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Fertility and Social Interaction

A Simulation Approach

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Introduction

Are people's decisions about when they have their first babies influenced by the people around them? And if so, how strong is this influence? Typically, decision-making about the transition to parenthood invokes career prospects, feelings of maturity or security, et cetera. But could it be that the people around us—our parents, our colleagues, our siblings—are a source of influence (intentionally or unintentionally), too? Let us consider the following three (cherry-picked) statements from narrative interviews carried out in Germany and Italy to get a first impression:

> "I have a sister who is three years older than I, and she has now two children, my godchildren... And I realize that I always look forward to meeting them, that I am often in contact with them and always try to be there at their crucial experiences [...] And I do realize that I would somehow also like to have this." (Keim 2011: 175)

> "I would like to have a child, because I feel that I miss it, I feel it today [...]. *What made you change your mind?* Well, most likely the birth of this child close to me. He is six weeks old." (Bernardi 2003: 546)

> "Ideally, I'd give the child to my sister or my mother or my mother-in-law. So, ideally within the family." (Keim 2011: 178)

These statements suggest the answer to be "Yes"—and if empirical evidence from surveys with thousands of respondents backed this up, the "Yes" would be even bolder. As soon as knowing this, of course, we would also like to know how strong their influence is at the population level.

Why is this so important to know? Worldwide we witness a delay of the transition to parenthood to ever higher ages. Given that we find evidence for what is indicated especially in the second quote above, people's fertility decisions may influence other people to have their first child earlier, would mean that this acts as a counter-force to the negative trend of delaying first childbearing. Thinking one step further, the indirect effect of having a first child later means that people might end up having less children altogether because of biological restrictions at higher ages. In this sense, indirectly, those interpersonal fertility-relevant effects may be seen to not only slow down the delay in first childbearing but to slow down the general population decline. Another important implication of this is that the potential for these effects to emerge may be coupled to circumstances in different spheres of social life, which may not be obvious at first sight. The third quote above gives the impression that the availability of (grand-)parents for child care may influence future parents' fertility decision-making. If for this availability people would tend to have their first child earlier, then decreasing (grand-)parental availability, e.g., by raising retirement ages or reducing institutionalized care for frail people in old-age, would again negatively affect the size of entire birth cohorts, both directly and indirectly.

To advance our understanding of these interpersonal fertility-relevant processes, I combine theoretical arguments and methodological approaches from both sociology and demography. More precisely, providing answers to the following two questions will be subject to the next 100 pages.

(1) Do the people we interact with have an effect on when we have our first child?

(2) How strong is this influence at the population level?

To address the first question, I specified models that statistically test for the influence of three interaction partners (colleagues, siblings, parents) based on large-scale German survey data. To address the second question, I converted the hard-to-grasp coefficients of these models into easy-to-understand birth counts by using a microsimulation.

What Does Fertility-Relevant Social Interaction Mean?

In a nutshell, what I name fertility-relevant social interaction effects is the idea that individuals' fertility decision-making is not independent of each other. The fertility decision (or more general, a specific characteristic) of one individual may influence, positively or negatively, the fertility decision of another individual. This straightforward idea may manifest in different ways but at the same time lead to very comparable structural outcomes.

For example, on the one hand, it may be thought of as a contagion process. One woman may give birth to a child and her decision about the timing of her birth may influence the timing of the birth of a child of another woman she interacts with. Ad ultimo, at least theoretically, one may envision this leading to a cascade of birth events in which multiple birth events would not have been timed the way they were timed without them being sequentially influenced by each other (Richter 2016; Arránz Becker

& Lois 2013). On the other hand, it may be thought of as an imitation behavior in which the presence of parents among the people surrounding an individual (i.e., the share of parents among the interaction partners) moderates the likelihood of having a child during a certain time-span (Kotte & Ludwig 2011).

In both cases, each stylized group of individuals moved from having fewer to more children over time. The argument of social interaction effects is that these groups, observed at an arbitrary point in time, would not have given birth to as many children if each individual would have decided upon the timing of having a child without taking into consideration the timing at which the individuals around them had their children.

This simplified exposé of the process of social interaction effects on fertility will be refined throughout this introduction. Besides providing a picture that is easy to comprehend, its purpose is to point out three aspects that will become important to understanding fertility-relevant social interaction effects. First, individuals' fertility decision-making may be influenced by a multitude of persons, such as parents, colleagues, neighbors, or friends, depending on how individuals' specific social networks are composed. Second, the influence exerted may come in various forms, or put differently, may unfold through different social mechanisms, such as learning or emotional contagion. This also means that the influence may be more or less overt. It may range from more or less unconscious reactions to very explicit demands for children or information gathering about childbearing. Third, the influence of others' fertility decision-making may be regarded as an unintentional consequence of their fertility decision-making—by having a child (i.e., moving to a higher parity), they (over time) unintentionally increase their social networks' overall fertility levels.

The idea that fertility behavior may be influenced by social interaction has only recently sparked demographers' interest. For decades, demographic research concentrated on socio-economic as well as normative factors to explain fertility behavior and, consequentially, fertility differentials between social groups or regional entities (Balbo et al. 2013). Social interaction only entered demographic inquiries when socio-economic factors reached their explanatory limits (Bongaarts & Watkins 1996). Since then it has been increasingly taken into consideration.

1.1 HISTORY AND STATE OF RESEARCH

The introduction of (fertility-relevant) social interaction effects to demography may most clearly be found in the seminal work by Bongaarts & Watkins (1996). Based on classical demographic transition theory[1] (Notestein 1945), which provides sets of descriptions about relationships between birth and death rates and industrial development, differences in the socioeconomic development between a number of developing countries since the 1960s were not sufficient to explain the differences in their fertility levels. To assess this puzzle, the authors introduced the idea that the diffusion of information about contraceptive use or lifestyle concepts differed between the countries and, hence, this accounted for the differences in fertility levels. Understanding social interaction effects as the outcome of a diffusion process of fertility-relevant information, particularly about contraceptive use, has guided research on the fertility decline especially in a large-scale project for Sub-Saharan Africa (Montgomery & Casterline 1996; Rosero-Bixby & Casterline 1993) but also for Thailand (Entwisle et al. 1996). The diffusion of lifestyle concepts, more general, has been featured in research about the fertility decline in Japan (Rindfuss et al. 2004).

How did the diffusion unfold? This stream of research introduced two mechanisms of social interaction that may lead to diffusion, social learning and social influence. Social learning refers to the process by which individual perceptions of relevant aspects of the fertility decision are changed by new information obtained from interaction partners. In other words, this newly obtained information reduces the uncertainty involved in the decision-making process about when (timing) and how many (quantum) children individuals want to have. Social influence is less clearly defined in this research and rather points to the idea that relevant others with whom people interact may have a direct impact (positive or negative) on the adoption of the diffusing information and thereby the fertility behavior. This stream of research found evidence for both mechanisms (Kohler 2001; Kohler et al. 2001).

While those authors focused on the aspect of diffusion of information in developing countries, social interaction entered demographic analyses

[1] For a sociologist, the term theory may seem a bit overstated here and this has been subject to considerable debate, or as Kirk (1996: 361) puts it: "Demography is a science short on theory, but rich in quantification."

on post-socialist countries as the idea of being a buffer against exogenous impacts of turbulent times. When the soviet union fell apart and people lacked the feeling of security, one of the most important preconditions for starting or enlarging a family, researchers asked whether it was the people with whom one interacts that provide this security in the form of social support (Bühler & Fratczak 2007; Bühler & Philipov 2005; Philipov et al. 2006). This research conceptualized social interaction effects primarily as multi-purpose social capital and highlighted the transfer of resources such as money or time on the basis of reciprocal exchange. It showed that familial ties were more important than having access to financial resources. The time transfers families provide may be interpreted as a buffer against the uncertainties and turbulences these times of state-wide transformation brought about.

Qualitative Research

In developed countries like Germany (Keim 2011) and Italy (Bernardi 2003), where contraceptives are available to everyone and where no far-reaching politicial transformations are currently in place, research on social interaction effects on fertility decision-making was more about exploring who the influential interaction partners are and through which mechanisms their influence unfolds (Bernardi & Klärner 2014; Bernardi et al. 2007; Keim 2011; Keim et al. 2009, 2013). Concerning the first question, they empirically identified parents, siblings, friends, colleagues, and extended kin as generally being considered interaction partners in individuals' fertility decision-making. Their findings suggest that parents and siblings are the most important interaction partners whereas colleagues seem to be less important. However, as typical for qualitative research, their case numbers remained rather low, which makes it difficult to generalize their findings beyond their samples. In other words, it is almost impossible to state which interaction partner's influence is stronger, especially at the population level. Let alone the question whether respondents overestimate the impact of certain interaction partners and potentially not recognize they were influenced by others. This may more clearly be seen with quantitative research. Concerning the second question, they provided thorough descriptions of the mechanisms through which social interaction effects may transpire. The *mechanisms* qualitative research identified are named social learning, social support, social pressure, and emotional contagion. The latter three may be seen as more fine-grained versions of the above mentioned mechanism of social influence. Throughout this dissertation I will draw on these four mechanisms (elaborating on them in reference to the specific context of the particular interaction partner) as they are the most appropriate and the most detailed account of mechanisms of fertility-relevant social interaction.

Social learning does not deviate too much from the way described earlier. Again, it refers to the process by which information obtained by observing (significant) others affects fertility decision-making, e.g., by changing attitudes concerning certain aspects of the decision. This process is thought to reduce uncertainty and the transition to parenthood as a crucial life-course transition features more uncertainty than transitions to higher order parities (having the second, or the third child). Therefore, this mechanism seems especially relevant for the transition to parenthood (Lois & Arránz-Becker 2014). In this regard, peers or siblings who recently became parents may act as role models (Bandura 1994) from whom individuals may abstract and infer on their own situation (Keim 2011). According to Bernardi (2003), this mechanism is particularly effective if the interacting individuals share similar contexts as the fit of the role model and oneself is closer. That being said, the learning process itself may appear in a variety of different forms. It ranges from small observations to lengthy discussions about the experiences of a pregnancy or evaluations of how children affect the partnership, leisure, or working life (Keim 2011). *Emotional contagion* refers to emotional reactions that individuals are not necessarily aware of. In this regard, e.g., childless siblings getting into direct contact with another sibling's newborn, or a small child more general, may become emotionally aroused, which may intensify their desire to have a child themselves (Bernardi 2003; Keim et al. 2013). *Social support* highlights that the opportunity to receive financial, instrumental or emotional support reduces the costs of childbearing and therefore eases the decision to have a child (Liefbroer 2005; Keim 2011). *Social pressure* influences fertility decision-making by means of sanctions and rewards. A typical example is parents expressing their wish to have a grandchild.

Quantitative Research

Partly drawing on this qualitative research, the scarce quantitative efforts using large-scale data centered on estimating the prevalence of social interaction effects of different interaction partners. As these endeavors place high demands on the data, for the most part, these effects have been estimated separately for specific types of interaction partners. If we imagine a dataset that would allow for testing multiple interaction partners at the same time this would mean to survey the focus respondents, their siblings, colleagues, parents, and extended kin. Unfortunately, obtaining data in this comprehensiveness is very unlikely due to survey non-response (Rossier & Bernardi 2009).

So far, quantitative evidence on the positive effects of siblings (Kuziemko 2006; Lyngstad & Prskawetz 2010; Chapter III), parents (Arránz Becker & Lois 2013; Hank & Kreyenfeld 2003; Thomése & Liefbroer 2013; Chapter IV), friends (Balbo & Barban 2014; Kotte & Ludwig 2011; Lois 2016) and colleagues (Asphjell et al. 2013; Pink et al. 2014 or Chapter II) has been gathered for Germany, Norway, Sweden, as well as the United States. Importantly, it has to be noted that oftentimes providing quantitative evidence for social interaction effects by a certain interaction partner has not been the primary aim of the research mentioned above. It rather had the form of a by-product or originated from a control variable used to more precisely estimate another effect in a statistical model. In addition, there also have been attempts to investigate interaction partners as conjunct groups in the form of so-called "relevant others", where the authors did not differentiate between different types of relationships (Lois 2016; Lois & Arránz Becker 2014). If it was their primary aim to test for evidence of a social interaction effect from a particular interaction partner, methodologically, the aforementioned studies typically make use of or approximate some form of discrete-time event history analysis (Allison 1982), in which they estimate whether the preceding birth of a child from an interaction partner has a positive impact on the transition rate to parenthood, or even the second birth. More precisely, they also investigate the time-shape of this impact and show that the impact of the birth of an interaction partner's child fades over time. In a few studies, efforts have been made to also test for the mechanisms through which the identified influence unfolds (e.g., Lois & Arránz Becker 2014). However, separating the mechanisms empirically is very difficult because although they may be theoretically distinct, specifying situations empirically in which rather one mechanism and not the other would be expected is not straightforward at all. That being said, quantitative research is currently more concerned with establishing empirical evidence of social interaction effects in general, prior to empirically disentangling the mechanisms through which they unfold.

Agent-Based Modeling Approaches

In addition to these, with regard to their statistical methodology, rather conventional approaches, in recent years, social interaction has also been investigated with a simulation technique called agent-based modeling (Macy & Willer 2002). This method has been advocated for in demographic inquiries (Billari & Prskawetz 2003; Billari et al. 2006; Grow & Van Bavel 2017). In a stylized way, it may be seen as an additional tool extending the widely applied technique of microsimulation by agency. Microsimulation (Billari et al. 2003) is widely used in demographic inquiries, especially for forecasting purposes of specific characteristics of populations (e.g., size or age structure). For a microsimulation to be applied, all it takes is transition rates according to which agents in the simulation make a transition to another state in the state space (e.g., having the first child or having the second child) according to a random number generator that attributes agents to these states. These models are heavily data driven. Agent-based models, in their extreme cases, may be purely theoretical based on decision rules for the agents according to which they act or retain the status quo. However, as agent-based models simply constitute a computational model (for which, typically, one mathematical solution does not exist) they may be calibrated using empirical data. Calibration in the jargon of agent-based modeling means that each agent within such a model will be equipped with a value from externally derived distributions of data from, e.g., official statistics, such as an specific age. This calibration element of agent-based modeling brings it conceptually closer to microsimulation and the concrete applications are not always straightforward to assign to either one (e.g., Massey & Zenteno 1999). Following Bijak et al. (2013), the most important distinguishing factor between the two is the agency of the actors. If they decide upon (albeit very simple) rules, it is rather an agent-based model, if they follow “blindly” in their decisions upon previously calculated probabilities, it is rather a microsimulation.

Agent-based modeling as such is not so new. But the repercussions of the calls for the study of social interactions now also transcend into both more applied and more conceptual accounts of how to structure demographic research, basically by highlighting ideas long known in sociology (Billari 2015), or, how some scholars would rather phrase it, re-known within the field of sociology (Boudon 2012). Agent-based modeling studies dealing with social interaction and fertility decision-making are scarce but recently gaining momentum. To the best of my knowledge, only three exist and I will sketch out all of them in the remainder of this section. Although reading the following is not necessary for understanding this dissertation, it may help some readers understand this dissertation better. The reason is that I do not build directly upon these agent-based models but we share a common goal, to shed light on the nexus between social interaction and fertility, and therefore they are part of the research stream to which this dissertation contributes to.

Aparicio Diaz and colleagues (2011) used one-sex simulation models calibrated by Austrian census data to examine whether fertility decisions of “relevant others” influenced transitions to parenthood. This study showed that the transition rate to motherhood increased with an increasing share of network members who had children. Social networks (individual-specific sets of "relevant others", or peer groups) were endogenously generated based on three agent characteristics, age, intended education, and parity. The strength of the influence of the social network follows an s-shape and was operationalized following an comparison of the agents’ own parity with that of the others’ fertility. For example, a childless woman calculates the share of mothers within her network and then is subject to stronger influence if the share of mothers within the network is higher. Modelling social interaction effects this way reminds of the mechanisms of social influence outlined earlier, from the demographic literature on diffusion information about contraceptives. Through modeling these social interaction effects the authors were able to trace the shift of first-birth probabilities in Austria during the period 1984 to 2004.

Fent et al. (2013) investigated the impact of family policies on both individual fertility decisions and aggregate fertility levels using an empirically calibrated agent-based model based on data from the Generations and

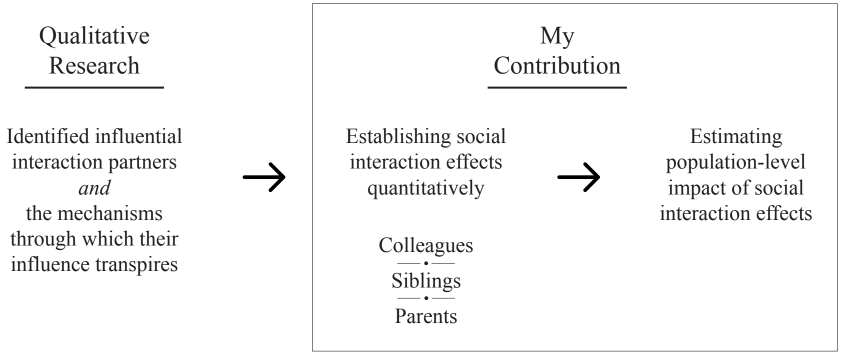
Gender Survey for Austria. They were able to separate the direct impact of family policies (i.e., fiscal alleviation) from the indirect effect of such policies due to diffusion processes. They showed that social interaction effects may positively reinforce the already positive impact of family policies both for completed cohort fertility and intended fertility by closing the gap between intended and realized fertility. However, the impact of differences in operationalizations of the social structure through which the diffusion may unfold are smaller than differences in the design of family policies.

Seizing the idea that social interaction may matter during fertility transitions (Coale & Watkins 1986), González-Bailón & Murphy (2013) designed an agent-based model to study the role of social interaction as an accelerating force in the French fertility transition over the nineteenth century (1831–1921). The decline in fertility in France during this period, especially with population-level differences across the different départements, was suspected to indicate a pattern of spatial diffusion, which suggested processes of social interaction. They found that introducing social interaction (via social learning and social pressure, also with regard to changes in religious beliefs during the French Revolution) led to more accurate reproductions of the actual demographic history in their simulation.

1.2 CONTRIBUTION OF THIS DISSERTATION

In the larger context of research on social interaction effects and fertility decision-making, important interaction partners have been identified, the mechanisms through which their influence unfolds have been specified, and it has been shown that social interaction effects may be the reason for accelerated fertility transitions across entire countries. But what is missing in this literature is a tangible measure of how strong the impact of social interaction effects on fertility is. From qualitative research alone, we may perhaps overestimate the influence as we may not extrapolate from the interviews to the entire population as the interviewees may themselves have subjectively overestimated the influence of particular interaction partners. Or we may even underestimate the impact if the influence operates more unconsciously to the people, as outlined earlier with regard to emotional contagion. Again, how strong are they? Is their impact small? If so, we might marginalize research on this source of influence on fertility. Is its impact large? If so, it would be beneficial to devote more research efforts

Figure 1.1. Contribution



to it to gain a better understanding. We do not know. Until now. In this dissertation, my aim is to provide a good estimate for how strong social interaction effects on fertility at the population level may be.

Broken down, this dissertation's aim is very basic, to provide one number, a birth count. This one number, of course, can not to be calculated out of nothing, so I carried out much preparatory work before, and on this way I contribute to several other aspects of the literature. Figure 1.1 shows how to locate the efforts of this dissertation in an idealtypical (sociological research paradigm) manner. On the left side, previous qualitative research is shown that both informs and guides my quantitative research. Based on their findings about who are the influential interaction partners I carried out large-scale empirical research on people's colleagues, siblings, and parents.[2] To them, I devote Chapters II–IV. All three chapters concentrate on women's transition to first birth. From these three chapters, I extract several coefficients from the statistical models and then plug them into a microsimulation to counterfactually estimate how many fewer first children would have been born at the population level if every woman would have made the decision independently, i.e., under the scenario that social interaction effects would not exist. This one number represents the strength of social interaction effects I want to provide. The next two sections outline how I proceed to achieve this.

[2] Unfortunately, an empirical large-scale analysis on friends was not feasible because of a lack of suitable data.

