Remote Work and Child Penalties

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Abstract

This paper studies how remote work affects child penalties in employment, using pseudo-event studies around childbirth in four Latin American countries. Using complementary empirical strategies at the individual, household, and regional level, I find that remote work reduces the child penalty by about 4 percentage points, relative to a baseline penalty of nearly 17 points. These effects operate through three main channels: (i) women in occupations that can be done from home are more likely to remain in their pre-child job; (ii) recent mothers are more likely to transition into remote work; (iii) women whose partners can work from home are more likely to remain employed. The post-pandemic increase in remote work brought higher-quality (formal, salaried and higher-paying) jobs, making these effects stronger. These findings suggest that policies supporting access to remote work may help reduce gender gaps in labor outcomes.

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1 Introduction

Gender inequality in labor market outcomes widens by the arrival of the first child, a phenomenon known as the child penalty (Kleven et al., 2019, 2024). A key driver of this penalty is that women disproportionately bear the burden of childcare, prompting many mothers to seek more flexible work arrangements, such as part-time jobs, self-employment, or informal work (Berniell et al., 2021; Kleven et al., 2019). In recent years, remote work—or work-from-home (WFH)—has emerged as an increasingly popular form of workplace flexibility, especially following the COVID-19 pandemic (Aksoy et al., 2022, 2023b; Barrero et al., 2021). This raises the question of whether remote work can help mitigate child penalties. While the flexibility of WFH may help mothers stay employed, thus supporting participation at the extensive margin, it may also lead them to select into lower-paying or more unstable remote jobs, potentially widening the gender gap.

In this paper, I examine how remote work affects child penalties using pseudo-event studies around childbirth in four Latin American countries (Argentina, Brazil, Chile and Colombia). These countries provide an ideal setting to study this question, as the continent has among the largest child penalties globally (Kleven et al., 2024) and remote work rates close to the global average (Aksoy et al., 2025). Using complementary empirical strategies at the individual, household, and regional level, I find that remote work reduces child penalties in employment by approximately 4 percentage points, relative to a baseline penalty of nearly 17 points. These effects operate through three main channels: (i) women in occupations that can be done from home are more likely to remain in their pre-child jobs; (ii) recent mothers are more likely to transition into remote work; and (iii) women whose partners can work from home are more likely to remain employed, suggesting that spouse's flexibility matters. Moreover, when focusing on the COVID-19-induced rise in remote work, I find that mothers increasingly shifted into remote jobs in formal or salaried employment, whereas before the pandemic, this transition primarily involved informal or self-employed remote work.

Estimating the impact of remote work on child penalties poses an empirical challenge due to the limited availability of long-term panel data tracking individuals around childbirth. I address this challenge by implementing a pseudo-event study approach, following the methodology of Kleven (2025). The idea of the approach is to construct a pseudo-panel at the individual level from repeated cross-sectional surveys by matching recent parents to observationally identical non-parents surveyed in earlier years. These matched non-parents resemble the true parents before childbirth, had they been observed. I then use the pseudo-panel to estimate event studies around the first childbirth. This pseudo-event study approach has been used and strongly validated against true panel estimates (Kleven, 2025; Kleven et al., 2024; Marchionni & Pedrazzi, 2023). I further validate this approach using panel data from Brazil.

Regarding the role of remote work in shaping child penalties, I exploit differences in the potential to work from home across occupations, based on Dingel and Neiman (2020) classification. I find evidence that remote work flexibility enables mothers to remain in their pre-child jobs. Women who were in occupations that can be done remotely before childbirth are more likely to remain employed after motherhood, and are more likely to remain in their pre-child jobs. Instead, women in non-WFH occupations are more likely to stop working or to switch jobs after childbirth.

Moving to the demand for remote work after childbirth, I find that recent mothers increasingly transition into remote arrangements. I leverage a survey question on respondents' primary place of work to measure actual WFH status, and find that women are 4.4 percentage points more likely to work remotely after childbirth, relative to men. Before the pandemic, these shifts mainly occurred through self-employment, informal work, and part-time remote positions. After the pandemic, on the other hand, the shift into WFH is driven largely by formal and salaried positions. This suggests that the recent surge in remote work improved mothers' access to higher-quality flexible jobs.

I then switch the empirical analysis from the individual to the household level to examine whether partners' job flexibility plays a relevant role in shaping child penalties. If fathers are able to work remotely, it may ease the childcare burden on mothers, and facilitate their return to work. I find that women whose partners can WFH are 4.4 pp more likely to remain employed after childbirth than those whose partners cannot, with this effect increasing after the pandemic. Moreover, women whose partners cannot WFH are more likely to transition into remote or flexible jobs themselves, suggesting important household complementarities in household labor supply decisions.

Beyond these individual and household-level mechanisms, I also document descriptive patterns in the prevalence and selection of remote work. I find a persistent increase in remote work across all four countries after 2020, along with a compositional change of remote workers. Before the pandemic, about 6.5% of workers primarily worked from home. These remote workers were more likely to work part-time, as self-employed, and in lower wage jobs. After conditioning on observables, they earned 20 log points less per hour than on-site workers. Following the COVID-19 pandemic, the share of workers WFH permanently rose by 4.4 percentage points, and remote jobs increasingly shifted toward formal and salaried positions. The hourly wage gap between remote and non-remote workers disappeared, suggesting a significant change in the composition of remote workers.

Finally, I leverage geographic variation in WFH potential across states—the largest subnational units in these countries—and the COVID-19-driven increase in remote work. Using state-level child penalties, I find that child penalties in employment decreased in regions where remote work increased more. Instrumenting for changes in WFH levels using the pre-pandemic share of jobs able to be done from home in the state, I find that moving from the 25th to the 75th percentile of remote work growth corresponds to a 3.85 pp reduction in the child penalty. A falsification test repeating the exercise within the pre-pandemic period—when there were no aggregate increases in remote work—yields no significant relationship between remote work and child penalties.

The results of this paper have potential policy implications. The role of remote work is particularly pressing now, as many firms and governments are scaling back WFH policies, requiring employees to return to the office (Ding & Ma, 2023; Flynn et al., 2024; Van Dijcke et al., 2024). My results suggest that such return-to-office mandates may widen gender gaps in labor market outcomes. Moreover, since household childcare and labor supply decisions are jointly determined, restricting the father's flexibility may have spillover effects on the mother's labor supply. More broadly, allowing mothers to work from home may lower the cost of raising children, easing some of the constraints contributing to declining fertility rates.

This paper contributes to several strands of the literature. First, it builds on the extensive work on child penalties (Adda et al., 2017; Andresen & Nix, 2022; Berniell et al., 2021; Kleven, 2025; Kleven et al., 2019, 2021, 2024; Lundborg et al., 2017) and gender inequality in the labor market (Antecol et al., 2018; Bertrand, 2020; Bertrand et al., 2010; Blau & Kahn, 2017; Goldin, 2014; Goldin & Katz, 2016; Kuziemko et al., 2018). A central insight from this literature is that large and persistent gender gaps in employment, earnings and hours emerge following the first childbirth, partly driven by transitions into more flexible work arrangements. I contribute by isolating the role of remote work, a more recent and potentially higher-quality form of flexibility, in shaping child penalties. I show that the option to WFH allows women to remain in their pre-child jobs, and that partner flexibility plays a relevant role, factors which are less studied in previous works.

Second, this paper adds to the growing literature on remote work and its labor market consequences (Adams-Prassl et al., 2022; Aksoy et al., 2022, 2023a; Alipour et al., 2021; Barrero et al., 2021; Barrero et al., 2023; Bloom et al., 2015; Davis et al., 2024; Dingel & Neiman, 2020; Emanuel & Harrington, 2024; Emanuel et al., 2023; Gibbs et al., 2023; Lewandowski et al., 2023; Pabilonia & Vernon, 2025; Zarate et al., 2024). Prior studies have highlighted the potential gains and losses associated with remote work, and its impact on productivity, time usage, workers' attrition and promotion in employment, as well as shifts in worker preferences for WFH practices. I contribute by documenting the evolution of work from home using harmonized survey data across countries and years. Specifically, I show how the composition of remote workers has changed substantially over time.

Finally, this paper contributes to the literature on gender differences in remote work preferences and outcomes (Aksoy et al., 2022; Berniell et al., 2023; Costoya et al., 2022; Drake et al., 2022; Emanuel & Harrington, 2024; Harrington & Kahn, 2023; Mas & Pallais, 2017; Nagler et al., 2024; Pabilonia & Vernon, 2022; Song, 2025; Tito, 2024; Wiswall & Zafar, 2018). Evidence from surveys and experiments consistently shows that women value the flexibility of WFH more than men and are more likely to select into remote arrangements. Moreover, recent studies highlight the important role of remote work in boosting female labor force participation in developed and developing countries (Ho et al., 2024; Jalota & Ho, 2024; Tito, 2024). I extend this literature by providing new evidence that women are more likely to transition into remote work after childbirth, with a considerable shift toward higher-quality (formal and salaried) remote jobs after the pandemic.

The paper is organized as follows. Section 2 describes the data and the empirical strategy. Section 3 examines remote work practices. Section 4 presents the main results and the mechanisms. Section 5 assesses the robustness of the results, and Section 6 concludes.

2 Data and Empirical Strategy

2.1 Data

I use nationally representative household surveys from Argentina, Brazil, Chile, and Colombia spanning 2010 to 2023, which are primarily repeated cross-sections. I restrict the sample to household heads and their partners, aged 25 to 45 at the time of their first childbirth. Following the literature, I infer the timing of first childbirth by computing the age of the oldest child in the household.

The surveys report respondents' employment status, weekly hours worked, and monthly earnings. To enable comparisons across countries and over time, I convert earnings into 2010 PPP-adjusted US dollars. I approximate the hourly wage rate by dividing monthly earnings by four times the weekly hours worked. Data Appendix A provides more details about the data and other variable definitions.

In addition to standard labor force information, the surveys contain two key remote work measures: (i) the potential to work from home (WFH), and (ii) actual WFH status. I measure WFH potential using Dingel and Neiman (2020) classification of whether an occupation can be performed remotely, which has been widely used and validated as a strong predictor of remote work (Bartik et al., 2020; Bick et al., 2023). At the 2-digit ISCO occupation level, this measure is a continuous variable from 0 to 1, which captures the share of workers in a given occupation who could WFH, based on their tasks. I refer to this measure as WFH potential.

Actual WFH status is derived from a survey question about the respondent's primary place of work, resulting in an indicator equal to 1 if the respondent primarily works from home, and 0 for any other workplaces. The wording of the question and answer options are largely consistent across countries, with minor variations. Appendix Table A.1 presents the question text and WFH classification. Importantly, unless otherwise noted, I exclude 2020 responses from the analysis due to known issues with sampling and response quality during the COVID-19 pandemic. Additionally, in Brazil, data on primary place of work is only available from 2018 onward.

The actual WFH status measure presents two notable features. First, it captures fully or primarily WFH workers—those working from home most or all the time—but may undercount hybrid workers, who share similar flexibility but opt to WFH only occasionally. Therefore, it likely understates the true prevalence of remote work, and should be interpreted as a lower bound on WFH uptake. Second, many remote workers in this setting may be self-employed or freelance workers for overseas clients (Cui & Solomon, 2024). For this reason, I include all employment types in the analysis: salaried workers, self-employed, and others.

Brazilian Short-Run Panel A key limitation of the data is the lack of a panel structure. To address this limitation, I leverage the rotating panel design of Brazil's PNAD-C, which interviews households for five consecutive quarters. I use the version by Mittelbach and Gonzaga (2020), which applies the linkage strategy from Ribas and Soares (2008) to track individuals within households, as made available by the Datazoom project.

I apply the same sample restrictions and focus on household heads and their partners, aged 25 to 45 at the time of their first childbirth. PNAD-C provides exact birth dates for children, which I use to determine the quarter of childbirth. I restrict the sample to respondents observed at least one quarter before the event. Given the survey structure, these restrictions yield an unbalanced panel spanning four quarters before to three quarters after childbirth. To measure WFH potential in Brazil, I use the adaptation of Dingel and Neiman (2020) classification to the Brazilian occupational structure developed by Góes et al. (2020), resulting in a binary indicator of whether a job can be done from home.

2.2 Empirical Strategy

A common empirical strategy in the child penalty literature involves estimating an event study around the first childbirth, employing panel data and the sharp variation around childbirth within an individual (Kleven et al., 2019). The main identification assumption of this event study strategy is that the timing of the first childbirth is uncorrelated with labor market outcomes, conditional on the set of controls.

A key empirical challenge of this approach is the limited availability of long-term panel data tracking individuals around childbirth, which is particularly salient in the context of developing countries. In these regions, the large informal labor market is not registered in any administrative dataset. To overcome this, I implement a pseudo-event study around childbirth, following the methodology of Kleven (2025) and Kleven et al. (2024), which construct a pseudo-panel from repeated cross-sectional surveys by matching recent parents with observationally similar non-parents in earlier years.

The main idea behind this methodology is to consider a recent parent and match them to a childless individual who is identical in observables to the true parent, had they been observed in previous surveys before childbirth. Thus, the matched non-parent is identical in observable characteristics to the actual parent. The pseudo-panel at the individual level is then used to estimate an event study around the first childbirth.

Formally, let τ denote the relative event period to the first childbirth, with $\tau = 0$ denoting the birth year of the first child. For a parent i of age a in calendar year y with characteristics X_i, I match a childless individual of age $a - n$ in year $y - n$ who has the same observables X_i. The matched non-parent is then assigned $\tau = -n$. For example, a 30 year old women observed in 2019 who had her first child that year will be matched to a 29 year old women in 2018, provided they have the same characteristics, and the latter will get assigned $\tau = -1$. I match up to five years prior to childbirth ($n = 5$), assigning equal weight to each match.[1]

By construction, the matched non-parent is identical in observables to the parent prior to childbirth. Following previous works using this methodology, I match based on age (as described previously), gender, education level (incomplete elementary, incomplete secondary, secondary or college), state of residence (the largest subnational division in the country), and marital or partner status (single or living with partner).

The described approach results in a pseudo-panel at the individual level, with relative event periods τ imputed for the surrogate parents. The pseudo-panel approach has been widely used and validated, producing point estimates that are nearly identical to those from a traditional panel event study, and even more precise (Kleven, 2025; Marchionni & Pedrazzi, 2023). I later discuss and show the validity of this approach to a proper event study in this context.

Pseudo-event study The previous approach yielded a pseudo-panel, which can be used to estimate a pseudo-event study around childbirth, following Kleven (2025). Formally, I estimate the following specification:

$$Y^{g}_{i,t} = \underbrace{\boldsymbol{\beta}^{g} \cdot \mathbf{D}^{\text{Event}}_{i,t}}_{\text{Event period dummies}} + \underbrace{\delta^{\text{Age}}_{g}}_{\text{Age FE}} + \underbrace{\gamma^{\text{Year}}_{g}}_{\text{Calendar Year FE}} + \underbrace{\boldsymbol{\alpha}^{c}_{g}}_{\text{Country FE}} + \varepsilon^{g}_{i,t} \tag{1}$$

Where $\mathbf{D}_{i,t}$ denotes the set of relative event period dummies for τ, omitting $\tau = -2$. Equation (1) is estimated separately for women and men ($g = w, m$), and controls non-

[1] If there are K matched non-parents, I assign the negative event period to all K and adjust the survey weights by dividing it by K.

parametrically for lifecycle and year trends by including age, calendar year, and country fixed effects.

