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INSTITUTE FOR EVALUATION OF LABOUR MARKET AND EDUCATION POLICY

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The health effects of a youth labor market activation policy[a]

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Abstract

We examine the health effects of a labor market activation policy, the Youth Job Guarantee, implemented in Sweden in 2007. To estimate the causal effects of this policy on health, we implement an RD-design using the age-eligibility threshold of the policy, together with detailed administrative data on health outcomes including measures of mental health. Health effects could arise indirectly via effects on employment, or directly, e.g., via an improved daily routine. In contrast to most of the existing literature on the health effects of ALMPs, our results indicate that the activation policy did not have clear positive effects on health one year after the start of the unemployment spell, measured by prescribed medication or healthcare visits.

Keywords: Labor market programs, activation, youth unemployment, mental health
JEL-codes: J68, I12, I18, H51

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1 Introduction

Over the past few decades, several countries have witnessed a rise in mental health problems among young adults and adolescents (see, e.g., Blanchflower et al. 2024; Krokstad et al. 2022; Twenge et al. 2019; Public Health Agency of Sweden 2018; Bor et al. 2014). In many OECD countries, an increasing proportion of sick leave and disability benefit claims of young adults is today attributed to mental illness (Hemmings and Prinz 2020). Mental health problems tend to be especially prevalent among young adults who lack employment (see, e.g., Hall et al. 2022; Strandh et al. 2014; Reneflot and Evensen 2014; Fergusson et al. 1997).[1] Furthermore, economic hardship in early adulthood has been shown to have negative long-term consequences (e.g. Oreopoulos et al. 2012). Active labor market programs (ALMPs) aim to improve individuals' chances of finding a job[2], but it has been suggested that they could also influence participants' well-being more broadly, including health outcomes. Despite several existing studies on the health impact of ALMPs, few have relied on convincing identification strategies, making it hard to know whether the estimates can be given a causal interpretation, and few have focused explicitly on youth.

What are the potential pathways through which activation could affect health? First, if activation succeeds in reducing the time spent in unemployment, the individual's health may improve because of a positive impact of employment – as opposed to unemployment – on health. Second, activation may have direct positive health impacts beyond those related to employment, as discussed by, e.g., Coutts et al. (2014). They note that activation provides social support and time structure, which could be absent during unemployment. ALMPs may also strengthen job seekers' self-efficacy, self-esteem, and other skills as well as help maintain social contacts. These factors may lead to improved mental health and psychosocial functioning among ALMP participants. However, negative health effects are also possible, e.g., if the activities are perceived as stressful, or through the stress of losing benefits if not participating (Bastiaans et al. 2023).

In this study, we examine the health impact of a major, nationwide youth activation program (the Youth Job Guarantee) that was introduced in Sweden in 2007. The focus of the program is on activities related to job search, but it can also involve short periods of training or work placement to gain work experience. We use data for the entire Swedish population covering the universe of unemployment spells combined with administrative data on prescribed medication, hospital admissions and medical contacts in specialized care. These data allow us to build objective, carefully selected measures of individual health that distinguish mental health problems from other health issues.

[1] This may partly be due to worse labor market prospects among individuals with poor mental health (as is shown in e.g. Hall et al. 2022), but several papers also convincingly show that unemployment has a causal negative impact on individuals' mental as well as physical health (e.g., Eliason and Storrie 2009; Browning and Heinesen 2012; Sullivan and von Wachter 2009).

[2] There is an extensive literature on the impact of different types of ALMPs on labor market related outcomes; see e.g. Card et al. (2018) and Kluve et al. (2019) for reviews.

We use a regression discontinuity (RD) design to estimate the effects of activation through the YJG program, using the fact that only individuals under 25 years of age are eligible for the program. Individuals under 25 years old are eligible if they have been unemployed for more than 90 days. Thus, our empirical strategy is essentially to compare how health outcomes develop among individuals who have just turned 25 before 90 days of unemployment (ineligible) to the same outcomes among those who are just below age 25 at 90 days of unemployment (eligible). In Hall et al. (2022), we used the same identification strategy to examine employment outcomes. We analyze the effect of program eligibility on the use of prescribed medication, hospital admissions, and medical contacts in specialized care, for reasons related to mental health or other health problems.

We find that activation did not have an impact on the use of prescribed medication or healthcare visits overall. Our results suggest that activation within the program also did not affect the use of medication to treat mental health problems. However, there is weak evidence, which varies by specification, that there may have been some modest, short-term effects when it comes to reducing the likelihood of healthcare visits related to mental health issues.

Turning to the mechanisms behind potential health gains, the employment effects of the program are modest and short-lived (Hall et al. 2022), a finding confirmed by our study. Therefore, any potential indirect health effects, arising through improved employment outcomes, would necessarily also be short-lived. This is one potential reason why we do not find evidence of beneficial health effects in the longer run. Our results further indicate that the direct health effects from activation are small as well.

The lack of clear health effects means that our findings differ from those of most previous studies on this topic, which generally have concluded that activation improves individuals' health and well-being (see the review by Puig-Barrachina et al. 2020). We discuss the relationship to earlier literature at length in Section 6, where a key takeaway is that findings from the earlier research may not fully generalize to other empirical settings and yet unstudied population groups of interest. We contribute to the earlier literature (e.g. Puig-Barrachina et al. 2020; Rose 2019; Caliendo et al. 2022) by providing an evaluation of a major nationwide activation program with clearly defined content, and focusing on youth, for whom mental health problems are strongly associated with poor labor market outcomes, and where successful interventions may have large returns, e.g., in terms of preventing economic hardship in the longer run. Further, we contribute by using a credible identification strategy based on an RD-setting and utilizing full-population administrative (as opposed to self-reported) health data, which further allows us to separate effects on mental health and other health outcomes. Some earlier studies, such as Caliendo et al. (2022) and Rose (2019), also use convincing empirical designs to examine health effects of ALMPs, but focus on other types of interventions (training and subsidized employment), and not on youth. There also exist a few RCTs on the impact of activation on self-reported health (e.g., Caplan et al. 1989; Vuori et al. 2002). These studies evaluate fairly small-scale interventions that include psychology-based coaching components rarely offered in typical active labor market programs. It is not clear whether similar, equally well-designed interventions are feasible at scale.

The paper proceeds as follows. Section 2 describes the labor market program studied as well as the Swedish healthcare system. The data is presented in Section 3, while Section 4 describes the empirical methodology. The results are presented in Section 5, where we also conduct a large battery of RD validity and robustness checks. We compare the results with the findings in the earlier literature in Section 6, where earlier work is also discussed in more detail. Section 7 concludes.

2 Institutional Background

2.1 The Youth Activation Program[3]

The activation program we study is the Youth Job Guarantee (YJG), which started in Sweden in December 2007. The program involves activation that starts 90 days after a person has registered as an unemployed job seeker at the Public Employment Service (PES), 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 to the program after 90 days of unemployment.[4] 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 benefits. If assigned to the program, the individual needs to participate until he/she finds a job or enrolls in education; i.e., individuals who are already in the program are not allowed to drop out when they turn 25. The maximum duration in the program is 15 months. Individuals who are still unemployed after 15 months are transferred to another activation program (the Job and Development Guarantee), which is targeted at long-term unemployed of all ages.

Figure 1 illustrates the structure of the program. The first three months (90 days) of an unemployment spell consist of open unemployment. After 90 days, the PES 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 program 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 content of the program is relatively flexible and should be tailored to the individual's specific needs.

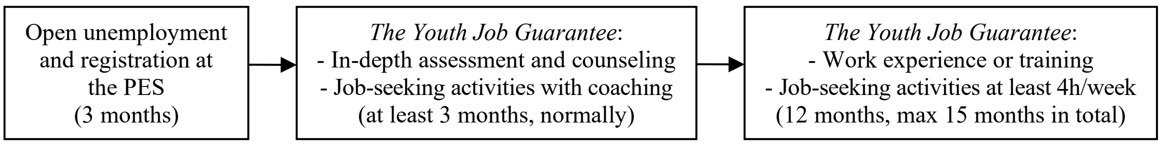

Figure 1. The Youth Job Guarantee Program

[3] This section draws heavily on Hall et al. (2022).

[4] Some rules of the program have changed over time. We describe the rules in place during the time period we study, that is 2008–2014.

The activities within the YJG program are supposed to imply full-time participation. However, based on a survey among participants in 2009, Martinson and Sibbmark (2010) conclude that this ambition is rarely met in practice. On average the participants reported that they in total spent 14 hours per week applying for jobs and participating in activities. The most common type of organized activities the participants were involved in included job-seeking activities with coaching, work placement, and study and career guidance.

For some unemployed individuals, program participation also affects the time-profile of the replacement rate in the unemployment insurance (UI): They receive a slightly lower replacement rate upon participation in the YJG, and the policy thus involves elements of both activation and financial incentives. However, Hall et al. (2022) show that this only concerns around 10 percent of the target group due to several exemptions.[5] Individuals who are not entitled to any UI benefits (because they do not fulfill the working condition[6]) receive a small monetary compensation if participating in the program (SEK 135 [EUR 11.5] per day for 2010).

2.2 The Healthcare System

In Sweden, healthcare is organized and financed by the public sector. The 21 regions are responsible for organizing healthcare services and all residents must be given equal access to quality care. The healthcare system is primarily financed by taxes raised from the residents in each region. The actual provider of healthcare can be either public or private. However, most private healthcare providers have signed agreements with the regions, which means they are entitled to the same compensation as public providers, and patients will pay the same fee irrespective of visiting a public or private healthcare provider. The regions are free to decide on patient fees, but a national cap on co-payments limits the individual's expenses to a maximum of SEK 1,150 (98 EUR) per 12 months.[7] The patient fee for a visit in primary care in 2021 amounted to SEK 200 (EUR 17) in most regions.

As in most other countries, certain medication can only be dispensed if prescribed by a physician while other drugs can be bought over the counter. Drugs for mental health problems are examples of medicines that require a prescription. Prescription drugs are subsidized by the state[8] and, like for patient fees, there is a national cap on the individual's yearly expenses. The individual will pay the full price for purchases up to SEK 1,175 (100 EUR), thereafter the subsidy system incrementally reduces the costs: the subsidy first amounts to 50 percent of the costs, then increases to 75 percent, and finally to 90

[5] An individual will be unaffected by the faster reduction of benefits if he/she (i) is only eligible for the basic UI benefits (individuals who have not been members of a UI fund long enough); or (ii) has earnings-related UI benefits exceeding a certain level; or (iii) has children. Moreover, Hall et al. (2022) show that changes in financial incentives are unlikely to explain the impacts they find on job-finding; the estimated effect is slightly larger among those unemployed who did not experience a benefit cut.

[6] The working condition restricts UI benefits to individuals who have worked, at least part time, for 6 out of the last 12 months prior to unemployment.

[7] Healthcare fees and subsidies are subject to adjustments over time and we here state the numbers that applied in 2021.

