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# The effect of monitoring unemployment insurance recipients on unemployment duration: evidence from a field experiment

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**Abstract.** Programme administration is a relatively neglected issue in 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. Treatment increases the monitoring of claims — claimants make more frequent visits to the employment office and face questioning about their search behaviour. 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 men.

**JEL classification:** J64, J65, P23.

**Keywords:** field experiment, monitoring, job search, unemployment insurance, Hungary.

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## 1. Introduction

Much of public economics does not consider the details of the administration of benefit programmes. The focus is on conditions for qualification, levels of payment, and lengths of entitlement. But how a programme is delivered in practice may be critical for its impact on individuals' behaviour. In the case of unemployment benefits, programme administration has been argued to be of crucial importance in determining the extent to which generous benefit systems actually influence unemployment in OECD countries (Nickell et al. 2005). However, the empirical evidence on the impact of benefit administration on getting people back to work is still limited. We add to knowledge by evaluating experimentally a simple change in administration of unemployment insurance (UI) that has the potential for a substantial impact on unemployment duration. Our field experiment uses a randomised control trial.<sup>1</sup>

The experiment was conducted in Hungary in 2003. 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 levels and lengths of entitlements.<sup>2</sup> As economies contracted in the early 1990s, the administration of benefits concentrated on delivery of payments. The subsequent recovery, and hence greater availability of jobs, prompts more consideration of benefit administration and the monitoring of job search activity.

Section 2 provides background to our experiment and describes its design. Monitoring of claims prior to the experiment was light – and lower than in the 1990s. Treatment in the experiment increased the monitoring of claims – claimants made more frequent visits to the employment office and faced questioning about their search behaviour. Randomisation was achieved by assigning claimants to treatment or control on the basis of date of birth. Section 3 reports results which show marked differences

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<sup>1</sup> Evidence from randomised control trials of unemployment benefit administration has grown in the USA, but is still thin on the ground in Europe. Recent US evidence includes Ashenfelter et al. (2005), Black et al. (2003) and Klepinger et al (2002). 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 (see also quasi-experimental evidence in McVicar 2008), Gorter and Kalb (1996) and van den Berg and van der Klaauw (2006) for the Netherlands, and Graversen and van Ours (2008) for Denmark.

<sup>2</sup> 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.

between the sexes in the effect of treatment on benefit duration and outflows to employment. Treatment has quite a large effect on women aged 30 and over, while we typically find no effect for younger women or for men. Section 4 interprets this finding and Section 5 draws conclusions.

## **2. UI administration and the experimental design**

### *2.1 Background to the experiment*

Administration of unemployment benefit typically attempts to restrict benefit to people who are unemployed on the standard ILO definition (OECD 2000: 130): out of work, able to enter work at short notice, and undertaking active steps to find work. Labour Force Survey (LFS) data for Hungary show over two-thirds of UI claimants classified as unemployed on these criteria in 1993 (a low to average figure for Central Europe at that time – Bardasi et al. 2001), but only a half in 2002. Throughout the period, women with benefit were less likely to be ILO unemployed than men.<sup>3</sup>

Various methods of monitoring can be used to restrict benefit to the ILO unemployed. One is to require claimants to report periodically for face-to-face interviews in which information is sought on job search activity and is provided on possible opportunities. LFS data show the proportion of UI claimants in Hungary who had visited a public employment office in the previous month. The offices are responsible for both administration of benefits and matching claimants to suitable registered vacancies. The relevant question was first asked in the LFS in 1999, when 72 percent of UI claimants had visited an office in the month prior to interview. But throughout 2000-3 the figure was below 60 percent. The fall coincided with new legislation requiring claimants to make visits at least once every three months. Existing law had required visits to be ‘regular’, with the frequency left to offices’ discretion. Far from tightening administration, the new law seems to have led many offices that had

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<sup>3</sup> See Micklewright and Nagy (2008, Table 1), where we also give the (lower) search figures for recipients of means-tested Social Benefit, which is available following UI exhaustion. Roughly equal percentages of the unemployed receive the two benefits. We also note that the percentage of the ILO unemployed receiving neither benefit rose substantially over 1993-2003.

required more frequent visits to take the three month period as standard (a conclusion borne out by our discussions with employment office staff).<sup>4</sup>

The frequency at which claimants had to return to employment offices prior to our experiment differed across the country.<sup>5</sup> The Hungarian public employment service is organised into 20 counties. Each county has considerable discretion to interpret legislation as it sees fit. Practice also varies from office to office within counties. 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 the experiment was to be conducted. In 16 offices, claimants were required to return every three months. In six offices the frequency was once a month and in the remaining six somewhere in between. (In all offices claimants could be contacted at any time and asked to attend in person to receive information on a vacancy that the office deemed suitable.) This variation is reflected in county-level differences in the percentage of claimants who had visited an employment office in the last month, recorded in LFS data. The 2003 figures for all 20 counties ranged from about 40 percent to over 70 percent.

