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

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November 2025

Abstract

Using a reform that increased parental leave generosity, we estimate workplace peer effects in leave-taking, focusing on fathers. Coworker fathers are more likely to take leave when exposed to more peer fathers affected by the reform. Effects are stronger in establishments with higher social capital and pre-reform leave use. We explain our findings showing that incumbent coworkers drive the effects, same-gender peer influences exceed cross-gender ones, the strongest peer effects run from higher- to lower-ranked occupations, and career penalties are absent for peer fathers. Peer effects extend to coworker fathers' partners, less so to coworker mothers' partners.

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

***JEL* codes:** J13, J16, J18, K31, M52

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1 Introduction

Widespread gender gaps in labor market outcomes persist. 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, and provide evidence on potential mechanisms behind them. 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, 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 establishments. We define as

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

peer parents the workers employed in an establishment in 2014 or 2015 who already had children between 3 and 5 years of age at that time. Their *coworker parents* are colleagues working in the same establishment in 2014 and 2015, who became parents for the first time within the following four years. This definition of coworker parents ensures that they are not directly affected by the reform, as they were not yet parents 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 27.5 percent among fathers and 15.4 percent 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 any 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 its 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 of peer fathers (mothers) of children aged 3-5 who took parental leave, relative to the total number of fathers (mothers) of children aged 0-5 employed in the establishment in 2014 and 2015. To address endogeneity concerns, we instrument this variable with a measure of potential exposure before the reform, i.e., the share of fathers (mothers) of children aged 3-5 over the total number of fathers (mothers) of children aged 0-5 in 2014. In other terms, we exploit variation in the age composition of employees' children across establishments before the reform. This identification strategy rests on the assumption that establishment-level characteristics are conditionally exogenous to the potential exposure to the reform, allowing the latter to serve as an exogenous shifter of parental leave take-up in the establishment. We account for sector-specific unobserved heterogeneity, besides controlling for a rich set of observable establishment characteristics, and perform additional analyses to assess the robustness and exogeneity of our instrument. A placebo test reveals that current peer fathers have only negligible effects on past coworkers, suggesting that unobserved establishment-level factors are unlikely to drive peer effects. We then document heterogeneous effects in peer effects, showing that they are not confined to specific groups of workers or establishments. Finally, we analyze peer effects among incumbent and newly hired coworker fathers, showing that they are present only for the former and not for the latter, providing additional support for the validity of our identification strategy. As to methods, given the skewness in the distributions of the variables considered, we estimate a Poisson model with a two-step GMM, and report marginal effects.

At the establishment level, we show that increasing the generosity of parental leave policies has a positive impact on parental leave take-up among peer fathers, confirming the instrument relevance. Estimating 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 percent increase in the share of peer fathers taking parental leave induces a 0.24 percent increase in the take-up rate among coworkers who become fathers in the following year. These effects are both statistically and economically significant and persist in the medium term, up to four years after the reform. 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 rule out the possibility that the increased parental leave utilization among coworkers stems from higher fertility rates in the medium term. Moreover, peer effects remain significant after controlling for coworker fathers' past leave take-up. In addition, we estimate peer effects of comparable magnitude among mothers.

To explore the mechanisms driving 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 between 2012 and 2014. Peer effects are approximately twice as large in prosocial establishments, even after controlling for establishment size. Moreover, peer 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. While establishment characteristics significantly influence the size of peer effects, they are not the sole drivers. In fact, we find negligible peer effects among newly hired coworker fathers who were not directly exposed to the increased parental leave take-up by peer fathers, reinforcing the interpretation that our estimates capture peer influence rather than reflecting workplace norms or selection. In addition, we estimate both same-gender and cross-gender peer effects. If peer influence extends beyond establishments, same-gender effects should be stronger than cross-gender effects. Our findings confirm this. Examining peer effects across occupational groups, we observe that influence is strongest when it flows from higher to lower occupational ranks, specifically from white-collar to blue-collar workers.

To further explore the drivers of peer effects, we show that peer fathers taking parental leave have career trajectories comparable to, or slightly better than, those of peer fathers who do not take leave. This suggests that taking parental leave does not significantly hinder career advancement, thereby mitigating concerns among coworker fathers about potential professional repercussions from increased leave take-up. For mothers, however, the findings on career consequences are more mixed, signaling the role of social norms independently of labor market factors.

Finally, we show that increased parental leave take-up among peer fathers positively influences the labor market outcomes of their coworkers' partners. These partners experience higher earnings, increased weeks worked, advancement into higher-paying occupations, and a greater likelihood of holding permanent contracts, although they also display increased 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 suggests 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, building on Dahl et al. (2014), that focuses on father-to-father peer effects in both professional and family networks by exploiting a paternity leave reform in Norway, we advance knowledge on peer effects in several ways. First, the reform we leverage applies to both fathers and mothers under a shared parental leave scheme, allowing us to assess the relative strength of same-gender versus cross-gender peer effects. Second, we investigate the indirect effects on father and mother coworkers' partners, providing novel evidence of improved labor market outcomes for female partners and near-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 face lower earnings losses (Bang, 2021).[2] Third, leveraging 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 establishments—a variable often difficult to measure in most empirical studies—also controlling for establishment size. The data also allow us to inform on worker groups that drive peer influence—primarily incumbents and employees in higher occupational ranks. Finally, we focus on Italy, a country characterized by lower gender equality and more conservative gender attitudes compared to Norway, thereby providing external validity in other contexts. Our findings highlight that social interactions within establishments can be an effective transmission mechanism for public policy.[3]

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 concerns related to non-random

[2]Since we focus on parents of older children rather than newborns, our analysis also relates to the literature showing that school closures and unexpected school events disproportionately increase mothers' childcare responsibilities (Buzard et al., 2025; Price and Wasserman, 2024). A higher involvement of fathers in childcare can reduce such asymmetries.

[3]Our paper is also broadly related to the literature examining peer effects in the workplace, which typically investigates how peers influence workers' productivity and wages, with findings ranging from null (Guryan et al., 2009) to modest (Cornelissen et al., 2017) or positive effects (Battisti, 2017; Hong and Lattanzio, 2025; Mas and Moretti, 2009; Messina et al., 2024) when individuals work alongside more productive or higher-wage colleagues. While our paper also examines workplace peer effects, it focuses specifically on their influence on program participation rather than productivity outcomes.

sorting within coworker networks. Dottori et al. (2024) study the same policy as we do and find positive peer effects among mothers. Compared to their study, we employ a different methodology and focus primarily on fathers.

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 earmarked leave. 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 lasting behavioral changes 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 parental leave use by fathers and mothers serves as the preliminary step toward analyzing peer effects at the establishment level, rather than being the primary focus of our analysis. Nevertheless, our findings suggest that parental leave reforms targeting both parents increase take-up by mothers and fathers, with relatively larger proportional increases observed among fathers.[4]

Finally, we also contribute to the social science literature by analyzing the importance of workplace interactions in shaping 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, influences across occupational ranks, and establishment-level social capital, in line with the insights from this strand of literature.

[4] We also relate to the literature exploiting family policy changes to study the effects of parental leave take-up on firm-level outcomes (Brenøe et al., 2024; Carta et al., 2024; Corekcioglu et al., 2024; Gallen, 2019; Huebener et al., 2024). None of these papers studies peer effects in program participation.

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 are entitled to a five-month mandatory maternity leave, which may be taken two (one) months before childbirth and three (four) months afterward.[5] The National Social Security Institute 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, to be taken within five months from childbirth, 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, while guaranteeing job protection. Each parent can take a maximum of 6 months of leave, which can be split very flexibly over time. When fathers take at least 3 months of leave, they are entitled to a maximum of 7 months rather than 6—for a total of 11 months considering both parents.[6]

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 2012 (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 account for more than 80 percent of total parental leave takers, with only little change over time.[7] The low replacement rate likely contributes to both the low take-up and the predominance of women among leave users in the Italian context. Indeed, it is more convenient for families to forgo the lower earner's income, usually the woman's, as they are frequently secondary earners.[8]

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 leave (see Table 1). In particular, the reform included the following main policy changes:

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

[6]The parental leaves we study differ from leaves granted for a child's sickness. In Italy, for children under age three, employed parents are entitled to an unlimited leave period, provided they alternate in its use; each child entitles parents to a separate leave. For children aged three to eight, parents can take up to five days per year, again alternating between them. In the private sector, these days are unpaid. These provisions remained unchanged around the reform we study.

[7]Parental leave duration averages 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.

[8]More details on the leave take-up over time in Italy are 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 percent 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 percent 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 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. 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;[9] *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 establishment level. In particular, we define as pro-social those establishments where at least one such leave was 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.[10]

Aggregate statistics on voluntary parental leave take-up and duration Figure A.1 shows the aggregate number of parental leaves 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 summer. 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.

[9] We are unable to identify childbirths occurring in couples where the mother was not employed at the time of birth *and* neither parent ever takes parental leave while employed.

[10] This is the finest establishment identifier that the data allow us 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 who took parental leave on the total number of fathers (mothers) of children aged 0-5 who were employed in the establishment in 2014 and 2015. We instrument this variable with a measure of potential exposure, given by the share of peer fathers (mothers) of children aged 3-5 on the total number of fathers (mothers) of children aged 0-5 in 2014.

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 focus on 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[11]—in the year of the reform (2015) with respect to the year before (2014), by calendar month with respect to the month in which the law was passed. 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 starts 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.[12] A_t is a

[11] We cannot use parents of older children as a control group, as they are also affected by the reform, though to a lesser extent. See Table 1.

[12] We measure the age of the child in June of each year t. We make this choice to avoid individuals switching treatment status between different calendar months within the same year. Notice that the

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 by each month 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 estimate the effect of taking parental leave on parents' labor market outcomes using a two-stage least squares (2SLS) framework. In the first stage, parental leave take-up is instrumented with eligibility to the reform. Specifically, we estimate:

$$\begin{aligned} P_{i,p,t} = \alpha_1 + \beta_1\big(D_{it} \cdot A_t \cdot K_p\big) + \gamma_1\big(D_{it} \cdot K_p\big) + \delta_1\big(A_t \cdot K_p\big) + \eta_1\big(D_{it} \cdot A_t\big) \\ + \zeta_1 D_{it} + \kappa_1 A_t + \lambda_1 K_p + \theta_i + u_{i,p,t}, \end{aligned} \tag{2}$$

$$\begin{aligned} y_{i,p,t+\tau} = \alpha_2 + \beta_2 \widehat{P}_{i,p,t} + \gamma_2\big(D_{it} \cdot K_p\big) + \delta_2\big(A_t \cdot K_p\big) + \eta_2\big(D_{it} \cdot A_t\big) \\ + \zeta_2 D_{it} + \kappa_2 A_t + \lambda_2 K_p + \theta_i + \varepsilon_{i,p,t+\tau}, \end{aligned} \tag{3}$$

where D_{it}, A_t, and θ_i are defined as in equation (1). Equation (2) represents the first stage, corresponding to a static version of equation (1). The dependent variable $P_{i,p,t}$ equals one if individual i takes parental leave in subperiod p, where we split each year $t = \{2014, 2015\}$ into two subperiods: January–July and August–December. K_p equals one for the August–December subperiod. Since the law was passed on June 25 and took effect after 15 days, July is treated as an adjustment month and included in the pre-reform period. $u_{i,p,t}$ is the first-stage error term.

We then use the predicted values $\widehat{P}_{i,p,t}$ from the first stage in the second-stage regression (3), estimated via 2SLS. The outcomes $y_{i,p,t+\tau}$ are measured for individual i in subperiod p and year $t + \tau$, with $\tau \in \{1, 2\}$. They include employment in the non-agricultural private sector, employment in the same establishment, cumulative earnings and days worked between t and $t + \tau$, and indicators for transitions across contract

potential misclassification of treatment status implies a downward bias in our estimates, as parents of children who turn 6 between July and December are classified as treated, but only partially experience the increased generosity of parental leaves, and parents of children who turn 3 between July and December are classified as control, but are actually eligible for the reform.

and qualification types (temporary to permanent, white-collar to manager, blue-collar to manager, and blue-collar to white-collar). $\varepsilon_{i,p,t+\tau}$ is the error term of the second stage.

4.2 Establishment-level analysis

Peer effects in parental leave take-up Our main goal is to quantify the influence of peer fathers' parental leave take-up on their male coworkers' leave-taking behavior.[13] We focus on establishments 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 aged between 3 and 5, 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 as peer fathers in year t. By focusing on first-time fathers in $t+k$, we ensure they were not directly exposed to the reform in year t. They may be indirectly exposed, as the reform also increases parental leave generosity for them. However, this applies equally to coworker fathers of peer fathers in both 2014 and 2015. The inclusion of a pre-reform control group therefore allows us to net out common trends in parental leave take-up associated with changes in benefit generosity.

To measure the exposure of establishments to the parental leave reform, we build a variable P_{jt}, defined as:

$$P_{jt} = \frac{N_{jt}^{L,\text{ Aug-Dec, 3-5}}}{N_{jt}^{\text{0-5}}},$$

where $N_{jt}^{L,\text{ Aug-Dec, 3-5}}$ denotes the number of peer fathers in establishment j and year t with children aged 3-5 who took parental leave between August and December, and $N_{jt}^{0\text{–}5}$ is the total number of peer fathers with children aged 0-5 in the same establishment and year.

To address potential endogeneity concerns, we build a measure of potential exposure, Z_j, defined as:

$$Z_j = \frac{N_{j,2014}^{3\text{–}5}}{N_{j,2014}^{0\text{–}5}},$$

where $N_{j,2014}^{3\text{–}5}$ is the number of peer fathers in establishment j with children aged 3-5, and $N_{j,2014}^{0\text{–}5}$ is the total number of fathers with children aged 0-5 employed in the same establishment in 2014 (before the reform).[14] The potential exposure Z_j exploits variation

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

[14]We exclude observations with $Z_j = 1$ corresponding to establishments in which all fathers have children aged 3 to 5. In other terms, we condition on the presence of at least one non-eligible father in the establishment. In our data, establishments with $Z_j = 1$ are almost exclusively very small firms in terms of parental composition: in 99% of cases, they are firms with at most two fathers of children aged 0–6 (92% with only one such father, 7% with two). In 99.9% of cases, they are firms with four such fathers, while in the remaining 72 establishments, the number of fathers with children aged 0–6 ranges

across establishments in the age composition of employees' children before the reform. Firms with a higher share of peer fathers with children aged 3-5 in 2014 were more exposed to the reform, as a larger fraction of their workforce became newly eligible for the more generous parental leave.

Formally, we estimate with a two-step GMM a Poisson model for the share of coworker fathers in establishment j that take parental leave in year $t+k$, where the key regressor of interest is the share of peer fathers in establishment j who took leave in year t. The latter variable is potentially endogenous, as peer take-up may reflect unobserved establishment-level characteristics that also influence coworkers' leave decisions. To account for this, we implement an instrumental variables (IV) strategy using a GMM estimator suitable for nonlinear models with endogenous regressors.

Our identification relies on exogenous variation in peer eligibility induced by the 2015 parental leave reform. Although we do not implement a two-stage least squares estimation, we provide evidence on the relevance of the instrument using a linear difference-in-differences specification that captures how the policy shock affects peer leave take-up across establishments with different baseline shares of eligible fathers. In addition to supporting instrument relevance, the difference-in-differences approach accounts for differential trends in establishments with varying exposure to eligible peer fathers. We therefore estimate the following equation:

$$P_{jt} = \alpha + \beta Z_j + \gamma A_t + \delta Z_j \cdot A_t + \eta X_j + \psi_{s(j)} + \epsilon_{jt}, \tag{4}$$

where P_{jt} and Z_j are defined above. A_t is a post-reform dummy equal to one in 2015, and X_j includes establishment-level covariates averaged over 2014-2015: average male and female earnings, average age by gender, log establishment size, share of female workers, share of temporary contracts, and share of white-collar workers. Sector fixed effects $\psi_{s(j)}$, broadly corresponding to NACE sections (13 categories), are also included. ϵ_{jt} is an error term robust to heteroskedasticity.

