THE IMPACT OF TIGHTENED UNEMPLOYMENT BENEFIT ELIGIBILITY ON YOUNG ADULTS' HOUSING AND USE OF PRIVATE SAFETY NETS

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Koen Declercq
Muriel Dejemeppe
Mathilde Pourtois

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Abstract

When access to unemployment benefits (UB) is restricted early in the career, young labour market entrants may rely more on private safety nets, such as the parental home, to insure against unemployment risk. Using a difference-in-differences strategy, this paper investigates the causal impact of a 2015 reform tightening UB eligibility on master's graduates' decision to leave the parental home in Belgium. We find that the reform delays home-leaving for young men, especially those who would have chosen shared housing. In contrast, young women respond by increasing labour supply, substituting market income for reduced public insurance to maintain residential independence. These results highlight that restricting UB can trigger broader behavioural responses through both family and labour market channels.

Keywords: *unemployment insurance, gender, home-leaving, insurance substitution, difference-in-differences*

JEL-Codes: C21; J08; J12; J18; J21 ; J65; J68

[1] Koen Declercq : CAPE, UCLouvain - Saint-Louis Bruxelles, Belgium (koen.declercq@uclouvain.be). Muriel Dejemeppe: IRES/LIDAM, UCLouvain, Belgium, and GLO (muriel.dejemeppe@uclouvain.be), Mathilde Pourtois : IRES/LIDAM, UCLouvain, Belgium (mathilde.pourtois@uclouvain.be). We are grateful to Bart Cockx, Matthieu Delpierre, Sébastien Fontenay, William Parienté, Sunčica Vujić, and seminar participants at UCLouvain and UMons, and participants of the 19th Belgian Day of Labour Economists for fruitful discussions and comments. We acknowledge IWEPS for funding this research, as well as the BCSS for providing the data.

1.Introduction

Leaving the parental home is a fundamental milestone in the transition to adulthood. While it reflects young adults' desire for residential independence, staying at home can also act as a private safety net against labour market risk. In Europe in 2023, the average age at which young people leave their parental homes varies considerably, ranging from approximately 22 in northern countries such as Denmark, Sweden, and Finland, to around 30 in eastern and southern nations, including Poland, Croatia, Slovenia, Spain, and Italy (Eurostat, 2024). Belgium—the country we focus on in this paper—stands in an intermediate position, with young people leaving the parental home at an average age slightly below 26. Beyond Europe, similar patterns are observed in the US: a substantial fraction of young adults remain in the parental home upon labour market entry, with 57.1% of youths aged 18-24 and 21.7% of youths aged 25-29 still living with their parents in 2023 (Loo, 2024).

Understanding the determinants of residential independence is important, as the timing of nest-leaving has lasting economic consequences. Delayed nest-leaving is associated with lower labour supply and slower human capital accumulation, with potential long-term effects on income and employment (Billari and Tabellini, 2010; Saydam and Raley, 2024). It may lead to postponed fertility decisions, with potential implications for demographic structures and the long-term sustainability of pension and social security systems in low-fertility European contexts (Angelini et al., 2022). Additionally, prolonged co-residence with parents is associated with poorer mental health among young adults (Howard et al., 2023; Seiffge-Krenke, 2013), which may, in turn, negatively affect labour market participation and productivity (Frijters et al., 2014; Veldman et al., 2017).

While various personal and household characteristics—such as education, gender, socioeconomic background, and cultural norms—play a role in nest-leaving decisions (see Cobb-Clark (2008) for a review), financial resources are consistently identified as a primary determinant.[2] An extensive sociological and demographic literature emphasizes their role in achieving residential independence, with lower financial means being correlated with later nest-leaving (Aassve et al., 2002; Angelini et al., 2022; Avery et al., 1992; Billari, 2004; Breidenbach, 2018; Gierveld et al., 1991; Iacovou, 2010; Schwanitz et al., 2017, 2021). This literature does not explicitly aim to estimate causal effects but rather highlights correlations and cross-country differences.

Although less extensive, the economic literature has also emphasized the importance of financial resources, conceptualizing co-residence with parents as a form of private insurance that can substitute for state-provided support in the event of unemployment. Early contributions were largely theoretical[3], but recent work has provided empirical evidence supporting this insurance mechanism. Matsudaira (2015) shows that young adults rely on shared housing with parents in response to deteriorating economic conditions. Landaud (2021) demonstrates that financial insecurity, particularly due to temporary employment or unemployment, delays the decision to establish residential independence.

The above-mentioned literature highlights the importance of financial resources—either earned from employment or from parental transfers—in achieving residential independence. Nonetheless, in this body of research, the role of state-provided financial resources remains comparatively underexplored

[2] More recently see, for example, Schwanitz et al. (2017) for the role of education, Andersson (2021) on the role of social status, parental economic status and immigration, Billari et al. (2019) for the role of the socioeconomic status, van den Berg et al. (2018) for the role of family structure.

[3] See, for example, the seminal paper by McElroy (1985), which formalizes the insurance mechanism using a Nash bargaining model. Later, Kaplan (2012) uses a calibrated life-cycle model (based on U.S. data) to show that co-residence buffers against labour market risk.

(Cobb-Clark, 2008; Jacob and Kleinert, 2008). Empirical research suggests that welfare regulations explain some of the cross-national differences in the age at which young people leave the nest (Aassve et al., 2002; Billari, 2004; Billari et al., 2001; Halla et al., 2016; Mulder et al., 2002), with earlier departures observed in countries with more extensive welfare states.[4] However, much of this literature shows correlations rather than causal effects, failing to account for unobserved heterogeneity (Cobb-Clark, 2008). Our paper addresses this gap by providing causal evidence that changes in state financial support—and their tightening—can reshape young adults' reliance on private safety nets, even when the primary aim of the policy is not housing-related.

To the best of our knowledge, only three papers use natural experimental settings to identify the causal impact of state financial support on nest-leaving decisions. Aparicio-Fenoll and Oppedisano (2015) study the implementation of a monthly rental subsidy designed to encourage household formation in Spain. They find that removing financial constraints for youths increases their probability of living apart from their parents by 22%. Van den Berg (2020) and Chatterji et al. (2022) both analyse welfare reforms that were not explicitly intended to affect nest-leaving but which ultimately did, as this paper does. Van den Berg (2020) demonstrates, using an event history model, that abolishing the monthly student grant system in the Netherlands significantly delayed residential independence, with a 45% decrease in the probability of leaving home among affected student cohorts. Chatterji et al. (2022) show that a policy reform allowing young adults to stay under their parents' health insurance until age 26 in the U.S. increases co-residence with parents by 7% to 18%. Our paper contributes to this recent literature by analysing how tightening eligibility for unemployment benefits (UB) affects not only residential independence but also young adults' allocation of risk between the state, the family, and the labour market.

To this end, we exploit a policy reform of the youth-specific unemployment insurance (UI) in Belgium. Education leavers can qualify for UB without prior work experience, but only after a one-year qualifying period. In 2015, the Belgian government unexpectedly restricted entitlement to this youth-specific UI to individuals entering the labour market before the age of 24. This reform made eligibility requirements for UB tighter for individuals aged over 24, since they now have to prove one year of full-time employment to qualify for regular UB. Since the average age of leaving the parental home in Belgium at the time of the reform was 25 (Eurostat, 2024), this reform led to a reduction in expected financial resources in case of unemployment for young people about to make the decision to leave the parental home. The negative relationship between financial resources and residential independence suggests that this reform—by lowering financial resources in case of unemployment—is likely to delay nest-leaving and, consequently, increase residence rate at the parental home.

We use a difference-in-differences design to isolate the causal impact of this reform on the residence rate at the parental home. Specifically, we compare the evolution of housing decisions among youths who unexpectedly faced tightened eligibility requirements for UB (treatment group) with those of youths who were not affected by the reform (control group). Our estimates are based on individual-level administrative data on master's graduates registering for the first time at the public employment service (PES) in the two years preceding and in the first year following the implementation of the reform. The evaluation sample is composed of master's graduates aged between 22 and 24 at the time of first registration, with those aged 24 constituting the treatment group.

Residential independence is not only a matter of financial resources. It is widely agreed that men and women follow distinct decision-making processes regarding nest-leaving, warranting separate analyses (del Rey et al., 2023). The literature consistently shows that, in developed countries, women

[4] In those papers, the importance of the welfare state is measured in terms of public spending.

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).[5] Even after controlling for observable characteristics, a sizable residual gender gap in home-leaving persists, suggesting a stronger preference for independent living among women (Blaauboer and Mulder, 2010; Consuelo Colom and Cruz Molés, 2024). This paper therefore estimates the causal effects separately by gender, contributing to the limited causal evidence on whether financial constraints influence housing decisions differently for men and women (Aparicio-Fenoll and Oppedisano, 2015; Chiuri and Del Boca, 2010; Matsudaira, 2015).[6]

We find that tightening UB eligibility only significantly influences housing decisions of young men, prompting them to delay their departure from the parental home. About one and a half years after their first registration as job seekers, their probability of living with their parents increases by 7 percentage points (from 67% to 74%). This increase appears to be temporary and primarily concerns men who would have settled in a house-sharing arrangement otherwise. The decline in shared housing persists over time, suggesting that UB eligibility suppresses the transitional step of co-housing between university and settling with a partner for some men. In contrast, tightening UB eligibility has no impact on the rate of co-residence with parents among women. We attribute this absence of effect to women's stronger preference for independent living, which motivates them to increase labour supply, effectively substituting market income for reduced public insurance in order to leave the parental home. Indeed, we find an increase in the employment rate among women following the reform, while no significant employment response is observed among men. In the longer term, this enhanced employment encourages women to settle with a partner rather than choosing house-sharing arrangements.

Beyond its contribution to the literature on residential independence, this paper highlights how welfare retrenchment through tighter UB eligibility can reshape young adults' allocation of risk between the state, the family, and the market. While there is an extensive literature on the impact of UB generosity on employment and job quality (see Schmieder and Wachter (2016) and Tatsiramos and van Ours (2014) for reviews), little attention has been given to its potential effects on decisions outside the labour market.[7] Spillover effects are plausible, as shown by Cockx et al. (2023), who found that stricter UB eligibility increased completion rates among master's students.[8] Choi and Valladares-Esteban (2020) and Lehwess-Litzmann and Nicaise (2020) highlight the importance of household composition in shaping responses to changes in unemployment insurance. Our contribution lies in analysing household composition as a behavioural outcome reflecting the substitution between public and private insurance in response to welfare retrenchment.

[5] Belgian data at the time of the reform confirm this trend: in 2015, women were leaving the parental home on average one and a half year earlier than men (Eurostat, 2024).

[6] Chiuri and Del Boca (2010) suggest that alleviating financial constraints could further accentuate the initial gender differences in residential independence. Aparici-Fenoll and Oppedisano (2015) find that the rental subsidy in Spain had stronger effects on residential independence of women and conclude that women are more responsive to institutional factors affecting their financial resources. Conversely, Matsudaira (2015) found that men are more responsive to changes in financial resources due to economic conditions.

[7] Some examples in this scarce literature are Kolsrud et al. (2018) analysing the consumption expenditures drop; Arslan et al. (2024) showing that more generous unemployment benefits decrease bank deposits; and Cylus and Avendano (2017) investigating the effect of receiving unemployment benefits on health.

[8] Cockx et al. (2023) study the same reform as we do in this paper, using a very similar identification strategy. They find that tightening eligibility requirements for UB significantly increased the rate of completion of Belgian students in higher education by 2.8pp and reduced dropout by 1.1pp.

The remainder of this paper is organised as follows. Section 2 describes the Belgian unemployment insurance system and details the 2015 reform. Section 3 details our empirical strategy. Section 4 presents the data and descriptive statistics. Section 5 discusses the results, and Section 6 concludes.

2.Institutional context

Belgium is a federal state that has, over time, decentralized several competences to regional authorities—Brussels, Flanders and Wallonia. As regards the labour market, the unemployment insurance (UI) system and the payment of unemployment benefits (UB) are organised at the federal level. The public employment services, which are in charge of counselling, job search assistance, intermediation services and training of unemployed and employed workers are administered by the three regions.

