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Workplace Peer Effects in Fertility Decisions^{*}

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Abstract

This paper examines how co-workers' fertility affects individual childbearing. Using matched employer-employee data from Italian social security records for 2016–2020, we estimate how fertility among colleagues in the same establishment and of similar age and occupation affects a worker's probability of having a child. We exploit variation generated by the 2015 Jobs Act, which weakened employment protection for newly hired workers and reduced their fertility. Focusing on incumbent workers hired before the reform and using the share of colleagues hired after the reform as an instrument for peer fertility, we find that a one-percentage-point increase in peer fertility at time $t-1$ raises a worker's probability of conceiving a child at time t by 0.2–0.4 percentage points, corresponding to a 6%–11% increase relative to the sample mean. A placebo test using colleagues in other establishments of the same firm yields no effect, consistent with direct workplace interaction playing a role. Estimated responses are concentrated among younger workers and at first births and are stronger among workers with longer tenure and in less competitive firms. Our findings indicate that the workplace is a relevant social context for fertility decisions and suggest that labor-market institutions may generate spillovers to workers not directly covered by policy changes.

Keywords: EPL, Fertility, Social Influence, Workplace

JEL Classification: C3, J13, J65, J41, M51

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1. Introduction

Childbearing decisions crucially depend on economic resources and labor-market conditions (see e.g., Becker, 1981; Francesconi, 2002; Currie and Schwandt, 2014; Cumming and Dettling, 2024),¹ but they are also embedded in social relationships. Individuals observe others' family transitions, exchange information about parenthood, and may respond to local norms concerning whether and when to have children. Identifying these social influences is difficult because peers select into common environments and may respond to the same shocks. Existing evidence documents peer effects in fertility within siblings, friends, religious communities, and neighborhoods (Evans *et al.*, 1992; Kohler, 2001; Heissel, 2021; Spolaore and Wacziarg, 2022; Rossi and Xiao, 2024).² However, the workplace remains comparatively understudied as a demographic context.

This paper contributes to filling this gap in the literature by studying how co-workers affect individual childbearing choices. The workplace is a central social environment, characterized by repeated interactions and close observation of others' life choices, which may influence preferences, expectations, and timing decisions. At the same time, the workplace has become increasingly relevant for fertility outcomes given the growing importance of work–life balance and employer policies. Despite this, existing evidence of workplace peer effects in fertility is relatively sparse and mixed. While studies for Sweden, Germany, and the Netherlands find positive peer effects (Asphjell *et al.*, 2013; Pink *et al.*, 2014; Buyukkececi *et al.*, 2020), Ciliberto *et al.* (2016) document negative peer effects in Denmark.

Our paper complements the literature by providing, to our knowledge, the first evidence on co-worker fertility spillovers in Italy. Our empirical strategy is related to Li and Zhang (2009) and Rossi and Xiao (2024), who exploit variation generated by China's family-planning policies to identify fertility spillovers across neighboring women and culturally proximate groups, respectively. We instead use policy-induced variation in fertility within workplace peer groups. Relative to previous workplace studies that rely on random effects (Pink *et al.*, 2014), placebo peer groups (Asphjell *et al.*, 2013), or instruments based on the fertility of co-workers' siblings (Ciliberto *et al.*, 2016; Buyukkececi *et al.*, 2020) to identify peer effects, our design has two distinguishing features. First, the identifying variation in peer fertility originates from a policy change that directly affects co-workers themselves, rather than being inferred from indirect social ties such as co-workers' siblings. Second, the approach applies to a substantially larger population of workers and does not require their co-workers' siblings to be observed.

We leverage rich administrative data from the Italian Social Security Institute (INPS), covering the universe of private sector employees from 2016 to 2020. Our identification strategy exploits variation in peer

¹ See also Adsera (2005), Milligan (2005), Lindo (2010), Del Bono *et al.* (2012), Cohen *et al.* (2013); González (2013), Lovenheim and Mumford (2013), Dettling and Kearney (2014), Huttunen and Kellokumpu (2016), De Paola *et al.* (2021), Clark and Lepinteur (2022), and Doepke *et al.* (2023).

² See also Bongaarts and Watkins (1996), Montgomery and Casterline (1996), Kuziemko (2006), Munshi and Myaux (2006), Li and Zhang (2009), Lyngstad and Prskawetz (2010), Kotte and Ludwig (2011), Balbo and Barban (2014), Fletcher and Yakusheva (2016), Cools and Hart (2017), Daudin *et al.* (2019), Buyukkececi *et al.* (2020), and Guo *et al.* (2026). There is also related evidence on peer effects in household formation (Aparicio-Fenoll and Oppedisano, 2016; Adamopoulou and Kaya, 2018), and parental leave take-up (Dahl *et al.*, 2014; Dottori *et al.*, 2023; Welteke and Wrohlich, 2019; Casarico *et al.* 2025).

fertility induced by the 2015 Italian Jobs Act, which reduced dismissal protection for workers hired on open-ended contracts after March 7, 2015 in firms with more than 15 employees. Although the reform did not mechanically reduce these workers' current wages, it increased both their perceived and actual risk of job loss. Boeri and Garibaldi (2019) document that, immediately following the reform, dismissals of workers on open-ended contracts increased by more than 50% in large firms, while remaining broadly stable in small firms. Bertoni et al. (2023) show that, among workers with low tenure, exposure to the Jobs Act increased fear of job loss by 17% and the probability of dismissal by 2%. The reform therefore affected fertility-relevant conditions mainly through uncertainty about future employment rather than through current income.

Employment insecurity may affect fertility through several related channels. A higher probability of job loss may reduce expected future income and increase uncertainty about the resources available for raising a child, even when current earnings are unaffected. More generally, because childbearing is a costly and partly irreversible life-course transition, uncertainty about future employment can increase the value of waiting and induce workers to postpone fertility until their employment prospects become clearer. If postponed births are not fully recuperated later, this can translate into lower completed fertility. The possibility of dismissal or career interruption may also increase the expected professional costs of childbearing, particularly for women. All three channels thus stem from the increased risk of future job loss. Although our data do not allow us to distinguish sharply between expected-income, uncertainty, and career-continuity mechanisms, the evidence suggests that changes in employment protection primarily affect fertility choices by increasing employment uncertainty (Bratti et al., 2005; Prifti and Vuri, 2013; Clark and Lepinteur, 2022; De Paola et al., 2022). Consistent with this interpretation, De Paola et al. (2021) document a two-percentage-point decline in fertility among female workers directly affected by the Jobs Act.

We focus on incumbent workers hired before the reform in firms with more than 15 employees, whose dismissal protection remained unchanged, and use the share of their co-workers hired under the new regime as an instrument for peer fertility. The 15-employee threshold determines whether workers hired after the reform were subject to the new dismissal-protection regime, but our design is not based on a local comparison of firms around that threshold. The identifying variation comes instead from differences in the share of post-reform hires across workplace–occupation–age peer groups among all firms above the threshold. As employment in covered firms might be unevenly distributed across regions and sectors and vary over time, our empirical model includes year, region and sector fixed effects, which absorb persistent differences in fertility and firm-size composition across these dimensions. In some specifications, we also include firm fixed effects to account for unobserved firm-level heterogeneity.

We operationalize the relevant peer group as co-workers in the same establishment, five-year age band, and broad occupational category. This definition is intended to approximate a salient reference group, characterized by more frequent exchanges, rather than to imply that interactions with all other colleagues are absent. Age similarity places workers at comparable stages of the reproductive and working life course, with similar fertility horizons, parity distributions, and timing decisions.³ Occupational similarity captures common

³ The five-year age interval balances social comparability with the need to retain sufficiently large peer groups.

tasks, schedules, earnings opportunities, career ladders, and exposure to similar work–family constraints. Consequently, the experience of parenthood among colleagues of similar age and occupation is likely to provide more transferable information about leave-taking, employer support, and career consequences than the experience of workers facing substantially different professional or life-course circumstances. Defining peers within age–occupation cells also ensures that exposure to Jobs Act hires is not confounded with the age composition of the establishment’s workforce.

Our first-stage estimates confirm the reform’s negative effect on fertility estimated by De Paola *et al.* (2021): moving from a peer group with no Jobs Act hires to one composed entirely of Jobs Act hires is predicted to lower average peer fertility by 1 to 2 percentage points. This is interpretable as an employment–uncertainty effect on fertility among fully exposed co-workers, consistent with postponement in response to a higher risk of future job loss. In the second stage, we find that a one-percentage point increase in peer fertility in year $t-1$ increases an individual’s likelihood of having a child in year t by 0.2 to 0.4 percentage points, corresponding to a 6% to 11% increase relative to the mean. This magnitude is consistent with existing evidence from other countries (Asphjell *et al.*, 2013; Buyukkececi *et al.*, 2020) and highlights the importance of social influence in fertility decisions.

The exclusion restriction requires that exposure to Jobs Act hires affect incumbent workers’ fertility only through its effect on their co-workers’ fertility. Restricting the sample to workers hired before the reform is important because these workers retained their pre-reform dismissal protection and were therefore not directly treated by the Jobs Act. Since the reform affects fertility mainly by increasing uncertainty about one’s own future employment, incumbents’ fertility should not respond directly to a change in a dismissal regime that does not apply to them.

This restriction is not, however, sufficient by itself to guarantee instrument validity. Working alongside less-protected colleagues could make job insecurity more salient to incumbent workers, signal a change in the firm’s personnel strategy, alter perceived opportunities for career advancement, or intensify internal competition. If these responses affected incumbents’ fertility independently of the fertility behavior of their colleagues, the exclusion restriction would be violated.

We provide several complementary pieces of evidence addressing this concern. First, we restrict the sample to firms with stable employment levels over time. This exercise reduces the possibility that exposure to Jobs Act hires captures firm expansion, contraction, or broader changes in workforce composition, although it cannot exclude perception-based channels. Second, using multi-establishment firms, we construct placebo peer groups from workers employed in other establishments of the same firm, with whom direct interaction is substantially less likely. Fertility among these “false peers” has no effect on individual fertility. This finding is inconsistent with explanations based on firm-wide shocks, although it does not rule out direct effects generated by interactions with new hires in the same establishment.

Finally, we use an alternative instrument based on peers’ access to municipal childcare services. This instrument generates variation in peer fertility that is unrelated to employment protection and therefore does not operate through the salience of the Jobs Act or through the presence of less-protected hires. The resulting

estimates closely mirror the baseline results, including in specifications with firm fixed effects. Although these exercises do not conclusively establish the validity of the exclusion restriction, the convergence of results across instruments exploiting different sources of variation provides support for interpreting the baseline estimates as workplace fertility peer effects.

We next examine how estimated peer effects vary across different workers' characteristics and different workplaces. We first consider gender differences and peer-group composition. Peer effects are smaller for women than for men and are further attenuated when women are exposed to female peers.⁴ Part of this gender difference reflects the different age composition of the female and male samples. This pattern is consistent with women facing higher expected time and career costs of childbearing, particularly early in their careers (Adda *et al.*, 2017; Kleven *et al.*, 2019). It is also compatible with female colleagues' experiences providing women with especially relevant information about the challenges of combining employment and parenthood.

We also consider heterogeneity over the reproductive life course, classifying women and men according to the gender-specific average age at first parenthood. Peer effects are positive and statistically significant among younger women and men, but not statistically distinguishable from zero among older workers. Because our outcome captures whether a conception resulting in a live birth occurs in a particular year rather than completed fertility, the effect among younger workers may reflect an acceleration of births that would otherwise have occurred later. The available observation window, however, does not allow us to distinguish conclusively between such tempo effects and effects on completed fertility or quantum effects.

We also find that peer effects are positive for first births but not for higher-order births. This pattern is compatible with information about parenthood being more valuable before individuals have children (Lyngstad and Prskawetz, 2010), although differences in constraints or selection across parity groups could also contribute. Moreover, estimated effects at first birth are smaller for women than for men, a finding consistent, among other explanations, with gender differences in the expected professional and time costs of parenthood.

