Establishment-level wage effects of entering motherhood

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We analyse the wage effects following employment breaks of women who enter
motherhood using a novel matching approach where mothers' wages upon return to
work are compared to those of their female colleagues within the same establishment.
Using an administrative German data set, we apply a fixed-effects propensity score
matching based on information two years before birth of the first child. Our results
yield new insights into the nature of the wage penalty associated with motherhood:
when matching with establishment-specific effects we find that first births reduce
women's wages by 19%, whereas ignoring the identifier and matching across all establishments would yield a wage cut of 26%. We therefore conclude that selection into
establishments is an important explanatory factor for the family pay gap.

JEL classifications: J13, J31, C14.

1. Introduction

This paper addresses the question as to why women with children are observed to
have lower wages than women without children. This 'family pay gap' is commonly
attributed to differences in employment experience—lower human capital formation or human capital depreciation during child-related employment breaks—and
differences in job flexibility or effort between mothers and non-mothers. An
alternative explanation is segregation; that is, selection of women who will eventually have children into more family-compatible occupations and establishments
at the price of lower earnings. In this case, a pay gap should be observed between
women intending to have children and those who are not planning to have a child
even before childbirth and subsequent career intermittence.

This paper attempts to disentangle the segregation effect from the wage effect
caused by a child-related employment break by drawing on longitudinal data of
female employees that include wages before and after parental breaks. We make use

of establishment-specific effects, as we are able to identify colleagues within the same establishment. Hence, by applying a semi-parametric estimation method based on matching, we compare the wage rate of each female employee who took a child-related employment break and returned to a full-time position within the same establishment with that of a continuously employed, but otherwise similar, colleague in the same establishment (the 'monozygotic twin colleague'). As our sample of selected mothers exhibits a strong attachment to the labour market, our results should be interpreted as a lower bound to the overall short-run wage effects of entering motherhood.

We find that first births reduce women's wages by about 19%, when applying an intra-establishment matching approach. Ignoring the establishment-specific effect and matching across all establishments (to a 'dizygotic twin colleague') yields a larger wage cut of 26%. Concluding from this result, selection into establishments is an important explanatory factor for the family pay gap since women with children are more likely to be found in companies with lower wages. However, selection does not explain the whole gap. Even compared to the immediate (mono-zygotic) establishment colleagues, mothers' wages are negatively affected upon return to employment.

Since the treatment effect is likely to depend on the duration of the employment interruption, we investigate the relation between the duration and the conditional wage differential between mothers and women without employment breaks in a subsequent regression analysis. As expected, the wage loss increases with the duration of the employment break.

2. What's new?

The wage penalty or 'family pay gap' has been investigated mostly for the United States (see evidence by Waldfogel, 1998a; Lundberg and Rose, 2000; Budig and England, 2001), the United Kingdom (Waldfogel, 1998b; Joshi et al., 1999), and for Scandinavian countries (Datta et al., 2002; Nielsen et al., 2004). Studies on Germany show that the wage penalty of motherhood is comparatively large. Depending on the individual educational and occupational background, one year out of work after childbirth is associated with a wage loss of about 12% (see estimation results by Ondrich et al., 2001; Beblo and Wolf, 2002a, 2002b; Kunze, 2002; Ejrnaes and Kunze, 2004; Schönberg and Ludsteck, 2007).[1] At the same time, Germany is known as a country with one of the most extensive parental leave legislations, comprising a mother protection period of 14 weeks and a parental leave period of up to three years (since 1992), during which the leave-taker's job is protected against dismissal. Although both parents are eligible for this leave and parents are allowed to switch the leave-taker several times, 98% of those on leave

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[1] However, the negative effect in Schönberg and Ludsteck (2007) is offset by a positive selection effect, resulting in a zero or even positive overall effect.

are women. In 2000 only 53% of mothers in West Germany and 70% in East Germany returned to employment directly after the formal leave period (Beckmann and Kurtz, 2001).

Lower wages of mothers may be caused by career intermittence due to child-birth and child rearing, but also by a reduced job attachment; hence, a decrease in commitment on the part of working mothers. Another prominent source of pay differences may be the occupational segregation of women intending to have children into lower-paying jobs or establishments with family-friendly job or company characteristics. As the underlying effects are manifold and complex, the size of the causal wage loss due to motherhood is difficult to measure.

Most studies use extended wage estimations to determine the average wage differential between women with employment breaks and continuously employed women. This procedure involves two major problems. Firstly, wage regressions represent a parametric approach, which relies on the assumption that the functional form is linear in its parameters. Secondly, the estimated wage effect of employment breaks is based on observed wage differentials of women working in different establishments. Taking into consideration that not only the wage level, but also the distribution of wages differs across establishments (see e.g., Davis and Haltiwanger, 1991; Bronars and Famulari, 1997; Abowd *et al.* 1999), standard wage regressions, which ignore these establishment-specific effects on wages, may lead to biased results.

We have attempted to overcome these shortcomings by applying a semi-parametric approach based on matching to determine the wage penalty of mothers relative to comparable non-mothers within the same establishment. This study hence provides two innovations in the analysis of family pay gaps: (i) we use establishment-specific effects to account for differences in the way establishments integrate and promote mothers returning to their jobs; and (ii) we use a semi-parametric estimation method, which imposes no restrictions on the functional form of the relationship between child-related employment breaks and wages.

The challenge of our research question is to determine which wage level a mother would have achieved if she had not given birth and taken an employment break within a specific observation period. Since this counterfactual outcome cannot be observed, we have to identify a control group of females without employment breaks that is comparable to our selection of females who give birth, with respect to the distribution of all variables affecting the wage determination process. A perfect counterpart for a mother would be a childless female colleague who works in the same company, in a comparable job, is of comparable age, has experienced the same career path, achieved the same educational level and exhibits the same unobservable characteristics—such as ability or motivation—potentially affecting the wage rate. As such, an ideal counterpart is difficult to find; we therefore propose a combined procedure of exact and propensity score matching to determine a useful control group. The exact matching consists of finding a similar colleague, based on the estimated propensity scores, within the same establishment. Let us call this match a monozygotic twin colleague (as opposed to a dizygotic

twin colleague found by matching across establishments). As a result, we compare women who give birth to their first child (treatment group 'mothers') and women who do not give birth during the observation period (control group 'non-mothers') but are continuously employed within the same establishment as the mothers, to accommodate segregation and unobserved establishment-specific effects. By propensity score matching based on information two years before birth (including wage level and wage growth), we take into account that women intending to become mothers may be less attached to the labour market even before having a child and therefore choose jobs or occupations with rather flat experience profiles but smaller expected wage cuts due to discontinuous employment patterns. Thus, our matching is designed to control for observable and unobservable features of women intending to have children and their employers.[2]

