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Poland on the Dole

Unemployment Benefits, Training, and Long–Term Unemployment during Transition

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by

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Abstract: We analyse the duration of unemployment spells in Poland using data from the Polish Labour Force Survey of August 1994. The effects on the duration of unemployment of important socio–economic and demographic characteristics are explored besides the impacts of the unemployment benefit system and training schemes. Finally, we investigate whether prior unemployment influences one's chances to find a job.
Entitlements to unemployment benefits prolong unemployment spell durations significantly. This effect is roughly of the same magnitude under the two benefit regimes that existed between 1990 and 1994, although the generosity of the unemployment benefit system has been reduced drastically in 1992. The results give credence to the view that the unlimited entitlement period of the old regime was not the main culprit for the widespread incidence of long–term unemployment.
Training programmes organised by labour offices should not be regarded as a panacea for the problems of the long–term unemployed. The results suggest that active labour market policies should perhaps be seen more as a tool for *social* rather than *economic* policy.
People with previous unemployment spells must expect to stay unemployed far longer than people who become unemployed for the first time. On the other hand, controlling for unobserved individual heterogeneity, we find that the probability of finding a job increases, especially for men, with the duration of unemployment.

Streszczenie i Wnioski: Analizujemy tutaj okres trwania bezrobocia w Polsce w oparciu o dwa parametryczne modele ekonometryczne, a mianowicie modele Weibulla z nieobserwowaną jednostkową heterogenicznością oraz bez niej. Analizowany jest wpływ istotnych społeczno-ekonomicznych i demograficznych cech jednostek, jak również oddziaływanie systemu zasiłków dla bezrobotnych i programów szkoleń dla bezrobotnych na okres trwania bezrobocia. W końcu, rozważamy czy fakt, iż było się wcześniej bezrobotnym wpływa na szansę znalezienia pracy.

Nasze wyniki badawcze są podobne do wyników innych badań dla grupy krajów Wyszechradzkich. Micklewright i Nagy (1995) twierdzą, że zmiany w zasiłkach dla bezrobotnych na Węgrzech, które miały podobny charakter do zmian w Polsce nie wpływają w sposób zasadniczy na okresy trwania bezrobocia. Również Ham, Svejnar i Terrell (1995) konkludują, że bodźce negatywne spowodowane prze system zasiłków dla bezrobotnych mają raczej mniejsze znaczenie w republice Czech i Słowacji. Jednakże ostatnie analizy Steinera (1996) dla Niemiec Zachodnich, oraz opracowanie Atkinsona i Micklewrighta (1991) wykazują, że oszacowane efekty nie są istotne ze względu na specyfikację modelu ekonometrycznego oraz charakter uprawnień do zasiłków. W związku z tym, nasze rezultaty należy traktować jako pierwszą próbę badawczą tych ważnych kwestii polityki publicznej. Dalsze badania

dotyczące Polski będą mogły wykorzystać dane pochodzące z Badania Modułowego na temat Polityki Rynku Pracy, które jest przeprowadzane właśnie w trakcie pisania tego tekstu. Nowy wzór kwestionariusza dostarczy więcej dokładnych danych o okresie trwania bezrobocia i pozwoli uzyskać nowe spojrzenie na efekty polityki rynku pracy w Polsce.

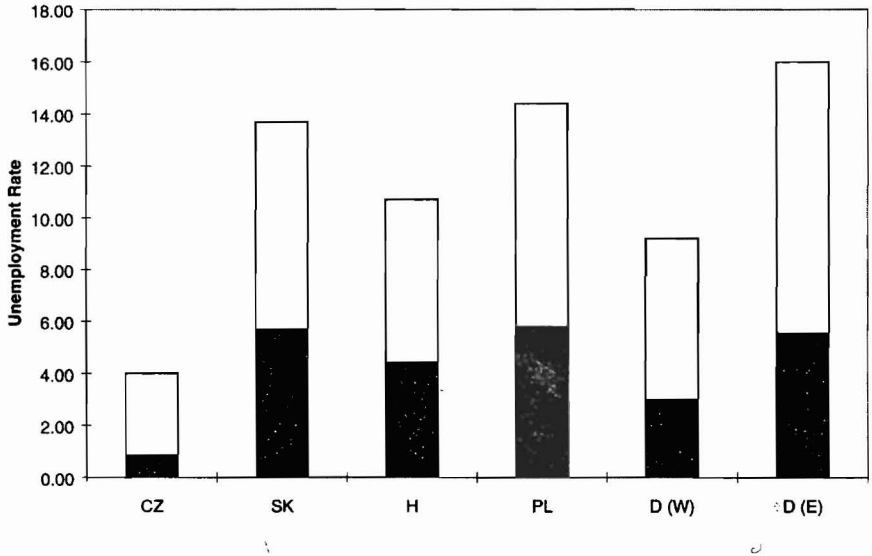
Rezultaty naszych badań nad długokresowym bezrobociem w Polsce są zgodne z naszymi rozważaniami a priori zaprezentowanymi w części 2. W szczególności, bezrobocie długokresowe wśród absolwentów ma głównie swoje źródło w osobliwościach ram instytucjonalnych rynku pracy w Polsce, na które składają się wysokie odprawy pieniężne przy zwolnieniach z pracy oraz efektywnie działająca opozycja dwóch głównych związków zawodowych: Solidarności i OPZZ-u. Te dwa czynniki ograniczają jednocześnie skalę zwolnień grupowych oraz rekrutację młodych ludzi. W rezultacie w Polsce mamy do czynienia ze stagnacyjnym charakterem zasobu bezrobocia, którego ofiarami są długokresowi bezrobotni. Obraz ten znajduje swoje potwierdzenie w wysokim odsetku osób, które wielokrotnie rejestrowały się jako bezrobotni; wydaje się, iż szanse na znalezienie stałej pracy są bardzo nikłe. Fakt bycia uprzednio bezrobotnym podwyższa oczekiwany okres pozostawania bezrobotnym co najmniej o rok. Wynika stąd, że koszty transformacji rozkładają się nierówno zarówno wśród siły roboczej, jak i wśród bezrobotnych. Skrócenie okresu pobierania zasiłku nie wydaje się by miało znaczący wpływ na bezrobocie długokresowe. Nie wydaje się także, by szkolenia wpływały pozytywnie na polepszenie sytuacji na rynku pracy. Opierając się na pracach Steinera i Krausa (1995) poświęconych Niemcom Wschodnim oraz opracowaniu Puhaniego i Steinera o Polsce, a także wyciągając wnioski z niniejszego opracowania można stwierdzić, że aktywna polityka rynku pracy, przynajmniej w jej obecnym kształcie winna być postrzegana jako instrument polityki społecznej, a mniej jako instrument polityki ekonomicznej. Choć można mieć pewne zastrzeżenia czy te analizy identyfikują zależności przyczynowe, to ogólne dowody zdają się popierać pogląd, że problem długookresowego bezrobocia nie może być rozwiązany ani za pośrednictwem aktywnej polityki rynku pracy ani poprzez redukcję zasiłków dla bezrobotnych.

1 Introduction

During the first phase of transition, the Polish unemployment rate increased from zero to 14% between 1990 and 1992. Although there are recent signs for a new downward trend in unemployment (Witkowski, 1995), the unemployment rate has remained high. At the same time, the share of the long–term unemployed (spells longer than 12 months) in the total unemployment stock has increased from 31% in 1992 to 40% in 1995 (Employment Observatory, 1995).

Figure 1 shows that in 1994, Poland had the highest unemployment rate amongst the Visegrád countries[1] with a long–term unemployment share comparatively high (40%) to the Czech Republic (21%) and both Western (33%) and Eastern Germany (35%), but similar to the Slovak Republic (42%) and Hungary (41%).

Figure 1: Long–Term Unemployment in Selected Economies 1994



Note: The height of the bars indicates the unemployment rate, whereas the share of the dark–shaded area in the total area of a bar indicates the share of long–term unemployment in total unemployment. Hence the top line of the dark–shaded area describes the long–term unemployment rate.

Source: For the Visegrád countries: Employment Observatory (1995); for Western [D(W)] and Eastern [D(E)] Germany: Statistisches Jahrbuch (1995).

[1] The Visegrád countries are the Czech Republic, Hungary, Poland, and Slovakia.

This shows that contrary to the hopes of many policy makers and economists, the transition is not characterised by a large unemployment pool with high in– and outflows, but mostly – as in the case of Poland – by a stagnating pool of mass unemployment. Hence the costs of transition in terms of unemployment are distributed fairly unequally across the population.

The aim of this paper is to provide an investigation into unemployment duration in Poland using data from both the Polish Labour Force Survey as well as its Supplement on the Evaluation of Labour Market Policies of August 1994. We will discuss a broad range of issues, such as the entitlement effects of unemployment benefits under the two benefit regimes Poland has seen after 1990, as well as the effect of training – both publicly and privately financed – on unemployment duration. In addition, we discuss the role of various socio–economic and demographic characteristics in long–term unemployment.

