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Evaluating the impact of a well-targeted wage subsidy using administrative data

Abstract

The paper measures the impact of a wage subsidy for long term unemployed workers, using a large administrative dataset from Hungary. While such subsidies are often promoted as an efficient way to speed up the recovery of the economy or to increase demand for low skilled workers, existing evidence on their employment effects is relatively limited and also somewhat mixed, especially in the case of transition economies.

We examine employment and wage outcomes in various model specifications. Results show a significant impact for men aged over 50, which is driven by the subgroup of those with lower secondary education. The subsidy for jobseekers with at least secondary education and aged over 50 is cost effective for men. We also present some evidence that this is not merely caused by substitution across various sub-groups of jobseekers.

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1. Introduction

There is a clear shift in European employment policy towards active labour market programmes (ALMP) as opposed to cash transfers for the unemployed and the recent crisis has further increased the demand for well designed policies.[2] The meta-analyses of existing empirical evidence on ALMPs have shown that the impact of such policies is determined predominantly by their type, while the business cycle or labour market institutions are much less important, or not at all (Card et al 2010, Kluve 2010).

According to Kluve (2010), wage subsidies and services/sanctions are the most effective in increasing reemployment rates. The effectiveness of wage subsidies however has been questioned on several accounts. First, not all empirical studies found positive and significant effects. In fact, the few existing papers on transition countries have all shown a neutral or negative impact (Kluve 2010, Betcherman et al 2004). Second, wage subsidies are relatively expensive, which implies that the magnitude of their effect is as important as its sign, i.e. only a relatively large impact can make such programmes cost effective. Third, the narrow targeting of subsidies may stigmatise recipients and reduce both take-up and effects (Katz 1996). Fourth, deadweight and substitution costs are likely to be high (Betcherman et al 2004).

The recent global financial crisis has increased the policy relevance of wage subsidies as a means to preventing the rise of long-term unemployment and speeding up recovery. Such subsidies are especially relevant for transition economies struggling to meet EU employment targets. The political and economic transition of 1989 has drastically reduced the employment rate of low-skilled workers in Eastern Europe, and the reduction has proved to be lasting in most countries. This has become one of the main causes behind the rise in long-term unemployment, economic inactivity and persistent poverty.

This paper aims to contribute to resolving three questions concerning the effectiveness of wage subsidies.

(1) As very few studies have been done in transition countries, it is unclear if the disparity of earlier findings is due to the transition environment or poor policy design. We assess a wage subsidy introduced in Hungary in 2007, which is a relatively well-designed scheme. It is very similar to the targeted payroll tax subsidies in Belgium and Finland and a targeted tax credit in the US, which earlier studies have shown to have some positive impact.

(2) We use a large administrative dataset, which include information on employment and wage as opposed to merely recording exit from the unemployment register. This will allow us to compute the costs and benefits of the scheme.

(3) The same dataset allows us to check if there is a substitution effect for close substitutes.

We assess a wage subsidy introduced in Hungary in 2007 for various subgroups among the long term unemployed. This subsidy offers a temporary reduction on payroll tax (social security contributions) to employers, and the reduction is largest for job seekers aged over 50 and those with only primary education. Eligibility is determined solely by observable characteristics of job seekers.

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[2] For a recent initiative by influential economists supporting wage subsidies as a means of speeding up the economic recovery see: http://delong.typepad.com/sdj/2010/02/economists-for-wage-subsidies.html

To identify the effect of the subsidy, we exploit the design of the programme and the availability of administrative data. As outcomes, we consider both re-employment and the wage earned in the new job. As the suitable control groups are eligible for a base subsidy, our estimates are interpreted as the *additional* effect of the extra subsidy for multiply disadvantaged groups.

The dataset we use is a 50 % random sample of the total working age population and includes information on registration at the public employment service, receipt of welfare provisions (including retirement) and employment and wages, for the period between 2002 and 2008. The size and depth of the dataset allows us to controll for a richer set of observable characteristics than most earlier studies.

Results show a significant positive effect on both re-employment probabilities and wages in the case of men aged over 50 and this result is robust to model specifications. We find no significant effect for women. Using the same dataset, we also estimate exit probabilities for close substitutes of the treatment group and find no indication of a substitution effect. Under cautious assumptions, the subsidy is cost effective for men with at least secondary education aged over 50.

In the rest of this paper, the next section briefly reviews existing relevant research on wage subsidies. Section 3 describes the Hungarian scheme in detail and summarises aggregate data on take-up. Section 4 describes the dataset and presents raw outcome measures. Section 5 outlines the objects of interest and identification strategy. Section 6 presents estimation results and discusses their robustness, while Section 7 presents a cost-benefit analysis.

