Journal of Public Economics 224 (2023) 104929

Contents lists available at ScienceDirect

Journal of Public Economics

journal homepage: www.elsevier.com/locate/jpube

# The consequences of job search monitoring for the long-term unemployed: Disability instead of employment? ☆,☆☆

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ARTICLE INFO

*Article history:*
Received 29 June 2021
Revised 1 June 2023
Accepted 8 June 2023
Available online 24 June 2023

*JEL Classification:*
I13
J64

*Keywords:*
Disability
Unemployment
Job search monitoring
Active labor market programs

ABSTRACT

We study the effect of job search monitoring (JSM) on individual labor market outcomes of the long-term unemployed. Exploiting the implementation of a JSM program targeted at jobseekers under the age of 49, we set up a regression discontinuity design that credibly identifies the program's causal effect on unemployment, employment, and disability insurance (DI) participation and participation in other social welfare programs within a three-year period. We find that JSM increases exits from unemployment to DI without affecting transitions into employment or other social welfare programs. We further find that the effect of JSM on DI materializes before any sanction can be imposed and monitored individuals are still 10 percentage points more likely to be on DI three years after the start of monitoring. Ultimately, exploring fiscal implications reveals that the decrease in unemployment transfers as a result of JSM is entirely offset by the increase in DI transfers.



## 1. Introduction

Long-term (LT) unemployment has been at the top of policymakers' and researchers' agendas for many years. In 2021, 39% of the EU's 15 million jobseekers––or 5.9 million people––had been unemployed for more than 12 months, and the Great Recession has imported this traditionally European concern to the United States. LT unemployment is problematic for both society and individuals. For society, it is costly in terms of social safety net expenditures and unused productive capacity. For individuals, long periods of unemployment increase the risk of poverty and social exclusion, have adverse effects on health, and lower overall life satisfaction (e.g., Gerdtham and Johannesson, 2003; Kassenboehmer and Haisken-DeNew, 2009).

Many countries have adopted job search monitoring (JSM) programs to foster a prompt return into employment after job displacement by limiting moral hazard issues within unemployment insurance (UI). However, it is unclear to what extent these programs can be effective at raising employment when targeted at the LT unemployed. On the one hand, since job search efforts decrease with unemployment duration, such programs might be particularly effective at sustaining job search efforts among LT unemployed individuals. On the other hand, since these individuals have a weak attachment to the labor market (Krueger et al., 2014), these programs could also foster exits from the labor market through other social safety net programs.

In this paper, we study the effects of a JSM program implemented in Belgium in 2004 that targets LT unemployed individuals, focusing on both transitions into employment and potential substitution effects with other social safety net programs. After

☆ All authors contributed equally to this work. The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

☆☆ We thank the editor and three anonymous referees for helpful comments and suggestions. This work is supported by the Belgian National Institute for Health and Disability Insurance (NIHDI). Elisabeth Leduc is grateful to the Fonds National de la Recherche Scientifique (FRS-FNRS) for financial support. We are grateful to Patrick Arni, Eve Caroli, Bart Cockx, Muriel Dejemeppe, Saskia Decuman, Nick Deschacht, Sébastien Fontenay, Thomas Murphy, Kim Oosterlinck, Guillaume Perilleux, François Perl, François Rycx, Ariane Szafarz, Todd Sorensen, Vincent Vandenberghe, Bruno Van der Linden, and Dieter Verhaest for helpful comments and suggestions as well as participants of the 2018 Belgian Day for Labour Economists, the 2019 DULBEA Workshop on the Economics of Disability, and the economics seminars at KULeuven, ULB, and the University of Namur.

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https://doi.org/10.1016/j.jpubeco.2023.104929
0047-2727/

15 months of unemployment, participants were invited to periodically visit their employment agency to be interviewed by a caseworker on their job search activities. The consequences of a negative evaluation ranged from a mere warning to permanent exclusion from UI. The focus of the Belgian JSM program on the LT unemployed contrasts with the choice of most other countries to start monitoring after just a few weeks. This allows us to evaluate the effects of JSM when it starts at a more advanced stage of the unemployment spell.

Our empirical strategy exploits the fact that between July 2007 and January 2013, the UI agency restricted the inclusion of UI beneficiaries into the JSM program to those who reached 15 months of unemployment before their 49th birthday. We use detailed individual information from register data on the universe of Belgian UI beneficiaries to set up a regression discontinuity (RD) design at age 49. We then estimate the effect of JSM, within a three-year period, on unemployment, employment, disability insurance (DI) participation as well as participation in other social welfare programs (SWP).[1]

We find that JSM leads to a significant 16.6 percentage point (pp) increase in the likelihood of LT unemployed individuals exiting unemployment. However, this increase is primarily driven by a higher probability of receiving DI benefits, with a 12.6 pp rise. In contrast, JSM does not seem to have a noticeable effect on the probability of LT unemployed individuals ever finding employment or engaging in other SWP. We find similar results when looking at the effects of JSM on the cumulative number of days spent in each status. For the LT unemployed, the "spillover effects" of JSM on other social safety net programs thus appear to be restricted to DI, reinforcing previous findings that DI is a close substitute to UI (Borghans et al., 2014; Andersen et al., 2019).

We provide further insights into the underlying mechanisms that are driving our findings. First, in a dynamic analysis, we document that the exits from UI to DI persist for up to three years after the monitoring procedure starts. These effects become evident before any sanctions can be imposed for non-compliance with job search requirements, suggesting that the mere threat of a sanction is enough to encourage individuals to transition to DI. Second, we provide more insights into the effects on DI. We show that JSM increases transitions to both short-term (ST) and LT DI, suggesting that health impairments are sufficiently serious to warrant entry into the LT DI program. These increases are driven by musculoskeletal and psychological disorders. In addition, individuals on the left and right of the cutoff exhibit similar exclusion rates from DI, indicating that those who transition to DI as a result of JSM are not in better health than other DI recipients. Third, in our analysis of fiscal implications, we show that the decrease in UI transfers as a result of JSM is almost entirely offset by the increase in DI transfers.

Our paper contributes to the literature on JSM programs by studying their effects on the LT unemployed population. An early literature developed job search models with endogenous search effort and sanctions to study the effect of JSM on the job finding rate (Van den Berg et al., 2004; Abbring et al., 2005; Van den Berg and van der Klaauw, 2006; Boone et al., 2007). This theoretical literature predicts that JSM increases job finding for individuals with initially low search efforts and reduces reservation wages, as the present value of UI decreases relative to employment. These predictions are confirmed by a large body of empirical work (e.g., Van den Berg et al., 2004; Lalive et al., 2005; Cockx and Dejemeppe, 2012; Arni et al., 2013; Lammers et al., 2013; Van den Berg & Vikström, 2014; Avram et al., 2018; Card et al., 2018; Arni and Schiprowski, 2019), although some find small or insignificant effects of JSM on employment (e.g., Van den Berg and van der Klaauw, 2006; Ashenfelter et al., 2005; Cockx et al., 2018). The effect of JSM on the job finding rate might be alleviated when the program is imposed on individuals who exhibit high search costs (e.g., due to difficulties writing a motivation letter or CV) or reservation wages that are close or equal to the legal minimum wage. This could be particularly relevant for the LT unemployed population, who have a weak connection to the labor market (Krueger et al., 2014).[2]

Moreover, our paper contributes to a literature documenting substitution effects between social safety net programs as a result of changes in eligibility rules or replacement benefits (e.g., Autor and Duggan, 2003; Karlström et al., 2008; Staubli, 2011; Staubli and Zweimüller, 2013; Borghans et al., 2014; Haller et al., 2020; Johnsen et al., 2022). In this vein, three papers directly focus on JSM to show that it can affect participation in other social safety net programs, although none focus on the LT unemployed population. Petrongolo (2009) finds that starting an unemployment spell soon after the introduction of the Jobseekers' Allowance (JSA) in the UK increases the likelihood of receiving disability benefits by 2.5%-3% in the following year. Lammers et al. (2013) find that imposing JSM on the unemployed aged 57.5–59.5 increases their probability of transitioning to DI within two years by 4.3 pp for men and 9.1 pp for women. Avram et al. (2018) show that imposing job search requirements on single parents increases their probability of receiving health-related benefits by 18 pp after nine months. These studies thus indicate that the implementation of JSM increases the occurrence of disability among the unemployed population, which echoes a literature showing that DI is a close substitute to UI (Black et al., 2002; Charles et al., 2018; Andersen et al., 2019).

Our key contribution to the literature is to assess the overall effect––including spillover effects on other social safety net programs––of implementing JSM on LT UI beneficiaries. Because of their weak attachment to the labor market, LT unemployed individuals might be particularly prone to substituting away from UI to other social safety net programs as a result of JSM. Our paper indeed suggests that in the case of weakly employable individuals (in this case the LT unemployed), substitution effects of JSM can overshadow labor supply effects.

In addition, we provide several novel insights on the mechanisms driving the effects of JSM. First, we complement existing studies showing that a sanction threat is sufficient to foster exits from UI (Black et al., 2003; Boone et al., 2009; Arni et al., 2013) by demonstrating that this also applies to the LT unemployed population but through DI instead of employment. Second, we show suggestive evidence that the effects of JSM on DI documented in this paper are driven by existing health impairments among the LT unemployed population. Finally, our fiscal analysis allows us to conclude that JSM, when targeted at the LT unemployed, generates fiscal spillovers to other social safety net programs that counterbalance the saving on UI spending.

The rest of the paper is outlined as follows. In the next section, we briefly explain the Belgian institutional setting. Section 3 describes the data used for conducting our estimations, and Section 4 presents our identification strategy. Section 5 outlines the empirical findings, and Section 6 ends with a concluding discussion.

[1] Other SWP include early retirement, social integration benefits ("CPAS" in French, "OCMW" in Dutch), professional illnesses, workplace accidents, and assistance to individuals with a handicap.

[2] This phenomenon occurs because of self-selection—due to ex-ante heterogeneity—and duration dependence—due to skill depreciation or duration-based employer screening (Mueller et al., 2021; Kroft et al., 2013).

## 2. Institutional context

### 2.1. The JSM program

In comparison with other JSM programs that were implemented in European countries, the Belgian program we study has some features that can be summarized along three dimensions: (i) lengthy intervals between interviews, (ii) severe sanctions following negative evaluations, and (iii) high discretionary power for caseworkers overseeing the evaluations. The first feature allows us to exploit the Belgian context to study the effects of JSM on the LT unemployed.

In 2004, the Belgian government introduced job search requirements and monitoring for LT unemployed individuals.[3] Before then, although activation programs were regularly proposed to UI beneficiaries by the Public Employment Services (PES), refusal to participate did not entail any sanction and job search was not monitored. The reform targeted all UI beneficiaries under the age of 50 who were unemployed for longer than 15 months. Due to the high number of targeted participants, the program was gradually phased in between July 2004 and July 2007, beginning with the youngest participants. The program was fully operational for any UI beneficiary under age 50 from July 2007 onward. To ensure that no UI beneficiary would be monitored after age 50, the federal UI agency decided to restrict the inclusion of new participants in the monitoring program to those who did not reach age 49 by their 15th month of unemployment.[4]

Fig. 1 illustrates the different steps of the JSM program. UI beneficiaries with an unemployment duration of 15 months are sent a notification letter informing them of their obligation to actively search for a job. The letter also says they will be invited to go to the UI agency at least 6 months later (i.e., after 21 months of unemployment) for an interview with a caseworker. In comparison with most other countries, the Belgian JSM thus starts very late in the unemployment spell and is characterized by infrequent meetings. UI recipients have meetings every four weeks in Switzerland (Arni et al., 2013), every four to six weeks in the Netherlands (Lammers et al., 2013), and every two weeks in the UK (Petrongolo, 2009; Avram, 2018). In all these countries, JSM starts at the beginning of the unemployment spell.

During the first interview, UI beneficiaries must report their job search activities over the last 12 months. In contrast to most JSM programs, the number of job applications is not defined before the first interview, giving substantial discretionary power to the caseworker during the evaluation process (Cockx et al., 2018).[5] At the end of the interview, the caseworker decides whether the job search efforts were sufficient. Following a positive evaluation, the jobseeker is sent a new notification letter eight months later, inviting them for another interview.[6] After a negative evaluation, an action plan is set up by the caseworker, and the jobseeker is invited to sign a contract in which they commit to taking a number of defined actions, such as applying for a defined minimum number of job openings, contacting the regional PES, and/or registering at a temporary work agency. No sanction is imposed after a negative first interview.[7]

The fulfillment of the contract is monitored during a second interview four months later. If the evaluation of this interview is positive, then the procedure starts again from the beginning and the jobseeker is invited for a new "first" interview four months later. If the evaluation is negative, a financial sanction is imposed[8] and they are invited to sign a new contract with new actions to be undertaken. The fulfillment of this new contract is then evaluated during a third and final interview four months later, with the jobseeker being excluded from UI if the evaluation is negative.[9] Compared with several other countries, exclusion from UI is a relatively severe sanction. In Switzerland, the maximum sanction is a 100% reduction in benefits for 60 working days (Arni et al., 2013), while it is a 30% cut in benefits for 16 weeks in the Netherlands (Lammers et al., 2013).

### 2.2. The DI system

In Belgium, the DI system is publicly provided by the National Institute for Health and Disability Insurance (NIHDI). The system guarantees insurance coverage for both employed and unemployed workers who meet a specified threshold of some minimum amount of seniority and prior earnings, safeguarding them against health shocks that may affect their ability to work. Disability payments are provided as a fraction of the individual's last monthly wage.[10]

For DI beneficiaries, the disability regime can be divided into two main categories depending on the length of the impairment. During the first 365 days of disability, individuals are considered ST disabled and are covered by a program called "primary incapacity." During the ST disability period, they are examined by a doctor designated by their health insurance fund to evaluate their ability to work.[11] To be recognized as disabled, their ability to work must be reduced by at least 66% with respect to their previous occupation.[12] ST DI benefits are equal to UI benefits.

If the impairment lasts more than a year, the individual enters the LT disability program. During the LT disability period, the remaining ability to work is evaluated directly by the NIHDI, whose screening process is more stringent. In practice, to be accepted into

[3] The launch of the JSM program was not the only reform to be implemented at the time. Other reforms included the systematic exchange of information between regional employment agencies and the national employment agency and increasing the financial means of regional employment agencies. Higher information exchanges could be used to impose eventual sanctions following the refusal of a suitable job offer or non-attendance at a meeting at the regional employment agency.

[4] Although the program's official rules state that monitoring starts after 13 months of unemployment with the reception of a notification letter, in practice, the letters were sent after 15 months of unemployment because the information on unemployment duration was accessible to the unemployment agency with a delay of two months.

[5] Initially, this was meant to provide requirements that would be adapted to the individual situation of each jobseeker, but many observers have criticized the creation of additional uncertainty and anxiety for the jobseeker around the risk of sanction.

[6] Note that in our setting, the "treatment" is defined as being subject to JSM until having a positive evaluation or exiting unemployment. Indeed, one positive interview is sufficient to become definitively exempt from JSM since our sample is close to the cutoff age and will thus not be eligible for a second notification letter.

[7] However, sanctions can be applied if the UI beneficiary refuses to sign the contract proposed by the caseworker.

[8] The sanction's size depends on one's position in the household. For single households and heads of household, monthly benefits are reduced to the legal minimum income for four months, which amounted in 2004 to 613 euros for single households and 817 euros for heads of household (SLPPES, 2004). For cohabitants, the benefits are completely removed for a duration of four months. If the UI recipient refuses to sign the second contract proposed, they are excluded from the UI scheme until they complete a full year of employment, which makes them eligible for UI again.

[9] The exclusion is immediate for cohabitants, while it occurs after a six-month reduction to the legal minimum income for single households and heads of household.

[10] Appendix A.1 provides a comparison of the financial conditions of UI and DI.

