Long-Term Effects of Hiring Subsidies for Unemployed Youths—Beware of Spillovers*

Andrea Albanese[a,b,c,d,e], Bart Cockx[b,c,d,f,g], Muriel Dejemeppe[c,e]

[a] *Luxembourg Institute of Socio-Economic Research (LISER), Luxembourg*

[b] *Department of Economics, Ghent University, Belgium*

[c] *IRES/LIDAM, UCLouvain, Belgium*

[d] *IZA, Bonn, Germany*

[e] *GLO, Essen, Germany*

[f] *CESIfo, Munich, Germany*

[g] *ROA, Maastricht University*

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Abstract

We use (donut) regression discontinuity design and difference-in-differences estimators to estimate the impact of a one-shot hiring subsidy targeted at 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, because they substitute private for public and self-employment, overall employment does not increase but is still better paid. For high school dropouts, no persistent gains emerge. Moreover, the neighboring attraction pole of Luxembourg induces a complete deadweight near the border.

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

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1 Introduction

It is well known that economic recessions generally affect the labor market position of young people more strongly than that of adults and that they can have long-lasting negative impacts on their careers (see, for example, Cockx, 2016; von Wachter, 2020, 2021). Finding appropriate policy responses to counter these impediments to successful careers is therefore listed high on the policy agenda (OECD, 2020). In this research, we study the effects on various labor market outcomes, both in the short and long run, of a very generous one-shot hiring subsidy targeted at low- and medium-skilled unemployed youths during the early recovery from the Great Recession in Belgium.

In December 2009, the Belgian government unexpectedly implemented a new hiring subsidy that entered into effect on the 1st of January 2010 for a limited period of two years. This scheme, called the *Win-Win Plan*, targeted unemployed youths with at most a high school degree. A firm hiring an unemployed high school dropout (graduate) younger than 26 was eligible for a monthly subsidy of €1,100 (€1,000), which was on average 48% (46%) of wage costs. The subsidy was granted for two years for a hiring in 2010 and for one year in 2011. Even though hiring subsidies existed pre-reform and the Win-Win plan also comprised subsidies for the long-term unemployed, the reinforcement of the subsidy rate for these youths was still substantial, as it implied a 19 (13) percentage points reduction of wage costs. It is this jump in the subsidy rate for youths just below the age threshold of 26 years that we exploit to identify the impact of the Win-Win subsidy on labor market outcomes. We apply a (donut) regression discontinuity analysis (Barreca et al., 2016) on a large sample drawn from the social security register data of unemployed individuals living in the southern part of Belgium. 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 persists beyond the end of the subsidy. Seven years after entry into unemployment, high school graduates accumulated 2.8 more quarters in employment, on average, and all of this additional employment is in high-paid jobs (above the median daily wage). However, the positive long-run effect for high school graduates is much smaller and less of a net gain for society once we take two negative spillover effects into account.

First, in the long run, the positive effects on private sector employment for high school graduates are counterbalanced by negative effects on public sector employment and self-employment. Consequently, the subsidy only enhances the recruitment of workers who would have found other jobs anyway. Nevertheless, we report some suggestive evidence that, in the long run, private sector employment is better paid and of higher quality. Second, we find evidence of a negative geographic spillover on the effectiveness of the hiring subsidy that is induced by the proximity of the economic pole of Luxembourg. Still, a cost-benefit analysis suggests that in the long-run there is no net public cost of the subsidy, in particular for high school graduates.

Our contribution to the literature is threefold. First, we contribute to the literature on the effectiveness of hiring and wage subsidies during and after their implementation. Within the canonical tax incidence model, which assumes a perfectly competitive labor market wage, subsidies can only increase employment if labor supply and demand are sufficiently elastic (Katz, 1996). In the early empirical literature, a consensus arose that labor supply is fairly inelastic. Therefore, a wage subsidy (or payroll tax reduction) is an ineffective instrument to boost employment, and instead just increases wages.[1] Recently, several studies have challenged this prediction from the canonical tax incidence model.[2] The influential study of Saez et al. (2019) is particularly relevant for our research. It demonstrated that payroll tax cuts targeted at youths in Sweden did not affect wages of the target group relative to other workers and that they substantially raised youth employment.[3] However, that study focused on an employment subsidy targeting all young employees, irrespective of their skill level, and not only new hires, which may induce a strong deadweight (Neumark, 2013),[4] and is, therefore, an expensive way of boosting employment. Furthermore, in contrast to our study, the subsidy was meant to boost the labor market integration of youths structurally and not as a temporary measure following an economic shock.

A study closer to our paper is Cahuc et al. (2019), who evaluate the introduction of a hiring subsidy during the Great Recession in France. These authors show that this policy was particularly effective in creating additional employment because it was (i) not anticipated, (ii) one-shot, and (iii) targeted at low-paid jobs close to the minimum wage. The Win-Win subsidy shares these features, with the exception that it is targeted at low-educated unemployed youths rather than at low-paid jobs in general. Because the Win-Win plan was particularly aimed at improving the labor market position of disadvantaged youths rather than boosting overall employment in the economy, we contribute to the literature on the effectiveness of youth employment programs.[5]

[1] See, for example, Gruber and Krueger (1991), Gruber and Krueger (1994); Gruber (1997), as well as Anderson and Meyer (1997, 2000).

[2] See, for example, Saez et al. (2012, 2021); Bozio et al. (2019); Westerberg (2021); Kim et al. (2022); Carbonnier et al. (2022).

[3] See also Skedinger (2014); Egebark and Kaunitz (2018).

[4] Furthermore, Neumark and Grijalva (2017) shows that hiring subsidies adopted in the US during the Great Recession were made more effective in boosting job growth by targeting them at the unemployed or by making them refundable when job-creation goals were not met. On the other hand, this research also reports that these hiring subsidies may generate more hiring than net employment growth because they encourage churning of employees.

[5] Blundell et al. (2004) study the effect of a hiring subsidy targeted at long-term unemployed youths, but they cannot disentangle its effect from the extensive counseling with which the subsidy was combined. Other studies can be criticized on similar grounds (Caliendo and Schmidl, 2016). Other researchers, such as Schünemann et al.

Second, we add evidence to the scarcer literature on the long-term effects of payroll tax cuts and hiring subsidies after their repeal. Saez et al. (2021) show that the positive effect on employment persists up to three years after the tax cut is no longer in place, or when the worker has aged out of her eligibility. Similarly, Batut (2021) evaluates the effect of the aforementioned hiring subsidy studied by Cahuc et al. (2019) and finds that the positive employment effects extend up to two years after its end. Sjögren and Vikström (2015) show that hiring subsidies targeted at the long-term unemployed continue to enhance the probability of employment after their expiration, although less strongly than before. We study the impact on labor market outcomes up to at least five years after the expiration of the subsidy. Considering these long-run effects also makes a cost-benefit analysis much more informative. We are not aware of any study that has conducted a cost-benefit analysis for hiring subsidies considering such a long-term perspective. We also investigate whether the level of education of the target population matters for the effectiveness of these subsidies.

Our findings show that the hiring subsidies are not effective for the long-term labor market integration of high school dropouts, which is in line with earlier literature concluding that "work-first" policies are not effective for low-skilled workers because the skill requirements of the jobs in which these workers end up are too limited to cause significant human capital accumulation on the job.[6] These findings have been challenged by several studies.[7] However, Autor et al. (2017) argue that by focusing on average effects some of these studies may mask considerable effect heterogeneity and high rates of 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.[8] In line with these nil-effects for disadvantaged workers, Cahuc et al. (2021) find based on a correspondence study that for high-school dropouts in France, past employment experience—whether it is subsidized or not—does not increase the callback rate if there has been no on-the-job training accompanied by skill certification. Our results for dropouts are in agreement with this study. In contrast, our finding that the positive employment and pay effects of the Win-Win hiring subsidy persist beyond the expiration of the subsidy for

(2015), Sjögren and Vikström (2015), Ciani et al. (2019), and Pasquini et al. (2019), have studied the effects of hiring subsidies targeted at the long-term unemployed, but these subsidies were not one-shot and they did not target youths.

[6] See, for example, Meghir and Whitehouse (1996); Card and Hyslop (2005); Blundell (2006).

[7] See Bloom et al. (2005); King and Mueser (2005); Dyke et al. (2006); Autor and Houseman (2010); Pallais (2014); Brewer and Cribb (2017); Riddell and Riddell (2020).

[8] The meta-study by Card et al. (2018) also finds evidence for treatment heterogeneity across different target groups. While they report that "work first" programs are relatively less effective than "human capital" programs for the long-term unemployed, the opposite holds true for disadvantaged workers, a finding which does not accord with the evidence of Autor et al. (2017). For youths, the effects are generally smaller than for adults, and the relative effects are not very different across program types (Ibid, pp. 923-924).

high-school graduates suggests that "work first" policies can be effective for medium-skilled youth. This could reflect that human capital only accumulates with experience in jobs in which the skill requirement exceeds some minimum threshold. Such an interpretation is reinforced by the finding that only for high school graduates the subsidy results in more hiring and work experience in larger firms. Recently, Arellano-Bover (2022) indeed demonstrated 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. We confirm this conclusion for medium-skilled youths, but not for high school dropouts.

Third, we contribute to the broader literature on displacement and spillover effects. Crépon et al. (2013) have shown that displacement at the expense of untreated unemployed can reduce the effectiveness of job-search assistance programs targeted at young (educated) jobseekers in France.[9] If such an externality is ignored, this can lead to a substantial overestimation of the program's impacts. However, the recent literature evaluating the effectiveness of hiring subsidies does not find much evidence for negative spillovers on non-participants.[10] Evaluating one-shot hiring credits for low wages workers during the Great Recession in France, Cahuc et al. (2019) detect no displacement effects, neither at the expense of incumbent workers nor to the detriment of ineligible firms. Furthermore, researchers typically find very little evidence of substitution away from ineligible groups in programs targeted at youths (see, for example, Blundell et al., 2004; Kangasharju, 2007; Pallais, 2014; Webb et al., 2016).[11] In line with this more recent literature, we do not find any evidence that the hiring subsidy would displace the employment of ineligible slightly older workers.

We find evidence of two negative spillover effects of different types. The first is a displacement that is only realized for eligible high school graduates in the long run and which is at the expense of public sector employment and self-employment. In the absence of the subsidy, these eligible workers would have accumulated work experience of a similar duration outside of the private sector. However, we also find that private sector jobs are higher paying than those in the public sector. The subsidy makes hiring in the private sector relatively more attractive than elsewhere, and private sector experience produces more marketable skills and, hence, higher wages.

Second, we find that geographic spillovers can reduce the effectiveness of the hiring subsidy as the policy is a complete deadweight loss within one hour of driving from the border with the economic pole of Luxembourg. Our data are sampled from a population living close to

[9]For further evidence of such substitution effects for job-search assistance programs, see Ferracci et al. (2014); Gautier et al. (2018); Cheung et al. (2019).

[10]Older studies have typically found that subsidized employment generates more important displacement effects for non-participants than other active labor market policies (see, for example, Dahlberg and Forslund, 2005). However, this evidence is typically based on less credible identification strategies.

[11]Saez et al. (2021) find *positive* spillover effects for workers who were previously exposed to the tax cut after they age out. We do not label these as "spillover effects" but, rather, as "long-run" effects after the expiration of the subsidy. In line with the recent evidence, they could not detect negative spillovers in untreated (slightly younger) cohorts (Ibid, footnote 14).

Luxembourg, which is a small country neighboring Belgium that is characterized by much better employment opportunities and higher wages, notably because of its favorable tax regime.[12] Together with the absence of language and legal barriers,[13] this attracts a large number of cross-border workers. In 2010 38,000 Belgian residents (11% of the workforce in Luxembourg) commuted every day to Luxembourg.[14] Through this massive amount of cross-border work, the labor market tightness in Luxembourg spills over to the neighboring region in Belgium. In such a tight labor market, each new job opportunity in Luxembourg crowds out another less attractive one over the border, making the hiring subsidy a complete deadweight loss on that side.

To the best of our knowledge, the literature has not investigated to what extent the effectiveness of an employment policy in one country can be unintentionally reduced by the proximity of a tight labor market across a country's border. 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 surveys). Our findings can be related to the theoretical prediction of Kline and Moretti (2013) within an equilibrium search model that place-based hiring subsidies are counterproductive in regions where the labor market is too tight. We find empirical support for this theoretical prediction in a region where labor market tightness spills over from an economic attraction pole nearby but across the country's border.

The 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.[15] This plan involved generous *one-shot* subsidies available for recruitment during two

[12]This paper is part of a larger research grant aimed at analyzing the determinants of cross-border employment from Belgium to Luxembourg, which was funded by the National Research Fund of Luxembourg (FNR – code: c17/SC/11700060/CrossEUwork).

[13]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.

[14]Together with the French and the German commuters, today cross-border workers amount to 46% of Luxembourg's workforce (Statec, 2022).

[15]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, which can be found at `https://drive.google.com/file/d/1FXFo7FP1q7HS8UFBydPlzAlJgKhlOSdm/view?usp=sharing`.

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, inducing 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 22.0%, while it was only 6.6% for the group aged 25-74 (Eurostat, 2022a).

In this paper, we evaluate the impact of the Win-Win subsidies targeted at 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).[16] 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 (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	minimum 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	minimum 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	minimum 12 months	18 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).[17] The employer

[16]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.

[17]Eligibility for the Win-Win subsidy did not require jobseekers to receive benefits during these periods of unemployment. However, 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

must then draft an Online 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 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 long-term unemployment benefit recipients.[18] 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.

As the Win-Win plan did not generate a much higher subsidy for hiring long-term unemployed individuals compared to the pre-existing Activa subsidy, we focus our empirical analysis on the effects of Win-Win on short-term unemployed youths with at most a high school degree. However, as explained in the next section, the data do not allow us to precisely delineate between the short-term and long-term unemployed. In the analysis, we take into account that the control group is partially eligible for a lower hiring subsidy.

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.[19] The initial SSC reduction amounted to €333/month for both groups, but after a while, it decreased, first to €133/month and then to €0. The pace of this decrease depended on the target group. High school dropouts were only eligible for the reduction in 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.

The Win-Win plan was the onset of an unprecedented decline in the cost of hiring low-educated youths. The subsidy amounted to 46% of wage costs, on average, for high school graduates marginally younger than 26, and 48% for dropouts.[20] At the age discontinuity thresh-

jobseeker during these periods. This complicates the procedure.

[18] 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.

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

[20] Wage costs are measured as the gross wage before taxes plus the employer SSC *net* of the aforementioned

old of 26, the subsidy amount increased relative to wage costs by only 13 percentage points (pp) for high school graduates and by 19 pp for dropouts, because some unemployed individuals older than 26 were also eligible for Activa or Win-Win subsidies targeting at the long-term unemployed.[21] The regression discontinuity design (RDD) exploits these jumps in the subsidy rate to identify the causal impact of the subsidy on various labor market outcomes.

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 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.[22] According to Eurostat (2022b), the Belgian Province of Luxembourg was the NUTS-2 region in the EU with the highest incidence of outgoing cross-border workers out of the employed population: 25% in 2010. Within this area, cross-border work is 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). As explained in the Introduction, 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 the border in Luxembourg.

In this sample, we retain first registrations as unemployed jobseekers at the public employment service (PES) between 2007 and 2012. The benchmark analysis in this paper is conducted on 9,935 young adults between the ages of 22 and 29 having at most a high school degree. Since

reductions. The shares are calculated within the sample of analysis described below for youths aged between 18 and 26 years at entry into unemployment in 2010, hired within one year, and for whom the employer was entitled to the Win-Win subsidy.

[21] See Figure A.2 in Online Appendix A for the corresponding graphs depicting these regression discontinuity design (RDD) estimates.

[22] Individuals living in municipalities with a higher incidence of cross-border work, generally closer to the border of Luxembourg, and those registered as unemployed jobseekers were over-sampled to enhance precision for these groups. The data are appropriately reweighted to take this stratification into account and be representative of the population of interest (Manski and Lerman, 1977; Cameron and Trivedi, 2005; Albanese and Cockx, 2019). Details can be found in Online Appendix B.

Win-Win was abolished by the end of 2011, we only retain unemployment spells that started in 2010, to include only treated individuals who do not lose treatment eligibility within the first year of unemployment.

We follow these young adults from the start of their unemployment spell and do *not* impose the eligibility condition that high school dropouts (graduates) are only eligible if they have been unemployed for a minimum of 3 (6) months in the last 4 (9) months (see Table 1). This is because our data do not measure unemployment duration with sufficient precision to impose this condition.[23] As a consequence, we can only identify *intention-to-treat* effects, and *not* average treatment effects of subsidy eligibility on the treated. Another consequence is that some individuals retained in the sample may be hired with a Win-Win subsidy before 3 (6) months of unemployment according to our definition, because these workers may have experienced unregistered unemployment that counts for subsidy eligibility but which was realized before the unemployment spell retained in the analysis.

To investigate the presence of displacement effects on ineligible individuals, we consider 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 also include entries into unemployment before 2010 and after 2011.

We consider several outcomes in the analysis, which can be grouped as exit rates to employment during the first quarters of unemployment and accumulated employment outcomes up to seven years later.[24] 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 employment *other* than salaried private sector employment as an outcome.

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.

As shown in Table 2, about 20% of eligible 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. In contrast, other outcomes are different. 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

[23]The data inform us only about registration status as an unemployed jobseeker at the end of each month. An unemployment spell starts in month t if one is registered as an unemployed jobseeker at the end of month t but not at the end of the preceding month, $t-1$. Short interruptions within a calendar month can therefore not be identified. Moreover, the duration of unemployment is computed based on how much time persons had been unemployed or assimilated to unemployment. We cannot infer unambiguously from the data periods assimilated to unemployment.

[24]See Online Appendix C.1 for a more detailed description of the outcomes.

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, the higher participation in the salaried private sector 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.

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 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 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

To estimate the causal impact of the Win-Win subsidy on the employment trajectories of eligible individuals, we exploit two eligibility conditions: age and time. Indeed, only unemployed individuals younger than 26 and recruitments in 2010 or 2011 are eligible for the Win-Win plan. 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 slightly older than 26 can be eligible for other *lower* hiring subsidies, such as *Activa* or Win-Win targeted at the long-term unemployed. This means that the counterfactual of eligibility for the hiring subsidy is not the absence of eligibility but the partial eligibility for lower hiring subsidies.

We cannot implement a standard RDD using age at unemployment entry as the forcing variable because the age cutoff of the subsidy is determined at hiring, which does not take place instantaneously at the start of unemployment. Youths slightly younger than 26 at unemployment entry will therefore immediately age out of eligibility. We solve this issue by implementing a so-called *donut* RDD (Barreca et al., 2016). For outcomes measured one year or more after entry into unemployment, we ignore the "partially" eligible units who are aged between 25 and 26 at entry and therefore age out of eligibility within the first year after entry. This selection creates a "hole" on the left of the discontinuity, which is filled by the prediction of the linear spline estimated using the data points to the left of the "hole".[25] This extrapolation allows us to identify the intention-to-treat effect for individuals registering at the age of 26.

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 close 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. This shows that the results are robust.

Finally, to take into account that the running variable, age, is grouped in monthly intervals, we cluster the standard errors by age in months (Lee and Card, 2008). In the benchmark analysis, this defines 72 clusters. The units are reweighted by their sampling weights to make inference on the population. Formally, the donut RDD estimates the following linear regression:

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

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., 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;

[25] For outcomes measured before 1 year, we shrink the "hole" to increase precision.

- $\mathbb{1}(z_i^0 \geq 26)$ 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^0 is the idiosyncratic error term (with zero conditional mean);
- Observations are reweighted by weights multiplying the sampling and the triangular kernel weights.

As previously mentioned, by applying the donut RDD the treatment effects are no longer completely non-parametrically identified. We, therefore, check that our findings are robust to a series of sensitivity analyses described in more detail in Section 5.2. Some of these analyses are based on the doubly robust conditional difference-in-differences (DiD) estimator proposed by Sant'Anna and Zhao (2020). This estimator compares the outcomes of the unemployed aged 24-25 to those aged 26-27 before and after the policy reform. The reader can find more details on the DiD estimation strategy in Online Appendix D.

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 slightly 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.

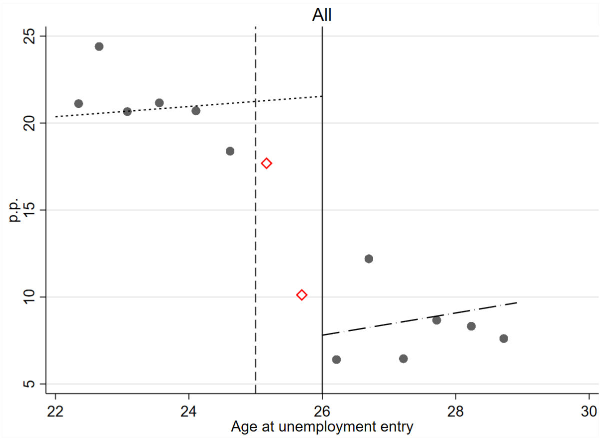
5.1 Main results

5.1.1 Impact on Salaried Employment Outcomes in the Private Sector

Transition Probabilities in the Short Run

We first present the impact of the Win-Win subsidy in the short run, i.e., within one year of entry into unemployment. In Figure 1, we illustrate the differential cumulative take-up of hiring subsidies as estimated by the *donut* RDD described in Section 4. As expected, the two red dots that represent the average take-up of Win-Win in the partially treated age range are substantially lower than the take-up rates for youths below the age of 25.

Figure 1: Discontinuity at Age 26 of the Cumulative Take-Up Rate of Hiring Subsidies Within One Year



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 take-up rate of hiring subsidies within one year, 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 . The effect estimated by the donut RDD estimator at 26 years old is +13.7 pp [5.2; 22.2] with a p-value of 0.002 and N = 8,560. Standard errors are clustered at the age level.

According to the linear spline, 21.5% of entrants into unemployment in 2010 who are slightly younger than 26 are hired into salaried private sector employment within one year with the support of a Win-Win subsidy. For those slightly older than 26 years of age, the fraction that is recruited and benefits from a hiring subsidy (i.e., Win-Win or Activa subsidy for the long-term unemployed) is only 7.8%. The differential take-up is therefore estimated to be 13.7 pp (significant at the 0.2% level). In Figure A.3 in Online Appendix A, we report the corresponding graphical analysis separately for high school graduates and dropouts. The differential take-up rate of the hiring subsidy is 17.5 pp for the former and 11.5 pp for the latter.

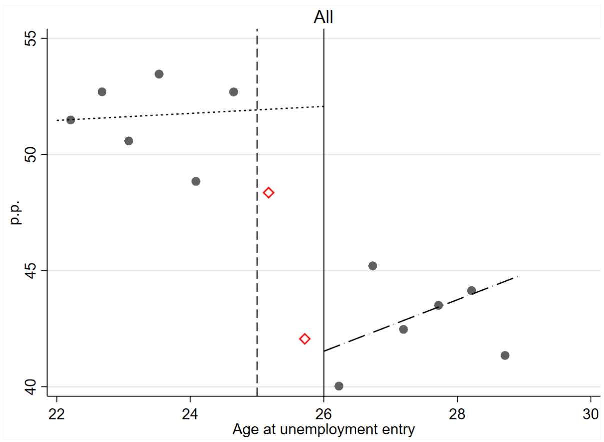
These differential take-up rates between the eligible and ineligible groups close to 26 years of age are a lower bound because they do not take into account that the average subsidy is almost half as large for those slightly older than 26.[26] We obtain the *adjusted* take-up rate by dividing the subsidy take-up by the education-specific generosity of Win-Win (i.e., either €1,000 or €1,100). Once we take these differences in generosity into account, the *adjusted* differential take-up rate increases from 13.7 pp to 17.3 pp for the full sample, from 17.5 pp to 20.3 pp for high school graduates, and from 11.5 pp to 15.7 pp for dropouts.

