Businesses, Buddies and Babies: Fertility and Social Interactions at Work

PRELIMINARY

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Abstract

This paper assesses the importance of peer effects on fertility timing using population-wide matched employer-employee panel data. We provide evidence on for whom, when and why co-workers' fertility decisions matter. A wide range of specification checks supports a causal interpretation of the estimated effects. We develop a stylized dynamic model of fertility timing decisions under uncertainty that provides us with predictions that allows us to discriminate between alternative underlying mechanisms consistent with the baseline results. Network externalities seem to be the primary mechanism through which peer effects may exacerbate fluctuations in fertility rates in our context.

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1 Introduction

When the payoffs from alternative actions are uncertain, the decisions made and experiences of peers may provide valuable guidance for own decisions. Learning from and mimicking peers actions is likely to be particularly important when own experiences are limited, information from external information sources are of limited relevance, decisions are irreversible and erroneous choices could be costly.

In many ways, the timing of childbearing is an example of a choice that fulfils these conditions. When deciding about when to have a child, women face a clear trade-off. Delayed motherhood is associated with higher risks of childlessness and adverse health outcomes for mothers and children (Mincer and Ofek, 1982; Royer, 2004; Miller, *forthcoming*). At the same time, childbearing is associated with costly career interruptions for women and postponing childbearing may have a large effect on lifetime earnings (Mincer and Polacheck, 1974; Albrecht, Edin, Sundström, and Vroman, 1999; Bertrand, Goldin and Katz, 2010). In addition, uncertainty about the net benefits of childbearing at a particular point in time may generate an option value of waiting with childbearing (Dixit and Pindyck, 1994; Ranjan, 1999; Iyer and Velu, 2006). New information brought about by peers' childbearing experiences can reduce such uncertainty and also lead to an increased number of births. Finally, social norms, conformity concerns and peer pressure within social networks has been suggested to lead to clustering of a wide range of actions, potentially also childbearing decisions.

A clear picture of the relevance of and the underlying mechanisms behind peer effects in fertility decisions is important from a policy perspective. For example, consider the sharp cohort size fluctuations observed in many countries during the last 60-70 years.[1] Fluctuating fertility rate has, and will put further strain on the education industry, social security, pension systems and has been linked to labor market prospects, inequality and productivity. [2] At the same time it has been suggested that strong enough social multipliers (Glaeser, Sacerdote and Scheinkman, 2003) can generate or at least exacerbate fluctuations in aggregate behavior. Yet little is known about the rele-

[1] Sweden displays large variation in fertility rates during the 20th century (see figure A1 in Appendix A, and Andersson (1996) and Hoem (1990) for further evidence). The total fertility rate is positively correlated with the business cycle, which has been suggested to be due to the tight link between the parental leave benefits and permanent employment (c.f. Björklund, 2006).

[2] See (Freeman (1979); Welch (1979); Easterlin (1975); Katz and Murphy (1992); Murphy and Welch (1992); Kohler (1997, 2001); Durlauf and Walker (1998); Higgins and Williamson (2002); Feyrer (*forthcoming*), In addition, prospects of accurately predicting the needs for daycare, schooling, and housing may be hampered by strong fluctuations in cohort sizes.

vance of peer influence on childbearing decisions, especially among adults, and even less is certain about the mechanisms behind it.[3]

In this paper we set out to fill these gaps by examining fertility peer effects in the workplace using panel data on monthly fertility decisions among 150,000 Swedish women and all of their co-workers over a period of eight years. Co-workers may constitute a particularly relevant peer group when it concerns fertility-timing decisions.[4] First, information about the job specific consequences of childbearing may be difficult to obtain from other social networks or sources. Second, the often high degree of similarity between co-workers and the day-to-day interactions suggest that direct social influences could be important within this peer group. Finally, unlike many other types of actions, childbearing decisions are easily observable enabling workers to learn from the experiences of their co-workers through observational learning about fertility choices and its consequences.

This is the first study assessing the influence of co-workers on fertility decisions, and few previous studies have used micro data to examine the role of social influences in fertility decisions for any peer group.[5] Unlike most previous studies focusing on social interactions and fertility decisions, we focus on timing of births.[6] This is partly because we believe that the timing decision is the key margin where peer influences is likely to matter most in our context, but also because the nature of timing of childbearing aid the identification of the effect of interest.

Two central econometric issues arise when attempting to identify the influence of peers' behavior on individual behavior (c.f. Manski, 1993; Moffitt, 2001). First, as peers may simultaneously influence each other, it is notoriously difficult to distinguish whether it is the individual that affects the group or the group that affects the individual. Second, because the place of work is a choice variable, women may sort into workplaces based on unobserved characteristics related to their fertility decisions. For example, family friendliness of jobs is a potentially significant determinant of many women's employment decisions (Herr and Wolfram, 2009), and friends and relatives are important channels for job search (Granovetter, 1995, Montgomery,

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[3] Kohler (2000)

[4]Keim, Klärner and Bernadi, (2009) asked people to rank the importance of differing peer groups in terms of their influence on the subjects childbearing and family formation decisions. 35% percent stated that co-workers had an *important* or *very important* influence on their fertility intentions and family formation plans (compared to e.g. 39% for cousins and 12% for neighbors). The order of stated importance is partners, children, three closest friends, parents, siblings, parents-in-law, other relatives, cousins, colleagues, neighbors, and acquaintances. Note that these figures only reflect the part of the influence that the respondents are aware of themselves and not subtler influences that may influence behavior.

[5] Those studies that have used micro data either looks at interactions within developing countries (Bloom et. al., 2008, Manski and Mayshar, 2003; Munshi och Myaux, 2006), among very young women (Crane 1991; Case and Katz, 1991) or within families (Kuziemko, 2006).

[6] Although our focus is on timing of childbearing, we also provide some suggestive evidence for impacts on completed fertility.

1991; Ioannides and Loury, 2004). Similarly, unobserved shocks that independently affect the timing of co-workers' fertility decisions could also lead to correlations in the timing of childbearing. For example, correlations in co-workers' childbearing could simply proxy for changes in firm policy, an increased risk of mass lay-offs, or other changes in conditions that influence childbearing of workers independently, rather than through true peer effects. For these reasons it is crucial to make sure that the perceived peer effect is not simply reflecting a spurious correlation in co-workers behavior induced by endogenous sorting of workers sharing similar preferences or other unobserved determinants of childbearing across firms.

The detailed and high frequency longitudinal data and the focus on the timing of childbearing allow us to address these issues. First, the simultaneity problem is mitigated by focusing on the influence of co-workers past childbearing. While using lagged behavior of a peer group to identify the effects of social influences breaks the simultaneity in outcomes, it 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 childbirths) allows us to relax the assumption of non-forward looking agents. It is arguably very difficult, both for the individual and the co-workers, to exactly predict when conception takes place. This key notion together with the possibility to consider a detailed lag-structure also allows us to form empirical predictions about the dynamic pattern that the estimated peer effects would follow if these were driven by correlated shocks and/or endogenous sorting.

We find that the estimated effect of a co-worker's recent childbearing on own childbearing follows a distinct dynamic pattern. During the first 12 months following the birth of a co-workers child the probability of having a child is largely unaffected, only to sharply increase after 13–18 months (9% increase) and then slowly decline. This dynamic pattern, which speaks against the standard sorting and correlated shocks hypotheses, is remarkably robust across specifications and subgroups and controls for non-parametric monthly duration dependence, time-effects, workplace size, regional unemployment rate, industry, and several important individual and co-worker characteristics.

It is still, however, possible that the correlations in fertility decisions simply reflect changes in unobserved circumstances affecting childbearing choices of all workers in a workplace. While we cannot completely rule out this possibility, we do provide several additional important pieces of evidence that strengthen the case for a causal interpretation of the results and our conclusions. We first test whether the peer effects are related to the similarity of the co-worker and the focal worker. In line with the literature on the formation of social ties we find stronger peer effects between "same-type" co-workers than "different-type" co-workers. Much more weight is put on the fertility decisions made by other female co-workers and co-workers who are close-in-age. However, we also find important asymmetries in this same-type pattern. For example, consistent with models giving weight to social status, employees are only affected by co-workers who have the same or higher, but not lower, educational attainments. We also find that while the number of previous children of the childbearing co-worker does not matter for first-time mothers, mothers with previous childbearing experiences are only influenced by co-workers having the same number of previous children. This finding is interesting since it says something about how social influences spread in the workplace, but it also speaks against the alternative hypothesis of common workplace specific shocks since these must be "type" specific in order to explain the observed effects.

Finally, we consider three falsification exercises where we test if the worker 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 in a different 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 time-varying unobserved shocks. 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 our baseline effect is spurious. We find no evidence of any similar influences from these placebo peers.

To get a deeper understanding about the underlying mechanism at work, we then develop a simple dynamic model of fertility timing decisions under uncertainty based on real options theory. In our model clustering in childbearing could occur for several reasons that we boil down to two broad mechanisms. First, due to the existence of irreversibilities associated with childbearing decision and the option of postponing childbearing for a later time, it may be optimal to postpone childbearing during periods of increased uncertainty. Co-worker's childbearing experience provide information which reduces the uncertainty about the (workplace specific) effects of childbearing which in turn increase the fertility rate among her peers.[7] Second, clustering may arise because payoffs of childbearing could directly depend on the childbearing of others (Schelling, 1960; Katz and Shapiro 1985, Arthur 1989; Becker, 1991). Such network externalities may for example stem the sharp changes in time-use after childbearing, because workers want to con-

[7]A frequently suggested example of the importance of social learning concerns the role of dissemination of information about the use of modern contraceptives (c.f. Behrman et al 2001; Munshi and Myaux, 2006). In our case information about contraceptives is likely of limited relevance, but individuals may still benefit from social or observational learning for example about the pros and cons of childbearing at a particular time (Montgomery and Casterline, 1996).

form to norms in the workplace[8], because they value joint parental leave[9] (Hamermesh, 2002), because of economies of scale (e.g. from coordinated childcare and the sharing of material expenses), new career opportunities, or simply because people do not want to be left out from conversation among peers centered around children.

