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When the Right to Work from Home Works

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Agar Brugiavini

When the Right to Work from Home Works

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When the Right to Work from Home Works *

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Abstract

We estimate the labor market effects of Italy's statutory priority to work from home (WFH) for parents of children under age twelve. Using population-wide administrative data and a sharp regression discontinuity design, we find that eligibility raises mothers' annual earnings by 1.6–2.3 percent in high-WFH-intensity firms, alongside increases in paid days and weeks. Mothers in low-WFH-intensity firms and fathers in general show no detectable effects. The estimates have an intention-to-treat interpretation and persist beyond the pandemic. Placebo tests show no effects before the pandemic, in firms that never adopted WFH, or among manual workers, consistent with a WFH mechanism.

Keywords: Flexible work arrangements; female labor supply; work–family policy; remote-work intensity; work organization; regression discontinuity

JEL classification: C31; J16; J22; J31; J81

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Quando il diritto al lavoro da remoto funziona[1]

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Sommario

Questo lavoro studia gli effetti sul mercato del lavoro della normativa italiana che riconosce ai genitori con figli di età inferiore ai dodici anni una priorità nell'accesso al lavoro da remoto (work from home, WFH). Utilizzando dati amministrativi sull'intera popolazione di riferimento, mostriamo che questa possibilità ha effetti positivi soprattutto per le madri che lavorano in imprese dove il lavoro da remoto è più diffuso. Per queste lavoratrici, l'accesso alla priorità si traduce in un aumento dei redditi annuali da lavoro compreso tra l'1,6 e il 2,3 per cento, accompagnato da un maggior numero di giorni e settimane retribuiti. Non emergono invece effetti per le madri occupate in imprese in cui il lavoro da remoto è poco utilizzato né, più in generale, per i padri. Gli effetti si mantengono anche dopo la fine della pandemia. Inoltre, non osserviamo differenze analoghe prima della pandemia, nelle imprese che non hanno mai adottato il lavoro da remoto o tra i lavoratori manuali. Nel complesso, questi risultati suggeriscono che una maggiore possibilità di lavorare da remoto può favorire la continuità lavorativa e i redditi delle madri.

Parole chiave: modalità di lavoro flessibili; offerta di lavoro femminile; politiche di conciliazione lavoro–famiglia; intensità del lavoro da remoto; organizzazione del lavoro; regressione discontinua

Classificazione JEL: C31; J16; J22; J31; J81

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1 Introduction

The rapid diffusion of remote work has transformed the organization of labor in advanced economies over the past decade. What began as an emergency response during the COVID-19 pandemic has evolved into a persistent feature of modern labor markets, reshaping how firms organize production, allocate tasks, monitor performance, and reconcile productivity with worker flexibility (Barrero et al., 2021; Adams-Prassl et al., 2022; Aksoy et al., 2022; Bloom et al., 2024). At the same time, policymakers across Europe and other OECD countries have increasingly introduced legal rights to access flexible working arrangements as part of broader work–family reconciliation strategies.[2] This legislative trend is particularly salient given the growing body of evidence suggesting that WFH can reduce child penalties for mothers while also exerting a positive influence on fertility decisions (Basso et al., 2026; Gulek and Langer, 2026).

Despite these developments, important questions remain unresolved. Even where remote work is technologically feasible, do statutory rights translate into effective access for caregivers and economically meaningful labor market gains? And when such rights are exercised, are the resulting benefits comparable across firms, or do they depend on how deeply remote work is embedded in production processes?

Such questions are especially relevant for mothers, who continue to bear a disproportionate share of childcare responsibilities and face substantial child penalties in earnings and career progression (Waldfogel, 1998; Ejrnæs and Kunze, 2013; Kleven et al., 2019, 2025; Alon et al., 2020; Mas and Pallais, 2020; Albanesi and Kim, 2021; Goldin, 2022). By reducing commuting and increasing schedule flexibility, WFH can ease work–family coordination constraints, strengthen mothers' labor market attachment, and mitigate some of the earnings losses associated with caregiving responsibilities (Allen et al., 2015; Chung and van der Lippe, 2020; Heggeness and Suri, 2021).

A key insight from recent research is that the effectiveness of remote work depends not only on occupational feasibility but also on organizational readiness (Dingel and Neiman, 2020; Bloom et al., 2015; Emanuel and Harrington, 2024; Barrero et al., 2023; Makridis, 2025). Consequently, statutory priority access to remote work may generate very different outcomes across firms even when workers face the same formal eligibility rules.

We study these issues in a distinctive institutional setting from Italy, where since 2017 parents of children under age 12 have been granted statutory priority access to remote work (*lavoro agile*), whenever their tasks are compatible with remote execution. We exploit the resulting age-based eligibility cutoff using a sharp regression discontinuity design (RDD). To do so, we combine matched employer–employee administrative records from the Italian National Institute of Social Security (INPS), covering the universe of

[2]For an overview of recent remote-work legislation, see https://global.lockton.com/us/en/news-insights/new-remote-working-legislation-around-the-world.

private-sector workers, with the universe of formally registered remote-work contracts collected by the Italian Ministry of Labor and Social Policies between 2018 and 2024.

A distinctive feature of this setting is that the statutory priority rule predates the widespread adoption of remote work. Although the right was introduced in 2017, WFH remained relatively uncommon until the COVID-19 pandemic induced firms to adopt remote-work practices on a much larger scale. This timing allows us to examine whether a pre-existing statutory right became economically consequential once remote work became an operationally relevant margin for firms.

Our main analysis therefore focuses on the post-2020 period. Because statutory WFH priority can only affect workers for whom remote work is a feasible option, we restrict the analysis to firms that implemented remote work at least once during 2020–2024 and to non-manual occupations. These restrictions define the relevant population for evaluating the policy and allow us to abstract from variation in the technological feasibility of remote work. The remaining heterogeneity therefore reflects differences in firms' intensity of WFH use and the extent to which statutory eligibility translates into meaningful labor market gains. To our knowledge, this is the first study to combine worker-firm administrative records with the universe of formally registered remote-work agreements to estimate the causal effects of statutory flexibility rights on parents' labor market outcomes.

We emphasize three main findings. First, eligibility for remote-work priority increases mothers' annual earnings by about one percent and raises paid weeks and paid days among eligible mothers. Second, these effects emerge only in firms with high remote-work intensity—that is, firms where remote work is more feasible and organizationally embedded. In these firms, wage gains reach roughly 2 percent, accompanied by an increase of about 0.3 paid weeks and nearly 2 additional paid days per year. Third, we find no significant effects for fathers and no detectable effects on promotions, full-time status, or sickness absence. The evidence therefore points to a clear channel: among parents with children near the age-12 eligibility threshold, statutory flexibility relaxes the day-to-day coordination constraints faced by mothers, increasing earnings through additional paid days worked rather than through changes in career progression, contractual status, or job design.

Our empirical design identifies an intention-to-treat (ITT) effect of statutory eligibility rather than the causal effect of WFH access itself. The estimated discontinuity therefore combines two components: the probability that eligibility translates into WFH access and the labor market gains generated by that access. This decomposition yields a testable empirical implication: if the estimated discontinuity reflects statutory WFH priority rather than age-related changes in parental labor supply, it should emerge only when remote work is operationally relevant and only in settings where eligibility can plausibly alter working arrangements. We evaluate this implication through a series of robustness checks and heterogeneity analyses.

Specifically, we examine three placebo settings. First, we exploit the fact that the statutory priority rule was already in place before the pandemic, when WFH remained negligible. This provides a direct test of whether legal eligibility alone generated a discontinuity before remote work became operationally relevant. We also examine firms that never adopted WFH and occupations that are inherently incompatible with remote execution. In each case, statutory eligibility exists formally but has little scope to affect working arrangements. Consistent with this interpretation, we find no discontinuity in any of these environments. We further show that the results are robust to alternative RDD specifications, multiple bandwidth selectors, donut designs, and a difference-in-discontinuities design comparing the pre- and post-pandemic periods.

The paper advances several strands of economic research. First, we contribute to the growing economics literature on remote work and hybrid organizations. Existing studies show that remote work can affect productivity, worker satisfaction, labor supply, and geographic mobility (Bloom et al., 2015; Choudhury et al., 2021; Aksoy et al., 2022; Barrero et al., 2023; Emanuel and Harrington, 2024; Fenizia and Kirchmaier, 2025). While much of this literature focuses on voluntary employer adoption or experimental settings, considerably less is known about the effects of legally mandated flexibility rights. Our paper shifts attention from the consequences of WFH *per se* to the effectiveness of statutory access to remote work as a labor market institution.

Second, the paper speaks to the literature on family policy, labor supply, and gender inequality. A large body of work documents that child-related labor market penalties fall disproportionately on mothers with young children (Goldin, 2014; Angelov et al., 2016; Adda et al., 2017; Kleven et al., 2019; Cortés and Pan, 2023; Kleven et al., 2025). Studies have long emphasized that flexibility and temporal control over work are central determinants of gender gaps in earnings and careers (see, among others, Bertrand et al., 2010; Goldin and Katz, 2016; Mas and Pallais, 2017, 2020), while recent evidence shows that family-friendly workplace policies can also help firms attract female talent and generate firm-level returns (e.g., Liu et al., 2023). More recently, research has shown that remote work can reshape child-related labor market penalties by increasing flexibility in the allocation of work and caregiving responsibilities (e.g., Basso et al., 2026; Gulek and Langer, 2026; Gallen et al., 2026).

We add to this literature in two ways. On the one hand, we study a statutory entitlement to remote work rather than voluntary remote-work arrangements. We show that legal flexibility rights improve mothers' labor market outcomes only when firms possess the organizational capacity to implement them effectively, highlighting a complementarity between labor market institutions and firm capabilities that has received limited attention in the gender-equality literature. On the other hand, whereas most existing studies focus on mothers of newborns or very young children, our design estimates the effect of statutory flexibility around age 12 — a later stage of the child-penalty trajectory, where

work–family constraints persist but take a different form. Our findings show that flexibility can improve mothers' labor market outcomes even during the school-age years, a comparatively understudied stage of the work–family life cycle.

Third, we relate our findings to research on organizational economics and management practices. A growing literature shows that firms differ substantially in their ability to adopt and implement new organizational technologies (Bresnahan et al., 2002; Bloom and Van Reenen, 2007; Bloom et al., 2012; Syverson, 2011; Bender et al., 2018). Recent work further emphasizes that successful hybrid work arrangements require substantial complementary investments (e.g., Barrero et al., 2021; Aksoy et al., 2022; Jiang et al., 2024). Consistent with these insights, our results document that firm-level WFH intensity is a key feature of the policy's effectiveness, a dimension that is directly observable in our data.

Our paper also contributes to the identification of the causal effects of remote-work policies. Identifying these effects remains challenging because workers and firms sort non-randomly into remote-work arrangements. Existing studies have addressed this identification challenge using a range of research designs, including instrumental-variable strategies and field experiments. We complement these approaches by exploiting a statutory age-12 eligibility threshold that generates plausibly exogenous variation in access to WFH. Our RDD therefore provides evidence based on a different institutional setting and source of identifying variation. Although the resulting RDD estimates are local to parents near the eligibility cutoff, the use of nationwide administrative data allows us to estimate this local effect across a broad range of firms, occupations, and organizational settings within the private sector.

Beyond the Italian case, the paper informs a broader policy debate. Across advanced economies, governments have increasingly introduced rights to request or access flexible working arrangements, including remote work. The European Union's Work–Life Balance Directive (2019/1158), for example, requires member states to strengthen flexible-work rights for parents and caregivers, while countries such as the United Kingdom, the Netherlands, and several Nordic economies have expanded legal protections surrounding flexible-work requests. Yet these policies often presume that formal rights translate automatically into effective flexibility. Our findings challenge this assumption. Legal entitlements alone do not guarantee meaningful access to flexibility; their effectiveness depends on whether WFH is sufficiently integrated into firms' work organization and feasible within workers' occupations. The same statutory right generates gains for mothers employed in non-manual jobs and in firms with high WFH intensity, while producing no detectable effects where remote work remains marginal or unavailable.

Because the conditions supporting WFH are unevenly distributed across firms, regions, and local digital environments, uniform legal rights may generate unequal gains across workers. WFH can therefore be a valuable tool for work–family reconciliation,

but statutory rights alone may disproportionately benefit mothers already employed in remote-compatible environments. More broadly, our results suggest that flexible-work rights are most effective, and more likely to generate broadly shared gains, when legal eligibility is complemented by the organizational and technological conditions needed to make flexibility effective in practice.

The remainder of the paper is organized as follows. Section 2 describes the institutional background and the data sources. Section 3 outlines the empirical strategy. Section 4 presents the benchmark results and additional labor market outcomes. Section 5 reports robustness checks and Section 6 presents the heterogeneity analysis. Section 7 concludes.

2 Institutional Setting and Data Sources

This section describes the institutional setting underlying the statutory priority rule for remote work in Italy and the administrative data used in the analysis.

2.1 The Italian WFH System

Remote work in Italy has evolved through a sequence of legislative reforms, collective bargaining arrangements, and technological investments, culminating in a regulatory framework that is strongly oriented toward worker protection. Although some firms adopted WFH before COVID-19, the current framework—legally defined as *lavoro agile*—treats it as a structured employment arrangement that combines worker protections with managerial discretion.

The cornerstone of this framework is Law 81/2017, which introduced *lavoro agile* as a contractual arrangement explicitly designed to promote work-life balance. Unlike traditional telework, the law emphasizes flexibility in both work location and working time, allowing employees to alternate between on-site and remote work without changing their contractual status. Such arrangements must be formalized through individual written agreements specifying workloads, rest periods, monitoring procedures, and the right to disconnect. The law also guarantees equal treatment between remote and on-site workers, preventing employers from using this flexibility to modify compensation, career progression, or employment protections.

The regulatory framework expanded substantially during the COVID-19 pandemic. Emergency decrees in 2020 temporarily suspended the requirement for individual agreements and allowed employers to activate WFH unilaterally. This change led to an unprecedented expansion in take-up, familiarized firms and workers with digital coordination tools, and reduced resistance to working from home. Although the emergency regime was subsequently phased out, later reforms reinstated the requirement for individual agreements while preserving greater operational flexibility. As a result, many companies—

especially larger and more digitally advanced firms—retained and institutionalized these practices.

A central feature of the institutional setting is the introduction of the statutory priority rule. Italian law requires that parents of children under age 12 (and caregivers of individuals with severe disabilities) receive priority access to WFH whenever their tasks are compatible with remote execution. Eligible workers therefore receive priority consideration in the allocation of remote-work arrangements. While the rule is legally binding, its practical effect depends on firms' ability to implement it, which in turn reflects organizational and technological constraints.

2.2 Data

Our empirical analysis uses confidential administrative data from the Italian National Social Security Institute (INPS) and the Italian Ministry of Labor and Social Policies, focusing on employed parents, who are the intended beneficiaries of the statutory priority rules.

We construct our sample in several steps. To identify mothers and fathers, we use administrative records from Italy's Universal Child Allowance (*Assegno Unico e Universale*), a child benefit program introduced in March 2022 that provides income support for families with dependent children up to age 21. Eligibility is universal and independent of employment status, and requires families to file an application with INPS. The program's exceptionally high take-up rate (about 94 percent among children born after 2003) allows us to identify parents of school-aged children reliably. We focus on parents whose youngest child was born between 2003 and 2017. For each parent, we compute the age of the youngest child, which underpins our identification strategy based on the statutory cutoff at age twelve.[3] Focusing on the youngest child ensures consistent classification of families with multiple children and improves comparability between households just below and just above the threshold.

Using parent identifiers from the Universal Child Allowance data, we link mothers and fathers to their labor market histories in a matched employer-employee dataset derived from UNIEMENS submissions, the mandatory reports filed by all private-sector, non-agricultural firms employing at least one salaried worker. The matched data span the 2018–2024 period and provide harmonized information at both the worker and firm level. At the individual level, we observe demographic characteristics such as age, nationality, and place of residence. Firm-level variables include sector, geographic location, workforce size, and firm age, measured as the difference between the observation year and the year of establishment.

For each worker-firm match, we observe annual information on contract duration

[3] In the case of twins, we only consider one child.

and termination reasons (e.g., layoffs and resignations), earnings, paid days and weeks, contract type (full-time versus part-time), and broad occupational category (blue-collar, white-collar, managerial).[4] We construct a full-time indicator equal to one for full-time contracts and zero otherwise. To capture within-firm upward mobility, we define a career-progression variable, *Career*, equal to one if a worker moves to a higher occupational category (from blue- to white-collar, or from blue/white-collar to managerial). Finally, we augment the data with measures of sick-leave behavior: (i) an indicator for at least one day of sick-leave absence during the year; (ii) an indicator for at least one short-term spell (one to three days); and (iii) the total number of sick-leave days during the year.

