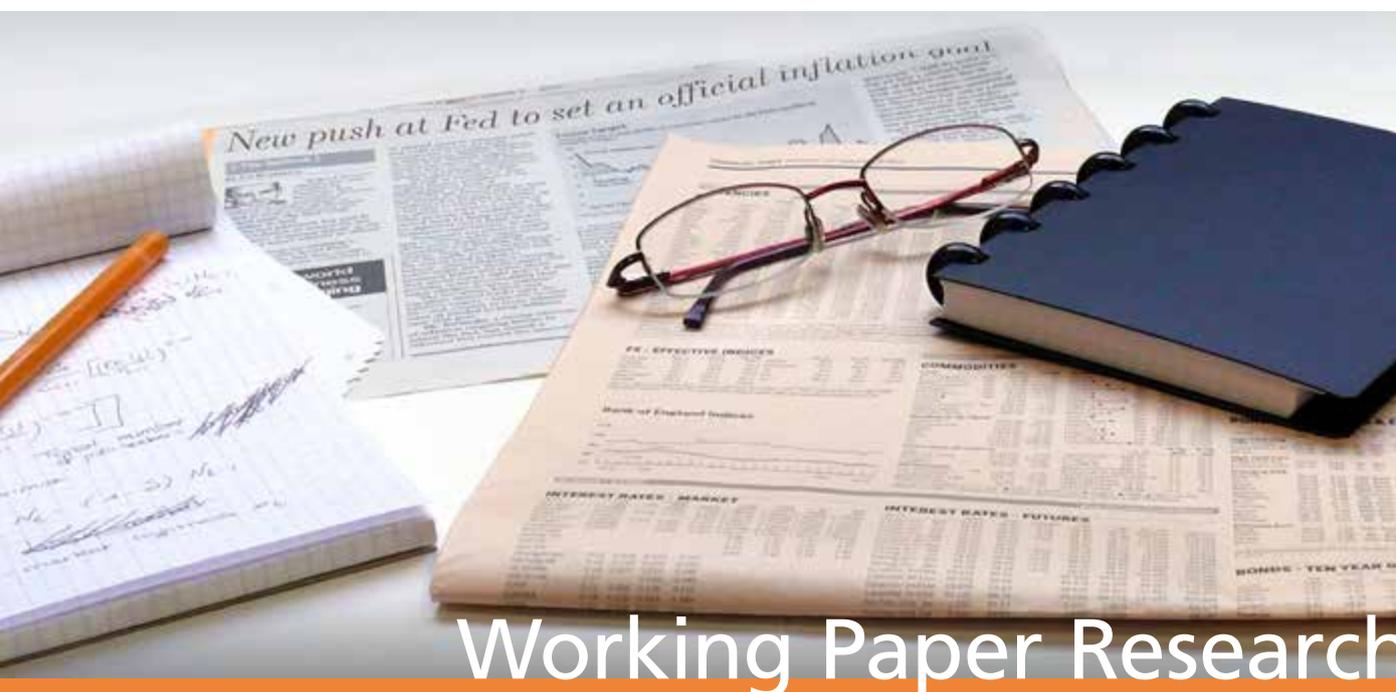


Scrapping the entitlement to unemployment
benefits for young labor market entrants:
An effective way to get them to work?



Working Paper Research

by Bart Cockx, Koen Declercq, Muriel Dejemepe,
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Abstract

We examine the impact of scrapping entitlement to unemployment insurance (UI) on job finding and employment of young labor market entrants. In Belgium, young labor market entrants with short or no employment record are eligible for non-means-tested UI after a one-year waiting period. This zero benefit period gives rise to an unusual inclining benefit profile. We exploit a policy change that restricted access to UI for two groups of job seekers in 2015: university graduates aged 25 and older at the end of their waiting period and high school dropouts younger than 21. At the moment when the reform was announced, many job seekers realized that they were not eligible anymore for UI by the end of their waiting period. We use a differences-in-differences approach to identify the causal impact of the reform. Our main finding is that losing eligibility to UI does not increase the employment probability of targeted youths.

JEL classification: J64, J65, J68

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1 Introduction

High youth unemployment rates are a particular concern for policy makers. Even after the economic crisis, youth unemployment has remained relatively high. In 2017, the unemployment rate was 11.9% among the group of 15-24-year-olds in OECD countries, while the total unemployment rate was only 5.8%. To financially support youths who enter the labor market upon leaving school, developed countries usually provide some form of social protection in case these youths do not immediately find a job. In most countries this consists in a means-tested welfare benefit, but in some others unemployed labor market entrants are entitled to unemployment insurance (UI) without means-test. The insurance principle is arguably justified because, while labor market entrants cannot have contributed to the funding of UI in the past, if risk averse, they are still willing to reimburse the cost of involuntary unemployment when employed in the future. Nevertheless, the benefits of UI must be traded-off against the costs of moral hazard (Baily, 1978; Chetty, 2008). UI for young entrants to the labor market is always accompanied by eligibility conditions that aim at reducing these costs. In Australia and New Zealand UI is provided immediately upon registration as job seeker, but imposes very strict job search requirements (Langenbucher, 2015). In Belgium, Denmark and Luxembourg entitlement to UI is subject to less strict job search requirements, but is postponed by a waiting period lasting up to one year in Belgium.¹ In Sweden, high school graduates were entitled to unemployment benefits (UB) from age 20 onwards until 2007 (von Buxhoeveden, 2019).

This paper evaluates the impact on employment outcomes of the abolishment of this scheme in Belgium for youths aged 25 or older and for high school dropouts younger than 21. As of 2015, these young labor market entrants do no longer have access to UI (unless they provide evidence of a sufficient employment record). A companion paper examines the effect on educational attainment (Cockx et al., 2019). The waiting period gives rise to an inclining benefit profile. In such a setting, employment incentives are maximal at the start of the waiting period. Later on, as one approaches the end of the waiting period, standard job search theory (Mortensen, 1977 and van den Berg, 1990) predicts that forward looking agents progressively decrease search effort and increase the reservation wage.² Empirical evidence about this prediction is lacking because inclining benefit profiles are rarely observed in current UI systems. However, the empirical evidence that the level and, especially, the length of benefit entitlements have significant negative effects on the job finding rate is by now well established (see e.g. Tatsiramos and Van Ours, 2014). Nekoei and Weber (2017) provide an explanation why empirical studies do not consistently find a systematic positive effect of UB on the quality of accepted jobs: While an extension raises the reservation wage, the more selective acceptance behavior implies that the unemployed remain longer unemployed which reduces the quality of the jobs found.³ Empirical studies generally do not find a smooth adjustment but well a “spike” in the exit rate out of unemployment shortly before benefits are exhausted, although the spike is less pronounced when considering the job finding rate instead of the exit rate from unemployment (see e.g. Card et al., 2007, Boone and van Ours, 2012 for a survey and Kyrä et al., 2019). DellaVigna et al. (2017) show that one can explain the presence of such

¹Based on age and educational attainment, young labor market entrants in Luxembourg are eligible for unemployment benefits after a waiting period of six months (Luxembourg Employment Agency, 2019). In Denmark, all labor market entrants who join an unemployment fund within two weeks after graduation can immediately obtain unemployment benefits. Those who register after this two-week deadline, are paid out unemployment benefits only after one year (A-Kasser, 2019).

²In a directed search model, qualitative predictions are similar (see e.g. Nekoei and Weber, 2017; Marinescu and Skandalis, 2019).

³Evidence on the effect of potential benefit duration on the post-unemployment job quality is mixed, with some studies finding a positive effect on subsequent jobs in terms of either higher wages or job stability (e.g. Tatsiramos, 2009, Centeno and Novo, 2009, and Nekoei and Weber, 2015). Other studies find negative or no effects of longer benefit durations on match quality (e.g. Lalive, 2007, Caliendo et al., 2013, Card et al., 2007, van Ours and Vodopivec, 2006, Le Barbanchon, 2016, and Schmieider et al., 2016).

spikes by introducing reference-dependent preferences into standard job search theory.

By studying the abolishment of the UI scheme in Belgium targeted to some specific groups of youth, we aim at providing more evidence on issues on which there is no consensus in the aforementioned literature. While there is agreement on the fact that UI generosity affects job search incentives, there is no agreement on whether behavioral responses to the level of UB are larger in the beginning of the unemployment spell or later on. This matters for the design of the UB profile over time (Shavell and Weiss, 1979; Hopenhayn and Nicolini, 1997; Shimer and Werning, 2008; Kolsrud et al., 2018). If these responses are higher early in the spell than later on this means that the moral hazard costs of raising UB are declining over time, which for a constant or increasing profile of consumption smoothing gains, would imply that an inclining tilt in the benefit profile is Pareto efficient. Kolsrud et al. (2018) find evidence for such decreasing behavioral responses over the unemployment spell in Sweden. By contrast, Lindner and Reizer (2019) report opposite results suggesting that efficiency requires a decreasing benefit profile. A notable difference between these studies is that the changes in behavioral reactions are studied at different moments in the unemployment spell: after 20 weeks in the former and after 90 days in the latter study. One hypothesis explaining these divergent results is that the behavioral responses are non-monotonic. The behavioral reactions in anticipation of a change in the benefit level may dominate early in the unemployment spell, but as time proceeds those who are most sensitive to monetary incentives leave unemployment. Consequently, the pool of individuals who remain unemployed consists increasingly of job seekers who are less sensitive to these incentives, leading to an eventual decline in the behavioral responses.⁴ If this hypothesis holds true, then we should expect that the behavioral reactions of abolishing the inclining tilt in the benefit profile for youths in Belgium has a weak behavioral response, because (i) the benefit reduction occurs late (only after one year) in the unemployment spell and (ii) the zero benefit level during the waiting period induces strong monetary incentives in the beginning of the unemployment spell, so that job seekers who are highly sensitive to monetary incentives leave unemployment at fast pace.

This paper is not the first study of the behavioral effects of waiting periods. Closest to our research is the study by Cockx and Van Belle (2019) who analyze a reform that raised the waiting period of the same UI scheme that we study here from nine to twelve months. Their analysis is targeted to university graduates. They found that raising the waiting period slightly, but statistically insignificantly, increased the transition rate to employment. Nevertheless, the extension of the waiting period did affect the job acceptance behavior of the unemployed graduates. They ended up in shorter term jobs paying less. This effect was larger for youths living in low income households, suggesting that their behavior was induced by liquidity constraints. The relatively small behavioral reactions are consistent with the aforementioned hypothesis, but they could also be the consequence of the modest size of the intervention: delaying the entitlement to UI for three months is minor relative to scrapping the benefit indefinitely as imposed on university graduates aged 25 and older that we study here.

von Buxhoeveden (2019) exploits age discontinuities in the Swedish UI-system for high school graduates. Youths were entitled to UI from age 20 onwards. The study matches individuals born in a given month to a comparison cohort that becomes unemployed at the same time, but is born one month later, and therefore has to wait an additional month before qualifying for UI. The employment hazard is found to decrease significantly by 1 percentage point (-12.5% in relative terms). This relatively high effect might be explained by the fact that the study includes relatively short waiting periods: Incentives kick in before individuals

⁴Note, as underlined by Kolsrud et al. (2018), that part of this declining sensitivity may be due to depreciating skills rather than heterogeneity, but we ignore this for the sake of simplifying the exposition.

highly sensitive to monetary incentives have left unemployment.⁵

Finally, Bolhaar et al. (2019) studied the impact of a waiting period of at most four weeks before entitlement to means-tested welfare benefits in the Netherlands. Compliance to job-search requirements during the waiting period is monitored before getting access to the benefits. Bolhaar et al. (2019) make use of an experimental setting in which some welfare applicants are assigned to a waiting period while others have immediate access to welfare benefits. They find that imposing a waiting period increases total working hours with on average 61 hours 26 weeks after the start of the waiting period. These important behavioral reactions can result from the fact that the considered waiting period is very short, that eligibility to benefits depends on a verification of compliance to job search requirements during this period and that this study considers individuals with very limited financial resources for which the cutting of benefits has stronger implications than for the population that we consider in this study.