Two key predictions derived from the model allow us to examine the relative importance of the information and network externalities. First, we show that the impact of increased uncertainty on the magnitude of the peer effect depends on the type of externality. Second, as women approach menopause the value of the option to wait decreases. If information (network) externalities matter most, the estimated peer effect should then decrease (increase) in magnitude. Using two proxies for workplace specific uncertainty we find that peer influences on individual childbearing are stronger when uncertainty is low then when uncertainty is high. We also find that the peer effect is increasing in magnitude as women age. Taken together, these two pieces of evidence provide support for network externalities being more important than information externalities in this context.

The distinction between these two social mechanisms is important if attempting to reduce costly fluctuations in fertility rates. If individuals only care about the decisions of others because they have something to learn about the net benefits of childbearing at a particular point in time, reduced uncertainty about the net benefits may reduce fluctuations in fertility rates. On the contrary if the network externality effects instead dominate, reduced uncertainty about net benefits may result in as strong or even stronger social multipliers.

That peers childbearing decisions matter for fertility timing decisions does not mean that incentives generated by public policy are irrelevant as explanations for fluctuations in fertility rates (see e.g. Hoem 1993; Björklund 2006). Nor is it likely that co-workers are the only peer group that matter for fertility decisions. Instead, we argue that the evidence provided in this paper suggest that effects of public polices aiming at affecting natality interact with social incentives through network externalities within social networks. This finding could help us better understand and evaluate the effects of policies intending to influences aggregate fertility rates.

The rest of the paper is structured as follows. Section 2 lays out the empirical strategy, section 3 describes the data, Section 4 presents the baseline results and robustness checks. In section 5 we develop the model we use to

[8] In the only study we know about where subjects were directly asked about the influence of peers in fertility decisions the authors concludes that with regards to e.g. co-workers "[…] one is either somewhat on the line and conforming, or one is deviant. Considerations about the timing of childbirth and the perception of […] own readiness often include this kind of evaluation" (Keim, Klärner and Bernadi, 2009; p.12).

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

attain the predictions we subsequently take to the data in order to learn more about the mechanism behind the observed peer effect. Section 6 summarizes and concludes.

2 Is there clustering in childbearing?

We begin by describing the baseline estimation method we use to identify the effect of the timing co-workers childbearing decisions on the timing of individual childbearing decision. We then discuss the potential empirical pitfalls and the ways we attempt to address the general concerns common to empirical studies attempting to identify peer effects using observational data.

2.1 Empirical specification

Timing of fertility is an intrinsically dynamic decision. We model the individual fertility decision as a function of co-workers past childbearing. The baseline empirical strategy follows the spirit of Kuziemko (2006) with some important modifications.[10] We estimate 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).[11] Our baseline specification is:

[10] Kuziemko (2006) estimate linear probability models and include individual fixed effects to identify the impact of siblings' childbearing on individual childbearing. She finds that the probability of having a child within the first 24 months after the birth of a sibling's child increases by 17% on average. We *do not* include individual fixed effects since it is unlikely to help identify the effects of interest. In a hazard model framework the closest equivalent of controlling for individual fixed effects is to exploit variation in timing of treatment across multiple spells allow for individual specific baseline hazards. This approach when we expect that the baseline hazard follows a reasonably similar pattern across spells, in which case controlling for the common baseline hazard across spells captures important unobserved determinants of the timing of exit. While this approach may be reasonable when it concerns e.g. unemployment or sickness absence spells, as clearly displayed in Figures A2 and A3, the baseline hazards of having the first and the second child are very different. Hence exploiting variation in timing of co-workers' childbearing across first and second birth spells is unlikely to provide a venue for identifying the impact of peer's childbearing decisions. Instead we rely on the falsification exercise discussed further below to try to rule out that the estimated effects not imply are caused by spurious correlations generated by unobserved factors shared by the employees.

[11] We have also re-estimated the model using a Maximum Likelihood estimator. This provided similar results and are available upon request

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

where the dependent variable Y_{ijtc} indicates whether employee *i* in workplace *j* had a child in calendar month *c* at duration month *t*. α_t is month of duration dummies that non-parametrically control for the fact that the baseline hazard of childbearing varies dramatically over the life cycle (as clearly illustrated in Figure A2 and A3). The variables "Any co-worker had a child within 12, 13–24 or 25–36 months" are indicators for whether a co-worker had a child within 12, 13-24 and finally 25-36 months prior to month *c*.[12] X_{ijtc} is a vector of individual background characteristics (marriage status and education), C_{ijtc} is a vector of co-worker and workplace background characteristics such as the previous number of children among the co-workers, age distribution, gender and educational attainments, and dummies controlling for establishment size in 10 worker intervals. In some specifications we also control for own tenure, sector (public/private), industry affiliation, regional location and the age of the establishment. η_c is calendar time (year $\times$ month) dummies that capture common macro shocks that influence fertility decisions and finally ε_{ijtc} is the error term. The reported standard errors are adjusted for common errors at the workplace level.

The main parameters of interest in equation (1) is β_1, β_2, and β_3. The estimates of these parameters intend to capture the dynamic impact of co-workers' recent fertility decisions on the likelihood of childbearing in a specific month. Our main analysis focus on how co-workers' childbearing affects the timing of first births since the variation in timing is largest for these births. We also report estimates for higher order births. We estimate equation (1) for women under risk of having her first, second and third child separately using OLS.[13] For first births duration dependence is controlled 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

[12] 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. It is important to note that peer effects may arise not only from if any co-worker recently had a child, but also from the share of co-workers who had a child. Empirically, since we focus on small and medium workplaces, this is not going to make much of a difference. In the robustness checks we do however provide evidence on this from regressions where we interact the baseline exposure variables with a dummy indicating if more than one co-worker gave birth to a child within the same time period.

[13] During our observation period higher order births are relatively uncommon.

the previous birth. Note that the combination of the duration dummies (months since age 20) and calendar time effects also accounts for general cohort effects.

2.2 Threats to identification

The parameters of interest in equation (1) and (2) are identified under the assumption that the timing of co-workers' childbearing is uncorrelated with omitted variables affecting individual childbearing, after controlling for age (in months) effects, calendar time effects and the other time-varying individual and co-worker characteristics.

When could this assumption be violated? Changes in labor market conditions could change the individuals' and the co-workers' fertility decisions simultaneously. Much of this variation in labor market conditions will be controlled for by the year×month dummies and the yearly regional unemployment rate. In some specifications we also include year-month×region×industry effects. However, firm and/or workplace level common shocks, such as increased risk of lay-offs, policy changes etc., that change the probability of childbearing for all co-workers could also violate our key identifying assumption. Additionally, if workers sort into workplaces based on unobserved characteristics e.g. childbearing preferences, we may find a spurious correlation between childbearing of co-workers' and the focal worker. Even though we are controlling for many important co-worker characteristics related to timing of childbearing (average number of children, share in fertile ages, share close-in-age (±4 years), share of co-workers with college education, share females, share married), individuals may still end up in the same workplace and have children at approximately the same time for unobserved reasons, despite that they are not influenced by each other directly.[14]

To get a first sense of the potential severity of these basic and general concerns we exploit the difficulty of foreseeing exactly when conception takes place and the longitudinal data to form predictions about how the estimates of β_1, β_2, and β_3 should behave if omitted factors are important. To

[14] 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 simply driven by endogenous sorting across workplaces.

see this clearly, suppose that two co-workers independently start trying to conceive at the same time (e.g. due to a change in firm policy). 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 individuals who start trying to conceive at the same time 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. $\beta_1 > \beta_2 > \beta_3$).

If instead the estimates simply reflect endogenous sorting of workers with similar fertility timing preferences across workplaces then we 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 influence) 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 $\beta_1 = \beta_2 = \beta_3$. In the following sections we will see that our estimates do not match either of these predictions.[15]

Our second line of defense builds on evidence from a large sociological literature documenting that individuals are much more likely to form social ties with "same type" peers than "other-type" peers within social networks.[16] To investigate whether this is true in our context we modify our model to allow the response to co-workers' childbearing to vary with respect to the similarity between the childbearing co-worker and the focal worker. Specifically we estimate

$$\begin{aligned} Y_{ijtc} = \Omega &+ \gamma_1(\text{Any co-worker had a child within 12 months} \times \text{TYPE})_{ijtc} \\ &+ \gamma_2(\text{Any co-worker had a child within 13-24 months} \times \text{TYPE})_{ijtc} \\ &+ \gamma_3(\text{Any co-worker had a child within 25-36 months} \times \text{TYPE})_{ijtc} \end{aligned} \qquad (2)$$

Where Ω corresponds to the right hand side of equation (1) and TYPE is an indicator variable equal to 1 if any of the co-worker who had a child in the previous periods are male/female, close-in-age (±4 years), have similar educational attainment (college/no college), or have the same number of previous children as the focal worker, and zero otherwise.

Finally, we assess the plausibility of the identifying assumptions using what we call "placebo peers". We re-estimate model (1), but instead of focusing on the impact of the true co-workers, we check whether the childbear-

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16 For a evidence of the relevance of homophily in social networks c.f. Currarini, Jackson and Pin (2009) and McPherson, Smith-Lovin and Cook (2001)

ing behavior in three alternative groups of individuals also affect the fertility decisions of the focal worker. The placebo co-workers we consider are:

i 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.

ii FUTURE CO-WORKERS: This placebo-peer group consists of the future co-workers to the female employees in our sample that switch workplace during the eight-year observation window.[17]

iii SIBLINGS OF 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.

