Long-Term Effects of Hiring Subsidies for Low-Educated Unemployed Youths*

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Abstract

We use regression discontinuity design and difference-in-differences methods to estimate the impact of a one-time hiring subsidy for low-educated unemployed youths in Belgium during the recovery from the Great Recession. Within a year of unemployment, the subsidy increases job-finding in the private sector by 10 percentage points. Over six years, high school graduates secure 2.8 more quarters of private employment. However, they transition from public jobs and self-employment, resulting in no net increase in overall employment, albeit with better wages. High school dropouts experience no lasting benefits. Additionally, in tight labor markets near Luxembourg's employment hub, the subsidy results in a complete deadweight loss.

Keywords: Hiring subsidies, youth unemployment, low-educated, regression discontinuity design, difference-in-differences, spillover effects
JEL classification codes: C21, J08, J23, J24, J64, J68, J61.

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1 Introduction

Economic recessions generally affect the labor market position of young people more strongly than that of adults. Following the 2008 Great Recession and the European sovereign debt crisis, the youth unemployment rate in the EU-27 increased from 16.0% in 2007 to 24.4% in 2013. Low-educated youths (less than high school education) were particularly affected, facing an unemployment rate increase from 19.5% to 31.0% over the same period (Eurostat, 2023). The empirical literature has demonstrated that recessions can have long-lasting consequences on the careers of youths (for example, Cockx, 2016; von Wachter, 2020, 2021). To counter these impediments to successful careers for youths, various policy interventions are often proposed, such as training, job-search assistance, monitoring, and direct job creation (see, for example, Caliendo and Schmidl, 2016). Finding the most appropriate policy responses is high on the policy agenda (OECD, 2020).

One frequently used policy is hiring subsidies for low-educated youths, which are often advocated to counteract negative demand shocks. The canonical economic literature shows that hiring subsidies can stimulate new employment if labor supply and demand are sufficiently elastic; otherwise, they are absorbed by higher wages (Katz, 1996). Other factors may also influence the effectiveness of hiring subsidies, such as the extent to which they are targeted at specific groups (e.g., workers near the minimum wage; Fougère et al., 2000), are one-shot and unanticipated (Cahuc et al., 2019), are implemented in a tight labor market (Kline and Moretti, 2013), or are conditional on job creation (Neumark and Grijalva, 2017). As hiring subsidies have a limited duration, new job opportunities may be short-lived and have no effect beyond the expiration of the subsidy. However, employment gains can persist if a worker's productivity has had the opportunity to be unveiled or has grown enough to justify the increase in wage costs after the subsidy ends. First, firms may use the subsidy as a screening device to reveal a worker's productivity and retain those who are highly productive (Farber and Gibbons, 1996; Altonji and Pierret, 2001; Pallais, 2014). Second, accumulation of firm-specific human capital during the subsidized period may raise productivity and encourage retention. Employment gains may also occur in other firms due to an accumulation of transferable human capital (Acemoglu and

Pischke, 1998, 2001; Autor, 2001; Adda and Dustmann, 2023). A final factor shaping the effectiveness of hiring subsidies is the spillover effect they may trigger on ineligible individuals and the substitution of unsubsidized jobs (see, for example, Crépon et al., 2013).

While the empirical literature has extensively studied hiring subsidies targeted at the long-term unemployed (Schünemann et al., 2015; Sjögren and Vikström, 2015; Ciani et al., 2019; Pasquini et al., 2019; Desiere and Cockx, 2022), much less is known about the effect of subsidies targeted at low-educated unemployed youths, in particular in the long run after the expiration of subsidies, and taking into account spillovers (Dahlberg and Forslund, 2005).

In this paper, we fill these gaps by studying the short- and long-run effects on various labor market outcomes of a generous one-shot hiring subsidy implemented in Belgium during the recovery from the Great Recession. This subsidy, called the *Win-Win Plan*, targeted young low-educated unemployed jobseekers. A firm hiring a high school dropout younger than 26 was eligible for a monthly subsidy of €1,100, which represented 48% of wage costs, on average. For high school graduates the amount was marginally lower and amounted to €1,000/month, i.e., 41% of their wage costs. The subsidy was granted for two years for hirings in 2010 and for one year for those in 2011. Since other, less generous, hiring subsidies existed for unemployed persons older than 26, the reform introduced an incremental reduction of wage costs of 24 percentage points for the high school dropouts and 18 percentage points for the graduates.

To estimate the intention-to-treat effect, we apply a one-sided donut regression discontinuity analysis (Barreca et al., 2016; Gerard and Gonzaga, 2021) that exploits the discontinuity in subsidy generosity at age 26. The estimation is based on a large sample of unemployed individuals living in the southern part of Belgium drawn from social security register data. We assess the robustness of our findings using the doubly robust semi-parametric difference-in-differences method of Sant'Anna and Zhao (2020).

In a nutshell, we find no evidence of incidence of the subsidy reinforcement on wages. The transition to private sector employment is raised by 10 percentage points within the first year of unemployment. For high school dropouts, the positive effect is short-lived and does not persist beyond the end of the subsidy period. In contrast, for high school graduates this positive effect on private employment does persist. Seven years after entry into unemployment, high school graduates accumulated an average of 2.8 more quarters in private employment, which is a relative increase of 28% with respect to the counterfactual of no Win-Win plan. Yet, this positive effect for high school graduates is counterbalanced by a decline in lower quality public employment as well as self-employment. Additionally, we demonstrate that labor market tightness induced by geographical proximity to the economic hub of Luxembourg diminishes the impact of the hiring subsidy. Lastly, we calculate the marginal value of public funds (MVPF) following the method proposed by Hendren and Sprung-Keyser (2020), indicating that the hiring subsidy is potentially self-financing for high school graduates. However, this estimate is very imprecise, so we do not recommend basing policy recommendations on this finding.

Our contribution to the literature is threefold. First, we provide new evidence on the effectiveness of a pure hiring subsidy targeted at low-educated young jobseekers. The added value of our analysis is that we consider outcomes up to five years after the expiration of the subsidy, longer than most existing studies, and that we apply different strategies for identifying the causal effects. Previous empirical evidence relating to similar target groups is inconclusive. The early studies estimating the effects of entering a subsidized job relied on the conditional independence assumption. In Sweden, Larsson (2003) finds that a hiring subsidy targeted at young high school graduates did not enhance the employment of participants up to two years after participation. Costa Dias et al. (2013) even show a negative effect of this policy after correcting for failures of the conditional independence assumption. In contrast, Caliendo et al. (2011) report large positive effects on the employment probability up to five years after being hired in temporary subsidized jobs in Germany for low-educated youths. Other studies estimate the impact of youth hiring subsidies mixed with other policy interventions such as job counselling (Blundell et al., 2004; Dorsett, 2006) or training (Bell et al., 1999; Brodaty et al., 2001). While the effects tend to be positive, it is difficult to pin them down to one specific intervention.[1] In a study similar to our paper, Cahuc et al. (2019) find a large intention-to-treat

[1] Another part of the literature evaluates the effect of payroll tax cuts targeted at all young employees and not only new hires. Wage subsidies are usually meant to boost the labor market integration of the target group structurally, and not as a temporary measure following an economic shock. Furthermore, as they are not targeted only at new hires, they tend to induce a strong deadweight (Neumark, 2013) and may therefore be an expensive way of boosting employment. Saez et al. (2019) and Saez et al. (2021) demonstrate that in Sweden, a wage subsidy in the form of a tax cut for businesses substantially increased youth employment, with the effect persisting three years after the tax cut was no longer in place (see also Skedinger, 2014; Egebark and Kaunitz, 2018).

effect on employment of a hiring subsidy targeted at low-wage workers in small firms that was introduced in France during the Great Recession. Batut (2021) shows that the effect persists up to two years after its end. The Win-Win subsidy that we analyze shares many features of the French hiring subsidy as it is also an unanticipated one-shot intervention, although it is targeted at low-educated unemployed youths rather than low-paid jobs in general.

Second, we provide evidence that hiring subsidies can have a positive long-term effect only in jobs where skill level exceeds a minimum threshold, i.e., a high school degree. This finding aligns with literature concluding "work-first” policies are ineffective for the lowest-skilled workers (i.e., high school dropouts) because the skill level of their jobs is too limited to generate significant human capital accumulation (Meghir and Whitehouse, 1996; Card and Hyslop, 2005; Blundell, 2006).[2] Roger and Zamora (2011), and Cahuc et al. (2021) corroborate this conclusion. They demonstrate that (hiring) subsidies for young high school graduates in France do not entail any effects, respectively, on the transition to open-ended contracts, and on callback rates, unless they were targeted at dropouts with certified on-the-job training. Similarly, Caliendo et al. (2011) report more positive employment effects of a hiring subsidy for youths with a high school degree than for those less educated.

However, our findings diverge by revealing a mechanism how "work-first" policies boost long-term earnings. The subsidy enables high school graduates to move from low-wage jobs in non-subsidized sectors, such as the public sector or self-employment, to private sector jobs offering better career prospects but typically rationed due to collectively bargained wage floors. This substitution goes against the existing literature showing crowding out of private sector jobs by public employment (Algan et al., 2002; Caponi, 2017; Fontaine et al., 2019). The fact that in our context the private sector jobs are the “good" jobs and the substituted public-sector jobs are the “bad" jobs, while this is usually the reverse, may explain this opposite finding.

Third, we are the first to demonstrate empirically that labor market tightness can moderate

[2] While several studies have challenged this view (e.g., Dyke et al., 2006; Autor and Houseman, 2010; Pallais, 2014; Riddell and Riddell, 2020), Autor et al. (2017) argue that by focusing on the average effects of job placement programs some of these studies may mask considerable effect heterogeneity and high program failure rates, particularly among the most disadvantaged participants. Specifically, the authors do not find any significant effects of direct-hire and temporary help job placements in the US on employment or earnings for participants in the lower tail of the earnings distribution, while among higher potential earners only direct hires foster positive effects. Temporary-help placements even lead to significant negative medium-term effects for this group.

the effectiveness of hiring subsidies, a prediction made by Kline and Moretti (2013).[3] We document that the hiring subsidy is a complete deadweight loss close to the border of Luxembourg, a small neighbor country of Belgium. Luxembourg is an economic hub with many attractive employment opportunities which, with the absence of language and legal barriers, attracts an important inflow of cross-border workers.[4] This important flow of cross-border work increases the labor market tightness at the other side of the border in Belgium which explains our finding.

This paper is structured as follows. Section 2 summarizes the institutional setting. The sampling scheme and data are described in Section 3. Section 4 presents the identification strategies and estimation methods. In Section 5, we present the empirical findings. The last section offers some concluding remarks.

2 Institutional Setting

In December 2009, the Win-Win plan was *unexpectedly* designed and adopted by the Belgian federal government for entry into force on January 1, 2010. It was only on January 18, 2010, that a press release from the Minister of Employment detailed the main features of the plan. This was directly followed by a large advertising campaign on radio, newspapers and Internet.[5] The plan involved generous *one-shot* subsidies available for recruitment during two years (2010 and 2011). The hiring subsidies were targeted at the most vulnerable groups of unemployed jobseekers, namely low-educated youths, older workers, and the long-term unemployed. The subsidy was implemented when the economic recovery was underway in Belgium, inducing employment to grow.[6] However, the unemployment rate was still peaking at a high level at the outset of 2010. Youths were particularly hard-hit: In 2009, the unemployment rate of people aged 15-24 rose to 20.4%, while it was only 7.9% for the group aged 25-74 (Eurostat, 2023).

In this paper, we evaluate the impact of this reform for what concerns the low-educated

[3] Previous studies have instead focused on understanding the conditions under which place-based policies can reduce regional inequalities (see Glaeser and Gottlieb, 2008; Kline and Moretti, 2014a,b for reviews).

[4] Today, 50,000 Belgian residents work in Luxembourg (11% of the workforce in Luxembourg; Statec, 2022).

[5] As in Cahuc et al. (2019), we use the Google Trends to verify that the introduction of the policy was unexpected. There are no searches for the policy name ("Plan Win-Win" or other variants) until January 2010 but many of them immediately afterwards, induced by the information campaign: see Figure A.1 in Online Appendix A.

[6] See Figure A.2 in Online Appendix A for a graphical illustration of the evolution of GDP, employment, and unemployment rates.

youths under 26 years of age (first two rows of Table 1). The age requirement was verified on the last day before hiring or on the date of the subsidy-eligibility card (see below). Private sector firms recruiting eligible youths benefited from a wage subsidy of about €1,000 per month for one year (if granted in 2011) or two years (if granted in 2010).[7] High school dropouts (graduates) became eligible after only 3 (6) months of registration as jobseekers within the last 4 (9) calendar months. Other jobseekers (post-secondary graduates or aged between 26 and 45) were entitled to a less generous subsidy of €750 per month during the first year (and €500 in the second year for recruitments realized in 2010) if they received unemployment benefits, but only to the extent that they had accumulated at least 12 months of unemployment over the last 18 months (but no more than 24 months over the last 36 months - last row of Table 1).

The Win-Win subsidy was not awarded automatically. The jobseeker had to prove sufficient unemployment duration to be eligible. To this end, the jobseeker had to fill out a form and request approval from the national Public Unemployment Agency (PUA).[8] The employer then had to draft an appendix to the employment contract, mentioning the subsidy amount that could be deducted directly from the gross salary of the beneficiary worker. The subsidy—referred to as the "work allowance"—was paid directly by the PUA to the worker. This arrangement allowed employers to reduce gross wages below the sectorally agreed-upon or legal minimum, binding only for contractual wages. This feature made it harder for workers to capture subsidy benefits as wage increases. The empirical analysis below finds no evidence of significant passthrough to wages. By contrast, had the subsidy been directly paid to employers without allowing wage reductions below these floors, negotiating pay raises would have been easier.

If the recruitment was on a part-time basis, the subsidy amount was reduced proportionally. In principle, a firm was not allowed to hire subsidized workers in replacement of other dismissed workers in the same function. The PUA monitored this, but given that only 16 out of the 60,000 examined Win-Win contracts were found to be violating this condition (ONEM,

[7] Specific public sector firms could also benefit from the scheme for the hiring of temporary contractual workers, but this represents a negligible fraction of take-up. In our sample, only 1% of hiring with a Win-Win subsidy was realized in the public sector.

[8] Eligibility for the Win-Win subsidy did not require jobseekers to receive benefits during the required periods of unemployment. If the unemployed individual was not claiming benefits, the regional Public Employment Service (PES) had to provide proof to the national PUA that this person was officially registered as an unemployed jobseeker during these periods. This complicates the procedure.

Table 1: Win-Win Hiring Subsidies for Low-Educated Youths and the Long-Term Unemployed Aged Below 45, 2010-2011

	Registration as unemployed jobseeker			Wage subsidy	
Target	during	in the last	Requirements	Amount	Duration
Youth no high school diploma	at least 3 months	4 months	Unemployed jobseeker aged below 26	€1,100/month	24 months (hiring in 2010) 12 months (hiring in 2011)
Youth up to high school diploma	at least 6 months	9 months	Unemployed jobseeker aged below 26	€1,000/month	24 months (hiring in 2010) 12 months (hiring in 2011)
Long-term unemployed	between 12 and 24 months	between 18 and 36 months	Insured unemployed jobseeker	€750/month €500/month	12 months (hiring in 2010 or 2011) + 16 months (hiring in 2010)

2011, p. 154), there are doubts about the extent to which non-compliance could be detected.

Insured unemployed jobseekers who were not eligible for the Win-Win subsidy could be eligible for "Activa", another hiring subsidy that was already in operation before the introduction of the Win-Win plan and which was kept in place. However, Activa was only targeted at long-term unemployment benefit recipients.[9] The subsidy amounted to €500 per month (for a maximum period of 16 months). Since Activa could not be combined with Win-Win, it was only relevant for the individuals not eligible for Win-Win. Both Win-Win and Activa could be combined with pre-existing deductions for employers' social security contributions.[10]

The Win-Win plan was the onset of an unprecedented decline in the cost of hiring low-educated youths, demonstrated by its successful adoption. From January 2010 to December 2011, Belgium saw the completion of 101,000 Win-Win employment contracts. Of these, 47% were allocated to high school dropouts and 23% to high school graduates, with both groups being under the age of 26. The remaining 30% were aimed at the long-term unemployed without any age constraints (ONEm11, p. 87).

In the empirical analysis, we exploit the discontinuity in the subsidy amount that the plan induces at age 26. In our sample of youths registering unemployment in 2010 and taking up a subsidy within one year (see Section 3), we observe that slightly below this age, the subsidy amounted to 41% of wage costs, on average, for high school graduates, and 48%

[9] More than 12 months over the last 18 months for those aged under 25 and more than 24 months over the last 36 months for those older than 25.

[10] These pre-existing measures do not pose a threat to our identification strategy (see Online Appendix C.1).

for dropouts.[11] At age 26, these shares drop sharply to 24% for both group, a decrease of 18 pp for high school graduates and 24 pp for dropouts. The subsidy amounts do not drop to zero, because unemployed aged 26 or older may be eligible for Activa or Win-Win for long-term unemployed if they had accumulated enough months in unemployment. The age-discontinuity at 26 years old therefore results from the sharp decline in the subsidy amount and the more stringent unemployment duration requirement when the low-educated jobseekers reach 26 years. The next sections explain how we exploit this discontinuity to estimate the impact of the Win-Win subsidy on labor market outcomes in the short and long run.

3 Data

The analysis relies on a sample of register data that are collected by various Belgian Social Security institutions and merged into one single database by the Belgian Crossroads Bank for Social Security (CBSS). These data allow reconstructing of individual labor market histories between 2003 and 2017 on a quarterly basis. The sample was originally collected to study cross-border work in Luxembourg from various perspectives. It consists of 125,000 individuals randomly drawn from a stratified population born between December 31, 1972, and December 31, 1990, who lived in Belgium at some point between 2006 and 2017, in a geographical area close to the border with Luxembourg.[12] According to Eurostat (2022), the Belgian Province of Luxembourg was the NUTS-2 region in the EU with the highest incidence of outgoing cross-border workers out of the employed population: 25% in 2010. Within this area, cross-border work is concentrated in the Grand Duchy of Luxembourg. In 2010, 96% of all cross-border work in our sample was to Luxembourg, while in the same year 92% of the total population living in Belgium but working in Luxembourg resided in the sampled areas of the Belgian provinces of Luxembourg or Liège (INAMI, 2010). The particular selection of individuals living close to the border with Luxembourg allows us to study how labor market tightness across borders can moderate the long-term impact of the reinforcement of the hiring subsidy.

[11] See Online Appendix C.4 for a detailed explanation of how these shares are calculated.

[12] Unemployed and individuals living in municipalities closer to the border of Luxembourg were over-sampled to enhance precision for these groups. The data are appropriately reweighted to take this stratification into account and be representative of the population of interest (Manski and Lerman, 1977; Cameron and Trivedi, 2005; Albanese and Cockx, 2019). Details on the sampling can be found in Online Appendix B.

In this sample, we retain first registrations as unemployed jobseekers at the public employment service (PES) between 2007 and 2012 and follow them with quarterly frequency from the start of their unemployment spell. We cannot determine whether a jobseeker satisfies the unemployment duration requirement for a Win-Win subsidy because we only have information about the unemployment status at the end of the month.[13] We therefore only identify *intention-to-treat* effects based on the age requirement for the Win-Win subsidy targeted at youths.

The benchmark analysis in this paper is conducted on 9,935 young adults with at most a high school degree and aged between 22 and 29. Since Win-Win was abolished by the end of 2011, we retain only unemployment spells that started in 2010 to include individuals who do not lose eligibility for the subsidy within their first unemployment year. To investigate spillover effects on ineligible individuals, we include higher-educated and older youths. Additionally, for the placebo analysis and the differences-in-difference (DiD) estimator implemented as robustness analysis, we include entries into unemployment before and after 2010.

We estimate the impact of the wage subsidy reinforcement on several outcomes, grouped into two categories: exit rates to employment within the first year of unemployment, i.e. in the short run, and employment outcomes extending up to seven years later, i.e. in the long run. We focus at first on private sector employment because most public sector jobs were excluded from the Win-Win subsidy. However, to investigate the mechanisms behind the effect on private sector jobs, we also consider employment types *other* than salaried private sector positions, such as public sector employment and self-employment.

In the empirical analysis, we control for predetermined explanatory variables such as gender, nationality, household composition, geographical location, work experience, and receiving unemployment benefits, which are measured at entry into unemployment. These covariates are aimed at increasing the precision of the RDD estimator or relaxing the parallel trend assumption of the DiD estimator, as explained in the next section. Descriptive statistics for the explanatory variables and the outcomes are shown in Online Appendix C.2 and C.3.

As Table 2 shows, around 20% of 22-25-year-olds enter a Win-Win job within a year of

[13] Because eligibility for the hiring subsidy is based on the number of days of unemployment over the last 4 or 9 months, the unemployment status at the end of the month cannot precisely capture this eligibility. Hence, some unemployment entries in our sample may benefit from Win-Win before reaching the eligibility threshold of unemployment duration as defined in our data, i.e. 3 and 6 months, respectively for dropouts and graduates.

becoming unemployed. The subsidy take-up does not differ much between the two levels of education. Take-up can occur only if (i) the unemployed satisfies the unemployment duration requirement of 3 or 6 months at the moment of hiring, (ii) the employer applies for the subsidy, and (iii) there is a formal approval by the PUA (see Section 2). This explains why it is so low.

For other outcomes, education level matters. The probability of starting a salaried private sector job within one year is 58% for eligible high school graduates, compared to 44% for eligible high school dropouts. Similarly, high school graduates worked 12.5 quarters in the private sector over the next seven years, while high school dropouts worked 8.2 quarters. For both groups, this outcome is about 4 quarters higher for those taking the Win-Win subsidy. However, this positive difference favoring Win-Win takers is offset by fewer quarters in other employment forms. The reduction is larger for graduates (2.5 vs. 4.7 quarters) than for dropouts (2.2 vs. 2.5 quarters). This evidence suggests substituting private sector employment for non-private sector employment, for which we provide causal evidence below.

Table 2: Selected Descriptive Statistics: Outcomes

	High school dropouts (22-25)		High school graduates (22-25)	
	All (1)	Win-Win (2)	All (3)	Win-Win (4)
Take-up Win-Win during any month within 1 year	0.19	1.00	0.20	1.00
	(0.39)	(0.00)	(0.40)	(0.00)
Employed in the private sector at the end of any quarter within 1 year	0.44	0.92	0.58	0.93
	(0.50)	(0.27)	(0.49)	(0.25)
Total quarters in the salaried private sector in 7 years	8.17	12.24	12.54	16.74
	(8.87)	(8.58)	(9.86)	(8.82)
Total quarters in other employment in 7 years	2.47	2.24	4.74	2.50
	(5.54)	(4.55)	(7.95)	(5.86)
N	2209	394	2838	520

Notes: Mean and standard deviation of the outcome variables. Different groups by column: (1) dropouts aged between 22 and 25 at unemployment entry, (2) dropout Win-Win takers within one year and aged between 22 and 25 at unemployment entry, (3) graduates aged between 22 and 25 at unemployment entry, (4) graduates Win-Win takers within one year and aged between 22 and 25 at unemployment entry.

We also compare the explanatory variables between young adults who start a subsidized job and those who do not.[14] In comparison to the latter, the former group tends to more commonly be of Belgian nationality, live alone, receive unemployment benefits at registration, have some previous work experience, and have previously benefited from activation policies. This means that the subsidized group is positively selected, and hence, the above descriptive statistics of outcomes cannot be given a causal interpretation.

[14] See Table C.2 in Online Appendix C.3.

4 Identification Strategies and Estimation Methods

4.1 Regression Discontinuity Design

To estimate the causal impact of the wage subsidy reinforcement resulting from the Win-Win plan on the employment trajectories of unemployment entrants, we exploit two eligibility conditions: age and calendar time.[15] Indeed, only jobseekers younger than 26 and recruitments in 2010 or 2011 are potentially eligible for the most favourable Win-Win subsidies. Our benchmark analysis relies on a regression discontinuity design (RDD) estimator that exploits the age eligibility cutoff at 26 for the unemployed registering in 2010. As mentioned in Section 2, workers older than 26 can be eligible for other *lower* hiring subsidies, such as *Activa* or Win-Win targeted at the long-term unemployed. This means that the counterfactual we estimate is not the absence of eligibility but the age-eligibility for less generous hiring subsidies.

Because the age cutoff is not determined at a fixed point in time, but at hiring, we cannot implement a standard RDD using age at unemployment registration as the forcing variable. Youths slightly younger than 26 at entry into unemployment will indeed gradually, along the unemployment spell, age out of eligibility for the higher subsidy for youths in the Win-Win plan. We address this issue as in Gerard and Gonzaga (2021) and ignore observations of individuals aged between 25 and 26 at the start of the unemployment spell. By doing so, we ensure that all individuals younger than 25 years do not age out of their eligibility for a higher subsidy before the end of the first year of unemployment. By dropping these observations, we create a “hole" to the left of the age cutoff of 26, which we fill by the prediction of a linear spline estimated using data points to the left of this “hole".

This approach resembles the so-called *donut* RDD (Barreca et al., 2016), which in the literature is used to solve another identification problem, namely *manipulation* around the threshold. In the latter problem the extrapolation is required at both sides of the discontinuity threshold, whereas here the extrapolation is implemented only on the left-hand side. This extrapolation enables us to identify the intention-to-treat (ITT) effects resulting from meeting the age and

[15] As mentioned in Section 3, unemployment duration’s imperfect measurement in the data prevents using eligibility thresholds based on duration for analysis. Consequently, we identify only intention-to-treat effects due to the satisfaction of the age-eligibility condition.

time eligibility conditions for at least one year after entering unemployment.[16] Details on the formal implementation of the estimator can be found in Online Appendix D, while the validation analysis, such as the balancing and density tests, is shown in Subsection 5.2.1.

In empirical applications implementing an RDD estimator, it has become standard practice to rely on the optimal bandwidth selector of Calonico et al. (2014). However, this selector aims to find the *local* non-parametric estimator that minimizes the mean square error at cutoff. Since we cannot use the observations in the donut to the left of the cutoff, this selector is not well defined. We therefore set the bandwidth ad hoc, at three years for each side of the discontinuity (outside the donut). In Section 5.2, we then test the sensitivity of the results to wider or narrower bandwidths, besides implementing other sensitivity analyses. This shows that the results are robust. Finally, to take into account that the running variable, age, is grouped in monthly intervals, we cluster the standard errors by age in months (Lee and Card, 2008). In the benchmark analysis, this defines 72 clusters. The units are reweighted by using the triangular kernel and the sampling weights to make inference on the population.

4.2 Difference-in-Differences

By applying the donut RDD the treatment effects are no longer completely non-parametrically identified. We, therefore, check whether the main RDD estimates are robust to a difference-in-differences (DiD) estimation strategy.

In the DiD design we contrast the evolution of the outcomes, each measured over a fixed horizon since entry into unemployment, between a treated group (aged between 24 and 25 at entry) and a control group (aged between 26 and 27). We consider two entry periods: 2008 and 2010. These are, respectively, the control period and the intervention period.[17] The treatment applies only to the treated group that entered unemployment in 2010. Those aged 24-25 at entry and registering into unemployment in 2010 are eligible for the Win-Win subsidy for at least one year of unemployment (up to 2 years) because these individuals are younger than 26 during the full period that the Win-Win subsidy was in place (2010-11).[18] By contrast, the members of

[16] In Online Appendix F, we discuss how to estimate the local average treatment effect (LATE) and argue that it represents a less relevant policy parameter in the context of hiring subsidies.

[17] We do not use the unemployed entering in 2009 since they quickly enter the treatment period in 2010.

[18] We do not consider those aged between 25 and 26 at entry into unemployed or unemployment registrations in

the treated group entering in 2008 are never eligible for the subsidy because they are older than 26 by 2010. Individuals in the control group are never eligible for the subsidy because they are already older than 26 from entry into unemployment.

The main identifying assumption is that the counterfactual outcomes in the absence of treatment of the treated and the control group follow parallel trends. A potential threat is that the business cycle has a steeper age gradient on employment outcomes for youths than for prime-aged workers, thereby invalidating the parallel trends assumption. This threat is less forceful here because the age differences between the treated [24-25] and control group [26-27] are only minor (See e.g., Meyer, 1995). In Section 5.2.2, we present a series of validation test that support the credibility of parallel trends. The treatment effect is then estimated by relaxing the parallel trend assumption to hold only conditional on our predetermined control variables. We do this by employing the doubly robust DiD estimator, as proposed by Sant'Anna and Zhao (2020). Further details can be found in Online Appendix E.

5 Empirical Findings

This section reports the empirical results of our analysis in three main sections. First, the findings are discussed based on graphical evidence resulting from the (donut) RDD, which is our benchmark identification strategy. The econometric estimates and associated statistics underlying these graphs are reported in Online Appendix H. Second, we demonstrate that our empirical findings are robust to various validation and sensitivity tests, and that the DiD estimator replicates the estimates. Finally, we present the results of a cost-benefit analysis following the marginal value of public funds framework proposed by Hendren and Sprung-Keyser (2020).

5.1 Main results

We start by documenting the incidence of the Win-Win subsidy on wages and employment in the private sector. We show that this reinforcement significantly enhances the transition to employment within one year of entry into unemployment for both high school dropouts and

2011 as they are potentially eligible for less than one year.

graduates, and that there is little incidence on wages. The effect on private sector employment and unconditional earnings persists beyond the expiration of the subsidy for high school graduates only. In a next subsection, we investigate where the persistent effects for graduates come from. We show that the subsidy reinforcement induces substitution effects between private employment and other precarious forms of employment in unsubsidized sectors and discuss the mechanism that brings this about. Next, we provide evidence that labor market tightness leveraged by the geographic proximity of the economic hub of to Grand Duchy of Luxembourg moderates the treatment effects for graduates. We also investigated whether subsidized employment generated negative spillovers on ineligible older workers. In line with the existing evidence (Blundell et al., 2004; Kangasharju, 2007; Pallais, 2014; Webb et al., 2016; Cahuc et al., 2019; Saez et al., 2021), we do not find evidence of such spillovers. For the latter analysis, the interested readers are referred to Online Appendix E.2.

5.1.1 Incidence of the Hiring Subsidy on Employment and Wages in the Private Sector

The Reinforcement of the Subsidy Conditional on Hiring

We estimate the impact of meeting the age-eligibility criteria for the Win-Win plan on the average subsidy amount *among hired jobseekers*. Two sources of non-compliance must be considered when analyzing this outcome.

First, there is non-compliance because long-term unemployed individuals older than 26 are also eligible for a lower subsidy than those younger than 26. By applying the RDD estimator to the average full-time equivalent subsidy *conditional on subsidy uptake*, we estimate that jobseekers younger than 26 (to the left of the age cutoff) are entitled to a subsidy of €1,056 per month when entering a subsidized job (the average of €1,100 per month for high school dropouts and €1,000 per month for graduates), while older jobseekers (to the right of the cutoff) receive €543 per month (the average of €750 per month for those eligible for the Win-Win subsidy for long-term unemployed and €500 per month for those eligible for the Activa plan). Thus, the additional reinforcement due to age eligibility is €513 per month.[19] Therefore, the comparison of individuals on both sides of the cutoff measures the effect of this reinforcement

[19] See Figure F.1 in Online Appendix F.

at the age threshold, rather than the total effect of the Win-Win subsidy for low-educated youths.

Second, irrespective of age, not all *hired* unemployed are eligible for a subsidy because for some the unemployment duration requirements are not met and for others firms do not comply with the administrative formalities, either because they are not informed or because they find the administrative hurdles too costly.[20] The share of subsidized hires among all hires (i.e., the *"attention rate"*) is 36% to the left of the age cutoff at 26 and 14% to the right.[21] The attention rate is lower to the right than to the left of the cutoff because the subsidy is lower and the eligibility criteria are stricter for youths aged 26 or more.[22] This partial eligibility reduces the expected subsidy that firms receive when hiring these unemployed youths. The average subsidy conditional on hiring on the left and right of the cutoff is 0.36x€1,056 = €380/month and 0.14x€543 = €76/month, respectively. The difference (€304 = €380 - €76) is the average subsidy reinforcement that firms receive for hiring workers slightly below age 26.

Panel (a) of Figure 1 shows the average subsidy received *conditional on being hired* within one year after entering unemployment, binned by age at entry into unemployment grouped into six-month intervals. The linear splines and the discontinuity at the age cutoff are estimated by the donut RDD procedure described in Section 4.1. The donut excludes the red diamonds reporting the expected subsidy for youths aged between 25 and 26 at entry into unemployment. These diamonds are excluded because the corresponding youths are only eligible for the Win-Win subsidy part of the year, i.e. until their 26^{th} anniversary. The linear splines to the left and right cut the age cutoff at the expected subsidy levels mentioned above, i.e. €380 and €76. The difference (€304/month) is highly significant (with a p-value close to 0%), which demonstrates that the treatment is strong. This reinforcement represents 12.7% of the full-time monthly wage cost in the absence of the subsidy reinforcement, as estimated by the wage costs to the right of the cutoff (€2,392) in panel (c).[23] This share does not differ by educational attainment.[24]

[20] Our data do not allow to distinguish between these three reasons.

[21] Online Appendix C.4 explains how these attention rates are estimated.

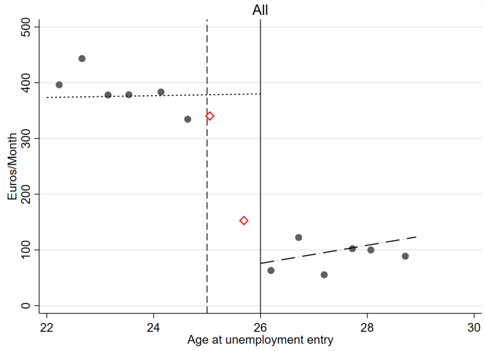
[22] Youths aged 26 or more must be at least one year unemployed to be eligible for the subsidy, while below 26 this requirement drops to 3 and 6 months, respectively for high school dropouts and graduates (see Table 1).

[23] Contrary to the expected subsidy for which the generosity is fixed, this wage cost is possibly a biased estimate of the mentioned counterfactual though we show in the next subsection that this bias is small.

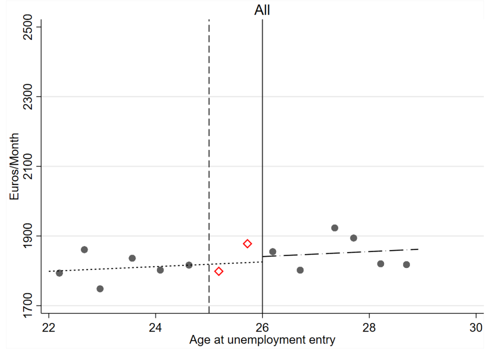
[24] For high school dropouts and graduates the differential monthly subsidy is €297 and €323, respectively (see Figure A.3 in Online Appendix A).

Figure 1: Discontinuity of Employment Outcomes in the Private Sector Within One Year

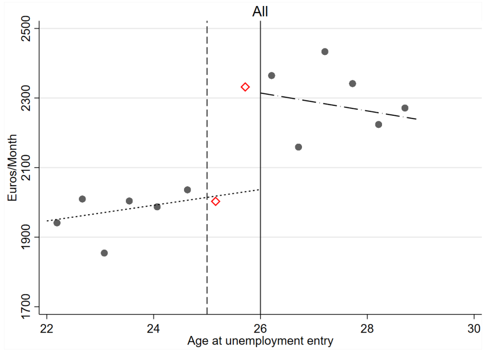
(a) Subsidy Amount Conditional on Employment



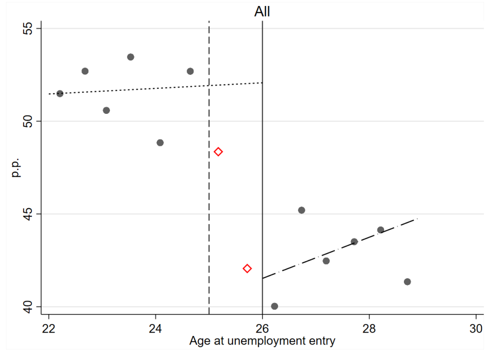
(b) Full-time Gross Wage Conditional on Employment



(c) Full-time Wage Costs Conditional on Employment



(d) Cumulative Transition Rate to Employment



Note: Donut RDD estimate on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. The outcome is (a) the amount of received subsidy (in full-time equivalent) conditional on hiring in the private sector, (b) the full-time gross wage conditional on hiring in the private sector, (c) the full-time wage cost conditional on hiring in the private sector and (d) the cumulative transition rate to private sector employment within one year after unemployment entry, which is plotted over six age-quantile-spaced bins on each side of the donut. The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but by removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. The effect estimated by the donut RDD estimator at 26 years old is (a) €304 [185; 423] with a p-value of 0.000 and N = 3,958, (b) –€15 [–104; 74] with a p-value of 0.733 and N = 3,958, (c) –€277 [–438; –117] with a p-value of 0.001 and N = 3,958 and (d) +10.5 pp [3.0; 18.1] with a p-value of 0.007 and N = 8,560.

The Incidence of the Hiring Subsidy on Wages

The effect of the hiring subsidy on employment depends on the extent to which workers can capture part of the subsidy through higher wages. Panel (b) of Figure 1 displays the RDD for the gross before-tax wage measured by dividing the FTE gross wage of youths hired within the first year since entry into unemployment by the number of quarters in employment. The gross wage does not display any discontinuity at the age cutoff of 26. Even if the subsidy is borne by the worker, there is a complete passthrough to the employer. Nevertheless, it is difficult to directly infer the effect on this outcome as it suffers from a "double selection problem", which may bias the estimates (Heckman, 1974). We obtain some insight into the extent to which this bias matters by comparing the RDD on the wage costs in panel (c) to the RDD on the average subsidy conditional on hiring reported in panel (a) of Figure 1.

Panel (c) displays the RDD on the FTE wage costs including employer's social insurance contributions *net* of wage subsidies. The point estimate is minus €277/month. This amount of cost savings at the cutoff is very close to the average subsidy reinforcement of €304/month estimated in panel (a) (which is by construction not affected by the selection bias mentioned above as the subsidy amounts are exogenously fixed). This suggests that the selection bias is only minor and therefore that the passthrough of the subsidy to gross before-tax wage, if any, is not large. This may be very much linked to the fact that the subsidy allows employers to reduce the wage below the legal or sectoral minimum wage by the amount of the subsidy. This differs from the usual implementation scheme in which the employer receives the subsidy but cannot reduce the wage below these floors (see Section 2).

An explanation for this limited incidence on the workers' wages is that pay scales in Belgium are negotiated in collective agreements covering all workers in a sector, and firm specific wage top-ups are not common. It is therefore difficult to negotiate a different wage for hired workers in a specific age range than for incumbents. However, we cannot exclude that there is wage passthrough shared by all workers in firms. Saez et al. (2019), for example, find no incidence on the net of payroll tax wages due to a cut in employer's social contributions for employed youths in Sweden. However, they do observe positive effects on the wages of incumbents. We do not have the data to check this. Nevertheless, even if such a passthrough exists, it cannot be important because hiring subsidies are targeted at a much smaller group in the firm than a payroll tax cut for all incumbent young workers. To the extent that labor demand is elastic, we expect a positive impact on the hiring rate.

The Effect on Hiring and the Labor Demand Elasticity in the Short Run

Panel (d) of Figure 1 shows the donut RDD estimates for the transition rate to private sector employment one year after unemployment in 2010 are 10.5 pp (significant at the 0.7% level), a 25% increase over the counterfactual. This demonstrates that the subsidy reinforcement for youths positively impacted this rate.

When we split the sample by education, binned data points become more noisy and estimates, therefore, less precise.[25] For high school graduates and dropouts, the point estimates are 8 pp and 13 pp (p-values, 0.25 and 0.02), which correspond to proportional increases of 15% and 40% relative to the counterfactual. However, the finding that the subsidy reinforcement does not significantly affect the hiring rate for high school graduates is not robust. We also report the evolution in the RDD estimates of these cumulative transition rates by educational attainment from one to six quarters after entry into unemployment.[26] This shows that the estimate at four quarters spikes downward for graduates (and upward for dropouts). There is therefore no clear evidence that the effect in pp differs between the two education groups. However, relative to the counterfactual, this implies a bigger effect for dropouts than graduates.

Next, we combine the information of panels (a) and (d) of Figure 1 to obtain an estimate of the employment elasticity with respect to change in wage costs induced by the incremental hiring subsidy, i.e. the labor demand elasticity to the extent that there is no passthrough to employees' wages. When we group the two education groups, this elasticity is estimated by the ratio of the proportional increase in the hiring rate (i.e. 25.4%) to the proportional decline in wage costs (i.e. -12.7%),[27] and is, hence, equal to -2.0.[28] The estimate of this elasticity is in the range reported in several studies evaluating the impact of hiring subsidies targeted at long-term unemployed: -2.5 and -2.2 in the studies of Ciani et al. (2019) and Pasquini et al. (2019) for Italy, and -1.0 for a hiring subsidy for prime-aged long-term unemployed in Belgium (Desiere and Cockx, 2022). By contrast, in Sweden the corresponding elasticities are reported to be lower. They range between -0.2 and -0.6 (Sjögren and Vikström, 2015).[29]

These elasticities only provide a sense of the magnitude of the subsidy effects on hiring in the short run. In the next section we investigate whether these effects persist beyond the end of the subsidy period in the long run.

[25] See see Figure A.4 in Online Appendix A.

[26] See Figure A.5 in Online Appendix A.

[27] This is computed by dividing the subsidy amount effect in panel (a) of Figure 1 (€304/month) by the predicted wage costs at the cutoff in panel (c) (€2,392/month). In Online Appendix C.5, we discuss how adjusting for minor bias in the predicted wage costs has negligible effects on elasticity.

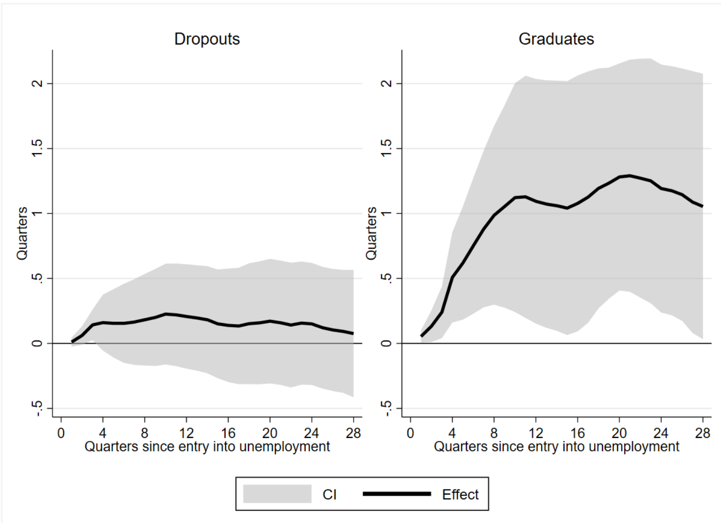
[28] For dropouts it is larger ($40.4\%/(-13.0)\% = -3.1$) than for the graduates ($14.6\%/(-13.1)\% = -1.1$), but in view of the lack of robustness of the effects by educational attainment, these point estimates are less reliable.

