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Does it Pay to be a Woman?
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Abstract

In countries with strong employment protection laws it is often considered to be unwise to hire a woman in childbearing age because she might get pregnant. However, such labour demand effects of job protection measures related to maternity leave are often rather anecdotal. To provide analytical evidence, this paper studies the impact of changes in maternity-related job protection in Germany on employment opportunities for women in childbearing age without children for whom the observed effects should be largely demand-related. Exogenous, discrete policy changes in the German labour market of the 1980s and 1990s constitute the setting for a difference-in-differences analysis of the transition into employment as well as wages. The data for this study are taken from the German Socio-Economic Panel and from the German Microcensus. Doubling the job-protected leave period from 6 months to 12 months between 1986 and 1988 led to an approximately 6% lower probability of being hired for women in childbearing age without a university degree. In addition, I find a 5-10% increase in wages for women in childbearing age who already have a job. Since this effect disappears when controlling for having a child in the future, this may indicate an increased need to signal commitment by increased effort after the reform.

Keywords: maternity leave legislation, gender pay gap, education, unemployment, difference-in-differences with group-correlated errors, quasi-natural experiment

JEL-Codes: J64, J23, J16, J31, K31, H55

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1 Introduction and literature review

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 (e.g. Saint-Paul 1997, 2000; Boeri 1999; Pissarides 2009; Blanchard and Wolfers 2000; Abritti and Müller 2013). Even though labour protection measures are not the sole reason for unemployment (e.g. Haefke et al. 2013), they are an important factor that raises the cost of adjustment, which can have an impact on labour market dynamics beyond labour supply (e.g. Lazear 1990; Nickell 1997; Goux et al. 2001; Cahuc and Postel-Vinay 2002; Liungqvist 2002). For example, case studies have shown that the possibility for flexible labour cost adjustment leads to less employment reduction (e.g. Krause and Uhlig 2012; Dias et al. 2013) and to better matching efficiency (Klinger and Rothe 2012).

While some studies have given indications of the effects of employment protection on different demographic groups (e.g. Addison and Teixera 2003) or different types of contracts (e.g. Blanchard and Landier 2002), few recent studies explicitly focus on labour demand. Exceptions are Freier and Steiner (2010) who estimate labour demand elasticities for Germany for different employment categories, Dräger and Marx (2012) who estimate the effect of workload fluctuations on the demand for temporary workers and Adam and Moutos (2014) who estimate industry-level labour demand elasticities for the EU. This study adds to this literature by looking at the labour demand effects of maternity leave legislation in Germany.

In addition, this study is related to previous work on the labour market effects of maternity leave legislation. Maternity leave legislation usually entails employment protection as well as some form of payments or a replacement income for women on leave, partially borne by the employer, but largely by the government.[1] Maternity leave legislation has often been analysed with a focus on mothers' labour market supply and outcomes for particular countries (e.g. Ondrich et al. 1996, 2002; Spiess and Wrohlich 2006; Dearing et al. 2007; Baker and Milligan 2008; Schönberg and Ludsteck, 2014) or with a focus on children's outcomes (Dustmann and Schönberg 2012). Solaz and Thévenon (2013) provide cross-country evidence on labour market effects in OECD countries and find that extensions of parental leave to up to two years have a small positive impact on female employment, but widen the gender wage gap. However, they also discuss the possibility that employers become more reluctant to hire women while acknowledging the difficulty to disentangle positive effects of mothers' labour market attachment from negative effects on employers' propensity to hire women. These

[1] In Germany, the employer has to pay the main fraction of the maternity benefits, which are paid for six weeks before delivery and for 2 months after delivery. Maternity pay is related to the woman's salary. This is similar to the employer's contribution to sick pay. However, any allowance a woman receives after those 2 months is paid by statutory social insurance.

negative effects have so far explicitly been analysed by Ruhm (1998) and Baum (2003) for the US. The results presented in this paper also complement these results with a case study for Germany.

Particularly generous maternity leave legislation can be interpreted as employment protection that has potentially adverse effects on the demand for female labour for three reasons. First, employers and the government face direct pecuniary costs of financing maternity pay. Second, the employer faces a higher expected cost of investing in the human capital of a female employee in childbearing age, because there is a higher risk of absence.[2] Mothers' human capital depreciates during long terms of absence (Datta Gupta and Smith 2000; Kunze 2002; Görlich and de Grip 2009), making it comparatively more expensive to re-integrate her in the same or an equivalent job and the and the additional training that has to be invested in a substitute employee (Ruhm 1998; Ondrich et al. 2002). Moreover, the job protection granted during maternity leave and the payment may strengthen the incentives to take (longer) leave (Schönberg and Lusteck 2014). In addition, there is a higher tendency to working part time when there is no sufficient availability of child care (e.g. Powell 1998; del Boca 2002). Both the second and the third aspect may induce employers to hire more men than women or only hire women at lower wages since they can avoid the higher expected costs when hiring women in childbearing age. This paper shows that the effect mainly materialises through a reduced probability of hiring women.

The German example is especially well-suited for analysing the effect of job-protected leave on employment opportunities, as during the 1980s and the 1990s the job-protected leave period of 3 years was one of the longest in the world (Thévenon and Solaz 2013). The law obliged the employer to offer a mother the same or an equivalent job after she returned from maternity leave. The job-protected leave period was extended several times, constituting a quasi-natural experiment because the job protection and payment period were mainly extended in order to benefit the child. Schönberg and Ludsteck (2014) have shown that this extension had the intended effect: the longer the statutory leave period, the longer mothers stayed out of the workforce. At the same time, that even the Federal Constitutional Court acknowledged the fact that this may have led to a lower propensity to hiring women in general. In a ruling on how much of maternity pay has to be borne by employers (Bundesverfassungsgericht 2003), the Federal Constitutional Court explicitly considered the fact that higher costs would raise employers' unwillingness to hire young women. Attempts to raise the statutory leave period in the UK in 2009 also led to concerns

[2]The period of actual leave-taking strongly depends on the length of the statutory job protection period (Ondrich et al. 1996, Gottschall and Bird 2003, Berger and Waldfogel 2004), but the German law allowed for extending the maximum leave period of three years for another three years if another child is born during the leave period.

that the related cost increase would cause employers to stop hiring young women. This paper substantiates these concerns by showing that every woman in childbearing age is adversely affected by more generous maternity leave legislation, irrespective of whether she will eventually have a child or not.

My results show that employers are more selective after the policy changes. The extension of both the job protection and the payment period from 6 to 18 months significantly reduces the probability of transition into employment by 6-7%. Women with a university degree were not affected.

A selection model for the wages of the newly hired confirms the selectivity at the employment margin; I only find some evidence for decreased wages for the newly hired of both the 1980s and the 1990s reform package. In contrast, especially after the 1992 reform, the wages of women in childbearing age who already had a job were 5-10% *higher* compared to the control groups used in this study. Since this effect disappears when controlling for the future number of children, the higher wages may indicate an increased need to signal labour market attachment by increased effort after the reform.

Section 2 gives a short introduction to maternity-related job-protection laws and lay-off costs in Germany and discusses data and econometric considerations, while section 3 contains results and sensitivity analyses. Section 4 concludes.

2 Data and econometric considerations

2.1 Data: the German Socio-Economic Panel

The data for this study come from the German Socio-Economic Panel (SOEP). The SOEP is an ongoing panel study of German households, which was started in 1984 (e. g. Wagner et al. 2007), containing rich information on the labour market situation of the participants. I use data from waves 1984–2000.[3] This time frame does not cover the introduction of maternity leave legislation of 1979 since the panel was only started in 1984, but it covers all other policy changes. As I would like to avoid confounding effects from the reforms which were enacted in 2001 to facilitate reconciling work and having children, I restrict the sample to years before 2001. Since the analysis includes a second order lag as explained below, the

[3]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.

effective period of analysis is therefore 1986-2000.

The population from which I draw the sample comprises all persons for whom information on labour market status is available and who could potentially be active in the labour force, i. e. 18–60 years of age.[4] Since the period includes the years of German reunification, I include the East German sample from 1991 onwards.[5] As the main policy changes took place between 1991 and 1993, I am particularly careful in adding the sample. The analysis includes a dummy variable for East Germans from 1990 onwards as well as a dummy for capturing the effects of the policy changes on the East German observations.

Information on job search, which I use in the analysis of transition into employment (table 2), i. e. information on whether someone who is not employed looks for a full-time job, a part-time job, or does not care, is not collected for the West German sample for 1990 and is not collected for the East German sample for 1991. In order to be able to use this variable for the years 1990-1991, I impute values for job search behaviour for the West German sample for the 1990 values based on the explanatory variables used in the model (described in section 2.3) and especially based on an individual's search behaviour in the years before and after the missing year. As a consequence, 2.44% of values on job search behaviour are imputed. I do not impute the missing values for the East German sample for 1991, because the underlying model for imputation would require information from 1990 and 1989 and information from 1989 is not available for the East German sample. Therefore, I prefer to include the East German sample only from 1993 onwards.[6] The resulting panel is unbalanced in the sense that it only includes individuals for whom labour market status is known and for whom the gross monthly wage is known if they earn a salary.

[Table 2 about here.]

From the population of observations for whom labour market status is known, the sample is drawn based on the characteristics which define the group potentially affected by the reforms of maternity leave legislation. First, I restrict the main analysis to individuals without university education, because the effect of maternity leave legislation on a mother's labour supply behaviour is not exclusively tied to the mere entitlement to job protection. The incentives to actually go on leave for an extended period of time are also strongly affected by the replacement income a mother receives during that period. However, after 6 months

[4] The average effective retirement age in Germany was only slightly above 60 in the 1990s.

[5] Reunification effects are present in the data, but they affected all sub-groups used in this study in the same way.

[6] To check whether the timing of including the East German sample in the model matters, I also estimated all specifications excluding the East German sample completely.

of leave, the replacement income is only paid if the household's income is below a certain threshold.[7] It is therefore useful to restrict the sample for the analysis to those women who would – potentially – be entitled to the full payment of maternity benefits as it can be assumed that employers would find it more likely that women with lower salaries would have a higher propensity to take (longer) leave. Evidence shows that indeed except for women with higher education German women took the full leave period of 3 years (Gottschall and Bird 2003, Büchel and van Ham 2004, Schönberg and Ludsteck 2014). As a consequence, women with a high level of education probably did not experience the same reduction in the probability of becoming hired. The SOEP data confirm this hypothesis somewhat.[8] Second, since job protection laws are comparatively rigid in Germany, it seems straightforward to assume that employers can only adjust their hiring behaviour for new hires. Therefore, I restrict the sample further to persons out of the labour force prior to the period of interest. This includes the unemployed but also those previously in education. Third, I exclude men aged 40 – 60, because these men should structurally differ from the group of interest, i. e. young women in childbearing age. This difference is so apparent that the assumption of selection on observables which is necessary for my identification approach (described below) is likely to be violated.

