JOB SEARCH MONITORING AND UNEMPLOYMENT DURATION IN HUNGARY: EVIDENCE FROM A RANDOMISED CONTROL TRIAL

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ABSTRACT

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Job Search Monitoring and Unemployment Duration in Hungary: Evidence from a Randomised Control Trial

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Abstract

The impact of the administration of unemployment benefits on time spent unemployed is a neglected issue in discussion of incentive effects in Central and Eastern Europe. We use Labour Force Survey data, administrative registers and inspection of benefit office practices to show that there is good reason to investigate this issue in Hungary. We then report on results from a field experiment of the impact of tightening the administration of benefits in which benefit claimants were randomly assigned to treatment and control groups. Treatment has quite a large effect on durations on benefit of women aged 30 and over while we find no effect for younger women or for men.

Keywords: experiment, job search, unemployment insurance, Hungary

JEL codes: J64, J65, P23

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1. Introduction

There is a short experience of administering unemployment benefit systems in Central and Eastern Europe. Income support for people searching for work did not exist prior to the 1990s due to an absence of open unemployment in planned economies. The debate about the behavioural impact of the new benefit systems in the region has been considerable but has focused on benefit levels and lengths of entitlements.[1] We look at the neglected issue of the administration of benefits. We use data from a field experiment with random assignment to treatment and control groups conducted in Hungary in 2003. Evidence from field experiments on monitoring job search behaviour by the unemployed is thin on the ground throughout Europe. And to our knowledge, there is no evidence at all for countries from Central and Eastern Europe.[2]

Hungary was the first former planned economy to introduce unemployment benefit. In the early 1990s administration focused on benefit delivery as the economy contracted sharply. There has been concern that the monitoring of job search activity of benefit recipients may be weak (a concern fuelled in part by the perceived importance of the hidden economy). The sustained recovery of the Hungarian economy from the mid-1990s, and hence the greater availability of jobs, prompts further consideration of the issue. Our work provides the first analysis of monitoring of job search by the Hungarian unemployed and of their frequency of contact with benefit administrators.

Section 2 uses Labour Force Survey data, administrative registers and our own enquiries into employment office practices to shed light on monitoring of job search by benefit claimants in Hungary. We make comparisons with other OECD countries where possible. Monitoring in Hungary is typically light and has declined in recent years. This provides the background for our experiment, the design of which is explained in Section 3. The results in Section 4 show marked differences between the sexes in the effect of treatment on benefit duration and outflows to employment. The treatment has quite a large positive effect on women aged 30 and over while we find no effect for younger women or for men. Section 5 concludes.

[1] See Boeri and Terrell (2002) for a summary. Examples include Ham et al (1998) for the Czech Republic and Slovakia, and Micklewright and Nagy (1999) for Hungary.

[2] The small European literature includes the early work by Royston (1983, 1984) and Dolton and O'Neill (1995) for the UK and Gorter and Kalb (1996) and van den Berg and van der Klaauw (2001) for the Netherlands. Evidence is much more common in the US e.g. Ashenfelter et al (2005), Black et al (2002) and the survey by Meyer (1995).

2. Job search monitoring: Hungary and other OECD countries

The administration of unemployment benefit can be thought of as attempting to restrict benefit to people who are unemployed in the sense of the standard ILO definition (OECD 2000, p130): out of work, able to enter work at short notice and undertaking active steps to find work. Table 1 summarises the degree of success of the Hungarian benefit system in achieving this aim, showing the proportion of unemployment benefit recipients who are classified in Labour Force Survey (LFS) data as unemployed by the ILO criteria. The figure of two-thirds in 1993 was low to average compared to those for other Central European countries (Bardasi et al 2001). And it has fallen substantially since, to less than a half by 2002. To take another yardstick, this is well below the figure of 75 percent or more found throughout the period in the UK, a country that has tightened its administration of benefit considerably (and where unemployment moved in a similar way to that in Hungary over the years in question).[3]

Women with unemployment benefit are less likely to be available and searching for work than men. This is also true of other Central European countries in the Bardasi et al comparisons (and of the UK), and a gender difference in behaviour turns out to be a key feature of results from our experiment. The difference in search and availability rates is even larger in most years between persons receiving contributory (and limited duration) unemployment insurance (UI) and those on means-tested social benefit (SB) provided by local government councils. (The latter can be paid where a person has insufficient contributions for UI or has exhausted entitlement.) Persons on SB are much less likely to be in the labour force. But it is clear that the fall in search and availability rates over time has occurred for claimants of both benefits. (SB and UI have been of roughly equal importance in terms of the benefit claimant stock since 1996.) The empirical literature on monitoring search in other countries tends to focus on UI. For practical reasons explained below, the same is true of our experiment, although we say what we can about SB in the rest of this section.

Restricting benefit to just the ILO unemployed can be tried through various forms of monitoring by the public employment service. One method is to require claimants to report periodically for face-to-face interviews in which information is sought on their job search activity (and is also provided on possible opportunities).

[3] UK figures are derived from LFS microdata for the years shown in Table 1. Unemployment rates in Hungary and the UK were 12.1 and 10.5 percent respectively in 1993 and 5.2 and 3.1 percent in 2002.

Figure 1 shows the proportion of the registered Hungarian unemployed who had visited a public employment office in the previous month, again based on LFS data. (The data are limited to 1999-2002 as the relevant question was not asked in earlier years.) The offices are responsible for both administration of benefits and for matching of the unemployed to suitable registered vacancies. The figure is again much lower for claimants on SB (and similar to that for people receiving no benefit at all): only 1 in 3 recipients in 2002 had been to the employment office in the previous month. But even for UI recipients the figure was little more than one half.