[29] These elasticities are not directly comparable to the standard wage elasticities of labor demand (Lichter et al., 2015), or the higher one for hiring subsidies reported by Cahuc et al. (2019). The latter elasticities measure how an increase of wage costs affects employment in firms, while the elasticity that we report here measure how this affects the probability that a worker is hired.

Cumulative Effects in the Long Run

Figure 2 shows, by educational attainment, the evolution of the cumulative number of quarters in *subsidized* employment from entry into unemployment in 2010 until seven years later.[30] This number should attain a maximum around quarter 11 when the Win-Win subsidy expires for all unemployment entries.[31] For high school graduates, the cumulative effect fluctuates after 11 quarters around the same level of slightly more than one quarter per person in this group. A general observation that also applies to the next graphs is that these long-term effects are estimated with considerable imprecision so we cannot say much about the quantitative effect sizes. On the other hand, when we estimate the same model by DiD on the individuals aged between 24 and 25, the point estimates differ hardly.[32]

Figure 2: Evolution of the RDD Effect on the Cumulative Number of Quarters in Subsidized Private-Sector Employment



Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the unconditional cumulative number of quarters in subsidized private sector employment by schooling level: dropouts (left) vs. graduates (right). The RDD estimator is implemented for each quarter after entry into unemployment until 7 years later. The donut RDD estimator removes observations for those aged between 25 and 26 at entry into unemployment and retains units aged between 22 and 25 (left of the cutoff) and 26 to 29 (right of the cutoff). The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. For dropouts (graduates), the effect at quarter 11 is +0.2 quarters [–0.2; 0.6] with a p-value of 0.279 and N = 4,176 (+1.1 quarters [0.2; 2.0], p-value 0.018 and N = 4,384).

[30] This is an unconditional outcome which considers all entrants into unemployment in 2010.

[31] The benefits expire between 4 and 11 quarters after entry into unemployment, depending on the calendar year in which the subsidy is claimed and when subsidized employment begins following the start of unemployment.

[32] See Figure A.6 in Online Appendix A.

The most striking observation is that around the expiration of the subsidy, the cumulative effect of the Win-Win Plan on the average number of quarters in subsidized employment is five times smaller for high school dropouts than for high school graduates. This result implies that subsidized employment tends to be of much shorter duration for dropouts and already suggests that the employment effect in the long run must be small for this group.

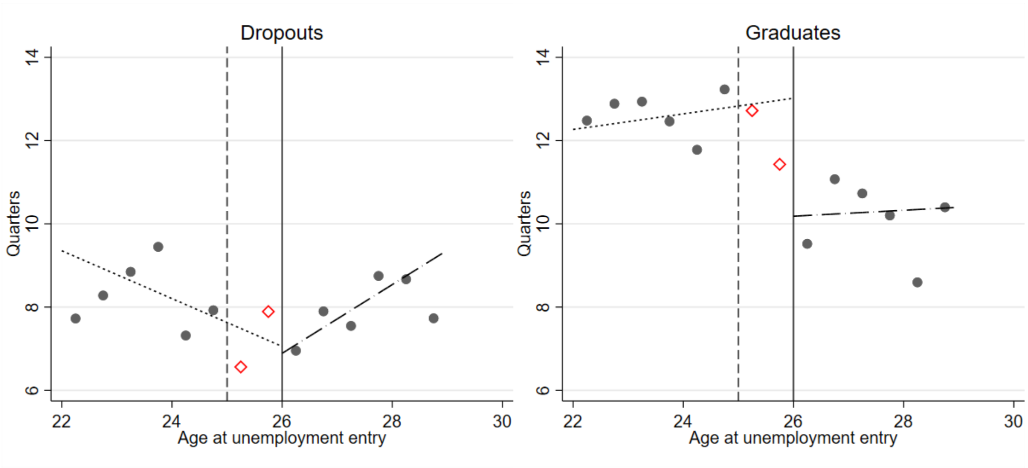
To test this hypothesis, we estimate the effect of the reinforcement by schooling level on the accumulated number of quarters in private sector employment seven years after entering unemployment. From Figure 3, we can deduce that the Win-Win subsidy did not affect the time spent in private sector employment for dropouts at such a long-time horizon. During the first 11 quarters, the average number of quarters in employment do slightly increase, but this effect is not statistically significant. Moreover, after the expiration of the subsidy, the estimated effect gradually drops to zero.[33] This is evidence that for dropouts, the hiring subsidy only accelerates the transition to short-term jobs and does not generate any persistent effect on employment.

In contrast, the panel to the right of Figure 3 shows that the Win-Win subsidy increases the average number of quarters in private employment up to 2.8 quarters seven years after entry into unemployment. This is an increase of 28% relative to the counterfactual of less favorable hiring subsidy conditions. This effect continues to grow beyond the end of the subsidy period and is statistically significant at the 5% level from quarter 12 onwards. We also estimate that the gains are in terms of FTE employment. After seven years, about three FTE quarters of employment are gained, on average (+26%).[34] The effects on gross wage earnings (assigning zero earnings to those who are not employed in the salaried private sector) follow a similar pattern to those on the number of quarters spent in private sector employment: no effect for dropouts, and for graduates, a steady increase until €14,600 after 5.5 years, beyond which the effect stabilizes.[35] Relative to the counterfactual, the increase after 7 years is 29%, which is only slightly larger than the effect in full-time equivalent quarters (26%). Combining these two pieces of evidence suggests that in the long run, the subsidy does not have a significant impact on the wage rate. This is corroborated by the effect estimates on the average full-time

[33] In Figure A.7 in Online Appendix A, we present the evolution of the RDD effects.
[34] See Figure A.8 in Online Appendix A.
[35] See Figure A.9 in Online Appendix A.

equivalent (FTE) gross wage over time, which indicate an effect that is consistently close to zero.[36] However, these wage effects are conditional on employment and therefore subject to the selection bias mentioned above.

Figure 3: Discontinuity at Age 26 for the Cumulative Number of Quarters in Private Sector Employment Seven Years after Entry into Unemployment



Note: Donut RDD estimate on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. The outcome is the cumulative number of quarters in private sector employment seven years after entry into unemployment by schooling level (dropouts vs. graduates), which is plotted over six age-quantile-spaced bins on each side of the donut. The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. The effect estimated by the donut RDD estimator at 26 years of age is +0.2 quarters [–2.4; 2.7] (p-value 0.897) and N = 4,176 for dropouts, while for graduates it is +2.8 quarters [0.7; 5.0] (p-value 0.011) and N = 4,384.

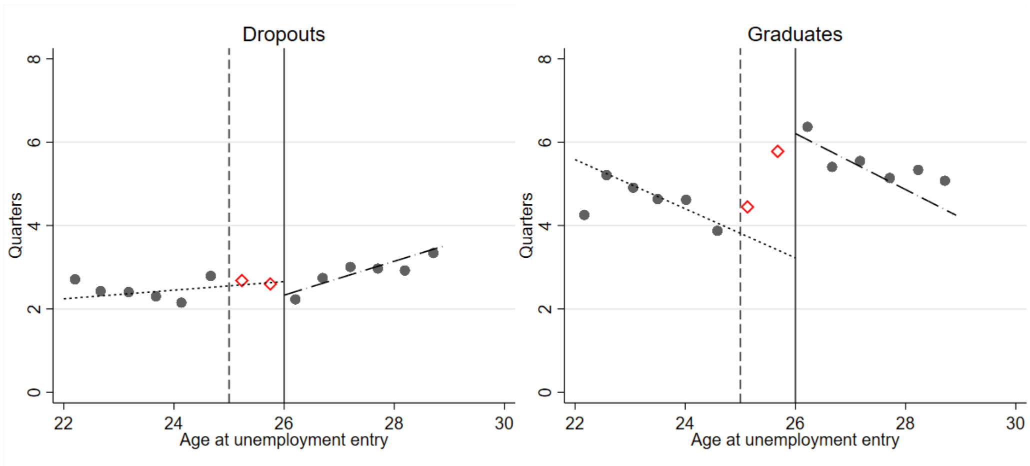
These findings suggest that "work-first" policies can be effective in the long run for high school graduates. The fact that the subsidy reinforcement does not entail any effect for high school dropouts is in line with the literature that argues that a minimum skill level is required for initiating a process of human capital accumulation on the job (see, for example, Card and Hyslop, 2005; Autor and Houseman, 2010; Cahuc et al., 2021). However, the mechanism is more complicated. In the next section we demonstrate that the job creation in the private sector for high school graduates comes at the expense of low-paid employment in other sectors.

[36] See Figures A.10 and A.11 in Online Appendix A.

5.1.2 Mechanisms

Figure 4 displays the donut RDD effect at the age cutoff of 26 years on the cumulative number of quarters in non-private sector employment, i.e., public sector employment and self-employment, seven years after entry into unemployment. For high school graduates, one can see that the plot displays opposite effects to the ones showed in Figure 3 for private employment. The point estimate is significantly negative and equal to –2.6 quarters. For dropouts it is slightly positive (+0.5) but non significantly. The overall effect on total employment is therefore very close to zero after 7 years for both dropouts and graduates.[37]

Figure 4: Donut RDD Plot of the Effect on the Cumulative Number of Quarters in Non-Private Sector Employment 7 Years After Entry into Unemployment



Note: Donut RDD estimate on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. The outcome is the cumulative number of quarters in non-private sector employment 7 years after entry into unemployment by schooling level (dropouts vs. graduates), which is plotted over six age-quantile-spaced bins on each side of the donut. The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. The effect estimated by the donut RDD estimator at 26 years of age is +0.5 quarters [–0.6; 1.5] (p-value 0.375) and N = 4,176 for dropouts, while for the graduates it is –2.6 quarters [–4.7; –0.6] with a (p-value 0.012) and N = 4,384.

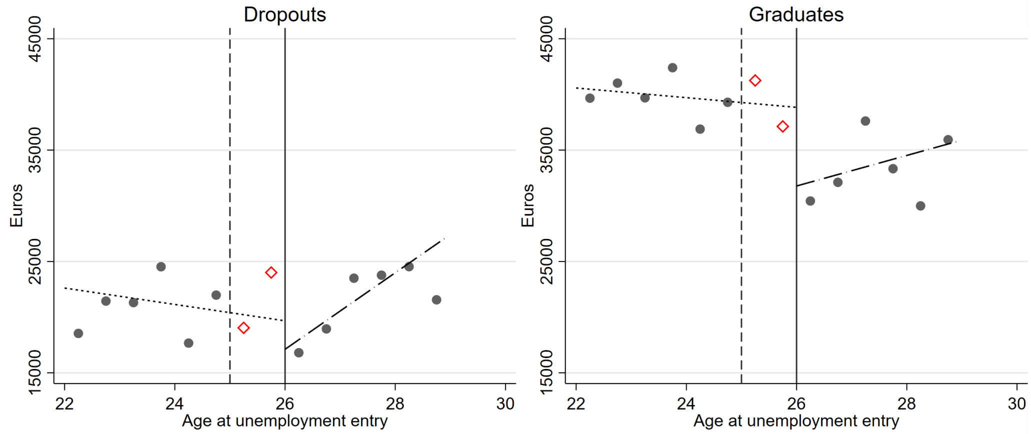
For high school graduates, 80% of the negative effect on non-private sector employment comes from reduced public employment (–2.1 quarters) and 20% from self-employment (–0.5 quarters).[38] An important observation is that this substitution only appears gradually after a couple of years. If we backtrack on cumulative transition rates in the short run (10.5 pp -

[37] See Figures A.12 in Online Appendix A.

[38] See Figures A.13-A.14 in Online Appendix A.

see Subsection 5.1.1), the enhanced job finding rate in the private sector comes from fewer youths remaining unemployed. No negative effect on the transition to self- or public sector employment is observed in the first year after entry into unemployment.[39]

Figure 5: Discontinuity at Age 26 for the Cumulative Labor Income Tax Revenue Seven Years after Entry into Unemployment



Note: Donut RDD estimate on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. The outcome is the cumulative tax revenue from labor income seven years after entry into unemployment by schooling level (columns: dropouts vs. graduates), which is plotted over six age-quantile-spaced bins on each side of the donut. The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. The effect estimated by the donut RDD estimator at 26 years of age is €2,558 [–7,561; 12,677] with a p-value of 0.616 and N = 4,176 for dropouts, while for graduates it is €7,063 [306; 13,820], with a p-value 0.041 and N = 4,384.

Even if the negative impact on non-private sector jobs offsets the positive effect on private sector employment, it is not a zero-sum game. The subsidy reinforcement increases the overall cumulative gross wage (unconditional on employment) over seven years by €5,651, although statistically insignificant (CI [–7,242; 18,544], p-value 0.385), and the paid labor income taxes, including employer and employee social security contributions, by €7,063 (CI [306; 13,820], p-value 0.041 - Figure 5).[40] This suggests that the earnings in the created private sector jobs are higher than in the substituted non-private sector jobs either because they pay less or offer a shorter working time. Looking deeper reveals that substituted public jobs are mostly contractual jobs operated by local authorities,[41] which are, for the low-skilled population, essentially in

[39] See Figures A.15 in Online Appendix A.

[40] We would expect the effect on gross wages to be larger than the effect on paid labor income taxes, but this inconsistency may be attributed to the large imprecision in the estimates.

[41] See Figure A.16 in Online Appendix A.

fields such as public works, construction, electromechanics and forest work (IWEPS, 2015). Such occupations are also in demand in the private sector, and, hence, substitutable. Unlike civil servants, who benefit from better conditions, contractual workers are employed with an open ended or fixed contract as in the private sector and often only work part-time.[42]

Regarding salary conditions, there is one additional piece of evidence indicating that the additional workers in the private sector gave up low-paying jobs. For each of the public and self-employed sectors separately, we compute the RDD estimator on the average quarterly earnings over seven years (conditional on employment in one of the two sectors). These increase by €2,500 (p-value = 0.025) in public jobs, and by €1,065 (p-value = 0.070) in self-employment.[43] This suggests that reallocated workers from unsubsidized to subsidized private firms earned less in the public (self-employment) sector than their nondisplaced counterparts. In the public sector these lower earnings are induced by lower hourly wages because the reduction of FTE employment in this sector is 1.9 which is very close to the reduction mentioned above of 2.1 quarters. The substituted self-employment cannot be decomposed in a similar way. We notice, however, that the gross earnings of the self-employed in the counterfactual of no subsidy reinforcement are very low: €870/quarter on average, which is in line with the evidence that in the last 20 years solo self-employment with low pay and social protection is on the rise in OECD countries as an intermediate category between employment and unemployment (Boeri et al., 2020). Lastly, since in Subsection 5.1.1 we showed no effect on FTE gross wages in the private sector (conditional on private sector employment), this implies that the reallocated workers receive similar pay conditions in the private sector as their counterparts.

Bringing these pieces of evidence together leads us to conclude that the reinforced hiring subsidy attract workers who would otherwise have found employment in low-paying jobs in unsubsidized sectors. In Belgium, private sector jobs are rationed because wages are fixed in sectoral collective agreements which include wage floors that differ by sector and that are usually binding for low-skilled youths. The reinforcement of the hiring subsidy relaxes the rationing and allows low-educated youths to enter higher-paying jobs in the private sector. In

[42] See Figure A.17 in Online Appendix A for the effect on earnings in the public sector.

[43] Estimate on average earnings at 7-year distance in (i) public sector jobs: €2,500, CI [330, 4,668], p-value 0.025, N=965; and (ii) self-employment: €1,065, CI [–93, 2,224], p-value 0.071, N=552.

the absence of the policy, most of these workers stay unemployed and would transit only after a while to a low-paying job in local public administrations and self-employment. This explains why we do not observe any substitution in the first year after unemployment entry.

These findings suggest that private sector jobs can crowd out lower-paying local public sector jobs. This goes against the existing literature that usually finds evidence for crowding out in the other direction (Algan et al., 2002; Caponi, 2017; Fontaine et al., 2019). The fact that in our context the subsidized private sector jobs are the “good" jobs and the substituted local public sector jobs are the “bad" jobs, while this is usually the reverse, may explain this opposite finding. The findings for high school graduates are also consistent with both signaling and human capital theory. On the one hand, the hiring subsidy reinforcement seems to provide an opportunity for some youths who would otherwise have been initially unemployed and in the long run ended up in low-paid self-employment or public sector jobs to reveal their abilities for better paid jobs in the private sector (Pallais, 2014). On the other hand, we cannot exclude that part of the long-run earnings gains for these workers is due to more intensive on-the-job training in these private sector positions, gradually building up thereby their earnings capacity in the long run (Ben-Porath, 1967; Blinder and Weiss, 1976; Mroz and Savage, 2006).

5.1.3 Moderation by Labor Market Tightness

In this subsection, we investigate the moderating role of labor market tightness, which is induced by the geographic proximity to the economic hub of the Grand Duchy of Luxembourg.

With a population of only 635,000 inhabitants, Luxembourg is one of the smallest countries in the European Union as well as one of the wealthiest. This is to a large extent related to the historically low corporate and personal tax rates that have attracted many multinational companies and led to the settlement of a large financial center. In this way, the country has developed into an economic hub in the region, offering more and better-paid employment opportunities. For example, in 2016 the net salary of Belgian cross-border workers aged 25-43 living in the Province of Luxembourg was 63% higher than that of local workers (Albanese et al., 2022). Due to this large economic asymmetry and the absence of language and legal barriers,[44] in

[44] French is a common official language on both sides of the border, and the freedom of movement has existed since 1944 when the Benelux customs union was founded between Belgium, The Netherlands, and Luxembourg.

2010 about 38,000 workers living in Belgium crossed the border to work in Luxembourg. This represents 11% of total employment in Luxembourg (Statec, 2022), and 25% of employment in the nearby Belgian Province of Luxembourg (Eurostat, 2022).

A consequence of the proximity of such an economic attraction pole is that the labor market is much tighter close to the border with Luxembourg than farther away. To that purpose, we divide the sample based on whether the jobseekers lived either within or beyond a 60-minute driving distance from the border.[45] In 2010, the employment rate for youths aged 25-34 living closer to the border with Luxembourg was 14 percentage points higher than those living farther away (77% versus 63%), and the unemployment rate was only half as high (10% versus 20%).[46]

We then look at the differential amount of subsidy received for hiring jobseekers at different border distance. We estimate that close to the border high school dropouts (graduates) finding a job within one year after unemployment entry are entitled additionally to €414 (€300) due to the subsidy reinforcement.[47] Afterwards, we display the long-run evolution of the RDD effect on the number of quarters in private sector employment in Figure 6 panel (a). It can be seen that this effect is not significantly different from zero. This means that close to the border the hiring subsidy is, as expected, a complete deadweight. In contrast, for jobseekers living further away, the subsidy reinforcement grants €249 (€377) more to the dropouts (graduates). This raised, for graduates only, employment by 3.7 quarters seven years after entry into unemployment (panel (b)). This represents a proportional increase of 38% relative to those older than 26, and larger than the overall effect for the full population reported in Figure 3.[48]

We also report the effect on the accumulated time spent in private sector employment estimated from a model in which we interact the splines and the treatment indicator of the donut RDD estimator with the travel distance from the border with Luxembourg, instead of splitting

[45] We use a 60-minute threshold since this is the observed median value in our sample. Furthermore, as shown in Figure A.18, the share of cross-border workers decreases consistently up to 60 minutes, after which it remains flat and close to zero. Information on the average commuting time by car from the neighborhood of an individual to the closest access point in Luxembourg is retrieved from TomTom data (date of reference: 28-05-2019, arrival at 9:00 am – `https://developer.tomtom.com/products/data-services`).

[46] The share of cross-border workers out of the total population near (far from) the border with Luxembourg was 21% (1%). These statistics are based on our calculations. We do not include individuals younger than 25 because a high fraction of these is still in education.

[47] See Figure A.19 in Online Appendix A.

[48] In Figures A.20-A.21 in Online Appendix A, we present the evolution of the RDD effects over seven years after entry into unemployment by border distance.

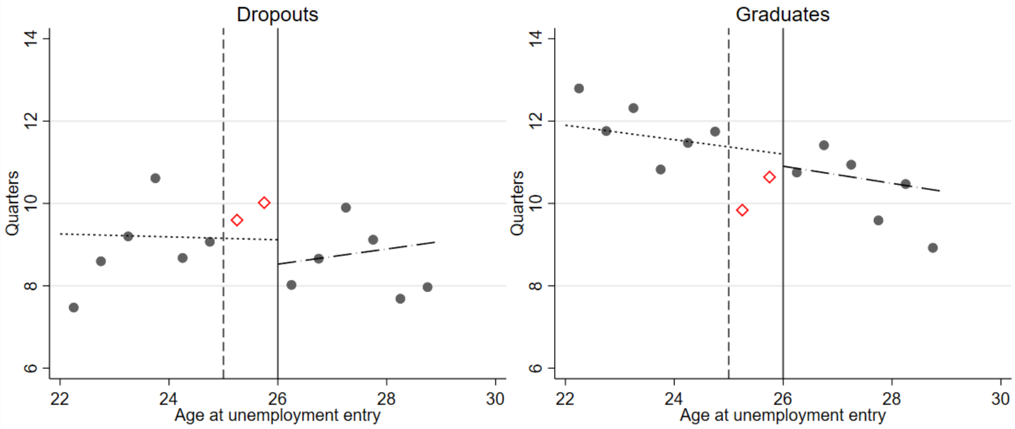
the sample into subgroups.[49] By predicting the effect over the distance for high school graduates, in linear and quadratic specifications, we see that the treatment effect becomes significant only from about 40 minutes from the border. Below this threshold, the effect is never significantly different from zero. Above it continues to increase, but the quadratic specification shows that it levels off beyond 60 minutes from the border. The corresponding effects for high school dropouts confirms that for this group, the effects are close to zero for any travel distance.[50]

[49] See Figures A.22 and A.23 in Online Appendix A.

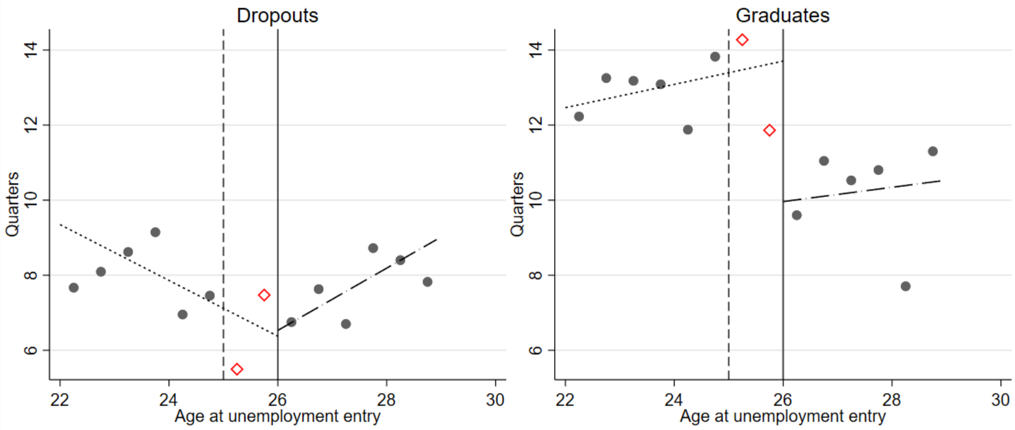
[50] We also estimate a similar interactive model for high school graduates with the number of quarters of cross-border work 7 years after unemployment as the outcome. As shown in Figure A.24 in Online Appendix A, no significant reduction in cross-border work is found.

Figure 6: Discontinuity at Age 26 for the Cumulative Number of Quarters in Private Sector Employment Seven Years after Entry into Unemployment – By Distance to the Border

(a) Close to the border



(b) Far from the border



Note: Donut RDD estimate on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. The outcome is the cumulative number of quarters employed in private sector employment seven years after entry into unemployment by schooling level (dropouts vs. graduates) by driving distance to the border: below one hour (a) or above one hour (b), which is plotted over six age-quantile-spaced bins on each side of the donut. The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. The effect estimated by the donut RDD estimator at 26 years of age is for dropouts (graduates) close to the border: 0.6 quarters [–2.0; 3.2] with a p-value of 0.653 and N = 1,443 (+0.3 quarters [–1.9; 2.4], p-value 0.786 and N = 1,939), while for dropouts (graduates) far from the border it is –0.2 quarters [–3.3; 3.0] with a p-value of 0.921 and N = 2,636 (3.7 quarters [0.7; 6.8], p-value 0.016 and N = 2,432).

The Belgian hiring subsidy does not induce firms to create new jobs close to the border with Luxembourg because most of the productive workforce is already employed or prefers working in that country. On the Belgian side of the border, vacancies are to a large extent for replacement hiring in essential occupations and not for new job creation. In our data the share of Belgian private sector employment out of the total employment is smaller close to the border with

Luxembourg than farther away[51] and those jobs tend to be in low-status occupations.[52] These stylized facts suggest that local employment is indeed in essential occupations, allowing those at home to get what they need day-to-day and for which labor demand is relatively inelastic.

Kline and Moretti (2013) argue that in a tight labor market where there is excessive job creation, subsidizing hires is inefficient because vacancies crowd each other out. We are the first to demonstrate empirically that labor market tightness can moderate the effectiveness of hiring subsidies, even if vacancy creation is across the border in firms that are not eligible for the hiring subsidy. The reason is that in the absence of mobility barriers, the labor market tightness in Luxembourg extends across the border into Belgium.

5.2 Additional Analyses

5.2.1 Sensitivity tests

In this section, we report validation tests.[53] First, we rerun the one-sided donut RDD estimator widening or narrowing the bandwidth. The results are close to the benchmark estimates. Second, we remove the conditioning variables from the RDD estimator and again obtain similar estimates. Third, we test the sensitivity of the results on the effect of the subsidy near the border by reducing the distance to 45 or 30 minutes by car, which show very similar results. Finally, we let the spline on the right of the donut predict the outcome inside the "hole" and estimate the treatment effect at age 25. Estimates are similar to those for individuals aged 26.

We also implement three placebo tests for the donut RDD estimator. First, we estimate whether we detect any statistically significant jump at age 26 for individuals entering unemployment before the introduction and after the abolition of the Win-Win plan (2008 and 2012). Second, we check whether at the age of 26 we find a significant discontinuity in the outcomes for the unemployed with a tertiary degree, who were not eligible for the Win-Win subsidy. Third, we implement several placebo tests that use false cutoff points of the forcing variable. Fourth, we apply the donut RDD estimator to detect jumps in the control variables at the discon-

[51] This was 42% (61%) within (beyond) 60 minutes driving distance from the border for workers aged 25-34.

[52] Our data show that near (far from) the border, 47% (38%) of jobs in the private sector are blue-collar jobs, 32% (26%) are part-time jobs, the average gross full-time daily salary is €100 (€104), 42% (32%) of jobs are in firms with fewer than 20 employees, and 16% (28%) are in firms with more than 500 employees.

[53] See Figures A.25-A.41 in Online Appendix A and Table H.1 in Online Appendix H.

tinuity. These placebo tests deliver insignificant estimates. Finally, we implement the density test done in the empirical literature (Cattaneo et al., 2020) with or without the donut. Both tests show a non-significant change of density at the discontinuity. Overall, all of these validation tests confirm the reliability of the treatment effect we have found for our treated population.

5.2.2 Difference-in-differences

In this subsection, we exploit the time variation in eligibility introduced by the implementation of the Win-Win reinforcement plan in 2010 and its subsequent abolition in 2012. This approach allows us to control for time-invariant unobserved heterogeneity between youths below and above age 26, under the parallel trend assumption of the difference-in-differences (DiD) estimator. This validation analysis confirms that the subsidy reinforcement has a long-term impact exclusively for high school graduates who entered unemployment during the reform period; this impact is not observed in other entry cohorts, either before or after the reform.[54] Moreover, the effect does not extend to other educational groups, such as high school dropouts and ineligible post-secondary graduates, which we use as a placebo group to test for the presence of age-specific time trends. Additionally, the magnitude and evolution of these long-term effects for high school graduates closely align with those identified in the RDD, further enhancing the credibility of our findings as discussed in the main results section.

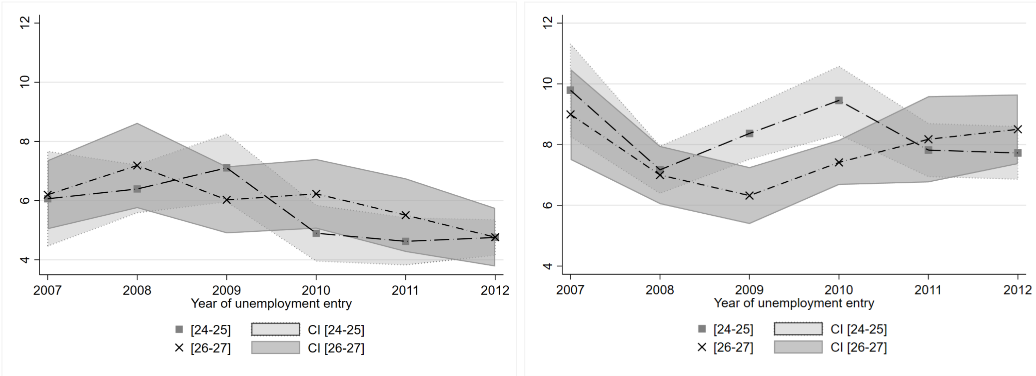
First, Figure 7 shows the evolution of cumulative number of quarters in private sector employment five years after entry into unemployment by entry cohort.[55] We do not observe any statistically significant difference between jobseekers aged 24-25 (treated) and 26-27 (controls) entering unemployment during either the pre-treatment period (2007 and 2008) or the post-intervention period (2012). For the entry cohorts of 2009 (treated in 2010) and 2010, we observe a jump in the outcomes only for the treated high school graduates, which reabsorbs when the subsidy is phased out in 2011.

[54] This specificity is reassuring considering that business cycle conditions in 2008 and 2012 were significantly different. If our findings were merely artifacts of these varying business cycle conditions, then the long-term effects for these two placebo cohorts (pre- and post-reform) would also be expected to differ.

[55] We stop the outcome at 5 years after entry into unemployment because this is the maximum observational period for the 2012 entries. See Figures A.42 and A.43 in Online Appendix A for the other cumulative outcomes.

Figure 7: Evolution over Entry Cohorts and Schooling Level of the Cumulative Number of Quarters in Private Sector Employment

(a) Dropouts (b) Graduates



Note: Evolution of the cumulative outcomes measured 5 years after entry into unemployment on the cumulative number of quarters in private sector employment by entry cohort and schooling level: dropouts (left) vs. graduates (right). The treated are aged 24-25 at unemployment entry, while controls are aged 26-27. The 2009 cohort is not used as control period since jobseekers registering in 2009 quickly enter the subsidized treatment period. Data are reweighted by the sampling weights.

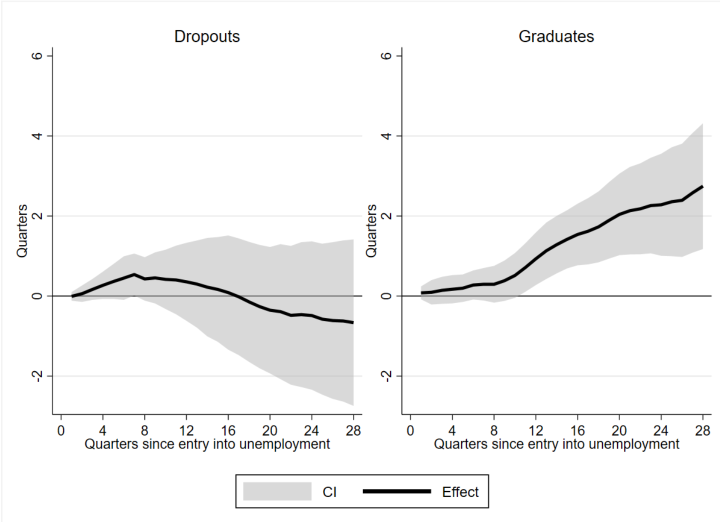
Second, we look at the evolution over three entry cohorts (2008, 2010 and 2012) and three education groups (high school dropouts, high school graduates, and post-secondary graduates, which we use as a placebo group) of the cumulative number of quarters in private sector employment, varying the elapsed time since entry into unemployment. Among all entry cohorts and education groups, we observe a significant difference only for the 2010-cohort of high school graduates. This finding remains robust to an IPW estimator controlling for compositional differences in observable characteristics.[56]

Finally, we formally estimate the treatment effect by implementing the doubly robust DiD estimator as explained in Section 4.2 and Online Appendix E. The results closely resemble those obtained by implementing the RDD estimator and are displayed in Figure 8. Importantly, the DiD estimator also replicates the effects on other outcomes and the heterogeneous response found for individuals living near or far from the border with Luxembourg.[57] Validation analyses on the trends during the pre-treatment periods and the unaffected placebo group of post-secondary graduates are presented in Online Appendix E.1.

[56] See Figures A.44-A.45 in Online Appendix A.

[57] See Figures A.46-A.48 in Online Appendix A.

Figure 8: Evolution of the DiD Effect on Cumulative Number of Quarters in Private Sector Employment Over Calendar Time at Registration



Note: Evolution of the effect estimated with a doubly robust DiD estimator (Sant'Anna and Zhao, 2020) and confidence intervals (CI) for the cumulative number of quarters in private sector employment since unemployment and by schooling level: dropouts (left) vs. graduates (right). The DiD estimator is implemented for each year after entry into unemployment until 7 years later. The treated are aged 24-25 at unemployment entry, while controls are aged 26-27. Units registering in (2008) 2010 are considered in the (pre-) treatment period. Data are reweighted by the sampling weights. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. For dropouts (graduates), the effect at 7 years is -0.7 quarters [–2.7; 1.4] with a p-value of 0.532 and N = 1,942 (+2.7 quarters [1.2; 4.3], p-value 0.001 and N = 1,839).

5.2.3 Cost-Benefit Analysis

In this final section, we implement a cost-benefit analysis of the subsidy reinforcement following the marginal value of public funds (MVPF) framework proposed by Hendren and Sprung-Keyser (2020) and designed for measuring the long-run effectiveness of policies. The MVPF is the ratio of the beneficiaries' marginal willingness to pay (WTP) for the reinforcement of the hiring subsidy to the net marginal cost (NC) to the government of this policy inclusive of any behavioral impact on the government budget. The MVPF can range from negative to positive values. It is convention to set it to plus infinity ($+\infty$) whenever the net cost to the government is negative and the WTP positive because in this case the policy has value (WTP>0) and finances itself (NC<0). We refer to Online Appendix G for details.

Overall, the Win-Win plan did not seem to impose much cost on the government, and for high school graduates, the reform may even have paid for itself, which is driven by the higher tax contributions. The overall MVPF for dropouts is 4, while for the graduates, it is $+\infty$. The policy is also more likely to be self-financing in areas of low labor market tightness. The point estimates of the MVPF are equal to $+\infty$ and 1, respectively. The finding that the hiring subsidy could be self-financing aligns with Cahuc et al. (2019), who report that the short-run net cost per created job of the hiring credit during the Great Recession in France equals zero. Our evidence indicates that, in the long run, the net cost to the government even becomes negative. A word of caution on these results, however, is needed as the confidence intervals of the MVPF are very wide. They always encompass zero and plus infinity, implying that we require a larger sample to make this cost-benefit analysis informative for any policy conclusions.

6 Conclusion

In this paper, we evaluate the employment effects of a temporary reinforcement of a hiring subsidy targeted at low- and medium-skilled unemployed youths during the recovery from the Great Recession in Belgium. A primary objective of this paper is to uncover to what extent such targeted and temporary hiring subsidies can be effective in reversing the long-term scarring effects that recessions can have on young workers. We contribute to the existing literature by focusing on long-term effects, considering the substitution of low-paying local public jobs and self-employment with subsidized private sector jobs, and studying the moderating effects of labor market tightness. The mentioned moderation could be identified by the specific sample that was drawn from a region close to the border with Luxembourg, a prosperous economic hub that attracts substantial cross-border work from Belgium. The main causal analysis exploits an eligibility age cutoff of 26 years for the hiring subsidy and is based on a one-sided donut regression discontinuity design (Gerard and Gonzaga, 2021) to estimate the intention-to-treat effect. The qualitative findings are robust to using an alternative identification strategy, i.e., the doubly robust semi-parametric difference-in-differences method of Sant'Anna and Zhao (2020) with treatment and control groups defined closely around the aforementioned age cutoff.

We show that the subsidy reinforcement accelerates job-finding in the short run by about 10 percentage points for both skill-level groups. The subsidy generates persistent employment effects exclusively for high school graduates. Seven years after entry into unemployment, high school graduates have accumulated about three quarters more employment in the private sector than in the counterfactual of eligibility for a substantially lower hiring subsidy. While employment gains in the short run stem from a reduction in unemployment, in the longer term, these are primarily due to a decrease in low-paying employment in the unsubsidized sectors, such as self-employment and public sector jobs. However, this substitution creates better career prospects for high school graduates who might otherwise be trapped in low-paid jobs. Our analysis also reveals that the tight labor market induced by the presence of the neighboring employment hub of Luxembourg across the border results in a complete deadweight loss for the creation of private sector jobs in an area near the border. The cost-best analysis suggests that the reinforcement of the hiring subsidy could pay for itself, especially for high school graduates living far from the border with Luxembourg. But this finding requires corroboration because of the lack of precision of the estimates emerging from this analysis.

Our results imply that targeting a pure hiring subsidy at high school dropouts during the recovery from a recession can at most accelerate the transition to temporary jobs and cannot persistently improve the labor market position of this group. The absence of such effects might be linked to the short duration and the low skill requirements of jobs that are available for dropouts. A minimum skill level seems to be a condition for the effectiveness of "work first" policies. For high school graduates, our policy conclusions are more positive. Even if the reform did not increase overall employment, it did open up the opportunity for youths to substitute in the long run better-paid jobs in the private sector for low-paid jobs in the local public sector and self-employment. Nevertheless, policymakers should be aware that these beneficial effects are only evident in slack labor markets. Furthermore, they should acknowledge that the inefficacy associated with labor market tightness, in the absence of mobility restrictions, can be influenced by conditions across the border. In a scenario marked by pressure on public spending, identifying factors that optimize the cost-effectiveness of public policies should be a top priority for both policymakers and researchers.

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Long-Term Effects of Hiring Subsidies for Low-Educated Unemployed Youths*

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Online Appendix

This Online Appendix contains additional figures related to the manuscript in Section (A), a description of the stratified sampling procedure in Section (B), an expanded description of the institutional context and descriptive statistics in Section (C), details on the regression discontinuity design estimator in Section (D), detailed information and results on the difference-in-differences estimator by Sant'Anna and Zhao (2020) in Section (E), an illustration of how the local average treatment effect (LATE) can be calculated in Section (F), details on the cost-benefit analysis in Section (G), and tables presenting the estimated treatment effects in Section (H). Additional estimation results are available from the authors upon request.

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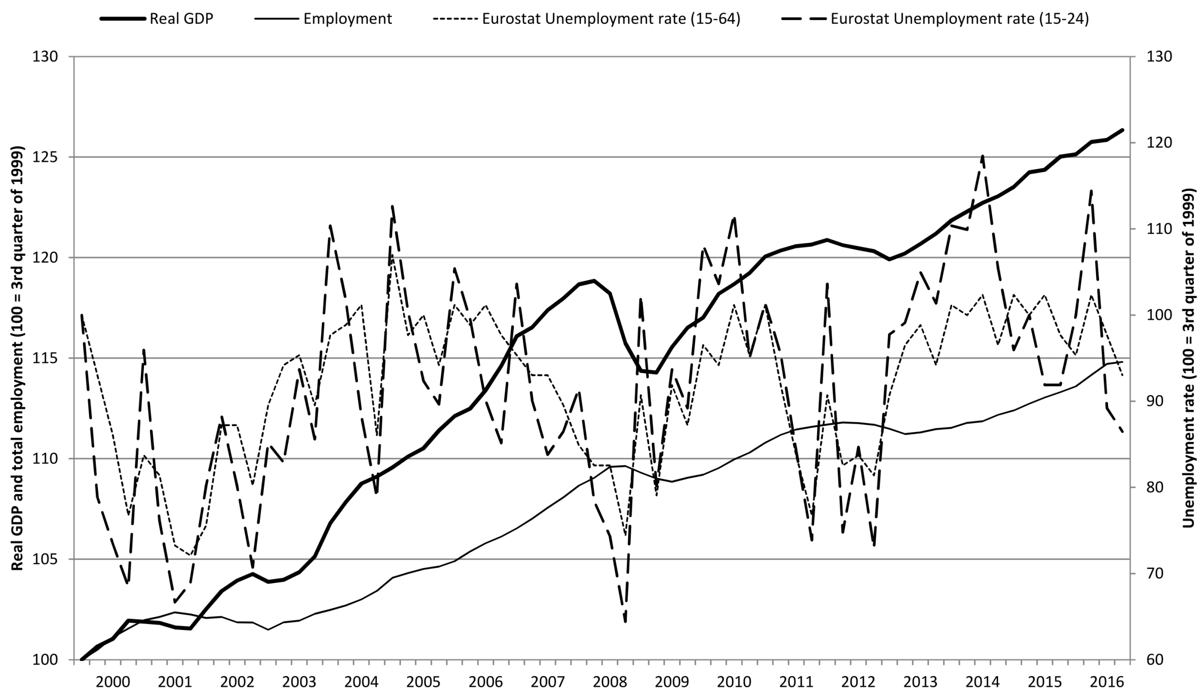
A Figures

Figure A.1: Google Search Index "Plan Win-Win"

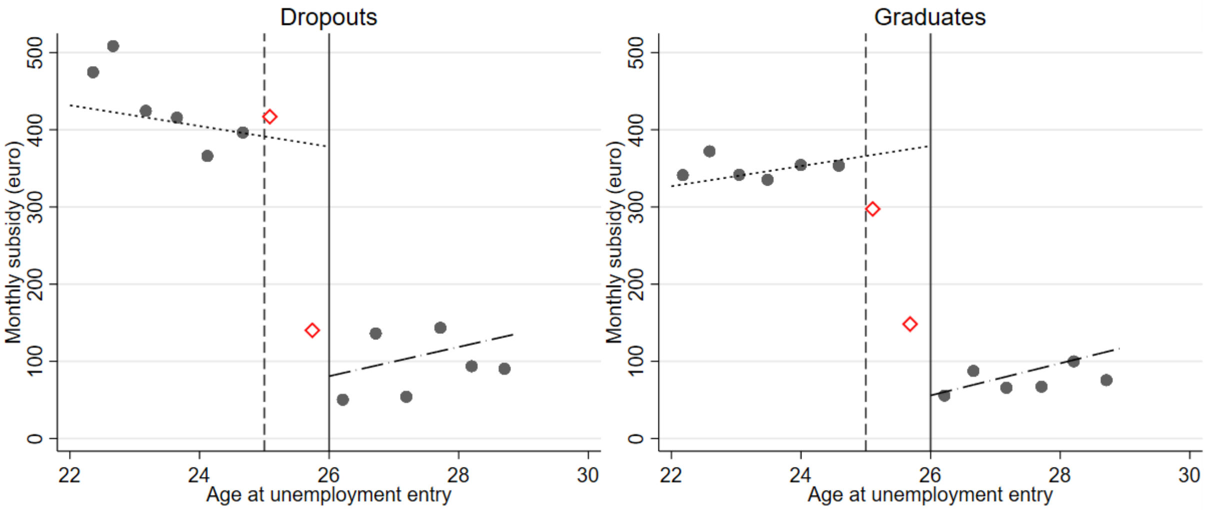
Google Trends search for "Plan Win-Win" from 1st of January 2009 until 31st of December 2010. The red line corresponds to the week before the 18th of January 2010.