Establishing Quantitative Evidence of Social Interaction Effects on First Births

For Germany, as shown before, quantitative empirical evidence for social interaction effects on fertility remains scarce. Evidence is available on the positive influence of parents (Arránz Becker & Lois 2013; Hank & Kreyenfeld 2003) on women's transition to have the first child but it was not these studies' main aim to estimate parental influence. Furthermore, Kotte & Ludwig (2011) showed cross-sectionally that not siblings but friends have a positive influence on the transition to the motherhood. Other research (Lois 2013, 2016; Richter et al. 2012) showed that the share of mothers within the circle of "relevant others" influences the transition to parenthood in Germany. Taken together, quantitative evidence for social interaction effects is far from established. For it's establishment, I want to contribute by exploiting the most comprehensive and up-to-date data sources available. For siblings and parents, I use the Panel Analysis of Intimate Relationships and Family Dynamics (pairfam; Huinink et al. 2011) and for colleagues the linked-employer-employee data (LIAB) provided by the Institute for Employment Research (IAB) at the Federal Employment Agency. All my models are based upon females only.[3]

The method I use to test statistically for the existence of social interaction effects is based upon previous international research that tried to identify social interaction effects for specific social interaction partners (Balbo & Barban 2014; Lyngstad & Prskawetz 2010). They used discrete-time event history analysis using logistic regressions (Allison 1982) to test whether the birth of a child to a sister or a friend, up to three years before, increased their sisters' or their friends' transition rates to parenthood. In general, the logistic regression models I estimate are following the logic specified in the equation below

$$log\left(\frac{\lambda_{it}}{1-\lambda_{it}}\right) = \alpha + r_t + r_t^2 + \beta_1 x_{it} + \beta_2 z_{it} + \epsilon_{it}. \tag{1.1}$$

The transition rate λ_{it} is the dependent variable and denotes the transition to motherhood for a woman i at time t. I model the time-dependency of the transition process in a parsimonious parametric form, the quadratic

[3] Social interaction effects on men have not yet been investigated. In general, men are only being slowly introduced into demographic research on fertility decision-making in the widest sense.

form of time, $r_t + r_t^2$, leading to a bell-shaped transition curve in each model. The process time starts at age 15. The term $\beta_1 x_{it}$ constitutes the vectors and the coefficient of the operationalization of the social interaction effect, which may differ between the chapters. The exponentiated coefficients from the individual models will be employed in the microsimulation in Chapter V. The term $\beta_2 z_{it}$ entails the vectors of control variables used to estimate the β_1 as precisely as possible. ϵ_{it} denotes the error term for women i at process time t.

Chapter II uses LIAB data covering the years 1993–2007 with a sample size of more than 33,000 female employees and finds a social interaction effect within two years after a colleague gave birth. Chapter III uses pairfam data covering the years 2008–2012 with a sample size of almost 2,000 females and finds a social interaction effect within the two years after a sibling gave birth. Chapter IV differs from the other two because parents (typically) may not experience birth events while their children think about childbearing. This chapter uses pairfam data[4] with a sample size of over 3,000 females and identified a positive social interaction effect. Daughters with their mothers nearby may anticipate future child care support and therefore have their first baby earlier as well as those daughters feeling social pressure to have their first child also had their first child earlier.

The effect size estimates extracted from these three chapters show the percentage increase in the transition rate to the transition to parenthood, calculated by exponentiating the coefficients of the discrete-time hazard models (i.e., e^{β_1}; see Figure 5.1 on page 74). For colleagues, a birth event of a colleague almost doubles the transition rate. For siblings, a birth event of a sibling increases the transition rate by around 1.5. For parents both social support and social pressure increase the transition rate by around 1.75.

Quantifying their Population-Level Impact

Importantly, however, the effect size estimates presented in Chapters II–IV do not provide a straightforward assessment of the impact of social inter-

4 Although the same dataset was used for two different interaction partners, it was not possible to analyze the effects of both siblings and parents jointly. The reason is that Chapter III is based on retrospective information from the fifth wave of pairfam and Chapter IV uses panel information from six waves of pairfam.

action effects on fertility. More clearly, the effect sizes allow for statements of the following type: The age-dependent transition rate to having the first pregnancy leading to a live birth increases by around 100% within the year after a colleague gave birth and by around 60% in the second year. Besides having established a statistically significant estimate of a social interaction effect that decreases over time, its scope remains hard to grasp. Is a 100% increase of a time-dependent transition-rate large? Leave aside the difficulty of comparing the estimates of the social interaction effects between each other in a reasonable way, e.g., are colleagues' effects larger than those of siblings? Questions like these are very hard to answer based upon the results presented in Chapters II–IV, but these are very important questions. We do not only want to know that the effects exist. To evaluate their importance for the demographic process of having children—the process deciding crucially about the size of a population, leading to an almost infinite amount of implications for a variety of social (and environmental) outcomes—we also want to know *how large* they are!

The microsimulation presented in Chapter V remedies this. It translates these hard-to-grasp percentaged hazard rate increases into easy-to-understand population-level birth counts in a counterfactual way (i.e., generating an "what-if" scenario). This allows for the main statement of this dissertation: In the year 2010, without social interaction effects, around 75,000 fewer children would have been born. This translates to around 23% of all first-born children in that year (not stating that they would never be born, but in some later year). Furthermore, from colleagues, around 22,000 first born children would have been born less. Compared to social interaction effects from siblings with around 3,000 children born less, social interaction effects from colleagues are much more important at the societal level. The most important interaction partner at the population level are the parents with around 50,000 fewer first born children. Translated into the well-known measure of country-wide fertility, the Total Fertility Rate (TFR), this indicates a decrease by around 15%, a drop from 1.37 to 1.17 children per woman. Based on these figures, it is easy to see that social interaction effects strongly influence fertility.

Is First Childbearing Influenced by Colleagues?

ABSTRACT

This chapter investigates whether colleagues' fertility influences women's transitions to parenthood. I draw on Linked-Employer–Employee data (1993–2007) from the German Institute for Employment Research comprising 33,119 female co-workers in 6,579 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.

2.1 INTRODUCTION

THIS chapter asks whether colleagues' fertility decision-making influences another colleagues' fertility timing. In other words, does fertility spread among colleagues? By selecting the workplace as a setting, I 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, 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.

This chapter is a reproduction of: Pink, S., Leopold, T., & Engelhardt, H. (2014). Fertility and Social Interaction at the Workplace: Does Childbearing Spread Among Colleagues? *Advances in Life Course Research*, 21, 113–122. Reuse in this dissertation permitted by publisher Elsevier. For the sake of consistency, I have rewritten this chapter from a first-person perspective.

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, I reconstructed a firm's entire history of birth events. These data enabled me 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, I estimated discrete-time hazard models based on a sample of 33,119 female co-workers observed longitudinally in 6,579 firms.

With regard to the the network of colleagues at the workplace empirical evidence is offered by two studies from economics. An analysis based on register data from Sweden examined whether colleagues' fertility decisions influenced each other (Asphjell et al. 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 et al. 2016). Taken together, these studies provide evidence in support of social interaction effects on fertility at the workplace, at least for Scandinavian countries.

2.2 THEORETICAL BACKGROUND

Which of the four mechanisms outlined earlier (social learning, social pressure, emotional contagion or social support) 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. 2013). 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, social learning 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. 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.

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.[1] 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.

[1] 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.

2.3 DATA AND METHODS

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 et al. 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 to 2007 and provide longitudinal information about a total of 1,845,707 employees in 43,623 firms.[2]

Identification of Birth Events

The LIAB data do not allow 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.[3] For mothers, this identification strategy has proven to be very effective (Schönberg 2009) because around 90% of working women take maternity leave after childbirth.[4] For two reasons, this is even more likely for first-time mothers: First, the proportion of women taking maternity leave after their first child was born is

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[2] 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.

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

[4] 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.

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 et al. 2010), the birth of a second child frequently occurs during an ongoing maternity leave. Therefore, my 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, I restrict my analysis to women (N = 772,379). Over the entire period of observation (1993–2007), I can identify a total of 15,284 birth events for this population.

Sample Selection

I proceeded in two steps to select an analytical sample. First, I 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 an increasing number of employees within a firm, this becomes increasingly unlikely (Hedström et al. 2008; Asphjell et al. 2013). This exclusion reduced the sample to 132,803 female employees observed in 11,662 firms. Second, my analysis focused on transitions to first birth. As noted above, my identification strategy was already tailored towards first births. Still, this procedure did not rule out higher parities. As I lacked data on parity, I 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 the data. The distributions of age at first birth in Germany provided a rationale to define this criterion. I 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.[5] 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 the data, I 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, I relied on the distributions calculated by Kreyenfeld (2007).[6] After this restriction,

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[5] This procedure is even more conservative since I focus on employed women who are known to be less fertile than those who do not work (Kreyenfeld 2010).

[6] Kreyenfeld (2007) presents these distributions for four birth cohorts (1962–1965, 1966–1969, 1970–1973, and 1974–1977); three educational groups from which

my analytical sample consisted of 33,119 female employees in 6,579 firms. Across the observation period, a total of 439 birth events were observed in this sample, 304 of which were first births.[7]

In addition to this analytical sample of women "at risk" of experiencing their first pregnancy, I 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).

To examine social interaction effects on the timing of first births, I 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 pos-

the data allows me to use two (intermediate and upper secondary school); and two areas of residence (East and West Germany). The age quartiles pertaining to the possible combinations of these three variables constituted my age thresholds. Table A2.1 shows the quartiles. Note that information about birth cohorts did not cover all birth cohorts of the youngest persons included in my sample. I thus extrapolated the age quartiles of the youngest birth cohort (1974–1977) to all following birth cohorts.

7 As a result of my sample selection, women enter the 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 my sample amounts to 2.3 years. Considering the national average age at first birth of 29 years (Statistisches Bundesamt 2013a: 93), the young age at first observation is a main reason for the small number of birth events included in the data. Furthermore, it is important to note that the birth events I 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 2.1, this pertains mostly to lower educated women.

sible in Germany.[8] The process ended either with right-censoring at the last month of observation or with an event (i.e., a pregnancy).

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). I 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.[9] I identify the outcome as a binary variable changing its value from 0 to 1 in the month of conception.

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., Kuziemko 2006; Asphjell et al. 2013; Lyngstad & Prskawetz 2010; Balbo & Barban 2014). To account for this, I 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 within (i) one year before, (ii) one to two years before, or (iii) two to three years before. These three indicators of social interaction effects were defined on the basis of the analytic sample as well as the supplementary sample. Compared to other research designs, the indicators used in this chapter have important advantages. Most importantly, they enabled me 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).

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

[9] In additional analyses (not shown), I 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.

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, I 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. Put differently, those who plan to have a child in the near future would select family friendly workplaces. Empirically, I might thus mistakenly infer social interaction effects from the observation of frequent birth events within a firm, although each woman's fertility is independent of exposure to her colleagues' birth events. I 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 me 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, I 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 me to account for the presence of unobserved time-constant characteristics shared by individuals within firms.

Control Variables

At the individual level, I 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, I 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.[10] 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, I included period dummies for every calendar year of the observation period to control for common shocks.[11] Table 2.1 provides a descriptive overview of all variables.[12]

Statistical Model

A woman's time-contingent propensity of becoming pregnant is given by the hazard rate λ_{ijt}. Within the 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.

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[10] 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).

[11] There were a few exceptions: in the years 1993, 1994, 1995, 1996, 1998, and 2007, the data included no or less than 10 events of first pregnancy. Therefore, I 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.

[12] I further tested the robustness of my 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 2.4 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 my analytic sample by more than 50%. Therefore, I only present the more parsimonious specification.

Table 2.1. Description of Variables (N = 33,119)

	Mean	SD	Min	Max	Share missing
Pregnancy (/100)	.03		0	1	
Colleague had a child within[a]					
one year before	.06		0	1	
one to two years before	.04		0	1	
two to three years before	.03		0	1	
Process time: Month[b]	115.15	62.03	1	329	
Individual characteristics					
East German	.43		0	1	
Migrant	.03		0	1	.60
Wage[c]	42.01	30.62	0	1,478	.55
Education (Ref: High)[d]					
Low	.80		0	1	3.24
Intermediate	.13		0	1	
Employment status (Ref. Full-time)					
In training	.29		0	1	.55
Part-time	.19		0	1	
Person months	916,347				

Note: Data are from LIAB Version 3 (1993–2007), own calculations, not weighted. 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.

The models are organized as follows. I 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). As employees are clustered within firms, I calculate robust standard errors. In Model 2, I add period dummies to control for common shocks. Finally, Model 3 adds a random effect to the equation. This final model is specified as follows:

$$log\left(\frac{\lambda_{ijt}}{1-\lambda_{ijt}}\right) = \alpha + r_t + r_t^2 + \beta_1 x_{ijt} + \beta_2 z_{ijt} + \beta_3 w_t + \epsilon_{ijt} + \eta_j. \qquad (2.1)$$

In this equation $r_t + r_t^2$ denote the linear and squared process time; $\beta_1 x_{ijt}$ are vectors for the three social influence dummies for woman i in firm j at time t; $\beta_2 z_{ijt}$ are vectors for individual characteristics; $\beta_3 w_t$ denotes the period dummies controlling for common shocks (Model 2); ϵ_{ijt} is the error term; 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.

2.4 RESULTS

Table 2.2 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).[13] The coefficients of my three key explanatory variables indicated social interaction effects of fertility at the workplace.

Model 1 provides evidence for the presence of social interaction effects.[14] 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 (Lyngstad & Prskawetz 2010; Kuziemko 2006; Asphjell et al. 2013; Balbo & Barban 2014). Importantly, such a pattern is very unlikely to be observed in the presence of selection effects. In Model 2, I tested the alternative explanation of common shocks by introducing period dummies to the equation. Under control for common shocks, the coefficients of the social interaction variables 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

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[13] 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 2013a: 93). National-level data on mean ages at different parities are not available prior to 2009 from official statistics.

[14] The moderate reductions in case numbers compared to the size of the analytic sample resulted from missing values (listwise deletion). Because my 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 2.2. Discrete-Time Hazard Models for the Transition to Parenthood (N = 33,119)

	Model 1			Model 2			Model 3		
	Est.	SE		Est.	SE		Est.	SE	
Colleague had a child within[a]									
one year before	1.18	(.18)	***	.99	(.17)	***	.71	(.19)	***
one to two years before	.73	(.23)	**	.70	(.22)	**	.47	(.21)	*
two to three years before	.34	(.18)		.45	(.17)	*	.21	(.24)	
Process time									
Months (/10)	.31	(.06)	***	.24	(.06)	***	.25	(.06)	***
Month squared (/1000)	-.09	(.02)	***	-.06	(.02)	**	-.06	(.02)	**
Period dummies		No			Yes			Yes	
Individual characteristics									
East German	.41	(.14)	**	.46	(.14)	**	.43	(.15)	**
Migrant	-1.33	(.66)	*	-1.29	(.66)		-1.34	(.72)	
Wage (/10)	-.11	(.03)	***	-.10	(.03)	***	-.09	(.03)	**
Education (Ref.: High)									
Low	-.22	(.20)		-.18	(.20)		-.22	(.22)	
Intermediate	-.37	(.26)		-.34	(.26)		-.37	(.27)	
Employment status (Ref. Full-time)									
In training	-1.41	(.24)	***	-1.48	(.25)	***	-1.36	(.29)	***
Part-time	-.49	(.18)	**	-.47	(.18)	**	-.46	(.17)	**
Constant	-9.58	(.54)	***	-8.72	(.53)	***	-9.10	(.56)	***
Log Likelihood	-2,521.44			-2,469.81			-2,460.72		
σ_u							.79	(.14)	
ρ							.16	(.05)	***
χ^2	282.31			318.58			195.28		
Person months	886,280			886,280			886,280		
Events	294			294			294		

Note: Data are from LIAB Version 3 (1993–2007), own calculations, not weighted. Firm clusters: 6,230. Robust standard errors in parentheses. Data are on a monthly basis. [a] At least one colleague had a child within the respective interval. $^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$

observed in the data. The size of the coefficients of the social interaction variables shows 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.

Figure 2.1 provides an illustration of my 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 Figure 2.1, the transition rate doubled in the first year after colleagues' birth

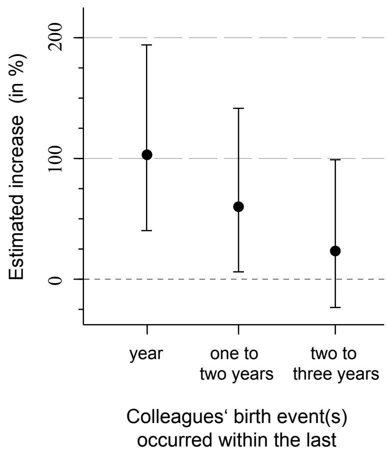

Figure 2.1. Transition Rate Increases following Colleagues' Childbearing

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.

The 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 transition rates to pregnancy, corroborating findings from previous studies (e.g., Kreyenfeld et al. 2010; Bundesinstitut für Bevölkerungsforschung 2012). 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 et al. 2012).

Additional Analyses

In additional analyses (not shown), I 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, I fitted Model 3 to a sample of women in firms not larger than 100 employees (N = 24,762 women in 5,286 firms) and, even more restrictively, to women in firms not larger than 50 employees (N = 12,905 women in 3,392 firms). In both analyses, I was 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 expectations regarding social interaction effects on fertility and increase confidence in the results presented above.

In a second set of additional analyses (not shown), I 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. I operationalized perceived similarity by age—a strategy which has been applied in previous research (Kuziemko 2006; Asphjell et al. 2013). Similarity in age refers to life course phases that reflect similarities in the conditions surrounding the transition to parenthood. Empirically, I defined two women as being 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, I 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 I found the same temporal pattern of social interaction effects.

2.5 DISCUSSION

Does fertility spread among colleagues at the workplace? To answer this question, this chapter 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, common shocks and selection.

How can we explain these effects? Based on previous qualitative research, I 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. My findings were consistent with this expectation although the LIAB data allowed only for crude approximations of similarity such as age. 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.

In addition, more detailed data are required to improve the generalizability and analytical precision of the results reported in this chapter. For instance, the lack of information about parity necessitated rather extensive sample restrictions and did not allow to test whether my results can be generalized to higher parities (Lyngstad & Prskawetz 2010).

2.6 APPENDIX

Table A2.1. First Quartiles of Age at First Birth

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 my definition of indicators based on the LIAB data, intermediate secondary school equals low education and upper secondary school equals intermediate education.

Is First Childbearing Influenced by Siblings?

ABSTRACT

This chapter asked whether siblings' childbearing events altered their sibling's transition rate to parenthood. Discrete-time hazard models (N = 1,850) using data of the German Family Panel (pairfam) show a considerable increase in the second year after a sibling gave birth, following an inverted u-shape over time. Furthermore, in accordance with the mechanisms outlined, the strength of the effect is much stronger when the siblings who gave birth are similar on salient characteristics (age, education, parity, and sex).

3.1 INTRODUCTION

In general, the study of family influences on family formation has a long tradition in both sociology and demography. The vast majority of this literature focusses on intergenerational effects observed in parent-child relationships (Fasang & Raab 2014; Liefbroer & Elzinga 2012). In terms of fertility, previous research consistently indicated positive intergenerational associations in the age of first birth and completed fertility (Kolk 2014; Murphy & Wang 2001). It has been highlighted, however, that "intergenerational effects may be too narrow to capture all of the important family influences" (Axinn et al. 1994: 67) and that in particular siblings might play a crucial role in affecting each other's family biographies (Raab et al. 2014). Although sibling relationships are less obligatory and more egalitarian than parent-child ties, they clearly differ from other peer relationships due to the shared family background and their long duration (Cicirelli 1991). In this sense, sibling ties can be considered intermediary ties that comprise certain aspects of both parent-child and peer relationships.

This chapter is a reproduction of a manuscript titled "Fertility and Social Interaction in the Family: Does Childbearing Spread between Siblings?", which is currently in preparation together with Prof. Thomas Leopold and Prof. Marcel Raab. For the sake of consistency, I have rewritten this chapter from a first-person perspective.

Despite some previous research on sibling effects on teenage pregnancies in the US (Powers & Hsueh 1997), so far, only two studies have examined sibling effects on fertility behavior during adulthood in developed countries. Lyngstad & Prskawetz (2010) utilized comprehensive Norwegian administrative data and identified a time-dependent social interaction effect on the transition to parenthood for women whose brother or sister had a child recently. Kotte & Ludwig (2011) used cross-sectional data of the German Family Panel (pairfam) and did not find evidence for cross-sibling influences. This result, however, was based on data which did not provide full information on siblings' fertility biographies. Moreover, the respective analysis did not distinguish between respondents' first births and higher-order fertility transitions. According to previous studies, however, peer group effects on fertility are particularly important for the transition to first birth (Kuziemko 2006; Lyngstad & Prskawetz 2010).

My analyses take advantage of a more recent release of the German Family Panel which includes rich information on siblings' birth biographies and allows for a more appropriate modelling strategy similar to the approach applied by Lyngstad & Prskawetz (2010). In addition to empirically establishing the existence of sibling effects on fertility for Germany using large-scale survey data, I elaborate on the potential mechanisms driving this effect. Based on tenets of social comparison and social learning I illustrate that the strength of sibling effects in fertility varies according to the degree of similarity among siblings.

3.2 THEORETICAL BACKGROUND

My discussion concentrates on the four central mechanisms of social influences on fertility decision-making, which have been identified by qualitative research and which I have presented earlier in the Introduction. In this section, I, again, outline the central theoretical ideas of these four mechanisms and discuss their relevance for social interaction effects between siblings.