The full sample includes male and female workers, with an average age of about 45 for mothers and 48 for fathers. Using the Universal Child Allowance records, we compute the exact age of the youngest child in years at each observation by combining birth year and birth month information. This ensures precise classification around the age-12 cutoff and accurate alignment between parental labor market outcomes and the child's developmental stage.

We merge the employer-employee records with administrative data on remote-work adoption collected by the Italian Ministry of Labor and Social Policies. These data come from mandatory *lavoro agile* declarations introduced in 2017, which require firms to report all remote-work arrangements to guarantee insurance coverage for off-site work accidents. The dataset provides annual firm-level information for 2018–2024. Records are consistently available since 2020 and include firm along with some key worker demographic characteristics, namely gender, year of birth, and citizenship. We compute, for each firm, the number of employees engaged in WFH by gender. This allows us to construct a firm-level measure of WFH intensity, defined as the share of female (male) employees engaged in remote work over the total number of female (male) employees in the firm.

Appendix Figures A.1 and A.2 provide a descriptive overview of remote-work adoption. Panels (a) and (b) of Figure A.1 report average firm-level WFH intensity across broad industry sectors for women and men, respectively, revealing pronounced sectoral differences consistent with task-based explanations of remote-work feasibility.[5]

[4]For individuals with multiple employment records within a given year, we retain the observation associated with the highest annual earnings, which typically represents the individual's primary employment. In cases where two or more observations report identical earnings, we select the record with the greatest number of days worked.

[5]WFH intensity is highest in transportation and ICT-related services and in financial and professional services, with rates ranging between roughly 11 and 21 percent across mothers and fathers. Intermediate levels of remote work are observed in capital-intensive manufacturing sectors (such as chemicals, pharmaceuticals, utilities, and energy, as well as machinery and automotive manufacturing) reflecting the presence of administrative, engineering, and coordination tasks alongside on-site production activities. In these sectors, WFH intensity generally ranges between 2 and 8 percent. In contrast, sectors characterized by manual, on-site, or customer-facing activities exhibit markedly lower adoption of remote work. Agriculture, construction, wholesale and retail trade, and arts and personal services all display WFH intensities below 5 percent, with retail trade at around 1 percent. Private education and health services also exhibit limited use of remote work.

Figure A.2 shows the evolution of WFH intensity over time for mothers and fathers. Prior to the pandemic, average intensity was below 0.5 percent in 2018–2019. In 2020, it rose sharply to nearly 8 percent for mothers and 4 percent for fathers. Although the share declined thereafter, it remained persistently above pre-pandemic levels—around 4–5 percent for mothers and about 3 percent for fathers in 2021–2022, and between 2 and 3 percent in 2023–2024. This pattern mirrors trends in the Italian Labor Force Survey and indicates that remote work has become a stable organizational feature rather than a transitory adjustment (see the evidence in Basso et al., 2026). Importantly, this descriptive evidence suggests that statutory priority access to WFH can become operative only once firms acquire the technological and organizational capacity to implement remote-work arrangements at scale.

Our working sample consists of parents employed in private, non-agricultural firms that adopted remote work at least once during 2020–2024.[6] Restricting the analysis to adopting firms mitigates selection concerns and ensures comparison among workers in organizational environments where WFH is feasible and the statutory priority rule can operate. We exclude manual workers because their tasks are generally tied to on-site production and cannot plausibly be performed remotely; including them would dilute treatment variation.[7] This restriction removes 906,543 observations for mothers and 1,689,532 for fathers. The resulting dataset contains roughly 2.3 million observations for each gender. The mothers' sample covers 77,181 firms in which at least one female employee engaged in WFH, while the fathers' sample includes 52,487 firms with at least one male WFH user.

Appendix Table A.1 reports summary statistics. Panel (a) presents the full sample of employed mothers and fathers separately, while panels (b) and (c) report the corresponding statistics for workers in high- and low-WFH-intensity firms, defined using the median of the firm-level WFH intensity distribution.[8] The comparison between high-

[6]We focus on the period 2020–2024 for two reasons. First, beginning in 2020, *lavoro agile* declarations are recorded systematically at the worker-week level through the Ministry's online portal, generating harmonized and comprehensive information on remote-work arrangements nationwide. Second, the pandemic accelerated the diffusion of remote-work technologies and managerial practices, generating meaningful variation across firms in their ability to integrate WFH into regular operations. These institutional and technological shifts make the post-2020 period the most relevant for assessing how statutory priority rights translate into actual labor market adjustments.

[7]For placebo analyses, we construct a comparison group consisting of parents employed in firms that never adopted remote-work arrangements during the sample period. Examining outcomes in these firms, where the statutory entitlement cannot affect actual working conditions, allows us to verify that the discontinuities observed in treated firms do not reflect unrelated age-based patterns or other confounding factors. We conduct a complementary placebo test on manual workers employed in firms that do use remote work. In this setting, the statutory entitlement should again not affect actual work arrangements.

[8]Panels (a) and (b) of Appendix Figure A.3 provide additional descriptive evidence on the relationship between firm-level remote-work intensity and mothers and fathers' labor market outcomes, respectively. The figure reports average annual wages of mothers/fathers across quartiles of the firm-level WFH intensity distribution. Both mothers and fathers employed in firms with higher WFH intensity earn systematically higher wages. Average annual earnings increase steadily from the lowest to the highest quartile of the WFH intensity distribution, with a difference of roughly €7,000 for mothers and €20,000 for fathers

and low-intensity firms reveals systematic differences in wages, weeks and days worked, citizenship, and firm characteristics (including size and age), especially among mothers, consistent with strong sorting across organizational environments with different remote-work capacity. Differences among fathers are substantially smaller.

3 Methodology

3.1 Empirical Strategy

This section describes the research design used to estimate the causal effect of the statutory priority rule for WFH on parents' labor market outcomes. We exploit a sharp regression discontinuity design (RDD) centered on the age threshold that grants parents of children under twelve priority access to remote-work arrangements. The institutional rule generates an externally imposed shift in eligibility that is plausibly orthogonal to individual labor market trajectories, firm policies, and unobserved determinants of wages and work activity.

Let Y_{ifst} denote a labor market outcome for parent i employed in firm f, sector s, and year t, such as log annual wages, paid weeks, or paid days. Let R_{it} be the running variable, defined as the normalized age of the youngest child in years, so that zero corresponds to the twelfth birthday. The treatment indicator equals one for parents eligible for WFH priority and zero otherwise, i.e., $T_{it} = \mathbb{1}(R_{it} \leq 0)$.

We estimate the following local linear specification on either side of the cutoff:

$$Y_{ifst} = \beta_0 + \beta_1 T_{it} + g(R_{it}) + X'_{it}\beta_2 + Z'_{ft}\beta_3 + \delta_t + \theta_s + \varepsilon_{ifst}, \tag{1}$$

where, $g(R_{it})$ is a first-order local polynomial fitted separately on each side of the cutoff. The vector X_{it} includes individual controls, such as workers' age and an indicator for immigrant status, while the vector Z_{ft} contains firm-level characteristics, namely firm age and the number of employees as a measure of firm size. To increase the precision of our estimates, we also include year fixed effects, δ_t, which absorb aggregate shocks common across workers, and one-digit sector fixed effects, θ_s, which capture systematic differences in wage-setting practices, remote-work feasibility, and labor demand conditions.

Consistent with best practices in the RDD literature, estimation employs the MSE-optimal bandwidth and the robust bias-correction procedure proposed by Calonico et al. (2020) to ensure valid inference in finite samples. The design therefore compares parents whose youngest child is marginally younger than twelve with those whose child is marginally older, under the identifying assumption that, absent the priority rule, potential outcomes evolve smoothly with the child's age.

between the bottom and top quartiles. While purely descriptive and not causal, this pattern highlights the strong correlation between readiness for remote work and parents' economic outcomes.

The parameter of interest, β_1, captures the discontinuous change in outcomes at the eligibility threshold. Given the sample restrictions described above — namely, firms with some remote-work adoption and non-manual occupations — the estimates pertain to parents employed in settings where the statutory priority rule can plausibly affect working arrangements. Within this sample, however, eligibility does not necessarily translate into remote-work access: actual access remains mediated by firms' implementation and workers' take-up. Formally, let π_t denote the probability that statutory eligibility translates into effective remote-work access, where this probability may depend on workers' caregiving constraints and the organizational environment.[9] The ITT effect at the threshold can then be decomposed as:[10]

$$\begin{aligned} \beta_1 &= \mathbb{E}[Y_{ifst} \mid T_{it} = 1] - \mathbb{E}[Y_{ifst} \mid T_{it} = 0] \\ &= \pi_t \underbrace{\mathbb{E}[Y_{ifst}(1) - Y_{ifst}(0)]}_{\text{causal effect of WFH access}}. \end{aligned} \tag{2}$$

where, $\mathbb{E}[Y_{ifst}(1) - Y_{ifst}(0)]$ denotes the causal effect of WFH access on the labor market outcome, while π_t captures the implementation margin—the extent to which statutory eligibility translates into effective remote-work access. The decomposition is intended to formalize the two margins underlying the ITT interpretation: eligibility can affect outcomes only insofar as it changes access, and the resulting effect depends on the labor market benefits of WFH access once granted. Accordingly, β_1 identifies the intention-to-treat (ITT) effect of statutory eligibility among parents in our estimation sample.

We expect the implementation margin, π_t, to be limited before the pandemic and in low-WFH environments, where eligibility has little practical scope to change working arrangements, and stronger in high-WFH-intensity firms after 2020. Differences in β_1 across groups or periods may therefore reflect variation in the implementation margin, variation in the benefits of WFH access, or both.

Appendix B presents a simple conceptual framework that provides a microfoundation for this decomposition. The framework shows how the ITT effect combines an implementation margin—the probability that statutory eligibility translates into remote-work access—and a benefit margin reflecting the labor-market gains generated by such access.

[9] Mandatory *lavoro agile* declarations do not identify WFH take-up at the individual level. Instead, they allow us to construct a measure of registered WFH arrangements at the firm-year-worker gender-worker age-worker citizenship cell level, as in Basso et al. (2026). Because these declarations contain no information on whether workers have children, let alone the age of their youngest child, the resulting cells cannot be conditioned on child age and therefore cannot support a first-stage discontinuity estimate at the age-12 eligibility threshold.

[10] This decomposition abstracts from heterogeneity in the causal effect of WFH access across workers. With heterogeneous treatment effects, the second term should instead be interpreted as the average effect among workers whose remote-work access responds to statutory eligibility.

3.2 RDD Validity Tests

Before turning to the main results, we assess the validity of the identifying assumptions underlying the regression discontinuity design. A key requirement is that the assignment variable—the age of the youngest child—cannot be manipulated around the eligibility threshold. We first test for continuity of the forcing variable using the local polynomial density estimator of Cattaneo et al. (2020). A discontinuity at the cutoff would indicate potential manipulation, such as parents misreporting children's ages to gain priority access to WFH arrangements.

Panels (a) and (b) of Appendix Figure A.4 plot the estimated density of the forcing variable around the threshold for mothers and fathers, respectively. In both cases, we find no evidence of a statistically meaningful discontinuity (mothers: difference $= -0.164$, p-value $= 0.869$; fathers: difference $= -0.750$, p-value $= 0.453$). This finding is consistent with the institutional context: children's ages are drawn from population registers and transmitted directly to INPS through administrative records, rather than self-reported by workers or employers. These records are routinely cross-validated across administrative databases, and false declarations carry criminal penalties, leaving effectively no scope for strategic manipulation of the assignment variable near the cutoff.

We next assess covariate continuity at the threshold. For each predetermined characteristic, we estimate a local linear regression of the covariate on a first-order polynomial in the running variable and an indicator for eligibility. A statistically insignificant coefficient on eligibility is interpreted as evidence of local random assignment (Lee, 2008). Appendix Table A.2 reports the corresponding estimates for mothers and fathers, using MSE-optimal bandwidths selected separately for each covariate. Across all specifications, treatment status does not significantly predict any predetermined characteristic for either mothers or fathers. The same results emerge when we use robust bias-corrected procedures.

As an additional diagnostic, Appendix Figure A.5 displays binned scatterplots of predetermined characteristics against the normalized age of the youngest child, together with fitted values from fourth-order locally weighted regressions estimated separately on each side of the cutoff (panel (a) for mothers and panel (b) for fathers). The graphical evidence closely mirrors the regression results, revealing no discernible discontinuities at the threshold. Taken together, the density tests, covariance balance checks, and graphical diagnostics strongly support the validity of the RDD and the interpretation of the estimated discontinuity as causal.

Finally, aside from the statutory priority access to remote work, no other policies exhibit a discontinuity at the age-12 threshold. Educational transitions in Italy do not coincide with age 12: entry into lower secondary school typically occurs earlier (around age 11), and there are no changes in compulsory schooling, school schedules, or childcare

entitlements at age 12. Likewise, eligibility for family benefits, tax credits, and child allowances does not shift discretely at this age. After-school programs and public childcare provision are determined at the municipal or school level and are not tied to the child's twelfth birthday.

The one exception is parental leave, for which eligibility also changes at the age-12 cutoff. Since our treatment indicator equals one for children below age 12, parents on the eligible side of the threshold retain access to parental leave and may use it to take paid absences from work when caregiving needs arise. If anything, this would reduce annual earnings just below the cutoff, generating a discontinuity opposite in sign to the one we estimate. Any bias arising from parental-leave eligibility would therefore attenuate, rather than inflate, the estimated effects of WFH priority.

More importantly, the concurrent parental-leave threshold is unlikely to threaten identification. First, if the estimated discontinuity were driven by parental-leave eligibility, one would expect a similar jump at the age-12 threshold before the large-scale diffusion of WFH. Yet our pre-pandemic placebo estimates in subsection 5.1 show no discontinuity in 2018–2019. Moreover, no comparable effects emerge among manual workers or in low-WFH-intensity firms, settings in which remote work is either technologically infeasible or only marginally used.[11]

Second, *Congedo Parentale* is a long-standing policy introduced in 2000 (Law 53/2000) and consolidated in 2001 (Legislative Decree 151/2001), well before both our sample period and the widespread adoption of remote work. Any discontinuity generated by parental-leave eligibility should therefore be time invariant. Consistent with this, our difference-in-discontinuities estimates explicitly difference out any time-invariant discontinuity at age 12 and closely mirror the baseline RDD results (see subsection 5.3). Taken together, this evidence indicates that the estimated discontinuity reflects statutory WFH eligibility rather than parental-leave entitlements.

4 The Impact of Statutory Priority to Work from Home

We now provide detailed evidence on the labor market effects of statutory WFH priority using the research design described in the previous section.

4.1 Main Results

The analysis proceeds in three steps. We first estimate the baseline effect of statutory WFH priority on the wages of mothers and fathers employed in non-manual occupations

[11] The potential role of parental leave is further limited by its age profile: take-up is concentrated in early childhood and declines sharply thereafter. According to the INPS 2025 Annual Report, average use stabilizes at approximately 13–14 days per year from age seven onward. See https://www.inps.it/it/it/dati-e-bilanci/rapporti-annuali/xxiv-rapporto-annuale.html.

and in firms that use remote work (i.e., firms that have adopted at least one remote-working contract). We then examine whether these effects vary with the intensity of firm-level WFH adoption, which captures how central remote work is within the firm's overall work organization. Finally, we study whether wage effects are accompanied by changes in paid work activity, shedding light on the channels through which statutory flexibility translates into labor market outcomes.

Baseline wage effects. Table 1 reports baseline RDD estimates with log annual wages as the outcome. The sample includes female (male) employees who are mothers (fathers) and is restricted to observations within the MSE-optimal bandwidth. Columns (1)–(4) progressively add individual controls, firm characteristics, and year–sector fixed effects. Panel (a) reports results for mothers, and panel (b) for fathers.

For mothers, the estimated discontinuity at the eligibility threshold is positive and statistically significant across all specifications. Bias-corrected coefficients range from 0.009 to 0.011, implying an average wage premium of about 0.9–1.1 percent at the cutoff.

The stability of the estimates across specifications supports a causal interpretation of the effect of eligibility for WFH priority, rather than one driven by compositional changes around the threshold. Parents may select into WFH-adopting firms when their children are young, anticipating the potential benefits of the rule. Such selection, however, is unlikely to generate a discontinuity at age 12, since it would typically occur well before the child approaches the eligibility cutoff. Nor is there a clear reason to expect parents to leave WFH-adopting firms precisely when the entitlement expires.

To further assess whether the expiration of WFH priority at age 12 induces compositional changes among employed parents, we test for discontinuities at the cutoff in dismissals for justified reason, voluntary resignations, and continued employment. These outcomes capture relevant margins through which the composition of workers observed around the threshold could change. We find no statistically significant discontinuities in any of these outcomes (results available upon request), reducing concerns that the estimated wage effects reflect selective changes in the composition of employed workers around the cutoff.

