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MEASURING PEER EFFECTS IN PARENTAL LEAVES: EVIDENCE FROM A REFORM

by Davide Dottori*, Francesca Modena** and Giulia Martina Tanzi***

Abstract

In this paper we estimate peer effects in parental leaves (PLs), analyzing whether mothers' choices may be influenced by prior decisions made by their female colleagues. We identify peer effects through an exogenous variation in the probability that peers take a PL driven by a reform implemented in Italy in 2015 which extended the time period over which parents can receive a paid PL, providing greater flexibility in its use. We focus on post-reform mothers and exploit the heterogeneity in the share of their peers who, due to their children's age, have been affected by the reform. Our findings show the existence of important peer effects: a 10 percentage point increase in the share of peers that took a PL in response to the reform results in mothers being 2.4 percentage points more likely to take a PL. We also find a positive effect on the amount of PLs taken and a negative effect on the probability of working part-time. As suggested by the heterogeneity analysis, signalling about employers' reaction to the use of PLs might be an important channel through which peer effects unfold.

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1. Introduction[1]

Despite the worldwide decline of gender gaps in education and in labor force participation, one of the most important hurdles on the path toward gender equality refers to the changes in the labor supply decisions when women become mothers (Bertrand, 2020). The negative impact of the birth of a child on mothers' labor market partecipation and on earnings, the so called "motherhood penalty", has been demonstrated over the past few years by a number of studies (Angelov et al., 2016; Lundborg et al., 2017; Kleven et al., 2019). This penalty comes from three factors: the reduction in the labor force participation, in the hours of work, and the decrease in the hourly earnings. Family policies, especially those that concern maternal and parental leave (ML and PL, respectively), may favor women's attachment to the labor market during the temporary interruptions of employment related to the motherhood, while retaining firm-specific or occupation-specific human capital (Olivetti and Petrongolo, 2017).[2]

In lights of these facts, policy discussions surrounding parental leave benefits and their use have become more prominent. The mothers' choices about PLs can be affected, among others, by their peers' decisions, with direct effects on the take-up and on the effectiveness of these programs. Individuals are embedded within networks of relationships, such as families, coworkers, neighbors and friends. The influence of these social interactions on labor supply has been proved important, because of information transmission and imitation, and may also help explaining differences on female labor supply and parental leave decisions between countries (Del Boca et al., 2000; Weinberg et al., 2004; Mas and Moretti, 2009; Fogli and Veldkamp, 2011; Cornelissen et al., 2017; Nicoletti et al., 2018; Welteke and Wrohlich, 2019).

This paper contributes to the growing literature on how peer effects influence individual behavior in the context of PLs. Specifically, we estimate the peer effects on whether and how frequently a mother takes PLs, focusing on the role of colleagues in the workplace and considering the share of peers who took a PL as our explanatory variable of interest for the peer effects. We focus only on mothers since in Italy, as a matter of fact, mothers are primarily responsible for the children's care within the household (Carta and Rizzica, 2018) and PL is used almost exclusively by them, representing over 90% beneficiaries of paid parental leave benefits (Zurla, 2022).

In order to correctly identify the peer effects,[3] we employ an instrumental variable

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[2]However, notice that extended maternity leave may have detrimental effects on female labor supply in the long run if it induces women to stay out of work for long enough periods (Olivetti and Petrongolo, 2017).

[3]The typical challenges to the identification of peer effects concern the existence of correlated characteristics among groups, the endogenous sorting of workers into peer groups and the reflection problem.

approach that exploits a quasi random variation in the costs of PL, due to a reform that took place in Italy in 2015. In order to help balancing between work and family responsibilities, this reform extended paid PL to children aged 4 to 6 years old, thus introducing a discontinuity in the cost of PL for parents with children of this age. In particular, before 2015 parents could be absent from work with a paid PL (at 30% of the usual wage) only up to their child's 3 years old and when the child turned 4 the leave was not paid anymore. On the contrary, after the implementation of the reform the age up to which it was possible to ask for a paid PL (at 30% of the usual wage) was raised to 6, without increasing the total number of days available. In the Italian context, where the use of PL is limited (OECD Family Database), we show that this generated a substantial increase of the probability of taking a PL, as we can observe an increase of the share of mothers of children aged 4-6 that actually took at least a day of PL.

We assess the peer effects for mothers with eligible children after the implementation of the reform (henceforth simply denoted as "working mothers") and we assign to each of them one or more peers, identified as mothers in the same establishment and occupational group who made PL decisions before her.[4] The reform allows us to classify the sample of peers into two categories, the pre-reform and the post-reform peers, according to the child's year of birth. In fact, mothers could have benefit of the reform only if they had children aged 4-6 years in the years following the introduction of the reform.[5] The assignment to the post-reform category, conditional on covariates, can be treated as good as random, since it depends only on the child's year of birth. In our specification, the share of post-reform peers over the total number of peers is therefore quasi-randomly assigned to the working mothers and it is used as instrument for our main variable of interest, the share of peers that actually take a PL.

This estimated peer effect can be interpreted as a LATE (local average treatment effect) of the reform-induced parental decision of the peer mothers on the parental leave decisions of the working mothers. Since the working mothers are all subject to the same post-reform regime, the reform has no direct effect on them apart from the different exposure to pre-and-post reform peers that they cannot manipulate. This strategy helps tackling potential endogeneity issues related to group composition and selection into groups, which are common threats to identification in the peer literature. The reflection problem,[6] which is also typical of the peer literature, is addressed imposing that the working mother's choices have to be always subsequent to those of the relevant peers.

[4]The identification of peers is explained more in detail in Section 3.2.

[5]A circumstance preventing these mothers from being potentially affected by the reform is having already exhausted all of their PLs during the first three years of their child's life. According to our data, anyway, only a small minority of mothers have done so.

[6]The reflection problem refers to the fact that it could be impossible to distinguish the effect of peers on the individual from the effect of the individual on peers because they are simultaneously determined.

In order to estimate the peer effects, we use a unique administrative linked employer-employee data, which covers the universe of employees for a sample of Italian firms (mainly made by medium-large firms) and allows us to assign to each individual a peer group based on the combination of the firm and the municipality (thus proxying the establishment) where she works, and on the occupation group. The richness of these data, combined with the variation generated by the reform, provides us with a unique opportunity to investigate the effect of peers among colleagues in the workplace.

As regard our results, there are three main messages we draw attention to. The first relates to the results of the first stage regression, in which we measure the effect of the reform on the parental leave choices. The reform had a clear positive effect: the likelihood that a mother takes a PL increases on average by about 9 percentage points according to whether she is pre- or post-reform. This result is quite interesting. The effect of the reform, in fact, could not be taken for granted because the reform did not increase the total number of available weeks but, by extending the time interval over which a paid PL can be taken, increased the degree of flexibility in their use (i.e., by relaxing the implicit cost of the flexibility). If there was actually no demand for such flexibility, little or no effects would have been seen and the identification power would be poor.

Second, the estimates of the main regressions highlight the importance of peer effects in this setting: a one standard deviation increase of the take up rate of peers, amounting at about 21 percentage points, corresponds to an increase of 16 per cent of the average value of the dependent variable. We find evidence of a positive peer effects also in the intensive margin: not only the likelihood of using PL increases, but also the number of weeks taken per worker does so, despite the fact that the law did not change the total available amount of weeks for each mother.

Interestingly, as a third message, we also find that the mothers that have more peers taking PL, are less likely to work part-time. This suggests that the parental leaves policy enables a better conciliation of working and private life, reducing the need to work part-time, and that peer effects have been important in this respect.

We show that the finding of a peer effect is substantially robust to a number of robustness checks related to the sample definition and to our instrumental variable.

We consider two main mechanisms that could channel the effect. First, observing peers may reduce the uncertainty concerning the specific costs and benefits of PL, in terms of career opportunities, wage-trajectories, and the possibility of combining family and work duties, in line with the social learning models. Second, peer effects could arise through the transmission of information, among the workers, about the existence of the parental leave program and its operational details. In the heterogeneity section, we find evidence in support for the former mechanism, as the peer effects seem to be especially evident for workers with a low tenure in the firm. More recently hired workers, in fact, face more uncertainty concerning the employer's reaction to their leave decision than mothers with longer tenure and, consequently, they are more susceptible to observed peer decisions.

Our paper contributes to the literature of maternity and family leave policies by

providing evidence on less explored issues. Despite this literature is broad (see Rossin-Slater, 2017, for a review), only a few and recent papers have in fact investigated the effects of peers on the individual's choices. The majority of works studied the long-term effects of parental leave policies on women's career,[7] on children, (Baker and Milligan, 2008; Lalive and Zweimüller, 2009; Ejrnæs and Kunze, 2013; Drange and Rege, 2013; Schönberg and Ludsteck, 2014; Baum and Ruhm, 2016; Corekcioglu et al., 2020) and on the effects on firms and coworkers' career (Gallen, 2016; Brenøe et al., 2020; Ginja et al., 2020; Huebener et al., 2021). The peer effects about the use of maternity and paternity decisions, as far as we know, are investigated only in Dahl et al. (2014) and in Welteke and Wrohlich (2019). Dahl et al. (2014) find the occurrence of important peer effects focusing on fathers and exploiting a reform in Norway that introduced an extra month of paid leave that can only be taken by fathers. Welteke and Wrohlich (2019) focused instead on mothers in Germany, considering a 2007 reform that encourages, in particular, high-income mothers to stay at home during the first 12 months following childbirth. According to their finding, also maternal decisions are significantly influenced by their peers' decisions, especially in situations with high uncertainty. Differently from these papers, we focus on parental leaves taken when children are older than 3 years old. These PLs can be taken by parents after the maternal compulsory leave of 5-months and they are aimed at easing the combination of work and life duties by endowing parents with additional time off work and higher flexibility over a longer interval of the childhood. Differently, the other studies focus on the functioning of leaves that work as extensions of the absence from work when children are still new born babies. It cannot be taken for granted that peer effects found in such context can be extended also to the framework of parental leave policies and it is thus worth adding evidence more about the less explored situation.