Face-to-face interviews are just one form of monitoring that can be used by a public employment service. For example, interviews are rare in many US states, which instead rely more on postal or phone reports by claimants of job search activity, with continued benefit conditional on satisfactory information being given (Andersen 2001). However, Hungary has very little of such other monitoring, underlining the importance of face-to-face interviews. For this reason, the sharp fall in 2000 in the proportion of claimants with recent visits to local employment offices is certainly noteworthy. This fall coincided with new legislation that required UI claimants to visit employment offices at least once every three months. Existing law had not stipulated any period, merely saying that visits should occur 'regularly', and their frequency had been left to employment office discretion. Far from tightening benefit administration as had been intended, the effect of the change in law seems to have been that many offices which had previously required more frequent visits took the three month period as the standard (a conclusion borne out by our discussions with employment office staff).

The evidence therefore suggests that administration of unemployment benefits in Hungary has weakened in recent years. This has been a period when one might have expected it to strengthen on account of an easing of labour market conditions and a growing economy. Against this background we now briefly outline aspects of the existing system in more detail that are relevant to our experiment.[4]

Industrialised countries differ substantially in the measures they undertake to monitor search and availability of benefit claimants. However, some common features can be identified (OECD 2000). Like most other Continental European countries (and unlike the US and the UK for example), Hungary does not require frequent reporting of

[4] It should be noted that there has also been a large fall in the proportion of the ILO unemployed stock who receive any benefits, from 59 percent in 1993 to only 35 percent in 2002 (with the UI coverage rate falling from 52 percent to 18 percent). This is not evidence that administration has tightened. The main explanation for the fall has been sharp cuts in entitlement periods to UI.

independent job search activity. In fact no such reporting is required. UI claimants must register with their local employment office and must then return regularly in person to continue to declare their availability, as noted earlier. But no records need be kept by the claimant of employers he or she has contacted or of other efforts to find a job and no checks are made of search activity during visits to the employment office.[5] In the past, all unemployed SB claimants also had to report regularly to an employment office but whether or not they are required to do so is now at the discretion of each local government (and no information exists on the range of different practices).

The frequency at which the UI claimant must return to the employment office differs across the country, illustrating a feature found in many other countries' monitoring activity: substantial within-country variation (OECD 2000). The Hungarian public employment service is organised by counties, of which there are 20. Each county has considerable discretion to interpret the relevant legislation as it sees fit. Practice varies from office to office within the counties as well. We collected information on office practices in Autumn 2002 from 28 offices (out of a national total of 170) spread over the six counties in which our field experiment was to be conducted. The counties were picked in part to provide a good spread of labour market conditions (see Section 3). Of these, 16 required that UI recipients returned every three months. In six offices the frequency was once a month and in the remaining six somewhere in between.[6] In addition, in all offices claimants could be contacted within this interval and asked to attend in person to receive information on a specific vacancy that the office deemed suitable.

This variation in administrative practices seems to be reflected in LFS data on the proportion of UI recipients in each county who have visited an employment office in the last month, although these data should also reflect regional variation in claimants' search behaviour (if the offices' vacancy lists are seen as worth consulting). Figure 2 shows that the 2003 figures ranged from about 40 percent to over 70 percent. (The much lower figures for SB recipients, not shown, are well correlated with those for UI claimants.)

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[5] The US represents an extreme contrast. Most states require UI claimants to report at least two employer contacts per week. (In the mid-1990s the director of the Hungarian county of Somozgy introduced a system in all offices whereby claimants had to get a form stamped by employers to certificate that a job had been applied for. The system lasted for about a year.)

[6] The precise interval may vary from that reported to us since claimants sometimes contact the office in advance and succeed in changing the stipulated date (on what may be reasonable grounds or may be suspect grounds).

Monitoring of any type will only be effective if there is a credible threat of sanctions in the case that a claimant does not comply with a request to come to the employment office, to provide information on job search behaviour, to follow-up on a suggested vacancy etc. Sanctions for on-going UI claims will typically involve suspension of payments or outright disqualification. In Hungary, missing an interview with the employment office is the classic explanation for the former while the latter is typically triggered by unreasonable refusal of a job offer generated through the local employment office or by behaviour that resulted in no offer being made (e.g. arriving drunk at an interview with an employer).[7]

Sanction rates in practice for the six counties covered by our experiment are shown in Figure 3. The data refer to all suspensions and disqualifications that are applied to on-going UI claims and are expressed in terms of the annualised number as a percent of the average UI stock. We compare them with rates defined on the same basis for other OECD countries, figures that vary greatly. And once more, there is substantial regional variation within Hungary. The county with the highest rate, Vas, is sanctioning claims at the same broad level as Australia and the Czech Republic, which are among the tougher OECD countries in the table, although nowhere near as tough as Switzerland or the US. Szolnok is at a similar level to the UK, Norway and Finland, while Csongrad and Komaron at the other end of the range are more akin to Belgium and Denmark. Of course, higher rates may reflect more frequent behaviour in need of sanction rather than a stricter application of the rules. But in the case of Hungary we think the latter to be the case. This leads us to expect that the impact of treatment in our experiment may vary geographically due to differences in the culture of sanctioning.[8]

The final background to our experiment is a major change in UI legislation that occurred in 2003 which provides for both an extension to UI and a re-employment bonus. Claimants with at least 180 days of entitlement (the maximum is 270 days), something generated by a good employment history, are now given the option three months before the expiry of their entitlement to ‘co-operate actively’ with the local employment office in return for a flat-rate extension to their entitlement for six months beyond the normal expiry date (the main UI scheme pays an earning-related benefit). If they obtain a job during this six month extension period and hold it until the end of the

[7] Suspension of benefit is usually until the claimant finally comes to the office. For second and subsequent offenses, the period of suspension uses up the claimant’s entitlement period.
[8] Note that there is variation within counties by employment office; about half the variation in sanction rates across employment offices is at this level rather than between counties.

period, they are given half the value of the saving in UI payments. In principle, 'active co-operation' involves more frequent visits to the employment office and undertaking specific job search activity chosen by the office. About half the UI inflow has an employment history that would qualify them for this extension.

