Université Catholique de Louvain (UCLouvain)
Faculté des sciences économiques, sociales, politiques et de communication (ESPO)
Economic School of Louvain (ESL)
Institut Wallon de l'Évaluation, de la Prospective et de la Statistique (IWEPS)

Youth Labour Market Integration: The Impact of Hiring Subsidies and Unemployment Benefits

Mathilde POURTOIS

October 16, 2025

Jury composition:

Prof. Muriel Dejemeppe (Supervisor, Secretary)	UCLouvain
Dr. Matthieu Delpierre (Supervisor)	IWEPS
Prof. Koen Declercq	UCLouvain
Prof. William Parienté (Chair)	UCLouvain
Prof. Sébastien Fontenay	Universitat Pompeu Fabra
Prof. Sunčica Vujić	University of Antwerp

« Les vérités sont compliquées, les clichés sont stables »
Orelsan

Acknowledgments

A PhD often feels like a solitary journey—one of deep introspection and of constantly pushing beyond oneself. But in truth, it only reaches the finish line thanks to the generous support of many people. My own journey unfolded in two acts: first at IWEPS and UCLouvain during the first four years, and then at UCLouvain Saint-Louis Bruxelles during the final two years. The length of this adventure made it especially rich in encounters—which also explains the length of these acknowledgements.

Without a doubt, the two people who contributed the most to this achievement are my supervisors, Muriel Dejemeppe and Matthieu Delpierre. Muriel, thank you for standing by me from the very first email to the final lines. Over the years, your intuition and your ability complex phenomena into words consistently impressed me. Thank you for passing on your scientific rigor, your determination to always dig deeper to understand what lies behind the data, and your attention to detail. Those are not just skills, but tools I will carry with me throughout my entire career. Matthieu, your support never wavered and your enthusiasm never dimmed—from my first day at IWEPS to today (even after I chose to pursue my career at IBSA—despite your valiant attempts to convince me to apply at IWEPS). You cheered every small achievement, which truly helped me reach the finish line. Beyond that, your readiness to challenge—and to ask me to justify—many of my choices taught me to question my decisions and own them. Your curiosity about any topic or method (and how quickly you grasp them!) has always amazed me and will remain an inspiration throughout my career.

Beyond my two supervisors, Koen Declercq—member of my supervisory committee—was also an incredible source of support and played a far greater role than the one originally assigned to him, especially during these last two years. You opened the doors of Saint-Louis to me—a peaceful refuge where I could finish my thesis—and stood by me throughout this final stretch. You always took the time to welcome me into your office to discuss my results. It was a pleasure to work with you on Chapter 2, and I am very grateful for your guidance in Chapter 3. Within my committee, I would also like to thank William Parienté. Your ability to identify the weaknesses and key issues of my work from a single presentation always impressed me. Thank you for your always constructive and helpful *feedback, and for the time and care you devoted to commenting on the private defence version of the manuscript.*

Alongside Muriel, Matthieu, Koen, and William, two other people completed my thesis jury—without whom the successful outcome of this thesis could not have been pronounced: Sébastien Fontenay and Sunčica Vujić. Thank you not only for agreeing to devote part of your time to my work, but especially for doing so with such dedication. I am truly grateful that you took the time to read each chapter in depth and to provide comments that significantly improved the quality of this final manuscript.

As mentioned above, my thesis unfolded in two acts. The first took place between Louvain-la-Neuve and Namur, shared between IRES and IWEPS. Let me begin with IRES, where I met countless talented young researchers—some of whom became friends more than colleagues. Countless beers shared, birthdays, Christmas dinners and Secret Santas, PhD apéros, a conference in Genoa with our feet practically in the water, a weekend in a castle and even a wedding: unforgettable memories. A special thanks to Chiara, one of the most wonderful people I met during my PhD, and to Nathan, whose proximity to my topic made his support especially valuable. Talking with you about our respective researches was always a pleasure—and made the journey feel a little less solitary. Thank you for that! Naming names always risks forgetting someone, but I still want to warmly thank Antoine, Arnaud, Arno, Charles, Diego, Dorothee, Elsa, Esmeralda, Florian, Francesco, François, Giulia, Janiya, Laura, Léo, Luigi, Margaux, Marion, Natalia, Silvia, Tiziano, Vincent, and many others. Each of you contributed—closely or from afar—to this success!

During my four years at IRES, I also had the chance to meet many inspiring professors. In particular, I want to extend a special thank you to Bruno Vander Linden for his insightful and helpful feedback on every single presentation I gave. Your kindness and dedication to doctoral students left a deep mark on me—thank you sincerely for that. Thanks as well to Vincent Bodart, who welcomed me into his forecasting team. The conjunctural analysis meetings were always stimulating. Initially more oriented toward microeconomics, I discovered that macro was not that bad after all! Thank you for this opportunity to learn and to work as part of a team—something I genuinely missed during the PhD. Beyond that, you were always an attentive ear: thank you!

During those first four years—interrupted by lockdowns—I also spent many days at IWEPS. The commute was fairly long, but always worth it. Thank you to all IWEPS colleagues for your warm welcome, your kindness, and your interest in my research and its progress! Each of you helped me find my place there, and I am sincerely grateful. At IWEPS, I would especially like to thank Muriel Fonder, who took the time on several occasions to support and help me in my research. IWEPS's involvement also greatly facilitated the completion of my first chapter, for which Forem data were essential. I am very grateful to the IWEPS staff who made this possible. Working with Forem was a unique learning opportunity. I also sincerely thank the Forem collaborators involved in the collaboration for their time and availability. My work was always met with genuine interest and enthusiasm, and it sparked many insightful discussions.

The final stage of my doctoral journey took place at Saint-Louis in Brussels. There, I found a true refuge—a familial atmosphere so characteristic of Saint-Louis—which allowed me to finish my thesis in real peace of mind. Thank you to the CEREC faculty members for welcoming me so warmly. You gave me the chance to complete my work in an environment I could not have hoped to be better, and I am deeply grateful for that. I want to extend special thanks to Jean-François Fagnart, Xavier Wauthy, and Gilles Grandjean—working with you as a teaching assistant was a real pleasure and taught me much more than I could have imagined. Thanks also to Tom Truyts, whose unwavering commitment to the projects he believes in—and the passion with which he drives them forward—will remain a lasting source of inspiration for me.

What made my time at Saint-Louis truly unforgettable, above all, were the fantastic colleagues I met there. I choose my words carefully when I say that you gave my PhD journey a genuine second wind. Thank you for the coffee breaks, the beers at Claire's, the karaoke nights, the lunches on the Club House terrace…Thank you Alexandre, Aron, Astrid, Audric, Callum, Carlos, Claudine, Cyril, Daniel, David, Emilia, François, Francesco, Hugues, Jean-Paul, Laure, Léa, Maxim, Rodrigo, Rory, Samuel, Soufiane, Tom—and anyone I may have forgotten. A special thanks to Audric, who convinced me that coming to Saint-Louis was an excellent idea; and to Maxim and Astrid, with whom I had the honour of sharing an office during these two years—two wonderful encounters that left a deep mark on me. Astrid, I know the *empty desk in front of you will be hard to look at… but keep going—your turn is coming!*

Last but not least, I want to thank my family and friends, who supported me throughout these years, helped me find a balance between professional and personal life, and gave me ways to escape when the data started taking up too much space in my mind. A special thank you to Denis—my partner, my rock, my best friend, and my cycling buddy. God knows it is not easy to live with a PhD student. You supported me from the very first second to the very last. Whenever I doubted myself, you never did—and I would not be here without you. You listened to every one of my presentations, drew dozens of diagrams for my slides, helped me synthesize ideas—and even solve Lagrangians at 1 a.m. on a Tuesday night. Your ability and willingness to engage with a field that is not yours always impressed me. You knew how to channel me and bring me back down to earth when I needed it most. I truly wish everyone could have a Denis in their life. I also want to thank my family—my parents, my sister Elise, and my brother-in-law Martin—for their unwavering support and encouragement. We so rarely take the time to appreciate how lucky we are to have a united family by our side: by having your back, they give you the strength to go further. Thanks also to Philippe, my godfather, never sparing with his compliment. And I cannot speak of family without mentioning my in-laws: thank you Arnaud, Bernadette, Clémence, Françoise, Julien, and Morgane. In-laws are not always a gift—but in my case, I found gold. You are the living proof that bonds of love can be just as strong as those of blood.

To all the friends who helped me escape the academic bubble over the past six years—thank you. From winter breaks in the mountains, climbing sessions and bike rides, to board game nights, long evenings spent reinventing the world, dinners with the girls of the bachelor and with the former scout staff—these moments of joy were essential. You reminded me that there was a world beyond the thesis, and for that, I am deeply grateful.

Finally, I could not end these acknowledgements without thanking my most faithful co-author: Nelson, my cat. He spent so much time with me over the past five years that he would almost deserve his name on the title page—but professionally, that's not quite acceptable. While his emotional support helped me through the low moments, his love for my keyboard was a bit more complicated to manage. If, by some misfortune, you find typos in this manuscript, do not hold it against me—I promise it was his paw.

Contents

General introduction

Entering the labour market marks a pivotal transition in the life course of every young individual. This transition can be challenging and does not necessarily follow a smooth path from school directly into stable employment. Around 10% of individuals aged 15 to 29 in European Union countries are neither in employment nor in education and training (NEET), reflecting a complete disengagement from both education and the labour market (Eurostat, 2025a). Youth unemployment rates also illustrate these difficulties: in 2024, the unemployment rate stood at about 11% among those aged 15 to 29, compared to 6% for the overall working age population (Eurostat, 2025b).

Educational attainment plays a crucial role in shaping labour market entry. Combining a low level of education with limited work experience makes integration particularly difficult: the NEET rate reaches about 11% among young Europeans with below upper secondary education, 13.5% among those with vocational upper secondary education, and only 7% among tertiary graduates. The educational gaps are even more striking in unemployment rates: among young Europeans aged 15 to 29, unemployment affects around 20% of those below upper secondary education, 10% of those with upper secondary education, and only 8% of tertiary graduates.

Economic literature has demonstrated that unemployment at the beginning of one's career has a negative impact on future employment prospects, earnings, and mental health (Bell and Blanchflower, 2011; Cockx and Ghirelli, 2016; Glatt and Wunnava, 2016; Schmillen and Umkehrer, 2017). Paying particular attention to labour market entrants is therefore crucial, as a lack of early job opportunities can leave lasting scars on subsequent labour market outcomes.

This concern has been acknowledged by many governments, which have undertaken steps to facilitate youth integration into the labour market. At the European level, the European Pillar of Social Rights Action Plan sets a target to reduce the share of individuals aged 15 to 29 who are NEET to below 9% by 2030, and the Youth Guarantee commits Member States to ensure that every person under 30 receives a quality offer of employment, training, apprenticeship, or education within four months of leaving education or becoming unemployed. At the national level, some governments have introduced employment or training programmes to facilitate the school-to-work transition. Others have also introduced youth-specific unemployment benefits to insure against early-career unemployment.

This thesis contributes to the evidence on policies supporting youth labour market integration by evaluating two Belgian interventions targeting new labour market entrants: hiring subsidies and youth-specific unemployment benefits. The thesis comprises three self-contained chapters: the first examines hiring subsidies, while the second and third evaluate two distinct reforms of youth-specific unemployment benefits.

All chapters rely on microeconometric evaluation methods—empirical strategies that identify causal effects in non-experimental settings when randomised trials are infeasible. In policy evaluation, the central challenge is selection bias: exposure to a policy is rarely random, so simple comparisons between treated and untreated individuals confound programme effects with pre-existing differences. Microeconometric designs address this by constructing credible counterfactuals from observational data, exploiting quasi-experimental variation that is plausibly as-if random—such as sharp eligibility thresholds or the timing of policy implementation. In this thesis, I employ two distinct evaluation methods. The first is a regression discontinuity design, which estimates local causal effects at an eligibility threshold under the assumption that potential outcomes evolve smoothly around this threshold. The second is a difference-in-differences design, which compares changes over time between treated and control groups, relying on the parallel trends assumption. Both methods are applied to individual-level administrative data and allow for the identification of the causal effects of the intervention or reform without confounding factors.

The first chapter examines the Impulsion plan, a regional hiring-subsidy reform introduced in Wallonia, the French-speaking region of Belgium, in July 2017. The scheme provides degressive monthly subsidies for up to three years to employers hiring unemployed job seekers, with particularly early eligibility for low-educated youths. Under this scheme, high school dropouts and graduates under age 25 become eligible respectively from their first day and after six months of unemployment, while all other job seekers qualify only after twelve months. Building on this priority treatment for young people with low education, a regression discontinuity design around the age-25 eligibility threshold identifies the causal effect of earlier access to hiring subsidies on the employment prospects of the targeted group. This chapter contributes to the literature by providing causal evidence on hiring subsidies targeting low-educated youths—a group largely neglected by previous studies mainly focusing on the long-term unemployed. Furthermore, it explores how local labour market tightness shapes the effectiveness of such policies, thereby offering insights into whether "work-first" policies can be effective under any economic conditions.

The second chapter examines whether early-career access to unemployment benefits affects nest-leaving among master's graduates. To address this, we exploit a reform of Belgium's youth-specific unemployment benefits. In Belgium, eligibility for the youth-specific unemployment benefits is less stringent than for the regular unemployment insurance benefit—notably, no prior work experience is required. The reform limited access to the youth-specific benefit to individuals entering the labour market before age 24—previously it was before age 29. After the reform, individuals aged 24 or older must satisfy the stricter eligibility conditions of the regular unemployment insurance scheme. At the time, the average age at which young people left the parental home in Belgium was about 25. Financial resources are a key determinant of nest-leaving; lower resources are consistently associated with delayed departure. Because the reform reduced the expected financial resources in case of unemployment for youths about to decide whether to leave the parental home, it is expected to delay their departure. Using a difference-in-differences design, this chapter contributes to the limited empirical literature on the role of state-provided financial resources in shaping youths' housing autonomy, as well as on gender-specific responses to financial constraints in nest-leaving decisions.

The third chapter focuses on the same youth-specific unemployment benefits system as the second chapter, but examines a different policy reform that tightened eligibility for low-educated individuals. As mentioned above, the Belgian youth-specific unemployment benefit is not conditioned on prior work experience. Rather, it hinges on the completion of a one-year compulsory job search period upon entering the labour market. The reform studied in the third chapter effectively extended the duration of the compulsory job search period prior to benefit entitlement for high school dropouts only. Remaining without benefits for a longer period strengthens their financial incentives to work. This chapter analyses how extending the compulsory job search period before granting unemployment benefits affects employment prospects among low-educated labour market entrants. According to standard job search theory, such a reform should incentivise young people to find a job more quickly. However, it may also reduce the quality of job matches, as individuals are more likely to accept lower-quality positions earlier in the search process in order to avoid prolonged unemployment. Using a difference-in-differences design, the chapter contributes to the literature on unemployment insurance providing benefits only after a no-benefit period by focusing on high school dropouts—an understudied group in this context. Another important contribution lies in its gender perspective, highlighting that seemingly gender-neutral unemployment policies may generate gender-differentiated outcomes

Taking everything together, the thesis places a particular emphasis on low-educated youths—who are the focus of two out of the three chapters (chapters 1 and 3). Studying them is interesting from a policy perspective, as their entry into the labour market is especially challenging. The thesis analyses how, at the start of their careers, early access to hiring subsidies and later eligibility for unemployment benefits shape their employment prospects. Belgium is a country where the unemployment rate of individuals with little schooling is relatively high: in 2024, among young people aged 15 to 29 with below upper secondary education, it reaches 25% compared to 20% on average in Europe. This makes Belgium a particularly suitable context for studying these questions. The remaining chapter (chapter 2) shifts the focus to master's graduates, for whom labour market integration is less challenging. For them, the outcome of interest is not employment prospects but housing decisions, in a context where young Europeans are leaving the nest later and later. Because delayed nest-leaving has been linked to poorer mental health, lower self-esteem, postponed fertility, and reduced labour supply (Angelini et al., 2022; Seiffge-Krenke, 2013), understanding its understudied determinants is particularly important.

The thesis also places a particular emphasis on gender, thereby contributing to the growing literature on the gendered effects of the welfare state. Two of the three chapters explicitly measure gender heterogeneity by estimating separate effects for men and women. This focus is motivated by the fact that preferences and opportunities at labour market entry often differ by gender, so formally identical policies can yield gender-differentiated outcomes.

Although this thesis focuses exclusively on policies implemented in Belgium, particular attention has been paid throughout the three chapters to producing findings of broader relevance—results that contribute to the scientific literature and hold interest beyond the Belgian context. In addition to these contributions to the economic literature, the thesis adopts a deliberately policy-oriented perspective. The analyses are conceived as applied research that provides science-based empirical evidence on the effectiveness (chapters 1 and 3) or unintended effects (chapter 2) of policies, thereby offering insights that are useful to decision-makers. Motivated by a strong personal commitment to conducting applied research on socially relevant and policy-salient issues, this thesis seeks not only to advance academic knowledge but also to inform public debate. Accordingly, the general conclusion translates the empirical evidence into policy recommendations.

3 Does delaying unemployment benefits improve employment for low-educated youth? The role of gender and part-time work

Access to unemployment benefits (UB) at an early career stage may help smooth the school-to-work transition but may also reduce job search incentives. To mitigate these disincentives, UB access may be delayed through a compulsory job search period. This paper analyses the impact of extending this period on employment outcomes for Belgian labour market entrants without a high school diploma. Using a difference-in-differences strategy, the analysis reveals that a longer job search period increases employment up to three years after labour market entry. The effect is concentrated among women. As they are more likely to work part-time, the increase essentially concerns part-time employment.

3.1. Introduction

Across Europe, the integration of low-educated youths into the labour market remains a major policy challenge. In 2023, 15% of the population aged 25-34 did not complete upper secondary education (Eurostat, 2025e). Like all young people, low-educated youths have minimal work experience, but they also have very limited skills, making their school-to-work transition particularly challenging. Consequently, their unemployment rate is substantially above the average, around 20% across Europe (Eurostat, 2025f).

The low-educated population consists at the same time of youths who leave general education without a diploma and thus enter the labour market without any vocational qualification and of youths who started but did not complete a vocational track. Some of them lack marketable skills and could be oriented toward training; others have experienced repeated school failure and are reluctant to return to any education or training. For the latter, on-the-job learning via employment supported by hiring subsidies is a potential solution, but empirical evidence suggests limited effectiveness (Albanese et al., 2024; Dejemeppe et al., 2025).[68] From a policy perspective, the priority is to channel these low-educated youths either into training or into employment to limit the scarring effects of early-career unemployment.

In case of prolonged unemployment, some countries provide unemployment benefits (UB) specific to young job seekers. Whereas regular UB hinge on sufficient prior contributions—often out of reach for new entrants—youth-specific UB lower eligibility requirements. Although UB alleviate financial pressure during job search, they may also discourage active job search or entry into training, thereby further increasing unemployment duration. To mitigate these risks, some countries require job seekers to complete a compulsory job search period before being entitled to UB, thereby creating an UB scheme where

[68] France, Belgium, Italy, Denmark, and Spain all provide subsidies targeted to youths with a low level of education.

benefits are granted only after a no-benefit period.[69] This paper examines how extending such a compulsory job search period affects work incentives, focusing on low-educated individuals at the early stages of their careers. Belgium provides an interesting setting to investigate this question. In 2015, the country reformed its youth-specific UB by effectively extending the length of the compulsory job search period for young labour market entrants with low educational attainment. This reform provides a unique setting to investigate the reaction of high school dropouts—a population at high risk of unemployment and that many European countries aim to support through targeted policies—to changes in work incentives. To preserve brevity, effects on training incentives are not developed in this paper.[70]

The Belgian youth-specific UB, known as the activation allowance, can be claimed one year after entering the labour market if the job seeker remains unemployed. During this one-year period, hereafter referred to as the qualifying period, individuals must demonstrate active job search. Afterwards, they may receive the activation allowance for up to three years. In 2015, the Belgian federal government tightened eligibility criteria for young school leavers without a high school diploma. Since then, they can no longer claim the activation allowance before the age of 21. For high school dropouts entering the labour market before the age of 20, this effectively extended the qualifying period. Indeed, upon completing their one-year qualifying period, they are still under the age of 21 and must wait until reaching 21 to claim the allowance.

In UB schemes of this type—where eligibility is granted only after an initial period without financial support—standard job search theory predicts that job search effort is at its highest level at the beginning of the qualifying period and gradually declines over time as individuals anticipate benefit entitlement (Mortensen, 1977). When the qualifying period is extended, the theory predicts that individuals sustain higher search effort throughout the qualifying period, leading to an increased job-finding rate. Empirical evidence on this theoretical prediction remains limited and mixed. Von Buxhoeveden (2019) finds that

[69] This the case in Luxembourg, Denmark, Sweden and Belgium. Among others, Denmark, France and Germany provides training scheme targeting low-educated youths.
[70] This omission does not reflect a lack of relevance of this issue but stems from the insignificance of the estimated effects.

immediate eligibility for UB reduces the job-finding rate of Swedish high school graduates. Bolhaar et al. (2019) focus on a compulsory job search period for welfare benefits rather than for UB, but reach a similar conclusion. Introducing a compulsory job search period increases job-finding rates and leads to a lasting reduction in welfare take-up. According to Bolhaar et al. (2019) and von Buxhoeveden (2019), a longer qualifying period should increase transitions into employment. Cockx and Van Belle (2019) yield contrasting evidence. They analyse a previous extension of the qualifying period for the activation allowance, which applied to all youths rather than being specifically targeted at the low-educated.[71] They find no impact on the job-finding rate and even report a decrease in the cumulative number of days worked. Their results indicate that the extended qualifying period induces job seekers to accept more short-term job offers at the expense of longer ones.

The divergence in findings may stem from differences in the populations analysed. While von Buxhoeveden (2019) studies young high school graduates, Bolhaar et al. (2019) examine a population of welfare applicants—25% of whom are under 25 and 72% of whom have no more than a high school degree. By contrast, Cockx and Van Belle (2019) focus on bachelor and master graduates. This paper instead considers very young individuals with low educational attainment. A major distinction across these groups lies in the liquidity constraint they face. It has been shown to play a key role in shaping responses to changes in UB, both theoretically (Chetty, 2008) and empirically (Basten et al., 2014; Landais, 2015).[72] The youths analysed here—low-educated youths from relatively poor families—are likely to face high liquidity constraints, comparable to the populations in von Buxhoeveden (2019) and Bolhaar et al. (2019). Accordingly, I expect my findings to align more closely with theirs. In particular, I anticipate that these youths will display stronger employment responses than the university graduates of Cockx and Van Belle (2019), who typically come from wealthier families better able to support them in case of unemployment.

---

[71] They study the scheme for school leavers as studied in this paper but focusing on a previous extension of the qualifying period from 9 to 12 months implemented in 2012.

[72] More generous benefits tend to prolong more unemployment spells among liquidity-constrained groups.

Beyond the impact on employment levels, the findings of Cockx and Van Belle (2019) raise the important issue of the quality of jobs accepted. The broad literature on UB generosity, while not directly addressing UB granted after a compulsory job search period, provides valuable insights on this question. Standard job search theory (Mortensen, 1977) predicts that reducing UB generosity decreases job seekers' reservation wages, leading them to lower their job acceptance criteria. Consequently, the quality of job matches declines, since individuals are more likely to accept lower-quality positions earlier in the search process to avoid prolonged unemployment. However, empirical evidence is mixed (see Le Barbanchon (2016) for a detailed discussion of the literature). Studying an extension of UB duration for individuals with low employability, Le Barbanchon (2016) finds longer unemployment spells but no effect on subsequent employment duration or wages, arguing that job seekers with very limited opportunities respond mainly by reducing search effort rather than improving match quality. Turning to UB granted after a compulsory job search period, von Buxhoeveden (2019) reports similar findings for high school graduates: immediate UB eligibility has no impact on entry-level wages, implying that lower job-finding rates result from reduced search intensity rather than increased selectivity. By contrast, Cockx and Van Belle (2019) conclude that extending the qualifying period leads to greater acceptance of short-term contracts among university graduates, suggesting lower acceptance standards. I expect the behaviour of high school dropouts in my sample to be closer to that observed by Le Barbanchon (2016) and von Buxhoeveden (2019).

