VATT Working Papers 101

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Helsinki 2017

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We are grateful to Mike Brewer, Matz Dahlberg, Peter Fredriksson, Tomi Kyyrä, Martin Lundin, Eva Mörk, Miikka Rokkanen, Matti Sarvimäki, Jouko Verho, participants at the IIPF Annual Congress 2014, IZA/IFAU conference on Labour Market Policy Evaluation 2014, CESifo Area Conference on Employment and Social Protection 2015, 2nd NORFACE 4Is Workshop 2016, EEA Congress 2016 and seminar participants at University of Copenhagen, KU Leuven, IFAU, HECER, Labour Institute for Economic Research, National Institute for Health and Welfare, and University of Turku for valuable comments. Funding from the Academy of Finland (Grant No. 252369), the Swedish Research Council for Health, Working Life and Welfare (FORTE, Grant No. 2011-1045), NORFACE (Grant No. 462-14-013) and Yrjö Jahnsson Foundation is gratefully acknowledged.

ISBN 978-952-274-209-4 (PDF)

ISSN 1798-0291 (PDF)

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Helsinki, December 2017

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Abstract

We study the dual role of active labour market policies: First, ALMP may perform a screening role by increasing the incentives for job search especially among individuals with good labour market prospects, already prior to programme participation. Second, actual programme participation may help individuals with poor labour market prospects. We examine whether this type of a pattern can be found in individual responses to a major nationwide youth activation programme in Sweden. We analyse individual responses to the programme using an RD design. We find that individuals with a high predicted probability of finding work respond to the threat of activation, whereas there is no effect for individuals with weak labour market prospects.

Key words: activation, unemployment, regression discontinuity, screening

JEL classes: J64, J68

1 Introduction

We examine empirically whether activation of the unemployed affects job finding rates mainly through helping those with otherwise poor labour market prospects to find work, or through persuading individuals with generally good labour market prospects to search more intensively for a job. The latter phenomenon would indicate the presence of a screening role of active labour market programmes, similar to a screening effect of workfare discussed in theoretical work initially in the context of poverty alleviation; the seminal contribution here is Besley and Coate (1992).[1] Such an effect is related to the so called threat effect of active labour market programmes (e.g. Black et al. 2003), whereby individuals respond to the presence of a programme already prior to actual participation. However, in the presence of screening, the threat effect is heterogeneous in such a way that it affects precisely those individuals with good labour market prospects. Despite a number of theoretical papers analysing the screening role of workfare, direct empirical evidence remains limited.

We analyse the pattern of individual responses to a major, nationwide youth activation programme (the Youth Job Guarantee) that was introduced in Sweden in 2007. The main focus of the programme was in activities related to job search. We use data on the entire Swedish population and covering the universe of unemployment spells during the period under study. Before turning to the empirical analysis, we illustrate how the screening effect may arise in a search theory framework where individuals differ in their labour market prospects.

Another distinguishing feature of our analysis is that in looking at the screening role and heterogeneous effects of activation, we are able to focus on a particularly rich set of background variables. In particular, in addition to more traditional background variables such as education and immigrant status, we have exceptionally good data on the individuals' past health and labour market history. The use of health data is motivated by the finding that individuals with poor past health – especially those with past mental health problems – are hugely overrepresented among individuals with poor labour market prospects.

In looking at the heterogeneous effects of the programme, we first classify individuals according to their predicted probability of finding work. We do this by using an empirical model estimated on out-of-sample data (i.e. data on unemployment spells in the year prior to the introduction of the programme). This simple approach avoids the problem of endogenous stratification that has been present in some earlier studies, as pointed out by Abadie et al. (2016). Individuals with a relatively high predicted probability of finding work are then

[1] We discuss related literature more extensively in Section 2.

classified as being in a relatively strong labour market position, and therefore more likely to be voluntarily unemployed. We use a regression discontinuity (RD) design to estimate the effects of the Youth Job Guarantee programme (YJG), using the fact that only individuals under 25 years of age are eligible for the programme. Under 25-year-olds are eligible if they have been unemployed for more than 90 days. Thus, our empirical strategy is essentially to compare the job finding rate among individuals who have just turned 25 before 90 days of unemployment (ineligible) to the job finding rate among those who are just below age 25 at 90 days of unemployment (eligible). We analyse separately the effect of programme eligibility on the probability of finding employment during the first 90 days of the unemployment spell (the threat effect) as well as at different points in time later on.

Clearly, there exists a large earlier literature on evaluating active labour market programmes, see e.g. Card et al. (2010) and Kluve (2010) for reviews. The most relevant studies for our paper are reviewed in Section 2. We contribute to this literature in a number of ways. First, we provide evidence on the screening role of activation programmes through examining how the effects of activation differ with respect to individuals' labour market prospects. While there are numerous theoretical papers on the screening effect of workfare/ALMP in different types of settings, we are not aware of earlier empirical evidence focussing directly on this issue. We study whether the pattern of exit (both regarding the timing as well as heterogeneity across individuals) from unemployment in response to the introduction of an activation programme is consistent with the idea that ALMP screens away from unemployment those individuals whom we would predict to have a good chance of finding a job even in the absence of activation. We also provide a brief conceptual framework that illustrates how the screening effect may arise in a labour market search model where individuals differ in their baseline probabilities of finding work. As we argue in Section 2, previous theoretical applications do not analyse the role of screening in the context of transitions from unemployment to work. Second, only a few studies examine whether activation programmes have had different impacts among the disadvantaged youth. Disadvantaged youth are an important group to look at, since preventing social exclusion is often a key motivation behind programmes targeted at youth. In all of the previous literature we know of, disadvantageousness is proxied by educational status, whereas we use a rich set of background information with extensive knowledge of individuals' past employment history and health. Finally, one of the conclusions in Kluve (2010) is that youth training programmes have a relatively low probability of showing positive effects, and it is of interest in itself to evaluate whether the large, nationwide Swedish activation programme yields more promising outcomes.

Our results show that there is a statistically significant threat effect associated with the programme: Programme eligibility increases the probability of finding employment before the programme starts by around 7 percent. Our results also indicate that the threat effect is mainly driven by groups with a more advantaged position in the labour market – we find no statistically significant threat effect for the group with the weakest labour market prospects. Moreover, we do not find any long term effects of the programme for any group: after about a year from the start of unemployment, job finding rates among the ineligible seem to have caught up with that of the eligible. The empirical patterns that we find are consistent with the idea that the programme performs a screening role. The main effect of the programme appears to be to screen away from unemployment those individuals who are able to find work on their own, whereas there appear to be no major positive effects for those in a poorer labour market position.

The paper proceeds as follows. Section 2 provides the theoretical background for our empirical analysis, and it also discusses earlier empirical work in the area. Section 3 describes the activation programme, while the data is described in Section 4. The empirical methodology and the results for the whole sample as well as subgroups are presented in Section 5. We also conduct a large battery of RD validity and robustness checks. Section 6 concludes.

2 Background and earlier literature

2.1 Theoretical background

Besley and Coate (1992) provided a seminal theoretical contribution on the screening role of workfare, arguing that work requirements in poverty alleviation programmes can function as a screening device between those who are truly in need of poor support and those who are not.[2] The result arises because high ability individuals have a higher opportunity cost of time and are therefore less willing to participate in workfare programmes. Kreiner and Tranaes (2005) provide a theoretical analysis of the screening role of workfare in the labour market context, and a similar model has been studied in Fredriksson and Holmlund (2006a). In this model, individuals who are voluntarily unemployed (or "non-workers" in their terminology)

[2] Cuff (2000) discusses the role of workfare in screening between the "deserving" and "undeserving" poor in a model where individuals differ (in addition to ability) in their disutility of work.

have a relatively high disutility of work, and a work requirement therefore makes claiming unemployment benefits a less attractive option for them.

A key notion in our analysis is that active labour market programmes (ALMP) may play a similar screening role as workfare. The potential similarity between workfare and ALMP has been noted also in Fredriksson and Holmlund (2006b). We take on board the idea from Kreiner and Tranaes (2005), that workfare/ALMP may be able to screen between individuals who are voluntarily and involuntarily unemployed. However, their framework is not directly applicable in our setting: We are interested in transitions into employment. In Kreiner and Tranaes' model, screening works through deterring non-workers from claiming unemployment benefits (pushing them onto minimum income support that is available without a work requirement), but it does not directly affect employment rates. We would like to capture the idea that voluntarily unemployed individuals would be able to find work if they wanted to (even in the absence of an activation programme), but do not do so if benefits are too high or easy to obtain.

To be more precise, what we mean by screening in this context is the following: Workers differ in their in job arrival rates, i.e. returns to search. For simplicity, we assume that there are two types of workers, low and high types. Neither job-arrival rates nor search effort are observable to the policy-maker, so policy cannot be conditioned on them. We show below that the threat of an activation policy increases the marginal return to search more for the high type (with a high job arrival rate) than the low type. Hence the two types respond differently to the policy, that is, the policy helps to separate between the types: the threat of activation increases the incentives to search more for the high type with good labour market prospects, and thus works towards higher increases in exit rates from unemployment by the high type already prior to the actual activation phase. In this sense the policy helps to deter in particular the high type from unemployment.[3] While it appears plausible that it would be a good idea also in practice to induce those with higher returns to search to exert more search effort (the model features no differences in search costs), a caveat is that we do not provide an explicit analysis of the welfare properties of the programme. Nevertheless, it is clear that the possibility to use activation as an (optimal) screening device hinges on whether the policy is

[3] Note that the high type individuals will naturally exit unemployment sooner than the low type individuals, ceteris paribus, even in the absence of activation. What we are interested in, however, is how an activation programme affects the search behaviour of different types of workers, i.e. whether *changes* in exit rates from unemployment after the introduction of the policy differ across types.

able to separate different types of individuals. We provide a theoretical illustration and empirical evidence on such heterogeneous responses.[4]

To illustrate this idea in a simple setting, we extend the work by Andersen and Svarer (2014), who study the role of workfare in a conventional search-theoretic model with moral hazard, to a situation where workers are heterogeneous. Andersen and Svarer consider a model where the unemployed face a certain probability of being required to participate in a workfare programme. This probability is denoted by p_{au}. We analyse how different types of job-seekers react to changes in the activation intensity (l_a), that is, the time they are required to participate in activation (conditional on being assigned to a programme). In this model, workfare is assumed to be useless per se, i.e. it does not affect workers' productivities or job-arrival rates. This assumption is common also in other related literature, and we adopt it in our main analysis, but we also comment below on the case where workfare (or activation) may have a beneficial effect on the likelihood of finding a new job.

Workers' instantaneous utility, h, depends on consumption (=disposable income), $(1-\tau)w$, where τ is the tax rate and w is income in employement, and on leisure. The utility for the individual when at work is then $h\big((1-\tau)w, 1-l_e\big)$, where l_e depicts working hours. When unemployed, utility is $h\big((1-\tau)b, 1-s_u\big)$, with s_u denoting search intensity and b denotes income when unemployed. The utility when the individual is required to participate in workfare is $h\big((1-\tau)b, 1-s_a-l_a\big)$. Income b is assumed to be the same for the unemployed irrespective or their activation status.

The value functions (with ρ denoting the interest rate) are

(1) $$\rho V^E = h\big((1-\tau)w, 1-l_e\big) + p_{ue}[V^U - V^E],$$

(2) $$\rho V^U = h\big((1-\tau)b, 1-s_u\big) + \alpha^i s_u[V^E - V^U] + p_{au}[V^A - V^U],$$

(3) $$\rho V^A = h\big((1-\tau)b, 1-s_a-l_a\big) + \alpha^i s_a[V^E - V^A].$$

In the above expressions, $\alpha^i s_u$ and $\alpha^i s_a$ are the probabilities of getting a job for the unemployed of type i with or without workfare, respectively. The probability of losing a job is p_{ue}. The job arrival rate conditional on search effort for individual i is denoted by α^i (whose value will be varied below). We assume that there are two groups of individuals, who differ in

[4] For example in Fredriksson and Holmlund (2006a), workfare achieves perfect screening between non-workers and workers (i.e. only workers claim unemployment insurance benefits), but it may still not be optimal policy as other policy instruments yield higher welfare. In our case, activation increases the benefits of search for both types, but more so for the high type; in this sense, screening is less than perfect. Another example of imperfect screening, albeit from a very different context, arises in optimal tax models where workers with different abilities choose different levels of work hours.

their job-finding rate, such that $\alpha^2 > \alpha^1$ The groups are large in the sense that α^i is taken as given by each individual, i.e. it is a macro variable that is determined in the model. The differences in job-finding probabilities may reflect differences in skills or personality traits across the groups. Of course, if the labour market were perfectly competitive, such differences should be reflected in wage levels. On the other hand, in reality there may be reasons why an employer may not be able to tailor wage offers fully to the personal characteristics of job candidates, which would show up as differences in the likelihood of receiving job offers.[5]

How does search effort for the unemployed change when activation intensity (l_a) increases? And how does the change depend on the job-finding rate – i.e. does more intense activation increase the benefits of search more for the high type (type 2)?

Denote the marginal benefit of search while in open unemployment by $B^U = \alpha^i[V^E - V^U]$. It is shown in Appendix A that $\frac{\partial B^U}{\partial l_a} > 0$. That is, as one would expect, the marginal benefit of search is increasing in the intensity of activation. Further, we have that $\frac{\partial B^U}{\partial l_a \partial \alpha} > 0$: increasing the intensity of activation increases the benefits of search more for the high type. Note that we have assumed that the two types do not differ in their valuation of leisure (i.e. the function h is the same for both types). Therefore the mechanism at play in our setting is different to that in Kreiner and Tranaes, who have examined the screening role of workfare when agents differ in their valuation for leisure. In our setting, screening can arise in the labour market context even with identical preferences for leisure.

Turning to effects that occur during the activation phase itself and denoting the marginal benefits of search in the activation phase by B^A, we show in Appendix A that $\frac{\partial B^A}{\partial l_a} > 0$ and $\frac{\partial B^A}{\partial l_a \partial \alpha} > 0$. Increasing the intensity of activation again increases the benefits of search more for the high type. This result holds in the simple setting where we have assumed that activation is unproductive in the sense that it has no direct effect on the job finding probability. This assumption is in line with earlier literature, which has concentrated on examining the conditions under which unproductive workfare is desirable. If on the other hand we allowed for the quite realistic possibility that activation could increase the job arrival rate, and more so for the low type, then the second result above could be overturned i.e. it is possible that activation would increase the benefit of search more for the low type. To keep the model tractable, we have not analyzed this case formally.

[5] Hall (2005) discusses sticky wages as a reason behind aggregate fluctuations in firm recruitment effort and job-finding rates. We conjecture that a similar mechanism might explain variation in job-finding probabilities within the population.

In this setting, active labour market policies may then work through two channels: (i) the threat of activation works towards deterring from unemployment especially those individuals who would be able to find work on their own but do not do so e.g. because benefits are too generous or easy to obtain (type 2); this is the *screening effect*; and (ii) participation in activation itself also increases job-finding rates; call this the *activation effect*[6]. If workfare is productive in the sense that it increases job-finding rates, and more so for individuals who are less likely to find work on their own (type 1), participation in activation may help especially these types of individuals to find a job. If both screening and activation effects are at work, we would expect to observe a certain type of pattern in exit from unemployment: Type 2 individuals would exit unemployment predominantly before actual activation starts, i.e. we would observe a threat effect for type 2 individuals.[7] Type 1 individuals, on the other hand, would enter the activation phase, and may find employment as a result. We aim to analyse whether such patterns are present in our data. In the empirical application, in line with the above framework, we use the predicted probability of finding work (in the absence of activation) as a measure to distinguish between type 1 and 2 individuals: if the person remains unemployed despite a high predicted probability (based on observable characteristics) of finding work, unemployment is more likely to be voluntary.

2.2 Previous empirical literature

Related to our focus on the screening role of workfare/ALMP, Fredriksson and Holmlund (2006) note that empirical evidence on the effects of workfare is limited, with papers on the threat effect of ALMP providing the most closely related evidence. A number of studies have documented the presence of a threat effect in the context of activation programmes. For instance, Black et al. (2003) find that unemployed workers react to the notification of an activation requirement in a US-based study. Using Danish data, Geerdsen (2006) shows that the exit rate from unemployment increases as individuals approach compulsory programme participation; and Rosholm and Svarer (2008) find that individuals react to a perceived risk of future programme participation. Threat effects have also been detected in the Swedish context by Hägglund (2011), who studied a pilot programme in three municipalities, and by Carling and Larsson (2005) and Forslund and Skans (2006), who studied an earlier youth activation programme. However as argued above, to provide evidence of screening, we should

[6] Besley and Coate (1992) discuss the *deterrent effect* of workfare, which relates to encouraging poverty-reducing investment. Participation in activation can also be seen as an investment that helps the individual find a job later on; however, in our context this should not be seen as a deterrent effect to the extent that unemployment is involuntary.

[7] If people also differed with respect to the discount rate, it could well be the case that people with a high job-finding rate discount future less; a situation that would further strengthen the pattern. DellaVigna and Paserman (2005) study the relationship between patience and job search effort, but they do not consider the role of activation.

find a pattern where the threat effect is heterogeneous such that individuals with good labour market prospects react to the threat of activation. While earlier examinations of threat effects have not focused on distinguishing between different types of workers, we use the predicted probability of finding work (in the absence of activation) as a proxy for an individual's labour market prospects and analyse whether the pattern of exit (both regarding the timing and heterogeneity across individuals) from unemployment after the introduction of activation supports the idea that there may be a screening role for ALMP.[8]

Let us next turn to papers that have examined whether activation programmes have different impacts among disadvantaged youth. There are only a few such papers, and they generally use low education as a proxy for being disadvantaged. Caliendo et al. (2011) evaluate a number of programmes in Germany and find persistently positive employment effects that are stronger for those with better education. Maibom et al. (2014) evaluates a randomised field experiment conducted in Denmark. The treated job seekers received more intensive support from caseworkers and mentors, and this was combined with other policies. They find that the treatment effect varies depending on the individual's education level, with no impact for those with basic education only. Finally, Hämäläinen et al. (2014) provide an impact evaluation of a Finnish activation programme similar to the Swedish one that we analyse, also targeted at youth. They find that the policy had positive but modest employment effects, and the effects are again concentrated to those with better education.[9]

Our paper is also related to literature on the relationship between health and unemployment. There is a large literature on this topic (see e.g. Eliason and Storrie 2009; Browning and Meinesen 2012) and we will not attempt to summarise it here. The focus in the present paper is not on the association between health and unemployment per se. Rather, we ask whether individuals with different health statuses (among other characteristics) react differently to activation policies. A related earlier paper is Nordberg (2008), who finds that individual health status affects the transition from vocational rehabilitation to work.

As elaborated in the Introduction, we contribute to the literature by providing evidence on the screening role of labour market programmes and by analysing whether the programmes are effective in helping the disadvantaged youth, using exceptionally rich data on individual background characteristics that may be related to one's position in the labour market. We do

[8] Rosholm and Svarer (2008) find that there is a strong threat effect from active labour market policies, but not for the long-term unemployed; this may be related to the notion of individuals in a poor labour market position not reacting to the threat of activation.

[9] Hämäläinen et al. (2014) are also interested in the health of job-seekers. The difference is that they use subsequent mental health as an additional outcome variable, whereas we concentrate on heterogeneous treatment impacts.

so not in the context of small pilot initiatives, but based on a country-wide major activation programme.

