Juggling Work and Family: Labor Market Effects of Working from Home on Parents with School-aged Children

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Abstract

We estimate the labor-market effects of Italy's statutory priority to work from home (WFH) granted to parents with children below age 12. Using population-wide administrative employer-employee data and a sharp age-based regression discontinuity design, we show that once remote work became operational during the pandemic, eligibility raised wages of mothers in high-WFH firms by 1.6–2.3 percent and increased paid weeks and days. No effects appear before the pandemic, in firms that never adopted WFH, in low-WFH environments, or for fathers. Impacts are concentrated in non-manual occupations and in technologically and managerially ready environments—smaller and younger firms, stronger broadband infrastructure, and Northern Italy. The results show that flexibility rights improve attachment only where firms and local conditions can implement them.

Keywords: Remote work; work-family balance; labor supply; statutory rights; regression discontinuity.

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

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1 Introduction

The rapid expansion of working from home (WFH) over the past decade—accelerated by the COVID-19 pandemic—has reshaped labor markets, organizational practices, and the allocation of work and family responsibilities. These changes are especially salient for mothers, who continue to bear a disproportionate share of childcare and household work and face large and persistent child penalties in pay and careers (Waldfogel, 1998; Kleven et al., 2019). By reducing commuting and schedule constraints, WFH has the potential to facilitate the reconciliation of professional and domestic duties and to sustain mothers' labor-market attachment around childbirth (Allen et al., 2015; Chung and van der Lippe, 2020).

At the same time, the effects of remote work are ambiguous. While flexibility may ease time constraints, it can also reinforce gender specialization in unpaid work or limit career progression if workplace cultures continue to reward physical presence. Moreover, the effectiveness of WFH depends on institutional design and on firms' technological and managerial capacity to implement it—factors that have historically constrained the success of flexible-work initiatives (Bresnahan et al., 2002; Bloom and Van Reenen, 2007). As a result, preferential access to remote work, increasingly adopted by policymakers, may generate heterogeneous and unintended consequences.

Motivated by these ambiguities, this paper studies whether statutory remote-work rights improve parents' labor-market outcomes at a stage of the life cycle when childcare demands remain high. We analyze Italy's Law 81/2017, which grants parents of children under age twelve priority access to remote-work arrangements. This is a low-cost legal entitlement, not a program with transfers or mandates. Exploiting the age threshold embedded in the policy, we compare mothers and fathers whose youngest child is just below the cutoff with those whose child has just crossed it. This institutional discontinuity supports a regression discontinuity design (RDD) that isolates the causal effect of gaining priority access to WFH and allows us to study how legal flexibility rights translates into labor-market outcomes depending on firms' technological and managerial capacity.

Our analysis relies on a novel linkage between population-wide employer-employee social-security records from the National Institute of Social Security (INPS) and the universe of formally registered remote-work contracts collected by the Italian Ministry of Labor and Social Policies. The Ministry data provide legally certified information on all remote-work arrangements, including firm identifiers, adoption periods, and selected worker characteristics such as gender and nationality, although individual identifiers are unavailable. We use these records to construct firm-level measures of remote-work intensity—the share of female (male) employees with an authorized WFH contract among all female (male) employees in the firm. Merging these measures with INPS data allows us to trace the timing and incidence of remote work and to follow parents' employment histories, wages, paid weeks, career outcomes, and sickness absences. This integration enables us to assess whether statutory priority mitigates the well-documented labor-market penalties associated with motherhood.

To address selection, we focus on parents employed in firms that adopt at least one remote-work arrangement in a given year. We find that eligibility increases mothers' wages and improves the continuity of paid work. In the full sample, statutory access raises annual wages by about 1 percent and increases paid work activity by roughly 0.3 additional weeks and 1.7 days per year. Although modest in magnitude, these effects are consistent with flexibility reducing fixed participation costs—such as commuting and coordination burdens—that disproportionately affect mothers. We find no corresponding effects for fathers.

The effects are strongly heterogeneous across firms. In high-intensity adopters of remote work, the wage premium doubles to 1.6–2.3 percent and gains in paid-work continuity become substantially larger, while estimates remain close to zero in low-intensity firms. This pattern underscores that statutory rights translate into effective flexibility only where firms possess complementary technological and managerial capacity. Across all specifications, we find no effects on full-time status, promotions, or sickness absences, indicating that statutory WFH primarily alleviates day-to-day coordination constraints rather than reshaping deeper contractual arrangements or career dynamics. Again, no effects emerge for fathers, even in firms with high male WFH adoption.

We conduct an extensive set of robustness checks. These include alternative RDD specifications, placebo tests in pre-pandemic years, a difference-in-discontinuity design comparing periods before and after the expansion of remote work, multiple bandwidth selectors, and donut designs around the threshold. The results show that the discontinuity becomes salient only once WFH is broadly feasible and is not driven by pre-existing differences. We also examine placebo environments—firms that never adopt remote work and occupations that are inherently non-teleworkable—where statutory priority should not operate. In both cases, no discontinuities appear, reinforcing the proposed mechanism and supporting a causal interpretation.

A central contribution of the paper is to show that statutory flexibility can increase mothers' earnings even without changes in formal hours or promotion prospects, but only when complementary organizational conditions are in place. Eligibility produces meaningful improvements in digitally prepared firms, in the Center-North, and in small and young organizations with more agile structures, while firms in the South, large and mature organizations, and municipalities with poor broadband connection exhibit no detectable response. Legal entitlements alone are therefore insufficient: the labor-market impact of flexibility depends critically on organizational and technological capacity.

The paper contributes to three strands of the literature. First, it speaks to research on flexible work and work-family reconciliation, which shows that flexibility can reduce work-family imbalance but is often constrained by managerial resistance and face-time norms (Allen et al., 2015; Chung and van der Lippe, 2020; Bloom et al., 2015; Lott and Klenner, 2018; Mas and Pallais, 2020). By studying a statutory entitlement rather than voluntary programs, we show how legal access interacts with heterogeneous firm capabilities.

Second, it contributes to the literature on gender gaps and the motherhood penalty (Becker, 1985; Waldfogel, 1998; Kleven et al., 2019), and on how fixed time costs and schedule constraints shape careers (Gicheva, 2013; Goldin, 2014; Cortés and Pan, 2023). A complementary strand argues that flexibility can mitigate some penalties by enabling continued labor force participation and smoothing hours adjustments (Bal and De Lange, 2015; Golden, 2001), though effects on promotion and long-term career progression remain ambiguous (Goldin and Katz, 2011; Kossek et al., 2023). Our findings indicate that statutory WFH raises mothers' earnings without affecting promotions or part-time work, and yields no effects for fathers, highlighting the gendered nature of flexibility benefits.

Third, the paper relates to organizational economics on how technology and management practices affect the returns to new work arrangements (Bresnahan et al., 2002; Bloom and Van Reenen, 2007; Barrero et al., 2021, 2023). We extend this framework, which typically focuses on information technologies, management quality, or HR practices, to flexibility policies, showing that even well-intentioned legislation generates uneven outcomes depending on firm readiness and organizational openness to WFH.

Overall, we show that the consequences of flexibility policies are deeply context-dependent: statutory rights do not necessarily homogenize worker experiences but can instead amplify differences across organizational environments. Aligning legal entitlements with firms' technological and managerial capacity is therefore essential if remote-work policies are to mitigate gender inequality rather than reproduce it. Although we exploit an Italian policy whose relevance became salient during the pandemic, our contribution is broader, as we uncover general mechanisms through which formal flexibility rights translate into labor-market outcomes in advanced economies where remote work has become a pervasive organizational technology.

The remainder of the paper is organized as follows. Section 2 describes the institutional background. Section 3 presents the data. Section 4 outlines the empirical strategy. Section 5 reports the main results. Section 6 analyzes additional outcomes. Section 7 interprets the findings. Section 8 presents robustness checks, Section 9 the heterogeneity analyses, and Section 10 concludes.

2 Institutional settings

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 experimentation with remote work predates COVID-19, the current system—legally defined as *lavoro agile*—treats remote work as a structured employment arrangement governed by statutory rights that interact with managerial discretion.

The cornerstone of this framework is Law 81/2017, which introduced *lavoro agile* as a contractual modality explicitly aimed at promoting work–life balance. Unlike traditional telework, the law emphasizes flexibility in both location and working time, allowing employees to alternate between on-site and off-site work without changing their contractual classification. Remote-work arrangements must be formalized through individual written agreements specifying workloads, rest periods, monitoring supervision procedures, and the right to disconnect, while preserving wage parity between remote and in-person workers. These provisions reflect the protective orientation of Italian labor institutions and ensure that remote work does not alter contractual rights or earnings.