The main contribution of this paper is to examine the impact of UB provided only after a no-benefit qualifying period on both employment and job quality, with a specific focus on high school dropouts. As discussed above, existing evidence on this topic is limited, mixed, and appears to be dependent on the population studied. High school dropouts have not yet been investigated in this context. Their difficult labour market integration and weak attachment to employment make them a particularly relevant group from a policy perspective. A second important contribution lies in the gender perspective of the paper, which adds to the literature highlighting that seemingly gender-neutral UB policies may generate gender-differentiated outcomes.

This gender perspective builds on a growing literature revealing systematic gender differences in job search behaviour, with potential implications for how men and women respond to unemployment benefits and related reforms. Relative to men, women tend to set lower reservation wages (Caliendo et al., 2017; Cortés et al., 2023; Le Barbanchon et al., 2021) and to value non-wage job attributes more strongly, such as schedule flexibility, job stability, shorter commutes, and less competitive environments (Basbug and Fernandez, 2025; Eriksson and Lagerström, 2012; Goldin, 2014; Wiswall and Zafar, 2018). Men often display higher reservation wages, lower risk aversion, and greater overconfidence about future earnings (Cortés et al., 2023). These differences shape search intensity and job acceptance decisions, explaining part of the gender wage gap and gendered occupational sorting. In the context of UB reforms, they imply that men and women may react differently to an identical reform, warranting a gender heterogeneity analysis. Given their higher risk aversion, I expect women to exhibit stronger employment responses than men to an extension of the qualifying period. In particular, I anticipate that their preference for flexible schedules will increase their likelihood of accepting part-time or short-term contracts. The gendered occupational sorting resulting from the differences mentioned above may also generate gender disparities in effects, if one gender disproportionately applies to sectors or occupations with more vacancies.

My empirical analysis draws on a difference-in-differences strategy exploiting the specific timing of the reform and the new age-eligibility criteria. I compare employment outcomes of high school dropouts subject to an extended qualifying period (treatment group) to those of high school dropouts for whom the qualifying period remained unchanged (control group) before and after the reform. The analysis is based on Belgian individual social security data, which provide information on the receipt of the activation allowance as well as the employment status, the number of days worked, and the type of job occupied. The impact on employment is assessed using both the job-finding and the employment rates. The former captures how quickly individuals transition into jobs, while the latter reflects the overall share of individuals employed at the end of a specific quarter. The analysis of employment is complemented by the cumulative number of days worked. In contrast to the job-finding and employment rates, this latter measure better captures short and unstable jobs typically held by high school dropouts. To provide additional insight on the quality of jobs accepted, I analyse employment by job type, distinguishing between short-term, part-time, and full-time positions.[73] Short-term jobs refer to temporary contracts facilitated through temporary work agencies. An increase in such contracts, as well as a rise in part-time contracts, is interpreted as evidence of reduced job acceptance standards.[74]

My main findings indicate that the extension of the qualifying period leads to higher job-finding and employment rates, as well as an increased number of days worked over a three-year horizon after first registration at the Public Employment Service (PES). These findings are consistent with the standard job search theory—which predicts an intensification of job search effort—as wells as with Bolhaar et al. (2019) and von Buxhoeveden (2019). Regarding the type of job accepted, I find that the employment effect appears to be largely driven by an increase in part-time employment. However, the gender heterogeneity analysis shows that women are particularly responsive to the extension of the qualifying period, whereas men are not. These results suggest that the observed increase in part-time employment does not result from the fact that individuals have lowered their job acceptance criteria. Rather, it reflects the characteristics of those who adjust their behaviour in response to the extended qualifying period—specifically, women, who were already more likely to accept part-time work prior to the reform. Altogether, these findings suggest that extending the qualifying period improved labour market outcomes among high school dropouts, but that the gains are concentrated among women and are therefore mainly realised in part-time employment. The data point to two potential mechanisms behind the gender-differentiated employment effect: differences in household composition and sectoral composition.

[73] While earnings are often used to assess job quality, they are less suitable here due to the limited wage distribution among low-educated youths and the high minimum wage in Belgium. Results of the analysis on wages are available upon request.

[74] While part-time work may, in some cases, reflect lifestyle preferences, this explanation is less relevant for younger individuals than for the general population. According to the Labour Force Survey, across all age groups, 10% of part-time workers report working part-time because they could not find a full-time job. This share increases markedly to 30% among individuals under 25 who are no longer in education, suggesting that, for this group, part-time employment is more often a constrained choice.

The paper is organised as follows. The next section summarises the institutional background. Section 3.3 describes the empirical strategy. Data, descriptive statistics on the evaluation sample, and the outcome variables are presented in Section 3.4. Section 3.5 presents and discusses the empirical findings. Some robustness analyses are reported in Section 3.6. The last section concludes.

3.2. Institutional context

In Belgium, school is compulsory until the age of 18. After this, young people may either continue their education or enter the labour market. For labour market entrants who remain unemployed after one year, a youth-specific unemployment insurance scheme—the activation allowance—is available. In contrast to regular UB, which require at least one year of full-time employment to qualify, the activation allowance is designed for young people and is characterized by less stringent eligibility criteria. Eligibility for these non-means-tested UB requires the completion of a one-year qualifying period. This period begins at labour market entry defined as the first registration at the Public Employment Service (PES) or the start of the first employment spell, whichever occurs earlier. There are no specific work requirements during this qualifying period, but eligibility at its end hinges on active job search. Job search efforts are monitored twice, in the 7th and 11th month of the qualifying period.

The activation allowance is a flat-rate benefit whose monthly amount is determined by household composition and age. Most beneficiaries are young individuals living with their parents and therefore considered as cohabitants, receiving €425 per month regardless of their age.[75] Those living alone receive €493 per month if they are below the age of 20 and €818 if they are older. Heads of household—accounting for a minority of beneficiaries—receive €1,105 monthly.[76] The activation allowance can

[75] In the evaluation sample, in the year before the reform, 80% of beneficiaries were living with their parents.
[76] In the evaluation sample, in the year preceding the reform, heads of household accounted for less than 1% of beneficiaries.

be granted to cohabitants for up to three years. For heads of household or individuals living alone, the benefit can be extended beyond three years until they reach their 33rd birthday.[77]

Before the 2015 reform, educational requirements for the activation allowance were low. High school dropouts in technical, artistic, or vocational education became eligible upon completion of the first three years of high school. Those in the general track were required to complete—but not necessarily required to pass—the last year of high school. In the year preceding the reform (September 2013-August 2014), 32,191 school leavers received the activation allowance for the first time, of whom at least 30% were high school dropouts (ONEM, 2014).[78]

In late December 2014, the federal government decided to tighten eligibility criteria for high school dropouts by introducing a minimum age of 21 to claim the activation allowance. Consequently, high school dropouts entering the labour market before age 20—and who would complete the one-year qualifying period before turning 21—face an effectively longer qualifying period. They must wait until age 21 to claim the activation allowance. When doing so, compliance with job search requirements is reassessed. The minimum-age criterion applies exclusively to high school dropouts; no minimum age is imposed on individuals who have completed high school.

The extension of the qualifying period depends on age at labour market entry: the younger the individual, the longer the extension. For example, a high school dropout who register for the first time at the PES aged 19 years and 10 months will be 20 years and 10 months at the end of the one-year qualifying period and must wait an additional two months to become eligible. Similarly, a high school dropout registering at age 19 faces a 12-month extension of the qualifying period. Since schooling is compulsory until 18 in Belgium, the maximum extension of the qualifying period implied by the reform is 24 months. The theoretical age at which young Belgians complete high school ranges between 17 and 19,

[77] The baseline benefit period can also be extended in some cases, such as returning to work or resuming education.

[78] This percentage is likely underestimated, as statistics from the National Employment Office report a significant number of individuals classified in the 'unknown' education category (around 20%). Some of them are likely to be high school dropouts.

depending on the track. However, grade repetition is prevalent in Belgium: at the time of the reform, 34% of students enrolled in the final two years of high school had already repeated at least one grade (OECD, 2025). Therefore, it is relatively common for students to drop out at an age older than 18.

The reform was unexpectedly announced in late December 2014 and formalized by law on January 1, 2015. Implementation was scheduled for the end of the ongoing school year, in September 2015. Although the tightening of eligibility criteria for the activation allowance had been planned in the federal government agreement of October 2014, it received little prior media attention. Moreover, the timing of the decision and the exact new age criterion came as a complete surprise. Accordingly, it is unlikely that this reform had been anticipated before late December 2014. From January 2015, however, labour market entrants could anticipate the extension of their qualifying period. This potential anticipation is further discussed in Section 3.3, which describes the empirical strategy.

If not eligible for the activation allowance, unemployed high school dropouts may access means-tested welfare benefits under certain conditions. These benefits aim to support individuals with insufficient financial resources and who cannot obtain them through personal effort or other means—for instance, because they remain in education or are ineligible for other benefits such as the activation allowance or regular UB. The determination of 'insufficient resources' is made by a social worker through a comprehensive assessment of the financial situation of the household where the claimant resides. This assessment considers both the financial means of the youth and those of the parents if the claimant lives in the parental home. Welfare support is a flat-rate benefit: €556 per month for cohabitants, €834 for single individuals, and €1,111 for heads of household in 2015. This is slightly higher than the activation allowance. Individuals eligible for welfare support—most likely because they live alone or because their parents have very limited financial resources—would not experience any change in financial means following the reform of the activation allowance. If they were receiving welfare support in the initial one-year qualifying period, they will most likely continue to benefit from it during the extension of their qualifying period. Consequently, since their financial situation remains essentially unchanged, they are unlikely to respond to the reform, as their incentives to work have not been modified.

3.3. Empirical strategy

The impact of the reform is evaluated using a difference-in-differences design comparing high school dropouts subject to an extended qualifying period (treatment group) to high school dropouts for whom the length of the qualifying period remains unchanged (control group), before and after the reform.

Defining the treatment and control groups requires two preliminary remarks regarding the selection of the evaluation sample. First, due to data limitations, the age at the start of the qualifying period cannot be determined for individuals whose qualifying period starts with an employment spell. A methodological concern follows as the extension of the qualifying period (the treatment) depends on age at labour market entry, measured by the first PES registration or the start of the first employment spell. To address this, the evaluation sample is restricted to individuals with less than one month of work experience before their first PES registration. For these individuals, the starting month of the qualifying period coincides with the month of first registration at the PES. Hence, I can accurately measure their age (in months) at the start of the qualifying period and identify whether they are treated. Second, to focus on individuals most likely to be affected by the reform, the sample is restricted to those who were not receiving means-tested welfare benefits at the time of their first registration at the PES. Data show that individuals who begin the qualifying period on means-tested welfare generally remain on it throughout the qualifying period. For them, an extended qualifying period would likely have little effect, since their financial incentives to work remain unchanged.[79]

Based on this selection, the control group consists of high school dropouts who were 20 years old at their first registration at the PES. Their

[79] According to my data, 72% of individuals benefiting from means-tested welfare benefits in the quarter before first registration were still receiving them one year later.

qualifying period is unaffected by the reform: one year later they reach 21 and can immediately claim the activation allowance.[80] The treatment group includes high school dropouts who register for the first time at the PES before age 20. After one year they are still under 21 and must wait until their 21st birthday, which extends their qualifying period. In the benchmark specification, treatment is defined as an extension of at least 6 months, implying a qualifying period of at least 18 months. The treatment group is therefore composed of individuals aged between 18 and 19 years and 6 months at their first PES registration. This definition ensures a notable difference in the qualifying period between the treatment and control groups.

A robustness analysis explores two alternative treatment definitions based on subgroups of the initial treatment group: individuals aged 19 years to 19 years and 6 months and those aged 18 at first PES registration. These alternative definitions allow me to compare employment responses to different treatment intensities. The older subgroup faces an extension of 6 to 12 months, whereas the younger subgroup faces an extension of 13 to 24 months. This robustness analysis is presented in Section 3.6.2.

September 2014 marks the beginning of the post-reform period. Individuals who started their qualifying period before September 2014 belong to the pre-reform cohort, while those who started afterwards are in the post-reform cohort. The post-reform period spans from September 2014 to August 2015, the last month before the actual implementation of the reform. Hence, the analysis focuses on individuals who began their qualifying period under the old rules but completed it under the new eligibility conditions. From January 2015 onwards, treated individuals may have anticipated the forthcoming extension of the qualifying period. Such anticipation could have incentivized some youths to remain in education or complete high school, in order to avoid the new minimum age criterion. This type of behavioural response could generate a

[80] Since age is measured in months, I exclude individuals who turned 20 during the month of first registration at the PES from the evaluation sample. At the end of their qualifying period, I do not know whether they already turned 21, and thus I cannot determine if they belong to the treatment or control group. Therefore, my control group consists of individuals aged between 20 years and 1 month and 20 years and 11 months at first registration at the PES.

compositional bias in the sample. However, Cockx et al. (2023) show that the reform did not lead to an increase in degree completion rates among students enrolled in high school in 2014–2015, which alleviates concerns about compositional bias in the sample. Extending the post-reform period beyond September 2015 would be inappropriate, since the absence of compositional changes among treated individuals cannot be guaranteed thereafter.

To ensure comparability across different periods, the pre-reform period spans from September 2012 to August 2014, and is further divided into two sub-periods. Pre-reform period 1 covers first registrations between September 2012 and August 2013, and pre-reform period 2 covers September 2013 to August 2014.

To identify the causal impact of extending the qualifying period by at least 6 months, I estimate a standard difference-in-differences model with an absorbing treatment and a non-staggered adoption. The estimated model is as follows:

$$Y_{itm} = \alpha + \gamma\, age_{itm} + \beta_1 T_{pre} + \beta_{post} T_{post} + \delta_1 D_i T_{pre1} + \delta_{post} D_i T_{post} + \sigma X_i + \varepsilon_{it} \quad (1)$$

Where Y_{itm} denotes the outcome of interest of high school dropout *i*, who started the qualifying period in month *m* of year *t*. age_{itm} is the age in months measured at the start of the qualifying period, which is used to define treatment. The treatment indicator D_i equals one for individuals in the treatment group (aged below 19 years and 6 months) and zero for those in the control group (aged 20). T_{pre1} and T_{post} are period indicators referring to pre-reform period 1 and the post-reform period, respectively. Pre-reform period 2 is the reference period. The coefficient of the interaction term between the post-reform indicator and the treatment indicator measures the average causal impact of the reform on the treatment group. Because not all high school dropouts claim the activation allowance at the end of their qualifying period and therefore actually experience the longer qualifying period, this coefficient should be interpreted as an Intention-to-Treat (ITT) effect. The coefficient of the interaction term between the pre-reform indicator and the treatment indicator tests the parallel trends assumption. A statistically significant coefficient would indicate that the treatment and control groups did not evolve in a similar way across the two pre-reform periods. Finally, X_i is a vector of pre-determined socio-demographic controls containing all the variables presented in Table A1 in Appendix A.

The main specification is a standard binary difference-in-differences design, with a treatment indicator equal to one for any individual whose qualifying period is extended by at least 6 months, regardless of the extension. The nature of the policy reform—increasing the qualifying period from 1 to 24 months depending on the age at labour market entry—creates a variation in treatment intensity that could have motivated the implementation of a continuous difference-in-differences design, similar to De Groot and Van der Klaauw (2019). Adopting their methodology, my treatment indicator in Equation (1) would be a discrete variable taking values 1 to 24, and the equation would include fixed effects for each level of treatment.

Although this design would leverage the variation in treatment intensity induced by the reform, it is not presented as the main specification due to the greater complexity in interpreting the resulting coefficients, the stronger parallel trends assumptions required for identification of treatment effects without selection bias, and the sensitivity of the estimator to the distribution of treatment intensity, as highlighted by Callaway et al. (2024).[81] Instead, treatment intensity is explored in the robustness analysis of Section 3.6.2, which distinguishes between extensions of 6 to 12 months and 13 to 24 months. This approach yields coefficients that are easier to interpret and is better suited to the characteristics of the evaluation sample, particularly the small size of the control group (see Section 3.4).

[81] Callaway et al. (2024) show that in a continuous treatment setting, the coefficient from a two-way fixed effects (TWFE) regression does not straightforwardly capture the average causal effect of a one-unit increase in treatment intensity. Identification without selection bias requires stronger parallel trends assumptions than in the binary case, which are generally difficult to test. Even under these stronger assumptions, the TWFE estimator produces a weighted average of treatment effects across different treatment intensities, with weights determined by the OLS estimation procedure rather than reflecting the actual distribution of treatment intensity. This weighting scheme further complicates the interpretation of the estimated causal effect in case of heterogeneous treatment effects. Moreover, they show that the TWFE estimator is undesirably sensitive to the size of the control group, a particular concern in the present setting.

3.4. Data

This paper relies on Belgian individual-level administrative data from the Cross Roads Bank of Social Security (CBSS).[82] The dataset provides a variety of socio-demographic information recorded either at the time of PES registration or on an annual basis.[83] Additionally, it gives quarterly information on the economic status—salaried employment, self-employment, unemployment, or disability—as well as various social benefits, such as the activation allowance and means-tested welfare benefits.

As explained in Section 3.3, the evaluation sample consists of high school dropouts who registered for the first time at the PES between September 2012 and August 2015, either aged 18 to 19 years and 6 months (treatment group) or aged 20 (control group). The evaluation sample is restricted to individuals with negligible work experience who were not receiving means-tested welfare benefits at the time of first registration at the PES. This selection results in a relatively small evaluation sample of 8,946 individuals.[84] The exact number of individuals in the treatment and control groups, both before and after the reform, is reported in Table A1 in Appendix A. The limited size of the evaluation sample is primarily driven by the small number of individuals in the control group, reflecting the fact that fewer high school dropouts register at the PES for the first time at age 20 or later. Combined with the restriction of the post-reform period to a single year, this results in a post-reform control group of slightly less than 700 individuals. Although the small size of the evaluation sample does not prevent the estimation of treatment effects, it should be kept in mind when analysing the results and their confidence intervals.

---

[82] The CBSS is a Belgian institution gathering individual data from all social security institutions.

[83] The dataset provides information on gender, nationality, migration background, measured as of January 1 each year, and on household composition and size, measured as of December 31 each year. The dataset also includes information on region of residence and type of educational track, recorded at first registration at the PES, as well as annual household income, calculated based on income observed in June and extrapolated over the full year.

[84] Including welfare beneficiaries at first PES registration would increase the sample by 1,241 individuals.

3.4.1. Descriptive statistics

Table A1 in Appendix A presents descriptive statistics by age group (treatment vs. control) before and after the reform. The sample consists mainly of Belgian nationals but is also characterized by a significant migration background: about 50% have at least one non-Belgian parent and roughly 60% at least one non-Belgian grandparent. These shares are well above the population benchmarks. National statistics between 2012 and 2015 indicate that only about 10% of Belgian nationals had at least one non-Belgian parent, while approximately 17% of all Belgian residents had a migration background—defined as either holding a nationality other than Belgian or having at least one non-Belgian parent (Statbel, 2025b). This overrepresentation is strongly associated with the low educational attainment of the sample. The vast majority enter the labour market from vocational or technical track. Most individuals (80%) are children living with their parents on December 31 of the year preceding their first registration at the PES.[85] On average, individuals in the sample live in four-person households with an annual gross household income of €46,000. Household income aggregates all income sources within the household. Given that most households comprise two parents and two children, a simple calculation implies roughly €1,900 in gross earnings per parent per month (assuming equal earnings across parents). For comparison—bearing in mind that my income measure includes non-wage sources—this amount is well below the Belgian mean gross monthly salary in 2015.[86]

---

[85] Despite the exclusion of means-tested welfare beneficiaries, the evaluation sample still includes 6% of single individuals. This proportion may appear high, given that single individuals are typically more likely to qualify for welfare benefits. This apparent inconsistency can be explained by two main factors. First, household composition is measured as of December 31, while welfare benefit receipt is assessed at PES registration. In between, some individuals may no longer be single—they might have moved back in with their parents or started living with a partner. If the household has sufficient financial resources, these individuals become ineligible for welfare benefits despite initially being classified as single. Second, some eligible individuals may simply not apply for welfare benefits. Welfare programs are known to suffer from high non-take-up rates, driven by factors such as administrative complexity and the stigma associated with being a welfare recipient.

[86] The mean gross monthly salary in Belgium in 2015 was about €3,500 (Statbel, 2025c).

Table A1 highlights several differences between treatment and control groups. By construction, the average age in the treatment group is lower than in the control group. In the treatment group, the average age is 19. This means that the qualifying period is extended on average by 12 months, leading to a two-year qualifying period. The control group (older age group) exhibits a higher share of individuals with a migration background, a lower share residing in Flanders, and higher shares in Brussels and Wallonia.[87] Moreover, individuals in the older cohort are more likely to have followed a general education track and less likely to first register at the PES at the end of a school year (July-September). These differences do not threaten identification provided that they remain constant before and after the reform. To assess this, I estimate the benchmark specification of Equation (1) using the pre-determined characteristics in Table A1 as outcomes. As shown in the last column of Table A1, there are small differences for a few variables. In particular, the probability of first registering at the end of a school year decreases for the control group but remains stable for the treatment group—a difference significant at the 1% level. Statistically significant differences also appear in regional composition, in the shares of individuals living alone or in house-sharing, and in the share with a non-EU nationality. Overall, magnitudes are small and the latter two are significant only at the 10% level. To ensure that these compositional changes do not affect my conclusions, I implement a weighted difference-in-differences strategy as a robustness check in Section 3.6.1.

3.4.2. Outcome variables

The analysis considers two sets of outcomes: benefit of the activation allowance and employment. They are observed monthly or quarterly over the three years following first registration at the PES, allowing the dynamics during and after the extension of the qualifying period to be tracked.

---

[87] The lower share of individuals residing in Flanders in the older age group is consistent with the lower incidence of school dropout and grade repetition in this region, compared to Brussels and Wallonia.

The outcome for the benefit of the activation allowance is a monthly indicator equal to one if the benefit is received in a given month between months 1 and 36. Treatment effects based on this variable measure the proportion of the treatment group who effectively fail to obtain the activation allowance due to the extension of the qualifying period. Since not all treated youths are at risk of long-term unemployment, this proportion reflects the actual risk of losing eligibility for the activation allowance, and by implication the subgroup most likely to adjust their behaviour in response to the reform.

Employment is recorded in three ways. First, the cumulative job-finding rate is defined as a binary variable equal to one if the individual transitions to employment at least once by a given quarter. Second, the employment rate records whether the individual is employed at the end of each quarter. Adding this second measure to the first provide a clearer picture of the impact of the reform on employment stability. Finally, to account for the frequent churning among young labour market entrants—who often alternate short jobs and unemployment, particularly those with low qualifications—I complement the two binary employment indicators above with a more granular employment measure. I use the cumulative number of days worked in full-time equivalent (FTE), measured quarterly and unconditionally on employment. As shown by Cockx and Van Belle (2019), this cumulative measure can reveal differences in the type of jobs accepted that job-finding and employment rates alone may miss. To further investigate job types and job quality, I disaggregate by contract type and report three additional outcomes: cumulative FTE days worked in full-time, part-time, and short-term contracts.[88] The last category refers to working contracts facilitated by temporary work agencies, which typically last no more than one day. While earnings are often used to assess job quality, they are less suitable here due to the limited wage distribution among low-educated youths and the high minimum wage in Belgium. [89] Additionally, job duration is not available in my data, making job type the best available indicator.

[88] In addition, analogous variables for cumulative job-finding and employment rates, disaggregated by job type, are reported in Appendix.
[89] Results of the analysis on wages are available upon request.