This innovation may have been inspired by examples from the US, both of UI extension schemes and re-employment bonuses. But it was introduced with no analysis of the likely take-up or impact (for which no data are yet available). And the definition in practice of the required 'active co-operation' is unclear. However, the new scheme does at least underline that the Hungarian benefit authorities are beginning to think more about job search monitoring, which provides further motivation for our experiment.

3. The experiment

Claimant behaviour suggested by LFS data and the current monitoring practices in Hungary had several implications for the experiment's design.[9]

First, the main instrument of monitoring in use at present is the requirement on the claimant to report regularly to the local employment office. Policy here appears to have inadvertently relaxed in recent years and an obvious choice was to explore its tightening. Second, since offices rarely ask about job search activity (and never formally monitor it), questioning claimants on this subject was again an obvious measure to experiment with. Third, although the SB scheme seems in considerable need of attention, we restricted the experiment to UI claimants. The organisation of SB by local governments meant there was no central authority with whom participation in the experiment could be agreed. Fourth, the existing variation across the country in monitoring practices implied that we had to be careful to standardise monitoring of the control group of claimants in the experiment as well as the group assigned to treatment. Finally, LFS data show women to be less likely to search and be available for work than men and we therefore wanted to cover both sexes in the experiment.

To add to this background, we had to recognise that office clerks overseen by their managers would be the persons actually administering the treatment. The culture of only light monitoring in the UI system affected what could be tried without risking a

[9] The experiment was planned by us in conjunction with the National Labour Centre. It was then adopted as a formal initiative of the Centre.

significant principal-agent problem: an experiment has to be 'doable' in the sense of allowing agents to carry it out conscientiously. (It must also be 'doable' in the legal sense, with the experimental treatment being permitted by law.)

Our experiment was therefore modest. Successful UI claimants were divided into treatment and control groups at the outset of their claims with their spells of UI administered as follows for the duration of the experiment:

Control: Visit the employment office every three months and face no questions on job search.

Treatment: Visit the employment office every three weeks and office clerks ask detailed questions on job search behaviour since the last visit.

Sanctions for failure to come to an interview remained as before (including any local variation in practices suggested in Section 2). The questions on job search information began to be asked of the treatment group at their first scheduled return to the office three weeks after initial registration for UI. These questions concerned methods of search undertaken since the last visit, numbers of contacts with employers, names and locations of up to three employers who were contacted, and reasons for lack of employer contact if none had taken place.

What effects do we hypothesise the treatment to have? More frequent contact with the employment office maintains the claimant's exposure to a major source of vacancies, underlines the link between receiving benefits and looking for work and disrupts any activity in the hidden economy thus decreasing its attraction relative to formal jobs. The questioning during visits again reinforces the benefit-search link and produces disutility for people who have to admit to little or no search activity. Although no formal sanctions were in fact applied to those who reported no search (the usual sanctions did apply if the three weekly interview was missed), claimants would have been uncertain as to the implications of failure or repeated failure to search.[10] These effects of the treatment should result in a greater level of search activity and consequently a higher exit rate to jobs from the UI register. Exits to inactivity could also increase (where individuals decline to search but decide to cease claiming benefit).

[10] Even claimants who had been recently unemployed would not have perfect knowledge of current regulations and office practices.

The experiment began in late April 2003. It covered all new UI claimants with 75-179 days of UI entitlement who registered in the following three months in six counties.[11] Information on marital status, household composition and circumstances (e.g. number of children of different ages, employment status of the spouse) was obtained from all claimants at initial registration for UI. The experiment lasted for 4½ months, implying that claimants in the treatment group made a maximum of 4 requested visits at three-weekly intervals to the employment office (after the initial visit for registration for UI). At the end of the experiment, each participating local office reverted to its previous practice of administering claims in progress, visits being requested of claimants at a frequency of anything between 1 and 3 months and with no questions on job search asked at these visits.

Claimants were allocated to treatment or control groups on the basis of their birthdays – odd days to treatment, even days to control – which amounts to random assignment. Participants were unaware of the existence of the experiment. In principle, individuals in the two groups could have talked to each other and discovering that their claims were being administered in different ways. However, in practice we think this very unlikely other than in perhaps a few isolated cases. The bulk of claimants were dealt with by employment offices in reasonable sized settlements where few claimants would know each other. All offices were changing their practices for a large group of claimants not covered by the experiment – see below – so variation in treatment within the same office should not have been cause for surprise.[12]

The restriction of the experiment to those with less than 180 days of entitlement was made in order to avoid persons eligible for the 2003 extended UI scheme described in the last section. This scheme offers an additional period of benefit and a re-employment bonus in return for vaguely specified additional job search activity. All aspects of the scheme's workings, including take-up and administration, were unknown at the time our experiment went into the field and we judged it sensible to exclude those eligible for it. The drawback of this decision is that the experiment was restricted to a group with a rather specific employment history: claimants with 75-179 days of entitlement have between 1 and 2½ years of insured employment in the four years prior to their claim. They have either had periods out of work, for example due to previous

[11] We excluded persons above the age of 50 on account of their proximity to retirement age.
[12] To the extent that any contamination between treatment and control groups did occur, our estimates should provide a lower bound on the impact of the changes in the administrative procedures concerned.

unemployment, or have joined the labour force during this time. About two thirds of those aged 30 and over (of either sex) have had a previous spell of UI during the four years and somewhat less than half of those under 30.

The six counties covered by the experiment contained a total of 48 employment offices (28 of which were included in our investigation of office practices described in the previous section). These six were chosen out of the total of 20 partly so as to give a mix of labour market conditions and existing rigour in application of eligibility rules and partly because they were counties with employment service mangers who we believed would oversee the experiment in an appropriate manner.[13] The conduct of the experiment was monitored by county managers and by the National Labour Centre with input from us.