2. Review of research on similar policy instruments

Similar targeted payroll tax subsidies have been used in Belgium, Finland, Germany, the Netherlands and the US. The 'Maribel subsidies' system in Belgium in the late 1990s offered a lump sum reduction in payroll taxes for employing manual workers, so that the relative size of the subsidy was highest for low wage workers. Goos and Konings (2007) use firm-level data to evaluate the effects of changes in this scheme and find significant positive effects on employment. Huttunen et al (2010) estimate the employment effects of a subsidy in Finland, which is targeted at the employers of low-wage older workers using difference-in-difference-in-differences. They find that the subsidy has no effects on the employment rate but it increases the probability of part-time workers obtaining full-time employment (the scheme is only available to full time workers). Katz (1996) reviews evaluations of the Targeted Jobs Tax Credit (TJTC), a similar scheme operating in the US until 1994 and concludes that it was effective in improving the earnings and employment of disadvantaged groups, especially when combined with training elements. However, studies examining changes in the TJTC rules or its performance compared to more sophisticated schemes in experiments have pointed to a potential problem with narrow targeting. When TJTC was used without additional services (training or counseling), take up tended to be low and reemployment effects were small (or even negative in some experiments). These results were attributed to the stigmatisation of recipients. However, some of these concerns were called into question by later analyses showing that non-random selection into the treatment group could explain poor performance. Dubin and Rivers (1993) recalculate the effects of one such experiment in Illinois, where the treatment group were long term unemployed individuals who were offered a voucher, which their new employer could submit and receive a lump sum payment. They find that, once controlling for self-selection, the programme significantly increased the likelihood of re-employment.

Schünemann et al (2011) estimate the effect of a wage subsidy for employers hiring long term unemployed, using information on eligibility rather than take-up, and applying an RDD in differences approach in order to exclude potential bias from unobserved effects. They find no impact on employment outcomes, nor on employment stability.[3] The authors suggest that this may be a consequence of using eligibility rather than take-up to identify the treatment effect since the latter combines the effect of the subsidy and finding a job. An alternative explanation is that this estimate applies to a particular sub-sample of long term job seekers: those who did not participate in any other labour market programme, had been unemployed for at least 2 weeks but worked for at least 6 months during the 12 months preceding (re)entry to the unemployment register, and were eligible for unemployment benefit but not exactly for 12 months.

There are very few empirical studies on Hungarian ALMPs and as far as we know, the reemployment effect of the Hungarian "START" scheme has not yet been evaluated. The two papers that evaluate the impact of traditional wage subsidies have somewhat conflicting results. O'Leary (1998) found negative or zero employment effects and a significant increase in earnings on the first job, except for job seekers aged over 45, where effects on both employment and earnings were positive and significant. Using data for 2010, Csoba et al (2012) estimate a 24-fold increase in log-odds of employment, which is a dubiously large effect, and is robust to controlling for an extensive set of individual characteristics. However, they do not control for the duration of the last unemployment spell.

3. The policy instrument and the context of its introduction

The Hungarian wage subsidy scheme examined in this paper was first introduced in October 2005 for school leavers (START) and was extended to various subgroups among the long term unemployed in July 2007 (START plusz and extra).

It is a quasi-voucher scheme that offers a temporary reduction on payroll tax (social security contributions) to employers hiring the holder of the 'voucher.' The amount of the subsidy varies across eligible groups, as summarised in Table 1. All long-term unemployed are eligible for START plus, and START extra doubles the subsidy for a selected subgroup with multiple disadvantages, ie. for jobseekers above 50 and those who completed primary education only. Eligibility for START extra can thus be earned in two ways: by accumulating unemployment spells (for the uneducated) or by reaching 50 years of age (for the educated).

[3] This is intriguing given that the size of the subsidy is rather large (60 % of the usual wage in the given occupation for 6 months and 40 % for another 6 months). However, the subsidy is conditional on submitting a claim at the local (or regional) job centre, and is granted at the discretion of the case worker and provided that there is available funding. Also, it requires one year of continued employment. Lastly, the discontinuity exploited by the paper is that eligibility is conditional on 12 months of prior unemploment, which may lead to an overestimation of programme effects if those with 11 months of unemployment would postpone their job entry in order to become eligible for the subsidy - see Brouilette and Lacroix (2010) on the problem of self-selection.

Table 1. Rules of the various START schemes at the time of introduction

Name	Eligibility	Amount of subsidy (% of total wage cost)*		Ceiling on subsidy
		1st year	2nd year	
START	School leavers: below 25 (30 for graduates), no prior paid job	14 %	7%	1.5x minimum wage (2x for he. graduates)
START Plusz	On parental leave or care allowance, or registered unemployed for 12 months within preceding 16 months, not eligible for old age pension	14 %	7%	2x minimum wage
START Extra	Over 50 or primary education only, and registered unemployed for 12 months within preceding 16 months, not eligible for old age pension	25 %	14 %	2x minimum wage

* In 2007, the employer's contribution was 32% of the gross wage, and this was waived in full during the first year of employing a person with a START extra voucher. The flat rate health contribution was waived in both years in all schemes, which was 1950 HUF a month (about 8 EUR), or around 3% of the minimum wage. The subsidy was further extended in 2009 and replaced by a new scheme in 2012.

