Long-Term Effects of a Mandatory Multistage Program: The New Deal for Young People in the UK

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PRELIMINARY
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Abstract

The New Deal For Young People is the major welfare-to-work program in the UK. It is a mandatory multistage policy targeted at the 18-24 year old unemployed. This paper investigates the effectiveness of the program in terms of enhancing the (re)employment probability of participant males. I exploit the eligibility rule to identify a suitable counterfactual relying upon a simple regression discontinuity design. By exploiting such a discontinuity I am able to non parametrically identify (Hahn et al., 2001) a local average treatment effect (LATE). While relying upon the non parametric local linear regression method I am able to push forward such a parameter to a "global" dimension, implicitly adding parametric structure. No evidence of possible general equilibrium as well as substitution effects is found by a cohort specific approach (before and after the program). The main result is that the program enhances employability by about 6-7%.

JEL: J18, J23, J38, C14

Keywords: Labour market policy evaluation, regression discontinuity, non parametric.

1 Introduction

In the past decade there has been an increasing interest on labour market policies, especially on the shift from benefits to work. A number of welfare-to-work policies have been implemented (Heckman et al., 1999; Boeri et al., 2000; Van Reenen, 2001; Blundell, 2002), some are targeted

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towards particular groups: disadvantaged, youths, lone parents and so on. The framework of such policies is generally twofold: first, benefits are only provided to those who comply with the requirements of the program and second, those requirements are normally aimed at improving skills and employability. Such distinctive characteristics are shared by the policy I evaluate in this work: the New Deal for Young People (NDYP) in the UK.

The NDYP is the major welfare-to-work program in the UK, about one million young britons have been involved by December 2003, it is targeted at 18-24 year old[1] unemployed (receiving Job Seeker Allowance, JSA[2]) for at least 6 months. It is a mandatory multistage program (Section, 2), where the sanction for non compliers is the withdrawal, at least temporarily, from the benefit. The first part of the program (gateway) is devoted to intensive job search, followed (if unsuccessful) by an option and eventually by a follow-through period (similar to the gateway, but shorter). The natural aim of the policy is to improve employability both at the extensive and intensive margins, while acquiring skills and motivation.

The program was launched in January 1998 in selected areas (pilot period) and extended to the entire UK (national roll-out) by April of the same year. I do not intend to cover the pilot period in this study. This has already been the topic of Blundell et al. (2004). They found for males an average treatment effect of about 10%, in terms of (re)employment probability, for the pilot group. Such estimate halved for the national roll out, however their data end in July 1999. Their treatment is intended as job search assistance, the main component of the gateway period, since the outcome of interest is defined as (re)employment probability within 4 months since entering the New Deal. While I define as treatment the whole program consisting of job search assistance, training/education, subsidies and job experience (voluntary sector or environmental services). I analyse the program in a long term perspective both from the viewpoint of the outcome and the time interval considered. The identification structure (Section, 3) imposes a minimal set of assumptions, consistently with the discontinuity design, and the estimation relies on nonparametric local linear regression[3].

According to politicians and program administrators we are looking at a success story in roughly all its component. Here is part of a piece written by Andrew Smith (2004) the former Secretary of State at the Department for Work and Pensions:

[1]Such an age bracket has been chosen for political and financial reasons. Before the implementation of the policy some cost simulations have been done in order to set the age eligibility limit to 24 years instead of a slightly older or younger age.

[2]JSA is the only unemployment insurance targeted at the group of interest in this work. In order to be eligible the unemployed has to be willing and able to work (previous employment history is not required) and before the introduction of the New Deal only weak requirements were imposed in order to receive JSA in principle indefinitely.

[3]Blundell et al. (2004) rely on a difference in difference matching estimator.

"The Government investment in the New Deal and Jobcentre Plus has helped to deliver one of the most effective labour market programmes in the World....".

While a program participant states (www.newdeal.gov.uk):

"If it weren't for New Deal, I wouldn't be here now. They helped me and they pushed me when I needed it. I've got a lot more confidence and I've got skills."

The main questions answered in this work are: is the policy really improving employment prospects for young males? Are the effects of the policy lasting over different cohorts?

The focus will be on the identification of the program effect on the (re)employment probability of participant males[4]. There are several potential outcomes of interest in this respect, i.e. probability of being employed at some point in time or probability of gaining employment in a given interval. I concentrate on the (re)employment probability within 18 months since starting the JSA spell[5], given 6 months of unemployment. Such an outcome allows to evaluate the effectiveness of the entire program not distinguishing among different type of treatments. Here treatment is understood as being a combination of job search assistance, training, subsidies and some work experience (voluntary sector or environmental services). However, concerns may be raised on possible anticipation effects and behavioural changes due to the mere existence of the program. I.e., if the program is perceived as being able to significantly increase the (re)employment probability it might produce a strong disincentive and a lower effort level since it would be beneficial to wait in open unemployment and benefit from the program. This would result in an upper bias in the estimate of the program effect given that the average treated would be of better quality than otherwise. Some of the participants could have found a job anyway. However, if such anticipation effects are relevant, it should be the case that the cost of waiting, receiving JSA instead of a proper salary, is a decreasing function of unemployment duration. It is quite costly[6] to wait for 6 months in open unemployment. While it might not be so costly to wait for a shorter period. Therefore, if an anticipation effect has to be noticed it should be relevant in the last few weeks before the sixth month. However, I cannot find evidence of such behaviour by looking at Figure 7, the survival functions do not present any

[4]The vast majority (75%) of participants are males. Furthermore, the NDYP is basically the only program available to young males while there are other programs for females not easily distinguishable and therefore source of potential identification problems.

[5]The starting date of the JSA should coincide with the start of the unemployment period. The 18 months cutoff point is due to the fact that the control group I am going to exploit later on would enter an ALMP after 18 months in open unemployment. A shorter period would not consider those participants who take the education and training option (Section, 2). For later cohorts, it might be that some of the controls had a previous spell in the program. However, this would still be consistent but the estimated effect would be the effect of the program at a given point in time.

[6]Even if stigma effects are discarded from the analysis. JSA is about 40 pounds per week while minimum wage is 4.5 pounds per hour.

sort of inducted behaviour at the tail and they are consistent with the same functions plotted for a cohort before the program was launched[7].

As standard in the evaluation literature the problem reduces to that of missing outcome (Heckman et al, 1999; Blundell and Costa-Dias, 2000). A given individual cannot be in two different states at the same time. He/she is either in the program or out of it. Therefore I have to identify a suitable missing counterfactual. In a non experimental study exercise, such a problem is exacerbated due to the nonexistence of an administered control group and in the specific case due to the global implementation of the program: everyone in the UK who is younger than 25 after 6 months in open unemployment is forced into the program.

In this work my approach (Section, 3) would be that of a Regression Discontinuity (RD) design (Thistlethwaite and Campbell, 1960; Hahn et al, 2001). It seems to be rather appropriate given the nature of the eligibility rule (six months of JSA, plus younger than 25). The intuition behind such an approach is that participation changes according to a known deterministic function at a discontinuity point. Unemployed slightly younger than 25 are in, while those slightly older are out. There are not other differences, apart from the treatment status, between treated and untreated in the neighborhood of the discontinuity[8]. This gives rise to a natural comparison, nonparametrically recovering a Local Average Treatment Effect parameter (LATE) under a very weak assumption. The LATE parameter extends to the ATE in the case of constant treatment effect or under particular smoothness conditions.

The data (Section, 4) for the analysis result from a combination of two datasets: the New Deal Evaluation Database (NDED), containing a very detailed set of information on virtually all participants and no information on controls, and the JUVOS data, a 5% sample of all unemployed claiming JSA in the UK. JUVOS contains information, on both treated and controls, such as: start and end date of JSA spell, occupation, date of birth. The NDED is meant to complement the JUVOS where the latter fails to have adequate information on exit from JSA as explained in Section 4.

Consistently with the non parametric identification strategy, the estimation (Section, 5) is implemented by Local Linear Regression (LLR) known to have desirable boundary properties (Fan, 1992; Porter, 2003). I am ultimately estimating at a boundary point, where the size of

[7]If anything there is a limited, not significant, evidence that potential treated (after the program) tend to leave unemployment at an higher rate in the last 15/20 days before the sixth month, therefore reinforcing the estimates. A similar analysis has also been performed for the control group, given the existence of the above mentioned program after 18 months in open unemployment. Also in this case there is no evidence of anticipatory behaviours, such results are available from the author upon request.

[8]Near the cutoff point the RD design mimics a random assignment and it is often referred to as a quasi-experimental method (Hahn et al., 2001; Porter, 2003).

the discontinuity is the parameter of interest. The difference between the two conditional mean functions from both sides of the discontinuity will recover the LATE. A simple Montecarlo study (Section, 6) is performed in order to confirm the appropriateness of the estimator proposed.