In Belgium, as in many countries, eligibility for regular UB hinges on sufficient prior contributions. Specifically, job seekers under the age of 36 must demonstrate a year of full-time employment within the preceding 21 months. Many young entrants into the labour market do not satisfy this criterion. To protect them against unemployment risk, young labour market entrants can qualify under weaker conditions for a youth-specific UI called the 'activation allowance'. The activation allowance is a non-means-tested UI specific for education leavers. Entitlement to this allowance starts after a qualifying period of twelve months, conditional on registration at the regional public employment service (PES) and meeting job search requirements. The one-year qualifying period starts at labour market entry, defined as the first registration as a job seeker at the regional PES or the beginning of the very first employment spell, whichever is earlier. In contrast to the regular UI, not only full-time employment counts for the qualifying period but also part-time employment and active job search. Only inactivity, sickness and resumed education extend the qualifying period.

The amount of the activation allowance varies with age and household composition. In 2015, the year of the reform under study in this paper, young entrants into the labour market living on their own without any partner or dependent were entitled to a monthly benefit of 818 euros above the age of 21. Young entrants living with their parents are considered as cohabitants and therefore received only 425 euros per month. They represent the vast majority of beneficiaries. The baseline time limit is set to three years upon receiving the activation allowance for cohabitants. For household heads with family responsibilities or singles, the benefit can extend beyond three years and may be claimed until their 33rd birthday.[9]

In late December 2014, concerned that the activation allowance could act as a disincentive to work, the federal government signed an agreement to tighten its age eligibility criteria. Before January 1, 2015, individuals had to claim the activation allowance for the first time before the age of 30. However, as from January 1, 2015, this age limit was reduced to 25. Because of the one-year qualifying period, education leavers now need to enter the labour market before their 24th birthday to be eligible for the activation allowance. Those who enter the labour market after turning 24 lose their prospective eligibility for the activation allowance. Due to the reform, eligibility requirements for UB became stricter for this group of job seekers. They must now work full-time for at least one year to qualify for the regular UB. Given that the average age of university graduates in Belgium at the time of the reform was slightly higher than 24 (OECD, 2024), most youths affected by this reform were master's graduates entering the labour market. This paper aims to investigate whether those master's graduates with a

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

tightened access to UB will stay longer at the parental home to compensate the loss of financial resources expected in case of long-term unemployment.

In practice, labour market entrants who lose eligibility for the activation allowance have very limited, if any, alternative sources of financial support in case of unemployment. Individuals with low financial means might qualify for means-tested welfare benefits. Such benefits are designed to support individuals living in households with insufficient financial resources and who cannot obtain them through personal effort or other means (such as employment, unemployment insurance or disability benefit). The welfare support consists of a flat-rate allowance: €556 per month for cohabitants, €834 for single individuals, and €1,111 for heads of household with dependent children. The means-test for those benefits is done at the household level. Since our analysis focuses on recent master's graduates entering the labour market and still living with their parents, the vast majority of the studied population is effectively ineligible for welfare support—unless their parents have very low incomes. The high likelihood of ineligibility combined with the high employment rate of master's graduates result in a very low take-up of means-tested welfare benefits. Indeed, only 0.6% of our evaluation sample received such benefits during the four years following their entry into the labour market.

Though the core idea of the activation allowance reform was mentioned in the government agreement in October 2014, it was barely mentioned in the public debate until December 31, 2014, when the reform was finalized by a ministerial council for implementation on January 1, 2015. This makes it unlikely that the reform was anticipated and influenced labour market or housing decisions before its implementation. Youths who started their qualifying period at age 24 or older and who were in their qualifying period at the time of the reform were suddenly informed on January 1, 2015, that they would no longer be eligible for the activation allowance if they were still unemployed upon completing their qualifying period.

3.Methodology

We exploit the natural experiment created by the timing and unexpected nature of the policy reform to identify its causal impact. We use a difference-in-differences approach to compare the evolution in housing decisions of youths who faced unexpectedly tightened eligibility requirements for UB (treatment group) with those of youths who were not affected by the reform and could start claiming the activation allowance one year after labour market entry if still unemployed (control group).

The exact definition of the treatment and control groups depends on age at the start of the qualifying period. Due to data limitations, we cannot identify the start of the qualifying period for youths whose qualifying period begins with an employment spell. Therefore, our evaluation focuses on youths with negligible work experience who worked for less than one month prior to their first registration as a job seeker.[10] For these individuals, we can identify age at the start of the qualifying period—the key determinant of the treatment status—as the starting month of the qualifying period coincides with the month of their first registration at the PES. The years of first registration at the PES determine the pre- and post-reform periods, with t=2014 being the post-reform period and t=2012, 2013 being the pre-reform period. The treatment group consists of master's graduates who registered for the first time at the PES at the age of 24.[11] If they register for the first time in 2014, they are suddenly informed on January 1, 2015, that they will fail to meet the age criteria at the end of their qualifying period and

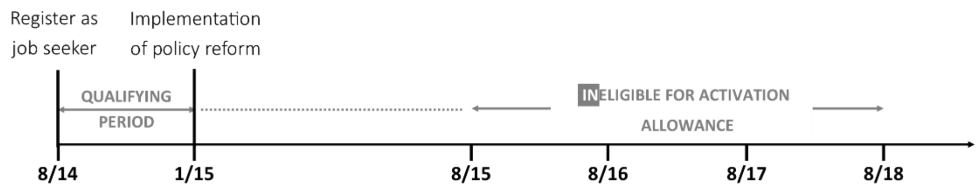
[10] Only standard forms of employment are considered when measuring work experience, with student jobs excluded. Further details on the sample selection process are provided in Section 4.

[11] The focus on individuals aged 24 only is due to data limitations. We have no data on older individuals.

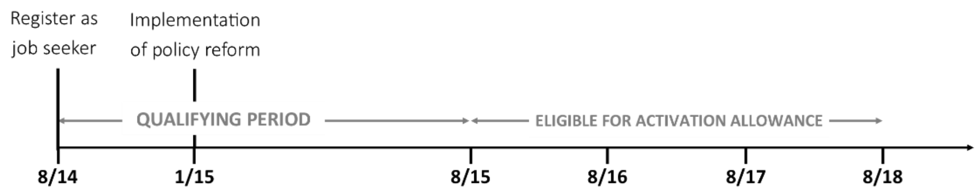
cannot claim the activation allowance.[12] Panel A of Figure 1 illustrates the trajectory of entitlement to the activation allowance of a master's graduate in the treatment group registering as a job seeker in August 2014. The control group consists of younger master's graduates, who registered for the first time at the PES at the age of 22 or 23 and are still eligible for the activation allowance after the implementation of the policy reform.[13] Panel B of Figure 1 illustrates the trajectory of entitlement to the activation allowance of a master's graduate in the control group.

Figure 1. Trajectory of entitlement to the activation allowance

Panel A: 24-year-old job seeker



Panel B: 23-year-old job seeker



Note: Age is measured at first registration as a job seeker.

To identify the causal impact of the reform on residential independence, we estimate the following difference-in-differences model:

$$Y_{itm} = \alpha + \gamma age_{itm} + \beta_{2012} T_{2012} + \beta_{2014} T_{2014} + \boldsymbol{\delta}\, D_i * T_{2014}$$
$$+ \textstyle\sum_{m=2}^{12} \rho_m M_m + \rho X_i + \in_i \quad (1)$$

Y_{itm} is the outcome of interest—residing at the parental home—of master's graduate *i*, registering for the first time at the PES in month *m* of year *t*. age_{itm} is the age in months measured at first registration at the PES. The treatment indicator D_i is equal to one for individuals in the treatment group, i.e. aged 24 at first registration at the PES and zero for individuals in the control group (aged 22 or 23). T_{2012} and T_{2014} are year indicators, 2014 being the post-reform period and 2013 being the reference year. The coefficient of the interaction term between the post-reform time indicator and the treatment indicator—$\boldsymbol{\delta}$—measures the average causal impact of the reform on the treatment group. Finally, dummies for the month of registration at the PES—M_m—are included and X_i is a vector of predetermined sociodemographic controls (see Table A2 in Appendix A).[14]

[12] Since age is measured in months, we exclude from the evaluation sample individuals who turned 24 during the month of first registration at the PES. At the end of their qualifying period, we do not know whether they already turned 25, and thus we cannot determine if they belong to the treatment or control group. Therefore, the treatment group consists of individuals aged between 24 years and 1 month and 24 years and 11 months at first registration at the PES.

[13] The focus on individuals aged 22 and 23 is due to data limitations. In Section 6, we conduct a robustness analysis restricted to individuals aged exactly 23 years in the control group to balance the size of the control and treatment groups and improve their comparability by reducing age differences.

[14] We verified that the treatment effects on the benchmark sample, estimated using Equation(1), are not sensitive to the exclusion of these control variables. Results are available upon request.

The estimated treatment effect δ measures the average impact of the tightened eligibility requirements for UB on Y_{itm}. Given that not all education leavers are at risk of long-term unemployment, the tightened eligibility requirements for UB will result in an effective loss of the activation allowance for only a subset of the treatment group. Therefore, our estimates should be interpreted as intention-to-treat (ITT) effects. In the first step of our empirical analysis, we estimate this proportion of the treatment group effectively losing the activation allowance. However, note that youths may still respond to the stricter eligibility criteria even if they ultimately do not need to claim the activation allowance after their qualifying period. Because of uncertainty about future employment, youths may change their behaviour because of an anticipation effect arising from the absence of insurance in case of unemployment.To test for the presence of parallel trends, we add an interaction term with the treatment indicator D_i and the pre-reform year indicator T_{2012} in the main specification

4.Data

To assess the impact of the policy reform on residential independence, we use individual-level administrative data from the Cross Roads Bank of Social Security (CBSS). Our dataset provides a variety of sociodemographic information recorded at the time of PES registration or annually, along with quarterly information on the economic status—work, unemployment or disability—and various social benefits. These benefits include the activation allowance, regular unemployment benefits, and means-tested welfare benefits.

Our database contains information on all master's graduates who registered for the first time in one of the three regional PES in 2012, 2013, or 2014 at the age of 22 to 24. In Belgium, registration at the PES is a common practice for master's graduates who have not secured employment following graduation, as it initiates the one-year qualifying period for the activation allowance. In academic years 2011-2012 to 2013-2014, just over 40% of master's graduates in the 22-24 age group registered at the PES at the end of their studies.[15] The remaining 60% likely found employment shortly after graduating, which explains their lack of PES registration. This high share of non-registered individuals reflects the generally high employment rate among master's graduates.

As explained in the previous section, in order to accurately identify the age at the start of the qualifying period—a key determinant of treatment status—we restrict the evaluation sample to individuals with negligible work experience prior to their first registration at the PES. This ensures that the qualifying period starts at the time of PES registration, allowing us to observe the individual's exact age at that point. Moreover, we focus on individuals who are living with their parents at the end of the year of first PES registration, which corresponds to the timing of the reform for those in the treatment group.[16] This second restriction ensures that individuals in the sample are still facing the decision to leave the parental home, which is our main outcome of interest. By restricting the sample in this way, we deliberately exclude studying returns to the parental home for individuals who had already left it. The

[15] Own calculations based on the number of individuals in our sample and the number master's graduates from all Belgian universities in academic years 2011-2012 to 2013-2014.

[16] When restricting the sample to individuals with negligible work experience, the sample size decreases from 22,018 to 19,305—a reduction of around 10%. Applying the additional restriction to individuals living with their parents further reduces the sample to 12,026, which constitutes our final sample. Table A1 in Appendix A shows that these two restrictions do not substantially alter the overall characteristics of the sample. However, the final sample includes fewer women and a somewhat more advantaged socioeconomic profile. This is expected, as women tend to leave the parental home earlier, and those still living with their parents are more likely to come from relatively well-off households.

choice is justified by the low prevalence of such behaviour: among the 7,279 individuals who were not residing in the parental home at the end of the registration year, only slightly more than 2% were living with their parents at the end of the first calendar year following registration, and a similar proportion at the end of the second year.[17]

4.1. Descriptive statistics

Table A2 in Appendix A presents descriptive statistics of our evaluation sample by age group (treatment versus control group) before and after the reform. Overall, the sample of young master's graduates is predominantly composed of Belgian youths living with their married parents and one sibling, in relatively high-income households.[18] Compared to the treatment group, the control group contains more women, more youths living with married parents, more master's graduates from science, technology, engineering, or mathematics (STEM), and includes individuals living in households with slightly higher income. These differences reflect, at least in part, the fact that the control group consists of individuals registering for the first time at the PES at a slightly younger age than those in the treatment group, which tends to include individuals with more favourable socio-economic characteristics and a stronger academic background.

