Peer Effects in Parental Leave Decisions

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Preliminary version – Please do not quote.

Abstract

The paper analyzes to what extent parental leave decisions of mothers with young children depend on the decisions made by their coworkers. In particular, we study peer effects at the workplace in the context of maternal leave behavior in Germany. Peer effects, which are defined as indirect effects of the behavior of a social reference group on individual outcomes, can result from a preference for conformity to social norms or the revelation of information about the consequences of a certain behavior.

The identification of peer effects bears various challenges due to correlated characteristics within social groups and endogenous group membership. We overcome these challenges by exploiting quasi-random variation in the costs of parental leave in a narrow window around a cutoff date, induced by a parental leave benefit reform in January 2007, which encourages high-income mothers to remain at home in the first 12 months after childbirth. Administrative linked employer-employee panel data (LIAB) enables us to assign a peer group to all individuals who work in the same establishment and occupational group. We limit our sample to mothers with above-median previous labor income, who gave birth after the reform. 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.

While there is a growing literature on peer effects, no study has yet looked at peer effects in the context of maternal leave decisions. We argue, however, that mothers with young children are particularly susceptible to peer behavior at the workplace due to the high social as well as career-related uncertainty that young mothers face. Our results suggest that mothers' decisions on the length of parental leave are significantly influenced by their coworkers' decisions. We find that a mother is 31 percentage points more likely to stay at home in first year if her peer mother(s) decide to do so as a response to the parental leave benefit reform.

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

JEL: C31, J22, D04

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1 Introduction

Individuals do not exist in isolation but are embedded within networks of relationships, such as families, coworkers, neighbors, friendships or socio-economic groups. While there is a long tradition in sociology and anthropology focusing on the importance of social structure, norms and culture, economists have long neglected social influences on individual behavior and focused on explaining individual behavior with economic incentives. One economic outcome variable that has been studied quite comprehensively is female and maternal labor supply. There is a large literature documenting trends in female labor force participation rates and hours worked over time and differences across countries.[1]. Numerous studies explain part of these changes over time or variation between countries based on standard economic models and attribute them to differences in financial incentives caused by institutional diversity. However, institutional differences commonly cannot explain all divergence in the labor supply behavior of women across countries.[2]

More recently, a growing strand of the literature on female labor supply has suggested alternative or complementary 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. Other studies have analyzed the influence of social interaction within geographic neighborhoods (Weinberg *et al.*, 2004, e.g.) and (Fogli and Veldkamp, 2011) or family networks (Del Boca *et al.*, 2000; Dahl *et al.*, 2014; Neumark and Postlewaite, 1998, e.g). In this study, we want to focus on the labor supply decisions of mothers with young children and how these are affected by peers at the workplace. We believe that this research question is relevant for several reasons. First, labor supply decisions of mothers with young children are subject to a lively public debate in many countries. Some people argue that mothers should stay at home and care for their children in the first years, others emphasize the importance of mothers' employment and their ability to earn their own living. Thus, mothers face a period of high uncertainty during the first years of life of their children and therefore labor supply decisions are likely to be particularly affected by the decisions of mothers' peers. Second, while many different social networks are important for these decisions, we believe that social networks at the workplace play a particularly important role. The workplace facilitates

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[1]Classical references for the evolution of womens' employment in the US are, among others, Goldin (1990) and Blau and Kahn (2006) For Germany, time trends in female employment patterns are documented e.g. in Fitzenberger and Wunderlich (2004). Cross-country differences are documented e.g. by Bick and Fuchs-Schündeln (2014) or Blundell *et al.* (2013)

[2]For example, the paper by Bick and Fuchs-Schündeln (2014) 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) that compares two culturally very similar countries such as Austria and West Germany that share many institutions but differ in the tax treatment of the family and the parental leave scheme shows that differences in financial incentives caused by these two institutions explain 20 percent of the total difference in the full time employment rate of mothers with children under age 10 in both countries. Attanasio *et al.* (2008) document the importance of childcare cost, while Fernandez and Wong (2014) stress that changes in divorce rates are central to explain the devolvement of female employment. For a more detailed summary of potential explanations derived in quantitative macro models, see Bick and Fuchs-Schündeln (2014).

the formation of social ties and thereby the transmission of social norms and influences and information transmission.

Social interaction effects can be generated through various channels and can be explained by different sociological, anthropological and economic theories, which will not be discussed here. It is our goal to identify the indirect effect of the behavior of a social reference group on individual outcomes, which will be referred to as peer effect in the following. Peer effects can result from a preference for conformity to the behavior of a certain social group, or the revelation of important information about the reactions to, and consequences of, a certain behavior. We are not able to distinguish between those channels.

The identification of social interaction effects in empirical analyses is challenging (see Manski, 1993; Blume *et al.*, 2010, for an overview). Recent papers (e.g. Dahl *et al.*, 2014; Brown, 2013) have suggested using policy reforms as instruments in order to address the identification challenges. We follow this approach and identify social interaction effects in the labor supply of mothers with young children using the exogenous variation that is introduced by the reform of parental leave benefit (*Elterngeld*) in 2007 in Germany.

The paper is organized as follows. In the next section we describe some stylized facts on female employment in Germany and discuss the institutional details of the 2007 parental leave reform that we will 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 descriptive statistics. Section 5 shows and discusses the results of the empirical analysis, while section 6 concludes.