One notable difference of the pseudo-event study framework is that this approach results in a sample which can be interpreted as eventually-treated respondents.[2] Thus, the set of parameters $\boldsymbol{\beta}^g$ recovers the average value of the outcome at an event period for gender g, relative to its pre-childbirth value at $\tau = -2$. The specification in equation (1) allows the estimation of immediate ($\tau = 0$) and medium-run differences ($\tau = 5$).

Difference-in-difference design I also consider a difference-in-difference design of equation (1) to obtain the average treatment effect, where I compare women and men before and after childbirth. Previous works have consistently found the absense of an effect of childbirth on men, where becoming a father is a "non-event" (Berniell et al., 2021; Kleven et al., 2019), which motivates the use of men as a control group in a difference-in-difference design. Note that a main identification assumption is the parallel trends between men and women in the absence of childbirth.

Formally, this consists of estimating the following specification, where $Post_{i,t}$ is an indicator equal to 1 after childbirth (when $\tau \geq 0$):

$$Y^g_{i,t} = \alpha \cdot Post_{i,t} + \beta \cdot Post_{i,t} \cdot Women_i + \delta^{\text{Age}}_g + \gamma^{\text{Year}}_g + \boldsymbol{\alpha}^{\text{Country}}_g + \varepsilon^g_{i,t} \tag{2}$$

Additionally, in several specifications, I condition on specific subsamples or based on observable characteristics (e.g., employed individuals, in certain occupations, or those primarily WFH). These groupings are based on their status as observed when surveyed. Empirically, I do this by interacting the event-period dummies from equation (1) or the differences-in-differences term from equation (2) with the corresponding indicator.

Threats to identification The pseudo-event study methodology raises two main identification concerns. First, standard threats related to event studies around childbirth. Second, potential biases from constructing the pseudo-panel by matching non-parents to parents.

The main assumption of an event study is that the timing of childbirth is uncorrelated with labor market outcomes. If mothers time childbirth strategically (e.g., after a promotion), estimates may be biased. While this cannot be completely ruled out, existing work has tested this by assigning placebo births to never-parents (Kleven et al., 2019), or using fertility shocks

[2] Kleven (2025) discusses formally the identification assumptions under which the pseudo-event study framework provides causal estimates of the child penalty.

such as IVF treatments (Lundborg et al., 2017), and obtaining similar estimates. Moreover, the absence of pre-trends suggests there is no consistent selection into or out of employment right before childbirth, supporting the validity of the assumption.

The pseudo-panel is built from repeated cross-sectional data. The underlying assumption is that, as the matched parents are identical in observable characteristics to the true parents had they been observed, they are similar in unobservable characteristics. As I show later, results using true panel data in Brazil closely mirror those from the pseudo-panel, validating the approach. Moreover, Kleven et al. (2024) compare estimates from the event study employing a pseudo-panel or an actual panel event study in eleven countries, finding very small differences between the two estimates.

Finally, the quality of the match will be affected by the set of characteristics used. More parsimonious matching (e.g., only by age and gender) may introduce selection bias (Kleven, 2025). As such, I follow the literature and match individuals using their age, gender, education level, state of residence, and marital or partner status.

Validation of the pseudo-event study The pseudo-event study assumes that matched non-parents are similar to future parents prior to childbirth, had they been observed at the time. This strategy can be validated using panel data by comparing estimates from the pseudo-event study to those from a true event study, using the same sample. When doing this exercise, previous studies have found that the panel event study estimates are numerically identical to the pseudo-panel event study estimates, in many countries (Kleven, 2025; Kleven et al., 2024; Marchionni & Pedrazzi, 2023).

Following this approach, I use the Brazilian panel to estimate both the pseudo- and actual event study around childbirth, and obtain similar estimates across these methods. There are two conceptual and empirical differences worth discussing between the two approaches. First, the pseudo-panel consists only of eventually-treated individuals, either the matched non-parents or the true parents. In the panel event study, in addition to eventually-treated, I include never-treated individuals, and assign them a placebo event as in Kleven et al. (2019). An additional difference is the time frame, as in the panel event study I replace yearly event dummies with quarterly ones, omitting the quarter before pregnancy (three quarters before birth).

Appendix Figure B.1 shows similar patterns for men and women across the two methods: a flat profile or non-event for men, and a decrease in employment for women. Appendix

Table C.1 compares the difference-in-difference estimates on employment, using the pseudo-panel and the short-panel samples. Column (1) shows that women in Brazil are 19.1 pp less likely to be employed post-childbirth, relative to men, in the pseudo-panel. Column (2) restricts the pseudo-panel to match the panel's observation window of at most one year before and after childbirth, resulting in a child penalty estimate of 18.54 pp. The short panel event study, in column 3, is slightly smaller (14.3 pp) but directionally and qualitatively very similar.

3 Remote Work

Before the COVID-19 pandemic, remote work was limited and relatively stable over time (Figure 1). Between 2010 and 2019, approximately 6.5% of workers reported to be primarily WFH, and this share remained constant across countries in this period.[3] The pandemic then acted as a large, exogenous shifter. Remote work peaked between 2020 and 2021, before stabilizing at a new equilibrium of approximately 10.9% —a 4.4 pp permanent increase. As this measure is based on self-reported primary place of work, it is a lower bound on the true shift in remote work practices, particularly for hybrid arrangements.

Appendix Figure B.2 shows WFH rates by country, gender, employment type, and education. Remote work has consistently been more prevalent among women. Prior to 2020, about 10% of women reported primarily WFH, compared to just the 3% of men. This reflects, in part, women's higher likelihood of working in occupations that can be done remotely.

In this context, WFH has not been primarily driven by salaried employees until recently. Panel (c) of the same figure shows that, pre-2020, WFH was concentrated among the self-employed: nearly 20% of self-employed workers reported working primarily from their home, compared to a negligible share of salaried workers. After the pandemic, WFH rose substantially, with about 5% of salaried employees now working primarily from home.

Before 2020, WFH was roughly equally distributed across education groups. Post-2020, WFH surged among college-educated workers, consistent with evidence from other settings (Aksoy et al., 2022; Barrero et al., 2021). At its peak, nearly 25% of college graduates worked primarily from home, compared to just 10% of workers with only elementary education.

[3] Panel (a) in Appendix Figure B.2 presents the time series from 2010, by country.

Differences in selection into WFH These patterns indicate substantial heterogeneity behind the aggregate WFH increase. In particular, the composition of remote workers has shifted significantly. Figure 2 documents differences in earnings, hours worked, and hourly wage rates between remote and non-remote workers. Prior to 2020, remote workers were more likely to work part-time, to be self-employed, and to earn lower wages. Even after controlling for observable demographic and job characteristics, remote workers earned about 20 log points less than non-remote workers (Appendix Figure B.3). This is consistent with negative selection into WFH prior to 2020, as also found in the US (Emanuel & Harrington, 2024).

After 2020, this gap narrows. The convergence is largely driven by increases in hours and earnings among remote workers, while outcomes for non-remote workers remain relatively unchanged. Although gaps in hours and earnings persist, the median wage rate is now identical across WFH and non-WFH workers. Overall, conditional on observables and even without them, there is no longer a difference between the wage rate of remote and non-remote workers.

Given that women were more likely to WFH, one might expect gendered differences in selection. Panel (b) and (c) of Appendix Figure B.3 show the regression of log hourly wage on a WFH indicator for women and men, separately, with and without controls. Among women, remote workers were employed in significantly lower-paying jobs before 2020 —even after controlling for occupational differences. The gap shrinks considerably after 2020 and becomes negligible when including controls. Among men, there were small or null differences in hourly wages between remote and non-remote men before 2020. After the pandemic, men working from home were in higher-paying roles, but these differences largely disappear after controlling for observables.

Taken together, these patterns suggest that remote workers were negatively selected prior to the pandemic: they were more likely to be self-employed, part-time and in low-paying roles. After 2020, selection into WFH shifts, still influenced by job characteristics, but now including more salaried, full-time, and better-paid workers.

4 Child Penalties

4.1 Child Penalties and Remote Work

Figure 3 and Table 1 present estimates of the child penalty at both the extensive and intensive margins. Figure 3(a) shows that, after childbirth, recent mothers are disproportionately and persistently more likely to stop working. Overall, childbirth has an immediate large negative effect on women's employment, and this effect persists over time. In contrast, becoming a father has minimal or null impact on men's labor market outcomes. This pattern, often referred to as a non-event for men, has been well-documented in both developed and developing countries (Aguilar-Gomez et al., 2022; Berniell et al., 2021; Kleven et al., 2019, 2024; Marchionni & Pedrazzi, 2023; Oaquim, 2022).

On average, I find a child penalty in employment of about 17 percentage points across the four countries in the sample. [4] The magnitude of the penalty is sizable and consistent with existing literature, though on the lower end of the distribution. For comparison, previous studies find a 22% penalty in Chile (Berniell et al., 2021) using panel data and an event study, a 34% penalty in Latin America using a pseudo-event study with household surveys (Marchionni & Pedrazzi, 2023), and a 37.8% penalty in Latin America using a pseudo-event study with census data (Kleven et al., 2024).

The difference can be explained by two factors. First, the estimand in this paper differs slightly from that used in prior work. While previous studies typically scale the event study estimates using a regression-based counterfactual—the average employment rate in the absence of a child—I report unscaled estimates throughout the paper. Second, I focus on a more recent time period, starting in 2010. Since child penalties in employment have declined over time (Kleven, 2025), more contemporaneous estimates are expected to be smaller.

Moving on to intensive margin responses, even among women who remain employed there is a child penalty in work hours. Panel (b) in Figure 3 shows that women are 10 pp more likely to work part-time after childbirth, among those who remain employed. For fathers, there is no change in their employment probability nor in their work hours.

The child penalty also materializes by mothers shifting towards lower-quality employment. Columns (3) and (4) in Table 1 show that, after childbirth, women are less likely to hold

[4] Country-specific estimates are of 21.62 pp in Argentina, 19.35 pp in Brazil, 15.41 pp in Chile and 10.72 pp in Colombia (see Table C.8)

formal jobs and more likely to be self-employed, patterns consistent with findings from previous studies (Aguilar-Gomez et al., 2022; Berniell et al., 2021; Marchionni & Pedrazzi, 2023). In the context of Latin America, these jobs are a source of flexible working arrangements, which can help mothers balance paid work with childcare responsibilities.

How COVID-19 affected child penalties The COVID-19 pandemic may have exacerbated existing inequality and affected women differently than men (Alon et al., 2020, 2022). To examine recent changes, I focus on the difference-in-difference design and interact the term with an indicator for respondents who became parents (or would have, if observed) after 2020. Panel B of Table 1 presents the results.

Relative to the pre-2020 period, the child penalty in employment slightly increased, as post-2020 mothers are 3.2 p.p. less likely to be employed than their pre-2020 counterparts. However, the intensive margin penalty declined slightly, as post-2020 working mothers are 1.8 pp less likely to be working part-time. Columns (3) and (4) show no statistically significant difference in employment type, as post-2020 mothers are equally likely to be working in formal, informal or self-employed as before. However, one key dimension that changed after the pandemic is the availability and nature of remote work.

Role of remote work To assess the role of remote work in shaping post-childbirth labor outcomes, I examine respondents' WFH status and their WFH potential (cols. 5-6 of Table 1, panels c-d of Figure 3). These outcomes are the probability of primarily working from home, and the WFH potential of the occupation, both conditional on employment.

After childbirth, the probability that mothers work remotely increases gradually, peaking about two years after childbirth, a pattern consistent with reentry into the labor force. Fathers, in contrast, show a slight but immediate decline in WFH probability. On average, women are 4.4 pp more likely than men to be WFH post-childbirth (Table 1, col. 5). The rise in remote work is even larger after 2020, as post-pandemic mothers are 2.44 pp more likely to work from home than those who gave birth earlier. Given that only about 7% of workers in the sample are working remotely, this increase substantial. Section 4.3 examines the growing demand for remote work in more detail.

There is also a modest increase of 2.7 pp in the probability, relative to men, that employed mothers are in occupations that can be done remotely (Table 1, col 6). The increase could be explained by either mothers sorting into WFH-compatible occupations, or by mothers in

WFH occupations being more likely to remain employed. Section 4.2 argues in greater detail that the latter mechanism, job continuity, is driving this pattern.

4.2 The Flexibility of Remote Work as a Mitigator of Child Penalties

Remote work flexibility may affect child penalties through two potential channels. First, the option to WFH may enable women to remain in their pre-childbirth job by making it easier to balance paid work and childcare. In this case, the post-childbirth increase in the probability of being in a WFH occupation among women (Figure 3, panel d) would then mechanically reflect employment continuity. Mothers without WFH-compatible jobs may be forced to stop working, whereas mothers who can WFH would remain at their job. Second, the option to WFH may be attractive after childbirth, prompting some women to switch occupations to gain flexibility. In that case, the increase would reflect occupational change.

To examine these mechanisms, I use two complementary approaches: (i) the Brazilian short panel, which allows conditioning on pre-childbirth occupation, and (ii) a proxy for job switching based on job tenure using the pseudo-panel.

In the Brazilian short panel, I define WFH occupation status based on whether the respondent was ever employed in such an occupation before childbirth, effectively restricting the sample to those employed before childbirth. For comparison, I assign placebo events to non-parents and classify them similarly based on their occupations before the placebo event.[5]

Panel (a) of Figure 4 shows the short-run child penalty in employment in Brazil by pre-child occupation. Women in non-WFH occupations experience an immediate and sizeable drop in employment. In contrast, the child penalty is much smaller for women in WFH occupations, a gap of about 5.5 pp (Appendix Table C.2). This suggests that mothers in WFH occupations are more likely to remain employed, while those in non-WFH occupations are less likely to be employed after childbirth.

However, the gap narrows one year after childbirth, raising the question of whether WFH flexibility mitigates the penalty permanently or simply delays it. To investigate this, I turn to the pseudo-panel and use job tenure as a proxy for job switching. Respondents are considered to have recently switched jobs if their tenure is less than two years.

[5]Results are robust to restricting the sample to those who became parents during the interview window (i.e., removing never-treated individuals), although doing so reduces sample size and statistical power.

Panel (b) of Figure 4 shows the probability of being in a recent job, by current occupation. While occupation is not an immutable trait, it tends to be persistent over time, making the current occupation a reasonable proxy for pre-child occupation in the pseudo-panel. Among men, childbirth has no impact on job tenure or job switching. Among women, tenure rises mechanically in the year of childbirth, likely reflecting maternity leave among those who remain employed.