The subsidy is largest for job seekers aged over 50 and those with only primary education: 25% of the total wage cost in the first year and 14 % in the second year, with a cap set at twice the minimum wage. The general subsidy available to all long term unemployed is 14 % in the first year and 7 % in the second year.

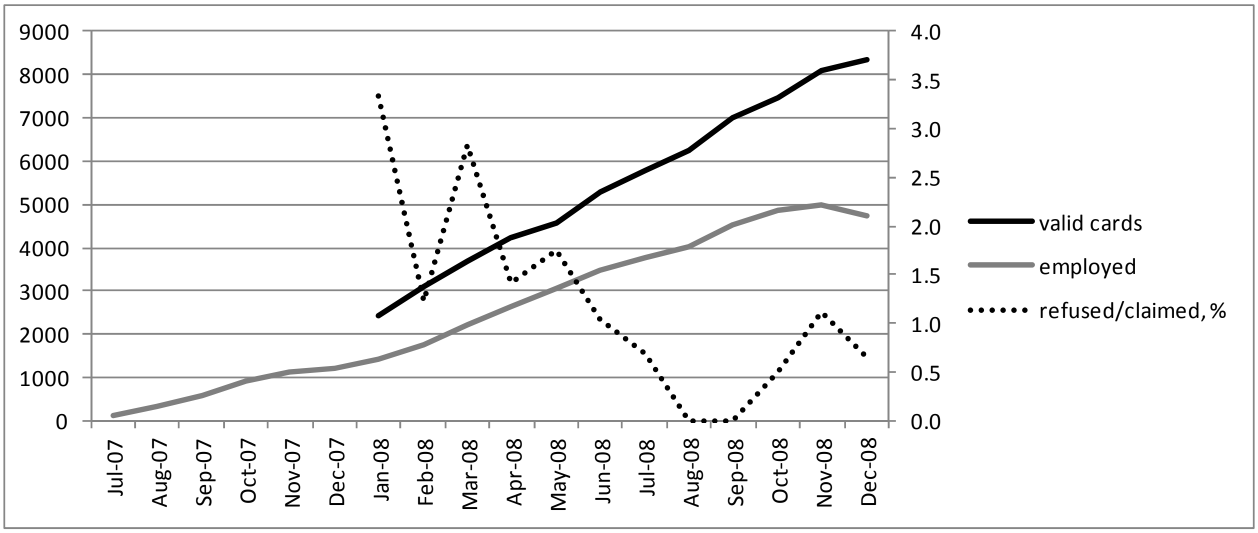
The scheme (all three variants) has been administered by the tax authority who issue a plastic card to eligible persons which indicates the type and eligibility period of the subsidy. Cards are issued only if claimed, but the evaluation of claims is automatic, with no discretion or further conditions beyond age, education and long term unemployed status. Job centres have been actively encouraging job seekers to claim the card.

The validity of the card and thus the period of eligibility starts on the day of issue. Jobseekers are therefore advised to claim the card immediately before starting in their job, so that their employer may be eligible for the maximum length of the subsidy. The timing of programme-participation is thus as follows: the job seeker 1) registers at the job centre, 2) becomes eligible for a START card, 3) finds a job 4) applies for a START card, 4) enters the job, 5) ends the employment spell within the subsidised period or stays employed. The subsidy lasts for a maximum of two years, so that past programme-participation impacts can only be measured using data for 2009 or later.

The START schemes are different from traditional wage subsidies in a number of ways, the most important being administration and further obligations. Traditional subsidy schemes require the employers to submit an application at a regional PES office and to guarantee that they will employ the beneficiary for at least as long as the benefit was provided. Since the Start schemes do not pose such requirements, we expect that its effect will be larger than that of the traditional wage subsidy.

Between July 2007 and December 2008, the START EXTRA card was claimed by 8,859 persons and issued to 8,392 persons. Less than 2 % of the claims were declined by the tax authority, and some 5 % was not issued for other, unknown reasons (e.g. the card holder withdrew the claim). During the same period, the number of persons employed with the subsidy started to grew steadily, peaking at 4,998 in November 2008 (Figure 1). This suggests that most cards have been claimed once the job seekers had a job offer, as recommended by job centres.

Figure 1 Number of valid cards and persons employed with a START extra subsidy between July 2007 and December 2008



Source: Aggregate administrative data by the Tax Authority, prepared on request for the Ministry for Labour. No monthly data available on claims in 2007.

In the ensuing analysis we focus on the START EXTRA scheme accessible to long-term jobseekers aged over 50 with at least secondary education. This covers half of the recipients (Table 2).