These three placebo peer groups are likely to share many of the unobserved characteristics and experience the same type of unobserved shocks as the focal worker and the true co-workers. However, *a priori* we do not expect to find a correlation between childbearing in either of these placebo peer groups and the focal worker *unless* i) the baseline peer effect simply reflects a spurious correlation induced by unobserved factors that affect the timing of childbearing, or ii) they are directly influencing the focal worker. However, note that if childbearing really is "contagious" then it is conceivable that the childbearing of siblings could influence 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 do not affect each other at all but just share unobserved determinants of timing of childbearing 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 results.

Note that although none of the placebo peer groups are perfect in isolation, they are imperfect in different ways. Hence, jointly they provide a fairly strong test against the alternative spurious correlation hypothesis. In Table A3 we provide descriptive statistics for the main sample as well as for the three placebo peer groups.[18]

[17] 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.

[18] The 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

3 Data

The data we use come from the IFAU-database that contains various administrative registers covering the entire Swedish population aged 16–65. In addition to detailed individual background characteristics (LOUISE) the data contain firm and workplace identifiers (RAMS). From the "multi-generation" register we add data on the full history of births as well as the month of birth of each child. This allows us to construct our measure of co-worker fertility and our binary outcome variable; whether the focal worker gave birth to a child in a given month or not.

We restrict the analysis to female workers between age 20 and 44 employed in a workplace with less than 50 employees.[19] We focus on women first of all because their fertility cycle is well-defined, but also because childbearing among women is associated with significant career interruptions. This restriction does not apply to the co-workers. That is, the analysis looks at the impact of both male and female co-workers' fertility on female workers fertility. The workplace size restriction is important since it allows us to focus on a well-defined peer group where interactions are likely to occur on a day-to-day basis.

We select a 50 percent random sample of women employed in 2004 and follow these eight years back in time (1997–2004). Hence, women are defined to be under risk of childbearing 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.[20] To avoid including individuals who are only loosely connected to the workplace we retain only workers with

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-workers' siblings' characteristics and we also include nine dummies for firm size where relevant. Note that since the three placebo-peer groups are fairly balanced on observed characteristics it is reasonable to expect that they are similar in terms of unobserved characteristics too.

[19] 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).

[20] 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 employment. However, note that almost all women in Sweden remain in employment after birth and hence attrition is therefore a minor concern.

yearly labor earnings above the 10th percentile.[21] Relatively few workers hold multiple jobs. For those who do, for simplicity, we assume that the workplace giving the primary source of earnings also is the main arena for social interaction.

Because time until pregnancy 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 women without previous children we define duration as the number of months from age 20 and up to their first birth (or censoring), and for mothers with one child (two children) duration is defined as the number of months from their previous child birth up to the second (third) or until they are censored. Individuals 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, sex composition, the share of co-workers with college education, workplace size, number of children of the co-workers, region of work and the sector (public/private) and 3-digit industry of employment.

Descriptive statistics for first, second and third order spells are reported in Appendix A, Table A1. In our sample, mothers to first-born children are, on average, 27.6 years old and employed by workplaces with 18 employees. The mean probability of having a child in a specific month is 0.005. The mean probability of having a second child is more than twice as high (0.011) reflecting that those who already have a child are much more likely to give birth to another child. The monthly probability of having a third child is only 0.002. These patterns reflect the two-child norm in Sweden.

As shown in Figure A2 in Appendix A 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 expected since our sample is restricted 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 Results

4.1 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 and calendar month fixed effects. The first, second and third row report the esti-

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

mates 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 are robust and consistent across specifications. The estimates of β_1 are small and not significantly different from zero but still precisely estimated. In contrast the estimates of β_2 and β_3 indicate a positive (and declining) association between the focal workers childbearing and the past childbearing of her co-workers. The pattern of the parameters does not change significantly when adding controls for individual marital status and college education in column (2) and co-worker and workplace controls in column (3) (see Table A2 in Appendix A for all controls).

Together the estimates suggest that the co-workers' fertility decisions primarily increase fertility with a lag of about one year after the birth of a co-worker's child. Evaluated at the mean probability of childbearing the full specified model suggests that individuals are on average 9% (0.00047/0.00523) more likely to have their first child 13–24 months after the birth of a co-worker's child. To put the estimates into perspective consider first that for example Del Bono, Weber and Winter-Ebmer (*forthcoming*) find that women are about 10% less likely to have a child in the first couple of years after losing their job.[22] The 12-24 month effect is also comparable to increasing the focal workers age by one (1) year in the age interval 20 through 30 or equivalently decreasing ones age between ages 30 through 40.

The dynamic and consistent pattern across specifications and (as we show below) sub-samples suggests that common unobserved shocks is not driving the results. 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. We do not find a significant increase in childbearing until 12–month after a birth of a co-workers' child. Similarly, as motivated above the pattern gives a first indication that the estimated effects are not consistent with a situation where endogenous sorting of workers is causing a spurious correlation in the timing of pregnancy.

We also explore the heterogeneity of the results with respect education attainments and marriage status and also report results for second and third order births. The estimates for these groups are very similarity to the main results. For brevity the full results are reported in Appendix B.

[22] Interestingly the magnitude of the social effect is 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, (2006); Ichino and Maggi (2000) and Hesselius, Johansson and Nilsson (2009) all find co-worker peer effects which are in the vicinity of our estimates, but for very different outcomes.

Table 1 Baseline estimates of co-workers' fertility on the probability of first birth.

	(1)	(2)	(3)
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)
Total nr of 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
Calendar time 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 and 1 percent level respectively. Standard errors robust for clustering at the workplace level are shown in parentheses. The level of analysis is the individual-month. Calendar time is defined at the Year×Month level. Individual characteristics include civil status and a dummy for college education. Workplace characteristics include establishment size dummies in intervals of ten employees, the regional (county/year) unemployment rate where the workplace is located, the number of previous children in the workplace and the share of fertile, close-in-age, female, married and college educated co-workers.

4.2 Robustness checks

As a first specification check in column (1) of Table 2 we replaced the three 12-month indicators of interest with six 6-months interval dummies. These estimates suggests that the baseline specification indeed seems to do a good job in modeling the dynamic impact of co-workers' childbearing on timing of fertility. The main impact shows up after 13–18 months and then declines until it turns insignificant after 31–36 months. Again, the absence of effects within the first 6 months strengthens the conclusion that omitted factors are not driving the estimated social effect. To further control for transitory unobserved shocks across regions (21 regions), 3-digit industries, and calendar time we add year×month×region×industry specific effects to in column (2). That is we now compare fertility decisions among employees in workplaces in the same 3-digit industry/region/calendar month with and without co-workers who recently had a child.[23] This does not change the results.

Next we assess if increasing the dose of exposure matters; that is if the number of children born within each period matters. 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 (3) provide a clear dose-response pattern of being exposed to the 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 simply driven by exposure to many births. We therefore stick to the more parsimonious specification for the remainder of the analysis. We also assess the relationship between workplace size and the magnitude of the peer effect. The largest effects are found in the smallest workplaces and then decreases (although not necessarily monotonically with workplace size). The results from this exercise can be found in Appendix B.

As common shocks does not seem to be able to explain the estimated peer effect 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 planning to have children systemati-

[23]The 3-digit industry classification is fairly detailed. As an example for the education sector this splits the sample into primary school, secondary school, higher education, and vocational school/adult education. In the manufacturing industry it distinguishes for example between production of rubber or plastic goods. In the hotel and restaurant business it distinguishes between workplace in the hotel, restaurant, camping, bar, and canteens/catering businesses. See http://www.foretagsregistret.scb.se/sni/040115snisorteradeng.pdf for full details.

cally 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 women with longer tenure, suggesting that sorting into establishments just before planning a pregnancy is not driving our results.

Table 2 Robustness checks.

	(1)	(2)	(3)	(4)	(5)
Sample:	Baseline	Baseline	Baseline	< 5 years of tenure	≥ 5 years of tenure
Any co-worker had a child within:					
1–6 months	0.00010	0.00007			
	(0.00008)	(0.00008)			
7–12 months	0.00012	0.00017**			
	(0.00008)	(0.00008)			
13–18 months	0.00048***	0.00050***			
	(0.00008)	(0.00008)			
19–24 months	0.00028***	0.00028***			
	(0.00008)	(0.00008)			
25–30 months	0.00016**	0.00017**			
	(0.00008)	(0.00008)			
31–36 months	0.00005	0.00005			
	(0.00008)	(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:					
12 months × 1(>1 birth)			0.00024**		
			(0.00012)		
13–24 months ×1(>1 birth)			0.00030***		
			(0.0001)		
25–36 months ×1(>1 birth)			0.00001		
			(0.00011)		
Duration dummies	Yes	Yes	Yes	Yes	Yes
Calendar time dummies	Yes	Yes	Yes	Yes	Yes
Calendar time × Industry ×Region	No	Yes	No	No	No
Individual char.	Yes	Yes	Yes	Yes	Yes
Workplace char.	Yes	Yes	Yes	Yes	Yes
Mean Y	0.00523	0.00523	0.00523	0.00523	0.00523
Observations	5,573,397	5,573,397	5,573,397	4,559,220	1,014,177

Notes: *,** and *** denote statistical significance at 10,5 and 1 percent level respectively. Standard errors robust for clustering at the workplace level are shown in parentheses. The level of analysis is the individual-month. Calendar time is defined at the Year×Month level. Individual characteristics include civil status and a dummy for college education. Workplace characteristics include establishment size dummies in intervals of ten employees, the regional (county/year) unemployment rate where the workplace is located, the number of previous children in the workplace and the share of fertile, close-in-age, female, married and college educated co-workers. The specification in column (2) additionally controls for Year×Month×Industry (3-digit)×County fixed effects and an indicator for public sector. 1(>1 birth) is a dummy variable equal to 1 if at least two co-workers gave birth to a child during the previous ≤12, 13-24 and 25-36 months.