While individuals in the control and treatment group are allowed to be different in a difference-in-differences design, changes over time in these variables should be the same in the treatment and control groups. To verify this, we use the model of Equation (1) with the predetermined sociodemographic characteristics as outcome variable and test whether the coefficient of the interaction term—$\boldsymbol{\delta}$—is statistically significantly different from zero. The last column of Table A2 in Appendix A shows that there are hardly no compositional changes between the two groups before and after the reform.[19]

4.2. Outcome variables

Our first outcome of interest is a binary variable indicating whether individuals actually receive the activation allowance. By construction, this outcome variable is zero for all individuals in the treatment group in the post-reform period. The treatment effect (in absolute value) represents the share of treated youths who, in absence of the policy reform, would have claimed the activation allowance.

Our second and main outcome of interest is residential independence. Specifically, we measure whether the young labour market entrant is residing at the parental home—i.e. living with one or both parents. Because the decision to leave the parental home involves not only whether to stay with parents but also with whom to live after leaving the nest, we identify three outside options upon leaving the parental home: (i) living alone, (ii) living with a partner, and (iii) living in a house-sharing

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[17] Including these individuals in the analysis yields results that are in line with the main findings presented in the paper. In particular, for the main outcome, the estimated treatment effect on men's probability of residing at the parental home decreases slightly from 7 to 6pp, but this difference is not statistically significant. Detailed results are available upon request.

[18] The annual household income variable covers income from all sources within the household. In the pre-reform year, the mean gross annual household income in the treatment group is €90,900. Given that most households in the sample consist of two parents and two children, it can be inferred that each parent earns approximately €3,800 per month. For comparison, even though our income variable is not limited to income from salaried employment, this figure is slightly above the mean gross salary in Belgium in 2015.

[19] More precisely, the proportion of Belgian individuals in the treatment group after the reform is slightly lower, but this difference is only statistically significant at the 10% level. Additionally, there is a modest decrease in registrations at the PES in November and an increase in December within the treatment group after the reform. These differences are statistically significant at the 10% and 5% levels, respectively.

arrangement.[20] For each of these alternatives, we construct a binary outcome variable, allowing us to explore heterogeneity in housing outcomes.

Finally, to explore potential mechanisms driving the observed effects on housing outcomes, we examine the employment rate. We construct a binary employment variable indicating whether the individual is employed, considering both salaried work and self-employment. Employment status is a critical factor, as securing employment represents a significant source of financial independence and may directly influence housing decisions.

Because household composition is only recorded at the very end of each calendar year, our analysis focuses on two key moments: the end of the first and of the second calendar year following the year of first registration at the PES—i.e. the year of labour market entry for individuals in our sample.[21] As most master's graduates start their qualifying period during the summer (see Table A2 in Appendix A), outcomes are measured roughly 16 and 28 months after registration at the PES for most of the evaluation sample. To be consistent, all other outcomes are also evaluated at these same two time points.

5. Empirical results

This section presents our main empirical results. First, we estimate the proportion of master's graduates who effectively lose the benefit of the activation allowance because they would have claimed it in absence of the reform, providing insights of treatment intensity. Second, we estimate the intention-to-treat effect of tightened eligibility requirements for UB on housing outcomes. Finally, we explore potential mechanisms behind observed gender differences in residential independence, focusing on shared housing and employment. Our main results are presented in graphs. The corresponding estimates of the treatment effects, the counterfactual outcomes, and the p-values from the tests for parallel trends and for differences in treatment effects between men and women are reported in the tables in Appendix A.

5.1. Benefit of the activation allowance

Figure 2 shows the impact of the policy reform on the probability of claiming the activation allowance. The corresponding estimates and tests for parallel trends can be found in Table A3 in Appendix A. The solid black line in Figure 2 shows the fraction of the treatment group that actually benefits from the activation allowance on December 31 of the first full calendar year after first registration as a job seeker (left panel) and on December 31 of the second full calendar year after first registration as a job seeker (right panel). The year of registration is represented on the horizontal axis. For a job seeker registering in 2014, outcomes are measured on December 31, 2015 and December 31, 2016. From all the job seekers registering at the age of 24 in 2012 (two years before the implementation of the reform) about 14% (7%) were claiming the activation allowance on December 31, 2013 (2014). By construction, this fraction drops to 0 for the treatment group in the post-reform period because treated individuals lose their eligibility for this allowance. The thin solid line shows the counterfactual outcome for the treatment group in absence of the reform surrounded by its 95% confidence interval.

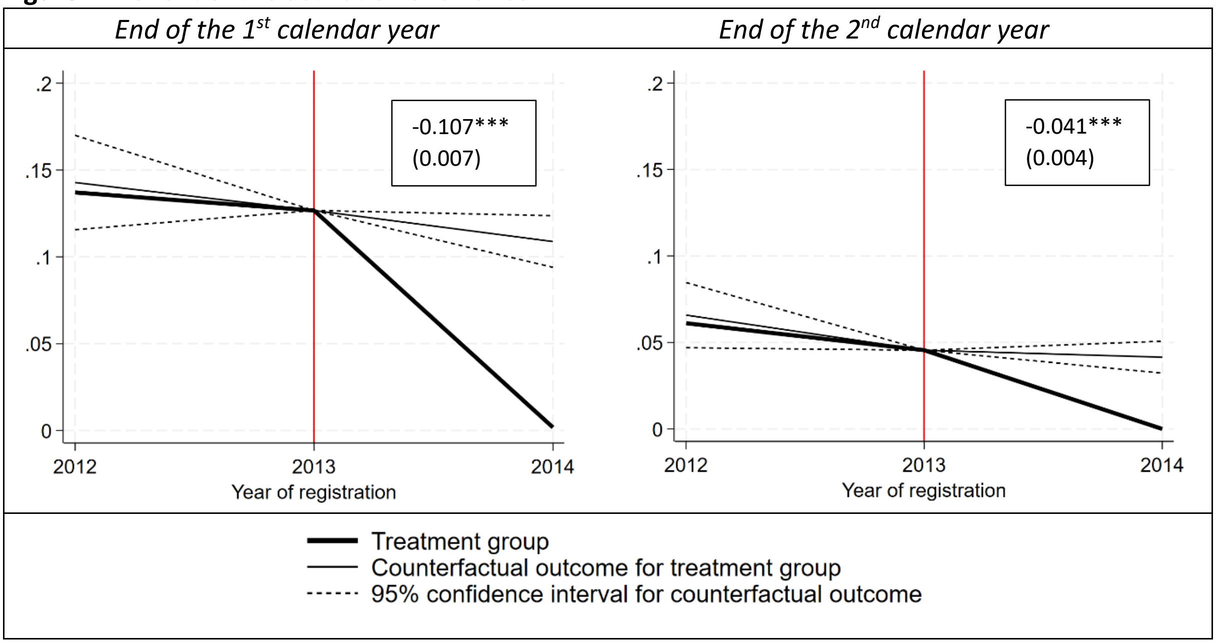
[20] Household composition data classify individuals aged over 18 as living with a partner if they either reside with the person they are married to or with an adult of the opposite gender with whom the age difference is less than 15 years. If an individual is neither living with their parents nor living alone nor living with a partner, they fall into the remaining category. In most cases, this category includes individuals who live with friends or family members other than their parents, which is why we refer to it as a house-sharing arrangement.

[21] It means, for example, that for an individual registering for the first time at the PES in 2014, we observe outcome variables at the end of December 2015 and December 2016.

The counterfactual outcome is computed using the model in Equation (1) and setting the interaction term between treatment and post-reform indicators equal to zero. In the year just before the reform, the reference year, the counterfactual is set at its observed value.

Based on Figure 2, we do not reject the assumption of parallel trends because the observed outcome of the treated group is contained within the 95% confidence interval of the counterfactual outcome in the pre-reform period. In the post-reform period, the difference between the observed and counterfactual outcome represents the treatment effect. It indicates that, in absence of the reform, 10.7% (4.1%) of individuals in the treatment group would have claimed the activation allowance at the end of the first (second) calendar year following their registration year. This treatment effect can be interpreted as the risk of effectively losing the activation allowance. The lower effective risk of loss at the end of the second calendar year compared to the first is explained by the lower likelihood of unemployment at that time.

Figure 2. Benefit of the activation allowance



Note: Evaluation sample = master's graduates registering for the first time at the PES in 2012, 2013, or 2014, with less than one month of work experience and still living at the parental home at the end of the registration year. Treatment group = 24-year-old job seekers. Control group = 22- and 23-year-old job seekers. Age is measured at the end of the month of first registration at the PES. The vertical red line indicates the last year before the reform. The thick solid line shows the observed outcome of the treatment group. The fine solid line represents the counterfactual outcome of the treatment group in the absence of treatment. The thin dotted lines indicate the 95% confidence interval of the counterfactual outcome. Treatment effects and standard errors (in brackets) are reported in the rectangular label on each graph. Table A3 in Appendix A reports the counterfactual outcomes, and p-values from the tests of the parallel trends assumptions.

Compared to the policies analysed by Aparicio-Fenoll and Oppedisano (2015) and van den Berg (2020), the fraction of the treatment group effectively affected by our intervention is much smaller. Therefore, we could anticipate a smaller effect on residential independence in our analysis. However, our above-mentioned estimates might underestimate the fraction of individuals in the treatment group who could react to the reform. First, due to future employment uncertainty, the perceived risk of losing the activation allowance may be higher. Anticipation effects could therefore imply that more than 10.7% and 4.1% of the treatment group react to the reform. Second, we measure the benefit of the activation allowance on December 31 for consistency with housing outcomes. However, in the months just before or just after, other individuals might also effectively lose the eligibility to the activation allowance and adapt their housing decisions in response. Estimating the fraction of the treatment group that would have claimed the activation allowance throughout the entire first and second calendar year indicates that a higher fraction of the treatment group is effectively affected by the reform.[22]

Figure 3 shows the fraction of treated men (Panel A) and women (Panel B) claiming the activation allowance by registration year. In the post-reform period (registration year 2014), the comparison of observed and counterfactual outcomes indicates that, in absence of the reform, 12.5% of treated women would have claimed the activation allowance at the end of the first calendar year following their registration year, compared to 8.9% of treated men. At the end of the second calendar year, these figures fall to 5.7% for women and 2.4% for men.[23] We conclude that the activation allowance is more important for female master's graduates than for male master's graduates.

