DISCUSSION PAPER SERIES

IZA DP No. 3820

The Effects of Maternity Leave Extension on Training for Young Women

Patrick A. Puhani
Katja Sonderhof

November 2008

Forschungsinstitut
zur Zukunft der Arbeit
Institute for the Study
of Labor

The Effects of Maternity Leave Extension on Training for Young Women

Patrick A. Puhani
Leibniz Universität Hannover,
SIAW, University of St. Gallen,
ERMES, Université Paris II
and IZA

Katja Sonderhof
Leibniz Universität Hannover

Discussion Paper No. 3820
November 2008

IZA

P.O. Box 7240
53072 Bonn
Germany

Phone: +49-228-3894-0
Fax: +49-228-3894-180
E-mail: iza@iza.org

IZA Discussion Paper No. 3820
November 2008

ABSTRACT

The Effects of Maternity Leave Extension on Training for Young Women*

Using three representative individual-level datasets for West Germany, we estimate the effect of the extension of maternity leave from 18 to 36 months on young women's participation in job-related training. Specifically, we employ difference-indifferences identification strategies using control groups of older women and older women together with young and older men. We find that maternity leave extension negatively affects job-related training for young women, even if they do not have children, especially when the focus is on employer-arranged training. There is tentative evidence that young women partly compensated for this reduction in employer-arranged training by increasing training on their own initiative.

JEL Classification: J16, J24, J58, J83

Keywords: policy, evaluation, education

Corresponding author:

Patrick A. Puhani
Leibniz Universität Hannover
Institut für Arbeitsökonomik
Königsworther Platz 1
D-30167 Hannover
Germany
E-mail: puhani@aoek.uni-hannover.de

* This research was supported by German Research Foundation (DFG) within the project 'Labour Market Effects of Social Policy' which is part of the research initiative 'Flexibility in Heterogeneous Labour Markets'. We are grateful to Christian Dustmann, Lena Edlund, Dominique Meurs, Uta Schönberg, Alfonso Sousa-Poza and seminar participants at the Universities of Darmstadt, Hannover and Hohenheim, ERMES, Université Paris II, the 2008 EEA, EALE and German Economic Association meetings, and participants at the research initiative's IAB and ZEW meetings in Nuremberg and Mannheim, respectively, for helpful comments. All remaining errors are our own.

1. Introduction

Most industrialized countries have some form of maternal leave policy that effectively grants employment protection to women around childbirth. Arguments in favor of this employment protection refer to the well-being of both young children and their mothers. From a labor perspective, employment protection through maternity leave might increase the attachment of mothers to their employer or the labor force in general. However, it may also have the opposite effect in that maternity leave combined with maternity benefits can be seen as a subsidy to leave the labor market temporarily with potential long-term consequences.

Whereas previous studies on maternity leave with employment and wages as outcome variables have frequently discussed the role of human capital accumulation and depreciation, we know of no study relating human capital investments like training directly to maternity leave.[1] Therefore, in this paper, we estimate the effect of prolonged maternity leave on the human capital investments of women of working and childbearing age. To this end, we exploit the natural experiment of a 1992 extension in the employment protection (maternity leave) period in Germany from 18 to 36 months, which propelled Germany to the top position in the ranking of legislatively mandated maternity leave durations among industrialized countries.[2] To assess the effect of this reform on the human capital investments of young women workers, we draw on three individual-level datasets, all of which ask information on job-related training for women and men of different age groups.

[1] Present discounted value of earnings, of which wage profiles and employment histories are major ingredients, might be the most appropriate outcome variable for the financial impact of maternity leave. However, measurement of the impacts on overall lifecycle wage and employment profiles is complicated by the frequent lack of long panel data. Conversely, impacts on wages at a certain point in the lifecycle may fail to take account of effects like steepened wage profiles. For example, when women have to bear a higher share of the costs of firm-specific training because of extended maternity leave, their early-career wages may fall, although Hashimoto's (1981) model would predict that they will also reap a higher share of the returns later in their careers. Thus, without data on lifecycle wage profiles, estimates with wages as the outcome might be difficult to interpret.

[2] See http://www.childpolicyintl.org/issuebrief/issuebrief5table1.pdf

It is well-established empirically that women are generally less attached to the labor force than men and that they receive less job-related training. For example, Barron, Black and Loewenstein (1993) show that U.S. workers with weaker attachment to the labor market are allocated to jobs offering less training, while women are employed in positions associated with shorter durations of job-related training. Similarly, Royalty (1996) finds a significant relationship in the U.S. between the predicted probability of job turnover and the probability of receiving training. Thus, the fact that women change their job positions more frequently accounts for about one fourth of the gender gap in training. For Britain, Green (1991) analyzes the differences in job-related training between young women and young men and between older women and older men. In comparison to young men, young women have less than half the chances of receiving training, although no differences are found between older women and older men.

Although these studies do not explicitly relate maternity leave to the incidence of training for women, they implicitly raise the question of whether prolonged maternity leave might affect job-related training for young women. The effect on training might be negative because a very long maternity leave reduces a young woman's labor market attachment, at least for the duration of the leave. As a consequence, employers should be less likely to invest in young women's human capital and place them in career paths with less training. Theoretically, the opposite effect might also prevail: if employers are forced to reemploy a woman even after a long leave, they might make the best of the situation and make up for lost human capital through intensified training. In the end, it is an empirical question which effect predominates.

Previous research has analyzed the relationships between both maternity leave and labor force participation and maternity leave and wages. For instance, Waldfogel (1999) finds no negative effects for the Family and Medical Leave Act's (FMLA) introduction of a 12-week maternity leave on the wages or employment of young women. Hashimoto et al. (2004)

also find the effects to be negligible. Indeed, Waldfogel (1998) suggests that maternity leave may even increase young women's employment and wages, a finding corroborated by Zveglich and van der Meulen Rodgers's (2003) investigation of a similar reform in Taiwan that introduced an 8-week maternity leave. Nevertheless, these findings contrast with those of Lai and Masters (2005) for Taiwan, as well as with Gruber's (1994) finding of a negative effect on wages of variations in maternity benefits across the U.S. They also contrast with the results of European studies that use reforms or other control group designs with longer maternity leave periods (up to three years). Among these, Ondrich et al. (2003) and Lalive and Zweimüller (2005), based on data from Germany and Austria, respectively, find that extended maternity leave results in short-run reductions in labor supply, while Schönberg and Ludsteck (2007) estimate negative long-run effects on wages in Germany. Likewise, in an analysis of policy variation in a panel of European countries, Ruhm (1998) reports increased employment due to parental leave (de facto maternity leave) but lower wages.

The remainder of this paper is organized as follows. Section 2 provides an overview of maternity leave regulations in Germany, especially with respect to the 1992 reform investigated here. Section 3 describes the datasets and the research design, after which Section 4 presents the difference-in-differences estimates of the effects of maternity leave extension on job-related training for women of child-bearing age. Overall, these estimates show that the extension reduces training for young women, even for those who do not have children. A separate look a different types of training shows that it is particularly employer-arranged training that has been reduced by the extension of maternity leave. Point estimates suggest that young women are in return trying to compensate the reduction in employer-arranged training by increasing training on their own initiative. Section 5 concludes the paper.

2. Maternity Leave in Germany

The duration of maternity leave as guaranteed by law in Germany exceeds that of other industrialized countries. For example, only since 1993 have U.S. federal regulations given women the right to take a 12-week maternity leave from work, even though many firms previously had their own maternity leave schemes. In contrast, as early as 1952 Germany enacted the first law protecting mothers (*Mutterschutzgesetz*) with a mandated 12-week maternity leave, which was extended in 1965 to 14 weeks (*i.e.*, six weeks before and eight weeks after the predicted birth date). In 1979, this maternity leave duration was extended to an optional additional four months (decided on by the mother), and since 1986 the government has repeatedly increased the maximum duration of maternity leave (see Table 1), with the largest increase being the 1992 extension of the maximum duration from 18 to 36 months.[3]

One intention of policy makers when increasing the maximum maternity leave duration was to protect women from unemployment following the birth of a child. Another was to improve the welfare of children. Since public childcare facilities for children younger than three years of age are not generally available in Germany (having only recently gained broader political support in the western part of the country), all women are supposed to be given the opportunity to care for their children for up to three years.

By law, women also have the right to return to a job with their previous employer following maternity leave, not necessarily the same job but one comparable to that held before the leave. Nevertheless, not all women take this opportunity to return to the labor force. For example, Ondrich et al. (2003) and Weber (2004) find that a longer duration of maternity leave has a negative impact on the probability of women returning to the labor market, a finding also reported by Lalive and Zweimüller (2005) for Austria. For the U.S.,

[3] Since 1986, fathers have also been allowed to take part of the leave, but, according to the Federal Ministry of Families, Seniors, Women and Youth, only 1.5 percent of fathers make use of this opportunity.