All employment outcomes are measured quarterly, from the first full calendar quarter following the quarter of registration at the PES up to the twelfth. For example, for individuals who registered for the first time in the last quarter of 2014 (October to December 2014), the end of the first (second) quarter after registration corresponds to March (June) 2015.[90] Accordingly, the first observation (corresponding to the end of quarter 1) is measured 3 to 6 months after first registration at the PES, depending on the exact registration month. The second observation, at the end of quarter 2, occurs 6 to 9 months after first registration, and so on. Employment is defined as wage employment and does not include self-employment.[91] Finally, in light of Cockx et al. (2023), which examines the same effect of the same reform on degree completion, one might also ask whether it affected participation in training after labour market entry. This issue is not analysed here.[92]

3.5. Results

The three following subsections presents the main empirical results. The first two report the effects of extending of the qualifying period by at least 6 months on the benefit of the activation allowance and on employment. The third subsection examines gender heterogeneity in employment effects, helping to refine the interpretation. For each outcome, a first figure plots treatment effects estimated separately at each time horizon up to three years after first registration at the PES, along with 95% confidence intervals. A companion figure displays the counterfactual outcome of the treatment group, providing a benchmark for comparison.

[90] The quarter of registration (quarter 0) is not considered in the analysis, as the data do not record the within-quarter timing of employment spells. Consequently, it is not possible to know whether these spells occurred before or after registration, information necessary to compute the job-finding rate and the cumulative number of days worked from the quarter of first registration onwards.

[91] Results on self-employment outcomes yield not statistically significant results and are available upon request.

[92] Focusing specifically on training provided by the Public Employment Service (PES), I find no significant effect of the reform on training participation. Results are available upon request.

Additionally, parallel-trends plots for each outcome are provided in Appendix B.

3.5.1. Benefit of the activation allowance

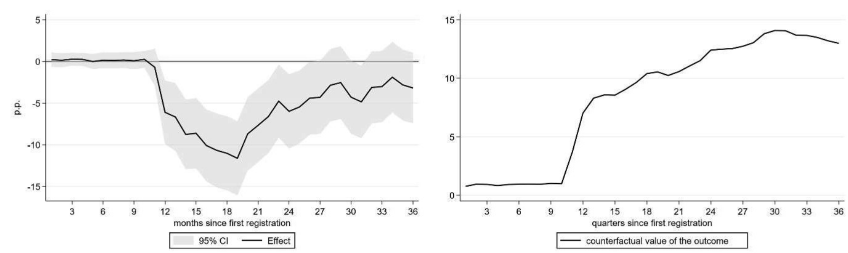
Figure 1 displays the monthly treatment effects on the receipt of the activation allowance, together with the corresponding counterfactual for the treatment group. The counterfactual, in the right graph, indicates the proportion of the treatment group that would have claimed the activation allowance in absence of the reform. The treatment effect, depicted in the left graph, gives the decrease in receipt implied by the extension of the qualifying period.

As expected, treatment effects are not statistically different from zero in months 1 to 12—the initial qualifying period—since nothing changes compared to the pre-reform period for both groups. Thereafter, treatment effects become negative and statistically significant, reflecting the extension of the qualifying period for the treated. The absolute value of the effect grows until month 19, reaching -11.6pp. One and a half years after first registration, slightly more than 10% of the treatment group would have claimed the activation allowance, but are unable to do so due to the extended qualifying period. This estimate provides strong grounds to expect an employment effect, as a non-negligible share of the treated is directly exposed to the reform and therefore likely to adjust behaviour. If all of them respond by increasing their labour supply, employment would rise by 10 pp. This estimate gives a lower bound of the employment effect: additional responses may arise from anticipation of the extended qualifying period, even among youths who would not ultimately have needed to claim.

Given the estimated treatment effects and counterfactuals, no treated individual benefits from the activation allowance until month 19, which is consistent with the post-reform qualifying period of at least 18 months. After month 19, the treatment effect starts to decrease as some individuals regain eligibility. By month 28, the treatment effect stabilises slightly below zero and remains negative throughout the remainder of the period, although generally not statistically significant. This pattern reflects that most treated individuals have regained eligibility, as the average qualifying period after the reform is 24 months in the treatment group. The persistent negative effect observed until month 36—even once most have regained eligibility—suggests a slight increase in employment among the treated. This result is in line with Bolhaar et al. (2019) who found that introducing a job search period for welfare benefits applicants led to a permanent decline in welfare benefits take-up.

To test for parallel trends, Figure B1 in Appendix B displays the coefficients of the interaction term between the treatment and the first pre-period dummies at each time horizon—δ_{pre1} in Equation (1). These estimates are not statistically significant for all time horizons, suggesting similar pre-reform trajectories in the receipt of the activation allowance across treatment and control groups.

Figure 1. Benefit of the activation allowance



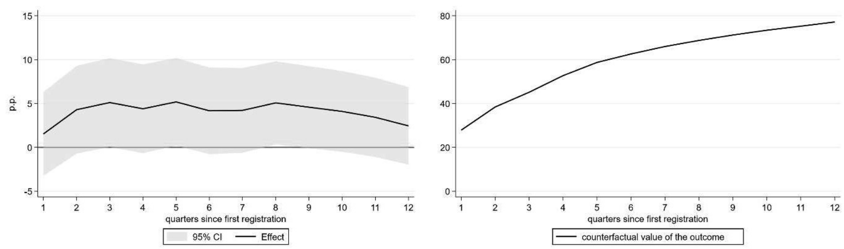
Note: The evaluation sample consists of high school dropouts registering for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes job seekers aged between 18 and 19 years and 6 months, while the control group consists of 20-year-old job seekers. Age is measured at the end of the month of first registration at the PES. Both graphs are derived from the estimation of the benchmark Difference-in-Differences (DiD) model (Equation 1) for each time horizon separately. The left graph displays the coefficient of the interaction term between the treatment and post-reform dummies, along with its 95% confidence interval at each time horizon. The right graph depicts the counterfactual outcome of the treatment group in absence of the reform. The counterfactual is predicted using the benchmark DiD model (Equation 1) setting the interaction effects between the treatment and post-reform dummies equal to zero. Table A2 in Appendix A provides the exact values of the treatment effect, its standard errors and p-value, as well as the counterfactual for each time horizon. Figure B1 in Appendix B displays the coefficients of the interaction term between the treatment and the first pre-period dummies.

3.5.2. Employment

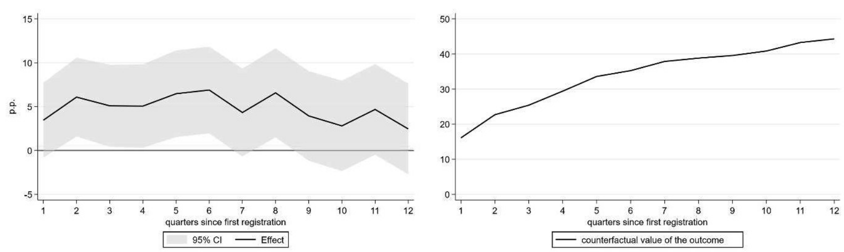
Figure 2 summarizes the impact of the reform on the three employment outcomes: (a) the job-finding rate, (b) the employment rate, and (c) the cumulative number of days worked in FTE, measured quarterly over the three years following first registration at the PES.

Figure 2. Employment outcomes

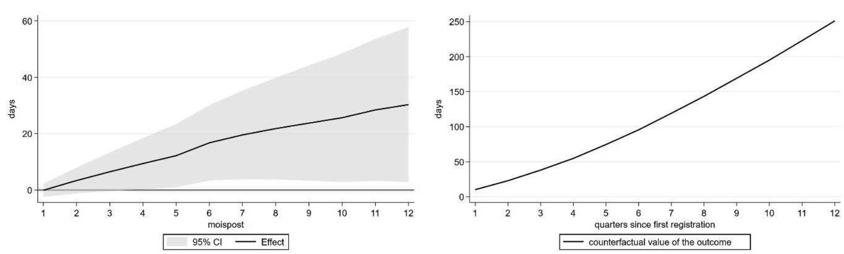
(a) Job-finding rate



(b) Employment rate



(c) Cumulative number of days worked in FTE



Note: The evaluation sample consists of high school dropouts registering for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes job seekers aged between 18 and 19 years and 6 months, while the control group consists of 20-year-old job seekers. Age is measured at the end of the month of first registration at the PES. Both graphs are derived from the estimation of the benchmark

Difference-in-Differences (DiD) model (Equation 1) for each time horizon separately. In each panel, the left graph displays the coefficient of the interaction term between the treatment and post-reform dummies, along with its 95% confidence interval at each time horizon. The right graph depicts the counterfactual outcome of the treatment group in absence of the reform. The counterfactual is predicted using the benchmark DiD model (Equation 1) setting the interaction effects between the treatment and post-reform dummy equal to zero. Tables A3 to A5 in Appendix A provide the exact values of the treatment effect, its standard errors and p-value, as well as the counterfactual for each time horizon. Figure B2 in Appendix B displays the coefficients of the interaction term between the treatment and the first pre-period dummies.

In Panel (a), the left graph suggests that extending the qualifying period increases the job-finding rate. Treatment effects are statistically significant at the 10% level from quarter 2 to 10, and in several quarters at the 5% level. From quarter 3 to 8, the effects remain stable at about 5pp. In quarter 3—the first quarter with significance at the 5% level—the job-finding rate of the treatment group increases by 5.1pp relative to the control group. The right-hand graph of Panel (a) shows that, in absence of the reform, 45.1% of the treatment group would have transitioned to at least one job by the end of quarter 3. The reform raised this to 50.2%, which correspond to a proportional increase of 11% in the job-finding rate. The counterfactual rises steadily over the three-year horizon. Therefore, by quarter 8, the same 5pp effect represents a smaller proportional increase of about 7% (5pp/68.7). The treatment effect declines after quarter 8, which corresponds to the average new end of the qualifying period. In quarters 11 and 12, it is no longer statistically significant, even at the 10% level.

Treatment effects on the employment rate, shown in Panel (b) of Figure 2, mirror those for the job-finding rate: extending the qualifying period significantly increases the employment rate from quarter 2 to quarter 8. Statistical significance is more consistent than for job-finding, with effects significant at the 5% level in all quarters 2 to 8 except quarter 7.[93] The average effect over this window is 5.8pp. The right graph of Panel (b) indicates that, absent the reform, the proportion of the treatment group employed at the end of the quarter rises from 22.7% in quarter 2 to 38.8% in quarter 8. Accordingly, the proportional increase in employment goes from 26.8% (6.1pp/22.7%) in quarter 2 to 16.9%

[93] In quarter 7, the treatment effect is statistically significant at 10%, with a p-value of 0.089.

(6.6pp/38.8%) in quarter 8. From quarter 8 onwards—coinciding with the end of the extended qualifying period in the treatment group—the effect declines and loses statistical significance.[94]

Panel (c) of Figure 2 reports the analysis on the cumulative number of days worked in FTE. Depicted in the right-hand graph, the counterfactual outcome exhibits a constant increase, reflecting both the increasing job-finding rate and the accumulation of days worked.[95] Given the relatively low employability of my sample, the number of days worked remain modest. Three years after first registration at the PES, the counterfactual for the treatment group reaches 250—roughly 11 months of FTE work—reflecting the weak labour market attachment of the sample. The left graph of Panel (c) shows that extending the qualifying period increases the number of days worked starting from the second quarter following first registration. This increase is statistically significant from quarter 4 onwards—when extension takes effect—and gradually increases over the three years. By the end of the twelve quarters, the estimated effect indicates 30 additional days worked, corresponding to an increase of slightly more than one FTE month. Between quarters 4 and 12, the treatment effect in absolute terms increases from 9 to 30 days. In relative terms, it decreases from 17% (9.4/54.8) to 12% (30/250).

Regarding the parallel trends assumptions, Figure B2 in Appendix B depicts the coefficients δ_{pre1} for the three employment outcomes at each time horizon. For the job-finding rate, the coefficient suggests a difference in the evolution of the treatment and control groups between the two pre-reform periods comparable to that observed after the reform. Although not statistically significant, this pattern may raise concerns regarding the identification strategy. By contrast, the coefficients for the other two outcomes remain consistently close to zero and are never statistically significant, which supports the validity of the parallel trends assumption for employment overall. Taken together, these results provide sufficient

[94] By the end of the three-year period, the counterfactual employment rate for the treatment group (in the absence of the reform) is roughly 45%. Against this benchmark, the proportion of individuals receiving the activation allowance at the same time—slightly below 15%—may seem low. Appendix C documents this limited take-up.

[95] Indeed, the outcome is not conditional on employment, meaning that if an individual did not work during the three years, the variable takes the value 0. It is computed in a cumulative way such that, if an individual did not work during a specific quarter, the value of the variable does not increase.

evidence that the parallel trends assumption holds for employment measures overall.

Overall, the analysis of the three employment outcomes indicates that extending the qualifying period had a positive impact on employment. The effect on the employment rate is stronger both in magnitude and statistical significance than that on the job-finding rate, suggesting that while treated high school dropouts find jobs slightly faster than the control group, they would be more likely to secure more jobs or longer employment positions. This intuition is verified by analysing the impact of the reform on the cumulative number of days worked in full-time equivalent (FTE). Three aspects of these findings are worth highlighting.

First, the employment effects are already visible from the second quarter after first registration at the PES. Measuring employment at the end of quarter 2 means that between 6 and 9 months have elapsed since first registration at the PES, depending on the month of first registration. By that time, some individuals have already attended their first evaluation meeting at the PES. During that visit, those not yet aware of the extension of their qualifying period were most likely informed by their PES counsellor, which may have triggered a behavioural reaction. The extension of the qualifying period takes effect 3 to 6 months later. Only from quarter 4 onwards, all the treatment group is actually subject to the extension of their qualifying period if still unemployed. Therefore, the effects in quarters 2 and 3—before the extension takes place—can be interpreted as anticipation effects. Anticipating the extension of their qualifying period beyond one year, some individuals work more early on. This provides evidence of forward-looking behaviour within the treatment group, as they react early to a change that actually occurs some months later in the spell.[96]

[96] These findings may appear to contrast with those of Cockx et al. (2023), who show that the reform did not increase degree completion rates among students who were enrolled in high school in 2014–2015. They interpret this result as evidence of a strong present bias among low-educated youths. It is important to emphasize that while Cockx et al. (2023) focus on students, the present analysis considers young individuals who have already entered the labour market and recently registered at the PES. This population is likely to be more aware of the activation allowance eligibility rules and may have received information about it upon registration. Unfortunately, I do not have access to detailed data on the information provided by PES agents, nor on how clearly the changes in eligibility criteria were communicated to these young job seekers.

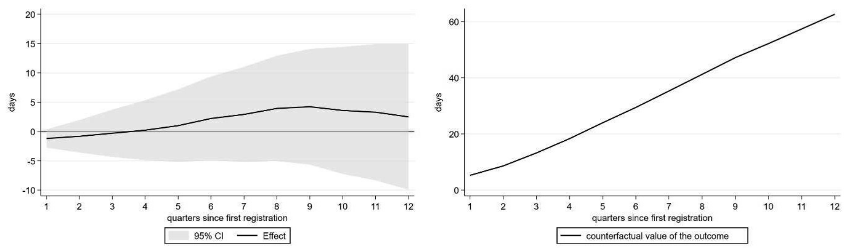
Second, while the employment effects emerge early, the evidence suggest that their persistence over time is limited. As the extended qualifying period ends, individuals regain eligibility for the activation allowance and incentives to work weaken. This is reflected in diminishing and statistically insignificant effects on job-finding and employment around quarter 8. Nevertheless, treatment effects across the three outcomes remain positive up to three years.

Third, the magnitude of the treatment effects is noteworthy. Between quarter 2 and 8, the estimated treatment effects on both the job-finding and employment rates are around 5pp. These are non-negligible effects when compared to the maximum proportion of the treatment group not claiming the activation allowance due to the extended qualifying period—11.6 percentage points at month 19.

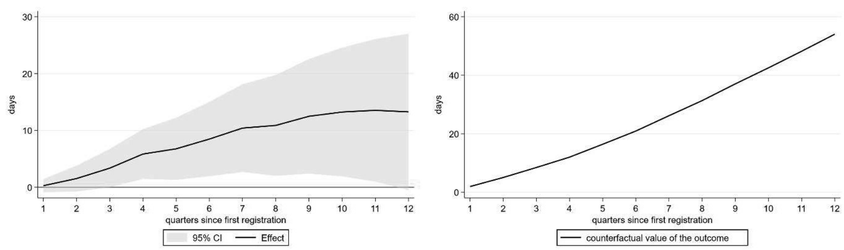
The increase in employment induced by the extended qualifying period aligns with Bolhaar et al. (2019) and von Buxhoeveden (2019), but contrasts with Cockx and Van Belle (2019), the study with the closest institutional context to this mine. Focusing on youths with a bachelor or master degree, they find that extending the qualifying period for the activation allowance from 9 to 12 months has no impact on the job-finding rate, but decreases the number of days worked in the quarter of exit from unemployment and the four subsequent ones. They conclude that extending the qualifying period encouraged treated individuals to accept more easily short-term job offers at the expense of longer ones. To better understand the divergence in findings, and to further investigate the quality of jobs accepted, I disaggregate employment by contractual arrangement. Figure 3 reports the analyses on the number of days worked in (a) short-term, (b) part-time, and (c) full-time jobs. Figure B3 in Appendix B displays the corresponding parallel trends tests, suggesting the parallel trends assumption holds. For brevity, figures relating to job-finding and employment rates by job type are only very briefly discussed and are available in Appendix B.

Figure 3. Cumulative number of days worked in FTE in (a) short-term (b) part-time (c) full-time jobs

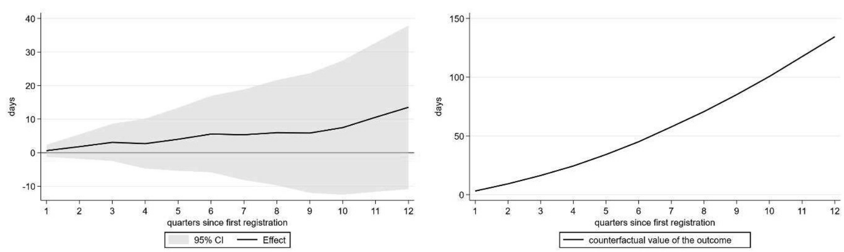
(a) Short-term



(b) Part-time



(c) Full-time



Note: The evaluation sample consists of high school dropouts registering for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes job seekers aged between 18 and 19 years and 6 months, while the control group consists of 20-year-old job seekers. Age is measured at the end of the month of first registration at the PES. Both graphs are derived from the estimation of the benchmark Difference-in-Differences (DiD) model (Equation 1) for each time horizon separately. In each panel, the left graph displays the coefficient of the interaction term between the treatment and post-reform dummies, along with its 95% confidence interval at each time horizon. The right graph depicts the counterfactual outcome of the treatment group in absence of the reform. The counterfactual is predicted using the benchmark DiD model (Equation 1) setting the interaction effects between the treatment and post-reform dummy equal to zero. Tables

A6 to A8 in Appendix A provide the exact values of the treatment effect, its standard errors and p-value, as well as the counterfactual for each time horizon. Figure B3 in Appendix B displays the coefficients of the interaction term between the treatment and the first pre-period dummies.

In Figure 3, the graphs depicting the counterfactuals indicate that approximately half of the total number of days worked over the three-year period are in full-time jobs, with the remainder equally divided between part-time and short-term jobs. In Panel (a), treatment effects show no increase in the cumulative number of days worked in short-term jobs. Panels (b) and (c) show that the increase in total days worked is driven by an increase in part-time and full-time jobs. Part-time employment appears to be the main driver, as treatment effects on days worked in part-time jobs are statistically significant from quarter 3 after first registration at the PES. The treatment effect increases until quarter 10 and then stabilizes. In quarter 12, the treatment effect is only statistically significant at the 10% level. The number of additional part-time days worked attributable to the extended qualifying period rises from 3.4 days in quarter 3 to 13.5 days in quarter 11, equivalent to approximately half a month of additional FTE work after three years. This represents proportional increases of 48% to 28%, respectively in quarter 3 and 11.

Treatment effects on days worked in full-time jobs remain relatively low until quarter 9 (below six additional days), then rise sharply. By quarter 12 after the first registration at the PES, effects on part-time and full-time days worked converge. However, the wide confidence intervals imply that the treatment effects on full-time jobs are not statistically significant. The sharp increase in full-time treatment effect after quarter 10 coincides with a stagnation in treatment effects on days in part-time jobs. This pattern may indicate that, by acquiring work experience in part-time jobs, treated individuals finally transition more to full-time jobs. This hypothesis is further supported by analysing the job-finding rate by job type. Figure B4 in Appendix B depicts the treatment effects on those outcomes. In quarter 11 and 12, the probability of having found at least one full-time job increases by slightly more than 5pp in the treatment group after the reform.

The evidence indicates that extending the qualifying period raises employment primarily through part-time jobs. In contrast to Cockx and Van Belle (2019), I find no increase in short-term jobs. Moreover, the increase of part-time employment contributes to a higher number of days worked and does not appear to crowd out other forms of employment. Consistent with this, Figure B5 in Appendix B shows a significant increase in the probability of part-time employment after the reform, persisting up to quarter 8—the average end of the extended qualifying period. Compared to Cockx and Van Belle (2019), the analysis suggests that low-educated individuals appear more responsive to work incentives than their higher-educated counterparts. Nevertheless, the aggregate rise in employment is largely driven by part-time work, with evidence of transitions to full-time jobs only emerging at the very end of the three-year period.

3.5.3. Heterogeneity analysis by gender

A growing literature documents systematic gender differences in job search behaviour. Relative to men, women have lower reservation wages (Caliendo et al., 2017; Cortés et al., 2023; Le Barbanchon et al., 2021) and place greater weight on non-wages job attributes such as schedule flexibility, job stability, shorter commutes, and less competitive work environments (Basbug and Fernandez, 2025; Eriksson and Lagerström, 2012; Goldin, 2014; Le Barbanchon et al., 2021; Wiswall and Zafar, 2018). Systematic differences in preferences and beliefs—in particular the lower risk aversion and higher overconfidence over future wages among men—have been shown to shape differences in job search behaviour and outcomes: higher reservation wage for men and tendency to accept job earlier for women (Cortés et al., 2023). Such divergences account for part of the actual gender wage gap and unequal distribution of men and women across occupation. They may also generate gender-specific responses to policy modifying work incentives of unemployed individuals. Accordingly, extending the qualifying period may induce differentiated adjustments in search intensity and in the timing and types of job accepted.

Beyond the literature on gender-specific aspects of job search, the finding that employment increase following the reform is concentrated in part-time jobs further motivates examining gender heterogeneity. Women are disproportionately more employed part-time jobs. The heterogeneity analysis also allows me to assess whether the increase in part-time employment observed after the reform reflects a lowering of job acceptance criteria—where all individuals become more willing to accept part-time work—or whether it results from a composition effect, whereby those who respond to the reform are individuals who were already more likely to work part-time prior to the reform.

The gender heterogeneity analysis is conducted by estimating Equation (1) separately on men and women. Since the evaluation sample is small, statistical power is limited, and results should be interpreted with caution.[97] Nonetheless, it offers suggestive evidence on potential underlying mechanisms.

Figures B6 to B8 in Appendix B present the treatment effects and counterfactual outcomes for the job-finding rate, the employment rate, and the cumulative number of days worked, disaggregated by gender. While counterfactual trends for job-finding and employment rates are only slightly lower for women compared to men, larger differences appear in the number of days worked. This reflects differences in the positions occupied: since women are more often in part-time jobs, they accumulate fewer days in FTE.[98] After three years, women have worked 58 fewer days than men on average (215.4 vs. 273.7 days). Treatment effects on the job-finding and employment rates are slightly higher for women, particularly at the beginning of the studied period, though no clear difference emerges. A more distinct gender pattern appears when considering the cumulative number of days worked. Over the three-year period, treatment effects are twice as large for women as for men. They become statistically significant from the third quarter onwards for women, while remaining insignificant for men.

Figure 4 below displays the estimated treatment effects on the cumulative number of days worked in (a) short-term, (b) part-time, and

[97] The evaluation sample comprises 8,946 individuals, of whom 3,668 are women and 5,278 are men.

