The wage penalties of heterogeneous employment biographies: An empirical analysis for Germany

Miriam BEBLO and Elke Wolf

beblo@zew.de, wolf@zew.de

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Abstract

This paper examines the wage effects of different types of career interruptions. We consider the timing and duration of non-employment spells by exploiting a data set of German social security accounts (IAB employment sample) supplemented with administrative data on the employees' entire working lives (IAB supplement sample I). These data allow us to distinguish between employment breaks due to registered unemployment, parental leave, training or other reasons. We can show that past employment breaks reduce the wage rate of both men and women. Due to the lower returns to "outdated" human capital the wage loss caused by a career interruption several years ago proves less harmful than that of a more recent break. Our IV fixed effects estimation results suggest that men's wages are more affected by past unemployment, while women's wage incomes are negatively related to out-of-the-labor-force periods.

JEL classifications: J22, J24, J31

Keywords: career interruptions, returns to experience, wage differentials, panel estimation.

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1 Introduction

Human capital accumulated on the job is considered one of the main determinants of an individual's wage rate. Empirical researchers usually measure job-related human capital as aggregated labor market experience, that is the amount of time spent in (self-)employment. However, an increasing number of empirical studies suggests that the pure sum of years employed does not unveil the whole picture of what determines an individual's work experience, because discontinuities in the employment biography are not explicitly considered. Two persons with the same total number of years worked may still differ in the number and timing of their work spells and career interruptions. These career interruptions – be they due to childbearing or rearing, further training, unemployment, illness/disability, or any other out-of-the-labor-force periods – are likely to generate wage effects, presumably mainly negative ones. As a result, discontinuities in the employment pattern not only imply interruptions in the current accumulation of human capital, but may cause severe wage cuts in the sequel, aggregate experience and other characteristics being equal. Due to the neglect of these peculiarities in an individual's employment biography, empirical evidence points to the fact that conventional specifications underestimate the return to experience, in particular during the first years of an individual's career (see e.g. Light and Ureta 1995, Murphy and Welch 1990).

One reason why the interruption of employment may not be wage-neutral refers to technical and organizational progress and innovations in the work process. Human capital acquired in previous years of employment may become obsolete after an interruption, if this knowledge is not maintained and updated during absence. In economic literature, the decay of human capital has been neglected for a long time. The effect of employment breaks on earnings was first investigated by Mincer und Polachek (1974) for women in the U.S.. From simple OLS regressions, they conclude that wage cuts due to periods out of work can be attributed to an interruption in the accumulation of human capital as well as a depreciation or atrophy of the human capital stock built up in the past. However, these results are at risk of being biased, because unobserved worker characteristics are expected to be correlated with both intermittent labor force participation and wages.

A number of studies tackle this problem by estimating fixed-effects models using panel data (Mincer and Polachek 1987, Mincer and Ofek 1982, Sundt 1987 as well as Licht and Steiner 1991). More recent analyses on the impact of intermittencies are provided by Kim and Polachek (1994), Light and Ureta (1995), Ferber and Waldfogel (1998) as well as Ureta and Welch (2001) using US data, Gupta and Smith (2000) for Denmark and Albrecht et al. (1998) for Sweden. The impact of employment breaks on the income profile of German women has been investigated among others by Galler (1991), Gerlach (1987), Licht and Steiner (1992), Kunze (2001) and by ourselves (Beblo and Wolf 2000). Whereas Gupta and Smith simply use the presence and number of children as a proxy for employment interruptions, Kim and Polachek introduce the number of years spent not working as a home-time variable in the wage equation. After controlling for heterogeneity and endogeneity associated with home time affected by a low wage rate, they detect skill atrophy. Licht and Steiner find a catch-up effect of wages following a break that partly offsets depreciation of human capital. Galler, who also considers the sequence of full-time spells, part-time periods and interruptions in the wage determination, observes this wage catch up only for formerly part-time employees who take up a full-time position again. Full-time experience gathered prior to a break has a weaker impact on the wage rate than that following a discontinuity.

Gerlach (1987) found that wages of German women fall with the length of time spent on the job prior to the first leave, using work experience and employment breaks at different points in life as explanatory variables. That is, the wage penalty is most pronounced for later breaks when a higher amount of human capital has already been accumulated, which is then at risk to depreciate. The impact of the exact timing of interruptions in Germany has been investigated explicitly in our preceding study (Beblo and Wolf 2000), where we estimate the depreciation rate on work experience due to non-work and part-time spells in an extension of the Mincer wage equation. The basic shortcoming of this approach, however, is that the limited number of observations compelled us to impose strong restrictions on the functional form of the human capital depreciation process. Nevertheless, the implications of our specification are consistent with the empirical results of Gerlach (1987).

Light and Ureta (1995) account for intermittency in the work history in the most flexible way. In their wage equation, they include a set of variables that measure the fraction of time worked during each year of a career. They also draw special attention to the timing of interruptions. A major drawback of this very flexible specification, however, is the large number of parameters to be estimated, requiring a large number of observations. A second shortcoming is that they do not distinguish between the impacts of different types of career interruptions. But there are good reasons to believe that the specific cause of the break influences the size of the wage penalty. Since participation in vocational training programs is generally much lower among unemployed or young mothers, skill obsolescence may be particularly relevant for these groups. Another reason are potential stigma or signaling effects. Several studies point to the fact that past unemployment spells evoke negative expectations on the side of the employer regarding the productivity of the potential employee. As a consequence, unemployed may be offered lower wages, everything else being equal.

To date, hardly anything is known about the long-run wage effects of different types of employment breaks. Does, for example, a one-year maternity leave cause the same wage cut as an unemployment spell or a sabbatical? And what kind of training yields higher returns, full-time education in school or learning-by-doing on the job? Taking up these open questions, Kunze (2001) distinguishes between three types of time out of work, namely unemployment, interruption and other non-work, using German data. By comparing OLS regression estimates and coefficients resulting from fixed effects estimations she concludes that female returns to experiences are underestimated if unobserved heterogeneity is not taken into account. For men, however, fixed effects estimates hardly differ from pooled OLS results. Also Albrecht et al. (1998) distinguish various types of career interruptions and conclude that the negative wage effect cannot only be attributed to the depreciation of human capital but also involves the signaling of career orientation. They find different impacts of formal parental leave and additional home care on subsequent labor income.

The aim of our paper is to shed more light on the wage effects of different types of employment breaks of German men and women. Following Light and Ureta's work history specification, we consider the impact of each single year of an individual's career. The work history model is advantageous to conventional specifications of the wage equation by measuring experience more precisely and by imposing less constraints on the shape of the wage-experience profile. We go beyond Light and Ureta in two different respects: As Kunze, we distinguish between various types of non-employment spells. But other than Kunze, we do not only take into account the timing but also the duration of each interruption. Moreover, we consider the endogeneity of individual employment decisions throughout the life cycle by applying the instrumental variable approach following Hausman and Taylor (1981) within a fixed-effects panel estimation to quantify the wage penalties of different career interruptions. We are able to distinguish time out of work by exploiting extensive and detailed information of individuals' biographies since the beginning of their careers. This information stems from a data set of German social security accounts (IAB employment sample) supplemented with additional administrative data on the individuals' entire working lives.

