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Businesses, buddies and babies:
social ties and fertility at work

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Businesses, buddies and babies:

social ties and fertility at work[♣]

by

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Abstract

We examine the influence that co-workers' have on each other's fertility decisions. Using linked employer-employee panel data for Sweden we show that female individual fertility increases if a co-worker recently had a child. The timing of births among co-workers of the same sex, educational level and co-workers who are close in age is even more influential. Consistent with models of social learning we find that the peer effect for first time mothers is similar irrespective of the birth order of the co-worker's child, while for higher order births within-parity peer effects are strong but cross-parity peer effects are entirely absent. A causal interpretation of our estimates is strengthened by several falsification tests showing that neither unobserved common shocks at the workplace level, nor sorting of workers between workplaces are likely to explain the observed peer effect. We also provide evidence suggesting that peers not only affect timing of births but potentially also completed fertility, and that fertility peer influences spills over across multiple networks. Our results forward the understandings of how individual fertility timing decisions are made and suggest that social interactions could be an important factor behind the strong inter-temporal fluctuations in total fertility rates observed in many countries.

Keywords: Peer effects, social preferences, co-workers
JEL-codes: J13

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1 Introduction

Through its effect on work experience and career choices, childbearing represent one of the most prevalent and costly types of career interruptions for women (Mincer and Polacheck, 1974, Albrecht et al., 1999, Bertrand, Goldin and Katz, 2010). When deciding about the timing of childbearing women face a trade-off: while delayed motherhood increases life time earnings, postponement is also associated with higher risks of childlessness and adverse health outcomes for mothers and children (Mincer and Ofek, 1982; Royer 2005; Miller 2006). To solve this non-trivial intertemporal optimization problem, individuals may use their social environment to acquire information about the costs and benefits associated with the timing of childbearing.

In this paper we use an extraordinary rich data covering the fertility decisions among 150,000 Swedish women in childbearing ages and *all* of their co-workers during an eight year period to identify co-workers' influences on the individual fertility timing decision and examine how such fertility peer effects operate. Co-workers constitute a particularly important peer group in this context since some of the costs associated with childbearing are likely to be workplace specific. Moreover, most of us interact with this peer group on a day-to-day basis and the frequency of exposure to childbearing among co-workers is typically high in comparison with other social networks.[1]

The difficulties of identifying peer effects are well-established (c.f. Manski, 1993; Moffitt, 2001). Mainly two issues arise when attempting to identify the effect of co-workers' childbearing on individual childbearing. The first is the simultaneity bias (or the "reflection problem") generated by the simultaneous feedback between the focal subject and the peer group. The second problem is that of omitted variable bias. In our setting, it is for example essential to consider that family friendliness of jobs is a potentially important determinant of many women's employment decisions (Herr and Wolfram, 2009), and that that friends and relatives are well utilized in job search (c.f. Montgomery, 1991; Ioannides and Loury, 2004). It is therefore of crucial importance to

[1] Surprisingly few studies have used micro data to test for the prevalence and importance of fertility spill-over effect in practice. Those studies that have used micro data either looks at interactions within developing countries (Manski och Mayshar, 2002; Munshi och Myaux, 2006), among very young women (Crane 1991; Case och Katz, 1991) or within families (Kuziemko 2006). Moreover, the main interest has been focused either on intermediate outcomes such as contraceptive use, or on completed fertility.

make sure that the estimated peer effect is not caused by endogenous sorting of workers across firms sharing similar preferences or other unobserved determinants of childbearing. Similarly, unobserved shocks that independently affect both the timing of individual and co-workers' fertility decisions could also lead to a spurious correlation between peers' behavior. For example, correlations in co-workers' childbearing could simply proxy for changes in firm policy, or an increased risk of mass lay-offs etc., rather than true peer effects.

The detailed data and the focus on the timing decision of childbearing help us address these issues. First, the reflection problem is mitigated by focusing on the influence of co-workers past childbearing. Using lagged behavior of a peer group to identify the effects of social influences is in general not a fail-proof plan, since it requires that the agents are not forward looking, or that the transmission of the social effect follows the assumed temporal pattern (Manski, 1993). In this context, the inherent random nature of the *exact* timing of conception together with the monthly data on childbirth allows us to relax the assumption of non-forward looking agents. It is very difficult, both for the individual and the co-workers, to exactly predict when conception takes place. This key notion together with a possibility to consider a detailed lag-structure allows us to form testable *a priori* predictions about the temporal pattern that the peer effects will need to follow in order for us to worry about simultaneity bias or endogenous selection.

Second, we also consider several falsification exercises where we test if the employee is affected by *(i)* the contemporaneous childbearing of future co-workers, *(ii)* the childbearing of the true co-workers' siblings, and finally *(iii)* the childbearing of the co-workers employed in the same firm but not in the same workplace. The individuals in these three "placebo peer groups" are likely to share many of the unmeasured attributes of the true co-workers and the focal worker, and are also likely to experience similar types of unobserved chocks as the focal worker. However, since they are not employed in the same workplace we do not expect them to influence the childbearing decisions of the focal worker unless the main effect is spurious.

The results indicate that co-workers' fertility increases the propensity of childbearing among their fellow co-workers. The estimated effect of co-worker's childbearing on the

probability of first birth follows an inverted u-shaped pattern with respect to time elapsed since the co-worker's child was born. The maximum effect (9 % increase in the probability of own childbearing) is reached about 13–24 months after the birth of a co-worker's child, and then declines. This observed social influence controls for non-parametric monthly duration dependence, time-effects, work place size, regional unemployment rate and several important individual and co-worker characteristics. The robust dynamic pattern of the main estimates across specifications and subgroups, and the results from the falsification tests jointly support a causal interpretation of the main effect.

Further explorations allow us to assess how individuals are influenced by their social environment in fertility. In line with a large sociological literature on friendship formation we document stronger social ties between same type co-workers than different type co-workers.[2] Much more weight is put on the fertility decisions made by female co-workers and co-workers who are close in age. However, there are also interesting deviations from this same-type pattern. For example, while the parity of the childbearing co-worker does not matter for first-time mothers, it is only the childbearing among co-workers' of the same parity that matters for mothers with previous childbearing experiences. These strong within-parity peer effects and complete absence of between-parity peer effects among higher order births allows us to speculate about alternative mechanisms and also gives further support to the validity of the identifying assumption that omitted variables is not driving the results. Omitted determinants of the individual's childbearing must be completely uncorrelated across women with different number of previous children within the workplace to spuriously generate the within-parity pattern just described.

Additionally our results indicate that social status may matters for social influences in the workplace since individuals are affected by co-workers who have the same or higher, but not lower, educational level. This result is consistent with evidence from

[2] This pattern confirms the same-different type distinction in the model of friendship formation in Currarini, Jackson and Pin (2009) and is also consistent with empirical evidence suggesting that individuals tend to distinguish between same and different rather than distinguishing in any finer ways (Marsden, 1988, McPherson et al, 2001).

laboratory experiments showing that individuals are influenced by those with higher, but not lower status.

Finally we also provide suggestive evidence that fertility spills-over between different social networks; childbearing decision seems to be transmitted from the childbearing of the sibling of a co-worker *via* the co-worker *to* the focal worker with an additional lag of approximately 18 months. While all models of social effects assume that this is the case, as far as we know this is the first time that such across-network spill-over effects has been confirmed empirically.

The results in this paper shed light on two well known empirical findings. First, the presence of the fertility peer effects contributes to the understandings of the remarkable fluctuations in total fertility rates observed in many countries. (see Figure A1 in Appendix for the evolution of the TFR in Sweden). While economists have traditionally focused on the effects of price and income in determining individual fertility the existence of social multipliers implies that relatively small differences in economic fundamentals can lead to large difference in aggregate fertility rates.

Second, we contribute to the understanding of network formation and the structure of social ties in the workplace. The tendency for individuals to associate with same-type peers is a robust observation in many networks (see McPherson, Smith-Lovin and Cook, 2001 for an overview of "homophily"). The strong heterogeneity in the peer effect between same and other type peers found in this study highlights first of all that a strict focus on average peer within e.g. the workplace or neighborhood may underestimate the true economic relevance of social networks. Second it also suggests that the same social effects found in one context cannot be expected in groups comprising different composition of group members since the effect of groups varies with group composition.[3] From a policy perspective our results suggest that social effects will be more pronounced in groups of similar people.

The remainder of this paper is organized as follows. Section 2 briefly describes the mechanisms through which peers may matter for fertility decisions. Section 3 describes

[3]Recent studies show that social interactions affect individual productivity, (Bandiera, Barankay and Rasul, 2007; Mas and Moretti, 2006) pension planning (Duflo and Saez, 2003), and work absence (Ichino and Maggi, 2000;

the data. Section 4 introduces the empirical model Section 5 presents the results and Section 6 concludes.

2 Conceptual framework

Why should individuals be influenced by their peers' childbearing? Two main arguments can be traced in earlier work.

First, peer effects can arise from social learning. This means that individuals deal with uncertainty by using their network as a source of information. A frequently suggested example of this mechanism is the role of social influences on the classical fertility transition through the dissemination of information about the use of modern contraceptives. In our case information about contraceptives is likely of limited relevance, but social learning may still be important since individuals can draw inference from the experiences of co-workers to learn about the pros and cons of childbearing (Montgomery and Casterline, 1996). Besides collecting information about the childbearing experience itself, learning in our context may also include learning about parental skills and institutional arrangements and requirements regarding parental leave (Kuziemko, 2006).

