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Fertility and social interaction at the workplace: Does childbearing spread among colleagues?

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ABSTRACT

This research investigates whether colleagues' fertility influences women's transitions to parenthood. We draw on Linked-Employer–Employee data (1993–2007) from the German Institute for Employment Research comprising 33,119 female co-workers in 6579 firms. Results from discrete-time hazard models reveal social interaction effects on fertility among women employed in the same firm. In the year after a colleague gave birth, transition rates to first pregnancy double. This effect declines over time and vanishes after two years. Further analyses suggest that the influence of colleagues' fertility is mediated by social learning.

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1. Introduction

The vast majority of micro-level studies explain fertility by socio-economic characteristics. A relatively new line of research posits that the decision to have a child is also influenced by interaction partners (Balbo, Billari, & Mills, 2013; Rossier & Bernardi, 2009). Pioneering studies in this direction revealed that social interaction within local communities explained regional differences in fertility levels within developing countries (Bongaarts & Watkins, 1996; Kohler, 2001; Montgomery & Casterline, 1996). In recent years, a growing number of studies have directed attention to social interaction effects on fertility within different networks (Bühler & Fratczak, 2007; Philipov, Spéder, & Billari, 2006).

The current understanding of these effects largely relies on qualitative studies. This fruitful line of research has not only revealed which interaction partners are most influential but has also provided considerable insight into the mechanisms behind social interaction effects on fertility (Keim, Klärner, & Bernardi, 2009; Keim, Klärner, & Bernardi, 2012). In contrast, large-scale representative studies attempting to identify and quantify these effects remain scarce.

In view of that, this study aims to provide a quantitative assessment of social interaction effects on fertility. Specifically, we ask whether colleagues' fertility increases the chance that a woman will become pregnant. In other words, does fertility spread among colleagues? By selecting the workplace as a setting for our study, we focus on a social network in which most individuals spend a considerable amount of their time and are very likely to be exposed to birth events among their interaction partners.

If these events are influential, in turn, a considerable number of colleagues will be affected, suggesting social multiplier effects and possible "chain reactions" of births and subsequent pregnancies within a firm. Furthermore,

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information that circulates at the workplace appears to be particularly relevant for fertility decisions because colleagues share a common context. In view of the far-reaching consequences of births and maternity leaves for working careers, the experiences of colleagues might constitute valuable information with regard to fertility decisions.

The analysis of social interaction effects on fertility at the workplace requires data that capture the entire network of colleagues. This requirement is met by the Linked-Employer–Employee (LIAB) data of the German Institute for Employment Research (IAB). The LIAB combines survey data on firms with process-generated data on the entire staff of a firm provided by the German Federal Employment Agency. Based on maternity leave reports, we reconstructed a firm's entire history of birth events. These data enabled us to examine whether and to what extent an employed woman's chance of becoming pregnant was influenced by her colleagues' preceding birth events. To investigate these effects empirically, we estimated discrete-time hazard models based on a sample of 33,119 female co-workers observed longitudinally in 6579 firms.

2. Theoretical background

How do interaction partners influence fertility decisions? This question originated in the work of Coale and Watkins (1986) who examined the decline of birth rates in modern societies. Their study was the first to posit that social interaction might cause regional variation in aggregate levels of fertility. Since then, numerous studies have investigated social interaction effects on fertility. Initially, this research focused mainly on the role of social interaction in the diffusion of contraceptive use in developing countries (Bongaarts & Watkins, 1996; Kohler, 2001; Montgomery & Casterline, 1996). In contrast, more recent research from developed countries has been interested in the realization rather than the prevention of births. These studies asked how – and why – interaction partners influence the decision of whether and when to have a child.

2.1. Empirical evidence for social interaction effects on fertility

Overall, the literature – particularly the qualitative work of Bernardi (2003), Bernardi, Keim, and von der Lippe (2007), Keim et al. (2009, 2012), and Keim (2011) – has provided ample evidence for the importance of social contacts from different interaction domains (family, friends, acquaintances, colleagues, and neighbors) for fertility decisions. Quantitative tests for such effects, however, remain rare. To our knowledge, only four published studies have examined social interaction effects on fertility quantitatively. Based on Norwegian register data, Lyngstad and Prskawetz (2010) investigated whether siblings' fertility decisions influenced each other. This study showed that the probability of becoming pregnant increased significantly in the 12 months following the birth of a niece or a nephew. Aparicio Diaz, Fent, Prskawetz, and Bernardi (2011) used simulation models calibrated by Austrian census data to examine whether fertility decisions of "relevant others" influenced transitions to parenthood. Based on agent-based models, this study showed that the transition rate to motherhood increased with the share of network members who had children. Kotte and Ludwig (2011) used pairfam data (Panel Analysis of Intimate Relationships and Family Dynamics; Huinink et al., 2011) to examine if contagion among siblings explained the transmission of fertility intentions and fertile behavior within a family. This study did not find evidence for fertility contagion between siblings. Birth events in the network of friends, however, appeared to increase the chance of becoming a parent. Using the same data, a further study examined the "contagiousness" of fertility in respondents' personal networks (Richter, Lois, Arránz Becker, & Kopp, 2012). This investigation indicated contagion effects on higher-order births in East Germany.[1]

