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How effective are hiring subsidies in reducing long-term unemployment among prime-aged jobseekers? Evidence from Belgium

Abstract

Hiring subsidies are widely used to create (stable) employment for the long-term unemployed. This paper exploits the abolition of a hiring subsidy targeted at long-term unemployed jobseekers older than 45 years of age in Belgium to evaluate its effectiveness in the short and medium run. Based on a triple-difference methodology, the hiring subsidy is shown to increase the job-finding rate by 13% without any evidence of spillover effects. This effect is driven by a positive effect on individuals with at least a bachelor's degree. However, the hiring subsidy mainly creates temporary short-lived employment: eligible jobseekers are not more likely to find employment that lasts at least 12 consecutive months compared with ineligible jobseekers.

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1   Introduction

Long-term unemployment remains a persistent challenge for European labor markets. Despite the economic recovery between 2014 and 2020, long-term unemployment remains stubbornly high. In 2018, across the 28 member states of the European Union (EU-28), 42.9% of jobseekers were unemployed for >12 months.[1]

A popular way to fight (long-term) unemployment is the use of hiring subsidies (European Commission [EC], 2019). The countries of the Organisation for Economic Co-operation and Development (OECD) spend 0.08% of their gross domestic product (GDP) on recruitment incentives, which amounts to 15% of total spending on active labor market programs (ALMPs).[2] A more fine-grained analysis[3] shows that, in 2015, recruitment incentives with specific provisions for the long-term unemployed[4] (LTU) were used in 20 out of the 28 EU countries, while recruitment incentives exclusively targeted at the LTU were used in eight countries. In terms of budgets, in 2015, EU countries spent, on average, one third of the budget allocated to recruitment incentives on recruitment incentives with at least specific provisions for the LTU. While comparing programs across countries is inherently challenging, these figures illustrate that hiring subsidies targeted at the LTU are common across the EU, which make evaluations of country-specific programs relevant beyond the specific case that is being evaluated.

This study aims at evaluating the effect of a hiring subsidy in Belgium targeted at jobseekers who have been unemployed for at least 6 months and are between 45 years and 48 years old. This is not the first study that aims at evaluating the impact of such hiring subsidies (see , e.g., Sjögren and Vikström, 2015; Ciani *et al.*, 2019; Pasquini *et al.*, 2019; Card *et al.*, 2018 for a survey). However, this study is the first to focus on prime-aged workers in the middle of their career. This subpopulation is of particular interest, as these workers are typically laid off after accumulating substantial labor market experience and firm specific human capital. There is ample literature that documents that displacement costs for such workers are substantial and enduring.[5] An interesting question is therefore whether hiring subsidies can reverse these costs and be a stepping stone to more-stable, permanent positions.

Within the literature evaluating time-limited in-work benefits at the supply side, it is commonly agreed that human capital does not improve by providing short-term work experience to low-skilled welfare recipients, inhibiting thereby enduring positive effects on employment and earnings (Blundell, 2002, 2006; Card and Hyslop, 2005; see Van der Linden, 2021, for a survey).[6] To the extent that tax incidence is neutral, this result is expected to hold as well for demand-side temporary recruitment subsidies. By contrast, for higher-educated workers, a temporary recruitment subsidy may allow to (re)build human capital or to reverse the negative signal of long-term unemployment (Cockx and Picchio, 2013).

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1   Eurostat (indicator: une_ltu_a), accessed April 2020.

2   https://stats.oecd.org/Index.aspx?DataSetCode=LMPEXP, accessed September 2020.

3   We are grateful to the OECD for sharing these data. The OECD (2019a) report provides more details of the potential of the Labour Market Policy (LMP) database to study policies targeted at the LTU; the OECD (2019b) report provides an in-depth evaluation of ALMPs targeted at the LTU in five countries.

4   The OECD defines a long-term unemployed jobseeker as a jobseeker with a continuous spell of unemployment lasting at least 1 year.

5   See, e.g., Jacobson *et al.* (1993), Couch and Placzeck (2010), Davis and von Wachter (2011); for some more-recent studies, see Seim (2019) and Lachowska *et al.* (2020); and for a recent survey, see Carrington and Fallick (2017). The latter survey shows that loss of specific human capital is not the only source of the cost due to displacement: it comprises also the loss of job matching, as well as backloaded compensation – of rents, of job contacts, and of mental and physical health, among others.

6   See Riddell and Riddell (2014, 2020) for criticism on this view.

We find that the hiring subsidy increased the job-finding rate by 13%. However, we also find that the effect on employment dissipates gradually and becomes zero after 1 year. Hence, the hiring subsidy does not create long-lasting stable employment. The effect on employment is larger and more persistent for high-educated jobseekers with at least a bachelor's degree, but the positive effect also gradually declines over time, suggesting that for this group also, it does not extend beyond the expiration of the subsidy. For low-skilled mid-career workers, the level of the wage subsidy may not be high enough to compensate for the loss in firm-specific human capital induced by the job loss. However, we argue that high-educated workers have acquired sufficient transferable skills to be hired in short-lived jobs with limited skill requirements. Such jobs can be made acceptable and/or profitable as long as the hiring subsidy is paid out, but not beyond this period. The finding that most of the effect is induced by hiring by temporary work agencies reinforces this interpretation and is in line with the evidence that the employment offered by these agencies is no stepping stone to more stable employment (Autor and Houseman, 2010; Givord and Wilner, 2015).

Our findings align to a large extent with the literature on the impact of recruitment subsidies for LTU workers. While most of these studies focus on jobseekers unemployed for at least 12 months, this literature nevertheless offers the most relevant reference point. The target group considered in our study consists of jobseekers unemployed for at least 6 months, a group that arguably shares many characteristics with jobseekers unemployed for 12 months.[7] According to the well-known review by Card *et al.* (2018) of 200 studies of ALMP, the LTU gain more from private sector employment programs, which typically include hiring subsidies, than other participants. A related paper by Card *et al.* (2016), summarizing the same studies but focusing on the LTU, further emphasizes this point. This conclusion is, however, based on only 10 studies, of which five have been published in academic journals. These studies are listed in Table C1 in Appendix C.

The systematic reviews of Vooren *et al.* (2019) and Levy-Yeyati *et al.* (2019) confirm that hiring subsidies in the private sector increase the likelihood of resuming work, but these studies do not report results for the LTU. The meta-analysis of Filges *et al.* (2018) also shows that the effect of private sector programs on reemployment tends to be higher than the effect of labor market training, direct employment programs in the public sector, or job search assistance, but the differences between these types of ALMPs are not statistically significant. The authors note that the number of studies is too low to examine effect heterogeneity across participant characteristics.

Three more recent studies, not included in the aforementioned reviews but closely related to our work, provide further evidence that hiring subsidies targeted at the LTU have positive effects. Sjögren and Vikström (2015) report positive effects of a hiring subsidy in Sweden targeted at jobseekers unemployed or out of the labor force for at least 1 year. Importantly, this is one of the few studies that examine the impact of recruitment subsidies on employment stability.[8] In contrast to our findings, they find that hiring subsidies create long-lasting stable jobs.

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7   The literature on hiring subsidies for older (long-term unemployed) workers is less relevant since these studies focus on jobseekers aged >55 years (Boockmann *et al.*, 2012; Huttunen *et al.*, 2013). In this literature, the subsidy aims at keeping workers as long as possible in the workforce and seeks to delay the decision to retire as much as possible (Albanese and Cockx, 2019). The retirement decision is not relevant for the 45- to 48-year age group that we consider in the analysis.

8   There is some evidence that hiring and wage subsidies can have long-lasting positive effects. Saez *et al.* (2021) provide credible evidence that a payroll tax cut targeted at young workers has persistent positive effects, even after the tax cut was abolished. Ciani *et al.* (2019) find positive effects of a hiring subsidy targeted at the long-term unemployed in Italy on permanent job contracts, a key stepping stone to stable employment. Lombardi *et al.* (2018) evaluate the impact on firm performance of a generous wage subsidy directed at the long-term unemployed in Sweden and report that roughly half of the subsidized workers remained in the firm after 5 years.

Ciani *et al*. (2019) and Pasquini *et al*. (2019) evaluate a hiring subsidy targeted at jobseekers in (Southern) Italy unemployed for at least 2 years using different identification strategies. Both studies report positive effects on the job-finding rate.

Apart from considering a different target population and examining employment stability, our study contributes to the literature in a few other respects. First, our study differs from a methodological perspective in that we rely on a triple-difference identification strategy and that we evaluate the effectiveness of the policy by studying its abolishment rather than its introduction. Furthermore, we study spillover effects. Scholars have criticized microeconometric evaluation studies on the grounds that they fail to take into account general equilibrium effects and that a positive employment effect for the target group may merely reflect a countervailing effect on the ineligible population (Crépon *et al*., 2013; Crépon and van den Berg, 2016; Cahuc *et al*., 2018; Gautier *et al*., 2018). We study whether the policy benefited the target group at the expense of slightly younger jobseekers or at the expense of the short-term unemployed, and we do not find any evidence for this.

The paper is organized as follows. The next section (Section 2) discusses the Belgian institutional setting and the recent labor market reforms. Section 3 describes the data from the Flemish public employment service (PES). Section 4 outlines the triple-difference approach. Next, in Section 5, we discuss the effect of abolition of the hiring subsidy on transition to employment and on employment stability and thereafter examine effect heterogeneity. The final section (Section 6) concludes.

## 2　Institutional Setting and Policy Reform

2.1　Long-term unemployment in Flanders

Belgium is one of the few countries where unemployment insurance (UI) is not time limited, although the benefit level decreases over time. UI amounts to 65% of the (capped) gross wage[9] of the previous employment spell in the first 3 months of unemployment and amounts to 60% in the following 9 months. It declines further afterward according to complicated rules up to a lump sum amount the level of which depends on the household type.[10] In order to receive UI, a jobseeker has to be registered at one of the four regional PES. While the UI is a federal competence, the services provided by the PES, such as job counseling, job search monitoring, and training, are regional competences. More recently, hiring subsidies targeted at disadvantaged groups has become a regional competence. This paper focuses on Flanders, the Dutch-speaking region in the north of Belgium.

Since UI is not time limited, many jobseekers registered at the Vlaamse Dienst voor Arbeidsbemiddeling en Beroepsopleiding (VDAB) (the Flemish PES) are LTU. In 2019, nearly two thirds of the 184,851 jobseekers were unemployed for >6 months, while half of these were unemployed for >1 year (Figure 1). From 2014 until the outbreak of the coronavirus disease-2019 (COVID-19) pandemic in March 2020, the number of jobseekers continuously decreased.

9　The cap decreases with duration of unemployment. In January 2022, it decreased from about €2,900/month during the first 6 months, over €2,700/month during the next 6 months to €2,520/month (€2,470/month for singles living without dependents) when unemployed for >1 year. (www.rva.be/nl/documentatie/infoblad/t67)

10　In January 2022, this floor per month was €622 for cohabitants without dependents, €1,198 for singles living alone, and €1,478 for individuals in charge of dependents (children or partner without household income). These lump sum amounts also define floors below which benefits cannot drop (www.rva.be/nl/documentatie/infoblad/t67).

Figure 1   The evolution of long-term unemployment in Flanders.
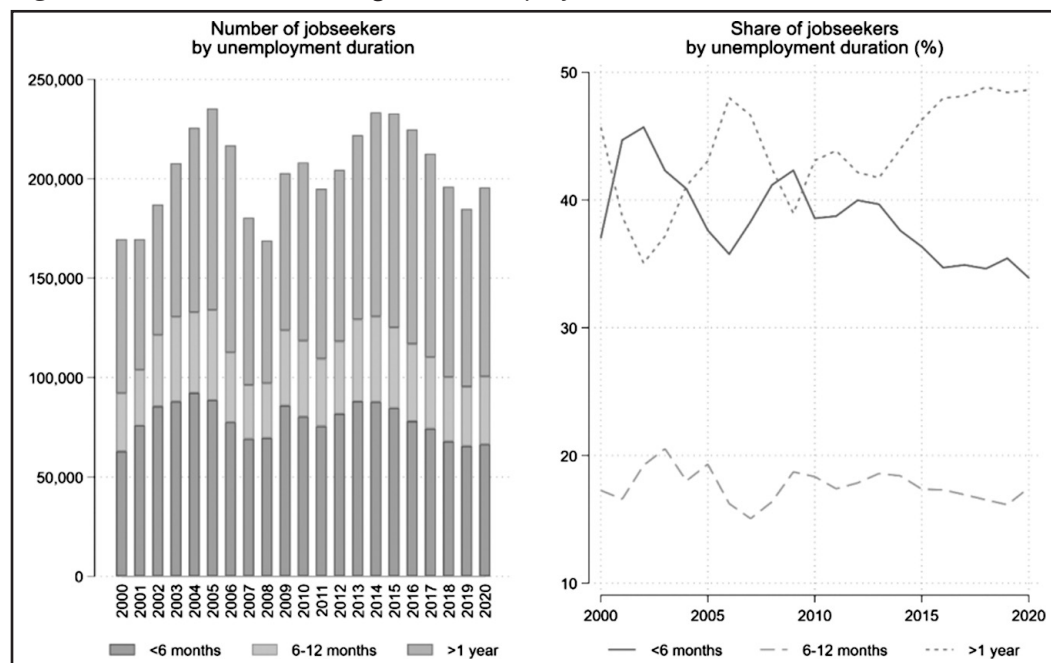
Source: Monthly average from Arvastat (https://arvastat.vdab.be/, accessed April 2021).

