Does it Pay to be a Woman?
An Estimation of the Labour Market Effects of Maternity Leave Legislation

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Abstract

It is often considered to be unwise to hire a woman in childbearing age for a highly demanding job because of the pregnancy risk. As an employer faces costs if an employee takes leave, the employer will either be more reluctant to hire a woman or the woman has to pay a risk premium. Recent research on maternity leave legislation has, however, mainly focused on the effects of maternity leave legislation on mothers. This paper tries to fill this gap and discusses the effects of German maternity leave legislation on women's labour market situation in general. In detail, I investigate the impact of maternity-related job protection on women's wages and the probability of finding employment empirically by looking at discrete changes in German maternity leave legislation, using a difference-in-differences approach. My results indicate that women with a university degree have to bear a risk premium of 5%–7% of gross wages if job protection is extended by 1 year. I also find significant negative effects of changes in the job protection period on the probability of finding a job for this group. These effects remain even if previous maternity leave is explicitly controlled for.

Keywords: paternity leave, gender pay gap, education, hiring decision, job protection, unemployment

JEL-Codes: J16, J23, J31, J71, K36

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1 Introduction

Recent research on labour market effects of maternity leave legislation has mainly focused on the labour supply of mothers (e. g. Spiess and Wrohlich 2006; Dearing et al. 2007; Schönberg and Ludsteck, 2007).[1] This analysis is, however, too narrowly focused. Maternity leave legislation does not only affect the labour supply of mothers. On top of that it also affects the demand for and payment of female employees, because to an employer who decides about hiring a new employee, every woman is a potential mother.

The employer has to take into account expected future costs in case the employee takes leave following the birth of a child. Besides the pecuniary costs, additional costs may include a lower productivity after the employee returns, because her human capital has depreciated (Datta Gupta and Smith 2000; Görlich and de Grip 2007), or the additional training that has to be invested in a substitute employee (Ruhm 1998; Ondrich et al. 2002). This can be enhanced by the uncertainty about when the absent employee will return and whether she will return at all (Waldfogel 1998).[2] Especially in high-skilled jobs, these costs are non-negligible.

In Germany, the average employment rate of mothers who have children younger than 3 years of age is about 25% (e. g. Geyer and Steiner, 2007). Reasons can be found in cultural or institutional regimes, e. g. the lack of childcare (Kreyenfeld and Hank 2000; Büchel and Spiess 2002; Büchel and van Ham 2004). In recent years, this meant that a woman who gives birth drops out of the labour force for the following three years. After returning, she then experiences a wage penalty (Ondrich et al. 2002; Beblo et al. 2007). Recent research always touches on the fact that maternity leave legislation may in fact worsen women's position on the labour market (e. g. Ondrich et al. 1996), but then redirects the focus to mothers. In contrast, I show that every woman is affected by the legislation and has to pay a risk premium, irrespective of whether she will have a child or not.

This paper also relates to the literature on employment protection and lay-off costs. In particular, the extension of the statutory, job-protected leave period can be considered an exogenous change in lay-off costs. The effects on the probability of finding employment should therefore mainly be related to the changes in the job protection period, which is a part of maternity leave legislation.

In detail, this paper empirically investigates the impact of maternity-related job protection on women's wages and employment opportunities by looking at discrete changes in

[1] Ruhm (1998) is an exception to this.

[2] The period of actual leave-taking strongly depends on the length of the statutory job protection period, refer e. g. to Ondrich et al. (1996), Gottschall and Bird (2003), and Berger and Waldfogel (2004).

German maternity leave legislation. The job protection period was extended several times between 1986 and 1992. I use data from the German Socio-Economic Panel for the year before and after each reform for the analysis. Building on the experimental nature of the reforms, I use a difference-in-differences approach. Women aged 18-40 constitute the treatment group, while men of the same age serve as a control group, because only 2% of them take leave after the birth of a child (Gottschall and Bird 2003).[3] To check for the robustness of the results, I compare the treatment group to women of age 45 and above, who are supposedly out of childbearing age.[4] Furthermore, I restrict the sample to West German individuals.

The results show that the probability of finding employment after the reform in 1992 decreases by around 9% for the treatment group. A more pronounced and significant effect emerges for women with a university degree, both for the 1986 and the 1992 reform. Moreover, the 1992 change in legislation is associated with an average risk premium of about 5-7% of gross wages for women with a university degree. For women without a university degree, an effect only shows for the 1986 policy change. This effect is associated with about a 2% decrease in the probability of finding a job. The results also indicate that the wage penalty associated with the reforms is not only restricted to those women who actually take leave.

Section 2 relates maternity-related job-protection to recent models on job protection laws and lay-off costs. The data, the sample and a descriptive overview are presented in section 3, while section 4 contains the detailed identification and estimation strategy as well as the regression results. Section 5 concludes.

2 The Model

Employment protection laws and implied lay-off costs have long been discussed as one of the main reasons for labour market rigidities and unemployment in Europe (Saint-Paul 1997). Often, the emphasis of research on employment protection lies on the welfare effects of externally imposed lay-off costs, which are not part of the employer-worker bargaining process. General equilibrium models of employment protection come to ambiguous conclusions on the effects of job protection lawson average employment. Ljungqvist (2002) shows that the effect on equilibrium employment strongly depends on the underlying model of the labour

[3]This is also why I refer to maternity leave legislation instead of paternity leave legislation in this paper. Although men could theoretically take paternity leave, too, they just do not. When instead referring to paternity leave in this paper, I refer to the 2001 and 2007 reforms, which did not only change the semantics (*Elternzeit*), but were also targeted explicitly at fathers.

[4]This strategy is similar to Ruhm (1998).

market.

Maternity leave legislation contributes to the set of employment protection laws, as in many countries the period of maternity-related employment protection has been extended quite generously. Germany is one of the countries which grants the longest period of statutory, protected leave of currently 3 years. Along the lines of the literature of job protection, maternity-related job protection increases lay-off costs for an employer. A woman has to be given the same or an equivalent job if she returns from maternity leave, although her human capital has depreciated in the meantime. In addition, the employer has to hire a substitute employee for the time of leave. As this is typically associated with a temporary job contract for the substitute employee, suitable candidates are often hard to find. In addition, there may be a waste of firm-specific training, because the substitute employee will have to leave the firm again when the leave-taker returns. As a result, maternity leave legislation implicitly increases lay-off costs.

The employer would bear a higher risk when hiring a woman, and thus has to be compensated for it by paying lower wages to women. The additional costs could be internalised in the employer-employee bargaining process, such that the female employee would bear a higher risk premium. In contrast to Schmitz (2004) there are real costs associated with taking leave after the birth of a child, which would be paid by the employee who causes this under symmetric information. That is, unlike in the Schmitz (2004) case, under symmetric information there would not necessarily be a loss in total surplus, given that the decision on how long to take maternity leave is not distorted. Under asymmetric information, however, some agents who do not become pregnant do not cause a cost, but have to pay an average risk premium to the principal, because the principal can only observe the average pregnancy risk (Scheubel, 2008).

Hypothesis 1: The Risk Premium caused by Maternity Leave Legislation
Increased job protection after maternity leave increases the risk premium to be paid by affected agents. Under asymmetric information, this applies to all female employees.