Social learning refers to the process by which information obtained by observing others affects fertility decision-making by influencing attitudes and changing behavior. This process is particularly relevant for the transitions to parenthood as it can help to reduce the uncertainty surrounding this crucial life course transition (Lois & Arranz-Becker 2014; Lyngstad &

Prskawetz, 2010). In this regard, peers or siblings who recently became parents may act as role models (Bandura 1994) from whom individuals infer on their own situation (Keim 2011). The learning process can appear in a variety of different forms. It ranges from small observations to lengthy discussions of the experiences of a pregnancy or evaluations of how children affect the partnership, leisure, or working life (Keim 2011). Social learning has shown to be of prime importance between siblings. Compared to other interaction partners, the content of sibling conversations is typically family-related and therefore bears a strong potential for valuable information in the form of fertility-related first-hand experiences (Keim et al. 2013).

Social contagion refers to emotional reactions that individuals are not necessarily aware of. In this regard, e.g., childless siblings getting into direct contact with another sibling's new born, or small child more general, may become emotionally aroused, which may intensify their desire to have a child themselves (Bernardi 2003; Keim et al. 2013). Compared to other interaction partners, contact to the children of siblings can be expected to be rather frequent. Besides the life-long bond between siblings, the reason is that, even in the minimal configuration, ideal-typical family-life ensures recurring contact throughout the calendar year. This begins shortly after the birth of the niece or a nephew and will continue in numerous family gatherings like birthdays and holidays (Keim 2011).

Social support highlights that the opportunity to receive financial, instrumental or emotional support reduces the costs of childbearing and therefore eases the decision to have a child (Liefbroer 2005; Keim 2011). It has been shown that siblings get emotionally closer and have more contact following the arrival of children (Connidis 1992), which would ease creating the opportunity structure for supportive relationships. Moreover, some indirect evidence from the US is suggesting that siblings' synchronization of fertility might be driven by the cost reducing effects of joint childrearing, e.g., by sharing clothes, buggies or baby beds (Kuziemko 2006). When it comes to hands-on child care, however, the level of support among siblings in Germany is negligible, especially when compared to parental support (Alt & Teubner 2007; Keim et al. 2009). As a result, I expect that social support plays a rather minor role in explaining social interaction effects on fertility.

Social pressure influences fertility decision-making by means of sanctions and rewards. A typical example is parents expressing their wish to have a grandchild. Evidence for direct social pressure from siblings, however, has not been found empirically (Keim et al. 2013; Keim 2011). One reason for this might be that, in comparison to voluntary friendship ties, the more obligatory relationship with siblings lacks sanctioning power. Contrary to a friendship, it is considered a lifelong bond and therefore usually is not subject to dissolution. Accordingly, differences in parental status might not threat the persistence of sibling ties as much as less binding friendships, especially if the share of parents among the friends is high (Keim 2011). However, the birth of a niece or a nephew may trigger indirect social pressure from parents or extended kin. Taken together, I expect that—despite some potential indirect effects—social pressure contributes little to explaining social interaction effects on fertility among siblings.

Similarity as a Driver of Interaction Effects

The previous section showed that the mechanisms pertain to different types of interaction partners to a varying degree. With regard to siblings, social contagion, social learning, and to a lesser extent social support appear to be the most relevant channels of social influence. All of these mechanisms require regular contact with the interaction partners and awareness of their fertility behavior. These prerequisites are usually met for siblings but also for other peers such as close friends. Both groups are important points of reference in decision-making processes because individuals consider them to be like themselves. The central role of similarity in social comparisons is well-established and has already been highlighted in Festinger's (1954) foundation of social comparison theory. Since then the importance of similarity in human relationships has been set out in several theoretical and empirical contributions. In view of the insights from this abundant literature, I advance the general hypothesis that social interaction effects are more likely to unfold among similar persons.

With regard to social learning, for instance, similarity between observer and social model has been shown to increase the effectiveness of the modeling process (Bandura 1977). According to this theory, social models can provide vicarious experiences which might affect a person's fertility decision-making by increasing self-efficacy. The strength of this effect is positively associated with the perceived similarity to the model (Bandura 1994; Keim 2011). In this sense, observing how a similar person becomes a parent might change the observer's beliefs from "I am not ready yet" to "if she can do it, I can do it, too." This change in perceived self-efficacy, in turn, might affect the timing of the transition to parenthood.

In addition, similarity amplifies social interaction effects because it breeds connection. This relation is based on the principle of homophily which posits that "contact between similar people occurs at a higher rate than among dissimilar people" (McPherson et al. 2001: 416). Accordingly, individuals are more likely to have frequent contact with new borns if their parents are considered more similar on salient characteristics. This contact, in turn, makes the emergence of social interaction effects (Keim et al. 2013) more likely. Contact and similarity also play a crucial role with regard to social support. They are both constitutive elements of strong ties, which are additionally characterized by emotional closeness, and reciprocal support. Qualitative research has shown that siblings only affect fertility decision-making if they are characterized as strong ties (Keim 2011; Keim et al. 2013). In these instances, sibling relationships reliably provide social support, fertility relevant information in terms of social learning, and exposure for emotional contagion to develop. In addition, strong ties are also better capable to exert social control. Among siblings, however, social pressure with regard to fertility behavior seems to occur only rarely (Keim et al. 2013).

In sum, the arguments presented in this section suggest that similarity amplifies social interaction effects between siblings. To my knowledge, this hypothesis has not been explicitly tested in previous studies. Research on fertility peer effects among friends, however, has approached similarity as a methodological problem of selection (Balbo & Barban 2014). According to the principle of homophily the formation and persistence of friendship ties is determined by the perceived similarity among friends (e.g., Kandel 1978). Therefore, it is challenging to disentangle if observed similarity in attitudes or behavior is the result of selection or of social influence (see Balbo and Barban 2014 for an innovative solution to this problem). The selection problem is considerably reduced in the case of siblings. In contrast to voluntary friendship ties the sibling relationship is rather ascribed and only rarely subject to dissolution (Cicirelli 1991). Even if sibling relationships are dissolved they are easier accessible for survey based research than dissolved friendship ties.

3.3 DATA AND METHODS

My empirical analysis employed data from five annual waves (2008-2012) of the German Family Panel (pairfam; Huinink et al. 2011).[1] Pairfam was designed as a stratified random sample of three birth cohorts (1971–1973; 1981–1983; 1991–1993) and provides information on 12,402 respondents. The questionnaire of the fifth wave comprised a sibling module which collected retrospective information on their birth biographies. Additional information required for the analysis came from the second and the third panel wave. Accordingly, my baseline sample was restricted to 2,980 women who participated in each of these three waves. In order to account for panel attrition (Müller & Castiglioni 2015) and the multi-cohort design of pairfam, I employed a combination of longitudinal as well the design and post-stratification weights provided by the pairfam user service (Brüderl et al. 2015).

I restricted the sample to heterosexual women being able to procreate by natural means and not having any adopted children. This reduced the sample size to 2,868 women. In a second step, I restricted the sample to respondents with one to four natural siblings.[2] This was necessary because the questionnaire only collected information on up to four siblings. In sum, this yielded an analytical sample of 1,850 female respondents.

Based on this sample I constructed a monthly person-period file that included life-history data from age 15 either up to the date of the last interview or until the transition to parenthood. The resulting file comprised 251,765 observations (i.e., person-months).

[1] This chapter uses data from the German Family Panel pairfam, coordinated by Josef Brüderl, Karsten Hank, Johannes Huinink, Bernhard Nauck, Franz Neyer, and Sabine Walper. pairfam is funded as long-term project by the German Research Foundation (DFG). Analyses are based on data from the first five waves of the German Family Panel (pairfam), release 6.0 (Brüderl et al. 2015).

[2] I restricted the analysis to natural siblings to keep the results comparable to Lyngstad & Prskawetz (2010). Additional analyses (results not shown) show that this restriction did not alter substantive conclusions – besides the strength of the effects being lower. I interpret this difference as further evidence for the similarity argument as being a natural sibling is equivalent to having the same parents – which refers (at the very least) to a similarity momentum in the family structure.

The Dependent Variable

An ideal test of social interaction effects on fertility decision-making would require information on the timing of the decision to become a parent. As the retrospective data do not include such information, I follow the analytical strategy applied in previous studies on fertility-relevant social interaction effects (Lyngstad & Prskawetz 2010; Pink et al. 2014 or Chapter II). Accordingly, my dependent variable is defined as a binary indicator changing its value from 0 to 1 in the first month of a pregnancy that led to a live birth. According to this definition the data include 843 birth events.

Independent Variables: Siblings' Birth Events

Previous studies have shown that social interaction effects on fertility are related to the time elapsed since exposure to the event of interest (Balbo & Barban 2014; Lyngstad & Prskawetz 2010). To account for this, I employed the dynamic modelling strategy of Chapter II, operationalizing social interaction effects by three time-varying dummy variables. These variables indicated whether a birth event of (at least) one sibling occurred (i) less than one year before, (ii) one to two years before, or (iii) two to three years before. As pairfam only provides yearly information on siblings' birth dates as well their birth biographies, I set the birth month of their children as well as their own birth month to January of the respective year. As a robustness check (results not shown), I also imputed the months 30 times based on random draws from a uniform distribution of 1 to 12, estimated the individual models, and combined them afterwards using Rubin's rules. This did not change substantive conclusions.

Similarity and Shared Family Background

By definition, siblings are more similar than other peers. They share genes and were socialized in the same family environment. These commonalities contribute to similarities in fertility behavior which are not the result of social interaction effects. Therefore, my analysis accounts for the shared family background of siblings in order to test if social interaction effects can be found after controlling for this baseline similarity (Feinberg et al. 2013).

To this end, I controlled for mother's age at first birth, mother's current age, sibship size, intact family structure, and parents' highest educational degree. Mother's age at first birth and sibship size were included to capture the intergenerational transmission of fertility (Barber, 2000; Murphy and Knudsen 2002). Including these indicators allowed me to test if social interaction effects operate independently of intergenerational transmission. However, previous research (Kotte & Ludwig 2011) did not provide evidence for this. Mother's age was sought as a crude approximation for mother's availability for child care support (see Chapter IV). Intact family structure was measured by a simple binary variable indicating if the respondents continuously lived with both parents up to age 18. This variable was included for two reasons. First, children from alternative family structures have been shown to become parents at younger ages than children from intact families (McLanahan & Percheski 2008). Second, siblings from non-intact families tend to have more conflicts (Poortman & Voorpostel 2009). Both reasons potentially bias the estimation of social interaction effects by increasing the risk of under- or overestimating the true effect size. Finally, parents' highest level of education was included to account for the social status of the family of origin.

In order to measure similarity, beyond shared family background I included variables which have been discussed in previous research on homophily and sibling interactions during adulthood (Keim 2011; Voorpostel et al. 2007). In particular, I focused on similarity in age, gender, education, and parental status. Similarity in age was restricted to an age difference not exceeding four years (Asphjell et al. 2013). Similarity in education was defined as having the same educational degree according to a categorical variable that distinguished between having no certificate (yet), basic secondary school, intermediate secondary school, and entrance qualification for university of applied sciences as well as higher education entrance qualification. Similarity in parental status referred to the current number of children and was given if respondent and sibling both were observed during the same parity transition (i.e., respondent has no children yet and the sibling has had a first born up to three years before). As this chapter is about the transition to parenthood, this pertains to precisely this transition (i.e., from parity 0 to parity 1).

Based on these four dimensions of similarity I refined my operationalization of social interaction effects and distinguished between two types of social interaction effects. One measuring the impact of birth events of more similar siblings, the other measuring the effect of birth events of less similar siblings. The similarity criterion was met if the siblings were similar on all four dimensions. That is, only if a similar aged sibling of the same sex and with the same level of education experienced the transition to parenthood. This strategy is suited to inspect whether social interaction effects on fertility are really stronger among more similar siblings.

Other Control Variables

In order to obtain meaningful baseline estimates of the transition to parenthood, I included additional variables which are usually considered in fertility research. As outlined in the Introduction, I 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, I included three important determinants of the transition to parenthood identified repeatedly by previous research, education, partnership status (i.e., being in a partnership, yes or no), and the part of Germany where the respondent grew up until age 18 (East or West Germany as well as migration if applicable). Table 3.1 provides a descriptive overview of all variables included in the models.

Statistical Model

I estimated two discrete-time event history models (Allison 1982). A woman's time-contingent propensity of becoming pregnant is given by the hazard rate λ_{it}. Within my discrete-time logistic regression model, the hazard rate is the conditional probability that a first pregnancy of woman i occurred at time t, under the condition that the woman was still childless.

The models are organized as follows. I start with the main model, adding all variables except those for similarity. In this model I establish the social interaction effect as was done in previous studies (Lyngstad & Prskawetz 2010). In Model 2, I apply the refined operationalization of social interaction effects differentiating between birth events of more and less similar siblings to inspect whether the effects are stronger among more similar sisters. The models are specified as

Table 3.1. Description of Variables (N = 1,850)

	Mean	SD	Min	Max	Share missing
Pregnancy (/100)	.33		0	1	
Sibling had a child within[a]					
one year before	.04		0	1	
one to two years before	.04		0	1	
two to three years before	.03		0	1	
More similar sibling had a child within[b]					
one year before	.01		0	1	
one to two years before	.01		0	1	
two to three years before	.01		0	1	
Less similar sibling had a child within					
one year before	.03		0	1	
one to two years before	.03		0	1	
two to three years before	.02		0	1	
Individual characteristics					
Process time: Month[c]	87.14	67.19	1	338	
Region (Ref.: West Germany)					
East Germany	.14		0	1	
East-West Migrant	.14		0	1	
Highest educational attainment (Ref.: low)					
intermediate	.32		0	1	
high	.56		0	1	
In a partnership	.51		0	1	
Familial characteristics					
Mother's age	48.42	7.58	28	84	3.08
Mother's age at first birth	24.26	4.24	15	42	3.08
Sibship size					
2	.28		0	1	
3+	.15		0	1	
Parents' highest educational attainment (Ref.: low)					3.09
intermediate	.36		0	1	
high	.32		0	1	
Family intact	.87		0	1	
Person months	251,765				

Note: Data are from the German Family Panel (pairfam), unweighted. Data are on a monthly basis. [a] At least one sibling had a child within the respective interval. [b] Similarity is indicated by a sibling who was at the time he or she had a child in the same age range as the focal sibling is at time t, the same parity, the same sex and the same highest educational attainment as of the fifth wave of the pairfam data. [c] Process time starts at age 15 and ends at first pregnancy with an event or is right-censored at the last month of observation.

$$log\left(\frac{\lambda_{it}}{1-\lambda_{it}}\right) = \alpha + r_t + r_t^2 + \beta_1 x_{it} + \beta_2 z_{it} + \epsilon_{it}. \tag{3.1}$$

In this equation, α denotes the constant and $r_t + r_t^2$ the linear and squared process time; $\beta_1 x_{it}$ are vectors for the three (or six in Model 2) social influence dummies for woman i at time t; $\beta_2 z_{it}$ are vectors for women's characteristics; ϵ_{it} is the error term.

3.4 RESULTS

Table 3.2 shows the estimated coefficients of the two multivariate models. The indicators for process time show the expected bell-shaped curve in all models. Also the control variables do not show any unexpected patterns, which is why I will not further discuss their strength and direction.

The models convey three findings. First, I find clear evidence for the presence of social interaction effects for women as shown in Model 1. Furthermore, their temporal shape corroborates previous research, which found it to be inverted u-shaped with its maximum in the second year after a birth event (Lyngstad & Prskawetz 2010). Whereas I find a non-significant increase in the first year, in the second year after sibling had a child, the transition rate to pregnancy increased markedly. The coefficient for the third year indicated a decline and becoming essentially zero. To better illustrate this, Figure 3.1 shows the estimated increase of the transition rate (in per cent; exponentiated coefficients). If a sibling had a child within one year before, this amounted to an increase in the transition rate by around 25% (although just missing the conventional 10%-threshold of statistical significance with $p = 0.12$), and this increase was even higher in the second year after the birth of the niece or nephew with around 70%. Third, turning to Model 2, I find that similarity gauges the strength of the effect. For less similar siblings, it leaves its temporal shape almost unaffected. Comparing both estimated increases, the one unfolding from the less similar sibling to the one from the more similar sibling, shows that the strength is 7.2 times higher in the first $((e^{0.99}-1)/(e^{0.21}-1) = 7.24)$ and 3.1 times in the second year.

Table 3.2. Discrete-Time Hazard Models for the Transition to Parenthood (N = 1,850)

	Model 1			Model 2		
	Est.	SE		Est.	SE	
Process time						
Months	.04	(.00)	***	.04	(.00)	***
Months squared	-.00	(.00)	***	-.00	(.00)	***
Sibling had a child within[a]						
one year before	.23	(.15)		.21	(.17)	
one to two years before	.52	(.19)	**	.44	(.23)	†
two to three years before	-.01	(.18)		-.02	(.20)	
More similar sibling had a child within[b]						
one year before				.99	(.35)	**
one to two years before				1.00	(.36)	**
two to three years before				-1.01	(.74)	
Individual characteristics						
Region (Ref.: West Germany)						
East Germany	.48	(.14)	***	.47	(.14)	**
East-West Migrant	.58	(.13)	***	.58	(.13)	***
Highest educational attainment (Ref.: low)						
intermediate	-.21	(.14)		-.19	(.14)	
high	-.63	(.18)	***	-.63	(.18)	***
In a partnership	1.92	(.32)	***	1.92	(.32)	***
Familial characteristics						
Mother's age	-.02	(.01)		-.02	(.01)	
Mother's age at first birth	-.01	(.02)		-.01	(.01)	
Sibship size						
2	-.01	(.13)		-.01	(.13)	
3+	.27	(.13)	*	.27	(.13)	*
Parents' highest educational attainment (Ref.: low)						
intermediate	-.23	(.13)	†	-.23	(.13)	†
high	-.15	(.15)		-.15	(.15)	
Family intact	.11	(.14)		.11	(.14)	
Constant	-9.06	(.59)	***	-9.11	(.59)	***
Person months		237,018			237,018	
Events		843			843	

Note: Data are from the German Family Panel (pairfam), own calculations, weighted results. Logistic regression coefficients are shown. Standard errors in parentheses. [a] The meaning of this indicator changes in Model 2 after similarity is introduced. In Model 2 it denotes only the subgroup of birth events by siblings not completely similar on the four characteristics age, highest educational attainment, parity and sex. [b] Similarity is indicated by a sibling who was at the time he or she had a child in the same age range as the focal sibling is at time t, the same parity, the same sex and the same highest educational attainment as of the fifth wave of the pairfam data. $^{\dagger}p < .10$, $^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$.

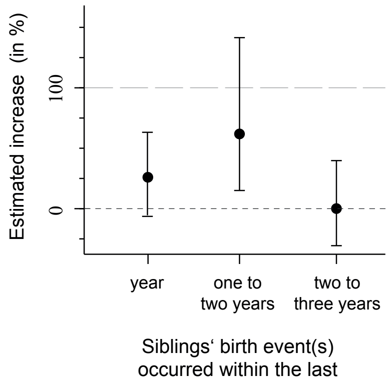

Figure 3.1. Transition Rate Increases following Siblings' Childbearing

3.5 DISCUSSION

Does fertility spread among siblings? Based on large-scale quantitative data from the German Family Panel I estimated discrete-time hazard models for the transition to the first pregnancy (leading to a live birth). The models showed a time-dependent, positive influence emanating from siblings' birth events that followed an inverted u-shaped pattern increasing in the first, peaking in the second and vanishing in the third year after the birth of a niece or nephew. This result echoed findings from German qualitative literature (Keim 2011) applying quantitative methods from international research that have been utilized previously to detect these effects (e.g., Balbo & Barban 2014; Lyngstad & Prskawetz 2010).

Although I identified a social interaction effect between siblings, I was not able to empirically separate the mechanisms through which it may have unfolded. However, going beyond current international research, I assessed the variability of the strength of the social interaction effect in relation to the mechanisms. Based on arguments of self-efficacy, homophily and contact, I argued that between siblings who are more similar to each other on salient characteristics, social interaction effects may especially be expected. Similarity is likely to be a situational characteristic that more likely activates all respective mechanisms. Empirical evidence supports this theoretical reasoning. Comparing the effect sizes that birth events of similar siblings have on the transition rate compared to that of non-similar ones the strength of the effects was considerably elevated. At the time the influence peaked this meant that the influence was more than three times larger if more similarity was given. Furthermore, even the temporal shape changed, remaining inverted u-shaped nevertheless, but distributing the peak to a larger time-span. These results are important for two reasons. First, though I was not able to delineate the contribution of each mechanism against each other, I provided quantitative evidence for their compound strength. Second, through the rather extreme operationalization of similarity it goes beyond establishing the effect by localizing the scope of social interaction effects through its variability.