With regard to the focal area of the present study – the network of colleagues at the workplace – no quantitative investigations have been published to date. Preliminary evidence, however, is offered by two unpublished studies. An analysis based on register data from Sweden examined whether colleagues' fertility decisions influenced each other (Asphjell, Hensvik, & Nilsson, 2013). This research showed that the probability of childbearing increased significantly in the second year after a colleague had given birth. This effect seemed to operate in a parity-specific fashion. For childless women, all childbearing events were influential whereas for women of higher parity only events experienced by same-parity women mattered. A further analysis based on Danish administrative data reported similar results (Ciliberto, Miller, Nielsen, & Simonsen, 2012). Taken together, these studies provide suggestive empirical evidence in support of social interaction effects on fertility at the workplace.

2.2. The mechanisms behind social interaction effects on fertility

The literature lists four mechanisms governing social interaction effects on fertility: social support, social pressure, social contagion, and social learning. *Social support* is defined as the opportunity to receive financial, instrumental, and/or emotional support from interaction partners. An obvious example is parental childcare assistance. The mechanism of *social pressure* influences decision-making by means of sanctions and/or rewards. Such pressure can be exerted, for instance, by parents who express their wish to have a grandchild. *Social contagion* refers to emotional reactions that individuals are not necessarily aware of. This mechanism operates, for instance, when contact with pregnant women or newborns affects the wish to have a child or the timing of parenthood. Finally, *social learning* refers to the process by which individual perceptions of relevant aspects regarding the fertility decision are changed by new information obtained

[1] In addition to these studies, two currently unpublished investigations of fertility-related social interaction effects among friends (Balbo & Barban, 2012) and siblings (Kuziemko, 2006) have also reported positive effects.

from interaction partners. According to Bernardi (2003), this mechanism is particularly effective if the interacting individuals share similar contexts.

2.3. Mechanisms of social interaction effects on fertility at the workplace

Which of these mechanisms is likely to mediate the influence of colleagues' fertility in the context of the workplace? For a number of reasons, social learning appears to be the most obvious candidate. This mechanism highlights the importance of colleagues as social models with the potential of changing existing beliefs about the feasibility and consequences of having a child. For instance, a childless woman can observe the impact of pregnancy and childbirth on a colleague's work and family life (Keim et al., 2012). How does this colleague reconcile the demands of work and family – both during her pregnancy and after childbirth? This type of information might be particularly important for fertility decisions as it emerges directly from a person's specific labor market context and cannot be obtained from other networks. It might change the previous belief that having a child is "not possible right now".

The psychological concept of self-efficacy provides further theoretical orientation with regard to social learning. According to this view, this mechanism can be expected to operate primarily among similar interaction partners. The greater the (perceived) similarity of a social model, the stronger its influence on a person's beliefs (Bandura, 1994). It is unlikely, for example, that a previously childless woman who becomes pregnant will change the beliefs of a colleague who already is a mother.[2] By the same token, it also seems unlikely that a pregnant 35-year-old woman constitutes a relevant social model to her 20-year-old colleague – even when both are childless. If both are situated within a similar stage of their life course, however, social interaction effects on fertility might transpire in the context of the workplace.

In this regard, it is important to note that these effects might be governed not only by the mechanism of social learning but also by the related principle of conformity. This principle refers to the unconscious tendency of adapting toward the behavior of the group in which one is embedded (Rossier & Bernardi, 2009). The fertility of colleagues might thus induce a mental process involving adaptation to typical or normal behavior (see Balbo & Barban, 2012; Bernardi, 2003; Montgomery & Casterline, 1996).