It is well-known that the share of the LTU among the unemployed population increases during economic booms and decreases during economic recessions (Machin and Manning, 1999). This regularity also holds in Flanders. All jobseekers benefited from the economic recovery from 2014 to 2019, but the positive impact was less pronounced for the LTU. In 2019, 65% of the jobseekers were unemployed for >6 months, up from 62% in 2015. Figure A1 in Appendix A shows that a similar pattern emerges when restricting the population to jobseekers aged 40–48 years. As we will explain in section 4, the procyclical behavior of long-term unemployment is the main reason why we opted for a triple-difference evaluation strategy rather than a more standard difference-in-differences (DiD) approach.

The Flemish PES has specific policies, such as training programs and job counseling, tailored to the needs of the LTU (Desiere *et al.*, 2018). Due to a tightening labor market, the PES has renewed and reinforced its attention to the LTU in recent years. In 2017, the PES started to (re)screen jobseekers who were unemployed for >1 year in order to activate them and launched a new tender for the activation of the LTU in 2018.

These programs are unlikely to interfere with the evaluation of the hiring subsidy targeted at jobseekers unemployed for >6 months. First, similar programs have existed before and after the abolition of the subsidy as this aspect of active labor market policies was already a regional competence before the reform. Second, the PES defines a "long-term unemployed jobseeker" as a jobseeker who is unemployed for at least 1 year. Our analysis focuses on jobseekers unemployed for 1–12 months. These jobseekers were not eligible for specific programs aimed at the LTU.

Until a few years ago, the PES applied specific rules when activating and monitoring jobseekers older than 50 years of age. Moreover, this group was eligible for a more generous hiring subsidy until 2014. We restrict the population to jobseekers aged 40–48 years to avoid the possibility that these specific policies for older jobseekers interfere with our evaluation of

the hiring subsidy. The PES did not have specific programs in place aimed at jobseekers aged 40–48 years over the entire period considered in the analyses.

Similar to the hiring subsidies, job search monitoring also has become a regional competence and has been subsequently reformed. Since 2016, job search monitoring in Flanders is the competence of the PES and no longer that of a separate federal institution. It is unlikely that the reform of the job search monitoring interferes with the evaluation of the abolition of the hiring subsidy because job search monitoring already occurred before the reform, was not – or, at best, only gradually – intensified after the reform, and did not specifically target jobseekers unemployed for >6 months.

2.2 Hiring subsidies targeted at the LTU

In 2014, wage and hiring subsidies targeted at disadvantaged groups became a regional competence. All four Belgian regions subsequently reformed and simplified the complex existing federal system of wage and hiring subsidies and substantially reduced the number of eligible groups. Flanders opted to abolish all hiring subsidies targeted at the LTU. This paper focuses on the abolition of the hiring subsidy targeted at jobseekers older than 45 years of age who were unemployed for at least 6 months over the past 9 months. After the reform, jobseekers in Flanders aged 25–55 years and unemployed for <12 months were no longer eligible for any hiring subsidy.[11]

Until January 1, 2017, employers hiring older, LTU jobseekers were entitled to a quarterly Social Security contribution (SSC) reduction of €1,000 during the first five quarters, followed by a quarterly reduction of €400 during the subsequent 16 quarters. Hence, the total maximum hiring subsidy amounted to €11,400 over 21 quarters. The subsidies were reduced (almost proportionally) for employees working part time. In contrast to many other hiring subsidies studied in the literature, no specific conditions were attached to receiving this subsidy: employers were free to offer open-ended or temporary contracts; were not obliged to offer training; did not have to retain the worker for a minimum number of months or offer a nonsubsidized job after the subsidy expired.

The hiring subsidies were not automatically awarded to the employers. Eligible jobseekers had to obtain a certificate from a federal institution proving their eligibility. Employers could then use this certificate to obtain SSC reductions. Eligible individuals (or their employer) could still apply for this certificate after they had been hired, but the subsidies were not granted retrospectively. Given data limitations, the take-up rate of the hiring subsidy is unknown, but the take-up rate of similar SSC reductions is estimated to be approximately 70% (Boucq and Novella, 2018; Desiere *et al.*, 2020).

From January 1, 2017, no new certificates were awarded to Flemish jobseekers, in effect abolishing the hiring subsidy in Flanders. The subsidies granted under the federal rules were gradually phased out. Jobseekers who had obtained a certificate before January 1, 2017, remained eligible for the hiring subsidy until the certificate expired. Employers who had hired an eligible jobseeker continued to receive subsidies until the end of 2018.

11　The other regions also abolished this specific hiring subsidy, but, in contrast to Flanders, introduced hiring subsidies for jobseekers unemployed for at least 12 months. These hiring subsidies do not depend on the jobseeker's age.

In the last quarter of 2016, 2,564 individuals benefited from the hiring subsidy in Flanders, of whom 369 individuals were hired in that quarter. The average, quarterly subsidy amounted to €506, while the total yearly cost of the policy amounted to €5.3 million in 2016.

The median full-time equivalent gross monthly wage (which excludes SSC paid by the employer) of individuals hired with a subsidy is €2,324 and ranges for half of these employees between €1,981 (3rd quartile) and €2,796 (1st quartile). Their income is lower because only one out of four individuals works on a full-time basis. Taking the aforementioned interquartile range as a measure of spread, the abolition of the subsidy increased labor costs by 11%–16% during the first five quarters after being hired and by at most 6% during the 16 subsequent quarters.[12]

Although the hiring subsidy was not targeted at temporary help agencies, these agencies seem to use this subsidy more intensively for hiring than firms that hire workers directly without the intermediation of such agencies. Between 2015 and 2016, 61% of the LTU aged 45–48 years hired with a subsidy were hired by temporary help agencies (Table A1 in Appendix A). The second most important sector, the cleaning sector, only accounts for 5% of the hires. A representative survey conducted among employers in Flanders in 2016 finds that about 42% of all recruitments are achieved through the mediation of temporary help agencies (Delagrange and Notebaert, 2018, p. 79).

There are two potential reasons why temporary help agencies make more intensive use of the hiring subsidy than firms that directly hire workers. First, as temporary help agencies are specialized in the hiring of workers, they are much better informed about the myriad of hiring subsidies that are available in the Belgian labor market, and therefore, they are more likely to make use of them. For instance, surveys indicate that about 40% of the employers in Flanders are not aware of the existence of hiring subsidies for specific target groups (Boucq and Novella, 2018, p. 10). Second, the Flemish PES announces on its website its partnership with the sector of temporary help agencies and that it regards temporary jobs as "stepping stones" to regular work. This, together with anecdotal evidence that caseworkers of the PES advise unemployed jobseekers to accept temporary jobs (Cockx and Van Belle, 2019), can explain why jobseekers are more likely to be hired by temporary work agencies than by direct hiring firms.

3 Data

The data set consists of all jobseekers who registered at the VDAB (the Flemish PES) between January 2012 and December 2018. The monthly labor market position of each jobseeker is known from registration until February 2019. This position is determined on the last day of each month. The administrative data distinguish several types of employment (e.g., sheltered work, part-time work in combination with unemployment benefits) that are linked to the administrative status of the jobseeker. The data set does not contain details on the sector of employment, the type of contract, the wage, or the number of days worked. We adopted VDAB's standard definition of employment but also considered a more stringent definition of work that excludes, among others, temporary help agency jobs lasting between 10 days and 20 days per month.

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12 These estimates do not take into account the so-called "tax shift", which gradually reduced employers' SSC, particularly for low-wage workers (irrespective of their prior unemployment duration or age), from 2016 to 2019. The tax shift partially offsets the negative impact of the abolition of the hiring subsidy on labor costs.

An unemployment spell starts in the month of registration at the PES and ends when the jobseeker resumes work. In the benchmark analysis, jobseekers who leave the labor force at a certain point in time are still considered unemployed.[13] The findings are robust to censoring unemployment spells when jobseekers leave the labor force (Table A6 in Appendix A).

The data set contains the socioeconomic characteristics of jobseekers, such as date of birth, level of education, language proficiency, and nationality. We restrict the population to jobseekers entitled to UI aged 40–48 years at the time of registration at the PES, excluding jobseekers aged 44 years. As we discuss in the next section (Section 4), the treatment group consists of LTU jobseekers aged 45–48 years, while jobseekers aged 40–43 years belong to the control group. Jobseekers aged 44 years are excluded from the sample because some of the jobseekers in this age group become eligible for the subsidy after being unemployed for 6–12 months.

Prior to the reform, jobseekers were eligible for a hiring subsidy after being unemployed for at least 6 months in the previous 9 months. Eligibility for a hiring subsidy is stipulated by law and closely monitored by a federal institution. Because we compare the job-finding rates of the short-term unemployed and the LTU, we have to identify the exact point in time that a jobseeker becomes eligible for a subsidy, but the time grouping of the unemployment data in months prevents us from determining this point for all jobseekers in the population. To address this issue, we retain in the sample only the individuals who were registered as employed for at least 4 consecutive months prior to registration at the PES. This restriction ensures that no selected jobseeker can become eligible for the subsidy with an elapsed unemployment duration of <6 months. This is because this restriction ensures that, at the time of registration, such workers can be unemployed at the most for 5 months in the previous 9 months, and that for each month following registration, the total time spent as unemployed can never exceed 5 months before the sixth month after registration. Hence, by restricting the data set to this subpopulation, we identify the impact of the policy on only jobseekers unemployed for at least 6 consecutive months.

This restriction implies that unemployment spells that started between January 2012 and April 2012 (i.e., the first 4 months in the data set) are discarded, because it cannot be checked whether these individuals were employed prior to registration at the PES. Of the remaining 189,154 unemployment spells, 14,556 spells are discarded because the labor market history in the past 4 months is not known, whereas 69,365 spells are discarded because the jobseeker did not work 4 consecutive months prior to registration.[14] Finally, 796 spells are discarded because the jobseeker's self-reported proficiency in Dutch, which is one of the covariates in the regressions, is missing. The final data set contains 78,536 unique individuals who account for 104,437 unemployment spells.

The triple-difference identification strategy, discussed in Section 4, contrasts the job-finding rates of younger (40–43 years old) vs. older (45–48 years old) and short-term (unemployed for <6 months) vs. long-term (unemployed for 6–12 months) unemployed jobseekers, before and after the reform. Table 1 compares the characteristics of jobseekers by age group and

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13   We opted for this approach because jobseekers classified by the Flemish PES as "out of the labor force" might still be actively looking for a job, while jobseekers classified as "actively looking for a job" might in practice be out of the labor force.

14   The PES only registers someone's labor market position after an individual has registered at the PES. We discarded 14,556 spells because these individuals were not known by the PES prior to becoming unemployed. These individuals were (most likely) employed or out of the labor force prior to registration at the PES, and one could assume that these individuals only became eligible for the subsidy 6 months after registration. We nevertheless discarded this group because we could not explicitly check that this eligibility condition was met. In Table A5 in Appendix A, we show that the main findings hold when including this group. We also show that our findings are robust to including jobseekers who did not work 4 consecutive months prior to becoming unemployed.

unemployment duration before and after the reform. The pre-reform and post-reform periods contain jobseekers who registered at the PES before and after June 1, 2016, respectively. These statistics describe the profile of the target group and also allow us to test whether the composition of the population is stable before and after the reform, which lends credence to the triple-difference identification strategy.

In general, jobseekers in both age groups have very similar characteristics and are vulnerable in several respects. About eight out of 10 jobseekers have at the most a lower-secondary (low) or an upper-secondary (medium) level of education; only about half report having an excellent proficiency in Dutch; and one out of five were born outside the EU-28.

Comparing the characteristics of short-term unemployed with those of LTU jobseekers within age groups shows that both before and after the reform, the LTU are generally more vulnerable than the short-term unemployed, but the differences are not particularly pronounced. In both periods and age groups, the LTU are more likely to have a low level of education and tend to be less proficient in Dutch.