What does Hypothesis 1 imply for the estimation of overall labour market effects of maternity leave legislation? First, hypothesis 1 restricts the effects to women, because in Germany men almost exclusively do not take up paternity leave at all. Second, the risk premium to be paid by women can either mean that they receive lower wages or that they are less likely to be hired. After all, a reduction in wages for women who are already employed and remain employed is unlikely. Still, a reduction in wages should be possible for new hires - if they are hired at all. The regression analysis therefore sheds light on the question whether

women who are not employed are more or less likely to find a job after the extension in the job protection period by one year. It also reveals whether women are paid less after the reform or whether this only had effects on wage growth.

> **Hypothesis 2: The Magnitude of the Risk Premium** The costs associated with an employee taking leave rise with the employees skill level, i. e. substitutability, and the extent of firm-specific training required to do the job. University graduates should therefore, on average, need to pay a higher risk premium.

Hypothesis 2 is only valid in this form if all skill groups take the same time of leave, because the costs associated with taking leave are obviously also related to the acutal period of absence. If a woman has a child in Germany, she takes the full leave period in most cases (e. g. Gottschall and Bird 2003; Büchel and van Ham 2004; Schönberg and Ludsteck 2007).

3 Data

3.1 The German Socio-Economic Panel

The German Socio-Economic Panel (SOEP) conveniently covers the periods of primary interest for my analysis: the years 1985 and 1986 as well as 1991 and 1992. The SOEP is an ongoing panel study of German households, which was started in 1984 (e. g. Wagner 1993), containing rich information on the labour market situation of the individuals. Information on an individuals' labour market situation is available on quite a detailed level. I use data from waves 1984 - 2006 from the German Socio-Economic Panel (SOEP).[5] The samples which I use for the analysis of the probability of finding employment and the analysis of wages differ. The first sample includes only persons who were unemployed in the previous period, while the second sample includes all persons who earn a wage, i. e. also persons who show longer spells of employment, to be able to implement a panel estimator. The analysis of wages is, however, also implemented for the first sample only, albeit in a pooled setting. The summary statistics in table 1 and table 2 are presented for 1992 for both samples.

Using SOEP data for the analysis however also implies some difficulties. The first one is that the reform most important for my analysis took place shortly after German reunification,

[5]The data used in this paper was extracted using the Add-On package PanelWhiz for Stata®. PanelWhiz (http://www.PanelWhiz.eu) was written by Dr. John P. Haisken-DeNew (john@PanelWhiz.eu). See Haisken-DeNew and Hahn (2006) for details. The PanelWhiz generated DO file to retrieve the data used here is available from me upon request. Any data or computational errors in this paper are my own.

which has had major repercussions on the labour market. In addition, these repercussions could have affected women differently than men, because of the higher participation rate of women in the former GDR. I aim to get around this mainly by only using SOEP Sample A, i. e. the original West German sample.

A second caveat is the information on education. For the analysis of the probability of finding employment, I restrict the sample to those individuals, who were previously unemployed. At the same time, only women in childbearing age should be affected by the reform of maternity leave legislation, which additionally restricts the sample to the young labour force aged 18 – 40. In this restricted sample, the number of university graduates is rather small. The sample size is sufficient when comparing women in childbearing age to men of the same age, but when reducing the sample further to women only for a comparison of women in childbearing age with women out of childbearing age, the sample of university graduates is too small. In these cases, I have to rely on the results of the comparison to men.

A third complication arises with the information on maternity leave spells. The explicit information on maternity leave spells is available in the SOEP only from 1990 onwards. It is important, however, to control for the negative wage effects after returning from maternity leave, and for the higher probability of re-entering employment because of the job protection law. For the analysis of the 1986 reform I therefore include dummy variables, which indicate whether a person had their first child in the previous year or in the current year. For the analysis of the probability of finding employment, this can approximate maternity leave for women, because if they had a child in the previous year, it is very likely that they also were on maternity leave. In the analysis of wages, this can capture the positive selection of women who quickly return to work after the birth of a child.

A word of caution is also required with regard to the information on wages. For the sake of comparability, I rely on harmonised data on gross monthly wages and monthly household income, which are provided in EUR. Moreover, I use imputed income and wage data.

3.2 Descriptive Overview

The data reveal that the usual differences in labour market status and wages between the genders are also present in the sample of the SOEP data. In addition, behavioural effects of maternity leave legislation also appear to be present. Around 60% of women without a university degree claim to be working (table 3).[6] This figure only seems to be rising significantly by the end of the millennium. Women with a university degree display a slightly

[6]Note that the figures are slightly higher than for the population as a whole because of the age restriction to the sample.

higher labour market participation, although the figures do not show the same positive trend towards the end of the millennium. Figures are lowest in the early 1990s at around 65%. This is much lower than in the previous years. The fall seems to be stronger than for women without a university degree. This can be interpreted as an indicator of the effects of a longer statutory leave period on the working behaviour of mothers.

In fact, the effect on mothers' working behaviour also seems to show in the type of job they choose. After returning to their jobs after a long leave period, they often prefer a part-time type of job. This may explain a drop in the number of working women in full-time jobs by the end of the 1990s for women with a university degree. The decrease in the percentage of women without a university degree in a full-time job seems to follow a more persistent downward trend.

But how do the reactions of mothers to the newly gained rights affect women in general? Do they have to pay for their colleagues' parental rights? In fact, the percentage of women with a university degree that has found a job if previously unemployed as dropped from 1991 to 1992, whereas the opposite is true for male university graduates (table 5). For the rest of the sample there is hardly any change. A drop of 4% in women with a university degree finding employment if previously unemployed is, however, not unusual, regarding the strong fluctuations in these figures. A check of the 'placebo' treatments, i. e. all years without a policy change, seems to be worth while. The drop is stronger, if only individuals are considered, who did not have a child in $t-1$, but the variation also remains in this sample.

Women's pay rose much less from 1991 to 1992 than men's pay, among individuals with a university degree (table 6). The change is also much stronger than the movements in the preceding years. It is, however, obvious that there are many explanations for fluctuations in wages or in wage growth. This makes the difference-in-differences strategy so appealing.

4 Results

4.1 Identification Strategy: A Difference-in-Differences Approach

The identification strategy exploits the fact that there were several exogenous changes to maternity leave legislation in Germany in the period covered by the SOEP, which creates a quasi-experimental setting that can be used for a difference-in-differences approach.

Maternity leave legislation in Germany was altered several times between 1979 and 2007. The early reforms, which granted only a relatively short period of 6 months in 1979

and 10 months in 1986 and basic maternity pay were primarily meant to promote the child's health. The latest changes to the law are mainly targeted to working mothers. The 2001 reform gives the right to a working parent to continue their job part-time instead of full-time after the birth of a child if they desire so. The 2007 reform then increased paternity pay (*Elterngeld*) for the first 12 months. It is granted for two additional months, if the other parent agrees to stay home with the child for these 2 months.

Increases in maternity benefits and maternity pay can indeed foster mothers' labour market attachment and speed up their return to work. Although job protection is also meant to simplify mothers' return to work, the effects should be rather adverse. Mothers are tempted to stay home for the whole protection period (Gottschall and Bird 2003), which makes the pregnancy of an employee more costly for the employer. Anecdotal evidence suggests that women do return to their jobs, but often accept a job, which is not of equal status as before or are fired for some other reason shortly after returning.