After cleaning the data, the sample for analysis was composed of 2,134 persons (1,115 treatment and 1,019 control), split almost equally between men and women. The appendix shows the composition of treatment and control groups in terms of observed characteristics (other than outcome variables). No differences between the two groups are significant at the 5 percent level.[14]

The outcomes of the treatment that we can observe are (a) time registered as unemployed as measured by duration of time on UI (and hence censored if UI entitlement exhausts) and (b) exit state (job, training scheme, death etc) if the spell finishes.[15] The latter is in principle an important advance over data that measure only the duration of claims (e.g. Royston 1983, 1984) since treatment might encourage exit from the labour force entirely rather than to work. We do not observe wages in post-unemployment jobs. The effects of treatment on exits to jobs are hypothesised to come through greater search activity and any reduction in the reservation wage that comes from the disutility of increased monitoring. Note that treatment could stimulate more job search without any impact on exits from UI. The additional search may not be sufficient to generate job offers, due to weak local labour demand or because it is merely token activity.

[13] This consideration seems also to have influenced selection of employment offices in the Dutch experiment analysed in ven den Berg and ven der Klaauw (2001) which was restricted to two offices with ‘a good reputation for carrying out counselling and monitoring activities in a highly orderly fashion’.
[14] We also tested for differences with four sub-samples: women aged less than 30, women 30 and over, men aged less than 30, and men 30 and over. Again, no significant differences were found other than for marital status among men aged over 30 (71 percent married in the control group and 62 percent married in the treatment group.)
[15] The effect of treatment is measured conditional on a UI claim being made. Changes to benefit administration could also change the propensity to make a claim.

4. Results

Table 2 shows the exit states from the UI register for treatment and control groups. More than two thirds of spells of unemployment are censored, either due to the ending of the period of the experiment or because the individual exhausted entitlement to UI.[16] There are only small differences between the distribution of the two groups across other states. Notably, there is a difference of only one point between the percentages leaving the register to get a job (a difference that is not significant) and there is virtually no difference in the very low percentages voluntarily ceasing their claims to UI (who presumably exit to inactivity or to hidden economy jobs). This impression of no impact from the treatment is strengthened by Figure 4 where we show Kaplan-Meier estimates of survival in the UI register. A small difference can be observed between the treatment and control groups after 60 days, with the treatment group leaving the register slightly more quickly, but a log rank test shows no significant differences between the two survival functions.

However, the picture changes when we disaggregate by gender and age. Results are summarised in Table 3. The first column shows the log-rank tests for differences in the survivor function between treatment and control groups, distinguishing between men and women and between persons aged 30 and over and those who are younger. There are still no significant differences for the men. But among the women aged over 30 the survivor functions differ significantly at the 10 percent level. Columns 2-4 show the extent of this difference – a quarter of the control group have exited after 102 days in the register but among the treatment group a quarter have gone by only 85 days. (Among younger women an apparently perverse result if found, with those in the control group leaving more quickly, but the difference in survival functions is completely insignificant.) Figures 5 and 6 show respectively the survivor function for the women aged over 30 and their (smoothed) hazard for exits to jobs, by far and away the most important exit state. Differences between the treatment and control groups emerge after about one month, at about the time when the experiment begins to bite.[17]

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[16] This reflects the low outflow rate from unemployment in Hungary and other Central European countries (Boeri and Terrell 2002, Micklewright Nagy 1999).

[17] 30 percent of women of this age in the treatment group leave to jobs compared to 23 percent of the control group, which may be compared with the figures for all men and women in Table 2.

We now estimate multivariate models of the exit to jobs hazard, including a dummy for membership of the treatment group. Why estimate these models given that membership of the treatment group is independent of individual and locality characteristics by design? First, the models allow us to compare the effect of the treatment with the effect of other characteristics, which therefore provide a yardstick. Second, they are convenient way for exploring whether the treatment effect varies with other characteristics to those explored in the earlier graphical analysis, i.e. whether there are interaction effects. That said, the relatively small sample sizes at our disposal and the high degree of censoring means it is difficult to estimate interaction effects with any precision.

We estimate a model for the hazard, h, of individual i registered in employment office e leaving unemployment at duration s and real time t, of the following form:

$$h_{iest} = g(s).f(T_i, \boldsymbol{X}_i, \boldsymbol{O}_e, \boldsymbol{Z}_t)$$

where T_i is a dummy for membership of the treatment group, X_i are other observed characteristics we control for, $\boldsymbol{O}_e$ are a vector of employment office dummies, and Z_t pick up real time effects. We model g, the base-line hazard, with a (exponential) function of a series of dummy variables for each two-week interval that turn on an off as the individual moves through a spell of unemployment (following the practice of Meyer 1990). The function f is specified as:

$$f(T_i, \boldsymbol{X}_i, \boldsymbol{O}_e, \boldsymbol{Z}_t) = exp(\alpha T_i + \boldsymbol{\beta X}_i + \boldsymbol{\gamma O}_e + \boldsymbol{\delta Z}_t).$$

This includes dummy variables for real time, $\boldsymbol{Z}_t$, namely months of the year, allowing the hazard to change directly with calendar time as well as duration (claimants enter the register over a three month period). The employment office dummies, $\boldsymbol{O}_e$, pick up fixed-effects associated with the strength of local labour demand or aspects of the employment office itself, such as the skills of the staff in matching the unemployed to vacancies. The impact of the treatment is assumed constant: it is not allowed to change with duration, s, or calendar time, t. This may seem inappropriate given the evidence of Figure 6. However, we estimate the model having first left-truncated the spell data so that we only model the hazard in the period following the initial interview at the employment office. Up to that point, individuals in the treatment group are not administered any ‘treatment’ (they are asked to return to the office sooner than the control group only at that interview). With the left-truncated data, there is a rough constant difference between the empirical hazards (estimated by the Kaplan-Meier method) for treatment and control groups for women aged 30 and over, justifying our imposition of an unchanging impact of the treatment in the parametric modelling.