Section 2 gives a brief account of the development of the Polish labour market and important labour market institutions during the transition process. The sample and the variables of the subsequent empirical analysis are introduced in Section 3. In Section 4 we describe the unemployment duration data by way of non–parametric survival curves. Econometric issues are discussed in Section 5. We present the estimation results in Section 6 and conclude with Section 7.

2 Labour Market Developments and Policies 1990–1994

2.1 Economic Developments and Factors Influencing Unemployment

The beginning of the transition process in 1990 coincided with a negative aggregate demand shock through the collapse of CMEA[2] trade and stringent budgetary policies to stabilise the macroeconomy. High redundancy payments imposed on firms provided a disincentive to mass lay–offs and to recruiting younger people. In addition, there was an effective opposition to mass lay–offs by the two main trade unions, the anti–communist 'Solidarity' union and the formerly communist 'OPZZ', both powerful players in the public sector (Kwiatkowski, 1996a). The result has been a high youth unemployment rate and a stagnant unemployment pool characterised by moderate inflows and low outflows.

The redundancy regulations were not only providing safety for older workers, but also endangering the structural adjustment process. At the start of the transition process, the Polish economy was dominated by obsolescent staple industry and a comparatively large agricultural sector. One may have expected the market to reform industry, diminish agriculture, and establish services. Before the transition in

[2] Council for Mutual Economic Assistance, also abbreviated COMECON.

1989, the employment shares of the economic sectors were 28% in agriculture, 34.9% in industry, and 37.1% in services. At the end of 1994, those figures were 26.3%, 29.1%, and 44.6%, respectively (Witkowski, 1995). Compared to other Visegrád countries, structural change in Poland was slightly on the modest side.

As far as sectoral shifts have occurred, we would expect a comparatively high unemployment problem in the primary and secondary sectors, and a comparatively small one in services. However, the structural change does not only affect the sectoral composition, but also brings about structural shifts within sectors and fundamental changes in the production process at the micro (firm) level. People with more flexibility and more general skills will find it easier to adapt to the new conditions and keep their jobs. Hence, apart from the industrial sector a person used to work in, the level of education and type of occupation can have an important influence on unemployment. Age as well as family status can also be proxies for a certain kinds of flexibility.

Restrictions to mobility, *e.g.* through rigidities in the housing market (Kwiatkowski, Janusz and Steiner, 1995), are likely to prevent job opportunities to equalise between rural and urban regions, but also between geographical regions such as voivodships. The voivodship unemployment rate can be seen as an indicator for aggregate demand accounting for these regional differences in employment opportunities.

2.2 The Unemployment Benefit Regimes

Unemployment benefits were enacted as early as 1989. During the first phase of transition, the duration of benefit payments was open–ended and the benefit level was 70% of the most recent wage for the first 3 months (Kwiatkowski, 1996b). For the following 6 months, the replacement ratio declined towards 50% and to 40% thereafter. People who had never been employed received benefits equal to the minimum wage. The qualifying conditions were loose, in that one just had to register with the labour office as unemployed in order to draw benefits. Until September 1990, there were no previous work requirements, which were then introduced with a minimum requirement of 180 days in employment within the previous 12 months. However, this rule did not apply to school leavers, people discharged under group lay–offs, nor men (women) with job tenures of at least 20 (15) years. Officially, people could have been put off the register if they refused two adequate jobs or participation in active labour market policy schemes. However, labour offices seem to have been very generous in this respect (Góra and Lehmann, 1995). Part of the increase in Polish unemployment may therefore be ascribed to the increase in labour supply induced by this generous unemployment benefit system (Steiner and Kwiatkowski, 1995).

However, the Act on Employment and Unemployment of October 1991, which took effect on January 1st, 1992, changed the Polish system of unemployment benefits drastically. The entitlement period is now principally limited to 12 months and the benefit level is a flat rate of 36% of the average wage[3] in the economy during the previous quarter. A further important change has been the introduction of a 3–month waiting period for school leavers. As a result of the regime change, the share of benefit claimants in the total unemployment stock decreased from 79.0% in December 1991 to 52.3% in December 1992 (GUS, 1995) and has remained of that order since. Whether this regime change has had an impact on the duration of unemployment is an important question which will be addressed in our empirical analysis below.

2.3 Active Labour Market Policies

Although Poland has started on active labour market policy (ALMP) as early as 1990, the fight against unemployment was not a declared aim of government policy until 1993 (Kwiatkowski, 1996a). In 1994, the share of ALMP participants in total unemployment has been about 14%. Of those, 48% were in subsidised employment (intervention works), 27% in direct job creation (public works), 23% in labour market training and retraining (public training), and 2% received loans to start their own business. In 1991, the share of ALMP participants in total unemployment was 7.7%, of which 31% were in intervention works, 16% in training, and 53% received loans (Kwiatkowski, 1996a). Public works were not established before 1992, but have become fairly important since then, whereas the scale of loans has shrunk drastically.

With the exception of perhaps public works, the main aim of these ALMP measures is to reintegrate the long–term unemployed into the labour market. The empirical evidence on the success of ALMPs in this regard is still developing. Kwiatkowski (1996a) reports that in 1994, the fraction of participants who found a job after completing their ALMP scheme was 40.3% after training, 33.1% after intervention works and 2.9% after public works. Puhani and Steiner (1996) find no positive effects of neither training nor works programmes on the incidence of unemployment in an econometric model of individual labour force behaviour. This study will amongst other things explore the effect of training on the duration of unemployment using data from the Polish Labour Force Survey. Unfortunately, there are not enough observations in the survey to look into the effects of intervention works, public works, or loans on the duration of unemployment.

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[3] There are exceptions to this rate. School leavers under 18 years of age receive 12% of the average wage, those above 18 receive 28%. In 'crisis areas' designated by the government, the rate is 52% if job loss occurred through a group lay–off.

3 Data and Variables

Our data base combines information from the first ten waves (May 1992 to August 1994) of the quarterly Polish Labour Force Survey (PLFS) with data from its Supplement on the Evaluation of Labour Market Policies of August 1994. The PLFS conducted by the Central Statistical Office (GUS) of Poland is a representative sample of the Polish population aged 15 and above. During the first four waves (until February 1993) the PLFS had been a pure panel, since then it has been a rotating panel. The duration data on unemployment spells – given in weeks – comes from retrospective information in the Supplementary Survey, where interviewees state for how long they were looking for a job the last time they were unemployed. We generally define unemployed as looking for a job off the job, *i.e.* those people who have been or are looking for a job whilst having a permanent full–time occupation are not included in the sample.[4] According to the ILO definition of unemployment, we do not count people as unemployed who are looking for a job although they are not ready to take one up in the reference week or the following one.

Table 1 contains summary statistics of the sample consisting of 4,353 men and 4,441 women who are or have been unemployed. The appendix has some detail on how we get down to that number. Section 2 has already discussed the potential significance of most of the listed variables for the duration of unemployment spells. Here we concentrate on the discussion of the definition of the variables. It is worth noting that only 41% of the male sample and only 37% of the female sample have completed spells. The extremely low number of completed spells means that the information content of the sample with respect to the distribution of the completed duration is comparatively low, as the censored spells (spells still in progress) contribute less information to the likelihood function than the completed spells (see Section 4 below). One reason for this low number of completed spells is that the share of people with previous unemployment spells is fairly high (about 25% for men and 19% for women) but we have no information on their previously completed spell lengths. Hence, we have undersampled the number of completed spell lengths considerably. Also, the observation period is relatively short. We believe that this lack of information can lead to a considerable bias of estimated spell lengths and should therefore be kept in mind throughout the remainder of this analysis.

[4] In Poland, unemployed people are allowed to work whilst drawing benefits as long as they do not earn more than half the minimum wage (Kwiatkowski, 1996b).