In our study the identification relies on a difference-in-differences strategy. It exploits a reform that was implemented unexpectedly by the Belgian government in 2015. Youths aged 25 and older were, as of January 1, 2015, disqualified from getting any UI after their one-year waiting period and, as of September 1, high school dropouts below 21 were also no longer eligible for UI.⁶ The cohorts targeted by the reforms who registered for the first time as job seekers after January 1 or September 1, 2014, respectively for those aged above 25 or below 21, were on December 31, 2014, suddenly informed that they were no longer entitled to UI at the end of their waiting period. By contrast, the waiting period of the youths registering as unemployed job seekers in 2013 (or earlier) came to an end before 2015. Consequently, they were not affected by the reform. Hence, the post- and pre-reform difference between youths affected and not affected by the reform can be estimated respectively within the 2014 and prior to 2014 entry cohorts.

The analysis makes use of administrative population data of all young people who registered for the first time as unemployed job seeker at the Public Employment Services (PES) between 2011 and 2014. We distinguish three regions with contrasting labor market performance (Flanders, Wallonia and Brussels). For privacy reasons we only have access to data grouped by age, educational degree and period of registration, and we observe only the following labor market outcomes six, twelve and eighteen months after registration: (1) the employment rate, (2) the job finding rate defined as the cumulative share of young people who, since registration at the PES, had at least one job experience. The outcomes at six and twelve months are used to study the behavioral adjustment due to the prospect of losing entitlement, while the outcome at eighteen months allows to detect an ex-post effect on the actual loss of benefits.

The findings of our analysis can be summarized as follows. Scrapping the entitlement to UB of young labor market entrants does not significantly affect their probability of being employed, neither before nor after the moment at which benefits could have been claimed in the absence of the reform. This result applies to both 24-year-old college graduates who were permanently excluded from the scheme and to 19-year-old high school dropouts who regained eligibility starting from age 21. While the estimated impact on the job finding rate is small and statistically insignificant for both target groups in Flanders, we do find evidence that in Wallonia, the less prosperous region in the south of Belgium, this (cumulative) job finding rate increased prior to the counterfactual end of the waiting period by four percentage points, but only for college graduates. This effect is however only significant at the 10% level. After the counterfactual end of the waiting period the estimated impact is of similar magnitude, but no longer statistically significant. Given that the probability of being employed is not affected, this result suggests that the perspective of a zero flat-rate benefit profile

⁵The waiting periods range from 3 to 13 months, but the study does not provide information about how the sample is distributed over these periods.

⁶The Council of Ministers took this decision on December 31, 2014.

incentivized some unemployed graduates to acquire a first but inconclusive job experience, while the effective loss of the entitlement to UI did not seem to have generated any additional job finding. For Brussels, we do not find any significant effect on the job finding rate of high school dropouts, nor on the one of master graduates. For the latter group the difference-in-differences approach is not appropriate to identify the causal effect of the reform on the job finding rate after 12 months since the “parallel trends” assumption fails to hold for this outcome.

These findings are consistent with our hypothesis that the reform, even if it is drastic, kicks in at a moment at which the individuals who are highly sensitive to monetary incentives have already left unemployment. The fact that we nevertheless find evidence that university graduates in Wallonia do react by accepting more temporary jobs, while there is no such evidence for Flanders can be explained as follows. First, since the labor market in Flanders is much tighter than in Wallonia, the aforementioned sorting process is more rapid in Flanders than in Wallonia. Second, since Walloon families are more credit constrained than in Flanders, they have higher job search incentives (see Fradkin et al. 2019).⁷ Third, the fraction of youths that is living at their parents’ home is persistently higher in Flanders than in Wallonia.⁸ Consequently, the share of youths for which the income loss induced by the scrapped benefits cannot be compensated by the family is larger in Wallonia than in Flanders.

The fact that there is a significant share of youth that hardly reacts to incentives can be due to an inadequate skill level such that employers don’t offer jobs in spite of a more intensive job search effort, or it can be the consequence of behavioral biases. First, youths are indeed found to be more present biased than adults (Lavecchia et al. 2014) making it less likely that they anticipate a future loss of UI entitlement (DellaVigna and Paserman 2005; Paserman 2008). Second, Mueller et al. (2019) find that unemployed job seekers are persistently over-optimistic in their beliefs with respect to job finding, especially (those who will become) long-term unemployed (see also Spinnewijn, 2015). These authors also stress the importance of heterogeneity in explaining the declining behavioral reactions over the unemployment spell. Finally, consistent with the hypothesis of loss aversion in prospect theory (Kahneman and Tversky, 1979; Tversky and Kahneman, 1992), abolishing the entitlement to a future benefit is expected to have a much smaller impact on behavior than the future withdrawal of existing benefits: individuals react stronger to monetary incentives if they are framed as losses rather than gains. Fryer et al. (2012), Levitt et al. (2012) and Hossain and List (2016) provide evidence of this in different contexts.

The remainder of this paper is organized as follows. Section 2 summarizes the institutional context and the policy reforms. Section 3 takes a first look at our dataset and provides the exact definition of the job finding and employment rate. Section 4 discusses the methodology and section 5 presents the results. Section 6 ends with some concluding remarks.

2 Institutional framework

Belgium is a federal state in which many competences have been decentralized to the regions: Flanders, Wallonia and Brussels.⁹ The payment of unemployment benefits is organized at the federal level while

⁷In 2016, the net taxable average income per capita in Wallonia is 16,787 compared to 19,102 in Flanders (Statbel, 2018). The average share of the population at risk of poverty (using the standard EU definition) amounted to 18.3% and 10.3% respectively in Wallonia and in Flanders in 2015.

⁸In 2014 this share was 82% in Flanders while only 68% in Wallonia (source: www.onem.be).

⁹Flanders is the Dutch-speaking part of Belgium, located in the North. About 58% of the population of 11 million inhabitants lives in Flanders. 32% of the population lives in Wallonia, the French-speaking part located in the South. The remaining 10% lives in Brussels, the bilingual part of the country.

Regional Public Employment Services [PES] are in charge of job search monitoring. In this section we first explain the pre-reform eligibility conditions for UB targeted to people who become unemployed on leaving education. Subsequently, we discuss the policy reform of 2015 that will be evaluated in this study.

2.1 The Activation Allowance

In Belgium, young labor market entrants with no or little work experience are eligible to non-means-tested UI based on age and educational qualifications. This unemployment benefit is called the “activation allowance” and aims to support young unemployed job seekers who, because they recently left education, could not contribute sufficiently to be entitled to the regular UI benefits (which requires proof of at least one year of full-time employment). The activation allowance is a flat rate non-means tested benefit, the level of which depends on age and family status. In 2015, young entrants to the labor market who live on their own without dependents were entitled to a monthly benefit of 494 euro under the age of 21 and 818 euro above the age of 21 (Onem, 2019). In Belgium, most unemployed youth still live with their parents.¹⁰ In 2014, it was the case for 82% and 68% of labor market entrants who claimed the activation allowance below the age of 25 in respectively Flanders and Wallonia (Onem, 2019). In 2015, cohabiting youth were paid out a monthly allowance of 425 euro. Since January 2012, the activation allowance is time limited. For non-heads of households with household income above a certain threshold the time limit was set at three years independently of the age. For other youths this time limit was set only from the age of 30 onwards. Before 2012, there was no time limit.

Young people who become unemployed on leaving education can obtain the activation allowance after completing a waiting period that starts at the moment they register as a job seeker at the PES or start working. Figure 1 shows the entitlement trajectory of an individual with no prior work experience.¹¹ The waiting period lasts at least one year and only periods that one is unemployed or employed count. This period is therefore extended in case of inactivity, such as sickness or resumed education. As such the activation allowance displays an inclining profile of benefits (zero during one year and positive afterwards).

During the waiting period, the PES assists unemployed people in finding a job by providing suggestions for possible jobs and offering training programs to increase the chances of the unemployed on the labor market. In the 6th and 11th month of the waiting period, job seekers must prove that they are actively searching for work by obtaining two positive evaluations by the PES of their job search effort.¹² If they do not, their waiting period is prolonged until they do.¹³

In case one is not entitled to the activation allowance one is still eligible for welfare benefits. Welfare benefits are means-tested and amounted to 556 euro per month for cohabitants and 834 euro for singles in 2015. Since the majority of young people who leave the education system are living with their parents, they generally do not qualify for these welfare benefits because of the means-test. Therefore, most youth who lose the eligibility to the activation allowance, lose the full amount of the benefit (425 euro per month). This

¹⁰Even if it is not known whether all these youths are living at their parents' home, as we only know that they are “cohabiting” with a “head of household” who has a higher income, we are quite sure that this is the case for the vast majority of the cohabitants. We therefore do not make this qualification in the main text.

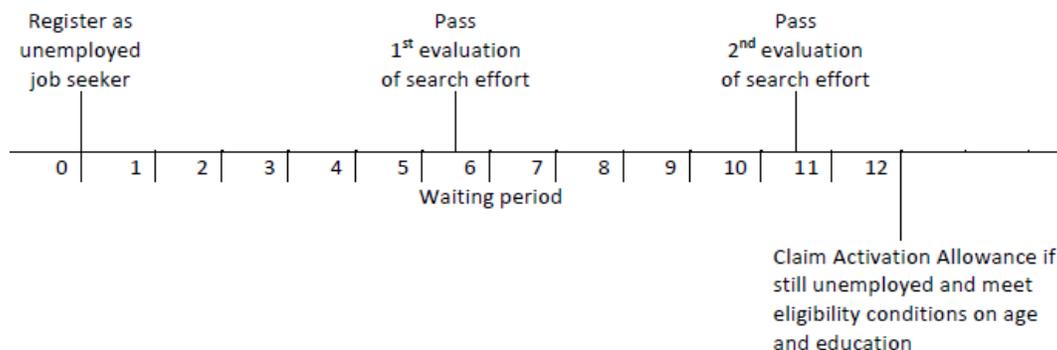
¹¹There is one exception for school-leavers registering as a job seeker in July. They can start their waiting period only from August 1.

¹²At the beginning of his waiting period, an individual receives a letter from the regional PES that informs him of his duty to actively search for work. He then is called for two face-to-face interviews, where a caseworker from the PES reviews any proof of his search effort.

¹³In case of a negative evaluation, individuals must request to be interviewed again. This new interview is carried out six months after their negative evaluation, at the earliest. Nonetheless, the percentage of individuals who get a negative evaluation is rather small, between 4% and 8%, as reported by Onem (2013) and Onem (2014).

could actually motivate some young people to move away from home and live independently, so that they can claim the welfare benefits for singles. This question cannot however be answered by this study.

Figure 1: Trajectory of entitlement to the activation allowance



2.2 The policy reforms

On December 31 2014, the Belgian government signed an agreement to strengthen the conditions for claiming the activation allowance as from January 1 2015. The aim of this reform was to cope with the pervasive incentives the activation allowance may create both in terms of decisions to work and to pursue education. It is unlikely that this reform has been anticipated before January 1, 2015. Even if the principle of the reform was part of the government agreement of October 2014, there had been very little discussion about it in press before its implementation. Moreover, the timing came as a complete surprise at the end of government negotiations on December 31, 2014.

