACS sample. We change the occupation group definitions, the geographical area, the level of interactions between state fixed effects, and others. By comparing all other results to Panel E - our baseline specification - a clear pattern emerges in that coefficients do not change significantly with the fixed effects interactions.

Second, our results may be driven by differential trends across broad industries or occupations. Table B.23 addresses this issue by incorporating year-by-broad-occupation and year-by-broad-industry fixed effects. The estimates remain statistically significant in nearly all specifications, with annual employment being the only outcome that exhibits a modest attenuation in some cases. In addition, Table B.24 controls for heterogeneous educational trends by comparing occupations within the same quintile of average years of schooling. The treatment effects persist, with annual employment being the one with the most attenuation. Taken together, these results indicate that our findings are not driven by differen-

[32]The index is constructed using responses from two O*NET surveys: the Work Context Questionnaire and the Generalized Work Activities Questionnaire. These surveys assess the importance of physical presence in task performance, allowing for an evaluation of whether specific tasks within each occupation could feasibly be performed remotely. The final occupation-level indicator is calculated as the average teleworkability score across tasks within each occupation.

[33]Consistent with our baseline job-posting measure, we define treatment as an indicator equal to one if the husband's occupation is at or above the median of the teleworkability distribution.

[34]Importantly, this classification does not account for demand-side frictions or social norms that may affect the practical implementation of remote work. For instance, while Dingel and Neiman (2020) classify teachers as capable of WFH, post-pandemic trends suggest that in-person instruction remains the preferred mode, largely due to parental demand (Barrero et al., 2023).

tial educational trends across occupations, nor by other unobserved characteristics within broad occupations or industries.

Third, we perform other exhaustive analysis: (a) varying the demographic controls of the households in Table B.25 and Table B.26; (b) restricting the sample to non self-employers and trimming top income households in Table B.27 and Table B.28 respectively; and, (c) dropping major occupation groups in Table B.29. Overall, all these findings indicate that our main results are not sensitive to the choice of controls, or sample restrictions both at the household and occupation level.

Placebo tests We conduct a standard timing falsification test by coding the timing of the Covid shock as starting on 2018 and estimating the baseline specification on the 2017-2020 ACS subsample - reference years being 2016-2019 - and defining the pre- and post-shock periods as 2016-2017 and 2018-2019, respectively. This data has no influence from the post-Covid era, and since the Covid shock is largely unexpected, we would expect no effects to appear on these estimations. We present the results from this placebo test in Table B.30. We find no effect on working hours and wages occurring around this artificial time of treatment, and very weak and small effects on part-time status. However, we do find effects on employment, which are consistent with the slight pre-trends shown in the dynamic DiD analysis.

Evidence from Alternative Datasets Estimate from alternative datasets generally support our results. First, we replicate our analysis using the 2017-2024 CPS sample, as described in Section 2. Although the CPS offers a smaller sample than the ACS, it provides comparable labor-market measures that allow an independent check of our main findings.[35] Consistent with this, Table B.31 shows reduced-form estimates for wives that mirror the ACS: women in higher-exposure households are about 2 percentage points more likely to be employed and in the labor force, report higher weekly hours, are roughly 3 percentage points less likely to work part-time, and earn approximately 8 percent higher wages.

Second, we replicate several of our main employment results for women in the SIPP data. We also exploit the 4-year panel component allows us to estimate a panel regression. Specif-

[35] Because CPS outcomes are measured monthly, we define the "during" and "post" periods as described in Section 2 for the ATUS sample. Namely, we define March 2020 as the starting point of the post-treatment period and March 2022 as the end date of the during period.

ically, we replace our baseline fixed effects with month-year and household-level fixed effects. We present results for several samples in Table B.32.[36] Although our dataset spans information from 2017 to 2023, we focus on the 2020 SIPP panel, which follows households from reference years 2019 to 2022. In practice, it is the only panel to cover household in the pre- and post-Covid periods.[37] The effects of spousal WFH shocks on women's labor supply follow the same sign as the ACS and CPS results, but we remark that estimates are much noisier. Point estimates for extensive margin are similar in magnitude but statistically significant, whereas working hours and wage effects are significant but much larger than what we found in previous datasets. Assuming the effect size enlargement is mostly noise, this could be due to power issues: Our sample is much smaller in these regressions and we are limited to around 500 unique households in these regressions.

Summary Taken together, these robustness and falsification checks support the validity of our identification strategy. The estimated effects are not driven by differential selection, sample restrictions, or alternative treatment definitions. Main estimates across different nationally representative datasets are similar either sign, although panel estimates are noisy.

6 Household Specialization

The remote work shock may have changed how households coordinate their market and home production decisions. Understanding and evaluating the changes in household specialization and time use is central to reconcile not only women's increase labor supply, but also interpret the overall effect of the WFH shock on households.

In this section, we argue that the large employment effects we observe for women with young children can be explained by changes in the household production function, particularly in the burden and organization of childcare duties. As discussed in Section 4, these changes may arise from two forces: (i) *spillovers* from husbands' increased presence at home and (ii) women's own access to work from home through assortative matching.

To explore how the intra-household allocation of time adjusts in response to the WFH shock, we estimate Equation (1) using variables from the ATUS, ACS and SIPP that capture both

[36] In addition to sampling households of dual parent households with children aged 0 to 8 years old, we restrict the sample to households that have not switched treatment status across different panel interviews.

[37] Due to logistical issues, the SIPP did not follow through the sample of new households that joined the sample in SIPP 2019.

the intensive and extensive margins of labor supply and household production. Our goal is to assess how time spent on household activities and childcare is redistributed within households, explaining with the lens of a collective perspective the observed increase in labor market participation among women in treated households.[38][39][40]

Market Production Time Use First, husbands experience a substantial change in their work location, consistent with being the directly treated group as Figure A.10 illustrates. These sheds more light on the validity of the first stage. They increase time working from home by 4.7 percentage points and appear to reduce time working outside the home. Although the latter estimate is noisy due to limited sample size, Figure 4 shows a negative and relatively stable effect across all post-treatment years. These shifts in work location are persistent over time, unlike the more transitory patterns observed for women. Notably, men do not exhibit any change in total hours worked. However, treated men consistently report reduced commuting time by 5 minutes on average. Efficiency-wise these results mean that men are working the same amount of time, but avoiding part of the sunk costs from commuting.[41]

Wives experience an increase in the time allocated to market work, which aligns with the labor supply effects Section 4 documents. As shown in Figure A.10, treated women increase on average the share of their day spent working after the pandemic by 7.5 percentage points, i.e., equivalent to nearly two additional hours.[42] This rise is driven mainly by a 5 percentage point increase in working from home, which is consistent with the 6.5 pp. increase in WFH uptake we report in Figure 4 . Consistent with the fact that most of increase in working is coming from work from home uptake, there is a significant reduction in the time spent

[38] For the ATUS, we restrict the sample to respondents who completed the time-use diary on a weekday and non-holiday day, in order to capture household dynamics on a typical workday.

[39] We evaluate the comparability between the two ATUS samples and the CPS, given that the ATUS samples are non-overlapping subsamples drawn from the CPS. Table B.36 presents the first and second moments of the distribution for key characteristics of husbands, wives, and households across the CPS sample and both ATUS subsamples. The CPS sample displays modest differences relative to the ATUS samples, particularly in the likelihood of treatment and in measures of household income and education. These patterns suggest that the ATUS subsamples tend to capture relatively wealthier and more educated households.

[40] The estimated effects might be driven by specific subsamples whose responses to the WFH shock diverge from those identified in the ACS results in Table 2 or the CPS results in Table B.31. We address this in two ways. First, both Table B.33 and Figure 4 show that the increase in women's employment following the WFH shock is statistically significant and persistent. Second, despite the smaller sample sizes in ATUS, Table B.37 shows that the estimated effects on wives' labor market outcomes are directionally consistent with those in CPS sample, which are also aligned with our main findings using the ACS sample.

[41] For the ATUS sample, the reduction is of around 17 minutes.

[42] A full day includes 24 hours.

commuting for most of the post-treatment years by almost 10% - 2.5 minutes. [43]

Household Production Time Use This change in the joint labor market choices came alongside with new patterns in the collective household production, as Figure 4 documents. Treated households exhibit meaningful shifts in the allocation of childcare responsibilities. Although the total time spent caring for children does not change significantly, there is a clear shift in how childcare is provided. Women in treated households reduce the amount of time spent on childcare as a primary activity by 2.7 pp - an approximately 25 percent decrease relative to the average pre-treatment values. While they are not reporting to be spending significant more time child-caring while working, although they are spending more time WFH. Additionally, there is no reduction in secondary childcare, as Table B.33 shows. In other words, women mainly cut back on primary childcare. As a result, secondary childcare becomes more prominent: its share of total childcare time rises by 8.4 p.p. relative to a pre-treatment mean of 64.5 percent.

Husbands, on average, do not exhibit significant changes in total time spent on overall childcare. However, as shown in Figure 4, their time spent on childcaring while working increases significantly by 1.5 percentage points. This is a 200% increase relative to baseline. This aligns with the fact that men are more likely to work from home due to their shock. These patterns point that women reduce time spent on primary childcare, and men appear to partially compensate while working. In terms of household division, there is a more equal distribution in the time spent childcaring between spouses. The gender gap in childcare time was of approximately 0.5 in the pre-treatment, as Table B.33 reflects, meaning that women spent the double of time childcaring compared to men. Treated households narrow the gap to approximately 0.75.[44][45]

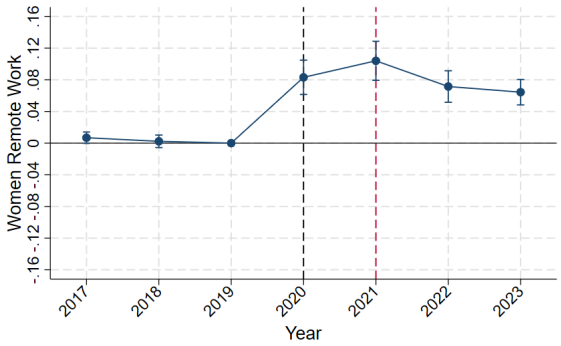
[43] In Table B.33, the ATUS commuting variable is not significant, which it is not striking. It is more difficult to detect an effect on a substantially smaller sample size and on an outcome whose variation is small, relative to others like share of time working.

[44] On the household production side, both men and women do not spend significantly any change in time in housework. Although this estimate is not precise for women, it consistently points in a negative direction, while virtually zero for men. Childcare might be more substitutable and proportionally more important in time to be able to detect significant changes, compared to other household tasks. This might explain the lack of precise effects on the other less prominent household activities.

[45] We conduct a standard placebo test by assigning the onset of the post-treatment period to March 2018. Table B.38 reports the results. We find no significant changes.

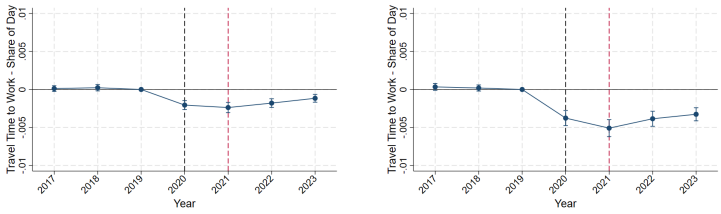
Figure 4: Dynamic Effect of Husbands' WFH Shock on Women's WFH Uptake and Households' Time-Use

(a) Women WFH Uptake (ACS)

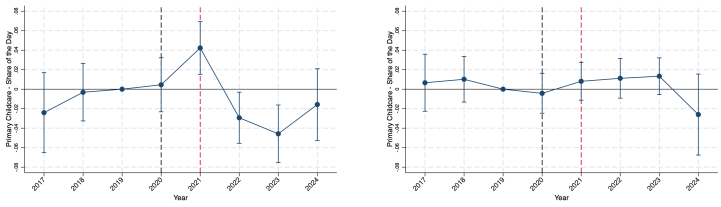


Women Men

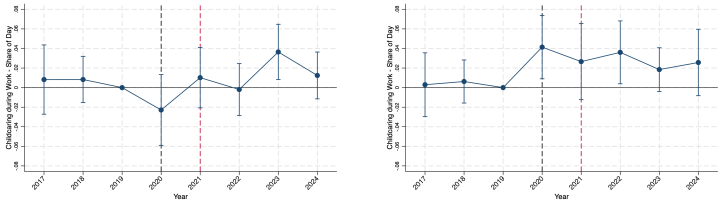
(b) Commuting (ACS)



(c) Primary Childcare (ATUS)



(d) Childcare while Working (ATUS)



Notes: The top center panel shows effect of spousal WFH shock on women's WFH uptake. Starting from the second row of subfigures, each panel to the left plots coefficients from a dynamic difference-in-differences specification comparing women whose husbands were in occupations with large WFH shocks to those in occupations with smaller changes. Panels to the right compare husbands directly. The omitted year is 2019. All regressions include state-by-time and husband-occupation fixed effects, and control for wife's age, number of children, and education levels of both spouses. Confidence intervals are shown at the 95% level and based on standard errors clustered at the husband's occupation level.

Childcare in SIPP Using the SIPP childcare topical modules, we are able to provide complementary evidence how household change their sources of childcare and hiring of daycare following shocks on spousal WFH options. We present some results in Table B.34. We estimate our baseline cross-section specification on the 2018-2024 SIPP panel datasets on a battery of childcare related variables. Our results show that in households shocked by the occupation-level WFH treatment, when the child's mother is working, the spouse is 6.4 pp more likely to be involved in childcare, or roughly a 13% increase over the baseline. On the other hand, the woman herself is not any more likely to be involved in childcare.

We also find that households exposed to the WFH shock are significantly less likely to rely on grandparents or other relatives, or on daycare, for day-to-day childcare. Treated households are 7.3 pp less likely to have any paid childcare, and although imprecisely estimated, their total childcare expenditure falls by about 16%. The dynamic DiD estimates in Figure A.11 show clear post-2019 declines in both the probability of paying for childcare and log childcare spending. Overall, these patterns are consistent with fathers becoming more available for childcare in treated households.

Household's Well-being As Table B.33 points out, women in treated households experience no significant change in time spent on personal care activities, such as sleeping and grooming. In contrast, they exhibit a 1.9 pp increase in time devoted to leisure activities, suggesting that the shift in labor supply and the new household specialization decisions do not fully crowd out their discretionary time. This may suggest that women are not worse-off in the sense that they are not taking more responsabilites by crowding out their personal time. For husbands, however, there is a significant reduction in leisure time, amounting to 2.4 percentage points.

Finally, it is plausible that children may be worse off given that our main results suggest that women in our treatment sample are more likely to be working, spend more time at work and, as found in this section, there is an overall reduction in the household's time spent child-caring as a primary activity. Although we cannot fully measure well-being with our current datasets, the SIPP has a handful related variables such as days that parents have dinner or go out with their child and the children's involvement in extracurricular activities. In Table B.35, we estimate our baseline specification on these outcomes, and we find no significant changes for these variables. This evidence suggests that – to the extent

we can measure well-being – children are not worse off by changes in household production and labor supply.

Summary Overall, these results underscore the role of intra-household reallocation in explaining the observed improvements in women's labor market outcomes. Couples seem to be collectively opting for women to reduce their primary childcare time in order to increase their working time, and husbands modestly compensate them as they are spending more time at home. In addition, there is a decline in reliance on extended family or formal daycare services. These changes are not coming at the cost of any worsening the children's or women's well-being from the related variables we can observe.

7 Conclusion

In this paper, we provide novel evidence on the intra-household labor market and household production spillovers induced by large and permanent changes in WFH arrangements of male spouses in dual parent households. Exploiting plausibly exogenous variation in the post-pandemic expansion of remote work across U.S. occupations, we show that increases in husbands' WFH exposure lead to sizable gains in wives' labor market outcomes—higher employment, wages, and hours worked, and a lower likelihood of part-time work. These effects are concentrated among households with children of child-rearing age and are not driven by changes in group composition, assortative matching, and are widely robust to specification changes and several falsification test. Our analysis of time use and childcare arrangements indicates that the increase in women's labor supply is partly facilitated by an intra-household reallocation of time. In particular, we find evidence of modest substitution in childcare responsibilities between spouses: women reduce the time they devote to primary childcare, while husbands slightly increase their involvement.

We highlight the importance of household-level dynamics when evaluating the labor market implications of job amenities like WFH. Our findings suggest that access to remote work arrangements for one partner can alleviate time constraints for the other, with implications for gender equality in labor force attachment and earnings. In future research, we hope to explore larger datasets covering the universe of U.S. households to better disentangle mechanisms underlying our results and follow-up on career progression for women entering the

labor force.

References

Aksoy, C. G., J. M. Barrero, N. Bloom, S. J. Davis, M. Dolls, and P. Zarate (2022, September). Working from Home Around the World. *Brookings Papers on Economic Activity 2022*(2), 281–360.