Once the control group is determined, we compare the wage rates of mothers and non-mothers before and after the mothers' employment break. We have information on wages directly upon return and 12 months after the end of the break. These dates are determined dynamically by the duration of the interruption chosen by the mother. We compare her wage rate with that of the respective (set of) control colleague(s) who is (are) working in the same establishment and on the same day. The mean difference in wages reflects the average effect of treatment on the treated of entering motherhood and experiencing a specific employment break before returning to the former employer. The wage effect is likely to differ across women due to heterogeneity in the duration of the employment interruption and other individual characteristics. In a subsequent regression analysis, we therefore investigate the differences in wage losses using the duration of the employment break as an explanatory variable.

Obviously, this approach places high demands on the data needed. We therefore base our analysis on a data set of process-generated information on all employees in Germany liable to social security, which is provided by the Institute for Employment Research (IAB).

The remainder of the paper is structured as follows. Section 3 presents our methodological approach. The data is described in Section 4. Section 5 discusses the results of the matching procedure and the second-step wage gap analysis. The final section concludes and discusses potential extensions of our approach.

3. Our econometric approach

The goal of this paper is to determine the average treatment effect on the treated (ATT) on the wage rate, that is, the average expected effect of entering motherhood and experiencing an employment break for all employed women who eventually

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[2] To our knowledge, the only study that exploits the econometric methodology of matching to analyse the wages of mothers is provided by Simonsen and Skipper (2006, 2008). In contrast to our approach, they cannot assign female employees to their firms and thus only compare women across establishments.

become mothers. We follow Rubin (1974) and identify the causal effect of the 'treatment' by comparing the wage rate of a mother after her parental leave period with the hypothetical situation of the same woman if she had not entered the stage of motherhood.

Let Y_1 denote the wage rate of mothers after returning to their former employer and let Y_0 denote the wage rate of women who did not interrupt their career due to childbearing. Let D be an indicator variable, which equals one if a woman took a parental leave employment break and equals zero if not. The ATT is given by:

$$E(Y_1|D=1)-E(Y_0|D=1)$$

Since the hypothetical situation $E(Y_0|D=1)$ cannot be observed for mothers, we have to find alternative ways to estimate the average wage of mothers with parental leave experience if they had been continuously employed. According to Heckman *et al.* (1999), two alternative approaches may be applied to estimate the average non-treatment outcome (in our case, the wage rate of a continuously employed non-mother): (i) a before-after comparison of mothers; or (ii) a comparison with a useful control group of non-mothers. The first approach assumes a constant average non-treatment outcome over time for the treated. It requires that mothers would have experienced a constant wage rate had they remained childless. This assumption does not hold if, for example, these women would otherwise have been promoted, their wage scales are tenure-based or macroeconomic shocks have taken place. Another fundamental problem applying to both approaches is the potential selection bias, which occurs if mothers differ from both women intending to have children and non-mothers, due to observable and unobservable characteristics. Due to these selection effects, the wage levels of mothers and non-mothers may already be different before the treatment.

To account for differences in observable characteristics, we refer to the Conditional Independence Assumption (CIA). Under CIA, it does not matter whether we estimate the average outcome of continuous employment based on information about mothers or non-mothers if they have similar observable characteristics (Imbens, 2004). This implies that:

$$E(Y_0|D=1, X) = E(Y_0|D=0, X)$$

To fulfil the CIA, the set of variables (X) should include the wage rate before treatment and all wage-determining characteristics. Based on the choice of (X), one can select the appropriate control group by propensity score matching algorithms. However, when is the appropriate point in time to compare the selected characteristics of mothers and non-mothers? Of course, the definition of the control group should be based on information before the observed career intermittence of mothers. Taking into consideration that becoming pregnant is not a fully exogenous event and women intending to have children may be more likely to substitute wage income for flexible working conditions (which are difficult to observe in general), we should compare future mothers and non-mothers
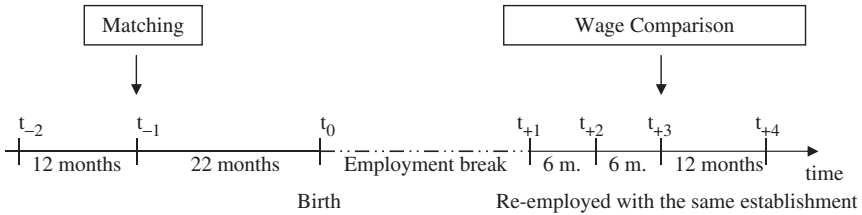
Fig. 1. Time frame of our evaluation approach

with respect to their wage rate and all wage-determining characteristics when the employment break is not yet a certain event. Since the shape of the wage profile just before the first birth might already be affected by the future event (see Ejrnaes and Kunze, 2004; in analogy to Ashenfelter's dip in labour market policy evaluation), we define our first observation point as 22 months before birth.

Figure 1 illustrates the time frame of our evaluation approach. At t_0, the mother gives birth to her child. To account for differences between women with and without parental leave breaks, we match mothers and non-mothers at time t_{-1}, assuming that the future pregnancy has not yet been anticipated, at least not in a way related to wages or wage-determining characteristics. The employment break due to motherhood lasts from t_0 to t_{+1}; that is, duration differs between individuals. At t_{+1}, the mother returns to her former employer:[3] t_{+1}, just as t_{+2}, t_{+3}, and t_{+4} are alternative observation points for wage comparisons with the mother's female colleagues (i.e. her matching partners) who are—still or again—working in the same establishment.[4]

The challenge concerning the measurement of the ATT is to determine the wage rate of a mother if she had not given birth to a child and interrupted her employment career for this reason. Given that this hypothetical outcome is not observable, we have to identify a control group of non-mothers that is comparable to the mothers with respect to the distribution of all variables affecting the wage determination process. As mentioned above, a perfect counterpart for a mother would therefore be a female colleague without children who has the same individual and labour market-related characteristics and exhibits the same unobservable characteristics potentially affecting the wage rate. It is obvious that the ideal counterpart is difficult to find, even if we had full information on all female colleagues.

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[3] The return to the job is defined as an employment spell of at least three months in length.