At the time of the 2003 experiment, no reporting by UI claimants of job search activity was required in Hungary. Claimants had only to register with their local employment office and then return regularly to continue to declare their availability for work. They needed to keep no records of employers contacted or of other efforts to find a job. No checks were made of search activity during visits to the employment office.

Monitoring will be more effective if there is a credible threat of sanctions following failure to comply. Sanctions for on-going UI claims in OECD countries typically involve suspension of payments for a fixed period 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 a job interview).<sup>6</sup>

Figure 1 shows sanction rates in 2002 for the six counties in the experiment. The data refer to all suspensions and disqualifications of on-going claims, and rates are expressed as the annualised number as a percent of the average UI stock. We compare

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<sup>4</sup> Hungary has little monitoring other than face-to-face interviews, e.g. claimants' postal or phone reports of job search activity, as used in many states in the USA (Andersen 2001).

<sup>5</sup> Substantial within-country variation in monitoring is found in many other countries (OECD 2000).

<sup>6</sup> 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.

the figures 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 sanctioned claims at the same broad level as Australia and the Czech Republic, which are among the tougher OECD countries in the graph, 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. Higher rates may reflect more frequent behaviour in need of sanction rather than a stricter application of the rules. We think the latter to be the case in Hungary. This leads us to expect that the impact of treatment in the experiment may vary geographically due to differences in the culture of sanctioning.<sup>7</sup>

The *prima facie* evidence therefore suggests that administration of UI in Hungary in 2003 had been weak since the early 1990s and weakened further in 2000.

## 2.2 Design of the experiment

The experiment began in late April 2003, covering new claimants registering in a three month period in six selected counties.<sup>8</sup> Claims were monitored for up to four months. The six counties contained 48 employment offices (28 of which were included in the investigation of office practices described earlier). Counties were chosen partly to give a mix of labour market conditions and existing rigour in UI administration and partly because they had employment service managers who we believed would oversee the experiment appropriately.<sup>9</sup>

Claimants were included in the experiment if they were aged below 50 and with 75-179 days of UI entitlement. Older claimants were excluded due to their greater proximity to retirement age (55 for women and 60 for men at the time). The restriction to those with at least 75 days of entitlement was to avoid UI claims that would be short by definition. The restriction to less than 180 days of entitlement was to avoid persons eligible for an extended UI scheme introduced in 2003 just as the experiment was due to begin. All aspects of that scheme's workings were unknown at the time and we judged it sensible to exclude claims eligible for extension. The drawback of these restrictions is

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<sup>7</sup> 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.

<sup>8</sup> The experiment was planned with the National Labour Centre and then adopted as a Centre initiative.

<sup>9</sup> Considerations of this type also 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).

that the experiment applied to a group with a specific employment history: claimants between 1 and 2½ years of insured employment in the four years prior to claim. They had either had periods out of work, e.g. due to previous unemployment, or had joined the labour force during this time. About two-thirds of those aged 30 and over in the sample had had a previous spell of UI during the four years and somewhat less than half of those under 30. Claimants satisfying the sample restrictions were divided into treatment and control groups at the outset of their claims on the basis of their birthdays – odd days of the month to treatment, even days to control. This amounts to random assignment. (We observed date of birth and could monitor assignment.)

Claimant behaviour suggested by LFS data and the existing state of UI administration had implications for the experiment's design. First, the main instrument of monitoring was the requirement to report regularly to the local employment office. Policy was inadvertently relaxed in 2000 and an obvious choice was to explore its tightening. Second, offices rarely asked about job search activity, so questioning claimants on this was again a natural measure to trial. Third, women appeared less likely to search and be available for work than men, and hence we included both sexes in the experiment. Fourth, the heterogeneity across the country in UI administration implied a choice between comparing the effect of treatment against a varied status quo and comparing against a homogenised control 'regime'. We chose the latter, in the hope of obtaining cleaner estimates of the treatment effect.

To add to this background, we had to recognise that office clerks, overseen by office and county managers, would be administering the treatment. A culture of light monitoring affected what could be tried without risking a significant problem of implementation: the experiment had to be 'do-able', allowing office clerks to carry it out conscientiously. Our experiment also had to be legal, with treatment and sanctions permitted by existing law.

Spells of UI were 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.



Questions on job search began to be asked of the treatment group at their first scheduled return to the office three weeks after initial UI registration. These questions concerned search methods used since the last visit (seven methods were asked about), contacts with employers (other than vacancies suggested by the office), names and addresses of up to three employers contacted (and the specific person contacted), reasons for lack of employer contact if none had taken place, and hours each week spent looking for work. Answers were recorded by the clerks on paper forms. Since each claim was monitored for up to four months, 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 UI registration). At the end of the experiment, in principle each participating office reverted to its previous practice of administering claims (visits at a frequency of between one and three months and no questions on job search asked at these visits – although see below on the latter).

