

Welteke, Clara; Wrohlich, Katharina

**Article — Accepted Manuscript (Postprint)**

## Peer effects in parental leave decisions

Labour Economics

**Provided in Cooperation with:**

German Institute for Economic Research (DIW Berlin)

*Suggested Citation:* Welteke, Clara; Wrohlich, Katharina (2019) : Peer effects in parental leave decisions, Labour Economics, ISSN 0927-5371, Elsevier, Amsterdam, Vol. 57, pp. 146-163, <https://doi.org/10.1016/j.labeco.2019.02.008>

This Version is available at:

<https://hdl.handle.net/10419/233825>

**Standard-Nutzungsbedingungen:**

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

**Terms of use:**

*Documents in EconStor may be saved and copied for your personal and scholarly purposes.*

*You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.*

*If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.*



<https://creativecommons.org/licenses/by-nc-nd/4.0/>

# Peer Effects in Parental Leave Decisions

Clara Welteke\* and Katharina Wrohlich†

February 21, 2019

## Abstract

We analyze whether mothers' parental leave decisions depend on their coworkers' decisions. The identification of peer effects bears various challenges due to correlated characteristics within social groups. We therefore exploit quasi-random variation in the costs of parental leave induced by a policy reform in Germany. The reform encourages mothers to remain at home during the first year following childbirth. Administrative linked employer-employee data enable us to assign a peer group to individuals who work in the same establishment and occupation. Our results suggest that parental leave decisions are significantly influenced by coworkers' decisions.

**Keywords:** peer effects, social interaction, labor supply, family policy

**JEL:** C31, J22, D04

---

\*Corresponding author; DIW Berlin and IZA Research Affiliate, [cwelteke@diw.de](mailto:cwelteke@diw.de)

†DIW Berlin, [kwrohlich@diw.de](mailto:kwrohlich@diw.de)



# 1 Introduction

Parental leave policies and maternal reactions to it are a widely discussed topic among policymakers and researchers. Policies that enable prolonged parental leave spells, including parental leave benefits and job protection policies, may help protect young families and encourage fertility. However, mothers who take long leaves after giving birth might loosen their labor market attachment with the well-known consequences of decreased career prospects and life-time earnings.<sup>1</sup> Furthermore, employment interruptions due to motherhood can result in greater gender inequality.<sup>2</sup>

Parental leave behavior, and more generally labor supply of mothers with young children, differs greatly across time and regions.<sup>3</sup> Numerous studies explain part of these changes over time or the variation between countries based on standard economic models, attributing them to differences in financial incentives caused by institutional diversity. However, institutional differences cannot explain all divergence in the labor supply behavior of mothers across countries.<sup>4</sup> More recently, a growing strand of the literature on female labor supply and parental leave decisions suggests alternative or complemen-

---

<sup>1</sup>For the non-linear relationship between maternal leave duration and labor market outcomes, see Ruhm (1998). For the effect of motherhood on wages, see e.g. Lundborg et al. (2017).

<sup>2</sup>Increasing maternal labor supply over the life-cycle is a key factor in closing the gender wage gap (e.g. Polachek and Xiang, 2014).

<sup>3</sup>Classical references for the evolution of women's employment in the US include, among others, Goldin (1990) and Blau and Kahn (2006). For Germany, time trends in female employment patterns are documented e.g. by Fitzenberger and Wunderlich (2004). Cross-country differences explored e.g. by Bick and Fuchs-Schündeln (2017) and Blundell et al. (2013)

<sup>4</sup>For example, the paper by Bick and Fuchs-Schündeln (2017) shows that differences in male labor supply behavior between US and Western Europe can be largely explained by economic variables such as the tax system and the distribution of wages. However, the same model can only explain about 40 percent of the difference in female labor supply based on these economic variables. Similarly, a paper by Dearing et al. (2007) comparing two culturally very similar countries – Austria and West Germany – shows that differences in financial incentives only explain 20 percent of the total difference in the full time employment rate of mothers with children under age 10 in both countries. Moreover, several papers show that, although mothers in East and West Germany have shared the same institutional setting for more than 20 years, there are still persistent differences in labor supply behavior (see Rosenfeld et al., 2004; Grunow and Müller, 2012).

tary explanations for differences between countries or over time. For example, Fernandez (2013) attributes part of the increase in women’s labor supply in the US over the last century to changes in culture. While there is a tradition in sociology and anthropology of focusing on the importance of social structure, norms, and culture, most economists have long neglected social influences on individual behavior. However, an increasing number of economic studies are based on the assumption that individuals do not exist in isolation but are embedded within networks of relationships, such as families, coworkers, neighbors, friends, or socio-economic groups. For instance, several studies analyze the influence of social interaction on labor supply within geographic neighborhoods (e.g. Weinberg, Reagan, and Yankow, 2004; Fogli and Veldkamp, 2011) and family networks (e.g. Del Boca, Locatelli, and Pasqua, 2000; Dahl, Løken, and Mogstad, 2014; Neumark and Postlewaite, 1998; Nicoletti, Salvanes, and Tominey, 2016).

The paper contributes to the growing literature on the question how peer effects influence individual decisions. Within this literature, this is the first paper to analyze peer effects among coworkers in the context of maternal leave decisions. We find that peer effects influence individual decisions significantly, using an innovative quasi-experimental research design that exploits variation stemming from a German parental leave benefit reform. While many different social networks are important for individual decisions, we believe that workplace social networks play a particularly important role as far as labor supply related decisions are concerned, because the workplace facilitates the formation of social ties and, thereby, the transmission of behavioral norms and career-related information. Previous literature on peer effects has shown that coworkers affect individual decisions in various ways. For example, Mas and Moretti (2009) and Hesselius et al.

(2009) show that peer effects play a role in the context of productivity of cashiers and worker absenteeism respectively. Cornelissen et al. (2017) find an effect of the long-term quality of a workers' peers on wages. Other papers have shown that peers at work can affect fertility (Pink et al., 2014; Asphjell et al., 2013), parental leave decisions (Dahl et al., 2014), and retirement plan decisions (Duflo and Saez, 2003).

In the following, we refer to peer effects as the effects of a social reference group's behavior on individual outcomes. There are several channels through which the change in parental leave behavior of a social reference group can influence individual decisions. These include preferences for conformity to social norms, learning about the costs and benefits of parental leave, as well as leisure complementarities.

We argue that preferences for conformity and the transmission of information about the costs and benefits of a (long) parental leave are both possible mechanism of peer effects in our context. Information transmission is expected to be particularly important in situations with high career-related uncertainty. If mothers are uncertain about the consequences of a long leave, they are more susceptible to observed peer decisions. Observing peer mothers at the workplace, more specifically the employers' reaction to peer mothers' parental leave choices, may change beliefs of uncertain mothers and thereby affect their own leave decisions. Other channels that can give rise to peer effects include the transmission of practical knowledge about the existence and organizational details of the parental leave program, as well as leisure complementarities or work externalities. Leisure complementarities would imply that mothers benefit from taking leave simultaneously, whereas work externalities occur if the absence of one mother makes it more difficult for her coworkers to take leave. While the transmission of practical knowledge can be ruled

out as a mechanism that drives our results because the parental leave benefit program is universal and well-known, leisure complementarities and work externalities are unlikely to be relevant because of the temporal distance of the parental leave of peer mothers and their coworkers.

In addition to presenting empirical estimates of the magnitude of peer effects in the context of parental leave decisions of mothers, we believe that our study has some policy relevant implications. When social interaction effects are quantitatively important, policy interventions on single agents might have large effects through so-called social multipliers (see Glaeser, Sacerdote, and Scheinkman, 2003). Although empirical studies frequently attempt to infer individual behavior from observed aggregate outcomes, when there is social interaction, aggregate coefficients will be larger than individual coefficients because there is a direct effect of policy changes on individual behavior and an indirect effect through the effects on the social reference group.

The identification of peer effects is challenging due to correlated characteristics within social groups and endogenous group membership (see Manski, 1993; Blume, Brock, Durlauf, and Ioannides, 2010, for an overview). Several studies (e.g. Dahl et al., 2014; Brown, 2013) suggest using policy reforms as instruments to address the identification challenges. We follow this suggestion and identify social interaction effects in the labor supply of mothers with young children using the exogenous variation introduced by the 2007 reform of the parental leave benefit (*Eltern geld*) in Germany, which, in particular, encourages high-income mothers to remain at home during the first 12 months following childbirth.<sup>5</sup>

---

<sup>5</sup>See Bergemann and Riphahn (2015), Geyer et al. (2015), Kluge and Schmitz (2018), and Kluge and Tamm (2013) for an analysis of the effects of the 2007 parental leave benefit reform on maternal employment.

We use administrative linked employer-employee data from the Institute for Employment Research (IAB), which enables us to assign a peer group to all individuals who work in the same establishment and occupational group. The identifying variation stems from the exposure of our sample to peer mothers who gave birth within a narrow window either before or after the parental leave benefit reform.

Our results suggest that maternal decisions regarding the length of their own parental leave are significantly influenced by their coworkers' decisions. We find that a mother is about 30 percentage points more likely to stay at home for the first year if her peer(s) decide(s) to do so in response to the parental leave benefit reform. This effect corresponds to the Local Average Treatment Effect (LATE). We also estimate the Intention to Treat Effect (ITT), showing that having peers who gave birth after the introduction of the new parental leave benefit increases the probability that a mother takes a leave of at least one year by 7 percentage points in contrast to mothers who have peers who gave birth shortly before this date. The results of analyses for those subgroups for whom uncertainty regarding the employer's reaction to parental leave decisions is expected to be higher, suggest that information transmission and the reduction of uncertainty that comes with observing peer behavior may be among the critical channels driving peer effects in our context.

The paper is organized as follows. In the next section we describe some stylized facts on maternal employment in Germany and discuss the institutional details of the 2007 parental leave reform that we use as an instrument in the empirical analysis. Section 3 sketches our methodological approach and explains our identification strategy. In Section 4, we describe our data set and present some selected descriptive statistics. The

baseline results of our empirical analysis and several robustness checks are presented in Section 5. This is followed by a discussion of the possible peer effect mechanisms based on heterogeneous effects for different subgroups in Section 6. Section 7 concludes.

## 2 Institutional Setting and Stylized Facts

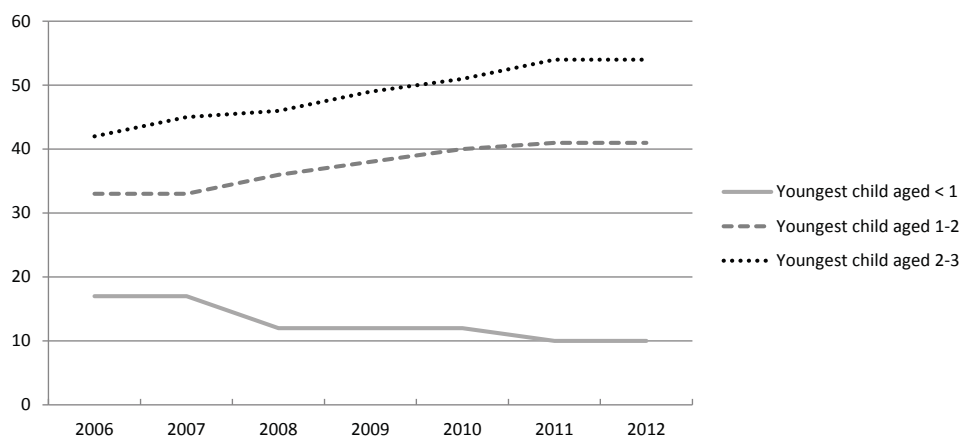
Maternal employment is of increasing policy relevance in many OECD countries, because low fertility rates and an increasing old-age dependency ratio underlie a growing imbalance in public finances. Traditionally, employment rates of mothers are relatively low in West Germany, compared to e.g. France, the UK or the Nordic countries. However, since 2006, the employment rate of mothers with children between 1-2 and 2-3 has been increasing, as seen in Figure 1. Over the same period, the employment rates of mothers with children below the age of one decreased. This can partly be explained by the parental leave benefit reform of 2007, which is central to our empirical analysis.

Before implementation of the parental leave benefit reform of 2007, families with a new born child were paid a cash benefit amounting to 300 Euro per month for a maximum period of 24 months (chosen by most parents), or 450 Euro per month for a period of 12 months, if at least one parent did not work more than 30 hours per week. This benefit, *Erziehungsgeld*, was means tested at the household level. Less than 50 percent of the families with new born children were eligible due to the income test.