4.3 Who is influencing whom?

Next we investigate whether the strength of the peer effect differs depending on the characteristics of the women and how these match the characteristics of the co-workers. The estimates are obtained using model (2) described in section 3.1. Overall, the full set of estimates from this specification follows the familiar pattern of the baseline results and is reported in Table B2 in Appendix B. Figure 2 summarizes the key findings by reporting only the estimated impact 13-24 months after the co-worker child is born (i.e. γ_2). The estimates in Figure 2 provide evidence that similarity, social status, and prior experiences all play distinct roles in the social transmission of fertility decisions in social networks. [24]

First, for comparison, the main effect (a 9% increase) for the full sample is repeated in the first row. However, as row 2 and 3 reveals the entire baseline peer effect seem to be driven by the influence of female co-workers. If a female co-worker recently gave birth the chance of giving birth to a child 13-24 months later increase by 13.5%, while childbearing among male co-workers' partners does not influence childbearing of the focal worker at all. Closer connections among female co-workers and/or gender-specific learning are both possible explanations for this result. We always control for the share of same type co-workers in the workplace and hence female co-workers' stronger influence is not simply explained by gender-segregated workplaces. Hence, our estimates reflect the additional impact women have on each other given the potential number of female-female ties.

The influences of co-workers who are close-in-age (±4 years) are substantially stronger (22%, row 5) than the impact of those of other ages (3%, row 4), suggesting that the experiences of co-workers in a similar stage of the life-cycle are more important.[25]

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[24] In terms of individual characteristics, we have also investigates whether the response to peers childbearing choices differs w.r.t civil status and education level. 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. Evaluated at the mean probability of having a child we find no remarkable difference in the reaction to peers based on own marriage. We also found that the peer influence for women with college education is stronger than for those without college education. This results squares poorly with that the peer influence should be due to economies of scale associated with coordinated childbearing.

[25] Remember that we always control for the stage of the fertility cycle using monthly duration dummies.

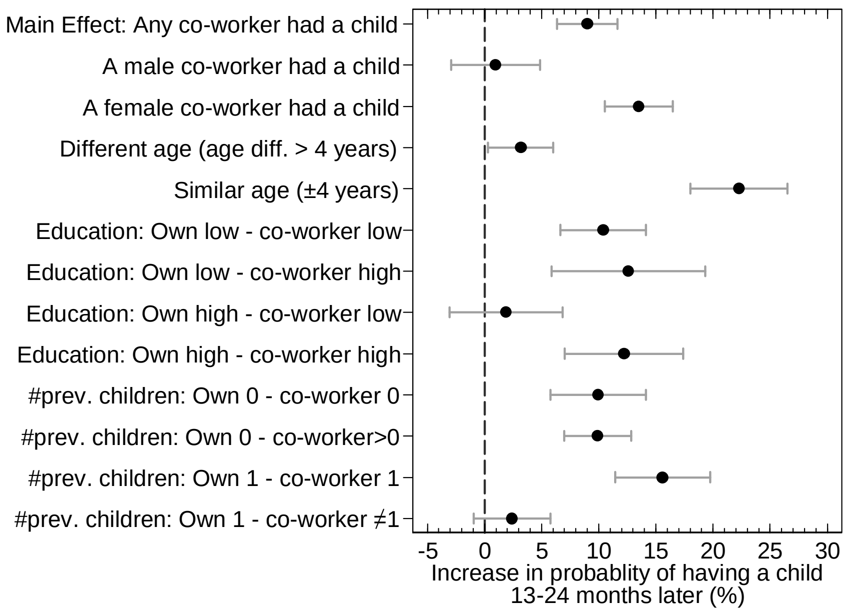

Figure 2 Increase in probability that an employee give birth 13-24 months after a same-type/different-type co-worker did. Point estimates of γ_2 are evaluated at the mean monthly childbirth probability along with 95% confidence intervals reported. For full results, see Appendix B Table B2.

College educated women seem to be affected by other college educated co-workers (12%, row 9) but not by those with lower education (row 8). On the contrary women without college education is similarly affected by both college educated co-workers and co-workers without college education (12% and 10%, rows 6 and 7). These asymmetric patterns suggest that social status matters (Akerlof and Kranton, 2000) and they are in line with studies showing that behavior among higher but not lower ranking peers influences decisions in laboratory experiments (Ball et al. 2001; Kumru and Vesterlund, 2010).[26]

Similarly, mothers with previous childbearing experiences are 16% more likely to give birth 13-24 months after a co-worker *with* previous children (row 12), whereas the influence from co-workers without previous children is negligible (row 13). Women without previous children are on the other hand similarly affected by both same order *and* higher order births (10%) (row 10 & 11). One explanation consistent with this asymmetric pattern is that since higher parity women already have personal experiences of child-

[26]This asymmetric pattern is unlikely to occur simply because individuals interact mainly with co-workers who have the same educational level. If so we would have expected both high and low educated women to primarily be influenced by their same type peers.

bearing, the decisions and experiences of first time mothers do not generate any new information of value to the focal worker.[27] Alternatively co-workers with children may have had the chance to form stronger ties because of a similar family situation, and hence have stronger effects on each others decisions.

In addition, these patterns cannot easily be explained by alternative hypothesis, such for example workers "taking turns" of childbearing, in order to ensure an uninterrupted conduct of business. Assuming that leave-related costs are the similar irrespectively of whether women are on leave with their first or second child, it is difficult to see why influences from first to second time mothers are completely absent. Furthermore, while workers with the same education level are likely to perform the same type of jobs these could potentially substitute for one another if one is having a child (and one of them wait with childbearing until the other worker is back), low education workers would have to substitute for high education workers, in order to generate the observed pattern.

To conclude, the results in this section lines up well with evidence on the formation of social ties within networks. As such the results in this section bring additional support for our interpretation of the estimated effects as reflecting social influences rather than a spurious effect driven by unobserved common factors. [28]

4.4 Placebo co-workers

Finally, Table 3 presents the estimates from the placebo co-worker falsification exercise outlined above. 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.

[27] 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 optimal timing of the second child.

[28] The only type of unobserved shocks that could explain these asymmetric parity and education specific peer effects patterns are workplace specific shocks that only affect childbearing decisions among women with previous children (college education) but not women without children (without college education). On the contrary unobserved shocks that affect childbearing decisions among women without previous children (without college education) must always also affect women with previous children (college education). The standard omitted variables that we worry could lead to spurious correlations in fertility decisions within the workplace are unlikely to generate such asymmetric patterns.

Table 3 Placebo co-workers.

	(1)	(2)	(3)	(4)	(5)
Sample:	Private firms with multiple workplaces	Private firms with multiple workplaces	Job switchers	Job switchers	All
Peer group	***True:*** *Same firm, same workplace*	***Placebo:*** *Same firm, different workplace*	***True:*** *Contemporary 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
Calendar time dummies	Yes	Yes	Yes	Yes	Yes
Individual char.	Yes	Yes	Yes	Yes	Yes
True co-work. char.	Yes	Yes	Yes	Yes	Yes
Placebo co-work. char.	No	Yes	No	Yes	Yes
Mean dependent variable	0.00503	0.00503	0.0058	0.0058	0.00523
Observations	1,066,052	1,066,052	729,767	729,767	5,403,084

Notes: *,** and *** denote statistical significance at 10,5 and 1 percent level respectively. Standard errors robust for clustering at the workplace level are shown in parentheses. The level of analysis is the individual-month. Calendar time is defined at the Year×Month level. Individual characteristics include civil status and a dummy for college education. Workplace characteristics include establishment size dummies in intervals of ten employees, the regional (county/year) unemployment rate where the workplace is located, the number of previous children in the workplace and the share of fertile, close-in-age, female, married and college educated co-workers. The specification in column (2) additionally controls for firm size using nine dummies (2–9, 10–19, 20–29, 30–39, 40–49, 50–99, 100–199, 200–499, >499 employees).

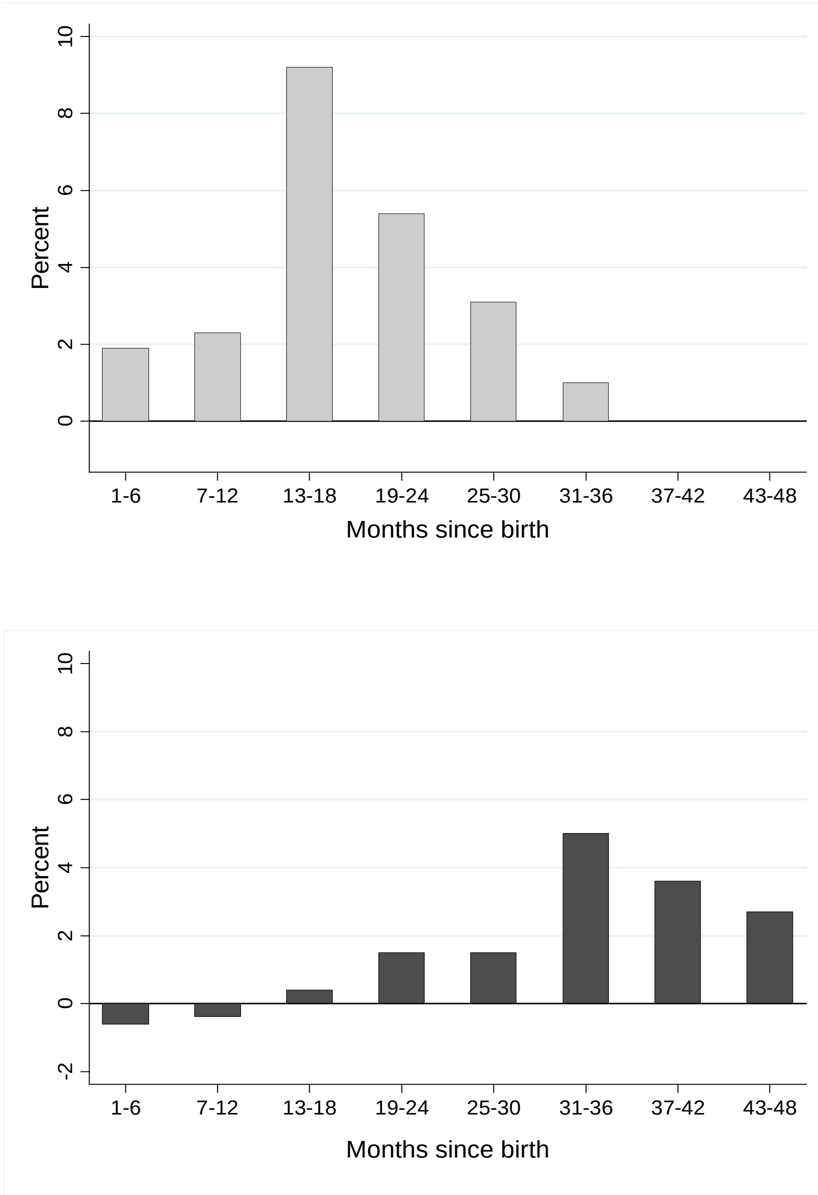

Figure 1 Spill-over effects between networks

The impact of co-workers' childbearing (top) and co-workers' siblings' childbearing (bottom).