2   Institutional Setting and Stylized Facts

Female employment is of prevailing policy relevance in Germany, because low fertility rates and an increasing old-age dependency ratio are posing a growing imbalance to public finances. Employment rates of mothers have been traditionally relatively low in West Germany, compared to other European countries such as France, UK or the Nordic countries. However, in the last decade, the employment rate of mothers with children above the age of one has been increasing (see Figure 1). For example, the employment rate of mothers with a child aged 2 to 3 years has been 42 percent in 2006 and has increased up to 54 percent until the year 2012. Also the employment rate of mothers with children aged 1 to 2 years or 3 to 6 years has been increasing. Equivalently, the mean duration of employment interruption after giving child birth has also been decreasing in the period from 2004 to 2010 (see Wrohlich *et al.*, 2012). At the same time, the employment rate of mothers with children below the age of one has been decreasing (see Figure 1).

Several policy reforms have taken place in the last decade, which most likely have affected maternal employment. In 2007, a new parental leave benefit (*Elterngeld*) has been introduced. This new benefit replaced an old scheme (*Erziehungsgeld*) that was means-tested and much less

Figure 1: Employment rate of mothers by age of the youngest child
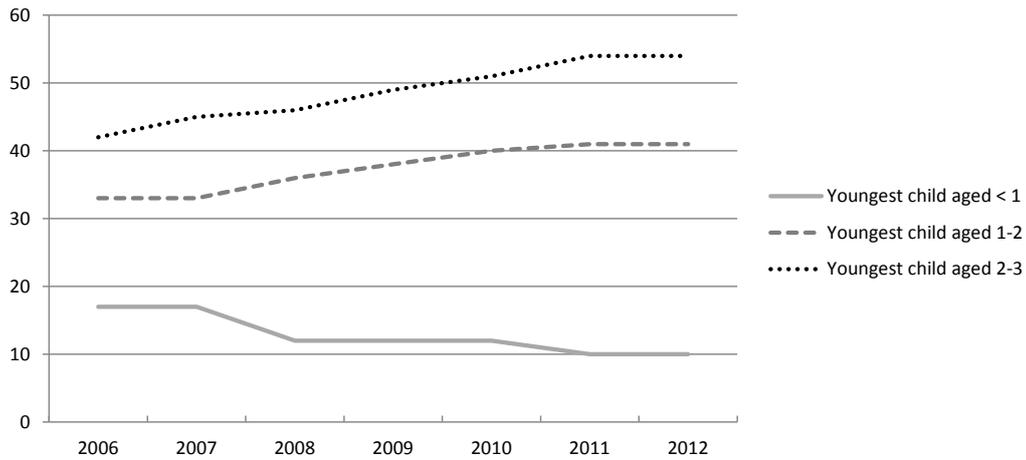
Source: Bundesministerium für Familie, Senioren, Frauen und Jugend, (2012).

generous, but paid for a longer period of time (24 months). The new *Elterngeld* is paid only for a period of 12 months[3]. It is not means-tested on household income and the amount of the benefit depends on earnings prior to birth, replacing 67% of previous net earnings but not exceeding 1,800 euro per month. The minimum amount of the *Elterngeld* is 300 Euro per month, which is equivalent to the monthly benefit payed under the previous *Erziehungsgeld*. The parental leave reform thus sets incentives to shorten employment interruptions in particular for mothers from low-income families who were eligible to the longer benefit under the old scheme. On the other hand, the reform sets incentives, in particular for high-income mothers, to stay at home during the first 12-14 months after childbirth through the high replacement rate. The change in incentives is visualized for exemplary mothers in Figure 2, which shows the amount of parental benefits payed to mothers with a gross labor income of 3000 Euro and 1000 Euro respectively, before and after the introduction of the reform.

Based on different methods and data-sets, several studies have analyzed the causal relationship between the parental leave benefit reform of 2007 and the development of mothers' employment over time. As expected, Kluve and Tamm (2013); Kluve and Schmitz (2014) and Geyer, Haan and Wrohlich (2015) find that the probability that mothers return to work in the first year after giving birth has declined in particular for high-income mothers as a result of the introduction of the *Elterngeld*. Furthermore, Geyer, Haan and Wrohlich (2015); ? and Kluve and Schmitz (2014) find that employment of mothers generally increases after the first 12 months compared to mothers who gave birth before the reform.

Kluve and Schmitz (2014) analyze not only the effect of the introduction of the *Elterngeld* on labor supply of mothers in the first and second year after giving birth but also in the third to

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[3]In case that both parents share parental leave, the total length increases to 14 months.