2.2 Identification: changes of maternity leave legislation

To identify an effect of strengthened labour protection measures, in particular dismissal protection combined with a higher replacement income, on labour demand and wages I use changes in maternity leave legislation in Germany during the 1980s and 1990s since these provide quasi-experimental conditions which can be used for identification. First, the changes were not motivated by changes in wages or labour demand. Second, the public discussion of the reforms took place only shortly before implementation, making anticipation effects unlikely (Schönberg and Ludsteck 2014). In order to rule out that differences in employment rates in my analysis are driven by increased childbearing as a response to the reform, I restrict the sample to women who – if they had a child at some point – would have their first child only two years after the reform or later. Third, unlike later reforms, the changes in the 1980s and 1990s only applied to mothers and not to fathers, thus likely affecting the demand for female and male labour differently.[9]

[7] Section B.1 in the supplementary appendix provides details.

[8] Effects for a sample restricted to individuals with tertiary education are not as strong and hardly significant, but the sample size is also quite small, because this group is highly selected. The average share of individuals with tertiary education is between 1% and 3% for both treatment group and the control groups, whereas the population average is between 10% and 13%.

[9] It can be argued that there was a *positive* effect on the demand for male labour. This is discussed below.

Maternity leave legislation was changed several times between 1979 and 2011 in Germany (table 1). In 1979, maternity leave legislation only granted a relatively short job-protected period of 6 months of leave and basic maternity pay and benefits. This was extended to 10 months of leave and basic maternity pay and benefits in 1986. Between 1988 and 1990 both the job protection period and the maternity benefits payment period were gradually extended. The job guarantee only applied when the full statutory leave period was taken. Contrary to later reforms, these changes were not meant to facilitate mothers' transition out and into the labour force. Maternity leave legislation was generously extended particularly to facilitate a mother's stay at home for longer periods after delivery, because this was believed to have positive effects on the child's well-being. Policy makers did not have the intention to reduce female labour supply; however this was accepted as a side-effect. Therefore, it can be anticipated that employers' expectations about women's behaviour after having a child adjusted to the extent that female labour supply adjusted. At the same time, the policy decision was not driven by labour demand, which is why I treat the policy changes as exogenous to labour demand.

[Table 1 about here.]

Extended periods of leave and enhanced dismissal protection only applied to mothers and in Germany almost exclusively the women took maternity leave (Gottschall and Bird 2003), thus restricting the group of the labour force affected by the reform to women who could (potentially) give birth. Therefore, employers' expectations should have mainly changed about women in childbearing age since these were potentially covered by the reforms. It may seen natural to assume that men of the same age and characteristics are a suitable control group. However, it is also possible to argue that the reform also had an effect on men, because employers' preference to hire men could have increased after the reform (this is the way in which Thévenon and Solaz, 2013, present the argument). Therefore, I also use two other control groups. As suggested in Ruhm (1998), women out of childbearing age, especially when they do not have children, should not be affected by such reforms either. In addition, women in childbearing age who already have at least the average number of children and can thus be expected to have finished their family planning are another potential control group.

Since it is clear that the control groups differ from the treatment group, it is important to control for those differences for a valid identification of an effect. For example, the costs associated with an employee's temporary leave rise with the employee's skill level and the extent of firm-specific training required to do the job. Women who are not in childbearing age have a higher level of experience than women who just started a job. Moreover, women with a higher skill level and thus higher returns to education might be less affected, because they have higher opportunity costs of leaving the labour market and would thus not take the full statutory leave period. The set of control variables used in this study is presented in tables 2 and 3.

2.3 Econometric considerations

To analyse the effects of the reform on specific groups of the population, I proceed similarly to those studies that estimate labour supply of mothers. For example, Schönberg and Ludsteck (2014), who use the same type of reforms for identification, compare the labour market outcomes of mothers who gave birth shortly before and shortly after the policy changes. Corresponding to my different focus, I compare the probability of finding a job and the entry wage between the treatment group and the four different control groups if a person has been recorded as out of the labour force previously.

2.3.1 Extensive margin: transition into employment

The first outcome of interest, $Y_{i,g,t}$ for individual i in group g at time t is the probability of being employed in a full-time or a part-time job in t. Each individual can be in one of the previously identified groups: women in childbearing age without children constitute the treatment group while men in the same age group, women with at least the average number of children and women out of childbearing age constitute three control groups.

I assume that the binary outcome $Y_{i,g,t}$ (indicating employment) for individual i in group g in year t is affected by a time-specific trend T_t that is common across groups, but not across time. The probability of being employed in a full-time or a part-time job is determined by present individual characteristics $\mathbf{Z_{i,g,t}}$ and past individual characteristics $\mathbf{Z_{i,g,t-1}}$:[10]

$$Y_{i,t,g} = \mathbf{a_g} + \mathbf{T_t} + \mathbf{Z_{i,g,t}}\boldsymbol{\beta}_1 + \mathbf{Z_{i,g,t-1}}\boldsymbol{\beta}_2 + D_{g,t}\gamma + \alpha_{\mathbf{i,g}} + \varepsilon_{i,g,t}. \tag{1}$$

$\mathbf{a_g}$, can either be a set of group dummies ($g \in 1,2,3,4$) or just a binary variable a_g indicating membership in the treatment group, which, if interacted with the relevant time dummies $\mathbf{T_t}$, give the differences-in-differences (DD) estimator $D_{g,t}$ for the policy effect on the treatment group. In contrast to the i.i.d. error term $\varepsilon_{i,g,t}$, the error term $\alpha_{i,g}$ is allowed to correlate

[10]Including present and past individual characteristics is meant to account for the fact that the current employment status is a result of being hired or not in the previous period, which in turn is determined by characteristics in the previous period. Only characteristics, which do not change over time, are thus taken from the present year.

within group g since using a DD model with a small number of groups automatically causes a group-correlated error problem and serial correlation, at least in the treatment indicator (Bertrand et al. 2004). Correcting the resulting biases is more difficult when the number of groups is small (e.g. Donald and Lang 2007).[11] Most approaches dealing with potential group-correlated errors in small samples involve some form of aggregation. This is achieved either for example in estimating the group fixed effects in a first stage (Donald and Lang 2007), or averaging residuals over groups (Bertrand et al. 2004) or using only group averages in the estimation, and thereby assuming $\boldsymbol{\beta_g} = \boldsymbol{\beta}$ (Wooldridge 2003).

In this paper, in addition to cluster-robust OLS (Liang and Zeger 1986) I therefore present only the residual aggregation technique discussed in Bertrand et al. (2004) and the two-step procedure suggested in Donald and Lang (2007), because poolability of the data for the time period of interest is not given and because there is serial correlation in the variables of interest.[12] Ideally, the sample should be pooled for years 1986 – 1997.[13] However, a Roy-Zellner test for the equality of coefficients, as suggested by Baltagi (1981), to test the assumption of equal effects of the control variables over time rejects poolability of the data before and after each reform. As an alternative, I pool the data, but define the group/year cells as distinct groups.

Note that this model is not a dynamic model with a lagged dependent variable for two reasons. For one, the sample is already conditioned on emploment status in the previous period, thus implicitly accounting for path dependency. For another, I use several aggregation techniques for dealing with serial correlation. These make the need for a dynamic model less pressing.

2.3.2 Intensive margin: entry wages

The second outcome of interest, $Y_{i,g,t}$ for individual i in group g at time t is the entry wage, given that the individual has moved from non-employment to a full-time or a part-time job in t. Estimating the effects of the reforms at the intensive margin is slightly more complicated, because this involves dealing with a selection process. The transition into employment is clearly affected by the reforms too, which is why the model estimated for the extensive

[11]If the number of groups is small, using the t-distribution for inference requires some more assumptions, as described in e. g. Donald and Lang (2007) and Wooldridge (2003). Even when the t-distribution is applicable, the degrees of freedom must be adjusted.

[12]The issue of serial correlation is discussed in supplementary appendix B.2.

[13]1986 is chosen as the start date, because this is the first year for which information, including lags, is available. 1997 is chosen as the end date, because major reforms were implemented in 2000 and these were discussed in the media beforehand.

margin determines the selection of the sample for which we can observe wages. Assume that the wage w is only observed if latent employment status Y^* is positive:

$$w = \begin{cases} w^* & \text{if} \quad Y^* > 0 \\ - & \text{if} \quad Y^* = 0 \end{cases}$$

The latent wage w^* for each individual i in each group/year cell is determined by a vector of explanatory variables $\mathbf{X_{i,g,t}}$, which contain, inter alia, the usual variables from a Mincer wage equation such as basic socio-demographic variables and education and skill levels (table 3), group and time effects and the effect of the policy intervention, captured by an interaction dummy, as in the model in equation (1):

$$w_i^* = \mathbf{a_g} + \mathbf{T_t} + \mathbf{X_{i,t}}\boldsymbol{\beta}_{w^*} + D_{i,g,t}\gamma + \alpha_{i,g,t} + \mu_{i,g,t}. \tag{2}$$

Whether the wage is observed is determined by a selection according to

$$Y_i = \mathbf{a_{g,t}} + \mathbf{T_t} + \mathbf{Z_{i,t}}\boldsymbol{\beta} + D_{i,g,t}\gamma + \nu_{i,t}, \tag{3}$$

Under the assumption that the error term $\mu_{i,t}$ in the wage equation (2) is jointly normal distributed with the error term $\nu_{i,t}$ in the selection equation (3) and homoskedastic, estimation with maximum likelihood is straightforward and a two step estimation procedure as in Heckman (e.g. Heckman 1979; Flinn and Heckman 1982; Leung and Yu 1996; Puhani 2000) can also be applied. I assume that the policy intervention may affect both the selection equation (3) and equation (2).

[Table 3 about here.]

Equation (3) contains different variables than equation (2). The covariates for the first stage are the variables in table 2, except for those which are specific to the out of the labour force sample, like a variable that measures whether someone out of the labour force is looking for a job. The covariates for the second stage are the variables in table 3, which only partly correspond to the variables in table 2. In particular, table 3 also contains information on the sector, the size of the firm and the occupational status in the previous period. In addition, educational variables like the type of degree and the years of education enter the second stage regression as contemporaneous values while they enter the first stage regression as a lag. This approach is fairly straightforward since the transition into employment from *t-1* to t should mainly depend on the applicant's characteristics in *t-1* while the current wage should depend on current experience, industry and skill level. Hence, the lagged variables can serve as exclusion restrictions. However, since the lagged variables in the first stage determine the employment status in the second stage and thus indirectly the wage if the wage is conditioned on previous employment status, they may be correlated with the error term of the second stage. Therefore I also use another variable as an exclusion restriction, information on worries about one's financial situation in the previous period. This variable can serve as the exclusion restriction because the intensity of worries about someones financial situation should determine job search intensity, but not determine a person's wage. In the sample used in this paper, the average net monthly household income in the previous period of newly hired persons was significantly lower than the net monthly household income of persons who were not employed and stayed not employed. In addition, I use a variable capturing future fertility as another exclusion restriction. If a woman plans to have children, this will affect her search behaviour or the type of job she accepts (Polacheck 1981), for example part-time versus full-time, but not her final wage. Any effect on wages should then only depend on the job search process.