A first glance of the strategy and results can be given by plotting the conditional mean functions ((re)employment probability by age) for several quarterly cohorts before and after the implementation of the program. In Figure 4 it is clearly visible that before the program the line is fairly smooth and consistent with the hypothesis of continuity of the non-program outcome, while in Figures 5 and 6 the same function exhibits a relevant discontinuity post-program exactly according to the eligibility rule. Those individual to the left of the discontinuity (treated) have a higher chance of gaining employment. The local parameter is of interest to policy makers on its own right, it defines the causal impact of the program on those individuals who are in the neighborhood of the cutoff point. Furthermore, it might be "the" parameter of interest if the idea under scrutiny is that of extending the policy marginally (to a slightly older group). However, I will try to give a more extensive interpretation of the findings relying upon the non parametric function arising from the estimates before and after the program. In so doing I will impose more structure to the problem and therefore lose the fully non parametric identification. In addition, I will follow a cohort specific approach. Blundell et al. (2004) found a significant effect of the program of about 10% for the pilot period, which almost halved for the national roll-out. It is therefore interesting to check whether the program has had only an initial effect due to die out over time. Such an approach will also give some crude evidence on the relevance of general equilibrium effects.

Almost by definition the RD method derives its appeal by the a priori consideration that control and treatment units are almost identical near the discontinuity, suggesting a high degree of substitutability. If the program has the effect of substituting treated for control units the estimates would be biased upwards. This concern mainly arises from the subsidised employment option[9], it might render treated cheaper than controls and therefore could displace the latter for the former. Nevertheless, Katz (1998) found that in a similar program the take-up rate for the subsidy is rather low and its employment impact negligible when left alone. In a targeted program, as the NDYP, receiving a subsidy can place a significant stigma on the participant. He is only employable thanks to the subsidy, otherwise he would have not found a job.

On the other hand relevant general equilibrium effects, i.e. increase in labour supply lowering equilibrium wages, would push employment up for treated and controls. It might also be that

[9] It is worth mentioning that such displacement might also arise from enhanced job search. However, in this respect it might simply be that the matching function is improved, vacancies are filled in more efficiently, without affecting the outcome of the control groups.

improved macroeconomic conditions, general equilibrium and substitution effects could roughly cancel out each other. I will devote part of this work to the investigation of such side effects. The rest of the paper is organised as follows: Section 2 describes various features of the program; Section 3 covers the identification strategy adopted; 4 carefully describes the data used; 5 describes the estimation strategy; 6 provides some montecarlo evidence on the performance of the estimator; 7 presents the results; 8 addresses the substitution puzzle and 9 concludes.

2 The Program

As from Figure 1 the NDYP is a sequential program, where different treatments are offered to the participants. Following a period of six months[10] in "open" unemployment 18 to 24 year old (JSA recipients) are forced into the program in order to be still eligible for the benefit. It is therefore a mandatory policy administered to everyone in the UK who, after six months of unemployment, are aged between 18 and 24.

The first four months (Gateway period) are (nominally[11]) devoted to intensive job search assistance and some basic skill training, eg. CV writing. Participants are obliged to meet a personal mentor once every two weeks and they have to report and prove the actions taken in order to gain employment. Such actions typically consist of applications, direct contact between possible employers and caseworker, etc..

While in the gateway the participant receives a benefit equal to the JSA (about 40 pounds per week). If a regular job is not found during the gateway, a second phase follows: the options. On the basis of personal considerations, given individual characteristics, the caseworker agrees with the participant on the option to be taken[12]. The option period can last from 6 to 12 months (full time training or education); such second stage is compulsory as well. Common practice among units of delivery was to try placing the unemployed in a subsidised job during the second month of treatment. In case of a subsidised employment, the treated receives the salary paid by the employer who gets, for a maximum of six months, a subsidy of 60 pounds per week plus 750 pounds as a one-off payment for the compulsory (minimum) one day a week training to be provided[13]. The second option, education or training, is targeted at youths

[10] Only a very small number of unemployed, not included in the analysis, can access the program earlier than the sixth month. This particular group is composed by ex-offenders, disable and unemployed lacking very basic skills (writing and reading difficulties).

[11] Nervertheless, in the data, some individuals enter an option during the gateway period. The first guidelines given by the government stated clearly that one could exit the gateway period only toward a regular job. Later, they were adjusted according to the *de facto* behaviour.

[12] This is not always the case since certain units of delivery tend to favour a particular option.

[13] Such subsidy seems quite generous when compared to the sort of hourly rate (close to the minimum wage) a typical participant would get. In a crude computation, the weekly subsidy plus the one-off payment would

lacking basic skills. It can last up to 12 months; while attending such courses the unemployed still receives his JSA payment. A third option is that of a voluntary sector job where the participant receives an amount at least identical to the JSA plus 400 pounds spread over the six months. The same monetary treatment is granted in the fourth option: Environmental Task Force, basically a governmental job, meant to be the last possible placement. Participant are allocated to these last two options in the third and fourth month of the gateway. Eventually a third phase follows: the follow-through, essentially maximum of 13 weeks similar to the initial gateway. It consists of intensive job search as well as training courses to maintain the skills acquired during the option period. The program was launched in January 1998 in selected areas (pilot period) and was extended to the rest of the nation in April of that year (national roll-out). The program has involved about 1 million young britons by December 2003, of which roughly 75% are males.

As mentioned earlier the aim of this work is to quantify the long run impact of the program in terms of (re)employment probability. The outcome of interest is defined as a treatment effect in the "Black box" (the shaded area in Figure 1). This is because I do not distinguish among different stages of the program (gateway or options), but I concentrate on the effect of the program as a whole. The outcome of interest is the (re)employment probability within 18 months since qualifying for JSA (basically, becoming unemployed)[14]. The choice of such an outcome is determined by the interest in long run effects of the program, while short run effects have already been investigated. The 18 months limit arises from the fact that unemployed older than 25 are forced in a similar program (New Deal for Long Term Unemployed) after 18 months of open unemployment. The latter would make a comparison on a longer time interval misleading.

Another important aspect to notice is that the program is one of global implementation and therefore there could be concerns about possible general equilibrium effects, dictated by the increase in the overall labour supply, denied by a partial equilibrium approach. However, if such effects are relevant they should be increasingly so as the program broadens and involves more and more individuals. I tackle this issue relying upon a cohort specific approach, namely I analyse the impact of the program for fifteen quarterly cohorts entering the program from

amount to about 50% of a weekly pay for a minimum wage worker, however the 750 pounds would have to repay for the loss of production due to the minimum of one day training. Under very simple assumptions (perfectly competitive markets) those 750 pounds would not be enough to compensate for that loss. In fact, taking the latter into account the subsidy would not be greater than 30%, but still generous though. However, job turnover could be itself quite costly making such an option not as appealing as it looks like at a first glance. This point seems to be confirmed by the low take up rate in the data, only a sixth of those entering an option would go for the subsidised job.

[14]Given at least 6 months of JSA, such condition is necessary to define program participation.

April 1998 to December 2001[15]. An initial test of the importance of general equilibrium effects is given by the simple time path of the program impacts. On the other hand I have also to consider possible substitution effects, if 18-24 year old are good substitute for 25-30 then we should see the former replacing the latter and therefore the program effect would be amplified by the substitution effect violating then the hypothesis according to which the non-treatment state is synonymous for non program state (SUTVA). I approach this potential source of bias by looking at treated and controls before and after the program as well as by comparing 31-36 to 25-30 year old before and after the program (Sections 3, 8).

3 Identification Strategy

As explained in the previous sections, participation in the program is compulsory and established by a deterministic rule: six months of JSA plus younger than 25. This gives an immediate comparison group or a so called "sharp" Regression Discontinuity (RD) design (Thistlethwayte and Campbell, 1960; Hahn et al, 2001) where the discontinuity in the treatment is given by the age rule informing the program. The RD approach is a quasi-experimental design where the known discontinuity is exploited for identification. If we believe that without the program individuals in the neighborhood of 25 years will perform the same, this allows to identify the effect of the program at least for those near the discontinuity under a local continuity assumption.

In a very simple chart the treatment function in a "sharp" RD design would look something like Figure 2.

Though it is quite often the case, i.e. geographic boundaries or eligibility rules, that policy designs give rise to some sort of discontinuity in the treatment function such an approach has not been used so often for evaluation purposes (Hahn et al, 2001). The advantage of such a method relies on the minimal set of assumptions required for the identification of a Local Average Treatment Effect (LATE) parameter[16].

Formally, let D be the program participation status. $D = 1$ for participants, $D = 0$ for non-participants. (Y^1, Y^0) be two potential outcomes, resulting from participation/non-participation respectively and $Y = Y^0 + D(Y^1 - Y^0)$ the observed outcome. The impact from participation is defined as $\beta = Y^1 - Y^0$. The eligibility rule $D = 1(A < a)$ is a known deterministic step function of A (age, continuous) and steps from 1 to 0 at a (25 years).

[15] This limit is imposed by the available data.

[16] The LATE can be on its own right an interesting parameter or even 'the' parameter of interest if the idea under scrutiny is that of extending the program marginally or to capture the effect of the program on the particular subgroup. Obviously the LATE does not translate into an ATE unless constant treatment effect is assumed or under some particular smoothness conditions.