5.2. Residential independence

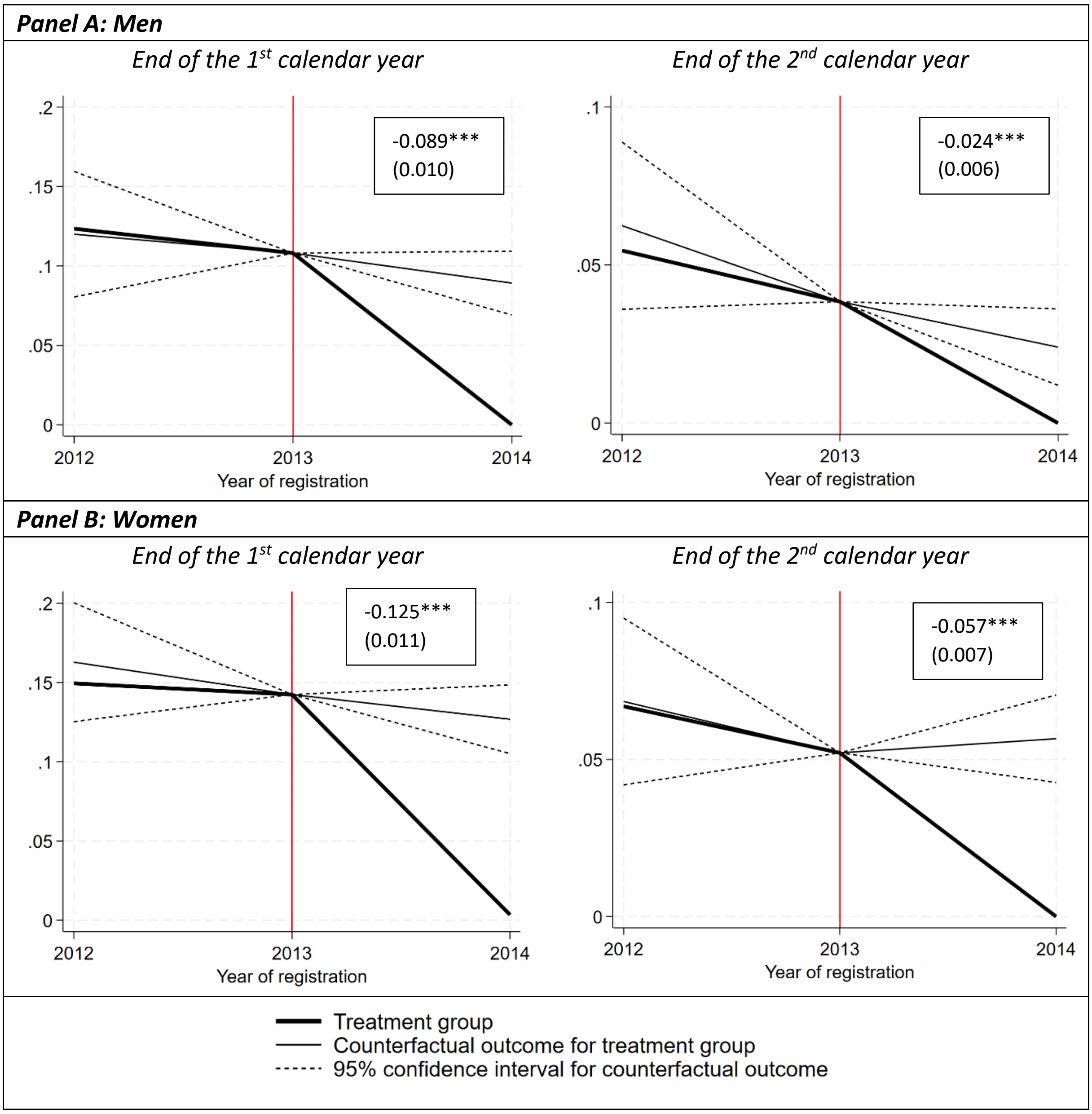
Figure 4 compares residence rates with parents among master's graduates who register for the first time at the PES at age 24 (treatment group) and at age 22 and 23 (control group). In the pre-reform period (registration year 2012), the observed outcome of the treated is contained within the 95% confidence interval of the counterfactual outcome. Therefore, we do not reject the assumption of parallel trends. The counterfactual outcome in the post-reform period indicates that, at the end of the first calendar year following the registration year, around 66% of treated master's graduates still reside with their parents. The reform increases residence with parents by 2.8 percentage points (pp) but this treatment effect is not statistically significant at the 5% level. At the end of the second calendar year following the registration year, the proportion of master's graduates staying with their parents falls just below 50% and the treatment effect is very close to zero.

Given the different decision-making processes of men and women with respect to residential independence (del Rey et al., 2023), Figure 5 shows the results for men (Panel A) and women (Panel B) separately. The left-hand graph of Panel A illustrates that tightening UB eligibility increases the proportion of men living with their parents at the end of the first calendar year following the registration year by 7pp, corresponding to a 10% increase (7.0pp/0.67) in the probability of co-residence with parents. At the end of the second calendar year (the right-hand graph of Panel B), the estimated effect is still slightly positive (1.8pp) but no longer statistically significant. Panel B shows that treatment effects are not statistically significant at either point in time for women. The p-value of the test for equal treatment effects for men and women at the end of the first calendar year is 0.038 (see Table A6 in Appendix A), indicating that treatment effects differ statistically between men and women at the 5% level. We conclude that, when eligibility requirements for UB are tightened, men reside with their parents for a longer period. The policy reform exacerbates the existing differences in co-residence rates between men and women: the proportion of men living with their parents increases from 67% to 74% after the reform, whereas the percentage of women remains stable at 64%.

[22] Running a similar difference-in-differences specification on the probability of receiving the activation allowance at least once during the first calendar year and during the first two calendar years following the registration year, we estimate that the share of the treatment group effectively affected by the reform increases to 15.8% and 17.7%, respectively.

[23] Again, running a similar difference-in-differences specification on the probability of receiving the activation allowance at least once during the first and the first two calendar years following the registration year yields higher estimates. For women, we estimate that the share of the treatment group effectively affected by the reform increases to 19.5% and 21.4%, respectively. For men, the corresponding estimates are 12.1% and 13.8%.

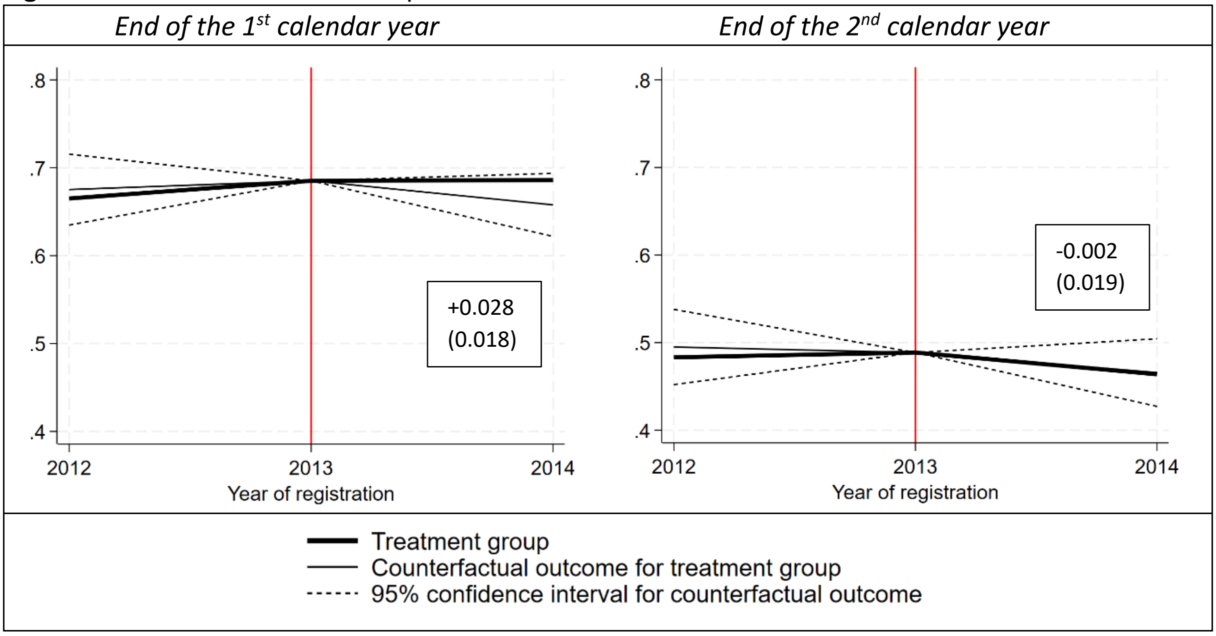
Figure 3. Benefit of the activation allowance by gender



Note: Evaluation sample = master's graduates registering for the first time at the PES in 2012, 2013, or 2014, with less than one month of work experience and still living at the parental home at the end of the registration year. Treatment group = 24-year-old job seekers. Control group = 22- and 23-year-old job seekers. Age is measured at the end of the month of first registration at the PES. The vertical line indicates the last year before the reform. The thick solid line shows the observed outcome of the treatment group. The fine solid line represents the counterfactual outcome of the treatment group in the absence of treatment. The thin dotted lines indicate the 95% confidence interval of the counterfactual outcome. Treatment effects and standard errors (in brackets) are reported in the rectangular label on each graph. Treatment effects are estimated separately for men and women. Tables A4 in Appendix A reports the counterfactual outcome, and p-values from tests of the parallel trends assumptions and gender equality in treatment effects.

Beyond the impact of the reform, it is interesting to examine the co-residence trends across the registration cohorts. The counterfactual outcomes shown in Figures 4 and 5 indicate that the rate of co-residence with parents appears stable or marginally increasing for cohorts registering at the PES in 2012 and 2013. In contrast, they suggest a declining trend between 2013 and 2014, more pronounced among men. This pattern aligns with aggregate data on the share of individuals aged 18-34 living with their parents (Eurostat, 2025a). The shift in trend observed after 2013 coincides with an improvement in macroeconomic conditions in Belgium following the Great Recession.[24] Economic conditions are known to be a key determinant of co-residence with parents, individuals leave earlier in good times, with stronger effects observed among men (see Lee and Painter (2010) and Matsudaira (2015)). The economic conditions affected the treatment and control groups similarly.

Figure 4. Rate of residence at the parental home



Note: Evaluation sample = master's graduates registering for the first time at the PES in 2012, 2013, or 2014, with less than one month of work experience and still living at the parental home at the end of the registration year. Treatment group = 24-year-old job seekers. Control group = 22- and 23-year-old job seekers. Age is measured at the end of the month of first registration at the PES. The vertical line indicates the last year before the reform. The thick solid line shows the observed outcome of the treatment group. The fine solid line represents the counterfactual outcome of the treatment group in the absence of treatment. The thin dotted lines indicate the 95% confidence interval of the counterfactual outcome. Treatment effects and standard errors (in brackets) are reported in the rectangular label on each graph. Tables A5 in Appendix A reports the counterfactual outcomes, and p-values from the tests of the parallel trends assumptions.

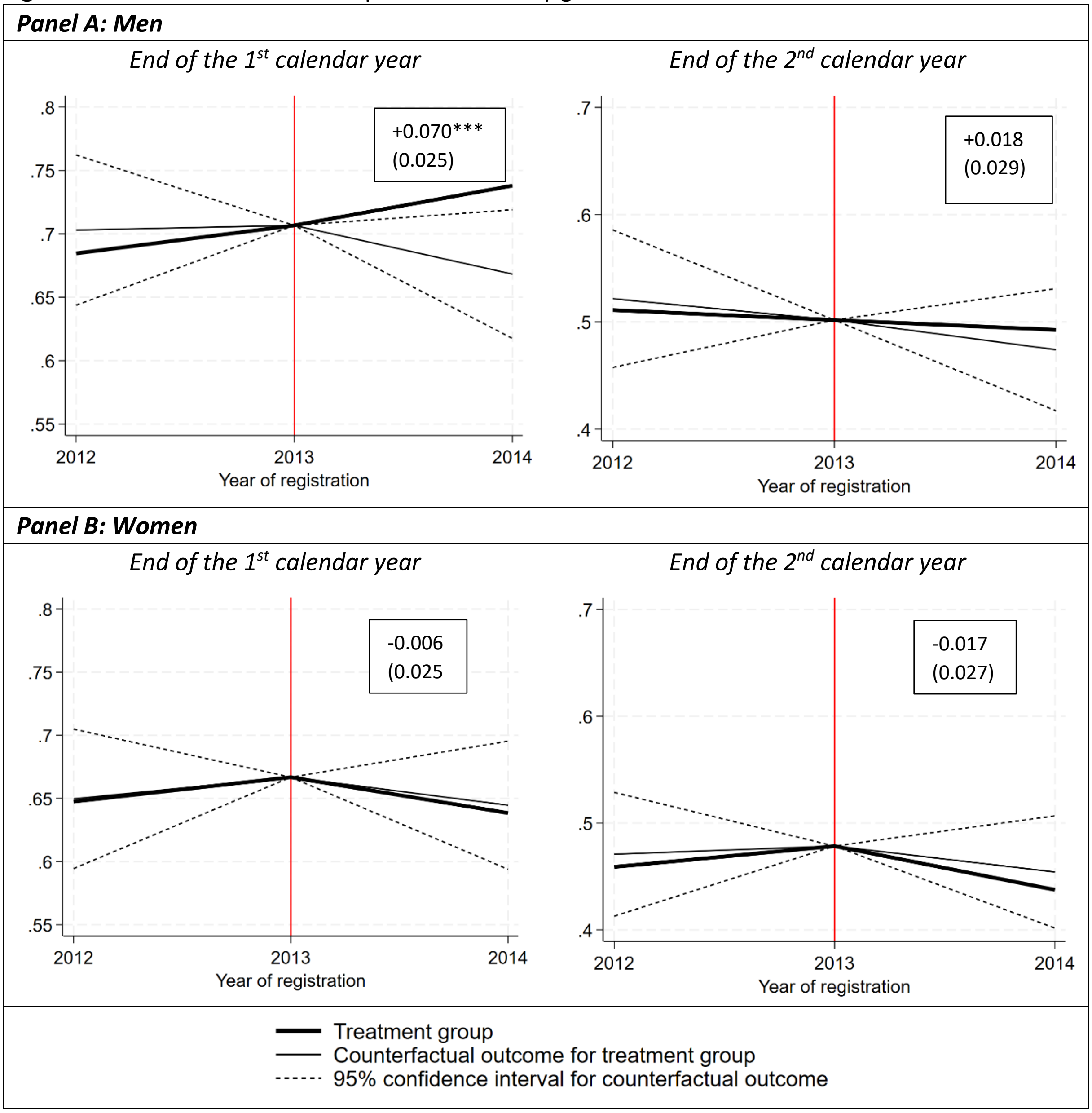
If male labour market entrants stay longer with their parents when access to UI becomes stricter, this raises the question of where they would have moved otherwise. Table 1 reports treatment effects on the probability of living alone, with a partner, or in a house-sharing arrangement measured at the end of the first and second calendar year following the registration year, along with the previously estimated effects on co-residence with parents. Results are reported separately for men (Panel A) and women (Panel B). The last line of the table provides the p-value for the test assessing the equality of treatment effects between men and women.