3 The youth activation programme

The activation programme we study is the Youth Job Guarantee (YJG) that started in Sweden in December 2007. The programme involves activation that starts 90 days after a person has registered as an unemployed jobseeker at the public employment service, and it involves all unemployed individuals who are under 25 years of age. That is, all individuals who have not yet turned 25 should be assigned after 90 days of unemployment.[10] The activation is mandatory for those in the targeted age group, and a refusal to participate could incur sanctions in the form of withdrawn unemployment or welfare benefits. If assigned to the programme, the individual needs to participate until he/she finds a job or enrols in education, i.e. individuals who are already in the programme are not allowed to drop out when they turn 25.[11]

Figure 1[12] illustrates the structure of the programme. The first three months (90 days) of an unemployment spell consists of open unemployment. After 90 days, the employment office undertakes an in-depth assessment of the situation of the individuals in the target group. In the first phase of activation that starts after 90 days, the programme mainly takes the form of job search assistance. After a further 90 days, the individuals who are still unemployed are transferred into a second phase of activation that, on top of job search activities, also can involve short periods of training or work placement to gain work experience. The motive behind the clear focus on job search assistance throughout the programme is to avoid the kind of lock-in effects that were shown to occur in previous youth programmes (Government Bill 2009/10:1).[13] The content of the programme is relatively flexible and should be tailored according to individual needs.

The activities within the YJG programme are supposed to imply full-time participation. However, based on a survey among participants in 2009, Martinsson and Sibbmark (2014)

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[10] Some rules of the programme have changed over time. We describe the rules in place during the time period we study, i.e. until February 2010.

[11] The maximum duration in the programme is 15 months. Individuals who are still unemployed after 15 months are transferred to another activation programme (the Job and Development Guarantee), which is aimed at long-term unemployed of all ages.

[12] All figures and tables are at the end of the paper.

[13] Until the end of 2006, unemployed youth were assigned to activities organised by the municipalities (mainly training or work placement) within the programmes Youth Guarantee (20—24-year-olds) and the Muncipality Youth Programme (18-19 year olds); see Carling and Larsson (2005) and Forslund and Nordström Skans (2006) for evaluations of the previous youth programmes.

conclude that this ambition is rarely met in practice. On average the participants reported that they spent 14 hours per week applying for jobs and participating in activities.

A further feature of the reform is that for some (well-defined) groups of unemployed, the unemployment benefit declines faster over time than it had done prior to the reform. During the time period we study, the earnings related unemployment benefit was normally 80 % of prior earnings for the first 200 days of unemployment, and declined to 70 % for the next 100 days. For some individuals participating in the Youth Job Guarantee programme, the rules were different: the 80 % replacement rate applied only for the first 100 days of unemployment, declined to 70 % for days 101-200 and further to 65% for days 201-300. Therefore, for some individuals, the reform involved elements of both activation and financial incentives. However, the individual was unaffected by the faster reduction of benefits if she (i) had children; or (ii) was only eligible for the basic unemployment benefit; or (iii) had an earnings related benefit that would have exceeded the maximum amount of benefits (SEK 680 [EUR 68] per day).

4 Data

We combine data on individual's employment status with information on their education, (past) health and other relevant personal characteristics. The data on unemployment spells come from the register of the Public Employment Service (PES), and the data on health status from hospital and drug registers provided by the National Board of Health and Welfare. The latter include yearly individual-level information on all purchases of prescribed medicine, all inpatient medical contacts[14] and all outpatient medical contacts in the specialised care.[15] To these registers we have also added a number of demographic variables from Statistics Sweden, information on unemployment benefit uptake from the Unemployment Insurance Funds, and information on sickness benefits as well as activity compensation (disability pension)[16] uptake from the National Social Insurance Board.

Our data cover the entire Swedish population, and we can observe all unemployment periods from 1991 to 24th of February 2010. The YJG programme was introduced in December 2007, and we analyse its effects in 2008 and 2009.[17] Our 2008 sample includes all individuals aged 19-29, who became unemployed between October 2007 and September 2008, and

[14] Refers to cases where the individual has been admitted to a hospital. In general this means that an overnight stay has been required.
[15] The hospital registers cover both public and privately operated health care and include ICD-codes for diagnoses.
[16] Individuals below age 30 are entitled to financial support if they are unable to work due to their functional impairment for at least a year.
[17] Combined health and labour market data are only available for these years.

therefore became eligible for the programme between January 2008 and December 2008, if they were still unemployed and below 25 years of age at that time. The 2009 sample is constructed in the same manner, but since the data end in February 2010, we sometimes need to restrict the sampling period in order to follow the unemployment spells long enough (e.g. when studying the probability of finding employment within a year, the sample is limited to spells beginning at least a year before). All analyses below are conducted using the combined 2008-09 data.

We assume that a person has found a job if she has left the PES register due to (unsubsidised) employment or has been registered as a temporary, hourly or part-time employee for at least one consecutive month.[18]

Table 1 provides descriptive statistics on the background characteristics of the individuals in the sample (excluding the health indicators). Column (1) includes all unemployed 19- to 29-year-old individuals; column (2) includes all participants in the YJG programme; and columns (3) and (4) include unemployed persons within one year from the eligibility cut-off age, that is, 24- and 25-year-old individuals, respectively. The 25-year-olds have a somewhat higher educational attainment and their previous earnings are higher than those of the 24-year-olds, reflecting the fact that they are older. In our main analysis in Section 5, we use an RD design, where the effects of the YJG programme are identified from a discrete change in programme eligibility and the probability of programme assignment at the threshold of turning 25. Therefore, what matters for our analysis is whether there are jumps in any of the background variables at the threshold. We examine this issue in Section 5.3.

Table 2 provides descriptive statistics for the main health indicators used in the analysis. One difference compared to Table 1 is that column (1) now includes all other Swedish residents who are 24 or 25 years old but who have not been unemployed in our data (whereas the data in Table 1 comes from the registers of the PES and hence includes only unemployed individuals). The purpose of this change is to provide a comparison of the health status of the unemployed individuals relative to others of the same age. Unemployed individuals (columns (3) and (4)) appear to have worse health than other individuals of their age (column (1)). For example, 15-16 percent of the unemployed 24- and 25-year-olds used a neurological drug the previous year and 9-10 percent used a drug for mental illness. Among other individuals of the same age, these numbers are 12 and 7 percent, respectively. On the other hand, the individuals in column (2) (all participants in the YJG programme) appear healthier than the 24- and 25-year-olds in our sample; this is likely explained by the fact that

[18] In Section 5.3 we check whether our results are robust to an alternative definition of employment.

the average individual in the YJG programme is younger than those in columns (3) and (4). There are very few differences between the individuals in columns (3) and (4).

Figures B.1, B.2 and B.3 in Appendix B provide some first descriptive analyses related to observed unemployment duration in our data. The graphs reveal that 24-year-olds (the target group of the programme) have shorter unemployment durations and better re-employment outcomes than 25-year-olds, when the sample is limited to individuals who are born during the same calendar year (to achieve better comparability between the groups). Analyses by differences in certain background characteristics show how those with compulsory education only and those who used a drug for a neurological conditions the previous year, remain unemployed longer than more highly educated individuals and individual who did not use such drugs. Later on in the paper we find that (past) mental health problems are particularly strongly concentrated among individuals with poor labour market prospects.

5 Empirical analysis

5.1 Empirical strategy

We use a regression discontinuity design to estimate the effects of the Youth Job Guarantee programme, using the fact that only individuals who were under 25 years of age at 90 days of the unemployment spell were eligible for the programme. Even though age may affect re-employment probabilities, we can expect individuals close to the eligibility cut-off to be similar to each other in all other respects, except that individuals on one side of the cut-off received the treatment (programme eligibility) and individuals on the other side did not. (The balance of background characteristics at the threshold is examined in Section 5.4.1.) Hence any differences in employment probability that we find between individuals on each side of the cut-off can be attributed to the YJG programme.

An important point to note is that the assignment variable in our application is not age per se, but age at a particular date (90 days after entering unemployment). Once assigned to the programme, individuals risked losing their unemployment benefits if they dropped out when they turned 25. Hence we avoid an often-encountered problem in age-based RD analysis, i.e. the possibility that reactions of individuals close to the cut-off age would be affected by anticipation of future changes in treatment status when they cross the age threshold (Lee and Lemieux 2010). Further, unlike RD-type designs using age as the assignment variable, in our case programme assignment is stochastic (as in regular RD): To the extent that one cannot fully control the date of becoming unemployed – in particular, whether the unemployment spell starts more or less than 90 days before one's 25th birthday - then programme assignment in our application is not deterministic.

However, a potential threat to a causal interpretation of our estimates is that the presence of the programme could affect individuals' decision to register at the PES. If there are individuals with detailed knowledge of the programme and the eligibility requirements before registering at the PES, even though they cannot fully control the time of becoming unemployed, some of them may choose to delay registration in order to avoid activation. [19] This would lead to sorting around the eligibility threshold.[20]

Figure 2 shows the number of individuals entering unemployment, by age at day 90 after the start of the unemployment spell (where age is measured relative to the cut-off age 25). There is no evidence of a decline in the number of individuals registering just before the eligibility cut-off or of a spike just after the cut-off. Hence, the figure does not suggest that individuals time their registration in order to avoid activation. This is also confirmed by the McCrary-test (McCrary 2008), which does not detect any discontinuity at the threshold.[21] (Related to the subgroup analysis that we perform below, Figure C.1 in the online appendix shows that there is no sorting for the different quartiles of predicted employment probabilities either.)

Finally, could identification be compromised by the existence of other programmes? Sweden has a rich set of training and job search assistance programmes also for unemployed individuals older than 25. However, as a result of the introduction of the YJG, programme participation is much more common among unemployed individuals under 25 years of age; see Figure 3. The figure shows that the likelihood of participating in some labour market programme increases sharply for 24-year-olds around 90 days of unemployment, whereas there is no such pattern for 25 year-olds. This is reassuring: First, it indicates that we do not need to worry about possible confounding effects arising from programme participation by older job-seekers. Second, the figure confirms that the difference in programme participation between the age groups indeed occurs after 90 days of unemployment, which ensures that we can obtain estimates for the threat effect (i.e. any possible effects observed before 90 days are not due to programme participation).

[19] Individuals are likely to be informed about the programme upon registration at the PES and/or during their first meeting with a caseworker, which should take place within 30 days of unemployment. However, individuals can also learn about the programme from information sheets available at the PES as well as from the PES website.

[20] Note that this type of response is unlikely among UI recipients as registration at the PES is required in order to receive UI benefits.

[21] McCrary (2008) develops a formal test of the null hypothesis of continuity of the density of the assignment variable at the cut-off, against the alternative hypothesis that there is jump in the density function at that point. We cannot reject the null; the test statistic has value 0.0097 and standard error 0.0158.

5.2 Results for the whole sample

We first present a graphical analysis of our data, with the purpose of analysing whether there are any jumps in the job finding probability at the YJG eligibility threshold (i.e. between 24- and 25-year-olds). We use four dummy variables to measure the effect on employment: These indicate whether the individual became employed during the first 90, 180, 270 and 365 days after entering unemployment. Hence, the first outcome (D_{90}) measures the threat effect, while the other outcomes (D_{180}, D_{270} and D_{365}) capture the total effect of programme eligibility after different length of time. It should be noted that the latter three outcomes capture a combination of the threat effect and possible programme effects. The causal effect of the programme itself (say the probability of finding work between days 90 – 180 of the unemployment spell, while the individual already participates in activation) cannot be estimated without stronger assumptions, as the individuals who remain unemployed at day 90 are no longer representative of the overall pool of unemployed.

The threat effect (or pre-programme effect) is analysed in Figure 4a. In the figure, the individuals in the data are arranged according to their age at day 90 after entering unemployment, and age is measured relative to the cut-off age 25. That is, the negative portion of the x-axis in Figure 4a consists of individuals who would become eligible for the YJG if remaining unemployed for 90 days. Individuals are divided into bins of one month, and we plot bin averages of the D_{90}-dummy. As our age variable is continuous - it is measured in days - a full-fledged RD analysis is possible. We fit local linear regressions of D_{90} on relative age using a triangle kernel and an optimal bandwidth (as defined by Imbens and Kalyanaraman 2012).[22] Bins with $x < -3$ and $x > 3$ are excluded from the figure for clarity, as we want to focus on individuals close to the eligibility cut-off. The solid line in the figure shows the fitted values from these regressions, and the dashed lines show the associated 95 percent confidence intervals.

Figure 4a indicates that there is a significant threat effect, even though it appears to be small: being eligible for the YJG programme (i.e. being under 25 years of age at 90 days of unemployment) increases the probability of finding employment during the first 90 days after entering unemployment spell by around 2 percentage points. Taking into account that about 28 percent of the 25-year-olds find employment within 90 days, this would correspond to an increase of about 7 percent.

[22] There are several different ways of calculating the optimal bandwidth in an RD design, with no clear consensus on which is the best one. We analyse the robustness of our results to a wide variety of bandwidths in Section 5.3.

Figures 4b-d present similar analyses of the effect at day 180, 270 and 365 after the onset of unemployment. That is, we look at the relationship between age and the D_{180}-, D_{270}- and D_{365}-dummies. The figures show statistically significant effects of programme eligibility also at day 180 and 270, but not at day 365. Hence, the figures suggest that job finding among those ineligible for the YJG programme starts to catch up later on during the unemployment period.

We next report RD-estimates of the effect of being eligible for the YJG programme for the different outcome variables. These results are shown in Table 3, and they confirm the results from the graphical analysis: The threat effect for the whole sample is approximately 2 percentage points, which corresponds to an increase of around 7 percent if we relate it to the average outcome among 25-year-olds. (The estimated effects reported in Table 3 are positive, as the observed drop in the employment probability at the threshold of turning 25 (Fig. 2) corresponds to a positive effect. That is, younger individuals – those who are eligible for the programme – have a higher probability of finding work.)

The employment probability remains higher among those who are eligible for the YJG programme also at day 180 and day 270 after registration at the PES. A year after the beginning of unemployment, the effects are no longer statistically significant.[23] Given that the effect within 180 days (or later) is not notably higher than the threat effect, the results indicate that participation in the activation measures in itself does not significantly affect employment probabilities. The overall effects of the programme can therefore largely be attributed to the threat effect.

How do our results compare with earlier estimates of the magnitude of the threat effect? Due to differences in programme details, empirical methods and the way results are reported, the comparison is not necessarily straightforward. For example, Black et al. (2003) find that the programme they study shortened the unemployment spell by two weeks. The corresponding estimate in Rosholm and Svarer is two and a half weeks. Papers that report time-specific hazards find large short-time increases in employment hazards (e.g. Hägglund 2011, Geerdsen 2006). The most closely linked papers to ours are perhaps those studying an earlier Swedish programme targeted at youth. Carling and Larsson (2005) find that the threat effect amounts to a 10 per cent increase in the job finding rate, but the impact quickly vanishes and reaches zero by 120 days. Forslund and Nordström Skans (2006) find that the probability of still being registered with the public employment service declined by 4% during the first 90 days of unemployment. Our result – an approximately seven per cent

[23] Since the data ends in February 2010, we sometimes need to restrict the sample in order to follow the unemployment spells long enough (e.g. when studying the probability of finding employment within a year, the sample is limited to spells beginning at least a year before).

increase in the job finding rate due to being eligible for the treatment – is well in line with these earlier studies.

When discussing the magnitude of the effects, however, it should be noted that the effects that we have reported are intention to treat effects, i.e. effects of programme eligibility. When interpreting the results, one must bear in mind that programme take-up is incomplete. The relationship between age (at 90 days of unemployment) and participation in the YJG programme is depicted in Figure 5. The figure is drawn in a similar way as Figures 4a-d, but the dependent variable is now a dummy for actual participation in the YJG programme. The bandwidth chosen is the same as in the estimation for the D_{180} dependent variable. The figure is drawn only for the relevant subpopulation, i.e. individuals whose unemployment spell lasted over 90 days.

Figure 5 reveals an interesting pattern. Take-up is practically zero for individuals over 25 years of age, as it should be. For most age groups below 25, take-up is around 50 percent, but it falls sharply before the 25-year threshold. The likely reason for this is that caseworkers have not been able to assign individuals to the programme straight away at 90 days of unemployment; rather, assignment takes some time (e.g. due to the high workload on caseworkers), and the individual's age is checked only at the time when programme assignment is considered. Some people who are close to 25 at day 90 have therefore turned 25 by that time, and are no longer eligible. There is nevertheless a statistically significant drop in take-up at the threshold of around 25 percentage points.

The effects that we have reported above in Table 3, correspond to a sharp RD design, and should be thought of as intention to treat effects – they are the effects of programme eligibility. On the other hand, Figure 5 clearly shows that programme assignment was very fuzzy. Using a fuzzy RD design, we get an estimate of, e.g., the threat effect of 0.153 (standard error 0.0463), i.e. an approximately 15 percentage point increase in the probability of finding work during the first 90 days after entering unemployment. Naturally, this effect is considerably higher than the sharp RD-estimate, since it is essentially a Wald/IV-estimate that involves dividing the sharp RD-estimate with the estimated jump in take-up at the threshold.

When take-up is incomplete, one would usually consider the fuzzy estimates to be preferable, as they take into account the fact that not everyone who is eligible actually receives the treatment. In our context, the fuzzy estimates are somewhat hard to interpret: how should one think of the threat effect on the "compliers", as the threat effect is about what happens before people actually enter the programme. On the other hand, if one considers the low actual take-up to affect the strength of the threat (if people know that the programme is not strictly enforced), the fuzzy estimate can be thought of as a meaningful measure of the threat effect, as it takes the strength of the threat into account. Nevertheless, since the sharp RD estimates are more straightforward to interpret in our context, we focus on them in following analyses.

5.3 Results by subgroups

We next turn to analyse how the effects of programme eligibility differ by individual background. From the point of view of our motivating idea – whether the programme functions as a screening device and/or whether it helps disadvantaged individuals with a difficult labour market position – we need a measure of an individual's labour market prospects overall (not yet thinking about any programme effects). To achieve this, we first take a look at how the individual background characteristics found in our data are related to the probability of finding employment during the first year of the unemployment spell *before the reform*. The results are presented in Table 4.

A number of groups stand out: Individuals with compulsory education only and those born outside the Nordic countries appear to have a clearly lower probability of finding a job than others. Regarding the health variables, individuals who received disability pension, who were treated for mental illness (including both inpatient and outpatient care) or took a neurological drug appear to have particularly low job finding rates.

To create a summary measure of the individual's labour market position, we use the model reported in Table 4 to predict employment probabilities for the individuals in our sample. We then divide the sample into quartiles by the predicted probabilities: those in the 1st quartile have the worst employment prospects, whereas those in the 4th quartile are most likely to find work (based on observable characteristics). Given that many characteristics (beyond, say, education) are related to labour market prospects, this procedure has clear advantages over concentrating on any single variable as a proxy for disadvantageousness. The approach is particularly attractive as it allows us to take full advantage of the richness of our data.[24]

Using a prediction model to classify individuals according to their labour market prospects is not uncommon in the programme evaluation literature; recent examples include Altmann et al. (2016) and Nekoei and Weber (2017). It is important to note that we estimate the

[24] This procedure has similarities to that in Black et al. (2003), who use subgroups by profiling scores to test whether the profiling score system used to allocate assistance programmes to the unemployed works as intended. The profiling score estimation appears to have used a very limited set of individual background characteristics (Berger et al. 1997).

prediction model on out-of-sample data (i.e. pre-reform data from 2007), and we therefore avoid any biases that might arise from endogenous stratification (Abadie et al. 2016).[25]

Descriptive statistics for the different quartiles are reported in Table B1 in the Appendix. There is a clear concentration of mental health problems in the 1st quartile: e.g., ten times more of the individuals in the 1st quartile were treated for mental illness in the past year, compared to individuals in the 4th quartile. It is also much more common for individuals in the 1st quartile to have received disability pension. On the other hand, the quartiles do not differ notably in the other health indicators. Even though low education and immigrant status are very important for labour market prospects, our data clearly indicates that past mental health problems are also crucial in this respect.