The regulatory framework expanded substantially during the COVID-19 pandemic. Emergency decrees in 2020 temporarily suspended the requirement for individualized agreements and allowed employers to activate remote work unilaterally. This shift generated unprecedented adoption, familiarized firms and workers with digital coordination tools, and reduced cultural resistance to remote arrangements. Although the emergency regime was later withdrawn, subsequent decrees reinstated individual agreements while preserving greater operational flexibility, leading many companies—especially larger and more digitally advanced firm—to institutionalize WFH practices.

A central feature for this study is the introduction of statutory priority rights. Italian law requires that parents of children under twelve (and caregivers of individuals with severe disabilities) receive priority access to remote-work arrangements whenever the job is deemed compatible with remote execution. Among otherwise comparable workers, eligible individuals must be granted access first. While the rule is legally binding for employers, its practical effectiveness depends on firm-level conditions—digital infrastructure, task modularity, and managerial practices—which determine whether remote work is operationally feasible.

Collective bargaining complements statutory rights. Sectoral agreements establish general principles (such as equipment provision, occupational safety, and work–life boundaries), while firm-level contracts regulate scheduling, the allocation of remote days, and performance monitoring. These multi-tiered rules interact with statutory priority, generating substantial heterogeneity across firms in the actual availability and intensity of remote work, even when legal entitlement is uniform.

Finally, technological and territorial disparities shape implementation. Italian firms differ widely in ICT adoption, broadband quality, and managerial sophistication, with persistent gaps between the Center–North and the South. Such variation determines whether statutory rights translate into effective flexibility, reflecting a well-documented complementarity between institutional design and organizational capability in work–family policies.

Overall, Italy provides a setting with a binding statutory rule, strong worker protections, heterogeneous digital preparedness, and large cross-firm variation in WFH feasibility. This structure allows us to study how statutory rights interact with organizational capacity—an issue central to the economics of flexible work.

3 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 the working dataset 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 submit an application to INPS. The program's exceptionally high take-up rate (about 94 percent among children born after 2003) allows us to reliably identify parents of school-aged children. 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.[1] 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 monthly 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 and termination reasons (e.g., layoffs, resignations), earnings, paid days and weeks, contract type (full-time versus part-time), and occupational category (blue-collar, white-collar, managerial).[2] We construct a full-time indicator equal to one for full-time contracts and

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

[2] For individuals with multiple employment records within a given year, we retain the observation asso-

zero otherwise. To capture within-firm upward mobility, we build a career-progression variable, *Career*, equal to one if a worker experiences a transition into a higher occupational rank (from blue- to white-collar, or from blue/white-collar to managerial). We further 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 in one year.

Our analytical sample includes mothers and fathers aged 18 to 70, 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 months at each observation by combining birth year and birth month information (and adjusting for the child's birth month). This ensures precise classification around the 12-year 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 accidents occurring off-site. The dataset provides annual firm-level information for 2018–2024, and from 2020 onward records WFH spells consistently at the worker–week level through an online portal. For each WFH spell, we observe the firm identifier, worker characteristics (gender, year of birth, citizenship), and the start and end week of the arrangement. We aggregate these spells to the annual level and 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.

Figure 1 and Appendix Figure A1 provide a descriptive overview of remote-work adoption. Figure 1, panels (a) and (b), reports 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.[3] Figure A1 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

ciated 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.

[3]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 show limited use of remote work.

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, which mirrors trends in the Italian Labor Force Survey conducted by ISTAT, indicates that remote work became a stable organizational feature rather than a transitory adjustment. 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.[4]

Our final sample consists of parents employed in private, non-agricultural firms that adopted remote work at least once during 2020–2024. 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.[5] We exclude manual workers because their tasks are typically tied to on-site production and cannot plausibly be performed remotely; including them would dilute treatment variation. 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.[6]

Table 1 reports descriptive statistics. Panel (a) presents the full sample of employed mothers and fathers separately, while panels (b) and (c) show the same statistics for workers in high- and low-WFH-intensity firms, defined using the median of the firm-level WFH intensity distribution. The splits in panels (b) and (c) reveal systematic differences in wages, work continuity, citizenship, and firm characteristics (including size and

[4]Panels (a) and (b) of Appendix Figure A2 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, where WFH intensity is measured as the share of female/male employees working remotely within the firm. A clear monotonic pattern emerges: 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 between the bottom and top quartiles. While purely descriptive and not causal, this pattern highlights the strong correlation between organizational readiness for remote work and parents' economic outcomes.

[5]For placebo analyses, we construct a comparison group consisting of mothers/fathers 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 laborers employed in firms that do use remote work.

[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, resulting in harmonized and complete information on remote-work arrangements nationwide. Second, the pandemic acted as a catalyst for the diffusion of remote-work technologies and managerial practices, generating meaningful variation across firms in their capacity 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.

age) across organizational environments with different remote-work capacity, particularly among mothers. By contrast, the corresponding differences for fathers are substantially smaller.

4 Econometric model and validity checks

4.1 Empirical strategy

This section describes the research design used to estimate the causal effect of the statutory priority rule for WFH in Italy on mothers' and fathers' 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 arguably 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 is equal to one for mothers 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:

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

Here, $g(R_{it})$ 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, i.e., firm age and the number of employees as a measure of firm size. To increase the precision of our estimates, we further include year fixed effects, δ_t, which absorb aggregate macroeconomic and policy shocks common, and one-digit sector fixed effects, θ_s, which capture systematic differences across industries in wage-setting practices, remote-work feasibility, and labor demand conditions. Finally, ε_{ifst} denotes the error term.

Consistent with best practices in the RDD literature, estimation employs the MSE-optimal bandwidth and the robust bias-correction procedure of 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 marginally older, under the identifying assumption that, absent the priority rule, potential outcomes evolve smoothly with the child's age.

The parameter of interest, β_1, recovers a local average treatment effect (LATE) at the eligibility threshold. Importantly, because individual-level WFH take-up is not observed and because WFH adoption depends on firms' organizational practices, the estimated discontinuity should be interpreted as an intention-to-treat (ITT) effect of gaining statutory priority, rather than the effect of actually working remotely. The institutional structure implies that the entitlement is binding only in firms where WFH is technologically and feasible and routinely implemented. Parents employed in such firms constitute the relevant compliers, as eligibility around the cutoff can plausibly change their probability of receiving a remote-work assignment. Conversely, parents in firms that do not offer remote work—i.e., never-takers—cannot be affected by the entitlement, and therefore do not contribute to the identifying variation.

Accordingly, β_1 identifies the ITT effect of statutory eligibility among parents employed in firms where remote work can be implemented, rather than the average effect for all eligible parents in the population. The empirical evidence fully supports this interpretation: substantial effects arise only for mothers in high-WFH-intensity firms, where the priority rule can meaningfully alter access to remote work, while no effects are detected in low or non-adopting firms. For fathers, we find no significant effects, likely reflecting gender asymmetries in caregiving responsibilities and in labor-market responses to children's developmental stages.

4.2 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 Figure 2 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 aligns with the institutional context: children's ages are drawn from population registers and transmitted directly to INPS, rather than self-reported by workers or employers. These records are routinely cross-validated across administrative databases, and false declarations carry criminal penalties, effectively eliminating 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). Table 2 reports the corresponding local linear regression 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, Figure 3 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 visual evidence confirms 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 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, child allowances, or parental leave does not shift discretely at this age. After-school programs and public childcare provision are determined at the municipal and school level and are not tied to the twelfth birthday of the child. Consequently, the only nationwide rule that shifts sharply at this cutoff is parents' legal priority access to WFH. This institutional insulation of the threshold strengthens the interpretation that any discontinuity in labor-market outcomes at age 12 reflects the activation of the WFH entitlement rather than coincident changes in schooling regimes, fiscal incentives, or childcare arrangements.

5 Main results

We present results from a sharp RDD that exploits the age-based eligibility rule of *lavoro agile* granting priority access to WFH. Because the design assigns eligibility rather than actual remote-work take-up, all estimates should be interpreted as intention-to-treat (ITT) effects of the priority rule. The discontinuities therefore capture the average impact of becoming eligible for WFH priority, combining both implementation and compliance responses by firms and workers. This interpretation is especially important for the heterogeneity analysis, which can be read as differences in the extent to which the statutory rule is effectively implemented across organizational environments.