Another important contribution of this paper concerns the evidence provided for the effects of the reform. To the best of our knowledge, this is the first study that analyzes the effect of this reform: it is thus important *per se* to document whether we observe changes after its introduction.[8] The assessment of its direct and indirect effects are particularly important for a country like Italy that is characterized by very low take up rate as regard parental leave policies, meaning that there exist frictions that reduce the probability of taking the leaves. Based on our estimation strategy, we are able to analyze the effects of the reform introduced in Italy in 2015. Our first-stage results are informative of the direct effects, while our IV results assess the indirect impacts operating through peer effects.

Finally, being based on a large matched employer-employee dataset, our paper also

[7]There are also papers that focus on fathers. For recent evidence on paternity leave, paternal involvement in childcare, and fathers' labor market outcomes, see Tanaka and Waldfogel (2007); Nepomnyaschy and Waldfogel (2007); Rege and Solli (2013); Almqvist and Duvander (2014); Cools et al. (2015).

[8]Only the XXI Annual Report of the Italian National Institute of Social Security (INPS, 2022) documents the change in the use of the PL after the reform, mainly focusing on fathers.

relates to the recent literature about peer effects in the workplace (Battisti, 2017; Cornelissen et al., 2017; Jarosch et al., 2021; Hong and Lattanzio, 2022) that exploits administrative data and arguably improves on the ground of external validity with respect to the majority of previous studies based on very specific settings (laboratory experiments, survey from a few firms or occupations). In this more recent stream of literature, our paper brings about new insights on the peer effects in the specific context of the parental leave decisions.

The rest of the paper is organized as follows. Section 2 presents the institutional setting of parental leave in Italy. Section 3 describes the data used and the definition of working mothers and peers groups; Section 4 shows the empirical strategy and discusses the identification issues. Results are set out in Section 5, where we perform some robustness checks and we investigate the existence of heterogeneous effects. Section 6 concludes.

2. Parental Leave in Italy: institutional setting

In Italy, maternity leave is compulsory and, at the time of the reform, women usually take up 2 months off from work before the due date and 3 months off after the baby's birth. If the pregnancy is considered at risk or if the workplace puts her health or her baby's in danger, women may ask their employers for more time off before the baby's due date. On the contrary, women can request to work up to 1 month before her due date with the approval of her doctor, in order to have a total of 4 months off after the birth of her child. For employed workers this obligatory 5-month absence from work is compensated at 80% of the salary. For some types of female workers (agricultural workers, home helpers and carers, domestic workers, unemployed or with a suspended employment contract, with obligatory absence from work), protection is granted if certain legal requirements are met.

After the end of the compulsory maternity leave period , the law provides for the existence of optional leaves (the so called parental leave), which are available to either or both parents and are meant to allow them to care for an infant or young child also after the end of compulsory leave period. In Italy, each parent can take up at most 6 months of PL, with a maximum of 10 months in total for each household (Zurla, 2022).[9] The first 6 months are paid at a 30% replacement rate if used before the child's 6th birthday, while the remaining months are unpaid.

In this paper we exploit a national reform that changed, from 2015,[10] the age up to which the mother can apply for paid parental leave. Before the reform, the PL was paid until the 3rd year of life of the child, after the age of 4 the parent can still ask for the PL, but it becomes unpaid afterwards. Starting from 2015, the PL becomes paid up to the 6th year of the child. Consequently, the reform introduced a discontinuity in

[9]The maximum is increased to 11 months if the father uses at least 3 months of PL.

[10]In particular, the discipline of parental leave has been the subject of two legislative decrees (n. 80 and n. 81 of 2015), both implementing the delegated law n. 183/2014 (so-called Jobs Act).

the PL cost for the parents with children aged 4/6 years. Moreover, the reform of 2015 stated the the possibility for parents to choose between daily and hourly use of PL and the reduction to 5 days (compared with the original 15 days) of the term within which the worker must notify the employer of the intention to take advantage of the leave (in the case of PL taken on an hourly basis, the term is further reduced to 2 days).[11] As shown in the next section, the changes introduced by this reform generated a sharp increase of the probability of taking a PL, because of the decrease in the cost of taking it and in the increased flexibility in its use. All these changes, initially envisaged on an experimental basis only for the year 2015, were made definitive and structural by Legislative Decree 148/2015, which entered into force on 24 September 2015.

In 2012, the Government has introduced other measures to facilitate familiar work-life balance that may have affected the choices in term of parental leaves of the mothers considered in this analysis. In particular, the Fornero reform in 2012 introduced an economic contribution of 600 euros per month to dependent working mothers for the purchase of babysitting services or for the payment of the nursery fees, as an alternative to each month of parental leave, for a maximum of six months and until 11 months after the end of the compulsory maternity leave period. In practice, those who opted for this solution could not be absent from work after compulsory maternity leave for the corresponding number of months for which they received the economic contribution. This option was extended until 2017-2018 and it was canceled by the Stability Law for 2019. This measure, of course, may have influenced the probability of taking a PL observed in our data and, unfortunately, we don't know in our data if the mothers asked for this contribution. However, this measure has been in effect from 2013 to 2018, meaning that all the mothers in our sample are evenly affected.[12]

Notice that we focused only on mothers' parental leave choices and on their effects on peers, without considering the fathers. This is due to the fact that in our data we are not able to recover the child's birth year for the fathers, differently from the mothers, for which the child's year of birth is indirectly computed using the compulsory maternity leave period (as explained in section 3). On the contrary, at the time of the reform, the paternity leave has not necessarily to be taken immediately after the child's birth. However, notice that in Italy, according to the official statistics (Aumayr-Pintar (2019)), the uptake of parental leave by fathers is among the lowest in comparison with the other European countries, meaning that the number of male peers would be extremely low and not suitable for analysis. At the same time, not having fathers among peers for mothers does not seem a main issue both because their number would be small and because is reasonable to assume that female workers are mainly affected by the parental leave choices of the peers of the same gender, given the so clear differences

[11] While the overall duration of the leave period remains unchanged, the 2015 reform also stated the extension to the first 12 years of the child's life, instead of the first 8 years, the period in which parents can abstain from work, even without compensation.

[12] The effects of the introduction of this economic contribution on maternal labor market performance (labor supply and wages) has been analyzed in Martino (2017).

observed in Italy between the behavior of mothers and fathers in term of parental leave.

3. Data and summary statistics

3.1. Data sources and the working sample

Our empirical analysis draws upon a unique employer-employees data set that covers the universe of employees for a sample of Italian firms. This data set matches two different sources of data. The first one is the Bank of Italy's Survey on Industrial and Services firms (INVIND), a representative sample of Italian industrial and services firms in the private non financial sector with 20 or more employees.[13] The second source is the Social Security administrative data set, provided by the Italian National Institute of Social Security (INPS), which contains information on the entire working histories of all workers who have ever transited in one of the INVIND firms between 2005 and 2018. For each worker we have information about the gender, year of birth, type of contract (temporary or open ended), working time (part-time or full-time), broad occupational codes (blue collars, employee white collars, supervisors and managers, etc.), annual gross earnings, number of weeks worked, municipality of work, and a firm identifier. This last information allows us to match our data with another database provided by INPS, covering the universe of Italian firms with at least one employee, which contains firms' characteristics which we use as controls in our main regression model.

In our database we have information about the number of weeks of maternity leaves taken in each year by the individual, which we use to identify the status of mothers and to recover the year of child's birth.[14] Notice that our mothers' working sample is made only of mothers who gave birth at a time in their lives when they were employed. For the mothers who gave birth in a non-employment period we do not have information on the maternal leave period and we are not able to include them in the working sample.

We then create a panel considering the working history of these mothers over the years 2007-2018, keeping all the information on the number of weeks of parental leave for each year, the job features (working time, type of contract, occupation group, municipality, job-tenure), the worker's demographics (age, presence of a second child, being born abroad), the worker's earnings one year before child-birth, and information concerning the firm (size, average wage, sector, etc.).[15]

Starting from this dataset, we restrict the analysis to the group of working mothers (henceforth, WM) who were continuously working in INVIND firms in the years of

[13]The reference population of this survey is composed of firms whose registered head office is in Italy, having 20 or more workers and belonging to various branches of activity in industry (excluding construction) and non-financial private services, or having 10 or more workers and belonging to the construction sector. The sample of the most recent surveys has been composed of around 5,000 firms (about 3,000 industrial firms, 1000 service firms and 600 construction firms). The sampling fractions are higher for firms with 50 or more workers and firms in the South and Islands. The sample is representative of 70% of total sales in the Italian economy; Carta et al. (2021)).