Examining the counterfactual outcomes, we observe that men and women differ in the type of household they settle in upon leaving the parental home. In absence of the reform, men are more likely than women to live in house-sharing arrangements. At the end of the first calendar year, 8.6% of men live in house-sharing arrangements compared to 5.5% of women. Conversely, women are more

[24] In Belgium, while the most important decline in GDP following the financial crises occurred between 2007 and 2010, growth remained very limited—close to zero—in 2012 and 2013. It is only from 2014 onwards that the economy showed a more sustained recovery, with annual GDP growth exceeding 1% between 2014 and 2019 (World Bank, 2025). This improvement was mirrored in the labour market: from early 2014, the number of job vacancies rose sharply, and the youth unemployment rate began to decline, having peaked in 2013 as a consequence of the crisis (Eurostat, 2025b; Statbel, 2025a). These developments signal the end of the post-crisis stagnation period in Belgium.

likely to live with a partner: 21.1% of women live with a partner at the end of the first calendar year, compared to 15.9% of men. These patterns are consistent across both time horizons, with the proportion of men living in house-sharing arrangements remaining approximately 3pp higher and the proportion of women living with a partner remaining 4pp higher at the end of the second calendar year.

Figure 5. Rate of residence at the parental home by gender



Note: Evaluation sample = master's graduates registering for the first time at the PES in 2012, 2013, or 2014, with less than one month of working experience and still living at the parental home at the end of the registration year. Treatment group = 24-year-old job seekers. Control group = 22- and 23-year-old job seekers. Age is measured at the end of the month of first registration at the PES. The vertical line indicates the last year before the reform. The thick solid line shows the observed outcome of the treatment group. The fine solid line represents the counterfactual outcome of the treatment group in the absence of treatment. The thin dotted lines indicate the 95% confidence interval of the counterfactual outcome. Treatment effects and standard errors (in brackets) are reported in the rectangular label on each graph. Treatment effects are estimated separately for men and women. Tables A6 in Appendix A reports the counterfactual outcome, and p-values from tests of the parallel trends assumptions and gender equality in treatment effects.

Regarding treatment effects among men, we find that the tightened UB eligibility reduces the probability of living in a house-sharing arrangement at the end of the first calendar year by 4.4pp, a result that is statistically significant at the 1% level. We also observe a 1.8pp decrease in the probability of living alone at the end of the first calendar year, although this effect is not statistically significantly different from zero. The estimated effect on the probability of living with a partner is close to zero and not significant. By construction, the estimated effect on co-residence with parents corresponds to the sum of the effects on the three alternative living arrangements. Our results indicate that the increase in co-residence with parents—amounting to 7pp—primarily stems from the decline in house-sharing arrangements. In other words, most men who remained longer with their parents after the reform would have otherwise opted for shared housing. The decrease in the probability of living in house-sharing arrangements persists until the end of the second calendar year following the registration year, with a reduced magnitude of -3pp, statistically significant at the 10% level only.

For women, all treatment effects are close to zero and not statistically significant at the end of the first calendar year. However, by the end of the second calendar year, treatment effects indicate a decrease in the probability of living in a house-sharing arrangement (-3pp), statistically significant at the 5% level, and an increase in the probability of living with a partner (+4.9pp), statistically significant at the 10% level. We interpret this as evidence of a switch from living in a house-sharing arrangement to cohabitation with a partner among women. This switching behaviour only appears at the end of the second calendar year following the registration year, indicating that tightening UB also affected housing decision among women, though with some delay. We discuss this further at the end of Section 5.3.

Table 1. Share of youths living with their parents, alone, with a partner and in a house-sharing arrangement

	End of the 1st calendar year				*End of the 2nd calendar year*			
	Parents	Alone	Partner	Shared housing	Parents	Alone	Partner	Shared housing
Panel A: Men								
Treatment effect (se)	0.070*** (0.025)	-0.018 (0.015)	-0.008 (0.021)	-0.044*** (0.013)	0.018 (0.029)	-0.010 (0.012)	0.023 (0.026)	-0.030* (0.016)
Counterfactual outcome	0.669	0.086	0.159	0.086	0.474	0.138	0.268	0.119
Panel B: Women								
Treatment effect (se)	-0.006 (0.025)	0.010 (0.015)	-0.001 (0.022)	-0.002 (0.012)	-0.017 (0.027)	-0.002 (0.018)	0.049* (0.026)	-0.030** (0.012)
Counterfactual outcome	0.645	0.088	0.211	0.055	0.454	0.140	0.314	0.090
P-value of equality test	0.038	0.224	0.815	0.018	0.373	0.757	0.474	0.993
N	**12,026**	**12,026**	**12,026**	**12,026**	**12,026**	**12,026**	**12,026**	**12,026**

Note: Evaluation sample = master's graduates registering for the first time at the PES in 2012, 2013, or 2014, with less than one month of working experience and still living at the parental home at the end of the registration year. Results are based on difference-in-difference regressions. Treatment group = 24-year-old job seekers. Control group = 22- and 23-year-old job seekers. Age is measured at the end of the month of first registration at the PES. Pre-reform years = 2012 and 2013. Post-reform year = 2014. The counterfactual outcome is the predicted outcome for the treated in the absence of treatment in the post-reform period. Treatment effects are estimated separately for men and women. The p-values of the equality tests indicate whether the treatment effects differ significantly between men and women.

5.3. Mechanisms

The finding that losing eligibility for unemployment benefits delays nest-leaving of young men aligns with Aparicio-Fenoll and Oppedisano (2015), Chatterij et al. (2022) and van den Berg (2020), who demonstrate that an increase in financial resources reduces the probability that youths reside with their parents. The key difference with those studies is that they identify impacts on both men and women, whereas our findings show an effect exclusively on men. Moreover, we are able to show that the adjustment by men comes at the expense of house-sharing arrangements. In this subsection, we first discuss the role of shared housing as an adjustment channel. We then provide some intuition on why the results for men align with the initial expectations, while those for women do not.

Shared housing offers greater flexibility and lower costs, making it an attractive transitional arrangement between living at the parental home and settling with a partner. Youths' choices regarding house-sharing arrangements are determined by individual preferences, housing market conditions, and labour market structures (Arundel and Ronald, 2016; Clark et al., 2018; Schwanitz and Mulder, 2015). In the baseline situation, before the reform, men in our evaluation sample are more likely than women to opt for shared housing, a pattern also documented by Schwanitz and Mulder (2015) across seven other European countries.[25]

Our results indicate that men who decide to stay longer with their parents when they are no longer eligible for the activation allowance would otherwise have transitioned to shared housing. The fact that men affected by the reform are primarily those who would have chosen shared housing, rather than living alone or with a partner, is consistent with the idea that this arrangement represents the most flexible and financially accessible form of independent living.

The monthly cost of a room in shared accommodation is relatively close to the amount of the activation allowance—around €400 in Brussels, the most expensive of the three Belgian regions (Observatoire des Loyers, 2015)—making it a viable pathway towards autonomy that is directly affected by the potential loss of the activation allowance. At baseline, 8.6% of men in our evaluation sample opt for shared housing as a transitional phase between the end of higher education and settling down with a partner, compared with 5.5% of women. However, the loss of eligibility to the activation allowance makes financing such a living arrangement more difficult, leading some men who would have opted for shared housing to postpone their departure from the parental home. In other words, by tightening eligibility to UB, the reform effectively suppresses a transitional phase for some men—between the end of higher education and settling with a partner—during which they would have chosen shared housing over remaining in the parental home.

For women, the rate of residence at the parental home remains unchanged. The literature shows that they have a stronger preference for independent living, reducing the probability that they would choose to stay longer with their parents. In European countries, women tend to leave the parental home earlier than men—a pattern that may reflect certain characteristics such as their higher levels of education or their tendency to find a partner at a younger age than men. However, Blaauboer and Mulder (2010) and Consuelo Colom and Cruz Molés (2024) show that this is not the only explanation: even after controlling for observable differences between men and women, a substantial portion of the gender gap in home-leaving remains unexplained, reflecting the stronger preference for independence among women. Sociological research provides further support for the idea that women prioritize autonomy, showing that they are more likely than men to leave the parental home when faced with an unpleasant atmosphere, family conflict, or poor housing conditions (Blaauboer and

[25]Greece, Portugal, Romania, Hungary, Ireland, France, Switzerland, and Austria.

Mulder, 2010; van den Berg et al., 2018). The findings of Matsudaira (2015) also support the idea that women have a stronger preference for independent living, as they are less likely than men to remain in the parental home under poor economic conditions.

Delaying nest-leaving constitutes a form of insurance against the risk of unemployment for men. The absence of a similar reaction among women may be explained by their ability to compensate through alternative sources of income in order to preserve autonomy from their parents. Panel B of Figure 6 shows that tightening UB eligibility increases the employment rate of women by 4.9pp and 6pp at the end of the first and second calendar years following the year of registration as a job seeker. These effects correspond to proportional increases of 6.7% (4.9pp/0.724) and 7.6% (6pp/0.792), respectively, relative to the baseline employment rate. In contrast, treatment effects on the employment rate of men are not statistically different from zero.