Because of the beneficial effects of increased maternity pay on the speedy return to work, which decreases the costs for the employer, I only focus on significant changes in the job protection period. In 1992, the only change in legislation was a rise in the job protection period by 1 year to 3 years. Other major changes took place in 1986, when the job protection period was raised from 6 months to 10 months. The rise from 10 months in 1986 to 2 years in July 1990 was gradual.

Unlike the recent changes in paternity pay, the reforms in the 1990s were introduced to benefit the child, so they should be exogenous to the mothers' and womens' labour market situation. This is especially true for women who are not mothers. Moreover, public discussion of the reforms typically took place only three months before the reform was implemented (Schönberg and Ludsteck 2007), so the main behavioural changes should have taken place when the reform was implemented and not when it was agreed upon in parliament.

For the difference-in-differences strategy to yield unbiased estimates, treatment must be assigned randomly. As women in childbearing age should be affected by the reform, because they are potential mothers, men of the same age and characteristics constitute a natural control group, because in Germany, it is almost exclusively the mothers who take maternity leave. Treatment is then only based on age and gender. As a robustness check for all regressions, I additionally compare the treatment group to a second control group. The second control group is composed of women aged 45 or older. This is meant to make a clear distinction between women who are definitely in childbearing age and women who are definitely not. This strategy of using men and women out of childbearing age as control groups is similar to Ruhm (1998).

Assumption 1: Treatment Period Agents react directly to the different set of economic conditions.

Treatment is defined as *Year 1992* or *Year 1986* in the cross-section models. In the panel approach for the estimation of the returns to education I assume the effect of treatment to last at most 3 years, i. e. from 1992 to 1994 and from 1986 to 1989. Assumption 1 implies that all years other than 1992 and 1986 in the panel thus make up a potential 'placebo' treatment for robustness checks in the cross-section analysis of employment opportunities.

Assumption 2: Treatment Effects The effect of the treatment on the treated is heterogenous. The magnitude depends on the costs, which an employee causes when they take leave.

Assumption 2 directly follows from the model. The effect of job protection n womens's employment situation should be stronger the stronger the adverse effects of job protection are on the employer. Education should strongly be related to these costs. I compare estimates for women with a university degree and the rest of the sample separately.[7]

Assumption 3: Selection on Observables Selection into treatment is random. Any differences, which remain, can be controlled with observable covariates.

4.2 Estimation Strategy: Employment Effects and Returns to Education

An employer can react to the increased period of job protection in two ways: either he can stop to hire women or at least women in childbearing age or he can adjust women's pay in order to account for higher expected cost in the case of pregnancy, given that the pregnancy risk has remained the same. In both cases, the effect should be stronger for women who are more costly to replace, that is, for women with a university degree. In estimating these effects, the approach should, however, be different.

First, employment opportunities after the policy change can be analysed best using a kind of first-differences approach. If employers are more reluctant to hire, it should be more difficult for individuals who do not have a full-time job to find one. A reasonable measure for

[7] A quantile regression approach according to returns to education or more detailed educational level might yield more precise estimates.

job opportunities is thus whether someone, who was not employed before, more easily finds a job after the reform. The variable of interest, measuring the opportunities of employment for those who are affected by the reform, is then the first difference in employment status for previously unemployed persons. Unemployed is used in the sense of not having a job or not being employed respectively, not necessarily only being registered as unemployed. Therefore, being unemployed also includes persons on leave or in education or military service. It does not, however, include marginally employed persons, or persons currently in vocational training. Broadening the sample by including these groups does not have a strong effect on the results.

Job protection laws in general are very rigid in Germany, so that any reaction can only show for employees, who are hired after the policy change. This is why I restrict the sample for analysing employment opportunities after the policy change to individuals who are not employed in the base period.

In detail, I compare the change in employment status in period t (1986 or 1992) with the change in employment status in period $t-1$ (1985 or 1991). Let e_t denote employment status in year t. Then I compare $e_t - e_{t-1}$ with $e_{t-1} - e_{t-2}$. [8] The change in employment status in period t will depend on personal characteristics in the base year, $t-1$. The model for the outcome in absence of treatment would read:

$$Y(0) = empl_t - empl_{t-1} = \beta_0 + \beta_t T + \beta_{tg} TG + \beta_{tt} T * TG + \boldsymbol{\beta_e} \mathbf{x_e}_{t-1} + u$$

where $t \in (1985, 1986)$ for the first policy change and $t \in (1991, 1992)$ for the second policy change. $T \in (0, 1)$ is a dummy indicating the treatment, that is, the year of change, while $TG \in (0, 1)$ is an indicator for the treatment group, such that $T{*}TG$ identifies the effect of treatment on the treated. $\mathbf{x_e}$ denotes a vector of personal characteristics that determine the probability of being hired and that are taken from the base year. Explanatory variables, which capture the characteristics of the current job, are taken from the current year.

Although this specification uses a binary variable as the dependent variable, I do not use a maximum likelihood estimator, but a linear probability model. A maximum-likelihood estimatior would not be appropriate for the interpretation of the interaction term (Ai and Norton (2003)).[9] In a first-step analysis, I compare the two relevant cross-sections for years 1991 and 1992 for both control groups. Treatment is defined as $Year = 1992$. As a robustness

[8] I only look at observations for whom $t-1$ ($t-2$ respectively) is equal to zero. This makes sure that I only compare those who find employment to those who do not, and not to those who are in employment and do not experience a change. As a result, $((e_t - e_{t-1}), (e_{t-1} - e_{t-2})) \in (0, 1)$.

[9] Unless a more complicated estimator is used (Athey and Imbens, 2006).

check I also estimate the effect of all 'placebo' treatments in the panel. An effect of maternity leave legislation on employment opportunities should show for the 1992 treatment.

If there is individual, time-constant heterogeneity present, which must be considered for consistent estimation, a more complicated procedure would be necessary. As unemployment lags are included as a control variable in the model, a dynamic panel data estimator would have to be used.

The estimation of the returns to education is, however, a case for explicitly considering the effects of unobserved individual heterogeneity (Card 1999). For estimating an effect on the returns to education, I make use of the panel structure of the SOEP. First, I use a within-transformation for estimating the effect of the reform on the level of wages. Second, I follow the Ondrich et al. (2002) approach in measuring the effect of the reform on wage growth, taking first differences.

The sample is restricted to individuals who work in a full-time or part-time job for the sake of comparability. Work-time related differences between these groups can be controlled for by weekly hours of work. The empirical model is a standard Mincer wage regression, which measures changes in the wage level. To get rid of individual heterogeneity, which exerts an influence on log wages, and which I assume to be constant over time, I first apply a within transformation to the full panel for years 1986 until 2001. Between these years, maternity leave legislation was changed only twice. In 1990 maternity benefits were raised, and the job protection period was raised by 10 months to 18 months. In 1992, only the protection period was changed quite substantially to 36 months. Indicator variables for treatment and the treated are specified according to the difference-in-difference strategy. The model then defines the log wage of individual i in period t to be

$$\mathbf{log\ wage_{it}} = \boldsymbol{\beta_y}\mathbf{year_t} + \beta_{tg}TG + \beta_{tt}TG * T + \boldsymbol{\beta_{ex}}\mathbf{x}_{it} + \boldsymbol{\varepsilon}_{it},$$

where $\mathbf{year_t}$ denotes year fixed-effects and $\mathbf{x}_{it}$ denotes the usual explanatory variables in a Mincer wage regression. It is important to note that any individual heterogeneity in wages is assumed to be controlled for by demeaning. The model does assume homogenous treatment effects according to assumption 2, that is, within educational groups.