By contrast, panel (b) shows no statistically significant discontinuity for fathers. This null result is consistent with evidence that fathers in Italy bear, on average, a smaller share of childcare and household responsibilities. As a consequence, greater access to flexibility is less likely to relax binding time constraints or induce systematic changes in fathers' labor supply and earnings.

Heterogeneity by firm-level WFH intensity. Table 2 examines whether the effect of WFH priority varies with the extent of remote-work use within firms. We split the sample at the median of firm-level WFH intensity, defined as the average share of female or male employees with registered WFH arrangements within the firm. This distinction

separates firms where remote work is used more extensively from those where it remains relatively marginal.

The results reveal substantial heterogeneity for mothers and no significant effects for fathers in either group. For mothers employed in higher-WFH-intensity firms, the discontinuity in log wages is positive, precisely estimated, and approximately twice the size of the baseline effect: bias-corrected coefficients range from 0.016 to 0.023, corresponding to wage gains of 1.6–2.3 percent.[12] In lower-intensity firms, by contrast, estimates are close to zero and never statistically significant.

As a benchmark for economic magnitude, our estimated 2-percent earnings gain corresponds to roughly 6 percent of the short-run motherhood wage penalty in Italy documented by Kleven et al. (2025).[13] Although these comparisons refer to different populations and institutional settings and should therefore be interpreted as indicative rather than exact, they suggest that the estimated effects are economically meaningful.

An additional consideration concerns the population identified by the RDD. Our estimates pertain to mothers whose youngest child is close to the age-12 eligibility threshold, whereas the statutory priority applies more broadly to mothers with younger children. The estimated 2-percent earnings gain should therefore be interpreted as a local effect for mothers at this relatively late stage of the child-penalty trajectory. To the extent that work–family coordination constraints are more pronounced when children are younger, the effects of WFH eligibility could also be larger earlier in the child-penalty trajectory. In this sense, our estimates may provide a conservative benchmark of the potential earnings effects of WFH eligibility for mothers of younger children.

Because our RDD identifies the ITT effect of statutory eligibility rather than the effect of WFH access itself, the stronger effects in high-intensity firms may reflect two distinct margins: a greater probability that eligibility translates into a change in working arrangements and larger benefits from WFH access once granted. In high-intensity firms, eligibility may be more likely to affect working arrangements, while established remote-work practices and infrastructure may also increase the gains from WFH access. In low-intensity firms, either or both margins may be weaker, attenuating the ITT effect. As discussed in Section 3, our design does not separately identify their contributions.

These findings suggest that legal eligibility alone is not sufficient for statutory WFH

[12] To assess whether the wage effects are specific to the pandemic period, we re-estimate the main specification separately for 2020–2021 and 2022–2024, distinguishing between high- and low-WFH-intensity firms (Appendix Table A.3). Among mothers in high-intensity firms, the estimated wage effect is positive and statistically significant during the pandemic and remains positive and of similar magnitude after 2021, although less precisely estimated. Estimates for low-intensity firms remain close to zero in both periods. This evidence indicates that the main results are not confined to the pandemic years. For fathers, we find no significant effect during the pandemic and only limited, statistically less robust evidence of wage gains after 2021 in high-intensity firms (results available upon request).

[13] The magnitude is also broadly comparable to the productivity gains reported by Bloom et al. (2015) among Chinese call-center employees who voluntarily worked from home: our estimate corresponding to about 15 percent of their effect.

rights to generate economic gains. The effects are concentrated in firms where remote work is a relevant margin of adjustment, consistent with evidence that the value and persistence of WFH depend on workplace characteristics, coordination requirements, task organization, and the broader feasibility of remote work (Bloom et al., 2015; Barrero et al., 2021; Barrero et al., 2023; Emanuel and Harrington, 2024; Makridis and Schloetzer, 2026).

Effects on paid work activity. Table 3 investigates whether the wage effects documented above are accompanied by changes in paid work activity, measured by paid weeks and paid days. Among mothers employed in high-WFH-intensity firms, eligibility for WFH priority increases paid work along both margins: eligible mothers work approximately 0.27 additional paid weeks and 1.76 additional paid days per year. These estimates are remarkably stable across specifications controlling for individual characteristics, firm attributes, and year–sector fixed effects. By contrast, the corresponding effects in low-WFH-intensity firms are small and statistically insignificant. Fathers exhibit no detectable changes in paid weeks or paid days in either group.[14]

The increase in paid weeks and paid days, together with the absence of a detectable effect on labor-market exit discussed above, provides a natural channel for the earnings gains documented in Table 2. The evidence suggests that statutory WFH priority operates primarily by reducing temporary interruptions to paid work arising from day-to-day caregiving demands, rather than through changes in labor-market exit at the eligibility threshold. This interpretation is consistent with the view that greater workplace flexibility relaxes short-run work–family coordination constraints, allowing mothers to supply paid work more regularly over the year (Goldin, 2014; Mas and Pallais, 2017; Kleven et al., 2019; Del Boca et al., 2020; Heggeness and Suri, 2021; Cortés and Pan, 2023).

Panel (a) of Appendix Figure A.6 plots the main outcomes for high-WFH-intensity firms against the normalized age of the youngest child around the cutoff. Each graph displays a fourth-order polynomial fit, where the number of bins is selected using the mimicking-variance evenly-spaced procedure based on polynomial regressions. Panel (b) displays the corresponding plots for low-intensity firms. This graphical evidence closely mirrors the previous regression results.

[14]We also examine cross-spousal spillovers, asking whether one partner's exposure to high-WFH-intensity firms affects the labor market outcomes of a spouse with limited or no access to remote work. Table A.4 considers fathers employed in non-WFH firms, while Table A.5 focuses on blue-collar fathers. In both cases, effects on wages, paid weeks, and paid days are small and statistically insignificant, indicating no detectable spillovers from wives' exposure to high-WFH-intensity firms to male partners. Results for mothers are also largely null. Tables A.6–A.7 show no significant effects for women employed in non-WFH firms. The only notable result concerns blue-collar women (Table A.7), who experience a modest but statistically significant wage decline when their partners are employed in high-WFH-intensity firms. Since this effect is not accompanied by changes in paid weeks or paid days, it does not appear to operate through changes in paid work activity. A possible interpretation is that household adjustments induced by one partner's greater flexibility increase caregiving demands placed on women employed in less flexible jobs. Overall, cross-spousal spillovers appear limited and, when present, are concentrated among blue-collar women whose partners work in high-WFH-intensity firms.

Given that wage and paid-work effects are concentrated among mothers employed in high-WFH-intensity firms, the remainder of the analysis focuses on this subset. This restriction allows us to study the mechanisms through which the statutory priority rule translates into labor market adjustments in environments where the entitlement is plausibly implemented. In settings where remote work remains marginal, eligibility rarely changes actual work arrangements, making further analysis less informative. Moreover, in light of the consistently null effects for fathers, the subsequent analysis concentrates on mothers, with results for fathers discussed more briefly in the text or reported in the appendix.

4.2 Additional Labor Market Outcomes

We extend the analysis beyond wages and paid work activity to examine whether eligibility for WFH priority affects other dimensions of job quality. Specifically, we examine three additional outcomes: the probability of working full-time, the likelihood of career advancement, and the probability of taking at least one sick-leave day during the year. Table 4 reports RDD estimates for these outcomes for mothers in high–WFH-intensity firms, the subset of employers for which the priority rule is actively implemented.

Column (1) shows no statistically significant discontinuity in the probability of working full-time at the eligibility threshold. Eligibility for WFH priority therefore does not induce mothers to increase formal contractual hours. This contrasts with evidence from Basso et al. (2026), who find that remote work reduces part-time employment among mothers of young children, suggesting that the margin along which flexibility operates depends on the age of the child. The full-time/part-time margin appears more responsive among mothers of younger children, while for mothers of children around age 12, flexibility operates primarily through days worked. This pattern is consistent with evidence that flexible work arrangements help workers stabilize existing schedules rather than expand them (e.g., Allen et al., 2015; Chung and van der Lippe, 2020), as well as with the decline in childcare needs as children grow older.

Column (2) reports no effect on the probability of promotion or occupational upgrading.[15] This result aligns with the literature emphasizing that career progression depends on organizational norms, face-to-face interactions, and informal networks that are slower to adjust to hybrid work environments (Goldin, 2014; Mas and Pallais, 2020). Even when flexibility is legally supported, eligibility alone may therefore be insufficient to alter promotion processes that remain tied to visibility or in-office presence.

[15] We also create forward-looking indicators of medium-term career prospects, identifying workers with potential for advancement within three and four years in the 2020–2021 cohorts. These variables capture both realized and prospective progression within the temporal limits of the data and firms' promotion horizons. When used as outcomes in the RDD framework, they yield similar results, showing no detectable effect of WFH priority on subsequent career advancement (results available upon request).

Finally, Column (3) shows no detectable effect on the probability of taking at least one sick-leave day during the year.[16] Although remote work can reduce commuting burdens and increase day-to-day autonomy, these channels do not necessarily translate into lower recorded sickness absence. Recent evidence suggests that remote work may increase sickness presenteeism—working while ill—which can offset potential declines in formal absence (Ruhle and Schmoll, 2021). A recent scoping review similarly documents substantial heterogeneity in telework's health effects, with many studies finding little change in sickness-related absences (Nowrouzi-Kia et al., 2024).[17]

Taken together, the null effects on full-time status and sickness absence suggest that the gains in paid days and weeks may reflect fewer interruptions to paid work, possibly related to childcare needs, rather than broader changes in contractual terms. The absence of effects on promotions operates on a different margin but is consistent with WFH priority having limited effects on career advancement. This is noteworthy because in-person interaction may facilitate communication and monitoring within organizations in ways that are harder to reproduce remotely (Choudhury et al., 2025). Overall, eligibility appears to ease short-run work–family coordination frictions without substantially altering longer-run, more structural features of employment (Chung and van der Lippe, 2020; Barbieri et al., 2025).

5 Robustness Checks

This section further assesses the credibility of the RDD design and provides additional evidence supporting the interpretation of the estimates as ITT effects of statutory WFH priority. The exercises address three complementary issues: (i) whether discontinuities arise in settings where the entitlement has little scope to affect working arrangements; (ii) the sensitivity of the results to alternative RDD specifications; and (iii) whether the discontinuity strengthened once remote work became operationally relevant, using a difference-in-discontinuities design that nets out any time-invariant jump at the age-12 threshold unrelated to WFH diffusion.

5.1 Placebo Tests

We first examine groups for which statutory priority should not translate into changes in working arrangements. These checks serve as mechanism-based placebo tests.

Non-WFH firms. Appendix Table C.1 reports RDD estimates for mothers employed in firms that never adopted WFH practices after 2020. In these environments, eligibility cannot affect actual work organization, so no effect should arise if our mechanism is correct.

[16] Using alternative sickness outcomes—such as the probability of a short-term spell or the total number of sickness days—we again find no discontinuity at the threshold (results available upon request).

[17] Similar negligible effects are found for fathers (results available upon request).

Consistent with this logic, the estimated wage coefficients are small, precisely estimated, and never statistically significant. This result helps rule out alternative explanations based on life-cycle adjustments around children's age (e.g., tenure accumulation, schooling transitions, or evolving labor supply profiles) and reinforces the ITT interpretation: statutory eligibility matters only where firms can implement remote work in practice.

Manual workers in high-WFH-intensity firms. Appendix Table C.2 repeats the analysis for manual workers employed in high-WFH-intensity firms. Although these firms actively use remote work, manual tasks are intrinsically location-bound and therefore generally incompatible with WFH. Accordingly, estimated effects on wages and paid work activity are small and statistically insignificant. This finding, consistent with the task-based nature of remote-work feasibility emphasized by Dingel and Neiman (2020), is conceptually important because it shows that, even within WFH-capable firms, statutory priority improves outcomes only for workers whose tasks are compatible with remote execution.

Pre-pandemic tests. Appendix Table C.3 estimates Equation (1) using pre-pandemic outcomes measured in 2018–2019, restricting the sample to firms classified as high-WFH-intensity in 2020, the first year in which remote work expanded at scale. Before the pandemic, remote work was rarely used, so statutory priority could not realistically change work assignments. Consistent with this institutional setting, the estimated coefficients are small, unstable in sign, and statistically indistinguishable from zero. These null effects support the identifying assumption of smooth potential outcomes around the cutoff and indicate that the pandemic transformed legal eligibility into economically meaningful access to WFH by expanding firms' organizational capacity to implement remote work.

Taken together, these three sets of placebo exercises show that the policy operates precisely through the channels identified in our analysis: when WFH is infeasible, unavailable, or tasks are incompatible with remote execution, no discontinuity emerges.[18]

5.2 Additional RDD-Specific Checks

We next verify that the results are not driven by specific implementation choices in the RDD design.

Donut RDD. Mothers just above the eligibility threshold, i.e., mothers whose youngest child has recently turned twelve, may have formally lost eligibility while still retaining previously granted remote-work arrangements. Such persistence could mechanically smooth the contrast around the cutoff and attenuate the estimated discontinuity, biasing the baseline effects toward zero.

[18] No significant effects emerge for fathers employed in non-WFH firms or for fathers holding blue-collar occupations in high-WFH-intensity firms.

Appendix Table C.4 reports donut RDD estimates that sequentially exclude observations immediately above the threshold, dropping mothers within 0.1 to 0.4 years past the cutoff. By removing observations for recently ineligible mothers, the comparison is restricted to workers who are clearly eligible and workers who are unambiguously no longer entitled to priority access. Across all donut widths, wage effects in high-WFH-intensity firms remain positive, stable, and statistically significant.

Bandwidth choice. Appendix Table C.5 evaluates the sensitivity of the results to alternative bandwidth selection rules. In particular, we report estimates using both conventional MSE-optimal bandwidths (MSE-TWO and MSE-SUM) and bandwidths minimizing coverage error rates (CER, CER-TWO, and CER-SUM).[19] Across all specifications, estimates for high-WFH firms remain positive, precisely estimated, and similar in magnitude. The consistency of the results across selectors that differ in both objective function (MSE versus CER) and implementation (single- versus two-step procedures) indicates that the baseline results are not driven by bandwidth choice and that the estimated discontinuity is genuinely local and statistically robust.

Fake age cutoffs. Appendix Figure C.1 reports placebo RDD estimates using log annual wages as the outcome at "fake" eligibility thresholds constructed symmetrically around the statutory age-12 cutoff. We consider nine placebo thresholds below and nine above the true cutoff, spaced every 0.2 years; to ensure sufficient distance from the actual discontinuity, the lowest and highest placebo thresholds start at 10.8 and 13.2, respectively. Across all placebo cutoffs, coefficients are close to zero and statistically insignificant. Only the true statutory cutoff yields a precise positive estimate. This pattern confirms that the observed effect is tied specifically to legal eligibility for WFH priority rather than to functional-form artifacts or arbitrary splits in the running variable.

5.3 Difference-in-Discontinuities

A remaining concern is that parental behavior may change discretely around children's ages for reasons unrelated to the WFH legislation, e.g., parental leave entitlements, schooling transitions or age-specific investments in children (Cunha and Heckman, 2007; Del Boca et al., 2014; Caucutt and Lochner, 2020; Attanasio et al., 2020). To address this possibility, we complement the RDD approach with the difference-in-discontinuities design used by Grembi et al. (2016), which compares the size of the eligibility discontinuity before and after remote work became operationally relevant. The logic is straightforward: if the discontinuity merely captured age-related parental behavior rather than the policy, similar jumps should appear both before and after the pandemic. Differencing the two

[19] The former ones maximize precision under mean squared error criteria, while the latter ones are designed to improve the finite-sample coverage of confidence intervals, often yielding slightly wider but more conservative local windows.

discontinuities therefore removes smooth age-related patterns and isolates the component attributable to the effective implementation of the policy.

Formally, we restrict the sample to observations with running variable $R_i \in [R_c - h, R_c + h]$, using outcome-specific MSE-optimal bandwidths.[20] We estimate the following local linear specification:

$$\begin{aligned} Y_{ifst} &= \varphi_0 + \varphi_1 R^*_{it} + T_{it}(\gamma_0 + \gamma_1 R^*_{it}) + \text{After}_t[\alpha_0 + \alpha_1 R^*_{it} + T_{it}(\beta_0 + \beta_1 R^*_{it})] \\ &\quad + X'_{it}\eta + Z'_{ft}\mu + \vartheta_s + \epsilon_{ifst}, \end{aligned} \tag{3}$$

where Y_{ifst} denotes the relevant labor market outcome (log annual wages, paid weeks, and paid days) of mother i, employed as a non-manual worker in firm f, sector s and year t; T_{it} is an indicator variable which equals one if the youngest child of mother i is below age 12 at time t; After_t an indicator for the post-pandemic period; and $R^*_{it} = R_i - R_c$ denotes the normalized assignment variable. We also include the same individual controls (X'_{it}) and firm-level covariates (Z'_{ft}) used in Equation (1), together with two-digit sector fixed effects θ_s. The difference-in-discontinuity coefficient, β_0, identifies the effect of statutory eligibility once remote work becomes operationally feasible in the post-pandemic period.[21] Because the entitlement operates through implementation, we estimate the model separately for firms with high- and low-WFH intensity. In high-WFH firms, eligibility can plausibly alter actual access to remote work; in low-WFH firms, instead, eligibility is unlikely to induce any material change in working arrangements.