Table 2. Persons ever employed with the START extra subsidy until December 2008

	aged below 50	aged 50 or over
primary education	1 298	1 690
secondary or higher		3 127

Source: Aggregate administrative data by the Tax Authority, prepared on request for the Ministry for Labour.

Between June 2007 and December 2008 there were no other new programmes introduced that would target especially the uneducated or older long-term jobseekers. The only other large programme intended for these groups was public works schemes, which had already been in place since 2001. In 2009, several new programmes were introduced: using EU funding, personalised services (e.g. mentoring) were introduced for hard-to-place groups and funding for the public works scheme run by municipalities was considerably increased, while active job search requirements for jobseekers aged over 55 were considerably relaxed.

The START EXTRA subsidy appears to be well targeted considering that reemployment probabilities are significantly lower for uneducated and older job seekers. Demand for older workers declined significantly in the 1990s, partly due to the sharp drop in their relative productivity and also due to discrimination (see Lovász 2012 for a review of empirical evidence). There is also some evidence that wage subsidies targeted at the long-term unemployed (as in the START EXTRA scheme) rather than at low-ability workers are more effective (Brown at al 2011).

4. Data and stylised facts

We use a dataset drawn from administrative records, constructed for research purposes by the Institute of Economics, Hungarian Academy of Sciences (IE-HAS). It contains the matched records of 50 % of the adult population including data from (1) health and (2) pension insurance, (3) the treasury, and the (4) unemployment register.[4] The sample was taken from the records of January 2002 of the health insurance fund which include all people residing in Hungary who ever had been covered by public health insurance. This practically covers the total population.

The resulting dataset is a panel of the employment and job search history of the working age population covering the period between January 2002 and December 2008. There is information on age, sex, dates of entering and exiting employment, earnings (pension insurance records), unemployment history and type and period of receiving various transfers (including disability benefits) and sick leave. The information on employment and transfers is used to reconstruct labour market status in each month during the observed period. The employment-outcome indicator exlcudes casual jobs as such jobs are not eligible for the START subsidy. There is no data on the actual claiming the START cards (which is recorded by the tax authority), or on the employer.

It must be noted that labour market status in this dataset may not correspond to the actual situation in the economic sense. A person receiving unemployment benefit is coded as unemployed, even if not actively looking for a job, while a person receiving old-age pension is coded as inactive, even if available for work and actively looking for a job.

In the original administrative data, information on employment, unemployment and transfer receipt is in continuous time, recording the type of spell and the dates of starting and ending for each spell. This was converted into a monthly structure.

Although the data are based on administrative records, the nature of the administrative sources itself may generate attrition: we lose information on people who leave the unemployment register and take up undeclared work, or become inactive without obtaining any form of social transfer. However, attrition does not seem particularly large in the age groups we examine and it is not very different between the groups eligible and not eligible for START.[5]

[4] For a detailed description of the data collected by the Pension and the Health Insurance Funds and in the Treasury, see Elek et al (2008).

[5] For jobseekers aged around 50, attrition is 1.04 % for those eligible and 1.28 % for those not eligible for START. For the low educated, attrition in the sample is 0.53% and 1.29 % respectively. Deaths are recorded in the data and spells ending in death are right censored in our estimates. The lack of information on unregistered jobs is not considered a problem as such jobs cannot (by design) benefit from the subsidy.

We observe only 18 months following the introduction of the programme, thus we cannot look at post-participation outcomes and we have right-censored spells in the dataset. None of the spells are left-censored and we have a quite long employment history available.

5. Objects of interest, identification strategy and raw effects

A wage subsidy can increase the demand for the beneficiary thus increasing probability of exit from registered unemployment or even increase wages when employed. Given that employing a programme participant is relatively cheaper, employment spells can last longer, everything being equal. As the time-frame of our data does not extend very much into the programme period, we shall be focussing on the exit from the registered unemployment spell and wages at entry to the subsidised job.

The thought-experiment we carry out is the following. We observe a group of registered unemployed with identical characteristics making them eligible for programme participation at a point in time. The programme is introduced. Some of the unemployed are randomly chosen to receive the benefits provided by the programme, while the others are left out. Neither of these groups takes part in other programmes and nothing happens that would change their chances to exit unemployment. As time passes by, we observe all exits from unemployment in both groups. To evaluate the programme, we examine the characteristics of the exits to employment: their timing, distribution by relevant characteristics. We also look at wages earned at the newly obtained jobs. If, for example, exits take a shorter time on average in the participant group than in the nonparticipant group, we conclude that the programme had an overall beneficial effect.