We then estimate the following two-step Poisson model using IV-GMM:

$$\mathbb{E}[Y_{j,t+k} \mid P_{jt}, X_j, \psi_{s(j)}] = \exp\left(\beta P_{jt} + \eta X_j + \psi_{s(j)}\right), \tag{5}$$

where $Y_{j,t+k}$ is the share of coworker fathers in establishment j who became fathers in year $t+k$, with $k \in \{1, 2, 3, 4\}$, and who took parental leave while remaining employed in the same establishment since year t.[15] P_{jt}, X_j and $\psi_{s(j)}$ are defined as above. The parameter of interest, β, captures the causal effect of peer leave behavior on coworker leave

between five and seven. Also, note that the construction of the variable Z_j is similar in spirit to that in Ginja et al. (2023), who also exploit potential exposure of firms to a parental leave reform, by computing the firm-level share of affected workers.

[15] In other terms, $Y_{j,t+k} = \frac{N^L_{j,t+k}}{N_{j,t+k}}$, where $N^L_{j,t+k}$ is the number of incumbent coworker fathers taking leave and $N_{j,t+k}$ is the total number of incumbent coworker fathers in establishment j and year $t+k$.

take-up, estimated using IV-GMM for a Poisson model as outlined in Wooldridge (2010), where the endogenous variable is instrumented as in equation (4).[16] Under standard IV assumptions, including instrument relevance and conditional exogeneity (see Section 6.2), the coefficient β in equation (5) is identified as a structural parameter in a GMM framework. It captures the causal effect of peers' parental leave take-up on that of coworkers, leveraging exogenous variation in peer behavior induced by differences in the pre-reform share of eligible fathers.

One potential concern for our identification strategy is measurement error in Z_j. Note that we observe both the date of birth and the timing of parental leave take-up over multiple years; hence, we are able to classify fathers accurately by their children's age group regardless of the leave-taking date. However, since the identification of eligible fathers relies either on their own parental leave take-up or on the observed employment status of their partners around the time of childbirth, it is possible that we misclassify some eligible fathers. Measurement error in Z_j arises if this misclassification differentially affects the number of fathers with children aged 0–2 versus those with children aged 3–5. Specifically, if fathers of younger children (0-2) are more likely to be undercounted, Z_j will overstate the true exposure share Z_j^*; conversely, if fathers of older children (3-5) are more likely to be undercounted, Z_j will understate the true exposure. If, however, undercounting is proportional across both groups, there is no measurement error, and our identification strategy is valid.

According to the OECD Family Database (Maternal employment rates),[17] the employment rate of Italian mothers around childbirth (children aged 0–2) has progressively increased in the early 2010s. In 2014, mothers of children aged 0–2 had an employment rate of 53.3 percent on average between 2012 and 2014 (when they gave birth), compared with an average of 52.4 percent between 2009 and 2011 (when mothers of 3-5 year old children gave birth). This trend implies progressively declining underreporting for younger cohorts (mothers of children aged 3-5, and their partners, are relatively more underreported than mothers of children aged 0–2). Formally, this means that we observe $Z_j + u_j = Z_j^*, \quad \text{where} \quad u_j < 0$, leading to downward bias in the measured exposure share. Provided that $\text{Cov}(u_j, \epsilon_{jt}) = 0$, the resulting bias corresponds to classical measurement error, which attenuates the estimated coefficient without invalidating identification. In particular, even if a correlation between u_j and ϵ_{jt} exists, it is unlikely to vary meaningfully across adjacent periods t. In this case, the bias introduced by measurement error would be absorbed by time fixed effects, which control for any time-specific shocks or persistent trends that might simultaneously affect the measurement error and the outcome.

[16]Due to the highly skewed distribution of parental leave take-up at the establishment level, we adopt a Poisson model and report marginal effects. For examples of Poisson models with non-count and proportion data, see Wooldridge (1999), Silva and Tenreyro (2006), and Cohn et al. (2022).

[17]The data are available at `https://www.oecd.org/en/data/datasets/oecd-family-database.html` (last access: 29 July 2025).

Another challenge may come from changes in fertility rates across cohorts. However, they do not affect measurement error, as these changes influence both Z_j and Z_j^* similarly.

Indirect effects on partners of coworker parents As we can link members of households in the data, we also examine the indirect effects of peers on labor market outcomes of partners of both coworker fathers and mothers. In this case, we employ a two-stage least squares approach to estimate the following equation:[18]

$$o_{j,t+k} = \alpha + \beta \widehat{P}_{jt} + \eta X_j + \psi_{s(j)} + \epsilon_{j,t+k}, \tag{6}$$

where the dependent variable $o_{j,t+k}$ includes average establishment-level annual earnings, weekly earnings, weeks worked, and shares in white-collar jobs, permanent positions, and full-time contracts among coworker partners.[19] $\widehat{P}_{jt}$ is instrumented with Z_j, and the other variables are defined as above.

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 their 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 to be 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

[18] We can use a linear model in this case because the distribution of the outcomes is not skewed.

[19] If the partner is not employed, we impute 0 annual earnings and 0 weeks worked. The other outcomes are instead conditional on having strictly positive earnings.

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.

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.[20]

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 (27.5 percent versus 15.4 percent).

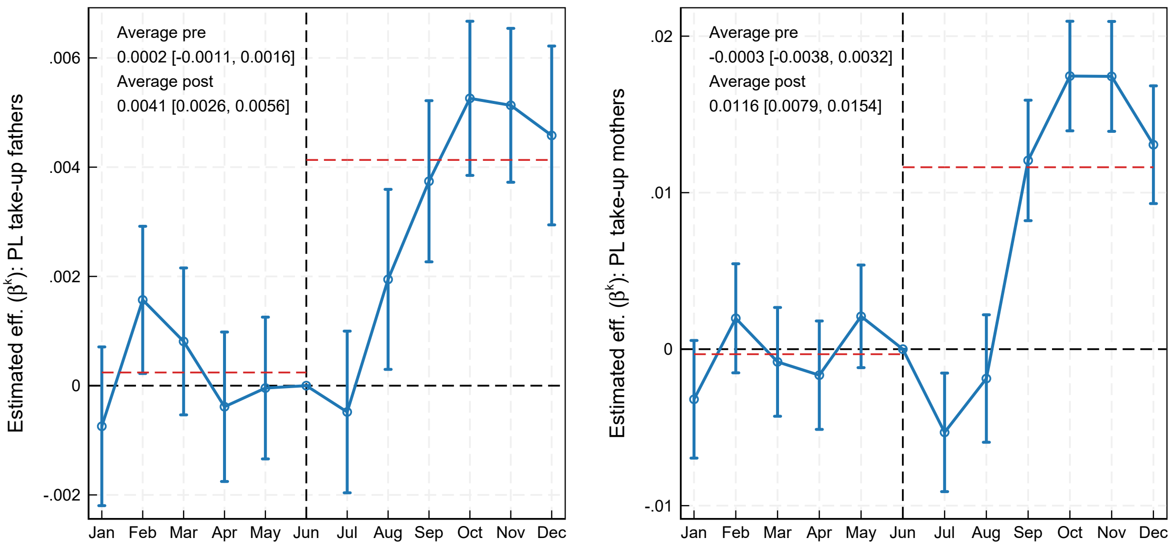
Additionally, heterogeneity analyses reveal that the increased take-up is entirely concentrated among workers with permanent contracts (Figure A.4).

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 duration 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

[20]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).

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

(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). 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 percent 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, which we also report alongside their confidence intervals.

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 group maintains a relatively flat trajectory, suggesting that there is no intertemporal strategic optimization of parental leave, at least in the time frame we consider.[21] Note

[21]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.

also that, as shown in Figure A.2, parental leave take-up is concentrated in short durations and follows specific seasonal patterns, indicating that take-up is driven by particular needs, such as seasonal illness. We further show the effects by the child's exact age in Figure A.7, which reports dynamic difference-in-differences estimates for parents with children aged 0 to 5 in June of each year. 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 aged 3, 4, and 5. The figure also shows an increase, especially among fathers, for parents of newborns (0 years). Such an increase may be related to the introduction, contextual to the policy we study, of hourly parental leave, which may be utilized heterogeneously depending on the child's age, and could particularly benefit 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 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 2-year-old children, 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 (3) for fathers, with the corresponding first stage (equation 2) in column 1 of Table A.1. Panel A of Table 3 reports the reduced form effect, which is interpreted as the effect of being eligible for the reform on future labor market outcomes. Eligibility does not come with career costs for peer fathers. 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. Panel B rescales these coefficients by the first stage, confirming—if anything—positive effects on the probabilities of employment and of conversion from temporary to permanent contract.

In contrast, mothers do have career costs in terms of some of the outcomes that we consider. Table A.2 reports the reduced form and 2SLS estimates (equations 2 and 3) for peer mothers in panels A and B, respectively. The first stage is shown in column (2) of Table A.1. Table A.2 indicates that after one year, mothers are less likely to be employed (only in the reduced form), 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 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 4), and compares them with the full population of establishments (column 1), with that of establishments employing at least two workers (column 2), and those employing also at least one father (irrespective of child's age, column 3). The average share of eligible peer fathers in the analysis sample is 9.4 percent, while that of eligible fathers taking parental leave is 0.2 percent (4.3 and 0.2 percent in column 3).[22] Both variables display a high degree of skewness.

Compared with the average establishment (columns 1 and 2), establishments in the analysis sample (column 4) 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

[22]We cannot report the share of fathers of 3-5-year-old children in columns 1 and 2, as we do not have information on the universe of fathers in the data.

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.
Panel A: Reduced form								
	1-year horizon							
Coefficient	0.0011*	-0.0002	-68.2	-0.16	0.0025***	0.0002	-0.0000	0.0006
	(0.0006)	(0.0014)	(48.9)	(0.11)	(0.0009)	(0.0004)	(0.0000)	(0.0004)
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	0.0013*	0.0003	-85.2	-0.00	0.0014*	0.0004	-0.0000	0.0004
	(0.0008)	(0.0012)	(51.9)	(0.11)	(0.0008)	(0.0004)	(0.0000)	(0.0004)
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
Panel B: 2SLS								
	1-year horizon							
Coefficient	0.0977*	-0.0164	-5938.3	-13.65	0.2190***	0.0192	-0.0008	0.0498
	(0.0554)	(0.1197)	(4290.6)	(9.34)	(0.0817)	(0.0329)	(0.0031)	(0.0345)
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	0.1170*	0.0306	-7579.2	-0.00	0.1265*	0.0384	-0.0016	0.0352
	(0.0691)	(0.1074)	(4673.6)	(9.74)	(0.0736)	(0.0335)	(0.0035)	(0.0333)
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 (3) in the reduced form (panel A) and in 2SLS (panel B). 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) in panel A, and the one on predicted parental leave take-up in panel B. Both panels report the effects for $\tau = 1$ and $\tau = 2$. 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: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

donor between 2012 and 2014. Possibly due to their larger size, and thus a potentially more hierarchical structure, 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. These differences mostly stem from the presence of fathers, not of fathers of young children, as the comparison of columns 3 and 4 shows.

6.2 Results

Reform effect at the establishment level and instrument validity The identification of the effects rests on the assumption that the instrument—the share of eligible

Table 4: Establishment-level descriptive statistics

	(1)		(2)		(3)		(4)	
	Full population		At least 2 employees		At least 2 employees & 1 father		Analysis sample	
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
Share fathers 3-5 yrs old	–	–	–	–	0.043	(0.190)	0.094	(0.189)
Share fathers 3-5 yrs old taking PL	–	–	–	–	0.002	(0.037)	0.002	(0.026)
Share female	0.482	(0.419)	0.455	(0.371)	0.273	(0.242)	0.273	(0.232)
Log firm size	1.120	(1.102)	1.660	(0.951)	2.681	(1.221)	2.884	(1.229)
Establishment size < 15	0.908	(0.289)	0.864	(0.343)	0.541	(0.498)	0.471	(0.499)
Establishment size 15-49	0.072	(0.258)	0.106	(0.308)	0.299	(0.458)	0.316	(0.465)
Establishment size ≥ 50	0.020	(0.142)	0.030	(0.172)	0.160	(0.367)	0.213	(0.409)
Mean age female	38.7	(9.1)	38.6	(8.4)	40.0	(7.1)	40.5	(6.7)
Mean age male	38.9	(9.2)	38.9	(8.4)	39.7	(5.7)	40.1	(5.4)
Mean earnings female	12,099	(9,786)	12,743	(10,008)	17,979	(11,564)	19,724	(11,086)
Mean earnings male	15,175	(14,695)	15,739	(14,159)	21,573	(15,481)	24,610	(15,065)
Temporary	0.194	(0.328)	0.216	(0.312)	0.189	(0.270)	0.149	(0.219)
White-collar	0.345	(0.426)	0.330	(0.394)	0.369	(0.365)	0.396	(0.358)
Pro-sociality	0.155	(0.361)	0.194	(0.395)	0.436	(0.496)	0.493	(0.500)
Gender pay gap $< 0\%$	0.410	(0.492)	0.410	(0.492)	0.327	(0.469)	0.301	(0.459)
Gender pay gap 0-20%	0.371	(0.483)	0.371	(0.483)	0.412	(0.492)	0.441	(0.496)
Gender pay gap $\geq 20\%$	0.219	(0.414)	0.219	(0.414)	0.261	(0.439)	0.258	(0.438)
North	0.488	(0.500)	0.505	(0.500)	0.620	(0.485)	0.651	(0.477)
Centre	0.211	(0.408)	0.209	(0.406)	0.193	(0.395)	0.188	(0.391)
South and Islands	0.302	(0.459)	0.286	(0.452)	0.186	(0.390)	0.161	(0.367)
Manufacturing	0.164	(0.371)	0.189	(0.392)	0.283	(0.450)	0.322	(0.467)
Utilities	0.006	(0.077)	0.007	(0.085)	0.016	(0.124)	0.017	(0.130)
Construction	0.120	(0.325)	0.122	(0.327)	0.130	(0.337)	0.115	(0.319)
Trade & hospitality	0.339	(0.473)	0.343	(0.475)	0.249	(0.432)	0.239	(0.426)
Info & communication	0.020	(0.141)	0.021	(0.144)	0.029	(0.168)	0.031	(0.174)
Finance & insurance	0.022	(0.145)	0.024	(0.152)	0.031	(0.172)	0.032	(0.175)
Prof. & admin services	0.130	(0.336)	0.120	(0.325)	0.107	(0.309)	0.095	(0.293)
Public & social services	0.055	(0.228)	0.047	(0.213)	0.029	(0.168)	0.030	(0.169)
N. firms	1,847,350		1,246,344		197,250		108,016	

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), for establishment with at least 2 employees and 1 father in column (3), and for those included in the analysis sample in column (4), i.e., those with at least 2 employees and 1 father of 0-5 year old children.

Table 5: Establishment-level effects of the reform on peer fathers' take-up

	(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 (4) 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 establishment size, share of female workers, 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: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

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 equation (4). 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 check, 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 a 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 effect. Specifically, establishments in the highest quantile of the instrument record 2.8 percentage points larger effects than those in the lowest one.