[8] The Dental and Pharmaceutical Benefits Agency determines whether or not a certain medicine should be subsidized.

percent. The cap for 2021 amounts to SEK 2,350 (200 EUR), implying that this is the maximum amount an individual will pay for prescribed medication during a 12-month period.

The fact that the state bears a large share of the cost of both healthcare and prescribed medication means that socio-economic status should have limited importance for access to healthcare and medication.

3 Data

We combine data on individuals' registered unemployment with information on prescribed medication, medical contacts, employment, benefit uptake, education, and other relevant personal characteristics. The data on unemployment spells come from the register of the Public Employment Service (PES), and include day-by-day information on open unemployment, program participation, and the reason why the unemployment period ends. It also includes several demographic variables such as sex, potential disabilities, and exact date of birth. To construct health outcomes, we use the Prescribed Drug Register as well as the Patient Register, which are both maintained by the National Board of Health and Welfare. The former register contains individual-level information on all purchases of prescribed medication, including the type of drug (ATC codes[9]) and the date of the prescription. The latter contains information on diagnoses (ICD-10 codes[10]) and dates for all inpatient medical contacts[11] as well as all outpatient medical contacts in specialized care.[12] Additionally, we have added several demographic variables from Statistics Sweden, such as education level, previous income, and presence of children. All data cover the entire Swedish population.

The YJG program was introduced in December 2007, and we analyze its effects in 2008–2014.[13] Our sample for year 2008, for example, includes all individuals aged 20–29 who became unemployed[14] between October 2007 and September 2008, and therefore became eligible for the program between January 2008 and December 2008 (if they were still unemployed and below 25 years of age at that time). The samples for the following years are constructed in the same manner. All analyses below are con-

[9] The drugs are classified by the Anatomical Therapeutic Chemical (ATC) classification system, which is controlled by the World Health Organization Collaborating Centre for Drug Statistics Methodology (WHOCC). This 5-level classification system divides drugs into different groups according to the organ or system on which they act and their therapeutic, pharmacological and chemical properties.

[10] ICD-10 refers to the 10th revision of the International Statistical Classification of Diseases and Related Health Problems, which is a medical classification list managed by the WHO.

[11] Refers to cases where the individual has been admitted to a hospital. In general, this means that an overnight stay has been required.

[12] The Patient Register covers both public and privately operated healthcare. However, it documents only those visits involving a physician, excluding consultations with other healthcare professionals. It is also important to note that visits in primary care are not included.

[13] The rules regarding program eligibility remained the same during this period. We stop sampling after 2014 as our database does not include health outcomes after 2015, and we want to be able to follow individuals for at least a year after the start of unemployment.

[14] A new unemployment spell starts when an individual becomes registered as openly unemployed at the PES, given that the person has not been registered during the previous 365 days.

ducted using the combined 2008–2014 samples. The data allow us to follow the individuals' labor market and health outcomes until the end of 2015.

We construct indicators of health problems at different points in time after the onset of unemployment from the ATC codes in the Prescribed Drug Register and the ICD codes in the Patient Register, in combination with the prescription/hospital visit date. We use four main health outcomes in the study:

1. An indicator for having purchased prescribed medication for any health problem.
2. An indicator for having purchased prescribed medication for mental health problems.
3. An indicator for hospital admission or medical contact in specialized care for any health problem.
4. An indicator for hospital admission or medical contact in specialized care for mental health problems.

In addition, in the robustness analysis, we use the number of healthcare contacts for any health or mental health problem as outcomes, which may better capture the severity of health problems. The mental health problems we consider are often related to stress, anxiety, or depression. Specifically, we consider individuals to suffer from mental health problems if they have been prescribed (and have purchased) any drug belonging to categories N05 (Psycholeptics; including, e.g., treatment of sleep disturbances and anxiety) or N06 (Psychoanaleptics; comprising e.g. antidepressants), or if they have had a healthcare contact in open specialized care or inpatient care for a diagnosis belonging to ICD-10 chapter F00-99 (mental, behavioral, and neurodevelopmental disorders) or chapter G47.0 (insomnia).

Besides studying the impact on health problems, we also show effects on transitions to employment. To do this, we define a person as having found a job if he/she becomes employed for at least 30 consecutive days. In the data, an unemployed person can become employed in two different ways: (i) A person is deactivated from the PES register, and the reason for leaving the register is regular employment.[15] (The reason for leaving the register is recorded by the caseworker.) (ii) A person remains in the PES register, but is registered as a temporary, hourly, or part-time employee.[16] In both cases we require the person to be employed for at least 30 consecutive days. In the first case, that means that the individual is not allowed to reappear in the register during the next 30 days.

Table 1 presents background characteristics for the individuals in the sample. Column (1) includes the full sample of unemployed persons aged 20–29; column (2) includes all YJG participants, and columns (3) and (4) include all unemployed 24- and 25-year-olds, i.e., individuals within one year from the YJG eligibility threshold. The 24- and 25-year-olds are similar in terms of most background variables, but the 25-year-olds have a higher education level, which reflects the fact that they are older.

[15] In the main analysis, we also treat 'New Start Jobs' – a type of subsidized employment – as regular employment. The reason is that all employers who hire an unemployed person who fulfills certain criteria are entitled to this subsidy. However, for the first year of our sampling period, the eligibility criteria for this subsidy differed somewhat for 24- and 25-year-olds. In Appendix A, we show that the estimated effects on employment are robust to changing the definition of employment to exclude this type of subsidized employment.

[16] A person can remain in the PES register if he/she has found employment but is still searching for other jobs.

They have also had time to accumulate more days in unemployment before the start of the current unemployment spell. In terms of prior health indicators (measured during the 365 days preceding the start of unemployment), the two groups are similar in terms of healthcare contacts and prescribed medication, although the number of prescription instances is slightly higher for the 25-year-olds. 11 percent of both groups received drugs for mental health problems, and 6 percent had a healthcare contact related to mental health problems. Our RD design will exploit the discrete change in program eligibility at the threshold of turning 25. Hence, what matters for our ability to identify the causal impact of program eligibility is whether there are any jumps in the background variables at this threshold. We examine this issue in Section 6.4.

Table 1. Descriptive statistics for our sample of unemployed individuals

	(1) All 20-29-year-olds	(2) All in the YJG program	(3) All 24-year-olds	(4) All 25-year-olds
Demographic variables				
Age at spell start + 90 days	24.31	22.05	24.50	25.50
Female	0.49	0.43	0.50	0.51
Registered disability	0.06	0.09	0.06	0.06
Compulsory education	0.18	0.17	0.18	0.18
Upper secondary education	0.64	0.75	0.60	0.54
Post-secondary education	0.18	0.07	0.22	0.27
Country of birth, Nordic	0.73	0.80	0.72	0.69
Country of birth, other European	0.08	0.07	0.08	0.09
Country of birth, non-European	0.19	0.13	0.20	0.22
Married, year t-1	0.10	0.04	0.10	0.12
First child before spell start	0.01	0.00	0.00	0.00
Unemployment history and prior earnings				
Total number of days in previous unemployment spells	183.93	112.75	209.07	239.03
Number of previous unemployment spells	1.36	0.79	1.52	1.77
Number of previous programs	0.56	0.84	0.64	0.64
Social assistance>0, year t-2	0.13	0.15	0.14	0.13
Income from work (100 SEK), year t-2	670.98	477.55	822.71	837.51
Employed, November year t-2	0.40	0.32	0.46	0.46
Prior health indicators (during the 365 days preceding spell start)				
Number of prescription times (any drug)	1.66	1.44	1.66	1.70
Any prescription	0.53	0.52	0.53	0.53
Any drug for mental health problems	0.10	0.09	0.11	0.11
Any healthcare contact	0.35	0.33	0.35	0.34
Any healthcare contact due to mental health problems	0.06	0.05	0.06	0.06
Number of observations	736 462	132 200	68 355	65 125

4 Empirical strategy

We use a regression discontinuity design to estimate the effects of eligibility for the YJG, using the fact that only individuals who have not yet turned 25 at 90 days of unemployment are eligible for the program. Even though age may be related to health outcomes, 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 receive the treatment (program eligibility) while those on the other side do not. Hence, any differences in health outcomes between individuals on each side of the cut-off can be attributed to program eligibility. The same empirical design has previously been used in Hall et al. (2022) to study the impact of the same activation program on employment and later earnings, showing that the program mainly resulted in a threat effect, i.e., increased transitions to employment before program start.

We estimate intention-to-treat impacts of activation, i.e., the impact of being eligible for the YJG program using the sharp RD design. While not all eligible job seekers participate in the activation program, the fuzzy design – where program eligibility would be used as an instrument for the take-up – would be hard to interpret causally. The reason is that the mandatory activities only start later in the unemployment spell, implying that those subject to these measures would be a selected group of individuals who have not found a job until the activation starts. Moreover, in our setting, being eligible for the program affected job finding patterns already before the start of the program.

An important point to note is that the assignment variable in this application is not age per se, but age at a particular duration of unemployment (90 days after registration at the PES). Once assigned to the program, individuals risk losing their unemployment insurance (UI) benefits if they drop out after turning 25. Hence, we avoid an often-encountered problem in age-based RD analyses, i.e. the possibility that reactions of individuals close to the cut-off age would be muted 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 program 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 program assignment in our application is not deterministic.

However, a potential threat to a causal interpretation of our estimates is that the program could affect unemployed individuals' decision to register at the PES. If there are individuals with detailed knowledge of the program eligibility requirements before registering at the PES, even though they cannot fully control the date of becoming unemployed, some of them may choose to delay registration to avoid activation.[17] This type of behavior would lead to sorting around the eligibility threshold.[18] To examine whether such sorting takes place, Figure 2 shows the number of individuals entering unemployment, by

[17] Individuals are likely to be informed about the program upon registration at the PES and/or during their first meeting with a caseworker, which should take place within 30 days of unemployment. Individuals can also learn about the program from the PES website. Individuals who have been unemployed previously may be aware of the program from previous contacts with the PES.

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

age at day 90 after the start of unemployment (where age is measured relative to the cut-off age of 25). The figure gives no indication that individuals would time their PES registration to avoid activation. This is supported by the RD manipulation test developed by Cattaneo et al. (2018).[19]

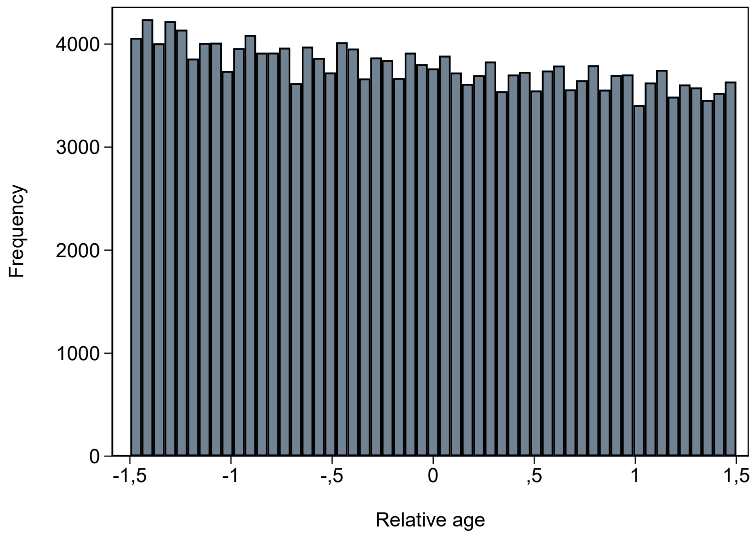

Figure 2. Number of individuals entering unemployment, by age at day 90 of the unemployment spell
Note: Age in years relative to the cut-off age of 25 on the x-axis.

