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Is There a Rationale to Contact the Unemployed Right from the Start? Evidence from a Natural Field Experiment

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ABSTRACT

Is There a Rationale to Contact the Unemployed Right from the Start? Evidence from a Natural Field Experiment

Active Labour Market Policies often exclusively target towards the long-term unemployed. Although it might be more efficient to intervene earlier in order to prevent long-term unemployment rather than to cure it, the climate of austerity in Eurozone countries is spreading a tendency to further reduce the basic counselling for those who become unemployed. This study investigates the impact on employment chances of a relatively light and inexpensive programme that is offered right after the start of the unemployment spell. It comprises of a collective information session followed by a short one-on-one interview. In a field experiment carried out with an employment office in Flanders, a random selection of clients (the treatment group) were invited to the programme within one month after being enrolled as unemployed, while the control group were scheduled to have the information session five months after becoming unemployed. We find a substantial intention- to-treat effect in the first four months after the start of the unemployment spell, and the early intervention seems especially beneficial for those with low education.

JEL Classification: D04, D61, J64, J68

Keywords: Active Labour Market Policies, unemployment, nudging, natural field

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1 Introduction

Unemployment has been an all-time issue on policy agendas, and became even more salient after the 2008 financial crises, from which unemployment rates in most countries have still not recovered many years later (European Commission, 2015). There are several good reasons to take unemployment as a major economic issue. First of all, it has a dramatic impact on the affected individuals and their families. People who are unemployed are less satisfied with their lives (Winkelmann and Winkelmann, 1998; Clark and Oswald, 1994) and do not mentally adapt to the fact of being unemployed (Clark et al., 2008). They also face a higher incidence of family problems such as marital break-up (Jensen and Smith, 1990). Moreover, there are the permanent scarring effects of long-term unemployment. Graduating in a recession has, for some groups of less advantaged graduates, long-term negative earning effects (Oreopoulos et al., 2012), and displaced workers are likely to suffer permanent income losses (Hijzen et al., 2010). And finally, even after being re-employed, one is likely not to fully recover in terms of mental health, possibly partly because of the fear of becoming unemployed soon again (Knabe and Rätzel, 2011). Apart from being traumatic at the individual and household level, high unemployment rates will jeopardize an economy's macroeconomic prospects. In the short-term, increased social benefit payments and reduced taxes - such as income taxes and VAT - tighten government budgets (Gerard et al., 2012), leading to higher debts or lower welfare.

Hence, it is not surprising that the design of labour market policies are constantly being debated in academia, in the media and at many policy levels. Despite the fact that public employment services, re-integration programmes and subsidies absorb a substantial amount of public spending, the impact of active labour market policies is not unambiguously positive (Card et al., 2010). Indeed, the mechanisms through which these programmes can affect unemployed workers' behaviour are rather complex. For example, intensive training programmes and subsidized jobs might lead to a locking-in effect (Van Ours, 2004), i.e., one does not have the time or effort to search for jobs due to the intensive training and coaching programme one has to participate in. However, extensive mandatory training programmes might also have a threat-effect, such that one sees a peak of the transition from unemployment to employment when the date after which such programmes become mandatory is approaching (Graversen and Van Ours, 2011). Such a peak in the exit rate is also commonly observed when reaching the date of unemployment benefit exhaustion (Lalive, 2007; Caliendo et al., 2013).

Many training or coaching programmes, especially the more expensive ones, will only be available or mandatory for those who have been unemployed for an extensive period of time. For example, Van der Klaauw and Van Ours (2013) study employment bonuses that are available for individuals with an unemployment spell that exceeds one year. Blundell et al. (2004) meticulously evaluate the employment effects of the extensive "New Deal" programme for 18-24 year olds in the United Kingdom, which involves mandatory job search assistance and wage subsidies. However, these treatments only start six months after first receiving job seekers allowance.

Moreover, in their search for further spending cuts, European governments try to replace many of their public services with digital self-service applications, and this trend can also be noticed in the labour market. For example, in the Netherlands the budget for the employment services was seriously tightened from October 2011 onwards, and in July 2013 face-to-face coaching was only available for 10% of job seekers, although after an unemployment spell of three months all clients would be invited for a one-on-one meeting as a monitoring device (UWV, 2013). Also in Flanders, since 2015 registration of the unemployed is being more centralized: while the unemployed had the option to come to a local office and to enrol face-to-face at the counter and have a chat for a few minutes with a caseworker, they are now being asked to enrol by telephone through the central service line or the Internet.

While obviously the long-term unemployed are likely the most vulnerable group with the largest distance to the labour market, one might find good reasons to intervene earlier into the unemployment spell in order to prevent long-term unemployment rather than to cure it. Indeed, the scarring effects of long-term unemployment such as lower mental health, decreased motivation and human capital, might imply that the late timing of such programmes dampens their efficiency. In addition, Kroft et al. (2013) conclude from a natural field experiment that, ceteris paribus, long-term unemployed applicants are less likely to receive a response from employers. Hence, it seems that governments are in search for the right balance. On the one hand, one aims to preserve sufficient resources for intensive re-integration programmes of vulnerable groups such as the long-term unemployed. On the other hand, one needs to make sure that unemployed workers for whom the distance to the labour market is not yet too far receive appropriate guidance and monitoring in order to accelerate the transition to work, and, most importantly, to prevent the unemployed ending up in a vicious circle of long-term unemployment.