Anderson, D. J., M. Binder, and K. Krause (2002). The motherhood wage penalty: Which mothers pay it and why? *American economic review 92*(2), 354–358.

Angelici, M. and P. Profeta (2024, March). Smart Working: Work Flexibility Without Constraints. *Management Science 70*(3), 1680–1705.

Barrero, J. M., N. Bloom, and S. J. Davis (2023, November). The Evolution of Work from Home. *Journal of Economic Perspectives 37*(4), 23–49.

Bertrand, M., C. Goldin, and L. F. Katz (2010, July). Dynamics of the Gender Gap for Young Professionals in the Financial and Corporate Sectors. *American Economic Journal: Applied Economics 2*(3), 228–255.

Bloom, N., R. Han, and J. Liang (2022, July). How Hybrid Working From Home Works Out. Technical Report w30292, National Bureau of Economic Research, Cambridge, MA.

Bloom, N., J. Liang, J. Roberts, and Z. J. Ying (2015, February). Does Working from Home Work? Evidence from a Chinese Experiment. *The Quarterly Journal of Economics 130*(1), 165–218.

Buckman, S. R., J. M. Barrero, N. Bloom, and S. J. Davis (2025). Measuring work from home. Technical report, National Bureau of Economic Research.

Canaan, S. (2022, April). Parental leave, household specialization and children's well-being. *Labour Economics 75*, 102127.

Chiappori, P.-A. (1992, June). Collective Labor Supply and Welfare. *Journal of Political Economy 100*(3), 437–467.

Choudhury, P., T. Khanna, C. A. Makridis, and K. Schirmann (2024). Is hybrid work the best of both worlds? evidence from a field experiment. *Review of Economics and Statistics*, 1–24.

DiNardo, J., N. Fortin, and T. Lemieux (1995). Labor market institutions and the distribution of wages, 1973-1992: A semiparametric approach.

Dingel, J. I. and B. Neiman (2020, September). How many jobs can be done at home? *Journal of Public Economics 189*, 104235.

Ejrnæs, M. and A. Kunze (2013). Work and wage dynamics around childbirth. *The Scandinavian Journal of Economics 115*(3), 856–877.

Flood, S., M. King, R. Rodgers, S. Ruggles, J. R. Warren, D. Backman, A. Chen, G. Cooper, S. Richards, M. Schouweiler, and M. Westberry (2024). IPUMS CPS: Version 12.0 [dataset].

Flood, S. M., L. C. Sayer, D. Backman, and A. Chen (2023). American Time Use Survey Data Extract Builder: Version 3.2 [dataset].

Ginja, R., J. Jans, and A. Karimi (2020). Parental Leave Benefits, Household Labor Supply, and Children's Long-Run Outcomes. *Journal of Labor Economics 38*(1), 261–320.

Ginja, R., A. Karimi, and P. Xiao (2023, January). Employer Responses to Family Leave Programs. *American Economic Journal: Applied Economics 15*(1), 107–135.

Goldin, C. (2014, April). A Grand Gender Convergence: Its Last Chapter. *American Economic Review 104*(4), 1091–1119.

Goldin, C. (2022). Understanding the economic impact of covid-19 on women. *Brookings Papers on Economic Activity 2022*(1), 65–139.

Hansen, S., P. J. Lambert, N. Bloom, S. J. Davis, R. Sadun, and B. Taska (2023). Remote work across jobs, companies, and space.

Harrington, E. and M. E. Kahn (2023). Has the rise of work-from-home reduced the motherhood penalty in the labor market.

Heggeness, M., P. Suri, et al. (2021). Telework, childcare, and mothers' labor supply.

Jalota, S. and L. Ho (2024). What works for her? how work-from-home jobs affect female labor force participation in urban india.

Kleven, H. (2022). The geography of child penalties and gender norms: Evidence from the united states.

Kleven, H. (2024). The eitc and the extensive margin: A reappraisal. *Journal of Public Economics 236*, 105135.

Kleven, H., C. Landais, and G. Leite-Mariante (2024, September). The Child Penalty Atlas. *The Review of Economic Studies*.

Le Barbanchon, T., R. Rathelot, and A. Roulet (2021). Gender differences in job search: Trading off commute against wage. *The Quarterly Journal of Economics 136*(1), 381–426.

Mas, A. and A. Pallais (2017, December). Valuing Alternative Work Arrangements. *American Economic Review 107*(12), 3722–3759.

Mas, A. and A. Pallais (2020). Alternative Work Arrangements. *Annual Review of Economics*.

Olivetti, C. and B. Petrongolo (2016). The evolution of gender gaps in industrialized countries. *Annual review of Economics 8*(1), 405–434.

Persson, P. and M. Rossin-Slater (2024, October). When Dad Can Stay Home: Fathers' Workplace Flexibility and Maternal Health. *American Economic Journal: Applied Economics 16*(4), 186–219.

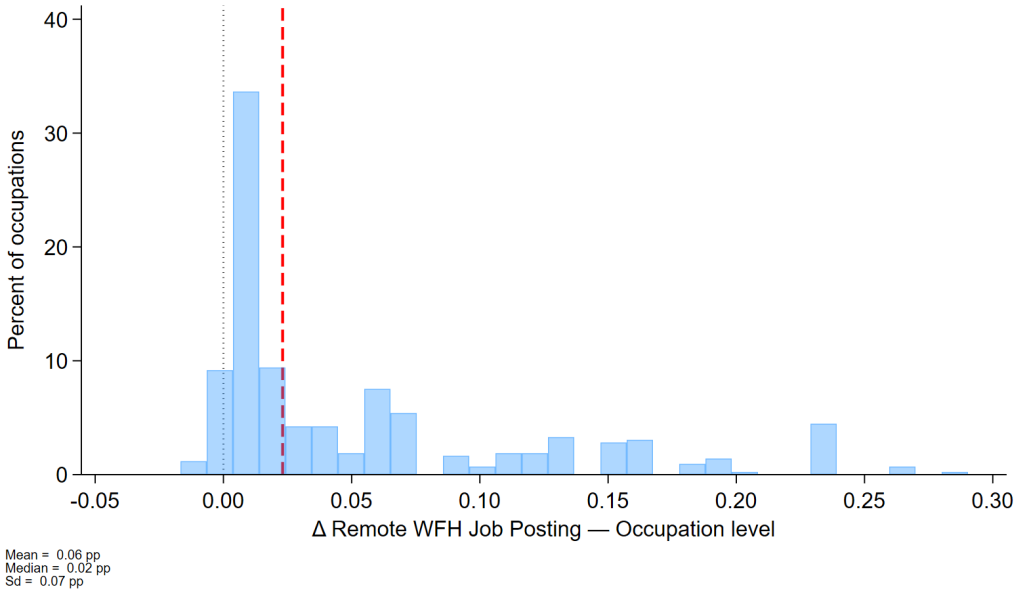
Ruggles, S., S. Flood, M. Sobek, D. Backman, G. Cooper, J. A. R. Drew, S. Richards, R. Rodgers, J. Schroeder, and K. C. Williams (2025). IPUMS USA: Version 16.0 [dataset].

Waldfogel, J. (1998). Understanding the 'family gap'in pay for women with children. *Journal of economic Perspectives 12*(1), 137–156.

Appendix

A Figures

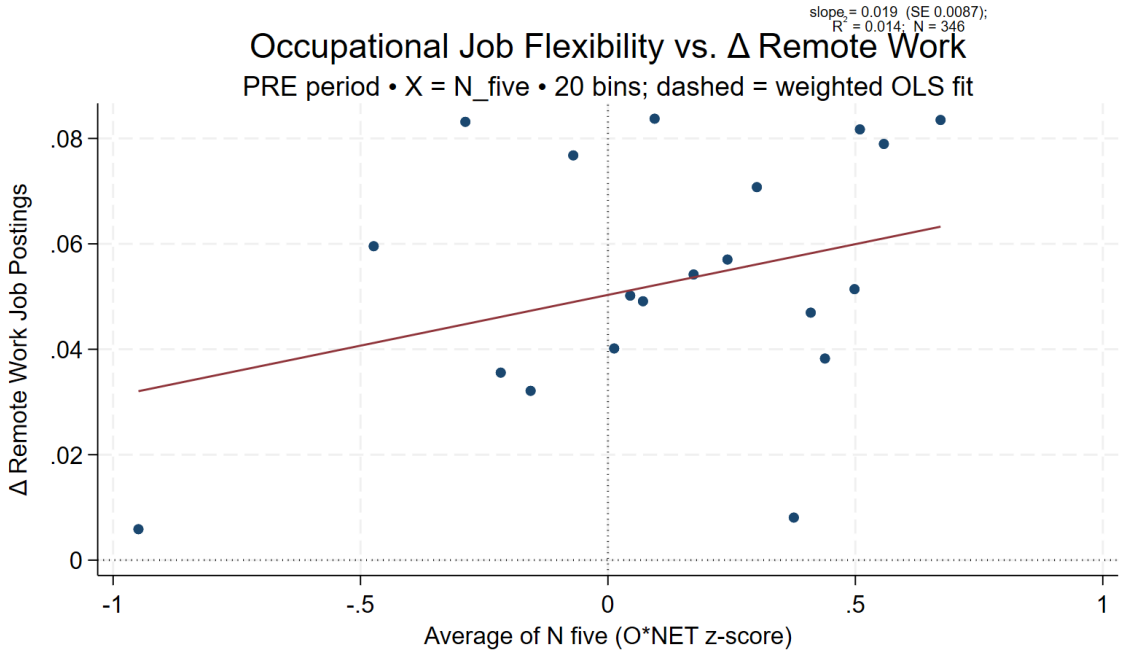
Figure A.1: Distribution of WFH Exposure Using Job Postings Data



Notes: Author's elaboration using WFH job postings data from Hansen et al. (2023). The figure shows the distribution of the difference in the share of hybrid and remote-work job postings between the post-pandemic period (2023–2024) and the pre-pandemic period (2019). In the paper, we define occupations with a WFH probability change at or above the median are classified as treated, and those below the median as control. We convert occupation codes from the original SOC 3-digit classification used in the job postings data to the occupation codes used in the ACS.

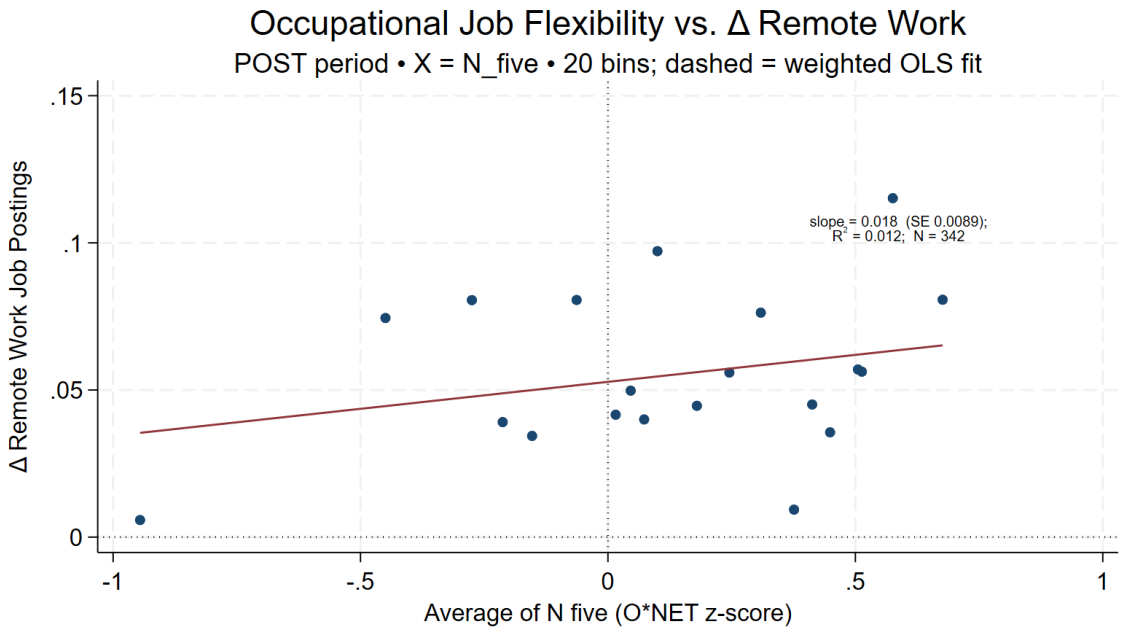
Figure A.2: Average of 5 Normalized Job Characteristics (O*Net)

(a) Pre



Occupations weighted by number of men in that occupation (period=pre).

(b) Post

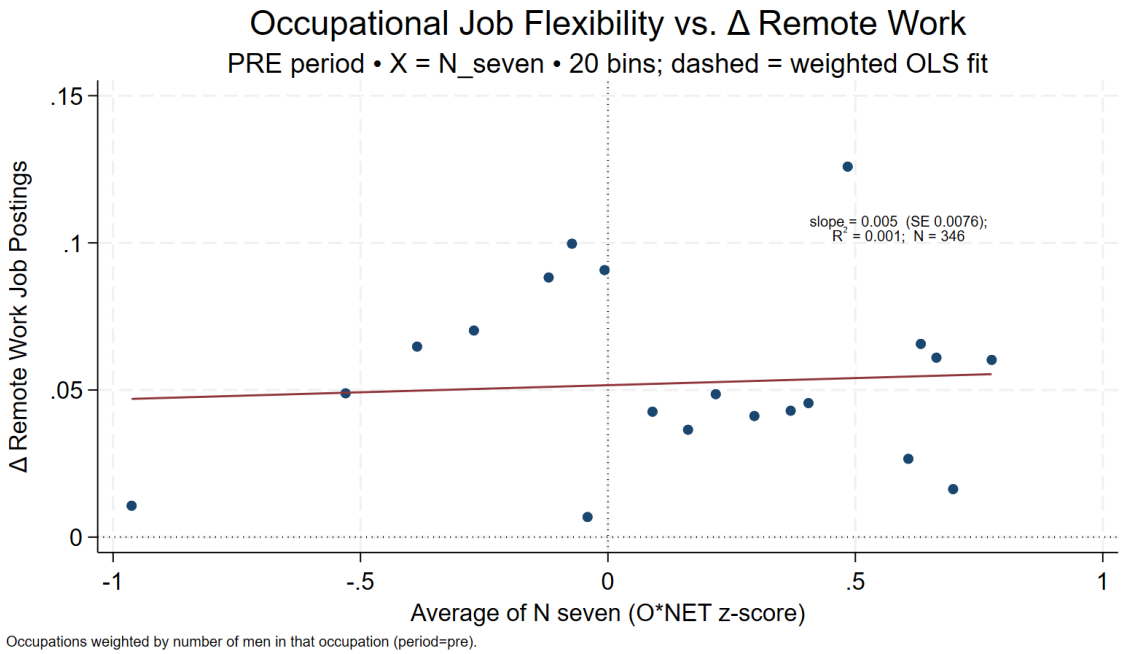


Occupations weighted by number of men in that occupation (period=post).

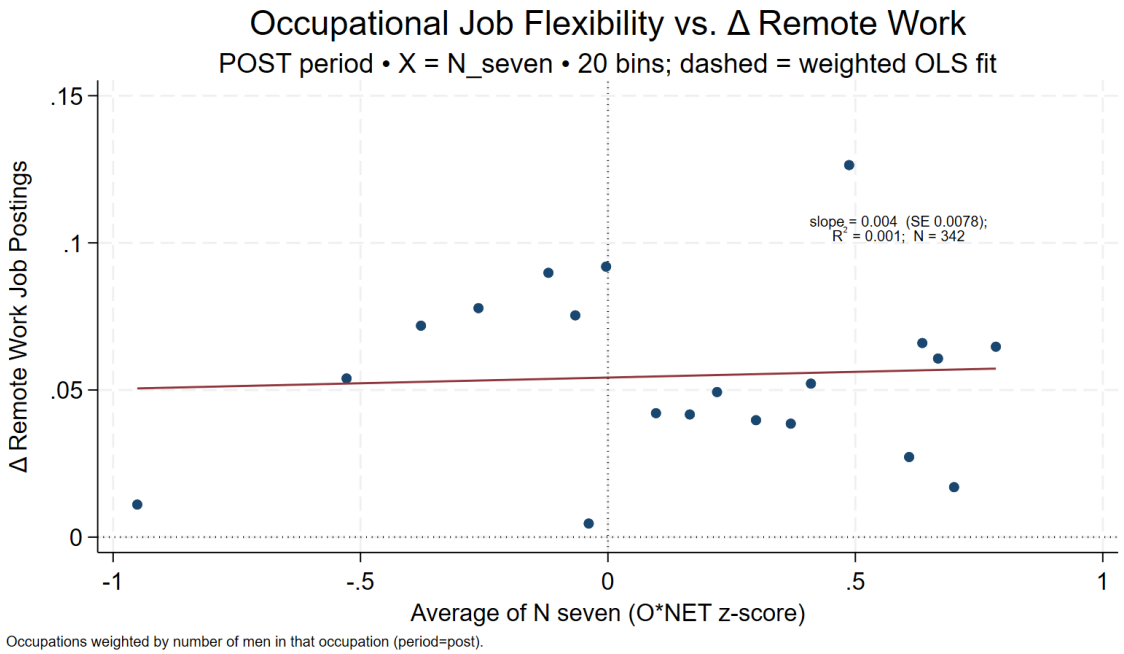
Notes: This figure plots the average of five normalized job characteristics from O*NET 2025 across Census 2010 occupations, using ACS male employment weights. The index—originally proposed by Goldin (2014)—includes *Time pressure*, *Contact with others*, *Establishing and maintaining interpersonal relationships*, *Structured work*, and *Freedom to make decisions*. Higher values indicate greater job inflexibility. Panel (a) uses pre-Covid weights (ACS 2019), while Panel (b) uses post-Covid weights (ACS 2023). The relationship between treatment intensity and this composite measure is relatively flat.