Next, we investigate the potential role of social norms using municipal-level proxies for local gender-role norms. Women are significantly less responsive to peer fertility in municipalities characterized by more traditional gender roles, whereas no comparable pattern emerges for men. This finding is consistent with workplace peers being a less salient reference group for women in contexts where fertility decisions may be more strongly shaped by partners, families, or local communities. However, because municipalities also differ in childcare provision, labor-market opportunities, economic conditions, and other characteristics, this heterogeneity should not be interpreted as a causal test of the social-norm channel.

Finally, we show heterogeneity by tenure and firm-level competition. Estimated peer effects are stronger among workers with longer tenure and in less competitive firms. These patterns are compatible with employment uncertainty and internal labor-market competition increasing the expected costs of responding to

⁴ Female colleagues' experiences may provide particularly relevant information about the difficulties of combining work and family life. Kuziemko *et al.* (2018) document that women frequently report that the consequences of parenthood are more difficult than they had anticipated.

peer fertility. Taken together, the heterogeneity results show that responsiveness to peer fertility varies across workers, workplaces, and stages of the reproductive and working life course, but they should not be interpreted as identifying the mechanisms underlying the average effect.

Overall, our findings indicate that the workplace is a relevant demographic context for fertility decisions. Under our identifying assumptions, they suggest that changes in co-workers' fertility induced by labor-market institutions may spill over to workers who are not directly exposed to the policy change, influencing their fertility behavior. We do not interpret this result as showing that workplace peer effects necessarily affect aggregate fertility or generate a social multiplier in equilibrium. Rather, it indicates that fertility decisions may be interdependent within workplaces and that such interdependence should be considered when assessing the consequences of labor-market reforms.

The remainder of the paper is organized as follows. Section 2 introduces the conceptual framework. Section 3 describes the institutional setting. Section 4 presents the data. Section 5 outlines the identification strategy and main results. Section 6 reports robustness checks and placebo tests. Section 7 examines heterogeneous effects. Section 8 concludes.

2. Conceptual Framework

To organize the discussion, we use a simple conceptual framework rather than a structural model. Consider a simplified setting in which, in each period t , individual i chooses whether or not to have a child. The individual's utility from having a child in period t is given by⁵:

$$U_i^t(\text{Child}_i^t = 1) = B_i^t + S(\bar{F}_{-i}^{t-1}) - C_i^t + \varepsilon_i^t$$

where B_i^t captures the baseline perceived benefits of having a child in period t , including individual preferences, partnership and parity circumstances, age-specific fecundity, the remaining reproductive horizon, and other life-course factors. The function $S(\bar{F}_{-i}^{t-1})$ captures the net influence of fertility among i 's co-workers in the previous period, excluding worker i . This influence may be positive or negative. The term C_i^t represents the expected economic, time, and career costs of childbearing. The residual term ε_i^t captures unobserved

⁵ The utility expression should be interpreted as describing an individual's latent propensity to pursue childbearing, rather than as implying a deterministic mapping from fertility preferences to an observed birth. A conception resulting in a live birth may fail to occur despite a positive propensity because of fecundity constraints, unsuccessful conception, or pregnancy loss. Conversely, it may occur without a prior intention when contraception is not used or fails and the pregnancy is carried to term. Although no direct estimate of the prevalence of unintended births is available, descriptive evidence indicates that reproductive control was widely practiced in Italy during our observation period. In 2019, 76% of sexually active women aged 18–49 reported using at least one contraceptive method, while 17% reported having used emergency contraception (Istat, 2024). In addition, voluntary termination of pregnancy has been legally available since 1978, and the induced-abortion rate declined from 6.5 to 5.4 per 1,000 women aged 15–49 between 2016 and 2020. Taken together, these indicators are consistent with unintended births accounting for a limited share of observed fertility.

determinants of fertility, including biological factors and imperfect control over conception and birth outcomes.

An observed conception resulting in a live birth at time t occurs when:

$$B_i^t + S(\bar{F}_{-i}^{t-1}) - C_i^t + \varepsilon_i^t > 0$$

Both the baseline perceived benefits and the expected costs of childbearing may vary systematically across workers, workplaces, and stages of the reproductive and working life course. Age is particularly relevant because it may affect these components as well as the salience of peer fertility. Younger workers have a longer reproductive horizon and greater flexibility to postpone childbearing, which may weaken their immediate response to peer fertility. At the same time, co-workers' fertility may be particularly salient at younger ages, when family-formation decisions are still being made. Older workers face a shorter remaining reproductive horizon, potentially making peer signals more consequential. Conversely, declining fecundity and greater progress toward completed family size may reduce responsiveness at older ages. These competing arguments make the expected age pattern theoretically ambiguous.

Expected childbearing costs may also differ by gender. Women continue to bear a disproportionate share of the time and career costs associated with parenthood, implying that the expected cost of childbearing may be particularly high for women, especially early in their careers (Adda et al., 2017; Kleven et al., 2019, Biasi and De Paola, 2025). Doepke and Kindermann (2019) show that women in several European countries are considerably more likely than men to oppose having an additional child and that disagreement within couples strongly predicts lower subsequent fertility. They relate this asymmetry to the unequal division of childcare, suggesting that women are more frequently the “binding” partner in fertility decisions. These considerations suggest that a given positive influence from workplace peers may translate less readily into an observed birth among women. An opposing prediction is also plausible, however. More binding biological constraints on the timing of fertility and the higher stakes associated with childbearing may make women more attentive to peer signals about the feasibility and costs of combining work and parenthood, potentially strengthening their response. The framework therefore yields no unambiguous prediction regarding gender differences in workplace peer effects.

Expected costs may also vary across workers depending on their employment conditions and workplace environments. In particular, the cost term C_i^t may depend on tenure, employment security, and the degree of competition within the firm. When employment is less secure or internal competition is stronger, taking leave or reducing labor supply may entail greater risks for career progression. Conversely, longer tenure or weaker workplace competition may reduce career uncertainty and the expected professional costs of childbearing, leaving more scope for peer influence to affect realized fertility. In this framework, workplace competition and career concerns are therefore not additional forms of social influence. Rather, they are constraints that may condition workers' ability or willingness to respond to peer fertility. This distinction motivates our heterogeneity analyses by tenure and firm-level competition.

The cost component in the conceptual framework also clarifies the role of employment protection in our empirical strategy. For workers hired after the Jobs Act, weaker dismissal protection did not mechanically reduce current wages but may have increased the expected costs of childbearing through greater uncertainty about future employment and heightened concerns about career continuity. Such uncertainty may induce workers to postpone childbearing until their employment prospects become more secure. In the framework, thus, the reform enters through the perceived probability of future job loss, while current wages are held fixed. This provides the conceptual basis for our first-stage relationship: weaker protection increases C_i^t among exposed workers and thereby reduces their fertility.

Whereas these individual and workplace characteristics shape the perceived benefits and costs of childbearing or condition the response to peer fertility, the net social-influence term $S(\bar{F}_{-i}^{t-1})$ captures changes induced by co-workers' fertility itself. This influence may reflect three conceptually distinct but empirically overlapping mechanisms: social contagion, social norms, and social learning. These mechanisms provide a useful framework for interpreting both the average peer effect and the heterogeneous effects explored in the empirical analysis, as differences across workers and workplaces may reflect the relative importance of these channels. However, such differences cannot be used to identify the mechanisms separately. The three channels may operate simultaneously, reinforce or offset one another, and generate similar empirical patterns across population subgroups.

Social contagion refers to affective or behavioural spillovers that lead individuals to adjust their fertility behaviour in response to that of socially proximate peers. Interacting with colleagues who have recently become parents may make childbearing more salient, evoke positive emotions, or increase the desire to share similar life experiences. Such responses may occur consciously through social comparison or less consciously through a tendency to coordinate important life-course transitions with members of one's social network (Montgomery and Casterline, 1996; Bernardi and Klärner, 2014). Contagion may be stronger when peers are perceived as socially similar. In our setting, co-workers of comparable age and occupational status may therefore be particularly salient reference points. This channel generally predicts a positive response to peer fertility, although a stronger response to similar or more salient peers would not, by itself, distinguish contagion from norms or learning.

Social norms operate through shared perceptions of what is typical or appropriate within a social group (Rindfuss et al., 1988; Fletcher and Yakusheva, 2016). Co-workers' childbearing may make parenthood at a given age or career stage appear more common or expected, whereas low peer fertility may normalize postponement or signal that parenthood is incompatible with professional commitment. In settings characterized by more traditional gender-role attitudes, co-workers' fertility may reinforce broader expectations surrounding parenthood and family formation, thereby amplifying workplace peer effects. An alternative prediction is equally plausible: if fertility decisions in traditional settings are shaped primarily by partners, families, or local communities, the workplace may be a less important reference group. Traditional gender norms may therefore either strengthen or weaken workplace peer effects. Heterogeneity across normative contexts can indicate which interpretation is more consistent with the evidence, but cannot identify

social norms as the causal mechanism because these contexts differ along other dimensions, including childcare provision, labor-market opportunities and economic conditions.

Social learning occurs when workers update their beliefs about the costs and benefits of childbearing by observing or communicating with colleagues. Peers provide information about parental leave, employer support, work–family balance, and the career consequences of parenthood (Montgomery and Casterline, 1996; Kohler, 2001; Bernardi, 2003; Yakusheva and Fletcher, 2015). Observing colleagues who return smoothly from leave and successfully combine work and family may reduce the perceived costs of parenthood and encourage fertility. Positive experiences may reduce perceived costs and encourage fertility, whereas difficulties combining work and parenthood may have the opposite effect. Such information may be especially valuable before the first birth and when peers face similar professional and family constraints. Female colleagues' experiences, for example, may be particularly informative to women. The resulting response can nevertheless be positive or negative depending on the experiences observed. Social learning can consequently strengthen or weaken the response to peer fertility.

Peer fertility could also affect realized fertility through changes in contraceptive behaviour or decisions following an unintended conception. However, the administrative data at our disposal do not contain the information needed to examine these responses separately, and their contribution is therefore subsumed in the reduced-form peer effect.

Our empirical analysis therefore estimates the average net effect of peer fertility and examines whether it varies by gender, age, parity, peer-group composition, local gender-role attitudes, tenure, and workplace competition. These heterogeneity patterns can indicate whether the results are compatible with particular mechanisms, but they do not constitute separate tests of social contagion, social norms, or social learning. Nevertheless, they are informative in their own right: by showing for whom and in which settings workplace peer effects are stronger or weaker, they help characterize the scope and distribution of fertility spillovers within firms, even when the underlying mechanisms cannot be separately identified.

3. Institutional Setting

Historically, the Italian labor market has been characterized by strict employment protection legislation (EPL). Initially, the EPL provisions applied mainly to firms with more than 15 employees, while smaller firms were largely exempt. Although a 1990 law introduced some restrictions on dismissals for small firms, employees in larger firms continued to enjoy stronger protection. The first attempt to reduce dismissal costs for firms above the 15-employee cut-off was made through the Fornero reform of 2012, which limited reinstatement in the case of unjustified dismissal and reduced severance compensation. The obligation of reinstatement was still present in many situations, however.

In 2015, the government led by Matteo Renzi implemented the "Jobs Act" as a second attempt to address labor market segmentation in Italy. These reforms greatly restricted the possibility of reinstatement, making it applicable only in cases of discriminatory or specific disciplinary dismissal. Unfair dismissals were

now to be compensated by a predetermined monetary payment based on seniority. The reduction in employment protection applied only to firms above the 15-employee threshold: specifically, to all new permanent hires in these firms after 7 March 2015, when the Law went into force, and not to workers hired previously, who are still covered by the reinstatement clause. Firms with a workforce below the 15-employee threshold remained largely unaffected by the reform, as the reinstatement clause did not apply to them even before.