[14]See Appendix A for more details about how child's year of birth is retrieved from the data source.

[15]The construction of the dataset is described more in detail in Appendix A.

analysis because only for these firms we can observe the whole group of colleagues in each year of work. The WM group is made of all the mothers with 4/6 years old children in the post-reform years 2016-2018, i.e all the children born in 2010-2013.

3.2. Identification of peers

The estimation of the peer effects depends on the accuracy with which one identifies the set of peers relevant to each individual (Carrell et al., 2009, 2013). We define the peers basing on the fact that they were working in the same firm, municipality (which combined with firm identifiers proxies the establishment) of the WM and were in the same occupation group. In our sample we keep only WM that have at least one peer mother with children of 4/6 years around the years of the reform. Our unit of analysis along the cross-section dimension is the WM.

The group of peer mothers is divided into a control and a treatment group, according to the the child's birth year. For any $WM_{i,t}$, the *Peers-pre* (control group) are all the mothers employed in the same firm-municipality-occupational group of $WM_{i,t}$ having 4-6 years old children before the reform, i.e., in 2013 and in 2014 (children born in 2007-10; Figure 1),[16] conditional on the fact that the peers remain employed at time t in the same firm of $WM_{i,t}$. The last condition is motivated by the fact that it is arguably more cautious to assume that the WM knows what her peer did if the peer is still there as the communication is arguably more feasible; anyhow, in Section 5.3 we address the robustness of our main results to the relaxation of this restriction. For *Peers-pre* mothers the parental leaves taken in the 4-6 years child-age range were not paid.

In 2015 the reform stated the possibility to take a paid PL until the sixth year of the child. For any $WM_{i,t}$, we thus have that the *Peers-post* mothers (treatment group) are all the mothers with children aged between 4 and 6 since 2015 and before t, who were working in the same firm, occupation group, municipality as $WM_{i,t}$ and who were not included in the WM sample. Again, we also impose the condition that the peers remain employed at time t in the same firm of $WM_{i,t}$. This implies that, differently from the group of *Peer-pre*, the group of *Peers-post* varies according to the cohort of WM's child. Figure 1, where cell values denote child's age, provides a simplified scheme. In order to be sure to correctly overcome the simultaneity problem, in the construction of our database we impose that the choices of the WM were always subsequent to those of the peers, thus avoiding overlapping time spans.

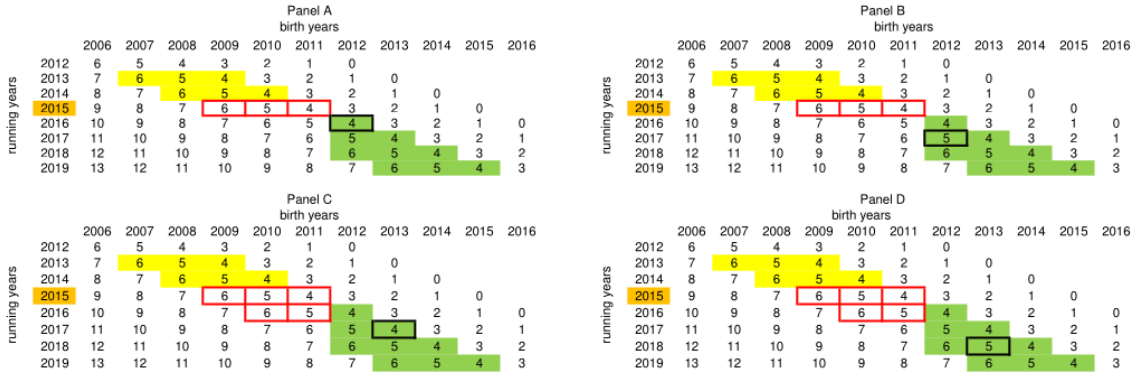
As an example, if $WM_{i,t}$ has a child born in 2012, i.e., 4 years old in $t = 2016$ (black framed cell in panel A of Figure 1), her *Peers-post* are all the colleagues with 4-6 years old children in 2015 (i.e., children born in 2009-11). In addition, when we observe her behavior in the subsequent years (panel B), when her child turns 5 years old, her *Peers-post* reference group remains the same (conditional on the fact that the

[16] We start from 2012 to take a limited pre-reform window in order to limit the risk of confounders. As a robustness check we will change the pre-reform window.

peers remain employed in the same firm of $WM_{i,t}$). As another example, if $WM_{i,t}$ has a child born in 2013, i.e., 4 years old in $t = 2017$, her *Peers-post* reference group is broader, since it is made of those peers with 4-6 years old children in 2015 and 2016 (i.e., children born in 2009-11; panels C and D of Figure 1).

Notice that in our setting a WM may be affected by several peers, both *pre* and *post*; we are thus going to consider, for each WM, the share of *Peers-post* over the total number of peers.

Figure 1: Identification of peer groups by year and child cohorts.



Notes. In each panel, cell values denote the child's age in a given year (in rows) for a given cohort (in columns). Green cells refer to WM (based on the eligible age of their child). For the WM framed in black, the yellow cells refer to the group of *Peers-pre*, while the red-framed cells refer to the group of *Peers-post*. In order to avoid simultaneity issues, for each WM the group of *Peers-post* does not include WM in the same year or in years when also the WM has an eligible child (i.e., the highest observation year of the red-framed cells is always lower than the one of the black-framed cell). In the baseline specification, the group of *Peers-post* does not include workers who in turn has been considered as WM in previous years (i.e., the red-framed cells never overlaps with the green cells).

3.3. Summary statistics

Our sample is made by 103,894 observations, 61,667 WM, working at 4,358 firms over 11,404 establishments.[17] The median firm, computed using the firm as unit of observation, has a size of about 130 employees, is 26 years old, and pay an average gross salary of 31,500 euros. About 64% of firms belong to the industrial sector, but in our sample firms in the service sector are on average larger and employ almost two thirds of workers in the sample.

Tables 1 and 2 provide descriptive statistics from our sample at the WM-year level, distinguishing among WM exposed only to *Peers-pre* (col. 1), those exposed only to

[17]We identify establishments as the combination of firm and municipality.

Peers-post (col. 4), and those mainly exposed to one of each peer group (col. 2 and 3), as well as the statistics for the overall sample (col. 5). We can observe that, overall, 32% of WM take at least one week of PL and, conditional on doing so, they take on average 8 weeks per year. Among peer mothers, who are mainly in the pre-reform group (about 70%), 21% use PL, taking on average 3 weeks per year. To each WM is associated a median number of 10 peers among mothers. About two fifths of WM in the sample work part-time, while less than 4% has a temporary contract; WM mostly carry out occupations in the non-managerial white-collar area, whereas the blue-collar area covers about one quarter. WM mainly work in the Center-North and they are relatively more present in the "trade, food and restaurants, and entertainment" sector class, where female employment is structurally higher.[18] In the establishments where WM work, on average about 6% of workers are foreign born (a value in line with the incidence of foreign born WM in the sample), 70% is aged between 25 and 49 years and female employees represent about a half of the total.

Disentangling by exposure to peer-groups, the table shows that the use of parental leave is higher for women exposed to *Peers-post* peers, both in terms of the extensive margin (the average share of WM that take a parental leave) and of the intensive one (number of weeks of parental leave). Similarly, also the use of parental leave by peers increases with the reform, for both margins. Firms' and workers' characteristics are very similar across treated and non-treated observations, with some differences for those exposed to only *Peers-post*, which appear to refer to younger firms, mainly belonging to the sector "trade, food, entertainment" and with a higher share of temporary contracts. We control for all these variables in the regressions, in order to take into account possible bias due to selection on observable characteristics.

4. Estimation strategy

The correct identification of peer effects is challenging due to three issues: correlated characteristics among groups, selection into groups and the reflection problem (see Manski, 1993; Blume, Brock, Durlauf, and Ioannides, 2010, for an overview). The first problem is relating to the fact that there may be observed or unobserved factors that are correlated among individuals who belong to the same group (i.e., workplace conditions) that might affect parental leave decisions, acting as confounders. The second issue refers to endogeneity of group's membership: the peer groups are usually formed endogenously and consequently it is empirically difficult to correctly distinguish the true peer effects from selection effects. For example, workers with preferences for a good balance between work and family life may sort into family-friendly firms. The third condition refers to the simultaneity problem, or reflection problem, which consists in the fact that interactions within a group are simultaneous, making impossible to

[18] These figures are rather in line with the distribution of female employees in the private sectors, a part from the share of temporary and of blue-collar workers which in our sample is considerably lower.

Table 1: Descriptive statistics, by exposure to the peers pre and post reform.