Table 1: Sample Means

Variable	Mean	
	Men	Women
age between		
18 and 25	0.375	0.372
26 and 35	0.278	0.298
36 and 45	0.252	0.255
46 and 55	0.095	0.075
single	0.424	0.315
single & new regime	0.377	0.274
single & children	0.005	0.041
children	0.403	0.530
no information on children	0.461	0.330
disabled	0.064	0.042
education		
higher	0.045	0.041
post–secondary	0.014	0.042
secondary vocational	0.184	0.266
secondary general	0.028	0.116
basic vocational	0.507	0.342
primary or less	0.223	0.193
occupation		
manager	0.030	0.020
professional	0.025	0.057
technician	0.082	0.152
white collar	0.028	0.115
personal services	0.064	0.234
farmer	0.112	0.086
industrial worker	0.390	0.158
simple blue–collar	0.130	0.031
other simple jobs	0.139	0.148

industry		
agriculture, forestry, fishing	0.148	0.094
mining, manufacturing	0.251	0.244
electricity, gas, water	0.014	0.005
construction	0.176	0.022
trade, repairs	0.102	0.212
catering	0.010	0.039
transport, communication	0.048	0.021
financial intermediation	0.008	0.018
real estates, renting	0.016	0.015
public administration	0.059	0.035
education	0.015	0.054
health, social work	0.014	0.065
other services, none, not known	0.173	0.214
place of residence		
100,000 inhabitants or more	0.222	0.255
20,000 to 99,999	0.205	0.215
19,999 or less	0.152	0.148
countryside	0.421	0.382
voivodship unemployment rate	19.858	19.264
previously unemployed	0.250	0.188
registered but no unempl. ben.	0.170	0.192
new regime	0.850	0.804
unemployment benefit	0.598	0.593
new regime & unempl. benefit	0.517	0.482
unempl. benefit until pension	0.105	0.101
school leaver	0.129	0.155
public training	0.015	0.021
self–financed training	0.027	0.025
employer– financed training	0.028	0.016
1st quarter	0.211	0.214
2nd quarter	0.319	0.299
3rd quarter	0.287	0.306
4th quarter	0.184	0.181
# observations	4353	4441

We define a *reference person* who usually (there are exceptions) belongs to the group with the highest sample share. Our reference person is married, aged between 36 and 45, has no children, no disabilities, his or her highest educational achievement is basic vocational, and he or she works as an industrial worker in the manufacturing and mining industry. The reference person lives in the countryside and has not been unemployed previous to the recorded spell which started in the quarter between April and June before 1992. He or she has been working before, *i.e.* is not a school leaver, and has not received unemployment benefits or participated in any training programme.

As far as possible, the variables have been adjusted to their values at the beginning of the spell. However, this was not possible for the *education*, *occupation* and *industry* variables.[5] As to occupation, we have information on a person's former occupation as well as his or her occupational degree, the latter being used in case the person has never been in employment.

We do not have labour force survey data on *voivodship unemployment rates* before May 1992 and so use the data of May 1992 for spells beginning earlier than that date. Alternatives would have been to use registered data before May 1992 or use registered data altogether. However, as mentioned in Subsection 2.2 above, the generosity of the unemployment benefit regime before 1992 (anybody could register and draw benefits) seems to make the voivodship unemployment rate based on registered data an unreliable indicator of local demand conditions. Labour force survey data would seem much more appropriate. To some extent we may hope that the local demand conditions of May 1992 are correlated with the conditions before that date. Although there was a significant rise in unemployment from 0% in 1990 to 12% (labour force survey data) at the beginning of 1992, the change in the demand for new (*e.g.* unemployed) labour has probably dropped much faster with the sudden decline in aggregate demand in 1990. It was only due to institutional reasons that the unemployment rate followed the developments in output with a lag (*cf.* Subsection 2.1). For this reasons, we believe that approximating local demand conditions before May 1992 by the regional unemployment rates as given by the labour force survey before 1992 is the best practical solution to this missing data problem. Also note that only 17% of the sample observations became unemployed in 1991 or before.

We include a dummy taking on the value of one for people who are or were *registered* with a labour office as looking for a job, but do or did not receive unemployment benefits. This variable may give some indication of the usefulness of labour offices in the matching process. As the new unemployment benefit regime of Janu-

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[5] It was possible, though, for school leavers, for whom we set industry equal to *other services, none, unknown*.

ary 1992 meant a drastic change in the incentive structure for unemployed people, we control for the time of entry into the spell by the *new regime* dummy.[6] This dummy is interacted with the *unemployment benefit* variable which states whether the person has received or receives unemployment benefits. This interaction term can be interpreted as the effect of the regime change (compare Hunt, 1995, and Steiner, 1996).

We do not know directly from the surveys whether a person is entitled to *benefits until retirement.* However, for those who were employed in August 1994 we know the years of work experience and infer that benefits are paid until retirement if years of work experience at the beginning of the spell exceeded 30 years for men or 25 years for women. For people currently not in employment, but who used to have a job, we guesstimate work experience to be age minus age after education. In case of people with a previous unemployment spell, we subtract one year of work experience. We subtract two further years for men due to military service, but only one year if someone has a university degree, as university graduates traditionally serve for a shorter period in Poland. For women, we also subtract one year for child rearing in case they have children under the age of 18. Years of schooling also have to be inferred from the educational categories in the survey. We have made the following assumptions on age after having completed education (assumed age in brackets): higher (24), post–secondary (21), secondary general (20), secondary vocational (19), basic vocational (17), primary (15), and less than primary (14). *School leavers* are defined as people with no work experience under 30 years of age (inclusively).

The *training* variables have the problem that we cannot identify when exactly a person has participated in a training scheme. In order to get somewhere near to identifying a causal effect of a training programme on the duration of unemployment, we should at least make sure that training did not occur after the end of the unemployment spell. Unfortunately, though, we only know whether someone is currently on the course (these people have *training* set equal to zero), has undergone training within the period August 1993 till August 1994 (these people have *training* set equal to zero if their unemployment spell ended before that period), or has finished a course before August 1993 (these people all have *training* set equal to one).

The *seasonal dummies* state the quarter a person has become unemployed.

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[6] For those benefit recipients who were unemployed at the time of the regime change, the entitlement period was set to another 12 months irrespective of their prior unemployment duration.

4 Descriptive Analysis

For three groups which are of key interest to us, people previously unemployed, unemployment benefit recipients and public training participants, we plot Kaplan–Meier estimates (Kaplan and Meier, 1958) of the survival curves[7] in Figures 2 to 4. The graphs carry the following interpretation: take a point on the vertical axis, which states the survival probability, say 0.50. Find the corresponding duration value on the horizontal axis, which is about 12 months for males (16 months for females) who have not been previously unemployed. Then the Kaplan–Meier estimate says that fifty percent of the group under consideration stay unemployed for at least 12 months. Or, after 12 months, fifty percent of the group were still without a job ('survived in unemployment'). Hence, 12 months is the median spell duration for this group. From these estimates, about 50% of men and 55% of women who experience their first unemployment spell are hit by long–term unemployment defined by a duration of at least 12 months. It is shown that for both men and women with previous unemployment spells, the estimated median duration is literally out of bounds. It is at least as long as 54 months. Although the estimates are likely to be somewhat inaccurate due to the small number of completed spells, it seems that there is some evidence for occurrence dependence in that the length of the current unemployment spell is strongly correlated with the existence of previous unemployment spells.

Figure 3 plots survival curves for unemployment benefit recipients and those without benefits in both benefit regimes. Let us look at men first. Although the survival curve for benefit recipients is significantly different from the one for those without benefits, the curves do not change significantly between the two regimes.[8] The estimated median duration for non–recipients lies between 9 and 12 months. Unemployment benefit recipients have a median of around 18 months. We may detect an effect of unemployment benefits in general here (although this might be a selection effect), but no effect of the regime change. For women, the difference between the curves of non–recipients is significant between the two regimes, but this is not the case for recipients. Hence, the curves of recipients and non–recipients have moved significantly closer together thus diminishing the entitlement effect according to these estimates (the medians are 11 and 16 months for non–recipients and 24 months for recipients). However, the entitlement effect is still present, *i.e.* the curves for recipients and non–recipients are still significantly different under the new regime.

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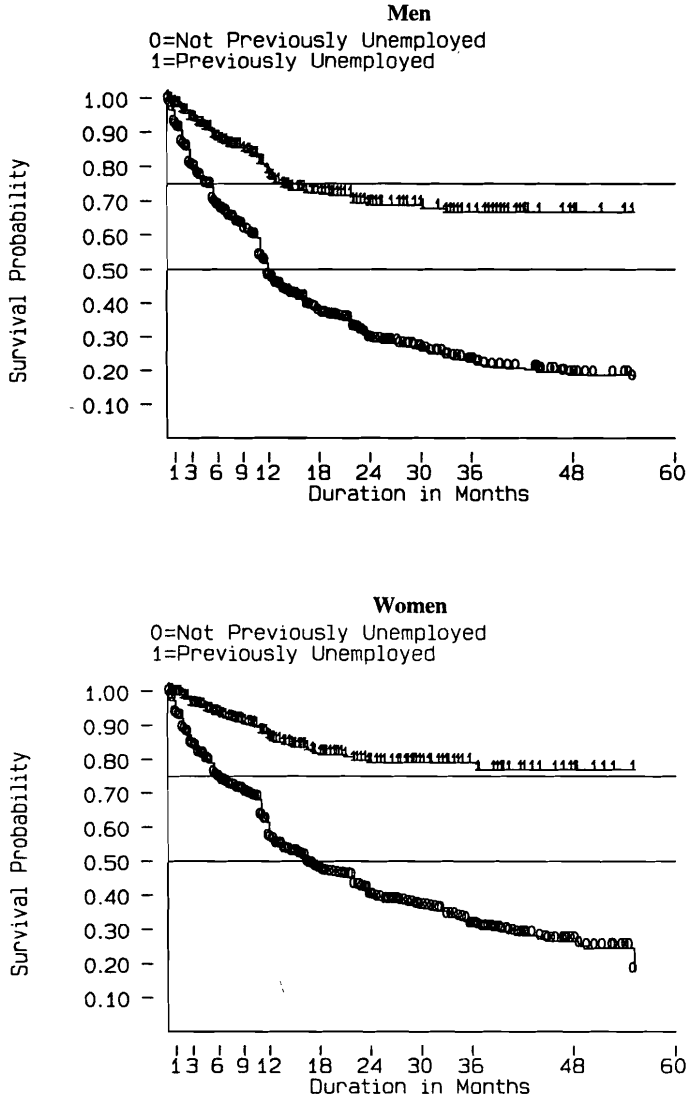
[7] The term 'survival' stems from the biometric literature where in clinical experiments the survival of a patient until death is analysed.