Sanctions for failure to come to an interview during the experiment remained as before (including any local variation), but claimants in the treatment group were by definition exposed to the threat of this sanction more frequently. Additional sanctions could not be applied to those reporting no search (this would have required legislative change), but claimants would not have known this; they 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.)<sup>10</sup>

Claimants were unaware of the experiment and issues of recruitment and drop-out do not arise. In principle, individuals in the two groups could have talked to each other and discovered that their claims were being administered in different ways. But we think this very unlikely in practice. Most claims 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 – the UI extension scheme referred to earlier – so variation in treatment within the same office would not have been cause for surprise.<sup>11</sup>

In general the experiment operated well. One of us (Nagy) joined National Labour Centre staff in training sessions for office staff and together with an assistant

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<sup>10</sup> Note that a sanctioned UI claimant could not receive means-tested Social Benefit as an alternative.

<sup>11</sup> We do not think there was ‘contamination’ between treatment and control groups (we monitored assignment based on birth date). However, if control claimants talked with those getting treatment and as a result felt pressure to search, our estimates should provide a lower bound on the impact of treatment.

visited many of the offices with county managers during the experiment's conduct. Most offices were very co-operative and some decided to continue with the questioning on search after the experiment ended. The forms recording the reported search activity of claimants in the treatment group were collected from the offices fortnightly. However, occasional reluctance from clerks was encountered and in one county we had reservations about the conduct of the experiment – we test the sensitivity of our results to its exclusion.

The outcomes that we observed are (i) time unemployed as measured by UI duration (and hence censored if 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 claim duration since treatment might encourage exit from the labour force rather than to work.<sup>12</sup> We did not observe post-UI wages or other aspect of jobs, such as the duration of employment. Hence we cannot estimate the effect of treatment on 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. More visits to the employment office also raises the claimant's exposure to a major source of vacancies. (Offices place about a third of claimants exiting the register to a job.) We therefore hypothesise that treatment results in a higher exit rate to jobs. However, we cannot rule out that treatment could stimulate search without any impact on job exits. Additional search may not be sufficient to generate 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 UI.

### *2.3 Sample characteristics*

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<sup>12</sup> For example, Manning (2009) finds the tightening of job search requirements in the UK to have increased exits to inactivity but not to employment.

The sample of claimants was composed of 2,134 persons (1,115 treatment and 1,019 control), split almost equally between men and women. 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 UI registration. Table 1 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. The sample has a slight majority of claimants, but our monitoring showed assignment had been conducted correctly.<sup>13</sup>

### 3. Results

Table 2 shows exit states from UI for treatment and control groups, both for the full sample and for three sub-groups defined on age and gender. Two thirds of spells were censored, either due to the ending of the experiment or because the individual exhausted UI entitlement.<sup>14</sup> There are only small differences between the distribution of the two groups across other states. Notably, for the full sample 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 from the full sample of no impact from treatment is strengthened by Kaplan-Meier estimates of survival in the UI register (Figure 2). A small difference can be observed between treatment and control groups after 60 days, with the former leaving UI slightly more quickly, but a log rank test shows no significant differences between the two survival functions (Table 3).

The picture changes when we disaggregate by gender and age. There are no significant differences for the men or for women aged under 30 in the distributions of exit states or in the survival functions. But among the women aged 30+, the percentages leaving to jobs (Table 2) and the survivor functions (Table 3) differ between treatment and control groups at the 6 percent and 8 percent levels respectively (two-tailed tests).

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<sup>13</sup> Note that a year contains more odd than even numbered days of the month. We also tested for differences in characteristics between treatment and control groups within four sub-samples: women aged under 30, women 30 and over, men aged under 30, and men 30 and over. No significant differences were found other than for marital status among men aged 30 and over (71 percent married among the controls and 62 percent married in the treatment group).

<sup>14</sup> This reflects the low outflow rate from unemployment in Hungary and other Central European countries (Boeri and Terrell 2002, Micklewright and Nagy 1999).

Nearly 30 percent of women of this age in the treatment group leave to jobs compared to 23 percent of those in the control group.<sup>15</sup> The last three columns in Table 3 illustrate the difference in the survival functions – a quarter of the control group of women 30+ exit after 102 days but among the treatment group a quarter have gone by only 85 days. (An apparently perverse result is found among younger women, but the difference in survival functions is completely insignificant.) Figures 3 and 4 show the survivor function for the women aged 30+ and their (smoothed) hazard for exits to jobs, by far and away the most important exit state. A difference between the treatment and control groups emerges after about one month, at about the time when the experiment begins to bite, and then stays broadly constant, with the hazard for the treatment group about 40 percent higher. These non-parametric results therefore show some evidence – albeit not strong – that treatment has an impact for women aged beyond their 20s.

We now estimate flexible parametric models of the job exit hazard, including a dummy for membership of the treatment group. These models control for any (observed) differences in composition of treatment and control groups as UI spells lengthen. They allow comparison of the effect of treatment with the impacts of other characteristics. And they provide a convenient way for exploring whether treatment effects vary with characteristics beyond those explored in the graphical analysis, i.e. whether there are interactions – although the relatively small sample sizes and the high degree of censoring means it is difficult to estimate some interaction effects with any precision.