	Only pre (N = 43,725)	Preval. pre (N = 33,814)	Preval. post (N = 20,806)	Only post (N = 5,549)	Total (N= 103,894)
Sh of WM taking PLs (mean)	0.26	0.36	0.35	0.34	0.32
	(0.44)	(0.48)	(0.48)	(0.47)	(0.47)
Sh of peers taking PLs (mean)	0.18	0.22	0.23	0.27	0.21
	(0.21)	(0.16)	(0.22)	(0.35)	(0.21)
Nr weeks of PL, WM (mean)	2.04	2.84	2.87	2.92	2.51
	(5.29)	(6.03)	(6.18)	(6.37)	(5.80)
Nr weeks of PL, peers (mean)	1.82	2.00	2.13	2.21	1.96
	(2.93)	(1.55)	(2.61)	(4.05)	(2.58)
Nr weeks of PL, WM (mean; ≥ 0)	7.81	7.79	8.19	8.53	7.93
	(7.90)	(7.82)	(8.10)	(8.39)	(7.95)
Nr weeks of PL, peers (mean; ≥ 0)	2.85	2.26	3.08	5.08	2.73
	(3.24)	(1.46)	(2.64)	(4.81)	(2.68)
Nr peers (median)	6	25	7	1	10
	(20)	(58)	(16)	(4)	(30)
Yearly salary, year before the birth (mean)	22,366	23,674	23,898	21,078	23,030
	(12,832)	(13,293)	(14,070)	(11,850)	(13,215)
Age (mean)	39.28	38.73	38.39	38.20	38.87
	(4.62)	(4.73)	(4.73)	(4.86)	(4.71)
Experience same firm (observed since 2005; mean)	9.09	9.86	8.63	5.93	9.08
	(3.53)	(3.59)	(3.72)	(3.98)	(3.72)
Part time (obs.)	18,669	15,113	7,239	2,552	43,573
	(42.7%)	(44.7%)	(34.8%)	(46.0%)	(41.9%)
Temporary contract (obs.)	1,596	872	943	491	3,902
	(3.7%)	(2.6%)	(4.5%)	(8.8%)	(3.8%)
More than one child (obs.)	16,810	12,540	7,651	1,992	38,993
	(38.4%)	(37.1%)	(36.8%)	(35.9%)	(37.5%)
Blue collar (obs.)	11,311	8,795	3,764	1,354	25,224
	(25.9%)	(26.0%)	(18.1%)	(24.4%)	(24.3%)
White collar (non managers; obs.)	29,822	23,537	15,262	3,905	72,526
	(68.2%)	(69.6%)	(73.4%)	(70.4%)	(69.8%)
White collar (managers; obs.)	2,592	1,482	1,780	290	6,144
	(5.9%)	(4.4%)	(8.6%)	(5.2%)	(5.9%)
Foreign born (obs.)	2,851	2,334	1,398	483	7,066
	(6.5%)	(6.9%)	(6.7%)	(8.7%)	(6.8%)

Notes: This table reports average values for some variables between 2016–2018, grouping workers on the basis of their exposure to the peer pre and post reform. PLs means parental leaves. Columns (1) and (4) include WM exposed only to pre and post reform peers. Columns (2) includes WM predominantly exposed to pre reform peers (i.e., with a share of post-reform peers lower than 50%). Columns (3) includes WM predominantly exposed to post reform peers (i.e., with a share of post-reform peers higher of equal to 50%). Column (5) includes all WM in the sample. Standard deviations or shares in parenthesis. For the number of peers it is reported the median and, in parenthesis, the inter-quartile difference.

Table 2: Descriptive statistics, by exposure to the peers pre and post reform.

	Only pre (N = 43,725)	Preval. pre (N = 33,814)	Preval. post (N = 20,806)	Only post (N = 5,549)	Total (N= 103,894)
Sector					
Other	308	113	95	27	543
	(0.7%)	(0.3%)	(0.5%)	(0.5%)	(0.5%)
Manufacturing	15,602	13,303	6,497	1,422	36,824
	(35.7%)	(39.3%)	(31.2%)	(25.6%)	(35.4%)
Trade, food and restaurant, entertainment	18,106	14,362	7,795	2,999	43,262
	(41.4%)	(42.5%)	(37.5%)	(54.0%)	(41.6%)
Other services	9,709	6,036	6,419	1,101	23,265
	(22.2%)	(17.9%)	(30.9%)	(19.8%)	(22.4%)
Firm age (mean)	24.67	27.37	22.95	19.75	
	(13.38)	(13.59)	(12.80)	(14.61)	
Class of firm size					
< 50	1,454	300	537	200	2,491
	(3.3%)	(0.9%)	(2.6%)	(3.6%)	(2.4%)
50-150	3,578	1,732	1,784	519	7,613
	(8.2%)	(5.1%)	(8.6%)	(9.4%)	(7.3%)
150-300	3,650	2,590	1,866	367	8,473
	(8.3%)	(7.7%)	(9.0%)	(6.6%)	(8.2%)
> 300	35,043	29,182	16,619	4,463	85,307
	(80.1%)	(86.3%)	(79.9%)	(80.4%)	(82.1%)
Average gross salary per worker in the firm					
< 25k	15,105	11,242	6,074	2,302	34,723
	(34.5%)	(33.2%)	(29.2%)	(41.5%)	(33.4%)
25k-32k	11,813	7,186	7,423	1,741	28,163
	(27.0%)	(21.3%)	(35.7%)	(31.4%)	(27.1%)
32k-40k	8,642	7,534	3,150	629	19,955
	(19.8%)	(22.3%)	(15.1%)	(11.3%)	(19.2%)
> 40k	8,165	7,851	4,159	877	21,052
	(18.7%)	(23.2%)	(20.0%)	(15.8%)	(20.3%)
Area					
North-West	15,730	13,567	6,327	1,685	37,309
	(36.1%)	(40.2%)	(30.5%)	(30.5%)	(36.0%)
North-Est	9,627	7,213	4,158	1,069	22,067
	(22.1%)	(21.4%)	(20.1%)	(19.3%)	(21.3%)
Center	11,104	8,361	6,119	1,336	26,920
	(25.5%)	(24.8%)	(29.5%)	(24.2%)	(26.0%)
South	7,118	4,619	4,124	1,440	17,301
	(16.3%)	(13.7%)	(19.9%)	(26.0%)	(16.7%)
Establishment composition					
Share of 25-49 years old workers (mean)	0.70	0.70	0.68	0.72	0.70
	(0.16)	(0.14)	(0.18)	(0.16)	(0.16)
Share of foreign born workers (mean)	0.06	0.06	0.06	0.07	0.06
	(0.09)	(0.07)	(0.09)	(0.09)	(0.08)
Share of female workers (mean)	0.51	0.51	0.51	0.56	0.51
	(0.23)	(0.21)	(0.22)	(0.25)	(0.22)

Notes: This table reports average values for some variables between 2016–2018, grouping workers on the basis of their exposure to the peer pre and post reform. PLs means parental leave. Columns (1) and (4) include WM exposed only to pre and post reform peers. Columns (2) includes WM predominantly exposed to pre reform peers (i.e., with a share of post-reform peers lower than 50%). Columns (3) includes WM predominantly exposed to post reform peers (i.e., with a share of post-reform peers higher of equal to 50%). Column (5) includes all WM in the sample. Standard deviations or shares in parenthesis.

distinguish the effect of peers on the individual from the effect of the individual on peers because they are simultaneously determined.

In order to address the correlated and endogeneity concerns, several papers in the literature (Dahl et al., 2014; Welteke and Wrohlich, 2019) suggest using policy reforms as instruments to address these identification challenges. Therefore, we employ an instrumental variable approach that exploits the policy reform that took place in Italy in 2015, described in Section 2. As will be shown in Section 5.1, this creates an exogenous change in the fractions of working mothers that take PLs when their children were 4-6 years old. With random variation in treatment (and group membership determined prior to treatment) the three identification issues described above no longer bias the estimates (Dahl et al., 2014). In our empirical model, we also include pre-determined control variables and fixed effects to take into account other possible remaining confounders.

In particular, we estimate the following equation:

$$Y_{ifmoct} = \alpha + \beta P_{fmoct} + \gamma_1 W_{it} + \gamma_2 W_{ft} + \delta_m + \delta_t + \delta_c + \delta_o + \epsilon_{ifmoct} \tag{1}$$

where Y_{ifmoct} is a dummy equal to one if a women i mother of a child born in cohort c employed at firm f located in municipality m in occupational group o at time t takes a PL. The time subscript t ranges from 2016 to 2018 as we focus on WM with 4-6 years old children in the years that follow the introduction of the reform (green cells in Figure 1). In the main specification, we consider the extensive margin (i.e., take or not a PL), but we also investigate the intensive margin (i.e., how many weeks of PLs a mother takes). P_{fmoct} is the share of women's peers that took a PL when their children were 4-6 years old. All effects are expressed on a yearly basis.

We control for the birth's cohort of the WM's child (δ_c) and for individual's characteristics (W_{it}, which includes age and age squared, occupation δ_o, dummy for foreign, dummy for temporary contracts,[19] dummy for having more than one child, dummy for low experience in the same firm, the yearly salary in the year before the birth[20]). Moreover, in addition to year dummies (δ_t), we include municipality fixed effects (δ_m) in order to confirm that our comparison across treatment groups over time does not reflect municipality-specific characteristics. We do not include a firm fixed effect, which is very demanding, but we deal with this concern inserting many controls at firm or establishment (firm/municipality) level. Namely, in vector W_{ft} we control for several characteristics of the firm (sector, the firm size, average gross salary per worker) and for the workforce composition in the establishment (share of 25-49 years old workers, share of foreign born workers, and share of female workers). Standard errors are clustered at the establishment (i.e., firm/municipality) level.