Starting in 2007, the new parental leave benefit, *Elterngeld*, replaced the *Erziehungsgeld*. Paid for a shorter period of time – 12 months if only one parent takes leave or 14



Figure 1: Maternal employment rates by age of youngest child



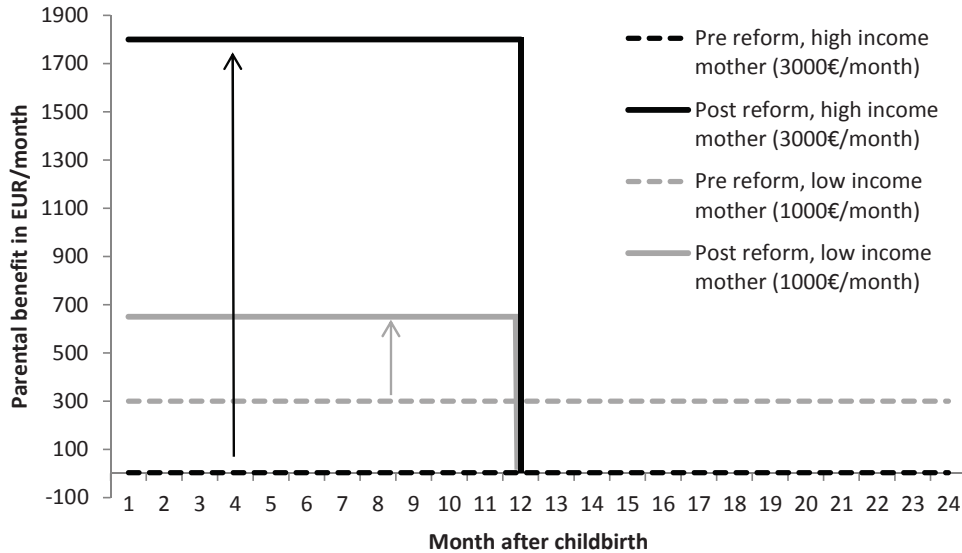
Source: Federal Ministry of Family Affairs, Senior Citizens, Women and Youth, 2014.

months if both parents take leave – *Elterngeld* is not means-tested on household income. The benefit awarded to parents depends on their earnings prior to birth, replacing 67% of previous net earnings, not to exceed 1,800 euro per month. The minimum amount of *Elterngeld* awarded is 300 Euro per month, which is equivalent to the monthly benefit paid under the previous *Erziehungsgeld*.

Thus, the financial incentives induced by this reform differ between high- and low-income mothers as well as between the first and second year after giving birth. Figure 2 shows the amount of parental leave benefits paid to mothers with a monthly gross labor income of 3000 Euro (high income) and 1000 Euro (low income) respectively, before and after the introduction of the reform. For low-income mothers, financial incentives did not change as much during the 12 months after giving birth, however there clearly is the incentive to shorten their leave after their child turns one. Only mothers with a very low income, which entitles them to less than 450 Euro *Elterngeld*, are incentivized by the re-

form to return to work in the first year. For medium- and high-income mothers, however, the reform provides incentives to stay at home during the first year after childbirth.

Figure 2: Benefits paid before and after the reform for exemplary mothers



Source: Hypothetical benefits that can be received by married mothers with a net income of 3000 Euro and 1000 Euro per month respectively, based on the online benefit calculator of the Federal Ministry of Family Affairs, Senior Citizens, Women and Youth. <https://www.familien-wegweiser.de/Elterngeldrechner/index.xhtml>, accessed July 20, 2014.

By setting strong incentives to stay at home for (exactly) 12 months, the introduction of the *Elterngeld* set an institutional norm that children should be cared for by their parents at home until their first birthday. Norms regarding parental leave still differ between East and West Germany. We explore how these differences affect peer effects in Section 6. Before German reunification in 1990, East German mothers were much less likely to be out of the labor force or work part-time than West German mothers (see Rosenfeld et al., 2004). In East Germany, parental leave benefits were paid for one year, called the *Geburtsjahr*, and public childcare was generally available. In West Germany, maternal labor supply was comparatively low and, by family policy, mothers

were encouraged to stay at home or work part time.

Since 2005, several other policy reforms have affected maternal employment. In particular, a number of child care reforms have been carried out that have successively increased the availability of subsidized child care for children below three years. As of August 2013 every child has a legal claim to a slot in a publicly subsidized childcare institution after the first birthday.<sup>6</sup>

## 2.1 Employment effects of the parental leave benefit reform

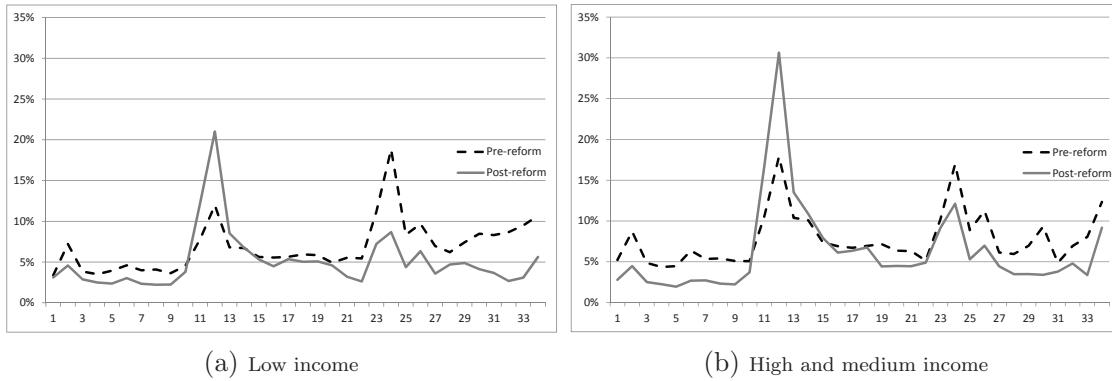
A graph of the hazard rates of mothers whose children were born before and after the reform, displayed in Figure 3, reveals that there are peaks after the mandatory maternity leave period of 2 months and then again at both 12 and 24 months. It can also be seen that with the introduction of the *Elterngeld*, mothers became much more likely to return to work after around 12 months and less likely to return during the first 10 months. This is especially true for medium- and high-income mothers (see Figure 3b).

Based on different methods and data-sets, several studies analyze the causal relationship between the parental leave benefit reform of 2007 and the development of maternal employment over time. As expected, Kluve and Tamm (2013), Kluve and Schmitz (2018) and Geyer, Haan, and Wrohlich (2015) find that the probability that mothers return to work during the 12 months following childbirth has declined, in particular for high-income mothers, as a result of *Elterngeld* being introduced. Furthermore, Geyer et al. (2015), Bergemann and Riphahn (2015), and Kluve and Schmitz (2018) find that the employment

---

<sup>6</sup>In the same period, also the supply of afternoon care for school-children has been increased dramatically by the large expansion of all-day schools (see Beblo et al., 2005; Marcus et al., 2013).

Figure 3: Hazard rates of returning to work, by length of parental leave spell in full months, before and after the parental leave benefit reform



*Notes:* The figures are based on all mothers who gave birth between 2000 and 2007, who returned to work within 36 months. The income threshold corresponds to a gross labor income of about €1800 per month.  
*Source:* IAB Linked Employer-Employee Panel (LIAB) LM 1993-2010.

of mothers who gave birth after the reform was implemented generally increases after the first 12 months compared to employment of mothers who gave birth before the reform.

Kluge and Schmitz (2018) analyze not only the effect of the *Elterngeld* introduction on the labor supply of mothers during the first and second year following childbirth but also on the third to fifth year after childbirth. Based on data from the German Microcensus, they find a large and significant increase in the employment rate of mothers with three to five year old children. However, the authors can only speculate about the mechanism that explains this causal (in a statistical sense) effect of the *Elterngeld*. As a possible explanation, the authors suggest that the new parental leave benefit changed social norms in the sense that it created a new social anchor point in time at which mothers go back to work. Bergemann and Riphahn (2015) also analyze the short- and medium-term maternal employment effects of the 2007 parental leave reform. In line with Kluge and Schmitz, they find that employment of young mothers increases and the average duration of the employment interruptions declines. The authors argue that a change in social norms

might partly explain the strong employment effect of the reform; in particular they show that mothers who may be more likely to be restricted by social norms, such as mothers living in the countryside, living in West Germany, or those with an external locus of control show a stronger response to the reform.

To sum up, the empirical findings concerning the 2007 parental leave reform in Germany find that the reform induced mothers to stay at home for the first year after childbirth, but increased the likelihood of maternal employment thereafter. Moreover, empirical findings show that the employment of mothers has changed more than can be explained by financial incentives alone. Some authors speculate that the reform changed social norms concerning the labor supply of mothers with young children. However, a formal test of this hypothesis is, so far, missing. In the next section, we outline how we use the introduction of *Elterngeld* as an instrument for the identification of peer effects with respect to the labor supply decisions of mothers with young children.

## 3 Methodological Approach

### 3.1 Identifying Peer Effects

The identification of social interaction is challenging because there are several explanations as to why members of a social group behave similarly or exhibit common characteristics. Manski (1993) distinguishes three types of effects that can explain why researchers observe similar outcomes of individuals belonging to the same group. The first is the endogenous effect or peer effect, which we aim to identify. Endogenous effects measure the influence of the decisions of the relevant peer group on individual decisions. The

second explanation is concerned with contextual effects, meaning that the individual is influenced by exogenous group characteristics. The third explanation originates from correlated effects, which means that individuals belonging to the same group tend to behave similarly because they share unobserved characteristics, either due to endogenous group formation or common shocks. Another challenge associated with the identification of social interaction effects stems from the simultaneity of interactions within a social group. Thus, it is not possible to determine whether an action is the cause of, or the result of, peer influence.

Several papers focus on the theoretical identification of interaction effects in social groups and networks. Blume et al. (2010), Brock and Durlauf (2001), Bramoullé, Djebbari, and Fortin (2009), and Blume, Brock, Durlauf, and Jayaraman (2015) address the conditions under which economic models with social interactions are identified. In most cases, the natural exclusion restriction induced by the structure of a social network enables the identification of the model. However, data with a known network structure are rarely available.

Thus, several studies assume that social interaction with respect to labor supply takes place within observed groups, including geographic neighborhoods (e.g. Weinberg et al., 2004; Maurin and Moschion, 2009) and family networks (e.g. Del Boca et al., 2000; Dahl et al., 2014; Neumark and Postlewaite, 1998; Nicoletti et al., 2016). We focus on the workplace as the relevant social network. This is based on the assumption that workplace peers matter for decisions regarding employment behavior. There are several studies suggesting that peer effects at the workplace play an important role. Hesselius et al. (2009) shows that peer-effects exist in the context of absenteeism; Mas and Moretti

(2009) focus on workplace peer effects in the context of productivity of cashiers for a large grocery chain, while Cornelissen et al. (2017) use linked employer-employee data to estimate wage peer effects.

Given the identification challenges, the use of quasi-experimental approaches is an increasingly popular way to identify peer effects. For example, Brown (2013) is able to identify peer effects among teachers of the same schools using random variation in the age composition between schools in Los Angeles, CA. We use a research design similar to Dahl et al. (2014), who estimate peer effects among brothers and coworkers in the context of paternity leave take-up in Norway. The identification challenges are avoided by exploiting exogenous variation in the costs of paternity leave induced by a family policy reform. They find that coworkers and brothers are more likely to take paternity leave if their peer was induced to take up leave by the reform.

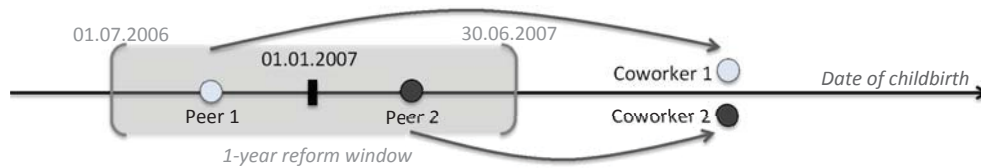
## 3.2 Empirical Strategy

To overcome the identification challenges mentioned above, we employ an instrumental variable research design that exploits the quasi-random variation in maternal leave spells due to the introduction of the new *Elterngeld* in January 2007. The parental leave benefit reform encourages, in particular, high-income mothers to stay at home during the first 12 months following childbirth and to benefit from the high income replacement rates under the new *Elterngeld*. This creates a change in the fractions of working mothers in the first year after childbirth. In this analysis, we focus on the reform-effect on the behavior of mothers during the first year following childbirth.

Our preferred sample consists of mothers who gave birth on or after July 1, 2007, but

on or before December 31, 2009. Those mothers, referred to as *coworkers*, must have at least one *peer* who gave birth sometime between July 1, 2006, and June 30, 2007. The sample is then divided into two groups, the control group consisting of those coworkers whose peers gave birth prior to the reform’s implementation and the treatment group consisting of those whose peers gave birth after its implementation. This is illustrated in Figure 4 for a simplified setting with only two groups and one birth per group within the reform-window. *Peer 1* in group 1 gives birth before the reform cutoff-date, and *Peer 2* in group 2 gives birth after the cutoff. Consequently, *Coworker 1* and *Coworker 2*, who both give birth after the reform was introduced, vary only in their exposure to peers who gave birth at different points in time.