The empirical estimates strongly support this interpretation. Appendix Table C.6 reports the results for high-WFH-intensity firms. The β_0 coefficient is positive and statistically significant for wages, paid weeks, and paid days. For wages, the estimate is about 0.017, corresponding to a 1.7 percent increase in annual earnings, alongside an increase in paid work activity of roughly 0.19 paid weeks and 1.2 paid days per year. These magnitudes closely mirror the baseline RDD estimates and indicate that the discontinuity becomes economically relevant only after remote work expands widely. By contrast, Appendix Table C.7 reports negligible and insignificant interaction effects for low-WFH-intensity firms. Thus, the evolution of the cutoff effect over time is confined to

[20] For every outcome, we derive two optimal bandwidths from local RDDs estimated separately in the pre-pandemic (2018–2019) and post-pandemic (2020–2024) periods. The bandwidth used in the difference-in-discontinuities specification is then defined as the average of the two.

[21] We additionally perform balance tests using the same difference-in-discontinuity specification (within the MSE-optimal bandwidth) as our main analysis. We regress observable characteristics on the forcing variable (age of the youngest child), an indicator for the post-2020 period, treatment status (having a youngest child aged 12 or below), and the key interaction between treatment status and the post-2020 indicator. We find that all observable characteristics are balanced around the age-12 cutoff before/after 2020. For every covariate, the coefficient on the difference-in-discontinuity estimator (the interaction term) is small in magnitude and statistically indistinguishable from zero at conventional levels, indicating no systematic compositional changes around the cutoff before versus after 2020. Results are available upon request.

organizational environments where WFH is actually implemented.[22]

6 Heterogeneity Analysis

This section examines heterogeneity in the impact of the WFH priority rule along firm and local-context dimensions. Because the baseline effects are concentrated in high-WFH-intensity firms, the analyses by geography, firm size, and firm age focus on this subsample, where statutory eligibility has greater scope to affect outcomes. Local broadband availability, by contrast, captures a contextual condition relevant to the effective use of remote work beyond firms' own WFH intensity. We therefore examine this dimension using the full sample of WFH-adopting firms, allowing us to assess heterogeneity by digital infrastructure without conditioning on high firm-level WFH intensity.

Center–North versus South. Table 5 shows that, even within high-WFH-intensity firms, the effects of the entitlement are concentrated in the Center–North. Eligible mothers experience statistically significant improvements in labor market outcomes, with annual wages rising by about 2.2 percent, accompanied by modest increases in paid weeks and paid days. By contrast, the estimated coefficients for the South are small and statistically insignificant, indicating no detectable effect.

This regional divide is consistent with persistent differences in the economic and productive environment across Italian regions. Firms in the South tend, on average, to display lower levels of management quality, digital adoption, and use of structured organizational practices (Bloom et al., 2016; Pellegrino and Zingales, 2017; Boeri et al., 2021). Although we do not observe these mechanisms directly, such differences may affect the extent to which formal WFH eligibility translates into effective flexibility at work. Where work organization is less formalized, or where remote coordination is less established, the statutory entitlement may have limited scope to change day-to-day work arrangements.

Agglomeration economies, skill complementarities, and innovation activity are also stronger in the Center–North, shaping the diffusion and returns of new organizational practices (Glaeser and Gottlieb, 2009; Duranton and Puga, 2020). More generally, successful implementation of WFH priority access requires clear rules of engagement *and* effective coordination (Aksoy et al., 2022; Flassak et al., 2023), while its productivity effects depend on the degree to which tasks can be modularized and monitored remotely (Jiang et al., 2024).

Firm size. Table 6 reveals marked heterogeneity by firm size: effects are largest among small firms (fewer than 16 employees), where eligibility raises both wages and paid work, weaker in medium-sized firms, and undetectable in large firms (more than 250 employees).[23]

[22] The same analysis on fathers reveals no significant wage effects.

[23] Small firms also exhibit a positive and statistically significant increase in full-time status, suggesting

Both margins of the ITT effect — implementation and benefit, discussed in Section 3 and developed in Appendix B — may contribute to this pattern. On the implementation margin, larger firms may already provide alternative mechanisms for reconciling work and family responsibilities, such as internal flexible-work policies, structured leave arrangements, or informal accommodations, reducing the extent to which statutory eligibility changes working arrangements at the age-12 threshold. Similarly, where firms provide broad access to remote work irrespective of caregiving status, statutory eligibility may induce relatively little additional remote-work use. On the benefit margin, more standardized personnel practices and wage-setting procedures may limit the extent to which greater workplace flexibility translates into additional paid days, paid weeks, or annual earnings. By contrast, more individualized employment arrangements in smaller firms may make statutory priority more consequential at the margin. The larger ITT effects observed in these firms can therefore reflect a stronger implementation margin, larger benefits from WFH access, or both.

Firm age. Table 7 shows that the effects of statutory WFH priority are concentrated among younger firms, defined as those below the median age of 23 years. In these firms, eligibility is associated with higher annual earnings, paid weeks, and paid days. By contrast, the corresponding estimates for older establishments are small and statistically insignificant.

Again, this heterogeneity may operate through the implementation margin, the benefit margin, or both, and our data do not allow us to distinguish between the two channels. The evidence nonetheless suggests that firm age is an additional dimension along which the effectiveness of statutory flexibility rights varies.

One plausible interpretation is that younger firms possess greater organizational adaptability: they tend to adopt new technologies more rapidly, experiment more readily with alternative work practices, and rely on less rigid organizational structures.[24] These features may strengthen both margins of the ITT effect. Greater organizational adaptability may make firms more responsive to eligibility-linked requests, increasing the likelihood that statutory priority translates into a change in working arrangements. It may also facilitate the integration of remote work into existing work practices, increasing the benefits workers derive from WFH access once granted.[25]

that statutory eligibility may affect formal working-time arrangements in this group (results available upon request). Results for fathers, also available upon request, point to a similar concentration of responses among small firms. Although wage effects remain statistically insignificant, the estimated coefficients are larger than in other size categories, and paid weeks and paid days increase modestly only within this group.

[24]Consistent with this view, younger firms and firms led by younger CEOs make more extensive use of remote work (Aksoy et al., 2026), while a broader literature links firm age to innovation and the diffusion of new organizational practices (Haltiwanger et al., 2013; Criscuolo et al., 2014).

[25]Experimental evidence on the benefits of WFH, such as the call-center study by Bloom et al. (2015), comes from environments characterized by highly modular tasks and standardized processes. Such conditions may be easier to develop within younger and more organizationally adaptable firms than in mature

Broadband availability. We next assess whether local digital infrastructure mediates the effectiveness of statutory WFH priority by examining variation in broadband quality across Italian municipalities. Productive remote work is inherently constrained by network capacity: insufficient internet quality at home may prevent legal entitlements from translating into effective flexibility.

We measure broadband quality using average ADSL download speed from the *Broadband Map* compiled by the Italian Communications Authority (AGCOM) in 2019, which captures pre-period connectivity conditions. Although ADSL technology was already being phased out, it provides a consistent nationwide benchmark of baseline digital infrastructure. We match this information to workers' municipalities of residence, which directly determine the technological feasibility of remote work.[26] Appendix Table C.8 shows that the effect of the policy is strongly conditioned by broadband availability. In municipalities above the median in download speed, WFH priority increases paid weeks and paid days and generates modest wage gains. In low-speed areas, effects are instead uniformly small and statistically insignificant. Technological constraints therefore attenuate the policy's effectiveness.

Better connectivity increases the likelihood that eligibility translates into effective remote work (Dettling, 2017; Hjort and Poulsen, 2019; Jin et al., 2023) , while also increasing the quality and usability of remote work once adopted, thereby enhancing its impact on firm productivity and workers' labour market outcomes. These findings highlight the complementarity between digital infrastructure and local economic environments (Couture et al., 2021). Broadband access strengthens labor force attachment and job flexibility (Dettling, 2017; Hjort and Poulsen, 2019; Jin et al., 2023) while enhancing firms' capacity to reorganize tasks and adopt digital work practices (Forman et al., 2012). When infrastructure constraints binds, however, even legally granted flexibility remains difficult to use effectively. This observed heterogeneity therefore reflects differences in local digital readiness across Italian municipalities, beyond the broader regional disparities documented above.

7 Conclusion

This paper studies how a statutory priority right to work from home affects parents' labor market outcomes, exploiting Italy's age-based eligibility rule. We show that eligibility raises mothers' annual earnings and increases paid days and paid weeks, but only in firms where remote work is sufficiently prevalent to make the statutory entitlement operational. The estimated effects are concentrated among mothers employed in high-WFH-intensity

firms operating in traditional sectors.

[26] Results are qualitatively similar when broadband quality is instead matched on the municipality in which the firm operates.

firms, while no comparable responses emerge for fathers. This gender asymmetry is consistent with unequal pre-existing childcare responsibilities and with the policy primarily benefiting the group facing tighter work–family coordination constraints.

The 2-percent increase in annual earnings we document is sizable, particularly given two features of our design that may render the estimate conservative. First, as an intention-to-treat effect, it combines the gains from WFH access with the probability that eligibility translates into a change in working arrangements. Second, identification comes from mothers whose youngest child is close to age 12, a stage of the child-penalty trajectory at which work–family constraints and associated labor market penalties may have already weakened.

The patterns we detect point to a common interpretation. Statutory WFH priority can ease day-to-day work–family coordination when legal eligibility translates into effective remote-work arrangements. Consistent with this interpretation, eligibility increases paid weeks, paid days, and annual earnings, while leaving promotions, full-time employment status, and sickness absence largely unaffected. Heterogeneity across regions, firm size, firm age, and local broadband availability further suggests that the effectiveness of statutory flexibility rights depends on the organizational and technological conditions under which they operate.

The timing of the effects reinforces this interpretation. Statutory WFH priority was already in place before the pandemic, yet we find no detectable effects during this period, when remote work remained uncommon. The effects emerge only after the widespread adoption of remote work during the pandemic and persist beyond the pandemic years. The Italian experience therefore illustrates a broader lesson: legal entitlements to WFH are not self-executing. Occupational compatibility with remote work, firm-level adoption, and local digital infrastructure all shape whether statutory eligibility translates into meaningful labor market gains. Without these complementary conditions, flexibility rights may leave existing inequalities largely unchanged and disproportionately benefit workers already employed in more remote-compatible environments.

This lesson has direct implications for policy design. As statutory rights to request or access flexible working arrangements continue to expand across advanced economies—including through the EU Work–Life Balance Directive and the UK's 2023 Flexible Working Act—strengthening legal protections alone is unlikely to be sufficient. Their effectiveness also depends on the organizational and technological conditions that allow firms to accommodate remote work in practice. Policies aimed at expanding flexibility rights should therefore take account of these complementary conditions, as well as the possibility that uneven access to them may generate unequal gains across workers.

Finally, our findings suggest that statutory WFH priority should be viewed as one component of a broader strategy to reduce gender inequalities in the labor market. Greater workplace flexibility can ease day-to-day work–family balance and improve mothers' la-

bor market outcomes, but lasting progress will also require policies that address childcare provision, the allocation of caregiving responsibilities, parental-leave design, and the incentives shaping career progression.

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Tables

Table 1: WFH and Wages

	(a) Mothers				(b) Fathers			
	(1) Wages	(2) Wages	(3) Wages	(4) Wages	(1) Wages	(2) Wages	(3) Wages	(4) Wages
Conventional	0.010*	0.010**	0.011**	0.009*	0.005	0.005	0.003	0.004
	(0.005)	(0.005)	(0.005)	(0.005)	(0.024)	(0.018)	(0.017)	(0.015)
Bias-corrected	0.011**	0.011**	0.011**	0.009**	0.008	0.008	0.006	0.007
	(0.005)	(0.005)	(0.005)	(0.005)	(0.024)	(0.018)	(0.017)	(0.015)
Robust	0.011*	0.011*	0.011*	0.009	0.008	0.008	0.006	0.007
	(0.006)	(0.006)	(0.006)	(0.006)	(0.025)	(0.018)	(0.018)	(0.016)
Mean outcome	10.133	10.132	10.133	10.133	10.749	10.750	10.744	10.747
Sample	Full	Full	Full	Full	Full	Full	Full	Full
Bandwidth	MSE	MSE	MSE	MSE	MSE	MSE	MSE	MSE
Bandwidth (h)	1.628	1.691	1.584	1.541	3.441	3.073	4.989	4.357
Kernel	TRIANG	TRIANG	TRIANG	TRIANG	TRIANG	TRIANG	TRIANG	TRIANG
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Year FE	No	No	Yes	Yes	No	No	Yes	Yes
Sector FE	No	No	No	Yes	No	No	No	Yes
No. obs. (Left)	1,251,982	1,251,946	1,251,946	1,251,946	1,228,401	1,228,366	1,228,366	1,228,366
No. obs. (Right)	1,063,457	1,063,410	1,063,410	1,063,410	1,087,525	1,087,461	1,087,461	1,087,461
Total Obs.	2,315,439	2,315,356	2,315,356	2,315,356	2,315,926	2,315,827	2,315,827	2,315,827

Notes: Nonparametric Sharp RDD estimates are reported. The outcome variable, shown at the top of each column, is the log of wages. The sample consists of female and male workers who are mothers/fathers, excluding manual workers within the MSE-optimal bandwidth in panels (a) and (b), respectively. Column (1) reports estimates without covariates. Column (2) additionally controls for mothers/fathers' age, a dummy equal to 1 for foreign nationals, firm size (number of employees), and firm age. Columns (3) and (4) further include year fixed effects and one-digit sector dummies, respectively. Robust standard errors, reported in brackets, are heteroskedasticity-consistent.

* $p < 0.10$, ** $p < 0.05$

Table 2: WFH and Wages: High- and Low-WFH-Intensity Firms

	(1) Wages High Intensity	(2) Wages High Intensity	(3) Wages High Intensity	(4) Wages Low Intensity	(5) Wages Low Intensity	(6) Wages Low Intensity
			(a) Mothers			
Conventional	0.014**	0.021***	0.021***	−0.000	0.000	−0.002
	(0.006)	(0.006)	(0.006)	(0.009)	(0.009)	(0.009)
Bias-corrected	0.016***	0.023***	0.023***	−0.003	−0.003	−0.005
	(0.006)	(0.006)	(0.006)	(0.009)	(0.009)	(0.009)
Robust	0.016**	0.023***	0.023***	−0.003	−0.003	−0.005
	(0.007)	(0.007)	(0.007)	(0.010)	(0.010)	(0.010)
Mean outcome	10.352	10.352	10.352	9.906	9.906	9.906
Bandwidth	MSE	MSE	MSE	MSE	MSE	MSE
Bandwidth (h)	1.732	1.354	1.345	1.412	1.290	1.194
Kernel	TRIANG	TRIANG	TRIANG	TRIANG	TRIANG	TRIANG
Controls	No	Yes	Yes	No	Yes	Yes
Year FE	No	Yes	Yes	No	Yes	Yes
Sector FE	No	No	Yes	No	No	Yes
No. obs. (Left)	630,498	630,472	630,472	621,484	621,474	621,474
No. obs. (Right)	528,254	528,221	528,221	535,203	535,189	535,189
Total Obs.	1,158,752	1,158,693	1,158,693	1,156,687	1,156,663	1,156,663
			(b) Fathers			
Conventional	0.001	0.001	−0.000	0.006	0.006	0.008
	(0.026)	(0.025)	(0.024)	(0.032)	(0.020)	(0.014)
Bias-corrected	0.003	0.005	0.003	0.009	0.008	0.010
	(0.026)	(0.025)	(0.024)	(0.032)	(0.020)	(0.014)
Robust	0.003	0.005	0.003	0.009	0.008	0.010
	(0.026)	(0.026)	(0.025)	(0.032)	(0.021)	(0.015)
Mean outcome	10.837	10.835	10.834	10.662	10.661	10.663
Bandwidth	MSE	MSE	MSE	MSE	MSE	MSE
Bandwidth (h)	3.381	4.155	4.542	3.746	3.886	3.266
Kernel	TRIANG	TRIANG	TRIANG	TRIANG	TRIANG	TRIANG
Controls	No	Yes	Yes	No	Yes	Yes
Year FE	No	Yes	Yes	No	Yes	Yes
Sector FE	No	No	Yes	No	No	Yes
No. obs. (Left)	619,122	619,096	619,096	609,279	609,270	609,270
No. obs. (Right)	539,500	539,447	539,447	548,025	548,014	548,014
Total Obs.	1,158,622	1,158,543	1,158,543	1,157,304	1,157,284	1,157,284

Notes: Estimates are from a nonparametric Sharp RDD. The outcome variable (reported at the top of each column) is the log of wages. Specifications (1)–(3) focus on high WFH-intensity firms, while specifications (4)–(6) focus on low WFH-intensity firms. The sample includes only female workers in panel (a) and male workers in panel (b) and excludes manual workers, using the MSE-optimal bandwidth. In columns (1) and (4), no covariates are included. Columns (2) and (5) control for mother/father's age, a foreigner dummy (1 = foreigner), firm size (number of employees), firm age, and year fixed effects. Columns (3) and (6) additionally include one-digit sector fixed effects. Robust standard errors (heteroskedasticity-consistent) are reported in brackets.

