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ABSTRACT

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

The administration of benefits is a relatively neglected aspect of the analysis of disincentive effects of unemployment benefit systems. We investigate this issue with a field experiment in Hungary involving random assignment of benefit claimants to treatment and control groups, a method of policy evaluation that is still rare in Europe. Treatment, involving a tightening of claim administration, has quite a large effect on durations on benefit of women aged 30 and over, while we find no effect for younger women or men.

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Keywords: field experiment, Hungary, job search and unemployment insurance

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1. Introduction

The administration of benefits is a relatively neglected issue in the analysis of disincentive effects of unemployment benefit systems. Most research has focused on benefit generosity, whether in terms of levels of payment or lengths of entitlement. And yet it is argued that the administration of benefits is of crucial importance in determining the extent to which generous benefit systems actually influence unemployment in OECD countries (Nickell et al. 2005). New papers have shed more light on the impact of administration and on optimal design (e.g. Abbring et al. 2005, Boone et al. 2007). But as a recent survey of incentives in benefit systems noted ironically, the empirical evidence on the impact of job search monitoring 'is not overwhelmingly large' (Fredriksson and Holmlund 2006: 373). We add to this evidence by assessing the impact of monitoring on the duration of unemployment with a field experiment in Central Europe involving random assignment of benefit claimants to treatment and control groups.

Evidence from randomised control trials of unemployment benefit administration has grown in the USA, but is still very thin on the ground in Europe.[1] European benefit systems typically take different forms to those in North America and the evidence from the US trials cannot be relied upon to indicate the likely impact elsewhere of changing benefit administration. Our data come from Hungary. To our knowledge, there has been no research to date on job search monitoring in Central and Eastern Europe and no use in the region of randomised control trials of benefit programmes.

The absence of open unemployment in planned economies meant that income support for people searching for work in Central and Eastern Europe did not exist prior to the 1990s. The debate about the behavioural impact of the new benefit systems has been considerable but, as elsewhere, has focused on benefit levels and lengths of entitlements.[2] As economies contracted sharply in the early 1990s, the administration of

[1] Recent US evidence includes Ashenfelter et al. (2005) and Black et al. (2003). Earlier research is surveyed by Meyer (1995) and Fredriksson and Holmlund (2006). The small European literature includes the early work by Royston (1983, 1984) and Dolton and O'Neill (1996) for the UK, Gorter and Kalb (1996) and van den Berg and van der Klaauw (2006) for the Netherlands, and Graversen and van Ours (2006) for Denmark.
[2] See Boeri and Terrell (2002) for a summary. Examples include Ham et al (1998) for the Czech Republic and Slovakia, Micklewright and Nagy (1999) for Hungary, and van Ours and Vodopivec (2006) for Slovenia.

benefits concentrated on delivery of payments to claimants. The subsequent recovery, and hence greater availability of jobs, prompts more consideration of the monitoring of job search activity of benefit recipients. We report on a randomised control trial of benefit administration in Hungary in 2003.

First we shed light on the extent of monitoring of job search by benefit claimants in Hungary, making comparisons with other OECD countries (Section 2). Monitoring at the time of our experiment is revealed as light – and lower than in the 1990s. This provides the background and the motivation for the experiment, the design of which we then outline (Section 3). Our results show marked differences between the sexes in the effect of treatment on benefit duration and outflows to employment (Section 4). 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. We then interpret this finding (Section 5) and draw conclusions (Section 6).

2. Job search monitoring

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: 130): out of work, able to enter work at short notice, and undertaking active steps to find work. In the UK, a country that has tightened its administration of unemployment benefit considerably in the last two decades, at least 75 percent of the stock of benefit recipients in the Labour Force Survey (LFS) were classified as unemployed on the ILO criteria over 1993-2002.[3] Table 1 gives the situation in Hungary over the same period. The figure of two-thirds in 1993 was low to average compared to those for other Central European countries (Bardasi et al. 2001). And subsequently it fell substantially, to less than a half by 2002.[4]

Throughout the period, women with benefit were less likely to be available and searching for work than men. The same is true of other Central European countries in the Bardasi et al. comparisons and of the UK as well. 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

[3] Figure based on analysis of LFS microdata.
[4] Note that unemployment rates in Hungary and the UK moved in a similar way over the period, e.g.12.1 and 10.5 percent respectively in 1993 and 5.2 and 3.1 percent in 2002.

benefit (SB) provided by local government councils. (The latter can be paid if a person has insufficient contributions for UI or has exhausted entitlement.) But it is clear that there has been a change over time for claimants of both benefits. (SB and UI have been of roughly equal importance in the claimant stock since 1996.) The literature on monitoring search in other countries tends to focus on UI.[5] For practical reasons explained below, the same is true of our experiment.