Figure A.3: Average of 7 Normalized Job Characteristics (O*Net)

(a) Pre

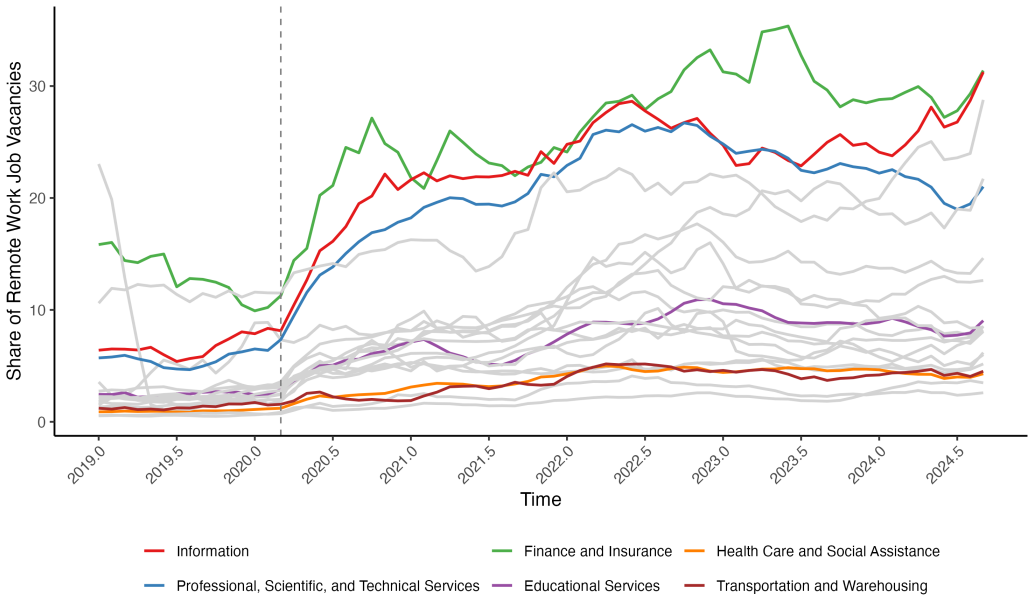


(b) Post



Notes: This figure extends the five-dimension index by adding two additional O*NET characteristics—*Face-to-face discussions* and *Frequency of decision making*—to construct a seven-dimension measure of job inflexibility, following Goldin (2014). Higher values indicate more structured, less substitutable jobs. Values are weighted by ACS male employment at the occupation level. Panel (a) uses 2019 weights (pre-pandemic), while Panel (b) uses 2023 weights. As with the five-variable index, treatment status is not systematically related to this flexibility measure.

Figure A.4: Evolution of WFH by industry (2 digits).



Notes: Author's elaboration using WFH job postings data from Hansen et al. (2023). Classification based on NAICS 2022 2-digits.

Figure A.5: Adjusted Labor Market Trends by Treatment Group

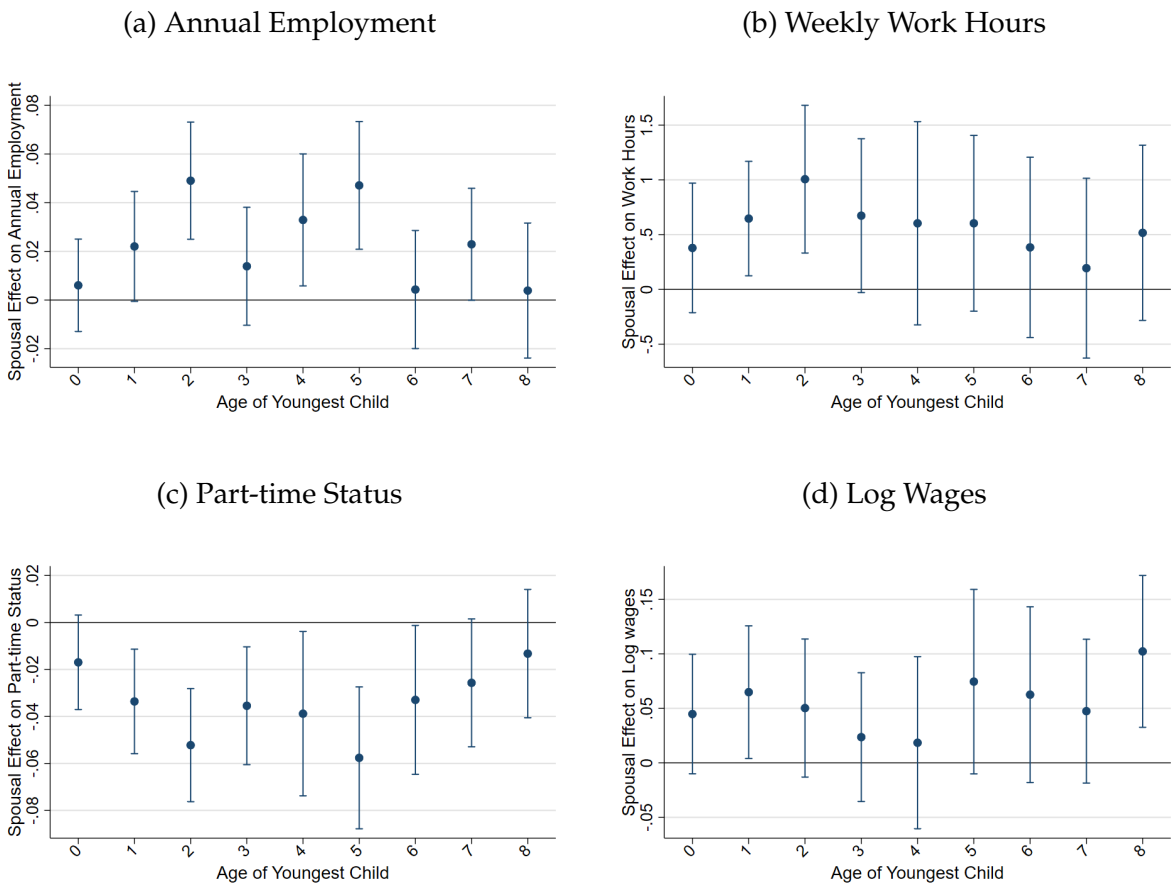


Notes: Each panel plots adjusted yearly trends in labor market outcomes for treated (blue line) and control (red line) groups. These estimates are the predicted values from separate regressions by group:

$$y_{i(H),s,t} = f_s + f_{o(H)} + \sum_{\substack{y=2017 \\ y\neq 2019}}^{2023} \beta^y \cdot 1(\text{year}(t) = y) + X_{i(H),t}\delta + \epsilon_{i(H),s,t},$$

where outcomes are residualized on state and husband-occupation fixed effects and controls for wife's age, number of children, and both spouses' education. The omitted year is 2019.

Figure A.6: Effect of Husbands' WFH Shock on Wives' Labor Market Outcomes, by Age of Youngest Child



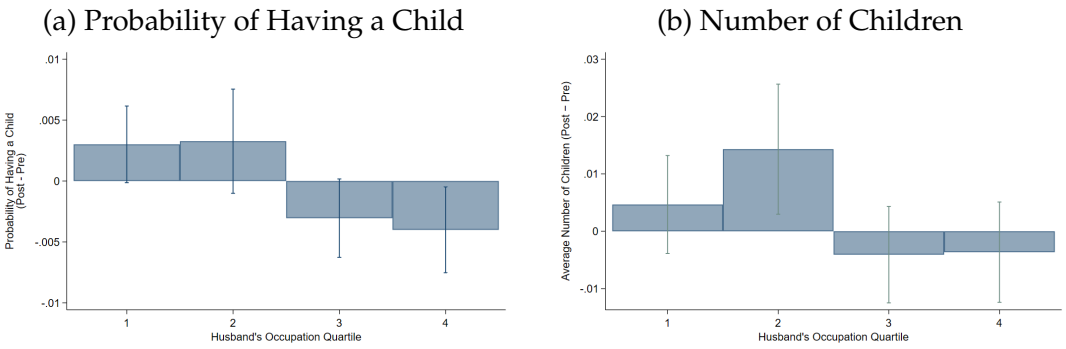
Notes: In each panel we estimate the baseline specification on 8 different subsamples of the ACS by age of the household's youngest child.

Figure A.7: Assortative Matching by Spousal WFH Exposure, Pre- and Post-COVID



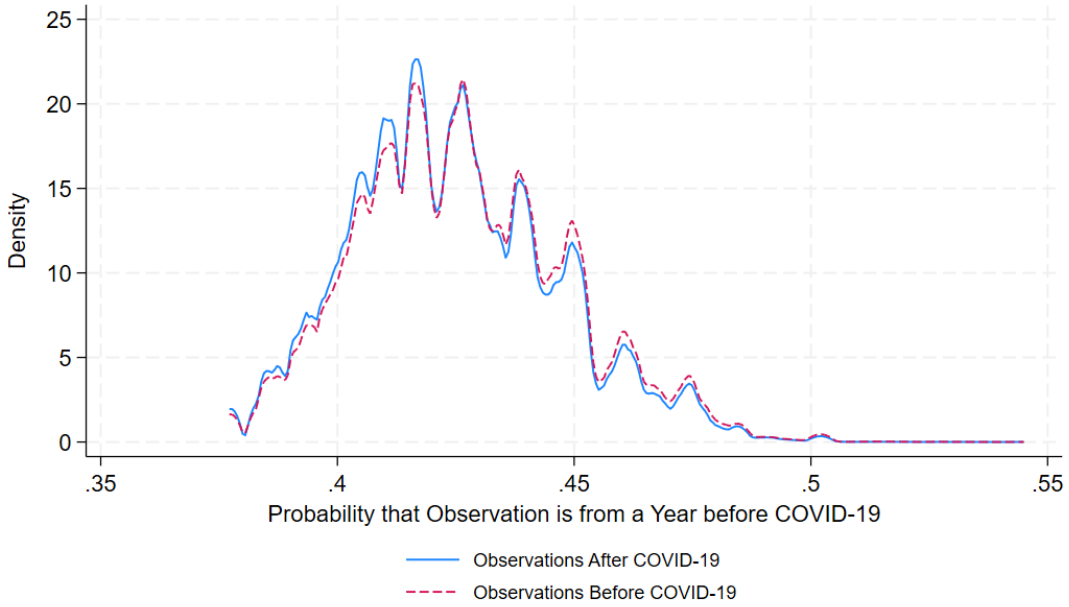
Notes: Each panel shows the distribution of wives across treatment quartiles conditional on their husband's occupation being in a given WFH shock quartile, separately for the pre-COVID (panel a) and post-COVID (panel b) periods, using pooled ACS data from 2017 to 2023 with children of child-rearing age. Quartiles are based on the occupation-level change in WFH probability described in Subsection 2.3. The figure is designed to assess whether assortative matching by WFH exposure changed differentially following the pandemic.

Figure A.8: Changes in Fertility Outcomes by Husbands' WFH Shock



Notes: Each panel plots the change in fertility-related outcomes following the onset of COVID-19 across quartiles of the husband's WFH exposure, defined by the occupation-level change in WFH probability as described in Subsection 2.3. Panel (a) displays the probability of having a child; Panel (b) reports the average number of children in the household. The figure uses pooled ACS data of dual households from 2017 to 2023 and is intended to assess whether fertility patterns shifted differentially across exposure groups in the post-pandemic period.

Figure A.9: Kernel Density Plot Showing Common Support for Reweighting



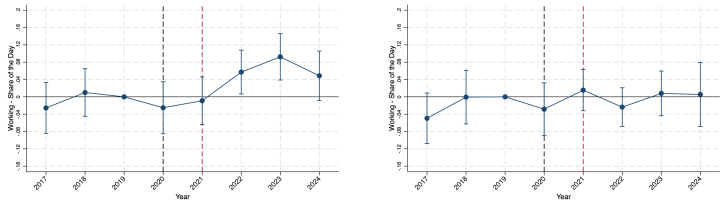
Notes: This figure uses the 2017–2023 ACS sample of dual-parent households with children of child-rearing age. It plots the estimated probability that an observation is from the pre-COVID-19 period, obtained from a logit model on a set of covariates including spouse occupation, wife's age and education, and state. The predicted values are derived from separate regressions by group:

$$\Pr(\text{Pre-COVID}) = f_s + f_{o(H)} + f_e + f_a + \varepsilon,$$

where f_s, $f_{o(H)}$, f_e, and f_a represent fixed effects for state, husband's occupation, wife's education, and age bin, respectively. The densities indicate good overlap in covariate distributions before and after COVID, supporting the use of reweighting strategies such as DiNardo-Fortin-Lemieux (DiNardo et al. 1995) and inverse-propensity score weighting.

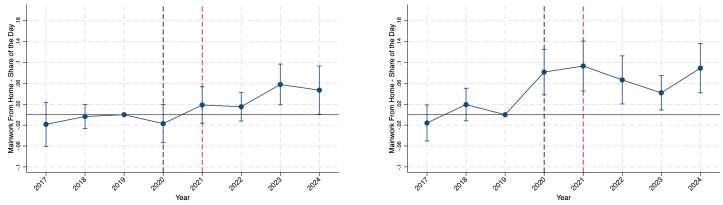
Figure A.10: Dynamic Effect of Husbands' WFH Shock on Share of the Day Spent in Other Labor and Household Production, by Year

(a) Working (ATUS)



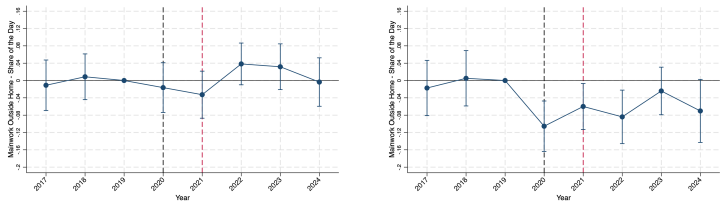
Women *Men*

(b) Mainwork From Home (ATUS)



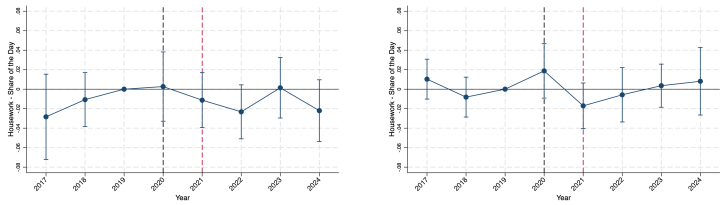
Women *Men*

(c) Mainwork Outside Home (ATUS)



Women *Men*

(a) Housework (ATUS)