A second explanation for why individuals could be influenced by the fertility of peers is that they may derive higher/lower utility from joint childbearing. Such network externalities arise when the utility from a specific activity depend on the number of participants (Katz and Shapiro, 1985). Utility from joint leisure (Hamermesh, 2000), or in our case joint parental leave, is an example of why network externalities may be influential in this setting. Joint parental leave may be especially relevant for Sweden since parents can (and women typically do) stay home for a relatively long time with their newborn child before returning to work. However, individuals may also benefit from sharing their childbearing experiences with co-workers who are in the same situation or by the direct economies of scale that arise from coordinated childcare and the sharing of material expenses.

Hesselius, Johansson and Nilsson, 2008). Except for Hesselius et al. all of these studies focus on social effects within a single firm.

So far, the mechanisms described are rather general and could apply to nearly any peer group. However, since childbearing for most women imply a non-negligible time of absence from work there may also be other forms of network externalities that are unique for the workplace setting.[4] A priori it is not evident in which direction childbearing of a co-worker affects individual fertility. For instance employees may draw inferences from their co-workers' labor market related consequences of childbearing or co-workers childbearing can give rise to peer pressure that alter the individual costs of childbearing. Additionally if employees' compete for e.g. promotion opportunities within the workplace they may take strategic considerations into account when deciding about whether and when to have children. This argument can be motivated by a human capital model where time out of work leads to loss of human capital, as well as by a signaling model where there is a penalty for being the "first-mover" in the workplace. Hence if individuals take the relative timing of childbearing into account it is easy to imagine how one worker's fertility can be very contagious within the workplace. The period of parental leave that typically occurs directly after birth may be costly also for the establishment, particularly in small workplaces where labor substitution is generally more difficult. If workers internalize such costs then the individual's probability of own childbearing may be reduced.

Just as described, there are several potential explanations for why social interactions can influence fertility. However, a priori it is not evident which of these mechanisms that dominates. Moreover, an important task of this paper is also to try to characterize what the social interactions look like. We return to this matter in Section 5.3 where we explore the way that the fertility peer effects operate with respect to individual, co-worker and workplace characteristics. This provides us with several interesting patterns that together also help us to draw some conclusions about the mechanisms described above.

[4] In Sweden mothers take 329 days of parental leave on average (which are fully financed through the social insurance system) during the first 4 years of a child's life (RFV 2004:14)

3 Data

The data used in this study comes from the IFAU database and it contains register information for the entire Swedish population aged 16–65. In addition to detailed individual background characteristics (LOUISE) the database holds information on the firm and workplace identifiers for all workplaces in which the individual is employed (RAMS). The data are further linked to the "multi-generation" register that holds information on the number of children born as well as the month of birth of each child. These data allow us to construct our measure of peer fertility and our binary outcomes variable; whether the focal worker gave birth to a child in a given month or not.

We focus on female workers between age 20 and 44 and restrict the analysis to workplaces with less than 50 employees.[5] The size restriction is important since it helps us to focus the analysis on a well-defined peer group where individuals in the network are likely to interact on a day-to-day basis. The rational for choosing to focus on women are first of all that their fertility cycle is well-defined, but also that both from the individual and the firms' perspective childbearing among women generates much more variation in terms of working hours and thereby costs due to the overwhelming share of parental leave time utilized by women. It should however be noted that this restriction does not apply to the co-workers. Thus the analysis looks at the impact of both male and female co-workers' fertility on female workers fertility.

To make the dataset manageable we select a random sample of 50 percent of the working individuals in 2004 and follow these eight years back in time (1997–2004). This means that women are defined to be under risk from 1997 through the end of 2004 as long as they are observed in a workplace, until the month when they give birth or until the month they turn 45.[6] To avoid including individuals who are only loosely connected to the workplace we retain workers with yearly labor income above the 10th

[5] The medical literature defines the childbearing age as years falling between 15 and 44 years old. However for simplicity we restrict our sample to individuals who were above 20 years old. Our choice is motivated by the fact that due to compulsory schooling in Sweden it is very rare that individuals start working and having children before this age. In 2004 only 3.4 percent of Swedish women had their first child before their 20th birthday and the average age at first birth were 29 and 31 for women and men respectively in 2004 (National Board of Health and Welfare).

[6] Since we require that the individuals should be working we include them in our sample only those years that we observe them in a workplace. This restriction implies that we will over sample individuals with stable positions on the labor market.

percentile[7]. For simplicity, for workers employed in multiple workplaces, we assume that the workplace giving the primary source of earnings also is the main arena for social interaction.[8] Because individual fertility as well as the social influence of peers may be different for women having their first, second and third child we consider up to three fertility spells. For the first-time mothers we define the duration as the number of months from age 20 and up to their first birth and for the second- and third-time mothers it is defined as the number of months from their previous child birth up to the second birth or until they are censored. Individuals who did not give birth during the observation period are followed from when they became fertile (had their previous child) and as long as they are of fertile age between 1997 and 2004.

We combine this data with time varying information on the co-workers in the particular year, month and workplace and create indicators for whether any co-worker had a child in a specific month. We also add information on the age structure, gender composition, the share of co-workers with college education, workplace size, number of children of the co-workers and the sector of employment (public/private).

Table A 1 reports summary statistics for the first, second and third order fertility spells respectively. We see that the typical women under risk of having her first child is 27.6 years of age and works in a workplace with 18 employees. Furthermore, the average probability of having a child in a specific month is 0.005 and it varies considerably in our sample (sd. 0.07). Column (2) shows that the mean probability of having a second child is more than twice as high as the monthly probability of having the first child (0.011) suggesting that those who already have a child are much more likely to give birth to a(nother) child. The probability of third birth is only 0.002. These patterns reflect rather short timing between first and second order births and the common practice in Sweden to stop reproducing after the birth of the second child.

Figure A2 and Figure A3 in Appendix show the baseline hazard functions for the first two births. The first graph illustrates that the likelihood of childbearing for first-time parents in our sample peaks around age 30. This is somewhat higher than the average age (29 years) which is likely due to the fact that we have restricted our sample

[7] The threshold is based on all employees at the labor market, both males and females.

to women with a relatively strong connection to the labor market. Figure A3 suggests that the probability of delivering the second child peaks after 28 months (2.3 years) and that most parents (70 percent) had their second child within 6 years from their first child.

4 Empirical specification

As discussed above we model the individual fertility decision as a function of co-workers past childbearing. An important feature of our setup is that we are able to provide evidence of what the lag structure of the transmission of the social effect looks like and we will see later in the estimation results that the estimated peer effect is consistent with that individuals indeed react to co-workers realized fertility and not with anticipation or joint planning of future childbearing.

To capture the dynamic pattern co-workers' fertility have on individual childbearing we estimate a conditional linear probability models which can be thought of as a linear approximation of a hazard model allowing for time-varying covariates, non-parametric duration dependence and time period effects (c.f. Allison, 1982).[9] Our baseline specification is

$$\begin{aligned} Y_{ijtc} = \alpha_t &+ \beta_1 (\text{Any co-worker had a child within 12 months})_{\text{ijtc}} \\ &+ \beta_2 (\text{Any co-worker had a child wihtin 13-24 months})_{\text{ijtc}} \\ &+ \beta_3 (\text{Any co-worker had a child wihtin 25-36 months})_{\text{ijtc}} \\ &+ X_{ijtc}\gamma + C_{ijtc}\delta + \eta_c + \varepsilon_{ijtc} \end{aligned} \tag{1}$$

where the dependent variable Y_{ijtc} indicates whether employee i in workplace j had a child in calendar month *c* and duration *t*. α_t is a measure of duration dependence and is non-parametrically specified using specific month of duration dummies. These dummies captures that the baseline hazard of childbearing varies over the fertility cycle. The variables "Any co-worker had a child within 12, 13–24 or 25–36 months" are

[8] The vast majority of the individuals in our data is only employed at single workplace.
[9] We have also re-estimated the model using a Maximum Likelihood estimator. This did not alter any of the conclusions.

indicators for whether a co-worker had a child within 12, 13-24 and finally 25-36 months prior to month c.[10] X_{ijtc} is a vector of individual background characteristics, C_{ijtc} is a vector of co-worker and workplace background characteristics such as the previous number of children to all co-workers, age distribution, gender and educational attainment composition, dummies controlling for establishment size in 10 worker intervals, sector of employment etc. η_c are calendar period (year*month) effects which are included to control for general trends in fertility and finally ε_{ijtc} is the error term.

The main focus in our analysis is how co-worker childbearing affects the timing of first births since the variation in timing is largest for these births (see Figure A2), but we also report estimates for higher order births. Hence we estimate equation (1) for individual at risk of having their first, second and third child separately using OLS.[11] For first births the duration dependence is accounted for by "months since age 20"–specific indicator variables up until the first birth (or until censoring) and for higher order births the number of months from the previous birth. Note that the combination of the duration dummies (months since age 20) and period (month*year) fixed effects also accounts for general cohort effects.[12]

The dynamic impact of fertility peer effect is captured by the parameters of interest β_1, β_2 and β_3 in equation (1). The estimates of these parameters measure the impact of co-workers' recent fertility on the likelihood of childbearing in a specific month. The maintained assumption for identification is that there are no unobservable determinants correlated with both the lagged timing of co-workers fertility and the focal worker's probability of giving birth to a child in month *c*. However, unobserved common shocks that change the probability of childbearing for all co-workers may generate precisely this type of spurious correlation between the fertility of the co-workers and the focal worker. While the period fixed effects accounts for general fluctuations in fertility (due to e.g. general policy shift in e.g. childcare allowances, or business cycle effects), there

[10] The variable "Any colleague had a child within 12 months" counts from t-1 to t-12. Hence by construction the dummy takes on the value zero if the colleague delivered in the *same* month as the individual. This implies that we avoid the possibility that two colleagues having a child together show up as one of them responding to the other.
[11] During our observation period higher order birth is uncommon.
[12] For the second and third births regressions we also include year of birth fixed effects.

can still be workplace specific shocks changing the probability of childbearing for all workers in a particular workplace e.g. increased job flexibility, management changes, or an increased risk of mass lay-offs.