Figure 2: Benefits payed before and after the reform for exemplary mothers
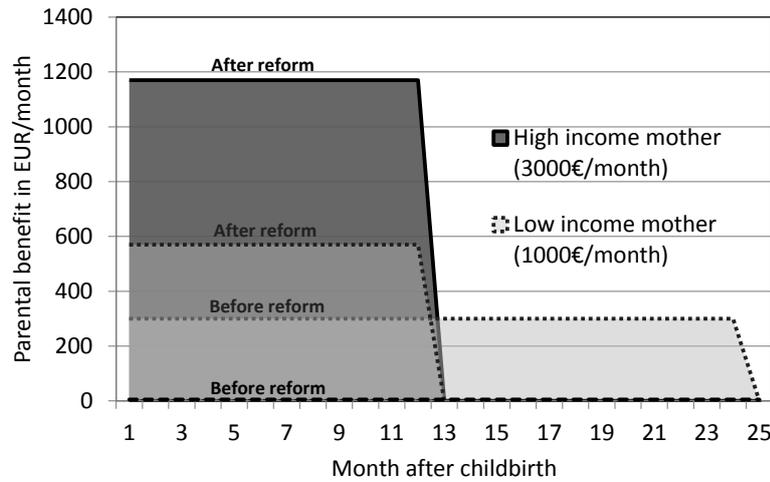
Hypothetical benefits that can be received by married mothers with a gross income of 3000 Euro and 1000 Euro per month respectively, based on the online benefit calculator of the Bundesministerium für Familie, Senioren, Frauen und Jugend.

fifth year after childbirth. They use an RDD framework in order to identify the causal effect of the *Elterngeld* on mothers' employment decisions. 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*. Since financial incentives did not change in the third or fourth year after giving birth, the behavioral response cannot be explained by standard economic theories. As a possible explanation, the authors suggest that the new parental leave benefit changed the social norms. Bergemann and Riphahn (2015) also analyzes the short and medium run maternal employment effects of the 2007 parental leave reform. They show 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 summarize the empirical findings concerning the 2007 parental leave reform in Germany so far: The reform induced mothers to work less in the first year, but return more likely in the second year after giving birth. Moreover, empirical findings show that the employment of mothers has changed stronger than can be explained by financial incentives only. Some authors speculate suggest that the reform has changed social norms concerning labor supply of mothers with young children. A formal test of this hypothesis, however, is missing so far.

In the next section of the paper, we outline how we use the introduction of the *Elterngeld* as an instrument for the identification of social interaction effects with respect to labor supply decisions of mothers with young children.

3 Methodological Approach

3.1 Identifying Peer Effects

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 are aiming 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 characteristics of the group. The third explanation comes from correlated effects, which means that individuals belonging to the same group tend to behave similarly because they share unobserved characteristics. Correlated effects can be distinguished into two challenges to the identification of peer effects: endogenous group formation and correlated unobservable characteristics due to common shocks.

The specific challenges associated with the identification of peer effects in the context of labor supply of mothers with young children can be summarized as follows:

- Peer effects are difficult to identify in the case of **correlated effects**, which are ensued by unobserved variables that are correlated among women who belong to the same social group. Often it cannot be excluded that contextual factors, such as workplace conditions, affect the decisions of employees in a company alike. Imagine, for instance, a manager who is openly supporting women who want go on parental leave. This would yield longer average duration of leave spells within a group, which could be incorrectly interpreted as a peer effect.

- The **endogeneity of social networks** due to sorting into an occupation or firm based on unobservable preferences and firm characteristics poses another challenge for identification. For example, if women with strong preferences for leisure sort into specific firms and occupations that signal family-friendliness, and are more likely to exhibit long maternal leave spells, peer effects are likely to be overestimated.

- Another challenge associated with the identification of social interaction effects stems from the **simultaneity** of interactions within a social group. It is thus not possible to identify whether an action is the cause or the effect of peer influence.

Several recent papers focus on the theoretical identification of interaction effects in social groups and networks. For example, Blume, Brock, Durlauf and Ioannides (2010) address the

problems of reflection, self-selection into social groups and correlated unobservable group characteristics, and discuss the identification of linear, spatial and discrete choice models with social interaction. Furthermore, Brock and Durlauf (2001), Bramoullé, Djebbari and Fortin (2009), and Blume *et al.* (2013) formulate conditions under which economic models with social interactions are identified. Most importantly, the researcher must know the structure of the social network and individual data on the behavior of the members of the social network must be available. 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 hardly available and in particular are not available for German mothers. Consequently, other identification strategies need to be found.

Given these identification challenges, empirical studies have employed sophisticated empirical strategies to identify peer effects. There are several studies that analyze various kinds of social interaction in the context of female labor supply using different methods. Early work on peer effects attempts to account for the identification challenges by explicitly including social interaction into structural labor supply models. For example, Neumark and Postlewaite (1998) introduce relative income concerns into women's utility functions and test whether labor supply decisions depend on the employment and income of sisters and sisters-in-law. The problem of self-selection into social groups is solved by looking at exogenously given peers. They find that relative income concerns can, to some extent, help to explain the observed increases in female labor force participation. Another example is given by Woittiez and Kapteyn (1998), who use survey data of married females in Dutch households to construct reference groups based on age and education questions. They find that habit formation and preference independence contribute significantly to the explanation of female labor supply in an extended neoclassical model which explicitly includes labor supply decisions of the reference group and lagged own labor supply.

Weinberg *et al.* (2004) find that social characteristics of a neighborhood are an important determinant of employment status. They account for endogenous selection into neighborhoods by using longitudinal data and controlling for a large number of variables that explain neighborhood heterogeneity. The authors show that specifications that do not control for neighborhood selection on the basis of time-invariant unobserved individual characteristics substantially overstate the social effects of neighborhoods. Maurin and Moschion (2009) also analyze neighborhood effects using the sex composition of the neighbors children as an instrument to account for non-random selection into neighborhoods that might be correlated with labor supply decisions. They show that there are positive and significant neighborhood peer effects using a French data set in which sampling units are not individuals but groups of 20 adjacent households, from which all individuals are interviewed.