Two years after childbirth, women in non-WFH occupations are significantly more likely to be in new jobs. In contrast, women in WFH occupations have a job switching behavior and tenure similar to their pre-childbirth levels, suggesting they are more likely to remain in the same job. If women were systematically switching into WFH occupations around childbirth, we would expect shorter tenure among those currently in WFH jobs. However, we observe the opposite, as women in WFH occupations are less likely to have recently switched jobs, implying they remained in their pre-birth jobs.

4.3 Workers' Demand for Remote Work

Women often shift to flexible work arrangements, such as part-time work, self-employment, or informal jobs in Latin America (Berniell et al., 2021; Oaquim, 2022). This prompts the question of whether recent mothers are also more likely to work from home.

Panel (c) of Figure 3 shows that women are indeed more likely to WFH after childbirth, with a gradual increase that peaks two years after the event. Overall, there is a 4.4 pp increase in WFH among men relative to men (Table 1, col 5). Given that only about 7% of workers in the sample report primarily working from home, this gender gap is sizable.

In contrast, men are *less* likely to WFH after becoming fathers, with an immediate and persistent decline. One possible explanation is that men shift to higher-paying jobs or increase their hours to meet household financial needs, especially if they become the primary earners. However, this explanation is not supported by the data: trends in hours worked and earnings for men who WFH and those who work on-site are very similar (Appendix Figures B.4).

An alternative explanation is intra-household specialization. After childbirth, recent mothers may take on more childcare and domestic responsibilities, while fathers specialize in production in the labor market. This explanation would lead women to spend more time at home and men to spend more time at their physical workplace, consistent with existing evidence on gender gaps in time spent on childcare.

Margins of Remote Work Remote work in this context often reflected lower-quality jobs, such as self-employment or informal work carried out from home. Table 2 decomposes the 4.4 pp post-childbirth WFH increase by employment type: part-time, self-employed, salaried, formal, or informal status.

Most of the post-childbirth WFH increase is driven by informal of self-employed remote work. Only about 0.5 pp is attributable to formal or salaried remote work (Panel A), suggesting that the observed flexibility is largely driven by lower-quality forms of employment.

However, the composition of remote work changes substantially after 2020. Mothers who became parents after 2020 are 2.4 pp more likely to WFH after childbirth compared to pre-2020 mothers. More importantly, the type of remote work also changed notably. After 2020, mothers are more likely to WFH in formal jobs or as salaried employees. While self-employment and part-time work still account for part of the increase, the post-COVID expansion of remote work enabled greater access to flexible, but also higher-quality, employment. These findings align with previous findings that selection into remote work shifter after 2020 to higher-quality jobs.

Overall, this suggests that most of the COVID-19-induced growth in remote work allowed mothers to go to better jobs, namely as salary employees and in formal status, highlighting the potential benefits of the flexibility of remote work in recent years.

Intensive margin responses by WFH status. The effect of WFH on hours worked is theoretically ambiguous. On the one hand, WFH reduces commuting and may allow for longer work hours. On the other hand, it could lead to a reallocation of time toward childcare, reducing hours worked. To examine this, I compare men and women before and after childbirth based on their WFH status or their WFH potential.

A key distinction is that actual WFH status reflects the realized work arrangements, which can change post-childbirth, whereas the WFH potential is based on occupational characteristics and serves as a proxy for pre-childbirth job flexibility.

Panel A of Appendix Table C.3 uses actual WFH status. Workers who WFH are more likely to be part-time and self-employed. Among recent mothers who WFH, these patterns are even more pronounced: they are more likely to be working part-time, be self-employed, and have recently switched job. This suggest that some mothers transition into remote work after childbirth, potentially to gain flexibility and balance paid and unpaid work.

Panel B instead considers WFH occupations. These workers are more likely to be part-time but less likely to self-employed or recent job switchers. In particular, recent mothers in WFH occupations are less likely to work part-time or work as self-employed.

Taken together, these results reflect two distinct mechanism. First, women who could WFH are more likely to remain working and thus retain their full-time and salaried jobs. Second, actual remote work reflects post-childbirth adjustments, where some women shift into more flexible arrangements as part-time or self-employed. Overall, the results imply that occupational flexibility enables labor force attachment, while realized remote work reflects a post-birth adjustment toward more flexible work arrangements.

4.4 The Role of Partner's Flexibility

While much of the literature has focused on women's occupational flexibility, recent works have emphasized the importance of partner flexibility in shaping women's labor outcomes (Bang, 2022). A mother's ability to work from home may help her balance paid work with household responsibilities, but if her partner can also work remotely, they may contribute more to childcare and housework. This intra-household flexibility could reduce the child penalty. I proxy partner flexibility using their occupation's potential to be done from home.

Figure 5 presents the child penalty in employment for men and women, separately by whether their partner has an occupation where they can WFH. For men, the partner's flexibility has no role on post-childbirth employment. For women, however, there is a sizable and persistent difference: those whose partners can WFH experience a significantly smaller drop in employment, both immediately after childbirth and until five years after.

On average, women whose partner can WFH are 4.4 percentage points more likely to be employed post-childbirth than those whose partner cannot WFH (Panel A, Table 3). They are also more likely to be formally employed and less likely to work remotely themselves. This pattern suggest intra-household complementary: when the partner cannot WFH, women may need to take on more flexible or remote jobs to accommodate their childcare needs. In contrast, when the partner can WFH, women can remain in non-remote or more stable employment.

However, these estimated do not reflect causal effects of the partner's WFH potential. First, respondents and partner's WFH potential are not randomly assigned, as occupational sorting is endogenous and likely reflects underlying preferences for workplace attributes and flexibility (Wiswall & Zafar, 2018). Second, due to positive assortative matching, women who can WFH are more likely to be partnered with men in jobs that can be done remotely. As a result, the observed differences may reflect the woman's own flexibility, rather than the partner's.

To address these concerns, I exploit the COVID-19 shock. While the pandemic dramatically increased actual WFH levels, it likely did not change the composition of occupations that can be done remotely, at least in the short run. Appendix Figure B.5 shows that the aggregate WFH potential remained stable over time within countries, reflecting persistent industry and occupational structures.

I implement a difference-in-difference design, comparing women with flexible versus non-flexible partners before and after 2020, and before and after childbirth. Panel B of Table 3 shows that, after 2020, the child penalty fell by an additional 2.5 pp for women whose partner can WFH, relative to those whose partner cannot. This design helps mitigate concerns around occupational sorting.

However, some identification limitations still remain. Due to positive assortative matching, I cannot completely separate whether the post-2020 effect is driven by the partner's flexibility, the woman's own WFH potential, or joint household dynamics. Nonetheless, these results suggest that women's labor market outcomes after childbirth depend not only on their own job flexibility, but also on that of their partner.

4.5 Geographical variation

More broadly, the regional supply of flexible or WFH jobs in a region might affect child penalties. In this section, I exploit geographic variation in the feasibility and supply of remote work at the state level —the largest subnational division in each country. I estimate the child penalty in employment by state $s \in S$ ($\#S = 91$) and time period $p \in \{2014/16, 2017/19, 2021/23\}$, using the difference-in-difference specification from equation (2) interacted it with state-period indicator. This yields a child penalty in employment for state s during period p, which I note as $\beta_{s,p}$. [6]

To assess the role of remote work, I compute changes in the child penalty before and after COVID-19 and regress them on the change in the share of workers primarily working from

[6] I define $\text{Penalty}_{s,p} \equiv -\beta_{s,p}$, so that a higher penalty corresponds to more adverse outcomes for women.

home in each state. To account for potential differences in remote work measurement across surveys, the preferred specification includes country-fixed effects. Formally, I estimate:

$$\Delta \text{Penalty}_{s,p} = \alpha \cdot \Delta \text{RemoteWork}_{s,p} + \delta_{c(s)} + \varepsilon_{s,p} \tag{3}$$

To addess potential endogeneity in the change in remote work, I instrument for it using a baseline measure of WFH potential: the share of jobs that could be done remotely in each state between 2017 and 2019. Appendix Figure B.5 shows that this measure is stable over time within countries, reflecting aggregate industry and occupation composition.

Instrument validity For the instrument to be valid, it must be both relevant and excluded.

Relevance is satisfied if the share of jobs which can be done from home increases remote work. Panel A in Table 4 presents the first stage regressions. Column (1) shows that a higher share of jobs that can be done remotely increases remote work after the COVID shock. This increase is significant and robust to the inclusion of country fixed effect (column 2), and additional controls of other sources of job flexibility, such as the share of part-time, public sector, self-employed and formal workers (column 3). In the preferred specification in column 2, a one pp increase in WFH potential increases remote work by 0.39 pp. Panel (a) of Appendix Figure B.7 shows that the state-level WFH potential predicts post-COVID changes in remote work. Moreover, panel (b) shows that this WFH potential does not correlates with pre-COVID changes in WFH, reinforcing the shock.

For the exclusion restriction to be satisfied, WFH potential should affect child penalties only through its effect on remote work increase. The most plausible violation of the exclusion restriction is that regions with higher WFH potential may have been less affected by the COVID-19 shock, leading to smaller child penalties due to better local labor market conditions rather than remote work itself. However, WFH potential is not significantly associated with changes in the log median household income in either the pre- or post-COVID-19 periods. [7] Overall, these findings suggests the exclusion restriction plausibly holds. I also consider an alternative empirical strategy partially relaxing this assumption.

Empirical Bayes correction Because state-period child penalties $\beta_{s,p}$ are estimated from a specification with multiple fixed effects, they may be noisy, particularly in smaller states

[7] The coefficient of WFH potential on the change in log median household income is 0.032 (0.196) for 2021/23 vs 2017/19 (post-COVID-19), and 0.268 (0.208) for 2017/19 vs 2014/2016 (pre-COVID-19), controlling for country fixed effects.

with fewer sample size. To address this, I implement an empirical Bayes correction similar to Gulek (2024) and Kleven (2025), shrinking noisy estimates toward the country-period average.

Formally, I assume that the state-period child penalty $\beta_{s,p}$ has a normal prior with country-period-specific mean $\mu_{c,p}$ and variance $\sigma^2_{c,p}$, respectively. Let $\hat{\beta}_{s,p}$ be the unscaled penalty estimate, with standard errors $SE_{s,p}$. The posterior is:

$$\beta^{\text{post}}_{s,p} = \left(\frac{\sigma^2_{c,p}}{\sigma^2_{c,p} + SE^2_{s,p}}\right) \cdot \hat{\beta}_{s,p} + \left(\frac{SE^2_{s,p}}{\sigma^2_{c,p} + SE^2_{s,p}}\right) \cdot \mu_{c,p}$$

The prior mean $\mu_{c,p}$ is set to the country-period child penalty, and the variance $\sigma^2_{c,p}$ equals the excess noise, relative to the effective variance. Formally, this means setting $\mu_{c,p} = \beta_{c,p}$ and $\sigma^2_{c,p} = \sum_{s':c(s')=c} w_{s,p}[(\hat{\beta}_{s,p} - \mu_{c,p})^2 - SE_{s,p}]$, where $w_{s,p}$ are the number of observations in state s during period p, relative to observations for the country-period.

For precise estimates, the posterior aligns closely with the estimate $\hat{\beta}_{s,p}$. For noisy estimates, it pulls toward the country-period average. Appendix Figure B.6 illustrates this in Brazil (where estimates are precise) and Argentina (where they are not).

Regression estimates Panel B of Table 4 reports the OLS, 2SLS, and reduced form estimates of equation (3). [8] These results are robust across specifications, with point estimates near one, even when considering more conservative measures of child penalties from the Empirical Bayes correction (columns 4 to 6).

In the preferred IV model (column 2), a one pp increase in remote work reduced child penalties by about 1.52 pp. Moving from the 25th to the 75th percentile in remote work growth (2.31 to 5.88 pp) implies a 5.4 pp reduction in the child penalty, or 3.85 pp using the more conservative model estimate in column 5.

Falsification test To verify the validity of the identification strategy, I replicate the analysis for the pre-COVID-19 period (2014/16 to 2017/19), when remote work rates were largely flat (see Appendix Figure B.2). First-stage estimates in Appendix Table C.5 are close to zero and not statistically significant. Panel (b) in Appendix Figure B.7 further shows no correlation between WFH potential and remote work changes in the pre-COVID-19 period.

Panel B in Appendix Table C.5 shows no significant relationship between remote work

[8] Appendix Table C.4 presents results using child penalties and remote work in levels, controlling for state and period fixed effect. The estimates are similar to those from the first-differences specification.

and child penalties before 2020, supporting that the observed effects are specific to the pandemic shock.

Alternative empirical strategy The previous identification strategy relies on the instrument validity, which might be too stringent. Instead, I leverage the individual-level variation in a difference-in-difference design, by comparing respondents in states with high and low WFH potential, before and after the COVID-19 shock. Employing the WFH potential of the state, I define an indicator for states who are above the median in each country.

Then, I estimate equation (2) and interact the difference-in-difference term with an indicator for states above the median—or with High WFH potential—and a post-2020 dummy. This approach compares women and men, before and after childbirth, across high- and low-WFH potential states before and after 2020, controlling for state fixed effects, in addition to the gender-specific age and calendar year fixed effects. This alternative design allows me to relax the exclusion restriction by relying instead on the parallel trend assumption that, absent COVID-19, trends in child penalties would have evolved similarly across states with high or low WFH potential.

Appendix Table C.6 reports the results from this specification, where we note that the child penalty became less severe for women in states with higher WFH potential. Column 3 shows that women in high-WFH-potential states were 4.4 p.p. more likely to be employed post-childbirth relative to women in low-WFH-potential regions, consistent with the cross-state analysis above.

5 Robustness

This section shows that the results are robust. First, I show that they are robust to controlling for other sources of flexibility, highlighting the role of WFH flexibility in driving the results. Second, I show they are robust to alternative definitions or measurement concerns.

Other sources of flexibility Remote work flexibility may correlate with other forms of job flexibility relevant for gender gaps in labor outcomes (Bang, 2022; Goldin, 2014). For example, jobs that can be done from home may also offer greater time flexibility or are more commonly offered on a part-time basis. In such cases, the observed effects might reflect general flexibility rather than flexibility specifically from remote work.

To account for this, I construct a proxy for part-time potential by computing the share of salaried employeed working part-time at the 2-digit occupation level in the pre-COVID period. I then re-estimate the main specifications controlling for this occupation-level part-time potential. By controlling for the part-time potential, I can isolate the role of remote work flexibility from other types of flexibility.

Results are robust to controlling for the part-time potential in the occupation. Appendix Figure B.8 shows the pseudo-event study on the probability of being in a WFH occupation or primarily WFH, controlling for part-time potential. Appendix Table C.7 shows the intensive-margin child penalty estimates after including this control. Across specifications, results are robust and stable, suggesting that remote work is a different form of flexibility relevant to post-childbirth employment.