Moreover, the identifying assumption could also be violated if co-workers share other unobserved characteristics that affect the timing of childbearing. One example is similar taste for childbearing. Sorting of this kind is a valid concern since family friendliness of jobs is an important determinant of many woman's employment decisions (Herr and Wolfram, 2009). It is furthermore well established that friends and relatives are a well-utilized way to find jobs (c.f. Montgomery, 1991; Ioannides and Loury, 2004). Sorting for these or other reasons associated with childbearing could lead to very homogenous workplaces and result in correlations between co-workers' childbearing even if they are not directly influenced by each other's behavior.[13]

However the difficulty in foreseeing exactly when conception takes place helps us to form expectations about how the estimates of the parameters of interest in equation (1) should behave for us to worry that omitted variables biases our estimator. First, suppose that two individuals (co-workers) start trying to conceive at the same time (e.g. due to unobserved common shocks at the workplace level). Due to the partly random nature of timing of conception some will conceive sooner than others. However, calculations in Kuziemko (2006) suggest the probability that the two individuals will end up having children more than 6 months apart is only around 14%. This implies that if unobserved common shocks are causing a spurious correlation between co-workers' fertility decisions then we expect the strongest effect to show up during the first 12 months period after the birth of a co-worker's child and then decline (i.e. $\beta_1 > \beta_2 > \beta_3$). Furthermore if the estimates simply reflect endogenous sorting of workers then we

[13] A simple but unfeasible path to follow in order to try to control for workers sorting would be to add workplace fixed effects to equation (1). However, considering that we have a panel stretching only over 8 years and that we include lagged dependent variables for up to 36 months (which would be what the "co-worker had a child" dummies would be characterized as in a within-workplace analysis) the within-workplace estimates would, as is well known, be severely downward biased using an OLS estimator (Nickell, 1981). An alternative way to solve this problem would be to aggregate the data to the workplace level and then run regressions using a GMM estimator. But since an important focus of our analysis is to study in which way peer effects operate in relation to individual characteristics we feel reluctant to take this measure, and instead focus on other ways to make sure that the peer effects are not driven by endogenous sorting across workplaces (see discussion below).

expect the timing of co-workers childbearing to be irrelevant. To make this clear, suppose that workers conceive independently of each other (i.e. no social interactions) with some given probability each month. Then since there is an equal chance to have a co-worker who gave birth within 12, 13–24, and 25–36 months we would expect that $\beta_1 = \beta_2 = \beta_3$. We will see that our estimates do not match either of these predictions. Moreover the validity of our main estimates is also strengthened by a number of robustness checks and falsification test. These are described in detail in Section 5.2. But first we turn to our main results.

5 Results

5.1 Main results

Column 1 of Table 1 shows the baseline estimates of the three β's from equation (1) capturing the impact of co-workers' childbearing on own fertility for first-birth women after controlling for duration dependence $\alpha(d)$ and period (year×month) fixed effects. The first, second and third row report the estimates of β_1, β_2 and β_3, i.e. the estimated impact of being exposed to a co-worker who had a child 1–12, 13–24 and 25–36 months ago respectively. The estimates of β_1 are small and not significantly different from zero, but still precisely estimated. In contrast the estimates of β_2 show a positive association between the focal workers childbearing and the past childbearing of her co-workers. Evaluated at the mean probability of childbearing this estimate suggests that individuals are on average 10.9 percent (0.00057/0.00523) more likely to have their first child 13–24 months after the birth of a co-worker's child. The inclusion of additional individual level controls for marital status, and college education (Column 2), as well as co-worker and workplace controls (Column (3)) does not alter the picture (see Table A2 in Appendix for all controls). The robustness of the estimates to the inclusion of these important covariates is reassuring since it suggests that bias due to omitted variables probably also are less of a concern.

Together the three estimates suggest that the co-workers' fertility decisions primarily increase fertility decisions with a lag of about one year after the birth of a co-worker's

child. Hence, for first order births the influence of peers' childbearing is not driven by the immediate news that a co-worker is expecting, since the peer effect shows up first after the co-worker's baby is born. Even more importantly, the inverted u-shaped pattern of the effect with respect to the time elapsed since a co-workers' child was born speaks against the alternative hypothesis of unobserved common shocks or that co-workers plan their births so to be able to enjoy joint maternity leave. As discussed above if unobserved common shocks would induce individuals to start trying to conceive simultaneously we would expect to find the largest effect within the first 6 months. In our case we do not even find a significant effect within the first 12–month period after a birth of a co-workers' child. This clear and consistent pattern across specifications and (as we show below) sub-samples suggests that common unobserved shocks is not driving the estimates of the social effect. Similarly, as motivated above the pattern does not seem to be consistent with a situation where endogenous sorting of workers is causing a spurious correlation in the timing of pregnancy.

Table 1 Baseline estimates of co-worker's fertility on the probability of **first** birth

Specification:	(1)	(2)	(3)
Sample:	*First births*	*First births*	*First births*
Any co-worker had a child within:			
1–12 months	0.00002	0.00003	0.00005
	(0.00007)	(0.00007)	(0.00007)
13–24 months	0.00057***	0.00056***	0.00047***
	(0.00007)	(0.00007)	(0.00007)
25–36 months	0.00029***	0.00028***	0.00013*
	(0.00007)	(0.00007)	(0.00007)
Duration dummies	Yes	Yes	Yes
Year × Month dummies	Yes	Yes	Yes
Own char.	No	Yes	Yes
Workplace char.	No	No	Yes
Mean Y	0.00523	0.00523	0.00523
Observations	5,575,497	5,575,497	5,573,397

Notes: *,** and *** denote statistical significance at 10/5/1 percent level respectively. Standard errors robust for serial correlation at the workplace level are shown in parenthesis. The level of analysis is the individual-month. In addition to the fixed effects indicated by the table regression (3) controls for establishment size dummies in intervals of ten employees as well as the regional (county/year) unemployment rate where the workplace is located. The workplace characteristics includes number of children of co-workers, the share of co-workers in fertile age, the share of co-workers, close-in-age, share of female co-workers, share married co-workers, share of co-workers with college education.

To put our estimates in perspective consider first that for example Del Bono et al. (2008) find that women are about 10% less likely to have a child in the first couple of

years after losing their job. Similarly Mörk et al. (2008) find that increasing childcare subsidies with the equivalent of USD 10,000 increased fertility for eligible Swedish couples by about 5-10% within 18 months. The magnitudes of the social effect are furthermore very similar to those found in recent studies also focusing on co-worker peer effects in general. For example, Mas and Moretti (2009); Falk and Ichino, (2008); Ichino and Maggi (2000) and Hesselius, Johansson and Nilsson (2009) all find co-worker peer effects which are in the vicinity of our estimates.

5.2 Robustness checks and falsification tests

5.2.1 Robustness checks

Our results are based on the maintained assumption that unobserved determinants of the timing of fertility are uncorrelated within the workplace. While we can never test this identifying assumption directly, the richness of the data allows us to design several indirect tests to assess the plausibility of this identifying assumption. But we start of by showing that the baseline results are robust to changes in the specification of our baseline model.

In column (1) of Table 2 we have re-specified the baseline model by replacing the three 12-month indicators of interest with six 6-months interval dummies. The estimates confirm that the baseline specification indeed does a god job in modeling the dynamic impact of co-workers' childbearing on timing of fertility. As before, in the first 1–6 and 7–12 month intervals the behavioral impact of being exposed to co-workers' childbearing is small and not statistically significantly different from zero. However, in month 13–18 the effect shoots up and then declines slowly until it turns insignificant after 31–36 months. Again, the absence of effects within the first 6 months strengthens the conclusion that common shocks are not driving the estimated social effect.

Next we assess whether increasing the dose of exposure that is the number of co-worker children born within each period, has additional effects. We do this by interacting the baseline variables of interest with dummy variables indicating whether more than one co-worker had a child 1–12, 13–24 and 25–36 months ago. The estimates in column (2) provide a clear dose-response pattern of being exposed to childbearing of several co-workers; the interaction terms are positive and of significant size. Controlling

for additional births does however leave the baseline estimates essentially unchanged suggesting that the main effect is not driven by exposure to many births. We therefore stick to the more parsimonious specification for the remainder of the analysis.

As common shocks do not seem to explain the observed peer effect pattern we now investigate whether sorting of workers based on e.g. child-friendliness of the workplace is a valid concern. It is important to remember that even in the baseline model we control for number of previous children in the workplace, which to a large degree should capture selective sorting. Still it is possible that workers who are planning to have a child systematically move to workplaces where childbearing is more frequent. As a first test of the validity of this concern we split the sample with respect to tenure and report the results separately in columns (3) and (4) of Table 2. Comparing the estimates we see that there are no major differences in the impact of peers on women with more and less than five years of tenure. If anything the effect seems to be somewhat stronger for those with longer tenure, suggesting that sorting into establishments just before planning a pregnancy is not driving our results.