We next estimate the effect of programme eligibility by quartiles of the predicted employment probabilities. The results are shown in Figures 6 and 7 (for the threat effect and the effect until day 180, respectively) and in Table 5.

We find no evidence that individuals in the most disadvantaged labour market position are affected by the threat of activation: The estimated threat effect is close to zero and statistically insignificant for the lowest quartile, while it is significant at the 5 percent level for the second and third quartiles and strongly significant for the top quartile. These results are thus consistent with the idea that individuals in a better labour market position may be more likely to respond to the threat of activation, and hence with the notion that activation programmes may work as a screening device. If we relate the estimated effects for quartiles 2-4 to the mean outcome among 25-year-olds, they correspond to an increase in the probability of finding employment during the first 90 days of by approximately 7 percent.[26]

The effect of programme eligibility remains statistically significant at the 5 percent level for quartile 4 also at 180 days after entering unemployment, though the effect in relative terms is somewhat smaller in size compared to the estimated threat effect (around 7 percent compared to 5 percent). The results also indicate that the effects for quartiles 2-4 are driven by the threat of programme participation, as entering the activation phase itself does not appear to strengthen the estimated effects for these groups. The effect within 180 days is

[25] The problem noted by Abadie et al. (2016) would arise if the prediction model were estimated on control group data and then used to form predictions for both treatment and control group. This would lead to over-fitting of observations in the control group.. In our case, both the control group and treatment group data come from 2008-2009, whereas the prediction model is estimated on data on unemployment spells in 2007. Nekoei and Weber (2017) use a similar procedure to avoid the problem of over-fitting.

[26] Also in quartile 1, the relative "effect" is about 7%, but we would not conclude that there is an effect in the 1st quartile. As noted above, the estimated effect itself is very small and far from being statistically significant, and the magnitude of the relative effect is driven by dividing this estimate by a small number i.e. the baseline job-finding rate in the 1st quartile. Note also that if anything, one would expect the relative effect to be *largest* for the first quartile, if the individuals in the first quartile were to react at all: as the baseline job-finding rate is very low (10% in the first 90 days of the unemployment spell), there should be much room for effects from policy interventions.

marginally significant also for the lowest quartile. While this provides suggestive evidence that some individuals in the lowest quartile respond to activation measures, the results do not provide strong support for the idea that benefits from activation would be concentrated among those most in need of assistance.

At later follow-up times, i.e. at day 270 and 365 after the onset of unemployment (not shown), there are no longer any statistically significant differences between the eligible and ineligible in terms of transitions to employment. Hence, while programme eligibility seems to have shortened unemployment spells for some of the unemployed individuals – in particular those with a more advantaged labour market position – we find no long term effects on employment for any of the groups.

Finally, as explained at the end of Section 5.2, we focus on intention to treat effects from a sharp RD design (rather than Wald estimates from a fuzzy RD design), because the distinction between compliers vs. non-compliers is somewhat hard to interpret in the context of the threat effect. It is nevertheless important to check that there is a significant jump in take-up at the age eligibility threshold in each quartile. Figure B.4 in the appendix confirms that this is the case. The jump is somewhat larger for the top quartiles, but still close to 20 percentage points for the lowest quartile. The simple fact that these individuals have registered at the PES implies that they should be willing and able to work, despite some individuals being in a more challenging position than others. The official rules for programme participation are similar regardless of individual background characteristics, and possible differences in subsequent programme assignment or take-up probabilities are likely to be hard for the individuals to predict a priori.

5.4 Validity and robustness checks

We now turn to assess the validity of our RD design. Since some of our main conclusions stem from the analysis of how the treatment effect varies by quartiles of predicted employment probabilities, we perform robustness checks both based on the entire sample and separately by quartiles. We discuss all robustness checks below, but for the sake of space, we report the detailed results by subgroup in a separate online appendix; see Appendix C.

5.4.1 Balance of background variables and robustness to covariates

First, we check whether there are any discontinuities in any pre-determined variables at the eligibility cut-off. When examining the balance of background variables at the threshold, we look at the following variables: gender, birthplace (dummy for being born outside the Nordic countries), being disabled, three education dummies, employment status the previous year, income from work the previous year, unemployment insurance receipt the previous year, social assistance receipt the previous year, being a parent in 2007, and the month of entry into unemployment.

We draw figures similar to Figure 4 for all the background variables, and run separate RD analyses – identical to those that we conducted for the outcomes of interest – for each background variable to estimate the magnitude of any possible jumps at the threshold. The results are depicted in Figure 8. To keep the dimensions of the figure manageable, we exclude dummies for the month of entry into unemployment, even though we have run balance checks also for those since the time of entry may influence employment prospects. In total, we have run balance checks for 36 background variables. Most of the background variables are balanced at the threshold. However, two variables have jumps that are statistically significant at the 5 % level (entering unemployment in October, where the estimated jump is -0.0095 (s.e. 0.0039), and having used more than two medicines in the previous year (0.0123, s.e. 0.0052)), and two variables at 10 % level (unemployment insurance receipt in the previous year (-0.0109, s.e. 0.0057) and social assistance receipt in the previous year (0.0098, s.e. 0.0051)).

Given that we have a large number of background variables, some statistically significant jumps are of course expected. The discontinuities that we observe do not seem to follow any particular pattern (e.g. indicating that individuals with background characteristics associated with good employment prospects would be concentrated on the left-hand-side of the threshold). To further ensure that our results are not driven by any kind of selection of individuals at the threshold, we check the robustness of our results to including controls for background characteristics. In addition to the variables included in Figure 8 and month dummies, the regressions also control for municipality fixed effects. Our results are robust to controlling for background characteristics: The estimates for the threat effect and the effect at day 180 remain highly significant and the point estimates stay very similar; see Table 6.

We have also checked the balance of the background variables and the robustness to adding covariates for the estimations by quartiles; see Figures C.2–C.5 and Table C.1 in the online appendix. [27] As in the main analysis, there are some statistically significant jumps for some of the background variables. Again, the discontinuities seem quite random and do not

[27] The dummy for disability pension is not included in the figures for quartiles 3 and 4, as there are too few individuals in these quartiles with disability pension to run an RD analysis. The dummy is however included as a control in the analysis with covariates for all quartiles.

appear to have any meaningful pattern.[28] It is also reassuring that the treatment impact and the pattern of reactions across the quartiles remain qualitatively the same when adding covariates.

5.4.2 Placebo tests

As a further robustness check, we carry out several placebo tests. First, our data allow us to examine the presence of pseudo-effects *before* the YJG programme was actually in place. However, individuals who became unemployed before the end of 2006 may still have been affected by the previous youth programme[29], and towards the end of 2007 individuals may start to anticipate that if they stay unemployed long enough, they will eventually become eligible for the YJG programme (from December 2007 onwards). For this reason, we limit this placebo check to examining the presence of a threat effect among those who became unemployed during January-June 2007. (Ending the sampling in June is a cautious yet somewhat ad hoc choice, since it is not clear when the first anticipation effects might occur, if there are any. The programme was first suggested already in April 2007 and the government bill was given in May, but on the other hand unemployed youth might not be very well informed about such policy plans. The results are not affected if we consider unemployment spells that started e.g. in January-August 2007 instead.) Figure 9 shows that there is no discontinuity at the threshold for this sample.

We have also examined whether there are placebo effects at the threshold between 23- and 24-year-olds (where age is again measured at day 90 of the unemployment spell, with this placebo threshold corresponding to -1 on the x-axis in Figure 4), as well as the threshold between 25- and 26-year-olds (+1 on the x-axis in Figure 4). There are no labour market programmes or other relevant policies that would be expected to cause a discontinuity in the probability of finding work at these thresholds. Indeed, all estimated effects are close to zero at both thresholds; see Table 7.

The same placebo tests have been performed for the estimations by quartiles (see Figure C.14 and Tables C.2—C.3 in the online appendix). The results from the placebo tests in 2007 by quartiles do not give rise to any concerns. Most of the placebos by quartiles for the thresholds

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[28] Quartile 2 might seem somewhat problematic: there are statistically significant and positive jumps in two sickness variables (had a neurological drug, had more than two medicines) as well as in the disability pension dummy. On the other hand, according to Table 4, having had more than two medicines is actually associated with better rather than worse employment prospects. There is also a *negative* jump in having received unemployment benefits the previous year. After adding covariates, the threat effect for quartile 2 is significant only at the 10 % level. The results for the other quartiles, as well as the overall pattern of reactions (no reaction for quartile 1, strongly significant reactions for the upper quartiles) are robust however.

[29] Until the end of 2006, unemployed 20—24-year-olds were assigned to activities organised by the municipalities within the programme Youth Guarantee. The Youth Guarantee was still in place during 2007, but no new unemployed individuals should have been assigned to this programme after the end of 2006.

of turning 24 and 26 are also not statistically significant, but there are some negative impacts of turning 24 (for quartile 3) and turning 26 (for quartile 2). However, as the corresponding estimated treatment impact in the main analysis is positive, these observations work against detecting a significant treatment impact. Further, it is important to note that the separate placebo tests at each threshold are not independent: e.g. both results at the threshold of turning 26 for quartile 2 are driven by the "effect" for D_{90} for this quartile at this threshold.

5.4.3 Robustness to bandwidth selection

Figures 10 and 11 plot the estimated effects (and the 95 percent confidence intervals) from the sharp RD design (the effects of programme eligibility) as a function of bandwidth. The figures show that our results are robust to bandwidth selection. The threat effect and the effect during days 1-180 become insignificant only at bandwidths far below the optimal bandwidth.[30] In online Appendix C we show figures similar to Figures 10 and 11 for the different quartiles of predicted employment probabilities; see Figures C.6–C.13. The estimates in particular for the second quartile turn out to be at bit sensitive to bandwidth selection, while the estimates for quartile 3 and 4 are fairly stable. All in all, the result that the very weakest individuals - those in the lowest quartile - do not respond to the threat of activation is very robust, while some individuals with better labour market prospects do.

5.4.4 Calonico et al. (2014) robust inference

Calonico et al. (2014) recognise that since implementing an RD design in practice normally requires using observations that are away from the cut-off value of the assignment variable, ignoring the resulting bias leads to biased confidence intervals for the estimated effects. We have examined the robustness of our results to using the robust inference procedure suggested by Calonico et al. (2014). The results are reported in Table 8.

The robust confidence intervals are naturally wider than their conventional counterparts. The threat effect (days 1-90) is now only significant at the 10 percent level for the whole sample (robust p-value 0.082), and the same applies to the effect for days 1-270 (robust p-values 0.094). The effect for days 1-180, on the other hand, remains statistically significant at the 5 percent level. A similar analysis for the subgroups is presented in online Appendix C,

[30] The finding that the effects of programme eligibility go toward zero for the smallest bandwidths (i.e. very close to the threshold) has a natural explanation in our case: this is explained by the behavior of take-up close to the threshold. Given that there is only a fairly small jump in take-up at the threshold, it would be surprising if we were to find large effects there. This conjecture is supported by the following finding: If we take into account incomplete take-up and examine the robustness of the Wald estimates from the fuzzy RD design (reported at the end of Section 5.1), the point estimates do not decline at small bandwidths, with the exception of the estimate for D_{180} at the smallest bandwidth of 10 percent of the optimum, when the estimates are very imprecise (see Table A.2).

Table C.4. The threat effect remains significant at the 10 percent level for quartile 3 and the effect for days 1-180 for quartiles 3 and 4.

5.4.5 Robustness to changes in the definition of employment

So far we have not considered a person employed if she received any type of subsidised employment. In 2008 the rules for eligibility to one type of subsidised employment, New Start Jobs, differed for individuals who had/had not turned 25 (thus, the same age cut-off as for the YJG programme): Employers could receive this subsidy if hiring a person who had been unemployed for at least 6 months if this person had not yet turned 25. Individuals who had turned 25 had to be unemployed for at least 12 months before employers would be entitled to the subsidy.[31] By disregarding all hires where the New Start Job subsidy was paid out we thus risk underestimating the effects of the YJG programme. However, as we show in Table 9, our estimates are very similar if we instead treat New Start Jobs as regular employment (this is also the case for the estimates by quartiles; see the Table C.5 in the online appendix). The most likely reason why our results are not affected is that few employers applied for this subsidy at the time, potentially due to lack of information; see Liljeberg, Sjögren and Vikström (2012).

5.4.6 Accounting for changes in financial incentives

For an overwhelming majority of the treated individuals (87 %), the programme involved participation in activation policies only. However, as we noted in Section 3, a proportion of the treated individuals were not only subject to activation policies, but also experienced changes in their financial incentives. Those unemployed who had children, who received the basic level of benefits only, or whose earnings-related benefit exceeded a cap level were excluded from being subject to changes in financial incentives. Given that the groups whose financial incentives changed were well defined, we can examine the effects of programme eligibility separately for groups whose financial incentives changed vs. those whose did not.

We would expect the programme to have stronger effects on individuals who experienced a cut in benefits in addition to activation. This is indeed what we find – see Table 10. However, the average effects (both before entering the programme and afterwards) are indeed positive also for those who did not face a cut in benefits: hence activation has an effect on job finding rates even in the absence of any explicit financial incentives.

It is important to note, however, that from these numbers we cannot derive causal estimates of the effects of financial incentives (compared to pure activation) on the

[31] From March 2009, the rules are the same for 24- and 25-year-olds: the six months rule was extended also to also cover 25-year-olds.

probability of finding work: the groups whose financial incentives changed may react also to activation in a different way than others. Nevertheless, it is useful to check that the effects change in the expected direction, and statistically significant impacts also remain for the subgroup without changes in financial incentives.

We cannot carry out an analysis analogous to that in Table 10 for the quartiles, as the sample of individuals who faced a benefit cut becomes too small for an RD analysis when divided into quartiles. Despite being unable to carry out a comparison, we can estimate the effects separately for the group whose financial incentives were not affected. The main pattern that we find is unaffected: the threat effect is insignificant for the first quartile and positive for the upper quartiles – see Table C.6 in the online appendix[32]. Alternatively, we can run the RD analysis while controlling for a dummy indicating whether an individual belonged to those population groups who were subject to the cut in benefits (if they were eligible for the programme). This allows for higher job finding rates for individuals who faced a cut in benefits, as well as different effects of financial incentives in each quartile (as we are carrying out the analysis separately for each quartile). All our results remain intact if we control for the effect of financial incentives in this way, as shown in Table C.7 in the online appendix.

6 Conclusion

In this paper, we start by pointing out that within a search-theoretic framework where job seekers differ in their underlying job-finding probability, individual responses to activation policies will follow a certain type of pattern: Individuals with a high job-finding probability respond already to the threat of activation, whereas individuals with a low job-finding probability might catch up during the actual activation phase. The former effect points towards a screening role of activation policies, whereas the latter effect would imply that activation truly helps those in need of assistance.

We have used a regression discontinuity design to study the existence of this type of a pattern of responses in the context of an activation programme targeted at young unemployed individuals (the Youth Job Guarantee programme) introduced in Sweden in 2007. The programme is a major country-wide activation policy that affects all young unemployed persons below the age of 25. The data used cover the whole population of job-seekers. The main novelty of the data set is that it contains very detailed information on individual characteristics, including register data on the health and labour-market

[32] The effect for the fourth quartile becomes statistically insignificant however, even though the point estimate is still three times larger than for the first quartile, as in our earlier analysis. Again, the loss in significance cannot be attributed to a causal effect of financial incentives, but may be due to a different (and smaller) sample.

background of the unemployed. We use this data to predict individual job-finding probabilities (in the absence of activation), and conduct sub-sample analysis using a procedure that avoids the problem of endogenous stratification.

Our results show that there is a statistically significant and robust threat effect associated with the programme; programme eligibility increases the probability of finding work before the programme starts by about 7 percent. The threat effect indeed follows a pattern consistent with the screening hypothesis: The threat effect appears to be mainly driven by individuals in a relatively good labour market position. On the other hand, we find no statistically significant threat effect among individuals with characteristics that predict poor prospects of finding a job (in particular low education, immigrant background, poor mental health). We do not find any longer term effects of the programme: about a year after registration at the employment service, job finding among the ineligible seems to have caught up with that of the eligible.

Mandatory activation can be seen as a way to reduce the moral hazard related to unemployment insurance, and the analysis in this paper indicates that it may indeed serve this purpose by screening those who are less in need of support away from the pool of transfer recipients. Hence, activation may be a way to preserve efficiency while maintaining high replacement rates for the unemployed. However, this policy conclusion comes with two important caveats. The first is that the size of the impact of the policy is modest, perhaps because the coverage of the actual activation (the take up) could be higher. Secondly, and perhaps more importantly, the type of policy conducted in Sweden was clearly not sufficiently supportive for those with challenging labour market prospects. Instead of training geared towards enhancing job-seeking skills, these youngsters are likely to need more thorough support, such as counselling, further education and greater emphasis on improved health.

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Figures for the main text

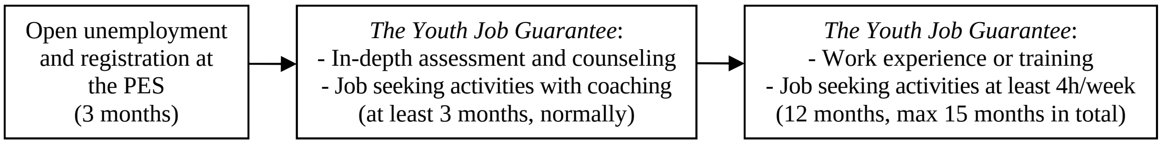

Figure 1: The Youth Job Guarantee Program

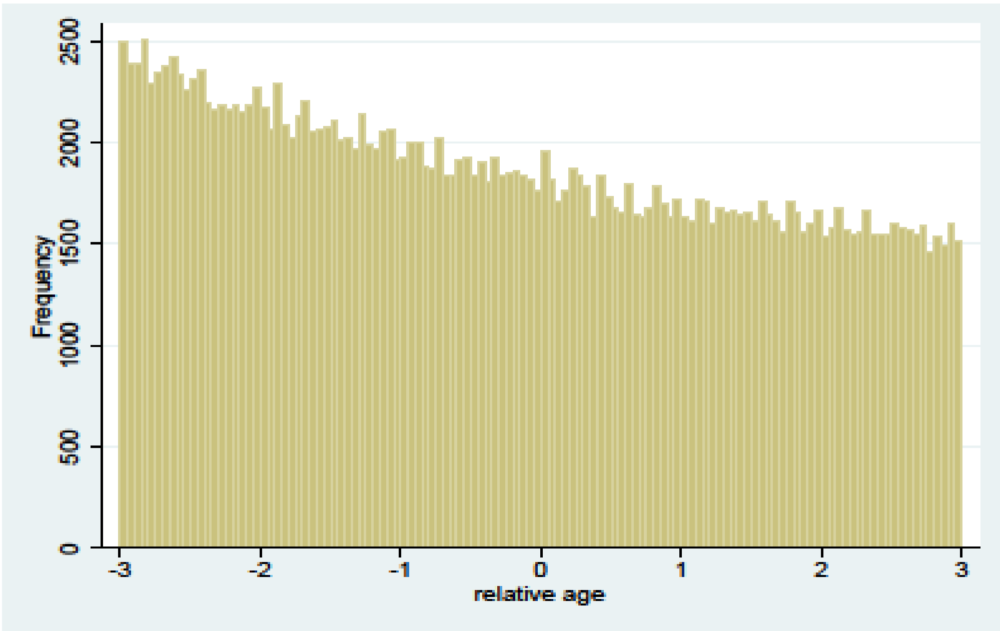

Figure 2: Number of individuals entering unemployment, by age at day 90 of the unemployment spell

Note: Age in years relative to the cut-off age 25 on the x-axis.