In a second step, the model is estimated with first-differences, to measure the change in wage growth. Following Ondrich et al. (2002), the difference in log wages can conveniently be used to estimate the wage ratio. With Δ denoting first differences, the model then reads:

$$\Delta log\ wage = \Delta\alpha + \beta_t year_t + \beta_{tg}\Delta TG + \beta_{tt}\Delta(T * TG) + \boldsymbol{\beta_{ex}}\boldsymbol{\Delta}\mathbf{x} + \Delta\varepsilon.$$

4.3 Regression Results: Difficulties in Finding a Job and A Wage Penalty

An extension in the job protection period by one year to three years worsens the position of women with a university degree on the job market. They face a lower probability of finding employment if they were not employed previously, and, in addition, have to pay a risk premium in the form of significantly reduced wages. The reduction in wages is especially strong if the woman starts a new job after previously not being employed. There are hardly any effects of the change in the job protection period on women without a university degree. Moreover, a minor extension in the job protection period in 1986 does not show significant effects on both groups.

The first possibility for the employer to react to changed economic conditions is just to refrain from hiring women in childbearing age. This can either imply increased hiring of men or less hiring altogether. As the model cannot differentiate between supply and demand effects, I aim to capture both supply and demand side effects by the appropriate controls in the model. It should then capture both search model aspects and matching model aspects. The first regression in table 7 shows the regression results for a pooled cross section of observations from either year 1985 and 1986 or year 1991 and 1992. The empirical model is applied to the sample of non-employed in the base period, i. e. the non-employed in 1984 and 1985 and 1990 and 1991. The question, which is to be answered with the regression analysis is whether non-employed women are less likely to find a job if they were not employed in the year before the reform was implemented.

Many women work in part-time jobs. One of the reasons for this could be that they rather accept a part-time job than no job at all. At the same time, an employer might be more likely to hire a woman if she applies for a full-time job, because then it is less likely that she needs to care for her children or that she might have children soon. This is why I also include a dummy, which is switched on if someone was looking for a full-time job in the previous period. This variable is a highly significant determinant of finding a job. If someone was actively looking for a full-time job, they were very likely to find one. Another dummy variable is switched on if a woman works in a full-time job. This is significant in the sample of women without a university degree. In addition, I control for the actual hours of work per week, which accounts for a possible second job. Other usual variables, which should determine employment opportunities are age, marital status, and to a lesser extent years of education in the previous year. Education should matter to a lesser extent, because I look at women with a university degree separately.

To isolate the effect on women in general, I include parental status in the previous

period as another control. The birth of the first child makes it less likely for women to look for a job. This should lower the probability of finding a job, because of not searching. For the same reason, being on maternity leave in the previous period should make the desire to return and thus the return granted by the job protection law more likely. Unfortunately, this information is only available from 1990 onwards, such that it can only be included as a control in the 1992 policy change analysis. As this variable does not appear in the 1986 policy reform analysis, the 1986 coefficient should be viewed with some caution. The 1992 policy reform analysis shows that it is important to control for previous maternity leave.

The 1992 policy reform had a significant negative impact on women with a university degree. Their opportunities of finding a job if previously not employed decreased by about 9% from 1991 to 1992. In contrast, the results show that there was hardly an effect on the rest of the sample, which underpins the argument that any reaction should affect those employees most who cause the highest cost for their employer if they take leave.

The analysis also includes an indicator variable for a temporary job contract. In the literature on job protection, temporary job contracts are mentioned as a means ofcounteracting the rigidities, which are enhanced by job protection legislation (e. g. Cahuc and Postel-Vinay 2002). Empirical research of fixed-duration contracts has shown that the introduction of such a type of contract increases flexibility and labour market flows (Goux et al. 2001; Blanchard and Landier 2001).[10] In fact, in the SOEP sample, starting on a temporary job contract also raises the probability of starting a job.

Results are fairly similar when young women are compared to women out of childbearing age. There is a negative impact on employment opportunities for young women in 1992, although the effect for young women is positive in general in this year. This is also the case when comparing young women to young men. Young women are more likely to find a job than young men. The magnitude of the effect of the policy reform on the employment opportunities of young women is similar in both analyses. While in the comparison to men, young women's probability of finding a job decreases by about 9%, in the comparison to women out of childbearing age, the probability of finding a job decreases by about 6%. Unfortunately, the estimation with the same covariates as in the comparison to men yields a significant estimate, which is not reliable, because the sample size is too small. The mentioned decrease of 6% is a result of a comparison of group means out of a sample if 306 observations, which is insufficient for consistent estimates.

A robustness check of the 'placebo' treatments, i. e. all other years in which no reform of maternity leave legislation was implemented, shows mixed results. First, for women

[10]Boeri (1999) is an exception to this. He presents a model and empirical evidence that temporary contracts can decrease the probability of the unemployed finding a job.

without a university degree, several 'placebo' treatments seem to have an effect, which are sometimes positive and sometimes negative, but always of the magnitude of 1% – 3% when young women are compared to young men (table 8). In the nineties, the effects for this group are insignificant and close to zero. In the sample of individuals with a university degree, treatment effects are never of a magnitude of more than 5%, except for the years of 1991, 1992, and 2000. In these years, the effect is negative and of a magnitude of 8% – 9%. The real treatment in 1992 is significant as well as the year 2000 effect. This was shortly before the part-time job guarantee type of reform of maternity leave legislation was implemented. As the implication of this reform should be less clear than the implications of the 1992 reform, I conclude that the effect in 2000 may well be caused by other effects, which deserve deeper investigation. A tentative guess would be that in the wake of a bursting stock market bubble, employers were more restrictive in hiring. Because women can still be expected to be more costly because of the pregnancy risk, an employer may stop to hire women before he stops to hire men. In fact, the significant treatment effect of the year 2000 for women with a university degree does not appear, when women in childbearing age are compared to women out of childbearing age. The effect of the other 'placebo' treatments is very similar when women in childbearing age are compared to women out of childbearing age (table 9). The 'placebo' treatments turn out to be significant in the sample without a university degree in the same years as in the comparison to men. A significant treatment effect appears for women with a university degree only in years 1991, and 1992. In comparing women in childbearing age to women out of childbearing age, the magnitude of the coefficients in 1991 and 1992 is also higher than in all other years. Alas, the sample size is much too small in these years for university graduates to yield reliable estimates.

The second possibility for the employer to react to changed economic conditions is to make women pay a risk premium for the possibility that some of them might have children. A useful approach to measuring a potential risk premium in pay is an estimation of the returns to education. In order to control for individual time-constant heterogeneity, I use the panel structure of the SOEP to implement a fixed-effects OLS estimator. To take into account also effects on the wage growth rate, I also implement a first-differences estimator in the spirit of Ondrich et al. (2002).

