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Abstract

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

We use a regression discontinuity design and difference-in-differences estimators to estimate the impact of a one-shot hiring subsidy for low-educated unemployed youths during the Great Recession recovery in Belgium. The subsidy increases job-finding in the private sector by 10 percentage points within one year of unemployment. Six years later, high school graduates accumulated 2.8 quarters more private employment. However, they substitute private for public and self-employment; thus, overall employment does not increase but is still better paid. For high school dropouts, no persistent gains emerge. Moreover, the neighboring employment hub of Luxembourg induces a complete deadweight loss near the border.

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

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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 employment 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 employment period may also raise productivity and therefore encourage retention. Employment gains may also occur in other firms thanks 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 negative spillovers that they might trigger on ineligible individuals and the displacement 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 spillover effects. In this paper, we fill these gaps by studying the short- and long-run effects on various labor market outcomes of a very generous one-shot hiring subsidy implemented in Belgium during the recovery from the Great Recession. This subsidy, called the *Win-Win Plan*, targeted young unemployed jobseekers with at most a high school degree. A firm hiring a high school dropout (graduate) younger than 26 was eligible for a monthly subsidy of €1,100 (€1,000), which represented 48% (41%) of wage costs, on average. 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 also existed for unemployed persons older than 26, the reform introduced an incremental reduction of wage costs of 24 (18) percentage points.

To estimate the intention-to-treat effect, we apply a one-sided donut regression discontinuity analysis (Barreca et al., 2016) 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 that the hiring subsidy raises the transition to private sector employment 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 employment, which is a relative increase of 28% with respect to the counterfactual of no Win-Win plan. Even though we show that the positive effect is smaller and less of a net gain for society once we take spillover effects into account, we still find that the subsidy created better remunerated jobs for this group.

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. Indeed, 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. 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 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

[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).

in jobs in which the skill level exceeds some minimum threshold, i.e., a high-school degree. This finding is in line with the literature concluding that "work-first" policies are not effective for the lowest-skilled workers (i.e., high school dropouts) because the skill level of the jobs in which these workers end up is too limited to generate significant human capital accumulation (Meghir and Whitehouse, 1996; Card and Hyslop, 2005; Blundell, 2006).[2] Such an interpretation is reinforced by our finding that the subsidy results in more work experience in larger firms for these medium-skilled youths. Recently, Arellano-Bover (2022) demonstrates that large firms are more conducive to human capital investment and that getting a first job at a larger firm leads to better long-term career outcomes. The differential impact of the hiring subsidy by worker education is also consistent with the findings of Caliendo et al. (2011) mentioned above. They report more positive employment effects of a hiring subsidy for youths with a high school degree than for those with lower educational qualifications. Roger and Zamora (2011) also find no impact of a hiring subsidy targeted at open-ended contracts for young high school dropouts in France.[3]

Third, we contribute to the literature on spillover effects. Older studies have typically found that subsidized employment generates more important displacement effects for non-participants than other active labor market policies (e.g., Dahlberg and Forslund, 2005). However, the recent literature on employment subsidies does not find much evidence for negative spillovers on non-participants (Blundell et al., 2004; Kangasharju, 2007; Pallais, 2014; Webb et al., 2016; Cahuc et al., 2019; Saez et al., 2021). In line with this more recent literature, we do not find any evidence that the hiring subsidy targeted at youths would displace the employment of ineligible workers. We instead provide evidence of two different spillover effects.

The first is a displacement at the expense of public sector employment and self-employment. In the absence of the subsidy, eligible workers would have accumulated work experience out-

[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 rates of program failure, 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.

[3] The null effect for high school dropouts is also in line with Cahuc et al. (2021), who, in a French correspondence study, show that past employment experience—whether it is subsidized or not—does not increase the callback rate of high school dropouts if there has been no on-the-job training accompanied by skill certification.

side of the private sector. Second, we find that geographic spillovers can reduce the effectiveness of the hiring subsidy as the policy is a complete deadweight loss near the employment hub of Luxembourg, a small country neighboring Belgium that is characterized by better employment opportunities. Together with the absence of language and legal barriers, the higher wages in Luxembourg are attractive for cross-border workers, such that the labor market tightness in Luxembourg spills over to the neighboring region in Belgium.[4] The ineffectiveness of the subsidy near the border with Luxembourg is in line with the theoretical prediction of the equilibrium search model of Kline and Moretti (2013), which suggests that place-based hiring subsidies are counterproductive in regions where the labor market is too tight. To the best of our knowledge, the literature has not investigated the extent to which the effectiveness of an employment policy in one country can be unintentionally reduced by the proximity of a tight labor market across the border.[5]

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.[6] It 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 already underway in Belgium, in-

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

[5] 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).

[6] As in Cahuc et al. (2019), we use the Google Trends website 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: see Figure A.1 in Online Appendix A.

ducing employment to grow. 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 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 (highly educated 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).

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)

The Win-Win subsidy was not awarded automatically. The jobseeker had to deliver proof of 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

[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

must then draft an appendix to the employment contract mentioning the subsidy amount that he could deduct directly from the net salary of the beneficiary worker. The subsidy – called the "work allowance" – was paid directly by the PUA to the worker. If the recruitment was on a part-time basis, the amount of the subsidy 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, 2011, p. 154), there are serious 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 (very) 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 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.[10] The initial SSC reduction amounted to €333/month for both groups, 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 lowering of 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.

unemployment. If the unemployed was not claiming benefits, the regional Public Employment Service (PES) had to deliver proof to the national PUA that this person was officially registered as an unemployed jobseeker during these periods. This complicates the procedure.

[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] 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.

The Win-Win plan was the onset of an unprecedented decline in the cost of hiring low-educated youths. 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% for dropouts.[11] At age 26, these shares drop sharply to 23% for high school graduates and 24% for dropouts, a decrease of 18 pp and 24 pp, respectively. 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.

The policy had a successful take-up. Between January 2010 and December 2011, 101,000 Win-Win employment contracts were concluded in Belgium, of which 47% were for high school dropouts and 23% for high school graduates, both below age 26, and the remaining 30% were for the long-term unemployed, without any age restriction (ONEM, 2011, p. 87).

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

[11] See Online Appendix C.3 for a detailed explanation of how these shares are calculated.

[12] Individuals living in municipalities with a higher incidence of cross-border work, generally live closer to the border of Luxembourg, and those registered as unemployed jobseekers were over-sampled to enhance precision

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 highly 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 a novel displacement effect that is induced by labor market tightness across borders.

In this sample, we retain first registrations as unemployed jobseekers at the public employment service (PES) between 2007 and 2012 and follow them with monthly 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 each month.[13] We therefore can 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, measured with monthly precision. Since Win-Win was abolished by the end of 2011, we only retain unemployment spells that started in 2010, to include only individuals who do not lose eligibility for the subsidy within the first year of unemployment. To investigate the presence of displacement effects on ineligible individuals, we also include higher-educated and older youths. In addition, for the placebo analysis and the differences-in-difference (DiD) estimator that we implement as robustness analysis, we include entries into unemployment before and after 2010.

We estimate the impact of the wage subsidy reinforcement on several outcomes, which we group into two sets: cumulative exit rates to employment during the first year of unemployment,

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.

[13] Because eligibility for the hiring subsidy is based on the number of days (divided by 25 to express in months) 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 the Win-Win subsidy for youths before reaching the eligibility threshold of unemployment duration as defined in our data, i.e. 3 and 6 months, respectively for dropouts and graduates.

and accumulated employment outcomes up to seven years later. We focus at first on private sector employment because most public sector jobs were excluded from the Win-Win subsidy. However, for the displacement analysis, we also consider *other* than salaried private sector employment, 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, experience, and receiving unemployment benefits receipt, 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.1 and C.2.

As shown in Table 2, about 20% of youths aged 22-25 enter a Win-Win job within one year after entry into unemployment. The take-up of the subsidy 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.[14]

For other outcomes the level of education does matter. The probability of starting a salaried private sector job within one year is 58% for eligible high school graduates, compared to only 44% for eligible high school dropouts. Similarly, high school graduates worked 12.5 quarters in the private sector, on average, over the next seven years, while high school dropouts worked only 8.2 quarters, on average. For both groups, this outcome is about 4 quarters higher for individuals taking up the Win-Win subsidy. However, this positive difference in a favor of Win-Win takers is partially compensated by a lower number of quarters spent in other forms of employment: 2.4 vs. 3.7 quarters for the Win-Win takers vs. the eligible population. The reduction is larger for high school graduates (2.5 vs. 4.7 quarters) than dropouts (2.2 vs. 2.5 quarters). This descriptive evidence already suggests the displacement of non-private sector employment by private sector employment, for which we provide causal evidence below.

[14] As a point of comparison, Cahuc et al. (2019) report a take-up of 24% for the hiring credit targeted to the hiring of low-wage workers in small firms during the Great Recession in France.

Table 2: Selective Descriptive Statistics: Outcomes

	Dropouts (22-25)		Graduates (22-25)	
	All (1)	Win-Win (2)	All (3)	Win-Win (4)
Take-up Win-Win 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.

Table C.2 in Online Appendix C.2 compares the explanatory variables between young adults who start a subsidized job and those who do not. 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 selective, and hence, the above descriptive statistics of outcomes cannot be given a causal interpretation.

4 Identification Strategies and Estimation Methods

4.1 Regression Discontinuity

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 unemployed individuals 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 counterfac-

[15] Recall, as mentioned in Section 3, because in the data unemployment duration is measured imperfectly, we cannot exploit the eligibility thresholds based on duration in the analysis. Consequently, we can only identify intention-to-treat effects.

tual 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 in unemployment will indeed gradually, along the unemployment spell, age out of eligibility for the higher hiring subsidy for youths in the Win-Win plan. To address this issue, we ignore observations of individuals aged between 25 and 26 at the start of the unemployment spell.[16] 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 of individuals aged between 25 and 26 we create a "hole" to the left of the age threshold 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 precise *manipulation* around the threshold. In the latter problem the extrapolation is required at both sides of the discontinuity threshold, whereas here the extrapolation is only required on the left-hand side. This extrapolation allows us to identify the intention-to-treat effects of the subsidy reinforcement during one year for youths aged 26 at entry in unemployment. Details on the formal implementation of the estimator can be found in Online Appendix D.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).

[16] In a sensitivity we shrink the "hole" to increase precision for outcomes measured before 1 year. Results are robust and available upon request.

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 series of sensitivity analyses described in Section 5.2. One of these analyses consists in implementing the doubly robust conditional difference-in-differences (DiD) estimator proposed by Sant'Anna and Zhao (2020) as explained in more detail in Online Appendix D.2.

The DiD estimator directly takes advantage of the existence of a pre-treatment period in our data to eliminate time-invariant unobserved heterogeneity. Similar to the RDD estimator, the treated and control groups are built according to the age at entry into unemployment. The post-treatment period is composed of unemployment registrations in 2010, while the pre-treatment period consists of unemployment registrations in 2008. We do not use the unemployed entering in 2009 since these individuals quickly enter the post-treatment period in 2010. We restrict our treated units to those satisfying two different age conditions. First, they should be older than 24 years old at the beginning of the calendar year of unemployment registration.[17] Second, they should be under the age of 25 at unemployment registration to guarantee that they have at least one year of age eligibility during the treatment period. Our control group is composed of individuals older than 26 but younger than 27, which we increase to 30 years old for robustness.

The main identifying assumption is that the counterfactual outcomes in the absence of treatment of the treated and the control group follow parallel trends. In Section 5.2, we show descriptives on the evolution of the outcomes over time. The treatment effect is then estimated by relaxing the parallel trend assumption to hold only conditional on our predetermined control variables shown in Table C.2 in Online Appendix C.2. Parallel trends before the treatment is tested by comparing the unemployed registrations of 2008 to those of 2007.

[17] This ensures that the unemployment entries of 2008 are above the age-eligibility threshold of 26 by the time they reach 2010 and therefore remain unaffected by the treatment.

5 Empirical Findings

This section presents the empirical findings of our analysis. We present (1) the impact of the Win-Win subsidy on short- and long-run salaried employment outcomes in the private sector; (2) how the geographic proximity of the economic hub of Luxembourg results in a full deadweight loss of the hiring subsidy close to the border; (3) whether the Win-Win subsidy displaces the employment of older ineligible workers and/or employment *other* than private sector salaried employment; (4) several placebo tests and robustness analyses; (5) a cost-benefit analysis. The results are reported graphically. The main econometric estimates and associated statistics underlying these graphs are reported in Online Appendix E.

5.1 Main results

5.1.1 Impact on Salaried Employment Outcomes in the Private Sector

Transition Probabilities in the Short Run

Panel (a) of Figure 1 shows that, conditional on being hired, the monthly subsidy amount drops sharply at age 26 from €380 to €76 per month. These amounts are estimated by the donut RDD described in Section 4.1. The donut excludes the red diamonds reporting the subsidy amounts for youths aged between 25 and 26 at entry in unemployment. These amounts should be excluded because these youths are only eligible for the high Win-Win subsidy during a part of the year, i.e. until their 26^{th} anniversary. The aforementioned amounts are the predictions of the subsidy amounts at age 26 using the estimated linear splines to the left and the right of age 26. The figure confirms that private sector firms have a significant benefit of hiring workers slightly younger than 26, relative to slightly older ones. The differential expected monthly subsidy at age 26 is therefore estimated to be €304 euros (significant at the 0% level), 13% of the average full-time monthly wage cost on the right of the cutoff (€2,392). This share does not differ by educational attainment.[18]

The drop of the subsidy amount at age 26 reported in Panel (a) of Figure 1 as a share of

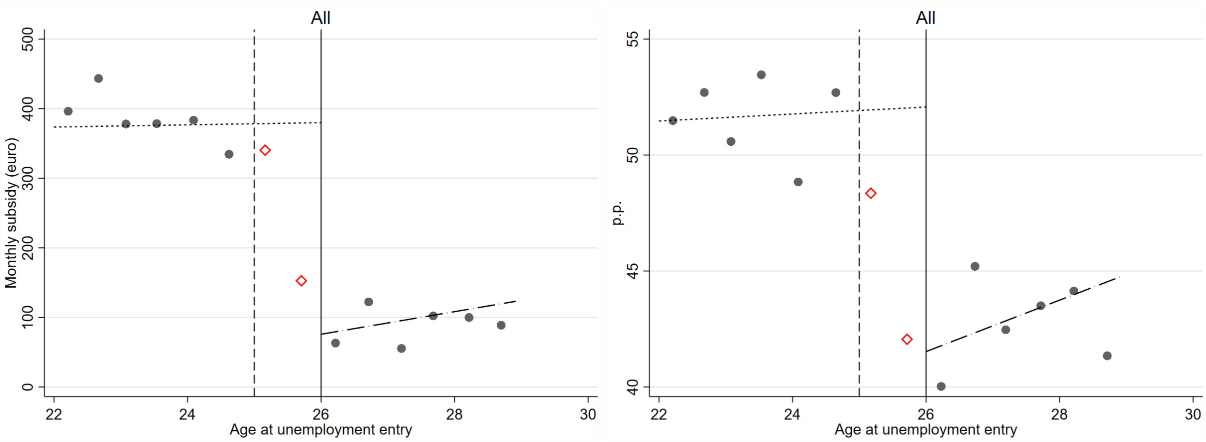
[18] For high school dropouts and graduates the differential monthly subsidy is €297 and €323, respectively (see Figure A.2 in Online Appendix A).

wage-costs is lower than the decline conditional on take-up reported at the end of Section 2. This is because not all hired youths are eligible for the subsidy, or employers do not take-up the subsidy. Some youths do not comply with the unemployment duration requirements or with the associated administrative formalities. Employers may not take-up the subsidy because of the high fixed costs induced by these formalities, which in particular for short-lived jobs may not counterbalance the benefits. We estimate the attention rate (i.e., the share of subsidized hires among all hires) to be 36% at the left-hand side of the age threshold at 26 and 14% at the right-hand side of this threshold.[19] The attention rate is lower to the right of the threshold because 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.

Figure 1: Discontinuity at Age 26 of Employment Outcomes Within One Year

(a) Subsidy Amount Conditional on Hiring in the Private Sector

(b) Cumulative Transition Rate to Private Sector 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 bandwidth is set at 3 years on each side of the donut. The outcome is (a) the amount of received subsidy (in full-time equivalent) conditional on hiring in the private sector and (b) 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.2. The effect estimated by the donut RDD estimator at 26 years old is (a) 304 euros [185; 423] with a p-value of 0.000 and N = 3,958 and (b) +10.5 pp [3.0; 18.1] with a p-value of 0.007 and N = 8,560. Standard errors are clustered at the age level. Standard errors are clustered at the age level.

Panel (b) of Figure 1 shows that the corresponding donut RDD estimates in the cumulative transition rate to private sector employment one year after entry into unemployment in 2010

[19] Online Appendix C.3 explains how these attention rates are estimated.

is equal to 10.5 pp (significant at the 0.7% level), a proportional increase of 25% relative to the counterfactual. This is clear evidence that the subsidy reinforcement for youths has had a positive impact on this transition rate.

When we split the sample by education, binned data points become more noisy and estimates, therefore, less precise (see Figure A.3 in the Online Appendix A). For high school graduates and dropouts, the corresponding point estimates are 8 pp and 13 pp (p-values, 0.25 and 0.02), respectively, 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. In Figure A.4 in Online Appendix A we report the evolution of the RDD estimates of these cumulative transition rates by educational attainment from one to six quarters after entry in unemployment. 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.

Finally, we combine the information of panels (a) and (b) of Figure 1 to obtain an estimate of the employment elasticity with respect to change in labor costs induced by the incremental hiring subsidy. 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 labour costs (i.e. -12.7%), and is, hence, equal to -2.0.[20] This estimate of the elasticity is in the range reported in several studies evaluating the impact of hiring subsidies targeted at long-term unemployed:[21] -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). However, the high elasticities we estimate only provide a sense of the magnitude of the subsidy

[20] For dropouts it is larger in absolute value ($40.4\%/(-13.0)\% = -3.1$) 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.

[21] 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.

effects on hiring in the short-run. It remains an open question whether these effects persist beyond the end of the subsidy period in the long-run.

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. This number should attain a maximum around quarter 11 when the Win-Win subsidy expires for all unemployment entries.[22] For high school graduates, we find a second even higher peak after 21 quarters, but this is a consequence of estimation imprecision: After 11 quarters, the cumulative effect fluctuates 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 little (see Figure A.5 in Online Appendix A).

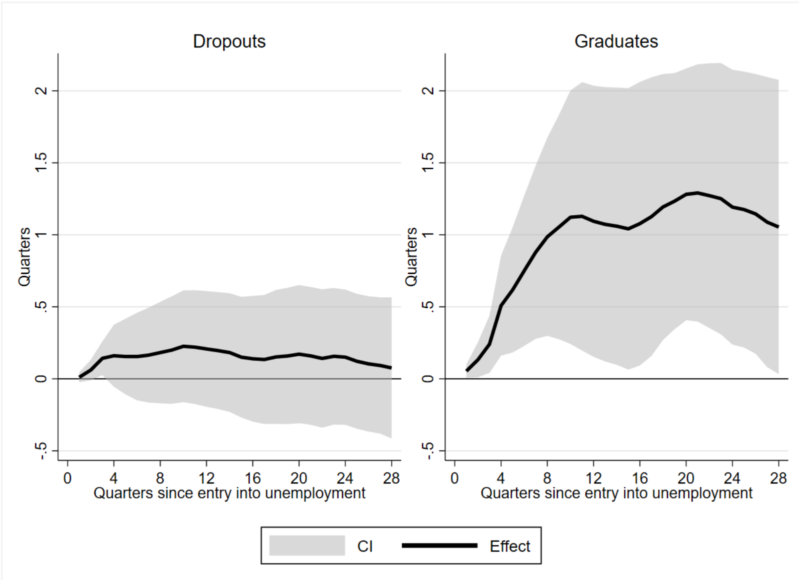
The most striking observation is that around the expiration of the subsidy, the effect of the Win-Win Plan on the average number of quarters in subsidized employment is five times smaller for high school dropouts than for high school graduates. This result implies that subsidized employment tends to 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 of the hiring subsidy by schooling level on the accumulated number of quarters in private sector employment from entry into unemployment in 2010 until seven years later. From Figure 3, we can deduce that the Win-Win subsidy did not affect the time spent in private sector employment for dropouts. During the first eleven quarters, the point estimates do exhibit a slight increase in the average number of quarters in employment, but this effect is not statistically significant. Moreover, after the end of the subsidy period (around 11 quarters after registration in unemployment), the estimated effect gradually falls back to zero and stays there. This is evidence that for dropouts,

[22] The benefits expire between 4 and 11 quarters after entry into unemployment, depending on the calendar year when the subsidy is taken and whether subsidized employment is entered right after the start of unemployment or later.

the hiring subsidy only accelerates the transition to short-term jobs and does not generate any persistent effect on employment.

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

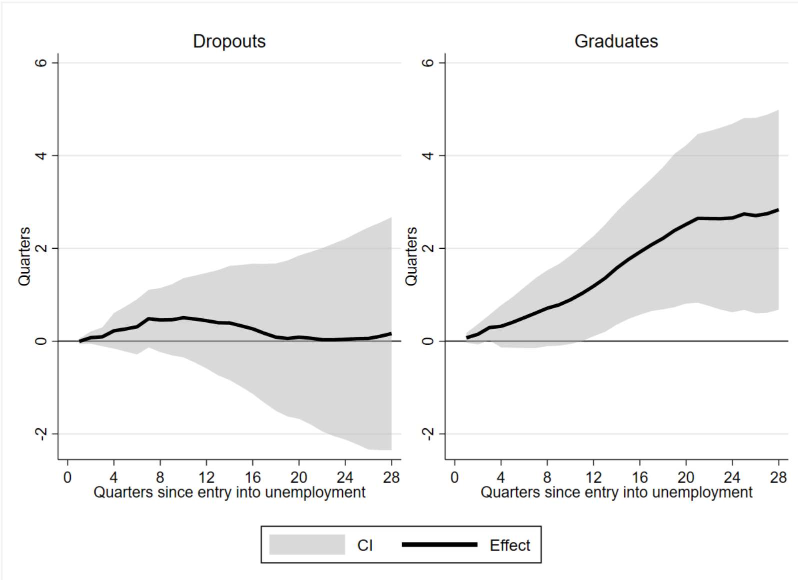


Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the cumulative number of quarters in subsidized private sector employment by 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.2. 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).

In contrast, from Figure 3 we observe that the Win-Win subsidy steadily increases the average number of quarters in employment up to 2.8 quarters seven years after entry into unemployment.[23] 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 twelve onwards. From Figure A.7, we can deduce that the gains are in terms of full-time equivalent (FTE) employment. After seven years, about three FTE quarters of employment are gained, on average (+26%).

[23] In Figure A.6, the corresponding RDD graphical evidence is reported seven years after entry into unemployment.

Figure 3: 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.2. 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).

The aforementioned findings for high school graduates are consistent with both signaling and human capital theory. On the one hand, the hiring subsidy can be effective by giving young inexperienced workers the chance to reveal their abilities, an opportunity that would otherwise not be given because of high recruitment costs. In this way, a temporary subsidy can have long-lasting effects (Pallais, 2014). On the other hand, by providing some initial work experience (“work first”), young people are given the opportunity to build up firm-specific and general human capital on the job that gradually enhances future employment opportunities, productivity, and wages, as will be discussed below (Ben-Porath, 1967; Blinder and Weiss, 1976; Mroz and Savage, 2006). In contrast, for dropouts this pathway does not seem to work, presumably because they enter new jobs that are too short-term and have skill requirements too low to initiate a process of human capital accumulation (see, for example, Card and Hyslop, 2005; Autor and Houseman, 2010; Cahuc et al., 2021).

From Figure A.8 in Online Appendix A, we deduce that the effects on gross wage earnings (assigning zero earnings to those who are not employed) 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, on average, after 5.5 years, beyond which the effect stabilizes. Relative to the counterfactual, the increase after 7 years is 29%, mirroring the proportional effect on employment. Combining these two pieces of evidence suggests that in the long run, the subsidy does not have any impact on the wage rate. However, this conclusion is premature because of the wide confidence intervals around these point estimates. To find more conclusive evidence, we estimate the effect of the subsidy on the cumulative number of quarters spent in private sector employment paying more than the median daily wage. Figure 4 reveals that in the long run, all additional employment is created in these high-paying jobs and that the effect on low-paying jobs is zero (see Figure A.9 in Online Appendix A).[24]

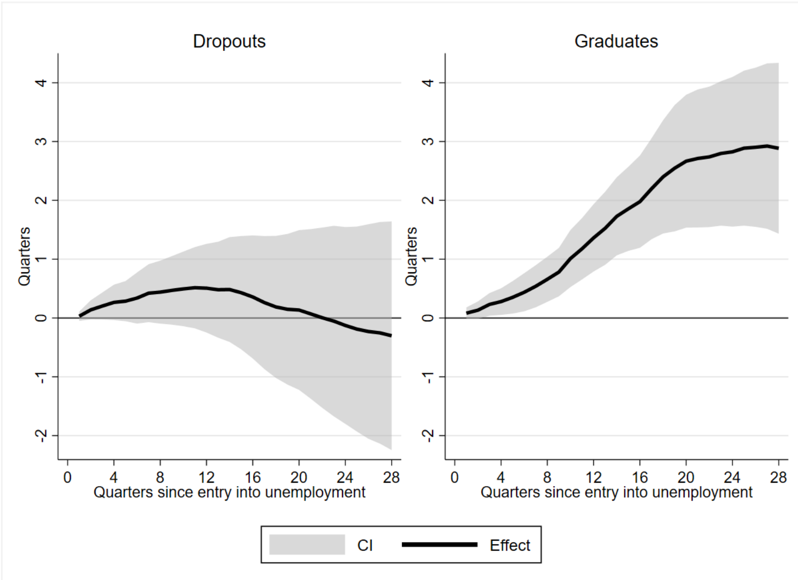
For high school graduates, the effect on the time spent in high-wage jobs is statistically significant already four quarters after entry into unemployment, while it is never statistically different from zero for high school dropouts. Importantly, it extends beyond the expiration of the Win-Win subsidy. This suggests that this effect is not reflecting a partial incidence of the subsidy on the wage.[25] Even if this gradual progression of high-wage employment beyond the subsidy period suggests that the aforementioned human capital explanation is at work, we cannot rule out a pure signaling explanation. Specifically, the enhanced accumulation of high-pay employment may just reflect the time profile of wages for high-ability workers who are hired because of the subsidy, rather than the accumulation of human capital generated by the additional work experience that the subsidy triggers.[26]

[24] Results are robust if we allow the median to be time-varying or education-specific.

[25] An additional reason why this does not reflect an incidence effect is that the subsidy is awarded for a limited period and is targeted at a very specific group. If there is a windfall gain from the subsidy, within-firm pay equity concerns will induce a sharing of this windfall with other workers (Saez et al., 2019) and, hence, dilute much of this wage gain.

[26] The impossibility of distinguishing between these two explanations is a consequence of the so-called "double selection" problem, which makes it difficult to determine whether the effect on employment is selective or not (Heckman, 1974).

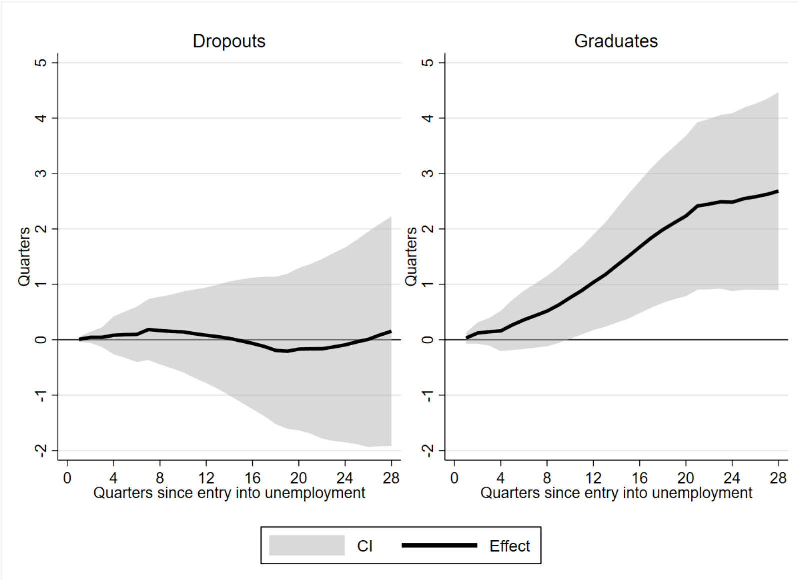
Figure 4: Evolution of the RDD Effect on the Cumulative Number of Quarters in a Private Sector Job Paying More than the Median Daily Wage



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 private sector job paying more than the median daily wage (€83.5) 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.2. Standard errors are clustered at the age level. For dropouts (graduates), the effect at 7 years is –0.3 quarters [–2.2; 1.6] with a p-value of 0.758 and N = 4,176 (+2.9 quarters [1.4; 4.3], p-value 0.000 and N = 4,384).