The analysis proceeds in three steps. We first estimate the baseline effect of WFH priority on wages for eligible mothers and fathers in firms that actively use WFH practices. We then examine heterogeneity across firms with different pre-existing intensities of remote-work adoption. Finally, we assess whether wage effects are accompanied by changes in paid work activity, shedding light on the underlying adjustment mechanisms.

Baseline wage effects. Table 3 reports baseline RDD estimates with log annual wages as the outcome. The sample includes female (male) employees who are mothers (fathers), excluding manual workers, and focuses on 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. Although modest in magnitude, the effect is economically meaningful given the relatively compressed wage distribution among female employees in Italy. The stability of the estimates across specifications indicates that the results reflect a causal impact of eligibility for WFH priority, rather than compositional shifts around the threshold.[7] As mentioned, these estimates capture ITT effects: they reflect the average wage response to gaining priority access to WFH, not the effect of actual remote-work take-up. The estimates therefore embed both the probability that the rule is implemented and the intensity with which eligible mothers are able to use flexible arrangements.

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

Heterogeneity by firm-level WFH intensity. Table 4 examines whether the ITT effect of WFH priority varies with firms' organizational capacity to accommodate remote work. We split the sample at the median of firm-level WFH intensity—defined as the average share of female/male employees working remotely—distinguishing firms where remote work is a routine component of job design from those where it remains marginal.

The results reveal substantial heterogeneity for mothers and no significant effects for

[7]To assess whether our wage estimates reflect pandemic-specific conditions, we re-estimate the main specification separately for 2020–2021 and 2022–2024, distinguishing between high- and low-WFH-intensity firms (Appendix Table A1). During the pandemic, eligible mothers in high-intensity firms experience a small but significant wage gain, while no effect appears in low-intensity firms. Importantly, this pattern persists after 2021: the post-pandemic coefficient for high-intensity firms remains positive and of similar magnitude—though less precisely estimated—whereas effects in low-intensity firms remain negligible. This suggests that the discontinuity at the 12-year threshold reflects a persistent adjustment in workplaces with greater WFH feasibility rather than a transitory pandemic artifact. For fathers, by contrast, we find no significant effect during the pandemic period and only a positive but modest wage gain in the post-pandemic period for high-intensity firms, indicating a weaker and less consistent response relative to mothers (results available on request).

fathers in either environment. For mothers employed in high-intensity-WFH 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. In low-intensity firms, instead, estimates are close to zero and never statistically significant.

Because our RDD identifies eligibility rather than take-up, this pattern can be interpreted as differences in implementation and compliance with the WFH priority rule, emphasizing the importance of organizational readiness. In high-intensity firms, remote work is already embedded in production processes, monitoring systems, and task allocation. In such environments, statutory priority translates into a meaningful, enforceable right that expands feasible flexibility and reduces coordination frictions. As a result, eligibility is more likely to affect actual work arrangements and, ultimately, wages. In low-intensity firms, by contrast, the entitlement is largely nominal: although eligibility exists in law, organizational constraints limit its practical use. Consequently, the ITT effect is attenuated because compliance and effective take-up are weak.

These findings highlight the complementarity between institutional rules and organizational capacity. Firms differ substantially in digital infrastructure, performance monitoring in hybrid settings, and managerial practices (Barrero et al., 2021; Emanuel and Harrington, 2024). Where such capabilities are strong, statutory flexibility can be converted into productivity-enhancing arrangements; where they are weak, legal rights alone are insufficient to generate economic effects (Bloom et al., 2015; Barrero et al., 2023).

Effects on paid work activity. Table 5 investigates whether wage effects are accompanied by changes in paid work activity, measured by paid weeks and paid days. For mothers in high-intensity WFH firms, eligibility leads to sizable and statistically significant increases in both margins: eligible mothers work about 0.27 additional paid weeks and 1.76 additional paid days per year. These estimates are stable across specifications with individual, firm, and time–sector controls. In low-intensity firms, the corresponding effects are small and statistically insignificant. Fathers' paid work activity does not respond in either environment. As with wages, these are ITT effects: they reflect the average impact of becoming eligible for WFH priority, combining actual behavioral responses with the degree of enforcement and feasibility within the firm. The concentration of effects in high-intensity firms indicates that priority access improves the regularity of paid work only when organizational conditions allow remote work to be effectively exercised.

These participation gains are consistent with reductions in time conflicts and coordination frictions, which are central to mothers' labor-supply decisions (Del Boca et al., 2020; Heggeness and Suri, 2021; Mas and Pallais, 2017: Kleven et al., 2019). Rather than altering formal hours or tasks, WFH priority improves the ability to sustain employment throughout the year in the presence of caregiving demands. Increased continuity of paid work therefore provides a natural mechanism behind the wage effects documented above.[8]

Panel (a) of Figure 4 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 determined using the mimicking-variance evenly-spaced method based on polynomial regression. Panel (b) reports the corresponding plots for low-intensity firms. This graphical evidence closely mirrors the previous regression results.

Given that wage and participation 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 WFH priority rule translates into labor-market adjustments in environments where the entitlement is plausibly implemented. In settings where remote work is marginal, eligibility rarely changes actual behavior, making further analysis less informative. Moreover, in light of the consistently null effects for fathers, subsequent sections concentrate on mothers, with results for fathers reported briefly in the text or in the appendix.

6 Further labor market outcomes

We extend the analysis beyond wages and paid work activity to assess 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 an indicator for whether the worker takes at least one sick-leave day during the year. Table 6 reports RDD estimates for these outcomes for mothers employed in high–WFH-intensity firms, the subset of employers for which the priority rule is plausibly implemented. Because the design identifies eligibility rather than actual take-up, the coefficients should again be interpreted as ITT effects of gaining statutory priority access to WFH.

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 expand formal contractual hours. This pattern is consistent with evidence that flexible work arrangements primarily help workers stabilize existing schedules rather than

[8]We also examine cross-spousal spillovers, asking whether one partner's employment in a high-intensity WFH firm affects the labor-market outcomes of a spouse who does not work remotely (Appendix B). Table B1 considers fathers employed in non-WFH firms, and Table B2 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' access to high-intensity WFH to male partners. For mothers (Tables B3–B4), results are likewise null for women in non-WFH firms. The only notable effect arises for blue-collar women (Table B4), who experience a modest but significant wage decline when their partners work in high-intensity WFH firms. The absence of effects on paid weeks or paid days points to adjustment along within-job margins rather than employment duration, plausibly reflecting increased household demands borne by women with limited flexibility. Overall, cross-spousal spillovers are limited and concentrated among blue-collar women whose partners work remotely.

expand them (e.g., Allen et al., 2015; Chung and van der Lippe, 2020). Column (2) reports no effect on the probability of experiencing a promotion or upgrade.[9] This result aligns with the literature emphasizing that career progression often 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 be insufficient to alter promotion processes that remain tied to visibility or in-office presence.

Finally, Column (3) shows no detectable effect on the probability of taking at least one day of sick leave per year.[10] While remote work can reduce commuting burdens and increase daily 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).[11]

These null results complement the positive effects on wages and paid work activity documented earlier. Eligibility for WFH priority appears to operate mainly through reductions in coordination frictions and improved regularity of work, rather than through changes in formal contracts, health-related absence behavior, or career ladders (Chung and van der Lippe, 2020; Barbieri et al., 2025). In other words, the rule improves how work is sustained over time, not how jobs are formally structured or rewarded within firms.

Overall, the evidence points to a nuanced pattern. WFH priority generates tangible benefits in terms of earnings and work continuity when organizational conditions allow the entitlement to be effectively implemented, yet its influence does not extend to job-quality margins that depend more fundamentally on personnel systems, promotion policies, and deeper organizational practices. Legal access to flexibility can relax short-run constraints, but it does not by itself transform the institutional features governing full-time status, health-related absence, or career progression.

[9] Using this measure, we also construct 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).

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

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

7 Discussion

The findings from Tables 3–6 convey a coherent pattern. Statutory priority access to WFH improves labor market outcomes for mothers, but only where firms have the organizational capacity to implement remote work in practice. Positive effects on wages and paid work continuity emerge in high–WFH-intensity firms, while no effects are detected in low-intensity establishments. The effects in high-intensity businesses are economically meaningful and scalable, given that the policy involves no transfers and only reallocates priority over work arrangements. As a benchmark, a 2 percent earnings gain corresponds to roughly 6 percent of the short-run motherhood wage penalty in Italy documented by Kleven et al. (2025) and to about 15 percent of the productivity gains found by Bloom et al. (2015) for Chinese call center employees who volunteered to WFH.