The paper proceeds as follows: After a description of the data and our sample chosen we illustrate the relevance of different types of career interruptions for women and men. We then present some theoretical considerations regarding the diverse impacts of different types of employment breaks. Panel estimations of female and male wages provide the empirical answers. The last section concludes.

2 Data set and description

To recognize the significance of our contribution, a description of the extensive data set used for our analysis is needed. The individual information underlying our research is based on the IAB employment sample and additional administrative data assembled at the state pension authorities. The IAB employment sample is a 1 % random sample of German social security accounts that has been made available for empirical researchers through the Institute for Employment Research (Institut für Arbeitsmarkt- und Berufsforschung IAB) in Nürnberg (see also Bender et al. 2000). These data cover the period between 1975 and 1995, that is, every person who was employed for at least one day from 1975 to 1995 and/or with claims to pension benefits is included in the parent population.[1] During this time, social security contributions were mandatory for all employees who earned more than a lower earnings limit. Civil servants, self employed and people with so-called marginal jobs, that is, jobs with less than 15 hours per week or temporary jobs which last 6 week at most are not covered by this sample.

Altogether, the employment sample represents about 80 percent of all West German employees and provides very precise information about each individual's average daily wage rate. If the wage rate exceeds the upper earnings limit (*"Beitragsbemessungsgrenze")*, the daily social security threshold is reported instead.[2] Note that the daily wage rate is therefore censored from above and truncated from below. The available wage information refers to employment spells that employers report to the Federal Employment Service. In this study, we use the wage information of those employment spells that include June 30th as the relevant day of each year between 1990 and 1995, that is, we have an unbalanced panel including six waves of subsequent years.

[1] These are people who, as employees, have paid contributions to the pension system or who have been covered by the pension system through contributions by the unemployment insurance or by being a parent (depending on the birth year of the child, a fixed number of years is counted as child caring time during which the non-working parent becomes entitled to receive pension benefits).

[2] Fitzenberger and Wunderlich (2000) show that this affects particularly the wage rate of high-skilled employees. According to their results, about 50 percent of high-skilled men earn wages above the upper earnings limit. Among high-skilled full-time females, this share amounts to at least 20 percent.

In order to have more details about all non-employment spells, we supplemented the employment sample with administrative data from the same data generating process, the IAB employment supplement sample I. This supplement sample provides information about the individuals' entire working lives and allows us to distinguish between different types of "non-active" periods, namely, unemployment, formal maternity leave, illness, disability, care for other persons, full-time education, military or civil service and simple out-of-the-labor-force spells. To our knowledge, there is no other large-sample longitudinal data set providing as much and as detailed information on individuals' employment histories in Germany. Moreover, as it concerns administrative data, we do not have to rely on self-reported or retrospective wages and biography information. Due to the large number of observations and due to the most accurate and precise information about individual biographies and wage rates, these panel data are very suitable for a comprehensive and exceptionally detailed analysis of heterogeneous employment patterns and wages.[3]

For the purpose of our analysis, we defined four types of non-employment spells. The first category, maternity or parental leave, accounts for employment breaks due to the birth of a child. These spells include the mandatory employment break of 14 weeks prescribed by the German law that is aimed at protecting working pregnant women and mothers of new-born babies. Non-employment spells due to unemployment are summarized in the second group. A third category captures time spent in school or vocational training. It also includes military service or, alternatively, civil service. The remaining activities other than being employed, such as illness (only if it exceeds 6 weeks), disability or care for other family members including children, are generally not associated with the accumulation of job-related human capital. We define them as out-of-the-labor-force periods or non-employment.

To make sure that the observed non-employment periods truly represent employment breaks, we have to identify the starting date of each individual's career. Therefore, we define the beginning of a career as soon as the individual has been reported in employment (and covered by social security) for at least five months during the previous year.[4] Non-employment spells prior to the career are set equal to zero. Since our current sample provides information about the employment history of the past 23 years, we exclude individuals older than 40 years, so as to have observations for everybody since the very beginning of his or her career. We restrict the analysis to those who are full-time employed at the cross-section dates June 30 of the years 1990 to 1995. We also limit our analysis to West Germans, since large parts of the human capital stocks accumulated by East Germans before 1990 became obsolete due to the German Unification (Puhani and Steiner, 1997). As a consequence, employment breaks at the time of the socialist regime in East Germany are likely to be less relevant for the wage rate obtained in a market economy.[5]

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3 For a comparative analysis of the employment penalties for motherhood in West Germany after 1945 see Kohlmann, Bender and Lang (2002).

4 In contrast to Light and Ureta (1995), we choose a less restrictive definition of the starting date of the career, because the employment spells reported in the IAB employment sample are related to social security contributions, which are in general not mandatory for students in temporary or second jobs.

5 Moreover, our observation period just covers the beginning of the fundamental transition process in East Germany when wages were strongly affected by institutional regulations, making the interpretation of wage differentials even more difficult.

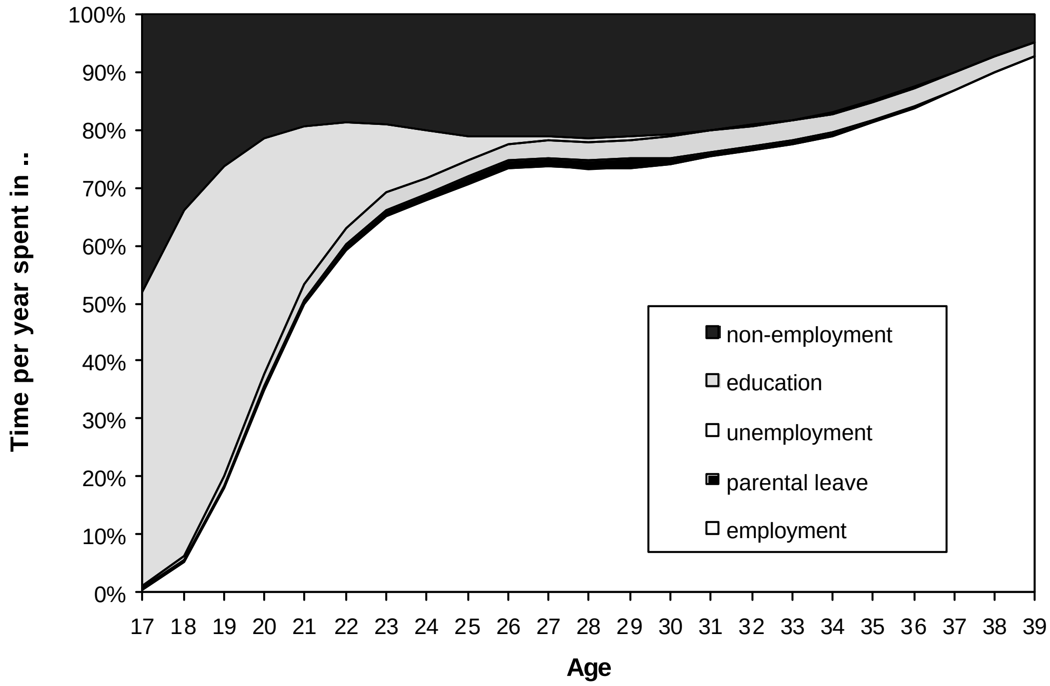
In addition to the process-dependent censoring and truncation of the data we chose to trim daily wages at the highest and lowest percentile. The resulting average gross daily wage rate of men is 80.8 euro and 62.5 euro for women. Since part-time employees are excluded from the sample (as well as homeworkers and apprenticees), this difference cannot be attributed to differences in the number of working hours. Apart from differences in qualification and sectoral segregation, this wage gap may be partly due to differences in human capital accumulated on the job. In our sample of individuals aged between 19 and 40 years, men have worked 16.1 years on average, whereas women spent 14.8 years in gainful employment. So as to provide more insight into the work histories of men and women, we refer to the graphical illustration in the next section.