Unlike social learning, the other mechanisms discussed in the literature – contagion, support, and pressure – appear unlikely to play a major role in mediating social interaction effects on fertility among colleagues. This is particularly true for social pressure, in the form of rewards to fertility or sanctions to childlessness. In the context of the workplace, fertility might even entail opposite effects. Social contagion, in the above definition of the term, is predicated on direct contact with the baby.[3] As ties to colleagues are, on average, less intimate compared to friends or relatives, intense contact with their newborns appears to be the exception rather than the rule. Finally, there are two reasons why the mechanism of social support appears to be equally unlikely to operate among colleagues in the context of the workplace. First, birth events usually imply job leaves, thus disrupting the opportunity structure for supportive interaction at the workplace. Second, colleagues cannot offer day care, the most important form of support to enable mothers' participation in the labor market.

3. Data and sample

An empirical test for social interaction effects on fertility at the workplace places high demands on the data. First, the analysis requires complete information about the network at the workplace (i.e., every single employee of a firm). Second, information on fertility must be available at least on a monthly basis in order to precisely identify the timing of events. The Linked-Employer–Employee data (LIAB) provided by the Institute for Employment Research (IAB) meet both of these requirements (Alda, Bender, & Gartner, 2005; Jacobebbinghaus, 2008). The LIAB combines survey data from the IAB establishment panel with process-generated data from the Federal Employment Agency. These data cover the years 1993–2007 and provide information about a total of 1,845,707 employees in 43,623 firms.[4]

3.1. Identification of birth events

The LIAB data do not allow us to identify birth events directly. It is possible, however, to reconstruct these events through process-generated data on the basis of employers' reports to the regionally responsible social security agencies (these agencies forward the information to the Federal Employment Agency). Employers notify the social security agencies about the duration of an employee's maternity leave. The starting date of a maternity leave corresponds to the date of birth of a child.[5] For mothers, this identification strategy has proven to be very effective (Schönberg, 2009) because around 90% of working women take maternity leave after childbirth.[6] For two reasons, this is even more likely for first-time mothers: first, the

[2] This argument has received support by the findings of Asphjell et al. (2013) who find social interaction effects between colleagues to be parity-specific.

[3] Similar arguments have been advanced by Bernardi (2003) who studied the networks of family and friends. Of course, direct contact with a baby can also occur at the workplace, for instance, if close colleagues visit the mother or the mother takes her child for a visit to the workplace.

[4] This study uses the longitudinal model of the Linked-Employer–Employee Data (LIAB) (Version 3, years 1993–2007) from the IAB. Data access was provided via on-site use at the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB) and subsequently remote data access.

[5] For more information (in German) on maternity leave, see Bundesministerium für Familie, Senioren, Frauen, und Jugend (2012).

[6] Schönberg (2009) shows that this identification strategy is very accurate, as it matches the true month of birth for at least 70% of births. In another 25% of cases, it is over- or underestimated by only one month.

proportion of women taking maternity leave after their first child was born is higher compared to any other parity (Schönberg, 2009). Second, maternity leaves can last up to three years. Since the spacing of births in Germany averages between two and three years (Kreyenfeld, Scholz, Peters, & Wlosnewski, 2010), the birth of a second child frequently occurs during an ongoing maternity leave. Therefore, our procedure primarily identifies first births. Men rarely take maternity leave in Germany (Cornelißen, 2005), which is also reflected in low case numbers in the LIAB data. Therefore, we restrict our analysis to women ($N = 772,379$). Over the entire period of observation (1993–2007), we can identify a total of 15,284 birth events for this population.

3.2. Sample selection

We proceeded in two steps to select an analytical sample. First, we only selected firms of 150 employees or less, averaged over their period of observation. The purpose of this sample restriction was to ensure the possibility of "exposure" to colleagues' fertility. With increasing number of employees within a firm, this becomes increasingly unlikely (Asphjell et al., 2013; Hedström, Liu, & Nordvik, 2008). This exclusion reduced the sample to 132,803 female employees observed in 11,662 firms. Second, our analysis focused on transitions to first birth. As noted above, our identification strategy was already tailored toward first births. Still, this procedure did not rule out higher parities. As we lacked data on parity, we applied a further exclusion criterion aimed at minimizing the probability that a woman had already given birth to a child before she was initially observed in our data. The distributions of age at first birth in Germany provided a rationale to define this criterion. We selected the first quartile of this distribution, that is, the age at which at least three out of four women did not have a child.[7] It is important to note that these first quartiles vary strongly, especially with respect to birth cohort, educational group, and East versus West Germany. As information about these variables was available in our data, we based the assignment of quartiles to each woman on her birth cohort, level of education, and area of residence at the time of first observation. In doing so, we relied on the distributions calculated by Kreyenfeld (2007).[8] After this restriction, our analytical sample consisted of 33,119 female employees in 6579 firms. Across the observation period, a total of 439 birth events were observed in this sample, 304 of which were first births.[9]

In addition to this analytical sample of women "at risk" of experiencing their first pregnancy, we selected a *supplementary sample* including those women who exceeded the first-quartile age threshold (see above) upon their first observation in the LIAB data. The rationale behind this supplementary sample was as follows: although these women were not included in the set of those at risk of a first pregnancy, their birth events might have still been influential for those at risk. Consequently, inclusion of this supplementary sample was required to reconstruct a complete history of birth events within each firm. The supplementary sample added another 297 observations of births which were included as independent events into the multivariate models (see below).