The contrast between high- and low–WFH-intensity highlights that policy design and workplace practices are complements: legal entitlements matter only insofar as firms can operationalize them. In contrast, we find no statistically significant effects for fathers in any specification or firm type, indicating that eligibility for WFH priority does not materially alter their labor market trajectories.

Because the empirical strategy identifies eligibility rather than actual take-up, the estimates should be interpreted as ITT effects of gaining statutory priority. From this perspective, the heterogeneity across firms reflects differences in implementation and compliance. In high-intensity firms, eligibility plausibly shifts the probability of actually working remotely, making the priority rule binding. In low-intensity firms, the entitlement remains largely nominal, and the ITT effect is close to zero. The results therefore speak not only to the impact of flexibility, but also to the importance of organizational readiness in translating legal rights into effective labor market changes.

From a theoretical standpoint, these results align with models of labor supply under household time constraints (Becker, 1985; Cortés and Pan, 2023), in which reductions in fixed costs of labor force participation primarily affect the partner bearing the greater share of caregiving responsibilities. Priority access to WFH lowers such fixed costs—most notably commuting time and the coordination of work and childcare—thereby relaxing constraints on mothers' effective labor supply and earnings. For fathers, who on average shoulder a smaller share of childcare and household production in Italy, increased flexibility is less likely to relax binding time constraints, providing a natural explanation for the absence of detectable effects.

At the same time, the null effects on full-time status, sickness days, and career advancement indicate that flexibility rights alone do not modify deeper features of job design. Contractual hours, health-related absence behavior, and promotion schemes are governed by internal labor markets and organizational norms that adjust more slowly than daily work arrangements. This distinction mirrors existing findings in the literature on family policy and gender gaps: short-run participation margins are more responsive to flexibility than longer-run career trajectories (e.g., Adda et al., 2017; Angelov et al., 2016; Bertrand et al., 2010; Kleven et al., 2019; Olivetti and Petrongolo, 2017).

The Italian case also illustrates a broader lesson about institutional adaptation. While the legal entitlement provides a universal framework for flexibility, its effectiveness depends on firm-level digitalization, managerial practices, and occupational composition. As remote work becomes a permanent feature of post-pandemic labor markets (Barrero et al., 2021), achieving gender equity at work requires not only statutory rights but also organizational environments capable of implementing them.

In sum, priority access to WFH raises mothers' wages and improves work continuity where firms can effectively deploy remote work, but it does not substantially alter full-time status, sickness incidence, or promotion prospects. Flexible work entitlements can ease day-to-day constraints and support earnings growth, yet broader improvements in job quality and career progression require complementary changes in organizational design and personnel practices.

8 Robustness checks

This section assesses the credibility of the RDD design and reinforces the interpretation of the estimates as ITT effects of statutory priority that operate only where WFH is actually implementable. The checks address three issues: (i) whether discontinuities arise in contexts where the entitlement cannot change behavior; (ii) whether results depend on our RDD implementation choices; and (iii) whether the discontinuity emerges only once remote work becomes operationally relevant after the pandemic. Overall, the evidence consistently shows that effects appear only for workers and firms for which the institutional rule can plausibly affect actual access to WFH, in line with the mechanism emphasized in the main analysis.

8.1 Placebo groups

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

Non-WFH firms. Table A2 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. Consistent with this logic, coefficients on wages are small, precisely estimated, and never statistically significant. This result rules out alternative interpretations based on life-cycle adjustments around children's age (e.g., tenure, schooling transitions, or labor-supply profiles) and reinforces the ITT interpretation: statutory eligibility matters only where firms can implement it.

Manual laborers in high-WFH-intensity firms. Table A3 repeats the analysis for manual laborers employed in high-WFH-intensity firms. Although these firms adopt remote work, manual tasks are intrinsically location-bound. Accordingly, estimated effects are small and imprecise for wages and paid activity. This finding is conceptually important: even within WFH-capable firms, statutory priority improves outcomes only for workers whose tasks are compatible with remote execution.

Pre-pandemic tests. Table A4 estimates equation (1) on pre-pandemic outcomes (2018–2019), restricting attention to firms classified as high-WFH-intensity in 2020, which is the first year with widespread WFH adoption. Before the pandemic, remote work was rarely used, so the statutory priority could not realistically change assignments. Consistent with this practice, coefficients are small, unstable in sign, and statistically indistinguishable from zero. These null results support the identifying assumption of smooth potential outcomes and show that the pandemic transformed legal eligibility into economically meaningful access to WFH. Effects emerge precisely when organizational capacity expanded.

Taken together, these placebo tests show that the policy operates exactly along the margins implied by the institutional setting: when WFH is infeasible, unavailable, or task-incompatible, no discontinuity appears.[12]

8.2 RDD-specific robustness

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

Donut RDD. Mothers just above the cutoff—i.e., mothers whose youngest child has just turned 12 years old—may have recently lost eligibility but still retain prior WFH arrangements, potentially smoothing the contrast and attenuating estimates. If so, baseline effects would be biased toward zero.

Table A5 reports donut RDD specifications excluding observations immediately above the cutoff (sequentially dropping mothers within 0.1 to 0.4 years past the threshold). By removing cases in which eligibility has just been lost, we ensure that the comparison takes place between mothers who are clearly entitled (age of the youngest child ≤ 12) and those who are unambiguously no longer eligible. Across all donut widths, wage effects in high-WFH firms remain positive, stable, and statistically significant. This confirms that the discontinuity is not driven by local persistence around the threshold and that the baseline estimates reflect the impact of statutory priority rather than mechanical smoothing around the age-12 cutoff.

Bandwidth choice. Table A6 evaluates alternative bandwidth selection rules. In particular, it reports estimates using both the conventional MSE-optimal bandwidth (MSE-

[12]No significant effects emerge for fathers in non-WFH firms, and for those in high-WFH firms holding blue-collar jobs.

TWO and MSE-SUM) and bandwidths minimizing coverage error rates (CER, CER-TWO, and CER-SUM).[13] Across all rules, estimates for high-WFH firms remain positive, precisely estimated, and similar in magnitude. The consistency of the results across selectors that differ both in 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. Figure A3 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 effect is tied to institutional eligibility for remote-work priority rather than to functional-form artifacts or arbitrary splits in the running variable.

8.3 Difference-in-discontinuities

A remaining concern is that parental behavior may change discretely around children's ages for reasons unrelated to the law, e.g., schooling stages (Cunha and Heckman, 2007; Caucutt and Lochner, 2020; Del Boca et al., 2014; Attanasio et al., 2020). To address this, we complement the RDD 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 simple: if the cutoff captured age-related behavior rather than the policy, similar jumps should exist before and after the pandemic. Taking the difference removes smooth age patterns and isolates the policy-induced component.

Formally, we restrict the sample to observations with running variable $R_i \in [R_c - h, R_c + h]$, computing a separate MSE-optimal bandwidth for each outcome.[14] We estimate the following specification using a local linear regression:

$$Y_{ifst} = \delta_0 + \delta_1 R^*_{it} + T_{it}(\gamma_0 + \gamma_1 R^*_{it}) + After_t[\alpha_0 + \alpha_1 R^*_{it} + T_{it}(\beta_0 + \beta_1 R^*_{it}] + X'_{it}\eta + Z'_{ft}\mu + \theta_s + \varepsilon_{ifst} \quad (2)$$

where Y_{ifst} denotes the relevant labor-market outcome (log annual wages, paid weeks, and

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

[14]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; we then take the mean of these two as the bandwidth employed in the difference-in-discontinuities specification.

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 $R^*_{it} = R_i - R_c$ is the normalized assignment variable. We also include individual covariates (X'_{it}) and firm-level controls (Z'_{ft}) as in equation (1), and two-digit sector fixed effects θ_s. ε_{ifst} is a stochastic error term. The difference-in-discontinuity coefficient, β_0, identifies the causal effect of statutory eligibility once remote work becomes an operational option in the post-pandemic period.[15]

Because the entitlement operates through implementation, we estimate the model separately by firm WFH intensity. In high-WFH firms, eligibility can shift actual access; in low-WFH firms, eligibility is unlikely to induce any material change.