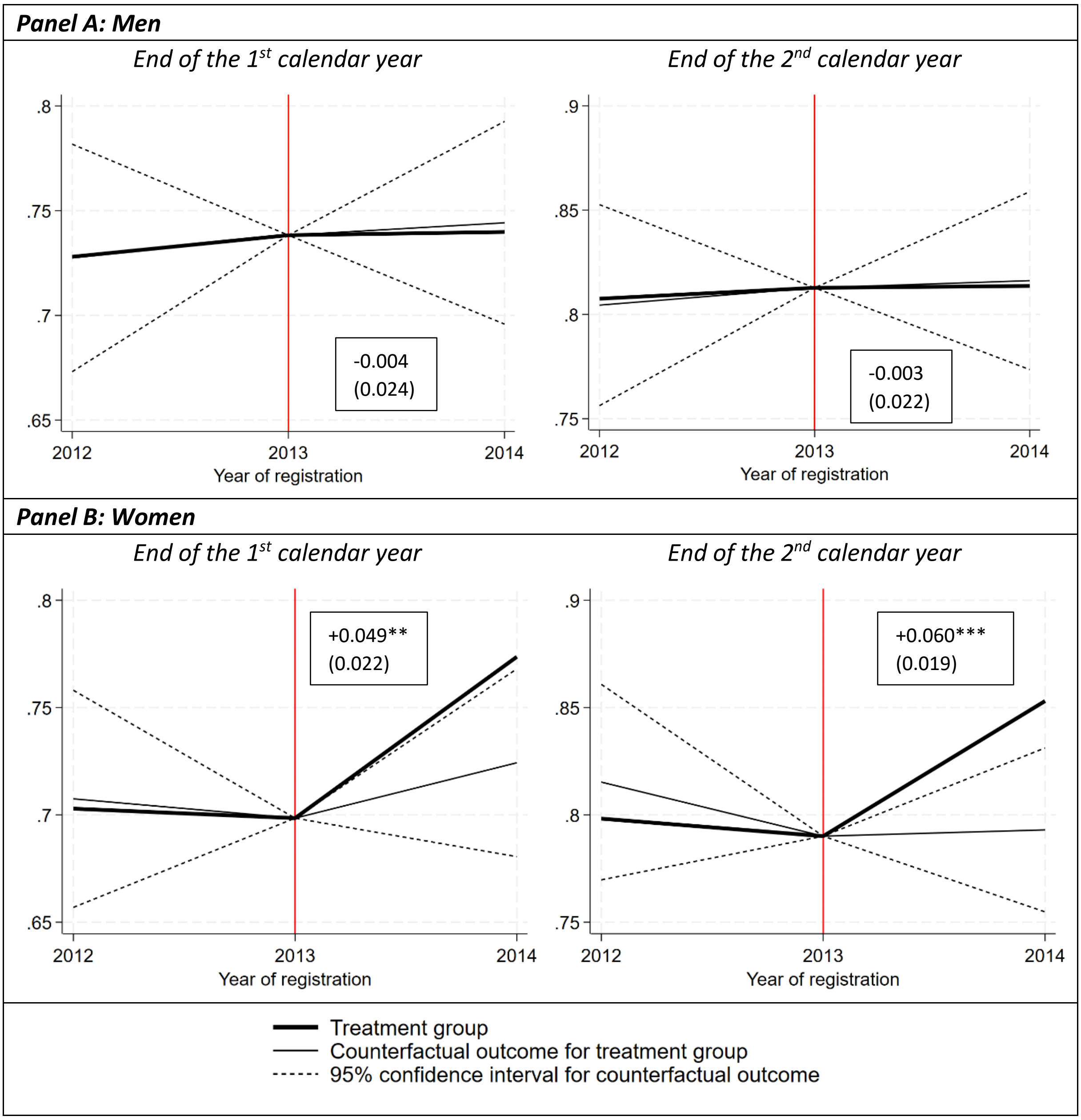
The increase in the employment rate among women at the end of the first and second calendar years following registration year is unlikely to be driven by random statistical variation at those specific points in time, as this conclusion holds across different time horizons. Figure B1 in Appendix B depicts the treatment effects at the end of each quarter from the first to the tenth quarter following initial registration at the PES. It shows that the positive treatment effect on employment for women, and the absence of an effect for men, remain consistent across time horizons.[26]

Moreover, the increase in the employment rate among women is specific to women still living at the parental home. Indeed, when conducting our analysis on an evaluation sample consisting only of master's graduates not residing with their parents at the end of the first registration year, individuals not included in the benchmark sample, treatment effects become slightly negative and not statistically significant for both genders (see Figure B2 in Appendix B). In other words, we do not find an increase in employment for young women who already left the parental home at the end of the registration year when eligibility for UB is tightened. This finding indicates that the increase in employment among women is mediated by housing circumstances, with the prospect of leaving the parental home providing an additional incentive to find a job. This pattern is consistent with the stronger preference for independence among women documented in the literature. When UB eligibility is tightened, some women who are still living with their parents but already planning to leave compensate for the loss of state financial resources by employment income. They do so to preserve financial independence and avoid prolonged co-residence with their parents, as evidenced by the fact that women who were already no longer living with their parents do not change their labour supply.

By the end of the second calendar year, we observe a shift from shared housing to living with a partner among women. We interpret this as a consequence of the increase in employment from the first study period onwards, which facilitates the transition away from shared housing toward settling with a partner. This increase in employment provides them with the financial resources to move directly into living with a partner, rather than opting for shared housing, the more affordable form of independent living arrangement.

[26] The tightened UB eligibility increases the employment rate of women by on average 5.8pp—statistically significant at the 5% level—between the fourth and tenth quarters after first registration. The fourth quarter corresponds to the end of the qualifying period, suggesting that women in the treatment group were more likely to find a job at the moment they would have become eligible for the activation allowance in absence of the reform.

Figure 6. Employment rate by gender



Note: Evaluation sample = master's graduates registering for the first time at the PES in 2012, 2013, or 2014, with less than one month of working experience and still living at the parental home at the end of the registration year. Treatment group = 24-year-old job seekers. Control group = 22- and 23-year-old job seekers. Age is measured at the end of the month of first registration at the PES. The vertical line indicates the last year before the reform. The thick solid line shows the observed outcome of the treatment group. The fine solid line represents the counterfactual outcome of the treatment group in the absence of treatment. The thin dotted lines indicate the 95% confidence interval of the counterfactual outcome. Treatment effects and standard errors (in brackets) are reported in the rectangular label on each graph. Treatment effects are estimated separately for men and women. Tables A7 in Appendix A reports the counterfactual outcome, and p-values from tests of the parallel trends assumptions and gender equality in treatment effects.

6. Sensitivity analysis

In this section, we examine the sensitivity of our results with respect to the selection of the evaluation sample and we correct p-values for multiple-hypothesis testing.

In the first two sensitivity analyses, we restrict the evaluation sample in two distinct ways. First, we limit the control group to 23-year-old job seekers instead of the larger control group of 22- and 23-

year-old job seekers. By reducing the age difference between the treatment and control groups, we improve their comparability and increase confidence in the assumption that, in the absence of the reform, the outcomes for both groups would have evolved in a similar way. Second, to enhance the homogeneity of the treatment and control groups, we restrict the sample to individuals registered during months with high registration volumes. These months—July, August, September, and October—are typically when individuals finish their studies in June or August and transition directly into the labour market. With this restriction, we exclude individuals who drop out of their studies mid-year or take several months off before entering the workforce, allowing us to focus on individuals with a more traditional academic-to-employment trajectory.

The results of these two sensitivity analyses are presented in Tables A8 to A10 in Appendix A and confirm our main findings. The two sensitivity analyses on the rate of residence at the parental home by gender confirm the positive and statistically significant effect of the policy reform on the residence rate of men at the end of the first calendar year following the registration year. The treatment effects are similar in magnitude but are statistically significant at the 5% level only, whereas they were significant at the 1% level in the benchmark analysis, which could be attributed to the reduced sample size. At the end of the second calendar year for men, and at both time horizons for women, the treatment effects are not statistically significant, consistent with the benchmark results. Further analyses on alternative living arrangements confirm that men who stay longer at the parental home would have primarily chosen house-sharing arrangements. The finding that women are more likely to be in employment at both time horizons is also confirmed by the two sensitivity analyses reported in Table A10.

In the last sensitivity analysis, we adjust p-values using both the single-step Bonferroni procedure and the Holm (1979) stepdown procedure, to account for multiple testing across outcomes and horizons. These two approaches control for the family-wise error rate, i.e. the probability of committing at least one type I error—a false discovery—when testing many different hypotheses. We report both conventional and adjusted p-values in Tables A11 and A12 in Appendix A. For men, both adjustment procedures confirm the increase in residing with parents and the decrease in shared housing at the end of the first calendar year following registration year. However, the effect on shared housing at the end of the second calendar year, which was significant at the 10% level in the benchmark analysis, is no longer statistically significant. Taken together, the two adjustment procedures confirm our main finding: men remain longer at the parental home rather than moving into shared housing.

For women, the absence of effect on residence at the parental home holds at both horizons. The increase in the employment rate at the end of the first calendar year is not statistically significant under either adjustment procedure (p-values are just above 0.10). However, by the end of the second calendar year the positive employment effect remains highly significant. Under both adjustment procedures, the treatment effects indicating a switch from shared housing to living with a partner at the end of the second calendar year are no longer statistically significant. Overall, the two adjustment procedures support our main conclusion for women: they work more in order to preserve their independence from their parents.

7. Conclusion

This paper exploits a reform that tightened unemployment benefit (UB) eligibility to estimate its causal impact on young adults' reliance on private insurance mechanisms and residential independence. In Belgium, education leavers are eligible for UB with little or no prior work experience, subject to a one-year qualifying period of active job search. As of January 2015, entitlement to these youth-specific benefits was restricted to individuals entering the labour market before the age of 24. Building on the literature on financial resources, risk-sharing, and residential independence, we hypothesise that restricting access to unemployment insurance increases reliance on private safety nets, notably the parental home, as informal insurance against labour market risk.

Using individual administrative data and a difference-in-differences design, we find that tightening UB eligibility delays home-leaving decisions for men only. Approximately one and a half years after labour market entry, the probability that men co-reside with their parents increases by 7 percentage points (from 67% to 74%). The evidence indicates that those who remain longer in the parental home would otherwise have primarily chosen shared housing. The reform therefore suppresses, for some men, shared housing as a transitional arrangement between university and partnership formation.

In contrast, we find no delay in women's departure from the parental home. Instead, women respond to the tightening of UB eligibility by increasing labour supply. Employment rates rise by about 5 percentage points and remain persistently higher over time. This response is concentrated among women who had not yet left the parental home, suggesting that their stronger preference for independent living drives increased job search efforts when public insurance becomes less accessible. In the longer run, higher employment encourages earlier partnership formation at the expense of shared housing. Hence, while the reform also reduces shared housing among women, it operates through a labour supply channel rather than through prolonged co-residence with parents. To our knowledge, this is the first study to document gender-differentiated housing responses to a tightening of youth social insurance benefits.

Taken together, our findings highlight that reforms tightening access to unemployment benefits may generate broader behavioural responses than intended. By altering the availability of public insurance, such reforms reshape the allocation of risk between the state, the family, and the market. They shift young adults' reliance toward private insurance mechanisms and affect not only labour market outcomes but also household formation trajectories. Accounting for these spillover effects is therefore crucial when evaluating policies targeting young labour market entrants.

Two caveats are worth noting. First, as household composition is measured annually, our estimates may understate short-term adjustments in living arrangements. Second, our analysis focuses on master's graduates, the group most directly affected by the reform. While relatively socioeconomically homogeneous, this population provides a clear setting to identify behavioural responses to changes in unemployment benefit eligibility. Examining whether similar insurance substitution mechanisms operate among individuals with different educational or socioeconomic backgrounds remains an important avenue for future research.

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Appendix A: Tables

Table A1. Descriptive statistics of the individuals in the overall database and evaluation sample

	Overall database	**Evaluation sample**
Women[(a)]	59.5	55.5
Belgian[(a)]	98.4	99
Migration background[(a)]		
At least one non-Belgian parent	15.2	15
At least one non-Belgian grandparent	36.7	36.7
Region of residence[(b)]		
Flanders	59.9	58.9
Wallonia	32.1	34
Brussels	8	7
STEM master[(b)]	20.8	22.6
Type of household[(c)]		
Married couple	47.7	76.6
One-parent	11.6	18.6
Unmarried couple	3	4.8
Living alone	9.4	/
Living with a partner	21.5	/
Living in a house sharing arrangement	6.8	/
Household size (mean) [(a)]	3.5	3.9
Number of siblings [(a)] (mean)	1	1
Annual household income	86,600	95,100
Month of first registration at the PES		
January	1.3	0.9
February	1.9	1.6
March	0.7	0.6
April	0.4	0.3
May	0.5	0.4
June	3.0	2.7
July	33.7	32.1
August	18.9	18.3
September	28.5	31
October	8	8.9
November	1.7	1.8
December	1.4	1.4
N	**22,018**	**12,026**

Note: Background characteristics for the (i) overall database and (ii) evaluation sample. (i) includes all master's graduates who registered for the first time in one of the three regional PES in 2012, 2013, or 2014 at the age of 22 to 24. (ii) includes master's graduates registering for the first time at the PES in 2012, 2013, or 2014, at the age of 22 to 24, with less than one month of work experience and still living at the parental home at the end of the registration year.