3 Unemployment, parental leave and other non-employment spells over the life-cycle

Figure 1 and 2 display the average activity patterns of women and men between the ages of 17 and 39. For a given age they show the percentage time per year spent in employment, maternity/parental leave, unemployment, schooling or other non-employment activities. As these life cycle illustrations are based on samples of 17 to 40 year-old women and men who are employed in either of the years 1990 to 1995, they do not necessarily provide a representative picture of the biography of an average German man or woman. Note also, that the number of persons underlying the graphs obviously decreases with age, because part of the sample has not reached the respective age yet.

The most striking difference in the patterns of women and men is that, while gainful employment increases steadily over the life cycle for both sexes, only women have a more or less constant employment share between age 26 and 30. Female annual labor force participation starts off at zero percent at the age of 17 and rises to above 90 percent at the age of 39, conditional on being employed at age 40. The proportion of women in schooling, on the contrary, diminishes rapidly until age 26 and can be neglected thereafter. Female unemployment plays an augmenting role over the age range, although with about 3.5 percent it is highest at age 30 and 31. There may be two explanations for this finding. First, job changes that are accompanied by search unemployment are likely to occur more frequently around this age and, second, these unemployment spells may be associated with or following a maternity leave.

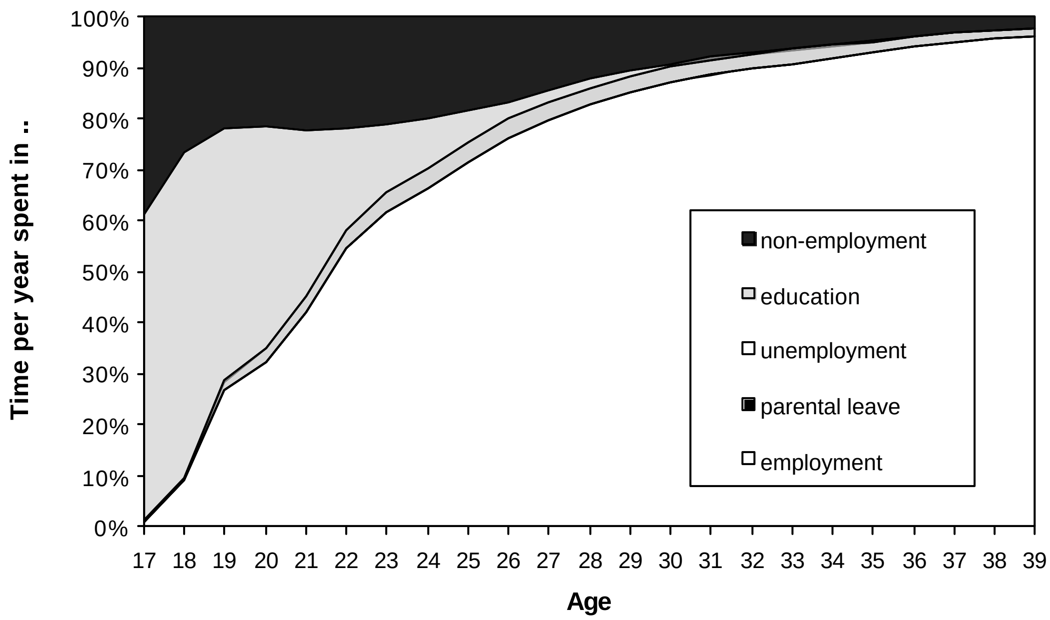
Figure 1: Employment and non-employment states over the early life cycle of women



Note: For a given age the share on the ordinate represents the percentage of time per year spent in employment, maternity/parental leave, unemployment, schooling or other non-employment activities.

Source: Matched IAB employment sample and supplement sample I, cross sections 1990-1995, own calculations.

Figure 2: Employment and non-employment states over the early life cycle of men



Note: see Figure 1.

Source: Matched IAB employment sample and supplement sample I, cross sections 1990-1995, own calculations.

It is well illustrated that formal maternity leave spells occur mainly prior to or during the plateau phase of gainful employment between the beginning of a woman’s 20s to about age 30. Their small magnitude is due to the parental leave legislation up to 1979 that allowed a maternity protection period of only 14 weeks (6 weeks before and 8 weeks after childbirth), which translates into a maternity share per year of about 27 percent. Additional parental leave of 4 months was not introduced before 1979. It was extended to 10 months in 1986 and gradually during the following years. Since 1992 parents of a newborn child had the right to interrupt work for a period of three years while they are guaranteed the right to return to their previous employer in a status adequate job. As the majority of our sample experienced their parenthood before this 1992 extension of the maximum parental leave period already, we only observe proportions above 1 percent between age 23 and 30 and a maximum proportion of 1.5 percent at age 28. That is, the average woman of this age (including mothers and non-mothers) has spent less than a week in maternity leave. If we focus our attention on mothers only, the fraction amounts to approximately 38 percent of a year (= 20 weeks) in maternity leave at age 28. After considering employment, unemployment, schooling and parental leave, the remaining of each year is attributed to activities out of the labor force. As mentioned above, these include all none human capital accumulating states such as illness, disability or care for other family members including children. Between age 20 and 32 non-employment takes up about 20 percent of a hypothetical average woman’s year, the importance of this status diminishes in the sequel as the graphs are conditioned on individuals being employed at age 40.

Also for men, employment increases monotonically with age and levels out above 90 percent from age 33 onwards. Schooling and non-employment spells both decrease with age, however, schooling at a much faster rate. Contrary to the female picture, we observe a kink at age 19 caused by compulsory military or civil service. Whereas unemployment periods seem to be relevant for men at all ages as well with a maximum exposure of 4 percent in the middle of the 20s, we do not observe any family–related pattern in the men’s figure. Parental leave, though applying also to fathers since 1992, is of such minor importance that we cannot even distinguish the black area in Figure 2.