3 Results

3.1 Descriptive analysis

Tables 2 and 3 show summary statistics for the treatment group in column (1) and the three control groups in columns (2) – (4) for all variables which are used in the multivariate analysis. As I analyse the effect on labour market entry (or the extensive margin) and the effect on wages (or the intensive margin), table 2 refers to the variables used for the analysis of the extensive margin and table 3 refers to the variables used for the analysis of the intensive margin. These variables comprise the usual socio-demographic information as well as information on job search behaviour, employment biography and determinants of the opportunity cost of working.

Table 2 reveals some important differences about the groups' employment patterns. The number of observations is smallest for men aged 18–40 (column 2), even when comparing it to all women aged 18–40 (columns 1 and 3), indicating a higher labour force participation rate of men since the sample is conditioned on those observations out of the labour force in *t-1*. Men are slightly older, which may be due to military or civil service. They are also better qualified; the share with a university degree or a vocational degree is approximately twice as large as for women. Correspondingly, their work experience is higher than the work experience of women aged 18–40. This is also reflected in previous employment spells, which are more frequent for men. Table 2 also reveals a very telling fact about job search behaviour: more women than men in the sample were willing to accept both full-time and part-time jobs, even though the sample is restricted to those women who would not have a child at least for the next two years, thus ruling out that this job search behaviour might just reflect a pregnancy. At the same time, the difference between the groups in terms of their desire to work is not that large: approximately 73% of women in the sample and 81% of men expressed that they would like to work. In sum, it is pivotal to look at the job starters or individuals at a very early stage of their working life in order to keep the groups comparable and controlling for those variables which reflect the main differences between the groups for the DD approach to yield reliable results.

The other two control groups, women with children (column 3) and women aged 41–60 (column 4) also differ from the treatment group, though in different respects. Women with children in the age group 18–40 (column 3) are older and the majority is married. Their labour market experience and their educational level is correspondingly higher. Moreover, after tax household income is highest for this group (even when compared to women aged 41–60 who should be more experienced and thus earn more). This suggests that in these households more often the partner also adds a significant share of the household income. More of them are looking for a part-time job. The summary statistics for women aged 41–60 reveal a different educational pattern for those cohorts: most of then have secondary education plus vocational training, while most of the younger women have at least higher secondary education. In addition, most of the women aged 41–60 look for a part-time job only.

Table 3 adds some important information on wages for the four groups. The gross hourly wage is lowest for women without children aged 18–40. This should not be surprising given that this group has the lowest level of experience: the average age in this group is 26 while the average age of the women with children is 30, the average age for men in the group 18–40 is 29 and the average age among women in the group 41–60 is 49. Moreover, their previous unemployment experience as well as job market experience is comparatively low. At the same time, educational levels are comparable. However, employment patterns differ. Women aged 18–40 mostly work in the service sector (40%) while almost one fifth (17%) only have a temporary job. Temporary employment is much lower for men, most of whom work in the industrial sector. Almost 7% of the women aged 18–40 had previously been in education while this is true for only 2% of the men aged 18–40. These numbers become more similar if we compare women aged 18–40 without children to men aged 18–40 without children.

[Figure 1 about here.]

Figures 1 and 2 show the key variables of interest, transition into employment and wages, for men and women. The treatment group, women without children, aged 18–40, is shown as a red line. The left panel of figure 1 illustrates that there was more volatility in women's transition into employment and suggests that the end of the 1980s could have marked a change in trend. While the transition rate into employment continuously increased for men (despite German reunification at the end of the 1980s/beginning of the 1990s), it appears that the transition rate for women decreased between the year with the first major change to maternity leave legislation, 1988, and the year with the last major change to maternity leave legislation, 1993. In other words, before 1988 transition rates were comparable between men and women and only slightly lower for women, while after 1993, the transition rate for men was double the transition rate for women. The right panel of figure 1 illustrates that these differences are not driven by the age group 18–25 who are probably the job starters, for whom trends and differences in transition rates stay broadly the same for the whole period. This suggests that employment prospects did not differ as much for men and women during the early phases of their careers. Instead, the main difference seems to have emerged during a phase in life which could be considered the 'prime age' for having children: ages 26–40.

[Figure 2 about here.]

Figure 2 shows the corresponding entry wages for the two groups. Interestingly, neither for the full sample (ages 18–40) nor for the younger group (ages 18–25) there seems to be a difference in trend or a larger difference between entry wages for men and women after the reforms. In other words, there seems to be a stable gender wage gap for those who manage to enter the labour market again. This could indicate that it is more difficult to reduce wages of women than to reduce the number of women hired.

[Figure 3 about here.]

Figures 3 and 4 complete the picture with a comparison for the other two control groups. Figure 3 presents a comparison of the extensive margin for women in the same age group, with and without children and for women without children, but in different age groups. It seems from the left panel of figure 4 that the transition into employment was fairly similar between women with and without children before 1989. However, the share increased more strongly for women with children after 1989 than for women without children. Levels seem to have adjusted again in the late 1990s. Even though at this stage it cannot be ruled out that these developments were driven by changes in maternity leave legislation, the timing suggests that this could also be a reunification effect since mothers in the former German Democratic Republic (GDR) usually worked and therefore more young women also had children (Büchel and Spiess 2004; Kreyenfeld 2004). Hence, the effect we see in the data could simply be related to the higher share of mothers in the former GDR. The right panel provides a comparison for women without children in two different age groups. Developments in those two groups are broadly similar, which suggests that the relevant employment effect appeared in the choice of men over women.

[Figure 4 about here.]

Figure 4 confirms this conjecture also in terms of entry wages. Neither in the left nor in the right panel there is a change in trend for any of the groups. While the effect of German reunification is clearly visible for all groups, it did not affect the difference in wages or the trend common to all groups.

3.2 Multivariate analysis

3.2.1 Extensive margin: employment opportunities

The basic anaylses for the extensive employment margin suggest that employers became more restrictive in hiring as dismissal costs were (implicitly) increased through more extensive employment protection for mothers. This evidence is presented in table 4. Table 4 contains all estimators discussed in section 2.3. In order to keep it simple and the coefficients straightforward to interpret, table 4 presents all estimators using OLS. Maximum likelihood estimation yields qualitatively the same estimates of marginal effects. None of the specifications in table 4 contains information on previous job type, because this information is only available up to 1994. Including this information where available does not affect the estimated coefficients.

[Table 4 about here.]

Column (1) in table 4 shows the estimated effects of the policy changes on the probability of being employed in period t if not employed in period $t-1$ using OLS without standard error correction and without additional controls. The model includes one treatment group, women aged 18–40 without children, and three control groups. The specification in column (2) uses Huber/Eicker/White standard errors and includes relevant socio-demographic and labour market specific controls, as listed in table 2. Column (3) presents a specification with an inflated number of groups, i. e. declaring each group/year cell, in total 60, to be a different group. The model is estimated using FGLS. Specification (4) uses the residual aggregation technique described by Bertrand et al. (2004). Residuals are aggregated to group/year cells after a regression on the control variables within the pooled sample in the first step. In the second step, the effect of a treatment dummy is checked for the treated units only. The degrees of freedom are reduced by the first step of aggregation. Year fixed effects are included in the first step. Specification (5) uses the type of residual aggregation suggested by Donald and Lang (2007). Residuals are defined as the difference between mean outcome and mean predicted outcome and are also aggregated to group/year cells after controlling for socio-demographic and labour market factors in the pooled sample. Year fixed effects are included in the second step. The first step regression also implies a reduction in the degrees of freedom.

Some reforms, even though implemented in several steps, cannot be treated as separate reforms. The first changes in the 1980s took place in 1986 and 1988, but both the 1988 and the 1986 change were announced jointly. After those reforms coming into effect, mothers were granted a full year of job-protected maternity leave. Similarly, the 1989 and 1990 reforms were also announced jointly. Therefore, the dummy for 1988 should be interpreted as capturing both the joint 1986/1988 reforms while there is a joint dummy for the 1989/1990 reforms.[14] All columns show policy estimates for the policy changes in 1992 and 1992 separately and jointly. This approach allows for an effect of either the extension in the job protection period (1992) or the corresponding extension of the benefit payment period (1993) or of both changes jointly, i.e. for the two reforms reinforcing each other.

The simple difference-in-differences model in column (1) suggests a significant negative effect of the 1986/1988 policy change (granting a full year of job-protected leave) on the likelihood that women in childbearing age moved into employment if they were not working in the previous period. The coefficient is largely the same when including controls and using robust standard errors in column (2). Using either the Bertrand et al. (2004) or the Donald and Lang (2007) aggregation techniques presented in column (4) and column (5) respectively suggests that the significance of this result is robust even when accounting for group-correlated errors.

In addition, the basic model suggests that there is no additional effect of the 1989/1990 reform package, which implied another increase of the job protection and payment period of 6 months, while there is somewhat weaker evidence of an impact of the prolongation of the job protection period and payment period in 1992/1993. In 1992, the job protection period was doubled from 18 to 36 months and in 1993, the payment period was increased by one year from 24 to 36 months. While none of the approaches indicate a significant effect of the

[14] It was not possible to include a joint dummy for the years 1986 and 1988 since 1986 is the first year which we can include in the analysis when using a full set of controls, including lags.

1989/1990 prolongation of job protection and benefit payments, the pooled model with the simple cluster correction and control variables indicates that there may also have been an effect of the extension of the job protection period in 1992. The model with covariates in column (2) suggests a 2% decrease of the employment rate of women in childbearing age as a consequence of the 1992 prolongation of the job protection period. However, the effect is not significant when accounting for group-correlated errors in a more sophisticated way.

These results are in line with the findings of Schönberg and Ludsteck (2014) who show that the extension from 2 to 6 months led to the highest number of delays in the return to work among mothers. Therefore, it is straightforward to conjecture that this changed behaviour also led to the strongest increase in employers' reluctance to hire women in childbearing age. The results render support to this conjecture.

3.2.2 Extensive margin: sensitivity analysis

In order to check the persistence of the effect I perform the same analysis for placebo treatments between 1993 and 2000, because in 2000 another policy change was announced. These placebo treatments are shown in table 5. Similar to the dummies for the refom years, I construct treatment dummies encompassing two years: 1995/1995, 1996/1997 and 1998/1999.[15] For the 1994/1995 placebo treatment, the OLS model with covariates indicates a small significant effect, possibly a persistence of the 1992/1993 reform. However, this effect is small compared to the effects 1986/1988 and even to the less significant 1992/1993 effect and does not remain significant when accounting for group-correlated errors in a more sophisticated way. All other years only indicate an effect close to zero, which is not significantly different from zero.

[Table 5 about here.]

The results do not change when using a more narrow definition of treatment group and control group and only comparing women aged 18–40 without children to men aged 18–40 without children. When restricting the sample to individuals with higher education, tentative results indicate insignificant effects close to zero.[16]

While the transition into employment may have been more difficult because of changes in labour demand, labour supply may also have reacted to the fact that it became more difficult to find a job. For example, women may have had to look for a job more intensively than

[15] I do not find significant effects for overlapping definitions of the placebo treatments.