[11] In Belgium, although the health care system is publicly supported at the national level, the reimbursement of medical expenses and ST disability benefits are made via public health insurance funds called "mutualities," which are funded by the NIHDI and act as intermediaries. In short, to benefit from Belgian medical coverage, individuals must register with a health insurance fund and pay quarterly contributions.

[12] An important change occurs after six months of ST disability: the reduction in the earning capacity is then evaluated with respect to any occupation that the worker could perform given their age, education, and experience (instead of their previous occupation). However, in practice we do not observe significant dropouts from DI after six months, which can mean that this condition is not strictly applied by doctors in their evaluations.

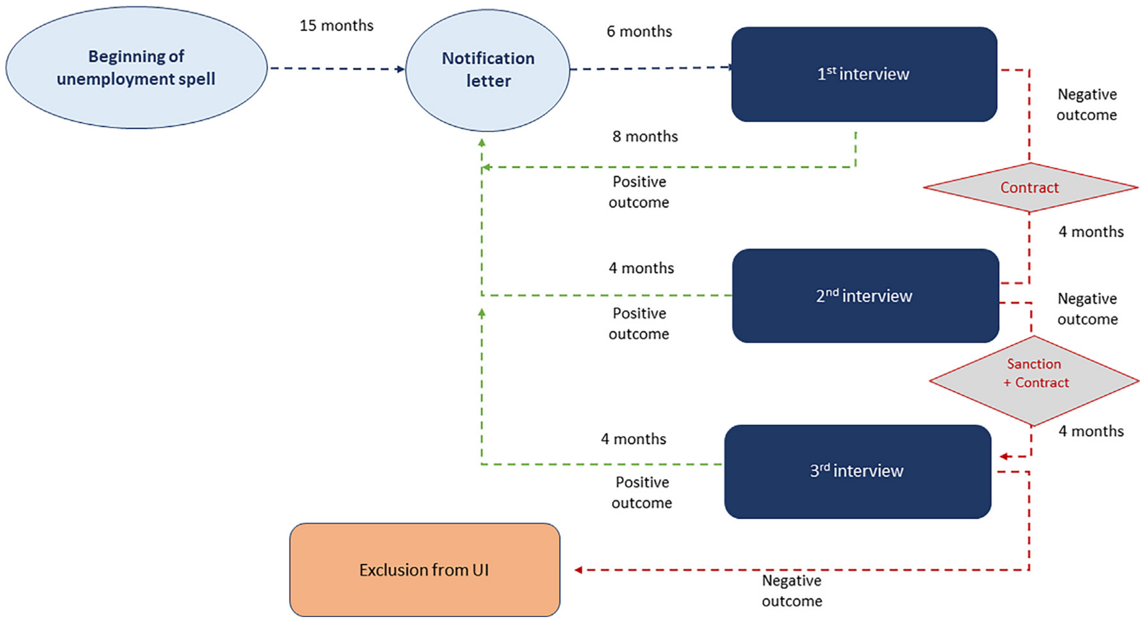


**Fig. 1.** The Belgian JSM Program. Notes: This figure illustrates the process of the job-search monitoring (JSM) program. When a worker is unemployed for 15 months, they receive a notification letter informing them that they will be invited for an interview six months later to evaluate their job search efforts over the 12 last months. The first interview thus occurs after 21 months of unemployment. If the outcome of the first interview is positive (i.e., the caseworker deems that job search efforts were sufficient), the unemployed worker is invited for another interview 14 months later (if they are still under 49 years old). If the outcome is negative (i.e., the caseworker deems that the worker's search efforts were insufficient), the worker is invited to sign a contract in which they commit to take a number of defined actions regarding their search efforts, but no financial sanctions are imposed. The fulfilment of this contract is then evaluated four months later during the second interview. If the outcome of the second interview is positive, the worker is invited for another interview ten months later. If the outcome is negative, then the worker is invited to sign a new, more extensive contract, and a financial sanction is imposed (ranging from a four-month reduction of benefits to the legal minimum income to a four-month suspension of unemployment benefits). In this case, a third and final interview occurs four months later. If the outcome of this final interview is positive, the worker is invited for another interview ten months later, and if it is negative, the worker is excluded from the unemployment scheme until they fulfil a full year of employment.

the LT disability program, a doctor at the applicant's health insurance fund (who oversaw the applicant during the ST period) submits the application to the NIHDI, which can either approve the doctor's conclusions or run its own internal evaluation and invite the applicant for a medical examination. The replacement rate for LT disability varies from 40% to 65%, depending on the individual's position in the household.

## 3. Data

### 3.1. Dataset construction

We use administrative data from the Belgian Labor Market Data Warehouse (LMDW) of the Crossroad Bank for Social Security (CBSS), which aggregates register data from governmental and social safety net institutions since 1998. We have access to individual information between January 2003 and December 2015 for any person who had a national registration number during that period.[13] Our dataset includes yearly information from national registers (for personal data) and taxable income (for data on income and transfers as well as participation in other SWP). It also includes quarterly information from the National Social Security Office (labor activity data), the National Unemployment Agency (unemployment data), the NIHDI (LT disability data), and the National Intermutualist Board (ST disability data).

In addition, the national UI agency provided us with detailed information on JSM, including the dates at which jobseekers were exposed to the different steps of the monitoring program (notification letters and interviews), the outcomes of interviews (positive, negative, or absent), and the enforced sanctions, if applicable. The unemployment duration computed by this agency for the purpose of JSM is not always identical to the time that elapsed since the date of unemployment entry, for two reasons. First, some unemployment periods (such as periods of non-active unemployment or part-time supported employment) are not counted in the JSM unemployment duration.[14] Second, the unemployment duration is reset to zero only after a continuous full-time employment period of 12 months such that unemployment spells following short periods of employment are added to the previous unemployment duration. Since the UI agency did not provide us with the unemployment duration it used to compute the month of dispatch of the notification letters, we reconstructed this variable by following the rules that were communicated to us. We can accurately predict the month of receiving a notification letter for about 54% of the observations. However, for the remaining observations, a notification letter arrived at a different time than predicted for 28% of cases, while 18% did not receive any notification letter within our observation window.

Because we study a reform that creates a discontinuity in the exposure to JSM at age 49, our sample is composed of all unemployment spells for individuals who, between July 2007 and December 2011, were aged between 44 and 54 when their unemployment duration reached 15 months (i.e., the predicted moment of dispatch of the notification letter). Moreover, to further improve the precision of our estimates, we remove individuals in the two age-month bins on the right of the cutoff (i.e., those aged 49.0 and 49.1 at the time they reached 15 months of unemployment). We do this because a significant share (20%) of these individuals receives a notification letter despite being in the control group.[15]

[13] See Appendix A.2 for more information on the data and how we construct the variables that we use in our regressions.

[14] In Belgium, active labor market programs offer unemployed workers the possibility of part-time work and retaining part of their unemployment benefits.

[15] Appendix A.6 shows that including these observations in our estimates produces very similar coefficients to our benchmark findings.

Finally, we drop any observation that has been subject to JSM during earlier unemployment spells to avoid possible selection mechanisms. To have a better idea of the share of all UI entries contained in our sample, we describe the different steps of the data selection process in Appendix A.2. We can see that the share of individuals reaching a duration of 15 months represents 6.8% of all entries into UI.[16]

Ultimately, we end up with a sample of 42,208 individuals. As shown in Table 1, our sample contains a majority of men (58%) and individuals formerly employed in blue-collar occupations[17] (59%) who hold a primary or secondary education degree (77%). Individuals in our sample had spent approximately half of their time in employment (467 days out of a maximum 936 days) in the three years preceding their unemployment spell. They had also spent a relatively high average number of past days on DI (105 days out of a maximum 936 days), but this observation is partly driven by extreme values, i.e., a few individuals exhibiting many past DI days.[18] Finally, the average time span since unemployment entry is equal to 13 months.[19]

### 3.2. Exit rates over the unemployment spell

Previous research has established that LT unemployed individuals are weakly connected to the labor market (e.g., Krueger et al., 2014; Mueller et al., 2021). Fig. 2 puts this in context for Belgium by documenting the progression of the rate of exit out of UI throughout the duration of an unemployment spell. Moreover, it sheds light on the relative importance of exits to employment, DI, and other SWP. Panel (a) shows a steep decrease in the hazard rate out of unemployment during the first six months after UI entry. As shown in panel (b), most of these exits are initially directed toward employment. Of those who exit UI after one month, 87% enter employment, while 10% and 3% enter DI and other SWP, respectively.

As unemployed jobseekers reach higher UI durations, they become less and less likely to exit unemployment. At the same time, the relative significance of exits to employment gradually decreases, whereas the relative significance of exits to DI and other SWP increases. After 15 months of unemployment, exits to employment represent 60% of all exits, while exits to DI and other SWP represent 25% and 15%, respectively. The significance of exits to DI and other SWP becomes even more pronounced for UI durations between 15 and 60 months. After 60 months, exits to employment represent only 27% of all exits, while exits to DI and other SWP represent 43% and 30%, respectively. All in all, these results imply that, as unemployment duration increases, unemployed individuals become increasingly likely to transition to DI or other SWP rather than to employment.

## 4. Empirical design

### 4.1. RD design

To identify the effect of JSM on the outcomes of interest, we exploit the fact that during the period of analysis (July 2007 to December 2011), the monitoring procedure was imposed on UI beneficiaries who were under 49 years old at their 15th month of unemployment. This allows us to implement an RD design in which we estimate the discontinuity at age 49 of several outcomes up to three years after treatment for UI beneficiaries whose unemployment duration is exactly 15 months.

Our design is modeled as follows:

$$Y_{ik} = \gamma + \beta \times 1[\mathrm{a}_{i0} < \mathrm{c}] + 1[\mathrm{a}_{i0} < \mathrm{c}] \times f_b(a_{i0} - c) + 1[\mathrm{a}_{i0} \geq \mathrm{c}] \times f_a(a_{i0} - c) + \boldsymbol{\delta} \times \boldsymbol{X}_i + \varepsilon_{ik}, \tag{1}$$

where $Y_{ik}$ is the outcome of interest for individual $i$, $k$ quarters after the predicted time of receiving the notification letter; $\gamma$ is a constant; and $1[\mathrm{a}_{i0} < \mathrm{c}]$ ($1[\mathrm{a}_{i0} \geq \mathrm{c}]$, respectively) is a Boolean indicating whether the individual's age is below (above, respectively) the cutoff age c. $f_{\mathrm{b}}$ and $f_{\mathrm{a}}$ are polynomial functions below and above the cutoff, respectively. $\boldsymbol{X}_i$ is a vector of individual control variables (including year of unemployment entry, gender, household category, a dummy for Belgian nationality, region, education, contract type of the last job, last daily wage, length of unemployment, and cumulative worked days and DI days in the three years preceding unemployment entry), and $\varepsilon_{ik}$ is an error term. In our analyses, we consider a maximum time horizon of three years (*k = 12*) following the predicted date of receiving the notification letter.[20]

In the benchmark analysis of Section 5.1, we consider three types of outcomes. First, we consider the probability of experiencing different steps of the JSM procedure as a mean to better characterize the treatment. To do so, we construct one dummy for each step of the JSM procedure (i.e., the reception of the notification letter; the first, second, and third interviews; and the enforcement of a sanction). As a placebo, we also construct a dummy equal to one if individuals are subject to a sanction outside the JSM procedure to check that other obligations than JSM are evaluated similarly across the two groups. Second, we consider the probability of being observed in different labor market statuses between *k = 0* and *k = 12.* In particular, we construct one dummy for always being unemployed[21]––i.e., being observed on UI every quarter *k (k = 0,1,...,12)* following the predicted notification date––and three dummies for being observed, respectively, in employment, DI, and other SWP at least one day between *k = 0* and *k = 12.*[22] Third, we consider the cumulative number of days spent in each status between *k = 0* and *k = 12.*

In the dynamic analysis of Section 5.2, we estimate discontinuities in the probability of being observed in a given status (UI, employment, DI, or other SWP) for each quarter *k (k = 0,1,...,12)* following the predicted notification date. This allows us to explore the time at which the effects start to materialize and whether they are persistent. It also allows us to put the effects into perspective with the predicted timing of monitoring interviews.

[16] Although only 6.8% of all entrants in our sample reach an unemployment duration of 15 months, this group represents a significant share of total UI spending, making them an important target group for active labor market programs.

[17] In Belgium, employment contracts are divided into two main categories according to whether the job involves mostly intellectual tasks (white-collar work) or physical tasks (blue-collar work).

[18] Looking at the whole distribution of past DI days in Appendix A.6 reveals that a small share of our sample has more than 900 days on DI over the last three years preceding unemployment entry. Appendix A.6 shows that the findings we present below are robust to removing these extreme values.

[19] Fig. A1 displays more information on the distribution of the time since unemployment entry. It shows that this time span is 0–13 months (respectively 14–18, ≥19 months) for about 30% (respectively 60%, 10%) of the sample.

[20] We study the effects three years after the start of monitoring so that we have sufficient time to pick up effects and see whether they last over a medium-run horizon.

[21] UI is composed of both job-seeking and non-job-seeking unemployment (including job training programs, supported part-time employment, or non-active unemployment). In our benchmark analyses, we consider individuals as "always on UI" if they remain in any of these statuses because the aim of JSM programs is to reduce UI spending by increasing non-subsidized employment. In Appendix A.8 we distinguish between these two outcomes and show that JSM reduces job-seeking UI but does not affect non-job-seeking UI.

[22] The outcomes are not necessarily mutually exclusive. An individual who works at least one day can also be on DI at least one day over the three years following the dispatch of the notification letter. Similarly, an individual can be registered as both having ever been employed and having remained on UI if they participated in, e.g., a supported (part-time) employment program while still receiving UI benefits. We show in Appendix A.8 that the effects on UI are not influenced by these "working" jobseekers.