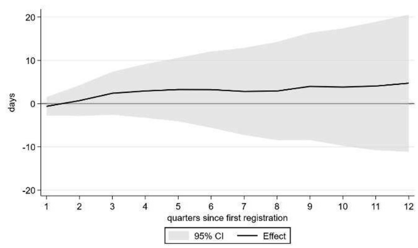
[98] The difference is type of jobs held is illustrated in Figure B9 in Appendix B displaying the counterfactual employment rates by job type. While there is not much difference in the employment rate in short-term jobs, there are significant disparities employment rate in part-time and full-time jobs. Over the three-year period, the proportion of treated women employed in part-time jobs is 2.5 times higher than that of men, while their participation in full-time jobs is half as high.

(c) full-time jobs, separately for men and women. Figure B10 in Appendix B presents the corresponding counterfactuals. The counterfactual outcomes reflect the gender difference in the type of jobs occupied. After three years, part-time jobs account for 39% of total days worked for women, compared to only 13% for men. Conversely, full-time jobs represent 60% of days worked among men and 40% among women. The share of short-term jobs is relatively similar, at 21% for women and 27% for men. Regarding treatment effects, although I cannot formally reject the hypothesis that effects are equal across genders, they are consistently larger for women, particularly in part-time and full-time employment. The positive treatment effects on the number of days worked in part-time jobs observed in the total sample appear to be largely driven by women. From quarter 4 to quarter 10, women work 11 to 23 additional days in part-time jobs, which corresponds to roughly one extra month of FTE work after three years. Treatment effects are also statistically significant at the 10% level in quarters 3 and 11. In contrast, treatment effects for men are on average twice as small and not statistically significant.

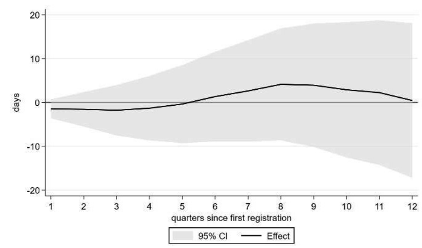
Figure 4. Treatment effects on the cumulative number of days worked in (a) short-term (b) part-time (c) full-time jobs by gender

(a) Short-term

Women

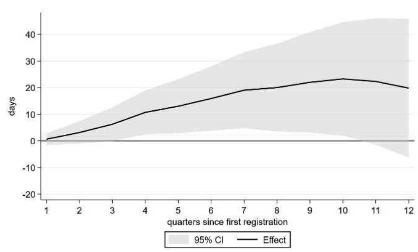


Men

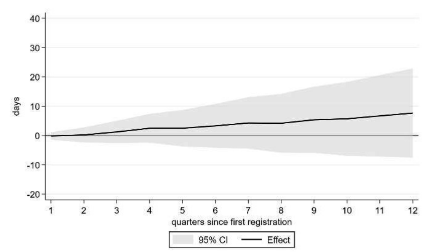


(b) Part-time

Women

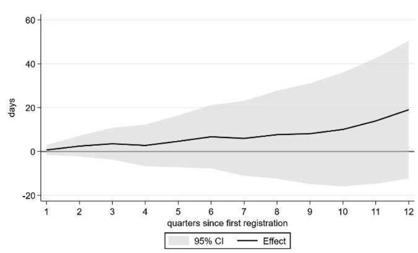


Men

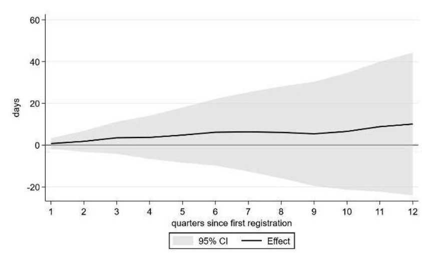


(c) Full-time

Women



Men



Note: The evaluation sample consists of high school dropouts registering for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes job seekers aged between 18 and 19 years and 6 months, while the control group consists of 20-year-old job seekers. Age is measured at the end of the month of first registration at the PES. All graphs are derived from the estimation of the benchmark

Difference-in-Differences (DiD) model (Equation 1) for each time horizon separately. The graphs display the coefficient of the interaction term between the treatment and post-reform dummies, along with its 95% confidence interval at each time horizon. Estimations are conducted separately for men and women, with sample sizes of N = 3,668 for women and N = 5,278 for men. Tables A9 to A11 in Appendix A provide the exact values of the treatment effect, its standard errors and p-value, as well as the counterfactual for each time horizon.

The heterogeneity analysis reveals that the observed increase in part-time employment is not driven by the reform lowering job acceptance standards and pushing individuals into such jobs. Rather, it reflects the characteristics of those who adjust their behaviour in response to the reform—specifically, women who were already more likely to accept part-time positions prior to its implementation. This finding for women high school dropouts aligns with the conclusions of Le Barbanchon (2016) and von Buxhoeveden (2019), who show that among low-employability job seekers and secondary school graduates, declines in job-finding rates tend to operate through reduced search intensity rather than increased selectivity. In line with that interpretation, the extension of the qualifying period appears to have operated through stronger search effort: among women high school dropouts, I observe an increase in part-time employment, but no shift in selectivity, as the types of jobs accepted by women reflect those they were accepting before the reform.

The greater responsiveness of women to the reform is consistent with their higher risk aversion. Two other potential explanations emerge from the data.

The first relates to the greater preference for housing autonomy among young women. In developed countries, women leave the parental home earlier than men (see, among others, Blaauboer and Mulder, 2010; Buck and Scott, 1993; Chiuri and Del Boca, 2010; Consuelo Colom and Cruz Molés, 2024; del Rey et al., 2023). This general pattern in the population reflects certain characteristics of women such as their tendency to form partnerships at younger ages. However, even after controlling for observable differences between men and women, a substantial portion of the gender gap in home-leaving remains unexplained (Blaauboer and Mulder, 2010; Consuelo Colom and Cruz Molés, 2024). This reflects the stronger preference for residential independence among women. The figures on co-residence with parents by gender in my evaluation sample—relatively homogeneous in terms of education and other characteristics—are consistent with this. Table A12 in Appendix A reports the share of men and women residing at the parental home at the end of the registration year, and at the end of the first and second calendar years thereafter. The sharper decline over time in the share of women living with their parents supports the view that women value housing autonomy more strongly.

The economic literature has emphasised the role of co-residence with parents as a form of informal insurance against unemployment risk, since it provides not only housing but also, in some cases, financial support (Declercq et al., forthcoming; Landaud, 2021; Matsudaira, 2015). Since women are more likely to have left the parental home, they rely less on such "informal" insurance. Consequently, changes in UB eligibility may prompt women to intensify their job search in order to preserve their independence, leading to higher employment rates. Given that women were already more likely to hold part-time jobs prior to the reform, the observed increase in employment is primarily reflected by a rise in part-time positions. According to this mechanism, women intensified their job search effort more than men, which translated into higher employment effects for women.

The second potential explanation relates to sectoral differences in job availability. Women may be more likely to be employed in sectors with higher vacancy rates or labour shortages, while men are more likely to work in saturated sectors with fewer available jobs. Indeed, for higher search effort to materialise into higher employment rates, there must be sufficient job opportunities. If men search in sectors with few available jobs, stronger search efforts may not translate into higher employment, as adequate positions are simply unavailable. Under this mechanism, women did not necessarily search harder than men, but they searched in sectors with more vacancies, making their effort translate more readily into jobs.

Table A13 in Appendix A presents the pre-reform sectoral distribution of the cumulative number of days worked at the end of quarter 12, disaggregated by gender. This provides insight into the gender differences in the relative importance of each sector in employment. The sectoral classification used aggregates the 21 NACE sections into 11 broader categories, following the approach adopted in the Belgian national accounts. This aggregation enables comparison with sector-level vacancy statistics, shown in the last column of Table A13 (Statbel, 2025d).

Table A14 in Appendix A details the correspondence between NACE sections and the aggregated categories.

Excluding the sciences and services sector—which accounts for a similar share of male and female employment and consists predominantly (90%) of short-term jobs unaffected by the reform—female employment is highly concentrated in two sectors: retail and non-profit. Those two sectors also have the highest number of unfilled vacancies. They account for 40% of all unfilled vacancies in 2014, indicating a strong labour demand. The accommodation and food sector also plays a significant role for women (approximately 10%), while the remaining sectors contribute marginally. In contrast, male employment is more evenly distributed across sectors. Although retail, non-profit, and accommodation and food sectors are also important for men, they represent only 40% of total employment for men, compared to 60% for women. Young men work relatively less in sectors with high job vacancy rates, which may partly explain gender differences in responsiveness to the reform. This pattern is consistent with general statistics on the unemployment rate of high school dropouts, which is about 2 percentage points lower for women than for men at the time of the reform (Eurostat, 2025f).

3.6. Robustness analysis

3.6.1. Weighted Difference-in-Differences

To account for the minor differences in the evolution of background characteristics between the treatment and control groups, a weighted difference-in-differences estimator is implemented. The idea of this approach is to make control individuals comparable to treated individuals by inverse probability weighting as proposed by Horvitz and Thompson (1952) and Hirano et al. (2003). The implementation follows the strategy of Albanese and Cockx (2019), weighting observations in the treatment and control groups before and after the reform such that their characteristics match those of the observations in the treatment group after the reform.

The estimation procedure involves two main steps. First, I estimate the probability of being treated conditional on individual characteristics. Second, I estimate the difference-in-differences model of Equation (1), weighting each observation by the normalized inverse probability of treatment. Standard errors are obtained through bootstrapping, with 500 replications of the estimation procedure. Appendix D provides a detailed description of the weighted difference-in-differences methodology. Table A15 in Appendix A presents descriptive statistics for the evaluation sample after applying the reweighting procedure. Treatment effects estimated using the weighted difference-in-differences approach are presented in Figures B11 to B16 in Appendix B.

Overall, this robustness analysis supports the main conclusion that extending the qualifying period increases the employment rate. While the results for the job-finding rate and the cumulative number of days worked are not statistically significant, their magnitudes are comparable to those in the benchmark specification, and the effects on the employment rate remain significant. More importantly, the weighted difference-in-differences estimates confirm that the increase in employment is concentrated in part-time jobs, as indicated by the rise in cumulative days worked in this category, and that this effect is primarily driven by women, as shown in the gender-based heterogeneity analysis.

3.6.2. Alternative treatment definitions

In the benchmark analysis, the treatment is defined as an extension of the qualifying period by at least 6 months. Consequently, the treatment group consists of individuals aged 18 years to 19 years and 6 months. In this section, I consider two alternative treatment definitions, each based on a subgroup of the initial treatment group. First, I restrict it to individuals aged 19 years to 19 years and 6 months. Second, I focus on individuals aged 18.

Considering these alternative definitions of the treatment group allows me to assess whether the inclusion of younger individuals, who may differ systematically from older ones, biases the results. It also enables a comparison of employment responses to different treatment intensities. Indeed, the older treatment group (aged 19 to 19.5 years) faces an extension of its qualifying period by between 6 and 12 months, with an average of 9 months given its mean age of 19 years and 3 months. While the younger treatment group (aged 18 years) faces an extension of its qualifying period by between 13 and 24 months, with an average of 18 months given its mean age of 18 years and 6 months. These alternative definitions yield two evaluation samples of about 5,000 individuals each, given the relatively small number of individuals aged between 18 and 18.5 in the sample (see Figure B17 for the age distribution of the treatment group).

Table A16 reports the descriptive statistics of the two alternative treatment groups. As expected, there are small differences between the younger and the older treatment groups, with the latter being closer to the control group given their proximity in age. However, the differences between the treatment groups and the control group remain relatively stable over time.[99] As shown in the last two columns of Table A16, there are only some minor statistically significant compositional changes in three variables. There is a small but statistically significant difference between the evolution of the control and younger treatment groups in terms of individuals registering in Flanders and registering at the end of a school year. For the older treatment group, the only compositional change—statistically significant at the 10% level—concerns the number of individuals registering in Brussels. Unlike in the benchmark sample, no significant compositional differences remain in household composition.[100] Overall, the older treatment group is not only closer to the control group in terms of observable characteristics but also appears to have fewer issues of compositional change.

Treatment effects and parallel trends tests for the two alternative treatment groups are presented in Figures B18 to B26. Figure B18 reports the results for the benefit of the activation allowance. Treatment effects reflect the difference in treatment intensity for the two samples. For the older treatment group, with an extension of the qualifying period of 6 to 12 months, treatment effects are statistically significant from months 12 to 22, but then converge to zero and become insignificant. For

[99] As for the benchmark sample, I use the benchmark specification of Equation (1) with pre-determined characteristics as outcome variables to test for the absence of compositional changes.

[100] This may, in part, reflect the smaller sample size, which lowers statistical power.

the younger treatment group, with a longer extension of the qualifying period, treatment effects remain high and statistically significant for a longer period, before stabilising below zero.

Regarding the job-finding rate and the employment rate, the estimated effects are very similar for both treatment groups. Compared to the benchmark sample, some statistical significance is lost, but the results point in the same direction. Moreover, as the parallel trends assumption for the job-finding rate is not always satisfied in the specification with the older treatment group, these findings should be viewed with caution.

Regarding the cumulative number of days worked in FTE, treatment effects are slightly larger and more often statistically significant for the older treatment group, reflecting a greater increase in the number of days worked in part-time jobs within this group. The analysis also confirms that the increase in part-time jobs is essentially driven by women. Although effects appear stronger and more statistically significant for the older treatment group, the confidence intervals overlap. There is no statistically significant difference between the two alternative treatment groups. Finally, regarding treatment effects on short-term and full-time jobs, no consistent patterns emerge.

From this analysis, I conclude that including the younger age group (aged 18) in the benchmark sample does not systematically bias the results. No substantial differences in treatment effects emerge between the two groups. A natural conclusion of this analysis could be that the difference in treatment intensity does not appear to trigger distinct behavioural reactions, as it does not substantially affect the magnitude of the employment effect. However, this may appear counterintuitive given the impact of the reform—extending the qualifying period beyond 12 months—on employment. The absence of differences in treatment effects plausibly stems from the high concentration of individuals around the age of 19, which reduces power to detect intensity-driven differences (see Figure B17 for the age distribution of the treatment group).

3.7. Conclusion

Low-educated youths entering the labour market are at high risk of unemployment. Access to UB at early career stages may help them smooth the school-to-work transition but may also reduce their job search incentives. To avoid excessive disincentives to work, UB access can be granted only after a job search period during which job search is compulsory, referred to as the qualifying period. This paper examines the impact of extending the qualifying period for UB on the employment outcomes of young labour market entrants without a high school diploma, exploiting a Belgian policy reform.

In Belgium, school leavers have access to a specific unemployment insurance—the activation allowance—after a one-year qualifying period. This period begins at labour market entry and no specific work requirements apply, but eligibility at its end depends on active job search. In September 2015, the federal government effectively extended this qualifying period for individuals entering the labour market before the age of 20 without graduating from high school. Using a difference-in-differences approach with individual-level administrative data, I compare employment outcomes between high school dropouts just under the age of 20, who are subject to an extended qualifying period, and those slightly over 20, who remain under unchanged eligibility rules.

The results indicate that extending the qualifying period leads to a persistent decline in activation allowance take-up, which reflects an increase in employment following the reform. The qualifying period extension increases both the job-finding and employment rates. These effects emerge early, suggesting anticipation effects, as individuals adjust their behaviour before the actual extension of the qualifying period. However, evidence indicates that the persistence of these effects is limited, as they decrease and lose statistical significance when individuals regain eligibility. The increase in employment translates into an increase in the cumulative number of days worked up to three years after first registration at the PES. These employment effects are consistent with an increase of job search intensity following the extension of the qualifying period.

Regarding the type of job accepted, I find that the increase in employment is essentially driven by part-time jobs, with an effect equivalent to one additional month of FTE work after three years. However, the results of the gender-heterogeneity analysis indicate that the effect of the extension of the qualifying period is limited to women. I conclude that the observed increase in part-time employment is not driven by the reform directly inducing individuals to take such jobs, but rather by the characteristics of those who respond to the reform—namely, women, who were already more inclined to accept part-time work prior to the reform. Therefore, there is no evidence of a decline in job acceptance criteria.

From a policy perspective, the main implication is that extending the qualifying period can serve as an effective work incentive for high school dropouts, as it increases their employment prospects. However, the employment gains are concentrated among a specific subgroup—young women—and are limited to part-time positions. These results highlight that seemingly gender-neutral policies may results in gender-differentiated outcomes. Further research is warranted to explore why the effects are concentrated among women, in particular whether they stem from gender differences in preferences or in the availability of the occupations they are looking for. Additional work is also needed to investigate other sources of heterogeneity in policy responses, such as migration background. Preliminary analyses suggest that the reform may have reinforced pre-existing employment disparities between migrant and non-migrant groups—an unintended consequence that policymakers may wish to avoid. Understanding the mechanisms behind these heterogeneous effects should therefore be a priority for future research.

Appendix

A - Tables

Table A1. Descriptive statistics

	Pre-reform		***Post-reform***	
	control	treated	Δ control	Δ treated
Age[(b)]	20.35	18.96	0.01	-0.01
Women[(a)]	40.43	42.46	-1.47	-2.09
Nationality[(a)]				
Belgian	91.73	92.71	-1.32	-1.37
EU	3.99	4.05	2.14	0.72
Other	4.29	3.16	-1.07	0.64*
Migration background[(a)]				
At least one non-Belgian parent	51.82	44.69	2.97	0.87
At least one non-Belgian grand parent	66.05	60.47	1.07	-0.33
Region of residence[(b)]				
Wallonia	50.56	45.01	1.04	0.55
Flanders	33.21	44.78	-2.92	-0.1**
Brussels	16.23	10.22	1.88	-0.45*
General educational track[(b)]	14.31	10.05	-1.43	-1.16
First registration between July and September	55.77	39.82	-1.28	0.04***
Household composition[(c)]				
Child living with parents	83.32	79.57	-0.67	-0.96
Single	6.03	6.39	-1.77	0.83*
Couple	4.32	5.68	1.16	0.71
House-sharing arrangement	6.33	8.3	1.28	-0.57*
Household size[(c)]	3.94	3.86	0.12	0.01
Annual household income	46,253	45,717	428	-1,235
N	**1,343**	**4,786**	**657**	**2,160**

Note: The evaluation sample consists of high school dropouts who registered for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes jobseekers aged between 18 and 19 years and 6 months, while the control group comprises 20-year-old jobseekers. Household size and household income are expressed in person and euros. For all other variables, the table gives the percentage of individuals in the treatment or control group that belong to the category. Outcomes in the post-reform period are expressed as changes relative to the outcome in the pre-reform period. * $p<0.10$, ** $p<0.05$, and *** $p<0.01$ in the Δ treated column indicate whether the change in the treatment group significantly differs from the change in the control group.

(a) Refers to variables measured on January 1 of the registration year. (b) Refers to variables measured at registration at the PES. (c) Refers to variables measured on December 31 of the calendar year before the registration year. Annual household income is defined as the mean income of all household members (including the individual studied) in June of the registration year, adjusted by the corresponding year's price index (reference year 2012), and annualized (multiplied by 12) for reporting purposes over the entire year.

Table A2. Benefit of the activation allowance

Months since first registration	***Effect***	***(s.e.)***	***P-value***	***Counterfactual value of the outcome***
1	0.002	(0.004)	0.629	0.008
2	0.001	(0.004)	0.750	0.010
3	0.003	(0.004)	0.494	0.009
4	0.002	(0.004)	0.540	0.008
5	0.000	(0.005)	0.983	0.009
6	0.001	(0.005)	0.778	0.010
7	0.001	(0.005)	0.822	0.010
8	0.002	(0.005)	0.735	0.009
9	0.001	(0.005)	0.882	0.010
10	0.002	(0.005)	0.629	0.010
11	-0.007	(0.011)	0.538	0.037
12	-0.061	(0.020)	0.002	0.070
13	-0.067	(0.021)	0.001	0.083
14	-0.088	(0.021)	0.000	0.086
15	-0.086	(0.022)	0.000	0.086
16	-0.101	(0.022)	0.000	0.091
17	-0.107	(0.023)	0.000	0.096
18	-0.110	(0.023)	0.000	0.104
19	-0.116	(0.023)	0.000	0.105
20	-0.087	(0.022)	0.000	0.102
21	-0.077	(0.023)	0.001	0.106
22	-0.066	(0.022)	0.003	0.110
23	-0.048	(0.022)	0.033	0.115
24	-0.060	(0.023)	0.008	0.124
25	-0.055	(0.022)	0.014	0.125
26	-0.044	(0.022)	0.048	0.125
27	-0.043	(0.022)	0.054	0.127
28	-0.029	(0.022)	0.197	0.130
29	-0.025	(0.022)	0.254	0.138
30	-0.043	(0.022)	0.055	0.141
31	-0.049	(0.022)	0.030	0.141
32	-0.031	(0.022)	0.155	0.137
33	-0.030	(0.022)	0.167	0.137
34	-0.019	(0.022)	0.382	0.135
35	-0.028	(0.022)	0.195	0.132
36	-0.032	(0.022)	0.141	0.130
N				**8,946**

Note: The evaluation sample consists of high school dropouts who registered for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes jobseekers aged between 18 and 19 years and 6 months, while the control group comprises 20-year-old jobseekers. Age is measured at the end of the month of first registration at the PES. Estimates are obtained from the benchmark Difference-in-Differences (DiD) model (Equation 1), estimated separately for each time horizon. The *second column reports the coefficient of the interaction term between the treatment and post-reform dummies, along with its standard errors. The third column presents the p-value, and the fourth shows the counterfactual value of the outcome. The counterfactual is predicted using the benchmark DiD model (Equation 1), setting the interaction between the treatment and post-reform dummies to zero.*

Table A3. Job-finding rate

Quarters since first registration	***Effect***	***(s.e.)***	***P-value***	***Counterfactual value of the outcome***
1	0.015	(0.024)	0.529	0.279
2	0.043	(0.025)	0.092	0.384
3	0.051	(0.026)	0.048	0.451
4	0.044	(0.026)	0.088	0.527
5	0.052	(0.025)	0.042	0.587
6	0.042	(0.025)	0.098	0.626
7	0.042	(0.025)	0.087	0.660
8	0.051	(0.024)	0.036	0.687
9	0.046	(0.024)	0.055	0.712
10	0.041	(0.023)	0.081	0.734
11	0.034	(0.023)	0.137	0.752
12	0.025	(0.023)	0.276	0.771
N				**8,946**

Note: The evaluation sample consists of high school dropouts who registered for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes jobseekers aged between 18 and 19 years and 6 months, while the control group comprises 20-year-old jobseekers. Age is measured at the end of the month of first registration at the PES. Estimates are obtained from the benchmark Difference-in-Differences (DiD) model (Equation 1), estimated separately for each time horizon. The second column reports the coefficient of the interaction term between the treatment and post-reform dummies, along with its standard errors. The third column presents the p-value, and the fourth shows the counterfactual value of the outcome. The counterfactual is predicted using the benchmark DiD model (Equation 1), setting the interaction between the treatment and post-reform dummies to zero.

Table A4. Employment rate

Quarters since first registration	*Effect*	*(s.e.)*	*P-value*	*Counterfactual value of the outcome*
1	0.035	(0.020)	0.112	0.161
2	0.061	(0.021)	0.008	0.227
3	0.051	(0.022)	0.033	0.254
4	0.051	(0.023)	0.037	0.294
5	0.065	(0.023)	0.010	0.336
6	0.069	(0.023)	0.006	0.352
7	0.043	(0.024)	0.089	0.379
8	0.066	(0.024)	0.011	0.388
9	0.039	(0.024)	0.130	0.396
10	0.028	(0.024)	0.285	0.408
11	0.047	(0.024)	0.075	0.433
12	0.025	(0.025)	0.351	0.443
N				**8,946**

Note: The evaluation sample consists of high school dropouts who registered for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes jobseekers aged between 18 and 19 years and 6 months, while the control group comprises 20-year-old jobseekers. Age is measured at the end of the month of first registration at the PES. Estimates are obtained from the benchmark Difference-in-Differences (DiD) model (Equation 1), estimated separately for each time horizon. The second column reports the coefficient of the interaction term between the treatment and post-reform dummies, along with its standard errors. The third column presents the p-value, and the fourth shows the counterfactual value of the outcome. The counterfactual is predicted using the benchmark DiD model (Equation 1), setting the interaction between the treatment and post-reform dummies to zero.