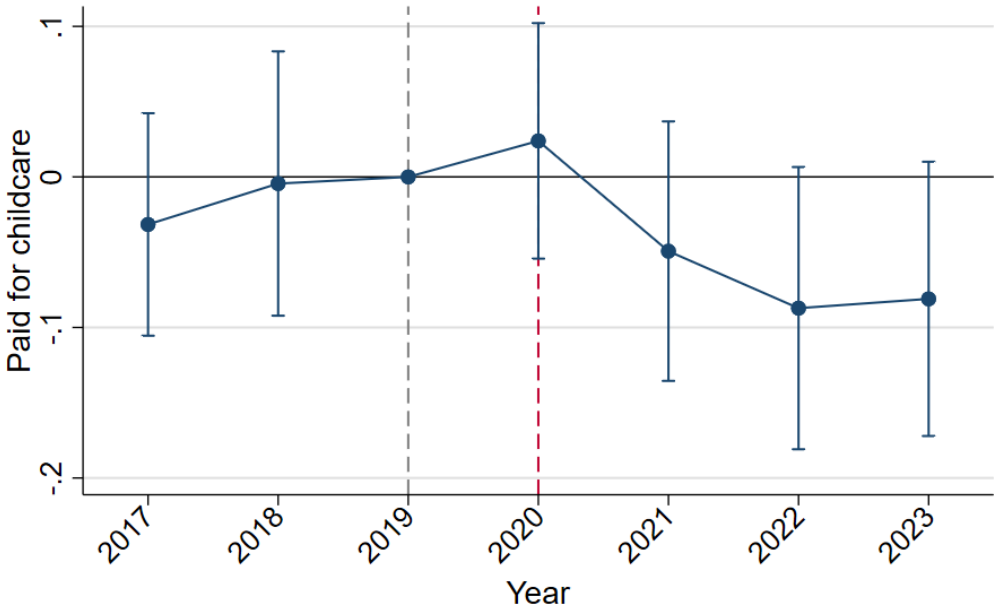


Women *Men*

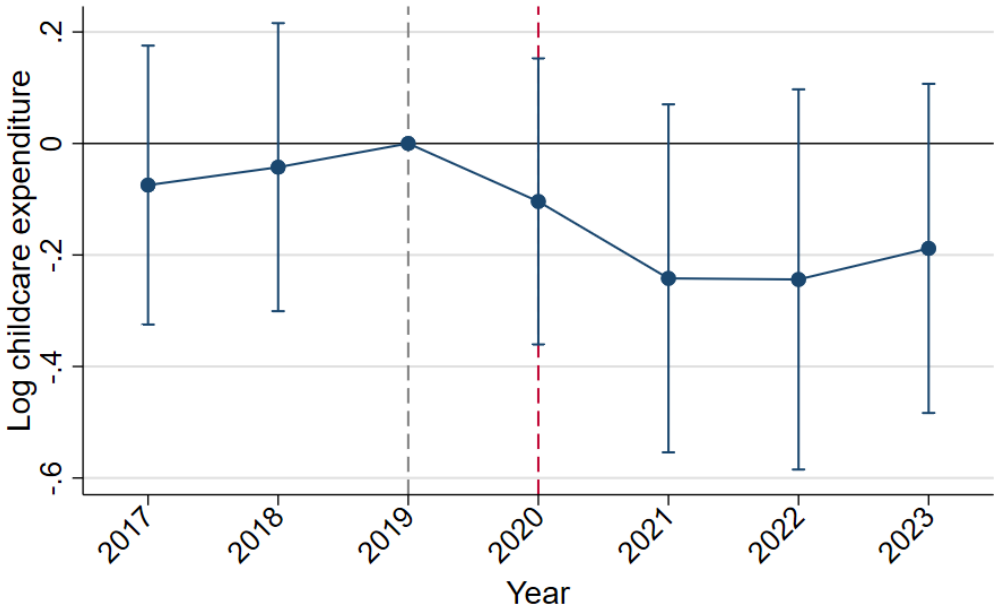
Notes: Each panel from the first column plots coefficients from a dynamic difference-in-differences specification comparing women whose husbands were in occupations with large WFH shocks to those in occupations with smaller changes. Those in the second column compare husbands directly. The omitted year is 2019. All regressions include state-by-time and husband-occupation fixed effects, and control for wife's age, number of children, and education levels of both spouses. Confidence intervals are shown at the 95% level and based on standard errors clustered at the husband's occupation level.

Figure A.11: Effect of Husband's WFH Shock on Daycare Hiring (SIPP)

(a) Paid for Daycare



(b) Log Daycare Expenditure



Notes: Each panel plots coefficients from Equation 2 using the 2018-2024 SIPP dataset. The omitted year is 2019. All regressions include state-by-time and husband-occupation fixed effects, and control for wife's age, number of children, and education levels of both spouses. Confidence intervals are shown at the 95% level and are based on standard errors clustered at the husband-occupation level.

B Tables

Table B.1: Top 15 ACS Occupations by Treatment Status

Treated Occupations	Control Occupations
Managers, nec (including Postmasters) (9.0%)	First-Line Supervisors of Sales Workers (6.4%)
Software Developers, Applications and Systems Software (5.6%)	Driver/Sales Workers and Truck Drivers (6.4%)
Computer Scientists and Systems Analysts/Network systems Analysts/Web Developers (3.9%)	Construction Laborers (3.4%)
Police Officers and Detectives (3.4%)	Elementary and Middle School Teachers (3.0%)
Chief executives and legislators/public administration (2.8%)	Physicians and Surgeons (2.8%)
Lawyers, and judges, magistrates, and other judicial workers (2.5%)	Electricians (2.7%)
General and Operations Managers (2.4%)	Laborers and Freight, Stock, and Material Movers, Hand (2.6%)
Sales Representatives, Wholesale and Manufacturing (2.4%)	Carpenters (2.4%)
Financial Managers (2.3%)	Retail Salespersons (2.4%)
Managers in Marketing, Advertising, and Public Relations (2.2%)	Secondary School Teachers (2.3%)
Constructions Managers (2.2%)	Postsecondary Teachers (2.3%)
Accountants and Auditors (2.1%)	Chefs and Cooks (2.3%)
Engineers, nec (1.8%)	Other production workers including semiconductor processors and cooling and freezing equipment operators (2.1%)
First-Line Supervisors of Production and Operating Workers (1.7%)	First-Line Supervisors of Construction Trades and Extraction Workers (2.1%)
Computer and Information Systems Managers (1.7%)	Janitors and Building Cleaners (2.0%)

Notes: This table lists the 15 most common occupations in the treated and control groups, respectively, based on the classification variable `occ2010` from the ACS. The treatment dummy is assigned at the occupation level, as described in Subsection 2.3. An occupation is classified as treated if the change in the share of remote/hybrid job postings before and after COVID-19, as measured using data from Hansen et al. (2023), is above the median across all occupations. Frequencies are expressed as percentages of the total number of observations within each group. Occupation labels correspond to the value labels associated with `occ2010`.

Table B.2: Difference between treated and control occupations using Occupational Employment Statistics (OES) 2019

	Treated	Control	Difference	S.E.	N
Panel A: Total Employment and Annual Wage					
Total Employment	349,041.91	585,176.13	-236,134.23	166,736.68	344
Mean annual wage	83,621.97	51,115.15	32,506.82***	8,439.12	343
Annual 10th percentile wage	42,130.15	29,890.22	12,239.93***	3,447.29	343
Annual 25th percentile wage	55,385.42	37,056.22	18,329.20***	5,247.30	343
Annual median wage (50th percentile)	75,556.84	47,040.04	28,516.80***	7,940.83	340
Annual 75th percentile wage	94,046.61	61,363.60	32,683.01***	8,254.64	338
Annual 90th percentile wage	108,187.12	78,071.17	30,115.95***	8,415.95	329
Panel B: Hourly Wage					
Mean hourly wage	40.39	23.87	16.53***	4.19	336
Hourly 10th percentile wage	20.60	14.04	6.57***	1.69	336
Hourly 25th percentile wage	27.14	17.36	9.78***	2.57	336
Hourly median wage (50th percentile)	36.88	21.99	14.89***	3.92	333
Hourly 75th percentile wage	45.58	28.59	16.99***	4.12	332
Hourly 90th percentile wage	52.25	36.45	15.79***	4.21	324

Notes: Balance table comparing treated and control occupations. All statistics are weighted by ACS employment counts at the Census 2010 occupation level, using husbands' occupations from survey year 2019. OES variables are reported at SOC 2018 (six-digit) in the source and were aggregated to Census 2010 occupations using employment-share weights; the OES reference period is May 2019. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.3: Difference between treated and control occupations using ACS 2019

	Treated	Control	Difference	S.E.	N
Panel A: Occupational Characteristics (ACS)					
Share of Women	0.41	0.40	0.01***	0.00	182,286
Age (mean)	37.06	35.81	1.25***	0.04	182,286
Share of Below Any College	0.10	0.37	-0.27***	0.00	182,286
Share of Below Any High-School	0.02	0.11	-0.10***	0.00	182,286
Share of College Graduates	0.68	0.32	0.35***	0.00	182,286
Potential Years of Education	15.13	13.80	1.33***	0.01	182,286
Potential Experience	15.93	16.01	-0.09**	0.04	182,286
Wage	57,146.44	35,772.07	21,374.38***	279.23	182,286
Share Working Part-Time	0.13	0.15	-0.02***	0.00	182,286
Usual Weekly Work Hours	41.49	40.91	0.58***	0.07	182,286

Notes: Balance table comparing treated and control occupations at the Census 2010 occupation level, using the ACS analysis sample for survey year 2019. All statistics are weighted by survey weights. The reference year is 2019 for all variables except wages, which refer to earnings in the previous year. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.4: Difference Between Treated and Control Occupations using O*NET 2025

	Treated	Control	Difference	S.E.	N
Panel A: Work Context					
Time Pressure	0.02	0.27	-0.25	0.19	342
Frequency of Conflict Situations	0.59	-0.02	0.61***	0.21	342
Contact with Others	0.44	-0.00	0.44**	0.22	342
Coordinate or Lead Others	0.67	-0.05	0.71**	0.30	342
Degree of Automation	0.18	-0.36	0.55***	0.15	342
Structured Work	-0.55	0.19	-0.74***	0.18	342
Duration of Typical Work Week	0.51	0.24	0.27	0.22	342
Face-to-Face Discussions	0.32	0.10	0.22	0.19	342
Freedom to Make Decisions	0.44	-0.22	0.66***	0.18	342
Frequency of Decision Making	0.28	0.25	0.03	0.19	342
Impact of Decisions on Coworkers or Company Results	0.40	0.13	0.27	0.20	342
Level of Competition	0.40	-0.10	0.50***	0.18	342
Physical Proximity	-0.25	0.20	-0.45**	0.23	342
Regular Work Schedule	-0.32	0.12	-0.44**	0.20	342
Work with a Workgroup or Team	0.48	-0.30	0.78***	0.26	342
Panel B: Work Activities					
Communicating with Supervisors, Peers, or Subordinates	0.74	-0.28	1.02***	0.19	342
Coordinating the Work and Activities of Others	0.70	-0.22	0.92***	0.23	336
Developing and Building Teams	0.75	-0.16	0.91***	0.23	338
Establishing and Maintaining Interpersonal Relationships	0.74	-0.23	0.98***	0.18	342
*Panel C: O*Net Indexes (Goldin, 2014)*					
Avg. of 5 normalized characteristics	0.22	-0.00	0.22***	0.08	342
Avg. of 7 normalized characteristics	0.24	0.05	0.19*	0.10	342

Notes: Balance table comparing treated and control occupations at the Census 2010 occupation level, using the ACS analysis sample restricted to survey year 2019, matched to the most recent O*NET occupational characteristics (2025). All statistics are weighted by ACS 2019 male employment counts at the Census 2010 occupation level. All O*NET variables are standardized to z-scores (demeaned and divided by the cross-occupation standard deviation). The O*NET indices of Goldin (2014) are constructed as the average of the normalized dimensions *Time pressure*, *Contact with others*, *Establishing and maintaining interpersonal relationships*, *Structured work*, and *Freedom to make decisions* (five-characteristic index); the seven-characteristic index additionally includes *Face-to-face discussions* and *Frequency of decision making*. Higher values of the indices indicate lower worker substitutability and greater job inflexibility, following Goldin (2014).
Measurement note: The current "Structured work" item differs from that used in the original period due to O*NET wording changes: the present question asks "How much freedom does the worker have in determining the tasks, priorities, or goals of the job?". We use this item and take its complement so that higher values correspond to more structure, aligning with the original construct.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.5: Descriptive Statistics of Main Labor Market Outcomes, by Treatment Group and Pre/Post-COVID Period

	Pre-COVID			Post-COVID		
	Mean	SD	N	Mean	SD	N
Panel A: Control Sample						
Spouse Employed (Last Week)	0.97	0.17	152,710	0.96	0.19	179,033
Spouse LFP (Last Week)	0.99	0.11	152,710	0.98	0.13	179,033
Spouse Usual Weekly Hours Worked (Last Year)	43.98	10.76	152,710	43.43	11.02	179,033
Spouse Part-time Status (Last Year)	0.07	0.25	152,710	0.07	0.26	179,033
Spouse Employed (Last Year)	1.00	0.00	152,710	1.00	0.00	179,033
Spouse Weekly Log Wage (Last Year)	6.51	0.77	152,710	6.52	0.82	179,033
Spouse Log Wage	10.26	0.84	152,710	10.25	0.88	179,033
Wife Employed (Last Week)	0.65	0.48	152,710	0.66	0.47	179,033
Wife LFP (Last Week)	0.67	0.47	152,710	0.69	0.46	179,033
Wife Usual Weekly Hours Worked (Last Year)	36.08	11.40	152,710	36.12	11.66	179,033
Wife Part-time Status (Last Year)	0.50	0.50	152,710	0.48	0.50	179,033
Wife Employed (Last Year)	0.67	0.47	152,710	0.68	0.46	179,033
Wife Weekly Log Wage (Last Year)	6.25	0.95	152,710	6.29	0.97	179,033
Wife Log Wage	9.77	1.13	152,710	9.81	1.16	179,033
HH Wage	67121.00	57388.74	152,710	68642.56	58769.98	179,033
Panel B: Treated Sample						
Spouse Employed (Last Week)	0.99	0.12	171,168	0.98	0.14	226,211
Spouse LFP (Last Week)	0.99	0.08	171,168	0.99	0.09	226,211
Spouse Usual Weekly Hours Worked (Last Year)	45.10	9.49	171,168	44.24	9.63	226,211
Spouse Part-time Status (Last Year)	0.03	0.17	171,168	0.03	0.18	226,211
Spouse Employed (Last Year)	1.00	0.00	171,168	1.00	0.00	226,211
Spouse Weekly Log Wage (Last Year)	7.06	0.74	171,168	7.06	0.78	226,211
Spouse Log Wage	10.84	0.78	171,168	10.83	0.82	226,211
Wife Employed (Last Week)	0.68	0.46	171,168	0.70	0.46	226,211
Wife LFP (Last Week)	0.70	0.46	171,168	0.72	0.45	226,211
Wife Usual Weekly Hours Worked (Last Year)	36.08	12.38	171,168	36.58	11.95	226,211
Wife Part-time Status (Last Year)	0.47	0.50	171,168	0.44	0.50	226,211
Wife Employed (Last Year)	0.70	0.46	171,168	0.72	0.45	226,211
Wife Weekly Log Wage (Last Year)	6.60	0.93	171,168	6.63	0.95	226,211
Wife Log Wage	10.11	1.18	171,168	10.16	1.17	226,211
HH Wage	103657.87	77572.03	171,168	106307.19	81648.96	226,211

Notes: This table reports descriptive statistics for key labor market outcomes across treated and control households, before and after the COVID-19 pandemic. The sample corresponds to the pooled 2017–2023 CPS basic monthly files, restricted to dual-parent households with children of child-rearing age. All estimates are computed using the CPS sampling weights.

Table B.6: Descriptive Statistics of Main Control Variables, by Treatment Group and Pre/Post-COVID Period

	Pre-COVID			Post-COVID		
	Mean	SD	N	Mean	SD	N
Panel A: Control Sample						
Spouse - Below HS Level	0.14	0.35	152,710	0.13	0.34	179,033
Spouse - HS Level	0.30	0.46	152,710	0.30	0.46	179,033
Spouse - Above HS Level	0.56	0.50	152,710	0.57	0.50	179,033
Spouse Age	36.68	7.03	152,710	37.02	7.00	179,033
Wife - Below HS Level	0.11	0.32	152,710	0.09	0.29	179,033
Wife - HS Level	0.21	0.41	152,710	0.21	0.41	179,033
Wife - Above HS Level	0.67	0.47	152,710	0.70	0.46	179,033
Wife Age	34.26	6.06	152,710	34.61	6.00	179,033
Number of own children in the household	2.16	0.92	152,710	2.15	0.92	179,033
Age of Youngest Child	3.42	2.57	152,710	3.44	2.58	179,033
Age of Oldest Child	8.02	5.29	152,710	8.00	5.36	179,033
Youngest Child 0-4	0.65	0.48	152,710	0.64	0.48	179,033
Youngest Child 5-8	0.35	0.48	152,710	0.36	0.48	179,033
Panel B: Treated Sample						
Spouse - Below HS Level	0.02	0.13	171,168	0.02	0.13	226,211
Spouse - HS Level	0.09	0.29	171,168	0.09	0.29	226,211
Spouse - Above HS Level	0.89	0.31	171,168	0.89	0.31	226,211
Spouse Age	37.83	6.31	171,168	38.12	6.19	226,211
Wife - Below HS Level	0.02	0.15	171,168	0.02	0.15	226,211
Wife - HS Level	0.09	0.28	171,168	0.09	0.28	226,211
Wife - Above HS Level	0.89	0.31	171,168	0.89	0.31	226,211
Wife Age	35.72	5.51	171,168	36.07	5.41	226,211
Number of own children in the household	2.02	0.86	171,168	2.02	0.86	226,211
Age of Youngest Child	3.35	2.58	171,168	3.40	2.59	226,211
Age of Oldest Child	6.92	4.75	171,168	6.97	4.79	226,211
Youngest Child 0-4	0.66	0.47	171,168	0.65	0.48	226,211
Youngest Child 5-8	0.34	0.47	171,168	0.35	0.48	226,211

Notes: This table presents summary statistics for key household- and individual-level control variables across treated and control groups, before and after the COVID-19 pandemic. The statistics are based on the pooled 2017–2023 ACS analysis sample, restricted to dual-parent households with children of child-rearing age. All estimates are computed using the CPS sampling weights.