4 A priori considerations with regard to the different wage effects of unemployment, parental leave and other non-employment spells

The various non-employment activities described above are likely to influence the sign and the size of the resulting wage penalty. As re-schooling or participation in training programs during the career are supposed to increase the individual human capital stock, we would expect positive wage effects of these periods. It is however unclear, to which extent full-time education in school (ignoring the ongoing income losses) pays off later on in life compared to learning-by-doing on the job.

During unemployment or family-related career interruptions, on the other hand, the existence of skill obsolescence may be particularly harming. Presumably, skill obsolescence is particularly relevant for employees whose work is concerned by rapid technological progress (e.g. jobs in the ICT sector). Furthermore, stigma or signaling effects may cause additional wage cuts for re-employed. Gibbons and Katz (1991) show for instance that unemployed who have been laid off are stigmatized and therefore offered lower wages because the prospective employer interprets the dismissal as a signal of their low productivity.[6] Other empirical studies also point to the fact that past unemployment spells evoke negative expectations on the side of the employer regarding the productivity of the potential employee (Berkovitch 1990). As a consequence, unemployed may be offered lower wages, everything else being equal. There is less evidence concerning the stigma effects of parental leave and other out-of-the-labor-force periods, though.

The fact that a woman or a man takes "time out" without being registered unemployed may be interpreted as a lack of career commitment by future employers. These voluntary non-employment spells then serve as a "bad" signal as well. As a consequence, the resulting wage cut may go beyond mere human capital depreciation. For Sweden, Albrecht et al. (1998) find that men face higher wage reductions than women subsequent to parental leave and household time from which they deduce that the signaling function of discontinuous employment profiles with respect to the individual's career orientation hits men in particular. Contrary to an intuitive understanding, there is less evidence for the existence of such gender-specific effects in Germany. In a recent survey, about 3000 mothers were asked whether their male partners called on (part of) the 3-year parental leave as well as for their assessment of the reasons if they had not (see Beckmann 2001). Among the reasons, the men's fear of being stigmatized by superiors and colleagues seemed to be of minor importance. We would therefore expect to find smaller effects of voluntary non-employment periods on male wages in Germany than in Sweden.

5 Estimation procedure and results

To quantify the long-run wage penalties associated with intermittent labor force participation and to identify possible stigma or signaling effects of unemployment, parental leave or out-of-the-labor-force periods, we have to perform wage estimations that consider all these activities performed over the life cycle. For this purpose, we follow the estimation procedure of Light and Ureta (1995) and use what they call a *work history model* to assess the returns to experience and the wage effects of employment discontinuities. In order to fully characterize past work experience, they construct an array of variables measuring the fraction of time worked during each year of the career. This set of experience variables is included in the wage equation, together with a set of dummy variables that are supposed to capture the wage effects of non-active spells. We extend their model by allowing for different types of non-employment. Furthermore, we exploit precise information about the duration of all employment or non-employment states. That is, we construct an array of share variables for each activity measuring the fraction of time spent in the specific activity during each year of the career. As a consequences, our wage model is able to capture both, individual differences in the timing of employment and specific non-employment periods as well as variations in the amount of human capital accumulated on the job and the duration of non-employment states.

When constructing these share variables we distinguish between being employed and four alternative non-employment states, that is, unemployment, parental leave, time spent in school or

[6] Gibbon and Katz compare the post-displacement wages of laid-off workers and those who have been displaced in a plant closing and find a higher average wage loss for the first group. As the market infers a lower productivity for laid offs whereas no such inference exists for closed-down employees their results can be interpreted as a stigma effect of lay offs.

vocational training and finally out-of-the-labor-force, by including a set of variables measuring the corresponding shares of time per year. Remember that these indicators only become relevant if non-employment spells occurred during the years after the start of a career. Any such activity prior to the first job is not considered as a career interruption.

We specify three different models. All three models use all available information about the past 20 years of an individual's career. Model 1 is a simple OLS version of the work history model using pooled data. This estimation may however suffer from the potential correlation between unobserved individual effects and some explanatory variables. To control for unobserved individual heterogeneity, Model 2 applies a fixed effects estimation procedure. In view of a possible endogeneity of the chosen individual employment pattern, Model 3 takes up the instrumental variable approach suggested by Hausman and Taylor (1981). In this last specification all activity shares are instrumented to account for the correlation of intermittent labor force participation and worker characteristics.

A general specification of the wage equation nesting the three models described above can be written as follows:

(1) $$\ln W_{it} = a + bY_i + g X_{it} + y\, S_{it} + m_i + e_{it}\,,$$

where the dependent variable $\ln W_{it}$ is the logarithm of the average gross daily wage rate deflated with the consumer-price-index of person i at time t. The vector Y_i denotes all time-invariant variables, such as schooling, occupation and industry sector.[7] X_{it} represent the explanatory variables that vary over the observation period for each individual. S_{it} is the set of variables describing the employment history of the past 20 years. Thereby, information from the past 10 years enters as annual variables whereas data from 11 to 20 years ago are aggregated into one variable.[8] The individual effects m_i capture wage differentials due to unobserved characteristics. Finally we have a transitory component e_{it}, which is assumed to be homoskedastic with mean zero.

While it seems plausible to assume that e_{it} is uncorrelated with the explanatory variables, this is not likely to hold for m_i. As a consequence, a random effects panel estimation yields inconsistent results.[9] In Model 2 and 3 we therefore apply a fixed effects estimator to determine the coefficients in equation (1). One drawback of this procedure is that it does not allow to determine the effects of time-invariant variables. Secondly, the within-group estimator is not fully efficient since it ignores

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[7] Obviously, these variables are rather time-invariant and should therefore be excluded from the simple fixed effects estimation.

[8] We also run estimations including single variables for all past 20 years. But since in Model 3 the single year coefficients do not significantly differ from year 11 onwards, we decided to aggregate them in our final specification to keep things as simple as possible.

[9] We applied an Hausman specification test to check the appropriateness of the random-effects estimator. As expected, the hypothesis that the differences in (the subset of) coefficients between the fixed effects and the random effects model are not systematic can be rejected.

variations across individuals in the sample. Thirdly, the endogeneity of the individual employment pattern is not taken into account adequately. The alternative approach suggested by Hausman and Taylor (1981) is based on the assumption that certain explanatory variables (X_{it} and/or S_{it}) or at least specific transformations of these variables are uncorrelated with the individual fixed effects m_i and can therefore serve as instruments for the endogenous variables (e.g. the work history variables). Hausman and Taylor show that deviations from the individual means of (all) time-varying variables are uncorrelated with m_i and produce unbiased estimates of g . The underlying idea is that, while the overall level of these variables is likely to be correlated with the unobserved individual effects m_i, the year-to-year variation is not. Furthermore they illustrate that the individual means of explanatory variables, that is variables in X_{it} which are uncorrelated with m_i provide valid instruments for those variables which are correlated with m_i. Applying this approach to the work history model allows us to estimate the returns to diverse employment patterns, taking into account unobserved heterogeneity and the endogeneity of past experience.