Figure A.2: Real GDP, domestic employment, and unemployment in Belgium



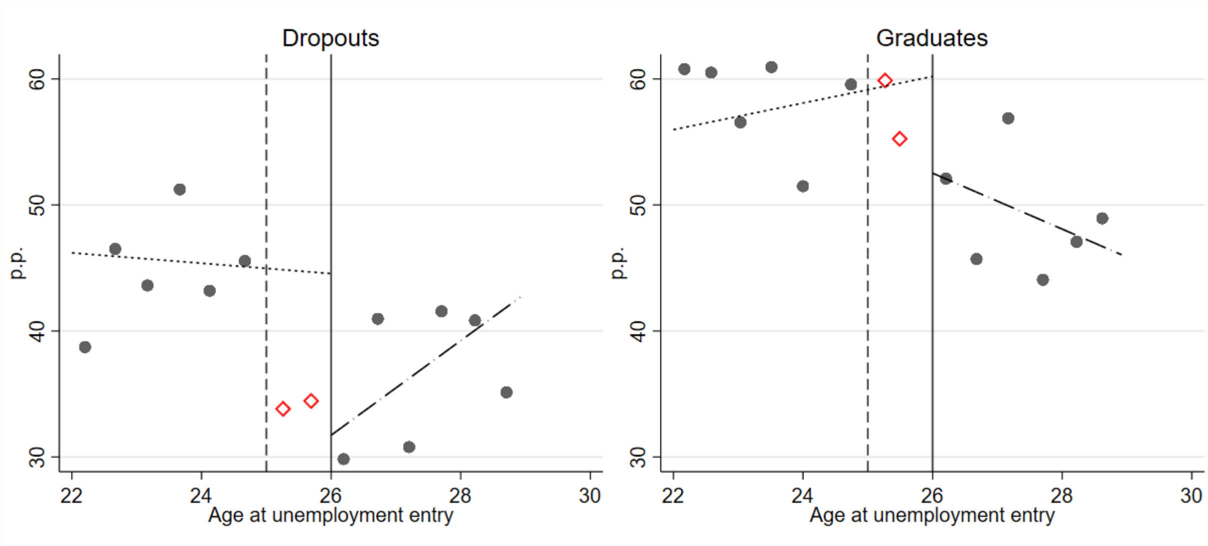
Real GDP and employment: National Bank of Belgium statistics. National accounts. Online at: http://stat.nbb.be/; Unemployment rate: Eurostat. Online at: http://ec.europa.eu/eurostatescry/data/database.

Figure A.3: Discontinuity at Age 26 of the Average Subsidy Amount Conditional on Employment Within One Year by Schooling Level



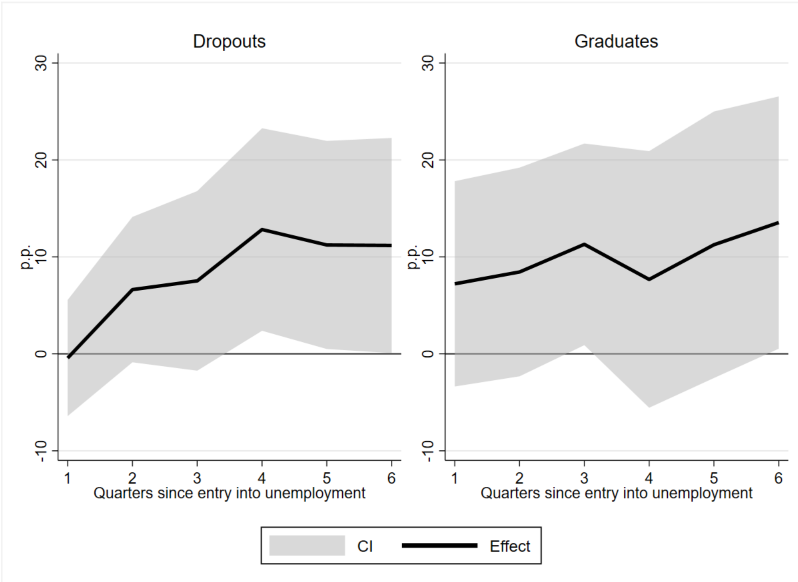
Note: Donut RDD estimate on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. The outcome is the amount of received subsidy (in full-time equivalent) conditional on employment within one year after unemployment entry by schooling level (columns: dropouts vs. graduates), which is plotted over six age-quantile-spaced bins on each side of the donut. The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. The effect estimated by the donut RDD estimator at 26 years of age is €297 [60; 535] with a p-value of 0.015 and N = 1,615 for dropouts, while for graduates it is €323 [181; 466] with a p-value of 0.000 and N = 2,343.

Figure A.4: Discontinuity at Age 26 of the Cumulative Transition Rate to Private Sector Employment Within One Year by Schooling Level



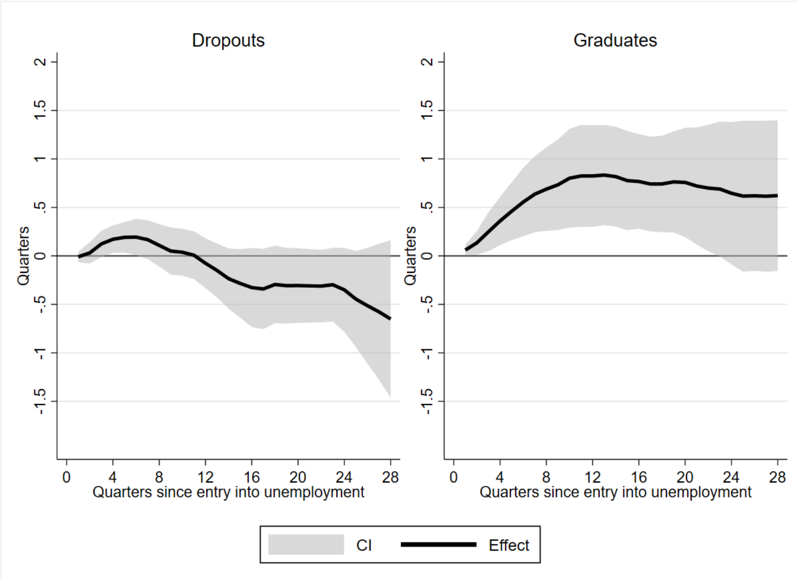
Note: Donut RDD estimate on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. The outcome is the cumulative transition rate to private sector employment within one year by schooling level (columns: dropouts vs. graduates), which is plotted over six age-quantile-spaced bins on each side of the donut. The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. The effect estimated by the donut RDD estimator at 26 years of age is +12.8 pp [2.8; 16.8] with a p-value of 0.017 and N = 4,176 for dropouts, while for graduates it is +7.7 pp [–5.5; 20.9] with a p-value of 0.107 and N = 4,384.

Figure A.5: Evolution of the RDD Effect on the Cumulative Transition Rate to the Private Sector Up to 6 Quarters After Entry into Unemployment



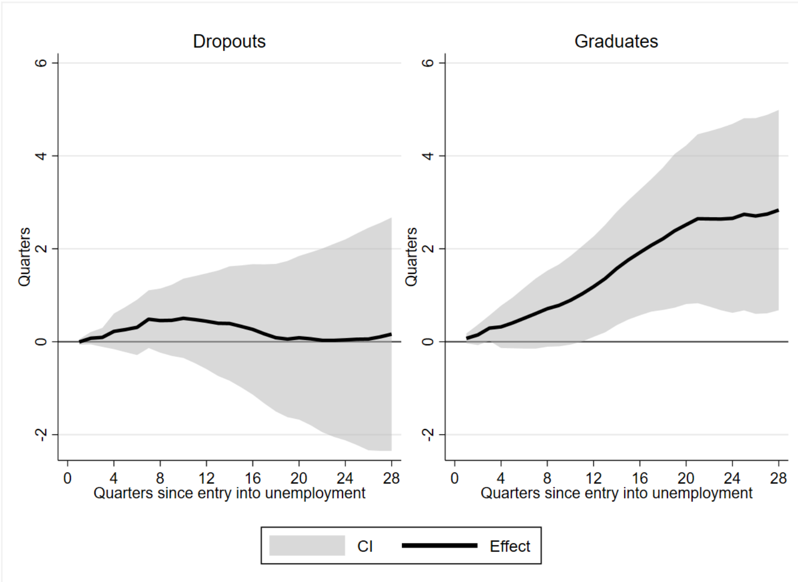
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the transition rate to the private sector up to 6 quarters after entry into unemployment by schooling level: dropouts (left) vs. graduates (right). The RDD estimator is implemented for each quarter after entry into unemployment until 6 quarters later. The donut RDD estimator removes observations for individuals aged between 25 and 26 at entry into unemployment and retains units aged between 22 and 25 (left of the cutoff) and 26 to 29 (right of the cutoff). In a sensitivity we shrink the "hole" to increase precision for outcomes measured before 1 year. Results are robust and available upon request. The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. For dropouts (graduates), the effect at 6 quarters is +11.2 pp [0.1; 22.3] with a p-value of 0.048 and N = 4,176 (+13.5 pp [0.5; 26.5], p-value 0.042 and N = 4,384).

Figure A.6: DiD Effect on the Cumulative Number of Quarters in Subsidized Private-Sector Employment



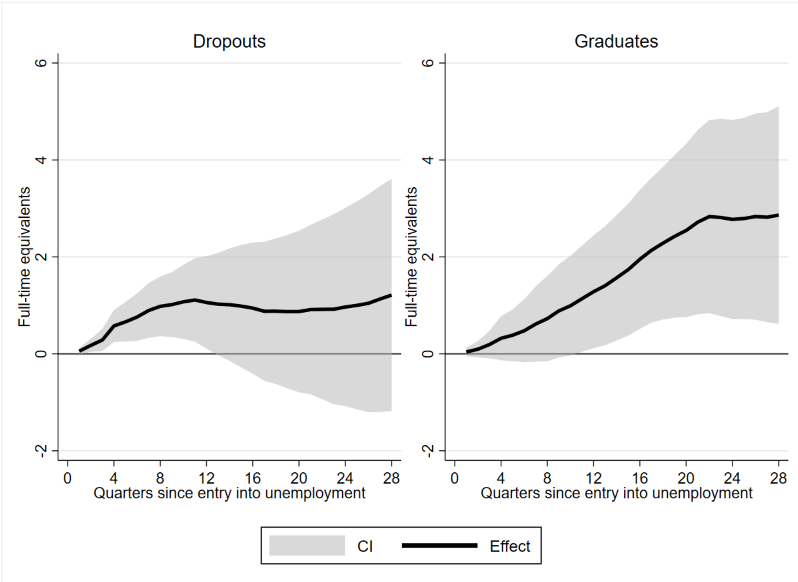
Note: Evolution of the effect estimated with a doubly robust DiD estimator (Sant'Anna and Zhao, 2020) and confidence interval (CI) for the cumulative number of quarters in subsidized private-sector employment by schooling level: dropouts (left) vs. graduates (right). The DiD estimator is implemented for each year after entry into unemployment until 7 years later. The treated are aged 24-25 at unemployment entry, while controls are aged 26-27. Units registering in (2008) 2010 are considered in the (pre-)treatment period. Data are reweighted by the sampling weights. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. For dropouts (graduates), the effect at 11 quarters is +0.1 quarters [–0.2; 0.2] with a p-value of 0.953 and N = 1,942 (+0.8 quarters [0.3; 1.4], p-value 0.002 and N = 1,839).

Figure A.7: Evolution of the RDD Effect on the Cumulative Number of Quarters in Private Sector Employment



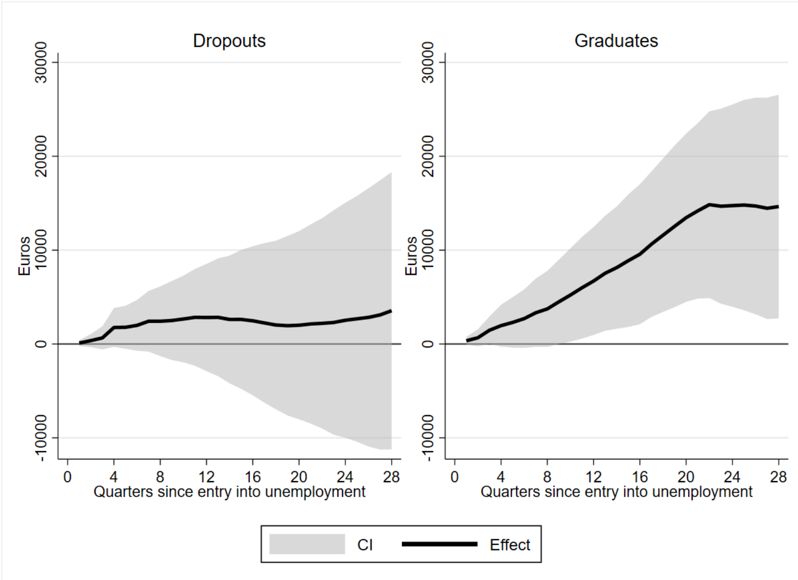
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the cumulative number of quarters in private sector employment by schooling level: dropouts (left) vs. graduates (right). The RDD estimator is implemented for each quarter after entry into unemployment until 7 years later. The donut RDD estimator removes observations for individuals aged between 25 and 26 at entry into unemployment and retains units aged between 22 and 25 (left of the cutoff) and 26 to 29 (right of the cutoff). The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. For dropouts (graduates), the effect at 7 years is +0.2 quarters [–2.3; 2.7] with a p-value of 0.897 and N = 4,176 (+2.8 quarters [0.7; 5.0], p-value 0.011 and N = 4,384).

Figure A.8: Evolution of the RDD Effect on the Cumulative Percentage of Full-Time-Equivalent Quarters in Private Sector Employment



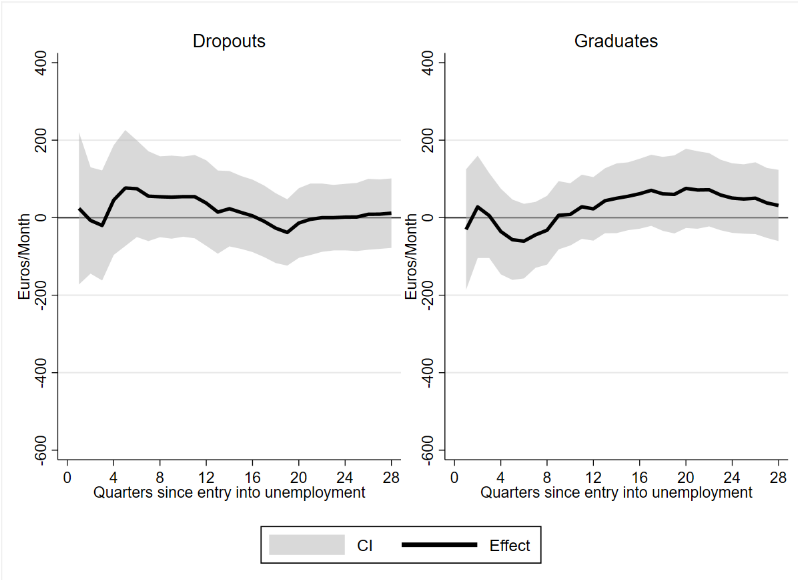
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the cumulative percentage of full-time equivalents private sector employment (1 for a full-time job in the quarter) in a private sector firm by schooling level: dropouts (left) vs. graduates (right). The RDD estimator is implemented for each quarter after entry into unemployment until 7 years later. The donut RDD estimator removes observations for individuals aged between 25 and 26 at entry into unemployment and retains units aged between 22 and 25 (left of the cutoff) and 26 to 29 (right of the cutoff). The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. For dropouts (graduates), the effect at 7 years is +1.21 full time equivalent quarters [−1.18; 3.60] with a p-value of 0.316 and N = 4,176 (+2.87 full-time equivalent quarters [0.62; 5.10], p-value 0.013 and N = 4,384).

Figure A.9: Evolution of the RDD Effect on the Cumulative Gross Wage in the Private Sector



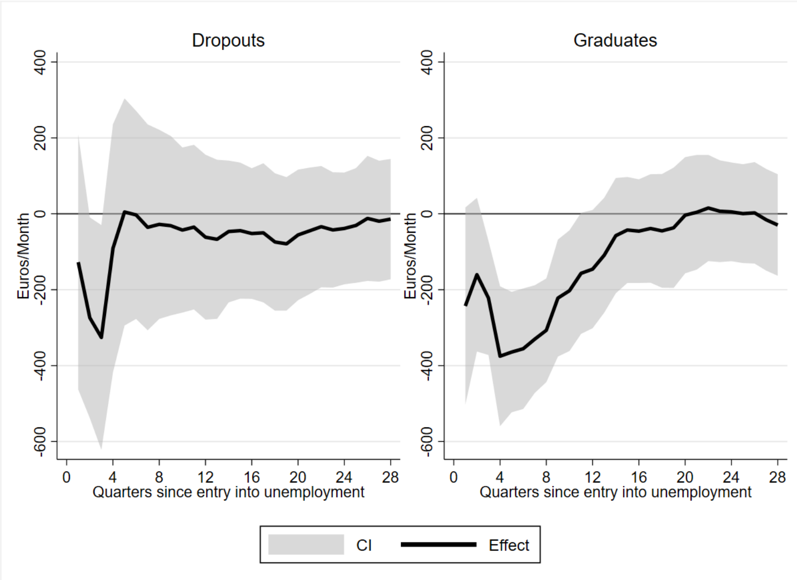
Note: Donut RDD estimates on the inflow sample of youth entering unemployment in 2010, using age at unemployment entry as the forcing variable with cutoff at 26. Evolution of the RDD effect and confidence intervals (CI) on the cumulative gross wage in a private sector firm by schooling level: dropouts (left) vs graduates (right). The RDD estimator is implemented for each quarter after entry into unemployment until 7 years later. The donut RDD estimator removes the observation aged between 25 and 26 at entry into unemployment and retains units aged between 22 and 25 (left of the cutoff) and 26 to 29 (right of the cutoff). The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. For the dropouts (graduates) the effect at 7-year distance is €3,548 [–11,224; 18,320] with a p-value of 0.633 and N = 4,176 (€14,646 [2,736; 26,555], p-value 0.017 and N = 4,384).

Figure A.10: Evolution of the RDD Effect on the Average Full-time Gross Wage in the Private Sector (Euros/Month Conditional on Employment)



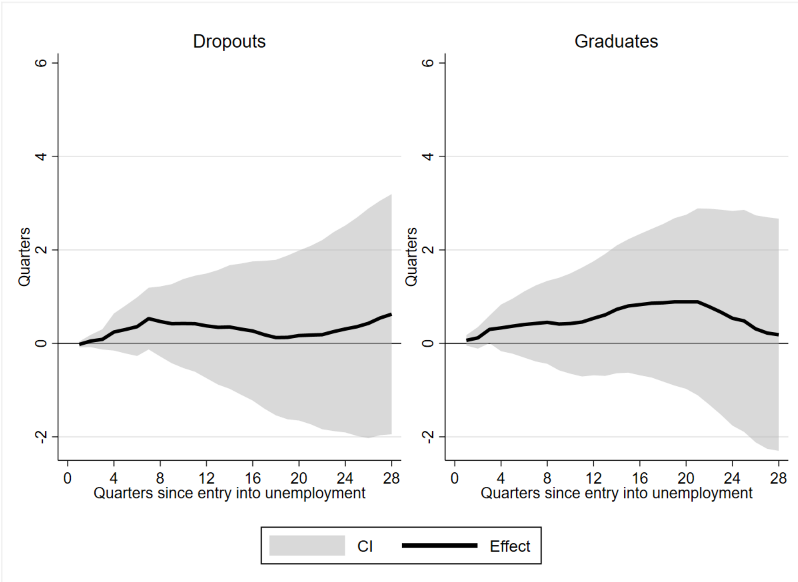
Note: Donut RDD estimates on the inflow sample of youth entering unemployment in 2010, using age at unemployment entry as the forcing variable with cutoff at 26. Evolution of the RDD effect and confidence intervals (CI) on the average full-time gross wage (euros/month) conditional on employment in a private sector firm by schooling level: dropouts (left) vs graduates (right). The RDD estimator is implemented for each quarter after entry into unemployment until 7 years later. The donut RDD estimator removes the observation aged between 25 and 26 at entry into unemployment and retains units aged between 22 and 25 (left of the cutoff) and 26 to 29 (right of the cutoff). The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. For the dropouts (graduates) the effect at 7-year distance is €11 [–77; 101] with a p-value of 0.795 and N = 2,811 (€31 [–60; 123], p-value 0.495 and N = 3,440).

Figure A.11: Evolution of the RDD Effect on the Average Full-time Wage Costs in the Private Sector (Euros/Month Conditional on Employment)



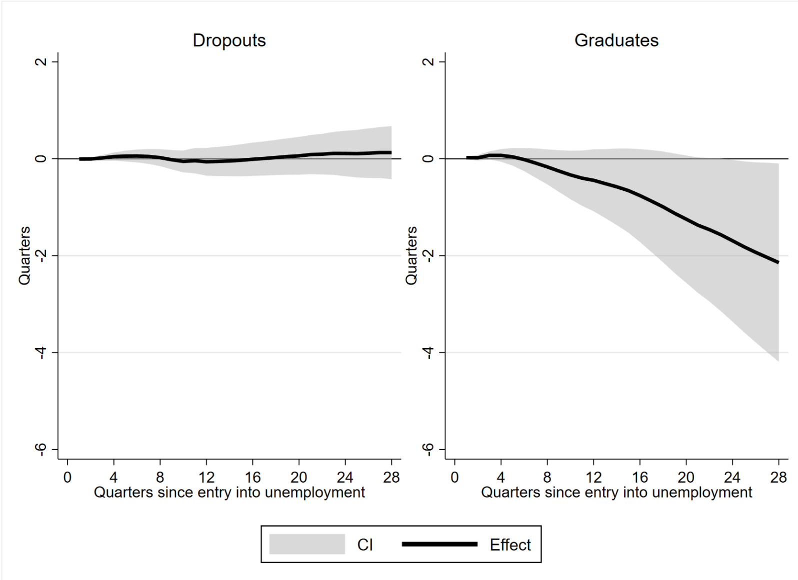
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the average full-time wage cost (euros/month) conditional on employment in a private sector firm by schooling level: dropouts (left) vs. graduates (right). The RDD estimator is implemented for each quarter after entry into unemployment until 7 years later. The donut RDD estimator removes observations for individuals aged between 25 and 26 at entry into unemployment and retains units aged between 22 and 25 (left of the cutoff) and 26 to 29 (right of the cutoff). The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. For dropouts (graduates), the effect at 7 years is –€14.0 [–172.4; 144.4] with a p-value of 0.861 and N = 2,811 (–€29.6 [–163.2; 104.1], p-value 0.661 and N = 3,440).

Figure A.12: Evolution of the RDD Effect on the Cumulative Number of Quarters in Any Employment



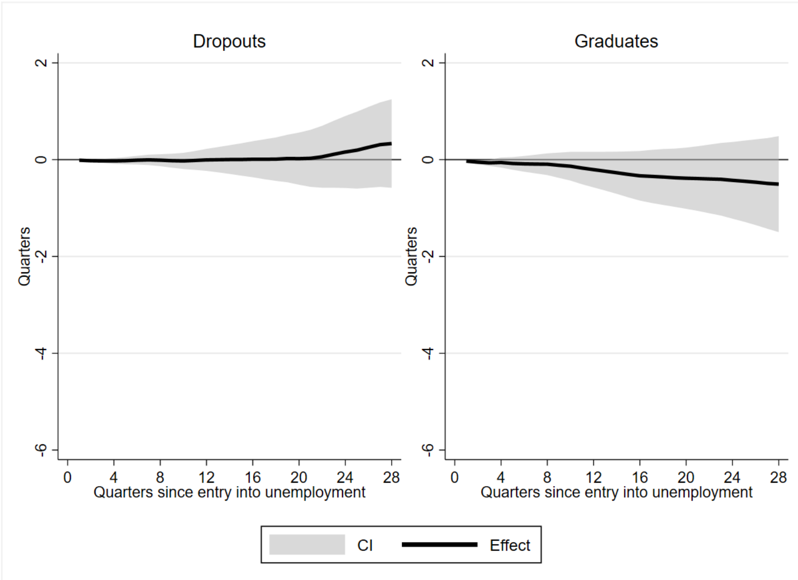
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the cumulative number of quarters in any employment by schooling level: dropouts (left) vs. graduates (right). The RDD estimator is implemented for each quarter after entry into unemployment until 7 years later. The donut RDD estimator removes observations for individuals aged between 25 and 26 at entry into unemployment and retains units aged between 22 and 25 (left of the cutoff) and 26 to 29 (right of the cutoff). The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. For dropouts (graduates), the effect at 7 years is 0.6 quarters [–2,0; 3.2] with a p-value of 0.629 and N = 4,176 (0.2 quarters [–2.3; 2.7], p-value 0.882 and N = 4,384).

Figure A.13: Evolution of the RDD Effect on the Cumulative Number of Quarters in Public Sector Employment



Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the cumulative number of quarters in a public sector firm by schooling level: dropouts (left) vs. graduates (right). The RDD estimator is implemented for each quarter after entry into unemployment until 7 years later. The donut RDD estimator removes observations for individuals aged between 25 and 26 at entry into unemployment and retains units aged between 22 and 25 (left of the cutoff) and 26 to 29 (right of the cutoff). The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. For dropouts (graduates), the effect at 7 years is 0.1 quarters [–0.4; 0.7] with a p-value of 0.643 and N = 4,176 (–2.1 quarters [–4.2; –0.1], p-value 0.040 and N = 4,384).

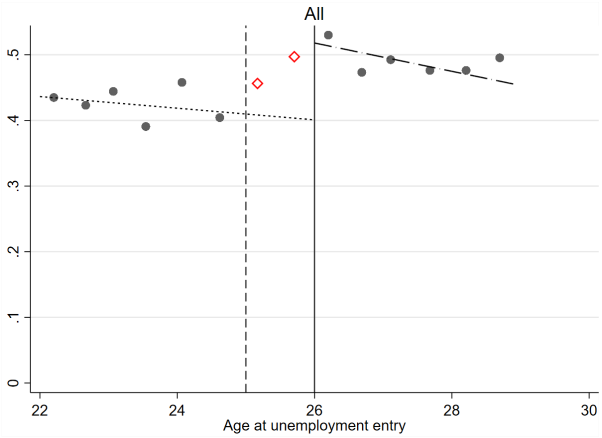
Figure A.14: Evolution of the RDD Effect on the Cumulative Number of Quarters in Self-Employment



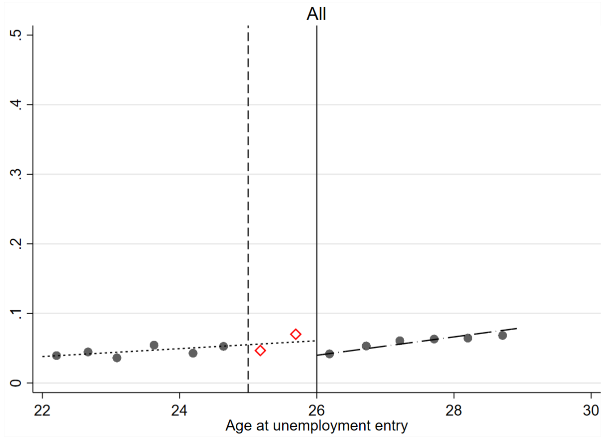
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the cumulative number of quarters in self-employment firm by schooling level: dropouts (left) vs. graduates (right). The RDD estimator is implemented for each quarter after entry into unemployment until 7 years later. The donut RDD estimator removes observations for individuals aged between 25 and 26 at entry into unemployment and retains units aged between 22 and 25 (left of the cutoff) and 26 to 29 (right of the cutoff). The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. For dropouts (graduates), the effect at 7 years is 0.3 quarters [–0.6; 1.2] with a p-value of 0.470 and N = 4,176 (–0.5 quarters [–1.5; 0.5], p-value 0.313 and N = 4,384).

Figure A.15: Discontinuity at Age 26 of Other Employment Outcomes Within One Year

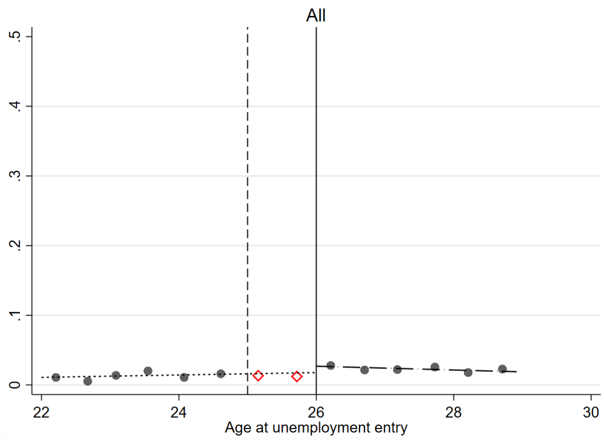
(a) Probability of remaining non-employed



(b) Cumulative Transition Rate to public sector

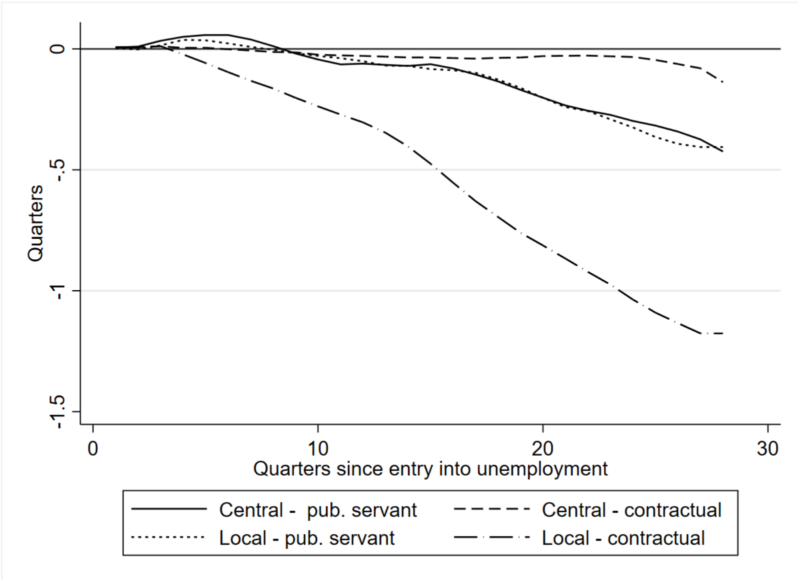


(c) Cumulative Transition Rate to self-employment



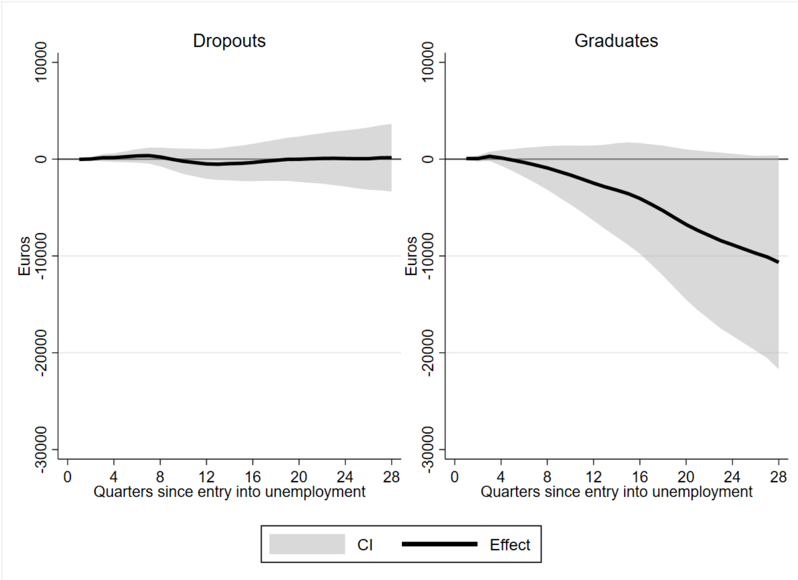
Note: Donut RDD estimate on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. The outcome is (a) remaining out-of-employment (b) transiting to public sector employment and (c) transiting to self-employment within one year after unemployment entry, which is plotted over six age-quantile-spaced bins on each side of the donut. The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but by removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. The effect estimated by the donut RDD estimator at 26 years old is (a) –11.7 pp [–20.8; –3.6] with a p-value of 0.005 and N = 3,958, (b) +2.1 pp [–0.1; 4.3] with a p-value of 0.065 and N = 3,958 and (c) –0.1 pp [–2.4; –0.5] with a p-value of 0.208 and N = 3,958.

Figure A.16: Evolution of the RDD Effect on the Cumulative Number of Quarters in Public Employment by Type - Graduates



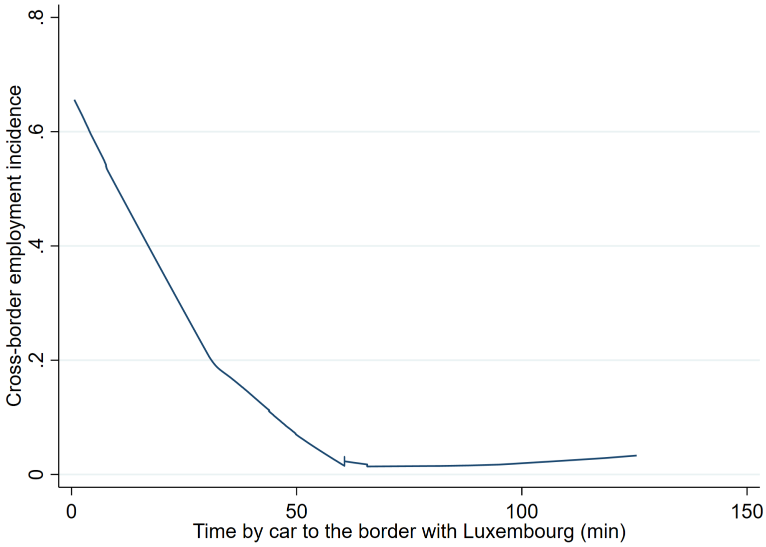
Note: Donut RDD estimates on the inflow sample of high-school graduates entering unemployment in 2010, using age at unemployment entry as the forcing variable with cutoff at 26. Evolution of the RDD effect on the cumulative number of quarters in public sector employment by type: central government and public servant, central government and contractual agent, local governments and public servant, local government and contractual agent. The RDD estimator is implemented for each quarter after entry into unemployment until 7 years later. The donut RDD estimator removes the observation aged between 25 and 26 at entry into unemployment and retains units aged between 22 and 25 (left of the cutoff) and 26 to 29 (right of the cutoff). The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. The effect at 7-year distance on the cumulative number of quarters in public sector employment in (i) central government and public servant: –0.4 [–1.0; 0.2] with a p-value of 0.194; (ii) central government and contractual agent: –0.1 [–0.5; 0.3] with a p-value of 0.505; (iii) local governments and public servant: –0.4 [–1.0; 0.2] with a p-value of 0.155; (iv) local government and contractual agent: –1.2 [–2.5; 0.2] with a p-value of 0.089. N = 4,384.

Figure A.17: Evolution of the RDD Effect on the Cumulative Gross Wage in the Public Sector



Note: Donut RDD estimates on the inflow sample of youth entering unemployment in 2010, using age at unemployment entry as the forcing variable with cutoff at 26. Evolution of the RDD effect and confidence intervals (CI) on the cumulative gross wage in a public sector firm by schooling level: dropouts (left) vs graduates (right). The RDD estimator is implemented for each quarter after entry into unemployment until 7 years later. The donut RDD estimator removes the observation aged between 25 and 26 at entry into unemployment and retains units aged between 22 and 25 (left of the cutoff) and 26 to 29 (right of the cutoff). The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. For the dropouts (graduates) the effect at 7-year distance is €154 [–3,337; 3,645] with a p-value of 0.930 and N = 4,176 (–€10,656 [–21,707; 393], p-value 0.058 and N = 4,384).

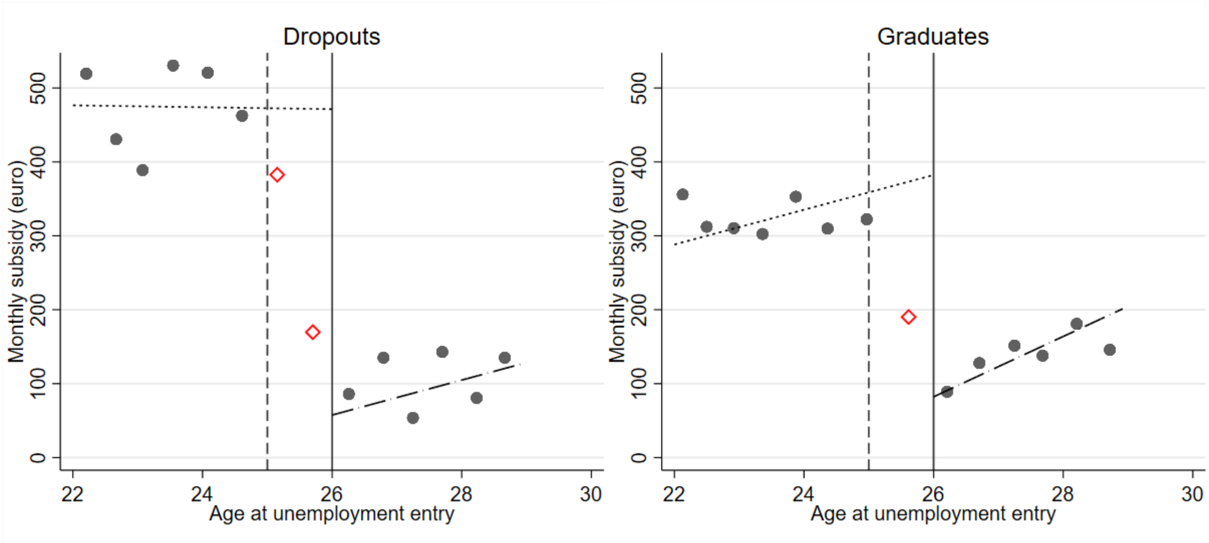
Figure A.18: Share of Workers Working Abroad Over Distance to the Border With Luxembourg (lowess)



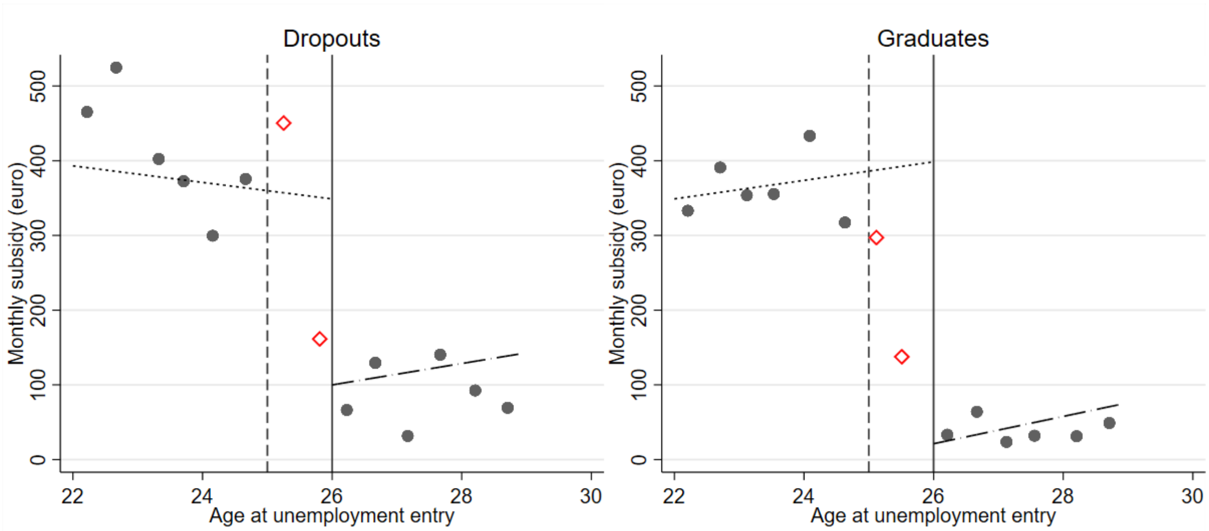
Note: Lowess smoothing (running-line least squares) for the share of workers working abroad (any country) according to minutes by car during rush hour (TomTom data) to the border with Luxembourg. This is calculated over the original full sample of 125,000 observations during the 4th quarter of 2009, trimming the units with a distance above the 99th percentile (126 minutes).