Figure 2 shows that the corresponding discontinuity at age 26 in the cumulative transition rate to private sector employment one year after entry into unemployment in 2010 is equal to

[26] In an RDD donut setting, we estimate the full-time amount of subsidy on the right of the cutoff to be 45% lower than on the left, a difference of €463/month. This amount is 41% for graduates and 49% for dropouts (€404/month and €525/month) (see Figure A.4).

10.5 pp (significant at the 0.7% level). This is evidence that the Win-Win subsidy has a clear positive impact on this transition rate. For high school graduates and dropouts, the corresponding point estimates are 8 pp and 13 pp (see Figure A.5 in Online Appendix A), which, in relative terms to the counterfactual, represent an increase of 15% and 40%, respectively. Results are less precise, especially for the graduates (p-value = 0.251 and 0.017, respectively). However, note that the smaller and insignificant effect for graduates is not representative. For instance, in quarters 3 and 6 from entry into unemployment the point estimates for graduates are larger (11 pp and 13 pp, 25% and 22% in relative terms) and significant at the 5% level, while for dropouts point estimates fall to 8 pp and 11 pp (28% and 31% in relative terms), remaining statistically significant (see Figure A.6 in Online Appendix A).

Figure 2: Discontinuity at Age 26 of the Cumulative Transition Rate to Private Sector Employment Within One Year



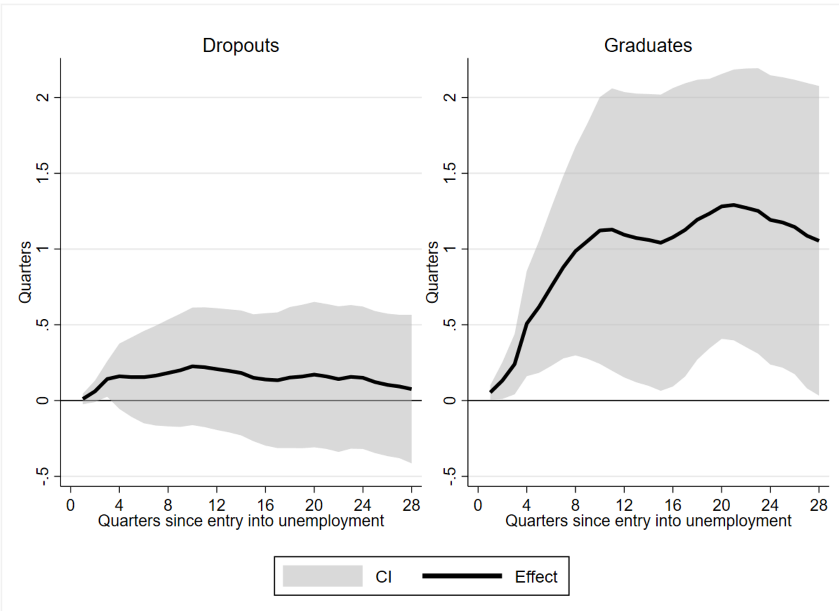
Note: Donut RDD estimate on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. The bandwidth is set at 3 years on each side of the donut. The outcome is the cumulative transition rate to private sector employment within one year, 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. The effect estimated by the donut RDD estimator at 26 years of age is +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.

Cumulative Effects in the Long Run

Figure 3 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. 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.7 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 can be partly explained by the lower differential take-up of the subsidy, which was estimated to be 50% higher for graduates (see above). However, most of this difference is due to the shorter duration of the subsidized employment. Consequently, while the absolute effect of the subsidy on the cumulative hiring rate is comparable for the two groups and the relative effect is higher for dropouts, the fact that subsidized employment tends to be of much shorter duration for dropouts already suggests that the employment effect in the long run must be small.

Figure 3: Evolution of the RDD Effect on the Cumulative Number of Quarters in Subsidized Private-Sector Employment



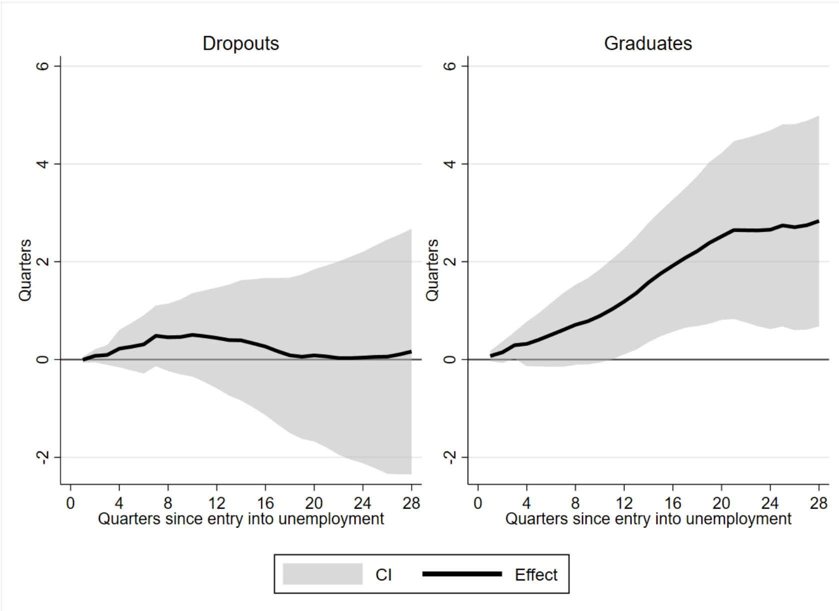
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the cumulative number of quarters in subsidized private sector employment by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

To test this hypothesis, we estimate the effect 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 4, 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, the hiring subsidy only accelerates the transition to short-term jobs and does not generate any persistent effect on employment.

In contrast, from Figure 4 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.[27] This is an increase of 28% relative to the counterfactual of lower hiring subsidies. 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.9, 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%).

[27] In Figure A.8, the corresponding RDD graphical evidence is reported seven years after entry into unemployment.

Figure 4: Evolution of the RDD Effect on the Cumulative Number of Quarters in Private Sector Employment

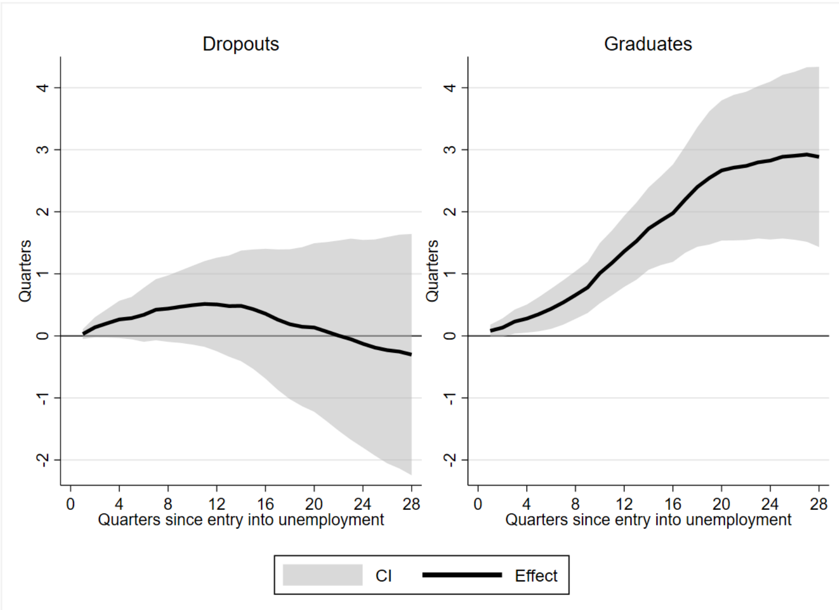


Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the cumulative number of quarters in private sector employment by schooling 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. 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.10 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 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 5 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.11 in Online Appendix A).[28]

Figure 5: 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. 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).

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

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

different from zero for high school dropouts. Importantly, it extends beyond the expiration of the Win-Win subsidy.[29] This suggests that this effect is not reflecting a partial incidence of the subsidy on the wage.[30] 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.[31]

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 large firms provide more learning opportunities from better peers and managers and a more productive environment. In line with this evidence, Arellano-Bover (2020) shows 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 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 6 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).[32] The patterns of these long-run effects are very similar to those displayed in Figure 5 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 contrast, the Win-Win subsidy does not stimulate large firms to hire high school dropouts, such

[29] 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 entry or later.

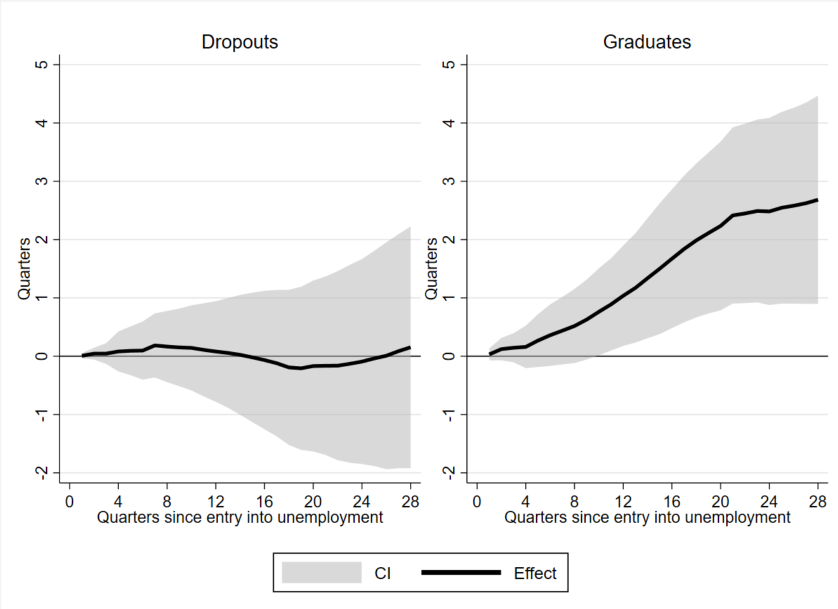
[30] An additional reason why this does not reflect an incidence effect is that the subsidy is awarded for a limited period and is targeted at a very specific group. If there is a windfall gain from the subsidy, within-firm pay equity concerns will induce a sharing of this windfall with other workers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

[31] 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).

[32] We use 50 employees as this 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.12 in Online Appendix A).

that no persistent effects emerge.[33]

Figure 6: 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. 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).

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. While it is a small country, it is also a very rich country. This is to a large extent related to the very low corporate and personal tax rate that has been in place for a long time. This has attracted many multinational companies and has 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 2020 the household disposable income per inhabitant in purchasing power parity was 31 percent higher

[33]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.13 in Online Appendix A).

in Luxembourg than in Belgium: €34,710 versus €26,401.[34] Due to this large economic asymmetry, 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, 2022b).

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 close to only half as high (11% versus 20%).[35]

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 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 Luxembourg. 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. The fact that in our data we find that the share of Belgian private sector employment is smaller close to the border with Luxembourg than farther away[36] and that those jobs tend to be in low-status occupations,[37] suggests 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 A.14 in Online Appendix A displays the evolution of the differential take-up rate of a hiring subsidy at age 26 within 60 minutes driving distance from the border with Luxembourg.[38] It can be observed that close to the border, one year after entry into unemployment the differential take-up of hiring subsidies is, for both high school dropouts and graduates slightly

[34] This is a lower bound of economic differences between the bordering regions of Belgium and Luxembourg since it also includes the richer northern region of Belgium (Flanders). Source: https://ec.europa.eu/eurostat/databrowser/view/sdg_10_20/default/table?lang=en.

[35] The share of cross-border workers 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.

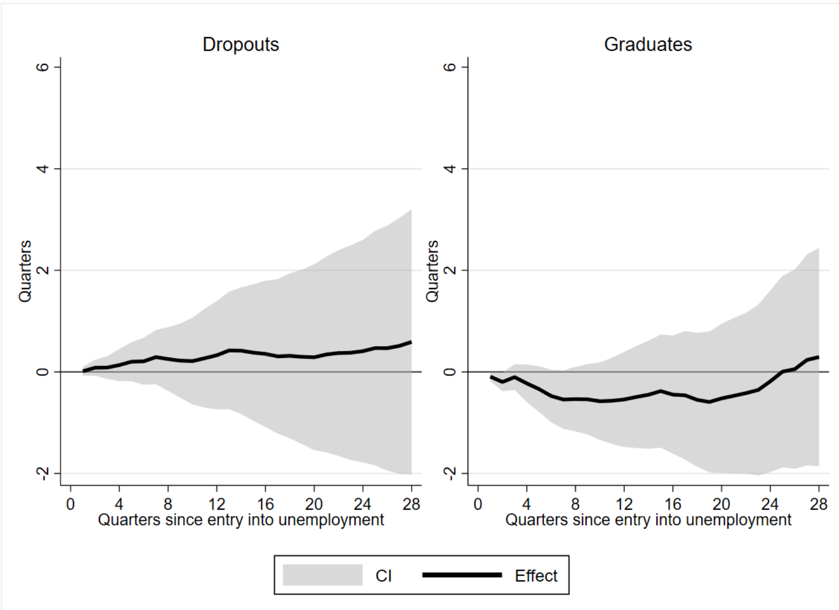
[36] In our data, this was 42% (61%) within (beyond) 60 minutes driving distance from the border for workers aged 25-34 in 2010.

[37] 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.

[38] We use a 60-minute threshold since this is the observed median value in our sample. Furthermore, as shown in Figure A.15, 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).

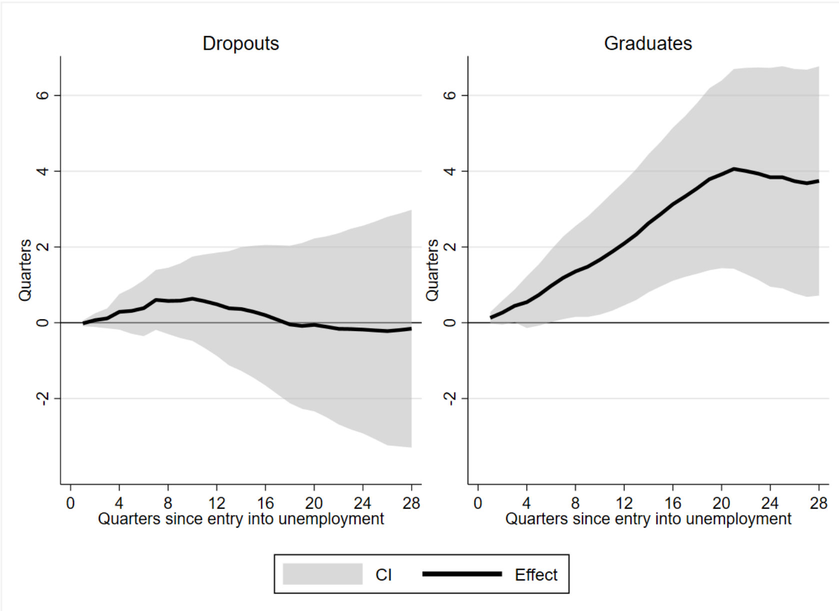
younger than 26 years of age, almost 15 pp higher than for those who are slightly older than 26. The long-run evolution of the RDD effect on the number of quarters in private sector employment is displayed in Figure 7. It can be seen that this effect is never significantly different from zero. This means that close to the border the hiring subsidy is, as expected, a complete deadweight. In contrast, for high school graduates slightly younger than 26 and living more than one hour's drive from the border, the differential take-up rate of the hiring subsidy is 20 pp, which increases the time spent in private sector employment by 3.7 quarters seven years after entry into unemployment (Figure 8). This represents a proportional increase of 38% relative to those slightly older than 26, and larger than the overall effect for the full population reported in Figure 4. For high school dropouts, the differential take-up rate is about 10 pp, while the effect on quarters in private sector employment is never significant, for the reasons explained when we reported the overall effect.

Figure 7: Evolution of the RDD Effect on the Cumulative Number of Quarters in Private Sector Employment – Close to the Border



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

Figure 8: 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).

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.16 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.17 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.

One could question whether the increasing effectiveness of the hiring subsidy with distance from the border could stem from a reduction in cross-border work: Youths living far from the border may prefer working closer to home and may give up the costly commuting time if new local job opportunities open up. We, therefore, 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. However, as can be seen in Figure A.18 in Online Appendix A, no significant reduction in cross-border work is found. The point estimates are actually more negative close to the border than far away.

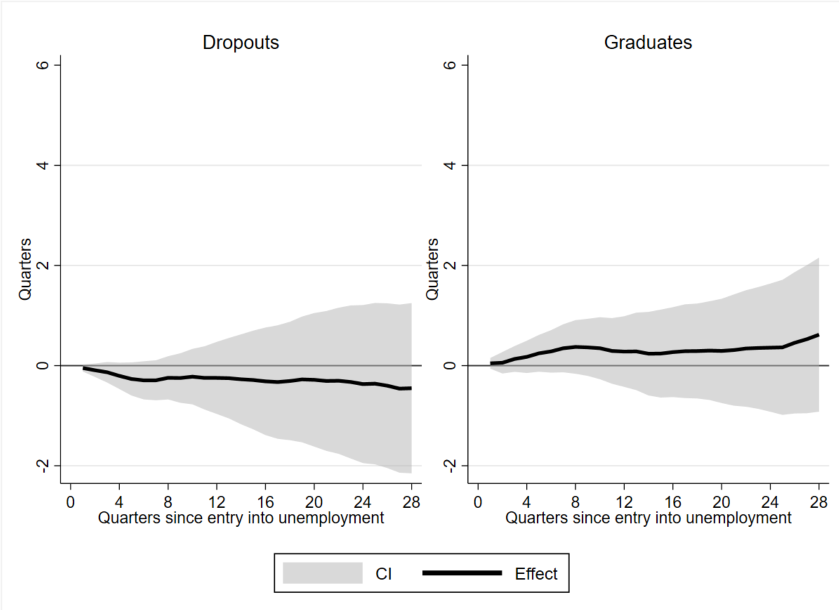
5.1.3 Displacement Effects

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, in particular for young 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?

To detect whether the positive effects of the Win-Win subsidy 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 Online Appendix D. 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 9). 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 10). 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 group 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.[39]

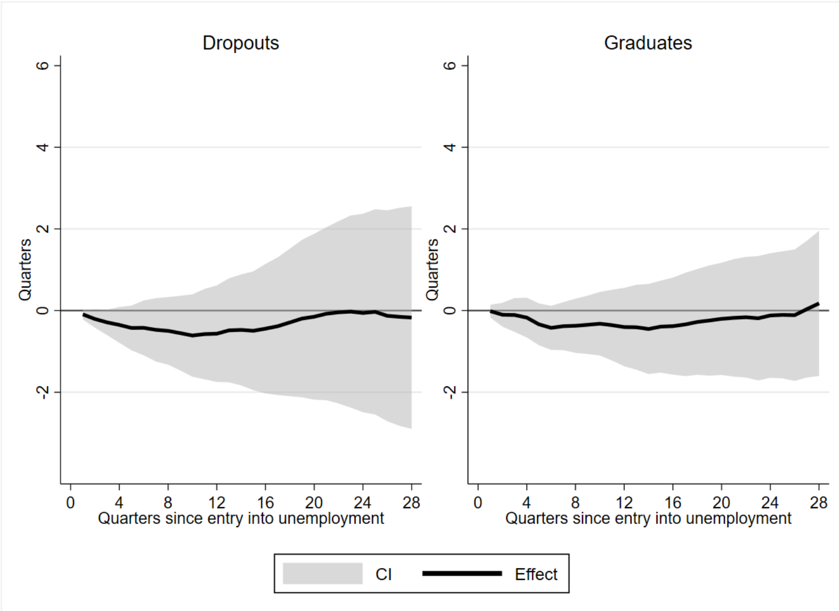
[39]These findings (available upon request) are robust to using different age ranges.

Figure 9: 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).

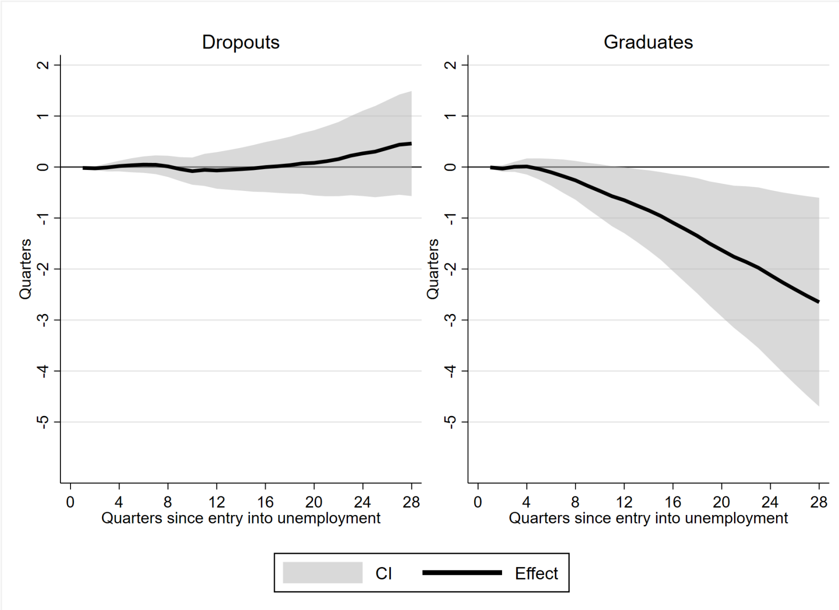
Figure 10: Evolution of the DiD Effect on the Cumulative Number of Quarters in Private Sector Employment: Cohort Aged 26-27 Living Far from the Border Compared to the Same-Age Cohort Living Close to the Border



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 11 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 employed in employment *other* than the private sector, i.e., public sector employment, self-employment, and cross-border employment, and Figure A.19 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 4 and A.8. The RDD effect on *other* employment declines initially, with some delay relative to the positive effect on private sector employment, but after 7 years is significantly negative and equal to –2.6 quarters. The overall effect on employment is therefore never significantly different from zero and is very close to zero after 7 years for both dropouts and graduates (Figure A.20 in Online Appendix A).

Figure 11: 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. 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).

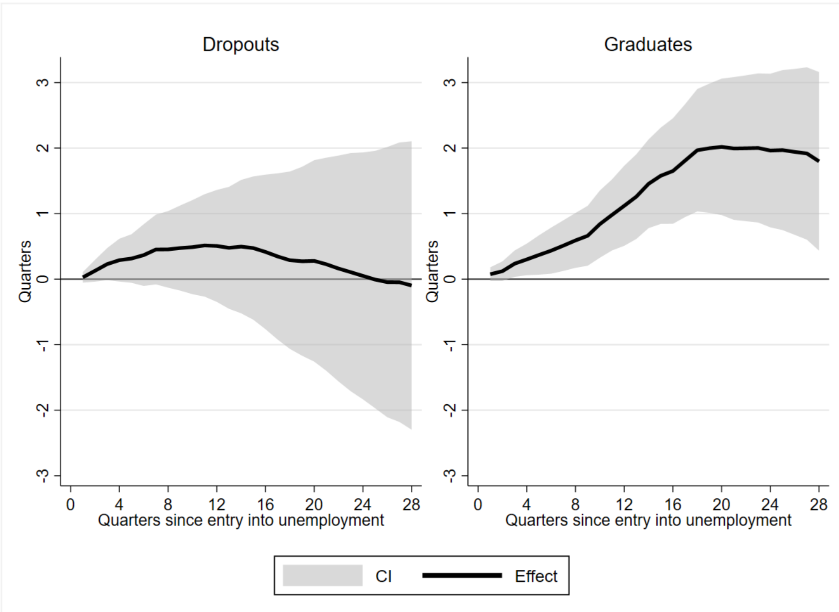
Because of a lack of data, we cannot measure the effect on earnings for self-employed and cross-border work. Nevertheless, since for high school graduates about 80% of the negative effect on *other* employment is due to public sector employment (Figure A.21 in Online Appendix A), it makes sense to consider the cumulative effect on earnings in the private or public sectors (Figure A.22 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.10) 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 12).[40] 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.23 and A.24 in Online Appendix A for the corresponding disconti-

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

nuity plot after 7 years). In addition, this group has accumulated one quarter less in low-paid jobs, but the effect is not significant (Figure A.25 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 employment.