Restricting benefit to just the ILO unemployed can be tried through various forms of monitoring. 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 provided on possible opportunities). Table 2 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 relevant question was not asked before 1999.) The offices are responsible for both administration of benefits and for matching of the unemployed to suitable registered vacancies.[6] The figure is again much lower for claimants on SB: only 1 in 4 recipients in 2003 had been to an employment office in the previous month – about the same as among the unemployed receiving no benefit at all. But even for UI recipients the figure was below 60 percent.

Face-to-face interviews are rarely used by the public employment service in many US states. Instead, they 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 shown in Table 2 in the percentage of claimants with recent visits to local employment offices is noteworthy. This coincided with new legislation requiring UI claimants to make visits at least once every three months. Existing law had not stipulated how often visits should be, merely saying that they should be ‘regular’, and their frequency had been left to offices’ discretion. Far from tightening benefit administration as had been intended, the effect of the change 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).

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[5] This is also the case for the literature on benefit levels and durations (Atkinson and Micklewright 1991).

[6] The offices place about a third of UI recipients who exit the register to a job (figures are not available for SB recipients), which shows that their matching role is not trivial.

Unlike the USA and the UK, most Continental European countries do not require frequent reporting of independent job search activity. In fact no such reporting was required in Hungary until 2005. Prior to this date, UI claimants had only to register with their local employment office and then return regularly to continue to declare their availability. They needed to keep no records of employers contacted or of other efforts to find a job and no checks were made of search activity during visits to the employment office.[7] Monitoring is even lighter for unemployed SB claimants; whether or not they are required to report regularly to an employment office is at the discretion of each local government (and no information exists on the range of different practices).

The frequency at which UI claimants 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 into 20 counties. 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. Of these offices, 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. 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 is reflected in LFS data on the percentage of UI recipients in each county who have visited an employment office in the last month (the data should also reflect regional variation in claimants' search behaviour). Figure 1 shows that the 2003 figures ranged from about 40 percent to over 70 percent. (Much lower figures for SB recipients, not shown, are strongly correlated with those for UI claimants.)

Monitoring of any type will only be effective if there is a credible threat of sanctions in the event of failure to comply. Sanctions for on-going UI claims in OECD countries typically involve suspension of payments for a fixed period or outright

[7] The USA 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.

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).[8]

Sanction rates in practice for the six counties covered by our experiment are shown in Figure 2. 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. These vary greatly and, once more, there is also substantial regional variation within Hungary. The county of Vas sanctions 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.[9]

The *prima facie* evidence suggests that administration of unemployment benefits in Hungary has been weak and was weakened further in 2000. Against this background we now describe our experiment.

3. The experiment

Claimant behaviour suggested by LFS data and the existing state of monitoring practices in Hungary had several implications for the experiment's design.[10]

First, the main instrument of monitoring in Hungary is the requirement on the claimant to report regularly to the local employment office. Policy was inadvertently relaxed in 2000 and an obvious choice was to explore its tightening. Second, at the time

[8] 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.
[9] 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.
[10] The experiment was planned by us in conjunction with the National Labour Centre. It was then adopted as a formal initiative of the Centre.

of our experiment offices rarely asked about job search activity (and never formally monitored it), questioning claimants on this subject was, again, an obvious measure to trial. Third, although the administration of SB seems even more in 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, LFS data show women to be less likely to search and to be available for work than men, and we therefore wanted to cover both sexes in the experiment. Finally, the marked heterogeneity across the country in monitoring practices implied that we had to choose between comparing the effect of treatment against this varied status quo and comparing against a homogenised control 'regime'. We chose the latter, in the hope of obtaining a cleaner estimate of the treatment effect.

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 significant principal-agent problem: any experiment must be 'do-able' in the sense of allowing agents to carry it out conscientiously. Our experiment also had to be 'do-able' in the legal sense, with treatment and sanctions permitted by existing law.