**Table 1**
Summary statistics - sample.

| | | (1) | (2) | (3) | (4) |
|---|---|---|---|---|---|
| | | Overall | Treatment | Control | RDD - LATE |
| Gender | Man | 0.580 | 0.590 | 0.570 | −0.010 |
| | | (0.490) | (0.490) | (0.500) | (0.060) |
| Type of household | Single-headed | 0.400 | 0.410 | 0.380 | −0.000 |
| | | (0.490) | (0.490) | (0.490) | (0.061) |
| | Multi-headed | 0.520 | 0.510 | 0.540 | 0.009 |
| | | (0.500) | (0.500) | (0.500) | (0.061) |
| | Other | 0.080 | 0.080 | 0.070 | −0.008 |
| | | (0.270) | (0.270) | (0.260) | (0.033) |
| | Household with children | 0.490 | 0.530 | 0.450 | −0.009 |
| | | (0.500) | (0.500) | (0.500) | (0.061) |
| Nationality | Belgian | 0.830 | 0.820 | 0.850 | 0.046 |
| | | (0.370) | (0.390) | (0.360) | (0.046) |
| Region | Brussels | 0.150 | 0.170 | 0.130 | 0.001 |
| | | (0.360) | (0.370) | (0.340) | (0.044) |
| | Flanders | 0.490 | 0.470 | 0.520 | 0.015 |
| | | (0.500) | (0.500) | (0.500) | (0.061) |
| | Wallonia | 0.360 | 0.370 | 0.350 | −0.005 |
| | | (0.480) | (0.480) | (0.480) | (0.059) |
| Level of education | Below tertiary | 0.770 | 0.750 | 0.780 | 0.030 |
| | | (0.420) | (0.430) | (0.410) | (0.051) |
| | Tertiary | 0.140 | 0.140 | 0.130 | −0.048 |
| | | (0.350) | (0.350) | (0.340) | (0.041) |
| | Other | 0.090 | 0.100 | 0.080 | 0.019 |
| | | (0.290) | (0.300) | (0.280) | (0.036) |
| Previous labor market position | Blue-collar | 0.580 | 0.590 | 0.570 | 0.056 |
| | | (0.490) | (0.490) | (0.490) | (0.060) |
| | White-collar | 0.330 | 0.320 | 0.330 | −0.011 |
| | | (0.470) | (0.470) | (0.470) | (0.057) |
| | Other | 0.090 | 0.090 | 0.100 | −0.044 |
| | | (0.290) | (0.280) | (0.300) | (0.037) |
| | Daily wage | 117 | 115 | 119 | 5.4 |
| | | (81) | (81) | (81) | (9.3) |
| | Number of DI days | 103.920 | 103.080 | 104.810 | −9.900 |
| | | (209.860) | (208.870) | (210.910) | (26.699) |
| | Number of worked days | 469.990 | 464.970 | 475.290 | 13.340 |
| | | (262.490) | (258.940) | (266.100) | (32.608) |
| | Length of UI (months) | 13.150 | 12.830 | 13.490 | −0.552 |
| | | (6.360) | (6.430) | (6.270) | (0.829) |
| Number of observations | | 42,208 | 21,688 | 20,520 | |

Notes: This table summarizes key information about our sample. Column (1) shows the average of each variable for the whole sample. Column (2) and (3) display the average of each variable for the treatment group (i.e., individuals aged [44–49[) and the control group (individuals aged [49;54[) respectively. Column (4) displays the results of estimating the discontinuity in each variable at the age of 49. Variable "Age" is the age of the individual at the predicted moment of dispatch of the notification letter (i.e., unemployment duration = 15 months). Past worked days and past days on DI refer to cumulated days during the three years preceding unemployment. The dataset contains all unemployment spells that reached an unemployment duration of 15 months between July 2007 and December 2011. We only keep individuals who received the letter for the first time to avoid potential dynamic selection bias. Robust standard errors are displayed in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Then, in Section 5.3, we dig deeper into the effect of JSM on DI by focusing on three types of outcomes. First, we distinguish between being observed in ST versus LT DI between $k = 0$ and $k = 12$. Second, we construct dummies for being on DI due to different types of health impairments (psychological disorder, musculoskeletal disorder, and other health impairments).[23] We also consider a dummy equal to one if a disability spell has been put to an end after a medical assessment at the NIHDI.[24]

Finally, in Section 5.4, we estimate equation (1) for cumulative gross income by income source (i.e., UI, employment, DI, or other SWP) as well as total gross transfers (i.e., the sum of transfers from UI, DI, and other SWP) and total net transfers (i.e., total gross transfers minus individual taxes).[25] This exercise allows us to study the extent to which the fiscal savings from reducing participation in UI are counterbalanced by increases in other social safety net spending.

Our benchmark specification uses a bandwidth of two years on each side of the cutoff, a linear polynomial and a triangular kernel. While the choice of a triangular kernel and a linear polynomial is standard in the RD literature, our bandwidth choice deviates from the data-driven approach proposed by Calonico et al. (2014) for two reasons. First, using a common bandwidth increases the comparability of results across all exit options because we include the same observations across all specifications. Second, our fuzzy RD design increases the volatility of the observations in a close neighborhood of the cutoff. Because the algorithm developed by Calonico et al. (2014) typically selects a small bandwidth (here ± 1.2 years on average across our four benchmark outcomes), the associated standard errors of the estimated discontinuities are

[23] In this exercise, we restrict our analysis to LT DI spells because we cannot observe the category of health impairment for ST spells (i.e. DI spells lasting less than one year).

[24] One limitation of this exercise is that this variable is provided only for disability spells that last more than one year. In other words, we can only assess whether a LT disability is refused or put to an end, conditional on the applicant being in the end of the first year of ST disability or already on LT disability. There are no rules that fix the frequency of these medical assessments.

[25] Labor income and transfers are net of social security contributions. We compute individual taxes by applying the legal tax rates on the different sources of income and transfers.

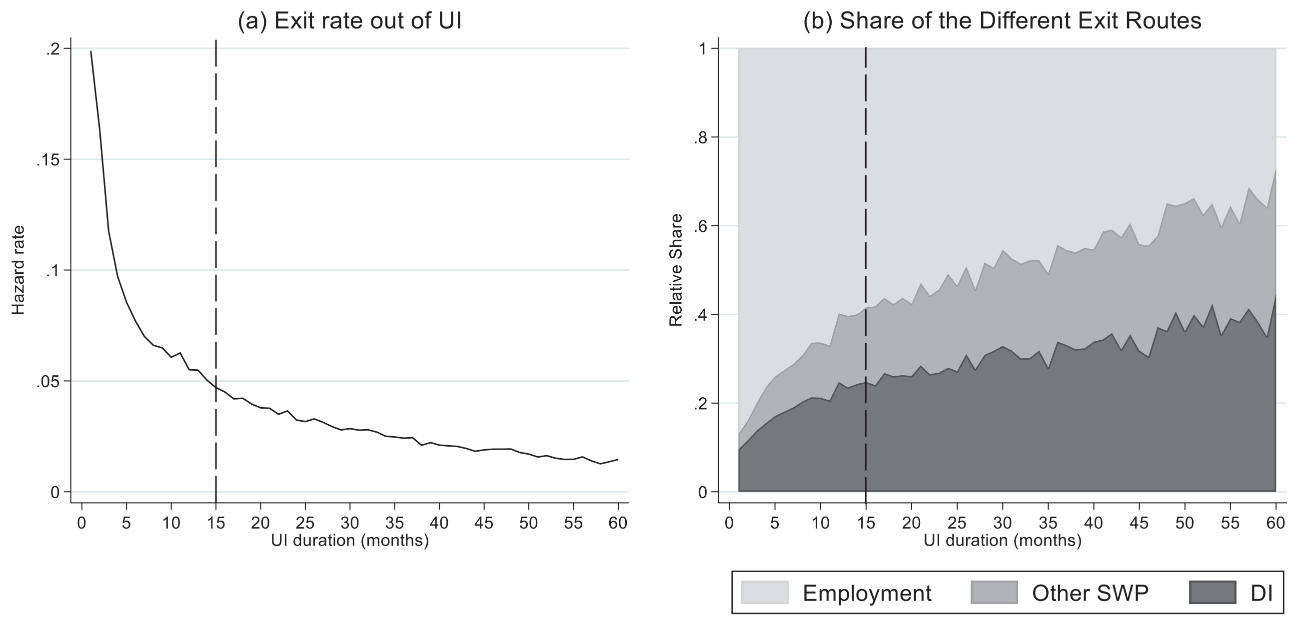


**Fig. 2.** Exit rates over the unemployment spell. Notes: Panel (a) of this Figure displays the hazard rate out of UI at different unemployment durations. Panel (b) shows, for different UI durations, the proportion of exits from UI that are towards employment, other SWP and DI. In these figures, we consider all UI spells that start between 2007 and 2011 for individuals aged 42.5–52.5 at UI entry.

very large even though the coefficients do not substantially change when the bandwidth size increases.[26]

Our RD design is fuzzy because the age at 15 months of unemployment does not perfectly predict who is included in the JSM program (i.e., who receives the notification letter). As stated in Section 3.1, our dataset lacks information on the unemployment duration that the UI agency used to dispatch the notification letters. Since we reconstructed this variable ourselves, there are some prediction errors relating to the time at which the JSM procedure starts (i.e., some individuals received the notification too early or too late, while others did not receive it at all). To account for the fuzzy nature of our RD, our preferred specification estimates equation (1) with 2SLS, where being older than 49 at the 15th month of unemployment is used as an instrument for receiving the notification letter. These coefficients should therefore be interpreted as a local average treatment effect (LATE) for those located around the age cutoff of 49.

### *4.2. Identification assumptions*

A first fundamental assumption of RD designs is the smoothness assumption, which requires that the conditional means of the potential outcomes are smooth near the cutoff.[27] The assumption implies there are no discontinuities in other variables (e.g., pre-existing policies or individual characteristics) besides the treatment assignment, which could affect the outcome. We test the validity of this assumption by two means. First, we estimate equation (1) with covariates as outcomes and show in column (5) of Table 1 that there are no pre-existing discontinuities in the covariates around the cutoff. Second, we run two placebo tests, which are shown in Appendix A.6. The first uses pre-reform observations to show there are no discontinuities in any outcome at the cutoff before the JSM program is implemented. The second shows that discontinuities in outcomes at age 49 for UI only appear near the duration of 15 months.

A second fundamental assumption in RD designs is the no-sorting assumption, i.e., that individuals cannot manipulate the assignment to treatment. This assumption would be violated if, for example, individuals could perfectly foresee the moment at which they would receive a notification letter and try to postpone it (through, e.g., temporary jobs, job trainings, or sickness periods) to reach age 49 by that time.[28] We test the validity of this assumption by looking at our sample's age distribution around the cutoff and running a McCrary density test. Fig. 3 provides a visual overview of the age distribution and shows no discontinuity in the density function at the cutoff, which is confirmed by a McCrary test (p-value = 0.797).

## 5. Findings

### *5.1. Benchmark results*

Table 2 shows the estimated discontinuity of experiencing different steps of the JSM procedure. It shows that 77% of individuals on the left of the cutoff receive a notification letter, i.e., are subject to JSM.[29] The discontinuity at age 49 in the probability of receiving a notification letter is equal to 50.8 pp, meaning that the intention-to-

[26] In Appendix A.6, we show that our benchmark coefficients are very close to those obtained with the data-driven bandwidth proposed by Calonico et al. (2014) even if the statistical significance is reduced. We also show that the effects of JSM on the number of days in, and income and transfers from, each status remain stable and highly statistically significant even when the bandwidth is selected using the approach proposed by Calonico et al. (2014). Together, these robustness checks are reassuring, as they show that our benchmark findings are not driven by the choice of a two-year bandwidth.

[27] Put more formally, the smoothness assumption refers to the fact that $E(Y(0)|X=x)$ and $E(Y(1)|X=x)$ are continuous in x around x=c, where c is the cut-off age and Y(0) and Y(1) are the potential outcomes.

[28] This is very unlikely for two reasons. First, the cutoff age of 49 was an internal rule that was not written in the official guidelines of the JSM program. UI beneficiaries and labor unions were indeed told that the JSM program would apply to any eligible UI beneficiary under age 50. Second, the UI agency computes the unemployment duration in a specific way, and individuals do not have access to this information, which further complicates the possibility of manipulating treatment.

[29] Appendix A.3 provides a visual representation of the discontinuity, around the age cutoff of 49, in the probability of receiving a notification letter. It also shows the discontinuity at age 49 in the probability of receiving the notification early (UI duration <15 months), at the right moment (UI duration = 15 months) or later than predicted (UI duration greater than 15 months).


**Fig. 3.** Sample age distribution. Notes: This figure shows the estimated density function of the age distribution in our sample. We can see that there is no discontinuity in the density function at age 49. This is confirmed by a McCrary density test, whose p-value (=0.797) is illustrated on the graph.

treat (ITT) estimates will be multiplied by a factor of approximately two. The table also shows that 35% of individuals on the left of the cutoff attend a first interview. The proportion of individuals in the treatment group who have a second and third interview decreases to 10% and 1%, respectively. In terms of sanctions, the table indicates that only 4% of individuals on the left of the cutoff receive a sanction at some point in the JSM procedure. Reassuringly, we find no discontinuity in the sanction rate for other motives, consistent with obligations outside JSM being similarly monitored for treated and control individuals.[30]

Table 3 and Fig. 4 present the estimated effects of JSM on the different labor market outcomes of interest. In the table, column (1) displays the mean predicted outcome on the right of the cutoff, i.e., at age 49. The ITT estimates presented in column (2) measure the discontinuities at the cutoff, whereas the LATE estimates in column (3) account for the fuzzy nature of our RD design. Fig. 4 provides a visual representation of the effects on the probability of being observed in different statuses.[31] In what follows, we use the LATE estimates to interpret the effects of JSM.

Panel (a) of Fig. 4 displays a discontinuity at age 49 in the probability of remaining unemployed over the whole post-treatment period. Column (3) of Table 3 shows that the estimated discontinuity is equal to $-16.6$ pp (i.e., a $-26.1$% proportional effect)[32] and is highly statistically significant. Looking at the other panels of Fig. 4, we observe that this decrease in unemployment probability is driven by an increased probability of receiving DI benefits and not by an increase in the probability of ever being employed. Table 3 indeed shows that JSM increases the probability of ever being on DI by 12.6 pp (+41.0%), while there is a small and statistically insignificant effect on the likelihood of ever being employed. The effect on other SWP is also small and statistically insignificant.

The second part of Table 3 shows the corresponding effect of JSM on the cumulative number of days spent in each status over the three years following the start of JSM. We estimate that JSM significantly decreases the number of days spent on UI by 88 days (-12.8%) and increases the number of days on DI by 60 days (+109.2%). In contrast, JSM does not significantly affect the number of days spent in employment or on other SWP.

In Appendix A.8, we complement these findings by looking at several additional types of outcomes. First, we assess whether the DI effect is driven by an increase in direct transitions from UI to DI or by an increase in indirect transitions through another status such as employment.[33] We find that the DI effect is driven by direct transitions from UI to DI rather than through more complex transition pathways. We also find that JSM reduces job-seeking UI but does not affect non-job-seeking UI (which includes participation in job trainings, part-time supported employment, and job search exemptions for various motives). Third, we show that JSM has no effect on the probability of being salaried or self-employed.

Together, these results suggest that JSM has not been effective at increasing the share of LT UI beneficiaries who found a job over the considered period. Instead, it has unintentionally increased DI take-up among those who would have remained unemployed in the absence of the program. The small and insignificant effect on employment participation is consistent with the mixed results from the JSM literature. The differences in the estimated effects across studies could be explained by the characteristics of monitored individuals, the particular design of the JSM programs, or local labor market tightness.

**Heterogeneity Analysis.** Appendix A.5 shows the heterogeneous effects of JSM across different subgroups of the LT unemployed population. Although this analysis lacks power to precisely distinguish differences in treatment effects between groups, it seems to support the hypothesis that LT unemployed individuals who are most at the margin of transiting to DI as a result of JSM have fewer labor market prospects (proxied by education) and are in worse health (proxied by the number of past DI days), while those most likely to transition to employment as a result of JSM are those with higher past wages.

**Robustness Checks.** To ensure that our findings are not influenced by model misspecification, we perform three robustness tests (see Appendix A.6). First, we check that our findings are robust to the choice of different bandwidth sizes. Second, we test the robustness of our estimates to the use of various polynomial orders and kernel forms. Third, we check that our estimates remain stable when we remove observations around the cutoff in a donut hole test. Our findings remain stable throughout these tests.

We also perform two placebo checks (which are also shown in Appendix A.6.) to ensure that our findings are not driven by factors other than JSM. We first estimate equation (1) using pre-reform data to rule out concerns about potential pre-existing discontinuities unrelated to the implementation of JSM. We then verify that our findings are not driven by discontinuities at age 49 that exist for all jobseekers during our observation window, irrespective of JSM, by exploring discontinuities at unemployment durations between 1 and 30 months. Both of these placebo tests support our conclusion that the discontinuities presented in our benchmark results are due to the implementation of JSM.

### 5.2. Dynamic analysis

We can better understand the determinants that drive the effects of JSM by looking at their evolution over time. To do so, we estimate equation (1) with dummies equal to one if an individual is observed on a given status in each quarter $t + k$ ($k = 0, 1,\ldots,12$) up to three years after JSM starts, where $t$ is the notification date and $k$ is measured in quarters. This analysis sheds light on the main

[30] Sanctions outside JSM cover several motives, e.g., refusing a suitable job offer, being absent at or abandoning a job training, or providing inexact information to the employment agency.

[31] Appendix A.4 also shows a visual representation of the effects of JSM on the cumulative number of days spent in each status.