While the estimates for the true co-workers are highly similar to the baseline estimates neither one of the three placebo co-worker regressions provides results that even vaguely resemble the main results.[29] The only estimate that is significantly different from zero in any of the three placebo peer group regressions is the 25–36 month lagged effect in the co-workers' sibling sample.

To further assess this pattern we estimated a model where we allowed co-workers' siblings to affect childbearing decisions of the focal worker in 6-months intervals for up to 48 months. The results are presented in the lower panel of Figure 1. For comparison we also show the 6-month interval estimates for the true co-workers in the upper panel. The parameter estimates are small and insignificant for the first 30 months after a birth to a co-worker's siblings but there is an effect showing up with a lag of 31–36 months, which then fades 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.

4.5 Alternative explanations

Staggered hiring, staggered childbearing?

One potentially relevant example of when these baseline predictions and robustness tests would fail to fully rule out sorting is the case of staggered hiring and promotions. Assume that the hiring process take place in a staggered manner, generating a uniform distribution of tenure in the workplace. Now combine this situation with workers having preference to have children just after some specific point in their career, for example after promotions. If promotions occur with regular intervals then it is possible to imagine a dynamic pattern similar to the ones shown above.

However, in some specifications presented above we do control for tenure at the plant and 3-digit industry dummies, which should soak up much of this potential spurious variation in childbearing clustering. In addition, most of our results on the heterogeneous influences of peers, and the placebo peer group estimates speak against this alternative hypothesis.

In an attempt to further try to rule out this possible alternative explanation, we investigated whether women are differentially influenced by peers with more and less years of tenure than themselves. If the peer effect was mechanically generated by the structure of hiring, this would result in a spu-

[29]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.

rious association from peers with longer tenure to those with shorter tenure in the workplace. The data clearly speaks against this explanation.

5 What drives the peer effect in timing of fertility?

5.1 A real options model of the timing of childbearing

Our goal in this section is to provide evidence on the relative importance of two broad peer effect mechanisms. First, peers' childbearing may directly affect women's utility of childbearing we denote this direct mechanism as *network externalities* (c.f. Katz & Shapiro, 1985; Becker 1991). Network externalities could generate the observed peer effect for example through the sharp changes in time spent socializing with friends before and after childbearing[30], or if joint leisure time is valued, childbearing of co-workers could reduce the value of leisure and lead to an increased desire to have children. Other examples include that individuals derive utility from conforming, joint parental leave or economies of scale (e.g. from coordinated childcare and the sharing of material expenses) as well as leave-related strategic considerations affecting own utility payoffs of having children at a particular point in time. Second, in addition to the network externalities, childbearing among peers may also provide information about the net benefits of childbearing. Co-worker's childbearing experience could provide workplace specific information about the net benefits of childbearing that are difficult or impossible to attain from other social networks or sources. For example, peers childbearing experiences could provide information about the effects of having children on wage growth or career opportunities after birth or about the possibilities of combing family and work. We interpret this mechanism as an *informational externality*.

To guide our effort we develop a simple theoretical model of fertility timing choices to provide direction for our effort to distinguish between the underlying peer mechanisms empirically. We keep things simple and make the assumptions necessary to be able to both specify a model that are consistent with the baseline result and provides use with predictions allowing us to say something about the underlying mechanisms.

Three important features of the fertility decision motivate our model. First, having children is an irreversible action which implies that some of the costs of childbearing cannot be recovered. Second, there is uncertainty about the future net benefits from having children related to e.g. future wage growth, career consequences and non-monetary outcomes of having children

[30] Cohabiting/married women with small children spend on an average day 30 percent less of their leisure time socializing with non-household members on an average day and over 50 percent less time on weekends compared to cohabiting/married women without young children (Based on our own calculations using data from the 2000 Swedish time-use survey.)

at a particular point in time. Third, women can influence the timing of childbearing as long as they are in fertile ages.

To capture these central features of the timing decision, we build on real-options theory.[31] In this class of models, the standard approach considers a firm facing an investment choice. The investment decision is assumed to be (partially or completely) irreversible, and there is uncertainty about the future returns from the investment. This generates real options on the investment decision. The real option value drives a wedge between the investment threshold under completely certain conditions and reversible investments, and the situation with uncertainty and irreversible investments. By postponing actions the firm can get more information about the future rewards and reduce, although not completely remove, uncertainty. Higher uncertainty increases the option value and makes firms more cautious in their investment decisions and thereby less responsive to changes in demand.

We apply the real options approach to the timing of the investment in childbearing in our specific context and build a model in discrete time where women who are not already mothers in each period may decide whether to have a child or not.[32] [33] This decision is modeled as a standard dynamic discrete choice optimization problem with a finite horizon, as the option of having a child has a time-to-maturity that approaches zero at a certain age.

In every period, women maximize the value of expected lifetime utility, an optimization problem which is subject to a standard household budget constraint. We specify the utility received in each period is a Cobb-Douglas function of consumption, c_t, leisure l_t and a utility multiplier ϕ^N.

$$u_t^N = \phi^N c_t^a l_t^b, \qquad a + b \leq 1. \tag{1}$$

For non-mothers, the multiplier is assumed time-invariant, and for mothers we assume that the multiplier is a stochastic variable,

$$u_t^M = \phi_t^M c_t^a l_t^b \tag{2}$$

The stochastic multiplier, ϕ_t^M, evolves according to a random walk process with continuous states, a drift parameter, μ_t, and an idiosyncratic shock, ε_t.

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[31] See e.g. Arrow (1968), Bernanke (1983), McDonald and Siegel (1986), Bertola (1988), Pindyck (1988) and Dixit and Pindyck (1994), Hassler (1996).

[32] We are not the first to realize the relationship between the irreversible investment decision of childbearing, uncertainty and fertility decisions. Ranjan (1999) and Iyer and Velu (2006) have developed models with similar implications. However, we are the first to link it to the peer effects literature and to test its implications empirically.

[33] Indeed, most women who chose to have a child will soon after be able to decide when to have a second child, and a similar model could be set up for this second step of family planning. However, here we ignore the decision of timing between births and extrapolate on the qualitative implications of the first step model.

$$\phi_t^M = \phi_{t-1}^M(1+\mu_t+\varepsilon_t), \qquad \varepsilon_t : N(0,\sigma_t). \tag{3}$$

This process incorporates the two channels through which we assume that social interactions among co-workers could affect their peers' fertility decisions. First, we allow the drift parameter, μ_t to be a function of p_t, the number of peers who are or were until recently pregnant.

$$\mu_t = \mu_0 + f(p_t), \qquad f(0)=0, \quad f'(p_t)=cf''(p_t), \quad c<0. \tag{4}$$

Equation (4) describes the *network externality* effects of peers' childbirths as the trend of the random walk process shifts as agents observe childbearing among peers. If network externality effect is negative, then $f'(p_t)<0$ and if the network effect is positive then $f'(p_t)>0$.

Second, the *information externality* effect comes into the picture via a reduction in the uncertainty (σ_t) about the stochastic variable, ε_t

$$\sigma_t = \sigma_0 + g(p_t), \qquad g(0)=0, \quad g'(p_t)\le 0, g''(p_t)\ge 0. \tag{5}$$

In other words we assume that women on average have an unbiased valuation of the net benefits of having a child, but that this value is not known with certainty and that an important part the uncertainty is job related. Through social or observational learning individual can reduce the uncertainty about future net benefits of childbearing.