Treatment and outcomes

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 with office clerks asking questions on job search behaviour since the last visit.

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 contacted, and reasons for lack of employer contact if none had taken place. Sanctions for failure to come to an interview remained as before (including any local variation in practices suggested in Section 2). Although no additional sanctions were applied to those who reported no search, claimants would have been uncertain as to the implications of failure or repeated failure to search. (Even claimants who had been recently unemployed would not have perfect knowledge of current regulations and office practices.) Note that a sanctioned UI claimant cannot receive means-tested SB.

The outcomes that we observe are (i) time unemployed as measured by duration of UI receipt (and hence censored if UI entitlement exhausts) and (ii) exit state (job, training scheme, death etc) if the spell finishes. Knowing the exit state is in principle an important advance over knowing only the duration of claims since treatment might encourage exit from the labour force entirely rather than to work. We do not observe wages in post-unemployment jobs or any other aspect of that job, such as the duration of employment. Hence we cannot measure the effect of treatment on the quality of job matches.

What effects do we hypothesise treatment to have on the measurable outcomes? The small literature that has developed theoretical models of monitoring shows that the threat of sanctions increases search effort (Fredriksson and Holmlund 2006). We expect more frequent contact with the employment office to underline the link between receiving benefits and looking for work and to disrupt any activity in the hidden economy thus reducing 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, which should reduce the reservation wage. The increased frequency of visits to the employment office also raises the claimant's exposure to a major source of vacancies. (The offices place about a third of UI recipients who exit the register to a job.) We therefore hypothesise that treatment results in a higher exit rate to jobs from the UI register. However, we cannot rule out that treatment could stimulate search without any impact on job exits. The additional search may not be sufficient to generate job offers, due to weak local labour demand or because it is merely token activity. Finally, exits to inactivity could also increase, where individuals decline to search but decide to cease claiming benefit.

Data collection

The experiment began in late April 2003. It covered all new UI claimants who had 75-179 days of UI entitlement who registered in the following three months in six selected 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. Each claim was monitored for a maximum of four months, implying that claimants in the treatment group made a maximum of four 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 one and three months and with no questions on job search asked at these visits. Claimants were unaware of the existence of the experiment and issues relating to recruitment and drop-out do not arise.

Claimants were allocated to treatment or control groups on the basis of their birthdays – odd days of the month to treatment, even days to control – which amounts to random assignment. In principle, individuals in the two groups could have talked to each other and discovered 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 fewer than 180 days of entitlement was made in order to avoid persons eligible for an extended UI scheme introduced in 2003 just as our experiment was due to begin. Since all aspects of the scheme's workings were unknown at the time our experiment went into the field we judged it sensible to exclude those eligible for the benefit extension. 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

[11] We excluded persons aged 50 or over on account of their proximity to retirement age.
[12] We believe 'contamination' between treatment and control groups did not occur, at least formally (we monitored the assignment based on date of birth). However, if claimants in the control group talked with those in the treatment group and as a result felt increased pressure to search, our estimates should provide a lower bound on the impact of the treatment.

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 unemployment, or have joined the labour force during this time. About two-thirds of those aged 30 and over (of either sex) in the sample had 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 counties 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 managers 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.

The sample of claimants 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 difference between the two groups is significant at the 5 percent level. And although the sample has a slight majority of claimants, we cannot reject the null hypothesis of correct assignment.[14]

4. Results

Table 3 shows the exit states from the UI register for treatment and control groups. More than two thirds of spells of unemployment were censored, either due to the ending of the period of the experiment or because the individual exhausted entitlement to UI.[15] There are only small differences between the distribution of the two groups across other

[13] Considerations of this type seem also to have influenced selection of employment offices in the Dutch experiment analysed by van den Berg and van der Klaauw, which was restricted to two offices with 'a good reputation for carrying out counselling and monitoring activities in a highly orderly fashion' (2006: 909).

[14] Note that a year contains more odd than even numbered days of the month (and in any case we were able to monitor the actual assignment based on birth date). We also tested for differences in characteristics between treatment and control groups within 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] This reflects the low outflow rate from unemployment in Hungary and other Central European countries (Boeri and Terrell 2002, Micklewright and Nagy 1999).