We specify the hazard,  $h$ , of individual  $i$  registered in employment office  $e$  leaving unemployment at duration  $s$  and calendar time  $t$ , as:

$$h_{iest} = g(s).exp(\alpha T_i + \beta X_i + \gamma O_e + \delta Z_t).$$

where  $T_i$  is a dummy for membership of the treatment group,  $X_i$  are other observed individual characteristics (measured at the start of the spell),  $O_e$  is a vector of employment office dummies that pick up fixed-effects associated with the strength of local labour demand or aspects of the employment offices themselves, such as skills of staff in matching the unemployed to vacancies, and  $Z_t$  pick up real time effects. The  $Z_t$

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<sup>15</sup> The sizes of treatment and control groups for women aged 30+ differ more than one would expect given the numbers of odd and even days each year and the total number of women of this age in the sample, but our monitoring showed assignment on the basis of birthday to have been correct.

are dummy variables for 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). 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).<sup>16</sup> The impact of treatment is assumed constant, unchanged with duration,  $s$ , or calendar time,  $t$ . This may seem inappropriate given the evidence of Figure 4. 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 for treatment and control groups for women aged 30+, 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. 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. In the light of the Kaplan-Meier estimates, we interact the treatment dummy,  $T_i$ , with dummies for women aged under 30 and for women aged 30+. The coefficients of these interaction terms show the marginal additional effect of treatment beyond that 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 treatment for men and for younger women is insignificantly different from zero, as in the earlier non-parametric analysis. However, for women aged 30+, we estimate the hazard to be 50 percent greater for the treatment group, *ceteris paribus* (the product of coefficients on the treatment dummy and the interaction term). This difference is significant at the 5 percent level.<sup>17</sup> As one would expect, estimating separate models for the three age/gender groups (not shown) produces very similar results – it is only for the women aged 30+ where treatment has

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<sup>16</sup> We also estimated the equation in Table 4 with a Cox model, which avoids any need to specify the form of  $g(s)$  at the cost of using only information on spell-length ranks. The estimated effects of treatment were virtually identical.

<sup>17</sup> In a model without employment office fixed effects the interaction term for the women aged 30+ gives an estimated hazard ratio of 1.49 with a t-statistic of 2.1. Treatment remains insignificant for other groups. Employment office fixed effects are significant at the 0.5 percent level (LR test with 47 degrees of freedom).

an apparent impact (hazard ratio = 1.60,  $t = 2.67$ ). While this impact is significant at conventional levels, it is worth emphasising the width of the 95 percent confidence interval for the hazard – 1.13 to 2.26 – which is quite broad.

Many of the other coefficients are insignificant. This is true of age (whether or not the age dummies for women are included and whether or not in logs), marital status, and spouse's employment status. Children aged 0-6 have a significant negative impact on the hazard for women and a positive but imprecisely determined impact for men. The education dummies work surprisingly poorly – 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.

Table 5 shows results of alternative specifications designed to check for variation of the treatment effect with individual and local characteristics, although the relatively small sample sizes hinder the precision of the estimates. In each case we show only the key parameters of interest, and results are given both for models estimated on the three sub-groups defined by age and gender and for the full sample. The top panel reports on interactions of the treatment group dummy with three dummies for marital status and spousal work status. For the women aged 30 and over, the results indicate that treatment has the most effect for those married with a working husband – *ceteris paribus* their hazard ratio is twice that for their counterparts who are in the control group. By contrast, treatment apparently has an insignificant impact for other married women and for single women of this age. However, some caution is needed since the hypothesis that the effect is the same for the three groups cannot be rejected at the 10 percent level. For younger women, treatment again has no significant impact, regardless of marital status. We should also note that for married men with working wives, treatment is estimated to reduce the hazard, a difference that is significant at the 5 percent level. This is difficult to rationalise, and serves as a warning of possible Type 1 error when considering the estimates of positive effects for women aged 30+.

We also investigated whether the treatment effect varied with the presence of young children aged 0 to 6, although as Table 1 shows children of this age are sufficiently infrequent to impede precise estimation of treatment interactions. (We tested without including the marital status and spouse working status interactions as well.) Treatment for women in their 20s has a positive effect that is just significant at the 5 percent level if (and only if) they have a young child, but no significant effect for the older women (where treatment itself remains significant for all women) or for men.

The bottom panel in Table 5 shows whether the effect of treatment varies with 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 treatment less rigorously in areas where jobs are scarce. We investigate this by interacting the treatment dummy with the employment office area unemployment rate. The rate is measured at March 2003 and for simplicity is not allowed to vary with calendar time,  $t$  (aggregate unemployment changed little during the experiment). 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. We show 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 (rather weakly determined) evidence in favour of the hypothesis that 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 5 percent level (two tailed test). The estimated hazard for a woman in the treatment group in an area with a 3½ percent unemployment rate is 2.02 times higher than that for a woman in the control group in the same area (or another with the same unemployment rate).<sup>18</sup> 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 values in the sample.) On the other hand, the unemployment rate itself is completely insignificant.