Figure 4: Sampling and identification



One challenge is that an individual may be affected by several peers. Thus, it is necessary to not just specify a window around the cutoff date, but also to the treatment assignment variable in cases where the coworker-mother had more than one peer giving birth in the window around the cutoff. For large peer groups, our research design has little power because with an increasing time-window around the reform date, the variation in exposure decreases. Our preferred specification sidesteps these issues by considering peer groups where there are one or more peer mothers in the reform-window, but only if they gave birth on the same side of the reform. We present estimation results from several



alternative sample specifications in Section 5.2.

The outcome of interest is a binary variable that equals one if the mother does not return to work within the first 10 months following childbirth.<sup>7</sup> Our model explains individual outcomes ( $y_{Cg}$ ) by average maternal leave decisions ( $\bar{y}_{Pg}$ ) of all peer mothers in group  $g$ . We instrument peer decisions by a reform dummy ( $z_g$ ) in the first stage regression.

$$\text{First stage: } \bar{y}_{Pg} = \alpha_0 + W'_g \alpha_1 + \lambda z_g + \epsilon_{Pg} \quad (1)$$

If the policy reform  $z_g$  is exogenous, the reform effect on peer leave behavior  $\lambda$  can be identified using a linear regression.<sup>8</sup> To balance observable differences between treated and non-treated mothers, we include individual, peer and firm characteristics  $W_g$  in both equations. The results from the first stage are then used to estimate the peer effect  $\hat{\delta}$  in the second stage (Equation 2). In the second stage regression, we estimate the probability that a coworker mother stays at home for at least 10 months following childbirth ( $y_{Cig}$ ), including the first stage fitted values  $\hat{y}_{Pg}$ .

$$\text{Second stage: } y_{Cig} = \beta_0 + W'_g \beta_1 + \delta \hat{y}_{Pg} + \epsilon_{Cig} \quad (2)$$

The treatment effect,  $\delta$ , is the local average treatment effect (LATE) of the reform induced employment interruption decision of the peer mothers on their coworker's maternal leave decisions. The LATE measures the effect on the subpopulation of *compliers*. In this

---

<sup>7</sup>We use 10 months instead of one year because the length of the leave spell is not observed exactly. All maternal leave spells that do not end within 10 months are coded as leave spells longer than 10 months, including censored observations.

<sup>8</sup>As a specification test, we include a function of the date of birth of the peer mothers' child, equal to  $f(x_{Pg}) = x_{Pg}$ , in the first stage regression to account for a (linear) time-trend in outcomes. However, while including peer mother date of childbirth does not change the point estimates, it does decrease efficiency. Therefore, we do not include  $f(x_{Pg})$  in our baseline specification.

context, *compliers* are those mothers who have peers who stayed at home throughout the first 10 months after childbirth in response to the parental leave benefit reform, and would not have done so in absence of the reform.

In the case of a single binary instrument, the 2SLS estimator is equivalent to a Wald estimator, which is equal to the reduced form estimate of the effect of the policy change on coworkers leave spells divided by the jump in peer outcomes due to the policy change. The reduced form effect can be interpreted as the intention to treat effect (ITT) of having a peer mother who gave birth after the reform on the coworkers' probability to stay at home for at least 10 months following childbirth.

Our empirical strategy yields consistent estimates of the LATE if several identifying assumptions are met. The first assumption is *independence* of the instrumental variable and potential outcomes. Peer mothers have to be as good as randomly assigned to the treatment group, receiving the new parental leave benefit, and the comparison group exposed to the previous regulation within a window around the reform.<sup>9</sup> For this to be true, we need to assume that individuals are not able to control the treatment assignment variable. Assuming that the timing of birth can only be influenced within a small time window, we can avoid cases where the date of birth is purposefully delayed by dropping observations very close to the first of January 2007. While mothers may have influenced the date of childbirth around the cutoff, it is very unlikely that mothers pre- or postponed childbirth for more than two weeks (see Tamm, 2013). To be certain, we drop all birth events that occur two weeks before and after the cutoff-date from our sample. Mothers

---

<sup>9</sup>The 2007 parental leave benefit reform is used as a natural experiment in several evaluations of the policy (Kluve and Tamm, 2013; Kluve and Schmitz, 2018; Bergemann and Riphahn, 2011; Wrohlich et al., 2012; Bergemann and Riphahn, 2015).

who give birth before and after the reform may still differ due to selective fertility in anticipation of the reform. For example, high-income mothers might have delayed pregnancy or even decided to have a child due to the new generous parental leave benefits. We argue that selective fertility is unlikely because there was no definitive knowledge before September 2006 that the policy would be implemented.<sup>10</sup>

Another identifying assumption is the *exclusion restriction*, which requires that the instrument operates through a single known channel, i.e. coworker outcomes are not affected by the parental leave benefit reform through channels other than peer behavior. All coworker outcomes are observed for mothers who give birth after the reform was implemented. Therefore, it can be assumed that whether a peer mother gave birth before or after the reform had no effect on coworkers' behavior other than through peer behavior. The *exclusion restriction* could nevertheless be violated if the maternal leave behavior of peer mothers induces others to change their fertility or employer, which in turn affects group composition and can thereby have an indirect effect on coworkers' maternal leave decisions. To test whether there are differential changes in group composition in the treatment and control group, we compare group sizes, exits, entries, and fertility after the reform-window and discuss the results in Section 5.2.

Another necessary assumption for the validity of our research design is that the reform effect is *monotone*, i.e. that no mother is more likely to get back to work within 10 months after giving birth as a response to the reform. This is granted because the parental leave

---

<sup>10</sup>The legislative process that led to the reform proceeded rapidly. The government coalition agreed on the reform only in May 2006, and the law passed parliament in September 2006. Consequently, most children born during the six months before and after January 1, 2007, were conceived before their parents knew that the reform would be in place by the time of birth (Kluve and Tamm, 2013). Even if the reform encouraged some women to get pregnant after September 2006, their babies were unlikely to be born before July 1, 2007. In line with this argument, Raute (2019) finds evidence for a positive fertility effect of the 2007 parental leave benefit reform starting nine months after the law was passed.

benefit reform did not reduce benefits for any mother in our sample, which excludes low-income mothers, during the first year after childbirth; for most mothers, benefits increased. This increase in benefits was particularly high for our sample of medium- and high-income mothers.

Our empirical strategy is able to circumvent the standard identification issues associated with social interaction effects. The problem of simultaneity is solved by the time dimension, which excludes the possibility of peer decisions being influenced by their coworkers who gave birth afterwards, assuming that mothers do not coordinate their leave beforehand. Bias due to correlated effects and endogenous group formation can be avoided because the parental leave reform is orthogonal to unobserved characteristics and therefore treated and non-treated mothers differ only in their exposure to peers who gave birth before and after the parental leave reform respectively. Consequently, we argue that the estimated effect can be attributed solely to the influence of peer mothers' behavior.

## 4 Data

The empirical analysis is based the longitudinal model 1993-2010 (LIAB LM 9310) of the Linked Employer-Employee Data of the Institute for Employment Research (IAB). The data were accessed via a guest stay at the Research Data Center (FDZ) of the German Federal Employment Agency (BA) at the IAB and subsequently by means of controlled remote data processing at the FDZ.<sup>11</sup> The IAB Establishment Panel is drawn from all German establishments with at least one employee subject to social insurance contribu-

---

<sup>11</sup>See [Dataset] Klosterhuber et al. (2013) for more information on the LIAB longitudinal model 1993-2010 (LIAB LM 9310) data documentation and methodology.

tions. The sample is stratified by sector, firm size, and federal state. The Establishment Panel is then linked to employment histories of all employees who were working in a panel firm between 1999 and 2009. As a result of the sampling methodology, large establishments, small federal states, small industries and the manufacturing industry in East Germany are overrepresented in the IAB establishment panel. Furthermore, civil servants and self-employed workers are not included in the data.

An advantage of the LIAB is that it includes individual employment histories as well as detailed firm characteristics such as developments in employment (production, turnover, working hours, investment, capacity utilization), and demand for personnel and labor expectations (vacancies, open positions, fluctuations, establishment employment policies). In addition, the LIAB includes information on firms' technology, organization structure, determinants of productivity, firm size and industry. We merged the LIAB with regional information on childcare coverage, population density, and unemployment rates from INKAR.<sup>12</sup>

When estimating peer effects, the challenge is to find a data set that contains micro data on an individual's social network. The researcher has to know (or assume to know) the relevant reference group. One of the main advantages of the LIAB is that it includes a large number of firms and individuals (in 2007, we observe 5,364 firms), and that the full network of employees working at the same firm can be identified. Therefore, this data set has already been used by several studies to analyze peer effects in different contexts. For example, Cornelissen et al. (2017) use the LIAB to analyze peer effects on wages, and Pink et al. (2014) study workplace peer effects on fertility.

---

<sup>12</sup>[Dataset] Bundesinstitut für Bau-, Stadt- und Raumforschung (BBSR) im Bundesamt für Bauwesen und Raumordnung (BBR), Bonn (2015).

Parental leave spells and events of childbirth are not directly observed in the LIAB. However, Schönberg (2009) shows that it is possible to identify maternal leave spells with sufficient accuracy by using employment interruptions.<sup>13</sup> Employment interruptions of at least 14 weeks are likely to be maternity leave spells due to the obligatory maternity leave period of 6 weeks before and 8 weeks after childbirth. Following Müller and Strauch (2017), we identify parental leave spells using information on employment interruptions due to entitlement to *other compensation by the statutory health insurance provider*, which is filed for two reasons: maternity leave or sickness allowance. The likelihood that an employment interruption is due to childbirth is increased by restricting the sample to women between the age of 18 and 40. We approximate the child’s date of birth as six weeks after the mother went on leave, which leads to some measurement error in the child’s month of birth.<sup>14</sup>

The sample is restricted to women who gave birth between July 1, 2007 and December 31, 2009, conditional on these mothers having a peer who gave birth between July 1, 2006 and June 30, 2007. The peer mother(s) belong to the same peer group as determined by occupation and firm identifier at the time of their respective date of childbirth. For small firms with 99 or fewer employees, peer groups are formed only by the firm identifier. Occupations are defined using two-digit occupational groups according to the German classification of occupations KldB 1988 (Bundesagentur für Arbeit, Nürnberg, 1988), which summarizes related occupations into 86 larger groups. Firm identifiers differ by establishment.

---

<sup>13</sup>Schönberg (2009) shows that after some appropriate restrictions are imposed, at least 90 percent of employment interruptions in the data are due to maternity leave in West Germany.

<sup>14</sup>For a detailed description of the identification of mothers in IAB administrative data, consult Müller and Strauch (2017).

The sample selection proceeds as follows. First, employed mothers who give birth in a one-year window around the parental leave benefit reform (January 1, 2007) are marked as potential *peer* mothers. We observe 4,375 births in the reform-window. In the next step, we group all women who work in the same firm and occupation and then count the number of births before and after January 01, 2007 within the reform-window. In our baseline specification, we keep only those peer groups where there are either births before or after the reform, dropping all groups where there are births on neither or both sides of the cutoff-date.<sup>15</sup> We lose another 2,845 birth events because there is at least one birth before and one after the reform within the specified window. The number of potential peer mothers is further reduced to 1,413 by dropping all groups where a birth event falls within the 28-day window around the reform date because of potential measurement error in birth dates and concerns about manipulation of the date of childbirth. Thereafter, we identify all *coworkers* of potential *peer* mothers, who work in the same firm and occupation, and mark those who give birth after their peers and after the reform-window. We drop all observations of birth events that occur after December 31, 2009 because our observation periods ends on December 31, 2010 and we need to observe the first year following childbirth. Furthermore, we include only the first observed birth if the same coworker gives birth more than once.

The outcome variable of interest is a binary indicator which equals one if the (coworker) mother does not return to work within 10 months after childbirth.<sup>16</sup> In the next step, we generate the treatment assignment variable based on peer mothers' date of childbirth. The treatment variable equals one if all peer mothers gave birth after the reform cutoff

---

<sup>15</sup>Most groups (about 83 percent) are lost because no woman gives birth within the reform-window.

<sup>16</sup>All maternal leave spells that do not end within 10 months are coded as leave spells longer than 10 months, including censored observations, i.e. we do not condition on observed return to work.

date. Peer outcomes and characteristics are defined by taking the mean if there is more than one peer mother.

Because the parental leave benefit reform had heterogeneous effects on different income groups, we divide the sample of *coworkers* by *peer* income.<sup>17</sup> We drop peer groups with a low average income to exclude the possibility that peer mothers' employment decreases in the first year as a response to the reform. After limiting our sample to peer mothers in the upper two-thirds of the income distribution (monthly income<sup>18</sup>  $\geq 1,814$  Euro), and their coworkers, we are left with a sample of 488 peer mothers and 1,340 coworkers.<sup>19</sup>

From the 12,070 birth events observed between July 2007 and December 2009, we are left with a sample of 1,340 coworkers, which amounts to 11 percent of all observed births over that time period (see Appendix Table A1 for an overview of the sample sizes of the baseline and several alternative specifications).