[19]It is important to control for this variable also because the length of the PL can not exceed the duration of the contract.

[20]We consider the salary in the year before the birth in order to prevent the bad control bias for this variable.

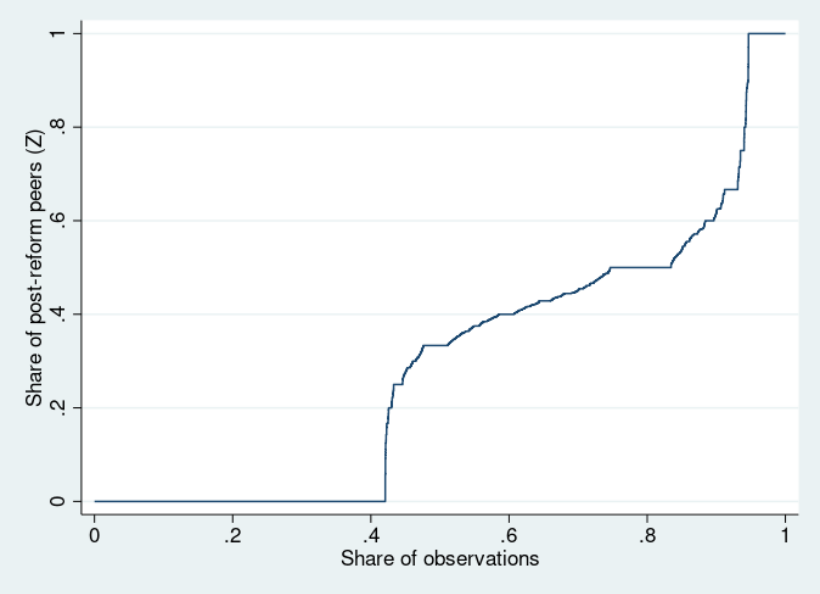
Our main parameter of interest is β which captures the effect of peers on WM' parental leave decisions. As discussed above, because of the concerns related with the peer effects analysis, OLS estimation of this equation is unlikely to retrieve the causal effect of peers on WM. We therefore need to find an exogenous variation in P_{fmoct} to obtain unbiased estimates of β. Our approach is to use the variation in the taking rates induced by the 2015 parental leave reform in an instrumental variables framework.

The first stage equation is thus the following:

$$P_{fmoct} = \alpha + \lambda Z_{fmoct} + \phi_1 W_{it} + \phi_2 W_{ft} + \theta_m + \theta_t + \theta_c + \theta_o + \varepsilon_{fmoct} \tag{2}$$

Where the share of women's peers that take PLs is regressed on the instrument, which is the share of post-reform peers over the total number of peers (Z_{fmoct}). This instrument captures the proportion of *Peers-post* (i.e., the peers that may have taken the PL after the reform); in this way we have a continuous treatment. Of course, this variable becomes binary (i.e., it takes values 0 or 1) if the co-worker has only peers pre- or post-reform. The parameter λ is the first stage estimate, and captures the increase of the probability of taking PL among peers, following implementation of the reform. Figure 2 plots the distribution of our instrumental variable (i.e., the share of post-reform peers); about 40% of observations have only pre-reform peers, and about 20% of them have at least 40% of peers that are post-reform.

Figure 2: Distribution of the share of post-reform peers.



Notes. Peers are computed according to working mothers' firm/municipality/ occupation/child's cohort as described in Section 3.2.

Thus, the estimated β is the local average treatment effect (LATE) of the reform

induced parental decision of the peer mothers on their WM' parental leave decisions. We are thus measuring the effect on the sub population of compliers, which are all the WM who have peers who took the PLs for their 4-6 year old children in response to the PLs benefit reform, and would not have done so in absence of the reform.

Our empirical strategy yields consistent estimates if the instrument's identifying assumptions are met. The first assumption refers to the relevance of the instrument: i.e., the correlation between the instrument and the endogenous variable. This assumption is dealt with more extensively in sub-section 5.1 and, as it can be tested, in our results we always report the F-stat from the first stage regression, which are quite reassuring.

The second assumption of the instrument is the exogeneity: i.e., peer's mothers have to be as good as randomly assigned to the treatment group. While this assumption cannot be directly tested, it arguably holds in our context because individuals are not able to control the treatment assignment variable, which is related to the year of the child's birth year. The birth, for both treated and control groups, occurred years before the reform, as eligible children had to be at least three years old when the reform was implemented.

Finally, the exclusion restriction requires that the instrument operates through a single known channel: i.e., WM outcomes should not be affected by the parental leave reform beyond the parental leave take-up of their peers. Notice that the reform did not directly affect the cost of taking leave for the WM, since they were all eligible for paid parental leave when they had eligible children in the post-reform period. So, it can be assumed that whether a peer mother had 4-6 year old children before or after the reform had no effect on WM' parental leave decisions other than through their choices in taking or not the parental leave. The possibility of other direct effects is curbed further when several covariates related to firms, workers and local contexts are controlled for.

Moreover, the problem of simultaneity in the estimation is solved by the time dimension: since the peers' PL choices are pre-determined, the possibility of peer decisions being influenced by the WM they are associated to is excluded.

5. Results

5.1. Reform effects and first-stage results

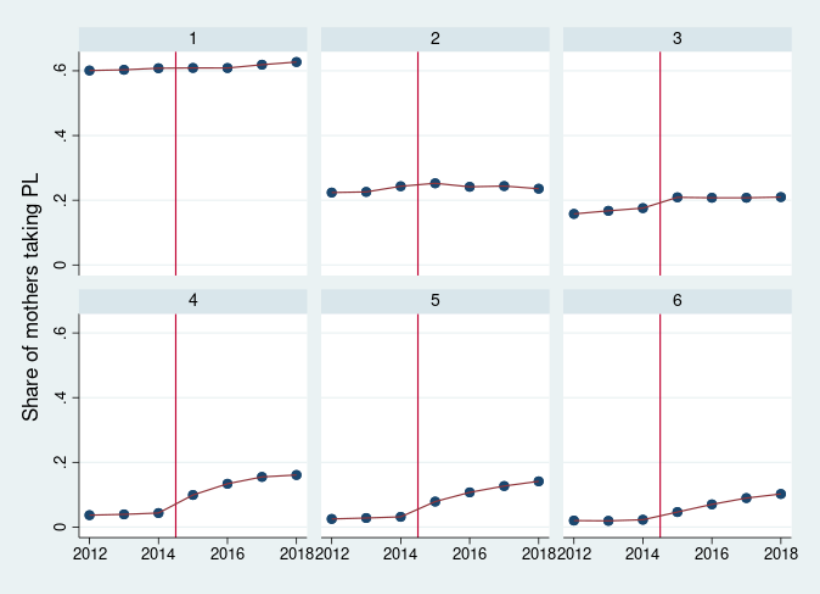
Our identification strategy relies on the discontinuity induced by the reform on the likelihood of taking parental leaves. Therefore, before going to the main results, in this section we present some evidence about the effect of the reform.

We firstly provide suggestive descriptive evidence for the reform effects for the more general dataset that comprehends mothers of non-INVIND firms as well.[21] Based on this, a graphical inspection reveals an increase of the share of mothers with 4-6 years

[21] We need to restrict the main analysis to INVIND firms in order to observe all co-workers, thus being able to assess peer effects.

old children taking at least one week of parental leave after the implementation of the reform (Figure 3).[22] This means that the reform had a visible effect on the mothers' PL decisions.[23]

Figure 3: Share of mothers taking at least a day of parental leave by child-age.



Notes. The caption above each graph refers to child's age. Shares are computed for working mothers with one child in the observed period.

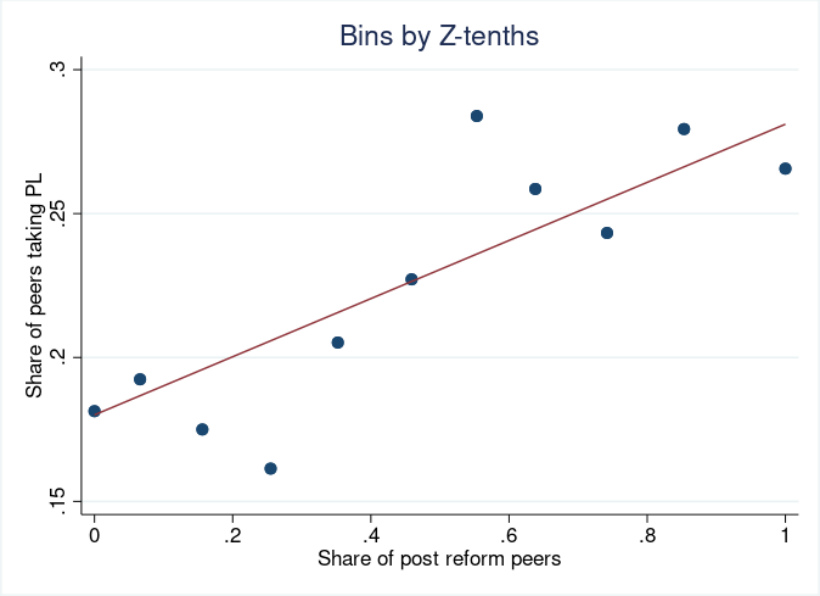
Next, we move to our working sample to show that the assumption on the relevance of the instrument (i.e., the correlation between the instrument and the endogenous variable) is met. Figure 4 shows observation bins for the share of post-reform peers for the WM in the sample (horizontal axis) and for the share of peers who actually take PLs (vertical axis). The red line is the fitting line from a linear regression on all observations. Both bins and the fitting line suggest a clear positive correlation between the share of post reform peers and the share of peers taking PL: for WM having more post-reform peers we observe a higher "exposure" to peers taking PL.