We do not observe substantial differences within groups before and after the reform. The main difference within groups is that jobseekers in the pre-reform period are more likely to be born in the EU-15 and tend to be more proficient in Dutch.

To test whether the composition of the population is stable before and after the reform, we estimate a triple-difference regression, which consists in regressing the jobseekers' characteristics on group dummies (i.e., older vs. younger jobseekers; and short-term unemployed vs. LTU jobseekers), period dummies (before vs. after the reform), and all the interaction terms between these dummies. The coefficient of the triple-interaction term, which is reported in the last column of Table 1, indicates whether the composition of the population has changed. All triple-interaction terms are small and insignificant at conventional levels, indicating that the composition of the population remained stable before and after the reform.

Table 1 also shows the characteristics of LTU jobseekers aged 45–48 years who have been hired with a subsidy. Individuals hired with the subsidy appear not to be "selected" based on observable socioeconomic characteristics. Jobseekers hired with a subsidy have similar characteristics as the LTU aged 45–48 years. If anything, individuals hired with a subsidy tend to have a lower level of education.

Descriptive evidence shows that subsidized jobs are not long-lasting ones. The share of employees for whom a subsidy was granted for several subsequent quarters quickly decreases with employment duration. About 70% of the individuals for whom a subsidy was granted in a first quarter also received a subsidy in the second quarter, while approximately 25% received a subsidy over four consecutive quarters. This illustrates that a large majority of subsidized jobs is destroyed after a few quarters.

Figure 2 explores the employment stability of individuals older than 45 years of age hired before the reform after being unemployed for 5 (full line) and 6 months (dashed line). Only individuals in the latter group are eligible for the subsidy. It shows the share of individuals employed for 1–65 consecutive months after being hired. Employment tends to be a short-lived process. Half of the individuals are employed for <10 consecutive months, while one in four is employed for at least 5 years. Although the differences are small and not statistically significant at conventional levels, the figure suggests that individuals hired without a subsidy have more stable employment spells than individuals hired with a subsidy. Individuals employed with and without

Table 1 Characteristics of jobseekers by age group and unemployment duration: Age group; Unemployment duration, before and after the reform

	Jobseekers aged 40–43 years				Jobseekers aged 45–48 years					Triple difference[2]
	Short-term unemployed		Long-term unemployed		Short-term unemployed		Long-term unemployed			
	Before	After	Before	After	Before	After	Before	After	Hired with a subsidy[1]	
Age, years	41.48	41.44	41.49	41.44	46.47	46.47	46.48	46.46	47.13	0.77
Men, %	54.12	54.77	54.76	54.10	53.80	54.84	54.83	54.13	56.84	0.43
Education, %										
Low	38.74	37.66	40.99	39.50	42.73	39.37	44.15	41.56	43.33	−1.18
Medium	39.69	39.01	38.72	37.59	37.47	38.53	36.41	37.10	43.73	−0.08
High	21.57	23.34	20.30	22.91	19.80	22.11	19.44	21.34	12.94	1.25
Proficiency in Dutch, %										
No	1.26	2.80	1.35	2.83	1.07	2.58	1.20	2.71	2.22	−0.05
Limited	11.22	14.17	13.25	16.06	10.06	13.08	11.79	15.48	9.61	−0.81
Good	37.40	32.47	37.70	32.32	39.04	35.13	39.50	35.32	37.60	−0.18
Excellent	50.13	50.57	47.70	48.79	49.83	49.21	47.51	46.50	50.57	1.04
Origin, %										
EU-15	74.46	68.08	70.33	63.86	80.16	73.80	76.42	70.15	77.97	−0.17
EU-13	2.16	3.46	2.44	3.58	1.76	2.97	1.95	3.35	3.11	−0.34
Other	23.38	28.47	27.24	32.56	18.08	23.23	21.63	26.50	18.92	0.52
No. of individuals	34,318	21,127	18,259	8,332	30,405	18,587	16,381	7,192	1,221	–

Notes: The population of short-term unemployed jobseekers consists of all jobseekers who registered at the PES; the population of LTU jobseekers consists of jobseekers who are still unemployed after 6 months. All characteristics, including the jobseekers' age, are determined at the time of registration at the PES. EU-15: Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Luxembourg, the Netherlands, Portugal, Spain, Sweden, and the United Kingdom. EU-13 (new EU member states): Bulgaria, Croatia, Cyprus, the Czech Republic, Estonia, Hungary, Latvia, Lithuania, Malta, Poland, Romania, Slovakia, and Slovenia. Low level of education: at the most lower-secondary education; medium level: upper-secondary education or postsecondary nontertiary education; high level: short-cycle tertiary education, bachelor's level, master's level, or doctoral level.

[1]This column presents the characteristics of individuals aged 45–48 years hired with the subsidy analyzed in this paper, which is targeted at jobseekers older than 45 years of age and unemployed for at least 6 months, between the second quarter of 2015 and the last quarter of 2016 (pre-reform period). We do not have this information for jobseekers hired before 2015 because we obtained data from Flemish institutions that are in charge of this policy since 2015 and only have data since then. We excluded the first quarter of 2015 because we present the characteristics of the new hires, not of the population of subsidized employees at a given moment in time.

[2]This column presents the coefficient of the triple-interaction term of regressing the jobseekers' characteristics on group dummies (young vs. old; short-term unemployed vs. LTU jobseekers), period dummies (before vs. after the reform), and all the interaction terms. None of these triple-interaction terms is significant at the 10% level.

Figure 2   Employment stability: share of individuals aged 45–48 years hired in Month 5 (not eligible for the subsidy) vs. in Month 6 (eligible for the subsidy) employed for 1–65 consecutive months after being hired.
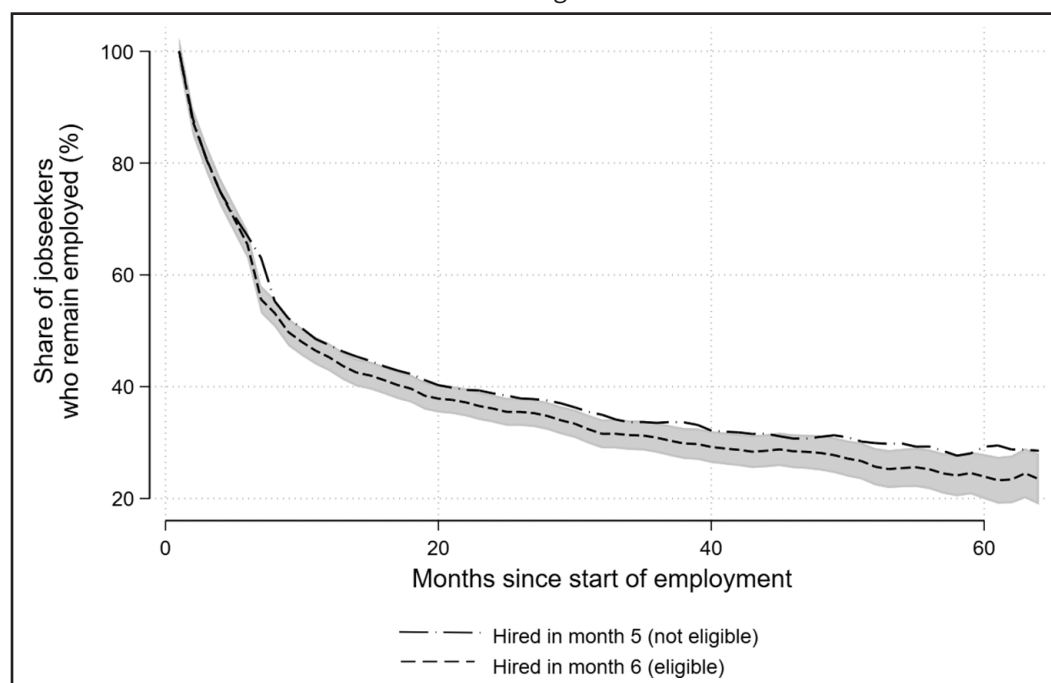
Notes: For clarity, only the 95% confidence interval for individuals hired in Month 6 are shown. The main message is that the employment rate of individuals hired in Month 5 (not eligible for the subsidy) is slightly higher than the employment rate of individuals hired in Month 6 (eligible for the subsidy), but the difference between both groups is not statistically significant. The CIs are constructed by regressing the employment rate on the number of months after registration (dummies), dummies indicating the group (jobseekers hired in Month 5 vs. hired in Month 6), and the interaction terms between these dummies.

a subsidy are equally likely to remain employed for 5 consecutive months. However, after being employed for 5 months, individuals employed with a subsidy are more likely to lose their job. However, this is just descriptive evidence that cannot be given any causal interpretation. In the next section (Section 4), we discuss the strategy that is followed to identify causal effects.

4 Identification Strategy

We aim at estimating the effect of abolition of the hiring subsidy on the transition to employment and on employment stability of LTU jobseekers. Our basic setup follows a triple-difference approach (Gruber, 1994; Cortes and Pan, 2013; Olden and Møen, 2020). This approach exploits differences in labor market prospects between short-term unemployed (unemployed for <6 months) and LTU jobseekers (unemployed for 6–12 months), across age groups (jobseekers aged 40–43 years vs. job-seekers aged 45–48 years[15]), and between periods (before and after the reform).

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15   Jobseekers aged 44–44.5 years at the time of registration at the PES are excluded because these jobseekers are not yet eligible at the 6-month threshold, but they will become eligible after 6–12 months. Jobseekers aged 44.5–45 years belong in theory to the treatment group, as these jobseekers will be eligible after being unemployed for 6 months. We nevertheless excluded them because these jobseekers might be less aware of the existence of the subsidy or might be less likely to apply for a certificate immediately after turning 45 years old. Including the 44.5- to 45-year-old group in the treatment group does not alter the main findings (Table A6 in Appendix A).

The following linear probability model is estimated:

$$y_{imt} = M_m + \beta_1 D_t + \beta_2 A_i + \delta_1 \left(I_m * D_t \right) + \delta_2 \left(A_i * I_m \right) + \delta_3 \left(A_i * D_t \right) \tag{1}$$
$$+ \lambda \left(A_i * D_t * I_m \right) + \eta X_i + year_t + \epsilon_{imt} \quad with\, m \epsilon [1,12]$$

where y_{imt} is the outcome of interest (i.e., the job-finding rate or an indicator of employment stability) of jobseeker i who registered at the PES in calendar month t and has been unemployed for m months. D_t takes, respectively, the value zero and one if individual i became unemployed before or after June 1, 2016.[16] Put differently, D_t is equal to zero before the reform, i.e., when older jobseekers are eligible for hiring subsidies after being unemployed for 6 months. I_m indicates whether the jobseeker has been unemployed for at least 6 months, while A_i indicates whether the jobseeker is older than 45 years of age at the time of registration at the PES. The dummies M_m model negative duration dependence and are required because the probability of finding a job decreases with unemployment duration. X_i is a vector of the jobseekers' characteristics, $year_t$ denotes the year in which the jobseeker became unemployed,[17] and ϵ_{imt} is the error term. By including the year when the jobseeker registered at the PES, we implicitly control for the business cycle at the time of inflow in unemployment.

We examine employment outcomes for jobseekers unemployed for 1–12 months and censor unemployment spells after 12 months. We do not consider the employment prospects of jobseekers unemployed for >12 months because this group was eligible for even more generous hiring subsidies before the reform. By construction, unemployment spells of jobseekers in the pre-reform period never cross the reform date so that jobseekers in this group are never affected by the abolition of the subsidy.

The parameter of interest, λ, indicates how labor market prospects of LTU jobseekers older than 45 years of age changed due to the abolition of the subsidy. Since we study the abolition of a subsidy intended to improve labor market prospects, λ is expected to be negative. Similar to previous authors (Schünemann *et al.*, 2015; Sjögren and Vikström, 2015; Pasquini *et al.*, 2019), we focus on the effect of being eligible for the subsidy, regardless of the actual take-up.

We estimate a triple difference, rather than a DiD, because the parallel trends of the DiD are rejected, as shown in Section 5.1. By contrast, the weaker identifying assumptions of a triple difference are not rejected. The identifying assumption of a triple difference is that the parallel trend in differences holds (Olden and Møen, 2020). In other words, in the absence of the treatment, the difference between the outcomes for long-term vs. short-term unemployed jobseekers younger than 45 years of age must trend similarly as the same difference for jobseekers older than 45 years of age. Importantly, the triple-difference estimator will not be biased if the business cycle affects the LTU differently than the short-term unemployed as long as the differential effect of the business cycle on short-term unemployed vs. LTU is similar for jobseekers younger and older than 45 years of age.