Country-specific results In this paper, I employ data from four different countries. One potential concern is that findings may be driven by a single country. Appendix Figure B.9 and Appendix Table C.8 display the child penalty estimates separately by country. In all countries, there is a persistent increase in remote work following childbirth. While magnitudes vary, the main pattern holds across all countries: child penalties in employment are sizable, and women are more likely to WFH after childbirth.

Alternative measure of WFH potential I use Dingel and Neiman (2020) WFH classification, based on U.S. O*NET tasks mapped to 2-digit ISCO codes. Though widely used and validated, this classification may reflect U.S.-specific occupational structures and may not fully capture WFH feasibility in Latin America (Delaporte & Pena, 2020; Garrote Sanchez et al., 2021; Gottlieb et al., 2021; Hatayama et al., 2020; Viollaz, 2022).

To address this, I replicate the main analysis using the more conservative WFH measure proposed by Gottlieb et al. (2021), tailored to developing countries. This alternative identifies fewer jobs as remote-feasible, thus resulting in a considerably lower baseline share of jobs that can be done remotely.

Results are robust to this alternative measure. Appendix Figure B.10 shows a similar increase in WFH potential for recent mothers relative to men. Mothers in WFH occupation (by this measure) are also less likely to have recently switched jobs post-childbirth, suggesting they are also more likely to remain in their pre-child jobs.

At the state level, this alternative measure is highly correlated with Dingel and Neiman (2020) measure. Appendix Figure B.11 shows an almost linear relationship between the share of jobs that can be done from home based on Dingel and Neiman (2020) and Gottlieb et al. (2021) measures. Regression estimates using the alternative measure are nearly identical (Appendix Table C.9).

Addressing endogenous WFH A final concern is that the decision to WFH is endogenous, as workers (and especially mothers) might select themselves to work remotely. To address this, I construct a leave-one-out (LOO) instrument: the average share of workers who WFH in the same industry, occupation and period (pre/post-2020), excluding the respondent. Panel C in Appendix Table C.3 reports 2SLS estimates using the LOO instrument. The results are consistent with the baseline OLS estimates, though noisier.

6 Conclusion

This paper examines the role of remote work in shaping child penalties in employment in four Latin American countries: Argentina, Brazil, Chile, and Colombia.

I first document a persistent increase in remote work following the COVID-19 pandemic, with rates stabilizing above pre-pandemic levels across all countries. Notable, the composition of remote workers has shifted: before 2020, remote work was concentrated in lower-paying, part-time or self-employed jobs. After 2020, it expanded to include more formal, salaried roles.

This paper provides evidence of the potential of remote work to mitigate child penalties using complementary strategies at the individual, household and regional levels. At the individual level, women are 4.4 pp more likely to WFH after childbirth, and those in occupations that can be done from home are significantly more likely to remain in their jobs after childbirth. At the household level, mothers whose partners can WFH are 4.4 pp more likely to remain employed post-child. At the regional level, following the COVID-19 shock, mothers in states with high WFH potential are 4.4 more likely to remain employed relative to those in low WFH potential.

The COVID-19 shock further amplified these effects. Post-2020, mothers are 2.4 pp more likely to WFH than before, especially in salaried and formal jobs. Similarly, the positive impact of partner flexibility increases: women whose partner can WFH after 2020 are 2.5 pp more likely to be employed after childbirth than similar women before the pandemic.

Overall, these findings suggest that remote work helps women remain in the labor market after childbirth by enabling them to remain in their previous jobs, providing a flexible work arrangements, and allowing for intra-household adjustments when partners can WFH.

A key policy implication is that expanding access to remote work may reduce gender gaps in employment after childbirth. This is particularly relevant now, as many firms and goverments are scaling back remote work policies. My findings suggest that return-to-office mandates may widen gender gaps in the labor market. Moreover, because household childcare and labor supply decisions are jointly determined, restricting fathers' ability to WFH may negatively affect mothers' employment outcomes.

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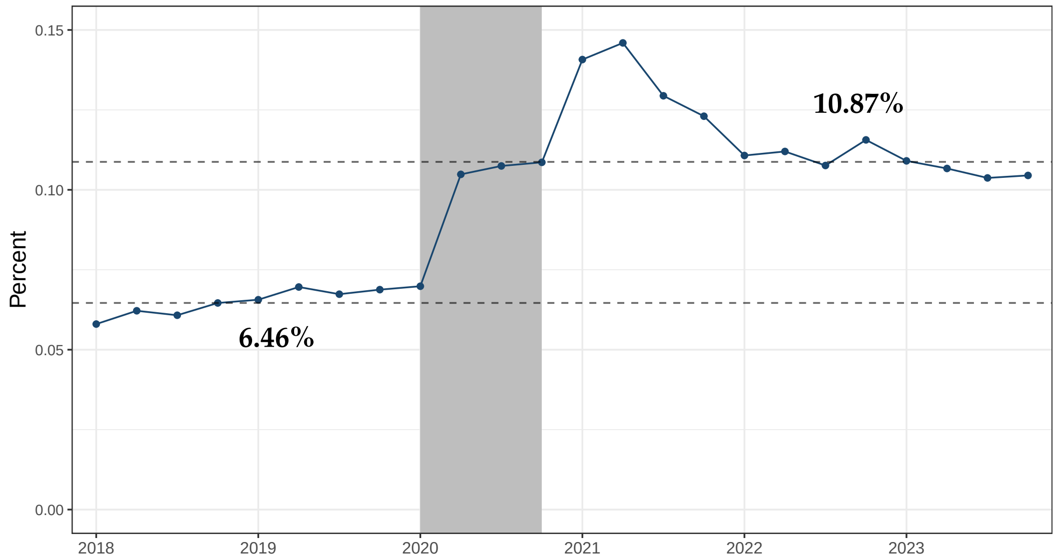
Figures

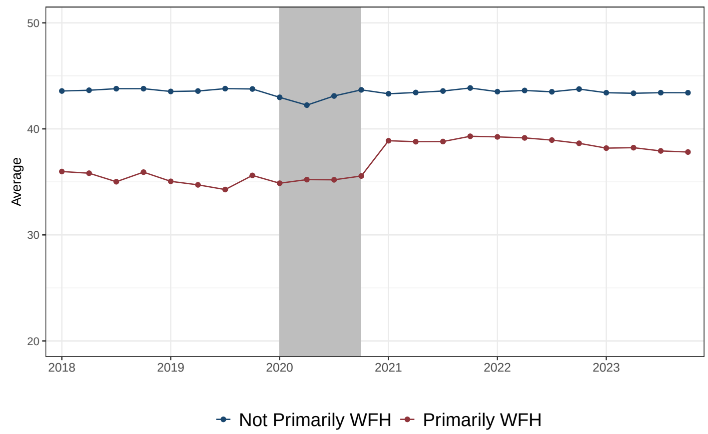
Figure 1: Remote Work Permanently Increased After the Pandemic



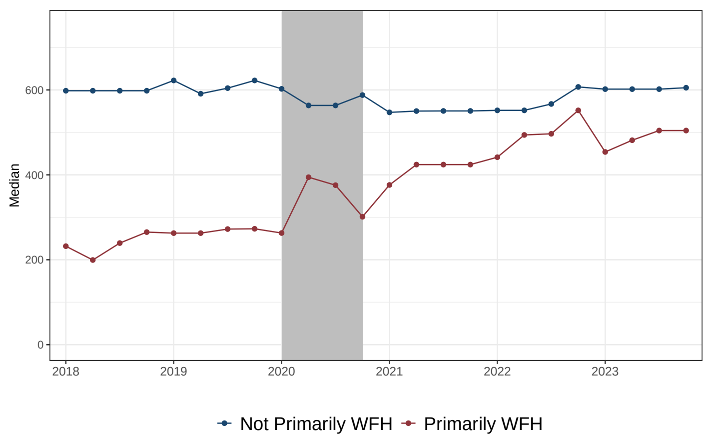
Note: The figure shows the share of respondents who primarily work from home among workers aged 25 to 45, in Argentina, Brazil, Chile and Colombia. Panel (a) in Appendix Figure B.2 shows the time series by country since 2010. Gray area represents responses in 2020, when there are issues with sampling and response quality due to the COVID-19 pandemic. Primarily WFH status is based on responses to a survey question on place of work, available in Appendix Table A.1, and Data Appendix A provides more detail about the data and variable definitions.

Figure 2: Hours and Earnings of Remote Workers Changed After 2020

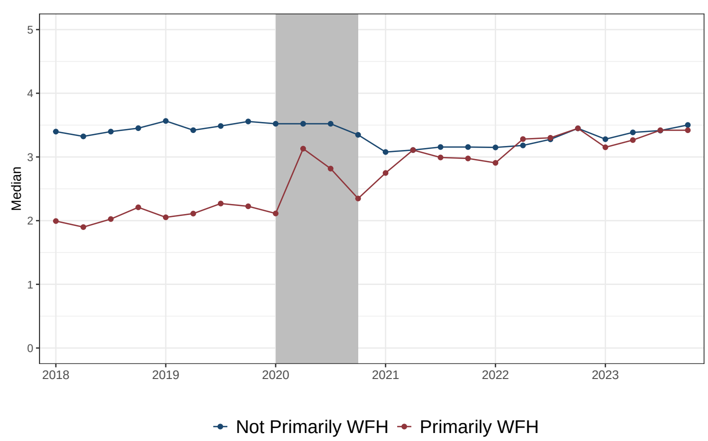
(a) Average Weekly Hours



(b) Median Monthly Earnings

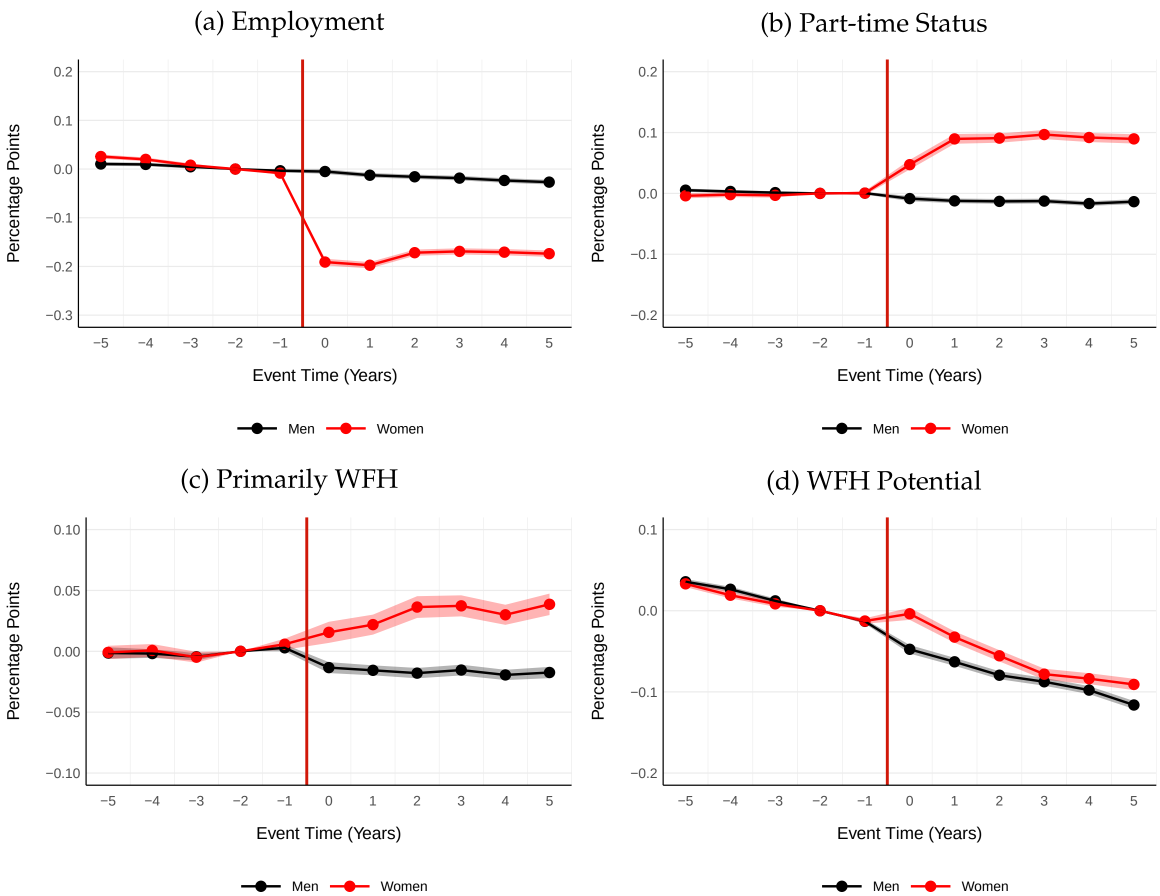


(c) Median Wage Rate



Note: Panel (a) shows the average weekly hours worked by WFH status. Panel (b) shows the median monthly earnings in 2010 PPP US$ by WFH status. Panel (c) shows the median hourly wage rate by WFH status. The gray area represents responses in 2020, where there are issues with sampling and response quality due to the COVID-19 pandemic, as well as missing responses in some countries. Data Appendix A provides more detail about the data and variable definitions.

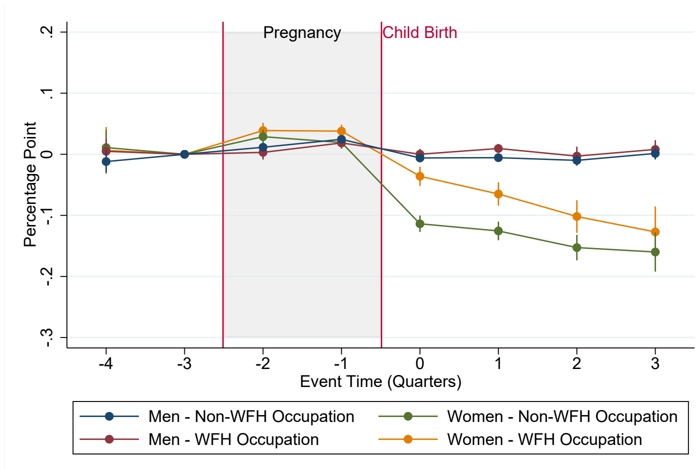
Figure 3: Child Penalties at Extensive and Intensive Margins



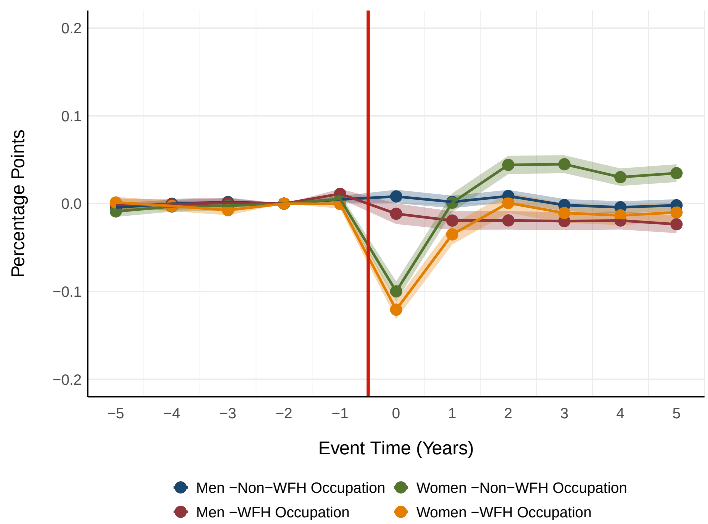
Note: The figure shows pseudo-event study estimates around the first childbirth ($\tau = 0$), calculated separately by gender. Estimates control for age, calendar year, and country fixed effects, based on equation (1). Panel (a) uses employment as an outcome. Panels (b)-(d) are conditional on employment. Panel (b) uses a dummy a part-time status (less than 40 weekly hours) as outcome. Panel (c) uses an indicator of working primarily WFH, as outcome. Panel (d) uses the WFH potential or the probability that the job can be done from home as an outcome. Data Appendix A provides more detail about the data and variable definitions.