Further, we assume that agent's expectations about peer decisions can be simplified to

$$E(p_{t+1}) = p_t \tag{6}$$

implying that that agents expect the proportion of peers with babies to remain constant after observing p_t. In other words, individuals do not take into account that their own decisions may influence the actions of their peers.[34] Combining equations (3) and (6) gives us

$$E_t\left(\phi_{t+1}^M\right) = \phi_t^M(1+\mu_t) \tag{7}$$

and

$$E_t\left(\sigma_{t+1}\right) = \sigma_t \tag{8}$$

[34] This is a common assumption in the peer effects literature, c.f. e.g. Blume, Brock, Durlauf, Ioannides (2010).

the budget constraint in the model is

$$c_t \leq w_t(1-l_t) \tag{9}$$

where the time to be allocated between leisure and wage generating labor is set to unity. The wage rate is w_t, and to assimilate wage the relative wage growth stagnation that we observe for mothers in our data we assume that wages grow at a constant rate, $\gamma \geq 0$, before motherhood, while the wage growth is zero after childbirth.

$$w_t = w_{t-1} + w_{t-1}\gamma(1-M_t) \tag{10}$$

Here, *Mt* is a dummy variable equal to one if the agent is a mother and zero otherwise. Agents maximize their utility in each period by choosing $c(w_t)$ and $l(w_t)$. Now, the value of motherhood is the expected value of a discounted utility flow into eternity

$$v_t^M(w_t, \phi_t^M, p_t) = E_t \sum_{s=0}^{\infty} \beta \phi_{t+s}^M c(w_t)^a l(w_t)^b \tag{11}$$

where v_t^M denotes the value of motherhood and β is the agent's discount rate. If a woman decides to conceive, she will incur a cost, *I*, in terms of utility which may be related to the need for monetary investments (new car, larger house/apartment, etc.), absence from work due to health problems or simply physical strains during the pregnancy. At time *t*, we express the option value of being a potential mother as

$$v_t^O(w_t, \phi_t^M, p_t) = \max \begin{cases} u^N(c(w_t), l(w_t) + \beta E_t v_{t+1}^O(w_{t+1}, \phi_{t+1}^M, p_t) \\ v^M(w_t, \phi_t^M, p_t) - I \end{cases} \tag{12}$$

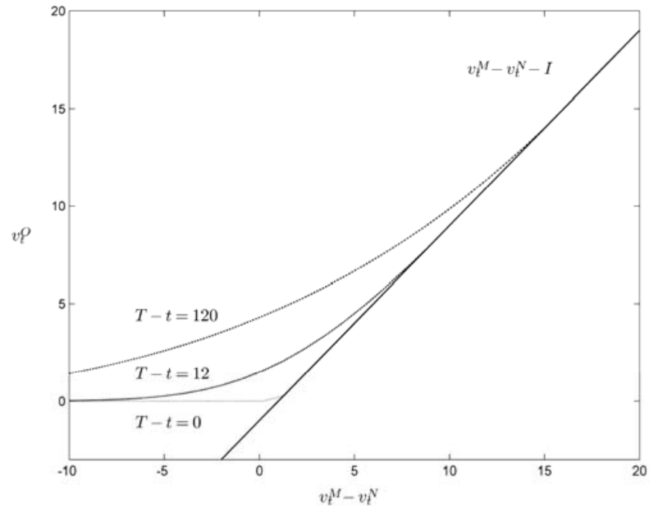
which displays the recursive nature of the optimization problem. Until the time of maturity, *T*, for the option of motherhood, the fertility decision can always be delayed one period, allowing the agent to receive utility of non-motherhood, u_t^N, while wages grow and a new shock to the multiplier ϕ is realized. In the last period before maturity the maximization problem becomes

$$v_T^O(w_T, \phi_T^M, p_T) = \max \begin{cases} v_{t+1}^N(w_T, \phi^N) \\ v^M(w_T, \phi_T^M, p_T) - I \end{cases} \tag{13}$$

where v^N, denotes the lifetime discounted value of non-motherhood after maturity, which is a function of optimal consumption, *c*, and leisure, *l*, with respect to future wage levels and the deterministic multiplier, ϕ^N. Both alternatives at time *T* are known to all agents and v_T^O is known for all levels of w_T and ϕ_T. The model is then solved by inserting v_T^O into the expression for the option value at time $t = T - 1$, and then repeat the procedure until $t = 0$

Figure 3 shows how the real option value of being a potential mother varies with the value of the underlying asset (the expected lifetime utility gain of motherhood). The curves represent the relationship under 1, 12 and 120 months until expiration date of the option (i.e. menopause), which is assumed to be known to all agents, while the straight line plots the gain in value from motherhood net of investment costs. The tangency point between this line and each of the three curves are investment thresholds in terms of expected lifetime utility gain. For $T - t = 0$, the investment rule is simply to chose motherhood if the expected gain net of costs is more than zero. For younger women with more time remaining until maturity, we see that the threshold may be much higher than the expected gain itself, because the option leaves the agent with a value of waiting one period to make the decision. Because the option value can never be less than zero, the increased chance of highly favorable and highly non-favorable outcomes implied by a longer horizon increases the option value and the fertility threshold. A corresponding effect can be seen in Figure 4 where higher levels of uncertainty implies a higher option value of postponing childbearing, which increases the individuals inaction range and decreases childbearing.

Figure 3: Option Values and Time to Maturity



Note: Model solution based on zero growth in wages, $\mu = 0$, and $\sigma = 0.2$

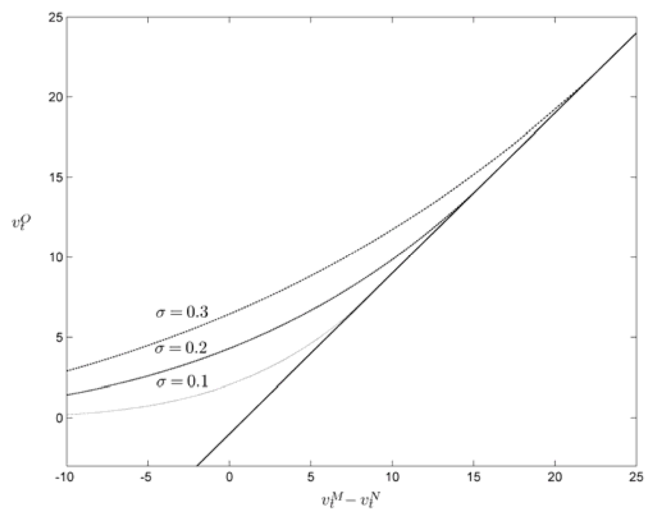


Figure 4: Option Values and Uncertainty

Accordingly, in Figures 3 and 4 individuals' childbearing decisions influence the decisions of their peers in two ways. As stated above the network externality effect comes in Equation (3) and (4) and have direct impact on the probability of childbearing by directly increasing or decreasing a woman's valuation of motherhood. If the network effect is positive, the expected gain from motherhood will increase, thus moving closer to or beyond the point where childbirth becomes optimal, increasing fertility in the peer group. If the network externality effect is negative, fertility in the rest of the group will decrease.[35] Alternatively as described individual childbearing decisions can influence co-worker's childbearing by reducing uncertainty. In Figure 4 this again leads to a reduction in the threshold and an increase in the fertility rate.

The fertility timing model outlined above provides us with testable predictions that differ depending on the underlying mechanism at work that we can take to the data.

1. If the baseline results are driven by network externalities the impact of childbearing among peers should decrease as uncertainty increases. That is the network externality hypothesis predicts that $\partial\beta / \partial\sigma < 0$. This result is closely related to a standard result in the real options literature where higher uncertainty induces a postponement of investment decisions and reduces the responsiveness to demand shocks (Bernanke, 1983; Hassler, 1996; Bloom et al., 2007).

2. If the baseline results are driven by information externalities the impact of childbearing among peers should increase as uncertainty increases. That is the information externality hypothesis predicts that $\partial\beta / \partial\sigma > 0$. This prediction builds on the convex relationship displayed in Figure 4 which implies that a proportional reduction in uncertainty have a larger impact on childbearing when uncertainty is high then when uncertainty is low.

3. If the baseline results are driven by information externalities as time to maturity goes to zero the importance of peers should decrease. That is $\beta \to 0$ when $T - t \to 0$.

4. If the baseline results are driven by network externalities as time to maturity goes to zero the importance of peers childbearing decisions should increase. That is $\beta_{T-t=0} < \beta_{T-t>0}$.

Next we describe how we take these predictions to the data.

[35]It is worthwhile to note that out of all the estimated coefficients above there negative point estimates are rare and in such cases they are almost never significant. This indicates that if the effects are driven by network externalities it seems as if positive network externalities dominate in our setting.

3.3. Distinguishing between information and network externalities

Measuring Uncertainty and Time-to-Maturity

An important issue in the empirical implementation of the first test is that we need a measure of workplace level uncertainty. We have chosen two measures of uncertainty. We first try to capture the job-related uncertainty using information on the tenure of the manager. The relevance of this measure is far from obvious. In short, we assume that, all else equal, working under a manager with long tenure is less uncertain than to work under a manager with short tenure. The intuition is that in workplaces with new management the perceived risk of re-organizations, changes in firm policies, reduced knowledge about the individual worker's productivity, changes in manager attitudes towards childbearing, etc., leads to higher uncertainty about the future. However, as the manager tenure increase, this uncertainty is resolved because information about the manager's attitudes and policies are revealed over time, and so is information about e.g. the employees' effort and productivity. [36]

An important concern with using manager tenure as a proxy for uncertainty is that high turnover firms will be overrepresented in firms with less tenured managers. If women sort into more or less stable workplace environments depending on their childbearing preferences this could bias our results. To mitigate this concern we add controls for individual and average co-worker tenure as well as year×month×region×industry effects.[37]

As a secondary measure of uncertainty we have also used the standard deviation of the workplace level churning rate. The churning rate is a measure of the excess turn-over of workers, and has previously been used to measure industry or labor market uncertainty. (c.f. Davis and Haltiwanger, 1999).

Finally, to assess the time to maturity prediction, we simply divid the fertility cycle into an early (age 20–27), primary (age 28–36) and late (age 37–44) stage, and estimate the baseline model for women in these differing ages separately.

[36] To identify the manager we use occupational codes and information on ownership. The data contains information on detailed occupational status for all establishments in the public sector and for a sample of private establishments. Information on ownership is available for all establishments in the economy. We identify the manager using the following hierarchical criteria: (1) Owner, (2) Top manager and (3) Middle manager. In case that there are multiple managers at the same level, we assume that the manager is the individual with the highest wage. Manager tenure is defined as years at the workplace (truncated in 1985). Note that for sampling reasons tenure is measured as the number of years the current manager have been employed in the workplace, i.e. irrespective of whether he/she occupied the manager position for the whole period or not. The mangers have an average tenure of 5.9 years (sd 5.07).