[16] Note however that the sample size is comparatively small.

before. Job search intensity is not a measure available in the SOEP data, but information on job search is available for the German Microcensus scientific use files. The German Microcensus is a representative 1% sample from the German population, which is collected since 1957, albeit not annually.[17]

For the period of interest in this paper, cross sections are available for years 1982, 1987, 1991 and 1993. Consequently, we can run DD models comparing the 1987 and the 1991 Microcensus files and comparing the 1991 and the 1993 Microcensus files to investigate whether there was a change in job search intensity among women after the respective reforms. This implies that the impact of the 1986/1988 reform package, the impact of the 1989/1990 reform package, and the joint impact of the 1992/1993 reforms can be investigated. Moreover, there is an additional advantage to having a look at the Microcensus files. Since the number of observations is considerably larger, even among those out of the labour force the share of individuals with a university degree is large enough to pursue a separate analysis for a highly educated sample.

The Microcensus files contain categorical information on both the length of the spell of unemployment and on the actual time of active job search from which I construct the measure of search intensity. If actual search time exceeds unemployment duration, the person has searched before actually becoming unemployed, which can be interpreted as a signal that the person had either anticipated unemployment, had been looking for a job change or knew that the job search process would probably be lengthy. Therefore, I define search intensity as actual time of job search net of the duration of unemployment. Consequently, positive values imply a high search intensity, while negative values imply a low search intensity. The distribution of this measure of job search intensity is shown in figure 5. The top panels of the figure show search intensity for women and men in childbearing age separately, while the bottom panels show search intensity for women younger than 41 as compared to women older than 41, but younger than 60. Both the bottom and the top panel of figure 5 show job search intensity for the four available cross sections of the Microcensus: 1982, 1987, 1991 and 1993.

[Figure 5 about here.]

[17] Since the question on job search is only posed to unemployed persons, the analysis of job search intensity can only be conducted for a sample of persons who are unemployed. The unemployed in in the 1991/1993 sample do not differ strongly from the 1991/1993 full sample. They are somewhat more educated, with a higher percentage owning a higher secondary school degree as their highest educational attainment. While this is also the case in the 1987/1982 sample, the unemployed here seem to be less educated than in the full sample. The unemployed are less likely to be married in both cross-sections. The full summary statistics are available from the author upon request.

The mode in all panels is 0, which is the standard case of starting to search when becoming unemployed. In 1987, as compared to 1982, there is an increase of the frequency of the top category for both men and women, but the reduction in the frequency of the bottom values is stronger for women, shown on the top left panel. The same spike of an increased frequency in the top category also shows when comparing women aged 41–60 to women aged 18–40, shown on the bottom left panel. It is not clear, however, whether the decrease in the bottom values was stronger among women aged 41–60 or among women aged 18–40. In 1991, search intensity is weaker for both men and women aged 18–40, displayed on the top right panel. The pattern is largely the same when comparing women aged 41–60 to women aged 18–40, displayed on the bottom right panel. These patterns suggest that significant changes in job search intensity are more likely to have taken place in the 1980s.

To find out whether there were systematic differences in the differences between the groups, I run a DD model on the categorical data. Tables 6 and 7 present the respective DD models for job search intensity. Since the dependent variable is a categorical variable, the magnitude of the coefficients from the ordered probit model is not comparable to the models with the SOEP data.

[Table 6 about here.]

[Table 7 about here.]

Table 6 indicates a significant effect on job search intensity among women aged 18–40 after the reform packages of the 1980s, but only when compared to women aged 41–60 and not when compared to men and only for women without a university degree. Table 7 which presents results for the 1992/1993 reform package indicates the same, but with respect to a different control group: a significant change in job search intensity among women aged 18–40 when compared to men, also only for women without a university degree. The effects of other determinants of job search are of similar magnitude for the sample without a university degree and for the sample with a university degree respectively.

The interesting implication of these findings is that they indicate a higher job search intensity for those years in which the analysis with SOEP data did not show significant effects for the transition into employment. Therefore, it is possible that the reform had effects on both demand and supply of labour from women in childbearing age. A consequence of this interpretation would be that the total effect could potentially be even larger.

A confirmation for the analysis with SOEP data mainly reflecting labour demand can be obtained from analysing the effect on other employment changes. When running a DD model on a depedent variable that only indicates change in employment status, I do not find significant effects. Moreover, the SOEP data do not show a significant effect of maternity leave reforms on the probability of women in childbearing age to end up in a temporary job.[18]

3.3 Intensive Margin: Wages

The second possibility for the employer to react to changes in the opportunity cost of hiring women could be a wage-related risk premium for the possibility that some of them might have children. As discussed in section 2.3 this requires a selection model taking into account selectivity already at the extensive margin. Similarly to the extensive margin, it is difficult for employers to change wage agreements in signed contracts, which is why any effect on wages should mainly show in the wages of newly hired persons.

To establish a baseline, table 8 first presents a DD model for wages of different groups of the working population and for different treatment groups, using a simple OLS model and a two-step approach with aggregating the mean predicted outcome, while not accounting for the selection of the sample. In particular, columns (1) – (3) present the results for the sample out of the labour force (OLF) in $t-1$. Columns (4) – (6) present the results for all persons employed in a full-time or part time job. Results in columns (2) and (5) have been derived using only men aged 18–40 as a control group. The dependent variable is the log hourly wage in Deutsche Mark (DM). There is only a marginally significant effect for the OLF sample in the 1980s which however disappears when using aggregation. The significant effect of the 1992 reform is not visible when using more than one control group and also disappears when using aggregation.

[Table 8 about here.]

As the coefficients in table 8 are derived from an OLS estimation on a selected sample, the estimators in table 9 take potential selection in the full sample into account. Moreover, since the second stage can include the full sample of employed, it is large enough to differentiate between a sub-sample with and without university education. First stage results

[18]These analyses as well as a table with marginal effects for the analyses on job search intensity can be obtained from the author upon request. Temporary job contracts are a means of counteracting 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). 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.

are not presented, but confirm the results from the extensive margin analysis. In particular, they also indicate negative employment effects for the 1980s reforms, and to some extent for the 1993 reform, albeit not as clearly as for the OLF sample.

The model presented in column (1) of table 9 corresponds to the models presented in table 8. The results in column (1) broadly confirm the OLS results in table 8. Although the coefficient for the 1986/1988 refom in column (1) of table 9 is similar to the coefficients in row 1 of table 8, it is not statistically significant. Moreover, even though table 8 suggests a negative joint effect of the 1992/1993 reform, this cannot be confirmed with the selection model in which both the separate and the joint effects of the 1992/1993 reform are statistically insignificant.

[Table 9 about here.]

At the same time, table 9 reveals some interesting findings about different segments of the labour market. First, the overall effect on the wages of 'labour market insiders' seems to have been positive. There is a significant positive effect in 1992 compared to 1990/1991 for all groups except for the OLF sample, which is highest for the highly educated group.[19] Second, the positive effect on wages in 1992 also seems to drive the positive joint effect of the 1992/1993 reforms. When treating all reforms between 1988 and 1993 as a single package and collapsing the data to two time periods (before 1988 and after 1993), the overall effect on the wages of women in childbearing age is negative (for the OLF sample and also for those who work) while the effect on the wages of women in childbearing age with a university degree is positive.

There are several explanations for a positive wage effect of the 1992 reform when not restricting the sample to the newly hired. When looking at the 1992/1993 reform package, the changes which came into effect in 1992 indeed seem to be the more important ones. Maternity leave was paid and the job protection period was doubled. It seems reasonable to assume that these changes solidified the incentives to exit the labour market when having a child – and also the corresponding expectations of employers. In 1993, only the payment period was extended.

Higher wages of insiders after these changes could reflect two phenomena. First, higher wages could be a reward for stronger labour market attachment, i.e. reflect employers' efforts

[19] It is possible to argue that these positive effects could be related to a reunification boom. The SOEP group has collected data for the eastern part of Germany since 1990, which has e.g. caused a spike in aggregate employment rates in the data set in 1990. However, this data composition issue has affected all groups used in this study in the same fashion. Moreover, correlation with GDP growth is also similar for employment rates and wage growth for all groups.

to raise the opportunity cost for going on leave. Second, higher wages could reflect a higher effort level of among women through a self-selection process. It is possible that those women who did not intended to have children had to make a higher effort to signal their labour market attachment to their employer, resulting also in higher wages. While the data set contains information on skill levels, effort and other ways of signalling are beyond the scope of the SOEP data set. One indication for the latter explanation playing a role is that the positive wage effect disappears when adding a variable on the (future) number of children as an explanatory variable.

3.3.1 Intensive margin: sensitivity analysis

To illustrate that the effects shown in the previous section are not just spurious or a simple time trend, table 10 provides an analysis of placebo treatments also for the intensive margin. Like before, these placebo treatments are defined as two-year periods. Column (1) and column (2) provide pooled OLS estimates with only Huber/Eicker/White standard errors, for the sample with 3 control groups in column (1) and for the sample with 1 control group in column (2). Column (3) and column (4) show results for the same samples, but with an additonal standard error correction by aggregation. Columns (5) and (6) complement these with an additional estimation of wages, not restricting the sample to those previously out of the labour force, for a sample with 3 control groups in column (5) and the sample with 1 control group in column (6). Since the population is different for columns (5) and (6) the different estimates should not be surprising.

While there are small differences in the coefficients between the different estimation approaches in columns (1) to (4) none of the coefficients are statistically significant. Only one coefficient in the selection model (with a different population) is marginally significant in column (5). One reason for the marginally significant coefficient when using the selection model (table 10, column (6)) may be the anticipation of the 2001 reform, which was announced in 2000. This granted a parent the right to part-time work and thus reduced the need for women without children to signal labour market attachment.

[Table 10 about here.]

4 Conclusion

This paper sheds light on the effects of maternity leave legislation on labour demand by analysing the labour market outcomes for women in childbearing age without childen. By using exogenous variation in the length of the job protection and benefit entitlement 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 assumes that a longer (protected) leave period imposes costs on the employer which leads to a change in labour demand for those employees who are potentially affected by the longer (protected) leave period. In particular, a long spell of leave requires finding and training a replacement and while it is associated with depreciation of the human capital of the employee who is on leave. As the employer cannot know which female employee becomes pregnant and when, a risk premium has to be borne by all female employees.

The contribution of the empirical analysis is twofold. First, I estimate whether employment opportunities change after changes in legislation which are exogenous to labour market dynamics with a difference-in-differences approach, using data from the German Socio-Economic Panel (SOEP). The analysis reveals that a doubling of the job-protected leave period from 6 months to 12 months between 1986 and 1988 led to an approximately 6% lower probability for women in childbearing age to be hired, mainly affecting women without a university degree. I do not find a negative effect on the probability of being hired for the next major change, a tripling of the job-protected leave period to 36 months between 1988 and 1992 in addition to 18 months of maternity pay. However, a complementary analysis with samples from the German Microcensus provides some evidence for intensified job search among women in childbearing age compared to other groups in the early 1990s. Second, I use a selection model to find out whether the wages of women in childbearing age without children were affected by the reforms. I find some evidence for decreased wages for the newly hired of both the 1980s and the 1990s reform package. However, especially after the 1992 reform, the wages of women in childbearing age who already had a job are 5-10% *higher* compared to the control groups used in this study. Since this effect disappears when controlling for the future number of children, the higher wages may indicate an increased need to signal labour market attachment by increased effort after the reform.

