Leave and Let Leave: Workplace Peer Effects in Fathers' Take-up of Parental Leave*

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Abstract

Relying on a reform that increased parental leave generosity, we estimate workplace peer effects in the use of leave, with a focus on fathers. Coworker fathers are more likely to take parental leave when exposed to a higher share of peer fathers, who are exogenously affected by the reform. This effect is stronger in larger establishments, those with higher levels of social capital and higher use of parental leave before the reform. We also document that own-gender peer effects are larger than cross-gender influences, and show the absence of career costs for fathers exposed to the reform, which provides an explanation for our findings. Peer effects extend to coworker fathers' partners, who experience an increase in earnings and labor supply. Peer effects are observed also for mothers, but the response of their partners is less pronounced.

Keywords: Parental leave, peer effects, career costs, female labor market participation

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1 Introduction

Widespread gender gaps in labor market outcomes remain persistent. Recent research points to the role of intra-household specialization in perpetuating these gaps, particularly after parenthood. During this life stage, women shift their time toward childcare and domestic production, leading to lower labor force participation and earnings, whereas men tend to specialize in paid work, reinforcing traditional economic roles within households (Kleven et al., 2019). To promote maternal workforce participation, the engagement of fathers in childcare is critical (Cortés and Pan, 2020), especially for households with moderate pre-existing gender gaps (González and Zoabi, 2021). In this respect, countries have implemented father-specific parental leave policies, whose success in increasing paternal involvement in childcare is contingent upon cultural and institutional contexts (Canaan et al., 2022; Olivetti and Petrongolo, 2017). Social interactions and peer influences can play a key role in affecting the impact of policies on parental leave take-up (Dahl et al., 2014), favoring their diffusion among fathers.[1]

This paper estimates workplace peer effects of parental leave take-up, with a focus on fathers. To this end, we leverage a 2015 reform in Italy, that increased the generosity of shared parental leave provisions. Specifically, the reform raised the replacement rate from 0 to 30 percent for parents of children aged 3-5, whereas such replacement rate before the reform applied only to parents of younger children. Within this context, we analyze peer effects across various degrees of network connections. First, taking advantage of the exogenous change in the eligibility criteria of reform-exposed workers, we estimate peer effects at the establishment level in leave adoption among coworkers who become parents. Second, we estimate the indirect effect of reform-induced leave take-up by peer workers on career outcomes of coworkers' partners. The policy change we exploit involves both fathers and mothers, facilitating a comparative analysis of its gender-specific and cross-gender effects, thereby adding novel evidence on the role of workplace interactions in program participation.

We conduct our analyses using administrative longitudinal matched employer-employee data collected by the Italian Social Security Administration (*Istituto Nazionale della Previdenza Sociale*, INPS), covering the universe of employees in the Italian non-agricultural private sector. The dataset allows us to reconstruct individual working histories and parental leave utilization, and, most importantly, the establishment-level network of coworkers in any given year. Additionally, leveraging data on couples, we are able to examine both professional and marital ties in a panel setting, allowing us to study the influence of peer fathers and mothers on coworkers who will become parents, as well as on the partners of these coworkers, who are employed in (potentially) different estab-

[1]While this paper focuses on labor market outcomes, prior research has investigated the effects of paternity leave policies also on fertility (Farré and González, 2019), child development (Cools et al., 2015; Farré et al., 2024), and household well-being (Kotsadam and Finseraas, 2011).

lishments. We define *peer parents* the workers employed in a given establishment who are directly affected by the reform, and *coworker parents* their colleagues who become parents in the four years following the reform. This definition of coworker parents ensures that they are not directly affected by the reform, as they had not yet become fathers at the time.

We begin by evaluating the impact of the reform on individual parental leave take-up, serving as the starting point for our subsequent analysis of peer effects. To this end, we estimate a dynamic triple-differences model that compares the parental leave take-up of treated and control parents (i.e., those with children aged 3 to 5 and 0 to 2 years, respectively), before and after the reform, and before and after June—the month marking the policy implementation. We find a positive effect on parental leave take-up, with an increase of 24.8% among fathers and 16.1% among mothers, relative to the pre-reform average take-up rate in the control group. These effects are concentrated among workers on permanent contracts and households that have already used parental leave in the past. They are not driven by parents of children of a specific age. For fathers, the effects we document primarily manifest on the extensive margin—more parental leaves taken—rather than on the intensive margin—i.e., their duration.

Having established that the reform effectively influenced parental leave take-up, we then exploit the policy impact at the establishment level to identify peer effects. To this end, we use variation in establishment-level exposure to the parental leave reform, measured by the share in 2014 of peer fathers (mothers) of children aged 3-5, relative to the total number of fathers (mothers) with children aged 0-5 employed in the establishment. We use this measure as an instrumental variable (IV) for the share of fathers (mothers) of children aged 3-5 who actually took parental leave. This identification strategy rests on the assumption that establishment-level characteristics are conditionally exogenous to the exposure to the reform, ensuring that the latter can be used as an exogenous shifter of parental leave take-up in the establishment. To further strengthen this, we control for a rich set of observable establishment characteristics and sector-specific unobservable heterogeneity in all our estimations. Given the skewness in the distributions of the variables considered, we use an IV Poisson model and report marginal effects. Our estimates show that, also at the establishment level, increasing the generosity of parental leave policies has a positive impact on parental leave take-up among peer fathers. First-stage estimates show that establishments with greater exposure to the reform experienced a more substantial increase in leave take-up during the reform year. Turning to the IV Poisson estimates of peer effects, we find that establishments with higher parental leave take-up among peer fathers experience a subsequent rise in leave utilization among their coworkers: a 1% increase in the share of peer fathers taking parental leave induces a 0.24% increase in the take-up rate among coworkers who become fathers in the following year. The effects are statistically and economically significant and persist in the medium term, up to four years after the reform. Among mothers, we estimate peer effects of comparable size. In addition, we provide evidence of the robustness of our estimates by showing the absence of pre-trends in peer effects on coworkers' parental leave take-up before the reform. We also show that the observed increase in parental leave utilization among coworkers is not attributable to a subsequent rise in fertility rates in the medium term.

To explore the mechanisms behind our results, we conduct heterogeneity analyses based on several establishment characteristics. Leveraging unique information in the INPS data, we construct an indicator of prosocial behavior at the establishment level, defined as the presence of at least one worker taking leave for blood donation in 2012-2014. The peer effects are approximately twice as large in prosocial establishments. Moreover, these effects are more sizable in larger establishments and in family-friendly ones, i.e., those with a history of parental leave utilization between 2012 and 2014. Heterogeneity analyses show that while establishment characteristics significantly influence the size of peer effects, they are not the sole drivers. In particular, we estimate both own-gender and cross-gender peer effects. If the role of peers is relatively more important than that of establishments, we would expect own-gender effects to be stronger than cross-gender effects. Our findings confirm this expectation.

To further explore the drivers of peer effects, we show that eligible peer fathers have career trajectories comparable to, or slightly more favorable than, those of non-eligible peer fathers. This observation suggests that taking a period of parental leave does not pose a significant threat to career advancement, thereby mitigating concerns among coworker fathers about the professional repercussions of increased parental leave take-up. For mothers, the findings on career consequences are more mixed, signaling the importance of social norms independently of labor market effects.

Finally, we show that peer fathers' increased parental leave take-up positively influences labor market outcomes of coworker fathers' partners. These partners experience higher earnings, increased weeks worked, advancement into higher-paying occupations, a greater likelihood of holding permanent contracts, but also a higher use of part-time contracts. In contrast, the effects of peer mothers on coworker mothers' partners are more muted. This result is important as it shows that peer effects can indirectly help close gender inequality within households.

Our paper contributes to the literature on peer effects in program participation. In particular, we complement in several ways Dahl et al. (2014), who focuses on father-on-father peer effects in both professional and family networks by exploiting a paternity leave reform in Norway. First, the reform we leverage applies to both fathers and mothers in a shared parental leave scheme. This setting allows us to assess the relative strength of own-gender and cross-gender peer effects. Second, we investigate the indirect effects on father and mother coworkers' partners, providing novel evidence on the improvement in labor market outcomes for female partners and almost null effects for male ones. In this perspective, our analysis aligns with the literature on the child penalty, which documents that mothers in couples where the partner has greater occupational flexibility experience lower earnings losses (Bang, 2021). Third, thanks to the richness of our data, we identify a number of employer characteristics associated with stronger peer effects, in particular the level of social capital within the establishment—a variable often difficult to measure in most empirical studies. Finally, we focus on Italy, a country characterized by lower gender equality and more conservative gender attitudes compared to Norway, to provide external validity in other contexts. Our findings highlight that social interactions within establishments can be an effective transmission mechanism for public policy.

Other relevant studies on peer effects in program participation include Carlsson and Reshid (2022) and Dottori et al. (2024). Unlike the former, who examine peer effects among coworkers in the use of parental leave in Sweden, our paper exploits variation due to a parental leave reform, thereby mitigating potential problems related to non-random sorting in the network of coworkers. While Dottori et al. (2024) study the same policy as we do, they employ a different methodology and focus exclusively on mothers.

Our paper is also related to the literature examining the impact of parental leave reforms on take-up within households, focusing particularly on fathers. Incentives for fathers to take parental leave vary, including monetary benefits and "daddy quotas." Prior research, such as Bartel et al. (2018) and Duvander and Johansson (2012), has shown that policies like paid family leave programs and reserved leave months for each parent increase the use of paternal leave. Jørgensen and Søgaard (2021) suggest that creating a less generous system with significant earmarked leave may be the most effective way to increase fathers' share of leave in Denmark. Ekberg et al. (2013) find short-term effects of "daddy months" on leave take-up but no subsequent behavioral effects within households. Conversely, Patnaik (2019) demonstrates that a parental insurance plan with reserved leave for fathers in Quebec significantly increased paternal participation and reduced gender specialization in household roles, as documented also for Spain for specific types of households in González and Zoabi (2021). In our paper, the direct effect of the reform on the use of parental leave by fathers and mothers serves as the first stage for analyzing peer effects at the establishment level rather than being the primary focus of our analysis. Nevertheless, the first-stage findings suggest that parental leave reforms targeting both parents increase take-up by mothers and fathers, with relatively larger proportional increases observed among fathers.

Finally, we also contribute to the social science literature by analyzing the importance of workplace interaction in influencing program participation. Previous descriptive studies underscore the relevance of higher-order beliefs in the workplace (Thébaud and Pedulla, 2016), career concerns (Johnsen et al., 2023; Krstic and Hideg, 2019), workplace culture (Brandth and Kvande, 2019; Haas and Hwang, 2019), information transmission

(Nicoletti et al., 2018), imitation (Akerlof and Kranton, 2000), and stigma (Celhay et al., 2022). Our paper emphasizes that increasing parental leave take-up among peer fathers within the workplace can alleviate barriers faced by coworker fathers in engaging with such programs, highlighting the importance of career concerns and establishment-level social capital, in line with the insights from this strand of the literature.

2 Background and institutional setting

Leave policies around childbirth Parental leave is a period of absence from work that parents can take after the mandatory maternity leave. In Italy, mothers have a five-month mandatory maternity leave entitlement, which may be taken either two (one) months before childbirth and three (four) months afterward.[2] The National Social Security Institute (INPS) covers 80 percent of the last salary for compulsory maternity leave, and many collective agreements require employers to provide the remaining 20 percent. A mandatory paternity leave was introduced in 2012: it started at just one day in 2013 and gradually extended to 10 days by 2022. This duration remains significantly shorter than in many other developed countries (OECD, 2023).

The voluntary parental leave lasts 10 months, 6 of which are paid at 30 percent of the last earnings and the rest at zero percent. Each parent can take a maximum of 6 months of leave.[3] According to data provided by OECD (2023), the use of parental leave is rather low in Italy in comparison with other developed countries. The number of children for whom the mother, the father, or both parents used parental leave at least once was approximately 333,000 in 2018, rising from 280,000 in 2014 (Figure A.1), in spite of the decline in the number of births and the population of children aged 0-6, which went from 3.8 million in 2014 to 3.4 million in 2019. Parental leave use is unbalanced across gender: mothers are more than 80 percent of total parental leave takers, with only little changes over time.[4] The low replacement rate helps explain both the low take-up and the fact that women are the main users of the leave. Indeed, it is more convenient to forgo the lower wage in the family, usually that of women, as they are more likely to be secondary earners.[5]

The reform of parental leave in 2015 The legislative decree 80/2015, passed on 25 June 2015, introduced a number of changes to the design of parental leaves (see Table 1). In particular, the reform included the following main policy changes:

[2]In the most recent years, not included in our empirical analysis, there is also the option of taking the entire five months after birth.

[3]Fathers that take at least 3 months of leave are entitled to get a maximum of 7 months of leave rather than 6 — for a total of 11 months considering both parents.

[4]Parental leave duration is just under 80 days for a child between 0 and 3 years of age, approximately 30 days for a child between 3 and 6 years of age, and 20 days for older children.

[5]More details on the leave take-up over time in Italy is reported in Section 3.

Table 1: Parental leave compensation and rules by child's age and over time

Period of utilization (age of the child)[(1)]	**0–2 years**	**3–5 years**	**6–7 years**	**8–11 years**
2001–2015	Replacement rate equal to 30% of the average daily wage	Means-tested.[(2)] Leave is available for parents who did not use it in the first 3 years or for the unused residual part	No compensation	No leave
2015 onwards	Replacement rate equal to 30% of the average daily wage		Means-tested.[(2)] Leave is available for parents who did not use it in the first 6 years or for the unused residual part	No compensation

Notes. (1) The age ranges shown should be understood as up to the 364th day of the indicated age. (2) Leave in this age range is compensated only if the individual income of the requesting parent is lower than 2.5 times the annual amount of the public minimum pension.

1. The extension of the period during which a benefit of 30 percent of the monthly wage is paid, with the maximum age of the child being raised from 3 to 5 years. There were no changes in the replacement rate for parents of children between 0 and 2 years of age.

2. The extension of the period during which parents can take parental leave, with an increase in the maximum age of the child from 8 to 12 years.

3. The possibility for parents of children between 6 and 8 years of age to receive the 30 percent replacement rate if their income is below 2.5 times the minimum pension.

4. The possibility for parents of children of any age to take the leave on an hourly basis.

The policy changes applied to parents whose children were younger than 8 years before the reform and to those whose children were between 8 and 12 years old and who had not already taken the maximum length of the leave. In the empirical investigation, we will exploit the first change listed above, and discuss possible confounding effects of the other policy changes.

3 Data

For our empirical investigation, we use administrative monthly and annual matched employer-employee data from the Italian National Social Security Institute (INPS), made available through the VisitInps program, from 2012 to 2019. This dataset covers all employees in the private non-agricultural sector since 1974. We merge different data sources: *i)* monthly and annual contribution records on worker histories (containing information on annual earnings, weeks worked, occupation, municipality of work, type of contract, its start and end dates, and the reason for termination), and demographic characteristics (gender, age, region of birth); *ii)* monthly and annual records on firms (i.e., sector and location); *iii)* the universe of maternity, paternity, and parental leave applications, which also allows the identification of childbirth episodes for working parents (and, therefore, the age of children), and the matching of workers in couples; *iv)* data on notional contributions for employees related to leave for blood donations, which we use to construct a measure of social capital at the firm level. In particular, we define as pro-social those establishments where there was at least one such leave taken in the period 2012-2014.

In the analysis of peer effects, our unit of observation is the establishment, which we define as the intersection between the fiscal code of the firm and the municipality of work of the individual.[6]

Aggregate statistics on parental leave take-up and duration Figure A.1 shows the aggregate number of parental leave benefits claimed between 2012 and 2018. Parental leaves are disproportionately used by mothers rather than fathers, although men have doubled their take-up, going from 32,000 in 2012 to 65,000 in 2018. There is a slight increasing trend across mothers, as well, whose take-up went from 248,000 to 268,000. Zooming-in on the years in which parental leave policies were changed, we observe an upward trend between 2014 and 2015 among fathers. Such extensive-margin increase is mostly due to an increase in short-duration parental leaves: Figure A.2 shows in panels A and B that the share of parental leave with one-day duration increased by 2 percentage points among both fathers and mothers between 2014 and 2015. This translated into a mild decrease in the overall average duration of parental leaves (panels C and D) over time. Fathers' parental leave take-up in both years follows a clear pattern, with the majority of them taking very short leaves (fewer than seven days). Panel C further highlights a seasonal trend, with parental leave take-up peaking in winter and early spring. In contrast, take-up of mothers appears more evenly distributed across months, though it declines more sharply during the summer. The joint interpretation of the top and bottom panels of Figure A.2 suggests that fathers' take-up is more responsive to short-term, ad hoc needs——potentially linked to seasonal illness or other time-specific demands——whereas mothers' take-up follows a more stable allocation pattern. This descriptive evidence suggests that the main margin of adjustment to policy is the extensive one—something we will corroborate more formally in the next sections.