This chapter has two major limitations that warrant future research in this domain. First, I was not able to empirically separate the mechanisms to investigate their relative contribution in bringing about the social interaction effect between siblings. However, delineating the qualitatively proposed mechanisms quantitatively has earlier been evaluated as a rather difficult endeavour as typically large-scale surveys lack the measurements to convincingly operationalize them (Rossier & Bernardi 2009). The second point is directly connected to this issue of data availability. Although I was able to measure similarity in sufficient detail to identify the theoretical predictions of the similarity argument, I was only able to highlight one specific part of this argument by testing a stark contrast. There are two reasons for this shortcoming. On the one hand, the measurements of similarity used can be assumed to only crudely reflect perceived similarity, which is the actual theoretical concept thought to gauge mechanism activation. On the other hand, the measurements were only few in number. As a result, I was forced to operationalize similarity as a binary construct rather than metric or ordinal. In case both reasons would not have applied, I would have been able to investigate the functional form of similarity. Estimating this would have been a necessary precondition to narrow down the scope of the strength of social interaction effects, which aside from investigating the relative impact of the mechanisms denotes a major research goal when investigating social interaction effects between siblings. To pursue this research in a quantitative fashion, especially when trying to specify the functional form via sequentially introduced fine-grained measures of similarity, more statistical power in terms of sample size would be needed. Administrative data bears a large potential to overcome these limitations, given that rich information from multiple registers may be combined.

Is First Childbearing Influenced by Parents?

ABSTRACT

Based on a cost-reduction argument, this chapter explored whether anticipated child care support from their mothers influenced adult daughters' decisions to have their first child. Using six waves of the German Family Panel (pairfam), discrete-time hazard models (N = 3,155 women) were estimated for the transition to the decision to have the first child. Anticipated child care support from the women's mothers was approximated by the travelling distance between adult daughters and their mothers, a measure whose suitability was tested empirically. The results indicated that women in a position to anticipate having access to child care support in the future decided to make the transition to parenthood earlier. This finding highlights both the strength of social interaction effects on fertility decision-making as well as the importance of intergenerational relationships for individual fertility histories already at their very beginning.

4.1 INTRODUCTION

Grandparental child care support to adult children has been (and remains) widespread in Germany. Grandparents look after more than one-third of the children under the age of three (Kügler 2007). Approximately every third grandmother is involved in child care activities on a regular basis (i.e., weekly or more often), and every second grandmother provides some form of child care at some point during the year (Attias-Donfut et al. 2005; Hank & Buber 2009). Grandparents even adapt their retirement preferences (Hochman & Lewin-Epstein 2013) as well as the timing of their retirement (van Bavel & de Winter 2013) to fit with their adult children's fertility behavior. In general, these parental time transfers are thought to relieve the adult children and consequentially reduce their costs of childbearing (Aassve et al. 2012; Thomése & Liefbroer 2013).

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Knowing about this tremendous (grand-)parental involvement and the attendant benefits for their adult children immediately gives rise to the question whether those adult children who anticipate having access to child care support from their parents in the future are more likely to decide to have a first child earlier. If this were the case, adult children who may anticipate child care support from their parents would be considerably advantaged compared to those who cannot. This would be an important finding for research on both intergenerational relationships and fertility decision-making. This chapter highlights the relevance of downward transfers from the older generation at the very beginning of the younger generation's fertility histories. More precisely, it clarifies that parental child care support not only matters for fertility decisions after it already has been experienced, such as having one's second or third child after the first child has already been cared for by its grandparents (Thomése & Liefbroer 2013). Rather the simple anticipation of such support facilitates and therefore positively affects the decision to become a parent in the first place.

My chapter assesses this question for the first time and therefore concentrates on the most likely recipient of child care support, the adult daughter, and the most likely provider, her mother (i.e., the maternal mother). The empirical analysis was based on 3,155 women from six waves of the German Family Panel and discrete-time hazard models for the transition to the decision to make the transition to motherhood using 11,427 person-years. The dependent process was operationalized as a combination of engagement in proceptive behavior (Miller & Pasta 1995), which gets very close to the timing of the actual decision, as well as being pregnant at the time of the interview. Anticipated maternal child care support was approximated by an extraordinarily detailed measure of travelling distance between daughters and their mothers. In other words, I will interpret it as a proxy of women's anticipation of (grand-)maternal child care availability. Although travelling distance lacked the central concept of anticipation, an empirical test confirmed that it reflects anticipated maternal child care support very well. Additionally, as geographical proximity may be related to fertility-relevant parental pressure, I net out the effect of social pressure.

4.2 THEORETICAL BACKGROUND

Previous research has repeatedly shown that both men and women anticipate that children will produce diverse costs (Fawcett 1988), and that these anticipated costs negatively influence the transition to parenthood (Liefbroer 2005). I argue that the reason for adult children to anticipate future child care support from their parents, and thus to incorporate their parents' availability and willingness to be caregivers into their decision-making, is that the expectation to receive support lowers their anticipated costs of childbearing (e.g., Aassve et al. 2012; Mathews & Sear, 2013; Thomése & Liefbroer 2013). Therefore, I expect that anticipated child care support (regardless of whether or not such support materializes) will facilitate the decision to become a parent and, as a consequence, positively influence the timing of the decision (Keim et al. 2013; Gray 2005). This argument is very general in nature and does not lose strength if usage of comprehensive formal child care is anticipated, too.

The Certainty of the Realization of Anticipated Child Care Support

How certain can adult children be that their parents in fact will enact their anticipation? This question pertains to the parents' motivation and availability (or ability) to assist with child care. Theoretical considerations unpacking the motivational structure are provided by evolutionary biological, sociological, and economic theories (Coall & Hertwig 2010).

The evolutionary perspective rests on the notion of inclusive fitness. (Grand-)parents provide child care support because they want to further their genes. In this respect, child care promotes child survival and consequently the survival of their genes (Hrdy 2009). The sociological perspective emphasizes parents as a "reserve army", family safeguards, or kin keepers (Dubas 2001; Hagestad 1986; Rosenthal 1985). It highlights the idea that parents' supportiveness in times of need is an "implicit understanding" (Hagestad 2006, 325) shared by adult children and their parents. The economic perspective stresses the cost-benefit nexus of fertility (Becker 1991). On the one hand, parents may act altruistically, incorporating their adult children's utility into their own (joint) utility function. Parents' caring for their offspring's children reduces the adult children's costs of childbearing, thus raising parents' utility (Goodfellow & Laverty 2003; Piliavin &

Charng 1990). On the other hand, parents may assist with child care in the interest of long-term reciprocity (Hollstein & Bria 1998). In return for their time transfers when their grandchildren are young, they expect to receive time transfers from their adult children in the form of care when they eventually become frail (Künemund et al. 2005; Silverstein & Giarrusso 2010). Taken together, all three disciplines provide strong arguments to assume that parents should meet the anticipation of their adult children (certainly to varying degrees) when they are requested.

Moreover, these theoretical considerations, from a couples' perspective, allow the specification of the intra-familial dyad along which time transfers will most likely flow. The evolutionary perspective suggests that of all grandparents, mostly the grandmothers (Hawkes et al. 1998; Hrdy 2009), and more specifically those on the mother's side, will primarily provide support because the maternal grandmother can be most sure of her genetic relationship to her grandchildren (Coall et al. 2014). The sociological perspective implies that because of the gendered nature of the grandparent role as essentially a maternal one (Hagestad 1986), grandmothers are expected to be more involved with their grandchildren, as they feel more responsible for intra-familial issues (Mulder & van der Meer 2009; Rosenthal 1985). Taken together with the concept of the mother-link (i.e., stronger and closer links between mothers and their daughters over the life course; Dubas 2001; Fischer 1991), maternal grandmothers are seen as particularly important providers of care for their adult daughters' offspring (Thomése & Liefbroer 2013). The economic perspective suggests that daughters are more likely than sons to receive child care support, as they are more likely to act reciprocally, as females mostly provide care for their elders (Kaptijn et al. 2013; Pillemer & Suitor 2006). These theoretical considerations lead to the following assumptions: Among adult children, daughters are most likely to receive support; among parents, women's mothers most likely will provide support. Therefore, within a couple's decision-making process about whether and when to have their first child, the woman's mother is most likely to be the prominent parent.

Geographical Proximity to Parents and Realized Child Care Support

Besides the parents being alive, the primary precondition for receiving parental child care support is geographical proximity between adult children and their parents. Previous research has shown repeatedly that in the presence of young children the probability of receiving child care support from grandparents is positively associated with geographical proximity: the shorter the distance between adult children and their parents, the greater the probability of such support (Belsky & Rovine 1984; Jappens & van Bavel 2012; Kaptijn et al. 2013; Kügler 2007; Waynforth 2012). The greater the geographical distance, the more time is consumed by travel, which renders flexible child care less and less feasible. Applied to the anticipation of receiving child care support, the results of this research suggest a negative relationship between geographical distance and anticipation.

Residential Distances Between Adult Children and their Parents

This raises the question about the general distribution of geographical distances between the generations. In Germany, most people live close enough to their extended family to visit members of different generations on short notice. This begins already at the initial move-out from the parental home, for which the median distance has been estimated at less than 10 kilometers (Leopold et al. 2012). Over the life course, moves predominantly remain local and rarely exceed 20 kilometers (Wagner 1989). Wagner (1989: 168) shows that at the age of 30, around 40% of those raised in villages and 60% of those raised in cities continued to reside in the same place. Isengard (2013) shows that over 60% of residential distances between parents and their adult children do not exceed 25 km; around 18% even denote co-residence. Tremendous residential distances of more than 100 kilometers apply to around 22%. These numbers suggest that the generations are not very regionally dispersed.

Alternative Explanations

The suggested positive relationship between geographical proximity and the decision to become a parent, however, could be accounted for by two alternative explanations: social pressure and endogeneity. First, previous research has shown that parents may be a source of social pressure when they demand grandchildren or urge their adult children not to have children yet (Balbo & Mills 2011; Barber & Axinn 1998; Bernardi 2003; Fried & Udry 1980; Keim et al. 2013). Importantly, the assertiveness of this pressure is strengthened by direct face-to-face contact, and more so if the contact is frequently repeated (Cialdini & Goldstein 2004; Rossier & Bernardi 2009). Social pressure is a dimension of social influence (Rossier & Bernardi 2009) and Latané et al. (1995) showed that the strength of social influence is an inverse function of geographical distance. In turn, the assertiveness is indirectly a function of geographical proximity, as geographical proximity translates into more frequent direct face-to-face contact (Frankel & DeWit 1989; Lawton, Silverstein, & Bengtson 1994; Hank 2007). Therefore, social pressure by parents might only be effective at influencing adult children's decision-making, or at least making it more effective, if exerted in face-to-face interaction.

Second, although the preceding provides evidence that adult children and their parents tend to live geographically close, this proximity might be endogenous to fertility decision-making. In other words, anticipated parental child care support may have factored into decisions about where to live. If this was the case, it would have at least two empirical implications. First, to assert the receipt of the support, those women anticipating child care support should refrain from relocating too far away from their mothers when they move. Second, in case they earlier moved to a destination which does not (easily) allow for maternal child care support, they should move closer before becoming pregnant. Previous research suggests that relocations with regard to fertility events occur quite frequently. Importantly, however, couples relocate mostly during pregnancy or soon after the birth of a baby (Feijten & Mulder 2002). These moves overwhelmingly remain local (Michielin & Mulder 2008) and their objective tends to be a more spacious dwelling or a more family-friendly environment (Del Boca et al. 2014; Feijten & Mulder 2002; Kulu & Washbrook 2014). Nevertheless, given that couples have to or want to move, they could also use such a move to optimize their anticipated caregiving configuration by moving closer to a potential caregiver, e.g., the woman's mother.

Closely Related Previous Research

Previous research closely related to my research question has been both qualitative and quantitative. Qualitative research has shown that German parents are important interaction partners at their children's transition to parenthood (Keim et al. 2009, 2013). A crucial factor within their children's decision-making process about their transition to parenthood was their parents' future availability to provide child care. The overwhelming majority of respondents declared that "parental support is expected as normal" (Keim et al. 2013: 8). Where such support was known to be lacking, this was considered a hindrance to childbearing and a major reason for not having children yet.

Quantitative empirical evidence was based on two studies using the German Socio-Economic Panel Study (GSOEP). Both approximated anticipated maternal child care support using geographical proximity to the maternal mother. However, there are two important differences to the current study that both very likely lead to an underestimation of the effect of anticipated maternal child care support. First, both studies used the birth of a first child as the dependent event. Yet this indicates a later point in time than that of the actual decision about whether to have a baby, and can thus be expected to be more subject to endogeneity, as the preparatory moves during pregnancy might have been used to move closer to the mother. Second, geographical proximity was measured as a binary indicator, mixing qualitatively different living situations. Focusing on the years 1996-2000, Hank & Kreyenfeld (2003) found that if their mother resided in the same house, in the neighbourhood, or in the same city/village but not more than a 15-minute walk away, women's transition rate to parenthood increased by 32%. Focusing on the years 1992-2007, Arránz Becker & Lois (2013) found that if a woman's mother or father lived in the respondent's household or vicinity, the transition rate increased by 36%. However, it should be noted that inspecting these effects was not the primary concern of these studies.

4.3 DATA AND METHODS

I employed six annual waves (2008-2013) of the German Family Panel (pairfam; Huinink et al. 2011).[1] The data were based on a stratified random sample of three birth cohorts (1971–73; 1981–83; 1991–93) and provided information on 12,402 respondents. All following analyses employ design

[1] This chapter uses data from the German Family Panel pairfam, coordinated by Josef Brüderl, Karsten Hank, Johannes Huinink, Bernhard Nauck, Franz Neyer, and Sabine Walper. pairfam is funded as long-term project by the German Research Foundation (DFG). Analyses are based on data from the first six waves of the German Family Panel (pairfam), release 7.0 (Brüderl et al. 2015).

weights to correct for disproportionate sampling (i.e., oversampling of age-groups) across cohorts (Brüderl et al. 2015).

The goal of the subsequent sample selection procedure was to create a sample that allowed me to investigate the anticipation argument for those for whom the theoretical considerations predicted it most likely to hold: women who are equipped with the biological and social means to decide to make the transition to parenthood and whose mothers are still alive. The richness of the surveyed fertility-relevant information allowed me to select this sample very precisely, proceeding in eight steps.

First, I restricted the gross sample to 6,373 female respondents. Second, I restricted the sample to the 4,150 women from the two youngest cohorts. I did not incorporate information from the third cohort to avoid introducing bias, because only a very small group of 292 childless women, on average 38 years old, remained after this (entire) sample selection procedure. Substantively, since they were in the last quarter of their fertile period and potentially facing biological challenges getting pregnant, their fertility decision-making very likely differed considerably from that of the two younger cohorts. Another more technical reason was that this group provided only 7.3% of the information mass for describing the later phases of the fertile period compared to the information that is used to describe the earlier phases. As a result, introducing the third cohort would have added a considerable amount of uncertainty in the estimation procedure. Third, as I was interested in the decision to make the transition to parenthood, I only considered the 3,372 women who were childless and not pregnant at their first observation. Fourth, 84 homosexual women were omitted from the analyses. Fifth, I excluded 112 women who were known to be infertile or who had a partner who was known to be infertile. Sixth, seven respondents were not subject to the analyses because they were observed only once with 14 years, one year before the process time began. Seventh, I excluded 14 respondents because the living distance to their mother was unknown to them. Eighth, I removed all women's observation years after the dependent event applied. The final sample comprised 3,155 women with 11,427 person-years of observation.

Dependent Variable: Decision to Have the First Child

The event of interest was a woman's decision to become a mother, i.e., trying to become pregnant by pursuing proceptive behavior (Miller & Pasta 1995). I operationalized the timing of this decision on an annual basis using a twofold approach. First, the dependent variable took a value of 1 in the respective survey year if the respondent declared that she had tried to become pregnant during the last 12 months (204 events). This question, however, was not asked of pregnant respondents. For them, second, the dependent variable took a value of 1 for their previous year of observation because then the decision to become a mother was most likely made closer to the previous wave (announcing a pregnancy after around the third month; time to pregnancy around three months). I attributed those latter events to one wave earlier (230 events). In total, this procedure yielded 434 events.

Approximation of Anticipated Maternal Child Care Support

I approximated anticipated maternal child care support by its central precondition: women's geographical proximity to their mothers. I operationalized geographical distance as travelling time, which was categorically measured by the question "How much time do you need to get to your mother's dwelling (on a normal day, using normal means of transportation)?" Travelling time is superior to metric distances, which are often calculated on the basis of geocoded distances "as the crow flies," as it captures the actual time consumption by the travel (Phibbs & Luft 1995). I combined these answer categories with detailed information about living arrangements to build a measure of five travelling (or living) distances: "same house (also same household and students denoting the maternal home as a second household)," "less than 10 minutes," "10 minutes to less than 30 minutes," "30 minutes to less than 60 minutes," and "more than 60 minutes."

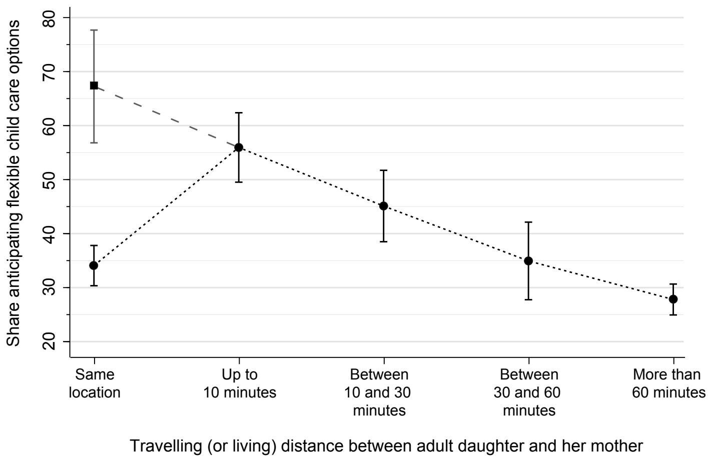
Although the travelling distance to the mother promises to be the best approximation of anticipated maternal child care support currently available and has been applied before for this purpose (Arránz Becker & Lois 2013; Hank & Kreyenfeld 2003), its appropriateness has not been tested empirically. Theoretically, the expectation is that the closer adult daughters live to their mothers, the more likely it is that they will anticipate maternal child care support because smaller distances should render the receipt easier (all else being equal). To test this expectation I employed a question asked in all but the 5th wave to respondents aged 20 and older. It refers precisely to the anticipation of child care, the element that is not explicitly captured (but assumed to be approximated) by the travelling distance.

In a two-step procedure respondents were first asked to what extent having access to flexible child care support was a necessary condition when they were deciding whether to have their first child ("I need access to flexible child care options for the child." Answer categories ranged from "Not at all" to "Absolutely" on a five-point scale). Next, they were asked whether they evaluated this prerequisite as being fulfilled. The term "flexible child care options" denotes only the pool of potential caregivers (both informal and formal) rather than specific individuals. However, previous research provides evidence for which types of relationships are likely to be considered when respondents evaluate who in their local support network is eligible to provide child care. In Germany, other informal providers of child care besides the closest kin (i.e., the child's own parents and grandparents) are seldom utilized. Siblings, friends, and neighbours are rarely used sources of child care support (Hank et al. 2004). Alt & Teubner (2007) found that grandparents were involved in care (at least one day of a typical week) for 34% of children under three years old. The share of other caregivers was far smaller. Siblings were involved in 5% of cases, friends in 5%, and extended kin in 3% of cases.

Taken together, the preceding provides good arguments to assume that (1) daughters consider their mothers to a large extent when determining their pool of potential caregivers and (2) the availability of the woman's mother should be the dominant factor in identifying "flexible child care options." For the empirical analysis, I retrieved the last observed values of these two steps for each respondent. Then I estimated the share of those that anticipated access to assistance with child care (the second question) over the five categories of travelling distance, among those that viewed having access as a prerequisite (the first question; at least value 3; N = 2,291). Figure 4.1 shows the results.

The relation between anticipated child care and travelling distance to the mother corroborates the expectation and thus justifies its use as an approximation. The longer the travelling time, the less adult daughters anticipate that their mothers will provide them with child care support. In other

Figure 4.1. Sensitivity Analysis of the Approximation (N = 2,291)



Note: Data are from the German Family Panel (pairfam), own calculations, weighted results.

words, the greater the distance, the less the mother is considered to be in the pool of caregivers—and because she is such an important caregiver, the less adult daughters have the impression that they have sufficiently flexible child care available to make the decision. This holds for adult daughters who reside apart from their mothers. The low affirmation share of those sharing the same location, leading optically to an inverse u-shaped relation, is counter to the theoretical expectation. Why should daughters residing closest to their mothers anticipate their help (almost) the least? Theoretical arguments suggest that their share affirming flexible child care should be the highest.