[4] We are aware that this setup gives rise to yet another source of selection bias, since the analysis is based on a comparison of individuals who remain in the establishment only, as regards both mothers and control observations. If we assume that establishment mobility is positively correlated with the expected wage loss, our estimates will provide a lower bound for the true wage effect of motherhood.

Hence, we propose a feasible alternative, a combination of exact and propensity score matching, to determine a useful control group.

As exact matching compares people with the same values of observed characteristics X, this method works only with a limited number of discrete X-variables or, alternatively, value ranges for continuous X-variables. The higher the number of variables selected and the larger the range of values these variables may take, the lower is the probability of finding an exact match. Propensity score matching reduces this 'curse of dimensionality' by defining a distance metric on X. Subsequent matching is based on the distance metric rather than the original X-vector. Rosenbaum and Rubin (1983) illustrate that the distance metric may be defined as: $P(X) = \Pr(D = 1|X)$.[5]

In our combined matching procedure, we first estimate a parametric probit model to predict the individual propensity score $P(X)$. Exact matching within the establishment, based on this propensity score, then ensures that treated and untreated women underlie the same unobserved fixed effect influencing the wage determination process within the establishment. In our setting, $P(X)$ describes the likelihood of becoming a mother and returning to a full-time job within the observation period for each individual in the sample. The vector (X) hence includes all variables presumably affecting motherhood and subsequent employment. Given the limited information about the household context in our data, we basically include information on age, education, current occupation and past employment history. Controlling for education and occupation is intended to account for unobserved individual heterogeneity affecting occupational choice. By conditioning on age, experience and tenure, we have attempted to consider different stages in the life cycle associated with the likelihood of maternity and labour market attachment. Education levels and working time also serve to make daily wages comparable. By including the number of female employees, we take establishment size effects into account. We further account for differences in hourly wage rates 22 months before birth.

We apply nearest neighbour matching (NNM) with replacement to keep the bias small. As the choice of the number of nearest neighbours is subject to a trade-off between bias and variance, we choose one neighbour, being aware that the variance may be high. Note that all pairs belong to the same establishment, so that we control for unobserved establishment-specific effects influencing the wage determination process. To restrict the differences between the nearest neighbours—which tend to be larger in smaller establishments—we define a caliper of 0.5. Sensitivity analyses using exact matching and alternative matching algorithms are discussed in Section 5.3.

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[5] The intuition behind the propensity score matching is that individuals with the same probability of 'participation'—that is, becoming a mother—can be paired for the purpose of comparison.

4. Data

The merit of our empirical analysis is nourished by the combination of several data sets that allow longitudinal comparisons between mothers and non-mothers within the same establishment. We draw on process-generated data provided by the Institute for Employment Research (IAB). These German register data are generated by an integrated notifying procedure for the public health insurance, statutory pension scheme and unemployment insurance, which was introduced in 1973. By law, employers have to provide information to the social security agencies for employees acquiring claims to the social security system. These notifications are required at the beginning and ending of any employment relationship. In addition, employers are obliged to provide an annual report for each employee who is employed on 31 December of each year and covered by social insurance. The reports include information on sex, year of birth, nationality, occupation, qualification, and gross wage rate of the employee. Furthermore, each spell includes information on the industry and a unique establishment identifier for the establishment where an individual is employed. According to the obligation to register with the state pension authorities, this data encompasses all persons who have paid contributions to the pension system or who have been covered by the pension system through contributions by the unemployment insurance or by being a parent. As a consequence, certain groups of employees are not covered by the data: (i) (temporary) civil servants or self-employed persons; and (ii) women who are employed in East Germany or abroad. The latter selection is necessary because the supplementary information on the nature of employment breaks is available for workers employed in West Germany only. Nevertheless, the register still represents about 80% of all employees on the labour market.[6]

We use two different samples from these register data (see Fig. 2). In Step 1, we combine the IAB employment sample with additional administrative data assembled at the state pension authorities (IAB employment supplement sample).[7] Both data sets can be linked by the social security number. The matched file contains a 1% random sample of the total German population that was gainfully employed for at least one day between 1975 and 1995 (for details see Bender et al., 2000). Based on the supplement sample, we have exact information about the individuals' entire working lives that allows us to distinguish between different types of 'non-working' periods, namely: unemployment, formal parental leave, illness, disability, care for other people, full-time education, military or alternative civilian service, and other spells outside the labour force. Furthermore, these data allow us to identify the fertility history of all women. Since the birth of children

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[6] Due to the nature of the data we do not have any information on the household background, such as the household income, the partner's employment status, etc.

[7] For first descriptive analyses with these data see Prinz (1997), for an analysis of the wage penalties of heterogeneous employment biographies see Beblo and Wolf (2002a, 2002b) and for the effects of entry into motherhood on women's employment dynamics see Bender et al. (2003).

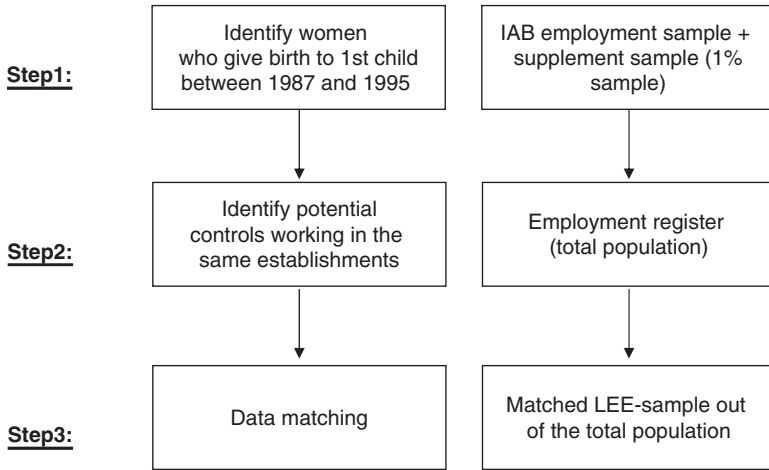|		
Step1:	Identify women who give birth to 1st child between 1987 and 1995	IAB employment sample + supplement sample (1% sample)
Step2:	Identify potential controls working in the same establishments	Employment register (total population)
Step3:	Data matching	Matched LEE-sample out of the total population

Fig. 2. Sampling procedure

increases the pension entitlement of the mother, the IAB employment supplement sample provides exact information about the number of children and the month of birth.[8]

Based on the exact information about fertility and employment history, we select our treatment group; that is, women who gave birth to their first child between 1987 and 1995. Since we are interested in the wage effects of parental leave periods, we further restrict the sample to women who were working 22 months before the birth of their first child and, after the employment break, returned to the same establishment for at least three months within our observation period up to 1999. After deleting observations with missing values, we are left with 1,390 observations of mothers.