To get some more insight into this question, we study whether the long-term effect of the subsidy comes about in an environment that is more conducive to on-the-job skill acquisition for high school graduates and not for dropouts. Recently, Arellano-Bover (2022) shows that young people employed in large German firms acquire more skills on the job than those employed in small firms because large firms provide more training, but also because they provide more learning opportunities from better peers and managers and a more productive environment. In line with this evidence, Arellano-Bover (2020) demonstrate that the returns to experience obtained at large firms in Spain are greater than those obtained in small firms. Similarly, Albanese et al. (2021) show that in Italy on-the-job training in larger firms is more effective at boosting permanent employment of youths in the training firm but also in other firms. We, therefore, investigate to what extent the time profile of the effect of the hiring subsidy on time spent in high-wage employment is reflected in the effect on the accumulation of time employed in a large firm. Such a finding would be more consistent with a human capital accumulation story than one in which the subsidy allows some workers to signal their ability.

Figure 5: Evolution of the RDD Effect on the Cumulative Number of Quarters in a Firm with More than 50 Employees



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 private sector job in a firm employing more than 50 employees 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.2. Standard errors are clustered at the age level. For dropouts (graduates), the effect at 7 years is 0.1 quarters [–1.9; 2.2] with a p-value of 0.883 and N = 4,176 (+ 2.7 quarters [0.9; 4.5], p-value 0.004 and N = 4,384).

Figure 5 displays the effect of the Win-Win subsidy on the cumulative number of quarters employed in a firm with more than 50 employees for high school dropouts (left panel) and graduates (right panel).[27] The patterns of these long-run effects are very similar to those displayed in Figure 4 for the effects on the time spent in higher-paying jobs. This suggests that for high school graduates, the hiring subsidy is effective in the long run when large firms use the subsidy for hiring new workers, presumably because they lead to more human capital investment. In

[27] We use 50 employees which is the observed median in the sample. The effect on the time spent in smaller firms is always small and never statistically different from zero, while the difference-in-differences estimator yields very similar results (Figure A.10 in Online Appendix A).

contrast, the Win-Win subsidy does not stimulate large firms to hire high school dropouts, such that no persistent effects emerge.[28]

5.1.2 Geographic Spillover Effects from the Economic Hub 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,[29] in 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. In 2010, the total employment rate for youths aged 25 to 34 living within one hour's driving distance from the border with Luxembourg was fourteen percentage points higher than those living farther away (77% versus 63%), and the unemployment rate was only half as high (10% versus 20%).[30]

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 argue that this conclusion is not affected, 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. The Belgian hiring

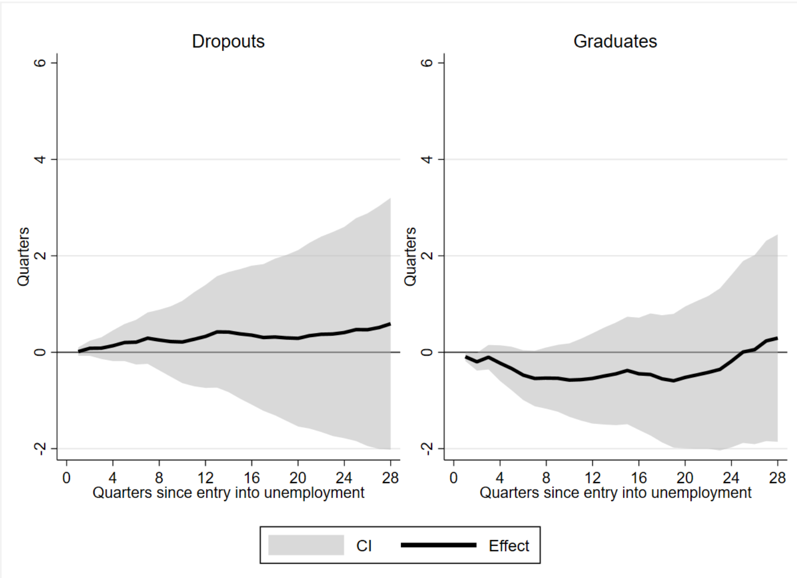
[28] We find evidence that for high school graduates the subsidy enhances the transition rate to a larger firm more than to a smaller one, while for high school dropouts the effect is equally divided among larger and smaller firms (see Figure A.11 in Online Appendix A).

[29] 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.

[30] 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.

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 employed population is smaller close to the border with Luxembourg than farther away[31] and those jobs tend to be in low-status occupations.[32] 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.

Figure 6: Evolution of the RDD Effect on the Cumulative Number of Quarters in Private Sector Employment – Close to the Border

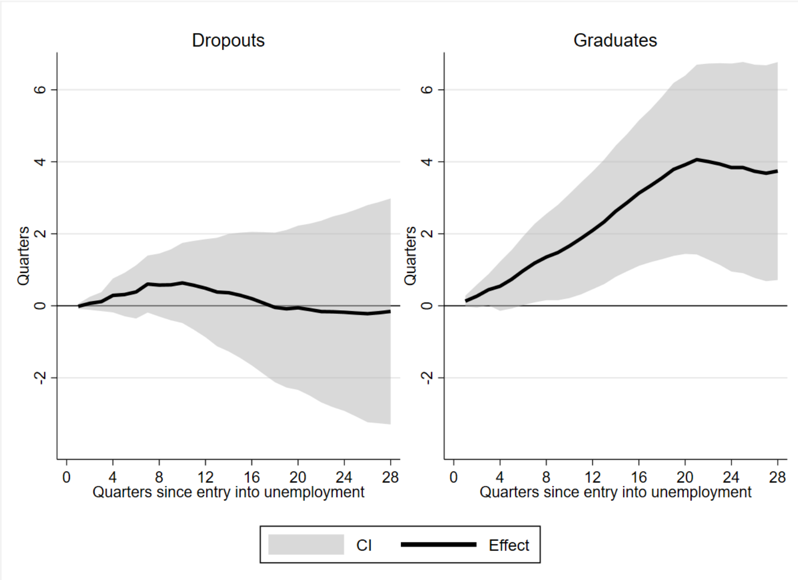


Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the cumulative number of quarters in private sector employment since entry 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).

[31] In our data, this was 42% (61%) within (beyond) 60 minutes driving distance from the border for workers aged 25-34 in 2010.

[32] 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.

Figure 7: 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.12 in Online Appendix A displays the differential amount of subsidy received for hiring jobseekers living within 60 minutes driving distance from the border with Luxembourg.[33] It can be observed 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. The long-run evolution of the RDD effect on the number of quarters in private sector employment is displayed in Figure 6. It can be seen that this effect is never 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 more than one hour's drive from the border, the subsidy reinforcement grants €249 (€377) more to the dropouts (grad-

[33] We use a 60-minute threshold since this is the observed median value in our sample. Furthermore, as shown in Figure A.13, 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).

uates). This raised, for graduates only, employment by 3.7 quarters seven years after entry into unemployment (Figure 7). 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.

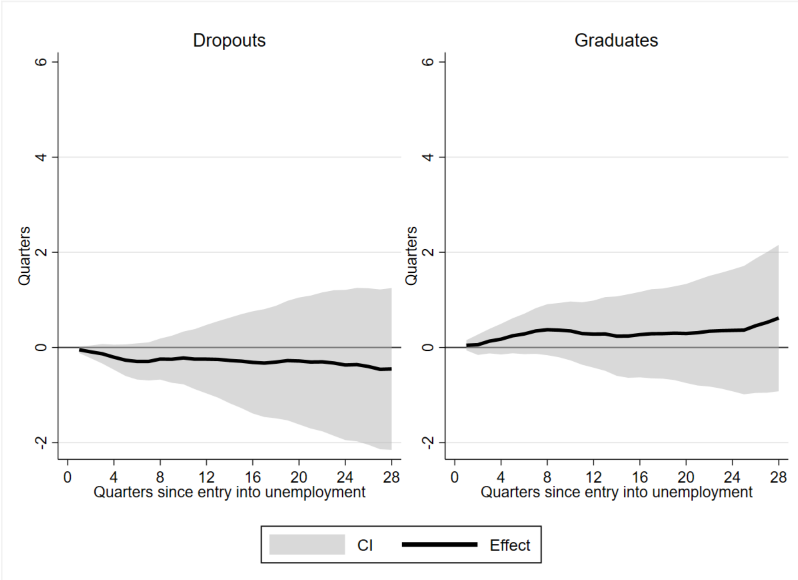
In Online Appendix A, we also report the effect on the accumulated time spent in private sector employment estimated from a model in which we interact the splines and the treatment indicator of the donut RDD estimator with the travel distance from the border with Luxembourg, instead of splitting the sample into subgroups. Figure A.14 displays the predicted effect over the distance for high school graduates, in linear and quadratic specifications. This reveals 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. Figure A.15 in Online Appendix A displays the corresponding effects for high school dropouts. It confirms that for this group, the effects are close to zero for any travel distance.[34]

5.1.3 Displacement Effects

Existing studies focus on displacement in the short run. However, we have seen that the Win-Win subsidy generates important long-run effects on private sector employment for high school graduates. An interesting question is thus whether displacement can reduce the long-run effectiveness of hiring subsidies. In this section, we, therefore, focus our analysis on the detection of potential long-run displacement effects in two different directions. First, does the reinforced private-sector employment for the highly skilled come at the expense of the slightly older high school graduates above the age cutoff of 26? Second, does accumulating employment in the private sector for eligible high school graduates come at the expense of their employment in other sectors, i.e., public sector employment, self-employment, and cross-border employment?

[34] 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.16 in Online Appendix A, no significant reduction in cross-border work is found.

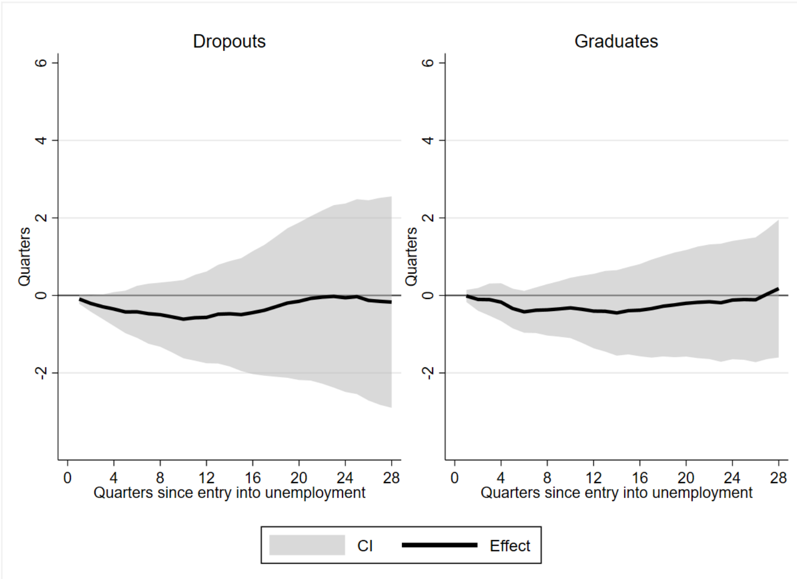
Figure 8: Evolution of the DiD Effect on the Cumulative Number of Quarters in Private Sector Employment: Cohort Aged 26-27 Compared to the Cohort Aged 30-35



Note: Evolution of the displacement 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(post)-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).

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 (see Sant'Anna and Zhao, 2020), as described in Section 4.2 and Online Appendix D.2. 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 8). 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 9). The rationale for the latter contrast stems from the argument that a displacement of 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 displacement effects on the ineligible group aged 26-27 are small throughout the 7 years since unemployment entry and never statistically different from zero.[35]

Figure 9: 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



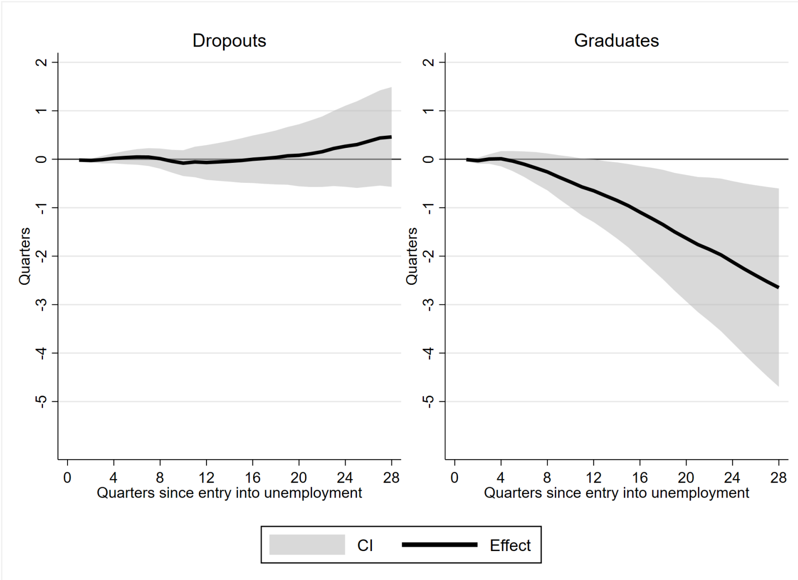
Note: Evolution of the displacement effect estimated with a doubly robust DiD estimator (Sant'Anna and Zhao, 2020) and confidence intervals (CI) for the cumulative number of quarters in private sector employment since entry into unemployment in 2010 by schooling level: dropouts (left) vs. graduates (right). 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(post)-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).

Figure 10 displays the evolution since entry into unemployment in 2010 of 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, self-employment, and cross-border employment, and Figure A.17 in Online Appendix A displays the corresponding discontinuity plot after 7 years. For high school graduates, one can see that the plot is nearly the mirror image of that in Figures 3 and A.6. The RDD effect on *other* employment declines initially, with some delay relative to the positive effect on private sector employment, but after 7 years it is significantly

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

negative and equal to –2.6 quarters. The overall effect on total employment is therefore never significantly different from zero and is very close to zero after 7 years for both dropouts and graduates (Figure A.18 in Online Appendix A).

Figure 10: Evolution of the RDD Effect on the Cumulative Number of Quarters in Non-Private Sector Employment

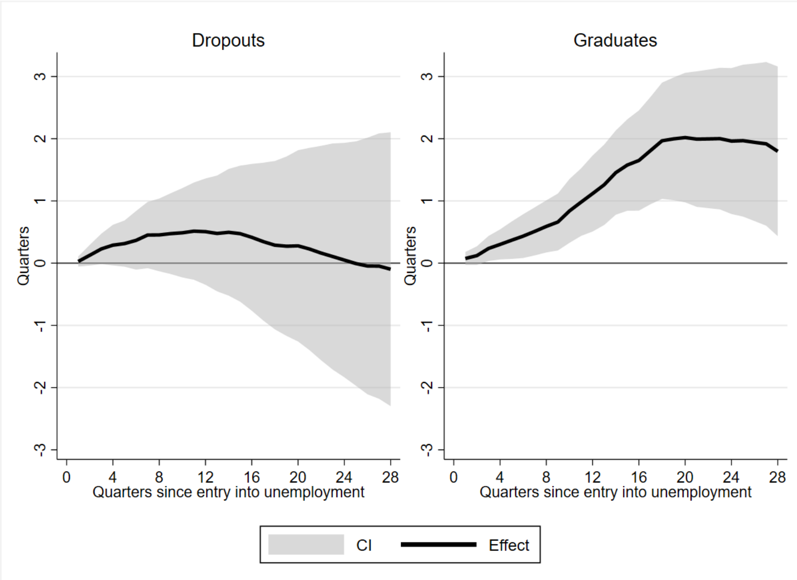


Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence intervals (CI) for the cumulative number of quarters in non-private sector employment (public sector employment, self-employment, and cross-border 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.2. Standard errors are clustered at the age level. For dropouts (graduates), the effect at 7 years is 0.5 quarters [–0.6; 1.5] with a p-value of 0.375 and N = 4,176 (–2.6 quarters [–4.7; –0.6], p-value 0.012 and N = 4,384).

Because of a lack of data, we cannot measure the effect on earnings for self-employed and cross-border work. Nevertheless, since for high school graduates about 80% of the negative effect on *other* employment is due to public sector jobs (Figure A.19 in Online Appendix A), it makes sense to consider the cumulative effect on earnings in the private or public sectors (Figure A.20 in Online Appendix A). Our estimates show a positive but statistically insignificant effect of about €4,000 (against €14,600 in the private sector only; see Figure A.8) at 7 years. Due to this lack of precision, we consider again the alternative of measuring the effect on the cumulative number of quarters in *high-wage* private or public sector employment (Figure 11).[36]

[36] The median daily salary is updated to also include public sector jobs, and it is €84.12/day.

Figure 11: Evolution of the RDD Effect on the Cumulative Number of Quarters in a Public or Private Sector Job Paying More than the Median Daily Wage



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 public or private sector employment paying more than the median daily wage (€84.1) 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.2. Standard errors are clustered at the age level. For dropouts (graduates) the effect at 7 years is –0.1 quarters [–2.3; 2.1] with a p-value of 0.930 and N = 4,176 (+1.8 quarters [0.4; 3.2], p-value 0.001 and N = 4,384).

We find that after 7 years, the marginally eligible group has still accumulated 1.8 more quarters (statistically significant at the 5% level) in a high-paid job than the marginally ineligible group, which is still two-thirds of the effect found for high-paid jobs in the private sector only (see Figures A.21 and A.22 in Online Appendix A for the corresponding discontinuity plot after 7 years). In addition, the eligible jobseekers have accumulated one quarter less in low-paid jobs, but the effect is not significant (Figure A.23 in Online Appendix A). While we find a full displacement of the positive effect on private sector employment, these findings suggest that the hiring subsidy did result in a *net* creation of higher-paying jobs.

In Section 5.1.1, we argued that the long-run effect on high-paid employment came about by large firms recruiting subsidized high school graduates and offering better on-the-job skill development. Even if this private sector job creation comes at the expense of public sector employment, the finding that the long-run effect on high-paid employment is not affected much suggests that the subsidy substitutes lower-quality public sector jobs, and possibly self-employment, for higher-quality private sector jobs.

5.2 Robustness Analyses

5.2.1 Sensitivity tests

In this section, we report a series of validation tests. First, we rerun the one-sided donut RDD estimator widening or narrowing the bandwidth. As shown in Figure A.24 in Online Appendix A, the results are close to the benchmark estimates. Second, we remove the conditioning variables from the RDD estimator and again obtain similar estimates (see Figure A.25 in Online Appendix A). 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. As shown in Figures A.26-A.29, the results are very similar. 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 (Figure A.30 in Online Appendix A).

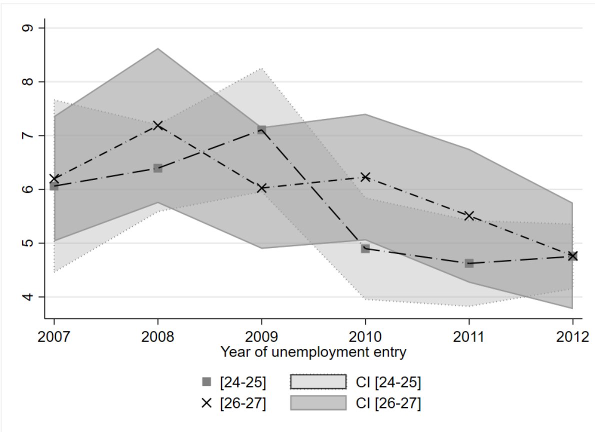
We also implement three placebo tests for the donut RDD estimator. First, we estimate whether we detect any statistically significant jump at age 26 for individuals entering unemployment before the introduction and after the abolition of the Win-Win plan (2008 and 2012). Second, we check whether at the 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 a series of placebo tests that use different false cutoff points of the forcing variable. Finally, we apply the donut RDD estimator to detect jumps in the control variables at the discontinuity. As shown in Figures A.31-A.37 and Table E.1 in the Online Appendix, these placebo tests deliver insignificant estimates. 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

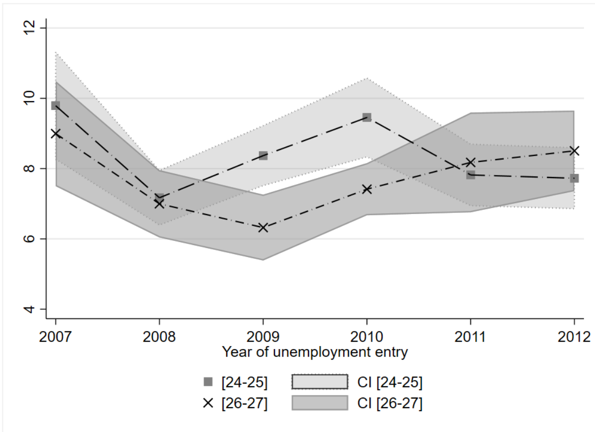
We use a DiD estimator to replicate the RDD estimates as explained in Section 4.2. As shown in Figure 12, descriptive evidence on the evolution of the cumulative number of quarters in private sector employment over time does not reveal any statistically significant difference for jobseekers aged 24-25 (treated) and 26-27 (controls) registering during the pre-treatment period (2007 and 2008).[37] For the unemployment registrations of 2009 (partially treated) and 2010, we observe a jump in the outcomes of the treated high school graduates, which reabsorbs when the subsidy started to be phased out in 2011.[38] This preliminary evidence corroborates the presence of a positive long-run effect for the high school graduates.

Figure 12: Evolution of Cumulative Number of Quarters in Private Sector Employment

(a) Dropouts



(b) Graduates



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

We formally implement the DiD estimator by comparing the outcomes of the two groups of jobseekers, registering in 2008 (pre-treatment) or 2010 (post-treatment).[39] The results are very similar to those obtained by implementing the RDD estimator and are shown in Figure 13. We also implement a placebo test (see Figure A.40 in Online Appendix A) comparing

[37] For this exercise we stop at 5 years after unemployment registration to have a sufficiently long time horizon also for the entries of 2011-2012. See Figures A.38 and A.39 in Appendix A for the other cumulative outcomes.

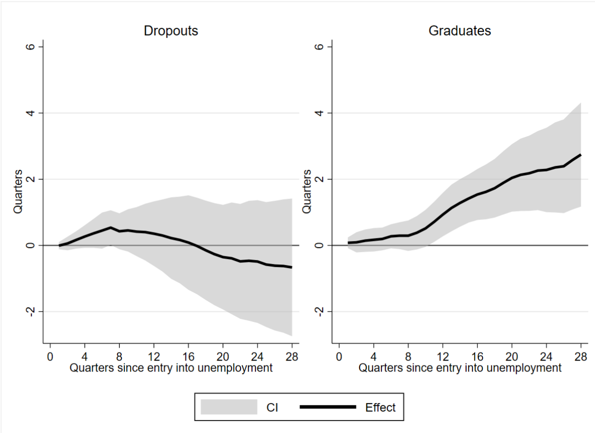
[38] Jobseekers registering in 2009 (2011) quickly enter (exit) the subsidized treatment period of 2010-2011.

[39] In a further sensitivity check, we progressively widen the size of the control group to the age of 30, which delivers very similar results. Results are available upon request.

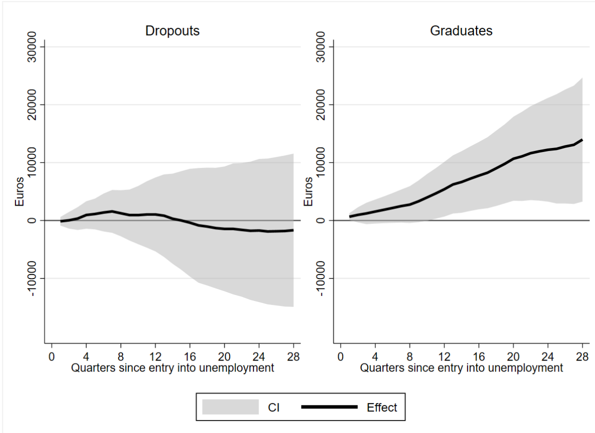
unemployment entries of 2008 to the ones of 2007. No estimates are statistically significant, affirming the reliability of the parallel trend assumption. Importantly, the DiD estimator also replicates the heterogeneous effects found for individuals living near or far from the border with Luxembourg (see Figures A.41-A.42 in Online Appendix A).

Figure 13: Evolution of the DiD Effect on Cumulative Outcomes

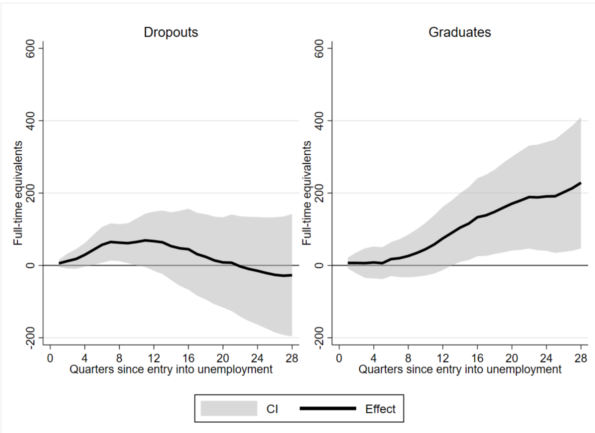
(a) Private sector employment



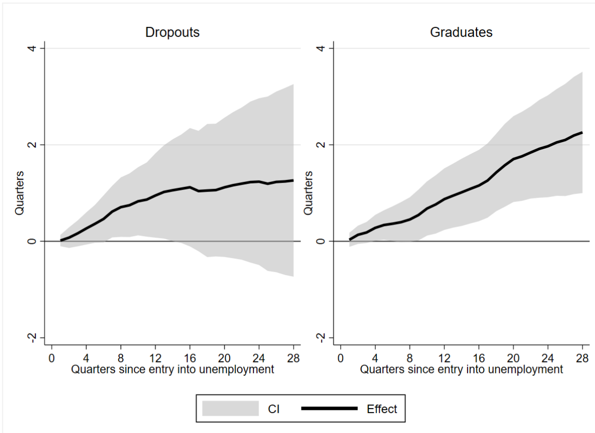
(b) Gross earnings



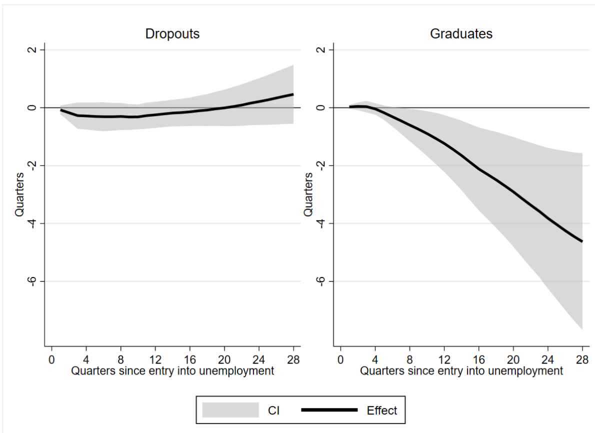
(c) Full-time equivalents



(d) High-paid employment



(e) 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 (100 for a full-time job in the quarter), (d) quarters in high-paid private sector jobs (earning above the median daily wage of €83.5), (e) quarters in self-, public, and cross-border 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- (post-) 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.2. Standard errors are clustered at the age level. N = 1,942 (dropouts) and 1,839 (graduates).

5.3 Cost-Benefit Analysis

In this final section, we implement a cost-benefit analysis of the subsidy reinforcement from the perspective of the government.[40] We, therefore, implement a similar donut RDD estimator, using as the outcome the average (cumulative) individual contribution to the net public revenues up to seven years after unemployment entry. This can be divided into three components: (i) contribution to the tax revenues (including Social Security contributions) from salaried private and public sector employment, (ii) expenditures due to hiring subsidies, and (iii) expenditures from unemployment benefits.[41]

Figure 14 displays the evolution of the donut RDD effect on the cumulative net public return of the subsidy with the age cutoff of 26 years, from entry into unemployment until seven years later. For dropouts, this effect is close to zero throughout, while for high school graduates the effect is close to zero during the first two years (roughly corresponding to the eligibility period of the subsidy) but increases and reaches €3,507 after seven years. The 95% confidence intervals always encompass zero, however.