[8] We use the log–rank test to test for the equality of two survival curves (StataCorp., 1995; Kalbfleisch and Prentice, 1980). The significance level is 5%.

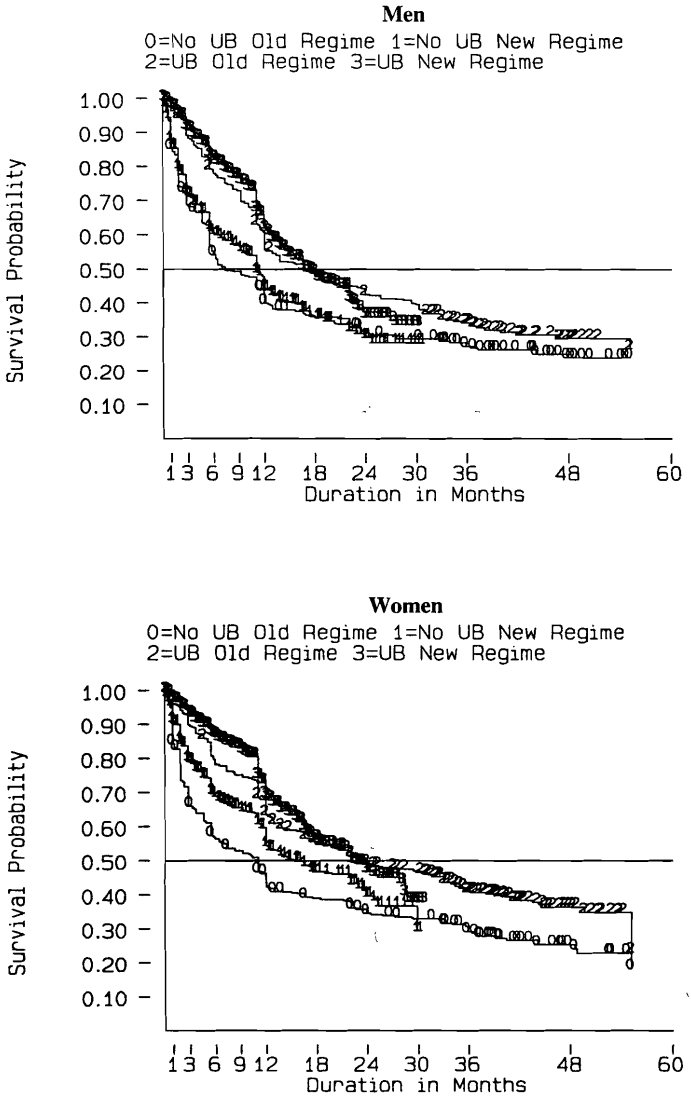
Figure 2: Kaplan–Meier Survival Estimates by Previous Unemployment Spell



Note: The numbers along the curves in this and the following charts indicate censored observations.

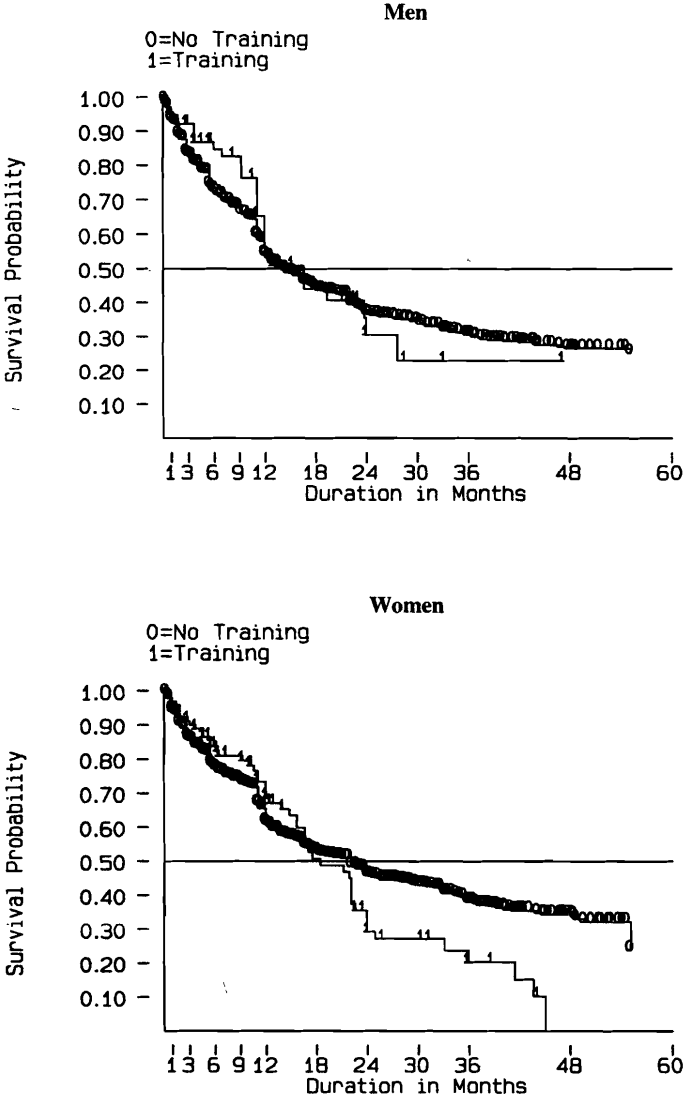
Source: Polish Labour Force Survey; own calculations.

Figure 3: Kaplan–Meier Survival Estimates by Unemployment Benefit and Regime



Source: Polish Labour Force Survey; own calculations.

Figure 4: Kaplan–Meier Survival Estimates by Public Training



Source: Polish Labour Force Survey; own calculations.

Finally, Figure 4 shows Kaplan–Meier estimates for people having participated in public training and those who have not. The difference between the survival curves of these two groups is insignificant for both sexes. However, the estimates are based on just 64 and 95 observations for men and women, respectively.

In order to make more specific statements of the impact of the variables just introduced on the duration of unemployment, we have to control for other important characteristics while testing the significance of the variables of interest. This can be done using the econometric models introduced in the following section.

5 Econometric Issues

5.1 Economic Theory

An important issue in the study of long–term unemployment is whether the probability to leave unemployment is related to the prior duration of the spell. A positive relationship between this probability and the duration of the spell is called positive duration dependence, which is one form of state dependence as described in Heckman and Borjas (1980).[9] There are explanations for both positive and negative duration dependence in economic theory.

In the neoclassical labour–leisure choice model, people can take up a job any time at a fixed, known, and constantly offered wage. Job search theoretic approaches, by contrast, in allowing for uncertainty concerning job offers, model people's labour market histories as stochastic processes (see Lippman and McCall, 1976, and Mortensen, 1986, for surveys). Just as in the standard neoclassical model, the optimal job search strategy can be shown to exhibit the reservation wage property[10] under certain assumptions. However, whereas the reservation wage in the neoclassical model is only a function of preferences and therefore fixed given a set of preferences, job search theory allows the reservation wage to change over the search period.[11] This is one way to think of duration dependence. The theory can hence give credence to the empirical observation of declining reservation wages over the unemployment spell (Kasper, 1967; Katz, 1974) by assumptions such as finite time horizons, over–

[9] Other forms of state dependence are occurrence dependence (the probability to find employment is related to the existence of previous spells), Markovian state and lagged duration dependence (see Heckman and Borjas, 1980 for details). The latter two will not be analysed in this paper.

[10] This means that each unemployed person has a reservation wage and will take up employment if and only if he or she is offered a job with a wage higher than the reservation wage.

[11] In fact, the neoclassical model is more flexible than that: the reservation wage in this model also depends on non–labour income. Given non–pathological preferences, if non–labour income runs out over the unemployment period the reservation wage will fall in the neoclassical model. Nevertheless, the job search model contains more realism through the explicit treatment of uncertainty.

optimism concerning employment opportunities (Lippman and McCall, 1976), risk aversion or credit constraints (Classen, 1979; Mortensen, 1986). *Ceteris paribus*, a reservation wage declining with the duration of the spell will increase the probability to leave unemployment. This is the case of positive duration dependence. However, there are also economic explanations for negative duration dependence. The depreciation of human capital may accelerate if labour is idle and long–term unemployment may signal low productivity to employers. As a consequence, offered wages as well as the job offer arrival rate may decline so that the probability to find employment falls.

5.2 Econometric Specifications

A reduced form specification of the job search model makes the probability to leave unemployment during time interval $[t, t+dt[$ conditional on having been unemployed up to time t the central focus of model building.