A fairly recent literature in behavioural economics has however indicated that it is often possible to significantly steer human behaviour in a very inexpensive way. Fellner et al. (2013) investigate through a natural field experiment how to improve compliance with TV license fees in Austria. They find that merely sending out a letter asking to declare a broadcasting receiver if there is one in the household dramatically increases tax compliance, and their analysis concludes that this is due to a perceived higher risk of being caught and sanctioned. Also through a natural field experiment, Altmann and Traxler (2014) find that sending out periodical reminders dramatically increases the incidence of people scheduling regular half-yearly appointments with their dentist. Zwane et al. (2011) and Crossley et al. (2014) find evidence that merely participating in a survey can affect people's behaviour, e.g. with regards to hygiene or savings. In a labour market context, Altmann et al. (2015) investigate through a large-scale natural field experiment the effect of an information brochure, sent out four to eight weeks after becoming unemployed, on the job finding rate of German unemployed job seekers: the brochure offers information about the labour market conditions as well as on evidence-based facts such as the effectiveness and importance of devoting time to job search, the consequences of unemployment (e.g. reduced mental health), and different alternative job search strategies. They conclude that sending out such brochures has a small positive effect on the exit rate out of unemployment, at least for those people who are at risk of becoming long-term unemployed. Given the inexpensive nature of the intervention, such a campaign can however be seen as highly cost-effective.

This paper aims to contribute to this latter strand of literature, and to improve our understanding of how rather cheap interventions can improve the transition from unemployment to work. Through a natural field experiment in a coastal region in Flanders, we endeavour to investigate whether contacting the unemployed right at the start (within the first four weeks of the spell) to attend a mandatory collective information session has a favourable impact on the job finding rate. On average, we find a positive though insignificant overall effect of being allocated to the treatment group on the job finding rate. However, we have strong evidence that the strategy has a major impact on those with low education, who are generally more at risk to become long-term unemployed. As we will argue, contacting the unemployed and organizing collective sessions is a relatively cheap policy instrument, and seems highly cost-effective.

The remainder of the paper is structured as follows. Section two gives an extensive background as to better situate our study. Section three documents the treatment, the randomization, descriptive statistics and evidence of compliance. Section four details the hypotheses and the main empirical model, while Section five discusses the main results and extensions. Section six concludes.

2 Institutional Background

Belgium is a federal country with three main states: Flanders in the North, Brussels in the centre, and Wallonia in the South. Flanders is Dutch-speaking, Brussels is bilingual (French and Dutch) and Wallonia is mostly French-speaking, with in the south-east a recognized German-speaking community of around 70,000 inhabitants. The governmental structure of Belgium is rather complex, and over the last decades, several reforms have been shifting powers, authorities, and financial responsibilities between the different levels.[1]

At the time of the implementation of the trial (January 2014), financing and payment of unemployment benefits is a national matter, and is embodied by the National Employment Office (NEO). The NEO is also responsible for judging the appropriateness of the job search efforts of the unemployed, and is allowed to impose benefit sanctions if job finding efforts are repeatedly found to be below the minimum threshold. In principle, however, unemployment benefits are indefinite in time. In contrast to most other European countries, those who are receiving unemployment benefits will not be referred to a means-dependent welfare benefit system after a certain period of time has elapsed. Unemployment benefits, however, are decreasing over time, the first decrease taking place three months after being into the system from a maximum of 65% to 60% of the last-earned wage (RVA, 2015).

The coaching, mentoring and training of the unemployed is a regional matter. In Flanders, the regional employment agency is called the Flemish Employment and Vocational Training Office (FETO). In case the unemployed do not comply with the rules of FETO (e.g. repeatedly do not turn up at appointments without legitimate reason), FETO will transmit the client's file to NEO which can then decide about sanctions.

[1]For a succinct overview, see the portal of the Belgian Government at http://www.belgium.be/en

3 The Randomized Treatment and Descriptives

3.1 The Treatment

FETO has a well-established practice across all its offices to divide the unemployed in three main age categories, the category below 25 (youngsters), the category in the age bracket 25-49 (middle-aged) and the category of 50 and above (elderly) (VDAB, 2015a). The youngsters are highly prioritized and are being coached very intensively: a social experiment with this group would lead to a randomized denial and is hence hard to justify. The older unemployed have traditionally enjoyed a softer regime and were not subject to surveillance. However, from April 2009 onwards, a coaching programme for the older unemployed was introduced, with the maximum age for whom the programme is mandatory increasing from 52 in April 2009 up to 57 from 2012 onwards (VDAB, 2014). Finally, there is the middle group of those aged 25 to 49. The target is to invite unemployed workers in this age group for a first collective information session three months after being enrolled as unemployed. However, the timing of sending out this first invitation can vary a lot across individuals, and thus controlling this variation by randomizing and dichotomizing the waiting list would not violate FETO's internal ethical standards. The research project would not compromise an efficient allocation of resources either (most in particular the working time of caseworkers), and the results of the research may be used to further improve FETO's labour mediation services. Therefore FETO agreed (both at the central level and at the level of the participating office) to facilitate the research project.