Klerman and Leibowitz (1990) show that because of better childcare facilities and less maternity leave protection, mothers return to the labor market sooner than in the past. Similarly, Waldfogel and Berger (2004) report that more than 80% of U.S. women working before childbirth return to work within 12 months after childbirth, while 55% return within 12 weeks after childbirth. In Germany, however, only around 55% of all women working before a first birth return to the labor market within 24 months (Gustafsson *et al.*, 1996).

Figure 1 shows calculations of the average maternity leave durations for women working before childbirth based on biographical information from the German Socio-Economic Panel (GSOEP). In the first graph, we calculate the average period out of the labor force due to childbirth by adding the duration of formal maternity leave to the number of months after the leave until a mother was reemployed. In the second, we plot the average duration of maternity leave taken by mothers who return to work directly when the official maternity leave ends. The difference between the two lines is driven by the fact that in Germany many mothers stay at home with their children for many years, even after their maternity leave entitlement has run out. It should also be noted that we have very few observations (between 10 and 70 per year), so the numbers shown here have high sampling variance.

For both graphs, we have censored all durations at 36 months (the maximum maternity leave duration in Germany since 1992) because we are only interested in how far maternity leave extension drives career breaks up to that limit. As it turns out, maternity leave extension is associated with an increase in average career break durations due to childbirth. Keeping in mind the sampling variance, career break durations increased from around 20 months in the late 1980s to around 25 months in the early 1990s. If we only consider mothers who return to work directly following the official maternity leave (which may be for shorter periods than the legal limit), we observe a sharper increase in career breaks due to maternity leave, from around 5 to 10 months in the 1980s to between 15 and 20 months (and over 25

months in one estimate) in the 1990s. Moreover, the pattern in the curve of Figure 2, which outlines the increase in the share of time spent in official maternity leave by *all* young women aged 20 to 35 (excluding post-leave career breaks), is similar to that showing the length of official maternity leave. This figure also plots the development of fertility, which has declined somewhat but not dramatically over the last two decades, meaning that the extension of maternity leave has seemingly had no overwhelming effect on birth rates.

Thus, Figure 1 suggests that, *ceteris paribus*, mothers' labor force attachment decreases through the direct effect of maternity leave extension, especially for those women who return to the labor force within the first three years after childbirth. As it is difficult for employers to predict who will become a mother and when, all else being equal, the extension of the leave period has probably decreased the expected job attachment of all female employees at childbearing age, even though, as discussed later in Section 3, other factors besides maternity leave expansion might be impacting the labor supply of young women.

The literature also indicates that job-related training is likely to at least partly entail investment in firm-specific human capital. Theoretical results in Becker (1962) and Hashimoto (1981) raise the hypothesis that the reduction in young women's job attachment due to prolonged maternity leave will decrease firms' willingness to invest in job-related training for women of childbearing age (or at least reduce their willingness to bear the costs). Likewise, young women's willingness to invest in job-related training may also decrease due to a reduction in expected returns to that investment. Alternatively, young women may want to compensate the reduced willingness of employers to invest in their human capital, by undertaking more training on their own initiative. It is, however, an empirical question which effect dominates. We evaluate the impact of extended maternity leave on the incidence of job-related training for young women in the following.

3. Data and Methodology

The Treatment Group and Data Sets

From the employer's perspective, extension of the maternity leave period constitutes an increase in employment protection for women of childbearing age. That is, if increased protection rights for young women are not reflected in implicit or explicit contracts that compensate employers for young women's extended maternity leave, women of childbearing age can expect diminished employment opportunities, such as less job-related training (cf. Lazear, 1990). However, unlike Schönberg and Ludsteck (2007), who consider extended maternity leave a treatment for mothers only and use mothers subject to shorter maternity leave as controls to measure labor force participation and wages as outcomes, we are interested in extended maternity leave rights as a treatment that affects *all* women of childbearing age with job-related training as an outcome of that treatment. Therefore, in our research design, the treatment group consists of all women of childbearing age, defined as those between 20 and 35 years of age. We exclude women between 36 and 39 because we cannot tell whether or not an employer perceives these women as being of childbearing age.[4]

In the subsequent analysis, we draw on three individual-level datasets that represent the West German workforce. East Germany was excluded because at the time of the reform, it was experiencing a major transition whose related factors are difficult to filter out from the effect of the maternity leave extension. In addition, the prereform points of observation are mostly from the 1980s when East Germany was under communist rule and thus excluded from the data. The three datasets used are the Report System [on] Further Education (*Berichtssystem Weiterbildung*, BSW)[5], the German Socio-Economic Panel (GSOEP),[6] and

[4] According to administrative birth records for Germany, 8.3 percent of all new mothers in 1990 were 36 years of age or older. This share is rising over time. For example, in the year 2000, it was already 11.5 percent. However, the share of all new mothers aged 40 or older is much lower at 1.8 and 2.5 percent in the years 1990 and 2000, respectively.

[5] More information on these data is available from the Central Archive for Empirical Social Research, University of Cologne web site: http://info1.za.gesis.org/DBKSearch12/SDesc.asp

the Qualification and Careers Survey (*Qualifikation und Berufsverlauf*, IAB-BIBB)[7]. A brief description of the datasets can be found in Appendix 2.

We restrict the sample to persons who are currently employed and hence attached to the labor market because by definition, persons not working cannot receive job-related training. Hence, we ignore the potential effect of extended maternity leave on training that works *directly* through (temporarily) reduced labor supply in order to focus on the effect for young women attached to the labor market (and thus potentially interested in job-related training). Nevertheless, because the three datasets we use measure the incidence of past job-related training for the last 1, 3 and 5 years (BSW, GSOEP, and IAB-BIBB, respectively), in the two of the datasets we cannot avoid capturing some of the potential direct effect through the reduced labor supply that results from maternity leave.

Despite differences in the size of the event window referred to by the various surveys, all three datasets exhibit a large degree of communality in training incidence, with training participation in the BSW and GSOEP varying between a quarter and a third (see Table 2). In the IAB-BIBB data, participation is somewhat higher (between a third and almost one half) because this survey asks for training during the previous 5 years (compared to 1 year in the BSW and 3 years in the GSOEP).

As Table 2 shows, all datasets report an increase in training participation over time, a finding that holds true for all age-gender groups. Moreover, consistent with the growing emphasis on lifelong learning, training participation increased more among older (aged 40–55) than younger workers (aged 20–35). Note, however, that in 1994, after the extension of maternity leave, young women had the lowest incidence of employer-arranged training but

[6] The GSOEP is probably the most frequently used individual-level data set for Germany. For more information, see http://www.diw.de/english/soep/29012.html

[7] The Qualification and Careers Survey (IAB-BIBB), which specializes in job descriptions, was also used by Spitz-Öner (2006). More information is available at http://www.gesis.org/ Datenservice/Themen/38Beruf.htm

the highest training incidence of training on the employee's initiative.[8] Neither of these facts held in 1988, before the extension of maternity leave. Formal testing of these before-after comparisons is carried out in Section 4.

Potential Control Groups

In tracking the development of job-related training of young women before and after the increase in the maternity leave period, we consider three demographic groups as reference points to construct control group designs: young men of similar age to the treatment group (20–35), women aged 40–55 years, and young men together with women and men aged 40–55. Similar treatment-control group designs are used in Gruber (1994), Ruhm (1998), Waldfogel (1999), and Lai and Masters (2005). We exclude persons older than 55 years from all analyses because this group's outcomes may be affected by other factors like early retirement, which may evolve differentially between men and women. In addition, training is less important to the older worker because the closer the retirement age, the lower the returns to investment.

Before comparing changes in training participation before and after the maternity leave extension for different age-gender groups, we check whether the extension of the maternity leave period did indeed lower young women's labor market attachment in relation to the potential control groups. This assessment is important because theory suggests that labor market attachment may be a key determinant of employers' willingness to support job-related training (Hashimoto, 1981). Likewise, observation of young women's labor force participation is important because general trends in female labor force participation may overlap with the effects of maternity leave on labor force participation and thus also influence job-related training. Hence, we must show an association between the German government's

[8] The question in the BSW asks whether job-related training was a) arranged by the company, b) arranged on the recommendation of a supervisor, or c) on your own initiative. We subsume answers a) and b) under "employer-arranged training".

extension of maternity leave duration and a decrease in young women's labor force participation relative to the control group. As Figure 1 has already shown, for young mothers, actual maternity leave periods have increased.