We consider three outcomes: 1) employment after 15 months, 2) exit to a job in calendar time, 3) entry wages. In outcome 2) we consider exits during the period starting in July 2007, when the subsidy was introduced. This differs from the usual approach of observing exits from the start of the last spell of unemployment, which varies in calendar time across individuals. However, given that that eligibility depends on the number of months spent in unemployment (accumulated in one unborken spell or several shorter spells), in the usual spell-duration estimates, the treatment dummy is negatively correlated with the spell length. To see why, consider the last spell including the month when the programme was introduced. Those with an unbroken long spell enter into treatment at a later point in their spell, while those who collected the 12 months in several shorter spells will enter into treatment sooner. Now assume that spell-duration is measured using a single continuous variable. Using standard auxiliary-regression arguments, one can show that in the case of a negative correlation between the treatment dummy and the variable measuring the spell length and a negative correlation between re-employment and spell length (time-decreasing hazard), the estimate of the treatment effect is downward-biased.

Let us label the two sub-programmes of START EXTRA as follows: P1) for the long-term unemployed aged over 50 (any education), and P2) for the long-term unemployed with primary education only (any age).

In the case of P1, the programme design allows us to exploit a discontinuity in eligibility to identify the programme effect. In this case, the treatment group is formed by those eligible for participation and are slightly above the age 50, while the control group is formed by those who are similar to them in all aspects, but stay slightly below age 50 during the observation period.

Those with at most primary education are excluded, as they are eligible for the same support through P2. Our estimates will therefore be local to the age around 50 and also to individuals with higher than primary education.

The discontinuity design strategy assumes that heterogeneity in the variable with the discontinuity is irrelevant in determinig outcomes. This is not completely so in our case, as age tends to reduce the chance of reemployment, but we can account for this in the estimation strategy. When defining the groups slightly below and slightly above 50, we are facing a trade-off. Opening up the age-windows and making the groups larger, we obtain more observations and hence more precise estimates. At the same time, groups become more heterogeneous with respect to age and the estimates become more prone to age-related effects that might be correlated with the outcome – a familiar trade-off between consistency and variance.

If the differential effect of extraneous factors on outcomes over time (such as seasonality or the business cycle) is to be taken into account separately from unemployment duration, it has to be controlled for using some statistical method, such as difference in differences strategy, where we look at the difference between the control and the treatment outcomes before and after the programme.

As already noted, we do not directly observe card claims and define treatment as eligibility for the START card. This is not only a pragmatic decision taken for the lack of better data, but can also be justified on the basis of the official information on claims, take-up and subsequent employment presented in section 3 above. As we have seen, the issuing of vouchers is almost automatic and most claims are shortly followed by employment. This suggests that claiming the voucher is a formal exercise, which is in reality most likely to happen after the outcome, that is, after the employer decided to hire the job seeker. 'True' take-up is knowledge of the scheme and of the age condition by either the employer or the job seeker. As the scheme was actively advertised in job centres, it does not seem far-fetched to assume take-up to be close to full and to consider all eligible job seekers as treated. The aggregate statistics seem to support this assumption as subsidised job entries are rather close to the number of card holders.[6]

Based on the above considerations the treatment group is defined as those aged between 50 and 52.5 and the control group includes those aged between 45.5 and 48.5 in June 2007. The 18-month gap between them ensures that no member of the control group becomes eligible for participation during the observed period. In other respects the two groups both fulfill the eligibility criteria at the time the scheme was introduced, i.e. they have accumulated 12 months of registered unemployment. As the scheme was not much advertised before its introduction, this is likely to eliminate any bias from waiting effects.

Reemployment effects are estimated in various specifications: probits for the probability of being employed 15 or 18 months after the introduction of the scheme and duration models (a modified Jenkins type probit) for the probability of exit to a job at any time after the introduction of the scheme (Jenkins 1995).

Wage effects are estimated in standard Mincer-type wage equations, where we use the interaction of the treatment dummy and a dummy indicating spells after the introduction of the programme.

[6] If the assumption does not hold, we can identify what is called in the medical literature the intention to treat effect (ITT). Although one can regard this as a limitation of the analysis, some would argue that it has at least as much policy relevance as the treatment effect itself (Schünemann-Lechner-Wunsch, 2011).

This is interpreted as the programme effect, ie. as a shift in the wage advantage (or more likely, disadvantage) of older jobseekers reentering employment.

Raw outcomes for P1 (workers aged over 50) and P2 (primary educated) are presented in Table 3 and Figures 3-4 below. For P1, the share of those employed in September 2008 is slightly higher in the treatment group and the survival function measured in calendar time also suggests some positive treatment effect for men. There is no visible positive programme-effect when survival is observed during the whole unemployment spell, nor for women. For P2, the share of those employed in September 2008 is significantly lower in the treatment group. This is not unexpected however, given the higher education level of the control group.