Hence, this study focusses on the middle aged, and the actual randomized treatment is *the timing of sending out* a first invitation to participate in a collective information session, which can be the start of more tailored coaching. This information session, which generally takes place a couple of weeks after being invited, will last around two hours in which groups of up to 30 individuals receive information about the working of FETO, the different subsidies that are available to study or to be reintegrated into the job market, the choice set of training courses (either full-time or during the evening). Next, the website of FETO will be explained and it will be shown how one can search for job vacancies, and how one can create an account to save search preferences or to set-up a customized periodical E-mailing of vacancies. At the end of the session, participants are invited to take a place behind a work station and customize their online profile on the

FETO website after which each participant has a short one-on-one interview with one of the two present caseworkers. Individuals in the treatment group will be contacted to attend such a collective information session right after the start of their unemployment spell, i.e., within the first four weeks. The control group will only receive an invitation around four to five months after they entered unemployment, with the aim to offer them an information session five months after entering unemployment. As we will see in Section 3.3, for many of the unemployed the time of actually attending the session will be seriously delayed or will in many cases never take place. Indeed, apart from ineligible absences, there are many eligible reasons to postpone a visit to an information session, such as having a job interview scheduled on that date. The procedure can be cancelled if after enrolment and being assigned to a treatment or control group, individuals are found not to be required to attend a session (because they have found a job, because of illness, being enrolled in a full-time course or not speaking Dutch properly[2], because they move to another area or because they have turned 50 years of age).

3.2 The Sample and the Randomization

Our partner hosting the experiment is a local FETO office responsible for a region at the Flemish coast. It is the first social experiment carried out in Flanders involving FETO. This particular FETO office is known to be keen to act as a front-runner, and enthusiasm and support from a participating office is obviously essential for a successful implementation of a randomized controlled trial.

The region of our case study is relatively poor, and its unemployment rate is rather high compared to the state level (Flanders), as is shown in Figure 1 for the period January 2010 until June 2015 and for the age bracket 25 to 49. The curve representing the case study region is at all times clearly above the curve depicting the situation at the state-level. Between January 2010 and June 2015, the unemployment rate depicted for the case study region fluctuates between 5.8 and 8.9, and is between 0.6 and 2.1 percentage points higher than the corresponding state-level unemployment rate.

The sample inflow spans the period from 1 January 2014 until 31 January 2015. After that date, the inflow in the experiment has been ceased since the implementation of new

[2]Foreigners who are found not to sufficiently master the language will be exempted and will be referred to a specialized trajectory including training to improve their language skills. See VDAB (2015a) for more information.

and more centralized procedures (see e.g. VDAB, 2015b) compromises comparability with the earlier inflow. We are, however, still able to track our sample after the inflow has been ceased. The labelling for the experiment was accomplished by the central IT services based in the headquarters in Brussels. Individuals were assigned to the experiment, on the first day of the unemployment spell when the following conditions were met:

- They are residing in the area for which the FETO office participating to the case study is responsible.
- They belong to the middle aged group (25-49).
- At the time of enrolment, the central database does not flag that the individual is impeded to participate (e.g. not speaking Dutch, being chronically ill).
- They did not attend any information session during the last two years.

The use of a random generator built into many statistical software packages would be an obvious way to divide the sample into a control a treatment group. However, the nature of the trial implies that the sample is building up continuously, which complicates this procedure. Since the intervention starts almost right from the start, any delays in assignment should be avoided. Hence, to make the randomization feasible and transparent, we agreed upon a randomization rule that is based on the day-of-month of the individual's date of birth. Those who were born on an even day of the month were allocated to the treatment group, while those born on an uneven day of the month were allocated to the control group. Since the date-of-birth is a variable included in the dataset, we could easily verify that the labelling was implemented correctly by the IT services.

3.3 Descriptives and Compliance

The total sample contains 1,549 individuals, of which 789 belong to the control group and 760 to the treatment group. The slightly larger size of the control group is in line with the fact that there are more uneven days in the eight months with 31 days in our research period.

Table 1 shows descriptive statistics of both the treatment and control group of baseline values of observable characteristics. A large proportion in our sample has low education (32.1% in the treatment group versus 29.8% in the control group). In both the treatment and control group, just under half the sample is female. Furthermore, 7.6% and 8.9% are labelled as foreigners in our sample for the treatment and control group respectively.[3] Finally, it is interesting to note that only less than a third of the unemployed enrolled themselves through the online platform (28.8% in the treatment group and 30.6% in the control group).

One might wonder to what extent the differences between the treatment and control group are statistically significant from zero, and whether we can assume that indeed the randomization has worked correctly. Since we have access to the exact date-of-birth in our data, we have been able to verify that the IT services have implemented the randomization procedure correctly: those born on an even day of the month were all labelled as "treatment group, and those born on an uneven day of the month were all labelled as "control group'. Table 2 shows the estimation results of a linear probability model, with the treatment dummy as the dependent variable and baseline characteristics as independent variables, as to check whether indeed the two groups can be argued to be similar. Only the coefficient on age is statistically different from 0, at the 5% significance level. Hence, it seems reasonable to assume that we are dealing with type I error, and we will attempt to mitigate this sampling error by including baseline characteristics as controls in our estimation models.