4. Methods

To examine social interaction effects on the timing of first births, we estimated discrete-time hazard models (Allison, 1982) with time-varying variables on a monthly basis. The process time started at age 15 – the time at which employment subject to social insurance contributions is legally possible in Germany.[10] The process ended either with right-censoring at the last month of observation or with an event (i.e., a pregnancy).

4.1. Dependent variable: conception

From a theoretical perspective, the event of interest was not the birth itself but the decision to allow a pregnancy (i.e., to stop using contraceptives). We operationalized the date of this decision by the first month of a pregnancy that led to a live birth (see Lyngstad & Prskawetz, 2010). This point in time equals the date of *conception* and was determined by the notification of a maternity leave minus nine months.[11] We identify the outcome as a binary variable changing its value from 0 to 1 in the month of conception.

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[7] This procedure is even more conservative since we focus on employed women who are known to be less fertile than those who do not work.

[8] Kreyenfeld (2007) presents these distributions for four birth cohorts (1962–1965, 1966–1969, 1970–1973, and 1974–1977); three educational groups from which our data allows us to use two (intermediate and upper secondary school); and two areas of residence (East and West Germany). The age quartiles pertaining to the twelve possible combinations of these three variables constituted our age thresholds. Table A1 in Appendix shows the quartiles. Note that information about birth cohorts did not cover all birth cohorts of the youngest persons included in our sample. We thus extrapolated the age quartiles of the youngest birth cohort (1974–1977) to all following birth cohorts.

[9] As a result of our sample selection, women enter our window of observation early in their careers, at an average age of only 22.4 years. By design of the data, women are only followed up over the duration of their tenure in the job (i.e., specific firm) in which they are initially observed. The average job tenure in our sample amounts to 2.3 years. Considering the national average age at first birth of 29 years (Statistisches Bundesamt, 2013: 93), the young age at first observation is a main reason for the small number of birth events included in our data. Furthermore, it is important to note that the birth events we observe are more likely to be experienced by women who remain in their first jobs for an extended period of time. As shown in Table 1, this pertains mostly to lower educated women.

[10] To protect confidentiality, the data contain only the year of birth. Therefore, we imputed the month of birth of each woman based on a draw from a uniform distribution ranging from 1 to 12.

[11] In additional analyses (not shown), we deducted further three months from this date to allow for time lags between the decision to allow a pregnancy and conception. As this procedure shifted the dependent process to an earlier date, it led to minor changes in time-varying predictor variables. These changes, however, did not affect the substantive results from the multivariate models.

4.2. Independent variables: colleagues' birth events

Previous studies have shown that social interaction effects are strongly related to the time elapsed since exposure to the event of interest (e.g., Asphjell et al., 2013; Balbo & Barban, 2012; Kuziemko, 2006; Lyngstad & Prskawetz, 2010). To account for this, we employed a dynamic modeling strategy, operationalizing social interaction effects by three time-varying dummy variables. These variables indicated whether a birth event of (at least) one colleague from the same firm occurred (i) less than 1 year before, (ii) 1–2 years before, or (iii) 2–3 years before. These three indicators of social influence were defined on the basis of the analytic sample as well as the supplementary sample. Compared to other research designs, the indicators of social influence used in this study have important advantages. Most importantly, they enabled us to exploit very precise (i.e., monthly) process-generated data about complete networks instead of having to rely on subjectively reported measures based on ego-centric survey techniques (Marsden, 1990, 2005).

4.3. Alternative explanations

It is important to take into account two alternative explanations that might resemble the empirical manifestation of social interaction effects: common shocks and selection on unobservables. In the context of our study, *common shocks* are factors that simultaneously affected fertility decisions of all individuals observed in the data. Examples are country-wide changes in social policy that are relevant to fertility such as a rise in childcare benefits, an increase in duration and/or compensation of maternity leave, implementation of family-friendly labor market policy, and so on. Common shocks of this kind could accelerate all individuals' timing of fertility. Empirically, joint responses to these common shocks could resemble time-contingent responses to colleagues' birth events. To control for common shocks, we included period dummies (year fixed-effects) into the models (cf. Asphjell et al., 2013).