Figure 3 to Figure 5 profile the development of the full time equivalent (FTE) labor force participation rates of our treatment group (young women, irrespective of whether they are mothers) in relation to various controls. Because we restrict our sample to employees, self-employed are excluded; however, the graphs are robust to the inclusion of self-employed workers. We expect no abrupt change in labor market participation owing to maternity leave extension because hesitation to exploit the extended leave to its full extent is quite plausible in the face of uncertainty about how the employer will deal with this new situation. This view is borne out by the gradual increase in the average maternity leave period exhibited in Figure 1 and Figure 2.

Figure 3 shows the full time equivalent (FTE) labor force participation of young women relative to the older women controls in two datasets: the GSOEP and the Micro Census[9]. Even though the GSOEP's smaller sample size results in more erratic results than the Micro Census data, both datasets suggest that young women's labor force participation has decreased over the last two decades relative to that of older women. It should also be noted that the more reliable evidence from the Micro Census data suggests a much deeper decline in young versus older women's labor force participation in the late 1980s and early 1990s; that is, exactly during the period when maternity leave duration was massively extended (from 6 to 10 months in 1986, 12 months in 1988, 15 months in 1989, 18 months in 1990, and 36 months in 1992). This decline in relative participation is sizeable, at about 5 percentage points between the 1980s and 1993 according to the Micro Census. This steep

[9] The Micro Census (MZ) is a one-percent sample of the population (the scientific community receives only a 70 percent sample of that one percent) and asks similar questions to a census. For political reasons, there has been no census in Germany since 1987, so the Micro Census acts as a substitute. For more information, see http://www.destatis.de/jetspeed/portal/cms/Sites/destatis/Internet/EN/press/abisz/Mikrozensus__e,templateId=renderPrint.psml

downward trend flattens in the mid-1990s, although it remains negative despite no further reforms to maternity leave.

Figure 4 presents a comparison between young women and young men. Although the labor force participation of the former is lower than that of the latter, young women have seemingly been catching up over time. Nevertheless, the Micro Census data clearly suggest that the long-run trend in catching up with young men stalled after 1992 (when the maternity leave period was doubled from 18 to 36 months) until about 2000. Hence, the short time series presented here is consistent with a permanent reduction in the labor force attachment of young women relative to their male peers. Taking into account that this reduction overlaps with an upward trend that dominates the data, we expect no decrease in young women's job-related training relative to young men. On the contrary, an increase is to be expected. This increase is actually observed in the data. However, because young men are not an adequate control group due to the trends observed here, we do not present the results with young men as the control group in this paper.[10]

The third alternative for the control group design compares young women to older women and relates this difference to young versus older men. Consequently, Figure 5 depicts the difference in the differences of FTE labor force participation rates between young and older women and young and older men. This development is similar to that for the older women control group: young women's labor force attachment declines relative to older women, and the gap between young and older women declines in relation to the gap between young and older men. This pattern holds true during the period of maternity leave expansion and in the years after 1992 until the (positive) difference between these two gaps remains constant or even increases again from the late 1990s onwards. Therefore, we expect a

[10] In results not shown here, it turns out, however, that despite of the catch-up in labor supply of young women in relation to young men, young women have lost in terms of *employer-arranged* training in relation to young men after the extension of maternity leave. When considering training *in general*, however, they have caught up. Yet, consistent with the relative labor supply trends shown here, this catch-up in terms of training in general was slowed down in the period when maternity leave was extended (compared to a placebo period).

decrease in the relative incidence of job-related training for young women with this control group design.

Based on the control group designs just presented, we estimate two sets of regression equations. The first is an estimate of the difference in training incidence between young and older women before and after the 1992 reform:

$$training_{it} = \alpha + \beta_1 X_{it} + \beta_2 after_{it} + \beta_3 young_{it} + \tau_1 (young_{it} \times after_{tt}) + \varepsilon_{it} \quad (1)$$

where training is an indicator variable that is equal to 1 if training has occurred. 'Young' and 'after' are dummy variables indicating whether a women is young (20 to 35 years) and whether an observation refers to a post-1992 time point. The vector X denotes other control variables. In this difference-in-differences setup, the effect of interest is τ_1, which we expect to be negative because of the relative labor supply developments shown in Figure 3. If we have panel data (as in the GSOEP) instead of repeated cross sections (as in the other two datasets), we adjust standard errors for clustering (Bertrand, Duflo and Mullainathan, 2004).

If older women and young and older men are used as controls, we estimate a difference-in-difference-in-differences model using the following equation:

$$\begin{aligned} training_{it} = {} & \alpha + \beta_1 X_{it} + \beta_2 after_{it} + \beta_3 female_{it} + \beta_4 young_{it} \\ & + \beta_5 (female_{it} \times young_{it}) + \beta_6 (female_{it} \times after_{it}) + \beta_7 (young_{it} \times after_{it}) \quad (2) \\ & + \tau_2 (female_{it} \times young_{it} \times after_{it}) + \varepsilon_{it} \end{aligned}$$

with τ_2 as the coefficient of interest, which, as argued in Figure 5, is expected to be negative. The regression results are presented below.

4. Results

Before-After Estimates by Age and Gender

We formalize the comparison of changes in training incidence in providing before-after estimates for the four age-gender groups: young women as the treatment group and older women and young and older men as the controls (see Table 3). We estimated results for four types of specifications. First, as would be appropriate if the before-after comparison was not confounded by any compositional effects or if any compositional effects were the outcome of extending the maternity leave period, we used no control variables (*e.g.*, if young women invested less in education, education would be endogenous and thus should not be controlled for). We then successively increased the set of control variables in specifications 2 through 4, first by including dummy variables for education (*i.e.*, high school diploma/A-level/Abitur and a tertiary polytechnic or university degree) and controlling for age and age squared to account for possible changes in the age distributions within age groups. In specification 3, we also added job characteristics using dummy variables for full-time, white-collar, and civil servant employment. Finally, in specification 4 we incorporated dummy variables for civil status (*i.e.*, for being married and having children). Thus, those variables most likely to be endogenous were included last in the four specifications. In other words, we believed that family status and children might be affected by extended maternity leave, whose original intention was to facilitate women's work-life-family balance in order to increase fertility. If so, the civil status variables should not be included among the controls. Similar arguments might apply to the occupational and educational variables, but probably to a lesser extent. It turned out that controlling for these sets of variables had only a minor impact on the estimates. Hence, in the subsequent tables we only report estimates based on the specification with the full set of control variables.

As Table 3 shows, according to the BSW data, the smallest increase in training incidence between the 1988 and 1994 surveys (referring to training in 1987 and 1993,

respectively) is among young women. That is, the point estimate in specification 4 exhibits an increase in training participation of 5.7 percentage points, significant only at the 10 percent level, compared to a 6.1 percentage point estimate for young men, significant at the 5 percent level. The point estimates for older women and men are even larger and highly significant, at 8.8 and 10.1 percentage points, respectively.

Although the difference between the increases in training for young women and men seem rather small (5.7 versus 6.1 percentage points), this contrast becomes much more pronounced when we distinguish between different types of training (only possible consistently over time in the BSW data). Young women's probability to have taken part in employer-arranged training only increased by 2.5 percentage points (statistically insignificant), the number for young men, however, is 7.0 percentage points (significant at the 5 percent level). By contrast, young women seem to have partially compensated for this divergence by investing more in training on their own (rather than their employer's) initiative: the increases in the training incidences for this type of training are 4.5 percentage points for young women (significant at the 10 percent level), but only 1 percent for young men (insignificant). Hence, overall young women have not only experienced somewhat lower increases in training than young men, but also a change in the type of training they receive in relation to young men: the before-after estimates suggest that employers were less interested in the training of young women in relation to young men after the extension of maternity leave. What is striking is that the BSW data report similar (and significant) increases in employer-arranged training for three age-gender groups: young men (7.0 percentage points), older women (7.3 percentage points) and older men (6.2 percentage points), but not for young women (insignificant 2.5 percentage points). As a result, young women compensated this development by an increase in their own initiative to obtain training.

When we compare the BSW results with the other two datasets, we can only consider training incidence in general, but not by the type of training (employer-arranged or not). As

can be seen in Table 3, similar to the BSW data, the GSOEP and the IAB-BIBB data show an increase in training of older relative to younger workers (irrespective of gender). Hence, the BSW data seem to measure the same thing as the other two data sets. However, they give better information on the type of training.

Note that for training arranged by the employer, the "age effect" in the increase in training is not observed any more (that is to say, although older workers have higher *general* training increases than younger workers, they do not exhibit higher *employer-arranged* training increases than young workers). Because training arranged by the employer is more relevant than training in general, we will put special emphasis on the BSW data in the following, but use GSOEP and IAB-BIBB data for robustness checks.