[32] Given our fuzzy RD design, proportional effects are obtained by $\frac{b_{\{fuzzyRD\}}}{\bar{Y}^{+}-b_{\{fuzzyRD\}}P(T)^{+}}$, where $b_{\{fuzzyRD\}}$ is the LATE coefficient, $\bar{Y}^{+}$ is the mean outcome for individuals aged $49^{+}$, and $P(T)^{+}$ is the probability of being treated for individuals aged $49^{+}$.

[33] To distinguish between these two explanations, we estimate equation (1), taking instead an outcome equal to one if the transition into DI is direct, and another outcome equal to one if the transition into DI is indirect.

**Table 2**
JSM procedure.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Overall | Treatment | Control | RDD - LATE |
| Notification Letter | 0.420 | 0.810 | 0.010 | −0.508 *** |
| | (0.490) | (0.390) | (0.120) | (0.023) |
| First interview | 0.180 | 0.350 | 0.010 | 0.466 *** |
| | (0.390) | (0.480) | (0.080) | (0.033) |
| Second interview | 0.050 | 0.090 | 0.000 | 0.134 *** |
| | (0.220) | (0.290) | (0.050) | (0.025) |
| Third interview | 0.010 | 0.010 | 0.000 | 0.018** |
| | (0.080) | (0.110) | (0.010) | (0.009) |
| JSM sanction | 0.020 | 0.040 | 0.000 | 0.048 *** |
| | (0.150) | (0.210) | (0.030) | (0.015) |
| Other sanction | 0.060 | 0.060 | 0.050 | −0.016 |
| | (0.240) | (0.240) | (0.230) | (0.028) |
| Number of observations | 42,208 | 21,688 | 20,520 | |

Notes: This table shows descriptive statistics about the JSM procedure. Column (1) shows the average of each variable for the whole sample. Column (2) and (3) display the average of each variable for the treatment group (i.e., individuals aged [44–49[) and the control group (individuals aged [49;54[) respectively. Column (4) displays the results of estimating the discontinuity in each variable at the age of 49. The dataset contains all unemployment spells that reached an unemployment duration of 15 months between July 2007 and December 2011. We only keep individuals who received the letter for the first time to avoid potential dynamic selection bias. Robust standard errors are displayed in parentheses. *p<0.1; **p<0.05; ***p<0.01.

**Table 3**
Benchmark results.

| | (1) | (2) | (3) |
|---|---|---|---|
| **Outcome** | Predicted outcome at age = 49+ | ITT | LATE |
| Always on UI | 0.601 | −0.082 *** | −0.157 *** |
| | | (0.018) | (0.059) |
| Ever on Employment | 0.560 | 0.024 | 0.037 |
| | | (0.019) | (0.057) |
| Ever on DI | 0.320 | 0.063 *** | 0.112** |
| | | (0.018) | (0.055) |
| Ever on other SWP | 0.142 | 0.016 | 0.028 |
| | | (0.013) | (0.043) |
| UI days | 685.3 | −48.1 *** | −87.6** |
| | | (12.5) | (39.4) |
| Worked days | 184.2 | 11.3 | 15.4 |
| | | (10.0) | (31.2) |
| DI days | 55.9 | 31.4 *** | 60.4 *** |
| | | (6.7) | (20.4) |
| Other SWP days | 63.4 | 8.2 | 15.3 |
| | | (7.3) | (23.6) |

Notes: This table displays the cumulative effect of JSM on each outcome three years following the reception of the notification letter. The first part shows estimated effects along the extensive margin, i.e., the effect of JSM on the probability of being observed at least one day in each status. The second part shows estimated effects on the number of days spent in each status. Column (1) displays the predicted outcomes on the right (age = 49 + ) of the cutoff. Column (2) displays the ITT coefficients and Column (3) displays the LATE coefficients. Control variables included in the regressions are: year of unemployment entry; gender; household category; a dummy for Belgian nationality; region; education; contract type of the last job; last daily wage; length of unemployment; and cumulative worked days and DI days during the three years preceding unemployment entry. The dataset contains all unemployment spells that reached an unemployment duration of 15 months between July 2007 and December 2011 for individuals who have not been previously monitored. Robust standard errors are displayed in parentheses. *p<0.1; **p<0.05; ***p<0.01.

drivers of transitions out of UI, i.e., whether these transitions occur right after receiving the notification letters, or later in the UI spell, i.e., after the first, second, or third interview.[34]

Fig. 5 visually displays the results of this dynamic analysis.[35] Panel (a) shows that the effect of JSM on the UI probability decreases between the second and fifth quarter after the notification date and then remains quite stable as of the sixth post-treatment quarter. The effect on UI exits thus materialize after the first monitoring interview (before any sanction can be imposed) but sharpen after the second and third interviews (when sanctions are taken after a negative evaluation). In panel (b), the point estimates remain small and statistically insignificant during the entire post-treatment period, suggesting that JSM did not significantly affect the rate of return to employment at any step of the JSM procedure.

Panel (c) shows that the effects on DI are already positive (but statistically insignificant) immediately after notification and until the third post-notification quarter.[36] The LATE coefficients become statistically significant at 5% from the fourth post-treatment quarter, i.e., between the moment of the second and third interview. The effect then remains stable over time and is equal to 10.0 pp three years after the notification date. We acknowledge that our statistical power does not allow us to clearly disentangle whether the DI entries are driven by the sole reception of the notification letter or by subsequent interviews or sanctions.

To complement this result, Appendix A.7 displays the LATE coefficients when the outcome is a dummy equal to one if the individual enters DI before ever having a first (second or third,

[34] The timing of interviews presented in Fig. 5 is based on the official Belgian JSM procedure shown in Fig. 1 and is thus theoretical.

[35] Appendix A.7 shows the point estimates at each post-treatment quarter.

[36] Appendix A.7 shows that the ITT estimates are already significant at 5% right after the reception of the notification letter, but the standard errors increase significantly in the LATE specification.

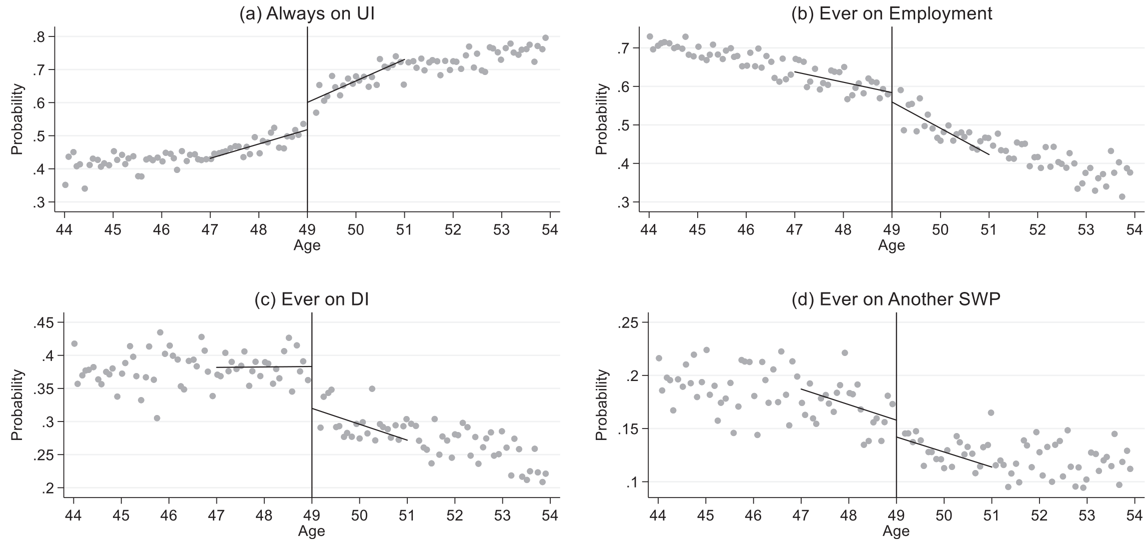


**Fig. 4.** Benchmark findings. Notes: This figure provides a visual representation of the discontinuity in the different labor market outcomes of interest at age 49. Panel (a) plots the proportion of individuals who have been always unemployed within a horizon of three years following the reception of the notification letter. Panel (b) plots the proportion of individuals who have worked at least one day within a horizon of three years following the reception of the notification letter. Panel (c) plots the proportion of individuals who experienced at least one day on DI within a horizon of three years following the reception of the notification letter. Panel (d) plots the proportion of individuals who have participated at least one day in other SWP (which include early retirement, social integration benefits, professional illnesses, workplace accidents, and assistance to handicapped individuals) within a horizon of three years following the reception of the notification letter.

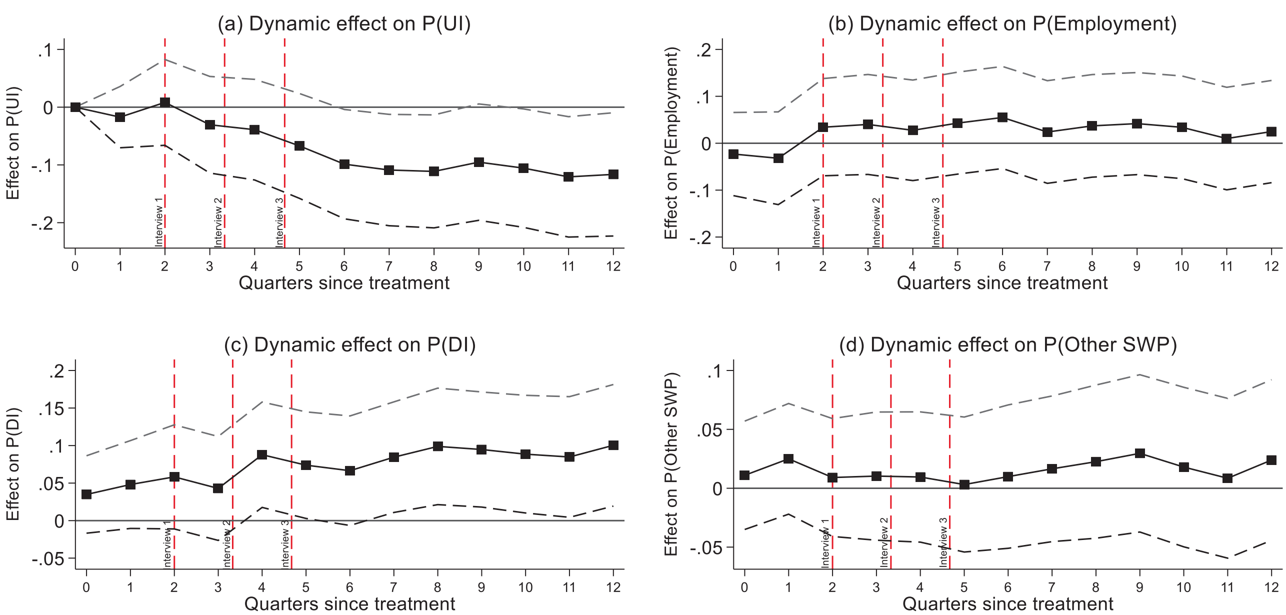


**Fig. 5.** Dynamic effect. Notes: This figure displays the dynamic effects of JSM on the probability of being on UI (Panel a), being employed (Panel b), being on DI (Panel c), and being on another SWP (Panel d). For all figures, the vertical axis represents the size of the estimated LATE. The horizontal axis indicates the number of quarters that follow the predicted month of reception of the notification letter. The dashed vertical red lines indicate the planned (theoretical) timing of interviews, based on the official Belgian JSM procedure. Black squares display the estimated coefficients with 95% confidence intervals. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

respectively) interview.[37] The LATE is equal to 8.4 pp before the second interview, suggesting that most exits to DI due to JSM occur before any sanction is enforced. This is in line with existing research showing that the mere threat of receiving a sanction is sufficient to affect the behavior of jobseekers because the threat increases the risk of a sanction in the future, which decreases the present value of unemployment and generates an outflow from UI (Black et al., 2003; Boone et al., 2009; Arni et al., 2013). Our findings show that for LT unemployed individuals, this outflow from UI occurs through an increase in DI rather than through employment.

### *5.3. Investigating the effects of JSM on DI*

From a theoretical perspective, it is unclear exactly which mechanisms would drive the DI effects of JSM (e.g., Low and Pistaferri, 2015; Haller et al., 2020). In this respect, one question

[37] Note that this analysis will tend to underestimate the effect of JSM on the probability of DI conditionally on having no interview because the control group does not have any interview by construction.

is whether the increased DI take-up is due to higher rates of "false acceptances" or to existing health impairments among the LT unemployed population.[38] In an attempt to shed light on this question, we first examine the effects of JSM on ST and LT participation in DI.

First, Table 4 shows that the effect of JSM is significant for both programs. This distinction is informative on the severity of health impairments and, consequently, sheds light on the prevalence of moral hazard among disability cases. If the DI effect is only short lived, one would expect a higher prevalence of moral hazard among disability inflows, e.g., due to individuals reporting a health impairment to avoid reporting job search activities or to postpone the moment of the interviews (as in Van den Berg et al., 2019). In contrast, observing a long-run effect on DI would indicate that those who switch from UI to DI have more severe health impairments, pointing to the existence of pre-existing health impairments or "hidden disability" among the LT unemployed population. This is especially relevant given that an additional medical screening occurs when individuals transition from ST DI to LT DI. Our findings that JSM increases both ST and LT DI thus support the hypothesis of existing health impairments among LT unemployed individuals.

We next examine how the probability of being excluded from DI (during a medical assessment by the NIHDI), conditional on receiving DI benefits,[39] varies around the JSM age cutoff. To do so, we exploit a variable that provides information on the decision made by the NIHDI-appointed doctor, following their medical assessment with the claimant, to evaluate the remaining ability to work. If more healthy individuals enter DI because of the JSM program, we should expect those on the left of the cutoff to have a higher exclusion rate from DI than those on the right. Table 4 provides the result of this analysis. The coefficient is negative and statistically insignificant, suggesting that individuals who enter DI as a result of JSM are not more likely to be excluded from DI. This supports the hypothesis that JSM triggers DI entries for individuals who are already in poor health. However, we acknowledge that the coefficient is imprecisely estimated, and therefore this result should be considered as indicative rather than conclusive.

Finally, we differentiate the effects of JSM on DI by type of health impairment. This allows us to examine whether the transition from UI to DI occurs for illnesses that are more likely to create a gray area between the two. In particular, we distinguish between mental conditions, musculoskeletal disorders, and other conditions. We do this because mental conditions and musculoskeletal disorders are often considered challenging to evaluate objectively,[40] and determining the point at which these illnesses makes an individual sufficiently disabled for DI entry may be less clear-cut compared to other conditions. Therefore, it might be unclear for individuals experiencing some of these conditions (and their doctors) whether they should be on UI or DI. Table 4 provides the results of this analysis. It shows that the bulk of the effect of JSM on LT DI is driven by a higher prevalence of mental conditions (+3.8 pp) or musculoskeletal disorders (+2.9 pp), while the effect on other conditions is smaller in magnitude and statistically insignificant. These findings indicate that individuals who are induced to transition from UI to DI

**Table 4**
DI effects.

| | (1) | (2) | (3) |
|---|---|---|---|
| **Outcome** | Predicted outcome at age = 49+ | ITT | LATE |
| Short-term DI | 0.310 | 0.058 *** | 0.100* |
| | | (0.018) | (0.055) |
| Long-term DI | 0.077 | 0.039 *** | 0.076** |
| | | (0.011) | (0.034) |
| P(Exclusion\|DI) | 0.202 | −0.069 | −0.134 |
| | | (0.045) | (0.145) |
| Psychological Disorders | 0.014 | 0.019 *** | 0.038** |
| | | (0.006) | (0.018) |
| Musculoskeletal Disorders | 0.025 | 0.015** | 0.029 |
| | | (0.007) | (0.021) |
| Other Impairments | 0.041 | 0.009 | 0.019 |
| | | (0.008) | (0.024) |

Notes: This table explores the effects of JSM on participation in DI. The first part shows the effect of JSM on participation in short-term (<1 year) and long-term (greater than 1 year) DI, over the three-year horizon following the reception of the notification letter. The second part displays the estimated discontinuity in the probability of being excluded from DI, conditional on ever being on DI, over the three-year horizon following the reception of the notification letter. The third part displays the effect of JSM separately by type of health impairment (psychological disorders, musculoskeletal disorders, and other impairments). The specification of bandwidth size, polynomial order, and kernel is the same as for the benchmark estimations. Robust standard errors are displayed in parentheses. *p<0.1; **p<0.05; ***p<0.01.

as a result of JSM are those experiencing certain health conditions where the distinction between UI and DI is less clear-cut.