Social interaction can affect individual behavior through various social groups or networks. While other research focusses on geographic neighborhoods (e.g. Weinberg *et al.*, 2004) or family networks (e.g. Del Boca *et al.*, 2000; Dahl *et al.*, 2014; Neumark and Postlewaite, 1998), we will focus on the workplace as the relevant social network. This is based on the assumption that

peers at the workplace matter for decisions regarding employment behavior. There are several recent studies that suggest that peer effects at the workplace play an important role. A study by Hesselius (2009) shows that peer-effects exist also in the context of absence from work; Mas and Moretti (2009) focus on peer effects at the workplace in the context of productivity of checkers for a large grocery chain; and Cornelissen, Dustmann and Schönberg (2013) use linked employer-employee data in order to estimate the effect of the long-term quality of a workers peers (measured by the average wage fixed effect of coworkers in the same firm and occupation) on workers wage.

Recently, the use of natural experimental approaches became increasingly popular to identify peer effects. Following this approach, we will use a reduced-form 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 problems of correlated effects, reflection and endogenous group membership are avoided by using a quasi-natural experiment exploiting the variation in the costs of paternity leave induced by a family policy reform. They find that coworkers and brothers are substantially more likely to take paternity leave if their peer was induced to take up leave by the reform. An analysis of the channels of social interaction suggests that information transmission about costs and benefits is most likely to drive the peer effects. Furthermore, the authors find that peer effects are likely to generate snow-ball effects over time, i.e. the effects on paternity leave take-up are magnified over time due to an increasing share of fathers affected by the reform, who in turn interact with other fathers and so on.

3.2 Empirical Strategy

To overcome the identification challenges mentioned above, we employ an instrumental variable research design exploiting the quasi-random variation in maternal leave spells due to the introduction of the new *Elterngeld* in January 2007. The *Elterngeld* increased the financial incentives for low-income mothers to return to work in the second year after childbirth. At the same time the reform encourages in particular high-income mothers to stay at home in the first year after childbirth and benefit from the high income replacement rates under the new *Elterngeld*. This creates a discontinuity in the fractions of working mothers in the first and second year after childbirth. We use this discontinuity in an instrumental variable research design to compare coworkers of mothers who gave birth before the cutoff with coworkers of mothers who gave birth after the cutoff. To do so, the sample is divided into mothers who gave birth in a one-year window around the policy change, referred to as peers, and their coworkers who gave birth after their peers and after the reform. Our final sample consists therefore only of mothers who gave birth at least six months after the introduction of the reform. Treatment and comparison group differ only in whether their peers gave birth before or after the reform.

The estimation of the peer effect is implemented using a two-stage estimator (2SLS), where the reform-induced discontinuity is employed as an instrument to estimate the effect of peer mothers on their coworkers. The problem can be described as a system of two simultaneous

equations, where the dependent variables of the first and second equation are the maternal leave decisions of peer mothers (y_{1g}) and their coworkers (y_{2g}) respectively. Our outcome of interest is a binary variable that equals to one if the mother does not return to work within the first 10 months after childbirth for above-median income mothers. The running variable x is defined as the date of birth of the peer mothers' child, normalized to $x_0 = 0$ at the day of the policy reform. The instrumental variable enters the equations in form of a binary indicator z_{1g} that is equal to one if a child is born after the reform ($x \geq x_0$):

$$(1) \text{ First stage: } y_{1g} = \alpha_1 + \lambda z_{1g} + \beta_1 w_g + \epsilon_{1g}$$

$$(2) \text{ Objective equation: } y_{2g} = \alpha_2 + \delta \bar{y}_{1g} + \beta_2 w_g + \epsilon_{2g}$$

Since the policy reform z_{1g} is exogenous λ can be identified using a linear regression of binary maternal leave decisions y_{1g} on the treatment variable z_{1g}. To balance observable differences between treated and non-treated mothers, we can include covariates w_g in both equations. The results from the first stage can be used to estimate the peer effect δ by estimating π from the reduced form equation and scaling by λ.

$$(3) \text{ Reduced form: } y_{2g} = \gamma_0 + \pi z_{1g} + \beta_3 w_g + u_g$$

In other words, the two-stage estimator 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 at the date of the policy change. The estimated treatment effect is the LATE (local average treatment effect) of the reform induced employment interruption decision of the peer mother on her coworker's maternal leave decision.

This 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. 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, the estimated effect can be attributed solely to the influence of peer mothers.

Several identifying assumptions are required for the internal validity of the instrumental variable research design. A key assumption is that individuals are not able to control the treatment assignment variable, i.e. the date of birth of their children. 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 around the first of January 2007.

A challenge is posed by the fact that one individual may be affected by several peers. It is therefore necessary to specify a window around the cutoff as well as a treatment assignment

variable in case of more than one peer in the window around the cutoff. For large peer groups, our research design has little power because if the window increases, the variation in exposure decreases. One specification that sidesteps these issues is to look at only those peer groups where there is only a single peer mother who gave birth in the reform window. This has the disadvantage that it results in a very small sample size. In our baseline sample specification we include peer groups where there are several peer mothers in the reform window, but only if they gave birth on the same side of the reform. Alternative specifications are possible, for example a coworker can be affected by the number of children born after the cutoff.