In Section 5.1, we argued that the long-run effect on high-paid employment for high school graduates came about by an environment conducive to investments in human capital that large firms, hiring these graduates thanks to the subsidy, offer. 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.

Figure 12: 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. 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).

5.2 Robustness Analyses

In this section, we report a series of validation tests on the main outcomes.

First, we use a difference-in-differences (DiD) estimator to replicate the RDD estimates. The DiD estimator makes use of a pre-treatment period (unemployment registration in 2008)[41] to account for time-invariant unobserved heterogeneity between the treated (aged 24-25 at unemployment registration) and the control group (aged 26-27)[42] during the treatment period (unemployment registration in 2010). The main identifying assumption is that the counterfactual outcomes in the absence of treatment of the treated and the control group follow parallel trends. We relax the parallel trend assumption to hold only conditional on our predetermined control variables and implement the doubly robust Sant'Anna and Zhao (2020) estimator. A full description of the estimator can be found in Online Appendix D. The results are very similar to those obtained by implementing the RDD estimator and are shown in Figure A.26 in Online Appendix A.[43] Importantly, the DiD estimator also replicates the heterogeneous effects found for individuals living near or far from the border with Luxembourg (see Figures A.28-A.29 in Online Appendix A).

Second, we rerun the donut RDD estimator widening or narrowing the bandwidth. As shown in Figure A.30 in Online Appendix A, the results are close to the benchmark estimates. Third, we remove the conditioning variables from the RDD estimator and again obtain similar estimates (see Figure A.31 in Online Appendix A). Fourth, 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.32-A.35, 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. Despite having a different target population, estimates are similar to those for individuals aged 26 (Figure A.36 in Online Appendix A).

We also implement three sets of placebo tests for the donut RDD estimator. First, we estimate whether we can 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.37-A.44 in Online Appendix A and Table E.1 in Online Appendix E, 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.

[41] We do not use the unemployed entering in 2009 since some of these individuals immediately enter the post-treatment period in 2010.

[42] 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.

[43] We also implement a placebo test (see Figure A.27 in Online Appendix A) using an even earlier pre-treatment period, with the entries from 2007. No estimates are statistically significant, affirming the reliability of the parallel trend assumption.

5.3 Cost-Benefit Analysis

In this final section, we implement a cost-benefit analysis from the perspective of the government.[44] We, therefore, implement a similar donut RDD estimator, using as the outcome the average (cumulative) individual contribution to the net public revenues. 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. This estimator is implemented for each year after entry into unemployment in 2010 until seven years later, separately for high school dropouts and graduates. The following observations are important. First, note that we calculate the net public return of the differential subsidy amount of the treated compared to the control units at age 26 and *not* the net return of the full subsidy relative to the counterfactual of no subsidy. Second, the data do not allow us to calculate the tax losses induced by the negative impact on self-employment. This slightly overestimates the benefits.

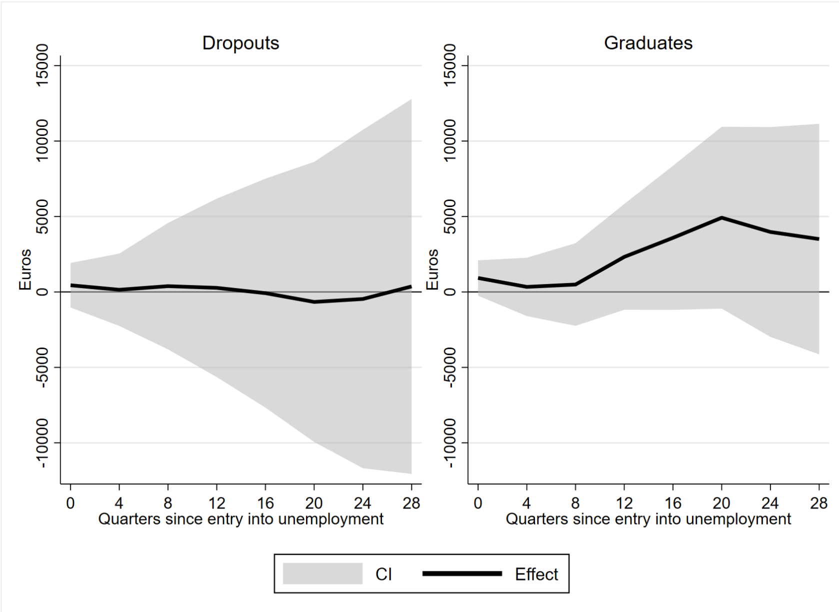
Figure 13 displays the evolution of the donut RDD effect on the cumulative net public return of the subsidy with the age cutoff of 26 years, from entry into unemployment until seven years later. For dropouts, this effect is close to zero throughout, while for high school graduates the effect is close 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.45 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 benefits at entry into unemployment.[45]

[44] 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.

[45] In a sensitivity analysis, we also implement the DiD estimator and find that the outcomes are robust (see Figure A.46 in Online Appendix A).

Figure 13: 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. 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.

6 Conclusion

In this paper, we evaluate the employment effects of a generous temporary 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 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 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 employment 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 for our target group private sector employment is better paid than public sector employment, 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 economic hub of Luxembourg across the border results in a complete deadweight loss for the creation of private sector employment in an area that lies within an hour's driving distance of this 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 has long-run positive effects beyond the subsidy period on high-pay private sector employment. Even if this private sector employment displaces public sector employment and self-employment, the evidence suggests that it is better paid. We argue that this better pay results from the fact that the subsidy stimulated the net hiring of graduates in large firms, which are more conducive to human capital investment (Arellano-Bover, 2020, 2022).

These findings suggest that policymakers could further improve the efficiency of the hiring subsidy by targeting it 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 requirement, such as high school dropouts, the hiring subsidy 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 the country border can weaken the intended effects, even in the long run. Finding an appropriate policy that can counter the long-term scarring effects on young people remains challenging but is certainly an important agenda for future research.

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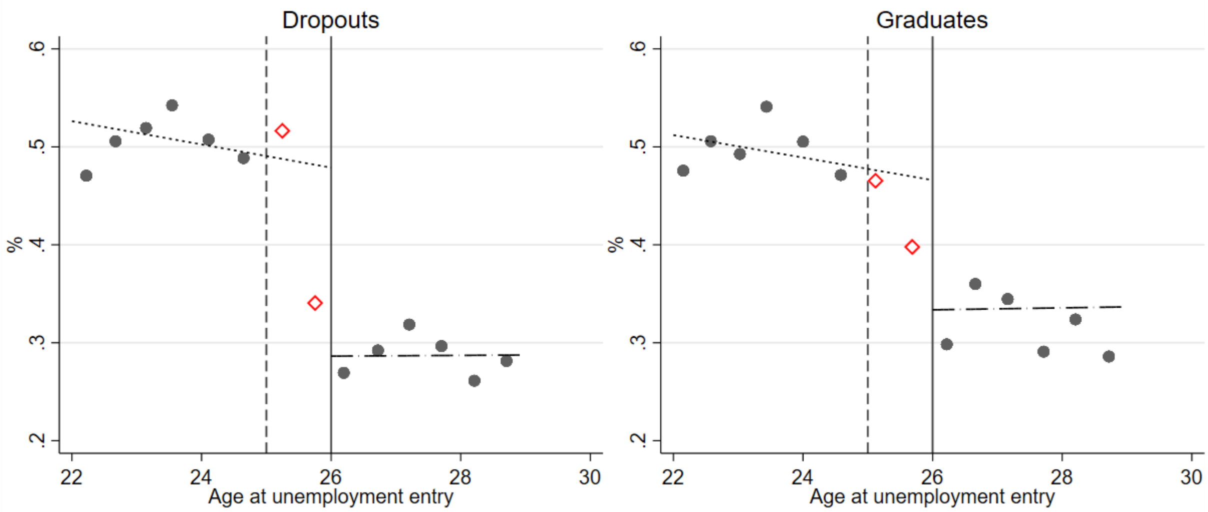
Appendix

A Figures

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

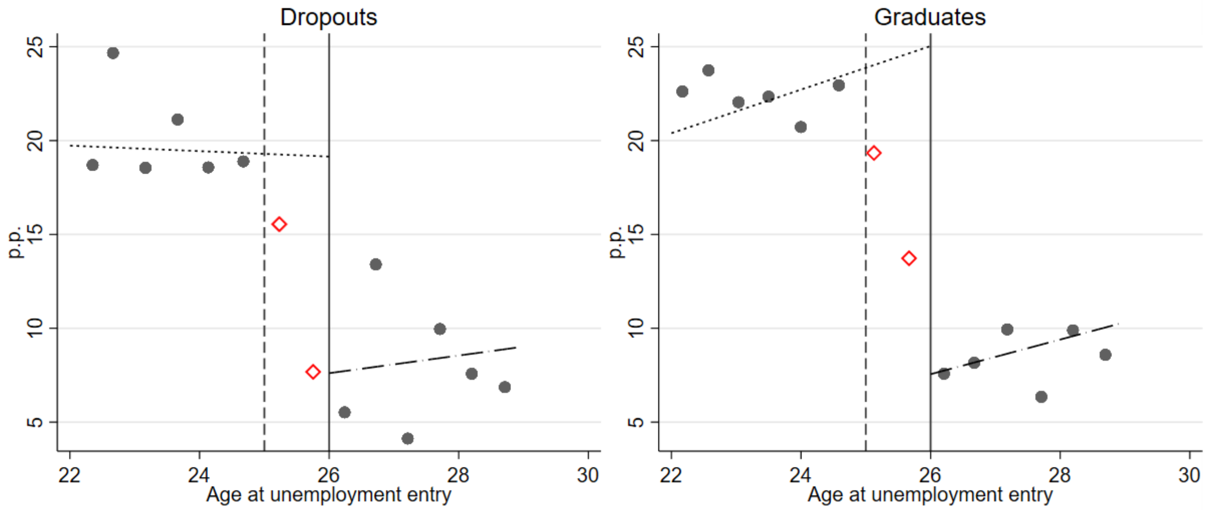
Google Trends search for “Plan Win-Win” from 1st of January 2009 until 31st of December 2010. The red line corresponds to the week before the 18th of January 2010.

Figure A.2: Discontinuity at Age 26 of Subsidized Wage Costs Conditional on Receipt of a Subsidy



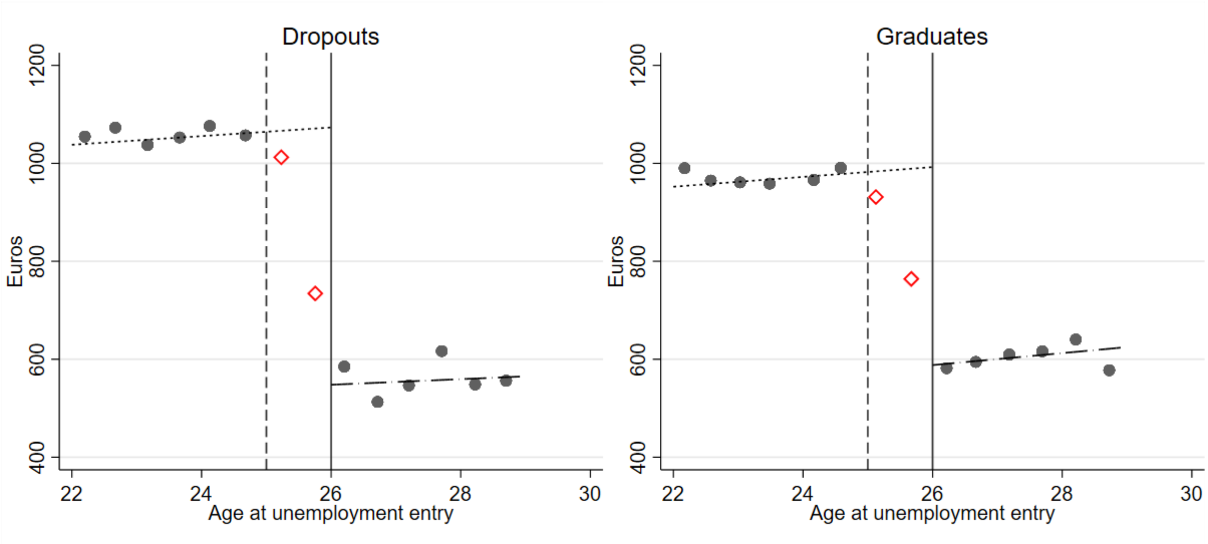
Note: Donut RDD estimate on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. The bandwidth is set at 3 years on each side of the donut. The outcome is the subsidized wage costs conditional on receipt of a subsidy by 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. Standard errors are clustered at the age level. The effect estimated by the donut RDD estimator at 26 years of age is +19.2 pp [13.2; 25.0] with a p-value of 0.000 and N = 4,176 for dropouts, while for graduates it is +13.2 pp [6.0; 20.4] with a p-value of 0.000 and N = 4,384.

Figure A.3: Discontinuity at Age 26 of the Take-Up Rate of the Hiring Subsidy Within One Year by Schooling Level



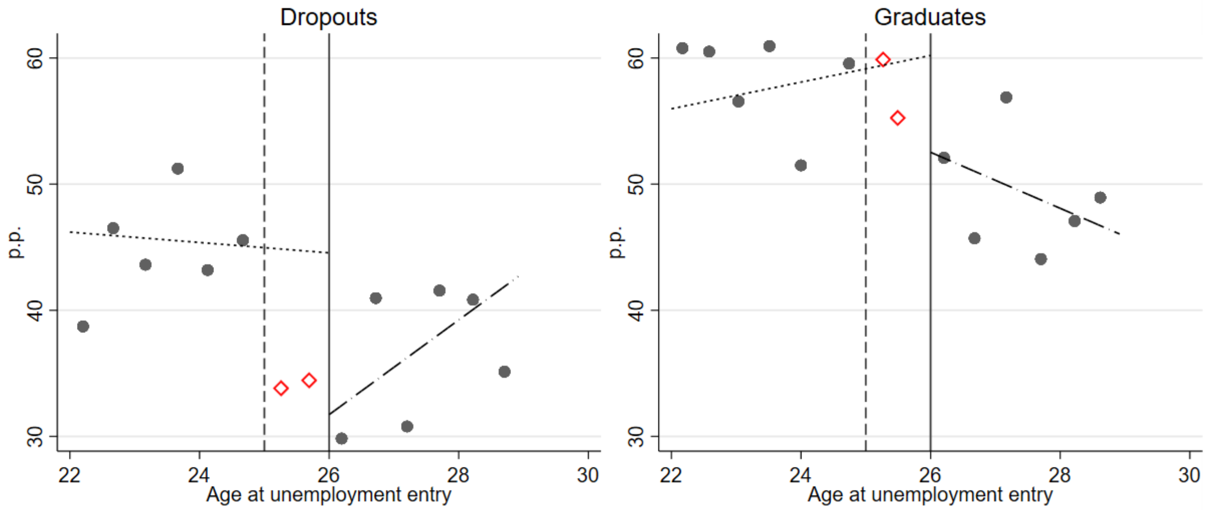
Note: Donut RDD estimate on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. The bandwidth is set at 3 years on each side of the donut. The outcome is the cumulative take-up rate of hiring subsidies within one year 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. Standard errors are clustered at the age level. The effect estimated by the donut RDD estimator at 26 years of age is +11.5 pp [–0.6; 23.7] with a p-value of 0.063 and N = 4,176 for dropouts , while for graduates it is +17.5 pp [7.1; 27.9] with a p-value of 0.001 and N = 4,384.

Figure A.4: Discontinuity at Age 26 of Monthly Amount of Subsidy Received Conditional on Receipt of a Subsidy



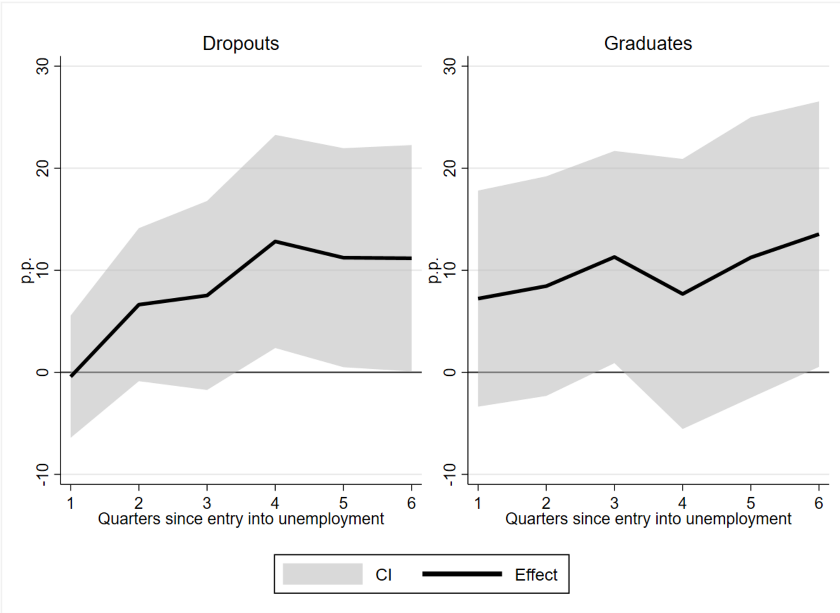
Note: Donut RDD estimate on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. The bandwidth is set at 3 years on each side of the donut. The outcome is the monthly amount of subsidy received conditional on receipt of a subsidy 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. Standard errors are clustered at the age level. The effect estimated by the donut RDD estimator at 26 years of age is +€525 [468; 582] with a p-value of 0.000 and N = 4,176 for dropouts, while for graduates it is +€404 [359; 450] with a p-value of 0.000 and N = 4,384.

Figure A.5: Discontinuity at Age 26 of the Cumulative Transition Rate to Private Sector Employment Within One Year by Schooling Level



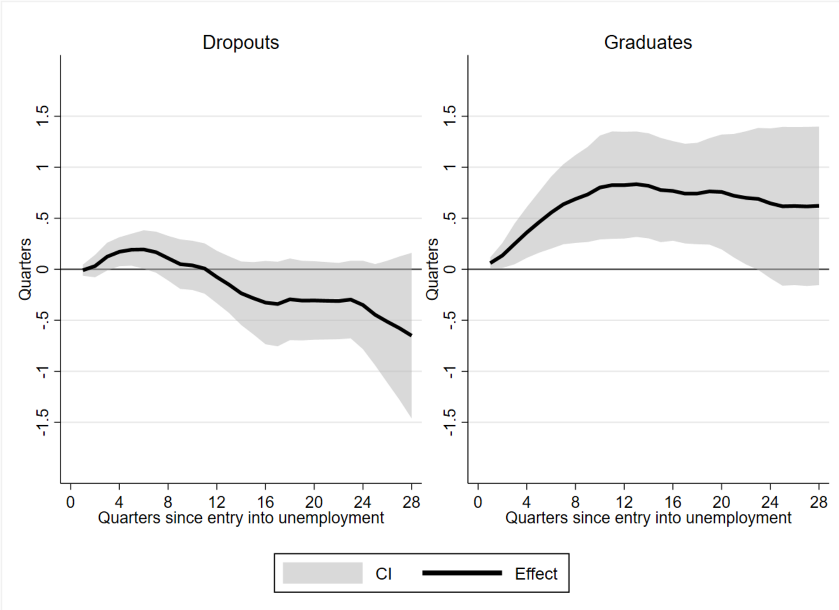
Note: Donut RDD estimate on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. The bandwidth is set at 3 years on each side of the donut. The outcome is the cumulative transition rate to private sector employment within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



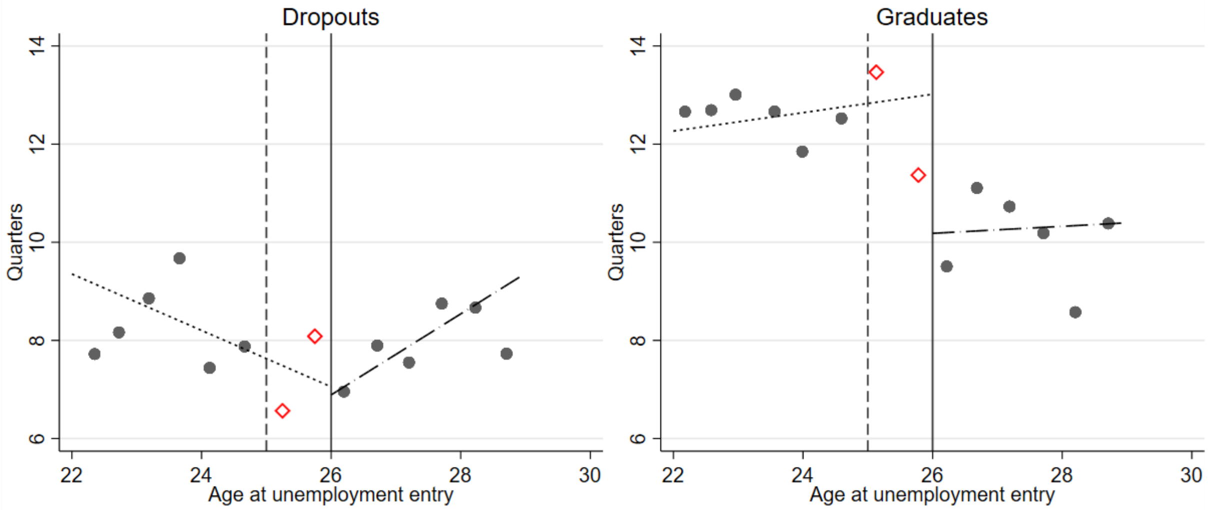
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the transition rate to the private sector up to 6 quarters after entry into 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. 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.7: Evolution of the DiD Effect on the Cumulative Number of Quarters in Subsidized Private-Sector Employment



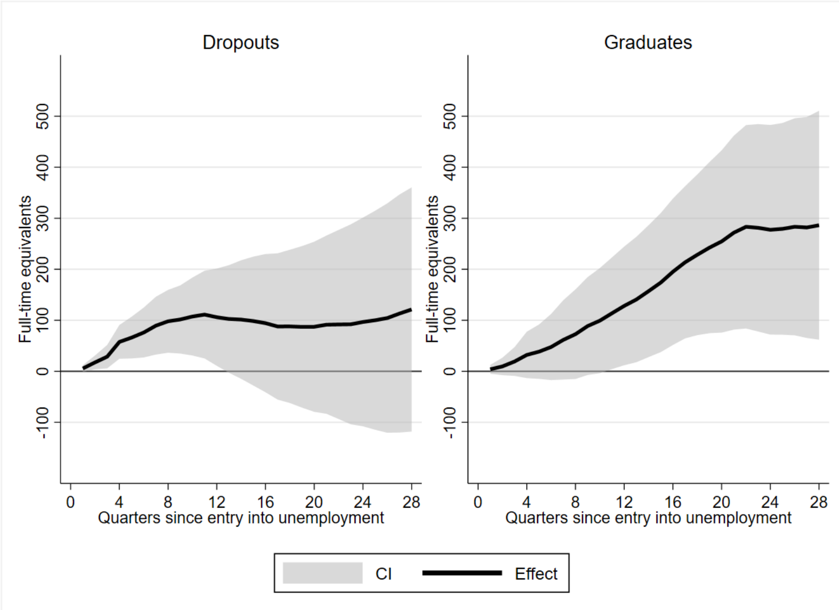
Note: Evolution of the effect estimated with a doubly robust DiD estimator (Sant'Anna and Zhao, 2020) and confidence interval (CI) for the cumulative number of quarters in subsidized private-sector employment by schooling level: dropouts (left) vs. graduates (right). The DiD estimator is implemented 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. 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.8: Discontinuity at Age 26 for the Cumulative Number of Quarters in Private Sector Employment Seven Years after Entry into Unemployment