[37] However, note that the results are very similar even if we leave these extra controls out.

Testing the predictions

So to test the first key prediction we re-estimate the baseline model separately for the women employed in workplaces with a new manager (less than four years of tenure) versus a tenured manager (four years or longer tenure).

Table 4, columns (1) show that the peer effect is significantly lower when uncertainty is high (new manager) compared to when it is low (tenured manager).[38] A similar pattern is found when we included an interaction term between the peer effect and the variance of the workplace churning rate. The results are summarized in Figure 5. However, while qualitatively consistent with the results from the manager tenure proxy of uncertainty, the point estimates in Figure 5 are not estimated with good enough precision to draw any stronger conclusions.

Columns (2)–(4) in Table 4 show that although women are influenced in all stages of the fertility cycle the impact is increasing in age, and strongest in the last stages of the fertility cycle. 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. [39]

[38] The baseline estimates for the sample of workers for whom we can identify their manager's tenure in the workplace are highly similar to the baseline results for the full sample.

[39] 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.

Table 4: Information or network externalities?

	(1)	(2)	(3)	(4)
Sample:	Full sample where manager tenure can be identified	Early (age 20-27)	Primary (age 28-36)	Late (age 37-44)
Any co-worker had a child within:				
12 months	0.0002	-0.00004	-0.00009	-0.00013
	(0.0002)	(0.00008)	(0.00025)	(0.00020)
13-24 months	0.00087***	0.00030***	0.00087***	0.00043**
	(0.0002)	(0.00008)	(0.00019)	(0.00020)
25-36 months	0.00043*	0.00007	0.00032*	0.00033
	(0.0002)	(0.00008)	(0.00019)	(0.00020)
High Uncertainty × Any co-worker had a child within:				
12 months	-0.00006			
	(0.00037)			
13-24 months	-0.00079**			
	(0.00033)			
25-36 months	-0.00016			
	(0.00034)			
Duration dummies	Yes	Yes	Yes	Yes
Calendar effects	Yes	Yes	Yes	Yes
Individual char.	Yes	Yes	Yes	Yes
Workplace char.	Yes	Yes	Yes	Yes
Time×Industry×Region eff.	Yes			
Own tenure	Yes			
Co-workers tenure	Yes			
Mean Y	0.0051	0.00409	0.00921	0.00297
Observations	921,655	3,838,904	1,324,836	409,657

Notes: *,** and *** denote statistical significance at 10, 5 and 1 percent level respectively. Robust standard errors clustered at the workplace level are shown in parentheses. Column 1 presents results from the high uncertainty regime (Short manager tenure) and Column 2 presents results from the low uncertainty regime (Long manager tenure). The level of analysis is the individual-month. Calendar time is defined at the Year×Month level. Individual characteristics include civil status and a dummy for college education. Workplace characteristics include establishment 5 workplace size categories (<10, 10-19, 20-29, 30-39, 40-49), 5 workplace age categories (0, 1-2, 3-4, 5-10, >10), the regional (county/year) unemployment rate where the workplace is located, the number of previous children in the workplace and the share of fertile, close-in-age, female, married and college educated co-workers. Columns (4) and (5) include Year×Month×Industry (3-digit)×region fixed effects, own tenure (0, 1-2, 3-4, 5-10, >10 years) and average tenure among the co-workers.

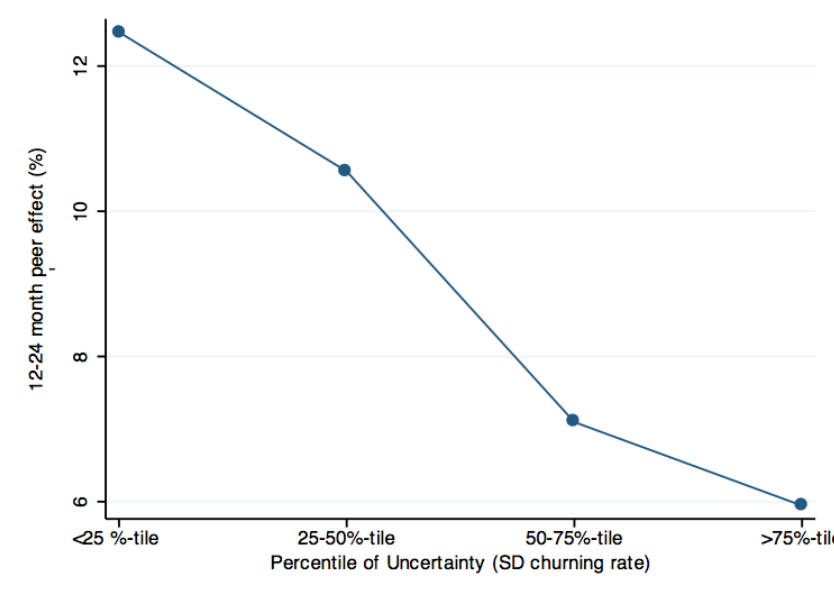


Figure 5: Peer effect and Uncertainty

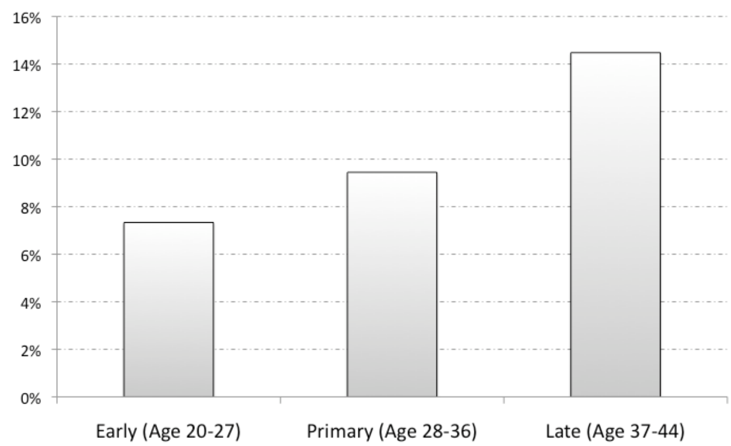


Figure 6: Peer influences at different stages of the life-cycle

In summary, based on the predictions derived from the model, both of these patterns seem more consistent with a larger role for network externalities than for information externalities in our context.

4 Conclusions

In this study we investigate how fertility decisions are transmitted in social networks. Specifically, we use unique matched employer-employee data and examine how recent births among co-workers affect the subsequent childbearing decisions among 150,000 Swedish women. We find that co-workers have a significant impact on the timing of childbearing; the average effect is comparable to increasing a woman's age by one year in the age interval 20 through 30. Consistent with the literature on the formation of social ties, same type peers are much more influential than other type peers. The results are also robust to a number of alternative specifications and falsification checks.

To understand the mechanisms through which the peer effects arise, we develop a theoretical model based on real-options theory that applies to fertility timing decisions under uncertainty that are consistent with our baseline results. In our model clustering of childbearing can arise because of network externalities or information externalities. The model provides us with two predictions that allow us to distinguish between the two mechanisms. Taking the predictions to the data, we find the data is more consistent with network externalities being the main underlying mechanism in this context.

The distinction between underlying mechanisms is potentially important from a policy perspective. For example, if attempting to reduce (or at least predict) fluctuations in fertility rates, it is important to understand the underlying mechanisms. If individuals only care about the decisions of others because they have something to learn about the cost/benefits of childbearing increased information may reduce fluctuations in fertility rates. On the contrary, with strong enough peer effects, if the network externality effects instead dominate then public policies aiming to reduce economic uncertainty with the intent to curb fluctuations in fertility rate may not work as planned. Our results suggest that reduced uncertainty seems to generate stronger social multipliers by giving a greater role to social networks in fertility decisions. Accordingly, depending on whether the economic incentives or social incentives dominate, the net impact of policies that reduce uncertainty is not clear, and they may even generate stronger fluctuation in the fertility rate. Of course this interpretation hinges on the assumption that our results generalize to how peers influence childbearing decision in other networks besides co-workers. Future research should examine the underlying nature of peer effects in other social networks.

The existence of peer effects in such an important decision as the timing of childbearing clearly suggest that social influences may be relevant also for other types of career related decisions. If family choices have the tendency to spread within networks then such peer effects may be 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 affect other labor supply related decisions such as exits from the labor force, moves to part-time work or the take-up of managerial positions are important and interesting questions for future research.

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Appendix A

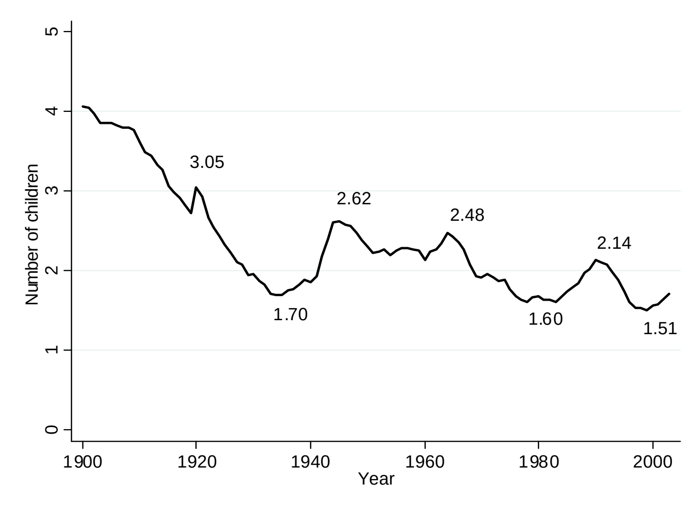

Figure A1 Total fertility rate, 1900-2003, *Source*: Socialstyrelsen (2005).