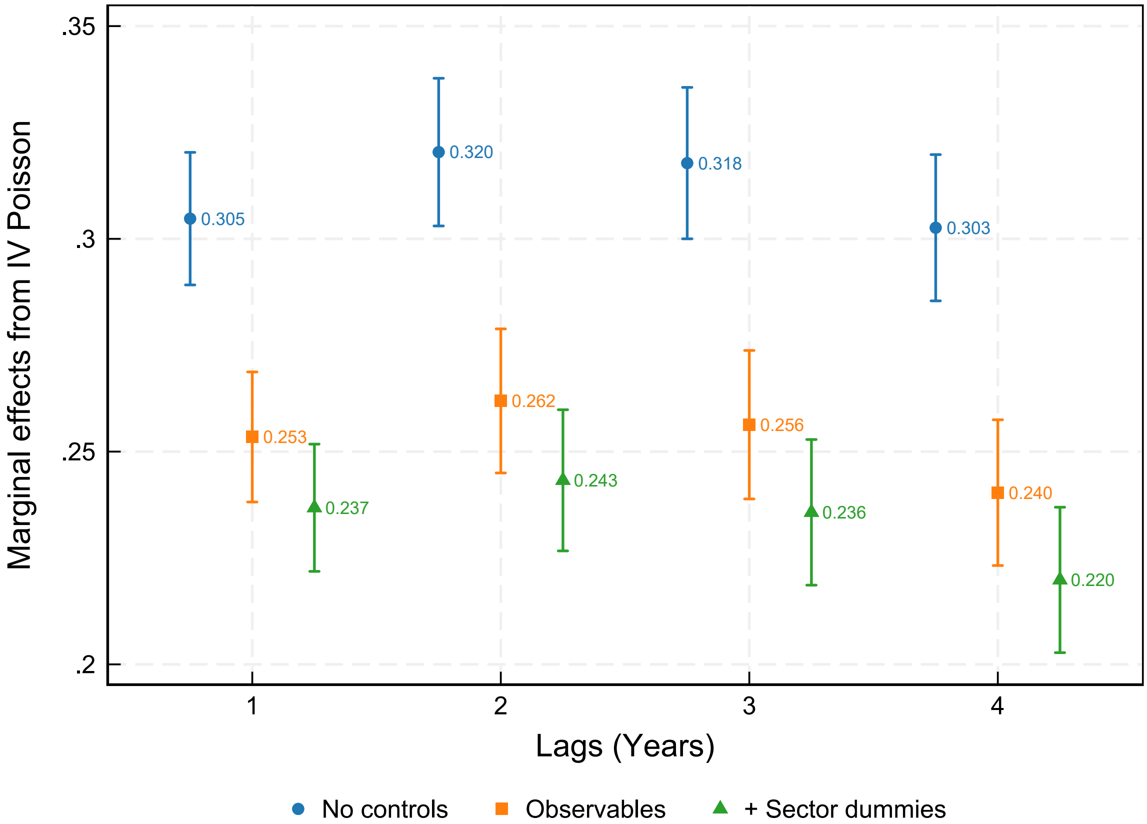
Regarding exogeneity, we test whether establishments with higher exposure differ systematically from those with lower exposure. Formally, Table A.3 presents a battery of regressions in which the instrument serves as the dependent variable, while progressively adding establishment-level control variables: log average female and male earnings, log average female and male age, log establishment size, the share of female workers, the share of workers with fixed-term contracts, the share of white-collar workers, and macro-sector and 2-digit sector fixed effects. Including additional covariates generally reduces the significance of coefficients. In particular, the instrument is mechanically correlated with men's age, because eligible peer fathers tend to be older, reflecting their having older children. This correlation, alongside smaller ones with female earnings and age, establishment size, and the white-collar share, survives the inclusion of sector dummies, as well.

We address these imbalances in several ways. First, we include the covariates, alongside the sector dummies, in our preferred specifications.[23] Second, we show that current peer fathers have only economically small effects on past coworkers, providing a placebo test that supports the absence of unobserved establishment-level factors driving the results (Figure 3). Third, we examine heterogeneous effects along multiple dimensions to assess whether peer effects are concentrated within specific groups. The results presented below show that peer effects are positive and statistically significant across all groups of covariates, also those that are correlated with the instrument (Figures 4 and A.20). Fourth, we show that peer effects are absent for newly hired coworker fathers, i.e., those that were not employed in the establishment in the baseline year t (Figure 5). This evidence reinforces the validity of our instrument.

Main estimates of peer effects Figure 2 reports the average marginal effects from equation (5), capturing the peer effects in parental leave take-up exerted by peer fathers on coworker fathers. The effects are positive and statistically significant in both unconditional and conditional (on observables and sector dummies) estimates. The coefficients indicate that a 10 percent increase in the share of peer fathers taking parental leave in a year determines a 2.4 percent increase in the share of coworker fathers taking

[23]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 potential exposure to the reform is unlikely to be correlated with the likelihood that family members of coworkers are themselves affected by the reform.

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



Notes. The figure reports the marginal effects from equation (5). 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 establishment size, share of female workers, 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 percent confidence intervals obtained from robust standard errors. The average take-up rate among coworker fathers is 3.2 percent at $k = 1$ and 3.5 percent between $k = 2$ and $k = 4$.

parental leave in the following year—or 0.6 percent for a one standard deviation increase (0.237×0.026). While the marginal effects tend to decrease over time, they remain largely stable across different model specifications and amount to 2.2 percent after four years.[24]

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 fathers generates an indirect peer effect on their incumbent coworkers, influencing their

[24]For computational reasons, we include 13 macro-sector dummies in equation (5). 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, panel A). Also, the choice of including controls measured as averages over 2014 and 2015 is innocuous: measuring them in 2014 (before the policy) does not make substantial differences (Figure A.11, panel B).

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.

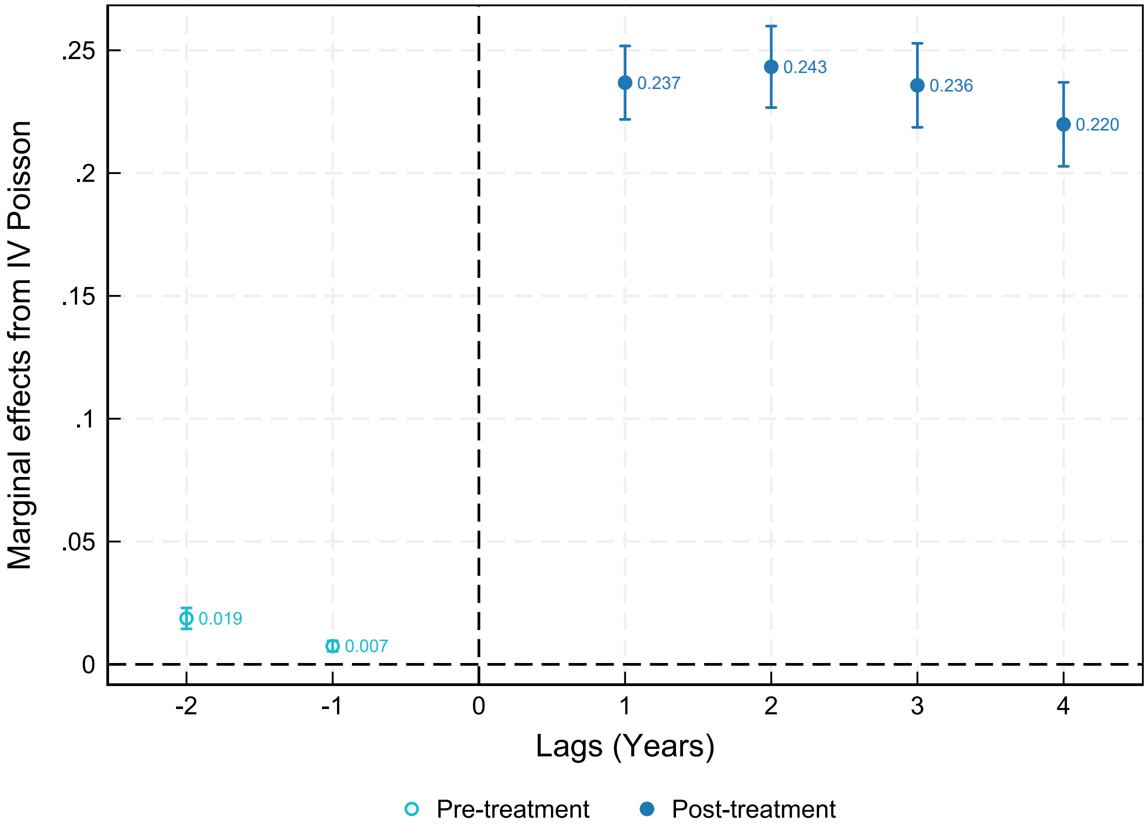
We further estimate peer effects among mothers. The results in Table A.4 indicate a positive and statistically significant policy effect on take-up, corresponding to an increase of approximately 1.8 percent in parental leave take-up among peer mothers after the reform. Figure A.12 reports the estimates 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.

Placebo effects on past coworkers Figure 3 evaluates whether establishments 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 (5), 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 economically small effects observed for past coworkers support the claim that our instrument is conditionally exogenous. If unobserved establishment-level heterogeneity were driving the results, we would expect to find larger placebo peer effects on past coworkers.

Robustness checks The estimates are similar in magnitude if we use the control function approach instead of GMM as estimation strategy (Figure A.13). Moreover, peer effects also capture an extensive margin effect. We report in Figure A.14 the coefficients from the estimation of equation (5) where we replace the outcome, the regressor, and the instrument with dummies for having at least one coworker father taking parental leave, at least one peer father taking parental leave, and at least one eligible peer father. The coefficients are always positive and statistically significant and are qualitatively similar to our main estimates. An additional concern relates to potential model misspecification when including controls. Mogstad and Torgovitsky (2024) recommend incorporating covariates in two-stage models as discrete variables.[25] Following their guidance, we re-estimate equation (5) by replacing continuous covariates with dummy variables for quartiles of each variable in X_j. Figure A.15 compares the estimates from our baseline specification with those obtained using discrete covariates. The confidence intervals largely overlap, and if anything, the baseline specification appears to provide a conservative estimate of

[25]Their discussion mainly concerns linear instrumental variable models in cross-sectional settings. We extend their approach to a non-linear specification estimated through two-step GMM.

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



Notes. The figure reports the marginal effects from equation (5). 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 percent confidence intervals obtained from robust standard errors.

peer effects.[26]

The peer effects may partly reflect coworker fathers increasing their fertility rate. Figure A.16 reports the estimates of equation (5) 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

[26] We estimate peer effects from the parental leave take-up of eligible peer fathers on the fertility decisions of coworker fathers—a distinct margin from that examined in the existing literature, which has documented positive peer effects in fertility itself (De Paola et al., 2025).

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).

In addition, the peer effects we estimate may reflect local conditions—such as culture or societal values specific to a municipality—rather than colleagues' interactions within single establishments. To test this hypothesis, we extend equation (5) 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.17 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.

Finally, peer effects may be reinforced by interactions between coworker fathers at time $t + k$ on top of those between peer and coworker fathers between t and $t + k$. To account for these additional interactions, we control in equation (5) for the share of coworker fathers taking parental leave up to lag k. Figure A.18 reports these estimates alongside the baseline. The peer effects controlling for coworker take-up are still sizable after four years and range between 1.2 and 1.6 percent for a 10 percent increase in peer fathers' take-up.

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 also provide a more favorable environment for parental leave take-up. Note, however, that the heterogeneous effects are not driven by establishment size: first, all regressions control for log establishment size; second, Figure A.19 in the Appendix shows that within different size groups the peer effect is always stronger in more pro-social establishments, with the difference being statistically significant except in a few cases. 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 workers 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 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.

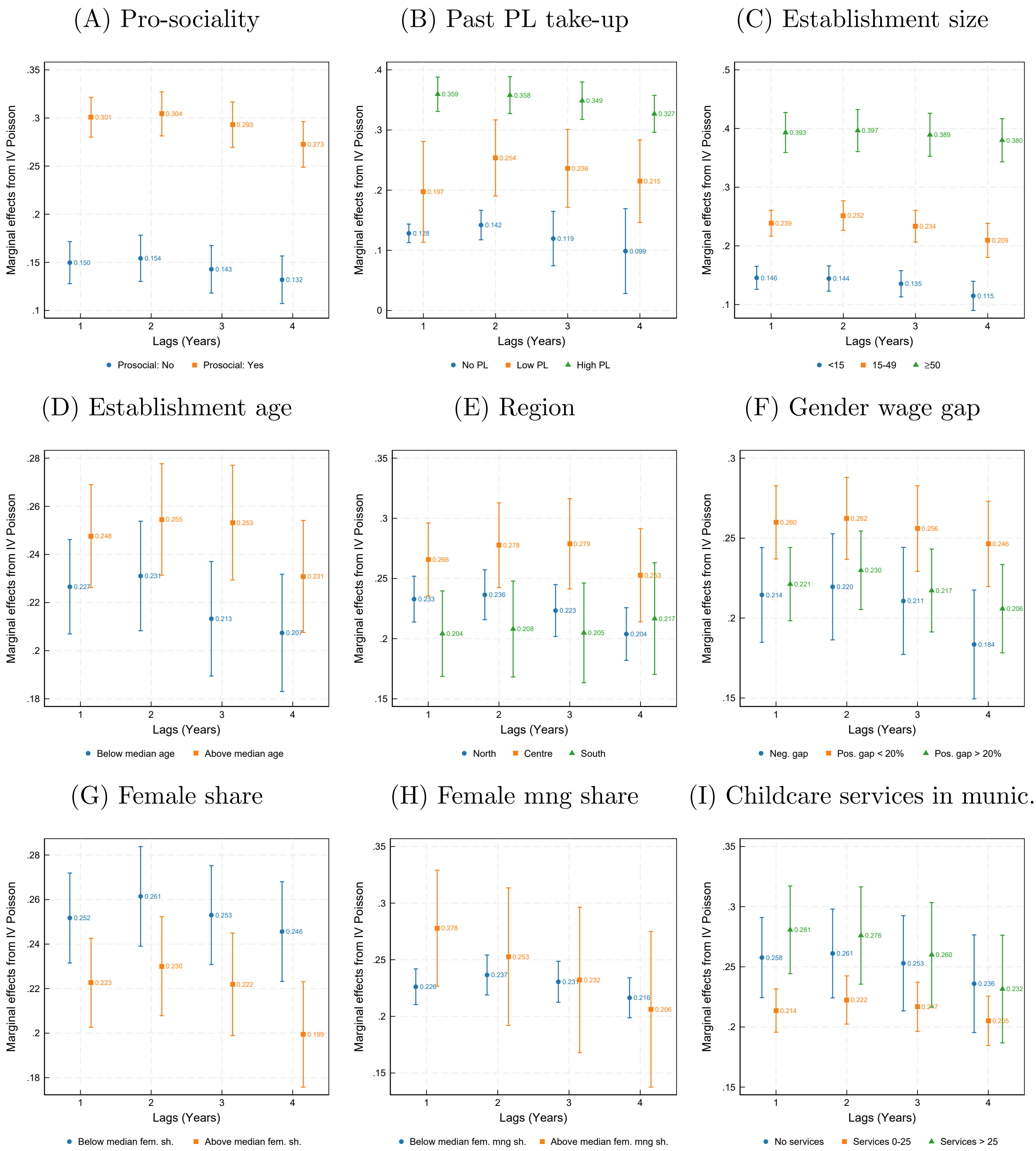
Panel C presents heterogeneity by establishment size, revealing that larger establishments have stronger peer effects. This finding is consistent with the notion that larger establishments, with more complex organizational structures, stronger union presence, and dedicated human resources departments, 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 and 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).

Figure A.20 presents additional heterogeneity analyses by establishment-level characteristics: male and female average earnings (Panels A and B), average age (Panels C and D), temporary contract share (Panels E and F), and white-collar share (Panels G and H). The estimated effects are larger in establishments with an above-median share of men and women with temporary contracts and in those with an above-median share of white-collar female workers.

Across all subgroups, however, the effects remain positive and statistically significant, including for those dimensions that show some residual correlation with the instrument (Table A.3)—such as female earnings, male age, firm size, and the white-collar share—further supporting the validity of our empirical strategy.

Figure 4: Heterogeneity in peer effects



Notes. The figure reports the marginal effects from equation (5), 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 percent confidence intervals obtained from robust standard errors.

6.3 Magnitudes

Table A.5 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 row *a* the total yearly number of parental leaves taken in 2014 by fathers and mothers. It reports in row *b* the average monthly take-up rate in 2014, before the reform, for the control group, i.e., parents of 0-2-year-old children. 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 (27.5 percent) is larger than that for mothers (15.4 percent). 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 10,400 additional parental leaves among fathers and 39,500 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.