Finally, after having checked the random distribution of individuals across the control and treatment group, an important question remains whether indeed the instructions have been followed and the experiment has been carried out correctly. Unfortunately, it was not possible to retrieve reliable data on the date individuals have received a first invitation for an information session as these are not systematically kept into the system. We do have, however, reliable attendance data, since these are being meticulously registered as they are important for monitoring purposes. As mentioned earlier, due to a variety of reasons many unemployed workers in the treatment group did not attend the information session: only 33% of the subjects in the treatment group have eventually attended a session. We can check whether the time elapsed between becoming unemployed and attending the session is in accordance with our experiment's template. Conditional

[3] One is labelled as foreigner if one of the unemployed worker's current or past nationalities is from outside the European Free Trade Association. We should emphasize that the proportion of foreigners among the unemployed in our case study region is larger than in our sample, since the experiment does not include those for whom it was known a priori that their knowledge of Dutch was insufficient to benefit from the information session.

on having followed the information session, individuals in the treatment group attended a session on average 72 days after the start of the unemployment spell, ranging from 16 to 361 days and with a standard error of 51 days. 50% of the treated individuals who actually attended an information session did so within 66 days after their inflow into the sample. As mentioned earlier, a serious delay in attending the information session does not mean that the employment agency did not comply with the template of the experiment. No-shows, holidays, an application interview, might all be reasons for a rescheduling. Moreover, only 77% of those who participated in the session did this during the unemployment spell of inflow: many would not have to come the first time since they were starting a job or interim work, but would then follow the session after the recurrence of unemployment.

The timings of the control group's attendance is more informative to judge compliance. Conditional on attending a session, individuals in the control group attended on average 208 days after entering unemployment, with a standard error of 57 days. Only two out of the 74 cases are clearly noncompliant, as they attended an information session 22 and 57 days after becoming unemployed, respectively, which is well before the threshold of five months set for the control group. The others were within the range of 157 and 364 days, which is in accordance with the experiment's template.

4 Baseline Empirical Framework and Pathways to Impact

4.1 Baseline Empirical Framework

Throughout our analysis, we will not measure the effect of the information session itself, but rather the effect of offering a monitoring and counselling procedure to unemployed workers on the outflow from unemployment to employment. The first step in this intervention is being contacted to attend a mandatory information session. Many will however not complete the procedure because they found a job before having to attend the information session. We actually measure an intention-to-treat effect. Indeed, despite filtering at the central level based on available data, after the start of the experiment the procedure has been cancelled for whom this information session was not suitable. We will not exclude these unemployed individuals from the experiment, since misclassifications and other issues will be detected more frequently in the treatment group than in the control group, and hence removing them would distort the random allocation. Moreover, the reason of cancellation is not always known and we obviously do not want to remove those who were exempted since they had a prospect to start a job in the short term.

The fact that there are individuals in the treatment group for whom the information session was not suitable is not likely to cause an overestimation of the impact of the procedure. From a policy perspective, obtaining information about an intention-to-treat effect is useful since it will usually not be possible from a practical point of view to contact only individuals who are suitable to attend: only after contacting them the employment service will get additional information which they can use to update their database. From an academic perspective, we should interpret these results as a lower bound of the effect that such an early intervention can have.

All individuals who enter our sample are initially unemployed. We will estimate the exit rate into part-time or full-time employment. Our data contain many other categories which the unemployed could transit to, e.g. full-time training, work-disabled etcetera. However, we will only concentrate on finding a job instead of a competing risk model for two sets of reasons. First, there are a few statistical reasons: our sample is too small to divide the data into many different outflow categories, and multiple categories might bias our estimates in case of irrelevant alternatives. Second, there are pragmatic concerns that weigh in. A batch procedure regularly updates the FETO datafiles with information about clients having found a job, through matching the FETO records with a central database of the Belgian social security. The transition to categories other than work would only be registered if they are entered manually (either by the caseworker or the unemployed), and since the treatment group is contacted earlier than the control group, we might risk measuring the correction of administrative files rather than an actual change in the unemployed's status.

For each individual, the research period is truncated at 120 days after being enrolled as unemployed because after that time the employment service will start inviting the control group for the information session, and other measures such as reduction of unemployment benefits might start influencing the results.[4] On average, 41% of all un-

[4]The baseline model will investigate whether during these 120 days, there is a transition to work, but will not investigate unemployment recurrence. We will however partly address this concern in one of the extensions.

employed workers have experienced a transition into work within these first 120 days. There obviously is considerable heterogeneity across groups. For example, the average transition rate for those with low education is only 34%, compared to 43% and 49% for those with intermediate and high education, respectively.

Exit rates from unemployment to employment are generally very much dependent on calendar time. The economic development as well as seasonal effects[5] will determine in- and outflow. Moreover, the limited number of observations requires us to make a careful trade-off between flexibility and efficiency. Hence, the regression model which seems most appropriate for the baseline analysis and which has been applied often in employment research (e.g. Dohmen and Pfann, 2004) is the Cox proportional hazard model.