Due to the sample selection by income, women in our sample have a higher average income, are more educated, have more work experience and are less likely to be unemployed or part-time employed than the average women giving birth in the same period. They work in larger firms due to the requirement of at least one peer birth during the sample period (see Appendix Table A2).<sup>20</sup>

Note that we do not distinguish between part-time and full-time employment in our paper. All employment spells with positive income are counted equally as employment,

---

<sup>17</sup>Note that the sample is divided by peer, not coworker, income because peer mothers are heterogeneously affected by the parental leave benefit reform.

<sup>18</sup>Monthly gross labor income is computed as an average over the last 12 months before beginning of the maternity leave period.

<sup>19</sup>It is not possible to use the exact income threshold because parental leave benefit eligibility was means-tested on the household level before the reform and we only have individual earnings.

<sup>20</sup>Descriptive statistics of all sample specifications can be found in Table A3 in the Appendix.



including marginal employment (*mini-jobs*). This helps to explain relatively high employment rates of mothers in our sample. About 20 percent of mothers (out of all mothers in the sample for whom return to employment is observed) return to part-time employment after the parental leave spell. About 3 percent return to marginal employment (see Appendix Table A2). We don't find any differences in the probability to return to part-time employment by leave duration in our sample.

## 5 Results

### 5.1 Baseline results

The first stage regression estimates the reform effect on the probability that a *peer* mother stays at home for at least 10 months after giving birth. We find that the reform significantly increased the likelihood of high-income mothers to stay at home during the first year by 21.5 percentage points (see column 1 in Table 1). It can also be seen graphically in Figure A1a in the Appendix that the fraction of mothers staying home during the first 10 months increased steeply by about 20 percentage points. Our first stage effect estimates exceed the results found in previous literature, which is due to the specific sample used in this analysis (mothers who were employed before giving birth and who have a relatively high income). Including various control variables lowers the point estimate of the reform effect only marginally. Significant first stage coefficients and an F-statistic above 10 alleviate concerns about a weak instrumental variable.

We estimate the reduced form effect of having a peer mother who gave birth after the reform on the coworkers' probability to stay at home for at least 10 months after

childbirth. This can be interpreted as intention to treat (ITT) effect. We find that the ITT is positive and significant, amounting to 6.8 percentage points without covariates and 5.3 percentage points if we include covariates (see Table 1), suggesting that having a peer who gave birth before or after the reform has an important impact on coworkers' parental leave behavior.

Peer effects can be computed by dividing the reduced form (ITT) by the first stage estimate. As shown in Table 1, we find a significant peer effect of about 28.2 to 31.5 percentage points, i.e. a mother is about 30 percentage points more likely to stay at home during the first 10 months if her peer mothers decide to do so as a response to the parental leave benefit reform. Including linear trends in the date of childbirth and several control variables does not change the results substantially. Note that the point estimate has to be interpreted with caution because small imprecisions in reduced form or first stage point estimates can lead to large changes in the estimated peer effects. In cases where a mother has several peers who gave birth in the one-year window around the reform, the treatment variable can lie between zero and one because it is defined as the average peer outcome. The peer effect is estimated as the effect of a change from zero to one, i.e. the effect of all peer mothers deciding to stay at home in the first year compared to none.

The large magnitude of the estimated peer effect can be due to the specific subgroup for which the effect is estimated. The effects have to be interpreted as local average treatment effects (LATE) of reform-induced changes in peer mothers' parental leave behavior on the leave taking behavior of coworkers. In other words, we cannot identify the average treatment effect on the treated (ATT) because we estimate the effect only for a

Table 1: First stage, reduced form (ITT) and peer effect (LATE) 2SLS-estimation results

$y_C$	First stage		Reduced form		Peer effect	
$z_g$	0.215*** (0.060)	0.188*** (0.058)	0.068*** (0.024)	0.053** (0.025)		
$\bar{y}_P$					0.315*** (0.116)	0.282* (0.148)
$x_C$ (Month of childbirth)		0.000 (0.001)		-0.001 (0.001)		-0.001 (0.001)
Age at childbirth		-0.003 (0.003)		-0.004 (0.003)		-0.003* (0.003)
Peer age		-0.011 (0.008)		-0.002 (0.003)		0.001 (0.005)
Prior earnings		0.000 (0.000)		0.000 (0.000)		0.000* (0.000)
Peer earnings		0.000 (0.000)		0.000 (0.000)		0.000* (0.000)
High education		-0.038 (0.045)		-0.004 (0.036)		0.006 (0.036)
Low education		-0.084 (0.063)		0.010* (0.052)		0.124** (0.058)
Peer high education		0.025 (0.071)		0.005 (0.035)		-0.002 (0.035)
Peer low education		0.265*** (0.102)		0.006 (0.125)		-0.068 (0.125)
Firm size		0.000** (0.000)		0.000* (0.000)		0.000 (0.000)
Number of peer births		-0.050** (0.020)		-0.014 (0.010)		0.000 (0.011)
Peer group size		0.001*** (0.000)		0.000* (0.000)		0.000 (0.000)
West Germany		0.074 (0.061)		0.007 (0.029)		-0.014 (0.034)
Constant	0.533*** (0.060)	1.016*** (0.258)	0.748*** (0.019)	0.931*** (0.142)	0.580*** (0.079)	0.645*** (0.250)
*** p<0.01, ** p<0.05, * p<0.1						
N	1340	1336	1340	1336	1340	1336
$R^2$	0.067	0.117	0.006	0.016		
Robust F(1,310)	13.003***	10.609***				
Pre-mean ( $z_P = 0$ )	0.563		0.748			
Post-mean ( $z_P = 1$ )	0.760		0.816			

Notes: The dependent variable  $y_C$  is defined as an indicator equal to one if the individual does not return to work within 10 months after childbirth. First stage and reduced form regressions include the same control variables as the corresponding 2SLS regression. Standard errors (in parentheses) are clustered on firm level. Corvariates are measured at the time of coworker or peer date of childbirth respectively.

Source: IAB Linked Employer-Employee Panel (LIAB) LM 1993-2010.

subpopulation of those coworkers whose peers stay at home during the first 10 months after childbirth if their child is born after the reform, and would have returned to work within 10 months if their child were born before the reform. The LATE can be expected to differ from the ATT because peer effects are likely to be heterogeneous across firms and occupations. We expect peer effects to be larger in the subgroup of compliers, which contains groups where peer mothers respond to the reform, and would have returned early in absence of the *Eltern geld*. We know from the first stage estimation that the subgroup of compliers amounts to about one-fifth of the total sample. However, we cannot observe the compliers directly because we do not observe counterfactual outcomes. One way to learn more about the group of compliers is to use the variation in the first stage across covariate groups (see Angrist and Pischke, 2009). A first stage analysis across covariate groups, displayed in Table A4 in the Appendix, reveals that compliers are more likely to have university education, be in the upper third of the income distribution and work in large and old firms.

## 5.2 Validity and robustness

We use an instrumental variable approach to estimate direct reform effects and peer effects in the previous section. In contrast to a regression discontinuity research design, we do not include a trend in the running variable in the baseline analysis. This is not a classical RD setting since the running variable is date of childbirth of peer mothers, which is not correlated with the outcome of interest. Consequently, the curves of the outcome variable to the left and right of the reform cutoff are flat (see Figure A1a and Figure A1b in the Appendix). In this case, the average treatment effect can be computed as the difference

in the mean value of the outcome on the right and left hand side of the cutoff. An RD approach would be less efficient since we know that if randomization were successful, then the running variable is an irrelevant variable in this regression (Lee and Lemieux, 2010). We repeat the first stage estimation including split trends in date of childbirth on both sides of the reform cutoff date to strengthen our argument. Including trends in the running variable does not make a difference in the full sample, which comprises all medium- and high-income women who gave birth in the reform window (see Appendix Table A5). However, first stage coefficients become insignificant in the smaller sample of analysis due to the loss in efficiency. Given our small sample size, the instrumental variable approach is therefore our preferred method.

A crucial underlying assumption of our empirical method is the independence of the instrumental variable and potential outcomes. We argue in Section 3.2 that women who give birth around the reform cutoff date are unable to intentionally sort into the treatment group. If whether a mother is exposed to peers who gave birth before or after the reform is purely random, treated and non-treated mothers should have the same distribution of covariates. Most observable individual characteristics of peer mothers in the treatment and the comparison group do not differ significantly (Appendix Table A6). A simple mean comparison of treated and non-treated coworkers (Appendix Table A7) points to significant differences in only one individual-level variable, namely the number of days employed in the firm. However, group- and firm-level characteristics differ significantly between treated and non-treated mothers if measured at the individual level. Coworker mothers, whose peers gave birth after the reform, are more likely to be in larger peer groups, and work in smaller firms with less temporary workers, more women, and higher

average wages. Variables such as group and firm size are inflated by counting large groups multiple times. The differences in these variables are insignificant if we compare means at the group or firm level. There is no plausible theoretical explanation why firms with women who give birth in the first half of 2007 should differ from firms with women who give birth in the second half of 2006. However, new firms are added to the LIAB every year, which can lead to random variation in the number and average characteristics of firms across years. To correct for imbalances, thereby improving the precision of our estimates, we include a number of additional covariates that potentially affect maternal leave decisions in Table 2. Including different sets of covariates does not change the point estimates considerably, nor does it lead to large gains in precision of the estimates.

Table 2: Inclusion of additional individual, firm and regional characteristics

	First stage	Reduced form	Peer effect	N	Controls
Baseline	0.215*** (0.060)	0.068*** (0.024)	0.315*** (0.116)	1340	No
Additional peer chars (1)	0.188*** (0.056)	0.049* (0.026)	0.259* (0.151)	1336	Yes
Additional coworker chars (1)	0.191*** (0.057)	0.052** (0.024)	0.272* (0.142)	1336	Yes
Firm level covariates (2)	0.212*** (0.067)	0.087** (0.037)	0.409* (0.217)	642	Yes
Regional covariates (3)	0.164*** (0.056)	0.062** (0.025)	0.378** (0.189)	1295	Yes
Occupational Fixed Effects	0.176*** (0.059)	0.057** (0.025)	0.322* (0.165)	1336	Yes
Industry Fixed Effects	0.209*** (0.063)	0.062* (0.033)	0.297* (0.175)	968	Yes

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

*Notes:* The dependent variable  $y_C$  is defined as an indicator equal to one if the individual does not return to work within the first 10 months after childbirth. First stage and reduced form regressions include the same control variables as the corresponding 2SLS regression. Standard errors (in parentheses) are clustered on the firm level. Control variables include month of childbirth, age of the mother, prior-to-birth earnings, as well as the education level of both coworkers and peer mothers. Firm size, number of peer births in reform-window, peer group size and a dummy for West Germany. Additional individual characteristics (1) include experience, tenure in firm, days in unemployment, part-time employment prior-to-birth and the number of children. Firm level covariates (2) include a dummy for old firms (> 10 years) and standardized wages, churn rate, median firm income, the share of female employees, part-time and temporary workers, and the share of mothers between 2000 and 2005 in the firm with a leave spell longer than 10 months. Regional covariates (3) include district childcare coverage, population density and unemployment rate.

*Source:* IAB Linked Employer-Employee Panel (LIAB) LM 1993-2010.

Mothers who give birth in the second half of 2006 can also differ from mothers who give birth in 2007 for reasons unrelated to the parental leave benefit reform. The seasonality of births and the existence of contemporaneous family policy reforms may result in differences in the characteristics of peer mothers. In particular, several child care reforms carried out between 2005 and 2007 successively increased the availability of subsidized child care for children younger than three,<sup>21</sup> which had sizeable employment effects on mothers (see Geyer et al., 2015). A closer look at Table A6 in the Appendix reveals that the treatment group has more observations. In other words, the sample of mothers who gave birth from January to June 2007 is larger than the sample of mothers who gave birth between July and December 2006. Our identification strategy is threatened if this is due to selective fertility around the introduction of the reform. We repeat our sampling procedure for seven hypothetical reform dates on January 1<sup>st</sup> of the respective years from 2002 to 2009 to see whether the year around the actual reform (2007) differs from previous and later years. The distribution of births per month within the different samples, displayed in Figure A2 in the Appendix, shows that there were comparatively few births in the second half of 2006. A formal difference-in-difference test of the difference in the number of births around the reform-cutoff compared to all non-treated years yields a significant treatment coefficient. The treatment coefficient can be interpreted as a 5 percentage point increase in the number of births in the six months following the reform-cutoff. This result suggests that selective fertility may be a problem. However, we argue that the coefficient is small in magnitude and therefore unlikely to change our peer effect estimates considerably. Furthermore, balanced covariates between treated and

---

<sup>21</sup>For an overview of the child care reforms see Spieß (2011).

non-treated mothers suggest that, even if there are more mothers who give birth after the reform, these mothers do not differ substantially from those who give birth before the reform. By limiting the sample of peer mothers to those who gave birth in an six-month (Sample A) and eight-month window (Sample B) around the reform, we minimize the probability of selected fertility. Reassuringly, we find positive reduced form and peer effects using a shorter window, despite the smaller variation in peer behavior, even though the coefficients are only partly significant (see Table 3).