Parameter estimates are reported in Table 4 in the form of hazard ratios. (The clustering of individuals in employment offices is taken into account in the calculation of standard errors.) For dummy variables, these estimates show the ratio of the hazard with the dummy turned on to that when it is turned off. In the case of age (entered continuously), it shows the proportional change of the hazard with a change of one year of age. We estimate models separately for women aged under 30, for women aged 30 or over, and for men. For reasons of space, we do not report the coefficients of the base-line duration dummies, the calendar month dummies, or the nearly 50 employment office dummies.

The estimated impact of the treatment for men and for younger women is, as in the earlier graphical analysis, insignificantly different from zero. However, for women aged 30+, we estimate the hazard to be 60 percent greater for the treatment group, ceteris paribus. This difference is significant at the one percent level.[18]

The other coefficients are often insignificant for all three groups. This is true of age, marital status, spouse’s employment status, and number of children aged 0-6 (there is some indication that the hazard declines with age for younger women). Education is surprisingly insignificant for men and for women it is only the college/university educated where there is a clear increase in the hazard over the base group of primary/less than primary.

Tables 5 and 6 test for variation of the treatment effect with individual and local characteristics. Table 5 investigates whether the impact of treatment differs for married people (marital status itself has no association with the hazard in Table 4). For the women aged 30 and over the data suggests that this is indeed the case, the hazard ratio for married women being 90 percent higher for the treatment group while for single women treatment has essentially no impact. An increased level of claim monitoring appears to stimulate married women of this age to search more successively but not

[18] In a model without the employment office fixed effects the hazard ratio for treatment for the women aged 30+ is 1.43 with a t-statistic of 2.2. The employment office fixed effects are significant in each model in Table 4 at the five percent level but not at the one percent level (LR test with 47 degrees of freedom).

single women. However, some caution is needed since the hypothesis that the effect is the same for the two groups, single and married, is only just rejected at the 10 percent level. For younger women and for men, treatment again has no significant impact, regardless of marital status.

Table 6 shows whether the effect of treatment varies with the level of local unemployment. Where labour demand is lower (as measured by higher unemployment), treatment may increase search behaviour but have less impact on exits to work. Or offices may administer the treatment less rigorously in areas where jobs are in short supply. We investigate this by interacting the treatment dummy with the employment office area unemployment rate. The rate is measured at March 2003 and is not allowed to vary with calendar time, *t*. This means that we cannot include the employment office dummies as well – all the impact of the employment office fixed effects is being forced into the local unemployment rate. Table 6 shows the results of models that include both the local unemployment rate and its interaction with the treatment group dummy. In the case of women aged over 30, there is some (weakly determined) evidence in favour of the hypothesis that the treatment has less effect where unemployment is higher: the coefficient on the treatment dummy remains significant at the one percent level and the interaction with the unemployment rate is just significant at the five percent level. The hazard for a woman in the treatment group in an area with a 3½ percent unemployment rate is 2.02 times higher that for a woman in the control group in the same area (or another with the same unemployment rate).[19] This falls to 1.46 at a 5½ percent unemployment rate and to 0.82 at 9 percent unemployment. (These rates are about the bottom decile, median and top decile levels faced by the sample.) On the other hand, the unemployment rate itself is completely insignificant.[20]

5. Conclusions

We have investigated a neglected issue in analysis of unemployment benefit systems in Central and Eastern Europe – their administration. Evidence from LFS data on changes in search behaviour over time and on geographical variation suggest strongly that this is

[19] Given that we report hazard ratios in Table 6, this calculation is obtained as follows: $2.02 = 3.56*(0.85^{3.5})$.
[20] We also estimated a model in which the employment office effects were forced through a variable indicating the level of sanctions applied by each office, with this variable then interacted with the treatment dummy. However, we found no evidence that treatment had a larger effect in offices with a record of more frequently sanctioning claims.

an issue worth considering in Hungary. We focus on claimants receiving UI but the limited evidence to hand on means-tested assistance benefit shows that search behaviour and contact with the employment service may be even more important to consider for recipients of this form of income support.

We assessed the impact of changing the administration of UI with a randomized control trial which may be the first field experiment of this type in Central and Eastern Europe. The modest changes we were able to investigate – involving more frequent visits to local employment offices and questions about job search activity – had an effect only for women aged over 30. This effect was appreciable (although not very well determined) and appears higher for married than single women.

Finally, we emphasize that the impact of benefit administration on search behaviour is far from being the only issue of concern surrounding unemployment benefit in Hungary today. Not only has the proportion of benefit recipients who are ILO/OECD unemployed fallen over time but the proportion of ILO/OECD unemployed who receive any benefit (UI or assistance benefit) has also fallen considerably. Both aspects of the benefit system need attention.

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Appendix. Characteristics of the treatment and control groups of UI claimants

Variable	Treatment group	Control group
Female, %	51.8	50.9
Age, average	32.7	32.6
Educational level, %		
primary or less	30.4	30.7
vocational	34.7	35.6
vocational secondary	18.8	19.3
general secondary	10.0	8.3
college or university	6.1	6.1
Household variables		
Married, %	50.7	53.5
Spouse employed, %	31.4	34.0
Children aged 0-3, av. no.	0.08	0.08
Children aged 4-6, av. no.	0.13	0.11
Children aged 7-10, av. no.	0.18	0.18
Children aged 11-14, av. no.	0.15	0.17
Employed persons, av. no	0.77	0.78
Pensioners, av. no	0.28	0.28
Women receiving childcare allowance, av. no	0.09	0.09
Number of observations	1,113	1,019

Note: no differences between treatment and control groups are significant at the 5% level. (Differences in educational level are investigated with a single chi-squared test with five d.f.)

Table 1. Percentage of recipients of unemployment benefit who are classified as unemployed according to the ILO definition:

	1993	1996	1999	2002
Unemployment Insurance (UI)	69	63	54	55
Social Benefit (SB)	52	54	48	39
Men	71	64	54	48
Women	60	51	47	42
All benefit recipients	67	58	51	45

Source: Labour Force Survey microdata
Note: UI is a contributory benefit of fixed term duration. SB is a means-tested benefit restricted to those exhausting UI entitlement.