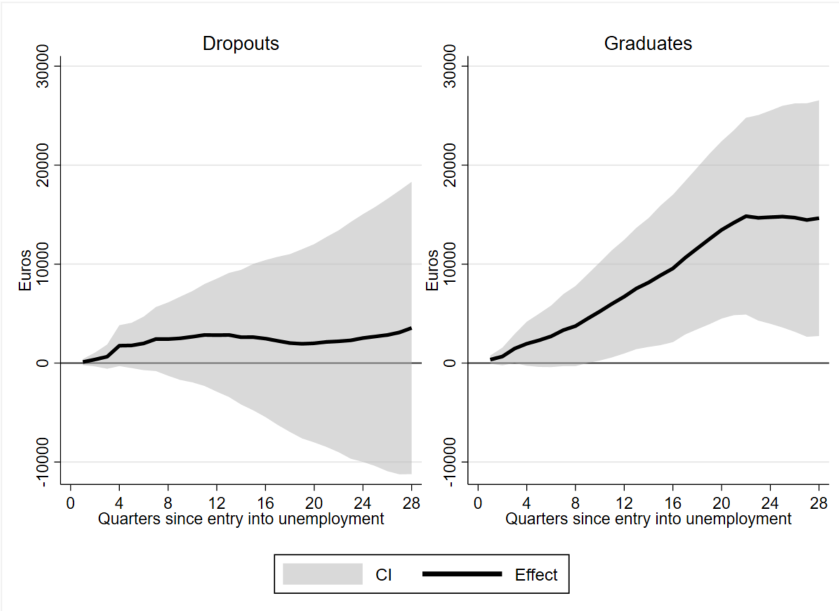
Note: Donut RDD estimate on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. The bandwidth is set at 3 years on each side of the donut. The outcome is the cumulative number of quarters employed in private sector employment 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. 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.9: Evolution of the RDD Effect on the Cumulative Percentage of Full-Time-Equivalent Quarters in Private Sector Employment



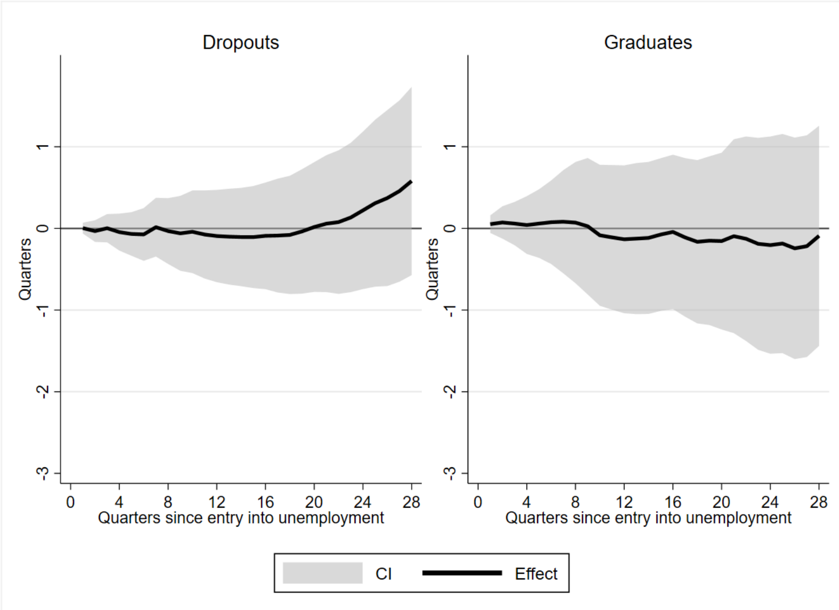
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the cumulative number of full-time-equivalents (100 for a full-time job in 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. 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.10: 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. 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).

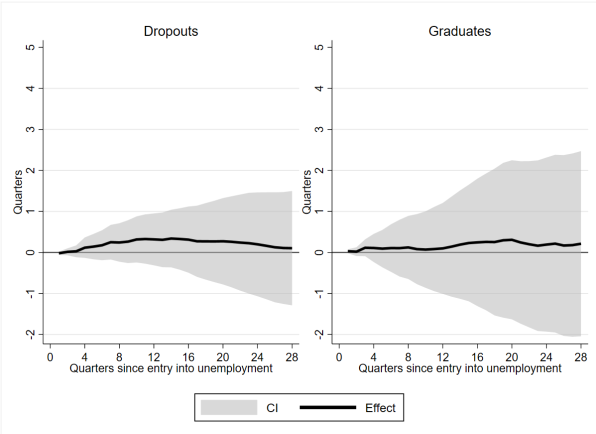
Figure A.11: Evolution of the RDD Effect on the Number of Quarters in a Private Sector Job with Earnings below the Median Daily Wage



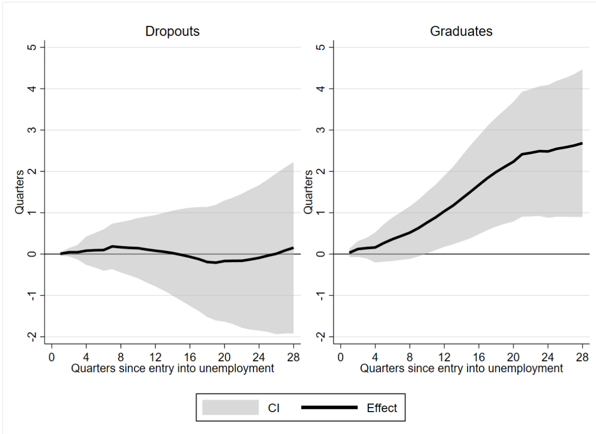
Note: Donut RDD estimates on the inflow sample of youth entering unemployment in 2010, using age at unemployment entry as the forcing variable with cutoff at 26. Evolution of the RDD effect and confidence intervals (CI) on the cumulative number of quarters in the private sector employment paying less 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. 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.12: Evolution of the RDD and DiD Effect on Accumulated Quarters of Employment by Firm Size

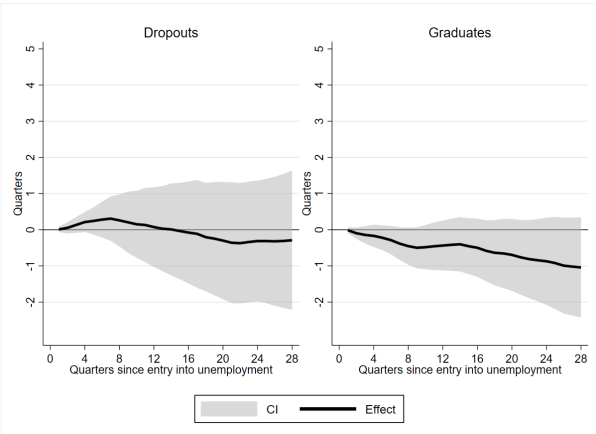
(a) RDD – smaller firms



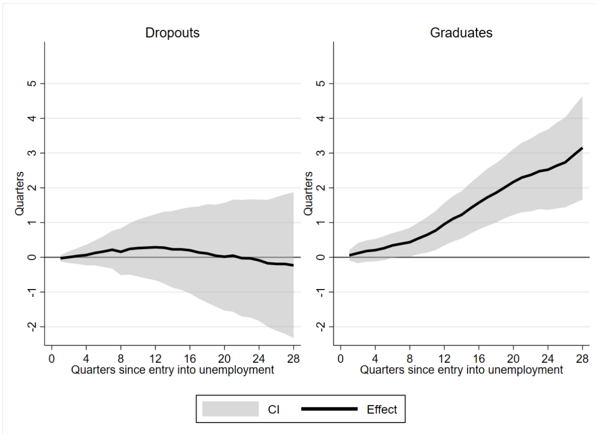
(b) RDD – larger firms



(c) DiD – smaller firms

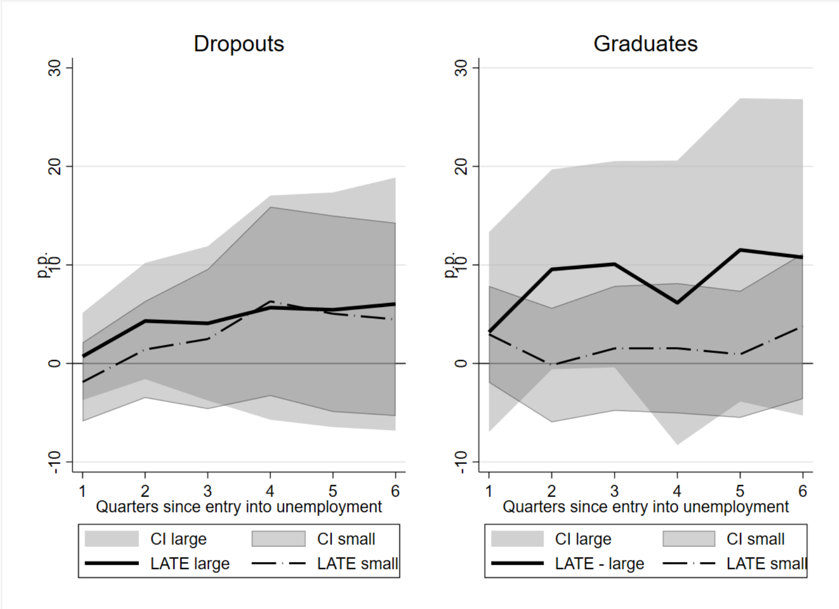


(d) DiD – larger firms



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. 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).

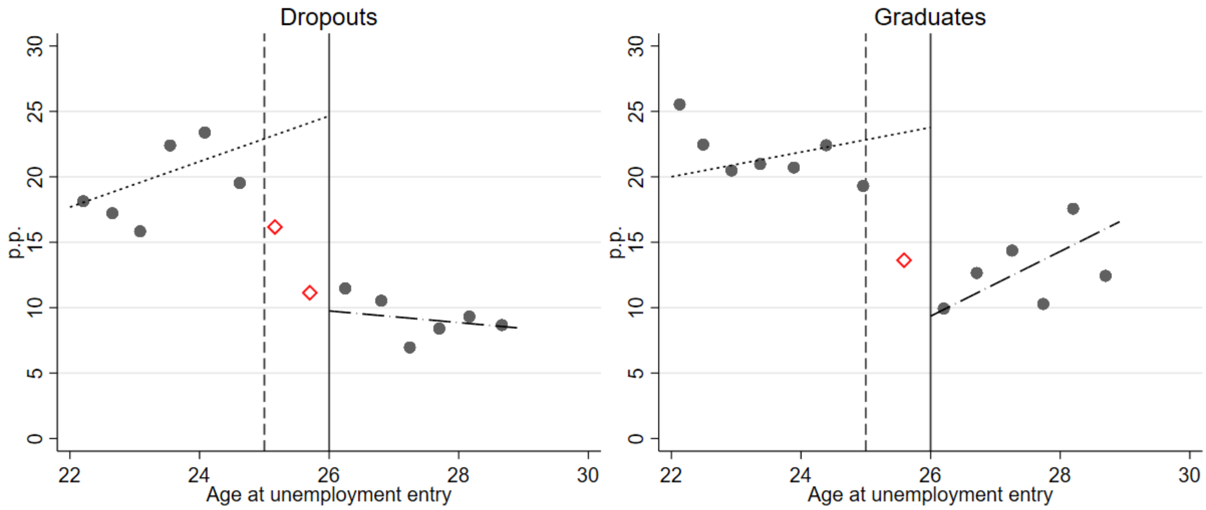
Figure A.13: Evolution of the RDD Effect on Entry in a Larger or Smaller Firm



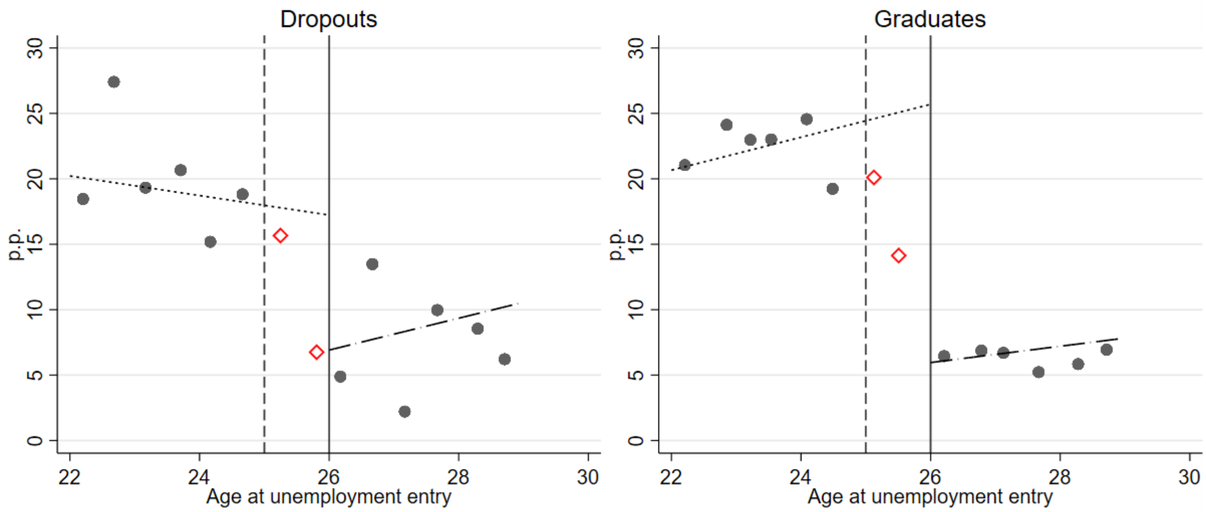
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the cumulative number of quarters in a firm with more (full line) or fewer (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. 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.14: Discontinuity at Age 26 of the Cumulative Take-Up Rate of Hiring Subsidies Within One Year – (a) Close to the Border and (b) Far from the Border

(a) Close

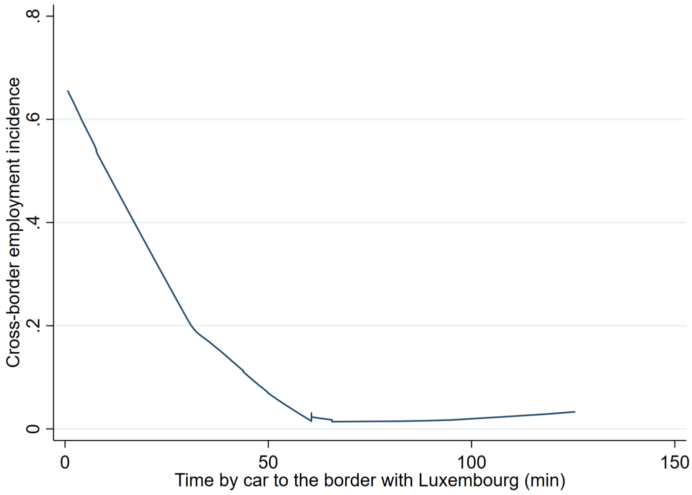


(b) Far



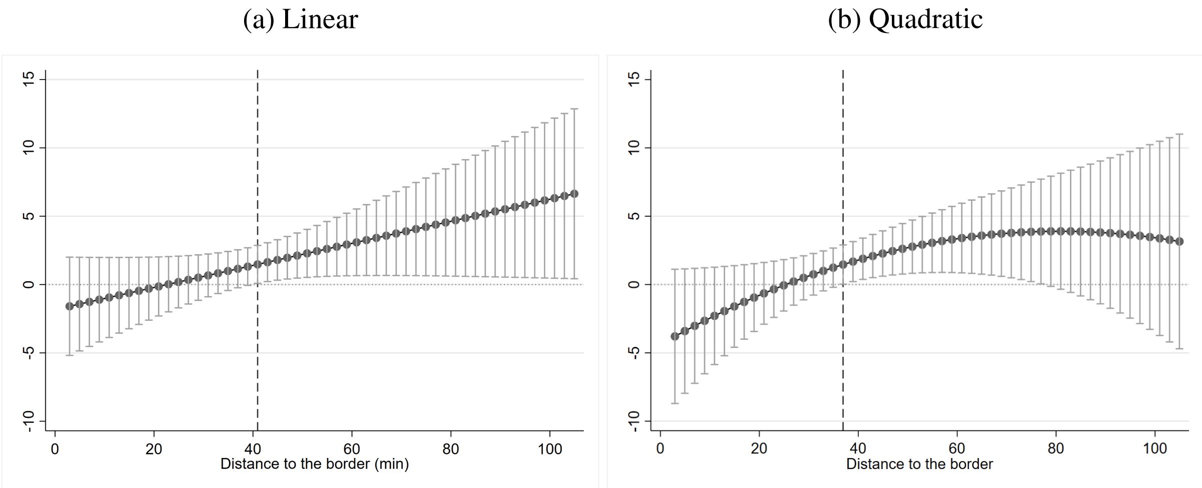
Note: Donut RDD estimate on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. The bandwidth is set at 3 years on each side of the donut. The outcome is the cumulative take-up rate of hiring subsidies within one year 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. 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 +14.9 pp [5.3; 24.5] with a p-value of 0.003 and N = 1,443, while for graduates it is +14.4 pp [5.0; 23.8] with a p-value of 0.003 and N = 1,939. For dropouts living far from the border, it is +10.3 pp [–4.4; 25.0] with a p-value of 0.166 and N = 2,636, while for graduates it is +19.7 pp and [5.8; 33.7] with a p-value of 0.006 and N = 2,432.

Figure A.15: 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).

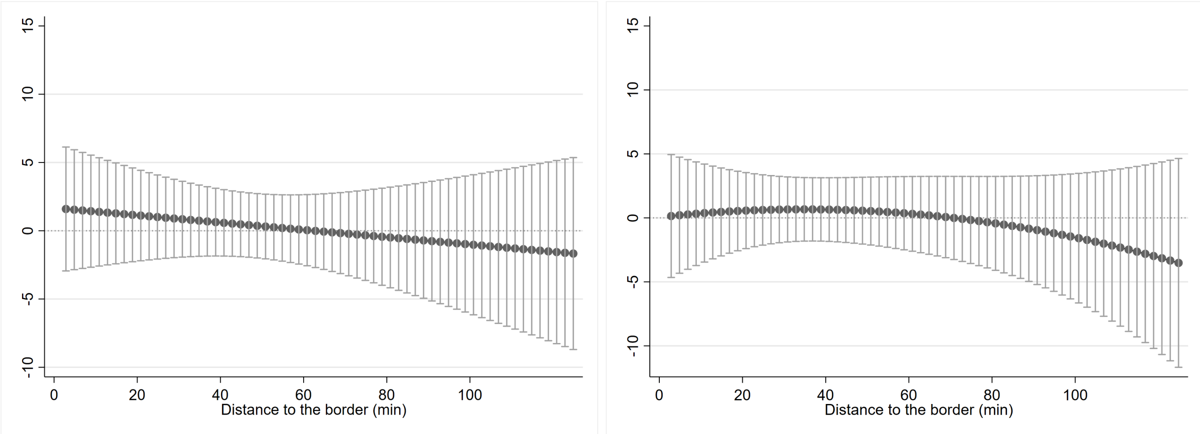
Figure A.16: Donut RDD Effect on the Cumulative Number of Quarters in Private Sector Employment 7 Years After Entry into Unemployment Interacted With Travel Time from the Border With Luxembourg – Graduates



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. 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.17: 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

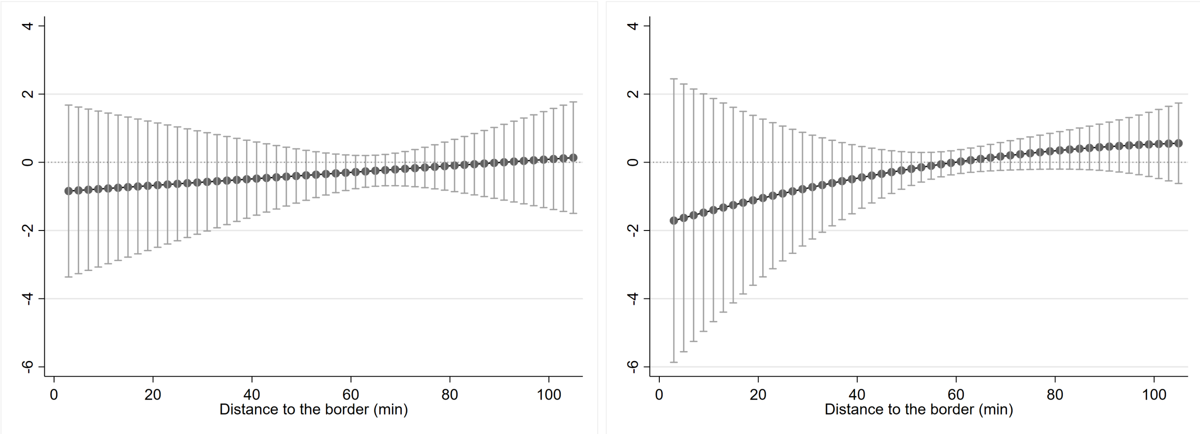
(a) Linear (b) Quadratic



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. 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).