The standard Mincer regression is extended by time fixed effects and business cycle effects. Moreover, I include a variable measuring whether a person's educational level corresponds to job requirements. This is meant to capture effects of women often accepting a job which is below their educational level after a long break or because the job is less demanding and so allows for the care of children. Weekly actual work hours are also meant to adjust for such an effect. An indicator for working in a full-time job also accounts for differences in pay because of work time. Differences in pay, which may result from the type

of job, are captured by including a harmonised measure of the Treiman prestige scale and a dummy variable for managerial responsibilities (i. e. a job of ISCO88 type 1). Here, too, an indicator for a temporary job contract is meant to capture differences, which may result from switching to only hiring on a temporary basis to alleviate the additional lay-off costs implied by job protection in maternity leave legislation. In all regressions (table 10 – table 13), a temporary job significantly reduces wages. Thus, temporary workers are not paid a premium as in Hagen (2001).

The employment history is approximated by a dummy variable, which is switched on if the person was unemployed in the previous year and a variable, which measures the years since being employed in a full-time job. For example, if someone was employed in a full-time job in $t-1$ and is still employed in the same job, the variable would take the value 0. This variable is meant to capture any human capital effects caused by past unemployment. One additional year of a gap between the current job and the last full-time job significantly reduces wages in all specifications. The same can be said for being unemployed in the previous period. This implies having to start the new job at a significantly lower wage.

The negative wage effect of actually taking maternity leave must neither be neglected. As the information on maternity leave is only available from 1990 onwards, I approximate this by two dummy variables measuring parenthood. The first indicates whether the first child was born in $t-1$ and the second one measures whether the first child was born in t. Two more variables are meant to capture a maternity effect in order to isolate the effect on women who are not mothers. First, an interaction term of a job with responsibilities (ISCO88 code 1) and having a child in t is meant to capture career or wage effects that having a child might have on the high-skilled. Second, as this effect is likely not to be important for men, another variable interacts this with the gender indicator, such that the effect on mothers can be separated from the effect on fathers. A negative effect of birth on mothers is confirmed in all regressions. The birth of a child in the previous period apparently reduces wages, but given that the woman is working already after a year, which is very unusual given the high take-up rate of the full leave period in Germany (Schönberg and Ludsteck 2007), a wage penalty of around 4% for women with a university degree does not seem unusual. Beblo et al. (2007) find a wage penalty of around 19% with a propensity score matching method. Their estimated penalty amounts to 26% if firm fixed effects are ignored. I find such a high penalty only for women in jobs with managerial responsibilities. The other coefficients in tables 10 to 13 appear of the expected sign, many significant.

There is a negative effect of treatment defined as years 1992 – 1994 on women with a university degree in childbearing age both if compared to men of the same age and if compared to women out of childbearing age. The effect is significant in both cases and of

similar magnitude (table 10 and table 12). The treatment effect of the 1986 reform is also negative, but not significant. These results confirm the previous analysis of the probability of finding a job to the extent that the 1992 reform seems to have had the most persistent effect on both employment opportunities and wages.

Robustness checks include a shortening of the treatment time horizon, the definition of 'placebo' treatments, the inclusion of an indicator of previous maternity leave, in order to verify that the negative effect measured is not the negative wage effect for mothers, a pooled estimation with job starters only, and an estimation of the effect of the reform on wage growth. If an indicator of being on maternity leave in $t-1$ is included, this obviously changes the composition of the sample. The treatment effect, however, persists. The effect of the 1992 reform on women's wage growth is also negative, albeit not significant in the same setting as above (tables 14 – 15). A pooled estimation of wages of only those persons who started a job after previously not being employed, that is, the sample from the previous analysis, shows a much stronger decrease in the wage level for women with a university degree of about 44%, which is highly significant. There is also a stronger decrease for women without a university degree. The coefficient is, however, not significant. These results apply to the comparison between men and women, as the sample of women only is too small. The same is true if the first differences analysis of wage growth is only applied to those who start a new job if previously not employed.

While there is no clear evidence for an effect of the 1986 policy change on women's labour market position, the 1992 policy change has worsened women's position on the labour market. In particular women with a university degree have to pay a risk premium even if they did not just return from maternity leave, which is typically associated with a fall in wages in the short run. In addition, if previously not employed, it is more difficult to move to gainful employment after the policy reform.

5 Conclusion

This paper sheds light on the effects of maternity leave legislation on the labour market outcomes for women, without restricting the analysis to mothers. By using exogenous variation in the length of the job protection period, the analysis uses a difference-in-differences strategy to show the effect of legislation changes on women's wages and employment opportunities.

The analysis is based on a model, which assumes that a long leave period imposes costs on the employer. As the employer cannot know which female employee becomes pregnant, a risk premium has to be borne by all females willing to participate in the labour market.

The contribution of the empirical analysis is twofold. First, I estimate whether employment opportunities change after the changes in legislation. The analysis shows that a prolongation of job protection by 4 months, from a level of 6 months, hardly has an effect on finding a job. This underpins previous analyses, which claim that maternity leave fosters women's labour market attachment, which decreases the probability of complete dropout after the birth of a child. A prolongation by a year, from a level of two years, however, has negative effects. These are significant for women with a university degree.

Second, the estimation of women's returns to education shows clear and consistent evidence that a prolongation by another year, in a case where the protection period already encompasses two years, results in a wage penalty of 5%–7%. Again, this most strongly affects women with a university degree, for whom replacement costs are highest.

One should, however, always keep in mind, that other changes in maternity leave legislation have positive effects on female labour supply (e. g. Ruhm 1998; Schönberg and Ludsteck 2007). The prevailing focus on supply side effects, however, does not cover the whole story. As my results indicate, too long a leave period worsens women's position in the labour market instead of easing the return into working life.

The selection into unemployment in the base period cannot be captured appropriately in the model, although I try to approximate this by controlling for the total time since the last full-time job. The model on employment opportunities would supposedly be more robust if the selection into unemployment in the base period would be modelled explicitly. This has, however, to be deferred to future work. The same is true for finding employment and the choice of work hours. Selection into employment and wage structure should not be looked at separately, so modelling these processes jointly would be an important addition to the model. Given the significance of the results, however, the evidence of the presence of effects of maternity leave legislation on women's – and not only mothers' – position in the German labour market, in particular on wages, seems compelling.

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Appendix

Table 1: Summary Statistics for Selected Variables (1992): Sample 1

Variable	Mean	Std. Dev.
Monthly household income (Euro)	2133.611	947.829
Current year age	29.874	5.957
Years of education	11.588	2.396
Size of company (categories 1 –5)	2.554	1.2
Weekly work hours	8.73	2.342
Dummy Variables		
Gender (D)	0.43	
Married (D)	0.518	
First child born in current yr. (D)	0.344	
Temporary job contract (D)	0.05	
Full-time job (D)	0.756	
In maternity leave in $t-1$ (D)	0.011	
No. of obs.	**1946**	

Table 2: Summary Statistics for Selected Variables (1992): Sample 2

Variable	Mean	Std. Dev.
Gross monthly income (Euro)	2147.957	1152.088
Years of education	11.863	2.485
Years with firm	10.981	10.12
Weekly work hours	9.304	1.889
Treiman prestige scale	43.52	11.882
Current year age	39.212	11.226
First child born in current yr. (D)	0.357	0.479
Time since last full-time job	0.229	0.99
Dummy Variables		
Gender (D)	0.319	
Education corresponds to job requ. (D)	0.658	
Temporary job contract (D)	0.018	
Unemployed in \ (t-1\ ) (D)	0.013	
Job of ISCO88 code 1 (D)	0.076	
University degree (D)	0.149	
Married (D)	0.67	
Full-time job (D)	0.933	
No. of obs.	**2787**	