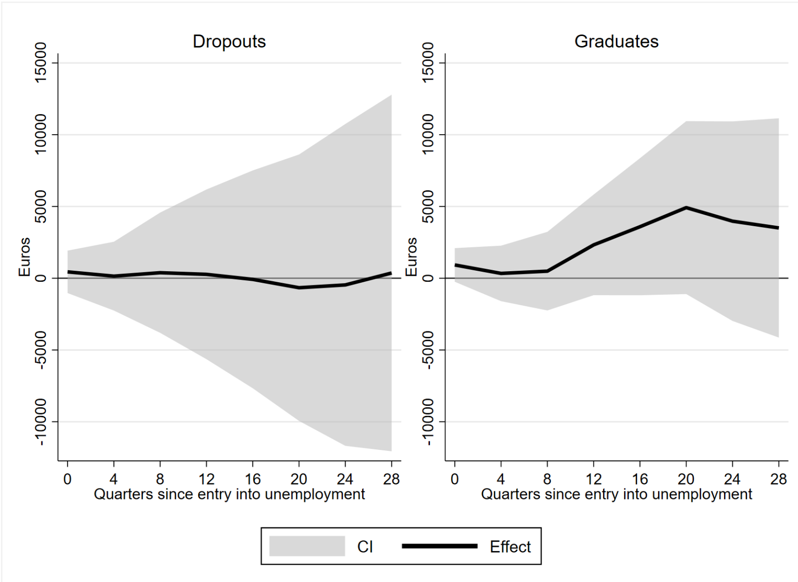
In Figure A.43 of Online Appendix A, we also report the evolution of the three components of the net benefit. Seven years after entry into unemployment, the decomposition is as follows for high school dropouts and graduates, respectively: €369 = €5,608 – €1,108 – €4,131 and €3,507 = €11,555 – €3,547 – €4,501. Notice that the subsidy expenditures are much lower for the dropouts than for the graduates because dropouts remain employed for a much shorter period than graduates. An unexpected result is that the hiring subsidy also increases the expenditure on unemployment benefits. This result may be explained by the fact that the level of the unemployment benefits depends on the employment history and the prior wage, which are enhanced by the subsidized employment period. Furthermore, since we are focusing on youths with little employment experience and only individuals who exceed some minimum experience threshold are entitled to unemployment benefits, only half of our sample claims

[40] Alternatively, we could have considered the perspective of society. However, this is beyond the scope of this paper as it would require an evaluation of the net value of created production as well as an estimate of the marginal cost of public funding to take into account the costs associated with distortive taxes used to finance the hiring subsidy. See Albanese and Cockx (2019) for an example of this perspective.

[41] Note the data do not allow us to calculate the tax losses induced by the negative impact on self-employment. This slightly overestimates the benefits.

benefits at entry into unemployment.[42]

Figure 14: Evolution of the RDD Effect on Cumulative Net Public Revenues (€)



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 net public revenues in euros 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 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.2. Standard errors are clustered at the age level. For dropouts (graduates), the effect at 7 years is €369 [–12,055; 12,790] with a p-value of 0.953 and N = 4,176 (€3,507 [–4,123; 11,145], p-value 0.363 and N = 4,384).

Overall, the hiring subsidy, therefore, did not seem to impose a cost on the government. For high school graduates, the subsidy seems to even generate a net long-run return to the government. This is induced by the long-run net increase in high-paid employment. Nevertheless, this conclusion requires corroboration because the confidence interval is very wide. The finding of a close-to-zero effect in the short run 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 net public cost of a hiring subsidy in the long run.

[42] In a sensitivity analysis, we also implement the DiD estimator and find that the outcomes are robust (see Figure A.44 in Online Appendix A).

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 and taking into account potential negative spillover effects. To study a novel geographic externality, the sample for analysis 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 (Barreca et al., 2016) 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. However, the subsidy generates persistent employment effects for high school graduates only. Seven years after entry into unemployment, high school graduates accumulated about three quarters more employment than in the counterfactual of eligibility for a substantially lower hiring subsidy. However, these long-run employment gains are found in the private sector only. When also taking into account jobs in the public sector and self-employment, the long-run employment gain is completely displaced by a corresponding decrease in employment in these other sectors. Nevertheless, because the remuneration is higher in the private than the public sector, the subsidy remains more effective for this group in the long run. The cost-benefit analysis reveals a net public cost close to zero. 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 employment in an area near the border.

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. The hiring subsidy also speeds up the transition to employment and results in higher pay in the long-run. We argue that these higher rates of pay stem from the enhanced hiring of graduates in large private firms, which are more conducive to human capital investment (Arellano-Bover, 2020, 2022).

These findings suggest that policymakers could further improve the efficiency of hiring subsidies by targeting them at firms with a proven record of effectively investing in young workers' skills, as proposed by Arellano-Bover (2022). Furthermore, for young workers who do not attain a minimum skill level, such as high school dropouts, hiring subsidies might better be preceded by a publicly supported classroom or on-the-job training program to first elevate skills to this minimum level. Nevertheless, we must be aware that negative spillovers across sectors and countries can weaken the intended effects, even in the long run. Finding an appropriate policy that can counter the long-term scarring effects of recessions on low-educated young people remains challenging but is certainly an important agenda for future research.

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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 (A) additional figures to the manuscript, (B) the description of the stratified sampling procedure, (C) additional descriptive statistics, (D) details on the Difference-in-differences estimator of Sant'Anna and Zaho (2020), (E) tables on the estimated treatment effects. Other estimation results are available from the authors upon request.

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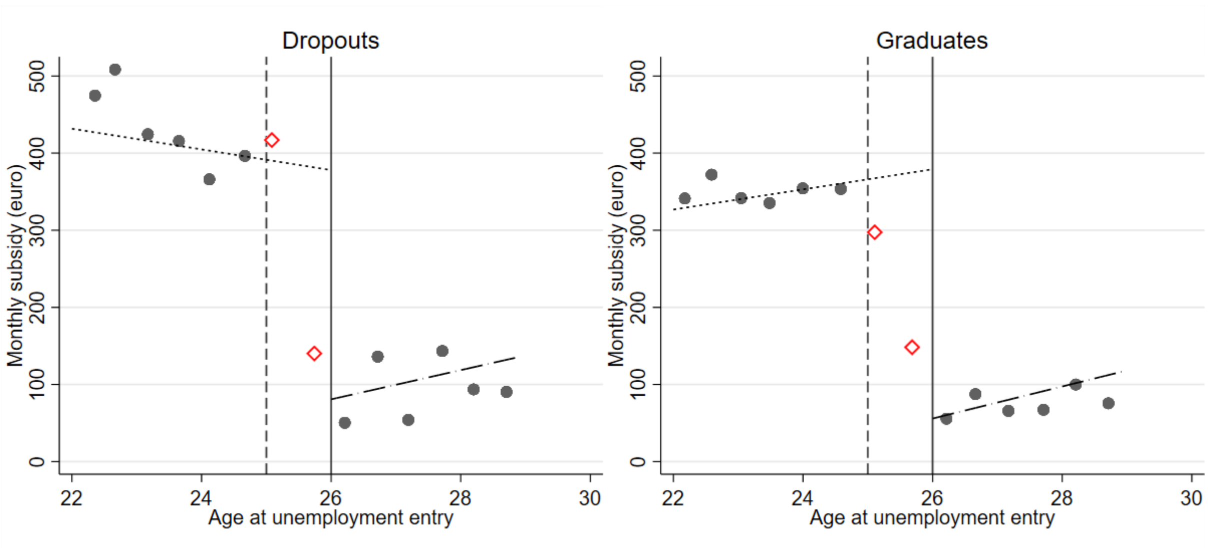
A Figures

Figure A.1: Google Search Index “Plan Win-Win”

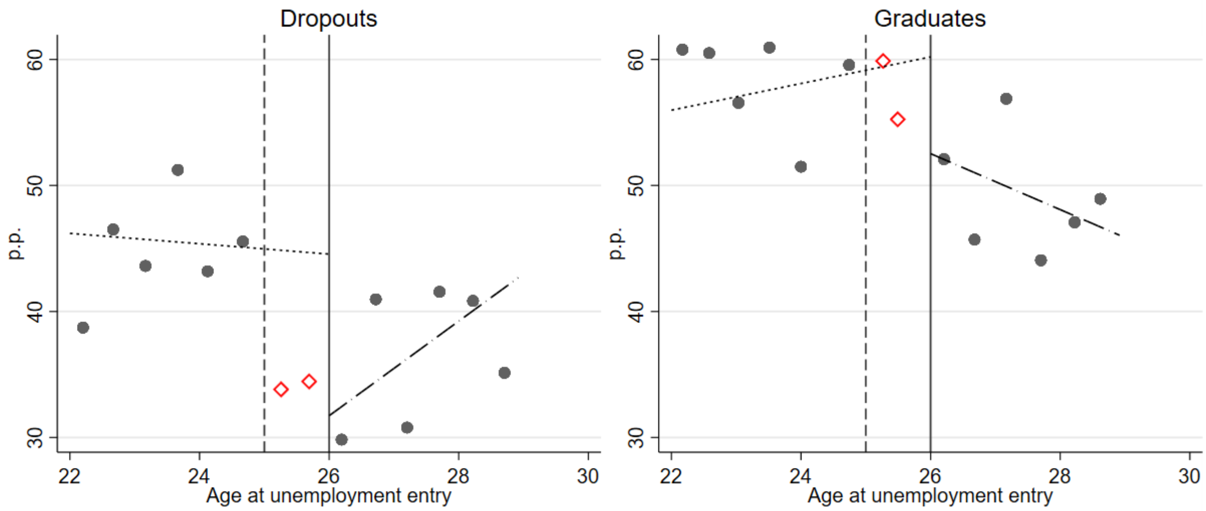
Google Trends search for “Plan Win-Win” from 1st of January 2009 until 31st of December 2010. The red line corresponds to the week before the 18th of January 2010.

Figure A.2: Discontinuity at Age 26 of the Average Subsidy Amount Conditional on Employment Within One Year by Schooling Level



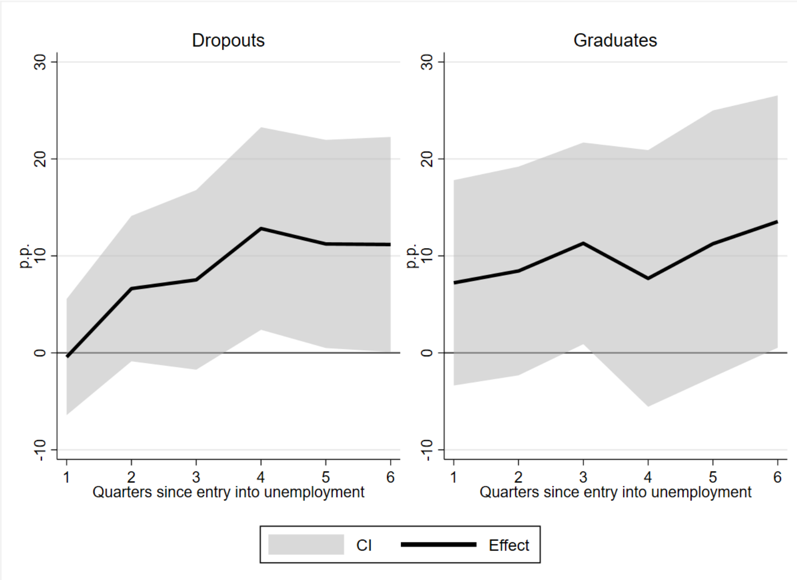
Note: Donut RDD estimate on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. The bandwidth is set at 3 years on each side of the donut. The outcome is the amount of received subsidy (in full-time equivalent) conditional 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.2. Standard errors are clustered at the age level. The effect estimated by the donut RDD estimator at 26 years of age is +297 euros [60; 535] with a p-value of 0.015 and N = 1,615 for dropouts, while for graduates it is +323 euros [181; 466] with a p-value of 0.000 and N = 2,343.

Figure A.3: Discontinuity at Age 26 of the Cumulative Transition Rate to Private Sector Employment Within One Year by Schooling Level



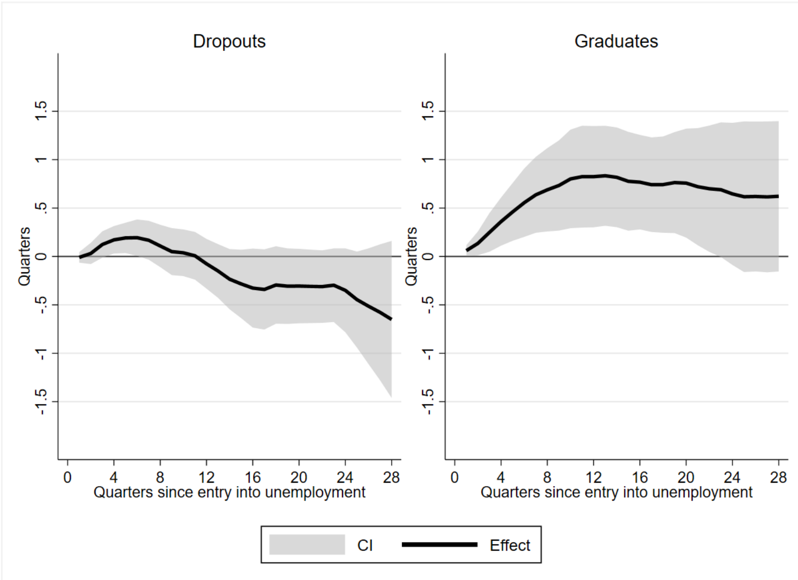
Note: Donut RDD estimate on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. The bandwidth is set at 3 years on each side of the donut. The outcome is the cumulative transition rate to private sector employment within 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.2. 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.4: Evolution of the RDD Effect on the Transition Rate to the Private Sector Up to 6 Quarters After Entry into Unemployment



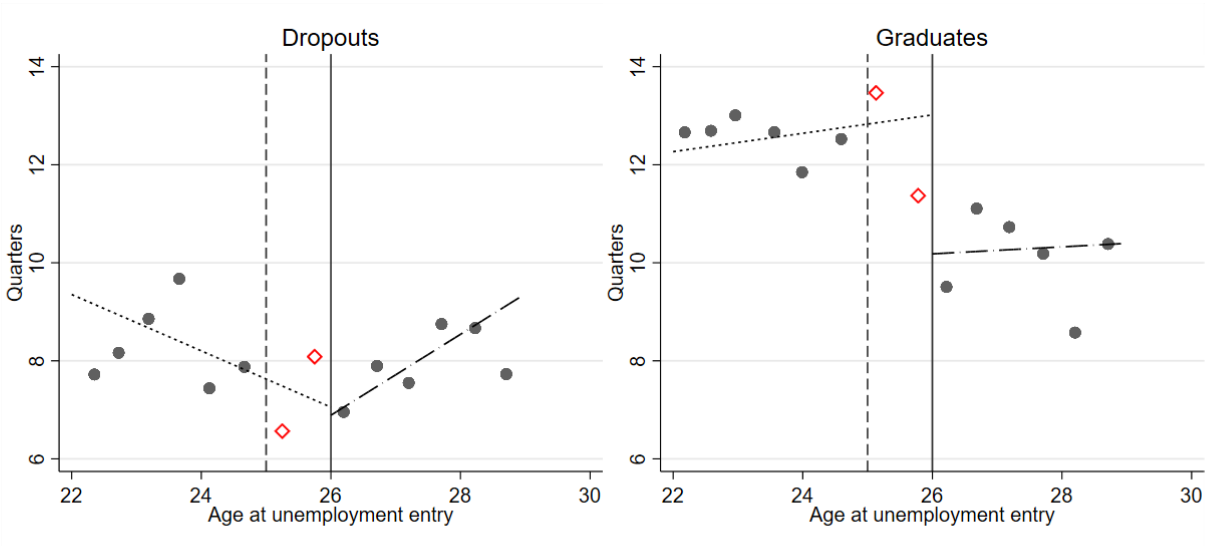
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the transition rate to the private sector up to 6 quarters after entry into unemployment by schooling level: dropouts (left) vs. graduates (right). The RDD estimator is implemented for each quarter after entry into unemployment until 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.2. 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.5: DiD Effect on the Cumulative Number of Quarters in Subsidized Private-Sector Employment



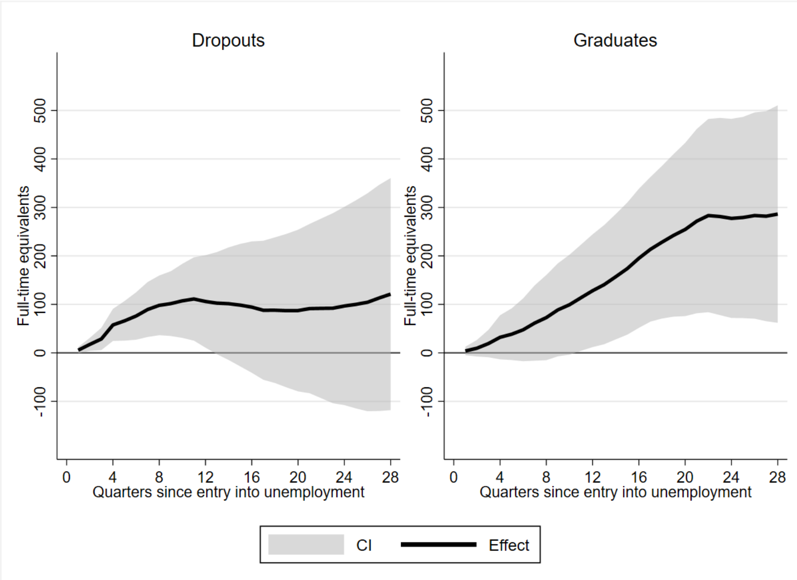
Note: Evolution of the effect estimated with a doubly robust DiD estimator (Sant'Anna and Zhao, 2020) and confidence interval (CI) for the cumulative number of quarters in subsidized private-sector employment by schooling level: dropouts (left) vs. graduates (right). The DiD estimator is implemented 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(post)-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.2. 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.6: Discontinuity at Age 26 for the Cumulative Number of Quarters in Private Sector Employment Seven Years after Entry into Unemployment



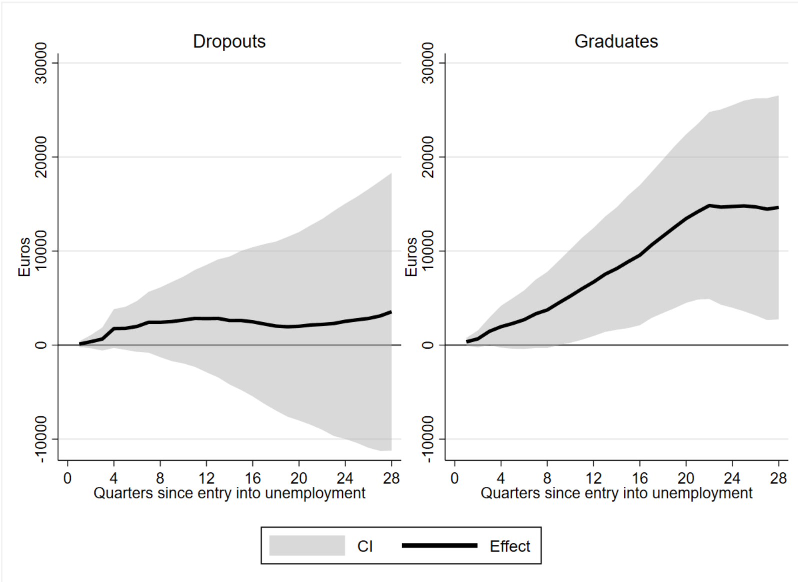
Note: Donut RDD estimate on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. The bandwidth is set at 3 years on each side of the donut. The outcome is the cumulative number of quarters employed in private sector employment 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.2. 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] with a p-value of 0.897 and N = 4,176 for dropouts, while for graduates it is +2.8 quarters [0.7; 5.0] with a p-value of 0.011 and N = 4,384.

Figure A.7: Evolution of the RDD Effect on the Cumulative Percentage of Full-Time-Equivalent Quarters in Private Sector Employment



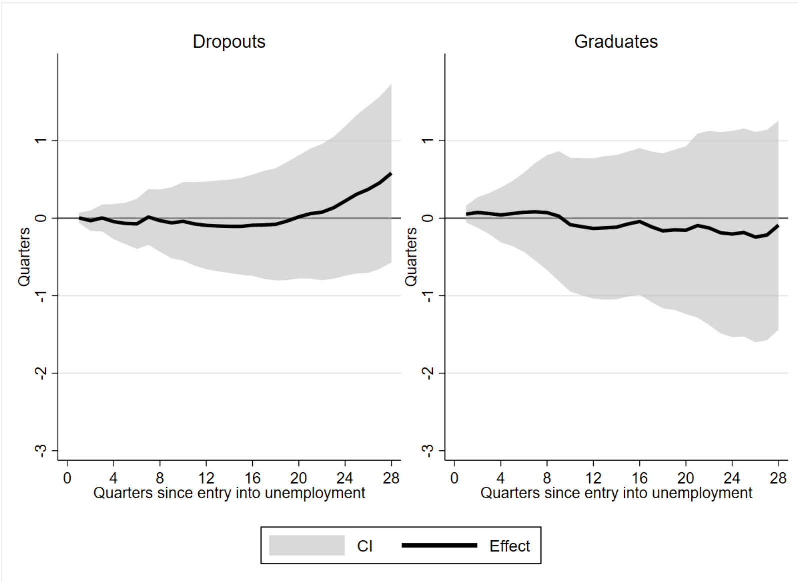
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the cumulative number of full-time-equivalents (100 for a full-time job in 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.2. 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.8: Evolution of the RDD Effect on the Cumulative Gross Earnings 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 earning 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.2. 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).

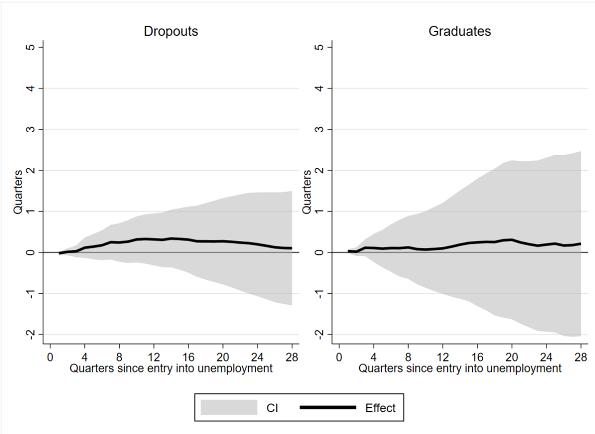
Figure A.9: Evolution of the RDD Effect on the Number of Quarters in a Private Sector Job with Earnings below the Median Daily Wage



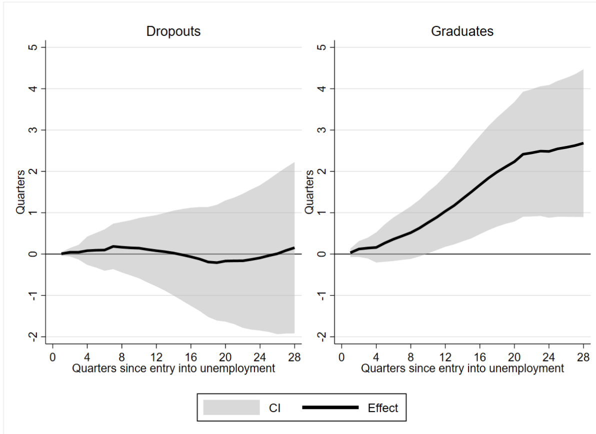
Note: Donut RDD estimates on the inflow sample of youth entering unemployment in 2010, using age at unemployment entry as the forcing variable with cutoff at 26. Evolution of the RDD effect and confidence intervals (CI) on the cumulative number of quarters in the private sector employment paying less than the median daily wage (€83.5) 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.2. Standard errors are clustered at the age level. For the dropouts (graduates) the effect at 7-year distance is 0.6 quarters [–0.6; 1.7] with a p-value of 0.318 and N = 4,176 (–0.1 quarters [–1.4; 1.3], p-value 0.894 and N = 4,384).

Figure A.10: Evolution of the RDD and DiD Effect on Accumulated Quarters of Employment by Firm Size

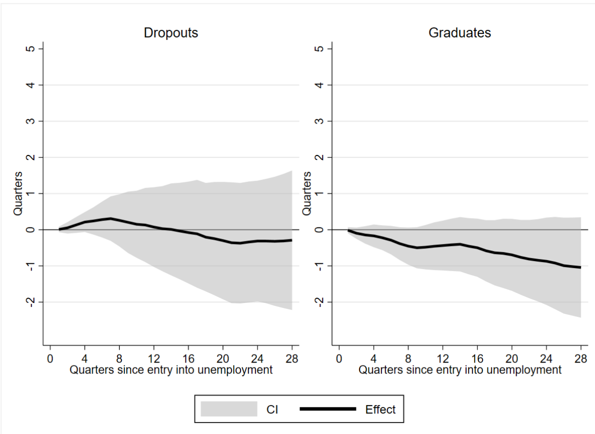
(a) RDD – smaller firms



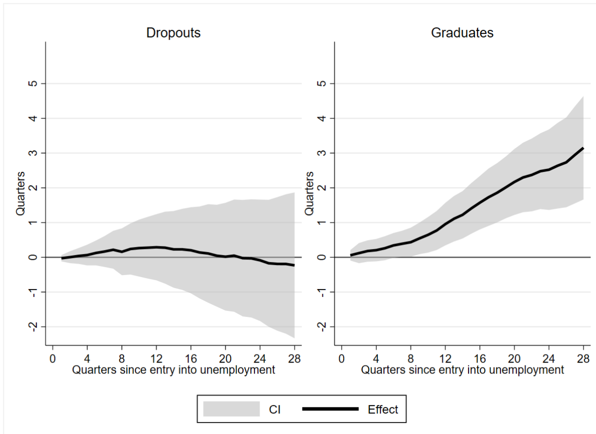
(b) RDD – larger firms



(c) DiD – smaller firms

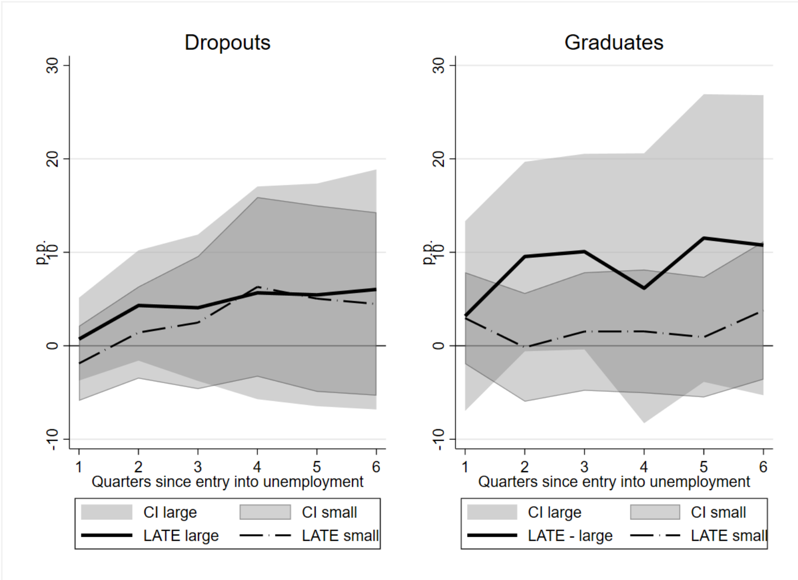


(d) DiD – larger firms



Note: Donut RDD (above) and doubly robust DiD (Sant'Anna and Zhao, 2020) estimates (below) on the inflow sample of youths entering unemployment in 2010. Evolution of the effects and confidence interval (CI) for the cumulative number of quarters in a firm with less (left) or more (right) than 50 employees by schooling level: dropouts (left columns) vs. graduates (right columns). The estimators are implemented for each quarter after entry into unemployment until 7 years later. The RDD estimator uses age at entry as the forcing variable, with a cutoff at 26. 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. 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(post)-treatment period. We control for the set of control variables shown in Table C.2 in Online Appendix C.2. Standard errors are clustered at the age level. RDD: For dropouts (graduates), the effect at 7 years on the number of quarters in a smaller firm is 0.1 quarters [–1.3; 1.5] with a p-value of 0.882 and N = 4,176 (0.2 quarters [–2.0; 2.5], p-value 0.851 and N = 4,384); the effect at 7 years on the number of quarters in a larger firm is 0.1 quarters [–1.9; 2.2] with a p-value of 0.883 and N = 4,176 (+2.7 quarters [0.9; 4.5], p-value 0.004 and N = 4,384). DiD: For dropouts (graduates), the effect at 7 years on the number of quarters in a smaller firm is –0.3 quarters [–2.2; 1.6] with a p-value of 0.768 and N = 1,942 (–1.0 quarters [–2.4; 0.3], p-value 0.140 and N = 1,839); the effect at 7 years on the number of quarters in a larger firm is –0.2 quarters [–2.3; 1.9] with a p-value of 0.829 and N = 1,942 (3.15 quarters [1.6; 4.6], p-value 0.000 and N = 1,839).