Geography may also be associated with tougher existing administration of UI or with variation in the rigour with which the treatment was administered, as noted in Section 2. We estimated a model in which 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. And the estimated impact of treatment hardly changes when we dropped the county where we had reservations about the conduct of the experiment.

As further checks of robustness, we estimated models allowing the effect of treatment to vary with the duration of unemployment (by month), with age, by whether the individual had previously received UI, and by UI entitlement (through dropping

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<sup>18</sup> Given that we report hazard ratios, this calculation is obtained as follows:  $2.02 = 3.56 \cdot (0.85^{3.5})$ .

individuals with less than 3 months entitlement). We also estimated the model allowing for unobserved heterogeneity following a gamma distribution. These specifications did not yield results that showed clear departures from the basic pattern in Table 4.

However, in some models the inclusion of interactions rendered the main effects insignificant and this underlines the lack of a high degree of precision in our estimates of the treatment effect.

#### 4. Discussion

The treatment effect appears appreciable for women aged 30+, especially for those married with a working husband, although we should recognise that the effect is not precisely determined. We can detect no significant positive effect for men and young women. We now address two questions. First, how does the finding of a gender difference compare with existing literature? Second, how should these results be interpreted?

The finding that women react more to treatment is in line with the general picture from the literature on differences between the sexes in the effects of labour supply policies. 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 evidence that monitoring has a greater impact on the behaviour of women. At the same time, the evidence is not extensive. Not all analyses investigate gender differences.<sup>19</sup> 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 findings appear to support the existing view, that view seems based on scanty evidence.

There are (at least) two alternative explanations for the experiment's results ('explanations' in the sense of descriptions of the observed behaviour). First, search effort of men and younger women is already high and the marginal return to additional effort encouraged by the treatment is zero. Men and younger women in the control group make frequent visits to employment offices to access vacancies of their own volition, so their contact with the offices is no lower than for their counterparts in the treatment group. For the older women, treatment does bring more contact in practice

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<sup>19</sup> For example, they are not reported in Ashenfelter et al. (2005) and Black et al. (2003).



with the offices' vacancies compared to the control group, and there is a positive return to additional search stimulated by treatment in terms of job offers generated.

Second, search effort of men and younger women is not high in the absence of treatment but the treatment does not produce additional search. The questions faced by the treatment group during visits to the employment office are answered with equanimity, with no disutility resulting. Treatment does mean in practice that additional visits are made to the employment offices but these visits do not result in better contact with vacancies. Only the women aged 30+ take advantage of the increased access to information on vacancies through the office visits. And only these women experience disutility from the additional visits and the questioning about job search, which increase the cost of leisure while unemployed, and react to a threat of sanctions if they do not increase their search activity.

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 judge between competing explanations.<sup>20</sup> The LFS data reported on in Section 2 show search behaviour of men in 2002 to be greater than that of women but do not support the hypothesis that search levels among men were high – part of the first explanation above. This favours the second explanation. Interestingly, the LFS data also show women aged 30+ receiving UI to be less likely to be classified as unemployed on the ILO criteria than both men and younger women with UI, i.e. in greater need of 'activation'. In leaning towards the second explanation, we cannot conclude which of the two elements of treatment – increased visits to the employment office and questioning during the visits – had the greater impact or indeed whether one had no impact at all. This is a result of the experiment having bundled together two different changes to UI administration.

On the face of it, the offices' questioning about search during the experiment appears rather a 'paper tiger' since there were no clear sanctions to be applied if the individual blithely responded that no search had been undertaken. But, as noted earlier, claimants would have been unsure of this. This provided an incentive to search. If the women aged 30+ were more risk averse, the potential sanction would have had more effect. (The review by Croson and Gneezy (2009) concludes firmly that women display greater risk aversion.) The effects of a potential benefit sanction have been explored by

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<sup>20</sup> 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.

Boone et al (2009) in a laboratory experiment and they find evidence of behaviour consistent with prospect theory (Kahneman and Tversky 1979): asymmetry in preferences at a reference point (the current wage offer in their experiment) coupled with strong loss aversion – a sharp disutility from benefit sanctions. However, they do not explore gender differences.<sup>21</sup> Another possibility suggested by this behavioural economics perspective, and one that does not rely on incentives, is that questioning about search set an ‘anchoring effect’ (see e.g. Camerer and Lowenstein 2004) in the claimant’s mind: that trying to find work is the expected behaviour for someone receiving unemployment benefit.<sup>22</sup> At the same time, it is unclear why this may have worked just for the older women (and note that all claimants in the experiment had worked in the previous four years otherwise they would not have received UI). Further experimental investigation in both the laboratory and the field is needed to provide more evidence on the impact of benefit sanctions in search models and how this impact varies between men and women.<sup>23</sup>

The issues dealt with in this paper are given further practical relevance in Hungary by changes to UI administration made since our experiment was conducted in 2003. 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. The claimant and the office also agree a personal ‘job search’ plan. 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 at all strict in practice. Notwithstanding, LFS data record a rise in the percentage of UI recipients who are classified as unemployed according to the ILO criteria i.e. reporting search and availability for work. The figure for 2005 was 66 percent, compared to 55 percent in each year 2002-4. The rise was particularly notable for women (up 15 points), with the percentage in 2005 no different from the figure for men. It is of course impossible to

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<sup>21</sup> Note that Boone et al’s experiment does not analyse job search effort – an offer arrives in each period and the individual’s decision is restricted to whether to accept. The authors report that they know of no other laboratory experiment of benefit sanctions.

