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DETERMINANTS OF NON-EMPLOYMENT
AND UNEMPLOYMENT DURATIONS
IN EAST GERMANY

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ABSTRACT

Following monetary union with the west in June 1990, the employment rate for east German 18-54 year olds fell from 89% to 73% in six years, and the decline for women was considerably larger. This employment fall is possibly the worst of any European transition economy, yet one might have expected the east German transition to have been the most successful. I seek insight into the problem by examining the determinants of transitions between non-employment (or unemployment) and employment, using the 1990-1996 survey years of the German Socio- Economic Panel. Individuals over fifty and women have much longer non-employment durations, but the presence of children, and hence child care, does not appear to be important. More skilled individuals, as measured by their education and 1990 wage, have shorter non-employment spells. I also present results for employment duration. The most important similarity between the duration of non-employment and employment is the influence of the 1990 wage, which is consistent with the theory that trade-union wage rises for the less-skilled reduced employment. The most important difference is that the addition of covariates, particularly the 1990 wage, explains most of the gender gap in employment duration but little in non-employment duration.

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Following monetary union with the west in June 1990, the employment rate for east German 18-54 year olds fell from 89% to 73% in six years. Early retirements caused a larger fall for 18-64 year olds, from 85% to 64%. These falls came about in the first two years of transition, after which employment and the employment rate stabilized but did not recover. This employment fall is possibly the worst of any European transition economy. Yet, one might have expected the east German transition to have been the swiftest and most successful. Thanks to unification with the west, well-tested legal, industrial relations and political systems were immediately available, as well as large transfers. These transfers have indeed cushioned the effect of the employment loss, and large real wage gains distinguish east German workers from their counterparts further east. However, the low employment rate is a clear indicator that east Germany is not on the track to economic efficiency, and may even be caused by the rise in wages. In this paper I seek insight into the problem by examining the determinants of transitions between non-employment and employment, using the 1990-1996 survey years of the German Socio-Economic Panel (GSOEP).¹

Lower employment rates and economic efficiency would not be incompatible if the reductions in employment rates represented voluntary reductions and convergence with western norms. However, this does not appear to be the case. Of the population aged 18-54 in 1994-1996, 10% were unemployed; that is, both searching and available for immediate work. A further 4% were available for immediate work but not searching, possibly due to discouragement. These two groups account mechanically for most of the fall in the employment rate. Further, the employment category includes the 4% of the population in public works jobs, as well as the beneficiaries of government

¹ The statistics cited in the introduction are from the sample-weighted GSOEP data.

subsidies. The 3% of the population in government training programs are variously counted as employed or not depending on their self-identification.

The situation is more dramatic for women than for men. For 18-54 year olds, the female employment rate fell by 4 percentage points more than the male rate, from 85% to 67%, and the 1994-1996 unemployment to population ratio was 12% for women and 8% for men. 6% of the female population was available for immediate work although not searching, compared to 3% for men. These statistics suggest that if the labor market difficulties of women could be reduced to “only” those of men, this alone would have an important impact on the overall labor market situation.

The main hypothesis with regard to the cause of the employment fall links the wage rises, which were brought about by trade unions, to the persistence of low labor demand. For women, possible effects of discrimination and the large post-communist reduction in child care are also invoked. My earlier paper (Hunt 1997) shed some light on these issues by examining the determinants of transitions to non-employment of individuals who had been working at the time of monetary union. An important observation in this earlier paper was the strong negative effect of the pre-monetary union wage on the risk of leaving employment. This effect was found to operate through the risk of layoff, and hence through labor demand rather than supply. It seems likely that despite changes in the labor market, the pre-monetary union wage continued to proxy for skill. Since workers with initially low wages were found to have the largest wage gains, the results were consistent with a negative effect of trade union wage gains on labor demand. Another possibility, supported by the case studies of Turner (1998), is that labor demand for all skill types fell, but firms wished to

hoard high skill workers fearing that they would otherwise emigrate.

Controlling for the pre-monetary union wage eliminated more than half the large gender gap in the hazard to non-employment. The presence of children in the household did not appear to play an important role. These results suggested that the gender gap may in large part have represented a skills gap, and that policy aimed at reducing the gender gap through mechanisms not targeting this skill gap might be misdirected. An alternative explanation is that rather than representing skill differences that are valued similarly in the capitalist system, the 1990 wage gaps represented a type of discrimination against women under communism that has carried over and is now finding expression in employment gaps. Turner (1998) reports the decision of Volkswagen to rehire few women from the Saxony auto plant it took over, following its decision to use rotating shifts.

In this paper I begin by presenting some further results on transitions to non-employment, before examining the determinants of transitions from non-employment to employment. As a sensitivity check, and for comparison with existing work, I also examine transitions out of unemployment. I assess the importance of age, gender, education and the presence of children, and I test whether the 1990 wage influences non-employment duration to the same extent as employment duration. I am also interested in whether as much of the gender gap in transitions to employment can be explained as was the case for transitions out of employment.

If the 1990 wage effect on employment duration reflected a greater departure of unskilled wages than skilled wages from the market-clearing wage, the effect might be similar (although opposite in sign). If it reflected labor hoarding behavior, it is possible that the effect on non-employment

duration could be smaller. Another possibility is that skill might have become devalued during a non-employment spell, and the 1990 wage might have a lower effect on re-employment chances. For example, even if there is no absolute skill depreciation, the non-employed are becoming relatively less skilled as their employed counterparts learn to put in western effort levels and to adopt work practices and techniques in some cases more modern than in the west (see Turner 1998). In addition to these labor demand considerations, the wage could also operate through labor supply for non-employment duration. For example, if high-skilled workers require a more specific match, they might tend to search longer, which might offset labor demand considerations.

My analysis improves on existing work on east German unemployment duration by testing sensitivity to different specifications, by testing sensitivity to the choice of sample, by considering non-employment as well as unemployment spells, by studying the effect of the 1990 wage, and by extending the sample period.

Focusing on the group which had been working at the time of monetary union, I find that the variables influencing the incidence and duration of non-employment are similar (results for unemployment duration and for broader samples are similar). Age and gender are the most important determinants, with individuals over fifty and women having much longer non-employment durations. Women with very young children have longer non-employment spells, but there are not enough such women for this to be an important factor in the gender gap. The provision of better child care might therefore not have a large impact.

More skilled individuals, as measured by their education and 1990 wage, have shorter non-employment spells. The effect of the 1990 wage is smaller than it had been for the incidence of

non-employment, although not significantly so. This symmetric result is thus consistent with the hypothesis that trade unions have pushed unskilled wages farther above the market clearing wage than skilled wages. However, I cannot rule out that the 1990 wage might also operate through labor supply for non-employment duration.

The most crucial difference between the duration of employment and non-employment is that the 1990 wage explains much less of the gender gap in the latter case: the raw 50% gender difference in both hazards is reduced by 60% when the 1990 wage is added in an employment duration regression, but by only 20% in the non-employment duration regression. The addition of covariates does not change the conclusion that overall the analysis is much less successful in explaining the non-employment duration gender gap than the employment duration gap. Apparently women's re-employment chances are affected by something unobservable that was much less important for employment duration. This could work through labor demand and possibly discrimination, a possibility the Volkswagen case study is consistent with, or through labor supply.

1 Background and Existing Literature

Monetary union between east and west Germany took place on 1 July 1990. Some economic changes had been occurring earlier in 1990, but monetary union marked the beginning of the east's shock therapy. East German wage contracts were converted to west German Marks at a rate of one for one (Krueger and Pischke 1995). By mid-1991 most industries' wage bargaining had been taken over by the western unions, who bargained at the industry level with the appropriate

employer federation. The unions successfully bargained for large wage increases, which varied considerably, however, according to the union's perception of the health of the industry.

In 1993, in the industry whose union is always at the vanguard of change in Germany, metal-working, employers revolted against the contracts designed to bring convergence with the west by 1994, and convergence was delayed until 1996. Employers generally sought ways to avoid paying the high wages, and did this either by reopening contracts, by illegally paying below the contract wage, or by not joining the employers' federation, and either concluding a firm-level agreement or not being a party to the wage contracts.