The most likely answer to this question lies in the extent to which those residing with their mothers have already considered the transition to parenthood as an action alternative. Previous research on fertility decision-making has repeatedly shown that a feeling of being ready for parenthood often precedes the birth of the first child (Buber-Ennser & Fliegenschnee 2013; Keim 2011). This, however, is typically only reached after a certain degree of maturation into adulthood has taken place. An integral part of this maturation process is establishing one's independence (Goldscheider et al. 2014; Luetzelberger 2014). To gain this independence, as is common for individualistic Northern European countries, in Germany, adult daughters leave their parental home rather early (Billari & Liefbroer 2010). Therefore, (still) residing in the parental household characterizes a situation in which the transition to parenthood typically is not considered an action alternative. As a result, for most respondents in this situation, the answer to the question about "flexible child care options" should denote rather a kind of thought experiment, an abstract event taking place far in the future, because becoming a parent is (at least) two steps ahead of them (first independence by moving out, then having a child), or even three if they do not have a partner yet. On average, those living at home estimate their transition to motherhood as coming 7.4 years after their last observation, which supports the argument that these women have not thought too much about the details of a situation that will take place far in the future. Taken together, this absence of having deliberated about parenthood, for which the positive endpoint would be a feeling of being ready to start a family, is the most likely reason that mothers have not yet become salient as a potential source of child care support.

This explanation is supported by two empirical findings. First, for some, a rather small group (N = 74), who are older, have a partner in a longer relationship, have deliberated with him about childbearing, and have come to the conclusion that, as a couple, they are ready to become parents themselves (expecting to become parents within the next 3.3 years), the mother has become salient as a source of support for her daughter. This is shown in Figure 4.1 in the single square at 67% that shows the highest share of affirmation overall. This corroborates the overall theoretical expectation of a decreasing anticipation of child care support with increased travelling time. Second, the mother becomes more salient as a child care giver the more independent the adult daughter's housing situation becomes, given a negligible change in distance. On the one hand, this finding is reflected in the large leap in anticipation shares between the first two categories. On the other hand, it is supported by further analyses, in which I split the category "same location" into "same household" (N = 556), "same house, parental/self-owned apartment" (N = 55), and "same house, rented apartment" (N = 30). Comparing the resulting shares indicated a step-by-step increase in affirmation with increasing independence from the mother. "Same house, rented apartment" featured the highest share (68%) while "same household" was the lowest (31%).

The suitability of travelling distance as an approximation of anticipated maternal child care support, carried out in the same manner, was further supported in additional analyses shown in Table A4.1. Travelling distance was not substantively related to the extent of (educational or occupational) career commitment. Therefore, I suspended the hypothesis that a negative effect of travelling distance would instead be an effect of the strength of career considerations, because those who lived further from their mothers could have been more dedicated to their careers and therefore delayed the transition to motherhood. Furthermore, travelling distance was not related to fertility-relevant characteristics such as tie strength to mother, ideal number of children, and realistic number of children as shown in Table A4.3.

Parental Pressure

As highlighted in the theory section, a positive effect of geographical proximity to the mother on the transition rate to the decision to become a parent might not only have measured anticipated child care support, but also reflected the likelihood (and/or the perceived intensity) of a woman's mother's social pressure to have grandchildren. To net out social pressure from the approximation of anticipated child care support, I took the respondents' perception of this pressure explicitly into account. This measure was used previously to detect the effect of perceived social pressure on fertility intentions (Balbo & Mills 2011) and the realization of positive intentions (Kuhnt & Trappe 2013) in Germany. Preceded by questions about long-term fertility plans and short-term fertility intentions, the pertinent question was: "My parents think that I should have a child." Answer categories ranged from "disagree completely" to "agree completely" on a five-point scale. In alignment with previous research (Kuhnt & Trappe 2013), I built a categorical variable denoting complete disagreement as "no," the three middle categories as "mildly," and complete agreement as "strongly" perceived pressure (cf., Liefbroer & Billari 2010).

Control Variables

Control variables comprised indicators at the familial, regional, and individual level. At the familial level, I controlled for mother's age, conflict with her, as well as number of siblings. Mother's age was sought to approximate her availability in terms of labor market attachment as well as health status (Aassve et al. 2012). Due to the survey design, the pairfam data did not entail more proxy information about maternal characteristics that would have allowed us to model her availability more precisely. Pairfam used a multi-actor design that surveyed the parents individually. Unfortunately, of the 3,155 women, only 42.2% had any information available from their mothers. Relationship quality has been shown to be a poor predictor of whether a woman's mother would provide child care support, except in the case of conflict-laden relationships (Keim 2011). Therefore, a binary variable indicated whether the respondent and her mother either fought with each other or were angry at each other "often" or "always" (compared to "never," "seldom," and "sometimes"). To control for family-size values (Murphy & Knudsen 2002) and the mothers' potential unavailability due to parallel child care claims from siblings (Kaptijn et al., 2010), I included the number of siblings.

At the regional level, I controlled for the urbanization of the municipality using the BIK region classification (BIK Aschpurwis + Behrens GmbH 2015) provided by the pairfam data. These indicators were sought to control for generations living closer to each other in urbanized areas due to more comprehensive labor, educational, and housing market opportunities (Isengard 2013), as well as to control for differing fertility levels between rural and urban areas (Hank 2001). I distinguished between rural regions up to 50,000 inhabitants (rural), periphery between 50,000 and 500,000 inhabitants (periphery smaller), periphery above 500,000 inhabitants (periphery larger), city centres between 50,000 and 500,000 inhabitants (city smaller), and city centres above 500,000 inhabitants (city larger). In addition, a binary variable indicated whether the respondent was located in Eastern Germany. This controlled for differing fertility profiles between the two parts of Germany, especially the tendency for women in the Eastern part to have children younger (Goldstein & Kreyenfeld 2011).

At the individual level, I included six important determinants of the transition to parenthood identified repeatedly by previous research. First, highest educational degree distinguished between basic secondary school or less (9 or fewer years of education), intermediate secondary school (10 or 11 years of education), or upper secondary school (12 or more years of education). Second, a binary indicator controlled for full-time educational enrolment as being in general secondary school or studying at a university, university of cooperative education, business school, or technical school. This indicator was sought to control for norms of role transition, i.e., completing education before starting a family (Blossfeld & Huinink 1991). Without controlling for educational enrolment, it might well be that living in the same location as the mother has a negative effect on the transition rate because a large part of those living in the same location were pursuing their education and postponed having a baby for this reason. Third, I controlled for relationship status and, fourth, for marital status. Fifth, I applied an indicator varying between 0 and 1 that captured the importance of the professional sphere (education or career) relative to other spheres of life (leisure time activities, social contacts, relationship, and family formation). Respondents were asked to distribute 15 tokens to these spheres of life to denote their relative importance (Bauer & Kneip 2013). This indicator was utilized to control for a general tendency to delay childbearing because of prioritizing one's career. Sixth, I controlled whether the respondent was full-time employed. Process time was measured in years beginning from the age of 15 onwards. Table 4.1 provides an overview of the variables.

As shown in Table 4.1, although it covers the larger part of the fertile period, the overall mean age of 20.7 years (at age 15 process time start with 1) indicates that the sample emphasizes its earlier phases. This feature results from the study's design as a sample of birth cohorts. Since the analysis sample comprised only those who were childless and not pregnant at their first observation, fewer respondents could be retained from the cohort born between 1981 and 1983 (N = 1,112) than from the cohort born between 1991 and 1993 (N = 2,043). For the following analyses, this means that the younger cohort captures rather births to less-educated respondents and the older cohort rather captures births to better educated respondents.

Multiple Imputation of Missing Data

Overall, the variables contained few missing cases. However, the operationalization of social pressure was surveyed only four times: in the first, second, fourth, and sixth wave. As a result, this variable had missing values in 14.7% of the person-years. Importantly, the missing mechanism was known, missing by survey design, and could be assumed to be unrelated to

Table 4.1. Description of Variables (N = 3,155)

	Mean	SD	Percentage missing	Range
Decision	.04			0 – 1
Mother lives within (ref.: more than 60 minutes)				
same location	.65			0 – 1
up to 10 minutes travel time	.06			0 – 1
10 to 30 minutes travel time	.06			0 – 1
30 to 60 minutes travel time	.05			0 – 1
Process time				
Years[a]	6.71	4.69		1 – 19
Familial characteristics				
Perceived social pressure (ref.: mild)				
None	.78		14.70	0 – 1
Strong	.08			0 – 1
Conflict with mother	.23		2.05	0 – 1
Mother's age (in years)	48.21	6.21	4.34	31 – 73
Siblings (ref.: none)				
1	.49		.35	0 – 1
2	.21			0 – 1
3+	.11			0 – 1
Regional characteristics				
Settlement structure (ref.: larger city)				
Rural	.25			0 – 1
Periphery (smaller)	.23			0 – 1
Periphery (larger)	.11			0 – 1
City (smaller)	.18			0 – 1
East Germany	.17			0 – 1
Individual characteristics				
In a partnership	.51			0 – 1
Married	.04		.56	0 – 1
In education	.51		.40	0 – 1
Full-time employed	.21		.07	0 – 1
Importance of professional life	.29	.12	.10	0 – 1
Education (ref.: high)				
low	.45		.14	0 – 1
intermediate	.21			0 – 1
Person years (overall)	11,427			

Note: Data are from the German Family Panel (pairfam), own calculations. [a] Years since age 15 with age 15 denoting the first year.

either the dependent variable or any of the independent variables. Therefore, I applied multiple imputation by a sequence of chained equations and generated 20 imputations (Royston & White 2011; Young & Johnson 2015) to both cohorts separately and combined them afterwards (Gelman et al.

1998). Means and standard deviations after imputation were almost identical to those presented in Table 4.1.

Statistical Model

I employed discrete-time event history analysis using logistic regression (Allison 1982). A woman's time-contingent propensity to make the decision to have her first child was given by the hazard rate λ_{it}. This hazard rate was the conditional probability that a woman *i*'s decision to have her first child occurred at time *t* – under the condition that the woman was still childless. I started with a baseline model (Model 1) to establish the effect of anticipated maternal child care support controlling only for process time and regional characteristics. In Model 2, I added indicators for social pressure as well as familial and individual characteristics to both better estimate the effect and to inspect the extent to which it remained. The models were specified as

$$log\left(\frac{\lambda_{it}}{1-\lambda_{it}}\right) = \alpha + r_t + r_t^2 + \beta_1 x_{it} + \beta_2 z_{it} + \beta_3 w_{it} + \beta_4 u_{it} + \epsilon_{it}. \qquad (4.1)$$

In this equation, α denotes the constant and $r_t + r_t^2$ the linear and squared process time; $\beta_1 x_{it}$ are vectors for the dummies measuring anticipated maternal child care support for woman *i* at time *t*; $\beta_2 z_{it}$ are vectors for the familial, $\beta_3 w_{it}$ for the regional, and $\beta_4 u_{it}$ for the individual characteristics; ϵ_{it} denotes the error term.

Endogeneity and Robustness Checks

Before turning to the results of the discrete-time hazard models, I precede with an empirical investigation of the endogeneity argument outlined in the theory section. The data allowed me to assess the argument's two empirical implications, comparing the previous moving behavior between those who decided to have their first child and those who did not; 2,389 respondents had information available for at least one earlier wave of observation than their last or the one in which they made their decision. For 647 respondents I observed a location change (67 who decided to have their first child, 580 who did not). Because of the low case numbers, I only considered the latest move.

On a related note, it has to be borne in mind that a move might also have happened before the first observation, which I was not able to assess, as the whole migration history of the distances between mothers and their daughters is not available in the pairfam data. The latter is also the reason why I was not able to empirically test the argument in a satisfactory way that daughters may remain close to their mothers because of the future child care support they anticipate to receive. However, analyses based on 20 year olds from the first cohort ($N = 693$) indicate that those who never moved out of reach (i.e., stayed within 30 minutes travelling time) are no different from those who did, with regard to the two measures introduced earlier, estimating the importance of flexible child care options ($p = 0.53$) as well as estimating the availability of flexible child care options ($p = 0.31$). There are also no differences when I compare the answers at later ages (up to 23 years). This at least suggests that staying close is not primarily driven by anticipating child care support. Furthermore, it should be noted that daughters who migrated outside Germany are not part of the analysis. This, however, seems to be a rather rare phenomenon, as official statistics for the year 2010 show that only around 0.3% of all 18–25 year old German women migrated (Statistisches Bundesamt 2012).

As outlined earlier, the first empirical implication predicted that those who made the decision should have been more reluctant to bridge distances that would have rendered receiving maternal child care support rather difficult. To test this conservatively, I denoted travelling distances of at least 30 minutes as such costly distances impeding on maternal time transfers. Empirically, this first implication did not seem to apply, as the data indicated the opposite: 19.4% of those having made the decision and 7.6% of those not having made the decision moved beyond a travelling distance of 30 minutes (this difference is statistically significant at $p < 0.03$). The second empirical implication was that those who made the decision and who lived at a rather long travelling time before to be more likely to have moved closer prior to the decision to set the stage. For this, I found both no substantive and no statistical difference ($p = 0.26$) between the groups: 4.5% of those who had decided to have a baby and 1.6% of those who had not decided to have a baby moved into close proximity (i.e., from beyond 30 minutes to within 30 minutes). Furthermore, the average time lag between the last move and the last observation in the data is 1.12 waves for those who did not make the decision and 0.96 waves for those who made the decision (see Table A4.2 for the distributions). This difference is neither substantively nor statistically significant (p = 0.29). These conclusions are further confirmed by a multinomial regression model under control of age, having a partner, being married, education level, and still being in education (N = 2,363; using non-imputed data). The dependent variable differentiates between the categories "remaining below 30 minutes distance," "remaining beyond 30 minutes distance," "moving from beyond to below," and "moving from below to beyond". The results are presented in Table A4.4. Having made the decision to have the first child did not have a statistically significant effect on having moved closer (p = 0.35) nor of having moved farther (p = 0.16). Taken together, these findings suggest that women who decided to make the transition to parenthood rather did not prepare this decision by adjusting their residential location beforehand to facilitate their future receipt of maternal child care support.

Furthermore, I conducted a series of robustness checks shown in Table A4.5. The results presented in the following section are robust to: (1) including the pairfam subsample DemoDiff (a sample including additional respondents from East Germany; person-years: 11,999); (2) including men in the analysis (24,541); (3) excluding singles (5,823); (4) including the third cohort (12,250); (5) excluding co-residency in the youngest cohort (4,702); and (6) excluding unplanned pregnancies from the youngest cohort (11,341).

4.4 RESULTS

Table 4.2 shows the estimated coefficients of the two discrete-time hazard models for the transition to the decision to make the transition to parenthood. I will concentrate on the main results, as the control variables merely confirmed what had already been well known from previous research.

Model 1, controlling for age and regional characteristics, establishes the effect of anticipated maternal child care support for adult daughters. More precisely, it conveys three important findings. First, it shows that those adult daughters who reside outside the parental home within a distance rendering flexible maternal child care support feasible (i.e., up to 30 minutes travelling time) may profit from this distance. They may be more likely to anticipate such support and thus are more likely to make the decision, compared to those who live more than one hour away and whose mothers

Table 4.2. Discrete-Time Hazard Models for Women's Transition to Making the Decision to Have the First Child (N = 3,155)

	Model 1 Est.	SE		Model 2 Est.	SE	
Process time						
Years[a]	.32	(.07)	***	.17	(.08)	*
Years squared	-.01	(.00)	*	-.00	(.00)	
Mother lives within (ref.: more than 60 minutes)						
same location	-.30	(.13)	*	-.06	(.15)	
up to 10 minutes travel time	.73	(.16)	***	.34	(.17)	*
10 to 30 minutes travel time	.87	(.16)	***	.62	(.17)	***
30 to 60 minutes travel time	.19	(.20)		-.23	(.22)	
Familial characteristics						
Perceived social pressure (ref.: mild)						
None				-.46	(.20)	*
Strong				.59	(.17)	**
Conflict with mother				.08	(.15)	
Mother's age (in years)				-.03	(.01)	**
Siblings (ref.: none)						
1				.26	(.17)	
2				.32	(.19)	†
3+				.89	(.22)	***
Regional characteristics						
Settlement structure (ref.: larger city)						
Rural	.47	(.15)	**	.23	(.17)	
Periphery (smaller)	.39	(.16)	*	.19	(.17)	
Periphery (larger)	.22	(.20)		.07	(.21)	
City (smaller)	-.02	(.17)		-.09	(.19)	
East Germany	.70	(.12)	***	.98	(.14)	***
Individual characteristics						
In a partnership				1.68	(.20)	***
Married				1.74	(.15)	***
In education				-.52	(.19)	**
Importance of professional life				-2.64	(.66)	***
Full-time employed				.15	(.14)	
Education (ref.: high)						
Low				1.02	(.17)	***
Medium				.53	(.14)	***
Constant	-5.74	(.34)	***	-4.68	(.78)	***
Person years		11,427			11,427	
Events		434			434	

Note: Data are from the German Family Panel (pairfam), own calculations, weighted results. Logistic regression coefficients are shown. Standard errors in parentheses. Model 2 is based on 20 sets of imputed data. [a] Years since age 15 (age 15 denotes the first year). $^{\dagger}p < .10$, $^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$

therefore rather do not offer a feasible option for flexible child care support. Statistically, however, both indicators do not differ (two-sided t-test

Table 4.3. Main Result

	Model 3		
	Est.	SE	
Mother lives within (ref.: more than 30 minutes)			
same location	.01	(.15)	
up to 30 minutes travel time	.54	(.14)	***
Person Years		11,427	
Events		434	

testing for the two estimates being different is not significant; $p = 0.15$). Substantively, this comes as no surprise, as it is very likely that at a travelling time of around 10 minutes the perceived marginal costs of travelling some more minutes are considerably lower than at around 30 minutes. In other words, a threshold of 30 minutes should better discriminate between women who do and do not anticipate childcare support from their mothers than a threshold of 10 minutes. The second finding corroborates this: those who live between 30 and 60 minutes away do not differ from those living more than 60 minutes away. Third, those living with their mothers in the same house (or even the same household) are markedly less likely to decide to have a baby than those for whom their mother is likely to be hardly or not at all available for flexible child care support. As outlined earlier, this seems to contradict the theoretical expectation, as living in the same location represents the shortest distance, and thus should render child care support very easy and, as a result, positively influence the decision to become a mother. However, this living situation is at the same time characterized by strong normative expectations to first complete one's education and to gain independence before having the first child. The data show that most respondents living in the same location actually reside in the same household and are younger, do not have a partner yet, let alone being married, and, importantly, are still in education. Therefore, once Model 2 controlled for these compositional differences by introducing familial and especially individual characteristics such as educational attainment and partnership status, the negative effect of living in the same location vanished.

Narrowed down to the essence of this chapter, the anticipation argument, these three findings lead to an ideal-typical distinction between three qualitatively different situations: not anticipating maternal support although it would be feasible, anticipating such support, and not anticipating such support because it would not be feasible. In a further model specification that is very similar to Model 2, I combined the travelling distance indicator to reflect these three groups (same location, within 30 minutes travelling time, over 30 minutes travelling time or unknown). This specification, shown in Table 4.3, provided my best estimate of the strength of the effect of adult daughters' anticipation of childcare support from their mothers: it increased the transition rate to the decision to have the first child by 71% ($e^{0.54}$; 95% confidence interval from 38% to 137.4%). The strength is remarkable, but rather medium-sized compared to the strength of the other effects. Whereas it is comparable to the strength of, e.g., perceiving strong parental pressure vs. mild parental pressure, and to the strength of a medium level of education vs. a high level of education, it is clearly weaker than the strength of established determinants of fertility such as being married or having been raised in a large family (i.e., three or more siblings) vs. being an only child.

4.5 DISCUSSION

Based on theoretical arguments from sociology, economics, and evolutionary biology, this chapter explored whether women who anticipated future child care support from their mothers were more likely to make the decision to have their first child. The timing of this decision was operationalized using a two-fold approach, with respondents either declaring having tried to become pregnant during the last 12 months or being pregnant at the time of the interview. Empirical evidence based on discrete-time hazard models (N = 3,155) using six waves from the German Family Panel revealed that a proxy measure of anticipation of such support increased the transition rate to motherhood by 71%.

Anticipated maternal child care support was approximated using detailed ordinal information on its primary precondition: geographical proximity, measured as the travelling (or living) distance between the adult daughter and her mother. Although this strategy has been employed by previous research (Arránz Becker & Lois 2013; Hank & Kreyenfeld 2003), I provided the first empirical investigation of both whether it approximated this anticipation at all and whether travelling distance was endogenous to the transition to parenthood. The empirical evidence suggested that the approximation of anticipated child care support from the woman's mother by means of travelling distance was warranted.

This chapter contributes to the literature on intergenerational relationships and fertility decision-making in two ways. First, more generally, it corroborates previous sociological and demographic research in pointing out the importance of social interaction for fertility decisions. Importantly, however, it goes beyond previous contributions in this specific line of research by demonstrating its effectiveness at the earliest point in time in women's fertility histories, at the decision to make the transition to motherhood. This was possible by exploiting a measure of proceptive behavior rather than either the timing of a live birth or the timing of the conception leading to a live birth, which are the events typically employed.