Table 2. Exits states from UI register

Exit state	Treatment group (%)	Control group (%)
Re-employment	23.9	22.8
Training	2.2	2.0
Other active measure	1.8	2.2
Disqualification	2.1	1.3
Claim ceased voluntarily	1.0	0.7
Other reason	0.4	0.4
Censored by UI exhaustion	46.3	44.5
Censored by experiment ending	22.5	26.3
Total	100.0	100.0
No. of observations	1,113	1,019

Table 3. Log rank test of difference in survivor functions between treatment and control groups

	Sample size	Log-rank test	Duration (days) at survival probability of 0.75		
		p-value	Control	Treatment	Difference
Men aged less than 30	503	0.312	98	95	3
Men aged 30 or older	534	0.578	105	105	0
Women aged less than 30	479	0.947	88	93	-5
Women aged 30 or over	615	0.076	102	85	17

Table 4. Model of the re-employment hazard (hazard ratios)

	Women		Men
	<30 years	30-49 years	
Treatment group	0.92	1.60	0.93
	(0.37)	(2.67)	(0.56)
Age	0.92	1.01	1.00
	(1.74)	(0.32)	(0.51)
Married	0.79	1.19	1.27
	(0.41)	(0.59)	(1.07)
Spouse employed	1.29	1.07	0.78
	(0.50)	(0.26)	(1.09)
No. of children aged 0-6	0.78	0.76	1.28
	(0.84)	(1.05)	(1.65)
Vocational school	1.39	0.95	1.20
	(1.15)	(0.26)	(0.83)
Vocational secondary school	1.36	0.64	1.28
	(0.67)	(1.42)	(1.04)
General secondary school	1.57	0.96	1.01
	(1.26)	(0.11)	(0.02)
College, university	4.25	2.46	1.75
	(3.30)	(3.09)	(1.69)
No. of observations	479	615	1037

Note: absolute values of t statistics in parentheses are from the test that the hazard ratio is equal to 1.0. Coefficients for the base-line hazard (dummy variables for different time intervals), the employment office dummies, and month dummies for calendar time are not reported. Standard errors take account of clustering of individuals in local employment offices.

Table 5. Interactions for marital status (hazard ratios)

	Women		Men
	<30 years	30-49 years	
Treatment group*Married	0.69	1.89	0.82
	(0.95)	(2.96)	(1.15)
Treatment group*Single	1.05	1.09	1.07
	(0.2)	(0.32)	(0.24)
Married dummy	0.98	0.86	1.44
	(0.04)	(0.52)	(1.50)
No. of observations	479	615	1037

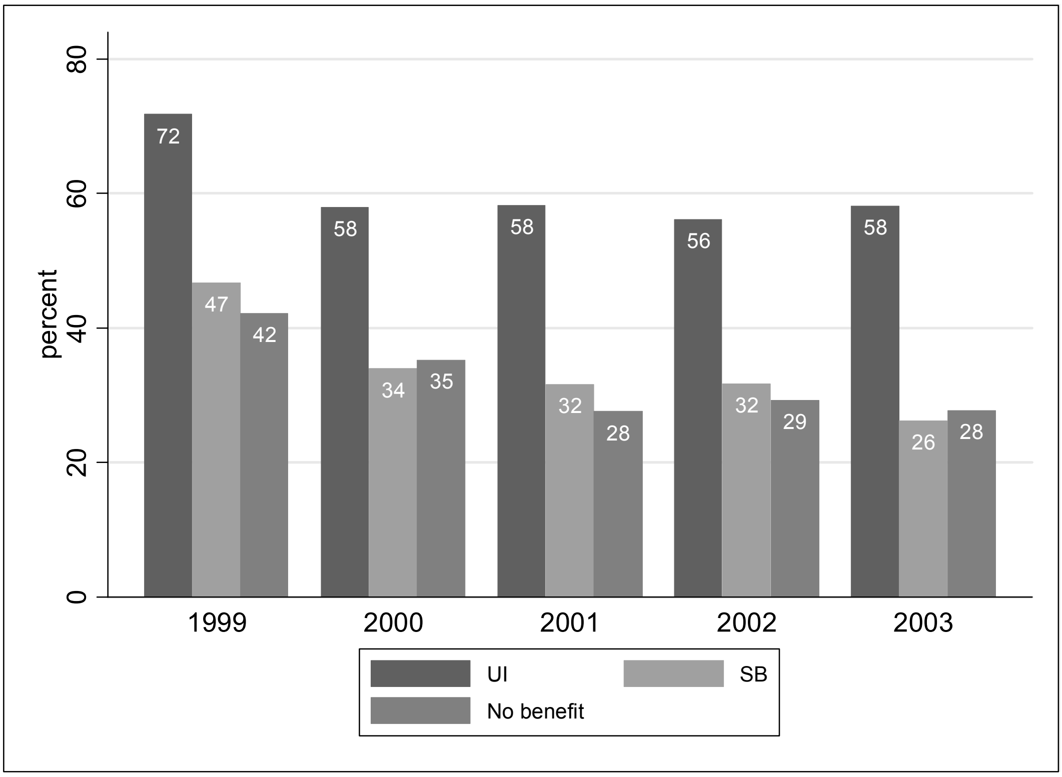
Note: The model is as in Table 4 with the addition of the interactions of the treatment dummy with marital status; absolute values of t statistics in parentheses are from the test that the hazard ratio is equal to 1.0.