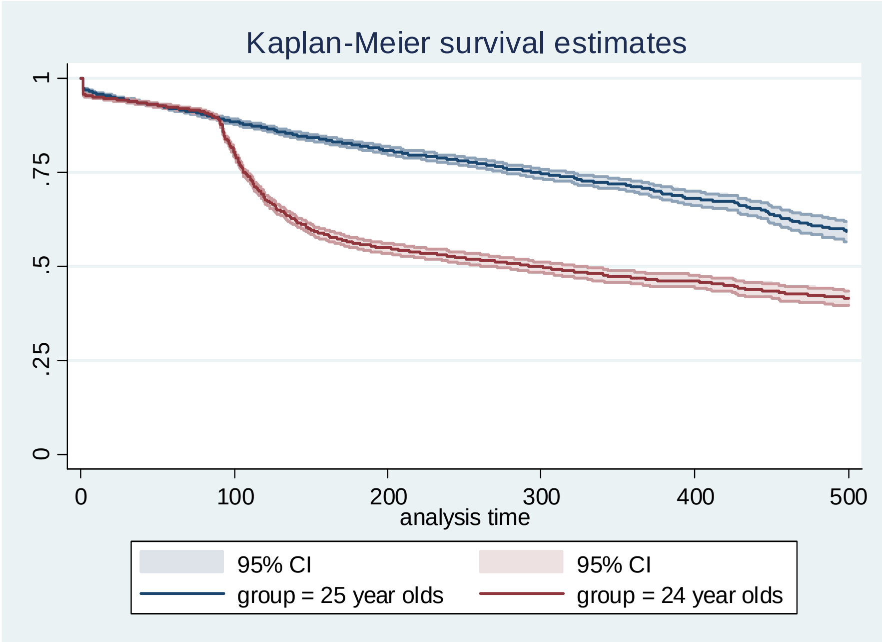

Figure 3: Kaplan-Meier survival estimates for duration until programme start for 24- and 25-year-olds in 2008 - 2009.

Note: The figure shows the probability of remaining in open unemployment relative to starting *any* labour market programme at different points of time during the unemployment spell. The individuals are divided into groups based on their age 90 days after entering unemployment. The sample is limited to 24- and 25-year-olds who are born during the same calendar year.

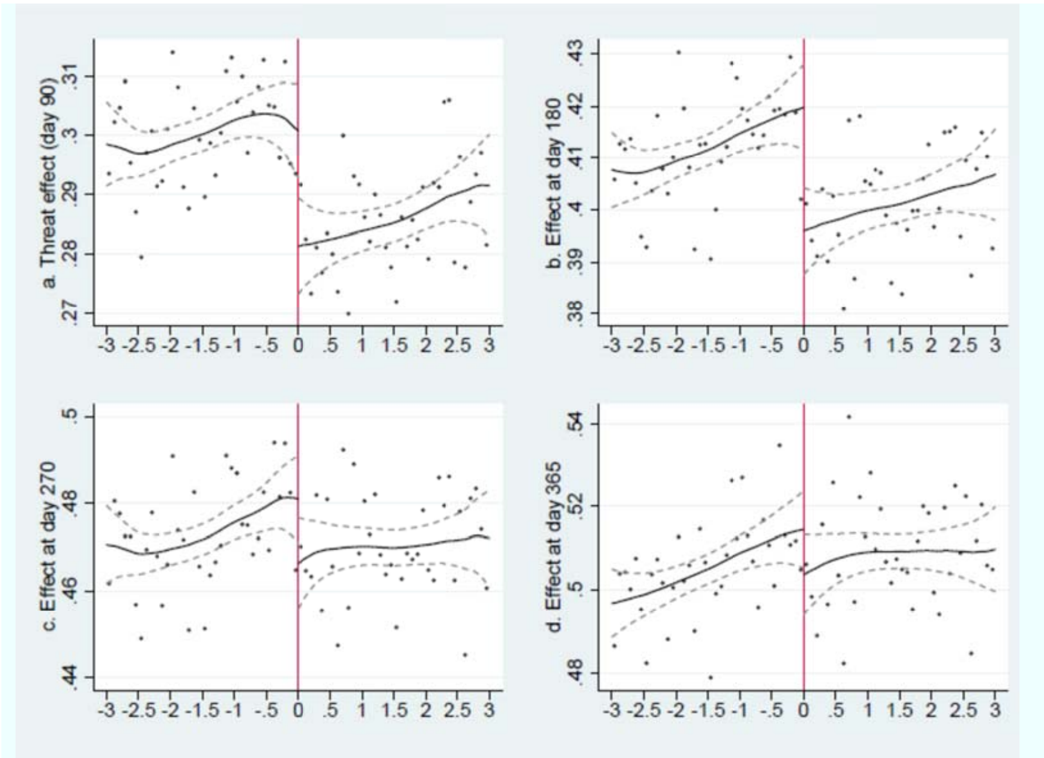

Figure 4: Effects of programme eligibility on the probability of finding employment

Note: Age in years relative to the cut-off age 25 on the x-axes and indicators for becoming employed during the first 90, 180, 270 and 365 days of unemployment on the y-axes. Age refers to the individual's age 90 days after entering unemployment.

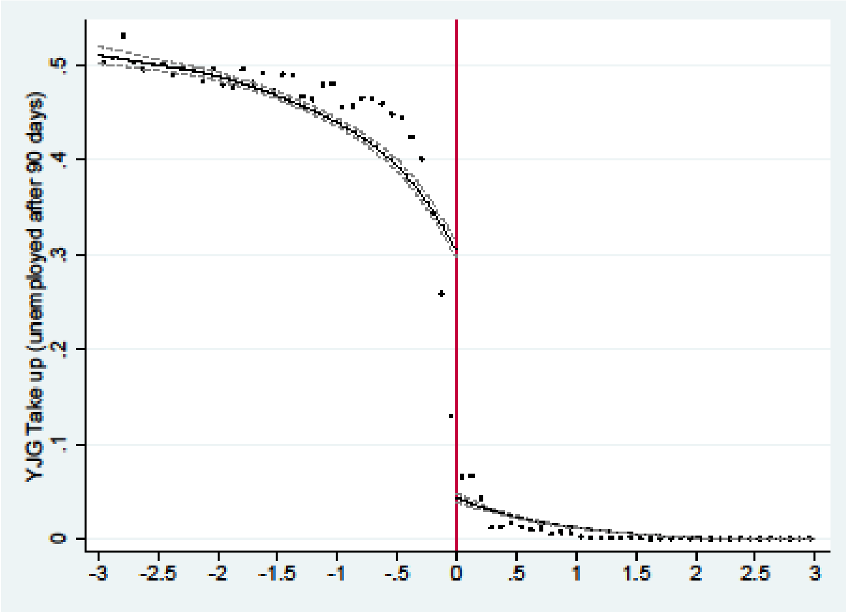

Figure 5: Youth Job Guarantee take-up

Note: Age in years relative to the cut-off age 25 on the x-axis and an indicator for participating in the programme on the y-axis. Age refers to the individual's age 90 days after entering unemployment. The figure is drawn only for individuals whose spell lasted over 90 days.

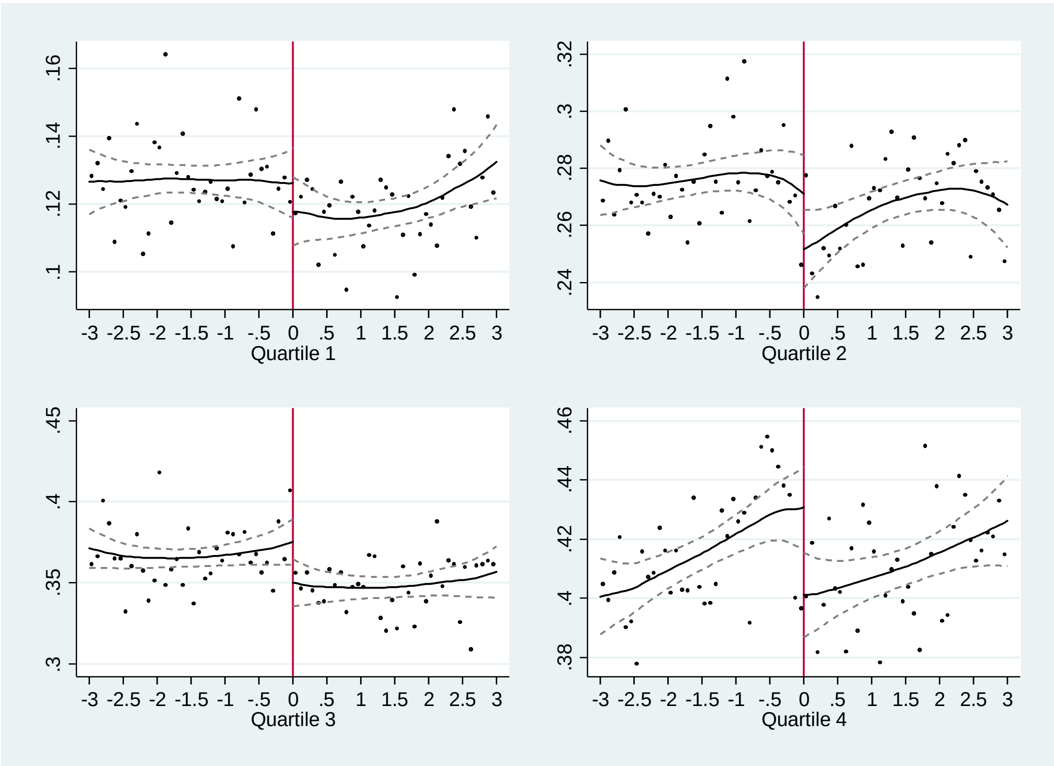

Figure 6: Effects of programme eligibility on the probability of becoming employed by day 90, by quartiles

Note: Age in years relative to the cut-off age 25 on the x-axis and indicators for becoming employed during the first 90 days of unemployment on the y-axes. Age refers to the individual's age 90 days after entering unemployment.

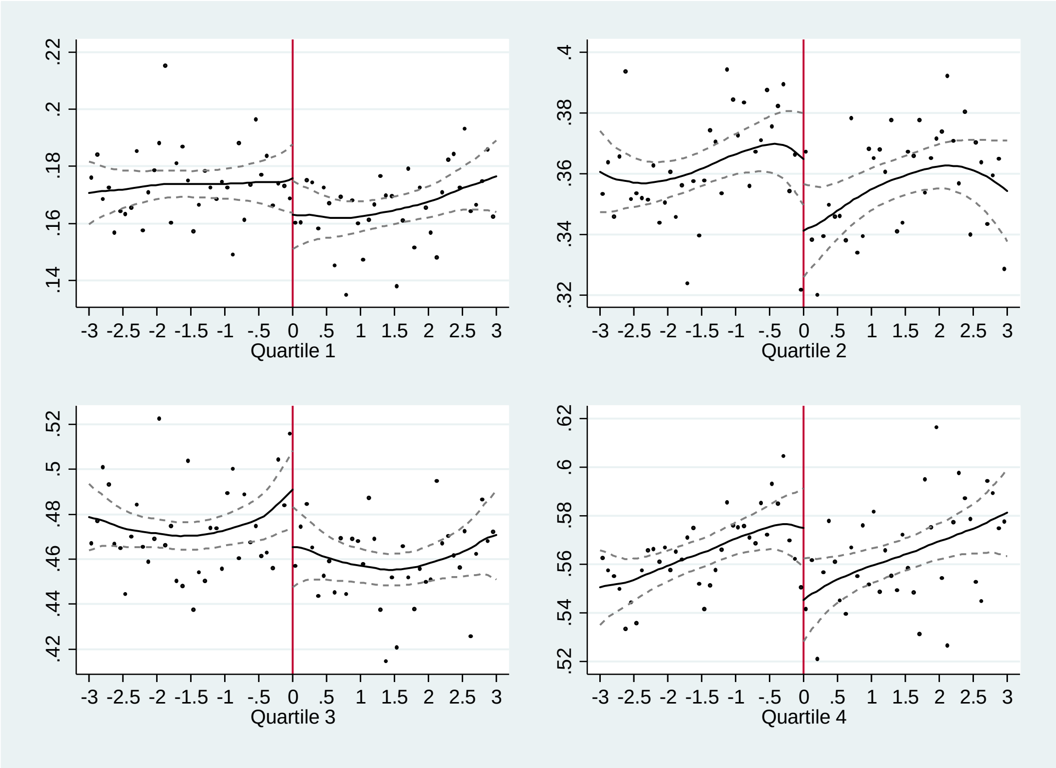

Figure 7: Effects of programme eligibility on the probability of becoming employed by day 180, by quartiles

Note: Age in years relative to the cut-off age 25 on the x-axis and indicators for becoming employed during the first 180 days of unemployment on the y-axes. Age refers to the individual's age 90 days after entering unemployment.

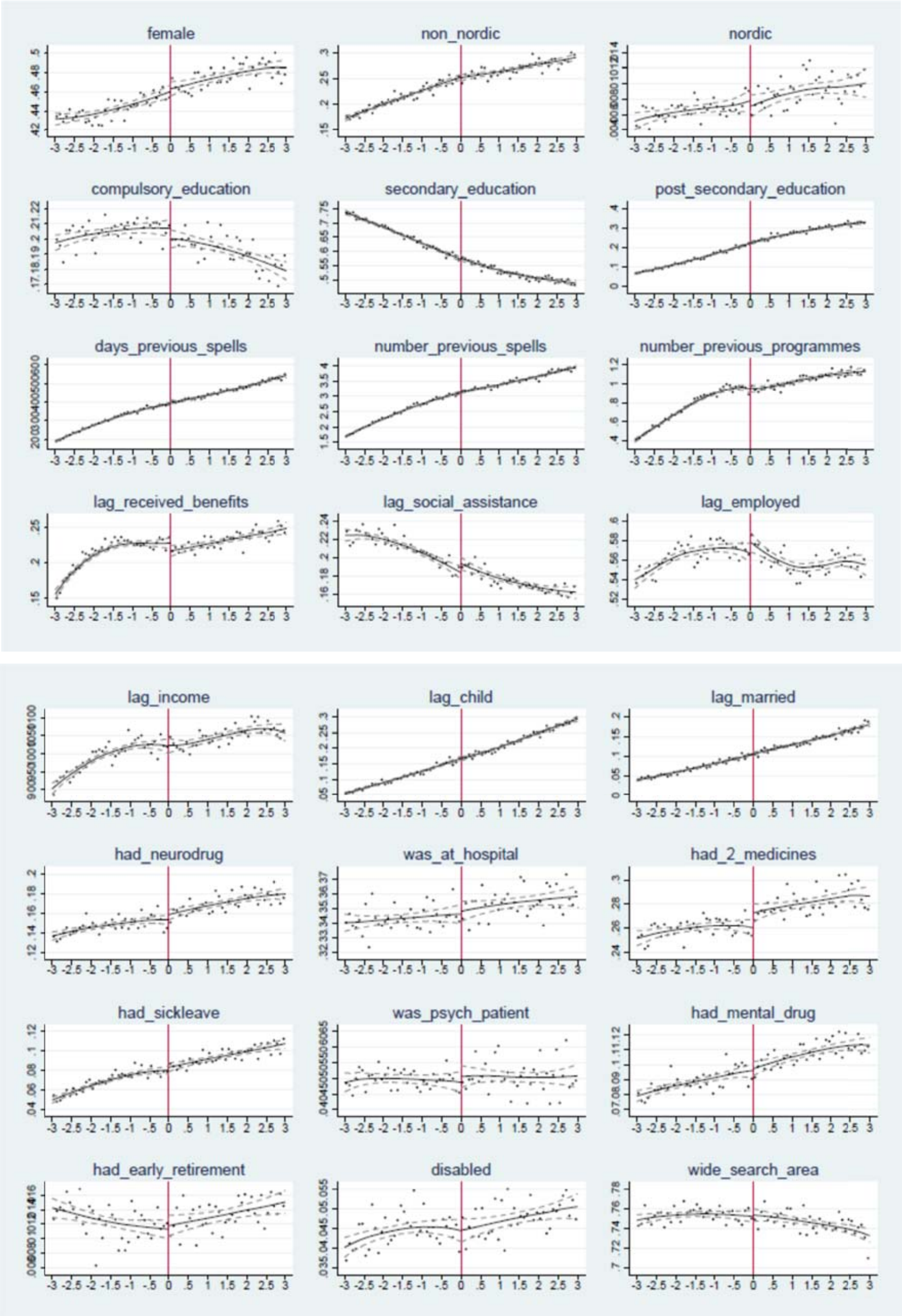

Figure 8: Balance of background variables

Note: Age in years relative to the cut-off age 25 on the x-axis. Age refers to the individual's age 90 days after entering unemployment. All the lagged variables and the sickness variables have been measured in the year prior to start of the unemployment spell. Other variables have been measured upon registration at the public employment service i.e. at the start of the spell. "Nordic" means having being born in another Nordic country (not Sweden). "Non-Nordic" means having being born outside the Nordic countries. "Wide search area" means that the person is interested in jobs within a wider geographical area. Full variable names are given in Table 1 and 2.

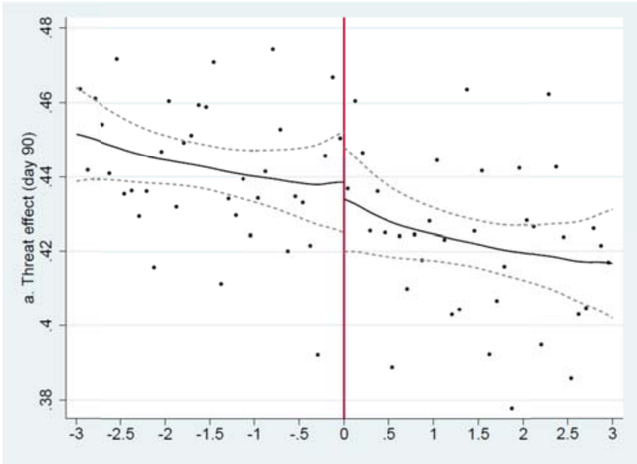

Figure 9: Placebo test: Threat effect in 2007

Note: Age in years relative to the cut-off age 25 on the x-axis and an indicator for becoming employed during the first 90 days of unemployment on the y-axis. Age refers to the individual's age 90 days after entering unemployment.

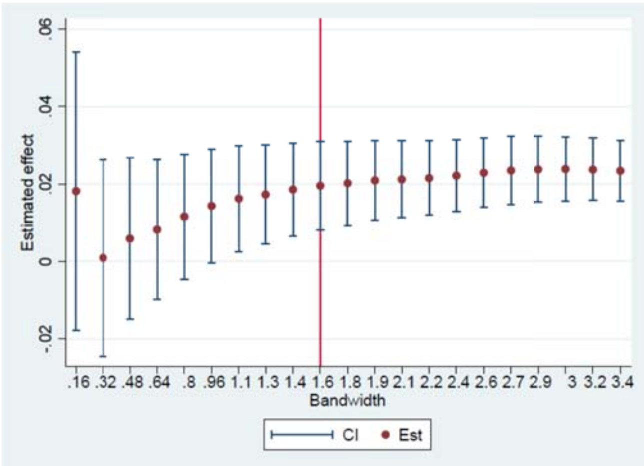

Figure 10: The RD estimate of the threat effect as a function of bandwidth

Note: The vertical line marks the Imbens-Kalyanaraman optimal bandwidth.