Second, more specifically, it more accurately estimated the strength of a social interaction effect between adult daughters and their mothers at the transition to parenthood (Arránz Becker & Lois 2013; Hank & Kreyenfeld 2003). I achieved this using two novelties. On the one hand, the effect was net of maternal (or parental) pressure for grandchildren, which was necessary to control for when approximating anticipated maternal child care using travelling distance to the mother, because travelling distance and social pressure were thought to be negatively correlated. Therefore, not controlling for social pressure would likely overestimate the effect (due to multiple imputation, an empirical test of this contention was not possible as it would have involved a comparison of nested but not same-sample logistic regression coefficients). The effect of social interaction due to social pressure amounted to an increase of the transition rate by 80%. On the other hand, the effect of anticipated childcare support from the woman's mother was calculated only for whom it applied to due to a comprehensive sample selection. The results of the multivariate analyses showed that the situation in which flexible maternal child care support would have been theoretically expected to be the highest also featured characteristics known to discourage the decision to become a parent very strongly, primarily being enrolled in education (Blossfeld & Huinink 1991). Consequentially, measures that incorporate living in the mother's household into the operationalization of anticipating child-care support would likely underestimate its effect.

This chapter had some empirical limitations. First, although I was able to exploit an extraordinarily rich indicator of geographical proximity (net of social pressure), it remained a proxy for anticipated maternal child care support and did not allow for statements about the actual availability of the future grandmother (i.e., the likelihood of realization), which is likely part of the formation of an anticipation. Using available information, I controlled for basic characteristics of the mother such as her age, conflict with her daughter, and number of children. Other important characteristics that have been shown to affect the likelihood of grandmothers' provision of child care in the presence of grandchildren, like her labor market attachment, health, number of other grandchildren, education, and partnership status (Hank & Buber 2009; Kaptijn et al. 2013), were not available due to the survey design. Furthermore, it is very likely that the individual strength of anticipation varies with the envisioned amount of support, which I was not able to measure. Second, although the results corroborated the assumption that the travelling distance was not endogenous to fertility at the time the decision was made, the sample on which this conclusion was based included only a fraction of the respondents and only the latest move. To more rigorously test this argument, complete histories of distances between adult daughters and their mothers since they first left home up to their main childbearing ages would be needed. In some years, this will be feasible using the pairfam data. Third, due to the survey design, the sample did not represent the complete fertile age span. Only ages 15 to 33 were included. Therefore, it should be noted that the generalizability of the findings is limited to this age span.

Lastly, this chapter emphasizes the continuing importance of the older generation for the fertility decisions of the younger generation. On the one hand, this has become evident from the anticipation argument. Women in the position to anticipate having access to maternal assistance with child care in the future were more likely to make the decision to have their first child than those whose mothers were not (as easily) available to them. On the other hand, women perceiving strong social pressure from their parents made their decisions faster. Taken together, the findings showed that the older generation was subject to expectations on the part of the younger generation, but that this also held the other way around – and both directions of expectations were elements of the younger generation's fertility decision-making process.

4.6 APPENDIX

Table A4.1. Relationship Between Travelling Distance and Career Commitment (N = 2,712)

	Mean	SE
Mother lives within		
same location	.31	(.00) *
up to 10 minutes travel time	.26	(.01) ***
10 to 30 minutes travel time	.25	(.01) ***
30 to 60 minutes travel time	.29	(.01)
more than 60 minutes	.30	(.00)

Note: Difference in mean values is statistically tested against the reference category "more than 60 minutes.' $^{*}p < .05$,** $p < .01$,*** $p < .001$

Table A4.2. Distribution of Respondents who Changed Residence by the Number of Years that Precede the Last Observation

	Respondents not having made the decision to have a first child		Respondents having made the decision to have a first child	
	Frequency	Percent	Frequency	Percent
Same Year	231	40.2	33	50.0
1 year before	166	28.9	14	21.2
2 years before	91	15.9	9	13.6
3 years before	48	8.4	8	12.1
4 years before	38	6.6	2	3.1
N	574	100	66	100

Table A4.3. Fertility-Relevant Characteristics Across Living Distances

	Tie strength to mother		Ideal number of children		Realistic number of children	
	Mean	SE	Mean	SE	Mean	SE
Mother lives within						
same location	4.38	(.02) ***	2.12	(.03) ***	1.89	(.02) **
up to 10 minutes travel time	4.31	(.06)	2.06	(.05)	1.77	(.05) *
10 to 30 minutes travel time	4.16	(.06)	2.08	(.05)	1.80	(.05)
30 to 60 minutes travel time	4.20	(.07)	2.18	(.07)	1.95	(.06)
more than 60 minutes	4.25	(.03)	2.19	(.03)	1.92	(.02)
N	2,889		2,845		2,649	

Note: a Values range from 1 = "not close at all" to 5 = "very close." Difference in mean values is statistically tested against the reference category "more than 60 minutes." $^{*}p < .05$,** $p < .01$,*** $p < .001$

Table A4.4. Multinomial Regression on Moving Behavior (N = 2,350)

	Est.	SE	
remaining below 30 minutes distance	(Base outcome)		
remaining beyond 30 minutes distance			
Decision	-1.14	(.18)	***
Process time	.05	(.01)	***
In a partnership	-.03	(.10)	
Married	-.11	(.23)	
Education (ref.: high)			
Low	-1.97	(.13)	***
Medium	-1.03	(.11)	***
In education	.18	(.11)	†
Constant	.50	(.16)	**
moving from beyond to below			
Decision	.67	(.71)	
Process time	.19	(.08)	*
In a partnership	.52	(1.13)	
Married	1.90	(.85)	**
Education (ref.: high)			
Low	-14.43	(.66)	***
Medium	-.07	(.87)	
In education	2.94	(.84)	***
Constant	-8.73	(1.50)	***
moving from below to beyond			
Decision	.65	(.47)	
Process time	.01	(.04)	
In a partnership	.73	(.45)	
Married	-25.01	(.33)	***
Education (ref.: high)			
Low	-.44	(.53)	
Medium	.21	(.43)	
In education	-1.62	(.69)	*
Constant	-3.58	(.58)	***

Note: Data are from the German Family Panel (pairfam), own calculations, weighted results. Multinomial logistic regression coefficients are shown. Standard errors in parentheses. $^{\dagger}p < .10$, $^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$

Table A4.5. Discrete-Time Hazard Models for Robustness Checks

	(0) Model 2		(1) Including DemoDiff		(2) Including both sexes		(3) Including only partnerships		(4) Including third cohort		(5) Excluding co-residency in cohort 1		(6) Excluding unplanned pregnancies from cohort 1	
	Est.	SE	Est.	SE	Est.	SE	Est.	SE	Est.	SE	Est.	SE	Est.	SE
Process time														
Years[a]	.17	(.08) *	.21	(.08) *	.27	(.06) ***	.24	(.10) *	.24	(.05) ***	.76	(.18) ***	.15	(.08) †
Years squared	-.00	(.00)	-.00	(.00)	-.01	(.02) **	-.01	(.00)	-.01	(.00) ***	-.03	(.01) ***	-.00	(.00)
Mother lives within (ref.: more than 60 minutes)														
same location	-.06	(.15)	-.02	(.15)	.11	(.12)	-.05	(.17)	-.17	(.14)	-.30	(.23)	-.08	(.15)
up to 10 minutes travel time	.34	(.17) *	.31	(.17) †	.42	(.13) **	.39	(.18) *	.15	(.15)	.39	(.18) *	.39	(.17) *
10 to 30 minutes travel time	.62	(.17) ***	.57	(.18) **	.51	(.13) ***	.54	(.18) **	.46	(.16) **	.64	(.19) ***	.62	(.18) ***
30 to 60 minutes travel time	-.23	(.22)	-.31	(.22)	.04	(.17)	-.29	(.23)	-.37	(.20)	-.22	(.24)	-.21	(.22)
Familial characteristics														
Perceived social pressure (ref.: mild)														
None	-.46	(.20) *	-.42	(.19) *	-.48	(.14) **	-.53	(.20) **	-.42	(.17) *	-.46	(.21) *	-.46	(.19) *
Strong	.59	(.17) **	.53	(.16) **	.43	(.12) ***	.64	(.19) **	.61	(.14) ***	.58	(.18) **	.68	(.15) ***
Conflict with mother	.08	(.15)	.12	(.15)	.20	(.12) †	-.05	(.20)	.12	(.14)	-.39	(.21) †	.05	(.16)
Mother's age (in years)	-.03	(.01) **	-.04	(.01) **	-.04	(.01) **	-.03	(.01) *	-.02	(.01) *	-.03	(.02) *	-.02	(.01) *
Siblings (ref.: none)														
1	.26	(.17)	.18	(.17)	.06	(.11)	.35	(.18) *	.31	(.15) *	.57	(.19) **	.26	(.17)
2	.32	(.19) †	.26	(.19)	-.00	(.13)	.42	(.20) *	.28	(.17)	.48	(.22) *	.29	(.20)
3+	.89	(.22) ***	.82	(.22) ***	.47	(.15) **	1.01	(.24) ***	.92	(.20) ***	.89	(.27) **	.84	(.22) ***
Regional characteristics														
Settlement structure (ref.: larger city)														
Rural	.23	(.17)	.27	(.16)	.13	(.12)	.17	(.18)	.08	(.15)	.21	(.20)	.22	(.17)
Periphery (smaller)	.19	(.17)	.28	(.17)	-.06	(.13)	.19	(.18)	.02	(.15)	.14	(.20)	.16	(.17)
Periphery (larger)	.07	(.21)	.12	(.21)	.05	(.15)	-.02	(.22)	-.11	(.20)	-.07	(.24)	.02	(.21)
City (smaller)	-.09	(.19)	.01	(.19)	-.10	(.13)	-.18	(.20)	-.10	(.16)	-.09	(.21)	-.08	(.19)
East Germany	.98	(.14) ***	.85	(.14) ***	.73	(.10) ***	1.06	(.15) ***	.95	(.13) ***	1.05	(.17) ***	1.02	(.14) ***
Individual characteristics														
In a partnership	1.68	(.20) ***	1.81	(.19) ***	1.95	(.14) ***	/	/	1.79	(.19) ***	2.41	(.35) ***	1.66	(.19) ***
Married	1.74	(.15) ***	1.69	(.14) ***	1.65	(.10) ***	1.64	(.14) ***	1.60	(.13) ***	1.63	(.15) ***	1.69	(.15) ***
In education	-.52	(.19) **	-.50	(.20) **	-.33	(.14) *	-.56	(.21) **	-.43	(.18) *	-.76	(.30) *	-.51	(.19) **
Importance of professional life	-2.64	(.66) ***	-2.57	(.68) ***	-2.46	(.48) ***	-3.08	(.71) ***	-2.96	(.58) ***	-3.91	(.72) ***	-2.50	(.65) ***
Full-time employed	.15	(.14)	.14	(.14)	.08	(.10)	.14	(.14)	.20	(.12)	.08	(.09)	.16	(.14)
Education (ref.: high)														
Low	1.02	(.17) ***	.98	(.17) ***	.71	(.11) ***	.99	(.18) ***	.91	(.15) ***	.99	(.23) ***	1.05	(.18) ***
Medium	.53	(.14) ***	.48	(.13) ***	.38	(.10) ***	.54	(.14) ***	.35	(.13) **	.48	(.15) **	.54	(.13) ***
Constant	-4.68	(.78) ***	-4.75	(.78) ***	-4.87	(.57) ***	-4.42	(.83) ***	-5.04	(.67) ***	-6.53	(1.44) ***	-4.64	(.59) ***
Person years		11,427		11,999		24,541		5,823		12,250		4,702		11,341
Events		434		455		781		400		513		359		427

Note: Data are from the German Family Panel (pairfam), own calculations, weighted results. Logistic regression coefficients are shown. Standard errors in parentheses. Models are based on 20 sets of imputed data. [a] Years since age 15 (age 15 denotes the first year). † $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$

Quantifying the Impact of Social Interaction on First Births at the Population Level

ABSTRACT

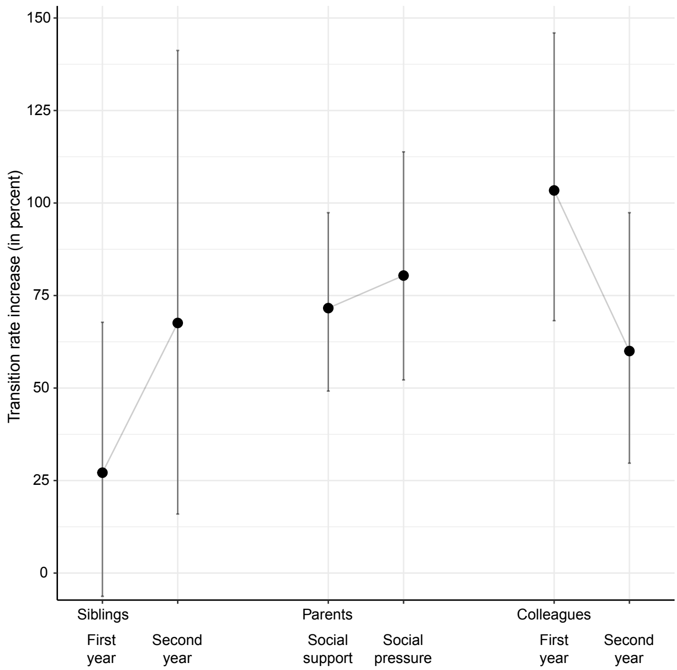
The impact of fertility-relevant social interaction effects remains unclear. This chapter engages in a counterfactual microsimulation for the year 2010 using the estimates from the previous three chapters as input parameters and translates them into population-level measures. Without fertility decision-making about first births being influenced by important others, this simulation suggests that about 75,000 fewer first children would have been born. This amounts to 22.5% of all first borns in this year. Parents' impact was the largest, followed by colleagues' about half as strong and siblings', whose aggregate-level effect was negligible. Taken together, social interaction effects amounted to an average decrease of the Total Fertility Rate by around 15%.

5.1 INTRODUCTION

This chapter builds cumulatively upon the results of the three previous chapters, which contributed to establishing fertility-relevant social interaction effects among colleagues, siblings, and parents. Technically, the chapters' main results constitute increases in transition rates to motherhood based on discrete-time hazard models estimated via logistic regression models (i.e., exponentiated regression coefficients). Based only on these results, shown in Figure 5.1, however, it is very hard to provide clear indication of the strength of these social interaction effects. What does, for example, a statement like the following tell: "In the year after a colleague gave birth, transition rates to first pregnancy double" (page 15). Taken as is, not too much. I would certainly wish for an easy-to-understand conversion of this hard-to-grasp exponentiated coefficient of a logistic regression model. My offer in this vein is a very simple measure that should be intuitively understood by basically everyone: birth counts.

This chapter is a reproduction of a single-authored manuscript titled "Fertility and Social Interaction: Quantifying the Population-Level Impact", which is currently in preparation.

Figure 5.1. Social Interaction Effect Estimates



Note: Values denote increases in transition rates to motherhood based on discrete-time hazard models estimated via logistic regression models and thus consitute exponentiated regression coefficients. Estimates obtained from Chapters II–IV. First and second year indicate the strength of the influence of a birth event by the respective interaction partner preceeding the current time point (i.e., the birth event occurred within one or two years earlier).

In the following, based upon a microsimulation for the year 2010, which takes the estimates from the three previous chapters as input parameters, I provide estimates of the population-level (i.e., macro-level) strength of social interaction effects on first births. Put differently, I counterfactually calculate how many first children would not have been born in a specific year if every women within the microsimulation would have made the decision whether to have her first child within that year independent of the (fertility-relevant) social interaction with colleagues, siblings, and parents. As a result, this chapter boils down to one single number, a number that expresses the strength of social interaction effects.

5.2 MICROSIMULATION

To estimate the population-level impact of social interaction effects, I have to generate a data structure based upon which the influence of certain interaction partners may be induced from. With regard to the three interaction partners under scrutiny (colleagues, siblings, and parents), the simulation model has to build up kinship ties between sisters and mothers and simulate a labor market in which the women of the population are connected to each other through the firms in which they work.

In total, I ran 150 models, each with a starting population of 50,000 women.[1] Mortality rates were taken from the human mortality database (provided by a consortium of demographic research institutes). For simplicity, in- and outmigration were not considered. The simulation was implemented in R (Version 3.3). Overall, the microsimulation proceeds in two steps: First, a calibration model (covering the years 1958–2008) to establish the kinship structure and, second, a main model (covering the years 2009–2010) to counterfactually estimate the strength of (fertility-relevant) social interaction effects.[2]

Calibration Model: Kinship Ties

The purpose of the calibration model is to generate artificial populations for the simulation of the societal impact of social interaction effects among siblings and parents. The general logic of this model is very simple and rests on few assumptions and input data. Starting out with a population of unconnected females, over time females will have (multiple) children and, thereby, year-by-year kin relations will emerge in this population. Although kin relations like cousins and grandchildren also emerge, I am only interested in two forms, mother-daughter relations and sister relations. This structure, then, allows investigating social interaction effects between women and their sisters as well as women and their mothers.

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[1] Part of this work was performed on the computational resource bwUniCluster funded by the Ministry of Science, Research and the Arts Baden-Württemberg and the Universities of the State of Baden-Württemberg, Germany, within the framework program bwHPC.

[2] In all figures, confidence intervals at 95% significance level are shown.

The time-span needed to arrive at this structure equals the upper bound of the fertile age-span (i.e., $\sim$49 years of age). After menopause, a woman of the initial population may not change her daughters' family structure anymore. Taken together, this means that around 50 years of information about fertility transitions are necessary to develop a completely connected kinship structure for those who are in their fertile age-span. To this end, it would be very good to have age-specific rates that indiciate the probability that a woman in a particular age will have a child. And it would be even better, if they would be parity-specific, meaning that I could employ several age-specific rates restricted to whether the women are still childless, have one child, have two children and so forth. For Germany, age- and parity-specific transition rates from official statistics are only available from 2009 onwards because until then official birth statistics did not record mothers' previous number of children when they gave birth (Statistisches Bundesamt 2010).

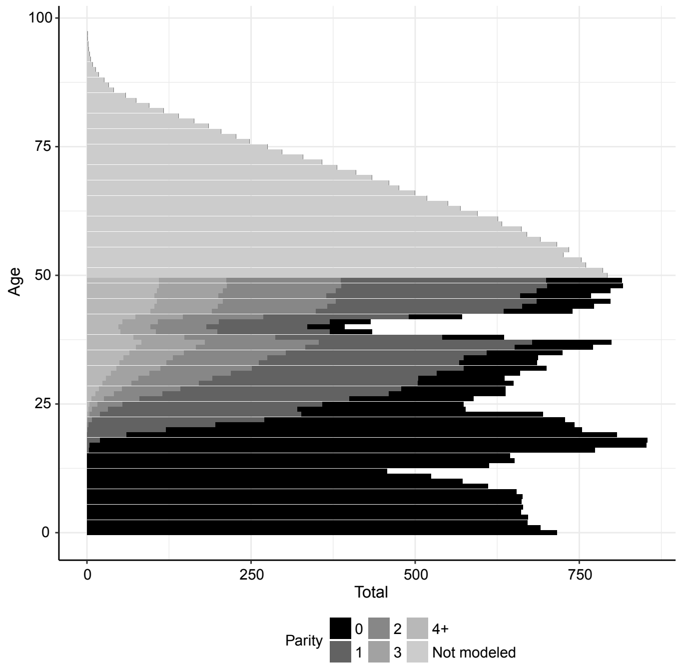
However, estimated parity- and age-specific transition rates are available for the years 1958 to 1985 (Birg et al. 1990), 1986 to 1995 (Kreyenfeld 2002), and 2001 to 2008 (Kreyenfeld et al. 2010). The unknown transition rates for the five years between 1996 and 2000 were imputed using linear interpolation.[3] As a result, the initial population resembles the female population as of 1958 with respect to their age-distribution on the basis of 50,000 actors. Proceeding in steps of one year, females give birth following the age- and parity-specific fertility rates of that specific year. Only female children become new members of the population.[4]

The precondition to implement fertility behavior, which follows age- and parity-specific transition rates, constitues an age-specific parity distribution, in my case that of the year 1958. Official statistics, however, provide this information at the earliest from 2008. Therefore, following the method proposed by Rallu & Toulemon (1994: 66), I estimate the age-specific parity distribution of 1958 for a synthetic cohort (of 1,000 childless 15-year old women; $N(15,0) = 1{,}000$) that follows the age- (x) and parity(r)-specific

[3] I handled each value of the age- and parity-specific transition rate as an element of a year-based time-series. Because these 140 time-series (four parities with information on 35 years each) were not subject to seasonality, linear interpolation was applicable.

[4] A constant sex ratio at birth of 0.487 was assumed throughout the entire microsimulation.