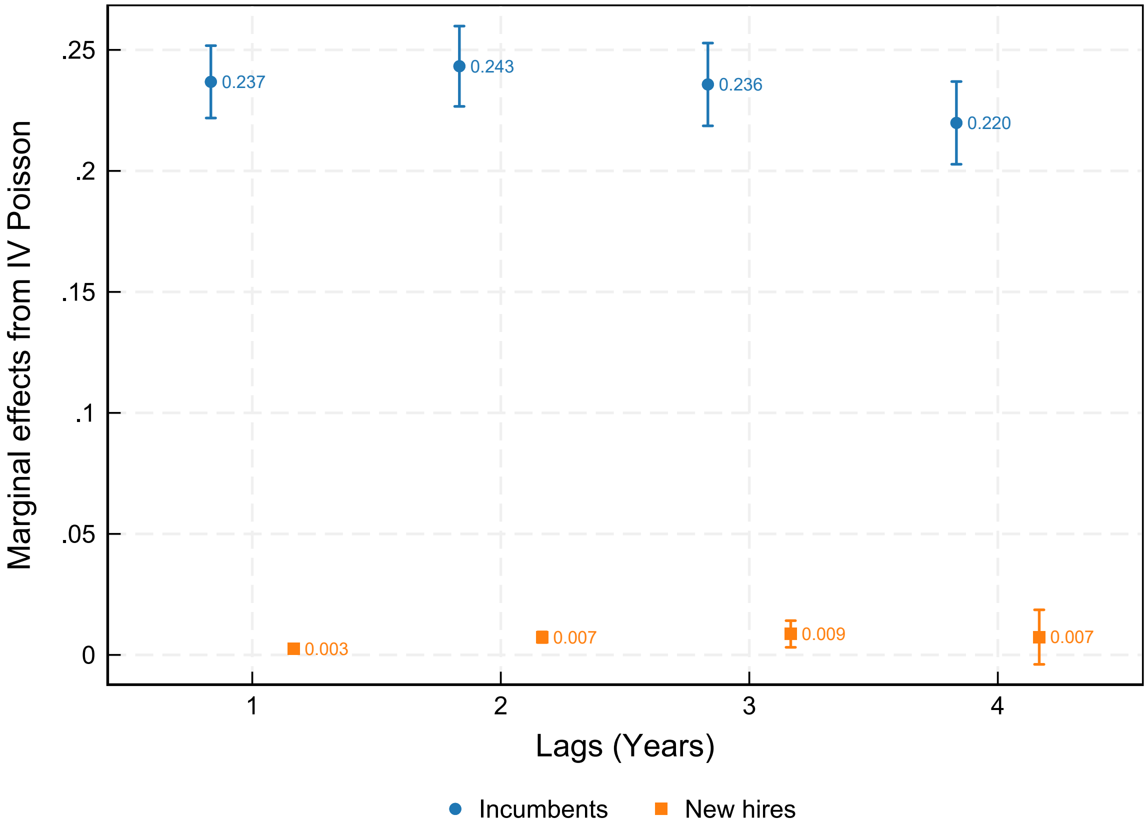
We then compute the additional yearly impact induced by peers by multiplying the direct policy effect (*e*) by the peer effect estimated at lag 1 (*f*; see Figure 2 for fathers and Figure A.12 for mothers). This calculation leads to an additional 246 parental leaves taken by fathers and 1130 taken by 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 provide evidence on peer effects beyond establishment-level effects, by comparing peer effects among incumbent and newly hired workers, and by examining same- and cross-gender peer effects. We then explore peer influences between blue- and white-collar workers to inform on the interplay between peer effects and hierarchies within establishments. Finally, we explore the role of career concerns as a potential mechanism behind peer effects.

Figure 5: Peer effects for incumbent and newly hired coworker fathers



Notes. The figure reports the marginal effects from the augmented equation (5) described in Section 6.4. 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 in blue dots (orange squares) is the share of incumbent (newly hired) coworker fathers 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 percent confidence intervals obtained from robust standard errors.

Peer effects for incumbents and new hires 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. Here we focus on the role of peers, on top of that of establishments, in influencing the effect that we document. To address this, we estimate peer effects among newly hired coworker fathers—workers not employed at the establishment in $t = \{2014, 2015\}$ but hired afterward and becoming fathers in $t + k$. We compare the estimates for these workers to our main peer effects. We expect to find smaller effects for newly hired workers, as they were not directly exposed to the reform-induced increase in take-up by peer fathers. The evidence in Figure 5 is consistent with this hypothesis. The peer effect for new hires is considerably smaller than that estimated for incumbents. Though small, its positive and statistically significant presence over three years is consistent with peer transmission among coworker fathers within the establishment.

Same- and cross-gender peer effects We measure both same-gender and cross-gender peer effects among both fathers and mothers. Our hypothesis is that, if peer influence plays a role, same-gender peer effects should be stronger than cross-gender peer effects. To this end, first, we augment equation (5) 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 (5)—using as outcome parental leave take-up among coworker mothers, in addition to coworker fathers.

Figure 6 reports the estimates. Focusing on parental leave take-up among coworker fathers, we find that the same-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 same-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 same-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 same-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.

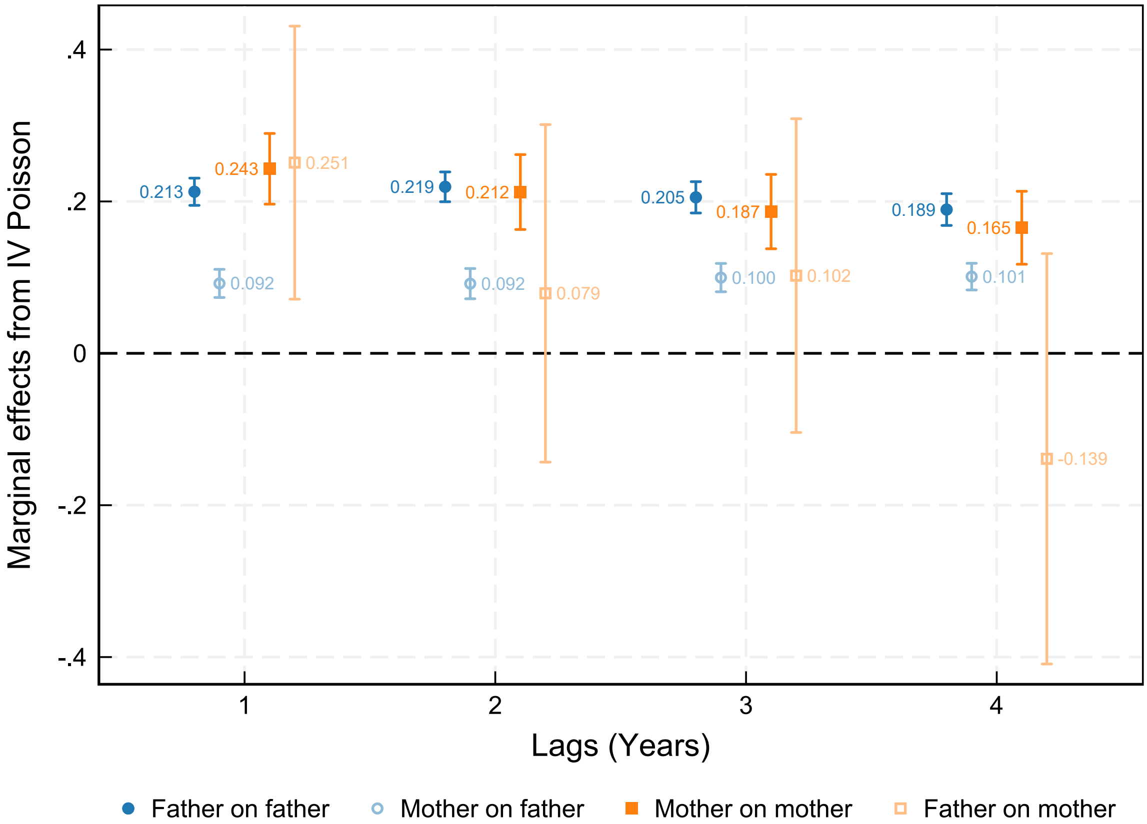
Same- and cross-occupation peer effects To further explore what influences the size and the direction of peer effects, we analyze peer effects within the same occupation and across occupational categories. To this end, we estimate equation (5) separately for coworker fathers' take-up in blue-collar and white-collar occupations, using the take-up of peer fathers within and across each occupational group as explanatory variables.[27] Figure 7 reports the results. Peer effects are larger for blue-collar than white-collar coworker fathers. The strongest peer effects are from higher-ranked peer fathers to lower-ranked coworker fathers (white-collar to blue-collar), consistent with a role model explanation. They are instead the lowest (and not statistically significant at lags 2 to 4) when exerted from blue-collar peer fathers to white-collar coworker fathers. With the exception of lag 1, peer effects within the same occupation are similar in size.

This evidence further reinforces that peer effects operate beyond establishment effects, with the direction of peer influence within the establishment hierarchy shaping the magnitude of the estimates.

Career costs Having established the importance of peer influence in shaping parental leave take-up, we investigate whether perceived career gains or costs associated with

[27] For this analysis, we restrict the sample to establishments with at least a blue-collar and a white-collar worker among both peer and coworker fathers. Hence, the coefficients in the full sample are not directly comparable to those from this analysis.

Figure 6: Same- and cross-gender peer effects: parental leave take-up by coworker fathers and coworker mothers

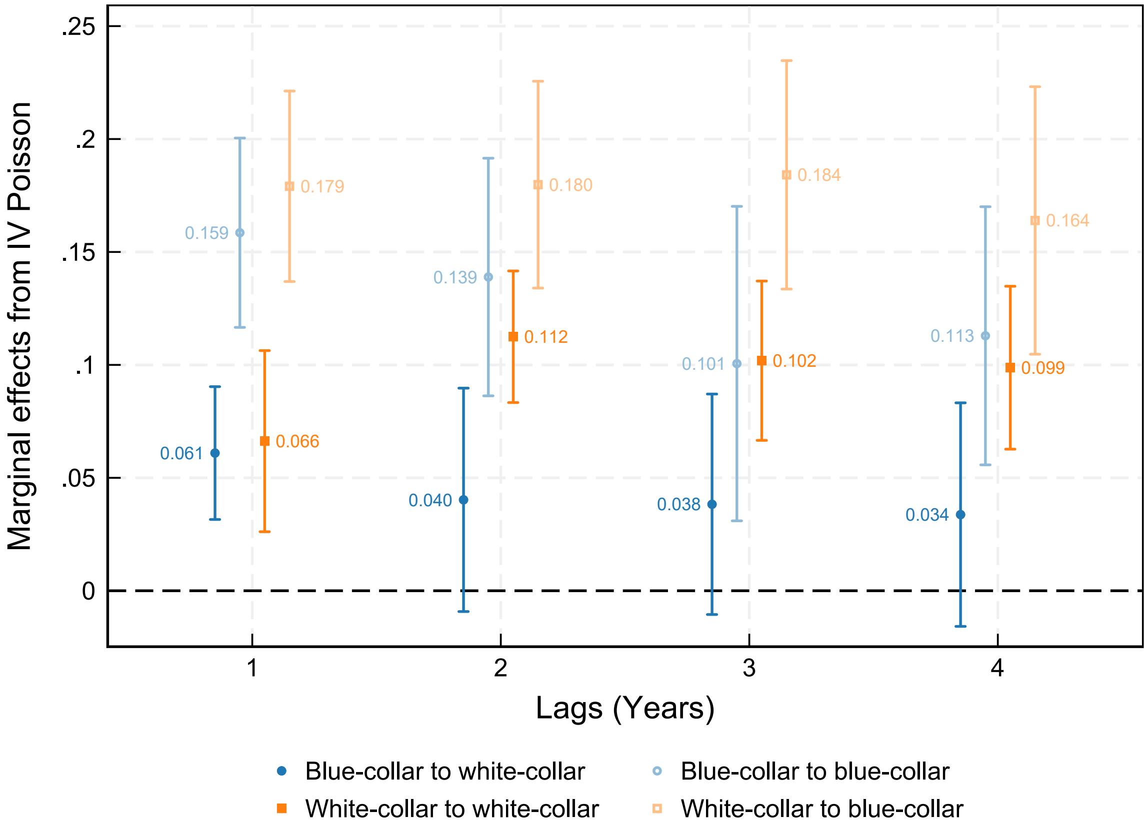


Notes. The figure reports the marginal effects from the augmented equation (5) described in Section 6.4. The coefficients are those on the share of peer fathers (mothers) of children between 3 and 5 years old on total fathers (mothers) of children between 0 and 5 years old in the establishment taking 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 percent confidence intervals obtained from robust standard errors.

leave might explain these 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 in 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 signal, influencing coworkers' perceptions of the potential career implications of taking 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.

Figure 7: Same- and cross-occupation peer effects: parental leave take-up by coworker fathers in blue- and white-collar jobs



Notes. The figure reports the marginal effects from the augmented equation (5) described in Section 6.4. The coefficients are those on the share of white-collar (blue-collar) peer fathers of children between 3 and 5 years old on total white-collar (blue-collar) 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 in dark (light) blue dots and orange squares is the share of white-collar (blue-collar) coworker fathers 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 percent confidence intervals obtained from robust standard errors.

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.2). 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. 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 us to analyze the responses of both male and female partners.

Figure 8 reports the estimates of equation (6), with the coefficients capturing the effect of a 1 percent increase in peer parents taking parental leave on the labor market outcomes of their coworkers' partners.[28] 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 400 and 500 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).

8 Conclusions

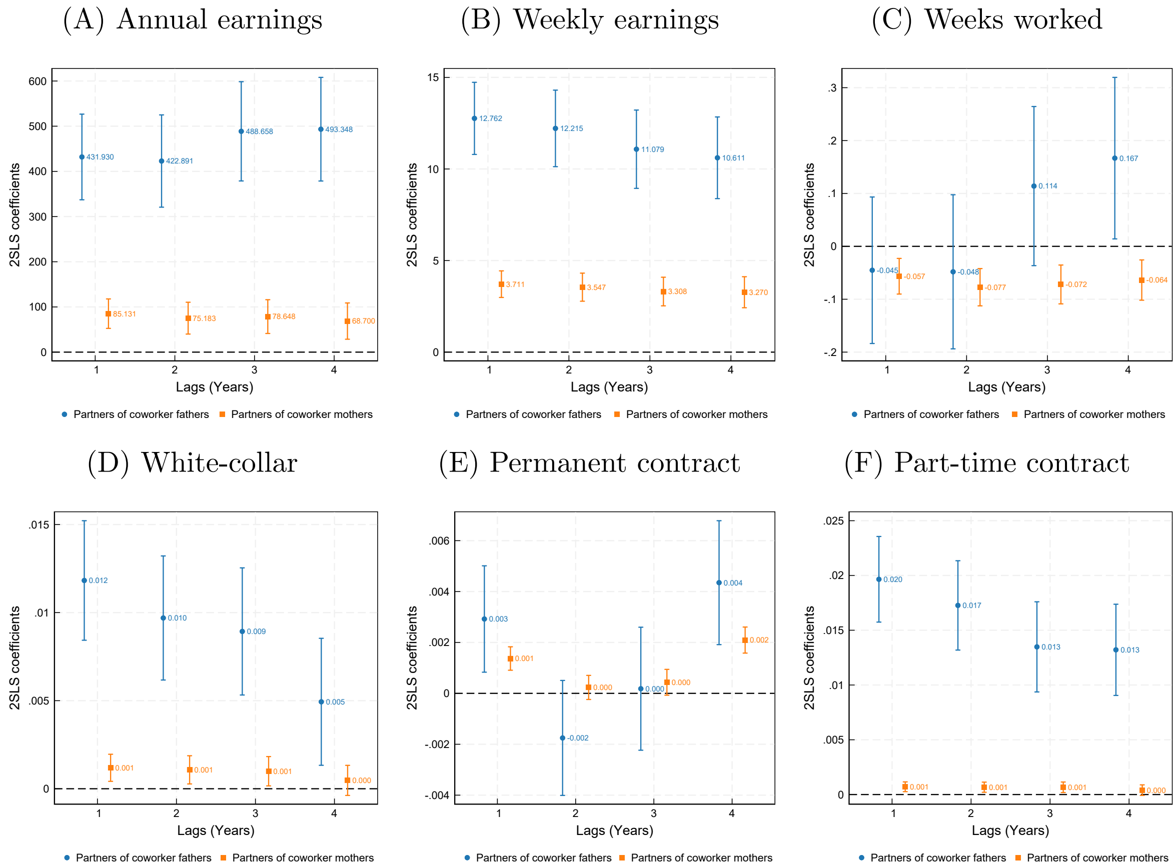
This study investigates the role of workplace peer effects in shaping fathers' take-up of parental leave, leveraging a reform of the Italian parental leave system. By analyzing administrative data that link employees, their workplaces, and their households, we identify and quantify the influence of peer 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 27.5 percent 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, 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

[28] This change in the independent variable is sizable, as it corresponds to approximately 40 percent of a standard deviation for fathers.