Average wage growth was enormous. The median real monthly wage for workers 18-54 rose 83% from 1990 to 1996, and rose most for the initially lowest paid. The bottom three deciles had a rise of more than 100%. (See Hunt 1997,1999.) The east-west ratio of nominal monthly wages, as reported by the Statistisches Bundesamt, was 75% in the first half of 1997 (and hence the hourly wage ratio was slightly lower, since eastern hours are longer, and end of year bonuses lower). It has been common to attribute the employment decline to the high wages, and especially in the early years some economists called for wage subsidies to remedy this (Bedau 1996, Bellmann 1994, Begg and Portes 1992). Hunt (1997) also provides some evidence supporting this hypothesis. Eastern GDP per capita for the first half of 1997 was 57% of the western figure, measured in current prices, reflecting the fact that eastern workers have progressed farther than easterners generally.²

One large government policy designed specifically for east Germany was an early retirement

² GDP figures come from the Bundesbank web page www.bundesbank.de, population and earnings ratio figures come from the Statistisches Bundesamt web page www.statistik-bund.de/presse/deutsch/pm/p7366042.htm.

program with western-level benefits, that allowed those 55 or older to retire. This program resulted in almost all workers eligible for the early retirement program taking advantage of it before it ended in December 1992. Public works jobs were also widely used. The western system of short-time work, where workers are put on reduced hours, and have their lost hours partially compensated by the government, was widely used, particularly in the first year of the transition. Government training programs have also been used to combat unemployment. In 1995, 1,047,000 individuals were registered as unemployed, 419,000 were in public works jobs or in a subsidized job, and 254,000 were in public training programs (Vogler-Ludwig 1996).

The greater fall in employment and rise in unemployment for women is a topic of wide discussion. Since the close attachment of women to the labor force is acknowledged (see Holst and Schupp 1992, for example), the hypothesis that this is driven by a desire on the part of women to adopt labor supply behavior more similar to that of women in the west is not given serious consideration. One hypothesis is discrimination, while the other is the great fall in child care (see, for example, Brinkmann et al. 1993). Under communism, child care had been cheaply and readily available, while after monetary union the number of children in child care dropped precipitously. However, it is difficult to distinguish the links between the fall in fertility and the fall in child care, particularly since child care availability is not measurable, only child care use. In other post-communist countries the reduction in child care is believed to have played an important role in reducing female labor supply. For the Czech Republic and Slovakia, for example, Chase (1996) finds that the fall in female labor supply is concentrated among young married women, and that young children have a larger deterrent effect on women's participation after communism

than during communism.

Two existing studies include a regression analyzing the determinants of the duration of unemployment, both using the GSOEP data. Steiner (1994) analyzes the hazard rate from unemployment to employment for all unemployed for the period from monetary union through 1992. His results differ somewhat from mine, apparently due to his earlier time frame and his inclusion of household income in the regressions. This is discussed further later in the paper. My original motivation for examining unemployment as well as non-employment spells was to reproduce his results. Berger et al. (1996) consider two states, full-time work and other, and examine the hazard rates between these two states for the group employed in July 1989, for the period July 1989 through 1991. Exact significance levels are not reported, but the authors find important effects of age and sex, and also some effects of education. Licht and Steiner (1994) analyze the hazard from employment to non-employment.

2 Data

The data used are from respondents to the German Socio-Economic Panel (GSOEP) who resided in east Germany when that region was first surveyed in June 1990, immediately before monetary union. Individuals who moved to western Germany or who subsequently worked in western Germany are not removed from the sample, and hence the analysis refers to eastern Germans, the group whose welfare is of interest, rather than to eastern Germany.

To analyze non-employment and unemployment, I use the “calendar” section of the GSOEP:

respondents 16 or older indicate for the calendar year preceding the interview what their labor force status was month by month. This allows the construction of spells from June 1990 through 1995, using the 1991–1996 survey years. Non-employment spells are composed of those months where the respondent was neither in full-time, part-time, irregular nor short-time employment. Unemployment spells are composed of months where the respondent either indicated being registered at the unemployment office, or indicated being in a training spell associated with payments from the labor office. Months in public training programs are treated like months of registered unemployment since individuals in this training, while clearly not employed in the usual sense, have a closer labor force attachment than those out of the labor force. This also ensures consistency with Steiner (1994). Of the observations in the unemployment spells, 18% are due to months in public training programs. The wage used is gross earnings in the month prior to the interview, not adjusted for end of year bonuses (weekly or hourly wages are not available). (See the Data Appendix for details on this and other variables).

3 Sample

One aim of the paper is to continue Hunt (1997)'s analysis of individuals who had been working at the time of monetary union. One sample is therefore the set of first non-employment spells for those members of the Hunt (1997) sample who experienced non-employment before the end of 1995. A second sample is the set of first unemployment spells for members of the Hunt (1997) sample experiencing unemployment before the end of 1995. A third sample is the set of first

unemployment spells for all individuals aged 18–65 who experienced some unemployment in the July 1990–1995 period. I refer to these samples as the non-employment and unemployment spells of experienced individuals, and the broad unemployment sample. I considered that the behavior of individuals not working and not unemployed might differ greatly between the majority of individuals who are closely attached to the labor force and the minority that did not work even under communism, and I therefore do not attempt to study non-employment spells for all non-employed. The Hunt (1997) sample focused on those working in June 1990 for whom a meaningful wage was available, and thus excluded the self-employed, workers in agriculture (15% of the work force), and apprentices. Workers under 18 years old or over 60 in 1990 were also excluded.

Some discussion of the difference between non-employment and unemployment spells in theory and in practice is appropriate. In theory someone experiencing an unemployment spell is available and searching for work. Non-employment spells then in theory include months of unemployment and months where the individual is out of the labor force. One would not generally expect to find transitions to employment from out of the labor force, but there could be exceptions to this, such as when a woman returns to her job from maternity leave, or when an individual finds a job so quickly that no period of unemployment is recorded. Therefore, one reason to look at non-employment as well as unemployment spells would be to look at the determinants of search and search success simultaneously, as well as the determinants of search success separately. In practice the categories are not defined as they should be in theory.

The GSOEP allows identification of spells of registration at the labor office and of public training. Registering at the labor office is a necessary condition for receiving unemployment

benefits, but other individuals might also be registered to receive job search help, or if their benefits are exhausted. Recipients of unemployment benefits need only be available for a job, and are not required to search on their own, although in principle the labor office is searching on their behalf. Conversely, individuals who are not working and not registered may also be searching for a job.

The extent of the blurring of the “true” unemployment and out of the labor force categories may be gauged from GSOEP questions put to respondents not employed at the survey date. For the years 1994–1996 respondents were asked both if they were available now were a suitable job presented to them, and whether they had searched in the last three months. For the purposes of calculating the “true” or “ILO” unemployment rate for this section and the introduction only, I count as unemployed individuals answering yes to both questions.

I consider months of spells that correspond to interview months, for the sample of experienced unemployed. About one quarter of the registered unemployed were either not searching or not available, and hence not “truly” unemployed. Of those in public training programs, almost half indicated that they were working and were therefore not asked the appropriate questions. Of the remainder, about half were “truly” unemployed. If those who indicated that they were working were not searching, an argument could be made for grouping these months with the months out of the labor force. In practice this makes little difference to the analysis below of the hazard to employment. Due to the empirical blurring of the categories, the analysis of unemployment spells is probably best seen as a sensitivity check and an opportunity to compare with other studies, rather than something allowing deep additional insight.

4 Descriptive Statistics

Tables 1 and 2 provide descriptive statistics for the non-employment and unemployment spells of individuals who had been working at the time of monetary union (experienced individuals), and the broad sample of unemployment spells, as well as for the younger subsets of these samples. The means are computed across all the person-month observations. The variables, which are those used in the regression analysis below, are time-varying with the exceptions of sex and the 1990 wage, and also age which refers to age at the start of the spell.

The first column of Table 1 describes the non-employment spells for all experienced individuals. 64% of the full sample is female. Also, 33% of the full sample is aged over 55 at the start of the spell, and a further 20% is aged 51-55. The sample age restriction (no older than sixty at monetary union) was chosen for consistency with employment duration analysis of Hunt (1997). However, due to the early retirement of most of those aged 55 or older at monetary union, the full sample of non-employment spells has a large share of spells of retired individuals whose behavior will be unaffected by most covariates. Therefore, it is worthwhile also to analyze a sub-sample of younger individuals who would not be facing a retirement option.