Our approach differs from the standard DiD and triple-difference method because we exploit the abolition of the subsidy to identify the average treatment effect on the treated (ATT)

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16 The subsidy was abolished on January 1, 2017. Jobseekers who registered at the PES in June 2016 are eligible for the hiring subsidy in December 2016. We nevertheless choose June 1, 2016, rather than July 1, 2016, as the cutoff date defining the pre-reform and post-reform periods, because jobseekers becoming eligible for the subsidy in December 2016 do not have much time to apply for the necessary certificates proving eligibility and are, therefore, unlikely to benefit from the subsidy. We show that the results are not sensitive to using a different cutoff.

17 The results do not alter when controlling for the quarter (rather than year) of inflow in unemployment.

rather than exploiting the introduction of a subsidy, which is more common. In our setup, the control group is never treated (as is standard), while the treatment group is treated in the pre-reform period and is not treated in the post-reform period. The post-reform period is thus the period during which both the treatment and the control groups are not treated, while this state of the world typically occurs in the pre-reform period in the standard setting. This implies that the parallel trend assumption should hold in the post-reform period (when both groups are not treated) and not necessarily in the pre-reform period (when the treatment group is treated). We report placebo tests for the post-reform period. As an additional placebo test, we also examine whether the job-finding rate of the short-term unemployed remains unaffected by the reform.

One additional concern with our approach is bias due to dynamic sorting. The linear probability model is a robust estimator of the average partial effect in settings with discrete duration data (Angrist, 2001), but it only holds in the absence of unobserved individual effects (see, e.g., Farbmacher and Tauchmann, 2021). Unobserved individual effects cause dynamic sorting because individuals with a higher unobserved "ability" are more likely to quickly resume work and, hence, leave the sample. One way to test for the presence of unobserved individual effects is to check whether the grouped residuals are autocorrelated over time. Dynamic sorting leads to negative autocorrelation. We follow Cockx and Ridder (2001) and test for this autocorrelation based on the Breusch–Godfrey Lagrange multiplier (LM) test of autocorrelation.

Like in DiD specifications, substitution and anticipation effects can bias the triple-difference estimate. We examine whether (1) LTU jobseekers were hired at the expense of the short-term unemployed; (2) whether older jobseekers were hired at the expense of younger jobseekers; and (3) whether the last eligible cohorts of jobseekers were hired at the expense of the first ineligible cohorts.

We first examine the effect of the subsidy on the job-finding rate. In this case, the outcome variable, y_{imt}, is an indicator that is equal to one if a jobseeker unemployed up to month $m-1$ resumes work in month m, and zero otherwise. This definition corresponds to a hazard rate such that the unemployment spell ends when the jobseeker resumes work. In this case, the estimated parameter of interest $\hat{\lambda}$ measures, in percentage points (pp), the extent to which the subsidy facilitates the transition to employment of the LTU.

After establishing the positive effect of the subsidy on the job-finding rate, we examine whether the subsidy created short-lived or long-lasting stable employment.[18] To this end, we reestimate Eq. (1) but use different outcome variables. First, we use a more stringent definition of employment, thereby excluding short-lived temporary help agency work. Second, we define indicators that capture employment stability. These indicators are hazard rates: they are equal to zero as long as the jobseekers do not start an employment spell that meets a certain condition, are equal to one if this condition is met, and are not defined thereafter. We define 12 indicators that are equal to one if employment that lasts X consecutive months is found, where X is in the range of 1–12.[19] Hence, the first indicator equals one if the jobseeker finds employment (which corresponds to the basic specification), the second indicator equals one if the

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18 We do not observe job-to-job transitions without an intervening unemployment spell. For this reason, the paper focuses on employment stability, not on job stability or on job tenure.

19 The indicator is only defined if the total time span is within the observed period, i.e., until February 2019. Hence, the regressions contain fewer observations when considering longer employment durations.

jobseeker finds employment that lasts at least 2 consecutive months, etc. We also experimented with alternative indicators. Rather than requiring that an employment spell last X consecutive months, these alternative indicators take the value one if a jobseeker is employed for X months over a 12-month period after being hired. This is a slightly less demanding definition of employment stability since it does not require continuous employment in the next 12 months after being hired. These alternative indicators allow testing whether short-lived employment spells serve as stepping stones to other (short-lived) spells within the next 12 months.

5 Results

5.1 Difference-in-differences vs. triple difference

Given the setting, it would be natural to estimate the abolition of the subsidy in a DiD framework, contrasting either short-term unemployed to LTU older jobseekers, or jobseekers younger than to those older than 45 years of age among the LTU.

Figure 3 illustrates the former approach and shows the job-finding rate of jobseekers aged 45–48 years as a function of elapsed unemployment duration before and after the reform. The impact of the abolition of the subsidy appears evident. Due to the improved economic conditions, the job-finding rate is higher for the "control" unemployment durations in 2017 than in 2015, but the job-finding rate abruptly decreases in 2017 at the moment the jobseeker would have become eligible for the subsidy before the reform. Reassuringly, such a

Figure 3 Exploring the effect of the abolition of the subsidy in a DiD framework: the job-finding rate of jobseekers aged 45–48 years as a function of elapsed unemployment duration, before and after the reform.
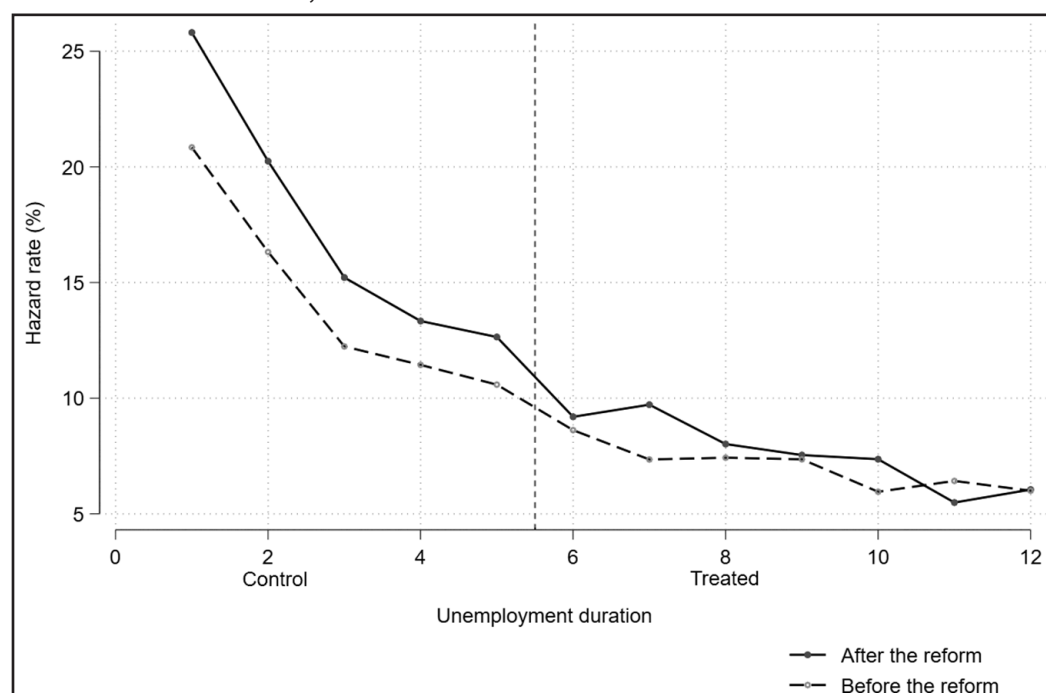
Notes: The vertical dashed line indicates the month in which jobseekers become eligible for the hiring subsidy prior to the reform. The pre-reform period consists of jobseekers who registered at the PES in 2015. The post-reform period consists of jobseekers who registered in 2017. DiD, difference-in-differences; PES, public employment service.

"jump" in the job-finding rate is not observed for jobseekers aged 40–43 years (Figure A2 in Appendix A), which suggests that the jump observed here can be attributed to the abolition of the subsidy.

Formal DiD estimates are reported in Appendix B. Contrasting short-term unemployed to LTU older jobseekers, as in Figure 3, suggests that the abolition of the subsidy decreased the job-finding rate by 3.52 pp [95% CI: −4.03 to −3.00]. The DiD model exploiting differences across age groups, rather than across unemployment duration, suggests that the abolition of the subsidy reduced the job-finding rate by 0.14 pp [95% CI: −0.62 to 0.34]. Hence, the abolition of the subsidy has a substantial negative effect (−38%[20] in relative terms) on the job-finding rate when exploiting differences across unemployment duration, but the effect is close to zero when exploiting differences across age groups.

Appendix B presents placebo tests that reject the validity of both DiD approaches. The business cycle causes biases in both approaches in opposite directions. Economic upswings have a stronger positive effect on the short-term unemployed than on the LTU, a well-known finding in the literature (Machin and Manning, 1999). Hence, comparing the job-finding rates of short-term unemployed and LTU older jobseekers (as in Figure 3) overestimates the negative effect of abolition of the subsidy since it does not capture the information that, relative to the short-term unemployed, the job-finding rate of the LTU would have deteriorated anyway after the reform due to the improved economic conditions. At the same time, we find evidence that the job prospects of all jobseekers aged 45–48 years, including the short-term unemployed who were not affected by the reform, improved more than those of younger jobseekers after the reform, most likely because the improved economic conditions were more beneficial to older than younger jobseekers. Hence, contrasting the job-finding rate of younger to older LTU jobseekers underestimates the negative effect of the abolition of the subsidy.

The triple-difference approach captures these business cycle effects and avoids wrongly attributing them to the abolition of the subsidy. The weaker identifying assumption of a triple difference, as we show in Section 5.2, is not rejected. The first regression in Table 2 estimates the benchmark specification. The triple-interaction term in the first specification indicates that the abolition of the subsidy reduced the job-finding rate by 1.18 [95% CI: 0.48–1.88 pp]. This is a relatively large effect. Given that the job-finding rate after being unemployed for 6 months is 9.2% after the reform, the hiring subsidy increased the job-finding rate by 13% [95% CI: 5–20%]. We also tested whether the effect on the job-finding rate varied by unemployment duration (Table A6 in Appendix A). While the effect appears to decrease with unemployment duration, a formal *F*-test rejects the hypothesis that these differences are statistically significant at the 5% level.

Other studies evaluating hiring subsidies targeted at the LTU also report positive effects on the job-finding rate. The results cannot be directly compared since the targeted population, the take-up rate, the subsidy amount, and the subsidy duration differ. Sjögren and Vikström (2015) find that a hiring subsidy in Sweden targeted at jobseekers unemployed or out of the labor force for at least 1 year increased the job-finding rate by 5.8%. This was the finding for the case that the payroll tax (31.4%) was no longer due for a period equal to their time in unemployment or out of the labor force, thereby decreasing labor costs on average by 24% during this

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20   −38%=−3.52/9.2 (where 9.2% is the job-finding rate of jobseekers aged 45–48 years unemployed for 6 months who registered at the PES in 2017).

Table 2 The impact of the abolition of the hiring subsidy on the job-finding rate: triple-difference estimates

	Benchmark	Ruling out:			More stringent definition of "work" (excluding short-lived temporary work)
		Substitution between age groups	Substitution between the short-term and long-term unemployed	Anticipation effects	
Age 45–48 years ## post-reform ## long-term unemployed (base level: 40–43 years)					
	−0.0118***		−0.0119***	−0.0133***	−0.00636**
	(0.00358)		(0.00377)	(0.00408)	(0.00273)
Post-reform ## long-term unemployed (base level: 35–37 years)					
## Age 38 years		0.00779**			
		(0.00389)			
## Age 39 years		0.000466			
		(0.00394)			
## Age 40 years		0.00170			
		(0.00399)			
## Age 41 years		−0.00158			
		(0.00404)			
## Age 42 years		0.00222			
		(0.00413)			
## Age 43 years		−0.000159			
		(0.00410)			
## Age 44–44.5 years		0.000781			
		(0.00558)			
Age 45–48 years ## post-reform ## unemployed for 5 months					
			−0.000717		
			(0.00667)		
No. of unemployment spells	104,437	281,701	104,437	89,291	104,437
No. of observations	602,916	1,686,890	602,916	516,196	753,119
R^2	0.032	0.033	0.032	0.032	0.013
Placebo tests					
Post-reform parallel trend test (*F*-test, *p*-value)	0.59	0.93	0.75	0.63	0.20
Effect on the short-term unemployed (*p*-value)	0.84	–	–	0.86	0.91

Notes: Results of estimation of the triple-differences regressions as specified by Eq. (1) are shown. The benchmark specification contrasts the job-finding rate across age groups (40–43 vs. 45–48 years old), unemployment duration (unemployed for 1–5 months vs. unemployed for 6–12 months), and period (before vs. after the reform). The other triple-difference regressions rule out substitution and anticipation effects by, respectively, using jobseekers aged 35–37 years as the control group, using jobseekers unemployed for 1–4 months as the control group, and excluding jobseekers who registered as unemployed in 2016 from the regression. The last triple-difference regression follows the same specification as the benchmark but uses a more stringent definition of "work", thereby excluding temporary agency work lasting between 10 days and 20 days per month. The following control variables are included in all regressions: unemployment duration (dummies), year of registration at the PES, level of education (low, medium, and high), self-reported proficiency in Dutch, region of origin, and sex. Full results of the benchmark specification are reported in Table A2 in Appendix A. The placebo tests verify whether the parallel trends assumption holds in the post-reform period and whether the abolition of the subsidy did not affect the job-finding rate of jobseekers unemployed for 5 months (see also Table A3 in Appendix A). Huber–White standard errors robust for heteroskedasticity are in parentheses.