Table 2 Robustness checks

Specification:	(1)	(2)	(3)	(4)
Sample:	Baseline	Baseline	< 5 years of tenure	> 5 years of tenure
1–6 months	0.00010			
	(0.00008)			
7–12 months	0.00012			
	(0.00008)			
13–18 months	0.00048***			
	(0.00008)			
19–24 months	0.00028***			
	(0.00008)			
25–30 months	0.00016**			
	(0.00008)			
31–36 months	0.00005			
	(0.00008)			
12 months		0.00002	-0.00001	0.00029
		(0.00008)	(0.00007)	(0.00021)
13–24 months		0.00043***	0.00044***	0.00059***
		(0.00008)	(0.00007)	(0.00021)
25–36 months		0.00013	0.00011	0.00040*
		(0.00008)	(0.00007)	(0.00021)
Multiple births:				

Specification:	(1)	(2)	(3)	(4)
12 months × (>1 birth)		0.00024**		
		(0.00012)		
13–24 months × (>1 birth)		0.00030***		
		(0.0001)		
25–36 months × (>1 birth)		0.00001		
		(0.00011)		
Duration dummies	Yes	Yes	Yes	Yes
Year × Month dummies	Yes	Yes	Yes	Yes
Own characteristics	Yes	Yes	Yes	Yes
Workplace characteristics	Yes	Yes	Yes	Yes
Mean Y	0.00523	0.00523	---	---
Observations	5,573,397	5,573,397	4,559,220	1,014,177

Note: see Table 1

5.2.2 Falsification tests: placebo co-workers

In Table 3 we continue to more rigorously assess the validity of the identifying assumptions. Here we re-estimate the specification in equation (1), but instead of focusing on the impact of the true co-workers, we now instead look at whether the childbearing behavior in three "placebo peer groups" also matter for individual childbearing. The placebo co-workers groups we consider are:

1) FIRM-LEVEL CO-WORKERS: These workers are employed in the same firm, region (21 regions), and 2-digit industry, but not in the same workplace as the focal worker. Since these are individuals that have sorted into the same firm-industry-region as the focal worker, they are first of all likely to experience the same type of shocks that could generate the type of spurious relationships between co-workers that we worry about. Secondly, for the same reasons they are likely to be highly similar to the focal worker even in terms of observed (this is shown in Table A3 and discussed below) and unobserved characteristics.

2) FUTURE CO-WORKERS: This placebo-peer group consists of the future co-worker's to the female workers in our sample that switch workplace during the observation period.[14] The idea is that the contemporaneous behavior of these future co-workers should not have any effect on the contemporaneous behavior of the focal worker unless i) they already are friends, or ii) they share unobserved characteristics

[14] To make sure that we capture actual job switchers we restrict the sample to women who switch jobs only once during the observation period and we require that the individual is observed for at least 2 years before and after the change in jobs.

that both induces them to sort into the same workplace at a later stage and that also affect the timing of childbearing.

3) SIBLINGS OF ACTUAL CO-WORKERS: This placebo-peer group is likely to share many of the co-workers observed and unobserved characteristics. They have experienced similar upbringing and might therefore have formed similar preferences for the timing of childbearing. If these unobserved preferences or characteristics are correlated between co-workers we expect that the childbearing of the co-workers siblings should not affect the focal worker unless i) they already know each other or ii) they share unobserved characteristics which affect the timing of childbearing. However, Kuziemko (2006) find evidence of fertility peer effect among siblings and hence if childbearing really is contagious then it is conceivable that the childbearing of siblings could also spill-over to the focal worker via the fertility decisions of the actual co-worker. In this case we would expect the effect to show up after the additional lag it takes for first the co-worker and then the focal worker to react. Alternatively, if the sibling, co-worker and the focal worker share unobserved characteristics, or if the sibling and the focal worker influence each other directly, we would expect to find a spurious placebo co-worker effect that follows the same pattern as the baseline effect of true co-workers.

Table 3 presents descriptive statistics for the main samples and the three placebo peer groups. We conclude from this table that observed characteristics of the true co-workers are all highly similar to the placebo peer groups. There are essentially two exceptions; the average number of co-workers in the average firm is naturally much higher than in the average workplace, and since the labor market is segregated with respect to gender the average share of females among the true co-workers is higher than that among the co-workers' sibling since this placebo group to a higher extent consist of brothers. In the empirical specification we address these differences by controlling for co-worker' sibling characteristics and we also include 9 dummies for firm size where relevant. Since the three placebo-peer groups are fairly balanced on observed characteristics it is reasonable to believe that they are similar in terms of unobserved characteristics as well. However, we do not expect to find the same fertility peer effects for these placebo workers as for the true co-workers unless the childbearing of the co-workers simply

proxy for some unobserved determinant that the focal worker, the co-workers and the placebo co-workers have in common.

Table 3 shows the estimates from these falsification tests. I.e. column (2) report the estimates for the first placebo peer group, "the firm co-workers", column (4) presents the results for second placebo peer group "the future co-workers", and column (5) shows the estimates for the third placebo peer group "co-workers' siblings". In addition since the placebo-tests restrict the samples to women who work in private firms with more than one workplace in column (1) and to those who switch jobs in column (3) for comparison we also report the impact of the true co-workers childbearing in each of these samples. While the estimates for these true co-workers are highly similar to the baseline estimates in Table 1, neither one of the three placebo co-worker regressions provides results that are even close to these results.[15]

Interestingly, the only estimate that is significantly different from zero in any of the placebo peer group regressions is the 25–36 month lagged effect in the co-workers' sibling sample. To further assess this intriguing pattern we estimated a regression where we allowed co-workers' siblings to affect childbearing decisions of the focal worker in 6-months intervals for up to 52 months. The results are presented and compared to the baseline 6-month interval estimates (from Table 2) in Figure 1 and 2. Interestingly the parameter estimates are small and insignificant for the first 30 months after a birth by a co-worker's siblings only to show up after a lag of 31–36 months and then fade out slowly. This suggests that the fertility decision spills over from the sibling of the co-worker via the co-worker to the focal worker supporting the notion that fertility decisions truly are contagious and that they may also spill-over across different networks.

To summarize, the results of the robustness checks in Table 2 and falsifications tests in Table 3 strongly support a causal interpretation of the baseline estimates. We now proceed by further investigation of the underlying mechanisms that can explain the fertility peer effects.

Table 3 Falsification exercises

	(1)	(2)	(3)	(4)	(6)
Sample:	Private firms with multiple workplaces	Private firms with multiple workplaces	Job switchers	Job switchers	All
Treated group:	***True:*** *Same firm, same workplace*	***Placebo:*** *Same firm, different workplace*	***True:*** *Contemporaneous co-workers*	***Placebo:*** *Future co-workers*	***Placebo:*** *The true Co-workers siblings*
Any co-worker had a child within:					
12 months	0.00012	0.00015	0.00026	-0.00003	0.00005
	(0.00016)	(0.00025)	0.00021	(0.00020)	(0.00007)
13-24 months	0.00067***	-0.00015	0.00072***	0.00015	0.00011
	(0.00015)	(0.00025)	0.00021	(0.00020)	(0.00007)
25-36 months	0.00019	0.00010	0.00032	0.00000	0.00031***
	(0.00016)	(0.00025)	(0.00022)	(0.00020)	(0.00007)
Duration dummies	Yes	Yes	Yes	Yes	Yes
Year×Month dummies	Yes	Yes	Yes	Yes	Yes
Own characteristics	Yes	Yes	Yes	Yes	Yes
True co-worker characteristics	Yes	Yes	Yes	Yes	Yes
Placebo co-worker characteristics	No	Yes	No	Yes	Yes
Mean Y	0.00503	0.00503	0.0058	0.0058	0.00523
Observations	1,066,052	1,066,052	729,767	729,767	5,403,084

Note: The specification in column (2) additionally control for firm size dummies in nine intervals (2–9, 10–19, 20–29, 30–39, 40–49, 50–99, 100–199, 200–499, >499 employees).

[15] One concern is that since the number of co-workers in the same firm can be much larger than the number of co-workers within the same workplace we have also estimated the "same firm different workplace" regression using only firm that have less than 50 employees in total. These estimates were very similar to the full placebo group sample estimates.