Taking the mean outcome difference for those marginally below (a^-) and above (a^+) the threshold a:

$$E[Y|a^-] - E[Y|a^+] = E[Y^0|a^-] - E[Y^0|a^+] + E[D\beta|a^-] - \underbrace{E[D\beta|a^+]}_{=0 \text{ by design}} \tag{1}$$

ASSUMPTION(1): $E[Y^0|A]$ continuous at a. Then the mean program effect on the treated

$$E[\beta|a^-] = E[Y|a^-] - E[Y|a^+] \tag{2}$$

is identified in the neighborhood of the threshold a.

However, I might observe Y^2 (non treated outcome) instead of Y^0 (non program outcome) since there might be substitution effects[17], treated might substitute controls at the threshold because they might be "cheaper".

Replacing Y^2 to Y^0 in the observed outcome and proceeding as before, instead of (2), by adding and subtracting the same quantity (ASSUMPTION (1)), I get:

$$E[Y|a^-] - E[Y|a^+] = \underbrace{E[Y^1|a^-] - E[Y^0|a^-]}_{E[\beta|a^-]} + \underbrace{E(Y^0|a^+) - E(Y^2|a^+)}_{SB} \tag{3}$$

Where $E[\beta|a^-]$ is the parameter of interest and SB the substitution bias. The substitution bias is potentially important if the subsidised employment option has a large take-up and if treated are effectively cheaper than controls. However, I can provide some evidence on the absence of any substitution bias.

By considering a cohort approach. Let me rewrite (3) as:

$$E(Y|a^-, c) - E(Y|a^+, c) = E(Y^1|a^-, c) - E(Y^0|a^-, c) + E(Y^0|a^+, c) - E(Y^2|a^+, c)$$

where c is a cohort after the program. Let me rewrite $E(Y^2|a^+, c) = E(Y^0|a^+, c) - SB$ and assuming: ASSUMPTION(2a): $E(Y^0|a^+, c) = E(Y^0|a^+, c')$ where c' is a cohort before the program. If $E(Y^0|a^+, c') = E(Y^2|a^+, c) \rightarrow SB = 0$. And $E[\beta|a^-]$ is identified.

It remains to justify why cohort c' is not affected by substitution. There are a number of reasons why this might be the case. Cohort c' is obviously taken before the program started,

[17] I left aside the discussion on possible general equilibrium effects because for the parameter I am identifying those effects should not be relevant. In the neighborhood of the discontinuity, even if there is an increase in labour supply (given the number of participant involved) easing the wage pressure and the equilibrium wage, such an effect should be common to treated and untreated and therefore should roughly cancel out.

the last cohort prior to the program will be the most similar to the one after the program given the economic environment. However, since the outcome I am considering spans over a year after the 6 months of unemployment, c' could in principle compete with cohort c and some of the others. In fact, substitution happens in the first 4 months of treatment, through subsidised placement, among similar individuals, if treated are cheaper than non treated, but for these two cohorts there are not similar individuals, since those in cohort c' have a different unemployment duration then those in cohort c when they are supposed to compete for the same job.

Another way to deal with the possible substitution bias is that of using as a reference group that of the slightly older otherwise identical unemployed. Namely, let me consider the 31 to 36 year old who should be pretty similar to the 25 to 30 year old but definitely a different group with respect to the 19-24. The slightly older should not be affected by substitution. Let me rewrite $E(Y^2|a^+) = E(Y^0|a^+) - SB$. The crucial assumption would be ASSUMPTION(2b): $E(Y^0|a^+) = E(Y^0|a_1^+)$ where $a_1^+ = 31^+$. Again, if $E(Y^0|a_1^+) = E(Y^2|a^+) \rightarrow SB = 0$. And $E[\beta|a^-]$ is identified.

4 Data

A ready made dataset does not exists for the purposes of this work. However, it is still possible to recover most of the information needed by combining an administrative dataset (New Deal Evaluation Database, NDED) purposely built and containing virtually all participants, and the publicly available 5% longitudinal sample of UK unemployed (JUVOS). In the latter, it is possible to identify treated and control group looking at the eligibility rule. It is known that 18-24 year old receiving JSA for 6 months constitute the eligible and almost entirely treated population. The JUVOS dataset contains date of birth, geographical region of residence, starting and end date of JSA spell, gender, usual and sought occupation and destination on exit from JSA, but has no information after the end of the JSA spell. There are a number of exit categories recorded: found a job, other benefit, retired, prison, attending court and education and training. The last two exits are one of the option of the NDYP while no equivalent exists for the control group, at least in the time interval considered. The controls who exit JSA for such destinations are almost certainly involved in small scale programs or simply decided on their own to acquire some training or education. In fact, such exit has half of the relevance for controls compared to treated. Given the presence of such exit categories and the structure of the JUVOS data I would not know whether an unemployed (whose reason for ending the JSA spell is training or education) will find a job within the relevant period. Therefore, for such observations I have to complement the JUVOS data with the administrative data set (NDED). The NDED contains a number of extremely detailed information on participants, i.e. date of entry and termination of New Deal spell, date of birth, region of residence, unit of delivery, type of actions taken to find a job, number of letters sent to potential employers, option attended, status after ending the treatment, reasons for leaving the New Deal and so on. From the NDED, I can recover the exact[18] exit rates to employment for participants (in the particular period of interest). Therefore by using this complementary information, I can input such exit rates for the treated in the JUVOS data. An example might be helpful in clarifying this point, suppose some treated (identified in the JUVOS data) end their JSA spell to improve their education or attend some training (education/training option) I would not know, from JUVOS only, whether they found a job within 18 months since their JSA experience started. However, I can get such information from the NDED, where I know exactly how many of them actually found a job in such a time interval and I can therefore input such information to the JUVOS data. Unfortunately, such a complementary information is not available for the control group, no controls are included in the NDED, however I can still define three different estimates of the parameter of interest by hypothesizing three alternative scenarios:

1. symmetric exit rates by age and cohorts for treated and controls;
2. all controls, who enrol into a training/education program in the time interval of interest, get a job in the time horizon considered;
3. none of the controls who attended some education/training course get a regular job by the time interval of interest.

These strategies will allow to define a best estimate, a lower and an upper bound respectively.

In order to avoid the inclusion of high-school kids 18 year old will be discarded from the analysis. I define fifteen quarterly cohorts, according to the date of entry in the program, spanning from April 1998 to December 2001 (Table, 1). Each cohort counts approximately a thousand observations or more and coherently with the RD design there is almost an identical number of treated and controls in each one. As written earlier the key of the identification relies on the discontinuity in the participation rule and on the apriori belief that in the neighborhood of such point unemployed are almost identical but for the treatment status. Such belief can be confirmed by looking at the occupational (usual and sought) distribution in the proximity of the discontinuity (Tables, 2 and 3). The narrower is the age interval considered the more similar are the distributions.

[18] As mentioned the NDED records information on all participants.

5 Estimation

The estimation of the parameter of interest is performed nonparametrically by Local Linear Regression (LLR)[19]. The LLR method consists in running several local linear weighted regressions where the weights are assigned according to a kernel function (satisfying some regularity conditions) and a bandwidth. In general, observations close to the estimation point are given larger weights while decreasing weights are assigned to those further away. The estimation in an RD design boils down to estimating at a boundary point, where y^- and y^+ are estimated using observations from the left and right of the discontinuity respectively. The estimate of y^- is given by $\hat{\alpha}$:

$$(\hat{\alpha}, \hat{\beta}) \equiv argmin_{\alpha,\beta} \sum_{i=1}^{n} (y_i - \alpha - \beta(a_i - a))^2 K\left(\frac{a_i - a}{h}\right) 1(a_i < a).$$

Where $K(.)$ is the Kernel function and h an appropriate bandwidth. It is a known result that constant kernel methods have poor boundary performances due to the lack of observations on one side of the boundary. Such a problem could even be exacerbated in the current context, given that I would compound the bias from both sides of the discontinuity. The LLR method proposed attains the optimal convergence rate due to the local linear approximation (Porter, (2003)) under fairly weak assumptions. A standard issue in nonparametric kernel or polynomial methods is that of choosing the "appropriate" bandwidth, or complexity of the model (Fan and Gjibels, (1996)), there is an obvious trade-off between bias and variance of the estimators in such context determined by the choice of the smoothing parameter. A too small bandwidth would cause an increase in the variance and might capture too much of the noise in the data, reducing to a simple interpolation of the data. On the other hand a large bandwidth would oversmooth the data, denying important features of the underlying data generating process. Such issue is resolved here by a direct plugin method for LLR elaborated in Rupert et al. (1995) (see Table 6, and a sensitivity analysis is performed to ensure the robustness of the results obtained[20]. The last estimation step reduces to applying the LLR to the left and right of the discontinuity and taking the difference of the two conditional mean functions estimated.

[19] Fan, (1992).