[6] This is the finer establishment identifier that the data allow to measure. We cannot distinguish different establishments within the same municipality.

4 Empirical strategy

We start by estimating the effect of the reform at the individual level on program take-up by parents, considering the allocation of leave within the household. In fact, differently from the setting in Dahl et al. (2014), in our framework of shared parental leave, endogenous intra-household decisions determine the take-up of both mothers and fathers.

We then examine the aggregate impact of the reform on parental leave take-up at the establishment level. This analysis serves as a first step to assess how changes in leave take-up by peer fathers influence their coworkers' decisions to take leave. To this end, we build a measure of establishment-level exposure to the reform as the share of peer fathers (mothers) of children aged 3-5 on the total number of fathers (mothers) employed in the firm in 2014. We use this measure as an instrumental variable (IV) for the share of peer fathers (mothers) of children aged 3-5 who actually took parental leave in 2015.

We discuss the empirical approach more formally, as well as the underlying assumptions for the identification of the causal effects of both approaches in the following sections.

4.1 Individual-level analysis

Program take-up To estimate the impact of the reform at the individual level, we sample working parents of children between 0 and 2 and 3 and 5 years of age in 2014 and 2015. We drop records with missing information on labor contracts and earnings. We further drop individuals who are younger than 15 and older than 64.

We use a dynamic triple-differences model, in which we compare eligible (treated) parents—those with children between 3 and 5 years of age—with non-eligible (control) parents—those with children between 0 and 2 years of age—in the year of the reform (2015) with respect to the year before (2014), by calendar month. The resulting model specification is as follows:

$$\begin{aligned} y_{imt} = \alpha + \sum_{k \neq \text{June}} \left[\beta^k D_{it} \cdot A_t \cdot \mathbf{1}(k=m) + \gamma^k D_{it} \cdot \mathbf{1}(k=m) + \delta^k A_t \cdot \mathbf{1}(k=m)\right] \\ + \eta D_{it} \cdot A_t + \zeta D_{it} + \kappa A_t + \theta_i + \lambda_m + \omega_{r(i)m} + \sigma_{s(i)m} + \epsilon_{imt}. \end{aligned} \tag{1}$$

The dependent variable, denoted as y_{imt}, is equal to one if individual i takes parental leave in calendar month m in year t. D_{it} is a dummy equal to one for parents of 3-5-year-old children and zero for parents of 0-2-year-old children in each year t. A_t is a dummy equal to one for year 2015 and zero for 2014. $\sum \mathbf{1}(k = m)$ are indicators for calendar months, where we exclude the dummy for June, i.e. the month when the reform was passed, which serves as a reference month. The model includes individual fixed effects (θ_i), capturing time-invariant characteristics of individuals that may affect their propensity to take parental leave. Additionally, calendar month (λ_m), region-by-month

($\omega_{r(i)m}$), and sector-by-month ($\sigma_{s(i)m}$) fixed effects control for month-specific, regional and sectoral time-varying unobserved shocks that impact parental leave take-up. Finally, ϵ_{imt} is an error term, which we cluster at the individual level.

The coefficients of interest are the β^k's, which capture how parental leave take-up of treated individuals changes compared to that of control individuals with respect to June during the post-reform period relative to the pre-reform period. The β^k's measure the average treatment on the treated (ATT) and can be interpreted as causal if the parallel trends assumption holds.

Labor market outcomes We evaluate labor market outcomes of eligible and non-eligible peer parents by estimating a 2SLS regression static version of equation (1). Specifically, we run the following model:

$$\begin{aligned} y_{i,p,t+\tau} = & \alpha + \beta D_{it} \cdot A_t \cdot K_p + \gamma D_{it} \cdot K_p + \delta A_t \cdot K_p + \eta D_{it} \cdot A_t \\ & + \zeta D_{it} + \kappa A_t + \lambda K_p + \theta_i + \epsilon_{i,p,t+\tau}, \end{aligned} \tag{2}$$

where D_{it}, A_t, and θ_i are defined as in equation (1). The outcomes $y_{i,p,t+\tau}$ are defined for individual i, in subperiod p—we collapse the data in two subperiods within the year: January-July and August-December—, and time $t+\tau$, where $t = \{2014, 2015\}$ while $\tau = \{1, 2\}$. K_p is a dummy equal to one for the subperiod August-December.[7] $\epsilon_{i,p,t+\tau}$ is an error term. The outcomes we consider are: being employed (in the non-agricultural private sector), being employed in the same establishment, the cumulative earnings and days worked between t and $t+\tau$, and dummies equal to one for switching from temporary to permanent contracts, from white-collar to manager, from blue-collar to manager, and from blue-collar to white-collar.

4.2 Establishment-level analysis: peer effects in parental leave take-up

Our main goal is to quantify the influence of peer fathers in parental leave take-up on their male coworkers.[8] We sample firms in year $t = \{2014, 2015\}$ that employed at least one father of a child between 0 and 5 years of age in 2014. We define eligible *peer* fathers as those with children between 3 and 5 years of age, consistently with the individual-level analysis, in year t. We then define *coworker* fathers as those who become fathers for the first time in year $t+k$, with $k = \{1, 2, 3, 4\}$, and who worked in the same establishment of peer fathers in year t. We focus on first-time coworker fathers in $t+k$ to avoid them being exposed to the reform in t.

[7] We consider July in the pre-reform period as the law was passed on June 25 and requires 15 days to become effective. Hence, July is still an adjustment period.

[8] In the Appendix, we also provide results on peer effects among mothers.

In order to measure the exposure of establishments to the parental leave reform, we build a variable Z_j that is equal to the share of peer fathers of children aged 3-5 on the total number of fathers of 0-5-year-old children employed in establishment j in 2014.[9] We then use this variable, interacted with a dummy for the post-reform period, as an instrument for the take-up of parental leave by fathers within establishment j.[10]

Formally, we adopt an instrumental variable Poisson estimation at the establishment level.[11] The equation of the first stage is a difference-in-differences specification taking the following form:

$$P_{jt} = \alpha + \beta Z_j + \gamma A_t + \delta Z_j \cdot A_t + \eta X_j + \psi_{s(j)} + \epsilon_{jt}, \tag{3}$$

where P_{jt} denotes the share of peer fathers of 3-5-year-old children taking parental leave between August and December (when the reform bites) in establishment j and year $t = 2014, 2015$ over total fathers of 0-5-year-old children. Z_j is the establishment-level exposure to the reform defined above. A_t is a dummy equal to one for 2015 and zero for 2014. X_j includes establishment-level controls, averaged over the years 2014 and 2015: average female and male earnings, average female and male age, log firm size, share of women, share of workers with temporary contracts, and share of white-collar workers). Finally, we also include dummies for sectors ($\psi_{s(j)}$), broadly corresponding to NACE sections (13 categories). ϵ_{jt} is an error term robust to heteroskedasticity.

The second-stage equation is the following:

$$Y_{j,t+k} = \alpha + \beta \widehat{P_{jt}} + \eta X_j + \psi_{s(j)} + \omega_{jt} \tag{4}$$

where $Y_{j,t+k}$ is the share of coworkers becoming fathers in $t+k$, with $k = \{1, 2, 3, 4\}$, who take parental leave in establishment j at time $t+k$ and who were employed by establishment j at time t. $\widehat{P_{jt}}$ is the parental leave take-up of peer fathers predicted in the first stage in establishment j at time t; X_j and $\psi_{s(j)}$ are defined as above. ω_{jt} is an error term robust to heteroskedasticity. For different lags k, we report the marginal effects of IV-Poisson estimates of β (as suggested in Wooldridge, 2010), which is our parameter of interest.

Under the assumption that the instrument Z_j is relevant and conditionally exogenous (see the discussion in Section 6.2), the parameter β in equation (4) identifies a local average treatment effect. In this context, complier establishments are those that experience an increase in peers' parental leave take-up due to an exogenous higher share of eligible

[9] We drop observations with $Z_j = 1$, i.e., cases in which all fathers in the firms have children between 3 and 5 years old. In other terms, we condition on having at least one non-eligible father in the firm.

[10] For a similar exposure variable on different policy reform and different outcomes, see Ginja et al. (2023).

[11] The choice of a Poisson model is motivated by the skewness of the parental leave take-up at the establishment level.

peer fathers before the reform. Our parameter of interest reflects the weighted average causal response in coworkers' parental leave take-up to a marginal change in the parental leave take-up of peer fathers, driven by the reform (Angrist and Imbens, 1995).

5 Individual-level analysis on program take-up and labor market outcomes

5.1 Descriptive statistics

Table 2 reports descriptive statistics for both fathers and mothers before the reform in June 2014 for control (parents of 0-2-year-old children) and treated (parents of 3-5-year-old children) groups.

The average age in the treated group is higher for both genders, in line with treated parents having older children. This is also reflected in treated parents having higher monthly earnings, likely due to their higher labor market experience thanks to higher age. In addition, treated parents are slightly more likely to be white-collar and to work outside of manufacturing. Finally, while treated fathers are marginally less likely to be on part-time and temporary contracts than control fathers, treated mothers are more likely part-time workers and equally likely to be in temporary jobs than control mothers. Overall, we work with a sample of approximately 3.8 million fathers and 2.9 million mothers.

5.2 Results: program take-up

Figure 1 shows the estimates of the dynamic difference-in-differences specification (equation 1), where the dependent variable is a binary indicator for parental leave take-up at the individual level for fathers (Panel A) and mothers (Panel B). We do not find statistically significant differences in the calendar months before the introduction of the policy between treated and control units in 2015 with respect to 2014, reassuring on the validity of the parallel trend assumption.[12]

The post-reform coefficients are positive and statistically significant for both fathers and mothers, indicating that the reform has an impact on the take-up of parental leave. The increase in take-up between August and December is three times larger for mothers than the corresponding increase for fathers (1.2 versus 0.4 percentage points, respectively). However, the increase relative to the average pre-reform take-up for fathers dominates that for mothers (24.8% versus 16.1%).

[12]The statistically significant coefficient in February for fathers can be explained by the severe influenza epidemic of 2015 relative to 2014 (see Figure A.3).

Table 2: Individual-level descriptive statistics

	Fathers		Mothers	
	Control	Treated	Control	Treated
Age	37.5	40.1	35.0	37.8
	(5.4)	(5.1)	(4.6)	(4.3)
Part-time	0.07	0.06	0.40	0.45
	(0.25)	(0.24)	(0.49)	(0.50)
Temporary	0.06	0.05	0.04	0.04
	(0.24)	(0.22)	(0.19)	(0.20)
Blue-collar	0.53	0.50	0.26	0.24
	(0.50)	(0.50)	(0.44)	(0.42)
White-collar	0.38	0.39	0.67	0.70
	(0.48)	(0.49)	(0.47)	(0.46)
Manufacturing	0.37	0.36	0.24	0.22
	(0.48)	(0.48)	(0.43)	(0.41)
Monthly earnings	2685.8	2907.7	1034.1	1336.4
	(2636.0)	(3117.2)	(1313.1)	(1590.4)
N. workers	2,675,570	1,111,256	1,694,417	1,216,763

Notes. The table reports means and standard deviations in parentheses of fathers' and mothers' characteristics in control (parents of children of 0-2 years old) and treated (parents of children of 3-5 years old) groups in 2014.

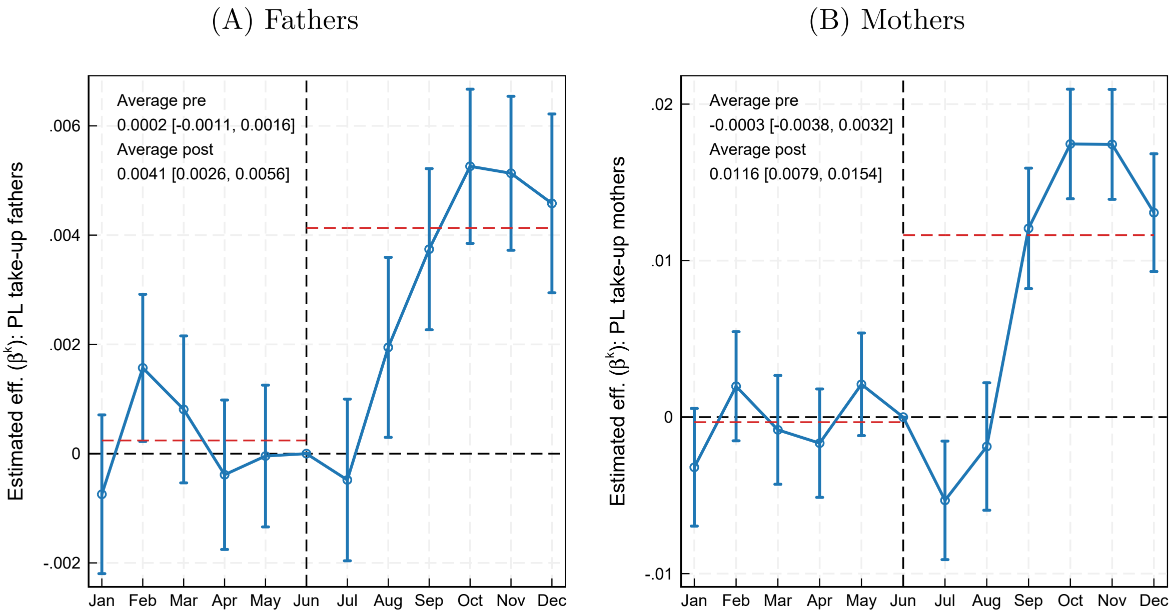
Additionally, heterogeneity analyses reveal that the increased take-up is entirely concentrated among workers with permanent contracts (Figure A.4).

The increase we document is on the extensive margin of whether or not parents take up more parental leave permits. In Figure A.5, we further investigate whether fathers also respond on the intensive margin by increasing the number of days of parental leave take-up. The figure shows that this is not the case: panels A and B show no statistically significant effects on the duration of parental leave or on the share of the household-level total duration of parental leave take-up.

These findings suggest that increasing generosity positively influences the use of parental leave by both parents on the extensive margin, but the burden of family care primarily remains on mothers.

Robustness Figure A.6 presents dynamic difference-in-differences estimates for eligible (3-5-year-old children) and non-eligible (0-2-year-old children) parents. A potential concern is that the triple difference estimates might be influenced by a decline in take-up among non-eligible parents due to temporal shifts—where take-up is postponed following the reform's extension of the replacement rate coverage period. However, the figure shows no evidence of such effects for either fathers (panel A) or mothers (panel B). Instead, it highlights a sharp increase in take-up among the eligible group, while the non-eligible

Figure 1: Parental leave take-up at the individual level



Notes. The figure reports event study coefficients from equation (1) on the triple interaction between the treatment dummy (having a child between 3 and 5 years old), the year dummy (equal to one for 2015) and the calendar month dummies (June used as a reference). The dependent variable is a dummy equal to one if the worker takes parental leave. Panel A reports the estimates for fathers and panel B for mothers. The vertical lines are 95% confidence intervals, obtained from cluster-robust standard errors at the worker level. The horizontal dashed lines are the average coefficients before (January-May) and after (August-December) the introduction of the policy.

group maintains a relatively flat trajectory. In fact, the intertemporal strategic optimization of parental leave is possible only if the take-up limit is non-binding. However, as shown in Figure A.2, parental leave take-up is concentrated in short durations and follows specific seasonal patterns. If take-up is driven by particular needs, such as seasonal illness, the hypothesis of intertemporal optimization becomes less plausible.[13] We further show the effects by single ages of a child in Figure A.7, which reports the dynamic difference-in-differences estimates for parents with children of different ages in June of each year (from 0 to 5 years old). The figure reveals a stark increase in take-up starting in the summer months for both fathers (panel A) and mothers (panel B) of children of 3, 4, and 5 years of age. The figure also shows an increase, especially among fathers, for parents of newborn children (0 years). Such an increase may be related to the introduction, contextual to the policy we study, of hourly parental leave, which may be exploited heterogeneously across different children's ages, and could benefit more parents of newborns. To this end, Figure A.8 reports estimates of equation (1) in which we exclude parents of 0-year-old children. The event study coefficients in the post-policy period are

[13]The lack of temporal shifts in the immediate aftermath of the policy does not rule out the possibility that they may emerge over a longer horizon. Since our analysis focuses on a short period around the policy's introduction, it is possible that individuals have not yet had sufficient time to re-optimize their parental leave decisions.

slightly larger as expected, given we are now discarding a positive effect in the control group, but broadly in line with our main estimates.