Table A7 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 (≈1.7 percent increase in annual earnings), with corresponding gains in employment continuity (≈0.19 additional weeks, and ≈1.2 additional days). These magnitudes closely mirror the baseline RD results and show that the discontinuity becomes economically relevant only after remote work expands. By contrast, Table A8 reports negligible and insignificant interaction effects in low-WFH firms. Thus, the evolution of the cutoff effect over time is confined to organizational environments where WFH is actually implemented.[16]

Across all robustness exercises, the same pattern emerges. Discontinuities appear only when three conditions hold simultaneously: (i) firms adopt remote work; (ii) workers' tasks are compatible with it; and (iii) organizational capacity expanded after the pandemic. This pattern reinforces our interpretation: the estimates capture ITT effects of statutory priority mediated by implementation and compliance, not generic age-of-child effects. Legal rights improve outcomes, but only when firms can operationalize them.

9 Heterogeneity analysis

This section examines heterogeneity in the impact of the WFH priority rule within high-WFH-intensity firms—that is, among employers for whom remote work is technologically feasible and organizationally embedded.

[15]We also perform a balance test using the same difference-in-discontinuity specification (within the MSE-optimal bandwidth) as our main analysis: we regress observable characteristics on the forcing variable (child's age), an indicator for the post-2020 period, treatment status (having a child aged 12 or younger), and the key interaction between treatment status and the post-2020 indicator. The results (available upon request) show 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. This evidence reinforces that the identifying variation is not driven by systematic compositional differences around the threshold.

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

Regional Heterogeneity: Center–North vs. South. Table 7 shows that, within high-WFH-intensity firms, the effects of the entitlement are concentrated entirely in the Center–North. Eligible mothers experience statistically significant improvements in labor-market outcomes, with wages rising by about 2.2 percent and modest gains in paid weeks and days. In contrast, coefficients for the South are uniformly small and insignificant, indicating no detectable impact.

This regional divide is consistent with well-documented differences in organizational practices, competitive pressures, and managerial quality across Italy. Firms in the South exhibit weaker management and slower adoption of modern HR, ICT, and performance-monitoring systems (Bloom et al., 2016; Pellegrino and Zingales, 2017; Boeri et al., 2021). Lower levels of structured management and delayed digital integration make it harder to substitute virtual for in-person supervision, limiting the effectiveness of remote work even when technically feasible.

Agglomeration forces, skill complementarities, and innovation activity are also stronger in the Center–North (Glaeser and Gottlieb, 2009; Duranton and Puga, 2020), shaping the diffusion and productivity payoff of organizational technologies. More generally, successful WFH adoption requires explicit rules of engagement and effective monitoring (Aksoy et al., 2022; Flassak et al., 2023), and its productivity effects depend on task modularity (Jiang et al., 2024). Where production processes are less formalized, statutory WFH rights translate into limited changes in daily work organization despite legal eligibility.

Firm Size: Organizational Flexibility vs. Bureaucratic Constraints. Table 8 reveals strong heterogeneity by firm size. Small firms (<16 employees) exhibit the largest responses in wages and work continuity, medium-sized firms show attenuated effects, and large firms (>250 employees) display no detectable responses.[17] Small firms are more agile and able to reorganize tasks rapidly (Penrose, 2009; Atkeson and Kehoe, 2005). Remote work reduces fixed costs from short absences and scheduling conflicts, increasing the value of mothers' continuous participation. Their reliance on tacit (informal) coordination and individualized task allocation further raises the marginal benefit of flexibility (Lazear and Gibbs, 2015).

By contrast, large organizations operate through formal internal labor markets, standardized job ladders, and bureaucratic decision-making (Doeringer and Piore, 2020). Team production, interdependent workflows, and rigid scheduling constrain individual-level flexibility even when WFH priority is granted. Evidence on contract–task complementarities shows that tighter coordination requirements reduce the gains from flexible

[17]Small firms also exhibit a positive and statistically significant increase in full-time work, reinforcing the view that WFH eligibility strengthens labor-force attachment in these settings (results available upon request). Moreover, the results for fathers (available upon request) suggest that any gains are concentrated among small firms (fewer than 16 employees): while wages are not significantly affected, the estimated wage coefficient is larger than for other firm-size categories, and paid weeks and paid days increase noticeably in this group.

arrangements (e.g., Cattani et al., 2023). Thus, organizational constraints—rather than the entitlement itself—shape whether WFH priority translates into meaningful changes in day-to-day work arrangements.

Firm Age: Young versus Mature Establishments. Table 9 shows strong treatment effects among young firms (below the median age of 23 years), while older firms show none. This pattern is consistent with the literature on organizations' life cycles. Young firms adopt frontier technologies and modern HR practices more readily and experiment with alternative work arrangements (Haltiwanger et al., 2013; Criscuolo et al., 2014). They are also more likely to operate in digitally intensive sectors, where remote work is especially relevant, and feature flatter structures and greater task flexibility, allowing WFH to reduce coordination delays and interruptions (Caroli and Van Reenen, 2001; Ichniowski et al., 1997). Empirical evidence confirms that younger firms and those led by younger CEOs permit substantially more remote work (Aksoy et al., 2026).

In contrast, mature firms face technological and organizational rigidities—legacy systems, weaker monitoring, and hierarchical evaluation—that limit productivity payoffs from remote work (Bloom and Van Reenen, 2007). Productivity in settings with interdependent workflows is particularly sensitive to coordination frictions (Bresnahan et al., 2002), making individual-level flexibility harder to implement without efficiency losses. Hence, structural features of older organizations may limit their returns to statutory WFH rights.[18]

Broadband Availability: The Role of Digital Infrastructure. We next assess whether local digital infrastructure mediates the effectiveness of WFH priority by examining broadband quality across Italian municipalities. Productive remote work is inherently constrained by network capacity: insufficient internet quality at home prevents legal entitlements from translating into actual 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 was being phased out, it provides a consistent nationwide benchmark of baseline infrastructure. We match this information to workers' municipalities of residence, which directly determine the technological feasibility of WFH.[19] Table 10 shows that the policy's effects are sharply conditioned by broadband availability. In municipalities above the median in speed, WFH priority increases paid days and weeks and yields modest wage gains. In low-speed areas, effects are uniformly small and insignificant. Thus, technological constraints attenuate the policy's effectiveness.

[18] Experimental evidence on the benefits of WFH—such as the call-center study in Bloom et al. (2015)—comes from environments with highly modular tasks and standardized processes. These conditions are less prevalent in mature firms in traditional sectors.

[19] Results are similar when broadband conditions are instead matched on the municipality in which the firm operates.

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) and enhances firms' capacity to reorganize tasks and adopt digital practices (Forman et al., 2012). When infrastructure binds, however, even legally granted flexibility remains underutilized. This observed heterogeneity therefore reflects differences in local digital readiness across Italian municipalities, beyond the regional disparities documented earlier.

Overall, the heterogeneity results indicate that flexible-work policies operate through firm capabilities and local environments. Their impact hinges on managerial quality, task design, internal coordination, and digital infrastructure (Bloom and Van Reenen, 2007; Syverson, 2011; Bender et al., 2018; Bartik et al., 2020; Aksoy et al., 2022). WFH priority improves outcomes only where remote work can be implemented effectively. This aligns with evidence that digitalization and modern work practices are complementary in generating performance gains (e.g., Bresnahan et al., 2002; Bloom et al., 2012).

10 Conclusion

This paper studies how a statutory priority right to remote work affects parents' labor-market outcomes, exploiting Italy's age-based eligibility rule. We show that the entitlement strengthens mothers' attachment to work and modestly raises earnings, but only in firms with the technological and managerial capacity to use remote work productively. No comparable effects emerge for fathers, consistent with unequal pre-existing childcare burdens and with the policy operating primarily on the group for whom work–family reconciliation is most binding. Although modest, the policy offsets a non-negligible share of short-run motherhood penalties.

Although Italy's remote-work framework is highly codified and supported by collective agreements, it provides is a scalable, low-cost legal entitlement. Our results indicate that institutional design alone does not ensure uniform benefits for caregivers. Where remote work is already embedded in day-to-day operations, priority access allows mothers to better reconcile paid work and childcare, sustaining participation and slightly increasing wages. These gains reflect reduced day-to-day time constraints rather than structural changes in hours or career paths. The absence of effects on promotions, full-time status, or sickness leave suggests that WFH mainly sustain short-run labor supply, while leaving deeper determinants of job quality and advancement largely unchanged.

Even among firms capable of offering remote work, the rule's impact varies sharply across regions, firm characteristics, and local technological conditions. The largest gains appear in the Center–North, in smaller and younger firms, and in municipalities with faster broadband access. These patterns indicate that WFH delivers benefits only when it complements firm capabilities and local infrastructure: legal entitlements expand opportunities where the supporting inputs are present, but translate into little practical change where digital constraints or rigid practices bind.