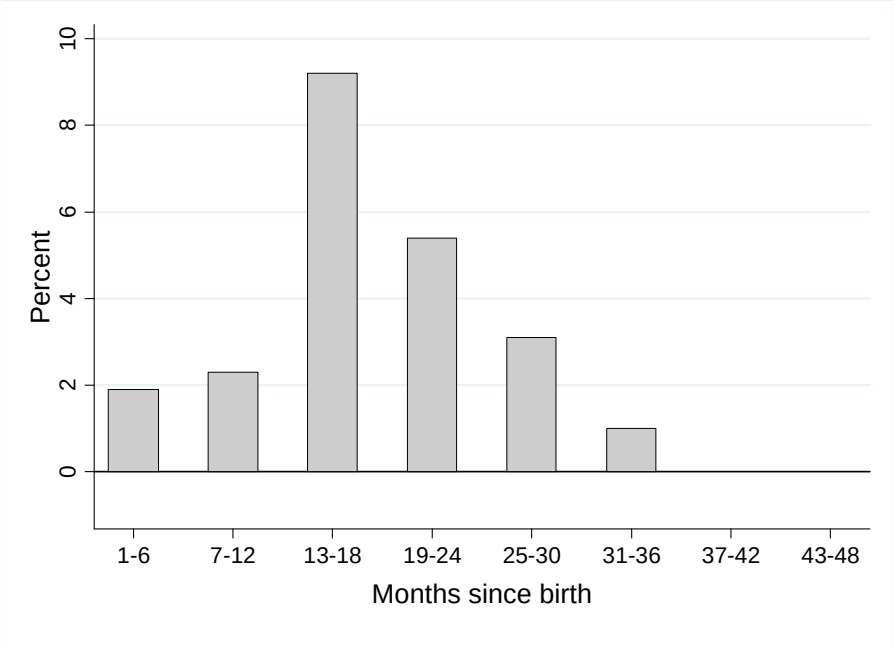

Figure 1 Peer effects of *true* co-workers' childbearing

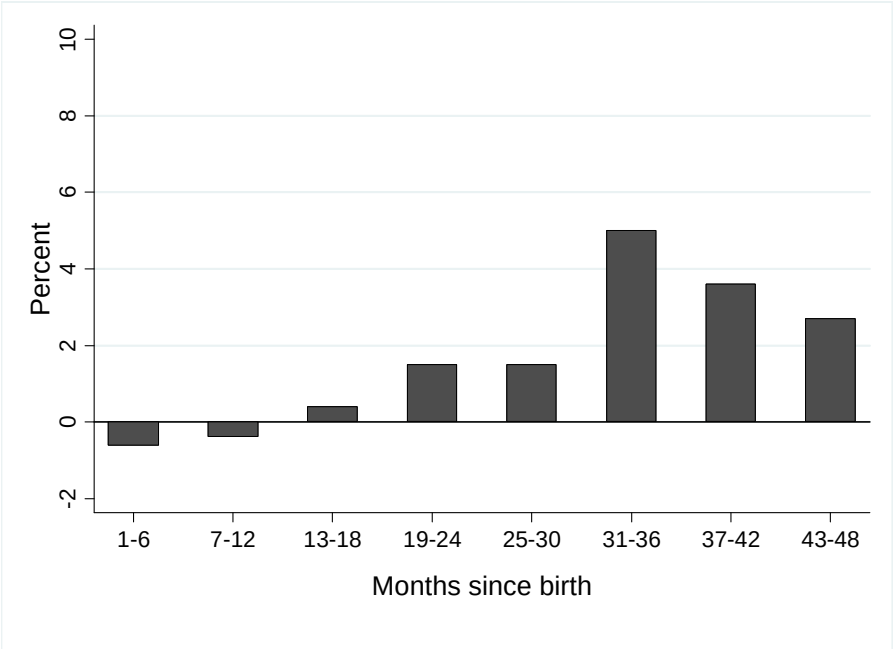

Figure 2 Peer effects of the co-workers' siblings' childbearing

5.3 Heterogeneity: individual and co-worker characteristics

Next we examine if the influence of the peers varies with respect the focal workers own, her co-workers and the workplace characteristics. In conjunction with the results we also discuss alternative explanations for the heterogeneous effects.

5.3.1 Own characteristics: fertility cycle, education and civil status

We begin in Table 4 by looking at whether the peer effect differs depending on where the individual is in the fertility cycle. We divide the fertility cycle into an early (age 20–27), primary (age 28–36) and late (age 37–44) stage.[16] Columns (1)–(3) in Table 4 show that women are influenced in all stages of the fertility cycle and in fact most strongly towards the later stages.[17] This pattern could be due to the formation of tighter bonds between older co-workers because of longer joint tenure at the workplace. Alternatively, due to the concaveness of the life cycle earnings profile the cost of career interruptions should be relatively lower towards the end of the fertility cycle. Thus the lower costs of reacting to peer influences is another explanation for why women respond more strongly to their peers in the later stages of the fertility cycle.

Since we do not have data on completed fertility for all workers in our sample, the distinction between pure timing effects and effects on completed family size is difficult. The fact that peers childbearing also influence women without previous children who are above their primary childbearing age does however indicate that social interactions may not only affect the timing of childbearing but also the decision of whether to have a child or not. We will return to this matter below when we investigate the influence of peers on higher order births.

Next we look at whether the response to peers childbearing choices differs between married and unmarried women. This effect is ex ante ambiguous since on the one hand unmarried women may on average have less stable relationships making them unable to react as fast as married women can. On the other hand, married women may be less prone to be affected by outside influences if they already have made plans about the

[16] Since we focus on women without any previous children the number of months under risk corresponds to their age.
[17] Evaluated at the mean, the estimates correspond to an increase in own childbearing of 7.3 percent in the early stage, 9.4 percent in the primary stage and 14.5 percent in the late stage of the fertility cycle.

timing of childbearing. However, it is important to remember that more than 2/3 of the first time mothers are unmarried at the birth of the first child in Sweden, suggesting that marriage status perhaps is not such an important factor with respect to peer influences on childbearing. In columns (4) and (5) we see that although the point estimates are larger for the married women, when evaluated with respect to the mean the effect is actually larger among unmarried (10.6 %) than among married (7.1%). However, when looking at the cumulated effect for the entire 13–36 month period, the effect is largest for the married co-workers. All in all there seems to be no remarkable difference in the reaction to peers based on own marriage.

Finally columns (6) and (7) present the peer effect for women with college and no college respectively. The estimates suggest that the peer influence for women with college education (column 6) is stronger than for those without college education (column 7), a results that squares poorly with that the peer influence should be due to economies of scale associated with coordinated childbearing.[18]

[18] For instance; parents can derive economic benefits due to the economies of scale that arise from coordinated childcare or from the sharing of necessary baby supplies (trolleys, clothes etc.). With the generous benefits attached to having children in Sweden we believe this effect to be of second order importance. First, Sweden has a generous parental leave benefit system which allows for benefits for 480 days (16 months). The benefit constitutes 80% up to a ceiling the first 390 days and another 90 days at flat rate. Furthermore childcare is heavily subsidized in Sweden and enrollment is very high. In 2004, 90 percent of all children 3-6 attended child care (National Board of Education).

Table 4 Differential peer effects w.r.t own characteristics

	Dependent variable: Individual had its first child in month t						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Early (age 20-27)	Primary (age 28-36)	Late (age 37-44)	Married	Not Married	College	No College
Any co-worker had a child within:							
12 months	-0.00004	-0.00009	-0.00013	-0.00030	0.00005	0.00010	0.00006
	(0.00008)	(0.00025)	(0.00020)	(0.00039)	(0.00007)	(0.00012)	(0.00009)
13-24 months	0.00030***	0.00087***	0.00043**	0.00117***	0.00041***	0.00044***	0.00054***
	(0.00008)	(0.00019)	(0.00020)	(0.00038)	(0.00007)	(0.00012)	(0.00009)
24-36 months	0.00007	0.00032*	0.00033	0.00063*	0.00010	0.00029**	0.00007
	(0.00008)	(0.00019)	(0.00020)	(0.00038)	(0.00007)	(0.00012)	(0.00009)
Dur. dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year×Month dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Own char.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Workpl. char.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean Y	0.00409	0.00921	0.00297	0.01645	0.00386	0.00562	0.00498
Observations	3,838,904	1,324,836	409,657	605,112	4,967,841	2,140,535	3,432,418

Note: see Table 2

5.3.2 Co-worker characteristics and similarity

In the economic as well as in the sociological literature it is well established that people who are similar tend to associate more (Bandiera, Barankay and Rasul, 2007; Currarini, Jackson and Pin, 2008; McPherson, Smith-Lovin and Cook, 2001). However, so far in the baseline model (equation 1) we have assumed that the fertility peer effect is homogenous irrespective of whom of the co-workers that is having a child. In this section we allow the response to co-workers childbearing to vary depending on how similar the childbearing co-worker is to the focal worker. This is implemented by estimating

$$\begin{aligned} Y_{ijct} = \Omega &+ \lambda_1 (\text{Any co-worker had a child within 12 months} \times \text{TYPE})_{ijct} \\ &+ \lambda_2 (\text{Any co-worker had a child 13-24 months ago} \times \text{TYPE})_{ijct} \\ &+ \lambda_3 (\text{Any co-worker had a child 25-36 months ago} \times \text{TYPE})_{ijct} \end{aligned} \qquad (2)$$

where Ω corresponds to the exact right hand side of equation (1) and TYPE is an indicator variable for if any of the co-worker who had a child in the previous periods

were either of the same sex, close-in-age, or had the same educational attainment as the focal worker. Since we only focus on the impact on female workers, the same sex indicator measures the impact of female co-workers. The same education indicator is based on the college/no college education definition and is hence equal to 1 if the co-worker has the same level of educational attainment. Finally, two co-workers are defined as being close in age if they are born less than four years apart.

Gender, age and education

In the top panel of Table 5 the estimates of the three β's are presented (which as before corresponds to the impact of any co-workers' childbearing), and in the bottom panel the estimates of the three λ's (which reflects the additional effect the childbearing of similar co-workers have). The total effect of a same-type co-worker is obtained by adding the main effect and the interaction effect. Starting out by looking at the differential impact of male and female co-workers in column (2) we find that the entire baseline peer effect seems to be driven by the influence of female co-workers (i.e. same sex).[19] More frequent interaction among female co-workers and/or gender-specific learning are both possible explanations for this result. In our model we always control for the fraction of same type co-workers in the workplace so the stronger influence that female co-workers exhibit cannot be explained by tighter friendships with other women due to workplace gender segregation but rather that they associate more given the fraction of female co-workers in the establishment.[20] The close-in-age specification estimates reported in column (3) suggest that the influence of co-workers who are close-in-age are substantially stronger than from other co-workers; individual fertility increases with 10 percent within the first 12 months and 18 percent after 13-25 months.