Figure A.8 also reports estimates excluding parents of children of 2 years old, as these individuals are in the control group in 2014 and in the treated group in 2015. The exclusion of such parents generates an anticipation in the timing of the treatment effect between April and May when the policy was approved in Parliament. Apart from this jump, the dynamics of coefficients mirror closely that of our main estimates, and the pre-trends are more visible for mothers—who are not the main object of our empirical investigation—than for fathers.

Our results may be subject to attenuation bias, as parents who have previously taken parental leave are identified as eligible. As noted earlier, each parent is entitled to a maximum of six months of leave, or seven months if the father takes at least three months. To address this concern, we estimate equation (1) across three subgroups based on prior household-level parental leave take-up: households that never used it, those that used it for less than 5 months, and those that used it for less than 10 months. Figure A.9 shows that the increase in take-up happens across parents with varying histories of parental leave use in the past. No significant differences are observed between mothers in households that never used parental leave and those in households that previously took it for less than 5 or 10 months. For fathers, the increase is concentrated in households where parental leave was previously used, while the estimated treatment effect is smaller for fathers in couples who never used parental leave.

5.3 Results: labor market outcomes

Table 3 reports the estimates of equation (2) for fathers. Being eligible to the reform does not come with career costs for them. The table shows that there is a marginally significant increase in the probability of employment (by approximately 0.1 percentage points at both 1 and 2-year lag) and in the probability of conversion from temporary to permanent contract (by 0.3 percentage points after one year and 0.1 percentage points after two years). The effects on employment probability in the same establishment, earnings, days, and promotions are all non-significant. In contrast, mothers do have career costs in terms of some of the outcomes that we consider. In particular, Table A.4 shows that after one year, they are less likely to be employed, they earn less and work a lower number of days, they have lower conversion probabilities from temporary to permanent positions, and are less likely to be promoted to managers. However, the effects on employment and days worked reverse after two years, and there is a marginally significant positive effect on the promotion probability from blue- to white-collar occupations. While there is less evidence on the (causal) career consequences of parental leaves for fathers, the more contrasting results for mothers are in line with the analyses and survey of the literature presented in

Table 3: Career outcomes of peer fathers

	(1) Empl.	(2) Same est.	(3) Cumul. earnings	(4) Cumul. days	(5) Temp. to perm.	(6) White-c. to manag.	(7) Blue-c. to manag.	(8) Blue-c. to white-c.
	1-year horizon							
Coefficient	0.11*	-0.02	-68.2	-0.16	0.25***	0.02	-0.001	0.06
	(0.06)	(0.14)	(48.9)	(0.11)	(0.09)	(0.04)	(0.003)	(0.04)
Observations	1,470,854	1,430,596	1,419,046	1,419,046	1,430,596	1,430,596	1,430,596	1,430,596
	2-year horizon							
Coefficient	0.13*	0.03	-85.2	-0.00	0.14*	0.04	-0.002	0.04
	(0.08)	(0.12)	(51.9)	(0.11)	(0.08)	(0.04)	(0.004)	(0.04)
Observations	1,470,854	1,365,026	1,391,853	1,391,853	1,365,026	1,365,026	1,365,026	1,365,026

Notes. The table reports estimates of equation (2). The coefficient reported is the one of the interaction between the treatment dummy (equal to one for fathers of 3-5-year-old children), the period dummy (equal to one for the period August-December), and the year dummy (equal to one for 2015). The top and bottom panels report the effects for $\tau = 1$ and $\tau = 2$, respectively. The dependent variables are: dummy for being employed in column 1; dummy for being employed in the same establishment in column 2; cumulative earnings over the 1 or 2 years in column 3; cumulative days worked over the 1 or 2 years in column 4; and dummies for switching from temporary to permanent contract (column 5), from white-collar to manager (column 6), from blue-collar to manager (column 7), and from blue-collar to white-collar (column 8). Cluster-robust standard errors at the worker level are reported in parentheses. Significance levels: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Olivetti and Petrongolo (2017).

6 Establishment-level analysis on peer effects in parental leave take-up

6.1 Descriptive statistics

Table 4 reports descriptive statistics on establishments included in our sample (column 3), and compares them with the full population of establishments (column 1) and with that of establishments employing at least two workers (column 2). The average share of eligible peer fathers is 9.4 percent, while that of eligible fathers taking parental leave is 0.2 percent. Both variables display a high degree of skewness.

Establishments in the analysis sample have a lower proportion of female employees, are larger, and have higher average age and earnings for both male and female workers. A greater share of these establishments is classified as prosocial, defined by having at least one blood donor between 2012 and 2014. Due to their larger size, the gender pay gap tends to be wider than in the average workplace. Additionally, over two-thirds of the sample is located in Northern Italy, compared to roughly half of establishments in the overall population. Lastly, the sample includes a higher proportion of manufacturing establishments, as well as a greater share of permanent and white-collar workers.

Table 4: Establishment-level descriptive statistics

	(1)		(2)		(3)	
	Full population		At least 2 employees		Analysis sample	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Share fathers 3-5 yrs old	–	–	–	–	0.094	(0.189)
Share fathers 3-5 yrs old taking PL	–	–	–	–	0.002	(0.026)
Share female	0.482	(0.419)	0.456	(0.371)	0.273	(0.232)
Log firm size	1.119	(1.102)	1.659	(0.950)	2.884	(1.229)
Establishment size < 15	0.908	(0.289)	0.864	(0.343)	0.387	(0.487)
Establishment size 15-49	0.071	(0.258)	0.106	(0.308)	0.351	(0.477)
Establishment size ≥ 50	0.020	(0.141)	0.030	(0.171)	0.261	(0.439)
Mean age female	38.7	(9.1)	38.6	(8.4)	40.5	(6.7)
Mean age male	38.9	(9.2)	38.9	(8.4)	40.1	(5.4)
Mean earnings female	12,131	(9,796)	12,777	(10,021)	19,724	(11,086)
Mean earnings male	15,230	(14,722)	15,793	(14,177)	24,609	(15,065)
Temporary	0.193	(0.328)	0.216	(0.312)	0.149	(0.219)
White-collar	0.345	(0.426)	0.331	(0.394)	0.396	(0.358)
Pro-sociality	0.154	(0.361)	0.194	(0.395)	0.554	(0.497)
Gender pay gap $< 0\%$	0.410	(0.492)	0.410	(0.492)	0.291	(0.454)
Gender pay gap 0-20%	0.371	(0.483)	0.371	(0.483)	0.446	(0.497)
Gender pay gap $\geq 20\%$	0.219	(0.413)	0.219	(0.413)	0.262	(0.440)
North	0.488	(0.500)	0.505	(0.500)	0.678	(0.467)
Centre	0.211	(0.408)	0.209	(0.407)	0.185	(0.388)
South and Islands	0.301	(0.459)	0.286	(0.452)	0.137	(0.343)
Manufacturing	0.164	(0.371)	0.190	(0.392)	0.322	(0.467)
Utilities	0.006	(0.077)	0.007	(0.085)	0.017	(0.130)
Construction	0.120	(0.325)	0.121	(0.326)	0.115	(0.319)
Trade & hospitality	0.339	(0.473)	0.343	(0.475)	0.239	(0.426)
Info & communication	0.020	(0.141)	0.021	(0.144)	0.031	(0.174)
Finance & insurance	0.022	(0.145)	0.024	(0.152)	0.032	(0.175)
Prof. & admin services	0.129	(0.336)	0.120	(0.325)	0.095	(0.293)
Public & social services	0.055	(0.228)	0.048	(0.213)	0.030	(0.169)
N. firms	1,842,255		1,242,569		108,017	

Notes. The table reports means and standard deviations (in parentheses) of establishment characteristics in 2014, for the full population of establishments in column (1), for establishments with at least 2 employees in column (2), and for those included in the analysis sample in column (3).

Table 5: Establishment-level first stage

	(1)	(2)	(3)	(4)
Year 2015	0.0013***	0.0014***	0.0014***	0.0013***
	(0.00009)	(0.00011)	(0.00011)	(0.00009)
Sh. eligible peer fathers	0.0245***	0.0216***	0.0212***	
	(0.00093)	(0.00102)	(0.00101)	
Sh. el. * Year 2015	0.0066***	0.0067***	0.0067***	0.0060***
	(0.00146)	(0.00152)	(0.00152)	(0.00126)
Controls	No	Yes	Yes	No
Sector fixed effects	No	No	Yes	No
Establishment fixed effects	No	No	No	Yes
Observations	199,787	169,589	169,589	183,540

Notes. The table reports estimates of equation (3) at the establishment level. "Sh. el." and "Sh. eligible peer fathers" indicate the share of peer fathers of children between 3 and 5 years old on total fathers of children between 0 and 5 years old in the establishment in 2014. The dependent variable is the share of peer fathers of children between 3 and 5 years old on total fathers of children between 0 and 5 years old in the establishment taking parental leave between August and December of either 2014 or 2015. Column 1 does not include additional controls. Column 2 controls for average female and male earnings, average female and male age, log firm size, share of women, share of workers with temporary contracts, and share of white-collar workers. Column 3 includes 13 dummies for macro-sectors (Nace Rev. 2 sections A, B, C, D-E, F, G-I, H, J, K, L, M-N, O-Q, R-U). Column 4 includes establishment fixed effects. Robust standard errors are reported in parentheses. Significance levels: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

6.2 Results

First stage at the establishment level and instrument validity The identification of the effects rests on the assumption that the instrument—the share of eligible peer fathers—is relevant and exogenous, conditional on the covariates included in the regression. We begin by testing the relevance in Table 5, which reports the OLS estimates of the first-stage equation (3). The coefficient of the interaction term represents the change in the take-up of parental leave of peer fathers (those with children aged 3-5) between August and December 2015—when the policy was in place—with respect to the same period in 2014, unaffected by the policy. Column (1) reports the estimates without additional control variables, and it shows that there is a positive and statistically significant 0.7 percentage point effect of the share of eligible peer fathers on their take-up of parental leave in 2015. Columns (2) and (3) include progressively establishment-level controls and sector dummies, leaving the coefficients almost unaltered. The specification in column (3) is our preferred one. As a robustness, column (4) includes establishment fixed effects, which only marginally affect the estimated coefficient. Therefore, the results at the establishment level confirm the relevance of the instrument and the positive effect of the reform on the take-up of parental leave by peer fathers.

Additionally, Figure A.10 provides descriptive evidence in favor of the monotonicity of the instrument. The figure reports the marginal effects of the first stage regression in which the outcome is the share of fathers of 3-5-year-old children taking parental leave on four quantiles of the share of eligible peer fathers. The monotonicity of the instrument implies that larger values of the share of eligible peer fathers should correspond to their larger take-up of parental leave. The figure presents evidence in line with this: larger values of the instrument are associated with a stronger first-stage effect. Specifically, establishments in the highest quantile of exposure record 2.8 percentage points larger effects than those in the lowest one.

Regarding exogeneity, we test whether more exposed establishments have different characteristics from less exposed ones. Formally, in Table A.1 we run a battery of regressions where the instrument is the dependent variable, and we progressively add establishment-level control variables: log average female and male earnings, log average female and male age, log firm size, the female share, the share of workers with fixed-term contracts, the share of white-collar workers, and macro-sector and 2-digit sector fixed effects. The inclusion of additional covariates generally reduces the significance of coefficients. In particular, the instrument is correlated with men's age because eligible peer fathers are older, most likely because they have older children. This correlation, alongside smaller ones with female earnings and age, firm size, and the white-collar share, survives the inclusion of sector dummies, as well. We address these imbalances by including the covariates, alongside the sector dummies, in all regressions.[14]

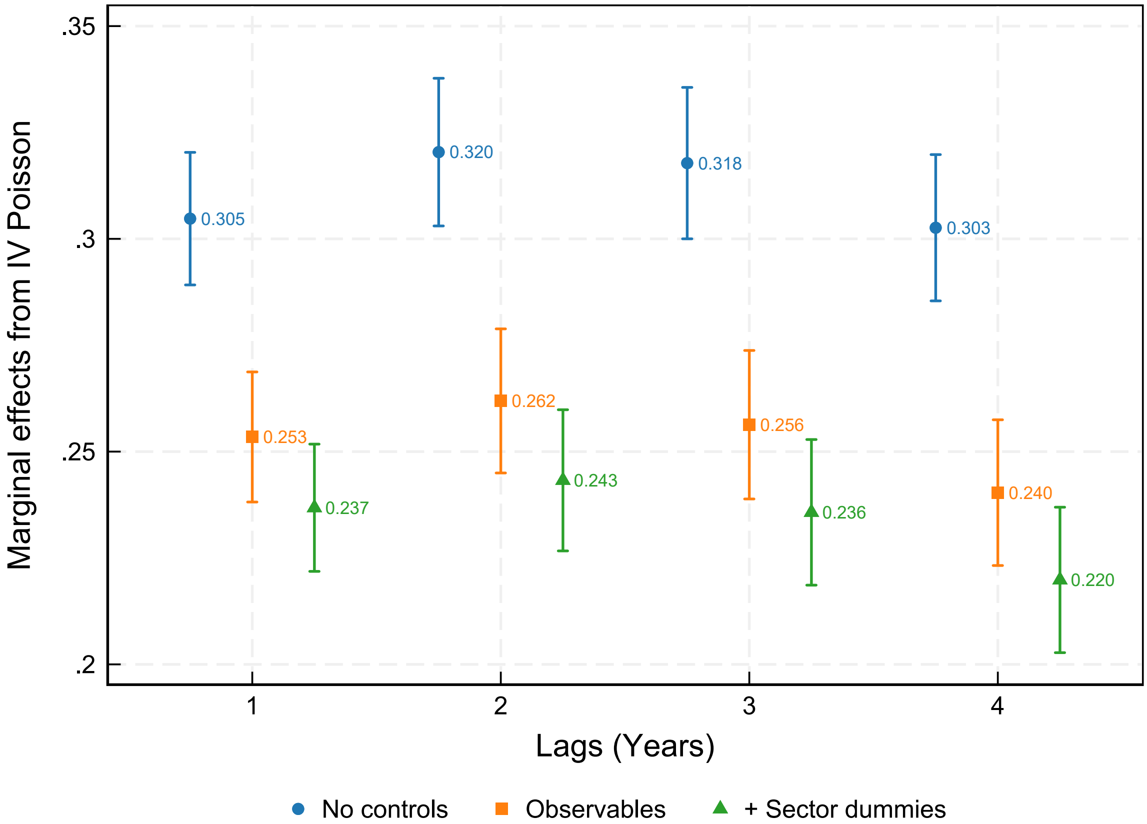
Main estimates of peer effects Figure 2 reports the average marginal effects from equation (4), capturing the peer effects in parental leave take-up exerted by peer fathers on coworker fathers. The local average treatment effects are positive and statistically significant in both unconditional and conditional (on observables and sector dummies) estimates. The coefficients indicate that a 10% increase in the share of peer fathers taking parental leave in a year determines a 2.4% increase in the share of coworker fathers taking parental leave in the following year. While the marginal effects tend to decrease over time, they remain largely stable across different model specifications, and amount to 2.2% after four years.[15]

Overall, our results highlight that an increase in the generosity of parental leave has two effects on fathers. First, the reform directly affects eligible peer fathers, increasing their likelihood of taking parental leave. Second, this increased take-up among peer

[14]Our analysis focuses on workplace peer effects, specifically how coworkers within the same establishment influence parental leave decisions. While broader social networks, such as family relationships, can also impact these decisions (Dahl et al., 2014), we argue that any potential bias from this omission is minimal. This is because the establishment-level exposure to the reform is unlikely to be correlated with the likelihood that family members of coworkers are themselves affected by the reform.