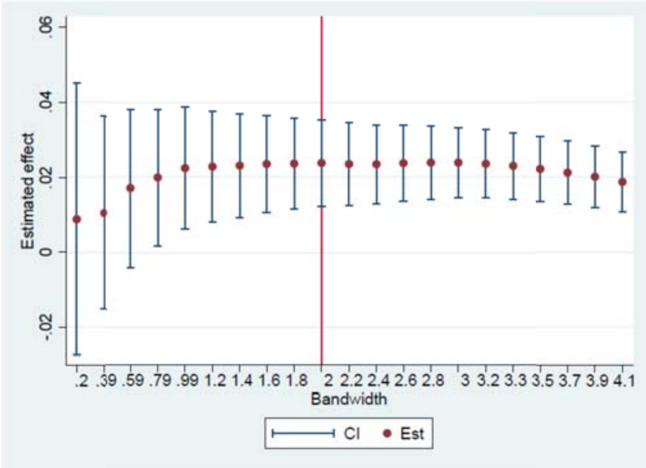

Figure 11: The RD estimate of the effect during day 1-180 as a function of bandwidth

Note: The vertical line marks the Imbens-Kalyanaraman optimal bandwidth.

Tables for the main text

Table 1: Descriptive statistics for our sample

	All			All in the YJG program			24-year-olds			25-year-olds		
Variables	N	Mean	Sd	N	mean	sd	N	mean	sd	N	mean	sd
No. of days in previous unemployment spells	335,521	378.3	441.9	45,765	312.1	310.6	37,796	370.2	369.9	34,777	415.1	420.0
No. of previous spells	335,521	2.921	2.785	45,765	2.341	1.905	37,796	2.940	2.451	34,777	3.240	2.744
No. of previous programs	335,521	0.839	1.688	45,765	1.245	1.615	37,796	0.945	1.642	34,777	0.964	1.747
Age at spellstart+90 days	335,521	25.06	2.698	45,765	22.88	1.160	37,796	24.49	0.289	34,777	25.49	0.289
Country of birth, Non-Nordic	335,521	0.238	0.426	45,765	0.169	0.375	37,796	0.241	0.428	34,777	0.258	0.438
Male	335,521	0.541	0.498	45,765	0.604	0.489	37,796	0.546	0.498	34,777	0.533	0.499
Unemployment benefits, 2007	322,488	0.224	0.417	45,366	0.235	0.424	36,285	0.256	0.436	33,260	0.250	0.433
Married, 2007	322,488	0.105	0.306	45,366	0.0437	0.204	36,285	0.0890	0.285	33,260	0.114	0.317
Social assistance, 2007	322,488	0.206	0.404	45,366	0.213	0.410	36,285	0.208	0.406	33,260	0.198	0.399
Employed, Nov. 2007	322,488	0.570	0.495	45,366	0.580	0.494	36,285	0.593	0.491	33,260	0.587	0.492
Income from work (SEK 100), 2007	322,488	979.7	969.1	45,366	951.0	896.9	36,285	1,011	970.6	33,260	1,026	997.5
Children, 2007	335,521	0.174	0.379	45,765	0.0830	0.276	37,796	0.146	0.353	34,777	0.183	0.386
Compulsory education	313,718	0.333	0.471	44,581	0.334	0.472	35,379	0.314	0.464	32,422	0.315	0.464
Upper secondary education (3 years)	313,718	0.485	0.500	44,581	0.604	0.489	35,379	0.515	0.500	32,422	0.455	0.498
Post-secondary education	313,718	0.182	0.386	44,581	0.0620	0.241	35,379	0.171	0.376	32,422	0.230	0.421

Table 2: Some health indicators, previous year

	Other 24- and 25-year-olds (not unemployed)			All in the YJG program			24-year-olds (in our sample)			25-year-olds (in our sample)		
Variables	N	Mean	Sd	N	mean	sd	N	mean	sd	N	mean	sd
Number of prescriptions	197,333	1.830	2.961	45,765	1.725	2.615	37,796	1.922	2.991	34,777	1.996	3.065
Had drug for neurological condition	197,333	0.119	0.323	45,765	0.129	0.335	37,796	0.153	0.360	34,777	0.164	0.370
Had drug for mental illness[a]	197,333	0.0692	0.254	45,765	0.0709	0.257	37,796	0.0945	0.293	34,777	0.102	0.302
Received sickness benefits	197,333	0.0557	0.229	45,765	0.0644	0.245	37,796	0.0789	0.270	34,777	0.0874	0.282
Received disability pension	197,333	0.00367	0.0605	45,765	0.00548	0.0739	37,796	0.0122	0.110	34,777	0.0123	0.110
Was treated at a hospital[b]	197,333	0.296	0.457	45,765	0.323	0.468	37,796	0.346	0.476	34,777	0.350	0.477
Was a psychiatric patient[b]	197,333	0.0316	0.175	45,765	0.0357	0.185	37,796	0.0492	0.216	34,777	0.0516	0.221

Note: [a]Drugs for mental illness is a subset of neurological drugs. [b]Includes both inpatient and outpatient care.

Table 3: Estimated effects of being eligible for the Youth Job Guarantee Programme (full sample)

	(1) Threat effect	(2) Effect within 180 days	(3) Effect within 270 days	(4) Effect within 365 days
Effect of program eligibility	0.0196***	0.0238***	0.0147**	0.0108
	(0.006)	(0.006)	(0.007)	(0.007)
N within bandwidth	117,202	133,473	87,848	105,595
Bandwidth	1.605	1.970	1.549	2.215
Mean of outcome among 25-year-olds	0.283	0.399	0.470	0.508

Notes: Estimates from local linear regressions using a triangle kernel and optimal bandwidth as defined by Imbens- Kalyanaraman. Standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent level.

Table 4: Relationship between background characteristics and the probability of finding employment within 365 days

	Year 2007
Has not completed upper secondary school	-0.149***
	(0.00332)
Country of birth, non-Nordic	-0.114***
	(0.00349)
Had a neurological drug	-0.0397***
	(0.00484)
Was treated at a hospital	-0.00241
	(0.00312)
Had more than two medicines	0.0240***
	(0.00336)
Received sickness benefits	-0.0110*
	(0.00567)
Was a psychiatric patient	-0.0588***
	(0.00740)
Had a drug for mental illness	-0.0330***
	(0.00686)
Received disability pension	-0.205***
	(0.0100)
Constant	0.118
	(0.113)
N	147,617
R-squared	0.153
Mean of the outcome	0.600

Notes: OLS-estimates. Heteroscedasticity robust standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent level. Other control variables: age and age squared at day 90, gender, post-secondary education, information on education is missing, born in another Nordic country, disability, no. of days in previous unemployment spells, no. of previous unemployment spells, no. of previous employment programs, a wide job search area, has children, lagged unemployment insurance take-up, lagged marital status, lagged social assistance take-up, lagged employment status, lagged income from work, and dummy variables for county and month of spell start.

Table 5: Effects of being eligible for the YJG program, by quartiles of predicted employment probabilities

	(1) Quartile 1	(2) Quartile 2	(3) Quartile 3	(4) Quartile 4
A. Threat effect	0.00843	0.0194**	0.0252**	0.0297***
	(0.007)	(0.010)	(0.010)	(0.010)
N within bandwidth	37,868	37,868	41,629	45,574
Bandwidth	2.101	2.089	2.278	2.368
Mean of outcome among 25-year-olds	0.116	0.258	0.348	0.406
B. Effect within 180 days	0.0153*	0.0220*	0.0227*	0.0280**
	(0.009)	(0.012)	(0.013)	(0.012)
N within bandwidth	34,552	30,021	29,261	31,730
Bandwidth	2.080	1.819	1.780	1.768
Mean of outcome among 25-year-olds	0.170	0.363	0.482	0.568

Notes: Estimates from local linear regressions using a triangle kernel and optimal bandwidth as defined by Imbens-Kalyanaraman. Standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent level.

Table 6: Robustness to adding covariates (full sample)

	Threat effect, with covariates	Days 1-180, with covariates
Effect of program Eligibility	0.0213***	0.0246***
	(0.00553)	(0.00548)
N	335521	312,082
Bandwidth	1.605	1.970
Mean of outcome among 25-year-olds	0.283	0.399

Notes: Estimates from local linear regressions using a triangle kernel and optimal bandwidth as defined by Imbens- Kalyanaraman. Standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent level.

Table 7: Placebo tests, comparing other age groups

	(1) Effect within 90 days	(2) Effect within 180 days	(3) Effect within 270 days	(4) Effect within 365 days
A. 23- vs. 24-year-olds	-0.00548	-0.00530	-0.00183	-0.00512
	(0.006)	(0.006)	(0.007)	(0.008)
N	119,803	120,462	93,004	80,276
Bandwidth	1.506	1.640	1.494	1.549
Mean of outcome among 24-year-olds	0.304	0.417	0.479	0.512
B. 25- vs. 26-year-olds	0.00118	-2.90e-05	-0.00110	-0.00152
	(0.006)	(0.007)	(0.007)	(0.007)
N	111,457	106,081	99,679	104,813
Bandwidth	1.634	1.687	1.890	2.366
Mean of outcome among 26-year-olds	0.283	0.400	0.469	0.511

Notes: Estimates from local linear regressions using a triangle kernel and optimal bandwidth as defined by Imbens- Kalyanaraman. Standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent level.

Table 8: Results using Calonico et al. (2014) robust inference procedure (full sample)

	(1) Threat effect	(2) Effect within 180 days	(3) Effect within 270 days	(4) Effect within 365 days
Effect of programme eligibility	0.0196	0.0238	0.0147	0.0108
Conventional p-value	0.001	0.000	0.047	0.111
Robust p-value	0.082	0.010	0.094	0.184
N within bandwidth	117,202	133,473	87,848	105,595
Bandwidth	1.605	1.970	1.549	2.215
Mean of outcome among 25-year-olds	0.283	0.399	0.470	0.508

Notes: Estimates from local linear regressions using a triangle kernel and optimal bandwidth as defined by Imbens- Kalyanaraman.

Table 9: Robustness to changes in the definition of employment (full sample)

	(1) Baseline estimates (Tab. 3, col. 1)	(2) New Start Jobs are treated as employment
A. Threat effect	0.0196***	0.0200***
	(0.006)	(0.006)
N within bandwidth	117,202	122,106
Bandwidth	1.605	1.670
Mean of outcome among 25- year-olds	0.283	0.282
B. Effect within 180 days	0.0238***	0.0243***
	(0.006)	(0.006)
N within bandwidth	133,473	137,644
Bandwidth	1.970	2.031
Mean of outcome among 25-year-olds	0.399	0.400
C. Effect within 270 days	0.0147**	0.0154**
	(0.007)	(0.007)
N within bandwidth	87,848	86,147
Bandwidth	1.549	1.519
Mean of outcome among 25-year-olds	0.470	0.471
D. Effect within 365 days	0.0108	0.0115*
	(0.007)	(0.007)
N within bandwidth	105,595	108,338
Bandwidth	2.215	2.272
Mean of outcome among 25-year-olds	0.508	0.510

Notes: Estimates from local linear regressions using a triangle kernel and optimal bandwidth as defined by Imbens- Kalyanaraman. Standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent level.

Table 10: Effects by benefit cut

	(1) Benefit cut Threat effect	(2) No benefit cut Threat effect	(3) Benefit cut Effect within 180 days	(4) No benefit cut Effect within 180 days
Effect of programme Eligibility	0.0306**	0.0186***	0.0349**	0.0229***
	(0.014)	(0.006)	(0.016)	(0.006)
N within bandwidth	20,355	110,939	18,602	124,316
Bandwidth	1.811	1.790	1.688	2.179
Mean outcome among 25-year-olds	0.307	0.279	0.474	0.366

Notes: Estimates from local linear regressions using a triangle kernel and optimal bandwidth as defined by Imbens- Kalyanaraman. Standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent level.

Appendix A. Derivation of the results in Section 2.1.

To examine how search effort is affected by activation intensity, and how this effect varies across types, equations (1)-(3) in the main text can be rewritten as

(4) $$(\rho + p_{ue})V^E = h\big((1-\tau)w, 1-l_e\big) + p_{ue}V^U,$$

(5) $$(\rho + \alpha s_u + p_{au})V^U = h\big((1-\tau)b, 1-s_u\big) + \alpha^i s_u V^E + p_{au}V^A$$

(6) $$(\rho + \alpha s_a)V^A = h\big((1-\tau)b, 1-s_a-l_a\big) + \alpha^i s_a V^E.$$

The comparative statics with respect to activation time are

(7) $$\frac{\partial V^A}{\partial l_a} = -h'_F < 0,$$

(8) $$\frac{\partial V^U}{\partial l_a} = \frac{p_{au}}{\rho+\alpha s_u+p_{au}}\frac{\partial V^A}{\partial l_a} < 0,$$

where $-h'_F$ is the derivative of the utility function with respect to leisure time. The individual maximizes his utility by choosing search effort, taking all macro-level variables (such as the job-finding rates α^i) as given. The first-order conditions are

(9) $$h'_F\big((1-\tau)b, 1-s_u\big) = \alpha^i[V^E - V^U],$$

(10) $$h'_F\big((1-\tau)b, 1-s_a-l_a\big) = \alpha^i[V^E - V^A].$$

On the left hand side of equations (9) and (10), one has the marginal cost of search, whereas the right-hand side captures the marginal benefits of search (the product of the job-finding rate and the value of a job).

Search effort while the individual is still in open unemployment is determined by (9). Denote the marginal benefit of search while in open unemployment by $B^U = \alpha^i[V^E - V^U]$. We have

(11) $$\frac{\partial B^U}{\partial l_a} = \frac{\alpha\partial[V^E-V^U]}{\partial l_a} = \alpha^i\left(\frac{p_{ue}}{\rho+p_{ue}} - 1\right)\frac{\partial V^U}{\partial l_a} > 0,$$

where the second equality follows from (4). That is, the marginal benefit of search is increasing in the intensity of activation. Next, let us examine how this effect differs by the job arrival rate α^i. Using (7) and (8), we have

$$\text{(12)} \qquad \frac{\partial B^U}{\partial l_a \partial \alpha} = \left(\frac{p_{ue}}{\rho+p_{ue}} - 1\right)\frac{\partial V^U}{\partial l_a} + \alpha^i\left(\frac{p_{ue}}{\rho+p_{ue}} - 1\right)\frac{\partial}{\partial \alpha}\left(\frac{p_{au}}{\rho+\alpha s_u+p_{au}}\frac{\partial V^A}{\partial l_a}\right)$$
$$= \left(\frac{p_{ue}}{\rho + p_{ue}} - 1\right)\frac{\partial V^U}{\partial l_a} - \alpha^i\left(\frac{p_{ue}}{\rho + p_{ue}} - 1\right)\frac{s_u p_{au}}{(\rho + \alpha s_u + p_{au})^2}\frac{\partial V^A}{\partial l_a}.$$

The first term of this expression is positive, while the second term is negative. To determine the sign of $\frac{\partial B^U}{\partial l_a \partial \alpha}$, again using (8) and combining terms shows that the above expression can be written as

$$\text{(13)} \qquad \frac{\partial B}{\partial l_a \partial \alpha} = \left(\frac{p_{ue}}{\rho+p_{ue}} - 1\right)\frac{p_{au}(\rho+p_{au})}{(\rho+\alpha s_u+p_{au})^2}\frac{\partial V^A}{\partial l_a} > 0.$$

Let us next turn to benefits of search during the actual activation phase. The marginal benefit of search, which we denote by B^A in the case of activation, is then given by the right-hand side of (10). We have that

$$\text{(14)} \qquad \frac{\partial B^A}{\partial l_a} = \frac{\alpha \partial[V^E - V^A]}{\partial l_a} = \alpha^i\left(\frac{p_{ue}}{\rho+p_{ue}}\frac{p_{au}}{\rho+\alpha s_u+p_{au}} - 1\right)\frac{\partial V^A}{\partial l_a} > 0,$$

where the latter equality again follows from (4) and (8). Differentiating the above expression with respect to α yields

$$\text{(15)} \qquad \frac{\partial B^A}{\partial l_a \partial \alpha} = \left(\frac{p_{ue}}{\rho+p_{ue}}\frac{p_{au}}{\rho+\alpha s_u+p_{au}} - 1\right)\frac{\partial V^A}{\partial l_a} - \alpha\frac{(\rho+p_{ue})p_{ue}p_{au}s_u}{(\rho+p_{ue})^2(\rho+\alpha s_u+p_{au})^2}\frac{\partial V^A}{\partial l_a} > 0.$$

Appendix B: Additional tables and figures

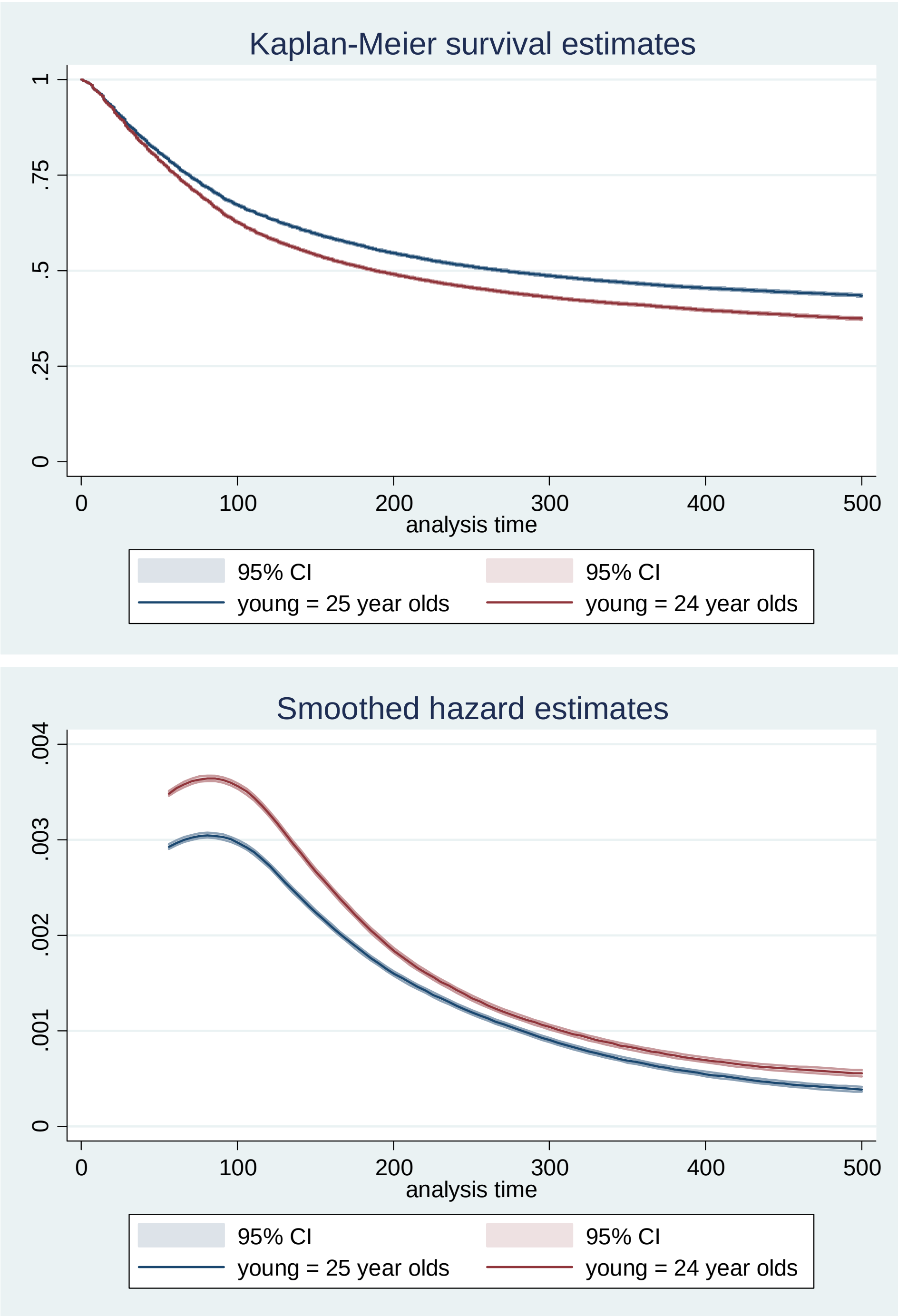

Figure B.1: Kaplan-Meier survival estimates for unemployment duration (upper panel) and smoothed hazard estimates for exits to employment (lower panel) for 24- and 25-year-olds in 2008 - 2009.