Finally we look at the impact of co-workers with the same versus different educational level as the focal worker. Interestingly these estimates suggest that whereas highly educated women are affected only by other highly educated peers (column (4)),

[19] Interestingly this is precisely the same pattern that Kuziemko (2006) finds when studying peer effects among siblings. In her case it was only sisters' and not brothers' childbearing that influenced the siblings' childbearing decisions.

[20] The feature of networks that similar individuals tend to associate more is often referred to as *"homophily"* and it can occur for various reasons. The literature often distinguish between *baseline homophily* which arises due to

low educated women are influenced by all co-workers regardless of educational level (remember that the total effect of same type co-workers in column (5) is the sum of the main effect and the interaction effect). If individuals interact mainly with co-workers who have the same educational level then we expect both high and low educated women to be primarily influenced by their same type peers. However, the anomalies in the same-type pattern that we find; no peer influence of low educated co-workers on high educated workers but a significant impact of high educated co-workers on low educated workers rather speak to a literature suggesting that besides similarity, individual interactions may also be determined by social status (c.f. Akerlof and Kranton, 2000). Moreover this result is in line with laboratory experiments suggesting that people are influenced by the behavior of individuals with higher, but not lower, social ranking than themselves (Kumru and Vesterlund, 2008).[21]

contact availability and *inbreeding bias* where same-type friendships form at rates that exceed these relative fractions in the population (see McPherson et al (2001) for an overview on homophily).

[21] Kumru and Vesterlund (2008) show that individuals are more likely to mimic the behavior of high-status individuals than low- status individuals in charitable contributions.

Table 5 Heterogeneous peer effects: similarity

	(1)	(2)	(3)	(4)	(5)
Co-worker type:	Baseline All co-workers:	Same sex Co-workers:	Close in age Co-worker:	Same education Co-workers: College	Same education Co-workers: No College
Any co-worker had a child within:					
12 months	0.00004	0.00007	-0.00031***	0.00011	-0.00035**
	(0.00007)	(0.00010)	(0.00008)	(0.00015)	(0.00016)
13-24 months	0.00048***	0.00016	0.00009	0.00011	0.00063***
	(0.00007)	(0.00011)	(0.00008)	(0.00014)	(0.00017)
24-36 months	0.00018**	0.00000	-0.00014*	0.00005	-0.00021
	(0.00007)	(0.00011)	(0.00008)	(0.00014)	(0.00017)
This type of co-worker had a child within:					
12 months		-0.00000	0.00088***	-0.00005	0.00052***
		(0.00012)	(0.00012)	(0.00019)	(0.00017)
13-24 months		0.00047***	0.00107***	0.00058***	-0.00011
		(0.00012)	(0.00012)	(0.00018)	(0.00018)
24-36 months		0.00026**	0.00096***	0.00042**	0.00034**
		(0.00012)	(0.00012)	(0.00018)	(0.00017)
Duration dummies	Yes	Yes	Yes	Yes	Yes
Year×Month dummies	Yes	Yes	Yes	Yes	Yes
Own char.	Yes	Yes	Yes	Yes	Yes
Workpl. char.	Yes	Yes	Yes	Yes	Yes
Mean Y	0.00523	0.00523	0.00523	0.00562	0.00498
Observations	5,575,497	5,575,497	5,575,497	2,140,535	3,432,418

Note: see previous Tables and text for the definition of variables.

Birth order

The baseline results in Table 1 reported the peer effect for women at risk of having their first child. In this section we examine whether co-workers also influence the timing of the second and third child. Since these women already had previous children they should have little use of further information from peers about the nature of childbearing. However, looking at second time mothers in column (2) of Table 6 we see that the peer influence is almost as strong as for first time mothers. Moreover, for this group of women peers childbearing increases the propensity of giving birth even within 12 months after they had a child. This is not surprising since couples who already have had a previous child are likely to be able to react sooner than couples who are about to have

their first child.[22] This difference between first and second time mothers is furthermore consistent with that learning about the nature of childbearing itself is more important for first time mothers since it may take some time before the most relevant experiences of the co-worker is actually realized.[23]

Even for women with two previous children we find some weak evidence (a 5% increase within 13–24 months) of a peer effect as suggested by column (3). Besides the astounding homogeneity of the timing of the effect across the birth orders, the fact that also third-order births may be influenced again indicates that peers may potentially also shift the preferences for optimal family size. Women having their third child are reacting somewhat slower to peer influences than second order births which consistent with that Swedish couples generally decide to stop trying to have more children after the second child is born. Hence, the time it takes women to re-negotiate the views of the optimal family size with partners may perhaps delay and mute any response to the influences of peers. This notion is also supported by the fact that the estimate for the 25-36 month interval for the third order births is only slightly lower than the 13-24 months estimates, while the differences between the same two coefficients for the first and second order births are considerably larger.

Related to the above finding that similar co-workers do exhibit stronger peer influence on each others childbearing decision, in column (4)–(6) of Table 6 we now look at whether individuals are differentially affected by co-workers who have the same number of previous children. Intuitively, this could be the case if there is some type of information that is unrelated to the childbearing experience in general but specific to the birth order of the child. For instance, mothers with one child might look at the behavior of their two-children peers to draw inferences of about the labor market consequences of having a second child, the organization of work and family with two kids, or the

[22] We have also estimated this model using 6-months intervals. The estimates from this more flexible specification show that the entire within 12 month effect is driven by women giving birth between 7 and 12 after the birth of a co-worker's child [est.: 0.00068 (std.err.: 0.00020)]. These estimates are retain for expositional purposes but are available upon request from the authors.

optimal timing of the second child. Another plausible alternative is that co-workers who already have a child have formed tighter bonds with the co-workers who already have a child.

The estimates in columns (4)–(6) are estimated using the model in equation (2), where TYPE now is equal to 1 if the co-worker who just gave birth previously had the same number of children, hence we now allow for parity-specific peer effects. Starting with the first-time mothers in Column (4) we find that these women are influenced by their co-workers' childbearing irrespectively of the birth order of the co-worker's child. In contrast, for second and third time mothers (Columns (5) and (6)), we find clear evidence of within-parity peer effects while cross-parity effects are completely absent. That is, the childbearing co-workers are only influencing behavior of the focal worker if they are having a child of the same birth order.

This clear pattern is interesting for at least two reasons. First, it suggests that the mechanisms underlying the peer effect seems to differ depending on own previous childbearing experience. Because women without children are equally influenced by the childbearing of any co-worker perhaps the main peer mechanism in this case is social learning about the childbearing experience itself. Arguably this type of information could be inferred from any co-worker irrespectively of their number of previous children. However, for higher order births individuals only use their same parity-peers to attain information about the specific experience of having a second or a third child, or the optimal timing of child spacing. Lower order childbearing among co-workers is however disregarded and do not change the optimal timing of their next child, potentially because births among lower-parity women do not generate any information or experience which is of any use for the focal worker.

Second, the within-parity peer effects for the higher order births provide additional evidence that our identifying assumptions are valid. To see this clearly; if omitted variables were to generate the effect in column (5) and (6) they must be uncorrelated

[23] Additional the quicker response among women about to have their second child could go through the information channel by diffusion of information about the peculiarities of the of the Swedish "speed premium" policy. This policy provides strong economic incentives for parents to space their children closely together. See Hoem, 1990 for more details on the Swedish speed premium. At the same time it is important to remember that the speed premium should

across parity groups in order to explain the pattern that we find. Standard omitted variables such as common unobserved shocks at the workplace level are unlikely to satisfy this condition: the shock would have to be due to something altering the childbearing incentives of the women of the same parity only and no one else. Thus we interpret these results as an additional and important piece of evidence that individuals responding to their co-workers' fertility rather than to a common unobserved shock.

Table 6 Heterogeneous peer effects: birth order

	(1)	(2)	(3)	(4)	(5)	(6)
SAMPLE:	1st birth	2nd birth	3rd birth	1st birth	2nd birth	3rd birth
Any co-worker had a child within:						
12 months	0.00004	0.00044**	-0.00005	0.00001	0.00020	-0.00007
	(0.00007)	(0.00017)	(0.00005)	(0.00012)	(0.00019)	(0.00006)
13–24 months	0.00048***	0.00083***	0.00010*	0.00047***	0.00023	0.00009
	(0.00007)	(0.00017)	(0.00005)	(0.00011)	(0.00019)	(0.00005)
24–36 months	0.00018**	0.00033**	0.00008	0.00024**	-0.00009	0.00007
	(0.00007)	(0.00017)	(0.00005)	(0.00011)	(0.00019)	(0.00005)
Any co-worker had a child of the **same birth order** within:						
12 months				0.00003	0.00029	0.00022
				(0.00013)	(0.00028)	(0.00028)
13–-24 months				0.00000	0.00151***	0.00040*
				(0.00012)	(0.00025)	(0.00022)
24–36 months				-0.00009	0.00104***	0.00040**
				(0.00011)	(0.00024)	(0.00019)
Dur. dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year×Month dummies	Yes	Yes	Yes	Yes	Yes	Yes
Own char.	Yes	Yes	Yes	Yes	Yes	Yes
Workpl. char.	Yes	Yes	Yes	Yes	Yes	Yes
Mean Y	0.00523	0.01105	0.00202	0.00523	0.01105	0.00202
Observations	5,573,397	2,015,434	3,729,137	5,573,397	2,015,434	3,729,137

Notes: *,** and *** denote statistical significance at 10/5/1 percent level respectively. Standard errors robust for serial correlation at the establishment level are shown in parenthesis. The level of analysis is the individual-month. In addition to the fixed effects indicated by the table all regressions control for establishment size dummies in intervals of ten employees. Each regression is estimated on a sample of individuals at risk of having their first, second and third child. Hence in the second sample we condition on having a first child. The higher mean probability in column (2) reflects that a high share of one child parents choose to have a second child and for the same reason the number of person/month observations is lower for the second order birth than for the other births.

have no impact on the estimated effect since we always compare mothers with the same distance from the previous child an hence any general speed premium effect should be controlled for by the duration dummies.