Table A5. Cumulative number of days worked in FTE

Quarters since first registration	*Effect*	*(s.e.)*	*P-value*	*Counterfactual value of the outcome*
1	-0.078	(1.186)	0.948	10.227
2	3.367	(2.332)	0.149	22.966
3	6.476	(3.483)	0.063	38.115
4	9.394	(4.620)	0.042	54.834
5	12.180	(5.720)	0.033	74.633
6	16.747	(6.823)	0.014	95.571
7	19.550	(8.010)	0.015	119.202
8	21.794	(9.207)	0.018	143.372
9	23.735	(10.411)	0.023	169.208
10	25.648	(11.645)	0.028	195.099
11	28.408	(12.847)	0.027	222.899
12	30.286	(14.024)	0.031	251.059
N				**8,946**

Note: The evaluation sample consists of high school dropouts who registered for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes jobseekers aged between 18 and 19 years and 6 months, while the control group comprises 20-year-old jobseekers. Age is measured at the end of the month of first registration at the PES. Estimates are obtained from the benchmark Difference-in-Differences (DiD) model (Equation 1), estimated separately for each time horizon. The second column reports the coefficient of the interaction term between the treatment and post-reform dummies, along with its standard errors. The third column presents the p-value, and the fourth shows the counterfactual value of the outcome. The counterfactual is predicted using the benchmark DiD model (Equation 1), setting the interaction between the treatment and post-reform dummies to zero.

Table A6. Cumulative number of days worked (FTE) in short-term jobs

Quarters since first registration	***Effect***	***(s.e.)***	***P-value***	***Counterfactual value of the outcome***
1	-1.187	(0.795)	0.135	5.287
2	-0.812	(1.418)	0.567	8.626
3	-0.291	(2.052)	0.887	13.243
4	0.227	(2.615)	0.931	18.339
5	1.005	(3.153)	0.750	23.987
6	2.224	(3.654)	0.543	29.456
7	2.917	(4.134)	0.480	35.311
8	3.939	(4.582)	0.390	41.232
9	4.229	(5.040)	0.401	47.175
10	3.595	(5.525)	0.515	52.171
11	3.296	(5.937)	0.579	57.373
12	2.503	(6.341)	0.693	62.614
N				**8,946**

Note: The evaluation sample consists of high school dropouts who registered for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes jobseekers aged between 18 and 19 years and 6 months, while the control group comprises 20-year-old jobseekers. Age is measured at the end of the month of first registration at the PES. Estimates are obtained from the benchmark Difference-in-Differences (DiD) model (Equation 1), estimated separately for each time horizon. The second column reports the coefficient of the interaction term between the treatment and post-reform dummies, along with its standard errors. The third column presents the p-value, and the fourth shows the counterfactual value of the outcome. The counterfactual is predicted using the benchmark DiD model (Equation 1), setting the interaction between the treatment and post-reform dummies to zero.

Table A7. Cumulative number of days worked (FTE) in part-time jobs

Quarters since first registration	*Effect*	*(s.e.)*	*P-value*	*Counterfactual value of the outcome*
1	0.275	(0.590)	0.641	1.965
2	1.535	(1.158)	0.185	5.051
3	3.367	(1.721)	0.050	8.470
4	5.848	(2.233)	0.009	12.007
5	6.763	(2.790)	0.015	16.413
6	8.478	(3.338)	0.011	20.935
7	10.413	(3.937)	0.008	26.174
8	10.878	(4.522)	0.016	31.336
9	12.484	(5.143)	0.015	37.052
10	13.234	(5.772)	0.022	42.530
11	13.534	(6.400)	0.034	48.198
12	13.260	(7.020)	0.059	54.038
N				**8,946**

Note: The evaluation sample consists of high school dropouts who registered for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes jobseekers aged between 18 and 19 years and 6 months, while the control group comprises 20-year-old jobseekers. Age is measured at the end of the month of first registration at the PES. Estimates are obtained from the benchmark Difference-in-Differences (DiD) model (Equation 1), estimated separately for each time horizon. The second column reports the coefficient of the interaction term between the treatment and post-reform dummies, along with its standard errors. The third column presents the p-value, and the fourth shows the counterfactual value of the outcome. The counterfactual is predicted using the benchmark DiD model (Equation 1), setting the interaction between the treatment and post-reform dummies to zero.

Table A8. Cumulative number of days worked (FTE) in full-time jobs

Quarters since first registration	*Effect*	*(s.e.)*	*P-value*	*Counterfactual value of the outcome*
1	0.614	(0.910)	0.500	3.134
2	1.807	(1.855)	0.330	9.237
3	3.083	(2.821)	0.275	16.327
4	2.699	(3.790)	0.476	24.429
5	4.006	(4.787)	0.403	34.174
6	5.552	(5.802)	0.339	45.053
7	5.353	(6.882)	0.437	57.717
8	5.966	(7.989)	0.455	70.737
9	5.860	(9.085)	0.519	85.159
10	7.481	(10.194)	0.463	100.624
11	10.562	(11.312)	0.351	117.506
12	13.530	(12.422)	0.276	134.388
N				**8,946**

Note: The evaluation sample consists of high school dropouts who registered for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes jobseekers aged between 18 and 19 years and 6 months, while the control group comprises 20-year-old jobseekers. Age is measured at the end of the month of first registration at the PES. Estimates are obtained from the benchmark Difference-in-Differences (DiD) model (Equation 1), estimated separately for each time horizon. The second column reports the coefficient of the interaction term between the treatment and post-reform dummies, along with its standard errors. The third column presents the p-value, and the fourth shows the counterfactual value of the outcome. The counterfactual is predicted using the benchmark DiD model (Equation 1), setting the interaction between the treatment and post-reform dummies to zero.

Table A9. Cumulative number of days worked (FTE) in short-term jobs by gender

Quarters since first registration	*Effect*	*(s.e.)*	*P-value*	*Counterfactual value of the outcome*
Women				
1	-0.627	(1.076)	0.560	3.432
2	0.688	(1.810)	0.704	5.549
3	2.390	(2.533)	0.345	8.610
4	2.923	(3.165)	0.356	11.875
5	3.250	(3.744)	0.385	15.477
6	3.235	(4.495)	0.472	19.582
7	2.813	(5.125)	0.583	23.934
8	2.909	(5.783)	0.615	28.348
9	3.974	(6.319)	0.529	32.764
10	3.820	(6.918)	0.581	36.669
11	4.061	(7.569)	0.592	41.003
12	4.709	(8.073)	0.560	45.080
N				**3,668**
Men				
1	-1.473	(1.103)	0.182	6.383
2	-1.564	(2.006)	0.436	10.518
3	-1.779	(2.933)	0.544	16.110
4	-1.310	(3.752)	0.727	22.358
5	-0.371	(4.537)	0.935	29.307
6	1.332	(5.215)	0.798	35.679
7	2.625	(5.901)	0.656	42.490
8	4.123	(6.511)	0.527	49.371
9	3.927	(7.174)	0.584	56.271
10	2.882	(7.862)	0.714	61.969
11	2.242	(8.411)	0.790	67.715
12	0.440	(8.998)	0.961	73.704
N				**5,278**

Note: The evaluation sample consists of high school dropouts who registered for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes jobseekers aged between 18 and 19 years and 6 months, while the control group comprises 20-year-old jobseekers. Age is measured at the end of the month of first registration at the PES. Estimates are obtained from the benchmark Difference-in-Differences (DiD) model (Equation 1), estimated separately for men and women at each time horizon. The second column reports the coefficient of the interaction term between the treatment and post-reform dummies. along with its standard errors. The third column presents the p-value. and the fourth shows the counterfactual value of the outcome. The counterfactual is predicted using the benchmark DiD model (Equation 1), setting the interaction between the treatment and post-reform dummies to zero.

Table A10. Cumulative number of days worked (FTE) in part-time jobs by gender

Quarters since first registration	*Effect*	*(s.e.)*	*P-value*	*Counterfactual value of the outcome*
Women				
1	0.646	(1.118)	0.564	2.570
2	3.153	(2.166)	0.146	7.485
3	6.252	(3.240)	0.054	12.921
4	10.713	(4.200)	0.011	18.672
5	13.026	(5.179)	0.012	25.526
6	15.924	(6.149)	0.010	32.246
7	19.058	(7.276)	0.009	40.611
8	20.033	(8.395)	0.017	49.139
9	22.007	(9.627)	0.022	58.134
10	23.295	(10.872)	0.032	66.860
11	22.324	(12.105)	0.065	75.886
12	19.823	(13.308)	0.136	85.202
N				**3,668**
Men				
1	-0.139	(0.663)	0.834	1.769
2	0.199	(1.308)	0.879	3.591
3	1.233	(1.936)	0.524	5.757
4	2.480	(2.509)	0.323	7.916
5	2.493	(3.171)	0.432	10.800
6	3.303	(3.811)	0.386	13.981
7	4.281	(4.476)	0.339	17.308
8	4.187	(5.116)	0.413	20.410
9	5.372	(5.763)	0.351	24.114
10	5.699	(6.427)	0.375	27.579
11	6.699	(7.094)	0.345	31.162
12	7.660	(7.757)	0.323	34.854
N				**5,278**

Note: The evaluation sample consists of high school dropouts who registered for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes jobseekers aged between 18 and 19 years and 6 months, while the control group comprises 20-year-old jobseekers. Age is measured at the end of the month of first registration at the PES. Estimates are obtained from the benchmark Difference-in-Differences (DiD) model (Equation 1), estimated separately for men and women at each time horizon. The second column reports the coefficient of the interaction term between the treatment and post-reform dummies. along with its standard errors. The third column presents the p-value. and the fourth shows the counterfactual value of the outcome. The counterfactual is predicted using the benchmark DiD model (Equation 1), setting the interaction between the treatment and post-reform dummies to zero.

Table A11. Cumulative number of days worked (FTE) in full-time jobs by gender

Quarters since first registration	*Effect*	*(s.e.)*	*P-value*	*Counterfactual value of the outcome*
Women				
1	0.692	(1.150)	0.548	2.272
2	2.465	(2.415)	0.307	7.189
3	3.535	(3.688)	0.338	12.004
4	2.763	(4.842)	0.568	16.986
5	4.647	(6.026)	0.441	22.858
6	6.717	(7.352)	0.361	29.902
7	5.971	(8.709)	0.493	38.123
8	7.677	(10.210)	0.452	46.584
9	8.126	(11.719)	0.488	56.075
10	10.051	(13.232)	0.448	65.911
11	13.935	(14.589)	0.340	76.073
12	19.042	(16.000)	0.234	86.209
N				**3,668**
Men				
1	0.759	(1.270)	0.550	3.480
2	1.812	(2.567)	0.480	10.448
3	3.525	(3.906)	0.367	18.907
4	3.706	(5.296)	0.484	28.901
5	4.806	(6.731)	0.475	41.043
6	6.181	(8.149)	0.448	54.294
7	6.353	(9.674)	0.511	69.730
8	6.106	(11.211)	0.586	85.634
9	5.421	(12.730)	0.670	103.180
10	6.577	(14.271)	0.645	122.215
11	8.852	(15.871)	0.577	143.357
12	10.091	(17.425)	0.563	164.535
N				**5,278**

Note: The evaluation sample consists of high school dropouts who registered for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes jobseekers aged between 18 and 19 years and 6 months, while the control group comprises 20-year-old jobseekers. Age is measured at the end of the month of first registration at the PES. Estimates are obtained from the benchmark Difference-in-Differences (DiD) model (Equation 1), estimated separately for men and women at each time horizon. The second column reports the coefficient of the interaction term between the treatment and post-reform dummies. along with its standard errors. The third column presents the p-value. and the fourth shows the counterfactual value of the outcome. The counterfactual is predicted using the benchmark DiD model (Equation 1), setting the interaction between the treatment and post-reform dummies to zero.

Table A12. Proportion of individuals residing with their parents

	Women	***Men***
End of registration year	76.3	90
End of 1st calendar year following registration year	55	76.6
End of 2nd calendar year following registration year	45.7	69.3
N	**3,668**	**5,278**

Note: The evaluation sample consists of high school dropouts registering for the first time at the PES between September 2012 and August 2015 with negligible work experience. Residence at the parental home is measured annually on the 31st of December. This table gives the proportion of individuals of the evaluation sample residing with their parents at the end of the year of first registration at the PES and in the two following years, separately for men and women.

Table A13. Proportion of the cumulative number of days worked by sector, disaggregated by gender (%)

	Women	***Men***
1. Industry	3.3	11.2
2. Construction	0.3	10.4
3. Retail and trade	30.3	16.8
4. Transport	1.4	5.6
5. Accommodation and food	10.4	11.2
6. Information and communication	0.1	0.6
7. Financial and insurance activities	0.5	0.0
8. Real estate	0.7	0.7
9. Science and services	28.4	29.8
10. Non-profit	18.9	11.2
11. Other services	5.7	2.4
Total	**100**	**100**

Note: The evaluation sample consists of high school dropouts registering for the first time at the PES between September 2012 and August 2015, with negligible work experience. For each sector and gender, I estimate the counterfactual number of days worked in a given sector by the treatment group in the absence of the reform. I then compare this to the total number of days worked across all sectors in order to measure the relative share of that sector in total employment. The counterfactual is predicted using the benchmark DiD model (Equation 1), setting the interaction terms between the treatment and post-reform dummies to zero. For women, N = 3,668; for men, N = 5,278.

Table A14. Sectoral classifications

Major Sectors	*Corresponding 21 NACE Sections*
1. Industry	A - Agriculture, forestry, and fishing
	C - Manufacturing
	D - Electricity, gas, steam, and air conditioning supply
	E - Water supply; sewerage, waste management, and remediation activities
2. Construction	F - Construction
3. Trade	G - Wholesale and retail trade; repair of motor vehicles and motorcycles
4. Transport	H - Transportation and storage
5. Accommodation and food	I - Accommodation and food service activities
6. Information and communication	J - Information and communication
7. Financial and insurance activities	K - Financial and insurance activities
8. Real estate	L - Real estate activities
9. Science and services	M - Professional, scientific, and technical activities
	N - Administrative and support service activities
10. Non-profit	O - Public administration and defence; compulsory social security
	P - Education
	Q - Human health and social work activities
11. Other services	R - Arts, entertainment, and recreation
	S - Other service activities
	T - Activities of households as employers; undifferentiated goods- and services-producing activities of households for own use
	U - Activities of extraterritorial organizations and bodies

Table A15. Descriptive statistics after reweighting

	Pre-reform		*Post-reform*	
	control	treated	Δ control	Δ treated
Age[(b)]	20.35	18.95	20.34	18.97
Women[(a)]	41.22	42.39	-1.66	-2.02
Nationality[(a)]				
Belgian	91.66	92.67	-0.90	-1.33
EU	4.04	4.05	1.45	0.72
Other	4.3	3.19	-0.55	0.61
Migration background[(a)]				
At least one non-Belgian parent	49.96	44.67	2.05	0.89
At least one non-Belgian grand parent	64.7	60.51	-0.16	-0.37
Region of residence[(b)]				
Wallonia	42.08	44.96	0.01	0.60
Flanders	44.43	44.68	-1.05	0.00
Brussels	13.49	10.36	1.04	-0.59
General education track[(b)]	13.83	9.98	11.82	8.89
First registration between July and September	56.62	39.63	0.90	0.23*
Household composition[(c)]				
Child living with parents	80.38	78.61	-0.33	0.00
Single	5.8	7.22	-0.89	0.00
Couple	5.71	5.69	1.16	0.70
House-sharing arrangement	8.10	8.41	0.07	-0.68
Household size[(c)]	3.82	3.73	0.10	0.02
Annual household income	47,014	45,290	269	-807
N	**1,326**	**4,786**	**643**	**2,160**

Note: The evaluation sample consists of high school dropouts who registered for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes jobseekers aged between 18 and 19 years and 6 months, while the control group comprises 20-year-old jobseekers. Household size and household income are expressed in person and euros. For all other variables, the table gives the percentage of individuals in the treatment or control group that belong to the category. Outcomes in the *post-reform period are expressed as changes relative to the outcome in the pre-reform period. * p<0.10, ** p<0.05, and *** p<0.01 in the Δ treated column indicate whether the change in the treatment group significantly differs from the change in the control group.*

(a) Refers to variables measured on January 1 of the registration year. (b) Refers to variables measured at registration at the PES. (c) Refers to variables measured on December 31 of the calendar year before the registration year. Annual household income is defined as the mean income of all household members (including the individual studied) in June of the registration year, adjusted by the corresponding year's price index (reference year 2012), and annualized (multiplied by 12) for reporting purposes over the entire year.

Table A16. Descriptive statistics in the alternative treatment groups

	Pre-reform			***Post-reform***		
	control	treated		Δ control	Δ treated	
		(1)	(2)		(1)	(2)
***Age*(b)**	20.35	18.64	19.24	0.01	0.0	-0.01
***Women*(a)**	40.43	42.94	42.03	-1.47	-5.24	0.54
***Nationality*(a)**						
Belgian	91.73	93.14	92.32	-1.32	-1.75	-1.02
EU	3.99	3.72	4.35	2.14	1.2	0.30
Other	4.29	3.01	3.28	-1.07	0.58	0.68
***Migration background*(a)**						
At least one non-Belgian parent	51.82	43.47	45.79	2.97	0.59	1
At least one non-Belgian grand parent	66.05	59.32	61.5	1.07	1.54	-1.96
***Region of residence*(b)**						
Wallonia	50.56	44.84	45.15	1.04	-1.29	2.06
Flanders	33.21	46.79	42.98	-2.92	1.16**	-1
Brussels	16.23	8.37	11.87	1.88	0.13	-1.06*
***General educational track*(b)**	14.31	9.75	10.89	-1.43	-1.1	-0.05
First registration between July and September	55.77	32.32	46.54	-1.28	0.57**	-0.95
***Household composition*(c)**						
Child living with parents	83.32	77.95	81.01	-0.67	-1.11	-0.94
Single	6.03	6.82	6.02	-1.77	1.07	0.65
Couple	4.32	6.15	5.26	1.16	0.20	1.16
House-sharing arrangement	6.33	8.99	7.68	1.28	-0.09	-0.92
Household size(c)	3.94	3.73	3.87	0.12	-0.05	0.02
Annual household income	46,253	45,300	46,087	428	-2,105	-551
N	**1,343**	**2,259**	**2,259**	**657**	**976**	**1,184**

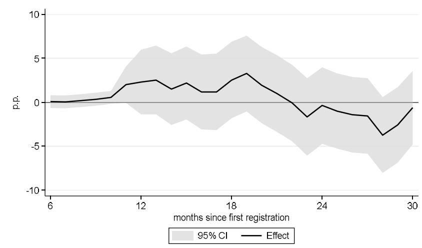
Note: The evaluation sample consists of high school dropouts who registered for the first time at the PES between September 2012 and August 2015 with negligible work experience.

*The treatment group (1) includes jobseekers aged 18 (2) includes individuals aged 19 to 19.5, while the control group comprises 20-year-old jobseekers. Household size and household income are expressed in person and euros. For all other variables, the table gives the percentage of individuals in the treatment or control group that belong to the category. Outcomes in the post-reform period are expressed as changes relative to the outcome in the pre-reform period. * p<0.10, ** p<0.05, and *** p<0.01 in the two Δ treated columns indicate whether the change in the treatment group significantly differs from the change in the control group.*

(a) Refers to variables measured on January 1 of the registration year. (b) Refers to variables measured at registration at the PES. (c) Refers to variables measured on December 31 of the calendar year before the registration year. Annual household income is defined as the mean income of all household members (including the individual studied) in June of the registration year, adjusted by the corresponding year's price index (reference year 2012), and annualized (multiplied by 12) for reporting purposes over the entire year.

B - Additional figures

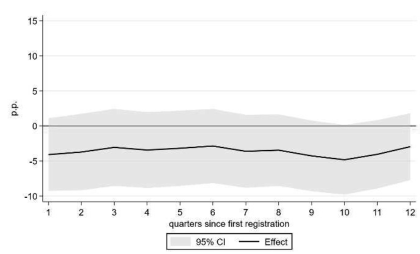
Figure B1. Test for parallel trends - Benefit of the activation allowance

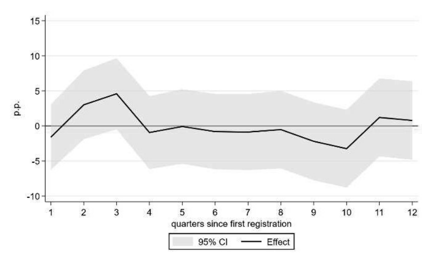


Note: The evaluation sample consists of high school dropouts who registered for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes jobseekers aged between 18 and 19 years and 6 months, while the control group consists of 20-year-old jobseekers. Age is measured at the end of the month of first registration at the PES. The graph displays the coefficient of the interaction term between the treatment and pre-period 1 dummies (δ_{pr}) along with its 95% confidence interval at each time horizon. The graph is derived from the estimation of the benchmark Difference-in-Differences (DiD) model (Equation 1) for each time horizon separately.

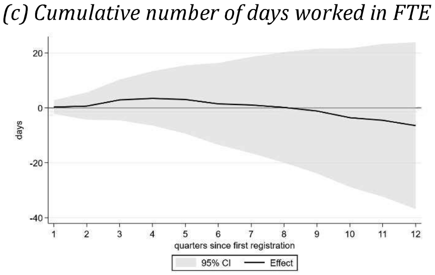
Figure B2. Test for parallel trends – Main employment outcomes

(a) Job-finding rate *(b) Employment rate*





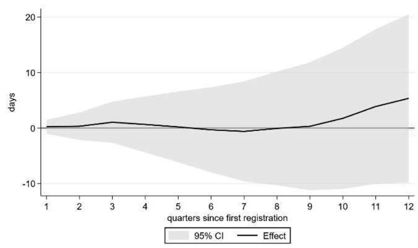
(c) Cumulative number of days worked in FTE



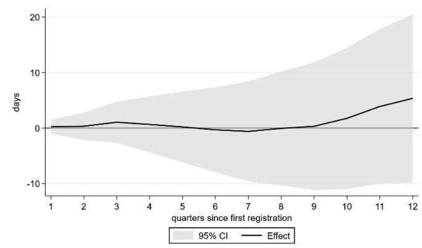
Note: The evaluation sample consists of high school dropouts who registered for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes jobseekers aged between 18 and 19 years and 6 months, while *the control group consists of 20-year-old jobseekers. Age is measured at the end of the month of first registration at the PES. The graph displays the coefficient of the interaction term between the treatment and pre-period 1 dummies (δ_{pre1}) along with its 95% confidence interval at each time horizon. The graph is derived from the estimation of the benchmark Difference-in-Differences (DiD) model (Equation 1) for each time horizon separately.*

Figure B3. Test for parallel trends - Cumulative number of days worked (FTE) by type of job

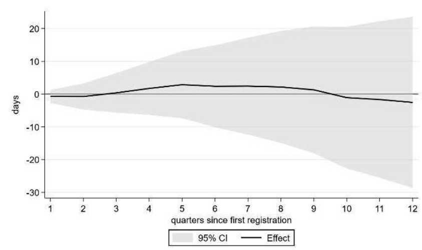
(a) Short-term



(b)Part-time



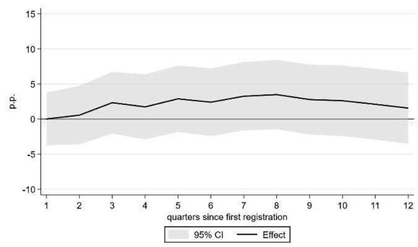
(c) Full-time



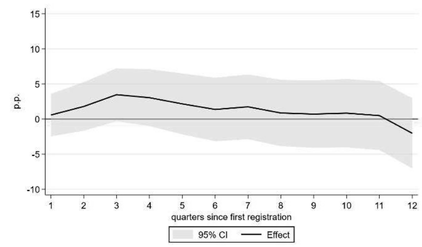
Note: The evaluation sample consists of high school dropouts who registered for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes jobseekers aged between 18 and 19 years and 6 months, while the control group consists of 20-year-old jobseekers. Age is measured at the end of the month of first registration at the PES. The graph displays the coefficient of the interaction term between the treatment and pre-period 1 dummies (δ_{pre1}) along with its 95% confidence interval at each time horizon. The graph is derived from the estimation of the benchmark Difference-in-Differences (DiD) model (Equation 1) for each time horizon separately.