Finally, could identification be compromised by the existence of other programs or other age-based policies? Sweden has a rich set of ALMPs also for unemployed individuals older than 25. However, due to the YJG, program participation is much more common among unemployed individuals under 25 years of age; see Figure 3. The figure shows the probability of remaining in open unemployment relative to starting *any* labor market program at different points of time during the unemployment spell. The individuals are divided into groups based on both which calendar year they are born and their age at 90 days after entering unemployment. The pink lines correspond to individuals who enter unemployment during the calendar year they turn 24 (eligible for the YJG); the blue lines correspond to individuals who enter unemployment during the calendar year they turn 25 (i.e., the group affected by the YJG eligibility cut-off), and the yellow lines correspond to individuals who enter unemployment during the calendar year they turn 26 (not eligible for the YJG). It is clear that the likelihood of participating in any ALMP increases sharply for 24-year-olds (and younger individuals) around 90 days of unemployment, whereas there is no such pattern for 25-year-olds (or older individuals). This is reassuring as it indicates that we do not need to worry about possible confounding effects arising from program participation by older job seekers.

[19] This test tests the null hypothesis of continuity of the density functions for control and treated units at the threshold, i.e., there is no manipulation of the density at the threshold. The p-value is 0.3680, which means that we cannot reject that there is no manipulation at the threshold.

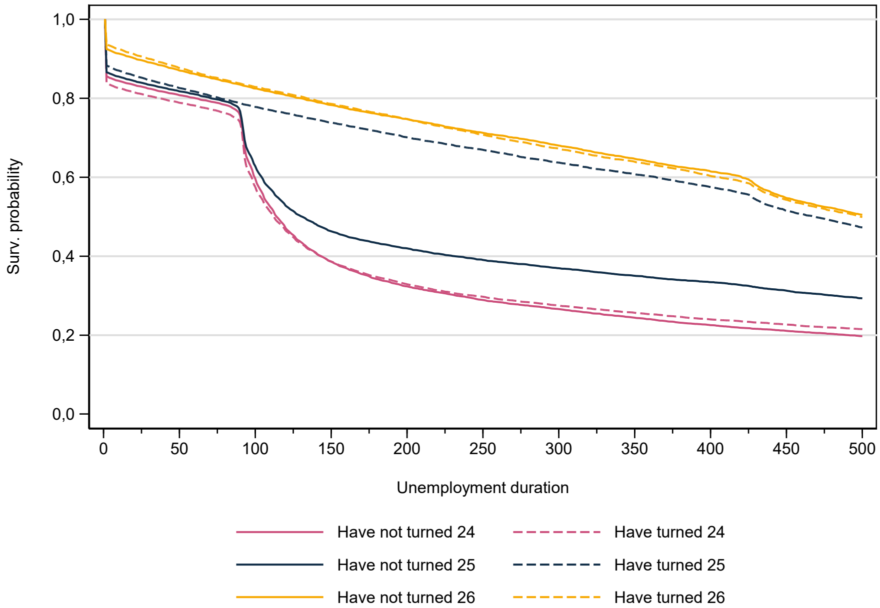

Figure 3. ALMP take-up, 2008–2014. Kaplan-Meier survival function estimates.

Note: The figure shows the probability of remaining in open unemployment relative to starting *any* labor market program at different points of time during the unemployment spell. The individuals are divided into groups based on both which calendar year they are born and their age 90 days after entering unemployment. The pink lines correspond to individuals who enter unemployment during the calendar year they turn 24; the blue lines correspond to individuals who enter unemployment during the calendar year they turn 25 (i.e., the group affected by the YJG eligibility cut-off), and the yellow lines correspond to individuals who enter unemployment during the calendar year they turn 26.

Turning to other age-based policies, a potentially relevant payroll tax cut was introduced in July 2007, where the reduction applied to individuals who had not yet turned 25 at the beginning of the year. Hall et al. (2022) show that this policy is unlikely to pose any problem for identifying the impact of YJG eligibility using this type of RD-design. The reason is that eligibility for the payroll tax cut is determined by the individual's year of birth, not by his/her age at a particular duration of unemployment as for YJG eligibility. Therefore, the eligibility cut-offs of the two policies do not in general coincide. Moreover, eligibility for the payroll tax reduction runs out in the calendar year when the individual turns 26, which means that individuals close to the cut-off for the tax reduction are only eligible for a very small subsidy. For this reason, there is no meaningful discontinuity in the subsidy amount at the eligibility threshold.

In sum, other activation programs or age-based tax policies are unlikely to compromise our analysis. The only potential confounding policy we are aware of is the New Start Jobs program, which constitutes subsidized employment targeted at certain subgroups of unemployed persons. The eligibility requirements for this subsidy differed slightly for 24- and 25-year-olds during the first year of our sampling period. In Appendix A, we show that our results are robust to how these jobs are treated in the analysis.

5 Results

5.1 Graphical analysis

In this section, we present graphical analyses, with the purpose of visualizing if there are any jumps in individuals' health outcomes at the YJG eligibility threshold, i.e., between 24- and 25-year-olds. In Figures 4–7, the individuals in the sample are arranged according to their exact age (based on daily data) at day 90 after entering unemployment, and age is measured relative to the cut-off age of 25. This means that the negative portion of the x-axes consists of individuals who will become eligible for the program if they remain unemployed for at least 90 days, whereas the positive portion consists of individuals who will not be eligible due to being above the relevant age threshold. We show results for the four different health outcomes listed above: an indicator for receiving any drug prescription (Figure 4); an indicator for receiving a drug prescription related to mental health problems (Figure 5); an indicator for having any hospital admission or visit in specialized care (Figure 6); and an indicator for having any hospital admission or visit in specialized care where a diagnosis related to mental health problems was registered (Figure 7). For all outcomes, we show results both for day 1–90 after the start of unemployment, i.e., before individuals can participate in the program, and for day 1–365. A jump upwards (downwards) at the cut-off for any of the health indicators would indicate that program eligibility leads to an improvement (decline) in health, as drug prescriptions and medical contacts would be higher (lower) among the unemployed that are not eligible for the YJG program. However, there is no clear indication of a jump at the threshold for any of these outcome variables.

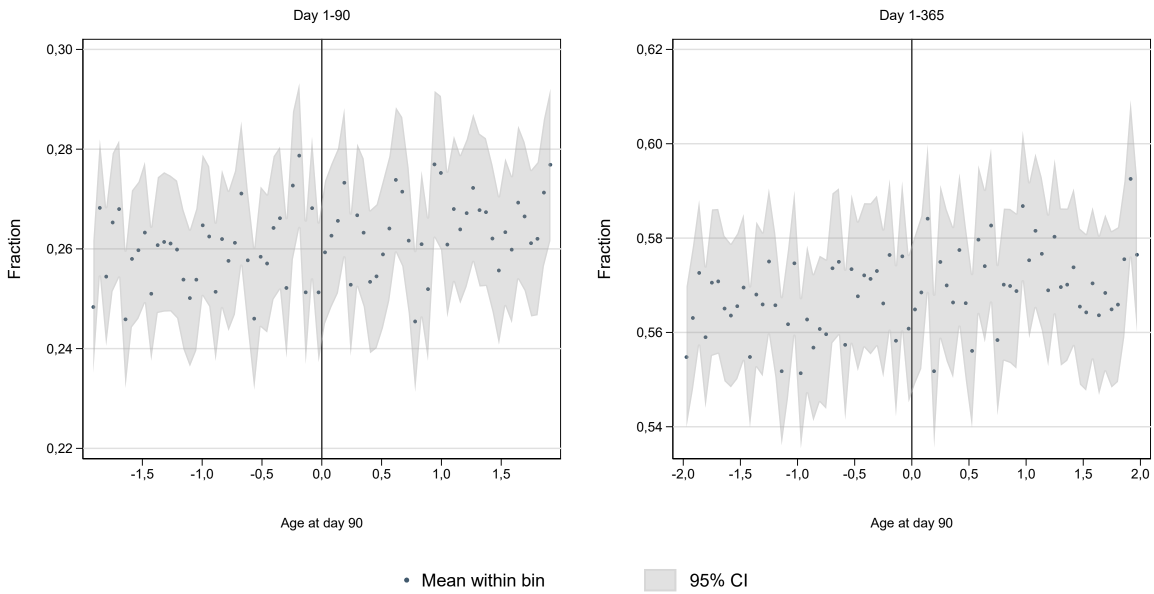

Figure 4. Effects of program eligibility on the probability of having received any drug prescription by day 90 and 365 after the start of unemployment, 2008–2014.

Note: The x-axis shows age in years relative to the cut-off age of 25. Age is measured using daily data and refers to the individual's age 90 days after entering unemployment. The figures are drawn for observations within the optimal bandwidth, using the optimal bandwidth algorithm from Calonico et al. (2014).

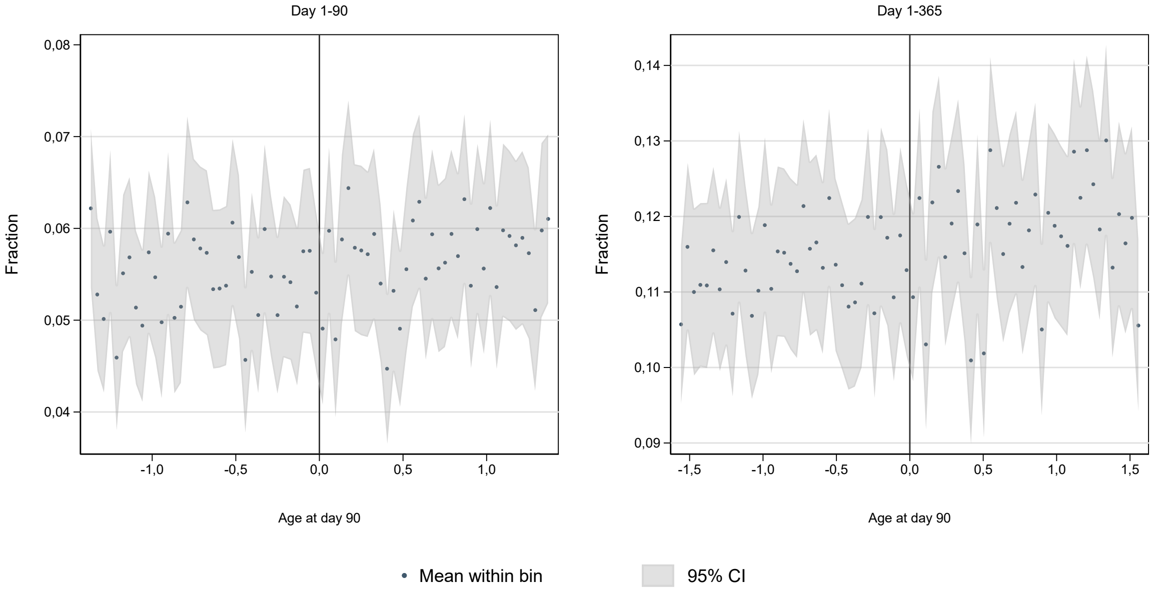

Figure 5. Effects of program eligibility on the probability of having received any drug prescription related to mental health problems by day 90 and 365 after the start of unemployment, 2008–2014.