As described in Section 3.2, the innovative aspect of our analysis is in measuring the relative loss in mothers' wages by comparing mothers' and non-mothers' wages within the same establishment. Hence, the control group has to be drawn from a sample of all colleagues of these 1,390 mothers selected in the first step. To do so, we make use of the Employment Statistics Register, which includes information about the total population of all individuals registered in the social security system (Step 2). The following procedure describes our strategy to identify all female colleagues of our treatment group:

(1) We identify the treatment group in the Employment Statistics Register.

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[8] Under very restrictive assumptions, it is possible to interpret specific gaps in the IAB employment sample as interruptions due to parental leave or national service (see e.g. Kunze 2002, p.11). An exact identification of childbirth, however, is only possible with the supplementary file. Schönberg and Ludsteck (2007) also use this supplementary file to impute times of parental leave into the total population.

(2) We identify the unique establishment number of every observation in the treatment group.
(3) We select all women who were employed in t_{-1} and t_{+3} in the identified establishments and did not take an employment break within this period.

After this selection and matching of the two groups in Step 3, the data set consists of 307,541 observations of potential control women.[9] Due to missing observations of selected variables, 1,229 mothers and 245,871 female colleagues enter the propensity score estimation. For a meaningful wage comparison, we further restrict our sample to women in full-time employment one year after the mothers' return to the job (in t_{+3}) because we do not have information about the number of working hours in part time jobs and thus cannot compute hourly wages. The use of daily wages, however, provides us with the full compensation package, including overtime pay, Christmas bonus and other premiums, which may also be lower after an employment break. We are left with 561 mothers and 224,825 female colleagues, for whom we have information on wages after the employment break as well as wages and individual characteristics 22 months before birth. This sample of mothers represents about 40% of mothers in Germany who are employed before having a child and return to any (full-time or part-time) work afterwards. Bearing in mind that our sample is very selective in terms of attachment to the labour market, our results may be interpreted as a lower bound to the overall short-run wage effects of entering motherhood. Note that the control group may also contain mothers, under the condition that they delivered their children before 1987 and were continuously employed during our observation period (that is, without an employment break of longer than 90 days).

Since mothers in small establishments are likely to have only few female colleagues, whereas mothers in large establishments tend to have more, the number of potential control observations per treated observation is very unequally distributed (see Table 1). While 9.5% of all treated observations have no female colleagues—and hence have to be ignored—and 7.8% of mothers have just one control observation, there is one case where we are able to identify the maximum of 7,159 potential control observations for one specific mother. According to Table 1, only about 63% of the treatment group are employed in establishments where we can identify at least ten potential control observations. Because of this ratio between mothers and potential control persons in the same establishments, we do not expect to find a comparable female colleague for each mother.

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[9] Since the selection of our treatment group is based on a 1% sample and the group of potential controls is drawn from the total population, this sampling procedure yields an oversampling of control observations. Given that the 1% sample of the total population (that is the supplement sample) is completely random, our analysis does not require weights to consistently estimate the probability of entering motherhood, such as in the case of choice-based sampling of the treatment group.

Table 1 Distribution of control persons

No. of potential control observations	% of mothers with control observations
0	9.53
1	7.66
2	4.99
3	3.57
4	2.85
5	1.43
6	1.43
7	1.25
8	1.07
9	1.78
10	1.43
>10	63.01

Source: Sample of 561 mothers (childbirth between 1987 and 1995) and 224,825 female colleagues (employed full time in t_{+3}), drawn from the IAB Employment Sample, IAB Employment Supplement Sample, Employment Statistics Register.

Figure 3 illustrates the average wage rates of the selected mothers and female colleagues before and after the mothers' employment break.[10] It is obvious that even 22 months before birth women intending to become mothers earn lower wages on average than their colleagues. Presumably, this wage differential is caused by differences in observed characteristics for the most part. Interestingly, pre-birth wage growth does not seem to differ between future mothers and their control group. After the employment break, the gap between mothers and women without comparable employment breaks becomes even greater. While the female colleagues experience an almost linear wage growth, mothers' wage profiles exhibit a sharp decline and hardly reach the level from 22 months before birth (t_{-1}) even two years after the end of the employment break (t_{+3}).

Figure 4 describes the duration of employment breaks of first-time mothers in our sample. Most of these women drop out of the labour market for up to 21 months and the average woman takes 18 months off—conditional on returning to a full-time position with the same employer thereafter. Only a negligible fraction of mothers returns to work within the first three months following birth; that is, after the end of the maternity protection period of eight weeks. It should be noted that the maternity leave legislation changed significantly during our