The government agreement involved two major reforms in the eligibility conditions for the activation allowance. Figure 2 provides an overview of the implied changes. First, as of January 1 2015, youth aged 25 or more at the time of the first claim can no longer benefit from the activation allowance.¹⁴ Given the one-year waiting period, individuals should register before their 24th birthday to retain eligibility. The aim of this reform was to increase youth's job search incentives. Second, starting from September 1 2015, school-leavers who did not successfully complete the sixth year of high school cannot claim the activation allowance before the age of 21. Before the reform, secondary school leavers who had chosen the academic track were required only to have completed, but not necessarily passed this sixth year. Students enrolled in other tracks (technical, artistic or vocational track), had just to complete the first three years of high school. The aim of this reform was to encourage young people to attain a high school degree, as this enhances chances on the labor market. However, for young people who had already left high school before completing their degree, the reform is also likely to affect their job seeking strategy. Since high school dropouts regain entitlement to the activation allowance from age 21 onwards, the reforms had more severe consequences for individuals leaving education at the age of 24 or older because they faced a permanent loss of the activation allowance. The reforms implied a change from an inclining to a flat (zero) benefit profile for individuals leaving education at the age of 24 or older. Since high school dropouts regain entitlement to UI from age 21

¹⁴Some young people may be exempted from the age limit of 25 if they were not able to apply for these benefits before the age of 25, for example because of an occupation as a salaried worker. Once a claim has started, non-heads of household with an income above a certain threshold are entitled for maximum three years, so at most until they are 28 years old; other job seekers are entitled maximum until the end of the month of their 33rd anniversary.

onwards, their new benefit profile remains inclined but the zero-benefit period is extended.

As a consequence of these reforms, unemployed youth aged 24 or older were no longer required to attend the two interviews with a caseworker from the PES to provide proof of their job search effort. The suppression of job search monitoring could induce a counterbalancing negative effect on search effort. However Cockx et al. (2018) find that monitoring affects only weakly search behavior of young unemployed workers in Belgium. High school dropouts who left school before the age of 20 were still required to attend both interviews because for them the reform implied only an increase in their waiting period until their 21st birthday.

Figure 2: Policy reforms

Requirement (at the time of claiming UI)	Before the reform	After the reform	Implementation date
Age	< 30	< 25	1 January 2015
Education	<u>General track:</u> Completed the 6 th year of HS <u>Other track:</u> Completed the 3 rd year of HS	< 21: Successfully completed HS ≥ 21: Same previous weaker requirement	1 September 2015

HS: High school

The 2015 policy changes had two features that are key for our identification strategy. First, their announcement on 31 December 2014 was rather unexpected. Therefore, these changes could not have affected any decision taken by the unemployed before 2015, in particular, their decision to register at the PES. Second, even though these policy changes did not affect the current stock of claimants, they did affect those who had registered as job seeker at the PES in the course of 2014, because their one-year waiting period could only end in 2015 when the reform became effective. Consequently, in terms of registration date at the PES, January 1 2014 is the threshold date that separates the pre- from the post-treatment period for youth registering at the PES at the age of 24 or older. For the high school dropouts, September 1 2014 is the threshold date for registering at the PES because the second policy reform came into effect only as from September 1 2015.¹⁵

3 Data

The empirical analysis is based on administrative grouped population data of young individuals who registered for the first time at the PES as unemployed job seekers, either soon after leaving the education system or after a too short previous job. This data was readily provided by the three Belgian regional PES: VDAB in Flanders, FOREM in Wallonia and Actiris in Brussels. The data are grouped by year of registration, period of registration within the year, age and education level. The latter two variables are measured at registration at the PES. For privacy reasons, the data do not contain other characteristics of job seekers.

Table 1 shows the number of young people registering for the first time as a job seeker between the age of 18 and 26.¹⁶ We consider three years in the control period (job seekers registering at the PES between

¹⁵The waiting period can be extended by periods out of the labor force or when job search effort is negatively evaluated by the PES. As a consequence, some job seekers who started their waiting period before January 1 2014 (September 1 2014) ended their waiting period after January 1 2015 (September 1 2015) and are thus affected by the reforms. This happens only for a small minority of job seekers.

¹⁶Individuals can also register for the first time as a job seeker and start their waiting period at the age of 17, if they have their birthday after July, or at the age of 27 or older. Our data do not contain these individuals.

2011 and 2013) and only one year in the treatment period (job seekers registering in 2014) because the decision to register as a job seeker could have been affected by the reform as from January 2015. In Flanders, 33032 individuals registered for the first time as a job seeker in 2011. Among them, 4982 registered at the age of 24 or older against 6287 in 2014. They were disqualified from claiming the activation allowance at the end of their waiting period. In 2014, 2677 individuals registered at the age of 18 or 19 without having obtained a high school degree and were also not eligible for the activation allowance until the age of 21. The corresponding numbers are similar for Wallonia, but relatively more job seekers did not obtain a high school degree. The final panel shows the corresponding numbers for Brussels.

Table 1: Job seekers starting the waiting period

	2011	2012	2013	2014
<i>Flanders</i>				
All job seekers (18-26)	33032	35206	35454	35029
≥ 24	4982	5785	6079	6287
≤ 19 and no high school degree	3312	2936	2902	2677
<i>Wallonia</i>				
All job seekers (18-26)	35692	34224	33495	32955
≥ 24	4807	5132	5269	5696
≤ 19 and no high school degree	5677	4867	4698	4465
<i>Brussels</i>				
All job seekers (18-26)	11661	11159	10324	11489
≥ 24	3711	3682	3710	4254
≤ 19 and no high school degree	1603	1332	1202	1054

Notes: Total number of individuals registering for the first time as an unemployed job seeker at the PES between the age of 18 and 26 between 2011 and 2014. Age is measured at the moment of registration as a job seeker.

Our main outcome of interest is the employment rate measured 6, 12 and 18 months after registration. This (group-level) outcome is defined as the share of unemployed individuals who are in employment at these moments. Unfortunately, this outcome is only available for Flanders and Wallonia, but not for Brussels. Furthermore, we also study the job finding rate. This outcome is defined as the cumulative share of unemployed individuals who found a job, lasting at least one day, within 6, 12 and 18 months after registration at the PES. There are some differences in the measurement of the job finding rate between the different regions in Belgium. First, in Flanders and Wallonia, only salaried jobs are considered, whereas, in Brussels, both salaried jobs and self-employment are taken into account. Second, the actual number of individuals who found a job is observed only in Brussels and Wallonia. In Flanders, employment is measured only on the last working day of the month.¹⁷ Consequently, the outcome measured for the Flemish region underestimates the true job finding rate. We measure the employment and job finding rate during and at the end of the waiting period (6 and 12 months after registration) and when non-treated job seekers can claim unemployment benefits (18 months after registration). Comparing employment and job finding rates of treated and non-treated labor market entrants measured 6 or 12 months after registration allows us to test whether these youths anticipate the future loss of entitlement to unemployment benefits. Comparing these outcomes 18 months after registration

¹⁷As from 2012, individuals employed by a temporary agency in Flanders and working at least 10 days in a given month are also considered in the group of people finding a job. While this change in the measurement did not affect the employment rate, the job finding rate observed in Flanders could be higher as from 2012. Given that we do not reject the assumption that the job finding rate follows a similar trend in the treatment and control groups for Flanders, we argue that this change in measurement does not affect our findings.

allows us to test whether in addition to the anticipation effect, the actual loss of eligibility to unemployment benefits further affects employment.

Young labor market entrants who registered as a job seeker at the age of 24 or older as from January 2014 are affected by the first policy reform and cannot start claiming the activation allowance at the end of their waiting period somewhere in 2015. We limit the analysis to the sample of 23- and 24-year-old job seekers and exclude younger and older job seekers in order to compare employment outcomes of similar individuals in the treatment and control group. In contrast to their elders, 23-year-old job seekers are not affected by the reform and can start claiming the activation allowance at the end of their waiting period. As most individuals registering for the first time as a job seeker at age 23 or 24 have completed a master's degree, we consider only graduates from master programs. Individuals registering as a job seeker in 2014 can only be treated as from January 2015. The employment and job finding rates within 6 months after registration can therefore not be affected for the group registering before July 2014. Consequently, to study the impact of the first policy change, we focus on the subsample of individuals who registered during the second half of the year. More precisely, given data availability, we focus on those who registered at the PES between June and October.¹⁸

Table 2 compares the employment and job finding rates of job seekers registering at the age of 23 and 24 before and after the policy reform for the three different regions in Belgium. The first panel shows that 72% (75%) of individuals registering between 2011 and 2013 as a job seeker at the age of 24 (23) in Flanders is employed 6 months after registration. Employment shares are higher when measured 12 or 18 months after registration. In Flanders (Wallonia), 16% (34%) of young labor market entrants in 2014 in the control group is not employed 12 months after registration, i.e. the scheduled end date of the waiting period. These figures give a rough approximation of the benefit take-up rates among this population.¹⁹

The corresponding job finding rates are higher, suggesting that some job seekers found only a temporary job experience and returned to unemployment. Employment and job finding rates are on average slightly higher for both the 23- and 24-year-old job seekers registering in 2014 reflecting the economic upturn after the recession. The next two panels show that the employment and job finding rates are lower in Wallonia and Brussels than in Flanders. This is most likely related to the much better labor market conditions in Flanders than in the other two regions. As Bodart et al. (2018) pointed out, in spite of the small size of the country, (un)employment rates are extremely varied, first and foremost in a north-south regional (linguistic) dimension. In 2015, the unemployment rate among the group of 15-24-years-old amounted to 15.2% in Flanders, 32.2% in Wallonia, and 36.2% in Brussels (Eurostat, 2019).

¹⁸Job seekers registering at the PES before June 2014 cannot have been treated within 6 months after registration. Given that an academic year does not end before June 30, only a small number of school-leavers registers as a job seeker in June.

¹⁹Not all individuals who are not in employment at the end of the one-year waiting period will start claiming UB. People not employed after 12 months could be back in education or in a training program of the PES. If during the waiting period, there is a spell in inactivity (including training), the counter of the waiting period is interrupted. Even if this is not often the case, a negative evaluation of search effort after 6 or 11 months also postpones the moment at which someone can claim the activation allowance.

Table 2: Employment rate and job finding rate of 23- and 24-year-old job seekers with a master's degree

	2011-2013		2014	
	Age 24 (T)	Age 23 (C)	Age 24 (T)	Age 23 (C)
<i>Flanders</i>				
Employment rate after 6 months	0.72	0.75	0.74	0.77
Employment rate after 12 months	0.80	0.82	0.83	0.84
Employment rate after 18 months	0.86	0.88	0.89	0.91
Job finding rate within 6 months	0.78	0.81	0.78	0.81
Job finding rate within 12 months	0.90	0.91	0.91	0.92
Job finding rate within 18 months	0.93	0.94	0.94	0.95
No. of individuals	3748	5541	1366	1979
<i>Wallonia</i>				
Employment rate after 6 months	0.50	0.53	0.53	0.57
Employment rate after 12 months	0.59	0.63	0.62	0.66
Employment rate after 18 months	0.70	0.72	0.60	0.62
Job finding rate within 6 months	0.60	0.62	0.60	0.59
Job finding rate within 12 months	0.72	0.74	0.74	0.72
Job finding rate within 18 months	0.77	0.79	0.80	0.79
No. of individuals	3253	3898	1211	1366
<i>Brussels</i>				
Job finding rate within 6 months	0.65	0.68	0.65	0.69
Job finding rate within 12 months	0.81	0.84	0.80	0.82
Job finding rate within 18 months	0.89	0.89	0.86	0.91
No. of individuals	793	619	248	194

Notes: Average outcomes for 23- and 24-year-old job seekers with a master's degree, registering between June and October in the period 2011-2014. Age is measured at the moment of registration as a job seeker. Individuals registering at the age of 24 were not eligible anymore for the activation allowance after the reform (treatment group). Individuals registering at the age of 23 can still claim the activation allowance and are assigned to the control group. The employment and job finding rates are measured within 6, 12 and 18 months after the month of registration.

School-leavers who did not complete high school and registered as a job seeker at the age of 19 or younger as from September 2014 are affected by the second reform and can start claiming the activation allowance only as from their 21st birthday. 20-year-old job seekers are not affected by this reform and continue to start claiming the activation allowance after the one-year waiting period. We disregard job seekers who completed only elementary school or the first stage of high school in order to exclude individuals with very long school delay. As the second policy change was enforced as from September 2015, job seekers who started their one-year waiting period before September 2014 are not affected by this reform. Therefore, we consider only job seekers who registered at the PES between September and December.