Figure A.19: Discontinuity at Age 26 of the Average Subsidy Amount Conditional on Employment Within One Year – (a) Close to the Border and (b) Far from the Border

(a) Close to the border

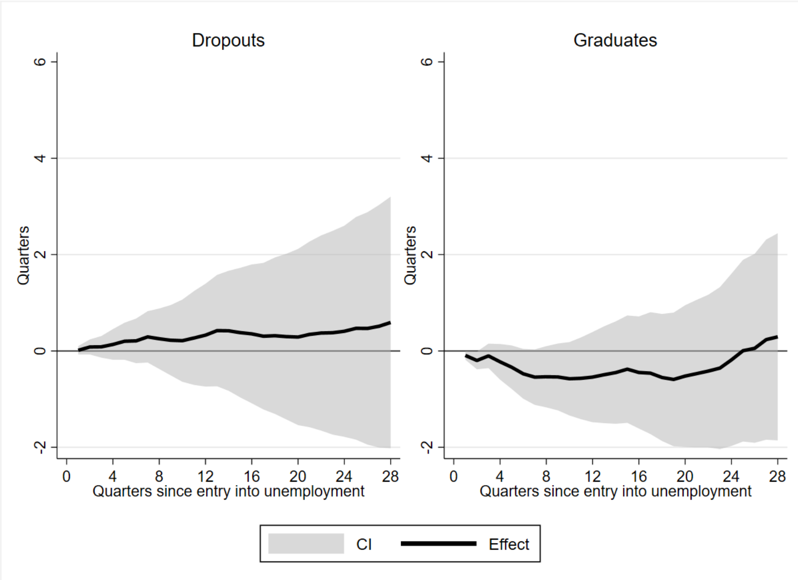


(b) Far from the border



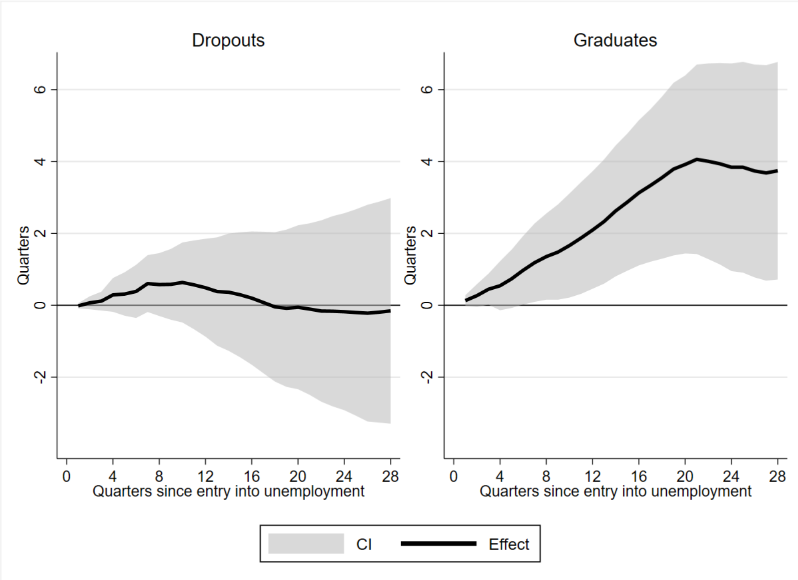
Note: Donut RDD estimate on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. The outcome is the amount of received subsidy (in full-time equivalent) conditional on employment within one year after unemployment entry by schooling level (dropouts on the left vs. graduates on the right) and border distance ((a) within or (b) more than 60 minutes by car from the border), which is plotted over six age-quantile-spaced bins on each side of the donut. The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. The effect estimated by the donut RDD estimator at 26 years of age for dropouts living near the border is €414 [235; 592] with a p-value of 0.000 and N = 597, while for graduates it is €300 [197; 404] with a p-value of 0.000 and N = 1,939. For dropouts living far from the border, it is €249 [–47; 545] with a p-value of 0.099 and N = 978, while for graduates it is €377 and [194; 561] with a p-value of 0.006 and N = 1,023.

Figure A.20: Evolution of the RDD Effect on the Cumulative Number of Quarters in Private Sector Employment – Close to the Border



Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the cumulative number of quarters in private sector employment since entry into unemployment in 2010 for individuals living within 60 minutes from the border by schooling level: dropouts (left) vs. graduates (right). The RDD estimator is implemented for each quarter after entry into unemployment until 7 years later. The donut RDD estimator removes observations for individuals aged between 25 and 26 at entry into unemployment and retains units aged between 22 and 25 (left of the cutoff) and 26 to 29 (right of the cutoff). The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2. Standard errors are clustered at the age level. For dropouts (graduates), the effect at 7 years is 0.6 quarters [–2.0; 3.2] with a p-value of 0.653 and N = 1,443 (+0.3 quarters [–1.9; 2.4], p-value 0.786 and N = 1,939).

Figure A.21: Evolution of the RDD Effect on the Cumulative Number of Quarters in Private Sector Employment – Far from the Border

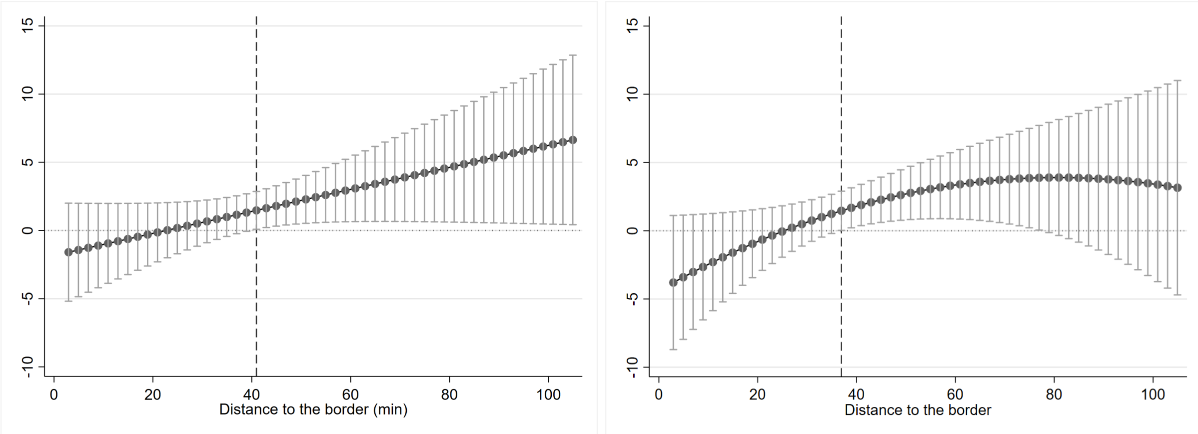


Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the cumulative number of quarters in private sector employment since entry into unemployment in 2010 for individuals living more than 60 minutes from the border by schooling level: dropouts (left) vs. graduates (right). The RDD estimator is implemented for each quarter after entry into unemployment until 7 years later. The donut RDD estimator removes observations for individuals aged between 25 and 26 at entry into unemployment and retains units aged between 22 and 25 (left of the cutoff) and 26 to 29 (right of the cutoff). The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2. Standard errors are clustered at the age level. For dropouts (graduates), the effect at 7 years is –0.2 quarters [–3.3; 3.0] with a p-value of 0.921 and N = 2,636 (+3.7 quarters [0.7; 6.8], p-value 0.016 and N = 2,432).

Figure A.22: Donut RDD Effect on the Cumulative Number of Quarters in Private Sector Employment 7 Years After Entry into Unemployment Interacted With Travel Time from the Border With Luxembourg – Graduates

(a) Linear

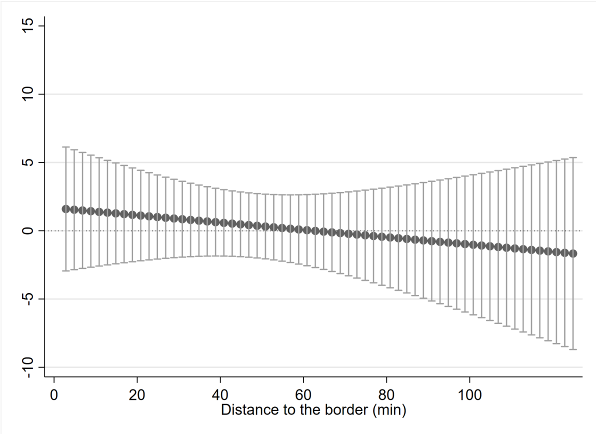
(b) Quadratic



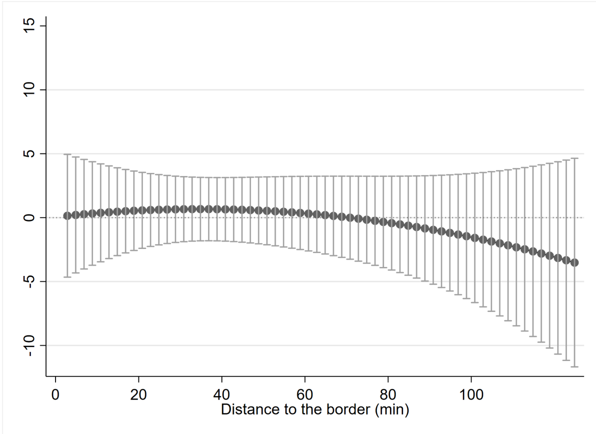
Note: Donut RDD estimate on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. The outcome is the cumulative number of quarters in private sector employment 7 years after entry into unemployment. We retain only high school graduates. The donut RDD estimator removes observations for individuals aged between 25 and 26 at entry into unemployment and retains units aged between 22 and 25 (left of the cutoff) and 26 to 29 (right of the cutoff). The predicted effect over the distance to the border from Luxembourg is obtained by implementing an RDD estimator in which the splines and the treatment dummy are interacted with the border distance (linear (a) or quadratic (b)). The 95% confidence intervals are also reported. The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. The dashed line shows the distance when the effect starts to be statistically significant. N = 4,384.

Figure A.23: Donut RDD Effect on the Cumulative Number of Quarters in Private Sector Employment 7 Years After Entry into Unemployment Interacted With Travel Time From the Border With Luxembourg – Dropouts

(a) Linear



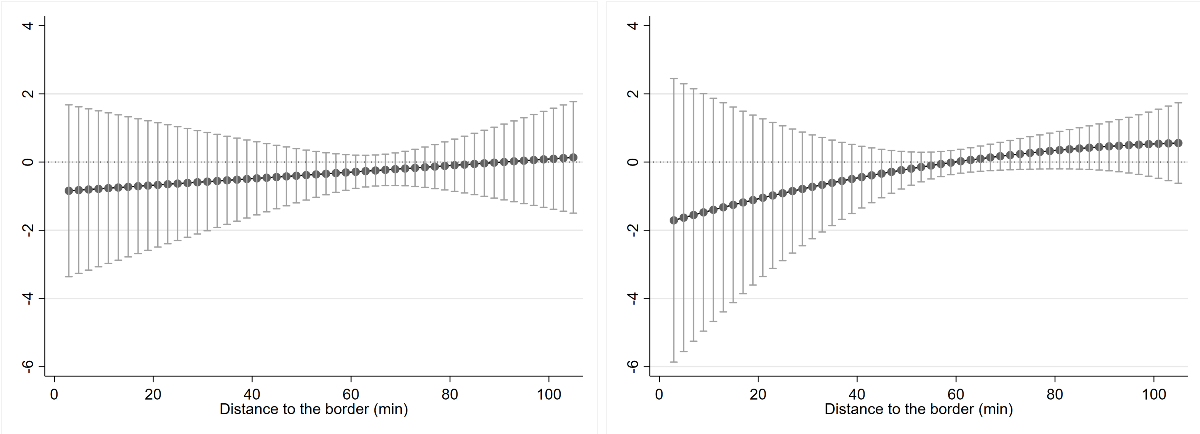
(b) Quadratic



Note: Donut RDD estimate on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. The outcome is the cumulative number of quarters in private sector employment 7 years after entry into unemployment. We retain only high school dropouts. The donut RDD estimator removes observations for individuals aged between 25 and 26 at entry into unemployment and retains units aged between 22 and 25 (left of the cutoff) and 26 to 29 (right of the cutoff). The predicted effect over the distance to the border from Luxembourg is obtained by implementing an RDD estimator in which the splines and the treatment dummy are interacted with the border distance (linear (a) or quadratic (b)). The 95% confidence intervals are also reported. The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. The dashed line shows the distance when the effect starts to be statistically significant. N = 4,176.

Figure A.24: Donut RDD Effect on the Cumulative Number of Quarters of Cross-Border Work 7 Years After Entry into Unemployment Interacted With Travel Time from the Border With Luxembourg – Graduates

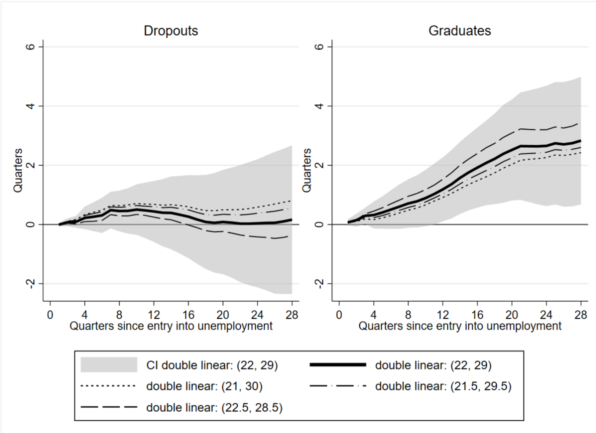
(a) Linear (b) Quadratic



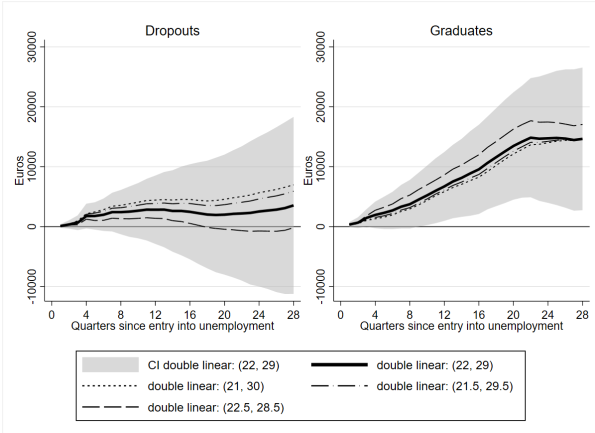
Note: Donut RDD estimate on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. The outcome is the cumulative number of quarters in cross-border employment 7 years after entry into unemployment. We retain only high school graduates. The donut RDD estimator removes observations for individuals aged between 25 and 26 at entry into unemployment and retains units aged between 22 and 25 (left of the cutoff) and 26 to 29 (right of the cutoff). The predicted effect over the distance to the border from Luxembourg is obtained by implementing an RDD estimator in which the splines and the treatment dummy are interacted with the border distance (linear (a) or quadratic (b)). The 95% confidence intervals are also reported. The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. The dashed line shows the distance when the effect starts to be statistically significant. N = 4,384.

Figure A.25: Evolution of the RDD Effect on Cumulative Outcomes: Changing the Bandwidth

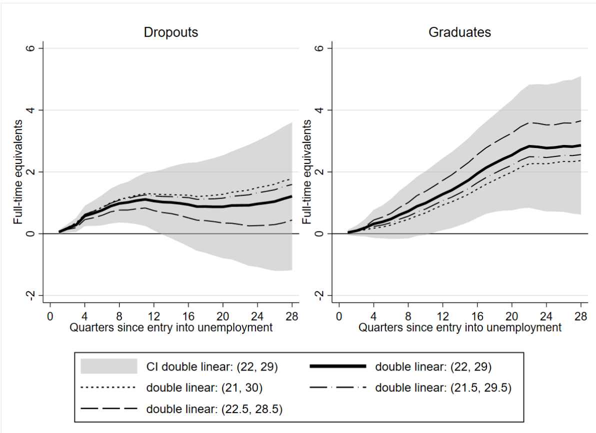
(a) Private sector employment



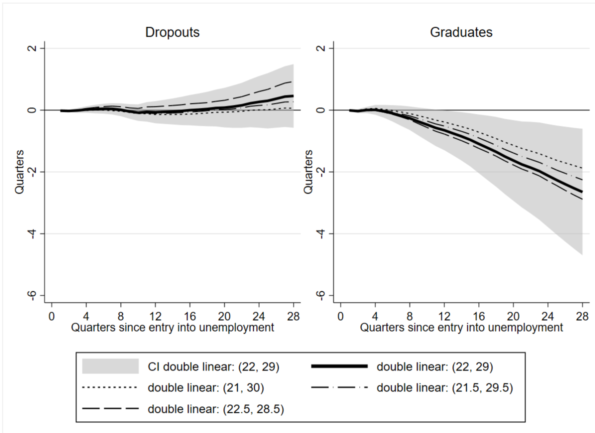
(b) Gross wage (private sector)



(c) Full-time equivalents (private sector)



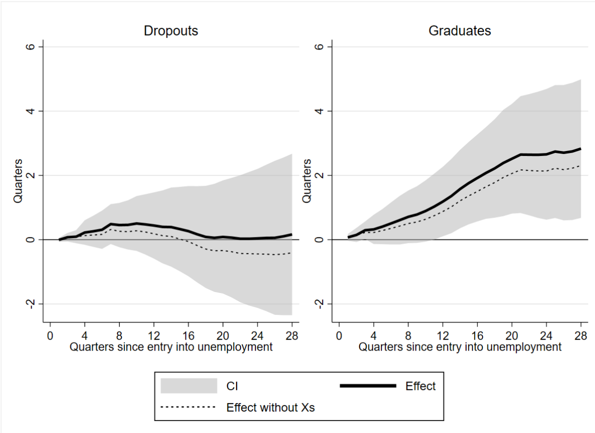
(d) Other employment



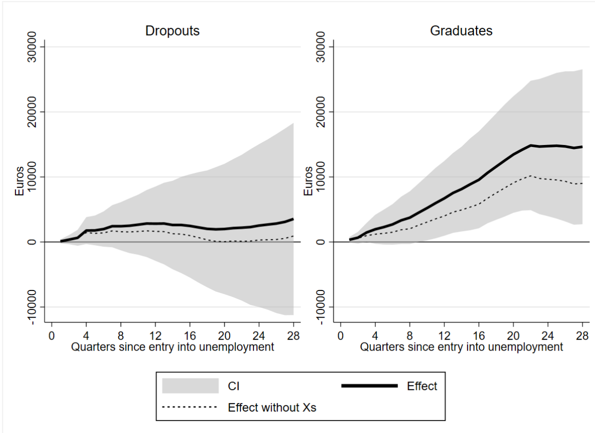
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence intervals (CI) for (a) the cumulative number of quarters in private sector employment, (b) gross remuneration in private sector employment, (c) full-time equivalents in private sector employment (1 for a full-time job in the quarter), (d) quarters in self- and public employment, since unemployment in 2010 and by schooling level: dropouts (left) vs. graduates (right). The RDD estimator is implemented for each quarter after entry into unemployment until 7 years later. The donut RDD estimator removes observations for individuals aged between 25 and 26 at unemployment and retains units aged between 22 and 25 (left of the cutoff) and 26 to 29 (right of the cutoff). The full line shows the point estimates and the confidence intervals for the benchmark bandwidth of 22-29 years old. The dashed lines show the point estimates for different bandwidth scenarios, i.e., 21-30, 21.5-29.5, 22.5-28.8. The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level.

Figure A.26: Evolution of the RDD Effect on Cumulative Outcomes: Removing the Xs

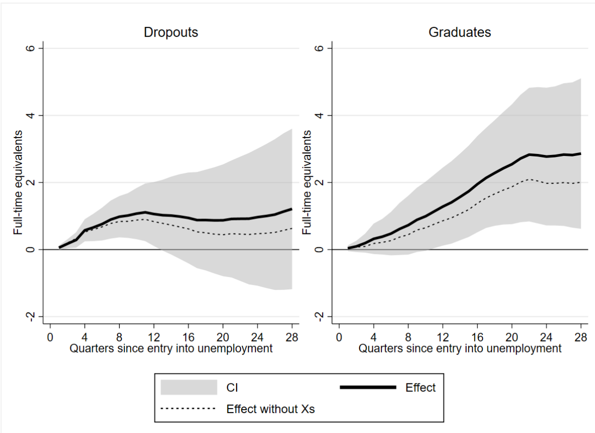
(a) Private sector employment



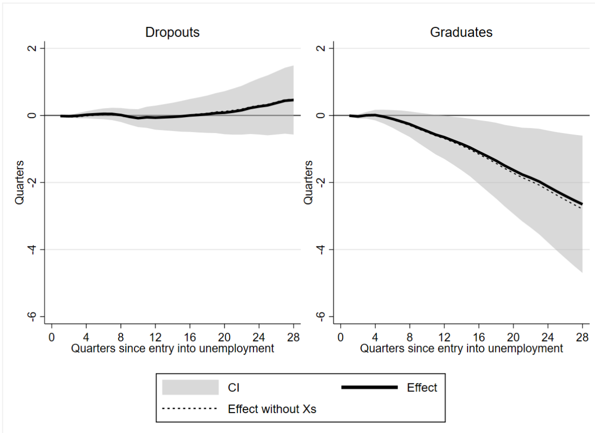
(b) Gross wage (private sector)



(c) Full-time equivalents (private sector)

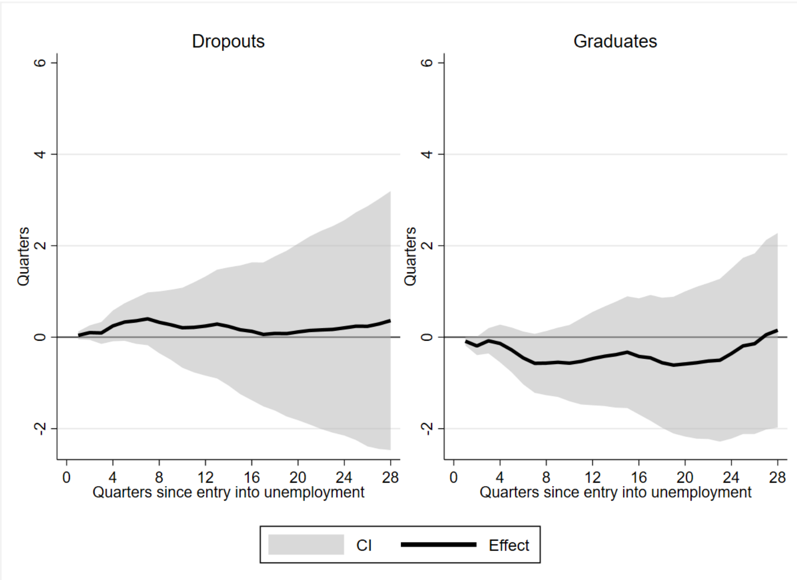


(d) Other employment



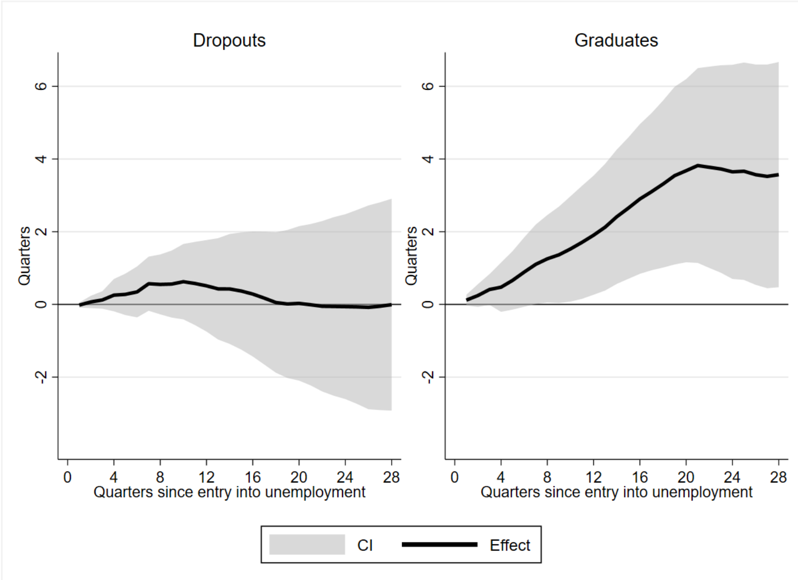
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence intervals (CI) for (a) the cumulative number of quarters in private sector employment, (b) gross remuneration in private sector employment, (c) full-time equivalents in private sector employment (1 for a full-time job in the quarter), (d) quarters in self- and public employment, since unemployment in 2010 and by schooling level: dropouts (left) vs. graduates (right). The RDD estimator is implemented for each quarter after entry into unemployment until 7 years later. The donut RDD estimator removes observations for individuals aged between 25 and 26 at unemployment and retains units aged between 22 and 25 (left of the cutoff) and 26 to 29 (right of the cutoff). The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. The dashed line shows the point estimates if we remove the Xs from the RDD estimator. Standard errors are clustered at the age level.

Figure A.27: Evolution of the RDD Effect on the Cumulative Number of Quarters in Private Sector Employment – Close to the Border (less than 45 minutes)



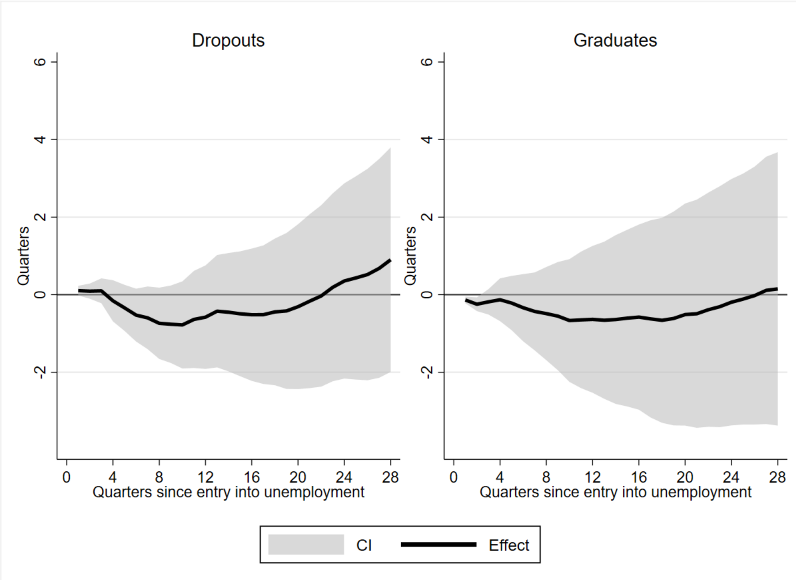
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the cumulative number of quarters in private sector employment since entry into unemployment in 2010 for individuals living within 45 minutes of the border by schooling level: dropouts (left) vs. graduates (right). The RDD estimator is implemented for each quarter after entry into unemployment until 7 years later. The donut RDD estimator removes observations for individuals aged between 25 and 26 at entry into unemployment and retains units aged between 22 and 25 (left of the cutoff) and 26 to 29 (right of the cutoff). The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. For dropouts (graduates), the effect at 7 years is 0.4 quarters [–2.5; 3.2] with a p-value of 0.800 and N = 1,278 (0.1 quarters [–2.0; 2.3], p-value 0.886 and N = 1,713).

Figure A.28: Evolution of the RDD Effect on the Cumulative Number of Quarters in Private Sector Employment – Far From the Border (more than 45 minutes)



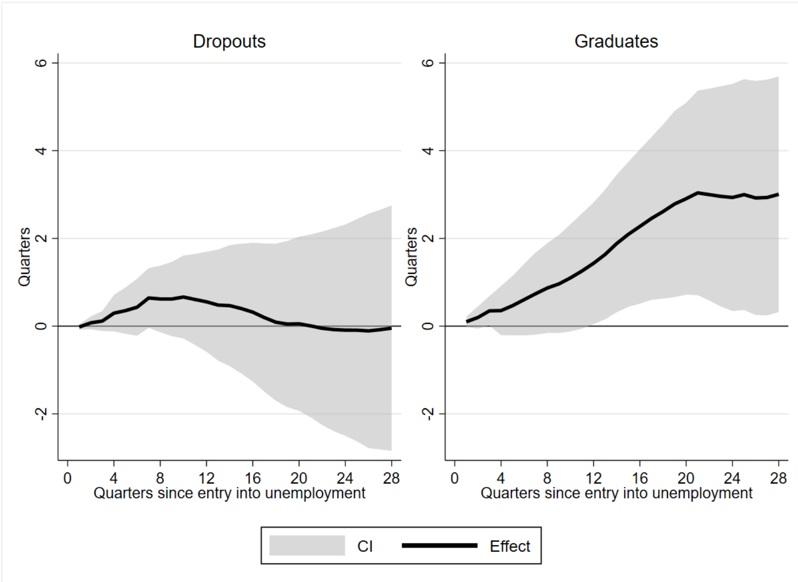
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the cumulative number of quarters in private sector employment since entry into unemployment in 2010 for individuals living more than 45 minutes from the border by schooling level: dropouts (left) vs. graduates (right). The RDD estimator is implemented for each quarter after entry into unemployment until 7 years later. The donut RDD estimator removes observations for individuals aged between 25 and 26 at entry into unemployment and retains units aged between 22 and 25 (left of the cutoff) and 26 to 29 (right of the cutoff). The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. For dropouts (graduates), the effect at 7 years is –0.0 quarters [–2.9; 2.9] with a p-value of 0.997 and N = 2,801 (3.6 quarters [0.5; 6.7], p-value 0.025 and N = 2,658).

Figure A.29: Evolution of the RDD Effect on the Cumulative Number of Quarters in Private Sector Employment – Close to the Border (less than 30 minutes)



Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the cumulative number of quarters in private sector employment since entry into unemployment in 2010 for individuals living within 30 minutes of the border by schooling level: dropouts (left) vs. graduates (right). The RDD estimator is implemented for each quarter after entry into unemployment until 7 years later. The donut RDD estimator removes observations for individuals aged between 25 and 26 at entry into unemployment and retains units aged between 22 and 25 (left of the cutoff) and 26 to 29 (right of the cutoff). The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. For dropouts (graduates), the effect at 7 years is 0.9 quarters [–2.0; 3.8] with a p-value of 0.536 and N = 618 (0.1 quarters [–3.4; 3.7], p-value 0.933 and N = 911).

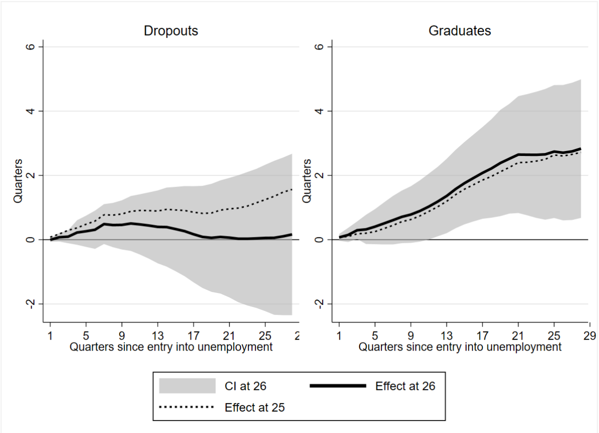
Figure A.30: Evolution of the RDD Effect on the Cumulative Number of Quarters in Private Sector Employment – Far From the Border (more than 30 minutes)



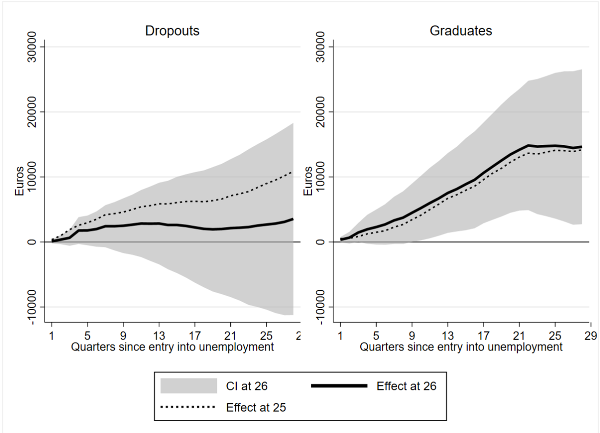
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the cumulative number of quarters in private sector employment since entry into unemployment in 2010 for individuals living more than 30 minutes from the border by schooling level: dropouts (left) vs. graduates (right). The RDD estimator is implemented for each quarter after entry into unemployment until 7 years later. The donut RDD estimator removes observations for individuals aged between 25 and 26 at entry into unemployment and retains units aged between 22 and 25 (left of the cutoff) and 26 to 29 (right of the cutoff). The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. For dropouts (graduates), the effect at 7 years is –0.1 quarters [–2.8; 2.7] with a p-value of 0.974 and N = 3,461 (3.0 quarters [0.3; 5.7], p-value 0.029 and N = 3,460).

Figure A.31: Evolution of the RDD Effect on Cumulative Outcomes: Effect at age 25

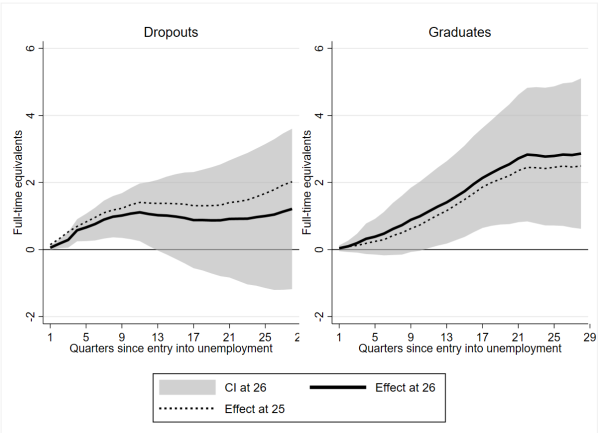
(a) Private sector employment



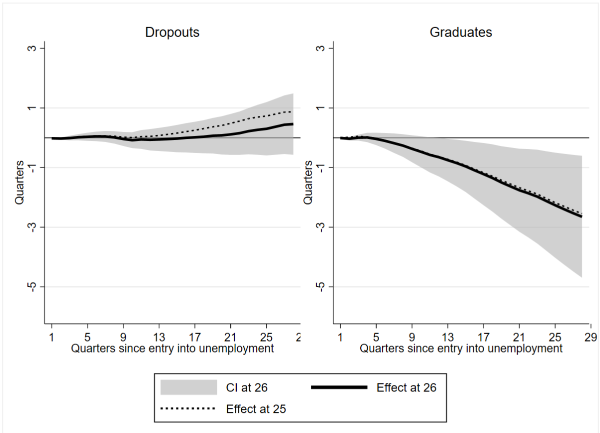
(b) Gross wage (private sector)



(c) Full-time equivalents (private sector)



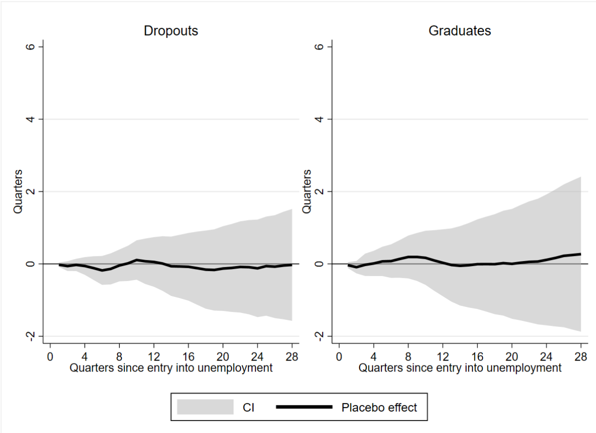
(d) Other employment



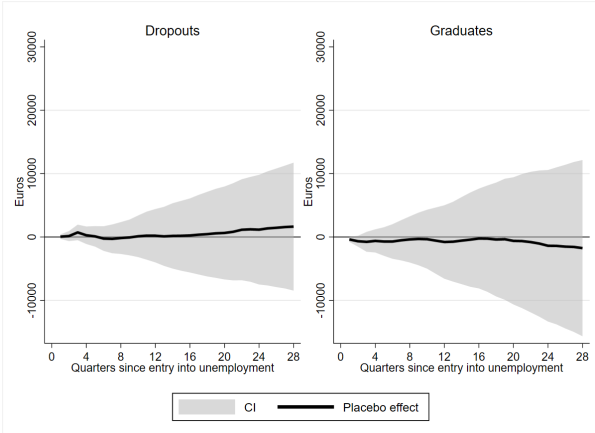
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26 (full line and CI) or 25 (dashed line). Evolution of the RDD effect and confidence intervals (CI) for (a) the cumulative number of quarters in private sector employment, (b) gross remuneration in private sector employment, (c) full-time equivalents in private sector employment (1 for a full-time job in the quarter), (d) quarters in self- and public employment, since unemployment in 2010 and by schooling level: dropouts (left) vs. graduates (right). The RDD estimator is implemented for each quarter after unemployment until 7 years later. The donut RDD estimator removes observations for individuals aged between 25 and 26 at unemployment and retains units aged between 22 and 25 (left of the cutoff) and 26 to 29 (right of the cutoff). For the effect at 26 (25), the outcome on the left (right) of the cutoff is predicted by the left (right) spline. The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level.

Figure A.32: Evolution of the RDD Effect on Cumulative Outcomes: Placebo in 2008

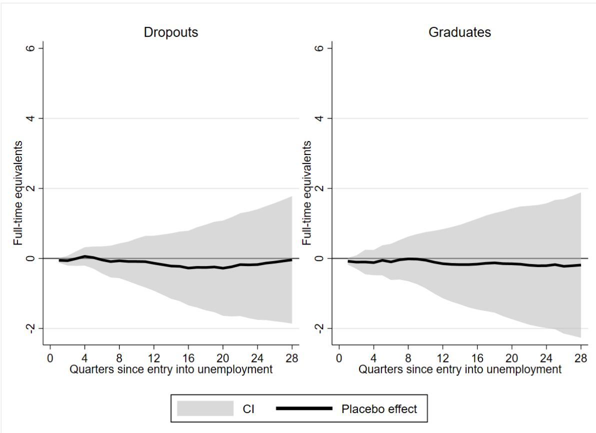
(a) Private sector employment



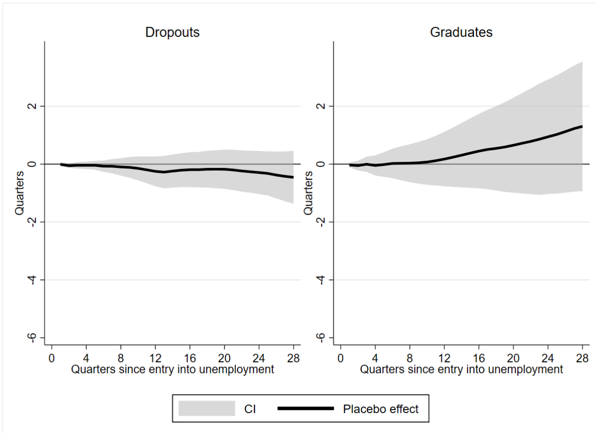
(b) Gross wage (private sector)



(c) Full-time equivalents (private sector)



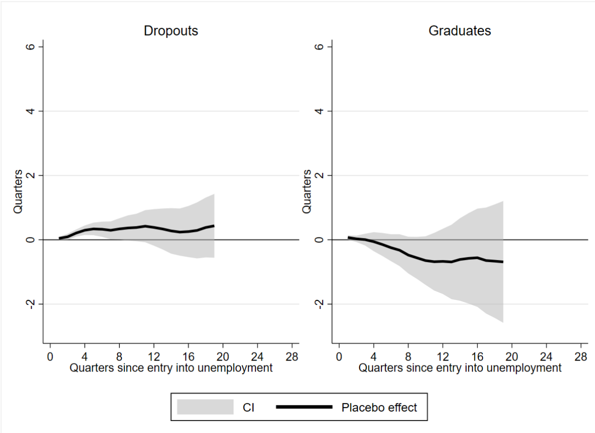
(d) Other employment



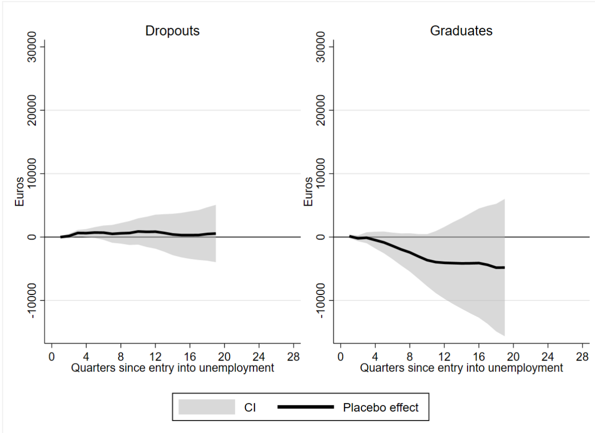
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2008, using age at unemployment entry as the forcing variable with placebo cutoffs. Evolution of the RDD placebo effect and confidence intervals (CI) for the cumulative (a) quarters in private sector employment, (b) gross remuneration, (c) full-time equivalents in private sector employment (1 for a full-time job in the quarter), (d) quarters in self- and public employment, by schooling level: dropouts (left) vs. graduates (right). The RDD estimator is implemented for each quarter after entry into unemployment until 7 years later. We retain only units registering unemployment in 2008 when no treatment was in place. The donut RDD estimator removes observations for individuals aged between 25 and 26 at unemployment and retains units aged between 22 and 25 (left of the cutoff) and 26 to 29 (right of the cutoff). The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. N = 3,780 (dropouts) and 3,986 (graduates).

Figure A.33: Evolution of the RDD Effect on Cumulative Outcomes: Placebo in 2012

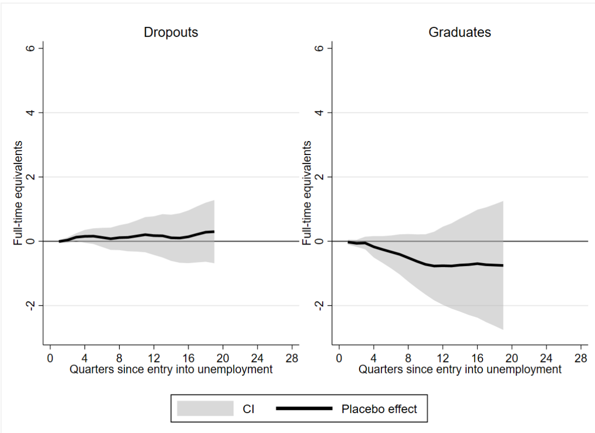
(a) Private sector employment



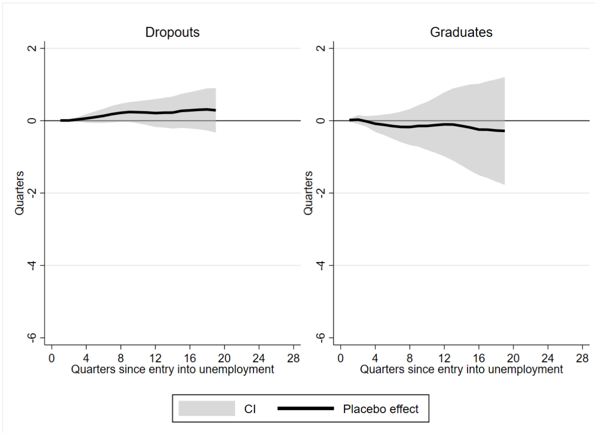
(b) Gross wage (private sector)



(c) Full-time equivalents (private sector)



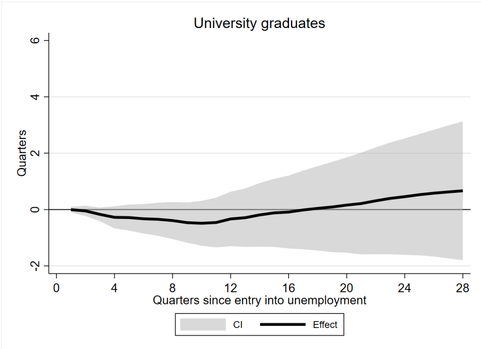
(d) Other employment



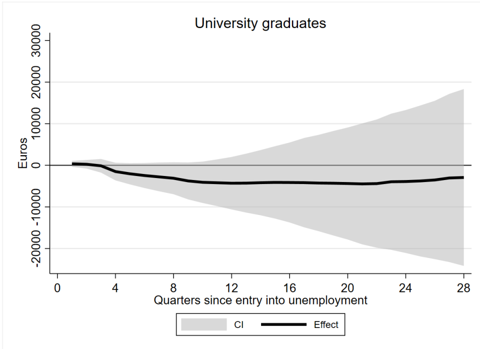
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2012, using age at unemployment entry as the forcing variable with placebo cutoffs. Evolution of the RDD placebo effect and confidence intervals (CI) for the cumulative (a) quarters in private sector employment, (b) gross remuneration, (c) full-time equivalents in private sector employment (1 for a full-time job in the quarter), (d) quarters in self- and public employment, by schooling level: dropouts (left) vs. graduates (right). The RDD estimator is implemented for each quarter after entry into unemployment until 7 years later. We retain only units registering unemployment in 2012, when no treatment was in place. The donut RDD estimator removes observations for individuals aged between 25 and 26 at unemployment and retains units aged between 22 and 25 (left of the cutoff) and 26 to 29 (right of the cutoff). The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. N = 4,468 (dropouts) and 4,234 (graduates).