With the Cox proportional hazard model, one can estimate the hazard rate $\lambda(t, t_0, A_T, X)$, which is the chance that one finds a job on a certain day t, conditional on the day t_0 of becoming unemployed, on a dummy A_T indicating whether one is allocated to the treatment group or not, and on a set of covariates X. The hazard rate can in turn be written as:

$$\lambda_0(t, t_0) \exp(\beta_0 + \beta_1 A_T + X\beta)$$

Where $\lambda_0(t, t_0)$ is a time-dependent baseline hazard, β_0 a constant and β a vector of coefficients to be estimated. Hence, the hazard rate is the baseline hazard multiplied by an exponential factor that depends on the values of A_t and X. The exponential function is used merely to ensure that the hazard rate will never turn negative. In the regression tables, we will show the exponentiated versions of the estimated coefficients, as they are easy to interpret as a proportional change in the baseline hazard rate. The exponentiated β-coefficients will always be strictly larger than 0: if $\beta_j > 1$, there is a positive association between the exit rate and x_j and vice versa.

The main independent variable is A_T, a dummy which takes one when being allocated to the treatment group, zero otherwise. Since the allocation to the treatment group is exogenous by construction, we do in principle not need to include controls. However, for completeness and to mitigate potential sampling bias, we will also show specifications including baseline covariates discussed in Section 3.3. We are well aware that the impact of the treatment might be heterogeneous across groups. E.g., Altmann et al. (2015)

[5]As Figure 1 illustrates, since our case study takes place in a coastal region, unemployment will peak in winter while in the remainder of Flanders unemployment peaks after the summer due to the labour market inflow of school leavers.

find that providing information has the largest impact for groups that are most at risk to become long-term unemployed. Therefore, we add an analysis which allows the treatment effect to be different for unemployed individuals with low, intermediate-level or high education, three groups across which we see a large heterogeneity in overall exit rates. Hence, the estimated hazard rate will be modified as:

$$\lambda_0(t, t_0) \exp(\beta_0 + \beta_1 A_T * E_L + \beta_2 A_T * E_{IL} + \beta_3 A_T * E_H + \beta_4 * E_L + \beta_5 * E_{IL} + X\beta)$$

With E_L, E_{IL}, and E_H dummies for low education, intermediate level education and high education respectively.

4.2 Pathways to Impact

Insights from recent behavioural economics teach us that we can change people's perceptions in a relatively inexpensive way. Being contacted early in the unemployment spell might lead to an increase in perceived social norms, that is, the expectations of friends and relatives. Since people tend to be sensitive to social norms on the importance of finding a job when choosing their actions (Ellickson, 1998), higher perceived social norms might encourage individuals to intensively search for jobs right from the start. Similarly, being contacted might lead to higher perceived monitoring. In a context of tax compliance, Fellner et al. (2013) find that rather neutral mailings can have a large impact on people's *perceived* chance of being inspected. We can expect that a very early intervention might positively affect the exit rate from unemployment through similar channels as receiving a letter to attend a mandatory information and coaching session conveys a message of strict monitoring.

Moreover, individuals might learn from the information session itself and effective counselling might lead to more successful job search. For example, research by Altmann et al. (2015) shows that merely providing information about job search strategies, the labour market and related issues will have a small positive impact on the job finding rate for some subgroups.

Furthermore, one might also expect that there are channels through which a negative impact can occur. The unemployed might feel offended to be contacted and hence monitored straight from the start.[6] Hence, one might decide to punish the employment

[6] In fact, we received some anecdotal evidence on this from the local FETO office.

agency in a way that is not too costly for themselves (Belot and Schröder, forthcoming). The latter channel is likely not to play an important role in this context, since delaying exit from unemployment (compared to the counterfactual) will always bring along substantial costs for the individual such as foregone income.

5 Results

5.1 Baseline Results

Table 3 contains the baseline results of our analysis. Column 1 presents a Cox proportional hazard model which only includes a treatment dummy. In Column 2, baseline controls have been added to the model. Columns 3 and 4 show models that are identical to the models displayed in Columns 1 and 2, respectively, but allow for a heterogeneous treatment effect across the different education levels.

The baseline specification in Column 1 shows a coefficient on the treatment dummy of 1.11, which means that the intention-to-treat leads to a multiplication of the baseline hazard rate by 1.11. Standard errors are, however, large such that the coefficient is not significantly different from one at conventional significance levels (P=0.19). Column 2 shows that including the available baseline controls hardly alters the coefficient on the treatment dummy. The coefficients on the controls reveal however some interesting patterns. The exit rate for those with low education is clearly lower than the exit rate for those with high education (with a coefficient of 0.66), and also being a foreigner is associated with almost a halving of the exit rate (coefficient of 0.56). However, the estimation results do not show any significant differences by gender and age, nor for those who enrolled through the Internet in the unemployment registration.

Columns 3 and 4 show that there are heterogeneous treatment effects across levels of education. Both models offer us almost identical results. Looking at Column 4, it turns out that, although being low educated is associated with a much lower exit rate compared to being high educated (coefficient of 0.54), the interaction term between low educated and the treatment dummy is large with a magnitude of 1.50 and a P-value of 0.01. This means that low-educated unemployed workers who are allocated to the treatment group have an exit rate which is 1.50 times higher than the exit rate of low-educated individuals in the control group. For these low-educated individuals, the impact of being allocated to the treatment group appears to be substantial, even if one only wishes to accept the lower bound of 1.09 of the 95% confidence interval as the actual impact of the intention-to-treat.