Table 6. Interactions for local unemployment rate (hazard ratios)

	Women		Men
	<30 years	30-49 years	
Treatment group*local unemp. rate (%)	1.13	0.85	1.04
	(1.03)	(1.97)	(0.80)
Local unemployment rate (%)	0.94	1.03	1.06
	(0.43)	(0.71)	(1.38)
Treatment group dummy	0.48	3.56	0.74
	(1.08)	(2.61)	(0.77)
No. of observations	479	615	1037

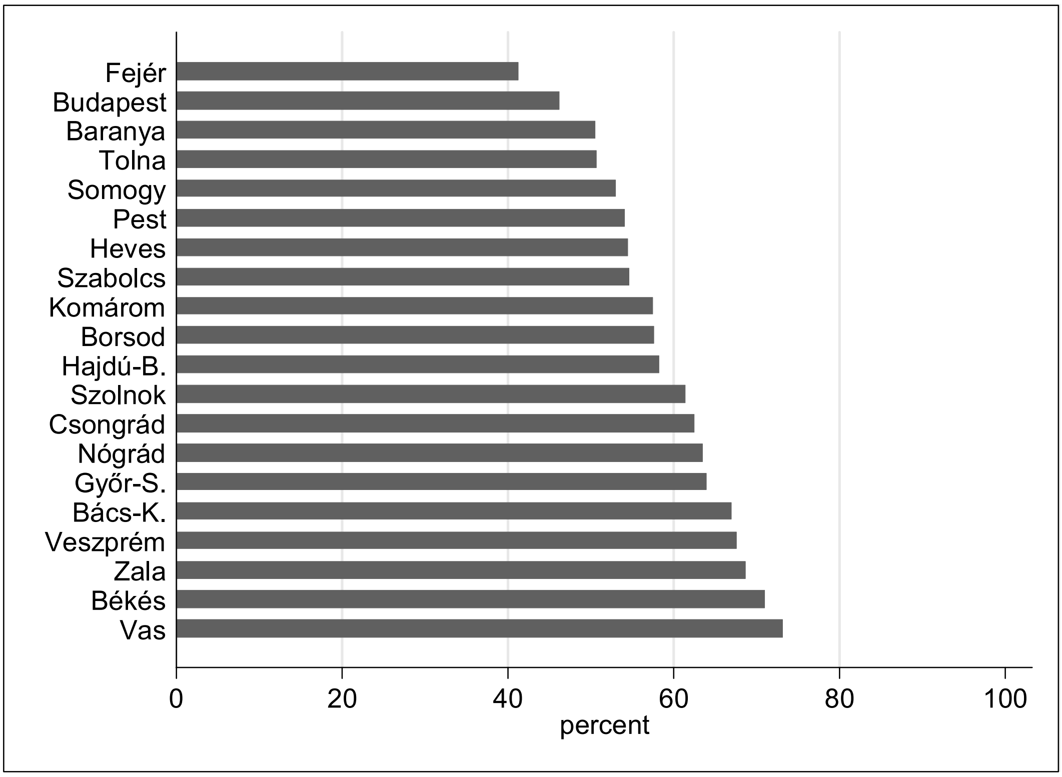
Note: The model is as in Table 4 with the addition of the local unemployment rate and its interaction with the treatment dummy and with the exclusion of local office fixed effects; absolute values of t statistics in parentheses are from the test that the hazard ratio is equal to 1.0.

Figure 1. Percentage of registered unemployed visiting an employment office within the last month



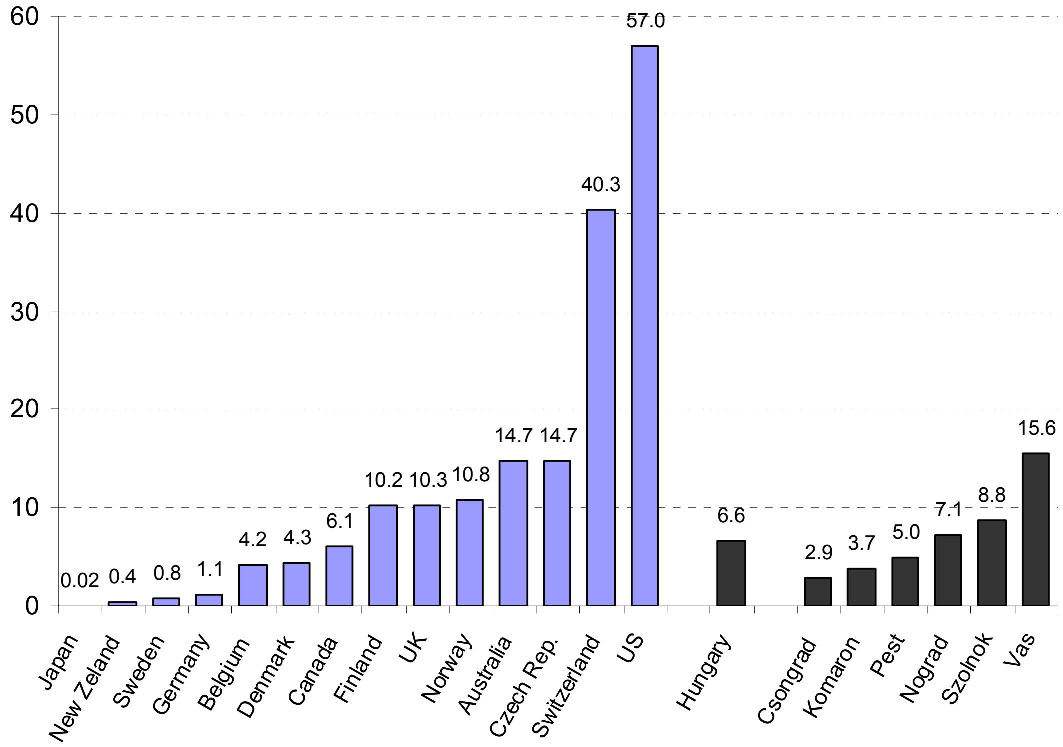
Source: Labour Force Survey microdata.
Note: UI is Unemployment Insurance, SB is Social Benefit.

Figure 2. Percentage of UI recipients visiting an employment office within last month by county, 2003



Source: Labour Force Survey microdata.