The findings carry clear policy implications. Flexibility rights are most effective when paired with investments that make them operational. Expanding broadband access, supporting digital adoption, and encouraging modern management practices are likely to be crucial if remote-work policies are to benefit workers broadly and equitably. Without these complementary conditions, flexibility rights risk amplifying existing inequalities by helping workers in technologically advanced or well-managed firms more than others.

At the same time, the limited effects on career progression underscore that flexibility alone cannot close deeper gender gaps. Remote work reduces short-run disruptions but does not address structural barriers such as visibility requirements, rigid promotion ladders, or unequal caregiving responsibilities. Policies aimed at longer-run equality must therefore go beyond flexibility to include childcare provision, parental leave design, and promotion systems that rely less on physical presence and more on measurable performance.

In many advanced economies, statutory rights to request flexible working arrangements have expanded over the past decade. The EU's Work–Life Balance Directive (2019/1158), for example, requires member states to grant parents and carers the right to request flexible working arrangements, which may include remote work, as part of broader work–family reconciliation policies. In the UK, the Employment Relations (Flexible Working) Act 2023 further strengthens earlier provisions by allowing employees to request flexibility from the first day of employment and by tightening procedural protections. While these frameworks do not establish a universal right to work from home, they reflect a broader shift toward the legal recognition of flexible work across Europe. Our findings show that priority rules can support mothers' labor-market attachment, but their success depends critically on the environment in which they operate. For flexibility policies to deliver durable and widely shared gains, they must be embedded in a broader strategy that promotes digital readiness, modern management practices, and more inclusive career structures.

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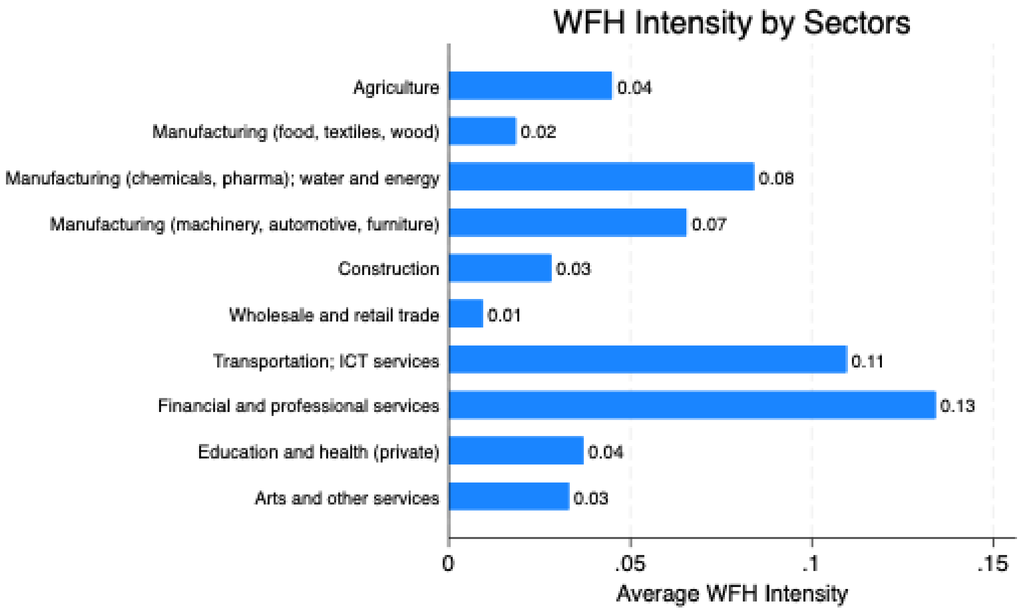
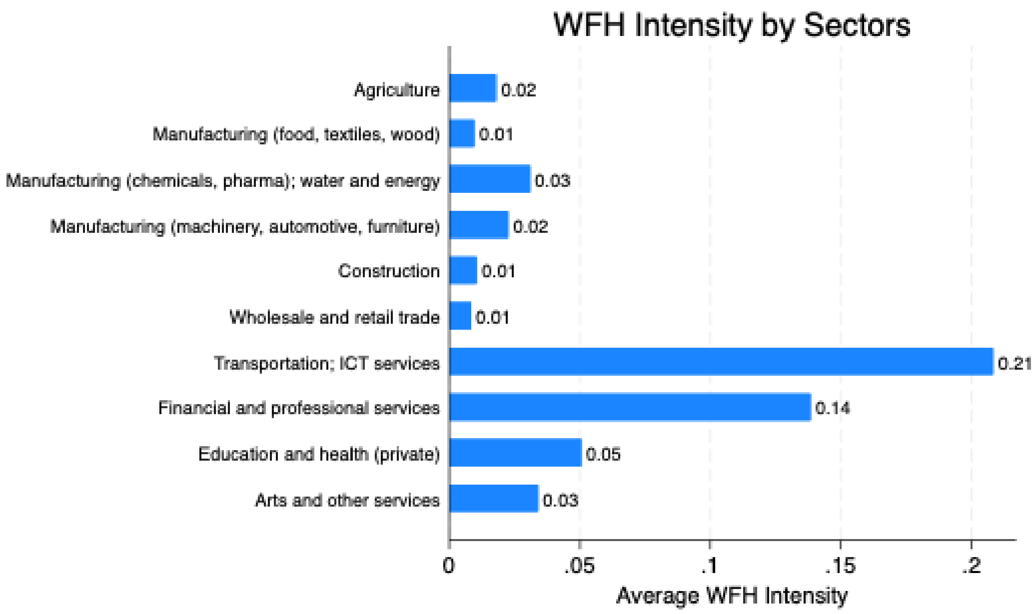
Figures and tables

Figure 1: Firm-level intensity of work from home by industry sector



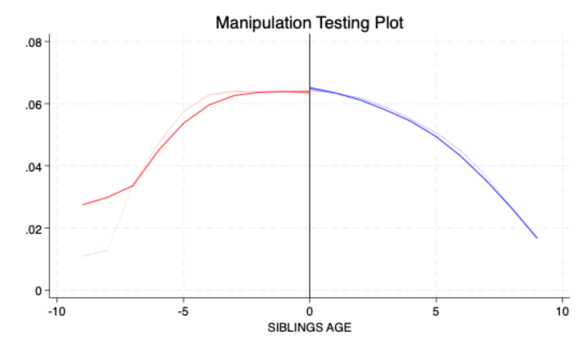
(a) Mothers



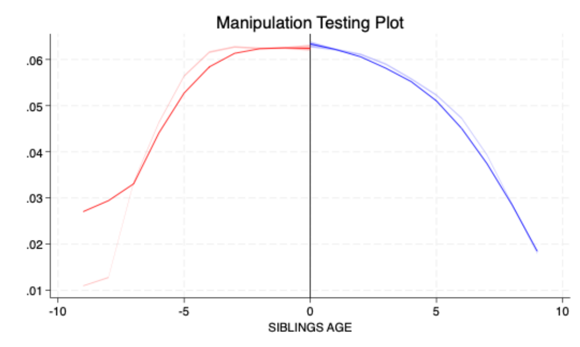
(b) Fathers

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 2: Manipulation of the forcing variable at the cut-off