The limit ($dt \to 0$) of this conditional probability is called the hazard[12] rate and can be written more formally as follows:

$$h(t) = \lim_{dt \to 0} \frac{Pr(t \le T < t+dt | T \ge t)}{dt}.$$

The hazard rate depends both on the supply side (captured in the reservation wage) and the demand side (captured in the offered wage), which would be the focus of model building in a structural model. Since we do not have data on reservation wages over the unemployment spell nor information on job offers, we are not able to identify a structural model without imposing rather arbitrary assumptions.

The hazard rate for each point in time t, $h(t)$, gives a complete description of the distribution of unemployment duration with density function $f(t)$ and distribution function $F(t)$. The survival function is defined as $1-F(t)$.

The relationships between the hazard $h(t)$, the survival function $1-F(t)$, and the density function $f(t)$ are given by (see, *e.g.*, Lancaster, 1990):

[12] The term 'hazard' again stems from the biometric literature (*cf.* footnote 7) where the probability to die is modelled.

$$h(t) = \frac{f(t)}{1 - F(t)},$$

$$1 - F(t) = e^{-\int_0^t h(\tau)d\tau},$$

$$f(t) = h(t)e^{-\int_0^t h(\tau)d\tau}.$$

In the following, we will employ two Weibull specifications. These models allow the hazard to vary only monotonically over time, which has made semi–parametric estimation techniques increasingly popular in the recent literature. These models generally do not specify the baseline hazard parametrically (if they specify it at all). The advantage is that spikes in the baseline hazard, which can plausibly occur, for example when benefits run out, can be detected this way. However, the Weibull specification will still give a very good and intuitive description of the type of state dependency as long as there are not too many striking spikes. For this reason, we have also estimated the baseline hazard non–parametrically, and found that a Weibull model is appropriate. A further strength of the Weibull model is that unobserved heterogeneity can be implemented and estimated straightforwardly in the econometric package *LIMDEP 7.0* (Greene, 1995).

5.2.1 Model 1: The Standard Weibull Model

We assume t to have a Weibull distribution with hazard rate

$$h_1(t) = \alpha\lambda^{\alpha} t^{\alpha-1}.$$

The hazard is time–dependent unless $\alpha = 1$ in which case the duration is exponentially distributed. The Weibull model thus allows for negative duration dependence (*e.g.* due to depreciating human capital; $\alpha < 1$) or positive duration dependence (*e.g.* through falling reservation wages; $\alpha > 1$).

Time–invariant regressors may enter the Weibull model in the form

$$\lambda = e^{-\beta' x} \qquad (5.1)$$

This implies the restriction that the variables do not influence the baseline hazard $\alpha t^{\alpha-1}$, which is assumed to be equal for all individuals and determines the type of duration dependence for everyone in the same way. The variables only determine the hazard relative to the one of the other individuals. To give an example, if we build the ratio between the hazards of two people who differ only in the value of the k^{th} dummy variable (1 for the person in the numerator, 0 for the person in the denominator), this hazard ratio (HR) will be equal to $e^{-\beta_k}$, where β_k is the coefficient

of dummy variable k. A HR of 2 means that the hazard for people in the corresponding category is twice as high as the one of those in the base category. As this proportion is constant over process time, the standard Weibull model belongs to the class of proportional hazard models.

5.2.2 Model 2: The Weibull Model with Unobserved Heterogeneity

One problem of model 1 is that it assumes that the variables in (5.1) completely account for the heterogeneity of the hazard rates between individuals. In general, though, we will not be able to observe all factors accounting for variations in the hazard rates between people, so we have an omitted variable problem. Failure to account for unobserved heterogeneity can lead to a negative bias on duration dependence (see Kiefer, 1988, for an illustration).

A standard practical method to alleviate the problem is to introduce unobserved heterogeneity by way of a multiplicative error term v in the form

$$h_2(t|\boldsymbol{x}, v) = \alpha t^{\alpha-1} e^{(-\beta'\boldsymbol{x})^{\alpha}} \cdot v.$$

For mathematical convenience v is often specified to be Gamma distributed with mean 1 and $\sigma_v^2 = \theta$ (see Steiner, 1990, for an application)[13].

Taking the expectation over v, the average hazard rate for individuals with observed characteristics $\boldsymbol{x}_i$ can be written (neglecting the index i):

$$h_2(t|\boldsymbol{x}) = \alpha t^{\alpha-1} e^{(-\beta'\boldsymbol{x})^{\alpha}} (1 + \theta t^{\alpha} e^{(\beta'\boldsymbol{x})^{\alpha}})^{-1}$$

Table 2 summarises the hazard, density, and distribution functions for models 1 and 2 as well as the formulae for the mean durations.

In both models 1 and 2, the contribution of a completed spell to the likelihood function is the density of the duration distribution, whereas the contribution of a censored spell is the survival function $1 - F(t)$ of the duration. Defining a censoring indicator c_i for person i ($c_i = 1$ means person i 's spell is completed), we can under the assumption of independent drawings and random censoring generally write the likelihood function as:

$$l(\beta, \alpha, \theta | \boldsymbol{t}, \boldsymbol{X}) = \prod_{i=1}^{N} f\left(t_i | \boldsymbol{x}_i, \beta, \alpha, \theta\right)^{c_i} \cdot \left(1 - F\left(t_i | \boldsymbol{x}_i, \beta, \alpha, \theta\right)\right)^{(1-c_i)} \qquad (5.2)$$

---

[13] This Gamma density function can be written $f(v) = \frac{\varphi^{\varphi}}{\Gamma(\varphi)} v^{\varphi-1} e^{-\varphi v}$; with parameter $\theta = 1/\varphi$.

The specific likelihood functions for models 1 and 2 are obtained by substituting the appropriate terms from Table 2 into the likelihood (5.2). The restriction implied by model 1 on model 2 is $\theta = 0$ and can be tested for.

Table 2: Formulae for Models 1 and 2

Model 1	Model 2
$h_1(t\|x,\beta,\alpha) = \alpha t^{\alpha-1} e^{(-\beta' x)^\alpha}$	$h_2(t\|x,\beta,\alpha,\theta) = \alpha t^{\alpha-1} e^{(-\beta' x)^\alpha} (1+\theta t^\alpha e^{(-\beta' x)^\alpha})^{-1}$
$f_1(t\|x,\beta,\alpha) = \alpha t^{\alpha-1} e^{(-\beta' x)^\alpha} e^{-\int_0^t h_1(\tau\|x)d\tau}$	$f_2(t\|x,\beta,\alpha,\theta) = \alpha t^{\alpha-1} e^{(-\beta' x)^\alpha} (1+\theta t^\alpha e^{(-\beta' x)^\alpha})^{-\sigma^{-2}-1}$
$1-F_1(t\|x,\beta,\alpha) = e^{-\int_0^t h_1(\tau\|x)d\tau}$	$1-F_2(t\|x,\beta,\alpha,\theta) = (1+\theta t^\alpha e^{(-\beta' x)^\alpha})^{-\sigma^{-2}}$
$Median_1(T) = (-ln(0.5))^{1/\alpha} e^{(\beta' x)}$	$Median_2(T) = \frac{\left(e^{-\theta ln(0.5)}-1\right)^{1/\alpha}}{\theta^{1/\alpha}} e^{(\beta' x)}$

Sources: Lancaster (1990) and own calculations.

The likelihood function (5.2) will be maximised with respect to β, α, and θ using the procedures programmed in *LIMDEP 7.0*, which estimate a transformation of (5.2) (for details see Greene, 1995).

5.3 Causality

When evaluating training programmes, we face the problem of endogeneity bias when trying to identify causal effects from programme participation dummies. The problem is that individuals in the training programmes may well be a self–selected group, selected, for the example, on their prospects of leaving unemployment fast. If those people whose unemployment spells labour offices expect to be above average are more likely to receive a placement on a public training scheme, this *association* between programme participation and duration in unemployment will be caught up in the estimated coefficient on the programme participation dummy. Policy makers, however, will be interested in the *causal effect* of the programme. More formally, the causal effect can be written as follows:

$$E(t_i|p_i = 1) - \underbrace{E(t_i|p_i = 0)}_{counterfactual},$$

where t_i is the duration in unemployment and $p_i = 1$ indicates that person i has participated in the training programme, and zero otherwise.

That is to say, we are looking for the difference in duration between those who have been on the programme and the counterfactual duration of unemployment of those same participants had they not participated in the programme. Yet as the latter value is by its counterfactual nature not observable, we can never be certain to estimate a causal effect, unless we have experimental data. Including a dummy variable for participation in training may select a control group distinctly different from the treatment group as far as expected unemployment duration is concerned. This difference will in addition to the causal effect be mingled into the coefficient on the training dummy thus subjecting the estimate to selection bias (Heckman and Hotz, 1991).