Figure A.34: Evolution of the RDD Placebo Effect on Cumulative Outcomes: Post-secondary Graduates

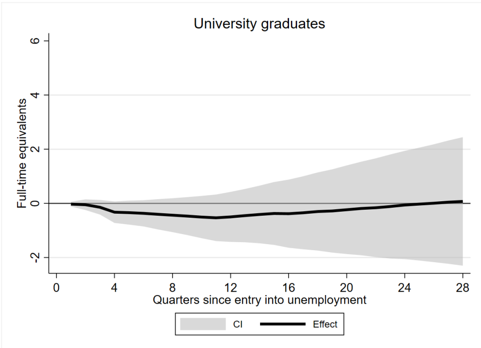
(a) Private sector employment



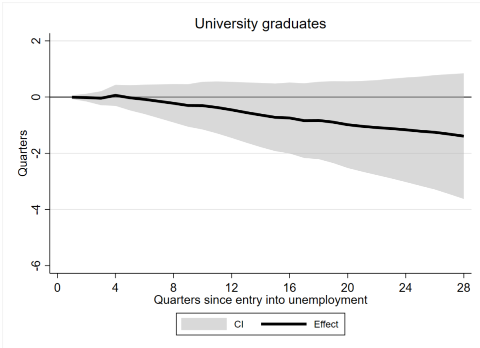
(b) Gross wage (private sector)



(c) Full-time equivalents (private sector)



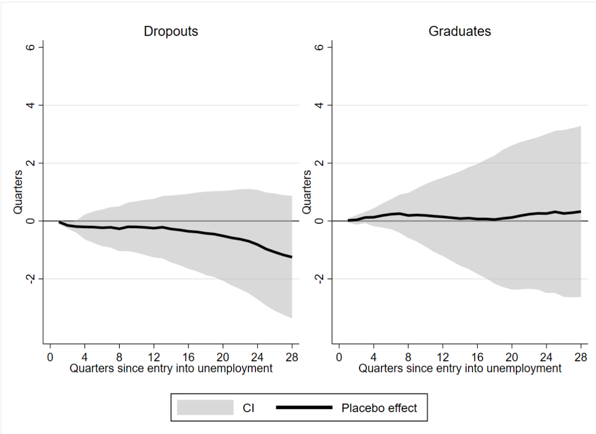
(d) Other employment



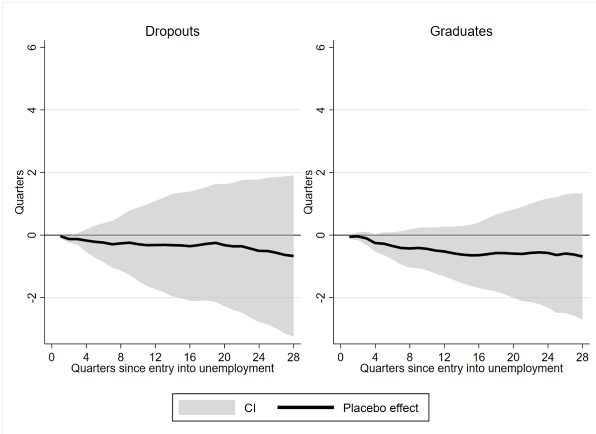
Note: Donut RDD estimates on the inflow sample of post-secondary graduates entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence intervals (CI) for (a) the cumulative number of quarters in private sector employment, (b) gross remuneration in private sector employment, (c) full-time equivalents in private sector employment (1 for a full-time job in the quarter), (d) quarters in self- and public employment, since unemployment in 2010. The RDD estimator is implemented for each quarter after entry into unemployment until 7 years later. The donut RDD estimator removes observations for individuals aged between 25 and 26 at unemployment and retains units aged between 22 and 25 (left of the cutoff) and 26 to 29 (right of the cutoff). The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. N = 3,993.

Figure A.35: Evolution of the RDD Effect on Cumulative Number of Quarters in Private Sector Employment: False Cutoffs

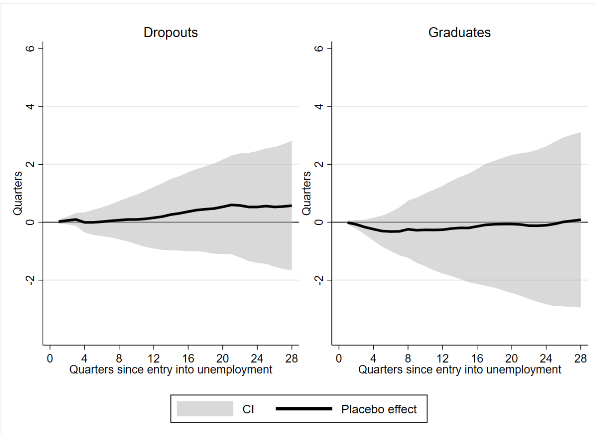
(a) 27



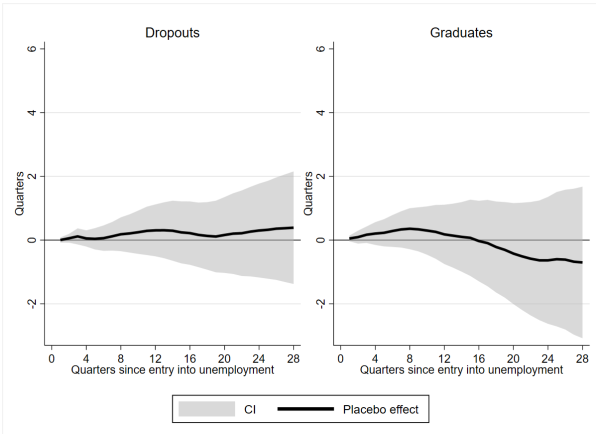
(b) 28



(c) 29



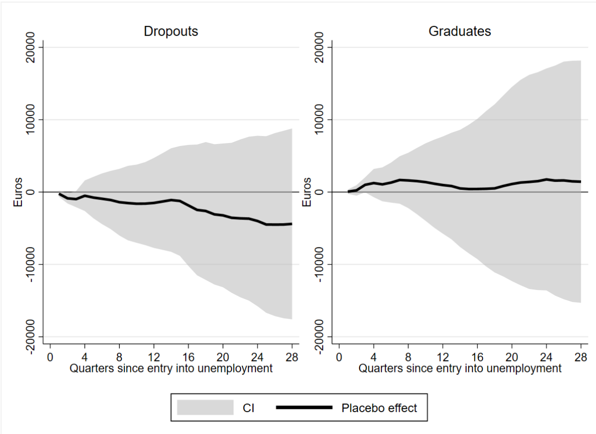
(d) 30



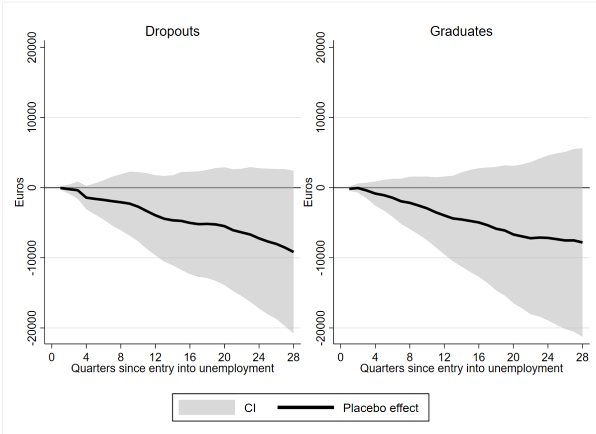
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with placebo cutoffs. The cutoff point and the one-year donut on its left are moved to (a) 27, (b) 28, (c) 29, and (d) 30 years of age. Evolution of the RDD effect and confidence interval (CI) for the cumulative quarters in private sector employment by schooling level: dropouts (left) vs. graduates (right). The RDD estimator is implemented for each quarter after entry into unemployment until 7 years later. As for the benchmark scenario, we maintain a one-year "hole" on the left of the discontinuity and impose a 3-year bandwidth on each side of the donut. The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level.

Figure A.36: Evolution of the RDD Effect on Cumulative Gross Wage in the Private Sector: False Cutoffs

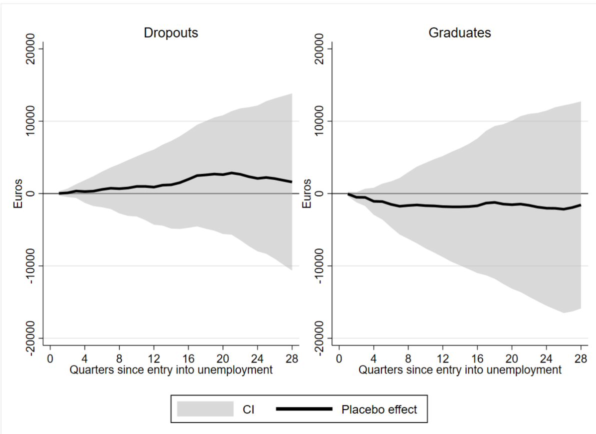
(a) 27



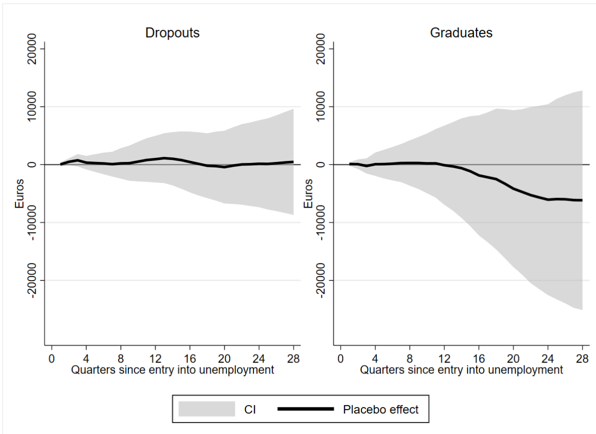
(b) 28



(c) 29



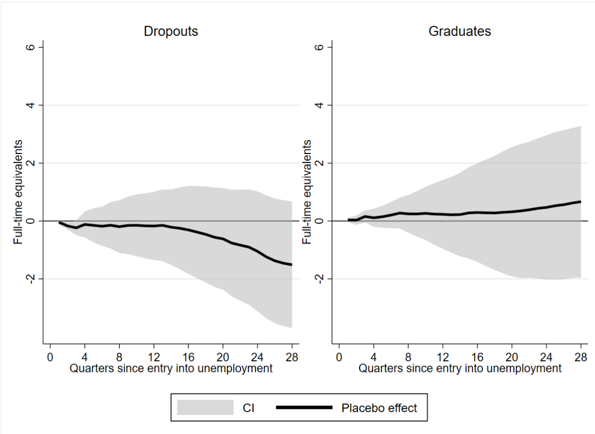
(d) 30



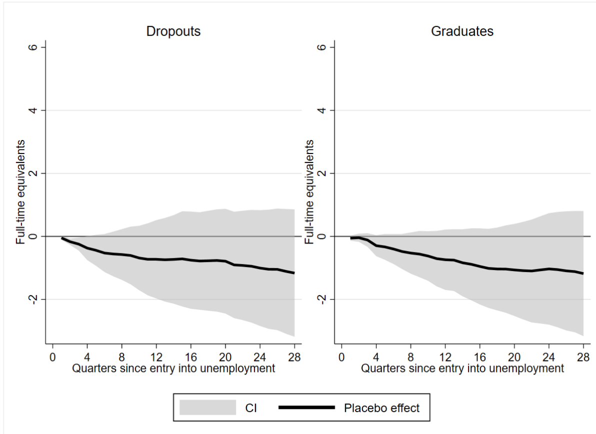
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with placebo cutoffs. The cutoff point and the one-year donut on its left are moved to (a) 27, (b) 28, (c) 29, and (d) 30 years of age. Evolution of the RDD effect and confidence interval (CI) for the cumulative gross remuneration in the private sector by schooling level: dropouts (left) vs. graduates (right). The RDD estimator is implemented for each quarter after entry into unemployment until 7 years later. As for the benchmark scenario, we maintain a one-year "hole" on the left of the discontinuity and impose a 3-year bandwidth on each side of the donut. The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level.

Figure A.37: Evolution of the RDD Effect on the Cumulative Percentage of Full-Time Equivalent Quarters in Private Sector Employment: False Cutoffs

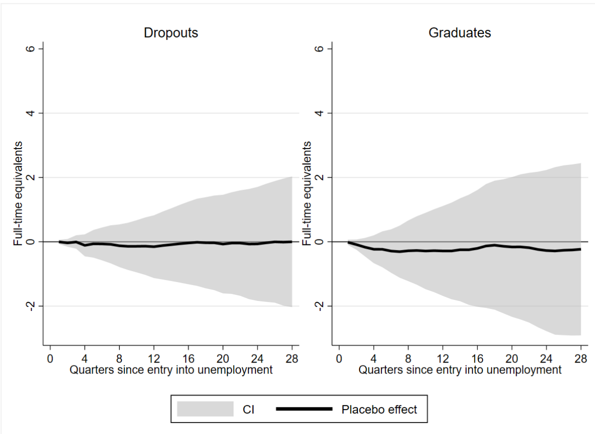
(a) 27



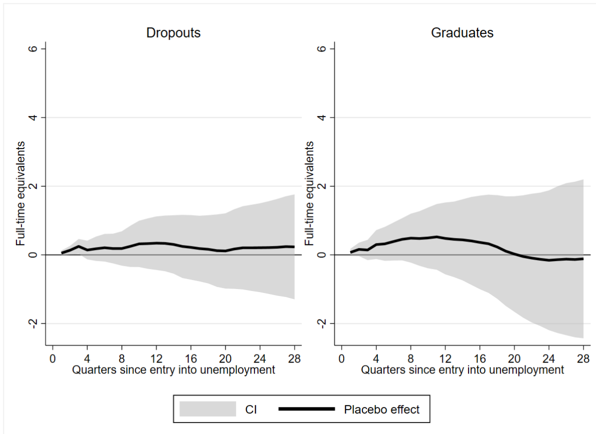
(b) 28



(c) 29



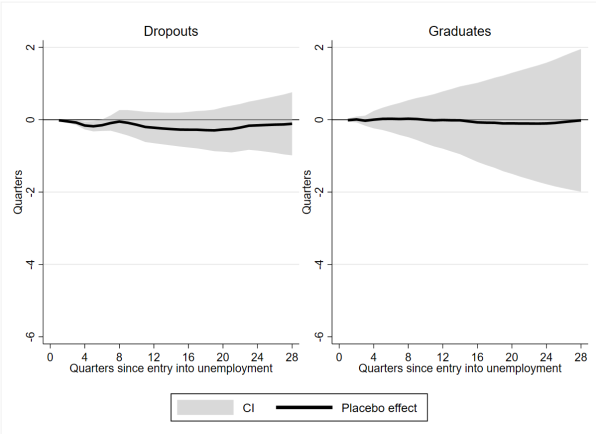
(d) 30



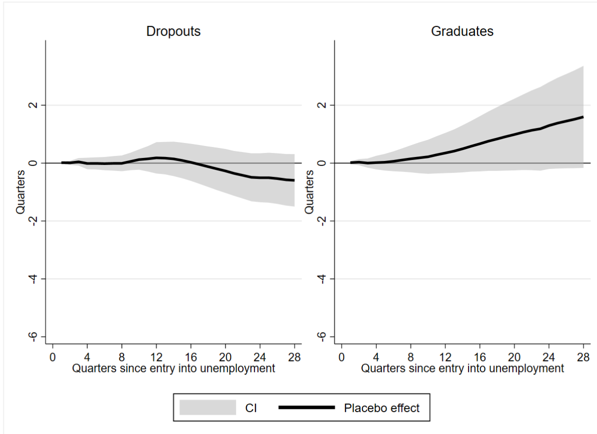
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with placebo cutoffs. The cutoff point and the one-year donut on its left are moved to (a) 27, (b) 28, (c) 29, and (d) 30 years of age. Evolution of the RDD effect and confidence interval (CI) for the cumulative full-time equivalents in private sector employment (1 for a full-time job in the quarter) by schooling level: dropouts (left) vs. graduates (right). The RDD estimator is implemented for each quarter after entry into unemployment until 7 years later. As for the benchmark scenario, we maintain a one-year "hole" on the left of the discontinuity and impose a 3-year bandwidth on each side of the donut. The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level.

Figure A.38: Evolution of the RDD Effect on the Cumulative Number of Quarters in Non-Private Sector Employment: False Cutoffs

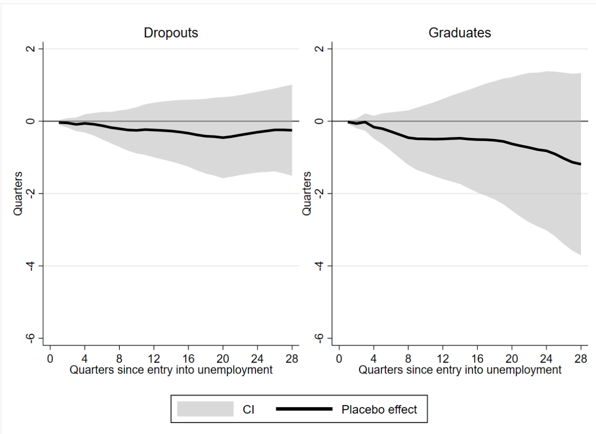
(a) 27



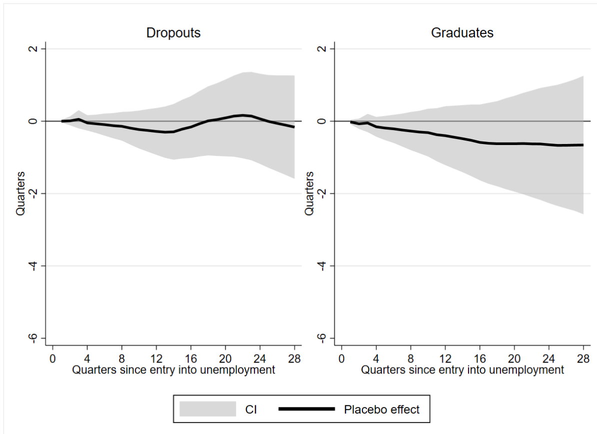
(b) 28



(c) 29



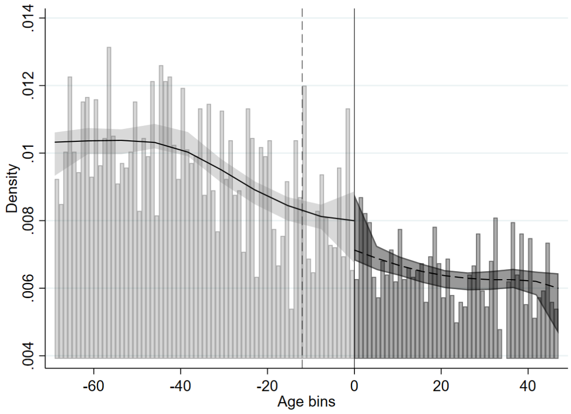
(d) 30



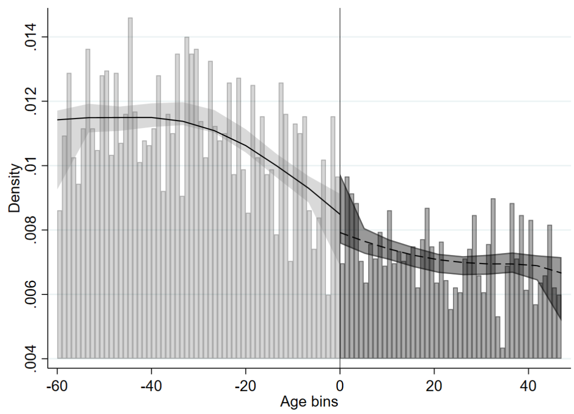
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with placebo cutoffs. The cutoff point and the one-year donut on its left are moved to (a) 27, (b) 28, (c) 29, and (d) 30 years of age. Evolution of the RDD effect and confidence interval (CI) for the cumulative number of quarters in self- and public employment by schooling level: dropouts (left) vs. graduates (right). The RDD estimator is implemented for each quarter after entry into unemployment until 7 years later. As for the benchmark scenario, we maintain a one-year "hole" on the left of the discontinuity and impose a 3-year bandwidth on each side of the donut. The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level.

Figure A.39: RDD density tests

(a) Density test (ignoring the donut)



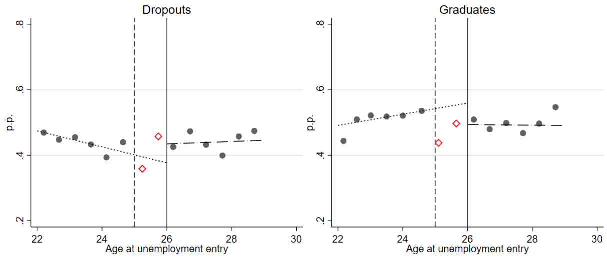
(b) Density test (filling the donut)



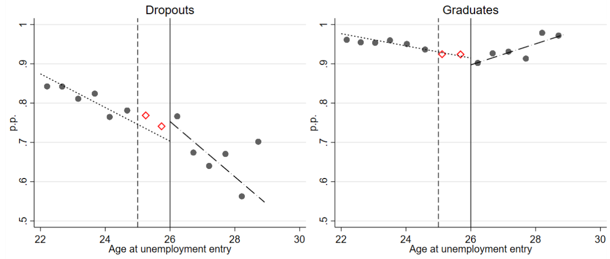
Note: RDD density test the density tests on the cutoff (age = 26) of Cattaneo et al. (2020). Results obtained using the Stata routine rddensity.ado using the default options: q = 3 (p = 2) polynomial for the bias (the estimation), triangular Kernel, density estimation without any restrictions (two-sample, unrestricted inference). Age is shown in bins of monthly-age (1 bin = 0.083 years old). In panel (b) the twelve bins inside the donut are dropped and the units on the left of the donut shifted to fill the donut. The p-values are 0.497 (panel a) and 0.533 (panel b).

Figure A.40: Discontinuity at Age 26 on the Covariates used in the RDD

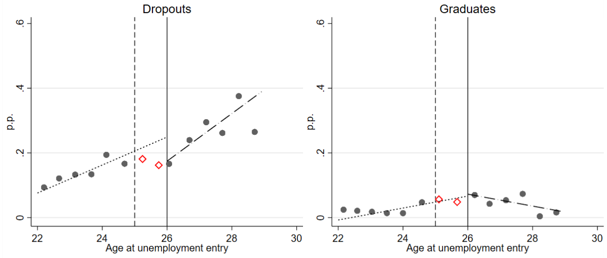
(a) Woman



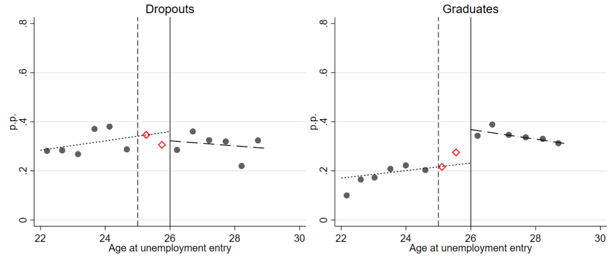
(b) Belgian nationality



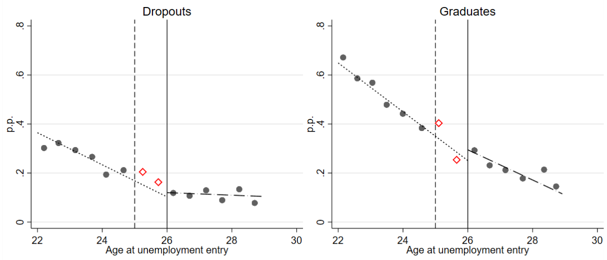
(c) Other nationality



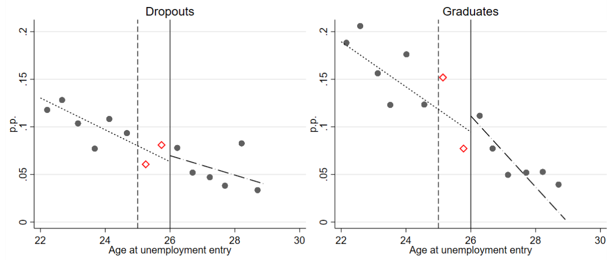
(d) One-person household



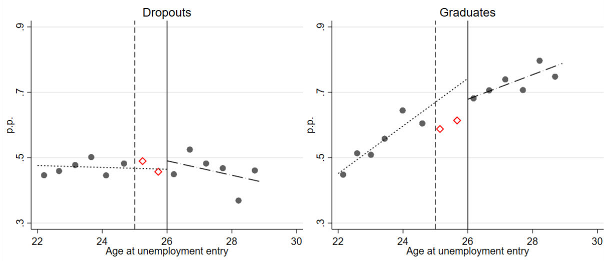
(e) Child of a dual-parent household



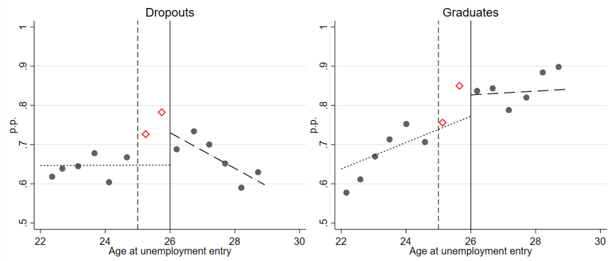
(f) Child of a single-parent household



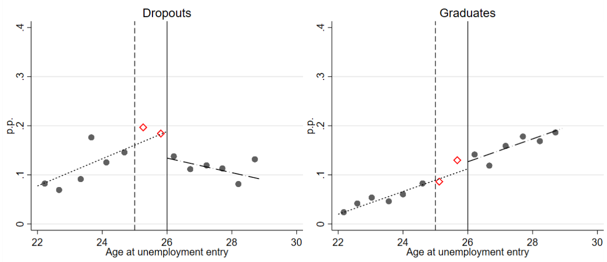
(g) Receiving unemployment benefits



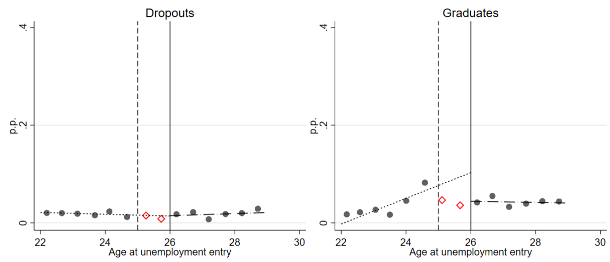
(h) Any experience between 1 and 4 years before



(i) Activation policy between 1 and 4 years before



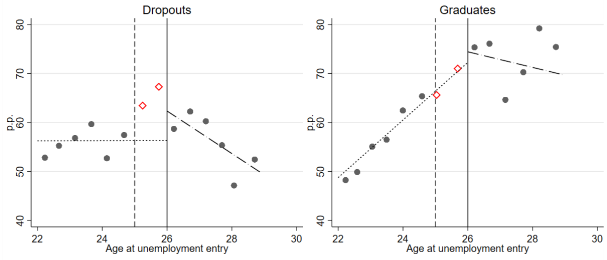
(j) Last job as cross-border worker



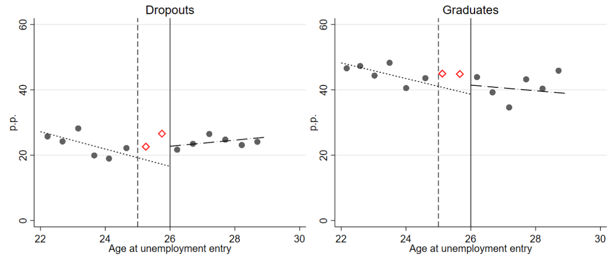
[Continue in the next page]

Figure A.40: Discontinuity at Age 26 on the Covariates used in the RDD (continue)

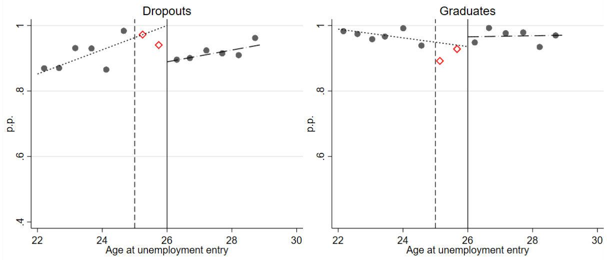
(k) Last job full-time equivalents (100=full-time)



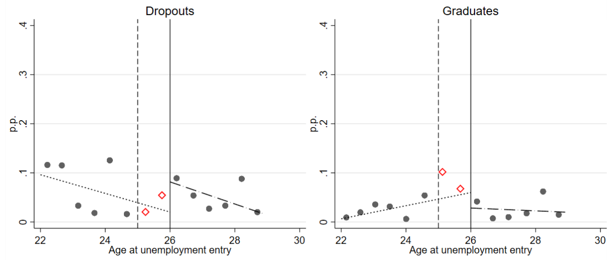
(l) Household full-time equivalents one year before



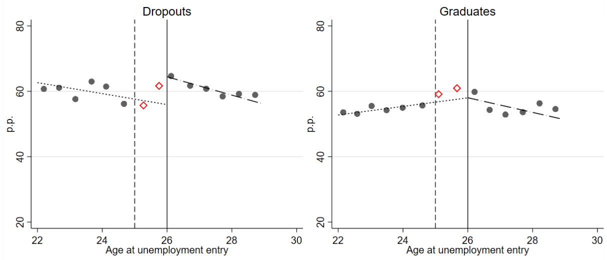
(m) Wallonia



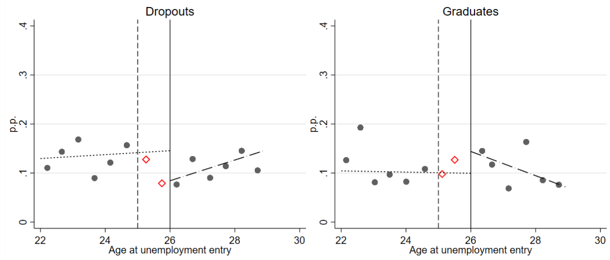
(n) Brussels



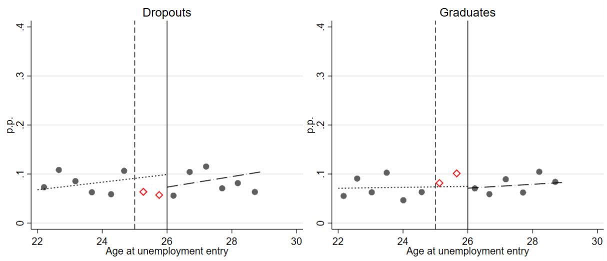
(o) Minutes by car to border with Luxembourg



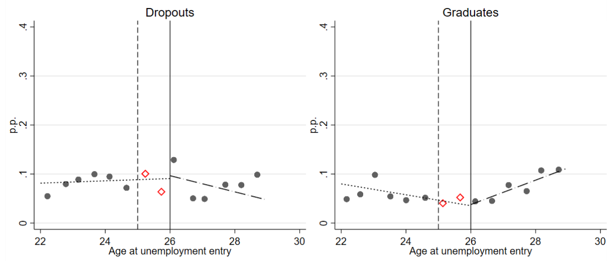
(p) January



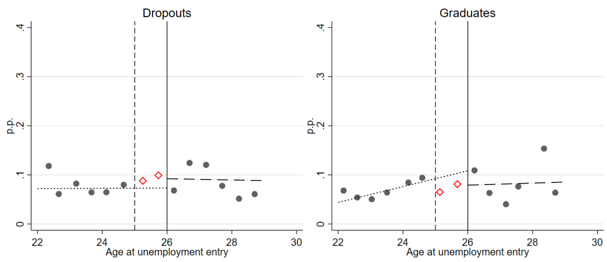
(q) February



(r) March



(s) April



(t) May

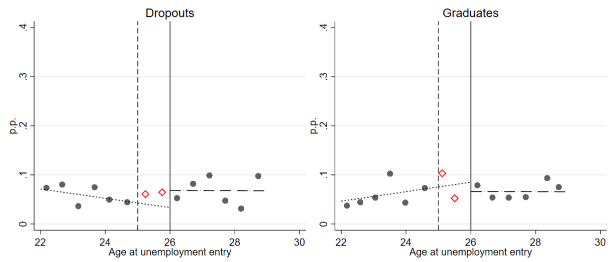


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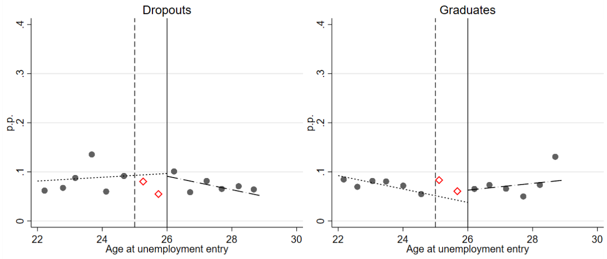
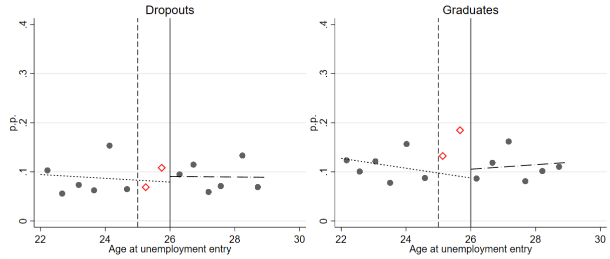
Figure A.40: Discontinuity at Age 26 on the Covariates used in the RDD (continue)

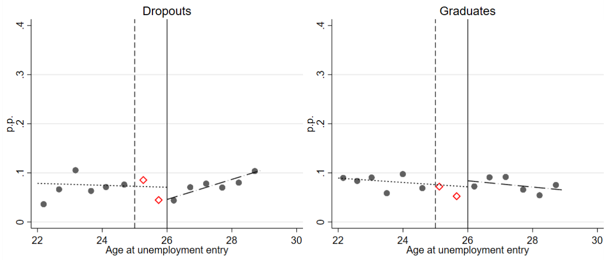
(u) June



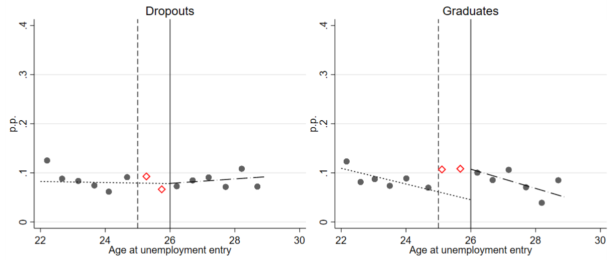
(v) July



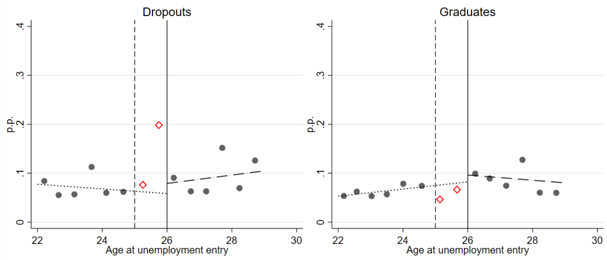
(w) August



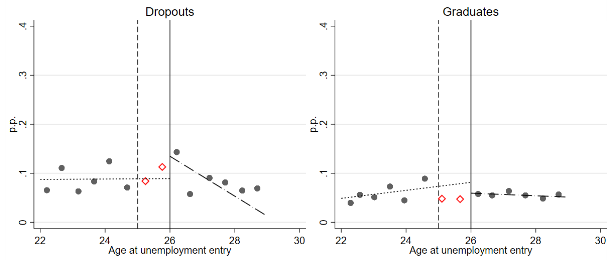
(x) October



(y) November



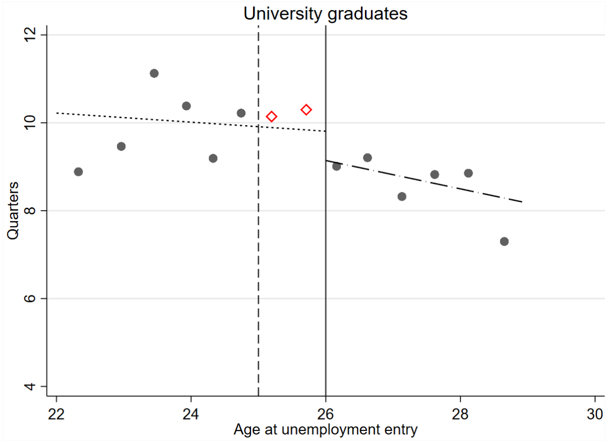
(z) December



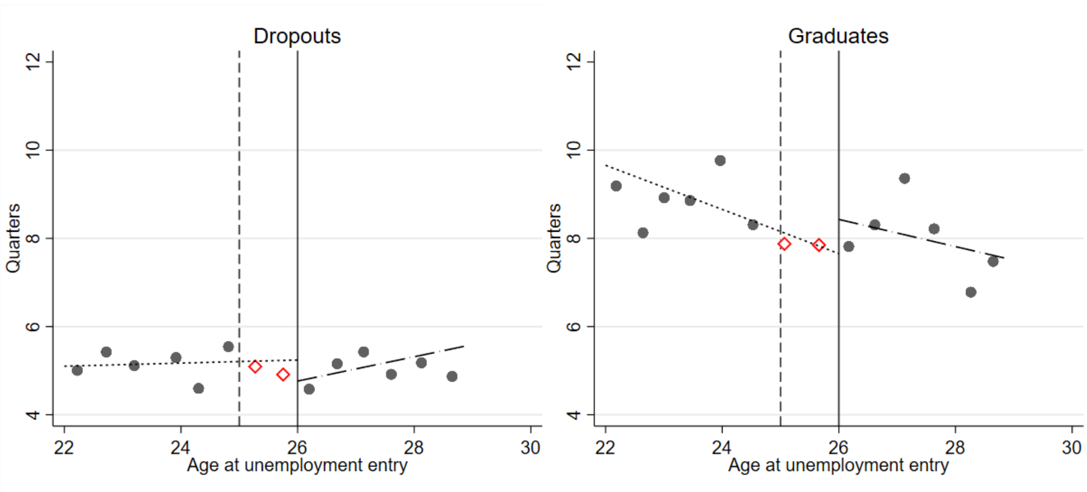
Donut RDD estimate on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. The left panels refer to high school dropouts, while the right panels refer to high school graduates. The outcome are the set of control variables shown in Table C.2, which are plotted over six age-quantile-spaced bins on each side of the donut. The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but by removing the units within the donut. The effect estimated are shown in Table H.1.

Figure A.41: RDD: Placebo Tests on Long Run Accumulated Quarters in Private Sector Employment

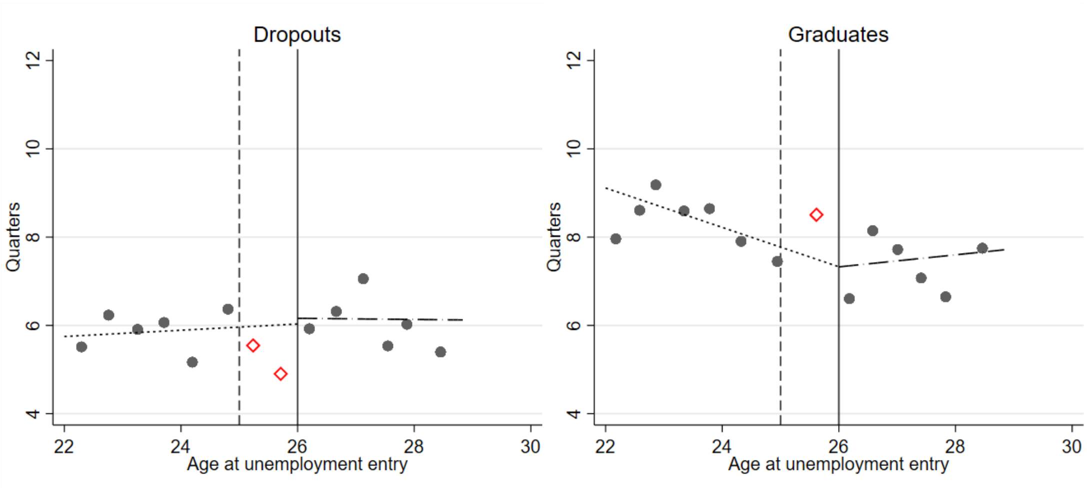
(a) Post-secondary graduates



(b) Unemployment entries in 2012



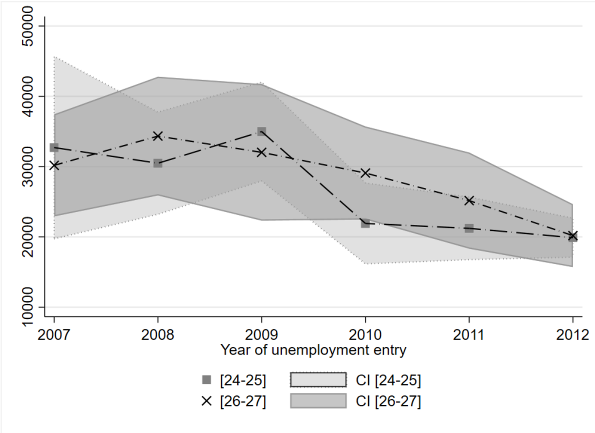
(c) Unemployment entries in 2008



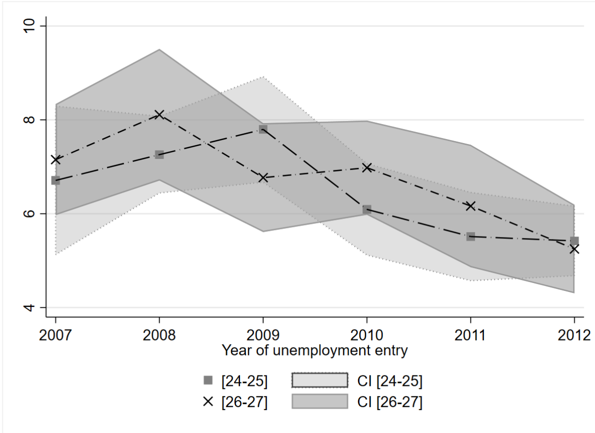
Note: Donut RDD estimate on the inflow sample of youths entering unemployment in 2010 (panel a), 2008 (panel b) or 2012 (panel c), using age at unemployment entry as the forcing variable with a cutoff at 26. Panel (b) and (c) retain high school dropouts (left panel) and graduates (right panel), while panel (a) retains post-secondary graduates only. For panel (a) and (c) the outcome is the cumulative number of quarters employed in private sector employment seven (five in panel (b)) years after entry into unemployment, which is plotted over six age-quantile-spaced bins on each side of the donut. The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but by removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3.