This analysis therefore offers evidence on the labour market effects of job protection measures in combination with a replacement income. If such measures represent an opportunity cost for the employer, the employer demands a risk premium from the group affected by the measure. Particularly in rigid labour markets like the German one in the 1980s and 1990s, such risk premia have to be borne by those newly hired. In my study, those potentially affected by the 1980s reform package found it more difficult to move into employment if previously not employed. Regarding the particular impact of job protection in the context of maternity leave, the critical spell which worsens the labour market position seems to be between 6 and 12 months of leave, confirms previous analyses on mothers' labour supply

(Schönberg and Ludsteck 2014).

While the policy reforms of the 1980s and 1990s may have had adverse effects on women in childbearing age by affecting labour demand and return to work may be associated with a wage penalty (Beblo et al. 2009), there were also positive effects on mothers' labour supply (inter alia Ruhm 1998; Schönberg and Ludsteck 2014). However, to effectively raise female labour force participation, both for mothers and women without children, both labour demand and labour supply effects should be considered. After all, women might decide for different jobs and careers in view of difficulties of finding a suitable position. In this context, recent reforms which have reduced the payment period for maternity benefits while raising the level and which have also aimed at providing better childcare facilities can help affecting female labour force participation in Germany positively.

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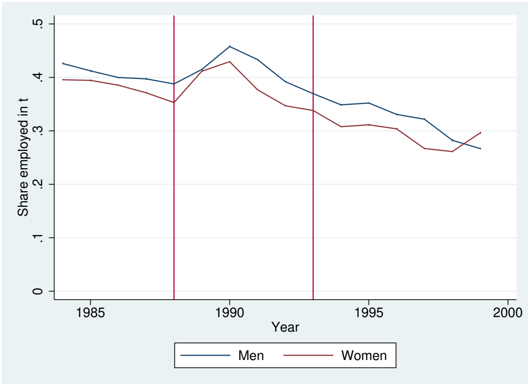
Appendix

Figures

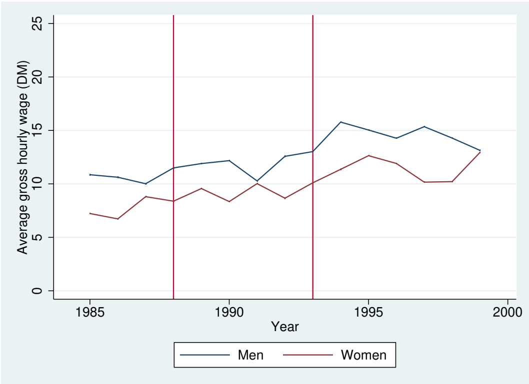
Figure 1: Extensive margin I

Average employment rate for persons previously out of the labour force who do not (yet) have children, ages 18–40. Vertical lines indicate years 1888 and 1993.



Average employment rate for persons previously out of the labour force who do not (yet) have children, ages 18–25. Vertical lines indicate years 1888 and 1993.

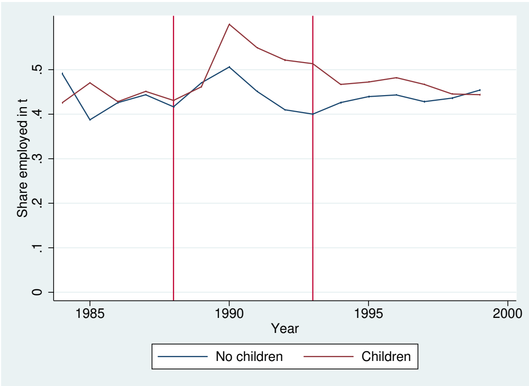
Figure 2: Intensive margin I



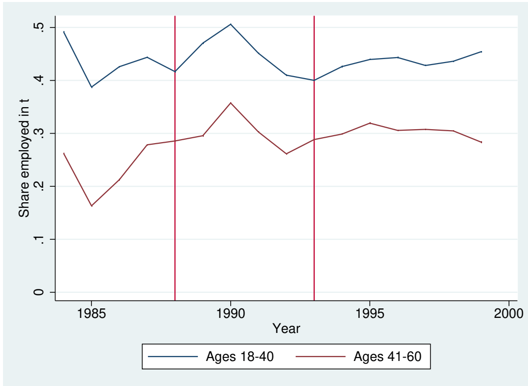
Average entry wage for persons previously out of the labour force who do not (yet) have children, ages 18–40. Vertical lines indicate years 1888 and 1993.

Average entry wage for persons previously out of the labour force who do not (yet) have children, ages 18–25. Vertical lines indicate years 1888 and 1993.

Figure 3: Extensive margin II

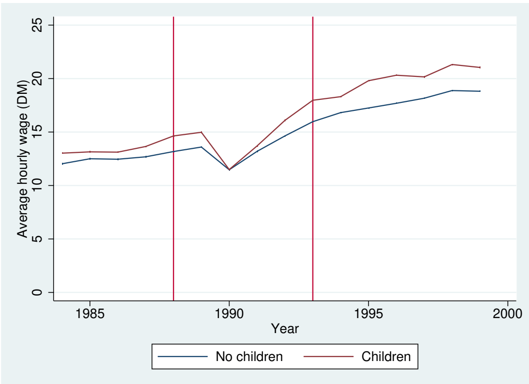


Average employment rate for women previously out of the labour force, ages 18–40. Vertical lines indicate years 1888 and 1993.

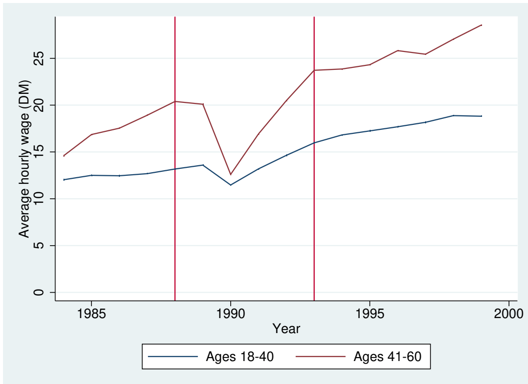


Average employment rate for women previously out of the labour force who do not (yet) have children. Vertical lines indicate years 1888 and 1993.

Figure 4: Intensive margin II

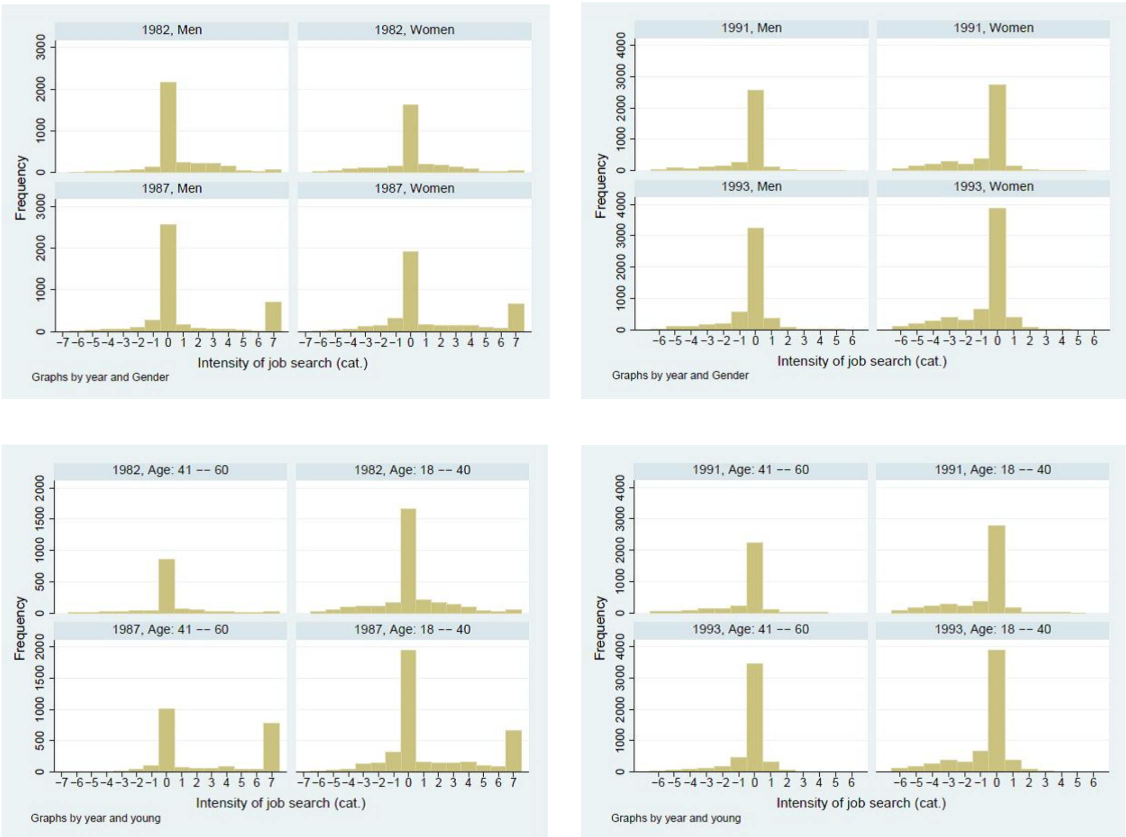


Average entry wage for women previously out of the labour force, ages 18–40. Vertical lines indicate years 1888 and 1993.



Average entry wage for women previously out of the labour force who do not (yet) have children. Vertical lines indicate years 1888 and 1993.

Figure 5: Job Search Intensity (cat.) in the Pooled Microcensus Samples, by Gender and Age



Job search intensity is measured as the time of search, which exceeds the actual period of unemployment, i. e. negative values imply that the actual period of unemployment is longer than the period of actual search. Coding of the original variables (both period of unemployment and period of job search): 1: less than 1 month, 2: 1-3 months, 3: 3-6 months, 4: 6-12 months, 5: 12-18 months, 6: 18-24 months, 7: 24 months and more. Top panels: men and women aged 18–40. Bottom panels: women aged 18–40 and women aged 41–60.