Figure 8: Effect of a 1 percent 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 (6). 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 percent confidence intervals obtained from robust standard errors.

post-reform. Peer effects are stronger in establishments 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 the effects are driven by incumbent coworkers, and that same-gender peer effects are more pronounced than cross-gender ones, reflecting the importance of workplace networks in determining parental leave adoption. Also, the establishment hierarchy matters, as the strongest peer effects run from higher- to lower-ranked occupations. In addition, our analysis finds no evidence of adverse career impacts for fathers eligible for 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 peer dynamics can amplify the impact of parental leave policies, as parental leave take-up within establishments helps normalize its use among fathers. Unlike previous studies, our setting reveals that—albeit both exist—same-gender peer effects are stronger than cross-gender ones, suggesting that gender-specific interactions play a key role in peer influence. Leveraging social dynamics in the workplace can promote policy effectiveness and extend to households, with better labor market outcomes for women.

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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: Aggregate first stage regression

	(1) Fathers	(2) Mothers
Coefficient	0.011***	0.028***
	(0.001)	(0.003)
Observations	1,470,854	1,083,034

Notes. The table reports the first-stage estimates from equation (2) for fathers and mothers in columns 1 and 2, respectively. The dependent variable is a dummy equal to one for individuals taking parental leave. The coefficient reported is the of the interaction between the treatment dummy (equal to one for parents 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). Cluster-robust standard errors at the worker level are reported in parentheses. Significance levels: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.2: 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.
Panel A: Reduced form								
	1-year horizon							
Coefficient	-0.0021**	-0.0004	-258.2***	-0.57**	-0.0026***	-0.0011***	-0.0000	0.0007
	(0.0009)	(0.0015)	(33.8)	(0.22)	(0.0009)	(0.0003)	(0.0000)	(0.0004)
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	0.0102***	-0.0014	-179.5***	0.56**	-0.0014*	-0.0007**	0.0000	0.0008*
	(0.0010)	(0.0014)	(36.2)	(0.22)	(0.0008)	(0.0003)	(0.0000)	(0.0004)
Observations	1,470,855	929,518	976,844	976,844	929,518	929,518	929,518	929,518
Panel B: 2SLS								
	1-year horizon							
Coefficient	-0.0268	-0.0162	-9404.2***	-20.78**	-0.0979***	-0.0411***	-0.0000	0.0271
	(0.0315)	(0.0571)	(1537.9)	(8.37)	(0.0349)	(0.0125)	(0.0007)	(0.0167)
Observations	1,083,034	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	0.5478***	-0.0481	-6518.0***	20.50**	-0.0493	-0.0247**	0.0005	0.0277*
	(0.0685)	(0.0492)	(1468.0)	(8.35)	(0.0300)	(0.0117)	(0.0008)	(0.0147)
Observations	1,083,034	929,518	976,844	976,844	929,518	929,518	929,518	929,518

Notes. The table reports estimates of equation (3) in the reduced form (panel A) and in 2SLS (panel B) for peer mothers. The coefficient reported corresponds to 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) in panel A, and to that on the predicted parental leave take-up in panel B. Both panels report the effects for $\tau = 1$ and $\tau = 2$. 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: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.3: 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 (4), 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.4: Establishment-level effects of the reform on peer mothers' take-up

	(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

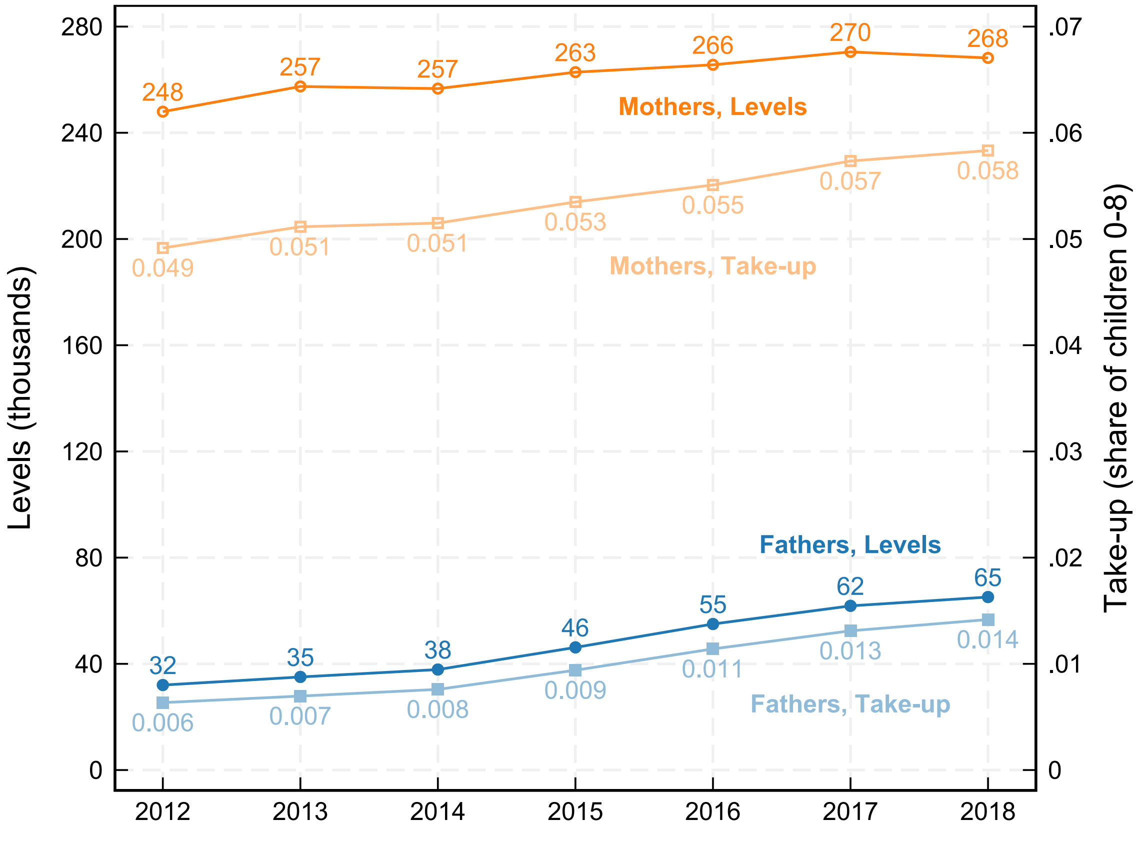
Notes. The table reports estimates of equation (4) at the establishment level for mothers. "Sh. el." and "Sh. eligible peer mothers" indicate the share of peer mothers of children between 3 and 5 years old on total mothers of children between 0 and 5 years old in the establishment in 2014. The dependent 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 establishment size, share of female workers, 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.5: 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 (%)	2.4	2.9
		[2.2, 2.5]	[2.5, 3.2]
$g = f * e$	**Indirect policy effect through peers** (N)	**246**	**1130**
		[146, 358]	**[662, 1696]**

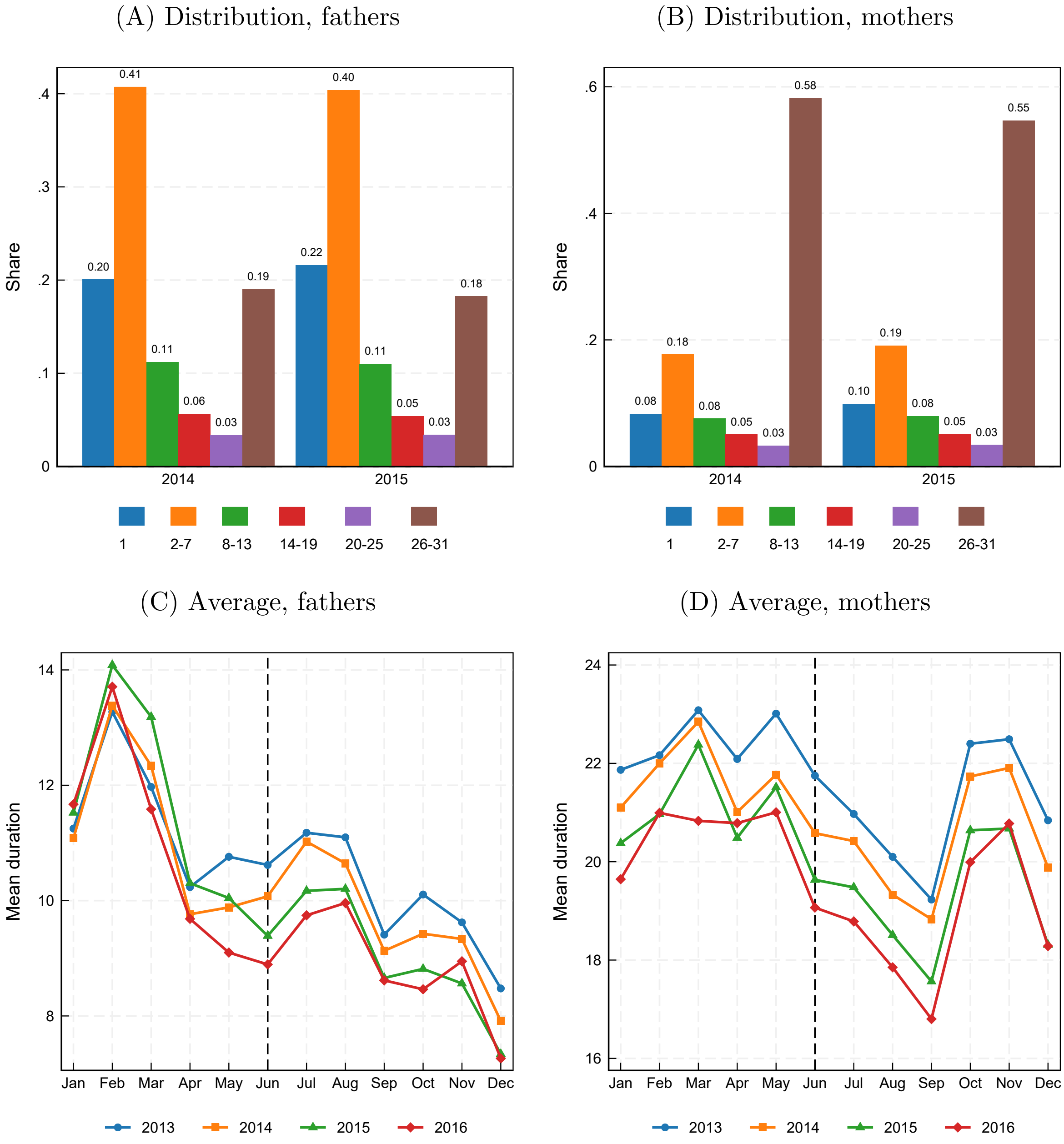
Notes. The table reports a back-of-the-envelope calculation of the magnitudes of the effects reported in the text for fathers and mothers. Row a is taken from Figure A.1 and refers to 2014. Row b is the average take-up rate in the control group (parents of 0-2-year-old children) in 2014. Row c is the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

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



Notes. The figure reports the number of parental leave episodes requested in INPS data between 2012 and 2018, and the parental leave take-up, measured by dividing the number of parental leaves taken by the number of children between 0 and 8 years old in each year (sourced from Italian National Statistical Institute, ISTAT).

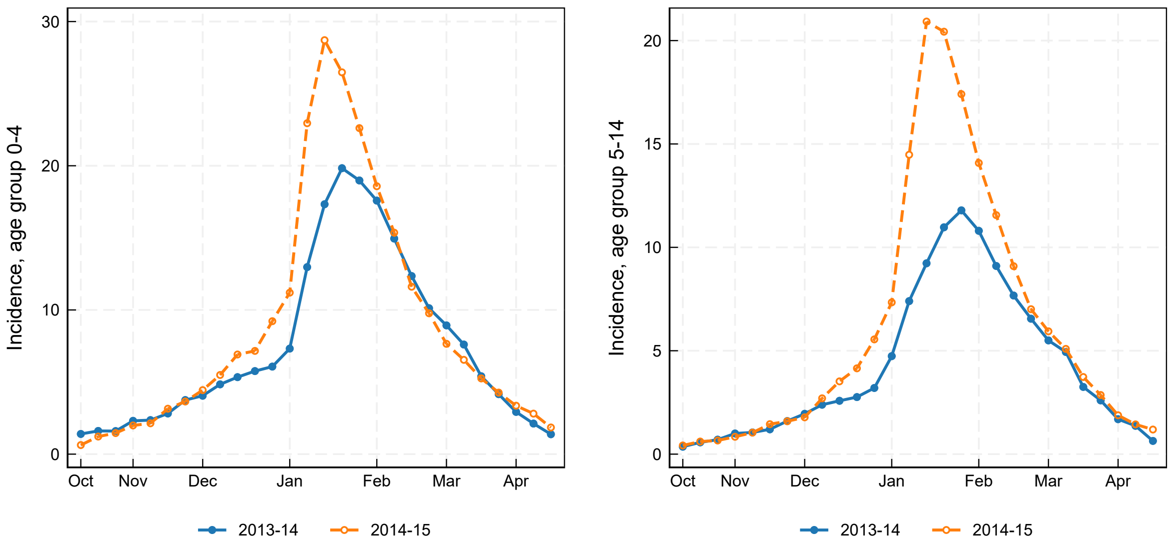
Figure A.2: Parental leave duration in days



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

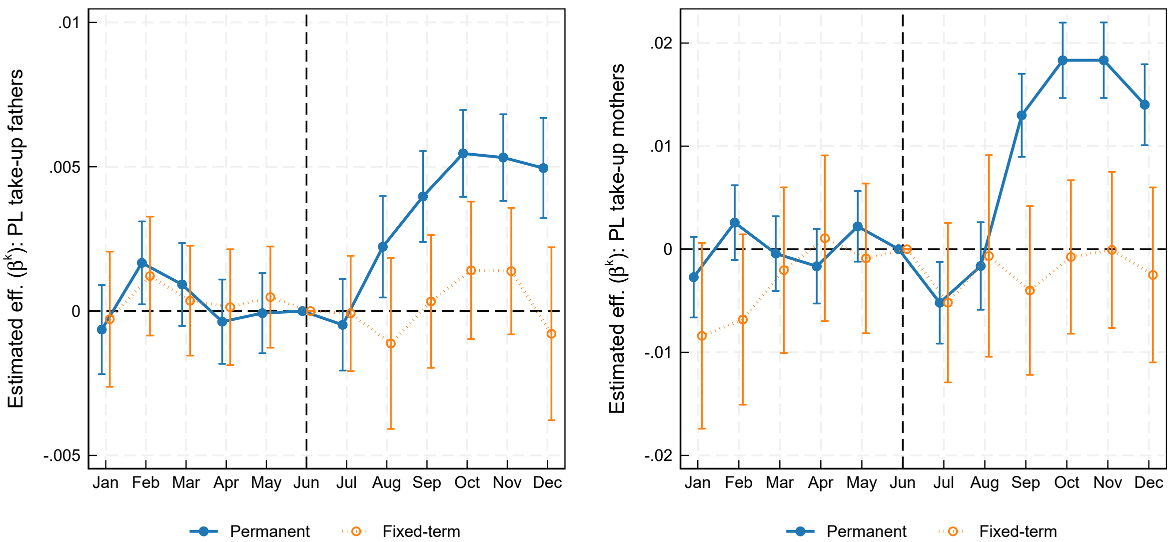
(A) Age group 0-4 (B) Age group 5-14



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

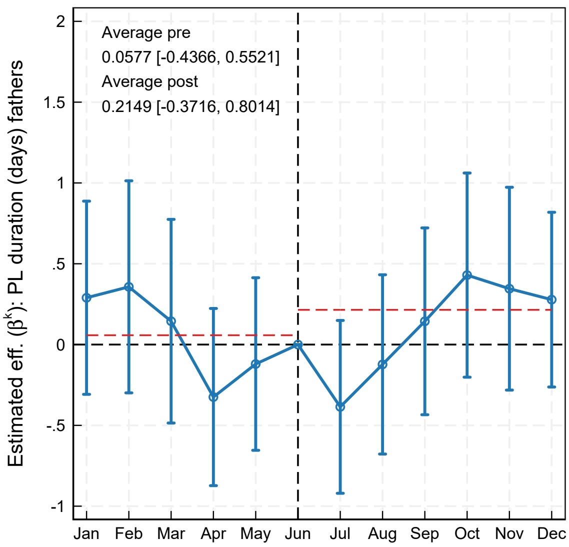
(A) Fathers (B) Mothers



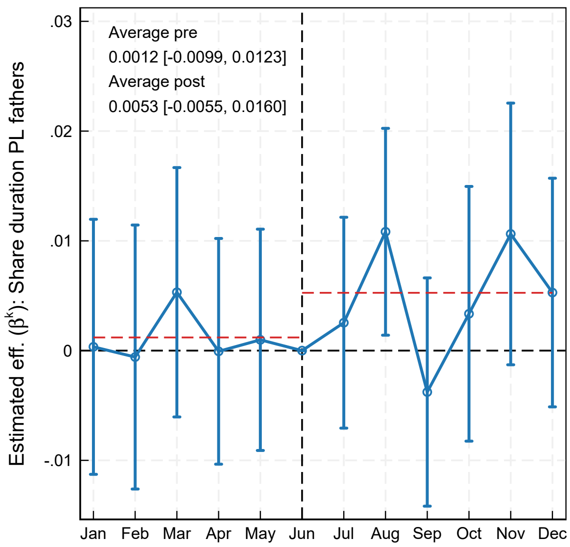
Notes. The figure reports event study coefficients from equation (1) on the triple interaction between the treatment dummy (having a child between 3 and 5 years old), the year dummy (equal to one for 2015) and the calendar month dummies (June used as a reference), separately for workers holding permanent 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 percent 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