Even if treatment assignment is purely random, the exclusion restriction could be violated if the maternal leave behavior of peer mothers induces others to change their fertility or employer, which in turn affects group composition and can thereby have an indirect effect on coworkers' maternal leave decisions. To test whether there are differential changes in group composition in the treatment and control group, we compare group sizes, exits, entries, and fertility after the reform-window (see Table A8 in the Appendix). We find that the peer group size is larger on average in the treatment group. Furthermore, more women and men join and leave their peer groups in the two years following the parental leave benefit reform in firms and occupations where peer mothers gave birth after the reform. However, these differences between the treatment and control group are not statistically significant and could therefore be due to random variation. Nevertheless, we cannot exclude the possibility that there are in fact relevant differences between the treatment and control group, which are undetectable given the small sample size.

Another set of robustness checks concerns the definition of the sample. While the sample used for the baseline specification shown in Table 1 consists of groups in which we observe peer mothers giving birth to a child either before or after the reform, Table 3



presents the results from four alternative definitions of the estimation sample. Sample C is specified as the baseline sample, however here we drop all observations with peer births occurring in December 2006 or January 2007 (rather than just a 4-week window around the reform date). Dropping peer groups where there is a birth in a 60-day window around the reform date does not change the results compared to our baseline specification. In the baseline specification, peer groups are defined by firm (establishment) and occupational group. In contrast, we assume that mothers interact with each other across occupational groups in sample specification D, where the whole firm is defined as the relevant peer group. This results in a sample with smaller firms on average, because the sample selection is still conditional on observing births on only one side of the reform cutoff date. We find smaller, insignificant first stage and ITT effects using this specification. In Sample E we restrict the sample to coworkers who gave birth to a child at least 9 months after the last peer mother within the group has given birth to a child in order to make sure that enough time has gone by that the coworkers may observe reactions to the peer's decisions. Reassuringly, point estimates for all three parameters of interest are very similar to the baseline specification.

Finally, we repeat the analysis using a sample without any restrictions on peer income (Sample F). In contrast to our baseline specification, we also include those peer groups where the (mean) peer income is in the bottom third of the income distribution. We exclude this group in our baseline specification due to heterogenous reform effects (low income mothers can be induced to return to work earlier due to the parental leave benefit reform) and the implied violation of the monotonicity assumption. The first stage effect disappears when we include all income groups, suggesting that the reform indeed did not

Table 3: Results from alternative sample specifications

	First stage	Reduced form	Peer effect	N	Controls
A. Six-month reform-window ( $\pm 3$ months)	0.196*** (0.067)	0.046* (0.025)	0.236 (0.152)	1482 <sup>1</sup>	No
	0.256*** (0.075)	0.028 (0.024)	0.111 (0.101)	1480 <sup>1</sup>	Yes
B. Eight-month reform-window ( $\pm 4$ months)	0.175*** (0.054)	0.047* (0.024)	0.267* (0.157)	1438 <sup>1</sup>	No
	0.183*** (0.057)	0.040* (0.023)	0.222 (0.143)	1434 <sup>1</sup>	Yes
C. 31 days domut around cutoff date	0.223*** (0.063)	0.065*** (0.024)	0.293** (0.113)	1245	No
	0.197*** (0.061)	0.049* (0.025)	0.247* (0.144)	1241	Yes
D. Peer groups = firms	0.103 (0.071)	0.033 (0.027)	0.315 (0.296)	1089	No
	0.116* (0.067)	0.034 (0.027)	0.296 (0.270)	1083	Yes
E. Coworker birth at least 9 months after peer birth	0.206*** (0.061)	0.070*** (0.025)	0.340*** (0.130)	1107	No
	0.169*** (0.060)	0.064** (0.027)	0.380* (0.195)	1105	Yes
F. Using all income groups	0.112 (0.046)	0.023 (0.020)	0.202 (0.176)	2158	No
	0.086 (0.044)	0.011 (0.020)	0.130 (0.236)	2149	Yes

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

*Notes:* The dependent variable  $y_C$  is defined as an indicator equal to one if the individual does not return to work within 10 months after childbirth. First stage and reduced form regressions include the same control variables as the corresponding 2SLS regression. Standard errors (in parentheses) are clustered on the firm level. Control variables include month of childbirth, age of the mother, prior-to-birth earnings and education level of both coworkers and peer mothers. Firm size, number of peer births in reform-window, peer group size and a dummy for West Germany.

<sup>1</sup>A smaller reform-window can lead to a larger sample of coworkers due to a longer time window for coworker births, and/or through the selection of more peer groups with peer births on only one side of the cutoff.

*Source:* IAB Linked Employer-Employee Panel (LIAB) LM 1993-2010.

induce low income mothers to stay at home for the first year following childbirth. Without a significant first stage, the estimates of reduced form and peer effects are meaningless.

Our baseline results, presented in Table 1 in Section 5.1, suggest that individual parental leave decisions are substantially influenced by the parental leave behavior of coworkers working in the same firm and occupation. If these results are truly peer effects, we expect both first stage and reduced form effects to disappear when we center the reform-window around a date when there was no change in the parental leave benefit regime. To test this hypothesis, we create a *placebo*-sample by re-centering the reform-window around January 1, 2006, using the same sample selection procedure as in our baseline specification. The results are displayed in Table 4. A significant first stage would hint at a difference in parental leave behavior of women who give birth in the first vs. the second half of a year (in this case January to June 2006 vs. July to December 2005). A significant reduced form effect would suggest that mothers who have a peer who gave birth between January and June 2006 differ from those who give birth in the second half of 2005. However, we find no first stage, reduced form, and consequently no peer effects using the placebo sample. We find a zero first stage reform effect if we include covariates in the regression. Therefore, the peer effect estimate, which is computed by dividing the reduced form by the first stage, is undefined.

## 6 Mechanisms

Peer effects can operate through several channels of social interaction. One mechanism through which peer effects could arise is the transmission of information about the

Table 4: Results from placebo sample

	First stage	Reduced form	Peer effect	N	Controls
Placebo sample using a fake reform date	-0.022 (0.062)	-0.017 (0.027)	0.805 (2.611)	1180	No
	0.000 (0.055)	-0.017 (0.026)	– –	1179	Yes
*** p<0.01, ** p<0.05, * p<0.1					

*Notes:* The dependent variable  $y_C$  is defined as an indicator equal to one if the individual does not return to work within 10 months following childbirth. First stage and reduced form regressions include the same control variables as the corresponding 2SLS regression. Standard errors (in parentheses) are clustered on firm level. Control variables include month of childbirth, age of the mother, prior-to-birth earnings and education level of both coworkers and peer mothers. Firm size, number of peer births in reform-window, peer group size and a dummy for West Germany.

*Source:* IAB Linked Employer-Employee Panel (LIAB) LM 1993-2010.

parental leave benefit program. Mothers may learn from their peers about the existence of the parental leave benefit program, its eligibility criteria, and the application procedure. However, we argue that this is not likely to be a relevant mechanism of peer effects in our context, because the program is well known and parents are generally informed about the organizational details of parental leave benefits. There is an easy-to-use online tool to compute expected benefits and the application form has to be filled in regardless of the length of the parental leave. An almost 100 percent take-up rate of at least some parental leave indicates that there is no lack of practical information in this context.

Despite the general knowledge about parental leave benefits, observing peers at work could reveal information on career related consequences as well as the workplace-specific costs and benefits of parental leave. Women may, for example, acquire information from their female peers that reduces uncertainty about post-birth career opportunities, wage-trajectories, and the possibility of combining family and work. In a standard social learning model, we would expect that women with more diffuse priors about the costs and benefits of parental leave should be more strongly influenced by their peers (see Goyal, 2011, for an overview of models of social learning in networks). In other words, women

are more likely to change their parental leave decisions after observing peer leave choices in situations where information about the consequences of leave choices is incomplete. Consequently, we expect stronger peer effects in situations with greater career-related uncertainty, if the revelation of information is a driving mechanism of peer effects in our context. For example, we expect stronger peer effects for women who have their first child and for mothers who have short tenure in the firm. Furthermore, we expect stronger peer effects in younger firms, firms with high turnover and job uncertainty, where workplace specific information is more valuable.

Table 5: Heterogenous effects - results for different subgroups

	First stage	Reduced form	Peer effect	N
Baseline	0.215*** (0.060)	0.068*** (0.024)	0.315*** (0.116)	1340
Tenure $\leq$ 2 years	0.241*** (0.086)	0.177*** (0.049)	0.735** (0.289)	374
Tenure $\leq$ 3 years	0.222** (0.086)	0.119*** (0.042)	0.535** (0.241)	542
Tenure $\leq$ 4 years	0.285** (0.081)	0.070* (0.040)	0.376 (0.244)	646
Tenure $>$ 4 years	0.221*** (0.062)	0.036 (0.031)	0.164 (0.140)	690
Including only first births	0.166*** (0.059)	0.060** (0.028)	0.364 (0.199)	1028
High layoff rate	0.146** (0.068)	0.033 (0.042)	0.228 (0.291)	663
Low layoff rate	0.249*** (0.071)	0.043 (0.035)	0.171 (0.149)	559
East Germany	0.169* (0.099)	0.019 (0.043)	0.110 (0.258)	463
West Germany	0.201*** (0.066)	0.074** (0.030)	0.369** (0.167)	873

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

*Notes:* The dependent variable  $y_C$  is defined as an indicator equal to one if the individual does not return to work within 10 months after childbirth. First stage and reduced form regressions include the same control variables as the corresponding 2SLS regression. Standard errors (in parentheses) are clustered on firm level. Control variables include month of childbirth, age of the mother, prior-to-birth earnings and education level of both coworkers and peer mothers. Firm size, number of peer births in reform-window, peer group size and a dummy for West Germany.

*Source:* IAB Linked Employer-Employee Panel (LIAB) LM 1993-2010.

In order to shed light on the mechanisms of peer effects, we split the sample into groups of coworkers with up to two, three, and four years of work experience at the present firm, and those with longer tenure (more than four years at the same firm). *Ceteris paribus*, we expect mothers with shorter tenure to face more uncertainty concerning the firm's reaction to their leave decision than mothers with longer tenure. As Table 5 shows, we find a larger peer effect of 73.5 percentage points for mothers with up to two years of tenure. The effect fades away as tenure increases. For mothers with more than four years of tenure, the point estimate falls to 16.4 and becomes statistically insignificant. We interpret these results as suggestive evidence for the hypothesis that peer effects are at least partly driven by the reduction of career-related uncertainty for mothers who wish to take longer leaves (longer than 10 months), because they can observe peers who took longer leaves in response to the parental leave benefit reform, as well as their employers' reactions to it. Similarly, we expect mothers to face higher uncertainty regarding the consequences of an extended parental leave if they gave birth to their first child, compared to higher order births. Most birth events observed in our sample are first births, therefore we cannot compare the different effects by number of children. An analysis including only first births yields a slightly higher point estimate of the peer effect. However, the difference is not statistically significant from the baseline estimate. Career-related uncertainty is also expected to be high in firms where the layoff rate is high. We split the sample into firms with high and low layoff rates and find that point estimates for peer effects are only insignificantly larger in the former group. Further sample divisions, e.g. by age or education, are impeded by the small number of observations per group.

Competing explanations to the transmission of career-related information include imi-

tation and herding behavior (Akerlof and Kranton, 2000; Banerjee, 1992). We summarize these explanations as preferences for conformity to norms within social reference groups. This can include peer pressure. Social norms regarding parental leave may differ by socioeconomic status, employer, occupational group and region. For example, norms regarding parental leave differ greatly between East and West Germany. Even though the overall duration of parental leave before the reform was longer on average in the West, it was also more common among high-income mothers to return within the first 10 months in West Germany compared to East Germany, where a one-year leave was a long standing tradition (see Figure A3 in the Appendix). The parental leave benefit reform changed societal norms toward a parental leave duration of one year. Consequently, this amounts to a larger change in social norms in West Germany than in the former East. Separate estimations for East and West Germany show that all three effects (first stage, reduced form and LATE) are larger in West than East Germany. Reduced form and peer effect estimates for East German mothers are not statistically significant. This could also be due to the relatively small number of observations in the sample of East German mothers. However, smaller effects in East Germany are consistent with a larger change in social norms in West Germany. A large fraction of East German mothers stayed at home for at least 10 months before the reform. As a result, the reform effect on our outcome variable is not as strong in the East as in the West.