** $p < 0.05$, *** $p < 0.01$.

Table 3: WFH and Paid Weeks and Days: High- and Low-WFH-Intensity Firms

	(1) Paid Weeks High Intensity	(2) Paid Days High Intensity	(3) Paid Weeks Low Intensity	(4) Paid Days Low Intensity
		(a) Mothers		
Conventional	0.249***	1.606***	−0.060	−0.305
	(0.082)	(0.519)	(0.132)	(0.822)
Bias-corrected	0.273***	1.756***	−0.111	−0.614
	(0.082)	(0.519)	(0.132)	(0.822)
Robust	0.273***	1.756***	−0.111	−0.614
	(0.094)	(0.600)	(0.149)	(0.935)
Mean outcome	49.134	298.077	49.112	266.757
Bandwidth	MSE	MSE	MSE	MSE
Bandwidth (h)	1.252	1.260	1.012	1.023
Kernel	TRIANG	TRIANG	TRIANG	TRIANG
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes
No. obs. (Left)	631,084	631,084	623,187	623,187
No. obs. (Right)	528,753	528,753	536,607	536,607
Total Obs.	1,159,837	1,159,837	1,159,794	1,159,794
		(b) Fathers		
Conventional	0.031	0.229	0.098	0.593
	(0.115)	(1.311)	(0.141)	(0.901)
Bias-corrected	0.052	0.389	0.110	0.597
	(0.115)	(1.311)	(0.141)	(0.901)
Robust	0.052	0.389	0.110	0.597
	(0.123)	(1.349)	(0.150)	(0.957)
Mean outcome	49.799	295.362	49.196	290.915
Bandwidth	MSE	MSE	MSE	MSE
Bandwidth (h)	2.484	2.857	2.750	2.616
Kernel	TRIANG	TRIANG	TRIANG	TRIANG
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes
No. obs. (Left)	619,596	619,596	610,071	610,071
No. obs. (Right)	540,167	540,167	549,176	549,176
Total Obs.	1,159,763	1,159,763	1,159,247	1,159,247

Notes: Nonparametric Sharp RDD estimates. The outcome variable is reported at the top of each column. Specifications (1) and (2) focus on high WFH-intensity firms, whereas specifications (3) and (4) focus on low WFH-intensity firms. The sample includes only female workers in panel (a) and male workers in panel (b) and excludes manual workers, using the MSE-optimal bandwidth. In each column we add the full set of controls. Robust standard errors (heteroskedasticity-consistent) are reported in brackets.

*** $p < 0.01$.

Table 4: WFH and Other Labor Market Outcomes: Mothers in High-WFH-Intensity Firms

	(1) Full time	(2) Career	(3) Sick leave
Conventional	0.003	−0.002	0.001
	(0.004)	(0.002)	(0.001)
Bias-corrected	0.004	−0.002	0.001
	(0.004)	(0.003)	(0.001)
Robust	0.004	−0.002	0.001
	(0.005)	(0.002)	(0.001)
Mean outcome	0.625	0.039	0.016
Bandwidth	MSE	MSE	MSE
Bandwidth (h)	1.927	1.963	2.578
Kernel	TRIANG	TRIANG	TRIANG
Controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes
No. obs. (Left)	631,084	631,084	558,571
No. obs. (Right)	528,753	528,753	417,475
Total Obs.	1,159,837	1,159,837	976,046

Notes: Nonparametric sharp RDD estimates are reported. The outcome variable is reported at the top of each column. The analysis focuses on high WFH-intensity firms. The sample includes female workers only and excludes manual workers within the MSE-optimal bandwidth. All columns include the full set of controls. Robust standard errors (heteroskedasticity-consistent) are reported in brackets.

Table 5: WFH, Wages, Paid Weeks, and Paid Days by Region: Mothers in High-WFH-Intensity Firms

	(1) Wages Center-North	(2) Paid weeks Center-North	(3) Paid days Center-North	(4) Wages South	(5) Paid weeks South	(6) Paid days South
Conventional	0.020***	0.254***	1.499***	0.006	0.129	1.200
	(0.006)	(0.087)	(0.547)	(0.014)	(0.200)	(1.258)
Bias-corrected	0.022***	0.279***	1.638***	0.005	0.118	1.156
	(0.006)	(0.087)	(0.547)	(0.014)	(0.200)	(1.258)
Robust	0.022***	0.279***	1.638**	0.005	0.118	1.156
	(0.007)	(0.101)	(0.638)	(0.017)	(0.240)	(1.510)
Mean outcome	10.398	49.271	290.199	10.072	48.276	282.206
Bandwidth	MSE	MSE	MSE	MSE	MSE	MSE
Bandwidth (h)	1.354	1.217	1.259	2.073	1.921	1.951
Kernel	TRIANG	TRIANG	TRIANG	TRIANG	TRIANG	TRIANG
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes
No. obs. (Left)	536,179	536,666	536,666	95,712	95,840	95,840
No. obs. (Right)	456,025	456,501	456,501	73,117	73,175	73,175
Total Obs.	992,204	993,167	993,167	168,829	169,015	169,015

Notes: Nonparametric sharp RDD estimates are reported. The outcome variable is reported at the top of each column. Specifications (1)–(3) focus on high WFH-intensity firms located in the Center-North, while specifications (4)–(6) focus on high WFH-intensity firms located in the South. The sample consists of female workers only and excludes manual workers within the MSE-optimal bandwidth. All columns include the full set of controls. Robust standard errors (heteroskedasticity-consistent) are reported in brackets.

** $p < 0.05$, *** $p < 0.01$.

Table 6: WFH, Wages, Paid Weeks, and Paid Days by Firm Size: Mothers in High-WFH-Intensity Firms

	(1) Wages <16	(2) Wages >16&<250	(3) Wages >250	(4) Paid weeks <16	(5) Paid weeks >16&<250	(6) Paid weeks >250	(7) Paid days <16	(8) Paid days >16&<250	(9) Paid days >250
Conventional	0.040**	0.027**	0.003	0.597*	0.158	0.126	2.696	1.054	0.840
	(0.020)	(0.012)	(0.006)	(0.312)	(0.150)	(0.087)	(1.934)	(0.944)	(0.551)
Bias-corrected	0.046**	0.031***	0.002	0.696**	0.181	0.115	3.207*	1.171	0.766
	(0.020)	(0.012)	(0.006)	(0.312)	(0.150)	(0.087)	(1.934)	(0.944)	(0.551)
Robust	0.046**	0.031**	0.002	0.696*	0.181	0.115	3.207	1.171	0.766
	(0.023)	(0.013)	(0.008)	(0.364)	(0.179)	(0.104)	(2.284)	(1.127)	(0.659)
Mean outcome	10.044	10.351	10.391	48.562	48.861	49.322	284.541	287.931	289.752
Bandwidth	MSE	MSE	MSE	MSE	MSE	MSE	MSE	MSE	MSE
Bandwidth (h)	1.838	1.465	1.595	1.482	1.577	1.438	1.628	1.611	1.457
Kernel	TRIANG	TRIANG	TRIANG	TRIANG	TRIANG	TRIANG	TRIANG	TRIANG	TRIANG
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. obs. (Left)	43,056	173,086	447,453	43,086	173,230	447,962	43,086	173,230	447,962
No. obs. (Right)	31,249	128,280	400,927	31,272	128,401	401,382	31,272	128,401	401,382
Total Obs.	74,305	301,366	848,380	74,358	301,631	849,344	74,358	301,631	849,344

Notes: Nonparametric sharp RDD estimates are reported. The outcome variable is reported at the top of each column. Specifications (1), (4), and (7) focus on firms with fewer than 16 employees; specifications (2), (5), and (8) focus on firms with 16–250 employees; and specifications (3), (6), and (9) focus on firms with more than 250 employees. The sample includes female workers only and excludes manual workers in high WFH-intensity firms within the MSE-optimal bandwidth. All columns include the full set of controls. Robust standard errors (heteroskedasticity-consistent) are reported in brackets.
* $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

Table 7: WFH, Wages, Paid Weeks, and Paid Days by Firm Age: Mothers in High-WFH-Intensity Firms

	(1) Wages <median	(2) Wages >median	(3) Paid weeks <median	(4) Paid weeks >median	(5) Paid days <median	(6) Paid days >median
Conventional	0.030***	0.008	0.342***	0.115	2.699***	0.437
	(0.010)	(0.007)	(0.124)	(0.095)	(0.811)	(0.616)
Bias-corrected	0.033***	0.008	0.380***	0.115	2.983***	0.371
	(0.010)	(0.007)	(0.124)	(0.095)	(0.811)	(0.616)
Robust	0.033***	0.008	0.380***	0.115	2.983***	0.371
	(0.011)	(0.008)	(0.143)	(0.114)	(0.927)	(0.737)
Mean outcome	10.254	10.449	48.693	49.556	286.062	291.875
Bandwidth	MSE	MSE	MSE	MSE	MSE	MSE
Bandwidth (h)	1.314	1.755	1.287	1.496	1.214	1.491
Kernel	TRIANG	TRIANG	TRIANG	TRIANG	TRIANG	TRIANG
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes
No. obs. (Left)	336,929	293,543	337,212	293,872	337,212	293,872
No. obs. (Right)	239,902	288,319	240,109	288,644	240,109	288,644
Total Obs.	576,831	581,862	577,321	582,516	577,321	582,516

Notes: Nonparametric sharp RDD estimates are reported. The outcome variable is reported at the top of each column. Odd-numbered specifications focus on young firms (below the median age), while even-numbered specifications focus on mature firms (above the median age). The sample includes only female workers and excludes manual workers in high WFH-intensity firms within the MSE-optimal bandwidth. All columns include the full set of controls. Robust standard errors (heteroskedasticity-consistent) are reported in brackets.

*** $p < 0.01$.

Online Appendix

When the Right to Work from Home Works

Marco Alberto De Benedetto, Maria De Paola, Marco Francesconi

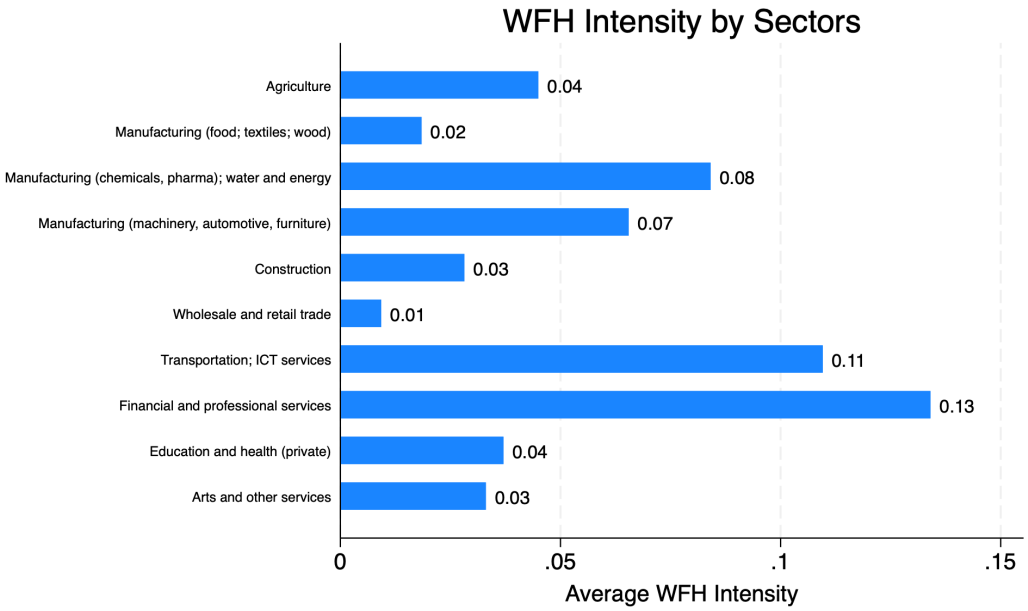
A. Supplementary Figures and Results
(covering Sections 2–4) i

B. Conceptual Framework
(covering Section 3) xiii

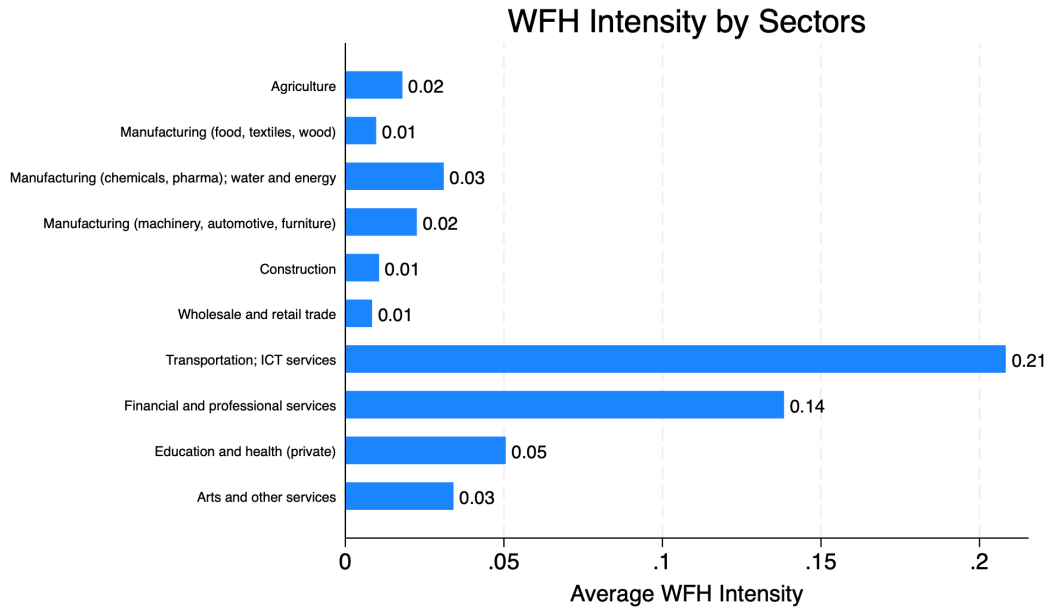
C. Robustness, Placebo and Heterogeneity Analyses
(covering Sections 5 and 6) xvii

A Supplementary Figures and Results

Figure A.1: Firm-level Intensity of Work from Home by Industrial Sector



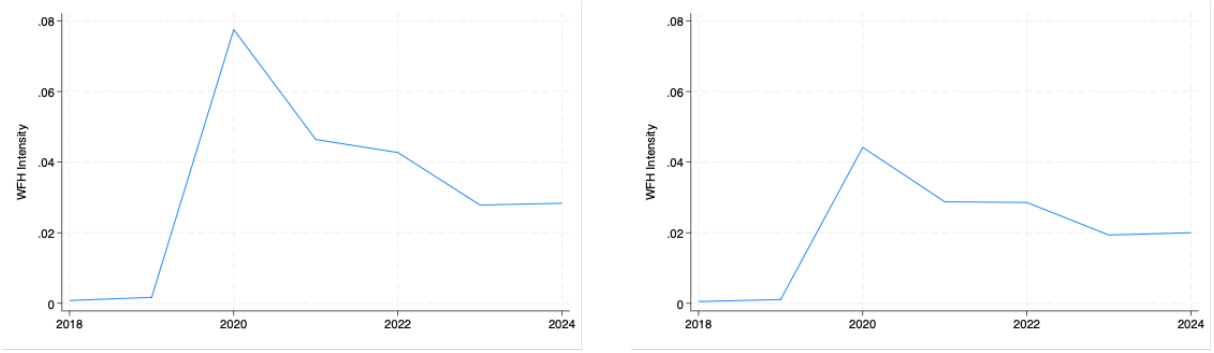
(a) Mothers



(b) Fathers

Notes: This figure reports the average firm-level intensity of work from home (WFH) across broad industry sectors. WFH intensity is defined as the share of female/male employees working remotely over the total number of female/male employees in the firm, averaged within each sector in panels (a) and (b), respectively. The underlying data come from mandatory remote-work declarations collected by the Italian Ministry of Labor and Social Policies and are matched to employer–employee administrative records. The sample covers private, non-agricultural firms in the period 2020–2024.