(a) Mothers

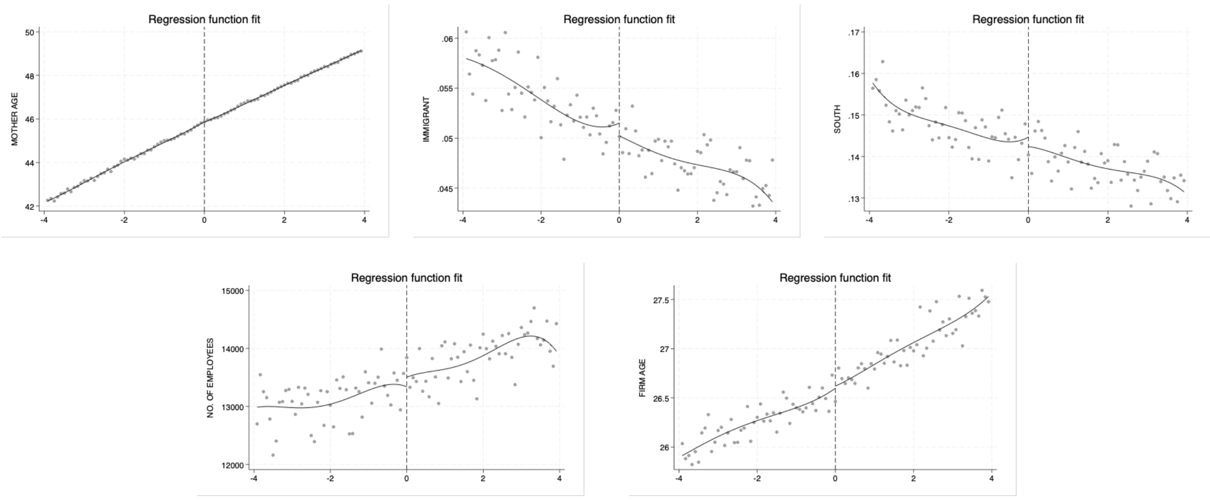


(b) Fathers

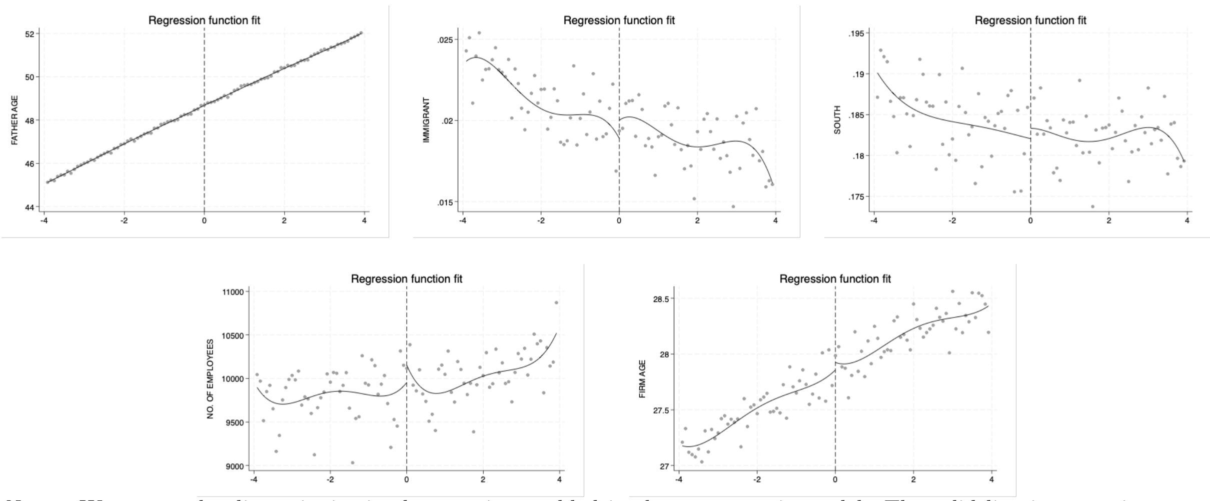
Notes. We test for the manipulation of the forcing variable at the threshold by employing a local polynomial density estimation method (Stata command: `rddensity`; forcing variable: Siblings Age).

Figure 3: Discontinuity in the covariates at the cut-off

Panel (a): Mothers



Panel (b): Fathers

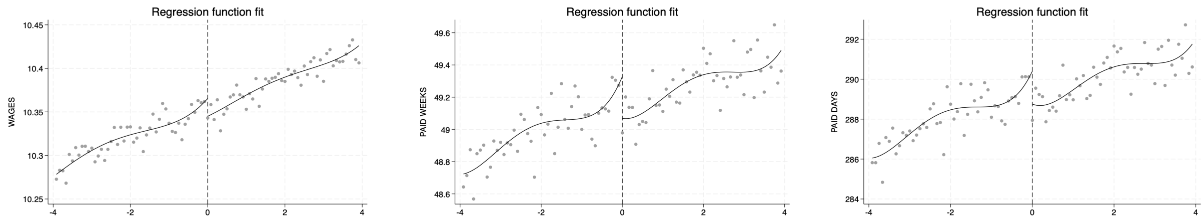


Notes. We report the discontinuity in the covariates added in the econometric model. The solid line is a running-mean smoothing (fourth order) of the variable on the vertical axis, performed separately on either side of the cut-off. The dots are the observed values averaged within bin.

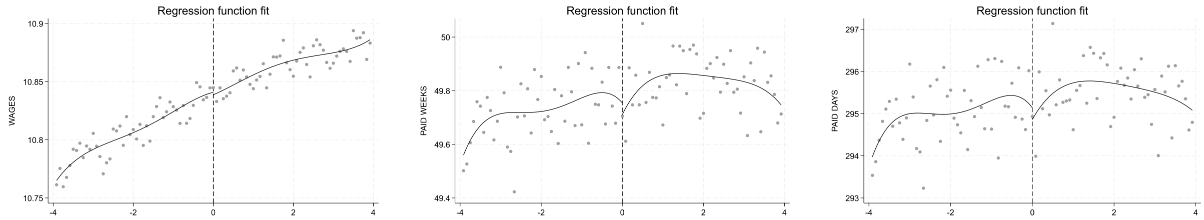
Figure 4: Discontinuity in the main LM outcomes at the cut-off. High and low WFH-intensity firms

Panel (a): High WFH-intensity firms

Mothers

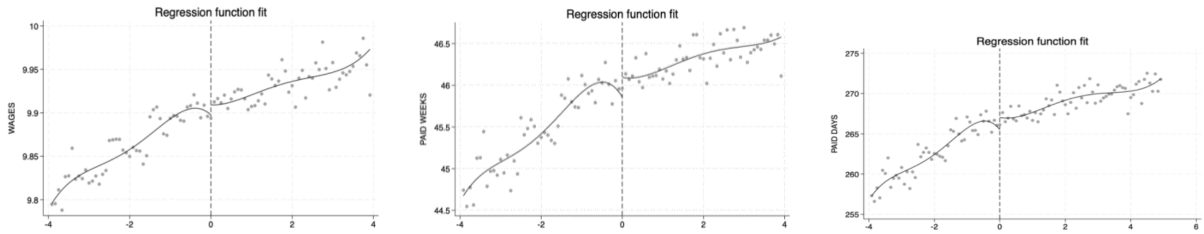


Fathers

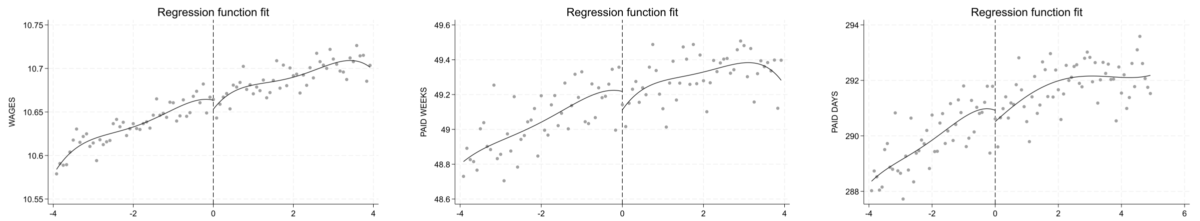


Panel (b): Low WFH-intensity firms

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 cut-off. The dots are the observed values averaged within bin.

Table 1: Descriptive statistics

Panel (a): Full sample
WFH 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
Siblings age	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

Panel (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
Siblings age	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

Panel (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
Siblings age	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 laborers) employed in the non-agricultural private sector during the period 2020–2024, in WFH firms. 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: Social Security (INPS) administrative archives. The WFH intensity index is based on data from the Ministry of Labor and Social Policies.

Table 2: Balance test on covariates

	(1)	(2)	(3)	(4)	(5)
Panel (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

	(1)	(2)	(3)	(4)	(5)
Panel (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 laborers within the MSE-optimal bandwidth for each covariate. Standard errors are robust to heteroskedasticity (shown in brackets). Significance at the 10 percent level is represented by *, at the 5 percent level by **, and at the 1 percent level by ***.