Note: The x-axis shows age in years relative to the cut-off age of 25. Age is measured using daily data and refers to the individual's age 90 days after entering unemployment. The figures are drawn for observations within the optimal bandwidth, using the optimal bandwidth algorithm from Calonico et al. (2014).

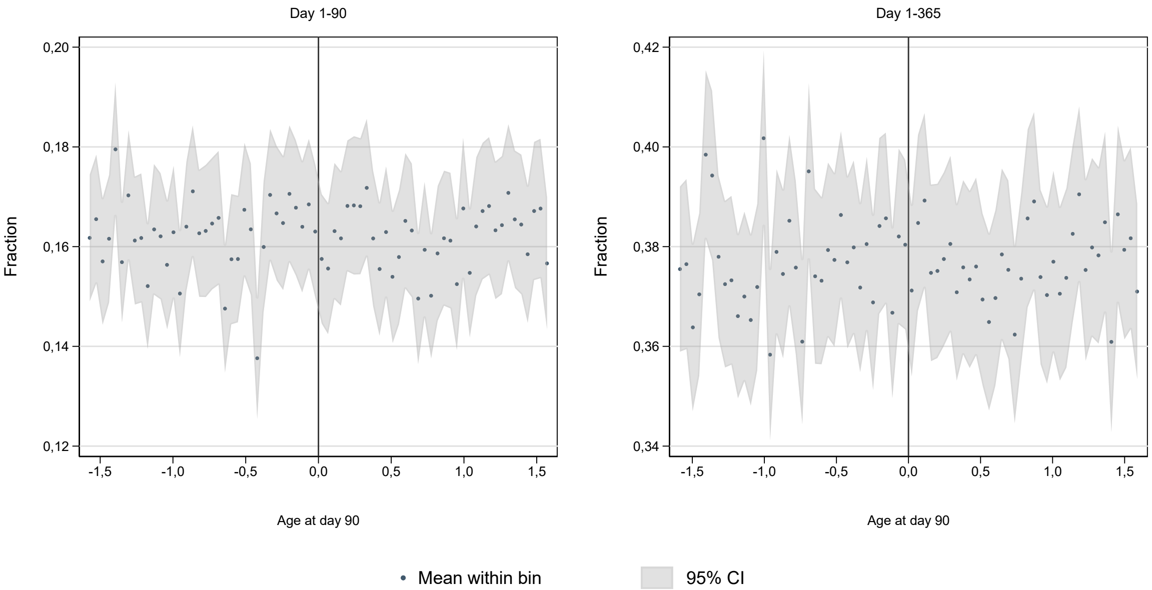

Figure 6. Effects of program eligibility on the probability of having had any medical contact by day 90 and 365 after the start of unemployment, 2008–2014.

Note: The x-axis shows age in years relative to the cut-off age of 25. Age is measured using daily data and refers to the individual's age 90 days after entering unemployment. The figures are drawn for observations within the optimal bandwidth, using the optimal bandwidth algorithm from Calonico et al. (2014).

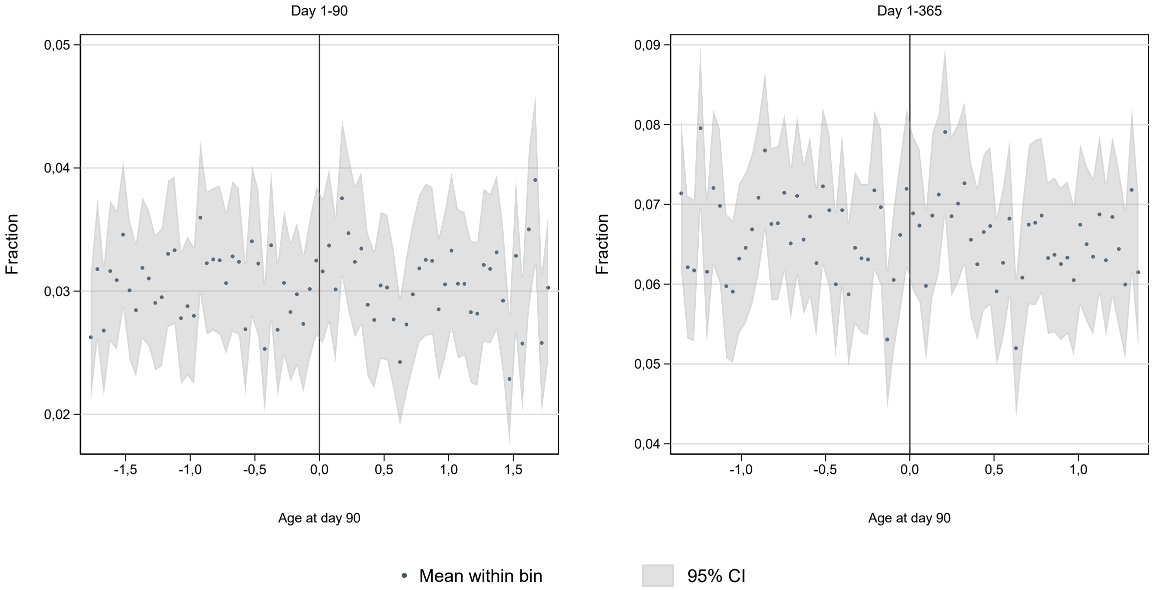

Figure 7. Effects of program eligibility on the probability of having had any medical contact due to mental health problems by day 90 and 365 after the start of unemployment, 2008–2014.

Note: The x-axis shows age in years relative to the cut-off age of 25. Age is measured using daily data and refers to the individual's age 90 days after entering unemployment. The figures are drawn for observations within the optimal bandwidth, using the optimal bandwidth algorithm from Calonico et al. (2014).

5.2 Main results

In Table 2, we show results from local linear regressions of the same outcome variables on relative age, using a triangular kernel and the optimal bandwidth algorithm from Calonico et al. (2014). As recommended by Cattaneo et al. (2019), the tables report conventional RD point estimates and p-values stemming from the bias-corrected standard errors developed in Calonico et al. (2014). The latter adjust for the potential bias that may arise when using observations that are far away from the cut-off value of the assignment variable in the estimations. In other words, the estimation of mean impacts and standard errors are 'decoupled', following a slightly different routine.

Panel A displays the estimated impact of program eligibility on prescribed medication; the first two columns show estimates for any drug and the last two for drugs related to mental health problems. A negative and statistically significant coefficient would indicate that those eligible for the YJG program suffer from health problems to a smaller extent. We again show results both for the first 90 days as well as for a full year after the onset of unemployment. All estimates are close to zero and statistically insignificant. Hence, there is no indication that program eligibility had any meaningful impact on the probability of being prescribed medication.

Panel B displays the results for medical contacts; first for any diagnosis (col. 1 and 2), and thereafter for diagnoses related to mental health problems (col. 3 and 4). None of the estimates are statistically significant at conventional levels (1 or 5 percent), but there is some indication of a potential beneficial impact on medical contacts related to mental health problems. However, this tendency is visible already during the first 90 days of unemployment, suggesting that any such impact is unlikely to be caused by activation itself. For all four outcomes, the confidence intervals allow us to rule out improvements larger than 0.9 percentage points during the first year after the onset of unemployment.

Table 2. Effects of program eligibility on the probability of being prescribed medication and healthcare visits

A. Drug prescriptions	Any drug prescription		Prescription mental health drug	
	(1) Day 1-90	(2) Day 1-365	(3) Day 1-90	(4) Day 1-365
RD estimates	0.000612	-0.000119	-0.000363	-0.000708
	(0.00379)	(0.00404)	(0.00232)	(0.00304)
Conventional p-value	0.872	0.977	0.876	0.816
Robust p-value	0.963	0.969	0.726	0.695
Observations in sample	736,462	736,462	736,462	73,6462
Nobs within bw left of cutoff	123,852	137,558	89,732	101,647
Nobs within bw right of cutoff	135,854	152,430	95,812	109,849
Bandwidth	1.939	2.163	1.388	1.581
Mean of dep. variable, age 25	0.2621	0.5706	0.0564	0.1164

B. Hospital admission or visit in specialized care	Any diagnosis		Diagnosis related to mental health	
	Day 1-90	Day 1-365	Day 1-90	Day 1-365
RD estimates	0.00283	0.00199	-0.00241	-0.00418
	(0.00351)	(0.00458)	(0.00156)	(0.00255)
Conventional p-value	0.420	0.665	0.122	0.101
Robust p-value	0.594	0.868	0.0935	0.0741
Observations in sample	736,462	736,462	736,462	736,462
Nobs within bw left of cutoff	102,551	103,590	115,072	88,895
Nobs within bw right of cutoff	110,748	111,762	125,214	95,063
Bandwidth	1.595	1.610	1.796	1.376
Mean of dep. variable, age 25	0.1605	0.3753	0.0308	0.0656

Notes: Results from local linear regressions using a triangular kernel and the optimal bandwidth algorithm from Calonico et al. (2014).

5.3 Program take-up

Figure 8, which is drawn for individuals who remained unemployed for at least 90 days, illustrates how eligibility for the YJG affected program take-up. For individuals younger than 25, take-up is generally around 45–50 percent.[20] The figure shows that there is a clear downwards trajectory in program take-up near the eligibility cut-off. A likely reason that take-up starts to fall already before the 25-year threshold

[20] The seemingly low participation rate could be due to capacity constraints delaying program participation, which could mean that more individuals leave unemployment before program start. It is also possible that some individuals decline to participate. The risk of losing benefits by declining participation does not concern individuals who are not entitled to UI benefits. Hall and Liljeberg (2011) report that 27 percent of unemployed 24- and 25-year olds in 2009 were not entitled to UI benefits. In 2008, the corresponding share was 37 percent.

is that caseworkers are not able to assign all individuals to the program right away at 90 days of unemployment (e.g., due to a high workload). If there is some delay in program assignment, this means that some individuals who are close to turning 25 may have already turned 25 by the time program assignment is considered (and, thus, are no longer eligible). For individuals older than 25, take-up is close to zero, as it should be. The setting would in principle call for a fuzzy RD-design, but we prefer the more conservative sharp RD estimates for the reasons described in Section 4. However, since the difference in take-up is small for individuals close to the eligibility cut-off, we also consider a donut hole design, excluding observations near the cut-off. These results are presented in Section 5.4.3.