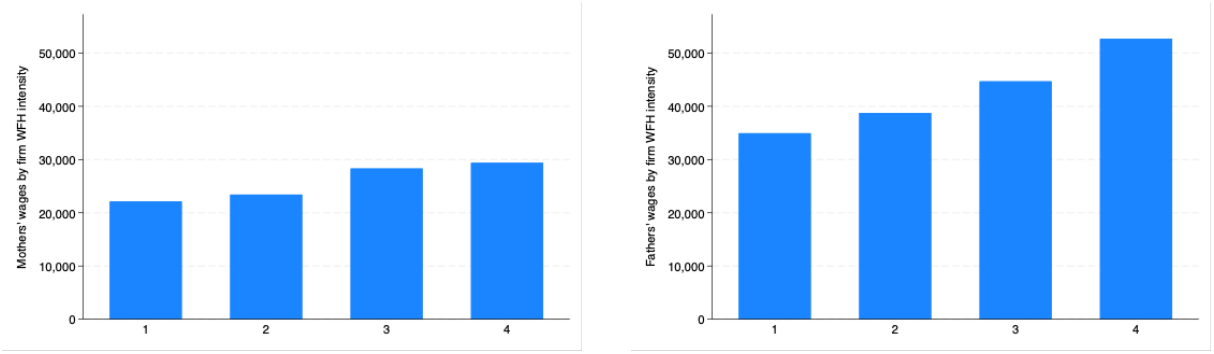
Figure A.2: Evolution of Firm-Level WFH Intensity Over Time



(a) Mothers (b) Fathers

Notes. This figure shows the evolution of average firm-level work-from-home (WFH) intensity over time. WFH intensity is measured as the share of female/male employees engaged in remote work within each firm and then averaged across firms by year in panels (a) and (b), respectively. The data are based on administrative declarations of remote-work arrangements collected by the Italian Ministry of Labor and Social Policies and matched to employer–employee records. The sample includes private, non-agricultural firms observed between 2018 and 2024. The figure highlights the sharp increase in remote-work adoption starting in 2020 and the persistence of WFH intensity above pre-pandemic levels in subsequent years. Trends are consistent with evidence from the Italian Labor Force Survey (ISTAT).

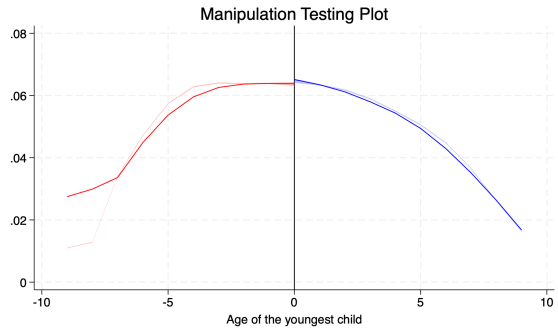
Figure A.3: Annual Earnings of Mothers and Fathers by Firm-Level WFH Intensity



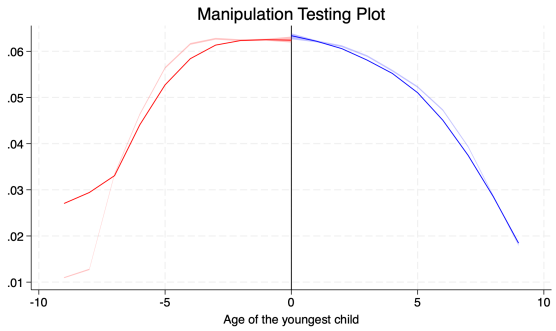
(a) Mothers (b) Fathers

Notes. Average annual earnings of mothers (panel a) and fathers (panel b) by quartiles of firm-level WFH intensity, defined as the share of female and male employees working remotely within the firm. Sample restricted to private-sector, non-agricultural firms with at least one remote worker during 2020–2024, excluding manual occupations.

Figure A.4: Manipulation of the Forcing Variable at the Cutoff



(a) Mothers

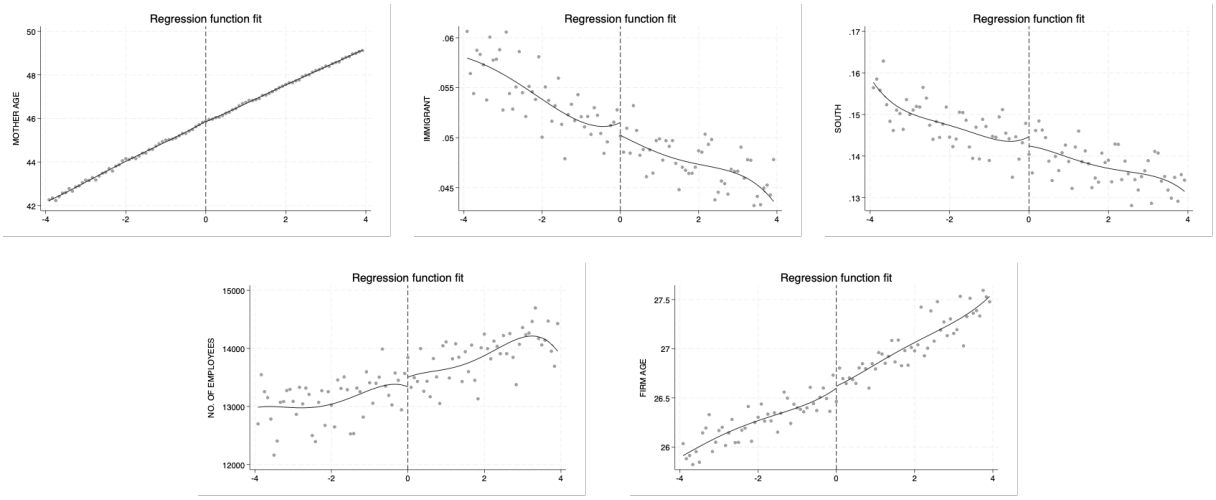


(b) Fathers

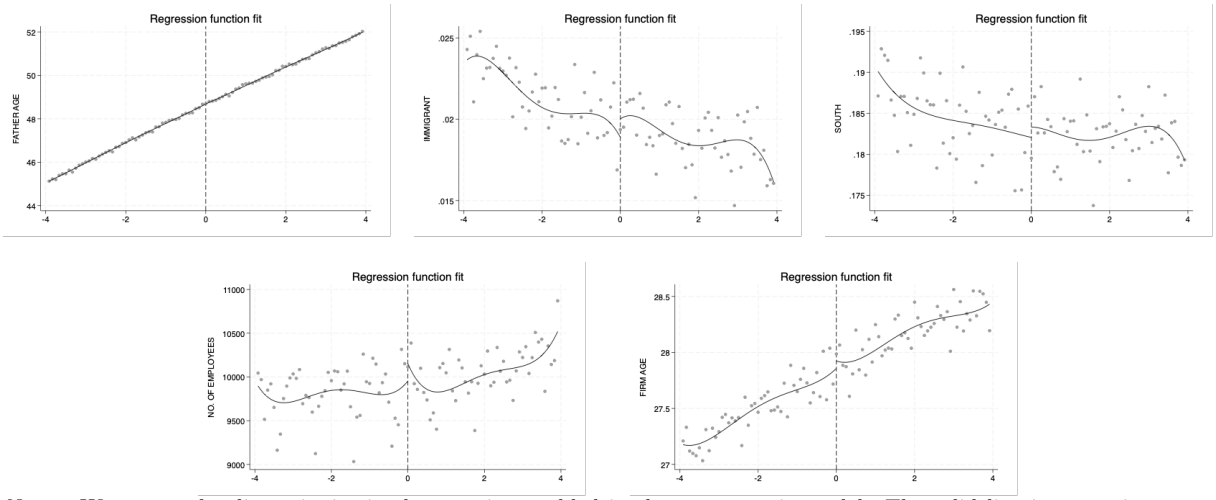
Notes: We test for the manipulation of the forcing variable (age of the youngest child) at the threshold by employing a local polynomial density estimation method

Figure A.5: Discontinuity in the Covariates at the Cutoff

(a) Mothers



(b) Fathers

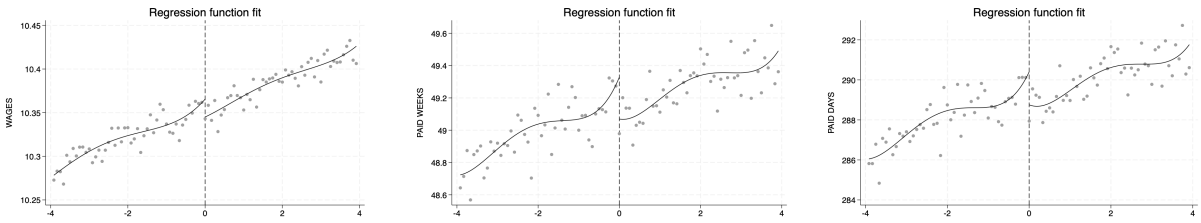


Notes: We report the discontinuity in the covariates added in the econometric model. The solid line is a running-mean smoothing (fourth order) of the variable on the vertical axis, performed separately on either side of the cutoff. The dots are the observed values averaged within bin.

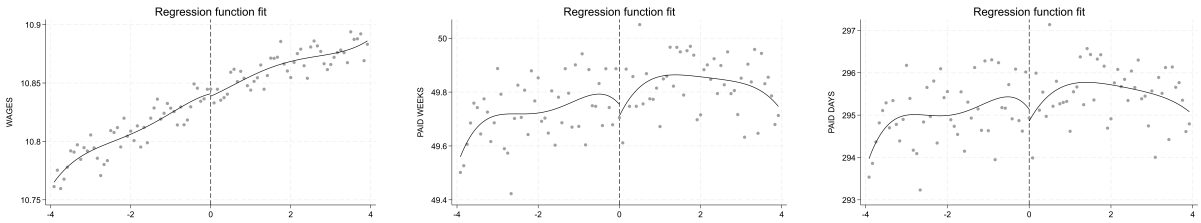
Figure A.6: Discontinuity in the Main Labor Market Outcomes at the Cutoff: High- and Low-WFH-Intensity Firms

(a) High WFH-Intensity Firms

Mothers

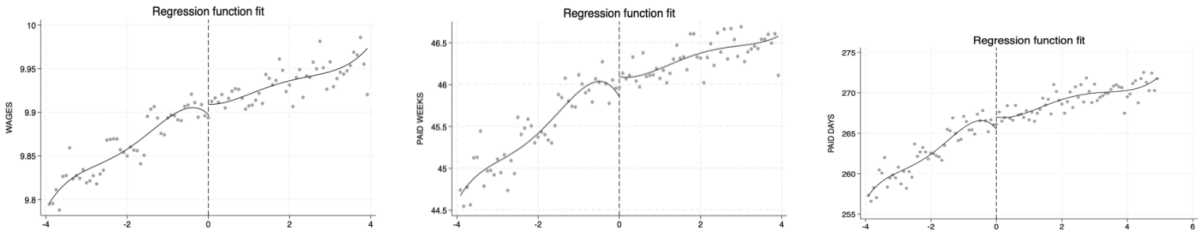


Fathers

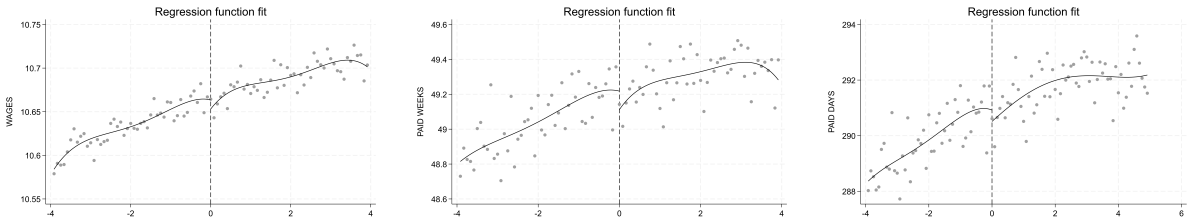


(b) Low WFH-Intensity Firms

Mothers



Fathers



Notes: We report the discontinuity in the labor market outcomes. The solid line is a running-mean smoothing (fourth order) of the variable on the vertical axis, performed separately on either side of the cutoff. The dots are the observed values averaged within bin.

Table A.1: Descriptive Statistics

(a) Full Sample: All Firms

	Mothers					Fathers				
	Obs	Mean	Std. Dev.	Min	Max	Obs	Mean	Std. Dev.	Min	Max
Wages	2,315,439	10.03	0.89	0	14.86	2,315,926	10.70	0.67	0	16.81
Paid weeks	2,319,742	45.77	12.33	0	52	2,319,166	49.21	8.31	0	52
Paid days	2,319,742	266.04	77.96	0	312	2,319,166	290.94	52.15	0	312
Full time	2,319,742	0.60	0.49	0	1	2,319,166	0.97	0.18	0	1
Career	2,319,742	0.03	0.16	0	1	2,319,166	0.05	0.23	0	1
Sick leave	1,934,919	0.02	0.127	0	1	1,935,762	0.01	0.07	0	1
Age of the youngest child	2,319,742	11.22	5.31	1	22.917	2,319,166	11.36	5.36	1	22.917
Parent age	2,319,742	44.81	6.61	18	70	2,319,166	47.78	6.93	18	70
Immigrant	2,319,742	0.05	0.22	0	1	2,319,166	0.02	0.15	0	1
No. employees	2,319,631	13,407.36	32,264.08	1	147,588	2,319,010	10,191.66	26,452.55	1	147,588
Firm age	2,488,631	26.38	16.17	0	100	2,319,010	27.60	16.46	0	102

(b) High WFH-Intensity Firms

	Mothers					Fathers				
	Obs	Mean	Std. Dev.	Min	Max	Obs	Mean	Std. Dev.	Min	Max
Wages	1,158,752	10.28	0.72	0	14.63	1,158,622	10.80	0.62	0	16.81
Paid weeks	1,159,909	47.84	9.68	0	52	1,159,866	49.57	7.60	0	52
Paid days	1,159,909	280.38	61.99	0	312	1,159,866	293.52	47.74	0	312
Full time	1,159,909	0.66	0.47	0	1	1,159,866	0.97	0.16	0	1
Career	1,159,909	0.04	0.19	0	1	1,159,866	0.06	0.24	0	1
Sick leave	976,117	0.02	0.127	0	1	973,832	0.01	0.07	0	1
WFH intensity index	1,159,909	0.84	0.18	0.43	1	1,159,866	0.78	0.20	0.41	1
Age of the youngest child	1,159,909	11.19	5.27	1	22.92	1,159,866	11.31	5.37	1	22.92
Parent age	1,159,909	45.39	6.31	18	70	1,159,866	48.01	6.83	18	70
Immigrant	1,159,909	0.03	0.18	0	1	1,159,866	0.02	0.13	0	1
No. employees	1,159,837	8,212.50	19,219.58	1	87,488	1,159,763	8,411.91	17,910	1	87,488
Firm age	1,159,837	26.86	16.52	0	100	1,159,763	26.93	16.37	0	93

(c) Low WFH-Intensity Firms

	Mothers					Fathers				
	Obs	Mean	Std. Dev.	Min	Max	Obs	Mean	Std. Dev.	Min	Max
Wages	1,156,687	9.78	0.97	0	14.86	1,157,304	10.62	0.70	0	15.64
Paid weeks	1,159,833	43.70	14.20	0	52	1,159,300	48.86	8.95	0	52
Paid days	1,159,833	251.70	88.88	0	312	1,159,304	288.35	56.10	0	312
Full time	1,159,833	0.54	0.50	0	1	1,159,300	0.96	0.20	0	1
Career	1,159,833	0.02	0.13	0	1	1,159,300	0.05	0.21	0	1
Sick leave	958,802	0.02	0.126	0	1	961,930	0.00	0.07	0	1
WFH intensity index	1,159,833	0.12	0.11	0	0.43	1,159,300	0.16	0.11	0	0.41
Age of the youngest child	1,159,833	11.25	5.35	1	22.92	1,159,300	11.42	5.35	1	22.92
Parent age	1,159,833	44.22	6.86	18	70	1,159,300	47.54	7.03	18	70
Immigrant	1,159,833	0.07	0.25	0	1	1,159,300	0.02	0.16	0	1
No. employees	1,159,794	18,602.41	40,725.90	1	147,588	1,159,247	11,972.20	32,749.61	1	147,588
Firm age	1,159,794	25.89	15.80	0	100	1,159,247	28.26	16.52	0	102

Notes: Panel (a) reports the main descriptive statistics for outcomes and observable characteristics of mothers/fathers (excluding manual workers) employed in the non-agricultural private sector during the period 2020–2024, in firms that adopted remote work at least once during 2020–2024. Panels (b) and (c) present corresponding statistics for mothers/fathers, with the sample split by the median value of the WFH intensity index. Data source: INPS administrative archives. The WFH intensity index is based on data from the Italian Ministry of Labor and Social Policies.