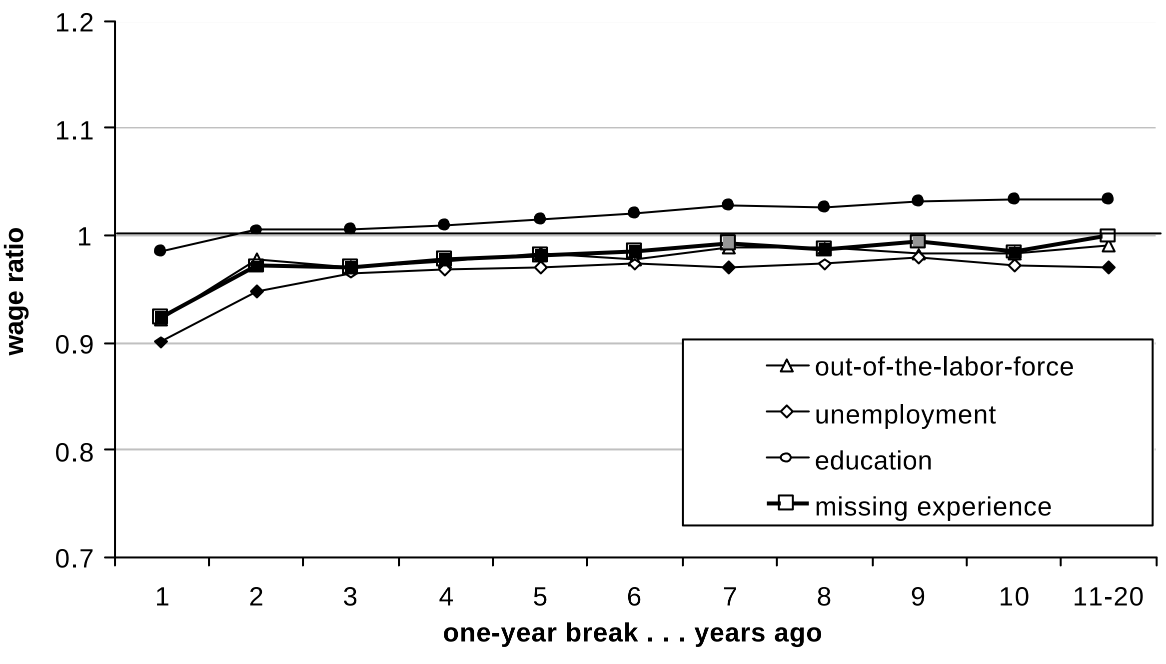
As pointed out by Hausman and Taylor, one needs to be quite careful in choosing explanatory variables that are supposed to be uncorrelated with the individual fixed effects. In our case, we assume that the year of birth, education level and the industry sector of each individual are time-invariant exogenous variables whereas the observation year and firm size are time-variant exogenous variables. Hence, we use both individual means and deviations from individual means (where applicable) as instruments for the endogenous work history variables, the activity shares, and the endogenous time-invariant variables. Since our main focus are the wage effects of discontinuous employment patterns, we confine ourselves to instrumenting the job status (white collar) as the only time-invariant variable in Model 3.

We run separate regressions for men and women. Due to the minor role of parental leave periods among men, we combine out-of-the-labor-force and parental leave into one set of share variables. The estimation results of Models 1 to 3 are displayed in Tables 1 to 6 in the Appendix, where Table 1 and 2 refer to the pooled OLS wage regression for men and women respectively, Tables 3 and 4 to the fixed effects panel regression and Tables 5 and 6 to the fixed effects specification with instrumented activity shares and job status. We will first draw our attention to the estimated returns to experience. The OLS estimates provide the highest coefficients for past employment experience. While men's wages increase by about 10 percent if they worked during the preceding year, female wages rise only about 8 percent. Over the whole work history, however, returns are more or less the same for women and men. Allowing for unobserved heterogeneity and endogeneity of the past employment pattern results in lower estimated returns to experience. Whereas for men the fixed effects equations yield similar coefficients, with or without instrumented activity shares, the regressions for women differ quite remarkably by specification. Taking into account endogenous intermittent labor force participation leads to much lower, mostly insignificant, returns to experience. Hence, the OLS regression is overestimating the true returns to experience due to selectivity effects. That is, workers with a high wage potential anticipate their higher returns and self-select into the group of those who are more attached to the labor market and have less career interruptions.

The most striking difference between the three models is that the size of the coefficient estimates for time spent working does not only decrease over time but also step by step (from Model 1 to Model 3). This is true for both men and women. Also the T-statistics vary remarkably between specifications. While past educational attainment does not have a statistically significant effect on the current wage rate according to the OLS estimates, the impact becomes stronger as soon as unobserved individual heterogeneity is captured through individual fixed effects. Taking into account the endogeneity of the individual employment pattern confirms the significant positive effect of education spells for men's earnings. For women, the wage effect becomes insignificant if training took place more than 3 years ago.

The wage effects of parental leave relative to out-of-the-labor-force periods also differ quite substantially across specifications. In the pooled regression parental leave spells are negatively correlated with a woman's wage rate but in Model 2 the influence vanishes while the wage penalty caused by out-of-the-labor-force periods becomes much more pronounced. This points to a selection effect in the way that mothers (who are identified by a parental leave spell) are over-represented among low-paid workers – be it because they sort themselves into those jobs ex ante, because they cope with these jobs after childbirth in exchange for more family-friendly working conditions and/or due to a stigma effect at work. This latter result still holds in Model 3. Being on formal parental leave does not impair a woman's wage growth, whereas out-of-the-labor-force spells significantly decrease her future wage rate. It is however noticeable that for men, no such effects can be observed in cases of "voluntary" non-employment while registered unemployment causes additional wage losses, at least if it occurred during the last 2 years (respectively 7 or more then 10 years ago).