[10] Wage information in the employment register is censored at the upper bound. Estimation strategies may be used to impute wages above this ceiling (see for example Gartner, 2005). We draw on the original wage information because our sample does not include many female employees above the threshold level. Thus, we may underestimate the wage losses of mothers who start above this level, if their wages fall below the threshold after the break. Likewise, we may underestimate the wage increases of non-mothers if their wages rise above the ceiling.
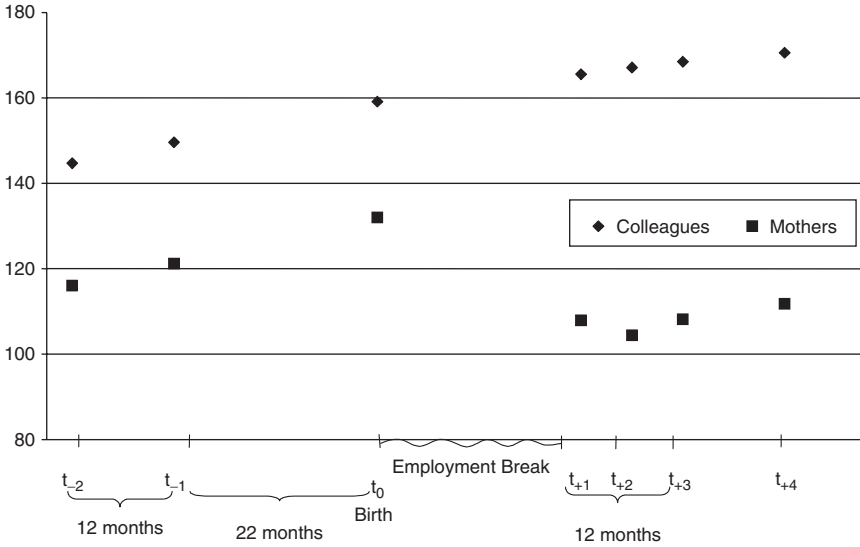
Fig. 3. Average wages of mothers and their female colleagues (potential
controls) before and after birth
Source: Sample of 561 mothers (childbirth between 1987 and 1995) and
224,825 female colleagues, drawn from the IAB Employment Sample,
IAB Employment Supplement Sample, Employment Statistics Register.
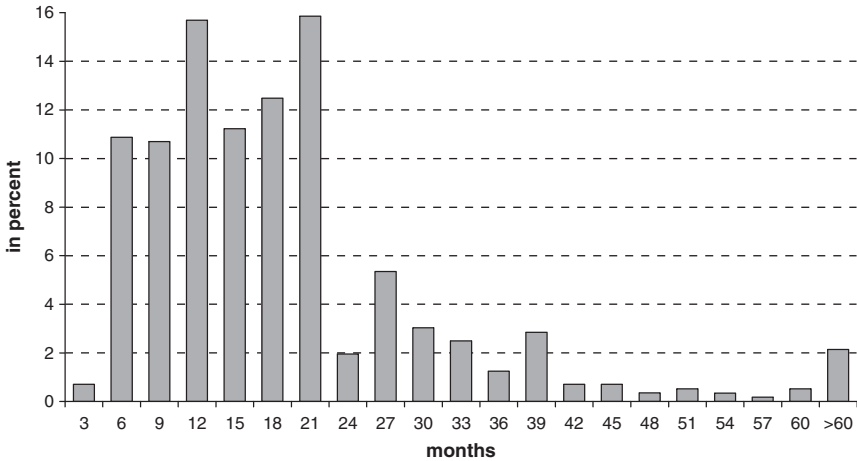Wages are in DM (German marks). 1 DM equals 0.51 €.

Fig. 4. Duration of mothers' employment breaks
Source: Sample of 561 mothers (childbirth between 1987 and 1995) drawn from
the IAB Employment Sample, IAB Employment Supplement Sample,
Employment Statistics Register.

observation period. Starting from ten months in 1987, the maximum leave dura-
tion increased up to 36 months as of 1992. On average, about 30% of mothers stay
out of work at the establishment for more than the maximum legal parental leave
period with a guaranteed return to a job of an adequately similar status. The share
of women prolonging their parental leave beyond the job-protected period differs
tremendously by year. While most of the women who first became mothers
between 1989 and 1991 did not return to their former jobs within the parental
leave period, this fraction declined to less than 14% in 1992.

The relatively short average leave duration of 1.5 years, for German standards,
underlines the sample selection of women, who seem to be more job-oriented than
the average.[11] Taking into consideration that all mothers in the sample work full
time (within a year upon return) provides another source of selection towards
women with a mix of generally higher-paying characteristics. When drawing con-
clusions from the estimated wage effects of motherhood, we will consider these
sources of systematic sample selection.

5. Matching results

5.1 Propensity score estimation

Table 2 presents the estimation results of a probit estimation of the likelihood
of becoming a mother at time t_0 and returning to the same employer conditional
on individual characteristics at time t_{-1}. At this step we use the full sample of
re-employed mothers regardless of their working time arrangement, to enhance the
efficiency of the estimation. Due to the lack of data, no information on the house-
hold background such as household composition, partner's employment status or
earnings, etc. can be considered. To determine differences with respect to the family
situation, we exploit all available individual information that might correlate with
the likelihood of having a child and the wage rate at the same time. Age enters the
equation nonlinearly by several splines, most of which are statistically significant.
Interestingly, college and university graduates have a higher probability of belong-
ing to the treatment group than employees with apprenticeship training. This result
may be driven by the choice of our treatment sample of mothers who return to
work within the observation period of nine years.

We find significant differences between occupational groups as well as between
blue and white-collar workers. We finally include a set of variables describing
the past employment history. These are intended to account for selection into
motherhood, as women intending to become mothers and women who do not
plan to have children may follow differing employment paths from the start of

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[11] The average duration among all women who work prior and (at any time) subsequent to birth in our
data set is three years. As the distribution is skewed, the median amounts to only 1.8 years. The median
duration of leave in our sample is 1.3 years.

Table 2 Probit estimation results of having a birth at time t_0

	Coefficient estimate	Standard error
Age splines		
<25	0.0317	0.0237
25–27	**0.0675**	0.0185
28–30	**−0.0447**	0.0176
31–33	**−0.1358**	0.0220
34–36	**−0.0859**	0.0293
37–39	**−0.2246**	0.0505
>=40	**−0.6922**	0.3056
Education level (ref. apprenticeship)		
No apprenticeship training	−0.0017	0.0403
College/university graduate	**0.4304**	0.0693
Wage splines (in DM per day)		
<80	−0.0028	0.0079
80–120	0.0002	0.0004
Tenure splines (in months)		
<20	0.0038	0.0044
20–40	−0.0011	0.0035
40–60	**−0.0092**	0.0037
60–80	**0.0146**	0.0037
80–100	−0.0013	0.0039
100–120	**0.0088**	0.0039
>120	**0.0089**	0.0016
Female employees splines (in no. of empoyees./100)		
<0.1	**−18.3304**	1.5021
0.1–0.2	−1.4956	1.0733
0.2–0.5	**−1.8573**	0.3028
0.5–1	**−0.5236**	0.1517
1–2	**−0.2933**	0.0695
2–5	**−0.1209**	0.0234
5–10	**−0.0479**	0.0161
10–20	−0.0193	0.0102
20–40	**−0.0121**	0.0056
>40	−0.0043	0.0046
Work experience in past 4 years	**−0.0209**	0.0032
Employment break (in months) >93 days	**−0.0305**	0.0043
No. of employment breaks (>31 days)	**−0.1822**	0.0377
Average wage growth in past 4 years	−0.0128	0.0371
Constant	1.0591	0.7411
Pseudo R squared	0.3973	
No. of observations	247,081	

Source: Sample of 1,229 mothers (childbirth between 1987 and 1995) and 245,871 female colleagues drawn from the IAB Employment Sample, IAB Employment Supplement Sample, Employment Statistics Register. Other control variables: occupational groups. Bold coefficients indicate a significance level of 5%.