Tables

Table 1: Maternity Leave Legislation in Germany

Time of Taking Effect	**Change**	**Published (*Bundesgesetzblatt*)**
January 1966	2 months of job-protected maternity leave.	November 16, 1965
May 1979	6 months of job-protected maternity leave.	June 30, 1979
January 1986	10 months of job-protected maternity leave.	December 6, 1985
January 1988	12 months of job-protected maternity leave.	December 6, 1985
July 1989	15 months of job-protected maternity leave.	July 7, 1989
July 1990	18 months of job-protected maternity leave.	July 7, 1989
January 1992	18 months of paid maternity leave; 36 months of job protection.	January 21, 1992
January 1993	24 months of paid maternity leave; 36 months of job protection.	January 21, 1992
December 1997	Incremental changes in the applicability rules.	–
January 2, 2001	36 months of job-protected leave for mothers and fathers simultaneously, 24 months of which paid; entitlement to part-time job with same employer upon request of employee; maternity benefits of 450 a€ month, if parent agrees to draw the benefits for 12 months only.	December 5, 2000
January 1, 2007	Paternity pay changed to 67% of last net income (max.: 1800/€month, min.: 300/€month) for the first 12 months, and for additional 2 months if the other parent takes leave for these two months; more generous rules for low-income parents or parents of more children younger than 3 years of age.	December 11, 2006

If not specified differently, the mother is, first, entitled to maternity pay, which is borne by the employer similar to sick pay, for the first two months after delivery and 6 weeks before delivery. Maternity benefits are flat-rate around 300 /€month, and paid by the government.

Changes to the law of a purely judicial nature, such as changes, which affect the right to go to court, are not displayed.

Table 2: Summary Statistics – Employment

	Women, 18–40, no children				Men, aged 18–40, no children				Women, 18-40, children				Women, 41–60			
Variable	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max
Employed (D)	0.12	0.32	0	1	0.22	0.41	0	1	0.14	0.35	0	1	0.07	0.25	0	1
German (D)	0.87	0.34	0	1	0.84	0.37	0	1	0.71	0.45	0	1	0.73	0.44	0	1
Age, L1	21.11	4.95	17.00	39.00	23.52	5.63	17.00	39.00	29.79	5.89	17.00	39.00	50.14	5.95	40.00	59
Married, L1 (D)	0.08	0.27	0	1	0.15	0.36	0	1	0.82	0.38	0	1	0.88	0.33	0	1
Experience: unemployed (yrs.), L1	0.23	1.00	0	17.80	0.48	1.21	0	11.10	0.54	1.15	0	15.70	0.63	1.63	0	26
Experience: full-time empl. (yrs.), L1	0.84	2.56	0	20.20	2.13	4.20	0	22.80	4.22	4.40	0	22.80	10.39	10.03	0	43
Experience: part-time empl. (yrs.), L1	0.26	1.30	0	20.00	0.21	0.75	0	12.00	0.75	1.75	0	20.00	2.79	5.46	0	34
Education (yrs.), L1	9.81	2.52	7.00	18.00	10.50	2.57	7.00	18.00	10.60	2.28	7.00	18.00	9.99	2.00	7.00	18
University degree, L1 (D)	0.02	0.13	0	1	0.02	0.15	0	1	0.03	0.17	0	1	0.02	0.14	0	1
Vocational degree, L1 (D)	0.16	0.36	0	1	0.30	0.46	0	1	0.54	0.50	0	1	0.49	0.50	0	1
HH income (*t-1*) - HH income (*t-2*), pre-government (DM 1000)	0.11	29.88	-536.70	132.92	-0.46	27.44	-490.25	149.60	-0.30	26.57	-945.60	155.12	-0.59	24.80	-777.48	149.12
HH income (*t-1*) - HH income (*t-2*), post-government (DM 1000)	0.76	19.45	-321.43	88.87	0.97	17.72	-263.81	124.11	1.18	16.83	-426.86	121.96	1.10	16.29	-354.20	124.11
Casmin: 2c , L1 (D)	0.19	0.39	0	1	0.26	0.44	0	1	0.07	0.26	0	1	0.02	0.13	0	1
Casmin: 2a , L1 (D)	0.07	0.25	0	1	0.10	0.30	0	1	0.23	0.42	0	1	0.12	0.32	0	1
Casmin: 2b , L1 (D)	0.19	0.39	0	1	0.12	0.32	0	1	0.06	0.23	0	1	0.02	0.15	0	1
Looking for full-time job, L1 (D)	0.52	0.50	0	1	0.68	0.47	0	1	0.15	0.35	0	1	0.07	0.26	0	1
Looking for part-time job, L1 (D)	0.05	0.21	0	1	0.02	0.14	0	1	0.38	0.49	0	1	0.15	0.35	0	1
Looking for full/part time, L1 (D)	0.10	0.30	0	1	0.05	0.23	0	1	0.08	0.27	0	1	0.05	0.21	0	1
Looking for a job: do not care, L1 (D)	0.09	0.29	0	1	0.05	0.22	0	1	0.06	0.23	0	1	0.02	0.13	0	1
East Germany (D)	0.12	0.32	0	1	0.12	0.32	0	1	0.11	0.31	0	1	0.13	0.33	0	1
Want to work, L1 (D)	0.73	0.44	0	1	0.81	0.39	0	1	0.42	0.49	0	1	0.14	0.35	0	1
Employed, L2 (D)	0.07	0.26	0	1	0.17	0.38	0	1	0.16	0.37	0	1	0.11	0.31	0	1
Year	1992	4	1986	1997	1992	4	1986	1997	1992	4	1986	1997	1991	4	1986	1997
Observations	2821				6136				9049				9127			

Sample conditioned on not being employed in *t-1* and on pre-government household income smaller than DM 200.000.
Only age group 18–60 between 1984 and 1997 included. Men older than 40 excluded.
(1): Women aged 18–40 without children.
(2): Women aged 18–40 with children.
(3): Men aged 18–40.
(4): Women aged 41–60.
Casmin classifications: 2a = secondary education plus vocational training. 2b = secondary education without vocational training. 2c = higher secondary education with and without vocational training.

Table 3: Summary Statistics – Wages

	Women, 18–40, no children				Men, aged 18–40, no children				Women, 18-40, children				Women, 41–60			
Variable	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max
Gross hourly wage (DM)	14.93	8.26	0	158.31	18.53	9.95	0	416.4288	16.24	8.14	0	187.39	19.65	9.31	0	156.22
German (D)	0.89	0.31	0	1	0.75	0.43	0	1	0.80	0.40	0	1	0.78	0.41	0	1
Age	26.01	6.12	16	40	49.10	5.56	41	86	30.84	6.12	17	40	29.86	6.25	18	40
Married (D)	0.21	0.40	0	1	0.79	0.41	0	1	0.68	0.46	0	1	0.56	0.50	0	1
Experience: unemployed (yrs.), L1	0.20	0.57	0	7.6	0.32	1.04	0	16	0.31	0.79	0	12	0.27	0.78	0	18
Experience: full-time empl. (yrs.), L1	5.16	5.55	0	24.6	18.03	10.56	0	47	7.14	5.52	0	25	8.74	6.29	0	25.1
Experience: part-time empl. (yrs.), L1	0.49	1.70	0	17	5.56	7.47	0	39	1.82	3.17	0	25	0.21	0.95	0	21
Education (yrs)	11.46	2.31	7	18	10.88	2.48	7	18	11.44	2.38	7	18	11.30	2.41	7	18
University degree (D)	0.06	0.23	0	1	0.06	0.23	0	1	0.06	0.23	0	1	0.07	0.25	0	1
Voc. Degree (D)	0.58	0.49	0	1	0.57	0.49	0	1	0.62	0.48	0	1	0.65	0.48	0	1
Casmin: 2c (D)	0.13	0.34	0	1	0.02	0.15	0	1	0.07	0.26	0	1	0.08	0.27	0	1
Casmin: 2a (D)	0.28	0.45	0	1	0.19	0.39	0	1	0.31	0.46	0	1	0.24	0.43	0	1
Casmin: 2b (D)	0.16	0.36	0	1	0.02	0.15	0	1	0.07	0.25	0	1	0.06	0.24	0	1
Firm size (categ.)	7.50	2.57	1	10	7.81	2.33	1	10	7.48	2.50	1	10	7.98	2.22	1	10
Temporary job (D)	0.18	0.38	0	1	0.04	0.19	0	1	0.09	0.29	0	1	0.09	0.29	0	1
Sector: service (D)	0.41	0.49	0	1	0.37	0.48	0	1	0.40	0.49	0	1	0.12	0.33	0	1
Sector: industry (D)	0.15	0.36	0	1	0.19	0.39	0	1	0.18	0.39	0	1	0.35	0.48	0	1
Sector: construction (D)	0.04	0.19	0	1	0.04	0.19	0	1	0.04	0.20	0	1	0.21	0.41	0	1
Sector: retail (D)	0.16	0.37	0	1	0.14	0.35	0	1	0.15	0.35	0	1	0.08	0.28	0	1
Sector: public service (D)	0.10	0.30	0	1	0.10	0.30	0	1	0.08	0.28	0	1	0.08	0.27	0	1
Not working, L1 (D)	0.01	0.09	0	1	0.02	0.12	0	1	0.03	0.18	0	1	0.00	0.07	0	1
Not working: irregular second job, L1 (D)	0.01	0.08	0	1	0.00	0.04	0	1	0.01	0.08	0	1	0.00	0.06	0	1
Not working: regular second job, L1 (D)	0.01	0.08	0	1	0.00	0.05	0	1	0.00	0.07	0	1	0.00	0.05	0	1
In education, L1 (D)	0.07	0.25	0	1	0.00	0.03	0	1	0.02	0.13	0	1	0.03	0.16	0	1
Unemployed, L1 (D)	0.02	0.15	0	1	0.02	0.15	0	1	0.03	0.17	0	1	0.02	0.15	0	1
Military service, L1 (D)	0.00	0.00	0	0	0	0	0	0	0	0	0	0	0.01	0.10	0	1
East Germany (D)	0.13	0.33	0	1	0.2318085	0.4220077	0	1	0.26	0.44	0	1	0.17	0.37	0	1
Year	1992	4	1985	1997	1992	4	1985	1997	1992	4	1985	1997	1991	4	1985	1997
Observations	5361.00				10362				10758				21034			

Sample conditioned on not being employed in *t-1* and on pre-government household income smaller than DM 200.000. Men older than 40 excluded.
Casmin classifications: 2a = secondary education plus vocational training. 2b = secondary education without vocational training. 2c = higher secondary education with and without vocational training.