Column 2 therefore contains means for experienced individuals who were aged fifty or less at the time their spell began, while columns 3 and 4 split this subsample by gender. Compared to men, women earned less in 1990, have more young children and are much more likely to be married or living in the west.³ The women's educational distribution is also somewhat different

³ The mean 1990 wages are coincidentally identical to the numbers reported in Hunt (1997) for the sample of workers. However, Hunt (1997) reports wages in 1985 Deutschmarks, whereas in this paper wages are in 1991 Deutschmarks. Hence, as expected, the subsample of 1990 workers

from the men's. Fewer women had "general schooling" as a highest qualification (no vocational or apprenticeship qualification or higher degree), but fewer had a tertiary degree ("university"). Women were hence more likely to have either an apprenticeship (implying the dual system of firm training and classroom education) or a vocational degree. The last line of the table reports the mean spell lengths: six months for younger men and fully seventeen months for younger women.

Table 2 describes the unemployment spells of experienced individuals and of the broad sample. The age distribution of the full sample is not as skewed to older age groups, as was the case for the non-employment spells, but the sub-sample aged fifty or less at the beginning of the spell is nevertheless examined. Median spell lengths are five and twelve months for experienced younger men and women respectively, so unemployment and non-employment spells are of similar length for men, while non-employment spells are considerably longer for women.

Figure 1 shows a series of Kaplan-Meier survival curves for the three different samples, while Figure 2 reproduces the equivalent curves for the subset of individuals who were 50 or younger when their spell began. The top left panel of Figure 1 shows curves for the non-employment spells of experienced individuals. The curves are computed separately for men and women, and the figure shows that the survival rate of women is considerably higher than that of men. Despite the fact that this group had been working at the time of monetary union, the survival rate flattens out at quite a high level. Retirements are in large part responsible for this, as the equivalent panel in Figure 2 indicates: younger non-employed males almost all eventually return to employment, and

who experienced non-employment had been lower-paid when working. The figures reported by gender in Table 1 are furthermore for the younger subset, which affects the means and gender gap slightly.

the survival rate of younger women in non-employment is also much lower. Since the retirement behavior of the men and women is not dissimilar, the male-female gap is larger for the younger sample.

The lower left panel of Figure 1 plots the survival rate for transitions from unemployment to employment for experienced individuals. Here, as elsewhere in the paper, the competing risks analysis of the transition from unemployment to employment is performed by recording spells with a transition to out of the labor force as right censored. Again the gender gap is important, and men but not women eventually seem to return to work. The equivalent plot in Figure 2 is similar, since retired individuals are not registered as unemployed. The lower right panel of Figure 1 and the equivalent panel for younger unemployed in Figure 2 plot the survival rate for escapes from unemployment to employment for the broad sample. The plots for the broad sample are similar to those for spells of experienced individuals. By comparison, Hunt (1995) shows that for transitions from unemployment to a job in west Germany, the survival rate at twelve months is about 40% for both sexes pooled, similar to the eastern male rate shown here. In the United States less than 10% of unemployment spells last more than twelve months (OECD 1998).

Finally, the upper right panel of Figures 1 and 2 plot the survival rate for escapes from unemployment to out of the labor force for experienced individuals (escapes to employment are recorded as censored). The survival rates do not look significantly different for men and women, and are based on only a small number of completed spells.

5 Results

The hazard model used to analyze the durations of spells is the Cox partial likelihood proportional hazards model. The hazard is assumed to be of the form

$$\lambda(t; z) = \lambda_0(t)e^{z(t)\beta}, \quad (1)$$

where $z(t)$ contains potentially time-varying covariates describing the individuals, and $\lambda_0(t)$ is the baseline hazard, which is allowed to be non-parametric (if the covariates are measured as deviations from means, the baseline hazard may be interpreted as the hazard for the mean individual). The likelihood function is

$$L = \prod L_i, \quad (2)$$

where

$$L_i = \frac{e^{\sum_j z_{ij}(t)\beta}}{[\sum_{h \in R^i} e^{z_h(t)\beta}]^{m_i}} \quad (3)$$

and where m_i is the number of individuals leaving a spell at time i , the set R^i contains all observations that could have left at time i , and z_{ij} are covariates for the j th observation leaving at time i (see Kalbfleisch and Prentice 1980, p.74). The Peto–Breslow approximation is used to deal with ties (more than one spell ending in a given period). Exponentiated coefficients are reported for ease of interpretation.

To set the stage for the analysis of the non-employment spells, the Appendix Table shows results of the hazard from employment to non-employment of those working in 1990, the topic of Hunt (1997).⁴ The first two columns show that the female hazard is 47% higher than the male

⁴ These results for employment spells were mentioned but not reported in Hunt (1997), which

hazard, but that this gender gap is reduced by almost two thirds to “only” 17% when the 1990 wage is controlled for. The subsequent columns divide the sample by sex and assess the competing risks of being laid off versus other types of separation. The effect of the 1990 wage is shown to operate similarly for men and women, but through the risk of layoff only, suggesting a labor demand story. For both men and women the most influential variables are the dummies for eligibility for early retirement, and for women only the presence of a child age 0–1 increases the non-layoff hazard. Since few women had a child aged 0–1, in part due to the fertility rate plummeting, children and hence child care are not judged to be an important factor. In fact, even under communism women had typically stayed at home for the first year of their baby’s life. Being married made men but not women less likely to leave employment, while women with apprenticeships but not men were more likely to leave employment than their counterparts with vocational training. These results thus account for the greater proportion of married women, women with small children, and women with apprenticeships amongst the non-employed in Table 1.

Turning to the determinants of non-employment and unemployment durations, I first investigate whether the 1990 wage is as important for these durations as it was for employment duration, in terms of the coefficient magnitude and in terms of explaining the gender gap. Table 3 examines the hazard to employment for the experienced non-employment sample and the experienced unemployment sample, and the younger sub-sample of each, controlling first for sex only and then adding the 1990 wage as a covariate. The odd-numbered columns of Table 3 show that the female hazard to employment is about half the male, or slightly more when the retired men and

focused on “attachment” spells where the individual might leave employment for up to four months.

women are included (in column 1). The raw gender gap is thus similar in the hazard rate from employment to non-employment and vice-versa (the female hazard is about 50% higher and lower respectively). In the even-numbered columns the log 1990 wage is added to the covariates. The columns other than column 1 indicate a significant (exponentiated) coefficient of between 1.5 and 1.7. The magnitude of 1.6 for the younger non-employment spells in column 4 implies that a 10% higher wage is associated with a 5% higher hazard rate to a job. The (exponentiated) magnitude of 0.49 on the wage in the Appendix Table (column 2) indicates that a 10% higher wage is associated with a 7% lower hazard rate to non-employment. The coefficient of 1.6 in Table 3 is not significantly different from one implying a 7% effect (a coefficient of 2), but the difference in the point estimates is a partial explanation of why the gender gap is reduced only by between 16% and 22% when the 1990 wage is added as a covariate in Table 3. Also, it might be argued that a 7% fall in a hazard is a larger effect than a 7% rise in a hazard, which would also suggest that the wage plays a lesser role in exits from non-employment. However, most of the difference must lie in some gender-specific effect that affects non-employment duration but not employment duration.

Table 4 investigates in more detail the determinants of the hazard from non-employment to employment for those who had been working at the time of monetary union. Column 1 controls for education, age and sex only, while column 2 also controls for marital status, presence of children of different ages, whether the individual was living in the west and the 1990 wage. In the third column the sample is restricted to those 50 or younger at the start of the spell. All regressions in this and subsequent tables also include year dummies. Dummies for federal state were always jointly insignificant, and hence were not included.