Table 3: WFH and wages. Nonparametric Sharp RDD

	Panel (a): Mothers				**Panel (b): Fathers**			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
	Wages	Wages	Wages	Wages	Wages	Wages	Wages	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 laborers 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. Statistical significance is denoted by * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

Table 4: WFH and wages. High vs. low WFH-intensity firms. Nonparametric Sharp RDD

	(1) Wages High Intensity	(2) Wages High Intensity	(3) Wages High Intensity	(4) Wages Low Intensity	(5) Wages Low Intensity	(6) Wages Low Intensity
Panel (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
Panel (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 laborers, 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. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: WFH and paid weeks and days. High vs. low WFH-intensity firms. Nonparametric Sharp RDD

	(1) Paid Weeks High Intensity	(2) Paid Days High Intensity	(3) Paid Weeks Low Intensity	(4) Paid Days Low Intensity
Panel (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
Panel (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 laborers, using the MSE-optimal bandwidth. In each column we add the full set of controls. Robust standard errors (heteroskedasticity-consistent) are reported in brackets. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: WFH and further LM outcomes. High WFH-intensity firms. Nonparametric Sharp RDD

	(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 laborers within the MSE-optimal bandwidth. All columns include 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 7: WFH, wages and paid weeks and days. Center-North vs. South (High WFH-intensity firms). Nonparametric Sharp RDD

	(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 laborers within the MSE-optimal bandwidth. All columns include 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 8: WFH, wages and paid weeks and days by firm size (High WFH-intensity firms). Nonparametric Sharp RDD

	(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 laborers 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. Statistical significance is denoted by * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

Table 9: WFH, wages and paid weeks and days by firm age (High WFH-intensity firms). Nonparametric Sharp RDD

	(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 laborers 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. Statistical significance is denoted by * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

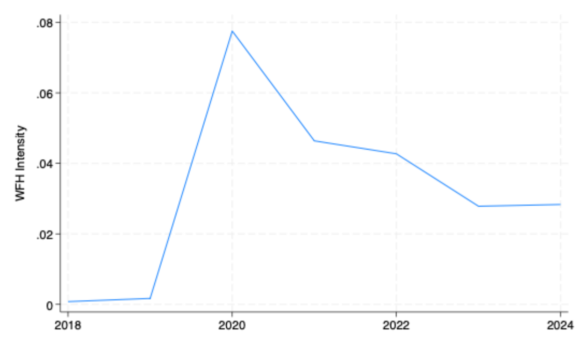
Table 10: WFH, wages and paid weeks and days. High vs. low broadband speed. Nonparametric Sharp RDD

	(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

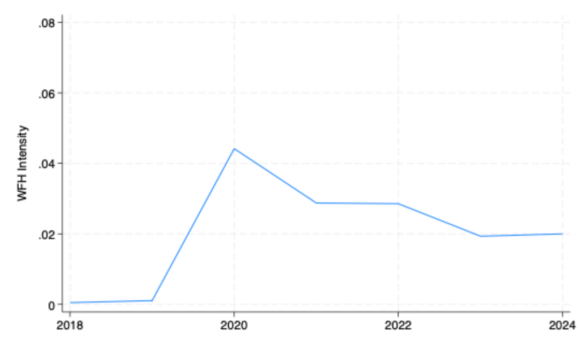
Notes. Nonparametric Sharp RDD estimates. The outcome variable is reported at the top of each column. Odd-numbered specifications focus on firms located in municipalities with high ADSL speed (above the median), whereas even-numbered specifications focus on firms located in municipalities with low ADSL speed (below the median). The sample consists of female workers and excludes laborers 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. Statistical significance is denoted by * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

A Further figures and robustness checks

Figure A1: Evolution of firm-level WFH intensity over time



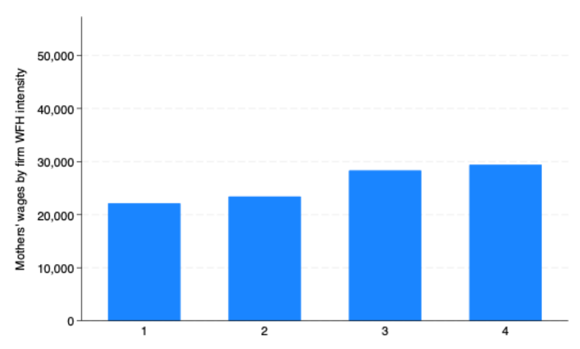
(a) Mothers



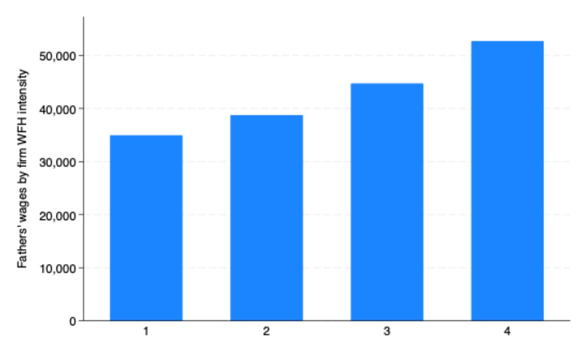
(b) Fathers

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 A2: Annual earnings of mothers and fathers by firm-level WFH intensity



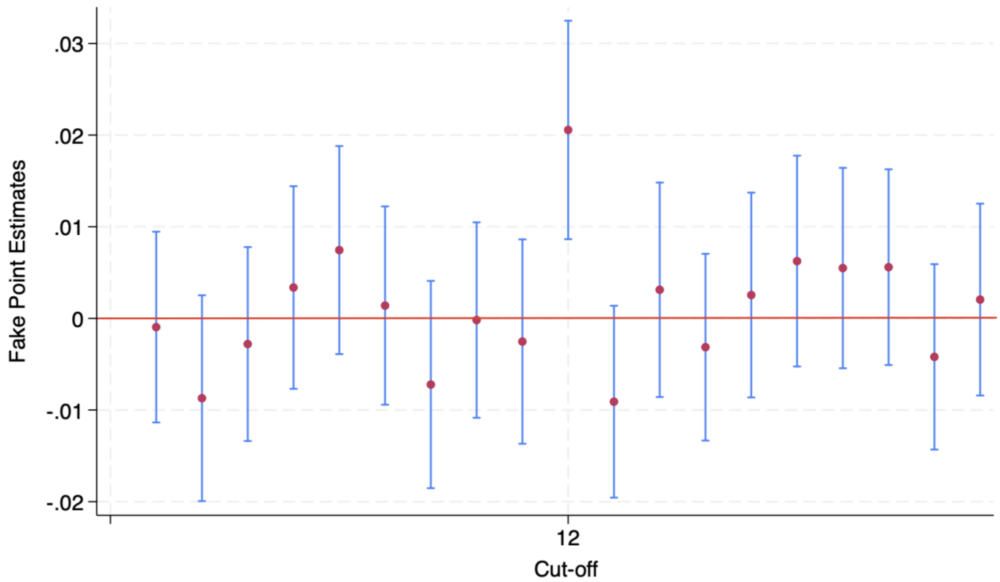
(a) Mothers



(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 and excluding manual occupations.

Figure A3: 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 A1: 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 laborers 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 A2: 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 A3: 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 laborers 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 A4: 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 laborers 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 A5: 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 laborers 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 A6: 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**	0.002**	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 laborers 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. Statistical significance is denoted by * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

Table A7: 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 laborers 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. Statistical significance is denoted by * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

Table A8: 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 laborers 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. Statistical significance is denoted by * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

B Spillover effects

Table B1: 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 female spouse's working-from-home (WFH) status on her male partner, conditional on the male spouse not working from home. The outcomes shown at the top of each column correspond to male labor market indicators. The sample includes married or cohabiting couples in which both partners are employed in the private sector. All specifications add the full set of controls. Standard errors are heteroskedasticity-consistent. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B2: Spillover effects of female spouse's remote work on male blue-collar spouses

	(1) Wages	(2) Wages	(3) Paid weeks	(4) Paid weeks	(5) Paid days	(6) 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 female spouse's working-from-home (WFH) status on her male partner, conditional on the male spouse being a blue-collar worker. The outcomes shown at the top of each column correspond to male labor market indicators. The sample includes married or cohabiting couples in which both partners are employed in the private sector. All specifications add the full set of controls. Standard errors are heteroskedasticity-consistent. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B3: Spillover effects of male spouse's remote work on Non-WFH female spouses

	(1) Wages	(2) Wages	(3) Paid weeks	(4) Paid weeks	(5) Paid days	(6) 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 male spouse's working-from-home (WFH) status on his female partner, conditional on the female spouse not working from home. The outcomes shown at the top of each column correspond to female labor market indicators. The sample includes married or cohabiting couples in which both partners are employed in the private sector. All specifications add the full set of controls. Standard errors are heteroskedasticity-consistent. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B4: Spillover effects of male spouse's remote work on female blue-collar spouses

	(1) Wages	(2) Wages	(3) Paid weeks	(4) Paid weeks	(5) Paid days	(6) 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 male spouse's working-from-home (WFH) status on his female partner, conditional on the female spouse being a blue-collar worker. The outcomes shown at the top of each column correspond to female labor market indicators. The sample includes married or cohabiting couples in which both partners are employed in the private sector. All specifications add the full set of controls. Standard errors are heteroskedasticity-consistent. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.