Note: The individuals are divided into groups based on their age 90 days after entering unemployment. The sample is limited to 24- and 25-year-olds who are born during the same calendar year.

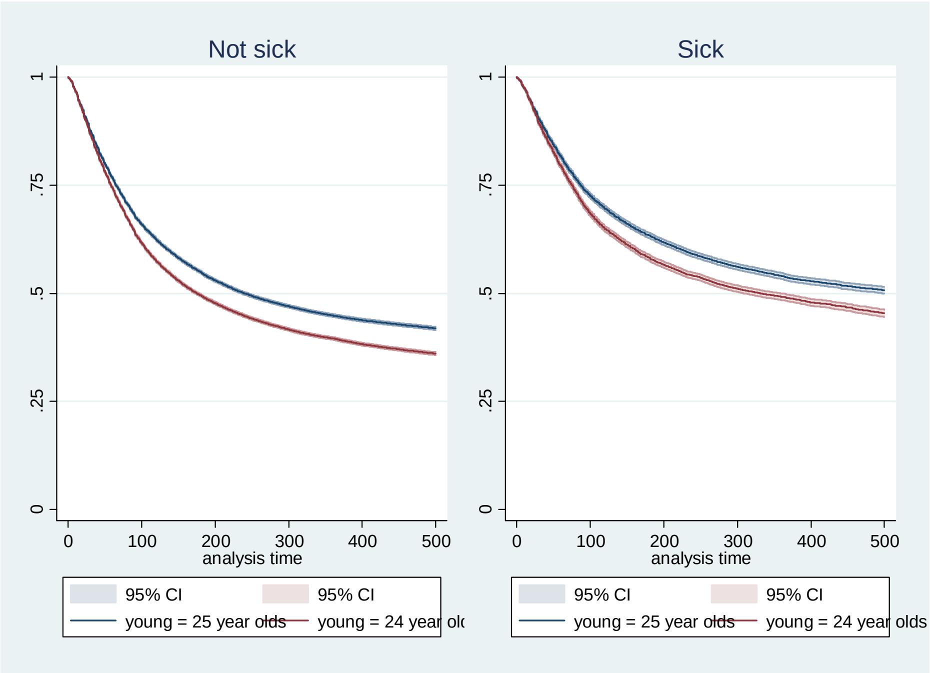

Figure B.2: Kaplan-Meier survival estimates for unemployment duration for individuals who used a neurological drug the previous year (right panel) or did not use such a drug (left panel), 2008 - 2009.

Note: The individuals are divided into groups based on their age 90 days after entering unemployment. The sample is limited to 24- and 25-year-olds who are born during the same calendar year.

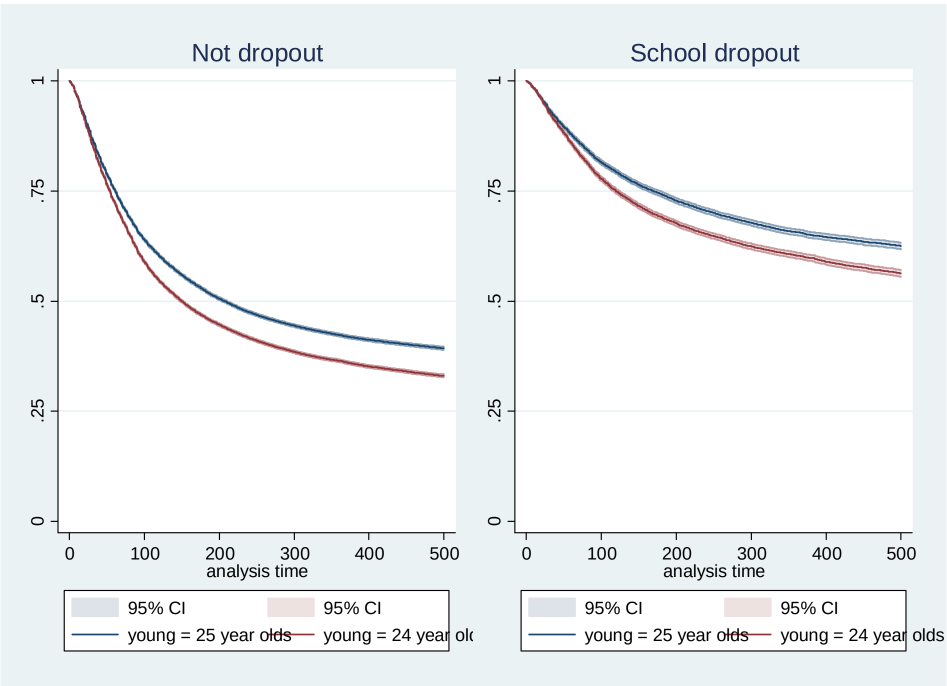

Figure B.3: Kaplan-Meier survival estimates for unemployment duration for school drop-outs (right panel) and others (left panel), 2008 - 2009.

Note: The individuals are divided into groups based on their age 90 days after entering unemployment. The sample is limited to 24- and 25-year-olds who are born during the same calendar year.

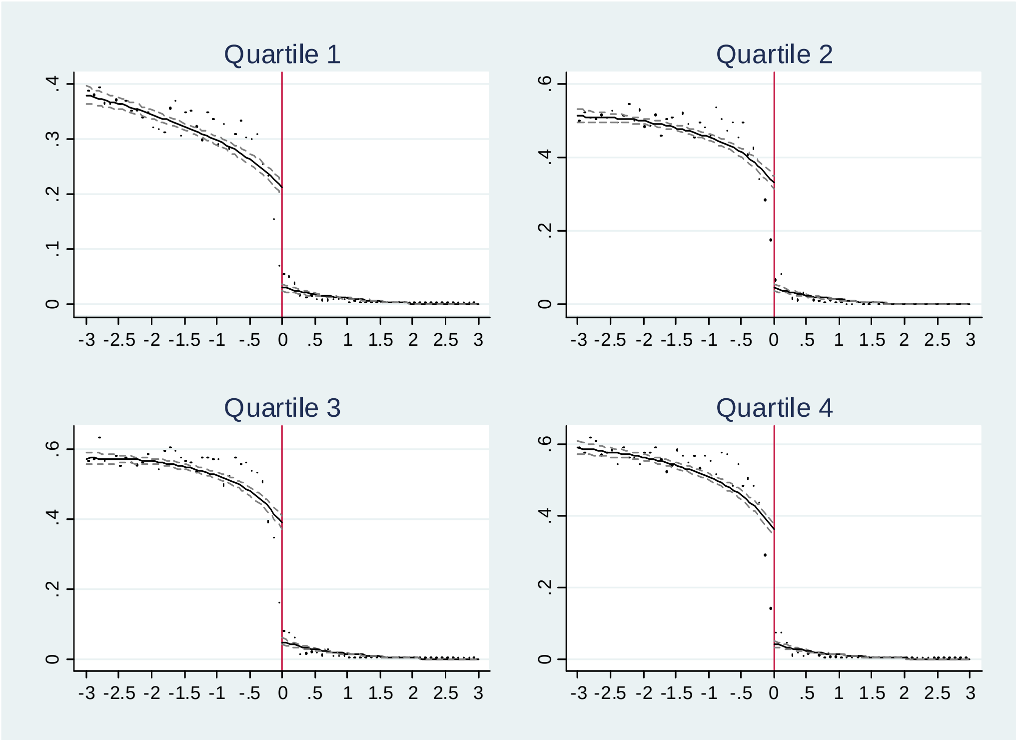

Figure B.4: Youth Job Guarantee take-up by quartiles of predicted employment probabilities (among individuals whose unemployment spell lasted longer than 90 days)

Note: Age in years relative to the cut-off age 25 on the x-axis and an indicator for participating in the programme on the y-axes. Age refers to the individual's age 90 days after entering unemployment.

Table B.1: Characteristics of the unemployed by employment probability quartiles

	(1) Quartile 1	(2) Quartile 2	(3) Quartile 3	(4) Quartile 4
Country of birth, non-Nordic	0.535	0.253	0.123	0.0419
Has not completed upper secondary school	0.519	0.176	0.0631	0.0255
Had a neurological drug	0.246	0.169	0.141	0.0814
Was treated at a hospital	0.411	0.355	0.334	0.292
Had more than two medicines	0.279	0.261	0.274	0.274
Received sickness benefits	0.0740	0.0931	0.0888	0.0633
Was a psychiatric patient	0.117	0.0484	0.0255	0.0101
Had a drug for mental illness	0.175	0.105	0.0750	0.0358
Received disability pension	0.0522	0.00330	0.000417	4.77e-05

Table B.2: Fuzzy RD estimates as a function of bandwidth

	Threat effect		Effect days 1-180	
Percentage of optimal bandwidth	Coef.	Std.Err.	Coef.	Std.Err.
10	5.204332	29.58232	-6.87527	25.17442
20	1.212817	1.830545	0.525073	7.567868
30	0.508782	0.337916	0.330101	0.610102
40	0.346634	0.167781	0.204172	0.231704
50	0.284859	0.108871	0.19177	0.13958
60	0.235573	0.080349	0.183498	0.098318
70	0.206274	0.064248	0.172676	0.075565
80	0.189631	0.054224	0.160803	0.061814
90	0.176193	0.047252	0.156324	0.052678
100	0.166565	0.042047	0.152594	0.046314

Appendix C (online appendix): Robustness of the RD-analysis by quartiles of predicted employment probabilities

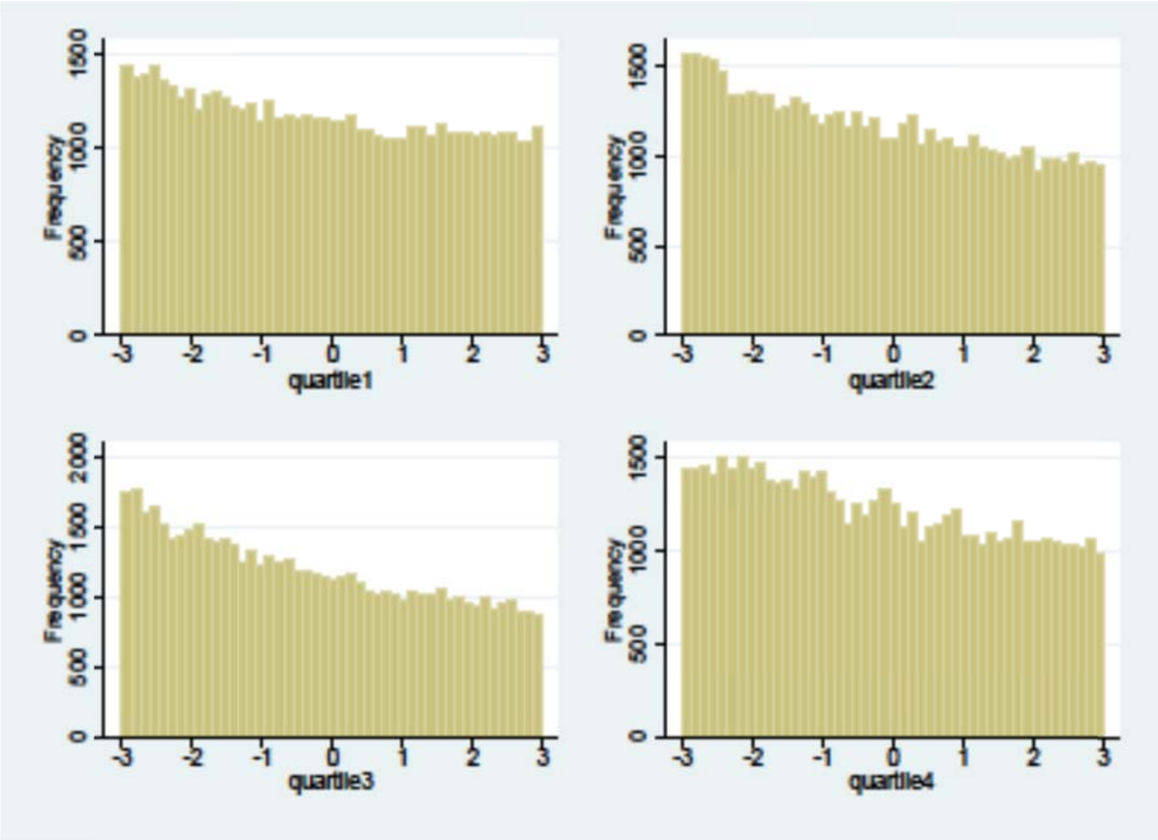

Figure C.1: Number of individuals entering unemployment, by age at day 90 of the unemployment spell

Note: Age in years relative to the cut-off age 25 on the x-axis.