5.3.3 Workplace characteristics: size and sector

Next we look at whether the estimated peer effect varies with respect to workplace characteristics, starting with workplace size. The marginal peer effect may differ by workplace size either because the true fertility peer effect differs between workplaces with different size, or because co-workers interact differently within different sized workplaces. Note, however, that it is a priori not possible to determine the direction of the bias if for example the true peer group consists of a smaller subset of workers within each workplace (c.f. Manski, 1993). With these caveats in mind we divided the sample into 3 groups based on number of employees and estimated one separate peer regression for each sample. These estimates are reported in Table 7. As seen in column (1)–(3) the largest estimated peer effect 13–24 months since the birth to a co-workers child is found in the smallest workplaces (2–10 employees) (15%) and in the largest workplaces considered (30–49 employees) (9%). The smallest peer effect is found in medium sized workplaces with 10–29 employees (7%). This u-shaped marginal peer effect pattern with respect to workplace size is further reinforced when dividing the sample into smaller size brackets (2–9, 10–19, 20–29, 30–39, 40–49); the marginal peer effect remains strongest in the smallest and largest workplaces and lowest for the medium sized workplaces with 20-29 employees (not reported).

One potential explanation consistent with this interesting workplace size pattern is that while the precision of our network measure decreases with workplace size, the frequency of exposure to co-worker childbearing increases with workplace size. Hence, when the network size becomes larger than a certain threshold the cumulative effect of multiple births among co-workers dominates the decreasing network precision effect. This is further consistent with the treatment-response pattern we found in Table 2; more exposure implies stronger peer effects. To explore whether more exposure can explain the peer effect in the largest workplaces we re-estimated the model including indicator for multiple births among co-workers 1–12, 13–24 and 25–36 months ago. As suggested by Table A4 in Appendix, including dummies for more than one birth does not change the u-shaped pattern of the peer effect with respect to the workplace size. Thus it seems as if exposure to multiple births cannot explain why the peer effect is stronger in larger workplaces than in middle-sized establishments.

Alternatively it could be that, as observed in other studies, when network size increases the possibility to form friendships with individuals of the same type (e.g. gender, age, parity) and hence the positive within-type specific fertility peer effects could dominate any general adverse trend in the quality of our network measure (Weinberg, 2006). In planned future work we intend to further empirically test this and other potential explanations that are most likely underlying the observed pattern.

Table 7 finally investigates if the marginal peer effect differs with respect to workplace sector. If employees take into account the costs of maternity leave imposed upon the establishment when deciding about own childbearing we would potentially see a weaker peer influence in the for-profit sector. However, as columns (4) and (5) shows there are significant spill-over effects of co-workers' childbearing both in public and private establishments. The effects are not significantly different from each other.[24]

Table 7 Heterogeneous peer effects: workplace characteristics

	(1)	(2)	(3)	(4)	(5)
	2–9 Employees	10–29 Employees	30–49 Employees	Public sector	Private sector
Any co-worker had a child within:					
12 months	-0.00019	0.00012	0.00002	0.00011	0.00000
	(0.00017)	(0.00010)	(0.00014)	(0.00014)	(0.00009)
13–24 months	0.00078***	0.00035***	0.00046***	0.00047***	0.00049***
	(0.00017)	(0.00009)	(0.00015)	(0.00014)	(0.00009)
24–36 months	-0.00005	0.00013	0.00022	0.00022	0.00014
	(0.00017)	(0.00010)	(0.00015)	(0.00014)	(0.00009)
Duration dummies	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
*Year*Month dummies*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Own char.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Workpl. char.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Mean Y	0.00512	0.00524	0.00535	0.00602	0.00494
Observations	1,760,442	2,664,386	1,148,125	1,523,316	3,733,621

Note: see Table 1

[24] It should be noted that the direct costs for employers associated with maternity leave in Sweden is zero and thus the only costs upon the establishment is indirect costs related to e.g. temporary human capital loss and labor substitution. With that in mind, individuals' internalizing the establishment's costs seems also inconsistent with the findings of strongest peer effects in the smallest workplaces where potential costs can be expected to be highest due to lower flexibility and opportunity for labor substitution between employees.

6 Conclusions

This paper explores the influences that co-workers have on their female fellow workers' fertility decisions. Our results suggest that there are non-trivial peer effects on the choice of timing of childbearing. The main analysis shows that the effect of being exposed to co-workers' childbearing increases the probability of own childbearing during the following 13–24 months to the same extent as lowering childcare costs with USD 10,000 (Mörk et al., 2008), and as much as the decreases in childbearing after job displacement (del Bono et al., 2008).

The average effect however masks substantial differences in the size of the peer effect depending on the individual characteristics and in particular how these match the characteristics of the co-workers. Childbearing among co-workers who are of similar age, female co-workers and co-workers of the same parity are more influential. In contrast, childbearing of male co-workers and exposure to childbearing of co-workers with lower order births than the focal worker has no influence at all on the timing of childbearing. Apart from affecting the timing of childbearing, social influences may be able to affect completed fertility; both third-order births and women in the late stage of the fertility cycle (age 37–44) without previous children are affected. We also provide suggestive evidence that fertility peer effect spills-over across social multiple networks; from siblings of a co-worker via the co-worker to the focal worker with a lag of about 31–36 months.

Which types of mechanisms are most consistent with the observed peer effects? One explanation is that since the timing of childbearing has large effects on future earnings in particular for women (see e.g. Bertrand, Goldin and Katz, 2009), co-workers use the experience that other women in the same situation are confronted with after childbirth. Given our results, this explanation seem more likely than for example joint planning, economies of scale or learning about the nature of the pregnancy and childbearing experience itself. For example, consistent with a model of social learning we find that the peer effect for first time mothers is similar irrespective of the birth order of the co-worker's child, while for higher order births within-parity effects are strong but cross-parity effects are entirely absent. These effects together with the results that the childbearing of more similar co-workers in other aspects are more important suggest

that observational learning (for example about optimal timing of childbearing) is an important mechanism. Additionally we show that individuals are only influenced by co-workers who have the same or higher educational level. The anomalies in this same-type pattern are interpreted in line with theories suggesting that social status may be important in explaining individual behavior.

The results presented in this paper have several implications for both researchers and policy makers. There still exists a considerable controversy among demographers on whether public policies have the potential to affect fertility rates at all (see e.g. Hoem, 2008). Since our findings provide evidence of significant spill-over effects within networks and potentially also across different social networks, when evaluating the impact of policies aiming to affect fertility it is important to take into account that the control group could also be indirectly affected. If missing to account for such spill-over effects the impact of the policy may be underestimated.

One should on the other hand also bear in mind that the net effect of a given policy is a combination of social interaction effects and the direct impact of the policy. Therefore the strong heterogeneity of the social effects found in this study suggest that caution is warranted before assuming that the same policy when applied in another context will have the same impact on fertility. In a different context the net effect of the same policy may yield considerably different effect depending on network composition and the number/strength of the social ties within the targeted treatment group.

Additionally the peer effects we find in such an important decision as the timing of childbearing clearly point at the importance of social influences also for other types of career related decisions. Many observers have e.g. claimed that (the lack of) female role models in leading positions are important for women's own propensity to consider similar career paths. Our findings suggesting that female employees are influenced by the behavior of their female, but not by their male co-workers lend some indirect support for these claims. If career and family choices have the tendency to spread within networks (for instance through observational learning) then such peer effects may be very important for understanding observed differences between men's and women's individual career choices and the organization of work and family. To uncover to what extent gender specific peer effects at work lie behind other trends in labor supply related

decisions; the choices to e.g. opt out of the labor force (see Bertrand et al., 2009), change to part-time work or to take up managerial positions, are important and interesting questions for future research.

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Appendix

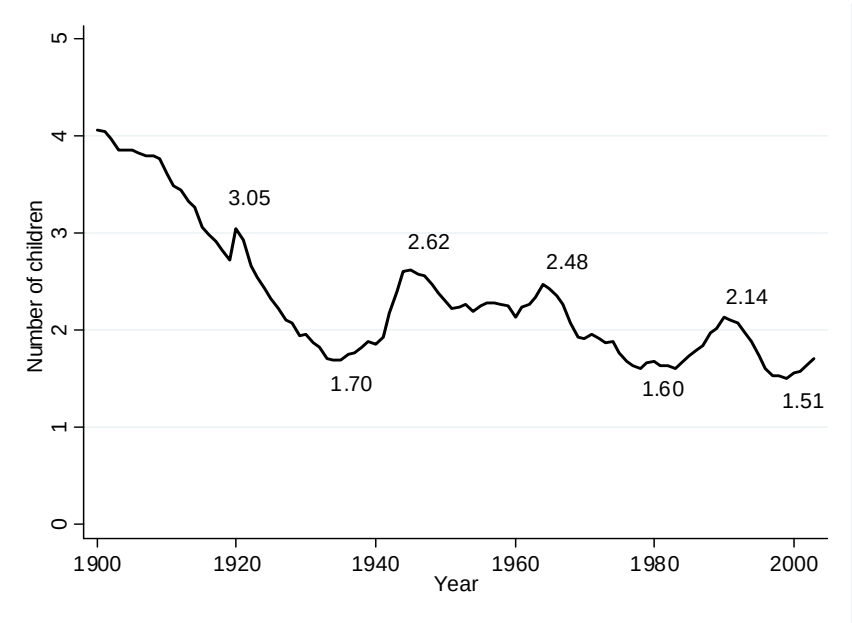

Figure A1 Total fertility rate, 1990-2003, *Source*: Socialstyrelsen (2005)