Table 3 Descriptive statistics for mothers and their female colleagues

	Raw data			Matched data (caliper = 0.5)		
	Mothers	Potential controls	Equal means?	Mothers	Selected controls	Equal means?
Characteristics (at time t_{-1})	Mean	Mean	T-test	Mean	Mean	T-test
Daily wage in DM	120.72	149.61	17.20	127.17	125.02	−0.92
Average wage growth in past 4 years	0.28	0.26	−1.15	0.30	0.28	−0.54
Age	28.73	32.90	13.51	28.71	28.83	0.45
No apprenticeship training	0.20	0.30	4.82	0.24	0.22	−0.70
Apprenticeship training	0.76	0.66	−4.67	0.72	0.74	0.67
College/university graduate	0.04	0.04	0.01	0.03	0.03	0.00
Work experience past 4 years	44.41	44.71	1.03	44.59	45.04	0.84
Past employment breaks (in months)	2.02	2.92	2.89	2.00	1.92	−0.22
No. of past employment breaks	0.24	0.30	2.40	0.21	0.20	−0.30
Tenure	60.92	70.19	5.11	62.82	62.50	−0.11
Unskilled worker, full time	0.20	0.27	4.35	0.24	0.23	−0.35
Skilled worker, full time	0.06	0.04	−2.87	0.04	0.05	0.65
White collar, full time	0.73	0.66	−3.05	0.70	0.70	0.00
No. of obs.	561	224,825		434	434	

Source: Sample of 561 mothers (childbirth between 1987 and 1995) and 224,825 female colleagues (employed full time in t_{+3}), drawn from the IAB Employment Sample, IAB Employment Supplement Sample, Employment Statistics Register. 1 DM (German mark) equals 0.51€.

their careers. However, the results are ambiguous. Not surprisingly, intermittent work histories tend to reduce the likelihood of entering motherhood and returning to the same establishment. Active labour market participation during the past four years (as a proxy for career orientation) has a negative effect on entering our treatment group, while tenure (measured in splines) within the same establishment increases the propensity to become a mother and return to the former employer. The average yearly wage growth during the last four years does not significantly affect the probability of belonging to the treatment group.

Table 3 compares the mean characteristics of the selected mothers and their potential and effective control colleagues before treatment; that is, at time t_{-1}, 22 months before birth. Evidently, the matching algorithm contributes to a balancing of the samples with respect to the relevant variables. 22 months before the date of birth the wage rates of future mothers are substantially lower than those of all potential controls, but slightly higher than those of their selected monozygotic twin colleagues. The fact that the potential control group exhibits even more and longer employment breaks within the past five years than the future mothers may indicate that at least part of the female colleagues have entered motherhood already before the start of our observation period, and therefore experienced a less continuous employment path on average. We will consider this peculiarity in the assessment of our estimation results in Section 6.

5.2 Wage effects

Tables 3 and 4 both show that the average wages of the mother samples and the respective control samples differ quite remarkably between the raw data and the selected individuals after matching. In the raw data set the control group receives a higher average wage rate than the future mothers, whereas in the matched sample wage levels are no longer significantly different.

A look at the average wage rates of mothers and their corresponding control colleagues, one year after re-entry into employment, indicates that the post-treatment wage outcome of mothers is substantially lower compared to their controls. While mothers' daily wage rates fall by 11 DM (5.62 €) between t_{-1} and t_{+3}, the potential control colleagues' wage rates increase by 19 DM (9.71 €) on average. The unmatched wage difference between mothers and controls in t_{+3} amounts to more than 60 DM (30.68 €), which is about 30% of the controls' wage level. The matched gap is 28 DM (14.32 €), which translates into an average wage cut of about 19% with respect to the control colleague's wage.

In contrast to the existing studies on the wage effect of an employment break, our data allow us to accommodate establishment-specific fixed effects. This aspect is important if establishments differ with respect to their average wage growth patterns. If, for example, women who are more likely to become mothers select into companies with steeper wage profiles and a variety of promotion opportunities, ignoring establishment-specific heterogeneity would tend to overestimate the true penalty in wages. In contrast, the expected wage loss of entering motherhood is underestimated if future mothers select into companies whose employees have rather stable wage rates. To test these hypotheses, we calculate the ATT based on our propensity score estimation but ignoring establishment-specific fixed effects. That is, we match across all potential control women[12] and not only within the same establishment (see Table 5).[13] The ATT significantly increases in this specification. That is, compared to all female employees across establishments, mothers lose almost 39 DM per day, which amounts to a wage drop of about 26%.

So far, we have considered only average effects across all women who became mothers between 1987 and 1995. An important source for individual differences in the child-related wage cut is the amount of time spent out of the labour market (see Fig. 2). According to human capital theory, a career interruption implies an interruption in the accumulation of (job-related) human capital (Mincer, 1974). In addition to the mere lack of experience, an employment break may lead to

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[12] Instead of using the whole universe of non-mothers in Germany, we match out of the 298,822 non-mothers in our sample of establishments. Thus, we are certain to produce a comparable result, because the number of establishments and the variation between the establishments remain constant across the two matching procedures.

[13] The distribution of the propensity score values of mothers and their potential controls are presented in the Appendix. This graph does, however, only refer to the matching across all potential control women. The support problem of intra-establishment propensity matching is addressed by applying a caliper of 0.5.

Table 4 Wage effects within and across establishments (in German marks)

	Raw data		Matching within establishments		Matching across establishments	
	before	after	before	after	before	after
Control group	149.82	169.17	125.02	143.57	149.82	146.87
Mothers	121.14	108.11	127.17	115.58	121.14	108.11
ATT in German marks		−61.06		−27.99		−38.76
ATT in%		36.1		19.5		26.4
# mothers		561		434		561
# control persons		215,819		434		561

Source: Sample of 561 mothers (childbirth between 1987 and 1995) and 215,819 female colleagues (employed full time in t_{+3} and without employment breaks of longer than 90 days), drawn from the IAB Employment Sample, IAB Employment Supplement Sample, Employment Statistics Register. 1 German mark equals 0.51€.