[20] All estimations are also performed according to a rule-of-thumb as in Rupert et al. (1995) and to $h_s = 1.06\hat{\sigma}n^{-.2}$, Silverman's rule, and half and twice h_{rot}. Naturally, the Silverman's rule is not suited for the LLR but it has been used only for a robustness check. The parameter estimates vary very little whatever selection criterion is adopted. Complete set of results is available from the author on request.

Standard errors have been obtained by bootstrap (300 replications[21]) for each cohort and for the whole sample.

6 Montecarlo Study

In this section I implement a simple montecarlo study on the performance of the estimator employed in the paper (β_{RD}) comparing it with a simple OLS (β_{OLS}) on the whole sample and a Wald estimator (β_W) on 10% of data around the discontinuity. The size of the discontinuity to be estimated is given in the Table 4 as β while the data generating process is $y = m(x) + \beta D(x < .5) + \epsilon$. Where $x \sim U[0,1]$, $\epsilon \sim N(0, \sigma_\epsilon^2)$ (σ_ϵ given in Table 4) and Φ is the standard normal cdf. The noise to signal ratio $\frac{\sigma_\epsilon^2}{\sigma_x^2}$ gives an indication of the complexity of the estimation. The study is based on 500 replications and performed for two different sample sizes ($n = 1000, 3000$). The bandwidth is chosen, as in the paper, according to a direct plugin method. The montecarlo evidence suggests a clear superiority of the proposed estimator in terms of precision with respect to the proposed Wald estimator[22]. The point estimates are quite precise. Comparing the order of the bias involved in the use of the Wald estimator as defined in the experiments gives striking results: it goes from 3 to more than 170 times.

The simple intuition on the quality of the estimates obtained by LLR relies upon the locality of the latter. When the underlying function giving rise to the discontinuity is still quite regular but characterised by a highly non linear behaviour fitting a local constant in the proximity of the discontinuity or a straight line on the whole sample is not a great idea. On the other hand, the decision to use a Wald estimator on 10% of the observation in the neighborhood of the discontinuity is arbitrary, I could have proposed different candidates all of which would still be based on an arbitrary selection method. In this respect, the advantage of the LLR estimator applied at the discontinuity point is due to the fact that the bandwidth is selected according to a consistent and objective criterion. It arises from the data generating process itself and therefore more reliable and accountable than in the former case.

7 Results

It is possible to summarize the results by referring to Table 5, where I present three sets of estimates named Best, Lower and Upper. As explained in Section 4 the three different

[21] The number of replications has been limited to 300 after few checks on the stability of the results. The estimation process for the figures produced in Table 5 takes about two weeks on a powerful server.

[22] The comparison with the OLS on the entire sample is per se not that meaningful given the idea behind the RD design.

sets of estimates derive from the fact that I had to “construct” three alternative scenarios given the available data. It is possible to recover the exit rates to employment for those participants who went through the education and training option (about 35% of those who took an option or about 15% of total participants) but for the lack of information on unemployed older than 25 who had left the JUVOS dataset for some training or education I have to rely on some assumptions. The “best” estimates assume exactly the same exit rates for treated and controls when the recorded exit from JUVOS is education and training. This could itself be a lower bound since treated should be expected to have a higher (re)employment chance from that option, given more structured courses offered. The second scenario “lower” relies on the assumption that all controls, who attended some training/education course, found a job in the reference period. It therefore qualifies as an extreme lower bound. In the third scenario, “upper”, none of the controls found a job in the reference period, which seems to be an extreme in the other sense. Analysing the parameter estimates it does not appear that the program effect is dying out, in fact it seems to be rather stable (Table 5) even after more than three years it has been launched. On average over the 15 cohorts it is possible to estimate a very precise parameter of about 6-7%. The time profile of the estimates does not seem to suggest relevant general equilibrium effects with possible differential impacts on the two groups (at least in local terms). This point is also confirmed by looking at the (re)employment probability for the two groups separately, they do not vary much and certainly not to be consistent with large general equilibrium effects[23].

On the other hand substitution does not seem to be relevant either. In case of large substitution effect we should see in the conditional mean functions a behaviour similar to Figure 3. The closer to the discontinuity the more substitutable individuals should be and therefore at the discontinuity the distance between the (re)employment probabilities should be larger. However, this is not the case given Figures 5 and 6; on both sides of the discontinuity the functions are almost completely flat. This suggests, combined with Figure 4 ((re)employment probability before the program), a “global” interpretation of the parameter estimates. However, such an extended interpretation obviously implies a stronger identification structure (i.e. constant treatment effect or particular smoothness). A test on the difference between the two non treated outcome before and after the program is also performed formally and in Figure 8, the null cannot be rejected. Such result is also confirmed by Figure 9, where the survival functions for 25-30 and 31-36 year old are plotted against each other before and after the program and they cannot be told apart at the conventional significance levels.

[23]Those figures are available from the author upon request.

The lack of evidence of general equilibrium and substitution effects can be explained by a number of factors. Firstly, the sort of general equilibrium effects I have in mind, arising from an increase in the labour supply lowering the equilibrium wage, require a substantial rise in the overall supply of labour, however, though the implementation of the program is global, it is not so massive to affect in a significant way the overall supply in the UK (Figures 10 and 11). As far as the substitution is concerned, it requires that treated individuals are cheaper than untreated, but this might not be the case if the cost of turnover is relatively high. Furthermore, treated are cheaper only in the case of the subsidised employment option, but the take-up rate of such a feature of the program is surprisingly low[24]. In fact only one over six treated who went through the option stage were allocated in a subsidised job adding to less than 7% of the new dealers. I have not covered possible general equilibrium effects arising from distortionary taxes devolved to the funding of the program for the simple reason that the program has been funded through the revenues from the privatisation processes initiated in those years.

8 Is There a Substitution Puzzle?

As mentioned throughout the paper the relevance of possible substitution effect between treated and control individuals is central to the identification structure. The program I consider here has a particular feature (subsidised employment option) that could raise concerns regarding the violation of the SUTVA and therefore the validity of the identification strategy[25]. I have spent a considerable part of the work trying to assess such an issue, and I do not find support for any major concern on the evaluation exercise I propose. Why is it then that there is not any substitution effect? In principle, the presence of a significant subsidy to employment should generate an incentive to substitute workers. Is the subsidy given to participants enough to create such an effect? As explained earlier, by comparing the sort of hourly rate participants should get to the amount of the subsidy granted (weekly plus one off payments) this add up to about 50% of the salary in the 6 months period for which such subsidy could last for. However, when considering the relevance of the subsidy there are few more things to be accounted for. Firstly, the one off payment has to cover the minimum one day per week of training participants must receive. On its own this would notably lower the previous percentage to 30%. Secondly, the subsidy only last for 6 months and might not be enough to compensate for the turnover

[24]Even the program administrators were surprised by such a low take up.

[25]It is worth mentioning that such displacement might also arises from enhanced job search. However, in this respect it might simply be that the matching function is improved, vacancies are filled in more efficiently, without affecting the outcome of the control groups.

costs. Thirdly, in a targeted program, as the one considered here, there might be an important stigma effect (Katz, 1998) attached to receiving a subsidy. The only way such a participant is able to get a job is through a discount on the wage received. He is probably not as productive as someone else in the population and while the subsidy could help him getting a job, it would signal to the market his bad type. These are three potential explanations on the absence of relevant substitution effects in the particular program under scrutiny. Are they convincing? I should now go back to the evidence. The very low take up rate for such an option (only 16% of participants who actually went through an option) was surprising even to the program administrators who were expecting a much higher one. The amount of evidence put forward in this respect seems to be clear cut in excluding relevant substitution bias (Figures 5, 6, 8 and 9). Either comparing cohorts of controls before and after the program, the actual outcomes in terms of employment probability with a prediction of how they should look like in case of any relevant substitution effect or, finally, comparing the control groups with a slightly older counterparts I do not find any important substitution pattern.

9 Conclusions

This paper evaluates, in a long-run perspective, the (re)employment effect of the major welfare-to-work policy in the UK: the New Deal for Young People (NDYP). The NDYP is a mandatory multistage program where treatments span from job search assistance at a first stance, to training, education, subsidy and reinstatement in the labour market through voluntary sector or environmental services. Throughout the paper treatment is intended as a combination of the above mentioned and therefore defined as a "black-box", whose opening is delayed to future research.

Under a minimal set of assumptions, consistently with the discontinuity design approach adopted, I identified a local treatment effect for males of about 6-7% using data on those individuals who entered the program from April 1998 to December 2001. Such an estimate, of interest on its own right, could, given the non parametric estimates of the conditional mean functions, be given a "global" dimension. However, this would imply the loss of the fully non parametric identification. Side effects of the program have also been investigated in order to confirm the robustness of the results. A large program can have general equilibrium effects on wages via the possible increase in labour supply. On the other hand, given that one of the treatment is a subsidy this may induce a certain degree of displacement of untreated individuals if they are effectively more costly than treated. This is especially true the more similar those two groups of individuals are. In case of a substantial substitution the parameter estimated would be biased upwards. However, I do not find any evidence of substitution and general equilibrium effects. Few indirect tests are produced in this sense thanks to a cohort specific approach adopted in the analysis.