Table B.7: Effect of Husbands' WFH Shock on Their Own Labor Market Outcomes

	Log Weekly Wage	Log Wage	Weekly Work Hours	Part-time Status
$Exposed_{o(h(i))} \times Post_t$	0.030***	-0.005	-0.590***	-0.004*
	(0.010)	(0.008)	(0.130)	(0.002)
Mean pre-treatment	6.447	10.588	44.740	0.046
Obs	514,151	729,120	729,120	729,120
R^2	0.18	0.34	0.09	0.06

Notes: We estimate Equation (1) on the ACS 2017-2023 sample on male spouse intensive margin labor supply outcomes.

Table B.8: Heterogeneous Effects on Women's Labor Market Outcomes by Quartile of Husbands' WFH Shock

	Employment	Log Weekly Wage	Log Wage	Usual Weekly Hours Worked	Part-time Status
$Exposed_{o(h(i))}(Quartile2) \times Post_t$	0.007	0.011	0.023	0.206	-0.008
	(0.007)	(0.016)	(0.018)	(0.168)	(0.009)
$Exposed_{o(h(i))}(Quartile3) \times Post_t$	0.019***	0.028**	0.051***	0.544***	-0.033***
	(0.006)	(0.014)	(0.016)	(0.145)	(0.007)
$Exposed_{o(h(i))}(Quartile4) \times Post_t$	0.034***	0.036***	0.067***	0.754***	-0.044***
	(0.008)	(0.013)	(0.015)	(0.152)	(0.007)
Mean pre-treatment	0.702	6.460	9.969	35.927	0.476
Obs	729,120	514,151	514,151	539,181	729,120
R^2	0.08	0.18	0.15	0.03	0.08

Notes: This table reports estimates from Equation (1), where treatment intensity is captured by quartiles of the husband's occupation-level change in WFH probability, as defined in Subsection 2.3. The sample is the 2017–2023 ACS sample of dual-parent households with children of child-rearing age. The baseline (omitted) category is the bottom quartile of WFH exposure. For simplicity we only present coefficients for the interactions of the treatment and post-Covid time dummies. All regressions control for the wife's age, education, and number of children, as well as the husband's education. We include state-by-time and husband-occupation fixed effects. Standard errors are clustered at the occupation level.

Table B.9: Effect of Husbands' Industry-Level WFH Shock on Wives' Labor Market Outcomes

	Employment	Log Weekly Wage	Log Wage	Usual Weekly Hours Worked	Part-time Status
$Exposed_{n(h(i))} \times Post_t$	0.013**	0.004	0.010	0.117	-0.009*
	(0.006)	(0.011)	(0.014)	(0.131)	(0.005)
$Exposed_{n(h(i))} \times Post_t \times During_t$	-0.005	0.009	0.020	0.188	-0.006
	(0.004)	(0.011)	(0.013)	(0.119)	(0.004)
Mean pre-treatment	0.692	6.439	9.950	35.993	0.481
$\beta_1 + \beta_2 = 0$ (p-val)	0.036	0.061	0.000	0.003	0.000
Obs	758,105	530,048	530,048	559,195	758,105
R^2	0.08	0.17	0.14	0.03	0.08

Notes: This table reports estimates from Equation (1), using the 2017–2023 ACS sample of dual-parent households with children of child-rearing age. Unlike in the main specification, treatment is defined using the husband's industry (classified at the 2012 Census industry level), based on the change in remote job postings before and after the COVID-19 pandemic. Treated households are those in which the husband's industry experienced a WFH probability increase above the median. We replace husband-occupation fixed effects with husband-industry fixed effects. All regressions include controls for the wife's age, education, and number of children, as well as the husband's education. State-by-time fixed effects are included throughout. Standard errors are clustered at the industry level.

Table B.10: Heterogeneous Effects of Husbands' WFH Shock on Wives' Labor Market Outcomes by Wives' Education Level

	Women Education Level				Men Education level			
	Below HS	HS	Some college	College Grad	Below HS	HS	Some college	College Grad
A. Outcome: Annual Employment								
$Exposed_{o(H)} \times Post_t$	0.045**	0.013	-0.002	0.031***	0.022	-0.006	0.004	0.026***
	(0.021)	(0.013)	(0.008)	(0.007)	(0.028)	(0.013)	(0.008)	(0.008)
Mean pre-treatment	0.392	0.564	0.678	0.773	0.531	0.678	0.726	0.714
Obs	36,110	97,529	188,412	407,046	45,195	131,857	193,871	358,156
R^2	0.07	0.04	0.05	0.05	0.07	0.05	0.05	0.06
B. Outcome: Weekly Working Hours								
$Exposed_{o(H)} \times Post_t$	1.448*	0.653*	0.390	0.641***	0.210	0.721**	0.109	0.485**
	(0.797)	(0.360)	(0.283)	(0.149)	(0.872)	(0.337)	(0.219)	(0.214)
Mean pre-treatment	33.962	34.202	34.524	36.961	35.208	35.833	35.974	35.986
Obs	15,307	58,240	134,368	331,195	25,567	93,160	147,282	273,124
R^2	0.08	0.04	0.04	0.03	0.05	0.03	0.03	0.04
C. Outcome: Part-time Status								
$Exposed_{o(H)} \times Post_t$	-0.054**	-0.025**	-0.014	-0.039***	-0.007	-0.017	-0.011	-0.029***
	(0.022)	(0.012)	(0.010)	(0.007)	(0.029)	(0.013)	(0.010)	(0.009)
Mean pre-treatment	0.742	0.618	0.526	0.395	0.624	0.497	0.455	0.468
Obs	36,110	97,529	188,412	407,046	45,195	131,857	193,871	358,156
R^2	0.05	0.04	0.05	0.06	0.06	0.05	0.05	0.07
D. Outcome: Log Wages								
$Exposed_{o(H)} \times Post_t$	0.026	-0.003	0.063**	0.065***	-0.092	0.068**	0.009	0.058***
	(0.074)	(0.041)	(0.027)	(0.016)	(0.078)	(0.031)	(0.025)	(0.020)
Mean pre-treatment	9.058	9.312	9.566	10.292	9.371	9.621	9.826	10.217
Obs	14,330	54,692	126,993	318,061	24,298	89,103	140,626	260,076
R^2	0.11	0.06	0.06	0.07	0.09	0.09	0.09	0.09

Notes: We estimate the same baseline specification as in Table 2 in the 2017-2023 ACS sample for different samples. For simplicity, we only present the coefficient for the post-Covid–by-treatment interaction. In this Table, we split the sample by the education level of the both spouses in the household.

Table B.11: Heterogeneous Effects of the Husbands' WFH Shock on Wives Labor Market Outcomes, by Women Age

	Women Age Bins					
	20-24	25-29	30-34	35-39	40-44	45+
A. Outcome: Annual Employment						
$Exposed_{o(H)} \times Post_t$	0.072**	0.034***	0.018*	0.023***	0.023***	0.018
	(0.030)	(0.011)	(0.009)	(0.007)	(0.009)	(0.019)
Mean pre-treatment	0.608	0.657	0.702	0.715	0.716	0.691
Obs	20,318	96,884	207,244	227,852	133,970	42,804
R^2	0.12	0.11	0.09	0.08	0.08	0.10
B. Outcome: Weekly Work Hours						
$Exposed_{o(H)} \times Post_t$	-0.759	1.284***	0.422**	0.383**	0.584*	0.265
	(0.843)	(0.316)	(0.211)	(0.195)	(0.308)	(0.452)
Mean pre-treatment	33.713	34.899	35.901	36.300	36.239	36.295
Obs	12,628	66,774	153,272	173,070	101,711	31,656
R^2	0.12	0.05	0.04	0.04	0.04	0.06
C. Outcome: Part-time Status						
$Exposed_{o(H)} \times Post_t$	-0.017	-0.043***	-0.033***	-0.033***	-0.040***	-0.016
	(0.028)	(0.011)	(0.009)	(0.009)	(0.011)	(0.019)
Mean pre-treatment	0.617	0.531	0.472	0.457	0.466	0.481
Obs	20,318	96,884	207,244	227,852	133,970	42,804
R^2	0.10	0.09	0.09	0.08	0.08	0.09
D. Outcome: Log Wages						
$Exposed_{o(H)} \times Post_t$	-0.176*	0.034	0.067***	0.062***	0.071***	0.029
	(0.106)	(0.030)	(0.020)	(0.024)	(0.027)	(0.041)
Mean pre-treatment	8.929	9.512	9.909	10.112	10.183	10.171
Obs	12,194	63,915	146,740	164,803	96,577	29,849
R^2	0.12	0.10	0.12	0.11	0.12	0.13

Notes: We estimate the same baseline specification as in Table 2 in the 2017-2023 ACS sample for different samples. For simplicity, we only present the coefficient for the post-Covid–by-treatment interaction. In this Table, we split the sample by the age of the female spouse in the household.

Table B.12: Heterogeneous effects of the WFH Husbands' Shock on Wives' Labor Market Outcomes by Predicted Pre-Treatment Employment Status

	Log Weekly Wage	Log Wage	Weekly Work Hours	Works Part-time	Works From Home	Time Work Commute
$Exposed_{o(H)} \times Post_t$	-0.008	0.046	1.959***	-0.031***	0.011	-0.895
	(0.030)	(0.035)	(0.384)	(0.012)	(0.012)	(0.604)
$Exposed_{o(H)} \times Post_t \times 1(Pred.Emp == 1)$	0.046	0.010	-1.529***	-0.034**	0.058***	-1.509**
	(0.031)	(0.037)	(0.398)	(0.014)	(0.011)	(0.670)
Mean pre-treatment	6.447	9.956	35.916	0.480	0.083	24.488
$\beta_1 + \beta_2 = 0$ (p-val)	0.001	0.000	0.001	0.000	0.000	0.000
Obs	514,151	514,151	539,181	729,120	482,358	482,298
R^2	0.18	0.15	0.03	0.11	0.09	0.04

Notes: We use random forest algorithm to predict the probability that women in our pre-Covid sample were employed. We feed controls set including the state of residence *for the pre-Covid sample* into our random forest model estimation. We use these estimations to predict the probability of employment out-of-sample. Then, if the predicted probability that a woman works is more than 50%, the model classifies them as employed in the pre-Covid era. This technique allows us to homogeneously classify all women in the sample. We fully interact the classification variable with our post-Covid and treament dummies. The coefficient on the triple interaction shows the differential effect of the WFH spousal shock on women that were likely to be working pre-Covid compared to unemployed women in the pre-Covid era.

Table B.13: Effect of Spousal WFH Shocks on Women's Labor Market Outcomes in Samples Without Children and with Older Children

	Employment	Log Weekly Wage	Log Wage	Usual Weekly Hours Worked	Part-time Status
A. No Children					
$Exposed_{o(h(i))} \times Post_t$	0.012**	0.013	0.020	0.154	-0.020***
	(0.005)	(0.012)	(0.014)	(0.140)	(0.005)
Mean pre-treatment	0.848	6.426	10.060	39.466	0.273
$\beta_1 + \beta_2 = 0$ (p-val)	0.075	0.845	0.745	0.442	0.194
Obs	413,037	351,526	351,526	364,229	413,037
R^2	0.06	0.16	0.16	0.03	0.06
B. Youngest Child is Teenager (12-17 Years Old)					
$Exposed_{o(h(i))} \times Post_t$	0.023***	0.005	0.023	0.595***	-0.029***
	(0.006)	(0.014)	(0.015)	(0.153)	(0.006)
Mean pre-treatment	0.778	6.437	10.000	37.325	0.391
$\beta_1 + \beta_2 = 0$ (p-val)	0.038	0.528	0.130	0.237	0.005
Obs	267,310	207,836	207,836	217,830	267,310
R^2	0.06	0.13	0.10	0.03	0.06
C. Youngest Child is Adult (18+ Years Old)					
$Exposed_{o(h(i))} \times Post_t$	0.007*	0.010	0.015	0.079	-0.014***
	(0.004)	(0.010)	(0.012)	(0.118)	(0.005)
Mean pre-treatment	0.821	6.389	10.012	39.104	0.304
$\beta_1 + \beta_2 = 0$ (p-val)	0.113	0.715	0.388	0.852	0.150
Obs	545,021	449,335	449,335	466,291	545,021
R^2	0.07	0.16	0.15	0.03	0.07

Notes: This table reports estimates from the cross-sectional specification described in Equation (1), using the 2017–2023 ACS sample of dual-parent households for three different samples. In Panel A we use a sample of households without children. In Panels B and C, we use a sample of household where the youngest child has 12 to 17 years old, and over 17 years old, respectively. All regressions include standard demographic controls (wife's age, number of children, spouses' education), as well as fixed effects for state-by-time and the husband's occupation. Standard errors are clustered at the husband's occupation level.

Table B.14: Effect of Wives' WFH Shocks on Their Husbands' Labor Market Outcomes

	Annual Employment	Log Weekly Wages	Log Wages	Weekly Work Hours	Part-time Status
$Exposed_{o(h(i))} \times Post_t$	0.005**	0.004	0.019**	-0.158	-0.006*
	(0.003)	(0.003)	(0.009)	(0.128)	(0.003)
Mean pre-treatment	0.920	-1.406	10.574	44.210	0.091
Obs	396,791	364,830	364,830	381,835	396,791
R^2	0.02	0.02	0.19	0.02	0.02

Notes: This table reports estimates from the cross-sectional specification described in Equation (1), using the 2017–2023 ACS sample of dual-parent households with children of child-rearing age. All regressions include standard demographic controls (wife's age, number of children, spouses' education), as well as fixed effects for state-by-time and the wife's occupation (or industry, as appropriate). Standard errors are clustered at the wife's occupation or industry level.

Table B.15: Effects of Husbands' WFH Shock on Wives' Intensive Margin of Labor Supply after Adding Wives' Occupation Fixed Effects

	Log Weekly Wage	Log wage	Work Hours	Part Time
$Exposed_{o(H)} \times Post_t$	0.029***	0.052***	0.477***	-0.027***
	(0.009)	(0.011)	(0.109)	(0.005)
Mean pre-treatment	6.469	9.981	36.489	0.266
Obs	514,144	514,144	514,144	514,144
R^2	0.34	0.33	0.15	0.16

Notes: This table presents estimates from Equation (1) on the sample of women working. Treatment is defined based on the change in the probability of WFH before and after the COVID-19 pandemic, using job postings data at the husband's occupation ($o(h(i))$, Census 2010) levels. The sample is the 2017–2024 CPS sample of dual-parent households with children of child-rearing age. All regressions include fixed effects for state-by-time, husband's occupation, and women's occupation fixed effects. Controls include the wife's age, education, and number of children, as well as the husband's education. Standard errors are clustered at the husband's occupation level.

Table B.16: Effect of Wives' WFH Occupation Shock on Their Own Labor Market Outcomes

	Log Weekly Wage	Log Wage	Usual Weekly Hours Worked	Part-time Status
$Exposed_{o(h(i))} \times Post_t$	0.038***	0.046**	0.185	-0.012**
	(0.015)	(0.018)	(0.150)	(0.005)
$Exposed_{o(h(i))} \times Post_t \times During_t$	-0.034**	-0.036*	-0.093	0.007
	(0.014)	(0.019)	(0.145)	(0.005)
Mean pre-treatment	6.446	9.959	36.522	0.269
$\beta_1 + \beta_2 = 0$ (p-val)	0.572	0.430	0.475	0.345
Obs	560,544	560,544	560,544	560,544
R^2	0.34	0.33	0.14	0.15

Notes: This table adapts our baseline cross-sectional specification to estimate the effect of women's WFH shocks on their own labor market outcomes, using the 2017–2023 CPS sample of dual-parent households with children of child-rearing age.

Table B.17: Effects of Husbands' Occupation and Women College Major WFH Shocks on Women Labor Supply

	Annual Employment	Log Weekly Wage	Log Wages	Weekly Work Hours	Part-time Status
$Exposed_{o(h(i))} \times Post_t$	0.010**	-0.016	-0.001	0.331**	-0.023***
	(0.005)	(0.011)	(0.014)	(0.142)	(0.006)
$Exposed_{e(i)} \times Post_t$	0.007	-0.068***	-0.085***	-0.270	-0.009
	(0.007)	(0.021)	(0.025)	(0.219)	(0.009)
$Exposed_{o(h(i))} \times Exposed_{e(i)} \times Post_t$	0.034***	0.156***	0.186***	0.774***	-0.030***
	(0.008)	(0.023)	(0.029)	(0.262)	(0.010)
Mean pre-treatment	0.698	6.447	9.956	35.916	0.480
Obs	729,120	514,151	514,151	539,181	729,120
R^2	0.10	0.23	0.19	0.04	0.10

Notes: This table presents estimates from Equation (1) in the first column for each outcome, and Equation (3) in the second column. Treatment is defined based on the change in the probability of WFH before and after the COVID-19 pandemic, using job postings data at the husband's occupation ($o(h(i))$, Census 2010) levels and the actual WFH uptake data by women's major college degree ($e(i)$). The sample is the 2017–2023 ACS sample of dual-parent households with children of child-rearing age. All regressions include fixed effects for state-by-time, husband's occupation, and women's major college degree. Controls include the wife's age, education, and number of children, as well as the husband's education. Standard errors are clustered at the husband's occupation level.