[15]For computational reasons, we include 13 macro-sector dummies in equation (4). However, even allowing for a finer sectoral classification, such as dummies for 2-digit NACE Rev. 2 sectors, yields very similar point estimates (see Figure A.11).

Figure 2: Peer effects: parental leave take-up by coworker fathers

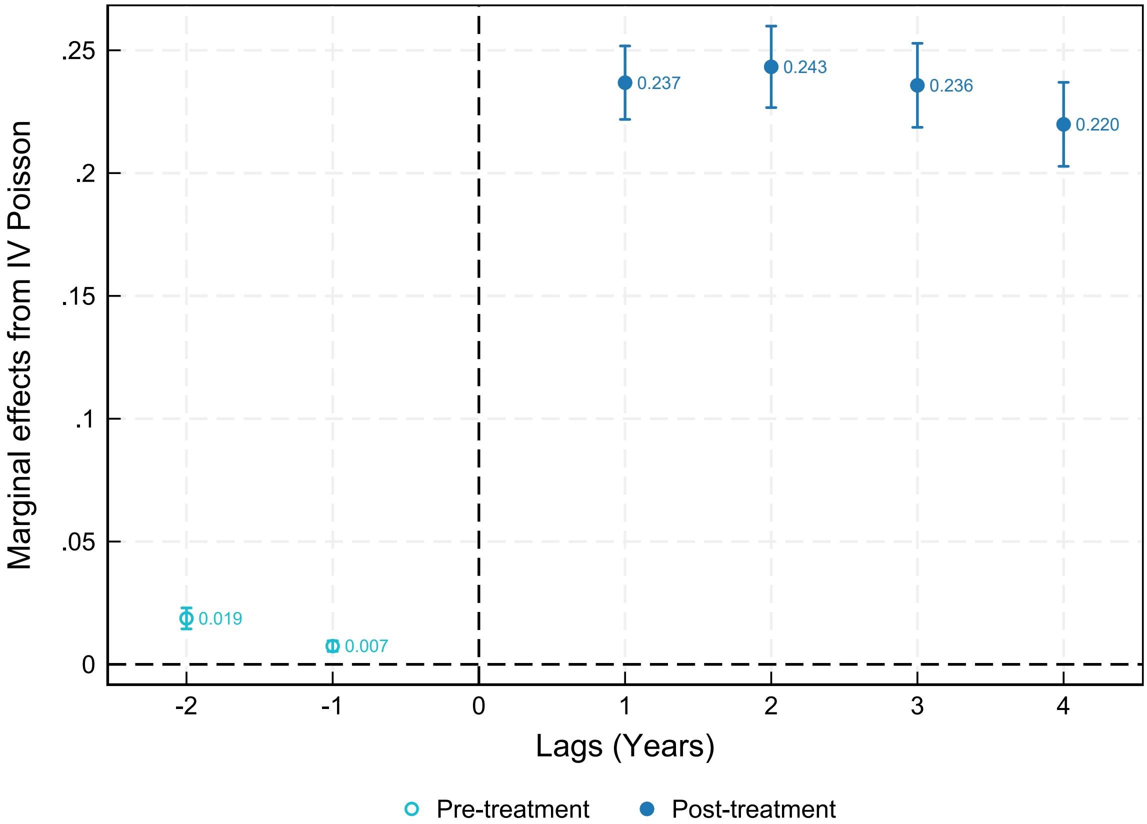


Notes. The figure reports the marginal effects from the IV Poisson estimation of equation (4). The coefficients are those on the share of peer fathers of children between 3 and 5 years old on total fathers of children between 0 and 5 years old in the establishment taking parental leave between August and December of either 2014 or 2015. The dependent variable is the share of coworker fathers taking parental leave at lag 1 to 4. In “No controls” we do not include additional covariates. In “Observables” we control for average female and male earnings, average female and male age, log firm size, share of women, share of workers with temporary contracts, and share of white-collar workers. In “Sector dummies” we include 13 dummies for macro-sectors (Nace Rev. 2 sections A, B, C, D-E, F, G-I, H, J, K, L, M-N, O-Q, R-U). Vertical lines are 95% confidence intervals obtained from robust standard errors.

fathers generates an indirect peer effect on their incumbent coworkers, influencing their likelihood of taking parental leave when they become fathers. From a policy perspective, our analysis underscores the significance of peer effects in evaluating the full range of impacts associated with parental leave programs.

We further estimate peer effects among mothers. The results in Table A.2 indicate a positive and statistically significant first-stage effect, corresponding to an increase of approximately 1.8% in parental leave take-up among peer mothers after the reform. Figure A.12 reports the estimates of peer effects on coworker mothers. In our preferred specification with control variables and macro-sector dummies, the peer effect is of comparable size to that estimated for coworker fathers.

Figure 3: Peer effects: parental leave take-up by coworker fathers and pre-trends



Notes. The figure reports the marginal effects from the IV Poisson estimation of equation (4). The coefficients are those on the share of peer fathers of children between 3 and 5 years old on total fathers of children between 0 and 5 years old in the establishment taking parental leave between August and December of either 2014 or 2015. The dependent variable is the share of coworker fathers taking parental leave at lag 1 to 4 in the post-treatment period and at lag -2 and -1 in the pre-treatment period. All regressions control for observables and macro-sector dummies. Vertical lines are 95% confidence intervals obtained from robust standard errors.

Robustness Figure 3 evaluates whether firms with a higher share of peer fathers taking parental leave were on different trends before the reform-induced increase in take-up rates. Specifically, we run two additional regressions based on equation (4), where the outcome is $Y_{j,t+k}$ with $k = \{-2, -1\}$. In other terms, the outcome in this case is the share of fathers taking parental leave one or two years before the reference year t. These placebo coefficients are statistically significant, but decreasing and very close to zero. A distinct discontinuity between the pre- and post-t periods further supports the validity of our empirical design.

The peer effects may partly reflect coworker fathers increasing their fertility rate. Figure A.13 reports the estimates of equation 4 using the number of new births over the total number of fathers in each establishment as a dependent variable. There is a small positive and statistically significant increase in the establishment-level fertility rate only after one year. The effect then decays and becomes not statistically distinguishable from zero from the second lag onward. Given that we still find a substantial peer effect between the second and the fourth lag, the higher fertility rate is unlikely to play an important role in parental leave take-up. In other terms, we can interpret our effects as mainly stemming from an increased take-up among coworker fathers who would have had children also in the absence of peer effects (i.e., an intensive margin effect) and not from fathers who had children because of peer effects (i.e., an extensive margin effect).

Finally, the peer effects we estimate may reflect local conditions—such as culture or societal values specific to a municipality—rather than factors intrinsic to single establishments. To test this hypothesis, we extend equation (4) by including a variable that captures the parental-leave take-up of fathers with 3-5-year-old children employed in the same municipality as establishment j, but working at different establishments ($j' \neq j$). We instrument this measure using the share of eligible peer fathers in the municipality employed at establishments $j' \neq j$. Figure A.14 presents the estimates from our preferred specification alongside those that account for local factors. The peer effect estimates decrease only slightly and remain statistically significant, providing reassurance that our findings are not merely driven by geographical differences in the intensity of peer effects.

Heterogeneity Figure 4 presents the results of heterogeneity analyses based on establishment characteristics aimed at assessing whether certain employer attributes are associated with stronger peer effects.

Among the dimensions to differentiate establishments, we introduce a novel measure of prosociality or social capital at the establishment level. Specifically, as outlined in Section 3, we classify establishments as prosocial or with high social capital if they had at least one blood donor among their employees in the period 2012-2014 (i.e., prior to the reform). Panel A shows that the peer effect is approximately twice as large in pro-social establishments as in non-pro-social establishments. The stronger peer effect observed in pro-social establishments may stem from either establishment- or worker-specific characteristics. As to the former, employers that allow workers to take leave for blood donations may provide a more favorable environment also for parental leave take-up. As to the latter, workers who give blood donations may signal higher time flexibility, which is then reflected in a higher parental leave take-up. These two channels may also operate at the same time, with employers who provide more favorable environments to their employees being able to attract workers with a higher preference for time flexibility.

Panel B reports heterogeneity by previous use of parental leave in the establishment. Specifically, we classify establishments into three groups based on the parental leave take-up of fathers during the period 2012-2014: (i) "No PL", where no parental leave was taken; (ii) "Low PL", where parental leave was taken, but the total duration was below the median of the distribution; and (iii) "High PL", where the duration of parental leave exceeded the median. We find stronger peer effects in establishments with higher use of parental leave by fathers before the reform, providing further evidence of the relationship between the work environment and the peer effect. Notwithstanding, even in establishments with no past use of parental leave, the peer effect is present and statistically significant.

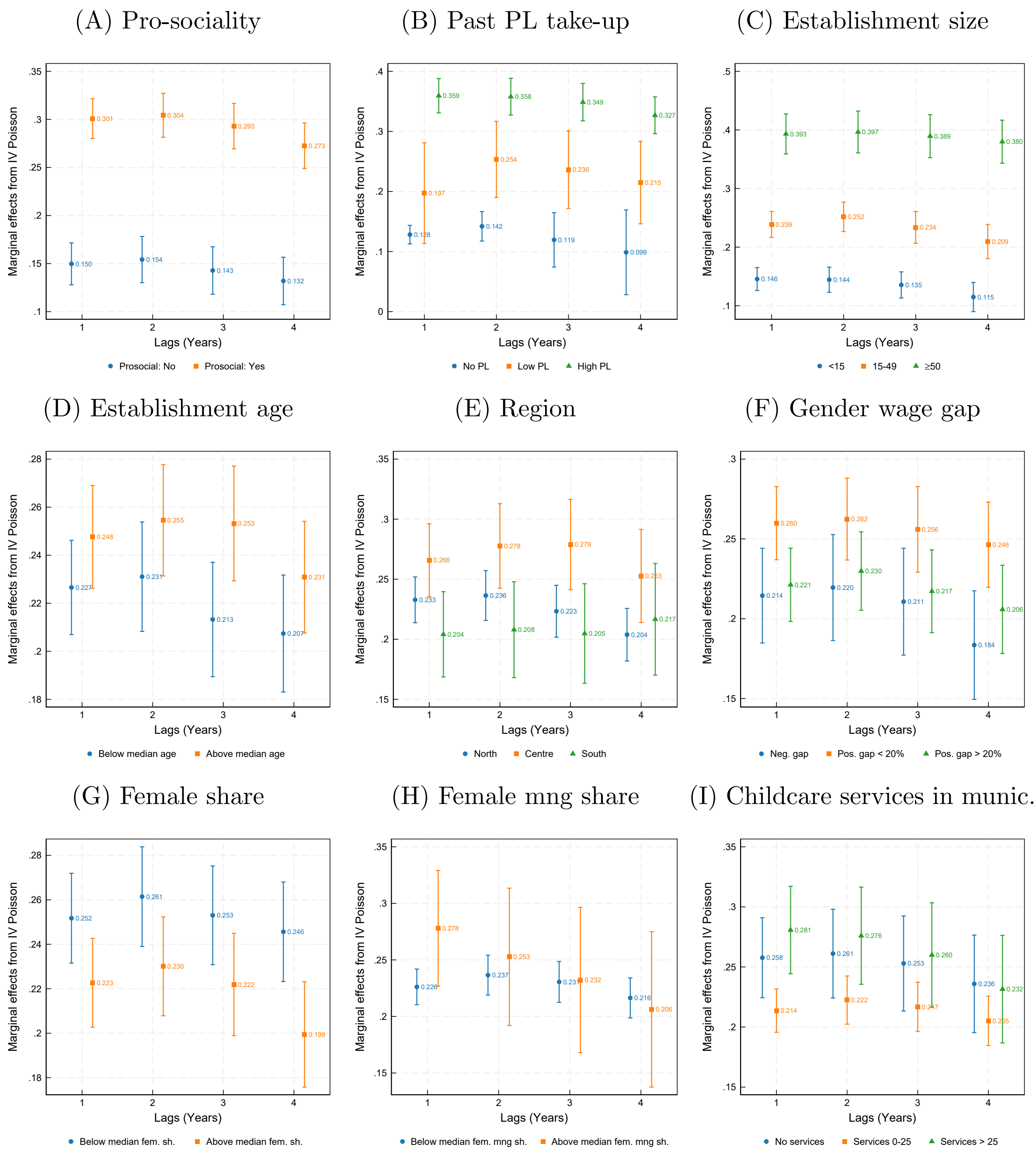
Panel C presents heterogeneity by establishment size, revealing that larger establishments have stronger peer effects. This finding is consistent with the notion that larger firms, with more complex organizational structures and dedicated human resources departments, are better equipped to disseminate information about leave policies, thereby facilitating peer influence. At the same time, larger establishments may find it easier to substitute—both in the internal or external labor market—workers on leave. Finally, in larger establishments, there might be more workers taking parental leave at the same time, reinforcing the peer effect.

There are instead small or null heterogeneous effects based on the establishment's founding date (establishment age; panel D), the region where the establishment is located (panel E), the within-establishment gender pay gap (panel F), the female share in the workforce (panel G) and in management positions (panel H), and the availability of childcare services in the municipality (panel I).

6.3 Magnitudes

Table A.3 quantifies the direct impact of the reform on parental leave take-up and the additional indirect impact induced by peer effects for both fathers and mothers. The table reports in rows a and b the total number of parental leaves taken and the average take-up rate (number of parental leaves divided by the number of parents) in 2014, before the reform. Both the number and the take-up rate of parental leave are considerably larger for mothers than for fathers. Row c reports the individual-level first stage, computed as the average of the triple difference event study coefficients β^k from equation (1) between August and December. Row d rescales the first stage by the average take-up before the reform, showing that in relative terms, the increase for fathers (24.8%) is larger than that for mothers (16.1%). We can, therefore, recover the size of the direct effect of the policy by multiplying the rescaled first stage (d) by the total number of parental leaves before the reform (a). We estimate an increase of approximately 9,400 additional parental leaves among fathers and 41,400 among mothers. We note that the direct policy effect corresponds closely to the observed increase in total leaves taken by fathers between 2014 and 2015 (Figure A.1). The estimated effect for mothers is, instead, much larger than the aggregate change in parental leaves in the same period (approximately 6,000). We point out, however, that the magnitudes computed here refer only to parents of 3-5-year-old children and, therefore, do not capture changes in take-up among parents of children in other age groups.

Figure 4: Heterogeneity in peer effects



Notes. The figure reports the marginal effects from the IV Poisson estimation of equation (4), separately for subgroups in each panel. The coefficients are those on the share of peer fathers of children between 3 and 5 years old on total fathers of children between 0 and 5 years old in the establishment taking parental leave between August and December of either 2014 or 2015. The dependent variable is the share of coworker fathers taking parental leave at lag 1 to 4. All regressions control for observables and macro-sector dummies. Vertical lines are 95% confidence intervals obtained from robust standard errors.

We then compute the additional yearly impact induced by peers by multiplying the direct policy effect (e) by the peer effect estimated at lag 1 (f; see Figure 2 for fathers and Figure A.12 for mothers). This calculation leads to additional 223 parental leaves taken by fathers and 1183 taken by mothers. Given the relative stability of the coefficients, the estimated indirect effects should be multiplied by four to get the overall effects over the lags taken into consideration.

Overall, this back-of-the-envelope calculation reveals that the magnitude of the effects is non-negligible, especially among fathers, given the very low take-up before the reform.