The positive impact of the program, in fact one of the few examples of an effective welfare-to-work policy, probably arises, in part, from the nature of the treated individuals. Those are young unemployed not particularly disadvantaged and besides a six months unemployment spell for such an age group is not that uncommon.

It has also been shown (van Den Berg et al., 2004) that policies where non compliers incur significant sanctions are on a theoretical and empirical ground capable of producing beneficial effects in terms of employment, for the simple fact that they push up the level of effort exerted by the unemployed. The mechanism being quite intuitive a worse outside option (withdrawal of the benefit) constitutes a large incentive. Katz (1998) in reviewing different ALMPs found that policies combining wage subsidies with job development, training and job search assistance appear to have been somewhat successful in improving the employment and earnings of specific targeted disadvantaged groups.

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Table 1: Quarterly Cohorts. Treated 19-24, Controls 25-30 years old

	Treated	Control	Total
Apr.-June '98	1,553	1,406	2,959
July-Sept.	1,486	1,345	2,831
Oct.-Dec.	1,217	1,165	2,382
Jan.-Mar. '99	1,458	1,342	2,800
Apr-June	1,480	1,334	2,814
July-Sept	1,323	1,280	2,603
Oct.-Dec.	1,059	1,098	2,157
Jan.-Mar. '00	1,450	1,263	2,713
Apr-June	1,376	1,202	2,578
July-Sept	1,197	1,026	2,223
Oct.-Dec.	970	957	1,927
Jan.-Mar. '01	1,144	1,055	2,199
Apr-June	1,279	1,119	2,398
July-Sept	1,109	935	2,044
Oct.-Dec.	894	856	1,750
Total	18,995	17,383	36,378

Table 2: Usual Occupation by Treatment Status (%)
and Age Bracket

Usual occupation	*19-30*		*22-27*		*24-25*	
	Treated	Control	Treated	Control	Treated	Control
Managers	1.04	1.96	1.64	1.87	2.10	1.78
Professional	0.92	2.08	1.35	2.25	1.39	2.16
Associate Prof., Technical	3.80	5.19	4.59	5.62	5.34	5.76
Admn. Secretarial	13.65	10.94	13.23	11.41	12.02	11.13
Skilled trades	11.91	14.17	12.62	13.22	13.91	12.65
Personal Service	5.66	5.05	5.69	5.19	5.59	5.76
Sales and Customer service	8.62	4.78	7.15	5.47	6.98	5.60
Process, Plant and Mach. operatives	8.56	11.63	9.06	10.92	9.79	10.75
Elementary occupation	45.83	44.19	44.66	44.05	42.88	44.41

Table 3: Sought Occupation by Treatment Status (%)
and Age Bracket

Sought occupation	*19-30*		*22-27*		*24-25*	
	Treated	Control	Treated	Control	Treated	Control
Managers	1.35	2.01	2.23	2.04	2.61	2.10
Professional	1.29	2.40	1.86	2.69	2.28	2.53
Associate Prof., Technical	4.93	6.26	5.87	6.94	6.06	7.43
Admn. Secretarial	15.06	11.58	14.53	12.40	13.67	12.23
Skilled trades	12.73	14.26	12.77	13.25	13.34	12.61
Personal Service	5.94	5.07	5.86	5.09	5.98	4.90
Sales and Customer service	9.48	5.49	7.93	5.91	7.69	6.22
Process, Plant and Mach. operatives	8.41	12.18	9.25	11.15	9.64	11.08
Elementary occupation	40.82	40.75	39.70	40.54	38.72	40.89

Table 4: Montecarlo Experiments

$m(x)$	β	σ_ϵ	noise/signal	$\hat{\beta}_{RD}$	SE	$\hat{\beta}_{OLS}$	SE	$\hat{\beta}^a_W$	SE
$.4x + .2x^2 - .7x^3 + .1x^4$	.1	.1	.14						
n=1000				.0928	.0330	.0963	.0162	.1301	.0207
n=3000				.0917	.0215	.0958	.0093	.1243	.0118
$.5\sin(6x)$	.1	.1	.06						
n=1000				.1110	.0338	.6824	.0231	.5826	.0163
n=3000				.0972	.0127	.6799	.0130	.5938	.0097
$\Phi(.5\sin(6x))$	.1	.1	.28						
n=1000				.1046	.0133	.3217	.0088	.1582	.009
n=3000				.0990	.0086	.3197	.005	.1549	.005
$\exp(x^{\cdot 2}) + .5\sin(6x)$	.1	.1	.13						
n=1000				.1019	.0333	.7457	.0293	.1428	.0347
n=3000				.0949	.0216	.7509	.0167	.1291	.0183

Note: a) Wald estimator takes only observations between .45 and .55.

Table 5: Treatment Effects (and Bounds), Bandwidth: Direct Plugin

	Best		*Lower*		*Upper*	
Cohort	Effect	Std.Err.	Effect	Std.Err.	Effect	Std.Err.
Apr.-June '98	.0872	*(.0346)*	.0336	*(.0323)*	.1166	*(.0336)*
July-Sept.	.0936	*(.0380)*	.0372	*(.0356)*	.1366	*(.0397)*
Oct.-Dec.	.0294	*(.0390)*	-.0015	*(.0372)*	.0784	*(.0418)*
Jan.-Mar. '99	.0787	*(.0384)*	.0303	*(.0394)*	.1365	*(.0391)*
Apr-June	.1126	*(.0380)*	.0327	*(.0318)*	.1808	*(.0404)*
July-Sept	.0742	*(.0358)*	.0458	*(.0362)*	.1389	*(.0353)*
Oct.-Dec.	.0093	*(.0412)*	-.0536	*(.0396)*	.0817	*(.0444)*
Jan.-Mar. '00	.0254	*(.0371)*	.0045	*(.0366)*	.0620	*(.0376)*
Apr-June	.0227	*(.0361)*	-.0478	*(.0339)*	.0680	*(.0390)*
July-Sept	.0981	*(.0394)*	.0464	*(.0356)*	.1750	*(.0384)*
Oct.-Dec.	.0231	*(.0437)*	.0026	*(.0415)*	.0683	*(.0409)*
Jan.-Mar. '01	.0032	*(.0382)*	-.0362	*(.0363)*	.0425	*(.0389)*
Apr-June	.1256	*(.0396)*	.0728	*(.0370)*	.1760	*(.0376)*
July-Sept	.0653	*(.0408)*	-.015	*(.0407)*	.1222	*(.0405)*
Oct.-Dec.	.1172	*(.0489)*	.0519	*(.0500)*	.1415	*(.0497)*
All	.0645	*(.0104)*	.0150	*(.0104)*	.1154	*(.0109)*

Note: Results based on bootstrapping 300 replications.

Table 6: Bandwidth: Direct Plugin

	Best		*Lower*		*Upper*	
Cohort	Treated	Control	Treated	Control	Treated	Control
Apr.-June '98	0.5792	0.3952	0.6420	0.4790	0.5927	0.4089
July-Sept.	0.6617	0.8908	0.7823	0.7088	0.5507	1.0079
Oct.-Dec.	0.5895	0.5861	0.6871	0.6406	0.9186	0.8052
Jan.-Mar. '99	0.6278	0.5965	0.9147	0.4200	0.5400	0.8553
Apr-June	0.4882	0.5285	0.5235	1.0403	0.5335	0.6900
July-Sept	0.7054	0.3623	0.6360	0.4120	0.6833	0.4295
Oct.-Dec.	0.6709	0.5584	0.8927	0.5853	0.4956	0.4877
Jan.-Mar. '00	1.0209	0.3853	1.1799	1.3131	0.6968	0.4073
Apr-June	0.7158	0.4590	0.5651	0.4014	0.5308	0.4173
July-Sept	0.5678	0.4505	0.4287	0.4686	0.4385	0.4150
Oct.-Dec.	0.7938	0.4863	0.5413	0.4092	0.6336	0.4789
Jan.-Mar. '01	0.5966	0.3803	0.4718	0.4846	0.4706	0.5355
Apr-June	0.4187	0.4103	0.4260	0.4305	0.4160	0.3960
July-Sept	0.5264	0.3493	0.5938	0.1371	0.5908	0.4113
Oct.-Dec.	0.7307	0.6288	0.5592	0.7961	0.7662	0.9806
All	0.4449	0.4581	0.5004	0.6116	0.4129	0.7611