Figure A.42: Evolution of the Cumulative Outcomes at 5 years distance - Dropouts

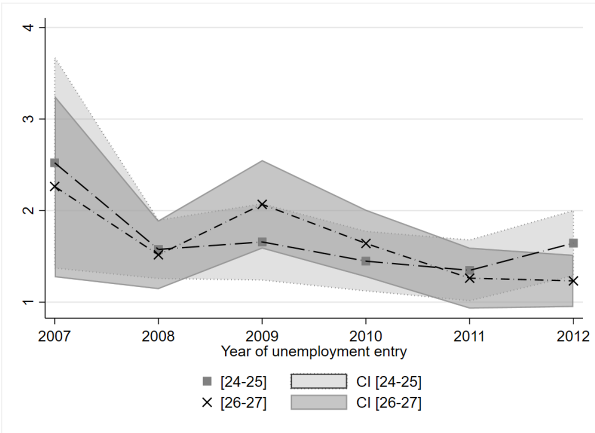
(a) Gross wage (private sector)



(b) Full-time equivalents (private sector)



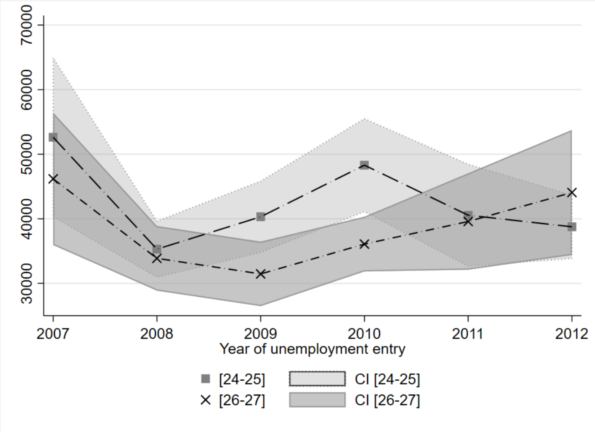
(c) Other employment



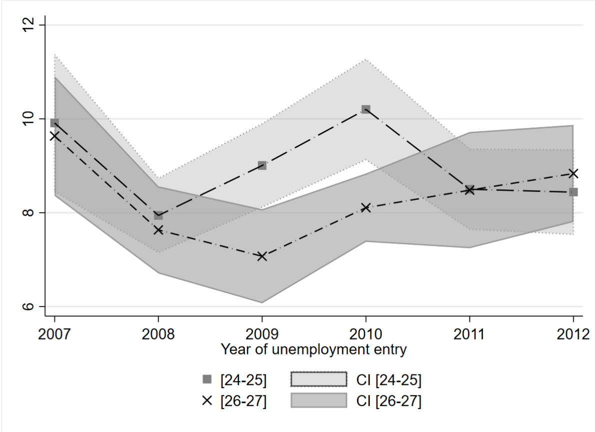
Note: Evolution of the cumulative outcomes measured at 5 years distance such as (a) gross remuneration in private sector employment, (b) full-time equivalents in private sector employment (1 for a full-time job in the quarter), (c) quarters in self- and public employment, since unemployment for dropouts. The treated are aged 24-25 at unemployment entry, while controls are aged 26-27. Data are reweighted by the sampling weights.

Figure A.43: Evolution of the Cumulative Outcomes at 5 years distance - Graduates

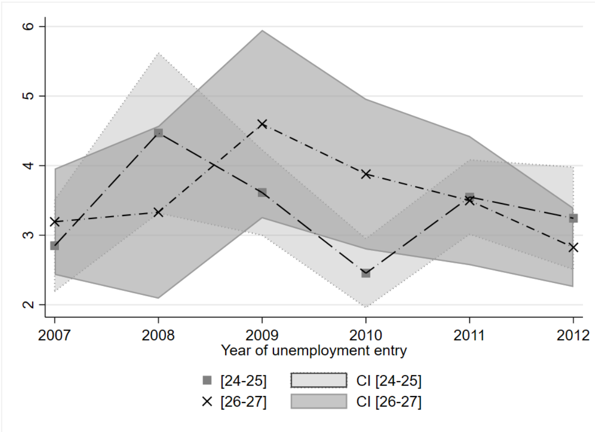
(a) Gross wage (private sector)



(b) Full-time equivalents (private sector)



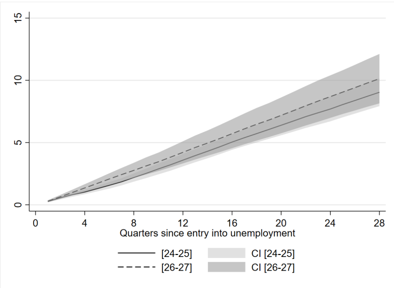
(c) Other employment



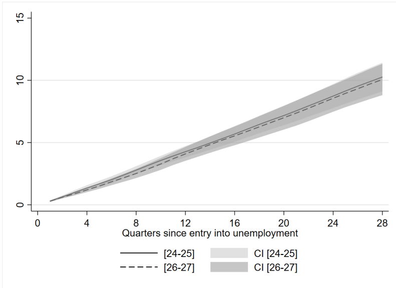
Note: Evolution of the cumulative outcomes measured at 5 years distance such as (a) gross remuneration in private sector employment, (b) full-time equivalents in private sector employment (1 for a full-time job in the quarter), (c) quarters in self- and public employment, since unemployment for graduates. The treated are aged 24-25 at unemployment entry, while controls are aged 26-27. Data are reweighted by the sampling weights.

Figure A.44: Evolution by Entry Cohort and Schooling Level of the Cumulative Number of Quarters in Private Sector Employment over the Elapsed Time since Registration

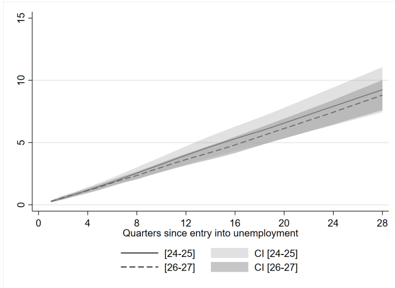
(a) Dropouts - 2008 entries



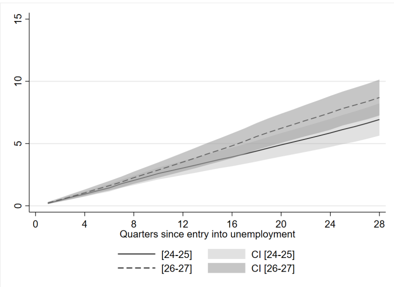
(b) High school - 2008 entries



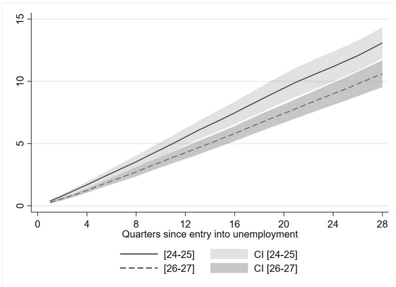
(c) Post-secondary - 2008 entries



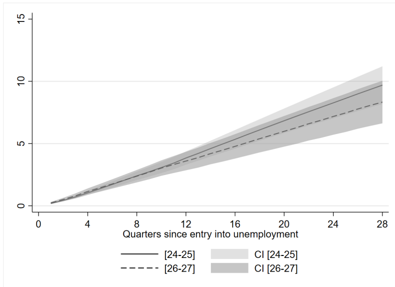
(d) Dropouts - 2010 entries



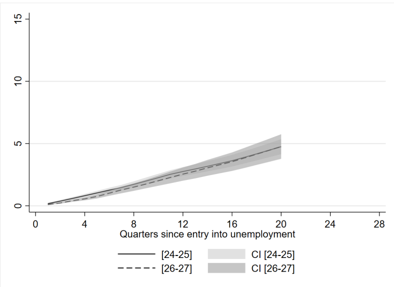
(e) High school - 2010 entries



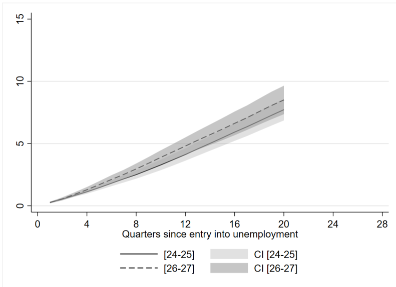
(f) Post-secondary - 2010 entries



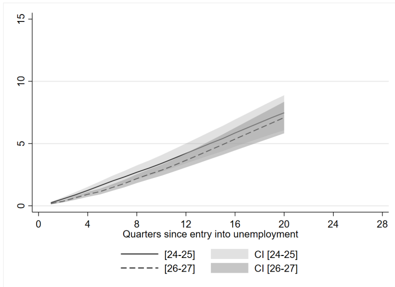
(g) Dropouts - 2012 entries



(h) High school - 2012 entries



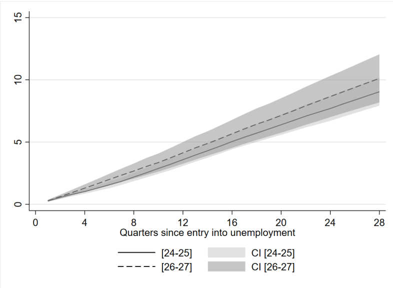
(i) Post-secondary - 2012 entries



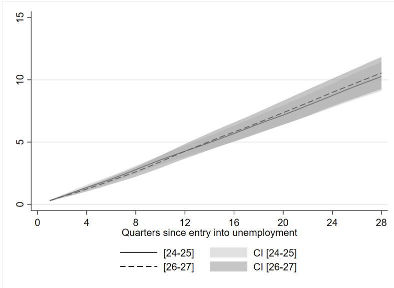
Note: Evolution by entry cohort and schooling level of the cumulative number of quarters in private sector employment over elapsed time since registration. The treated are aged 24-25 at entry into unemployment, while controls are aged 26-27. Data are reweighted by the sampling weights. Standard errors are clustered at the individual level.

Figure A.45: Evolution by Entry Cohort and Schooling Level of the Cumulative Number of Quarters in Private Sector Employment over Elapsed Time since Registration (Controlling for Observables)

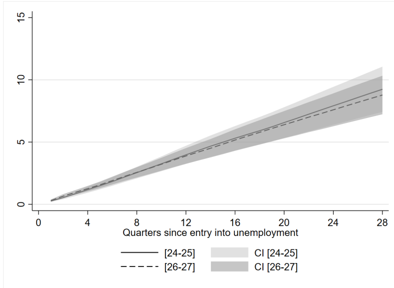
(a) Dropouts - 2008 entries



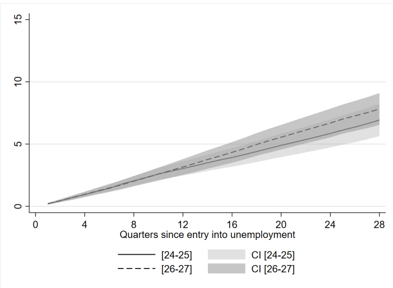
(b) High school - 2008 entries



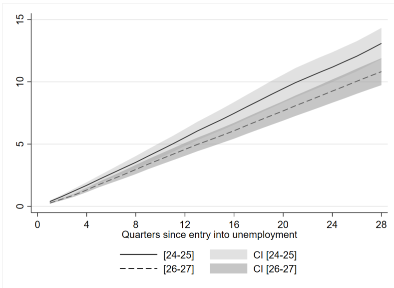
(c) Post-secondary - 2008 entries



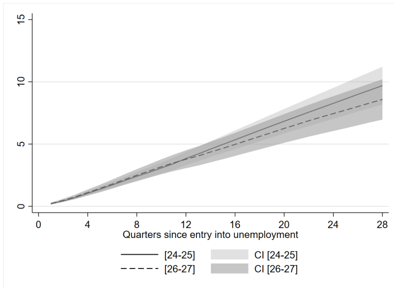
(d) Dropouts - 2010 entries



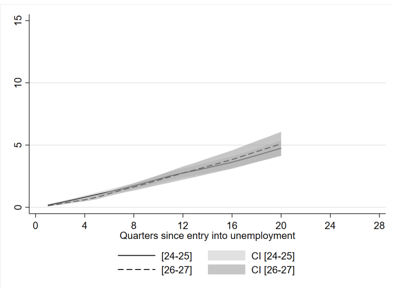
(e) High school - 2010 entries



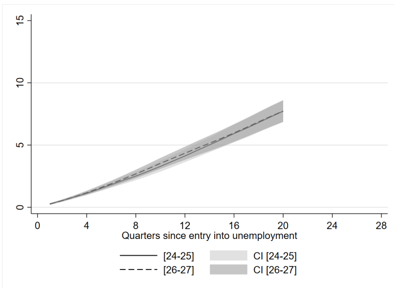
(f) Post-secondary - 2010 entries



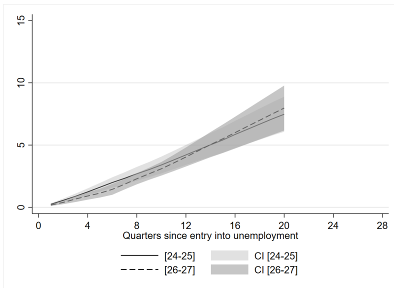
(g) Dropouts - 2012 entries



(h) High school - 2012 entries



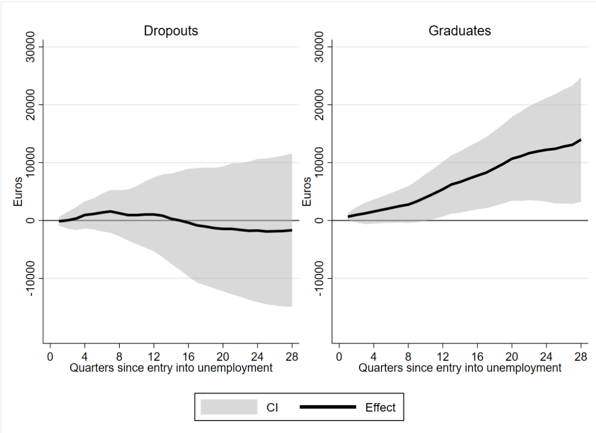
(i) Post-secondary - 2012 entries



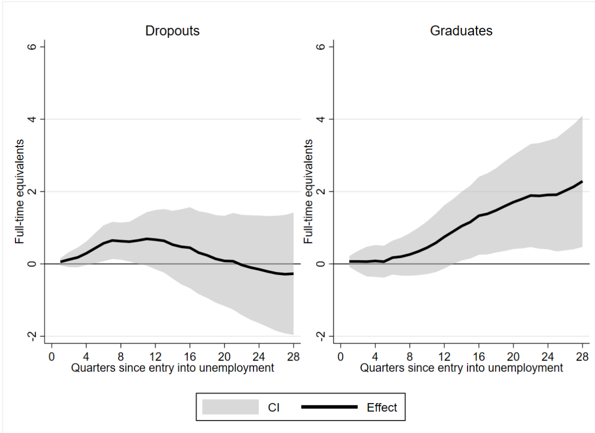
Note: Evolution by entry cohort and schooling level of the cumulative number of quarters in private sector employment over the elapsed time since registration. The treated are aged 24-25 at unemployment entry, while controls are aged 26-27. Data are reweighted by the sampling weights multiplied by the IPW weights to control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the individual level.

Figure A.46: Evolution of the DiD Effect on Cumulative Outcomes

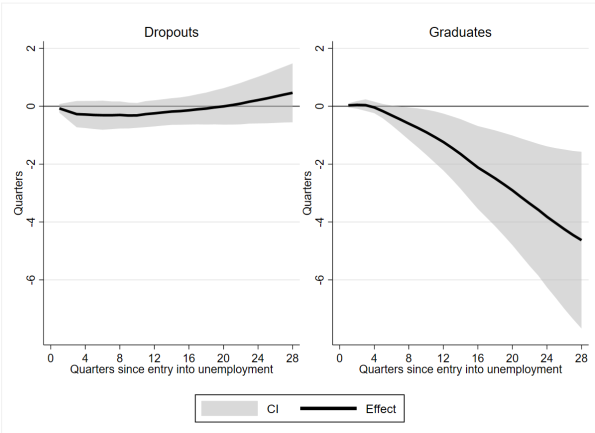
(a) Gross wage (private sector)



(b) Full-time equivalents (private sector)



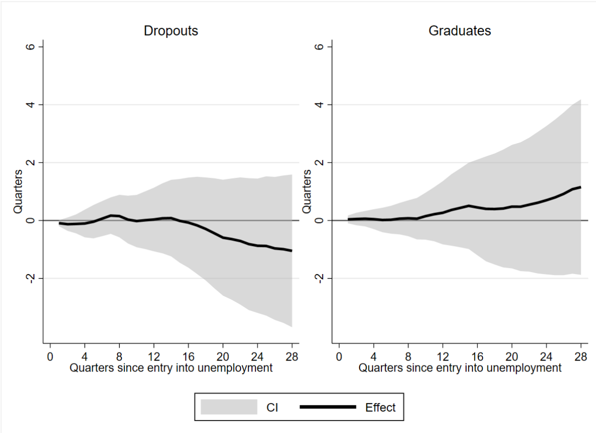
(c) Other employment



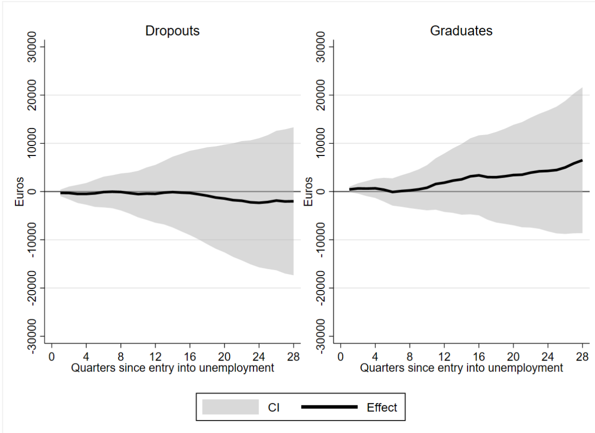
Note: Evolution of the effect estimated with a doubly robust DiD estimator (Sant'Anna and Zhao, 2020) and confidence intervals (CI) for (a) gross remuneration in private sector employment, (b) full-time equivalents in private sector employment (1 for a full-time job in the quarter), (c) quarters in self- and public employment since unemployment and by schooling level: dropouts (left) vs. graduates (right). The DiD estimator is implemented for each year after entry into unemployment until 7 years later. The treated are aged 24-25 at unemployment entry, while controls are aged 26-27. Units registering in (2008) 2010 are considered in the (pre-) treatment period. Data are reweighted by the sampling weights. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. N = 1,942 (dropouts) and 1,839 (graduates).

Figure A.47: Evolution of the DiD Effect on Cumulative Outcomes: Near the Border

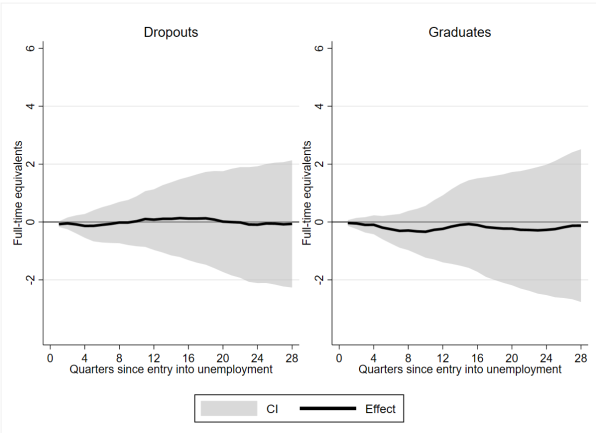
(a) Private sector employment



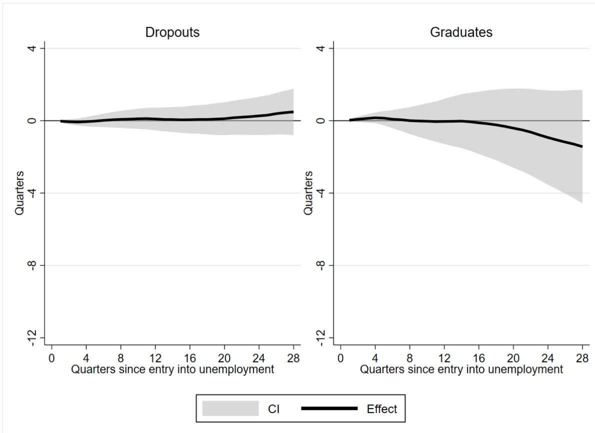
(b) Gross wage (private sector)



(c) Full-time equivalents (private sector)



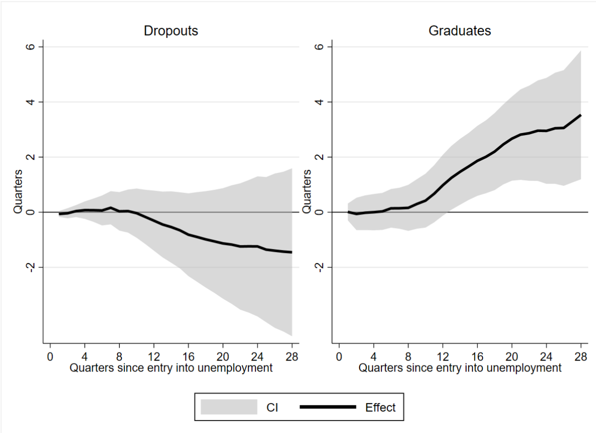
(d) Other employment



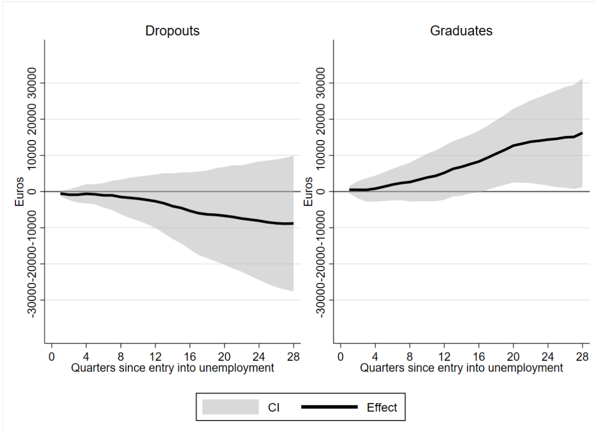
Note: Evolution of the effect estimated with a doubly robust DiD estimator (Sant'Anna and Zhao, 2020) and confidence intervals (CI) for (a) the cumulative number of quarters in private sector employment, (b) gross remuneration in private sector employment, (c) full-time equivalents in private sector employment (1 for a full-time job in the quarter), (d) quarters in self- and public employment, since unemployment and by schooling level: dropouts (left) vs. graduates (right). The DiD estimator is implemented for each year after entry into unemployment until 7 years later. The treated are aged 24-25 at unemployment entry, while controls are aged 26-27. Units registering in (2008) 2010 are considered in the (pre-)treatment period. We retain only units living within 60 minutes by car from the border with Luxembourg. Data are reweighted by the sampling weights. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. N = 1,237 (dropouts) and 1,069 (graduates).

Figure A.48: Evolution of the DiD Effect on Cumulative Outcomes: Far from the Border

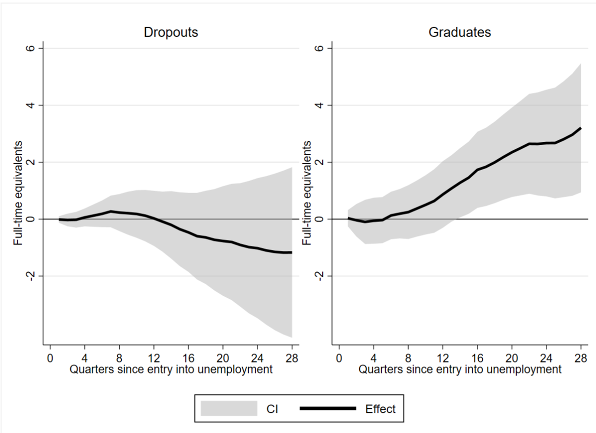
(a) Private sector employment



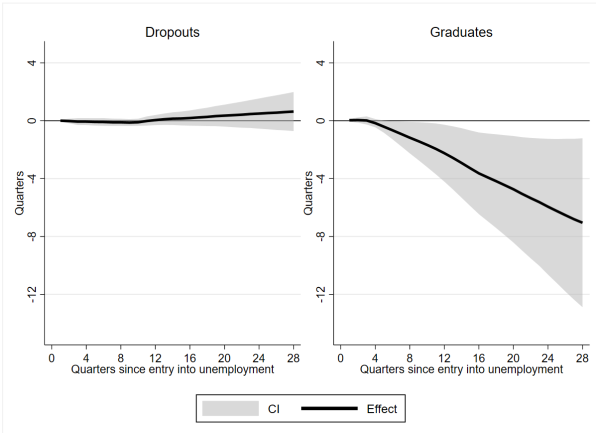
(b) Gross wage (private sector)



(c) Full-time equivalents (private sector)



(d) Other employment



Note: Evolution of the effect estimated with a doubly robust DiD estimator (Sant'Anna and Zhao, 2020) and confidence intervals (CI) for (a) the cumulative number of quarters in private sector employment, (b) gross remuneration in private sector employment, (c) full-time equivalents in private sector employment (1 for a full-time job in the quarter), (d) quarters in self- and public employment, since unemployment and by schooling level: dropouts (left) vs. graduates (right). The DiD estimator is implemented for each year after entry into unemployment until 7 years later. The treated are aged 24-25 at unemployment entry, while controls are aged 26-27. Units registering in (2008) 2010 are considered in the (pre-)treatment period. We retain only units living more than 60 minutes by car from the border with Luxembourg. Data are reweighted by the sampling weights. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. N = 677 (dropouts) and 766 (graduates).

B Description of the Stratified Sampling Procedure

We select the population born between December 31, 1972, and December 31, 1990, and retain only individuals who lived in the Province of Luxembourg or the selected municipalities of the provinces of Liège and Namur (see Figure B.1) between January 1, 2006, and January 1, 2017. This group of individuals defines the "population of interest", which is divided into 10 strata.

1. The population is first divided into 5 mutually exclusive geographical strata sorted by the incidence of cross-border employment (darker blue in Figure B.1) based on the 2011 census:[1]

   - 1st stratum: Individuals who between January 1, 2006, and January 1, 2017, lived in one of the municipalities where the incidence of cross-border employment in 2011 was above 30.6%;
   - 2nd stratum: Among the individuals not selected in the 1st stratum, take all individuals who in the same period lived in one of the municipalities where the incidence of cross-border employment in 2011 was between 14.9% and 30.5%;
   - 3rd stratum: Among the individuals not selected in the 1st and 2nd strata, take all individuals who in the same period lived in one of the municipalities where the incidence of cross-border employment in 2011 was between 5.6% and 14.8%;
   - 4th stratum: Among the individuals not selected in the 1st, 2nd, and 3rd strata, take individuals who in the same period lived in one of the municipalities where the incidence of cross-border employment in 2011 was between 1.7% and 5.5%;
   - 5th stratum: All other individuals.

2. Divide each stratum into two additional sub-strata depending on whether the individuals are registered as new unemployed jobseekers in the regional public employment offices (FOREM and ADG) between 2008 and 2013:

   - Individuals who are registered as unemployed jobseekers in any month between 2008 and 2013 but who were not registered in the previous calendar month;

[1] Source: https://www.census2011.be/analyse/flux_fr.html

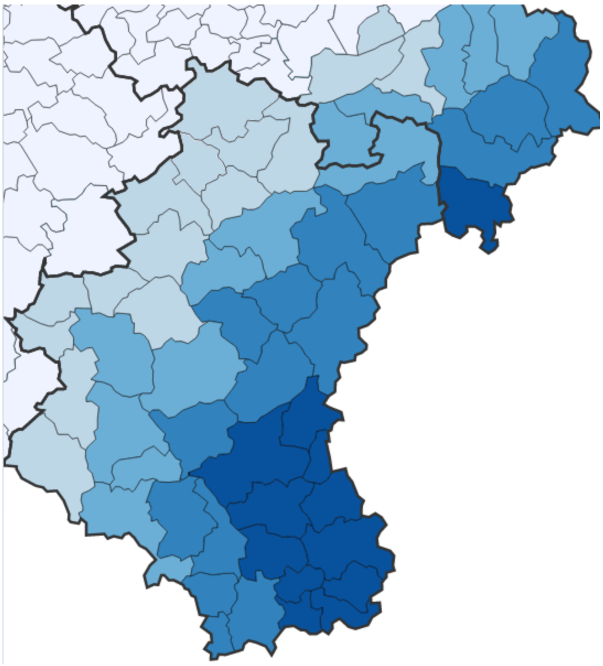
- All other individuals.

A random sample without replacement is drawn from each of the 10 strata using a random number generator. The number of individuals thus selected varies according to the strata and is shown in Table B.1. We oversample the geographical strata near Luxembourg (first geographical strata) and people registering as unemployed jobseekers. The data are appropriately reweighted by the corresponding sampling weights to take this stratification into account and be representative of the population of interest (Manski and Lerman, 1977; Cameron and Trivedi, 2005; Albanese and Cockx, 2019). In total, the sample consists of 125,000 individuals.

Table B.1: Stratification and Sample Size

Geographic strata	Unemployed jobseekers 2008-2013	Population Size	Sample Size	% Sampled
1	Yes	12,391	11,500	92.8%
1	No	24,731	18,500	74.8%
2	Yes	10,207	9,000	88.2%
2	No	17,747	12,500	70.4%
3	Yes	8,699	7,000	80.5%
3	No	13,880	9,000	64.8%
4	Yes	10,563	8,000	75.7%
4	No	18,777	11,000	58.6%
5	Yes	56,903	34,000	59.8%
5	No	96,988	4,500	4.6%
All	All	270,886	125,000	46.1%

Figure B.1: Population of Interest and Strata



Note: The population of interest is stratified into five geographical strata according to the percentage of cross-border workers over the active population in the municipality (Census 2011 – SPF Economie, see https://www.census2011.be/analyse/flux_fr.html): [0.0%; 1.6%], [1.7%-5.5%], [5.6%; 14.8%], [14.9%-30.5%], [30.6%-60.7%]. Darker blue areas have a higher probability of sampling. The fifth stratum is not shown on the map and comprises the municipalities of Liège and Namur.

C Institutional Context and Descriptive Statistics

C.1 Pre-existing Deductions of Social Security Contributions

Both Win-Win and Activa could be cumulated with pre-existing deductions of employers' social security contributions (SSC): the *structural reduction* of €133 per month, increased by a supplement for low wages, and the so-called *target group reduction*. The latter comprised reductions in SSC for the same long-term unemployed targeted by Activa, as well as for high school dropouts up to the age of 26.[2] The initial SSC reduction amounted to €333/month, but after a few months of employment, it decreased, first to €133/month and then to €0. The pace of this reduction depended on the target group. High school dropouts were only eligible for the lower SSC until the end of the quarter in which they turned 26. Therefore, the SSC reduction decreased gradually to zero at the age discontinuity threshold of 26. In contrast, the Win-Win subsidy was paid beyond the age of 26 as the age requirement had to be met only at hiring.

C.2 Outcomes

In the analysis, we consider the following outcomes. First, we focus on the cumulative quarterly transition rate during the first quarters after unemployment entry to 1) any subsidized private sector job and 2) any salaried private sector employment.[3] The exit rates allow us to evaluate whether the hiring subsidy can speed up recruitment, but it cannot inform us about whether the subsidy can persistently reinforce the employment of beneficiaries. This is why we also consider the following cumulative outcomes up to 7 years after unemployment registration: the number of quarters 3) in salaried private sector employment, 4) in subsidized employment only and 5) the full-time equivalent[4] number of quarters in salaried private sector employment, 6) the cumulative gross remuneration earned in the salaried private sector, and 7) the number of

[2] There also exists an SSC reduction for higher-educated youths aged below 30 years, but this subsidy is much smaller: €100/month for those 20 years old or younger, decreasing linearly with age to zero at age 30.

[3] Note that our database does not contain information on the type of contract and employment is observed only on the last day of a given quarter. The other outcomes we consider are the subsidy amount, both in absolute value and relative to wage costs, conditional on finding a subsidized job. We then use these amounts to construct an adjusted measure of take-up, considering the different generosity of the subsidies on both sides of the discontinuity.

[4] Note that this measures the full-time equivalent percentage in the job occupied at the end of the quarter. It does not take into account the fraction of time worked within the quarter.

quarters in any salaried public sector and self-employment.

Tables 2 and C.1 shows the descriptive statistics on the outcomes for the benchmark sample of unemployment registration in 2010. Column 1 of Table C.1 refers to the full sample used in the benchmark analysis: individuals aged between 22 and 29 at unemployment entry. Column 2 restricts the sample to potentially eligible individuals aged between 22 and 25, while column 3 focuses on those taking up the Win-Win subsidy within one year. Finally, the next columns divide columns 2 and 3 by educational attainment: column 4 (6) focuses on high school dropouts (graduates), while column 5 (7) considers only those who take up the subsidy.

About 16% of the full sample takes up a subsidized job within one year after unemployment registration. This share is higher among younger individuals satisfying the age condition for eligibility for the (youth version of the) Win-Win subsidy: 21%. Among eligible youths, almost the totality of subsidized jobs is supported by the Win-Win plan, due to its greater generosity compared to the other subsidies. Other outcomes used in the analysis are also shown in Table C.1.

Table C.1: Descriptive Statistics: Outcomes

	All			Dropouts		Graduates	
	22-29 All (1)	22-25 All (2)	22-25 Win-Win (3)	22-25 All (4)	22-25 Win-Win (5)	22-25 All (6)	22-25 Win-Win (7)
Take-up any subsidy in 1 year	0.16	0.21	1.00	0.20	1.00	0.21	1.00
	(0.36)	(0.41)	(0.00)	(0.40)	(0.00)	(0.41)	(0.00)
Total quarters in subsidized	1.37	1.66	4.60	1.47	4.41	1.80	4.75
salaried private sector employment in 7 years	(2.71)	(2.92)	(3.36)	(2.90)	(3.71)	(2.93)	(3.06)
Total full-time equivalents	10.7	11.3	15.5	9.0	13.6	13.1	16.96
in the salaried private sector in 7 years	(9.4)	(9.4)	(8.4)	(8.8)	(8.2)	(9.5)	(8.2)
Total gross remuneration from	49403.62	52649.83	71652.34	37351.15	54858.14	64802.51	84327.85
the salaried private sector in 7 years	(56736.51)	(56990.80)	(54843.67)	(46757.98)	(46109.37)	(61307.64)	(57484.31)
Total quarters in any	13.76	14.34	17.19	10.64	14.48	17.28	19.24
employment in 7 years	(9.80)	(9.67)	(8.43)	(9.35)	(8.51)	(8.89)	(7.78)
Total gross remuneration from	60960.13	64322.31	78284.42	43084.42	59814.50	81192.88	92224.70
public/private sector in 7 years	(59216.19)	(58975.74)	(54087.65)	(48276.16)	(45636.69)	(61218.62)	(55804.22)
N	9935	5047	914	2209	394	2838	520

Notes: Mean and standard deviation of the outcome variables. Different groups by column: (1) all the sample aged between 22 and 29 at unemployment entry, (2) all the sample aged between 22 and 25 at unemployment entry, (3) Win-Win takers within one year and aged between 22 and 25 at unemployment entry, (4) dropouts aged between 22 and 25 at unemployment entry, (5) dropout Win-Win takers within one year and aged between 22 and 25 at unemployment entry, (6) graduates aged between 22 and 25 at unemployment entry, (7) graduates Win-Win takers within one year and aged between 22 and 25 at unemployment entry.

C.3 Control Variables

In Table C.2, we show differences in observable characteristics regarding the following dimensions: gender, nationality (Belgian, European, Other), household composition (single, child of a couple, child of a single parent, other), the calendar month of unemployment registration, receiving unemployment benefits at registration as a jobseeker, region of residence, distance to the border with Luxembourg in minutes by car during rush hours, employment history in the last 4 years (having any employment experience or benefitting from any activation policy), information on the last job (full-time equivalents and cross-border job), and the combined full-time equivalent work of all members of the household in the calendar year before the unemployment spell.

Table C.2: Descriptive Statistics: Control Variables

	All			Dropouts		Graduates	
	22-29 All (1)	22-25 All (2)	22-25 Win-Win (3)	22-25 All (4)	22-25 Win-Win (5)	22-25 All (6)	22-25 Win-Win (7)
Age at unemployment registration	25.09 (2.01)	23.38 (0.86)	23.37 (0.88)	23.40 (0.86)	23.38 (0.88)	23.36 (0.87)	23.37 (0.88)
Woman	0.47 (0.50)	0.48 (0.50)	0.42 (0.49)	0.44 (0.50)	0.33 (0.47)	0.51 (0.50)	0.50 (0.50)
Graduate	0.51 (0.50)	0.56 (0.50)	0.57 (0.50)	0.00 (0.00)	0.00 (0.00)	1.00 (0.00)	1.00 (0.00)
Belgian nationality	0.85 (0.36)	0.89 (0.31)	0.92 (0.28)	0.81 (0.39)	0.88 (0.33)	0.95 (0.21)	0.95 (0.22)
EU27 nationality	0.04 (0.20)	0.04 (0.18)	0.03 (0.16)	0.05 (0.22)	0.03 (0.16)	0.02 (0.15)	0.03 (0.17)
Other nationality	0.11 (0.32)	0.08 (0.26)	0.06 (0.23)	0.14 (0.35)	0.10 (0.30)	0.02 (0.15)	0.02 (0.15)
One-person household	0.27 (0.45)	0.24 (0.42)	0.30 (0.46)	0.31 (0.46)	0.40 (0.49)	0.18 (0.38)	0.23 (0.42)
Child of a dual-parent household	0.19 (0.39)	0.27 (0.44)	0.29 (0.45)	0.16 (0.37)	0.23 (0.42)	0.36 (0.48)	0.33 (0.47)
Child of a single-parent household	0.10 (0.30)	0.14 (0.34)	0.12 (0.33)	0.11 (0.31)	0.09 (0.29)	0.16 (0.37)	0.14 (0.35)
Other household	0.43 (0.50)	0.36 (0.48)	0.29 (0.45)	0.43 (0.49)	0.28 (0.45)	0.30 (0.46)	0.30 (0.46)
Receiving unemployment benefits	0.54 (0.50)	0.51 (0.50)	0.62 (0.48)	0.47 (0.50)	0.59 (0.49)	0.55 (0.50)	0.65 (0.48)
Any experience between 1 and 4 years before unemployment entry	0.71 (0.46)	0.66 (0.47)	0.75 (0.43)	0.64 (0.48)	0.76 (0.43)	0.67 (0.47)	0.75 (0.43)
Any activation policy between 1 and 4 years before unemployment entry	0.11 (0.31)	0.08 (0.27)	0.12 (0.32)	0.11 (0.32)	0.16 (0.37)	0.05 (0.22)	0.09 (0.28)
Last job as cross-border worker (1 and 4 years before)	0.03 (0.17)	0.03 (0.17)	0.02 (0.13)	0.02 (0.13)	0.02 (0.13)	0.04 (0.19)	0.02 (0.13)
Last job full-time equivalents (1 and 4 years before)	60.35 (43.16)	55.99 (44.07)	62.58 (42.18)	55.66 (44.93)	67.61 (42.16)	56.24 (43.37)	58.79 (41.85)
Household full-time equivalents one year before unemployment	33.94 (31.94)	35.36 (32.31)	37.79 (31.33)	23.46 (27.47)	27.93 (27.89)	44.82 (32.73)	45.22 (31.75)
Wallonia	0.94 (0.24)	0.94 (0.23)	0.95 (0.22)	0.91 (0.29)	0.91 (0.29)	0.97 (0.17)	0.99 (0.12)
Flanders	0.01 (0.12)	0.01 (0.11)	0.00 (0.07)	0.02 (0.14)	0.01 (0.08)	0.01 (0.07)	0.00 (0.06)
Brussels	0.05 (0.21)	0.05 (0.21)	0.04 (0.21)	0.07 (0.26)	0.09 (0.28)	0.03 (0.16)	0.01 (0.11)
Minutes to Luxembourgish border by car during rush hours	57.85 (24.04)	56.96 (23.02)	57.15 (21.26)	59.87 (24.15)	59.64 (22.80)	54.64 (21.80)	55.26 (19.84)
N	9935	5047	914	2209	394	2838	520

Notes: Mean and standard deviation of the explanatory variables. Different groups by column: (1) all the sample aged between 22 and 29 at unemployment entry, (2) all the sample aged between 22 and 25 at unemployment entry, (3) Win-Win takers within one year and aged between 22 and 25 at unemployment entry, (4) dropouts aged between 22 and 25 at unemployment entry, (5) dropout Win-Win takers within one year and aged between 22 and 25 at unemployment entry, (6) graduates aged between 22 and 25 at unemployment entry, (7) graduates Win-Win takers within one year and aged between 22 and 25 at unemployment entry.

C.4 Subsidized Wage Costs and Attention Rate

Wage costs are measured as the gross before taxes plus the employer SSC *net* of the aforementioned pre-existing reductions (Section C.1). To accommodate for differences in working time, we report wages, SSC and subsidies in full-time equivalent per month. We predict the average wage cost at age 26 by using a linear spline on the wage costs of the unemployed youths in our sample who were aged between 26 and 29 and hired in the private sector within one year of entry into unemployment. The corresponding point estimate of wage costs is €2,283 for dropouts and €2,458 for graduates, and, hence, the share of the Win-Win subsidy for youths is, respectively, $1,100/2,283 = 0.48$ for dropouts, and $1,000/2,458 = 0.41$ for graduates.

We then calculate the shares of subsidized wage costs as follows. The full-time equivalent amount of the subsidy paid-out to a 26 year old eligible worker is an average of the Activa (€500) and the Win-Win subsidy for long-term unemployed (€750) among 26 year old weighted by the relative subsidy take-up. This average is estimated similarly as the gross wage costs at age 26, i.e. based on the prediction of a linear spline estimated on the subsample of youths between 26 and 29 years old, considering whom took-up one of these two subsidies within one year of entry in unemployment. The corresponding average is €548 for dropouts and €588 for graduates. Dividing these amounts by the corresponding wage costs yields $548/2,283 = 0.24$ for dropouts and $588/2,458 = 0.24$ for graduates.

The attention rate is obtained by the ratio of the average subsidy amount conditional on hiring to the amount conditional on take-up: to the left (right) it is $380/1,056 = 0.36$ ($76/543 = 0.14$), where €1,056 (€543) is the estimated average of €1,100 (€548) for dropouts and €1,000 (€588) for graduates. The fact that our sample contains ineligible individuals for the Win-Win subsidy (notably because they do not all comply to the unemployment duration requirement at the moment of hiring) explains why this attention rate is lower than the 47% reported by Cahuc et al. (2019) for the hiring subsidy awarded to small firms in the Great recession in France.