***and **denote statistical significance at the 1% and 5% levels, respectively.

period. Doubling the subsidy or doubling the duration of the subsidy increased the job-finding rate by 28% and 65%, respectively. Ciani *et al.* (2019) and Pasquini *et al.* (2019) evaluate a subsidy targeted at jobseekers in (Southern) Italy unemployed for at least 2 years using different identification strategies. This hiring subsidy covered 50–100% of the payroll tax of 32.7% for 3 years, thereby reducing labor costs by 12–25%. Ciani *et al.* (2019) report that the subsidy increased the weekly chance of finding a permanent job by 41%, while, according to Pasquini *et al.* (2019), the subsidy increased the job-finding rate by 36%.

In order to make the findings of these studies more comparable, we calculate the elasticity of the transition from unemployment to employment with respect to the labor cost. The Belgian hiring subsidy reduced the labor cost by 13% at the median wage level and increased the transition to employment by 13% [95% CI: 5–20%], so that the elasticity equals −1 [95% CI: −1.54 to −0.38]. Similar calculations yield an estimate of this elasticity in Italy of −2.2 and −2.5, respectively, for the studies of Pasquini *et al.* (2019) and Ciani *et al.* (2019). By contrast, in Sweden, the elasticities are −0.24 for the single subsidy and −0.59 for the double subsidy. While these are crude estimates that warrant careful interpretation, they nevertheless indicate that our estimates are in-between those for Sweden and Italy, and that hiring subsidies targeted at the LTU are effective, at least in terms of the hiring rate. However, as we discuss in Section 5.3, this effectiveness is transitory.

At the bottom of Table 2, we report the results of two placebo tests. First, we test whether the job-finding rate evolved in parallel in the treatment and control groups in the post-reform period. This approach corresponds to pretending that the reform took place in a "placebo" year after the reform (e.g., in 2017 rather than in 2016) and testing whether the triple-interaction term is insignificant in this case. As explained earlier, the credibility of our DiD approach requires that the parallel trend in the post-reform period – when both treatment and control groups are not treated – holds. This is the case.[21] The second placebo test examines whether the job-finding rate of the short-term unemployed is unaffected by the reform. It tests whether the job-finding rate of jobseekers unemployed for <5 months has changed relative to the job-finding rate of jobseekers unemployed for exactly 5 months. This placebo test shows that the reform did not affect the job-finding rate of the short-term unemployed, thereby further confirming the validity of the triple-difference approach.

To rule out bias due to dynamic sorting, we test for autocorrelation based on the Breusch–Godfrey LM test of autocorrelation. To this end, the population is split into groups using all possible combinations of the explanatory variables included in the regression, and the average error in each group is calculated. The grouped error is regressed on the lagged grouped error and the explanatory variables using the inverse of the variance of the dependent variable as weights (Table A7 in Appendix A). We find a small, but significant, positive autocorrelation of 0.053.[22] The presence of a small positive autocorrelation, rather than negative autocorrelation, indicates that dynamic sorting does not bias our findings and justifies the choice of a

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21　The parallel trend also holds in the pre-reform period, which indicates that the effectiveness of the hiring subsidy is not sensitive to the business cycle. This is noteworthy because recent studies have found that wage and hiring subsidies are more effective during downturns (Benzarti and Harju, 2021) and in regions with higher levels of unemployment (Saez *et al.*, 2021).

22　In Table 2, we report Huber–White standard errors robust for heteroskedasticity. We could also cluster the standard errors at the individual level to take into account that a minority of the jobseekers have multiple unemployment spells, which could explain the small, positive autocorrelation. In the baseline specification, the clustered standard errors are equal to 0.00361, only slightly larger than the robust standard errors (0.00358).

linear probability model. This analysis complements earlier (descriptive) evidence, presented in Table 1, which showed that the observable jobseekers' characteristics remained stable before and after the reform. It also implies that jobseekers aged 40–48 years are a homogeneous population, with little (un)observed heterogeneity among jobseekers.

Finally, we examine whether the results are sensitive to the cutoff date, which determines whether jobseekers are classified in the pre-reform or post-reform period. In the baseline specification, we assume that jobseekers who registered at the PES after June 1, 2016, are not eligible for the hiring subsidy. However, jobseekers who registered at the PES in June 2016 become eligible for the subsidy in December 2016. In practice, these jobseekers do not have much time to obtain the necessary documents proving eligibility. For this reason, we classified these jobseekers in the control group. The results are not sensitive to the choice of the cutoff. The results remain unaltered under the following conditions: (1) using July 1, 2016, as the cutoff date; (2) excluding jobseekers who registered at the PES in June 2016 from the regression; and (3) excluding jobseekers who registered at the PES in the period April–June 2016 (as these jobseekers may not have enough time to apply for the necessary certificates). These sensitivity analyses are presented in Table A6 in Appendix A.

5.2 **Substitution and anticipation effects**

The triple-difference approach exploits differences across unemployment duration, age, and time periods. Substitution and anticipation effects can bias the triple-difference estimates. Regressions 2–4 in Table 2 examine and rule out the possibility that the presence of substitution or anticipation effects bias the findings. This also indicates the absence of general equilibrium effects: the positive effects on the job-finding rate of the target group are not at the expense of ineligible groups.

First, we consider eight age groups to examine whether, prior to the reform, employers hired older jobseekers eligible for the hiring subsidy at the expense of younger jobseekers. Using the triple-difference approach, we evaluate whether the job-finding rates of jobseekers younger than 44.5 years improved after the abolition of the subsidy. To do so, we compare the job-finding rate of jobseekers by age in the age group 38–44 years to the job-finding rate of jobseekers aged 35–37 years. Under the assumption that the substitutability decreases with the age differential, then, if substitution effects do matter, the positive effect of the subsidy on the older eligible unemployed will be at the expense of the job-finding rate of those below the eligibility threshold, and, more so, the closer the age is to the eligibility threshold. For this reason, we also included jobseekers aged 44–44.5 years in the regressions – a group excluded in the baseline specification – because this age group is just below the age eligibility threshold, and, therefore, most likely to be negatively affected by the hiring subsidy. There is no evidence that the job-finding rate of jobseekers just below the eligibility threshold improved after the abolition of the hiring subsidy (Table 2, Regression 2).

Second, we examine whether, prior to the reform, the LTU were hired at the expense of the short-term unemployed. Again, we find no evidence of these substitution effects. The job-finding rate of jobseekers aged 45–48 years and unemployed for 5 months did not improve after the abolition of the subsidy relative to the job-finding rate of jobseekers in

the same age group unemployed for <5 months (Table 2, Regression 3).[23] This indicates that employers did not hire the LTU at the expense of jobseekers who were just below the eligibility threshold.

Third, we examine anticipation effects. The decision to abolish hiring subsidies for the LTU took time and was not (unexpectedly) announced at a specific point in time. Hence, employers could have anticipated the reform and hired more LTU jobseekers just before the abolition of the subsidy. The reform was, however, barely discussed in the Flemish media[24], which makes it likely that many employers were unaware of the reform, which reduces the scope for anticipation effects. We examine anticipation effects by excluding jobseekers who became unemployed in 2016 from the regressions. Jobseekers who registered in the first two quarters of 2016 remained eligible for a hiring subsidy until the end of 2018, whereas jobseekers who registered during the last two quarters of 2016 were never eligible for the subsidy. Employers may have tried to hire the last cohort of eligible jobseekers at the expense of the first cohort of ineligible jobseekers. Excluding jobseekers registered in 2016 from the regressions does not, however, alter the main findings (Table 2, Regression 4).

5.3 Employment stability

While the subsidy increased the job-finding rate of the LTU, the question remains whether the subsidy successfully created stable employment. Descriptive evidence presented earlier indicates that subsidized employment tends to be a short-lived process. Moreover, search and matching models predict that hiring subsidies increase job creation and job destruction, leading to less-stable jobs (Kaas and Kircher, 2015; Mortensen and Pissarides, 2001). Most previous studies have not focused on employment stability, although creating stable jobs is often an explicit objective of hiring subsidies. One notable exception is the study by Sjögren and Vikström (2015), who present evidence that workers holding subsidized jobs are more likely to retain their job even after the subsidy expires. A recent evaluation of a payroll tax cut for young workers in Sweden also finds persistent effects after the workers' age moves out of eligibility and even after abolition of the policy (Saez *et al.*, 2021).

To test the effect of the subsidy on employment stability, we again rely on the triple-difference approach but use different outcome variables. We first use a more stringent definition of "employment", which excludes temporary help agency work. Next, we define two related outcome variables that capture employment stability. The first outcome is defined as finding a job and remaining employed for 1–12 consecutive months after being hired, while the second outcome is defined as finding a job and being employed for 1–12 (not necessarily consecutive months) in the next 12 months after being hired. The second indicator allows us to test whether short-lived employment spells created by the subsidy serve as a stepping stone to other (short-lived) employment spells within the next 12 months.

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23 We also compared the job-finding rate of jobseekers unemployed for 5 months with the job-finding rate of jobseekers unemployed for 1 month, 2 months, 3 months, or 4 months. None of the triple-interaction terms is significant at conventional levels, providing additional evidence that the job-finding rate of jobseekers unemployed for 5 months did not improve after abolition of the subsidy.

24 We conducted a search of the Flemish media using GoPress. A few newspaper articles in 2015 mentioned that the existing hiring subsidies would be reformed and suggested that the hiring subsidies for the long-term unemployed might be abolished. In 2016, several newspaper articles discussed the reform in more detail.

As mentioned earlier, the Flemish PES records different types of "work". The regressions presented so far consider any type of employment as a positive outcome. The fifth regression in Table 2 uses a more stringent definition of work, which excludes several types of atypical work, most importantly, temporary help agency work that lasted between 10 days and 20 days per month. Excluding atypical work as a positive outcome halves the triple-interaction term to −0.6 pp, but the effect remains statistically significant at the 5% level. This is a first indication that the hiring subsidy mainly created short-lived employment and had a more-limited effect on the creation of more-stable employment.