Finally, we test more formally the occurrence of a positive and significant relationship between the share of post reform peers and the share of peers taking PLs

[22]Figure 3 is computed on the sample of employed mothers with only one child-birth in the whole period. Results are substantially analogous, but with a slight upward parallel shifted, when we consider all mothers in the sample.

[23]Notice that we also see a small increase for mothers of children aged 3, which is presumably relating to the fact that some of those children turned 4 years old during the year (we do not observe the exact period of the year when PLs are taken) or some 4 years old children might have been erroneously classified in the 3 years age class (as explained in the Appendix A).

Figure 4: Correlation between the share of post reform peers and the share of peers taking parental leave.



Notes. Peers are computed according to working mothers' firm/municipality/occupation/child's cohort as described in Section 3.2. Each dot represents the average value within a bin, where bins are defined by the two extreme values and interior classes with a step of ten per cent points: e.g., 0, (0,0.1), [0.1,0.2), ... [0.9,1), 1. The red line comes from a linear fitting on all observations.

in the framework of our empirical model by showing the first stage results and the associated F statistics. This statistic is conventionally required to be higher than 10 in order to exclude issues of weak instruments. In Table 3 we show the estimation of the first-stage regression presented in Eq. 2. In the first column we use the most parsimonious version of our empirical model controlling for year, cohort and municipality fixed-effects. Column (2) includes worker's level controls and column (3) include also firm characteristics. We can observe that coefficients (λ in Eq. 2) are positive and significant in all specifications, thus implying that the reform induced an increase of the share of mothers that take parental leave. In particular, the likelihood that a peer takes a PL increases on average by about 9 percentage points according to whether the mothers are pre- or post-reform. It is a significant impact not only from a statistical viewpoint, but also from an economic one: it implies that a one standard deviation in the instrument increases the endogenous variable by about one eighth of its standard deviation (i.e., 2.6 out of 21 percentage points). For all specifications, the first stage F-statistics are well above 10.

Table 3: First-stage results: post-reform peers and peers' parental leaves.

	(1)	(2)	(3)
	Baseline	Worker ctrl	All ctrl
Post-reform peers (%)	0.093***	0.092***	0.085***
	(0.011)	(0.011)	(0.010)
F-stat	69.43	73.77	67.87
Observations	103,508	102,751	102,724
Worker ctrl	No	Yes	Yes
Firm ctrl	No	No	Yes
Year FE	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes
Child age FE	Yes	Yes	Yes

Note: estimations according to equation (2). Dependent variable: share of peers taking a parental leave (PL). Column (2) includes workers' characteristics (age and age squared, occupation, dummy for foreign, dummy for temporary contracts, dummy for having more than one child, dummy for low experience in the same firm); column (3) includes also firm's characteristics (sector, firm size, average gross salary, share of 25-49 years old workers, share of foreign born workers, share of female workers). Standard errors in parenthesis: * p<0.10, ** p<0.05, *** p<0.01.

5.2. Baseline results

The baseline results are displayed in Table 4. Similarly to Table 3, the first column shows the most parsimonious version of Equation (1) with the variable of interest (P_{fmoct}), year, cohort and municipality dummies. Column (2) augments the model with worker's level controls and column (3) adds on top of that also features at the

firm level, as described in equation (1).[24] Table 4 also shows the results from the first stage (coefficient and F-statistics) that we discussed in the previous sub-section.

As for the impact of peers on parental leave choices, according to our estimates a 1 percentage point increase of the share of women's peers that took PLs determines a rise of 0.24 percentage points in the probability that a woman takes at least one week of PL (see column (3)). In terms of standard deviation, a one standard deviation increase of the take up rate of peers corresponds to an increase of 16 per cent of the average value of the dependent variable. Without including firms' characteristics the estimated coefficient is slightly higher. The magnitude of the coefficient is in line with the one found in Welteke and Wrohlich (2019), where a mother is about 30 percentage points more likely to stay at home for the first year if her peers decides to do so. Moreover, our estimated effect is higher than the one found in Dahl et al. (2014) for fathers, who are 11 percentage points more likely to take paternity leave if their peer was exogenously induced to take up leave.

As regards the other regressors, the signs of the estimated coefficients are in line with expectations. It is interesting to notice that the probability of taking a PL decreases if the mother has a temporary contract; moreover, the dummies for low experience in the same firm and for foreign mothers display a negative coefficient. These results are consistent with workers in less sound job positions taking less PL. On the contrary, the probability increases with the age of the mother and if the mother has more than one child, maybe capturing the fact that the mother may take PLs for the other children. There are no significant differences according to the worker's occupation group. As regard the firms' characteristics, our results show that the likelihood for the mothers to ask for a parental leave increases with the size of the firms and with the share of workers aged 25-49 years in the establishment; on the contrary, the likelihood decreases with firm's age, with firm's average gross salary and with the shares of foreign born and female workers in the establishment.[25]

So far we have considered the impact of peers' parental leave choices on the WM's probability of taking at least one (full or partial) week of parental leave (i.e., the extensive margin). Now we investigate the peer effects on the intensive margin: i.e., how many weeks of parental leave a mother takes. Notice that the reform did not increase the total number of weeks available for parental leaves; however, the actual use of parental leaves was on average far below this limit, so the reform might have impacted also the number of weeks taken, and peer effects might have been effective also in this respect. The results, set out in Table 5, show that the number of parental leave weeks is positively affected by the intensity of the use of parental leaves by peers. The estimated coefficient implies that three more PLs taken by peers implies about one more PL taken by the WM.

[24] As more controls and fixed effects are added, the number of available observations slightly reduces. The results and the effect of controls are however basically the same if we estimate all columns on the sample defined by the observations available for column (3).

[25] Results for the full set of regressors are available upon request.

Table 4: Parental leave and peer effects. Baseline results.

	(1) Baseline	(2) Worker ctrl	(3) All ctrl
Peers taking PLs (%)	0.318***	0.344***	0.243**
	(0.107)	(0.103)	(0.108)
Observations	103,508	102,751	102,724
Adjusted R^2	0.007	0.118	0.123
Worker ctrl	No	Yes	Yes
Firm ctrl	No	No	Yes
Year FE	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes
Child age FE	Yes	Yes	Yes
F-stat	69.43	73.77	67.87
First stage coeff.	0.093***	0.092***	0.085***
	(0.011)	(0.011)	(0.010)
Dep.var.mean	0.317	0.317	0.317
Dev.std. X	0.210	0.210	0.210

Note: IV estimations according to equation (1). Dependent variable: dummy equals to one if the mother takes a parental leave (PL). Column (2) includes workers' characteristics (age and age squared, occupation, dummy for foreign, dummy for temporary contracts, dummy for having more than one child, dummy for low experience in the same firm); column (3) includes also firm's characteristics (sector, firm size, average gross salary, share of 25-49 years old workers, share of foreign born workers, share of female workers). Standard errors in parenthesis: * p<0.10, ** p<0.05, *** p<0.01.

Finally, we explored the effects of peers' parental leave on the part-time decisions of the WM. In fact, as part-time positions are often requested because of the difficulties in balancing working life and family care, a stronger use of parental leave may help mothers to maintain a full-time position and, at the same time, to have more flexible working hours. So, there may exist a negative relationship between the use of PL and the choice of a part-time work.[26] Results, shown in Table 6, indicate that having peers taking PLs negatively impacts the probability of working part-time, thus suggesting that parental leave may be an alternative solution of part-time work. In particular, according to our estimates a 10 percentage point increase of the share of women's peers that took parental leave determines a reduction of 6.1 percentage points in the probability that a woman works part-time (see column (3)).[27]

[26]This explain the fact that we do not insert the part time work among the individual controls in the regression, since it is itself a potential outcome of the treatment.

[27]A possible alternative interpretation is that employers could be more reluctant to concede part-time to workers who already use PLs. As an indirect way to investigate this issue, we check whether the results are confirmed or vanish if the sample is restricted to large firms, for which the obstacles posed by the employers are arguably less compelling. Should the coefficient lose its significance, it could be interpreted as supportive to this alternative interpretation. However, by estimating the model on the sample of firms with at least 200 employees, we find that the coefficient remains negative and significant with even a slightly higher magnitude: $-0.677^{***}(0.177)$. We conclude that the most likely

Table 5: Parental leave and peer effects. Intensive margin.