(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 registration year. Annual household income is defined as the sum of the incomes 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. Descriptive statistics of the evaluation sample

	Pre-reform		Post-reform	
	Control	**Treated**	**ΔControl**	**ΔTreated**
Women(a)	57	53.2	-1.5	-1
Belgian(a)	99.2	98.7	0	-0.9*
Migration background(a)				
At least one non-Belgian parent	13.6	17.4	0.8	2
At least one non-Belgian grandparent	36.4	41.4	-3.2	-3.1
Region of residence(b)				
Flanders	61.4	53.7	-1	-1.2
Wallonia	32.3	37	1.3	1.3
Brussels	6.3	9.3	-0.3	-0.1
STEM master(b)	23.2	19.3	1.1	1.7
Type of household(c)				
Married couple	77.8	74.2	-0.6	-0.8
One-parent	17.8	20.6	0.2	-0.3
Unmarried couple	4.4	5.2	0.4	1.1
Household size (mean) (a)	3.9	3.9	-0.1	-0.1
Number of siblings (a) (mean)	1	1	0	0
Annual household income	96,300	90,900	700	1,300
Month of first registration at the PES				
January	1.3	0	-0.1	0
February	1.9	0.4	0.2	0.4
March	0.7	0.3	-0.1	-0.2
April	0.4	0.3	-0.1	-0.2
May	0.3	0.4	0.1	-0.2
June	3.0	2.5	-0.3	-0.7
July	34.3	23.4	1.9	3.5
August	20.2	18.1	-4.3	-2.8
September	27.8	35.9	3.2	2.3
October	7.6	13.0	0	-1.7
November	1.4	3.9	-0.4	-1.6*
December	1.1	1.8	-0.1	1.2**
N	**5,849**	**2,127**	**2,909**	**1,141**

Note: Background characteristics for the treatment and control groups before and after the reform. Outcomes in the post-reform period are expressed as changes relative to the outcome in the pre-reform period. Treatment group = 24-year-old job seekers. Control group = 22- and 23-year-old job seekers. Age is measured at the end of the month of first registration at the PES. * 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 registration year. Annual household income is defined as the sum of the incomes 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 A3. Benefit of the activation allowance

	End of the 1st calendar year	*End of the 2nd calendar year*
Treatment effect	***-0.107 ******	***-0.041 ******
(se)	***(0.007)***	***(0.004)***
P-value	0.000	0.000
Counterfactual	0.108	0.041
P-value of the parallel trends test	0.575	0.530
N	**12,026**	**12,026**

Note: Evaluation sample = master's graduates registering for the first time at the PES in 2012, 2013, or 2014, with less than one month of work experience and still living at the parental home at the end of the registration year. Treatment group = 24-year-old job seekers. Control group = 22- and 23-year-old job seekers. Age is measured at the end of the month of first registration at the PES. Results are based on the estimation of Equation (1). The counterfactual outcome is the predicted outcome for the treatment group in absence of the treatment in the post-reform period. The parallel trends assumption is tested by including an interaction term between the pre-reform year dummy and the treatment dummy in the regression. * p<0.10, ** p<0.05, *** p<0.01.

Table A4. Benefit of the activation allowance by gender

	End of the 1st calendar year		*End of the 2nd calendar year*	
	Men	**Women**	**Men**	**Women**
Treatment effect	***-0.089******	***-0.125******	***-0.024******	***-0.057******
(se)	***(0.010)***	***(0.011)***	***(0.006)***	***(0.007)***
p-value	0.000	0.000	0.000	0.000
Counterfactual outcome	0.089	0.127	0.024	0.057
P-values of various tests				
Equality of the effects		0.023		0.001
Parallel trendss	0.613	0.217	0.341	0.985
N	**5,330**	**6,696**	**5,330**	**6,696**

Note: Evaluation sample = master's graduates registering for the first time at the PES in 2012, 2013, or 2014, with less than one month of work experience and still living at the parental home at the end of the registration year. Treatment group = 24-year-old job seekers. Control group = 22- and 23-year-old job seekers. Age is measured at the end of the month of first registration at the PES. Treatment effects are estimated by running Equation (1) separately for men and women. The counterfactual outcome is the predicted outcome for the treatment group in absence of the treatment in the post-reform period. The parallel trends assumption is tested by including an interaction term between the pre-reform year dummy and the treatment dummy in the regression. To formally assess whether treatment effects differ by gender, we re-estimate Equation (1) with all parameters interacted with a gender dummy. The p-value associated with the triple term interaction between treatment, post-reform, and gender dummies tests the null hypothesis of equal treatment effects across genders. * p<0.10, ** p<0.05, *** p<0.01.

Table A5. Rate of residence at the parental home

	End of the 1st calendar year	*End of the 2nd calendar year*
Treatment effect	***0.028***	***-0.002***
(se)	***(0.024)***	***(0.019)***
P-value	0.123	0.922
Counterfactual	0.658	0.466
P-value of the parallel trends test	0.420	0.420
N	**12,026**	**12,026**

Note: Evaluation sample = master's graduates registering for the first time at the PES in 2012, 2013, or 2014, with less than one month of work experience and still living at the parental home at the end of the registration year. Treatment group = 24-year-old job seekers. Control group = 22- and 23-year-old job seekers. Age is measured at the end of the month of first registration at the PES. Results are based on the estimation of Equation (1). The counterfactual outcome is the predicted outcome for the treatment group in absence of the treatment in the post-reform period. The parallel trends assumption is tested by including an interaction term between the pre-reform year dummy and the treatment dummy in the regression. * p<0.10, ** p<0.05, *** p<0.01.

Table A6. Rate of residence at the parental home by gender

	End of the 1st calendar year		*End of the 2nd calendar year*	
	Men	**Women**	**Men**	**Women**
Treatment effect	***0.070****	***-0.006***	***0.018***	***-0.017***
(se)	***(0.025)***	***(0.025)***	***(0.029)***	***(0.027)***
p-value	0.007	0.812	0.524	0.533
Counterfactual outcome	0.669	0.645	0.474	0.454
P-values of various tests				
Equality of the effects		0.038		0.373
Parallel trends	0.267	0.984	0.507	0.653
N	**5,330**	**6,696**	**5,330**	**6,696**

Note: Evaluation sample = master's graduates registering for the first time at the PES in 2012, 2013, or 2014, with less than one month of work experience and still living at the parental home at the end of the registration year. Treatment group = 24-year-old job seekers. Control group = 22- and 23-year-old job seekers. Age is measured at the end of the month of first registration at the PES. Treatment effects are estimated by running Equation (1) separately for men and women. The counterfactual outcome is the predicted outcome for the treatment group in absence of the treatment in the post-reform period. The parallel trends assumption is tested by including an interaction term between the pre-reform year dummy and the treatment dummy in the regression. To formally assess whether treatment effects differ by gender, we re-estimate Equation (1) with all parameters interacted with a gender dummy. The p-value associated with the triple interaction term between treatment, post-reform, and gender dummies tests the null hypothesis of equal treatment effects across genders. * p<0.10, ** p<0.05, *** p<0.01.

Table A7. Employment rate by gender

	End of the 1st calendar year		*End of the 2nd calendar year*	
	Men	**Women**	**Men**	**Women**
Treatment effect	***-0.004***	***0.049*****	***-0.003***	***0.060******
(se)	***(0.026)***	***(0.022)***	***(0.022)***	***(0.019)***
p-value	0.859	0.027	0.907	0.002
Counterfactual outcome	0.744	0.724	0.816	0.792
P-values of various tests				
Equality of the effects		0.106		0.032
Parallel trends	0.887	0.612	0.759	0.217
N	**5,330**	**6,696**	**5,330**	**6,696**

Note: Evaluation sample = master's graduates registering for the first time at the PES in 2012, 2013, or 2014, with less than one month of work experience and still living at the parental home at the end of the registration year. Treatment group = 24-year-old job seekers. Control group = 22- and 23-year-old job seekers. Age is measured at the end of the month of first registration at the PES. Treatment effects are estimated by running Equation (1) separately for men and women. The counterfactual outcome is the predicted outcome for the treatment group in absence of the treatment in the post-reform period. The parallel trends assumption is tested by including an interaction term between the pre-reform year dummy and the treatment dummy in the regression. To formally assess whether treatment effects differ by gender, we re-estimate Equation (1) with all parameters interacted with a gender dummy. The p-value associated with the triple interaction term between treatment, post-reform, and gender dummies tests the null hypothesis of equal treatment effects across genders. * p<0.10, ** p<0.05, *** p<0.01.

Table A8. Sensitivity analyses on housing outcomes by gender with treatment group aged 23

	End of the 1st calendar year				*End of the 2nd calendar year*			
	Parents	Alone	Partner	Shared housing	Parents	Alone	Partner	Shared housing
Panel A: Men								
Treatment effect (se)	0.067** (0.026)	-0.011 (0.016)	-0.004 (0.022)	-0.042*** (0.014)	0.017 (0.028)	-0.007 (0.021)	0.023 (0.027)	-0.031* (0.018)
Counterfactual outcome	0.671	0.080	0.166	0.084	0.476	0.135	0.269	0.120
Panel B: Women								
Treatment effect (se)	-0.012 (0.037)	0.011 (0.022)	0.002 (0.032)	-0.004 (0.012)	-0.011 (0.040)	-0.006 (0.029)	0.046* (0.026)	-0.028* (0.023)
Counterfactual outcome	0.651	0.087	0.209	0.053	0.449	0.145	0.317	0.089
P-value of equality test	0.041	0.323	0.607	0.028	0.503	0.963	0.548	0.875
N	**9,916**	**9,916**	**9,916**	**9,916**	**9,916**	**9,916**	**9,916**	**9,916**

Note: The sensitivity analysis presented in the table is made on the following alternative sample: master's graduates registering for the first time at the PES in 2012, 2013, or 2014, with less than one month of work experience and still living at the parental home at the end of the registration year. Treatment group = 24-year-old job seekers. Control group = 23-year-old job seekers. Age is measured at the end of the month of first registration at the PES. Treatment effects are estimated by running Equation (1) separately for men and women. The counterfactual outcome is the predicted outcome for the treatment group in absence of the treatment in the post-reform period. The parallel trends assumption is tested by including an interaction term between the pre-reform year dummy and the treatment dummy in the regression. To formally assess whether treatment effects differ by gender, we re-estimate Equation (1) with all parameters interacted with a gender dummy. The p-value associated with the triple interaction term between treatment, post-reform, and gender dummies tests the null hypothesis of equal treatment effects across genders. * p<0.10, ** p<0.05, *** p<0.01.

Table A9. Sensitivity analyses on housing outcomes by gender with treatment group registered between July and October

	End of the 1st calendar year				*End of the 2nd calendar year*			
	Parents	Alone	Partner	Shared housing	Parents	Alone	Partner	Shared housing
Panel A: Men								
Treatment effect (se)	0.069** (0.027)	-0.025* (0.016)	0.002 (0.022)	-0.045*** (0.013)	0.020 (0.031)	-0.016 (0.020)	0.032 (0.027)	-0.034** (0.017)
Counterfactual outcome	0.671	0.091	0.151	0.086	0.477	0.137	0.269	0.116
Panel B: Women								
Treatment effect (se)	0.000 (0.038)	0.003 (0.022)	0.005 (0.032)	-0.007 (0.018)	-0.025 (0.041)	-0.007 (0.028)	0.060** (0.038)	-0.027* (0.022)
Counterfactual outcome	0.637	0.094	0.211	0.056	0.453	0.145	0.313	0.088
P-value of equality test	0.074	0.191	0.940	0.038	0.271	0.758	0.454	0.738
N	**10,834**	**10,834**	**10,834**	**10,834**	**10,834**	**10,834**	**10,834**	**10,834**

Note: The sensitivity analyses presented in the table is made on the following alternative sample: master's graduates registering for the first time at the PES between July and October 2012, 2013, or 2014, with less than one month of working experience and still living at the parental home at the end of the registration year. Treatment group = 24-year-old job seekers. Control group = 22- and 23-year-old job seekers. Age is measured at the end of the month of first registration at the PES. Treatment effects are estimated by running Equation (1) separately for men and women. The counterfactual outcome is the predicted outcome for the treatment group in absence of the treatment in the post-reform period. The parallel trends assumption is tested by including an interaction term between the pre-reform year dummy and the treatment dummy in the regression. To formally assess whether treatment effects differ by gender, we re-estimate Equation (1) with all parameters interacted with a gender dummy. The p-value associated with the triple interaction term between treatment, post-reform, and gender dummies tests the null hypothesis of equal treatment effects across genders. * p<0.10, ** p<0.05, *** p<0.01.