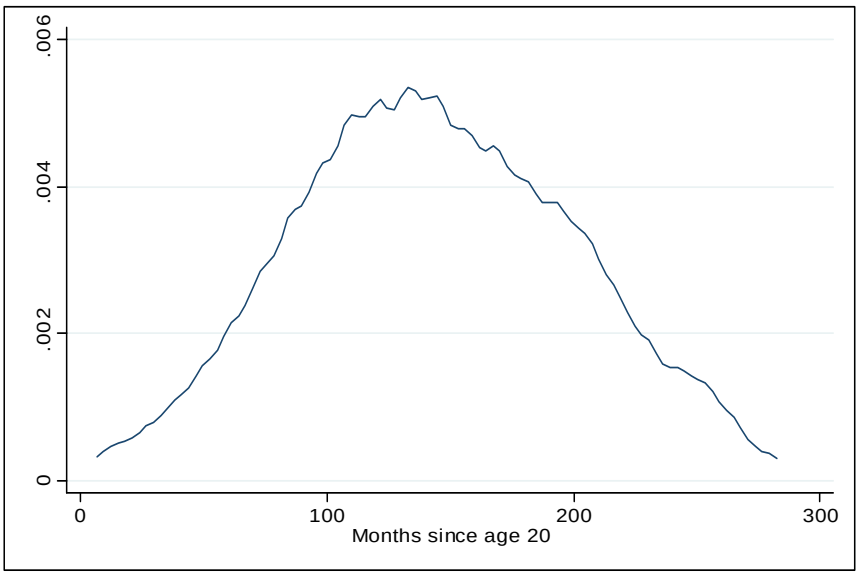

Figure A2 Baseline hazard of first order births

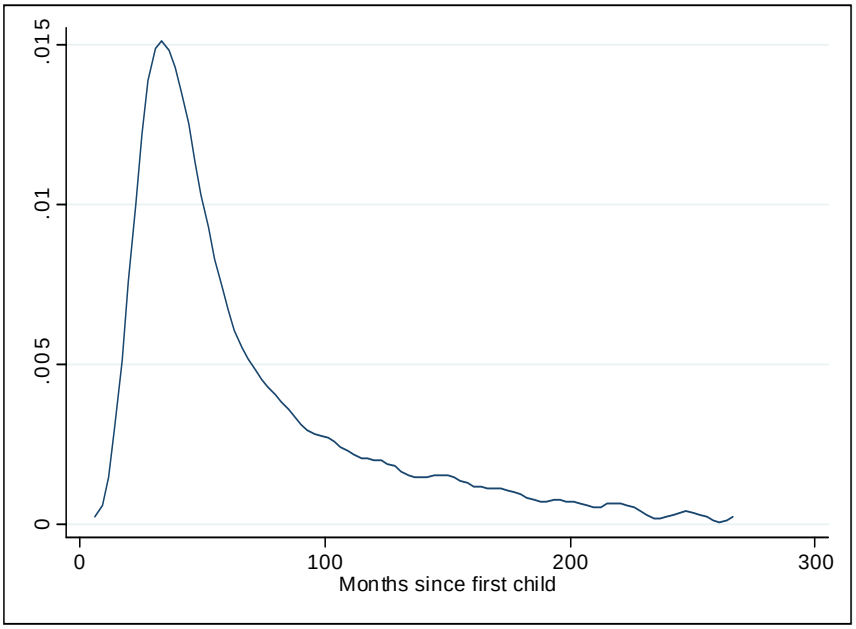

Figure A3 Baseline hazard of second order births

Table A 1 Descriptive statistics

	First birth		Second birth		Third birth	
	Mean	Sd	Mean	Sd	Mean	Sd
Had a child in current month	0.00523	0.07210	0.01115	0.10454	0.00201	0.04482
Age	27.6	5.4	32.5	5.1	35.3	4.3
College education	0.38	0.49	0.31	0.46	0.31	0.46
Number of children to co-workers	20.5	18.9	23.6	20.2	25.6	20.8
Share fertile co-workers	0.67	0.25	0.62	0.25	0.59	0.25
Share close in age co-workers	0.23	0.20	0.20	0.18	0.20	0.17
Share female co-workers	0.65	0.29	0.66	0.30	0.67	0.31
Establishment size	18.2	12.5	18.1	12.6	18.2	12.4
Public sector	0.27	0.45	0.34	0.47	0.40	0.49
Private sector	0.73	0.45	0.66	0.47	0.60	0.49
Observations	5,575,497		2,015,434		3,730,264	
Individuals	139,020		60,534		73,518	

Table A2 Baseline estimates of co-workers' fertility on the probability of first birth

	(1)	(2)	(3)
	All	All	All
Any co-worker had a child within:			
12 months	0.00001	0.00001	0.00004
	(0.00007)	(0.00007)	(0.00007)
13–24 months	0.00057***	0.00056***	0.00048***
	(0.00007)	(0.00007)	(0.00007)
24–36 months	0.00033***	0.00033***	0.00018**
	(0.00007)	(0.00007)	(0.00007)
Married		0.01184***	0.01177***
		(0.00016)	(0.00016)
College education		0.00034***	0.00030***
		(0.00008)	(0.00008)
No children to all co-workers			0.00005***
			(0.00000)
Share fertile co-workers			0.00017
			(0.00015)
Share close-in-age co-workers			0.00051***
			(0.00017)
Share female co-workers			0.00087***
			(0.00011)
Share married co-workers			0.00026
			(0.00016)
Share co-workers with college edu.			0.00034***
			(0.00012)
Duration dummies	Yes	Yes	Yes
Year*Month dummies	Yes	Yes	Yes
Own characteristics	-	Yes	Yes
Establishment characteristics	-	-	Yes
Mean Y	0.00523	0.00523	0.00523
Observations	5,575,497	5,575,497	5,573,397

Notes: *,** and *** denote statistical significance at 10/5/1 percent level respectively. Standard errors robust for serial correlation at the establishment level are shown in parenthesis. The level of analysis is the individual-month. In addition to the fixed effects indicated by the table regression (3) controls for establishment size dummies in intervals of ten employees.

Table A3 Descriptive statistics for true and placebo peer groups

SAMPLE:	Private firms with multiple workplaces		Job switchers		All	
	True: *Same firm same workplace*	***Placebo:*** *Same firm different workplace*	***True:*** *Current co-workers*	***Placebo:*** *Future co-workers*	***True:*** *All co-workers*	***Placebo:*** *Co-workers siblings*
Age	35.3	36.2	37.6	36.1	36.7	38.2
	(7.3)	(6.4)	(7.1)	(7.0)	(7.6)	(8.0)
Total # of children	18.5	1,178	20.3	19.9	20.5	19.05
	(16.4)	(2196)	(18.6)	(18.5)	(18.9)	(17.93)
Female	0.64	0.64	0.66	0.65	0.65	0.49
	(0.27)	(0.26)	(0.29)	(0.29)	(0.29)	(0.211)
Fertile	0.69	0.66	0.64	0.63	0.65	0.57
	(0.22)	(0.18)	(0.24)	(0.23)	(0.24)	(0.242)
High Edu.†	0.58	0.57	0.30	0.32	0.31	0.27
	(0.25)	(0.20)	(0.28)	(0.28)	(0.28)	(0.215)
Married	0.35	0.36	0.41	0.39	0.38	0.36
	(0.22)	(0.18)	(0.24)	(0.24)	(0.24)	(0.224)
This peer had a child within:						
12 months	0.39	0.81	0.34	0.39	0.36	0.36
	(0.49)	(0.40)	(0.47)	(0.49)	(0.479)	(0.480)
13-24 months	0.42	0.82	0.38	0.40	0.39	0.36
	(0.49)	(0.39)	(0.49)	(0.49)	(0.488)	(0.479)
25-36 months	0.42	0.82	0.37	0.38	0.37	0.34
	(0.49)	(0.38)	(0.48)	(0.49)	(0.484)	(0.472)
Observations	1,066,052	1,066,052	730,356	730,356	5,575,497	5,385,787

Notes:† High education is defined as having college education. The co-worker characteristics are calculated at the individual-year level.

Table A4 Frequency of exposure and workplace size

	(1)	(2)	(3)	(4)	(5)	(6)
Nr of Employees	2-9	10-29	30-49	2-9	10-29	30-49
12 months	-0.0002	0.0001	0.00002	-0.0003*	0.0001	-0.0001
	(0.0002)	(0.0001)	(0.0001)	(0.0002)	(0.0001)	(0.0002)
13–24 months	0.0008***	0.0004***	0.0005***	0.0009***	0.0002**	0.0004**
	(0.0002)	(0.0001)	(0.0002)	(0.0002)	(0.0001)	(0.0002)
24–36 months	-0.0001	0.0001	0.0002	-0.00005	0.0002	0.0001
	(0.0002)	(0.0001)	(0.0002)	(0.0002)	(0.0001)	(0.0002)
Duration dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year*month dummies	Yes	Yes	Yes	Yes	Yes	Yes
Own char.	Yes	Yes	Yes	Yes	Yes	Yes
Est. char.	Yes	Yes	Yes	Yes	Yes	Yes
More than one child	-	-	-	Yes	Yes	Yes
# Obs.	1,760,442	2,664,386	1,148,125	1,760,442	2,664,386	1,148,125

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