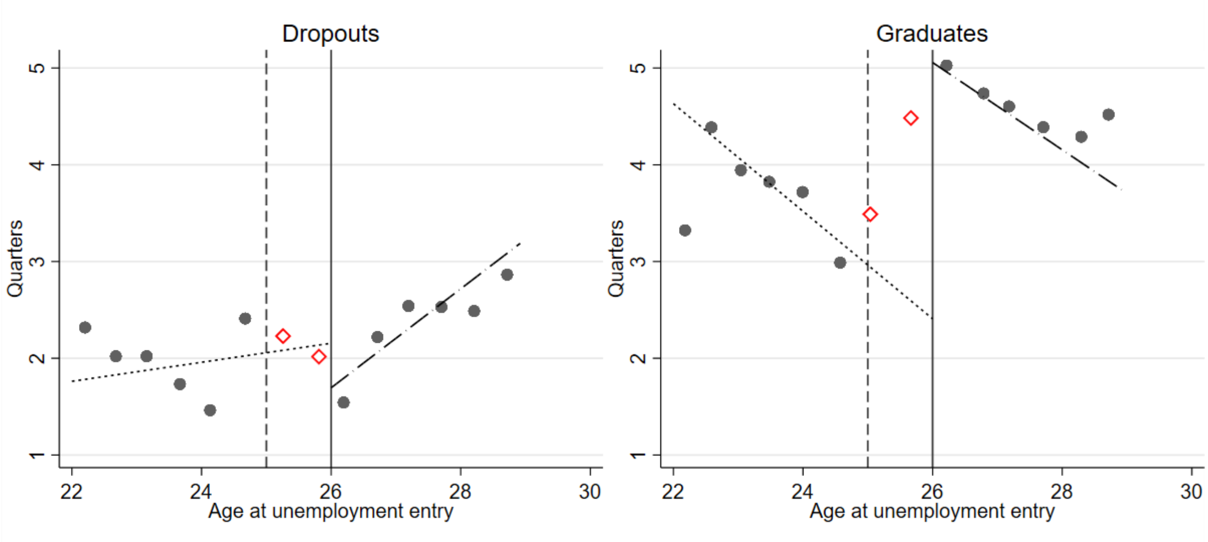
Figure A.18: Donut RDD Effect on the Cumulative Number of Quarters of Cross-Border Work 7 Years After Entry into Unemployment Interacted With Travel Time from the Border With Luxembourg – Graduates

(a) Linear (b) Quadratic



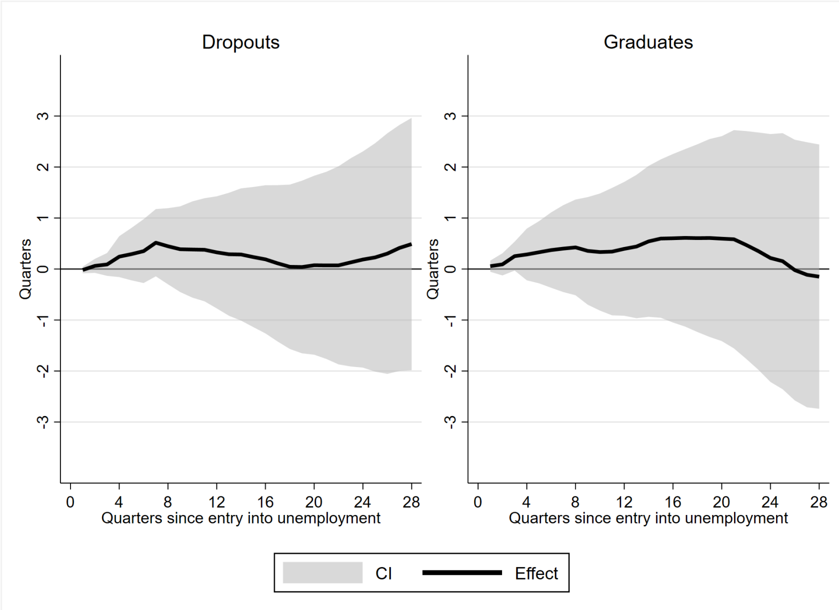
Note: Donut RDD estimate on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. The bandwidth is set at 3 years on each side of the donut. The predicted effect over the distance to the border from Luxembourg is obtained 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. 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.19: Donut RDD Plot of the Effect on the Cumulative Number of Quarters in Non-Private Sector Employment 7 Years After Entry into Unemployment



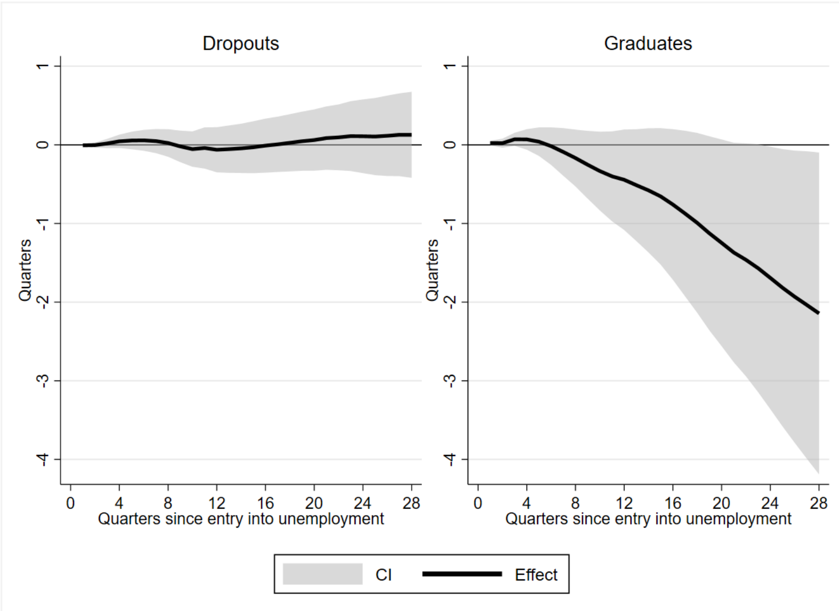
Note: Donut RDD estimate on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. The bandwidth is set at 3 years on each side of the donut. The outcome is the effect on the cumulative number of quarters in non-private 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. 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.20: Evolution of the RDD Effect on the Cumulative Number of Quarters in Any Employment



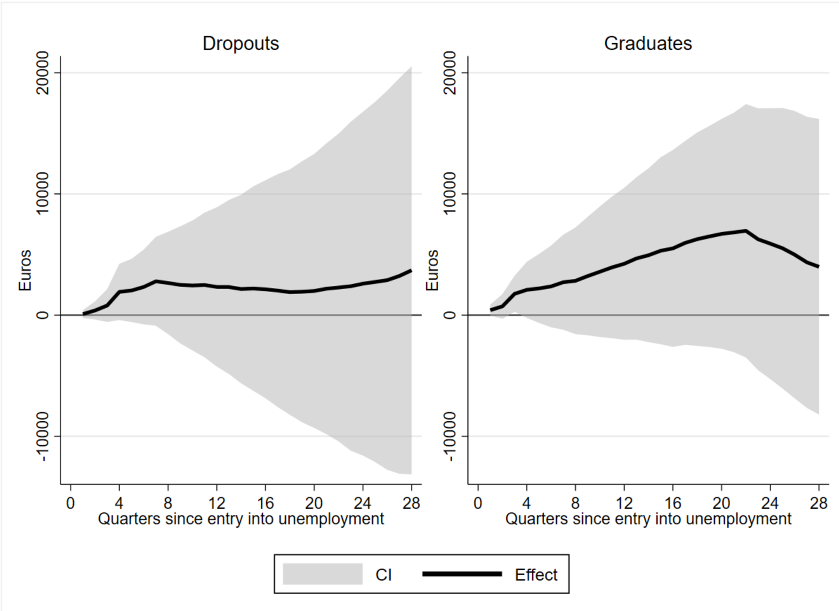
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the cumulative number of quarters in any employment by schooling level: dropouts (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. 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.21: Evolution of the RDD Effect on the Cumulative Number of Quarters in Public Sector Employment



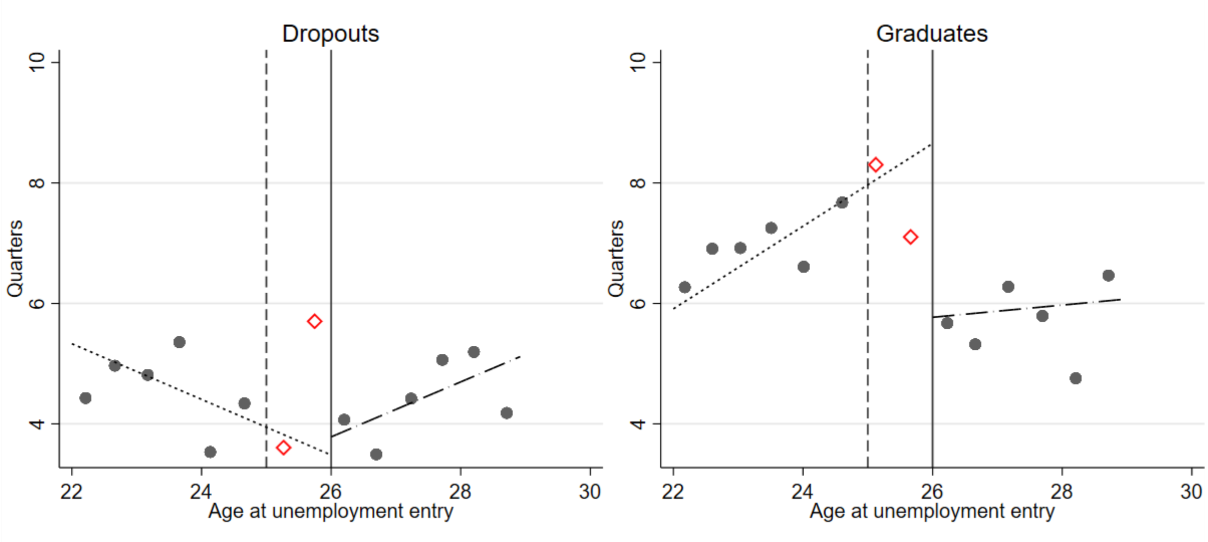
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the cumulative number of quarters in a public sector firm by schooling level: 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. 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.22: Evolution of the RDD Effect on Cumulative Earnings in the Private or Public Sector



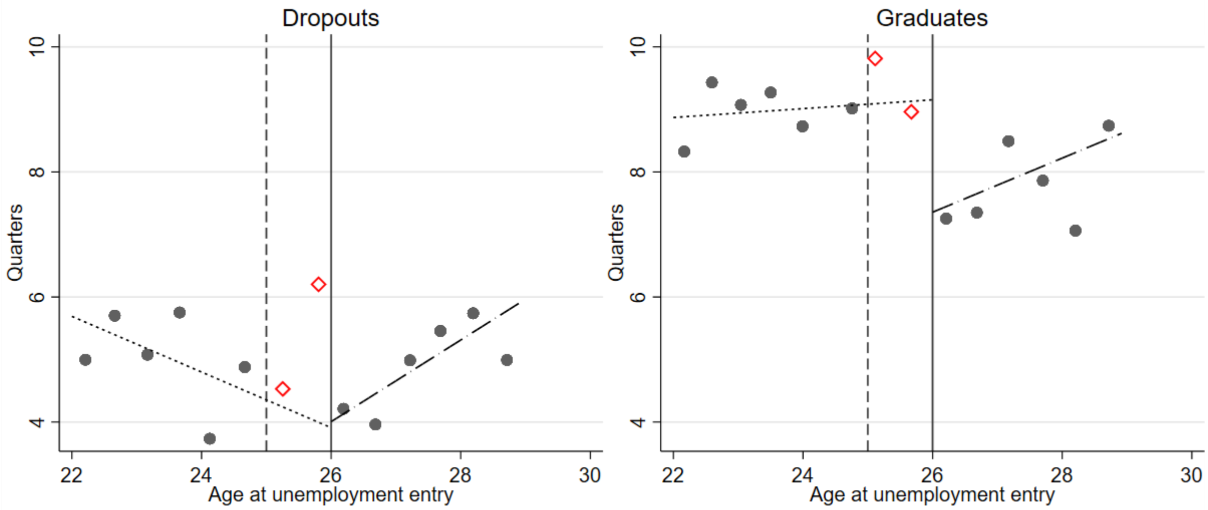
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the cumulative gross earnings in a private or public sector firm by schooling 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. 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.23: 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



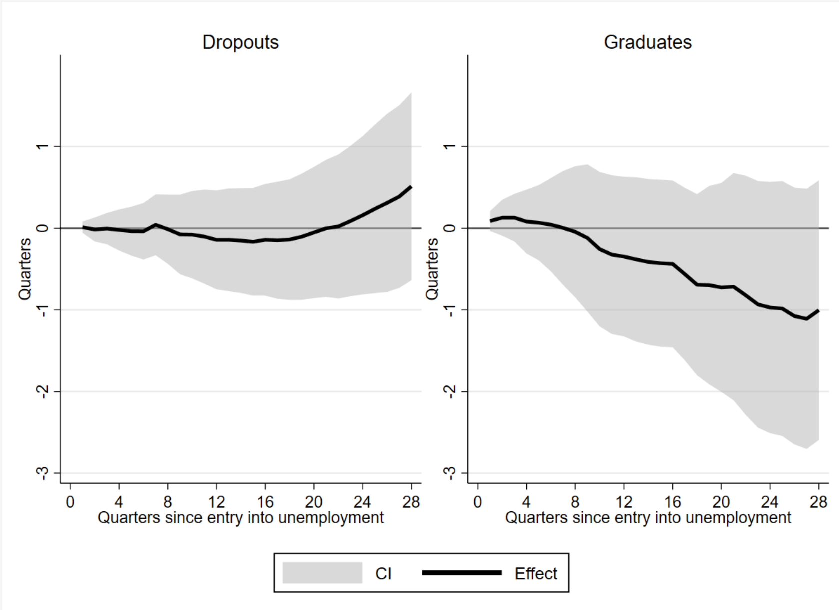
Note: Donut RDD estimate on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. The bandwidth is set at 3 years on each side of the donut. The outcome is the effect on the cumulative number of quarters in a private sector 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. 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.24: 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. 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.

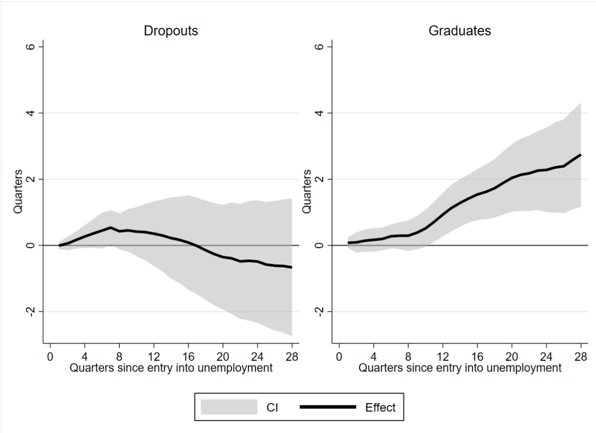
Figure A.25: Evolution of the RDD Effect on the Cumulative Number of Quarters in a Public or Private Sector Job Paying Less Than the Median Daily Wage



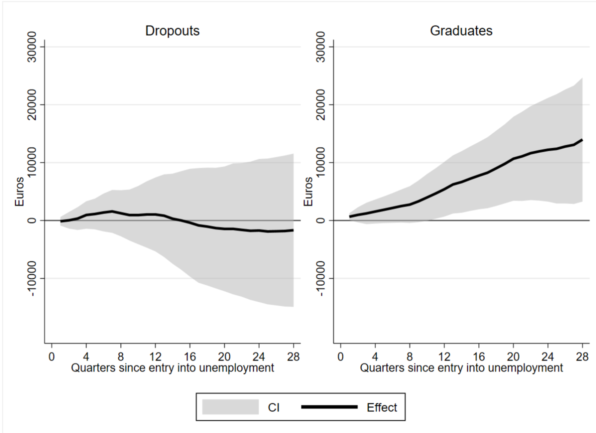
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the cumulative number of quarters in public or private sector employment paying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

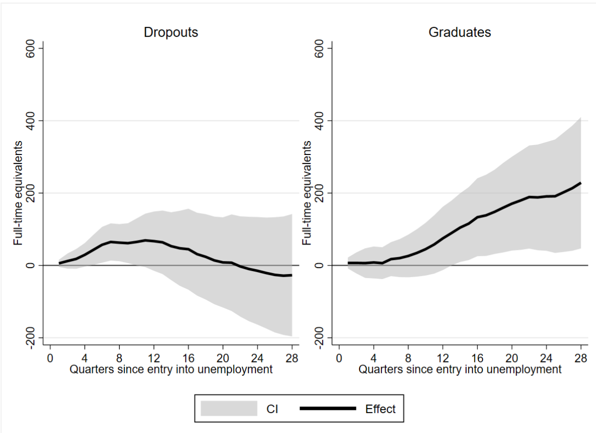
(a) Private sector employment



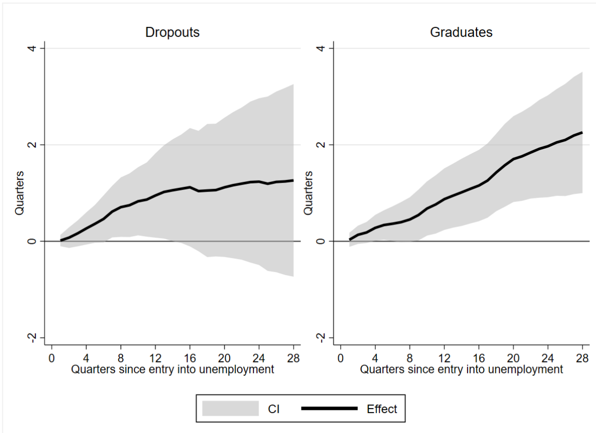
(b) Gross earnings



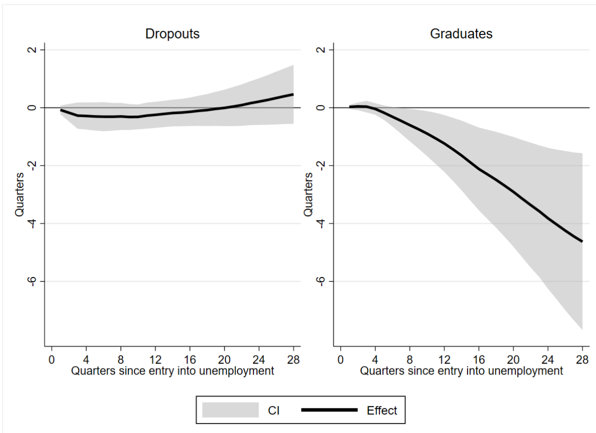
(c) Full-time equivalents



(d) High-paid employment



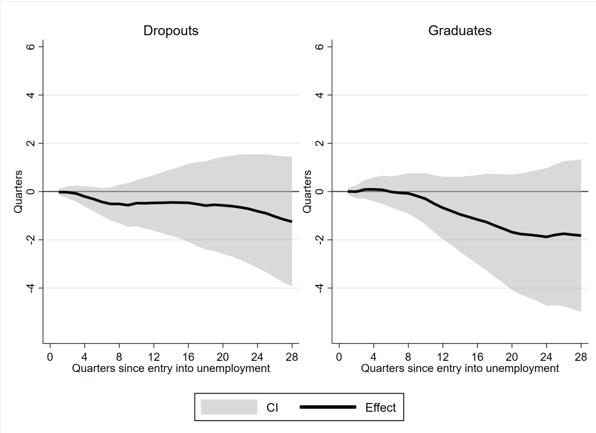
(e) Other employment



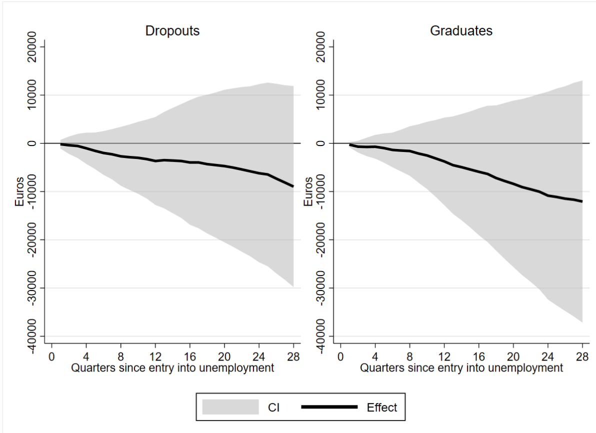
Note: Evolution of the effect estimated with a doubly robust DiD estimator (Sant'Anna and Zhao, 2020) and confidence intervals (CI) for (a) the cumulative number of quarters in private sector employment, (b) gross remuneration in private sector employment, (c) full-time equivalents (100 for a full-time 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 in 2010 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. Standard errors are clustered at the age level. N = 1,942 (dropouts) and 1,839 (graduates).

Figure A.27: Evolution of the DiD Placebo Effect on Cumulative Outcomes

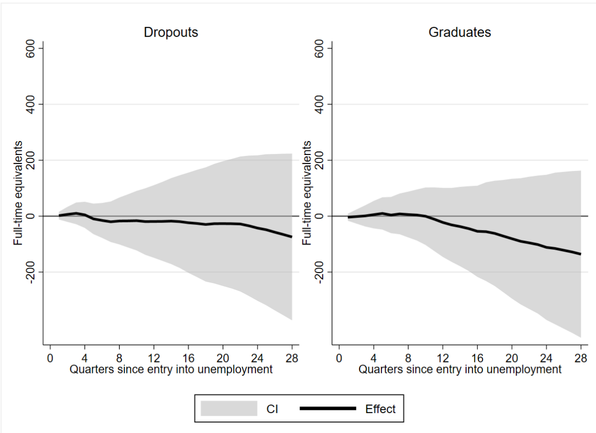
(a) Private sector employment



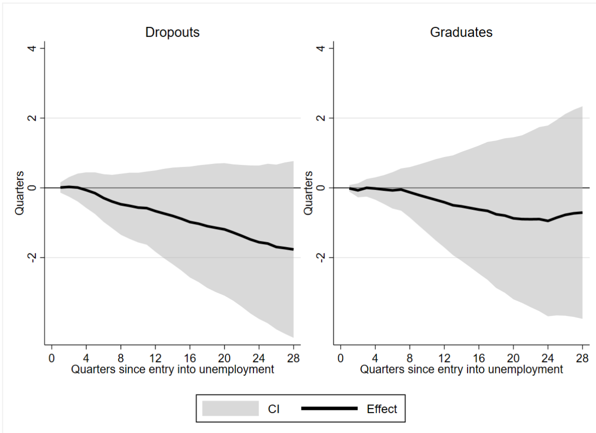
(b) Gross earnings



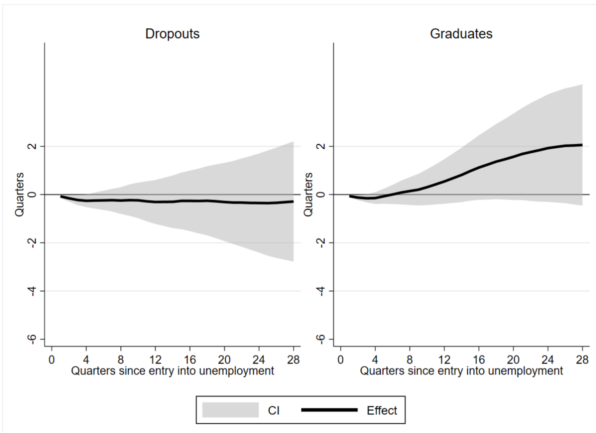
(c) Full-time equivalents



(d) High-paid employment



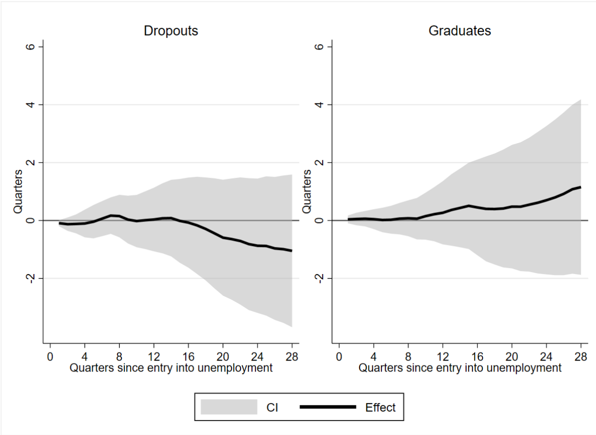
(e) Other employment



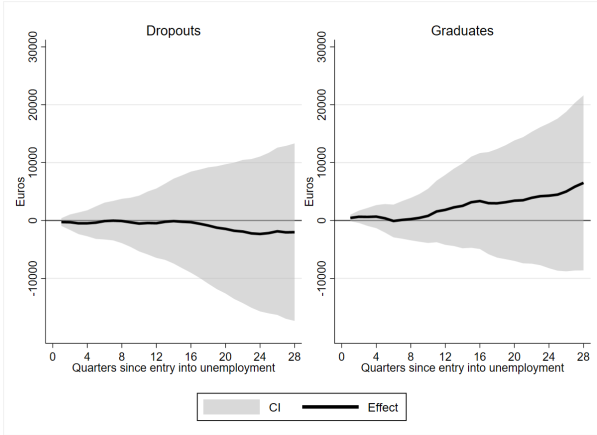
Note: Evolution of the placebo effect estimated with a doubly robust DiD estimator (Sant'Anna and Zhao, 2020) and confidence intervals (CI) for (a) the cumulative number of quarters in private sector employment, (b) gross remuneration in private sector employment, (c) full-time equivalents (100 for a 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. Standard errors are clustered at the age level. N = 1,714 (dropouts) and 1,599 (graduates).