### 5.4. Fiscal implications

Since our findings raise the question of the net effect of JSM on government transfers, we investigate the discontinuity in income and transfers at age 49. Table 5 presents the mean predicted outcome at age 49 in column (1), the ITT estimate in column (2), and the LATE estimate in column (3) for six outcomes: UI transfers, labor income, DI transfers, other government transfers, total transfers, and net transfers (i.e., total transfers, net of taxes). These findings are illustrated in Fig. 6.

Table 5 shows that JSM decreases average UI transfers by 3,756 euros and increases DI transfers by 2,843 euros over the three years following the start of monitoring. In contrast, and in line with our benchmark findings, we find no statistically significant increase in labor income or other transfers. Overall, panels (e) and (f) of Fig. 6 show that JSM has no effect on the amount of (net) government transfers. The decreased UI transfers are almost entirely offset by an increase in DI transfers. This is confirmed by Table 5, which shows that, ultimately, JSM decreases (net) transfers to treated individuals by a little more than 700 euros over three years (although the coefficient is statistically insignificant). Given the expenditure necessary to implement JSM programs (namely the cost of counselors and administrative monitoring), we conclude that the program is unlikely to have been effective in reducing overall government expenditures.

Another question related to this fiscal analysis is whether the effect of JSM on UI and DI transfers has been sufficiently important to have a sizable effect on the total amount of unemployment and disability transfers. In this regard, we provide a back-of-the-envelope computation by multiplying the LATE coefficient associated with UI and DI transfers by the number of individuals who effectively received a notification letter over the period 2007–2011 (see Appendix A.9). We then compare the obtained values to the total amount of UI and DI transfers over the same period. Given the local nature of our RD design, this exercise requires the assumption that the effect of JSM is homogeneous across age

[38] A third explanation might be that JSM programs increase health problems related to the stress caused by the risk of a negative income shock occurring with a sanction. However, this effect would likely constitute an aggravating factor rather than the whole explanation for the observed DI effect.

[39] For this exercise, we must condition our sample on DI receipt. Indeed, because JSM increases participation in DI, there will be more individuals who receive DI on the left of the cutoff than on the right. Therefore, even if exclusion rates are identical among both groups, there will be more exclusions on the left. By conditioning on DI receipt, we can compare whether exclusion rates vary around the cutoff.

[40] See, e.g., Autor and Duggan (2006).

**Table 5**
Benchmark results – income.

| | (1) | (2) | (3) |
|---|---|---|---|
| **Outcome** | Predicted outcome at age = 49+ | ITT | LATE |
| UI transfers | 28,011 | −2,059 *** | −3,756** |
| | | (514) | (1,564) |
| Labor earnings | 16,310 | 561 | 616 |
| | | (972) | (3,003) |
| DI transfers | 2,840 | 1,480 *** | 2,843 *** |
| | | (334) | (1,020) |
| Other transfers | 510 | 171 | 302 |
| | | (160) | (491) |
| Total transfers | 31,361 | −408 | −612 |
| | | (520) | (1,528) |
| Net transfers | 20,931 | −506 | −714 |
| | | (670) | (2,037) |

Notes: This table displays the cumulative effect of JSM on income and transfers (in 2013 euros) by source, three years following the reception of the notification letter. Total gross transfers are defined as the sum of taxable transfers from UI, DI and other SWP (net of social contribution). Total net transfers are the sum of taxable transfers from UI, DI and other SWP, minus personal income taxes. Taxes are computed by the authors according to official taxation rules in Belgium. Column (1) displays the predicted outcomes on the right (age = 49 + ) of the cutoff. Column (2) displays the ITT coefficients and Column (3) displays the LATE coefficients. Control variables included in the regressions are: year of unemployment entry; gender; household category; a dummy for Belgian nationality; region; education; contract type of the last job; last daily wage; length of unemployment; and cumulative worked days and DI days during the three years preceding unemployment entry. The dataset contains all unemployment spells that reached an unemployment duration of 15 months between July 2007 and December 2011 for individuals who have not been previously monitored. Robust standard errors are displayed in

groups and all other characteristics correlated with age. We therefore decide to narrow our analysis to individuals aged 40–49. We also make the conservative assumption that the fiscal effects do not last more than three years. Multiplying the number of individuals who received a notification letter by the LATE coefficients, we estimate that UI savings amount to 392.32 million euros, while DI costs amount to 296.95 million euros. Dividing these amounts by total UI transfers (7.41 billion euros) and DI transfers (6.09 billion euros) over the period 2007–2011 for individuals aged 40–49, we find that the decrease (increase) in UI (DI) transfers due to JSM represents 5.29% (4.78%) of total UI (DI) transfers.

## 6. Conclusion and discussion

In this study, we provide novel insights on the employment and social safety net substitution effects of JSM targeted at LT UI beneficiaries. We analyze a JSM program that was set up in 2004 in Belgium and imposed job search requirements for individuals who had been unemployed for more than 15 months. We credibly identify the causal effect of the policy using an RD design, exploiting the fact that over the period of analysis, only individuals who were under 49 years of age when they reached 15 months of unemployment were included in the program. Our empirical analysis uses a rich set of administrative data, allowing us to follow their participation in employment and social safety net programs up to three years after the JSM's first step (i.e., the reception of the notification letter).

Our key findings can be summarized as follows. First, we find a positive and statistically significant effect of JSM on UI exits, entirely driven by exits toward DI rather than by employment or other SWP. Second, we find evidence that the effect of JSM on UI exits to DI begins to materialize before any sanction is enforced following non-compliance with the job search requirements. Third, we show that DI effects persist up to three years after the reception of the notification letter and are driven by psychological and musculoskeletal illnesses. LT DI recipients exhibit similar exclusion rates from DI regardless of their treatment status, supporting the hypothesis that JSM triggers DI entries for individuals who are


**Fig. 6.** Effect of JSM on income and transfers. Notes: This figure provides a visual representation of the discontinuity at age 49 in the amount of income and transfers received. Total gross transfers are defined as the sum of taxable transfers from UI, DI and other SWP (net of social contribution). Total net transfers are the sum of taxable transfers from UI, DI and other SWP, minus personal income taxes. Taxes are computed by the authors according to official taxation rules in Belgium. For each panel, the vertical axis measures the effect of JSM on cumulative income and/or transfers within a horizon of three years following the predicted month of dispatch of the notification letter. The horizontal axis measures the age at the predicted month of dispatch of the notification letter. Each point corresponds to a monthly average.

already in poor health. Finally, we find that the decrease in cumulative UI transfers per individual caused by the JSM program is largely offset by an increase in cumulated DI transfers. This finding suggests that the fiscal effect of the program is very close to zero and potentially negative if we were to account for implementation and operating costs.

One question that remains is why JSM increases transitions to DI. On the one hand, it reduces the value of UI with respect to DI, e.g., due to increased search efforts and the risk of being sanctioned. On the other hand, it could function as an "information shock" about the appropriate program in which the individual belongs, e.g., due to more frequent contacts with caseworkers. The extent to which the information shock drives the effects of JSM on DI is important from a policy perspective because if JSM reallocates individuals into the appropriate program, then DI transitions are not necessarily an undesirable side effect of the policy. Our findings that (i) transitions to DI are highest among those who had already experienced DI spells in the past, (ii) DI effects persist in the long run, and (iii) DI exclusion rates are similar for treated and non-treated individuals all support the hypothesis of the information shock. However, because we do not observe health directly, we cannot provide a definitive answer on the key driving mechanism. We thus leave this question as an avenue for future research.

Irrespective of the mechanisms driving the DI effects of JSM, our findings open the question of how best to assist these marginal DI applicants in the UI pool to reenter employment. In this respect, DI programs have long been criticized for their insufficient provision of activation policies, making DI an absorbing state. In recent years, most developed countries have started to adopt an approach to DI that is more oriented toward fostering reintegration of DI recipients into the labor market (Garcia-Mandico et al., 2022). However, there is little literature on the effectiveness of activation programs targeted at DI recipients. The evidence that does exist suggests that vocational rehabilitation, employer subsidies, and supported employment services (in some cases coupled with a medical component) could be effective in fostering the return of DI recipients into the labor force (e.g., Fontenay and Tojerow, 2022).

Finally, our findings also show that JSM targeted at LT UI beneficiaries does not foster a return to employment and is therefore not cost-effective. Two factors could explain this finding: the design of the Belgian JSM program and the population targeted by the program. First, regarding the program's design, Cockx et al. (2018), who use a structural job search model to analyze the same JSM program as ours but for younger LT unemployed, attribute the ineffectiveness of the Belgian JSM policy to three factors: (i) the imprecision of monitoring technology, (ii) interviews occurring at a late stage in the unemployment spell, and (iii) sanctions being taken only after a second negative interview. The authors show that improving the program along these lines could increase the speed at which LT unemployed jobseekers enter employment. Second, it might be that even well-designed JSM programs targeted at individuals with a weak labor market attachment (namely the LT unemployed) can improve job finding only marginally. In this respect, these policies are likely to be outperformed by programs involving a higher degree of human capital investment, as suggested by the *meta*-analysis of Card et al. (2018). Ultimately, because the LT unemployed typically face multiple barriers to employment, the most appropriate active labor market programs for these populations are likely to be a mix of policies that target several of these employment barriers at the same time.

## Data availability

The data that has been used is confidential.

## Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

## Appendix

### *A.1. Comparison of legal UI and DI benefits*

Comparing the generosity of each program is interesting for helping understand the incentive effects that can influence spillover effects between UI and DI.[41] Table A1 shows the replacement rates of UI and DI (with ceiling and floor amounts) for the year 2010. This table shows that while the replacement rates are similar across durations and household category, their floor and ceiling benefits differ. In fact, minimum (maximum) DI benefits exceed minimum (maximum) UI benefits by 221 (675) euros on average, which reflects the manner in which DI benefits are computed. During the first six months of disability, DI benefits are legally established at the same level as UI benefits. Then, from the sixth month of disability, the former UI beneficiary is subject to the same conditions as formerly employed DI beneficiaries, whose minimum and maximum DI benefits are much higher than UI benefits. From this perspective, the DI program is more generous than the UI program and, as a result, once a formerly unemployed individual enters LT DI, they face financial incentives to remain disabled, even after recovery.

[41] Note that a particular feature of the Belgian UI system is that unemployment payments are not limited over time.

**Table A1**
Legal Replacement Rates for UI and DI (Year 2010).

| | Unemployment Insurance | | | Disability Insurance | | |
|---|---|---|---|---|---|---|
| | Replacement rate | Minimum benefits | Maximum benefits | Replacement rate | Minimum benefits | Maximum benefits |
| **Heads of household** | | | | | | |
| 1–6 months | 60% | 1,008 | 1,324 | 60% | 1,008 | 1,324 |
| 7–12 months | 60% | 1,008 | 1,234 | 60% | 1,256 | 1,883 |
| >12 months | 60% | 1,008 | 1,154 | 65% | 1,281 | 2,057 |
| **Single** | | | | | | |
| 1–6 months | 60% | 847 | 1,324 | 60% | 634 | 1,324 |
| 7–12 months | 60% | 847 | 1,234 | 60% | 1,005 | 1,883 |
| >12 months | 54% | 847 | 1,034 | 55% | 1,025 | 1,741 |
| **Cohabitant** | | | | | | |
| 1–6 months | 60% | 634 | 1,324 | 60% | 634 | 1,324 |
| 7–12 months | 60% | 634 | 1,234 | 60% | 862 | 1,883 |
| >12 months | 40% | 634 | 769 | 40% | 879 | 1,266 |

Notes: This table summarizes the rules of calculation for the amount of UI and DI benefits for the year 2010 (end of the year). For each insurance, replacement rates and the minimum and maximum benefits (in 2010 euros) are indicated. The computation of DI benefits for former UI beneficiaries is the following: during the first six months of disability, the replacement benefits are the same as the unemployment benefits. Between the seventh and the 12th month of disability, the replacement rate is the same as for unemployment, but the floor and ceiling are adapted depending on the household category. Finally, from the 12th month of disability onward, the replacement rate is adapted according to the household category and remains flat over time. Sources: ONEM, NIHDI, and our own computations.

## A.2. Sample structure

*Description of the data*

Basic individual information comes from the National Register, which collects information from all Belgian municipalities including on gender, nationality, birthdate, municipality of residence, and household category. Information on disability is provided by the National Institute for Health and Disability Insurance (NIHDI). The data contain information on start and end dates, budget, number of days, and a detailed code for the category of illness. Data on an individual's labor market activities originates from the National Social Security Office (NSSO), which collects quarterly information on any salaried job. It includes information on the sector of activity, share of full-time work, occupation, employer size, and earnings. We obtained information on unemployment benefits from the federal UI agency. The data contains precise information on any individual who is registered in a program managed by this institution. The information includes details on the individual's history of active and inactive unemployment, participation in activation programs and/or job trainings, extra holidays, and any career interruptions. For all of these programs, we have access to monthly information on the number of days and budget. The federal UI agency also provided us with detailed information on the JSM program, including the dates of the various steps of the monitoring program (notification letters and interviews), the outcomes of the interviews (positive, negative, or absent), and sanctions that have been enforced. To see whether an individual is registered in the national social assistance program, we rely on information provided by the LMDW, which indicates whether an individual has been registered within a public social action center at the end of a specific quarter. Finally, we obtained information on all sources of annual row earnings from the LMDW, which collects annual row earnings by social safety net institution.

*Definition of the outcomes*

*Benchmark outcomes:*

- **Always on UI:** Being observed at least one day on UI (all programs confounded) every quarter (k = 0,1,. . .,12) following the predicted date of reception of the notification letter.
- **Ever on employment:** having been in registered employment (salaried or self-employed) at least one day during the three years following the predicted date of reception of the notification letter.
- **Ever on DI:** The individual has received at least one payment from the NIHDI in the three years following the predicted date of reception of the notification letter.
- **Ever on other SWP:** The individual has been registered as receiving at least one payment from the Occupational Diseases Fund, the Work Accidents Fund, the Handicapped People Fund, the Pension Fund, or the Public Social Action Centre in the three years following the predicted moment of reception of the notification letter.

*DI outcomes:*

- **Ever on psychological disorders:** The individual has received at least one payment from the NIHDI in the category "LT disability–psychological disorders" in the three years following the predicted date of reception of the notification letter.
- **Ever on musculoskeletal disorders:** The individual has received at least one payment from the NIHDI in the category "LT disability––musculoskeletal disorders" in the three years following the predicted date of reception of the notification letter.
- **Ever on other impairments:** The individual has received at least one payment from the NIHDI in the category "LT disability––other impairments" in the three years following the predicted date of reception of the notification letter.
- **Ever excluded|DI:** LT disability has been either been refused or ended after a medical assessment by a doctor of the NIHDI.

*Fiscal outcomes:*

- **UI transfers:** The sum of taxable UI (net of social contributions) transfers in the three years following the predicted date of reception of the notification letter.
- **Labor earnings:** The sum of taxable labor earnings (net of social contributions) in the three years following the predicted date of reception of the notification letter. Since earnings are only available on an annual basis, we estimate quarterly earnings by multiplying the annual levels by the proportion of annual working days that are registered for each quarter of the year.