Table 3 shows the corresponding outcomes for the 19- and 20-year-old job seekers who completed only the second stage of high school and thus did not obtain a high school degree. Employment and job finding rates are considerably lower for this group than for the group of university graduates in Table 2. Only 30% of 19-year-old job seekers without a high school degree is employed 6 months after registration at the PES in Flanders. In Flanders (Wallonia), 59% (79%) of young labor market entrants in 2014 in the control group is not employed 12 months after registration, i.e. the scheduled end date of the waiting period. These figures which roughly approximate the benefit take up rates in the control group, are much larger than those observed

for university graduates. Overall employment outcomes are slightly better for job seekers in the control group (20-year-old at the moment of registration at the PES).

Table 3: Employment rate and job finding rate of 19- and 20-year-old job seekers without a high school degree

	2011-2013		2014	
	Age 19 (T)	Age 20 (C)	Age 19 (T)	Age 20 (C)
<i>Flanders</i>				
Employment rate 6 months after	0.30	0.33	0.28	0.27
Employment rate 12 months after	0.33	0.39	0.35	0.41
Employment rate 18 months after	0.34	0.39	0.37	0.43
Job finding rate within 6 months	0.38	0.43	0.34	0.36
Job finding rate within 12 months	0.50	0.53	0.47	0.54
Job finding rate within 18 months	0.56	0.59	0.51	0.59
No. of individuals	711	399	228	135
<i>Wallonia</i>				
Employment rate 6 months after	0.16	0.17	0.16	0.16
Employment rate 12 months after	0.19	0.22	0.15	0.21
Employment rate 18 months after	0.24	0.26	0.20	0.24
Job finding rate within 6 months	0.35	0.37	0.35	0.31
Job finding rate within 12 months	0.48	0.49	0.52	0.51
Job finding rate within 18 months	0.54	0.56	0.58	0.57
No. of individuals	1278	848	413	277
<i>Brussels</i>				
Job finding rate within 6 months	0.31	0.36	0.22	0.28
Job finding rate within 12 months	0.44	0.49	0.36	0.45
Job finding rate within 18 months	0.52	0.56	0.46	0.56
No. of individuals	460	381	99	118

Notes: Average outcomes for 19- and 20-year-old job seekers who completed only the second stage of high school and registered between September and December in the period 2011-2014. Age is measured at the moment of registration as a job seeker. Individuals registering at the age of 19 were not eligible anymore for the activation allowance after the reform (treatment group). Individuals registering at the age of 20 can still claim the activation allowance and are assigned to the control group. The employment and job finding rates are measured within 6, 12 and 18 months after the month of registration.

4 Empirical Strategy

To estimate the causal impact of both policy changes, we make use of the difference-in-differences approach and compare employment outcomes in the treatment group before and after the policy reform with employment outcomes in the control group. Data limitations prevent us from delineating treatment and control groups perfectly. For both reforms the treatment groups consist of individuals who are in the waiting period for the activation allowance at the end of 2014, but who, as a consequence of the reform, unexpectedly fail to meet the new eligibility criteria. By contrast, in the data we measure the labor market outcomes of *all* individuals who registered in 2014 as unemployed job seeker at the PES: We cannot identify the sub-population for which the waiting period is ongoing at the end of 2014 because some job seekers could already have found a stable job before 2015. Consequently, not all job seekers in the treatment group are effectively treated. Furthermore, for a subgroup of the considered population the waiting period may already have expired before

the end of 2014, because we cannot exclude that some individuals, who registered at the PES for the first time in 2014, started their waiting period already in 2013. This concerns young people who immediately started working upon labor market entry - which counts for the waiting period. Therefore, since we cannot exclude non-compliance for the aforementioned reasons, the identified treatment effects must be interpreted as *intention-to-treat* effects.

In the evaluation of both policy changes, we estimate a linear probability model using ordinary least squares and robust standard errors. We expand the grouped data to the individual level using the number of individuals within each group. We perform this expansion to correctly estimate the standard errors of our estimators at the micro data level (Angrist and Pischke, 2013, p. 40). To estimate the causal impact of the reform, we estimate the following difference-in-differences model:

$$Y_{it} = \alpha + \beta D_i + \gamma_t T_t + \delta D_i \times T_{2014} + \epsilon_{it},$$

for $t \in \{2011, \dots, 2014\}$, and where Y_{it} is a dummy equal to one if individual i , who registered at the PES during year t is employed after 6, 12 or 18 months after registration (alternatively, has worked at least one day within 6, 12 or 18 months after registration). D_i is an indicator equal to one if the job seeker is affected by one of the two policy changes. T_s is an indicator equal to one if the individual registered at the PES in year s and zero otherwise. δ is the difference-in-differences estimator that measures the impact of the reform on the outcome.

This model allows for any potential time-constant difference between the employment outcomes of job seekers in the control group and the treatment group, captured by β , and time effects common to both, captured by γ_t . Nonetheless, it rules out additional time effects that differ between the treated and control group in the absence of the reform. In particular, this model relies on the assumption that both groups would have followed a common trend in their employment outcomes if the reform had not taken place. Under this identifying assumption, the parameter δ captures any deviation from this common trend induced by the reform, that is, its causal effect. To formally assess the validity of our common trends assumption, we test whether the employment rate and job finding rate of the treated and control groups had a common trend during the pre-treatment period 2011-2013. Therefore, we estimate a similar regression with interaction effects between the treatment groups and year dummies on the pre-reform period. The parallel trends assumption is rejected if these interaction effects are jointly significant from zero.

5 Results

In this section, we discuss the empirical results. We start by evaluating whether the first policy change affected the employment and job finding rates of the 24-year-old job seekers with a master's degree. We present both a graphical and corresponding econometric analysis of the difference-in-differences model. Next, we study the second policy change and evaluate employment outcomes of 19-year-old job seekers without a high school degree.

5.1 24-year-old job seekers with a master's degree

Job seekers who registered at the PES in 2014 at the age of 24 or older cannot start claiming the activation allowance anymore at the end of their one-year waiting period in 2015. The loss of eligibility to the activation allowance could therefore have intensified job search or decreased the reservation wage of job seekers resulting

in a higher job finding and employment rate.

Figure 3 compares the employment and job finding rates of 23- and 24-year-old job seekers who registered at the PES with a master's degree between June and October. Outcomes are shown before and after the reform for the three regions in Belgium (Flanders, Wallonia and Brussels). The employment and job finding rates are measured 12 months after the first registration as a job seeker. The year of registration is on the horizontal axis. Individuals who registered at the age of 23 were not affected by the reform and could start claiming the activation allowance after their one-year waiting period in 2015. The thick solid line shows the employment and job finding rate for 24-year-old job seekers who suddenly realized on January 1, 2015 that they could not claim the activation allowance at the end of their waiting period. In Flanders (panel A) approximately 80% of job seekers in the treatment group is employed one year after registration. Approximately 90% has at least once transited to employment between registration as a job seeker and one year later. The thin solid line presents the counterfactual outcome of the treatment group in the absence of the policy reform. The 95% confidence interval of the counterfactual outcome is also shown in the graphs.²⁰

Comparing the observed outcomes of the treated (thick solid line) with their counterfactual outcome in absence of the reform (thin solid line) provides a first assessment of the parallel trends assumption. From both graphs, we see that the observed outcome of the treatment group in Flanders remains within the 95% confidence interval of their counterfactual outcome of the treatment group before the policy reform. This suggests that the parallel trends assumption is not rejected for both outcome variables. In the estimation of the difference-in-differences models we formally test for parallel trends by a joint F-test. In 2014, the year of registration where job seekers are affected by the policy reform, the observed outcome is also similar to the counterfactual outcome of the treated and remains within the 95% confidence interval. The figures seem to suggest that young labor market entrants do not anticipate the loss of future eligibility to the activation allowance because neither employment nor job finding of treated job seekers was significantly affected just before the moment when non-treated job seekers can start claiming UB.

The next panel shows the corresponding outcomes for Wallonia. The employment and job finding rates are lower than in Flanders and also remain within the 95% confidence interval of the counterfactual outcome of the treated in the pre-reform period. In 2014, the job finding rate almost exceeds the 95% confidence interval. This suggests that the reform could have raised the job finding rate in Wallonia. By contrast, there is no statistically significant increase in the employment rate. The final panel shows that the observed job finding rate in Brussels stays within the 95% confidence interval of the counterfactual outcome both before and after the policy reform. Confidence intervals are wider because of the smaller sample size for this region as illustrated in Table 2. While the figure suggests that the policy change did not have any significant effect in Brussels, the stark fluctuations of this rate in the pre-treatment period suggest that no firm conclusions can be made for this region. Figure A1 in Appendix shows the corresponding figures for the employment outcomes measured 6 months after registration. Employment and job finding rates measured after 6 months are lower than after 12 months, but the trends before and after the reform are similar.

Figure A2 in Appendix shows the corresponding graphical analysis for the employment and job finding rates measured 18 months after registration. Measuring employment outcomes 18 months after registration

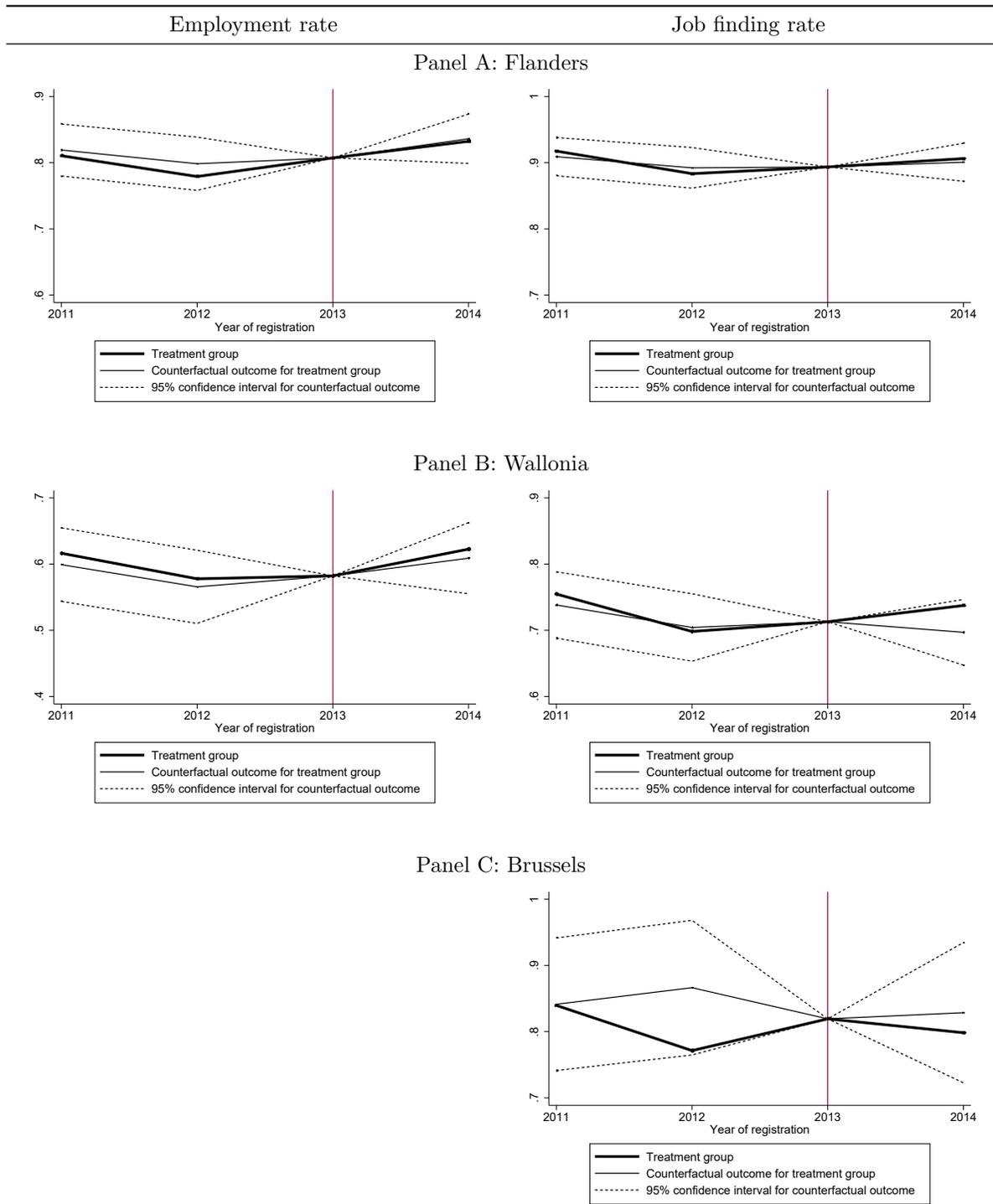
²⁰We constructed similar graphs as in Albanese and Cockx (2019). The counterfactual outcome of the treatment group in absence of the policy reform is predicted by a difference-in-differences model with interaction effects between the treatment group and time dummies for the pre- and post-reform periods. The counterfactual outcome of the treatment group is obtained by setting these interaction effects to zero. The corresponding 95% confidence interval is computed from the standard errors of the interaction effects between time dummies and the treatment group. The counterfactual outcome is shifted to the level of the observed outcome of the treated in the year before the policy reform. By construction, the confidence interval is zero for this year (the reference one).

allows us to test whether the actual loss of entitlement to UI affects employment in addition to the anticipation effect towards the end of the waiting period. The outcomes for the control and treatment group follow a parallel trend in the pre-reform period. In 2014, the employment and job finding rates remain within the 95% confidence interval. There is also some positive effect on job finding in Wallonia, but, contrary to the effect measured at 12 months, it is further way from the upper bound of the 95% confidence interval.