Figure A.28: Evolution of the DiD Effect on Cumulative Outcomes: Near the Border

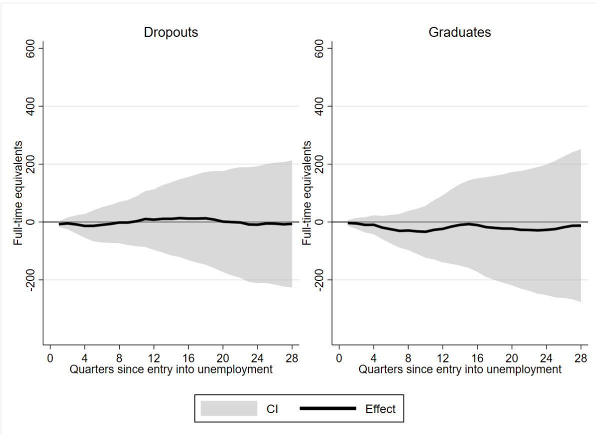
(a) Private sector employment



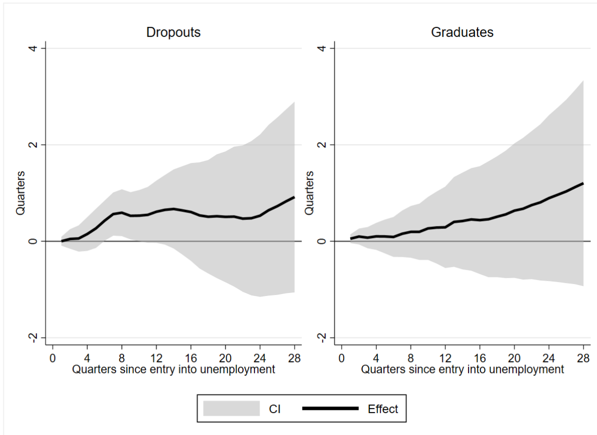
(b) Gross earnings



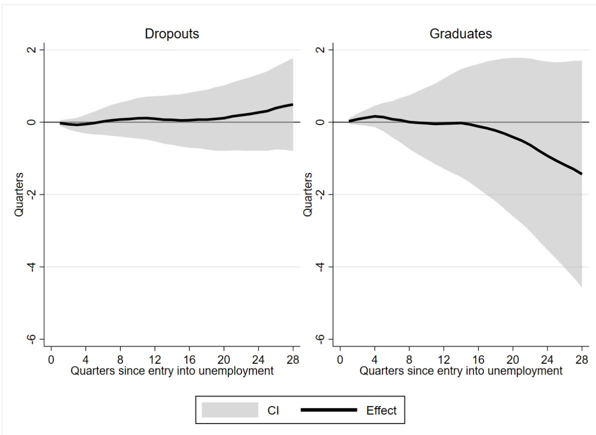
(c) Full-time equivalents



(d) High-paid employment



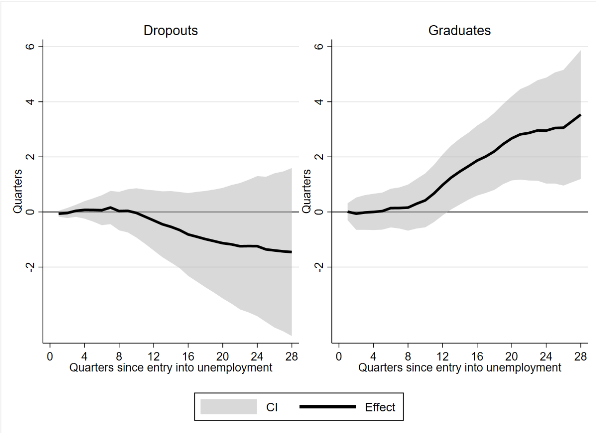
(e) Other employment



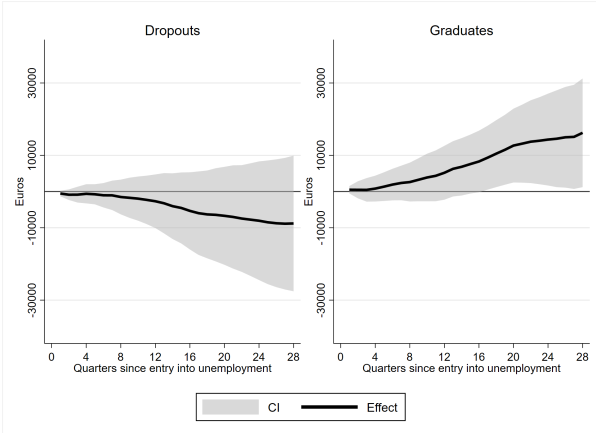
Note: Evolution of the effect estimated with a doubly robust DiD estimator (Sant'Anna and Zhao, 2020) and confidence intervals (CI) for (a) the cumulative number of quarters in private sector employment, (b) gross remuneration in private sector employment, (c) full-time equivalents (100 for a full-time 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 in 2010 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. Standard errors are clustered at the age level. N = 1,237 (dropouts) and 1,069 (graduates).

Figure A.29: Evolution of the DiD Effect on Cumulative Outcomes: Far from the Border

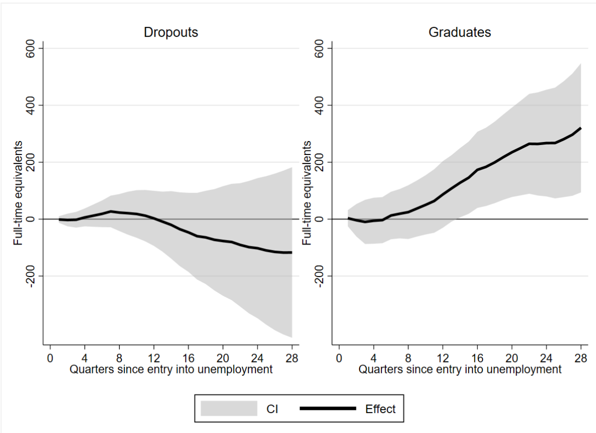
(a) Private sector employment



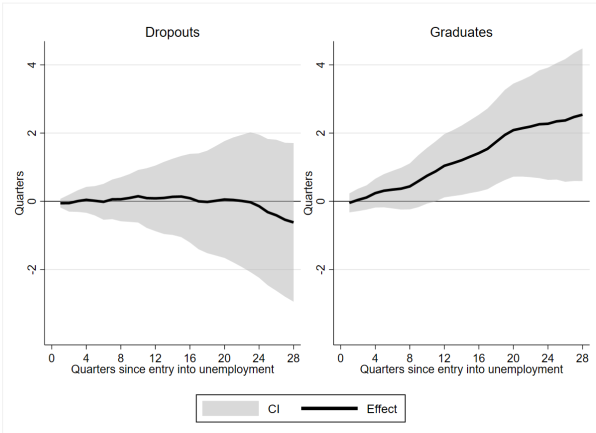
(b) Gross earnings



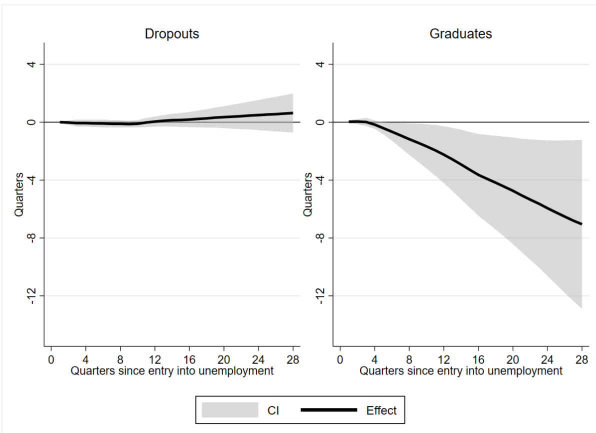
(c) Full-time equivalents



(d) High-paid employment



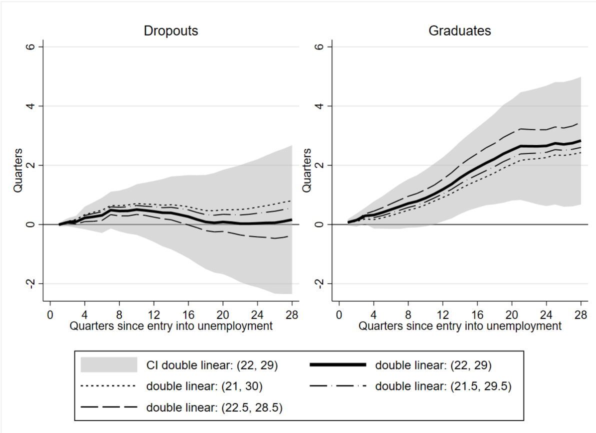
(e) Other employment



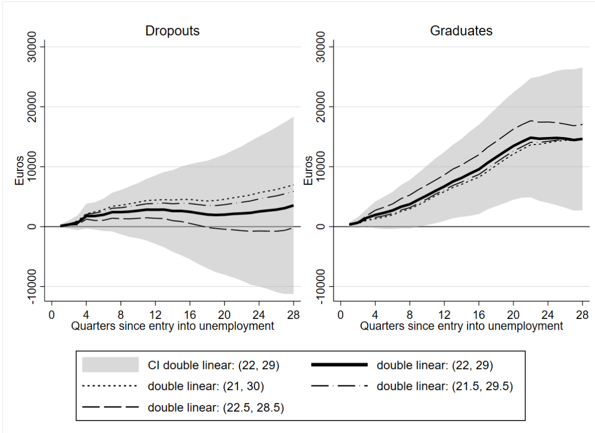
Note: Evolution of the effect estimated with a doubly robust DiD estimator (Sant'Anna and Zhao, 2020) and confidence intervals (CI) for (a) the cumulative number of quarters in private sector employment, (b) gross remuneration in private sector employment, (c) full-time equivalents (100 for a full-time 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 in 2010 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. Standard errors are clustered at the age level. N = 677 (dropouts) and 766 (graduates).

Figure A.30: Evolution of the RDD Effect on Cumulative Outcomes: Changing the Bandwidth

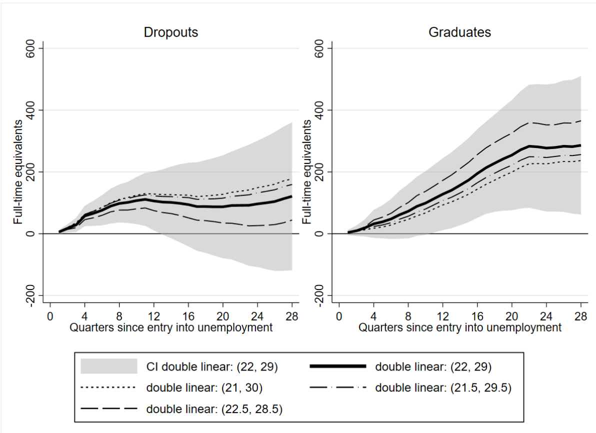
(a) Private sector employment



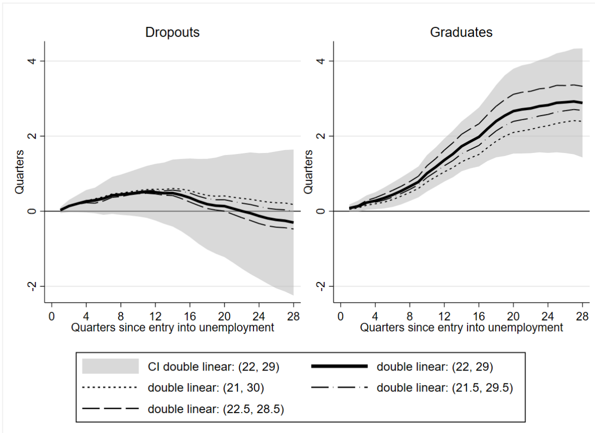
(b) Gross earnings



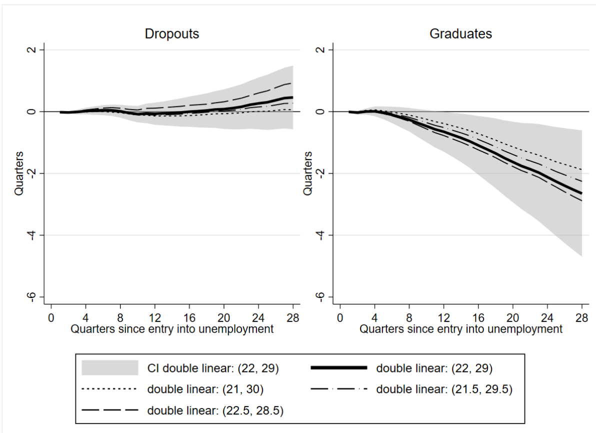
(c) Full-time equivalents



(d) High-paid employment



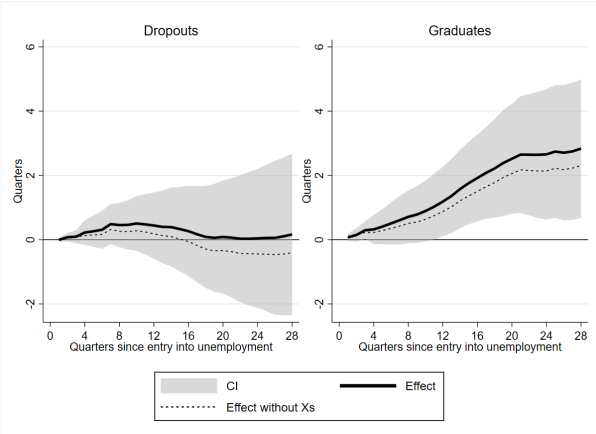
(e) Other employment



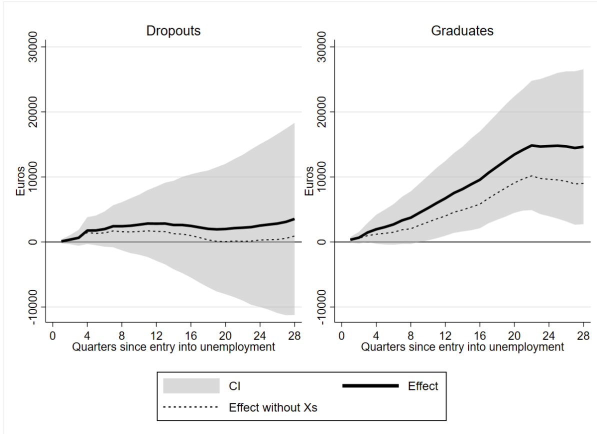
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence intervals (CI) for (a) the cumulative number of quarters in private sector employment, (b) gross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

Figure A.31: Evolution of the RDD Effect on Cumulative Outcomes: Removing the Xs

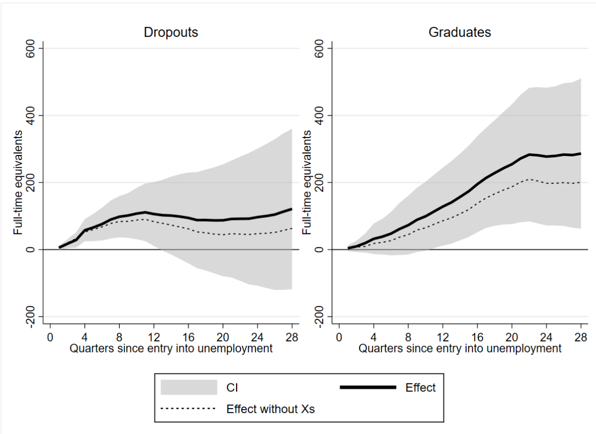
(a) Private sector employment



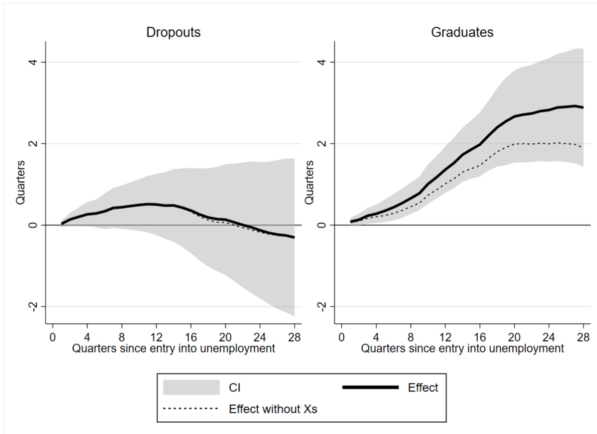
(b) Gross earnings



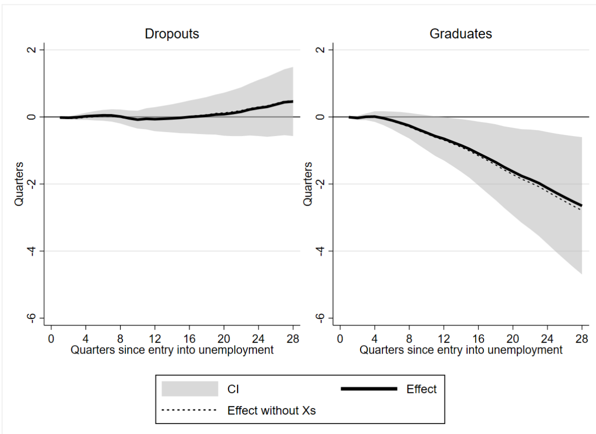
(c) Full-time equivalents



(d) High-paid employment

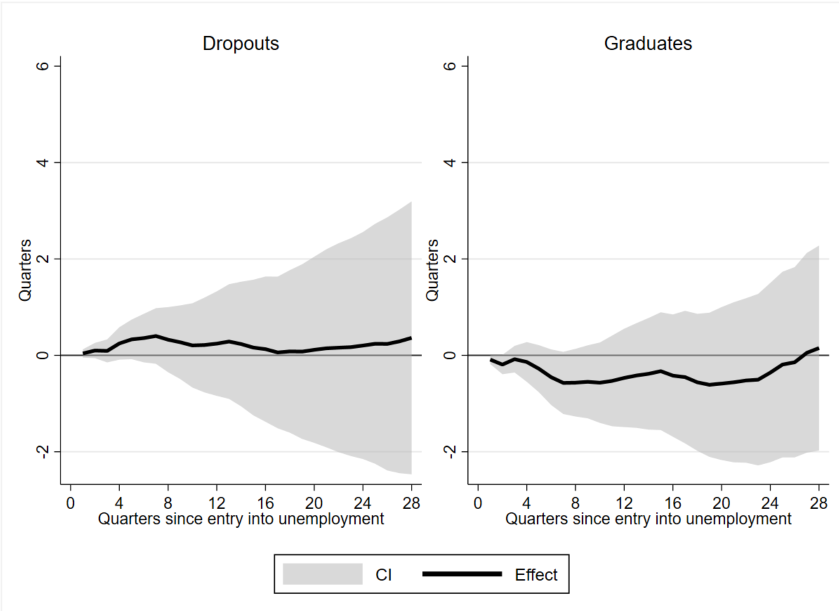


(e) Other employment



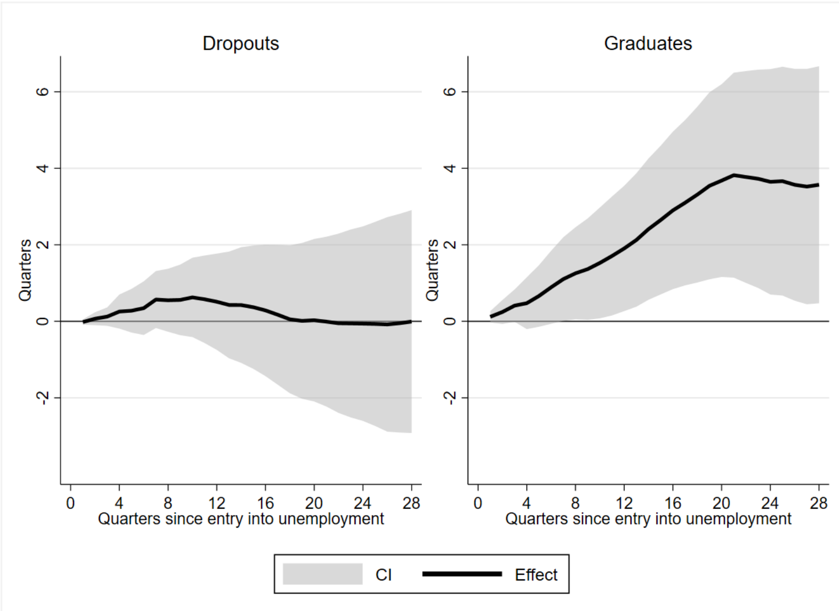
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence intervals (CI) for (a) the cumulative number of quarters in private sector employment, (b) gross 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. 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.32: Evolution of the RDD Effect on the Cumulative Number of Quarters in Private Sector Employment – Close to the Border (less than 45 minutes)



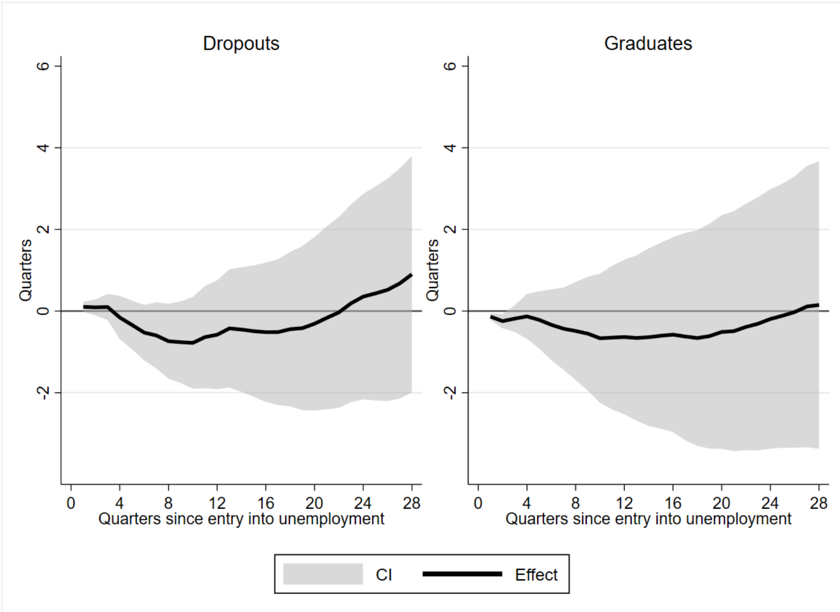
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the cumulative number of quarters in private sector employment since entry 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. 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.33: Evolution of the RDD Effect on the Cumulative Number of Quarters in Private Sector Employment – Far From the Border (more than 45 minutes)



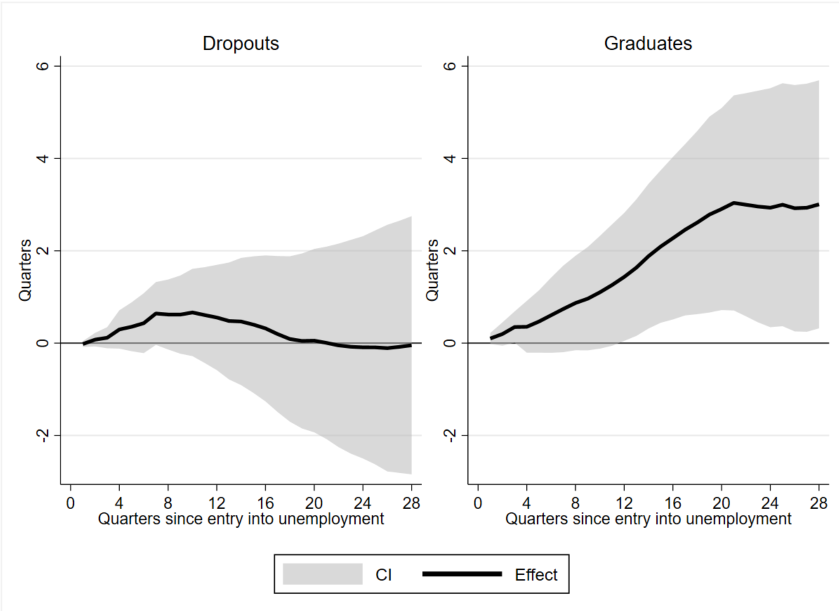
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the cumulative number of quarters in private sector employment since entry 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. 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.34: 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. 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).