Table B.18: Effect of Husbands' WFH Shock on Marriage, Divorce, and Fertility Outcomes

	Married in the last 12mm	Divorced in the last 12mm	Had a child in the last 12mm
$Exposed_{o(H)} \times Post_t$	0.001	0.000	-0.007**
	(0.002)	(0.000)	(0.003)
Mean pre-treatment	0.029	0.001	0.173
Obs	729,120	728,956	729,120
R^2	0.04	0.00	0.32

Notes: This table reports estimates using data from the pooled ACS sample from 2017 to 2023. The outcomes—marriage, divorce, and fertility—are defined over the 12 months preceding the interview date. Occupation is measured only at the time of the interview, so we implicitly assume that individuals remained in the same occupation throughout the prior 12-month period.

Table B.19: Effect of Husbands' WFH Shock on Fertility Outcomes

	Pr(At least one child)	Number of Children
$Exposed_{o(h(i))} \times Post_t$	-0.011***	-0.019**
	(0.003)	(0.008)
$Exposed_{o(h(i))} \times Post_t \times During_t$	0.006**	0.022***
	(0.002)	(0.007)
Mean pre-treatment	0.740	1.510
$\beta_1 + \beta_2 = 0$ (p-val)	0.021	0.664
Obs	1,715,167	1,715,167
R^2	0.08	0.11

Notes: This table reports estimates from Equation (1) using the pooled 2017–2023 ACS data. The outcomes include the probability of having had a child in the past year and the number of children in the household. Treatment is defined based on the husband's occupation-level WFH exposure, as detailed in Subsection 2.3. All regressions include controls for the wife's age, number of children, and education, as well as the husband's education. State-by-time and husband-occupation fixed effects are included. Standard errors are clustered at the occupation level.

Table B.20: Effect of Husbands' WFH Shock on Wives' Labor Market Outcomes Using Reweighting Techniques

	Employment	Log Weekly Wage	Log Wage	Usual Weekly Hours Worked	Part-time Status
A. Using DFL Weights					
$Exposed_{o(h(i))} \times Post_t$	0.024***	0.028***	0.052***	0.579***	-0.035***
	(0.005)	(0.010)	(0.012)	(0.116)	(0.006)
$Exposed_{o(h(i))} \times Post_t \times During_t$	-0.009*	-0.014	-0.008	0.074	0.006
	(0.005)	(0.010)	(0.012)	(0.113)	(0.005)
Mean pre-treatment	0.698	6.447	9.956	35.916	0.480
$\beta_1 + \beta_2 = 0$ (p-val)	0.000	0.050	0.000	0.000	0.000
Obs	729,120	514,151	514,151	539,181	729,120
R^2	0.09	0.18	0.15	0.03	0.08
B. Using IPW					
$Exposed_{o(h(i))} \times Post_t$	0.023***	0.031***	0.053***	0.558***	-0.035***
	(0.005)	(0.011)	(0.012)	(0.118)	(0.006)
$Exposed_{o(h(i))} \times Post_t \times During_t$	-0.009*	-0.015	-0.009	0.071	0.006
	(0.005)	(0.010)	(0.012)	(0.114)	(0.005)
Mean pre-treatment	0.698	6.447	9.956	35.916	0.480
$\beta_1 + \beta_2 = 0$ (p-val)	0.000	0.035	0.000	0.000	0.000
Obs	729,120	514,151	514,151	539,181	729,120
R^2	0.08	0.18	0.14	0.03	0.08

Notes: This table reports estimates from Equation (1), using the 2017–2023 ACS sample of dual-parent households with children of child-rearing age. Panel A applies reweighting using DiNardo-Fortin-Lemieux (DFL) weights (DiNardo et al., 1995), while Panel B uses inverse-propensity score weights (IPW). Both methods aim to correct for potential changes in group composition before and after the COVID-19 period. Details on the construction of these weights are provided in Section 5.

Table B.21: Robustness of ACS Results to Alternative Definitions of WFH Shock

	Employed	Log Weekly Wage	Log wages	Usual work hours	Part-time status
A. Shock: Difference in average 2023-2024 vs 2019 WFH vacancy rates at Census 2010 4d level (Original)					
$Exposed_{o(h(i))} \times Post_t$	0.024***	0.030***	0.052***	0.569***	-0.035***
	(0.005)	(0.010)	(0.012)	(0.117)	(0.006)
Mean pre-treatment	0.702	6.460	9.969	35.927	0.476
Obs	729,120	514,151	514,151	539,181	729,120
R^2	0.08	0.18	0.15	0.03	0.08
B. Shock: Difference in 3 month moving average 2023-2024 vs 2019 WFH vacancy rates at Census 2010 4d level					
$Exposed_{o(h(i))} \times Post_t$	0.024***	0.030***	0.052***	0.569***	-0.035***
	(0.005)	(0.010)	(0.012)	(0.117)	(0.006)
Mean pre-treatment	0.702	6.460	9.969	35.927	0.476
Obs	729,120	514,151	514,151	539,181	729,120
R^2	0.08	0.18	0.15	0.03	0.08
C. Shock: Difference in July 2023 vs July 2019 WFH vacancy rates at Census 2010 4d level					
$Exposed_{o(h(i))} \times Post_t$	0.025***	0.028***	0.050***	0.524***	-0.035***
	(0.005)	(0.010)	(0.012)	(0.117)	(0.006)
Mean pre-treatment	0.702	6.460	9.969	35.927	0.476
Obs	729,120	514,151	514,151	539,181	729,120
R^2	0.08	0.18	0.15	0.03	0.08
D. Shock: WFH teleworkability a-la Dingel and Neumann at Census 2010 4d level					
$Exposed_{o(h(i))} \times Post_t$	0.029***	0.017*	0.040***	0.512***	-0.037***
	(0.005)	(0.010)	(0.012)	(0.121)	(0.005)
Mean pre-treatment	0.704	6.464	9.973	35.937	0.474
Obs	721,305	510,086	510,086	534,874	721,305
R^2	0.08	0.18	0.15	0.03	0.08
E. Shock: Difference in average 2023-2024 vs 2019 WFH vacancy rates at SOC 3d level					
$Exposed_{o(h(i))} \times Post_t$	0.023***	0.032***	0.055***	0.580***	-0.036***
	(0.005)	(0.011)	(0.013)	(0.126)	(0.006)
Mean pre-treatment	0.703	6.462	9.970	35.930	0.476
Obs	726,511	512,849	512,849	537,805	726,511
R^2	0.08	0.18	0.14	0.03	0.08
F. Shock: Difference in 3 month moving average 2023-2024 vs 2019 vacancy rates at SOC 3d level					
$Exposed_{o(h(i))} \times Post_t$	0.023***	0.033***	0.057***	0.604***	-0.036***
	(0.005)	(0.011)	(0.013)	(0.126)	(0.006)
Mean pre-treatment	0.703	6.462	9.970	35.930	0.476
Obs	726,511	512,849	512,849	537,805	726,511
R^2	0.08	0.18	0.14	0.03	0.08
G. Shock: Difference in July 2023 vs July 2019 WFH vacancy rates at SOC 3d level					
$Exposed_{o(h(i))} \times Post_t$	0.024***	0.031***	0.054***	0.572***	-0.037***
	(0.005)	(0.011)	(0.013)	(0.127)	(0.006)
Mean pre-treatment	0.703	6.462	9.970	35.930	0.476
Obs	726,511	512,849	512,849	537,805	726,511
R^2	0.08	0.18	0.14	0.03	0.08
H. Shock: WFH teleworkability a-la Dingel and Neumann at SOC 3d level					
$Exposed_{o(h(i))} \times Post_t$	0.025***	0.029***	0.050***	0.540***	-0.036***
	(0.005)	(0.010)	(0.013)	(0.129)	(0.006)
Mean pre-treatment	0.704	6.464	9.973	35.937	0.474
Obs	721,307	510,088	510,088	534,876	721,307
R^2	0.08	0.18	0.14	0.03	0.08

Notes:: In panel A, an occupation is treated if the difference in average 2023 and 2024 vs 2019 remote job vacancy shares is above the median of all other Census 2010 4d level occupations. Panel B uses the 3 month moving average version of the WFH monthly variables. In panel C, an occupation is shocked if the difference in July 2023 vs July 2019 WFH or remote job vacancy shares is above the median of all other Census 2010 4d level occupations. In panel D, an occupation is shocked if its Dingel and Neumann teleworkability index is above the median of all other Census 2010 4d level occupations. Panels E, F, G, H are analogous to A, B, C, D respectively, with the change that occupations are compared at the SOC 2018 3d level. All regressions include fixed effects for state-by-time and husband's occupation. Controls include the wife's age, education, and number of children, as well as the husband's education. Standard errors are clustered at the husband's occupation level.

Table B.22: Robustness of ACS Results to Alternative Fixed Effects Specifications

	Employed	Log Weekly Wage	Log wages	Usual work hours	Part-time status
A. FE: Year + Spouse Occupation (Group of 16) + State					
$Exposed_{o(h(i))} \times Post_t$	0.011*	0.066***	0.081***	0.415***	-0.023***
	(0.006)	(0.013)	(0.013)	(0.131)	(0.007)
Mean pre-treatment	0.698	6.447	9.956	35.916	0.480
Obs	516,559	364,843	364,843	382,431	516,559
R^2	0.07	0.17	0.14	0.03	0.07
B. FE: Year-state + Spouse Occupation (Group of 16)					
$Exposed_{o(h(i))} \times Post_t$	0.011*	0.065***	0.080***	0.388***	-0.021***
	(0.006)	(0.013)	(0.013)	(0.132)	(0.007)
Mean pre-treatment	0.698	6.447	9.956	35.916	0.480
Obs	516,559	364,843	364,843	382,431	516,559
R^2	0.07	0.17	0.14	0.03	0.07
C. FE: Year-state + Spouse Occupation (Group of 16)-by-state					
$Exposed_{o(h(i))} \times Post_t$	0.011*	0.066***	0.080***	0.394***	-0.022***
	(0.006)	(0.013)	(0.013)	(0.135)	(0.007)
Mean pre-treatment	0.698	6.447	9.956	35.916	0.480
Obs	516,559	364,843	364,843	382,431	516,559
R^2	0.08	0.18	0.14	0.03	0.07
D. FE: Year + Spouse Occupation (4d) + State					
$Exposed_{o(h(i))} \times Post_t$	0.025***	0.031***	0.055***	0.595***	-0.036***
	(0.005)	(0.010)	(0.012)	(0.116)	(0.006)
Mean pre-treatment	0.698	6.447	9.956	35.916	0.480
Obs	516,557	364,841	364,841	382,429	516,557
R^2	0.08	0.18	0.15	0.03	0.08
E. FE: Year-state + Spouse Occupation (4d) (Baseline)					
$Exposed_{o(h(i))} \times Post_t$	0.024***	0.030***	0.053***	0.566***	-0.035***
	(0.005)	(0.010)	(0.012)	(0.117)	(0.006)
Mean pre-treatment	0.698	6.447	9.956	35.916	0.480
Obs	516,557	364,841	364,841	382,429	516,557
R^2	0.08	0.18	0.15	0.03	0.08
F. FE: Year-state + Spouse Occupation (4d)-by-state					
$Exposed_{o(h(i))} \times Post_t$	0.021***	0.031***	0.056***	0.588***	-0.033***
	(0.005)	(0.011)	(0.013)	(0.124)	(0.006)
Mean pre-treatment	0.698	6.449	9.957	35.916	0.480
Obs	513,864	361,815	361,815	379,454	513,864
R^2	0.12	0.23	0.19	0.08	0.12
G. FE: Year-MSA + Spouse Occupation (4d)					
$Exposed_{o(h(i))} \times Post_t$	0.023***	0.028**	0.052***	0.567***	-0.035***
	(0.006)	(0.013)	(0.015)	(0.147)	(0.006)
Mean pre-treatment	0.692	6.531	10.041	35.972	0.484
Obs	395,604	277,637	277,637	290,838	395,604
R^2	0.09	0.19	0.15	0.04	0.09

Notes: This table presents estimates from alternative versions of Equation (1) using the 2017–2023 ACS sample of dual-parent households with children of child-rearing age, varying the fixed effects (FE) structure. Panel E reports the baseline specification, which includes state-by-time and husband-occupation fixed effects. Panel A includes separate fixed effects for month-year, state, and broad occupation groups (collapsed into 16 categories such as Business/Finance, Education Professionals, Healthcare, Sales, Construction, etc.). Panel B adds state-by-time fixed effects, and Panel C includes state-by-time and broad occupation group fixed effects. Panels D to F replicate Panels A to C, respectively, but use detailed 4-digit Census occupation fixed effects instead of the broader groupings. Panel G includes MSA fixed effects, as an alternative to state fixed effects. Controls include the wife's age, education, and number of children, as well as the husband's education. Standard errors are clustered at the husband's occupation level.

Table B.23: Robustness of ACS Results to Adding Broad Occupations and Industries Time Trends

	Employed	Log Weekly Wage	Log wages	Usual work hours	Part-time status
A. FE: Year-state + Spouse Occupation (4d) (baseline)					
$Exposed_{o(h(i))} \times Post_t$	0.024***	0.030***	0.053***	0.566***	-0.035***
	(0.005)	(0.010)	(0.012)	(0.117)	(0.006)
Mean pre-treatment	0.698	6.447	9.956	35.916	0.480
Obs	516,557	364,841	364,841	382,429	516,557
R^2	0.08	0.18	0.15	0.03	0.08
B. Year-State + Spouse Occupation (4d) + year-by Spouse Occ (group of 16)					
$Exposed_{o(h(i))} \times Post_t$	0.012	0.011	0.021	0.208	-0.024**
	(0.008)	(0.016)	(0.019)	(0.195)	(0.009)
Mean pre-treatment	0.698	6.447	9.956	35.916	0.480
Obs	516,557	364,841	364,841	382,429	516,557
R^2	0.09	0.18	0.15	0.03	0.08
C. Year-State + Spouse Occupation (4d) + year-by Spouse Even Broader Occ (group of 4)					
$Exposed_{o(h(i))} \times Post_t$	0.013**	0.029**	0.042**	0.330**	-0.020***
	(0.006)	(0.013)	(0.016)	(0.154)	(0.007)
Mean pre-treatment	0.698	6.447	9.956	35.916	0.480
Obs	516,557	364,841	364,841	382,429	516,557
R^2	0.08	0.18	0.15	0.03	0.08
D. Year-State + Spouse Occupation (4d) + year-by Spouse Major Industry					
$Exposed_{o(h(i))} \times Post_t$	0.016***	0.024**	0.049***	0.573***	-0.030***
	(0.005)	(0.011)	(0.013)	(0.133)	(0.006)
Mean pre-treatment	0.703	6.455	9.964	35.945	0.476
Obs	497,557	353,234	353,234	370,206	497,557
R^2	0.09	0.19	0.15	0.03	0.08
E. Year-State + Spouse Occupation (4d) + year-by Spouse Broad Ind (group of 4)					
$Exposed_{o(h(i))} \times Post_t$	0.021***	0.026**	0.048***	0.529***	-0.030***
	(0.005)	(0.011)	(0.013)	(0.126)	(0.006)
Mean pre-treatment	0.703	6.455	9.964	35.945	0.476
Obs	497,557	353,234	353,234	370,206	497,557
R^2	0.09	0.19	0.15	0.03	0.08

Notes: This table presents estimates from alternative versions of Equation (1) using the 2017–2023 ACS sample of dual-parent households with children of child-rearing age, adding year-by-broad occupation or industry fixed effects. Panel A is our benchmark estimation. Panel B adds time trends of a broader occupation classification with 16 categories. Panel C does the same, but with an even broader classification, with 3 categories (blue-collar workers, professionals and those in service/sales occupations). Panel D and E are analogous, but using the industry classification. Panel E divides whether workers are in the public sector, primary sector, private services sector and manufacture. Controls include the wife's age, education, and number of children, as well as the husband's education. Standard errors are clustered at the husband's occupation level.