Difference-in-Differences Estimates

As Table 4 demonstrates, job-related training is much more common among white-collar than among blue-collar workers (*e.g.*, 33 versus 13 percent in the 1988 BSW survey). Among white-collar workers, training participation is higher in larger than in very small firms (28 versus 17 percent in the 1988 BSW survey), perhaps because the latter find it more difficult to substitute for workers who are currently in training. Maternity leave reform should be more likely to have an impact on a group of workers with a high training incidence. We thus also report estimates where we restrict the sample to white-collar workers in firms with more than 20 employees to see whether the estimates for this subsample are more pronounced than those for all workers. Unfortunately, the information on firm size varies between datasets so that in the IAB-BIBB data, the firm-size limit must be set to 10 instead of 20 employees. In the BSW and GSOEP data, however, we are able to limit the sample to white-collar workers in firms with at least 20 employees.

Table 5 presents the difference-in-differences estimates for the three datasets and two control group designs.[11] As argued in connection with relative labor supply developments (see Section 3), we expect young women to lose in terms of training incidence relative to older women because of the (accelerated) decrease in their labor supply after maternity leave was extended. The point estimates in Table 5 generally confirm this hypothesis, and the findings are statistically significant for two of the three datasets (GSOEP and IAB-BIBB).The point estimates are -4.9, -13.5, and -9.6 percentage points for the BSW, GSOEP, and IAB-BIBB datasets, respectively.[12]

Additionally, because average training participation differs between datasets, we also provide estimates of the change in training participation for young women relative to the prereform level. The resulting estimates imply a relative decline in training participation by 19, 44, and 29 percent in the BSW, GSOEP, and IAB-BIBB datasets, respectively. When restricted to white-collar workers in firms with more than 20 employees, the effects are even larger at -6.4, -21.8, and -13.1 percentage points in the BSW, GSOEP, and IAB-BIBB datasets, respectively. Especially large and significant are the estimates in those datasets that refer to a longer event window, such as training in the previous 3 and 5 years (the GSOEP and IAB-BIBB, respectively). As pointed out previously, the longer the event window, the larger the estimates will be in absolute value in that they include the direct effect of prolonged maternity leave on job-related training through temporary reduction of the labor supply due to maternity leave. Moreover, although the BSW, which only refers to the previous year, also suggests a large effect (a 19 percent reduction in job-related training for young women, and also 19 percent reduction when the sample is restricted to white-collar workers in larger firms), the coefficient estimate is not significant. Therefore, we interpret

[11] Again, because control variables do not make a noteworthy difference to the estimates, we only report the specifications for the full set of control variables.

[12] As was the case with the before-after estimates, there is hardly any variation in the estimates across specifications with different control variables.

these estimates as only tentative evidence that extended maternity leave reduces the incidence of job-related training for young women *in general* (below, we will see that there is ample evidence that *employer-arranged* training has been reduced).

In a second set of estimates using older women and young and older men as controls, we use a difference-in-difference-in-differences strategy to compare the changes in training incidence of young versus older women in relation to the changes of young versus older men. Based on the relative labor supply behavior reported earlier (see Figure 5), we expect negative estimates for τ_2, a hypothesis confirmed by all point estimates (see Table 5, last column). For the BSW, GSOEP, and IAB-BIBB datasets, respectively, the point estimates suggest a -1.8,
-5.5, and -2.0 percentage point change in young women's training participation. The estimates are not statistically significant. When restricted to white-collar workers in firms with more than 20 employees, the corresponding estimates are -8.7, -14.6 (significant at the 10 percent level), and -4.9 in the BSW, GSOEP, and IAB-BIBB datasets, respectively.

Note that the just presented investigation of the impact of maternity leave reform on the incidence of any job-related training makes no distinction between types of training, which, unlike schooling, is poorly classified in most surveys. Nevertheless, unlike the other two datasets, the BSW data has information on whether training was arranged directly by the employer or taken on the employee's own initiative (information lacking in the other datasets).[13] We therefore apply the same estimates as above but distinguish between different types of training.

[13] The GSOEP provides information on these training aspects, but the questions are inconsistent across the years.

Employer-Arranged Training versus Training on the Worker's Initiative

Only the BSW data provide information on the role of the employer in job-related training. In the following, we calculate separate estimates for job-related training arranged directly by the employer and training on the employee's own initiative. The incidence of the two types of training for the four age-gender groups in the BSW data is reported in Table 2. Whereas in the first year of observation (1988), 26 percent of all workers in the sample received some type of job-related training, only 14 percent received training arranged by the employer.

Table 6 shows estimation results for these two types of training using the same control group designs as before. Again, we report two blocks of estimates, one for the full sample and one for white-collar workers in firms with more than 20 employees. The fact that only a few estimates are statistically significant may be due to the sample size. However, it should be noted that all the point estimates for employer-arranged training are negative. For the subsample of white-collar workers in larger firms, by contrast, all point estimates for training on the employee's initiative are positive (albeit not statistically significant), but smaller than the negative ones for employer-arranged training. Hence, young women seem at best to have partially compensated for the reduced interest in training by their employers.

For employer-arranged training, the point estimates indicate a reduction in young women's training participation of 4.6 or 7.0 percentage points depending on the control group. For young white-collar women, these estimates are larger and statistically significant, at 9.6 and 15.7 percentage points. In relation to the training incidence before maternity leave extension, these point estimates are huge, implying a reduction in employer-arranged training of between 35 and 54 percent for all young women and between 53 and 87 percent for young white-collar women in firms with more than 20 employees. Moreover, estimated increases in training on the employee's initiative lie between 20 and 43 percent. We must not overemphasize these large numbers because of the large standard errors attached to the estimates. Nevertheless, point estimates are in a similar range irrespective of the control

group chosen (for training on the employee's initiative this is only true if the sample is restricted to white-collar workers in larger firms).

Effects for Young Women Without Children

Maternity leave extension should affect women of childbearing age even if they currently have no children because they are at risk of leaving the employer for up to three years with a right to return. This risk is enough to make them part of the treatment group. Therefore, to check whether the results so far also apply to women without children, we repeat the estimates provided in Table 5 and Table 6 for young women who do not have children as treatment group (the control groups remain unchanged). These results, presented in Table 7 and Table 8, show very similar point estimates to those obtained for the full samples, which include young women with children. However, as shown in Table 8, in the BSW estimates for different types of training, none of the estimates remain statistically significant once young women with children are excluded (*cf.* Table 6). However, the point estimates still remain consistently negative and economically significant both for general training and for employer-arranged training, but positive in three out of four cases for training on the employee's initiative. The statistical insignificance may simply be the result of reduced sample size and correspondingly large standard errors. In sum, there is some evidence that maternity leave extension has reduced job-related training even for young women without children. Again it seems that this result is mainly driven by a reduction in employer-arranged training, which has only been partly compensated by training on young female employees' own initiative.

Placebo Estimates

The estimates so far seem to suggest that young women receive less employer-arranged job-related training because of the extension of maternity leave from 18 to 36 months.

Methodologically, we have relied on the difference-in-differences assumption that – in the absence of maternity leave extension – the training gap between treatment and control group would have remained constant. Because this identifying assumption is not testable and because there may be differential trends in training participation between treatment and control groups even in the absence of training, we carry out placebo estimates. This is to say, we estimate the same difference-in-differences models for a period in which no change in maternity leave took place. Because extensions have been frequent since 1979 and more data are available for recent years, we choose a postreform period for such estimates. However, owing to data availability constraints, we can only use 1997 and 2003 data from the BSW and 2000 and 2004 data from the GSOEP and must exclude the IAB-BIBB, whose last two waves occurred in 1991 and 1998.

The placebo estimation results for the three categories of training in the BSW data are provided in Table 9 for both the full sample and the subsample of white-collar workers in firms with more than 20 employees. These placebo estimates correspond to the results for the extension of maternity leave given in Table 6. Whereas in Table 6 the estimates for employer-arranged training are all negative and somewhat similar in magnitude, with half of them being statistically significant, none of the placebo estimates in Table 9 are simultaneously negative and statistically significant. In general, the placebo test is not so convincing when all workers – including blue-collar – are considered (upper panel of Table 9). There are, however, clear contrasts in the point estimates of the reform and placebo periods both for training in general and for employer-arranged training when we restrict the sample to white-collar workers in larger firms, *i.e.* the group of workers for whom job-related training seems most relevant. This comparison provides further support for the hypothesis that young women's participation in training has been held back by the maternity leave extension. Concerning training on the employee's initiative the point estimates in the placebo time period are similar to the ones for the period when maternity leave was extended. Hence,

the evidence that young women partly compensated the reduction in employer-arranged training by their own initiative is only tentative.

The placebo estimates based on the GSOEP also support the assumption that our previous results on reduced training for young women due to extended maternity leave were not spurious. Whereas our GSOEP-based estimates using older women and older women together with young and older men as controls were significantly negative (see Table 5), the corresponding placebo estimates, given in Table 10, are all positive and insignificantly different from zero. Moreover, as the table shows, these placebo results hold for both the full sample and white-collar workers in firms with more than 20 employees.