Table 3: Percentage of Sample 1 Working

Year	No University Degree		University Degree	
	Men	**Women**	**Men**	**Women**
1987	.7852303	.6114519	.9322034	.7560976
1988	.791372	.5966268	.9034091	.7982456
1989	.8151448	.6313056	.9230769	.7184466
1990	.8076923	.6601942	.922619	.7450981
1991	.821119	.6440154	.9268293	.6938776
1992	.8220408	.6375682	.9135802	.6442308
1993	.8121911	.6263031	.9112426	.65
1994	.7934509	.6090164	.9217877	.6944444
1995	.8034483	.6163166	.956044	.7241379
1996	.8133577	.6066946	.9430052	.7304348
1997	.8306063	.631016	.9578947	.773913
1998	.8336735	.630137	.9468085	.7165354
1999	.8755412	.6741028	.9395604	.8306451
2000	.8717366	.6619718	.9638554	.8
2001	.8734491	.6373391	.969697	.8053097
2002	.8563612	.6634731	.9736842	.7844828
2003	.8640916	.6674817	.951049	.7476636
2004	.847793	.6937173	.965035	.75
2005	.8229342	.685633	.986014	.7943925
No. of Obs.	20530	21984	3342	2211

The sample is made up of individuals aged between 18 and 40 in SOEP sample A.

Table 4: Percentage of the Working in Sample 1 Employed in a Full-time job

Year	No University Degree		University Degree	
	Men	**Women**	**Men**	**Women**
1987	.8391609	.5685164	.9575757	.6521739
1988	.8455949	.566467	.9559748	.6593407
1989	.8483456	.5818399	.9483871	.6756757
1991	.8575668	.5682102	.9662162	.6721311
1992	.8897959	.571066	.9178082	.671875
1993	.885212	.5870712	.9210526	.6451613
1994	.9019396	.5789474	.9390244	.6081081
1995	.9005464	.5716216	.9482759	.6049383
1996	.8828932	.5657143	.9171271	.675
1997	.8909953	.5470588	.9055555	.7011494
1998	.8936446	.5492308	.9161677	.7111111
1999	.8745342	.5204082	.9411765	.6407767
2000	.8795812	.5674419	.9746835	.6206896
2001	.871612	.5443686	.9625	.6703297
2002	.8704	.5129151	.9864865	.6888889
2003	.8628762	.4990654	.9703704	.7051282
2004	.8623188	.4720617	.9562044	.686747
2005	.8783505	.4476987	.929078	.7108434
2006	.8651163	.4617225	.9674796	.6666667
No. of Obs.	15580	12801	2963	1546

The sample is made up of individuals aged between 18 and 40 in SOEP sample A, who are unemployed in t.

Table 5: Percentage of Sample 1 Finding a Full- or Part-time Job (if previously not employed)

Year	No University Degree		University Degree	
	Men	**Women**	**Men**	**Women**
1987	.0385424	.0458515	.0511364	.0826446
1988	.0409656	.043609	.0571429	.1009174
1989	.0401211	.069128	.0898204	.0412371
1990	0	0	0	0
1991	.0411664	.0376712	.0457516	.1149425
1992	.0365093	.0457686	.0636943	.0752688
1993	.0366071	.054126	.0679012	.0888889
1994	.0436364	.0451671	.0809249	.1153846
1995	.0541284	.0495575	.0449438	.0849057
1996	.0494665	.041629	.0687831	.1153846
1997	.0469867	.0494665	.0695187	.0833333
1998	.0446927	.0420513	.0520231	.092437
1999	.0486188	.0572597	.0444444	.1487603
2000	.0277136	.0495258	.0365854	.0571429
2001	.0377834	.040724	.0306748	.0727273
2002	.0207469	.056962	.0328947	.1160714
2003	.0323054	.0514801	.0214286	.0693069
2004	.0299213	.0588235	.0357143	.1037736
2005	.0332168	.0251852	.048951	.1165049
2006	.036965	.0392157	.0155039	.0430108
No. of Obs.	18531	19576	3113	2011

The sample is made up of individuals aged between 18 and 40 in SOEP sample A, whom were unemployed in $t-1$ and found full-time or part-time employment in t.

Table 6: Average gross wage (EUR) in Sample 2

Year	No University Degree		University Degree	
	Men	**Women**	**Men**	**Women**
1986	1563.319	1146.193	2650.47	1757.372
1987	1613.147	1147.469	2679.634	1783.76
1988	1675.577	1172.305	2615.655	2012.385
1989	1751.921	1219.101	2762.716	1965.447
1991	1897.007	1313.745	2723.438	1992.148
1992	1997.277	1393.211	2912.933	2088.339
1993	2147.264	1485.388	3213.137	2264.853
1994	2194.471	1488.321	3118.796	2217.124
1995	2224.395	1558.275	3508.699	2168.914
1996	2377.334	1632.857	3350.844	2276.117
1997	2375.002	1648.205	3348.111	2238.752
1998	2403.445	1614.685	3313.329	2243.531
1999	2415.04	1636.23	3466.024	2178.748
2000	2524.236	1716.312	3663.588	2383.95
2001	2586.803	1739.735	4096.646	2488.475
No. of Obs.	20530	21984	3342	2211

The sample is made up of individuals aged between 18 and 40 in SOEP sample A.

Table 7: Regression results – Employment Opportunities (control group: men)

	Univ. Degr 86	1986	Univ. Degr 92	1992
	(1)	(2)	(3)	(4)
Year * gender (D)	.040	-.028	-.086	.007
	(.025)	(.014)**	(.050)*	(.013)
Gender (D)	-.032	.040	.070	-.010
	(.020)*	(.010)***	(.049)	(.010)
Years of education in $t-1$	-.017	.002	-.007	.005
	(.006)***	(.003)	(.008)	(.003)*
Married in $t-1$ (D)	.007	.005	-.024	.002
	(.015)	(.008)	(.024)	(.007)
Log household income (EUR) in $t-1$	.078	-.135	-.256	-.219
	(.224)	(.106)	(.685)	(.135)
Log household income (EUR) in $t-1$ sq.	-.007	.007	.014	.013
	(.015)	(.007)	(.045)	(.009)
Unemployed in $t-2$ (D)			.135	.075
			(.113)	(.025)***
Unemployed in $t-3$ (D)			-.014	.053
			(.070)	(.019)***
First child born last year (D)	-.043	.087	-.043	.033
	(.062)	(.011)***	(.065)	(.020)*
Age in $t-1$	-.040	.021	-.023	.006
	(.029)	(.006)***	(.061)	(.006)
Age in $t-1$ sq.	.0006	-.0004	.0003	-.0001
	(.0004)	(.0001)***	(.0009)	(.0001)
Would like a full-time job, lag 1 (D)	.836	.575	.752	.583
	(.061)***	(.027)***	(.088)***	(.053)***
Temporary job contract (D)	.021	.027	.241	.034
	(.032)	(.019)	(.085)***	(.029)
Weekly work hours	-.001	-.002	.002	-.010
	(.001)	(.001)**	(.010)	(.002)***
In maternity leave in $t-1$ (D)			.971	.796
			(.093)***	(.074)***
Full-time job (D)	-.026	.077	-.021	.043
	(.030)	(.012)***	(.051)	(.011)***
Obs.	515	4039	419	3577

Other controls include: ISCO88 occupation code, .