The introduction of the Jobs Act, with its reduced employment protection for new hires, constitutes the basis for our identification strategy that we discuss in Section 5.

4. Data and Descriptive Statistics

Our analysis relies on a matched employer- employee dataset, provided by the Italian Social Security Institute (INPS), which encompasses the universe of private-sector, non-agricultural firms with at least one employee.⁶ In the cases in which the worker has multiple job contracts in a year, we retain only the primary job, defined as the one with the highest annual earnings.⁷ For each worker-firm record, we have access to detailed information on contract start and end dates, contract type (permanent vs. temporary, full-time vs. part-time), occupation type (blue-collar, white-collar, managerial), annual earnings, number of days worked, reasons for termination (e.g. layoff, resignation), and unique identifiers for both the worker and the firm, thanks to which we can link these records to others containing essential details such as workers' gender, date of birth, citizenship, municipality of residence, ATECO-07 sector, and business location. Additionally, we observe a measure of the company workforce ("*forza aziendale*"), presented as full-time equivalent, on a yearly basis, which we use to identify the 15-employee threshold.⁸

To measure fertility, we use INPS administrative records on compulsory maternity leave for female workers. Because no equivalent mandatory administrative record of fatherhood is available for men, we identify their children through the Universal Child Allowance (*Assegno Unico e Universale*, AUU). Introduced in 2022, the AUU provides a national benefit to all households with children under age 21. This administrative register records identifiers for both parents and the child's date of birth. Although dates of birth are obtained from administrative documentation rather than respondent recall, births may be missed if no AUU application is filed. Among children born between 2015 and 2020, AUU take-up is approximately 95% in the eligible Italian population. We therefore treat AUU applications as a high-coverage measure of births at the individual level.⁹ In the yearly panel, we date conception as the child's birth date minus nine months. Our outcome

⁶ These data cover the universe of labor contracts from the UNIEMENS modules, which all Italian firms must file with INPS.

⁷ The few ties are resolved by picking the primary job at random.

⁸ As noted by Boeri and Garibaldi (2019), this measure is a good proxy of the measure adopted by the Italian labor courts, which considers the average number of full-time open-ended contracts in the last 6 months, the full time equivalent of the number of part-time open-ended contracts in the last 6 months, and the average number of fixed-term employees hired in the last 24 months weighted by their effective job duration.

⁹ In our data, 97% of births independently identified through mothers' compulsory maternity-leave records are also recovered through the AUU. The mothers associated with the remaining 3% of unmatched births differ systematically:

variable “*Child*” is an indicator for individuals who conceived a child that resulted in an observed live birth within a specific year during the period 2016–2020.¹⁰ We focus on workers of reproductive age — women aged 16–46 and men aged 16–56 — employed in firms with more than 15 employees and hired before the Jobs Act took effect on March 7, 2015. These workers remained covered by the pre-reform employment-protection regime and were therefore not directly subject to the reduction in dismissal protection.

For each worker, the establishment identifier allows us to construct annual workplace peer groups. In our main analysis, the peer group at time t contains co-workers in the same establishment (i.e. workplace), occupation (blue-collar, white-collar, manager), and five-year age band. This definition is intended to balance social proximity and statistical support: a common establishment makes interaction feasible, occupation groups workers with related tasks and status, and age approximates a comparable life-course stage. Similarity may make a colleague's transition more informative and behaviorally salient.¹¹ Our main explanatory variable is the fertility rate of each worker's peer group at time $t-1$ (lagged), constructed from co-workers' births during 2015–2019.

Table A.1 presents descriptive statistics for the analysis sample. We observe 11,008,833 worker-year observations from 111,521 firms and 134,571 distinct establishments. Our definition based on establishment, occupation, and five-year age group yields 1,458,812 peer groups. The fertility outcome equals one in 3.7% of worker-year observations: that is, 3.7% of observations record a conception during the year that subsequently resulted in a live birth. This figure is an annual outcome mean, rather than the proportion of workers who had at least one child over the entire 2016–2020 period.¹² Women account for 32.5% of the observations, while 10.7% of workers have an immigrant background. On average, workers are 41 years old, have 7.6 years of tenure, and have accumulated 19.1 years of labor-market experience. Approximately 47% are blue-collar workers, 45% are white-collar workers, and 6% are managers; 15% work part-time. Most observations are located in Northern Italy (63.5%), followed by the Centre (19.4%) and the South (17%). The median peer group contains 23 co-workers, and, on average, 16.7% of a worker's peers were hired after the implementation of the Jobs Act.

they have higher average earnings and are more likely to have an immigrant background. Although we cannot test whether the same selection characterizes fathers, a similar pattern is nevertheless plausible because AUU eligibility and applications are determined at the household level. The implications of this undercoverage for estimates involving fathers are ambiguous. If non take-up is unrelated to the instrument and to treatment effect heterogeneity, conditional on the included controls, it would only attenuate the estimates, roughly in proportion to the share of unrecovered births. If non-take-up instead covaries with the instrument, workplace composition, or heterogeneous responses to peer fertility, the direction of the resulting bias cannot be determined. Given the 97% recovery rate in the maternal sample, the magnitude of any bias is plausibly limited.

¹⁰ Our administrative data do not reveal fertility intentions or contraceptive behavior and do not capture unsuccessful conceptions, miscarriages, or induced abortions.

¹¹ The definition is nonetheless an empirical approximation, not a claim that influence is homogeneous within cells or absent across their boundaries. Slightly older colleagues may be especially informative as near-future role models, and cross-occupation contact may matter in small establishments. Fixed bands also separate workers who are close in age but fall on opposite sides of a cutoff. Appendix Table A.6 relaxes the occupation restriction, considers co-workers in immediately older age groups and considers gender-specific peer groups.

¹² Among women, the annual rate is 4.4%, compared with a general fertility rate of approximately 4.0% among resident women aged 16–46 in Italy over 2016–2020 (authors' calculations based on ISTAT data). Fertility in our sample is therefore comparable to that of the corresponding population.

The relatively high mean age reflects the sample's composition and the requirements of our identification strategy. Men constitute 67.5% of observations and have a mean age of 42, compared with 38 among women.¹³ Using the same sex-specific age ranges, the corresponding population-weighted means based on ISTAT data are 38 and 33, respectively.¹⁴ This difference partly reflects our focus on incumbent workers hired before March 7, 2015, which excludes younger post-reform entrants as focal workers. Our estimates therefore apply to incumbent private-sector workers and average responses across different stages of the reproductive life course, a dimension examined explicitly in Section 7.

The sample is also restricted to firms above the statutory size threshold. The share of employment in these firms is higher in the North-East (53%), North-West (51%), and Centre (43%) than in the South (39%) and Islands (31%), and is particularly high in industry (48%); construction and services are more prevalent among smaller firms. Although these differences do not by themselves invalidate the instrument, they limit external validity. Our estimates are therefore most informative about incumbent workers in covered firms and should not be extrapolated mechanically to small-firm employees or weakly represented settings.

5. Identification Strategy and Main Results

Our analysis provides empirical evidence on how individual fertility decisions are influenced by the fertility behavior of co-workers (peers). Identifying peer effects raises well-known econometric challenges (Manski, 1993; Lyle, 2007). The first is the *reflection problem*, whereby peer fertility is determined simultaneously, making it difficult to distinguish the influence from mutual determination. The second concerns *correlated effects*: unobserved group-level characteristics — such as workplace-specific factors — may jointly affect individual fertility choices. The third challenge is *selection*, as peer groups are typically formed endogenously and individuals may sort into groups based on unobserved traits correlated with fertility.

To estimate peer effects, we regress an individual's probability of conceiving a child on the average fertility of workplace peers using the following Linear Probability Model:

$$Child_i^t = \beta_0 + \beta_1 \bar{F}_{-i}^{t-1} + \beta_2 X_i^t + u_i^t \quad (1)$$

where $Child_i^t$ is an indicator equal to 1 if individual i conceives a child resulting in a live birth in year t . The variable $\bar{F}_{-i}^{t-1}$ denotes the average fertility rate in year $t-1$ among all i 's co-workers in the same establishment, occupation (blue collar, white collar, manager) and age group (≤ 20 , 21-25, 26-30, 31-35, 36-40, >40). The vector X_i^t includes individual characteristics (female, age, age squared, tenure, experience, immigrant, type of

¹³ Among women, the 25th, 50th, 75th, and 95th percentiles of age in the analysis sample are 35, 39, 43, and 46 years, respectively; among men, they are 37, 43, 48, and 53 years.

¹⁴ Population benchmarks are authors' calculations using ISTAT resident-population counts by single year of age, gender, and calendar year, pooled over 2016–2020 and restricted to the same age ranges used in the analysis. The benchmarks refer to the resident population rather than the employed population and are reported only to characterize the external validity of the sample, not to imply that the analysis sample should reproduce the population distribution.

occupation, region of residence, year and sector dummies) and peer-group characteristics (% of female peers, % immigrant peers, % part-time peers, average peer tenure, and average peer experience), and u_i^t is an error term. Standard errors are clustered at the firm level.

The OLS estimate of β_1 may be biased due to correlation between peer fertility and the error term. We therefore adopt an instrumental variable approach, exploiting the 2015 Jobs Act reform, which reduced employment protection for newly hired workers and has been shown to lower fertility (De Paola *et al.*, 2021). Specifically, we study workers hired before the reform in large firms during 2016–2020 and examine how their fertility responds to variation in peer fertility driven by exposure to co-workers hired under the new regime. Equation (1) is thus estimated using Two-Stage Least Squares (2SLS).

In the first stage, we model peer fertility ($\bar{F}_{-i}^{t-1}$) as a function of the share of co-workers hired after the Jobs Act within the same workplace, occupation, and age group:

$$\bar{F}_{-i}^{t-1} = \pi_0 + \pi_1 JA_{-i}^{t-1} + \pi_2 X_{-i}^{t-1} + e_{-i}^{t-1} \quad (2)$$

Where JA_{-i}^{t-1} denotes the fraction of peers exposed to lower job security under the reform. We expect $\pi_1 < 0$, reflecting lower fertility among peers hired under the Jobs Act. X_{-i}^{t-1} includes peer-group characteristics such as average age, tenure, experience, and the shares of female, immigrant, and part-time workers.

We verify the relevance of our instrument by estimating equation 2. The results are reported in Table A.2 in the Appendix. Column 1 includes individual and peer controls; columns 2–4 progressively add year, region, and sector fixed effects. The preferred specification in column 4 shows that moving from zero to full exposure to Jobs Act hires reduces average peer fertility by nearly 2 percentage points. This effect closely matches De Paola *et al.* (2021), who estimate a two-percentage-point reduction using Labor Force Survey data. The coefficient is highly statistically significant, with a first-stage F -statistic exceeding 1,000.

Restricting the sample to workers hired before the reform ensures that the focal workers did not directly lose statutory protection. It does not, however, rule out violations of the exclusion restriction. Validity additionally requires that the peer-group share of Jobs Act hires affect incumbent fertility only through those peers' fertility, conditional on the covariates and fixed effects. Direct changes in incumbents' perceived insecurity or career competition would violate that condition. We address these potential violations through extensive robustness checks and a placebo test in Section 6. Under the exclusion restriction, the 2SLS estimate of β_1 captures the causal effect of peer fertility on individual fertility decisions.¹⁵

Table 1 presents the baseline 2SLS estimates.¹⁶ Under the exclusion restriction, the coefficient in column 1 implies that a one-percentage-point increase in average peer fertility raises a worker's annual probability of having a child by 0.45 percentage points. Control variables behave as expected. Fertility exhibits

¹⁵ Reduced-form estimates, reported in Table A.3 in the Appendix, show a significant negative effect of the fraction of co-workers hired under the Jobs Act on the probability of conceiving a child for workers hired before the Jobs Act.