Table 4 presents the output of the difference-in-differences estimation for the employment and job finding rates within respectively 6, 12 and 18 months after registration as a job seeker. We present only the coefficients of the treatment effects and the counterfactual outcome of the treated in absence of the policy reform as predicted by our model. Table A1 in Appendix shows the complete set of coefficient estimates. To test for parallel trends, we estimate placebo regressions for the difference-in-differences model on the pre-reform period and include interaction effects between the treatment group with time dummies. The p-value of the F-test that the interaction effects are jointly insignificant from zero is reported in the Table. Based on these p-values, we can conclude that the parallel trends assumption is never rejected at the 5% level except for Brussels when the job finding rate is measured after 12 months. In the latter case only, the difference-in-differences approach is not appropriate to identify the causal effect of the reform.

The different specifications show that losing entitlement to the activation allowance did not significantly increase the employment rate and the job finding rate of 24-year-old job seekers in Belgium. The point estimates are very close to zero for all treatment effects estimated in Flanders and for the treatment effects on employment estimated in Wallonia. Only in this region, we estimate a positive effect for the job finding rate measured within 12 months after registration an effect that is significant at the 10% level. The job finding rate increased by 3.7% points. In absence of the reform, 66.1% of job seekers would have found a job within 12 months after registration. This fraction increases to 69.8% because of the policy reform, a proportional increase of 5.6%. Given that the probability of being employed is not affected in Wallonia, this suggests that the policy incentivized some Walloon unemployed graduates to acquire a first but inconclusive job experience. The positive effect on job finding is nevertheless estimated with a certain degree of uncertainty since the 90% confidence interval spans a range of positive values from 0.004 to 0.070.

Figure 3: Employment outcomes of 24-year-olds with a master's degree (12 months after registration)



Notes: Treatment group = 24-year-old job seekers. Control group = 23-year-old job seekers. Outcomes are measured 12 months after registration at the PES for job seekers who register between June and October after having obtained a master's degree. The vertical line is drawn at the last period before the reform. The thick solid line shows the observed outcome of the treatment group. The fine solid line shows the counterfactual outcome of the treatment group in absence of the treatment. The thin dotted lines are the 95% confidence interval for the counterfactual path.

Table 4: Employment rate and job finding rate: 24-year-old job seekers (difference-in-differences)

	Employment rate			Job finding rate		
	6 months	12 months	18 months	6 months	12 months	18 months
Panel A: Flanders						
Treatment effect	-0.004 (0.018)	0.005 (0.015)	-0.000 (0.013)	-0.004 (0.017)	0.006 (0.012)	0.006 (0.010)
Counterfactual probability	0.740	0.827	0.888	0.787	0.901	0.934
Parallel trends: p-value	0.843	0.642	0.551	0.591	0.531	0.531
Observations	12634	12634	12634	12634	12634	12634
Panel B: Wallonia						
Treatment effect	-0.010 (0.023)	0.004 (0.022)	0.004 (0.022)	0.032 (0.023)	0.037* (0.020)	0.021 (0.019)
Counterfactual probability	0.542	0.619	0.596	0.566	0.661	0.776
Parallel trends: p-value	0.458	0.821	0.848	0.765	0.477	0.998
Observations	9728	9728	9728	9728	9728	9728
Panel C: Brussels						
Treatment effect				-0.017 (0.052)	0.004 (0.043)	-0.041 (0.035)
Counterfactual probability				0.666	0.794	0.904
Parallel trends: p-value				0.445	0.086	0.117
Observations				1854	1854	1854

Notes: Treatment group = 24-year-old job seekers. Control group = 23-year-old job seekers. Age is measured at the moment of registration as a job seeker. Control period = 2011-2013, treatment period = 2014. Outcomes are measured for job seekers who register between June and October after having obtained a master's degree. The counterfactual outcome is the predicted outcome for the treated in absence of the treatment in the post-reform period. To test for parallel trends, we estimate similar regressions with interaction effects between the treatment groups and year dummies. The parallel trends assumption is rejected if these interaction effects are jointly significant from zero in the pre-reform period. The p-value of this test is reported. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Our results for the employment rate are not in line with the predictions from job search theory (Mortensen, 1977 and van den Berg 1990). The prospect of being disqualified from UB should increase job search effort and decrease selectivity in job acceptance behavior, either by decreasing reservation wages or accepting less stable jobs, as the point where the loss of benefits entitlement is approached. There are several potential explanations for the weak behavioral reactions of scrapping future entitlement to UI.

First, even if it implied a substantial income loss, the reform kicks in at a moment at which the individuals who are highly sensitive to monetary incentives have already left unemployment. This sorting process operates rapidly since the absence of benefits during the one-year waiting period provides strong work incentives from the outset of the period. Since the labor market in Flanders is much tighter than in Wallonia, the sorting process is even more rapid in the former region. This can explain why we find evidence that university graduates in Wallonia do react by accepting more temporary jobs close to the end of the waiting period, while there is no such evidence for Flanders. Another reason of this diverging result is that we consider a population of youth of whom the overwhelming majority still cohabitate and can therefore rely on financial support from the family. Because of liquidity constraints, such compensation is less likely in Wallonia, the less prosperous region of the country, than in Flanders.

Second, behavioral biases may also explain why there is a significant share of young labor market entrants that hardly reacts to incentives. For instance, youths have been found to be more present biased than adults (Lavecchia et al. 2014) making it less likely that they anticipate a future loss of UI entitlement. Focusing on the job seekers' perceptions about their employment probabilities, Mueller et al. (2019) also provide evidence that unemployed job seekers are persistently over-optimistic in their beliefs with respect to job finding, especially those (who will become) long-term unemployed (see also Spinnewijn, 2015).

5.2 19-year-old job seekers without a high school degree

Job seekers who registered at the PES in 2014 at the age of 19 or younger without having completed high school cannot start claiming the activation allowance anymore before their 21st birthday. For those job seekers, the treatment lengthens their waiting period by up to one year, which could have intensified job search and increased their job acceptance probability.

Figure 4 compares the employment and job-finding rates of 19- and 20-year-old job seekers without a high school degree before and after the reform for Flanders, Wallonia and Brussels. The employment rate and job finding rate are measured 12 months after registration at the PES between September and December. Individuals who registered at the age of 20 in 2014 were not affected by the reform and could start claiming the activation allowance after their one-year waiting period in 2015. The graphs display the observed outcomes of the treatment group (thick solid line) and the counterfactual outcome of the treated in absence of the policy reform (thin solid line) surrounded by its 95% confidence interval. Employment and job finding rates are substantially lower for the group of high school dropouts compared to those of the master graduates in Figure 3. From the graphs, the parallel trends assumption is marginally violated in 2012 for the employment rate in Flanders. In 2014, the observed employment and job finding rate are not significantly different from the counterfactual outcome of the treated. Notice that due to the smaller sample size confidence intervals are wider for this age group than for the older job seekers in Figure 3. Figure A3 and Figure A4 in the Appendix show the corresponding graphs for the employment rate and job finding rate measured within respectively 6 and 18 months after registration for job seekers without a high school degree. These figures shows that trends are parallel between the treatment and control group in the pre-reform period, but also in the year after the policy reform.

Table 5 presents the output of the difference-in-differences estimation for the employment rate and the job finding rate. The parallel trends assumption is never rejected. We do not find a significant effect nor on employment nor on the job finding. The point estimates are for some outcomes relatively large in Flanders, but vary between positive and negative values in a non systematic and coherent way. In Wallonia, point estimates seem to suggest a positive treatment effect on the job finding rate only, even more so within 6 than 12 or 18 months. However, even this result corroborates the one found for the university graduates in Wallonia, the uncertainty around its estimation is very high. It should therefore be interpreted with much caution. Table A2 in appendix shows the complete set of coefficient estimates. For Brussels, treatment effects are negative, but imprecisely estimated due to the smaller sample size.

Scrapping the entitlement to UB of 19-year-old high school dropouts does not significantly affect their probability of being employed or their job finding rate, neither before nor after the moment at which benefits could have been claimed in the absence of the reform. The absence of behavioral reactions for this group are consistent with the explanations proposed for the 24-year-old college graduates, but they could also be the consequence of the less radical change of the eligibility conditions: delaying the entitlement to UI for some months (up to 12) is less stringent than scrapping the benefit indefinitely as imposed on university graduates

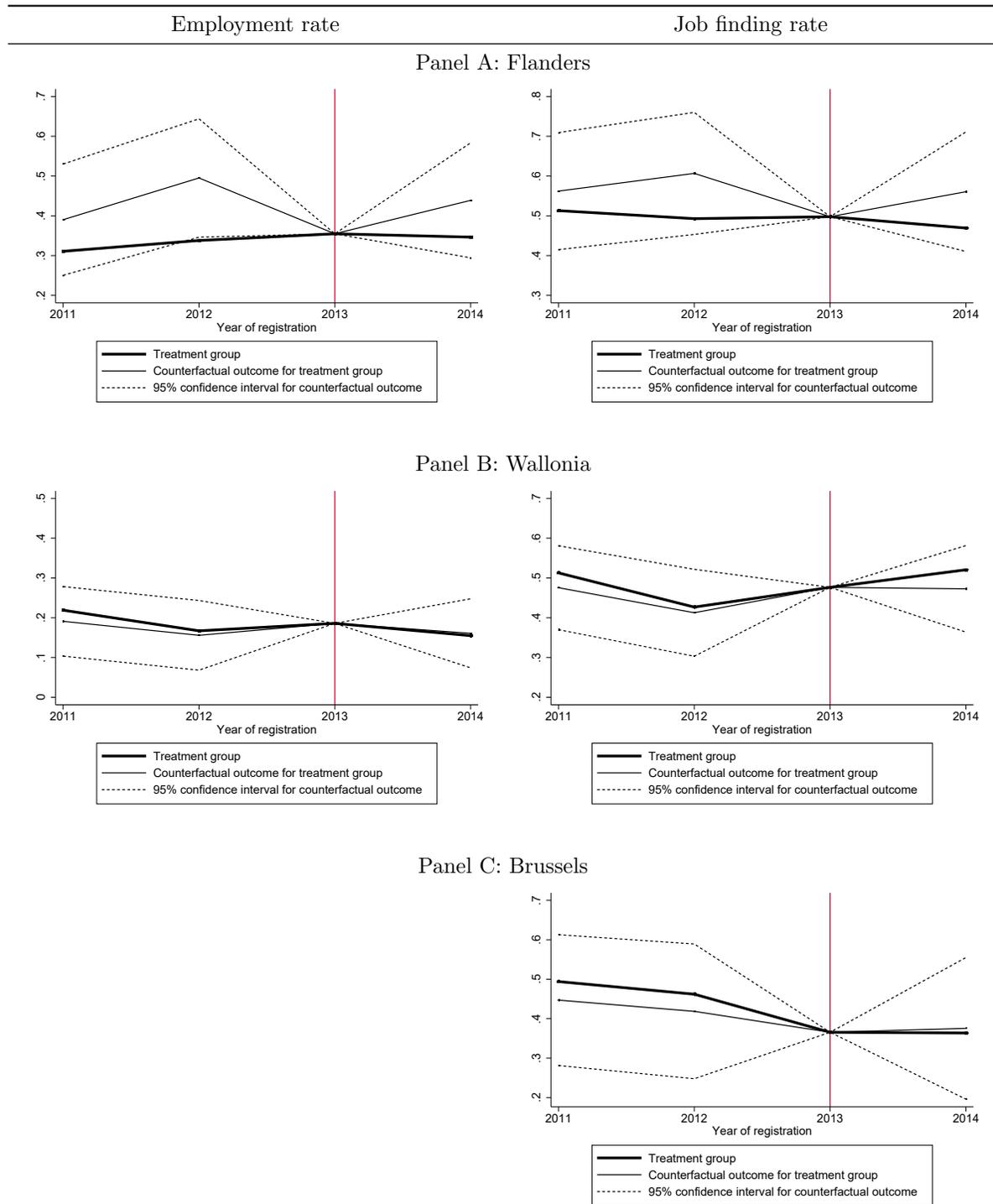
aged 25 or older.