	(1) Baseline	(2) Worker ctrl	(3) All ctrl
Avr annual weeks PLs peer	0.483***	0.400**	0.339**
	(0.160)	(0.161)	(0.163)
Observations	103,508	102,751	102,724
Adjusted R^2	-0.036	0.091	0.098
Worker ctrl	No	Yes	Yes
Firm ctrl	No	No	Yes
Year FE	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes
Child age FE	Yes	Yes	Yes
F-stat	34.39	28.05	26.79
First stage coeff.	0.692***	0.634***	0.609***
	(0.118)	(0.120)	(0.118)
Dep.var.mean	2.512	2.515	2.515
Dev.std. X	2.571	2.572	2.572

Note: IV estimations. Dependent variable: annual weeks of WM' PL. Column (2) includes workers' characteristics (age and age squared, occupation, dummy for foreign, dummy for temporary contracts, dummy for having more than one child, dummy for low experience in the same firm); column (3) includes also firm characteristics (sector, firm size, average gross salary, share of 25-49 years old workers, share of foreign born workers, share of female workers). Standard errors in parenthesis: * p<0.10, ** p<0.05, *** p<0.01.

5.3. Robustness

In this section we perform some robustness checks, considering, as outcome, the probability of taking a parental leave. The first exercise was connected to our instrumental variable. As discussed in Section 4, multiple peers, both pre and post, can affect the same individual. As a robustness check, we sidestep this issue by focusing on the sample of WM with either pre or post reform peers, but not both; in this case Z_{fmoct} is a dummy variable equal to 1 if the working mother has at least one post reform peer, without having any pre reform peer. Results, reported in Table 7, confirm our previous findings, and the estimated effects are even higher in magnitude.

The second set of robustness exercises were related to the sample definition. Table 8 presents several checks. First, we check whether results are driven by very large firms, which, given their size, may have peculiar family policies and facilities. The results are set out in column (1) of the Table 8 where we exclude firms with total number of employees higher than the 95th percentile of the distribution; the point estimates of the variable of interest are very similar to the baseline regression (column (3) of Table 4). Second, we focus on the sample of mothers with only one child-birth in the whole period, because the behavior of mothers with more than one child can be different

interpretation is that PLs, by easing the work-life balance, allows to reduce the employee's need for part-time.

Table 6: Part-time and peer effects.

	(1) Baseline	(2) Worker ctrl	(3) All ctrl
Peers taking PLs (%)	-0.570***	-0.562***	-0.609***
	(0.169)	(0.146)	(0.154)
Observations	103,508	102,751	102,724
Adjusted R^2	-0.071	0.089	0.015
Worker ctrl	No	Yes	Yes
Firm ctrl	No	No	Yes
Year FE	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes
Child age FE	Yes	Yes	Yes
F-stat	69.43	73.77	67.87
First stage coeff.	0.093***	0.092***	0.085***
	(0.011)	(0.011)	(0.010)
Dep.var.mean	0.420	0.420	0.420
Dev.std. X	0.210	0.210	0.210

Note: IV estimations. Dependent variable: dummy equals to one if the mother works part-time. Column (2) includes workers' characteristics (age and age squared, occupation, dummy for foreign, dummy for temporary contracts, dummy for having more than one child, dummy for low experience in the same firm); column (3) includes also firm characteristics (sector, firm size, average gross salary, share of 25-49 years old workers, share of foreign born workers, share of female workers). Standard errors in parenthesis: * p<0.10, ** p<0.05, *** p<0.01.

(and, in addition, in our database we cannot precisely target the child for which the parental leave is taken). The magnitude of the peer effects coefficient is higher than the baseline and it is more precisely estimated.

Third, in column (3) we estimate the regression on the sub-sample of WM considering only the year 2016, in order to overcome possible changes in firms' hiring strategies induced by the parental leave reform. Results are qualitatively confirmed, the estimated coefficient is higher and it is more precisely estimated.

Fourth, we enlarge the number of *Peers-pre* by including also mothers with 4-6 years old children in 2012 (column (4)). Results remain basically unchanged with respect to the baseline regression.

As a further robustness check we include among peers-post also the peers that have been WM. This means that, in this specification, we are not only considering the direct effect of the peers-pre and peers-post, but also the effects of PLs taken by WM on other WM with younger children that can take the PLs later on. In fact, the peer effects cascade through the firm network as the first peer interacts with the working mother, which consequently becomes a peer for another mother, and so on. Results, reported in column (5), confirm our previous findings with a positive and significant effect, which is even higher in magnitude likely because comprehensive of the indirect effects.

Finally, we relax the restriction that the peers remain employed at time t in the same firm of the working mother. As explained above, this condition was motivated

Table 7: Parental leave and peer effects. Robustness check with Z=0,1.

	(1) Baseline	(2) Worker ctrl	(3) All ctrl
Peers taking PLs (%)	0.336**	0.445***	0.385**
	(0.151)	(0.153)	(0.161)
Observations	48,760	48,437	48,423
Adjusted R^2	-0.026	0.082	0.092
Worker ctrl	No	Yes	Yes
Firm ctrl	No	No	Yes
Year FE	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes
Child age FE	Yes	Yes	Yes
F-stat	31.15	29.07	23.88
First stage coeff.	0.085***	0.080***	0.072***
	(0.015)	(0.015)	(0.015)
Dep.var.mean	0.270	0.271	0.271
Dev.std. X	0.234	0.234	0.234

Note: IV estimations according to equation (1). Sample: WM with only pre or post reform peers. Dependent variable: dummy equals to one if the mother takes a parental leave (PL). Z is a dummy equal to one if the working mother has at least one post reform peer. Column (2) includes workers' characteristics (age and age squared, occupation, dummy for foreign, dummy for temporary contracts, dummy for having more than one child, dummy for low experience in the same firm); column (3) includes also firm's characteristics (sector, firm size, average gross salary, share of 25-49 years old workers, share of foreign born workers, share of female workers). Standard errors in parenthesis: * p<0.10, ** p<0.05, *** p<0.01.

by the fact that communication may be more feasible if peers and co-worker work together. However, this may lead to a selection bias, since the fact that a potential peer is no longer in the same firms could be a signal itself, for example if it is due to firing. For this reason, we replicated the baseline estimates by including also drop-out peers (column (6)). Results for the variable of interest are quite similar to the baseline presented in Table 4, suggesting that there is no systematic selection in drop-out peers with respect to the signal they convey to their working mother.[28]

5.4. Heterogeneous effects

To refine our analysis and detect the possible mechanisms behind the peer effects in parental leave choices, we explore heterogeneity in the results by interacting our main independent variable with worker's and peers' characteristics. As mentioned in section 1, there are two main mechanisms that can explain the existence of peer effects in parental leave decisions. From one hand, having peers taking parental leave may disclose the information about the employers' reaction to them, reducing uncertainty

[28] As the sample is mainly made out of workers with open-ended contracts, it is possible that drop-outs mainly reflect usual job-to-job transitions.

Table 8: Parental leave and peer effects. Robustness check changing the sample definition.

	(1) No large firms	(2) Without >1 child	(3) Year 2016	(4) Enlarge peers-pre	(5) Enlarge peers-post	(6) Include drop-out peers
Peers taking PLs (%)	0.241**	0.407***	0.581***	0.246**	0.314***	0.264***
	(0.110)	(0.140)	(0.167)	(0.111)	(0.104)	(0.099)
Observations	99,416	63,954	39,510	103,214	102,928	105,878
Adjusted R^2	0.122	0.010	0.082	0.124	0.123	0.126
Worker ctrl	Yes	Yes	Yes	Yes	Yes	Yes
Firm ctrl	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Child age FE	Yes	Yes	Yes	Yes	Yes	Yes
F-stat	67.40	45.10	41.54	65.56	77.92	81.89
First stage coeff.	0.083***	0.076***	0.124***	0.088***	0.090***	0.091***
	(0.010)	(0.011)	(0.019)	(0.011)	(0.010)	(0.010)
Dep.var.mean	0.316	0.201	0.291	0.317	0.317	0.312
Dev.std. X	0.210	0.205	0.208	0.200	0.208	0.204

Note: IV estimations according to equation (1). Dependent variable: dummy equals to one if the mother takes a parental leave (PL). Column (1) excludes large firms with total number of employees >95pc of the distribution. Column (2) excludes mothers with more than one child. Column (3) estimates the regression on the sub-sample of WM in the year 2016 only. Column (4) enlarges the window of peers pre, including also mothers with 4-6 years children in 2012. Column (5) enlarges the window of peers post, including also the peers that have been coworkers. Column (6) includes also drop-out peers. Regressions control for workers' characteristics (age and age squared, occupation, dummy for foreign, dummy for temporary contracts, dummy for having more than one child, dummy for low experience in the same firm) and firm's characteristics (sector, firm size, average gross salary, share of 25-49 years old workers, share of foreign born workers, share of female workers). Standard errors in parenthesis: *p<0.10, ** p<0.05, *** p<0.01.

concerning the possible negative consequences of the parental leave choices. On the other hand, having peers taking parental leave may promote the diffusion - among the other workers - of the information regarding the existence of these programs and their institutional details. Since, in principle, both these channels could be at work, this heterogeneity analysis may also help to shed light on which is the main mechanism behind these peer effects.