Table 7: Pairwise T-tests on Treatment Effects

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
1	0														
2	-0.04	0													
3	1.15	1.14	0												
4	0.17	0.20	-0.94	0											
5	-0.35	-0.30	-1.44	-0.50	0										
6	0.36	0.38	-0.80	0.17	0.69	0									
7	1.53	1.51	0.39	1.31	1.80	1.18	0								
8	1.32	1.30	0.13	1.09	1.61	0.95	-0.28	0							
9	1.33	1.30	0.11	1.09	1.61	0.95	-0.30	-0.02	0						
10	-0.12	-0.08	-1.20	-0.28	0.21	-0.46	-1.56	-1.36	-1.36	0					
11	1.19	1.18	0.11	0.99	1.46	0.86	-0.27	-0.01	0.00	1.24	0				
12	1.74	1.70	0.54	1.49	2.00	1.37	0.13	0.42	0.44	1.75	0.40	0			
13	-0.71	-0.65	-1.76	-0.84	-0.35	-1.04	-2.10	-1.93	-1.94	-0.55	-1.76	-2.31	0		
14	0.50	0.52	-0.59	0.32	0.81	0.17	-0.96	-0.73	-0.72	0.59	-0.66	-1.12	1.13	0	
15	-0.43	-0.38	-1.37	-0.55	-0.12	-0.72	-1.69	-1.51	-1.51	-0.30	-1.40	-1.85	0.19	-0.82	0