Figure B4. Job-finding rate by type of job

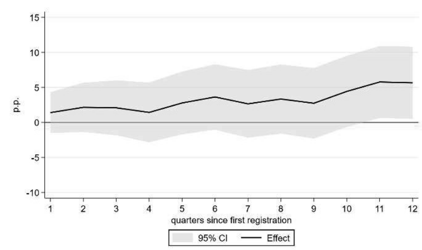
(a) Short-term



(b)Part-time



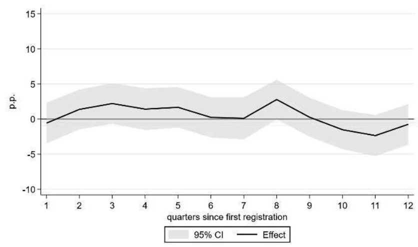
(c) Full-time



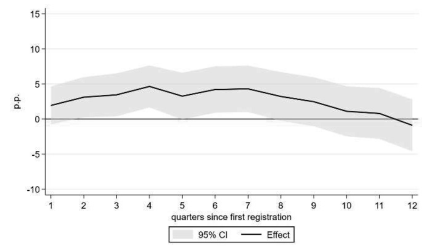
Note: The evaluation sample consists of high school dropouts who registered for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes jobseekers aged between 18 and 19 years and 6 months, while the control group consists of 20-year-old jobseekers. Age is measured at the end of the month of first registration at the PES. All graphs are derived from the estimation of the benchmark Difference-in-Differences (DiD) model (Equation 1) for each outcome variable at each time horizon separately. Each graph displays the coefficient of the interaction term between the treatment and post-reform dummies, along with its 95% confidence interval at each time horizon.

Figure B5. Employment rate by type of job

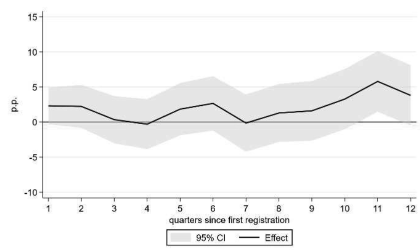
(a) Short-term



(b)Part-time



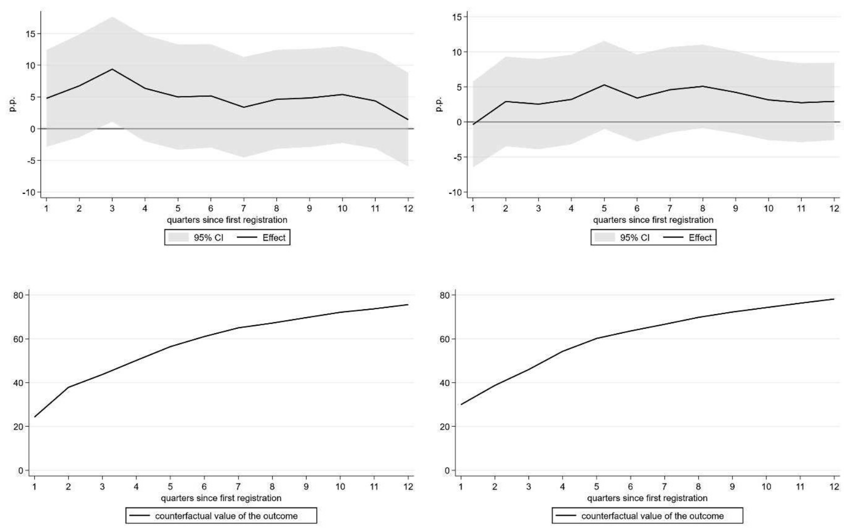
(c) Full-time



Note: The evaluation sample consists of high school dropouts who registered for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes jobseekers aged between 18 and 19 years and 6 months, while the control group consists of 20-year-old jobseekers. Age is measured at the end of the month of first registration at the PES. All graphs are derived from the estimation of the benchmark Difference-in-Differences (DiD) model (Equation 1) for each outcome variable at each time horizon separately. Each graph displays the coefficient of the interaction term between the treatment and post-reform dummies, along with its 95% confidence interval at each time horizon.

Figure B6. Job-finding rate by gender

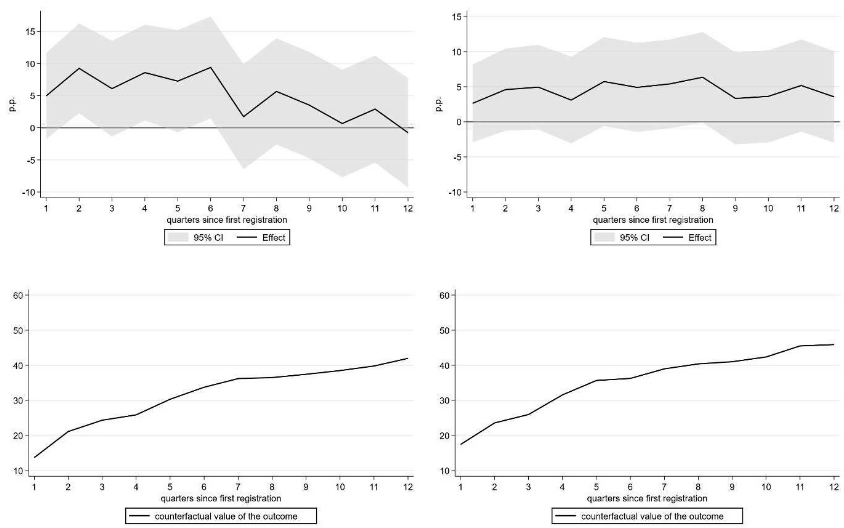
Women *Men*



Note: The evaluation sample consists of high school dropouts registering for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes jobseekers aged between 18 and 19 years and 6 months, while the control group consists of 20-year-old jobseekers. Age is measured at the end of the month of first registration at the PES. All graphs are derived from the estimation of the benchmark Difference-in-Differences (DiD) model (Equation 1) for each time horizon separately. Estimations are conducted separately for men and women, with sample sizes of N = 3,668 for women and N = 5,278 for men. The two graphs on the left refer to women, while the two on the right refer to men. For each gender, the upper graph displays the coefficient of the interaction term between the treatment and post-reform dummies, along with its 95% confidence interval at each time horizon. The lower graph depicts the counterfactual outcome of the treatment group in absence of the reform. The counterfactual is predicted using the benchmark DiD model (Equation 1) setting the interaction effects between the treatment and post-reform dummies equal to zero.

Figure B7. Employment rate by gender

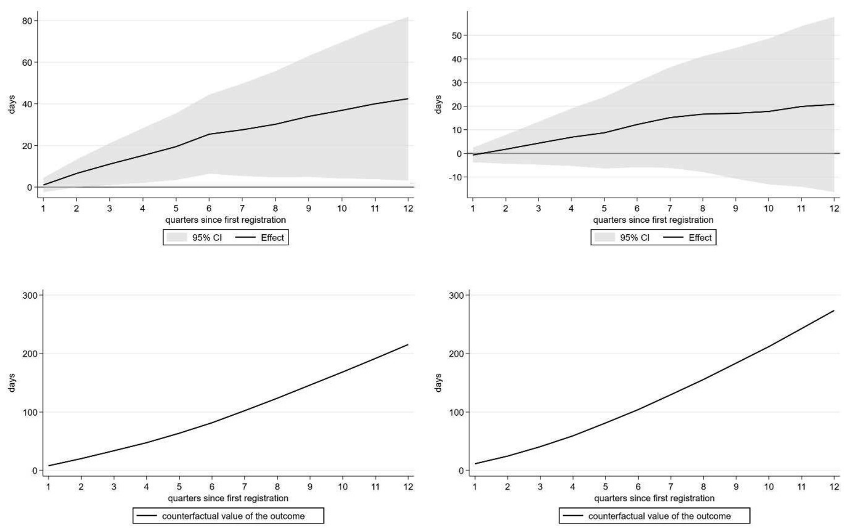
Women *Men*



Note: The evaluation sample consists of high school dropouts registering for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes jobseekers aged between 18 and 19 years and 6 months, while the control group consists of 20-year-old jobseekers. Age is measured at the end of the month of first registration at the PES. All graphs are derived from the estimation of the benchmark Difference-in-Differences (DiD) model (Equation 1) for each time horizon separately. Estimations are conducted separately for men and women, with sample sizes of N = 3,668 for women and N = 5,278 for men. The two graphs on the left refer to women, while the two on the right refer to men. For each gender, the upper graph displays the coefficient of the interaction term between the treatment and post-reform dummies, along with its 95% confidence interval at each time horizon. The lower graph depicts the counterfactual outcome of the treatment group in absence of the reform. The counterfactual is predicted using the benchmark DiD model (Equation 1) setting the interaction effects between the treatment and post-reform dummies equal to zero.

Figure B8. Cumulative number of days worked (FTE)

Women *Men*

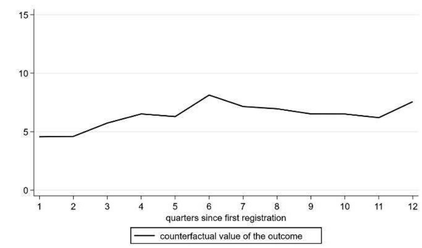


Note: The evaluation sample consists of high school dropouts registering for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes jobseekers aged between 18 and 19 years and 6 months, while the control group consists of 20-year-old jobseekers. Age is measured at the end of the month of first registration at the PES. All graphs are derived from the estimation of the benchmark Difference-in-Differences (DiD) model (Equation 1) for each time horizon separately. Estimations are conducted separately for men and women, with sample sizes of N = 3,668 for women and N = 5,278 for men. The two graphs on the left refer to women, while the two on the right refer to men. For each gender, the upper graph displays the coefficient of the interaction term between the treatment and post-reform dummies, along with its 95% confidence interval at each time horizon. The lower graph depicts the counterfactual outcome of the treatment group in absence of the reform. The counterfactual is predicted using the benchmark DiD model (Equation 1) setting the interaction effects between the treatment and post-reform dummies equal to zero.

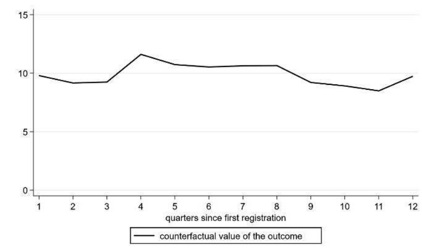
Figure B9. Counterfactual employment rate in (a) short-term (b) part-time (c) full-time jobs by gender

(a) Short-term

Women

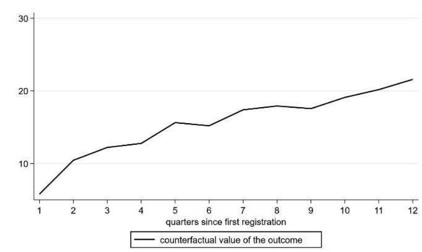


Men

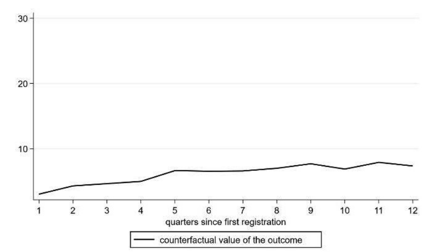


(b) Part-time

Women

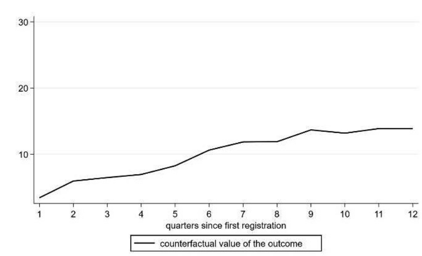


Men

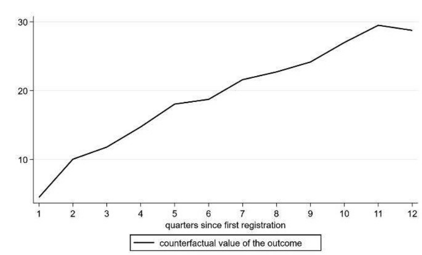


(c) Full-time

Women



Men



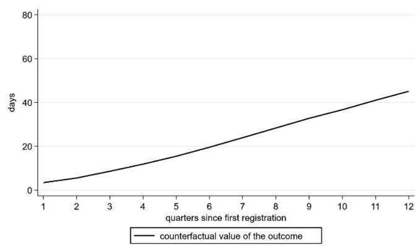
Note: The evaluation sample consists of high school dropouts registering for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes jobseekers aged between 18 and 19 years and 6 months, while the control group consists of 20-year-old jobseekers. Age is measured at the end of the month of first registration at the PES. All graphs are derived from the estimation of the benchmark

Difference-in-Differences (DiD) model (Equation 1) for each outcome variable at each time horizon separately. Estimations are conducted separately for men and women, with sample sizes of N = 3,668 for women and N = 5,278 for men. The graphs on the left refer to women, while the graphs on the right refer to men. In all cases, the graph depicts the counterfactual outcome of the treatment group in absence of the reform. The counterfactual is predicted using the benchmark DiD model (Equation 1) setting the interaction effects between the treatment and post-reform dummies equal to zero.

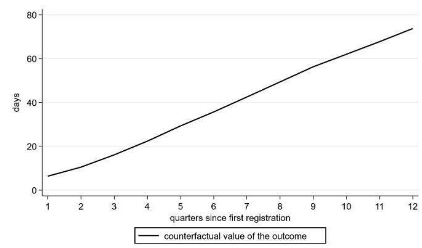
Figure B10. Counterfactual cumulative number of days worked in (a) short-term (b) part-time (c) full-time jobs by gender

(a) Short-term

Women

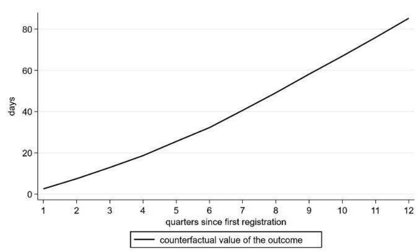


Men

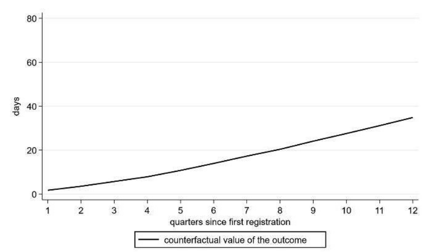


(b) Part-time

Women

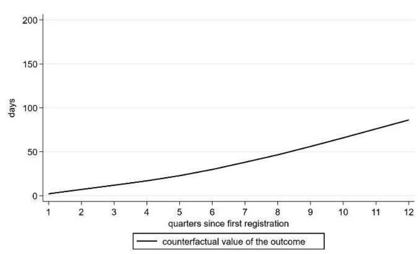


Men

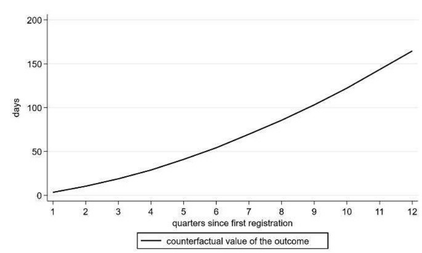


(c) Full-time

Women



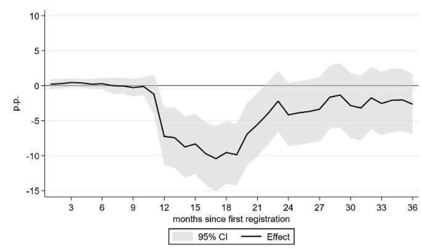
Men



Note: The evaluation sample consists of high school dropouts registering for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes jobseekers aged between 18 and 19 years and 6 months, while the control group consists of 20-year-old jobseekers. Age is measured at the end of the month of first registration at the PES. All graphs are derived from the estimation of the benchmark

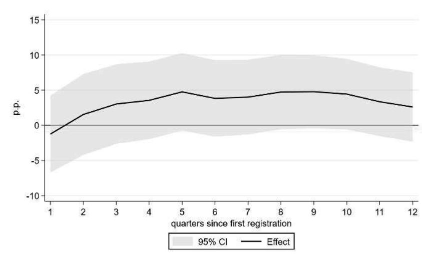
Difference-in-Differences (DiD) model (Equation 1) for each outcome variable at each time horizon separately. Estimations are conducted separately for men and women, with sample sizes of N = 3,668 for women and N = 5,278 for men. The graphs on the left refer to women, while the graphs on the right refer to men. In all cases, the graph depicts the counterfactual outcome of the treatment group in absence of the reform. The counterfactual is predicted using the benchmark DiD model (Equation 1) setting the interaction effects between the treatment and post-reform dummies equal to zero.

Figure B11. Benefit of the activation allowance



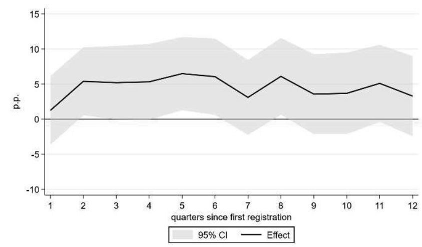
Note: The evaluation sample consists of high school dropouts registering for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes jobseekers aged between 18 and 19 years and 6 months, while the control group consists of 20-year-old jobseekers. Age is measured at the end of the month of first registration at the PES. The graph presents treatment effects estimated separately for each time horizon using the weighted Difference-in-Differences model described in Appendix D, along with their corresponding 95% confidence intervals.

Figure B12. Job-finding rate



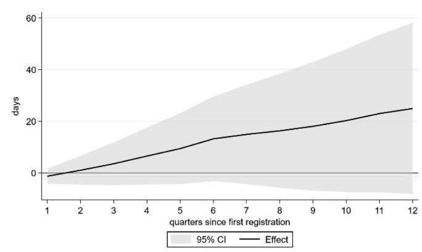
Note: The evaluation sample consists of high school dropouts registering for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes jobseekers aged between 18 and 19 years and 6 months, while the control group consists of 20-year-old jobseekers. Age is measured at the end of the month of first registration at the PES. The graph presents treatment effects estimated separately for each time horizon using the weighted Difference-in-Differences model described in Appendix D, along with their corresponding 95% confidence intervals.

Figure B13. Employment rate



Note: The evaluation sample consists of high school dropouts registering for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes jobseekers aged between 18 and 19 years and 6 months, while the control group consists of 20-year-old jobseekers. Age is measured at the end of the month of first registration at the PES. The graph presents treatment effects estimated separately for each time horizon using the weighted Difference-in-Differences model described in Appendix D, along with their corresponding 95% confidence intervals.

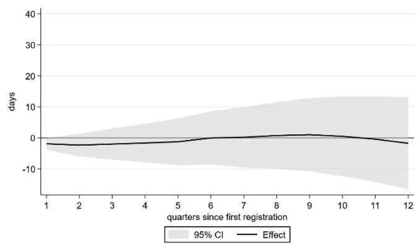
Figure B14. Cumulative number of days worked in FTE



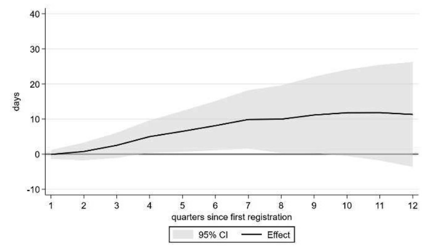
Note: The evaluation sample consists of high school dropouts registering for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes jobseekers aged between 18 and 19 years and 6 months, while the control group consists of 20-year-old jobseekers. Age is measured at the end of the month of first registration at the PES. The graph presents treatment effects estimated separately for each time horizon using the weighted Difference-in-Differences model described in Appendix D, along with their corresponding 95% confidence intervals.

Figure B15. Cumulative number of days worked in FTE by type of job

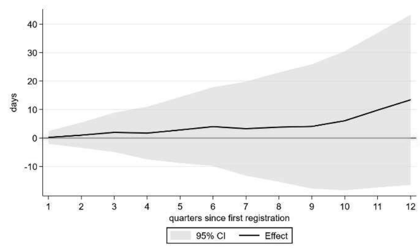
(a) Short-term



(b)Part-time

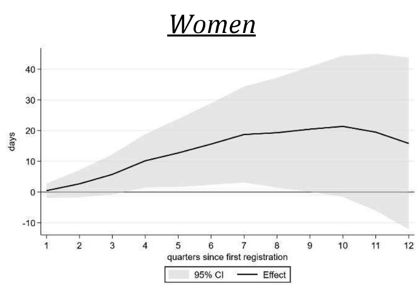


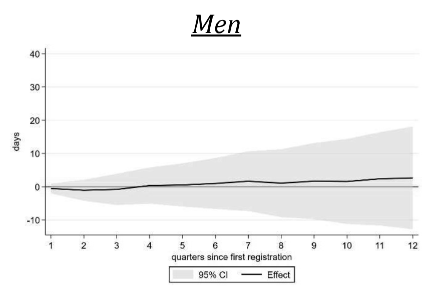
(c) Full-time



Note: The evaluation sample consists of high school dropouts registering for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes jobseekers aged between 18 and 19 years and 6 months, while the control group consists of 20-year-old jobseekers. Age is measured at the end of the month of first registration at the PES. The graph presents treatment effects estimated separately for each time horizon using the weighted Difference-in-Differences model described in Appendix D, along with their corresponding 95% confidence intervals.

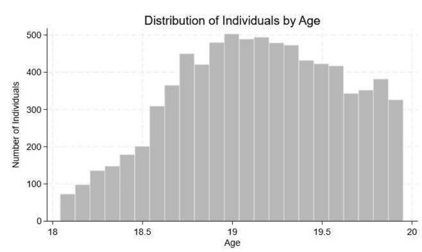
Figure B16. Cumulative number of days worked in part-time jobs by gender





Note: The evaluation sample consists of high school dropouts registering for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes jobseekers aged between 18 and 19 years and 6 months, while the control group consists of 20-year-old jobseekers. Age is measured at the end of the month of first registration at the PES. The graph presents treatment effects estimated separately for each time horizon using the weighted Difference-in-Differences model described in Appendix D, along with their corresponding 95% confidence intervals. Estimations are conducted separately for men and women.

Figure B17. Distribution of age in the treatment group

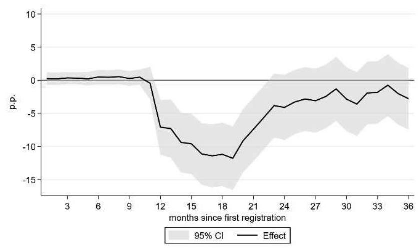


Note: Distribution of age in months among high school dropouts registering for the first time at the PES between September 2012 and August 2015 with negligible work experience and aged below 20.

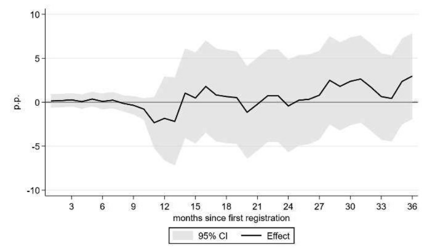
Figure B18. Benefit of the activation allowance

(a) 6 to 12 months (19 to 19.5)

Treatment effect

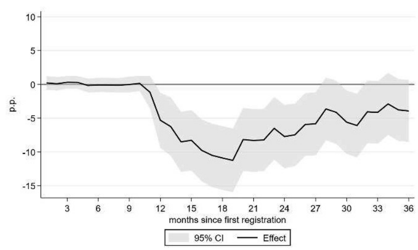


Parallel trends

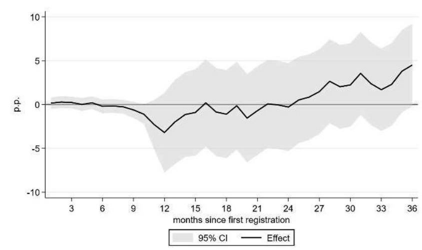


(b) 13 to 24 months (18)

Treatment effect



Parallel trends

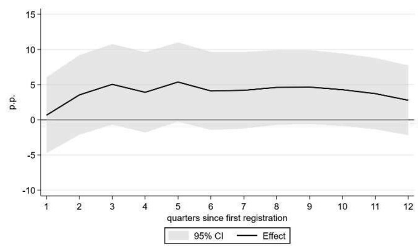


Note: The evaluation sample consists of high school dropouts registering for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes jobseekers aged (a) between 19 and 19 years and 6 months (b) 18 years, while the control group consists of 20-year-old jobseekers. Age is measured at the end of the month of first registration at the PES. All graphs are derived from the estimation of the benchmark Difference-in-Differences (DiD) model (Equation 1) for each time horizon separately. For (a) N=5,070; (b) N=4,637.