C.5 Biased-adjusted Elasticity

The numerator of the elasticity is the percentage points effect on the hiring rate (i.e. the discontinuity displayed in panel (d) of Figure 1) divided by the counterfactual hiring rate in the absence of the more generous hiring subsidy for those aged less than 26. We measure the latter by the level of hiring rate at the right-hand side of the age cutoff at 26 in panel (d) of Figure 1. The denominator of the elasticity is the subsidy amount (i.e., the discontinuity of €304/month displayed in panel (a) of Figure 1) divided by the counterfactual wage costs in the absence of the more generous hiring subsidy for those aged less than 26.

To estimate the latter, we must consider the level of the wage costs to the right of the age cutoff in a RDD on the wage costs at hiring. We estimate a monthly wage cost on the right of the cutoff of €2,392, which is €277 higher than the wage cost estimated on the left of the cutoff (€2,115 - see panel (c) of Figure 1). However, these estimated wage costs suffer from the so-called "double selection problem" (Heckman, 1974). The wage costs to the right of the age cutoff are not representative for the counterfactual wage costs in the absence of the more generous hiring subsidy for those hired to the left of the cutoff. Indeed, thanks to the more generous subsidy to the left of the cutoff more workers are hired and, hence, the hired workers to the left and to the right of the age cutoff are likely not comparable even if their age only differs marginally. This biases the estimate of the elasticity.

Even if theoretically the sign of the bias in the estimate of this elasticity cannot be determined, we argue that the downward bias dominates, but is negligible. The composition of hired workers to the left of the cutoff differs from that of the right essentially for two reasons. First, the Win-Win subsidy on the left induces the hiring of workers who are not eligible to a subsidy on the right of the cutoff. These are essentially short-term unemployed because on the right only long-term unemployed are eligible to the subsidy. These short-term unemployed are generally more productive and, hence, paid higher wages. This means that the wage costs on the right of the age cutoff are a downward biased for the counterfactual wage costs of youths hired on the left of the cutoff. Second, among the long-term unemployed who are hired thanks to the subsidy both on the left and the right of the cutoff, we expect the workers on the left to be less productive than on the right because the more generous subsidy on the left allows to compensate for this lower productivity. This implies that the wage costs to the right of the age cutoff overestimate the counterfactual wage costs of the corresponding subgroup of workers hired on the left.

We argue that the first mentioned downward bias on the counterfactual wage costs dominates the positive bias. The reason is that a downward (upward) bias on the right will reduce (increase) the magnitude of the discontinuity in the wage costs displayed in panel (c) of Figure 1. Because discontinuity in panel (c) of Figure 1 (€277/month) is lower than the unbiased one (€304/month) displayed in panel (a) of Figure 1, the bias is downward.[5] If we adjust the estimated wage costs to the right of the cutoff (€2,392/month) by the bias (+27=304-277), we obtain an adjusted counterfactual wage estimate of €2,419 per month. The larger counterfactual wage costs result in a slightly smaller proportional reduction (12.6% instead of 12.7%, i.e., 304/2419). Consequently, the unbiased elasticity is revised to 2.02 from 2.00. The negative bias is therefore negligible.

[5] The subsidy amount is by construction not affected by the selection bias mentioned above as the subsidy amounts are exogenously fixed.

D RDD Estimator

Formally, the one-sided donut RDD consists in estimating the following linear regression:

$$y_i^t = \alpha^t + \delta^t \cdot \mathbb{1}(z_i^0 < 26) + \beta^t \cdot (z_i^0 - 26) \cdot \mathbb{1}(z_i^0 < 26) + \gamma^t \cdot (z_i^0 - 26) \cdot \mathbb{1}(z_i^0 \geq 26) + \mu^t \cdot X_i^0 + \varepsilon_i^t \tag{1}$$

for $z_i^0 < 25 \mid z_i^0 \geq 26$, where:

- y_i^t is the outcome for individual i at elapsed duration t since entry into unemployment;
- $\mathbb{1}(\cdot)$ is the indicator function equal to 1 if the argument is true;
- α^t is the constant for the outcomes measured at time t;
- z_i^0 is the forcing variable for individual i, i.e., the age at the month of registration;
- $\beta^t(z_i^0 - 26) \cdot \mathbb{1}(z_i^0 < 26)$ is the linear relationship between the forcing variable and the outcome to the left of the cutoff;
- $\gamma^t(z_i^0 - 26) \cdot \mathbb{1}(z_i^0 \geq 26)$ is the linear relationship between the forcing variable and the outcome to the right of the cutoff;
- $\mathbb{1}(z_i^0 < 26)$ is a dummy indicator equal to 1 if the individual satisfies the age-eligibility condition, i.e., age below 26 at the month of registration. The associated parameter δ^t is the intention-to-treat effect at the cutoff at time t;
- X_i^0 are the control variables mentioned in Section 3, included to increase the precision of the estimates but removed in a sensitivity analysis;
- ε_i^t is the idiosyncratic error term (with zero conditional mean);
- Observations are reweighted by sampling weights and the triangular kernel weights.

E Difference-in-Differences

We estimate the conditional differences-in-differences estimator by exploiting different parts of the data-generating process. First, we implement the outcome regression approach of Heckman et al. (1997), which predicts the evolution of the counterfactual outcome in the absence of treatment $Y(0)$ given the explanatory variables (X). Second, the conditional difference-in-differences estimator can be implemented by the semi-parametric inverse-probability weighting (IPW) of Abadie (2005). This estimator controls for differential parallel trends by estimating the propensity score of treatment given the Xs and reweighting the observation by the inverse of this propensity score. Our DiD estimator follows Sant'Anna and Zhao (2020), who integrate these two models to obtain a doubly robust estimator, which just requires one of the two model specifications to hold. The treatment effects for the treated group D at time t are estimated by the following model:

$$\widehat{ATT}^t = E\left[\left(\frac{D_i}{E\left[D_i\right]} - \frac{\frac{\widehat{p}_i(X_i^0)(1-D_i)}{1-\widehat{p}_i(X_i^0)}}{E\left[\frac{\widehat{p}_i(X_i^0)(1-D_i)}{1-\widehat{p}_i(X_i^0)}\right]}\right)\left(Y_{i,2010}^t - Y_{i,2008}^t - \widehat{m}(X_i^0)^t\right)\right], \quad (2)$$

where D is equal to 1 for the treated group (age 24-25 at unemployment entry) and 0 otherwise (age 25-26 at unemployment entry). The estimated propensity score of belonging to the treated group given the covariates X is $\widehat{p}_i(X_i^0)$. $Y_{i,2010}^t$ ($Y_{i,2008}^t$) is the observed outcome at time t for individual i entering into treatment during the treatment (pre-treatment) period 2010 (2008). The outcome regression approach of Heckman et al. (1997) is integrated into the model by estimating the common time effect and identifying it on the control group, given X, and then extrapolating to the treated group with the same X. This time effect is integrated into the estimator by subtracting $m(X)^t = E[Y_{2010}^t - Y_{2008}^t | X, 1-D=1]$ from the observed outcome evolution of the individuals. The IPW-DiD estimator of Abadie (2005) is integrated into the model by reweighting the outcome of the control group by the inverse of the propensity score, which is normalized to improve their finite sample performance as shown in Busso et al. (2014). Under correct propensity score estimation, those in the reweighted control group have the same X characteristics as the treated group, and the parallel trend is required to hold conditionally.

Confidence intervals are obtained by using influence functions as explained in Sant'Anna and Zhao (2020), which we cluster by age.[6]

E.1 Validation tests

Given that the jobseekers registering in 2008 faced different business cycle condition compared to the ones of 2010, the identifying assumptions of the DiD estimator may be questioned. In particular, age-specific responses to changes in economic conditions may invalidate the results. To test for the presence of age-specific trends we conduct a series of validation tests. First, we run the DiD estimator by comparing two pre-treatment periods: the jobseekers registering in 2008 with those of 2007. Second, we repeat the same exercise but using as placebo period the unemployment entries in 2012, when the subsidy had already been abolished. As the state of the business cycle was also different during these years, compared to 2008, age-specific responses should lead to significant placebo effects. Finally, we estimate a placebo effect by running the same DiD estimator comparing the entries in 2008 to the ones in 2010, but considering the unaffected population of post-secondary graduates, who may also show age-specific trends.

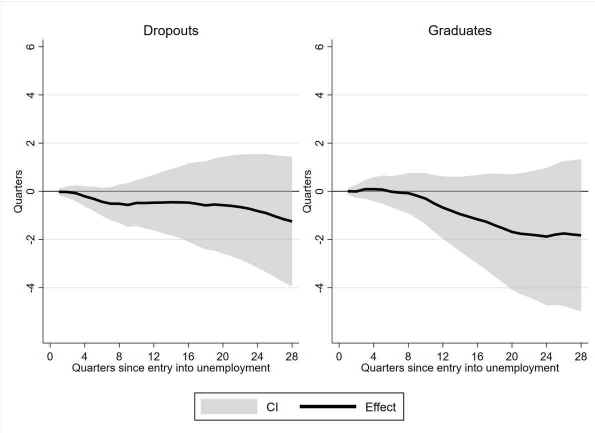
No estimates are statistically significant, affirming the reliability of the parallel trend assumption (see Figures E.1 and E.2). The fact that we do not observe significant age-trends is not surprising since we compare age groups that are very close to each other (i.e. 24-25 vs. 26-27 years old), also resulting in a small difference in levels during the pre- and post-treatment periods (see Figure 7), which is important when assessing the credibility of the parallel trend assumption (e.g. Meyer, 1995).[7]

[6] As shown in Sant'Anna and Zhao (2020) the doubly robust DiD is "improved" to make it doubly robust also for inference.

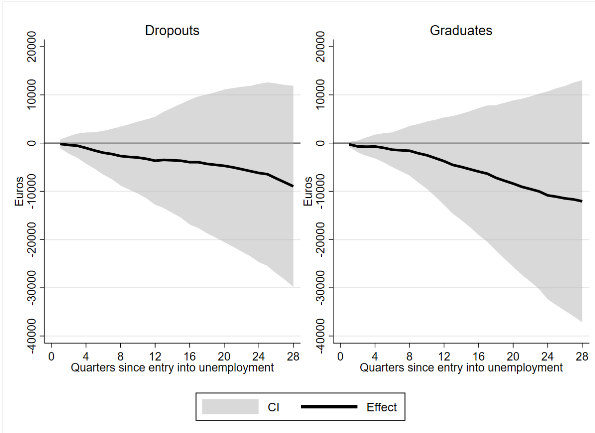
[7] In a further sensitivity check, we progressively widen the size of the control group up to age 30. This yields very similar results available upon request.

Figure E.1: Evolution of the DiD Pre-trends on Cumulative Outcomes

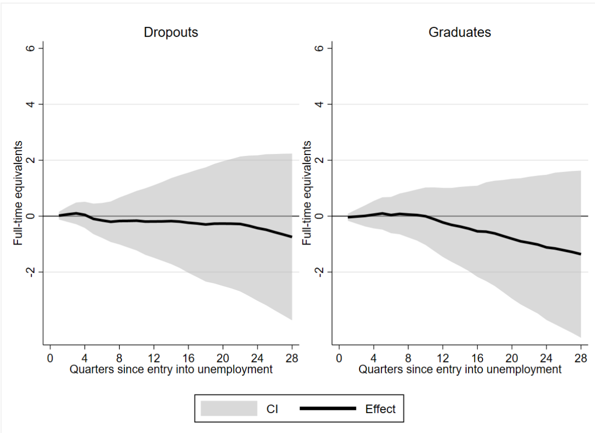
(a) Private sector employment



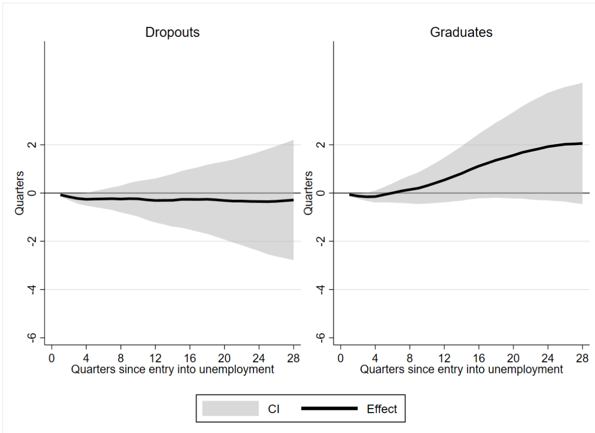
(b) Gross wage (private sector)



(c) Full-time equivalents (private sector)



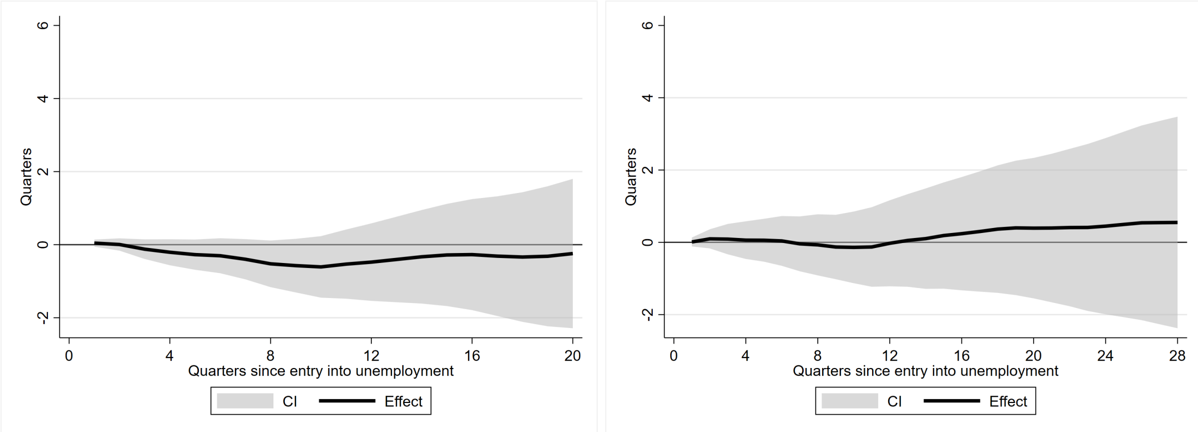
(d) Other employment



Note: Evolution of the placebo effect estimated with a doubly robust DiD estimator (Sant'Anna and Zhao, 2020) and confidence intervals (CI) for (a) the cumulative number of quarters in private sector employment, (b) gross remuneration in private sector employment, (c) full-time equivalents in private sector employment (1 for a full-time job in the quarter), (d) quarters in self- and public employment, since unemployment entry and by schooling level: dropouts (left) vs. graduates (right). The DiD estimator is implemented for each year after entry into unemployment until 7 years later. The treated are aged 24-25 at unemployment entry, while controls are aged 26-27. Units registering in (2007) 2008 are considered in the (pre-)treatment period. Data are reweighted by the sampling weights. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. N = 1,714 (dropouts) and 1,599 (graduates).

Figure E.2: Placebo Tests on Cumulative Number of Quarters in Private Sector Employment

(a) High-school graduates entering in 2008 vs 2012 (b) Post-secondary graduates entering in 2008 vs 2010



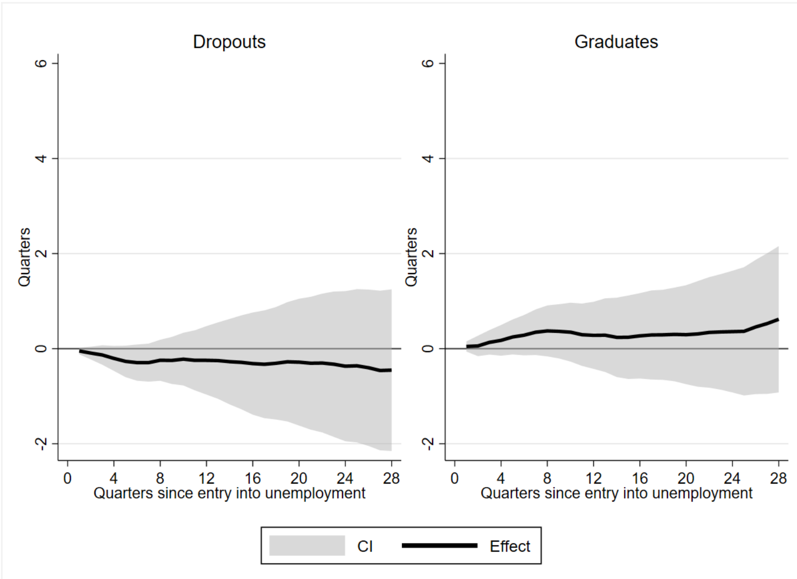
Note: Evolution of the placebo effect estimated with a doubly robust DiD estimator (Sant'Anna and Zhao, 2020) and confidence intervals (CI) for the cumulative number of quarters in private sector employment since unemployment entry. Panel (a) shows the estimates considering the high school graduates registering in 2012 as the treatment period (instead of 2010). Panel (b) shows the effects in 2010 for the post-secondary graduates. The DiD estimator is implemented for each year after entry into unemployment until 7 years later. The treated are aged 24-25 at unemployment entry, while controls are aged 26-27. Data are reweighted by the sampling weights. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level.

E.2 Spillovers on Older Workers?

An interesting question is whether spillover effects on older workers reduces the long-run effectiveness of hiring subsidies.

To detect whether the effects of Win-Win on private sector employment of high school graduates come at the expense of older workers, we implement two doubly robust DiD estimators. In the analyses, we estimate the effect on the cumulative employment outcomes of individuals who are at the margin of not being eligible for the Win-Win subsidy because they are slightly older than the cutoff age. The first DiD analysis compares the evolution of the cumulative number of quarters in private sector employment for youths aged [26, 27) to the older cohort aged [30, 35) (Figure E.3). The second one compares the same outcome for youths aged between 26 and 27 living far (more than one hour) from the border to youths of the same age living close to the border (Figure E.4). The rationale for the latter contrast stems from the argument that a spillover on the ineligible group can only be present if there is an effect for the eligible age group. Based on the findings in the previous section, there is only a treatment effect on the eligible jobseekers living far from the border; therefore, the ineligible group living close to the border can serve as a control group for the ineligible group living far from the border. Both figures show that the spillover effects on the ineligible group aged 26-27 are small throughout the 7 years since unemployment entry and never statistically different from zero.[8]

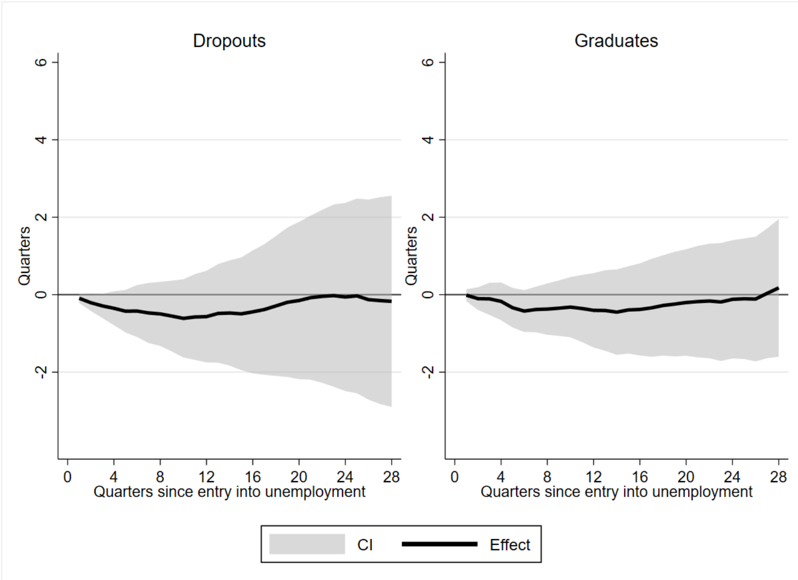
Figure E.3: Evolution of the DiD Effect on the Cumulative Number of Quarters in Private Sector Employment: Cohort Aged 26-27 Compared to the Cohort Aged 30-35



Note: Evolution of the spillover effect estimated with a doubly robust DiD estimator (Sant'Anna and Zhao, 2020) and confidence intervals (CI) for the cumulative number of quarters in private sector employment since entry into unemployment in 2010 by schooling level: dropouts (left) vs. graduates (right). The DiD estimator is implemented for each quarter after entry into unemployment until 7 years later. The treated are aged 26-27 at unemployment entry, while controls are aged 30-35. Units registering in (2008) 2010 are considered in the (pre-)treatment period. Data are reweighted by the sampling weights. We control for the set of control variables shown in Table C.2. Standard errors are clustered at the age level. For dropouts (graduates), the effect at 7 years is –0.4 quarters [–2.1; 1.2] with a p-value of 0.603 and N = 6,710 (+0.6 quarters [–0.9; 2.2], p-value 0.430 and N = 4,202).

[8] These findings (available upon request) are robust to using different age ranges.

Figure E.4: Evolution of the DiD Effect on the Cumulative Number of Quarters in Private Sector Employment: Cohort Aged 26-27 Living Far from the Border Compared to the Same-Age Cohort Living Close to the Border



Note: Evolution of the spillover effect estimated with a doubly robust DiD estimator (Sant'Anna and Zhao, 2020) and confidence intervals (CI) for the cumulative number of quarters in private sector employment since entry into unemployment in 2010 by schooling level: dropouts (left) vs. graduates (right). The DiD estimator is implemented for each year after entry into unemployment until 7 years later. We retain only units aged 26-27 at unemployment entry. The treated (controls) live more (less) than 60 minutes by car from the border with Luxembourg. Units registering in (2008) 2010 are considered in the (pre-)treatment period. Data are reweighted by the sampling weights. We control for the set of control variables shown in Table C.2. Standard errors are clustered at the age level. For dropouts (graduates), the effect at 7 years is –0.2 quarters [–2.9; 2.5] with a p-value of 0.902 and N = 1,315 (+0.2 quarters [–1.6; 2.0], p-value 0.845 and N = 1,111).

F Local Average Treatment Effect

There has been some debate in the literature on whether the effect of actual participation in a program, i.e., the local average treatment effect (LATE) in a fuzzy Regression Discontinuity Design (RDD) setting, or the eligibility, i.e., the intention-to-treat (ITT), is the parameter of interest in the context of hiring and wage subsidies (see e.g. Boockmann et al., 2012; Schünemann et al., 2015). The main objective of a hiring subsidy is to enhance the transition from unemployment to employment among an eligible population at high risk of unemployment. The ITT effect provides a quantitative measure of the extent to which this objective is attained. The LATE, by contrast, measures the effect on the outcome of interest of the hiring subsidy for actual recipients of the subsidy. Subsidy receipt is fundamentally different from program participation in common evaluation studies in that hiring is a prerequisite for subsidy take-up. This means that the LATE measures the treatment effect of a sub-population for which the subsidy was already partly successful. The LATE can therefore be a misleading effectiveness measure because it can take-on a high value, i.e., be effective even if the subsidy only marginally increases the hiring rate of the eligible population, i.e., even if there are only few treated individuals. Furthermore, in contrast to treatments in most other contexts, this treatment is only under the partial control of the worker and the policy maker because firms cannot be forced to hire jobseekers. In our opinion, within the context of hiring subsidies, the LATE, is a less relevant policy parameter than the ITT effect.

We calculate the LATE, as in the standard RDD literature with a fuzzy design. In the standard RDD framework, a Wald estimator is used to estimate the LATE: the intention-to-treat (ITT) effect on the transition rate to employment divided by the effect on subsidy receipt. Under the assumption of monotonicity (where individuals' treatment status does not change inversely with their assignment), there are no defiers (individuals acting contrary to their assignment). In our context it means that all workers younger than 26, who would have been hired with a less generous subsidy had they been older, are hired with the generous Win-Win subsidy (always takers). If there are no defiers, the Wald estimator can be interpreted as the treatment effect for compliers, who, in this scenario, are jobseekers hired in a subsidized position solely due to their age making them eligible for the generous Win-Win subsidy.

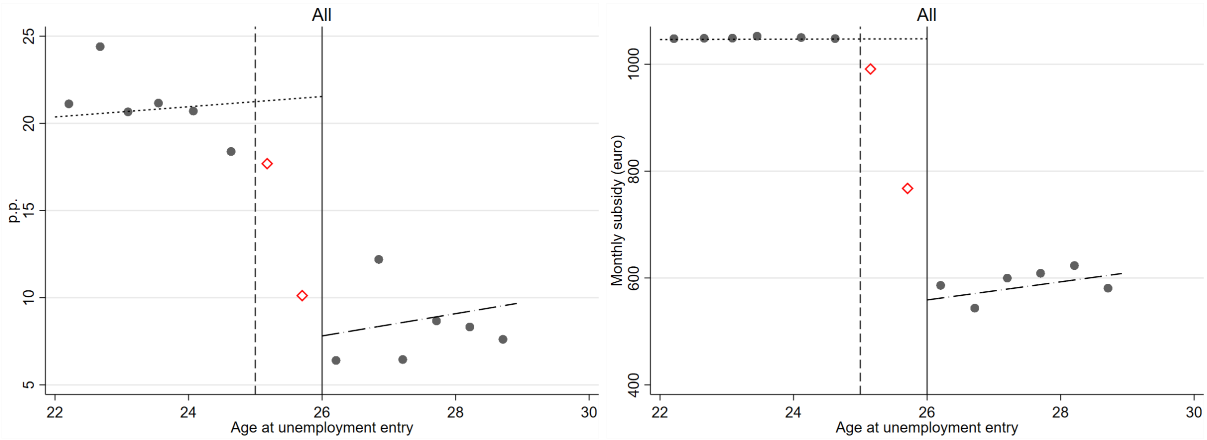
Panel (a) of Figure F.1 illustrates the RDD on the take-up rate of a subsidy. The take-up rate to the left of the age discontinuity at 26 is 21.5%, while it is only 7.8% to the right of this discontinuity. Therefore, the jump at the threshold on the take-up rate is 13.7 percentage points. Because the ITT on the transition rate to employment is 10.5 percentage points (see Panel (d) of Figure 1), the Wald estimate of the LATE is 77 percentage points (= 10.5/13.7). This means that 77 percent of the individuals, who started a subsidized job solely because they were younger than 26 (i.e. the compliers), would not have obtained employment without taking up the subsidy. Symmetrically, this indicates that 23% of the compliers would have found a job even without the subsidy, representing the deadweight loss for the compliers.

We can also calculate the total deadweight loss, representing the proportion of the additional subsidy per jobseeker that is wasted to subsidize employment for individuals who would have found employment in any case. To determine the deadweight loss, we first need to ascertain the average subsidy amount for individuals hired with the subsidy on both sides of the age threshold of 26. These values are obtained through an RDD analysis of the subsidy amounts for youths hired with the subsidy, as illustrated in Panel (b) of Figure F.1. To the left of the threshold (for individuals younger than 26), this amount is €1,056/month, calculated as the average of €1,000/month for graduates and €1,100/month for dropouts. To the right of the threshold (for individuals older than 26), the average subsidy amount is €543/month, reflecting the average amount firms are eligible for when hiring older long-term unemployed individuals.

We multiply these amounts by the relative shares of jobseekers who take up the subsidy to determine the average subsidy amount paid per *jobseeker* on each side of the age cutoff: €227/month (0.215*€1,056) for those younger than 26 and €42/month (0.078*€543) for those 26 and older. The difference, €185/month, represents the additional subsidy amount per *jobseeker* attributable to Win-Win age eligibility, serving as the denominator of the deadweight loss ratio. To identify the numerator—the portion of this subsidy amount spent on individuals who would have been employed even without the subsidy—we deduct from €185/month the average subsidy paid to compliers exclusively hired because of the subsidy. Given that the proportion of compliers in the total population is 0.137, and 77% of these have found a job solely due to the subsidy (the LATE), €111/month per jobseeker (€1,056*0.137*0.77) is allocated for compliers hired due to the subsidy. Thus, the numerator of the deadweight loss—representing the subsidy paid for compliers who would have found employment even without the subsidy—is €74/month (€185 - €111). Consequently, the total deadweight loss ratio is 40% (€74/€185).

Figure F.1: Discontinuities at Age 26 on the Hiring Subsidy Take-Up Within One Year

(a) Take-up Rate | (b) Monthly Amount of Subsidy Among Takers



Note: Donut RDD estimate on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. The outcome is (a) the cumulative take-up rate of hiring subsidies within one year and (b) full-time monthly amount of subsidy received conditional on receipt of a subsidy, which is plotted over six age-quantile-spaced bins on each side of the donut. The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but by removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. The effect estimated by the donut RDD estimator at 26 years old is a) +13.7 pp [5.2; 22.2] with a p-value of 0.002 and N = 8,560 and b) €513 [475; 553] with p-value of 0.000 and N = 1,272.

G Cost-Benefit Analysis

In this Appendix, we implement a cost-benefit analysis of the subsidy reinforcement following the marginal value of public funds (MVPF) framework proposed by Hendren and Sprung-Keyser (2020) and designed for measuring the long-run effectiveness of policies.[9] The MVPF is the ratio of the beneficiaries' marginal willingness to pay (WTP) for the reinforcement of the hiring subsidy to the net marginal cost (NC) to the government of this policy inclusive of any behavioral impact on the government budget. The MVPF can range from negative to positive values. It is convention to set it to plus infinity ($+\infty$) whenever the net cost to the government is negative and the WTP positive because in this case the policy has value (WTP>0) and finances itself (NC<0).

We estimate the NC of subsidy reinforcement by implementing a similar donut RDD estimator as the one presented above, using as outcome the (cumulative) individual discounted contribution to the net public expenditures seven years after unemployment entry. The outcome therefore includes both the mechanical cost of the subsidy reinforcement and the behavioral response to the policy on the government's expenditures, i.e. the fiscal externality. This cost can be divided into three components: (i) contributions to the tax revenues (including payroll taxes) from labor income and unemployment benefits, (ii) expenditures on hiring subsidies, and (iii) expenditures on unemployment benefits.

The WTP for the subsidy is the sum of three components: (i) the WTP for the hiring firms, (ii) the WTP for the unemployed young workers who are hired due to the subsidy reinforcement, and (iii) the marginal WTP for the incumbent workers in these firms.[10] Since we estimated the incidence on the wage to be close to zero, this last component is set to zero.[11]

The WTP for hiring firms consists in the sum of this value for the infra-marginal and the marginal firms. The infra-marginal firms are those that also would receive a lower hiring sub-

[9] The Policy Impacts Library contains estimates of the MVPF for a wide range of policy measures.

[10] We follow Paradisi (2021), who formally derives the WTP for the payroll tax cut for young workers implemented in Sweden and analyzed by Saez et al. (2019).

[11] In principle, part of the producer's surplus included in (i) could be shared with incumbents as documented in Saez et al. (2019). We do not have the data to identify such sharing, but as this value is contained in (i), this would only marginally affect the MVPF through a differential tax treatment of this transfer (see also the next footnote).

sidy in the counterfactual of no Win-Win plan, i.e. the always takers. The cumulative take-up rate of the hiring subsidy at the age cutoff of 26 measures the number of infra-marginal firms relative to all retained youths of that age in the sample. Since there is no passthrough to wages, the WTP for these infra-marginal firms is the cumulative discounted sum of the statutory subsidy reinforcement received over the considered time span of seven years.[12]

The marginal firms are those that are induced to hire in response to the subsidy reinforcement, i.e. the compliers. The number of marginal firms can be measured by the jump in this take-up rate at the age cutoff at 26. Because these firms would not have taken up the subsidy for hiring workers older than 26, the marginal cost of taking up the hiring subsidy must be at least equal to the value of the subsidy for those aged 26 or more. This means that WTP for these marginal firms must be smaller than the subsidy reinforcement. In principle, we should estimate the marginal hiring cost to be able to estimate the WTP for these marginal firms. However, this is difficult. We therefore follow a commonly used shortcut and value the WTP for these firms to 50% of the subsidy reinforcement (Finkelstein and Hendren, 2020, p. 153).

Finally, the WTP for the unemployed young workers who are hired due to the subsidy reinforcement is estimated by a donut RDD on the cumulative discounted net after tax earnings seven years since entry into unemployment. Even if we know from the results section that there is no effect on the time spent in employment once we consider other than private sector employment, the earnings are higher, which may enhance the WTP.

We follow Hendren and Sprung-Keyser (2020) by choosing a real discount rate of 3% in the benchmark simulation and consider also 1% and 10% in a sensitivity analysis. In order to calculate a 95% confidence interval around the MVPF estimates, we implement a non-parametric bootstrap that incorporates the estimation of the different components.[13]

Table G.1 reports for six scenarios the local estimates and 95% confidence intervals of the MVPF of the reinforcement of the hiring subsidy for 26-years-olds: by education level (high school dropouts or graduates) and by the distance to the border (less or more than 60 minutes,

[12] Contrary to Paradisi (2021), we assume that the firm does not pay corporate taxes on this windfall benefit and that this neither reduces the net cost of the policy reform to the government.

[13] There arises a deep conceptual problem if both WTP<0 and NC<0. We follow Hendren and Sprung-Keyser (2020) by augmenting the confidence intervals to account for this: see their Online Appendix H.

all).[14] A first observation is that the confidence intervals of the MVPF are very wide. They always comprise zero and the upper bound is always equal to plus infinity, meaning that the policy pays for itself, i.e. the net cost to the government in the denominator of the MVPF is negative. These wide confidence intervals imply that it is hazardous to base any policy conclusions on this cost-benefit analysis. Nevertheless, there are some main takeaways.

Table G.1: MVPF and its Components 7 years After Entry Into Unemployment

Border	Education	MVPF	CI_MVPF	NC	CI_NC	WTP	CI_WTP	N
All	Dropouts	4.07	[–0.66; +∞]	–900	[–12,036; 11,048]	3,660	[–5,519; 12,498]	4,176
All	Graduates	+∞	[–7.62; +∞]	3,087	[–6,474; 11,219]	5,396	[–6,098; 14,311]	4,384
Far	Dropouts	+∞	[–1.17; +∞]	395	[–14,174; 16,341]	4,220	[–8,503; 15,886]	2,636
Far	Graduates	+∞	[-∞; +∞]	6,378	[–5,434; 16,555]	7,439	[–5,645; 19,032]	2,432
Near	Dropouts	0.27	[–1.19; +∞]	–4,998	[–15,294; 5,489]	1,344	[–7,764; 10,053]	1,443
Near	Graduates	1.00	[–1.24; +∞]	–1,692	[–15,004; 9,960]	1,685	[–9,671; 11,188]	1,939

Notes: Donut RDD estimates on the marginal value of public funds (MVPF) (Hendren and Sprung-Keyser, 2020) seven years after unemployment entry and its components, i.e. the beneficiaries' marginal willingness to pay (WTP) and the net marginal cost (NC), on an inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Results are by education level and distance to the border. The MVPF is the ratio of the beneficiaries' WTP to the NC to the government. The WTP is the sum of the WTP for (i) the hiring firms, (ii) the unemployed young workers, and (iii) the incumbent workers in these firms. The NC is the cumulative individual discounted contribution to the net public expenditures. The outcomes are discounted with a 3% rate. We retain only individuals aged $[22-29)$ and then remove the units aged $[25,26)$ since they have less than 1 year of potential eligibility for the Win-Win subsidy (i.e., the donut). Data are reweighted by the triangular kernel that multiplies the sampling weights. We control for age with a linear spline for each side of the cutoff and the set of control variables shown in Table C.2 in Online Appendix C.3. We report the absolute effect, the bootstrapped confidence interval (9999 repetitions), and the number of units. Bootstrapped standard errors are clustered at the age level.

In line with the findings reported above, for high school graduates, the policy is more likely to be self-financing far from the border to Luxembourg than close to it. The point estimates of the MVPF are equal to $+\infty$ and 0.9, respectively. Without this regional confinement, the point estimate stays at $+\infty$, but this does not reveal that the surplus for the government is reduced from €6,378 to €3,087 on average per person over these seven years. As illustrated in Figure G.1, the government's overall surplus for graduates is primarily influenced by increased tax contributions (€6,670, surpassing the costs associated with reinforcing the hiring subsidy (€3,657). Notably, expenditures on unemployment benefits remain unchanged (€–69).[15]

For high school dropouts we find, in line with the results reported above, generally lower estimates for the MVPF than for graduates. The overall MVPF declines from $+\infty$ to 4. Compared to the high school graduates, the tax contributions and the hiring subsidy increase by a smaller

[14] The sensitivity analyses for discount rates equal to 1% and 10% are reported in Table G.2. The MVPF are generally modestly increase with the discount rate.

[15] In Figure G.2, we also demonstrate that the results remain robust when employing a difference-in-difference identification strategy.

amount (€2,311 and €963, respectively), while the expenditure for unemployment benefits increases (€2,253 - see Figure G.1). This is explained by the fact that entitlement depends on the private sector employment history, which is enhanced by the policy reform. Furthermore, only half of our sample claims benefits at entry into unemployment because of an insufficient employment eligibility threshold. Finally, far from the border, the MVPF increases to $+\infty$, but net gain for the government (€395) is 16 times smaller than that for graduates (€6,378).

Overall, the Win-Win plan, therefore, did not seem to impose much cost on the government. For high school graduates, especially those living far from the border, the reform may even have paid for itself. Nevertheless, this conclusion requires corroboration because the confidence intervals are very wide. The finding that the hiring subsidy could pay for itself is in line with Cahuc et al. (2019), who report that the short-run net cost per created job of the hiring credit during the Great Recession in France is equal to zero. However, we are not aware of any other research that estimates the WTP of such reinforcement of a hiring subsidy in the *long run*.

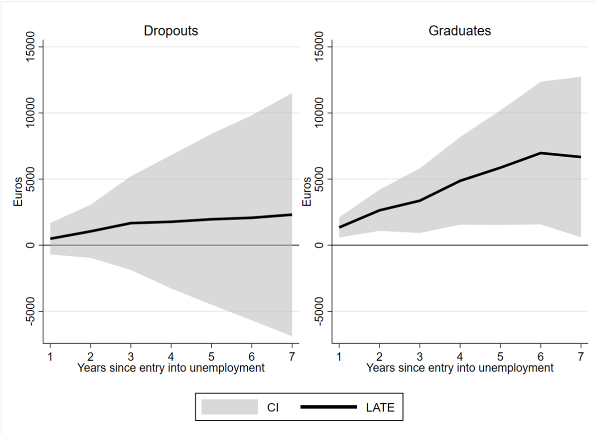
Table G.2: MVPF and its Components with Different Discount Rates

Border	Education	Discount_rate	MVPF	CI_MVPF	NC	CI_NC	WTP	CI_WTP	N
All	Dropouts	1	3.97	[–0.66; +∞]	–971	[–12,575; 11,888]	3,856	[–6,060; 13,331]	4,176
All	Dropouts	10	4.55	[–0.63; +∞]	–687	[–9,543; 9,210]	3,122	[–4,257; 10,640]	4,176
All	Graduates	1	+∞	[–21.82; +∞]	3,189	[–6,870; 12,070]	5,490	[–6,890; 14,871]	4,384
All	Graduates	10	+∞	[–20.38; +∞]	2,766	[–4,843; 9,299]	5,074	[–5,840; 12,810]	4,384
Far	Dropouts	1	+∞	[–1.22; +∞]	424	[–14,590; 16,634]	4,451	[–8,651; 16,686]	2,636
Far	Dropouts	10	+∞	[–1.20; +∞]	332	[–11,646; 12,823]	3,589	[–7,103; 13,215]	2,636
Far	Graduates	1	+∞	[-∞; +∞]	6,560	[–6,124; 17,710]	7,386	[–6,774; 19,339]	2,432
Far	Graduates	10	+∞	[–62.69; +∞]	5,778	[–4,145; 13,970]	7,451	[–4,092; 17,405]	2,432
Near	Dropouts	1	0.27	[–1.00; +∞]	–5,320	[–16,395; 6,094]	1,445	[–8,218; 10,799]	1,443
Near	Dropouts	10	0.26	[–1.13; +∞]	–4,075	[–12,146; 4,526]	1,068	[–6,348; 8,466]	1,443
Near	Graduates	1	1.05	[–1.50; +∞]	–1,680	[–16,003; 10,584]	1,757	[–10,576; 12,136]	1,939
Near	Graduates	10	0.89	[–1.28; +∞]	–1,683	[–12,397; 7,616]	1,499	[–7,890; 9,433]	1,939

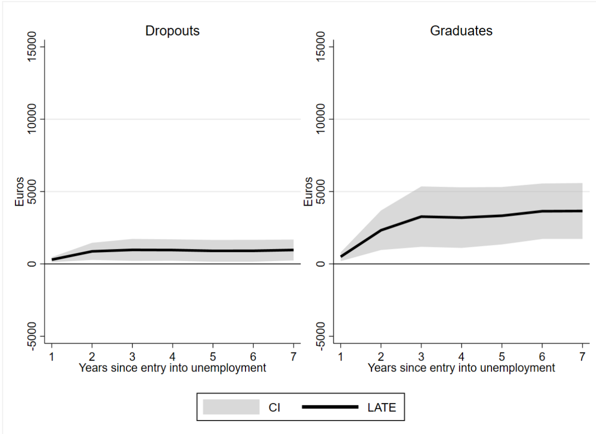
Notes: Donut RDD estimates on the marginal value of public funds (MVPF) (Hendren and Sprung-Keyser, 2020) seven years after unemployment entry and its components, i.e. the beneficiaries' marginal willingness to pay (WTP) and the net marginal cost (NC), on an inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Results are by education level, discount rate and distance to the border. The MVPF is the ratio of the beneficiaries' WTP to the NC to the government. The WTP is the sum of the WTP for (i) the hiring firms, (ii) the unemployed young workers, and (iii) the incumbent workers in these firms. The NC is the cumulative individual discounted contribution to the net public expenditures. The outcomes are discounted with a 1% or 10% rate. We retain only individuals aged $[22-29)$ and then remove the units aged $[25,26)$ since they have less than 1 year of potential eligibility for the Win-Win subsidy (i.e., the donut). Data are reweighted by the triangular kernel that multiplies the sampling weights. We control for age with a linear spline for each side of the cutoff and the set of control variables shown in Table C.2 in Online Appendix C.3. We report the absolute effect, the bootstrapped confidence interval (9999 repetitions), and the number of units. Bootstrapped standard errors are clustered at the age level..