4 Data

The empirical analysis will be based on the administrative linked employer-employee data set (LIAB) provided by the Institute for Employment Research (IAB) at the German Federal Employment Agency (BA).[4] The LIAB includes individual employment histories generated from administrative data provided by firms and social security data from 1993 to 2011. Within this data set, the individual employment histories are matched with the annual IAB Establishment Panel data, which includes detailed firm characteristics such as developments in employment (production, turnover, working hours, investment, capacity utilization), demand for personnel and labor expectations (vacancies, open positions, fluctuations, establishment employment policies), technology and organization and determinants of productivity in addition to firm and branch size, industry, as well as regional characteristics.

When estimating peer effects, the challenge is to find a data set that contains micro data on an individual's entire relevant social network. The researcher has to know (or assume to know) the relevant reference group. This makes it difficult to use the available household survey data (e.g. SOEP) for the analysis of peer effects, as those generally sample disconnected units instead of entire networks of individuals. Moreover micro data on the behavior of individuals within the relevant reference group need to be available. For Germany, not many such data sets exist.

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 all 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, Pink *et al.* (2013) analyze social interaction effects related to fertility decisions. They analyze whether the pregnancy of a coworker affects the pregnancy of female colleagues and conclude that there is a significant peer effect. Cornelissen *et al.* (2013) use the LIAB to analyze peer effects in wages.

A challenge is posed by the fact that maternal leave times are often not registered as such in the LIAB employment history data. Furthermore, the date of birth of children is not recorded. However, it is possible to identify maternal leaves and childbirths with sufficient accuracy by

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[4]Due to restrictions concerning data protection legislation the LIAB data can only be analyzed via on-site use in the research data center of the institute of employment research (IAB).

using the starting point and duration of absence from work to estimate the date of childbirth (Schönberg, 2009). Employment interruptions of at least 14 weeks among women between the age of 18 and 40 are likely to be maternal leave spells due to the obligatory maternity leave of 6 weeks before and 8 weeks after childbirth. With the help of the IAB[5], we were able to identify 121,804 maternal leave spells and birth events from 2000 to 2010.

A graph of the distribution of leave spells of mothers whose children were born in 2006 and 2007 (Figure 1) reveals that there are peaks after the mandatory maternity leave period (2 months) and after around 12 months. It can also be seen that there was a shift from earlier returns to around 12 months from 2006 to 2007, which is due to the introduction of the *Elterngeld*.

Figure 1: Distribution of parental leave spells (number of full months in which mother stays at home after childbirth)
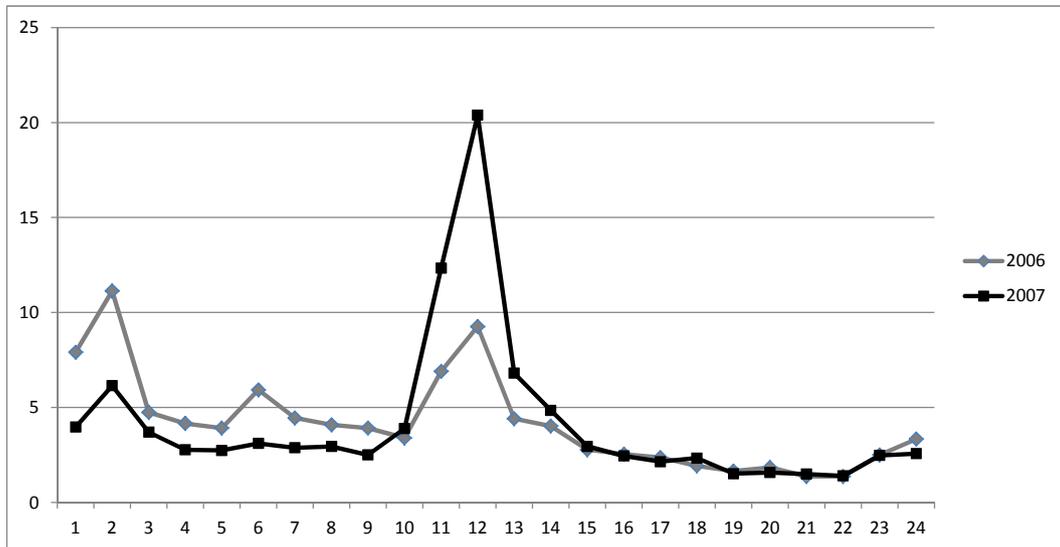
Note: the figure is based on our own analysis using the LIAB. Fractions are calculated out of all women in the sample who gave birth in a given year.

Given our identification strategy, we restrict the sample to those women who gave birth within a six-month (July 2006 - June 2007) window around the reform[6], and their female coworkers who gave birth thereafter and belong to the same peer group formed by occupation (defined by occupational main-groups according to KldB 1988) and firm identifier. For small firms, peer groups are formed either only by the firm identifier if the firm has up to 150 employees. When defined like this, the median number of mothers in a peer group is 16 (see Table 1), with a mean of 1.7 births per group in the six-month window around the reform. Note that the median and mean firm size is much larger because we observe several peer groups per firm.

Another limitation arises with the right-censoring of the observed maternal leave spells. We restrict the sample to those mothers whose employment interruptions are fully observed. When

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[5]We thank Dana Müller and Katharina Strauch for their help.

[6]The specification of the reform-window is subject to change based on future empirical and theoretical analysis.