¹⁶ The OLS estimates of equation (1) are reported in Table A.4 in the Appendix.

a concave relationship with age, peaking at 32.8 years; immigrants have a higher probability of childbearing; and greater labor market experience and part-time employment are associated with lower fertility.

Table 1. Individual Fertility and Fertility of Peers. 2SLS Estimates

	(1)	(2)	(3)	(4)
<i>Second-stage regressions</i>				
Peer Fertility Rate	0.4538*** (0.0299)	0.4044*** (0.0276)	0.4088*** (0.0273)	0.4023*** (0.0276)
Female	-0.0049*** (0.0003)	-0.0052*** (0.0002)	-0.0050*** (0.0002)	-0.0049*** (0.0002)
Age	0.0041*** (0.0001)	0.0042*** (0.0001)	0.0041*** (0.0001)	0.0041*** (0.0001)
Age Squared	-0.0001*** (0.0000)	-0.0001*** (0.0000)	-0.0001*** (0.0000)	-0.0001*** (0.0000)
Immigrant	0.0081*** (0.0003)	0.0081*** (0.0003)	0.0086*** (0.0003)	0.0088*** (0.0003)
Tenure	-0.0003*** (0.0000)	-0.0002*** (0.0000)	-0.0002*** (0.0000)	-0.0002*** (0.0000)
Experience	-0.0004*** (0.0000)	-0.0004*** (0.0000)	-0.0004*** (0.0000)	-0.0004*** (0.0000)
Part-Time	-0.0082*** (0.0002)	-0.0083*** (0.0002)	-0.0084*** (0.0002)	-0.0083*** (0.0002)
Blue-Collar	-0.0021*** (0.0002)	-0.0022*** (0.0003)	-0.0028*** (0.0003)	-0.0023*** (0.0003)
White-Collar	0.0010*** (0.0003)	0.0012*** (0.0003)	0.0009** (0.0003)	0.0012*** (0.0003)
Peer Fraction of Female	0.0039*** (0.0005)	0.0044*** (0.0005)	0.0049*** (0.0005)	0.0056*** (0.0005)
Peer Fraction of Immigrants	-0.0064*** (0.0004)	-0.0063*** (0.0004)	-0.0050*** (0.0004)	-0.0043*** (0.0005)
Peer Tenure	0.0000 (0.0000)	0.0000 (0.0000)	0.0000 (0.0000)	0.0000 (0.0000)
Peer Experience	-0.0005*** (0.0000)	-0.0005*** (0.0000)	-0.0005*** (0.0000)	-0.0005*** (0.0000)
Peer Fraction of Part-Time	0.0023*** (0.0005)	0.0019*** (0.0005)	0.0011* (0.0005)	0.0012* (0.0005)
<i>First-stage regressions</i>				
Fraction Jobs Act Peers	-0.0142*** (0.0004)	-0.0189*** (0.0006)	-0.0191*** (0.0005)	-0.0188*** (0.0005)
<i>Weak identification test</i>				
Kleibergen-Paap rk Wald F-statistic	1182.13	1139.43	1218.67	1195.91
<i>Weak-instrument-robust inference</i>				
Anderson-Rubin Wald test p-value	0.0000	0.0000	0.0000	0.0000
Year dummies	NO	YES	YES	YES
Region dummies	NO	NO	YES	YES
Sector dummies (90)	NO	NO	NO	YES
Mean of dependent variable	0.037	0.037	0.037	0.037
SD of dependent variable	0.189	0.189	0.189	0.189
Median peer group size	23	23	23	23
Observations	11,008,833	11,008,833	11,008,833	11,008,833

Notes: Each column reports first- and second-stage estimates from the 2SLS regression. The dependent variable, *Child*, is a dummy that takes the value 1 for individuals who conceived a child. The treatment variable, *Peer Fertility Rate*, measures the average fertility rate among co-workers in the same workplace, occupation, and age group. The instrumental variable, *Fraction Jobs Act Peers*, measures the fraction of co-workers in the same workplace, occupation and age group, hired after the Jobs Act. Standard errors in parentheses are clustered at the firm level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Sources: INPS Archives and *Assegno Unico e Universale* records.

The estimated peer effect remains stable and statistically significant across columns 2-4 as year, region, and sector fixed effects are progressively included, with a magnitude of approximately 0.40 percentage points. In our preferred specification in column 4, this corresponds to an increase of approximately 11% relative to the sample mean. This result indicates that, on average, positive social influences from co-workers prevail, and aligns closely with findings for Sweden and the Netherlands reported by Asphjell et al. (2013) and Buyukkececi et al. (2020).

6. Robustness Checks

In Table 2, we assess the robustness of our baseline 2SLS estimates along several dimensions. First, in column 1 we include age dummies instead of controlling for age quadratically to capture the non-linear and age-specific nature of fertility decisions. While the magnitude of the estimated peer effect shrinks to 0.2 percentage points (corresponding to a 6% increase relative to the mean), the sign and significance of the coefficient are consistent with our baseline result.

Table 2. Robustness Checks

	(1) Age FEs	(2) Firm FEs	(3) Firm size fixed over sample period	(4) Firm size change in [p25, p75] over sample period	(5) Peer fertility at year t-2	(6) Peer fertility at year t-3
<i>Second-stage regressions</i>						
Peer Fertility Rate	0.2120*** (0.0433)	0.3562*** (0.0355)	0.5321*** (0.1477)	0.3601*** (0.0635)	0.4049*** (0.0312)	0.3878*** (0.0359)
<i>First-stage regressions</i>						
Fraction Jobs Act Peers	-0.0114*** (0.0005)	-0.0174*** (0.0006)	-0.0187*** (0.0027)	-0.0195*** (0.0012)	-0.0189*** (0.0006)	-0.0212*** (0.0008)
<i>Weak identification test</i>						
Kleibergen-Paap rk Wald F-stat	585.41	793.35	46.08	270.51	882.20	622.12
<i>Weak-instrument-robust inference</i>						
Anderson-Rubin Wald test p-val	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Mean of dependent variable	0.037	0.037	0.037	0.035	0.035	0.033
SD of dependent variable	0.188	0.188	0.189	0.183	0.184	0.177
Median peer group size	23	23	10	21	23	25
Observations	11,008,833	11,008,833	224,666	1,836,445	7,534,282	4,839,823

Notes: Each column reports first- and second-stage estimates from the 2SLS regression. The dependent variable, *Child*, is a dummy that takes the value 1 for individuals who conceived a child. The treatment variable, *Peer Fertility Rate*, measures the average fertility rate among co-workers in the same workplace, occupation, and age group. The instrumental variable, *Fraction Jobs Act Peers*, measures the fraction of co-workers in the same workplace, occupation and age group, hired after the Jobs Act. Each specification includes individual controls, peer group controls and year, region, and sector dummies as in Table 1, column 4. Standard errors in parentheses are clustered at the firm level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Sources: INPS Archives and *Assegno Unico e Universale* records.

Second, in column 2 we augment our baseline specification with the inclusion of firm fixed effects so as to account for unobserved firm-level heterogeneity. Reassuringly, the main estimate remains positive and strongly significant, although somewhat smaller in magnitude.

Third, in column 3 we replicate our baseline analysis in a sample of firms that did not change their size during the period of analysis. The concern is that the workers in firms with larger shares of newly hired colleagues might interpret this as a signal that the firm is expanding or is favoring more flexible hires, which might directly induce them to prioritize career opportunities over family formation, regardless of peer fertility. By keeping firm size fixed, we reduce the scope for the potential path from exposure to newly hired colleagues to individual fertility via those unmeasured confounding factors. The estimate in column 3 is still strongly significant, and becomes larger in magnitude, possibly reflecting the much smaller and more selected sample. To ensure that this latter result is not driven by a few firms, we run this test on a sample of firms that experienced just a minor change in size over the period under scrutiny, i.e., those with a change in size between the 25th and the 75th percentile. This allows us to have a much larger sample size compared to column 3. Notably, the estimate in column 4 remains significant and positive, but with a smaller magnitude than in column 3.

Finally, in columns 5 and 6, we check whether our baseline results change when peer fertility is measured two years prior (at year $t-2$ rather than at $t-1$), which implies losing the first year of observation, or three years prior (at year $t-3$), which implies losing the first two years of observation. The results obtained from these two exercises are remarkably similar to our results in Table 1, column 4. Although our 2016–2020 observation window prevents us from examining longer lags with adequate and comparable outcome coverage, the estimates in columns 5–6 suggest that the results are not confined to the one-year lag used in the baseline specification and remain visible through $t-3$. However, because peer fertility and exposure to Jobs Act hires are persistent over time, these estimates cannot pin down the duration of peer influence. While this persistence might be consistent with longer-lasting social learning about career costs, we interpret this possibility cautiously, as our estimates do not allow us to distinguish this mechanism from social contagion or changes in perceived workplace norms.

In Table A.5 in the Appendix, we also show that our baseline results are robust to alternative thresholds to define large firms, namely 20 or 50 employees (columns 1 and 2, respectively). Furthermore, in columns 3–5 we re-run the analysis excluding firms with more than one establishment (where large firm size could mask several smaller establishments) and progressively reducing their size. We also find that the magnitude of the effect increases when moving from relatively larger (≤ 250 employees) to relatively smaller firms (≤ 50 employees), in line with the idea that social interactions tend to be more salient in smaller firms.

We further assess the robustness of our baseline results to alternative definitions of the peer group. In our baseline analysis, the peer group consists of employees in the same workplace, age group, and occupation. Here we introduce three alternative definitions: the first includes employees from the same workplace and age group, regardless of occupation; the second includes co-workers in the same workplace and occupation but in the immediately older age group, as they may represent a near-future reference group for the focal worker; and the third includes co-workers in the same workplace, occupation, age group, and gender. The estimates reported in Appendix Table A.6 indicate that our baseline findings do not hinge on the way we define the peer group. In particular, under the second definition, a one-percentage-point increase in fertility among co-workers

in the immediately older age group is associated with an approximately 0.2-percentage-point increase in individual fertility, suggesting that peer effects may extend beyond the five-year age groups used in our baseline specification.

We also consider the following alternative instruments: (i) a dummy equal to one if the share of peers hired under the Jobs Act exceeds the 75th percentile; and (ii) two dummy variables indicating medium exposure (share between the 25th and 75th percentiles) and high exposure (share above the 75th percentile) to Jobs Act peers. The results, reported in Appendix Table A.7, show that our findings remain qualitatively unchanged.

Finally, column 3 of Appendix Table A.7 examines whether the response to peer fertility is nonlinear and exhibits threshold behavior. Because the distribution of peer fertility is strongly right-skewed — the median is zero and the 90th percentile is approximately 10% — we divide exposure into three mutually exclusive categories. Low exposure corresponds to zero peer fertility; medium exposure to peer fertility above zero and at or below the 90th percentile; and high exposure to peer fertility above the 90th percentile. The low-exposure category is omitted. We treat the medium- and high-exposure indicators as endogenous and instrument them with the corresponding indicators for medium and high exposure to co-workers hired under the Jobs Act. The estimates are consistent with a threshold response. The coefficient on medium exposure is not statistically distinguishable from zero, whereas the coefficient on high exposure is positive and statistically significant. Relative to the omitted category of zero peer fertility, the 2SLS coefficient implies that exposure to a peer-fertility rate above the 90th percentile (i.e., above 10%) increases the probability of conception by approximately 6 percentage points in the following year. This magnitude is large relative to the full-sample annual conception rate of 3.7% and should be interpreted as an IV estimate specific to the high-exposure margin rather than as an effect applying to the average worker. Although the results indicate that the response is concentrated at high levels of peer fertility, the grouped specification does not identify the precise threshold at which peer influence becomes salient.