6.4 Mechanisms

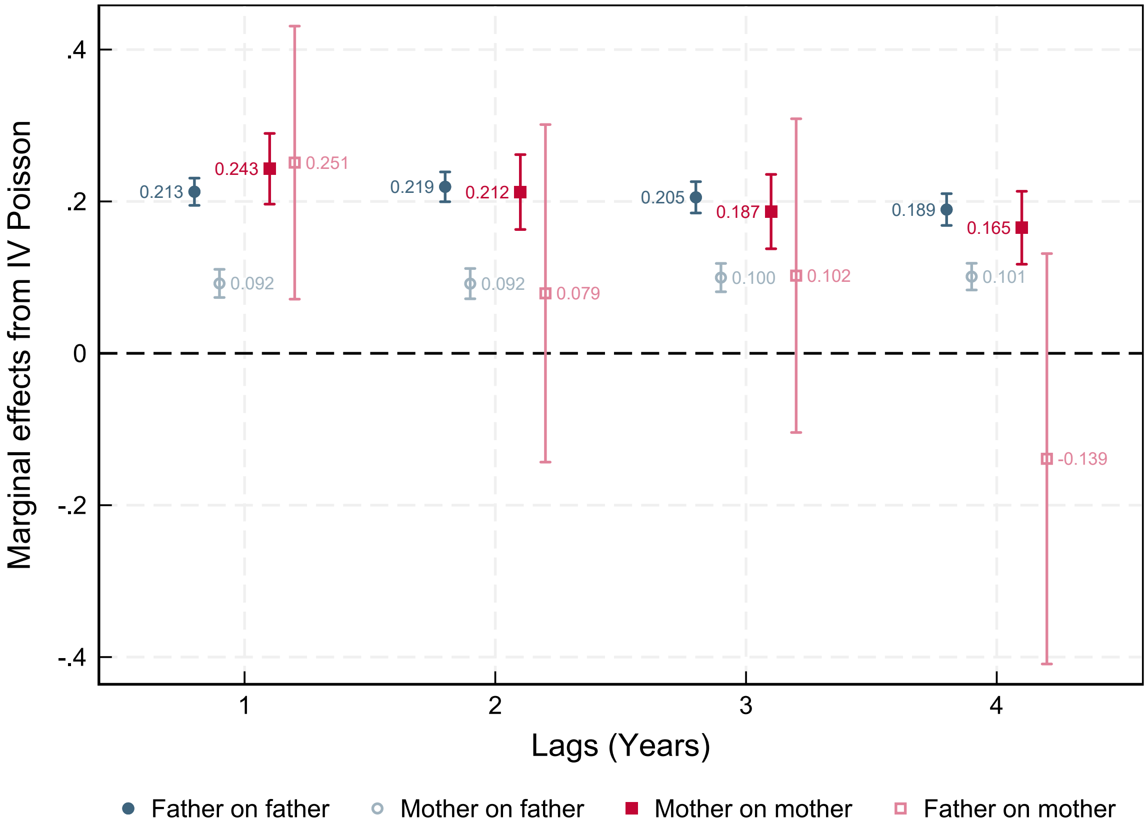
In this section, we explore potential mechanisms underlying the baseline results. We first discuss the role of establishments versus that of colleagues in explaining the peer effect. Second, we explore the role of career concerns in shaping these dynamics.

Peers vs. establishment: own- and cross-gender peer effects The evidence shown so far indicates the presence of positive and statistically significant peer effects among fathers, and the importance of the establishment environment in amplifying these peer effects. One may wonder about the relative role of establishment vs. peers in determining the effect that we document. To address this point, we measure both own-gender and cross-gender peer effects among both fathers and mothers. Our hypothesis is that if peers are relatively more important than establishments, own-gender peer effects should be stronger than cross-gender peer effects. To this end, first, we augment equation (4) with a term capturing parental leave take-up among peer *mothers* (i.e., the share of mothers of children between 3 and 5 years old taking parental leave, instrumented with the share of eligible peer mothers). Second, we estimate this regression—the augmented equation (4)—using as outcome parental leave take-up among coworker mothers, in addition to coworker fathers.

Figure 5 reports the estimates. Focusing on parental leave take-up among coworker fathers, we find that the own-gender (father-to-father) peer effect is roughly twice as large as the cross-gender (mother-to-father) peer effect. Focusing on mothers, we find own-gender (mother-to-mother) effects similar to the ones we estimate for fathers. At lag 1, we also find that cross-gender (father-to-mother) peer effects are not statistically distinguishable from own-gender effects. From lag 2 onward, the cross-gender peer effect becomes smaller (and even negative at lag 4) and statistically not significant, while the own-gender effects remain positive and significant.

This evidence, albeit suggestive, is in line with the hypothesis that the effect of peers matters, on top of that of the establishment.

Figure 5: Own-gender and cross-gender peer effects: parental leave take-up by coworker fathers and coworker mothers



Notes. The figure reports the marginal effects from the IV Poisson estimation of the augmented equation (4) described in Section 6.4. The coefficients are those on the share of peer fathers (mothers) of children between 3 and 5 years old on total fathers (mothers) of children between 0 and 5 years old in the establishment taking parental leave between August and December of either 2014 or 2015. The dependent variable in blue dots (red squares) is the share of coworker fathers (mothers) taking parental leave at lag 1 to 4. All regressions control for observables and macro-sector dummies, as described in Section 4.2. Vertical lines are 95% confidence intervals obtained from robust standard errors.

Career costs Having shown that peers are important in determining the effects that we document, we explore the role of career gains or costs associated with parental leave take-up as a potential explanation for peer effects. On the one hand, workers may be reluctant to take leave due to concerns about potential negative impacts on their career trajectories. On the other hand, if no such costs are observed on peer parents, coworkers may be more inclined to take leave themselves. In other words, the careers of peer parents taking parental leave could serve as a signaling mechanism, influencing coworkers' perceptions of the career implications of leave.

Evidence in section 5.3 points to the absence of such career costs for peer fathers in a reduced form setting and shows that eligible peer fathers, who are proven to respond to the reform by increasing their use of parental leave, experience career trajectories that are either similar to or slightly better than those of non-eligible peer fathers (Table 3).

This lack of perceived career penalties associated with taking leave may help explain the observed increase in parental leave take-up among coworker fathers.

For mothers, the findings on career implications are more ambiguous. If considerations about career trajectories are relevant, coworker mothers may place more weight on the fact that peer mothers are not penalized in terms of employment probability, rather than on the earnings loss that they face (columns 1 and 3 of Table A.4). At the same time, we cannot rule out explanations related to social norms that induce mothers to take leave, irrespective of labor market considerations.

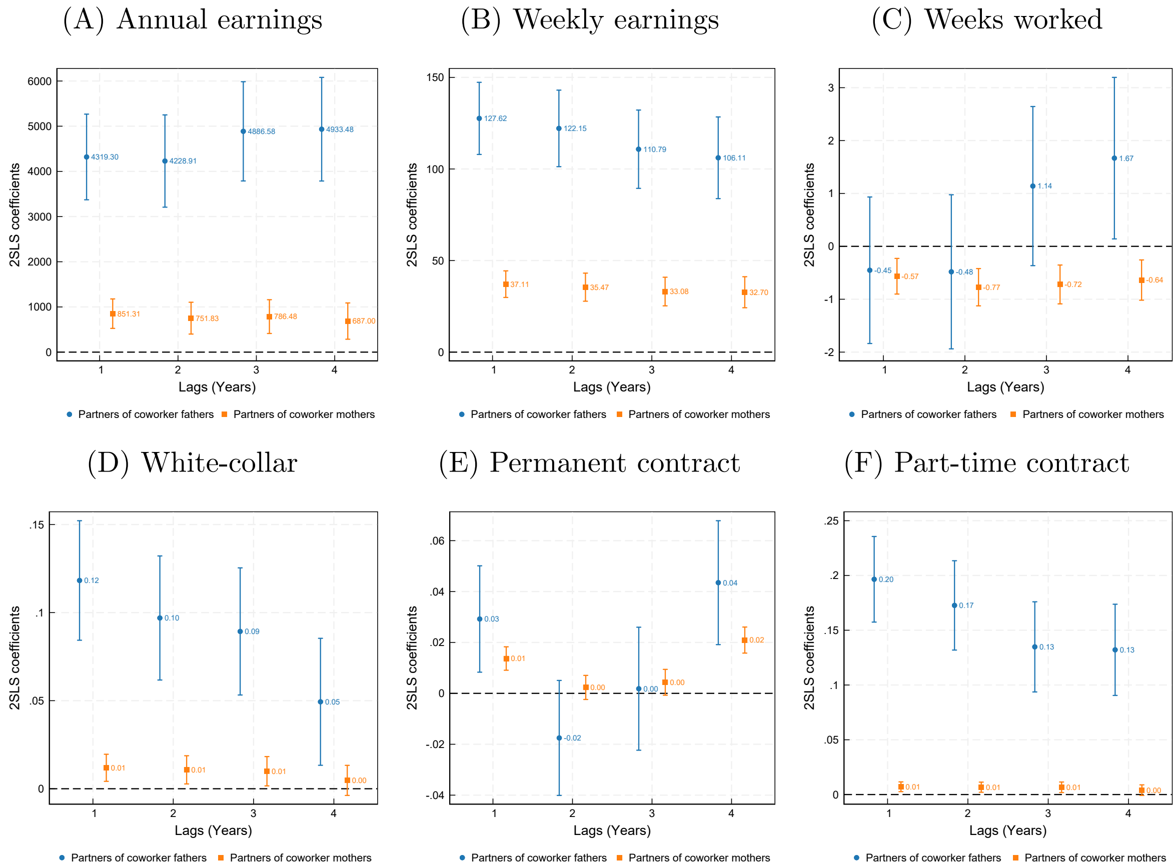
7 Indirect effects on partners of coworkers

In this section, we investigate whether the increase in parental leave take-up among parents exposed to the reform is associated with improvements in the labor market performance of their coworkers' partners. We separately examine the outcomes of the partners of both coworker fathers and mothers, employing a two-stage least squares approach to estimate equation (4), with dependent variables including earnings, labor supply, and occupational outcomes. This analysis brings novel evidence on the network effects of policy take-up on partners of coworker parents. The unique setting of our study enables to analyze the responses of both male and female partners.

Figure 6 reports the results, with the coefficients capturing the effect of a 10% increase in peer parents taking parental leave on the labor market outcomes of their coworkers' partners. The figure reveals substantial heterogeneity between female and male partners. Across all outcomes considered, the responses of female partners consistently exceed those of male partners, providing suggestive evidence that increasing paternal time at home positively influences female involvement in the labor market, whereas the reverse effect (from mothers to male partners) appears less pronounced. Panel A shows an increase in annual earnings between 4,000 and 5,000 euros over the four-year period, mostly driven by higher weekly earnings (panel B) and a greater number of weeks worked in later periods (panel C). The rise in weekly earnings is consistent with an increased probability of holding a white-collar occupation (panel D) and securing a permanent contract (at least in some of the periods examined, panel E). However, the better employment prospects are largely concentrated in part-time positions (panel F). For male partners, we observe more subdued, yet still positive, responses in terms of earnings as well as the probability of holding white-collar occupations and permanent contracts.

Using a quasi-natural experiment, our results complement the literature on the child penalty, which, through various methodologies and in different contexts, has documented a lower penalty for mothers in couples where the father has greater occupational flexibility (Bang, 2021).

Figure 6: Effect of a 10% increase in the share of peer fathers (mothers) taking parental leave on spouses of coworker fathers (mothers)



Notes. The figure reports 2SLS estimates of equation (4), as described in Section 7. The coefficients are those on the share of peer fathers of children between 3 and 5 years old on total fathers of children between 0 and 5 years old in the establishment taking parental leave between August and December of either 2014 or 2015. The dependent variables are establishment-level averages of outcomes of coworker partners. All regressions control for observables and macro-sector dummies. Vertical lines are 95% confidence intervals obtained from robust standard errors.

8 Conclusions

This study investigates the role of workplace peer effects in shaping fathers' take-up of parental leave, leveraging a reform of the Italian parental leave system. By analyzing administrative data that links employees, their workplaces, and their households, we identify and quantify the influence of peer fathers on the adoption of parental leave policies.

First, we show that the reform, which increased the generosity of parental leave, had a direct impact on the take-up rates among eligible peer fathers and mothers. Fathers, in particular, experienced a 24.8% increase in parental leave take-up, which, while smaller in absolute terms compared to mothers, represents a relatively larger proportional increase given their historically low take-up rates.

Second, in an instrumental variable approach exploiting the exposure of establishments to the parental leave reform, we document the presence of peer effects on coworker fathers, persisting for up to four years post-reform. Peer effects are stronger in larger establishments, those with higher levels of social capital, and those with a history of parental leave utilization.

When exploring the role of peers and establishment characteristics in influencing our findings, we find that own-gender peer effects are more pronounced than cross-gender ones, reflecting the importance of workplace networks in determining parental leave adoption. In addition, our analysis finds no evidence of adverse career impacts for fathers taking parental leave, which likely alleviates concerns among coworkers about potential professional repercussions and provides an explanation for peer effects.

Last, the spillover effects extend beyond the workplace, as the increased take-up of leave by peer fathers positively influences the labor market outcomes of their coworkers' partners. Partners of coworker fathers experience significant improvements in earnings, employment stability, and occupational advancement, while the effects on partners of coworker mothers are less pronounced. These results suggest that paternal leave adoption can thus contribute to reducing intra-household gender inequalities.

Overall, we show that workplace dynamics through peers can amplify the impact of parental leave policies, as the visibility of parental leave take-up within establishments helps normalize its use among fathers. Differently from the existing literature, our setting allows us to show that—albeit both being present—own-gender peer effects are more important than cross-gender ones. This supports that interactions within gender are more relevant to peer influences. Our findings emphasize that, in designing leave policies, the leverage of social dynamics in workplace behaviors can promote changes in program participation and trickle down to households.

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Online Appendix to
Leave and Let Leave: Workplace Peer Effects in Fathers' Take-up of Parental Leave

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Additional Tables and Figures

Table A.1: Regression of instrument on observables and sector fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Log avg earnings women	0.028***	0.015***	0.010***	0.010***	0.006***	0.007***	0.008***	0.004*	0.005*	0.005**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Log avg earnings men		0.022***	0.021***	0.000	0.002	0.002	0.005	-0.010***	-0.005	-0.004
		(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Log avg age women			0.079***	0.006	0.008	0.007	0.009	0.014*	0.016**	0.015*
			(0.007)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Log avg age men				0.296***	0.281***	0.290***	0.293***	0.285***	0.276***	0.278***
				(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.011)
Log firm size					0.010***	0.010***	0.009***	0.011***	0.010***	0.009***
					(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Female share						0.042***	0.041***	0.010*	0.001	0.002
						(0.005)	(0.005)	(0.006)	(0.006)	(0.007)
Fixed-term share							0.023***	0.019***	0.014**	0.010
							(0.007)	(0.007)	(0.007)	(0.008)
White-collar share								0.058***	0.044***	0.042***
								(0.004)	(0.005)	(0.005)
Constant	-0.010	-0.104***	-0.332***	-0.942***	-0.918***	-0.953***	-1.016***	-0.846***	-0.859***	-0.872***
	(0.016)	(0.020)	(0.028)	(0.034)	(0.034)	(0.035)	(0.039)	(0.041)	(0.043)	(0.044)
Macro-sector FE	No	No	No	No	No	No	No	No	Yes	No
2-digit sector FE	No	No	No	No	No	No	No	No	No	Yes
Observations	111008	111008	111008	111008	111008	111008	111008	111008	111008	111006

Notes. The Table reports the coefficients of an OLS regression at the establishment-level where the dependent variable is Z_j introduced in equation (3), i.e., the share of peer fathers of children between 3 and 5 years old on total fathers of children between 0 and 5 years old in the establishment in 2014. Each column includes an additional control variable. Macro-sector dummies are for 13 sector groups (Nace Rev. 2 sections A, B, C, D-E, F, G-I, H, J, K, L, M-N, O-Q, R-U). Robust standard errors are reported in parentheses. Significance levels: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.2: Establishment-level first stage among mothers

	(1)	(2)	(3)	(4)
Year 2015	0.0076***	0.0082***	0.0082***	0.0076***
	(0.00019)	(0.00023)	(0.00023)	(0.00019)
Sh. eligible peer mothers	0.1126***	0.1032***	0.1025***	
	(0.00182)	(0.00204)	(0.00204)	
Sh. el. * Year 2015	0.0191***	0.0185***	0.0184***	0.0189***
	(0.00283)	(0.00298)	(0.00298)	(0.00258)
Controls	No	Yes	Yes	No
Sector fixed effects	No	No	Yes	No
Establishment fixed effects	No	No	No	Yes
Observations	237,903	187,989	187,989	217,044