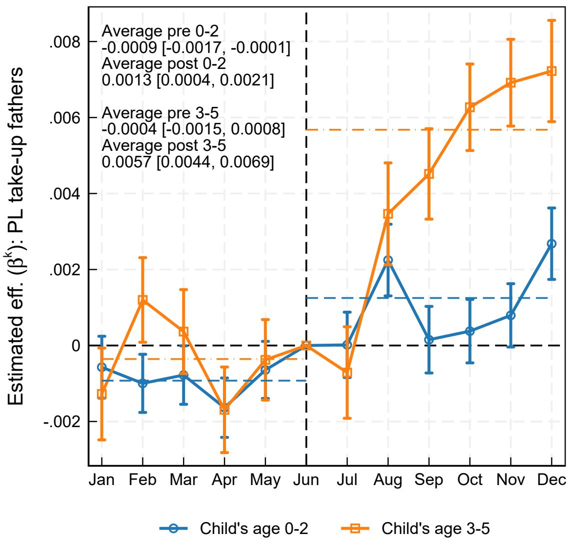
(B) Share of household PL duration



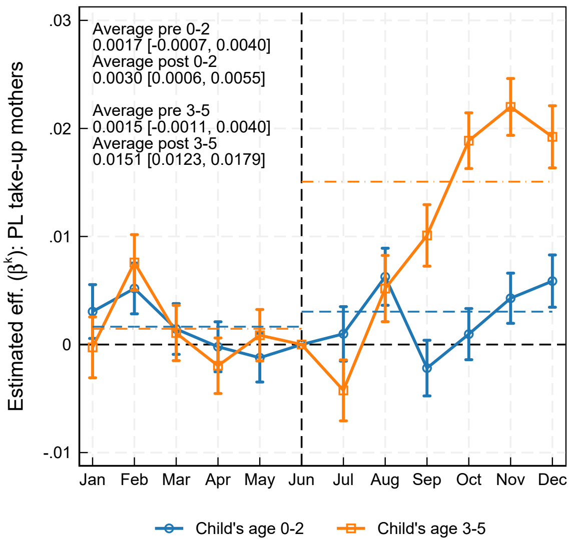
Notes. The figure reports event study coefficients from equation (1) on the triple interaction between the treatment dummy (having a child between 3 and 5 years old), the year dummy (equal to one for 2015) and the calendar month dummies (June used as a reference). The dependent variable is the duration in days of parental leave for fathers in Panel A 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 percent 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, which we also report alongside their confidence intervals.

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

(A) Fathers

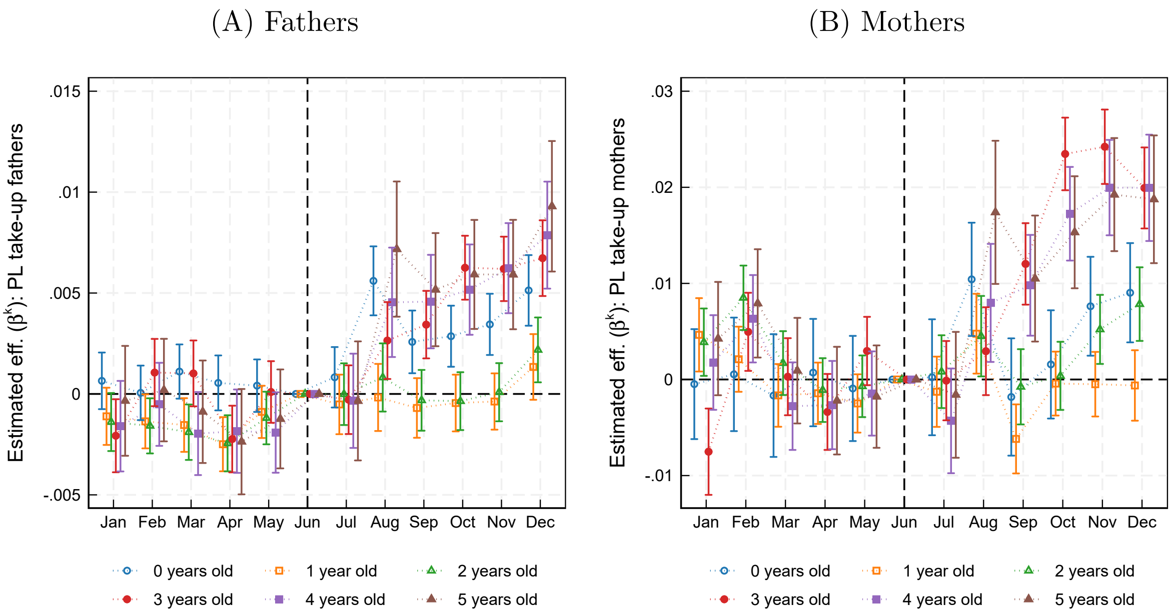


(B) Mothers



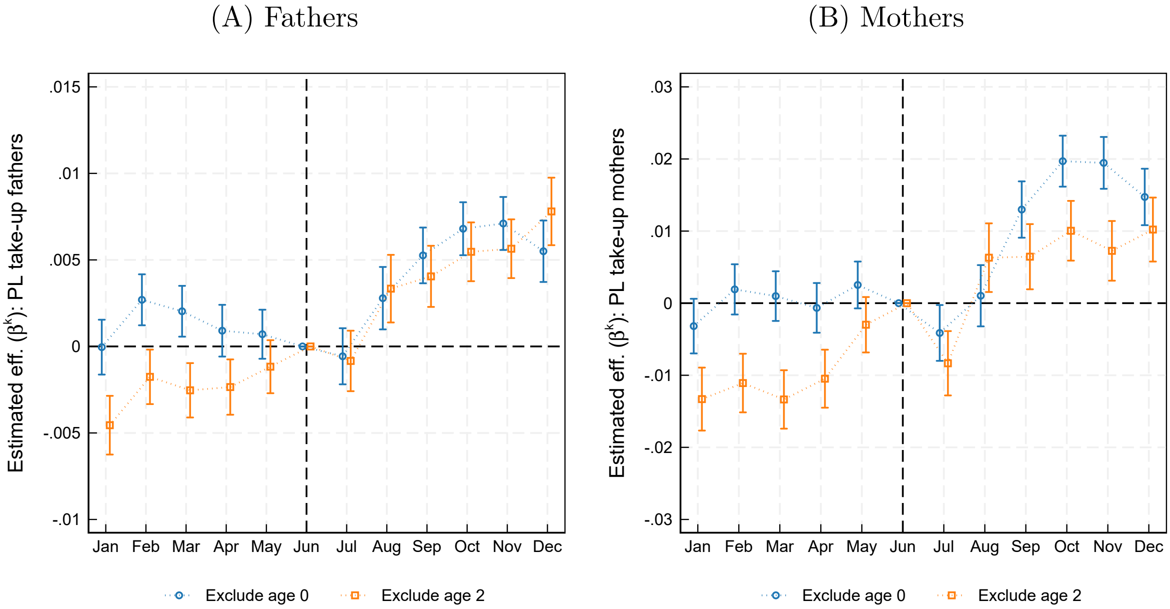
Notes. The figure reports event study coefficients from a difference-in-differences specification on the year dummy (equal to one for 2015) and the calendar month dummies (June used as a reference), separately for workers with children in different age groups in June of each year. The dependent variable 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 percent 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, which we also report alongside their confidence intervals.

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



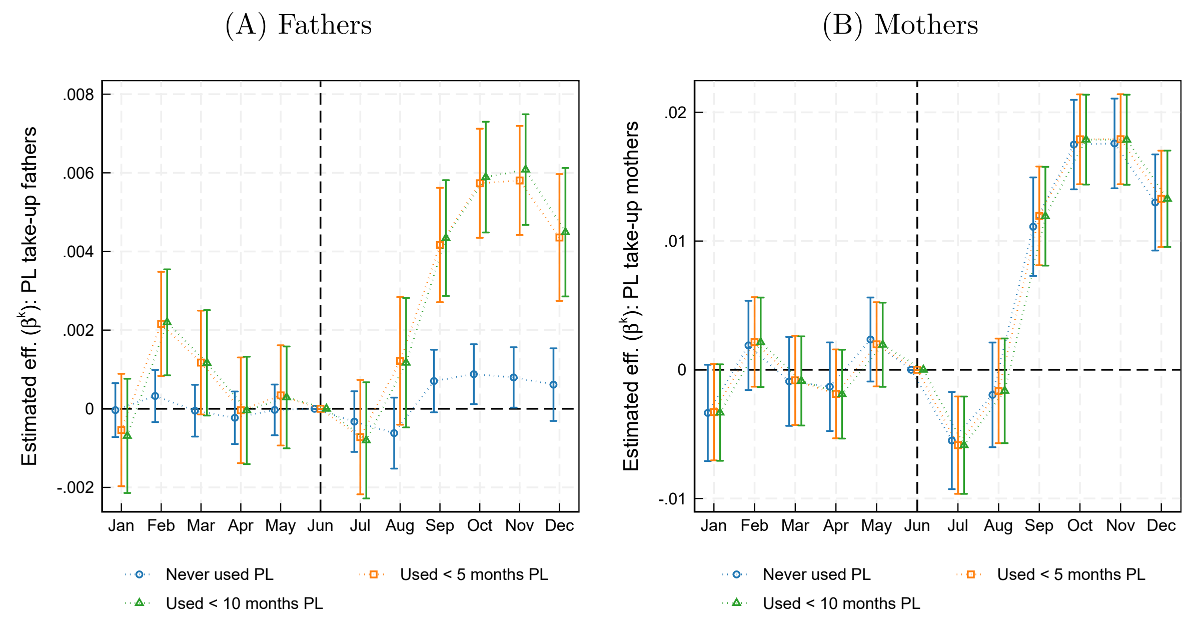
Notes. The figure reports event study coefficients from a difference-in-differences specification on the year dummy (equal to one for 2015) and the calendar month dummies (June used as a reference), separately for workers with children of different ages in June of each year. The dependent variable is 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 percent 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



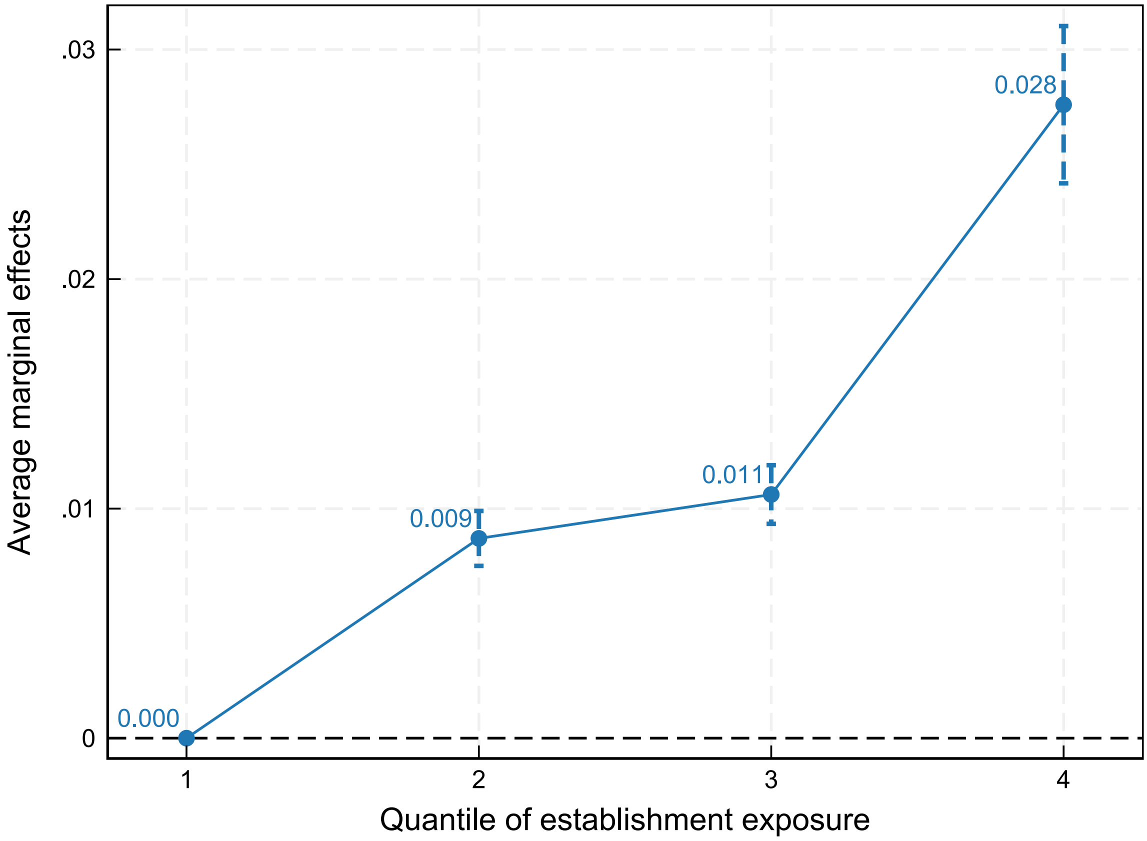
Notes. The figure reports event study coefficients from equation (1) on the triple interaction between the treatment dummy (having a child between 3 and 5 years old), the year dummy (equal to one for 2015) and the calendar month dummies (June used as a reference). The estimates are conducted after excluding 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 percent confidence intervals obtained from cluster-robust standard errors at the worker level.

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



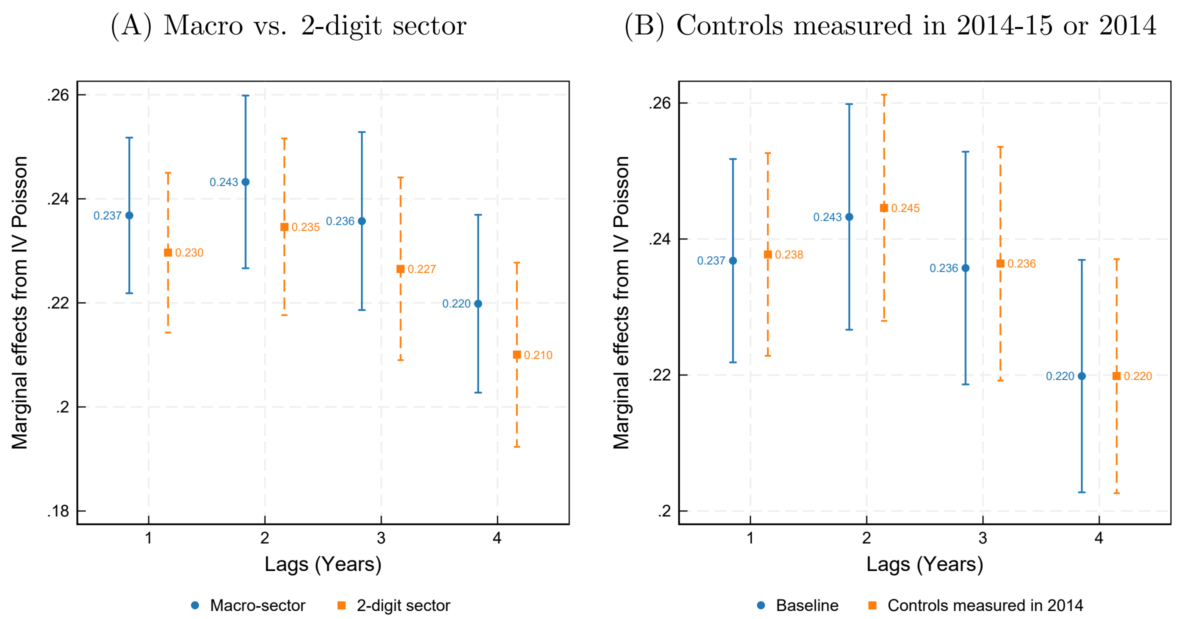
Notes. The figure reports event study coefficients from equation (1) on the triple interaction between the treatment dummy (having a child between 3 and 5 years old), the year dummy (equal to one for 2015) and the calendar month dummies (June used as a reference). The estimates are conducted after keeping only 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 percent confidence intervals obtained from cluster-robust standard errors at the worker level.