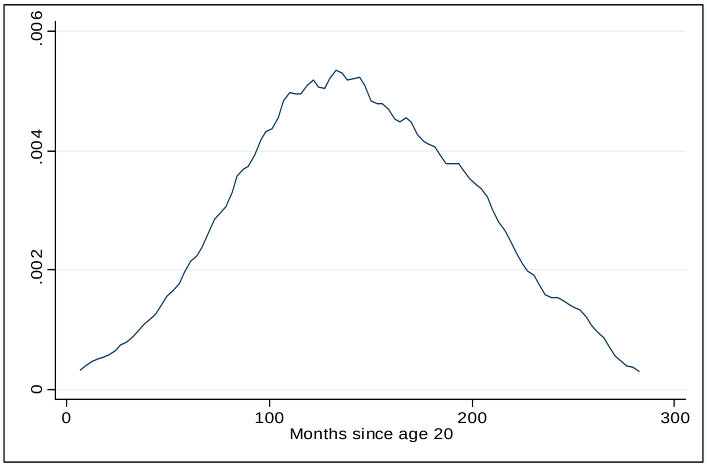

Figure A2 Smoothed baseline hazard of first births.

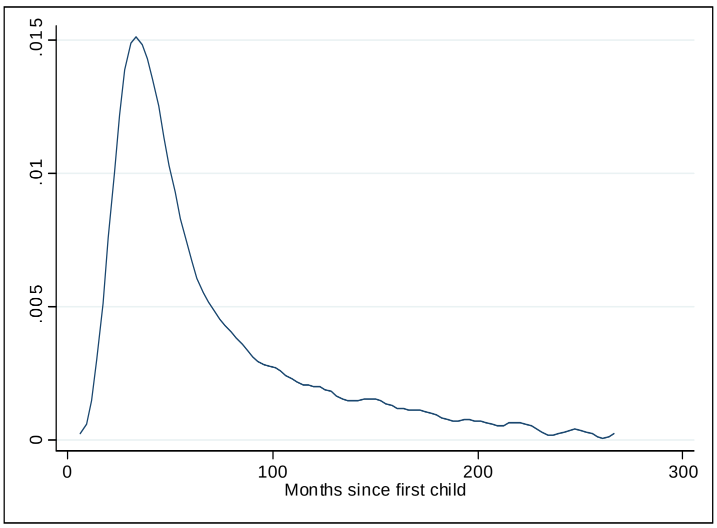

Figure A3 Smoothed baseline hazard of second births.

Table A1 Descriptive statistics.

Sample:	First birth		Second birth		Third birth	
	Mean	Sd	Mean	Sd	Mean	Sd
Had a child in current month	0.005	0.072	0.011	0.105	0.002	0.045
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 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-work.*	***Placebo:*** *Future co-work.*	***True:*** *All co-work.*	***Placebo:*** *Co-work. 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	18.5	1,178	20.3	19.9	20.5	19.05
children	(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	0.42	0.82	0.38	0.40	0.39	0.36
months	(0.49)	(0.39)	(0.49)	(0.49)	(0.488)	(0.479)
25-36	0.42	0.82	0.37	0.38	0.37	0.34
months	(0.49)	(0.38)	(0.48)	(0.49)	(0.484)	(0.472)
Obs.	1,066,052	1,066,052	730,356	730,356	5,575,497	5,385,787

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

Appendix B: Additional results

This section provides a more detailed discussion and the full regression results summarized in Figure 2 in Section 5.1 in the main text. It also provides additional results with respect to the heterogeneity of the fertility peer effect depending on own characteristics, the degree of similarity between the focal individual and the co-workers and workplace size.

B1 Who is influencing whom? Gender, age and education

Table B2 presents the full results from estimation of model (2) described in Section 5.1. 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 γ '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.

First and foremost we find that the entire baseline peer effect seems to be driven by the influence of female co-workers (i.e. same sex). 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. The estimates reported in column (2) suggest that the influence of co-workers who are close-in-age is 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.

We also 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 3), 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 (4) 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 asymmetric pattern we find w.r.t. the worker/co-worker education are in line with the literature on the importance of social status (Akerlof and Kranton, 2000), and with recent laboratory experiments suggesting that behavior by higher, but not lower, social ranking individuals are influential (Kumru and Vesterlund, 2010).

Birth order

The baseline results reported the peer effect for women at risk of having their first child. Here, we also 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 (6) of Table B2 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.[40]

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 (7). 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.

In the last three columns of Table B2 we look at whether individuals are differentially affected by co-workers who have the same number of previous children. 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 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.

[40] 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.: 0.0002)]. These estimates are retain for expositional purposes but are available upon request from the authors.

The estimates in columns (8)–(10) 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. We find that first-time mothers are influenced by all childbearing co-workers' irrespectively of the birth order of the co-worker's child (column 8). In contrast, second and third time mothers (Columns 9 and 10), are only influenced by co-workers with the same number of previous children.

Table B2 Heterogeneous peer effects: Gender, age, education and birth order.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
SAMPLE:	Same sex (female) co-workers:	Close in age (± 4 years) co-worker:	Same education co-workers: College	Same education co-workers: No College	1st birth	2nd birth	3rd birth	1st birth	2nd birth	3rd birth
12 months	0.00007	-0.00031***	0.00011	-0.00035**	0.00004	0.00044**	-0.00005	0.00001	0.00020	-0.00007
	(0.00010)	(0.00008)	(0.00015)	(0.00016)	(0.00007)	(0.00017)	(0.00005)	(0.00012)	(0.00019)	(0.00006)
13–24 months	0.00016	0.00009	0.00011	0.00063***	0.00048***	0.00083***	0.00010*	0.00047***	0.00023	0.00009
	(0.00011)	(0.00008)	(0.00014)	(0.00017)	(0.00007)	(0.00017)	(0.00005)	(0.00011)	(0.00019)	(0.00005)
24–36 months	0.00000	-0.00014*	0.00005	-0.00021	0.00018**	0.00033**	0.00008	0.00024**	-0.00009	0.00007
	(0.00011)	(0.00008)	(0.00014)	(0.00017)	(0.00007)	(0.00017)	(0.00005)	(0.00011)	(0.00019)	(0.00005)
This type of co-worker had a child within:										
12 months	-0.00000	0.00088***	-0.00005	0.00052***				0.00003	0.00029	0.00022
	(0.00012)	(0.00012)	(0.00019)	(0.00017)				(0.00013)	(0.00028)	(0.00028)
13–-24months	0.00047***	0.00107***	0.00058***	-0.00011				0.00000	0.00151***	0.00040*
	(0.00012)	(0.00012)	(0.00018)	(0.00018)				(0.00012)	(0.00025)	(0.00022)
24–36 months	0.00026**	0.00096***	0.00042**	0.00034**				-0.00009	0.00104***	0.00040**
	(0.00012)	(0.00012)	(0.00018)	(0.00017)				(0.00011)	(0.00024)	(0.00019)
Dur. dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Own char.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Workpl. char.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean Y	0.00523	0.00523	0.00562	0.00498	0.00523	0.01105	0.00202	0.00523	0.01105	0.00202
Observations	5,575,497	5,575,497	2,140,535	3,432,418	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 and 1 percent level respectively. Standard errors robust for clustering at the workplace level are shown in parentheses. The level of analysis is the individual-month. Calendar time is defined at the Year×Month level. Individual characteristics include civil status and a dummy for college education. Workplace characteristics include establishment size dummies in intervals of ten employees, the regional (county/year) unemployment rate where the workplace is located, the number of previous children in the workplace and the share of fertile, close-in-age, female, married and college educated co-workers.

Network Size

In this section we examine if the observed fertility peer effect varies with respect to workplace size. The peer effect may differ by workplace (network) size either because the true fertility peer effect differs between workplaces with different size, or because co-workers interact differently within different sized workplaces.[41]

To explore the relevance of network size effects in this case we divided the sample into three groups based on the number of employees and estimated a separate regression for each sample. These estimates are reported in Table B3. As seen in columns (1)–(3) the largest estimated peer effect 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 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 explanation consistent with the seemingly u-shaped 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, as the network size becomes larger the cumulative influence of multiple births among co-workers potentially dominates the decreasing "network precision" effect. This is further consistent with the dose-response pattern we found in Table 2; more exposure implies stronger peer effects.

Alternatively, individuals may interact differentially within different sized networks. For example, on average the number of social ties and the tendency to associated disproportionally with "same-type" peers increases with network size (c.f. Currarini, Jackson and Pin, 2009; Weinberg 2007). Hence, when network size increases the possibility to form more ties with individuals of the same type also increase and the strong within-type specific peer effects (reported in Figure 2) could potentially dominate the negative "network precision" effect.[42]

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[41] 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).

[42] We also investigated 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. The effects are not significantly different from each other (not reported). 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.

To explore whether more exposure or more homophily can explain the observed u-shaped peer effect pattern with respect to workplace size we re-estimated the model and included an indicator for if more than one co-worker gave birth 1–12, 13–24 and 25–36 months ago to control for differences in exposure between the different sized workplaces. As shown in the three last columns in Table B3, including dummies for more than one birth, if anything, reinforces the u-shaped pattern. Thus at least it seems as if higher exposure to births cannot explain why the peer effect is stronger in larger workplaces than in middle-sized, instead suggesting that workers in large workplaces have more ties and/or more same-type ties.

Table B3 Workplace size.

	(1)	(2)	(3)	(4)	(5)	(6)
Workplace size:	2-9	10-29	30-49	2-9	10-29	30-49
1-12 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
Calendar time	Yes	Yes	Yes	Yes	Yes	Yes
Individual.char.	Yes	Yes	Yes	Yes	Yes	Yes
Workplace characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Dummy for more than one child	-	-	-	Yes	Yes	Yes
Mean of Y	0.00512	0.00524	0.00535	0.00512	0.00524	0.00535
Observations	1,760,442	2,664,386	1,148,125	1,760,442	2,664,386	1,148,125

Notes: see Table B2