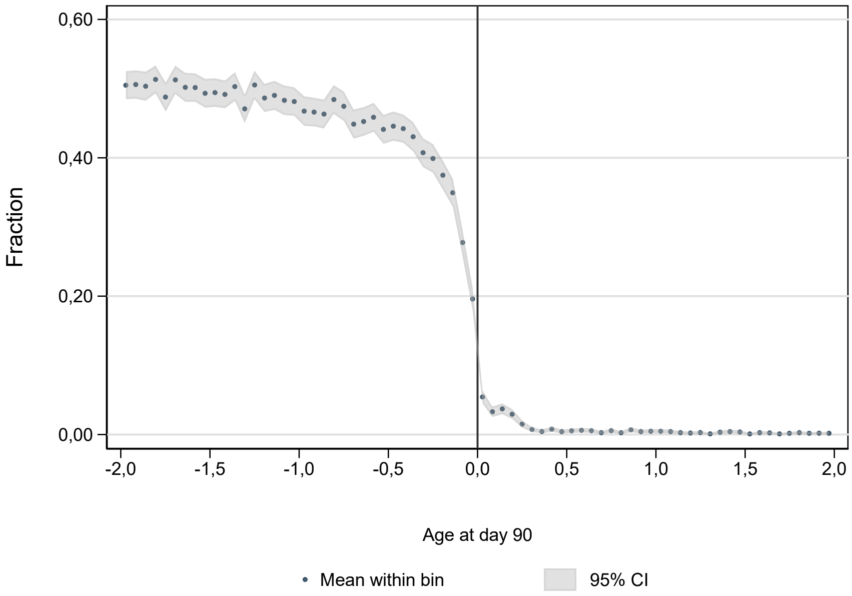

Figure 8. Effects of YJG eligibility on program take-up, 2008–2014

Notes: Age in years relative to the cut-off age of 25 on the x-axis and an indicator for participating in the program on the y-axis. Age is measured using daily data and refers to the individual's age 90 days after entering unemployment. The figure is drawn for individuals who remained unemployed for at least 90 days.

5.4 Validity and robustness checks

We now turn to assess the validity of our RD design. We start by examining whether there are any discontinuities in background variables at the YJG eligibility cut-off and if our results are robust to controlling for background characteristics. We also examine if the results are sensitive to the choice of bandwidth, and we carry out placebo analyses where the threshold is moved away from the true eligibility cut-off. The results from these analyses are presented in Section 6.4.1. After these standard RD validity checks, we examine if our conclusions change if we instead use the number of healthcare visits as an outcome, to better capture the severity of potential health problems; see Section 6.4.2. Last, in Section 6.4.3, we show results from a donut hole design, excluding individuals closest to the cut-off for whom the difference in program take-up is rather small.

5.4.1 Standard RD validity checks

In Table 3 we check whether there are any discontinuities in pre-determined variables at the YJG eligibility cut-off. We estimate the same regression model as above but with the outcome variable replaced with several of the background variables presented in Table 1, including health indicators measured the year before the individual became unemployed. The results show that the covariates are generally balanced around the cut-off. Importantly, there is no statistically significant difference in prior health status. There is, however, a significant difference in employment status, measured two years earlier. Finding one significant difference is not necessarily a major issue, given that we test simultaneously for several differences. In Table A2 in the appendix we also show that our results are robust to controlling for covariates: all effect estimates stay very similar when controls are added to the model.

Table 3. Balance of background variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Demographic variables	Female	Upper secondary education	Post-secondary edcuation	European, nonnordic country of birth	Non European country of birth	Married	Disability
RD estimates	-0.00447	-0.00828	0.00487	0.00399	0.00664	0.00467	-0.00396
	(0.00570)	(0.00602)	(0.00447)	(0.00262)	(0.00428)	(0.00251)	(0.00264)
Conventional p-value	0.433	0.169	0.276	0.128	0.121	0.0621	0.134
Robust p-value	0.344	0.121	0.160	0.170	0.0927	0.0824	0.160
Obs. in sample	1017235	1017233	1017233	1013980	1013980	1017235	1017235
Obs. within bw left of cutoff	71873	63272	86807	106053	83987	139517	71172
Obs. within bw right of cutoff	75771	66320	92455	115087	89252	154969	74840
Bandwidth	1.107	0.971	1.341	1.660	1.303	2.196	1.095
Mean of dep.variable, age 25	0.5122	0.5446	0.2725	0.0899	0.2202	0.1178	0.0571

	(8)	(9)	(10)	(11)	(12)
B. Labor market history and prior earnings	Previous days in unempl.	Previous programs	Social assistance (t-2)	Income from work (t-2)	Employed (t-2)
RD estimates	-0.193	-0.00271	-0.00385	-21.53	-0.0195
	(0.854)	(0.00306)	(0.00344)	(12.63)	(0.00659)
Conventional p-value	0.822	0.375	0.262	0.0882	0.00313
Robust p-value	0.733	0.421	0.249	0.0652	0.00201
Obs. in sample	1017235	1017235	1017235	1017235	1017235
Obs. within bw left of cutoff	62575	79600	90389	58163	53700
Obs. within bw right of cutoff	65600	84058	96636	60565	55875
Bandwidth	0.960	1.227	1.400	0.890	0.822
Mean of dep. variable, age 25	29.3013	0.0606	0.1283	837.51	0.4612

C. Prior health indicators (during 365 days before spell start)	(13) Any prescription	(14) Presciption mental drug	(15) Medical contact any reason	(16) Medical contact mental health
RD estimates	-0.00488	-0.000896	-0.000651	-0.00131
	(0.00524)	(0.00321)	(0.00442)	(0.00240)
Conventional p-value	0.351	0.780	0.883	0.585
Robust p-value	0.240	0.684	0.770	0.524
Observations in sample	1017235	1017235	1017235	1017235
Nobs within bw left of cutoff	84735	85934	107229	90389
Nobs within bw right of cutoff	90037	91396	116348	96833
Bandwidth	1.308	1.328	1.672	1.401
Mean of dep. variable, age 25	0.5258	0.1101	0.3439	0.0603

Notes: Results from local linear regressions using a triangular kernel and the optimal bandwidth algorithm from Calonico et al. (2014).

In Figures 9 and 10 we examine whether our results are sensitive to the choice of bandwidth. The impact of program eligibility on drug prescriptions is not significant for any bandwidth considered (see Figure 9), whereas the impact on healthcare visits for mental health reasons is borderline significant for wider bandwidths (see Figure 10). This may be related to the fact that the difference in the take-up rate is greater between individuals further away from the cut-off value, which is something we will return to in Section 6.4.3. But, of course, these individuals are also less comparable to those in the immediate vicinity of the threshold value.

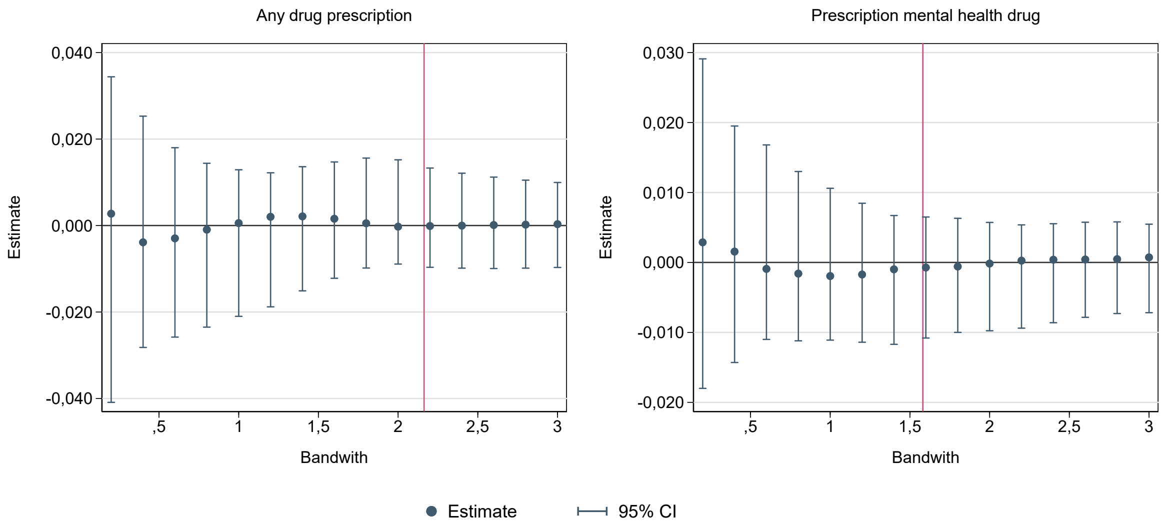

Figure 9. RD-robust estimates of effects of program eligibility on drug prescriptions, using different bandwidths.

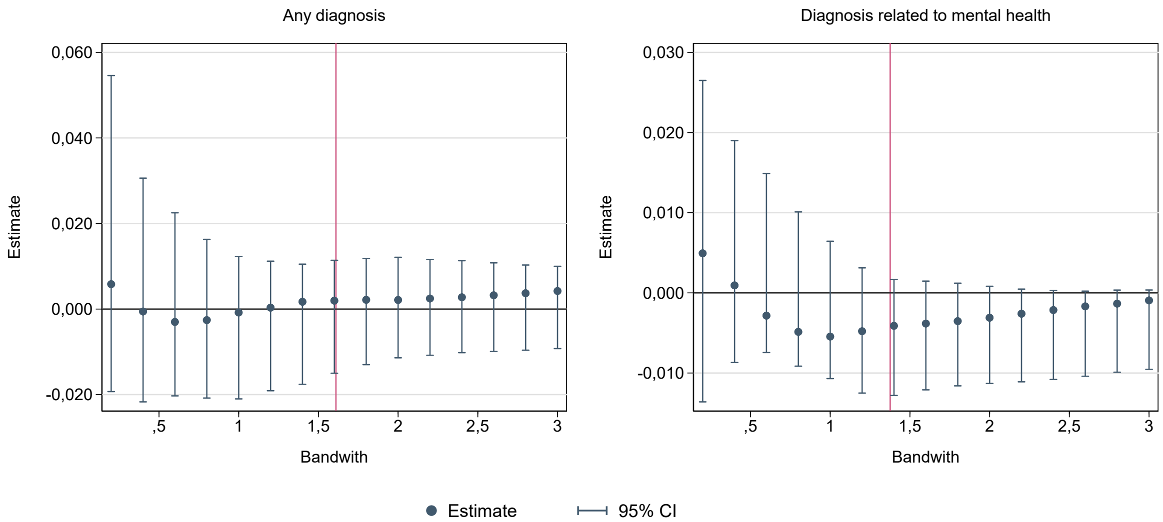

Figure 10. RD-robust estimates of effects of program eligibility on medical contacts, using different bandwidths.