Figure 5.2. Age- and Parity Distribution of the Initial Population



Note: Information for the year 1958. Calculations based on age- and parity-specific fertility rates of Birg et al. (1990) applying the method proposed by Rallu & Toulemon (1994).

transition probabilities $q(x, r)$ of the year 1958. The logic of the interpretation is identical to that of the TFR, one of demography's most prominent fertility measures. Starting out with the number of childless women,

for $x \geq 16, r = 0$

$$N(x, 0) = N(15, 0) \times \prod_{15 \leq y < x} (1 - q(y, 1)) \tag{5.1}$$

the number of women of each parity at each age is calculated with reference to the number of women with one parity lower

for $x \geq 16, r \geq 1$

$$N(x, r) = \sum_{15 \leq y < x} \left(N(y, r - 1) \times q(y, r) \times \prod_{y < z < x} (1 - q(z, r + 1)) \right). \tag{5.2}$$

Calculating the shares based on the numbers of women at each age and parity and applying these shares to the official statistics' age-specific population size provides the estimate for the parity distribution shown in Figure 5.2.[5] In other words, it shows (1) the number of women in the initial population in a one-year age-group and (2) how many of these women have how many children.

Accuracy of the Simulated Population

The combination of these few, precise ingredients (age distribution; parity distribution; age- and parity-specific transition rates) yields a population (on average, with a size of 46,621 women) that very closely resembles that of Germany in 2008. Figure 5.3 shows the average population-level characteristics of both the evolution of these characteristics over time (panels (a) and (b)) as well as their values in the target year (panels (c) to (e)).

Albeit not identical, the simulated population mirrors the German population with regard to a wide array of conventional fertility and overall population measures to a great extent. Most generally, as shown in Panel (a), the TFR is well met, featuring little deviation of the simulated from the empirical TFR by only -1.5%. For the average age at first birth (Panel (b)) the deviation from estimates obtained from perinatal statistics amounts to only 1.7% (i.e., by 0.5 years). Panel (c) shows that the age distributions overlap strongly although the relative size of the age-group of 15–49 subject to fertility transitions is by -5.1% slightly underrepresented. Panel (d) shows only a small average deviation of 0.6% in the share of childless women over the seven age-groups defining the fertile age-range. Based on additional analyses from the Socio-Economic Panel Study (SOEP; the largest representative household survey in Germany; Schupp 2009), Panel (e) indicates that family characteristics are realistically modelled. It shows that the sibship size, the average sibship size for sisters, the average age difference between sisters, as well as the average age difference between daughters and their mothers deviate only negligibly from the SOEP esti-

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[5] From official statistics, the earliest information on German age-specific parity distributions was available for the German Democratic Republic (GDR) for the year 1986. Employing this as the initial parity distribution indicated that the simulated population was robust to alternating specifications. However, on a variety of measures the method proposed by Rallu & Toulemon (1994) was superior, providing higher accuracy.

mates. Taken together, this amount of convergence on this array of indicators underscores the suitability of the simulated population to investigate the population-level strength of social interaction effects on the transition to motherhood.

Main Model: Social Structure

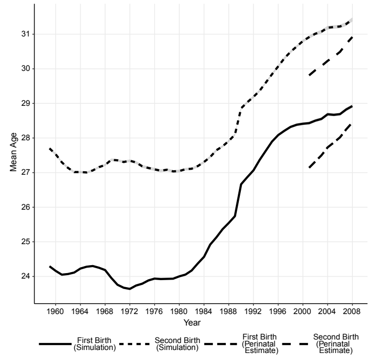
The vantage point of the main model is the year 2008, as produced by the calibration model. From there it proceeds for two years to 2010 in the in the same way as the calibration model, but based on input data from official statistics. The reason for the two years is that the input parameters from Chapters II and III require two years of prior information for their application. The main model seeks to estimate the population-level impact of the social interaction effects for siblings, parents, and colleagues as depicted in Figure 5.1. For siblings, the population-level impact of social interaction effects can be estimated from the kinship structure right away. Estimation for parents and colleagues necessitates additional information.

For parents, the overall effect is composed of two mechanisms, social support and social pressure whose strength has been estimated in Chapter IV. Social support is based on the notion of (potential) availability of the mother for child care provision, approximated with reference to their spatial location in terms of travelling distance. In Chapter IV, I only found a positive effect for those women living outside the parental home with travelling distances not exceeding 30 minutes. Within the simulation model, women are ascribed with this travelling distance in two steps. First, age-specific probabilities to live together with the parents are used to designate whether a daughter lives in the same household as her mother. This information is obtained from official statistics, which provide these information for the years 2009 and 2011 (Statistisches Bundesamt 2011, 2013b). For 2010, I imputed missing values using mean replacement between the preceeding and the following year in the respective age category. Second, for those daughters not living with their parents, empirical mean estimates from the entire pairfam study (six waves) designate the age-specific share living within a travelling distance of 30 minutes from the mother. The obtained empirical distribution was smoothed using a moving average (MA(3)). Social pressure emphasizes more or less explicitly expressed desires of parents for grandchildren. Chapter IV operationalized this using

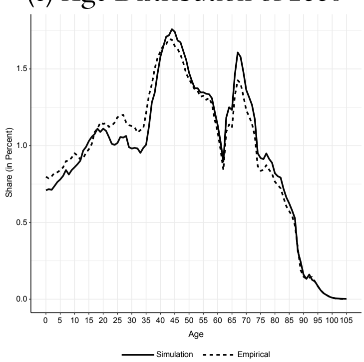
Figure 5.3. Accuracy of the Simulated Populations

(a) TFR

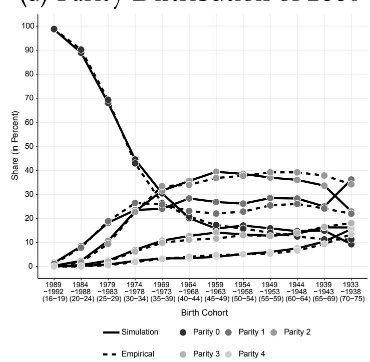
(b) Mean Age at First and Second Child Birth



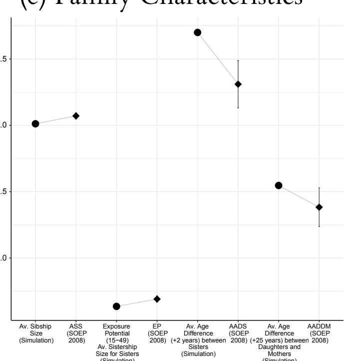
(c) Age Distribution of 2008



(d) Parity Distribution of 2008



(e) Family Characteristics[a]



Note: Averages from 150 simulation runs. [a] Own analyses of the Socio Economic Panel Study of the year 2008, weighted results.

the pairfam question “My parents think that I should have a child.” Answer categories ranged from “disagree completely” to “agree completely” on a five-point scale. I constructed a categorical variables in alignment with previous research (Kuhnt and Trappe 2013) denoting complete disagreement as “no”, the three middle categories as “mildly”, and complete agreement as “strongly” perceived pressure (cf., Liefbroer and Billari 2010). Again, based on the entire pairfam study I smoothed the obtained empirical distribution using a moving average (MA(4)).

For colleagues, I had to simulate a labor market, in which female employees are attached to businesses so that they may be colleagues. Empirically, different businesses or firms differ in size and, consequentially, show varying numbers of female employees. While this feature introduces complexity, it was indispensable to accommodate variation in the likelihood of social interaction effects to unfold. Put differently, more women in a firm indicate more birth events (i.e., more potential exposure) and thus render social interaction effects in such a firm more likely.

My labor market simulation is a reconstruction of the detailed assessment of females, and specifically their employment in certain firms, in the German labor market, which was provided by Fischer et al. (2009). Their assessments are based on results from the IAB Establishment Panel (Bellmann 2014) as of 2008, a representative survey among around 15,500 businesses. My generation of the business landscape, following closely the descriptions provided by Fischer et al. (2009), proceeded in three steps. First, based on the sample size within the simulation model, I estimated the size of the workforce from which I then inferred the number of firms to be generated. Although the following calculations are straightforward, they are necessary because the authors’ descriptions also include male employees, which I have to take into account. The authors have 15,456 establishments in their data and 34,184 employees (0.45 establishments per employee). The share of females across all employees is 44%. Thus, determining the number of firms of each simulation requires knowing the female workforce. This is achieved on a yearly basis by age-specific employment probabilities for the year 2015 (differentiating between mothers and childless women; Institut Arbeit und Qualifikation 2017). These probabilities designated females’ attachment to the labor market.

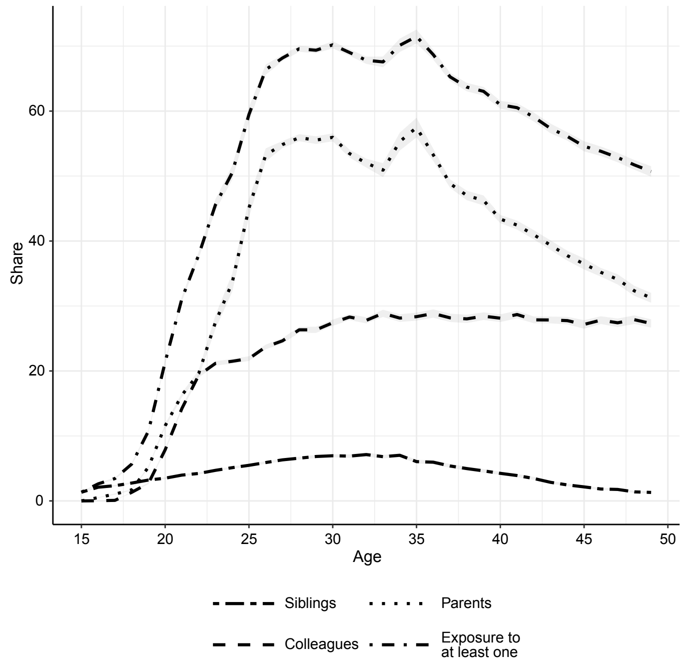
As stated before, firms vary in staff size and share of female employees. Following the nomenclature of the Federal Employment Agency, these firms were distributed according to four categories of staff size: 72% were very small businesses (two to nine employees), 23% small businesses (ten to 49 employees), 4% medium-sized businesses (50 to 250 employees), and 1% large firms (more than 250 employees). The concrete size of each firm within each of these four categories is estimated from a uniform distribution.[6] This strategy generates more jobs than necessary; however, this should not introduce bias because it generates them randomly. Additionally, within these categories, these firms vary according to the extent to which they are either male-dominated (less than 30% females), balanced (between 30% and 70% females) and female-dominated (more than 70% females). The authors report the combination of these two characteristics and the shares across their sample of establishments. Based on these distinctions and figures, I repeatedly generated business landscapes that mirrored the empirical patterns. Female employees, then, were distributed randomly to firms. Through the estimation of the business landscape, especially through very large firms, the workforce supply was not met. Therefore, I removed firms larger than 50 employees. Besides technical necessity, this restriction to smaller businesses also had a substantive reason. While I miss information about departments, larger firms may overestimate the exposure to colleagues' birth events because colleagues' actual propensity to know each other likely decreases with staff size (see Chapter II). Then, in the year 2009 the female workforce is newly determined and distributed across the businesses. In the year 2010, 10% of the female workforce are randomly redistributed to other workplaces (Bielenski et al. 2003). Taken together, this describes the employment landscape in which female employees may be influenced by the birth events of their female colleagues. For 2010, 40.7% of all women constitute the female workforce and they work in 3,279 firms, on average.

Calculation: Population-Level Impact of Social Interaction

Based on this main model, the population-level impact of social interaction effects on childless women emanating from their siblings, parents, and

6 In addition to Fischer et al. (2009), I used extracts from the IAB Establishment Panel that allowed to forge a more fine-grained distribution of firm sizes. As a result, I used information about the shares of firms within each category according to two more categories. Categories used were 1–4 and 5–9, 10–19 and 20–49, 50–99 and 100–250, as well as 250–499 and 500–5000+.

Figure 5.4. Exposure to Social Interaction Effects



Note: Averages from 150 simulation runs. Year is 2010.

colleagues, as shown in Figure 5.4, may be estimated. I pursue this in a counterfactual fashion. Technically, each year a random number θ_k from a uniform distribution ([0,1]) is drawn for each of the k childless females being their fertile age-span. If this random number is lower than the age- and parity-specific probability to have a child ϕ_j (where j denotes the respective one-year age-group), the formerly childless female becomes a mother, giving birth to a child (i.e., $x_k = 1$). The population extends by the number of children born.[7]

The narrative of the *counterfactual* approach is that the s social interaction effects (three; one for each interaction partner) already manifested in ϕ_j, the age- and parity-specific probability to have a child. ϕ_j is known from of-

7 Women with higher order parities also have children and, by this, contribute with their birth events to the social interaction effects on childless women. However, these women are not subject to the investigation of social interaction effects as such.

ficial statistics. Consequentially, the interaction partners' population-level impact may (retrospectively) be calculated out of ϕ_j.

Technically, the metric of the social interaction effects $\hat{\beta}_s$ depicted in Figure 5.1 discloses percentage changes in the estimated transition rate to parenthood. Consequentially, the objective in each model run is to estimate the transition rate $\hat{\delta}_{ks}$ for each female k and for each social interaction effect s added to which this percentage increase of social interaction effect s sums up to the population-level transition rate ϕ_j. Formally, the equation

$$\phi_j = \hat{\delta}_{ks} + \hat{\beta}_{ks} \times \hat{\delta}_{ks} \times x_k \tag{5.3}$$

has to be solved $k \times s$ times for the year 2010 in each of the 150 model runs. The unknown is $\hat{\delta}_{ks}$.

The reason that $\hat{\delta}_{ks}$ has to be estimated for each childless female k is that all social interaction effects s reflect variation in their strength among females as not every childless women is affected by social interaction in the same way (Bernardi 2003; Keim 2011). Therefore, the strength of each social interaction effect $\hat{\beta}_{ks}$ equals a random draw from a normal distribution with the respective mean and its standard deviation of the estimates depicted in Figure 5.1.[8] Taken together, formally, given a birth event (x_k = 1), for each combination of female k and social interaction effect s the simulation calculates

$$\hat{\delta}_{ks} = \frac{\phi_j}{1 + \hat{\beta}_{ks}}. \tag{5.4}$$

The respective birth event x_k, then, is assumed to (among other unspecified reasons) result from the social interaction effect s if $\hat{\delta}_{ks} < \theta_k \leq \phi_j$ holds. In this case, $\hat{\gamma}_{ks}$ = 1 indicates that the birth event presumably is due to the social interaction effect s. This analytical strategy allows me to estimate the population-level impact of social interaction effects emanating from siblings, parents, and colleagues, simply by subtracting birth events.

[8] More precisely, the strength of social interaction effects consitute exponentiated logistic regression coefficients from the three previous chapters. However, the calculation of the effect strength warrants further clarification. Take for example a situation in which one colleague had a child within one and another colleague had a child within two years before. Then, both parts of the time-dependent effect apply and the social interaction effect equals the exponentiated sum of both logistic regression coefficients (minus 1, of course), not the sum of both coefficients being exponentiated individually and added together afterwards. In the latter case, the effect strength would be overestimated for lower values and underestimated for larger values of $\hat{\beta}_{ks}$.

Adjustment: Simultaneous Effects

The above specification will overestimate social interaction effects systematically. The reason is that one childbirth will be attributed multiple times to different social interaction effects if they appear in conjunction (i.e., $\sum_s \hat{\gamma}_{ks} > x_k$ is possible). This ultimately overestimates both the single as well as the added overall impact of social interation effects. However, the rationale per se is not the problem. It is very likely that a woman may experience multiple social interaction effects at the same time (Keim et al. 2009). The question rather is to what extent each of those contribute to the overall effect. The consequence for the microsimulation is that the population-level estimates have to be corrected downwards.

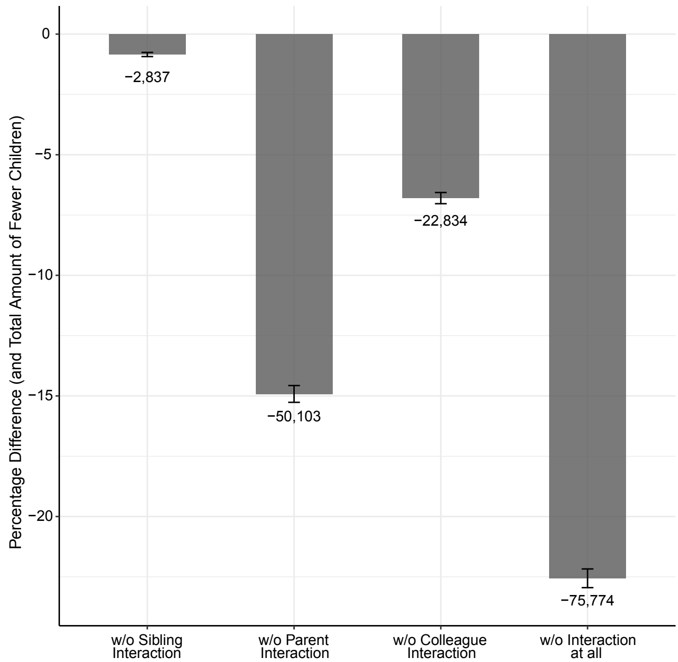
In each case, in which two or more social interaction effects were attributed to a birth event, they are modelled to share their impact in the way that the one with the strongest impact (i.e., lowest $\hat{\delta}_s$) marks the lower bound and the random number the upper bound. Within this range, another interaction effect may claim room, which is expressed in an individual-specific percentage. Thereby, I make full use of the randomness of the process and respect varying degrees to which women may respond to influences emanating from different interaction partners.

An example may clarify this. For a woman k for which two social interaction effects $\hat{\delta}_{k1}$ and $\hat{\delta}_{k2}$ are found (i.e., they both suffice the condition that each is smaller than θ_k), I calculate two shares for each, $\hat{\gamma}_{k1}$ and $\hat{\gamma}_{k2}$, indicating their relative contribution to the overall social interaction effect. Following this logic, $\hat{\gamma}_{k1} = (\theta_k - \hat{\delta}_{k1})/((\theta_k - \hat{\delta}_{k1}) + (\theta_k - \hat{\delta}_{k2}))$ provides exactly this share. As a result, this prevents one birth event to be attributed twice or even three times in the counterfactual calculation of the population-level impact. Rather, it is attributed, for example, with three quarters to parents and with one quarter to siblings (i.e., $\sum_s \hat{\gamma}_{ks} = x_k$ always holds). The equation

$$\hat{\gamma}_{ks} = \begin{cases} \frac{\theta_k - \hat{\delta}_{ks}}{\sum_{r \in \{s | \hat{\delta}_{ks} < \theta_k\}} (\theta_k - \hat{\delta}_{kr})}, & \hat{\delta}_{ks} < \theta_k \leq \phi_j \\ 0, & \hat{\delta}_{ks} > \theta_k \leq \phi_j \end{cases} \tag{5.5}$$

summarizes the aforementioned formally. Taken together, this leads to a superior estimation of social interaction effects because it avoids attributing the individual social interaction effects multiple times.

Figure 5.5. Estimated Number of Fewer First Born Children

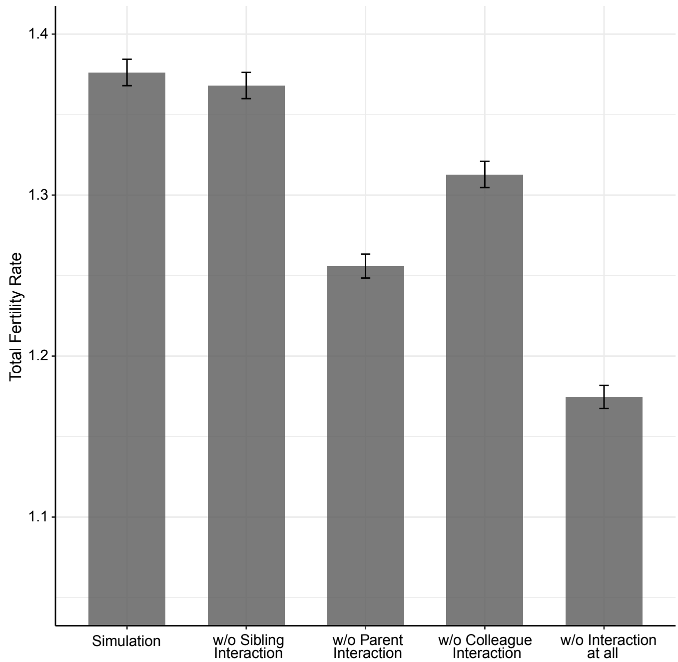


Note: Averages from 150 simulation runs. Numbers below bars indicate the estimated total decrease of children born in Germany in 2010. Baseline is the average of all simulation runs. In total, 329,952 first children were born in this year. Altogether, 662,685 children were born (Statistisches Bundesamt 2017).