- **DI transfers:** The sum of taxable DI transfers (net of social contributions) received in the three years following the predicted moment of reception of the notification letter. Since transfers are only available on an annual basis, we estimate quarterly DI transfers by multiplying the annual levels by the proportion of annual DI days that are registered for each quarter of the year.
- **Transfers from other SWP:** The sum of other taxable transfers (net of social contributions) in the three years following the predicted moment of reception of the notification letter. Since transfers are only available on an annual basis, we estimate quarterly transfers by dividing annual transfers by four.
- **Total gross transfers:** UI transfers + DI + Transfers from other SWP. These amounts are all taxable transfers net of social contributions.
- **Total net transfers:** UI transfers + DI transfers + Transfers from other SWP––Personal Income Taxes. Personal income Taxes are computed by the authors according to official taxation rules in Belgium.

*Definition of active and non-active unemployment*

Among the data provided by the federal UI, one variable (FICHE7) identifies the type of transfers that each person receives. We use this variable to define jobseeking and non-jobseeking unemployment:

- **UI jobseekers** are fully compensated, unemployed jobseekers (FICHE7 codes: 1,2,4,5,47).
- **UI non-jobseekers can be** (1) exempted from active job search for age or family reasons (FICHE7 codes:7,8); (2) undergoing job training (FICHE7 codes: 9,10,11,13,14,15,26,27,28); (3) in part-time, supported employment (FICHE7 codes: 16,17,18,19,31,32,45,48,54,55,56); (4) in an old-age unemployment program called Conventional Early Retirement (FICHE7 codes: 36,37,38,39,97,98,99,360,370,361,371); or (5) in supported relief work (very few individuals, FICHE7 codes: 29,30).

*Sample selection*

We begin by selecting all unemployment spells (as a jobseeker or non-jobseeker) that began between January 2006 and December 2011 for individuals between 40 and 54 years of age at the month of entry (997,928 spells). From this sample, we retain individuals who reached an unemployment duration of 15 months between July 2007 and December 2011 and were categorized as jobseekers (67,916 individuals, i.e., 6.8% of the initial sample). Then, we condition on being aged between 44 and 54 at the 15th month of unemployment (46,173 individuals). Finally, we keep individuals who have not already been included in the JSM program in an earlier spell, i.e., for individuals who receive more than one notification letter, we keep the spell that includes the first notification letter. (42,208 individuals). Table A.2 summarizes the different steps of the selection of our final sample.

**Table A2**
Steps of the selection of our sample.

| Condition | Number of spells |
|---|---|
| Total number of entries into UI between 2006 m1-2011 m12 for individuals aged 40–54 at entry | 997,928 |
| UI Duration = 15 months between 2007 m7 and 2011 m12 & Jobseeker | 67,916 |
| Aged [44–54[ at the predicted moment of notification | 46,173 |
| Has not been monitored during an earlier spell | 42,208 |

Notes: This table summarizes each step of the selection of our sample.

**Table A3**
Robustness of findings to different trends and kernel choices.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| **Outcome** | Linear trend – Uniform kernel | Quadratic trend – Triangular kernel | Quadratic trend – Uniform kernel | Bias Correction – Triangular kernel | Bias Correction – Uniform kernel |
| Always on UI | −0.157 *** | −0.140 | −0.140 | −0.157 *** | −0.163 *** |
| | (0.059) | (0.094) | (0.111) | (0.059) | (0.049) |
| Ever on Employment | 0.037 | 0.017 | −0.012 | 0.037 | 0.044 |
| | (0.057) | (0.092) | (0.108) | (0.057) | (0.048) |
| Ever on DI | 0.112** | 0.109 | 0.104 | 0.112** | 0.113** |
| | (0.055) | (0.088) | (0.103) | (0.055) | (0.046) |
| Ever on other SWP | 0.028 | −0.017 | 0.009 | 0.028 | 0.050 |
| | (0.043) | (0.069) | (0.082) | (0.043) | (0.037) |

Notes: This table displays the robustness of our benchmark results to changes in the polynomial order and/or the form of the kernel used in the estimation of the discontinuity (Columns (1)-(3)), as well as the bias-corrected estimates proposed by Calonico et al. (2014) with uniform and triangular kernel (Columns (4)-(5)). The dataset contains all unemployment spells that reached an unemployment duration of 15 months between July 2007 and December 2011. Robust standard errors are displayed in parentheses. *p<0.1; **p<0.05; ***p<0.01.

**Table A4**
Robustness of effects to different bandwidth choices.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | 2 years Bandwidth (Benchmark) | 3 years Bandwidth | 4 years Bandwidth | Calonico et al. (2014) Bandwidth | Calonico et al. (2014) - Bias corrected |
| Always on UI | −0.156 *** | −0.167 *** | −0.185 *** | −0.134** | −0.129* |
| | (0.059) | (0.040) | (0.031) | (0.068) | (0.068) |
| Ever on Employment | 0.036 | 0.055 | 0.065** | 0.005 | −0.007 |
| | (0.057) | (0.039) | (0.030) | (0.058) | (0.058) |
| Ever on DI | 0.112** | 0.105 *** | 0.103 *** | 0.099 | 0.095 |
| | (0.055) | (0.038) | (0.029) | (0.062) | (0.062) |
| Ever on other SWP | 0.028 | 0.034 | 0.039* | 0.016 | 0.011 |
| | (0.043) | (0.030) | (0.023) | (0.045) | (0.045) |
| UI days | −87.602** | −74.273 *** | −78.099 *** | −90.184** | −95.748** |
| | (39.369) | (26.671) | (20.715) | (42.993) | (42.993) |
| Worked days | 15.445 | 7.275 | 7.314 | 21.064 | 24.923 |
| | (31.249) | (21.094) | (16.376) | (33.879) | (33.879) |
| DI days | 60.402 *** | 49.111 *** | 43.868 *** | 70.616 *** | 76.386 *** |
| | (20.372) | (14.167) | (11.068) | (20.984) | (20.984) |
| Other SWP days | 15.343 | 18.103 | 23.213* | 11.402 | 8.941 |
| | (23.646) | (16.080) | (12.477) | (25.852) | (25.852) |
| UI transfers | −3756** | −3660 *** | −4032 *** | −3144* | −3090 |
| | (1565) | (1067) | (830) | (1896) | (1896) |
| Labor earnings | 616 | 307 | 600 | 396 | 509 |
| | (3003) | (2013) | (1558) | (3189) | (3189) |
| DI transfers | 2843*** | 2307 *** | 2113 *** | 3481 *** | 3756 *** |
| | (1020) | (710) | (556) | (1019) | (1019) |
| Other transfers | 302 | 359 | 435 | 34 | −103 |
| | (491) | (352) | (277) | (523) | (523) |
| Total transfers | −612 | −994 | −1484* | 728 | 979 |
| | (1529) | (1056) | (826) | (1699) | (1699) |
| Net transfers | −714 | −728 | −1169 | 135 | 193 |
| | (2037) | (1383) | (1073) | (2253) | (2253) |

Note: This table displays the sensitivity of coefficients to changes in the bandwidth size, for our benchmark results, effects on the number of days in each status, and fiscal effects. We show estimated coefficients for a bandwidth of two years (our benchmark identification strategy), a bandwidth of three years, a bandwidth of four years, and the bandwidth size proposed by the data-driven approach of Calonico et al. (2014), including the bias corrected procedure. The table shows that our findings are overall very robust to changes in the bandwidth size, although our benchmark findings loose statistical significance when the bandwidth corresponds to the one proposed by Calonico et al. (2014). Robust standard errors are displayed in parentheses. *p<0.1; **p<0.05; ***p<0.01.

**Table A5**
Dynamic effect of JSM.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Quarter after notification | UI | | Employment | | DI | | Other SWP | |
| | ITT | LATE | ITT | LATE | ITT | LATE | ITT | LATE |
| 1st Quarter | 0.000 | 0.000 | −0.005 | −0.023 | 0.020** | 0.036 | 0.007 | 0.011 |
| | (0.000) | (0.000) | (0.015) | (0.045) | (0.008) | (0.026) | (0.007) | (0.023) |
| 2nd Quarter | −0.010 | −0.017 | −0.011 | −0.032 | 0.025** | 0.048 | 0.014** | 0.025 |
| | (0.009) | (0.027) | (0.016) | (0.050) | (0.010) | (0.030) | (0.007) | (0.024) |
| 3rd Quarter | 0.002 | 0.009 | 0.022 | 0.034 | 0.031 *** | 0.058* | 0.006 | 0.009 |
| | (0.012) | (0.038) | (0.017) | (0.053) | (0.011) | (0.035) | (0.008) | (0.026) |
| 4th Quarter | −0.017 | −0.030 | 0.025 | 0.040 | 0.023** | 0.043 | 0.006 | 0.010 |
| | (0.013) | (0.043) | (0.017) | (0.054) | (0.011) | (0.035) | (0.009) | (0.028) |
| 5th Quarter | −0.021 | −0.039 | 0.018 | 0.027 | 0.046 *** | 0.088** | 0.005 | 0.010 |
| | (0.014) | (0.044) | (0.018) | (0.055) | (0.012) | (0.036) | (0.009) | (0.028) |
| 6th Quarter | −0.036** | −0.067 | 0.026 | 0.043 | 0.040 *** | 0.074** | 0.002 | 0.003 |
| | (0.015) | (0.046) | (0.018) | (0.055) | (0.012) | (0.036) | (0.009) | (0.029) |
| 7th Quarter | −0.051 *** | −0.098** | 0.032* | 0.055 | 0.036 *** | 0.066* | 0.005 | 0.010 |
| | (0.015) | (0.048) | (0.018) | (0.056) | (0.012) | (0.037) | (0.010) | (0.031) |
| 8th Quarter | −0.056 *** | −0.109** | 0.016 | 0.024 | 0.045 *** | 0.084** | 0.008 | 0.016 |
| | (0.015) | (0.049) | (0.018) | (0.056) | (0.012) | (0.038) | (0.010) | (0.032) |
| 9th Quarter | −0.057 *** | −0.111** | 0.023 | 0.037 | 0.052 *** | 0.099** | 0.011 | 0.023 |
| | (0.016) | (0.050) | (0.018) | (0.056) | (0.013) | (0.040) | (0.010) | (0.033) |
| 10th Quarter | −0.048 *** | −0.095* | 0.025 | 0.042 | 0.050 *** | 0.094** | 0.015 | 0.030 |
| | (0.016) | (0.051) | (0.018) | (0.056) | (0.013) | (0.039) | (0.010) | (0.034) |
| 11th Quarter | −0.054 *** | −0.105** | 0.021 | 0.034 | 0.047 *** | 0.088** | 0.009 | 0.018 |
| | (0.016) | (0.052) | (0.018) | (0.056) | (0.013) | (0.040) | (0.011) | (0.035) |
| 12th Quarter | −0.061 *** | −0.120** | 0.009 | 0.010 | 0.045 *** | 0.084** | 0.004 | 0.008 |
| | (0.016) | (0.053) | (0.018) | (0.056) | (0.013) | (0.041) | (0.011) | (0.035) |
| 13th Quarter | −0.058 *** | −0.116** | 0.016 | 0.025 | 0.053 *** | 0.100** | 0.012 | 0.024 |
| | (0.017) | (0.054) | (0.018) | (0.056) | (0.013) | (0.041) | (0.011) | (0.035) |

Notes: This figure displays the effect of JSM on the probability of being in UI, employment, DI. and other SWP k quarters (k = 0,1,. . .,12) after the predicted date of notification. For each outcome, we report the ITT and the LATE coefficients obtained by estimating equation (1). Control variables included in the regressions are: year of unemployment entry; gender; household category; a dummy for Belgian nationality; region; education; contract type of the last job; last daily wage; length of unemployment; and cumulative worked days and DI days during the three years preceding unemployment entry. The dataset contains all unemployment spells that reached an unemployment duration of 15 months between July 2007 and December 2011 for individuals who have not been previously monitored. Robust standard errors are displayed in parentheses. *p<0.1; **p<0.05; ***p<0.01.

### A.3. Computation of the unemployment duration

According to the official rules for the computation of unemployment duration (which served as the basis for sending the notification letters), the moment from which the duration is computed is defined in the following way:

- For the first unemployment spell: the day of registration.
- For subsequent unemployment spells: the first day of registration that follows full-time employment that lasted *at least 12 months over a 15-month period.*

From the moment when unemployment duration starts, any compensated day of active unemployment (including job training days) is recorded. The statuses exempt from this definition are: (1) periods of temporary unemployment; (2) exemptions for social or family reasons; (3) part-time subsidized jobs if the subsidized job exceeds one-third of a full-time equivalent; (4) periods of unemployment with a medical impairment between 33% and 66% (before January 2012); and (5) periods preceding an indefinite exclusion from UI after a negative monitoring interview.

These rules have two consequences for our analysis. First, the unemployment duration as computed by the national UI agency does not exactly correspond to the time elapsed since entry into unemployment. Fig. A1 displays the duration of time since the unemployment entry in our sample. It shows that this time span is 0–13 months (resp. 14–18, $\geq$19 months) for about 30% (resp. 60%, 10%) of the sample.

Second, even after carefully following the rules provided by the national UI agency to compute the UI duration, there are some prediction errors in the moment individuals are notified. Indeed, some individuals receive the letter in a different month from what was predicted while others do not receive any letter at all. There are multiple reasons for these prediction errors. First, because we only have employment data on a quarterly basis, it is difficult to assess exactly whether the unemployment duration should be reset for new unemployment spells. Indeed, small imprecisions in the reset timing might generate large errors in the predicted moments of notification. Second, for part-time supported employment, we do not know exactly whether individuals work more or less than one-third of a full-time equivalent, which might create some errors. Third, our data do not provide information on the percentage of disability for individuals on UI. Therefore, we wrongly predict that individuals with a percentage of disability between 33% and 66% will receive a notification letter. Fourth, we noticed that some job trainings were not accounted for in the duration of unemployment, but these internal rules are not systematic, which impedes us from improving our prediction algorithm.

Fig. A.2 exhibits the distribution of these prediction errors, i.e., the time span between the predicted and the actual month of notification. We can see that a bit<50% of individuals who were notified received the notification letter in the predicted month.

Finally, Fig. A.3 provides a visual representation of the discontinuity in the probability of receiving a notification letter at age 49 in Panel (a) and the probability of receiving the notification letter in advance (UI duration < 15 months), at the predicted date (UI duration = 15 months), and later than predicted (UI duration greater than 15 months), respectively, in Panels (b), (c) and (d). We can see that the probability of receiving the notification letter decreases by 50.8 pp when reaching the cutoff age of 49, and that this discontinuity is entirely driven by the discontinuity in the probability of receiving the notification for matched individuals.