Notes. The table reports estimates of equation (3) at the establishment level for mothers. "Sh. el." and "Sh. eligible peer mothers" indicate the share of peer mothers of children between 3 and 5 years old on total mothers of children between 0 and 5 years old in the establishment in 2014. The dependent variable is the share of peer mothers of children between 3 and 5 years old on total mothers of children between 0 and 5 years old in the establishment taking parental leave between August and December of either 2014 or 2015. Column 1 does not include additional controls. Column 2 controls for average female and male earnings, average female and male age, log firm size, share of women, share of workers with temporary contracts, and share of white-collar workers. Column 3 includes 13 dummies for macro-sectors (Nace Rev. 2 sections A, B, C, D-E, F, G-I, H, J, K, L, M-N, O-Q, R-U). Column 4 includes establishment fixed effects. Robust standard errors are reported in parentheses. Significance levels: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.3: Magnitudes of the effects

		Fathers	Mothers
a	Total parental leaves pre (N)	37,855	256,652
b	Parental leave take-up rate pre (%)	1.5	7.5
c	Individual-level first stage ($p.p.$)	0.4	1.2
		[0.3, 0.6]	[0.8, 1.5]
$d = c/b$	Rescaled first stage (%)	27.5	15.4
		[17.4, 37.5]	[10.5, 20.4]
$e = d * a$	**Direct policy effect** (N)	**10,394**	**39,538**
		[6581, 14,207]	**[26,822, 52,255]**
f	Peer effect lag 1 (%)	2.3	2.9
		[2.2, 2.5]	[2.5, 3.2]
$g = f * e$	**Indirect policy effect through peers** (N)	**242**	**1130**
		[144, 353]	**[662, 1696]**

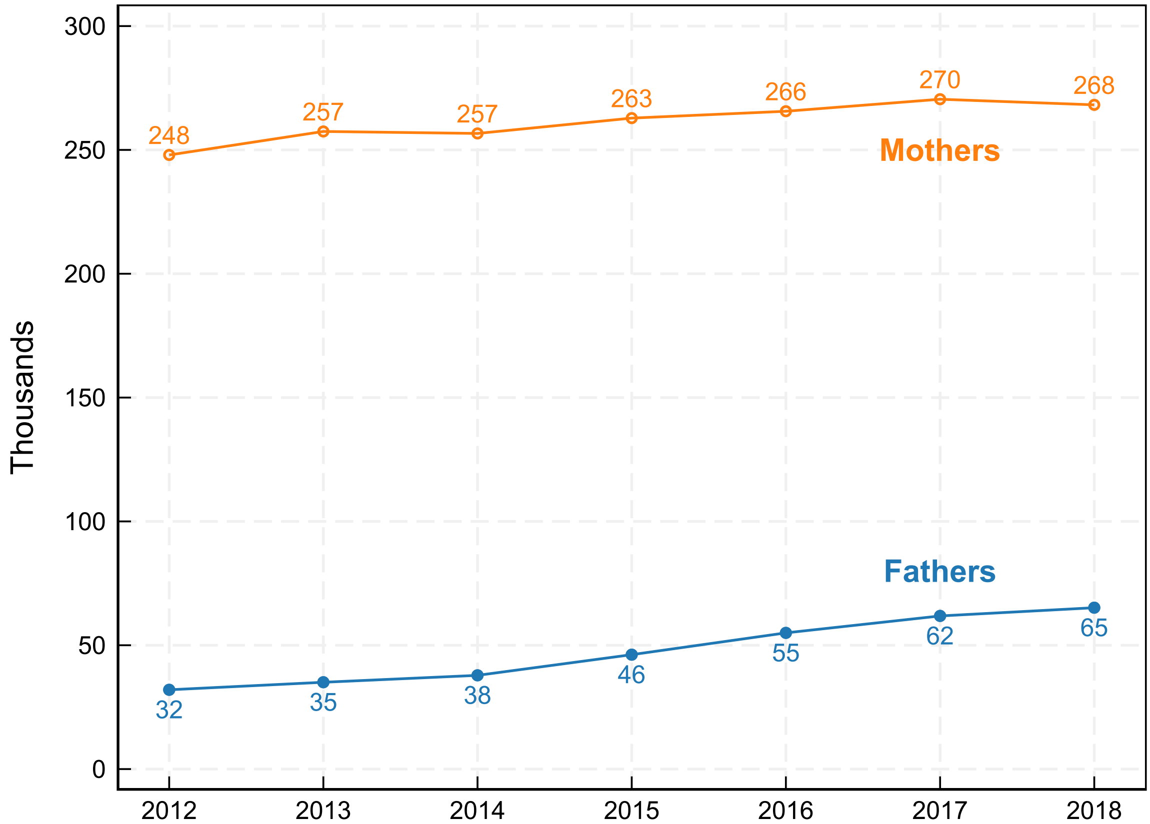
Notes. The table reports a back-of-the-envelope calculation of the magnitudes of the effects reported in the text for fathers and mothers. Row a is taken from Figure A.1 and refers to 2014. Row b is the average take-up rate in the control group (parents of 0-2-year-old children) in 2014. Row c is the average of event study coefficients in Figure 1 between August and December. Row f is the marginal effect of the IV Poisson at lag 1 for the estimate with control variables and sector dummies in Figure 2 for fathers and Figure A.12 for mothers.

Table A.4: Career outcomes for peer mothers

	(1) Empl.	(2) Same est.	(3) Cumul. earnings	(4) Cumul. days	(5) Temp. to perm.	(6) White-c. to manag.	(7) Blue-c. to manag.	(8) Blue-c. to white-c.
	1-year horizon							
Coefficient	-0.21**	-0.04	-258.2***	-0.57**	-0.26***	-0.11***	-0.000	0.07
	(0.09)	(0.15)	(33.8)	(0.22)	(0.09)	(0.03)	(0.002)	(0.04)
Observations	1,470,855	1,016,309	1,004,357	1,004,357	1,016,309	1,016,309	1,016,309	1,016,309
	2-year horizon							
Coefficient	1.02***	-0.14	-179.5***	0.56**	-0.14*	-0.07**	0.002	0.08*
	(0.10)	(0.14)	(36.2)	(0.22)	(0.08)	(0.03)	(0.002)	(0.04)
Observations	1,470,855	929,518	976,844	976,844	929,518	929,518	929,518	929,518

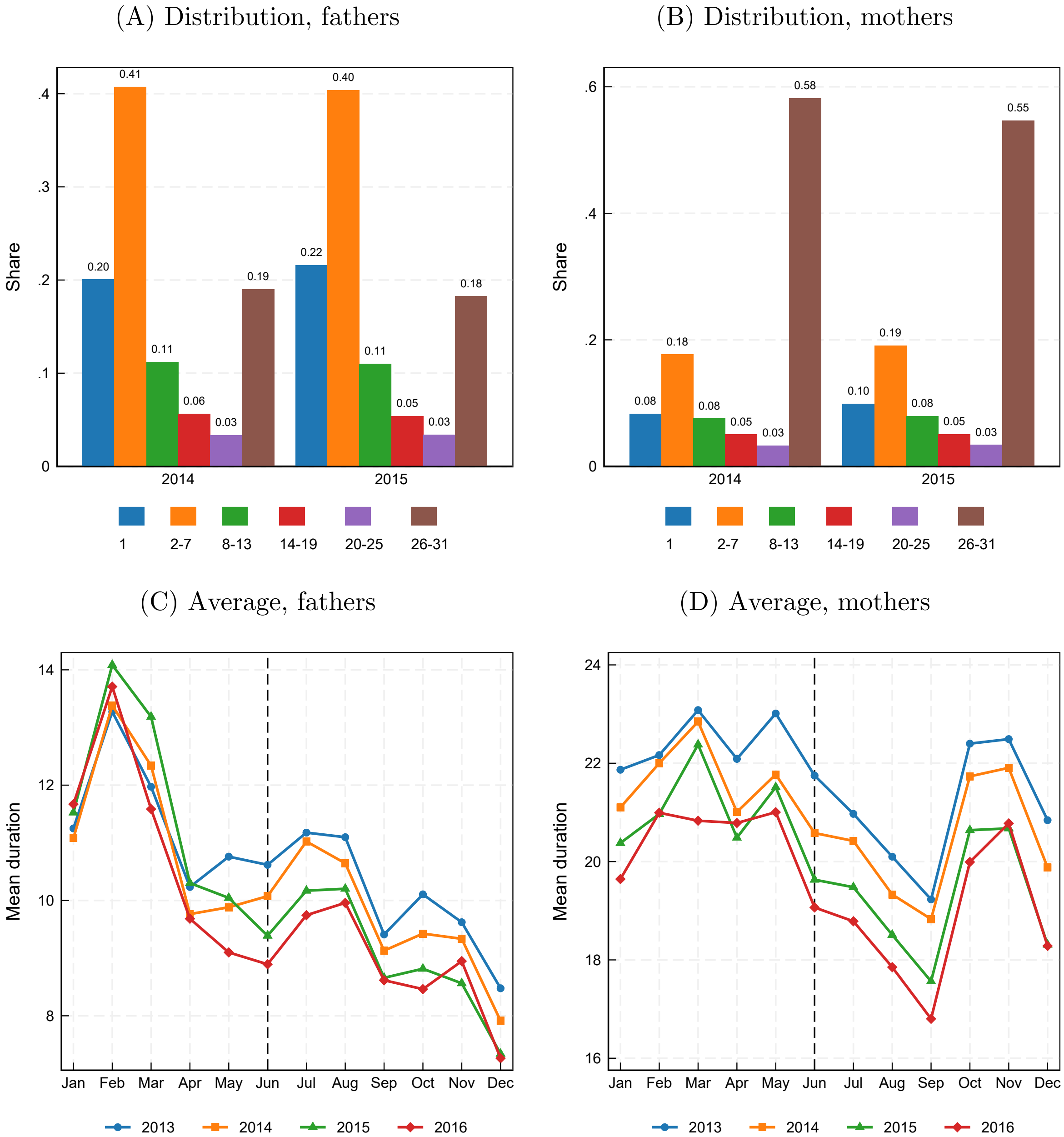
Notes. The table reports estimates of equation (2) for peer mothers. The coefficient reported is the one of the interaction between the treatment dummy (equal to one for mothers of 3-5-year-old children), the period dummy (equal to one for the period August-December), and the year dummy (equal to one for 2015). The top and bottom panels report the effects for $\tau = 1$ and $\tau = 2$, respectively. The dependent variables are: dummy for being employed in column 1; dummy for being employed in the same establishment in column 2; cumulative earnings over the 1 or 2 years in column 3; cumulative days worked over the 1 or 2 years in column 4; and dummies for switching from temporary to permanent contract (column 5), from white-collar to manager (column 6), from blue-collar to manager (column 7), and from blue-collar to white-collar (column 8). Cluster-robust standard errors at the worker level are reported in parentheses. Significance levels: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure A.1: Number of parental leaves requested between 2012 and 2018



Notes. The figure reports the number of parental leave episodes requested in INPS data between 2012 and 2018.

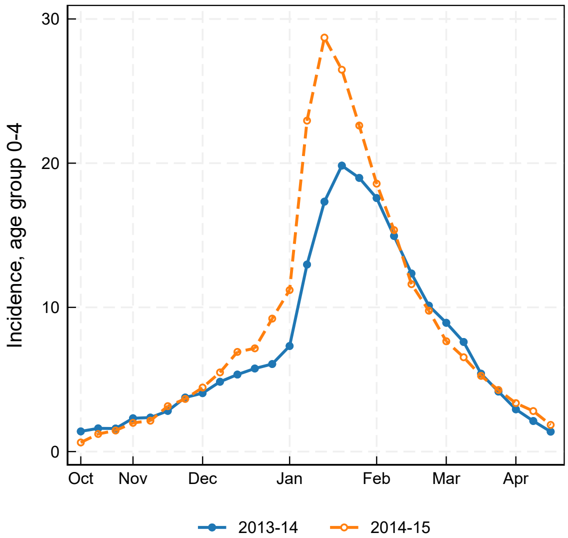
Figure A.2: Parental leave duration in days



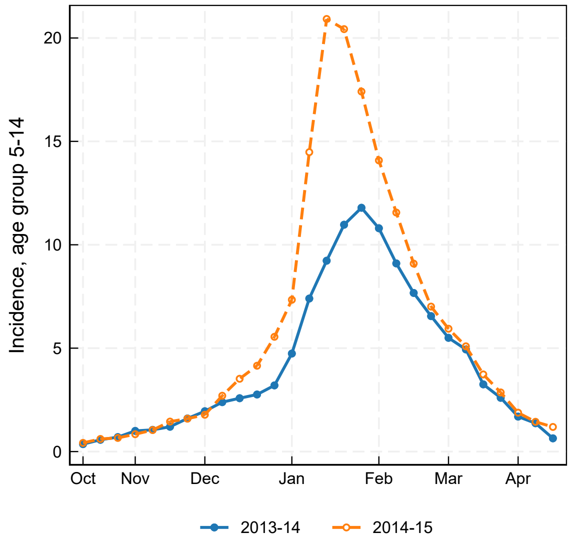
Notes. The figure reports the distribution of parental leaves by duration in days for fathers and mothers in panels A and B. It reports the average duration between 2013 and 2016 by calendar month for fathers and mothers in panels C and D.

Figure A.3: Weekly cases of influenza in 2014 and 2015

(A) Age group 0-4



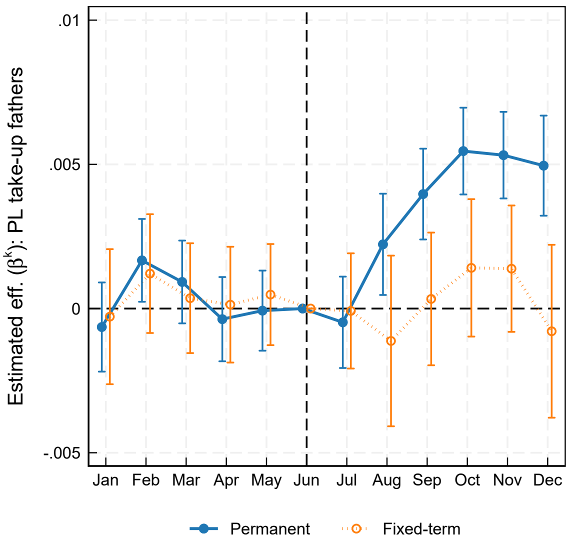
(B) Age group 5-14



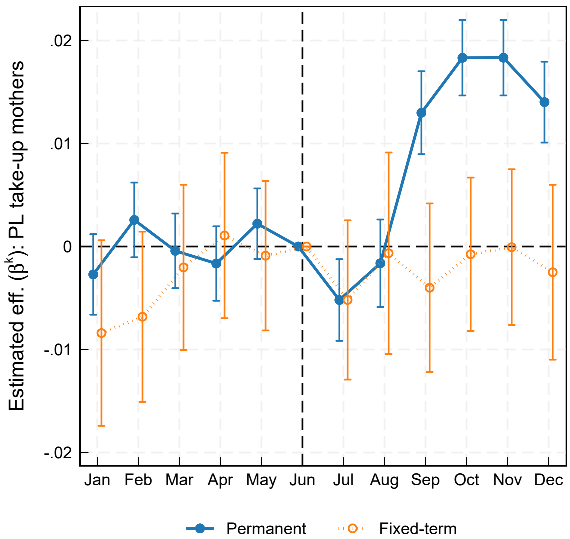
Notes. The figure reports the incidence of influenza cases in the season 2013-14 and 2014-15 by week. The data are sourced from RespiVirNet, the integrated surveillance of respiratory viruses of the Italian National Institute of Health.