The first three columns paint a similar picture. As expected, older individuals, who are likely to have retired, are much less likely to leave non-employment. The hazard for those over 55 at the start of the spell is only 4% of that of the omitted 36-45 year old group, and the 51-55 year old hazard is 30% of that of the reference group. The next most important factor is gender, with the female conditional hazard only half the male hazard, despite the presence of more covariates than in Table 3. The small number of individuals with only general schooling have half the conditional hazard rate of the omitted group with vocational training, while those with an apprenticeship have 70–80% of the hazard of those with vocational training. The 1990 wage has a significant positive effect on the hazard rate. Since workers with vocational training are better paid than those with an apprenticeship, these results are consistent with shorter spells for more skilled individuals. Married individuals also have a higher hazard rate, by 38%, compared to unmarried individuals. The presence of a child age 0–1 reduces the hazard by 40%. If we compare with the results of the Appendix Table, we confirm that similar variables affect the transitions between employment and non-employment, but in opposite directions: age, sex, marital status, education, the 1990 wage and the presence of a child aged 0–1.

Columns 4 and 5 split the sample of column 3 by gender. The main difference lies in the coefficients on the child variables. Not surprisingly, the reduced hazard due to the presence of a child age 0–1 in columns 2 and 3 was being driven by women. The presence of such a child reduces the female hazard by 60% (a smaller magnitude than the 700% effect found for employment duration in the Appendix Table). The presence of older children appears to have no significant effect. More surprisingly, men with children appear to have significantly higher hazard rates by

40–60%. This beneficial effect of children for men was not found for employment duration, and is in fact not robust to modifications to the sample of non–employment spells. The regressions have been repeated with all non–employment spells included (not just the first), and for a sample that, while restricted to those who were working in 1990, includes individuals who had been in agriculture or self–employment or who had had a missing wage (the wage is therefore omitted from the regressions). In these regressions the coefficients on the child dummies for men had point estimates of about 1.25 and were insignificant (these results are not reported).

To assess the importance of the child variables I can predict the twelve month survival rates for men and women based on the coefficients of columns 4 and 5, then find the predicted rates under the counter–factual of children having no effect. The predicted gender gap in the survival rate at twelve months is 0.30. If children aged 0–1 are constrained to have no effect on women, the gap falls to 0.25, a modest fall due to the scarcity of young children. If the coefficients for all ages of children are set to zero for women, the gap falls to 0.23 (but the coefficients on older children are in fact insignificant). The large but puzzling coefficients for men mean that if the child coefficients are set to zero for both men and women the predicted gap falls considerably, to 0.15. Since the mechanism through which children would affect the male hazard is unclear, however, to say that half the gap has been explained would be misleading.

In light of these results, one could ask whether reduced child care was likely to have had an important effect. Reduced child care might have increased the impact of a child, but we can see this could not have had a large effect given the small number of young children. Reduced child care could have reduced the fertility rate, but this effect would have had to be very large for child

care to be an important player. The effects of children may in fact be overstated, if young children are endogenous in the way envisioned by Witte and Wagner (1995). They believe that women who become unemployed increase their fertility as the opportunity cost of doing so falls.

General schooling is the only variable other than children to have a significant coefficient in the male regression. The unreported coefficients on the year dummies for the regressions are of some interest. For women they indicate a falling hazard over time, while for men no pattern is present. The general effects of the two modifications to the sample described above are to reduce significance slightly, particularly for the wage, in the case of the repeated spell sample, and to increase significance slightly in the case of the sample including more former workers (these results are not reported).

Table 5 performs similar regressions to those of Table 4 to analyze the competing risk of escaping to a job from unemployment for experienced individuals (spells with exits to out of the labor force are recorded as censored). The covariates are the same as in the corresponding columns in Table 4. Coefficients on dummies for federal state are generally significant, but since they affect other coefficients little, for consistency with Table 4 the results of the regressions including these dummies are not reported. A more interesting issue is that of possible inclusion of information about receipt of unemployment insurance. Most individuals (about 90%) who had been working at the time of monetary union had enough experience to be eligible for unemployment insurance. Therefore, if dummies for receipt are included in the regressions for non-employment of Table 4, they principally proxy the state of registered unemployment, and are significantly but uninterestingly positive. Similarly in the unemployment regressions of Table 5, the dummies

principally proxy the state of registered unemployment versus a government training program. If included their coefficients are again positive and significant and large, indicating that those on the training programs are probably doing much less search, but indicating nothing about the disincentives of unemployment insurance. Therefore no information about unemployment insurance is included in the regressions presented. Its existence may well lengthen spells, but a much more subtle approach would be needed to tease out the magnitude of the disincentive.

The conditional female hazard is again close to half the male hazard in Table 5. Age effects are somewhat smaller in the first two columns of Table 5 than the corresponding columns of Table 4, presumably due to the absence from the sample of those older workers who are retired. As for the skill coefficients, the coefficients on the 1990 wage are significant for the regressions pooling men and women, but not generally as large as in Table 3 now that additional covariates are present. The coefficients are more similar to those in Table 4. The coefficient on apprenticeship is negative and generally significant and of similar magnitude to that of Table 4, while the negative coefficient on general schooling is significant only in column 1. The coefficient on living in the west is now large and significant, however: the hazard rate for a person living in the west is more than double that of someone in the east, and this effect is driven by women (see columns 4 and 5). Few unemployed men in fact reside in the west. The coefficient on being married is now insignificant.

In columns 4 and 5 we see that the coefficients on children are much less significant than in Table 4, the only significant one being on the presence of a child age 7–11 in the male regression. This is in fact the only significant coefficient in the male regression, and the hypothesis that all coefficients are zero can only be rejected at the 10% level. The unreported coefficients on the year

dummies in columns 4 and 5 indicate that the hazard rate for women is declining, as was the case for the non-employment spells, while there is again no pattern for men. If the regressions of Table 5 are run with all spells included, rather than just first spells of unemployment, the results are similar, except that the coefficients are slightly less significant (these results are not reported). Adding individuals who had been excluded for having originally worked in agriculture etc. makes the coefficients slightly more significant (these results are also not reported).

Table 6 considers the hazard rate to employment for the broad sample of unemployment spells; that is, it includes spells of individuals who were not working at monetary union. For this reason the 1990 wage is not included as a covariate, but otherwise the specifications are the same as in Tables 4 and 5. The results are similar to those in Table 5 for most variables, and the gender gap remains about 50%. In similar analysis for west Germany in the 1980s, Hunt (1995) found the female hazard to be 78% of the male hazard from unemployment to employment, and one's prior might have been that the gap would be lower in east Germany where women are more attached to the labor force. Thus, while the sign of the effect is not surprising when compared to other labor markets, the magnitude is.

The child coefficients are all individually insignificant in Table 6. The coefficients are somewhat sensitive to the choice of age range for the presence of children: if the youngest category is defined as children age 0–3, its coefficient is 0.65 in the female regression and significant. The significance of the coefficient on young children in the samples of the earlier tables was not sensitive to the exact choice of age.

For this sample there are no significant variables in the male regression, and the chi-squared

test cannot reject the hypothesis that all the coefficients are zero. The sample size of 451 male spells is not especially small, and the lack of significant coefficients is surprising.

The finding using the broad sample that the female hazard is reduced by the presence of children aged 0–3 agrees with the results of Steiner (1994), who uses a similarly broad sample of unemployment spells. Another of Steiner’s findings is that gender has an effect only through interaction terms (the coefficient on the gender dummy alone is insignificant). This result is markedly different from mine, and my replication of Steiner’s specification suggests that this is due to his inclusion of household income (and its interaction with sex) as well as his much shorter time period. It is difficult to ensure that the household income variable (which refers to the month before the survey) is really matched to the beginning of a spell, which is essential, and the coefficients on sex as well as age are sensitive to exactly how the matching is done. However it is done, I can make sex and age significant by extending the time period somewhat: I noted above that the hazard for women but not men is falling over time.

The analysis of the competing risk of leaving unemployment for the state of out of the labor force has also been carried out, although results are not reported. For the sample of experienced individuals, a child aged 0–1 or living in the west raises the hazard greatly, for women only. Individuals aged 55 or older also have a significantly higher hazard. The results for the broad sample are similar (in addition, married individuals have a significantly lower hazard, individuals with only general schooling higher). The coefficients on sex and the 1990 wage are always insignificant.

6 Conclusion

An earlier paper (Hunt 1997) found that the duration of employment spells in east Germany was strongly positively related to the wage a worker had been receiving at monetary union in 1990. The fact that this effect operated through labor demand, and that those with the lowest 1990 wage had received the highest wage rises, appeared consistent with the hypothesis that wage rises negotiated by trade unions have reduced employment.