Figure A.10: Testing the monotonicity of the instrument



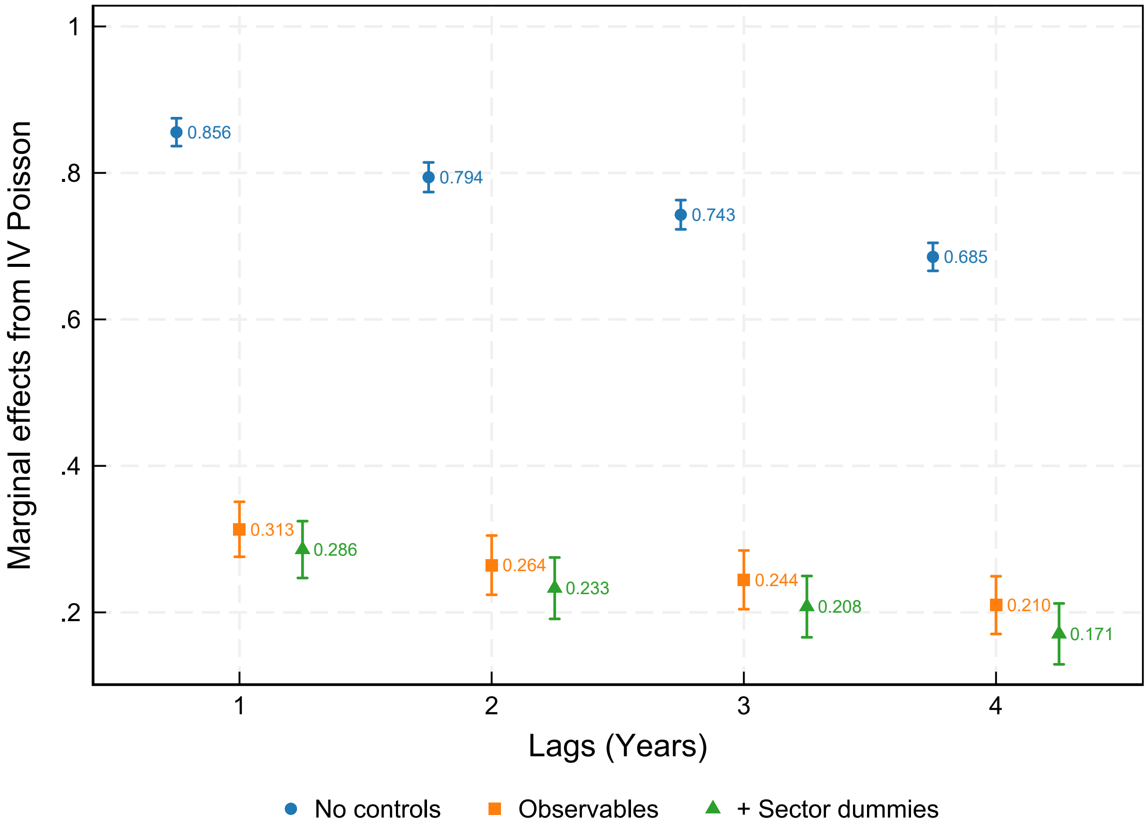
Notes. The figure reports the average marginal effects of a regression of the establishment-level share of fathers of 3-5-year-old children on total fathers of 0-5-year-old children taking parental leave, on four quantiles of the instrument measuring the establishment potential exposure to the reform,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



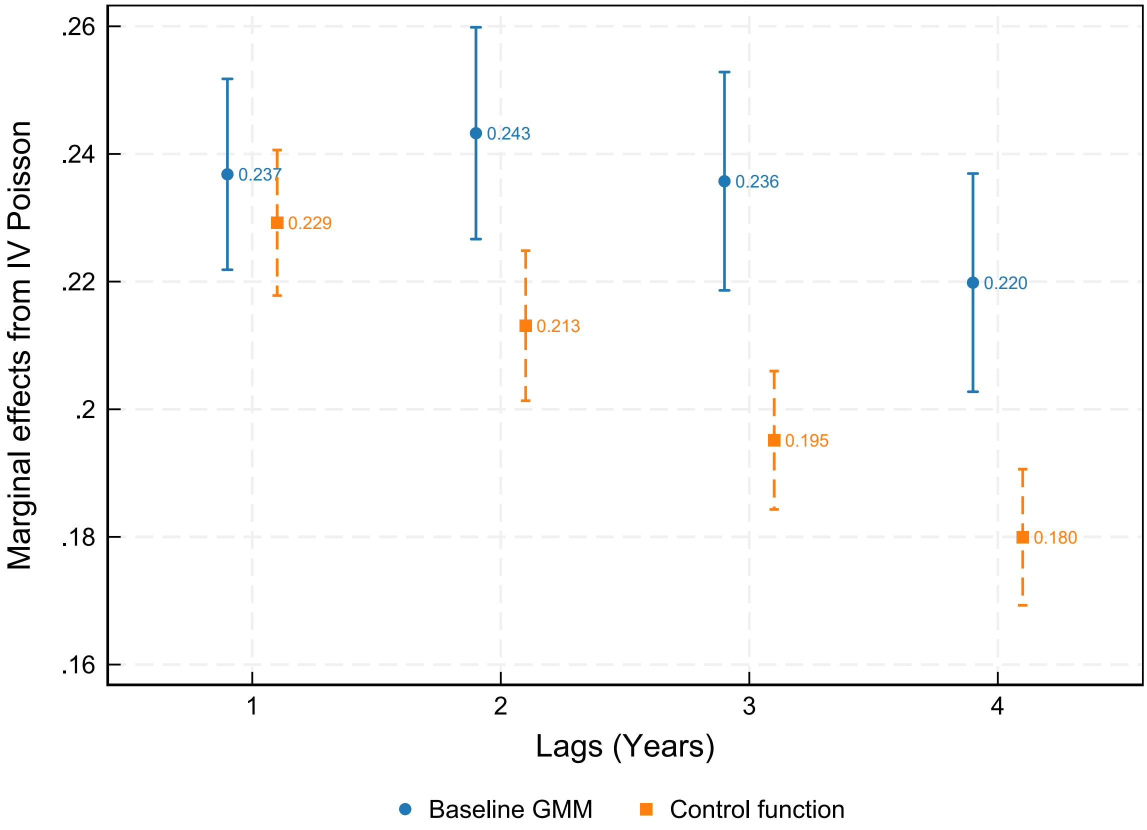
Notes. The figure reports the marginal effects from equation (5). The coefficients are those on the share of peer fathers of children between 3 and 5 years old on total fathers of children between 0 and 5 years old in the establishment taking parental leave between August and December of either 2014 or 2015. The dependent variable is the share of coworker fathers taking parental leave at lag 1 to 4. In Panel A, 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. In Panel B, we compare the baseline estimates (Figure 2) that use controls averaged over 2014 and 2015 with estimates conditional on control measured in 2014 only. Vertical lines are 95 percent confidence intervals obtained from robust standard errors.

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



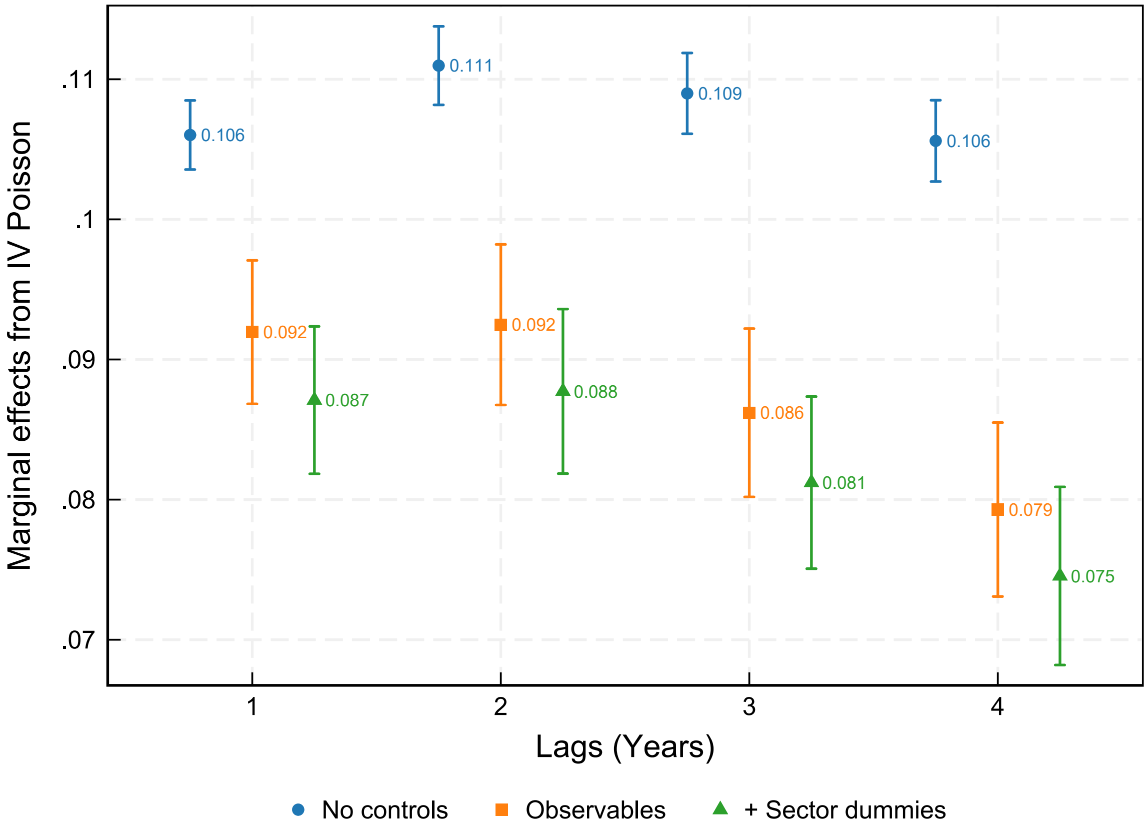
Notes. The figure reports the marginal effects from equation (5). The coefficients are those on the share of peer mothers of children between 3 and 5 years old on total mothers of children between 0 and 5 years old in the establishment taking parental leave between August and December of either 2014 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 establishment size, share of female workers, 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 percent confidence intervals obtained from robust standard errors.

Figure A.13: Peer effects estimated with GMM or control function



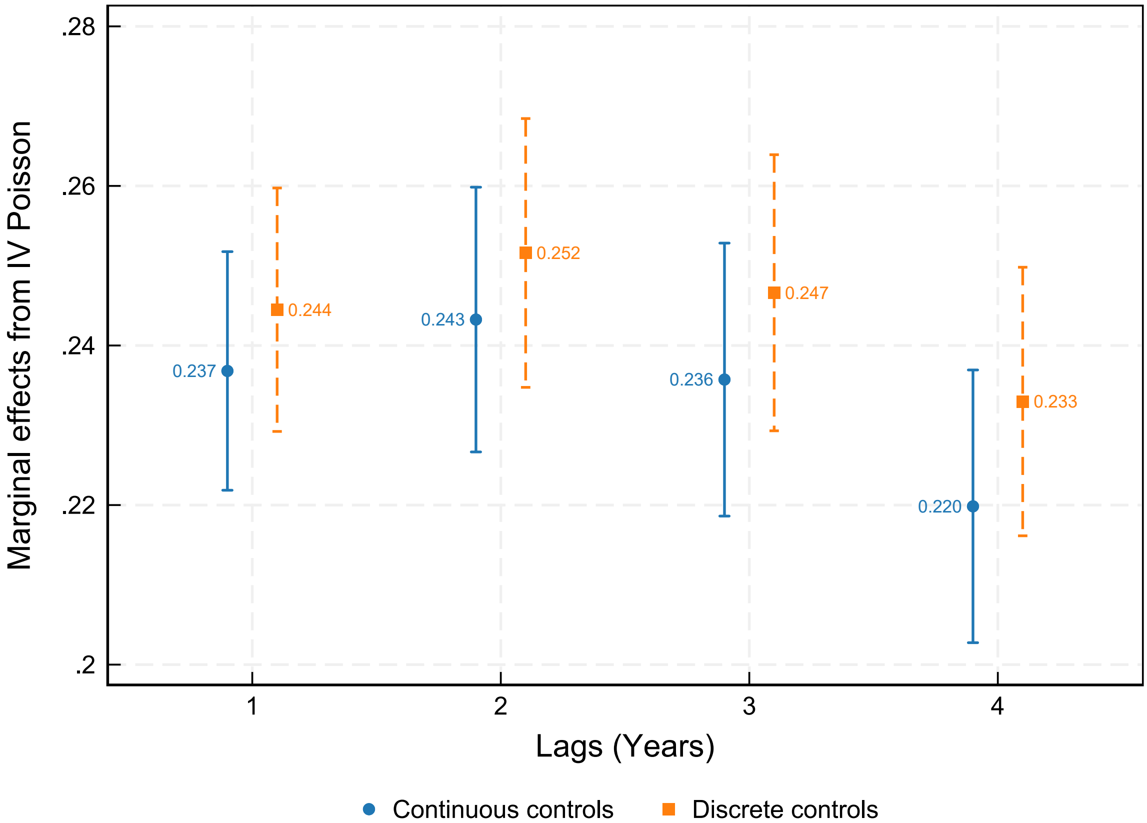
Notes. The figure reports the marginal effects from equation (5) estimated with GMM or the control function approach. The coefficients are those on the share of peer fathers of children between 3 and 5 years old on total fathers of children between 0 and 5 years old in the establishment taking parental 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 percent confidence intervals obtained from robust standard errors.

Figure A.14: Peer effects with binary indicators



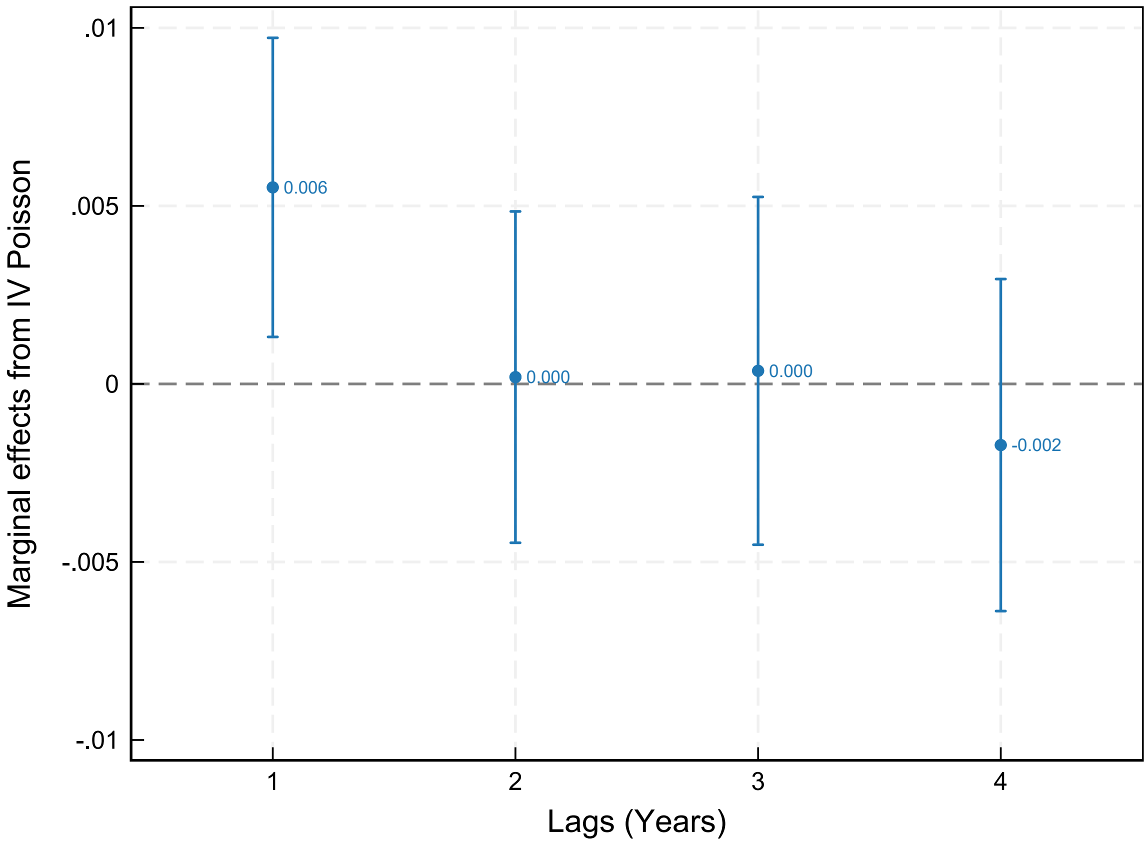
Notes. The figure reports the marginal effects from equation (5) with binary indicators instead of continuous variables. Specifically, the coefficients are those on a dummy equal to 1 if the share of peer fathers of children between 3 and 5 years old on total fathers of children between 0 and 5 years old in the establishment, taking parental leave between August and December of either 2014 or 2015, is greater than 0. The dependent variable is a dummy equal to 1 if the share of coworker fathers taking parental leave at lag 1 to 4 is greater than 0. The regressions control for observables and macro-sector dummies. Vertical lines are 95 percent confidence intervals obtained from robust standard errors.