5. Conclusion

Even though policies that support the family-work balance are contentious on both sides of the Atlantic, maternity leave that guarantees a post-leave right to return to work is an important component of family policies. Whereas some countries like the United States opt for very short maternity leave periods (*i.e.*, 12 weeks), Germany lies at the other extreme, having extended maternity leave with a right to return to work with the same employer from 18 to 36 months, which ranks in the highest maternity leave durations in industrialized countries. In this paper, we use difference-in-differences estimates to investigate the effect of this extension on the human capital investment of young women workers.

Specifically, drawing on three individual-level datasets that represent West German workers, we measure participation in job-related training as a proxy for human capital investment, taking care to consider long-term trends in labor force participation when interpreting our difference-in-differences estimates using alternative control groups. Similar to the previous literature, we choose older women and older women together with young and older men as control groups. We particularly focus on one dataset which distinguishes between employer-arranged training and training on the employee's initiative.

We find significant evidence that maternity leave extension reduced employer-arranged training for young women. There is also tentative evidence that young women partly compensated for this reduction in employer-arranged training by undertaking more training on their own initiative.

Taken together with extant findings on extended maternity leave in European countries, our results point to negative economic consequences of protective measures like maternity leave of up to three years (as in Germany) for all young working women, even those without children. These negative effects must be weighed against the potential job security benefits for those who become mothers and the potential benefits for their children. However, as Dustmann and Schönberg's (2008) regression discontinuity estimates illustrate, this latter may be close to zero.

References

Barron, J.M., D.A. Black and M.A. Loewenstein (1993) Gender Differences in Training, Capital, and Wages. Journal of Human Resources 28: 343-364.

Becker, G. (1962) Investment in Human Capital: A Theoretical Analysis. Journal of Political Economy 70: 9-49.

Bertrand, M., E. Duflo and S. Mullainathan (2004) How Much Should We Trust Differences-in-Differences? Quarterly Journal of Economics 119: 249-275.

Dustmann, Ch. and U. Schönberg (2008) The Effect of Expansions in Leave Coverage on Children's Long-Term Outcomes. IZA Discussion Paper No. 3605, Bonn.

Green, F. (1991) Sex Discrimination in Job-Related Training. British Journal of Industrial Relations 29: 295-304.

Gustafsson, S.S., C.M.M.P. Wetzels, J.D. Vlasblom and S. Dex (1996) Women's Labor Force Transitions in Connection with Children: A Panel Data Comparison Between Germany, Sweden and Great Britain. Journal Population Economics 9: 223-246.

Gruber, J. (1994) The Incidence of Mandated Maternity Benefits. American Economic Review 84: 622-641.

Hashimoto, M. (1981) Firm-Specific Human Capital as a Shared Investment. American Economic Review 71: 475-482.

Hashimoto, M., R. Percy, T. Schoellner and B.A. Weinberg (2004) The Long and Short of It: Maternity Leave Coverage and Women's Labor Market Outcomes. IZA Discussion Paper No. 1207, Bonn.

Klerman, J.A. and A. Leibowitz (1990) Child Care and Women's Return to Work After Childbirth. American Economic Review 80: 284-288.

Kreyenfeld, M. (2001) Employment and Fertility – East Germany in the 1990s. Rostock University, doctoral thesis.

Lai, Y.-Ch. and S. Masters (2005) The Effects of Mandatory Maternity and Pregnancy Benefits on Women's Wages and Employment in Taiwan, 1984-1996. Industrial and Labor Relations Review 58: 274-281.

Lalive, R. and J. Zweimüller (2005) Does Parental Leave Affect Fertility and Return-to-Work? Evidence from a "True Natural Experiment". IZA Discussion Paper No. 1613, Bonn.

Lazear, E. (1990) Job Security Provisions and Employment. Quarterly Journal of Economics 105: 699-726.

Ondrich, J., C.K. Spiess, Q. Yang and G.G. Wagner (2003) The Liberalization of Maternity Leave Policy and the Return to Work after Childbirth in Germany. Review of Economics of the Household 1: 77-110.

Royalty, A.B. (1996) The Effects of Job Turnover on the Training of Men and Women. Industrial and Labor Relations Review, 49: 506-521.

Ruhm, Ch. J. (1998) The Economic Consequences of Parental Leave Mandates: Lessons from Europe. Quarterly Journal of Economics 113: 285-317.

Schönberg, U. and J. Ludsteck (2007) Maternity Leave Legislation, Female Labor Supply, and the Family Wage Gap. IZA Discussion Paper No. 2699, Bonn.

Spitz-Öner, A. (2006) Technical Change, Job Tasks and Rising Educational Demands: Looking Outside the Wage Structure. Journal of Labor Economics 24: 235-270.

Waldfogel, J. (1998) The Family Gap for Young Women in the United States and in Britain: Can Maternity Leave make a Difference? Journal of Labor Economics 16: 505-545.

Waldfogel, J. (1999) The Impact of the Family and Medical Leave Act. Journal of Policy Analysis and Management 118: 281-302.

Waldfogel, J. and L.M. Berger (2004) Maternity Leave and the Employment of New Mothers in the United States. Journal of Population Economics 17: 331-349.

Weber, A.M. (2004) Wann kehren junge Mütter auf den Arbeitsmarkt zurück? Eine Verweildaueranalyse für Deutschland [When Do Young Mothers Return to the Labor Market, A Duration Analysis for Germany, in German]. ZEW Discussion Paper No. 04-08, Mannheim.

Wohn, K. (2007) Effizienz von Weiterbildungsmessung [Efficiency of Measuring Training Participation, in German]. Rat für Sozial- und Wirtschaftsdaten [German Council for Social and Economic Data], Working Paper No. 19.

Zveglich, Jr., J.E. and Y. van der Meulen Rodgers (2003) The Impact of Protective Measures for Female Workers. Journal of Labor Economics 21: 533-555.

Table 1: Increase of Maximum Maternity Leave Duration

Year	Duration	Maternity leave
1979	4 months	Introduction of a 4-months maternity leave, which can be taken in addition to the 14 weeks retention period. Maternity benefits up to 750 deutschmarks (about €375) per month) paid by the government
1986	10 months	Maternity leave can be taken by mother or father. Both are allowed to work for up to 19 hours per week. Maternity leave can be exchanged once between mother and father. Less than 2 percent of men take this opportunity. Parental benefits of 600 deutschmarks (about €300) per month paid for 10 months by the government
1988	12 months	Duration of maternity leave is extended to 12 months. Parental benefits of 600 deutschmarks (about €300) per month paid for 12 months by the government
1989	15 months	Duration of maternity leave is extended to 15 months. Parental benefits of 600 deutschmarks (about €300) per month paid for 15 months by the government
1990	18 months	Duration of maternity leave is extended to 18 months. Parental benefits of 600 deutschmarks (about €300) per month paid for 18 months by the government
1992	36 months	Duration of maternity leave is extended to 36 months. Demand for maternity leave can be exchanged three times between mother and father. Parental benefits of 600 deutschmarks (about €300) per month paid for 24 months by the government

Source: Kreyenfeld (2001).

Table 2: Descriptive Statistics: Training Participation

a) All datasets	**BSW**		**GSOEP**		**IAB-BIBB**	
	1988	1994	1989	2000	1991	1998
All	0.26	0.34	0.30	0.36	0.35	0.42
Young Women	0.26	0.30	0.31	0.35	0.33	0.37
Older Women	0.18	0.31	0.16	0.34	0.26	0.39
Young Men	0.32	0.36	0.38	0.38	0.37	0.39
Older Men	0.25	0.36	0.32	0.37	0.39	0.51
n	3,112	2,147	2,764	5,639	16,682	17,564

b) Detailed information only in BSW	**BSW** Training arranged by employer		**BSW** Training on one's own initiative	
	1988	1994	1988	1994
All	0.14	0.19	0.12	0.14
Young Women	0.13	0.15	0.13	0.16
Older Women	0.09	0.16	0.10	0.14
Young Men	0.16	0.22	0.15	0.14
Older Men	0.16	0.22	0.08	0.14
n	3,112	2,147	3,112	2,147

Source: Report System Further Education (BSW); German Socio-Economic Panel (GSOEP); Qualification and Careers (IAB-BIBB); own calculations.