In this paper I complete the study of transitions between employment and non-employment by focusing on the duration of non-employment (and also unemployment, for which results are similar). An important finding is that the 1990 wage has a negative effect on non-employment duration, insignificantly different in absolute value from its effect on employment duration. This is consistent with the hypothesis that trade unions have contributed to the continued low employment. It is possible, however, that the magnitude for non-employment spells is similar due to a smaller effect through labor demand and an additional effect operating through labor supply.

I find that in general the variables influencing the duration of employment and non-employment are the same. Age and gender are the most important determinants, with workers over fifty and women having much longer non-employment durations. More skilled individuals, as measured by their education and 1990 wage, have shorter non-employment spells. The presence of a very young child increases non-employment duration for women (and reduces employment duration), but the low fertility rate means that this does not explain much of the gender gap in the hazard rate. This suggests that, unless fertility is very responsive to child care, the reduction in child care was not a major determinant of the employment gender gap.

An important difference between the employment and non-employment analysis is that while the 1990 wage explained 60% of the gender gap in transitions to non-employment, it explains only 20% of the gender gap in transitions out of non-employment. This indicates that the gender-related issues in ending and beginning a spell of employment are not the same. Whether the additional effects relevant for returning to employment operate through labor demand, and possibly discrimination, or through a labor supply effect not captured by marital status and the presence of children could not be determined.

Data Appendix

An individual who indicates being in vocational training in the calendar file could be in firm training, including apprenticeship, in private training outside the firm, or in a government training program for the unemployed. Through 1994 vocational training spells are identified as this last type if the income calendar file indicated that “Umschulung/Fortbildung” payments from the labor office had been received in any portion of the spell. After 1994 a vocational training spell was thus identified if these payments had been received in the year of the spell (the months of the payments no longer being available). Individuals undergoing firm training generally indicate simultaneously being employed, and are counted as such.

The number of first non-employment spells examined should equal the number of uncensored employment spells examined in the Appendix Tables, if both types are drawn from the group working in 1990 (experienced individuals). In fact there are 991 non-employment spells and 937 uncensored employment spells. About half the difference is due to non-employment spells beginning in 1995 (the employment analysis was based only on data through 1994). Most of the rest is due to cases where the employment spell was interrupted by some months of missing data. The employment spell is thus recorded as censored, but the non-employment spell is retained despite the possibility that it is the second and not the first non-employment spell since 1990.

The wage used is gross earnings in the month prior to the interview, not adjusted for end of year bonuses. Wages for respondents living in the west are adjusted using the western consumer price index (1991=100). The eastern consumer price index is adjusted so that a meaningful comparison of the price levels between east and west may be made, using the results of a 1991 study of the relative price levels in the east and west (Krause 1994). The wages of those living in the east are thus deflated with this adjusted index, so that the real wages of those in the east and west should have comparable purchasing power. It should be noted, however, that the price index for the east in 1990 is not considered as reliable as in the subsequent years.

Information about the month of birth of a child is used in constructing the dummies for presence of children. The youngest age category is constructed in such a way as to allow anticipation of a birth by one month.

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Table 1: Means of Non-Employment Spells, Experienced Individuals
(Standard Deviations of Non-Dummy Variables in Parentheses)

Variable	(1)	(2)	(3)	(4)
	All	Age <=50	Men <=50	Women <=50
Sex (female=1)	0.64	0.72	0	1
Log wage 1990	7.25 (0.38)	7.19 (0.35)	7.43 (0.28)	7.10 (0.33)
General schooling?	0.10	0.07	0.10	0.06
University?	0.07	0.06	0.09	0.05
Apprenticeship?	0.64	0.69	0.67	0.69
Vocational training?	0.19	0.18	0.14	0.19
Age <=35 ?	0.25	0.53	0.56	0.52
Age 36-45 ?	0.16	0.34	0.32	0.34
Age 46-50 ?	0.06	0.14	0.13	0.14
Age 51-55 ?	0.20	0	0	0
Age >55 ?	0.33	0	0	0
Married?	0.76	0.70	0.56	0.76
Child 0-1 ?	0.05	0.11	0.03	0.14
Child 2-6 ?	0.10	0.21	0.13	0.22
Child 7-11 ?	0.14	0.30	0.19	0.34
Living in west?	0.01	0.02	0.004	0.03
Number of Spells (Exits)	991 (564)	679 (501)	274 (229)	405 (272)
Median spell length (months)	19	12	6	17
Total observations	21311	10089	2776	7313

Notes:

- a. Averages are over all person-month observations.
- b. Variables are time-varying except for age, which refers to the beginning of the spell, sex, and the 1990 wage.
- c. "Experienced individuals" refers to individuals who were working in June 1990.

Table 2: Means of Unemployment Spells
(Standard Deviations of Non-Dummy Variables in Parentheses)

Variable	Experienced individuals				Broad sample	
	(1)	(2)	(3)	(4)	(5)	(6)
	All	Age <=50	Men <=50	Women <=50	Men <=50	Women <=50
Sex (female=1)	0.69	0.71	0	1	0	1
Log wage 1990	7.21 (0.36)	7.20 (0.35)	7.44 (0.27)	7.10 (0.33)	--	--
General schooling?	0.06	0.05	0.03	0.06	0.06	0.10
University?	0.06	0.07	0.12	0.04	0.11	0.04
Apprenticeship?	0.69	0.72	0.69	0.73	0.68	0.70
Vocational training?	0.18	0.17	0.17	0.17	0.15	0.16
Age <=35 ?	0.37	0.47	0.50	0.46	0.59	0.55
Age 36-45 ?	0.29	0.37	0.37	0.36	0.30	0.30
Age 46-50 ?	0.13	0.16	0.13	0.18	0.11	0.15
Age 51-55 ?	0.17	0	0	0	0	0
Age >55 ?	0.05	0	0	0	0	0
Married?	0.76	0.75	0.67	0.78	0.58	0.75
Child 0-1 ?	0.02	0.02	0.04	0.01	0.04	0.02
Child 2-6 ?	0.17	0.21	0.22	0.21	0.22	0.31
Child 7-11 ?	0.26	0.33	0.22	0.37	0.24	0.34
Living in west?	0.01	0.01	0.005	0.01	0.01	0.01
Number of Spells (Exits)	722 (571)	587 (479)	236 (202)	351 (277)	451 (391)	627 (512)
Median spell length (months)	10	9	5	12	5	12
Total observations	9182	7165	2068	5097	3690	8997

Notes:

- a. Averages are over all person-month observations.
- b. Variables are time-varying except for age, which refers to the beginning of the spell, sex, and the 1990 wage.
- c. "Experienced individuals" refers to individuals who were working in June 1990.

Table 3: Effects of Sex and 1990 Wage on Transitions to Employment,
 Experienced Individuals
 (Exponentiated coefficients;t-statistics in parentheses)

Variable	Non-employment Spells				Unemployment Spells Hazard to employment			
	All		Age <=50		All		Age <=50	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sex (female=1)	0.66 (-4.9)	0.72 (-3.4)	0.50 (-7.6)	0.58 (-5.4)	0.54 (-6.4)	0.64 (-4.0)	0.53 (-6.4)	0.63 (-4.0)
Log wage 1990	--	1.23 (1.5)	--	1.60 (3.1)	--	1.57 (2.9)	--	1.74 (3.2)
Chi-square (d.f)	23.3 (1)	25.6 (2)	55.5 (1)	65.7 (2)	40.6 (1)	48.9 (2)	39.0 (1)	49.5 (2)
Spells (escapes)	991 (564)		679 (501)		722 (454)		587 (406)	

Notes:

- a. Estimation is by Cox partial likelihood proportional hazard.
- b. In the estimation of the competing risk hazard to employment (columns 5-8), transitions to out of the labor force are recorded as censored.
- c. Age refers to the beginning of the spell.
- d. "Experienced individuals" refers to individuals who were working in June 1990.