A potential remedy to this causality problem when lacking experimental data would be to instrument the programme participation dummies out, *e.g.* through replacing them by the predicted participation probability from an auxiliary regression. However, our data does not allow us to estimate this probability with any precision (see also Puhani and Steiner, 1996), and so this method cannot be sensibly applied here.

Of course, the causality problem would disappear if labour offices and private agents assign people randomly into training courses, but the results of Puhani and Steiner (1996) point to this generally being not the case. As a consequence, interpreting the estimated effects of programme participation in the following section as causal effects should be taken with a grain of salt. In what follows, we discuss the effects of the variables listed in Section 3 on unemployment duration by applying the models just described.

6 Estimation Results

Table 3 reports estimated hazard ratios (see previous section) together with their t–values. Shaded coefficients are significant at least at the 5% level. Further, in Table 4 we present an interpretation of the hazard ratios (HR) in terms of the median duration of unemployment. The numbers state the effect of being in the corresponding category on the median duration evaluated for the reference person.[14] Given the small differences in the estimated median durations for the reference person between models 1 and 2, the estimated effects of the variables differ only marginally between the models. The median unemployment duration for the reference person is, especially in the case of women, far smaller than the median for the average person. There are only small differences between the models but large ones between

[14] As the model is non–linear, these effects depend on the initial values of the x–variables and would thus be different if evaluated for the average person.

the sexes. The estimated median durations for the average person are very high (about 18 months for men and 23 months for women in model 2).

We will proceed as follows. First, the effects of socio–economic and demographic characteristics will be described. Then we will look at the effect of the variables related to unemployment and training. Finally, the issue of duration dependence will be discussed in the light of our two models.

6.1 Socio–Economic and Demographic Characteristics

• Young *age* increases the chances to find a job, which is consistent with the view that young people are more flexible both geographically and vocationally. However, we observe a very small and highly significant hazard ratio for school leavers who are mostly in the youngest age group. It is shown that being a school leaver more than compensates the effect of young age, so that the estimated duration of unemployment for school leavers is longer than the one of unemployed people between 46 and 55 years of age. This result is in line with the common view that Poland faces an unusually large youth unemployment problem (Steiner and Kwiatkowski, 1995; Puhani, 1995).

• Because of the inclusion of interaction effects, the interpretation of the *single* variables requires some care. The *single* hazard ratio (HR) can be calculated as $e^{\beta_S} \times e^{\beta_{SN}\overline{N}} \times e^{\beta_{SC}\overline{C}}$, where e^{β_S} is the HR for *single*, $e^{\beta_{SN}\overline{N}}$ is the HR for *single & new regime*, $\overline{N}$ is the sample share of the category *new regime*, $e^{\beta_{SC}\overline{C}}$ is the HR for *single & children*, and $\overline{C}$ is the sample share of the category *children*. Tables A1 and A2 in the appendix report the HR and Effects on the Median for the categories affected by interaction effects, respectively.[15]

It follows that *ceteris paribus* male singles are slightly more likely to be hit by long–term unemployment than married males. For females, the effect is negligible and goes in the other direction in model 1. In model 2, the effect is insignificant. Longer unemployment durations of singles can have various reasons. First, (male) singles are usually not expected to have to care for other people, so that the urgency to find employment is less pressing for them. Second, on the demand side, employers may view married people as more reliable in carrying responsibility for others. Finally, employers may see a higher 'moral obligation' to employ married people due to other family members which depend on this income. These hypotheses are consistent with the fact that there is no remarkable effect for single women, as firms

[15] For Effects on the Median, we calculate the approximate effect in case of *singles* as $\beta_S + \beta_{SN}\overline{N} + \beta_{SC}\overline{C}$.

are likely to be more reluctant to employ married than single women, because the former might leave the firm sooner because of family responsibilities.

• The effects of *children* (see Tables A1 and A2 in the appendix) are also consistent with this view of the relationship between labour supply, employment opportunities and the division of labour within the family. For men there are no effects, which is somewhat surprising, as we would expect fathers to be searching more intensively. The fact that women with children take longer to find a job can be explained both by higher search costs through child rearing as well as efficiency considerations on the side of employers, who fear that mothers may have more children and incur costs to the firm by going on maternity leave. There are no additional effects for neither single men nor women with children (the interaction dummies are not significant. However, unemployment durations for singles are a little shorter under the new regime, especially for males. So far, we have not found an explanation for this effect.

• The *disabled*, especially disabled men, have longer spell durations than non–disabled people. Explanations can be fewer job offers as well as higher search costs. The fact that the effect is weaker for women than for men might be explained by a selection effect: whereas men are usually still the income providers in most families, women more often have the option of retreating into non–participation and rely on the income of a husband.

Table 3: Estimation Results in Terms of Hazard Ratios

Variable	Men				Women			
	Model 1		Model 2		Model 1		Model 2	
	HR	t	HR	t	HR	t	HR	t
age between (36 and 45)								
18 and 25	2.056	–9.64	2.023	–8.37	1.534	–5.38	1.548	–4.85
26 and 35	1.209	–2.98	1.229	–2.94	0.915	1.27	0.869	1.84
46 and 55	0.983	0.17	1.016	–0.15	0.853	1.41	0.906	0.77
single	0.650	3.69	0.623	3.63	0.866	1.27	0.890	0.86
single & new regime	1.361	–2.61	1.404	–2.60	1.336	–2.35	1.244	–1.53
single & children	0.750	1.01	1.001	–0.00	1.008	–0.05	1.070	–0.40
children	1.107	–1.36	1.106	–1.18	0.725	3.96	0.677	4.27
no information on children	0.788	2.63	0.790	2.40	0.914	0.94	0.860	1.39
disabled	0.530	6.33	0.488	6.84	0.569	4.35	0.602	3.81
education (basic vocational)								
higher	1.309	–2.09	1.281	–1.62	2.325	–5.85	2.591	–5.65
post–secondary	1.370	–1.83	1.520	–2.09	1.677	–3.82	1.673	–3.38
secondary vocational	1.405	–5.29	1.360	–4.20	1.505	–5.62	1.471	–4.74
secondary general	1.260	–1.49	1.154	–0.87	1.230	–2.15	1.222	–1.91
primary or less	0.940	0.96	0.971	0.43	0.782	2.92	0.804	2.53
occupation (industrial worker)								
manager	0.903	0.78	0.928	0.49	0.562	3.27	0.587	2.66
professional	1.208	–1.09	1.257	–1.10	0.589	3.27	0.568	3.12
technician	0.700	3.41	0.714	3.06	0.399	7.28	0.382	7.38
white collar	1.011	–0.09	1.023	–0.14	0.436	6.80	0.413	6.76
personal services	1.011	–0.11	0.986	0.12	0.580	4.83	0.561	4.76
farmer	0.765	1.54	0.661	2.38	0.582	2.06	0.594	2.00
simple blue–collar	0.953	0.63	0.964	0.44	0.553	3.28	0.533	3.46
other simple jobs	1.036	–0.46	1.001	–0.01	0.850	1.54	0.825	1.79

industry (mining, manufact.)								
agriculture, forestry, fishing	0.689	2.62	0.732	2.16	1.019	–0.08	0.966	0.14
electricity, gas, water	1.587	–2.73	1.650	–2.58	2.552	–3.41	2.859	–2.95
construction	1.015	–0.21	1.008	–0.11	0.850	0.85	0.898	0.55
trade, repairs	1.109	–1.29	1.094	–0.91	1.920	–6.45	1.890	–5.99
catering	0.817	1.21	1.087	–0.36	1.329	–1.76	1.335	–1.78
transport, communication	1.107	–0.87	1.017	–0.14	1.595	–2.68	1.569	–2.40
financial intermediation	1.521	–1.81	1.483	–1.51	2.921	–6.61	3.079	–5.83
real estates, renting	1.259	–1.44	1.276	–1.26	1.631	–2.40	1.497	–1.76
public administration	1.359	–2.89	1.384	–2.79	2.245	–5.88	2.227	–5.11
education	1.744	–2.95	1.660	–2.36	1.823	–4.73	1.786	–4.17
health, social work	1.277	–1.43	1.241	–1.12	2.368	–7.64	2.332	–6.95
other serv., none, not known	1.150	–1.35	1.136	–1.03	1.301	–2.06	1.195	–1.34
place of resid.. (countryside)								
100,000 inhabitants or more	1.329	–4.36	1.306	–3.70	1.308	–3.82	1.352	–3.89
20,000 to 99,999	1.080	–1.19	1.111	–1.44	1.082	–1.06	1.104	–1.24
19,999 or less	1.126	–1.61	1.143	–1.72	0.894	1.36	0.953	0.54
voivodship unemployment rate	0.984	2.91	0.985	2.57	0.983	2.87	0.981	2.96
previously unemployed	0.374	12.41	0.401	12.15	0.287	10.69	0.307	11.13
registered but no unempl. ben.	0.718	4.85	0.615	6.32	0.649	5.79	0.583	6.59
new regime	1.449	–4.92	1.219	–2.07	0.995	0.06	0.840	1.89
unemployment benefit	0.639	4.96	0.458	6.92	0.455	8.79	0.357	9.84
new regime & unempl. benefit	0.892	1.10	1.008	–0.06	1.175	–1.54	1.299	–2.20
unempl. benefit until pension	0.159	7.51	0.195	7.70	0.336	6.14	0.336	6.63
school leaver	0.444	5.95	0.430	5.58	0.468	4.87	0.473	4.64
public training	0.917	0.41	0.872	0.62	1.277	–1.50	1.301	–1.39
self–financed training	1.385	–2.39	1.302	–1.81	1.355	–2.20	1.384	–2.04
employer– financed training	1.945	–5.12	1.761	–3.61	2.797	–6.95	2.736	–5.14
1st quarter	0.945	0.86	0.916	1.22	0.854	2.21	0.873	1.71
3rd quarter	0.651	6.80	0.636	6.65	0.640	6.53	0.634	6.13
4th quarter	0.824	2.97	0.827	2.57	0.806	2.84	0.803	2.64
constant	0.078	16.39	0.161	10.20	0.115	12.81	0.247	7.11
$\hat{\theta}$ (s.e.)	–	–	0.862	(0.129)	–	–	0.823	(0.140)
$1/\hat{\alpha}$ (s.e.)	0.999	(0.022)	0.791	(0.029)	1.033	(0.024)	0.842	(0.032)
log likelihood	–4,141.3		–4,109.8		–4,016.3		–3,990.6	
# observations	4,353		4,353		4,441		4,441	