Table 3. Raw outcomes: employed 15 months after (in September 2008)

P1	Not employed	Employed	Ratio
Control	1419	505	26.3%
Treat	1311	502	27.7%
Difference			1.4%

6. Estimation results

Regression results show by and large the same picture as the raw outcomes presented above. Marginal effects estimated in various model specifications (see main results below, full regression output in the Appendix) tend to be small but positive and significant for men, and insignificant for women. Results are robust to the definition of employment and unemployment in the data. Control variables take the expected signs. The preferred specification (presented below) includes controls for age, education, and past work history.

For men, the positive effect is driven by job seekers with lower secondary vocational education, who constitute 74 % of the sample. For the higher educated, there is no significant effect, which may be due to the ceiling on the subsidy (which reduces the value of the subsidy at high wages) or possibly to stigma effects, which may be stronger in white collar occupations.

Table 5. Employment effects of P1

	15 months after	18 months after	Calendar time duration	Calendar time duration for lower secondary vocational
Men	0.1040**	0.0782	0.0144***	0.0164**
Women	0.0638	0.1040	0.0016	-0.0034

*** p<0.01, ** p<0.05, * p<0.1 See full regression results in Tables B1 and B2 in the Appendix.

The subsidy for job seekers aged over 50 has a significant positive effect (the effect of the interaction of the treatment and the programme period) on the subsequent wages of men.

Table 6. Wage effects of P1

	Men	Women
eligibility (aged over 50)	-0.200*	-0.0302
	(0.114)	(0.151)
after June 2007	0.147**	0.340***
	(0.0614)	(0.0933)
eligibility * after June 2007	0.157*	0.0978
	(0.0893)	(0.132)

*** p<0.01, ** p<0.05, * p<0.1 See full regression results in Tables B3 in the Appendix.

For women, the subsidy has no significant effect either on employment, or on wages. A possible explanation is that older women are less likely to actively look for a job, which lowers the potential impact of any wage subsidy that is by design dependant on job search. An earlier result by Micklewright and Nagy (2004) points to a similar direction: they found that a mild tightening of job search criteria for unemployment benefit recipients had a significant positive effect on the probability of reemployment only in the case of women aged over 30.

The above results give gross estimates of the program effect which is valid only if the control group was not affected by the program, for example through a replacement of employees by long-term unemployed similar to them and eligible to the subsidy. As a crude check for displacement effects, we examine the probability of becoming unemployed for the employed population in the period between 2005 and 2008, that is, around the time of introducing the subsidy. We use the same administrative dataset (which covers 50 % of the employed population) and for each educational level estimate a probit regression with dummies for each year, for the age group below 50 and their interactions as well as controls for prior work history.

The results show a U shape evolution of exit probabilities: exits are somewhat less likely before and after 2007 but this is only significant for educated workers (see Table C1 in the Appendix). This makes sense in view of general economic growth trends: there was very little growth in 2007, compared to before 2007 of the first half of 2008. We find no significant trend in the job loss probabilities of workers aged below 50 with secondary or higher education. Workers with primary education are more likely to exit in 2007 and 2008 compared to 2006. Recalling that the positive effect of START EXTRA presented above applied to men with lower secondary vocational education, these results suggests that the effect of the programme is unlikely to be caused by substitution.

7. Cost benefit analysis

The total cost of the scheme is relatively modest, compared e.g. to re-training or public works programmes in Hungary. Between July 2007 and December 2008, the START EXTRA scheme cost a total of 1 billion HUF per annum. This amounts to 593 EUR per person (HLM 2011, not controlling for right censoring in employment spells).

Neglecting the costs of administration, which are likely to be very low, the cost of the programme is the additional subsidy (on top of START PLUSZ available to all long term unemployed). The short term benefits of the programme include savings on social assistance expenditure and employee's social security contributions (17 % of the gross wage).[7] Long term benefits may include social security contributions after the subsidy expired, longer employment spells in the subsequent work history, postponed retirement and savings on health care costs. For lack of empirical evidence on the magnitude on these long term effects (in Hungary), we concentrate on the short term balance. The average employment spell starting after the subsidy was introduced lasts 5 months, so we can safely abstract from the fact that the subsidy is lower in the second year.

Assuming that there is no deadweight loss at all, the additional cost of the programme amounts to 44 thousand HUF per month for each additional worker entering employment and yields 22+26=48 thousand HUF per month, if the worker had been on social assistance. In this case the benefits clearly exceed the cost. If employers draw the subsidy for all eligible workers they hire, not only those they would not have hired in absence of the subsidy, for each genuine new hire yielding 48 thousand HUF, the government must spend 0.032/0.014*44=101 thousand HUF. In this case the cost exceeds benefits at least in the short run. The long run net effect may still be positive.

As a crude measure of deadweight we compare the number of subsidised job entries as recorded by the Tax Authority to the number of entries by potentially eligible job seekers as observed in our dataset. The former figure is 3,127 and we observe over 17 thousand job entries (multiplied by a weight of 2 so that it becomes comparable) in our dataset. Lastly, the additional employment for older men resulting from the subsidy, would be around 2,543 for the same period, assuming that the effect we estimated can be extended to workers much older than 50 as well. This suggests that there may be some deadweight in the programme but that it is not very large. If it is less than 20% of subsidised jobs, as suggested by the above figures, then the programme is cost efficient even in the short term.