Table 1: Average and median peer group size and firm size in sample (above median income only)

	Median	Mean	N
Peer group	16	28.4	759
Firm	335	1071.9	624

looking at the fraction of mothers returning within the first year after birth, we have to drop all observations of birth events that occur after December 31, 2009 because our observation periods ends on December 31, 2010. In the baseline specification of our empirical analysis we include 2912 mothers giving birth between July 2008 and December 2009 and their respective peers, who gave birth between July 2006 and June 2007.

5   Preliminary results

For our identification strategy to be valid, one requirement is that the parental leave reform did in fact have a significant effect on maternal leave behavior. Geyer *et al.* (2015) and Kluve and Tamm (2013) have shown that the *Elterngeld* reform significantly reduced labor supply of mothers in the first year after giving birth. The effects are larger for women with higher income and women who were employed before childbirth. In contrast to previous literature, we estimate the effect of the *Elterngeld* reform using administrative labor market data, focussing solely on employed women with above median average gross labor income. Our outcome variable of interest is whether a mother stays at home during the first 10 months after giving birth.

It can be seen in Figure 2 that the fraction of women staying home during the first 10 months increased steeply from about 60% to 80% around the reform date (01/2007= 0) for the group of high-income women, who benefit most from the new *Elterngeld* policy. For mothers with a below-median income, the fraction increased to a lesser extent.

Assuming that within a small window around the introduction of the reform, the reform is as good as randomly assigned, we can estimate the causal effect of the *Elterngeld* reform using an indicator whether or not a child was born after the reform as independent variable in a simple linear or probit regression model. A comparison of characteristics of women who gave birth before and after the reform (Table 4) supports the assumption that treatment is as good as randomly assigned in a 6 month window around the introduction of the reform. Our results suggests that the fraction of mothers who do not return to work within 10 months after giving birth increased by about 21 percentage points among high income mothers due to the reform (see Table 2). In comparison to the existing literature, we find larger effects of the reform, which is due to the specific sample selection of our analysis.

The estimation of the peer effect is done in two steps. First, we estimate the reduced form effect of having a peer mother who gave birth after the reform on coworker outcomes. The results can be interpreted as intention to treat (ITT) effects. We find positive and significant

reduced form effects of about 7 percentage points, suggesting that whether having a peer who gave birth before or after the reform has an impact on coworkers leave behavior (see Table 3, column (1) and (2)). The first stage effect of the reform on peer behavior is even larger for our peer effect analysis due to the specification of our sample, which excludes very small and very large firms where there are either too few or too many births per peer group in the reform window.

In a second step we scale the intention to treat effect up by our first stage effect. This step is necessary because not all peer mothers react to the reform. We estimate the pure peer effect using two-stage least squares and we find significant peer effects of about 31 percentage points (see Table 3, column (1) and (2)). In other words, a mother is 31 percentage points more likely to stay at home in first year if her peer mothers decide to do so as a response to the *Elterngeld* reform. Note that in cases where a mother has several peers who gave birth in the six month 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.

Table 2: Preliminary 2SLS results

	(1)	(2)	East	West	(3)	(4)	(5)
Peer effect	0.315***	0.295**	0.134	0.373**	0.272*	0.291**	0.313**
	(0.121)	(0.124)	(0.234)	(0.129)	(0.148)	(0.15)	(0.11)
First stage	0.209***	0.206***	0.209**	0.21***			0.249***
	(0.059)	(0.057)	(0.099)	(0.067)			(0.061)
Reduced form	0.066***	0.061**					
	(0.025)	(0.025)					
Controls	no	yes	yes	yes	yes	yes	yes
Donut	no	no	no	no	no	no	yes
Linear trend	yes	yes	yes	yes	yes	yes	yes
N	1363	1363	423	940	1028	1055	1451

Dependent variable: $y_c = 1$ if mother does not return to work within 10 months after childbirth. Control variables include the age of the mother, pre-birth labor income, work experience overall and in current firm, a dummy for West Germany, migration status and education levels in the baseline specification (2) - (5). Specification (3) includes additional firm characteristics and specification (4) includes only first births. In specification (5), peer births within a 30-day donut around the introduction of the reform are dropped from the sample. Source: LIAB. Sample of employed mothers at the time of childbirth with above median gross labor income. Standard errors in parantheses.

The estimated peer effects can 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 subpopulation of those coworkers whose peers stay at home during the first 10 months after childbirth if their child was born after the reform, and

those who return to work within 10 months if their child is 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 firms and occupations where mothers respond to the parental leave benefit reform, which is in line with the extensive magnitude of the peer effects we find.

A crucial underlying assumption of these results is the comparability of treated and non-treated individuals. Given our assumption that 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. A simple mean comparison of treated and non-treated coworkers (Table 7) to significant differences in certain variables, namely the number of days employed in the firm, number of children and date of childbirth. To correct for imbalances and thereby improve precision of our estimates, we include a number of covariates that potentially affect maternal leave decisions in our regressions. However, including covariates does not change the magnitude or significance of our results considerably, as displayed in columns (2) to (5) of Table 3. Not only leave taking behavior can entail peer effects. There can also be peer effects in fertility among mothers who give birth after the parental leave benefit reform. First note that a general increase in fertility due to the reform does not threaten our identification strategy as mothers who give birth after January 2007 are affected equally by such a change. Furthermore, we argue that there is no considerable change in fertility of mothers in a six months window around the reform as the reform was only formally announced in September 2006.