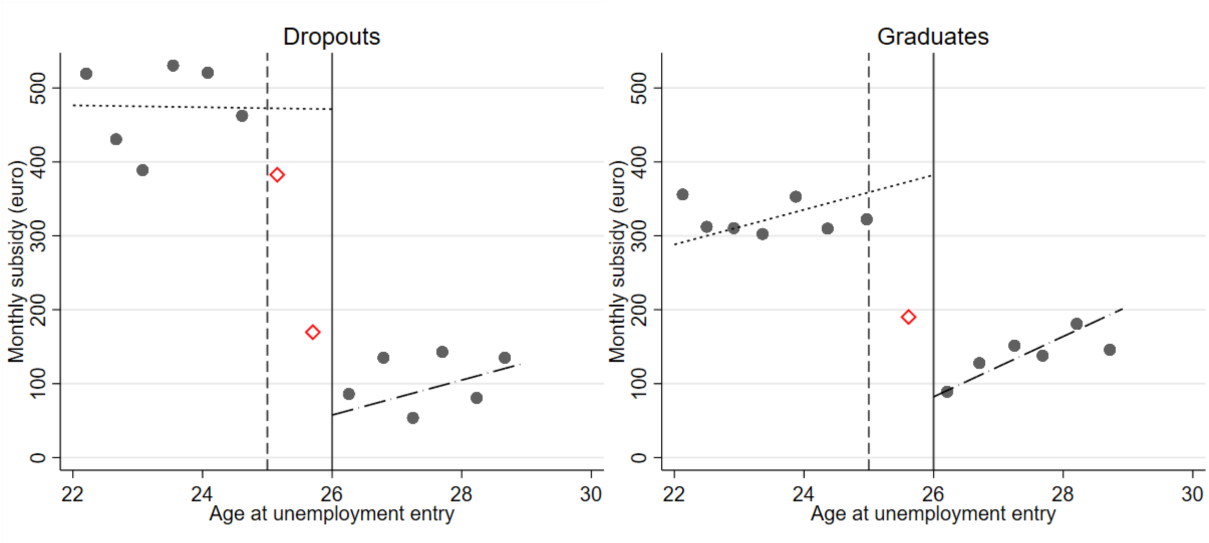
Figure A.11: Evolution of the RDD Effect on Entry in a Larger or Smaller Firm



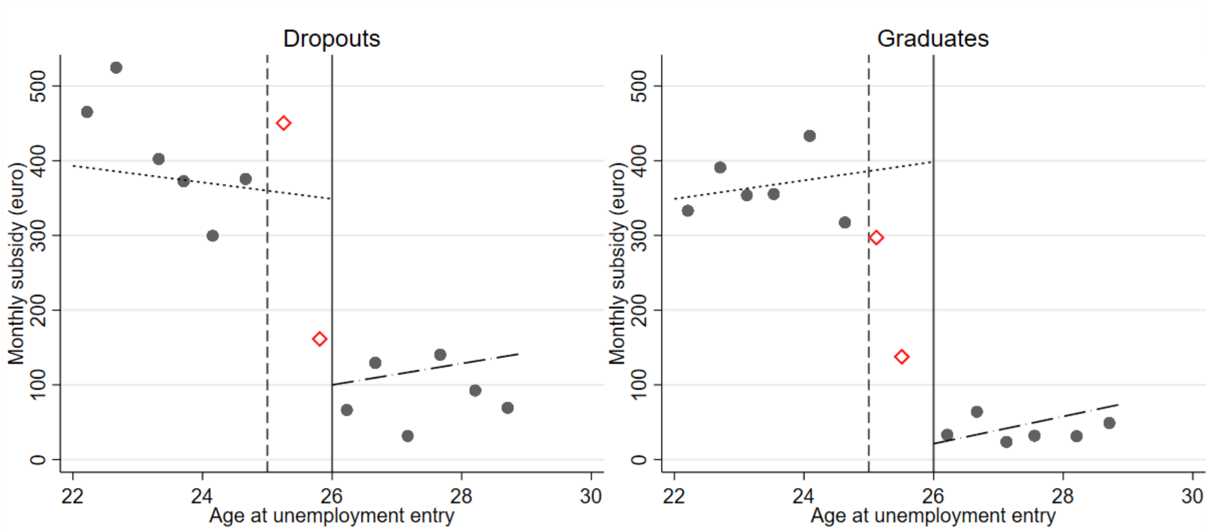
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the cumulative number of quarters in a firm with more (full line) or fewer (dashed line) than 50 employees 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.2. Standard errors are clustered at the age level. For dropouts, the effect at 5 quarters on smaller (larger) firms is 5.0 (5.5) quarters [–5.0; 15.0] ([–6.5; 17.3]) with a p-value of 0.316 (0.365) and N = 4,176. For graduates, the effect at 5 quarters on smaller (larger) firms is 11.5 (0.9) quarters [–3.9; 26.9] ([–5.5; 7.4]) with a p-value of 0.140 (0.775) and N = 4,384.

Figure A.12: 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

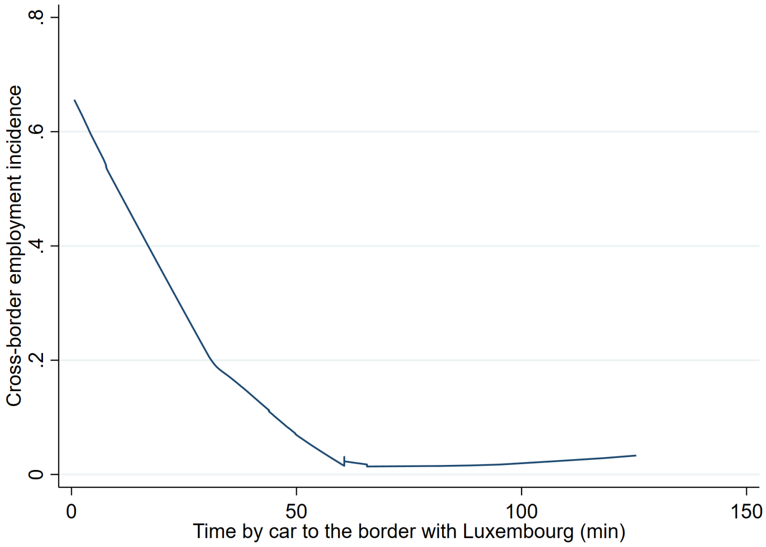


(b) Far



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 bandwidth is set at 3 years on each side of the donut. 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.2. 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 euros [235; 592] with a p-value of 0.000 and N = 597, while for graduates it is 300 euros [197; 404] with a p-value of 0.000 and N = 1,939. For dropouts living far from the border, it is 249 euros [–47; 545] with a p-value of 0.099 and N = 978, while for graduates it is 377 euro and [194; 561] with a p-value of 0.006 and N = 1,023.

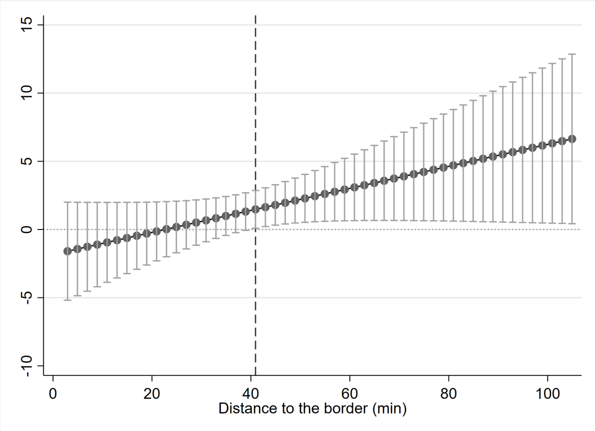
Figure A.13: Share of Workers Working Abroad Over Distance to the Border With Luxembourg (lowess)



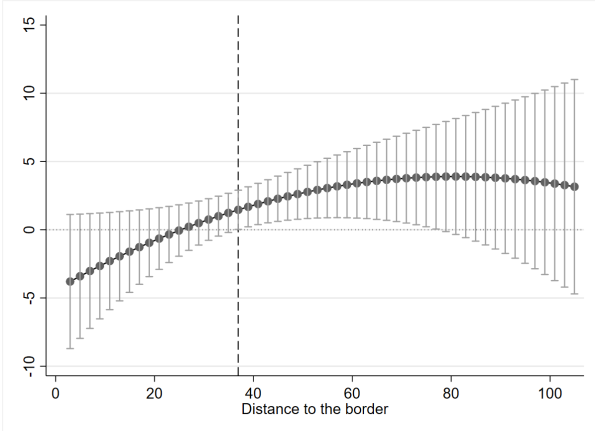
Note: Lowess smoothing (running-line least squares) for the share of workers working abroad (any country) according to minutes by car during rush hour (TomTom data) to the border with Luxembourg. This is calculated over the original full sample of 125,000 observations during the 4th quarter of 2009,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

Figure A.14: 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



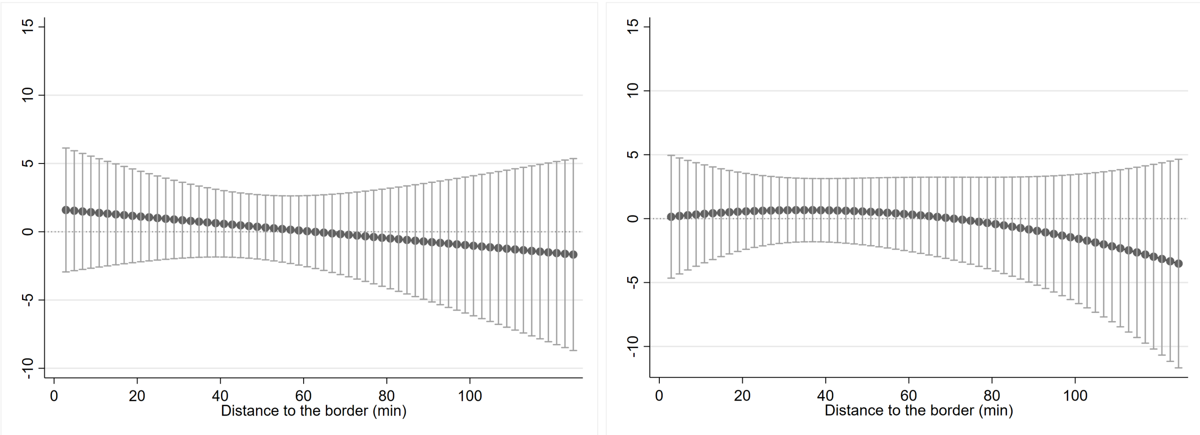
(b) Quadratic



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 bandwidth is set at 3 years on each side of the donut. 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.2. Standard errors are clustered at the age level. 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 dashed line shows the distance when the effect starts to be statistically significant. N = 4,176 (dropouts) and 4,384 (graduates).

Figure A.15: 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

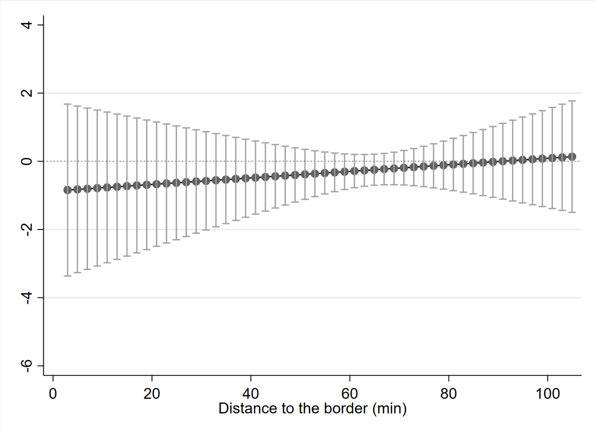
(a) Linear (b) Quadratic



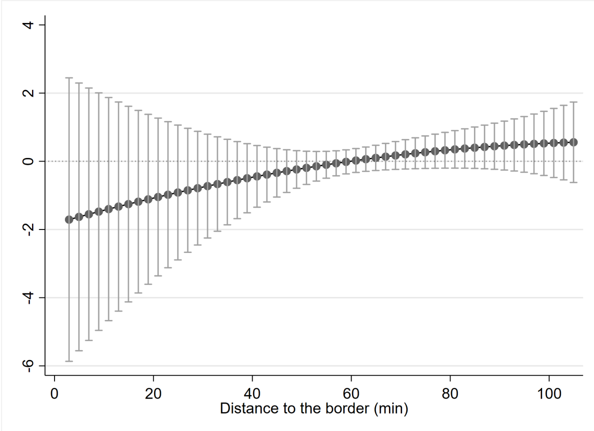
Note: Donut RDD estimate on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. The bandwidth is set at 3 years on each side of the donut. The predicted effect over the distance to the border from Luxembourg is obtained 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.2. Standard errors are clustered at the age level. 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). N = 4,176 (dropouts) and 4,384 (graduates).

Figure A.16: 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

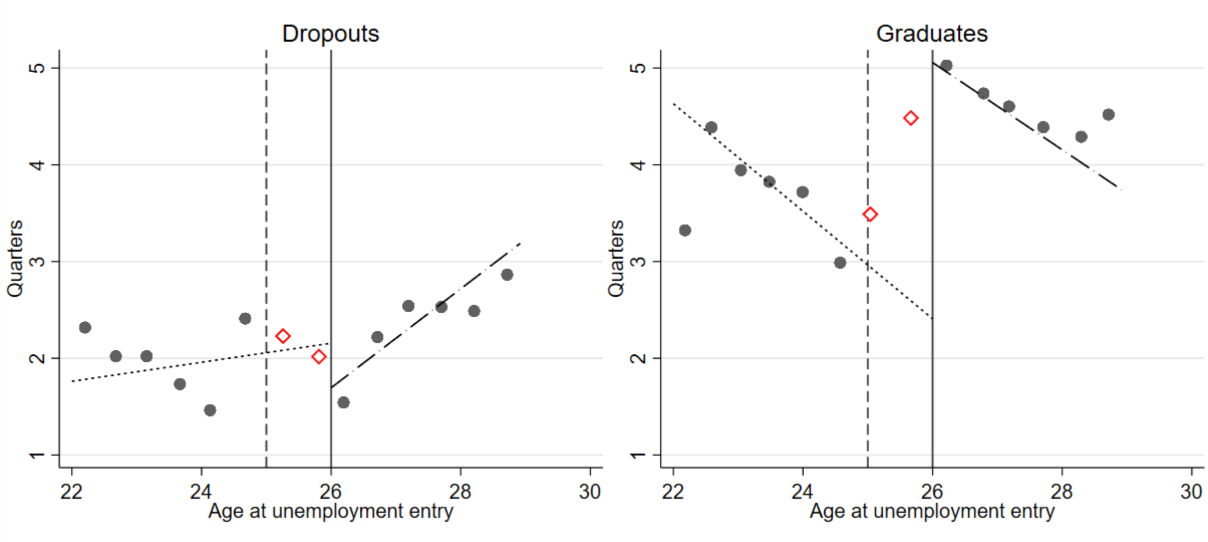


(b) Quadratic



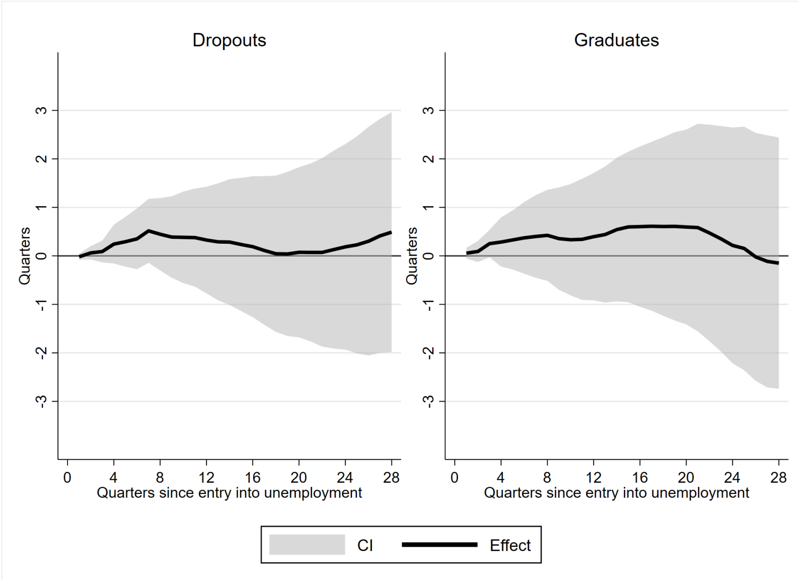
Note: Donut RDD estimate on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. The bandwidth is set at 3 years on each side of the donut. The predicted effect over the distance to the border from Luxembourg is obtained 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.2. Standard errors are clustered at the age level. 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). N = 4,176 (dropouts) and 4,384 (graduates).

Figure A.17: Donut RDD Plot of the Effect on the Cumulative Number of Quarters in Non-Private Sector Employment 7 Years After Entry into Unemployment



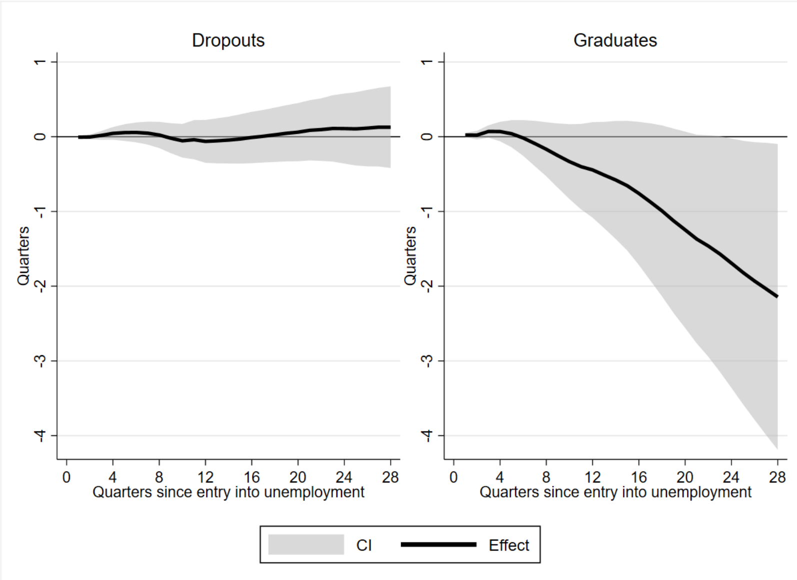
Note: Donut RDD estimate on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. The bandwidth is set at 3 years on each side of the donut. The outcome is the effect on the cumulative number of quarters in non-private sector employment 7 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.2. 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] with a p-value of 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 of 0.012 and N = 4,384.

Figure A.18: Evolution of the RDD Effect on the Cumulative Number of Quarters in Any Employment



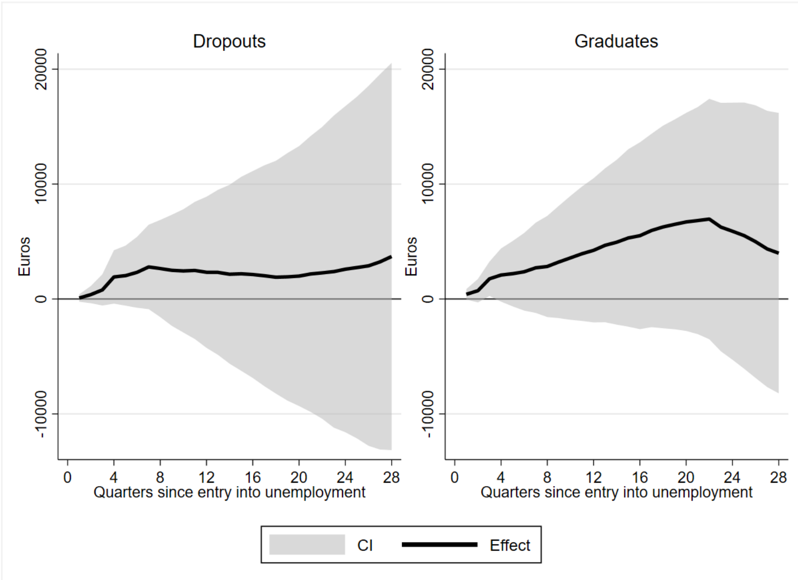
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the cumulative number of quarters in any employment by schooling level: dropouts (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.2. Standard errors are clustered at the age level. For dropouts (graduates), the effect at 7 years is 0.5 quarters [–2,0; 3.0] with a p-value of 0.733 and N = 4,176 (–0.1 quarters [–2.7; 2.5], p-value 0.931 and N = 4,384).

Figure A.19: Evolution of the RDD Effect on the Cumulative Number of Quarters in Public Sector Employment



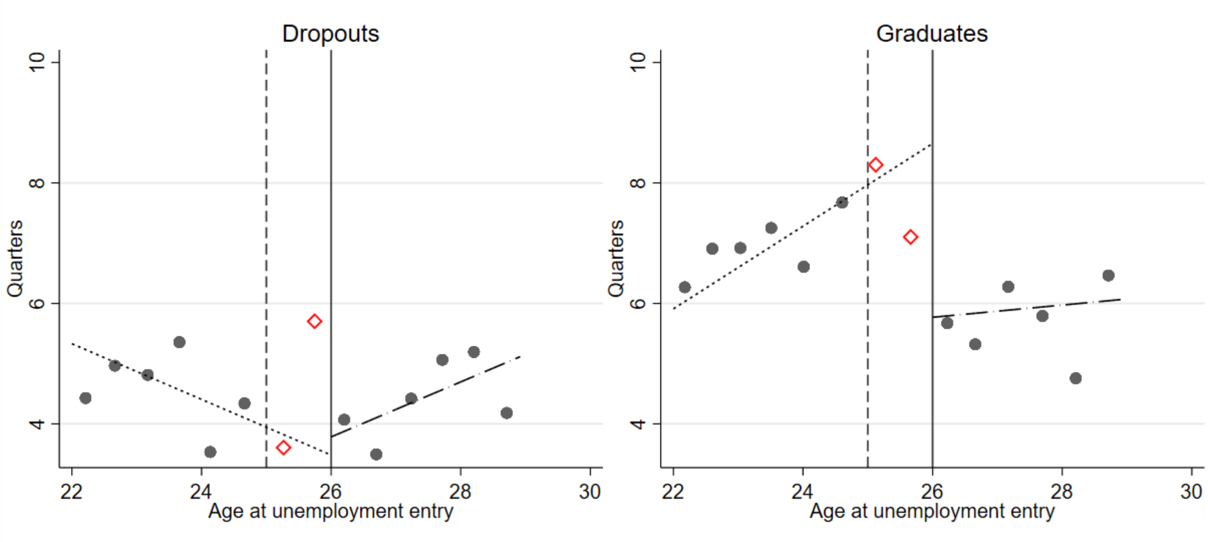
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the cumulative number of quarters in a public sector firm by schooling level: 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.2. 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.20: Evolution of the RDD Effect on Cumulative Earnings in the Private or Public Sector



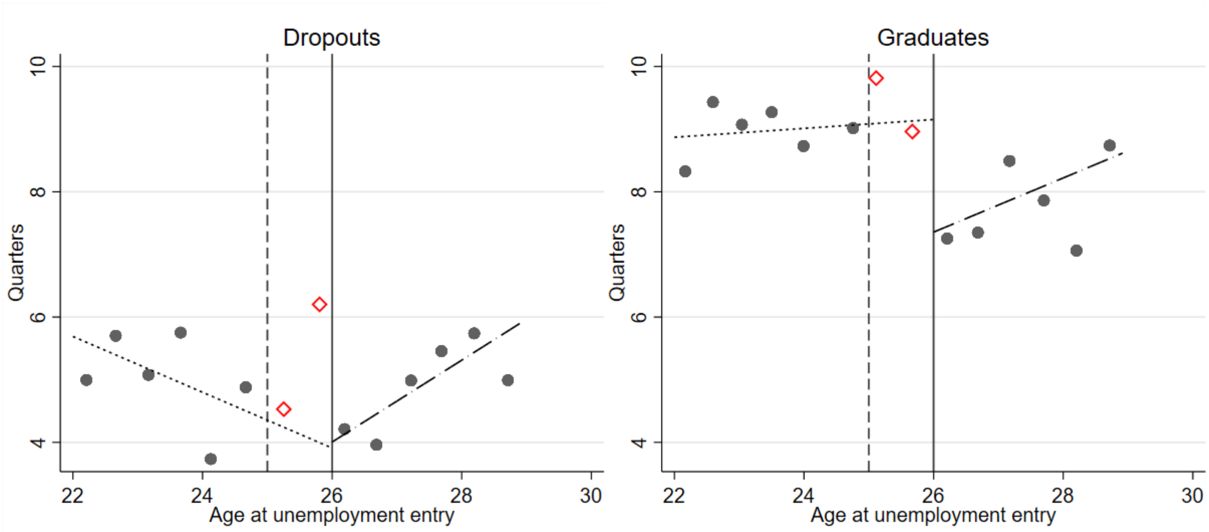
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the cumulative gross earnings in a private or public sector firm by schooling 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.2. Standard errors are clustered at the age level. For dropouts (graduates), the effect at 7 years is €3,701 [–13,142; 20,545] with a p-value of 0.663 and N = 4,176 (€3,989 [–8,211; 16,190], p-value 0.516 and N = 4,384).

Figure A.21: Discontinuity at Age 26 of the Cumulative Number of Quarters in a Private Sector Job Paying More Than the Median Daily Wage, 7 Years After Entry Into Unemployment



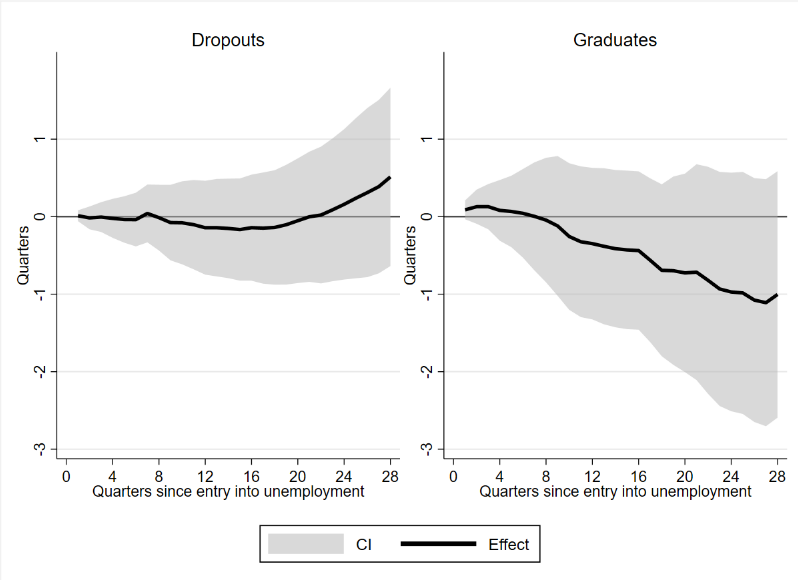
Note: Donut RDD estimate on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. The bandwidth is set at 3 years on each side of the donut. The outcome is the effect on the cumulative number of quarters in a private sector job paying more than the median daily wage (€83.5) 7 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.2. Standard errors are clustered at the age level. The effect estimated by the donut RDD estimator at 26 years of age is –0.3 quarters [–2.2; 1.6] with a p-value of 0.758 and N = 4,176 for dropouts, while for graduates it is +2.9 quarters [1.4; 4.3] with a p-value of 0.000 and N = 4,384.

Figure A.22: Discontinuity at Age 26 of the Cumulative Number of Quarters in a Public or Private Sector Job Paying More Than the Median Daily Wage, 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 bandwidth is set at 3 years on each side of the donut. The outcome is the effect on the cumulative number of quarters in a public or private sector job paying more than the median daily wage (€84.1) 7 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.2. Standard errors are clustered at the age level. The effect estimated by the donut RDD estimator at 26 years of age is –0.1 quarters [–2.3; 2.1] with a p-value of 0.930 and N = 4,176 for dropouts, while for graduates it is +1.8 quarters [0.4; 3.2] with a p-value of 0.010 and N = 4,384.