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 Kaplan-Meier estimates of survival in the UI register (Figure 3). 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.

The picture changes when we disaggregate by gender and age: see Table 4. 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 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 is found, with those in the control group leaving more quickly, but the difference in survival functions is completely insignificant.) Figures 4 and 5 show 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. Overall, the raw data show 30 percent of women of this age in the treatment group leaving to jobs compared to 23 percent of those in the control group.

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 characteristics additional to those explored in the earlier graphical analysis, i.e. whether there are interactions. 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 calendar 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, $\boldsymbol{X}_i$ are other observed characteristics we control for, $\boldsymbol{O}_e$ is a vector of employment office dummies, and $\boldsymbol{Z}_t$ pick up real time effects. We model *g(s)*, the base-line hazard, with an exponential function of a series of dummy variables for each two-week interval that turn on and off as the individual moves through a spell of unemployment (following Meyer 1990). The function *f* is also specified as exponential. Hence:

$$h_{iest} = exp(g(s) + \alpha T_i + \boldsymbol{\beta X}_i + \boldsymbol{\gamma O}_e + \boldsymbol{\delta Z}_t).$$

This includes dummy variables for calendar time, $\boldsymbol{Z}_t$, namely months of the year, allowing the hazard to change directly with real 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 5. However, to estimate the model we condition on survival until the initial interview at the employment office. This is because 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. From that point onwards, 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 5 in the form of hazard ratios. 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 insignificantly different from zero, as in the earlier graphical analysis. 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.[16]

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 6 and 7 test for variation of the treatment effect with individual and local characteristics. Table 6 investigates whether the impact of treatment differs for married people (marital status itself has no association with the hazard in Table 5). 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. 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 7 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

[16] 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).

into the local unemployment rate. Table 7 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).[17] 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.[18]

5. Discussion

The effect of treatment appears to be appreciable for women aged over 30, and may be higher for married than for single women. But it is insignificant for men and young women. How should these results be interpreted? And is the finding of a gender difference in line with existing literature?

There are three alternative explanations for the results from the experiment. First, claimants of all ages and both sexes raise their search effort as a result of treatment, but the search effort of men and younger women is already high and the marginal return to their additional search effort is zero. Second, monitoring in the treatment is not binding for men and younger women and no additional search results. Their search effort is already high and the questions faced by the treatment group during visits to the employment office are answered with equanimity, with no disutility resulting. Men and younger women in the control group make frequent visits to the employment office of their own volition in order to access the vacancies on offer, so their contact with the employment office is no lower than for their counterparts in the treatment group. Third, treatment does in practice bring increased frequency of contact

[17] Given that we report hazard ratios in Table 7, this calculation is obtained as follows: $2.02 = 3.56*(0.85^{3.5})$.

[18] 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.

with the employment office but this of itself does not result in greater job search among the men and younger women. Only the women aged over 30 take advantage of the increased access to information on vacancies and only these women experience disutility from the questioning about job search and perceive a threat of sanctions if they do not increase their search activity.

We cannot conclude with certainty which of these explanations is correct. We do not have the detailed information on actual search activity of both the treatment group *and* the control group that would allow us to dismiss or to accept any explanation.[19] However, we do have information on reported search at the visits to the employment office for the treatment group (only) and we also have the background analyses of the LFS reported in Section 2. The LFS data suggest search behaviour of men to be greater than that of women (see Table 1) but do not support the hypothesis that search among men is high per se. Further analysis of the data showed women aged over 30 receiving UI to be less likely to be classified as unemployed on the ILO criteria than both men and younger women.

The data on search reported by the treatment group during the experiment show a mixed picture. Table 8 provides a summary of the situation at the first requested visit to the employment office (that is three weeks after the initial registration interview), when the treatment group were questioned about their search activity for the first time. It also summarises the change between the first and second visits for those still unemployed (which is great majority given the low outflow rate). Around 90 percent of men and younger women reported at least one method of search in the previous three weeks. This seems out of line with the lower levels of search implied by the LFS data but the specific nature of the sample in the experiment in terms of employment history and the timing (very early in the unemployment spell) need to be borne in mind. The figure for women aged 30 or over is also high, but somewhat lower than for the other groups – 81 percent. (The difference in the figure from that for the rest of the treatment group is significant at the one percent level.) The percentage reporting three or more methods differed little between the four groups. A somewhat higher percentage of men than women reported having actually visited an employer. (The figures are very similar for those being able to name an employer visited.) Among claimants still unemployed at