As mentioned in Section 3.2, we chose the day-of-month of one's date-of-birth to divide the sample into a treatment and control group rather than a random generator, and this was for practical reasons. However foreigners who come from less developed countries often do not have an official birth certificate, and their registered date of birth might then be a guestimate. This guestimate is then often the first day of the month or year, which would jeopardize the compliance of our experiment. Indeed, from the 128 foreigners in our data, four of them are born on January 1, which is an unusual high number. In total, 12 of them are born on the first day of the month. hence, Table 5 shows results of similar specifications as displayed in Column 3 and Column 4 of Table 3 again, but now after excluding the 12 non-natives who were born on the first day of the month. We see that the interaction term of being treated and having low education decreases somewhat, but still remains significant with a P-value of 0.02.

5.2 Cost-Benefit Analysis

The baseline models give us an idea of the shift in exit rates between the treatment and control group. For a cost-benefit analysis of this labour market policy instrument, it is desirable, to estimate the actual difference for treatment and control groups in number of days worked during the 120-days time span after a workers inflow into unemployment. The latter would also be a response to the concern that our Cox proportional hazard models do not take into account unemployment recurrence. Therefore, in Table 6, we show similar analyses as in Column 3 and Column 4 of Table 3, but now analyzing the data using a Zero-inflated Poisson count model.

The dependent variable is now the number of days one has been in regular part-time or full-time work during the 120 days after entering unemployment, which equals to zero for around 59% of the sample.[7] The Zero-Inflated Poisson Model consists of two equations. First, there is a Logit equation which estimates the odds of having worked 0 days, and a Poisson equation which estimates the workdays conditional on having worked a strictly positive number of days. The results of each of both models are

[7] As for interim work we cannot measure the exact number of days that these occasional jobs have taken. Therefore we cannot take account of these employment spells in our analyses.

presented across three columns. A first column shows us the marginal effect on workdays conditional on having worked a strictly positive number of days, a second column the change in log of odds of having worked zero days, and a final column offers us the overall marginal effect on the number of workdays.

Both models again show us a significant impact on the lower educated subsample. According to the full model, being treated and low-educated changes the log odds of having worked Zero days by -0.5. Interestingly, conditional on having worked a positive number of days, being allocated to the treatment group reduces the number of days worked by 0.06 days. The overall marginal effect, however, is positive and amounts to 6.5 (P-value of 0.04). After converting this latter number to a five-day working week by multiplying by 5/7, we find that allocation to the treatment group leads to an increase of 4.7 working days for those with low education in the 120-days time span after becoming unemployed.

An approximate cost-benefit analysis can teach us that such a programme is very cost-effective in the absence of crowding-out effects.[8] FETO has advised us that the total cost of one information session can be estimated at EUR 785.[9] Since generally 30 individuals are invited for each information session, this boils down to around EUR 26 per head. Concerning the benefits, NEO advised us that on average, the daily benefit payment to a low-educated unemployed individual amounts to EUR 38. This means that the procedure is already cost-effective if it would return one additional day of employment within these 120 days. Our estimate of 4.7 days is hence clearly above this threshold.

5.3 Increasing the Time Span

Finally, one might like to obtain an idea of the longer-term consequences for employment of being allocated to the treatment group. Therefore, table 6 repeats the second model

[8]One major concern when studying labour market policies is that higher job finding rates for treated individuals goes at the cost of lower job finding rates for untreated individuals (Crépon et al., 2013; Gautier et al., 2012), an issue which is especially salient when it concerns wage subsidies or subsidized programmes. Although we are not able to provide insight into general equilibrium effects with this setting, we can nevertheless learn about the impact on human behaviour: whether or not there are crowding-out effects will depend on the labour market conditions and will hence vary from case study to case study.

[9]EUR 750 is staff cost, half a day administration plus two times half a day for the two caseworkers being present at the information day. The cost of a room is EUR 35 for half a day.

displayed in Table 5, but now looking at the number of workdays within 150 and 180 days after entering unemployment. Hence, we now allow our research period to overlap with the time period in which individuals allocated to the control group are being contacted as well.

The overall marginal effect of being allocated to the treatment group and having low education increases to 9.0 when we extend our research period to a 150-days time span, and to 10.8 days when we extend the time span to 180 days (P-values of 0.03). After converting the results to a five-day working week, we obtain that being allocated to the treatment group increases the number of days worked by 6.4 in a 150-days time span and 7.7 in a 180-days time span.

As to further obtain an idea as to whether the difference in employment status between treatment and control group persists or rather diminishes over time, Table 7 shows us marginal effects of Probit models with the same independent variables as in Table 6. The dependent variables of the specifications are dummies which take the value one if the individual is in work on day 120, 150 or 180 respectively after entering the sample. If we again concentrate on those with low education, Column 1 of Table 7 shows us that being allocated to the treatment group increases the probability of being into work on day 120 with almost 11 percentage points (P-value =0.02). For day 150 and 180, this effect decreases to around 6 percentage points, and the estimates are not significant any more at conventional significance levels. The reasons of this slight convergence over time are obviously speculative. It might be that the individuals in the control group have taken a slightly longer "break" after becoming unemployed before looking for or accepting a job. Moreover, after four to five months, individuals in the control group are being contacted as well and become hence subject to the same procedures as those in the treatment group.