Table 8: Regression results – Employment Opportunities (control group: men)

University degree

	Year 1985	Year 1986	Year 1987	Year 1988	Year 1989	Year 1990
	(1)	(2)	(3)	(4)	(5)	(6)
Gender * Year (D)	-.007	.040	-.009	.051	-.029	
	(.030)	(.025)	(.025)	(.036)	(.037)	
Obs.	544	515	519	507	478	228

No university degree

	Year 1985	Year 1986	Year 1987	Year 1988	Year 1989	Year 1990
	(1)	(2)	(3)	(4)	(5)	(6)
Gender * Year (D)	.055	-.028	.006	.011	.032	
	(.011)***	(.014)**	(.013)	(.013)	(.014)**	
Obs.	4210	4039	4014	3966	3864	1925

University degree

	Year 1991	Year 1992	Year 1993	Year 1995	Year 1996	Year 1997	Year 1998	Year 1999
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Gender * Year (D)	.085	-.086	.004	.030	-.041	.029	-.018	.002
	(.055)	(.050)*	(.032)	(.042)	(.044)	(.043)	(.045)	(.038)
Obs.	209	419	424	452	493	516	528	524

No university degree

	Year 1991	Year 1992	Year 1993	Year 1994	Year 1995	Year 1996	Year 1997	Year 1998
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Gender * Year (D)	-.017	.007	.011	-.008	.0008	-.002	.005	-.016
	(.010)	(.013)	(.012)	(.013)	(.013)	(.014)	(.014)	(.014)
Obs.	1810	3577	3492	3375	3304	3225	3094	2944

University degree

	Year 1999	Year 2000	Year 2001	Year 2002	Year 2003	Year 2004	Year 2005	Year 2006
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Gender * Year (D)	.036	-.086	.022	.051	-.049	.016	-.020	-.035
	(.039)	(.035)**	(.023)	(.035)	(.032)	(.026)	(.029)	(.030)
Obs.	524	512	497	491	466	447	444	422

No university degree

	Year 1999	Year 2000	Year 2001	Year 2002	Year 2003	Year 2004	Year 2005	Year 2006
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Gender * Year (D)	.012	.0006	-.009	.032	-.016	-.005	-.030	.018
	(.014)	(.014)	(.014)	(.015)**	(.016)	(.015)	(.014)**	(.015)
Obs.	2892	2890	2721	2470	2381	2293	2051	1822

Table 9: Regression results – Employment Opportunities (control group: women older than 45)

University degree

	Year 1985	Year 1986	Year 1987	Year 1988	Year 1989	Year 1990
	(1)	(2)	(3)	(4)	(5)	(6)
Age < 41 * Year (D)	.035	-.0003	.013	.076	-.028	
	(.041)	(.039)	(.056)	(.056)	(.057)	
Obs.	212	193	190	189	177	84

No university degree

	Year 1985	Year 1986	Year 1987	Year 1988	Year 1989	Year 1990
	(1)	(2)	(3)	(4)	(5)	(6)
Age < 41 * Year (D)	.051	-.019	.008	.003	.021	
	(.015)***	(.019)	(.018)	(.017)	(.019)	
Obs.	2494	2451	2394	2362	2309	1148

University degree

	Year 1991	Year 1992	Year 1993	Year 1994	Year 1995	Year 1996	Year 1997	Year 1998
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Age < 41 * Year (D)	-.439	-.173	-.023	.005	-.050	.045	.033	-.014
	(.143)***	(.068)**	(.041)	(.057)	(.058)	(.044)	(.053)	(.052)
Obs.	75	152	159	173	190	199	213	227

No university degree

	Year 1991	Year 1992	Year 1993	Year 1994	Year 1995	Year 1996	Year 1997	Year 1998
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Age < 41 * Year (D)	.021	-.008	.026	.008	-.020	.003	.007	-.020
	(.043)	(.018)	(.017)	(.017)	(.017)	(.017)	(.018)	(.018)
Obs.	1101	2145	2081	2017	1968	1932	1843	1725

University degree

	Year 1999	Year 2000	Year 2001	Year 2002	Year 2003	Year 2004	Year 2005	Year 2006
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Age $<$ 41 * Year (D)	-.039	-.035	.031	.002	-.013	.010	-.079	.051
	(.046)	(.037)	(.037)	(.053)	(.048)	(.044)	(.041)*	(.033)
Obs.	237	239	240	246	241	238	243	250

No university degree

	Year 1999	Year 2000	Year 2001	Year 2002	Year 2003	Year 2004	Year 2005	Year 2006
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Age $<$ 41 * Year (D)	.025	-.025	.011	.018	-.0002	-.004	-.041	.019
	(.018)	(.019)	(.020)	(.019)	(.019)	(.019)	(.019)**	(.019)
Obs.	1707	1740	1670	1566	1575	1542	1418	1332

Table 10: Regression Results – The Effect of the 1992 Reform on Log Wages (Control Group: Men)

	University Degree	No University Degree
	(1)	(2)
Dep. var: Log gross monthly wage (EUR)		
Treatment Indicators		
Year 1992 – 1994 * gender (D)	-.051	-.008
	(.027)*	(.009)
Year 1992 – 1998 (D)	.047	.039
	(.025)*	(.009)***
Control Variables		
Education corresponds to job requ. (D)	.054	-.003
	(.020)***	(.007)
Years of education	.014	.018
	(.013)	(.006)***
Years with firm	-.002	.008
	(.005)	(.001)***
Years with firm sq.	-.0003	-.0003
	(.0003)	(.00007)***
Weekly work hours	.040	.021
	(.004)***	(.001)***
Treiman prestige scale	.0007	.002
	(.0008)	(.0004)***
Temporary job contract (D)	-.114	-.054
	(.023)***	(.011)***
Current year age	.150	.100
	(.024)***	(.005)***
Current year age sq.	-.001	-.0008
	(.0004)***	(.00008)***
Married (D)	-.002	.022
	(.019)	(.007)***
First child born in current yr. (D)	.072	-.021
	(.029)**	(.011)*
First child born last year (D)	-.016	-.043
	(.029)	(.012)***
Unemployed in $t-1$ (D)	-.153	-.057
	(.033)***	(.012)***
Job of ISCO88 code 1 (D)	.012	.014
	(.030)	(.014)
Time since last full-time job	-.061	-.027
	(.009)***	(.002)***
Full-time job (D)	.222	.411
	(.039)***	(.014)***
Managerial job * child born prev. yr. (D)	.058	.013
	(.051)	(.024)
Managerial job * child prv. yr. * gender (D)	-.409	.054
	(.112)***	(.045)
Obs.	3134	18326

Panel estimator using a within transformation. The sample contains only persons aged 18 – 40, who are employed in a full-time or part-time job. Treatment is defined as years 1992 – 1994. The treatment group consists of the women in the sample. Other control variables include year indicators and business cycle indicators.