[19] In retrospect the experiment should have involved a more rigorous 'process' evaluation that attempted to uncover more about how the observed differences in outcomes came about. As well as quantitative data on search behaviour from the control group to compare with information on the treatment group, one would want more qualitative data from employment offices on the conduct of the experiment.

the second three-weekly interview, the overall percentage reporting a visit to an employer actually fell for men while rising for both younger and older women by three points and five points respectively. On the other hand, the percentage reporting three or more methods of search rose by only one point for women aged over 30 but by as much as ten points for young men.

On balance, we think this pattern is more consistent with the third explanation offered above than with either of the first two. Note that this third explanation hypothesises effects from both the increased visits to the employment office and the questioning during these visits. However, we cannot conclude which of these two elements had the greater impact or indeed whether one had no impact at all – a result of the experiment having bundled together two different changes to monitoring.

How do these results accord with the existing literature? The finding that women react more to treatment is in line with the general picture from the literature of differences between the sexes in the effects of labour supply policies. And specifically in the case of job search monitoring, Martin and Grubb (2001) and Bergemann and van den Berg (2006) both conclude in their reviews of the evidence that monitoring has a greater impact on the behaviour of women. At the same time, the evidence is not extensive – as noted in the introduction to this paper – and not all analyses report any investigation of gender differences.[20] Bergemann and van den Berg's review for European countries covers only three studies, which in fact show a very mixed pattern of results. While our results appear to support the existing view, that view seems to be based on rather scanty evidence.

6. Conclusions

We have investigated a relatively neglected issue in the literature on the effects of unemployment benefit in OECD countries: their administration. We showed that less than half of benefit recipients were unemployed by the ILO criteria in 2002 in the country on which we focus, Hungary, where – as elsewhere in Central and Eastern Europe – there has been no research on benefit administration to date. Our analysis was based on a randomized control trial which is probably the first field experiment of a

[20] For example, differences between men and women are not reported in Ashenfelter et al. (2005) and Black et al. (2003).

benefit programme in the region and one of only few on the administration of unemployment benefit anywhere in Europe.

The experimental treatment, involving more frequent visits to local employment offices and questions about job search activity, had an effect only for women aged over 30. The finding of a greater impact for women appears to support an emerging picture from other studies. But the canvas still lacks much paint and we suggest that future research pays more attention to the issue of gender differences in the effects of job search monitoring. Our own evidence for Hungary relates to a specific group of claimants in terms of employment histories who were all receiving UI in the early part of their unemployment spells. The administration of means-tested assistance benefit (which is typically received much later in a spell of unemployment) needs to be investigated further, something true in many other countries as well.

Since our experiment was conducted in 2003, the Hungarian government has changed aspects of the administration of unemployment benefit in ways that partly reflect features of our experiment. From 2005, UI claimants have been required to sign a contract when claiming benefit, agreeing to search for work and to report search activity when visiting employment offices. Reflecting this emphasis, UI has been renamed "Job Seekers' Benefit" (as in the UK for example). However, monitoring of search behaviour does not seem to be strict; as in our experiment, the offices' questioning appears rather a 'paper tiger'. Notwithstanding, LFS data record a rise in the percentage of UI recipients who are classified as unemployed according to the ILO criteria. The figure for 2005 was 66 percent, compared to 55 percent in both 2002 (see Table 1) and 2003-4. The rise has been particularly notable for women, with the percentage reporting search and availability for work in 2005 no different from the figure for men. While it is impossible to conclude from this evidence that the apparent increase in search behaviour was a result of the policy change (or even whether search, as opposed to the reporting of search, actually increased), it does underline the 'live' nature of policy surrounding benefit administration in Central Europe and the possible differences between men and women that we have investigated.