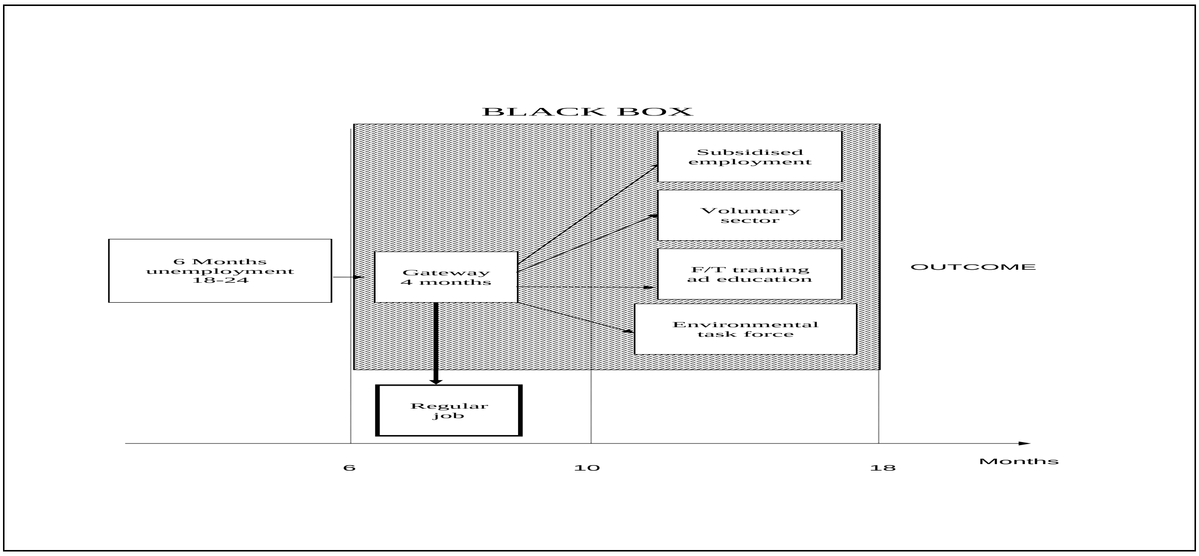

Figure 1: The program

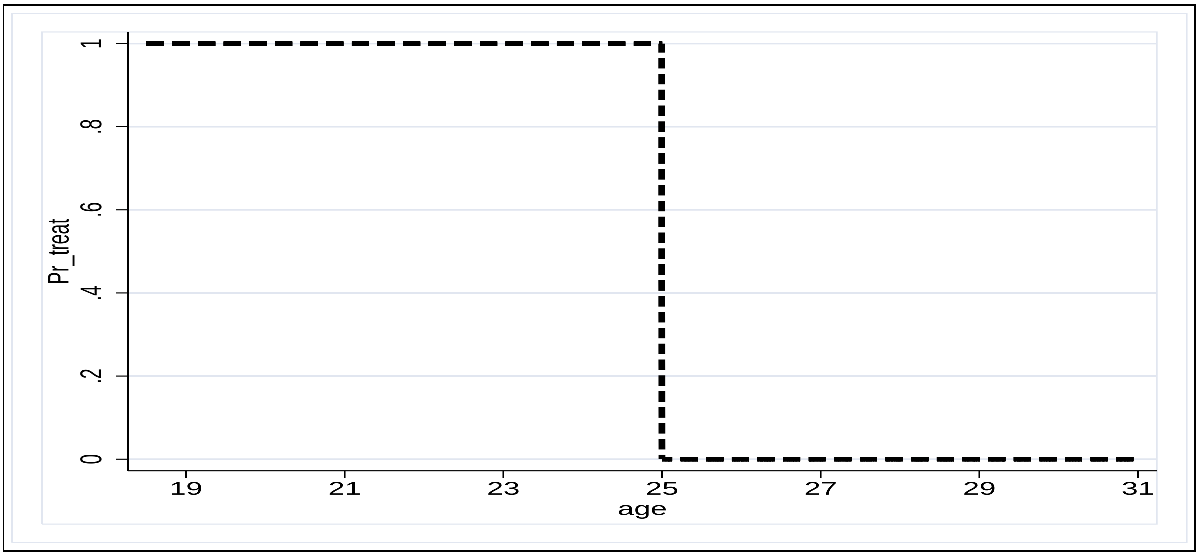

Figure 2: The “sharp” RD design

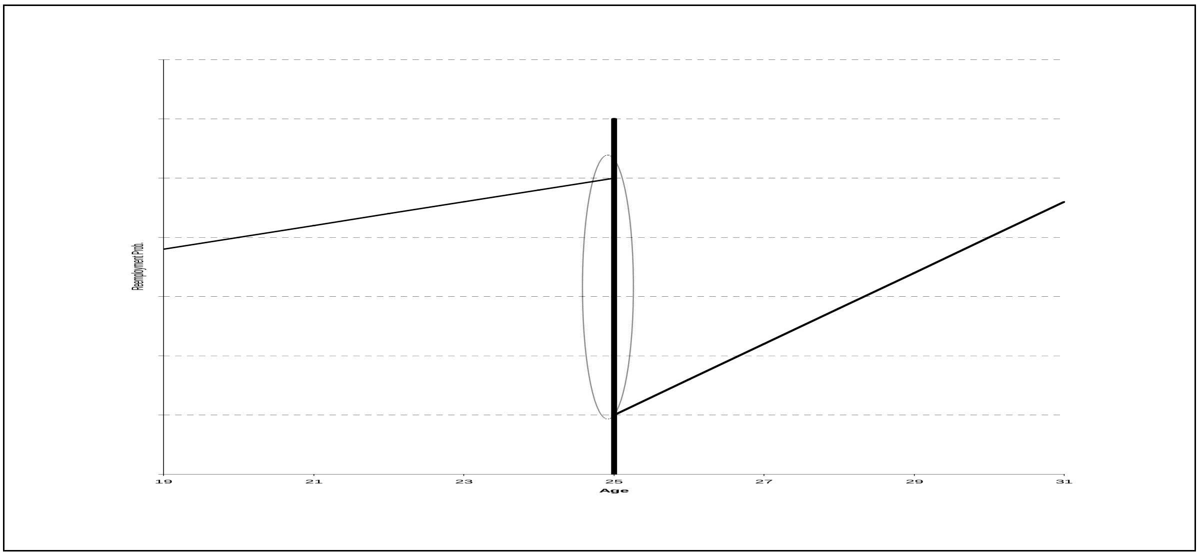

Figure 3: (Re)Employment probability by age

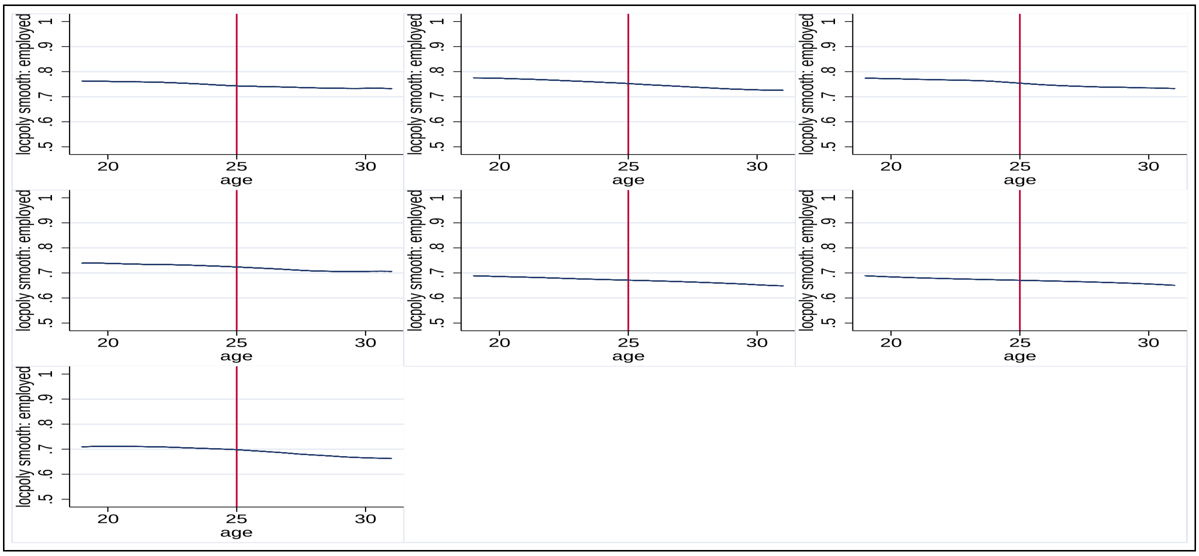

Figure 4: Pre-Program (re)employment probabilities by cohort and age

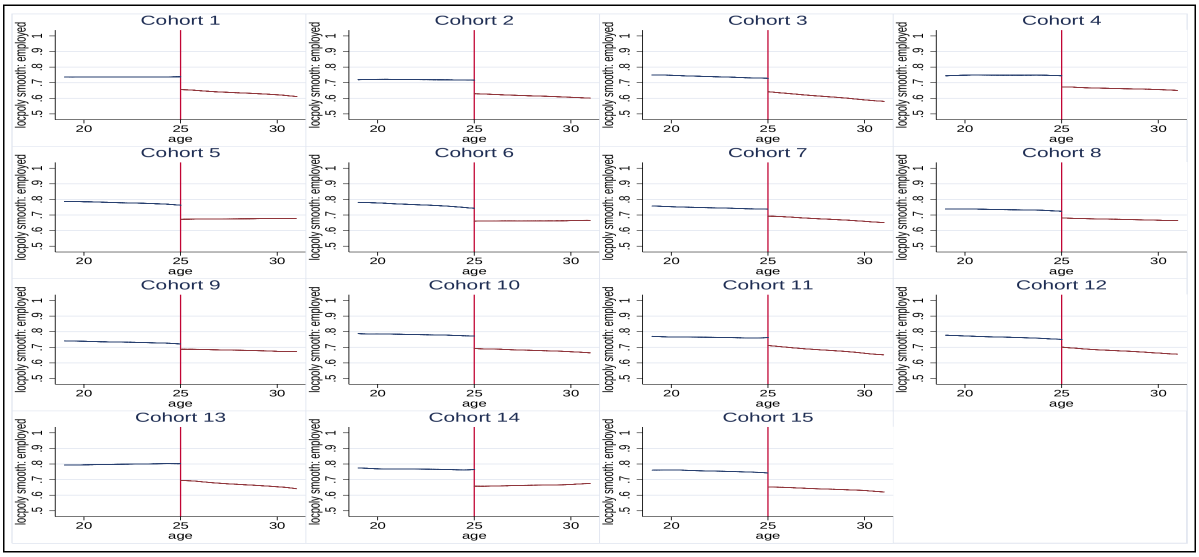

Figure 5: Post-Program (re)employment probabilities by cohort and age

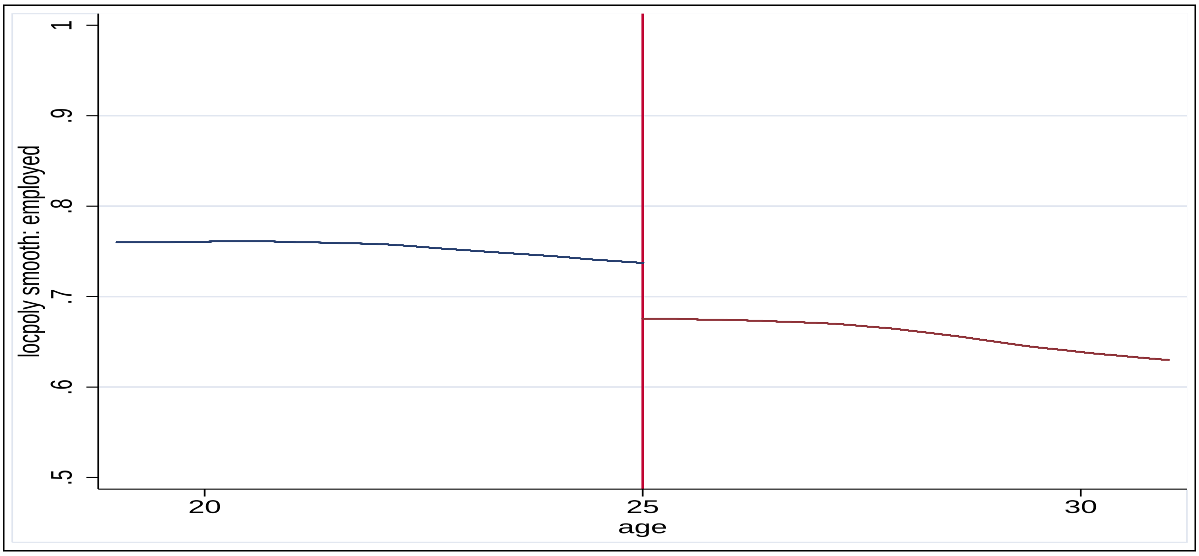

Figure 6: Post-Program (re)employment probabilities by age (all cohorts)

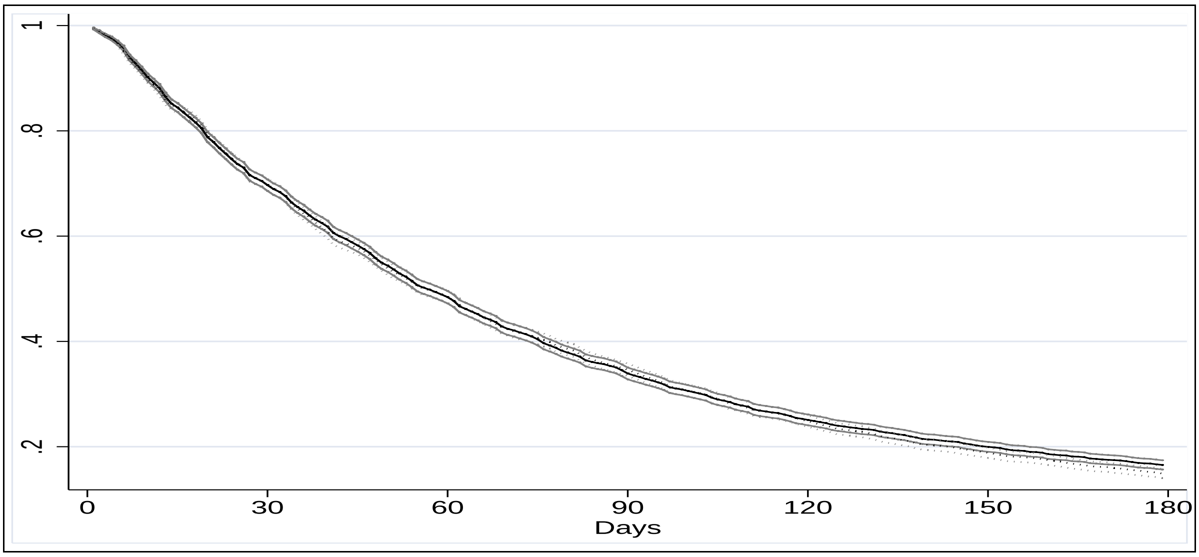

Figure 7: Kaplan-Meyer survival probabilities and 95% confidence intervals for 2 cohorts of 19-24 year old in the first 6 months of unemployment
Note: pre-program, solid line; post-program, dashed line.

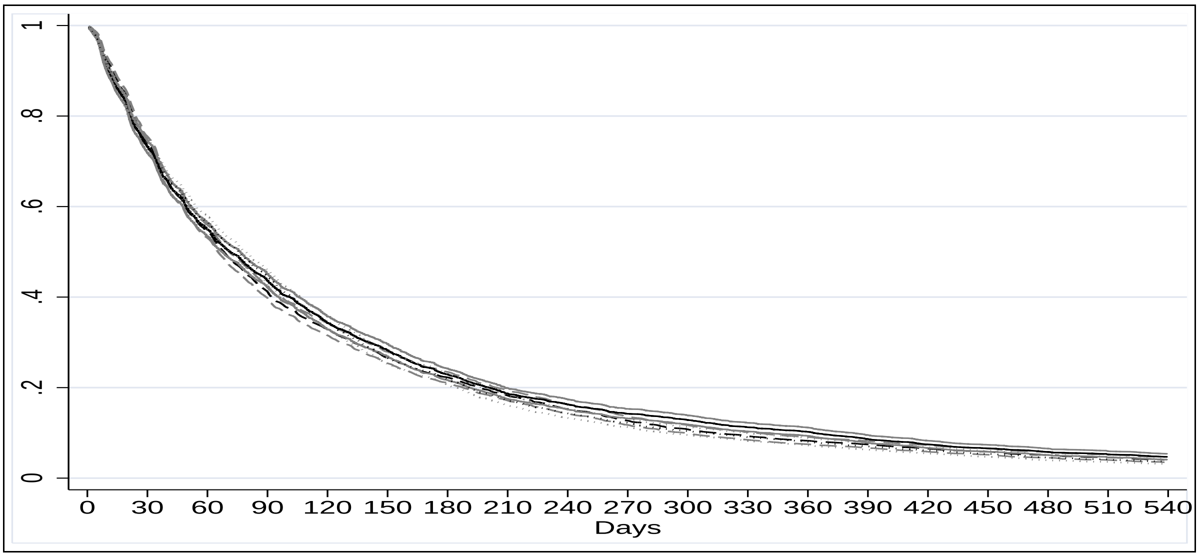

Figure 8: Kaplan-Meyer survival probabilities and 95% confidence intervals (non treated)
Note: Before (solid and dashed line) after (dotted line) the program