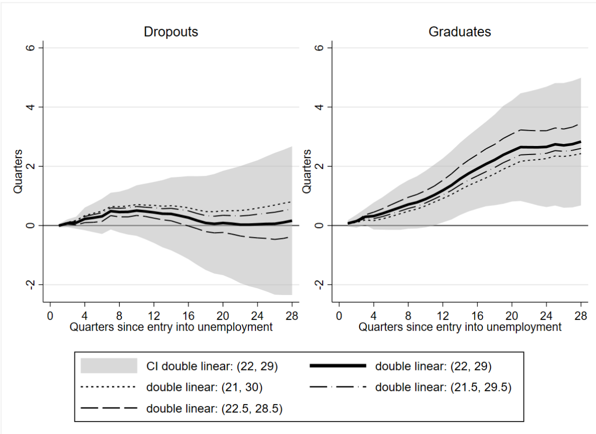
Figure A.23: Evolution of the RDD Effect on the Cumulative Number of Quarters in a Public or Private Sector Job Paying Less Than the Median Daily Wage



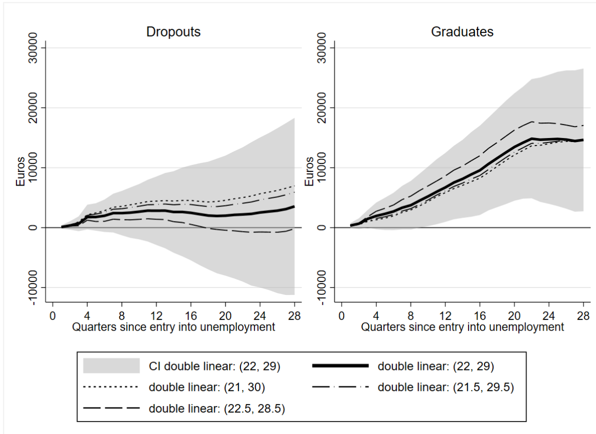
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the cumulative number of quarters in public or private sector employment paying less than the median daily wage (€84.1) 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.2. Standard errors are clustered at the age level. For dropouts (graduates), the effect at 7 years on low-paying employment is 0.5 quarters [–0.6; 1.7] with a p-value of 0.376 and N = 4,176 (–1.0 quarters [–2.6; 0.6], p-value 0.212 and N = 4,384).

Figure A.24: Evolution of the RDD Effect on Cumulative Outcomes: Changing the Bandwidth

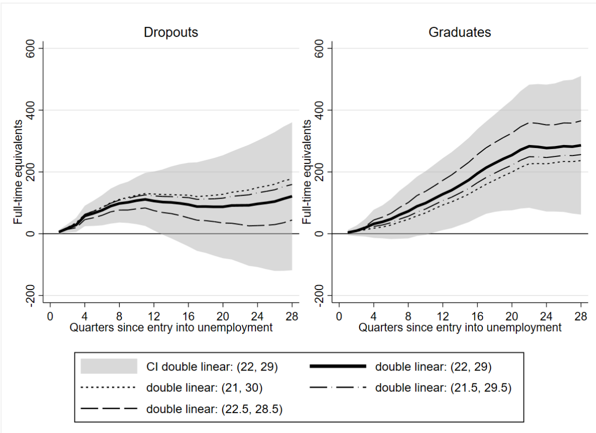
(a) Private sector employment



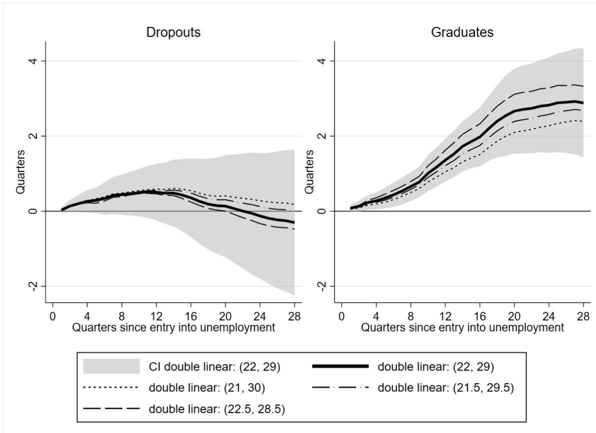
(b) Gross earnings



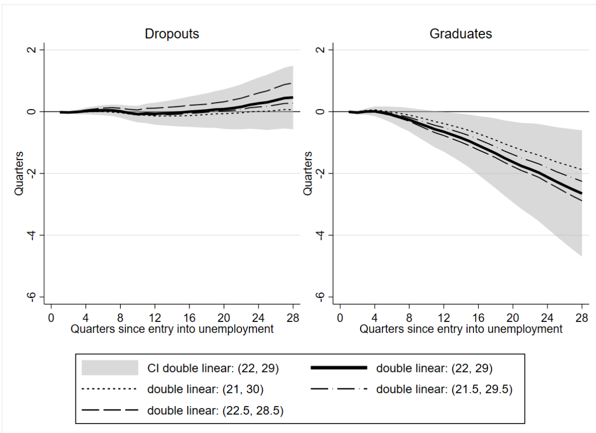
(c) Full-time equivalents



(d) High-paid employment



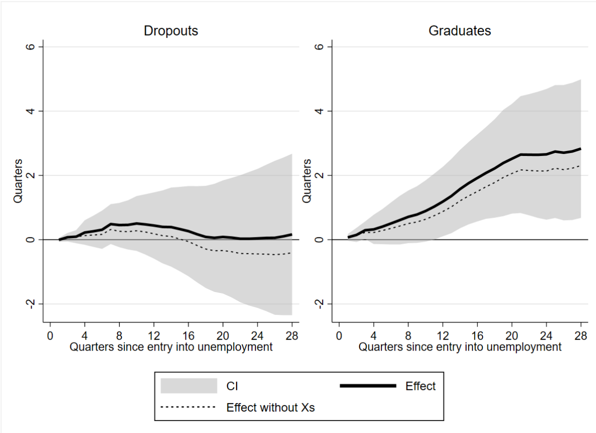
(e) Other employment



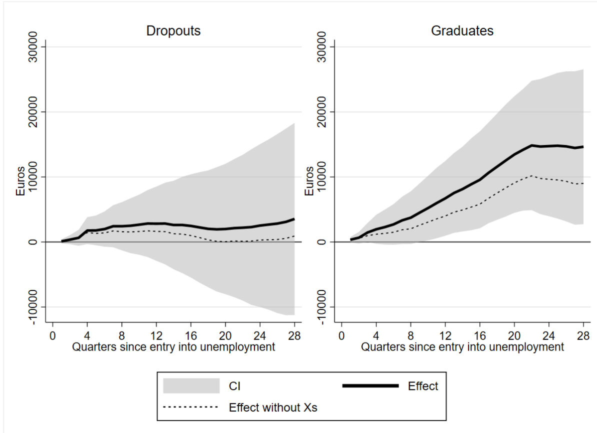
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence intervals (CI) for (a) the cumulative number of quarters in private sector employment, (b) gross remuneration in private sector employment, (c) full-time equivalents (100 for a full-time job in the quarter), (d) quarters in high-paid private sector jobs (earning above the median daily wage of €83.5), (e) quarters in self-, public, and cross-border 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.2. Standard errors are clustered at the age level.

Figure A.25: Evolution of the RDD Effect on Cumulative Outcomes: Removing the Xs

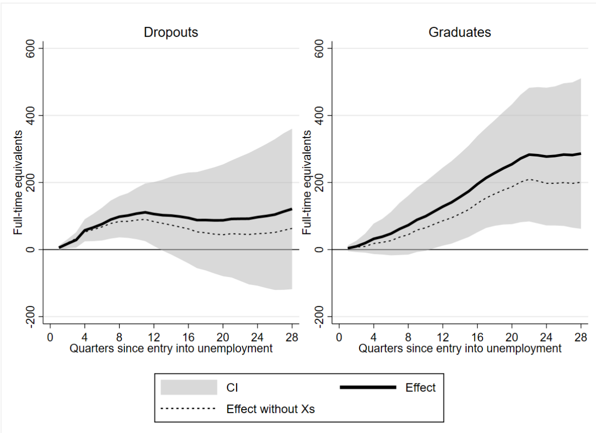
(a) Private sector employment



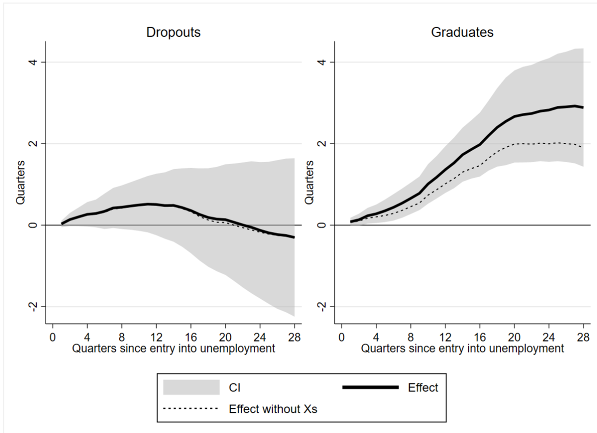
(b) Gross earnings



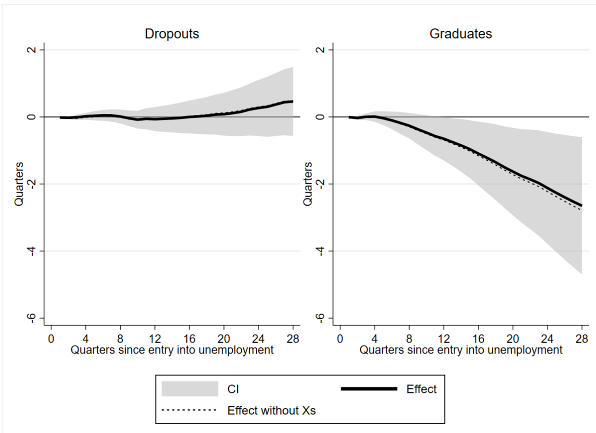
(c) Full-time equivalents



(d) High-paid employment

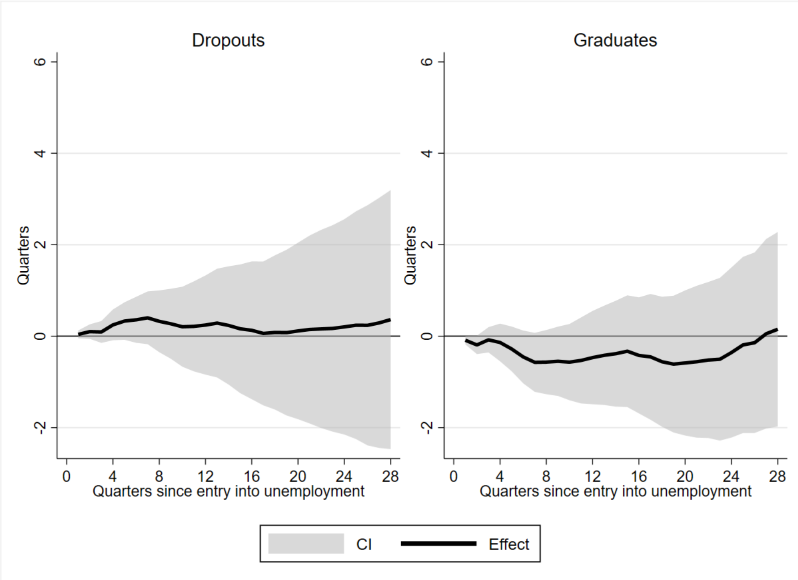


(e) Other employment



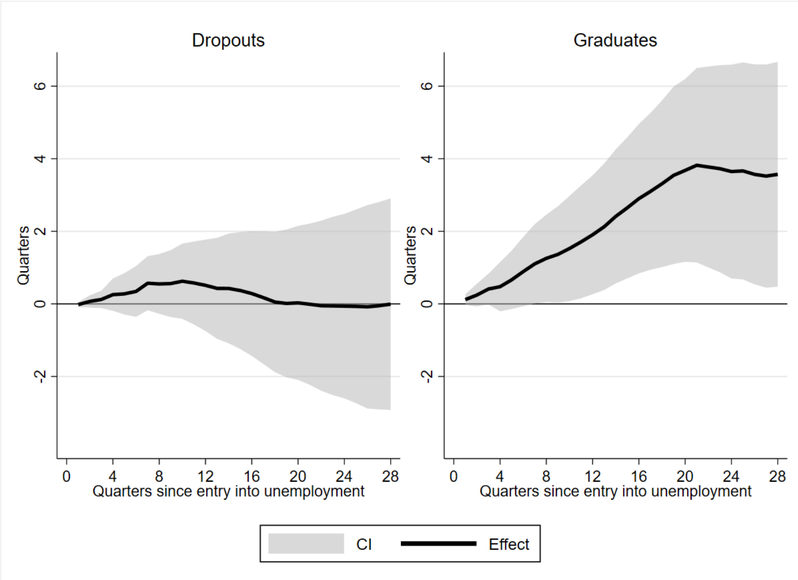
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence intervals (CI) for (a) the cumulative number of quarters in private sector employment, (b) gross remuneration in private sector employment, (c) full-time equivalents (100 for a full-time job in the quarter), (d) quarters in high-paid private sector jobs (earning above the median daily wage of €83.5), (e) quarters in self-, public, and cross-border 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.2. 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.26: Evolution of the RDD Effect on the Cumulative Number of Quarters in Private Sector Employment – Close to the Border (less than 45 minutes)



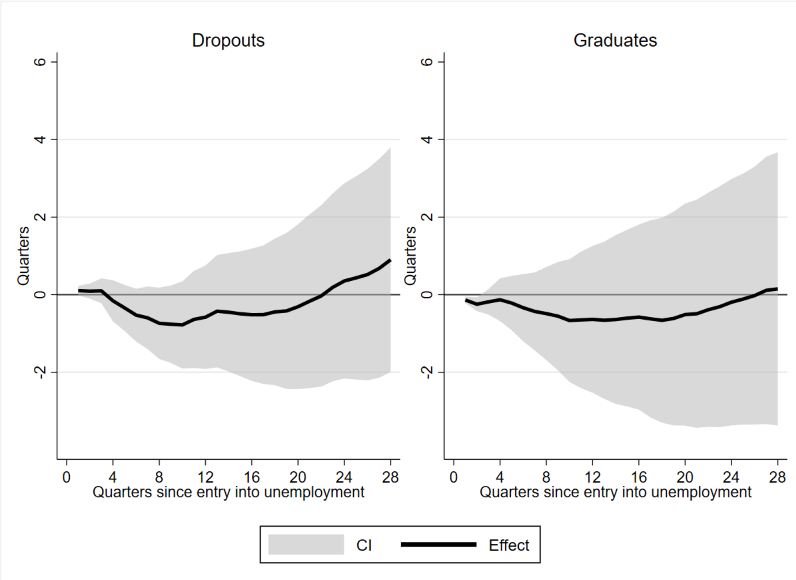
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the cumulative number of quarters in private sector employment since entry 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.2. 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.27: Evolution of the RDD Effect on the Cumulative Number of Quarters in Private Sector Employment – Far From the Border (more than 45 minutes)



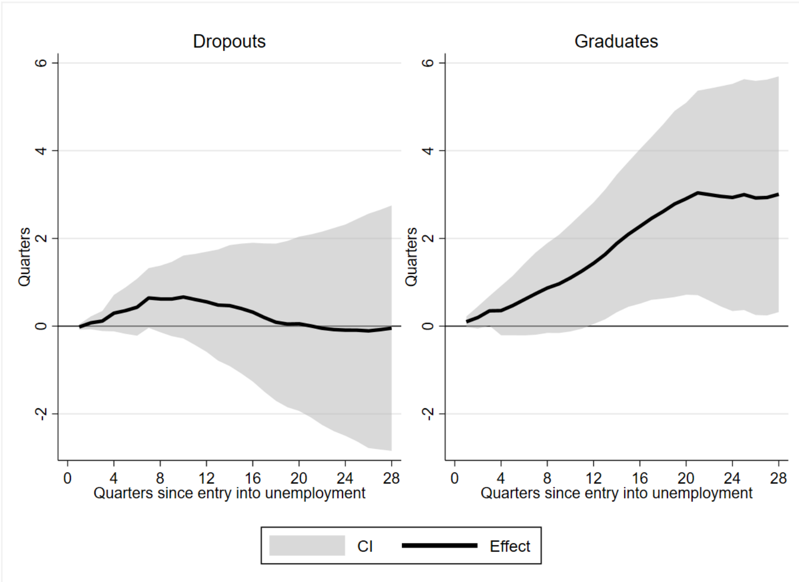
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the cumulative number of quarters in private sector employment since entry 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.2. 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.28: 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.2. 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).

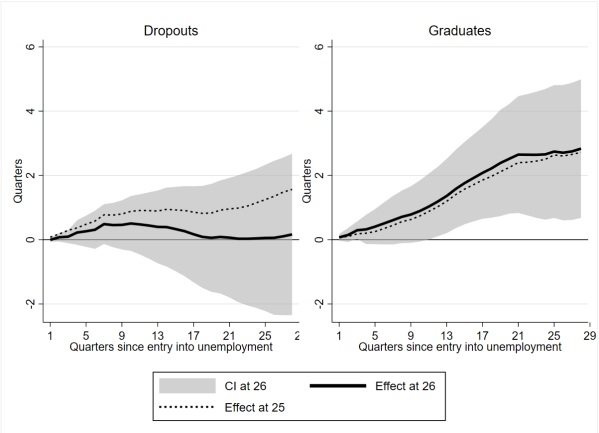
Figure A.29: Evolution of the RDD Effect on the Cumulative Number of Quarters in Private Sector Employment – Far From the Border (more than 30 minutes)



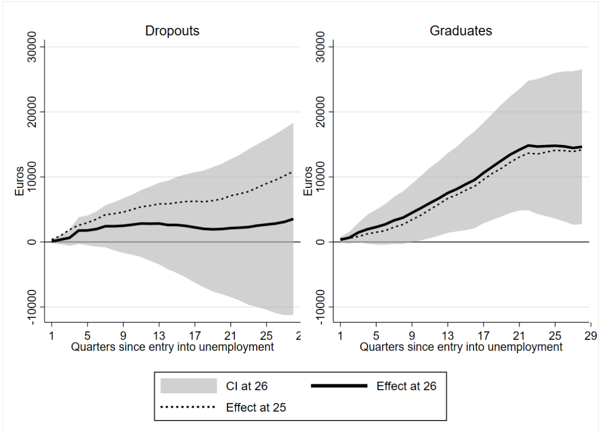
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the cumulative number of quarters in private sector employment since entry 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.2. 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.30: Evolution of the RDD Effect on Cumulative Outcomes: Effect at age 25

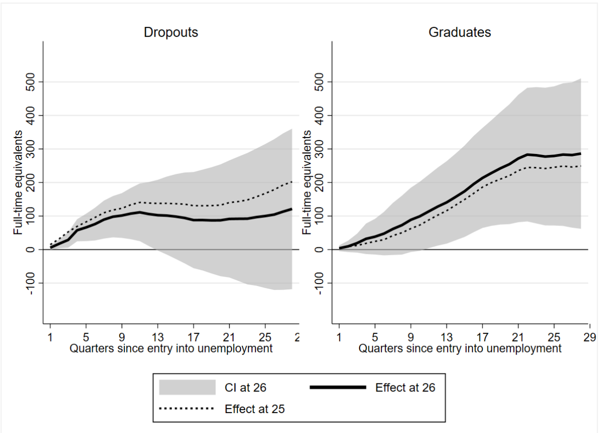
(a) Private sector employment



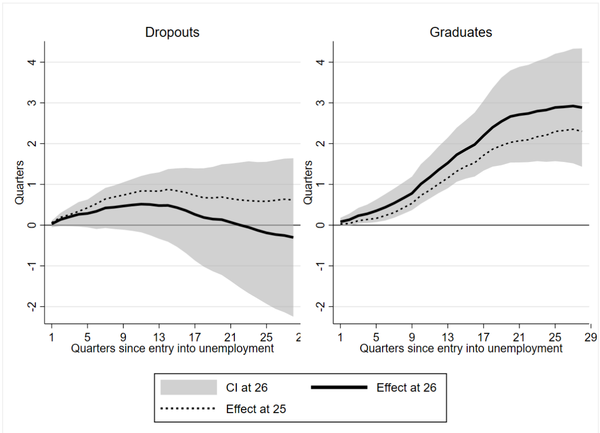
(b) Gross earnings



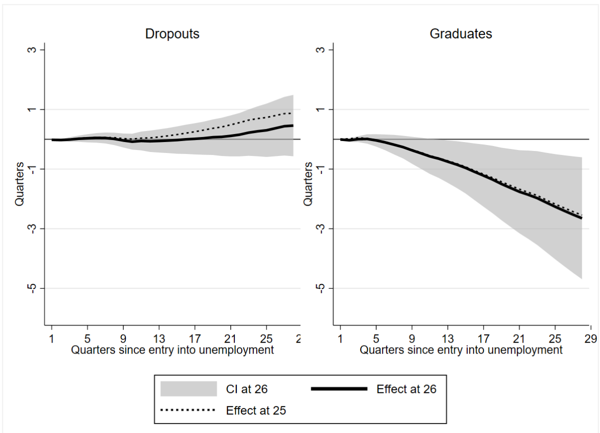
(c) Full-time equivalents



(d) High-paid employment



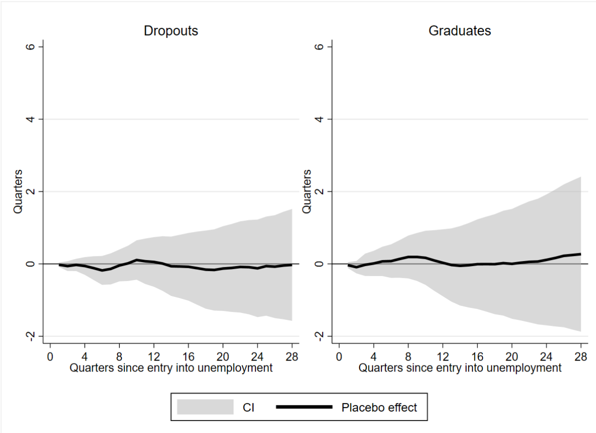
(e) Other employment



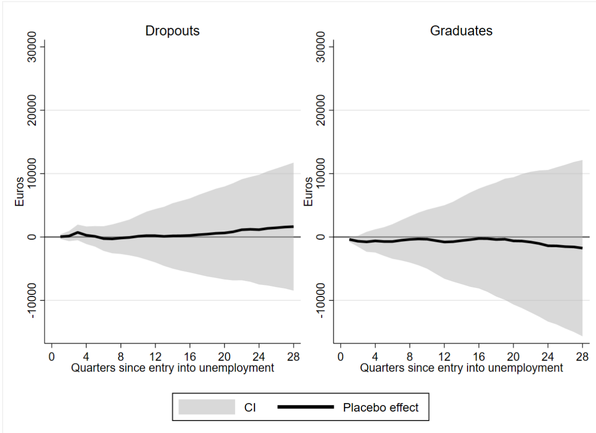
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26 (full line and CI) or 25 (dashed line). Evolution of the RDD effect and confidence intervals (CI) for (a) the cumulative number of quarters in private sector employment, (b) gross remuneration in private sector employment, (c) full-time equivalents (100 for a full-time job in the quarter), (d) quarters in high-paid private sector jobs (earning above the median daily wage of €83.5), (e) quarters in self-, public, and cross-border 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). 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.2. Standard errors are clustered at the age level.

Figure A.31: Evolution of the RDD Effect on Cumulative Outcomes: Placebo in 2008

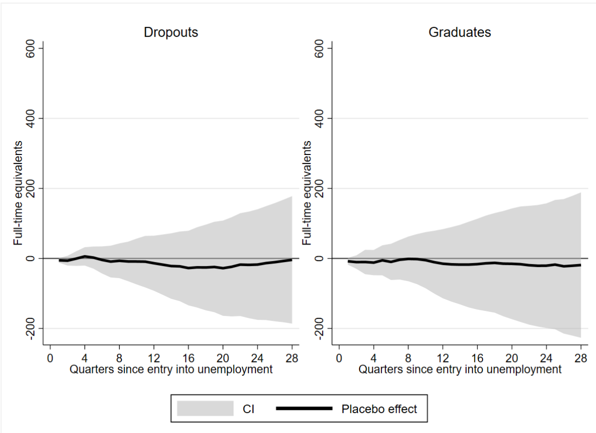
(a) Private sector employment



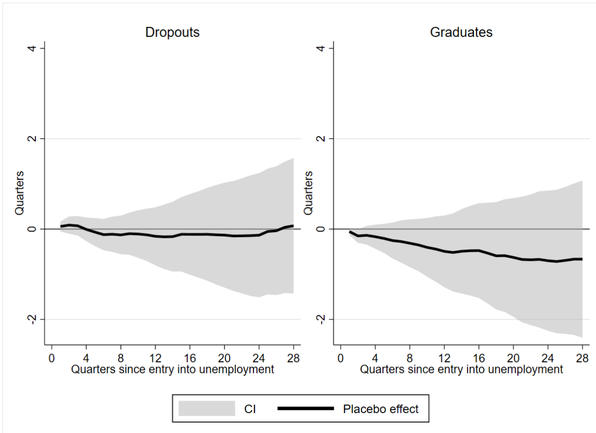
(b) Gross earnings



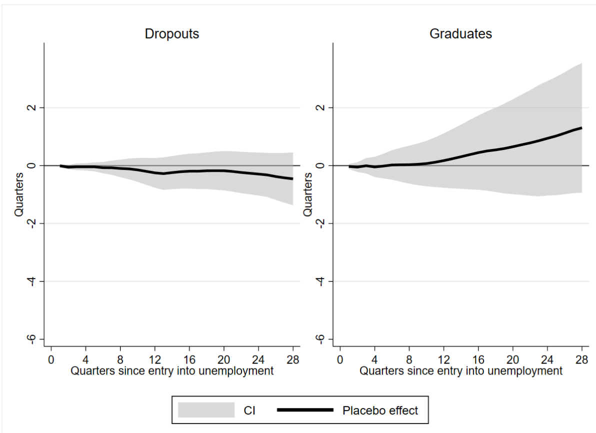
(c) Full-time equivalents



(d) High-paid employment



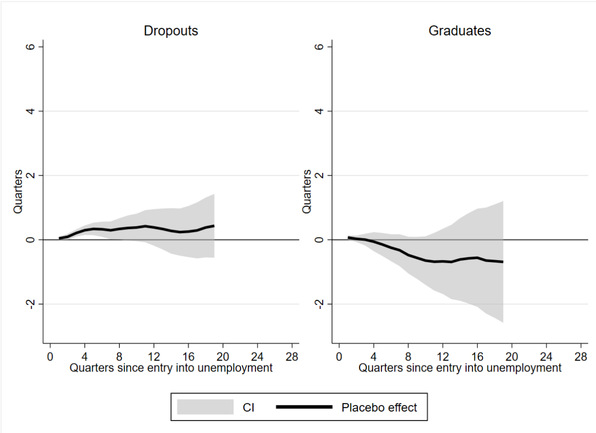
(e) Other employment



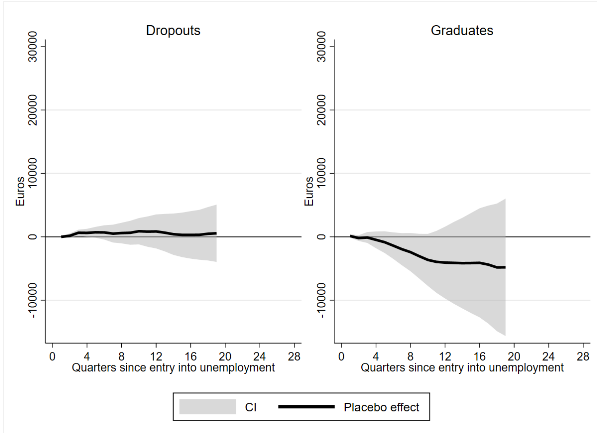
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2008, using age at unemployment entry as the forcing variable with placebo cutoffs. Evolution of the RDD placebo effect and confidence intervals (CI) for the cumulative (a) quarters in private sector employment, (b) gross remuneration, (c) full-time equivalents (100 for a full-time job in the quarter), (d) quarters in high-paid private sector jobs (earning above the median daily wage of €83.5), (e) quarters in self-, public, and cross-border 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.2. Standard errors are clustered at the age level. N = 3,780 (dropouts) and 3,986 (graduates).