Table A10. Sensitivity analyses on employment rate by gender

	End of the 1st calendar year		*End of the 2nd calendar year*	
	[23 ;24[	**July-Oct**	**[23 ;24[**	**July-Oct**
Men				
Treatment effect	***-0.002***	***-0.015***	***-0.007***	***0.000***
(se)	***(0.026)***	***(0.026)***	***(0.023)***	***(0.023)***
p-value	0.925	0.557	0.761	0.988
Counterfactual outcome for the treatment group	0.742	0.750	0.821	0.822
Women				
Treatment effect	***0.043****	***0.055*****	***0.062******	***0.065******
(se)	***(0.035)***	***(0.035)***	***(0.031)***	***(0.030)***
p-value	0.069	0.016	0.003	0.001
Counterfactual outcome for the treatment group	0.730	0.731	0.791	0.794
P-values of equality test	0.198	0.042	0.027	0.032
N	**9,916**	**10,834**	**9,916**	**10,834**

Note: The sensitivity analyses presented in the table are made on two alternative evaluation samples. (1) Evaluation sample = master's graduates registering for the first time at the PES in 2012, 2013, or 2014, with less than one month of working experience and still living at the parental home at the end of the registration year. Treatment group = 24-year-old job seekers. Control group = 23-year-old job seekers. (2) Evaluation sample = master's graduates registering for the first time at the PES between July and October 2012, 2013, or 2014, with less than one month of working experience and still living at the parental home at the end of the registration year. Treatment group = 24-year-old job seekers. Control group = 22- and 23-year-old job seekers. Age is measured at the end of the month of first registration at the PES. Treatment effects are estimated by running Equation (1) separately for men and women. The counterfactual outcome is the predicted outcome for the treatment group in absence of the treatment in the post-reform period. The parallel trends assumption is tested by including an interaction term between the pre-reform year dummy and the treatment dummy in the regression. To formally assess whether treatment effects differ by gender, we re-estimate Equation (1) with all parameters interacted with a gender dummy. The p-value associated with the triple interaction term between treatment, post-reform, and gender dummies tests the null hypothesis of equal treatment effects across genders. * p<0.10, ** p<0.05, *** p<0.01.

Table A11. Adjusted p-values for men

	End of the 1st calendar year			*End of the 2nd calendar year*		
	Benchmark	**Bonferroni**	**Holm**	**Benchmark**	**Bonferroni**	**Holm**
Benefit of the activation allowance	0.000	0.000	0.000	0.000	0.007	0.007
Housing outcomes (residing with…)						
Parents	0.007	0.044	0.029	0.524	1.000	1.000
Alone	0.246	1.000	0.737	0.603	1.000	1.000
Partner	0.708	1.000	1.000	0.358	1.000	1.000
Shared housing	0.001	0.005	0.005	0.065	0.388	0.323
Employment rate	0.859	1.000	1.000	0.907	1.000	1.000
N	**5,330**	**5,330**	**5,330**	**5,330**	**5,330**	**5,330**

Note: Evaluation sample = master's graduates registering for the first time at the PES in 2012, 2013, or 2014, with less than one month of work experience and still living at the parental home at the end of the registration year. Treatment group = 24-year-old job seekers. Control group = 22- and 23-year-old job seekers. Age is measured at the end of the month of first registration at the PES. Treatment effects are obtained by jointly estimating Equation (1) across all outcomes using a seemingly unrelated regression. For each outcome and time horizon, we report the p-value of the interaction between the treatment and the post-reform year indicators adjusted for multiple testing using Bonferroni and Holm procedures (controlling the family-wise error rate). For each outcome and time horizon, the first, second, and third columns report conventional, Bonferroni-adjusted, and Holm-adjusted p-values, respectively.

Table A12. Adjusted p-values for women

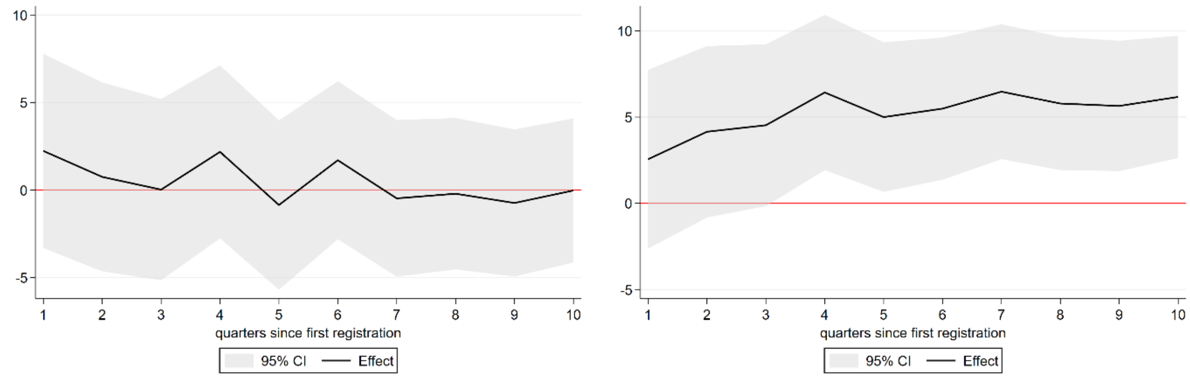
	End of the 1st calendar year			*End of the 2nd calendar year*		
	Benchmark	**Bonferroni**	**Holm**	**Benchmark**	**Bonferroni**	**Holm**
Benefit of the activation allowance	0.000	0.000	0.000	0.000	0.000	0.000
Housing outcomes (residing with…)						
Parents	0.812	1.000	1.000	0.533	1.000	1.000
Alone	0.529	1.000	1.000	0.919	1.000	1.000
Partner	0.981	1.000	1.000	0.053	0.318	0.159
Shared housing	0.840	1.000	1.000	0.037	0.220	0.147
Employment rate	0.027	0.157	0.131	0.002	0.013	0.010
N	**6,696**	**6,696**	**6,696**	**6,696**	**6,696**	**6,696**

Note: Evaluation sample = master's graduates registering for the first time at the PES in 2012, 2013, or 2014, with less than one month of work experience and still living at the parental home at the end of the registration year. Treatment group = 24-year-old job seekers. Control group = 22- and 23-year-old job seekers. Age is measured at the end of the month of first registration at the PES. Treatment effects are obtained by jointly estimating Equation (1) across all outcomes using a seemingly unrelated regression. For each outcome and time horizon, we report the p-value of the interaction between the treatment and the post-reform year indicators adjusted for multiple testing using Bonferroni and Holm procedures (controlling the family-wise error rate). For each outcome and time horizon, the first, second, and third columns report conventional, Bonferroni-adjusted, and Holm-adjusted p-values, respectively.

Appendix B: Additional figures

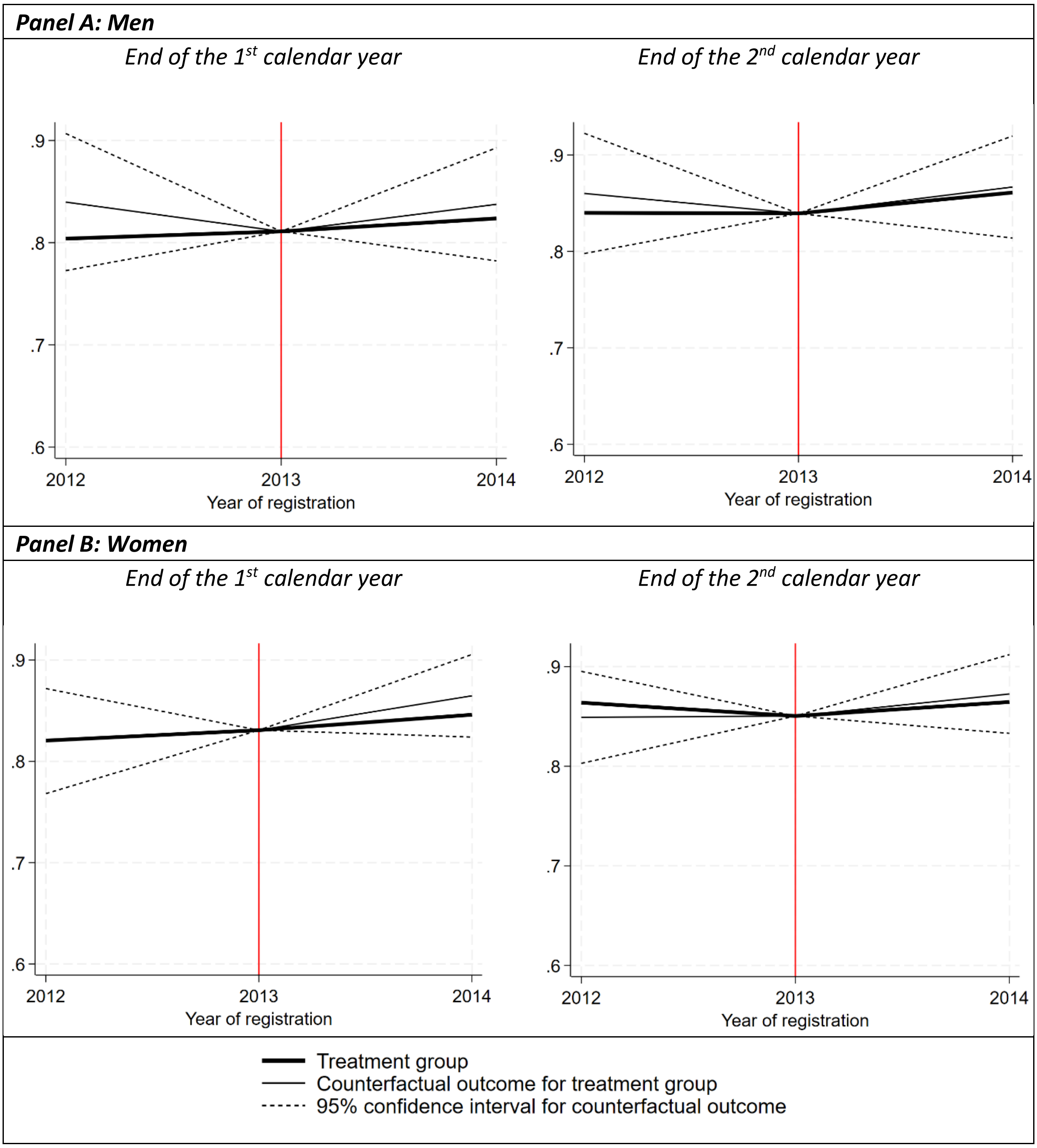
Figure B1. Employment rate at the end of each quarter from quarter 1 to 16

Men *Women*



Note: Evaluation sample = master's graduates registering for the first time at the PES in 2012, 2013, or 2014, with less than one month of working experience and still living at the parental home at the end of the registration year. Treatment group = 24-year-old job seekers. Control group = 22- and 23-year-old job seekers. Age is measured at the end of the month of first registration at the PES. Treatment effects are estimated by running Equation (1) separately for men and women. The counterfactual outcome is the predicted outcome for the treatment group in absence of the treatment in the post-reform period. The graph displays the coefficient of the interaction term between the treatment and post-reform dummies ($\boldsymbol{\delta}$), along with its 95% confidence interval at each time horizon.

Figure B2. Employment rate **for youths residing OUT of the parental home at the end of the registration year by gender**



Note: Evaluation sample = master's graduates registering for the first time at the PES in 2012, 2013, or 2014, with less than one month of work experience and living OUT OF the parental home at the end of the registration year. Treatment group = 24-year-old job seekers. Control group = 22- and 23-year-old job seekers. Age is measured at the end of the month of first registration at the PES. Graphs are based on the estimation of Equation (1) for men and women separately. N=2,964 for women; N=1,615 for men.

INSTITUT DE RECHERCHE
ÉCONOMIQUES ET SOCIALES

Place Montesquieu 3

1348 Louvain-la-Neuve

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