Table 5: Employment rate and job finding rate: 19-year-old job seekers (difference-in-differences)

	Employment rate			Job finding rate		
	6 months	12 months	18 months	6 months	12 months	18 months
Panel A: Flanders						
Treatment effect	0.043 (0.056)	-0.016 (0.061)	-0.004 (0.061)	0.018 (0.060)	-0.039 (0.063)	-0.046 (0.062)
Counterfactual probability	0.234	0.363	0.560	0.320	0.508	0.560
Parallel trends: p-value	0.528	0.117	0.366	0.806	0.347	0.366
Observations	1473	1473	1473	1473	1473	1473
Panel B: Wallonia						
Treatment effect	0.013 (0.033)	-0.020 (0.035)	-0.014 (0.038)	0.062 (0.042)	0.030 (0.045)	0.021 (0.044)
Counterfactual probability	0.147	0.175	0.215	0.284	0.491	0.558
Parallel trends: p-value	0.959	0.818	0.797	0.892	0.781	0.962
Observations	2816	2816	2816	2816	2816	2816
Panel C: Brussels						
Treatment effect				-0.006 (0.067)	-0.044 (0.075)	-0.064 (0.076)
Counterfactual probability				0.228	0.407	0.529
Parallel trends: p-value				0.509	0.833	0.845
Observations				1058	1058	1058

Notes: Treatment group = 19-year-old job seekers. Control group = 20-year-old job seekers. Age is measured at the moment of registration as a job seeker. Control period = 2011-2013, treatment period = 2014. Outcomes are measured for job seekers without a high school degree who register between September and December. The counterfactual outcome is the predicted outcome for the treated in absence of the treatment in the post-reform period. To test for parallel trends, we estimate similar regressions with interaction effects between the treatment groups and year dummies. The parallel trends assumption is rejected if these interaction effects are jointly significant from zero in the pre-reform period. The p-value of this test is reported. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure 4: Employment outcomes of 19- and 20-year-olds without a high school degree (12 months after registration)



Notes: Treatment group = 19-year-old job seekers. Control group = 20-year-old job seekers. Age is measured at the moment of registration as a job seeker. The vertical line is drawn at the last period before the reform. The thick solid line shows the observed outcome of the treatment group. The fine solid line shows the counterfactual outcome of the treatment group in absence of the treatment. The thin dotted lines are the 95% confidence interval for the counterfactual path.

6 Concluding Remarks

This paper evaluated the impact on the transition to work of a policy reform in Belgium that restricted the access to a particular unemployment benefit (UB) scheme for youths who left education, but who acquired insufficient work experience to be eligible for the regular unemployment insurance (UI). Under certain conditions these youths could claim unemployment benefits after a waiting period of one year. As of 2015, the Belgian government unexpectedly scrapped entitlement to these benefits for youths older than 25 and for high school dropouts younger than 21. The reforms implied a change from an inclining to a (zero) benefit profile for individuals leaving education at the age of 24 or older. Since high school dropouts regain entitlement to UI from age 21 onwards, their new benefit profile remains inclined but the zero-benefit period is extended.

Based on grouped data of university graduates aged 23 and 24 at the start of unemployment and on high-school dropouts aged 19 and 20 we used a difference-in-differences approach to find an answer to our research question. Our main finding is that scrapping the entitlement to UB of young labor market entrants did not significantly affect their probability of being employed, neither before nor after the moment at which benefits could have been claimed in the absence of the reform. This result applies to both 24-year-old college graduates who were permanently excluded from the scheme and to 19-year-old high school dropouts who regained eligibility starting from age 21. It should be stressed that this conclusion holds in both Flanders and Wallonia despite their major differences in terms of labor market performance. We also consider another outcome: the cumulative share of young people who, since registration as unemployed, have at least one job experience (called the job finding rate). While the estimated impact on the job finding rate is small and statistically insignificant for both target groups in Flanders, we found evidence that in Wallonia, the less prosperous region in the south of Belgium, this (cumulative) job finding rate increased prior to the counterfactual end of the waiting period by four percentage points, but only for college graduates. This effect is however only significant at the 10% level. Similar to the other two regions, we did not find an effect on the job finding rate for high school dropouts in Brussels. We also did not find an effect on the job finding rate of master graduates, but our evaluation method is not appropriate for the job finding rate measured after 12 months for this group.

We argued that these findings are consistent with those of Kolsrud et al. (2018) for the regular UI scheme in Sweden, namely that the behavioral reaction to changes in the benefit generosity is smaller for long-term than for short-term unemployed, likely because the zero benefit level during the one-year waiting period induced the youths most sensitive to monetary incentives to leave unemployment prior to the moment that the anticipation of the loss of the UI entitlement could start to bite.²¹

From a policy perspective these findings suggest that the scrapping of the benefit eligibility at the end of the waiting period has not been the right decision. While the gain in terms of reducing the moral hazard cost is found to be non significant, the lost consumption smoothing value is likely to be great, in particular because this value is increasing over the unemployment spell.

Nevertheless, we must be careful in drawing this policy conclusion, because of a number of limitations that we faced in this study. First, the consumption smoothing value of the long-term unemployed youths might not be so high as for prime aged workers in a regular UI scheme. This is because the vast majority of these youths is still living at their parents' home. Parents might indeed absorb this income loss. Moreover, to the extent that their parents could not cover the income loss, these youths might still claim means-tested

²¹We cannot exclude the alternative explanation that individuals were all equally sensitive to monetary incentives at the start of unemployment and that this sensitivity depreciates over the unemployment spell. Mueller et al. (2019), however, report evidence that heterogeneity is the dominant explanation.

welfare benefits. However, stigma and administrative hurdles could have prevented the take-up of these means-tested benefits. In addition, these alternative arrangements for covering the income loss may come at a cost of restricting the process of parental emancipation. Currently, the required data to study these issues are lacking.

A second limitation of this study is that the results should be interpreted as intent-to-treat estimates and, hence, as lower bounds of the actual treatment effects. There are two main reasons for this. First, we only had data available for youths as from the start of their first registration in 2014 as job seeker. Since the policy reform was only decided at the very end of 2014, all exits from unemployment in the course of 2014 could not have been influenced by the policy reform: The treatment effect applies only to the subpopulation that is still unemployed at the start of 2015. Second, because the waiting period does not only include periods of unemployment, but also periods of employment, individuals who registered for the first time in 2014 may have started their waiting period prior to this moment. This is the case for youths who found a temporary job immediately after leaving education without any intervening spell of unemployment. For these individuals the one-year waiting period may therefore have ended before the reform was enacted.

Our study provides useful insight on the work distortionary effect of a UI scheme which, given its inclining tilt profile, has received little attention in the past. A third limitation is however that we only had access to grouped data and to a limited set of outcome variables. Consequently, we could neither analyze treatment heterogeneity, nor investigate to what extent the reform pushed these youths into poverty or affected other dimensions of job quality than duration. An avenue of future research is therefore to address these limitations by collecting new data.