Table 4: Estimated Effects on the Median Duration of Unemployment in Months

Variable	Men		Women	
	Model 1	Model 2	Model 1	Model 2
age between (36 and 45)				
18 and 25	–6.3	–4.1	–2.9	–1.9
26 and 35	–2.1	–1.5	0.8	0.8
46 and 55	0.2	–0.1	1.4	0.6
single	6.6	4.9	1.3	0.7
single & new regime	–3.3	–2.3	–2.1	–1.1
single & children	4.1	0.0	–0.1	–0.4
children	–1.2	–0.8	3.1	2.6
no information on children	3.3	2.1	0.8	0.9
disabled	10.9	8.4	6.3	3.6
education (basic vocational)				
higher	–2.9	–1.8	–4.7	–3.4
post–secondary	–3.3	–2.7	–3.3	–2.2
secondary vocational	–3.5	–2.1	–2.8	–1.8
secondary general	–2.5	–1.1	–1.5	–1.0
primary or less	0.8	0.2	2.3	1.3
occupation (industrial worker)				
manager	1.3	0.6	6.4	3.8
professional	–2.1	–1.6	5.8	4.2
technician	5.3	3.2	12.4	8.8
white collar	–0.1	–0.2	10.7	7.8
personal services	–0.1	0.1	6.0	4.3
farmer	3.8	4.1	5.9	3.7
simple blue–collar	0.6	0.3	6.7	4.8
other simple jobs	–0.4	0.0	1.5	1.2

industry (mining, manufacturing)				
agriculture, forestry, fishing	5.5	2.9	–0.2	0.2
electricity, gas, water	–4.5	–3.2	–5.0	–3.6
construction	–0.2	–0.1	1.5	0.6
trade, repairs	–1.2	–0.7	–4.0	–2.6
catering	2.7	–0.6	–2.0	–1.4
transport, communication	–1.2	–0.1	–3.1	–2.0
financial intermediation	–4.2	–2.6	–5.4	–3.7
real estates, renting	–2.5	–1.7	–3.2	–1.8
public administration	–3.2	–2.2	–4.6	–3.0
education	–5.2	–3.2	–3.7	–2.4
health, social work	–2.7	–1.6	–4.8	–3.1
other services, none, not known	–1.6	–1.0	–1.9	–0.9
place of residence (countryside)				
100,000 inhabitants or more	–3.0	–1.9	–1.9	–1.4
20,000 to 99,999	–0.9	–0.8	–0.6	–0.5
19,999 or less	–1.4	–1.0	1.0	0.3
voivodship unemployment rate	0.198	0.121	0.140	0.103
previously unemployed	20.5	12.0	20.5	12.3
registered but no unempl. ben.	4.8	5.0	4.5	3.9
new regime	–3.8	–1.4	0.0	1.0
unemployment benefit	6.9	9.5	9.9	9.9
new regime & unempl. benefit	1.5	–0.1	–1.2	–1.3
unempl. benefit until pension	64.8	33.1	16.3	10.8
school leaver	15.4	10.7	9.4	6.1
public training	1.1	1.2	–1.8	–1.3
self–financed training	–3.4	–1.9	–2.2	–1.5
employer– financed training	–6.0	–3.5	–5.3	–3.5
1st quarter	0.7	0.7	1.4	0.8
3rd quarter	6.6	4.6	4.6	3.2
4th quarter	2.6	1.7	2.0	1.3
median duration at mean	19.68	17.61	25.02	23.15
median duration of reference person	12.25	8.03	8.25	5.47
# observations	4,353	4,353	4,441	4,441

• Women with higher levels of *education* in general have shorter unemployment spells probably reflecting the relative scarcity of university graduates caused by negligent policies under the old regime. In the case of men, the situation is not so pronounced, but similar.

• As far as *occupations* are concerned, it is remarkable that for both sexes, industrial workers and people in *other simple jobs* have *ceteris paribus* amongst the shortest spell lengths. These groups were probably not thrown in a completely new working environment after the introduction of a market economy and were therefore able to find new employment fairly quickly. On the other hand, jobs demanding more skills (technical and white–collar jobs[16]) will be more difficult to find by the unemployed who have become outsiders. The wider span for females than for males of the effects may suggest that the comparatively large share of females in technical occupations, pushed by the socialist regime, is not supported any more by the market. As far as unemployment in white–collar jobs is concerned, it also worth noting that labour shedding has been widespread in administrative units of public enterprises filled mainly by women (OECD, 1993).

• The *industry* effects support our considerations on economic restructuring of Section 2. It is in the service industries and the public utilities where unemployment durations are shortest. Durations are also comparatively short for people from the public administration. This is true for both men and women.

• The variables accounting for *geographical factors* have expected effects. In large urban areas, spells are shorter which is consistent with lower search costs as well as lower costs when accepting a new job (*e.g.* travelling expenses and moving costs). The effect on the voivodship unemployment rate, which is taken as a proxy for local demand conditions, is consistent with low demand for labour generating fewer job offers, more discouragement and hence longer spells. The effects of the *seasonal dummies* show that joining the unemployment pool between July and September one can expect to stay unemployed longer than becoming unemployed in any other quarter.

[16] Here, especially people who find it difficult to keep track with the computer revolution, will have difficulties.

6.2 Previous Spells, Benefits, and Training

• People who have been *unemployed previously* have unemployment spells far longer than those who have not. Thus the evidence on occurrence dependence from Section 2 is supported under the *ceteris paribus* restriction. Noting the high sample share of this group (25% and 19% for men and women in Table 1, respectively), this is an indication that the costs of unemployment are distributed very unequally not only across the population, but also across the unemployment population.

• Those who *register* at a labour office *without drawing benefits* remain unemployed for a longer period than those who do not register at all. One interpretation may be that counting on the labour offices to find you a job is not a good strategy, as they do not work very efficiently. Another interpretation would be that we observe a selection effect in that on average, people who register at a labour office to find a job are not as fit as those who choose not to register.

• The effects of *unemployment benefits* (see Tables A1 and A2 in the appendix) on the duration of unemployment are positive, highly significant, and large. However, the estimated effects of the benefit regime change are negligible (see the interaction effect in Table 3), although the generosity of the benefit regime has been reduced drastically. The results give credence to the view that the unlimited entitlement period of the old regime was not the main culprit for the widespread incidence of long–term unemployment..

• However, an *entitlement period up to the age of pension* has a very large and positive effect on the duration of unemployment (43 months for males and 20 months for females according to the estimates of model 2 in Table A2). These estimates should not be overinterpreted, as we do not observe directly whether someone falls under this category, but have to infer it from a persons age, education, etc. (see Section 3). However, there can be an economic explanation for why the entitlement effects are stronger for men than for women. Given a traditional division of labour within the family where the husband provides the largest share of income, men will have greater incentives to make use of their entitlements rather than to withdraw from the labour force than women, even if they are not searching for a job any more. On the other hand, of course, it would be rational for both sides to take what is theirs.

• *Public training* has no significant effect on the duration of unemployment. One may argue that the zero effect of public training can result from two opposite effects compensating each other. The first may be that those selected for a public training course are likely to be people with problems in the labour market which manifest themselves in longer durations (see our discussion of causality in Section 5.3). If training has the effect of reducing the spell length of the participants and we have a

selectivity problem of the kind just mentioned, we may estimate a zero effect although public training *ceteris paribus* improves employment opportunities and search efforts. However, we are controlling for a wide set of individual characteristics and so the result of a zero effect suggests that public training is at least not a panacea for the problems of the long–term unemployed.