<sup>22</sup> Seen this way, questioning gave a ‘nudge’ towards a change in behavior. Thaler and Sunstein define a nudge ‘as any aspect of the choice ‘architecture’ that alters people’s behaviour in a predictable way without forbidding any options or significantly changing their economic incentives’ (2008: 6). The literature’s discussion of anchors also suggests that questioning about search would have been more effective if individuals had also been asked about what they *intended* to do (rather than about just past search behaviour) and that if it had been underlined that their peers searched, i.e. that search is a social norm.

<sup>23</sup> Further research may need to recognize that punishments (i.e. sanctions) can undermine ‘intrinsic’ motivation, the desire to carry out a task for its own sake. Bénabou and Tirole (2003) discuss the conditions when rewards and punishments have such undesired effects.

conclude that the apparent increase in search behaviour was a result of the policy change (or even that search, as opposed to the reporting of search, actually increased). But it does underline the live nature of policy surrounding benefit administration in Central Europe, including the issue of anchors or norms for search, and the possible differences between men and women in their behaviour.

## **5. Conclusions**

Programme administration is a relatively neglected issue in the analysis of disincentive effects of unemployment benefit systems in OECD countries, especially outside the USA. We have investigated its impact with a field experiment with randomised assignment, conducted in Hungary. The treatment, involving more frequent visits to local employment offices and questions about job search activity, seems to have had an effect only for women aged 30 or over (an effect not determined with great precision). The experiment, and our investigation of institutional details of employment office practices in preparation for it, suggest that the Hungarian authorities were right to take issues of benefit administration more seriously – as they have done subsequently (although it is not the only aspect of unemployment benefit that is worthy of attention, with issues of coverage also prominent).

The finding of a greater impact for women has support in some other studies. But the evidence is scanty and we suggest that future research on the effects of benefit administration – whether in the laboratory or in the field – pays more attention to gender differences. 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.

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**Table 1. 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
<i>Educational level, %</i>		
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
<i>Demographic variables, %</i>		
Married	50.7	53.5
Spouse employed	31.4	34.0
Has children aged 0-3	8.0	7.0
Has children aged 4-6	11.8	10.0
Has children aged 7-10	16.4	15.6
Has children aged 11-14	14.4	13.0
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 2. Exit states from UI register**

Exit state	All claimants		Men		Women aged under 30		Women aged 30+	
	T %	C %	T %	C %	T %	C %	T %	C %
Re-employment	23.9	22.8	20.7	22.4	22.9	23.3	29.5	22.9
Training	2.2	2.0	2.6	0.8	2.5	3.8	1.5	2.5
Other active measure	1.8	2.2	1.3	1.8	2.5	2.9	2.1	2.2
Disqualification	2.1	1.3	3.0	1.8	1.7	0.8	0.9	0.0
Claim ceased voluntarily	1.0	0.7	1.3	1.0	0.8	0.4	0.6	0.4
Other reason	0.4	0.4	0.4	0.4	0.4	0.8	0.3	0.7
Censored – UI exhaustion	46.3	44.5	46.6	44.8	42.9	41.7	48.2	46.2
Censored – experiment end	22.5	26.3	24.2	27.0	26.3	26.3	17.0	25.1
Total	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0
No. of observations	1,113	1,019	537	500	240	240	336	279

Note: T and C denote treatment and control groups respectively.

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

	Sample size	Log-rank test p-value	Duration (days) at survival probability of 0.75		
			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 under 30	479	0.947	88	93	-5
Women aged 30+	615	0.076	102	85	17



**Table 4. Model of the re-employment hazard**

	Hazard ratio	t-statistic
Treatment group	0.92	0.63
Treatment * Woman aged less than 30	1.09	0.34
Treatment * Woman aged 30 or older	1.61	2.41
Woman aged less than 30	1.14	0.73
Woman aged 30 or older	1.14	0.72
Age	0.99	0.51
Married man	1.22	0.94
Married woman	0.97	0.12
Spouse employed, man	0.83	0.81
Spouse employed, woman	1.12	0.56
No. of children aged 0-6, man	1.27	1.63
No. of children aged 0-6, woman	0.69	2.50
Vocational school	1.05	0.31
Vocational secondary school	1.03	0.15
General secondary school	1.11	0.70
College, university	2.18	4.74
No. of observations		2,131

Note: absolute values of t statistics 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 of Treatment group dummy with other characteristics (hazard ratios)**