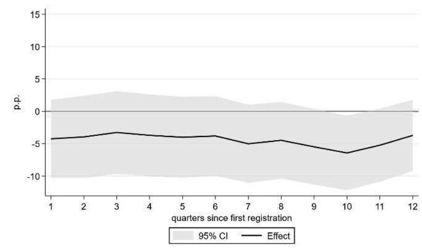
Figure B19. Job-finding rate

(a) 6 to 12 months (19 to 19.5)

Treatment effect

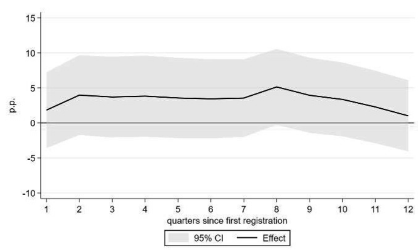


Parallel trends

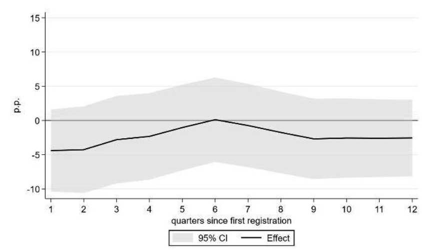


(b) 13 to 24 months (18)

Treatment effect



Parallel trends

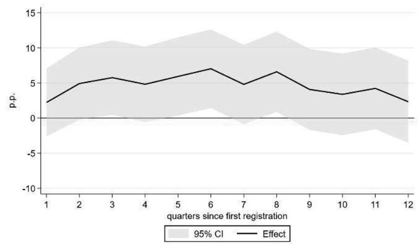


Note: The evaluation sample consists of high school dropouts registering for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes jobseekers aged (a) between 19 and 19 years and 6 months (b) 18 years, while the control group consists of 20-year-old jobseekers. Age is measured at the end of the month of first registration at the PES. All graphs are derived from the estimation of the benchmark Difference-in-Differences (DiD) model (Equation 1) for each time horizon separately. For (a) N=5,070; (b) N=4,637.

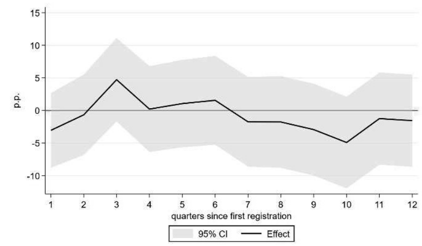
Figure B20. Employment rate

(a) 6 to 12 months (19 to 19.5)

Treatment effect

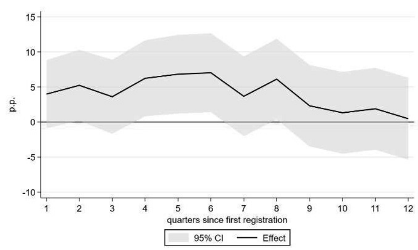


Parallel trends

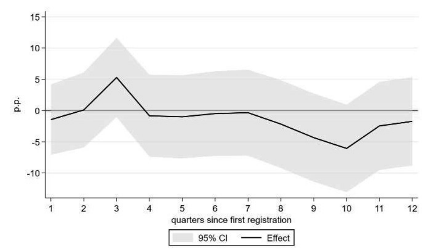


(b) 13 to 24 months (18)

Treatment effect



Parallel trends

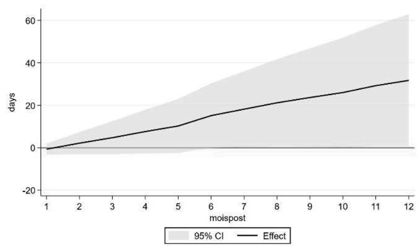


Note: The evaluation sample consists of high school dropouts registering for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes jobseekers aged (a) between 19 and 19 years and 6 months (b) 18 years, while the control group consists of 20-year-old jobseekers. Age is measured at the end of the month of first registration at the PES. All graphs are derived from the estimation of the benchmark Difference-in-Differences (DiD) model (Equation 1) for each time horizon separately. For (a) N=5,070; (b) N=4,637.

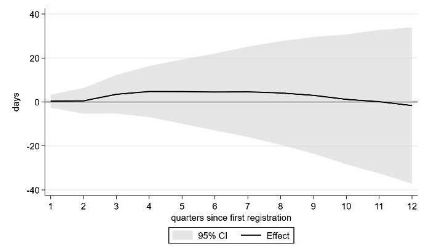
Figure B21. Cumulative number of days worked in FTE

(a) 6 to 12 months (19 to 19.5)

Treatment effect

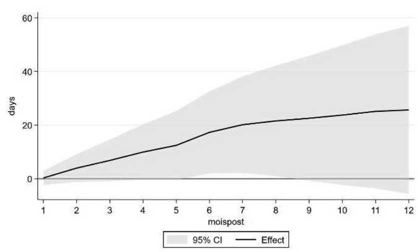


Parallel trends

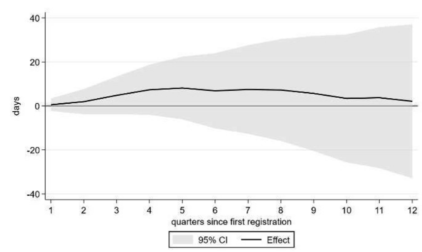


(b) 13 to 24 months (18)

Treatment effect



Parallel trends

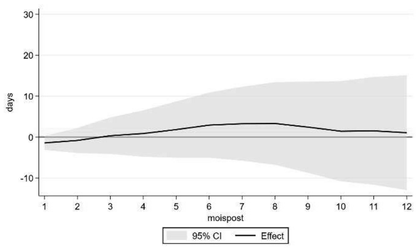


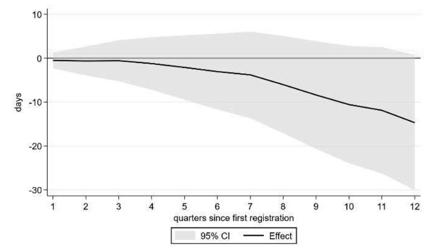
Note: The evaluation sample consists of high school dropouts registering for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes jobseekers aged (a) between 19 and 19 years and 6 months (b) 18 years, while the control group consists of 20-year-old jobseekers. Age is measured at the end of the month of first registration at the PES. All graphs are derived from the estimation of the benchmark Difference-in-Differences (DiD) model (Equation 1) for each time horizon separately. For (a) N=5,070; (b) N=4,637.

Figure B22. Cumulative number of days worked in short-term jobs

(a) 6 to 12 months (19 to 19.5)

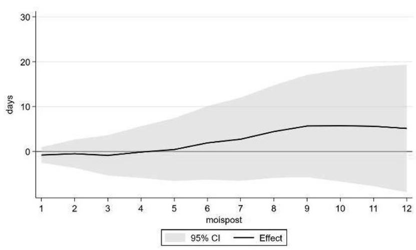
Treatment effect | *Parallel trends*

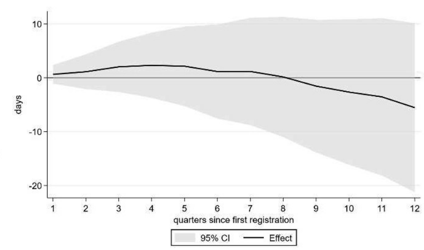




(b) 13 to 24 months (18)

Treatment effect | *Parallel trends*



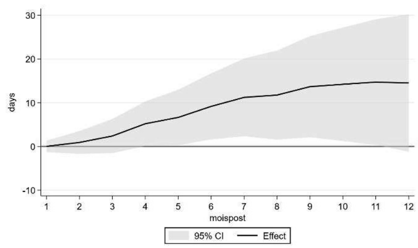


Note: The evaluation sample consists of high school dropouts registering for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes jobseekers aged (a) between 19 and 19 years and 6 months (b) 18 years, while the control group consists of 20-year-old jobseekers. Age is measured at the end of the month of first registration at the PES. All graphs are derived from the estimation of the benchmark Difference-in-Differences (DiD) model (Equation 1) for each time horizon separately. For (a) N=5,070; (b) N=4,637.

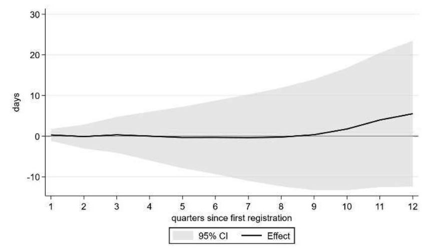
Figure B23. Cumulative number of days worked in part-time jobs

(a) 6 to 12 months (19 to 19.5)

Treatment effect

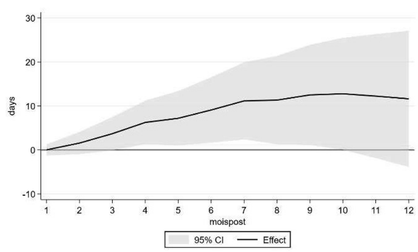


Parallel trends

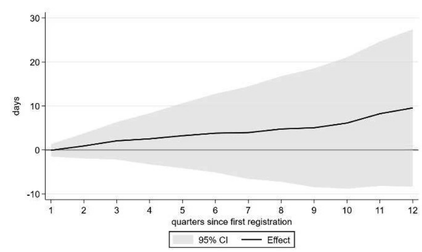


(b) 13 to 24 months (18)

Treatment effect



Parallel trends

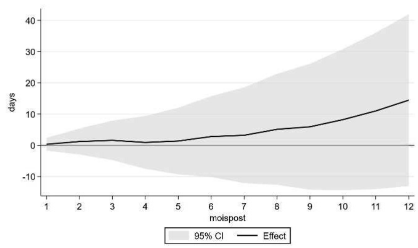


Note: The evaluation sample consists of high school dropouts registering for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes jobseekers aged (a) between 19 and 19 years and 6 months (b) 18 years, while the control group consists of 20-year-old jobseekers. Age is measured at the end of the month of first registration at the PES. All graphs are derived from the estimation of the benchmark Difference-in-Differences (DiD) model (Equation 1) for each time horizon separately. For (a) N=5,070; (b) N=4,637.

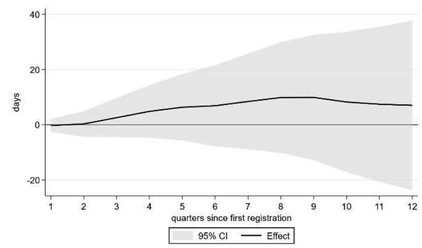
Figure B24. Cumulative number of days worked in full-time jobs

(a) 6 to 12 months (19 to 19.5)

Treatment effect

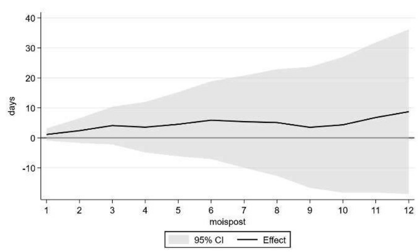


Parallel trends

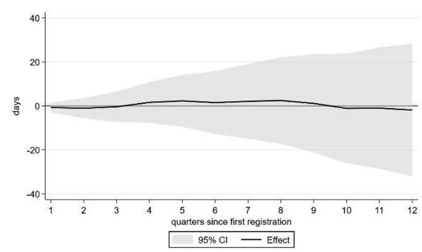


(b) 13 to 24 months (18)

Treatment effect



Parallel trends

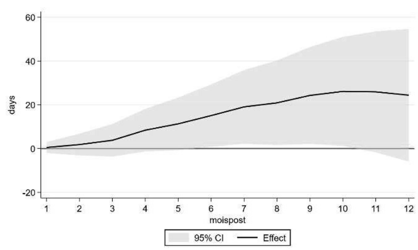


Note: The evaluation sample consists of high school dropouts registering for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes jobseekers aged (a) between 19 and 19 years and 6 months (b) 18 years, while the control group consists of 20-year-old jobseekers. Age is measured at the end of the month of first registration at the PES. All graphs are derived from the estimation of the benchmark Difference-in-Differences (DiD) model (Equation 1) for each time horizon separately. For (a) N=5,070; (b) N=4,637.

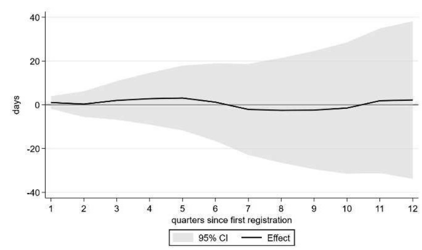
Figure B25. Cumulative number of days worked in part-time jobs for women

(a) 6 to 12 months (19 to 19.5)

Treatment effect

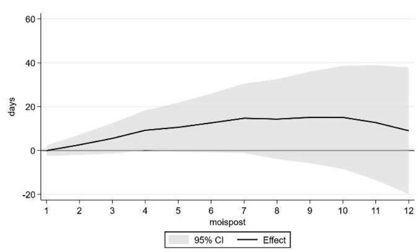


Parallel trends

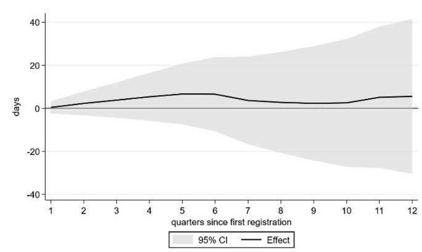


(b) 13 to 24 months (18)

Treatment effect



Parallel trends

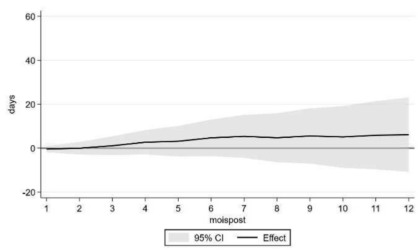


Note: The evaluation sample consists of high school dropouts registering for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes jobseekers aged (a) between 19 and 19 years and 6 months (b) 18 years, while the control group consists of 20-year-old jobseekers. Age is measured at the end of the month of first registration at the PES. All graphs are derived from the estimation of the benchmark Difference-in-Differences (DiD) model (Equation 1) for each time horizon separately. For (a) N=5,070; (b) N=4,637.

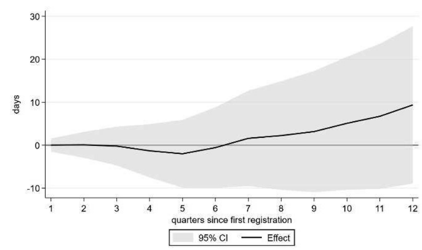
Figure B26. Cumulative number of days worked in part-time jobs for men

(a) 6 to 12 months (19 to 19.5)

Treatment effect

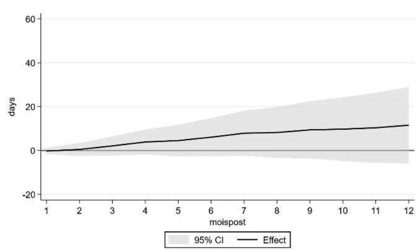


Parallel trends

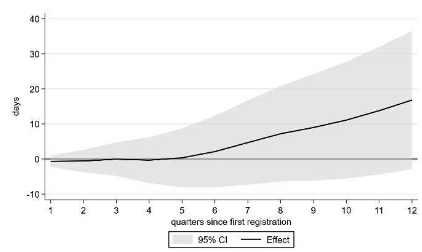


(b) 13 to 24 months (18)

Treatment effect



Parallel trends



Note: The evaluation sample consists of high school dropouts registering for the first time at the PES between September 2012 and August 2015 with negligible work experience. The treatment group includes jobseekers aged (a) between 19 and 19 years and 6 months (b) 18 years, while the control group consists of 20-year-old jobseekers. Age is measured at the end of the month of first registration at the PES. All graphs are derived from the estimation of the benchmark Difference-in-Differences (DiD) model (Equation 1) for each time horizon separately. For (a) N=5,070; (b) N=4,637.

C - The low take-up rate of the activation allowance

In the absence of the reform, 13% of the treatment group would have received the activation allowance three years after first registration at the PES. Although this may appear low given the high prevalence of unemployment among high-school dropouts at the point in time (about 55%), it aligns with aggregate data from the National Employment Office (ONEM) for the same population. From 2012 to 2014, approximately 7,000 out of roughly 50,000 registered jobseekers under age 25 who left secondary school without a diploma received the activation allowance each year, a share close to 14%. The limited take-up observed in the evaluation sample is therefore consistent with administrative statistics.[101]

To shed light on this limited take-up, note that three years after first registration at the PES, only 30% of the treatment group remain registered at the PES, which is required to benefit from the activation allowance.[102] In addition to these 30%, 45% are employed, 3% are self-employed, and 22% are neither working nor registered (many likely returned to education, although this cannot be verified in the data). Among those still registered (30%), just over 40% receive the activation allowance (corresponding to 13% of the treatment group in the counterfactual without reform), 11% receive regular unemployment benefits, 8% receive family allowances, 8% receive means-tested welfare benefits, 1% receive invalidity benefits. The remaining 32% receive no financial support.

Two main reasons may explain why the latter group does not receive the activation allowance despite being unemployed. First, some individuals experience frequent transitions between employment and unemployment—common among low-skilled workers—and may not (yet) have claimed the allowance. Second, others may not meet the

---

[101] Figures on activation-allowance beneficiaries draw on ONEM annual reports and supplementary ONEM tabulations disaggregated by educational attainment. These annual reports are publicly accessible on the ONEM website. The stock of registered jobseekers is estimated from publicly available registration statistics published by the three regional public employment services (PES). Full computational details are available upon request.
[102] Note that the reform has no significant impact on the probability of registration at the PES.

eligibility requirement of two positive evaluations of active job search effort. In the year preceding the reform, a little over 20% of job search evaluations at months 7 and 11 were negative—roughly half due to missed appointments and half due to insufficient search effort. These ONEM figures cover all jobseekers; among high-school dropouts, the corresponding rates are plausibly higher.

D - The weighted Difference-in-Differences estimation procedure

Building upon Albanese and Cockx (2019), the estimation proceeds as follows:

1. Estimate the propensity score $S(X_i)$ of belonging to the treatment group in the post-reform period by logistic regressions using observed background characteristics X_i for observation i. This estimation is performed separately for each analysis period (the two pre-reform periods and the post-reform period).
2. Trim the data following Crump et al. (2009) by excluding observations with propensity score below 0.1 or above 0.9.
3. Re-estimate propensity scores as in point 1 on the trimmed sample and denote them by p^v where $V = \{T_{pre1};\ C_{pre1}; T_{pre2};\ C_{pre2}; C_{post}\}$
4. Estimate the DiD Equation (1) by weighted least squares using the following normalized weights:
   $$PW_i = \frac{P^v(X_i)}{1 - P^v(X_i)} / \frac{1}{N^v} \sum_{i=1}^{N^v} \frac{P^v(X_i)}{1 - P^v(X_i)} \text{ with } PW_i = 1 \, for \ i \in T_{post}$$
   Where N^v is the number of observations in each group V. Following Busso et al. (2014) the individual weights are normalized by the average weight in that group. This normalization ensures that the within-group average weight equals 1.

General conclusion

The three chapters of this dissertation investigate how the design of hiring subsidies and unemployment benefits shapes the early career trajectories of young labour market entrants. As outlined in the general introduction, the overarching question is how public policies can facilitate smoother school-to-work transitions in order to mitigate the long-term scarring effects of early unemployment, particularly for low-educated youths. Two chapters focus on this latter group, while the third examines university graduates at the onset of their professional lives. To address these issues, the thesis applies rigorous microeconometric methods—regression discontinuity and difference-in-differences designs—to construct credible counterfactuals and identify causal effects of policy interventions.

While the chapters share a common focus on how labour market policies shape early careers, the labour market entry of high school dropouts, high school graduates and master's graduates differs sharply. Distinguishing between these groups is therefore essential for drawing nuanced conclusions and extracting the broader policy lessons of this research.

Chapters 1 and 3 shed light on the labour market integration of young people with limited schooling. These individuals face very high risks of unemployment and scarring effects on long-term employability and earnings, which motivates the design of targeted labour market policies.

Chapter 1 shows that granting early access to hiring subsidies for low-educated youths does not raise job-finding rates or total time spent in employment. Under favourable economic conditions, when labour demand is already high, the subsidies are largely captured by firms without creating additional jobs. For high school graduates, we show that the ineffectiveness arises from the tightness of the labour market. These findings underscore the value of calibrating labour market policies to the business cycle or local labour market conditions, with cyclical or geographical targeting offering one avenue to improve effectiveness. However, according to our findings, such targeting would be effective only for youths with a minimal level of skills. Indeed, for high school dropouts, the results show that granting earlier access to hiring subsidies is ineffective regardless of labour market conditions, underscoring that “work-first” policies alone are insufficient for very low-educated individuals. To improve their long-term employment prospects, policies that enhance marketable skills would likely be more effective. Training and education programmes that equip these youths with the competencies demanded by employers are therefore likely to be more effective than subsidies that simply lower hiring costs.

Chapter 3 also focuses on high school dropouts, but approaches the issue from the supply side of the labour market. It shows that extending the qualifying period granting access to youth-specific unemployment benefits strengthens their work incentives, resulting in higher employment. These employment gains are concentrated among women and are primarily observed in part-time positions, reflecting the initial overrepresentation of women in part-time work. The findings suggest that, for very low-educated youths, tightening eligibility rules can stimulate employment when job opportunities exist. Indeed, women are particularly represented in sectors with high levels of unfilled vacancies. More importantly, the results indicate that the observed employment gains stem from increased job-search intensity rather than from lowered job acceptance criteria, indicating that individuals are not pushed into lower quality jobs—a finding of particular relevance for policymakers. However, policymakers should also be aware that the persistence of the employment effect over time appears limited. Taken together with the fact that employment gains are largely confined to part-time positions, this indicates that the policy reform fails to foster sustainable integration into employment.

A possible explanation for the absence of effects among men is highlighted: the lower availability of jobs in the sectors in which they are more likely to work. This hypothesis warrants further investigation. If confirmed, this would reinforce the argument that training and occupational guidance—particularly towards sectors with more unfilled vacancies—are crucial for improving employment outcomes among very low-educated labour market entrants, potentially yielding more lasting employment gains.

Chapter 2 examines the case of master’s graduates, for whom the transition to employment is generally less difficult, but for whom the main challenge lies in making several important life-course decisions simultaneously. This chapter shows that labour market policies—

through the financial support they provide at a crucial moment in life—can spill over into other life-course choices. Specifically, it finds that for men, tightening unemployment benefits eligibility at the start of their careers delayed nest-leaving by increasing co-residence with parents. For women, by contrast, stricter eligibility did not postpone leaving the parental home. Instead, they increased their employment rate to preserve residential independence.

From a policy perspective, these findings illustrate that seemingly gender-neutral reforms may trigger gender-differentiated behavioural responses. This also underscores the need for policymakers to be mindful of unintended consequences when introducing reforms at times of multiple, interdependent life decisions. The initial objective of the reform was to increase employment. However, the literature suggests that delayed nest-leaving may, in the longer term, reduce labour supply. While my data do not allow me to test this directly, this hypothesis deserves further investigation through longitudinal analyses to assess whether staying longer with parents is ultimately detrimental to men's employment. If so, the original objective of the reform would not be achieved and might even be undermined.