Figure A.15: Peer effects with continuous or discrete control variables



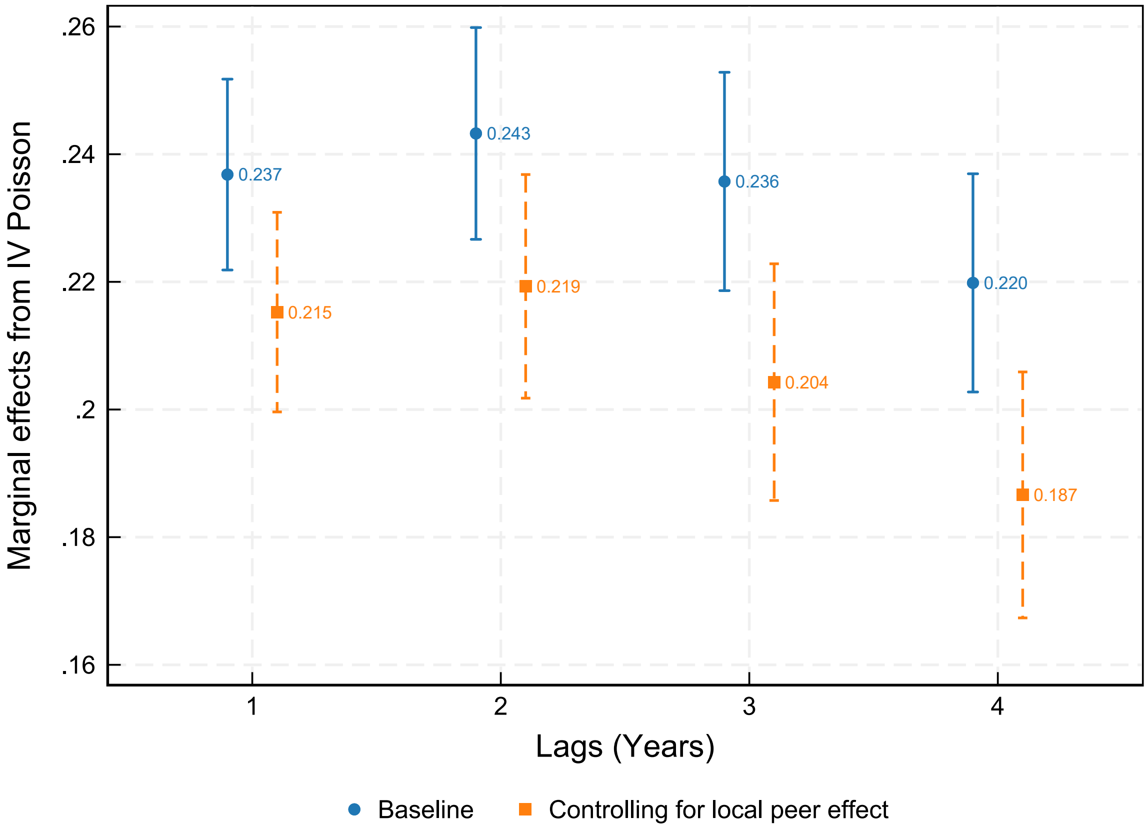
Notes. The figure reports the marginal effects from equation (5) where the observable controls are included as continuous variables (as in the main text) or as discrete categorical variables, corresponding to quartiles of the respective distributions. The coefficients are those on the share of peer fathers 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 macro-sector dummies. Vertical lines are 95 percent confidence intervals obtained from robust standard errors.

Figure A.16: Peer effects on fertility among fathers



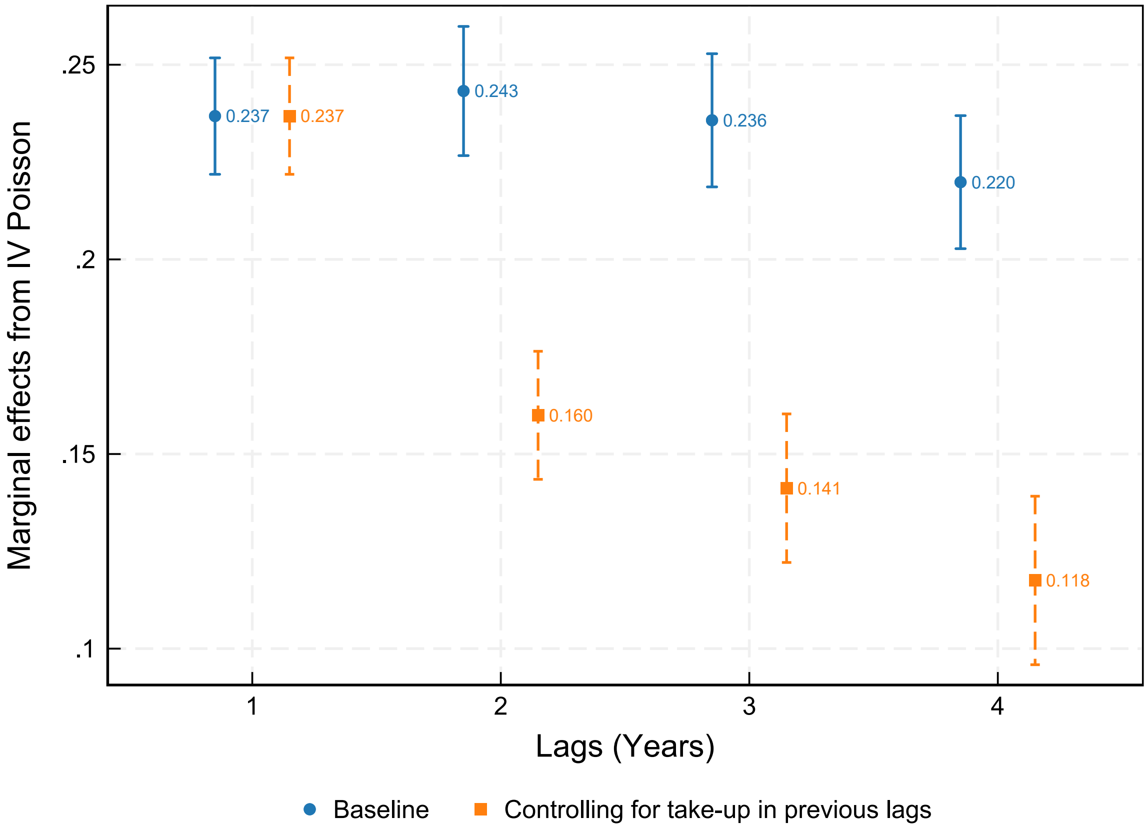
Notes. The figure reports the marginal effects from equation (5). The coefficients are those on the share of peer fathers of children between 3 and 5 years old on total fathers of children between 0 and 5 years old in the establishment taking parental leave between August and December of either 2014 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 percent confidence intervals obtained from robust standard errors.

Figure A.17: Peer effects controlling for local factors



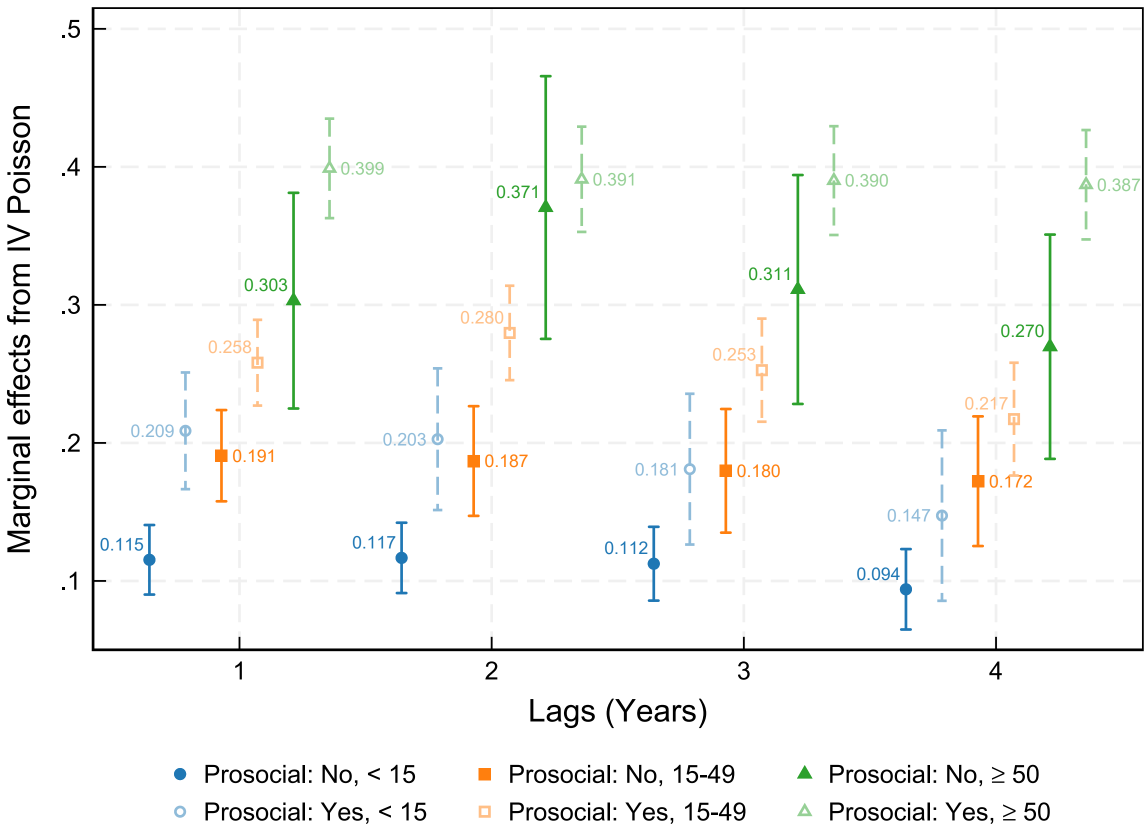
Notes. The figure reports the marginal effects from equation (5) for the "Baseline" estimate. The "Controlling for local peer effect" estimate includes as a control the share of peer fathers in the same municipality who are employed in a different establishment taking parental leave, instrumented with the analogous 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 percent confidence intervals obtained from robust standard errors.

Figure A.18: Peer effects controlling for past coworkers' parental leave take-up



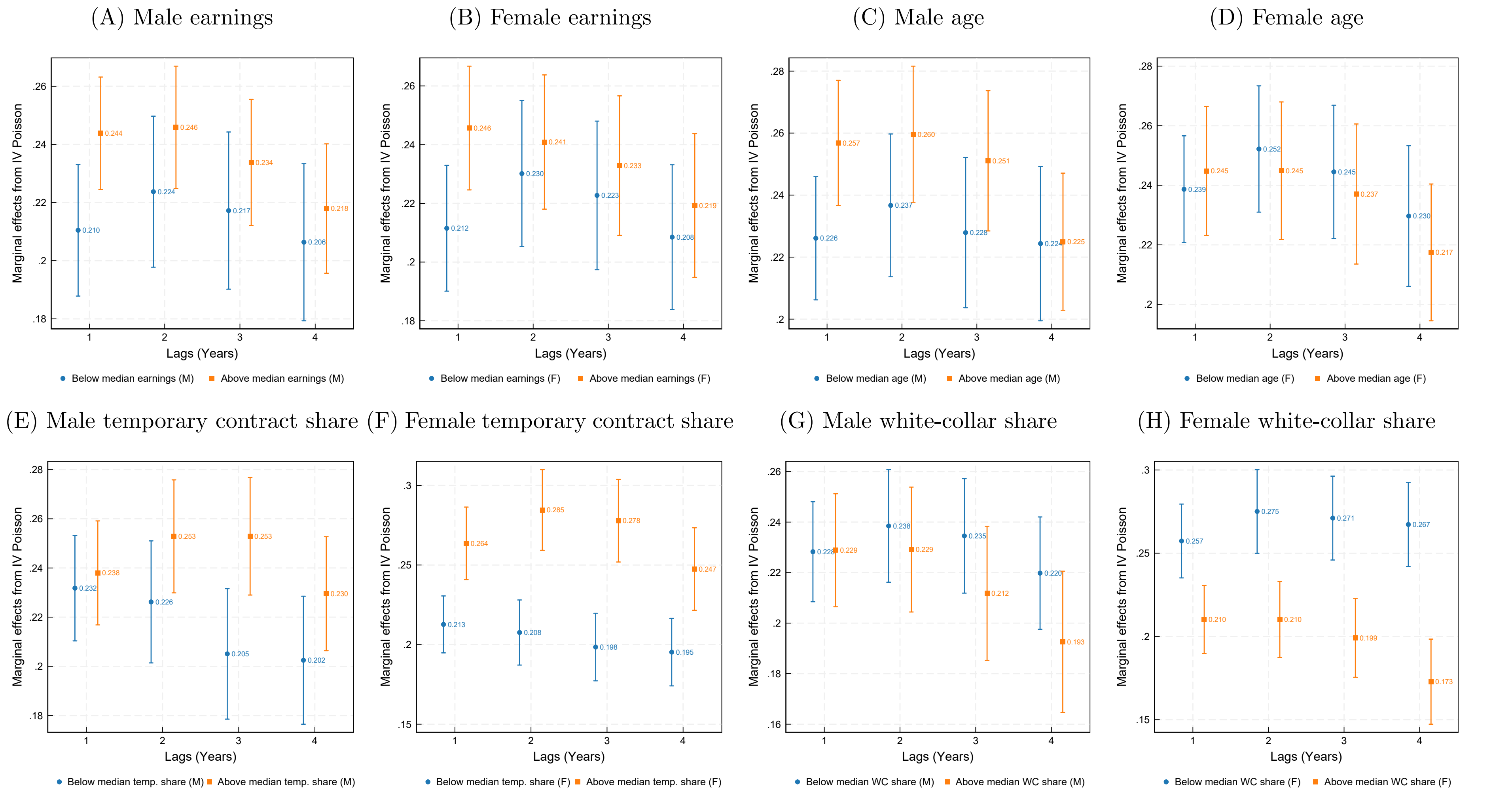
Notes. The figure reports the marginal effects from equation (5). At lags $k > 1$ the right hand side is augmented with the share of coworker fathers taking parental leave up to $k-1$. The coefficients are those on the share of peer fathers of children between 3 and 5 years old on total fathers of children 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 percent confidence intervals obtained from robust standard errors.

Figure A.19: Heterogeneity in peer effects by establishment size and prosociality



Notes. The figure reports the marginal effects from equation (5), separately for subgroups based on establishment size and prosociality (measured with the presence of blood donors in the past). 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 percent confidence intervals obtained from robust standard errors.

Figure A.20: Heterogeneity in peer effects, additional subgroups



Notes. The figure reports the marginal effects from equation (5), 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 percent confidence intervals obtained from robust standard errors.