To further examine employment stability, we redefine the outcome as finding a job and remaining employed for several (consecutive) months in the next 12 months after being hired, again including all types of work. Figure 4 visualizes the results of the triple-difference regressions for both outcomes. The subsidy has a more pronounced effect on finding short-lived employment than on longer-lasting employment. The effect of the abolition of the subsidy converges toward zero with increasing employment duration. While the abolition of the subsidy reduced the likelihood of being employed for at least 1 month by 1.18 pp, the abolition reduced the likelihood of being employed for at least 6 and 12 consecutive months by 0.41 and 0.05 pp, respectively. In relative terms, this implies that the hiring subsidy increased the job-finding rate by 13%, but it only increased the probability of being employed for at least 6 and 12 consecutive months by 6.6% and 1.5%, respectively. This is in line with the graphical evidence presented in Figure 2, which suggests that the employment trajectories

Figure 4   The effect of abolition of the subsidy on the probability of finding employment lasting 1–12 (consecutive) months.
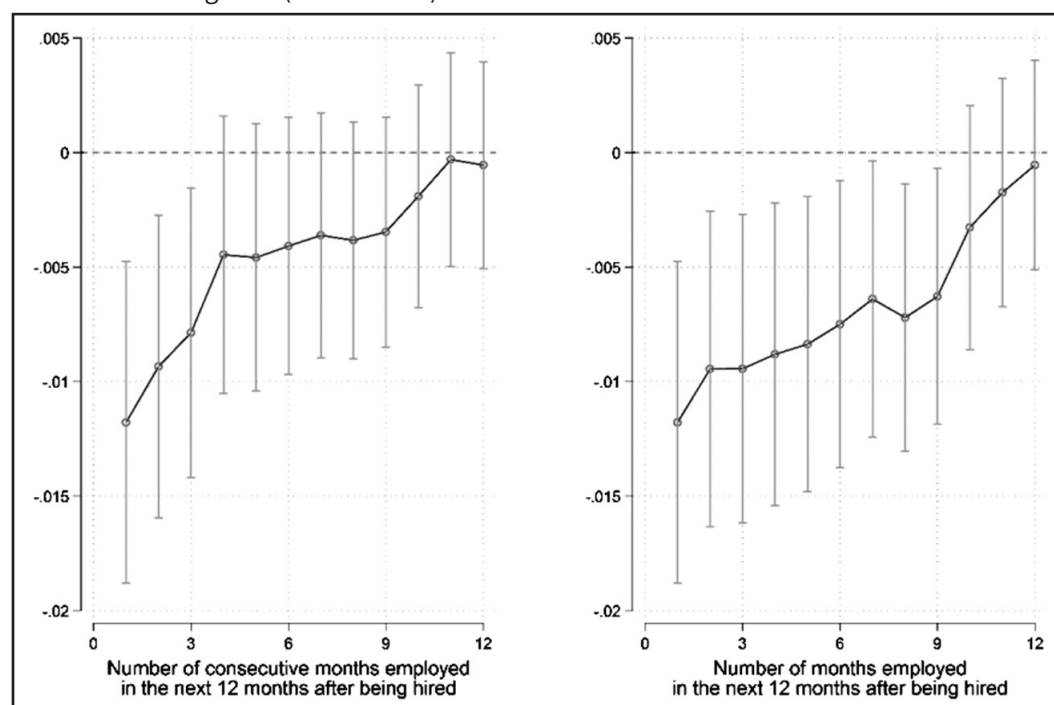
Notes: Each graph shows the triple-interaction term with its 95% confidence interval obtained by estimating 12 triple-difference regressions following the benchmark specification as in Table 2. The outcome is an indicator taking the value of one if the jobseeker found a job and remained employed for 1–12 (consecutive) months, and zero otherwise. Full results of the triple-difference regressions are reported in Table A4 in Appendix A.

of individuals hired with a subsidy were less stable than those of individuals hired without a subsidy.

Abolition of the subsidy has only a slightly larger effect on the number of months worked (not necessarily consecutive) over a 12-month period after being hired (Figure 4, right panel) than on the number of consecutive months worked over the same period (Figure 4, left panel). Hence, the short-lived employment spells created by the subsidy do not serve as stepping stones to other employment spells in the next 12 months.

The results remain qualitatively similar when using the more stringent definition of employment (Figure A3 in Appendix A). While the subsidy's effects on the job-finding rate are more limited when excluding atypical work, the effect of the subsidy on stable employment still tends toward zero for employment spells that last at least 12 (consecutive) months.

5.4 **Effect heterogeneity**

In this section, we examine whether certain groups of jobseekers benefited more from the hiring subsidy. We explore effect heterogeneity along two dimensions: gender and level of education. These dimensions are important for three reasons. First, systematic reviews of ALMPs often report estimates along these dimensions. For instance, the review of Card *et al.* (2018) reports a larger impact for women and for disadvantaged groups. Second, we compare our findings with those of Sjögren and Vikström (2015), who report larger effects for LTU men and LTU individuals without university education. Third, a heterogeneity analysis can shed light on the underlying mechanism explaining the results.

Table 3 examines the effect of abolition of the subsidy on the job-finding rate and on employment stability by gender and level of education. The hiring subsidy is more effective for jobseekers with at least a bachelor's degree. It also appears to be more effective for women (+21.4% in relative terms) than men (+7.8%), but the difference between the effects on men and women is not statistically significant (Table 3, Column 1).[25] For jobseekers with at least a bachelor's degree, the subsidy increases the job-finding rate by 2.6 pp, whereas the effect size is only 0.7 pp for the lower-educated group.[26] In relative terms, the subsidy increased the job-finding rate by 29.6% among jobseekers with at least a bachelor's degree and by 8.0% among jobseekers without a bachelor's degree. The pattern is qualitatively similar when using a more stringent definition of work as the outcome (Table 3, Column 2), but the impact of the subsidy is then less than half as large for the high-educated group (+1.1 pp), suggesting that more than half of the effect is induced by enhanced hiring in very short-lived temporary jobs. The impact is also smaller for the lower-educated group (0.5 pp), but the effect declines less, so that the effect difference between the lower- and higher-educated groups is no longer statistically significant at the 10% level.

For all groups, the effect of the subsidy decreases with employment duration, confirming that the subsidy did not create stable employment (Table 3, Column 3). For the higher educated, the effect is still significantly positive for remaining employed for 6 consecutive months, but

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25 The post-reform placebo tests also hold for each of the subsamples. This observation rules out the possibility that the effect heterogeneity observed here can be attributed to a failure of the parallel trend for specific groups.

26 We grouped jobseekers with low and medium levels of education because we did not observe differences between these groups.

Table 3 The effect of the subsidy by gender and level of education

	Job-finding rate		Job stability
	Benchmark specification	**More stringent definition of "work" (excluding short-lived temporary work)**	**Remaining employed for 6 consecutive months**
Sex			
Men	−0.0076	−0.0048	−0.0044
	(0.0118)	(0.0039)	(0.0039)
Women	−0.0168***	−0.0083**	−0.0041
	(0.0052)	(0.0038)	(0.0042)
Education			
Less than a bachelor's degree	−0.0074*, a	−0.0051*	−0.0005[a]
	(0.004)	(0.003)	(0.0032)
At least a bachelor's degree	−0.0264***, a	−0.0113*	−0.0163**, a
	(0.0077)	(0.0062)	(0.0064)

Notes: The table shows the triple-interaction term obtained by estimating triple-difference regressions on different subsamples following the benchmark specification as in Table 2. Huber–White standard errors robust for heteroskedasticity are in parentheses.

***, **, and *denote statistical significance at the 1%, 5%, and 10% levels, respectively.

The letter "a" indicates statically significant differences between estimates within the same column under the same heading at the 5% level. For instance, in the first column, the effect on jobseekers without a bachelor's degree is significantly different from the effect on jobseekers with at least a bachelor's degree. The job-finding rate among men, women, and jobseekers without and with a bachelor's degree aged 45–48 years who registered at the public employment service in 2017 and are unemployed for 6 months is, respectively, 10.3%, 7.9%, 9.3%, and 8.9%. This information is used to compute the effect of the reform in relative terms.

it is only 60% of its effect on the job-finding rate, and it further decreases to 35% of this effect (not reported in Table 3) when considering 12 instead of 6 consecutive months. Unfortunately, as the subsidy can last as long as 21 quarters, the data do not allow to check whether the effect for the higher-educated group converges to zero after the subsidy expires, but the pattern of the effect does suggest this.

Our findings are the opposite of those of Sjögren and Vikström (2015), who report larger effects for individuals without a university education and also for men. They interpret these findings as suggestive evidence that the private sector, which tends to employ more men, is more responsive to the policy, and that labor demand is more sensitive at the lower end of the pay distribution, which explains why the hiring subsidy is more beneficial to lower-educated workers. The latter explanation is not relevant in our setting because individuals with a bachelor degree hired with the subsidy have only slightly higher wages than individuals without a bachelor degree (mean monthly gross wage of €2,489 vs. €2,139, respectively), presumably as a consequence of the relatively high minimum wages in Belgium [see, e.g., Rycx and Kampelmann (2013)], and that dismissed workers among the high educated are more negatively selected than among the low educated.

We see the following explanations for our findings. First, our analysis focuses on laid-off "mid-career" individuals. At this stage of the career, job loss usually goes in pair with substantial losses in firm-specific labor market experience and human capital. Consequently, the hiring subsidy and the temporary spells of employment cannot compensate for this loss, making it

hard without retraining to materialize an enduring – lasting beyond the subsidization period – labor market reintegration. The finding that higher-educated workers are more likely to be temporarily hired than lower-educated workers owing to the subsidy could be related to their acquisition of more general, transferable skills: this makes them more likely to be hired in different jobs than those from which they were laid-off; the subsidy may compensate for the higher wage costs that are required to make these jobs acceptable or may make up for the productivity gap linked to the reallocation.

Second, we already highlighted that the subsidies were mainly used by temporary help agencies. Out of all hires with a subsidy, roughly 60% were hired by temporary help agencies. It is noteworthy that groups that traditionally rely more on the services of temporary help agencies, namely, (men and) lower-educated jobseekers, are also the groups that benefited less from the subsidy. The subsidies seem to have provided a windfall profit for temporary help agencies, which would anyway have offered temporary jobs to low-educated jobseekers. However, without a subsidy, the hiring of jobseekers with a university degree did not seem profitable for these agencies. By the above argument, the wage subsidy could therefore create additional employment for these workers. However, this employment is not long-lasting or does not seem to be a stepping stone to other, more persistent employment, probably because temporary work is not skill enhancing and may even stigmatize workers (Autor and Houseman, 2010; Givord and Wilner, 2015). This is in line with the findings of Cockx and Van Belle (2019) and Cockx *et al.* (2020), who report that, in Belgium, temporary work does not cause long-lasting employment for unemployed youth. In addition, it can also explain why, as reported in Section 3, most of the subsidized jobs ended before the subsidy was exhausted. Temporary help agencies offer temporary jobs, which are highly unlikely to exceed the maximum duration of the subsidy (about 5 years).

6 Conclusion

Using a triple-difference approach, this paper studies the effectiveness of a hiring subsidy targeted at jobseekers older than 45 years of age and unemployed for at least 6 months. The subsidy increases the job-finding rate by 13%. This corresponds to an elasticity of of −1 for the transition from unemployment to employment with respect to the labor cost, which is in-between the estimates from evaluations of hiring subsidies targeted at the LTU in Sweden and Italy.

However, the subsidy mainly created temporary employment and had a more-limited effect on entering stable employment, i.e., employment that lasted several months. The effect of the subsidy gradually decreased with the duration of the employment spell, and the subsidy did not improve the likelihood of finding employment that lasted at least 12 (consecutive) months. Most subsidized employment spells were destroyed after a few quarters, well before the subsidy expired. Moreover, the subsidy was more effective for individuals with at least a bachelor's degree, while jobseekers without a bachelor's degree did not benefit from the subsidy. For these reasons, our study points toward a rather limited potential of hiring subsidies to create (stable) employment for the specific target group of prime-aged jobseekers unemployed for at least 6 months considered in this study. This observation contrasts with the rather favorable evaluations in Italy and Sweden and with ALMP meta-analyses that tend to conclude that hiring subsidies are an effective instrument.

These findings are in line with the hypothesis that the displacement costs of job loss for these mid-career workers are so substantial that a temporary subsidy is insufficient for an enduring reintegration into the labor market. Therefore, the type of policies that can be successful for this target group remains an open question. In view of the involved substantial loss in skills and the specificity of these skills, it seems natural that such policies should involve some education or training to provide these workers with the necessary skills to relocate to a new industry (Jacobsen *et al.*, 2011). A number of studies find supportive evidence for this. Jacobsen *et al.* (2005a, 2005b) show that enrollment in community college education can dramatically improve the long-term earnings prospects of displaced workers aged 35 years or older in the United States. Similarly, Picchio and Van Ours (2013) find positive effects of training on the employment prospects of older workers. Cockx *et al.* (1998), evaluating labor market policies in Belgium from the 1990s, show that subsidized on-the-job training created more stable employment than pure hiring subsidies. For this target group, the evaluation of programs with a training content is therefore a promising direction for future research.

Declarations
Availability of data and materials

Competing interest
None.

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Authors' contribution
SD and BC designed the study. SD requested the data and conducted the empirical analysis. SD and BC drafted the manuscript. Both authors read and approved the final manuscript.

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Appendixes

Appendix A

Table A1 The five most important sectors in terms of new hires with a subsidy in each quarter (2015Q2–2016Q4)

Sector	Share of new hires with a subsidy, 2015Q2–2016Q4
Temporary employment agency activities	61%
Cleaning activities	4%
Restaurants and mobile food service activities	2%
Retail sale in nonspecialized stores	2%
Other social work activities without accommodation	2%

Notes: Sector classified based on the NACE three-digit classification. The table shows the share of long-term unemployed jobseekers aged 45–48 years hired on average in each quarter with a subsidy by sector.