Table 3: Before-After Estimates

a) All datasets

BSW - Training in general	
Young Women	0.057*
n=1,188	(0.031)
Older Women	0.088***
n=1,016	(0.029)
Young Men	0.061**
n=1,405	(0.029)
Older Men	0.101***
n=1,456	(0.027)
GSOEP - Training in general	
Young Women	0.002
n=1,716	(0.030)
Older Women	0.128***
n=1,849	(0.024)
Young Men	-0.004
n=2,175	(0.028)
Older Men	0.039*
n=2,539	(0.023)
IAB-BIBB - Training in general	
Young Women	0.012
n=7,513	(0.012)
Older Women	0.104***
n=6,823	(0.012)
Young Men	0.006
n=9,560	(0.011)
Older Men	0.085***
n=10,072	(0.010)

b) Detailed information only in BSW data

BSW - Training arranged by employer	
Young Women	0.025
n=1,188	(0.024)
Older Women	0.073***
n=1,016	(0.022)
Young Men	0.070***
n=1,405	(0.025)
Older Men	0.062***
n=1,456	(0.024)
BSW - Training on one's own initiative	
Young Women	0.045*
n=1,188	(0.027)
Older Women	0.047***
n=1,016	(0.023)
Young Men	0.010
n=1,405	(0.024)
Older Men	0.066***
n=1,456	(0.020)

Note: *, ** and *** denote significance at the 10%, 5% and 1% level, respectively. The estimates are based on regressions with the following set of control variables: age, age squared, dummy variables for high school (A-level, *Abitur*) and tertiary degrees, dummy variables for full-time employment, white-collar job, civil-service employment, for being married and for having children. Because control variables only have a minor impact on these estimates, we only report the results with the full set of controls.

Source: Report System Further Education (BSW); German Socio-Economic Panel (GSOEP); Qualification and Careers (IAB-BIBB); own calculations.

Table 4: Training Participation for Subgroups

	Blue-collar worker	White-collar worker	White-collar worker in firms with more than 20 employees	White-collar worker in firms with less than 20 employees
BSW	0.13	0.33	0.28	0.17
GSOEP	0.13	0.41	0.34	0.16
IAB-BIBB	0.20	0.44	0.37	0.25

Note: Figures refer to the survey years before the reform: 1988 (BSW), 1989 (GSOEP) and 1991 (IAB-BIBB).
Source: Report System Further Education (BSW); German Socio-Economic Panel (GSOEP); Qualification and Careers (IAB-BIBB); own calculations.

Table 5: Difference-in-Differences Estimates

	DiD Control group: Older women	DiDiD Control group: Older women and all men
BSW		
Full sample	-0.049	-0.018
	(0.042)	(0.057)
Relative deviation	-0.19	-0.07
n	2,204	5,065
White-collar workers in firms with more than 20 employees	-0.064	-0.087
	(0.058)	(0.082)
Relative deviation	-0.19	-0.26
n	1,378	2,873
GSOEP		
Full sample	-0.135***	-0.055
	(0.038)	(0.052)
Relative deviation	-0.44	-0.18
n	3,508	8,146
White-collar workers in firms with more than 20 employees	-0.218***	-0.146*
	(0.056)	(0.080)
Relative deviation	-0.51	-0.34
n	1,991	4,362
IAB-BIBB		
Full sample	-0.096***	-0.020
	(0.017)	(0.022)
Relative deviation	-0.29	-0.06
n	14,336	33,968
White-collar workers in firms with more than 10 employees	-0.131***	-0.049
	(0.024)	(0.032)
Relative deviation	-0.31	-0.11
n	8,448	18,216

Note: *, ** and *** denote significance at the 10%, 5% and 1% level, respectively. The estimates are based on regressions with the following set of control variables: age, age squared, dummy variables for high school (A-level, *Abitur*) and tertiary degrees, dummy variables for full-time employment, white-collar job, civil-service employment, for being married and for having children. Because control variables only have a minor impact on these estimates, we only report the results with the full set of controls.

Source: Report System Further Education (BSW); German Socio-Economic Panel (GSOEP); Qualification and Careers (IAB-BIBB); own calculations.

Table 6: Difference-in-Differences Estimates – Results for Different Types of Training – BSW

	DiD	**DiDiD**
	Control group:	Control group: Older women and all
Full sample	Older women	men
Job-related training	-0.049	-0.018
(general)	(0.042)	(0.057)
Relative deviation	-0.19	-0.07
Job-related training	-0.046	-0.070
(arranged by employer)	(0.033)	(0.047)
Relative deviation	-0.35	-0.54
Job-related training	-0.003	0.049
(on one's own initiative)	(.035)	(.043)
Relative deviation	-0.02	0.38
n	2,203	5,065
White-collar workers in firms with more than 20 employees		
Job-related training	-0.064	-0.087
(general)	(0.058)	(0.082)
Relative deviation	-0.19	-0.26
Job-related training	-0.096**	-0.157**
(arranged by employer)	(.047)	(.077)
Relative deviation	-0.53	-0.87
Job-related training	0.030	0.065
(on one's own initiative)	(.049)	(.065)
Relative deviation	0.20	0.43
n	1,378	2,873

Note: *, ** and *** denote significance at the 10%, 5% and 1% level, respectively. The estimates are based on regressions with the following set of control variables: age, age squared, dummy variables for high school (A-level, *Abitur*) and tertiary degrees, dummy variables for full-time employment, white-collar job, civil-service employment, for being married and for having children.
Source: Report System Further Education (BSW); own calculations.

Table 7: Difference-in-Differences Estimates for Young Women Without Children

	DiD Control group: Older women	DiDiD Control group: Older women and all men
BSW		
Full sample	-0.069	-0.024
	(0.049)	(0.063)
Relative deviation	-0.23	-0.08
n	1,712	4,568
White-collar workers in firms with more than 20 employees	-0.049	-0.060
	(0.067)	(0.088)
Relative deviation	-0.14	-0.17
n	1,106	2,598
GSOEP		
Full sample	-0.170***	-0.088
	(0.044)	(0.057)
Relative deviation	-0.45	-0.23
n	2,972	7,610
White-collar workers in firms with more than 20 employees	-0.246***	-0.173**
	(0.060)	(0.083)
Relative deviation	-0.52	-0.37
n	1,735	4,106
IAB-BIBB		
Full sample	-0.103***	-0.014
	(0.019)	(0.024)
Relative deviation	-0.28	-0.04
n	11,784	31,416
White-collar workers in firms with more than 10 employees	-0.133***	-0.044
	(0.026)	(0.035)
Relative deviation	-0.30	-0.10
n	7,101	16,869

Note: *, ** and *** denote significance at the 10%, 5% and 1% level, respectively. The estimates are based on regressions with the following set of control variables: age, age squared, dummy variables for high school (A-level, *Abitur*) and tertiary degrees, dummy variables for full-time employment, white-collar job, civil-service employment, for being married and for having children. Because control variables only have a minor impact on these estimates, we only report the results with the full set of controls.

Source: Report System Further Education (BSW); German Socio-Economic Panel (GSOEP); Qualification and Careers (IAB-BIBB); own calculations.

Table 8: Difference-in-Differences Estimates for Young Women Without Children – Results for Different Types of Training – BSW

	DiD	**DiDiD**
Full sample	Control group: Older women	Control group: Older women and all men
Job-related training (general)	-0.069	-0.024
	(0.049)	(0.063)
Relative deviation	-0.23	-0.08
Job-related training (arranged by employer)	-0.033	-0.056
	(0.040)	(0.052)
Relative deviation	-0.25	-0.43
Job-related training (on one's own initiative)	-0.035	0.029
	(0.039)	(0.048)
Relative deviation	-0.22	0.22
n	1,712	4,568
White-collar workers in firms with more than 20 employees		
Job-related training (general)	-0.049	-0.060
	(0.067)	(0.088)
Relative deviation	-0.14	-0.17
Job-related training (arranged by employer)	-0.062	-0.123
	(0.056)	(0.077)
Relative deviation	-0.34	-0.68
Job-related training (on one's own initiative)	0.011	0.057
	(0.053)	(0.069)
Relative deviation	0.06	0.32
n	1,106	2,601

Note: *, ** and *** denote significance at the 10%, 5% and 1% level, respectively. The estimates are based on regressions with the following set of control variables: age, age squared, dummy variables for high school (A-level, *Abitur*) and tertiary degrees, dummy variables for full-time employment, white-collar job, civil-service employment, for being married and for having children.
Source: Report System Further Education.