6 Conclusion

As discussed in the introduction, unemployment has been an all-time important social issue, and is especially salient in the era of the post-2008 financial crises. While it is well-known that unemployment is as a drama at the individual level and is a burden to a society's economy, there is still a lot to learn about which kind of active labour market policies are effective in which context. Although governments are well-aware of the importance to tackle unemployment, austerity measures have often led to directing resources to the long-term unemployed, and to economize on the coaching of those who freshly entered an unemployment spell. There might however be good reasons to believe that early interventions are most effective as they could prevent long-term unemployment. If workers are unemployed for a longer time they might find it much harder to get back on track because of the well-documented scarring effects reducing mental and physical health (see e.g. Knabe and rätzel, 2011) and because of the negative signal a long unemployment spell sends to potential employers (Kroft et al., 2013). Fortunately, we have learnt from recent behavioural economics that even cheap interventions can induce behaviour changes (see e.g. Altmann and Traxler, 2014; Fellner et al., 2013) and hence in this paper, we evaluate whether contacting the unemployed right after the start to attend a mandatory information session has a positive impact on the transition from unemployment into work. While those allocated to the treatment group were contacted within the first four weeks of the unemployment spell, those allocated to the control group were contacted around four to five months after entering unemployment with as an aim to offer them a session five months after registration.

We find that contacting the unemployed at the start of the cycle will indeed lead to a positive impact: especially those with low education will benefit, and will have worked 4.7 days more than their counterparts in the control group during the first four months after entering unemployment. Since many individuals will in the end never attend an information session, we hypothesize that contacting in itself to attend a mandatory session serves as a nudge to start applying for and accepting jobs much earlier into the unemployment spell.

Obviously our results should not be used to argue that the intervention should only be applied to those with low education: while the intervention is very cheap, it would be cost-effective even if on average, it would lead to less than one additional day of employment. Our sample however (n =1,549) does not offer us sufficient statistical power to measure such small effects with statistical significance. Moreover, in other institutional contexts, the size and the distribution of the impact of similar early interventions might be different from the one we studied, and it is clear that potential crowding-out effects are equally context-dependent. The main message that can be taken from our study is that relatively cheap interventions (with a mandatory component) targeting freshly-unemployed individuals can have a significant impact on the transition into work, making these interventions highly cost-effective. Our paper hence contributes to the recent behavioural economics literature that has shown that small nudges can induce relatively large behaviour changes. Moreover, it can contribute to an on-going policy debate on how to allocate resources to active labour market policies: it shows that a minimum availability of human coaching for those who have just become unemployed is likely to be very cost-effective,

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Table 1: Baseline Characteristics Treatment and Control Group

Characteristic	Treatment	Control	T - C
% low education	32.1	29.8	2.3
% intermediate education	49.1	53.4	-4.3
% female	47.4	49.3	-1.9
average age	35.7	36.5	-0.8
% foreigner	7.6	8.9	-1.3
% enrolment through Internet	28.7	30.5	-1.8
% inflow Quarter 1	26.7	24.8	-1.9
% inflow Quarter 2	19.3	18.2	1.1
% inflow Quarter 3	24.5	24.1	0.4

Table 2: Investigating Statistical Differences between Treatment and Control Group

	treatment
low education	-0.003
	(0.039)
intermediate education	-0.051
	(0.036)
age	-0.004
	(0.002)**
female	-0.020
	(0.026)
enrolment through Internet	-0.036
	(0.029)
foreigner	-0.047
	(0.047)
inflow month 2	-0.060
	(0.064)
inflow month 3	-0.034
	(0.058)
inflow month 4	-0.019
	(0.062)
inflow month 5	-0.009
	(0.066)
inflow month 6	-0.048
	(0.061)
inflow month 7	-0.031
	(0.058)
inflow month 8	-0.055
	(0.060)
inflow month 9	-0.026
	(0.056)
inflow month 10	-0.100
	(0.056)*
inflow month 11	-0.056
	(0.062)
inflow month 12	-0.083
	(0.067)
inflow month 13	-0.026
	(0.054)
Constant	0.722
	(0.079)***
R^2	0.01
N	1,549

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Standard Errors in Parentheses

Table 3: Estimation of Transition to Work Using Cox Proportional Hazard Models: Baseline Results

	spec 1	spec 2	spec 3	spec 4
treatment	1.109	1.099		
	(0.088)	(0.087)		
treatment * low educ			1.511	1.497
			(0.242)***	(0.240)**
treatment * intermediate educ			0.994	0.979
			(0.107)	(0.106)
treatment * high educ			1.007	1.024
			(0.173)	(0.176)
low education		0.666	0.513	0.541
		(0.079)***	(0.090)***	(0.096)***
intermediate education		0.871	0.872	0.888
		(0.089)	(0.126)	(0.130)
female		0.897		0.901
		(0.071)		(0.072)
age		0.998		0.997
		(0.005)		(0.005)
enrolment through Internet		1.104		1.108
		(0.096)		(0.097)
foreigner		0.562		0.562
		(0.101)***		(0.101)***
N	1,549	1,549	1,549	1,549

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Standard Errors in Parentheses

Coefficients Are Exponentiated

Table 4: Estimation of Transition to Work Using Cox Proportional Hazard Models: Removing Foreigners Born on First Day of Month

	spec 1	spec 2
treatment	1.091	
	(0.086)	
low education	0.670	0.550
	(0.080)***	(0.097)***
intermediate education	0.869	0.883
	(0.089)	(0.129)
female	0.890	0.895
	(0.071)	(0.071)
age	0.998	0.997
	(0.005)	(0.005)
enrolment through Internet	1.103	1.107
	(0.096)	(0.097)
foreigner	0.600	0.597
	(0.110)***	(0.109)***
treatment * low educ		1.463
		(0.234)**
treatment * intermediate educ		0.979
		(0.106)
treatment * high educ		1.017
		(0.175)
N	1,537	1,537