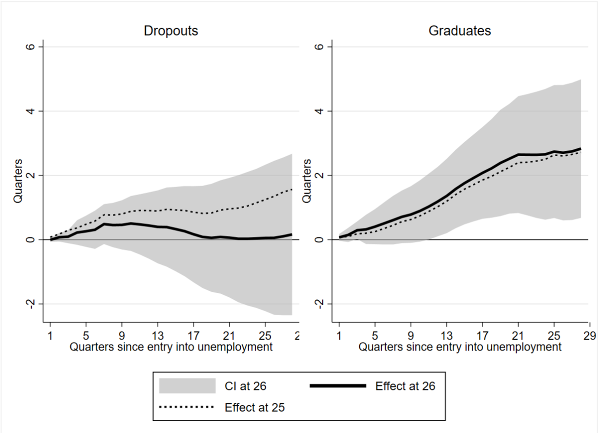
Figure A.35: Evolution of the RDD Effect on the Cumulative Number of Quarters in Private Sector Employment – Far From the Border (more than 30 minutes)



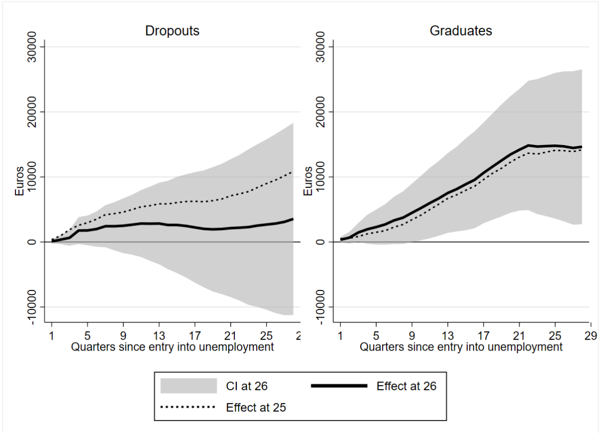
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence interval (CI) for the cumulative number of quarters in private sector employment since entry 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. 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.36: Evolution of the RDD Effect on Cumulative Outcomes: Effect at age 25

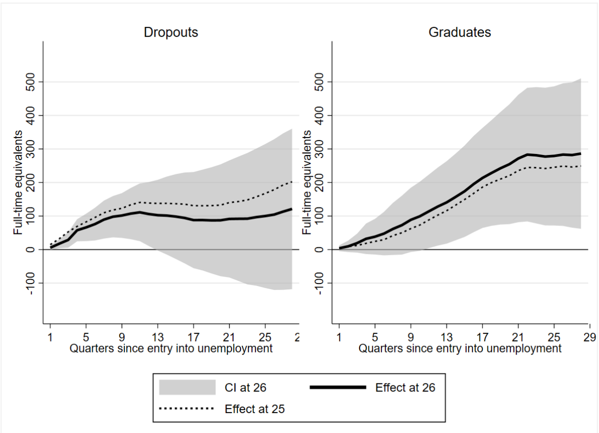
(a) Private sector employment



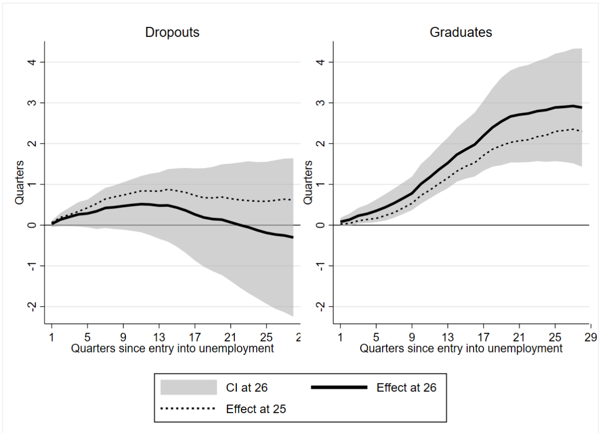
(b) Gross earnings



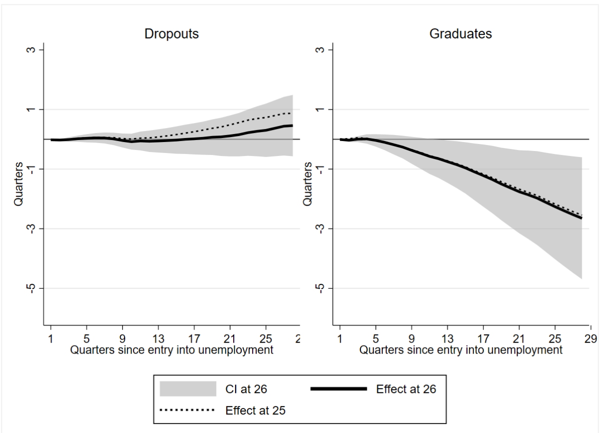
(c) Full-time equivalents



(d) High-paid employment



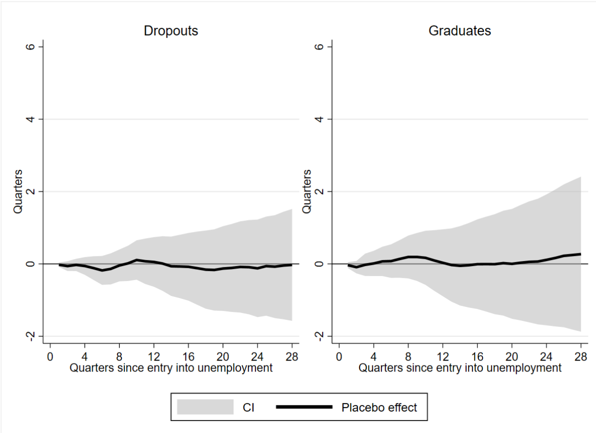
(e) Other employment



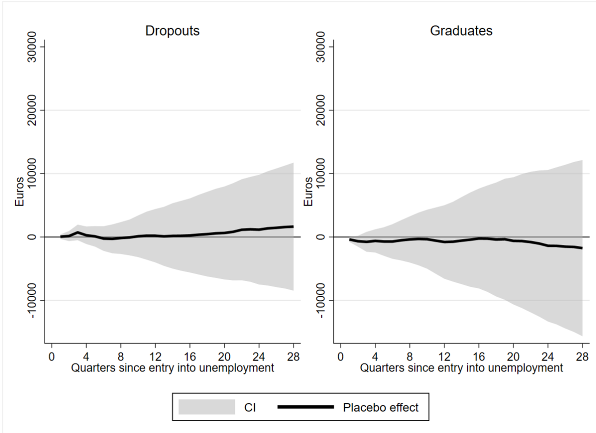
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26 (full line and CI) or 25 (dashed line). Evolution of the RDD effect and confidence intervals (CI) for (a) the cumulative number of quarters 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. Standard errors are clustered at the age level.

Figure A.37: Evolution of the RDD Effect on Cumulative Outcomes: Placebo in 2008

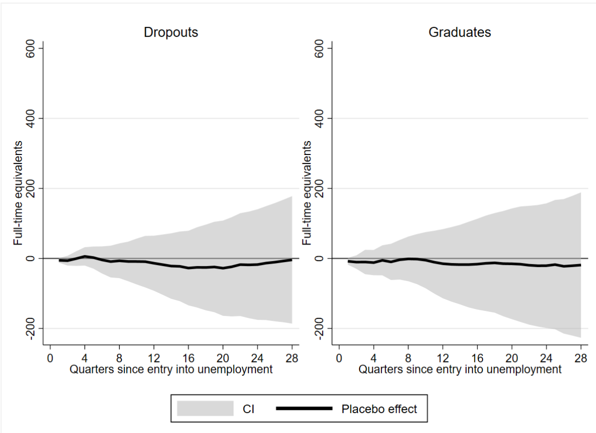
(a) Private sector employment



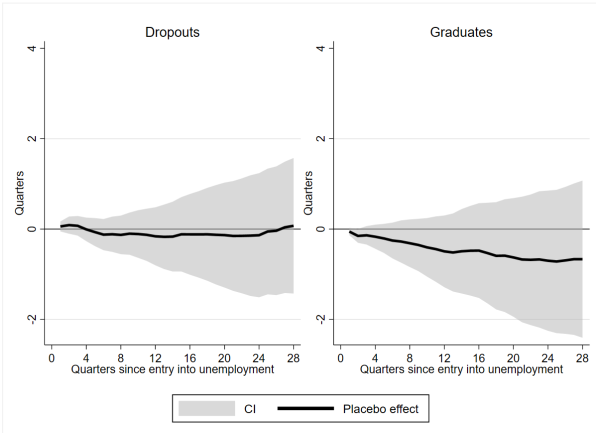
(b) Gross earnings



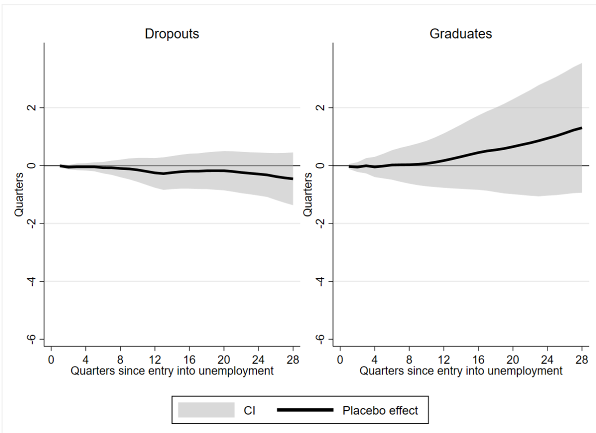
(c) Full-time equivalents



(d) High-paid employment



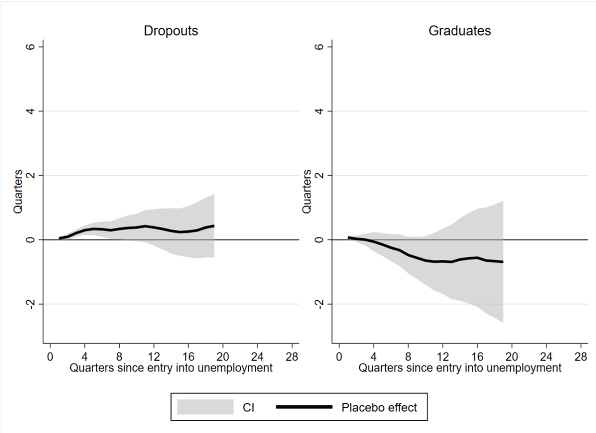
(e) Other employment



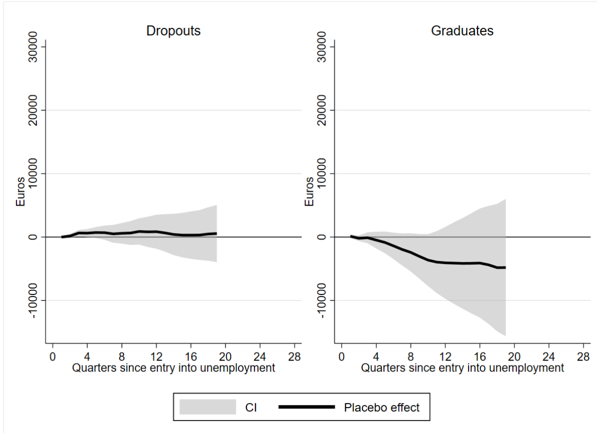
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2008, using age at unemployment entry as the forcing variable with placebo cutoffs. Evolution of the RDD placebo effect and confidence intervals (CI) for the cumulative (a) quarters in private sector employment, (b) gross 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. Standard errors are clustered at the age level. N = 3,780 (dropouts) and 3,986 (graduates).

Figure A.38: Evolution of the RDD Effect on Cumulative Outcomes: Placebo in 2012

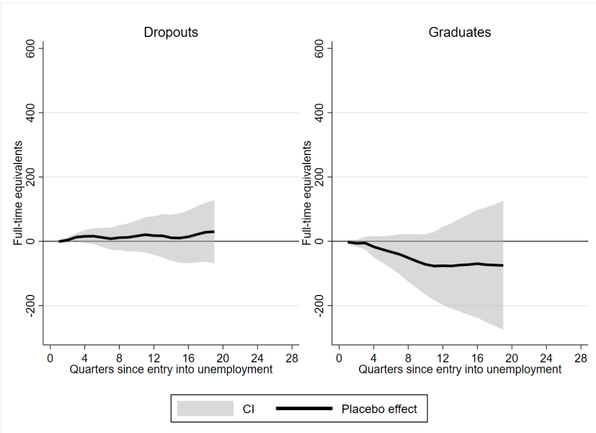
(a) Private sector employment



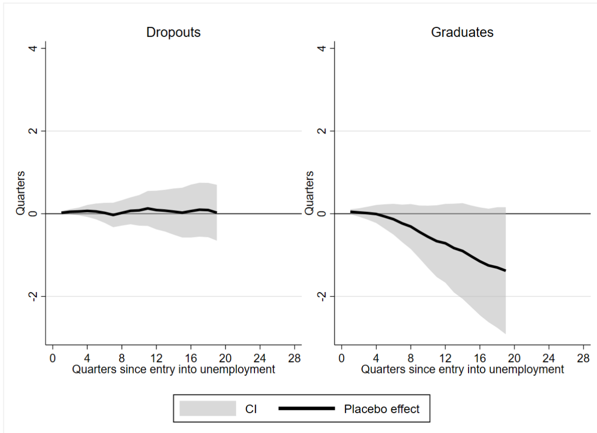
(b) Gross earnings



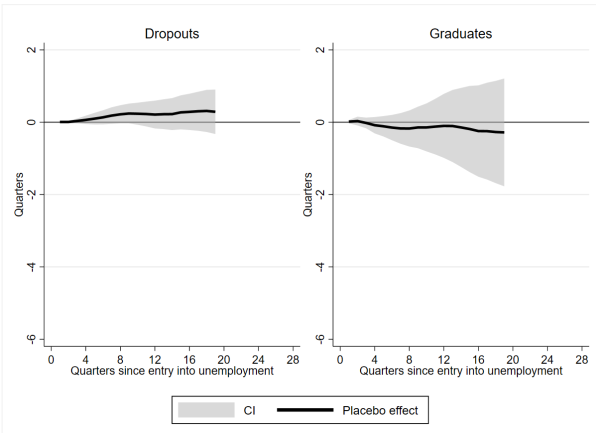
(c) Full-time equivalents



(d) High-paid employment



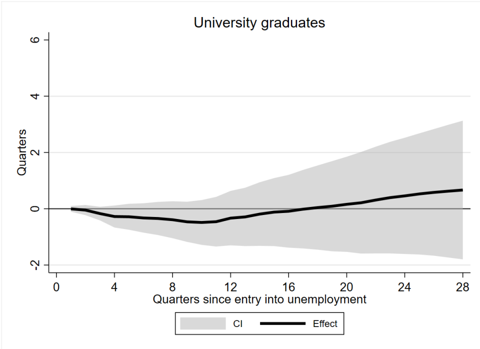
(e) Other employment



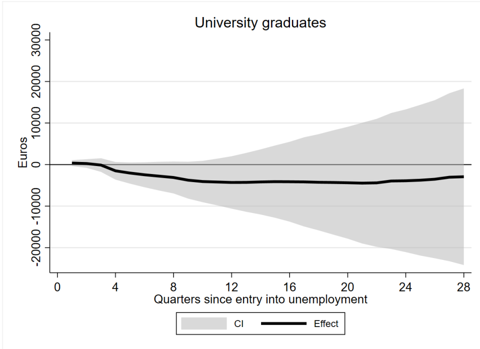
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2012, using age at unemployment entry as the forcing variable with placebo cutoffs. Evolution of the RDD placebo effect and confidence intervals (CI) for the cumulative (a) quarters in private sector employment, (b) gross 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. Standard errors are clustered at the age level. N = 4,468 (dropouts) and 4,234 (graduates).

Figure A.39: Evolution of the RDD Effect on Cumulative Outcomes: University Degree

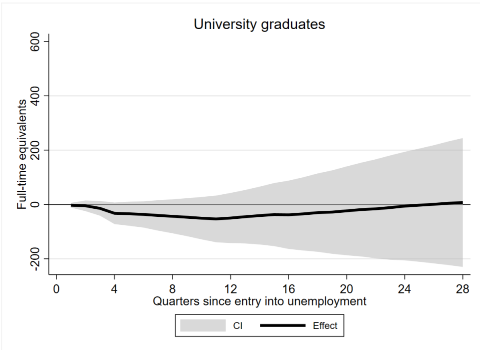
(a) Private sector employment



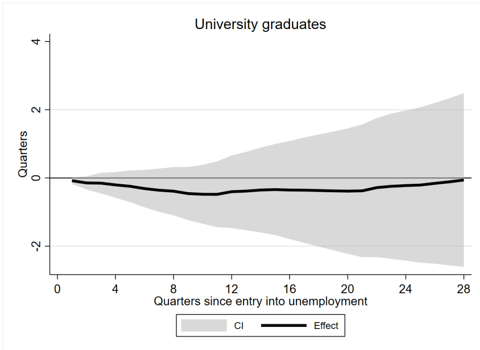
(b) Gross earnings



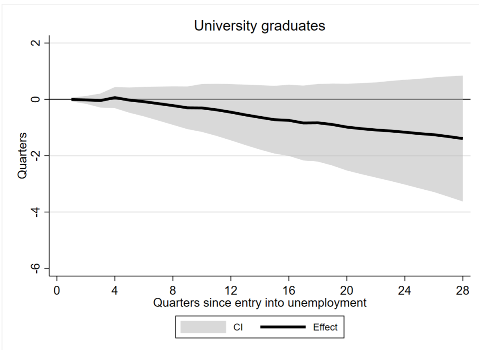
(c) Full-time equivalents



(d) High-paid employment



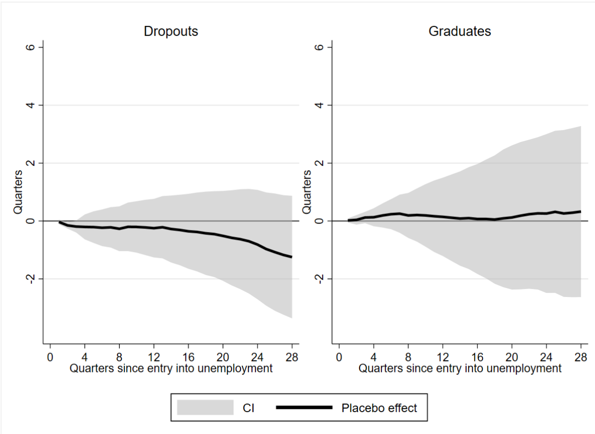
(e) Other employment



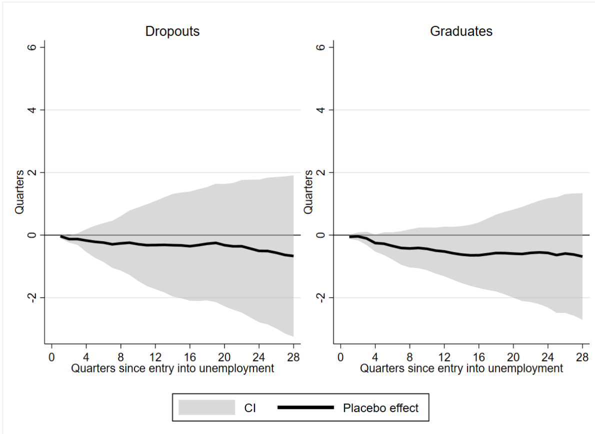
Note: Donut RDD estimates on the inflow sample of youths with a university degree entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence intervals (CI) for (a) the cumulative number of quarters in private 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. Standard errors are clustered at the age level. N = 3,993.

Figure A.40: Evolution of the RDD Effect on Cumulative Number of Quarters in Private Sector Employment: False Cutoffs

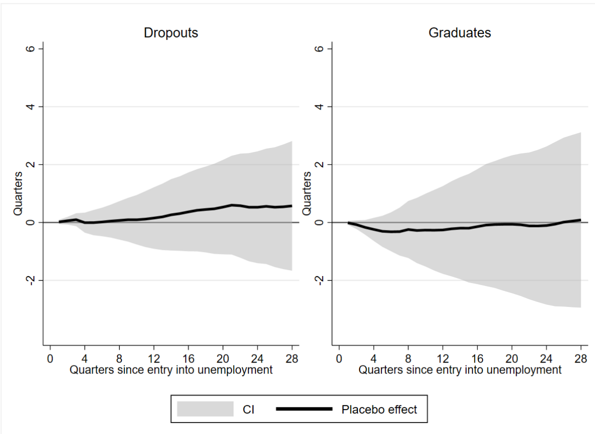
(a) 27



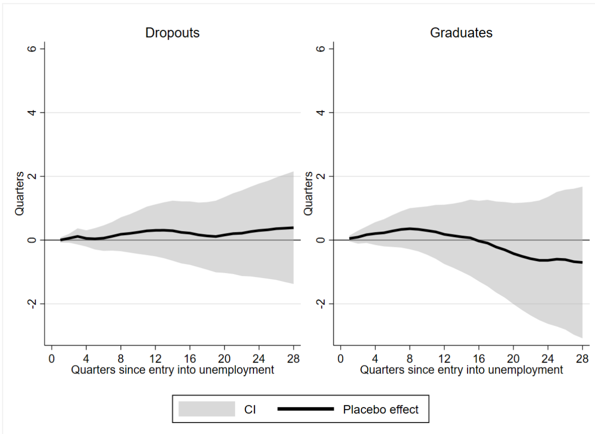
(b) 28



(c) 29



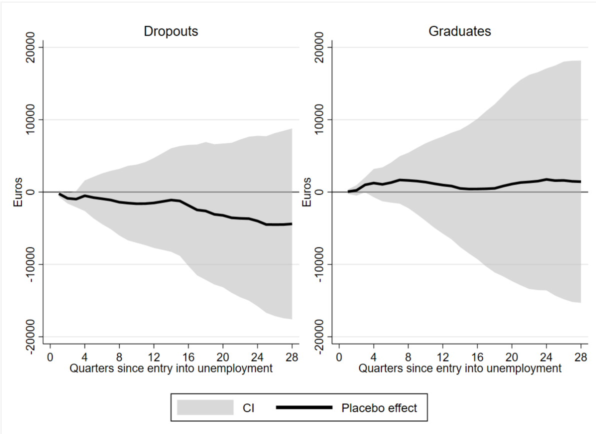
(d) 30



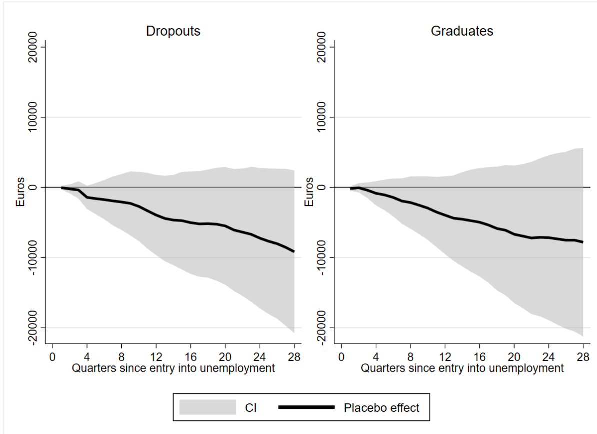
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with placebo cutoffs. The cutoff point and the one-year donut on its left are moved to (a) 27, (b) 28, (c) 29, and (d) 30 years of age. Evolution of the RDD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