Table A.2: Balance Test on Covariates

	(1)	(2)	(3)	(4)	(5)
			(a) Mothers		
	Mother age	Immigrant	South	No. employees	Firm age
Conventional	0.001	0.001	0.000	−30.937	−0.046
	(0.026)	(0.001)	(0.002)	(159.768)	(0.093)
Bias-corrected	0.003	0.001	0.000	−22.060	−0.044
	(0.026)	(0.001)	(0.002)	(159.768)	(0.093)
Robust	0.003	0.001	0.000	−22.060	−0.044
	(0.031)	(0.001)	(0.002)	(190.123)	(0.111)
Sample	Full	Full	Full	Full	Full
Bandwidth	MSE	MSE	MSE	MSE	MSE
Bandwidth (h)	1.742	1.994	3.173	2.651	1.996
Kernel	TRIANG	TRIANG	TRIANG	TRIANG	TRIANG
No. of obs. (Left)	1,254,323	1,254,323	1,254,323	1,254,271	1,254,271
No. of obs. (Right)	1,065,419	1,065,419	1,065,419	1,065,360	1,065,360
Total Observations	2,319,742	2,319,742	2,319,742	2,319,631	2,319,631
			(b) Fathers		
	Father age	Immigrant	South	No. employees	Firm age
Conventional	−0.009	−0.001	−0.001	−82.369	−0.047
	(0.028)	(0.001)	(0.002)	(124.600)	(0.093)
Bias-corrected	−0.011	−0.001	−0.001	−135.900	−0.054
	(0.028)	(0.001)	(0.002)	(124.600)	(0.093)
Robust	−0.011	−0.001	−0.001	−135.900	−0.054
	(0.034)	(0.001)	(0.002)	(144.050)	(0.111)
Sample	Full	Full	Full	Full	Full
Bandwidth	MSE	MSE	MSE	MSE	MSE
Bandwidth (h)	1,827	2,336	3,677	2,860	2,077
Kernel	TRIANG	TRIANG	TRIANG	TRIANG	TRIANG
No. of obs. (Left)	1,229,727	1,229,727	1,229,727	1,229,667	1,229,667
No. of obs. (Right)	1,089,439	1,089,439	1,089,439	1,089,343	1,089,343
Total Observations	2,319,166	2,319,166	2,319,166	2,319,010	2,319,010

Notes: Nonparametric Sharp RDD estimates. We focus on female and male workers in panels (a) and (b), respectively, and exclude manual workers within the MSE-optimal bandwidth for each covariate. Standard errors are robust to heteroskedasticity (shown in brackets).

Table A.3: WFH and Wages During and After Pandemic (High and Low WFH-Intensity Firms). Nonparametric Sharp RDD

	(1) Wages 2020-2021 High WFH	(2) Wages 2020-2021 Low WFH	(3) Wages 2022-2024 High WFH	(4) Wages 2022-2024 Low WFH
Conventional	0.013*	−0.001	0.008	0.012
	(0.007)	(0.013)	(0.006)	(0.010)
Bias-corrected	0.014*	−0.004	0.010*	0.013
	(0.007)	(0.013)	(0.006)	(0.010)
Robust	0.014*	−0.004	0.010	0.013
	(0.009)	(0.015)	(0.007)	(0.012)
Mean outcome	10.242	9.788	10.386	9.814
Bandwidth	MSE	MSE	MSE	MSE
Bandwidth (h)	1.697	1.507	2.215	2.148
Kernel	TRIANG	TRIANG	TRIANG	TRIANG
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes
No. obs. (Left)	400,385	240,728	382,113	228,720
No. obs. (Right)	234,304	141,423	432,471	255,212
Total Obs.	634,689	382,151	814,584	483,932

Notes. Nonparametric Sharp RDD estimates. The outcome variable is reported at the top of each column. The sample consists of female workers and excludes manual workers within the MSE-optimal bandwidth. In each column we add the full set of controls. Robust standard errors (heteroskedasticity-consistent) are reported in brackets.
* $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

Table A.4: Spillover Effects of Female Spouse's Remote Work on Non-WFH Male Spouses

	(1) Wages	(2) Wages	(3) Paid weeks	(4) Paid weeks	(5) Paid days	(6) Paid days
Conventional	-0.006	-0.005	0.056	0.061	0.261	0.276
	(0.011)	(0.011)	(0.175)	(0.176)	(1.065)	(1.065)
Bias-corrected	-0.004	-0.003	0.101	0.104	0.517	0.517
	(0.011)	(0.011)	(0.175)	(0.176)	(1.065)	(1.065)
Robust	-0.004	-0.003	0.101	0.104	0.517	0.517
	(0.013)	(0.013)	(0.207)	(0.208)	(1.258)	(1.261)
Mean outcome	10.208	10.209	46.317	46.325	270.368	270.452
Partner in a different firm	Yes	No	Yes	No	Yes	No
Bandwidth	MSE	MSE	MSE	MSE	MSE	MSE
Bandwidth (h)	3.189	3.071	2.111	2.089	2.259	2.246
Kernel	TRIANG	TRIANG	TRIANG	TRIANG	TRIANG	TRIANG
Workers Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	No	Yes	Yes	No	Yes	Yes
Sector FE	No	No	Yes	No	No	Yes
No. of obs. (Left)	146,671	147,178	146,986	147,493	146,986	147,493
No. of obs. (Right)	134,819	135,243	135,274	135,698	135,274	135,698
Total Observations	281,490	282,421	282,260	283,191	282,260	283,191

Notes. The table reports estimates of the spillover effects of a mother's eligibility for statutory WFH priority on her male partner's labor market outcomes, conditional on the male spouse not working from home. The outcomes shown at the top of each column correspond to the male partner's wages, paid weeks, and paid days. The sample includes married or cohabiting couples in which both partners are employed in the non-agricultural private sector. All specifications include the full set of controls. Standard errors are heteroskedasticity-consistent.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5: Spillover Effects of Female Spouse's Remote Work on Male Blue-Collar Spouses

	(1)	(2)	(3)	(4)	(5)	(6)
	Wages	Wages	Paid weeks	Paid weeks	Paid days	Paid days
Conventional	-0.009	-0.009	-0.143	-0.124	-0.650	-0.549
	(0.010)	(0.010)	(0.163)	(0.162)	(1.033)	(1.018)
Bias-corrected	-0.009	-0.008	-0.112	-0.095	-0.448	-0.375
	(0.010)	(0.010)	(0.163)	(0.162)	(1.033)	(1.018)
Robust	-0.009	-0.008	-0.112	-0.095	-0.448	-0.375
	(0.013)	(0.012)	(0.194)	(0.193)	(1.227)	(1.210)
Mean outcome	9.987	9.998	45.698	45.812	264.576	265.415
Partner in a different firm	Yes	No	Yes	No	Yes	No
Bandwidth	MSE	MSE	MSE	MSE	MSE	MSE
Bandwidth (h)	3.704	3.739	3.566	3.423	3.490	3.418
Kernel	TRIANG	TRIANG	TRIANG	TRIANG	TRIANG	TRIANG
Workers Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	No	Yes	Yes	No	Yes	Yes
Sector FE	No	No	Yes	No	No	Yes
No. of obs. (Left)	109,871	113,408	110,110	113,648	110,110	113,648
No. of obs. (Right)	98,360	101,303	98,693	101,642	98,693	101,642
Total Observations	208,231	214,711	208,803	215,290	208,803	215,290

Notes. The table reports estimates of the spillover effects of a mother's eligibility for statutory WFH priority on her male partner's labor market outcomes, conditional on the male spouse being a manual worker. The outcomes shown at the top of each column correspond to the male partner's wages, paid weeks, and paid days. The sample includes married or cohabiting couples in which both partners are employed in the non-agricultural private sector. All specifications include the full set of controls. Standard errors are heteroskedasticity-consistent.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.6: Spillover Effects of Male Spouse's Remote Work on Non-WFH Female Spouses

	(1)	(2)	(3)	(4)	(5)	(6)
	Wages	Wages	Paid weeks	Paid weeks	Paid days	Paid days
Conventional	0.003	0.003	0.220	0.214	1.463	1.431
	(0.014)	(0.014)	(0.192)	(0.192)	(1.160)	(1.160)
Bias-corrected	0.001	0.001	0.206	0.200	1.371	1.336
	(0.014)	(0.014)	(0.192)	(0.192)	(1.160)	(1.160)
Robust	0.001	0.001	0.206	0.200	1.371	1.336
	(0.017)	(0.017)	(0.230)	(0.230)	(1.385)	(1.385)
Mean outcome	9.616	9.618	44.310	44.310	255.915	255.916
Partner in a different firm	Yes	No	Yes	No	Yes	No
Bandwidth	MSE	MSE	MSE	MSE	MSE	MSE
Bandwidth (h)	2.507	2.496	2.684	2.684	2.909	2.906
Kernel	TRIANG	TRIANG	TRIANG	TRIANG	TRIANG	TRIANG
Workers Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	No	Yes	Yes	No	Yes	Yes
Sector FE	No	No	Yes	No	No	Yes
No. of obs. (Left)	113,369	113,411	113,674	113,716	113,674	113,716
No. of obs. (Right)	115,816	115,854	116,081	116,119	116,081	116,119
Total Observations	229,185	229,265	229,755	229,835	229,755	229,835

Notes. The table reports estimates of the spillover effects of a father's eligibility for statutory WFH priority on his female partner's labor market outcomes, conditional on the female spouse not working from home. The outcomes shown at the top of each column correspond to the female partner's wages, paid weeks, and paid days. The sample includes married or cohabiting couples in which both partners are employed in the non-agricultural private sector. All specifications include the full set of controls. Standard errors are heteroskedasticity-consistent.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.7: Spillover Effects of Male Spouse's Remote Work on Female Blue-Collar Spouses

	(1)	(2)	(3)	(4)	(5)	(6)
	Wages	Wages	Paid weeks	Paid weeks	Paid days	Paid days
Conventional	−0.070*	−0.073**	-0.162	-0.175	-1.514	-1.381
	(0.038)	(0.037)	(0.485)	(0.470)	(3.036)	(2.933)
Bias-corrected	−0.084**	−0.086**	-0.287	-0.288	-2.427	-2.211
	(0.038)	(0.037)	(0.485)	(0.470)	(3.036)	(2.933)
Robust	−0.084*	−0.086**	-0.287	-0.288	-2.427	-2.211
	(0.043)	(0.043)	(0.570)	(0.554)	(3.538)	(3.430)
Mean outcome	9.016	9.061	38.202	38.656	213.503	216.525
Partner in a different firm	Yes	No	Yes	No	Yes	No
Bandwidth	MSE	MSE	MSE	MSE	MSE	MSE
Bandwidth (h)	1.770	1.721	2.154	2.135	2.056	2.058
Kernel	TRIANG	TRIANG	TRIANG	TRIANG	TRIANG	TRIANG
Workers Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	No	Yes	Yes	No	Yes	Yes
Sector FE	No	No	Yes	No	No	Yes
No. of obs. (Left)	30,949	32,080	31,105	32,237	31,105	32,237
No. of obs. (Right)	34,717	35,998	34,856	36,137	34,856	36,137
Total Observations	65,666	68,078	65,961	68,374	65,961	68,374

Notes. The table reports estimates of the spillover effects of a father's eligibility for statutory WFH priority on his female partner's labor market outcomes, conditional on the female spouse being a manual worker. The outcomes shown at the top of each column correspond to the female partner's wages, paid weeks, and paid days. The sample includes married or cohabiting couples in which both partners are employed in the non-agricultural private sector. All specifications include the full set of controls. Standard errors are heteroskedasticity-consistent.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B Conceptual Framework

This appendix presents a simple conceptual framework that illustrates the mechanisms underlying the ITT interpretation developed in the main text. The framework is not intended as a structural model of labor supply, but rather as a parsimonious representation of how statutory WFH eligibility may affect annual labor market outcomes.

To keep the exposition simple, the framework abstracts from the firm, sector, and time dimensions that appear in the empirical specification. Accordingly, individual outcomes are indexed only by the worker, so that, for example, Y_i should be interpreted as the same labor market outcome denoted by Y_{ifst} in Equation (1) of the main text. The implementation probability, however, is allowed to depend on the prevailing technological and institutional environment, indexed by τ.

Consider an economy in which workers are employed but differ in the intensive margin of labor supply. Specifically, let $n_i \in [0, 1]$ denote the fraction of the year during which worker i supplies labor, where higher values correspond to greater intensity (i.e., more paid weeks and paid days). Worker i then chooses the intensive margin of labor supply n_i to maximize:

$$U_i = w_i n_i - \psi_i(n_i) - \kappa_i(d_i, c_i) n_i,$$

where w_i denotes the return to labor input, $\psi_i(n_i)$ is an increasing and convex cost of supplying labor throughout the year, and $\kappa_i(d_i, c_i)$ captures the unit coordination costs associated with supplying labor throughout the year. These costs can be interpreted as the intensive margin analogue of the fixed costs emphasized in the canonical labor-market participation literature (e.g., Cogan, 1981; Altonji and Paxson, 1988), reflecting the time and organizational burdens, such as commuting, arranging childcare, managing unexpected family responsibilities, and coordinating work and family obligations throughout the year (Dickens and Lundberg, 1993; Blundell et al., 2008; Chetty et al., 2011). Because mothers typically bear a larger share of caregiving responsibilities, these costs are expected to be higher for them on average (e.g., Le Barbanchon et al., 2021).

We model the coordination costs associated with working throughout the year as

$$\kappa_i(d_i, c_i) = \phi_i(c_i) - \lambda_i(c_i) d_i, \tag{B.1}$$

where $\phi_i(c_i)$ denotes baseline coordination costs and $\lambda_i(c_i) > 0$ captures the extent to which remote work alleviates those costs. The value of flexibility, $\lambda_i(c_i)$, is expected to be larger for workers facing tighter caregiving constraints (e.g., mothers) or longer commuting distances.[i] As $\frac{\partial \kappa_i}{\partial d_i} = -\lambda_i(c_i) < 0$, remote work then reduces coordination costs.

[i] While we model $\lambda_i(c_i)$ as depending primarily on worker characteristics, organizational factors — such as digital infrastructure and firm-level support for remote work — may also influence how effectively remote work reduces coordination costs once granted.

The first-order condition for this problem is

$$w_i - \psi_i'(n_i^*) - \kappa_i(d_i, c_i) = 0,$$

which implies

$$\psi_i'(n_i^*) = w_i - \kappa_i(d_i, c_i). \tag{B.2}$$

Totally differentiating condition (B.2) yields

$$\frac{\partial n_i^*}{\partial \kappa_i} = -\frac{1}{\psi_i''(n_i^*)} < 0, \tag{B.3}$$

i.e., higher coordination costs reduce the intensive margin of labor supply.

From expression (B.3), it is clear that a reduction in coordination costs increases the optimal intensive margin of labor supply, i.e.:

$$\frac{\partial n_i^*}{\partial d_i} = \frac{\partial n_i^*}{\partial \kappa_i}\frac{\partial \kappa_i}{\partial d_i} = \frac{\lambda_i(c_i)}{\psi_i''(n_i^*)} > 0.$$

In other words, WFH lowers the marginal cost of supplying labor, increasing the number of paid days and weeks worked over the year. In the empirical analysis, this mechanism corresponds directly to increases in paid weeks and paid days.

Access to WFH is not a direct choice variable but depends on statutory eligibility and firm implementation:

$$d_i = \pi(c_i, \tau)T_i, \tag{B.4}$$

where $T_i = \mathbb{1}[\text{youngest child} < 12]$ denotes eligibility for statutory priority (the running variable in the RDD setup; see Equation (1)) and $\pi(c_i, \tau) \in [0, 1]$ is the probability that eligibility translates into actual remote-work access. Implementation probabilities are expected to be low in technological and organizational environments where remote work is not feasible, such as occupations requiring physical presence. They became substantially higher following the widespread diffusion of WFH after 2020, particularly in firms where WFH could be readily implemented.

Now, substituting (B.4) into the cost function (B.1) yields

$$\kappa_i = \phi_i(c_i) - \lambda_i(c_i)\pi(c_i, \tau)T_i.$$

The effect of implementation capacity is therefore

$$\frac{\partial \kappa_i}{\partial \pi(c_i, \tau)} = -\lambda_i(c_i)T_i < 0,$$

that is, higher implementation probability lowers coordination costs and increases labor-market attachment. Consequently, holding the value of flexibility $\lambda_i(c_i)$ constant, statu-

tory eligibility should have larger effects in firms and occupations characterized by higher implementation probabilities — that is, where remote work is technologically feasible, does not require physical presence, and is organizationally embedded. Conversely, when implementation capacity is limited — for instance, for tasks that inherently require physical presence, or in firms where $\pi(c_i, \tau) \approx 0$ — eligibility produces little change in actual working arrangements, and the labor-market effects of the policy should be negligible.