Figure 4: Women Able to WFH Are More Likely to Remain Employed at Their Pre-Child Job

(a) Employment, by Pre-Child Occupation

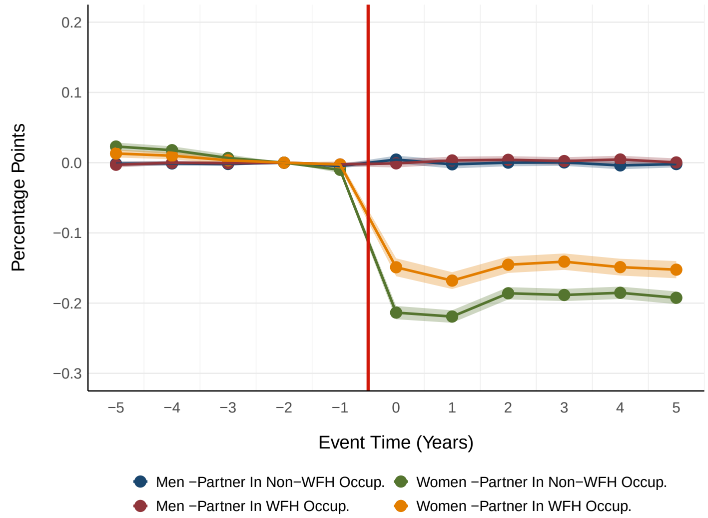


(b) Probability of Job Switch, by Current WFH Occupation



Note: Panel (a) shows the event study around childbirth on the probability of employment, based on respondents pre-child occupation, using quarterly panel data in Brazil. The event study is estimated separately by gender and WFH/non-WFH occupation status, and controlling for age and calendar quarter fixed effects. Panel (b) shows the pseudo-event study around childbirth on the probability of having recently switched a job (a tenure of less than two years), based on respondents current occupation when surveyed, and conditional on employment. The pseudo-event study is estimated separately by gender and WFH/non-WFH occupation status, and controlling for age, calendar year, and country fixed effects, based on equation (1). Data Appendix A provides more detail about the data and variable definitions.

Figure 5: Women Whose Partner Can WFH Are More Likely to Remain Employed



Note: The figure shows pseudo-event study estimates around the first childbirth ($\tau = 0$) on the probability of employment, calculated separately by gender and partner's WFH occupation. Sample is restricted to those living with a partner. Estimates control for age, calendar year, and country fixed effects, based on equation (1). Data Appendix A provides more detail about the data and variable definitions.

Tables

Table 1: Child Penalties at Extensive and Intensive Margins

	(1) Employment	(2) Part-Time	(3) Formal	(4) Self-Employed	(5) Primarily WFH	(6) WFH Potential
Panel A						
Women × Post	-16.66***	9.860***	-1.210***	1.615***	4.397***	2.711***
	(0.1989)	(0.2070)	(0.2368)	(0.2168)	(0.2244)	(0.2052)
Observations	12,600,573	10,491,529	10,491,529	10,491,529	2,875,392	10,440,530
R^2	0.112	0.084	0.043	0.028	0.039	0.059
Mean Dep. Var.	78	19.23	69.82	22.95	7.09	37.04
Panel B						
Women × Post	-16.08***	10.34***	-1.551***	1.776***	3.989***	2.668***
	(0.2250)	(0.2315)	(0.2666)	(0.2417)	(0.2706)	(0.2328)
After2020 × Women × Post	-3.195***	-1.782***	1.061	-0.1682	2.439***	-0.0662
	(0.4925)	(0.5784)	(0.6516)	(0.6146)	(0.6210)	(0.5597)
Observations	12,600,573	10,491,529	10,491,529	10,491,529	2,875,392	10,440,530
R^2	0.11182	0.08444	0.04283	0.02806	0.03949	0.05941
Mean Dep. Var.	78	19.23	69.82	22.95	7.09	37.04
Sample:	All	Workers	Workers	Workers	Workers	Workers

Note: All regression include gender-specific age, calendar year and country fixed effects, based on equation (2). "Post" refers to post-childbirth. All outcomes (except the last column) are binary variables, multiplied by 100 to be interpreted as percentage points. Column (1) uses employment status. Columns (2)-(6) restrict attention to respondents who are working, and uses a part-time indicator (less than 40 weekly hours), formally employed, self-employment, primarily WFH, and the WFH potential of the occupation to be done from home. Panel A estimate the baseline difference-in-difference specification from equation (2), and Panel B interacts this term with "After 2020", an indicator for parents (or those who would have become parents) after 2020. Data Appendix A provides more detail about the data and variable definitions.

Table 2: Women Are More Likely to Work from Home After Childbirth

	(1)	(2)	(3)	(4)	(5)	(6)
		Primarily WFH And:				
	Primarily WFH	Part-Time	Self-Employed	Employee	Formal	Informal
Panel A						
Women × Post	4.397***	3.590***	3.835***	0.5237***	0.4287***	3.969***
	(0.2244)	(0.1555)	(0.1912)	(0.1209)	(0.1190)	(0.1972)
Observations	2,875,392	2,875,392	2,875,392	2,875,392	2,875,392	2,875,392
R^2	0.039	0.028	0.020	0.031	0.036	0.020
Mean Dep. Var.	7.09	2.73	4.83	1.92	2.46	4.64
Panel B						
Women × Post	3.989***	3.217***	3.536***	0.428***	0.049	3.940***
	(0.271)	(0.187)	(0.237)	(0.122)	(0.104)	(0.253)
After2020 × Women × Post	2.439***	1.752***	1.683***	0.820*	1.821***	0.618
	(0.621)	(0.398)	(0.477)	(0.440)	(0.465)	(0.463)
Observations	2,875,392	2,875,392	2,875,392	2,875,392	2,875,392	2,875,392
R^2	0.03949	0.02774	0.02061	0.03125	0.03638	0.02038
Mean Dep. Var.	7.09	2.73	4.83	1.92	2.46	4.64
Sample:	Workers	Workers	Workers	Workers	Workers	Workers

Note: All regression include gender-specific age, calendar year and country fixed effects, based on equation (2). "Post" refers to post-childbirth, and the sample is conditional on working. All outcomes are binary variables, multiplied by 100 to be interpreted as percentage points. Column (1) uses primarily WFH as an the outcomes, and columns (2)-(6) interact this WFH status with part-time status, self-employment or salaried employment, and formal or informal status. Panel A estimate the diff-in-diff specification, and Panel B interacts this term with "After 2020", an indicator for parents (or those who would have become parents) after 2020. Data Appendix A provides more detail about the data and variable definitions.

Table 3: Women Whose Partner Can WFH Are More Likely to Remain Employed

	(1) Employment	(2) Part-Time	(3) Formal	(4) Self-Employed	(5) Primarily WFH	(6) WFH Potential
Panel A						
Women × Post	-20.36***	10.40***	-3.874***	2.586***	5.027***	1.062***
	(0.2428)	(0.3087)	(0.3516)	(0.3293)	(0.3406)	(0.3033)
Women × Post × Partner In WFH Occup	4.430***	-0.5041	2.079***	-0.4531	-0.8817*	2.053***
	(0.3539)	(0.4532)	(0.4869)	(0.4775)	(0.5221)	(0.4505)
Observations	6,449,670	5,494,353	5,494,353	5,494,353	1,502,929	5,468,501
R^2	0.14404	0.09082	0.04690	0.02345	0.03939	0.10474
Mean Dep. Var.	80	21.41	75.78	21.13	8.28	43.89
Panel A						
Women × Post	-20.20***	11.40***	-4.472***	2.842***	4.728***	1.063***
	(0.2690)	(0.3443)	(0.4012)	(0.3694)	(0.4277)	(0.3480)
Women × Post ×Partner In WFH Occup	3.708***	-1.376***	2.737***	-0.5888	-0.6551	1.960***
	(0.3988)	(0.5147)	(0.5608)	(0.5387)	(0.5685)	(0.5175)
Women × Post ×Partner In WFH Occup × After2020	2.522***	2.853***	-2.288**	0.4843	-0.1128	0.1823
	(0.8308)	(1.059)	(1.117)	(1.129)	(1.158)	(1.033)
Observations	6,449,670	5,494,353	5,494,353	5,494,353	1,502,929	5,468,501
R^2	0.14410	0.09098	0.04731	0.02362	0.04219	0.10486
Mean Dep. Var.	80	21.41	75.78	21.13	8.28	43.89
Sample:	All	Workers	Workers	Workers	Workers	Workers

Note: All regression include gender-specific age, calendar year and country fixed effects, based on equation (2). "Post" refers to post-childbirth. Sample is restricted to those living with a partner. All outcomes (except the last column) are binary variables, multiplied by 100 to be interpreted as percentage points. Column (1) uses employment status. Columns (2)-(6) restrict attention to respondents who are working, and uses a part-time indicator (less than 40 weekly hours), formally employed, self-employment, primarily WFH, and the WFH potential of the occupation to be done from home. Panel A estimate the diff-in-diff specification, interacting the term with an indicator whether the partner is in an occupation that can be done from home, and Panel B furthers interacts it with "After 2020", an indicator for parents (or those who would have become parents) after 2020. The sample is, by construction, limited to respondents who have a partner living with them. Data Appendix A provides more detail about the data and variable definitions.

Table 4: Child Penalties Decreased in States Where Remote Work Increased More

Panel A - First Stage

	(1)	(2)	(3)
Outcome: *Δ Share Primarily WFH*			
WFH Potential (2017-2019)	0.327***	0.389***	0.503***
	(0.097)	(0.088)	(0.091)
Observations	91	91	91
R^2	0.274	0.338	0.498
Ftest	11.362	19.640	30.606
Country FE		✓	✓
Other controls			✓

Panel B - Regression estimates

	(1)	(2)	(3)	(4)	(5)	(6)
	Unscaled Penalty			Unscaled Penalty (EBC)		
	OLS	IV	RF	OLS	IV	RF
Outcome: *Δ Child Penalty*						
Δ Share Primarily WFH	-0.975***	-1.522**		-0.769***	-1.079**	
	(0.315)	(0.653)		(0.266)	(0.451)	
WFH Potential (2017-2019)			-0.592***			-0.419***
			(0.189)			(0.132)
Observations	91	91	91	91	91	91
Country FE	✓	✓	✓	✓	✓	✓

Note: All regressions are weighted by population in each state, and compute the change between the values in the 2021/2023 period to the previous 2017/2019 period. WFH Potential (2017-2019) is defined as the average share of jobs that can be done from home in the state, computing the average Dingel and Neiman (2020) value by 2-digit occupation. Panel A shows the first stage regression, where the outcome is the change in the share primarily WFH. Column 1 includes no control, column 2 adds country fixed effects, and column 3 adds state-level controls (baseline share of part-time, public sector, self-employed or formal workers). Panel B shows the OLS, IV and reduced form (RF) regression estimates, including country fixed effects. Unscaled Penalty is done by jointly estimating the diff-in-diff specification in equation (2), interacting the term with a state-period indicator. Unscaled Penalty - EBC (with Empirical Bayes Correction) weights the unscaled penalty towards a prior.

APPENDIX

A Data Appendix

Appendix Table A.1 list the data sources for each country, as well as the coverage period and the wording on the question of primarily WFH. The question on primary place of work is remarkably similar across countries, with the main limitation being that this question has been available in Brazil only since 2018, and for those who don't report working on a farm, ranch, farmhouse, or similar.

Additionally, I rely on the Inter-American Development Bank (2024) "Harmonized Household Surveys of Latin America and the Caribbean" project, which provides codes to harmonize variables from Argentina, Brazil and Colombia. I take advantage of this harmonization from these codes for most of the variable definitions. For example, being formal consists of contributing to any retirement pension system.

I compute occupation-level measures at the ISCO 2-digit level for Argentina, Brazil and Colombia, and at the 1-digit level for Chile. To do so, I employ Chávez Molina et al. (2020) crosswalk for Argentina. For Colombia, I use a crosswalk from occupations to ISCO at the 2-digit level until 2021, when the Colombian classification of occupation changed considerably and it's then mapped to ISCO. In Brazil, I take advantage of the occupational classification, consistent with ISCO at the 2-digit level. In Chile, there is a direct mapping at the 1-digit level.

Given these occupation crosswalks to ISCO, I employ Dingel and Neiman (2020) classification of occupations at the 2-digit level. For Chile, I compute the 1-digit share of jobs that can be done from home, using the occupation mix in Argentina, Brazil and Chile. I consider as "WFH occupations" or "Non-WFH occupations" those who have a value above or below 0.5, respectively. Additionally, in the Brazilian short-panel, I employ Góes et al. (2020) mapping of occupations that can be done from home, based on Dingel and Neiman (2020).

I compute earnings as the gross labor income from the main occupation. Earnings are not available in Chile for the entire period, and are not available in 2020 Q2 and Q3 in Colombia. To ease comparison of monetary values over time and across countries, I use PPP adjustments to convert everything to US dollars and then deflact by US inflation. Relatedly, I define a worker as part-time if they work less than 40 hours.

Leave-one-out average WFH I instrument WFH status by computing a leave-one-out (LOO) average WFH within a cell. I define a cell as the 2-digit occupation, 1-digit industry classification, and time period (before 2020 or after 2020). By taking the average within occupation and industry, the LOO average recovers industry and occupation-specific WFH potential. For example, an accountant working in the automobile sector might be more likely to work on site, as everyone else works on site, while a similar accountant working for a tech company might be WFH more frequently. I take into account the COVID-19 driven increase in remote work by also defining the time period in the LOO average construction.

Within a cell, the LOO average for a respondent is then constructed by calculating the average WFH (among working respondents), and using it as an instrument for actual WFH status. In the difference-in-difference regressions, when needed, I then interact this LOO average with the corresponding covariates (a "Post" childbirth indicator, a "Women" indicator, and the interaction).