Taken together, the three chapters underscore the importance of heterogeneity in assessing the effectiveness of labour market policies. The results show that the same type of intervention—whether subsidies or benefits reforms—can produce divergent outcomes depending on educational attainment, gender, and local economic conditions. Hiring subsidies fail to improve job finding for high school dropouts, yet may yield modest gains for graduates in some specific economic conditions. Stricter unemployment benefits affect men and women differently: while men delay household formation, women increase their labour supply to preserve autonomy. Extending the qualifying period for youth unemployment benefits entails gender-differentiated effects, with employment gains arising only among women. These findings highlight that labour market policies cannot be evaluated in isolation but must be situated within the broader socioeconomic context in which young people navigate their school-to-work transitions.

This focus on heterogeneity also opens promising avenues for further research. Beyond education, gender, and labour market conditions, other dimensions—such as migration background, socioeconomic status, or family resources—deserve closer scrutiny to ensure that policies do not inadvertently produce unintended consequences on some groups or reinforce pre-existing differences. In this dissertation, heterogeneity by migration background emerges as a particularly important question that deserves closer investigation. Chapters 2 and 3 indicate that a non-negligible share of labour market entrants in Belgium has at least one non-Belgian grandparent—among both high school dropouts and master's graduates. The employment gains observed for certain groups, such as women in Chapter 3, may be less pronounced for individuals with a migration background, as they face greater barriers to employment. The effect on housing autonomy is also likely to differ. On the one hand, the age at which individuals leave the parental home is strongly shaped by cultural norms: young people with a migration background may have different preferences than those without. On the other hand, they may also face different constraints: the data show that household income tends to be lower among households of youths with a migration background. Differences between young people with and without a migration background reasonably suggest that differentiated effects can be expected, though this remains hypothetical and more research is needed to reach robust conclusions.

Beyond heterogeneity, a second avenue for future research relates to the time horizon of the evaluations. The analyses in this thesis remain relatively short, spanning one to three years. Longitudinal analyses would be required to assess whether the observed employment gains and delayed nest-leaving persist, attenuate, or intensify over time, as well as to identify other long-term consequences. Longitudinal analyses would be needed to assess whether observed employment gains, delayed nest-leaving persist, attenuate or intensify over time or other long-term consequences. Finally, while the results point toward the importance of skill enhancement for the most disadvantaged groups, the design of effective training policies—whether through financial incentives, improved guidance, or better alignment with local labour demand—remains an open question that future work should address.

In conclusion, this dissertation contributes to the labour economics literature by providing new causal evidence on how hiring subsidies and unemployment benefits affect the early careers of young people in Belgium. By distinguishing between low-educated youths and university graduates, the dissertation highlights the diversity of challenges facing young labour market entrants and the ways in which policies can interact with their circumstances. The results carry a clear message for policymakers: effective support for youth transitions requires attention to heterogeneity—by education, by gender, and by economic context—alongside a recognition of the broader life-course consequences of labour market reforms.

Bibliography

Aassve, A., Billari, F., Mazzuco, S., & Ongaro, F. (2002). Leaving home: A comparative analysis of ECHP data. Journal of European Social Policy, 12(4), 259-275.

Albanese, A., & Cockx, B. (2019). Permanent wage cost subsidies for older workers. An effective tool for employment retention and postponing early retirement? Labour Economics, 58, 145-166.

Albanese, A., Dejemeppe, M. & Cockx, B. (2024). Long-term effects of hiring subsidies for low-educated unemployed youths. Journal of Public Economics, 235, 105137.

Andersson, A. (2021). Social capital and leaving the nest: Channels and housing tenures. Social Networks, 65, 8-18.

Angelini, V., Bertoni, M., & Weber, G. (2022). The long-term consequences of a golden nest: Socioeconomic status in childhood and the age at leaving home. Demography, 59(3), 857-875.

Aparicio-Fenoll, A., & Oppedisano, V. (2015). Fostering Household Formation: Evidence from a Spanish Rental Subsidy. The B.E. Journal of Economic Analysis & Policy, 15(1), 53-84.

Arslan, Y., Degerli, A., & Kabas, G. (2024). Unintended Consequences of Unemployment Insurance Benefits: The Role of Banks. Management Science.

Arundel, R., & and Ronald, R. (2016). Parental co-residence, shared living and emerging adulthood in Europe: Semi-dependent housing across welfare regime and housing system contexts. Journal of Youth Studies, 19(7), 885-905.

Autor, D., & Houseman, S. (2010). Do temporary-help jobs improve labor market outcomes for low-skilled workers? Evidence from "Work First". American Economic Journal: Applied Economics, 2(3).

Autor, D., Houseman, S. & Kerr, S. (2017). The Effect of Work First Job Placements on the Distribution of Earnings: An Instrumental Variable Quantile Regression Approach. Journal of Labor Economics, 35(1), 149-190.

Avery, R., Goldscheider, F., & Speare, A. (1992). Feathered nest/gilded cage: Parental income and leaving home in the transition to adulthood. Demography, 29(3), 375-388.

Basbug, G., & Fernandez, R. (2025). Gendered Job Search: An Analysis of Gender Differences in Reservation Wages and Job Applications. ILR Review, 78(1), 217-239.

Basten, C., Fagereng, A., & Telle, K. (2014). Cash-on-hand and the Duration of Job Search: Quasi-experimental Evidence from Norway. The economic journal, 124(576), 540-568.

Batut, C. (2021). The longer-term impact of hiring credits. Evidence from France. Labour Economics, 72, 102052.

Behrenz, L., & Månsson, J. (2021). Why don't employers hire long-term unemployed entitled to a wage subsidy? The employer's perspective on subsidised employment. Economic & Industrial Democracy, 44(1), 161-183.

Bell, D., & Blanchflower, D. (2011). Young people and the Great Recession. Oxford Review of Economic Policy, 27(2), 241-267.

Bermúdez-Barrezueta, N., Desiere, S., & Tarullo, G. (2025). Hiring subsidies and temporary work agencies (LIDAM Discussion Paper IRES 2025/03).

Billari, F., & Tabellini, G. (2010). Italians are late: Does it matter? In Demography and the economy (p. 371-412). University of Chicago Press.

Billari, F., Hiekel, N., & Liefbroer, A. C. (2019). The Social Stratification of Choice in the Transition to Adulthood. European Sociological Review, 35(5), 599-615.

Billari, F., Philipov, D., & Baizán, P. (2001). Leaving Home in Europe: The Experience of Cohorts Born Around 1960: Leaving Home in Europe. International Journal of Population Geography, 7(5), 339-356.

Billari. (2004). Becoming an adult in Europe: A macro (/micro)-demographic perspective. Demographic research, 3, 15-44.

Blaauboer, M., & Mulder, C. (2010). Gender differences in the impact of family background on leaving the parental home. Journal of Housing and the Built Environment, 25(1), 53-71.

Bodart, V., Dejemeppe, M. & Van der Linden, B. (2018). The labor market in Belgium, 2000–2016. The IZA World of Labor, 428.

Bolhaar, J., Ketel, N., & van der Klaauw, B. (2019). Job Search Periods for Welfare Applicants: Evidence from a Randomized Experiment. American Economic Journal: Applied Economics, 11(1), 92-125.

Boockmann, B., Zwick, T., Ammermüller, A. & Maier, M. (2012). Do hiring subsidies reduce unemployment among older workers? Evidence from natural experiments. Journal of the European Economic Association, 10(4), 735-764.

Breidenbach, A. (2018). Constrained from leaving or comfortable at home? Young people's explanations for delayed home-leaving in 28 European countries. International Journal of Comparative Sociology, 59(4), 271-300.

Brown, A. (2015). Can hiring subsidies benefit the unemployed. The IZA World of Labor, 163.

Brown, A., & Koettl, J. (2015). Active labor market programs-employment gain or fiscal drain? IZA Journal of Labor Economics, 4, 1-36.

Buck, N., & Scott, J. (1993). She's leaving home : But why? An analysis of young people leaving the parental home. Journal of Marriage and the Family, 863-874.

Cahuc, P., Carcillo, S. & Minea, A. (2021). The Difficult School-to-Work Transition of High School Dropouts: Evidence from a Field Experiment. Journal of Human Resources, 56(1), 159-183.

Cahuc, P., Carcillo, S. and Le Barbanchon, T. (2019). The effectiveness of hiring credits. The Review of Economic Studies, 86(2), 593-626.

Caliendo, M., & Schmidl, R. (2016). Youth unemployment and active labor market policies in Europe. IZA Journal of Labor Policy, 5(1), 1-30.

Caliendo, M., Lee, W., & Mahlstedt, R. (2017). The gender wage gap and the role of reservation wages: New evidence for unemployed workers. Journal of Economic Behavior & Organization, 136, 161-173.

Callaway, B., Goodman-Bacon, A., & Sant'Anna, P. (2024). Difference-in-differences with a continuous treatment (NBER Working Paper 32117).

Calonico, S., Cattaneo, M., Farrell, M., & Titiunik, R. (2017). rdrobust: Software for regression-discontinuity designs. The Stata Journal, 17(2), 372-404.

Card, D., Kluve, J., & Weber, A. (2018). What works? A meta analysis of recent active labor market program evaluations. Journal of the European Economic Association, 16(3), 894-931.

Cattaneo, M., Idrobo, N., & Titiunik, R. (2019). A practical introduction to regression discontinuity designs: Foundations. Cambridge University Press.

Chatterji, P., Liu, X., & Yörük, B. (2022). Health insurance and the boomerang generation: Did the 2010 ACA dependent care provision affect geographic mobility and living arrangements among young adults? Contemporary Economic Policy, 40(2), 243-262.

Chetty, R. (2008). Moral hazard versus liquidity and optimal unemployment insurance. Journal of political Economy, 116(2), 173-234.

Chiuri, M., & Del Boca, D. (2010). Home-leaving decisions of daughters and sons (IZA Discussion Papers 4867).

Choi, S., & Valladares-Esteban, A. (2020). On households and unemployment insurance. Quantitative Economics, 11(1), 437-469.

Clark, V., Tuffin, K., Bowker, N., & Frewin, K. (2018). A fine balance: A review of shared housing among young adults. Social and Personality Psychology Compass, 12(10), e12415.

Cobb-Clark, D. (2008). Leaving Home: What Economics Has to Say about the Living Arrangements of Young Australians. Australian Economic Review, 41(2), 160-176.

Cockx, B., & Desiere, S. (2022). How effective are hiring subsidies in reducing long-term unemployment among prime-aged jobseekers? Evidence from Belgium. IZA Journal of Labor Policy, 12(1).

Cockx, B., & Ghirelli, C. (2016). Scars of recessions in a rigid labor market. Labour Economics, 41, 162-176.

Cockx, B., & Van Belle, E. (2019). Waiting longer before claiming, and activating youth: No point? International Journal of Manpower, 40(4), 658-687.

Cockx, B., Declercq, K. & Dejemeppe, M. (2023). Tightening eligibility requirements for unemployment benefits. Impact on educational attainment. Economics of Education Review, 95, 102424.

Consuelo Colom, A., & Cruz Molés, M. (2024). Gender gap in residential independence and employment of young people. The Economic and Labour Relations Review, 35(1), 140-162.

Cortés, P., Pan, J., Pilossoph, L., Reuben, E., & Zafar, B. (2023). Gender Differences in Job Search and the Earnings Gap : Evidence from the Field and Lab*. The Quarterly Journal of Economics, 138(4), 2069-2126.

Crump, R., Hotz, V., Imbens, G., & Mitnik, O. (2009). Dealing with limited overlap in estimation of average treatment effects. *Biometrika*, *96*(1), 187-199.

Cylus, J., & Avendano, M. (2017). Receiving Unemployment Benefits May Have Positive Effects On The Health Of The Unemployed. Health Affairs, 36(2), 289-296.

De Groot, N., & Van der Klaauw, B. (2019). The effects of reducing the entitlement period to unemployment insurance benefits. Labour Economics, 57, 195-208.

Declercq, K., Dejemeppe, M., & Pourtois, M. (forthcoming). Tightening Eligibility Requirements for Unemployment Benefits: Impact on Housing Autonomy of Young Labour Market Entrants.

Dejemeppe, M., Delpierre, M., & Pourtois, M. (2025). Hiring subsidies for low-educated unemployed youths are ineffective in a tight labor market. The BE Journal of Economic Analysis & Policy, (0).

del Rey, A., Stanek, M., & García-Gómez, J. (2023). Long goodbyes: Pathways of leaving home by gender and destination in Spain. European Sociological Review, 39(6), 973-991.

Eriksson, S., & Lagerström, J. (2012). The Labor Market Consequences of Gender Differences in Job Search. Journal of Labor Research, 33(3), 303-327.

Eurostat. (2023). Unemployment rate by sex, age, educational attainment level and NUTS 2 regions (%) (LFST_R_LFU3RT). https://ec.europa.eu/eurostat/databrowser/view/LFST_R_LFU3RT__custom_3665839/default/table?lang=en (accessed March 16, 2023).

Eurostat. (2024). Estimated average age of young people leaving the parental household by sex (yth_demo_030). https://ec.europa.eu/eurostat/databrowser/view/YTH_DEMO_030__custom_12831160/default/table?lang=en (accessed June 20, 2024).

Eurostat. (2025a). Young persons neither in employment nor in education and training by educational attainment level (NEET rates) (edat_lfse_21) https://ec.europa.eu/eurostat/databrowser/view/edat_lfse_21__custom_18048150/default/table (accessed on September 15, 2025).

Eurostat. (2025b). Unemployment rate by sex, age, educational attainment level and NUTS 2 regions (%) (lfst_r_lfu3rt). https://ec.europa.eu/eurostat/databrowser/view/LFST_R_LFU3RT__custom_3665839/default/table?lang=en (accessed September 15, 2025).

Eurostat. (2025c). Share of young adults aged 18-34 living with their parents by self-defined current economic status (ilc_lvps09). https://ec.europa.eu/eurostat/databrowser/view/ilc_lvps09__custom_17409987/default/table?lang=en (accessed August 4, 2025)

Eurostat. (2025d). Unemployment by age and sex (une_rt_a). https://ec.europa.eu/eurostat/databrowser/view/une_rt_a__custom_17386367/default/table?lang=en (accessed September 2, 2025)

Eurostat. (2025e). Population by educational attainment level, sex and age (%) (edat_lfse_03). https://ec.europa.eu/eurostat/databrowser/view/edat_lfse_03/default/table?lang=en (accessed on February 10, 2025).

Eurostat. (2025f). Unemployment rate by sex, age, educational attainment level and NUTS 2 regions (%) (lfst_r_lfu3rt). https://ec.europa.eu/eurostat/databrowser/view/LFST_R_LFU3RT__custom_3665839/default/table?lang=en (accessed May 9, 2025).

Forem. (2018). Le dispositif « Impulsion » - Une première évaluation.

Forem. (2022). Insertion au travail des jeunes Wallons inscrits comme demandeurs d’emploi pour la première fois en 2021. https://www.leforem.be/content/dam/leforem/fr/documents/chiffres-et-analyses/analyse-insertion-jeunes-2021.pdf.

Frijters, P., Johnston, D., & Shields, M. (2014). The Effect of Mental Health on Employment: Evidence from Australian Panel Data. Health Economics, 23(9), 1058-1071.

Gerard, F., & Gonzaga, G. (2021). Informal Labor and the Efficiency Cost of Social Programs: Evidence from Unemployment Insurance in Brazil. American Economic Journal: Economic Policy, 13(3), 167-206.

Gierveld, J., Liefbroer, A., & Beekink, E. (1991). The Effect of Parental Resources on Patterns of Leaving Home among Young Adults in the Netherlands. European Sociological Review, 7(1), 55-71.

Glatt, J., & Wunnava, P. (2016). Help Not Wanted: The Dismal Science of Youth Unemployment's Scarring Effect (IZA Discussion Papers 10069).

Goldin, C. (2014). A Grand Gender Convergence: Its Last Chapter. American Economic Review, 104(4), 1091-1119.

Halla, M., Lackner, M., & Scharler, J. (2016). Does the welfare state destroy the family? Evidence from OECD member countries. The Scandinavian Journal of Economics, 118(2), 292-323.

Hirano, K., Imbens, G., & Ridder, G. (2003). Efficient Estimation of Average Treatment Effects Using the Estimated Propensity Score. Econometrica, 71(4), 1161-1189.

Holm, S. (1979). A simple sequentially rejective multiple test procedure. Scandinavian Journal of Statistics, 6(2), 65–70.

Horvitz, D., & Thompson, D. (1952). A Generalization of Sampling Without Replacement from a Finite Universe. Journal of the American Statistical Association, 47(260), 663-685.

Howard, A., Li, A., & Bentley, R. (2023). Parental co-residence and young adults' mental health. Plos one, 18(11).

Iacovou, M. (2010). Leaving home: Independence, togetherness and income. Advances in Life Course Research, 15(4), 147-160.

Jacob, M., & Kleinert, C. (2008). Does unemployment help or hinder becoming independent? The role of employment status for leaving the parental home. European sociological review, 24(2), 141-153.

Kaplan, G. (2012). Moving Back Home: Insurance against Labor Market Risk. Journal of Political Economy, 120(3), 446-512.

Kitao, S., Şahin, A. & Song. S. (2011). Hiring subsidies, job creation and job destruction. Economics Letters, 113(3), 248-251.

Kline, P., & Moretti, M. (2013). Place based policies with unemployment. American Economic Review, 103(3), 238-243.

Landais, C. (2015). Assessing the Welfare Effects of Unemployment Benefits Using the Regression Kink Design. American Economic Journal: Economic Policy, 7(4), 243-278.

Landaud, F. (2021). From employment to engagement? Stable jobs, temporary jobs, and cohabiting relationships. Labour Economics, 73, 102077.

Le Barbanchon, T. (2016). The effect of the potential duration of unemployment benefits on unemployment exits to work and match quality in France. Labour Economics, 42, 16-29.

Le Barbanchon, T., Rathelot, R., & Roulet, A. (2021). Gender Differences in Job Search: Trading off Commute against Wage*. The Quarterly Journal of Economics, 136(1), 381-426.

Leboutte, R. (2009). A space of European de-industrialisation in the late twentieth century: Nord/Pas-de-Calais, Wallonia and the Ruhrgebiet. European Review of History: Revue Europeenne d'histoire, 16(5), 755-770.

Lee, D., & Card, D. (2008). Regression discontinuity inference with specification error. Journal of Econometrics, 142(2), 655-674.

Lee, K., & Painter, G. (2010). What Happens to Household Formation in a Recession. Journal of Urban Economics.

Lehwess-Litzmann, R., & Nicaise, I. (2020). Surprisingly small: Effects of "generous" social benefits on re-employment of (quasi-) jobless households. Journal of International and Comparative Social Policy, 36(1), 76-91.

Matsudaira, J. (2015). Economic conditions and the living arrangements of young adults: 1960 to 2011. Journal of Population Economics, 29(1), 167-195.

McElroy, M. (1985). The Joint Determination of Household Membership and Market Work: The Case of Young Men. Journal of Labor Economics, 3(3), 293-316.

Mortensen, D. (1977). Unemployment Insurance and Job Search Decisions. ILR Review, 30(4), 505-517.

Mulder, C., Clark, W., & Wagner, M. (2002). A comparative analysis of leaving home in the United States, the Netherlands and West Germany. Demographic Research, 7, 565-592.

Neubäumer, R. (2012). Bringing the unemployed back to work in Germany: Training programs or wage subsidies? International Journal of Manpower, 33(2), 159-177.

Neumark, D. (2013). Spurring job creation in response to severe recessions: Reconsidering hiring credits. Journal of Policy Analysis and Management, 32(1), 142-171.

Neumark, D., & Grijalva, D. (2017). The employment effects of state hiring credits. ILR Review, 70(5), 1111-1145. https://doi.org/10.1177/0019793916683930

Observatoire des loyers. (2015). https://slrb-bghm.brussels/sites/default/files/2023-04/observatoire-des-loyers-2015.pdf

OECD. (2020). The Future for Low-Educated Workers in Belgium. Éditions OCDE.

OECD. (2024). Mean age of enrolled students, new entrants and graduates (dsd_eag_uoe_non_fin_stud@df_uoe_nf_mean_age). https://data-explorer.oecd.org (accessed July 17, 2024)

OECD. (2025). Repetition rate by level of education (dsd_eag_uoe_non_fin_stud@df_uoe_nf_dist_rptr). https://data-explorer.oecd.org (accessed May 20, 2025)

ONEM. (2014). L'ONEM en 2014 Volume 2 : indicateurs du marché du travail et évolution des allocations.

Pasquini, A., Centra, M. and Pellegrini, G. (2019). Fighting long-term unemployment: Do we have the whole picture? Labour economics, 61, 101764.

Saydam, A., & Raley, K. (2024). Slow to launch: Young men's parental coresidence and employment outcomes. Journal of Marriage and Family, 86(4), 1009-1033. https://doi.org/10.1111/jomf.13001

Schmieder, J., & von Wachter, T. (2016). The Effects of Unemployment Insurance Benefits: New Evidence and Interpretation. Annual Review of Economics, 8(1), 547-581.

Schmillen, A., & Umkehrer, M. (2017). The scars of youth: Effects of early-career unemployment on future unemployment experience. International Labour Review, 156(3-4), 465-494.

Schünemann, B., Lechner, M. and Wunsch, C. (2015). Do long-term unemployed workers benefit from targeted wage subsidies? German Economic Review, 16(1), 43-64.

Schwanitz, K., & Mulder, C. (2015). Living Arrangements of Young Adults in Europe. Comparative Population Studies, 40(4), Article 4.

Schwanitz, K., Mulder, C., & Toulemon, L. (2017). Differences in leaving home by individual and parental education among young adults in Europe. Demographic Research, 37, 1975-2010.

Schwanitz, K., Rampazzo, F., & Vitali, A. (2021). Unpacking intentions to leave the parental home in Europe using the Generations and Gender Survey. Demographic Research, 45, 17-54.

Seiffge-Krenke, I. (2013). "She's Leaving Home. . ." Antecedents, Consequences, and Cultural Patterns in the Leaving Home Process. Emerging Adulthood, 1(2), 114-124.

Sjögren, A., & Vikström, J. (2015). How long and how much? Learning about the design of wage subsidies from policy changes and discontinuities. Labour Economics, 34, 127-137.

Statbel. (2025a). https://statbel.fgov.be/fr/themes/population/structure-de-la-population/menages#figures (accessed May 16, 2025)

Statbel. (2025b). Diversité selon l'origine en Belgique. https://statbel.fgov.be/fr/themes/population/structure-de-la-population/origine#figures. (accessed on February 4, 2025)

Statbel. (2025c). Salaires mensuels bruts moyens. https://statbel.fgov.be/fr/themes/emploi-formation/salaires-et-cout-de-la-main-doeuvre/salaires-mensuels-bruts-moyens#figures. (accessed on August 27, 2025)

Statbel. (2025d). Emplois Vacants. https://statbel.fgov.be/fr/themes/emploi-formation/marche-du-travail/emplois-vacants#figures (accessed August 21, 2025)

Tatsiramos, K., & van Ours, J. (2014). Labor market effects of unemployment insurance design. Journal of Economic Surveys, 28(2), 284-311.

van den Berg, L. (2020). The impact of student financial aid reforms on leaving home: Evidence from the Netherlands. Population, Space and Place, 26(2), e2281.

van den Berg, L., Kalmijn, M., & Leopold, T. (2018). Family Structure and Early Home Leaving: A Mediation Analysis. European Journal of Population, 34(5), 873-900.

Veldman, K., Reijneveld, S., Verhulst, F., Ortiz, J., & Bültmann, U. (2017). A life course perspective on mental health problems, employment, and work outcomes. Scandinavian Journal of Work, Environment & Health, 43(4), 316-325.

von Buxhoeveden, M. (2019). Unemployment insurance and youth labor market entry (IFAU Working paper 2019:12).

Wiswall, M., & Zafar, B. (2018). Preference for the Workplace, Investment in Human Capital, and Gender*. The Quarterly Journal of Economics, 133(1), 457-507.

World Bank. (2025). GDP growth (annual %) – Belgium. https://data.worldbank.org/indicator/NY.GDP.MKTP.KD.ZG?locations=BE (accessed September 2, 2025)