First of all, we fully interact our model with a dummy capturing if the worker has been hired in the firm since less than 5 years (low tenure variable, Table 9). We think that the importance of the information about the employees' reactions may be stronger for WM with a low experience in the firm. More recently hired workers have less knowledge about the internal dynamics of the company and about the employers' attitudes and, therefore, they consider, as a very important signal, their peers' previous behavior. The importance of this channel is confirmed by the results: we find that the coefficient of the "low tenure" variable is positive and statistically significant while the main effect is not statistically significant, meaning that the peer effects mainly operate for this type of workers, rather than for workers that have already been in the firm for a longer period.

Second, we fully interact our model with a dummy which takes value 1 for WM with a small number of peers (less than 3), in order to consider if there exist any differences according to how many peers the WM interact with. A greater number of relevant peers may imply easier information diffusion. Moreover, it could be correlated with working at larger establishment, where presence of information channels (such as union representatives) may be higher. In this respect, having a small number of observed peers may reveal lower information and, because of that, be associated with a lower probability of taking parental leaves. We find that the estimated coefficient of the interaction term is negative, but not statistically significant: the beneficial effect of peers is hence statistically the same and does not significantly depend on the number of relevant peers. We conclude that, between the two channels of signaling and knowledge of the norm, the former seems to be more important: observing parental leaves taken by peers is informative on employer's reaction and firm policy.

Table 9: Parental leave and peer effects. Heterogeneity.

	(1)	(2)
	All ctrl	All ctrl
Peers taking PLs (%)	0.107	0.277**
	(0.117)	(0.123)
× low tenure	0.598**	
	(0.240)	
× few peers		-0.074
		(0.196)
Observations	102,724	102,724
Adjusted R^2	0.117	0.131
Worker ctrl	Yes	Yes
Firm ctrl	Yes	Yes
Year FE	Yes	Yes
Municipality FE	Yes	Yes
Child age FE	Yes	Yes
Interaction on Covariates	Yes	Yes
Interacted variable	lowtenure	fewpeer
F-stat	42.62	11.10
First stage coeff.	0.090***	0.108***
	(0.010)	(0.015)
Dep.var.mean	0.317	0.317
Dev.std. X	0.210	0.210

Note. Dependent variable: dummy equals to one if the mother takes a parental leave (PL). Z is a dummy equal to one if the coworker has at least one post reform peer. Column (1) interacts with the dummy low tenure which is equal to one if the employee has low experience (less than 5 years) in the same firm; column (2) interacts with the dummy for having few peers (lower than 3). Regressions control for workers' characteristics (age and age squared, occupation, dummy for foreign, dummy for temporary contracts, dummy for having more than one child, dummy for low experience in the same firm) and firm's characteristics (sector, firm size, average gross salary, share of 25-49 years old workers, share of foreign born workers, share of female workers). Standard errors in parenthesis: * p<0.10, ** p<0.05, *** p<0.01.

6. Conclusions

Workplace social networks may play a particularly important role in influencing the parental leave choices and labor supply related decisions: observing working mothers' behavior and the employers' reaction facilitates the transmission of practical knowledge about the existence and the organizational details of the parental leave program and it reduces uncertainty concerning the consequences of leave choices. The workplace thus facilitates the transmission of behavioral norms and career-related information. While the literature that studies the effects of parental and maternal leave policies is broad, a few papers focused on the peer effects in this context.

In this work we focused on the effect on the probability of taking a parental leave that is generated by the leave decisions previously taken by the peers of the workplace, with children of the same age. We exploit a unique linked employer-employee database, which covers a representative sample of the Italian firms. In order to correctly measure the peer effects, we employ an instrumental variable approach that exploits a reform that took place in Italy in 2015, which extended the child's age up to which the mother can apply for paid parental leave and that generated a sharp increase of the probability of asking for it. This estimation strategy is able to circumvent the reflection problem and accounts for endogenous sorting of workers into peer groups and firms.

Our findings confirm the existence of peer effects in parental leave decisions: the decision of mothers regarding parental leave is influenced not only by financial or familiar considerations, but also by their peers' behavior. In particular, according to our estimates, a one standard deviation increase of the take up rate of peers corresponds to an increase of 16 per cent of the average value of the dependent variable. Our findings also show that positive peer effects exist along the intensive margin as well (i.e., the number of weeks taken). As emerged in the heterogeneity analysis, the channel driving these effects may be related to the fact that observing peers' choices reduces uncertainty on employers' behavior and on the consequences of PL, which in turn increases take-up among these individuals.

The positive peer effect has another important consequence on the mothers' supply of work. In fact, mothers that have more peers that take parental leaves are not only more likely to take themselves the leaves, but they are also less likely to take a part-time contract. Parental leaves are thus relevant tools to handle difficulties in the conciliation of working life with family care, helping the mothers to maintain a full-time position.

These results are important in order to understand the effects of social interactions on individual behavior, with important implications from a policy point of view. In fact, observing peers may be a relevant channel in the reinforcing the take-up of policy measures that help protecting young families and encouraging fertility. As our data are mainly representative of medium and large firms, we must be aware that our findings may not necessarily be the same for smaller firms, where the diffusion of work-life practices can be different, potentially affecting the scope and the role of peer effects. Further research could broaden the available evidence by explicitly focusing on small firms' workers.

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Appendix A. Dataset construction

We identify working mothers who gave birth to a child during the period 2007-2018 through a code assigned to maternity leaves (ML) in the registry of worker's events. For every year, we observe the number of full weeks and partial weeks in which ML are taken (for partial weeks we do not know the exact number of days). We consider total weeks, including both partial and full weeks. We identify maternity events as periods of contiguous ML, taking into account they may overlap across two consecutive calendar years. We keep in the analysis only instances of total periods above the legal minimum amount of ML weeks (a lower amount could be due to errors or particular situations such as hiring during maternity period). We focus on mothers that had at most two maternity events in the period: they represent almost the totality of the dataset. For every year, we observe parental leaves (PL) as number of weeks in the year, but we cannot know when they are precisely taken. This implies that we can only measure the child's age at the year level. Therefore, in order to have the child age, we need to compute the presumed year of birth. This is straightforward for maternity events occurring within a single year. For maternity events overlapping across two years, we consider the amount of ML weeks in the second year and we compute the month where they end starting from January and presume the date of birth as three months before of the ending month. For example, for a maternity episode between 2013 and 2014 for a total of 26 weeks, of which 19 in 2014, we consider that the maternity leave episode ends in May (19/4.3, where 4.3 is the average number of weeks in a month) and presume that birth occurred in February, assigning 2014 as year of birth. This assumption may generate measurement errors in assigning the correct birth's year since it may be that the delivery date could have been 4 months before the end of the maternity leave period instead of 3 months, as explained in the Section 2. However, this would imply a wrong classification of the year of birth only in the particular case of children born in December, which we would assume born in January of the following year. Moreover, the possibility to take 4 months after the baby's birth holds only for mothers taking no more than five months of total maternity leave: these mothers represent only a minority of the total sample.

We attach information on mothers' date and country of birth from the registry with worker details. We perform some further refinements by excluding particular and very marginal cases, such as 0.06% of cases where ML are assigned to male workers (errors, mother's death at delivery, adoption, ecc.), cases where mother's age at delivery is older than 50 and mothers that prematurely died during the years of our analysis.

We can then create a balanced panel dataset from 2005 to 2015 with the mothers that in the whole period gave birth to a child. For each mother and year, we associate the number of full, partial and total weeks taken as PL. We do not consider other leaves to which a worker is entitled under other circumstances (Law 104 regarding serious illness of a relative, children with handicap, hourly permission, paid leaves acknowledged to low income mothers, etc.). The parental leaves that we consider are those directly involved by the reform.

We associate to each mother in every year an identifier of the firm at which the mother worked in that year. If for a mother there is more than one firm in a given year, we consider the firm where the mother received the higher total earnings.

From the firm identifier it is possible to recover information on some firm features from the registry of the universe of firms. In this dataset we also have the firm fiscal code, by means of which we can identify the firms belonging to the INVIND sample (since the worker dataset includes the whole history of workers who happened to work in INVIND firms, it includes also work experiences in other firms). For each combination of worker-year-firm we associate the following variables from the worker dataset: broad occupation type (blue collar, white collar employees, white collar supervisor and managers), the part-time/full-time duration of the job, the condition of open-ended contract vs temporary worker, the municipality of job, the gross wage, the amount of days worked. We winsorize few outliers due to particularities or errors at 53 weeks. About 94% of the mothers in the dataset have taken in the first 3 years less than the legal maximum amount of PL, so they are potentially entitled to take other PLs in the following years.

For each worker, we compute the observed tenure (since 2005, our initial year), the wage one year before the child-birth, a dummy equal to 1 when a second child is present, worker's age, and a dummy for the worker being born abroad. We also complement the dataset with further information about the firm from the firm registry, such as size (average number of employees in the year), average salary paid, sector. We are also able to add further information at the establishment level (proxied by municipality/firm combination) about the workforce composition (share of female employees, share of foreign born employees, share of 25-49 workers) by collapsing data at the municipality-firm level from the workers' registry matched with the firm registry.

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