Figure A.41: Evolution of the RDD Effect on Cumulative Gross Earnings in the Private Sector: False Cutoffs

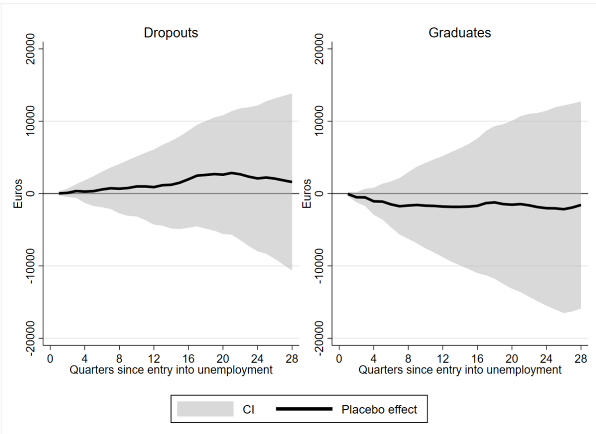
(a) 27



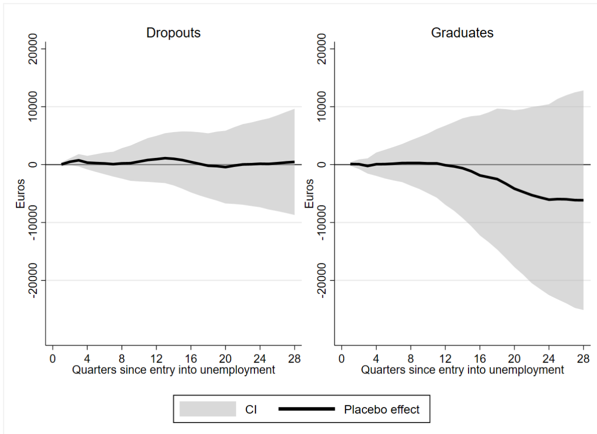
(b) 28



(c) 29



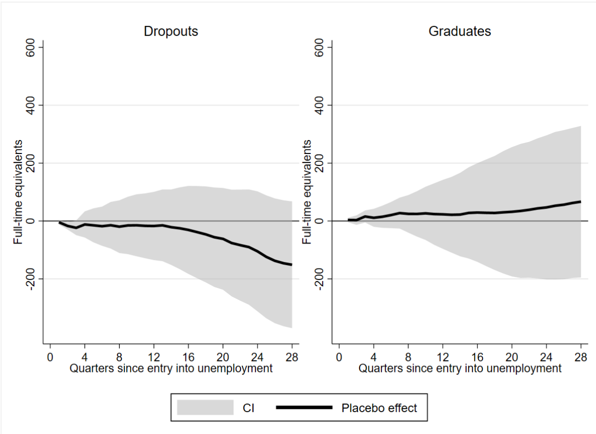
(d) 30



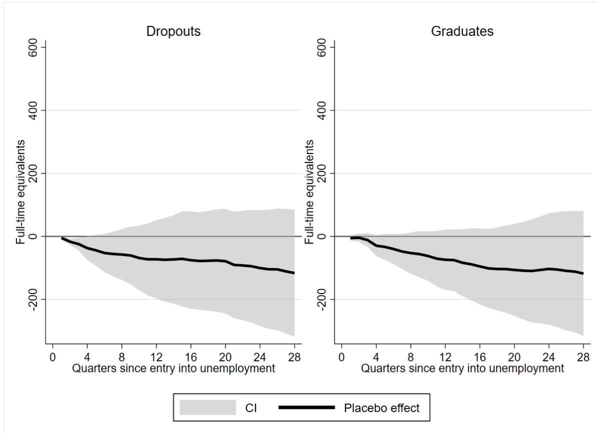
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with placebo cutoffs. The cutoff point and the one-year donut on its left are moved to (a) 27, (b) 28, (c) 29, and (d) 30 years of age. Evolution of the RDD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

Figure A.42: Evolution of the RDD Effect on the Cumulative Percentage of Full-Time Equivalent Quarters in Private Sector Employment: False Cutoffs

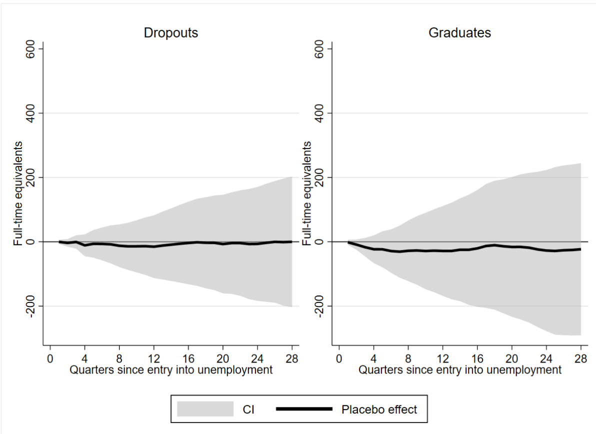
(a) 27



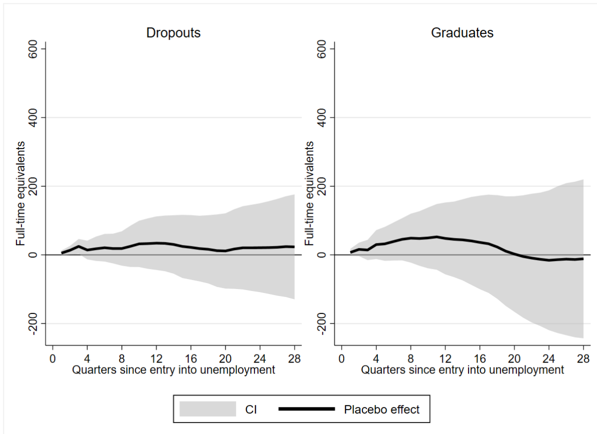
(b) 28



(c) 29



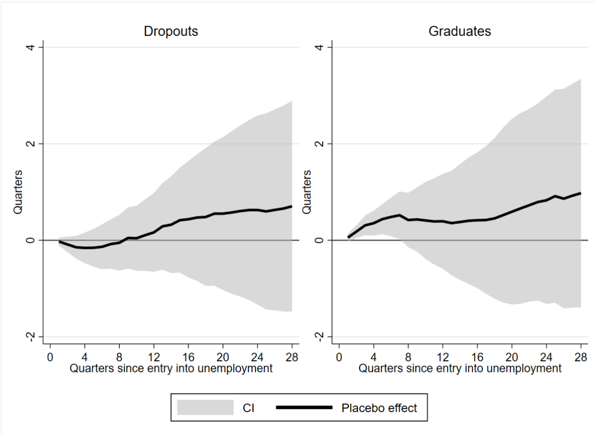
(d) 30



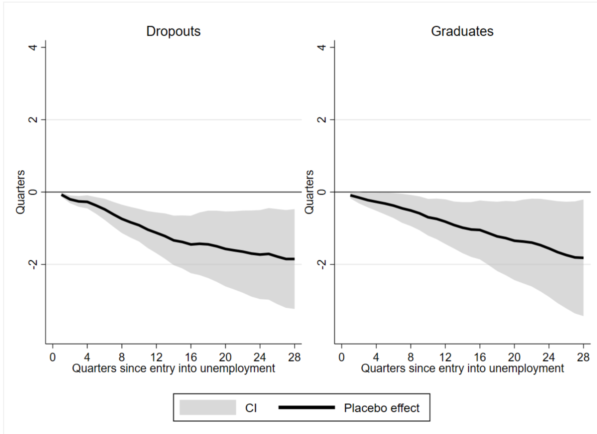
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with placebo cutoffs. The cutoff point and the one-year donut on its left are moved to (a) 27, (b) 28, (c) 29, and (d) 30 years of age. Evolution of the RDD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

Figure A.43: Evolution of the RDD Effect on the Cumulative Number of Quarters in a Private Sector Job Paying More Than the Median Daily Wage: False Cutoffs

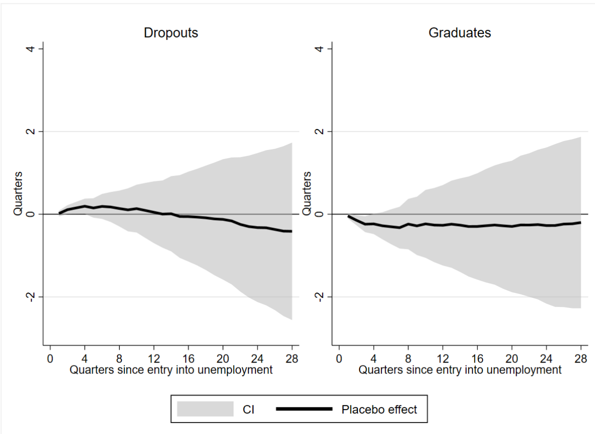
(a) 27



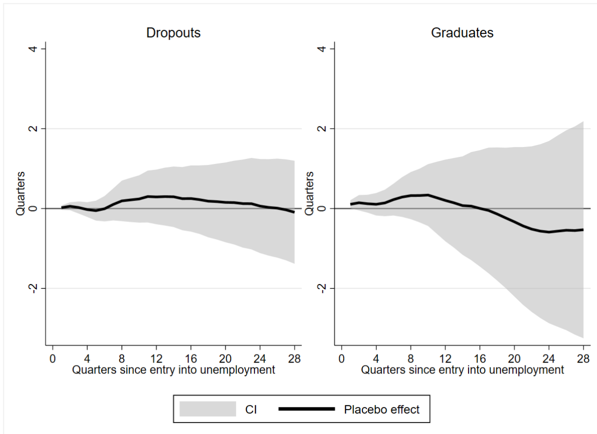
(b) 28



(c) 29



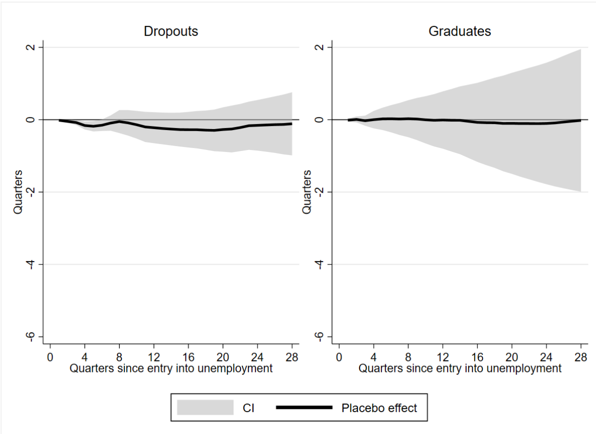
(d) 30



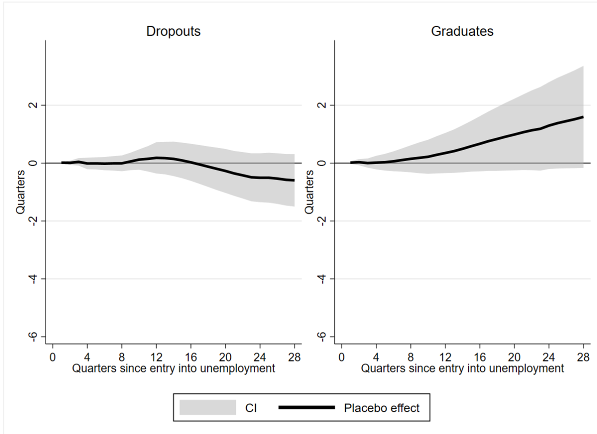
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with placebo cutoffs. The cutoff point and the one-year donut on its left are moved to (a) 27, (b) 28, (c) 29, and (d) 30 years of age. Evolution of the RDD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

Figure A.44: Evolution of the RDD Effect on the Cumulative Number of Quarters in Non-Private Sector Employment: False Cutoffs

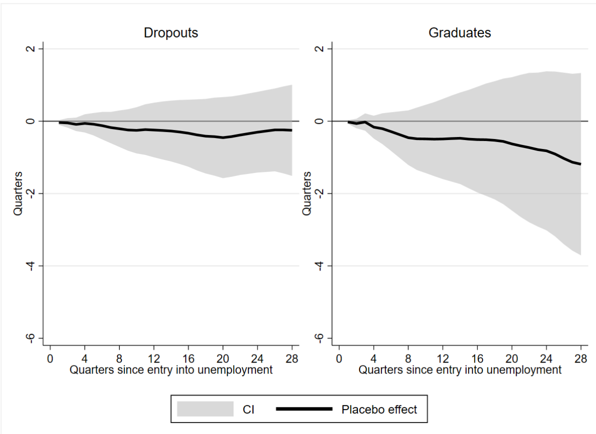
(a) 27



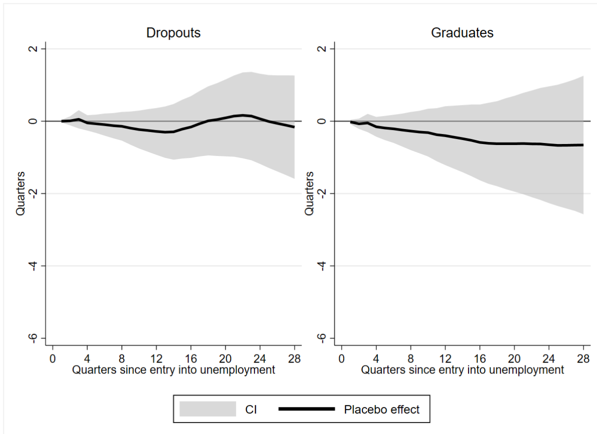
(b) 28



(c) 29



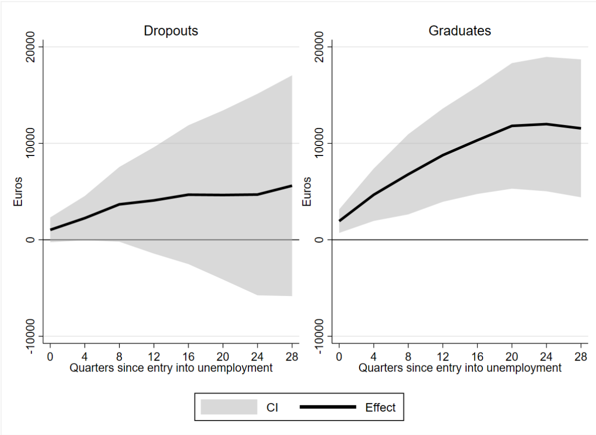
(d) 30



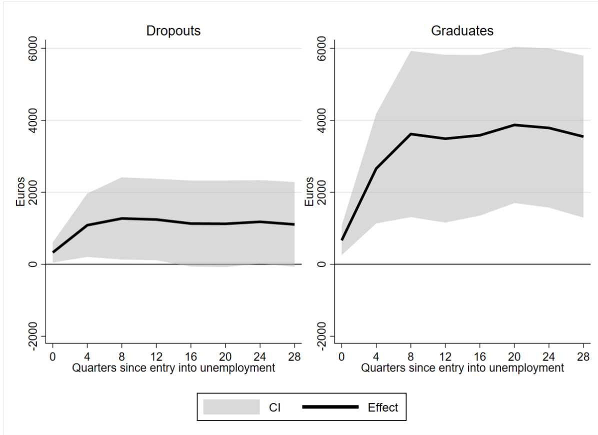
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with placebo cutoffs. The cutoff point and the one-year donut on its left are moved to (a) 27, (b) 28, (c) 29, and (d) 30 years of age. Evolution of the RDD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

Figure A.45: Evolution of the RDD Effect on Components of the Cost-Benefit Analysis (€)

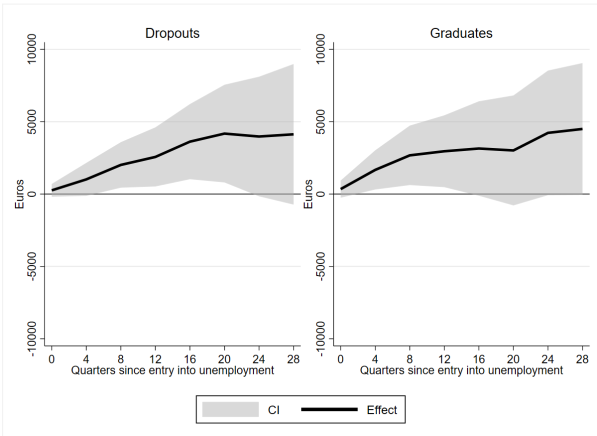
(a) Tax revenue



(b) Hiring subsidy expenditure



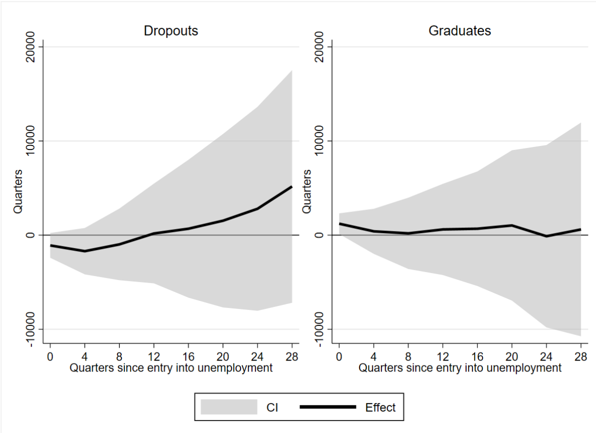
(c) Unemployment benefits expenditure



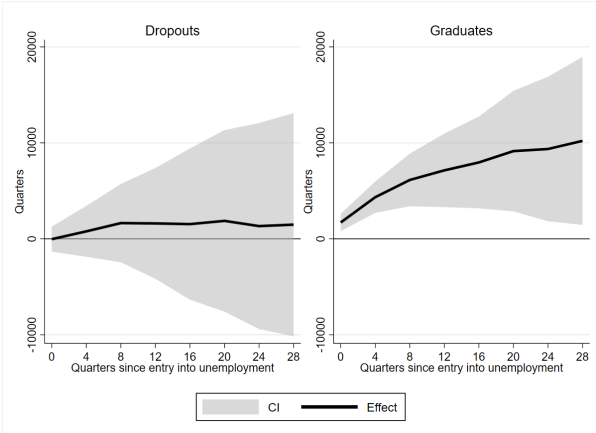
Note: Donut RDD estimates on the inflow sample of youths entering unemployment in 2010, using age at unemployment entry as the forcing variable with a cutoff at 26. Evolution of the RDD effect and confidence intervals (CI) for (a) tax revenue collected by the government, (b) expenditure for hiring subsidies, (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. 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.46: Evolution of the DiD Effect on Components of the Cost-Benefit Analysis (€)

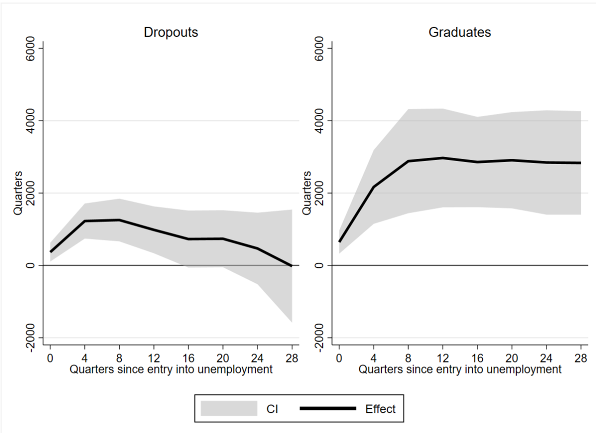
(a) Net return



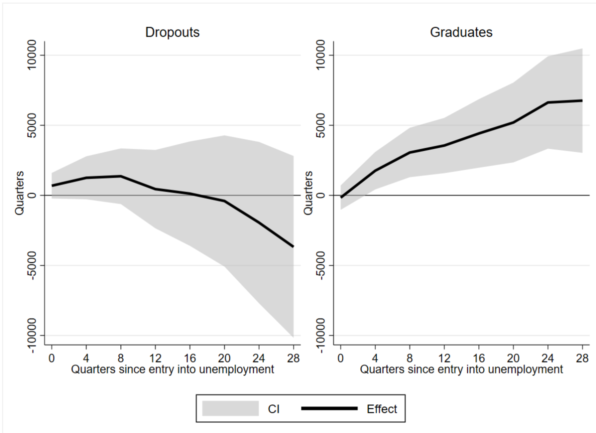
(b) Tax revenue



(c) Hiring subsidy expenditure



(d) Unemployment benefits expenditure



Note: Evolution of the placebo 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. 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:[46];
   - 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;
   - All other individuals.

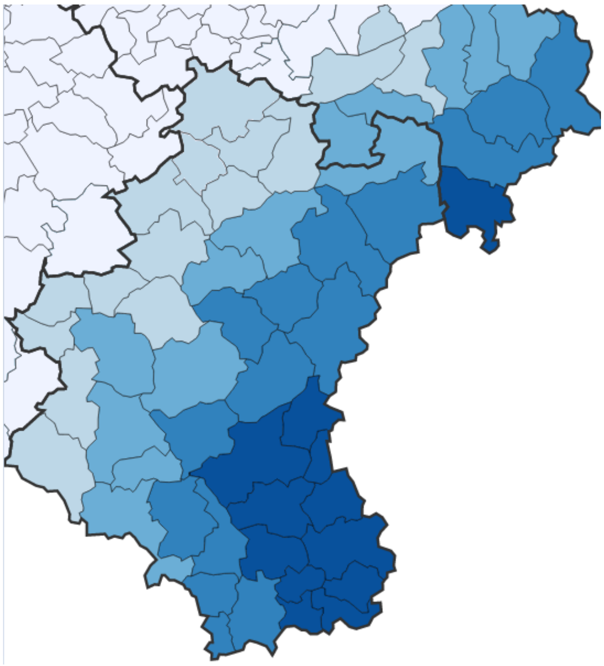
A random sample without replacement is drawn from each of the 10 strata using a random number generator. The number of individuals thus selected varies according to the strata and is shown in Table B.1. We oversample the geographical strata near Luxembourg (first geographical strata) and people registering 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.

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

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.[47] 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 and 4) in subsidized employment only. Other outcomes include the number of quarters spent 5) in the first firm in which the worker was hired and 6) in firms other than the first one, 7) the full-time equivalent[48] (100 if full-time) number of quarters in salaried private sector employment, 8) the cumulative gross remuneration earned in the salaried private sector, the number of quarters in a 9) high- or 10) low-paying salaried private sector job,[49] the number of quarters in a 11) large or 12) small firm in a salaried private sector job (fewer or more than 50 employees), and 13) the number of quarters in any salaried public sector employment, self-employment, or cross-border work.[50]

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 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

[47] 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.

[48] 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.

[49] 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).

[50] 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.

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

D DiD Estimator of Sant'Anna and Zhao (2020)

In a secondary analysis, we test whether our findings are confirmed by a difference-in-differences (DiD) estimator, which directly takes advantage of the existence of a pre-treatment period in our data. 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. However, a complication arises when we estimate effects on outcomes beyond the first year. In this case, the treated individuals of the pre-treatment period have their outcomes measured during the 2010-2011 period, which may also be affected by the treatment if they are unemployed. To deal with this issue, 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 (C1). This ensures that the unemployment entries of 2008 are above the age-eligibility threshold of 26 by the time they reach 2010. Second, they should be under the age of 25 at unemployment registration (C2) to guarantee that they have at least one year of age eligibility. Our control group is composed of individuals older than 26 but younger than 27, which we increase to 30 years old for robustness.

For identification, the DiD estimator relies on the assumption that both groups would have followed a common trend in the untreated outcomes if the subsidy had not been implemented. To relax this assumption, we make this conditional on the set of predetermined characteristics shown in Table C.2 in Online Appendix C. Finally, we test for parallel trends during the pre-treatment period by comparing the unemployed registrations of 2008 to those of 2007.

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:

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

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 a multiplier-type bootstrap procedure and clustered by age (see Callaway and Sant'Anna, 2021).

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.