Table B.24: Robustness of ACS Results to Adding Educational Time Trends by Occupation

	Employed	Log Weekly Wage	Log wages	Usual work hours	Part-time status
A. FE: State + Spouse Occ (4d) + Years of Education (Quintiles) -year					
$Exposed_{o(h(i))} \times Post_t$	0.013*	0.042***	0.059***	0.427**	-0.022***
	(0.007)	(0.015)	(0.018)	(0.170)	(0.008)
Mean pre-treatment	0.698	6.448	9.956	35.915	0.480
Obs	516,362	364,707	364,707	382,294	516,362
R^2	0.08	0.18	0.15	0.03	0.08
B. FE: Year-State FE + Spouse Occ (4d) + Years of Education (Quintiles) -year					
$Exposed_{o(h(i))} \times Post_t$	0.012*	0.040***	0.057***	0.407**	-0.021***
	(0.006)	(0.015)	(0.018)	(0.168)	(0.008)
Mean pre-treatment	0.698	6.448	9.956	35.915	0.480
Obs	516,362	364,707	364,707	382,294	516,362
R^2	0.08	0.18	0.15	0.03	0.08

Notes: This table presents estimates from alternative versions of Equation (1) using the 2017–2023 ACS sample of dual-parent households with children of child-rearing age, adding year-by-quintiles in average years of education of occupations fixed effects. Panel A controls by state, occupation and the educational trends fixed effects. Panel B alternatively includes state-by-year fixed effects. Controls include the wife's age, education, and number of children, as well as the husband's education. Standard errors are clustered at the husband's occupation level.

Table B.25: Robustness of ACS Results to Sequential Removal of Demographic Controls

	Control Set 1	Control Set 2	Control Set 3	Control Set 4	Control Set 5 (Baseline)
Outcome: Employment					
$Exposed_{o(h(i))} \times Post_t$	0.019***	0.019***	0.020***	0.024***	0.024***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Mean pre-treatment	0.698	0.698	0.698	0.698	0.698
Obs	729,120	729,120	729,120	729,120	729,120
R^2	0.03	0.05	0.05	0.08	0.08
Outcome: Log Weekly Wage					
$Exposed_{o(h(i))} \times Post_t$	0.019*	0.018*	0.024**	0.030***	0.030***
	(0.011)	(0.011)	(0.010)	(0.011)	(0.010)
Mean pre-treatment	6.447	6.447	6.447	6.447	6.447
Obs	514,151	514,151	514,151	514,151	514,151
R^2	0.11	0.11	0.15	0.18	0.18
Outcome: Log wages					
$Exposed_{o(h(i))} \times Post_t$	0.040***	0.039***	0.046***	0.053***	0.052***
	(0.013)	(0.013)	(0.012)	(0.012)	(0.012)
Mean pre-treatment	9.956	9.956	9.956	9.956	9.956
Obs	514,151	514,151	514,151	514,151	514,151
R^2	0.08	0.09	0.12	0.15	0.15
Outcome: Usual work hours					
$Exposed_{o(h(i))} \times Post_t$	0.526***	0.528***	0.551***	0.569***	0.569***
	(0.117)	(0.117)	(0.116)	(0.116)	(0.117)
Mean pre-treatment	35.916	35.916	35.916	35.916	35.916
Obs	539,181	539,181	539,181	539,181	539,181
R^2	0.02	0.02	0.03	0.03	0.03
Outcome: Part-time status					
$Exposed_{o(h(i))} \times Post_t$	-0.030***	-0.030***	-0.032***	-0.035***	-0.035***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Mean pre-treatment	0.480	0.480	0.480	0.480	0.480
Obs	729,120	729,120	729,120	729,120	729,120
R^2	0.03	0.05	0.06	0.08	0.08
Control: Number of children	No	Yes	Yes	Yes	Yes
Control: Women's age (5y sbin)	No	No	Yes	Yes	Yes
Control: Women's education level	No	No	No	Yes	Yes
Control: Husband's education level	No	No	No	No	Yes

Notes: This table reports results from estimating Equation (1) on the 2017–2023 ACS sample of dual-parent households with children of child-rearing age. The baseline specification, shown in Column 5, includes controls for the wife's age (in 5-year bins), number of children in the household, and both spouses' education levels. Columns 1–4 sequentially remove these controls to assess the sensitivity of the results to the inclusion of covariates. All specifications include state-by-time and husband-occupation fixed effects.

Table B.26: Robustness of ACS Results to Adding Controls for Children's Age

	Control Set 1 (Baseline)	Control Set 2	Control Set 3	Control Set 4	Control Set 5
Outcome: Employment					
$Exposed_{o(h(i))} \times Post_t$	0.024***	0.023***	0.024***	0.024***	0.024***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Mean pre-treatment	0.698	0.698	0.698	0.698	0.698
Obs	729,120	729,120	729,120	729,120	729,120
R^2	0.08	0.09	0.09	0.09	0.09
Outcome: Log weekly Wage					
$Exposed_{o(h(i))} \times Post_t$	0.030***	0.029***	0.029***	0.029***	0.029***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
Mean pre-treatment	6.447	6.447	6.447	6.447	6.447
Obs	514,151	514,151	514,151	514,151	514,151
R^2	0.18	0.18	0.18	0.18	0.18
Outcome: Log wages					
$Exposed_{o(h(i))} \times Post_t$	0.052***	0.051***	0.052***	0.052***	0.052***
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
Mean pre-treatment	9.956	9.956	9.956	9.956	9.956
Obs	514,151	514,151	514,151	514,151	514,151
R^2	0.15	0.15	0.15	0.15	0.15
Outcome: Usual work hours					
$Exposed_{o(h(i))} \times Post_t$	0.569***	0.561***	0.580***	0.570***	0.571***
	(0.117)	(0.117)	(0.117)	(0.116)	(0.116)
Mean pre-treatment	35.916	35.916	35.916	35.916	35.916
Obs	539,181	539,181	539,181	539,181	539,181
R^2	0.03	0.03	0.03	0.03	0.03
Outcome: Part-time status					
$Exposed_{o(h(i))} \times Post_t$	-0.035***	-0.035***	-0.036***	-0.035***	-0.035***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Mean pre-treatment	0.480	0.480	0.480	0.480	0.480
Obs	729,120	729,120	729,120	729,120	729,120
R^2	0.08	0.08	0.08	0.08	0.08
Control: Baseline controls	Yes	Yes	Yes	Yes	Yes
Control: Age youngest child	No	Yes	No	Yes	Yes
Control: Age oldest child (raw)	No	No	Yes	Yes	No
Control: Age oldest child (4y bins)	No	No	No	No	Yes

Notes: This table reports estimates from Equation (1) using the 2017–2023 ACS sample of dual-parent households with children of child-rearing age. Column 1 reproduces the baseline specification, which includes controls for the wife's age (in 5-year bins), number of children in the household, and both spouses' education levels. In Columns 2 through 5, we sequentially add controls for the age of the youngest and oldest child, both as continuous variables and in four-year bins. All regressions include state-by-time and husband-occupation fixed effects.

Table B.27: Robustness to Excluding Households with Self-Employed Husbands

	Employed	Log Weekly Wage	Log wages hours	Usual work status	Part-time
$Exposed_{o(h(i))} \times Post_t$	0.024***	0.030***	0.052***	0.569***	-0.035***
	(0.005)	(0.010)	(0.012)	(0.117)	(0.006)
Mean pre-treatment	0.702	6.460	9.969	35.927	0.476
Obs	729,120	514,151	514,151	539,181	729,120
R^2	0.08	0.18	0.15	0.03	0.08

Notes: This table reports results from Equation (1) using the 2017–2023 ACS sample of dual-parent households with children of child-rearing age. We restrict the sample to households where the husband is classified as an employee (i.e., excluding households in which the husband reports being self-employed). The treatment is defined based on the husband's occupation-level WFH shock, as in the baseline specification. All regressions include state-by-time and husband-occupation fixed effects, as well as controls for the wife's age, number of children, and both spouses' education levels.

Table B.28: Robustness of Wage Results to Trimming High-Income Households

	Employed	Log Weekly Wage	Log wages hours	Usual work Status	Part-time Worked
A. No income trim (Baseline)					
$Exposed_{o(h(i))} \times Post_t$	0.024***	0.030***	0.052***	0.569***	-0.035***
	(0.005)	(0.010)	(0.012)	(0.117)	(0.006)
Mean pre-treatment	0.702	6.460	9.969	35.927	0.476
Obs	729,120	514,151	514,151	539,181	729,120
R^2	0.08	0.18	0.15	0.03	0.08
B. Trim top 1% income earners					
$Exposed_{o(h(i))} \times Post_t$	0.023***	0.028***	0.051***	0.567***	-0.034***
	(0.005)	(0.010)	(0.012)	(0.119)	(0.006)
Mean pre-treatment	0.701	6.442	9.948	35.850	0.478
Obs	721,440	507,515	507,515	532,429	721,440
R^2	0.08	0.17	0.14	0.03	0.08
C. Trim top 5% income earners					
$Exposed_{o(h(i))} \times Post_t$	0.022***	0.021**	0.044***	0.584***	-0.034***
	(0.005)	(0.011)	(0.012)	(0.120)	(0.005)
Mean pre-treatment	0.700	6.400	9.903	35.727	0.479
Obs	691,176	485,825	485,825	509,937	691,176
R^2	0.09	0.16	0.13	0.03	0.08

Notes: This table reports robustness checks on women's log wage outcomes using the 2017–2023 ACS sample of dual-parent households with children of child-rearing age. We examine whether our baseline wage results are sensitive to the exclusion of households at the top of the income distribution. Panel A reports the baseline specification without trimming, included for comparison. Subsequent panels show results after progressively trimming the top percentiles of the income distribution. The treatment is based on the husband's occupation-level WFH shock, as described in Subsection 2.3. All regressions include state-by-time and husband-occupation fixed effects, as well as controls for the wife's age, number of children, and both spouses' education levels.

Table B.29: Test: Leave-One Occupation Out using Census 2d

	Employed	Log Weekly Wage	Log wages	Usual work hours	Part-time Status
Benchmark Estimate	0.024	0.030	0.053	0.566	-0.035
Average Bootstrap Estimate	0.024	0.030	0.053	0.566	-0.035
Average Bootstrap SE	0.005	0.011	0.012	0.117	0.006
P-Statistic	0.000	0.000	0.000	0.000	0.000

Notes: The table reports the main estimates of the analysis for the labor outcomes of women in the ACS sample between 2017 and 2023. Benchmark Estimate reports estimates from Equation (1). Average Boostrap Estimate is the average from the estimates obtained from dropping in every iteration a different 2-digit occupation at a time. The Average Standard Error is the mean from the standard errors in each iteration from leaving a different occupation out from estimations. The P-statistic captures the percentage of estimates from the iterations that were not significant at the 5% level. All regressions include fixed effects for state-by-time and husband's occupation. Controls include the wife's age, education, and number of children, as well as the husband's education. Standard errors from each iteration are clustered at the husband's occupation level.

Table B.30: Effect of Husbands' WFH Shock on Wives Labor Market Outcomes Using a Placebo Shock Timing

	Annual Employment	Log Weekly Wage	Log Wages	Weekly Work Hours	Part-time Status
$Exposed_{o(h(i))} \times Post_t$	0.011***	-0.008	-0.004	0.052	-0.008*
	(0.004)	(0.010)	(0.011)	(0.114)	(0.004)
Mean pre-treatment	0.689	6.423	9.933	35.902	0.490
Obs	408,112	286,403	286,403	300,223	408,112
R^2	0.09	0.19	0.15	0.03	0.08

Notes: This table reports results from a placebo test in which the COVID-19 treatment period is artificially shifted to 2018, so that the post-Covid period comprises 2018 and 2019. The specification follows the baseline cross-sectional model, estimated using the 2017–2020 ACS subsample where the reference years are 2016-2019.

Table B.31: Effect of Husbands' WFH Shock on Wives' Labor Market Outcomes Using the CPS

	Monthly CPS sample			*March CPS sample*				
	Employed Last Week	Work hours last week	Part-time status	Employed Last Year	Log Weekly Wages	Log Wages	Usual Weekly Work Hours	Part-time Status
$Exposed_{o(h(i))} \times Post_t$	0.024**	0.222	0.003	0.035**	0.027	0.082**	1.171***	-0.030*
	(0.011)	(0.197)	(0.008)	(0.015)	(0.032)	(0.036)	(0.348)	(0.016)
Mean pre-treatment	0.675	36.867	0.307	0.672	6.507	10.040	36.286	0.235
Obs	395,431	254,834	251,572	58,927	39,785	39,785	41,930	39,785
R^2	0.12	0.06	0.05	0.11	0.20	0.17	0.06	0.06

Notes: This table reports estimates from Equation (1) using the 2016–2024 CPS sample of dual-parent households with children of child-rearing age. The Monthly CPS sample tracks individuals monthly in a short panel rotational basis, which we do not exploit, and does not ask about income. The March CPS tracks income in addition to other variables and annual employment variables. All regressions control for the wife's age, education, and number of children, as well as the husband's education. Specifications include state-by-time and husband-occupation fixed effects. Standard errors are clustered at the occupation level.

Table B.32: Effect of Husbands' WFH Shock on Wives' Labor Market Outcomes Using the SIPP Panel Component

	Employed last month	Log Weekly Wages	Log Monthly Wages	Avg Weekly Work Hours	Part-time Status
$Exposed_{o(h(i))} \times Post_t$	0.019	0.136	0.343**	3.853*	-0.098
	(0.055)	(0.136)	(0.155)	(2.005)	(0.089)
Mean pre-treatment	0.698	4.277	7.750	35.015	0.309
Total Obs	7,646	5,075	5,089	5,287	5,287
Unique HHs	576	417	417	430	430
R^2	0.89	0.86	0.87	0.85	0.85

Notes: The panel specification replaces cross-section FE with household-level and time-level (i.e. month-year) fixed effects. In addition we also ensure that treatment status has not change from treated to untreated (or viceversa) in the sample (i.e. we enforce that spouses must have been in similarly exposed occupations across any waves they appear). We use a sample of households that joined the SIPP on 2020 and thus were covered during reference years 2019 to 2022. In practice, this is the only subsample of the SIPP that covers the pre- and post-Covid period. All regressions include controls for the wife's age, education, and number of children, as well as the husband's education. Standard errors are clustered at the husband's occupation level.

Table B.33: Effect of Husbands' WFH Shock on Time Spent in Labor and Household Supply - Share of the Day

	Time Spent in Labor Production						*Time Spent for Personal Care and Leisure*	
	Working	Main Work	Main Work From Home	Main Work Outside Home	Secondary Work	Commuting	Personal Care	Leisure
Wives:								
$Exposed_{o(h(i))} \times Post_t$	0.072***	0.071***	0.049***	0.022	-0.000	0.001	-0.010	0.025**
	(0.017)	(0.017)	(0.013)	(0.016)	(0.001)	(0.002)	(0.011)	(0.011)
$Exposed_{o(h(i))} \times Post_t \times During_t$	-0.080***	-0.083***	-0.039**	-0.044**	0.002**	-0.000	0.005	-0.030***
	(0.020)	(0.021)	(0.016)	(0.019)	(0.001)	(0.002)	(0.011)	(0.011)
Mean pre-treatment	0.172	0.170	0.025	0.145	0.001	0.012	0.374	0.112
$\beta_1 + \beta_2 = 0$ (p-val)	0.675	0.547	0.398	0.228	0.097	0.906	0.591	0.656
Obs	11,235	11,235	11,235	11,235	11,235	11,235	11,235	11,235
R^2	0.83	0.83	0.85	0.82	0.91	0.81	0.80	0.84
Husbands:								
$Exposed_{o(h(i))} \times Post_t$	0.013	0.013	0.051***	-0.038**	0.004	-0.012***	0.006	-0.023*
	(0.016)	(0.016)	(0.011)	(0.018)	(0.003)	(0.004)	(0.011)	(0.014)
$Exposed_{o(h(i))} \times Post_t \times During_t$	-0.016	-0.012	0.049***	-0.061**	-0.003	-0.000	0.023**	0.010
	(0.021)	(0.022)	(0.018)	(0.025)	(0.004)	(0.004)	(0.011)	(0.014)
Mean pre-treatment	0.317	0.309	0.028	0.280	0.007	0.029	0.350	0.116
$\beta_1 + \beta_2 = 0$ (p-val)	0.859	0.985	0.000	0.000	0.882	0.001	0.005	0.320
Obs	11,440	11,440	11,440	11,440	11,440	11,440	11,440	11,440
R^2	0.84	0.84	0.85	0.85	0.79	0.83	0.82	0.82