Table A2 The impact of abolition of the hiring subsidy on the job-finding rate: full results of the benchmark specification

	Benchmark
Unemployment duration (base level: Month 1)	
Month 2	−0.0407***
	(0.00183)
Month 3	−0.0841***
	(0.00181)
Month 4	−0.0998***
	(0.00183)
Month 5	−0.109***
	(0.00185)
Month 6	−0.111***
	(0.00213)
Month 7	−0.122***
	(0.00213)
Month 8	−0.129***
	(0.00213)
Month 9	−0.132***
	(0.00214)
Month 10	−0.141***
	(0.00212)
Month 11	−0.141***
	(0.00215)
Month 12	−0.143***
	(0.00217)

(*Continued*)

Table A2 Continued

	Benchmark
Post-reform	0.0250***
	(0.00275)
Age 45–48 years	−0.00487***
	(0.00144)
Age 45–48 years ## post-reform	0.0101***
	(0.00261)
Post-reform ## long-term unemployed	−0.0231***
	(0.00247)
Post-reform ## age 45–48 years ## long-term unemployed	**−0.0118***
	(0.00358)
Year of registration at the PES (base level: 2012)	
2013	−0.00498***
	(0.00147)
2014	0.00309**
	(0.00154)
2015	0.0131***
	(0.00159)
2016	0.0115***
	(0.00205)
2017	0.0162***
	(0.00283)
2018	0.0205***
	(0.00298)
Education (base level: high level)	
Low	−0.00236**
	(0.00116)
Medium	0.00423***
	(0.00115)
Man	0.00759***
	(0.000845)
Proficiency in Dutch (base level: no proficiency)	
Limited proficiency	−0.00967***
	(0.00332)
Good proficiency	0.00455
	(0.00325)
Excellent proficiency	0.00491
	(0.00329)
Origin (base level: EU-15)	
EU-13	−0.0196***
	(0.00261)
Other	−0.0250***
	(0.00110)
Constant	0.197***
	(0.00387)
No. of unemployment spells	104,437
No. of observations	602,916
R^2	0.032

Notes: Huber–White standard errors robust for heteroskedasticity are in parentheses.
***and **denote statistical significance at the 1% and 5%levels, respectively.
EU, European Union; PES, public employment service.

Table A3   Placebo tests for the benchmark analysis

	Parallel trend	Placebo test for the short-term unemployed
Age 45–48 years ## long-term unemployed ##		
2012	0.00494	
	(0.00613)	
2013	0.00441	
	(0.00534)	
2014	−0.000794	
	(0.00559)	
2016 Q1–Q2	0.00574	
	(0.00762)	
2016 Q3–Q4	**−0.00516**	
	(0.00715)	
2017	**−0.0101***	
	(0.00616)	
2018	**−0.0137***	
	(0.00721)	
Age 45–48 years ## after reform ## unemployed for (base level: 5 months)		
Less than 5 months		0.00134
		(0.00668)
6–12 months		−0.0112*
		(0.00650)
No. of unemployment spells	104,437	104,437
No. of observations	602,916	602,916
R^2	0.032	602,916
Pre-reform parallel trend (*F*-test, *p*-value)	0.74	
Post-reform parallel trend (*F*-test, *p*-value)	0.59	
No effect on the short-term unemployed (*p*-value)		0.84

Notes: The regressions include the same (control) variables as the benchmark specification (Table A2 in Appendix A). For the sake of conciseness, only the variables relevant for the placebo tests are shown. The post-reform placebo test does not reject the equality of the coefficients printed in bold, indicating that the parallel trend holds after the reform. The placebo test examining the effect of the abolition on the short-term unemployed rejects that the job-finding rate of jobseekers unemployed for <5 months was affected by the reform. We also test whether the parallel trend holds in the pre-reform period. This trend holds, which indicates that the effectiveness of the hiring subsidy does not vary over the business cycle. Huber–White standard errors robust for heteroskedasticity are in parentheses.

*denotes statistical significance at the 10% level.

Table A4 The effect of the abolition of the hiring subsidy on finding a job and remaining employed for 1–12 consecutive months (Figure 4, left panel in the main text plots the results)

	Employed for at least:											
	1 month	2 months	3 months	4 months	5 months	6 months	7 months	8 months	9 months	10 months	11 months	12 months
Effect	−0.0118***	−0.00934***	−0.00786**	−0.00445	−0.00459	−0.00408	−0.00361	−0.00383	−0.00347	−0.00191	−0.000313	−0.000546
	(0.00358)	(0.00337)	(0.00322)	(0.00308)	(0.00297)	(0.00286)	(0.00273)	(0.00264)	(0.00256)	(0.00248)	(0.00238)	(0.00230)
No. of unemployment spells	104,437	104,437	104,158	103,949	103,699	103,444	103,157	102,837	102,726	102,601	102,430	102,280
No. of observations	602,916	602,323	601,458	600,846	600,165	599,403	598,250	597,607	597,227	596,763	596,209	595,688
R^2	0.032	0.028	0.026	0.024	0.022	0.021	0.021	0.022	0.022	0.022	0.021	0.020
Post-reform placebo test (F-test, p-value)	0.58	0.73	0.81	0.62	0.84	0.88	0.64	0.96	0.93	0.78	0.32	0.37
Effect on the short-term unemployed (p-value)	0.91	0.85	0.75	0.94	0.94	0.77	0.89	0.87	0.73	0.50	0.44	0.39

Notes: The regressions include the same variables as the benchmark specification (Table A2 in Appendix A). The number of observations decreases when considering longer durations of employment because the unemployment spells are censored after February 2019. Huber–White standard errors robust for heteroskedasticity are in parentheses.

***and **denote statistical significance at the 1%, and 5% levels, respectively.

Table A5   The effect of the subsidy for different populations

	Job-finding rate	Job stability: remaining employed for 6 consecutive months
Benchmark specification	−0.0118***	−0.00408
	(0.00358)	(0.00286)
Population: all jobseekers	−0.0101***	−0.00521**
	(0.00269)	(0.00209)
Population: all jobseekers who worked 4 months prior to registration + jobseekers whose unemployment history is not known	−0.0110*** (0.00330)	−0.00417 (0.00265)

Notes: The regressions include the same variables as the benchmark specification (Table A2 in Appendix A). In the benchmark specification, the population consists of jobseekers who have worked 4 consecutive months prior to becoming unemployed. The population in the second regression (Row 2) includes all jobseekers regardless of the number of months worked prior to becoming unemployed. The population in the third regression (Row 3) excludes jobseekers who did not work 4 consecutive months prior to becoming unemployed but includes jobseekers whose labor market history is not known. Huber–White standard errors robust for heteroskedasticity are in parentheses.

***and **denote statistical significance at the 1% and 5% levels, respectively.

Table A6 The effect of the subsidy on the job-finding rate: robustness checks

	Censor spells when jobseekers leave the labor market	Treatment group includes jobseekers aged 44.5–48 years	Sensitivity to classification of jobseekers into pre-reform and post-reform periods			Effect by month
			Using July 1, 2016, as cutoff date	Excluding jobseekers registering in June 2016	Excluding jobseekers registering in the period April–June 2016	
Effect	−0.0125***	−0.0113***	−0.0113***	−0.0116***	−0.0115***	
	(0.00394)	(0.00347)	(0.00363)	(0.00364)	(0.00365)	
Age 45–48 years ## post-reform						
## Unemployed for 6 months						**−0.0168****
						(0.00664)
## Unemployed for 7 months						**−0.00731**
						(0.00678)
## Unemployed for 8 months						**−0.00979**
						(0.00684)
## Unemployed for 9–12 months						**−0.0262****
						(0.00292)
No. of unemployment spells	104,437	110,930	104,437	103,322	101,296	104,437
No. of observations	545,580	640,138	602,916	596,171	583,026	602,916
R^2	0.028	0.032	0.032	0.032	0.033	0.032

Notes: The regressions include the same variables as the benchmark specification (Table A2 in Appendix A). In the first regression, unemployment spells are censored when jobseekers leave the labor market. In the second regression, jobseekers aged 44.5–45 years at the time of registration at the PES are included in the treatment group. In the baseline specification, jobseekers who registered at the PES after June 1, 2016, are classified in the post-reform period. Regressions 3–6 examine the sensitivity of the results to this cutoff date by the following approaches: (1) using July 1, 2016, as the cutoff; (2) excluding jobseekers registering at the PES in June 2016 from the regression; and (3) excluding jobseekers who registered at the PES in the period April–June 2016. The last regression examines if the effect of the abolition of the subsidy depends on the unemployment duration. Huber–White standard errors robust for heteroskedasticity are in parentheses.

***and **denote statistical significance at the 1% and 5% levels, respectively.

PES, public employment service.

Table A7　The Breusch–Godfrey LM test on grouped errors

|	**Weighted OLS**
Lagged grouped error	0.0526***
	(0.0109)
Number of observations	9,742
R^2	0.021

Notes: This table reports the results of the Breusch–Godfrey LM test for autocorrelation of the grouped error terms. We first obtain the individual error terms after estimating Eq. (1) (Table A2 in Appendix A) and compute the average error by group. A group is defined by the combination of the explanatory variables included in the regression: unemployment duration (dummies), year of registration at the PES, level of education (low, medium, and high), self-reported proficiency in Dutch (three levels), country of origin (three levels), and sex. The grouped error term is then regressed on the lagged error grouped error term and the explanatory variables. Observations are weighted by the inverse of the variance of the dependent variables, which equals the number of observations in a group divided by $\hat{y}_{mk}\left(1-\hat{y}_{mk}\right)$, with $\hat{y}_{mk}$ being the predicted probability of resuming work in group *k* in month *m*.

LM, Lagrange multiplier; OLS, ordinary least squares; PES, public employment service.

Figure A1　The evolution of long-term unemployment in Flanders among jobseekers aged 40–48 years.
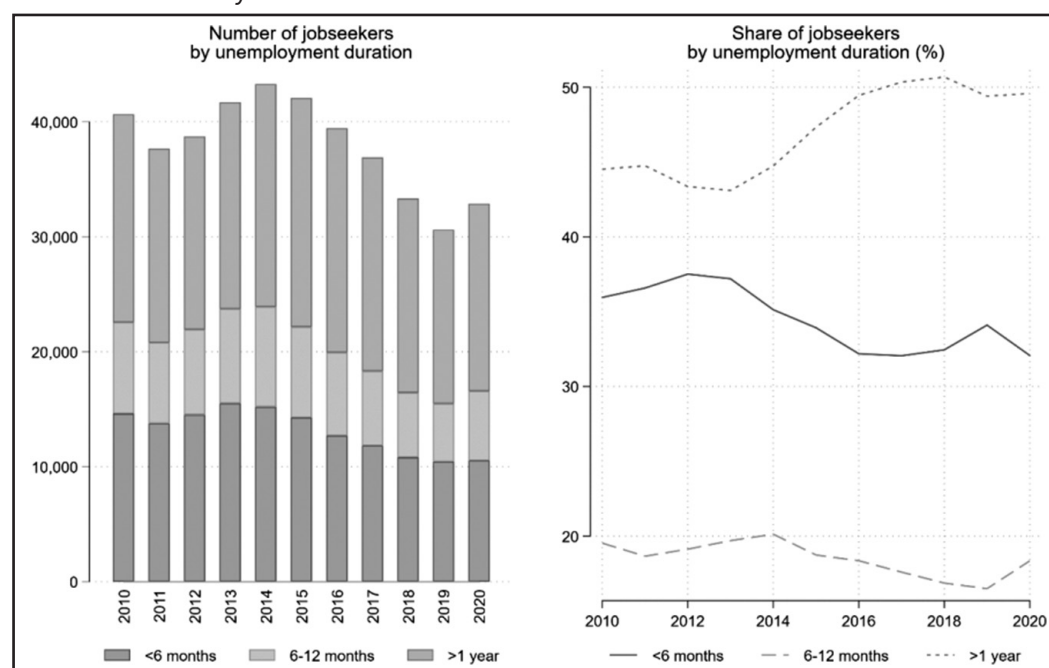

Source: Monthly average from Arvastat (https://arvastat.vdab.be/, accessed April 2021).