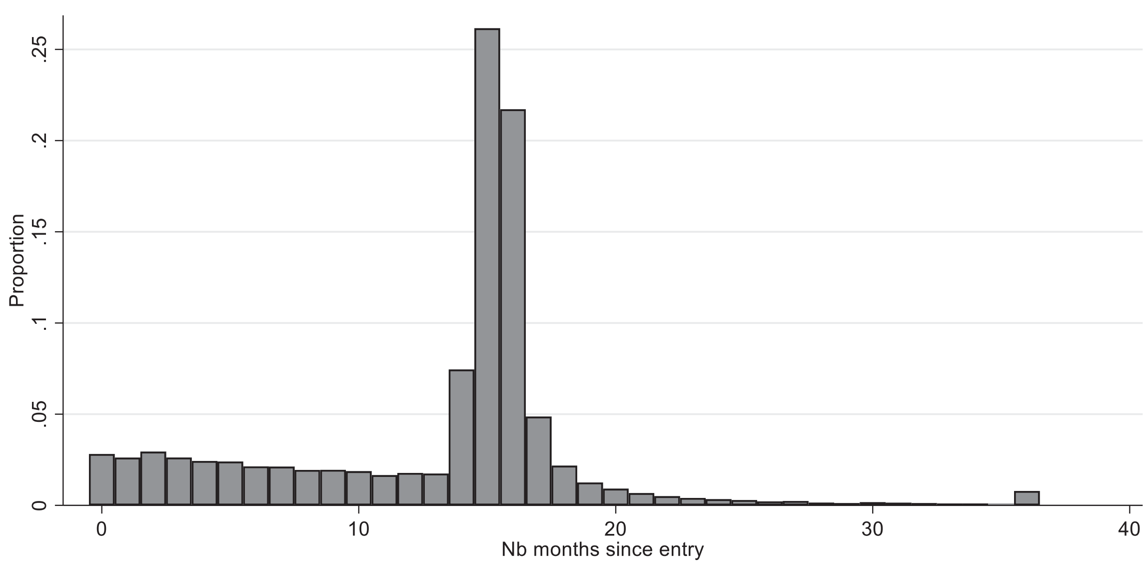


**Fig. A1.** Distribution of the time elapsed since unemployment entry.

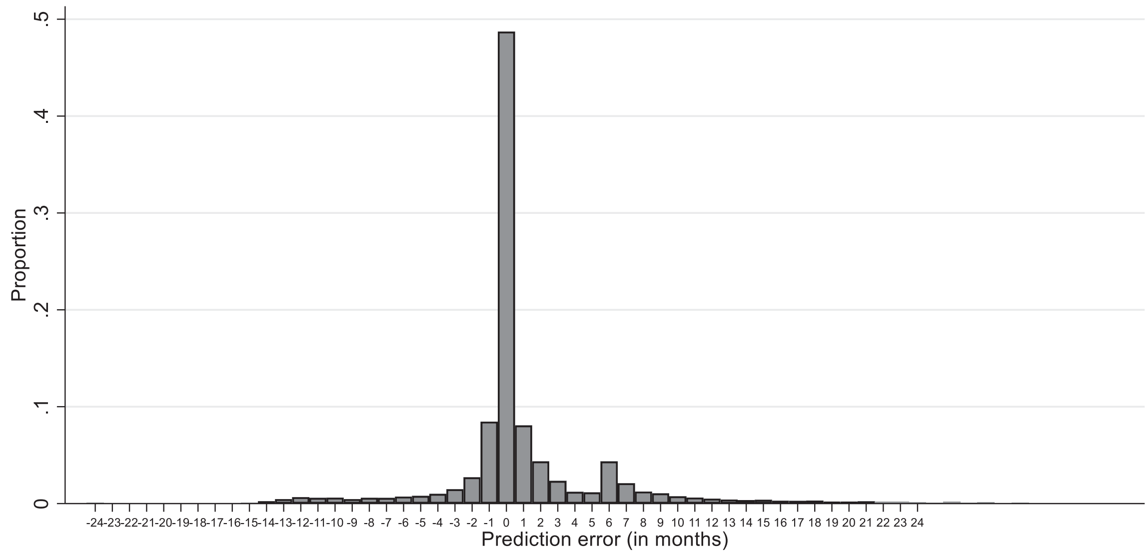


**Fig. A2.** Distribution of prediction errors. Notes: This figure displays the distribution of prediction errors, i.e., the difference between the observed and the predicted month of dispatch of the notification letters. Any error outside the interval [-24;24] has been assigned the value of −24 or 24. We can see that a bit<50% of the letters were sent during the predicted month.

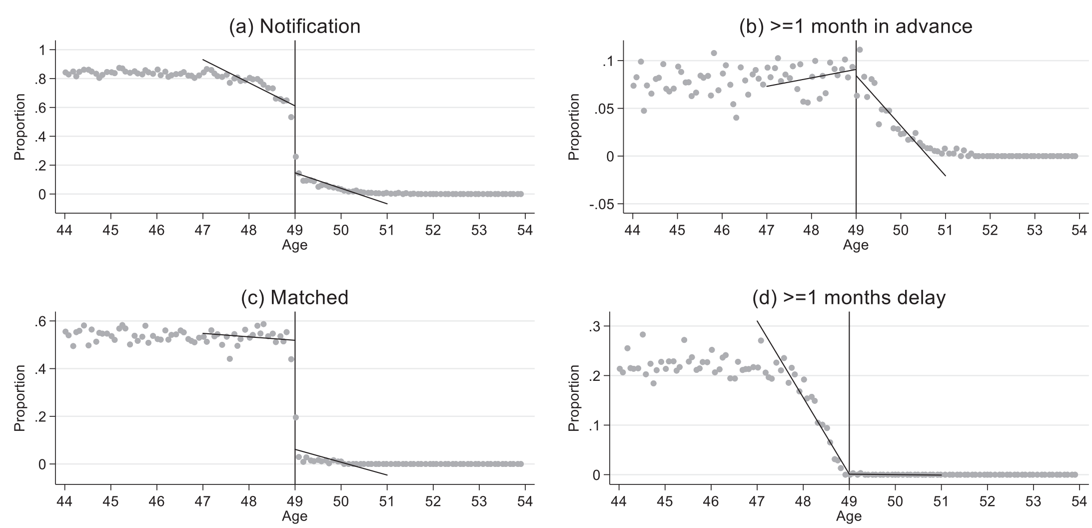


**Fig. A3.** Distribution of prediction errors by age. Notes: This figure displays the size of the prediction errors as a function of the age of individuals at the predicted moment of dispatch of the notification letter. Panel (a) displays the proportion of individuals who received a notification letter regardless of the moment at which the letter was received. Panel (b) displays the proportion of individuals who have been notified more than one month before the predicted month of notification. Panel (c) displays the proportion of matched individuals, i.e., individuals who received the letter one month before to one month after the predicted month of notification. Panel (d) displays the proportion of individuals who have been notified more than one month after the predicted month of notification. We can see that the probability of receiving the notification letter decreases by 50.8 pp when reaching the cutoff age of 49, and that this discontinuity is entirely driven by the discontinuity in the probability of receiving the notification for matched individuals.

### A.4. Effects of JSM on cumulative number of days

Fig. A.4 provides a visual representation of the discontinuity in the number of days spent in unemployment, employment, DI and other SWP at age 49.

### A.5. Heterogeneity

Looking at heterogeneous effects across subgroups is a first step to pinning down the mechanisms behind our results and may also help policymakers design targeted activation programs for specific groups of individuals. In this respect, previous studies have shown that individuals who are in poor health, less educated, female, blue collar, have a lower job attachment, and live in deprived areas tend to be overrepresented among DI recipients (Autor and Duggan, 2006; Bratsberg et al., 2013; De Brouwer and Tojerow, 2019). We thus estimate our benchmark results, separating our sample along several characteristics: number of UI days over the last three years (above/below median), number of DI days over the last three years (above/below median), local unemployment rate (above vs below median), last daily wage (above vs. below median), last occupation (white- vs. blue-collar), education (tertiary vs. non-tertiary degree), and gender.

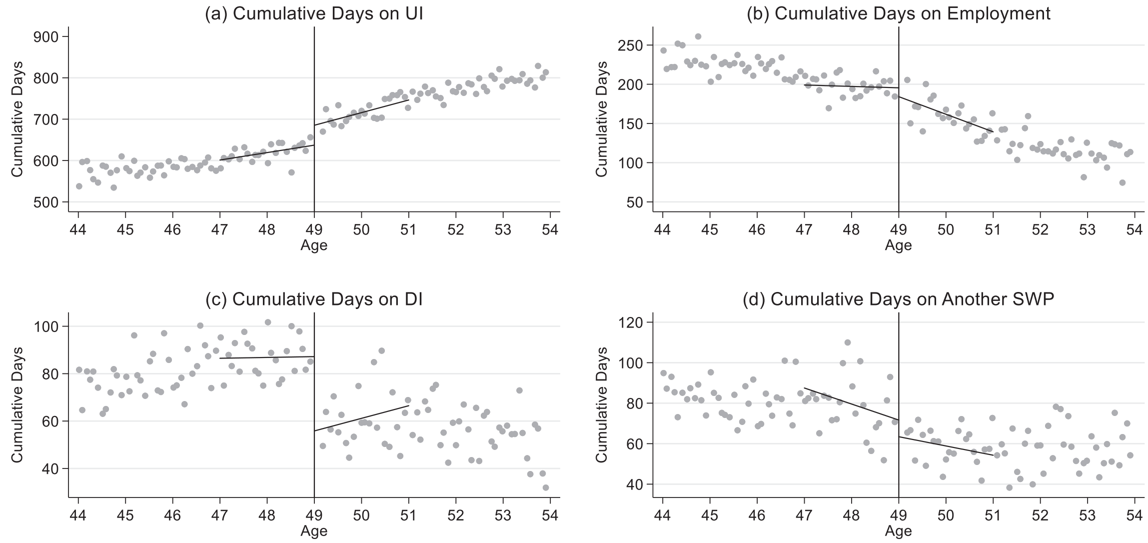


**Fig. A4.** Effect of JSM on cumulative days in each labor market status. Notes: This figure provides a visual representation of the discontinuity in the number of days spent in unemployment, employment, DI and other SWP at age 49. Panels (a), (b), (c) and (d) respectively plot the cumulative number of days spent in UI, employment, DI and other SWP, within a horizon of three years following the reception of the notification letter.

The results of the heterogeneity analysis are presented in Figure A5, which reports the LATE estimates for each outcome and each subgroup along with 95% confidence intervals. Although it does not have sufficient power to distinguish differences in treatment effects between groups, this analysis allows us to draw a few insights. First panel (a) shows that JSM appears to decrease the

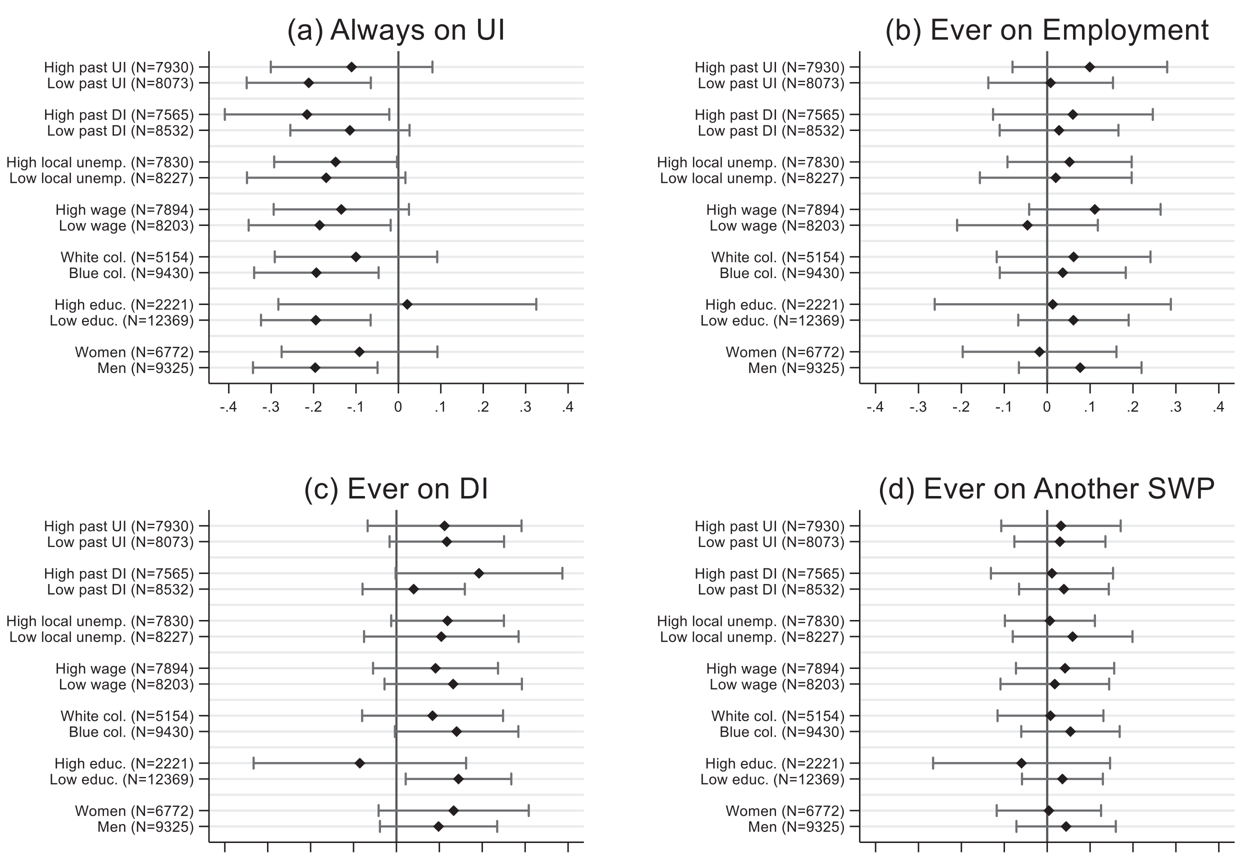


**Fig. A5.** Heterogeneous effects. Notes: This figure investigates how the treatment effect of JSM varies with a number of characteristics of jobseekers. We show estimated treatment effects for individuals with low (under median) versus high (above median) number of days on UI in the previous three years; low (under median) versus high (above median) number of days on DI in previous three years; low (under median) versus high (above median) unemployment rates in the local labor market at the predicted date of notification; low (under median) versus high (above median) past wages, white- versus blue-collar, low (maximum secondary) versus high (tertiary) education; and man versus woman. Coefficients display the fuzzy RD estimates with 95% confidence intervals.

probability of being always unemployed for all groups, except the highly educated. Second, panel (b) shows that employment increased relatively more for those who had higher wages before entering UI (although the coefficient is not statistically significant). This could reflect a stronger labor market attachment or higher potential wages on the labor market. Third, panel (c) shows that the effect of JSM on DI is larger for the less educated and individuals with higher past DI days than for the highly educated and individuals with lower past DI days. The effect on DI is otherwise quite homogeneous across groups (and positive for all, except the highly educated), with slightly higher point estimates for blue-collar workers. Finally, panel (d) reveals no clear pattern regarding the effect of JSM on other SWP, the coefficients being small in magnitude and never statistically significant. Overall, this analysis supports the hypothesis that the LT unemployed who are most at the margin of transiting to DI as a result of JSM have fewer labor market prospects (proxied by education) and are in worse health (proxied by the number of past DI days), while those most likely to transition to employment as a result of JSM are those with higher potential wages (proxied by past wages).

### A.6. Robustness checks

*Main robustness checks*

Figure A6 and Table A4 show how the LATE coefficients of our benchmark outcomes evolve with the size of the bandwidth. In this figure, we report both the bandwidth used in our benchmark estimations (indicated by the full vertical line) and the MSE optimal bandwidth proposed by Calonico et al. (2014) (indicated by a dashed vertical line). We can see that the size of the coefficients remains very stable for any bandwidth size above one year, while the confidence intervals shrink rapidly between zero and two years. This explains why none of the coefficients are statistically significant under the data-driven bandwidth selection procedure proposed by Calonico et al. (2014), despite the coefficients being similar in magnitude. This result is likely due to the fact that we estimate a fuzzy RD design, which creates additional volatility of the observations near the age threshold. Taking a longer bandwidth than the one suggested by Calonico et al. (2014) does not change the magnitude of the coefficients, but significantly reduces the standard errors.

Columns (2)-(5) in Table A3, display the LATE coefficients for the benchmark outcomes estimated with higher polynomial orders and alternative kernel forms to ensure that our results are robust to changes in specification. Columns (6)-(7) of the table report the bias-corrected LATE estimates proposed by Calonico et al. (2014), and Column (1) reports our benchmark results for ease of comparison. This table shows that the point estimates are remarkably similar across the different specifications, although results lose statistical significance with a quadratic polynomial.