As an additional validity check, we examine whether there are any placebo effects at the threshold of having turned 23 years old at day 90 of unemployment. Since both 22- and 23-year-old job seekers should be assigned to the YJG program after 90 days of unemployment, there is no reason why we should expect any discontinuity in health outcomes at this age threshold. Indeed, the estimated placebo effects are small and statistically insignificant; see Table 4.[21]

Table 4. Placebo-analysis: comparing 22- and 23-year-olds

	(1) Any drug, day 1-365	(2) Mental drug, day 1-365	(3) Any hospital visit, day 1-365	(4) Hospital visit, mental health, day 1-365
RD estimates	-0.0107	0.00568	-0.00208	0.00308
	(0.00589)	(0.00321)	(0.00571)	(0.00314)
Conventional p-value	0.0688	0.0772	0.716	0.326
Robust p-value	0.0806	0.0833	0.930	0.303
Observations in sample	736,462	736,462	736,462	736,462
Nobs within bw left of cutoff	66,313	85,957	66,877	59,970
Nobs within bw right of cutoff	71,126	93,857	71,577	63,820
Bandwidth	0.917	1.198	0.923	0.827
Mean of dep. Variable, age 23	0.5649	0.1099	0.3761	0.0668

Notes: Results from local linear regressions using a triangular kernel and the optimal bandwidth algorithm from Calonico et al. (2014).

Comparing health outcomes of 24- and 25-year-old job seekers *before* the YJG program was introduced, i.e., before December 2007, constitutes another possible placebo analysis. However, individuals who became unemployed before the end of 2006 were affected by a previous youth guarantee program (with the same age eligibility cut-off); see Forslund and Skans (2006). Hence, the sample of job seekers that can be used in such a placebo analysis is much smaller, resulting in much larger standard errors. Since the possibility of detecting significant pseudo-effects is much smaller, we find it less suitable as a placebo analysis.

5.4.2 Number of healthcare visits as outcome

The health indicators we have used as outcome variables may not fully capture the severity of potential health problems. Therefore, we also examine if there is an impact of being eligible for the YJG on the number of hospital admissions or visits in specialized care.[22] Again, we examine both healthcare visits

[21] We focus on 22- and 23-year-olds, rather than 23- and 24-years-olds, as the latter group falls within our optimal bandwidth, which could make it problematic to use as a placebo. Comparing 26- and 27-year-olds would constitute another possible placebo analysis. However, changes in eligibility rules for payroll tax cuts affecting 26-year-olds during this time period (see Egebark and Kaunitz 2018), makes this age group less ideal for a placebo analysis.

[22] In contrast, the number of prescription drugs as an outcome would not capture well how severe the health issues which are addressed by these prescriptions are.

for any reason and for reasons related to mental health problems. The results, reported in Table 5, indicate that program eligibility did not influence these outcomes either.

Table 5. Effect of program eligibility on number of hospital admissions or visit in specialized care

	Any diagnosis		Diagnosis related to mental health	
	(1) Day 1-90	(2) Day 1-365	(3) Day 1-90	(4) Day 1-365
RD estimates	0.00791	0.000670	-0.00129	-0.00447
	(0.00688)	(0.0219)	(0.00338)	(0.00978)
Conventional p-value	0.250	0.976	0.702	0.647
Robust p-value	0.438	0.820	0.615	0.513
Observations in sample	736,462	736,462	736,462	736,462
Nobs within bw left of cutoff	131,867	110,469	110,138	126,100
Nobs within bw right of cutoff	145,403	119,932	119,511	138,533
Bandwidth	2.069	1.721	1.717	1.976
Mean of dep., age 25	0.2642	1.0363	0.0514	0.197

Notes: Results from local linear regressions using a triangular kernel and the optimal bandwidth algorithm from Calonico et al. (2014).

5.4.3 Donut hole design

In Section 5.3 we saw that there is only a modest drop in the take-up of activation in the immediate vicinity of the eligibility cut-off value. This modest difference in take-up between the treated and the control individuals in our main specification could potentially explain why we do not find any clear impact of program eligibility on health outcomes. Table 6 presents the results from a donut hole design, where those within a month of the threshold age are excluded. That is, we exclude individuals who turn 25 years old +/- 30 days from the cut-off (day 90 in the unemployment spell). This change implies that the difference in the YJG take-up rate for individuals in the treated and the control group is greater (see Figure 8).

The results for all outcomes, apart from healthcare visits for mental health issues, remain statistically insignificant. The estimate for the probability of having any healthcare visit related to mental health problems (outcome d) becomes statistically significant and negative, implying that those eligible for the program suffer less from mental health problems. This pattern is visible already before day 90 of unemployment and stays at the same level in relative terms after program start, suggesting that this health improvement is unlikely to be caused by activation within the program. In the bottom panel of the table, we see that the impact on the number of healthcare visits for mental health reasons (a measure of the severity of the health issue) remains insignificant also for the donut hole design. Since there is no impact on this outcome, the effect for any healthcare visit related to mental health might be related to a reduction in less severe mental health issues.

Table 6. Estimates using a donut hole design

	(1) Day 1-90	(2) Day 1-365	(3) Day 1-90	(4) Day 1-365
	a) Any drug prescription		*b) Prescription mental health drug*	
RD estimates	0.00284	0.00324	-0.000820	-0.000331
	(0.00430)	(0.00546)	(0.00269)	(0.00334)
Conventional p-value	0.508	0.552	0.761	0.921
Robust p-value	0.634	0.580	0.673	0.820
Observations in sample	725,166	725,166	725,166	725,166
Obs within bw left of cutoff	113,319	92,498	83,070	100,914
Obs within bw right of cutoff	124,750	100,458	89,386	110,094
Bandwidth	1.863	1.527	1.375	1.662
Mean of dep. variable, age 25	0.2621	0.5712	0.0565	0.1165
	c) Any hospital or special care visit		*d) Visit with mental health diagnosis*	
RD estimates	-0.00137	0.00146	-0.00373	-0.00786
	(0.00469)	(0.00536)	(0.00195)	(0.00308)
Conventional p-value	0.770	0.785	0.0555	0.0106
Robust p-value	0.623	0.998	0.0477	0.00705
Observations in sample	725,166	725,166	725,166	725,166
Obs within bw left of cutoff	74,293	92,321	89,330	76,902
Obs within bw right of cutoff	79,375	100,271	96,938	82,330
Bandwidth	1.236	1.523	1.478	1.277
Mean of dep. variable, age 25	0.1608	0.3752	0.0306	0.0656
	e) No of hospital/specialized care visits (any reason)		*f) No of hospital/specialized care visits with mental health diagnosis*	
RD estimates	0.000433	-0.0284	-0.00362	-0.0204
	(0.00838)	(0.0281)	(0.00450)	(0.0130)
Conventional p-value	0.959	0.311	0.422	0.117
Robust p-value	0.858	0.225	0.374	0.0932
Observations in sample	725,166	725,166	725,166	725,166
Obs within bw left of cutoff	106,535	87,454	81,138	93,318
Obs within bw right of cutoff	116,594	94,575	87,350	101,442
Bandwidth	1.752	1.445	1.346	1.540
Mean of dep., age 25	0.2649	1.0402	0.0513	0.198

Notes: Results from local linear regressions using a triangular kernel and the optimal bandwidth algorithm from Calonico et al. (2014). Individuals who turn 25 years old +/- 30 days from the cut-off (day 90 after the start of unemployment) are excluded.

5.5 Mechanisms: Effects on job finding

Activation may affect individuals' mental health directly, e.g., through improved daily routines, more social support, and/or improved skills and self-esteem. But effects could also arise indirectly if program participation leads to a higher (or lower) rate of job-finding. Below, we show how program eligibility affects the chances of finding employment. That is, we replicate the results in Hall et al. (2022) but with data for a longer time period.[23] The effect on employment is captured by two dummy variables,

[23] Hall et al. (2022) use data for 2008–2010, while we use data for 2008–2014.

indicating whether the individual has found a job 90 and 365 days after entering unemployment. The first outcome measures the threat effect of the program, while the other outcome captures the total effect of program eligibility (i.e., a combination of the threat effect and possible program effects) during the first year after the start of unemployment.

Figure 11 presents a graphical analysis similar to the one we presented for the health outcomes. In line with the results in Hall et al. (2022), the patterns in the figure suggest that the positive employment effects of the program are small and short-lived: The figure to the left indicates that there may be a threat effect of program eligibility, i.e. those eligible for the YJG are more likely to find a job *before* being assigned to the program than those who are not eligible. However, the figure to the right suggests that job-finding among the ineligible starts to catch up later on during the unemployment period. A year after the start of unemployment, there is no visible jump in the job-finding rate at the eligibility threshold.

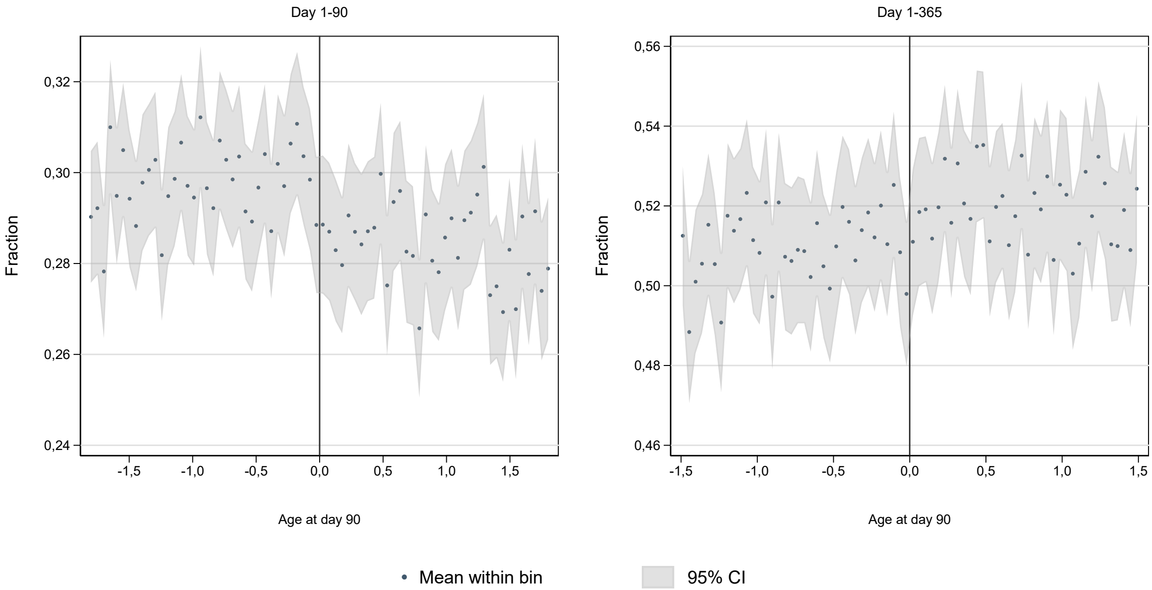

Figure 11: Effects of YJG eligibility on the probability of finding employment, 2008–2014

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

The estimates in Table 7 confirm the presence of a threat effect: there is a statistically significant, although modest, increase in the probability of finding employment before individuals are assigned to the program. In terms of magnitude, the threat effect corresponds to a 1 percentage point increase in employment probability during the first 90 days of unemployment, or an increase of 4 percent if we relate the estimate to the average outcome among 25-year-olds. If we use a follow-up horizon of one year, the estimated impact is close to zero and statistically insignificant. In the appendix (Table A2), we replicate a robustness check performed in Hall et al. (2022) and show that the results are robust to changes in the definition of employment (excluding New Start Jobs, a type of subsidized employment, from the definition of getting employed).