Figure A.4: Parental leave take-up at the individual level, by contract type

(A) Fathers



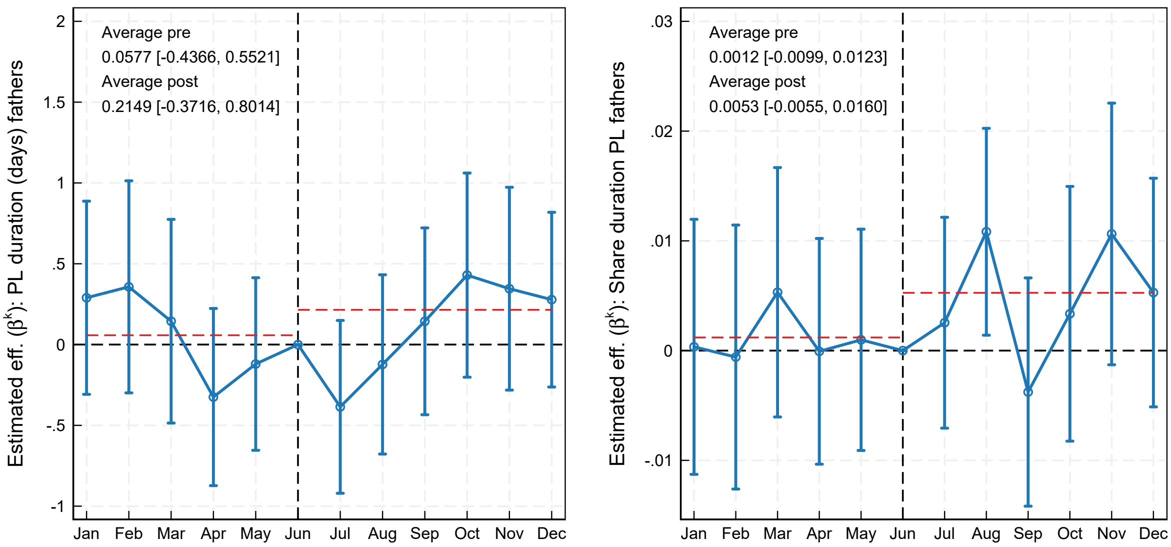
(B) Mothers



Notes. The figure reports event study coefficients from equation (1) on the triple interaction between the treatment dummy (having a child between 3 and 5 years old), the year dummy (equal to one for 2015) and the calendar month dummies (June used as a reference), separately for workers holding permanent and fixed-term contracts in June. The dependent variable is a dummy equal to one if the worker takes parental leave. Panel A reports the estimates for fathers and panel B for mothers. The vertical lines are 95% confidence intervals obtained from cluster-robust standard errors at the worker level.

Figure A.5: Parental leave duration at the individual level, fathers

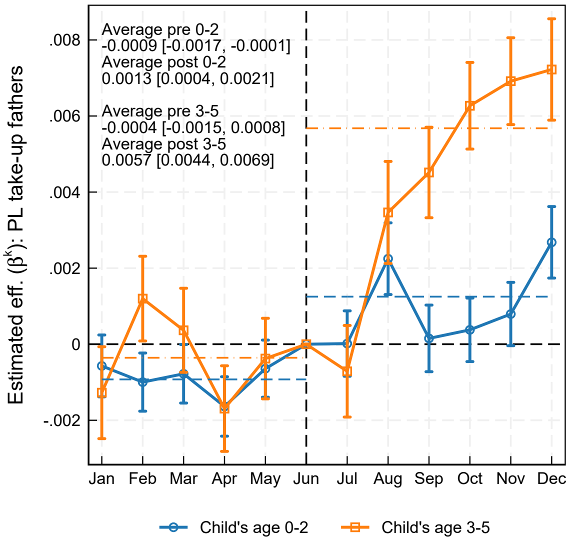
(A) Duration PL (B) Share of household PL duration



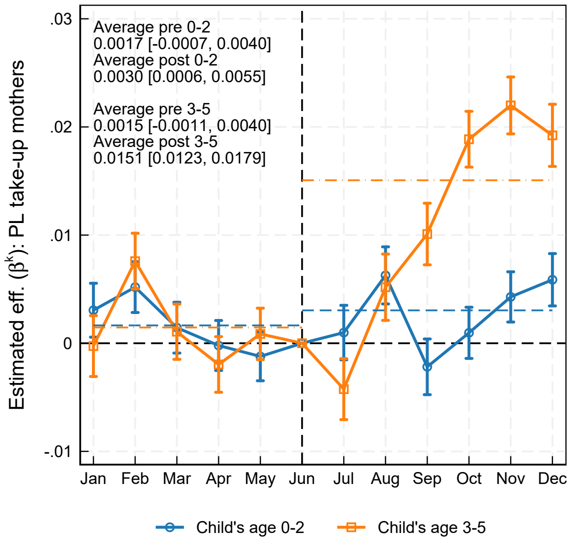
Notes. The figure reports event study coefficients from equation (1) on the triple interaction between the treatment dummy (having a child between 3 and 5 years old), the year dummy (equal to one for 2015) and the calendar month dummies (June used as a reference). The dependent variable is the duration in days of parental leave in Panel A for fathers and the fathers' share of total household duration of parental leave in panel B (i.e., duration in days for fathers divided by duration in days for the household). The vertical lines are 95% confidence intervals obtained from cluster-robust standard errors at the worker level. The horizontal dashed lines are the average coefficients before (January-May) and after (August-December) the introduction of the policy.

Figure A.6: Parental leave take-up at the individual level, by age group of child

(A) Fathers



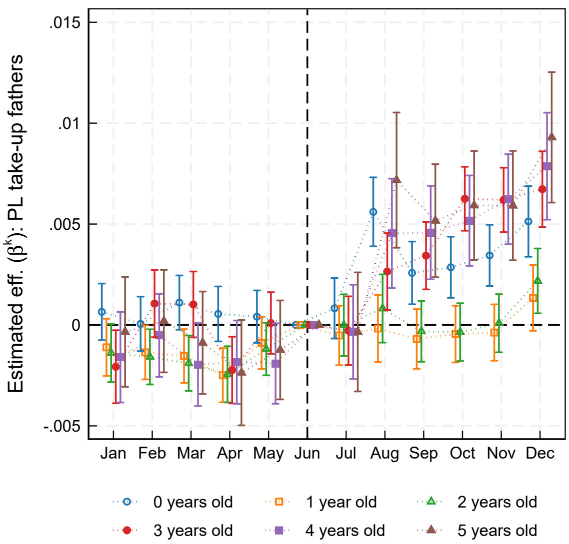
(B) Mothers



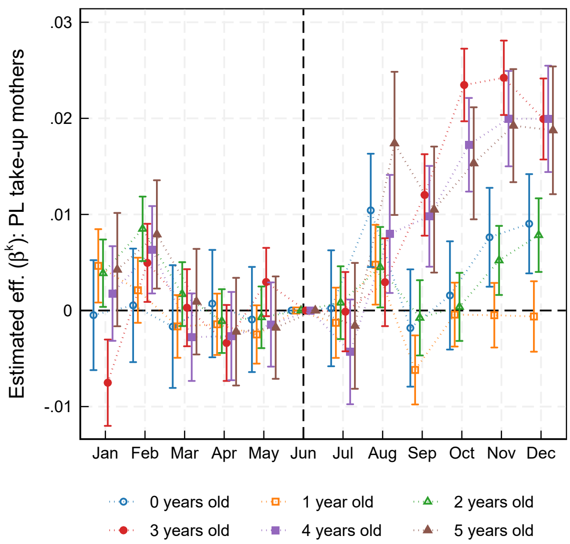
Notes. The figure reports event study coefficients from a difference-in-difference specification on the year dummy (equal to one for 2015) and the calendar month dummies (June used as a reference), separately for workers with children in different age groups in June of each year. The dependent variable is a dummy equal to one if the worker takes parental leave. Panel A reports the estimates for fathers, panel B for mothers. The vertical lines are 95% confidence intervals obtained from cluster-robust standard errors at the worker level.

Figure A.7: Parental leave take-up at the individual level, by age of child

(A) Fathers

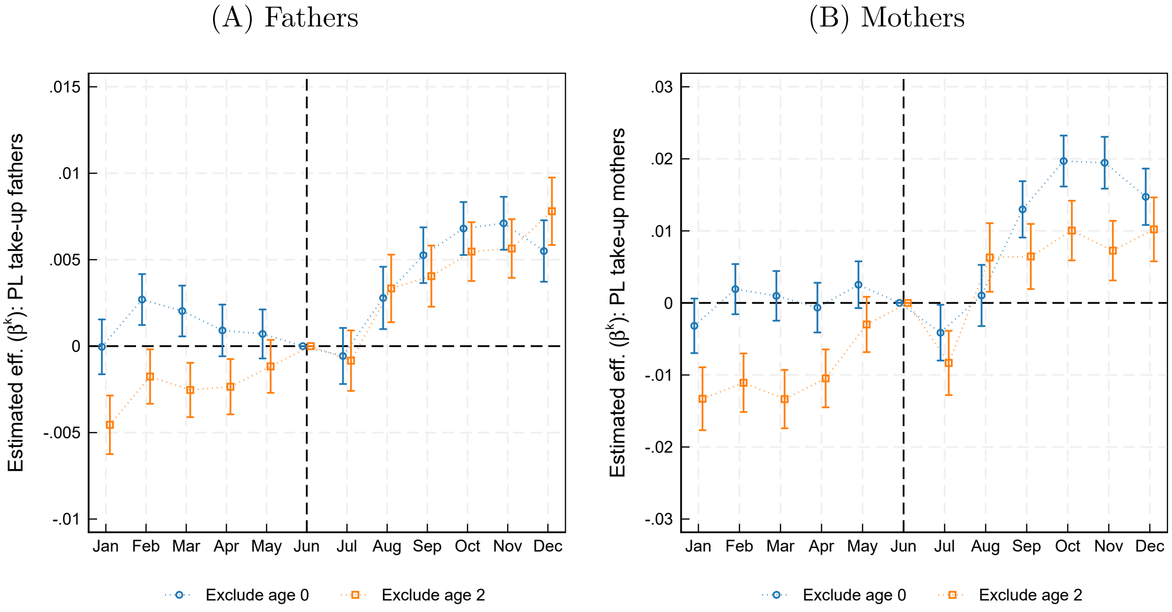


(B) Mothers



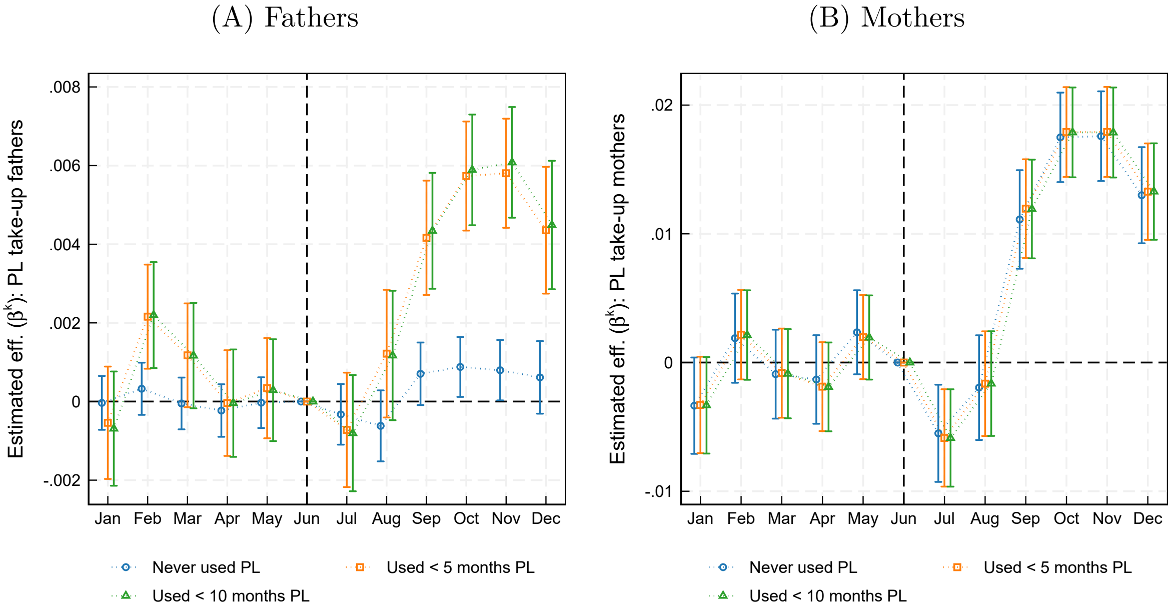
Notes. The figure reports event study coefficients from a difference-in-difference specification on the year dummy (equal to one for 2015) and the calendar month dummies (June used as a reference), separately for workers with children of different ages in June of each year. The dependent variable is a dummy equal to one if the worker takes parental leave. Panel A reports the estimates for fathers and panel B for mothers. The vertical lines are 95% confidence intervals obtained from cluster-robust standard errors at the worker level.

Figure A.8: Parental leave take-up at the individual level, excluding children of 0 or 2 years old



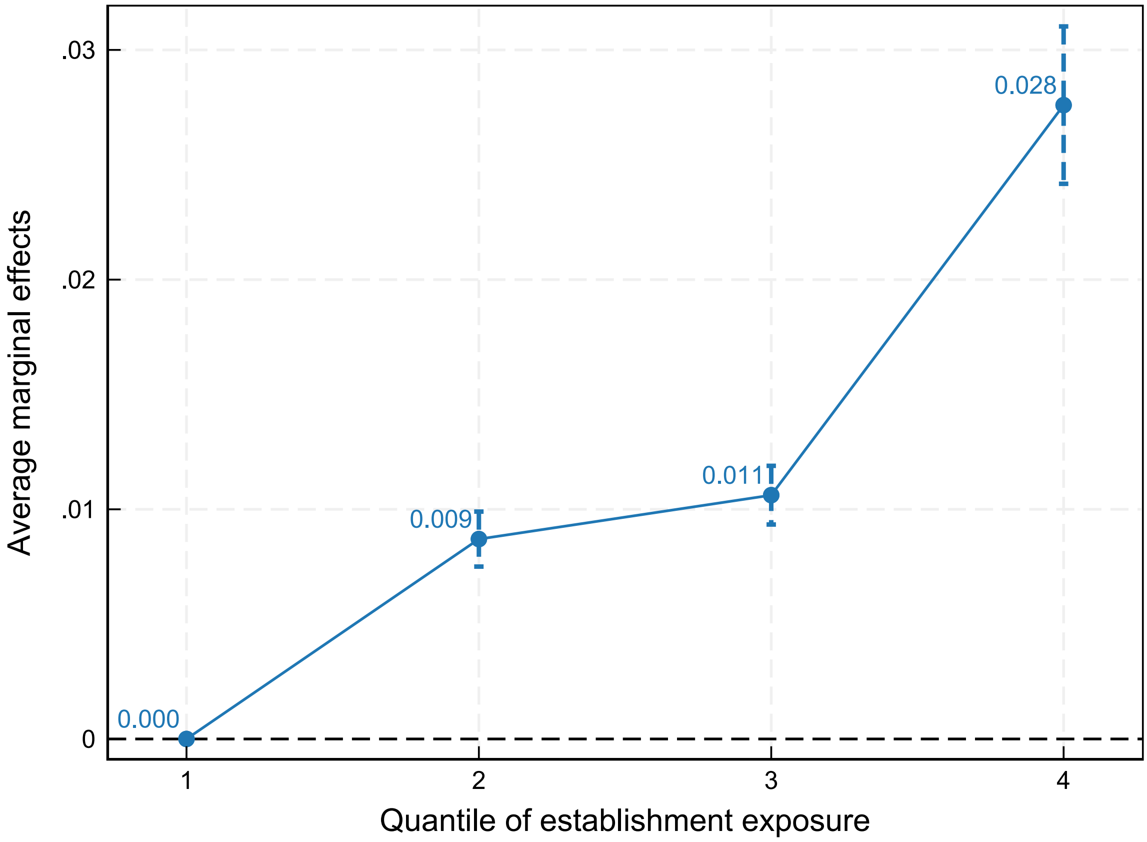
Notes. The figure reports event study coefficients from equation (1) on the triple interaction between the treatment dummy (having a child between 3 and 5 years old), the year dummy (equal to one for 2015) and the calendar month dummies (June used as a reference). The estimates are conducted after excluding parents of newborn children (age 0) or 2-year-old children (age 2). The dependent variable is a dummy equal to one if the worker takes parental leave. Panel A reports the estimates for fathers and panel B for mothers. The vertical lines are 95% confidence intervals obtained from cluster-robust standard errors at the worker level.