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Standard Errors in Parentheses

Coefficients Are Exponentiated

Table 5: Estimation of Days Worked within 120 Days after Inflow Using Zero-Inflated Poisson Models

	Poisson	Logit	Overall marginal	Poisson	Logit	Overall marginal
treatment * low educ	-0.067	-0.529	6.657	-0.063	-0.517	6.510
	(0.019)***	(0.196)***	(3.183)**	(0.020)***	(0.199)***	(3.186)**
treatment * intermediate educ	-0.011	-0.007	-0.182	-0.014	0.023	-0.739
	(0.013)	(0.144)	(2.329)	(0.013)	(0.146)	(2.336)
treatment * high educ	-0.041	-0.033	-0.591	-0.063	-0.045	-0.976
	(0.022)*	(0.241)	(3.908)	(0.022)***	(0.245)	(3.917)
low education	0.115	0.886	-11.080	0.103	0.822	-10.250
	(0.022)***	(0.227)***	(3.676)***	(0.022)***	(0.234)***	(3.747)***
intermediate education	0.062	0.207	-1.635	0.048	0.207	-1.989
	(0.018)***	(0.200)	(3.234)	(0.019)**	(0.205)	(3.284)
female				0.022	0.166	-2.045
				(0.010)**	(0.106)	(1.698)
age				0.000	0.002	-0.023
				(0.001)	(0.007)	(0.116)
enrolment through Internet				0.009	-0.159	2.768
				(0.011)	(0.121)	(1.930)
foreigner				-0.109	0.668	-13.477
				(0.024)***	(0.214)***	(3.434)***
N			1,549			1,549

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Standard Errors in Parentheses

The second model includes month-of-inflow dummies.

Table 6: Estimation of Days Worked within 150 and 180 Days after Inflow Using Zero-Inflated Poisson Models

	150-days time span			180-days time span		
	Poisson	Logit	Overall marginal	Poisson	Logit	Overall marginal
treatment * low educ	0.052	-0.334	9.011	0.080	-0.256	10.823
	(0.016)***	(0.192)*	(4.080)**	(0.014)***	(0.190)	(5.036)**
treatment * intermediate educ	-0.009	0.052	-1.426	-0.060	-0.073	-1.162
	(0.011)	(0.145)	(3.068)	(0.010)***	(0.144)	(3.829)
treatment * high educ	-0.024	0.019	-1.312	-0.053	-0.033	-1.869
	(0.018)	(0.245)	(5.193)	(0.016)***	(0.246)	(6.507)
low education	-0.018	0.734	-16.128	-0.047	0.723	-21.414
	(0.019)	(0.229)***	(4.862)***	(0.016)***	(0.228)***	(6.041)***
intermediate education	0.006	0.191	-3.763	-0.009	0.190	-5.461
	(0.015)	(0.205)	(4.342)	(0.013)	(0.205)	(5.429)
female	-0.006	0.123	-2.830	-0.014	0.105	-3.498
	(0.008)	(0.105)	(2.219)	(0.007)**	(0.104)	(2.762)
age	0.000	0.002	-0.030	0.001	0.006	-0.098
	(0.001)	(0.007)	(0.152)	(0.000)**	(0.007)	(0.189)
enrolment through Internet	0.020	-0.146	3.834	0.065	-0.012	3.645
	(0.009)**	(0.119)	(2.531)	(0.008)***	(0.119)	(3.161)
foreigner	-0.120	0.660	-18.465	-0.112	0.658	-23.029
	(0.019)***	(0.205)***	(4.364)***	(0.017)***	(0.200)***	(5.320)***
N			1,549			1,549

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Standard Errors in Parentheses

All models include month-of-inflow dummies.

Table 7: Marginal Effects of Probit Estimations on Working at Day 120, 150 and 180 Respectively after Inflow in Sample

	at day 120	at day 150	at day 180
treatment * low educ	0.283	0.164	0.147
	(0.123)**	(0.119)	(0.119)
treatment * intermediate educ	0.003	-0.036	0.027
	(0.091)	(0.091)	(0.090)
treatment * high educ	0.032	-0.053	0.023
	(0.153)	(0.153)	(0.152)
low education	-0.534	-0.465	-0.420
	(0.145)***	(0.142)***	(0.142)***
intermediate education	-0.152	-0.160	-0.128
	(0.128)	(0.128)	(0.128)
female	-0.086	-0.050	-0.059
	(0.066)	(0.065)	(0.065)
age	-0.002	-0.004	-0.006
	(0.005)	(0.004)	(0.004)
enrolment through Internet	0.101	0.037	-0.007
	(0.075)	(0.075)	(0.075)
foreigner	-0.413	-0.387	-0.389
	(0.130)***	(0.125)***	(0.125)***
N	1,549	1,549	1,549

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Standard Errors in Parentheses

All models include month-of-inflow dummies.

Figure 1: The Course of Unemployment over Time at the State Level and the Case Study Region: Ages 25-49