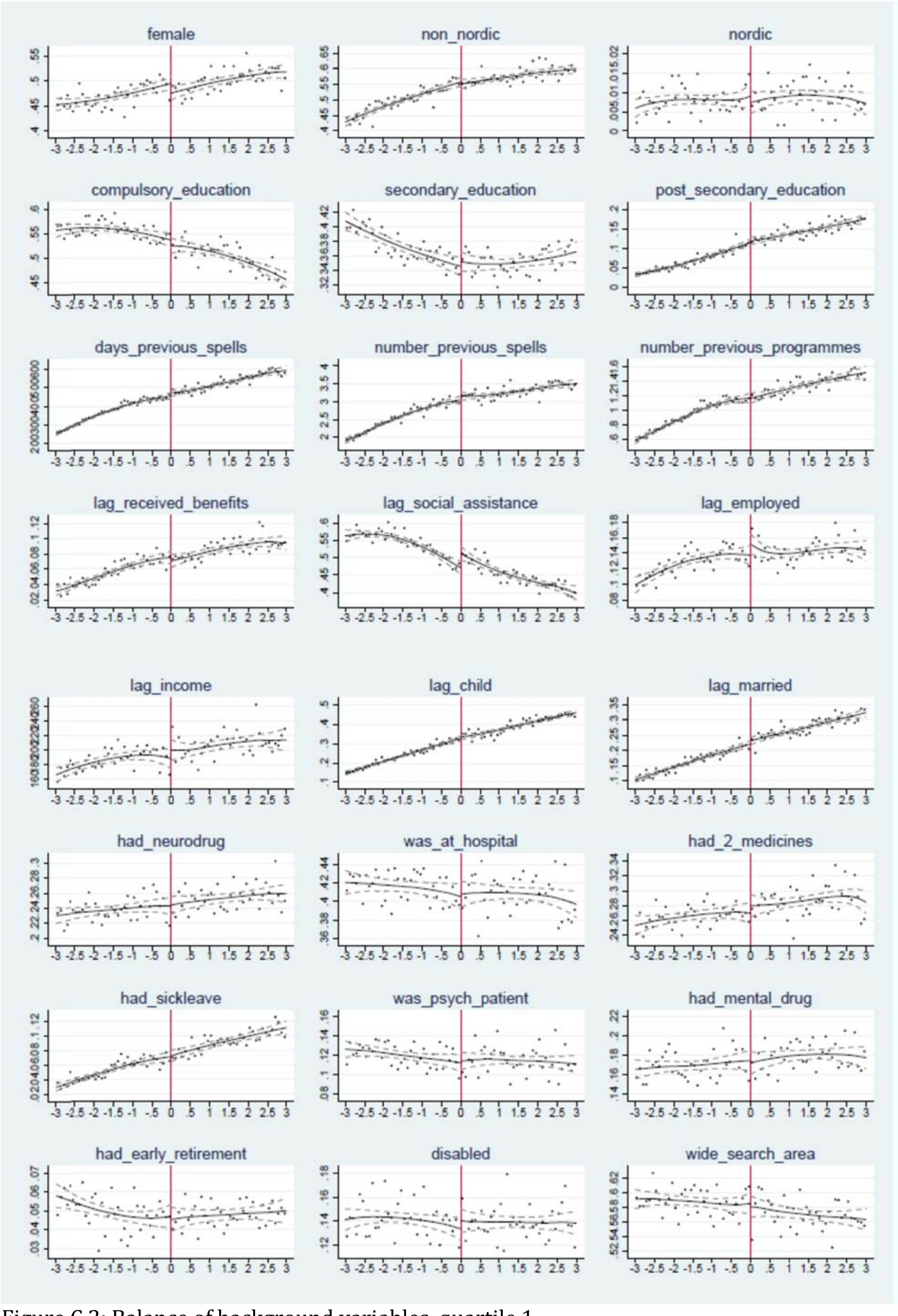

Figure C.2: Balance of background variables, quartile 1

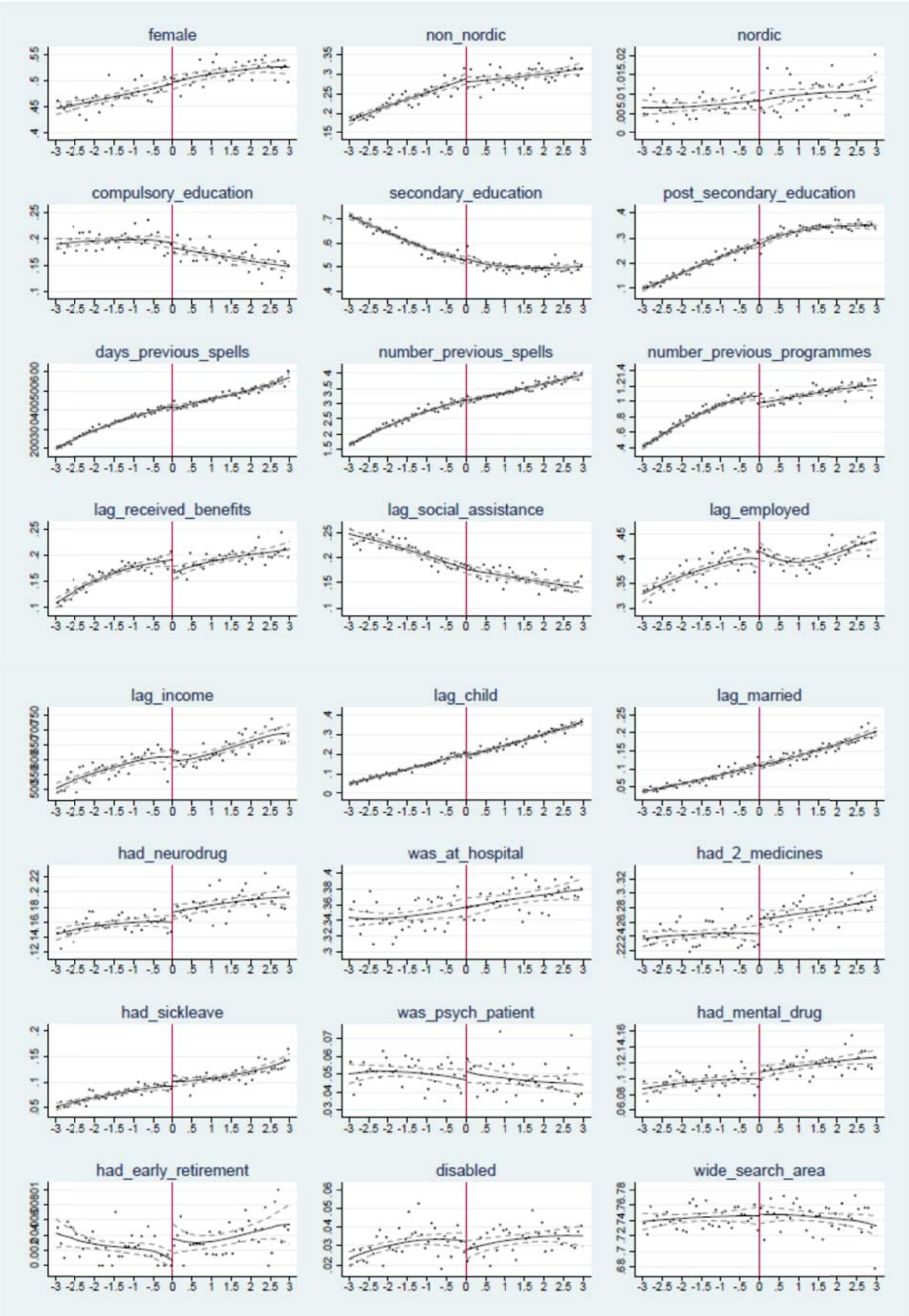

Figure C.3: Balance of background variables, quartile 2

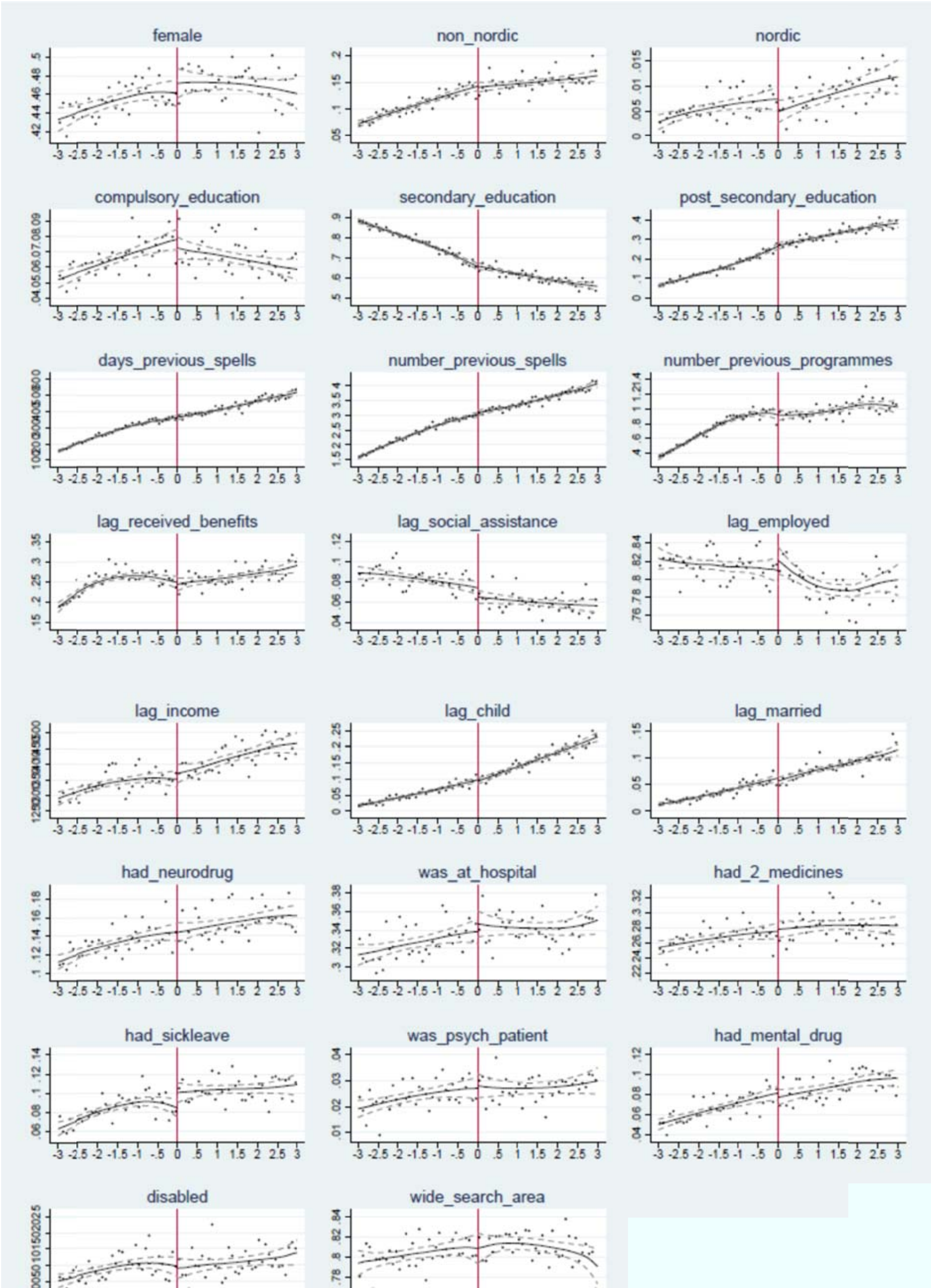

Figure C.4: Balance of background variables, quartile 3.

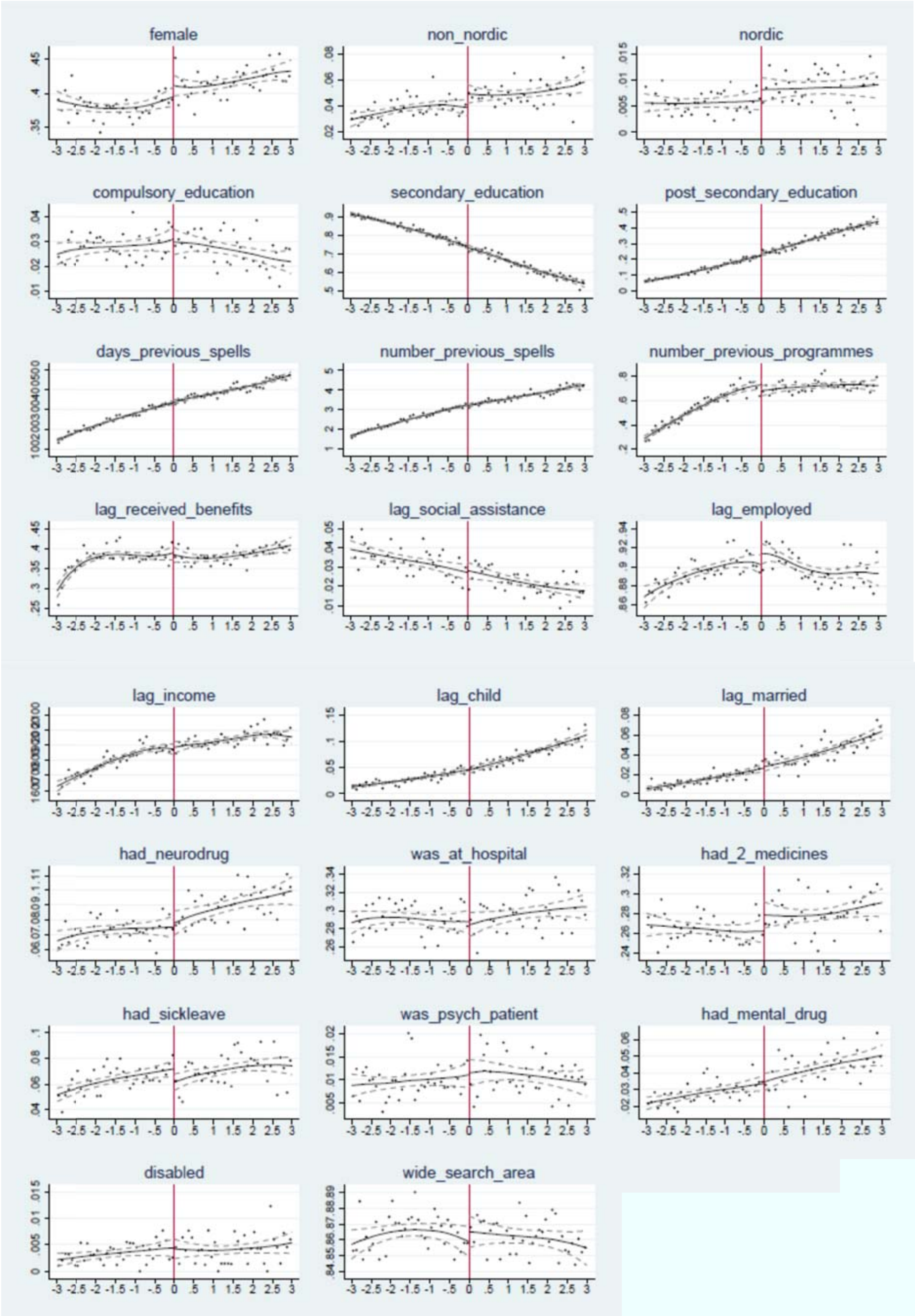

Figure C.5: Balance of background variables, quartile 4.

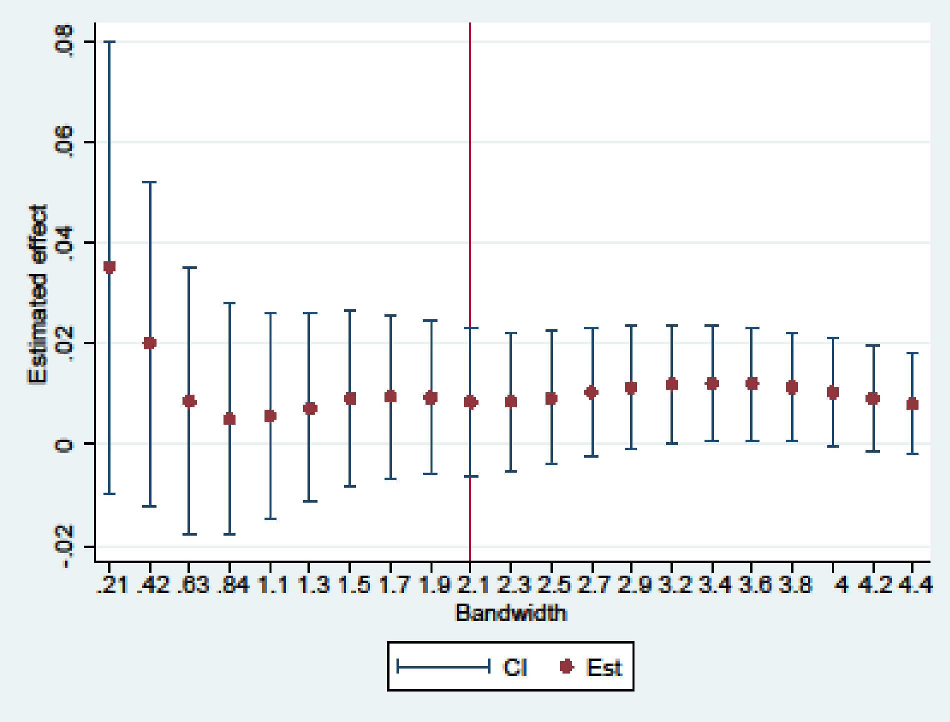

Figure C.6: The RD estimate of the threat effect as a function of bandwidth, quartile 1

Note: The vertical line marks the Imbens-Kalyanaraman optimal bandwidth.

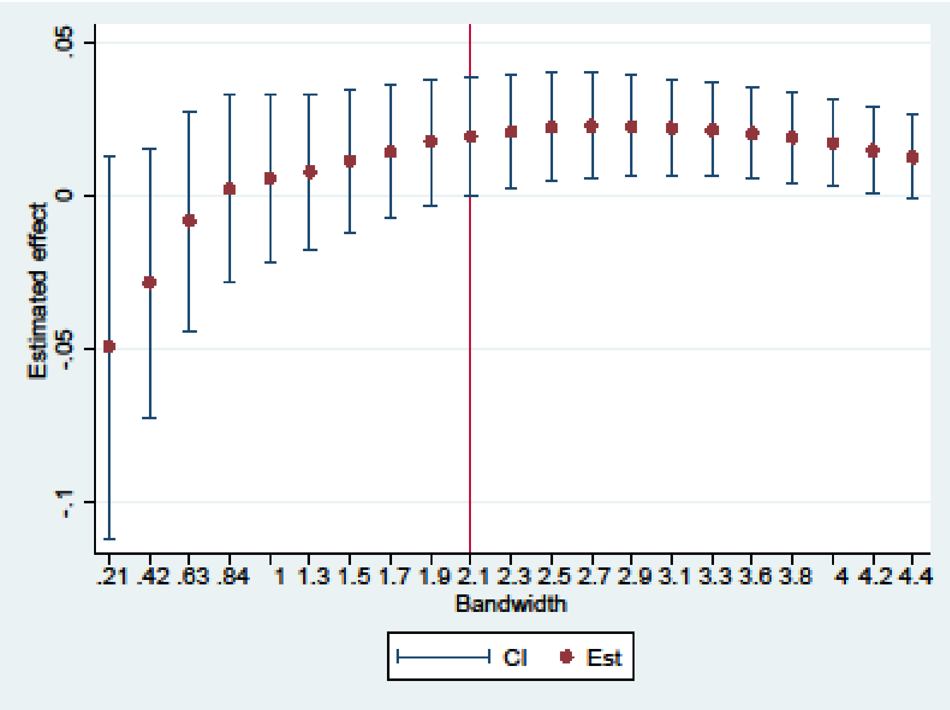

Figure C.7: The RD estimate of the threat effect as a function of bandwidth, quartile 2

Note: The vertical line marks the Imbens-Kalyanaraman optimal bandwidth.

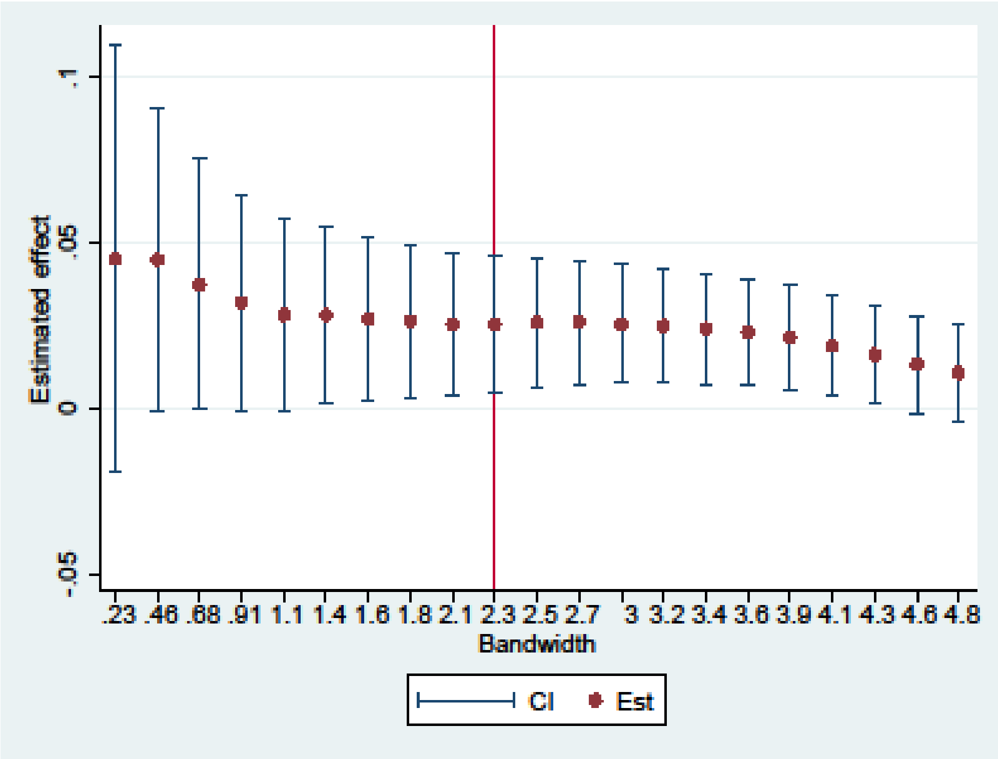

Figure C.8: The RD estimate of the threat effect as a function of bandwidth, quartile 3

Note: The vertical line marks the Imbens-Kalyanaraman optimal bandwidth.

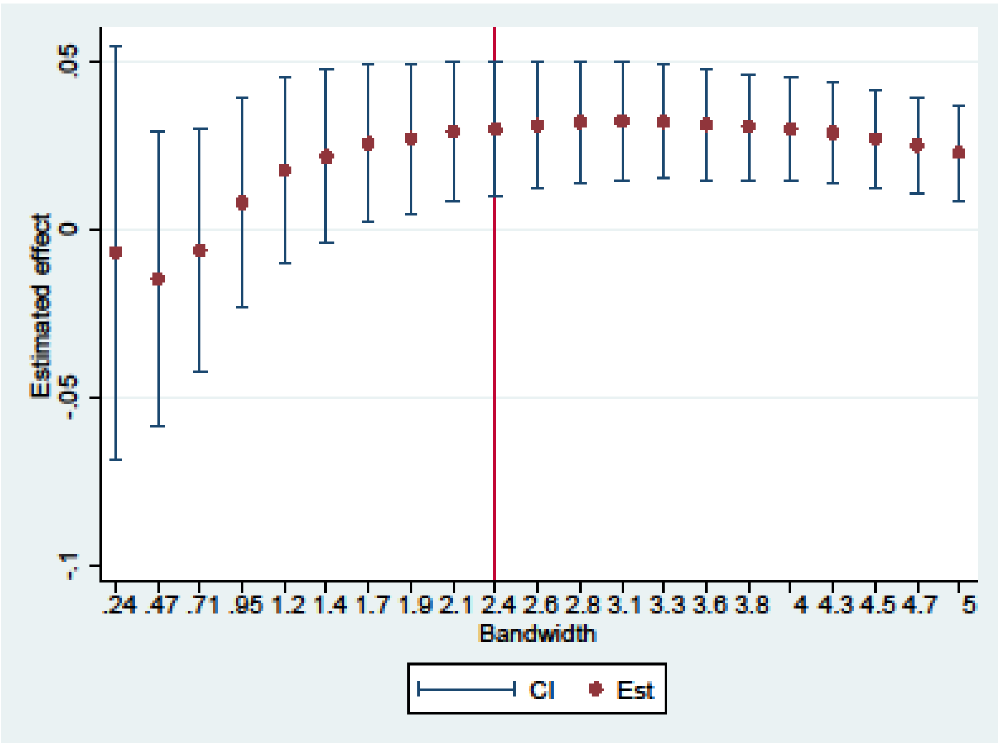

Figure C.9: The RD estimate of the threat effect as a function of bandwidth, quartile 4

Note: The vertical line marks the Imbens-Kalyanaraman optimal bandwidth.