Where the parental leave benefit reform did not change social norms substantially (e.g. in East Germany), peer effects are expected to be smaller. Note that we distinguish between information transmission in settings with career-related uncertainty and settings where social norms are in flux. We are, however, unable to clearly distinguish these two

channels of peer effects empirically. Based on stronger peer effects for mothers with short tenure and East German mothers, we argue that information transmission about the costs and benefits of a long parental leave, as well as preferences for conformity to changing social norms may constitute relevant channels of peer effects in settings where the reform had large direct effects. Our results are consistent with a social learning model where the information provided by peers reduces social and career-related uncertainty.

Previous literature suggests that leisure complementarities are another potential source of peer effects in labor supply decisions (see Alesina et al., 2006). A peer mother who is enjoying a long parental leave may induce her coworker to do the same, so that time can be spent together. On the other hand, the opposite could be true and the absence of one mother may reduce the probability that her coworkers will simultaneously take a long leave. In particular, in small firms, the absence of an employee can increase the workload and responsibilities of her coworkers and thereby make a (long) parental leave more costly. However, in our context, peer mothers and their coworkers give birth with a temporal distance and hence do not, generally speaking, take leave at the same time. Consequently, the scope for complementarities is limited because leave spells of peers and their coworkers often do not overlap. As a test of leisure complementarities, we restrict the sample to mothers whose peers gave birth at least nine months earlier. We find similar effects (Sample E, Table 3) and, hence, conclude that this is unlikely to be an important channel of peer effects in our context.



## 7 Conclusion

The decision of mothers regarding how long to take parental leave in order to take care of her children is influenced not only by financial considerations but also by peer behavior. In this paper, we estimate the quantitative importance of peers' decisions on the parental leave decisions of mothers, in particular on the probability to return to work within the first 10 months after giving birth.

We use exogenous variation in the length of parental leaves of mothers induced by a parental leave benefit reform in Germany in 2007 to identify causal peer effects. The reform strongly increased financial incentives to take a leave of one year, especially for medium- and high-income mothers. Using linked employer-employee data, this methodology allows us to identify the peer effect as the local average treatment effect for the group of mothers with coworkers who decided for a longer leave due to the reform. For this group, we find a statistically significant and large peer effect: if a mother has a peer who opted for a longer leave due to the reform, the probability that she will take parental leave for at least 10 months is about 30 percentage points higher than if her peer returned to work after no more than 10 months. This strong effect shows that the influence of peers is quantitatively important. The results are robust to a large set of different specifications with respect to the definition of the peer group, the definition of the estimation sample, as well as the inclusion of covariates.

Our results suggest that preferences for conformity and the transmission of information about the costs and benefits of a (long) parental leave may be relevant mechanism of peer effects in our context. Information transmission is expected to be particularly

important in situations with high career-related uncertainty. We show that for the subgroup of women with short tenure at the same firm, a group that supposedly faces more uncertainty regarding the employer's reaction to leave decisions, the peer effect is larger than for the group with longer tenure. Preferences for conformity to peer behavior are expected to be particularly important in situations with changing social norms. In East Germany, where the parental leave benefit reform did not change social norms substantially, peer effects are expected to be smaller. Separate estimations for East and West Germany show that both direct reform effects and peer effects are larger in West than in East Germany.

Our results are also interesting from a policy point of view. We show that just the fact that a mother (who gave birth to a child after the reform has been implemented) has a peer who gave birth shortly after the introduction of the new parental leave scheme increases her probability of taking a longer leave by 7 percentage points in contrast to mothers with peers who gave birth to a child shortly before the reform. This effect, which can be interpreted as intention to treat effect, shows that policy reforms have an impact on the individuals' choices that go far beyond the immediate behavioral reaction due to changes in financial incentives.

## References

- AKERLOF, G. A. AND R. E. KRANTON (2000): “Economics and identity,” *Quarterly Journal of Economics*, 715–753.
- ALESINA, A. F., E. L. GLAESER, AND B. SACERDOTE (2006): “Work and Leisure in the US and Europe: Why so Different?” in *NBER Macroeconomics Annual 2005, Volume 20*, MIT Press, 1–100.
- ANGRIST, J. D. AND J.-S. PISCHKE (2009): *Mostly Harmless Econometrics: An Empiricist’s Companion*, Princeton University Press.
- ASPHJELL, M. K., L. HENSVIK, AND J. P. NILSSON (2013): “Businesses, Buddies, and Babies: Fertility and Social Interactions at Work,” Working Paper Series, Center for Labor Studies 2013:8, Uppsala University, Department of Economics.
- BANERJEE, A. V. (1992): “A simple model of herd behavior,” *The Quarterly Journal of Economics*, 797–817.
- BEBLO, M., C. LAUER, AND K. WROHLICH (2005): “Ganztagschulen und Erwerbsbeteiligung von Müttern: eine Mikrosimulationsstudie für Deutschland,” ZEW Discussion Papers 05-93, Centre for European Economic Research, Mannheim.
- BERGEMANN, A. AND R. T. RIPHAHN (2011): “The introduction of a short-term earnings-related parental leave benefit system and differential effects on employment intentions,” *Schmollers Jahrbuch: Journal of Applied Social Science Studies/Zeitschrift für Wirtschafts-und Sozialwissenschaften*, 131, 315–325.

- (2015): “Maternal Employment Effects of Paid Parental Leave,” IZA Discussion Paper 9073, IZA - Institute for the Study of Labor, Bonn.
- BICK, A. AND N. FUCHS-SCHÜNDELN (2017): “Taxation and Labor Supply of Married Couples across Countries: A Macroeconomic Analysis,” *The Review of Economic Studies*.
- BLAU, F. AND L. KAHN (2006): “Changes in the Labor Supply Behavior of Married Women: 1980-2000,” *Journal of Labor Economics*, 25, 393–438.
- BLUME, L., W. BROCK, S. DURLAUF, AND R. JAYARAMAN (2015): “Linear Social Interactions Models,” *Journal of Political Economy*, 123, 444 – 496.
- BLUME, L. E., W. A. BROCK, S. N. DURLAUF, AND Y. M. IOANNIDES (2010): “Identification of social interactions,” in *Handbook of Social Economics*, ed. by J. Benhabib, A. Bisin, and M. O. Jackson, Elsevier, vol. 1.
- BLUNDELL, R., A. BOZIO, AND G. LAROQUE (2013): “Extensive and Intensive Margins of Labor Supply,” *Fiscal Studies*, 34, 1–29.
- BRAMOULLÉ, Y., H. DJEBBARI, AND B. FORTIN (2009): “Identification of Peer Effects Through Social Networks,” *Journal of Econometrics*, 150, 41–55.
- BROCK, W. A. AND S. N. DURLAUF (2001): “Discrete choice with social interactions,” *The Review of Economic Studies*, 68, 235–260.
- BROWN, K. (2013): “The link between pensions and retirement timing: Lessons from California teachers,” *Journal of Public Economics*, 98, 1–14.

- BUNDESAGENTUR FÜR ARBEIT, NÜRNBERG (1988): *Klassifizierung der Berufe: systematisches und alphabetisches Verzeichnis der Berufsbenennungen*, <https://statistik.arbeitsagentur.de/>.
- CORNELISSEN, T., C. DUSTMANN, AND U. SCHÖNBERG (2017): “Peer Effects in the Workplace,” *American Economic Review*, 107, 425–56.
- DAHL, G. B., K. V. LØKEN, AND M. MOGSTAD (2014): “Peer Effects in Program Participation,” *American Economic Review*, 104, 2049–74.
- [DATASET] BUNDESINSTITUT FÜR BAU-, STADT- UND RAUMFORSCHUNG (BBSR) IM BUNDESAMT FÜR BAUWESEN UND RAUMORDNUNG (BBR), BONN (2015): “Indikatoren und Karten zur Raum- und Stadtentwicklung. INKAR,” [www.inkar.de](http://www.inkar.de).
- [DATASET] KLOSTERHUBER, W., J. HEINING, AND S. SETH (2013): “Linked-employer-employee-data from the IAB: LIAB longitudinal model 1993-2010 (LIAB LM 9310),” FDZ-Datenreport 08/2013 (en), IAB Nürnberg.
- DEARING, H., H. HOFER, C. LIETZ, R. WINTER-EBMER, AND K. WROHLICH (2007): “Why are mothers working longer hours in Austria than in Germany? Evidence from a comparative microsimulation study,” *Fiscal Studies*, 28, 463–495.
- DEL BOCA, D., M. LOCATELLI, AND S. PASQUA (2000): “Employment Decisions of Married Women: Evidence and Explanations,” *Labour*, 14, 35–52.
- DUFLO, E. AND E. SAEZ (2003): “The Role of Information and Social Interactions in Retirement Plan Decisions: Evidence from a Randomized Experiment,” *The Quarterly Journal of Economics*, 118, 815.

- FEDERAL MINISTRY OF FAMILY AFFAIRS, SENIOR CITIZENS, W. AND Y. (*Bundesministerium für Familien, Senioren, Frauen und Jugend*) (2014): “Dossier Müttererwerbstätigkeit: Erwerbstätigkeit, Erwerbsumfang und Erwerbsvolumen,” [www.bmfsfj.de/bmfsfj/service/publikationen/dossier-muettererwerbstaetigkeit](http://www.bmfsfj.de/bmfsfj/service/publikationen/dossier-muettererwerbstaetigkeit).
- FERNANDEZ, R. (2013): “Cultural Change as Learning: The Evolution of Female Labor Force Participation over a Century,” *American Economic Review*, 103, 472–500.
- FITZENBERGER, B. AND G. WUNDERLICH (2004): “The Changing Life-Cycle Pattern in Female Employment: A Comparison of Germany and the UK,” *Scottish Journal of Political Economy*, 51, 302–328.
- FOGLI, A. AND L. VELDKAMP (2011): “Nature or Nurture? Learning and the Geography of Female Labor Force Participation,” *Econometrica*, 79, 1103–1138.
- GEYER, J., P. HAAN, AND K. WROHLICH (2015): “The effects of family policy on mothers’ labor supply: Combining evidence from a structural model and a quasi-experimental approach,” *Labour Economics*, 36, 84–98.
- GLAESER, E. L., B. I. SACERDOTE, AND J. A. SCHEINKMAN (2003): “The Social Multiplier,” *Journal of the European Economic Association*, 1, 345–353.
- GOLDIN, C. (1990): *Understanding the Gender Gap: An Economic History of American Women*, New York: Oxford University Press.
- GOYAL, S. (2011): “Chapter 15: Learning in Networks,” in *Handbook of Social Economics*, Elsevier, vol. 1.

- GRUNOW, D. AND D. MÜLLER (2012): “Kulturelle und strukturelle Faktoren bei der Rückkehr in den Beruf: ostdeutsche, westdeutsche und ost-west-mobile Mütter im Vergleich,” *Zeitschrift für Familienforschung. Sonderheft*, 55–77.
- HESSELIUS, P., J. P. NILSSON, AND P. JOHANSSON (2009): “Sick of your Colleagues’ Absence?” *Journal of the European Economic Association*, 7, 583–594.
- KLUVE, J. AND S. SCHMITZ (2018): “Back to Work: parental benefits and mothers’ labor market outcomes in the medium-run,” *Industrial and Labor Relations Review*, 71, 143–173.
- KLUVE, J. AND M. TAMM (2013): “Parental leave regulations, mothers’ labor force attachment and fathers’ childcare involvement: evidence from a natural experiment,” *Journal of Population Economics*, 26, 983–1005.
- LEE, D. S. AND G. F. LEMIEUX (2010): “Regression Discontinuity Designs in Economics,” *Journal of Economic Literature, American Economic Association*, 48, 281–355.
- LUNDBORG, P., E. PLUG, AND A. W. RASMUSSEN (2017): “Can Women Have Children and a Career? IV Evidence from IVF Treatments,” *American Economic Review*, 107, 1611–37.
- MANSKI, C. F. (1993): “Identification of endogenous social effects: The reflection problem,” *The Review of Economic Studies*, 60, 531–542.
- MARCUS, J., J. NEMITZ, AND C. K. SPIESS (2013): “Ausbau der Ganztagschule:

- Kinder aus einkommensschwachen Haushalten im Westen nutzen Angebote verstärkt,” *DIW-Wochenbericht*, 80, 11–23.
- MAS, A. AND E. MORETTI (2009): “Peers at Work,” *The American Economic Review*, 99, 112–145.
- MAURIN, E. AND J. MOSCHION (2009): “The Social Multiplier and Labor Market Participation of Mothers,” *American Economic Journal: Applied Economics*, 1, 251–272.
- MÜLLER, D. AND K. STRAUCH (2017): “Identifying mothers in administrative data,” FDZ-Methodenreport 13, Research Data Center (FZD) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB).
- NEUMARK, D. AND A. POSTLEWAITE (1998): “Relative income concerns and the rise in married women’s employment,” *Journal of Public Economics*, 70, 157 – 183.
- NICOLETTI, C., K. G. SALVANES, AND E. TOMINEY (2016): “The Family Peer Effect on Mothers’ Labour Supply,” IZA Discussion Paper 9927, IZA - Institute for the Study of Labor, Bonn.
- PINK, S., T. LEOPOLD, AND H. ENGELHARDT (2014): “Fertility and social interaction at the workplace: Does childbearing spread among colleagues?” *Advances in Life Course Research*, 21, 113 – 122, fertility over the Life Course.
- POLACHEK, S. AND J. XIANG (2014): “The Gender Pay Gap Across Countries: A Human Capital Approach,” IZA Discussion Papers 8603, Institute for the Study of Labor (IZA).