6.1. Placebo Test

One concern with our estimation strategy is that our instrument might be capturing unobservable firm characteristics that affect the fertility rates of newly hired and incumbent workers alike. To investigate this issue, in Table 3 we focus on firms with multiple workplaces (about 1 million workers in our sample are employed in such firms) and conduct a placebo test using workers in other plants of the same firm to construct an artificial, fake peer fertility rate. The idea is that workers' fertility decisions should not respond to changes in the fertility rate of peers of the same firm with whom they do not actually interact, i.e. those in other workplaces.

In Table 3, column 1, we check whether the impact of peer fertility on individual fertility is driven by the average fertility rate in other workplaces of the same firm. Importantly, the coefficient of the average fertility rate of workers in other plants of the same firm is now much smaller (almost close to zero) and indeed

has no significant effect on the individual probability of having a child. This finding is inconsistent with explanations based on firm-wide shocks and reassures us that our main effect does not represent a spurious correlation. In addition, to ensure that the sample of multi-plant firms is not driving our results, in column 2 of Table 3 we replicate our main regression using the placebo sample (but with the actual peers) and find comparable results to those in Table 1, column 4.

Table 3. Placebo Test: Using Peer Fertility Rate in Other Establishments of the Same Firm

	(1) Placebo test	(2) Main effect in the placebo sample
<i>Second-stage regressions</i>		
Peer Fertility Rate	0.0645 (0.0415)	0.3449*** (0.0537)
<i>First-stage regressions</i>		
Fraction Jobs Act Peers	-0.0171*** (0.0020)	-0.0190*** (0.0013)
<i>Weak identification test</i>		
Kleibergen-Paap rk Wald F-statistic	76.17	227.12
<i>Weak-instrument-robust inference</i>		
Anderson-Rubin Wald test p-value	0.0000	0.0000
Mean of dependent variable	0.036	0.036
SD of dependent variable	0.186	0.187
Median peer group size	29	32
Observations	2,720,667	2,679,680

Notes: Each column reports first- and second-stage estimates from the 2SLS regression. The dependent variable, *Child*, is a dummy that takes the value 1 for individuals who conceived a child. In column 1, the treatment variable, *Peer Fertility Rate*, measures the average fertility rate among co-workers in the same occupation and age group but in a different workplace of the same firm, while the instrumental variable, *Fraction Jobs Act Peers*, measures the fraction of co-workers in the same occupation and age group but in a different workplace of the same firm, hired after the Jobs Act. In column 2, the treatment and instrumental variables are measured as described in Table 1. Each specification includes individual controls, peer group controls, and year, region, and sector dummies as in Table 1, column 4. Standard errors in parentheses are clustered at the firm level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Sources: INPS Archives and *Assegno Unico e Universale* records.

6.2. Alternative Instrumental Variable

Another potential concern with our estimation strategy, which might not be alleviated by the placebo test, is that exposure to co-workers hired after the Jobs Act (as captured by our instrumental variable) may affect incumbent workers' fertility through channels that are unrelated to co-workers' fertility choices. For example, working alongside colleagues with weaker employment protection could heighten the salience of job insecurity, alter perceptions of future career prospects or internal competition, or signal changes in the firm's workforce strategy. Any such direct effect on incumbent workers' fertility would violate the exclusion restriction.

In Table 4, we address this concern by using an alternative instrument that relies on differences in access to childcare services among co-workers. Specifically, using municipality-level data on public childcare provision from ISTAT,¹⁷ we exploit variation in the share of peers residing in municipalities where public

¹⁷ *Indagine su nidi e servizi integrativi per la prima infanzia 2019-2020*.

childcare services are available. Access to these services may reduce the expected costs of childbearing and thereby affect peers' fertility, while being less directly connected to employment protection or perceptions of job security within the firm. Encouragingly, the results obtained using this alternative instrument closely mirror our main findings, even when firm fixed-effects are included in column 5. The convergence of results across instruments based on different sources of variation supports interpreting the baseline estimates as workplace peer effects.

Table 4. Additional Robustness Check: Using IV Based on Peers' Childcare Exposure

	(1)	(2)	(3)	(4)	(5)
<i>Second-stage regressions</i>					
Peer Fertility Rate	0.5220*** (0.0353)	0.4735*** (0.0346)	0.4721*** (0.0336)	0.4683*** (0.0338)	0.4400*** (0.0461)
<i>First-stage regressions</i>					
Fraction Peers with Childcare	0.0089*** (0.0003)	0.0099*** (0.0004)	0.0105*** (0.0004)	0.0104*** (0.0004)	0.0113*** (0.0005)
<i>Weak identification test</i>					
Kleibergen-Paap rk Wald F-statistic	710.70	595.91	692.61	719.16	460.38
<i>Weak-instrument-robust inference</i>					
Anderson-Rubin Wald test p-value	0.0000	0.0000	0.0000	0.0000	0.0000
Year dummies	NO	YES	YES	YES	YES
Region dummies	NO	NO	YES	YES	NO
Sector dummies (90)	NO	NO	NO	YES	NO
Firm dummies	NO	NO	NO	NO	YES
Mean of dependent variable	0.037	0.037	0.037	0.037	0.037
SD of dependent variable	0.189	0.189	0.189	0.188	0.189
Median peer group size	23	23	23	23	23
Observations	10,845,350	10,845,350	10,845,350	10,845,350	10,845,350

Notes: Each column reports first- and second-stage estimates from the 2SLS regression. The dependent variable, *Child*, is a dummy that takes the value 1 for individuals who conceived a child. The treatment variable, *Peer Fertility Rate*, measures the average fertility rate among co-workers in the same workplace, occupation, and age group. The instrumental variable, *Fraction Peers with Childcare*, measures the fraction of co-workers residing in municipalities covered by childcare services. Standard errors in parentheses are clustered at the firm level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Sources: INPS Archives and *Assegno Unico e Universale* records.

7. Heterogeneous Responses

This section examines whether the estimated response to peer fertility varies across workers, stages of the reproductive and working life course, peer-group composition, and local and firm-level environments. As discussed in Section 2, these analyses do not separately identify social contagion, social norms, or social learning. The same empirical pattern may be generated by several mechanisms operating simultaneously, and the characteristics considered below may also be correlated with other observed or unobserved determinants of fertility. We therefore interpret the results as documenting heterogeneity in the net peer effect and as assessing whether particular patterns are compatible with the mechanisms described in the conceptual framework.

We begin with gender differences. Columns 1 and 2 of Table 5 show that a one-percentage-point increase in peer fertility is associated with a 0.32-percentage-point increase in women's probability of having a child and a 0.49-percentage-point increase in men's probability. Relative to the respective outcome means,

these estimates correspond to increases of approximately 7% for women and 15% for men. The difference is smaller in absolute terms: the coefficient for women is approximately two thirds of that for men, while the larger proportional difference partly reflects women’s higher baseline fertility rate in our sample.

Table 5. Heterogeneous Responses: by Gender

	(1) Female	(2) Male
<i>Second-stage regressions</i>		
Peer Fertility Rate	0.3235*** (0.0372)	0.4882*** (0.0398)
<i>First-stage regressions</i>		
Fraction Jobs Act Peers	-0.0235*** (0.0007)	-0.0148*** (0.0006)
<i>Weak identification test</i>		
Kleibergen-Paap rk Wald F-statistic	995.97	674.27
<i>Weak-instrument-robust inference</i>		
Anderson-Rubin Wald test p-value	0.0000	0.0000
Mean of dependent variable	0.044	0.033
SD of dependent variable	0.205	0.180
Observations	3,583,868	7,424,965

Notes: Each column reports first- and second-stage estimates from the 2SLS regression. The dependent variable, *Child*, is a dummy that takes the value 1 for individuals who conceived a child. The treatment variable, *Peer Fertility Rate*, measures the average fertility rate among co-workers in the same workplace, occupation, and age group. The instrumental variable, *Fraction Jobs Act Peers*, measures the fraction of co-workers in the same workplace, occupation and age group, hired after the Jobs Act. Each specification includes individual controls, peer group controls and year, region, and sector dummies as in Table 1, column 4. Standard errors in parentheses are clustered at the firm level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Sources: INPS Archives and *Assegno Unico e Universale* records.

The conceptual framework in Section 2 does not yield an unambiguous prediction about this gender difference. On the one hand, the greater economic, time, and career costs of childbearing borne by women may make it less likely that a given positive influence from co-workers translates into an observed birth (Adda et al., 2017; Kleven et al., 2019; Doepke and Kindermann, 2019). On the other hand, the higher stakes of childbearing and women’s shorter reproductive horizon may make information from peers more relevant, potentially strengthening their response. The smaller estimate for women is therefore compatible with gender differences in the expected costs of parenthood, but it does not establish that these costs are the mechanism responsible for the observed pattern.¹⁸

We next examine whether the response varies over the reproductive life course. In Table 6, we divide female and male workers into three age groups defined relative to the gender-specific mean age at first parenthood in Italy during 2016–2020: 31 years for women and 35 years for men. Column 1 examines women aged 31 or younger and men aged 35 or younger. Column 2 considers the immediately older age group (women

¹⁸ The smaller 2SLS estimate for women can be understood by examining the first-stage and reduced-form estimates. Although the reduced-form estimates are approximately -0.007 for both women and men, as shown in Table 5, the first-stage coefficient is larger in absolute value for women than for men (-0.0235 versus -0.0148), indicating that exposure to co-workers hired under the Jobs Act produces a larger reduction in peer fertility in the female sample. As a result, the similar reduced-form effects are scaled by a larger first-stage coefficient for women, yielding a smaller 2SLS estimate. This comparison should nevertheless be interpreted cautiously, as gender-specific 2SLS estimates may place different weights on the responses of workers whose fertility is affected by the instrument.

aged 32–35 and men aged 36–40), while column 3 focuses on women older than 35 and men older than 40. Peer effects are positive and statistically significant among women and men in the youngest group. Among women in the intermediate group, the estimate remains positive but is smaller, whereas the corresponding estimate for men is close to zero and not statistically significant. For women older than 35, the estimate is not statistically distinguishable from zero. For men older than 40, however, the instrument has essentially no predictive power for peer fertility — the first-stage F-statistic is 0.37 — so the corresponding 2SLS estimate is not informative about the presence or absence of peer effects in this group.

Table 6. Heterogeneous Responses: by Gender and Age

	(1)	(2)	(3)
Panel A: Female			
	Aged up to 31	Aged 32-35	Aged above 35
<i>Second-stage regressions</i>			
Peer Fertility Rate	0.3713*** (0.0634)	0.2601*** (0.0781)	-0.1672 (0.1194)
<i>First-stage regressions</i>			
Fraction Jobs Act Peers	-0.0364*** (0.0016)	-0.0321*** (0.0018)	-0.0078*** (0.0008)
<i>Weak identification test</i>			
Kleibergen-Paap rk Wald F-statistic	523.31	314.26	96.85
<i>Weak-instrument-robust inference</i>			
Anderson-Rubin Wald test p-value	0.0000	0.0000	0.1386
Mean of dependent variable	0.086	0.096	0.025
SD of dependent variable	0.281	0.294	0.155
Observations	457,007	572,440	2,554,421
Panel B: Male			
	Aged up to 35	Aged 36-40	Aged above 40
<i>Second-stage regressions</i>			
Peer Fertility Rate	0.3393*** (0.0662)	-0.0402 (0.2246)	-4.1077 (7.1777)
<i>First-stage regressions</i>			
Fraction Jobs Act Peers	-0.0180*** (0.0009)	-0.0069*** (0.0012)	-0.0003 (0.0005)
<i>Weak identification test</i>			
Kleibergen-Paap rk Wald F-statistic	364.88	30.64	0.37
<i>Weak-instrument-robust inference</i>			
Anderson-Rubin Wald test p-value	0.0000	0.8567	0.0077
Mean of dependent variable	0.070	0.062	0.014
SD of dependent variable	0.255	0.241	0.116
Observations	1,486,710	1,318,293	4,619,962

Notes: Each column reports first- and second-stage estimates from the 2SLS regression. The dependent variable, *Child*, is a dummy that takes the value 1 for individuals who conceived a child. The treatment variable, *Peer Fertility Rate*, measures the average fertility rate among co-workers in the same workplace, occupation, and age group. The instrumental variable, *Fraction Jobs Act Peers*, measures the fraction of co-workers in the same workplace, occupation and age group, hired after the Jobs Act. Columns 1–3 divide workers into three age groups defined relative to the gender-specific mean age at first parenthood in Italy during 2016–2020, which was 31 years for women and 35 years for men. Column 1 includes women aged 31 or younger and men aged 35 or younger. Column 2 includes the immediately older age group—women aged 32–35 and men aged 36–40. Column 3 includes women older than 35 and men older than 40. Each specification includes individual controls, peer group controls and year, region, and sector dummies as in Table 1, column 4. Standard errors in parentheses are clustered at the firm level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Sources: INPS Archives and *Assegno Unico e Universale* records.