Table 5 OLS estimation results of intra-establishment wage effects (dependent variable: control's wage—mother's wage)

	Coeff. estimate	Std. error
Duration of employment break (ref. < 0.5 years)		
0.5–1 years	**0.2057**	0.0797
1–2 years	**0.2513**	0.0745
2–3 years	**0.2804**	0.0929
>3 years	**0.3153**	0.1037
Age (ref. >= 40 years)		
<25	0.7061	0.4602
25–29	0.7462	0.4571
30–34	0.6883	0.4575
35–39	0.5073	0.4629
Education level (ref. apprenticeship training)		
No apprenticeship training	−0.0262	0.0618
College/university graduate	**−0.3722**	0.1335
Constant	−0.5360	0.4601
Pseudo R squared	0.0875	
No. of observations	434	

Source: Sample of 434 mother-control pairs (child birth between 1987 and 1995) drawn from the IAB Employment Sample, IAB Employment Supplement Sample, Employment Statistics Register. Additional control variables include: occupational group dummies. Bold coefficients indicate a significance level of 5%.

a deterioration of the human capital stock already attained. Human capital acquired in previous years of employment is presumably more likely to become obsolete the longer the interruption lasts. Apart from the impact on human capital, career interruptions may also cause wage cuts due to productivity-related or signalling effects. Given incomplete information, a future employer might interpret

an interruption as a signal for a lower productivity level on the part of the applicant. Several (theoretical) studies attempt to explain why, for instance, past unemployment spells evoke negative expectations on the side of the employer regarding the productivity of the potential employee (see, among others, Vishwanath, 1989; Berkovitch, 1990; or Lockwood, 1991). As a consequence, a woman who returns to work directly after or before the end of her formal leave period is more likely to be seen as a job-oriented employee. A woman who extends her child-related break beyond the legal time frame, on the contrary, may be expected to devote less time and effort to her career and hence be offered a lower wage rate. Finally, there might be selection at work. Assuming that the utility of childcare is randomly distributed among women, women with lower wages are likely to return to work later, because their opportunity costs for childcare are relatively low.

To see how the duration of an employment break is related to a mother's wage cut, we run a linear regression, where the wage differences after matching are conditioned on the time out of work. Thus, we are able to calculate group-specific treatment effects for mothers with different characteristics, for example employment breaks of different length. Table 5 presents the coefficient estimates of a linear regression of the individual wage differences on the mothers' character-istics. In accordance with the theoretical reasoning above, we find that longer interruptions are associated with a lower relative wage on return. Age is not sig-nificantly related to the wage differential, but college and university graduates experience a significantly lower wage loss due to child-related leave. Finally, the occupation dummies (not presented in the table) indicate lower wage losses for semi-professionals (for the most part, nurses and social service professionals as well as kindergarten and other teachers, most of whom are employed in the public sector where wages are strongly determined by age and not by actual experience) and for jobs that require manual skills (which may be less subject to skill obsoles-cence due to changing tasks) compared to the reference group of skilled commer-cial and administrative occupations. In an alternative estimation model, the hypothesis that wage cuts are smaller if women return to work within their job protection period was not supported by the data.[14]

5.3 Sensitivity analyses

To get an idea of the robustness of the results presented, we now perform several sensitivity analyses. We first present the results of an exact matching approach. Furthermore, we check the sensitivity of the propensity score matching results with respect to different matching algorithms.

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[14] Whereas the dummy variable indicating whether the employment break exceeds the maximum length of job protection has a positive and significant coefficient estimate in a regression without additional control variables, the coefficient becomes insignificant once we control for the actual duration of career intermittence.

Table 6 Results of alternative matching specifications

	Fixed-effects matching (kernel) (1)	Fixed-effects matching (NN no caliper) (2)	Fixed-effects matching (caliper = 0.1) (3)	Exact matching (strict) (4)	Exact matching (loose) (5)
ATT in German marks	−26.76	−31.55	−27.81	−23.13	−23.48
ATT in %	18.8	21.9	18.8	16.3	17.0
# mothers	434	507	336	196	266

Source: Total sample of 561 mothers (childbirth between 1987 and 1995) and 215,819 female colleagues (employed full time in t_{+3} and without employment breaks of longer than 90 days), drawn from the IAB Employment Sample, IAB Employment Supplement Sample, Employment Statistics Register. 1 German mark equals 0.51 €.

Given that the number of comparison observations is small for some mothers—namely those working in small establishments (see Table 1)—we test out kernel matching (KM) as an alternative matching algorithm. Our baseline matching applies the most commonly used nearest neighbour matching (NNM) with replacement to keep the bias small. To restrict the differences between the nearest neighbours—which tend to be larger in smaller establishments—we define a caliper of 0.5. Matching more than one nearest neighbour increases the bias, while the variance of the match becomes smaller. Therefore, we secondly apply a kernel matching (KM) with an Epanechnikov kernel to make sure that only women within the same establishment are still selected as appropriate matches.[15] Bearing in mind that control observations are numerous in the full sample but asymmetrically distributed across establishments—mothers in small companies have fewer potential counterparts, whereas mothers in bigger establishments are more likely to have more adequate matches—kernel matching is especially helpful because it exploits additional data when available but does not rely on bad matches where close neighbours do not exist.

The second column of Table 6 describes the average wage effect based on our baseline matching algorithm with an Epanechnikov kernel and a bandwidth of 0.5. As the baseline model (NNM with caliper 0.5) and the kernel matching yield very similar results, the trade-off between bias and variance does not seem to be too severe in our case. If, however, the nearest neighbour is not restricted to a certain range (see column 3 with no caliper applied), all mothers working in an establishment with at least one female colleague are taken into account and the ATT increases by almost 4 DM compared to the baseline matching. This rather large difference seems plausible if we think of a future mother in a small establishment with only one female colleague for a comparison. If their propensities to become a mother within the next 22 months differ noticeably, we would also

[15] A normal kernel is less appropriate in our setting, because it would rely on all potential control observations—irrespective of whether they work in the same establishment or not.

expect that differences in their observed and unobserved characteristics yield significant wage differentials. Applying a caliper means skipping these observations and hence reducing the resulting wage differential.

For a sensitivity analysis with respect to the type of matching, we compare the results to those of an exact matching. Exact matches are selected with respect to the establishment, occupation (80 categories), age (with a maximum deviation of two years), education (three categories), working time status (full/part time), total work experience (with a maximum deviation of 20% or 30% if no colleague could otherwise be identified), and daily gross earnings (with a maximum deviation of 10 or 20% respectively). All information entering the matching procedure again refers to 22 months before entering motherhood (t_{-1} in Fig. 1). In the case of more than one female colleague matching the criteria, a control observation is generated by calculating averages for all variables across all selected colleagues. Due to the curse of dimensionality, exact matching is not capable of providing an appropriate control in all dimensions and for all mothers. Therefore, the number of observations in the matching sample reduces to 196.