The results are robust to several specifications. In column (3) we include additional firm characteristics as covariates to control for potential differences between treated and non-treated coworkers. In column (4) we display the results for a sample of mothers who gave birth to their first child only. We exclude observations of peer births 30 days before and after the introduction of the *Elterngeld* reform in column (5). We do however find differences in peer effects for East and West Germany. While there is a large positive peer effect for West German mothers, we cannot find the same effect for East German mothers.

In addition to the baseline analysis, we repeat the analysis using different specifications of peer groups and the running variable. For example, peer groups can be formed by firms only, based on the assumption that social interactions take place across occupations in small and medium-size firms. Another possibility is to vary the variables that define peer groups, for example the education level can be an important determinant of social groups. The optimal definition and size of peer groups is subject to further theoretical and empirical analysis.

6 Conclusion

Labour supply decisions of mothers, in particular mothers with young children, are influenced by social norms and peer behavior. In this paper, we estimate the quantitative importance of peers' decisions on the labour supply of mothers with young children, in particular on the

probability to return to work within the first year after giving birth.

We find strong evidence for social interaction effects regarding labor supply decisions of mothers with young children among coworkers at the workplace. Using exogenous variation in the probability to return to work in the first year after giving birth induced by a parental leave benefit reform in Germany, we can identify social interaction effects among co-workers. We find that the probability to return to work in the first year after giving birth is higher for mothers with coworkers who return in the first year after giving birth (and would not have done so in absence of the reform) by about 31 percentage points. This result is robust to several specification tests.

Our results show that peer behavior and social norms play an important role in labor supply decisions of mothers with young children. These peer effects can explain why we see changes in employment patterns over time as well as differences in employment between countries that can not solely be explained by financial incentives induced by institutional differences. The results are also relevant from a policy point of view. Reforms that change financial incentives to work in one direction or the other not only have direct labor supply effects - the direct response of individuals to financial incentives - but also indirect effects through social interaction. These social interaction effects might increase direct responses to economic policy reforms considerably.

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7   Appendix

Table 3: COMPARISON OF PEER MOTHERS GIVING BIRTH BEFORE AND AFTER THE REFORM

	Before reform				After reform				All births in reform window			
Variable	median	mean	sd	n	median	mean	sd	n	median	mean	sd	n
Return within 10 months		0.44	0.50	208		0.24	0.43	290		0.32	0.47	498
Return within 11 months		0.46	0.50	208		0.27	0.45	290		0.35	0.48	498
Return within 12 months		0.52	0.50	208		0.38	0.49	290		0.44	0.50	498
Return within 11-15 months		0.23	0.42	208		0.33	0.47	290		0.29	0.45	498
Return within 24 months		0.78	0.41	208		0.77	0.42	290		0.77	0.42	498
Age at childbirth	32	32.49	4.35	208	32	32.14	4.05	290	32	32.29	4.18	498
Monthly labor income	2723.7	3005	868.4	208	2909	3091.9	905.7	290	2843.3	3055.6	890.5	498
Days in employment	2950	3334.3	1656.5	208	2970.5	3222	1480.7	290	2950	3268.9	1555.9	498
Days in firm	2081.5	2524.3	1705.7	208	2039.5	2325.6	1624.4	290	2060	2408.6	1660.1	498
High education		0.21	0.41	208		0.18	0.38	290		0.19	0.39	498
Low education		0.05	0.21	208		0.02	0.15	290		0.03	0.18	498
Migration background		0.04	0.20	208		0.05	0.22	290		0.05	0.21	498
Number of children		1.17	0.40	208		1.19	0.43	290		1.18	0.42	498
Date of childbirth	-96	-92.63	51.09	208	92	94.34	50.98	290	35	16.25	105.45	498
Firm size	393	1339.1	3556.9	133	292	852.0	1960.7	179	335	1059.6	2761.9	312
West Germany		0.71	0.45	208		0.66	0.47	290		0.68	0.47	498

Table 4: COMPARISON OF PEER AND COWORKER MOTHERS

Variable	Coworkers				Peers				All births in sample			
	median	mean	sd	n	median	mean	sd	n	median	mean	sd	n
Return within 10 months		0.22	0.41	1363		0.32	0.47	498		0.25	0.43	1861
Return within 11 months		0.24	0.43	1363		0.35	0.48	498		0.27	0.44	1861
Return within 12 months		0.37	0.48	1363		0.44	0.50	498		0.39	0.49	1861
Return within 11-15 months		0.39	0.49	1236		0.29	0.45	498		0.36	0.48	1734
Return within 24 months		0.76	0.43	871		0.77	0.42	498		0.76	0.43	1369
Age at childbirth	32	32.08	4.26	1363	32	32.29	4.18	498	32	32.13	4.24	1861
Monthly labor income	2807.1	2923.4	1143.4	1363	2843.3	3055.6	890.5	498	2816.5	2958.8	1082.1	1861
Days in employment	3029	3216.6	1541.1	1363	2950	3268.9	1555.9	498	3014	3230.6	1544.9	1861
Days in firm	1652	2135.1	1759.1	1363	2060	2408.6	1660.1	498	1803	2208.3	1736.9	1861
High education		0.21	0.41	1363		0.19	0.39	498		0.21	0.41	1861
Low education		0.03	0.17	1363		0.03	0.18	498		0.03	0.18	1861
Migration background		0.06	0.23	1363		0.05	0.21	498		0.05	0.23	1861
Number of children		1.24	0.45	1363		1.18	0.42	498		1.22	0.44	1861
Date of childbirth	599	617.2	267.03	1363	35	16.25	105.5	498	439	456.38	354.97	1861
Firm size	335	1084.2	2780.4	312	335	1059.6	2761.9	312	335	1071.9	2769	624
West Germany		0.69	0.46	1363		0.68	0.47	498		0.69	0.46	1861