*A) Marital status and employment status of the spouse*

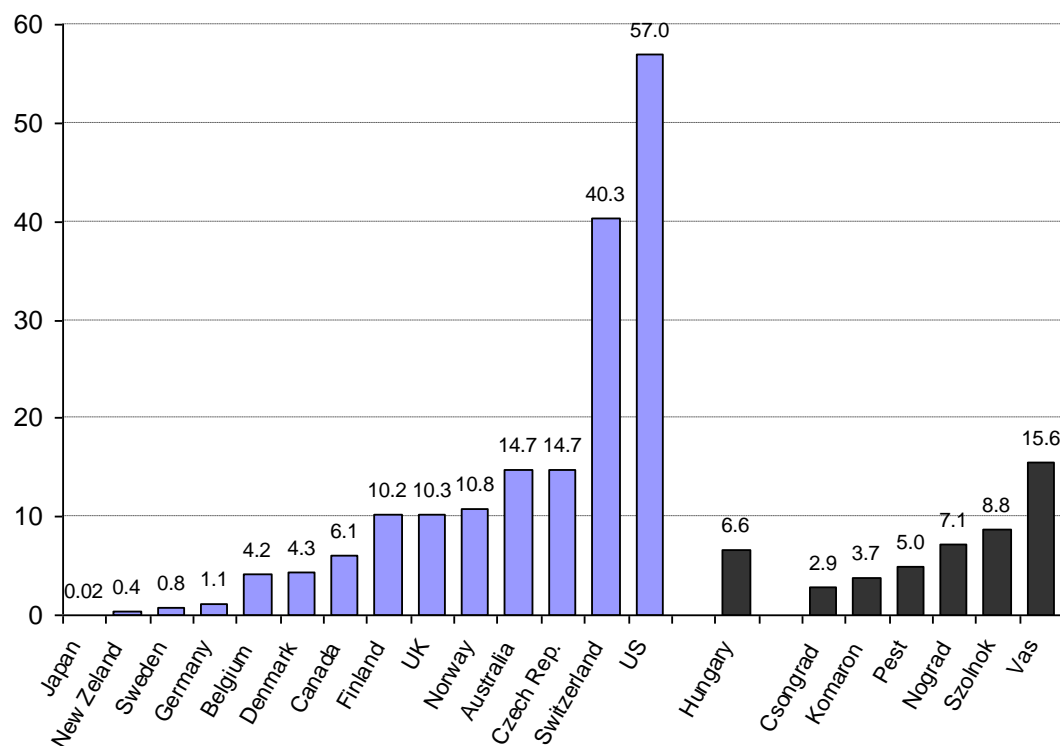
	Women		Men	All
	<30 yrs	30-49 yrs		
Treatment group*married & spouse works	0.82 (0.52)	2.10 (2.72)	0.48 (2.19)	1.09 (0.58)
Treatment group*married & spouse not working	0.20 (1.19)	1.27 (0.53)	1.13 (0.49)	1.03 (0.13)
Treatment group*single	1.05 (0.16)	1.09 (0.33)	1.07 (0.27)	1.13 (0.72)
Married and spouse works	1.16 (0.44)	0.87 (0.72)	1.37 (1.14)	1.11 (0.74)
Married and spouse not working	1.47 (0.61)	1.10 (0.22)	1.24 (0.82)	1.18 (1.00)
No. of observations	479	615	1,037	2,131

*B) Local unemployment rate*

	Women		Men	All
	<30 years	30-49 years		
Treatment group*local unemployment rate (%)	1.13 (1.03)	0.85 (1.97)	1.04 (0.80)	0.98 (0.44)
Local unemployment rate (%)	0.94 (0.43)	1.03 (0.71)	1.06 (1.38)	1.03 (0.74)
Treatment group dummy	0.48 (1.08)	3.56 (2.61)	0.74 (0.77)	1.23 (0.65)
No. of observations	479	615	1,037	2,131

Note: The models in panels A and B are as in Table 4 with the addition of the interactions shown and with the following exceptions. In the model in panel A the treatment group dummy itself is dropped but is interacted with the three groups as shown. In the model in panel B, the local office fixed effects are excluded. Absolute values of t statistics in parentheses are from the test that the hazard ratio is equal to 1.0. Standard errors take account of clustering of individuals in local employment offices.