7 References

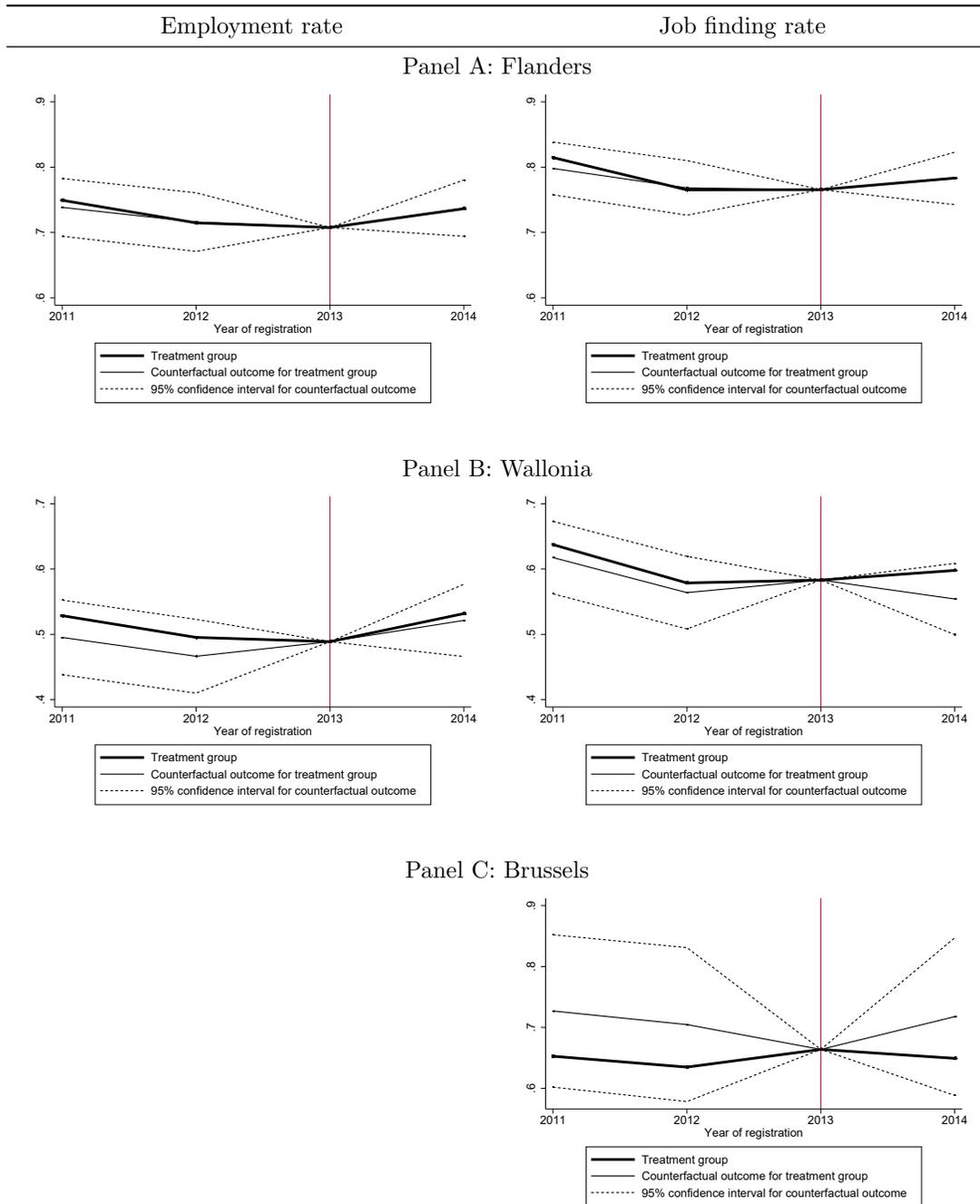
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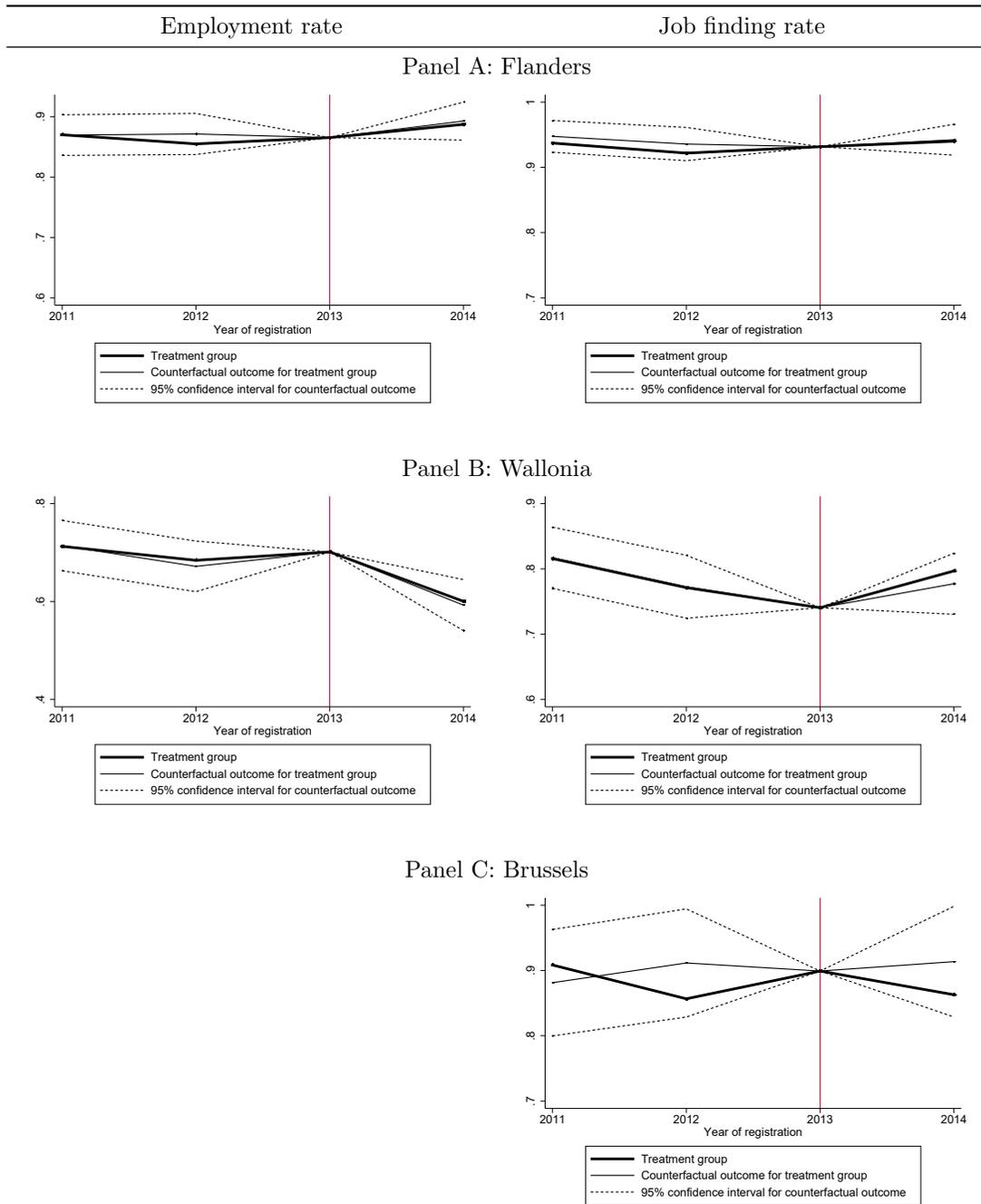
8 Appendix

Figure A1: Employment outcomes of 24-year-olds with a master's degree (6 months after registration)



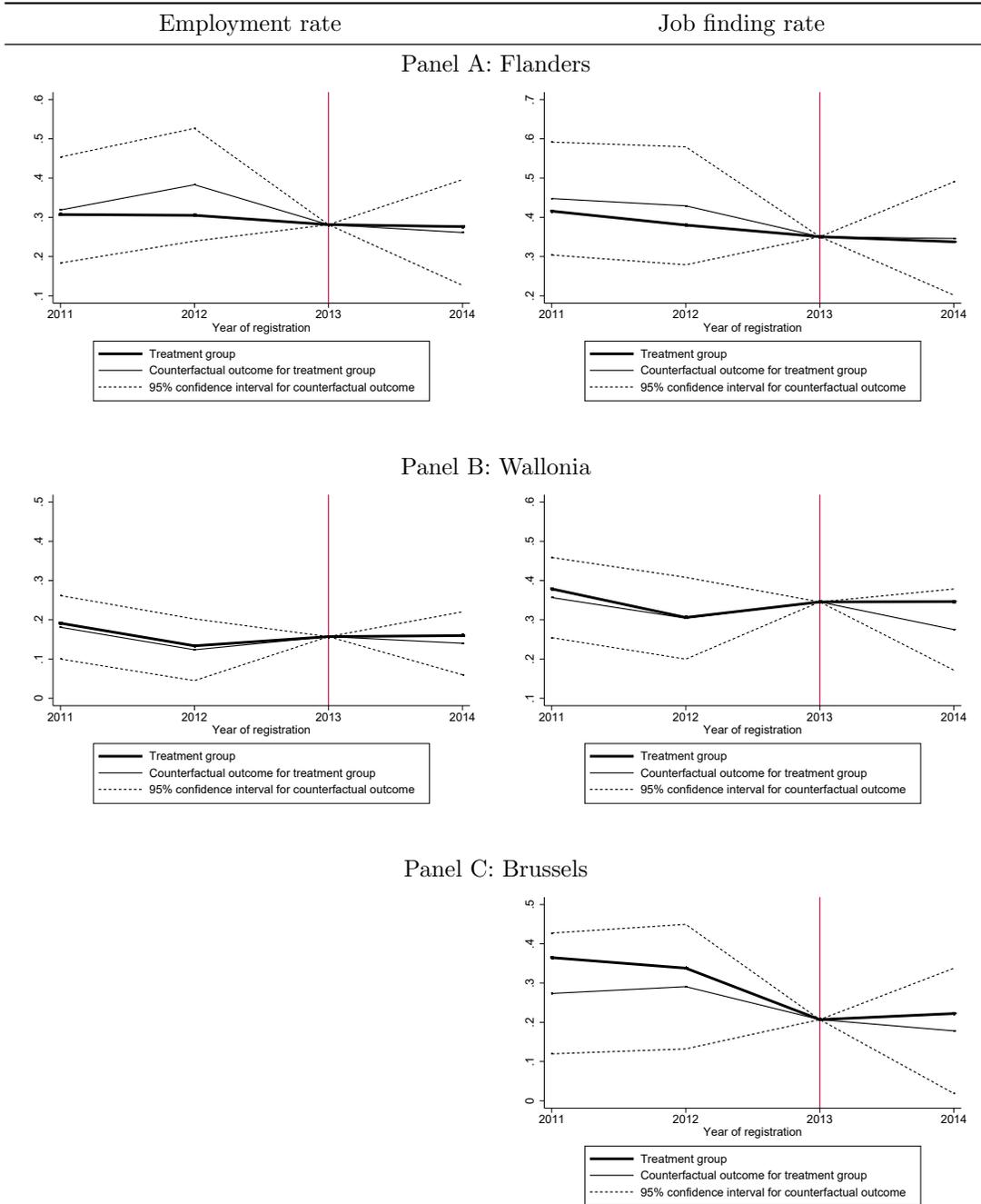
Notes: Treatment group = 24-year-old job seekers. Control group = 23-year-old job seekers. Age is measured at the moment of registration as a job-seeker. Outcomes are measured 6 months after registration at the PES for job seekers who register between June and October after having obtained a master's degree. The vertical line is drawn at the last period before the reform. The thick solid line shows the observed outcome of the treatment group. The fine solid line shows the counterfactual outcome of the treatment group in absence of the treatment. The thin dotted lines are the 95% confidence interval for the counterfactual path.

Figure A2: Employment outcomes of 24-year-olds with a master's degree (18 months after registration)



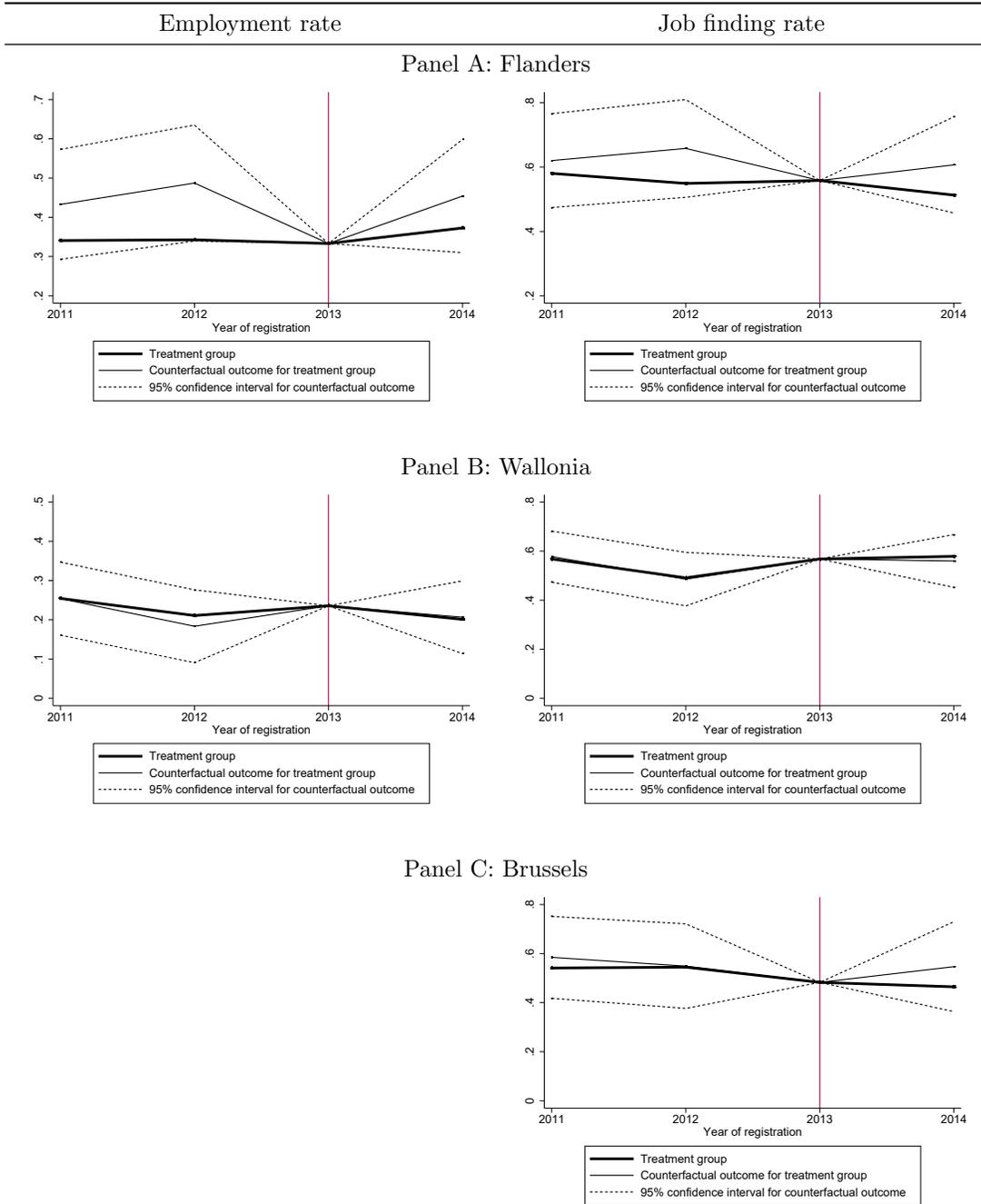
Notes: Treatment group = 24-year-old job seekers. Control group = 23-year-old job seekers. Age is measured at the moment of registration as a job-seeker. Outcomes are measured 18 months after registration at the PES for job seekers who register between June and October after having obtained a master's degree. The vertical line is drawn at the last period before the reform. The thick solid line shows the observed outcome of the treatment group. The fine solid line shows the counterfactual outcome of the treatment group in absence of the treatment. The thin dotted lines are the 95% confidence interval for the counterfactual path.