	Time Spent in Household Production							
	Housework	Cooking	Shopping	Primary Childcaring	Secondary CC while working	Total Secondary CC	Total Childcaring	Prop. Secondary CC in Total CC
Wives:								
$Exposed_{o(h(i))} \times Post_t$	-0.007	-0.006	-0.006**	-0.025**	0.012	0.011	-0.014	0.060**
	(0.008)	(0.005)	(0.003)	(0.011)	(0.007)	(0.020)	(0.020)	(0.030)
$Exposed_{o(h(i))} \times Post_t \times During_t$	0.009	0.008	0.004	0.050***	-0.022**	-0.005	0.045*	-0.064**
	(0.011)	(0.006)	(0.003)	(0.013)	(0.011)	(0.022)	(0.025)	(0.032)
Mean pre-treatment	0.093	0.045	0.016	0.106	0.016	0.228	0.334	0.645
$\beta_1 + \beta_2 = 0$ (p-val)	0.831	0.625	0.519	0.027	0.363	0.786	0.190	0.882
Obs	11,235	11,235	11,235	11,235	11,235	11,235	11,235	11,046
R^2	0.84	0.85	0.82	0.82	0.83	0.82	0.82	0.83
Husbands:								
$Exposed_{o(h(i))} \times Post_t$	0.001	0.002	-0.002	0.000	0.015**	0.011	0.012	-0.053
	(0.008)	(0.003)	(0.002)	(0.008)	(0.007)	(0.018)	(0.019)	(0.044)
$Exposed_{o(h(i))} \times Post_t \times During_t$	0.001	-0.006*	0.004	0.004	0.022	0.019	0.023	0.035
	(0.011)	(0.003)	(0.003)	(0.007)	(0.013)	(0.029)	(0.029)	(0.047)
Mean pre-treatment	0.041	0.016	0.007	0.051	0.008	0.127	0.177	0.649
$\beta_1 + \beta_2 = 0$ (p-val)	0.811	0.125	0.318	0.531	0.004	0.223	0.142	0.713
Obs	11,440	11,440	11,440	11,440	11,440	11,440	11,440	10,585
R^2	0.83	0.88	0.85	0.84	0.80	0.80	0.81	0.83

Notes: We use data from the the 2017-2024 ATUS panel datasets. We show results for the time spent in labor, household and personal activities available in ATUS. Time-use variables are recorded by one member in the household. Then, as Section 2 points out, we have two draws from ATUS: the men-respondent sample and the women-respondent sample. While the former has data on the time spent doing activities by husbands, the latter will have for wives. There is a first panel that has dependent variables that capture time spent in labor production, as a share of the whole day: working, working at their main work, working at their main work from home and outside home, working at their secondary work and commuting to work. Those that do not work should have a 0 imputed in all working variables. There is a panel with dependent variables related to personal care (i.e. sleeping or taking a shower) and leisure (ie. going to religious events or sport activities). Lastly there is a household panel with dependent variables that capture the time spent doing the following activities: housework, cooking, shopping, primary childcaring (report doing childcaring as main activity), childcaring while working, total childcaring as secondary activity (childcaring while doing primary activities), total childcaring (the sum of primary and secondary childcaring) and the share of secondary childcaring on total childcaring. We restrict our sample to weekdays and non-holiday days. All regressions include fixed effects for state-by-time and husband's occupation. Controls include the wife's age, education, and number of children, as well as the husband's education. Standard errors are clustered at the occupation level.

Table B.34: Effect of Husbands' WFH Occupation Shocks on Childcare Time and Child-Related Activities

	Childcare by Wife while Working	Childcare by Spouse while Wife Works	Childcare by Grandparent	Childcare by Relative	Childcare by Non-relative	Childcare by Daycare	Paid for Childcare	Log Childcare Expenditure
A. Entire sample								
$Exposed_{o(h(i))} \times Post_t$	0.004	0.064*	-0.001	0.023	-0.009	-0.053*	-0.073**	-0.165
	(0.032)	(0.036)	(0.031)	(0.032)	(0.021)	(0.028)	(0.031)	(0.117)
Mean pre-treatment	0.330	0.485	0.416	0.454	0.186	0.266	0.484	4.710
Obs	60,264	60,264	85,379	85,379	85,379	85,379	68,047	32,910
R^2	0.15	0.16	0.15	0.14	0.13	0.18	0.19	0.33

Notes: We use data from the the 2018-2024 SIPP panel datasets. We estimate our baseline cross-section specification which has state-by-month-year and spouse occupation fixed effects. We show results for childcare variables available in the SIPP, which ask households with minor of age children if in a typical week, the following are involved at least one day in looking after children: The wife and her spouse (when the wife is working), a grandparent, another relative (includes grandparent), a daycare. We also include a dummy on whether the family pays for daycare, and self-reported log expenditure in childcare. All regressions include fixed effects for state-by-time, and husband's occupation. Controls include the wife's age, education, and number of children, as well as the husband's education. Standard errors are clustered at the husband's occupation level.

Table B.35: Effect of Husbands' WFH Shock on Child Well-Being and Activities

	Days wife has dinner with child	Days spouse has dinner with child	Days wife takes child on outings	Days spouse takes child on outings	Child is in club/activity after school	Child takes lessons after school	Child plays sports in/out of school
A. Entire sample							
$Exposed_{o(H)} \times Post_t$	-0.029	0.082	0.175	0.080	0.056	0.042	-0.085
	(0.086)	(0.180)	(0.234)	(0.245)	(0.059)	(0.062)	(0.060)
Mean pre-treatment	6.532	5.817	3.533	2.677	0.353	0.486	0.535
Obs	44,620	44,620	31,873	31,873	27,705	27,705	27,705
R^2	0.18	0.23	0.25	0.24	0.27	0.32	0.30

Notes: We use data from the the 2018-2024 SIPP panel datasets. We estimate our baseline cross-section specification which has state-by-month-year and spouse occupation fixed effects. We show results for childcare variables available in the SIPP, which ask households with minor of age children if in a typical week, the following are involved at least one day in looking after children: The wife and her spouse (when the wife is working), a grandparent, another relative (includes grandparent), a daycare. We also include a dummy on whether the family pays for daycare, and self-reported log expenditure in childcare.

Table B.36: Mean and SD from CPS Compared to Women-Respondent and Men-Respondent in the ATUS Sample

	CPS Sample		ATUS Women-Resp Sample		ATUS Men-Resp Sample	
	Mean	SD	Mean	SD	Mean	SD
Panel A: Husband Characteristics						
Husband In Shocked Occupation (Treatment)	0.51	0.50	0.56	0.50	0.61	0.49
Husband Employed (Last Year)	0.91	0.29	0.92	0.27	0.93	0.25
Husband Weekly Wage	1716.38	1444.07	1848.99	1235.62	1521.41	1686.14
Husband Wage	50893.82	60012.17	57051.88	67951.30	57558.83	55853.45
Husband Usual Work Hours	43.51	9.35	43.88	9.40	43.62	9.05
Husband Part-time (Last Year)	0.03	0.18	0.03	0.16	0.04	0.19
Husband - Below HS Level	0.08	0.27	0.05	0.22	0.04	0.21
Husband - HS Level	0.23	0.42	0.18	0.38	0.15	0.36
Husband - Above HS Level	0.69	0.46	0.77	0.42	0.81	0.40
Husband Age	37.67	6.82	37.92	6.33	37.93	6.43
Panel B: Wife Characteristics						
Wife Employed (Last Year)	0.67	0.47	0.68	0.47	0.71	0.45
Wife Weekly Wage	2249.58	3613.50	1712.12	1970.28	2820.38	4708.61
Wife Wage	25180.64	40157.91	28400.31	43506.77	29172.28	50473.90
Wife Usual Work Hours	37.13	10.58	37.44	11.03	36.79	10.83
Wife Part-time (Last Year)	0.22	0.42	0.26	0.44	0.23	0.42
Wife - Below HS Level	0.06	0.24	0.05	0.21	0.03	0.18
Wife - HS Level	0.17	0.38	0.11	0.32	0.10	0.30
Wife - Above HS Level	0.76	0.42	0.84	0.37	0.86	0.34
Wife Age	35.23	5.93	35.52	5.35	35.84	5.50
Panel C: HH Characteristics						
HH Wage	76074.47	77836.21	85452.19	83882.00	86731.10	77692.49
Number of own children in household	2.08	0.89	2.01	0.86	2.01	0.84
Age of Youngest Child	3.48	2.54	3.39	2.54	3.47	2.55
Age of Oldest Child	7.54	5.10	6.89	4.54	6.87	4.54
Youngest Child 0-4	0.64	0.48	0.66	0.47	0.65	0.48
Youngest Child 5-8	0.36	0.48	0.34	0.47	0.35	0.48

Notes: The table reports mean and standard deviations between the different samples: the benchmark CPS sample from the main findings and the ATUS samples. There are two ATUS samples: women respondents sample and men respondents sample. The former is when wives are those that report their time diary of their activities, while the latter is when husbands report them. Panel A captures variables related to husband characteristics, Panel B is related to wives characteristics and Panel C are related to household variables.

Table B.37: Effect of Husbands' WFH Shock on Wives' ATUS Labor Market Outcomes

	Women-Reported Sample					*Men-Reported Sample*				
	Employed	Log Weekly Wages	Log Wages	Weekly Working Hours	Part-time Status	Employed	Log Weekly Wages	Log Wages	Weekly Working Hours	Part-time Status
$Exposed_{o(h(i))} \times Post_t$	0.047	-0.054	0.085	4.404**	-0.117*	0.073	-0.134	0.017	1.962	-0.046
	(0.060)	(0.079)	(0.105)	(1.883)	(0.064)	(0.057)	(0.128)	(0.122)	(1.539)	(0.058)
Mean pre-treatment	0.722	6.667	10.160	35.894	0.300	0.666	6.739	10.266	35.517	0.294
Obs	7,989	5,052	5,274	5,476	5,780	8,175	4,687	4,831	5,506	5,688
R^2	0.84	0.94	0.93	0.89	0.90	0.85	0.94	0.95	0.91	0.92

Notes: We use data from the the 2017-2024 ATUS panel datasets. Time-use variables are recorded by one member in the household. Then, as Section 2 points out, we have two draws from ATUS: the men-respondent sample and the women-respondent sample. We use Labor Market Outcomes of wives from ATUS data, different from the ones from the CPS surveys. While in the women-respondent sample, wives report their own labor outcomes, in the men-respondent, their spouses respond for them. All regressions include fixed effects for state-by-time and husband's occupation. Controls include the wife's age, education, and number of children, as well as the husband's education. Standard errors are clustered at the husband's occupation level.

Table B.38: Effect of Husbands' WFH Shock on Time Spent in Labor and Household Supply - Share of the Day (Placebo Timing)

	Time Spent in Labor Production						*Time Spent for Personal Care and Leisure*	
	Working	Main Work	Main Work From Home	Main Work Outside Home	Secondary Work	Commuting	Personal Care	Leisure
Wives:								
$Exposed_{o(h)} \times Post_t$	-0.024	-0.018	-0.003	-0.015	-0.001	-0.006	-0.008	-0.013
	(0.025)	(0.025)	(0.015)	(0.030)	(0.002)	(0.005)	(0.012)	(0.018)
Mean pre-treatment	0.166	0.165	0.025	0.140	0.001	0.013	0.373	0.108
Obs	4,488	4,488	4,488	4,488	4,488	4,488	4,488	4,488
R^2	0.86	0.86	0.88	0.85	0.86	0.84	0.87	0.88
Husbands:								
$Exposed_{o(h)} \times Post_t$	-0.024	-0.025	0.004	-0.029	0.002	-0.010	0.014	0.031
	(0.032)	(0.030)	(0.015)	(0.032)	(0.007)	(0.008)	(0.014)	(0.026)
Mean pre-treatment	0.324	0.312	0.026	0.286	0.009	0.036	0.353	0.113
Obs	4,483	4,483	4,483	4,483	4,483	4,483	4,483	4,483
R^2	0.88	0.88	0.85	0.87	0.86	0.83	0.87	0.86

	Time Spent in Household Production							
	Housework	Cooking	Shopping	Primary Childcaring	Secondary CC while working	Total Secondary CC	Total Childcaring	Prop. Secondary CC in Total CC
Wives:								
$Exposed_{o(h)} \times Post_t$	0.023	0.011	-0.007	0.027	0.062	-0.001	0.026	-0.037
	(0.016)	(0.007)	(0.004)	(0.019)	(0.097)	(0.032)	(0.034)	(0.045)
Mean pre-treatment	0.097	0.047	0.017	0.110	0.104	0.237	0.347	0.641
Obs	4,488	4,488	4,488	4,488	2,605	4,488	4,488	4,407
R^2	0.89	0.86	0.85	0.85	0.93	0.87	0.86	0.86
Husbands:								
$Exposed_{o(h)} \times Post_t$	-0.012	-0.002	0.003	0.010	-0.031	0.023	0.033	0.026
	(0.010)	(0.005)	(0.003)	(0.012)	(0.055)	(0.039)	(0.045)	(0.059)
Mean pre-treatment	0.035	0.014	0.007	0.051	0.031	0.115	0.166	0.629
Obs	4,483	4,483	4,483	4,483	3,932	4,483	4,483	4,122
R^2	0.86	0.84	0.88	0.84	0.84	0.85	0.85	0.86

Notes: We use data from the the 2017-2024 ATUS panel datasets. We show results for the time spent in labor, household and personal activities available in ATUS. Time-use variables are recorded by one member in the household. Then, as Section 2 points out, we have two draws from ATUS: the men-respondent sample and the women-respondent sample. While the former has data on the time spent doing activities by husbands, the latter will have for wives. There is a first panel that has data on the time spent in labor production, capturing the share of the whole day - over 24 hours - that respondents spent: working, working at their main work, working at their main work from home and outside home, working at their secondary work and commuting to work. There is a panel on personal care (i.e. sleeping or taking a shower) and leisure (ie. going to religious events or sport activities). Lastly there is household panel with the time spent doing the following activities: housework, cooking, shopping, primary childcaring (report doing childcaring as main activity), childcaring while working, total childcaring as secondary activity while doing other activities, total childcaring (the sum of primary and secondary childcaring) and the share of secondary childcaring on total childcaring. We restrict our sample to weekdays and non-holiday days. All regressions include fixed effects for state-by-time and husband's occupation. Controls include the wife's age, education, and number of children, as well as the husband's education. Standard errors are clustered at the husband's occupation level. We exploit a fake timing as in Table B.30 by moving the arrival of the pandemic shock to March 2018.

C Discussion of Identification Strategy

Our empirical design estimates the causal effect of an increase in the husband's WFH probability on the wife's labor market outcomes. A key data limitation, however, is that in many surveys – including the ACS – occupational information is observed only for individuals currently employed. This constraint implies two challenges. First, we cannot systematically observe a wife's own occupation when she is not employed, preventing us from measuring her WFH exposure during non-employment spells. Second, because women's own exposure is unobserved for part of the sample, our baseline estimates necessarily combine the *spillover* effect arising from husbands' WFH exposure with the *direct* effect operating through women's own WFH exposure, which in the model is generated by assortative matching, as explained in Section 3.

To illustrate the implications, consider a simplified version of our baseline specification:

$$y_{it} = \alpha + \beta_0 D^{o(h(i))} + \gamma D_t + \beta_h D^{o(h(i))} \times D_t + u_{it}, \tag{4}$$

where $D^{o(h(i))} = 1$ if the husband of woman i is in a treated occupation (i.e., one with a large WFH shock), and $D_t = 1$ in post-COVID years.

Suppose that unobserved variation in the wife's own WFH exposure enters the error term as:

$$u_{it} = \beta_1 D^{o(i)} + \beta_w D^{o(i)} \times D_t + \beta_2 D^{o(i)} \times D^{o(h(i))} + \beta_{hw} D^{o(i)} \times D^{o(h(i))} \times D_t + \varepsilon_{it}, \tag{5}$$

where $D^{o(i)} = 1$ if the wife herself works in a treated occupation.

Even if we assume the cross-occupation interaction terms β_2 and β_{hw} are zero, the difference-in-differences estimator $\hat{\beta}_h$ from (4) identifies:

$$\begin{aligned}\hat{\beta}_h = \beta_h + \beta_1 \cdot \Big[& \underbrace{P(D^{o(i)} = 1 \mid D^{o(h)} = 1, \text{Post}) - P(D^{o(i)} = 1 \mid D^{o(h)} = 1, \text{Pre})}_{\Delta P(D^{o(i)}=1 \mid D^{o(h)}=1)} \\ & - \underbrace{\big(P(D^{o(i)} = 1 \mid D^{o(h)} = 0, \text{Post}) - P(D^{o(i)} = 1 \mid D^{o(h)} = 0, \text{Pre})\big)}_{\Delta P(D^{o(i)}=1 \mid D^{o(h)}=0)} \Big] \\ & - \beta_w \cdot \Big(P(D^{o(i)} = 1 \mid D^{o(h)} = 1, \text{Post}) - P(D^{o(i)} = 1 \mid D^{o(h)} = 0, \text{Post}) \Big).\end{aligned} \tag{6}$$

Even under the assumption that assortative matching is stable before and after COVID—i.e.,

$$\Delta P(D^{o(i)} = 1 \mid D^{o(h)} = 1) = \Delta P(D^{o(i)} = 1 \mid D^{o(h)} = 0) = 0,$$

the estimator $\hat{\beta}_h$ will still load on the direct effect β_w through differences in wives' occupational exposure across treated and untreated husbands in the post period.

Section 5 presents several exercises designed to separate these two components and quantify the relative contributions of *spillover* and *direct* effects.