Figure 4: Wage effects of a one-year break for men

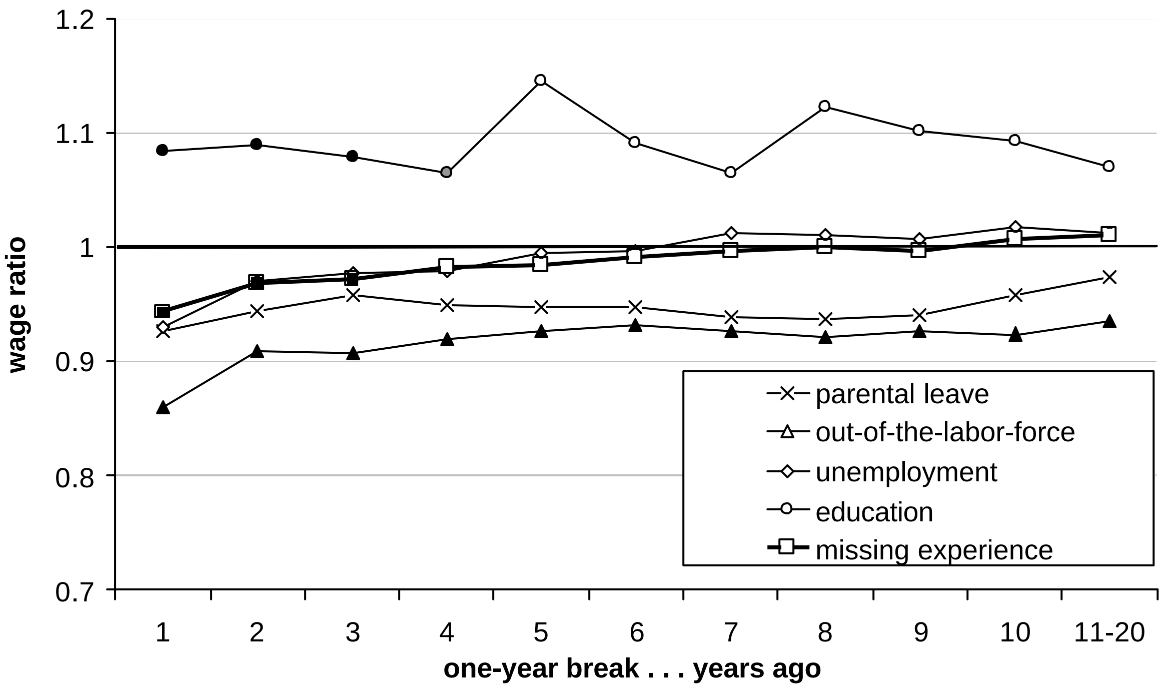


Note: The wage ratio is defined as the quotient of the predicted wage rate after a one-year break that happened a given number of years ago relative to the wage rate of a continuously employed man. Black (grey) symbols indicate a statistically significant effect at the 5-(10-)percent level.

Source: Matched IAB employment sample and supplement sample I, cross sections 1990-95, own calculations based on the estimation results of Model 3.

We now confine our analysis to the estimation results of Model 3 as it represents the most appropriate specification at hand. In Figure 4 it is illustrated once again that for men any experience of unemployment or out-of-the-labor-force during the past years yields a lower wage rate than if they have been gainfully employed. The bold line describes the hypothetical wage penalty caused only by missing experience. The vertical difference between this curve and the other lines demonstrates the additional wage effects of the other non-employment activities. The biggest wage cut stems from unemployment, particularly very recent unemployment spells. It is however noticeable that unemployed men hardly suffer an additional wage reduction, since the difference between the missing-experience curve and the unemployment curve seems rather neglectable. This suggests that signaling or stigma effects do not come into play here. Another interesting finding is that the wages of men with employment intermittencies catch up fairly quickly. If unemployment took place at least two years ago, the causal effect on the current wage rate is not significant anymore (compare white and black symbols or see Table 1 in the Appendix). In this case, the wage cut of about 3 percent can almost fully be attributed to foregone job-experience.

Figure 5: Wage effects of a one-year break for women



Note: The wage ratio is defined as the quotient of the predicted wage rate after a one-year break that happened a given number of years ago relative to the wage rate of a continuously employed woman. Black (grey) symbols indicate a statistically significant effect at the 5-(10-)percent level.

Source: Matched IAB employment sample and supplement sample I, cross sections 1990-95, own calculations based on the estimation results of Model 3.

A very intuitive result is that the wage ratio of men in education stays constantly above the other curves. Provided that the training was completed more than 1 years ago, schooling provides even higher returns than learning-by-doing on the job. But if participation in a training program took place very recently, the accompanying human capital effects could only partly compensate the wage reductions due to missing experience. Learning-by-doing on the job is still more beneficial in this case.

The financial consequences of employment discontinuities are much more striking for women (see Figure 5). While unemployment and formal maternity leave spells do not reduce their current earnings potential substantially, employment breaks caused by out-of-the-labor-force periods are followed by severe wage penalties. These penalties are much higher than the pure effect due to missing experience. Hence, contrary to unemployment and parental leave, there may be a signaling effect accompanied with this type of non-employment. This result is particularly striking as the endogeneity of intermittent labor force participation has been taken into account through instrumental variable estimation of the annual employment and non-employment shares. Moreover, the impact of out-of-the-labor-force periods does not seem to level out over time. Even women who stayed out of the labor market more than 10 years ago have to accept wage losses. One explanation may be that a large number of mothers does not return directly to the labor market after the end of formal maternity leave but moves to non-employment. As a consequence, the resulting wage loss only becomes apparent after these women enter the labor market again, which may not happen before the child has reached the school age.

Another result is that for women, contrary to the male finding, human capital accumulated in training programs pays off much more than learning-by-doing on the job. The education curve lies well above 1, that is to say that the wage rate of a woman enrolled in education compared to the wage of an employed woman at a given year is always above 100 percent no matter how long ago the schooling activity. But the attendance of a training course or further education does not necessarily result in a wage gain. If participation in a training program took place more than 4 years ago, the accompanying human capital effects could not compensate the wage reductions due to missing experience and learning-by-doing on the job would have been more beneficial.

6 Discussion

In this paper, we analyze the wage effects of different types of employment breaks of German men and women. Following Light and Ureta's work history specification we consider the impact of each single year of an individual's career on the wage rate. The work history model is advantageous to conventional specifications of the wage equation, because it allows to measure the impact of experience more precisely and imposes less constraints on the shape of the wage-experience profile. We go beyond Light and Ureta in two different respects: First, we distinguish between different types of non-employment spells. Second, we do not only take into account the timing but also the duration of each interruption. We perform fixed-effects panel estimations with instrumented labor force intermittencies to quantify the wage penalties of different career interruptions.

Our estimation results suggest that the wage penalties of discontinuous employment biographies are very different, in sign and in size, for women and men. Whereas men's wages seem to be rather affected by recent unemployment experience, out-of-the-labor-force spells lead to substantial wage cuts for women even if they occurred several years ago. Training, on the contrary, generates positive wage effects especially for men. Allowing for unobserved individual heterogeneity and endogeneity of the work history results in lower estimated returns to experience, particularly for women.

At this stage our paper still represents work in progress. There are several directions in which our analysis shall be extended. The next step will be to disaggregate our sample into groups of different education levels and occupations to investigate the impact of segregation on the labor market and on the returns to experience for women and men.