The fourth column of Table 6 gives the results of the strict exact matching algorithm and the fifth column those of a less strict matching specification. In the latter, we do not balance the samples of mothers and selected control observations with respect to employment experience. Compared to the stricter exact matching, the number of observations increases by 35% but the matching results change very little.

It may be of concern that our strict criteria in the exact matching lead to a considerable reduction in the number of observations. Applying a smaller caliper in the NNM also reduces the number of observations. Being stricter on the matching criteria, however, does not affect the results severely. We therefore conclude that our chosen model specification does not really suffer from the small sample size. Relaxing the matching criterion to a reasonable degree does in fact increase the number of observations, but does not change the general result that women entering motherhood have a 23 to 32 DM lower daily wage one year after they return to their job.[16]

Based on this variety of sensitivity analyses, we conclude that the treatment effect of entering motherhood and taking an employment break lies somewhere between 16.3 and 21.9%—depending on whether we draw on pure propensity score matching, which might have the drawback of not fully capturing unobserved heterogeneity between treatment and control groups, or on exact matching, which suffers from a small sample size. After all, the result of 19.5% from our baseline matching

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[16] Another alternative approach would be a difference-in-difference estimator (DiD) that exploits the difference in the before-after wage changes of treated and control observations. Instead of applying DiD, which relies on a different identifying assumption than propensity score matching, we prefer to match on the lagged outcome variable wage$_{t-1}$ to reduce selection based on unobservable characteristics. However, the interested reader may be referred to estimation results based on a conditional DiD presented in Beblo et al. (2006), which yield wage effects of comparable size.

model seems to provide a comparatively robust measure of the wage penalty of entering motherhood.

6. Conclusions

In this study, we examine the penalty in mothers' pay due to the birth of their first child using a novel semi-parametric approach based on matching with fixed establishment effects. Using data from the IAB Employment Sample and additional administrative data assembled at the state pension authorities (IAB Employment Supplement Sample), we can identify all women working in the same establishment. Hence, we match each female employee who took a child-related employment break with a monozygotic twin colleague in the same establishment who did not take such a break. Due to intra-establishment matching, unobserved establishment-specific heterogeneity can be fully taken into account. Selection in observable and unobservable individual characteristics is accommodated by different matching and estimation algorithms.

In accordance with previous findings for Germany, our results indicate a substantial wage cut for mothers upon return to work. Although we confine the analysis to a selection of women who return to full-time work with the same employer, mothers' wages are about 19% lower than those of their female colleagues with comparable characteristics 22 months before entering motherhood. We interpret our results as a lower bound to the overall short-run wage effects of entering motherhood for two reasons. Firstly, our treated population is very selective in terms of its labour market attachment, as we only consider women who worked prior to the birth of their first child. Secondly, the women selected return to a full-time position within our observation period.

Note that most of the wage loss results from the fact that their female colleagues experience significant wage growth in the meantime. But even in absolute terms, the average real wage after a maternity break is slightly lower than that same woman's wage before entering motherhood.

Interestingly, the pre-treatment wages of future mothers are equal or even slightly higher than their control groups' once we apply our matching procedures. This finding indicates a negative selection into motherhood based on observable characteristics and a positive selection based on unobservable characteristics with respect to the wage level. As a result, the before-after comparison of the wage rates of mothers and non-mothers yields even larger wage cuts due to motherhood and a career interruption. Our establishment-specific information provides further insight into the sorting of women into establishments. Since the ATT is significantly larger as soon as we ignore establishment-specific effects by comparing dizygotic twin colleagues, we conclude that women who plan to have children are more likely to work in companies with lower wage growth rates, be it because they anticipatorily sort into these establishments or because of sorting into motherhood after realizing the flatter wage profile. As a consequence, studies ignoring these establishment-specific effects are likely to overestimate the true costs of entering

motherhood. Note also that this revealed establishment segregation is consistent and adds to the literature on occupational sex segregation, where women in general are found to be more likely to work in occupations with lower short-term wage penalties for family-related career interruptions.[17]

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[17] Görlich and de Grip (2009, in this issue), for example, show this for Germany.

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Appendix

Figure A1 illustrates the predicted linear index from the propensity score estimation for the sample of mothers (black line) and the sample of all possible control persons (dashed line). The likelihoods of entering motherhood and taking parental leave in the group of mothers and the potential control group do not overlap over the whole range of values.
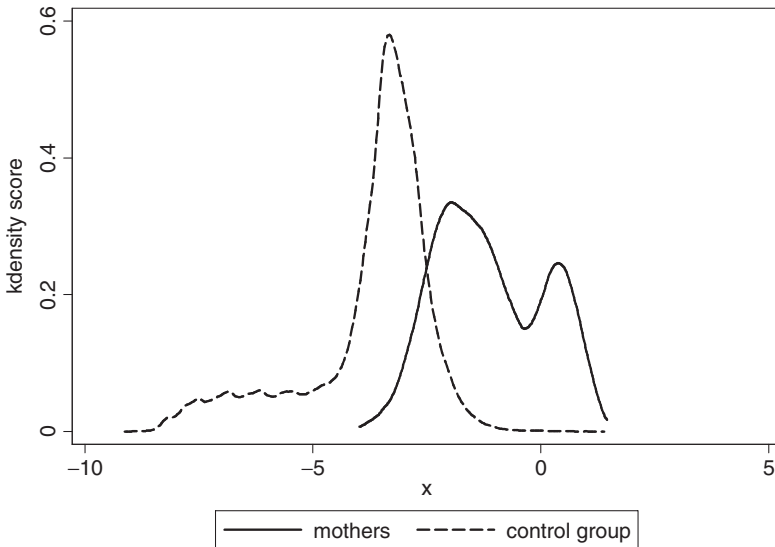

Fig. A1. Kernel densities of the propensity scores of all mothers and possible controls.

Note: Propensity scores are based on the estimation results presented in Table 1.
Source: Total sample of 561 mothers (childbirth between 1987 and 1995) and 215,819 female colleagues (employed full time in t_{+3} and without employment breaks of longer than 90 days), drawn from the IAB Employment Sample, IAB Employment Supplement Sample, Employment Statistics Register.