Table 9: Placebo Tests – BSW

	DiD Control group: Older women	**DiDiD** Control group: Older women and all men
Full sample		
Job-related training (general)	-0.007	-0.096
	(0.051)	(0.071)
Job-related training (arranged by employer)	0.024	-0.042
	(0.045)	(0.061)
Job-related training (on one's own initiative)	-0.035	-0.055
	(0.040)	(0.057)
n	1,817	3,736
White-collar workers in firms with more than 20 employees		
Job-related training (general)	0.088	0.001
	(0.070)	(0.099)
Job-related training (arranged by employer)	0.069	-0.014
	(0.065)	(0.092)
Job-related training (on one's own initiative)	0.016	0.017
	(0.055)	(0.084)
n	1,075	2,128

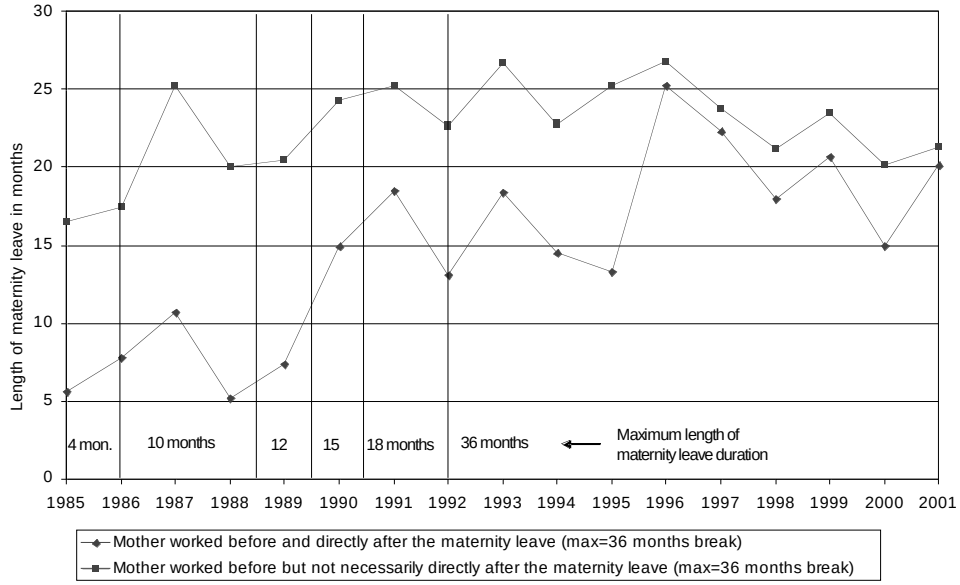
Note: *, ** and *** denote significance at the 10 %, 5 % and 1 % level, respectively. The estimates are based on regressions with the following set of control variables: age, age squared, dummy variables for high school (A-level, *Abitur*) and tertiary degrees, dummy variables for full-time employment, white-collar job, civil-service employment, for being married and for having children.
Source: Report System Further Education (BSW); own calculations.

Table 10: Placebo Tests – GSOEP

	DiD Control group: Older women	**DiDiD** Control group: Older women and all men
Full sample		
Job-related training	0.029	0.023
(general)	(0.034)	(0.054)
n	4,232	9,426
White-collar workers in firms with more than 20 employees		
Job-related training	0.059	0.033
(general)	(0.057)	(0.078)
n	2,478	5,207

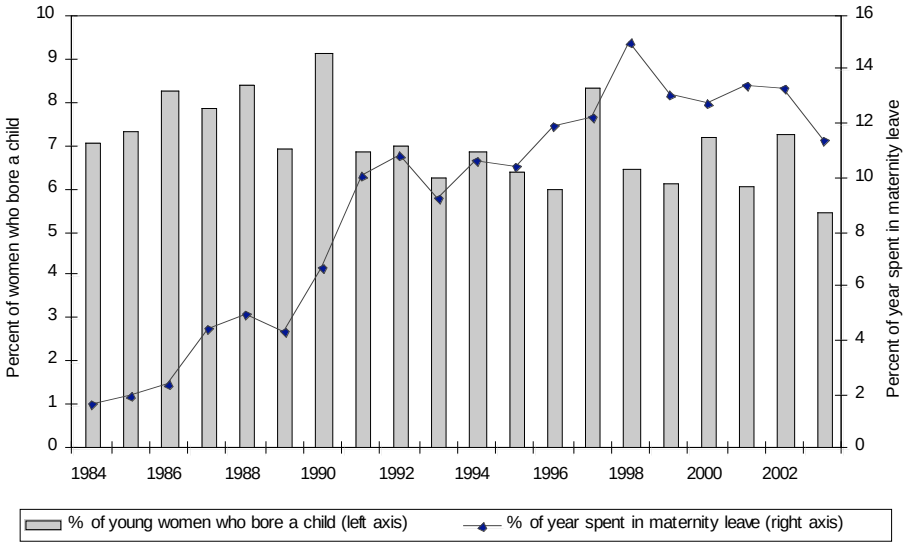
Note: *, ** and *** denote significance at the 10 %, 5 % and 1 % level, respectively. The estimates are based on regressions with the following set of control variables: age, age squared, dummy variables for high school (A-level, *Abitur*) and tertiary degrees, dummy variables for full-time employment, white-collar job, civil-service employment, for being married and for having children.
Source: German Socio-Economic Panel (GSOEP); own calculations.

Figure 1: Average Length of Maternity Leave Taken



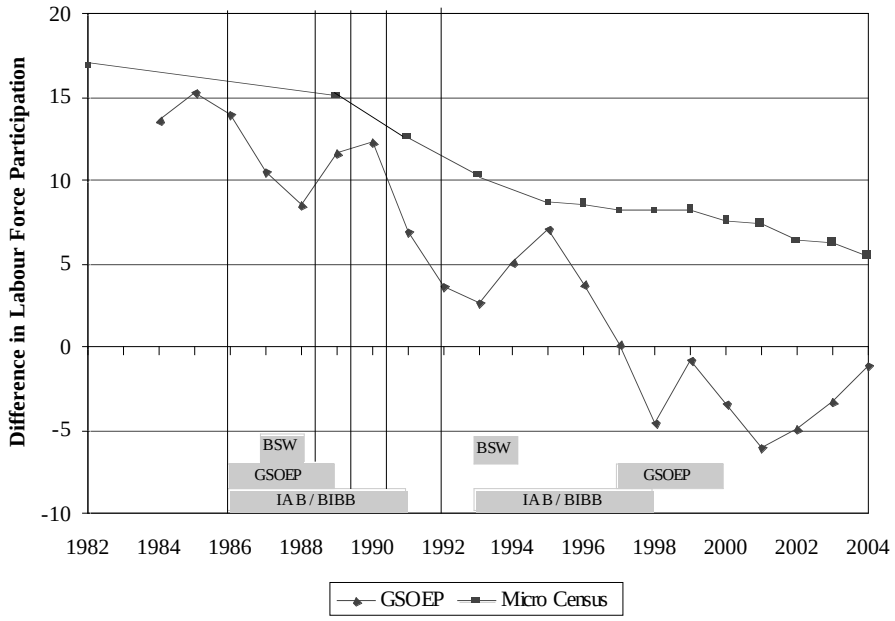
Note: All durations longer than 36 months were censored to 36 months. Vertical lines show the timing of the reforms to increase maternity leave duration. The length of maternity leave is measured in months for women between 20 and 35 years of age who started their maternity leave in the year before the interview. In the top line, we add the durations of official maternity leave and post maternity leave career breaks, which are common in Germany. The lower line only considers official maternity leave for mothers who return to the labor market directly after their official maternity leave.
Source: German Socio-Economic Panel (GSOEP); own calculations.

Figure 2: Young Women's Labor Force Participation and Birth Rates



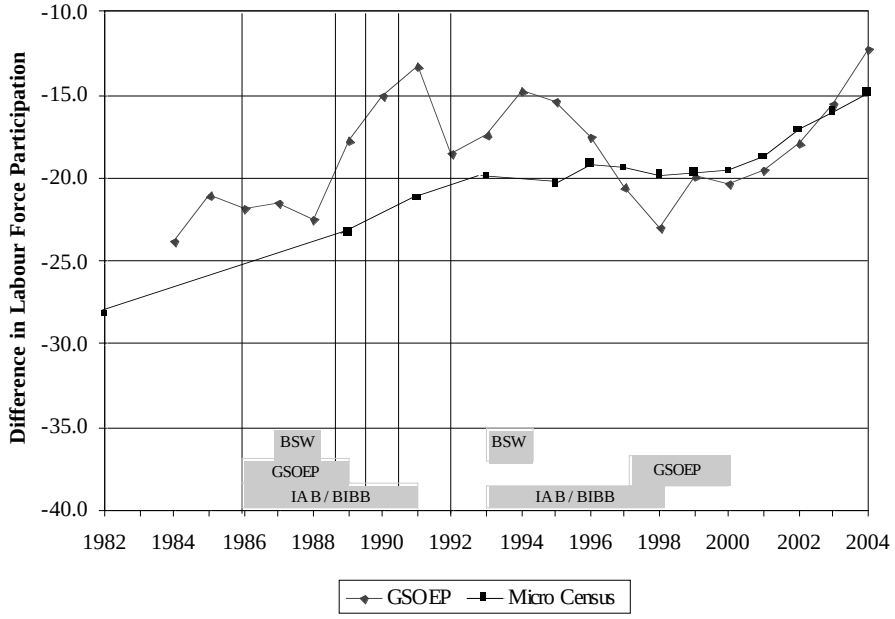
Note: These results refer to all women aged between 20 and 35 years. Percentage rate of year spent in maternity leave gives an idea of how long young women, on average, are absent due to maternity leave each year.
Source: German Socio-Economic Panel (GSOEP); own calculations.