Selection effects might occur if women choose to work in firms that match their fertility preferences. If this is the case, women with strong preferences to have children would cluster in firms. In other words, those who plan to have a child in the near future would select family friendly workplaces. Empirically, we might thus mistakenly infer social influence from the observation of frequent birth events within a firm, although each woman's fertility is independent of exposure to her colleagues' birth events. We accounted for selection effects in three ways. First, if certain clusters (i.e., firms) exhibit higher fertility levels, this increase is unlikely to vary over time (Asphjell et al., 2013). In this respect, our dynamic modeling technique using three time-varying dummy variables allowed us to detect the temporal shape of unfolding effects. Second, selection into firms as well as fertile behavior might also co-vary with observed characteristics of women. To account for this, we introduced a number of controls at the individual level. Third, if self-selection into firms depended on other factors, inclusion of a random effect at firm level enabled us to account for the presence of unobserved time-constant characteristics shared by individuals within firms.

4.4. Control variables

At the individual level, we controlled for process time using linear and quadratic terms to allow for a bell-shaped process of transition to pregnancy. The process time starts at age 15 and counts upwards in monthly intervals. As further controls, we included education, wages, employment status, and migration background. Education was measured by two indicator variables for low and intermediate levels of education. High education was the reference category.[12] Wages were measured in Euros per day. Two binary variables controlled for employment status, the first indicating phases of training and the second part-time employment. Migration background was assigned if a woman's citizenship was not German. Finally, we included period dummies for every calendar year of the observation period to control for common shocks.[13] Table 1 provides a descriptive overview of all variables.[14]

4.5. Statistical model

A woman's time-contingent propensity of becoming pregnant is given by the hazard rate λ_{ijt}. Within our discrete-time logistic regression model, the hazard rate is the conditional probability that a first pregnancy of woman *i* in firm *j* occurred at time *t* – under the condition that the woman was still childless.

Our models are organized as follows. We start with a baseline model adding only the indicator variables for social influence as well as the controls for process time and individual characteristics (Model 1). In Model 2, we add period dummies to control for common shocks.[15] Finally, Model 3 adds a random effect to the equation. This final model is specified as follows:

$$\log\left(\frac{\lambda_{ijt}}{1-\lambda_{ijt}}\right) = \alpha_t + \beta_1'\theta_{ijt} + \beta_2'X_{ijt} + \beta_3'\psi_t + \varepsilon_{it} + \eta_j.$$

[12] The category of low education comprises levels of up to GCSE with or without vocational training. Intermediate education refers to the baccalaureate with or without vocational training. Those with high education hold a university degree (or a degree from a university of applied sciences).

[13] There were a few exceptions: in the years 1993, 1994, 1995, 1996, 1998, and 2007, our data included no or less than 10 events of first pregnancy. Therefore, we generated three compound dummy variables encompassing the years 1993–1996, 1997–1998, and 2006–2007. All other calendar years were captured by a dummy indicating only that specific year.

[14] We further tested the robustness of our findings by adding controls at firm level (number of staff, share of female staff, share of employees in part-time work, sector (private/public), and presence of a works council). All findings reported in Section 5 were robust to these controls, and none of them added explanatory power to the model. Because of large shares of missing data, however, inclusion of these controls reduced our analytic sample by more than 50%. Therefore, we only present the more parsimonious specification.

[15] As employees are clustered within firms, we calculate robust standard errors.

In this model α_t denotes the linear and squared process time; $\beta_1'\theta_{ijt}$ are vectors for the three social influence dummies for woman i in firm j at time t; $\beta_2'X_{ijt}$ are vectors for individual characteristics; $\beta_3'\psi_t$ denotes the period dummies controlling for common shocks (Model 2); finally, η_j is a random effect shared by all colleagues within a firm j (Model 3). This random effect captured unobserved time-constant factors that colleagues shared within a firm, indicating time-constant unobserved differences between firms.

5. Results

5.1. Main findings

Table 2 shows the estimated coefficients of the three multivariate models. The indicators for process time showed the expected bell-shaped curve in all models. The transition rate to parenthood reached its maximum at approximately 29.3 years (Model 1).[16] The coefficients of our three key explanatory variables indicated social interaction effects of fertility at the workplace.