Table 4: Determinants of Transitions from Non-Employment, Experienced Individuals

(Exponentiated Coefficients; t-statistics in parentheses)

Variable	All		Age<=50	Men<=50	Women<=50
	(1)	(2)	(3)	(4)	(5)
Sex (female=1)	0.48 (-8.5)	0.53 (-6.3)	0.54 (-5.7)	--	--
Log wage 1990	--	1.37 (2.1)	1.41 (2.1)	1.28 (0.9)	1.57 (2.2)
General schooling?	0.40 (-4.0)	0.48 (-3.1)	0.45 (-3.1)	0.37 (-2.3)	0.55 (-1.8)
University?	0.92 (-0.5)	0.86 (-0.9)	0.83 (-1.0)	0.81 (-0.8)	1.02 (0.1)
Apprenticeship?	0.70 (-3.5)	0.79 (-2.1)	0.79 (-2.1)	0.77 (-1.4)	0.75 (-1.9)
Age <=35 ?	1.01 (0.1)	1.12 (1.0)	1.12 (1.0)	1.05 (0.3)	1.15 (0.9)
Age 46-50 ?	1.12 (0.8)	1.12 (0.8)	1.14 (0.9)	1.26 (1.0)	1.04 (0.2)
Age 51-55 ?	0.30 (-7.6)	0.29 (-7.6)	--	--	--
Age >50 ?	0.04 (-10.7)	0.03 (-10.6)	--	--	--
Married?	--	1.38 (2.8)	1.38 (2.7)	1.09 (0.5)	1.44 (2.2)
Child 0-1 ?	--	0.61 (-2.6)	0.62 (-2.6)	1.58 (1.6)	0.40 (-3.9)
Child 2-6 ?	--	1.20 (1.6)	1.19 (1.5)	1.48 (2.3)	0.98 (-0.1)
Child 7-11 ?	--	0.97 (-0.3)	0.96 (-0.4)	1.41 (2.1)	0.79 (-1.6)
Living in west?	--	1.37 (1.1)	1.19 (0.5)	0.91 (-0.1)	1.11 (0.3)
Chi-square (d.f)	438 (12)	462 (18)	108 (16)	38 (15)	54 (15)
Spells (escapes)	991 (564)		679 (501)	274 (229)	405 (272)

Notes:

- a. Estimation is by Cox partial likelihood proportional hazard.
- b. All regressions also include year dummies.
- c. Variables are time-varying except for age, which refers to the beginning of the spell, sex, and the 1990 wage.

Table 5: Determinants of Transitions from Unemployment to Employment,
 Experienced Individuals
 (Exponentiated coefficients; t-statistics in parentheses)

Variable	All		Age <=50	Men <=50	Women <=50
	(1)	(2)	(3)	(4)	(5)
Sex (female=1)	0.52 (-6.7)	0.57 (-5.0)	0.58 (-4.5)	--	--
Log wage 1990	--	1.45 (2.0)	1.48 (2.0)	1.84 (1.7)	1.31 (1.1)
General schooling?	0.51 (-2.5)	0.63 (-1.6)	0.59 (-1.6)	0.88 (-0.2)	0.45 (-1.9)
University?	0.91 (-0.5)	0.86 (-0.8)	0.76 (-1.4)	0.77 (-0.9)	0.88 (-0.4)
Apprenticeship?	0.68 (-3.3)	0.78 (-2.0)	0.74 (-2.3)	0.81 (-1.1)	0.64 (-2.5)
Age <=35 ?	1.04 (0.3)	1.08 (0.6)	1.09 (0.7)	1.07 (0.3)	1.14 (0.7)
Age 46-50 ?	1.12 (0.7)	1.12 (0.7)	1.14 (0.8)	1.45 (1.6)	1.01 (0.0)
Age 51-55 ?	0.53 (-3.7)	0.52 (-3.7)	--	--	--
Age >55 ?	0.12 (-4.2)	0.12 (-4.1)	--	--	--
Married?	--	1.23 (1.7)	1.23 (1.6)	1.03 (0.1)	1.13 (0.7)
Child 0-1 ?	--	0.99 (-0.0)	1.03 (0.1)	1.09 (0.2)	0.98 (-0.0)
Child 2-6 ?	--	1.04 (0.3)	1.03 (0.2)	1.24 (1.1)	0.89 (-0.6)
Child 7-11 ?	--	1.06 (0.5)	1.06 (0.5)	1.44 (2.1)	0.81 (-1.3)
Living in west?	--	2.22 (2.5)	2.74 (3.0)	0.84 (-0.2)	3.87 (3.5)
Chi-square (d.f)	113 (12)	126 (18)	72 (16)	24 (15)	42 (15)
Spells (escapes)	722 (454)		587 (406)	236 (184)	351 (222)

Notes:

- Estimation is by Cox partial likelihood proportional hazard.
- All regressions also include year dummies.
- Variables are time-varying except for age, which refers to the beginning of the spell, sex, and the 1990 wage.

Table 6: Determinants of Transitions from Unemployment to Employment,
Broad Sample
(Exponentiated coefficients; t-statistics in parentheses)

Variable	All		Age <=50	Men <=50	Women <=50
	(1)	(2)	(3)	(4)	(5)
Sex (female=1)	0.51 (-9.3)	0.50 (-9.6)	0.50 (-8.9)	--	--
General schooling?	0.62 (-3.0)	0.66 (-2.6)	0.64 (-2.7)	0.88 (-0.5)	0.48 (-3.0)
University?	0.93 (-0.5)	0.94 (-0.4)	0.84 (-1.1)	0.80 (-1.1)	1.01 (0.1)
Apprenticeship?	0.74 (-3.4)	0.78 (-2.7)	0.74 (-3.2)	0.81 (-1.4)	0.68 (-3.1)
Age <=35 ?	1.04 (0.5)	1.06 (0.7)	1.05 (0.6)	0.98 (-0.1)	1.11 (0.8)
Age 46-50 ?	0.94 (-0.5)	0.94 (-0.5)	0.95 (-0.4)	1.11 (0.5)	0.81 (-1.1)
Age 51-55 ?	0.56 (-4.2)	0.55 (-4.2)	--	--	--
Age >55 ?	0.15 (-5.9)	0.15 (-5.9)	--	--	--
Married?	--	1.17 (1.7)	1.13 (1.3)	1.10 (0.6)	1.08 (0.6)
Child 0-1 ?	--	0.98 (-0.1)	1.00 (-0.0)	1.29 (1.0)	0.70 (-1.0)
Child 2-6 ?	--	1.01 (0.2)	1.02 (0.2)	1.10 (0.7)	0.92 (-0.6)
Child 7-11 ?	--	1.02 (0.2)	1.03 (0.3)	1.18 (1.2)	0.89 (-1.0)
Living in west?	--	2.34 (3.8)	2.67 (4.3)	1.92 (1.6)	3.03 (3.9)
Chi-square (d.f)	179 (12)	194 (17)	115 (15)	20 (14)	41 (14)
Spells (escapes)	1287 (824)		1078 (747)	451 (347)	627 (400)

Notes:

- Estimation is by Cox partial likelihood proportional hazard.
- All regressions also include year dummies.
- Variables are time-varying except for age, which refers to the beginning of the spell, sex, and the 1990 wage.