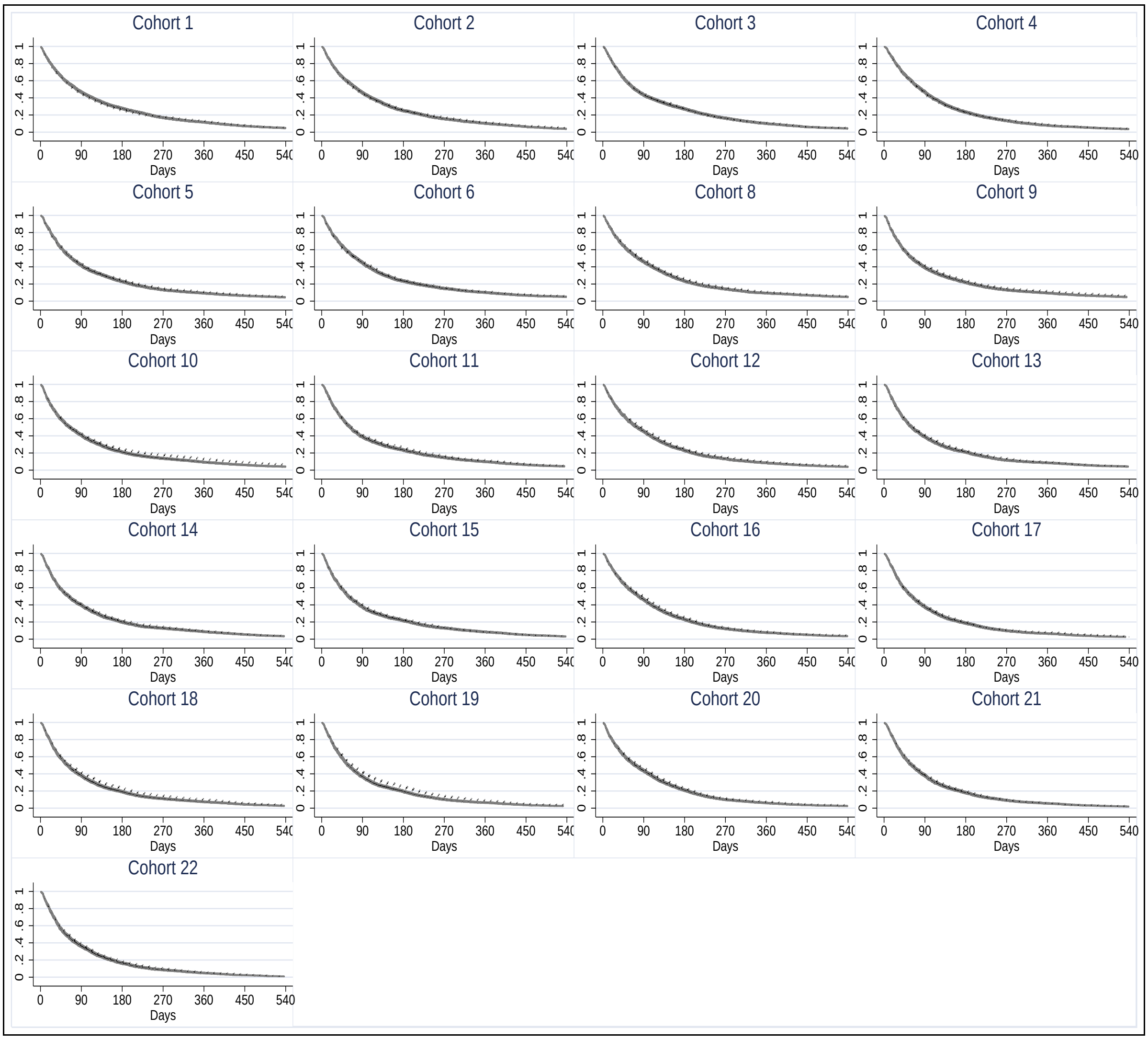

Figure 9: Kaplan-Meyer survival probabilities and 95% confidence intervals

Notes: Solid: 25-30 year old. Dotted: 31-36 year old. Cohorts 1 to 6 are before the program.

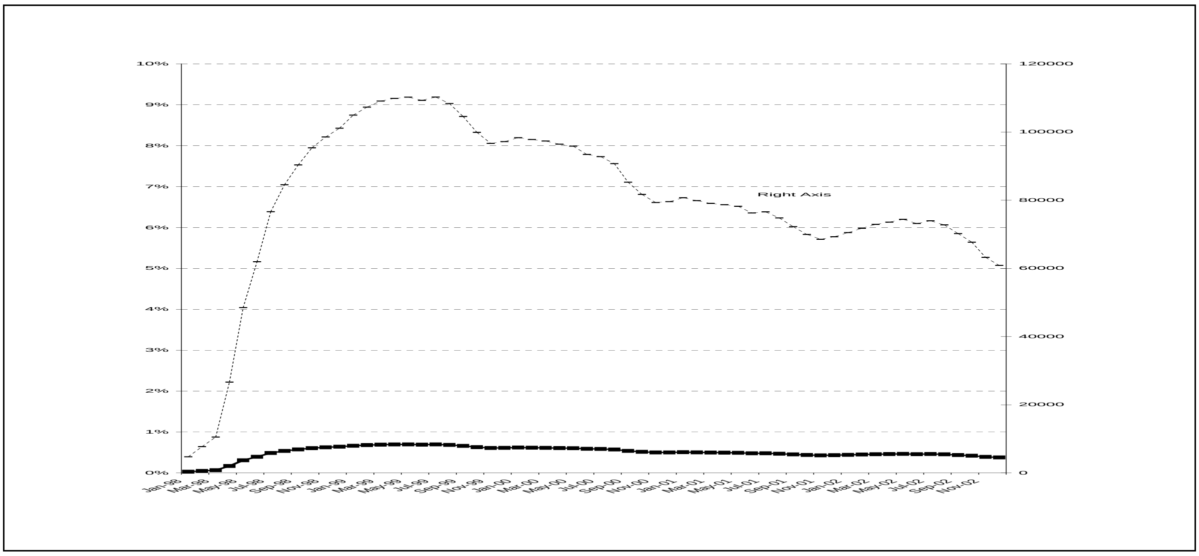

Figure 10: New Deal participants (males)

Note: as % of active males, solid line; number, dotted line.

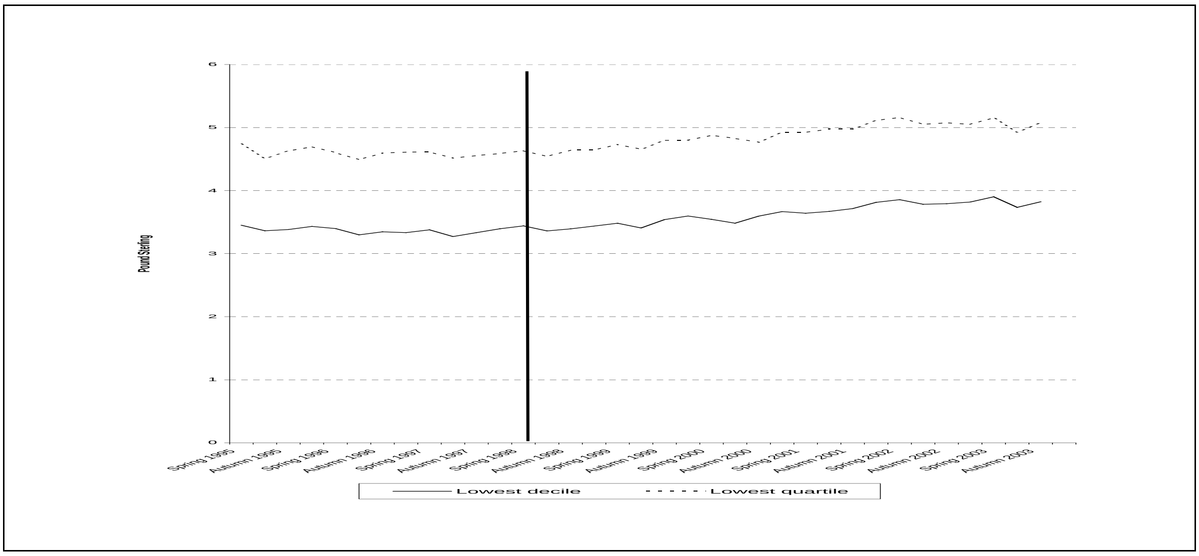

Figure 11: Hourly earnings (1995Q1 prices)

Source: LFS.