Table 5: COMPARISON OF MOTHERS WHO'S PEERS GAVE BIRTH BEFORE AND AFTER THE REFORM

Variable	Peer birth(s) before reform				Peer birth(s) after reform				All coworkers in sample			
	median	mean	sd	n	median	mean	sd	n	median	mean	sd	n
Return within 10 months		0.26	0.44	496		0.19	0.40	867		0.22	0.41	1363
Return within 11 months		0.27	0.45	496		0.22	0.41	867		0.24	0.43	1363
Return within 12 months		0.39	0.49	496		0.36	0.48	867		0.37	0.48	1363
Return within 11-15 months		0.39	0.49	457		0.39	0.49	779		0.39	0.49	1236
Return within 24 months		0.77	0.42	331		0.75	0.43	540		0.76	0.43	871
Age at childbirth	32	32.26	4.26	496	32	31.97	4.26	867	32	32.08	4.26	1363
Monthly labor income	2845.6	2992.7	1123.8	496	2777.1	2883.7	1153.3	867	2807.1	2923.4	1143.4	1363
Days in employment	2973	3212.3	1574.5	496	3037	3219.0	1522.6	867	3029	3216.6	1541.2	1363
Days in firm	2138	2507.8	1783.6	496	1233	1921.9	1709.7	867	1652	2135.1	1759.1	1363
High education		0.24	0.43	496		0.20	0.40	867		0.21	0.41	1363
Low education		0.03	0.17	496		0.03	0.18	867		0.03	0.17	1363
Migration background		0.05	0.22	496		0.06	0.24	867		0.06	0.23	1363
Number of children		1.20	0.41	496		1.25	0.47	867		1.24	0.45	1363
Date of childbirth	564	595.2	265.5	496	622	629.77	267.22	867	599	617.18	267.03	1363
Firm size	401	1370.0	3568.1	133	293	871.77	1989.89	179	335	1084.2	2780.4	312
West Germany		0.68	0.47	496		0.69	0.46	867		0.69	0.46	1363
Group size	15	22.84	23.13	170	17	32.64	76.44	211	16	28.27	59.08	381
Births in group	1	1.14	0.58	171	1	1.29	1.11	212	1	1.22	0.91	383
Peer date of birth (latest)	-85	-84.02	50.22	496	142	120.30	54.02	867	64	45.95	111.55	1363

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Figure 2: Fraction of mothers who stays at home for at least 10 months after childbirth
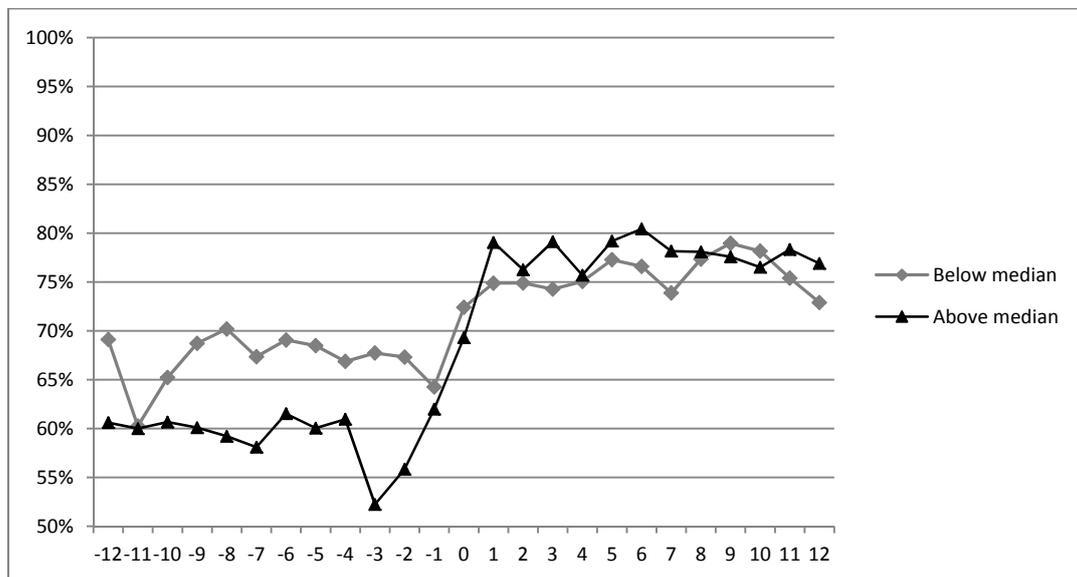
Note: the figure is based on our own analysis using the LIAB. Fractions are calculated out of all women in the sample who gave birth in a given month. Please note that our sample is conditional on employment before the observed maternity. Furthermore, large firms are overrepresented in the LIAB and statistics are unweighed. Therefore the displayed results are not representative of the German population and maternal leave spells are likely to be shorter in our sample.