For comparative purposes, we also take a look at self–financed as well as employer–financed training.

• People who *(re–)train* themselves and pay for it *out of their own pockets* have short unemployment durations (only significant at the 10% level for men in model 2). Unemployment durations of people who have been paid a training course by their employers are even shorter. This is an expected result as employers would only finance the training of their more competitive employees, so that unemployment spells of these people probably constitute mostly frictional unemployment. Hence again, as with publicly–financed training, we probably also observe a selection effect here. The same is true for people who finance their own training, as these will be the workers who are keen to move on.

6.3 Duration Dependence

In Subsection 6.2, we found that the existence of a previous unemployment spell increases the expected duration of unemployment significantly (heavy occurrence dependence). In this subsection, we will explore the issue of duration dependence of the hazard rate. Figure 5 plots estimated hazard rates for both men and women for both models 1 and 2 (see Table 3 for formulae). Looking at model 1 first, we see that the hazard rate for men continuously lies above the one for females. As Table 3 shows, the estimated interval $\left[1/\hat{\alpha} \pm 2\hat{\sigma}_{1/\hat{\alpha}}\right]$ contains 1, which means that we do not reject the hypothesis that $\alpha = 1$. Hence, only from model 1, we would conclude that there is no duration dependence for men nor women, *i.e.* hazard rates are constant over time. However, allowing for unobserved heterogeneity, we clearly reject model 1 in favour of model 2 for both sexes, as θ is significantly different from zero. In model 2 we see that there is in fact positive duration dependence.

The difference between the curvature of the hazard rates estimated in models 1 and 2 can be explained by a sorting effect: the hazard integrated over v is a weighted average of the hazards of high–risk and the low–risk groups. But as the relative weight of the high–risk groups diminishes continuously over time as members of the high–risk group leave the unemployment state more quickly, the average hazard will asymptotically run towards the hazard of the lowest risk group. In Figure 6, we plot the estimated hazards for $v = 1$ (*cf.* Section 5.2).

Figure 5: Estimated Hazard Rates at Sample Means (Expectation over ν)

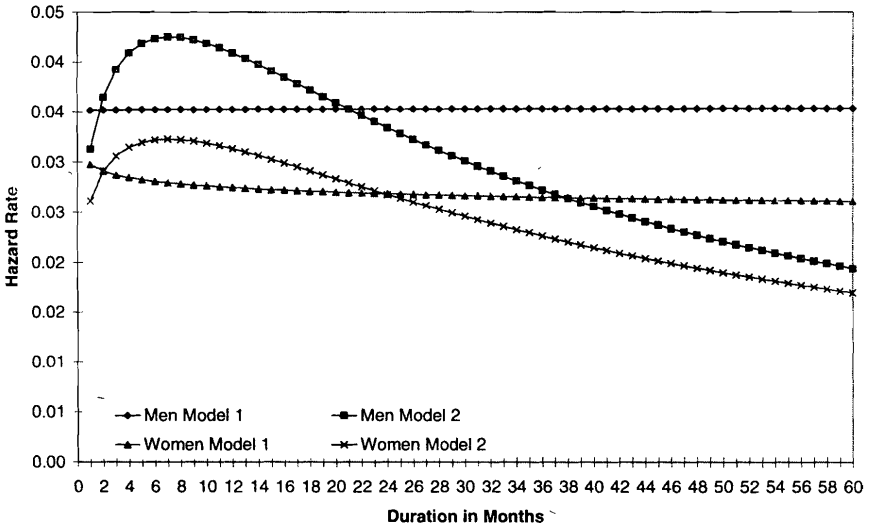
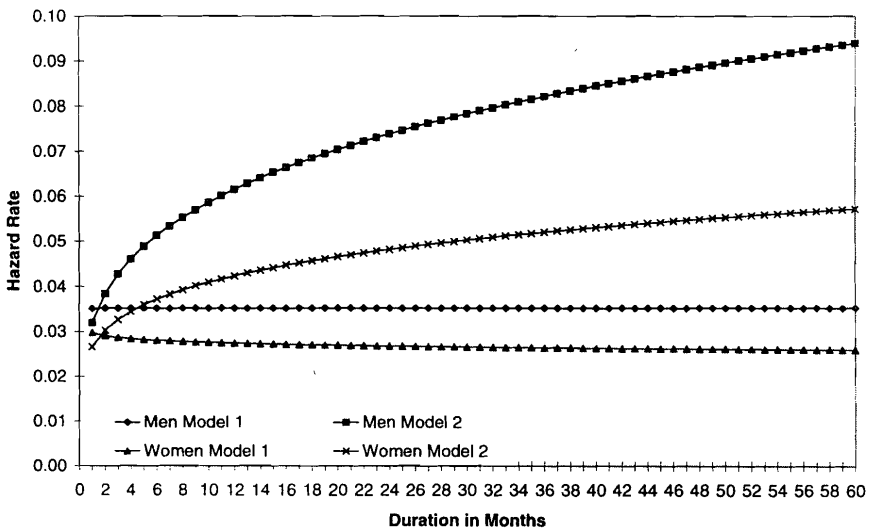


Figure 6: Estimated Hazard Rates at Sample Means (ν=1)



7 Conclusions

Our results are similar to the ones of other studies for the Visegrád countries. Micklewright and Nagy (1995) find that unemployment durations are not much affected by changes in unemployment benefits in Hungary, which saw a benefit regime change similar to the one of Poland. Also, Ham, Svejnar, and Terrell (1996) conclude that the negative incentives caused by the unemployment benefit system are rather minor in both the Czech and the Slovak Republics. However, Steiner's (1996) recent study for Western Germany as well as the review of Atkinson and Micklewright (1991) show that the estimated effects are not robust with respect to the specification of the econometric model and the modelling of the benefit entitlement. In this light, our results for Poland should be seen only as a first exploration of these important issues of public policy.

The conclusions from the econometric models are also in line with the *a priori* considerations of Section 2. In particular, the long unemployment durations for school leavers have their roots mainly in the institutional peculiarities of the Polish labour market, where high redundancy payments and effective opposition of the two main trade unions 'Solidarity' and 'OPZZ' prevented mass lay–offs. This delayed the restructuring process and blocked the recruitment of young people. The result is a low turnover of the unemployment pool causing many people to get trapped in long–term unemployment. This is also illustrated by the high share of people exhibiting multiple unemployment spells: once unemployed, it seems very hard to find a permanent job again. Having been unemployed before increases the expected duration of unemployment by more than a year. It follows that the costs of transition in terms of unemployment are distributed very unequally across both the labour force and the unemployment population. The reduction of the unemployment benefit entitlement period does not seem to have had an impact on long–term unemployment which would be worth mentioning. Nor do training programmes organised by labour offices seem to have done much good. From Steiner and Kraus (1995) on Eastern Germany, Puhani and Steiner (1996) on Poland, and this study, one may come to the conclusion that active labour market policies, at least in their current form, should be seen more as a *social* and less as an *economic* policy device. Although one may have reservations whether these studies identify causal effects, the evidence overall seems to support the view that the long–term unemployment problem cannot be solved by active labour market policies nor reductions in unemployment benefits.

Appendix

The number of persons both in the Polish Labour Force Survey of August 1994 and its Supplement on the Evaluation of Labour Market Policies is 47,393. From these 10,634 state that they are or were looking for a job. We then reduce the sample size by those who were properly employed whilst looking for a job (down to 9,391), those who say they are looking for a job, but are not unemployed according to ILO recommendations as they are not ready to take up a job within the following week (down to 9,132), and those who are not aged between 18 and 55 at the beginning of their unemployment spell (down to 8,794). So we are left with 4,353 men and 4,441 women.

Table A1: Hazard Ratios for Categories Affected by Interaction Effects in Table 3

Category	Men		Women	
	Model 1	Model 2	Model 1	Model 2
single	0.753	0.832	1.098	1.100
single & new regime	1.067	1.071	1.272	1.127
single & children	0.702	0.920	0.799	0.769
children	0.980	1.106	0.727	0.691
new regime	1.542	1.414	1.199	1.051
unemployment benefit	0.478	0.388	0.464	0.394
new regime & unempl. benefit	0.776	0.546	0.522	0.374
unempl. benefit until pension	0.092	0.090	0.174	0.148

Note: Shaded numbers indicate that at least one of the coefficients that go into their calculation is significant at least at the 5% level. The principle that underlies the calculations in outlined in Section 6.1.

Table A2: Effects on Median for Categories Affected by Interaction Effects in Table 4

Category	Men		Women	
	Model 1	Model 2	Model 1	Model 2
single	5.472	2.890	–0.429	–0.378
single & new regime	2.075	1.065	–1.521	–0.299
single & children	6.720	2.117	2.671	2.067
children	0.541	–0.771	3.110	2.501
new regime	–4.286	–2.457	–1.339	–0.048
unemployment benefit	15.007	12.956	10.536	9.943
new regime & unempl. benefit	10.057	10.526	9.685	10.395
unempl. benefit until pension	73.010	42.566	25.228	19.663

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