Figure G.1: Evolution of the RDD Effect on Components of the Cost-Benefit Analysis (€)

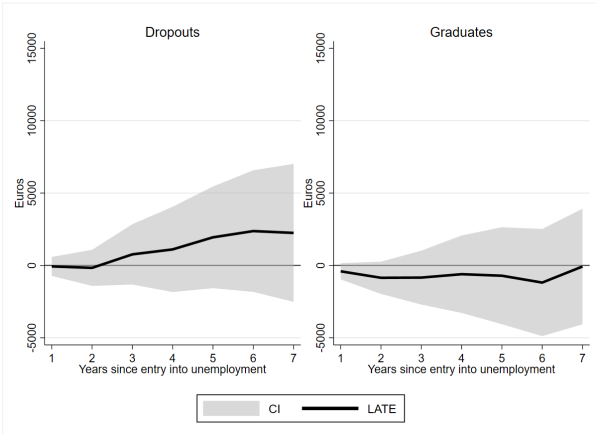
(a) Tax revenue



(b) Hiring subsidy expenditure



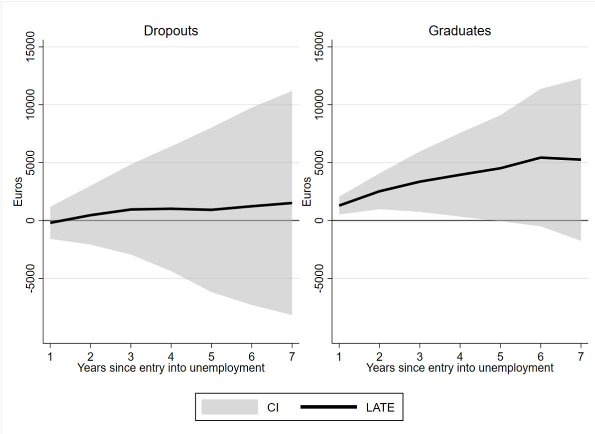
(c) Unemployment benefits expenditure



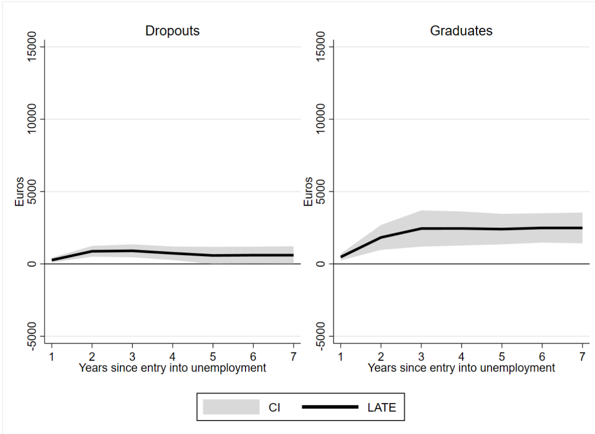
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence intervals (CI) for (a) tax revenue collected by the government, (b) expenditure for hiring subsidies, (c) expenditure for unemployment benefits, in euros (discounted at 3%) and by schooling level: dropouts (left) vs. graduates (right). The RDD estimator is implemented for each year after entry into unemployment until 7 years later. The donut RDD estimator removes observations for individuals aged between 25 and 26 at unemployment and retains units aged between 22 and 25 (left of the cutoff) and 26 to 29 (right of the cutoff). The two local linear splines are estimated on the reweighted observations by using the triangular kernel and the sampling weights but removing the units within the donut. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. For dropouts (graduates), the effect at 7 years on (a) tax revenue is €2,311 [–6,889; 11,511] with a p-value of 0.618 (€6,670 [595; 12,745], p-value 0.032); the effect at 7 years on (b) hiring subsidy expenditure is €963 [–242; 1,685] with a p-value of 0.001 (€3,657 [1,726; 5,589], p-value 0.000); the effect at 7 years on (c) unemployment benefits expenditure is €2,253 [–2,521; 7,028] with a p-value of 0.350 (–€69 [–4,062; 3,924], p-value 0.972). N = 4,176 (dropouts) and 4,384 (graduates).

Figure G.2: Evolution of the DiD Effect on Components of the Cost-Benefit Analysis (€)

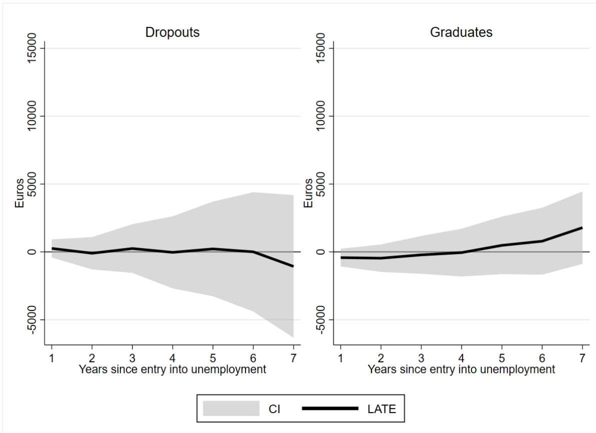
(a) Tax revenue



(b) Hiring subsidy expenditure



(c) Unemployment benefits expenditure



Note: Evolution of the effect estimated with a doubly robust DiD estimator (Sant'Anna and Zhao, 2020) and confidence intervals (CI) for (a) tax revenue collected by the government, (b) expenditure for hiring subsidies, (c) expenditure for unemployment benefits, in euros (discounted at 3%) and by schooling level: dropouts (left) vs. graduates (right). The DiD estimator is implemented for each year after entry into unemployment until 7 years later. The treated are aged 24-25 at unemployment entry, while controls are aged 26-27. Units registering in (2008) 2010 are considered in the (pre-)treatment period. Data are reweighted by the sampling weights. We control for the set of control variables shown in Table C.2 in Online Appendix C.3. Standard errors are clustered at the age level. For dropouts (graduates), the effect at 7 years on (a) tax revenue is €1,514 [–8,168; 11,197] with a p-value of 0.759 (€5,261 [–1,755; 12,277], p-value 0.141); the effect at 7 years on (b) hiring subsidy expenditure is €607 [–2.5; 1,216] with a p-value of 0.051 (€2,484 [1,419; 3,549], p-value 0.000); the effect at 7 years on (c) unemployment benefits expenditure is –€1,067 [–6,327; 4,192] with a p-value of 0.691 (€1,790 [–876; 4,457], p-value 0.188). N = 1,942 (dropouts) and 1,839 (graduates).

H Tables

H.1 Main Tables

Table H.1: Effect on Xs: RDD Estimates

	Discontinuity	CI	P_value	N_left	N_right
(A) Dropouts					
Woman	-0.06	[-0.21; 0.10]	0.455	2,209	1,967
Belgian nationality	-0.05	[-0.16; 0.06]	0.347	2,209	1,967
Other nationality	0.07	[-0.03; 0.18]	0.153	2,209	1,967
One-person household	0.04	[-0.07; 0.15]	0.496	2,209	1,967
Child of a dual-parent household	-0.02	[-0.08; 0.05]	0.581	2,209	1,967
Child of a single-parent household	-0.01	[-0.06; 0.05]	0.822	2,209	1,967
Receiving unemployment benefits	-0.03	[-0.16; 0.10]	0.694	2,209	1,967
Any experience between 1 and 4 years before unemployment entry	-0.08	[-0.22; 0.06]	0.246	2,209	1,967
Any activation policy between 1 and 4 years before unemployment entry	0.05	[-0.04; 0.15]	0.273	2,209	1,967
Last job as cross-border worker (1 and 4 years before)	0.00	[-0.02; 0.01]	0.935	2,209	1,967
Last job full-time equivalents (1 and 4 years before)	-6.03	[-20.78; 8.72]	0.418	2,209	1,967
Household full-time equivalents one year before unemployment	-6.16*	[-13.15; 0.83]	0.083	2,209	1,967
Wallonia	0.11*	[-0.02; 0.24]	0.099	2,209	1,967
Brussels	-0.06	[-0.19; 0.07]	0.355	2,209	1,967
Minutes by car during rush hours to border with Luxembourg	-8.51**	[-15.47; -1.55]	0.017	2,209	1,967
January	0.06	[-0.03; 0.15]	0.183	2,209	1,967
February	0.03	[-0.08; 0.13]	0.642	2,209	1,967
March	-0.01	[-0.08; 0.07]	0.883	2,209	1,967
April	-0.02	[-0.08; 0.04]	0.528	2,209	1,967
May	-0.03	[-0.08; 0.01]	0.114	2,209	1,967
June	0.01	[-0.05; 0.07]	0.852	2,209	1,967
July	-0.01	[-0.08; 0.06]	0.732	2,209	1,967
August	0.02	[-0.04; 0.09]	0.433	2,209	1,967
October	0.00	[-0.04; 0.04]	0.986	2,209	1,967
November	-0.02	[-0.08; 0.04]	0.518	2,209	1,967
December	-0.05	[-0.14; 0.05]	0.331	2,209	1,967
(B) Graduates					
Woman	0.07	[-0.05; 0.18]	0.278	2,838	1,546
Belgian nationality	0.02	[-0.08; 0.11]	0.716	2,838	1,546
Other nationality	-0.01	[-0.10; 0.09]	0.901	2,838	1,546
One-person household	-0.14**	[-0.26; -0.01]	0.030	2,838	1,546
Child of a dual parent household	-0.04	[-0.14; 0.05]	0.353	2,838	1,546
Child of a single parent household	-0.02	[-0.11; 0.08]	0.717	2,838	1,546
Receiving unemployment benefits	0.06	[-0.06; 0.19]	0.310	2,838	1,546
Any experience between 1 and 4 year before unemployment entry	-0.05	[-0.16; 0.05]	0.307	2,838	1,546
Any activation policy between 1 and 4 year before unemployment entry	-0.01	[-0.11; 0.08]	0.761	2,838	1,546
Last job as cross-border worker (1 and 4 year before)	0.06	[-0.02; 0.14]	0.140	2,838	1,546
Last job full-time equivalents (1 and 4 year before)	-2.17	[-13.63; 9.30]	0.708	2,838	1,546
Household full-time equivalents one year before unemployment	-2.78	[-9.43; 3.87]	0.407	2,838	1,546
Wallonia	-0.03	[-0.12; 0.06]	0.509	2,838	1,546
Brussels	0.03	[-0.05; 0.12]	0.461	2,838	1,546
Minutes by car during rush hours to Luxembourgish border	-0.04	[-6.66; 6.59]	0.991	2,838	1,546
January	-0.04	[-0.15; 0.06]	0.382	2,838	1,546
March	0.00	[-0.04; 0.03]	0.888	2,838	1,546
February	0.00	[-0.09; 0.09]	0.937	2,838	1,546
April	0.03	[-0.08; 0.14]	0.603	2,838	1,546
May	0.02	[-0.05; 0.09]	0.577	2,838	1,546
June	-0.03	[-0.07; 0.02]	0.316	2,838	1,546
July	-0.02	[-0.07; 0.03]	0.459	2,838	1,546
August	-0.01	[-0.07; 0.04]	0.660	2,838	1,546
October	-0.06	[-0.15; 0.03]	0.182	2,838	1,546
November	-0.01	[-0.08; 0.05]	0.646	2,838	1,546
December	0.02	[-0.05; 0.10]	0.556	2,838	1,546

Notes: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. We retain only individuals aged [22-29) and then remove the units aged [25, 26) since they have less than 1 year of potential eligibility for the Win-Win subsidy. Data are reweighted by the triangular kernel that multiplies the sampling weights. We control for age with a linear spline for each side of the cutoff. Panel (A) refers to high school dropouts, while panel (B) refers to high school graduates. The dependent variables consist of the set of control variables shown in Table C.2. We report the absolute effect, its confidence interval, and the p-value, as well as the number of units for each side of the cutoff. Standard errors are clustered at the age level. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table H.2: Summary Table: RDD Estimates on the Job-Finding Rate

	Take-up t=3 (1)	Take-up t=4 (2)	Take-up t=5 (3)	Take-up t=6 (4)	Job finding t=3 (5)	Job finding t=4 (6)	Job finding t=5 (7)	Job finding t=6 (8)
(A) Dropouts								
Effect at 26	11.54**	11.54*	9.66	10.16*	7.53	12.83**	11.23**	11.18**
CI	[2.24; 20.83]	[–0.62; 23.70]	[–2.09; 21.41]	[–1.56; 21.89]	[–1.73; 16.79]	[2.38; 23.27]	[0.50; 21.96]	[0.08; 22.27]
p-value	0.016	0.063	0.105	0.088	0.110	0.017	0.040	0.048
Effect in %	187.51	151.67	104.52	105.41	27.87	40.42	32.39	30.79
N (left)	2,389	2,209	2,209	2,209	2,389	2,209	2,209	2,209
N (right)	1,967	1,967	1,967	1,967	1,967	1,967	1,967	1,967
(B) Graduates								
Effect at 26	10.69***	17.48***	14.69***	16.02***	11.30**	7.68	11.26	13.54**
CI	[3.02; 18.36]	[7.09; 27.88]	[4.64; 24.75]	[5.89; 26.14]	[0.90; 21.70]	[–5.55; 20.91]	[–2.48; 25.00]	[0.54; 26.55]
p-value	0.007	0.001	0.005	0.002	0.034	0.251	0.107	0.042
Effect in %	151.08	231.39	135.52	134.61	25.22	14.62	19.37	22.46
N (left)	3,034	2,838	2,838	2,838	3,034	2,838	2,838	2,838
N (right)	1,546	1,546	1,546	1,546	1,546	1,546	1,546	1,546

Notes: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. We retain only individuals aged [22-29) and then remove the units aged [25, 26) since they have less than 1 year of potential eligibility for the Win-Win subsidy (i.e., the donut). Data are reweighted by the triangular kernel that multiplies the sampling weights. We control for age with a linear spline for each side of the cutoff and the set of control variables shown in Table C.2. Panel (A) refers to high school dropouts, while panel (B) refers to high school graduates. The dependent variables consist of the accumulated hazard rate for a subsidized job (columns 1 to 4) and any job in the private sector (columns 5 to 8) measured at 3 quarters (columns 1 and 5), 4 quarters (columns 2 and 6), 5 quarters (columns 3 and 7), and 6 quarters (columns 4 and 8) from unemployment entry. We report the absolute effect, its confidence interval, and the p-value, as well as the relative effect in % and the number of units for each side of the cutoff. Standard errors are clustered at the age level. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table H.3: Summary Table: RDD Estimates 7 Years After Unemployment Entry

	Employment (1)	Remuneration (2)	FTE (3)	Other Empl. (4)
(A) Dropouts				
Effect at 26	0.16	3,547.90	1.21	0.46
CI	[–2.35; 2.68]	[–11,224.06; 18,319.86]	[–1.18; 3.61]	[–0.57; 1.49]
p-value	0.897	0.633	0.316	0.375
Effect in %	2.38	11.61	15.63	27.18
N (left)	2,209	2,209	2,209	2,209
N (right)	1,967	1,967	1,967	1,967
(B) Graduates				
Effect at 26	2.83**	14,646.03**	2.86**	–2.65**
CI	[0.68; 4.99]	[2,736.28; 26,555.78]	[0.62; 5.11]	[–4.70; -0.60]
p-value	0.011	0.017	0.013	0.012
Effect in %	27.83	28.57	25.94	–52.39
N (left)	2,838	2,838	2,838	2,838
N (right)	1,546	1,546	1,546	1,546

Notes: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. We retain only individuals aged [22-29) and then remove the units aged [25, 26) since they have less than 1 year of potential eligibility for the Win-Win subsidy (i.e., the donut). Data are reweighted by the triangular kernel that multiplies the sampling weights. We control for age with a linear spline for each side of the cutoff and the set of control variables shown in Table C.2. Panel (A) refers to high school dropouts, while panel (B) refers to high school graduates. The dependent variables consist of accumulated outcomes measured up to 7 years after unemployment entry and include (1) quarters in private sector employment, (2) gross remuneration in the private sector, (3) full-time equivalents in private sector employment (1 for a full-time job in the quarter), and (4) quarters in self- and public employment. We report the absolute effect, its confidence interval, and the p-value, as well as the relative effect in % and the number of units for each side of the cutoff. Standard errors are clustered at the age level. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table H.4: Summary Table: RDD Estimates 7 Years After Unemployment Entry – By Proximity to the Border

	Employment (1)	Remuneration (2)	FTE (3)	Other Empl. (4)
(A) Dropouts - near				
Effect at 26	0.59	3,378.12	1.11	–0.10
CI	[–2.02; 3.20]	[–8,060.21; 14,816.45]	[–1.06; 3.29]	[–1.47; 1.27]
p-value	0.653	0.558	0.311	0.883
Effect in %	6.93	8.65	11.71	–3.73
N (left)	788	788	788	788
N (right)	655	655	655	655
(B) Graduates - near				
Effect at 26	0.29	–1,786.53	–0.62	–1.25
CI	[–1.86; 2.44]	[–13,699.81; 10,126.75]	[–2.55; 1.31]	[–3.38; 0.87]
p-value	0.786	0.766	0.524	0.242
Effect in %	2.69	–3.20	–5.19	–24.78
N (left)	1,312	1,312	1,312	1,312
N (right)	627	627	627	627
(C) Dropouts - far				
Effect at 26	–0.16	2,975.47	1.16	0.66
CI	[–3.29; 2.98]	[–15,738.54; 21,689.48]	[–2.00; 4.32]	[–0.67; 1.99]
p-value	0.921	0.752	0.467	0.326
Effect in %	–2.38	10.42	15.82	46.52
N (left)	1,376	1,376	1,376	1,376
N (right)	1,260	1,260	1,260	1,260
(D) Graduates - far				
Effect at 26	3.74**	19,900.47**	3.96**	–3.04***
CI	[0.72; 6.77]	[1,893.83; 37,907.11]	[0.94; 6.98]	[–5.26; -0.83]
p-value	0.016	0.031	0.011	0.008
Effect in %	37.57	39.14	36.24	–61.70
N (left)	1,517	1,517	1,517	1,517
N (right)	915	915	915	915

Notes: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. We retain only individuals aged [22-29) and then remove the units aged [25, 26) since they have less than 1 year of potential eligibility for the Win-Win subsidy (i.e., the donut). Data are reweighted by the triangular kernel that multiplies the sampling weights. We control for age with a linear spline for each side of the cutoff and the set of control variables shown in Table C.2. Panels (A) and (C) refer to high school dropouts, while panels (B) and (D) refer to high school graduates. Panels (A) and (B) refer to individuals living within 60 minutes of the border with Luxembourg by car, while panels (B) and (D) refer to individuals living more than 60 minutes away from the border with Luxembourg by car. The dependent variables consist of accumulated outcomes measured up to 7 years after unemployment entry and include (1) quarters in private sector employment, (2) gross remuneration in the private sector, (3) full-time equivalents in private sector employment (1 for a full-time job in the quarter), (4) quarters in self- and public employment. We report the absolute effect, its confidence interval, and the p-value, as well as the relative effect in % and the number of units for each side of the cutoff. Standard errors are clustered at the age level. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

H.2 Spillover

Table H.5: Spillover on 26-27 – Age Comparisons: DiD Estimates 7 Years After Unemployment Entry

	Employment (1)	Remuneration (2)	FTE (3)	Other Empl. (4)
(A) Dropouts				
Effect on 26–27	–0.45	–4,724.75	–0.82	–0.64
CI	[–2.15; 1.25]	[–14,406.23; 4,956.74]	[–2.58; 0.94]	[–1.61; 0.33]
p-value	0.603	0.339	0.360	0.197
N (treated)	1,315	1,315	1,315	1,315
N (controls)	5,395	5,395	5,395	5,395
(B) Graduates				
Effect on 26–27	0.62	–344.85	0.47	0.47
CI	[–0.92; 2.16]	[–12,486.92; 11,797.23]	[–1.04; 1.98]	[–0.83; 1.78]
p-value	0.430	0.956	0.542	0.476
N (treated)	1,111	1,111	1,111	1,111
N (controls)	3,091	3,091	3,091	3,091

Notes: Doubly robust DiD estimates (Sant'Anna and Zhao 2020) on the inflow sample of youths entering unemployment in 2010 (treatment period) or 2008 (pre-treatment period). The treated are aged 26-27 at unemployment entry, while controls are aged 30-35. Data are reweighted by the sampling weights. We control for the set of control variables shown in Table C.2. Panel (A) refers to high school dropouts, while panel (B) refers to high school graduates. The dependent variables consist of accumulated outcomes measured up to 7 years after unemployment entry and include (1) quarters in private sector employment, (2) gross remuneration in the private sector, (3) full-time equivalents in private sector employment (1 for a full-time job in the quarter), (4) quarters in self- and public employment. We report the absolute effect, its confidence interval, and the p-value, as well as the number of units for the treated and the control group. Standard errors are clustered at the age level. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table H.6: Spillover on 26-27 – Geographical Comparisons: DiD Estimates 7 Years After Unemployment Entry

	Employment (1)	Remuneration (2)	FTE (3)	Other Empl. (4)
(A) Dropouts				
Effect on 26–27	–0.17	–9,922.76	–1.46	–2.57***
CI	[–2.90; 2.55]	[–25,631.96; 5,786.44]	[–4.60; 1.67]	[–3.97; -1.17]
p-value	0.902	0.216	0.361	0.000
N (treated)	654	654	654	654
N (controls)	457	457	457	457
(B) Graduates				
Effect on 26–27	0.18	–193.73	0.33	0.75
CI	[–1.60; 1.95]	[–11,784.99; 11,397.54]	[–1.65; 2.32]	[–1.14; 2.63]
p-value	0.845	0.974	0.741	0.437
N (treated)	857	857	857	857
N (controls)	458	458	458	458

Notes: Doubly robust DiD estimates (Sant'Anna and Zhao, 2020) on the inflow sample of youths entering unemployment in 2010 (treatment period) or 2008 (pre-treatment period) and aged 26-27. The treated live more than 60 minutes from the border, while controls live less than 60 minutes from the border. Data are reweighted by the sampling weights. We control for the set of control variables shown in Table C.2. Panel (A) refers to high school dropouts, while panel (B) refers to high school graduates. The dependent variables consist of accumulated outcomes measured up to 7 years after unemployment entry and include (1) quarters in private sector employment, (2) gross remuneration in the private sector, (3) full-time equivalents in private sector employment (1 for a full-time job in the quarter), (4) quarters in self- and public employment. We report the absolute effect, its confidence interval, and the p-value, as well as the number of units for the treated and the control group. Standard errors are clustered at the age level. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

H.3 Sensitivity

Table H.7: DiD Estimates 7 Years After Unemployment Entry

	Employment (1)	Remuneration (2)	FTE (3)	Other Empl. (4)
(A) Dropouts				
Effect on 24–25	–0.66	–1,675.93	–0.27	0.46
CI	[–2.74; 1.42]	[–14,914.58; 11,562.72]	[–1.96; 1.42]	[–0.55; 1.48]
p-value	0.532	0.804	0.754	0.372
N (treated)	1,018	1,018	1,018	1,018
N (controls)	924	924	924	924
(B) Graduates				
Effect on 24–25	2.75***	13,982.40**	2.29**	–4.63***
CI	[1.18; 4.32]	[3,267.26; 24,697.54]	[0.47; 4.10]	[–7.68; -1.57]
p-value	0.001	0.011	0.014	0.003
N (treated)	1,054	1,054	1,054	1,054
N (controls)	785	785	785	785

Notes: Doubly robust DiD estimates (Sant'Anna and Zhao, 2020) on the inflow sample of youths entering unemployment in 2010 (treatment period) or 2008 (pre-treatment period). The treated are aged 24-25 at unemployment entry, while controls are aged 26-27. Data are reweighted by the sampling weights. We control for the set of control variables shown in Table C.2. Panel (A) refers to high school dropouts, while panel (B) refers to high school graduates. The dependent variables consist of accumulated outcomes measured up to 7 years after unemployment entry and include (1) quarters in private sector employment, (2) gross remuneration in the private sector, (3) full-time equivalents in private sector employment (1 for a full-time job in the quarter), (4) quarters in self- and public employment. We report the absolute effect, its confidence interval, and the p-value, as well as the number of units for the treated and the control group. Standard errors are clustered at the age level. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table H.8: Border Proximity: DiD Estimates 7 Years After Unemployment Entry

	Employment (1)	Remuneration (2)	FTE (3)	Other Empl. (4)
(A) Dropouts - near				
Effect on 24–25	–1.05	–2,009.26	–0.65	0.49
CI	[–3.69; 1.59]	[–17,346.87; 13,328.35]	[–2.26; 2.13]	[–0.79; 1.77]
p-value	0.436	0.797	0.954	0.454
N (treated)	362	362	362	362
N (controls)	315	315	315	315
(B) Graduates - near				
Effect on 24–25	1.15	6,507.45	–0.12	–1.44
CI	[–1.88; 4.19]	[–8,614.68; 21,629.59]	[–2.76; 2.51]	[–4.57; 1.70]
p-value	0.455	0.399	0.926	0.370
N (treated)	436	436	436	436
N (controls)	330	330	330	330
(C) Dropouts - far				
Effect on 24–25	–1.45	–8,813.88	–1.17	0.63
CI	[–4.50; 1.59]	[–27,560.02; 9,932.26]	[–4.17; 1.82]	[–0.72; 1.98]
p-value	0.350	0.357	0.443	0.357
N (treated)	636	636	636	636
N (controls)	601	601	601	601
(D) Graduates - far				
Effect on 24–25	3.54***	16,234.66**	3.21***	–7.05**
CI	[1.20; 5.87]	[1,219.84; 31,249.48]	[0.94; 5.48]	[–12.89; -1.21]
p-value	0.003	0.034	0.006	0.018
N (treated)	597	597	597	597
N (controls)	472	472	472	472

Notes: Doubly robust DiD estimates (Sant'Anna and Zhao, 2020) on the inflow sample of youths entering unemployment in 2010 (treatment period) or 2008 (pre-treatment period). The treated are aged 24-25 at unemployment entry, while controls are aged 26-27. Data are reweighted by the sampling weights. We control for the set of control variables shown in Table C.2. Panels (A) and (C) refer to high school dropouts, while panels (B) and (D) refer to high school graduates. Panels (A) and (B) refer to individuals living within 60 minutes of the border with Luxembourg by car, while panels (C) and (D) refer to individuals living more than 60 minutes away from the border by car. The dependent variables consist of accumulated outcomes measured up to 7 years after unemployment entry and include (1) quarters in private sector employment, (2) gross remuneration in the private sector, (3) full-time equivalents in private sector employment (1 for a full-time job in the quarter), (4) quarters in self- and public employment. We report the absolute effect, its confidence interval, and the p-value, as well as the number of units for the treated and the control group. Standard errors are clustered at the age level. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table H.9: DiD-Placebo Estimates 7 Years After Unemployment Entry

	Employment (1)	Remuneration (2)	FTE (3)	Other Empl. (4)
(A) Dropouts				
Effect on 24–25	–1.25	–8,959.82	–0.74	–0.29
CI	[–3.94; 1.44]	[–29,807.63; 11,887.99]	[–3.72; 2.24]	[–2.78; 2.21]
p-value	0.363	0.400	0.625	0.821
N (treated)	896	896	896	896
N (controls)	818	818	818	818
(B) Graduates				
Effect on 24–25	–1.82	–12,065.72	–1.36	2.06
CI	[–4.99; 1.34]	[–37,167.45; 13,036.00]	[–4.35; 1.63]	[–0.46; 4.58]
p-value	0.258	0.346	0.373	0.109
N (treated)	858	858	858	858
N (controls)	741	741	741	741

Notes: Doubly robust DiD estimates (Sant'Anna and Zhao, 2020) on the inflow sample of youths entering unemployment in 2008 (treatment placebo period) or 2007 (pre-treatment placebo period). The treated are aged 24-25 at unemployment entry, while controls are aged 26-27. Data are reweighted by the sampling weights. We control for the set of control variables shown in Table C.2. Panel (A) refers to high school dropouts, while panel (B) refers to high school graduates. The dependent variables consist of accumulated outcomes measured up to 7 years after unemployment entry and include (1) quarters in private sector employment, (2) gross remuneration in the private sector, (3) full-time equivalents in private sector employment (1 for a full-time job in the quarter), (4) quarters in self- and public employment. We report the absolute effect, its confidence interval, and the p-value, as well as the number of units for each side of the cutoff. Standard errors are clustered at the age level. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table H.10: Bandwidth Sensitivity: RDD Estimates 7 Years After Unemployment Entry

	Employment (1)	Remuneration (2)	FTE (3)	Other Empl. (4)
(A) Dropouts, [21; 30)				
Effect at 26	0.80	6,977.28	1.78*	0.06
CI	[–1.28; 2.89]	[–5,232.69; 19,187.25]	[–0.21; 3.76]	[–0.78; 0.90]
p-value	0.445	0.259	0.078	0.886
Effect in %	11.15	21.41	22.18	3.42
N (left)	3,042	3,042	3,042	3,042
N (right)	2,628	2,628	2,628	2,628
(B) Graduates, [21; 30)				
Effect at 26	2.43***	14,648.54***	2.37**	–1.88**
CI	[0.66; 4.20]	[5,118.43; 24,178.66]	[0.53; 4.21]	[–3.63; -0.12]
p-value	0.008	0.003	0.012	0.036
Effect in %	23.39	28.37	21.33	–38.98
N (left)	3,882	3,882	3,882	3,882
N (right)	1,991	1,991	1,991	1,991
(C) Dropouts, [21.5; 29.5)				
Effect at 26	0.56	5,916.83	1.60	0.27
CI	[–1.69; 2.81]	[–7,320.90; 19,154.57]	[–0.55; 3.74]	[–0.65; 1.19]
p-value	0.622	0.377	0.142	0.558
Effect in %	7.95	18.79	20.28	16.05
N (left)	2,615	2,615	2,615	2,615
N (right)	2,313	2,313	2,313	2,313
(D) Graduates, [21.5; 29.5)				
Effect at 26	2.60***	14,601.21***	2.56**	–2.25**
CI	[0.68; 4.53]	[4,070.98; 25,131.45]	[0.54; 4.59]	[–4.12; -0.39]
p-value	0.009	0.007	0.014	0.019
Effect in %	25.31	28.39	23.14	–45.48
N (left)	3,350	3,350	3,350	3,350
N (right)	1,782	1,782	1,782	1,782
(E) Dropouts, [22.5; 28.5)				
Effect at 26	–0.37	–178.70	0.44	0.93
CI	[–3.19; 2.45]	[–16,635.76; 16,278.36]	[–2.25; 3.14]	[–0.23; 2.08]
p-value	0.793	0.983	0.743	0.113
Effect in %	–5.54	–0.60	5.80	55.78
N (left)	1,835	1,835	1,835	1,835
N (right)	1,657	1,657	1,657	1,657
(F) Graduates, [22.5; 28.5)				
Effect at 26	3.45***	17,060.23**	3.66***	–2.88**
CI	[0.96; 5.94]	[3,402.92; 30,717.54]	[1.19; 6.12]	[–5.23; -0.54]
p-value	0.007	0.015	0.004	0.017
Effect in %	34.61	33.86	33.51	–56.81
N (left)	2,279	2,279	2,279	2,279
N (right)	1,285	1,285	1,285	1,285

Notes: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. We retain only individuals aged [21-30) (Panels A and B), [21.5-29.5) (Panels C and D), or [22.5-28.5) (Panels E and F), and then remove the units aged [25, 26) since they have less than 1 year of potential eligibility for the Win-Win subsidy (i.e., the donut). Data are reweighted by the triangular kernel that multiplies the sampling weights. We control for age with a linear spline for each side of the cutoff and the set of control variables shown in Table C.2. Panels (A, C, E) refer to high school dropouts, while panels (B, D, F) refer to high school graduates. The dependent variables consist of accumulated outcomes measured up to 7 years after unemployment entry and include (1) quarters in private sector employment, (2) gross remuneration in the private sector, (3) full-time equivalents in private sector employment (1 for a full-time job in the quarter), (4) quarters in self- and public employment. We report the absolute effect, its confidence interval, and the p-value, as well as the relative effect in % and the number of units for each side of the cutoff. Standard errors are clustered at the age level. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table H.11: Not Controlling for Xs: RDD Estimates 7 Years After Unemployment Entry

	Employment (1)	Remuneration (2)	FTE (3)	Other Empl. (4)
(A) Dropouts				
Effect at 26	–0.41	916.29	0.63	0.48
CI	[–3.12; 2.30]	[–15,981.06; 17,813.65]	[–2.04; 3.30]	[–0.62; 1.58]
p-value	0.765	0.914	0.638	0.384
N (left)	2,209	2,209	2,209	2,209
N (right)	1,967	1,967	1,967	1,967
(B) Graduates				
Effect at 26	2.31**	9,011.24	2.01*	–2.79**
CI	[0.19; 4.43]	[–3,726.40; 21,748.87]	[–0.04; 4.06]	[–4.99; -0.58]
p-value	0.033	0.163	0.055	0.014
N (left)	2,838	2,838	2,838	2,838
N (right)	1,546	1,546	1,546	1,546

Notes: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. We retain only individuals aged [22-29) and then remove the units aged [25, 26) since they have less than 1 year of potential eligibility for the Win-Win subsidy (i.e., the donut). Data are reweighted by the triangular kernel that multiplies the sampling weights. We control for age with a linear spline for each side of the cutoff. Panel (A) refers to high school dropouts, while panel (B) refers to high school graduates. The dependent variables consist of accumulated outcomes measured up to 7 years after unemployment entry and include (1) quarters in private sector employment, (2) gross remuneration in the private sector, (3) full-time equivalents in private sector employment (1 for a full-time job in the quarter), (4) quarters in self- and public employment. We report the absolute effect, its confidence interval, and the p-value, as well as the relative effect in % and the number of units for each side of the cutoff. Standard errors are clustered at the age level. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table H.12: Effect at 25: RDD Estimates 7 Years After Unemployment Entry

	Employment (1)	Remuneration (2)	FTE (3)	Other Empl. (4)
(A) Dropouts				
Effect at 25	1.56	10,855.31*	2.02**	0.87**
CI	[–0.40; 3.53]	[–503.39; 22,214.01]	[0.12; 3.93]	[0.10; 1.65]
p-value	0.117	0.061	0.038	0.028
Effect in %	25.80	42.93	28.46	73.87
N (left)	2,209	2,209	2,209	2,209
N (right)	1,967	1,967	1,967	1,967
(B) Graduates				
Effect at 25	2.72**	14,176.41**	2.49**	–2.54*
CI	[0.43; 5.01]	[2,454.64; 25,898.17]	[0.31; 4.67]	[–5.36; 0.28]
p-value	0.021	0.018	0.026	0.076
Effect in %	26.88	27.52	22.44	–46.19
N (left)	2,838	2,838	2,838	2,838
N (right)	1,546	1,546	1,546	1,546

Notes: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 25. We retain only individuals aged [22-29) and then remove the units aged [25, 26) since they have less than 1 year of potential eligibility for the Win-Win subsidy (i.e., the donut). The outcome on the left (right) of the cutoff (age 25) is predicted by the left (right) spline. Data are reweighted by the triangular kernel that multiplies the sampling weights. We control for age with a linear spline for each side of the cutoff and the set of control variables shown in Table C.2. Panel (A) refers to high school dropouts, while panel (B) refers to high school graduates. The dependent variables consist of accumulated outcomes measured up to 7 years after unemployment entry and include (1) quarters in private sector employment, (2) gross remuneration in the private sector, (3) full-time equivalents in private sector employment (1 for a full-time job in the quarter), (4) quarters in self- and public employment. We report the absolute effect, its confidence interval, and the p-value, as well as the relative effect in % and the number of units for each side of the cutoff. Standard errors are clustered at the age level. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table H.13: Placebo Before (After) Win-Win: RDD Estimates 7 (5) Years After Unemployment Entry

	Employment (1)	Remuneration (2)	FTE (3)	Other Empl. (4)
(A) Dropouts - 2008				
Effect at 26	–0.03	1,638.21	–0.04	–0.46
CI	[–1.57; 1.52]	[–8,463.14; 11,739.57]	[–1.86; 1.78]	[–1.37; 0.45]
p-value	0.972	0.747	0.964	0.322
Effect in %	–0.32	4.16	–0.42	–18.48
N (left)	2,161	2,161	2,161	2,161
N (right)	1,619	1,619	1,619	1,619
(B) Graduates - 2008				
Effect at 26	0.27	–1,757.46	–0.19	1.31
CI	[–1.87; 2.41]	[–15,663.56; 12,148.64]	[–2.27; 1.89]	[–0.94; 3.55]
p-value	0.802	0.802	0.856	0.249
Effect in %	2.65	–3.47	–1.70	30.52
N (left)	2,679	2,679	2,679	2,679
N (right)	1,307	1,307	1,307	1,307
(C) Dropouts - 2012				
Effect at 26	0.26	303.43	0.14	0.29
CI	[–0.54; 1.05]	[–3,438.05; 4,044.91]	[–0.68; 0.96]	[–0.22; 0.79]
p-value	0.522	0.872	0.732	0.258
Effect in %	7.76	2.06	3.71	34.36
N (left)	2,296	2,296	2,296	2,296
N (right)	2,172	2,172	2,172	2,172
(D) Graduates - 2012				
Effect at 26	–0.56	–4,105.20	–0.70	–0.24
CI	[–2.09; 0.97]	[–12,709.97; 4,499.57]	[–2.37; 0.98]	[–1.51; 1.02]
p-value	0.469	0.345	0.411	0.701
Effect in %	–9.33	–13.93	–11.00	–11.69
N (left)	2,635	2,635	2,635	2,635
N (right)	1,599	1,599	1,599	1,599

Notes: Donut RDD estimates on the inflow sample of youths entering unemployment in 2008 (panels A and B) or 2012 (panels C and D), using age at unemployment entry as the forcing variable with a cutoff at 26. We retain only individuals aged [22-29) and then remove the units aged [25, 26) since they have less than 1 year of potential eligibility for the Win-Win subsidy (i.e., the donut). Data are reweighted by the triangular kernel that multiplies the sampling weights. We control for age with a linear spline for each side of the cutoff and the set of control variables shown in Table C.2. Panels (A and C) refer to high school dropouts, while panels (B and D) refer to high school graduates. The dependent variables consist of accumulated outcomes measured up to 7 years after unemployment entry and include (1) quarters in private sector employment, (2) gross remuneration in the private sector, (3) full-time equivalents in private sector employment (1 for a full-time job in the quarter), (4) quarters in self- and public employment. We report the absolute effect, its confidence interval, and the p-value, as well as the relative effect in % and the number of units for each side of the cutoff. Standard errors are clustered at the age level. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table H.14: Placebo on Post-secondary Graduates: RDD Estimates 7 years After Unemployment Entry

	Employment (1)	Remuneration (2)	FTE (3)	Other Empl. (4)
Post-secondary graduates				
Effect at 26	0.67	–2,927.69	0.07	–1.39
CI	[–1.80; 3.13]	[–24,185.35; 18,329.96]	[–2.30; 2.45]	[–3.63; 0.84]
p-value	0.591	0.784	0.951	0.219
Effect in %	7.29	–4.40	0.70	–12.27
N (left)	2,408	2,408	2,408	2,408
N (right)	1,585	1,585	1,585	1,585

Notes: Donut RDD estimates on the inflow sample of post-secondary graduates entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. We retain only individuals aged [22-29) and then remove the units aged [25, 26) (i.e., the donut). Data are reweighted by the triangular kernel that multiplies the sampling weights. We control for age with a linear spline for each side of the cutoff and the set of control variables shown in Table C.2. We retain only individuals with a tertiary degree. The dependent variables consist of accumulated outcomes measured up to 7 years after unemployment entry and include (1) quarters in private sector employment, (2) gross remuneration in the private sector, (3) full-time equivalents in private sector employment (1 for a full-time job in the quarter), (4) quarters in self- and public employment. We report the absolute effect, its confidence interval, and the p-value, as well as the relative effect in % and the number of units for each side of the cutoff. Standard errors are clustered at the age level. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table H.15: Placebo on False Cutoffs (27-30): RDD Estimates 7 Years After Unemployment Entry

	Employment (1)	Remuneration (2)	FTE (3)	Other Empl. (4)
(A) Dropouts, 27				
Effect at 27	–1.25	–4,398.17	–1.51	–0.11
CI	[–3.36; 0.87]	[–17,585.45; 8,789.11]	[–3.70; 0.68]	[–0.98; 0.76]
p-value	0.244	0.508	0.173	0.798
Effect in %	–15.25	–11.03	–16.97	–4.59
N (left)	2,125	2,125	2,125	2,125
N (right)	1,960	1,960	1,960	1,960
(B) Graduates, 27				
Effect at 27	0.33	1,436.83	0.67	–0.02
CI	[–2.63; 3.28]	[–15,305.01; 18,178.66]	[–1.95; 3.29]	[–1.99; 1.95]
p-value	0.826	0.865	0.612	0.986
Effect in %	2.99	2.60	5.80	–0.39
N (left)	2,442	2,442	2,442	2,442
N (right)	1,422	1,422	1,422	1,422
(C) Dropouts, 28				
Effect at 28	–0.68	–7,809.94	–1.18	1.60*
CI	[–2.71; 1.34]	[–21,255.85; 5,635.97]	[–3.17; 0.81]	[–0.16; 3.36]
p-value	0.502	0.251	0.240	0.075
Effect in %	–7.06	–16.05	–11.02	40.43
N (left)	2,074	2,074	2,074	2,074
N (right)	1,149	1,149	1,149	1,149
(D) Graduates, 28				
Effect at 28	–0.67	–9,173.91	–1.16	–0.59
CI	[–3.25; 1.91]	[–20,771.64; 2,423.82]	[–3.19; 0.86]	[–1.50; 0.31]
p-value	0.607	0.119	0.255	0.195
Effect in %	–7.57	–21.21	–12.29	–23.40
N (left)	2,045	2,045	2,045	2,045
N (right)	1,646	1,646	1,646	1,646
(E) Dropouts, 29				
Effect at 29	0.58	1,599.39	0.00	–0.25
CI	[–1.67; 2.82]	[–10,642.33; 13,841.11]	[–2.03; 2.03]	[–1.51; 1.01]
p-value	0.609	0.795	1.000	0.692
Effect in %	6.99	4.00	0.00	–7.64
N (left)	2,025	2,025	2,025	2,025
N (right)	1,666	1,666	1,666	1,666
(F) Graduates, 29				
Effect at 29	0.09	–1,572.30	–0.23	–1.19
CI	[–2.94; 3.12]	[–15,878.16; 12,733.56]	[–2.91; 2.45]	[–3.71; 1.33]
p-value	0.954	0.827	0.864	0.349
Effect in %	0.96	–3.28	–2.33	–20.51
N (left)	1,763	1,763	1,763	1,763
N (right)	1,038	1,038	1,038	1,038
(G) Dropouts, 30				
Effect at 30	0.39	474.16	0.23	–0.16
CI	[–1.38; 2.16]	[–8,696.98; 9,645.31]	[–1.29; 1.76]	[–1.59; 1.26]
p-value	0.659	0.918	0.760	0.819
Effect in %	5.35	1.29	2.83	–4.78
N (left)	1,967	1,967	1,967	1,967
N (right)	1,601	1,601	1,601	1,601
(H) Graduates, 30				
Effect at 30	–0.70	–6,148.58	–0.11	–0.66
CI	[–3.08; 1.68]	[–25,116.82; 12,819.67]	[–2.43; 2.20]	[–2.57; 1.26]
p-value	0.556	0.518	0.921	0.493
Effect in %	–7.40	–12.14	–1.13	–13.63
N (left)	1,546	1,546	1,546	1,546
N (right)	931	931	931	931

Notes: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a false cutoff at 27 years of age (panels A and B), 28 years of age (panels C and D), 29 years of age (panels E and F), and 30 years of age (panels G and H). We retain only individuals aged over the false cutoff point minus 4 years (including 1 year of "hole") and not older than the cutoff plus 3 years. Panels (A, C, E, G) refer to high school dropouts, while panels (B, D, F, H) refer to high school graduates. See Table C.2 for a description of the outcomes. Standard errors are clustered at the age level. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.