Appendix Table: Determinants of Transitions from Employment to Non-employment
(Exponentiated coefficients; t-statistics in Parentheses)

	All		Women		Men	
	All Exits	All Exits	Layoffs	Other Exits	Layoffs	Other Exits
Sex (female=1)	1.47 (5.8)	1.17 (2.1)	--	--	--	--
Log wage 1990	--	0.49 (-8.0)	0.57 (-3.0)	1.16 (0.5)	0.49 (-2.4)	1.51 (1.1)
General schooling?	--	--	1.73 (1.7)	1.13 (0.3)	1.47 (0.8)	5.95 (3.3)
University?	--	--	0.99 (-0.0)	0.93 (-0.2)	0.95 (-0.2)	0.69 (-1.1)
Apprenticeship?	--	--	1.46 (2.2)	1.14 (0.6)	0.81 (-1.0)	1.52 (1.7)
Age <=35 ?	--	--	0.89 (-0.7)	2.98 (3.5)	0.66 (-1.8)	2.20 (2.0)
Age 46-60 ?	--	--	0.71 (-1.5)	0.72 (-0.6)	1.30 (1.1)	0.60 (-0.9)
Age 51-55 ?	--	--	0.99 (-0.0)	0.69 (-0.6)	1.18 (0.6)	1.87 (1.3)
Age >55 ?	--	--	0.97 (-0.1)	0.70 (-0.5)	2.98 (3.3)	2.30 (1.6)
Married ?	--	--	1.27 (1.4)	0.80 (-1.2)	0.65 (-1.9)	0.64 (-1.5)
Child 0-1 ?	--	--	1.41 (0.6)	7.39 (5.5)	1.41 (1.1)	1.27 (0.5)
Child 2-6 ?	--	--	0.95 (-0.3)	1.03 (0.1)	1.02 (0.1)	0.70 (-1.0)
Child 7-11 ?	--	--	0.95 (-0.3)	0.64 (-1.7)	1.25 (1.1)	1.11 (0.3)
Tenure in years	--	--	0.99 (-1.1)	0.99 (-1.2)	0.98 (-1.9)	1.02 (1.7)
Eligible for early pension 1?	--	--	--	60.9 (6.2)	--	10.5 (2.8)
Eligible for early pension 2?	--	--	--	62.0 (6.7)	--	23.8 (7.8)
Spells (exits)	2122 (937)		1073 (250)	1073 (156)	1049 (175)	1049 (122)
Chi-square (d.f.)	34.5 (1)	93.9 (2)	37.9 (13)	322 (15)	28.5 (13)	275 (15)

Notes:

- Observations for 1990-1994, individuals who were working in June 1990.
- Estimation is by Cox partial likelihood proportional hazards.

- c. All variables refer to 1990 except the child and marital status variables, which are time-varying.
- d. For some uncensored spells, reason for the spell ending is missing.

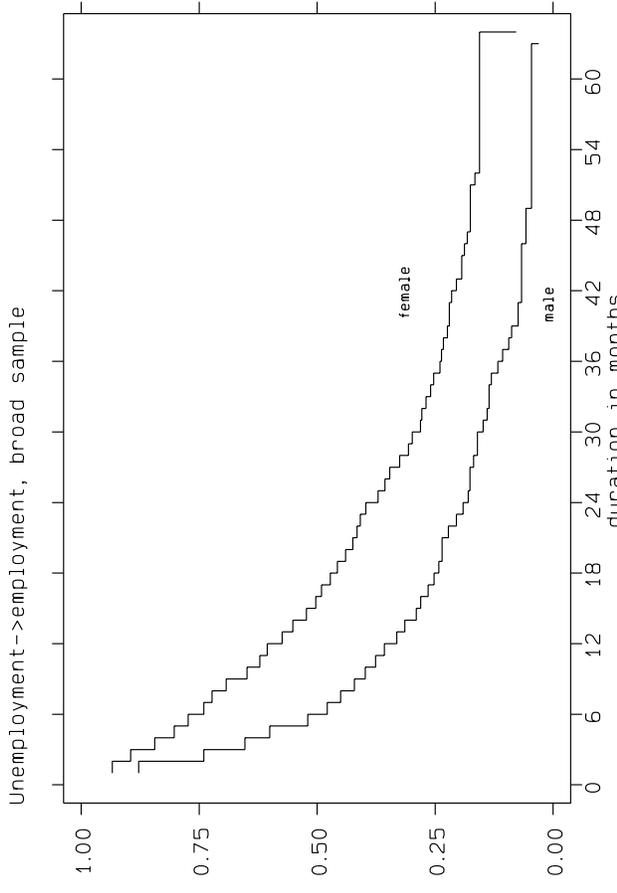
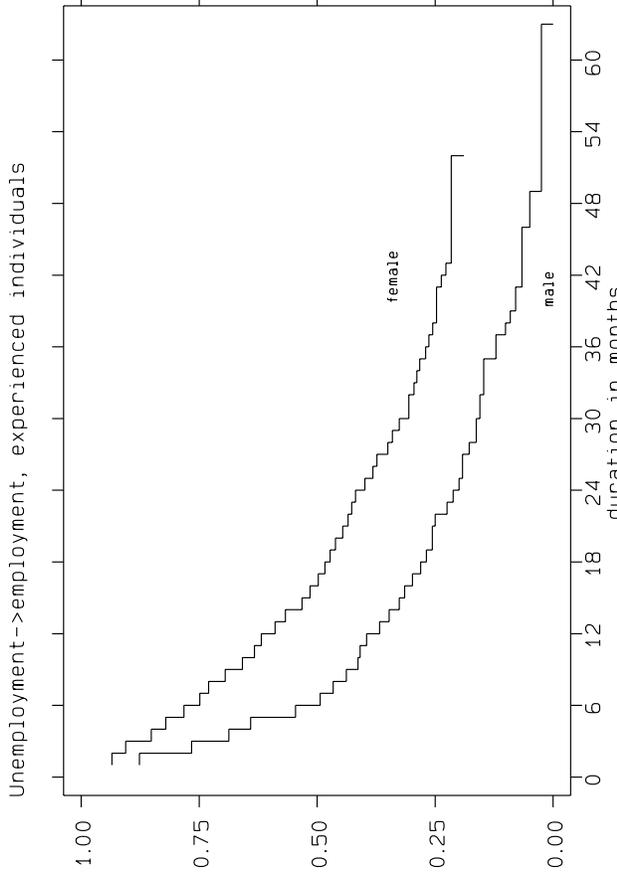
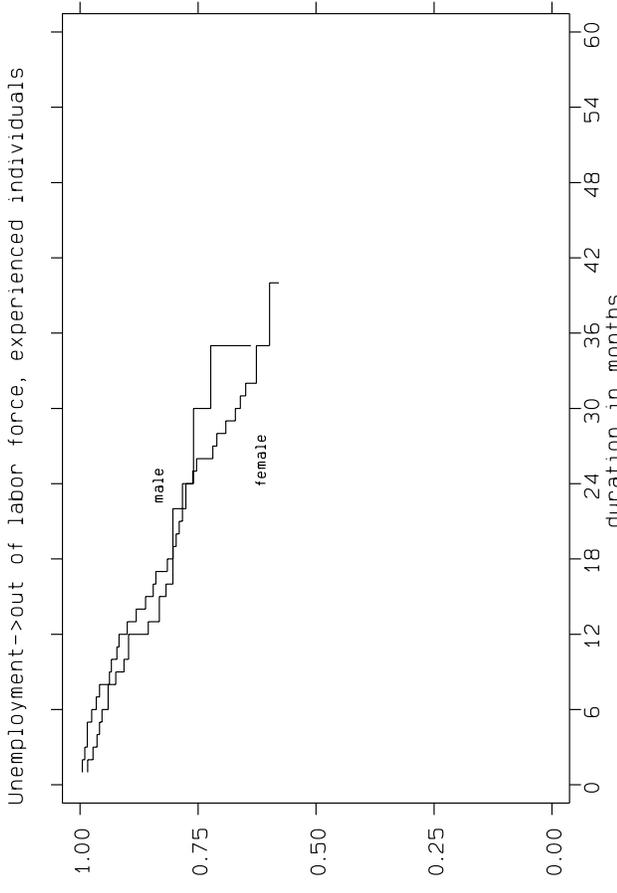
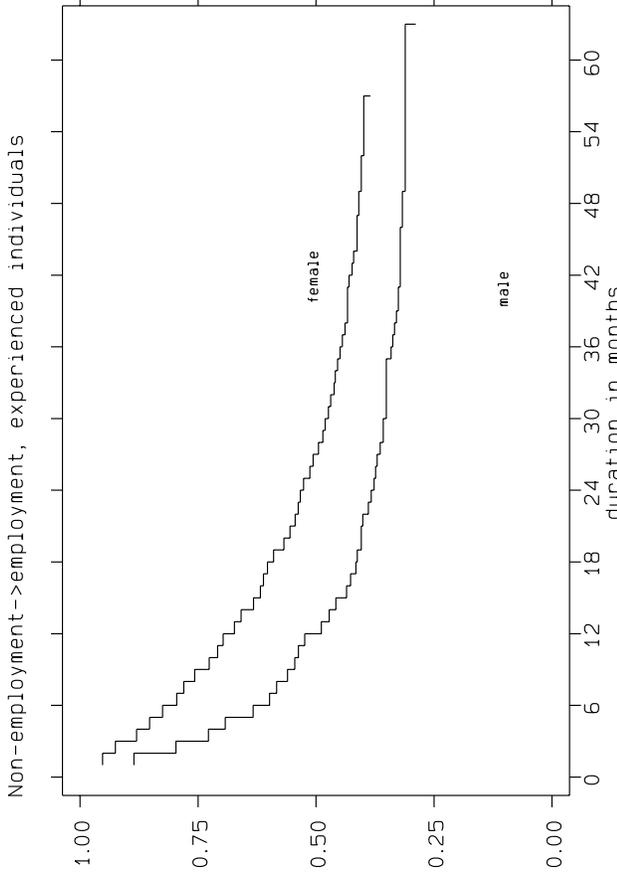