Finally, and to be even-handed, we should note that the administration of benefits is not the only matter of concern surrounding income support for the unemployed in Central Europe. There is also the question of benefit coverage. The proportion of ILO unemployed in Hungary who *do* receive any benefit (UI or assistance benefit) fell from around 60 percent in the early 1990s to little more than a third ten years later, a level it has stayed at since. A change in the composition of the unemployed pool as unemployment fell may be one factor. But the main explanation is legislative change, in particular sharp cuts in entitlement periods to UI. Most people that are unemployed in Hungary according to the ILO criteria therefore receive no benefits. This underlines that issues of living standards as well as incentives need to be considered in the policy debate on unemployment benefits.

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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: Hungarian 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. Percentage of registered unemployed visiting an employment office within the last month

	1999	2000	2001	2002	2003
Unemployment Insurance (UI)	72	58	58	56	58
Social Benefit (SB)	47	34	32	32	26
No benefit	42	35	28	29	28

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 3. Exit 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 4. 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 5. 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 shown 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 6. 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 5 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 7. 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

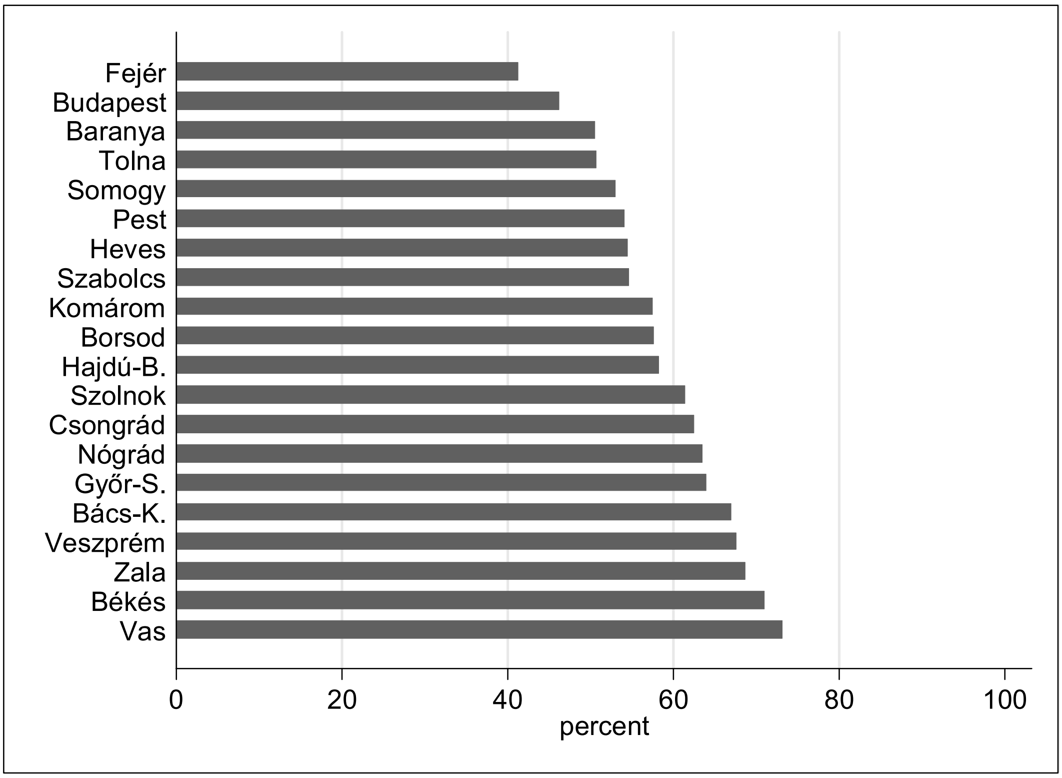
Note: The model is as in Table 5 with the addition of the local unemployment rate and its interaction with the treatment dummy and with the exclusion of local office fixed effects; absolute values of t statistics in parentheses are from the test that the hazard ratio is equal to 1.0.