Figure A.9: Parental leave take-up at the individual level, excluding parents with past use of parental leave



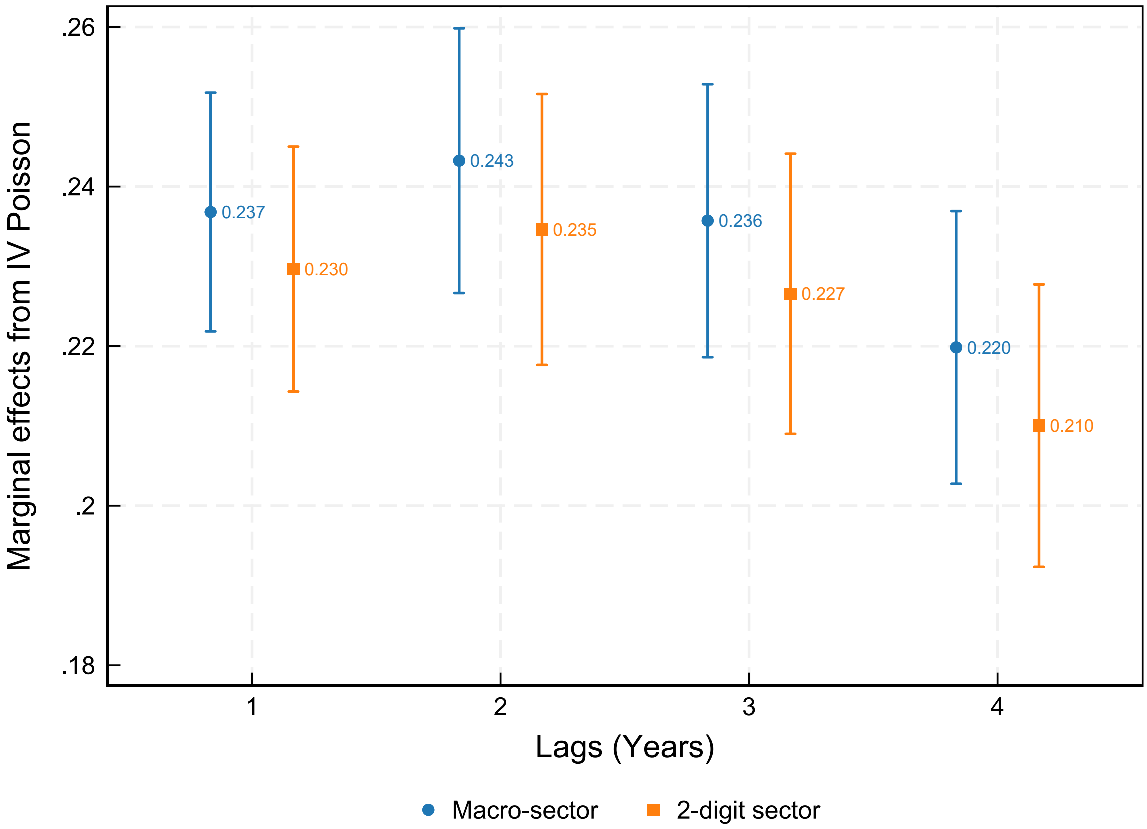
Notes. The figure reports event study coefficients from equation (1) on the triple interaction between the treatment dummy (having a child between 3 and 5 years old), the year dummy (equal to one for 2015) and the calendar month dummies (June used as a reference). The estimates are conducted after excluding parents who never used parental leave before 2014 or have used less than 5 or 10 months. The dependent variable is a dummy equal to one if the worker takes parental leave. Panel A reports the estimates for fathers and panel B for mothers. The vertical lines are 95% confidence intervals obtained from cluster-robust standard errors at the worker level.

Figure A.10: Testing the monotonicity of the instrument



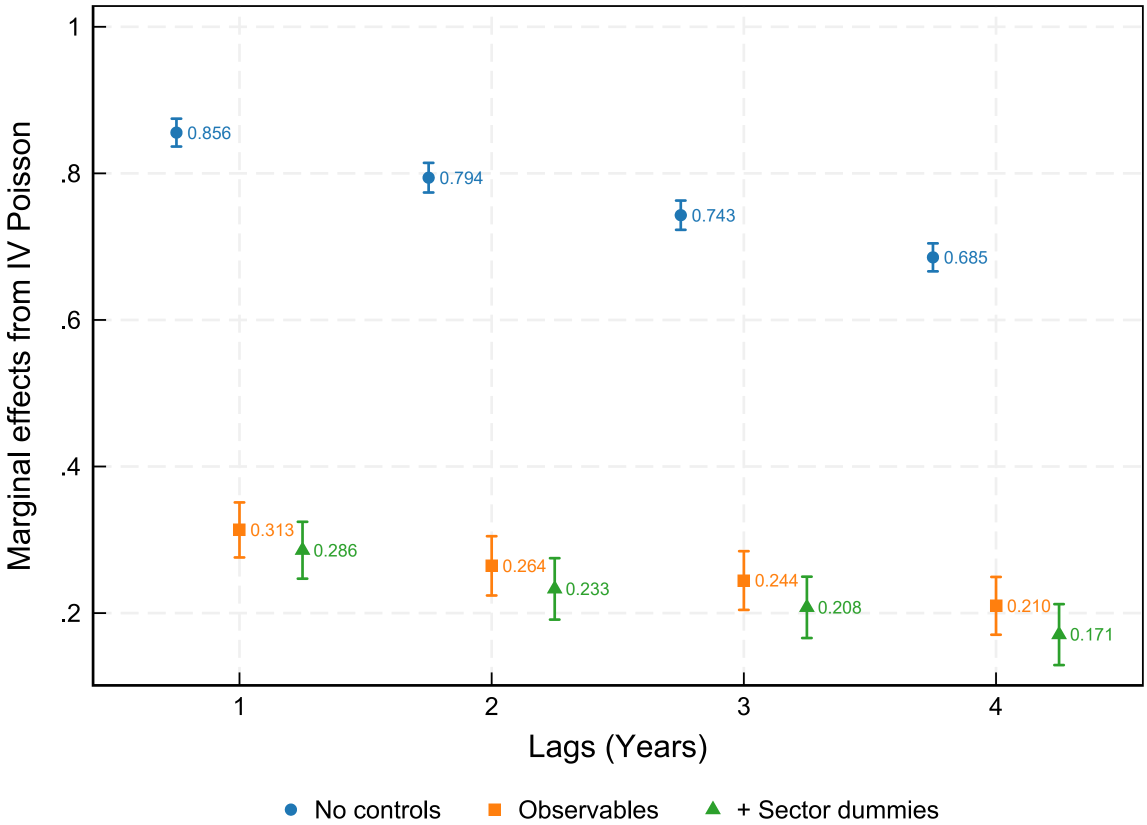
Notes. The figure reports the average marginal effects of a regression of the establishment-level share of fathers of 3-5-year-old children on total fathers of 0-5-year-old children taking parental leave, on four quantiles of the instrument measuring the establishment exposure to the reform, i.e., the establishment-level share of fathers of 3-5-year-old children on total fathers of 0-5-year-old children in 2014

Figure A.11: Peer effects, robustness to different sector definitions



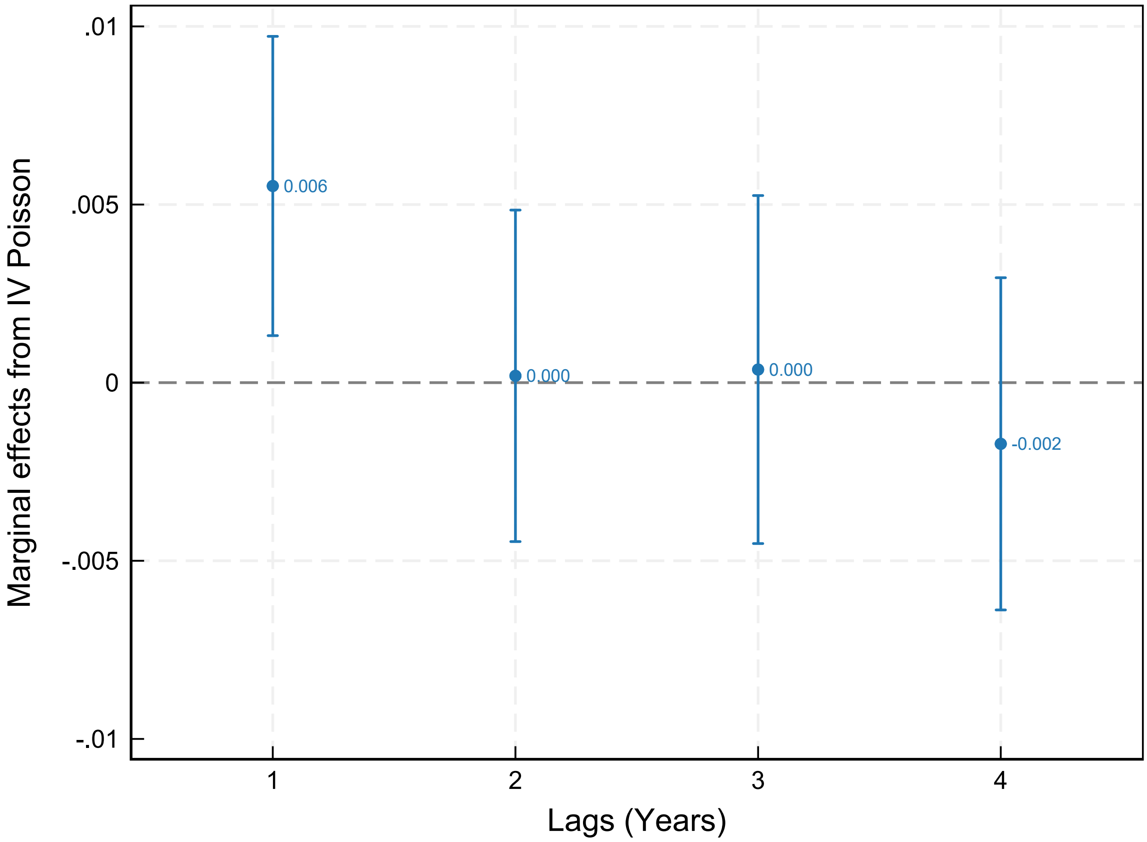
Notes. The figure reports the marginal effects from the IV Poisson estimation of equation (4). The coefficients are those on the share of peer fathers of children between 3 and 5 years old on total fathers of children between 0 and 5 years old in the establishment taking parental leave between August and December of either 2014 or 2015. The dependent variable is the share of coworker fathers taking parental leave at lag 1 to 4. In "Macro-sector" we replicate Figure 2 and include observables and 13 dummies for macro-sectors (Nace Rev. 2 sections A, B, C, D-E, F, G-I, H, J, K, L, M-N, O-Q, R-U). In "2-digit sector" we replace the macro-sector dummies with Nace Rev. 2 2-digit sector dummies. Vertical lines are 95% confidence intervals obtained from robust standard errors.

Figure A.12: Peer effects: parental leave take-up by coworker mothers



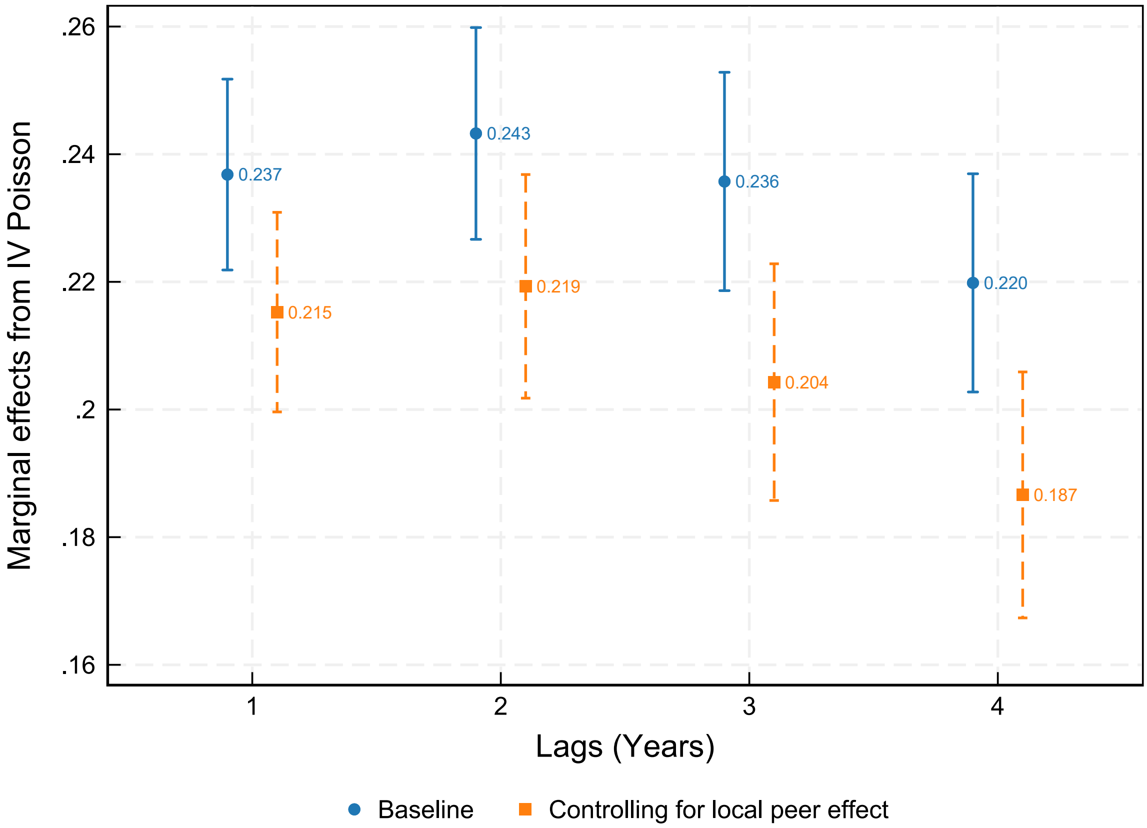
Notes. The figure reports the marginal effects from the IV Poisson estimation of equation (4). The coefficients are those on the share of peer mothers of children between 3 and 5 years old on total mothers of children between 0 and 5 years old in the establishment taking parental leave between August and December of either 2014 or 2015. The dependent variable is the share of coworker mothers taking parental leave at lag 1 to 4. In "No controls" we do not add additional covariates. In "Observables" we control for average female and male earnings, average female and male age, log firm size, share of women, share of workers with temporary contracts and share of white-collar workers. In "Sector dummies" we include 13 dummies for macro-sectors (Nace Rev. 2 sections A, B, C, D-E, F, G-I, H, J, K, L, M-N, O-Q, R-U). Vertical lines are 95% confidence intervals obtained from robust standard errors.

Figure A.13: Peer effects on fertility among fathers



Notes. The figure reports the marginal effects from the IV Poisson estimation of equation (4). The coefficients are those on the share of peer fathers of children between 3 and 5 years old on total fathers of children between 0 and 5 years old in the establishment taking parental leave between August and December of either 2014 or 2015. The dependent variable is the share of coworker fathers having a child in lag 1 to 4. The regressions control for observables and macro-sector dummies. Vertical lines are 95% confidence intervals obtained from robust standard errors.

Figure A.14: Peer effects controlling for local factors



Notes. The figure reports the marginal effects from the IV Poisson estimation of equation (4) for the "Baseline" estimate. The "Controlling for local peer effect" estimate includes as a control the share of peer fathers in the same municipality who are employed in a different establishment taking parental leave, instrumented with the analogue share of eligible peer fathers. The coefficients are those on the share of peer fathers of children between 3 and 5 years old on total fathers of children between 0 and 5 years old in the establishment taking parental leave between August and December of either 2014 or 2015. The dependent variable is the share of coworker fathers taking parental leave at lag 1 to 4. The regressions control for observables and macro-sector dummies. Vertical lines are 95% confidence intervals obtained from robust standard errors.