Table 4: Regression Results – Employment Effects

	Pooled OLS	Pooled OLS	FGLS	Pooled OLS	Two-Step	Group/ year cells	Obs.
	(1)	(2)	(3)	(4)	(5)		
Year: 1988 (D)	-.056	-.061	-.063	-.056	-.052	12	7030
	(.027)**	(.011)**	(.025)**	(.031)	(.023)*		
Year: 1989–1990 (D)	-.032	-.026	-.027	-.024	-.035	20	11095
	(.021)	(.016)	(.020)	(.039)	(.023)		
Year: 1992 (D)	-.071	-.022	-.022	-.019	-.020	28	15147
	(.025)***	(.007)**	(.023)	(.043)	(.027)		
Year: 1993 (D)	-.055	-.015	-.018	-.015	-.013	32	17573
	(.010)**	(.008)	(.023)	(.040)	(.026)		
Year: 1992–1993 (D)	-.066	-.022	-.023	-.019	-.018	32	17573
	(.019)***	(.006)**	(.018)	(.030)	(.019)		
Control variables included	NO	YES	YES	YES	YES		
Year fixed effects	YES	YES	YES	YES	YES		
Standard error correction	NO	YES	YES	YES	YES		
		group level	group level	aggr.	group level		
Distribution for test statistic (last stage)	$t(3 + years)$	$t(48)$	$z(32)$	$t(g-3)$	$t(g-3)$		
Number of groups (last stage)	4	60	4	4	4		

*: significant at the 10% level, **: significant at the 5% level, ***: significant at the 1% level. Significance levels are derived using the t($g-2$) distribution and a two-sided test. A table including confidence intervals can be obtained from the author upon request.
Years in the table indicated the difference-in-differences estimates, i.e. the interaction effect (which is an interaction dummy in the first column for the pooled sample, but a year dummy for those approaches with a first step of aggregation).The sample consists of persons out of the labour force in $t-1$. Treatment periods are chosen according to the laws coming into effect. Treatment years are compared to years *before* the treatment. Additional control variables as shown in table 2.
(1): No correction.
(2): Huber/Eicker/White standard errors.
(3): Random effects model with each group/year cell defined as a distinct group.
(4): Residual aggregation as described in Bertrand et al. (2004).
(5): Residual aggregation as described in Donald and Lang (2007). Assumption: difference between the difference in employment rates is i.i.d.

Table 5: Extensive margin: placebo treatments

	Pooled OLS	Pooled OLS	FGLS	Pooled OLS	Two-Step	Group / year cells	Obs.
	(1)	(2)	(3)	(4)	(5)		
Year: 1994–1995 (D)	-.031	-.008	-.005	-.006	.002	44	24937
	(.018)*	(.001)**	(.017)	(.026)	(.018)		
Year: 1996–1997 (D)	-.022	-.003	-.003	-.004	-.010	52	29755
	(.017)	(.006)	(.017)	(.023)	(.016)		
Year: 1998–1999 (D)	.015	.010	.006	.006	-.008	60	34079
	(.018)	(.018)	(.020)	(.023)	(.016)		
Control variables included	NO	YES	YES	YES	YES		
Year fixed effects	YES	YES	YES	YES	YES		
Standard error correction	NO	YES	YES	YES	YES		
		group level	group level	aggr.	group level		
Distribution for test statistic (last stage)	$t(3+years)$	$t(48)$	$z(32)$	$t(g-3)$	$t(g-3)$		
Number of groups (last stage)	4	60	4	4	4		

*: significant at the 10% level, **: significant at the 5% level, ***: significant at the 1% level. Significance levels are derived using the t$(g-2)$ distribution and a two-sided test. A table including confidence intervals can be obtained from the author upon request.
Years in the table indicated the difference-in-differences estimates, i.e. the interaction effect (which is an interaction dummy in the first column for the pooled sample, but a year dummy for those approaches with a first step of aggregation). The sample consists of persons out of the labour force in $t-1$. Treatment = $year \geq 1992$ is associated with gradual changes in the length of job protection from 1988 to 1990 and in 1992 and with gradual changes in the length of paid leave from 1988 to 1990. Treatment = $year \geq 1993$ is associated with a change in the length of paid leave. Additional control variables as shown in table 2.
(1): No correction.
(2): Huber/Eicker/White standard errors.
(3): Random effects model with each group/year cell defined as a distinct group.
(4): Residual aggregation as described in Bertrand et al. (2004).
(5): Residual aggregation as described in Donald and Lang (2007). Assumption: difference between the difference in employment rates is i.i.d.

Table 6: Regression results – Intensity of Job Search 1987 and 1991

	(1)	(2)	(3)	(4)
Dep. var.: Intensity of job search (cat.)				
Year: 1991 * gender (D)	.047 (.067)	-.201 (.191)		
Gender (D)	-.035 (.038)	.047 (.104)		
Year 1991 * age $\leq$ 40 (D)			.250 (.074)***	-.243 (.296)
Age $\leq$ 40 (D)			-.187 (.085)**	.012 (.384)
Year: 1991 (D)	.440 (.049)***	.739 (.178)***	.246 (.056)***	.793 (.294)***
Age	.035 (.025)	.255 (.145)*	.020 (.011)*	.177 (.062)***
Age sq.	-.0003 (.0004)	-.003 (.002)	-.0002 (.0001)	-.002 (.0008)**
Married (D)	.084 (.038)**	.332 (.116)***	-.011 (.048)	.387 (.183)**
German (D)	.111 (.046)**	.141 (.155)	.260 (.059)***	.082 (.204)
Imm. available for job (D)	.632 (.036)***	.808 (.094)***	.542 (.042)***	.813 (.129)***
Was fired (D)	1.054 (.040)***	1.372 (.141)***	.743 (.044)***	1.230 (.178)***
Quit job volunt. (D)	.913 (.053)***	.999 (.148)***	.629 (.058)***	1.007 (.184)***
Quit job temp. (D)	.383 (.086)***	.786 (.240)***	.229 (.083)***	.466 (.318)
Town size	-.003 (.004)	-.006 (.012)	.004 (.005)	-.001 (.016)
Household head (D)	-.186 (.049)***	.073 (.123)	-.040 (.062)	.137 (.200)
Job re-training in the past (D)	-.153 (.075)**	-.277 (.154)*	-.108 (.095)	-.520 (.203)**
Duration of job re-training (cat.)	.039 (.020)**	.078 (.035)**	.029 (.027)	.103 (.047)**
Only search full-time (D)	.051 (.033)	.115 (.089)	.032 (.038)	-.096 (.116)
Only search part-time (D)	.023 (.070)	-.129 (.221)	.048 (.051)	-.243 (.215)
Profession: manuf. (D)	-.032 (.039)	-.605 (.322)*	-.055 (.053)	.035 (.478)
Profession: engineering (D)	-.029 (.098)	-.043 (.239)	-.080 (.134)	-.063 (.296)
Profession: services (D)	-.097 (.037)***	.203 (.097)**	-.090 (.040)**	.195 (.137)
Obs.	6194	711	5057	448

Intensity of job search measured as time of job search net of time since unemployed; higher values indicate longer job search.

Coefficients are taken from an ordered probit model on the 1991 and 1993 cross sections of the German Microcensus.(1) Men and Women 18–40 without a university degree. (2) Men and women 18–40 with a university degree. (3) Women 18–41 and women 41–60 without a university degree. (4) Women 18–40 and women 41–60 with a university degree.

Table 7: Regression results – Intensity of Job Search 1991 and 1993

	(1)	(2)	(3)	(4)
Dep. var.: Intensity of job search (cat.)				
Year: 1993 * gender (D)	.025	.465		
	(.077)	(.223)**		
Gender (D)	.031	-.136		
	(.062)	(.162)		
Year 1993 * age $\leq$ 40 (D)			.090	.396
			(.080)	(.264)
Age $\leq$ 40 (D)			-.065	-.378
			(.103)	(.322)
Year: 1993 (D)	.195	-.314	-.115	-.237
	(.100)*	(.203)	(.127)	(.261)
Age	-.068	.298	-.017	.211
	(.034)**	(.178)*	(.014)	(.060)***
Age sq.	.001	-.004	.0003	-.002
	(.0006)**	(.003)	(.0002)*	(.0007)***
Married (D)	-.108	.129	-.126	.091
	(.048)**	(.178)	(.057)**	(.186)
German (D)	-.031	.154	.003	.048
	(.056)	(.178)	(.068)	(.195)
Imm. available for job (D)	.069	.005	.069	.204
	(.053)	(.159)	(.052)	(.175)
Was fired (D)	.433	.790	.293	.420
	(.045)***	(.149)***	(.043)***	(.145)***
Quit job volunt. (D)	.353	.356	.318	.280
	(.059)***	(.192)*	(.061)***	(.241)
Quit job temp. (D)	-.385	-.254	-.340	-.786
	(.082)***	(.287)	(.088)***	(.278)***
Town size	-.031	.101	.016	-.003
	(.016)**	(.052)*	(.016)	(.060)
Household head (D)	-.031	.025	-.094	.152
	(.064)	(.211)	(.072)	(.235)
Job re-training in the past (D)	-.429	.095	-.278	.117
	(.058)***	(.130)	(.073)***	(.144)
Duration of job re-training (cat.)	-.029	.045	.011	.010
	(.013)**	(.026)*	(.016)	(.031)
Only search full-time (D)	-.072	-.167	-.071	-.264
	(.046)	(.134)	(.046)	(.164)
Only search part-time (D)	-.463	-.408	-.170	-.515
	(.082)***	(.315)	(.056)***	(.224)**
Profession: manuf. (D)	.129	-.483	.242	-.269
	(.058)**	(.244)**	(.065)***	(.424)
Profession: engineering (D)	.193	-.123	.328	-.158
	(.130)	(.228)	(.159)**	(.248)
Profession: services (D)	.133	.117	.240	.061
	(.058)**	(.148)	(.060)***	(.165)
Obs.	3687	375	3905	324

Intensity of job search measured as time of job search net of time since unemployed; higher values indicate longer job search.

Coefficients are taken from an ordered probit model on the 1991 and 1993 cross sections of the German Microcensus.(1) Men and Women 18–40 without a university degree. (2) Men and women 18–40 with a university degree. (3) Women 18–41 and women 41–60 without a university degree. (4) Women 18–40 and women 41–60 with a university degree.

Table 8: Regression Results – Wages – OLS

	OLF	OLF	Aggr. (OLF)	Full/part time	Full/part time	Aggr. (full/part time)	Group/ year cells
	(1)	(2)	(3)	(4)	(5)	(6)	
Year: 1988 (D)	-.166	-.209	-.196	-.002	-.020	-.035	16
	(.099)*	(.112)*	(.154)	(.025)	(.026)	(.133)	
Obs.	623	623	16	7407	7407	16	
Year: 1989–1990 (D)	-.038	-.030	-.054	-.00007	-.008	.026	24
	(.081)	(.092)	(.117)	(.016)	(.017)	(.102)	
Obs.	1071	1071	24	11423	11423	24	
Year: 1992 (D)	-.024	-.121	.012	.008	-.086	.018	32
	(.080)	(.093)	(.152)	(.025)	(.026)***	(.166)	
Obs.	1666	1666	17232	17232	32		
Year: 1993 (D)	-.009	-.114	-.019	.027	-.004	.031	36
	(.067)	(.079)	(.151)	(.021)	(.023)	(.170)	
Obs.	2026	2026	36	20001	20001	36	
Year: 1992–1993 (D)	-.017	-.125	-.003	.016	-.050	.026	36
	(.058)	(.067)*	(.113)	(.018)	(.018)**	(.125)	
Obs.	2026	2026	36	20001	20001	36	
Additional cluster correction	NO	NO	YES	NO	NO	YES	

*: significant at the 10% level, **: significant at the 5% level, ***: significant at the 1% level. Significance levels are derived using the t distribution and a two-sided test. A table including confidence intervals can be obtained from the author upon request. Treatment periods are chosen according to the laws coming into effect. Treatment years are compared to years *before* the treatment. Additional control variables as shown in table 3.

(1): Sample: out of the labour force in $t-1$. Huber/Eicker/White standard errors. Reference category: men aged 18–40, women aged 18–40 with children, women aged 41–60.