8. Conclusions

The paper measures the impact of a wage subsidy for long term unemployed workers in Hungary, using administrative data. While such subsidies are often promoted as an efficient means to speed

[7] We neglect revenues from the personal income tax as this was practically zero at the relevant wage levels.

up recovery or to increase demand for low skilled workers, existing evidence on their employment effects is somewhat mixed, especially in the case of transition economies.

We examine employment and wage outcomes in various model specifications. Results show a significant impact on the reemployment rate and wages of men aged over 50. The overall positive impact on employment is driven by the largest subgroup of those with lower secondary education. The evolution of job loss probabilities around the introduction of the programme suggests that the positive employment effect of the programme is not merely caused by substitution across various sub-groups of jobseekers.

For women, the subsidy has no significant effect. A possible explanation is that older women are less likely to actively look for a job, which lowers the potential impact of any wage subsidy that is by design dependant on job search.

The subsidy for jobseekers with at least secondary education and aged over 50 is cost effective for men, even considering its short term benefits only. The overall efficiency of the programme could be improved by narrowing the target group to jobseekers with less than upper secondary education and possibly by supplementing it with incentives for job search, especially for women.

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Appendix

Table A1. Summary statistics for the treatment and control group for June 2007

	Treatment			Control		
Variable	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.
sex	2043	0.592	0.49	2195	0.559	0.50
age (years)	2043	50.800	0.77	2195	46.488	0.97
lower secondary education	2043	0.617	0.49	2195	0.643	0.48
upper secondary education	2043	0.320	0.47	2195	0.304	0.46
tertiary education	2043	0.063	0.24	2195	0.052	0.22
student (months) July06-June07	2043	0.012	0.38	2195	0.005	0.26
training before July07 (dummy)	2043	0.011	0.11	2195	0.017	0.13
over 50% disabled (dummy)	2043	0.037	0.79	2195	0.029	0.72
sick leave (ever b July07)	2043	0.0015	0.04	2195	0.0014	0.04
labour income and transfers						
reservation wage (HUF/month)	981	71055	22528	1171	71730	23260
current income (HUF/month)	2043	9960	27631	2195	10349	27605
smallholder (ever before)	2043	0.020	0.14	2195	0.026	0.16
disab benefit months July06-June07	2043	0.854	3.00	2195	0.697	2.77
UB months July06-June07	2043	1.816	3.82	2195	0.918	2.54
UA months July06-June07	2043	5.718	5.55	2195	6.187	5.46
disab benefit months b July06	2031	1.485	6.16	2191	1.227	5.57
UB months b July06	2031	2.554	4.06	2191	2.534	4.02
UA months b July06	2031	7.160	10.82	2191	7.888	11.13
Labour market participation						
months worked July06-June07	2043	0.385	0.92	2195	0.434	0.97
casual work July06-June07	2043	0.164	0.64	2195	0.198	0.70
months worked b July06	2036	17.724	17.77	2191	15.095	15.87
casual work b July06	2036	0.159	0.83	2191	0.168	0.80
student (months) b July 06	2036	0.027	1.20	2191	0.000	0.00

Notes: Retrospective data cover the period starting in January 2002, except for transfers, where we have data from Jan 2004. b=before

Table B1. Marginal effects of treatment and age in various model specifications of reemployment probabilities (probit) for men and women, Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

	Specification	1	2	3	4	5	6
MEN							
	Treat	0.00161	0.0122**	0.0122**	0.0137**	0.0144***	0.00275
		(0.00185)	(0.00542)	(0.00542)	(0.00551)	(0.00558)	(0.00188)
	Age		-0.00242**	-0.00243**	-0.00257**	-0.00262**	
			(0.00109)	(0.00109)	(0.00109)	(0.00109)	
	Obs No.	21,202	21,202	21,202	21,182	21,142	21,142
WOMEN							
	Treat	-0.00156	0.00342	0.00272	0.00124	0.00156	-0.00203
		(0.00228)	(0.00628)	(0.00629)	(0.00630)	(0.00628)	(0.00228)
	Age		-0.00115	-0.00110	-0.000873	-0.000834	
			(0.00132)	(0.00133)	(0.00132)	(0.00132)	
	Obs No.	15,889	15,889	15,889	15,887	15,840	15,840

Where the explanatory variables of the different specifications are as follows:

1. Treat
2. Treat, age
3. Treat, age, education
4. Treat, age, education, employment history
5. Treat, age, education, employment history, regional unemployment
6. Treat, education, employment history, regional unemployment

Table B2. Marginal effects for the reemployment probability (probit) for men, Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