Model 1 provides clear evidence for the presence of social interaction effects.[17] In the year after a colleague had a child, the transition rate to pregnancy increased markedly. In the second year after exposure to a birth event, this positive effect declined somewhat but remained sizable and statistically significant. The coefficient for the third year indicated a further decline and was no longer different from zero at conventional levels of statistical significance. This temporal shape of the social interaction effect on fertility at the workplace was consistent with theory as well as previous results (Balbo & Barban, 2012; Asphjell et al., 2013; Kuziemko, 2006; Lyngstad & Prskawetz, 2010). Importantly, such a pattern is very unlikely to be observed in the presence of selection effects (see Section 4.3). In Model 2, we tested the alternative explanation of common shocks by introducing period dummies to the equation. Under control for common shocks, the coefficients of our social influence variables declined somewhat in magnitude but remained sizeable and statistically significant. Finally, Model 3 accounted for time-constant unobserved factors by including a random effect. This coefficient was positive and statistically significant, indicating the importance of shared factors at the workplace that were not observed in our data. The size of the coefficients of our social influence variables declined further after inclusion of the random effect. Despite this reduction, however, we still observed clear evidence for social interaction effects in the first and second year after a colleague had a child. Overall, the robustness of these coefficients increased confidence in the finding that fertility spreads among female colleagues at the workplace.

Fig. 1 provides an illustration of our main findings, showing the estimated increase of the transition rate (in percent) for the three key predictor variables in each of the models. In view of possible alternative explanations, Model 3 can be regarded as providing the best estimation of social interaction effects on fertility at the workplace. As shown in the right panel of Fig. 1, the transition rate doubled in the first year after colleagues' birth events (CI: 1.4; 2.9). In the second year, this point estimate still amounted to 1.6 (CI: 1.1; 2.4), whereas in the third year, the estimated increase by a factor of 1.2 was no longer significantly different from zero (CI: −0.2; 2.0). To illustrate, the increase in the transition rate found in the first year after a colleague had given birth corresponds to the increase in the baseline transition rate from age 20 to 24, controlling for other factors.

Our results on the controls for individual characteristics were consistent across all models and largely in line with previous research. First, women from East Germany had higher transitions rates to pregnancy, corroborating findings from previous studies (e.g., Bundesinstitut für Bevölkerungsforschung, 2012; Kreyenfeld et al., 2010). The negative coefficient of wages is consistent with opportunity costs of children (Kreyenfeld, 2010). Controlling for wages, the indicator variables for education did not show significant effects. As expected, the transition to parenthood was less likely during phases of training (Blossfeld & Huinink, 1991; Kreyenfeld, 2010). Finally, part-time employment, even under control of wages, significantly decreased the transition rate to pregnancy. This finding is consistent with increased economic uncertainty associated with this type of employment (Kalleberg, 2000; Mills & Blossfeld, 2003; Vignoli, Drefahl, & De Santis, 2012).

5.2. Additional analyses

In additional analyses (not shown), we tested (a) whether the social interaction effects varied with firm size and (b) whether these effects transpired primarily between similar colleagues, as expected by the mechanism of social learning.

Regarding firm size, Hedström et al. (2008) argued that there are better opportunities to meet and interact with colleagues in smaller firms, suggesting greater effects of the indicator variables for social interaction. To test this contention, we fitted Model 3 to a sample of women in firms not larger than 100 employees (N = 24,762 women in 5286 firms) and, even more restrictively, to women in firms not larger than 50 employees (N = 12,905 women in 3392 firms). In both analyses, we were able to reproduce the substantive findings presented above. Moreover, the effect of the first year after a colleague gave birth was slightly larger (6% increase) in firms of up to 100 employees but considerably larger (37% increase) in firms with up to 50 employees. The findings from these additional analyses are thus consistent with theoretical

[16] This maximum was calculated by the first derivative of the quadratic function of process time. This estimate is slightly higher compared to recent national level data on mean ages of women giving birth to their first child. The mean ages at motherhood in Germany were 28.9 in 2009, 29.0 in 2010, and 29.1 in 2011 (Statistisches Bundesamt, 2013: 93). National-level data on mean ages at different parities are not available prior to 2009.

[17] The moderate reductions in case numbers compared to the size of the analytic sample resulted from missing values (listwise deletion). Because our analysis was based on process-generated data, listwise deletion is unlikely to bias results as the process generating missing data can be assumed to operate randomly (i.e., missing completely at random).

Table 1
Descriptive statistics ($N = 33{,}119$ women).