Figure A.32: Evolution of the RDD Effect on Cumulative Outcomes: Placebo in 2012

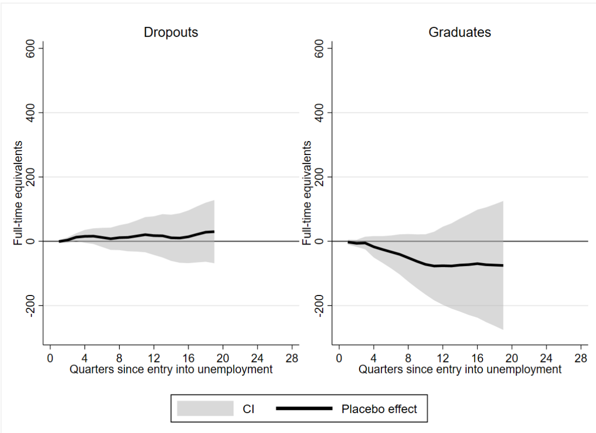
(a) Private sector employment



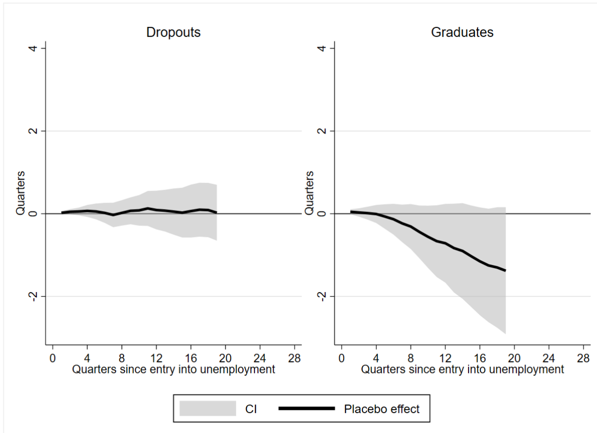
(b) Gross earnings



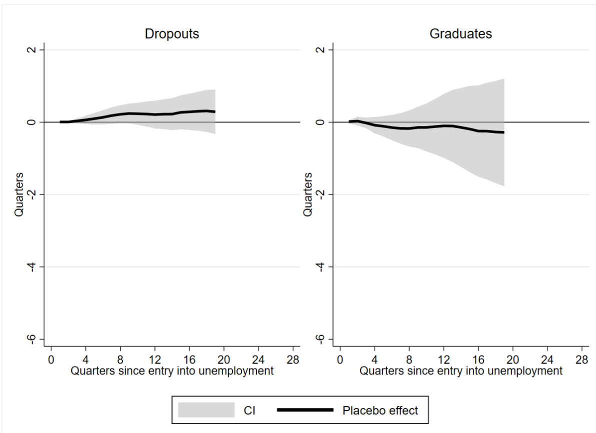
(c) Full-time equivalents



(d) High-paid employment



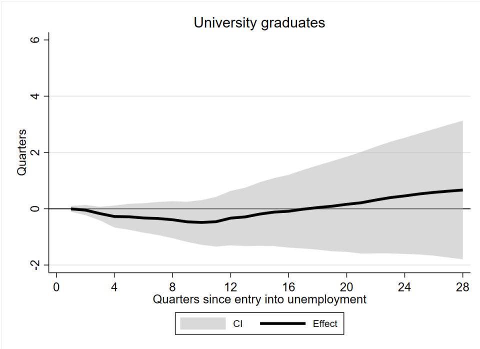
(e) Other employment



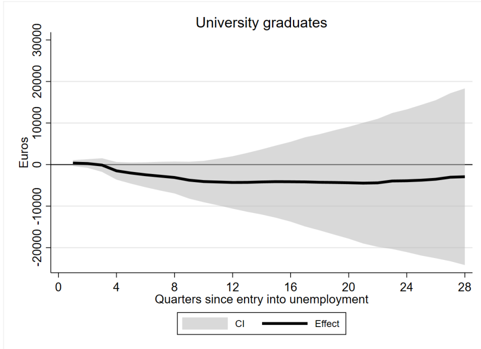
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2012, using age at unemployment entry as the forcing variable with placebo cutoffs. Evolution of the RDD placebo effect and confidence intervals (CI) for the cumulative (a) quarters in private sector employment, (b) gross remuneration, (c) full-time equivalents (100 for a full-time job in the quarter), (d) quarters in high-paid private sector jobs (earning above the median daily wage of €83.5), (e) quarters in self-, public, and cross-border 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.2. Standard errors are clustered at the age level. N = 4,468 (dropouts) and 4,234 (graduates).

Figure A.33: Evolution of the RDD Effect on Cumulative Outcomes: University Degree

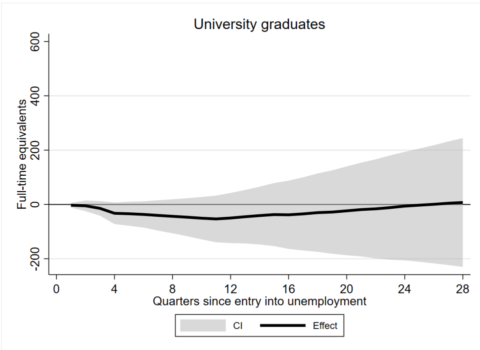
(a) Private sector employment



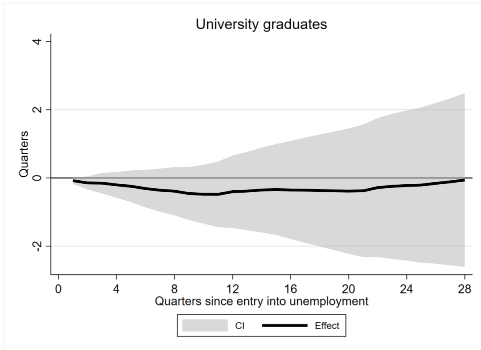
(b) Gross earnings



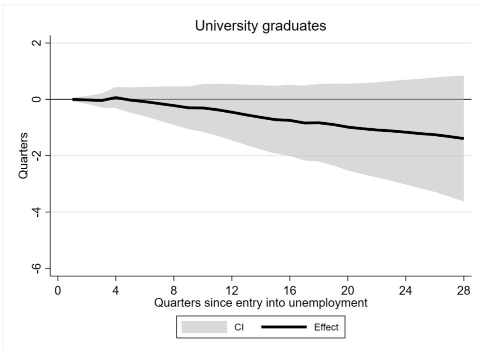
(c) Full-time equivalents



(d) High-paid employment



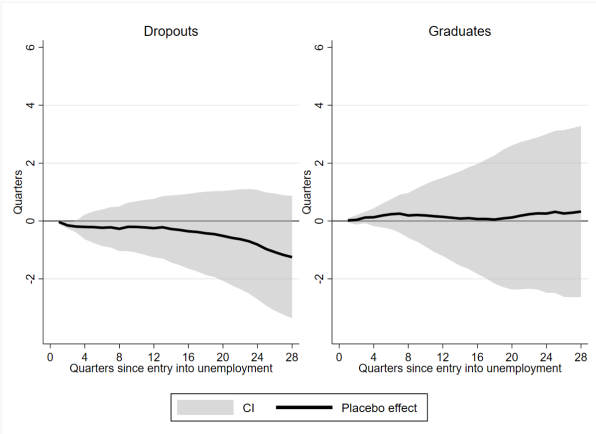
(e) Other employment



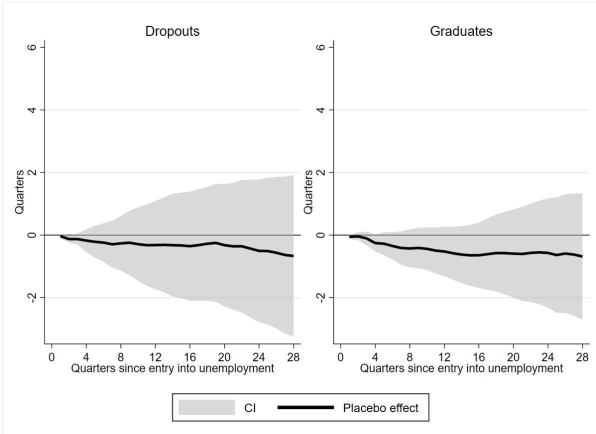
Note: Donut RDD estimates on the inflow sample of youths with a university degree entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence intervals (CI) for (a) the cumulative number of quarters in private sector employment, (b) gross remuneration in private sector employment, (c) full-time equivalents (100 for a full-time job in the quarter), (d) quarters in high-paid private sector jobs (earning above the median daily wage of €83.5), (e) quarters in self-, public, and cross-border 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.2. Standard errors are clustered at the age level. N = 3,993.

Figure A.34: Evolution of the RDD Effect on Cumulative Number of Quarters in Private Sector Employment: False Cutoffs

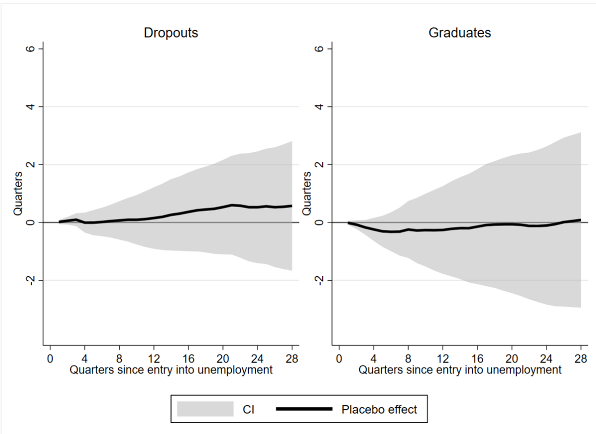
(a) 27



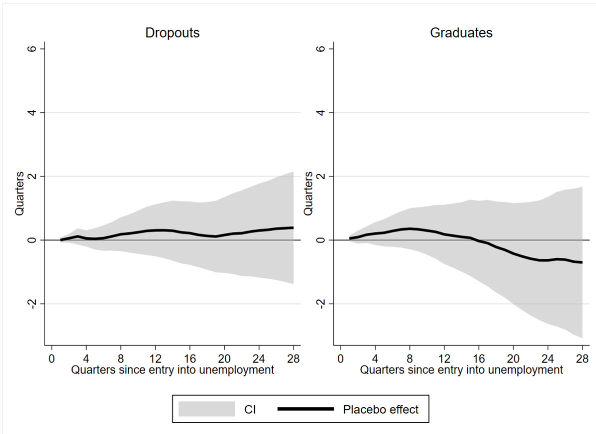
(b) 28



(c) 29



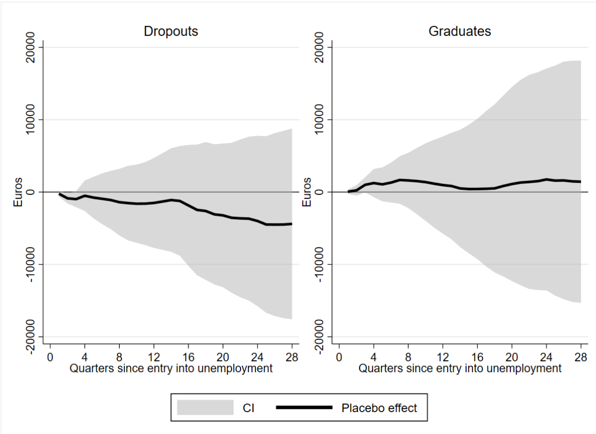
(d) 30



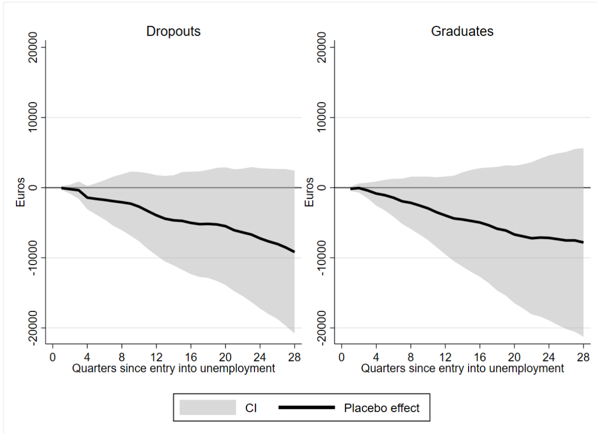
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with placebo cutoffs. The cutoff point and the one-year donut on its left are moved to (a) 27, (b) 28, (c) 29, and (d) 30 years of age. Evolution of the RDD 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.2. Standard errors are clustered at the age level.

Figure A.35: Evolution of the RDD Effect on Cumulative Gross Earnings in the Private Sector: False Cutoffs

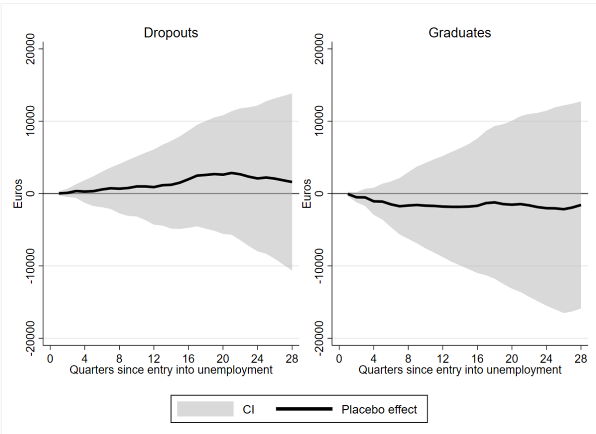
(a) 27



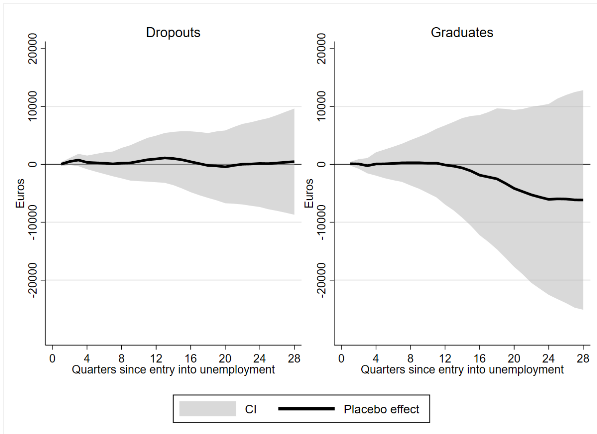
(b) 28



(c) 29



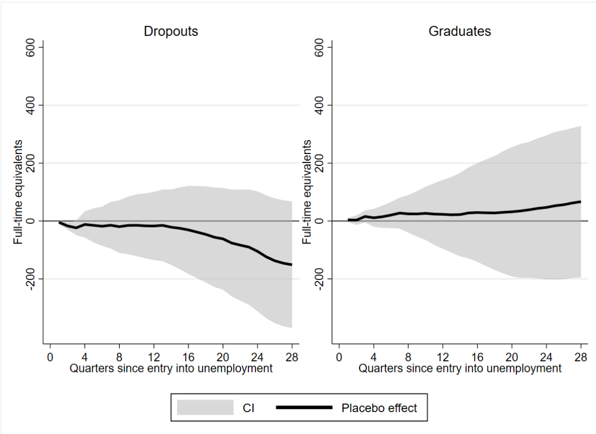
(d) 30



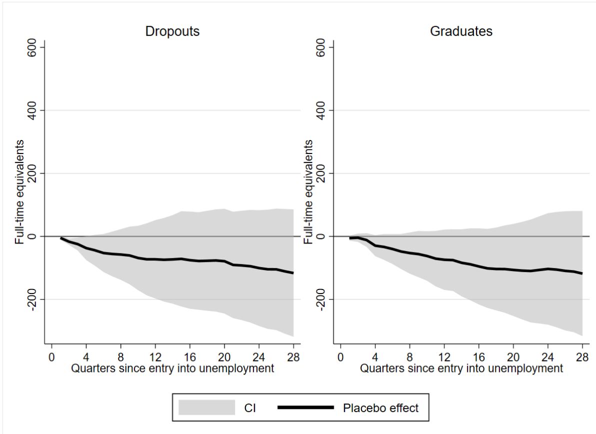
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with placebo cutoffs. The cutoff point and the one-year donut on its left are moved to (a) 27, (b) 28, (c) 29, and (d) 30 years of age. Evolution of the RDD effect and confidence interval (CI) for the cumulative gross remuneration 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.2. Standard errors are clustered at the age level.

Figure A.36: Evolution of the RDD Effect on the Cumulative Percentage of Full-Time Equivalent Quarters in Private Sector Employment: False Cutoffs

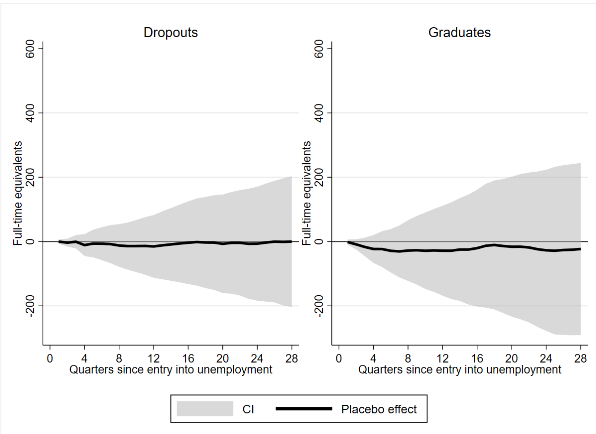
(a) 27



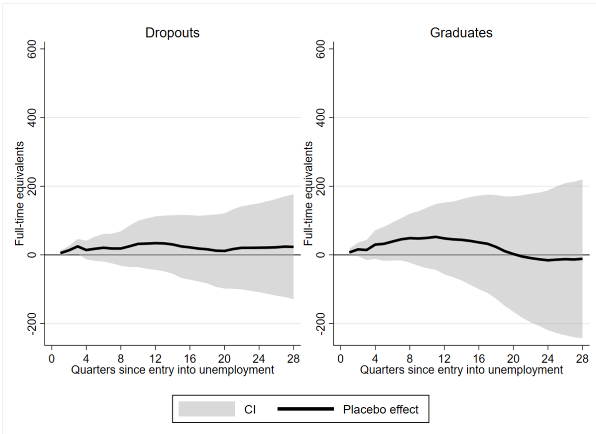
(b) 28



(c) 29



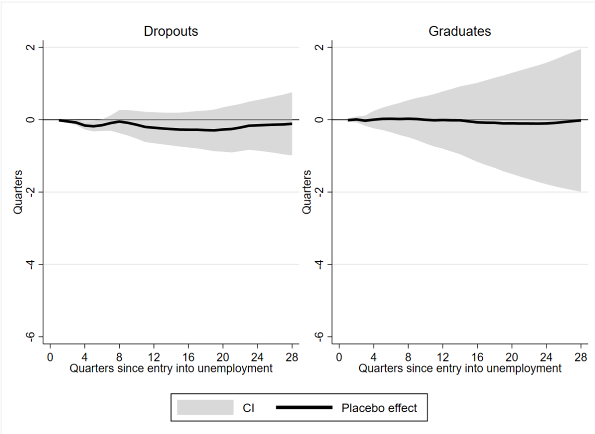
(d) 30



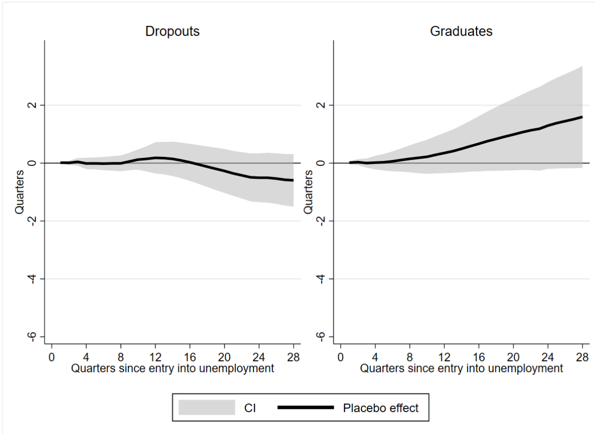
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with placebo cutoffs. The cutoff point and the one-year donut on its left are moved to (a) 27, (b) 28, (c) 29, and (d) 30 years of age. Evolution of the RDD effect and confidence interval (CI) for the cumulative full-time equivalents (100 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.2. Standard errors are clustered at the age level.

Figure A.37: Evolution of the RDD Effect on the Cumulative Number of Quarters in Non-Private Sector Employment: False Cutoffs

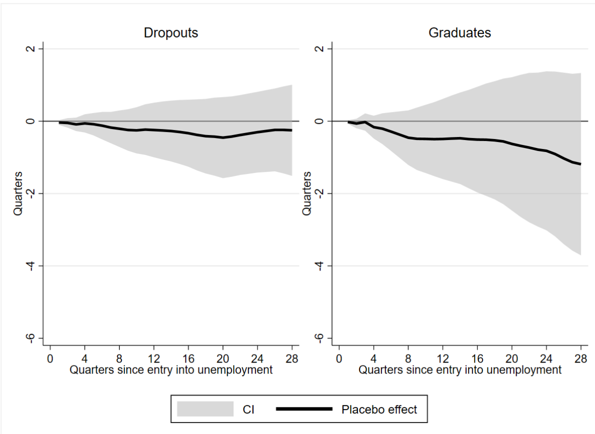
(a) 27



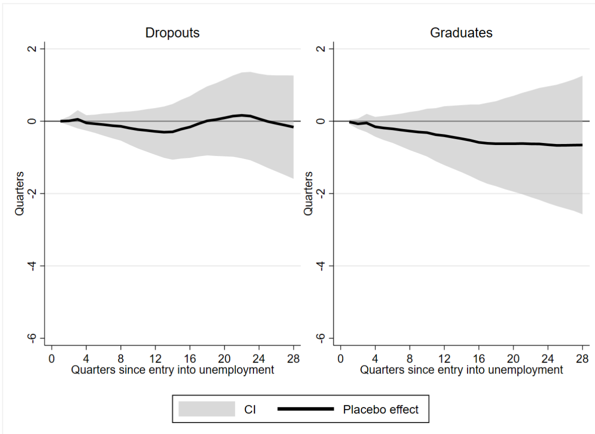
(b) 28



(c) 29

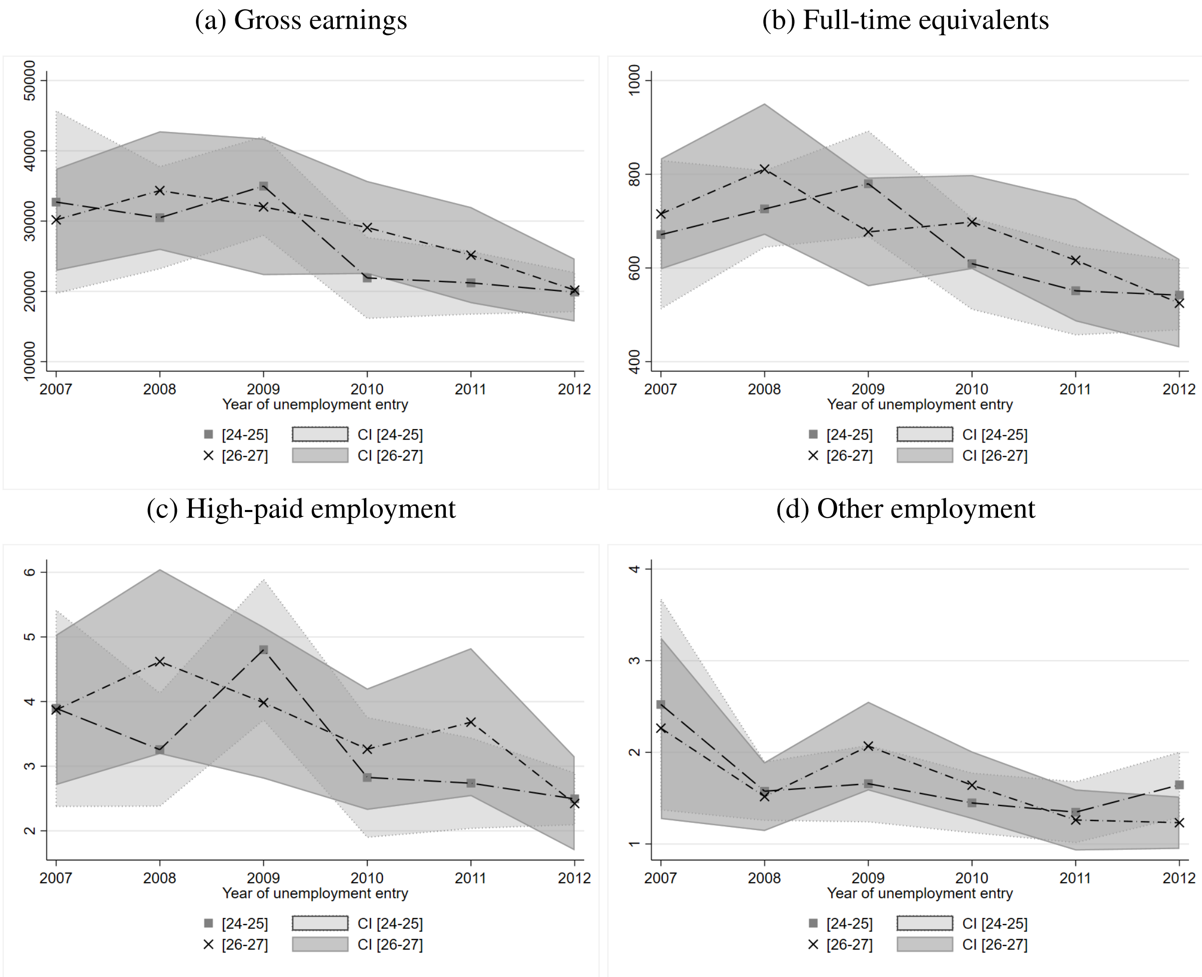


(d) 30



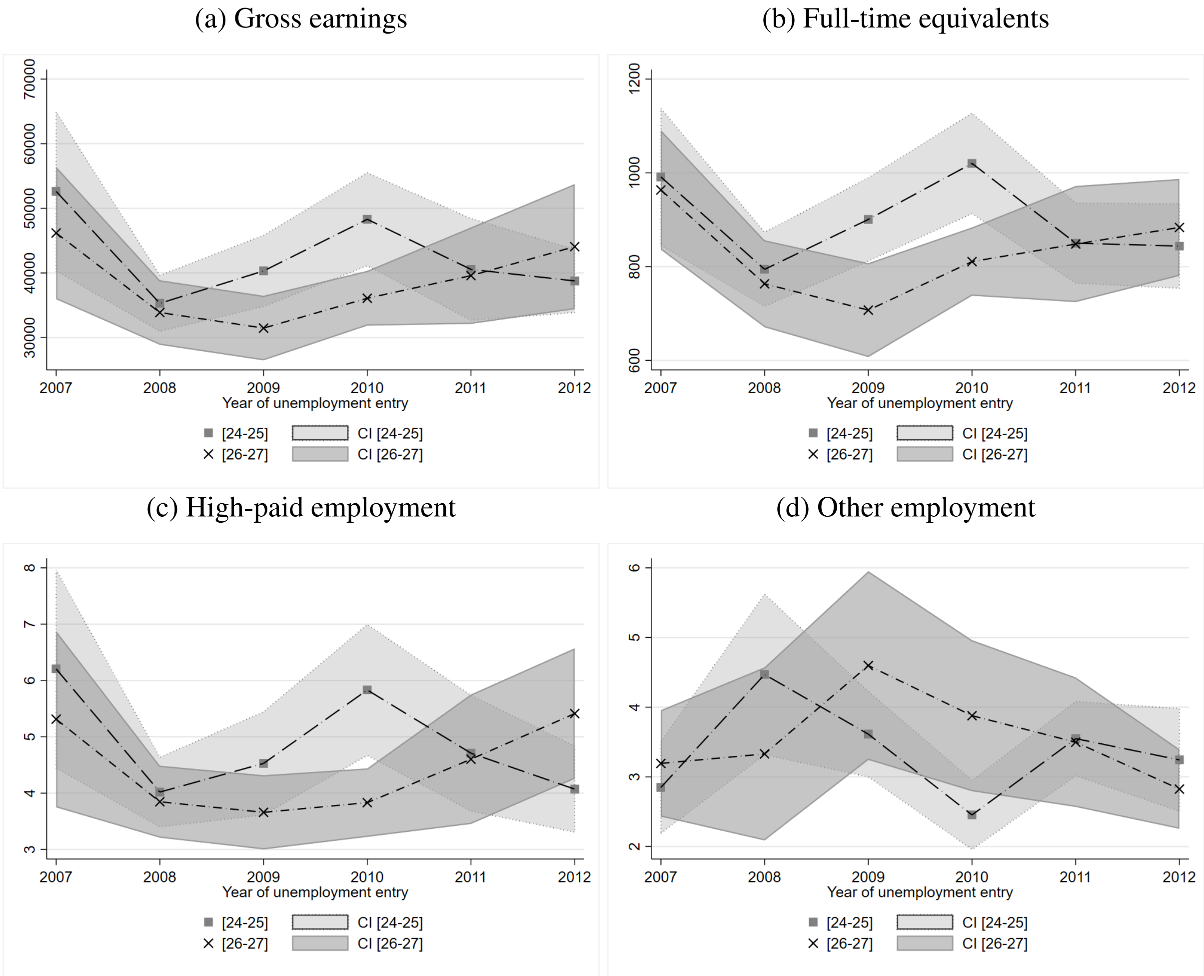
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with placebo cutoffs. The cutoff point and the one-year donut on its left are moved to (a) 27, (b) 28, (c) 29, and (d) 30 years of age. Evolution of the RDD effect and confidence interval (CI) for the cumulative number of quarters in self-, public, and cross-border 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.2. Standard errors are clustered at the age level.