The age-related pattern may reflect several, non-mutually exclusive factors. Age is associated with biological fecundity, parity, partnership circumstances, tenure, career stage, and the remaining reproductive horizon. The stronger estimated response among younger workers may indicate that peer fertility is particularly salient while family-formation decisions are still being made, whereas older workers may already have reached their desired parity or have less scope for adjusting the timing of fertility. Our outcome, however, records whether a conception resulting in a live birth occurs in a given year rather than completed fertility. A positive effect among younger workers may therefore represent an acceleration of a birth that would otherwise have occurred later, without changing eventual family size. At older reproductive ages, a change in timing could be more consequential for completed fertility because the scope for further postponement is more limited. Given the relatively short observation period, we cannot distinguish conclusively between these tempo and quantum responses.

The age-specific estimates further qualify the pooled gender comparison observed in Table 5. The coefficients in column 1 of Table 6 indicate that, within the youngest group, the estimated peer effects are similar for women and men (0.37 for women aged 31 or younger and 0.34 for men aged 35 or younger) and their difference is not statistically significant ($p=0.727$). For the intermediate age groups in column 2, the point estimates diverge (0.26 for women and -0.04 for men), but their difference is also not statistically significant ($p=0.207$). No informative comparison is possible for the oldest groups in column 3. Taken together, the age-specific estimates provide no evidence of statistically significant gender differences in peer effects, although they reveal substantial heterogeneity over the reproductive life course. This suggests that the pooled coefficients in Table 5 combine substantial age-related heterogeneity with differences in the age composition of the female and male samples.¹⁹

Table 7 explores heterogeneity by birth order. The estimates are positive for first births but become substantially smaller for higher-order births and, among women, turn negative. One possible interpretation is that information from co-workers is particularly valuable before the transition to parenthood, when uncertainty about the consequences of having a child is greatest (Lyngstad and Prskawetz, 2010). Once workers have become parents, their own experience may become a more important source of information. For women in particular, observing colleagues' experiences may also make the time and career costs of an additional child more salient. Workplace substitution constraints could provide another explanation if overlapping parental leaves increase workloads or affect career opportunities. These mechanisms cannot be distinguished with our data, however, and the birth-order pattern could also reflect differences in age, desired family size, or the timing of subsequent births.

We further investigate whether the gender of the observed peers matters. Appendix Table A.8 estimates the effects of female and male peer fertility separately. Both female and male workers exhibit larger estimated responses to male peers' fertility than to female peers' fertility. This pattern is compatible with the possibility that the costs of parenthood are more visible when female colleagues have children, thereby

¹⁹ Because the gender-specific groups cover different absolute age ranges and we do not conduct a formal decomposition or reweighting exercise, these results do not establish how much of the apparent pooled gender difference reflects differences in age composition rather than gender-specific responsiveness.

moderating an otherwise positive response. Male colleagues' transitions to parenthood may instead reveal fewer workplace costs. Other explanations are also possible, including gender differences in workplace interaction and occupational sorting.

Table 7. Heterogeneous Responses: by Gender and Birth Order

	(1) Female N. pre-existing children=0	(2) N. pre-existing children>=1	(3) Male N. pre-existing children=0	(4) N. pre-existing children>=1
<i>Second-stage regressions</i>				
Peer Fertility Rate	0.2075*** (0.0375)	-0.4376*** (0.1044)	0.8791*** (0.0732)	-0.0854 (0.0533)
<i>First-stage regressions</i>				
Fraction Jobs Act Peers	-0.0204*** (0.0007)	-0.0404*** (0.0016)	-0.0088*** (0.0005)	-0.0245*** (0.0009)
<i>Weak identification test</i>				
Kleibergen-Paap rk Wald F-statistic	794.08	610.90	268.93	749.23
<i>Weak-instrument-robust inference</i>				
Anderson-Rubin Wald test p-value	0.0000	0.0000	0.0000	0.11
Mean of dependent variable	0.028	0.150	0.023	0.060
SD of dependent variable	0.166	0.356	0.149	0.237
Observations	3,126,891	456,977	5,250,641	2,174,324

Notes: Each column reports first- and second-stage estimates from the 2SLS regression. The dependent variable, *Child*, is a dummy that takes the value 1 for individuals who conceived a child. The treatment variable, *Peer Fertility Rate*, measures the average fertility rate among co-workers in the same workplace, occupation, and age group. The instrumental variable, *Fraction Jobs Act Peers*, measures the fraction of co-workers in the same workplace, occupation and age group, hired after the Jobs Act. Each specification includes individual controls, peer group controls and year, region, and sector dummies as in Table 1, column 4. Standard errors in parentheses are clustered at the firm level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Sources: INPS Archives and *Assegno Unico e Universale* records.

Appendix Table A.9 provides complementary evidence by examining the gender composition of the peer group. Peer effects among women decline as the share of female co-workers increases, and a similar attenuation is observed among men in more female-dominated peer groups. These patterns are consistent with the possibility that information about the costs of combining employment and motherhood moderates responses to peer fertility, although differences in social proximity, occupational composition, or workplace organization may also contribute to the observed heterogeneity.

We next assess heterogeneity across local gender-role environments in Table 8. We use two geographically disaggregated indicators constructed at the municipal level: the share of women among municipal council members (panel A) and the take-up rate of paternity leave in 2018 (panel B). Greater female political representation and higher paternity-leave take-up are interpreted as indicators of less traditional local gender roles. Both measures reveal substantial variation within broader geographical areas.

The estimates across local contexts indicate that workplace peer effects tend to be weaker in municipalities characterized by more traditional gender roles for both women and men. This finding is consistent with the alternative argument discussed in Section 2, whereby fertility decisions in more traditional contexts may be more strongly influenced by partners, families, or local communities, reducing the relative importance of workplace colleagues as a reference group. It does not, however, identify social norms as the

causal mechanism. Paternity-leave take-up and women's political representation may also capture differences in childcare provision, female labor-market opportunities, income, political participation, and other local economic and institutional conditions. The results should therefore be interpreted as descriptive heterogeneity across local contexts rather than as causal effects of gender norms.

Table 8. Heterogeneous Responses: by Gender and Type of Gender Norms

	(1)	(2)	(3)	(4)
	Female		Male	
	More traditional gender norms	Less traditional gender norms	More traditional gender norms	Less traditional gender norms
Panel A: Share of women among municipal council members	Share of women <=median	Share of women >median	Share of women <=median	Share of women >median
<i>Second-stage regressions</i>				
Peer Fertility Rate	0.2856*** (0.0544)	0.3514*** (0.0477)	0.4369*** (0.0661)	0.5107*** (0.0471)
<i>First-stage regressions</i>				
Fraction Jobs Act Peers	-0.0243*** (0.0010)	-0.0228*** (0.0008)	-0.0132*** (0.0007)	-0.0158*** (0.0006)
<i>Weak identification test</i>				
Kleibergen-Paap rk Wald F-statistic	634.72	825.60	325.66	657.21
<i>Weak-instrument-robust inference</i>				
Anderson-Rubin Wald test p-value	0.0000	0.0000	0.0000	0.0000
Mean of dependent variable	0.045	0.043	0.034	0.033
SD of dependent variable	0.208	0.202	0.181	0.180
Observations	1,526,238	2,057,630	3,257,222	4,167,743
Panel B: Paternity leave take-up rates in 2018	Paternity leave take-up rate <=median	Paternity leave take-up rate >median	Paternity leave take-up rate <=median	Paternity leave take-up rate >median
<i>Second-stage regressions</i>				
Peer Fertility Rate	0.3019*** (0.0556)	0.3393*** (0.0470)	0.4560*** (0.0583)	0.5036*** (0.0522)
<i>First-stage regressions</i>				
Fraction Jobs Act Peers	-0.0225*** (0.0010)	-0.0243*** (0.0008)	-0.0143*** (0.0007)	-0.0151*** (0.0006)
<i>Weak identification test</i>				
Kleibergen-Paap rk Wald F-statistic	545.19	822.83	362.02	554.81
<i>Weak-instrument-robust inference</i>				
Anderson-Rubin Wald test p-value	0.0000	0.0000	0.0000	0.0000
Mean of dependent variable	0.045	0.043	0.034	0.033
SD of dependent variable	0.206	0.203	0.181	0.179
Observations	1,609,121	1,974,747	3,548,036	3,876,929

Notes: Each column reports first- and second-stage estimates from the 2SLS regression. The dependent variable, *Child*, is a dummy that takes the value 1 for individuals who conceived a child. The treatment variable, *Peer Fertility Rate*, measures the average fertility rate among co-workers in the same workplace, occupation, and age group. The instrumental variable, *Fraction Jobs Act Peers*, measures the fraction of co-workers in the same workplace, occupation and age group, hired after the Jobs Act. Municipalities with more traditional gender norms are those with below-median share of women among municipal council members in panel A and those with below-median paternity leave take-up rates in panel B. Each specification includes individual controls, peer group controls and year, region, and sector dummies as in Table 1, column 4. Standard errors in parentheses are clustered at the firm level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Sources: INPS Archive, *Assegno Unico e Universale* records.

Appendix Table A.10 compares native and immigrant workers and examines whether the response among immigrants varies with the immigrant share of the peer group. The estimated effect is statistically

significant for both groups and is larger relative to the outcome mean among immigrants than among natives (approximately 12% versus 10%).²⁰ Among immigrants, the effect is concentrated in peer groups with a relatively high immigrant share. This pattern is compatible with social proximity or a shared background increasing the salience of peer behaviour. Immigrant status, however, encompasses highly heterogeneous countries of origin and is correlated with occupation, earnings, employment security, family networks, and other characteristics. We therefore do not interpret this evidence as a direct test of traditional gender norms.

Finally, we examine heterogeneity across working-life and firm characteristics. Table 9 reports estimates by tenure. Peer effects are stronger among workers with longer tenure, for both women and men. Longer tenure may reduce uncertainty about continued employment and career progression, leaving greater scope for social influence to translate into fertility behaviour. It may also capture stronger workplace attachment and more sustained interaction with co-workers. At the same time, tenure is correlated with age, earnings, occupation, and selection into longer employment relationships, so the results cannot isolate employment security or career concerns as the operative channel.