- RAUTE, A. (2019): “Can financial incentives reduce the baby gap? Evidence from a reform in maternity leave benefits,” *Journal of Public Economics*, 169, 203–222.
- ROSENFELD, R. A., H. TRAPPE, AND J. C. GORNICK (2004): “Gender and work in Germany: Before and after reunification,” *Annual Review of Sociology*, 103–124.
- RUHM, C. J. (1998): “The Economic Consequences of Parental Leave Mandates: Lessons from Europe,” *The Quarterly Journal of Economics*, 113, 285–317.
- SCHÖNBERG, U. (2009): “Does the IAB employment sample reliably identify maternity leave taking? A data report,” *Zeitschrift für Arbeitsmarkt Forschung*, 42, 49–70.
- SPIESS, C. K. (2011): “Vereinbarkeit von Familie und Beruf—wie wirksam sind deutsche Care Policies?” *Perspektiven der Wirtschaftspolitik*, 12, 4–27.
- TAMM, M. (2013): “The Impact of a Large Parental Leave Benefit Reform on the Timing of Birth around the Day of Implementation,” *Oxford Bulletin of Economics and Statistics*, 75, 585–601.
- WEINBERG, B., P. REAGAN, AND J. YANKOW (2004): “Do Neighborhoods Affect Hours Worked? Evidence from Longitudinal Data,” *Journal of Labor Economics*, 22, 891–924.
- WROHLICH, K., E. BERGER, J. GEYER, P. HAAN, D. SENGÜL, C. K. SPIESS, AND A. THIEMANN (2012): “Elterngeld Monitor. Forschungsprojekt im Auftrag des Bundesministeriums für Familie, Senioren, Frauen und Jugend,” *Politikberatung kompakt*.

# A Appendix

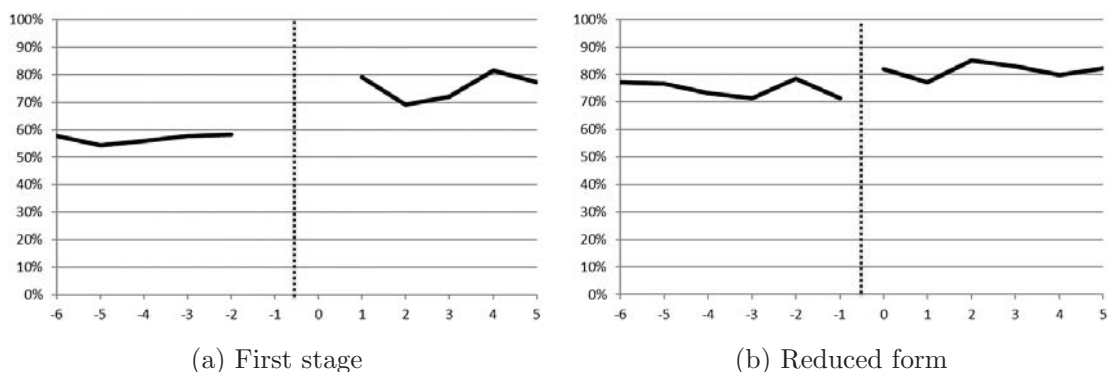
Table A1: Sample sizes relative to total number of births (07/2007 - 12/2009)

	Baseline	Sample A	Sample B	Sample C	Sample D	Sample E
Coworkers	1340	1482	1438	1245	1089	1107
<i>Percent of births</i>	<i>11.1%</i>	<i>11.2%</i>	<i>11.2%</i>	<i>10.3%</i>	<i>9.0%</i>	<i>9.2%</i>
Peer mothers	488	345	417	450	334	467

Baseline specification: either births before or after the cutoff date in the reform-window  
 Sample A: Limit reform-window to 6 months (3 before and 3 after reform).  
 Sample B: Limit reform-window to 8 months (4 before and 4 after reform).  
 Sample C: Limit reform-window to births that occur at least 30 days before/after cutoff.  
 Sample D: Peer groups are defined to be equal to firms.  
 Sample E: Restrict coworker birth to be at least 9 months after last peer birth.

*Source:* IAB Linked Employer-Employee Panel (LIAB) LM 1993-2010.

Figure A1: Fraction of mothers who do not return within 10 months by peers' month of childbirth (January 2007 = 0)



*Source:* IAB Linked Employer-Employee Panel (LIAB) LM 1993-2010.

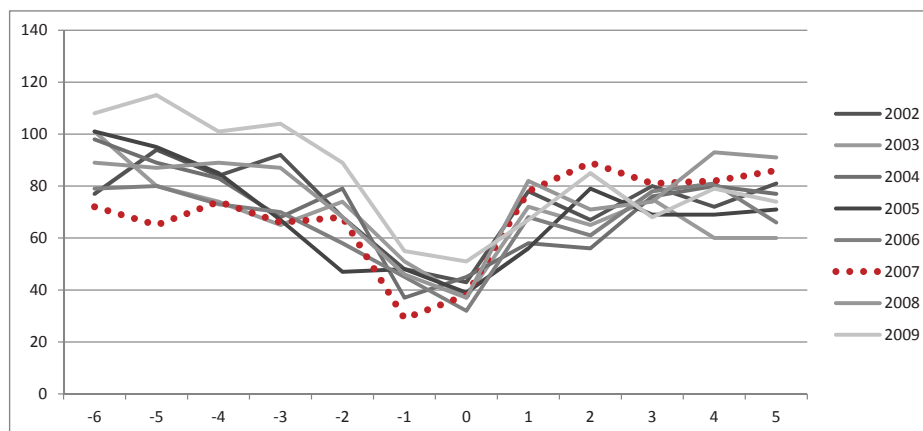
Table A2: Baseline sample characteristics in comparison to all observed mothers

	All mothers			Sample		
	Mean	SD	N	Mean	SD	N
No return within 10 months	0.79	0.41	12069	0.79	0.41	1340
Return within 11-15 months	0.40	0.49	11111	0.42	0.49	1222
No return within 24 months	0.25	0.44	7682	0.24	0.43	848
Age at childbirth	31.79	4.52	12069	32.14	4.25	1340
Prior earnings	2365.38	1159.50	12069	2842.40	1165.95	1340
Days in employment	3196.19	1562.36	12069	3224.46	1557.75	1340
Days in firm	2448.60	1770.78	12069	2124.74	1782.24	1340
Days in unemployment	128.17	339.66	12069	89.34	238.32	1340
Return to part-time	0.26	0.44	9197	0.20	0.40	1028
Return to mini-job	0.05	0.21	9197	0.03	0.18	1028
High education ( $\geq$ college)	0.13	0.33	12069	0.21	0.41	1340
Low education ( $<$ highschool)	0.04	0.20	12069	0.03	0.18	1340
Migration background	0.05	0.22	12069	0.06	0.23	1340
Number of children	1.32	0.52	12069	1.24	0.44	1340
Date of childbirth (Jan 01, 2007 is zero)	621.75	258.20	12069	620.95	266.26	1340
West Germany	0.59	0.49	12069	0.65	0.48	1340
Employer change upon return	0.18	0.38	9197	0.18	0.38	1028
Group size	31.29	110.59	3031	28.99	54.70	375
Births in group	1.09	4.75	3321	1.20	0.83	378
Firm size	268.90	722.53	1927	722.72	1482.01	311
Old firm ( $\geq$ 10 years)	0.80	0.40	1933	0.93	0.26	311
Standard wages	0.33	0.47	1146	0.37	0.48	243
Median gross daily income in firm	83.51	30.88	1884	104.09	26.12	311
Share of part-time workers in firm	0.25	0.26	1812	0.21	0.20	294
Share of women in firm	0.53	0.28	1825	0.51	0.25	297
Share of temporary workers in firm	0.09	0.16	1815	0.09	0.14	295
District childcare coverage	17.91	13.40	380	20.26	14.85	252
District population density	530.71	689.02	380	556.53	731.64	252
District unemployment rate	7.74	3.80	380	8.13	4.09	252

Notes: 'All mothers' refers to all women in the LIAB who gave birth between July 2007 and December 2009.

Source: IAB Linked Employer-Employee Panel (LIAB) LM 1993-2010. Characteristics are measured at the time of childbirth, except where otherwise specified.

Figure A2: Distribution of births per month in distance to January of each year



Calculations are based on the baseline sample specification sample with either births before or after January 1<sup>st</sup> in a one-year window around the (hypothetical) reform-date.

Table A3: Descriptive statistics for alternative sample specifications

Variable	Sample A		Sample B		Sample C		Sample D		Sample E	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
No return within 10 months	0.79	0.41	0.80	0.40	0.79	0.40	0.80	0.40	0.79	0.41
Return within 11-15 months	0.42	0.49	0.41	0.49	0.41	0.49	0.43	0.49	0.42	0.49
Age at childbirth	32.15	4.13	32.18	4.21	32.11	4.28	32.08	4.32	32.11	4.24
Prior earnings	2795.56	1145.58	2823.50	1156.49	2832.75	1167.70	2643.53	1128.26	2858.52	1169.13
Days in employment	3273.18	1541.68	3283.11	1563.48	3215.60	1558.36	3299.74	1544.15	3208.54	1574.58
Days in firm	2353.49	1763.51	2229.04	1728.47	2085.90	1773.23	2108.55	1764.13	2168.87	1771.72
Days in unemployment	77.13	225.53	93.06	250.60	91.05	243.27	99.70	292.32	87.86	238.03
Part-time employed	0.29	0.45	0.26	0.44	0.25	0.44	0.25	0.43	0.24	0.43
High education ( $\geq$ college)	0.19	0.39	0.18	0.38	0.20	0.40	0.12	0.33	0.21	0.41
Low education ( $<$ highschool)	0.03	0.18	0.05	0.22	0.03	0.17	0.04	0.21	0.03	0.17
Migration background	0.07	0.25	0.05	0.23	0.06	0.23	0.05	0.21	0.06	0.23
Number of children	1.30	0.49	1.26	0.47	1.24	0.45	1.26	0.47	1.25	0.45
Date of childbirth (01/01/07= 0)	613.85	261.57	577.21	283.21	622.89	267.28	632.75	268.06	695.57	238.93
West Germany	0.65	0.48	0.68	0.47	0.64	0.48	0.63	0.48	0.65	0.48
Employer change upon return	0.15	0.36	0.17	0.37	0.18	0.38	0.17	0.37	0.17	0.38
Group size	43.16	70.09	34.69	59.84	29.37	56.48	20.99	52.42	30.04	55.93
Births in reform-window	2.09	1.81	1.67	1.34	1.21	0.85	0.57	0.94	1.23	0.84
Firm size	837.08	1709.27	775.57	1618.56	734.16	1528.60	284.48	395.10	740.71	1525.45
Old firm ( $\geq$ 10 years)	0.94	0.25	0.93	0.26	0.93	0.25	0.92	0.26	0.93	0.25
Standard wages	0.36	0.48	0.37	0.48	0.37	0.48	0.32	0.47	0.36	0.48
Median daily income in firm	105.14	25.73	102.85	25.67	104.21	25.35	99.29	24.14	104.55	25.99
Share of part-time workers	0.24	0.22	0.23	0.22	0.21	0.19	0.18	0.19	0.21	0.19
Share of women in firm	0.54	0.26	0.53	0.26	0.51	0.25	0.47	0.25	0.51	0.25
Share of temporary workers	0.08	0.10	0.09	0.13	0.10	0.14	0.07	0.11	0.10	0.14
District childcare coverage	19.69	14.77	19.37	14.65	20.61	14.93	20.69	15.18	20.83	14.88
District population density	588.78	765.47	574.91	743.99	566.95	737.46	587.36	773.88	559.39	736.35
District unemployment rate	8.19	3.92	8.10	4.01	8.21	4.15	8.16	4.18	8.07	4.08
N	1482		1438		1245		1089		1107	

Sample A: Limit reform-window to 6 months (3 before and 3 after reform).

Sample B: Limit reform-window to 8 months (4 before and 4 after reform).

Sample C: Limit reform-window to births that occur at least 30 days before/after cutoff.

Sample D: Peer groups are defined to be equal to firms.

Sample E: Restrict coworker birth to be at least 9 months after last peer birth.

Source: IAB Linked Employer-Employee Panel (LIAB) LM 1993-2010.

Characteristics are measured at the time of childbirth, except where otherwise specified.