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Appendix Table 1: OLS wage equation for men (Model 1)

	Employment		Unemployment		Out-of-the-labor-force		Education		Other control variables		
	Coef.	T-Stat.	Coef.	T-Stat.	Coef.	T-Stat.	Coef.	T-Stat.		Coef.	T-Stat.
$share_{t-1}$	.1001	18.11	-.0679	-7.19	-.0475	-5.28	.0372	4.42	Firm size (ref. < 20 employees)		
$share_{t-2}$	.0438	7.26	-.0429	-4.37	.0010	0.11	.0020	0.23	20	-.0171	-6.42
$share_{t-3}$	.0364	6.00	-.0258	-2.64	-.0118	-1.26	.0004	0.04	50	.0135	6.06
$share_{t-4}$	.0288	4.74	-.0232	-2.40	-.0055	-0.58	.0034	-0.39	100	.0340	13.48
$share_{t-5}$	.0283	4.62	-.0177	-1.85	.0081	0.85	.0114	1.30	500	.0520	28.90
$share_{t-6}$	.0243	3.89	-.0170	-1.79	-.0034	-0.35	.0093	1.04	1000	.0790	30.40
$share_{t-7}$	.0146	2.30	-.0263	-2.81	.0031	0.32	.0016	0.18	1000m	.1321	70.52
$share_{t-8}$	.0224	3.51	-.0132	-1.39	.0070	0.71	.0080	0.89	Constant	4.6956	1306
$share_{t-9}$	.0164	2.61	-.0108	-1.11	-.0049	-0.49	.0057	0.65	Year (ref. 1995)		
$share_{t-10}$	.0189	4.18	-.0157	-1.83	.0032	0.42	.0036	0.53	1990	-.0173	-8.16
$share_{t-11}$	.0051	14.36	-.0321	-9.95	.0024	2.35	.0011	0.77	1991	.0027	1.27
									1992	.0147	6.99
									1993	.0028	1.35
									1994	-.0060	-2.88
# of observations		143,553									
Adj R^2		0.3725									

Source: Matched IAB employment sample and supplement sample I, cross sections 1990-95, own calculations. Additional control variables include education level and job status.

Appendix Table 2: OLS wage equation for women (Model 1)

	Employment		Unemployment		Parental leave		Out-of-the-labor-force		Education		Other control variables		
	Coef.	T-Stat.	Coef.	T-Stat.	Coef.	T-Stat.	Coef.	T-Stat.	Coef.	T-Stat.		Coef.	T-Stat.
$share_{t-1}$	.0790	8.78	-.0556	-3.25	-.2039	-5.60	-.0786	-5.95	.0282	0.97	Firm size (ref. < 20 empl.)		
$share_{t-2}$	.0394	4.06	-.0205	-1.19	-.3140	-10.54	-.0635	-4.59	.0324	1.23	20	-.0052	-1.07
$share_{t-3}$	.0484	4.77	-.0126	-0.72	-.2506	-9.62	-.0196	-1.38	.0032	0.12	50	.0337	7.83
$share_{t-4}$	.0347	3.26	-.0195	-1.10	-.2967	-12.28	-.0379	-2.56	-.0286	-0.95	100	.0886	18.95
$share_{t-5}$	.0317	2.82	-.0174	-0.95	-.2562	-10.98	-.0127	-0.81	.1222	1.97	500	.1539	46.43
$share_{t-6}$	.0232	1.95	-.0236	-1.25	-.2568	-10.56	-.0213	-1.30	.0351	0.44	1000	.2054	40.77
$share_{t-7}$	.0191	1.50	-.0140	-0.73	-.2778	-11.05	-.0242	-1.39	-.0432	-0.50	1000m	.2587	62.7
$share_{t-8}$	.0082	0.62	-.0092	-0.45	-.3046	-11.67	-.0210	-1.16	.0107	0.12	Constant	4.3269	667
$share_{t-9}$	.0335	2.47	-.0106	-0.50	-.2079	-7.65	.0109	0.59	.0716	0.77	Year (ref. 1995)		
$share_{t-10}$	.0003	0.03	-.0544	-2.78	-.1938	-8.01	-.0136	-0.94	-.0130	-0.17	1990	-.0410	-10.12
$share_{t-11}$	.0019	2.71	-.0264	-3.19	-.0921	-12.07	-.0012	-0.67	.0629	5.17	1991	-.0174	-4.38
											1992	.0090	2.31
											1993	.0015	0.39
											1994	-.0080	-2.11
# of observations			74,561										
Adj R^2			0.3441										

Source: Matched IAB employment sample and supplement sample I, cross sections 1990-95, own calculations. Additional control variables include education level and job status.

Appendix Table 3: Fixed effects wage equation for men (Model 2)

	Employment		Unemployment		Out-of-the-labor-force		Education		Other control variables		
	Coef.	T-Stat.	Coef.	T-Stat.	Coef.	T-Stat.	Coef.	T-Stat.		Coef.	T-Stat.
share$_{t-1}$	.0805	23.74	-.0178	-3.30	.0008	0.16	.0677	12.75	Firm size (ref. < 20 employees)		
share$_{t-2}$	.0295	9.01	-.0170	-3.16	.0109	2.14	.0350	6.87	20	.0053	2.56
share$_{t-3}$	.0303	9.15	.0043	0.77	.0051	0.97	.0379	7.17	50	.0159	7.97
share$_{t-4}$	.0238	7.26	-.0007	-0.14	.0023	0.43	.0351	6.39	100	.0273	12.24
share$_{t-5}$	.0202	6.10	-.0031	-0.56	.0057	1.06	.0375	6.57	500	.0425	22.13
share$_{t-6}$	.0167	4.98	-.0044	-0.80	-.0023	-0.43	.0405	6.97	1000	.0547	20.97
share$_{t-7}$	.0080	2.35	-.0159	-2.88	-.0010	-0.18	.0390	6.60	1000m	.0732	28.41
share$_{t-8}$	.0146	4.28	-.0066	-1.19	.0068	1.22	.0440	7.31	Constant	4.8477	235
share$_{t-9}$	.0073	2.16	-.0077	-1.35	-.0064	-1.14	.0429	7.09	Year (ref. 1995)		
share$_{t-10}$	.0167	5.98	-.0059	-1.10	.0029	0.59	.0529	9.52	1990	-.0591	-6.83
share$_{t-11}$	.0015	0.82	-.0246	-6.02	-.0048	-1.41	.0378	7.67	1991	-.0307	-4.40
									1992	-.0106	-2.00
									1993	-.0153	-4.23
									1994	-.0175	-8.84
# of observations	143,553										
# of groups	32,057										
within-group R^2	0.1744										
Corr. (u_i * Xb)	0.0940										

Source: Matched IAB employment sample and supplement sample I, cross sections 1990-95, own calculations.