	M	*SD*	Min.	Max.	Person months
Pregnancy (/100)	0.03		0	1	916,347
Indicators of social influence[a]					
01–12 months	0.06		0	1	916,347
12–24 months	0.04		0	1	916,347
24–36 months	0.03		0	1	916,347
Control variables					
Process time: month[b]	115.15	62.03	1	329	916,347
Wage[c]	42.01	30.62	0	1478	911,278
East German (ref.: west German)	0.43		0	1	916,347
Migrant (ref.: native)	0.03		0	1	910,865
Education (ref.: high)[d]					
Low	0.80		0	1	886,693
Intermediate	0.13		0	1	886,693
Employment position (ref.: full-time)					
In training	0.29		0	1	911,268
Part-time	0.19		0	1	911,268

Note: Data are from LIAB Version 3 (1993–2007); unweighted. Data are on a monthly basis.
[a] At least one colleague had a child within the respective interval.
[b] Process time starts at age 15 and ends at first pregnancy with an event or is right-censored at the last month of observation.
[c] Daily wage in Euros.
[d] Low = up to GCSE with/without vocational training; intermediate = baccalaureate with/without vocational training.

expectations regarding social interaction effects on fertility and increase confidence in the results presented above.

In a second set of additional analysis (not shown), we aimed to cast more light on the mechanism of social learning, arguably the most convincing explanation for social interaction effects on fertility at the workplace. As argued in the theoretical background, social learning is more likely if ego perceives a colleague who recently had a child as being similar. We operationalized perceived similarity by age – a strategy which has been applied in previous research (Asphjell et al., 2013; Kuziemko, 2006). Similarity in age refers to life course phases that reflect similarities in the conditions surrounding transitions to parenthood. Empirically, we defined two women as being

Table 2
Discrete time logistic regression models for the transition to motherhood ($N = 32{,}999$ women).

	Model 1	Model 2	Model 3
Social influence			
Colleagues' birth event(s) occurred within the last...			
01–12 months	1.18 (0.18)***	0.99 (0.17)***	0.71 (0.19)***
12–24 months	0.73 (0.23)**	0.70 (0.22)**	0.47 (0.21)*
24–36 months	0.34 (0.18)	0.45 (0.17)*	0.21 (0.24)
Process time			
Month (/10)	0.31 (0.06)***	0.24 (0.06)***	0.25 (0.06)***
Month^2 (/1000)	−0.09 (0.02)***	−0.06 (0.02)**	−0.06 (0.02)**
Period dummies	No	Yes	Yes
Individual characteristics			
East German	0.41 (0.14)**	0.46 (0.14)**	0.43 (0.15)**
Migrant	−1.33 (0.66)*	−1.29 (0.66)	−1.34 (0.72)
Wage (/10)	−0.11 (0.03)***	−0.10 (0.03)***	−0.09 (0.03)**
Education (ref.: high)			
Low	−0.22 (0.20)	−0.18 (0.20)	−0.22 (0.22)
Intermediate	−0.37 (0.26)	−0.34 (0.26)	−0.37 (0.27)
Employment status			
In training	−1.41 (0.24)***	−1.48 (0.25)***	−1.36 (0.29)***
Part-time	−0.49 (0.18)**	−0.47 (0.18)**	−0.46 (0.17)**
Constant	−9.58 (0.54)***	−8.72 (0.53)***	−9.10 (0.56)***
N (person months)	886,280	886,280	886,280
N (events)	294	294	294
Log likelihood	−2521.44	−2469.81	−2460.72
σ_u			0.79 (0.14)
ρ			0.16 (0.05)***
χ^2	282.31	318.58	195.28

Note: Data are from LIAB Version 3 (1993–2007); unweighted. See Table 1 and text for details on the variables. Firm clusters: 6230. Robust standard errors in parentheses.
* $p < 0.05$.
** $p < 0.01$.
*** $p < 0.001$.

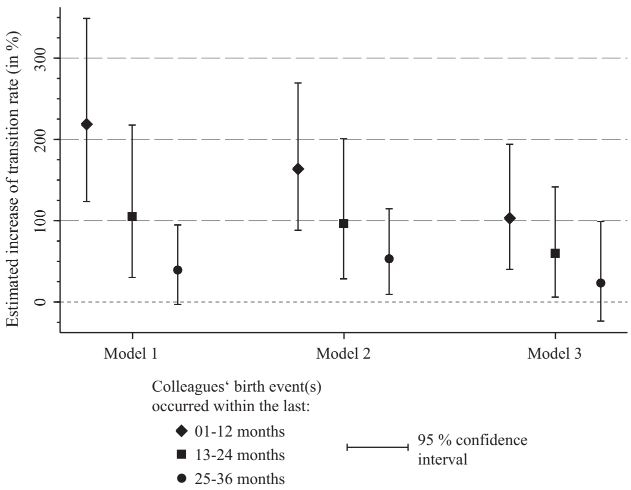

Fig. 1. Social interaction effects. *Note*: Data are from LIAB Version 3 (1993–2007); unweighted. Model 1 includes process time and individual characteristics; Model 2 includes process time, individual characteristics and common shocks. Model 3 includes process time, individual characteristics, common shocks, and a random effect at firm level. The three time intervals pertain to the main independent variables capturing colleagues' influence on fertility. The dummy variables equal 1 if at least one colleague had a child within the respective interval.

similar if their birth dates were included within an interval of two years. Conditioning our three key predictor variables on age similarity and estimating their effects based on the specification of Model 3, we found evidence consistent with the mechanism of social learning. The increase in transition rates in the year after an age-similar colleague gave birth was about 30% larger compared to a colleague who was more than two years older or younger. For both groups of colleagues we found the same temporal pattern of social interaction effects.