Figure 3. Sanctions and disqualifications of unemployment benefit for behaviour during claim (yearly figures) per 1000 persons in claimant stock



Source: Figures for Hungary are for 28 of the 40 employment offices included in the experiment described in Section 3. Figures for other countries are from OECD (2000 Table 4.2).

Note: Figures refer only to sanctions and disqualification applied during a period of unemployment to successful claims for benefit (loss of benefit due to voluntary quitting is not included). Hungarian figures refer to UI claimants only.

Figure 4. Survival in UI register, all men and women

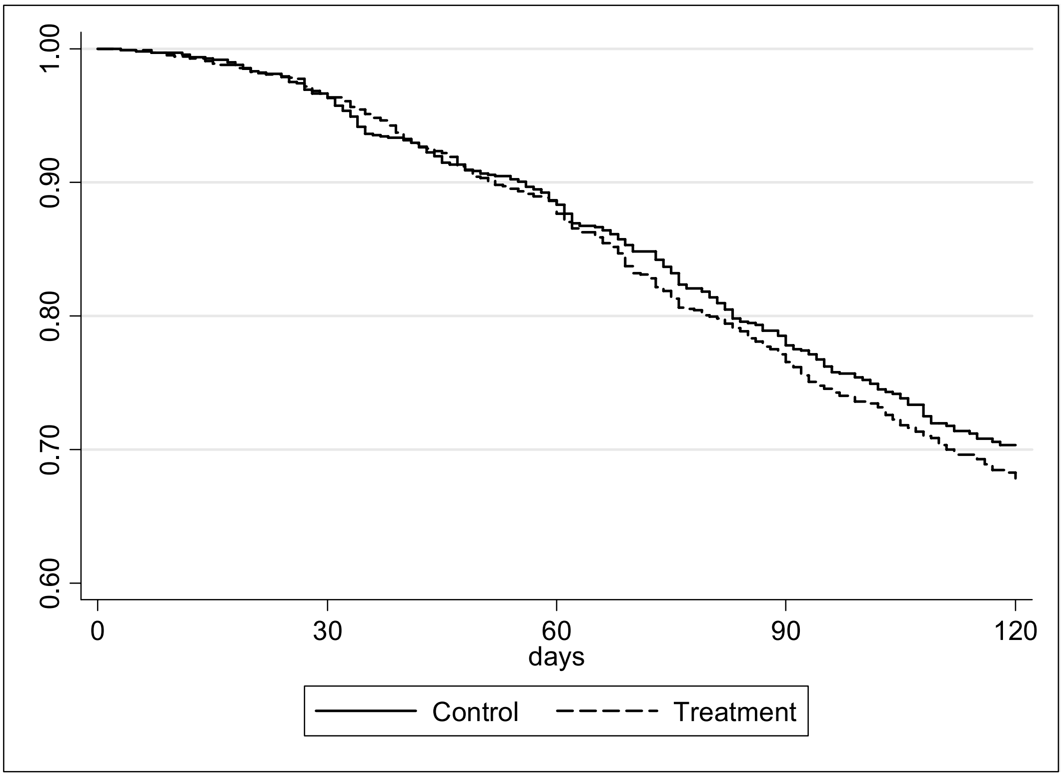


Figure 5. Survival in UI register, women aged 30 or over

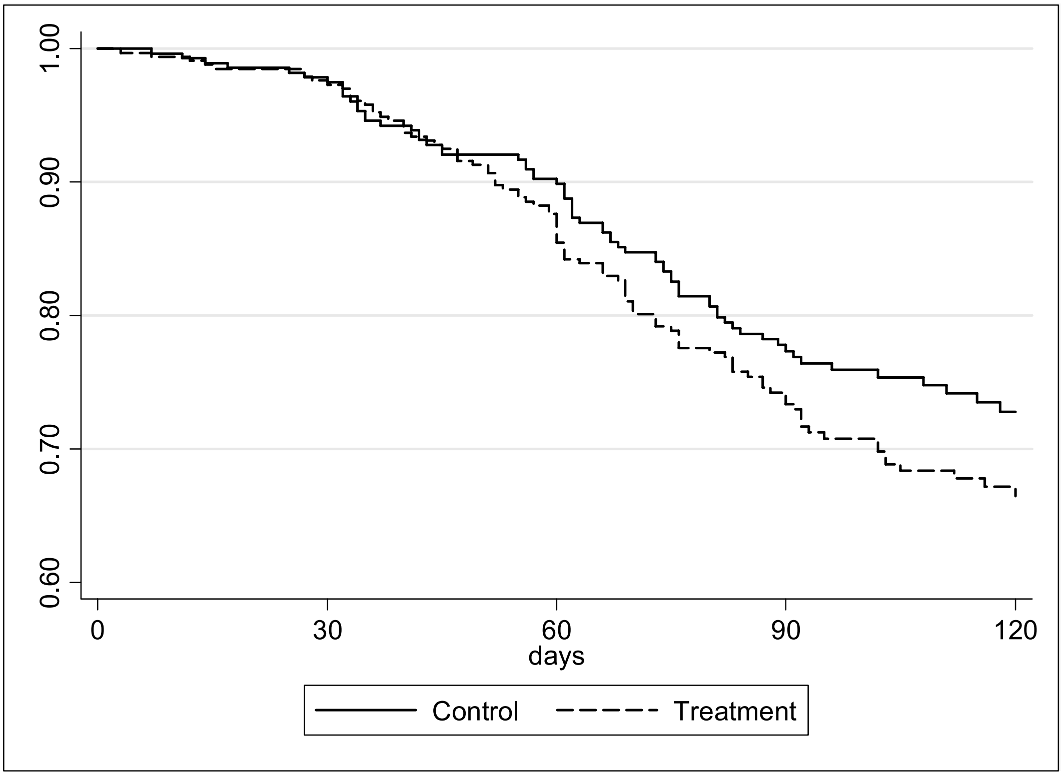


Figure 6. Hazard to exit to employment, women aged 30 or over