Appendix Table 4: Fixed effects wage equation for women (Model 2)

	Employment		Unemployment		Parental leave		Out-of-the-labor-force		Education		Other control variables		
	Coef.	T-Stat.	Coef.	T-Stat.	Coef.	T-Stat.	Coef.	T-Stat.	Coef.	T-Stat.		Coef.	T-Stat.
$share_{t-1}$	.0656	11.16	-.0088	-0.92	-.0062	-0.20	-.0856	-11.07	.1424	8.09	Firm size (ref. < 20 employees)		
$share_{t-2}$	.0377	6.88	.0047	0.51	-.0152	-0.53	-.0583	-7.85	.1194	7.44	20	.0041	1.18
$share_{t-3}$	.0353	6.15	.0102	1.05	-.0026	-0.09	-.0618	-7.96	.1070	6.54	50	.0335	9.40
$share_{t-4}$	.0243	4.16	.0003	0.03	-.0221	-0.78	-.0603	-7.50	.0820	4.51	100	.0533	13.66
$share_{t-5}$	.0215	3.57	.0146	1.43	-.0279	-0.98	-.0546	-6.48	.1501	4.15	500	.0793	23.88
$share_{t-6}$	.0147	2.35	.0088	0.84	-.0351	-1.22	-.0553	-6.38	.0969	2.59	1000	.0928	19.43
$share_{t-7}$	.0093	1.42	.0184	1.71	-.0483	-1.66	-.0674	-7.44	.0661	1.60	1000m	.1024	21.38
$share_{t-8}$	.0067	0.99	.0116	1.05	-.0532	-1.79	-.0754	-8.08	.1174	2.87	Constant	4.6650	121
$share_{t-9}$	.0104	1.51	.0116	1.01	-.0457	-1.52	-.0664	-6.98	.1011	2.34	Year (ref. 1995)		
$share_{t-10}$	-.0001	-0.02	.0155	1.37	-.0383	-1.28	-.0797	-9.42	.0826	2.16	1990	-.0989	-5.23
$share_{t-11}$	-.0043	-1.11	.0062	0.67	-.0249	-0.87	-.0708	-12.51	.0586	2.85	1991	-.0627	-4.12
											1992	-.0271	-2.36
											1993	-.0221	-2.86
											1994	-.0201	-4.88
# of observations			74,561										
# of groups			19,132										
within-group R^2			0.2054										
Corr. (u_i * Xb)			-0.1308										

Source: Matched IAB employment sample and supplement sample I, cross sections 1990-95, own calculations.

Appendix Table 5: IV fixed effects wage equation for men (Model 3)

	Employment		Unemployment		Out-of-the-labor-force		Education		Other control variables		
	Coef.	T-Stat.	Coef.	T-Stat.	Coef.	T-Stat.	Coef.	T-Stat.		Coef.	T-Stat.
share$_{t-1}$	.0793	14.84	-.0259	-2.58	-.0016	-.15	.0649	8.03	Firm size (ref. < 20 employees)		
share$_{t-2}$	.0289	5.72	-.0255	-2.43	.0059	.63	.0337	4.42	20	.0049	1.35
share$_{t-3}$	.0299	7.00	-.0052	-.56	.0001	.01	.0361	4.12	50	.0144	4.20
share$_{t-4}$	.0230	5.22	-.0092	-1.01	-.0019	-.18	.0327	3.92	100	.0254	6.79
share$_{t-5}$	.0193	3.90	-.0108	-1.12	.0019	.20	.0348	3.64	500	.0423	12.24
share$_{t-6}$	.0154	3.58	-.0118	-1.33	-.0065	-.67	.0361	4.18	1000	.0564	13.89
share$_{t-7}$	.0074	1.86	-.0221	-2.67	-.0046	-.49	.0351	3.97	1000m	.0746	17.12
share$_{t-8}$	.0135	3.68	-.0138	-1.50	.0016	.17	.0396	4.18	Constant	4.8238	163.64
share$_{t-9}$	.0064	1.70	-.0141	-1.61	-.0113	-1.17	.0382	4.07	Year (ref. 1995)		
share$_{t-10}$	.0155	4.36	-.0134	-1.43	-.0017	-.20	.0481	4.93	1990	-.0602	-5.61
share$_{t-11}$	.0005	.27	-.0299	-3.80	-.0088	-1.14	.0338	3.48	1991	-.0318	-3.75
									1992	-.0116	-1.79
									1993	-.0162	-3.65
									1994	-.0180	-7.71
									Job status (ref. blue collar)		
									White coll.	.1010	5.60
# of observations			143,553								
# of groups			32,057								
within-group R^2			0.1751								
Corr. (u_i * Xb)			0.2361								

Source: Matched IAB employment sample and supplement sample I, cross sections 1990-95, own calculations. All activity shares and the dummy variable for white collar workers are instrumented.

Appendix Table 6: IV fixed effects wage equation for women (Model 3)

	Employment		Unemployment		Parental leave		Out-of-the-labor-force		Education		Other control variables		
	Coef.	T-Stat.	Coef.	T-Stat.	Coef.	T-Stat.	Coef.	T-Stat.	Coef.	T-Stat.		Coef.	T-Stat.
$share_{t-1}$	.0582	5.52	-.0141	-.63	-.0179	-.23	-.0929	-4.63	.1395	3.35	Firm size (ref. < 20 employees)		
$share_{t-2}$	.0312	2.85	.0009	.05	-.0255	-.36	-.0644	-4.11	.1176	2.91	20	.0043	.62
$share_{t-3}$	.0283	2.77	.0060	.32	-.0141	-.21	-.0685	-3.81	.1045	2.70	50	.0341	4.46
$share_{t-4}$	.0175	1.56	-.0041	-.22	-.0333	-.48	-.0666	-3.50	.0798	1.74	100	.0535	7.15
$share_{t-5}$	.0149	1.37	.0103	.53	-.0389	-.55	-.0608	-3.25	.1510	.85	500	.0789	11.59
$share_{t-6}$	.0079	.64	.0046	.21	-.0464	-.64	-.0619	-3.00	.0952	.63	1000	.0923	10.96
$share_{t-7}$	.0025	.23	.0143	.69	-.0596	-.83	-.0742	-3.62	.0650	.26	1000m	.1016	10.59
$share_{t-8}$	-.0001	-.01	.0076	.37	-.0644	-.82	-.0818	-3.95	.1153	.50	Constant	4.7646	25.24
$share_{t-9}$	.0034	.27	.0073	.35	-.0574	-.73	-.0733	-3.44	.1011	.36	Year (ref. 1995)		
$share_{t-10}$	-.0068	-.66	.0114	.48	-.0499	-.60	-.0858	-3.95	.0824	.47	1990	-.1333	-2.82
$share_{t-11}$	-.0113	-1.16	.0016	.08	-.0376	-.47	-.0780	-4.72	.0574	.26	1991	-.0901	-2.37
											1992	-.0477	-1.69
											1993	-.0358	-1.87
											1994	-.0269	-2.78
											Job status (ref. blue collar)		
											White coll.	-.0396	-.30
# of observations			74,561										
# of groups			19,132										
within-group R^2			0.2064										
Corr. (u_i * Xb)			-0.1038										

Source: Matched IAB employment sample and supplement sample I, cross sections 1990-95, own calculations. All activity shares and the dummy variable for white collar workers are instrumented.