Table A.1: Household surveys and question on Working from Home

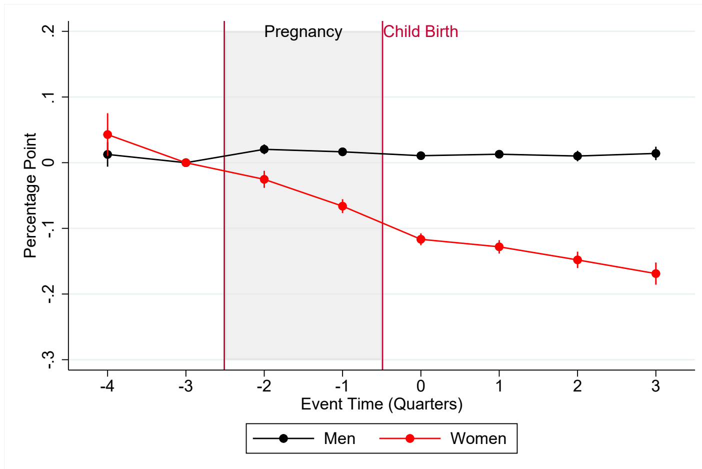
Country	Survey	Years	Primarily WFH Question	Answers to Primarily WFH	Answers to Not Primarily WFH
Argentina	Encuesta Permanente de Hogares	2010-2023	Where do you mainly perform your tasks?	6. In this house (in no exclusive place).	1. In a local/office/establishment/business/farm. 2. In a stall or fixed street kiosk. 3. In vehicles: bicycle, motorcycle, cars, boats. 4. In vehicles for transporting people and goods. 5. On construction sites. 7. At employer's or partner's home. 8. At costumer's home/premises. 9. In the street/public spaces/ambulant. 10. Other place.
Brazil	Pesquisa Nacional por Amostra de Domicilios - Continua	2012-2023(1)	I) Did you usually work at the establishment of this business/company? II) So where did you usually perform this work?	I) No II) 4. At home, in an exclusive place. 5. At home, without an exclusive place	I) Yes. II) 1. On the premises of another business. 2. Location designated by employer or client. 3. Home of employer, partner or customer. 6. In a motor vehicle 7. On a public road or area 8. Other location
Chile	Encuesta Nacional de Empleo	2010-2023	During last week, where did you mainly perform your tasks?	5. In your own home	1. Client's or employer's premises or office 2. Employer's or client's home 3. At your own or rented premises or office 4. In the office or workshop attached to your home (on the same premises) 6. On the street or public road. 7. At construction, mining or similar. 8. In an agricultural property 9. Other places
Colombia	Gran Encuesta Integradora de Hogares	2010-2023	Where do you primarily perform your work?	a. In this dwelling.	b. In other homes c In a kiosk. d. In a vehicle. e. Door-to-door. f. On the street (ambulant and stationary). g. Fixed premises, office, factory, etc. h. Countryside or rural areas. i. At a construction site. j. In a mine or quarry. k. Other

Note: (1) The Primarily WFH question has been available in Brazil only since 2018, and for those who don't report working on a farm, ranch, farmhouse, or similar.

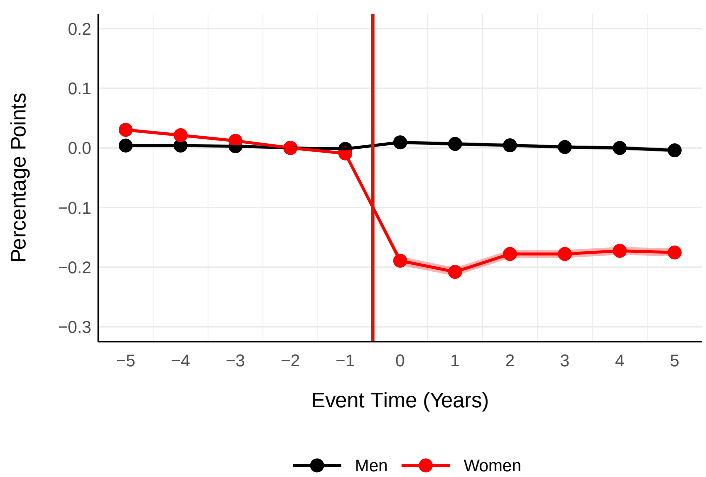
B Appendix Figures

Figure B.1: Event study and pseudo-event study estimates in Brazil are similar

(a) Event study



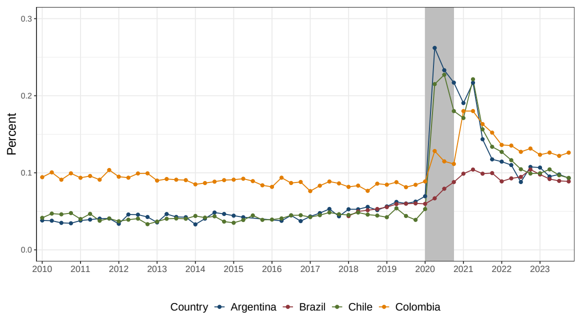
(b) Pseudo-event study



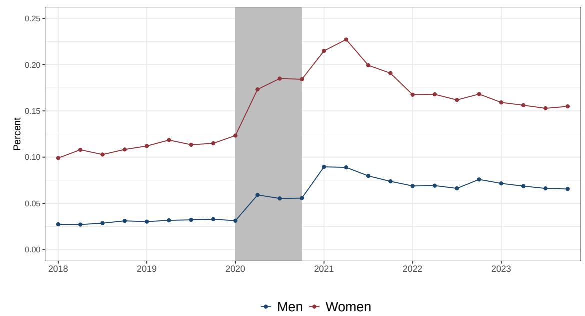
Note: Panel (a) shows the event study around childbirth on the probability of employment, using quarterly panel data in Brazil. The event study is estimated separately by gender, and controlling for age and calendar quarter fixed effects. The omitted period is three quarters before childbirth ($\tau = -3$), before pregnancy. Panel (b) shows the pseudo-event study around childbirth on the probability of employment, using the pseudo-panel in Brazil. The pseudo-event study estimated separately by gender, and controlling for age and calendar year fixed effects, based on equation (1). The omitted period is two years before childbirth ($\tau = -2$), before pregnancy.

Figure B.2: Share primarily WFH, by different demographic and groups

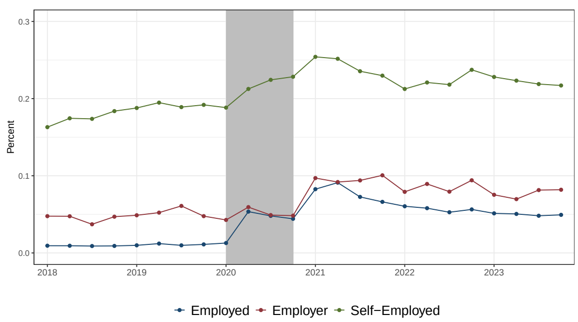
(a) By Country



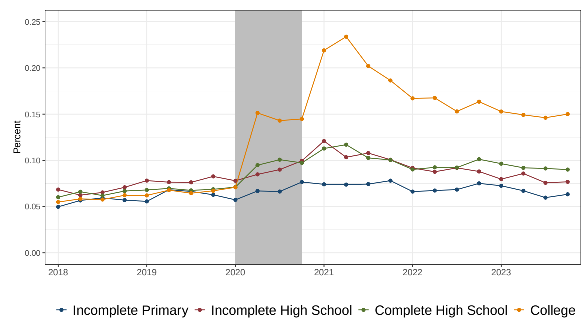
(b) By Gender



(c) By Employment Type



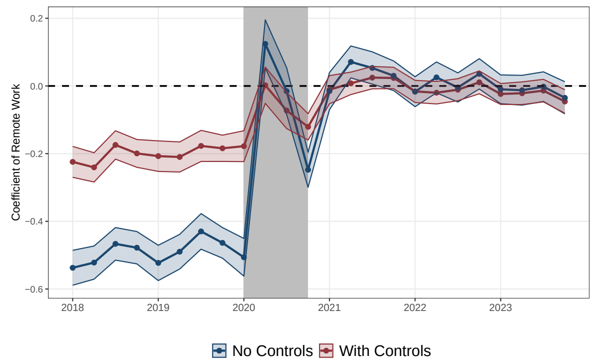
(d) By Education



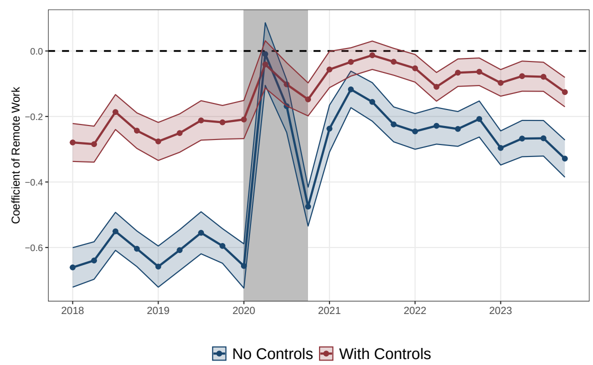
Note: The Figures display the share of respondents who primarily WFH, by different demographic groups or countries. Panel (a) shows the share by country, since 2010. Panels (b)-(d) show the share of respondents who primarily WFH by gender (men or women), employment type (salaried employees, employers and self-employed), and by education level, respectively.

Figure B.3: Regression coefficient of $log(\text{HourlyWage})$ on WFH indicator

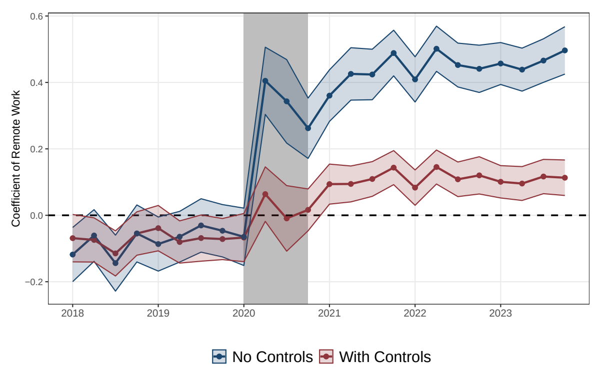
(a) Combined



(b) Women



(c) Men

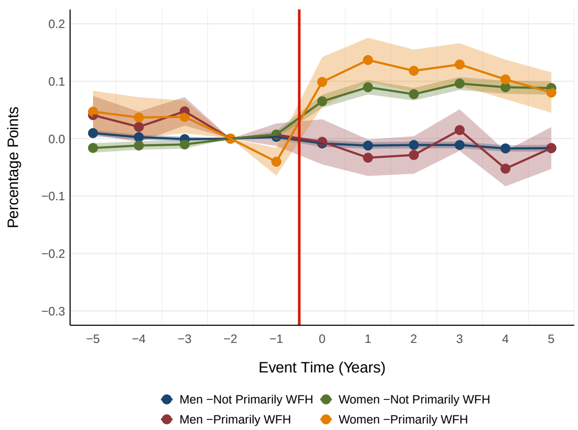


Note: The figure shows regression estimates of the log hourly wage rate on an indicator of primarily WFH, estimated separately by quarter. The red line shows the same regression coefficient controlling for 2-digit occupation, education level, state, self-employment, salaried employment, gender and 1-year age fixed effects, also estimated separately by quarter. Panel (a) shows the estimate combined for women and men, while panels (b) and (c) show the estimates only for women or men, respectively. Monthly earnings are converted to 2010 PPP US$ currency for comparison. Wage rate is defined as the ratio of monthly earnings and (four times) the weekly hours worked. Data Appendix A provides more detail about the data and variable definitions.

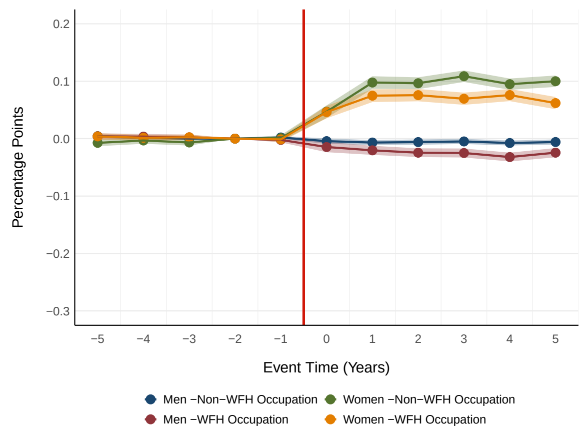
Figure B.4: Child Penalty in Part-time status and Earnings, by WFH status

Panel A: Part-time status

(a) By current WFH status

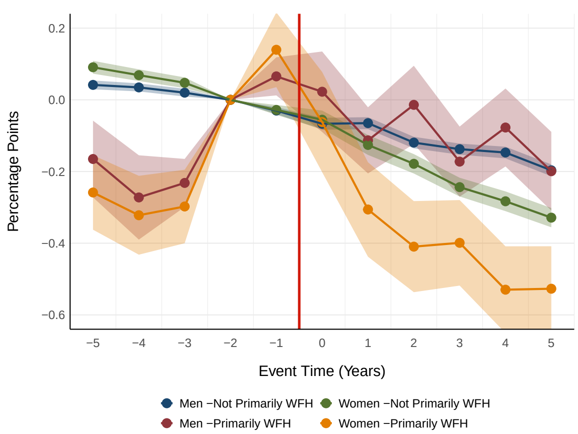


(b) By current WFH potential

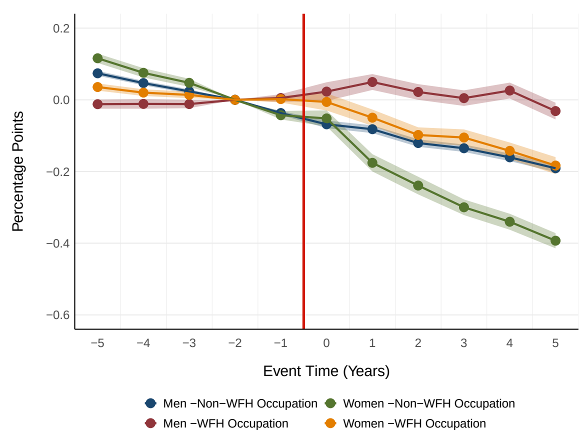


Panel B: Earnings

(c) By current WFH status

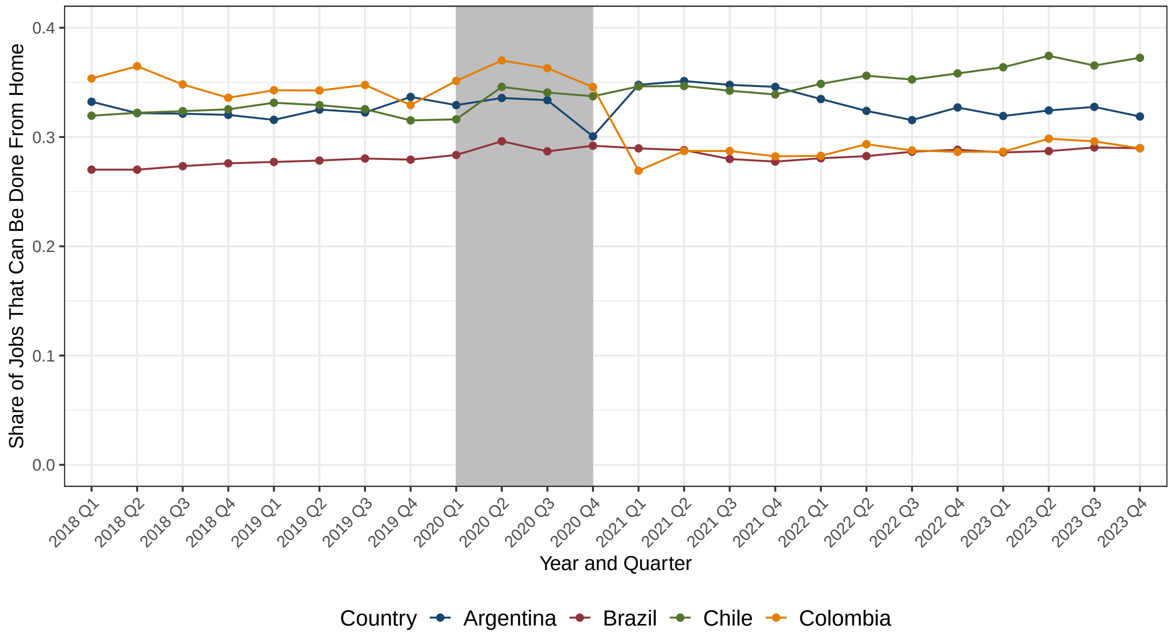


(d) By current WFH potential



Note: The figure shows pseudo-event study estimates around the first childbirth ($\tau = 0$), calculated separately by gender and conditional of working. Estimates control for age, calendar year, and country fixed effects, based on equation (1). Panel (a) divides respondents if they primarily WFH or not when observed. Panel (b) divides respondents by their WFH potential when observed. The outcome in panel (a) is an indicator, equal to 1 if the respodent worked less than 40 weekly hours, conditional on working. The outcome in panel (b) is the log of monthly earnings, conditional on working.