The non-existing employment impacts during the actual activation period may be one of the reasons for the absence of clear health impacts. If the main mechanism related to potentially advantageous health impacts of ALMPs is linked with greater employment probability, this mechanism is shut down in our case. Our results indicate that the direct health effects of the program are also small.

Table 7. Estimated effects of YJG eligibility on the probability of finding employment

	Day 1-90	Day 1-365
RD estimates	0.0123	-0.00760
	(0.00404)	(0.00487)
Conventional p-value	0.00232	0.119
Robust p-value	0.0145	0.179
Observations in sample	736462	736462
Obs within bw left of cutoff	116902	97426
Obs within bw right of cutoff	127606	104774
Bandwidth	1.827	1.511
Mean of dep., age 25	0.2852	0.5206

Note: Estimates from local linear regressions using a triangle kernel and the optimal bandwidth algorithm from Calonico et al. (2014). Standard errors in parentheses. 'New Start Jobs' (a type of subsidized employment) are included in the definition of employment.

5.6 Heterogeneity

The health impacts of activation may differ depending on the initial health status of the affected group. For example, if a person already had a prescription before he/she became unemployed, an indicator variable measuring the take-up of such medication would not react even if the person's health deteriorated due to unemployment. Table 8 reports results when we split the sample based on previous medication for mental health problems. The impacts remain largely insignificant for both those with and without prior medication. Among those who had an earlier prescription for medication to treat mental health problems, there is a statistically significant impact on healthcare visits related to mental health (Panel D, Column 3). However, the reaction is only statistically significant before any activation starts, suggesting that it is not caused by activation within the program. Since we have seen that the program had a threat effect on transitions to employment, the health improvement may instead be caused by these individuals finding employment quicker.

Table 8. Effects of program eligibility on drug prescriptions and healthcare visits. Separate effects depending on prior health status.

	(1)	(2)	(3)	(4)
	No prescription of mental health drugs before unemployment		Had prescription of mental health drug before unemployment	
	Day 1-90	Day 1-365	Day 1-90	Day 1-365
A. Any drug prescription				
RD estimates	0.000107	-1.96e-05	0.00513	0.000772
	(0.00371)	(0.00458)	(0.0133)	(0.00958)
Conventional p-value	0.977	0.997	0.699	0.936
Robust p-value	0.927	0.989	0.861	0.909
Observations in sample	659,380	659,380	77,082	77,082
Obs within bw left of cutoff	114,454	108,527	13,102	11,410
Obs within bw right of cutoff	127,159	119,955	13,333	11,699
Bandwidth	2.023	1.913	1.834	1.603
Mean of dep. variable, age 25	0.222	0.5326	0.5865	0.8776
B. Prescription mental health drug				
RD estimates	0.000816	0.00100	-0.00387	-0.0102
	(0.00117)	(0.00223)	(0.0122)	(0.0140)
Conventional p-value	0.485	0.652	0.752	0.469
Robust p-value	0.541	0.709	0.686	0.432
Observations in sample	659,380	659,380	77,082	77,082
Obs within bw left of cutoff	100,154	91,950	15,110	11,234
Obs within bw right of cutoff	109,678	100,020	15,311	11,510
Bandwidth	1.758	1.609	2.108	1.577
Mean of dep. variable, age 25	0.0156	0.052	0.3863	0.637
C. Any hospital admission or visit in specialized care				
RD estimates	0.00427	0.00358	-0.00544	-0.00918
	(0.00374)	(0.00490)	(0.0152)	(0.0124)
Conventional p-value	0.254	0.465	0.720	0.460
Robust p-value	0.272	0.580	0.601	0.379
Observations in sample	659,380	659,380	77,082	77,082
Obs within bw left of cutoff	78,275	86,810	9,816	13,813
Obs within bw right of cutoff	83,905	93,904	10,022	14,058
Bandwidth	1.361	1.514	1.372	1.930
Mean of dep. variable, age 25	0.1335	0.3406	0.3784	0.6566
D. Hospital visit or visit in specialized care with mental health diagnosis				
RD estimates	-0.000245	-0.00141	-0.0242	-0.0235
	(0.000914)	(0.00186)	(0.0127)	(0.0147)
Conventional p-value	0.789	0.449	0.0559	0.110
Robust p-value	0.626	0.415	0.0477	0.0963
Observations in sample	659,380	659,380	77,082	77,082
Obs within bw left of cutoff	111,030	80,836	9,827	10,268
Obs within bw right of cutoff	123,024	87,092	10,045	10,527
Bandwidth	1.959	1.408	1.375	1.438
Mean of dep., age 25	0.0098	0.0298	0.2005	0.3551

Notes: Results from local linear regressions using a triangular kernel and the optimal bandwidth algorithm from Calonico et al. (2014). The individuals are divided into groups based on prescriptions during the 365 days preceding the current unemployment spell.

We have also examined if effects differ by gender, but we find no significant differences; see Table A3.

6 Discussion

Our main finding is that there is weak evidence, at best, of any health benefits of the Swedish Youth Job Guarantee program overall. There is some indication, however, that the probability of healthcare visits related to mental health problems may have decreased in the short run. How do our results compare with the findings in earlier work on the subject?

Puig-Barrachina et al. (2020) provide a recent systematic review of the literature pertaining to the health impacts of ALMPs.[24] The vast majority of the papers have been published in other fields (such as social policy, public health, and psychology) rather than economics. They divide the studies into three categories: experimental work, quasi-experimental research, and other research. The first category includes randomized controlled trials, such as the evaluation of the U.S. JOBS and the Finnish Työhön job search programs; see Caplan et al. (1989) and Vuori et al. (2002). The quasi-experimental research covered in the review mostly refers to longitudinal studies with individual fixed effects specifications or even straightforward before-after comparisons. Puig-Barrachina et al. (2020) conclude that ALMPs have a positive impact on self-reported (mostly mental) health and the quality of life. They do not attempt to glean mean impact sizes from the studies, which would be very difficult because of the multitude of measures used. They also point out that significant knowledge gaps pertain to understanding the relation between the details of the programs, the target population, and the associated impacts.

There are some relevant studies not covered by the Puig-Barrachina et al. (2020) review. These include Rose (2019), which examines the impacts of German ALMPs on well-being using propensity score matching combined with difference-in-differences. Rose finds strong positive impacts from programs that resemble regular work, such as subsidized work or self-employment. The impact of participating in training is also positive but much smaller. The paper perhaps closest to ours is Caliendo et al. (2022). They study the effects of ALMPs in Sweden using a conditional difference-in-differences approach and data on prescribed medication. Their focus is on the impact of training programs and benefit sanctions. They find that training improves cardiovascular and mental health and lowers sickness absence, whereas sanctions have a short-term negative effect on mental health. Bastiaans et al. (2023) also use data on mental health medication and study the impact of activation among long-term inactive welfare recipients in the Netherlands. Exploiting the staggered implementation of the activation program, they find that activation improves mental health for those already on mental health medication before the program, while having little effect on labor market outcomes.

[24] Other surveys include those by Coutts et al. (2014) and Vinokur and Price (2015).

Hence, the bulk of the papers published to date suggest that ALMPs have the potential to improve individuals' well-being and mental health. Most studies rely on self-reported health measures, whereas our approach is based on various indicators of healthcare use based on administrative data. We therefore do not assess possible effects on self-reported stress or depression symptoms, for example. However, the few earlier papers that use administrative data on prescribed medication have also found significant positive health effects at least for some subgroups (Caliendo et al. 2022; Bastiaans et al. 2023). Therefore, differences in the types of outcome measures used are likely not the sole driver of the differences in the findings.

Another potential reason for differences in the results could relate to program design: More comprehensive and long-lasting interventions may have the potential for stronger health effects. The programs studied in earlier research differ greatly in terms of the intensity of the activation and the length of the intervention. At one end of the spectrum are the experimental studies, such as Caplan et al. (1989) and Vuori et al. (2002). They both examine the impacts of a week-long intervention, comprising 5 half-day sessions (totaling about 20 hours) led by two trainers who facilitate the learning of job search skills and the motivation to use them effectively. The training content is based on theory from psychology and includes job-search training, diagnosing appropriate goals, and finding ways to reach those goals. At the other end of the spectrum is the Swedish training program analyzed by Caliendo et al. (2022), which typically lasts about six months. The German interventions, examined by Rose (2019), vary in length and content, from short-term initiatives of three months, consisting of computer and language courses, to subsidized employment and long-term training lasting up to three years, leading to, e.g., a vocational degree.

The YJG program we examine, where typical job seekers participate in activation for approximately 14 hours a week until they find a job, or for 15 months at most, is reasonably comparable with some of the interventions studied earlier. The YJG program also focus on job-search activities with coaching and career advice. In terms of the intensity and length of the intervention, it represents a middle ground in comparison with the other programs. Its length clearly exceeds the one-week intervention in the JOBS RCT but falls short of the long adult education programs examined in the German context. Hence, it does not seem that the weaker health effects in the context of the YJG would necessarily be explained by a lower program intensity or duration. Also, the length of the follow-up period in health measurement is comparable. In Vuori et al. (2002) it is 6 months, Caliendo et al. (2022) use outcomes between 1 and 12 months after the treatment, while we follow individuals for 9 months after they become eligible to participate in the program.

One potential reason for the weak health effects in our context is that the program has little direct impact on job-finding, and the employment effects arise mainly before actual activation (i.e., a pre-program or threat effect). Therefore, the potential beneficial health impact that might arise as a result of new employment is arguably almost non-existent. Positive health impacts would have to stem from the activation itself, but our results indicate that those impacts are typically not significant. While this may be one of the reasons for the absence of clear health impacts in our setting, some of the earlier papers have found positive health impacts also in the absence of employment gains (Bastiaans et al. 2023; Caliendo et al 2022).

It is also possible that differences in econometric methodology and identification, which in our case is arguably strong in the vicinity of the age cut-off utilized in the RD design, may lead to differences in measured impacts. Finally, the target populations in the different studies also vary considerably. Our unique focus is on measuring the benefits of labor market activation for young individuals at a relatively early stage in their careers. It is notable that some of the earlier studies most comparable to ours (in particular Bastiaans et al. 2023) focus on a very different population (long-term inactive welfare participants) and only find significant effects for some subgroups, indicating that the findings may not generalize to other populations of interest. This also underscores the conclusion of the Puig-Barraghina et al. (2020) review, that important knowledge gaps remain in terms of understanding the health impact of ALMP for different target populations.