Figure 1: Kaplan-Meier Survival Rates

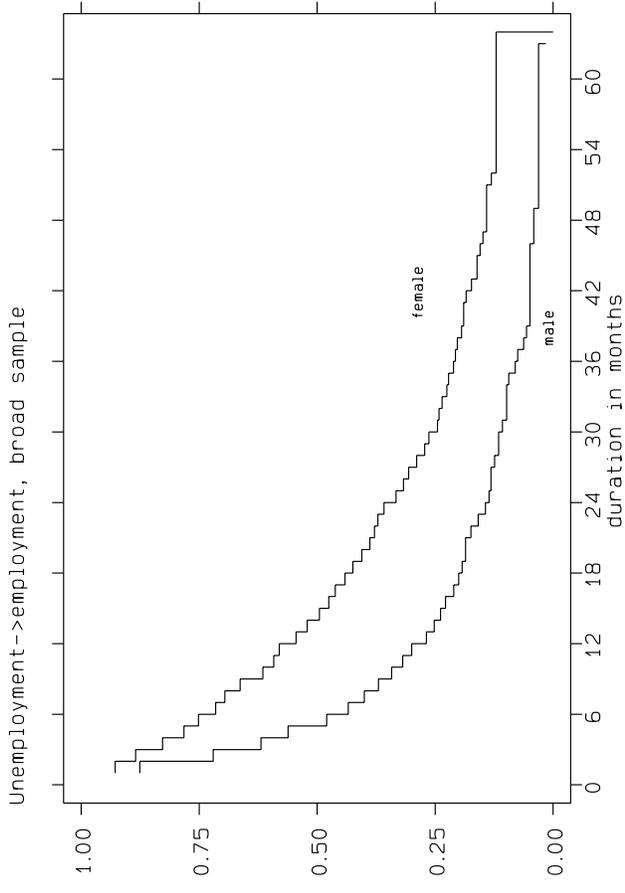
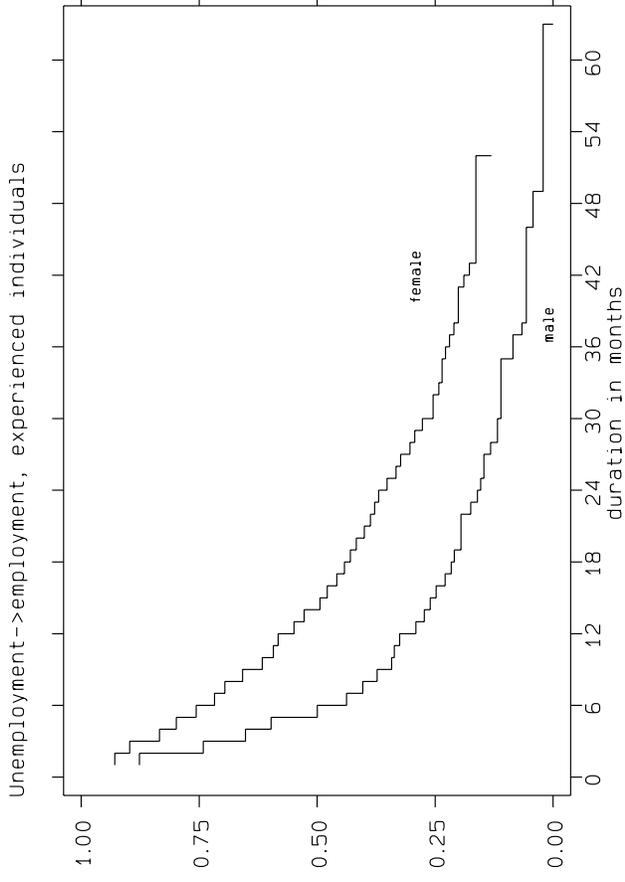
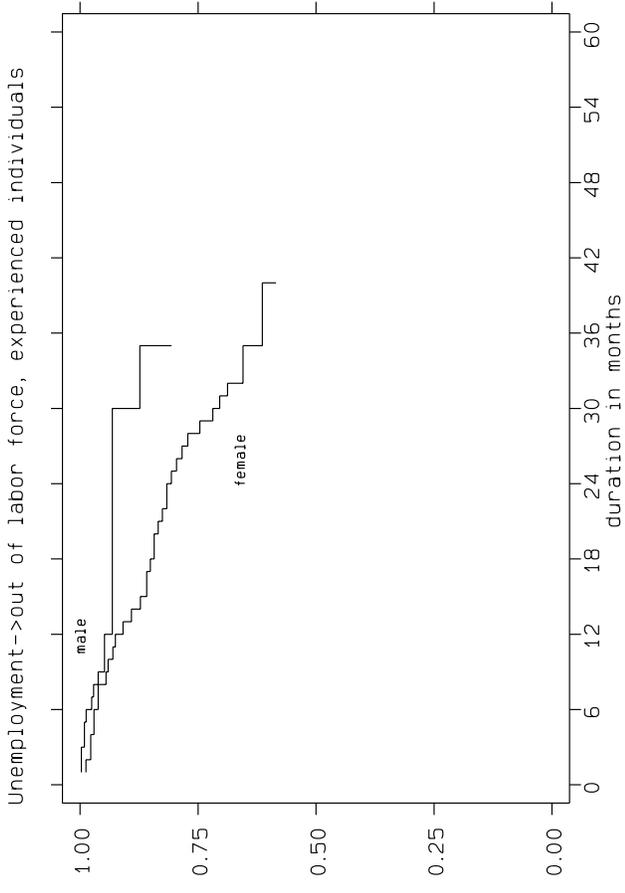
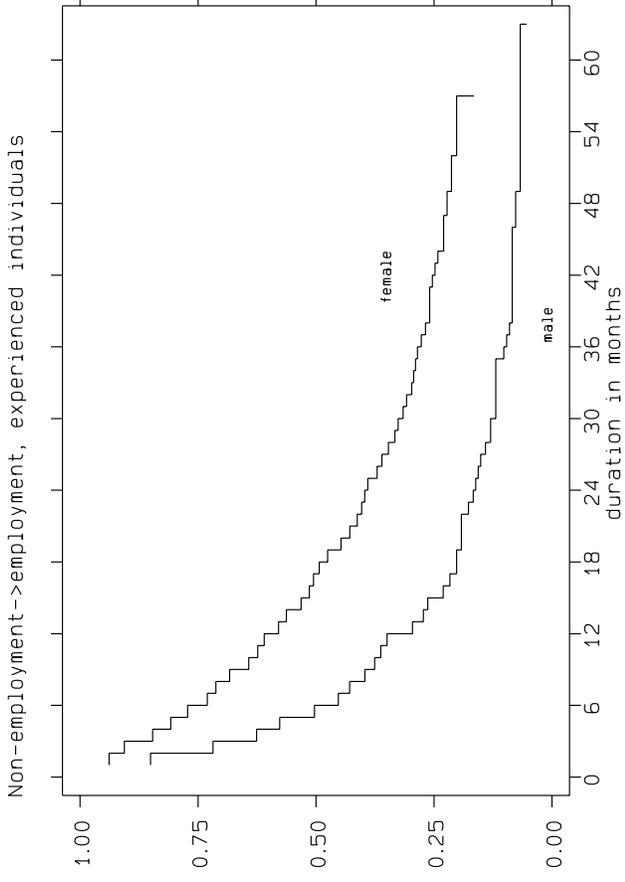


Figure 2: Survival Rates for Individuals 50 or Younger