5.3 RESULTS

How strong is the impact of social interaction effects at the population level—or more precisely, how many fewer children would have been born in 2010, given that all females would have made their decision to make their transition to parenthood without being influenced by the others' fertility-relevant behavior? Figure 5.5 provides an answer. Taking the overall number of first children born in the simulation without counterfactually calculating out social interaction effects as the baseline, after subtracting the counterfactuals, the overall population-level impact of social interaction effects amounts to 22.5% fewer first children, or put differently, roughly one out of four first born children. The population-level impact of parents is the strongest, followed by colleagues, and lastly siblings. On average, the

Figure 5.6. Estimated Decrease of the TFR



Note: Averages from 150 simulation runs. The TFR of 2010 was 1.365 children per woman.

impact of colleagues amounts to a 6.8% reduction in children born, those of parents to 14.9%, and those of siblings to 0.8%.

Putting this in perspective to the 329,952 first children born in 2010 in Germany (Statistisches Bundesamt 2017), the societal impact of social interaction effects becomes even clearer. No colleague interaction would have resulted in 22,834 fewer children, no parent interaction in 50,103 fewer children, and no sibling interaction would have resulted in 2,837 fewer children born, on average. Taken together, the societal impact of social interaction effects amounts to 75,774 fewer first born children.

Additionally, another well-known metric on which the societal impact may be substantively interpreted is the TFR. Compared to the absolute number of fewer first born children, the TFR take into account the distribution of the mothers' age as well as higher order parity birth events. The basis then constitutes the 662,685 children born in 2010 (Statistisches Bundesamt 2017). Figure 5.6 shows the results for the TFR. The hierarchy of effects does not change. On average, without social interaction effects the TFR would have been 14.6% lower and drop from 1.37 to 1.17.

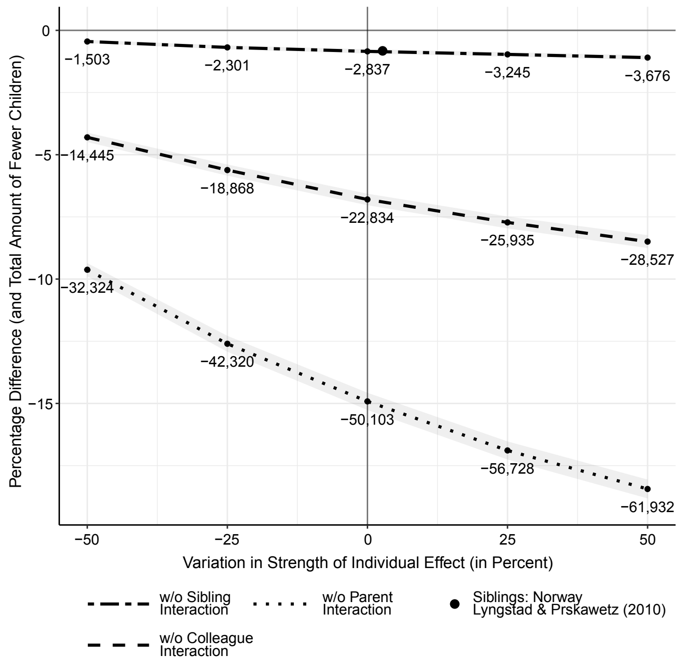
Sensitivity Analysis

The major parameter affecting the population-level estimates presented in Figure 5.5 is the input effect size estimates presented in Figure 5.1. This section sheds light on how sensitive the population-level estimates are with regard to changes in the input estimates. The strategy to investigate this sensitivity is to show to what extent the population-level estimates change given that the input estimates change in percentage steps, ranging from a 50% decrease to a 50% increase. This shows how far the population-level estimate would have gone, e.g., overestimated if the input estimate would have been overestimated, too. The results of this sensitivity analysis are shown in Figure 5.7. Additionally, I present a point estimate of interaction effects calculated from the (comparable) hazard model of the previous literature that used Norwegian data (Lyngstad & Prskawetz 2010).[9] The differences on the x-axis of Figure 5.7 are percentage differences in sums of exponentiated coefficients. While one input parameter changes, all others remained constant.

Figure 5.7 shows that even if the sibling effect would have been drastically over- or underestimated, the population-level impact of siblings with 944 fewer children remains negligible. Plugging in the estimates from Lyngstad & Prskawetz (2010) who analysed Norwegian register data were 2.7% larger and lead to only a marginal difference of 69 fewer children born. This does not hold for the other two interaction partners. Even the difference between a 25% under- and a 25% overestimation between colleagues amounts to a difference of 7,067 first children. This holds especially true for parents, whose sensitivity is the largest. Unfortunately, neither for parents nor for colleagues previous literature provided estimates, which I could have used for comparison.

[9] The point estimate is not on the lines in Figure 5.7 because it is composed of two parameters.

Figure 5.7. Sensitivity Analysis with Regard to Estimate Variation



Note: Averages from 150 simulation runs. Numbers below dots show the estimated total decrease of first children born in 2010 Micro-Germany.

5.4 DISCUSSION

Based on an artificial German female population in the year 2010, this chapter estimated the strength of social interaction effects from siblings, parents, and colleagues on the transition to motherhood at the population level. While the previous chapters provided empirical evidence for positive effects of interaction partners on women's timing to have their first child, the simulation took both their methodological approaches as well as their findings as the vantage point to translate their substantively hard-to-interpret findings into more readily understandable measures.

This chapter features two main results. First, the population-level strength of social interaction effects amounts to 75,000 fewer first born children. This figure is the population-level point estimate for the impact of social interaction effects based on three interaction partners. This decrease in children born equals a decrease of the TFR by around 15%. Second, from all three interaction partners, the population-level impact on fertility behavior is strongest for parents' influence. Colleagues' influence amounts to around two-thirds of the parents' impact and the social interaction effects emanating from siblings are almost negligible at the population level.

This microsimulation was designed following the advice given in any simulator's handbook, to keep it as simple as possible. Therefore, limitations and future research are discussed in conjunction in this section as limitations of the model are at the same time potential extensions of the model to explicitly assess these limitations.

First, the calibration model employed to generate the population did not consider in- or outmigration. Women living in Germany with a migrant background are thus not explicitly considered as another layer of precision. However, they contribute to the fertility-relevant information plugged into the models, for example the age- and parity-specific fertility rates. To respect migrants in this simulation adequately, migrant-specific fertility rates would have been needed.

Second, the figures for the strength of the social interaction effects, both the overall as well as those of the individual interaction partners, as shown in sensitivity analyses, vary considerably with regard to the hazard rate increase estimates. In other words, the accuracy of the population-level impact estimate depends upon the accuracy of these input parameters. All chapters employed the largest, most up-to-date datasets available, which of course, had their weaknesses as discussed in the discussion sections of the three previous chapters. Concerning this simulation, however, easily adjusted as soon as superior estimates become available.

Third, I restricted the interaction effects to be constant with regard to age. It is well conceivable that women may be both more susceptible and/or more subject to significant others' influence at different times of their fertile life-span. While there is neither theoretical nor empirical evidence for this in Germany, Mynarska (2010) showed for Poland, a very religious catholic country, that parental pressure sets in beyond the threshold of age 30 because mothers want their daughters to become mothers as this constitutes the religious norm and waiting beyond age 30 may jeopardize motherhood because of biological constraints. However, because of data restrictions it was not possible to retain age-specific social interaction effects in the first place. Therefore, they were not applicable in my simulation. At the same time, this also means that I was not able to estimate the impact of social interaction effects on the timing of first childbearing because the counterfactual approach was only to a small amount time-dependent through the timing variation in the exposure distributions.

Fourth, this research, as well as all the previous studies, concentrated on the transition to parenthood. This is justified for this analysis as it is the first of its kind and its aim was to estimate population-level figures for this transition. Nevertheless, it may be expected that for the transition to the second child the results will differ from those presented here. Following Lyngstad & Prskawetz (2010) who found negligible sibling effects for the second birth, we may expect an even smaller population-level impact. Following Thomése & Liefbroer (2013), there are good arguments to expect parental influence to be even larger at the transition to the second birth. Child care efforts increase drastically and own parents may well be taken into consideration because they reduce the costs drastically with two children. Whether the population-level effect will be larger or smaller, both in absolute and relative terms, remains an open question, which, however, may be easily assessed with an extension of the microsimulation presented.

Fifth, an explicit time-perspective was missing. The goal of this research was to provide one number for the size of the effects. Inspecting the dynamics of these fertility-relevant social interaction effects is a promising avenue for future research. This simulation study may provide a starting point for this because it may be extended to capture more years following 2010 to trace the process.

Discussion

ARE people's decisions about when they have their first babies influenced by the people around them? And if so, how strong is this influence? This dissertation sought to provide quantitative answers to both these questions, exploiting theoretical arguments and methodological approaches from both sociology and demography, concentrating on colleagues, siblings, and parents as interaction partners.

With regard to the first question, my main findings are as follows. For colleagues (Chapter II), I found a positive social interaction effect following an inverted u-shape pattern over time: In the year after a colleague gave birth, other colleagues more likely had their first pregnancy. This effect declined over time and vanished after two years. Social learning is the most likely mechanism to unfold this effect. At the workplace, fertile colleagues become influential as social models that change other colleagues' previous beliefs about the feasibility and consequences of having a child, inducing learning processes. This reduces uncertainties that surround the decision to have the first child. For siblings (Chapter III), I also found a positive social interaction effect, also following an inverted u-shape pattern: In the year after a sibling gave birth, her sister was more likely to have her first pregnancy. This effect peaked in the second year and vanished in the third year. The most likely mechanisms were emotional contagion and social learning. Contrary to colleagues, between siblings, emotional contagion is very likely as exposure to the newborn of a sibling is very likely. Furthermore, the effect from siblings was considerably stronger between more similar sisters. Similarity was thought to more likely activate both mechanisms. For parents (Chapter IV), I also found a positive social interaction effect, composed of two mechanisms, which could be separated empirically: Being able to anticipate future receipt of child care (social support) as well as experiencing social pressure both made the decision to have a first child more likely. These three findings contributed to quantitatively establishing social interaction effects on fertility decision-making, as evidence so far remained scarce (Balbo & Barban 2014). Furthermore, they shed more light on the underlying mechanisms, both by discussing the mechanisms for each specific interaction partner as well as providing empirical evidence for which of these mechanisms may unfold the social interaction effects from these different interaction partners.

Regarding the second question, "How strong is this influence?", employing these results in a microsimulation (Chapter V) provided the first estimate for the population-level impact of (fertility-relevant) social interaction effects: Without the influence of these three interaction partners, 75,774 fewer first children would have been born in the year 2010 (i.e., 22.5% of all first borns in this year). This number constitutes the population-level point estimate for social interaction effects based on these three interaction partners. This decrease in children born translates into a decrease of the TFR by 14.6%. Furthermore, ranking the three interaction partners, the population-level impact on first births is strongest for parents' influence (50,103 first children). Colleagues' influence is less than half of the parents' impact (22,834) and the social interaction effect emanating from siblings is almost negligible at the population-level (2,837), due to the small exposure to siblings' birth events.

Although the numbers above are certainly not exact, they provide a benchmark to get an impression of how strong social interaction effects on fertility *may* be—and around 75,000 fewer first born children certainly is impressive.

Over- or Underestimation?

How accurate is this number? We have to inspect how variable it is due to the way it was calculated. While the generation of the Micro-Germany of 2010, based upon which this number is calculated, is not a parameter on which I was able to vary too much, the most important parameter influencing the population-level estimate is the estimates from the three chapters (II, III, and IV), namely the percentage changes in transition rate increases that constitute my best estimates for social interaction effects of colleagues, siblings, and parents.

Methodologically, one complication is that the percentage changes in the discrete-time hazard rates constitute exponentiated coefficients of logistic regression models and, thus, are as such not comparable due to scaling effects (Karlson et al. 2012). In other words, one may not state that the effect of colleagues is larger than that of siblings, although in a arithmetic comparison it indeed is larger. That being said, another complication makes the percentage changes difficult to compare: they are based upon different fertility rates. The second and third chapter employed a monthly transition to first pregnancy leading to a live birth and the fourth a yearly transition to the compound event of the decision to become a mother as well as to the first pregnancy. It remains an open question to what extent an increase of 80% of the monthly transition rate of Chapters II and III is comparable to a 80% increase in the yearly transition rate of Chapter IV. It even remains an open question to what extent the increases in the two monthly transition rates may be compared. In my ambition to provide the first population-level measure of the impact of social interaction effects on first births, I had to assume their comparability. As a result, I attributed them straight-forwardly to the population-level fertility-rates in the microsimulation. To my knowledge, there is no statistical method that would allow re-calculating the percentage changes into comparable scales. As soon as a statistical methodology becomes available that allows for a correction in this regard, I can easily update my estimates. I hope this explicitness of the problem stimulates future research in this direction.

Are the estimates provided in Chapters II-IV over- or underestimated? And how does that translate into the population-level estimates? Concerning colleagues (Chapter II), I assume the effect to be overestimated. The reason is the lack of important fertility-relevant control variables in the process-generated data. Most importantly, the data did not provide information about a crucial precondition of having a first child, having a partner. In addition, I was not able to control for other important family background characteristics that I was able to control for in the other two chapters. Additionally, I was not able to adequately take the family-friendliness of the workplace into account, which was indicated to matter.

Concerning siblings (Chapter III), the estimation seems to be very accurate. Important control variables were available in the pairfam data. However, it may be the case that the sibling effect entails the parental effects to some extent, and vice versa. Not controlling for parents pressure or anticipated support may overestimate the sibling effect if both older siblings experience these social interaction effects from their parents to the same extent and if there are many families in which this configuration applies. Using data in which longitudinal information on both siblings and parents are available is the precondition to shed more light on the extent of potential bias. However, with regard to the almost negligible population-level impact of sibling effects this seems not to be the most urgent problem to address.

Concerning parents (Chapter IV), I assume the effect to be overestimated for both mechanisms, social support and social pressure. First, I was only able to approximate anticipated support, as, e.g., discussions or agreements between future grandparents and their adult daughters were not available. Approximating anticipated support using the travelling distance naturally overestimates the effect because living at a distance that enables flexible child care does not necessarily also mean that flexible child care is actually anticipated. In addition to this, I was not able to control for the availability of the mother, e.g., by her labor market attachment. This also speaks for an overestimation of the effect. Second, I only included social pressure in the simulation that would have accelerated the timing of having the first child. The reason was that I wanted to provide an upper bound for the strength of social interaction effects of parents rather than a mixture of effects that cancel each other out. Now, at least, we know that the effect most likely is not larger than 50,000 fewer first born children. Taken together, both mechanisms were likely overestimated. At least for the idea that parental effects may be moderated by sibling effects, Kotte & Ludwig (2011) did not find cross-sectional evidence for Germany with the pairfam data. Therefore, the additivity assumed in this dissertation seems to be justified in this regard.

Following these arguments, my population-level estimate should be interpreted as an upper bound, and the overestimation relates to the two interaction partners with the highest impact. Figure 5.7 (page 89) helps us to see how strong the decrease in impact may be. Assuming I would have drastically overestimated the parent effect by 50% and I would have not-so-drastically overestimated the colleague effect by 25%, we would have a new best guess of the strength of social interaction effects on first births in Germany of around 57,000 fewer first born children born[1], which amounts to nearly 17% of all first born children. Albeit that this would not be as huge

[1] To be exact, the impact estimate of 57,054 may not be directly read from Figure 5.7 because two input parameters are varied at the same time (the colleague as well as parent estimate). The figure varies only one parameter at a time, holding all others constant. As a result, the entire main model had to be estimated again. The corresponding estimate for colleagues amounts to 22,247 and the estimate for parents to 31,624 fewer first born children.

as the original best guess, it is nevertheless very strong. The truth most likely lies somewhere in between these two estimates.

A further complication, or a limiting factor, to specifying an overall estimate is that not all interaction partners that may influence the decision to have a first child (Keim 2011) were subject to quantitative analysis in this dissertation. Acquaintances in the widest sense may not be so important because they are not seen so often. The prime suspect for whom I would have liked to also estimate an effect are friends. Balbo & Barban (2014) showed for the US, based on school data, that there seems to be an effect of friends. For Germany, unfortunately, to the best of my knowledge, there is no dataset that would allow for a serious analysis of the influence of friends. Leave aside the methodological complications for taking the endogeneity into account because friendships can be assumed to be most strongly subject to fertility-relevant decision-making when making or maintaining friendships. However, cross-sectional evidence using the share of parents among the friends suggests a positive impact of friends for Germany (Kotte & Ludwig 2011). A further suspect would be neighbors (Keim 2011). They may also influence the fertility decision-making, albeit that the relationship boundaries with neighbors may blur with regard to friends and the decision on where to live may be endogenous to fertility decision-making. Nevertheless, neighbors live next door and may provide frequent exposure to young children, similar to siblings or friends. Thinking about these additional interaction partners, we may suspect the population-level impact of social interaction effects to increase. How large this increase may be, however, will be subject to future research.

The Broader Picture

On a broader level, the counterfactually estimated social interaction effects in the form of birth counts do not mean that these women will never make the transition to parenthood. It means that they will not make the transition in a specific year. How may we put this in perspective? Without social interaction effects we may expect a quite strong delay in the first fertility transition, indicating an elevated age at first childbearing. This highlights that fertility-relevant social interaction effects act as a counter-force to the general trend of delaying the transition to motherhood observed in nearly every country of the world (Balbo et al. 2013). Ad ultimo, this also has long-term consequences. A decreasing strength of social interaction would mean decreased completed (cohort) fertility because delaying the transition to parenthood reduces the time available to have further children because of biological restrictions in higher ages. In turn, a further long-long-term consequence is that these reduced cohort sizes lead, again, to fewer births. The point is, disclosed by the results of this dissertation, fertility-relevant social interaction effects seem to constitute powerful forces apparently slowing down the population decline in a long-term perspective. How strong their impact really is remains an open question, and may surely be subject to another dissertation, but from the numbers presented in Chapter V, social interaction may be suspected to be among the most important factors slowing down the population decline. To put it in a exaggerated nutshell, childless women not talking to other women about having children will lead to population decline.

Another important question is whose population-level impact is the largest among the social interaction partners under study. From the results in Chapter V we see that parental influence, e.g., in the form of support for child care, is of utmost importance. This is especially interesting for policy-makers because it reveals a relationship between two population-level measures that may not be obvious at first glance: Increasing women's retirement ages will lead to decreases in overall fertility levels. To this end, changes in closeness of family members as well as changes in (grand-)mothers obligations (e.g., labor market attachment or caring for their own frail parents) will directly translate into Germany's reproductive capacity. Therefore, this dissertation's results may warn policy makers that policies designed to increasing (grand-)mothers' burdens to provide child care will ultimately negatively affect the fertility of the younger generation, and thereby the size of future generations in the long run.

Whereas siblings' population-level impact was negligible, because of the low exposure to sisters' birth events, this did not hold for colleagues. Women have many colleagues, which leads to more exposure and thereby provides women with a multitude of available social models from which they may learn and reduce their uncertainties when making the decision to become a mother. Knowing this, we may expect considerable social multiplier effects of policies that are designed to reduce the costs of childbearing, for example by easing the combination of work and family life (Fent et al. 2013).

Importantly, I want to highlight that these multiplier-effects do not necessarily have to stay within the bounds of specific relationship types. Social interaction effects may the thought of as a complex endogenous process. Take for example a working woman who would have had her first child later but knows that her own mother will take care of her child (i.e., reducing her costs of childbearing through shared childrearing). This decision may ease her colleague's decision to have a child because she may learn from her, e.g., through observing her gathering information on how to take parental leave or the reactions on the colleague's pregnancy from other colleagues as well as the boss. Again, this colleague's sister may be influenced by her sister's decision through the direct contact to her sister's newborn, which again may affect the decision of the sister's colleagues, and so on and so forth. Sketching out these cascades, it becomes obvious that social interaction effects are composed of direct (i.e., peer-to-peer) as well as indirect (i.e., unknown-over-peer-to-unknown) effects. This highlights that, e.g., fertility-relevant policies introduced in a specific domain may transpire into other domains. Further evidence is needed that decomposes these direct and indirect effects and their relative contributions as well as evidence opening the black box of how these envisioned cascades of birth events may unfold over time (Lois & Arránz Becker 2014).

Lastly, in a very general methodological stance, the underlying notion of this dissertation may be seen as a contribution to an intensifying discussion that spans across a wide array of scientific disciplines. It centers around the difference between statistical and substantive difference (Wasserstein & Lazar 2016) and places great emphasis on changing scientists' behavior to translate their (statistically significant) findings into measures from which their weight or their importance may directly be interpreted (King et al. 2000). In this regard, Chapters II–IV face the same (substantive) limitation: I found significant (time-decreasing) social interaction effects, but their magnitude and thus their scope were barely accessible (from the discrete-time transition-rate models alone). Only with the simulation in Chapter V, estimating how many fewer children would have been born or the extent to which the TFR would have decreased, both researchers and policy-makers may grasp the scope of social interaction effects. In this regard, birth counts is a measure so simple that almost everybody should be able to relate to it—and therefore understand that fertility-relevant social interaction effects are impressively powerful.

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