(2): Sample: out of the labour force in $t-1$. Huber/Eicker/White standard errors. Reference category: men aged 18–40.

(3): Aggregation of mean predicted outcome to group/year cells after controlling for other wage determinants. Differencing between treatment and control units before and after treatment is achieved by regressing the mean predicted outcome on group, year and interaction dummies. Only persons out of the labour force in $t-1$.

(4): Sample: working in part time or full time job in $t-1$. Huber/Eicker/White standard errors. Reference category: men aged 18–40, women aged 18–40 with children, women aged 41–60.

(5): Sample: working in part time or full time job in $t-1$. Huber/Eicker/White standard errors. Reference category: men aged 18–40.

(6): Aggregation of mean predicted outcome to group/year cells after controlling for other wage determinants. Differencing between treatment and control units before and after treatment is achieved by regressing the mean predicted outcome on group, year and interaction dummies. Only persons working in a full or part time job in $t-1$.

Table 9: Regression Results – Wages – Selection Model

	(1)	(2)	(3)	(4)	(5)
Year: 1988 (D)	-.153	.024	.014	.016	.002
	(.102)	(.027)	(.032)	(.032)	(.130)
Obs.	514	16379	4983	4819	164
Censored obs.	–	9120	2856	2816	40
Years in sample	1984-1988	1984-1988	1984-1988	1984-1988	1984-1988
Year: 1989–1990 (D)	.135	-.016	-.018	-.037	.135
	(.112)	(.025)	(.025)	(.026)	(.085)
Obs.	446	15732	5549	5317	232
Censored obs.	–	8162	2792	2746	46
Years in sample	1988-1990	1988-1990	1988-1990	1988-1990	1988-1990
Year: 1992 (D)	-.112	.046	.104	.096	.188
	(.125)	(.028)*	(.027)***	(.027)***	(.111)*
Obs.	386	17123	6105	5840	256
Censored obs.	–	8122	2889	2839	50
Years in sample	1990-1992	1990-1992	1990-1992	1990-1992	1990-1992
Year: 1993 (D)	-.088	.034	.023	.049	-.097
	(.127)	(.031)	(.059)	(.060)	(.136)
Obs.	309	14205	5179	4935	244
Censored obs.	–	6609	2410	2358	52
Years in sample	1992-1993	1992-1993	1992-1993	1992-1993	1992-1993
Year: 1992–1993 (D)	.041	.052	.109	.108	.137
	(.018)	(.021)**	(.027)***	(.030)***	(.095)
Obs.	1071	24198	8678	8290	388
Censored obs.	–	11491	4145	4065	80
Years in sample	1990-1993	1990-1993	1990-1993	1990-1993	1990-1993
OLF sample	YES	NO	NO	NO	NO
Reference: men only	YES	NO	YES	YES	YES
Lower education sample	NO	NO	NO	YES	NO
Higher education sample	NO	NO	NO	NO	YES

*: significant at the 10% level, **: significant at the 5% level, ***: significant at the 1% level. Significance levels are derived using the t distribution and a two-sided test. Mills ratio significant at the 10% level in all regressions. Treatment periods are chosen according to the laws coming into effect. Treatment years are compared to years *before* the treatment. Additional control variables for first stage as in table 2 and for second stage as in table 3. All samples restricted to individuals not (yet) having a child in t or $t+1$.

(1): No selection model since conditioned sample.

(2): Exclusion restriction for first stage: lagged age (D), lagged married (D), lagged experience, lagged education, lagged sense of being worried about Germany's economic situtation(D) , having a child in the future (D).

(3): Exclusion restriction: lagged age (D), lagged married (D), lagged experience, lagged education, lagged sense of being worried about Germany's economic situtation(D).

(4): Exclusion restrictions for first stage: lagged age (D), lagged married (D), lagged experience, lagged education, lagged sense of being worried about Germany's economic situtation(D).

(5): Exclusion restriction for first stage: lagged age (D), lagged married (D), lagged experience, lagged education, lagged sense of being worried about Germany's economic situtation(D).

Table 10: PLACEBO TREATMENTS – WAGES

	Pooled	Pooled	Aggr.	Aggr.	Selection	Selection	Group/ year cells
	(1)	(2)	(3)	(4)	(5)	(6)	
Year: 1994–1995 (D)	.006	.065	.141	.122	-.009	-.019	44
	(.085)	(.061)	(.113)	(.087)	(.017)	(.018)	
Obs.	2697	2697	44	44	49237	49237	
Censored obs.	–	–	–	–	25178	25178	
Year: 1996–1997 (D)	.074	.041	.093	.094	-.016	.003	52
	(.052)	(.058)	(.126)	(.118)	(.015)	(.016)	
Obs.	3338	3338	52	52	61248	61248	
Censored obs.	–	–	–	–	31050	31050	
Year: 1998–1999 (D)	-.081	-.037	.045	.053	-.027	.005	60
	(.070)	(.083)	(.135)	(.134)	(.014)*	(.015)	
Obs.	3996	3996	60	60	73103	73103	
Censored obs.	–	–	–	–	36797	36797	
Add. standard error correction	NO	NO	YES	YES	NO	NO	
Sample: OLF	YES	YES	YES	YES	NO	NO	
Reference: men only	NO	YES	NO	YES	NO	YES	
OLS	YES	YES	YES	YES	NO	NO	
Distribution for test statistic (last stage)	$t(41)$	$t(34)$	*t(group/ year cells - 4)*	*t(group/ year cells - 2)*	$\chi^2(38)$	$\chi^2(38)$	

*: significant at the 10% level, **: significant at the 5% level, ***: significant at the 1% level. Significance levels are derived using the t distribution and a two-sided test. Treatment periods are chosen according to the year in which the respective law came into effect. Treatment years are compared to years *before* the treatment without policy changes. Additional control variables as shown in table 3.
(1): Only years 1986–1994, because information on previous job not collected thereafter. Only persons out of the labour force in $t-1$.
(2): Huber/Eicker/White standard errors. Only persons out of the labour force in $t-1$.
(3): Estimation of the employment probability for the pooled sample with OLS, accounting for individual heterogeneity. Differencing between treatment and control units before and after treatment is achieved by regressing the mean predicted outcome on group, year and interaction dummies. Only persons out of the labour force in $t-1$.
(5): Heckman type selection model. Selection equation on being employed in a full or part time job. Exclusion restrictions: lagged age (D), lagged married (D), lagged experience, lagged education, lagged sense of being worried about Germany's economic situtation(D), having a child in the future (D). (6): Heckman type selection model. Selection equation on being employed in a full or part time job. Exclusion restrictions: lagged age (D), lagged married (D), lagged experience, lagged education, lagged sense of being worried about Germany's economic situtation(D).

Supplementary Appendix

B.1 Background: maternity leave legislation in Germany

In the 1980s and 1990s a woman in Germany received maternity *pay* for the first two months after delivery, when she was not allowed to work. In addition, she could receive maternity *pay* for the 6 weeks preceding delivery if she decided to quit work already before delivery. Until 2006, she then received maternity *benefits* (about DM 600 a month) from the government up to the maximum duration of the maternity benefits payment period. She enjoyed dismissal protection for the dismissal protection period.

Although all mothers were entitled to the whole job protection and basic benefit *payment* period, not all mothers were granted the actual payment of maternity benefits. The amount of payment was conditioned on the taxable household income of the preceding year. As long as this income was below a threshold of DM 20,000, a woman would receive the full benefit of DM 600. If taxable household income of the previous year would be between DM 20,000 and DM 41,400 the benefit would be reduced. If taxable household income exceeded DM 41400 in the previous year, the mother would not be entitled to maternity benefits.

In contrast to the changes in the 80s and 90s, the latest changes to the law were mainly targeted at working mothers. In 2000, another reform gave a working parent the right to continue their job part-time instead of full-time after the birth of a child if they desired to do so. This explicitly included men, hardly any of whom took parental leave before although they would have been entitled to do so. A reform in 2007 then redesigned the benefits structure. A replacement income close to the last net salary is now granted for a full year (*Elterngeld*). Paternity benefits are granted for two additional months, if the other parent agrees to stay home with the child for these 2 months. This was meant to encourage leave-taking by men.

B.2 Serial Correlation in the SOEP Data

When looking at specific variables such as employment rates or wages, serial correlation is very likely to affect the time series. This has to be carefully considered, especially when the coefficient is positive, because this may exert a downward bias on standard errors. In order to get an idea how serial correlation might look like in the SOEP data, I proceed in two steps.

First, analogous to Bertrand et al. (2004), I look at a correlogram of residuals for both employment rates and log wages. Residuals are obtained by regressing the dependent variable on group and year dummies for the relevant sample. Coefficients are presented in table B.1. The coefficient for first order serial correlation is positive and relatively high for employment, employment rates,[20] and log wages. The analysis indicates highly significant serial correlation for employment rates and wages. For wages, the magnitude of first order serial correlation, a coefficient of 0.61, diminishes quite quickly to 0.16 for second order serial correlation and to 0.03 and 0.02 for third and fourth order serial correlation. This may in fact be indicative for an AR(1) process. With respect to employment and employment rates, a pure AR(1) process seems unlikely.

Table B.1: Serial Correlation

	Employment	Empl. Rates	(Log) Wages
	(1)	(2)	(3)
Residuals			
Residuals, lag 1	.058	.377	.608
	(.040)	(.006)***	(.007)***
Residuals, lag 2	.142	-.041	.169
	(.008)***	(.006)***	(.007)***
Residuals, lag 3	.078	.006	.037
	(.008)***	(.005)	(.007)***
Residuals, lag 4	.060	-.042	.021
	(.007)***	(.003)***	(.005)***
Obs.	20817	20817	19367

Serial correlation coefficients from a simple OLS regression of the OLS residuals on their lags. OLS residuals are derived from an OLS regression of the dependent variable on group and year dummies. The sample is previously unemployed persons of age 18 –40, except for column 3. The sample for the regression in column 3 is persons of age 18 –40, who earn a salary.

Second, assuming an AR(1) process, I run a feasible GLS model of employment for the previously not employed aged 18 –40 accounting for serial correlation. The main aim is to get an idea of the potential impact of serial correlation. Note that this focuses on the potential presence of serial correlation on the individual level only and not on correcting a potential correlation of group errors. The Barghava-Franzini-Narendranthan (1982) Durbin-Watson statistic for testing the hypothesis of zero serial correlation is 1.28, while the Baltagi-Wu (1999) LBI statistic is 2.19.[21] The evidence against zero serial correlation is thus slightly ambiguous, in line with the first impression from the correlogram of OLS group residuals. Both test statistics reject the null of zero serial correlation in the case of log wages.[22]

[20] The employment rate is defined as the rate of employed persons in the age group 18 – 40 over the non-employed persons in this age group. Average employment rates are computed for state/year cells.

[21] The results from this model can be obtained from the author upon request.

[22] The Barghava-Franzini-Narendranthan (1982) Durbin-Watson statistic is 1.17 and the Baltagi-Wu (1999) LBI statistic is 1.72.