Annual earnings are given by

$$Y_i = w_i n_i,$$

where w_i can be interpreted as earnings per unit of effective labor supplied. Therefore, even if earnings per day (or per unit of labor supplied) remain unchanged, a higher intensive margin increases annual earnings mechanically through a larger number of paid days and paid weeks. The model thus predicts positive effects on annual wages together with improvements in the intensive margin of labor supply, while leaving broader features of job design — such as promotions, contractual hours, or career advancement — largely unaffected.

Importantly, d_i is not observed in the data. Our empirical strategy therefore does not recover the effect of actual remote-work take-up, but rather the intention-to-treat effect of eligibility for WFH priority. Within this framework, the intention-to-treat effect identified by the RDD estimator in Section 3 can be written as

$$\begin{aligned} \text{ITT} &= \mathbb{E}[Y_i \mid T_i = 1] - \mathbb{E}[Y_i \mid T_i = 0] \\ &= \pi(c_i, \tau) \times \underbrace{\mathbb{E}[Y_i(1) - Y_i(0)]}_{\text{causal effect of WFH access}}, \end{aligned} \tag{B.5}$$

so that the estimated discontinuity can be interpreted as the product of the probability that statutory eligibility translates into remote-work access and the underlying causal effect of WFH access on labor market outcomes. In our empirical specification (given by Equation (1)), the ITT estimand in (B.5) corresponds to the RDD coefficient β_1. The estimated discontinuity can therefore be written as

$$\beta_1 = \pi(c_i, \tau)\mathbb{E}[Y_i(1) - Y_i(0)],$$

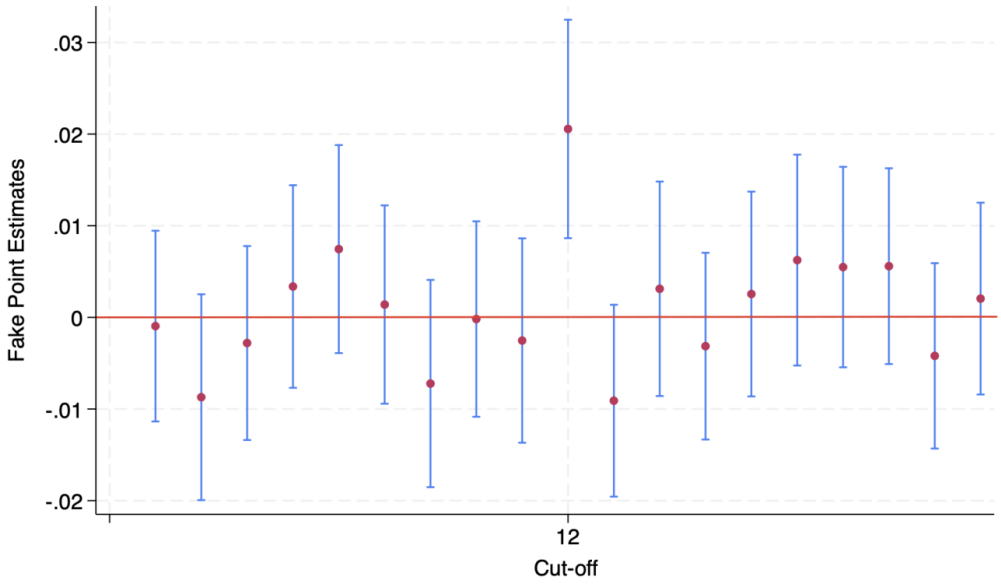
which coincides with expression (2) in the main text. Here, we allow the implementation probability to explicitly depend on workers' caregiving constraints and the organizational environment through $\pi(c_i, \tau)$. For expositional simplicity, the main text suppresses this heterogeneity and denotes the implementation probability by π_t, without affecting the interpretation of the ITT decomposition.

This framework also provides a natural interpretation of the heterogeneity analyses presented in Section 6. Differences in estimated ITT effects across firms, occupations, and

local environments may arise because implementation probabilities differ across organizational settings, because the gains associated with WFH access differ once remote work is adopted, or because both mechanisms operate simultaneously. The empirical analysis documents this heterogeneity but does not seek to separately identify these two channels.

C Robustness and Placebo Analyses

Figure C.1: Placebo RD Estimates Using Fake Eligibility Thresholds (High-WFH Firms)



Notes. The figure plots point estimates and 95 percent confidence intervals from separate local-linear RDDs using log annual wages as the outcome. Each dot corresponds to a "fake" age cutoff obtained by shifting the statutory threshold symmetrically in 0.2-year increments (nine below and nine above age 12). The first placebo cutoff is set at 10.8 and the last at 13.2 to ensure sufficient distance from the actual threshold. All specifications use an MSE-optimal bandwidth and triangular kernel. Only the estimate evaluated at the true statutory cutoff at age 12 is statistically significant.

Table C.1: WFH and Mothers' LM Outcomes in Non-WFH Firms. Nonparametric Sharp RDD

	(1) Wages	(2) Wages	(3) Wages	(4) Wages
Conventional	0.011	0.009	0.008	0.007
	(0.008)	(0.006)	(0.006)	(0.006)
Bias-corrected	0.011	0.009	0.008	0.007
	(0.008)	(0.006)	(0.006)	(0.006)
Robust	0.011	0.009	0.008	0.007
	(0.009)	(0.007)	(0.007)	(0.006)
Mean outcome	9.162	9.163	9.164	9.164
Bandwidth	MSE	MSE	MSE	MSE
Bandwidth (h)	2.197	2.020	1.823	1.780
Kernel	TRIANG	TRIANG	TRIANG	TRIANG
Controls	No	Yes	Yes	Yes
Year FE	No	No	Yes	Yes
Sector FE	No	No	No	Yes
No. obs. (Left)	2,459,581	2,458,419	2,458,419	2,458,419
No. obs. (Right)	2,414,561	2,413,464	2,413,464	2,413,464
Total Obs.	4,874,142	4,871,883	4,871,883	4,871,883

Notes. Nonparametric Sharp RDD estimates. The outcome variable is reported at the top of each column. The sample consists of female workers in Non-WFH firms within the MSE-optimal bandwidth. Robust standard errors (heteroskedasticity-consistent) are reported in brackets. Statistical significance is denoted by * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

Table C.2: WFH and LM Mothers' Outcomes on Blue-Collar mothers. High WFH-intensity firms. Nonparametric Sharp RDD

	(1) Wages	(2) Paid weeks	(3) Paid days
Conventional	−0.002	0.490	3.294
	(0.021)	(0.410)	(2.653)
Bias-corrected	0.003	0.639	4.293
	(0.021)	(0.410)	(2.653)
Robust	0.003	0.639	4.293
	(0.025)	(0.470)	(3.032)
Mean outcome	9.875	46.976	270.025
Bandwidth	MSE	MSE	MSE
Bandwidth (h)	2.062	1.516	1.469
Kernel	TRIANG	TRIANG	TRIANG
Controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes
No. obs. (Left)	24,366	24,405	24,405
No. obs. (Right)	30,133	30,196	30,196
Total Obs.	54,499	54,601	54,601

Notes. Nonparametric Sharp RDD estimates. The outcome variable is reported at the top of each column. The sample consists of female manual workers in high WFH-intensity firms within the MSE-optimal bandwidth. In each column we add the full set of controls. Robust standard errors (heteroskedasticity-consistent) are reported in brackets. Statistical significance is denoted by * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

Table C.3: WFH and Mothers' Wages. Before Pandemic (High WFH-Intensity Firms). Nonparametric Sharp RDD

	(1) Wages 2018	(2) Wages 2018	(3) Wages 2018	(4) Wages 2019	(5) Wages 2019	(6) Wages 2019
Conventional	0.006	0.005	0.006	−0.006	−0.005	−0.004
	(0.011)	(0.010)	(0.010)	(0.008)	(0.008)	(0.008)
Bias-corrected	0.006	0.006	0.007	−0.009	−0.008	−0.006
	(0.011)	(0.010)	(0.010)	(0.008)	(0.008)	(0.008)
Robust	0.006	0.006	0.007	−0.009	−0.008	−0.006
	(0.013)	(0.013)	(0.012)	(0.010)	(0.010)	(0.009)
Mean outcome	10.067	10.067	10.067	10.088	10.088	10.088
Bandwidth	MSE	MSE	MSE	MSE	MSE	MSE
Bandwidth (h)	1.716	1.734	1.726	2.237	2.193	2.220
Kernel	TRIANG	TRIANG	TRIANG	TRIANG	TRIANG	TRIANG
Workers Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	No	Yes	Yes	No	Yes	Yes
Sector FE	No	No	Yes	No	No	Yes
No. obs. (Left)	362,050	362,050	362,050	391,928	391,928	391,928
No. obs. (Right)	113,946	113,946	113,946	150,445	150,445	150,445
Total Obs.	475,996	475,996	475,996	542,373	542,373	542,373

Notes. Nonparametric Sharp RDD estimates. The outcome variable is reported at the top of each column. The focus is on WFH firms in 2020 observed before the pandemic (2018 and 2019). The sample consists of female workers and excludes manual workers within the MSE-optimal bandwidth. In each column we add the full set of controls. Robust standard errors (heteroskedasticity-consistent) are reported in brackets. Statistical significance is denoted by * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

Table C.4: WFH and Mothers' Wages. High WFH-Intensity Firms. Nonparametric Donut Sharp RDD

	(1) Wages	(2) Wages	(3) Wages	(4) Wages
Conventional	0.027***	0.017**	0.033***	0.010*
	(0.007)	(0.007)	(0.009)	(0.006)
Bias-corrected	0.029***	0.019***	0.037***	0.011*
	(0.007)	(0.007)	(0.009)	(0.006)
Robust	0.029***	0.019**	0.037***	0.011
	(0.008)	(0.008)	(0.010)	(0.007)
Mean outcome	10.352	10.353	10.352	10.354
Donut	0–0.1	0–0.2	0–0.3	0–0.4
Bandwidth	MSE	MSE	MSE	MSE
Bandwidth (h)	1.247	1.485	1.354	2.348
Kernel	TRIANG	TRIANG	TRIANG	TRIANG
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes
No. obs. (Left)	630,472	630,472	630,472	630,472
No. obs. (Right)	515,754	509,074	502,554	496,422
Total Obs.	1,146,226	1,139,546	1,130,026	1,126,894

Notes. Nonparametric Donut Sharp RDD estimates. The outcome variable is reported at the top of each column. The sample consists of female workers in high WFH-intensity firms within the MSE-optimal bandwidth. In each column we add the full set of controls. Robust standard errors (heteroskedasticity-consistent) are reported in brackets.

* $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

Table C.5: WFH and Mothers' Wages. High WFH-Intensity Firms. Nonparametric Sharp RDD with Different Bandwidths

	(1) Wages	(2) Wages	(3) Wages	(4) Wages	(5) Wages
Conventional	0.021***	0.017***	0.018**	0.020**	0.024***
	(0.006)	(0.005)	(0.009)	(0.008)	(0.008)
Bias-corrected	0.024***	0.019***	0.019**	0.020**	0.025***
	(0.006)	(0.005)	(0.009)	(0.008)	(0.008)
Robust	0.024***	0.019***	0.019**	0.020**	0.025***
	(0.007)	(0.006)	(0.009)	(0.009)	(0.008)
Mean outcome	10.363	10.353	10.351	10.357	10.351
Bandwidth	MSE-TWO	MSE-SUM	CER	CER-TWO	CER-SUM
Bandwidth (h)	1.254–2.442	1.596	0.669	0.624–1.215	0.794
Kernel	TRIANG	TRIANG	TRIANG	TRIANG	TRIANG
Controls	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes	Yes
No. obs. (Left)	630,472	630,472	630,472	630,472	630,472
No. obs. (Right)	528,221	528,221	528,221	528,221	528,221
Total Obs.	1,158,693	1,158,693	1,158,693	1,158,693	1,158,693

Notes. Nonparametric Sharp RDD estimates. The outcome variable is reported at the top of each column. The sample consists of female workers in high WFH-intensity firms within different optimal bandwidths. In each column we add the full set of controls. Robust standard errors (heteroskedasticity-consistent) are reported in brackets.
* $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

Table C.6: WFH and Mothers' Wages, Paid Weeks and Days. High WFH-Intensity Firms. Diff-in-Discontinuity Estimates

	(1) Wages	(2) Wages	(3) Paid weeks	(4) Paid weeks	(5) Paid days	(6) Paid days
Treatment	−0.010	−0.010	−0.050	−0.050	−0.327	−0.319
	(0.007)	(0.006)	(0.097)	(0.096)	(0.611)	(0.610)
After	0.030***	0.010*	−0.454***	−0.558***	−4.256***	−4.932***
	(0.005)	(0.005)	(0.075)	(0.075)	(0.477)	(0.476)
Treatment*After	0.017**	0.017**	0.189*	0.194*	1.199*	1.221*
	(0.008)	(0.008)	(0.114)	(0.114)	(0.721)	(0.719)
Constant	10.185***	8.927***	48.500***	43.242***	289.994***	258.484***
	(0.032)	(0.034)	(0.560)	(0.589)	(3.434)	(3.624)
Bandwidth	MSE	MSE	MSE	MSE	MSE	MSE
Controls	No	Yes	No	Yes	No	Yes
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.186	0.211	0.031	0.036	0.044	0.049
Total Obs.	490,980	490,958	441,132	441,102	441,132	441,102

Notes. Diff-in-discontinuity estimates. The outcome variable is reported at the top of each column. The sample consists of female workers in high WFH-intensity firms within the MSE-optimal bandwidth. In even-numbered columns we add the full set of controls. Robust standard errors (heteroskedasticity-consistent) are reported in brackets.
* $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

Table C.7: WFH and Mothers' Wages, Paid Weeks and Days. Low WFH-Intensity Firms. Diff-in-Discontinuity Estimates

	(1) Wages	(2) Wages	(3) Paid weeks	(4) Paid weeks	(5) Paid days	(6) Paid days
Treatment	0.004	0.003	0.144	0.148	0.915	0.947
	(0.008)	(0.008)	(0.120)	(0.119)	(0.761)	(0.754)
After	0.014**	−0.002	−0.738***	−0.898***	−6.360***	−7.441***
	(0.006)	(0.006)	(0.094)	(0.093)	(0.595)	(0.590)
Treatment*After	0.001	0.002	−0.115	−0.111	−0.695	−0.663
	(0.009)	(0.009)	(0.142)	(0.141)	(0.901)	(0.893)
Constant	10.243***	9.253***	50.096***	40.728***	299.098***	243.221***
	(0.022)	(0.024)	(0.370)	(0.412)	(2.307)	(2.564)
Bandwidth	MSE	MSE	MSE	MSE	MSE	MSE
Controls	No	Yes	No	Yes	No	Yes
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.235	0.255	0.133	0.145	0.137	0.152
Total Obs.	528,075	528,065	452,419	452,403	452,419	452,403

Notes. Diff-in-discontinuity estimates. The outcome variable is reported at the top of each column. The sample consists of female workers in low WFH-intensity firms within the MSE-optimal bandwidth. In even-numbered columns we add the full set of controls. Robust standard errors (heteroskedasticity-consistent) are reported in brackets.
* $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

Table C.8: WFH, Wages, Paid Weeks, and Paid Days by Municipality's Broadband Speed: Mothers in WFH Firms

	(1) Wages >median	(2) Wages <median	(3) Paid weeks >median	(4) Paid weeks <median	(5) Paid days >median	(6) Paid days <median
Conventional	0.011	0.005	0.160*	0.019	1.265**	−0.031
	(0.007)	(0.007)	(0.097)	(0.102)	(0.604)	(0.652)
Bias-corrected	0.012*	0.004	0.143	−0.009	1.186**	−0.214
	(0.007)	(0.007)	(0.097)	(0.102)	(0.604)	(0.652)
Robust	0.012	0.004	0.143	−0.009	1.186	−0.214
	(0.008)	(0.008)	(0.115)	(0.119)	(0.724)	(0.763)
Mean outcome	10.129	10.137	47.538	47.682	277.628	278.342
Bandwidth	MSE	MSE	MSE	MSE	MSE	MSE
Bandwidth (h)	1.618	1.646	1.426	1.262	1.459	1.232
Kernel	TRIANG	TRIANG	TRIANG	TRIANG	TRIANG	TRIANG
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes
No. obs. (Left)	633,293	618,653	634,511	619,760	634,511	619,760
No. obs. (Right)	535,098	528,312	536,168	529,192	536,168	529,192
Total Obs.	1,168,391	1,146,965	1,170,679	1,148,952	1,170,679	1,148,952

Notes: Nonparametric Sharp RDD estimates. The outcome variable is reported at the top of each column. Odd-numbered specifications focus on firms located in municipalities with high ADSL speed (above the median), whereas even-numbered specifications focus on firms located in municipalities with low ADSL speed (below the median). The sample consists of female workers and excludes manual workers in WFH firms within the MSE-optimal bandwidth. All columns include the full set of controls. Robust standard errors (heteroskedasticity-consistent) are reported in brackets.
* $p < 0.10$, ** $p < 0.05$