Table A4: Complier characteristics - first stage results across subgroups

Dep. Variable = $y_P$	Baseline	High education	High income ( $> 1/3$ )	Large firms ( $\geq 300$ )	Old firms ( $> 10y$ )
$z_P$	0.185*** (0.045)	0.192** (0.093)	0.258*** (0.064)	0.232*** (0.059)	0.207*** (0.046)
Age at childbirth	0.175*** (0.045) -0.011 (0.006)	0.188* (0.095) -0.022 (0.015)	0.237*** (0.063) -0.018 (0.009)	0.222*** (0.057) -0.009 (0.006)	0.203*** (0.045) -0.011 (0.006)
Prior earnings	0.000* (0.000)	0.000* (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
High education	-0.025 (0.057)		0.053* (0.072)	-0.015 (0.066)	-0.024 (0.060)
Low education	0.228** (0.107)		0.320** (0.157)	0.241** (0.093)	0.296*** (0.085)
Firm size	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Number of peer births	-0.036 (0.022)	-0.046 (0.062)	-0.043 (0.030)	-0.033 (0.025)	-0.034 (0.024)
Peer group size	0.000** (0.000)	0.000 (0.000)	0.001** (0.000)	0.000* (0.000)	0.000*** (0.000)
West Germany	0.030 (0.046)	0.198* (0.108)	0.075* (0.076)	-0.009 (0.058)	0.046* (0.047)
Constant	0.561*** (0.036)	0.525*** (0.074)	0.526*** (0.054)	0.560*** (0.048)	0.564*** (0.037)
N	488	86	239	293	441
R-sq	0.037	0.039	0.074	0.062	0.076
Relative likelihood	1.00	1.04	1.39	1.25	1.12

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Source: IAB Linked Employer-Employee Panel (LIAB) LM 1993-2010. Characteristics are measured at the time of childbirth, except where otherwise specified.

Table A5: First stage reform effect estimation including trends in date of childbirth

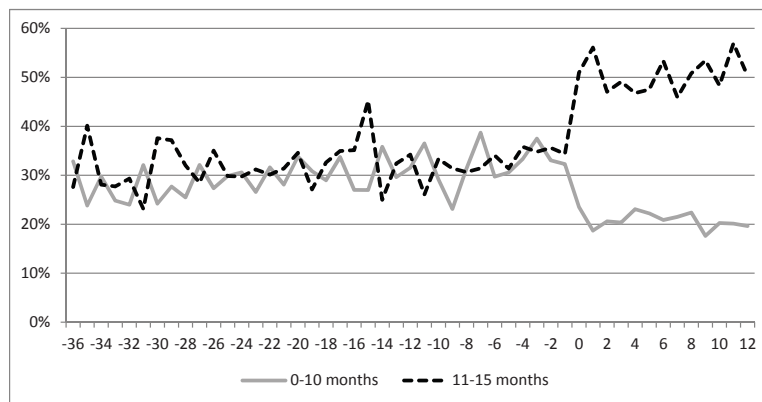
	Without trend		With trend	
	Coef.	SE	Coef.	SE
Our sample	0.198***	(0.043)	0.185***	(0.048)
			0.142	(0.102)
			0.104	(0.107)
Full sample	0.177***	(0.017)	0.164***	(0.019)
			0.178***	(0.040)
			0.157***	(0.042)
Control variables	No	Yes	No	Yes

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

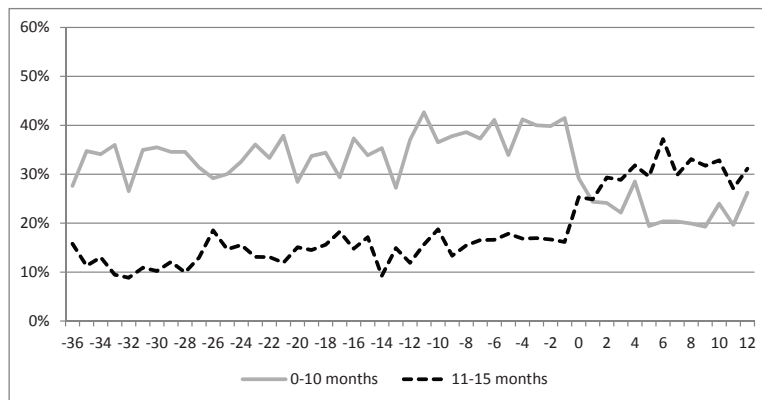
Notes: 'Full sample' refers to all medium- and high-income women in the LIAB who gave birth between July 2006 and June 2007. Regressions with trend include separate trends on both sides of the cutoff.

Source: IAB Linked Employer-Employee Panel (LIAB) LM 1993-2010.

Figure A3: Fraction of mothers who stays at home for 0-10 months and 11-15 months after childbirth by region



(a) East Germany



(b) West Germany

Notes: Fractions are calculated out of all women in the sample who gave birth in a given month, in distance to the parental leave benefit reform (January 2007 = 0).

Source: IAB Linked Employer-Employee Panel (LIAB) LM 1993-2010.

Table A6: Comparison of peer mothers giving birth before and after the parental leave benefit reform

Variable	Before reform					After reform					t	
	Median	Mean	SD	N		Median	Mean	SD	N	Difference		DE(Diff)
Return within 10 months		0.56	0.50	205			0.75	0.44	283	-0.18	0.04	<b>-4.26</b>
Age at childbirth	33	32.55	4.29	205		32	32.27	4.31	283	0.28	0.39	0.72
Monthly labor income	2654	2888.59	929.46	205		2716	2929.80	942.36	283	-41.21	85.74	-0.48
Days in employment	2980	3382.79	1632.08	205		3007	3278.12	1547.76	283	104.67	146.49	0.71
Days in unemployment	0	100.53	265.37	205		0	59.96	185.71	283	40.57	21.57	1.88
Part-time employed		0.11	0.31	205			0.16	0.37	283	-0.05	0.03	-1.68
High education ( $\geq$ college)		0.20	0.40	205			0.16	0.37	283	0.03	0.04	0.92
Low education ( $<$ highschool)		0.04	0.19	205			0.02	0.13	283	0.02	0.02	1.36
Migration background		0.05	0.22	205			0.06	0.23	283	-0.01	0.02	-0.38
Number of children	1	1.15	0.39	205		1	1.22	0.45	283	-0.06	0.04	-1.69
Group size	20	28.22	27.90	205		25	66.62	154.21	281	-38.40	9.40	<b>-4.08</b>
Births in reform window	1	1.22	0.53	165		1	1.33	0.90	213	-0.10	0.07	-1.41
Firm size	448	1741.27	3273.80	205		385	1030.48	2084.38	283	710.79	260.07	<b>2.73</b>
Old firm ( $>$ 10 years)		0.92	0.28	205			0.89	0.31	283	0.02	0.03	0.87
Standard wages in firm		0.36	0.48	168			0.48	0.50	230	-0.12	0.05	<b>-2.40</b>
Churn rate in firm		0.04	0.13	195			0.03	0.03	261	0.01	0.01	0.80
Share of women in firm	0.40	0.44	0.27	201		0.47	0.49	0.26	274	-0.05	0.02	<b>-2.23</b>
Share of temporary workers in firm	0.04	0.09	0.14	201		0.04	0.08	0.11	274	0.01	0.01	0.87
District population density	397	910.86	1061.40	196		399	962.94	1076.73	273	-52.08	99.97	-0.52
District unemployment rate	11.0	11.95	4.78	196		9.7	10.47	4.79	273	1.48	0.45	<b>3.30</b>

Source: IAB Linked Employer-Employee Panel (LIAB) LM 1993-2010. Characteristics are measured at the time of childbirth, except where otherwise specified.

Table A7: Comparison of mothers whose peers gave birth before and after the parental leave benefit reform

Variable	Peer birth(s) before reform				Peer birth(s) after reform				t		
	Median	Mean	SD	N	Median	Mean	SD	N		Difference	DE(Diff)
Return within 10 months		0.75	0.43	472		0.82	0.39	868	-0.07	0.02	<b>-2.83</b>
Age at childbirth	32	32.14	4.31	472	32	32.13	4.23	868	0.01	0.24	0.02
Monthly labor income	2686	2865.14	1145.57	472	2730	2830.03	1177.36	868	35.11	66.16	0.53
Days in employment	2933	3176.90	1595.28	472	3072	3250.32	1537.27	868	-73.42	90.08	-0.82
Days in firm	2146	2525.32	1782.26	472	1158	1906.91	1745.04	868	618.41	101.18	<b>6.11</b>
Days in unemployment	0	98.31	225.45	472	0	84.46	245.03	868	13.85	13.30	1.04
High education ( $\geq$ college)		0.24	0.42	472		0.20	0.40	868	0.04	0.02	1.61
Low education ( $<$ college)		0.03	0.17	472		0.03	0.18	868	0.00	0.01	-0.49
Migration background		0.05	0.22	472		0.06	0.24	868	-0.01	0.01	-1.05
Number of children	1	1.21	0.41	472	1	1.25	0.46	868	-0.04	0.02	-1.64
Date of childbirth (Jan 01, 2007 = 0)	580	606.76	264.47	472	617	628.67	267.06	868	-21.91	15.18	-1.44
Group size	28	40.04	34.01	472	49	197.86	310.59	864	-157.83	10.68	<b>-14.78</b>
Number of peer mothers	1	1.33	0.76	472	1	2.86	2.69	868	-1.53	0.10	<b>-15.67</b>
Peer births 07/04-06/06	3	3.12	2.69	472	1	2.37	2.89	868	0.74	0.16	<b>4.70</b>
Firm size	582	1891.36	3097.89	472	620	1300.21	1732.87	868	591.15	154.25	<b>3.83</b>
Old firm ( $>$ 10 years)		0.91	0.28	472		0.87	0.33	868	0.04	0.02	<b>2.38</b>
Standard wages in firm		0.38	0.49	402		0.53	0.50	715	-0.15	0.03	<b>-5.06</b>
Churn rate in firm		0.04	0.05	449		0.04	0.04	777	0.00	0.00	-0.70
Share of women in firm	0.44	0.45	0.25	465	1	0.53	0.22	829	-0.09	0.01	<b>-6.15</b>
Share of temporary workers in firm	0.05	0.14	0.19	461	0	0.09	0.11	823	0.05	0.01	<b>5.00</b>
District childcare coverage	19	24.29	16.52	454	20	22.93	14.42	845	1.36	0.92	1.47
District population density	7	13.47	12.80	454	9	12.84	11.64	845	0.63	0.72	0.87
District unemployment rate	8	9.21	4.02	454	8	8.57	4.31	845	0.64	0.24	<b>2.65</b>

Source: IAB Linked Employer-Employee Panel (LIAB) LM 1993-2010. Characteristics are measured at the time of childbirth, except where otherwise specified.



Table A8: Peer group size, entries, exits, and number of births

Group size	Control group				Treatment group				t
	Median	Mean	SD	N	Median	Mean	SD	N	
Women only, 07/2006 - 06/2007	17	25.1	24.1	165	20	30.3	37.7	213	-1.6
Women only, 07/2007 - 06/2008	16	24.7	24.5	165	20	35.4	80.6	213	-1.8
Women only, 07/2008 - 06/2009	17	24.4	26.2	165	20	35.2	81.9	213	-1.8
Women and men, 07/2006 - 06/2007	38	56.2	66.0	165	41	69.6	97.7	213	-1.6
Women and men, 07/2007 - 06/2008	39	56.8	67.2	165	41	77.8	153.2	213	-1.8
Women and men, 07/2008 - 06/2009	38	55.8	68.4	165	42	78.2	153.2	213	-1.9
Women or men joining the peer group									
Entries peer group, 07/2007 - 06/2008	4	7.4	11.1	165	4	15.2	85.2	213	-1.3
Entries peer group, 07/2007 - 06/2009	8	13.6	20.9	165	9	25.2	106.2	213	-1.6
Women or men leaving the peer group									
Exits peer group, 07/2007 - 06/2008	3	6.2	20.4	165	3	7.2	22.6	213	-0.5
Exits peer group, 07/2007 - 06/2009	4	13.9	29.3	165	4	15.7	37.2	213	-0.5
Women joining the peer group									
July 2007 - June 2008	3	5.1	7.2	165	3	10.2	56.8	213	-1.3
July 2007 - June 2009	5	9.8	14.7	165	6	17.2	72.3	213	-1.5
Women leaving the peer group									
July 2007 - June 2008	1	2.7	4.8	165	1	3.7	8.3	213	-1.5
July 2007 - June 2009	2	5.5	7.9	165	2	7.2	13.6	213	-1.5
Number of births in peer group									
July 2007 - June 2008	1	1.4	1.4	165	1	1.7	4.3	213	-1.0
July 2007 - June 2009	2	2.5	2.2	165	2	3.3	8.0	213	-1.4

Source: IAB Linked Employer-Employee Panel (LIAB) LM 1993-2010.