Table 11: Regression Results – The Effect of the 1986 Reform on Log Wages (Control Group: Men)

	University Degree	No University Degree
	(1)	(2)
Dep. var: Log gross monthly wage (EUR)		
Treatment Indicators		
Year 1986–1988 * gender (D)	-.022	-.003
	(.041)	(.015)
Years 1986 – 1989 (D)		
Control Variables		
Education corresponds to job requ. (D)	.050	-.019
	(.036)	(.014)
Years of education		.092
		(.047)*
Years with firm	.014	-.006
	(.010)	(.003)
Years with firm sq.	-.002	.00007
	(.0007)***	(.0002)
Weekly work hours	.026	.007
	(.006)***	(.001)***
Treiman prestige scale	-.001	.003
	(.002)	(.0008)***
Temporary job contract (D)	-.097	-.051
	(.040)**	(.018)***
Current year age	.149	.136
	(.064)**	(.014)***
Current year age sq.	-.001	-.001
	(.001)	(.0002)***
Married (D)	-.102	.020
	(.041)**	(.015)
First child born in current yr. (D)	.001	-.041
	(.054)	(.020)**
First child born last year (D)	-.044	.001
	(.060)	(.022)
Unemployed in $t-1$ (D)	-.036	-.052
	(.057)	(.021)**
Job of ISCO88 code 1 (D)	-.041	.020
	(.062)	(.032)
Years since last full-time job	.016	-.162
	(.049)	(.023)***
Managerial job * child born prev. yr. (D)	-.034	-.120
	(.123)	(.047)**
Managerial job * child prev. yr. * gender (D)		-.248
		(.112)**
Full-time job (D)	.124	.202
	(.090)	(.044)***
Obs.	980	6509

Panel estimator using a within transformation. The sample contains only persons aged 18 – 40, who are employed in a full-time or part-time job. Treatment is defined as years 1992 – 1994. The treatment group consists of the women in the sample. Other control variables include year indicators and business cycle indicators.

Table 12: Regression Results – The Effect of the 1992 Reform on Log Wages (Control Group: Women older than 45)

	University Degree	No University Degree
	(1)	(2)
Dep. var: Log gross monthly wage (EUR)		
Treatment Indicators		
Year 1992–1994 * age < 41 (D)	-.068	.002
	(.039)*	(.014)
Year 1992 – 1994 (D)	.075	.044
	(.044)*	(.017)***
Age < 41 (D)	-.072	.007
	(.043)*	(.021)
Control Variables		
Education corresponds to job requ. (D)	.113	.006
	(.038)***	(.010)
Years of education	.041	.003
	(.021)**	(.009)
Years with firm	-.002	.014
	(.005)	(.002)***
Years with firm sq.	.00007	-.0003
	(.0002)	(.00006)***
Weekly work hours	.049	.021
	(.005)***	(.001)***
Treiman prestige scale	.0009	.002
	(.001)	(.0006)***
Temporary job contract (D)	-.152	-.020
	(.032)***	(.016)
Current year age	.132	.081
	(.015)***	(.004)***
Current year age sq.	-.001	-.0005
	(.0002)***	(.00005)***
Married (D)	-.040	-.009
	(.030)	(.011)
First child born in current yr. (D)	.054	-.024
	(.049)	(.018)
First child born last year (D)	-.137	-.296
	(.056)**	(.022)***
Unemployed in $t-1$ (D)	-.198	-.059
	(.044)***	(.019)***
Job of ISCO88 code 1 (D)	.052	.110
	(.053)	(.033)***
Managerial job * child born prev. yr. (D)	-.239	-.014
	(.082)***	(.042)
Years since last full-time job	-.038	-.015
	(.007)***	(.002)***
Full-time job (D)	.141	.283
	(.035)***	(.014)***
Obs.	1365	9762

The sample contains women, who are employed in a full-time job or a part-time job. Treatment is defined as the period between 1992 and 1998. The treatment group consists of all members of the sample aged 18 – 40. The control group consists of all members of the sample older than 45. Other control variables include year indicators and business cycle indicators.

Table 13: Regression Results – The Effect of the 1986 Reform on Log Wages (Control Group: Women older than 45)

	University Degree	No University Degree
	(1)	(2)
Dep. var: Log gross monthly wage (EUR)		
Treatment Indicators		
Year 1986 – 1989 * age < 41 (D)	-.038	-.019
	(.098)	(.036)
Years 1986 – 1989 (D)		
Age < 41 (D)		
Control Variables		
Education corresponds to job requ. (D)	.056	-.005
	(.075)	(.026)
Years of education		.007
		(.063)
Years with firm	.008	-.004
	(.029)	(.005)
Years with firm sq.	-.0005	.0004
	(.001)	(.0003)
Weekly work hours	.033	.005
	(.010)***	(.002)**
Treiman prestige scale	-.006	.004
	(.004)	(.001)**
Temporary job contract (D)	-.196	-.030
	(.068)***	(.033)
Current year age	.093	.106
	(.079)	(.021)***
Current year age sq.	-.0004	-.0008
	(.001)	(.0002)***
Married (D)	-.037	.026
	(.077)	(.026)
First child born in current yr. (D)	-.028	-.058
	(.100)	(.034)*
First child born last year (D)	-.270	-.176
	(.166)	(.047)***
Unemployed in $t-1$ (D)	-.066	-.070
	(.078)	(.040)*
Job of ISCO88 code 1 (D)	.489	.080
	(.138)***	(.108)
Years since last full-time job	.003	-.102
	(.053)	(.024)***
Managerial job * child born prev. yr. (D)		-.326
		(.135)**
Full-time job (D)	.033	.141
	(.100)	(.045)***
Obs.	393	3092

The sample contains women, who are employed in a full-time job or a part-time job. Treatment is defined as the period between 1992 and 1998. The treatment group consists of all members of the sample aged 18 – 40. The control group consists of all members of the sample older than 45. Other control variables include year indicators and business cycle indicators.

Table 14: Regression Results – The Effect of the 1992 Reform on Wage Growth (Control Group: Men aged 18–40)

	University Degree	No University Degree
	(1)	(2)
Variables in first differences		
Year 1992–1994 * gender (D)	-.033	.005
	(.031)	(.011)
Year 1992–1994 (D)		
Weekly work hours	.027	.010
	(.006)***	(.002)***
Temporary job contract (D)	-.054	-.036
	(.035)	(.017)**
Age		
Age sq.	-.003	-.001
	(.0007)***	(.0002)***
Child born current yr. (D)	-.0009	-.021
	(.020)	(.011)**
Child born last yr. (D)	-.022	-.002
	(.031)	(.014)
Obs.	2502	14445

First difference estimates. Treatment is defined as the period between 1992 and 1998. Other controls are the same as in the estimation using a within transformation.

Table 15: Regression Results – The Effect of the 1992 Reform on Wage Growth (Control Group: Women aged 45 or older)

	University Degree	No University Degree
	(1)	(2)
Variables in first differences		
Year 1992–1994 * age < 40 (D)	-.118 (.044)***	-.0004 (.018)
Year 1992–1994 (D)		
Year 1992–1994 (D)		.056 (.017)***
Weekly work hours	.035 (.009)***	.009 (.004)**
Temporary job contract (D)	-.106 (.059)*	-.014 (.021)
Age		
Age sq.	-.002 (.0004)***	-.0007 (.0001)***
Child born current yr. (D)	-.016 (.040)	-.037 (.018)**
Child born last yr. (D)	-.026 (.125)	-.160 (.070)**
Obs.	1050	7421

First difference estimates. Treatment is defined as the period between 1992 and 1998. Other controls are the same as in the estimation using a within transformation.