6. Discussion

Does fertility spread among colleagues at the workplace? To answer this question, this study used monthly-based data from the LIAB, testing whether the transition rate to pregnancy increased in the years after colleagues had experienced birth events. The analysis provided strong empirical support for social interaction effects, indicating increased rates in the first year and, to a lesser extent, in the second year after colleagues' birth events. These effects were more pronounced in smaller firms and proved to be robust to alternative explanations of common shocks and selection.

How can we explain these effects? Based on previous qualitative research, we argued that social learning constitutes the most relevant mechanism mediating social interaction effects on fertility in the context of the workplace. In this sense, fertile colleagues exert influence as social models that change previous beliefs about the feasibility and consequences of having a child, thus inducing a learning process in childless women. The effectiveness of this learning process, and therefore the strength of social influence emanating from the role model, is expected to increase with perceived similarity. Our findings were consistent with this expectation although the LIAB data allowed only for crude approximations of similarity such as age. Relying on age as the operationalization of similarity, of course, we were unable to disentangle social learning from related principles such as conformity. Hence, colleagues similar in age might also constitute the relevant sub-group in which women are primarily embedded and to which they are (unconsciously) adapting. Once more detailed data are available, further analyses based on the idea of similarity between colleagues might advance our understanding of the mechanisms behind social interaction effects on fertility at the workplace.

More detailed data are also required to improve the generalizability and analytical precision of the results reported in this study. For instance, our lack of information about parity necessitated rather extensive sample restrictions and did not allow us to test whether our results can be generalized to higher parities (see Asphjell et al., 2013; Lyngstad & Prskawetz, 2010).

Despite these limitations, our study yields a number of important contributions. First, we have presented one of the first quantitative assessments of social interaction effects on fertility. In doing so, our study not only corroborates findings from the qualitative literature but also offers a measure of the strength of these effects. Indeed, our findings suggest not only that childbearing spreads among colleagues at the workplace but also that these interaction effects represent crucial factors in the overall process transition to parenthood. In view of this, we consider it important that sociological perspectives on social interaction effects enter the current debate about low fertility rates in Germany. As decisions to have a child are taken under considerable uncertainty (Kreyenfeld, 2010), the experience of social contacts appears to be particularly relevant. Our findings suggest that employed women who have a child may cause chain reactions among their colleagues. Considering such social multiplier effects, social policies aimed at improving the reconciliation of work and family might thus gain additional momentum in stimulating fertility.

At a more general level, our study contributes to the developing literature investigating social interaction effects on fertility not only at the workplace (Asphjell et al., 2013; Ciliberto et al., 2012), but also in other networks, such as the family (Kuziemko, 2006; Lyngstad & Prskawetz, 2010) and the circle of friends (Balbo & Barban, 2012). For future research along these lines, we see at least two important directions. First, we still know relatively little about the mechanisms mediating social interaction effects on fertility. Second, current studies mostly restrict the analysis to only one network. A simultaneous account of different networks of varying size and quality of social relationships (interaction frequency, intimacy, etc.) could provide additional insight showing, for instance, which social contacts are most influential and in which networks social interaction effects are most important from a quantitative point of view (see Hedström et al., 2008). These designs are also well-suited to address potential spillover effects across different domains of interaction. This way, social interaction effects on fertility could be examined not only at the workplace, in the family, in the neighborhood, or in the circle of friends, but also across network boundaries.

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Appendix

Table A1
First quartiles of age at first birth (Kreyenfeld, 2007).

Birth cohort	Secondary school			
	Intermediate		Upper	
	East	West	East	West
1962–1965	20	25	21	28
1966–1969	20	25	24	28
1970–1973	21	25	26	29
1974–1977	23	25	27	29

Note: The cells represent the respective first quartiles of the distributions according to specific combinations of birth cohort, educational group, and East versus West Germany calculated by Kreyenfeld (2007: 109–112). In our definition of indicators based on the LIAB data, intermediate secondary school equals low education and upper secondary school equals intermediate education.

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