7 Conclusion

We contribute to the literature on potential health effects of active labor market programs by examining the health effects, especially pertaining to mental health, of a major, nationwide, youth activation program in Sweden, the Youth Job Guarantee. We utilize a regression discontinuity setting – leveraging the fact that only those who have not yet turned 25 at 90 days after the start of the unemployment spell are eligible for the program – and population-wide administrative health data covering prescription medication, hospital admissions, and visits in specialized care to measure the health impacts of the policy intervention.

We find that being eligible for the YJG program did not have clear impacts on health outcomes, measured by the take-up of prescription drugs or medical contacts overall. In the main specification, the impact on medical contacts related to mental health problems is also statistically insignificant, but the effect becomes positive and significant – meaning a lower likelihood of having medical contacts for these reasons – if we use a wider bandwidth or a donut hole design, where those within a month of the eligibility cut-off age are dropped from the analysis. A possible reason for the latter finding is that among these groups, the difference in the take-up rate of activation policies is greater between the treated and control individuals. The positive impact is visible already before day 90 of unemployment, that is, before individuals are assigned to the activation program. This suggests that the observed health improvement is unlikely to be caused by activation within the program. Further, the impact on the number of medical contacts for mental health reasons, a measure of the severity of the illness, remains insignificant also in the donut hole design. This suggests that the observed health improvement is likely to be related to a reduction in less severe mental health issues.

Confirming the findings of our earlier study, Hall et al. (2022), we find that the program led to a positive threat effect on employment, increasing the likelihood of finding a job before activation starts. However, the program does not have significant employment impacts in the longer term, during actual participation in the activation measures. Our findings are therefore consistent with a story where the modest positive short-term employment effect of program eligibility leads to modest health gains, whereas there are no direct health effects from activation itself.

These findings, we would argue, are valuable, given that the consensus reading of the literature in related surveys is that ALMP also brings positive health impacts, and our results are less positive in this respect. Our results are based on examining an existing large-scale program, in which tailored individual-level support may not be feasible. Hence, one needs to be cautious when considering the broader, non-employment, gains of such programs.

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Appendix: Additional results

A.1 Robustness of the main results to controlling for covariates

Table A2. Robustness to controlling for covariates.

	(1) Day 1-90	(2) Day 1-90	(3) Day 1-365	(4) Day 1-365
	Main result (no covariates)	Covariates included	Main result (no covariates)	Covariates included
A. Any drug prescription				
RD estimates	0.000612	0.00148	-0.000119	0.000895
	(0.00379)	(0.00347)	(0.00404)	(0.00391)
Conventional p-value	0.872	0.671	0.977	0.819
Robust p-value	0.963	0.758	0.969	0.665
Observations in sample	736,462	733,677	736,462	733,677
Nobs within bw left of cutoff	123,852	130,395	137,558	124,110
Nobs within bw right of cutoff	135,854	143,766	152,430	136,154
Bandwidth	1.939	2.056	2.163	1.952
Mean of dep. variable, age 25	0.2621	0.2621	0.5706	0.5706
B. Prescription mental health drug				
RD estimates	-0.000363	0.000590	-0.000708	0.000560
	(0.00232)	(0.00164)	(0.00304)	(0.00228)
Conventional p-value	0.876	0.719	0.816	0.805
Robust p-value	0.726	0.877	0.695	0.851
Observations in sample	736,462	733,677	736,462	733,677
Nobs within bw left of cutoff	89,732	128,117	101,647	118,382
Nobs within bw right of cutoff	95,812	141,120	109,849	129,308
Bandwidth	1.388	2.019	1.581	1.859
Mean of dep. variable, age 25	0.0564	0.0564	0.1164	0.1164
C. Hospital admission or visit in specialized care: any diagnosis				
RD estimates	0.00283	0.00425	0.00199	0.00356
	(0.00351)	(0.00355)	(0.00458)	(0.00456)
Conventional p-value	0.420	0.231	0.665	0.434
Robust p-value	0.594	0.292	0.868	0.510
Observations in sample	736,462	733,677	736,462	733,677
Nobs within bw left of cutoff	102,551	92,629	103,590	95,735
Nobs within bw right of cutoff	110,748	99,431	111,762	102,894
Bandwidth	1.595	1.443	1.610	1.492
Mean of dep. variables, age 25	0.1605	0.1605	0.3753	0.3753
D. Hospital admission or visit in specialized care: diagnosis related to mental health				
RD estimates	-0.00241	-0.00300	-0.00418	-0.00274
	(0.00156)	(0.00171)	(0.00255)	(0.00217)
Conventional p-value	0.122	0.0784	0.101	0.207
Robust p-value	0.0935	0.0610	0.0741	0.228
Observations in sample	736,462	733,677	736,462	733,677
Nobs within bw left of cutoff	115,072	75,074	88,895	83,668
Nobs within bw right of cutoff	125,214	79,050	95,063	88,871
Bandwidth	1.796	1.160	1.376	1.298
Mean of dep. variable, age 25	0.0308	0.0308	0.0656	0.0656

Notes: Results from local linear regressions using a triangular kernel and the optimal bandwidth algorithm from Calonico et al. (2014). Columns 2 and 4 control for sex; foreign background; level of education; being married; having children; registered disability; prior unemployment and participation in ALMP; prior earnings, employment, receipt of social assistance as well as whether the person had any prescribed drug or healthcare visit for mental health problems during the 365 days preceding unemployment (see Table 1 for details).

A.2 Robustness to changes in the definition of employment

In the main analysis, we consider a person to have found a job if he/she has left the PES register due to regular employment or has been registered as a temporary, hourly, or part-time employee for at least 30 consecutive days. We also treat one type of subsidized jobs – 'New Start Jobs (NSJ)' – as regular employment. The reason is that all employers who hire an unemployed person who fulfills certain criteria are entitled to this subsidy. Including NSJ as employment may not be an innocuous choice if those receiving these jobs would not obtain a normal job in the same manner. Moreover, in 2008, the eligibility rules for NSJ differed for individuals who had/had not turned 25 (thus, the same age cut-off as for the YJG): Employers could receive the NSJ subsidy if hiring a person who had been on disability pension, sick leave, or had been unemployed for at least 6 months if this person had not yet turned 25. Individuals who had turned 25 had to have been on disability benefits, sick leave, or been unemployed for at least 12 months before employers would be entitled to the subsidy. Therefore, the 24-year-olds in our sample may become eligible for NSJ earlier on in the unemployment spell than their 25-year-old counterparts, in particular if they have been on disability benefits or on sick leave prior to unemployment. By including all hires where the NSJ subsidy was paid out in our definition of employment, we therefore risk overestimating the effects of the YJG program on job finding (while we risk underestimating the effects if we do not treat NSJ as regular jobs). Note, however, that from March 2009, these rules were changed to be the same for 24- and 25-year-olds. Hence, this potential problem essentially only concerns individuals who became unemployed during the first year of our sampling period. Table A2 shows that our estimates are similar independently if we exclude New Starts Jobs from our definition of employment, in line with the results in Hall et al. (2022).

Table A2. Estimated effects of YJG eligibility on the probability of finding employment, excluding New Start Jobs from the definition of employment

	Day 1-90	Day 1-365
RD estimates	0.0119	-0.0126
	(0.00397)	(0.00517)
Conventional p-value	0.00266	0.0145
Robust p-value	0.0171	0.0585
Observations in sample	736,462	736,462
Obs within bw left of cutoff	120,008	86,807
Obs within bw right of cutoff	131,221	92,636
Bandwidth	1.877	1.343
Mean of dep. variable, age 25	0.2822	0.5052

Note: Estimates from local linear regressions using a triangle kernel and the optimal bandwidth algorithm from Calonico et al. (2014). Standard errors in parentheses.

A.3 Heterogeneity by gender

Table A3. Separate estimates for men and women.

	(1) Males Day 1-90	(2) Males Day 1-365	(3) Females Day 1-90	(4) Females Day 1-365
A. Any drug prescription				
RD estimates	-0.00108	0.000612	0.00373	0.00238
	(0.00530)	(0.00694)	(0.00559)	(0.00529)
Conventional p-value	0.839	0.930	0.505	0.653
Robust p-value	0.889	0.814	0.530	0.535
Observations in sample	376,497	376,497	359,965	359,965
Obs within bw left of cutoff	44,872	45,853	68,101	65,714
Obs within bw right of cutoff	49,205	50,366	72,685	69,976
Bandwidth	1.429	1.462	2.074	1.997
Mean of dep. variable, age 25[a]	0.1668	0.4052	0.353	0.7281
B. Prescription mental health drug				
RD estimates	0.00135	0.00140	-0.00198	-0.00154
	(0.00292)	(0.00425)	(0.00348)	(0.00404)
Conventional p-value	0.644	0.741	0.569	0.704
Robust p-value	0.690	0.753	0.456	0.576
Observations in sample	376,497	376,497	359,965	359,965
Obs within bw left of cutoff	46,438	43,418	46,770	66,470
Obs within bw right of cutoff	51,043	47,381	49,033	70,811
Bandwidth	1.482	1.380	1.411	2.022
Mean of dep. variable, age 25	0.0462	0.0953	0.0662	0.1366
C. Any hospital admission or visit in specialized care				
RD estimates	-0.00856	-0.00407	0.0129	0.00644
	(0.00431)	(0.00615)	(0.00557)	(0.00710)
Conventional p-value	0.0469	0.508	0.0202	0.364
Robust p-value	0.0755	0.558	0.0703	0.562
Observations in sample	376,497	376,497	359,965	359,965
Obs within bw left of cutoff	53,905	51,156	48,182	46,008
Obs within bw right of cutoff	60,223	56,811	50,565	48,144
Bandwidth	1.733	1.640	1.453	1.386
Mean of dep. variable, age 25	0.1261	0.3037	0.1933	0.4436
D. Hospital admission or visit in specialized care with mental health diagnosis				
RD estimates	-0.00415	-0.00508	-0.000788	-0.00240
	(0.00231)	(0.00316)	(0.00219)	(0.00329)
Conventional p-value	0.0725	0.108	0.719	0.467
Robust p-value	0.0618	0.0883	0.612	0.343
Observations in sample	376,497	376,497	359,965	359,965
Obs within bw left of cutoff	47,896	54,668	63,268	55,305
Obs within bw right of cutoff	52,752	61,084	67,172	58,330
Bandwidth	1.527	1.758	1.920	1.673
Mean of dep. variable, age 25	0.0285	0.0634	0.0329	0.0678

Notes: Results from local linear regressions using a triangular kernel and the optimal bandwidth algorithm from Calonico et al. (2014). [a,]Any drug prescription includes certain birth control methods, which is likely to explain the significantly higher baseline for women compared to men.