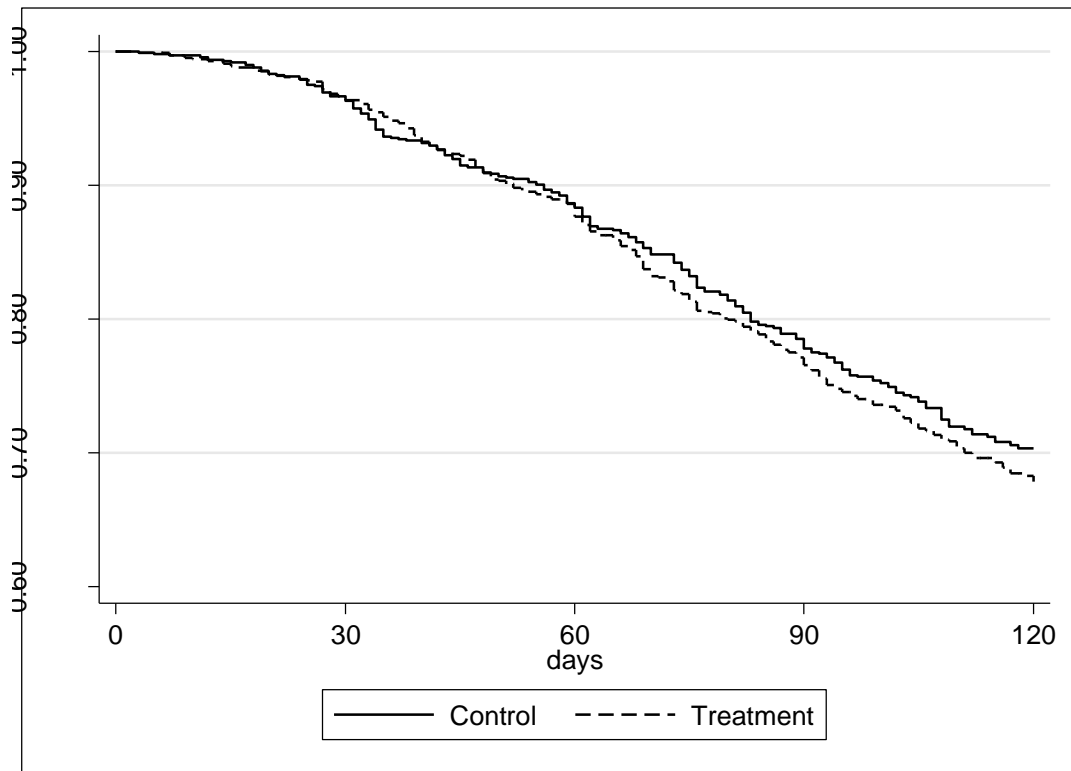
**Figure 1. Sanctions and disqualifications of unemployment benefit for behaviour during claim (yearly figures) per 1000 persons in claimant stock**

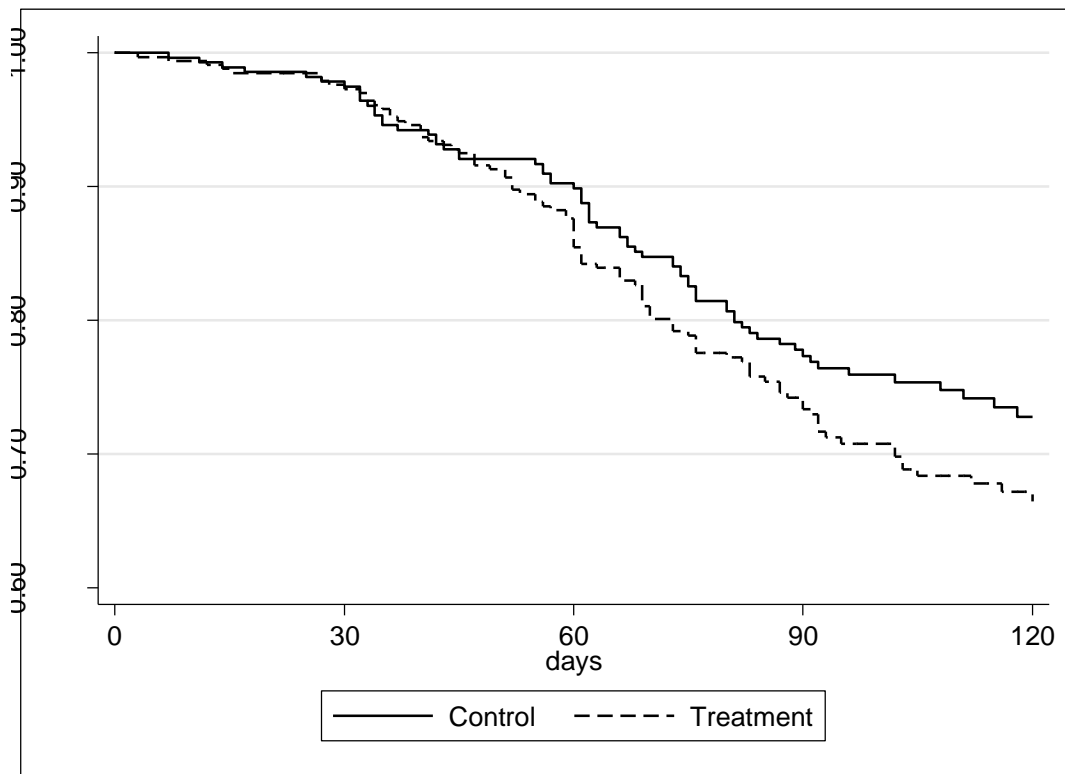


Source: Figures for Hungary (dark bars) are for 28 of the 40 employment offices included in the experiment described in Section 3 and are averages for 2000, 2001 and the first six months of 2002. Figures for other countries (light bars) are from OECD (2000 Table 4.2).

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 2. Survival in UI register, all men and women**



**Figure 3. Survival in UI register, women aged 30+****Figure 4. Hazard to exit to employment, women aged 30+**