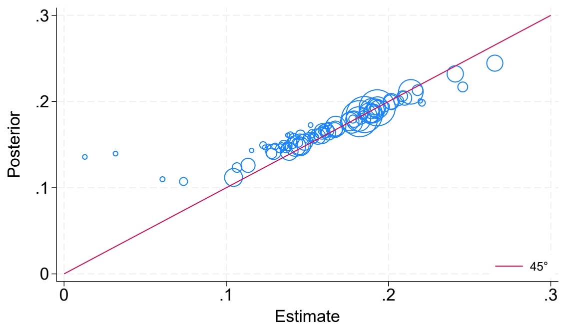
Figure B.5: The share of jobs that can be done from home is constant over time



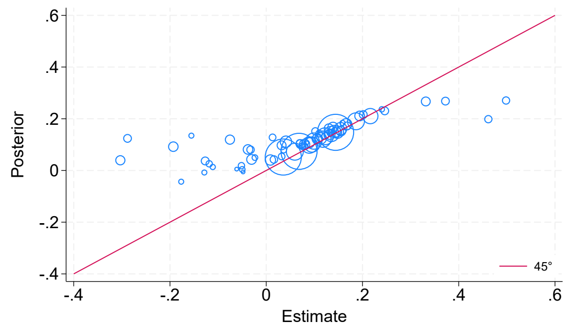
Note: The figure reports the share of jobs that can be done from home, based on Dingel and Neiman (2020) classification of whether an occupation can be performed remotely, based on ISCO 2-digit value. The Colombian classification of occupation changed considerably since 2021, which drives the change in 2021. See Data Apendix A for more details about the construction of this variable.

Figure B.6: Empirical Bayes Correction

(a) Brazil - All respondents



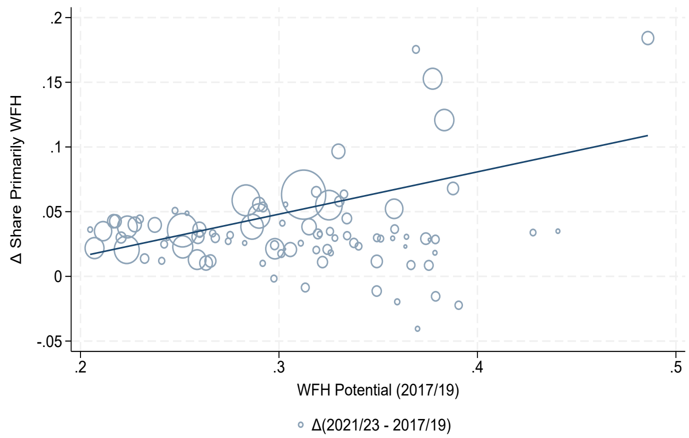
(b) Argentina - College graduates



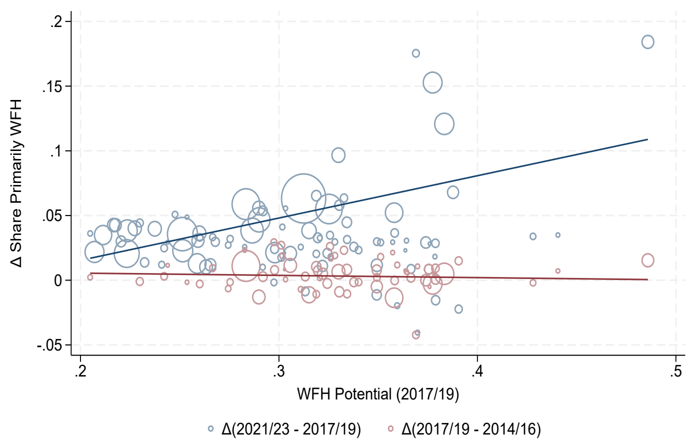
Note: The figures report unscaled estimates and the posterior, following an Empirical Bayes Correction, for all respondents in Brazil in Panel (a) and for college graduates in Argentina in Panel (b). Each circle represents a state-period estimate, and the size is proportional to the population in the state.

Figure B.7: WFH potential predicts post-COVID remote work uptake

(a) Positive correlation post-COVID



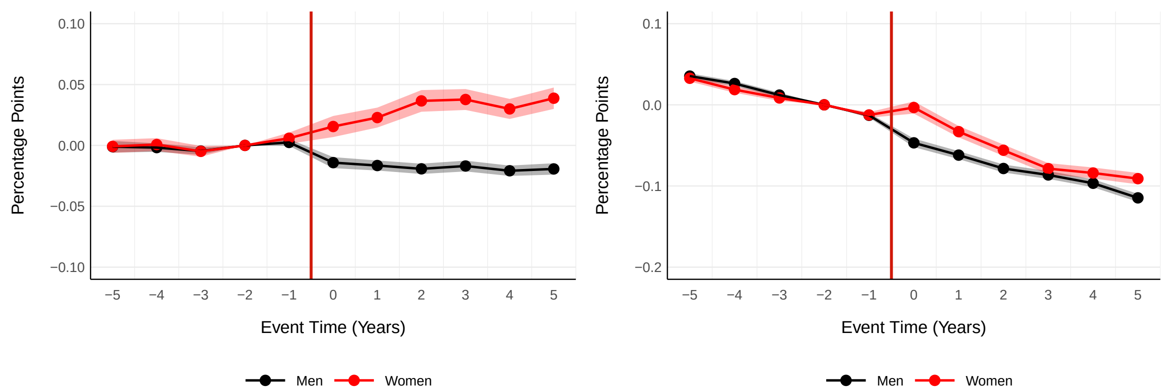
(b) No correlation pre-COVID



Note: Weighted by population in each state. WFH potential is defined as the average share of jobs that can be done from home in the state during the baseline period (2017/2019), computing the average Dingel and Neiman (2020) value by 2-digit occupation. The vertical axis shows the change in the share of respondents primarily WFH between two periods. Panel A shows the relationship using the change before and after COVID (2021/23 - 2017/19), while Panel B shows the absense of a relationship in the pre-pandemic period (2017/19 - 2014/16).

Figure B.8: Estimates are robust to controlling for other sources of flexibility

(a) Primarily WFH (b) Can WFH

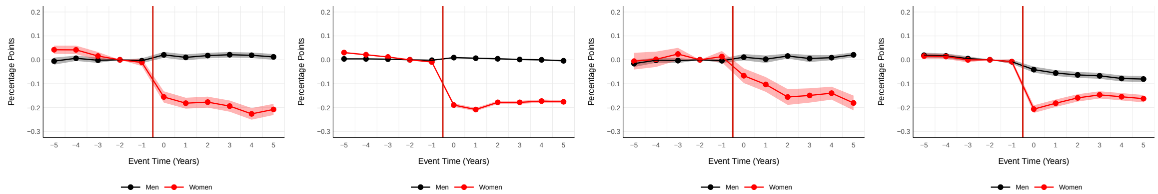


Note: The figure shows pseudo-event study estimates around the first childbirth ($\tau = 0$), calculated separately by gender. Estimates control for age, calendar year, and country fixed effects, based on equation (1). Estimates are conditional on employment and control for the part-time potential of the occupation, captured by the pre-COVID share of salaried employees who work part-time by 2-digit occupation. Panel (a) uses an indicator of working primarily WFH as outcome, and panel (b) uses the WFH potential or the probability that the job can be done from home as the outcome.

Figure B.9: Child penalties at the extensive and intensive margin, by country

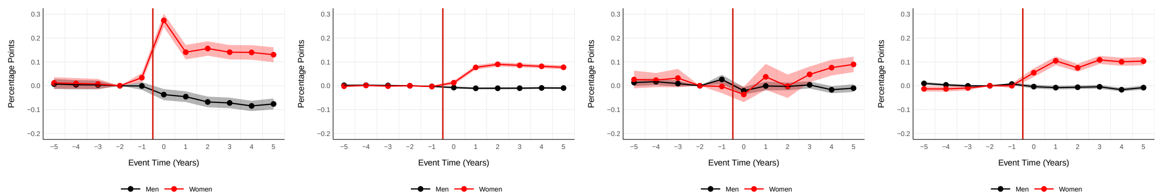
Panel A: Employment

(a) Argentina (b) Brazil (c) Chile (d) Colombia



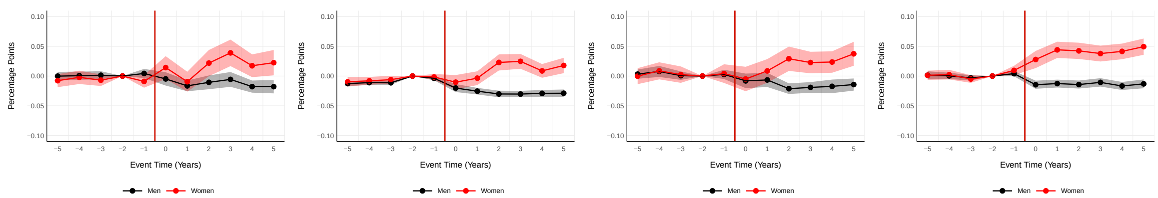
Panel B: Part-time status

(e) Argentina (f) Brazil (g) Chile (h) Colombia



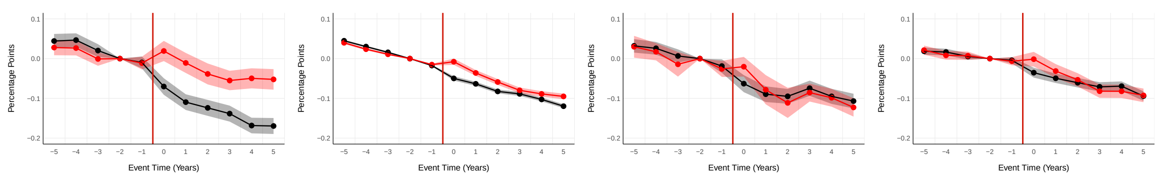
Panel C: Primarily WFH

(i) Argentina (j) Brazil (k) Chile (l) Colombia



Panel D: WFH potential

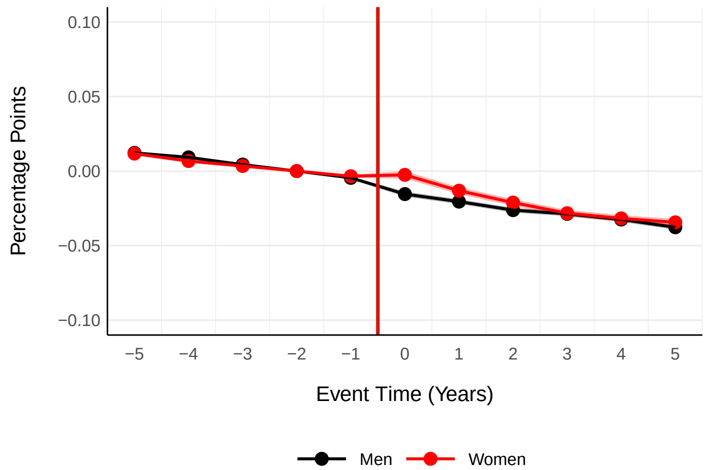
(m) Argentina (n) Brazil (o) Chile (p) Colombia



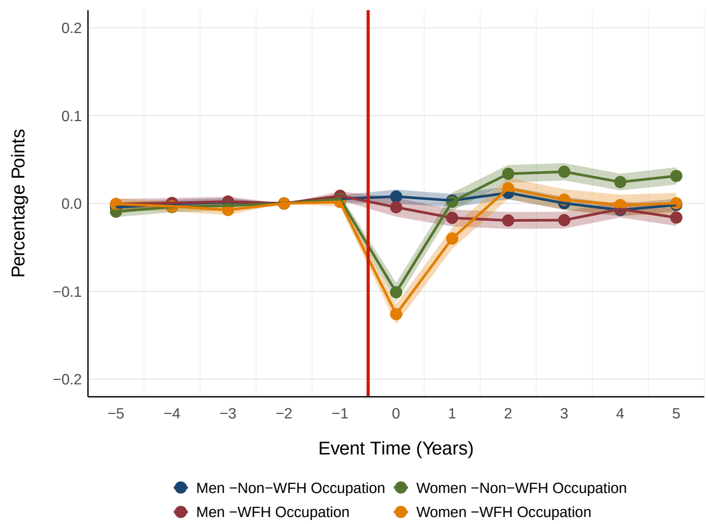
Note: The figure shows pseudo-event study estimates around the first childbirth ($\tau = 0$), calculated separately by gender. Estimates control for age, calendar year, and country fixed effects, based on equation (1). Panel (a) uses employment as an outcome. Panels (b)-(d) are conditional on employment. Panel (b) uses a dummy a part-time status (less than 40 weekly hours) as outcome. Panel (c) uses an indicator of working primarily WFH, as outcome. Panel (d) uses the WFH potential or the probability that the job can be done from home as an outcome.