VARIABLES	Men, all levels of education (excluding primary)	Men with lower secondary vocational education
Treat	0.0144***	0.0164**
	(0.00558)	(0.00684)
Age	-0.00262**	-0.00308**
	(0.00109)	(0.00128)
Lower secondar vocational	-0.00234	
	(0.0107)	
A -levels	-0.00602	
	(0.0112)	
A –levels with vocational	-0.000701	
	(0.0110)	
Post secondary vocational	-0.00393	
	(0.0114)	
College	-0.0138	
	(0.0111)	
University	0.00537	
	(0.0151)	
Regional unemployment	0.0395***	0.0469***
	(0.0139)	(0.0167)
training before July07 (dummy)	-7.86e-06	-3.15e-05
	(0.000355)	(0.000416)
months worked July06-June07	0.00687***	0.00620***
	(0.00152)	(0.00172)
casual work July06-June07	0.00281*	0.00267
	(0.00148)	(0.00171)
months worked (short) July06-June07	0.0172	
	(0.0258)	
months worked b July06	0.000261***	0.000337***
	(9.76e-05)	(0.000120)
casual work b July06	-0.000201	-5.54e-05
	(0.00125)	(0.00142)
months worked (short) b July06	-0.000281	0.000813
	(0.000421)	(0.000799)
disability benefit July06-June07	-0.000147	9.28e-05
	(0.000823)	(0.00102)
UB months July06-June07	-0.000577	-0.000919
	(0.000508)	(0.000614)
UA months July06-June07	-0.000982***	-0.000753***
	(0.000217)	(0.000255)
disability benefit months July06-June07	-0.000207	-0.000326
	(0.000393)	(0.000492)
UB months b July06	-0.000227	-0.000256
	(0.000331)	(0.000415)
UA months b July06	-9.48e-05	-0.000136
	(0.000112)	(0.000126)
Observations	21,142	15,525

Table B3. Marginal effects in various model specifications of wage outcomes (probit) for men and women, Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

	VARIABLES	(1)	(2)	(3)	(4)
MEN					
	treat	-0.113	-0.165	-0.212*	-0.200*
		(0.0945)	(0.118)	(0.115)	(0.114)
	dummy for programme period	0.167**	0.164**	0.156**	0.147**
		(0.0662)	(0.0658)	(0.0644)	(0.0614)
	interaction term	0.135	0.135	0.148	0.157*
		(0.0974)	(0.0961)	(0.0924)	(0.0893)
	age		0.0629	0.0649	0.100
			(0.342)	(0.342)	(0.347)
	Constant	6.168***	4.347	4.531	3.705
		(0.0642)	(8.288)	(8.323)	(8.430)
	Observations	471	471	471	471
	R-squared	0.088	0.090	0.137	0.174
WOMEN					
	treate	-0.102	-0.0595	-0.0473	-0.0302
		(0.129)	(0.149)	(0.155)	(0.151)
	dummy for programme period	0.309***	0.316***	0.323***	0.340***
		(0.0888)	(0.0889)	(0.0925)	(0.0933)
	interaction term	0.108	0.101	0.0948	0.0978
		(0.132)	(0.132)	(0.137)	(0.132)
	age		-0.173	-0.0982	-0.127
			(0.379)	(0.382)	(0.384)
	Constant	6.057***	10.50	8.790	9.519
		(0.0869)	(9.251)	(9.292)	(9.357)
	Observations	359	359	359	359
	R-squared	0.145	0.147	0.177	0.235

Where the explanatory variables of the different specifications are as follows:

1. Treat
2. Treat, age
3. Treat, age, education
4. Treat, age, education, employment history

Table C1 Job exit probabilities for men between 2005 and 2008, by level of education (probit)

	Pimary education	Lower secondary	Upper secondary or higher
VARIABLES	.	.	.
Year 2005	-0.0282	-0.088	-0.127***
	(0.00560)	(0.00543)	(0.00975)
Year 2006	-0.0381	-0.0919	-0.127***
	(0.00515)	(0.00571)	(0.0108)
Year 2007	reference		
Year 2008	-0.0428	-0.0779	-0.110***
	(0.00325)	(0.00486)	(0.00999)
Year 2005 * age below 50	-0.00733	-0.00152	0.0323
	(0.00561)	(0.00730)	(0.0205)
Year 2006 * age below 50	-0.0104*	-0.000477	0.00836
	(0.00561)	(0.00739)	(0.0202)
Year 2008 * age below 50	-0.00188	-0.00469	-0.00236
	(0.00355)	(0.00412)	(0.00876)
Age below 50	0.0143***	0.0036	-0.0048
	(0.0035)	(0.0040)	(0.0098)
Age (years)	0.0050***	0.0015**	-0.0016
	(0.0006)	(0.0007)	(0.0021)
Controls for work history			
Observations	108,347	97,566	23,511

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1