Figure A.38: Evolution of the Cumulative Outcomes at 5 years distance - Dropouts



Note: Evolution of the cumulative outcomes measured at 5 years distance such as (a) gross remuneration in private sector employment, (b) full-time equivalents (100 for a full-time job in the quarter), (c) quarters in high-paid private sector jobs (earning above the median daily wage of €83.5), (d) quarters in self-, public, and cross-border employment, since unemployment and by schooling level: dropouts (left) vs. graduates (right). The treated are aged 24-25 at unemployment entry, while controls are aged 26-27. Data are reweighted by the sampling weights.

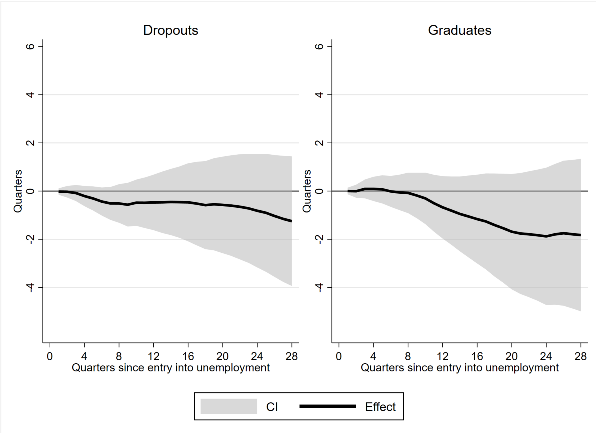
Figure A.39: Evolution of the Cumulative Outcomes at 5 years distance - Graduates



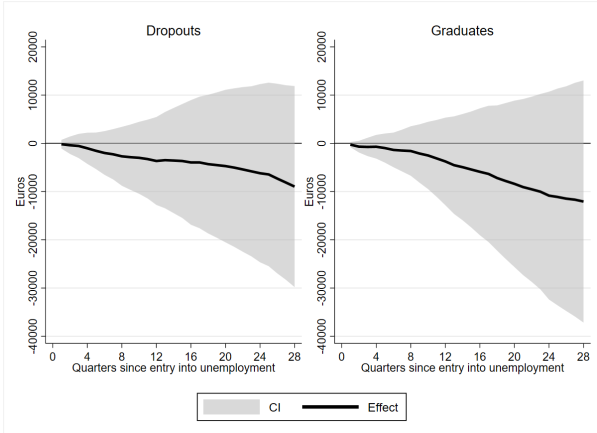
Note: Evolution of the cumulative outcomes measured at 5 years distance such as (a) gross remuneration in private sector employment, (b) full-time equivalents (100 for a full-time job in the quarter), (c) quarters in high-paid private sector jobs (earning above the median daily wage of €83.5), (d) quarters in self-, public, and cross-border employment, since unemployment and by schooling level: dropouts (left) vs. graduates (right). The treated are aged 24-25 at unemployment entry, while controls are aged 26-27. Data are reweighted by the sampling weights.

Figure A.40: Evolution of the DiD Placebo Effect on Cumulative Outcomes

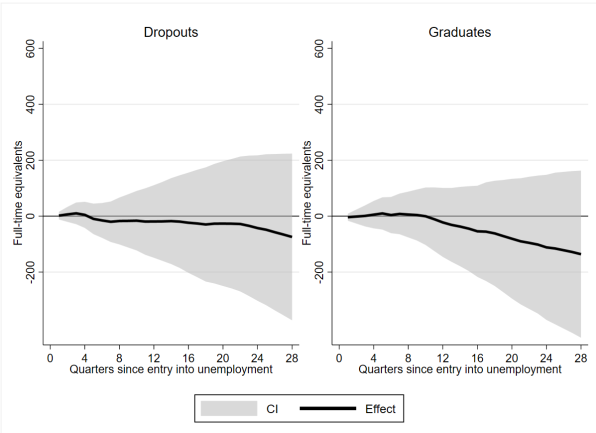
(a) Private sector employment



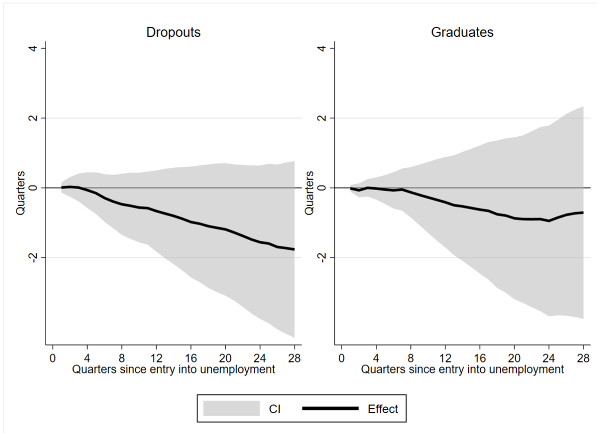
(b) Gross earnings



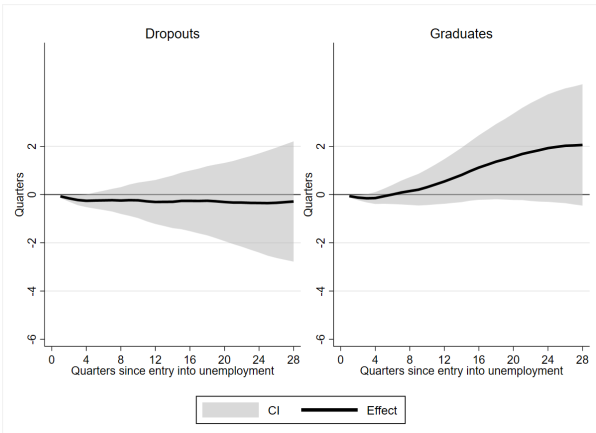
(c) Full-time equivalents



(d) High-paid employment



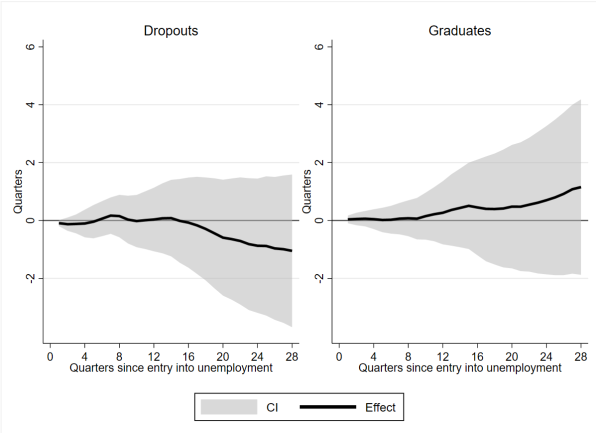
(e) Other employment



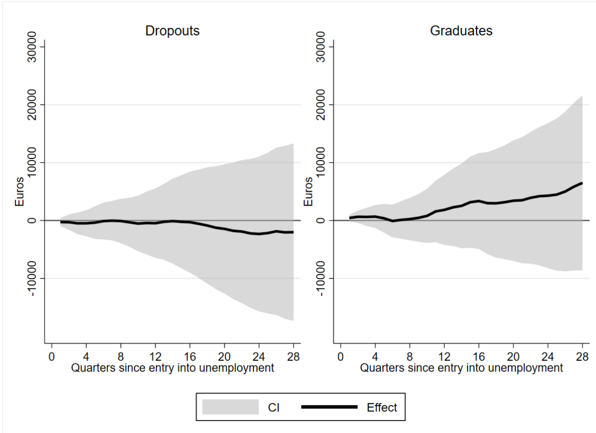
Note: Evolution of the placebo effect estimated with a doubly robust DiD estimator (Sant'Anna and Zhao, 2020) and confidence intervals (CI) for (a) the cumulative number of quarters in private sector employment, (b) gross remuneration in private sector employment, (c) full-time equivalents (100 for a full-time job in the quarter), (d) quarters in high-paid private sector jobs (earning above the median daily wage of €83.5), (e) quarters in self-, public, and cross-border 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(post)-placebo 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.2. Standard errors are clustered at the age level. N = 1,714 (dropouts) and 1,599 (graduates).

Figure A.41: Evolution of the DiD Effect on Cumulative Outcomes: Near the Border

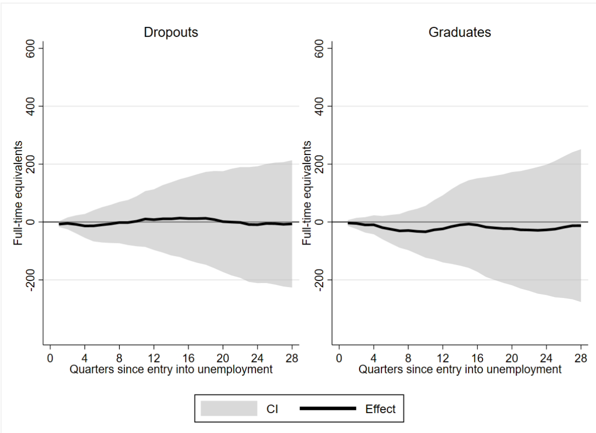
(a) Private sector employment



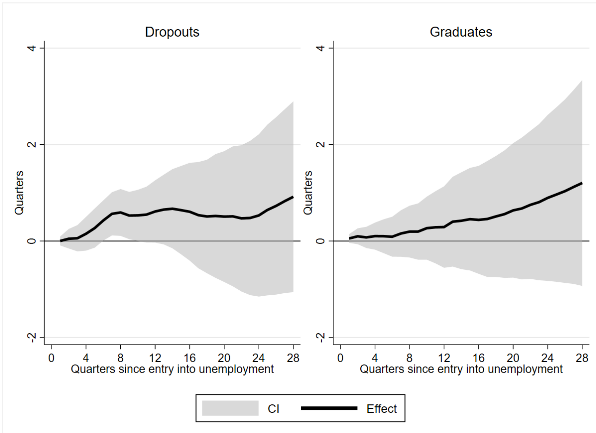
(b) Gross earnings



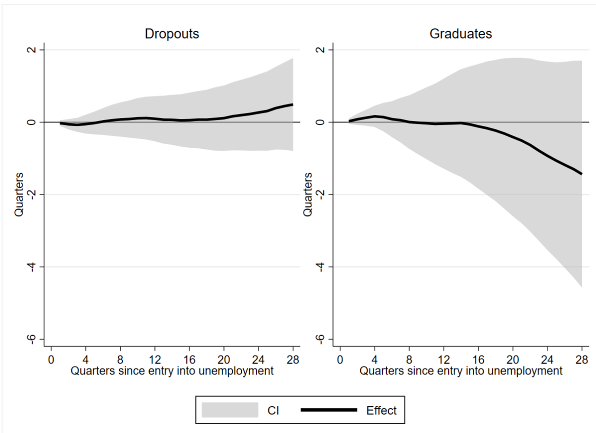
(c) Full-time equivalents



(d) High-paid employment



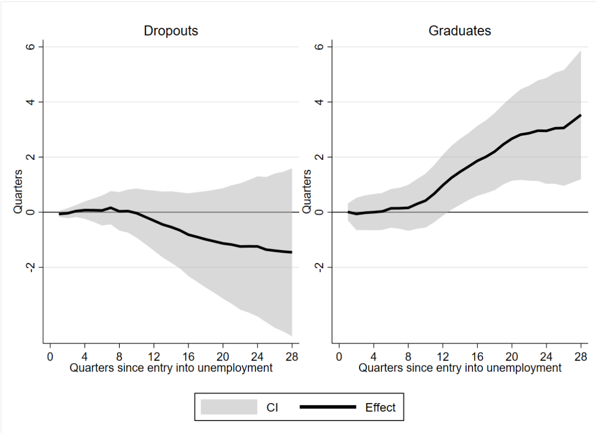
(e) Other employment



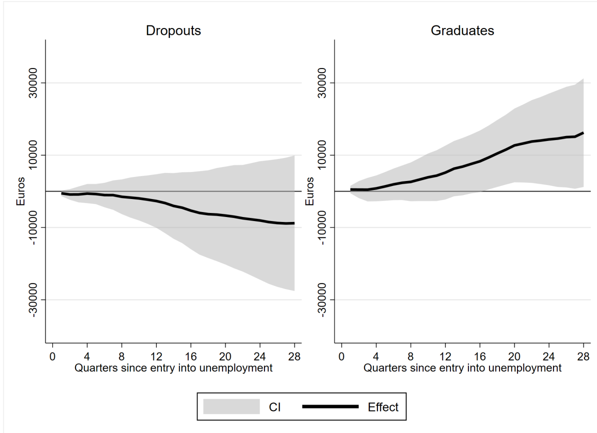
Note: Evolution of the effect estimated with a doubly robust DiD estimator (Sant'Anna and Zhao, 2020) and confidence intervals (CI) for (a) the cumulative number of quarters in private sector employment, (b) gross remuneration in private sector employment, (c) full-time equivalents (100 for a full-time job in the quarter), (d) quarters in high-paid private sector jobs (earning above the median daily wage of €83.5), (e) quarters in self-, public, and cross-border 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(post)-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.2. Standard errors are clustered at the age level. N = 1,237 (dropouts) and 1,069 (graduates).

Figure A.42: Evolution of the DiD Effect on Cumulative Outcomes: Far from the Border

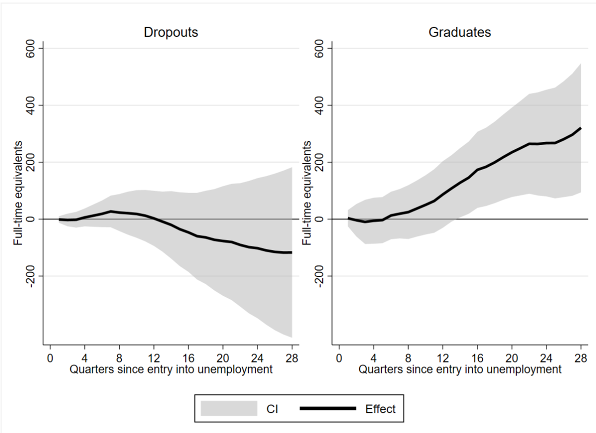
(a) Private sector employment



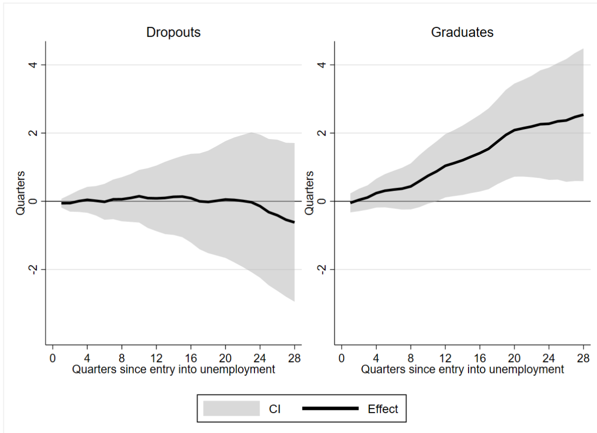
(b) Gross earnings



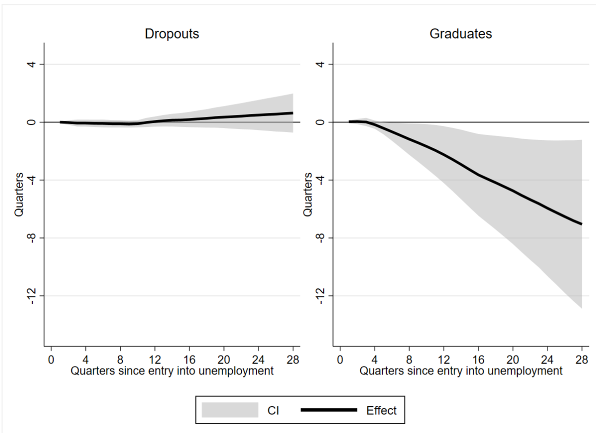
(c) Full-time equivalents



(d) High-paid employment



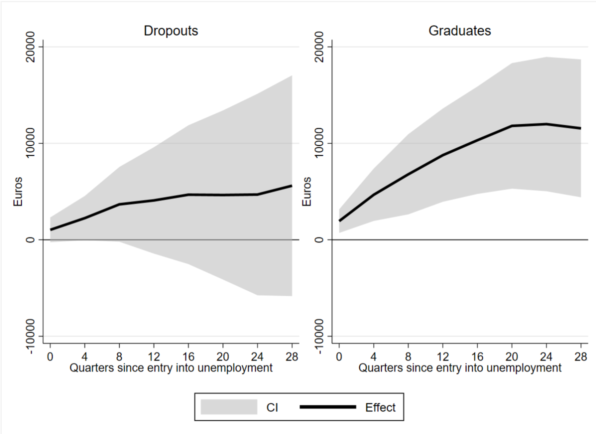
(e) Other employment



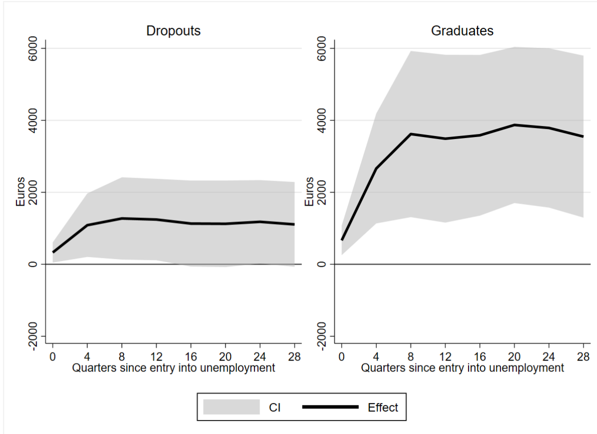
Note: Evolution of the effect estimated with a doubly robust DiD estimator (Sant'Anna and Zhao, 2020) and confidence intervals (CI) for (a) the cumulative number of quarters in private sector employment, (b) gross remuneration in private sector employment, (c) full-time equivalents (100 for a full-time job in the quarter), (d) quarters in high-paid private sector jobs (earning above the median daily wage of €83.5), (e) quarters in self-, public, and cross-border 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(post)-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.2. Standard errors are clustered at the age level. N = 677 (dropouts) and 766 (graduates).

Figure A.43: Evolution of the RDD Effect on Components of the Cost-Benefit Analysis (€)

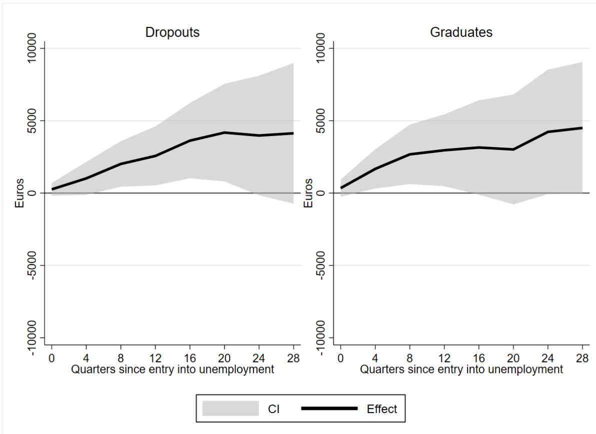
(a) Tax revenue



(b) Hiring subsidy expenditure



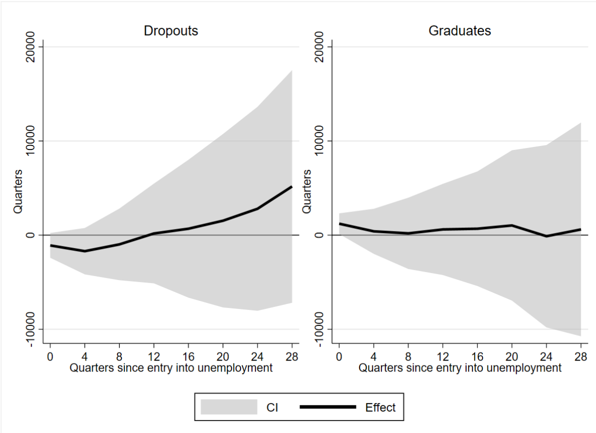
(c) Unemployment benefits expenditure



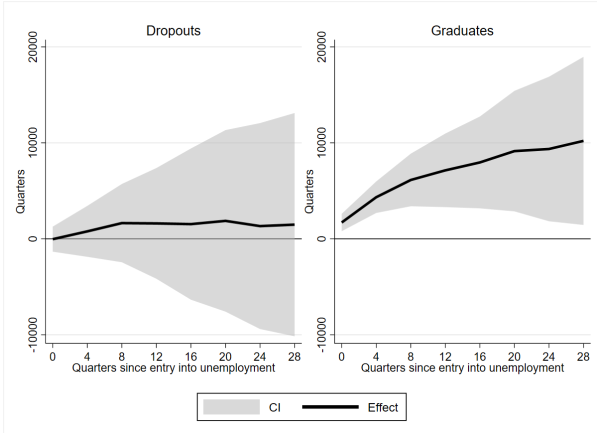
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence intervals (CI) for (a) tax revenue collected by the government, (b) expenditure for hiring subsidies, (c) expenditure for unemployment benefits, in euros 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.2. Standard errors are clustered at the age level. For dropouts (graduates), the effect at 7 years on (a) tax revenue is €5,608 [–5,836; 17,052] with a p-value of 0.332 (€11,555 [4,406; 18,704], p-value 0.002); the effect at 7 years on (b) hiring subsidy expenditure is €1,108 [–69; 2,285] with a p-value of 0.065 (€3,547 [1,296; 5,798], p-value 0.002); the effect at 7 years on (c) unemployment benefits expenditure is €4,131 [–725; 8,989] with a p-value of 0.094 (€4,501 [–53; 9,056], p-value 0.053). N = 4,176 (dropouts) and 4,384 (graduates).

Figure A.44: Evolution of the DiD Effect on Components of the Cost-Benefit Analysis (€)

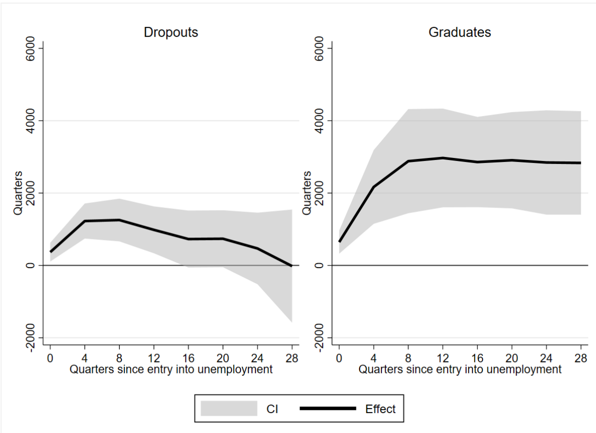
(a) Net return



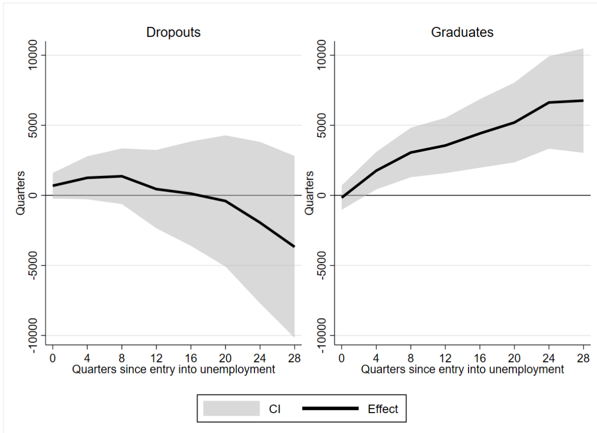
(b) Tax revenue



(c) Hiring subsidy expenditure



(d) 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) the net return to the government, (b) tax revenue collected by the government, (c) expenditure for hiring subsidies, (d) expenditure for unemployment benefits, in euros 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(post)-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.2. Standard errors are clustered at the age level. For dropouts (graduates), the effect at 7 years on (a) the net return to the government is €5,174 [–7,181; 17,529] with a p-value of 0.412 (€609 [–10,743; 11,961], p-value 0.916); the effect at 7 years on (b) tax revenue is €1,479 [–10,152; 13,110] with a p-value of 0.803 (€10,206 [1,446; 18,966], p-value 0.022); the effect at 7 years on (c) hiring subsidy expenditure is €–19 [–1,582; 1,544] with a p-value of 0.981 (€2,833 [1,403; 4,264], p-value 0.000); the effect at 7 years on (d) unemployment benefits expenditure is €–3,675 [–10,160; 2,809] with a p-value of 0.266 (€6,763 [3,036; 10,489], p-value 0.000). N = 1,942 (dropouts) and 1,839 (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

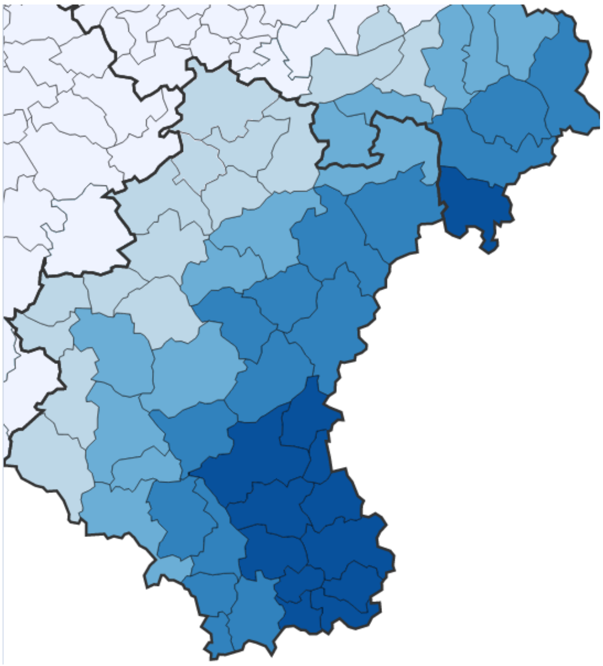
- All other individuals.

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 Descriptive Statistics

C.1 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.[2] 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[3] (100 if full-time) number of quarters in salaried private sector employment, 6) the cumulative gross remuneration earned in the salaried private sector, the number of quarters in a 7) high- or 8) low-paying salaried private sector job,[4] the number of quarters in a 9) large or 10) small firm in a salaried private sector job (fewer or more than 50 employees), and 11) the number of quarters in any salaried public sector employment, self-employment, or cross-border work.[5]

Tables 2 and C.1 shows the descriptive statistics on the outcomes for the benchmark sample of unemployment registration in 2010. Column 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

[2] 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.

[3] 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.

[4] We split the jobs into two groups depending on whether the average daily gross wage earned in the quarter is above or below the median wage earned within seven years from entry into unemployment, i.e., €83.5 (2010 prices), for the aforementioned sample of 9,935 young adults. Results are robust to using an education-specific or a time-varying threshold (for each quarter after unemployment entry).

[5] We have information on cross-border work from health insurance data. The share of cross-border workers that we observe in the Province of Luxembourg is only slightly smaller than the one observed in Labor Force Survey data.