Table 8. Search reported by the treatment group, 1st and 2nd visits

Sex and age-group	used any method	1st visit (%) used 3+ methods	visited an employer	change 1st to 2nd visit (% points) used 3+ methods	visited an employer
Men <30	87	57	56	+10	–4
Men ≥30	90	62	56	+7	–4
Women <30	88	64	49	+6	+3
Women ≥30	81	61	48	+1	+5

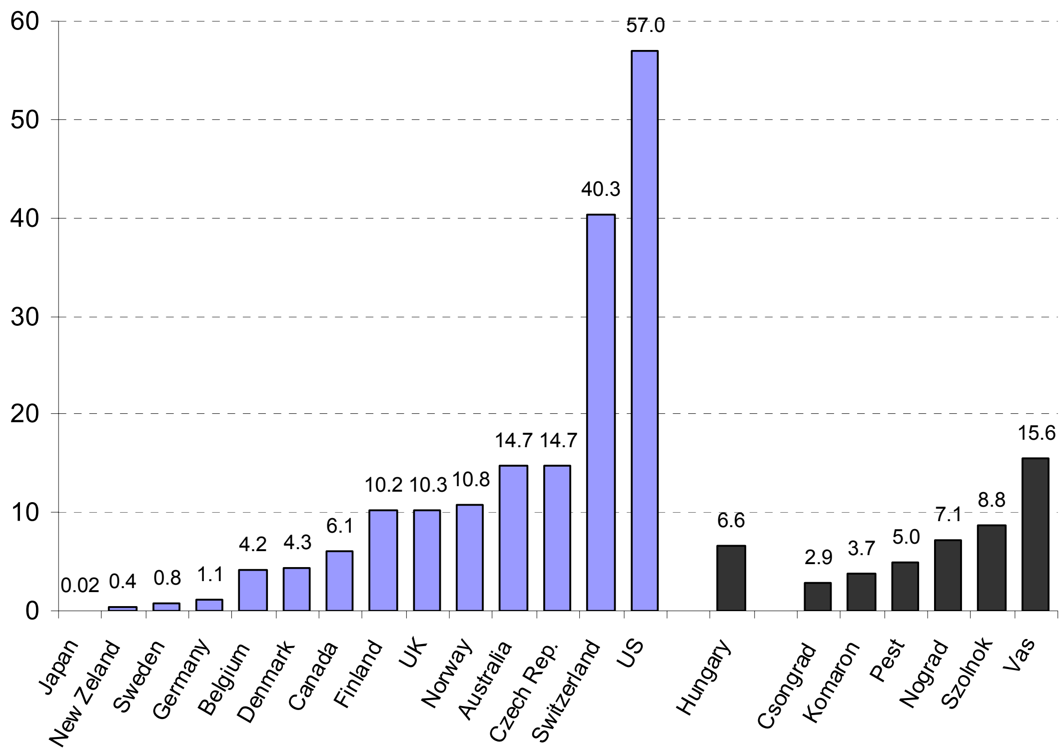
Note: the first three columns relate to all persons in the treatment group unemployed at their first interview three weeks after registration. The last two columns relate to those persons still unemployed at their second interview.

Figure 1. Percentage of UI recipients visiting an employment office within last month by county, 2003



Source: Labour Force Survey microdata.

Figure 2. Sanctions and disqualifications of unemployment benefit for behaviour during claim (yearly figures) per 1000 persons in claimant stock



Source: Figures for Hungary (dark bars) are for 28 of the 40 employment offices included in the experiment described in Section 3 and are averages for 2000, 2001 and the first six months of 2002. Figures for other countries (light bars) 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 3. Survival in UI register, all men and women