Figure B.10: Estimates are robust to an alternative measure of WFH potential

(a) WFH Potential

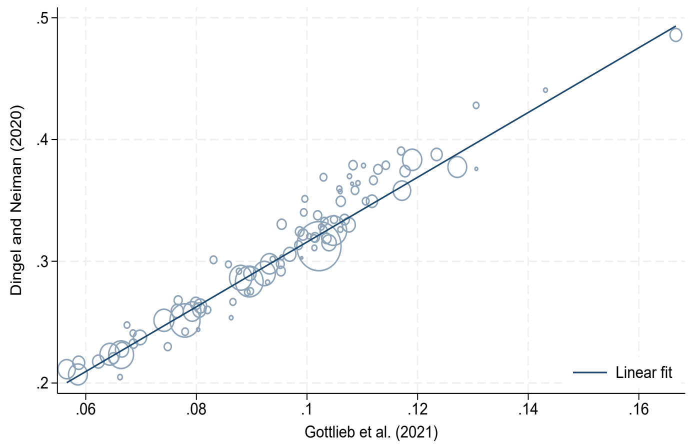


(b) Probability of Job Switch, by Current WFH Occupation



Note: The figure shows pseudo-event study estimates around the first childbirth ($\tau = 0$), calculated separately by gender. Estimates control for age, calendar year, and country fixed effects, based on equation (1). WFH potential is based on Gottlieb et al. (2021) 2-digit occupation classification. Panel (a) uses these values as an outcome, with a difference-in-difference point estimate of 0.557 pp (0.067) based on equation (2). Panel (b) divides respondents based on their WFH potential, where the outcome is an indicator of having recently switched a job (i.e., tenure of less than two years), based on respondents current occupation when observed.

Figure B.11: Dingel and Neiman (2020) and Gottlieb et al. (2021) capture similar WFH potential



Note: The figure shows the share of jobs in a state that can be done from home according two these two alternative WFH measures, defined at the 2-digit occupation level. Circles are proportional to the population in each state. While its levels are different, the linear fit shows a strong relationship between the two.

C Appendix Tables

Table C.1: Event study and pseudo-event study estimates in Brazil are similar

	(1)	(2)	(3)
	Pseudo-Panel		Panel
Women × Post	-19.11***	-18.54***	-14.342***
	(0.1802)	(0.4108)	(0.341)
Observations	11,427,241	2,854,415	4,223,205
R^2	0.125	0.136	0.130
Mean Dep. Var.	80.83	81.18	73.83

Note: The table reports event-study estimates of child penalty on employment. Column (1) reports the pseudo-panel DID estimates using all event periods, column (2) restricts it to observations in $\tau \in \{-1, 0\}$ years priors to childbirth, and column (3) reports the panel estimates. All regression control for age and calendar period (year or quarter) gender-specific fixed effects.

Table C.2: Child penalty in employment is smaller for women in WFH occupations

	(1)	(2)	(3)
Sample:	Overall	WFH Occup.	Non-WFH Occup.
Women × Post	-14.342***	-6.989***	-12.324***
	(0.341)	(0.623)	(0.487)
Observations	4223205	458353	1784251
R^2	0.130	0.027	0.033

Note: The table shows short-run diff-in-diff estimates on the probability of employment around the first childbirth ($\tau = 0$), calculated separately by gender and pre-childbirth occupation. The sample consists of respondents in Brazil, who ever report to be in a WFH or non-WFH occupation before childbirth. Estimates control for gender-specific age and calendar quarter fixed effects.

Table C.3: Intensive Margin Responses, by current WFH status and WFH potential

	(1)	(2)	(3)
	Part-Time	Self-Employed	Recent Job
***Panel A:** By Current WFH Status*			
Primarily WFH	12.01***	45.05***	-0.8655
	(0.4028)	(0.5499)	(0.6125)
Women × Post	10.31***	0.9031**	-1.104***
	(0.3242)	(0.3523)	(0.4008)
Primarily WFH × Women × Post	3.505***	7.733***	3.655***
	(1.156)	(1.337)	(1.320)
Observations	2,667,641	2,667,641	2,667,641
R^2	0.13568	0.08722	0.07816
***Panel B:** By Current WFH Occupation*			
WFH Occupation	4.021***	-3.759***	-5.389***
	(0.1522)	(0.2186)	(0.2238)
Women × Post	11.81***	4.762***	0.2334
	(0.4045)	(0.4493)	(0.4754)
WFH Occupation × Women × Post	-1.660***	-2.314***	-0.6373
	(0.5366)	(0.6052)	(0.6428)
Observations	2,667,641	2,667,641	2,667,641
R^2	0.11822	0.02968	0.08375
***Panel C:** Instrumenting Current WFH status with LOO average*			
Primarily WFH	13.66***	62.26***	-40.43***
	(1.283)	(2.130)	(2.224)
Women × Post	10.61***	0.6760	0.1820
	(0.4162)	(0.4908)	(0.5072)
Primarily WFH × Women × Post	2.988	4.718	-0.0764
	(3.306)	(5.111)	(4.729)
Observations	2,697,191	2,697,191	2,697,191
Sample:	Workers	Workers	Workers

Note: All regression include gender-specific age, calendar year and country fixed effects, based on equation (2), and interacting with current WFH status or WFH potential. "Post" refers to post-childbirth, and the sample is conditional on working. All outcomes are binary variables, multiplied by 100 to be interpreted as percentage points. Panel A interacts the diff-in-diff term with an indicator if the respondent is primarily WFH when surveyed, Panel B does the same with an indicator if the respondent is in a WFH occupation, and Panel B instruments the primarily WFH status with a leave-one-out (LOO) average. This LOO average share of respondents who WFH is computed within a cell (defined as the 2-digit occupation, 1-digit industry, and time period, i.e., before or after 2020) and then interacted with the corresponding dummies (a "Post" childbirth indicator, a "Women" indicator, and the interaction). Columns (1)-(3) use indicator for part-time status (less than 40 weekly hours), self-employment status, or having recently switched job job (i.e., tenure of less than two years). Data Appendix A provides more detail about the data and variable definitions.

Table C.4: Child penalty regressed on remote work, in levels

	(1) Unscaled Penalty	(2) Unscaled Penalty (EBC)
Outcome: *Child Penalty*		
Share Primarily WFH	-1.090***	-0.917***
	(0.221)	(0.190)
Observations	246	246

Note: Regression include state and period fixed effects, and are weighted by population in each state. The outcome is the child penalty in employment by state and period (2014/16, 2017/2019 and 2021/23).Scaled Penalty is done by estimating the child penalty regression for each state separately. Unscaled Penalty is done by jointly estimating the diff-in-diff specification in equation (2), interacting the term with a state-period indicator. Unscaled Penalty - EBC (with Empirical Bayes Correction) weights the unscaled penalty and a prior.

Table C.5: Falsification test using changes between 2017/19 and 2014/16

Panel A - First Stage

	(1)	(2)	(3)
Outcome: Δ *Share Primarily WFH*			
WFH Potential (2017-2019)	-0.017	0.020	0.038**
	(0.041)	(0.013)	(0.015)
Observations	64	64	64
R^2	0.006	0.507	0.562
Ftest	0.176	2.311	5.994
Country FE		✓	✓
Other controls			✓

Panel B - Regression estimates

	(1)	(2)	(3)	(4)	(5)	(6)
	Unscaled Penalty			Unscaled Penalty (EBC)		
	OLS	IV	RF	OLS	IV	RF
Outcome: Δ *Child Penalty*						
Δ Share Primarily WFH	-2.260	14.839		-0.969	12.284	
	(1.989)	(18.005)		(0.863)	(11.930)	
WFH Potential (2017-2019)			0.290			0.240*
			(0.239)			(0.131)
Observations	64	64	64	64	64	64
Country FE	✓	✓	✓	✓	✓	✓

Note: All regressions are weighted by population in each state, and compute the change between the values in the 2017/2019 period to the previous 2014/2017 period. WFH Potential (2017-2019) is defined as the average share of jobs that can be done from home in the state, computing the average Dingel and Neiman (2020) value by 2-digit occupation. Panel A shows the first stage regression, where the outcome is the change in the share primarily WFH. Column 1 includes no control, column 2 adds country fixed effects, and column 3 adds state-level controls (baseline share of part-time, public sector, self-employed or formal workers). Panel B shows the OLS, IV and reduced form (RF) regression estimates, including country fixed effects. Unscaled Penalty is done by jointly estimating the diff-in-diff specification in equation (2), interacting the term with a state-period indicator. Unscaled Penalty - EBC (with Empirical Bayes Correction) weights the unscaled penalty and a prior.

Table C.6: Child penalty in employment decreased in regions with high WFH potential

	(1)	(2)	(3)
Outcome: *Employment*			
Women × Post	-16.72***	-16.63***	-15.34***
	(0.1952)	(0.2767)	(0.3179)
Women × Post × High WFH Potential		0.0589	-1.196***
		(0.3303)	(0.3866)
Women × Post × High WFH Potential × After2020			4.360***
			(0.7373)
Observations	12,600,573	12,595,627	12,595,627
R^2	0.13673	0.13773	0.13786

Note: All regression include gender-specific age, calendar year and country fixed effects, based on equation (2). "Post" refers to post-childbirth. "High WFH Potential" is a dummy for states that have a share of jobs that can be done from home above the median in the country, and "After 2020" is an indicator for parents (or those who would have become parents) after 2020. The outcome is the probability of employment, multiplied by 100 to be interpreted as percentage points. Data Appendix A provides more detail about the data and variable definitions.

Table C.7: Estimates are robust to controlling for other sources of flexibility

	(1) Part-Time	(2) Formal	(3) Self-Employed	(4) Primarily WFH	(5) WFH Potential
Women × Post	9.086***	-1.150***	1.783***	4.535***	2.581***
	(0.2027)	(0.2370)	(0.2173)	(0.2244)	(0.2052)
Observations	10,441,708	10,441,708	10,441,708	2,864,605	10,440,530
R^2	0.13343	0.04474	0.03059	0.04297	0.06120
Mean Dep. Var.	19.23	69.82	22.95	7.09	37.04

Note: All regression include gender-specific age, calendar year and country fixed effects, based on equation (2). "Post" refers to post-childbirth. All outcomes (except the last column) are binary variables, multiplied by 100 to be interpreted as percentage points. Estimates are conditional on employment and control for the part-time potential of the occupation, captured by the pre-COVID share of salaried employees who work part-time by 2-digit occupation. The outcomes are a part-time indicator (less than 40 weekly hours), formally employed, self-employment, primarily WFH, and the WFH potential of the occupation to be done from home. Data Appendix A provides more detail about the data and variable definitions.

Table C.8: Child penalties in extensive and intensive margins, by country

	(1) Employment	(2) Part-Time	(3) Formal	(4) Self-Employed	(5) Primarily WFH	(6) WFH Potential
Panel A: Argentina						
Women × Post	-21.62***	21.69***	-0.3799	3.448***	3.557***	10.79***
	(0.6293)	(0.8815)	(0.9091)	(0.6620)	(0.4720)	(0.7549)
Observations	212,999	180,392	180,392	180,392	176,248	175,408
R^2	0.141	0.136	0.012	0.012	0.059	0.067
Mean Dep. Var.	83.91	39.98	64.79	15.39	5.41	43.2
Panel B: Brazil						
Women × Post	-19.35***	8.057***	-0.2872	0.4009*	3.342***	2.576***
	(0.1824)	(0.2279)	(0.2213)	(0.2089)	(0.3053)	(0.2041)
Observations	10,991,106	9,204,749	9,204,749	9,204,749	1,593,739	9,165,336
R^2	0.127	0.046	0.009	0.012	0.033	0.055
Mean Dep. Var.	81.11	19.98	76.07	19.85	7.02	34.95
Panel C: Chile						
Women × Post	-15.41***	4.360***	4.675***	1.270	3.267***	1.135
	(0.8218)	(1.146)	(1.064)	(0.7936)	(0.5028)	(0.9046)
Observations	201,234	174,982	174,982	174,982	173,999	171,288
R^2	0.100	0.055	0.011	0.011	0.057	0.088
Mean Dep. Var.	85.86	16.12	73.94	12.39	5.25	43.69
Panel D: Colombia						
Women × Post	-10.72***	10.65***	-3.608***	3.808***	5.193***	0.8406
	(0.5011)	(0.4644)	(0.6197)	(0.5717)	(0.3562)	(0.5189)
Observations	1,195,234	931,406	931,406	931,406	931,406	928,498
R^2	0.068	0.056	0.018	0.008	0.044	0.058
Mean Dep. Var.	70.93	12.63	57.92	31.52	7.59	39.78
Sample:	All	Workers	Workers	Workers	Workers	Workers

Note: All regression include gender-specific age, and calendar year fixed effects, based on equation (2), without the country indicator. "Post" refers to post-childbirth. All outcomes (except the last column) are binary variables, multiplied by 100 to be interpreted as percentage points. Column (1) uses employment status. Columns (2)-(6) restrict attention to respondents who are working, and uses a part-time indicator (less than 40 weekly hours), formally employed, self-employment, primarily WFH, and the WFH potential of the occupation to be done from home. Panel A estimate the diff-in-diff specification, and Panel B interacts this term with "After 2020", an indicator for parents (or those who would have become parents) after 2020. Data Appendix A provides more detail about the data and variable definitions.

Table C.9: Robustness to more conservative WFH measures

Panel A - First Stage

	(1)	(2)	(3)
Outcome: *Δ Share Primarily WFH*			
WFH Potential (2017-2019)	0.976***	1.121***	1.512***
	(0.254)	(0.236)	(0.257)
Observations	91	91	91
R^2	0.333	0.392	0.522
Ftest	14.730	22.656	34.586
Country FE		✓	✓
Other controls			✓

Panel B - Regression estimates

	(1)	(2)	(3)	(4)	(5)	(6)
	Unscaled Penalty			Unscaled Penalty (EBC)		
	OLS	IV	RF	OLS	IV	RF
Outcome: *Δ Child Penalty*						
Δ Share Primarily WFH	-0.975***	-1.546**		-0.769***	-1.106**	
	(0.315)	(0.601)		(0.266)	(0.430)	
Alternative WFH Potential (2017-2019)			-1.733***			-1.240***
			(0.545)			(0.406)
Observations	91	91	91	91	91	91
Country FE	✓	✓	✓	✓	✓	✓

Note: All regressions are weighted by population in each state, and compute the change between the values in the 2017/2019 period to the previous 2014/2017 period. WFH Potential (2017-2019) is defined as the average share of jobs that can be done from home in the state, computing the average Gottlieb et al. (2021) value by 2-digit occupation. Panel A shows the first stage regression, where the outcome is the change in the share primarily WFH. Column 1 includes no control, column 2 adds country fixed effects, and column 3 adds state-level controls (baseline share of part-time, public sector, self-employed or formal workers). Panel B shows the OLS, IV and reduced form (RF) regression estimates, including country fixed effects. Unscaled Penalty is done by jointly estimating the diff-in-diff specification in equation (2), interacting the term with a state-period indicator. Unscaled Penalty - EBC (with Empirical Bayes Correction) weights the unscaled penalty and a prior.