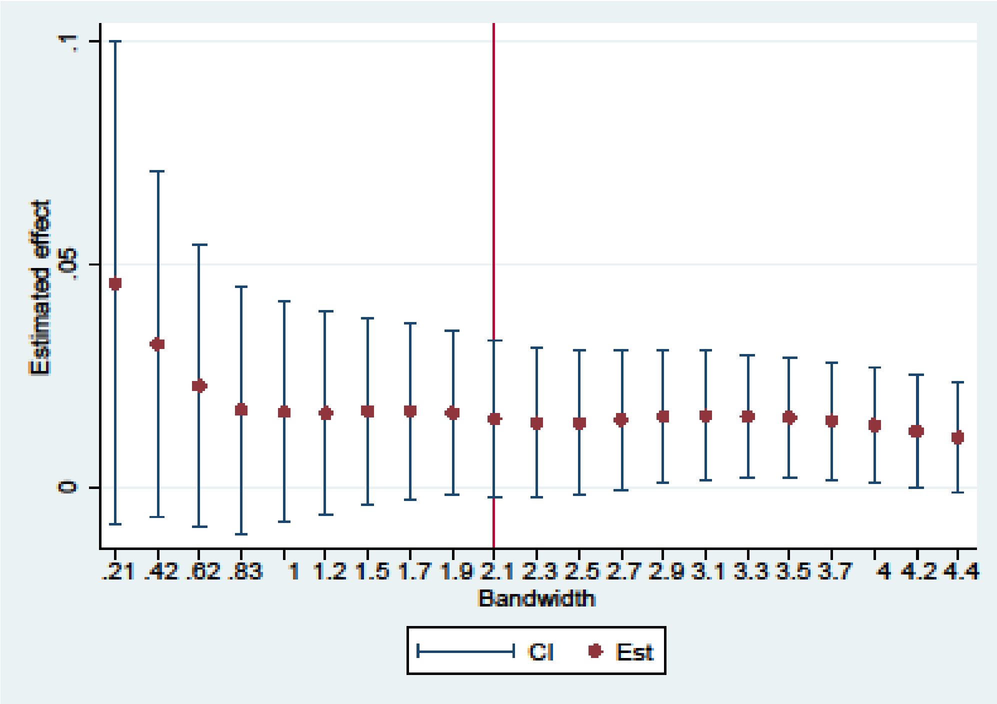

Figure C.10: The RD estimate of the effect during day 1-180 as a function of bandwidth, quartile 1

Note: The vertical line marks the Imbens-Kalyanaraman optimal bandwidth.

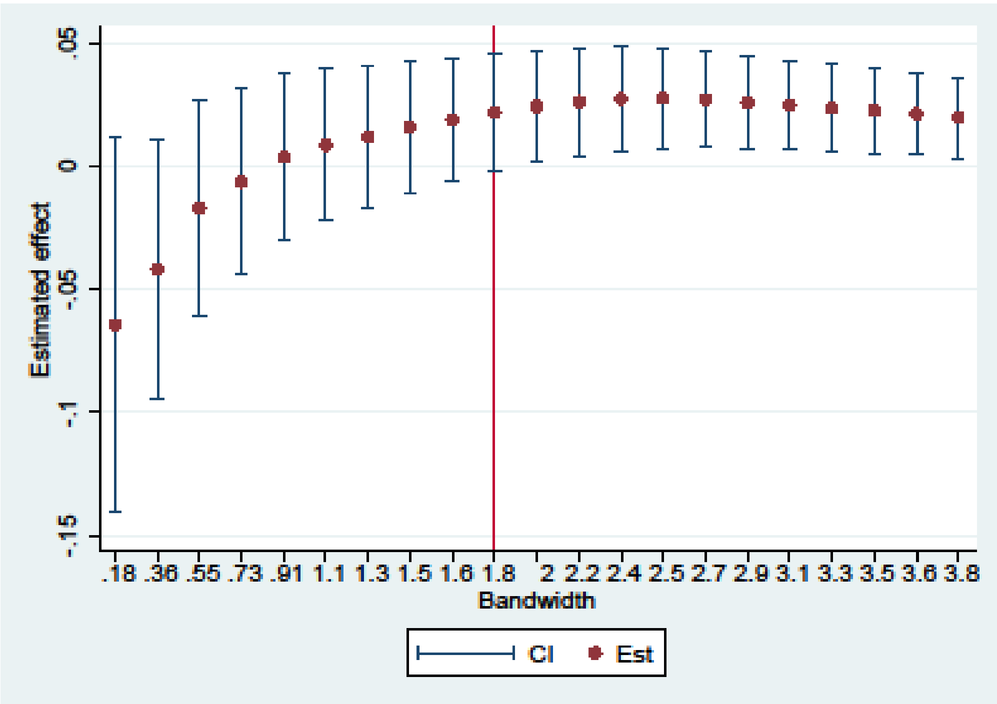

Figure C.11: The RD estimate of the effect during day 1-180 as a function of bandwidth, quartile 2

Note: The vertical line marks the Imbens-Kalyanaraman optimal bandwidth.

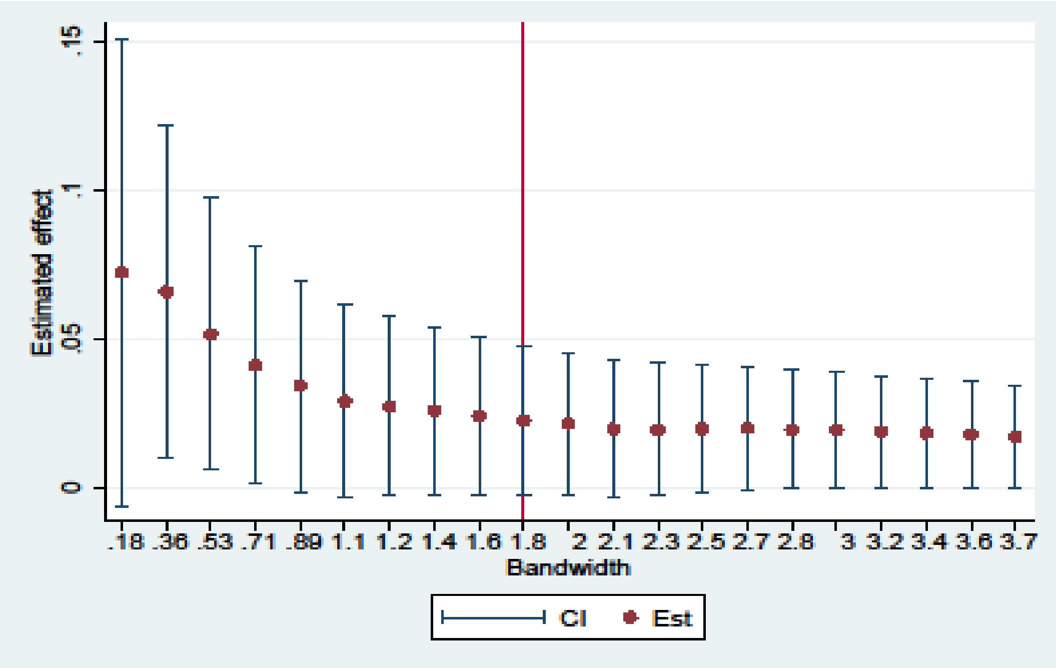

Figure C.12: The RD estimate of the effect during day 1-180 as a function of bandwidth, quartile 3

Note: The vertical line marks the Imbens-Kalyanaraman optimal bandwidth.

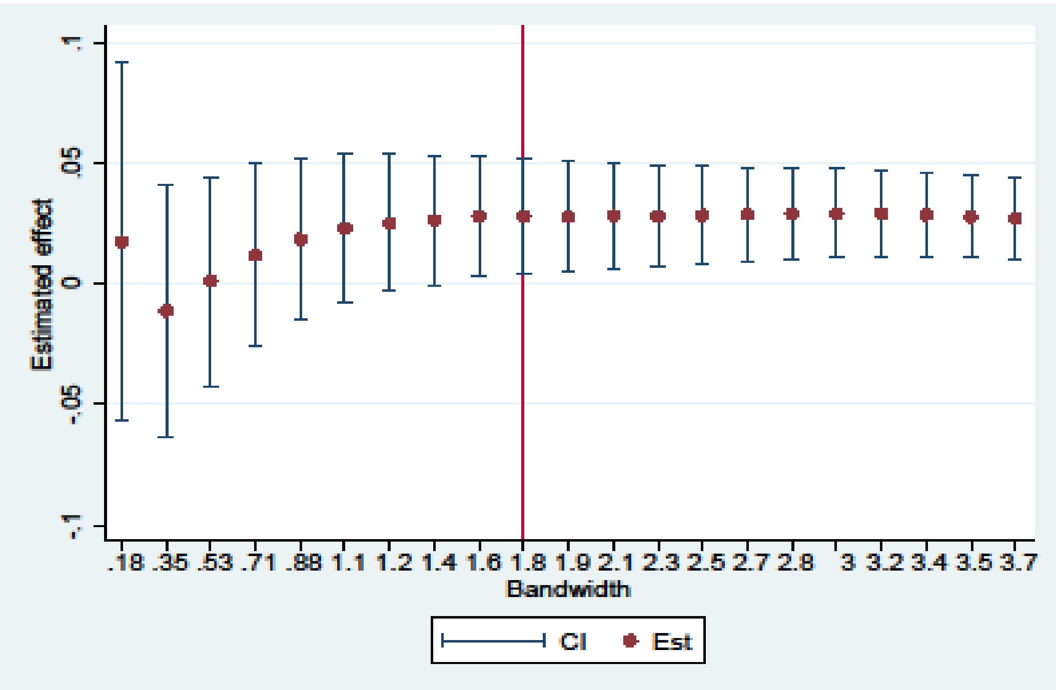

Figure C.13: The RD estimate of the effect during day 1-180 as a function of bandwidth, quartile 4

Note: The vertical line marks the Imbens-Kalyanaraman optimal bandwidth.

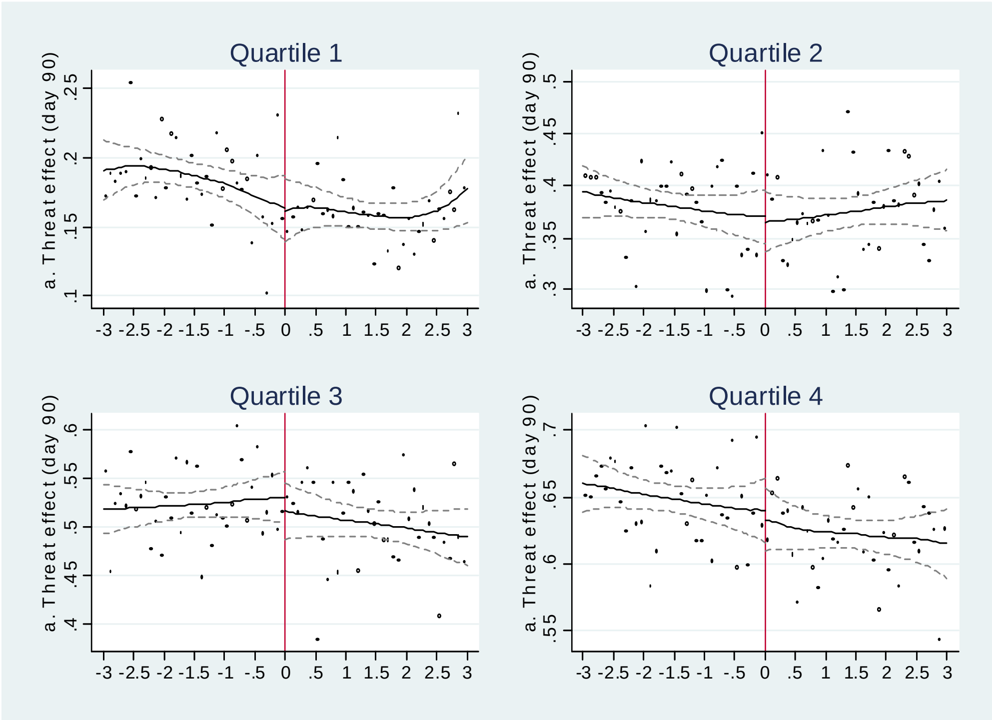

Figure C.14: Placebo tests: Threat effect in 2007, by quartiles

Note: Age in years relative to the cut-off age 25 on the x-axis and an indicator for becoming employed during the first 90 days of unemployment on the y-axis. Age refers to the individual's age 90 days after entering unemployment.

Table C.1: Estimated effects of being eligible for the YJG programme at day 90 and 180, by quartiles. Robustness to adding covariates

	Threat effect, with covariates	Days 1-180, with covariates
A. Quartile 1	0.00728	0.0137
	(0.00723)	(0.00877)
N	83,880	77,763
Bandwidth	2.101	2.080
B. Quartile 2	0.0239*	0.0263*
	(0.010)	(0.0118)
N	83,880	77,505
Bandwidth	2.089	1.819
C. Quartile 3	0.0313**	0.0287*
	(0.0102)	(0.0124)
N	83,880	77,153
Bandwidth	2.278	1.780
D. Quartile 4	0.0293**	0.0263*
	(0.00990)	(0.0118)
N	83,879	79,659
Bandwidth	2.368	1.768

Notes: Estimates from local linear regressions using a triangle kernel and optimal bandwidth as defined by Imbens-Kalyanaraman. Standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent level.

Table C.2: Placebo tests of turning 24, by quartiles

	(1) Effect within 90 days	(2) Effect within 180 days
A. Quartile 1		
23- vs. 24-year-olds	-0.00502	0.00165
	(0.008)	(0.010)
N	33,395	29,957
Bandwidth	1.755	1.702
B. Quartile 2		
23- vs. 24-year-olds	7.22e-05	-0.00693
	(0.011)	(0.012)
N	35,981	33,388
Bandwidth	1.819	1.848
C. Quartile 3		
23- vs. 24-year-olds	-0.0158	-0.0240**
	(0.012)	(0.011)
N	30,752	41,238
Bandwidth	1.520	2.204
D. Quartile 4		
23- vs. 24-year-olds	-0.0152	-0.00752
	(0.011)	(0.011)
N	39,447	36,260
Bandwidth	1.914	1.857

Notes: Estimates from local linear regressions using a triangle kernel and optimal bandwidth as defined by Imbens- Kalyanaraman. Standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent level.

Table C.3: Placebo test of turning 26, by quartiles

	(1) Effect within 90 days	(2) Effect within 180 days
A. Quartile 1		
25- vs. 26-year-olds	0.00103	-0.000107
	(0.007)	(0.009)
N	38,590	35,672
Bandwidth	2.224	2.230
B. Quartile 2		
25- vs. 26-year-olds	-0.0202*	-0.0201*
	(0.010)	(0.012)
N	34,263	31,016
Bandwidth	2.032	2.005
C. Quartile 3		
25- vs. 26-year-olds	-0.00107	0.00865
	(0.012)	(0.012)
N	31,612	34,946
Bandwidth	1.923	2.322
D. Quartile 4		
25- vs. 26-year-olds	0.00752	-0.00680
	(0.012)	(0.012)
N	31,375	29,588
Bandwidth	1.777	2.203

Notes: Estimates from local linear regressions using a triangle kernel and optimal bandwidth as defined by Imbens- Kalyanaraman. Standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent level.

Table C.4: Results using Calonico et al. (2014) robust inference procedure, by quartiles

	(1) Quartile 1	(2) Quartile 2	(3) Quartile 3	(4) Quartile 4
Effect within 90 days	0.00843	0.0194	0.0252	0.0297
Conventional p-value	0.253	0.050	0.015	0.003
Robust p-value	0.404	0.976	0.062	0.236
N within bandwidth	37868	37868	41629	45574
Bandwidth	2.101	2.089	2.278	2.368

	(1) Quartile 1	(2) Quartile 2	(3) Quartile 3	(4) Quartile 4
Effect within 180 days	0.0153	0.0220	0.0227	0.0280
Conventional p-value	0.089	0.070	0.077	0.022
Robust p-value	0.111	0.570	0.072	0.087
N within bandwidth	34552	30021	29261	31730
Bandwidth	2.080	1.819	1.780	1.768

Notes: Estimates from local linear regressions using a triangle kernel and optimal bandwidth as defined by Imbens- Kalyanaraman.

Table C.5: Robustness to changes in the definition of employment, by quartiles.

	(1) Threat effect	(2) Effect within 180 days
A. Quartile 1		
Baseline estimates (Table 5, Col. 1)	0.00843	0.0153*
	(0.007)	(0.009)
Estimates when New Start Jobs are treated as employment	0.00850	0.0155*
	(0.008)	(0.009)
N within bandwidth	36,630	35,757
Bandwidth	2.036	2.149
B. Quartile 2		
Baseline estimates (Table 5, Col. 2)	0.0194**	0.0220*
	(0.010)	(0.012)
Estimates when New Start Jobs are treated as employment	0.0188*	0.0200
	(0.010)	(0.012)
N within bandwidth	38,492	29,979
Bandwidth	2.125	1.817
C. Quartile 3		
Baseline estimates (Table 5, Col. 3)	0.0252**	0.0227*
	(0.010)	(0.013)
Estimates when New Start Jobs are treated as employment	0.0242**	0.0230*
	(0.010)	(0.012)
N within bandwidth	41,135	31,013
Bandwidth	2.251	1.883
D. Quartile 4		
Baseline estimates (Table 5, Col. 4)	0.0297***	0.0280**
	(0.010)	(0.012)
Estimates when New Start Jobs are treated as employment	0.0312***	0.0307**
	(0.010)	(0.012)
N within bandwidth	44,102	31,176
Bandwidth	2.295	1.735

Notes: Estimates from local linear regressions using a triangle kernel and optimal bandwidth as defined by Imbens- Kalyanaraman. Standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent level.

Table C.6: Effects of being eligible for the YJG programme by quartiles of employment probabilities for those who faced no benefit cut

	(1) Quartile 1	(2) Quartile 2	(3) Quartile 3	(4) Quartile 4
A. Threat effect	0.00692	0.0217**	0.0281**	0.0187
	(0.008)	(0.010)	(0.012)	(0.013)
N within bandwidth	34464	34148	32197	26391
Bandwidth	1.958	2.062	2.233	1.927
B. Effect in 180 days	0.0141	0.0245*	0.0302**	0.0135
	(0.009)	(0.013)	(0.014)	(0.013)
N within bandwidth	33814	28270	24480	26301
Bandwidth	2.080	1.887	1.921	2.059

Notes: Estimates from local linear regressions using a triangle kernel and optimal bandwidth as defined by Imbens- Kalyanaraman. Standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent level.

Table C.7. Effects of being eligible for the YJG programme by quartiles of employment probabilities, controlling for benefit cut

	Threat effect, no covar. (Table 5)	Threat effect, controlling for benefit cut	Days 1-180, no covar. (Table 5)	Days 1-180, controlling for benefit cut
A. Quartile 1	0.00843	0.00820	0.0153*	0.0147
	(0.007)	(0.007)	(0.009)	(0.009)
N	83880	83880	77763	77763
Bandwidth	2.101	2.101	2.080	2.080
B. Quartile 2	0.0194**	0.0233**	0.0220*	0.0259**
	(0.010)	(0.010)	(0.012)	(0.012)
N	83880	83880	77505	77505
Bandwidth	2.089	2.089	1.819	1.819
C. Quartile 3	0.0252**	0.0317***	0.0227*	0.0281**
	(0.010)	(0.010)	(0.013)	(0.013)
N	83880	83880	77153	77153
Bandwidth	2.278	2.278	1.780	1.780
D. Quartile 4	0.0297***	0.0296***	0.0280**	0.0268**
	(0.010)	(0.010)	(0.012)	(0.012)
N	83879	83879	79659	79659
Bandwidth	2.368	2.368	1.768	1.768

Notes: Estimates from local linear regressions using a triangle kernel and optimal bandwidth as defined by Imbens- Kalyanaraman. Std errors in parentheses. */**/*** denotes significance at the 10/5/1 percent level.