Figure A3: Employment outcomes of 19- and 20-year-olds without a high school degree (6 months after registration)



Notes: Treatment group = 19-year-old job seekers. Control group = 20-year-old job seekers. Age is measured at the moment of registration as a job seeker. Outcomes are measured 6 months after registration at the PES for job seekers without a high school degree who register between September and December. The vertical line is drawn at the last period before the reform. The thick solid line shows the observed outcome of the treatment group. The fine solid line shows the counterfactual outcome of the treatment group in absence of the treatment. The thin dotted lines are the 95% confidence interval for the counterfactual path.

Figure A4: Employment outcomes of 19- and 20-year-olds without a high school degree (18 months after registration)



Notes: Treatment group = 19-year-old job seekers. Control group = 20-year-old job seekers. Age is measured at the moment of registration as a job seeker. Outcomes are measured 18 months after registration at the PES for job seekers without a high school degree who register between September and December. The vertical line is drawn at the last period before the reform. The thick solid line shows the observed outcome of the treatment group. The fine solid line shows the counterfactual outcome of the treatment group in absence of the treatment. The thin dotted lines are the 95% confidence interval for the counterfactual path.

Table A1: Employment rate and job finding rate: 24-year-old job seekers (difference-in-differences)

	Employment rate			Job finding rate		
	6 months	12 months	18 months	6 months	12 months	18 months
Panel A: Flanders						
Treatment effect ($Age_{24} * T_{2014}$)	-0.004 (0.018)	0.005 (0.015)	-0.000 (0.013)	-0.004 (0.017)	0.006 (0.012)	0.006 (0.010)
Age24	-0.031*** (0.009)	-0.017** (0.008)	-0.018** (0.007)	-0.025*** (0.009)	0.006*** (0.012)	-0.012** (0.005)
T_{2011}	0.775*** (0.009)	0.828*** (0.008)	0.885*** (0.007)	0.833*** (0.008)	0.927*** (0.005)	0.951*** (0.004)
T_{2012}	0.748*** (0.009)	0.803*** (0.008)	0.880*** (0.007)	0.794*** (0.008)	0.903*** (0.006)	0.937*** (0.005)
T_{2013}	0.740*** (0.009)	0.820*** (0.008)	0.880*** (0.006)	0.794*** (0.008)	0.908*** (0.006)	0.939*** (0.005)
T_{2014}	0.771*** (0.009)	0.845*** (0.008)	0.906*** (0.007)	0.787*** (0.009)	0.915*** (0.006)	0.946*** (0.005)
Counterfactual probability	0.740	0.827	0.888	0.542	0.901	0.934
Parallel trends: p-value	0.843	0.642	0.551	0.591	0.531	0.531
Observations	12634	12634	12634	12634	12634	12634
Panel B: Wallonia						
Treatment effect ($Age_{24} * T_{2014}$)	-0.004 (0.017)	0.006 (0.012)	0.006 (0.010)	0.032 (0.023)	0.037* (0.020)	0.021 (0.019)
Age24	-0.025*** (0.009)	-0.015** (0.006)	-0.012** (0.005)	-0.020 (0.012)	-0.017 (0.011)	-0.011 (0.010)
T_{2011}	0.833*** (0.008)	0.927*** (0.005)	0.951*** (0.004)	0.653*** (0.011)	0.764*** (0.010)	0.827*** (0.009)
T_{2012}	0.795*** (0.008)	0.903*** (0.006)	0.937*** (0.005)	0.597*** (0.011)	0.720*** (0.010)	0.782*** (0.009)
T_{2013}	0.794*** (0.008)	0.908*** (0.006)	0.939*** (0.005)	0.609*** (0.011)	0.731*** (0.010)	0.751*** (0.010)
T_{2014}	0.813*** (0.009)	0.915*** (0.006)	0.946*** (0.005)	0.586*** (0.013)	0.717*** (0.012)	0.787*** (0.011)
Counterfactual probability	0.787	0.901	0.934	0.566	0.661	0.776
Parallel trends: p-value	0.591	0.531	0.531	0.765	0.477	0.998
Observations	12634	12634	12634	9728	9728	9728

Notes: Treatment group = 24-year-old job seekers. Control group = 23-year-old job seekers. Age is measured at the moment of registration as a job seeker. Control period = 2011-2013, treatment period = 2014. Outcomes are measured for job seekers who register between June and October after having obtained a master's degree. The counterfactual outcome is the predicted outcome for the treated in absence of the treatment in the post-reform period. To test for parallel trends, we estimate similar regressions with interaction effects between the treatment groups and year dummies. The parallel trends assumption is rejected if these interaction effects are jointly significant from zero in the pre-reform period. The p-value of this test is reported. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A1 (continued): Employment rate and job finding rate: 24-year-old job seekers (difference-in-differences)

	Employment rate			Job finding rate		
	6 months	12 months	18 months	6 months	12 months	18 months
Panel C: Brussels						
Treatment effect ($Age24 * T_{2014}$)				-0.017 (0.052)	0.004 (0.043)	-0.041 (0.035)
Age24				-0.025 (0.025)	-0.031 (0.020)	-0.003 (0.017)
T_{2011}				0.688*** (0.025)	0.855*** (0.020)	0.894*** (0.017)
T_{2012}				0.667*** (0.026)	0.827*** (0.020)	0.879*** (0.017)
T_{2013}				0.667*** (0.028)	0.836*** (0.023)	0.899*** (0.018)
T_{2014}				0.691*** (0.033)	0.825*** (0.027)	0.907*** (0.021)
Counterfactual probability				0.666	0.794	0.904
Parallel trends: p-value				0.445	0.086	0.117
Observations				1854	1854	1854

Notes: Treatment group = 24-year-old job seekers. Control group = 23-year-old job seekers. Age is measured at the moment of registration as a job seeker. Control period = 2011-2013, treatment period = 2014. Outcomes are measured for job seekers who register between June and October after having obtained a master's degree. The counterfactual outcome is the predicted outcome for the treated in absence of the treatment in the post-reform period. To test for parallel trends, we estimate similar regressions with interaction effects between the treatment groups and year dummies. The parallel trends assumption is rejected if these interaction effects are jointly significant from zero in the pre-reform period. The p-value of this test is reported. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: Employment rate and job finding rate: 19-year-old job-seekers (difference-in-differences)

	Employment rate			Job finding rate		
	6 months	12 months	18 months	6 months	12 months	18 months
Panel A: Flanders						
Treatment effect ($Age19 * T_{2014}$)	0.043 (0.056)	-0.016 (0.061)	-0.004 (0.061)	0.018 (0.060)	-0.039 (0.063)	-0.046 (0.062)
Age19	-0.033 (0.029)	-0.052* (0.030)	-0.053* (0.030)	-0.043 (0.033)	-0.032 (0.031)	-0.026 (0.031)
T_{2011}	0.335*** (0.030)	0.364*** (0.031)	0.399*** (0.031)	0.461*** (0.032)	0.544*** (0.032)	0.604*** (0.032)
T_{2012}	0.356*** (0.032)	0.420*** (0.033)	0.420*** (0.033)	0.432*** (0.033)	0.548*** (0.034)	0.597*** (0.033)
T_{2013}	0.304*** (0.030)	0.379*** (0.031)	0.357*** (0.031)	0.384*** (0.032)	0.511*** (0.033)	0.567*** (0.033)
T_{2014}	0.267*** (0.038)	0.415*** (0.042)	0.430*** (0.043)	0.363*** (0.041)	0.541*** (0.043)	0.585*** (0.042)
Counterfactual probability	0.234	0.363	0.560	0.320	0.508	0.560
Parallel trends: p-value	0.528	0.117	0.366	0.806	0.347	0.366
Observations	1473	1473	1473	1473	1473	1473
Panel B: Wallonia						
Treatment effect ($Age19 * T_{2014}$)	0.013 (0.033)	-0.020 (0.035)	-0.014 (0.038)	0.062 (0.042)	0.030 (0.045)	0.021 (0.044)
Age19	-0.008 (0.017)	-0.031* (0.018)	-0.023 (0.019)	-0.027 (0.021)	-0.014 (0.022)	-0.012 (0.022)
T_{2011}	0.198*** (0.017)	0.245*** (0.019)	0.281*** (0.020)	0.400*** (0.022)	0.520*** (0.022)	0.583*** (0.022)
T_{2012}	0.140*** (0.017)	0.199*** (0.019)	0.226*** (0.020)	0.335*** (0.022)	0.443*** (0.023)	0.501*** (0.023)
T_{2013}	0.168*** (0.018)	0.223*** (0.019)	0.262*** (0.020)	0.376*** (0.023)	0.498*** (0.023)	0.580*** (0.023)
T_{2014}	0.155*** (0.022)	0.206*** (0.024)	0.238*** (0.026)	0.310*** (0.028)	0.505*** (0.030)	0.570*** (0.030)
Counterfactual probability	0.147	0.175	0.215	0.284	0.491	0.558
Parallel trends: p-value	0.959	0.818	0.797	0.892	0.781	0.962
Observations	2816	2816	2816	2816	2816	2816

Notes: Treatment group = 19-year-old job seekers. Control group = 20-year-old job seekers. Age is measured at the moment of registration as a job seeker. Control period = 2011-2013, treatment period = 2014. Outcomes are measured for job seekers without a high school degree who register between September and December. The counterfactual outcome is the predicted outcome for the treated in absence of the treatment in the post-reform period. To test for parallel trends, we estimate similar regressions with interaction effects between the treatment groups and year dummies. The parallel trends assumption is rejected if these interaction effects are jointly significant from zero in the pre-reform period. The p-value of this test is reported. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A2 (continued): Employment rate and job finding rate: 19-year-old job-seekers (difference-in-differences)

	Employment rate			Job finding rate		
	6 months	12 months	18 months	6 months	12 months	18 months
Panel C: Brussels						
Treatment effect ($Age19 * T_{2014}$)				-0.006 (0.067)	-0.044 (0.075)	-0.064 (0.076)
Age19				-0.052 (0.033)	-0.042 (0.035)	-0.030 (0.035)
T_{2011}				0.397*** (0.033)	0.529*** (0.034)	0.583*** (0.034)
T_{2012}				0.391*** (0.034)	0.498*** (0.035)	0.569*** (0.035)
T_{2013}				0.280*** (0.034)	0.421*** (0.037)	0.505*** (0.037)
T_{2014}				0.280*** (0.041)	0.449*** (0.046)	0.559*** (0.046)
Counterfactual probability				0.228	0.407	0.529
Parallel trends: p-value				0.509	0.833	0.845
Observations				1058	1058	1058

Notes: Treatment group = 19-year-old job seekers. Control group = 20-year-old job seekers. Age is measured at the moment of registration as a job seeker. Control period = 2011-2013, treatment period = 2014. Outcomes are measured for job seekers without a high school degree who register between September and December. The counterfactual outcome is the predicted outcome for the treated in absence of the treatment in the post-reform period. To test for parallel trends, we estimate similar regressions with interaction effects between the treatment groups and year dummies. The parallel trends assumption is rejected if these interaction effects are jointly significant from zero in the pre-reform period. The p-value of this test is reported. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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