Table 9. Heterogeneous Responses: by Gender and Tenure

	(1) Female Tenure <=median	(2) Tenure >median	(3) Male Tenure <=median	(4) Tenure >median
<i>Second-stage regressions</i>				
Peer Fertility Rate	0.3229*** (0.0406)	0.5952*** (0.0657)	0.4757*** (0.0465)	0.5848*** (0.0603)
<i>First-stage regressions</i>				
Fraction Jobs Act Peers	-0.0266*** (0.0009)	-0.0219*** (0.0010)	-0.0153*** (0.0006)	-0.0174*** (0.0010)
<i>Weak identification test</i>				
Kleibergen-Paap rk Wald F-statistic	909.69	476.61	637.44	310.23
<i>Weak-instrument-robust inference</i>				
Anderson-Rubin Wald test p-value	0.0000	0.0000	0.0000	0.0000
Mean of dependent variable	0.053	0.029	0.039	0.026
SD of dependent variable	0.224	0.163	0.194	0.160
Observations	2,193,761	1,390,107	4,152,264	3,272,701

Notes: Each column reports first- and second-stage estimates from the 2SLS regression. The dependent variable, *Child*, is a dummy that takes the value 1 for individuals who conceived a child. The treatment variable, *Peer Fertility Rate*, measures the average fertility rate among co-workers in the same workplace, occupation, and age group. The instrumental variable, *Fraction Jobs Act Peers*, measures the fraction of co-workers in the same workplace, occupation and age group, hired after the Jobs Act. Each specification includes individual controls, peer group controls and year, region, and sector dummies as in Table 1, column 4. Standard errors in parentheses are clustered at the firm level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Sources: INPS Archives and *Assegno Unico e Universale* records.

Table A.11 in the Appendix examines the role of firm-level competition using the dispersion of wage growth within the firm. Peer effects are weaker in firms with greater wage-growth dispersion, for both women and men. This pattern is compatible with the conceptual framework: when internal competition or uncertainty about career progression raises the expected professional cost of childbearing, workers may be less responsive

²⁰ According to ISTAT, the largest foreign communities in Italy are those from Romania (21.5% of the foreign population), Morocco (8.4%) and Albania (8.3%).

to positive influences from their peers. Wage-growth dispersion may nevertheless capture other aspects of firm heterogeneity, personnel policies, or firm-level shocks. Consequently, this analysis provides suggestive evidence that workplace conditions moderate peer responses, rather than a causal test of career concerns.

Taken together, the results show that the estimated response to peer fertility varies systematically across gender, age, parity, peer-group composition, local context, tenure, and workplace competition. The patterns are generally compatible with social influence being conditioned by the expected costs of childbearing and by the relevance of co-workers as a reference group. They do not, however, allow us to determine which of the mechanisms described in Section 2 accounts for any particular dimension of heterogeneity.

8. Conclusions

This paper examines whether fertility among co-workers affects individual childbearing decisions. Using matched employer–employee data from the Italian Social Security Institute (INPS) for 2016-2020 and an instrumental variable strategy, we provide evidence that co-workers’ fertility significantly affects individual fertility behavior, an area that has received limited attention in the peer effects literature.

Our identification exploits variation in workplace fertility generated by the 2015 Jobs Act, which reduced dismissal protection - and thus fertility - among new hires on open-ended contracts in firms with more than 15 employees. We focus on incumbent workers in firms above the statutory size threshold hired before the reform, whose employment protection remained unchanged, and instrument peer fertility with the share of their co-workers hired after the reform. Our 2SLS estimates imply that a one-percentage-point increase in co-workers’ fertility in year $t-1$ increases an individual’s probability of conceiving a child in year t by 0.2 to 0.4 percentage points, or about 6 to 11% relative to the sample mean.

Our results are robust to a wide range of checks, including alternative definitions of peer groups, the use of different samples based on firm size, restricting the sample to firms with relatively constant size over the sample period, and employing an alternative instrumental variable based on peers’ access to municipal childcare. In addition, a placebo test based on workers in other establishments of the same firm produces no effect, which is inconsistent with explanations based on firm-wide shocks.

The heterogeneity analysis documents substantial variation across workers and stages of the reproductive and working life course. First, estimated peer effects are somewhat similar for women and men, though they are smaller for women when female peers are the reference group. Second, peer effects are positive and statistically significant among younger women and men but are not statistically distinguishable from zero among older workers. Third, peer effects are positive for first births but not for higher-order births. These patterns are compatible with information about parenthood being particularly relevant before the first birth and with career-related costs conditioning workers’ responses to peer fertility. They do not, however, identify information sharing or any other specific mechanism as the source of the estimated effect. In addition, although the stronger peer effects observed among younger workers may reflect changes in the timing of births, our analysis does not allow us to distinguish conclusively between tempo and quantum effects.

Additional heterogeneity is observed across local and workplace contexts. Women are less responsive to peer fertility in municipalities characterized by more traditional gender-role norms, whereas no comparable pattern emerges for men. This finding is compatible with the workplace being a less salient reference group where fertility decisions are shaped more strongly by partners, families, or local communities. Yet municipalities also differ along many other social and economic dimensions, so this pattern should not be interpreted as a causal test of social norms. Peer effects are also stronger among workers with longer tenure and in less competitive firms. These results are compatible with employment uncertainty and internal labor-market competition increasing the expected costs of responding to peer fertility, but they are not uniquely attributable to career concerns.

Taken together, our evidence places the workplace alongside families, friends, and neighbourhoods as a social context for fertility decisions. More specifically, our findings indicate that fertility decisions may be interdependent among co-workers who are similar in age and occupational status. Conditional on our identifying assumptions, policy-induced changes in the fertility of such co-workers may be reflected in the behaviour of incumbent workers who are not directly covered by the reform. This suggests that evaluations of labour-market reforms focusing only on directly affected workers may miss part of their demographic consequences. Importantly, our estimates do not establish whether these responses alter only the timing of births or also completed fertility, nor do they identify their implications for aggregate fertility. Future research combining longer fertility histories with direct measures of workplace networks, communication, and beliefs is needed to distinguish tempo from quantum effects and to identify the underlying mechanisms more sharply.

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APPENDIX

Table A.1. Descriptive Statistics

Variable	Mean	S.D.	Min	Max
<i>Outcome variable</i>				
Child	0.037	0.189	0	1
<i>Treatment variable</i>				
Peer Fertility Rate	0.035	0.070	0	1
<i>Instrumental variables</i>				
Fraction Jobs Act Peers (main)	0.167	0.208	0	1
Fraction Peers with Childcare (alternative)	0.676	0.267	0	1
<i>Controls</i>				
Female	0.325	0.469	0	1
Age (female workers)	38.547	5.525	16	46
Age (male workers)	42.131	7.336	16	56
Immigrant	0.107	0.310	0	1
Tenure	7.574	2.918	1	12
Experience	19.145	7.327	0	32
Part-Time	0.152	0.359	0	1
Blue-Collar	0.472	0.499	0	1
White-Collar	0.447	0.497	0	1
Manager	0.064	0.245	0	1
North	0.635	0.481	0	1
Centre	0.194	0.396	0	1
South	0.170	0.376	0	1

Notes: Sample: 11,008,833 observations. The sample includes female employees aged 16-46 and male employees aged 16-56, with permanent contracts in private-sector firms.

Sources: INPS Archives and *Assegno Unico e Universale* records.

Table A.2. First Stage Estimates. Peer Fertility and Fraction of Peers Hired under Jobs Act

	(1)	(2)	(3)	(4)
	Peer Fertility Rate			
Fraction Jobs Act Peers	-0.0142*** (0.00041)	-0.0189*** (0.00056)	-0.0191*** (0.00055)	-0.0188*** (0.00054)
Individual controls	YES	YES	YES	YES
Peer group controls	YES	YES	YES	YES
Year dummies	NO	YES	YES	YES
Region dummies	NO	NO	YES	YES
Sector dummies (90)	NO	NO	NO	YES
<i>Weak identification test</i>				
Kleibergen-Paap rk Wald F-statistic	1182.13	1139.43	1218.67	1195.91
<i>Weak-instrument-robust inference</i>				
Anderson-Rubin Wald test p-value	0.00000	0.00000	0.00000	0.00000
Observations	11,008,833	11,008,833	11,008,833	11,008,833

Notes: Each column reports estimates from OLS regression. The dependent variable, *Peer Fertility Rate*, measures the average fertility rate among co-workers in the same workplace, occupation, and age group. The instrumental variable, *Fraction Jobs Act Peers*, measures the fraction of co-workers in the same workplace, occupation, and age group hired under the Jobs Act. Individual controls: Female, Age, Age Squared, Immigrant, Tenure, Experience, Part time. Peer group controls: % Females, % Immigrants, Avg. Tenure, Avg. Experience, % Part time. Standard errors in parentheses are clustered at the firm level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Sources: INPS Archives and *Assegno Unico e Universale* records.

Table A.3. Reduced-Form Estimates

	(1)	(2)	(3)	(4)
Fraction Jobs Act Peers	-0.0064*** (0.0005)	-0.0077*** (0.0006)	-0.0078*** (0.0006)	-0.0076*** (0.0006)
Individual controls	YES	YES	YES	YES
Peer Group controls	YES	YES	YES	YES
Year dummies	NO	YES	YES	YES
Region dummies	NO	NO	YES	YES
Sector dummies (90)	NO	NO	NO	YES
Observations	11,008,833	11,008,833	11,008,833	11,008,833

Notes: Each column reports estimates from the OLS regression. The dependent variable, *Child*, is a dummy that takes the value 1 for individuals who conceived a child. The variable *Fraction Jobs Act Peers* measures the fraction of co-workers in the same workplace, occupation and age group, hired after the Jobs Act. Individual controls: Female, Age, Age Squared, Immigrant, Tenure, Experience, Part time. Peer group controls: % Females, % Immigrants, Avg. Tenure, Avg. Experience, % Part time. Standard errors in parentheses are clustered at the firm level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.
Sources: INPS Archives and *Assegno Unico e Universale* records.

Table A.4. OLS Estimates

	(1)	(2)	(3)	(4)
Peer Fertility Rate	0.0461*** (0.0014)	0.0460*** (0.0014)	0.0448*** (0.0014)	0.0434*** (0.0013)
Individual controls	YES	YES	YES	YES
Peer Group controls	YES	YES	YES	YES
Year dummies	NO	YES	YES	YES
Region dummies	NO	NO	YES	YES
Sector dummies (90)	NO	NO	NO	YES
Observations	11,008,833	11,008,833	11,008,833	11,008,833

Notes: Each column reports estimates from the OLS regression. The dependent variable, *Child*, is a dummy that takes the value 1 for individuals who conceived a child. The variable *Peer Fertility Rate* measures the average fertility rate among co-workers in the same workplace, occupation, and age group. Individual controls: Female, Age, Age Squared, Immigrant, Tenure, Experience, Part time. Peer group controls: % Females, % Immigrants, Avg. Tenure, Avg. Experience, % Part time. Standard errors in parentheses are clustered at the firm level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.
Sources: INPS Archives and *Assegno Unico e Universale* records.

Table A.5. Robustness Checks: Alternative Firm Size Categories

	(1) Firm size ≥20	(2) Firm size ≥50	(3) Firm size ≤250	(4) Firm size ≤150	(5) Firm size ≤50
<i>Second-stage regressions</i>					
Peer Fertility Rate	0.4118*** (0.0287)	0.4233*** (0.0331)	0.3994*** (0.0334)	0.4077*** (0.0363)	0.4309*** (0.0407)
<i>First-stage regressions</i>					
Fraction Jobs Act Peers	-0.0191*** (0.0006)	-0.0207*** (0.0008)	-0.0179*** (0.0005)	-0.0176*** (0.0005)	-0.0171*** (0.0006)
<i>Weak identification test</i>					
Kleibergen-Paap rk Wald F-stat	1073.64	719.74	1293.57	1141.15	704.85
<i>Weak-instrument-robust inference</i>					
Anderson-Rubin Wald test p-val	0.0000	0.0000	0.0000	0.0000	0.0000
Mean of dependent variable	0.037	0.037	0.037	0.037	0.036
SD of dependent variable	0.189	0.189	0.188	0.188	0.187
Median peer group size	26	42	11	9	6
Observations	10,424,671	8,296,683	5,565,783	4,668,235	2,514,017

Notes: Each column reports first- and second-stage estimates from the 2SLS regression. The dependent variable, *Child*, is a dummy that takes the value 1 for individuals who conceived a child. The treatment variable, *Peer Fertility Rate*, measures the average fertility rate among co-workers in the same workplace, occupation, and age group. The instrumental variable, *Fraction Jobs Act Peers*, measures the fraction of co-workers in the same workplace, occupation and age group, hired after the Jobs Act. In columns 3-5 we consider only firms with one establishment. Each specification includes individual controls, peer group controls and year, region, and sector dummies as in Table 1, column 4. Standard errors in parentheses are clustered at the firm level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Sources: INPS Archives and *Assegno Unico e Universale* records.