Figure 3: **Difference Between Young and Older Women's Labor Force Participation – Full-time Equivalents**



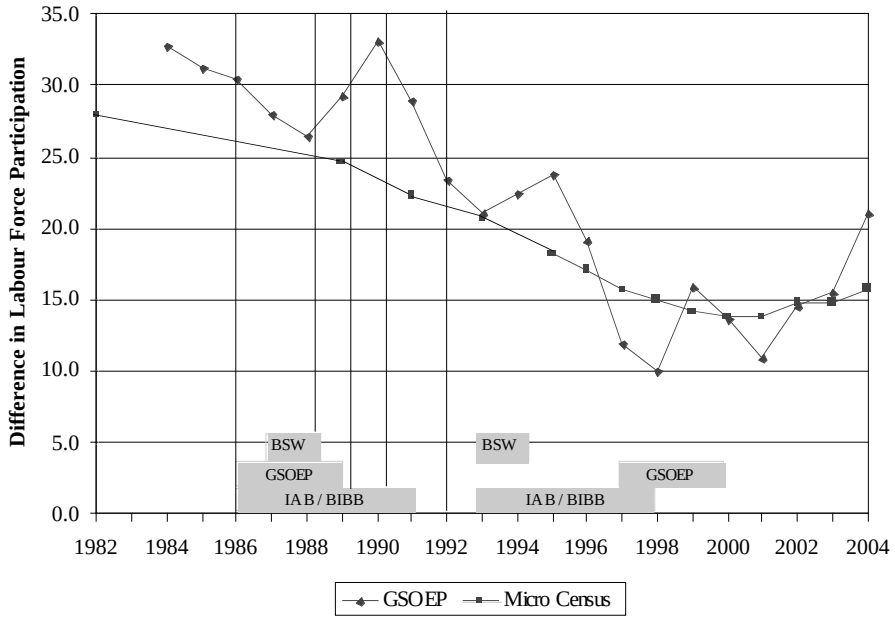
Note: The boxes at the bottom of the graphs indicate the event windows referred to in the training questions in the respective surveys. As mentioned in the text, the BSW refers to job-related training in the previous year, whereas the GSOEP and IAB-BIBB data refer to the previous three and five years, respectively. Vertical lines show the timing of the reforms to increase maternity leave duration.
Source: Micro Census (MZ); German Socio-Economic Panel (GSOEP); own calculations.

Figure 4: Difference Between Young Women's and Young Men's Labor Force Participation – Full-time Equivalents



Note: The boxes at the bottom of the graphs indicate the event windows referred to in the training questions in the respective surveys. As mentioned in the text, the BSW refers to job-related training in the previous year, whereas the GSOEP and IAB-BIBB data refer to the previous three and five years, respectively. Vertical lines show the timing of the reforms to increase maternity leave duration.
Source: Micro Census (MZ); German Socio-Economic Panel (GSOEP); own calculations.

Figure 5: Difference in Difference Young and Older Persons' Labor Force Participation Between Men and Women – Full-time Equivalents



Note: The boxes at the bottom of the graphs indicate the event windows referred to in the training questions in the respective surveys. As mentioned in the text, the BSW refers to job-related training in the previous year, whereas the GSOEP and IAB-BIBB data refer to the previous three and five years, respectively. Vertical lines show the timing of the reforms to increase maternity leave duration.
Source: Micro Census (MZ); German Socio-Economic Panel (GSOEP); own calculations.

Appendix 1: Summary Statistics

	BSW		GSOEP		IAB-BIBB	
	1988	1994	1989	2000	1991	1998
Training (All)	0.25	0.33	0.30	0.36	0.35	0.42
High school	0.20	0.25	0.20	0.30	0.18	0.26
University	0.15	0.17	0.12	0.20	0.16	0.14
Age	37.2	37.8	37.1	39	36.9	38.2
Age between 20 and 35	0.51	0.51	0.53	0.47	0.54	0.48
White-collar Worker	0.53	0.53	0.53	0.61	0.51	0.56
Blue-Collar Worker	0.34	0.34	0.36	0.30	0.40	0.34
Civil Servant	0.12	0.13	0.11	0.10	0.09	0.10
Female	0.40	0.39	0.42	0.44	0.41	0.43
Married	0.68	0.66	0.58	0.55	0.75	0.74
Children	0.41	0.48	0.31	0.32	0.34	0.42
Working full-time	0.83	0.81	0.86	0.81	0.88	0.84
n	3,112	2,147	2,764	5,639	16,682	17,564

Source: Report System Further Education (BSW); German Socio-Economic Panel (GSOEP); Qualification and Careers (IAB-BIBB); own calculations.

Appendix 2: Data Description

The three datasets used are the Report System [on] Further Education (Berichtssystem Weiterbildung, BSW), the German Socio-Economic Panel (GSOEP), and the Qualification and Careers Survey (Qualifikation und Berufsverlauf, IAB-BIBB).

The BSW is relatively unknown compared to the other datasets. The BSW survey was conducted seven times (1979, 1982, 1985, 1988, 1991, 1994, 1997, 2000 and 2003) by the Federal Ministry for Education and Research (*Bundesministerium für Bildung und Forschung*); data are provided by the Central Archive for Empirical Social Research, University of Cologne. Each survey year, about 7,000 persons between 19 and 64 years are interviewed orally (this includes employed and non-employed people). The BSW dataset is at present the only regular representative survey containing all kinds of training incidences in Germany.[14] In contrast to the other datasets, questions on training are the focus of this survey. We take the year 1988 as observations before and 1994 as observations after the reform. Questions on job-related training refer to the last 12 months.

The GSOEP is an individual-level dataset with panel structure. It is the largest representative longitudinal study of private households in Germany. The same private households, persons and families have been surveyed annually since 1984. In this dataset we have information on whether a person took part in job-related training in the last three years. Observations before the reform refer to 1989 and observations after the reform to the year 2000. The GSOEP has been conducted since 1984, but questions on job-related training started in 1989 and were only repeated in 1993, 2000, and 2004. We do not use 1993 because in asking for training during the last three years, this wave barely covers the 1992 reform.

The IAB-BIBB data are a representative survey of employed persons, which was conducted in 1985, 1991, and 1998. It focuses on job descriptions and detailed information

[14] For more information, please see:
http://www.bmbf.de/pub/berichtssystem_weiterbildung_9.pdf

on qualification profiles and occupational development. Each survey wave consists of more than 34,000 observations; questions on job-related training refer to the last five years.

Although there are some questions on job-related training in the German Micro Census (Mikrozensus, MZ), this dataset is not suitable for this analysis, because training participation is underrepresented there.[15] As pointed out by Wohn (2007) there are several reasons why training participation in the MZ is underrepresented compared to the BSW training participation. Since the other two datasets (GSOEP and IAB-BIBB) have comparable training incidences to the BSW, we focus on these three datasets in the regression analyses and use the Micro Census data only for descriptive analyses (see Figures 3 to 5).

The choice of datasets is driven by information on job-related training at the individual level both before and after the maternity leave extension of 1992. Because the treatment group comprises all women of childbearing age, actual information on maternity leaves was not required for a dataset to be used here. Nevertheless, problems do arise in the dataset comparison. First, all three datasets measure the outcome variable, job-related training, for a different period of time: the last five years in the IAB-BIBB data, the last three years in the GSOEP, and the last 12 months in the BSW. The second difficulty stems from the needs of our difference-in-differences analysis. Not only does it require training incidence observations before and after the maternity leave extension, but these can only be done properly by focusing on the most drastic reform, that which lengthens maternity leave from 18 to 36 months. However, the post-1992 reform surveys differ enormously in timing: 1994 for the BSW, 1998 for the IAB-BIBB, and 2000 for the GSOEP. Variation also exists in the timing of the pre-1992 reform surveys, which refer to the following years: BSW, 1988; GSOEP, 1989; and IAB-BIBB, 1991. Obviously, these differences must be taken into account. For example, by asking for training in the five years previous to 1991, the prereform

[15] In our analysis, training participation in the Micro Census was only less than half as high as in the other three datasets.

survey refers to a period during which three smaller extensions of maternity leave benefits occurred (see the grey-shaded boxes in Figures 3 to 5). The surveys also differ somewhat in their sample sizes, with the largest, the IAB-BIBB, containing more than 16,000 observations per wave. GSOEP and BSW are smaller, the former with over 2,700 observations in 1989 but more than 5,000 in 2000 because of refreshment samples, and the latter with more than 3,000 and 2,000 observations before and after the reform, respectively.