Figure A2   The job-finding rate of jobseekers aged 40–43 years as a function of the elapsed unemployment duration, before and after the reform.
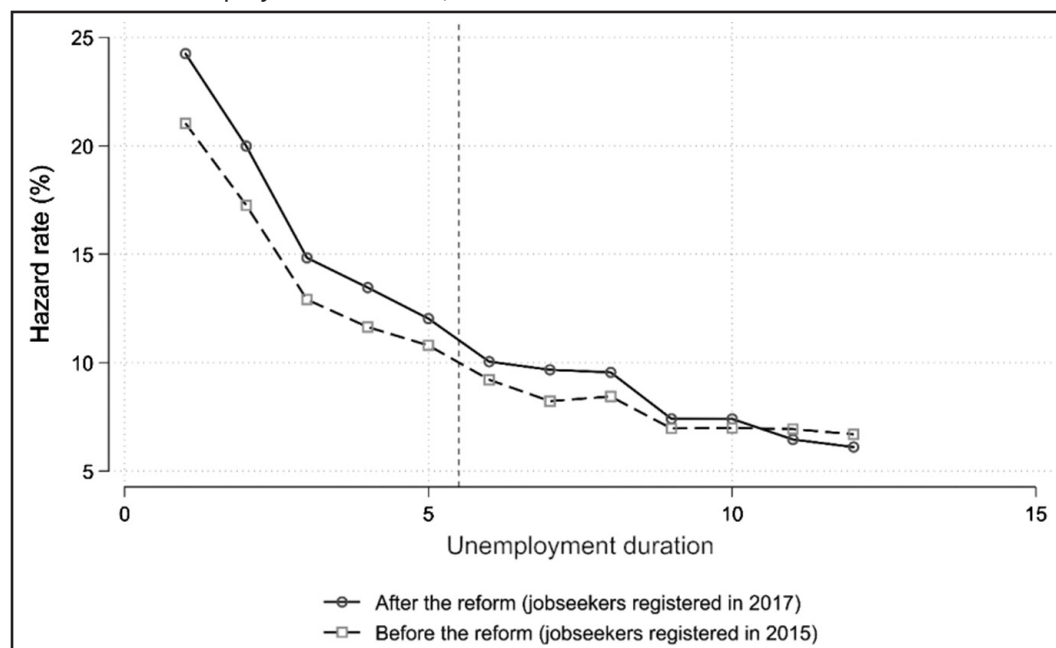
Notes: The vertical dashed line indicates the month in which jobseekers aged 45–48 years become eligible for the hiring subsidy prior to the reform.

Figure A3   The effect of the abolition of the subsidy on the probability of finding employment lasting 1–12 (consecutive) months using the more stringent definition of employment.
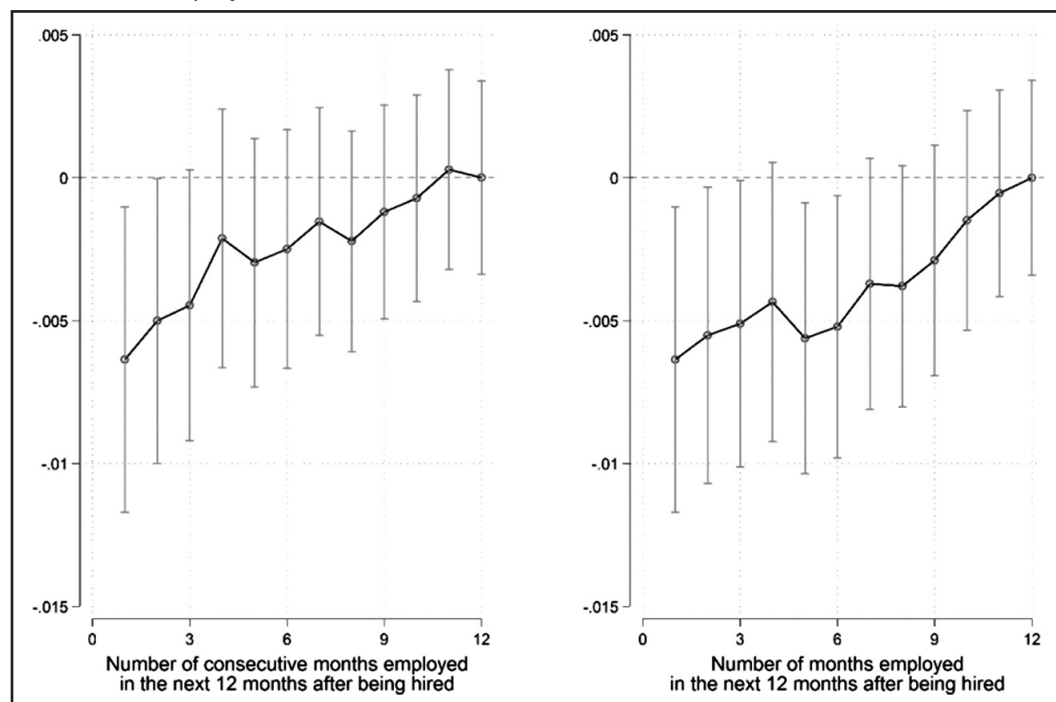
Notes: Each graph shows the triple-interaction term with its 95% CI obtained by estimating 12 triple-difference regressions following the benchmark specification as in Table 2. The outcome is an indicator taking the value of one if the jobseeker found a job and remained employed for 1–12 (consecutive) months, and zero otherwise. The definition of employment follows a more stringent definition of work, thereby excluding temporary work lasting between 10 days and 20 days per month.

Appendix B Placebo tests for difference-in-differences (DiD) specifications

This section presents placebo tests showing that DiD approaches are not valid. Two distinct DiD models can be estimated. The first DiD exploits the point that subsidy eligibility depends on the unemployment duration. It compares the job-finding rate of short-term unemployed to long-term unemployed (LTU) jobseekers aged 45–48 years (see also Figure 3 in the main text). The second DiD exploits the factor that only older jobseekers are eligible for the subsidy and compares the job-finding rate of LTU jobseekers aged 40–43 years with the job-finding rate of LTU jobseekers aged 45–48 years.

We start by exploiting differences across unemployment duration among older jobseekers. This approach shows that the job-finding rate of the LTU aged 45–48 years decreased by 3.52 percentage points after the abolition of the subsidy, relative to the short-term unemployed

Table B1   DiD exploiting differences across unemployment duration among older jobseekers and placebo tests

	DiD: short-term vs. long-term unemployed, among jobseekers aged 45–48 years	Placebo test 1: post-reform parallel trends	Placebo test 2: jobseekers aged 40–43 years
Post-reform ## long-term unemployed	−0.0352***		−0.0230***
	(0.00264)		(0.00248)
Long-term unemployed ## year (base level: 2015)			
2012		0.00626	
		(0.00444)	
2013		0.0162***	
		(0.00384)	
2014		0.00572	
		(0.00403)	
2016 Q1–Q2		−0.000136	
		(0.00548)	
2016 Q3–Q4		**−0.0194***	
		(0.00523)	
2017		**−0.0283***	
		(0.00448)	
2018		**−0.0377***	
		(0.00522)	
No. of observations	283,469	283,469	319,447
R^2	0.034	0.034	0.031
Post-reform parallel trend (*F*-test, *p*-value)		**0.012**	

Notes: The first DiD regression compares the job-finding rate between the short-term unemployed and the LTU among jobseekers aged 45–48 years, before and after the reform. The second DiD regression tests whether the job-finding rate of short-term unemployed and LTU older jobseekers evolved similarly before and after the reform. The third DiD regression compares the job-finding rate between the short-term unemployed and the LTU among jobseekers aged 40–43 years, before and after the reform. All regressions include the unemployment duration (dummies), year of registration at the PES, educational level, proficiency in Dutch, origin, sex, and age. Huber–White standard errors robust for heteroskedasticity are in parentheses.

***and **denote statistical significance at the 1% and 5% levels, respectively. The post-reform parallel trend test, shown at the bottom of Column 2, tests whether the post-reform interaction terms (printed in bold) are jointly equal, thereby testing the parallel trend in the post-reform period.

Table B2   DiD exploiting differences across age groups for the long-term unemployed and placebo tests

	DiD: young vs. older long-term unemployed jobseekers	Placebo test 1: post-reform parallel trends	Placebo test 2: short-term unemployed
Post-reform ## age 45–48 years	−0.00138		0.0101***
	(0.00245)		(0.00261)
Age 45–48 years ## year (base level: 2015)			
2012		0.00769*	
		(0.00394)	
2013		0.00220	
		(0.00346)	
2014		0.00463	
		(0.00363)	
2016 Q1–Q2		0.00444	
		(0.00495)	
2016 Q3–Q4		**0.00652**	
		(0.00473)	
2017		**0.00102**	
		(0.00407)	
2018		−**0.00195**	
		(0.00538)	
No. of observations	241,611	241,611	361,305
R^2	0.005	0.005	0.020
Post-reform parallel trend (*F*-test, *p*-value)		0.34	

Notes: The first DiD regression compares the job-finding rates between younger (40–43 years old) and older (45–48 years old) LTU jobseekers, before and after the reform. The second DiD regression tests whether the job-finding rates of the LTU in both age groups evolved similarly after the reform (i.e., *F*-test testing whether the coefficients printed in bold are equal). The third DiD regression compares the job-finding rates of short-term unemployed jobseekers in both age groups, before and after the reform. All regressions include the unemployment duration (dummies), year of registration at the PES, educational level, proficiency in Dutch, origin, sex, and age. Huber–White standard errors robust for heteroskedasticity are in parentheses.

***and *denote statistical significance at the 1% and 10% levels, respectively. The post-reform parallel trend test, shown at the bottom of Column 2, tests whether the post-reform interaction terms (printed in bold) are jointly equal, thereby testing the parallel trend in the post-reform period.

in the same age group (Table B1 in Appendix B, Regression 1). However, the two placebo tests are rejected, thereby rejecting the validity of this DiD model.

The first placebo test examines the parallel trend assumption (Table B1 in Appendix B, Regression 2). In the absence of the treatment, the job-finding rate of the short-term unemployed and LTU should have evolved similarly. This parallel trend assumption is rejected because the interaction terms between unemployment duration and the year of registration at the PES after the reform (i.e., when the subsidy was abolished, printed in bold) are not equal, implying that the job-finding rate evolved differently after the reform for the short-term unemployed vs. long-term unemployed.

The second placebo test examines the job-finding rate of younger jobseekers, a group that is not affected by the reform (Table B1 in Appendix B, Regression 3). It shows that the job-finding rate of LTU jobseekers aged 40–43 years decreased by 2.30 pp after the reform relative to the job-finding rate of the short-term unemployed in the same age group. This casts further doubt on the validity of the DiD approach. As we discuss in the main text, the job prospects of the LTU,

regardless of the age group, deteriorated after the reform. It is well-known that the job prospects of the LTU are less sensitive to the economic cycle than those of the short-term unemployed (Machin and Manning 1999). These business cycle effects are not captured by the DiD approach.

The second DiD compares the job-finding rate across age groups among LTU jobseekers. The DiD estimates indicate that the job-finding rate of the LTU aged 45–48 years decreased by 0.14 pp after the abolition of the subsidy relative to the job-finding rate of the LTU aged 40–43 years (Table B2 in Appendix B, Column 1). The first placebo test examines the parallel trend assumption, which is not rejected (Table B2 in Appendix B, Regression 2). The job-finding rate of LTU jobseekers aged 40–43 years trended similarly as the job-finding rate of LTU jobseekers aged 45–48 years after the reform. The second placebo test compares the job-finding rate of short-term unemployed jobseekers across age groups (Table B2 in Appendix B, Regression 3). The short-term unemployed should not be affected by the policy. We observe, however, that the job-finding rate of the short-term unemployed aged 45–48 years increased by 1.01 pp after the reform, relative to the job-finding rate of the short-term unemployed aged 40–43 years, suggesting that the job prospects of all older jobseekers improved after the abolition of the subsidy. This placebo test is therefore rejected.

Appendix C Review by Card *et al.* (2018)

Table C1   Studies on the effectiveness of private employment programs for the long-term unemployed included in the review by Card *et al.* (2018) (studies not included in the bibliography)

Title	Authors	Year of publication	Journal	Country
Active labor market policies in Denmark: A comparative analysis of postprogram effects	Blanche	2011	U	Denmark
Evaluating the impact of a well-targeted wage subsidy using administrative data	Cseres-Gergely, Scharle, Földessy	2012	U	Hungary
The effectiveness of targeted wage subsidies for hard-to-place workers	Jaenichen, Stephan	2011	*Applied Economics*	Germany
Evaluation of subsidized employment programs for long-term unemployed in Bulgaria	Mihaylov	2011	*Economic Studies*	Bulgaria
Do long-term unemployed workers benefit from targeted wage subsidies?	Schünemann, Lechner, Wunsch	2013	*German Econ Rev*	Germany
Employment subsidies: A fast lane from unemployment to work?	Forslund, Johansson, Lindquist	2004	U	Sweden
Income support policies for part-time workers: A stepping-stone to regular jobs? An application to young long-term unemployed women in Belgium	Cockx, Robin, Goebel	2010	U	Belgium
Dynamic treatment assignment: The consequences for evaluations using observational data	Fredriksson, Johansson	2008	U	Sweden
Do active labor market policies help unemployed workers to find and keep regular jobs?	Van Ours	2000	Book	Slovakia
The locking-in effect of subsidized jobs	Van Ours	2004	*Journal of Comparative Economics*	Slovakia

Note: "U" refers to "unpublished".