Table A.6. Robustness Checks: Alternative Peer Group Definitions

	(1)	(2)	(3)
<i>Second-stage regressions</i>			
Peer Fertility Rate (same age group)	0.4197*** (0.0341)		
Peer Fertility Rate (same occupation, immediately older age group)		0.1916*** (0.0266)	
Peer Fertility Rate (same age group, occupation, and gender)			0.3264*** (0.0397)
<i>First-stage regressions</i>			
Fraction Jobs Act Peers	-0.0120*** (0.0004)	-0.0222*** (0.0009)	-0.0099*** (0.0004)
<i>Weak identification test</i>			
Kleibergen-Paap rk Wald F-stat	712.95	630.01	522.40
<i>Weak-instrument-robust inference</i>			
Anderson-Rubin Wald test p-val	0.0000	0.0000	0.0000
Mean of dependent variable	0.037	0.036	0.036
SD of dependent variable	0.189	0.186	0.188
Median peer group size	23	37	25
Observations	11,008,833	8,185,420	10,592,914

Notes: Each column reports first- and second-stage estimates from the 2SLS regression. The dependent variable, *Child*, is a dummy that takes the value 1 for individuals who conceived a child. The treatment variable, *Peer Fertility Rate*, measures the average fertility rate among co-workers in the same workplace and age group in column 1, in the same workplace, occupation and immediately older age group in column 2, and in the same workplace, age group, occupation, and gender in column 3. The instrumental variable, *Fraction Jobs Act Peers*, measures the fraction of co-workers in the same workplace and age group, hired after the Jobs Act in column 1, in the same workplace, occupation and immediately older age group in column 2, and in the same workplace, age group, occupation, and gender in column 3. Each specification includes individual controls, peer group controls and year, region, and sector dummies as in Table 1, column 4. Standard errors in parentheses are clustered at the firm level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Sources: INPS Archives and *Assegno Unico e Universale* records.

Table A.7. Robustness Checks: Nonlinearities

	(1)	(2)	(3)
<i>Second-stage regressions</i>			
Peer Fertility Rate	0.2581*** (0.0586)	0.2461*** (0.0409)	
Peer Fertility Rate above 90 th perc.			0.0628*** (0.0154)
Peer Fertility Rate between 50 th and 90 th perc.			-0.0023 (0.0015)
<i>First-stage regressions</i>			
Fraction Jobs Act Peers above 75 th perc.	-0.0008*** (0.0002)	-0.0014*** (0.0003)	-0.0370*** (0.0017)
Fraction Jobs Act Peers between 25 th and 75 th perc.		-0.0006*** (0.0002)	-0.0322*** (0.0013)
<i>Weak identification test</i>			
Kleibergen-Paap rk Wald F-stat	278.54	269.69	61.19
<i>Weak-instrument-robust inference</i>			
Anderson-Rubin Wald test p-val	0.0000	0.0000	0.0000
Mean of dependent variable	0.037	0.037	0.037
SD of dependent variable	0.189	0.189	0.189
Median peer group size	23	23	23
Observations	11,008,833	11,008,833	11,008,833

Notes: Each column reports first- and second-stage estimates from the 2SLS regression. The dependent variable, *Child*, is a dummy that takes the value 1 for individuals who conceived a child. The treatment variables, *Peer Fertility Rate*, measures the average fertility rate among co-workers in the same workplace, occupation, and age group in column 1 (our main definition). *Peer Fertility Rate above 90th perc.* and *Peer Fertility Rate between 50th and 75th perc.* are dummy variables equal to one if *Peer Fertility Rate* exceeds the 90th percentile or falls between the 50th and 75th percentiles, respectively, of the corresponding distribution. The instrumental variables, *Fraction Jobs Act Peers above 75th perc.* and *Fraction Jobs Act Peers between 25th and 75th perc.* are dummy variables equal to one if the fraction of co-workers in the same workplace, occupation, and age group who were hired after the Jobs Act exceeds the 75th percentile or falls between the 25th and 75th percentiles, respectively, of the corresponding distribution. Each specification includes individual controls, peer group controls and year, region, and sector dummies as in Table 1, column 4. Standard errors in parentheses are clustered at the firm level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Sources: INPS Archives and *Assegno Unico e Universale* records.

Table A.8. Heterogeneous Responses to Peers of Same and Opposite Gender

	(1) Female	(2) Male	(3) Female	(4) Male
<i>Second-stage regressions</i>				
Peer Fertility Rate (Female peers)	0.2597*** (0.0275)	0.2987*** (0.0329)		
Peer Fertility Rate (Male peers)			0.4088*** (0.1048)	0.5479*** (0.0542)
<i>First-stage regressions</i>				
Fraction Jobs Act Peers	-0.0291*** (0.0008)	-0.0195*** (0.0012)	-0.0090*** (0.0011)	-0.0106*** (0.0006)
<i>Weak identification test</i>				
Kleibergen-Paap rk Wald F-statistic	1337.37	268.28	63.14	319.23
<i>Weak-instrument-robust inference</i>				
Anderson-Rubin Wald test p-value	0.0012	0.0000	0.0000	0.0000
Mean of dependent variable	0.044	0.033	0.041	0.033
SD of dependent variable	0.205	0.179	0.199	0.179
Observations	3,332,142	3,922,425	2,890,585	7,260,772

Notes: Each column reports first- and second-stage estimates from the 2SLS regression. The dependent variable, *Child*, is a dummy that takes the value 1 for individuals who conceived a child. The treatment variable, *Peer Fertility Rate*, measures the average fertility rate among co-workers in the same workplace, occupation, and age group. The instrumental variable, *Fraction Jobs Act Peers*, measures the fraction of co-workers in the same workplace, occupation and age group, hired after the Jobs Act. Each specification includes individual controls, peer group controls and year, region, and sector dummies as in Table 1, column 4. Standard errors in parentheses are clustered at the firm level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Sources: INPS Archives and *Assegno Unico e Universale* records.

Table A.9. Heterogeneous Responses by Gender and Fraction of Female Peers

	(1) Female % Female peers ≤ median	(2) Female % Female peers > median	(3) Male % Female peers ≤ median	(4) Male % Female peers > median
<i>Second-stage regressions</i>				
Peer Fertility Rate	0.6290** (0.2009)	0.3130*** (0.0379)	0.6103*** (0.0784)	0.3710*** (0.0429)
<i>First-stage regressions</i>				
Fraction Jobs Act Peers	-0.0104*** (0.0011)	-0.0253*** (0.0008)	-0.0096*** (0.0007)	-0.0219*** (0.0008)
<i>Weak identification test</i>				
Kleibergen-Paap rk Wald F-statistic	82.75	928.39	168.66	754.14
<i>Weak-instrument-robust inference</i>				
Anderson-Rubin Wald test p-value	0.0012	0.0000	0.0000	0.0000
Mean of dependent variable	0.027	0.046	0.029	0.041
SD of dependent variable	0.163	0.210	0.168	0.199
Observations	475,793	3,108,075	4,612,200	2,812,765

Notes: Each column reports first- and second-stage estimates from the 2SLS regression. The dependent variable, *Child*, is a dummy that takes the value 1 for individuals who conceived a child. The treatment variable, *Peer Fertility Rate*, measures the average fertility rate among co-workers in the same workplace, occupation, and age group. The instrumental variable, *Fraction Jobs Act Peers*, measures the fraction of co-workers in the same workplace, occupation and age group, hired after the Jobs Act. Each specification includes individual controls, peer group controls and year, region, and sector dummies as in Table 1, column 4. Standard errors in parentheses are clustered at the firm level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Sources: INPS Archives and *Assegno Unico e Universale* records.

Table A.10. Heterogeneous Responses by Immigrant Status and Share of Immigrant Peers

	(1)	(2)	(3)	(4)
	Native	Immigrant	Immigrant % Immigrant peers ≤ median	Immigrant % Immigrant peers > median
Peer Fertility Rate	0.3490*** (0.0309)	0.5230*** (0.0628)	0.2891 (0.2020)	0.5770*** (0.0681)
<i>First-stage regressions</i>				
Fraction Jobs Act Peers	-0.0179*** (0.0006)	-0.0218*** (0.0010)	-0.0162*** (0.0016)	-0.0224*** (0.0011)
<i>Weak identification test</i>				
Kleibergen-Paap rk Wald F-statistic	972.13	516.15	95.93	409.88
<i>Weak-instrument-robust inference</i>				
Anderson-Rubin Wald test p-value	0.0000	0.0000	0.0000	0.0000
Mean of dependent variable	0.036	0.043	0.044	0.043
SD of dependent variable	0.187	0.203	0.205	0.202
Observations	9,825,607	1,183,226	172,421	1,010,805

Notes: Each column reports first- and second-stage estimates from the 2SLS regression. The dependent variable, *Child*, is a dummy that takes the value 1 for individuals who conceived a child. The treatment variable, *Peer Fertility Rate*, measures the average fertility rate among co-workers in the same workplace, occupation, and age group. The instrumental variable, *Fraction Jobs Act Peers*, measures the fraction of co-workers in the same workplace, occupation and age group, hired after the Jobs Act. Each specification includes individual controls, peer group controls and year, region, and sector dummies as in Table 1, column 4. Standard errors in parentheses are clustered at the firm level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Sources: INPS Archives and *Assegno Unico e Universale* records.

Table A.11. Heterogeneous Responses: by Gender and Competition Within Firm

	(1)	(2)	(3)	(4)
	Female Wage growth dispersion ≤ median	Female Wage growth dispersion > median	Male Wage growth dispersion ≤ median	Male Wage growth dispersion > median
<i>Second-stage regressions</i>				
Peer Fertility Rate	0.3904*** (0.0850)	0.3060*** (0.0420)	0.5188*** (0.0884)	0.4552*** (0.0466)
<i>First-stage regressions</i>				
Fraction Jobs Act Peers	-0.0222*** (0.0014)	-0.0235*** (0.0009)	-0.0127*** (0.0011)	-0.0148*** (0.0006)
<i>Weak identification test</i>				
Kleibergen-Paap rk Wald F-statistic	268.41	747.68	130.99	538.30
<i>Weak-instrument-robust inference</i>				
Anderson-Rubin Wald test p-value	0.0000	0.0000	0.0000	0.0000
Mean of dependent variable	0.040	0.045	0.031	0.036
SD of dependent variable	0.195	0.208	0.172	0.185
Observations	896,092	2,687,776	3,080,452	4,344,513

Notes: Each column reports first- and second-stage estimates from the 2SLS regression. The dependent variable, *Child*, is a dummy that takes the value 1 for individuals who conceived a child. The treatment variable, *Peer Fertility Rate*, measures the average fertility rate among co-workers in the same workplace, occupation, and age group. The instrumental variable, *Fraction Jobs Act Peers*, measures the fraction of co-workers in the same workplace, occupation and age group, hired after the Jobs Act. Each specification includes individual controls, peer group controls and year, region, and sector dummies as in Table 1, column 4. Standard errors in parentheses are clustered at the firm level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Sources: INPS Archives and *Assegno Unico e Universale* records.