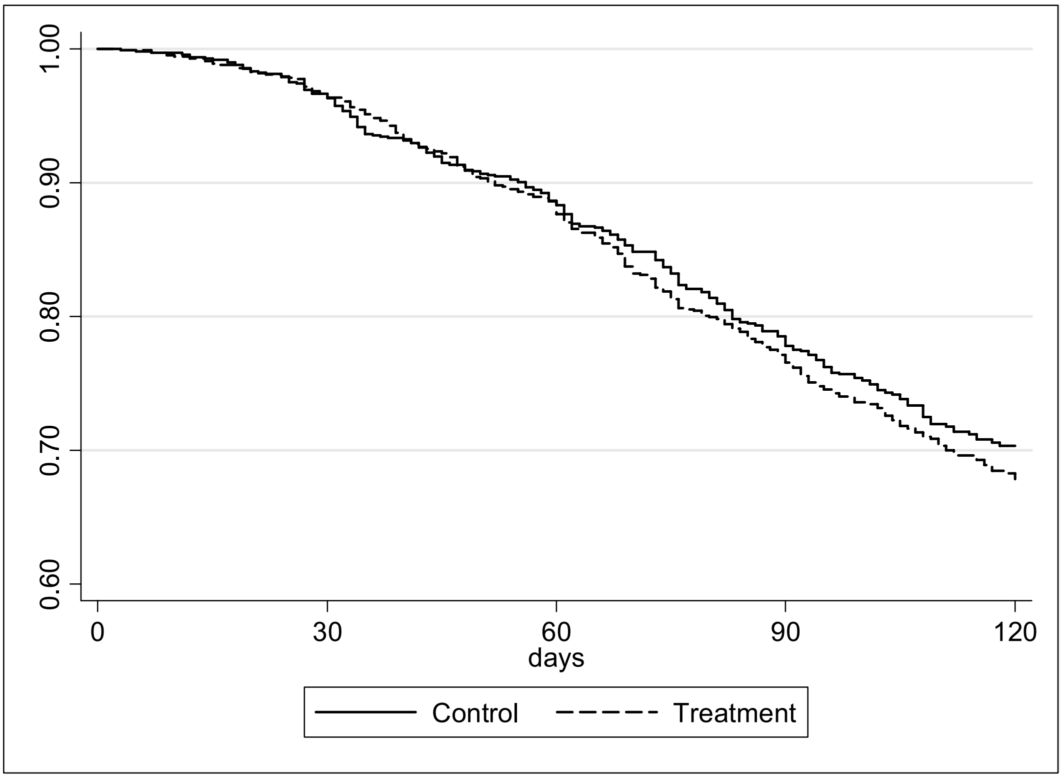


Figure 4. Survival in UI register, women aged 30 or over

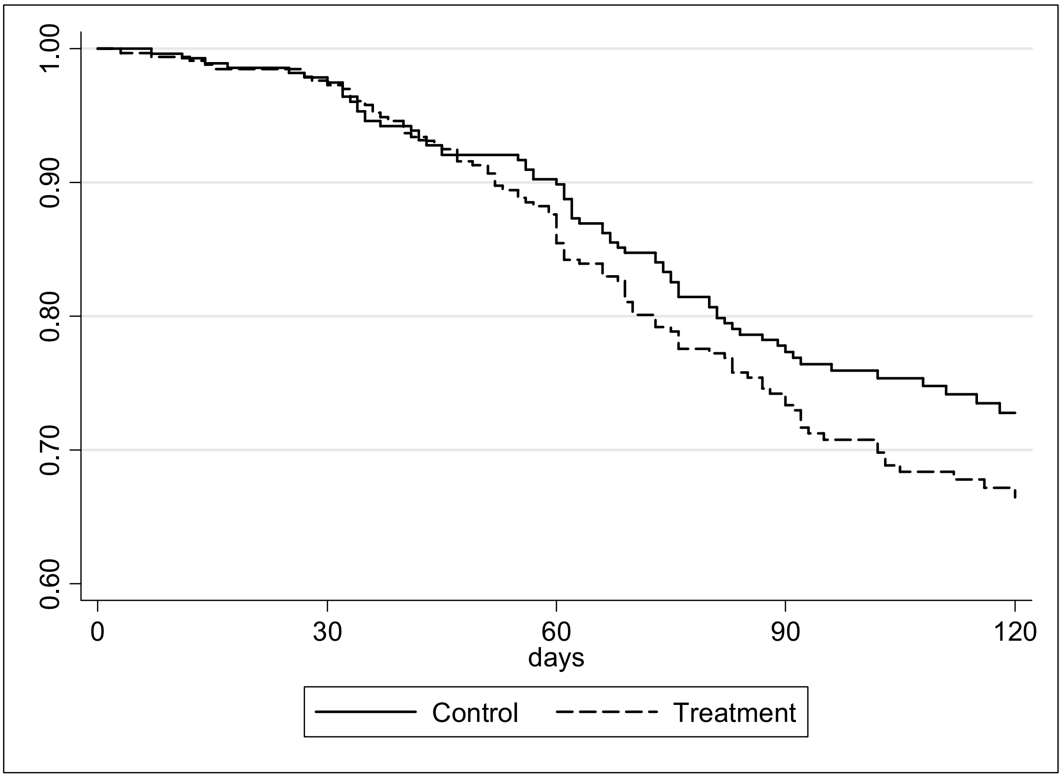


Figure 5. Hazard to exit to employment, women aged 30 or over