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. No large differences are observed in subsidy take-up if we split the sample by educational level. Differences are instead observed when we look at the probability of starting a salaried private sector job within one year: 58% of eligible graduates find a job, compared to only 44% of eligible dropouts. This is also reflected in the total number of quarters worked in the private sector over the next 7 years: 12.5 quarters for eligible graduates and 8.2 for eligible dropouts aged 22-25. For both educational groups, this outcome is about 4 quarters higher for individuals taking up the Win-Win subsidy. However, their higher participation in the salaried private sector is partially compensated by a lower number of quarters spent in other forms of employment such as the public sector and self- and cross-border employment: 2.4 vs. 3.7 quarters for the Win-Win beneficiaries vs. those eligible in the overall population. The reduction is larger for high school graduates (2.5 vs. 4.7 quarters) than dropouts (2.2 vs. 2.5 quarters). This might suggest the presence of some displacement effects from the job opportunities created in the private sector. 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 quarters in higher paid	5.66	5.82	7.58	4.44	6.31	6.91	8.54
jobs in the salaried private sector in 7 years	(8.07)	(8.09)	(8.35)	(7.04)	(7.17)	(8.68)	(9.02)
Total quarters in higher paid	6.92	7.16	8.21	4.88	6.82	8.98	9.26
jobs in private/public sector in 7 years	(8.61)	(8.63)	(8.41)	(7.26)	(7.16)	(9.19)	(9.11)
Total full-time equivalents	1070.63	1130.03	1551.18	902.74	1361.21	1310.57	1694.56
(100) in the salaried private sector in 7 years	(938.85)	(945.36)	(838.64)	(881.17)	(822.19)	(955.76)	(823.04)
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 lower paid	4.04	4.56	6.96	3.52	5.67	5.39	7.94
jobs in the salaried private sector in 7 years	(6.24)	(6.63)	(7.86)	(5.66)	(7.13)	(7.20)	(8.24)
Total quarters in lower paid	4.74	5.19	7.41	4.14	6.04	6.02	8.44
jobs in private/public sector in 7 years	(6.52)	(6.85)	(7.97)	(5.90)	(7.19)	(7.43)	(8.38)
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) dropout 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.2 Control Variables

In Table C.2 in Online Appendix C, 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 years 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) dropout 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 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. 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 in 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 (€700) 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 €539 for dropouts and €572 for graduates. Dividing these amounts by the corresponding wage costs yields $539/2,283 = 0.24$ for dropouts and $572/2,458 = 0.23$ 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 $380/1,042 = 0.36$ ($76/553 = 0.14$), where €1,042 (€553) is the estimated average of €1,100 (539) for dropouts and €1,000 (572) 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.

D Estimators

D.1 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.

D.2 DiD Estimator

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 outcome 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:

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

where D is equal to 1 for the treated group and 0 otherwise. 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]

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

E Tables

E.1 Main Tables

Table E.1: Effect on Xs: RDD Estimates 7 Years After Unemployment Entry

	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 (i.e., the donut—this is [25.25, 26 for t=3]). 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 of entry-level. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table E.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 of entry-level. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

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

	Employment (1)	Remuneration (2)	FTE (3)	High-Paid Empl. (4)	Low-Paid Empl. (5)	Other Empl. (6)
(A) Dropouts						
Effect at 26	0.16	3,547.90	121.24	-0.30	0.58	0.46
CI	[-2.35; 2.68]	[-11,224.06; 18,319.86]	[-118.08; 360.55]	[-2.25; 1.64]	[-0.57; 1.73]	[-0.57; 1.49]
p-value	0.897	0.633	0.316	0.758	0.318	0.375
Effect in %	2.38	11.61	15.63	-7.97	20.89	27.18
N (left)	2,209	2,209	2,209	2,209	2,209	2,209
N (right)	1,967	1,967	1,967	1,967	1,967	1,967
(B) Graduates						
Effect at 26	2.83**	14,646.03**	286.34**	2.89***	-0.09	-2.65**
CI	[0.68; 4.99]	[2,736.28; 26,555.78]	[62.06; 510.63]	[1.43; 4.34]	[-1.44; 1.26]	[-4.70; -0.60]
p-value	0.011	0.017	0.013	0.000	0.894	0.012
Effect in %	27.83	28.57	25.94	50.01	-2.14	-52.39
N (left)	2,838	2,838	2,838	2,838	2,838	2,838
N (right)	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 accumulated outcomes measured up to 7 years after unemployment entry and include (1) quarters in private sector employment, (2) gross remuneration, (3) full-time equivalents (100 for a full-time job in the quarter), (4) quarters in high-paid private sector jobs (earning above the median daily wage of €83.5), (5) quarters in low-paid private sector jobs (earning below the median daily wage of €83.5), and (6) quarters in self-, public, and cross-border 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 of entry-level. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

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

	Employment (1)	Remuneration (2)	FTE (3)	High-Paid Empl. (4)	Low-Paid Empl. (5)	Other Empl. (6)
(A) Dropouts - near						
Effect at 26	0.59	3,378.12	111.43	-0.66	1.40*	-0.10
CI	[-2.02; 3.20]	[-8,060.21; 14,816.45]	[-106.39; 329.24]	[-2.66; 1.34]	[-0.25; 3.05]	[-1.47; 1.27]
p-value	0.653	0.558	0.311	0.511	0.096	0.883
Effect in %	6.93	8.65	11.71	-14.07	41.51	-3.73
N (left)	788	788	788	788	788	788
N (right)	655	655	655	655	655	655
(B) Graduates - near						
Effect at 26	0.29	-1,786.53	-62.01	0.22	0.19	-1.25
CI	[-1.86; 2.44]	[-13,699.81; 10,126.75]	[-255.08; 131.05]	[-1.38; 1.83]	[-1.19; 1.56]	[-3.38; 0.87]
p-value	0.786	0.766	0.524	0.781	0.787	0.242
Effect in %	2.69	-3.20	-5.19	3.71	4.17	-24.78
N (left)	1,312	1,312	1,312	1,312	1,312	1,312
N (right)	627	627	627	627	627	627
(C) Dropouts - far						
Effect at 26	-0.16	2,975.47	115.84	-0.29	0.29	0.66
CI	[-3.29; 2.98]	[-15,738.54; 21,689.48]	[-200.25; 431.93]	[-2.82; 2.25]	[-1.21; 1.79]	[-0.67; 1.99]
p-value	0.921	0.752	0.467	0.822	0.699	0.326
Effect in %	-2.38	10.42	15.82	-7.96	11.03	46.52
N (left)	1,376	1,376	1,376	1,376	1,376	1,376
N (right)	1,260	1,260	1,260	1,260	1,260	1,260
(D) Graduates - far						
Effect at 26	3.74**	19,900.47**	395.67**	3.97***	-0.34	-3.04***
CI	[0.72; 6.77]	[1,893.83; 37,907.11]	[93.77; 697.57]	[1.89; 6.06]	[-2.10; 1.42]	[-5.26; -0.83]
p-value	0.016	0.031	0.011	0.000	0.699	0.008
Effect in %	37.57	39.14	36.24	68.64	-8.43	-61.70
N (left)	1,517	1,517	1,517	1,517	1,517	1,517
N (right)	915	915	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, (3) full-time equivalents (100 for a full-time job in the quarter), (4) quarters in high-paid private sector jobs (earning above the median daily wage of €83.5), (5) quarters in low-paid private sector jobs (earning below the median daily wage of €83.5), (6) quarters in self-, public, and cross-border 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 of entry-level. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table E.5: Cost-Benefit Analysis: RDD Estimates 7 Years After Unemployment Entry

	Net return (1)	Subsidy cost (2)	Tax collected (3)	Paid UB (4)
(A) Dropouts				
Effect at 26	367.69	1,108.33*	5,607.77	4,131.76*
CI	[-12,055.54; 12,790.92]	[-69.22; 2,285.87]	[-5,836.55; 17,052.09]	[-725.85; 8,989.37]
p-value	0.953	0.065	0.332	0.094
Effect in %	12.21	75.05	19.42	16.94
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	3,506.37	3,547.08***	11,555.25***	4,501.81*
CI	[-4,132.96; 11,145.69]	[1,296.50; 5,797.66]	[4,406.10; 18,704.40]	[-52.97; 9,056.58]
p-value	0.363	0.002	0.002	0.053
Effect in %	17.10	149.07	24.70	18.84
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 (in euros) and include (1) net return for the public budget, (2) subsidy cost, (3) tax returns, and (4) paid unemployment benefits. 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 of entry-level. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

E.2 Spillover

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

	Employment (1)	Remuneration (2)	FTE (3)	High-Paid Empl. (4)	Other Empl. (5)
(A) Dropouts					
Effect on 26-27	-0.45	-4,724.75	-82.27	-1.09	-0.64
CI	[-2.15; 1.25]	[-14,406.23; 4,956.74]	[-258.41; 93.87]	[-2.65; 0.48]	[-1.61; 0.33]
p-value	0.603	0.339	0.360	0.174	0.197
N (treated)	1,315	1,315	1,315	1,315	1,315
N (controls)	5,395	5,395	5,395	5,395	5,395
(B) Graduates					
Effect on 26-27	0.62	-344.85	46.85	-0.51	0.47
CI	[-0.92; 2.16]	[-12,486.92; 11,797.23]	[-103.88; 197.58]	[-2.02; 1.00]	[-0.83; 1.78]
p-value	0.430	0.956	0.542	0.510	0.476
N (treated)	1,111	1,111	1,111	1,111	1,111
N (controls)	3,091	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, (3) full-time equivalents (100 for a full-time job in the quarter), (4) quarters in high-paid private sector jobs (earning above the median daily wage of €83.5), and (5) quarters in self-, public-, and cross-border 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 of entry-level. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

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

	Employment (1)	Remuneration (2)	FTE (3)	High-Paid Empl. (4)	Other Empl. (5)
(A) Dropouts					
Effect on 26-27	-0.17	-9,922.76	-146.45	0.31	-2.57***
CI	[-2.90; 2.55]	[-25,631.96; 5,786.44]	[-460.37; 167.47]	[-2.37; 2.99]	[-3.97; -1.17]
p-value	0.902	0.216	0.361	0.821	0.000
N (treated)	654	654	654	654	654
N (controls)	457	457	457	457	457
(B) Graduates					
Effect on 26-27	0.18	-193.73	33.45	-0.91	0.75
CI	[-1.60; 1.95]	[-11,784.99; 11,397.54]	[-164.68; 231.58]	[-2.24; 0.41]	[-1.14; 2.63]
p-value	0.845	0.974	0.741	0.175	0.437
N (treated)	857	857	857	857	857
N (controls)	458	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). The treated are aged 26-27 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, (3) full-time equivalents (100 for a full-time job in the quarter), (4) quarters in high-paid private sector jobs (earning above the median daily wage of €83.5), and (5) quarters in self-, public, and cross-border 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 of entry-level. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

E.3 Sensitivity

Table E.8: DiD Estimates 7 Years After Unemployment Entry

	Employment (1)	Remuneration (2)	FTE (3)	High-Paid Empl. (4)	Other Empl. (5)
(A) Dropouts					
Effect on 24-25	-0.66	-1,675.93	-27.07	1.26	0.46
CI	[-2.74; 1.42]	[-14,914.58; 11,562.72]	[-196.20; 142.07]	[-0.73; 3.26]	[-0.55; 1.48]
p-value	0.532	0.804	0.754	0.215	0.372
N (treated)	1,018	1,018	1,018	1,018	1,018
N (controls)	924	924	924	924	924
(B) Graduates					
Effect on 24-25	2.75***	13,982.40**	228.58**	2.26***	-4.63***
CI	[1.18; 4.32]	[3,267.26; 24,697.54]	[47.02; 410.13]	[1.00; 3.52]	[-7.68; -1.57]
p-value	0.001	0.011	0.014	0.000	0.003
N (treated)	1,054	1,054	1,054	1,054	1,054
N (controls)	785	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 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, (3) full-time equivalents (100 for a full-time job in the quarter), (4) quarters in high-paid private sector jobs (earning above the median daily wage of €83.5), and (5) quarters in self-, public, and cross-border 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 of entry-level. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table E.9: Border Proximity: DiD Estimates 7 Years After Unemployment Entry

	Employment (1)	Remuneration (2)	FTE (3)	High-Paid Empl. (4)	Other Empl. (5)
(A) Dropouts - near					
Effect on 24-25	-1.05	-2,009.26	-6.50	0.92	0.49
CI	[-3.69; 1.59]	[-17,346.87; 13,328.35]	[-226.37; 213.37]	[-1.06; 2.90]	[-0.79; 1.77]
p-value	0.436	0.797	0.954	0.363	0.454
N (treated)	362	362	362	362	362
N (controls)	315	315	315	315	315
(B) Graduates - near					
Effect on 24-25	1.15	6,507.45	-12.50	1.20	-1.44
CI	[-1.88; 4.19]	[-8,614.68; 21,629.59]	[-276.46; 251.47]	[-0.93; 3.34]	[-4.57; 1.70]
p-value	0.455	0.399	0.926	0.269	0.370
N (treated)	436	436	436	436	436
N (controls)	330	330	330	330	330
(C) Dropouts - far					
Effect on 24-25	-1.45	-8,813.88	-117.16	-0.62	0.63
CI	[-4.50; 1.59]	[-27,560.02; 9,932.26]	[-416.81; 182.49]	[-2.94; 1.71]	[-0.72; 1.98]
p-value	0.350	0.357	0.443	0.603	0.357
N (treated)	636	636	636	636	636
N (controls)	601	601	601	601	601
(D) Graduates - far					
Effect on 24-25	3.54***	16,234.66**	321.08***	2.54**	-7.05**
CI	[1.20; 5.87]	[1,219.84; 31,249.48]	[94.20; 547.95]	[0.59; 4.48]	[-12.89; -1.21]
p-value	0.003	0.034	0.006	0.011	0.018
N (treated)	597	597	597	597	597
N (controls)	472	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 and 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. 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 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, (3) full-time equivalents (100 for a full-time job in the quarter), (4) quarters in high-paid private sector jobs (earning above the median daily wage of €83.5), and (5) quarters in self-, public, and cross-border 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 of entry-level. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table E.10: DiD-Placebo Estimates 7 Years After Unemployment Entry

	Employment (1)	Remuneration (2)	FTE (3)	High-Paid Empl. (4)	Other Empl. (5)
(A) Dropouts					
Effect on 24-25	-1.25	-8,959.82	-74.37	-1.77	-0.29
CI	[-3.94; 1.44]	[-29,807.63; 11,887.99]	[-372.47; 223.73]	[-4.30; 0.77]	[-2.78; 2.21]
p-value	0.363	0.400	0.625	0.172	0.821
N (treated)	896	896	896	896	896
N (controls)	818	818	818	818	818
(B) Graduates					
Effect on 24-25	-1.82	-12,065.72	-135.96	-0.71	2.06
CI	[-4.99; 1.34]	[-37,167.45; 13,036.00]	[-434.86; 162.94]	[-3.76; 2.34]	[-0.46; 4.58]
p-value	0.258	0.346	0.373	0.649	0.109
N (treated)	858	858	858	858	858
N (controls)	741	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, (3) full-time equivalents (100 for a full-time job in the quarter), (4) quarters in high-paid private sector jobs (earning above the median daily wage of €83.5), and (5) quarters in self-, public, and cross-border 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 of entry-level. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table E.11: Bandwidth Sensitivity: RDD Estimates 7 Years After Unemployment Entry

	Employment (1)	Remuneration (2)	FTE (3)	High-Paid Empl. (4)	Other Empl. (5)
(A) Dropouts, [21; 30)					
Effect at 26	0.80	6,977.28	177.67*	0.18	0.06
CI	[-1.28; 2.89]	[-5,232.69; 19,187.25]	[-20.62; 375.97]	[-1.47; 1.83]	[-0.78; 0.90]
p-value	0.445	0.259	0.078	0.830	0.886
Effect in %	11.15	21.41	22.18	4.57	3.42
N (left)	3,042	3,042	3,042	3,042	3,042
N (right)	2,628	2,628	2,628	2,628	2,628
(B) Graduates, [21; 30)					
Effect at 26	2.43***	14,648.54***	236.90**	2.39***	-1.88**
CI	[0.66; 4.20]	[5,118.43; 24,178.66]	[52.95; 420.86]	[1.12; 3.67]	[-3.63; -0.12]
p-value	0.008	0.003	0.012	0.000	0.036
Effect in %	23.39	28.37	21.33	41.27	-38.98
N (left)	3,882	3,882	3,882	3,882	3,882
N (right)	1,991	1,991	1,991	1,991	1,991
(C) Dropouts, [21.5; 29.5)					
Effect at 26	0.56	5,916.83	159.57	-0.01	0.27
CI	[-1.69; 2.81]	[-7,320.90; 19,154.57]	[-54.69; 373.82]	[-1.78; 1.77]	[-0.65; 1.19]
p-value	0.622	0.377	0.142	0.994	0.558
Effect in %	7.95	18.79	20.28	-0.18	16.05
N (left)	2,615	2,615	2,615	2,615	2,615
N (right)	2,313	2,313	2,313	2,313	2,313
(D) Graduates, [21.5; 29.5)					
Effect at 26	2.60***	14,601.21***	256.37**	2.68***	-2.25**
CI	[0.68; 4.53]	[4,070.98; 25,131.45]	[54.05; 458.70]	[1.35; 4.02]	[-4.12; -0.39]
p-value	0.009	0.007	0.014	0.000	0.019
Effect in %	25.31	28.39	23.14	46.53	-45.48
N (left)	3,350	3,350	3,350	3,350	3,350
N (right)	1,782	1,782	1,782	1,782	1,782
(E) Dropouts, [22.5; 28.5)					
Effect at 26	-0.37	-178.70	44.29	-0.47	0.93
CI	[-3.19; 2.45]	[-16,635.76; 16,278.36]	[-225.02; 313.61]	[-2.60; 1.66]	[-0.23; 2.08]
p-value	0.793	0.983	0.743	0.659	0.113
Effect in %	-5.54	-0.60	5.80	-12.55	55.78
N (left)	1,835	1,835	1,835	1,835	1,835
N (right)	1,657	1,657	1,657	1,657	1,657
(F) Graduates, [22.5; 28.5)					
Effect at 26	3.45***	17,060.23**	365.65***	3.33***	-2.88**
CI	[0.96; 5.94]	[3,402.92; 30,717.54]	[118.90; 612.40]	[1.73; 4.92]	[-5.23; -0.54]
p-value	0.007	0.015	0.004	0.000	0.017
Effect in %	34.61	33.86	33.51	58.25	-56.81
N (left)	2,279	2,279	2,279	2,279	2,279
N (right)	1,285	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, (3) full-time equivalents (100 for a full-time job in the quarter), (4) quarters in high-paid private sector jobs (earning above the median daily wage of €83.5), and (5) quarters in self-, public, and cross-border 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 of entry-level. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table E.12: Not Controlling for Xs: RDD Estimates 7 Years After Unemployment Entry

	Employment (1)	Remuneration (2)	FTE (3)	High-Paid Empl. (4)	Other Empl. (5)
(A) Dropouts					
Effect at 26	-0.41	916.29	63.13	-0.31	0.48
CI	[-3.12; 2.30]	[-15,981.06; 17,813.65]	[-203.64; 329.90]	[-2.62; 2.00]	[-0.62; 1.58]
p-value	0.765	0.914	0.638	0.790	0.384
N (left)	2,209	2,209	2,209	2,209	2,209
N (right)	1,967	1,967	1,967	1,967	1,967
(B) Graduates					
Effect at 26	2.31**	9,011.24	200.95*	1.90**	-2.79**
CI	[0.19; 4.43]	[-3,726.40; 21,748.87]	[-4.15; 406.04]	[0.16; 3.64]	[-4.99; -0.58]
p-value	0.033	0.163	0.055	0.033	0.014
N (left)	2,838	2,838	2,838	2,838	2,838
N (right)	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. 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, (3) full-time equivalents (100 for a full-time job in the quarter), (4) quarters in high-paid private sector jobs (earning above the median daily wage of €83.5), and (5) quarters in self-, public, and cross-border 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 of entry-level. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table E.13: Effect at 25: RDD Estimates 7 Years After Unemployment Entry

	Employment (1)	Remuneration (2)	FTE (3)	High-Paid Empl. (4)	Other Empl. (5)
(A) Dropouts					
Effect at 25	1.56	10,855.31*	202.27**	0.62	0.87**
CI	[-0.40; 3.53]	[-503.39; 22,214.01]	[11.98; 392.55]	[-1.00; 2.23]	[0.10; 1.65]
p-value	0.117	0.061	0.038	0.448	0.028
Effect in %	25.80	42.93	28.46	18.54	73.87
N (left)	2,209	2,209	2,209	2,209	2,209
N (right)	1,967	1,967	1,967	1,967	1,967
(B) Graduates					
Effect at 25	2.72**	14,176.41**	249.27**	2.30***	-2.54*
CI	[0.43; 5.01]	[2,454.64; 25,898.17]	[31.38; 467.16]	[0.80; 3.80]	[-5.36; 0.28]
p-value	0.021	0.018	0.026	0.003	0.076
Effect in %	26.88	27.52	22.44	40.58	-46.19
N (left)	2,838	2,838	2,838	2,838	2,838
N (right)	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 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). 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, (3) full-time equivalents (100 for a full-time job in the quarter), (4) quarters in high-paid private sector jobs (earning above the median daily wage of €83.5), and (5) quarters in self-, public, and cross-border 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 of entry-level. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table E.14: Placebo Before (After) Win-Win: RDD Estimates 7 (5) Years After Unemployment Entry

	Employment (1)	Remuneration (2)	FTE (3)	High-Paid Empl. (4)	Other Empl. (5)
(A) Dropouts - 2008					
Effect at 26	-0.03	1,638.21	-4.10	0.07	-0.46
CI	[-1.57; 1.52]	[-8,463.14; 11,739.57]	[-186.12; 177.92]	[-1.43; 1.57]	[-1.37; 0.45]
p-value	0.972	0.747	0.964	0.923	0.322
Effect in %	-0.32	4.16	-0.42	1.49	-18.48
N (left)	2,161	2,161	2,161	2,161	2,161
N (right)	1,619	1,619	1,619	1,619	1,619
(B) Graduates - 2008					
Effect at 26	0.27	-1,757.46	-18.94	-0.67	1.31
CI	[-1.87; 2.41]	[-15,663.56; 12,148.64]	[-226.62; 188.74]	[-2.41; 1.07]	[-0.94; 3.55]
p-value	0.802	0.802	0.856	0.448	0.249
Effect in %	2.65	-3.47	-1.70	-10.22	30.52
N (left)	2,679	2,679	2,679	2,679	2,679
N (right)	1,307	1,307	1,307	1,307	1,307
(C) Dropouts - 2012					
Effect at 26	0.26	303.43	14.16	0.06	0.29
CI	[-0.54; 1.05]	[-3,438.05; 4,044.91]	[-67.84; 96.15]	[-0.57; 0.70]	[-0.22; 0.79]
p-value	0.522	0.872	0.732	0.841	0.258
Effect in %	7.76	2.06	3.71	3.53	34.36
N (left)	2,296	2,296	2,296	2,296	2,296
N (right)	2,172	2,172	2,172	2,172	2,172
(D) Graduates - 2012					
Effect at 26	-0.56	-4,105.20	-69.56	-1.15*	-0.24
CI	[-2.09; 0.97]	[-12,709.97; 4,499.57]	[-237.40; 98.28]	[-2.45; 0.15]	[-1.51; 1.02]
p-value	0.469	0.345	0.411	0.083	0.701
Effect in %	-9.33	-13.93	-11.00	-32.27	-11.69
N (left)	2,635	2,635	2,635	2,635	2,635
N (right)	1,599	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, (3) full-time equivalents (100 for a full-time job in the quarter), (4) quarters in high-paid private sector jobs (earning above the median daily wage of €83.5), and (5) quarters in self-, public, and cross-border 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 of entry-level. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table E.15: Placebo on University Graduates: RDD Estimates 7 years After Unemployment Entry

	Employment (1)	Remuneration (2)	FTE (3)	High-Paid Empl. (4)	Other Empl. (5)
(A) University graduates					
Effect at 26	0.67	-2,927.69	7.28	-0.06	-1.39
CI	[-1.80; 3.13]	[-24,185.35; 18,329.96]	[-229.95; 244.51]	[-2.61; 2.49]	[-3.63; 0.84]
p-value	0.591	0.784	0.951	0.963	0.219
Effect in %	7.29	-4.40	0.70	-0.77	-12.27
N (left)	2,408	2,408	2,408	2,408	2,408
N (right)	1,585	1,585	1,585	1,585	1,585

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) (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, (3) full-time equivalents (100 for a full-time job in the quarter), (4) quarters in high-paid private sector jobs (earning above the median daily wage of €83.5), and (5) quarters in self-, public, and cross-border 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 of entry-level. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table E.16: Placebo on False Cutoffs (27-30): RDD Estimates 7 Years After Unemployment Entry

	Employment (1)	Remuneration (2)	FTE (3)	High-Paid Empl. (4)	Other Empl. (5)
(A) Dropouts, 27					
Effect at 27	-1.25	-4,398.17	-151.23	0.71	-0.11
CI	[-3.36; 0.87]	[-17,585.45; 8,789.11]	[-370.25; 67.80]	[-1.48; 2.89]	[-0.98; 0.76]
p-value	0.244	0.508	0.173	0.522	0.798
Effect in %	-15.25	-11.03	-16.97	14.70	-4.59
N (left)	2,125	2,125	2,125	2,125	2,125
N (right)	1,960	1,960	1,960	1,960	1,960
(B) Graduates, 27					
Effect at 27	0.33	1,436.83	66.85	0.98	-0.02
CI	[-2.63; 3.28]	[-15,305.01; 18,178.66]	[-194.88; 328.59]	[-1.39; 3.35]	[-1.99; 1.95]
p-value	0.826	0.865	0.612	0.413	0.986
Effect in %	2.99	2.60	5.80	15.19	-0.39
N (left)	2,442	2,442	2,442	2,442	2,442
N (right)	1,422	1,422	1,422	1,422	1,422
(C) Dropouts, 28					
Effect at 28	-0.68	-7,809.94	-117.99	-1.82**	1.60*
CI	[-2.71; 1.34]	[-21,255.85; 5,635.97]	[-316.56; 80.58]	[-3.43; -0.21]	[-0.16; 3.36]
p-value	0.502	0.251	0.240	0.028	0.075
Effect in %	-7.06	-16.05	-11.02	-31.87	40.43
N (left)	2,074	2,074	2,074	2,074	2,074
N (right)	1,149	1,149	1,149	1,149	1,149
(D) Graduates, 28					
Effect at 28	-0.67	-9,173.91	-116.35	-1.85***	-0.59
CI	[-3.25; 1.91]	[-20,771.64; 2,423.82]	[-318.52; 85.82]	[-3.23; -0.47]	[-1.50; 0.31]
p-value	0.607	0.119	0.255	0.009	0.195
Effect in %	-7.57	-21.21	-12.29	-35.76	-23.40
N (left)	2,045	2,045	2,045	2,045	2,045
N (right)	1,646	1,646	1,646	1,646	1,646
(E) Dropouts, 29					
Effect at 29	0.58	1,599.39	-0.02	-0.41	-0.25
CI	[-1.67; 2.82]	[-10,642.33; 13,841.11]	[-203.12; 203.07]	[-2.56; 1.73]	[-1.51; 1.01]
p-value	0.609	0.795	1.000	0.702	0.692
Effect in %	6.99	4.00	-0.00	-8.15	-7.64
N (left)	2,025	2,025	2,025	2,025	2,025
N (right)	1,666	1,666	1,666	1,666	1,666
(F) Graduates, 29					
Effect at 29	0.09	-1,572.30	-23.08	-0.20	-1.19
CI	[-2.94; 3.12]	[-15,878.16; 12,733.56]	[-290.94; 244.77]	[-2.27; 1.87]	[-3.71; 1.33]
p-value	0.954	0.827	0.864	0.847	0.349
Effect in %	0.96	-3.28	-2.33	-3.47	-20.51
N (left)	1,763	1,763	1,763	1,763	1,763
N (right)	1,038	1,038	1,038	1,038	1,038
(G) Dropouts, 30					
Effect at 30	0.39	474.16	23.39	-0.09	-0.16
CI	[-1.38; 2.16]	[-8,696.98; 9,645.31]	[-129.42; 176.20]	[-1.38; 1.20]	[-1.59; 1.26]
p-value	0.659	0.918	0.760	0.885	0.819
Effect in %	5.35	1.29	2.83	-1.96	-4.78
N (left)	1,967	1,967	1,967	1,967	1,967
N (right)	1,601	1,601	1,601	1,601	1,601
(H) Graduates, 30					
Effect at 30	-0.70	-6,148.58	-11.40	-0.53	-0.66
CI	[-3.08; 1.68]	[-25,116.82; 12,819.67]	[-242.77; 219.97]	[-3.25; 2.19]	[-2.57; 1.26]
p-value	0.556	0.518	0.921	0.696	0.493
Effect in %	-7.40	-12.14	-1.13	-8.65	-13.63
N (left)	1,546	1,546	1,546	1,546	1,546
N (right)	931	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 of entry-level. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.