Next, Figure A7 displays the results of a donut-hole test, which allows us to see how the results vary when we progressively exclude the observations that are closest to the threshold. This test ensures that our results are not influenced by manipulation or local non-linearities around the threshold. We can see that although statistical significance decreases as we remove more observations around the cutoff, the magnitude of the coefficients remains stable.

Next, in Figure A8, we verify that our findings are not driven by pre-existing discontinuities that are not related to the implementation of JSM, e.g., due to the existence of other activation programs or replacement benefits rules. To do so, we estimate Equation (1) using pre-reform data, i.e., by selecting individuals who would have been monitored between January 2003 and July 2006,[42] if the monitoring program had already been in place at that time. Note that we can only estimate the ITT coefficients here, since no one in this exercise is actually treated. As expected, we find negligible and statistically insignificant estimates, thus allowing us to rule out the concern that our findings are due to pre-existing discontinuities.

Finally, we verify that our findings are not driven by discontinuities at age 49 that exist for all jobseekers during our observation window, irrespective of JSM, by exploring discontinuities at other unemployment durations than 15 months. Specifically, we select individuals with an unemployment duration between one and 30 months and estimate the discontinuity at age 49 for each unemployment duration using the same model specification as in the benchmark results. Figure A9 displays the ITT coefficients of each regression (i.e., for each unemployment duration). Reassuringly, most of the coefficients are small and statistically insignificant, except around the duration of 15 months for the outcomes "Always on unemployment" and "Ever on DI." For these two outcomes, the coefficients close––but not equal––to the 15-month unemployment duration are also significantly different from zero. This can be explained by the fact that as we get closer to the discontinuity at 15 months, the polynomial estimated for neighboring unemployment durations is influenced by this discontinuity.

### Additional robustness checks

In our summary statistics, we observe that the mean number of past DI days is quite high while the mean number of past worked days is quite low. Figure A10 displays the entire distribution of these two variables. For past DI days, we observe that although the vast majority of individuals experienced a low number, the mean is influenced by a small number of individuals who have a high number of past DI days. For past worked days, we observe that a significant share of individuals has not worked at all during the three years preceding unemployment entry.

To check that our results are robust to the exclusion of extreme values, we run additional robustness tests in which we exclude individuals with more than 900 days on DI and individuals with no worked days 0–3 years before entry into UI. In addition, we run an additional test to see whether the results vary according to the time elapsed since entry into UI. To do so, we divide our sample in two parts according to whether (1) the UI spell started<15 months, or (2) the UI spell started 15 months or more before the predicted date of notification. Figure A11 provides graphical evidence of the results. We can see that the results are robust to the exclusion of extreme values. Interestingly, we observe that the effects on UI and DI are smaller and statistically insignificant for individuals whose UI spell started<15 months before the predicted date of notification. The benchmark results are driven by individuals whose UI spell started 15 month or more before the predicted date of notification.

[42] We use data before July 2006 to preclude the possibility that these individuals effectively received a notification letter at the predicted date of reception. Remember that individuals between the ages of 40 and 50 began to be monitored progressively between July 2006 and June 2007.

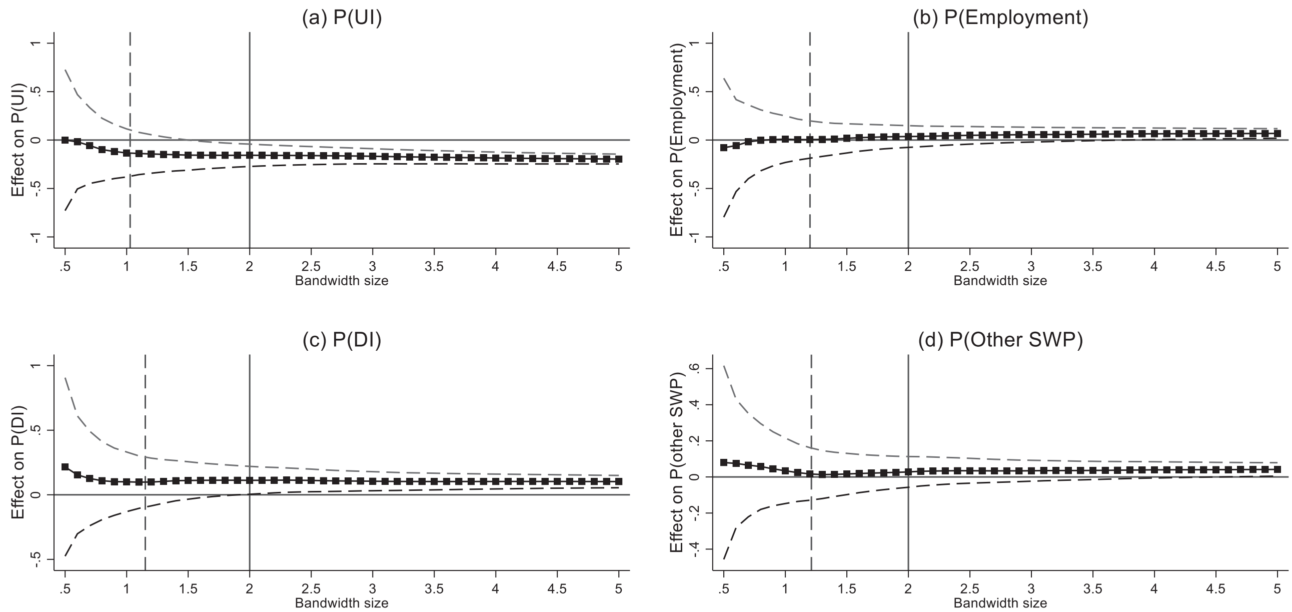


**Fig. A6.** Sensitivity test – bandwidth size. Notes: This figure displays the sensitivity of the benchmark coefficients to changes in the bandwidth size. The vertical axis represents the size of the LATE coefficients, and the horizontal axis displays the size of the bandwidth used on each side of the cutoff. The dashed vertical line corresponds to the bandwidth separately computed for each outcome using the procedure proposed in Calonico et al. (2014). The full black vertical line corresponds to the bandwidth used in our estimations. Black squares display the estimated coefficients with 95% confidence intervals.

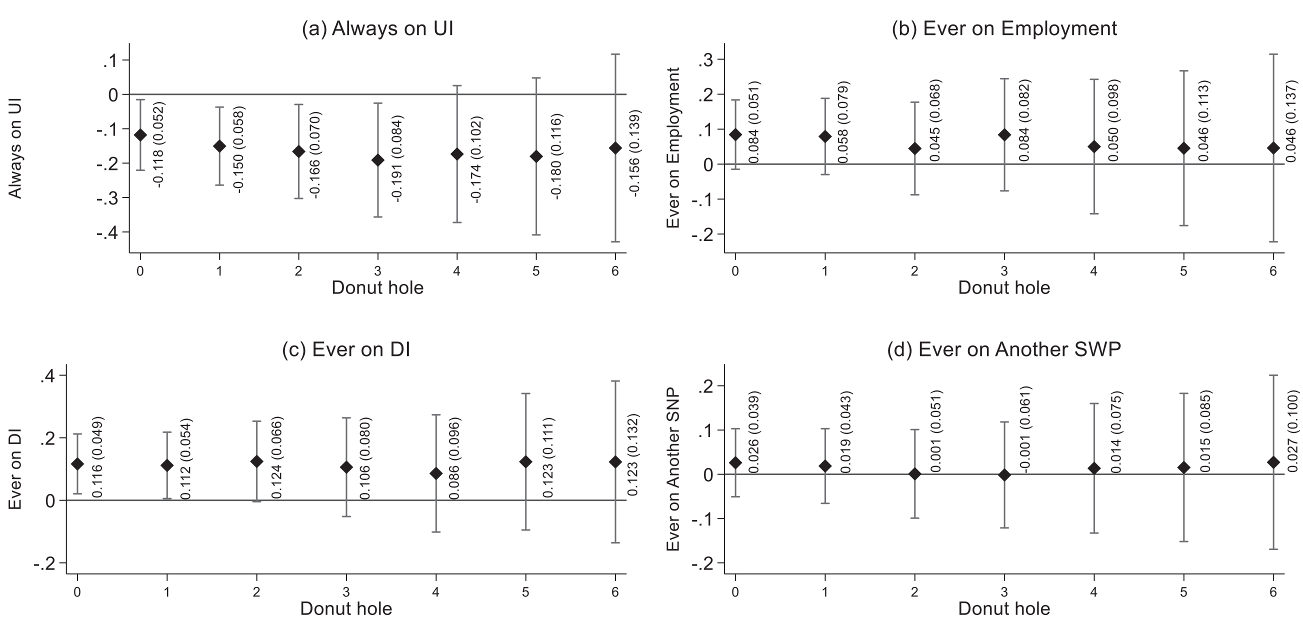


**Fig. A7.** Sensitivity test – donut-hole test. Notes: This figure displays the sensitivity of our benchmark findings when we drop individuals who are closest to the cutoff. For all figures, the vertical axis represents the size of the LATE coefficients. The horizontal axis indicates the number of months that we delete from our estimations on each side of the cutoff. We report the value of each coefficient (and standard errors in parentheses) next to each dot in the graphs. Estimates are shown with 95% confidence intervals.

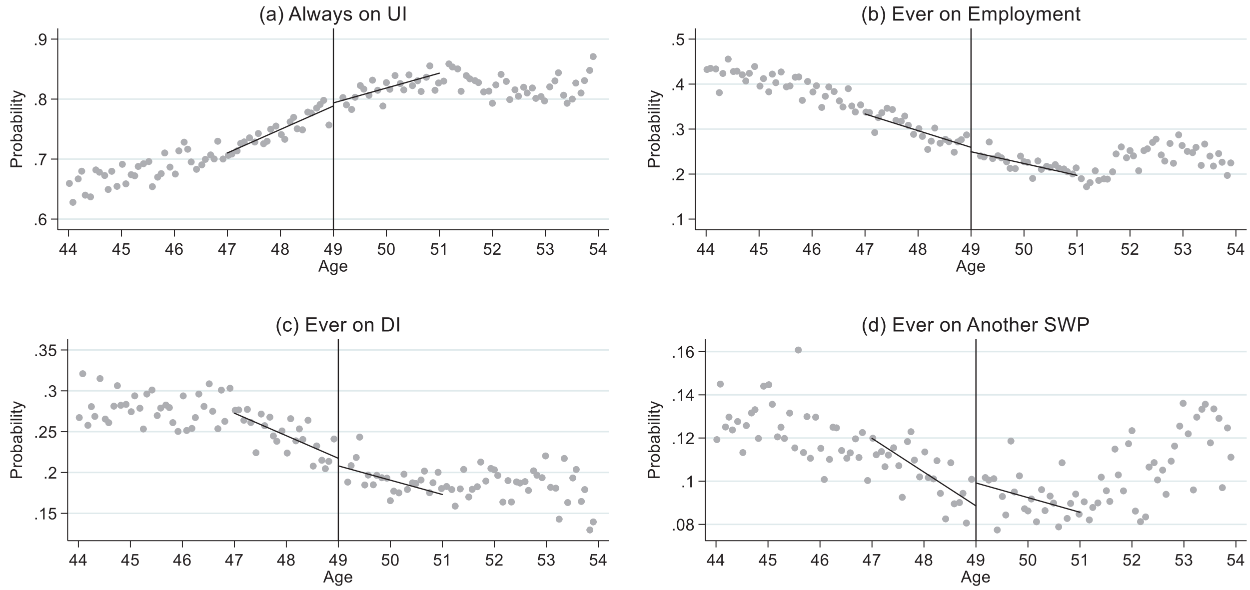


**Fig. A8.** Placebo test – pre-reform discontinuity. Notes: This figure displays the discontinuities of our benchmark outcomes when using pre-reform data. Specifically, we select individuals who would have been monitored between January 2003 and July 2006 if the monitoring program had already been effective at that time, and estimate Equation (1) using these observations. Estimated coefficients are all close to zero and statistically insignificant.

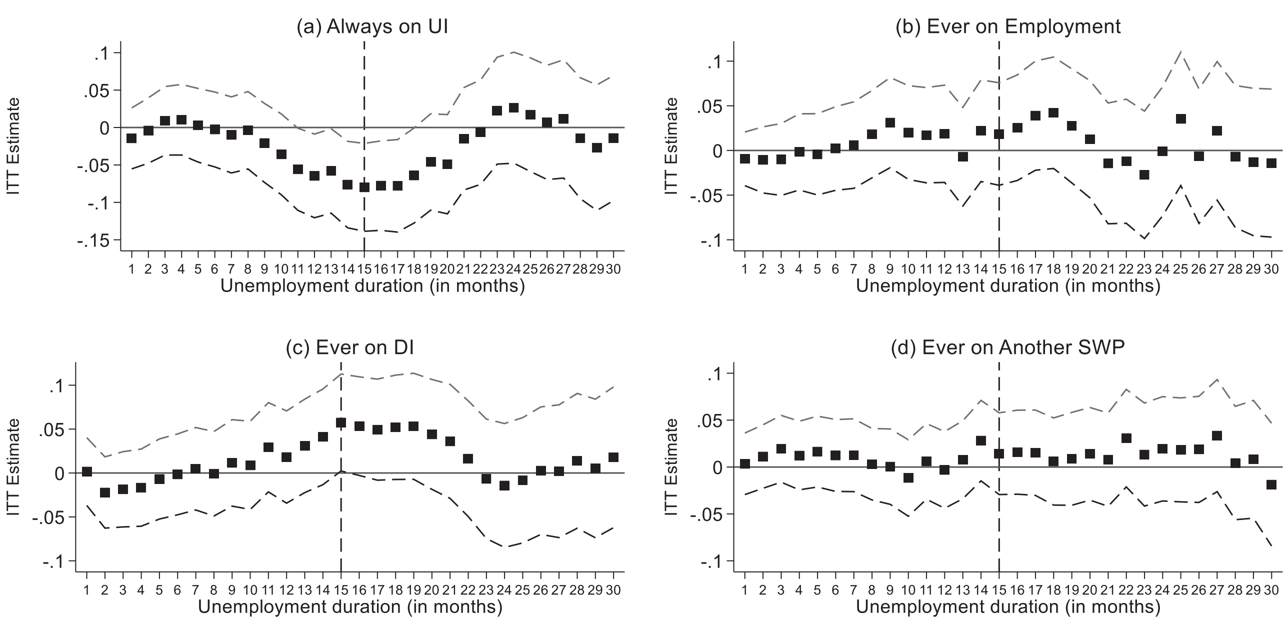


**Fig. A9.** Placebo test – discontinuities at alternative unemployment durations. Notes: This figure displays the discontinuities of our benchmark outcomes for different unemployment durations. Specifically, we select individuals around the age cutoff with an unemployment duration between one and 30 months and estimate the discontinuity at age 49 at each unemployment duration. Reassuringly, the discontinuity in employment and DI is centered around the 15-month unemployment duration.

### A.7. Dynamic effect of JSM on benchmark outcomes

Table A5 displays the effect of JSM on the probability of being in UI, employment, DI. and other SWP k quarters (k = 0,1,...,12) after the predicted date of notification.

Table A6 and Figure A12 display the effects of JSM on the probability of being on DI, in combination with having been through different steps of JSM.

**Table A6**
DI effects at different stages of JSM.

| | (1) | (2) | (3) |
|---|---|---|---|
| **Outcome** | Predicted outcome at age = 49+ | ITT | LATE |
| DI before first interview | 0.304 | −0.000 | −0.012 |
| | | (0.017) | (0.053) |
| DI before second interview | 0.314 | 0.049 *** | 0.084 |
| | | (0.018) | (0.055) |
| DI before third interview | 0.319 | 0.064 *** | 0.112** |
| | | (0.018) | (0.055) |
| Benchmark | 0.320 | 0.064 *** | 0.112** |
| | | (0.018) | (0.055) |

Notes: This table displays the effects of JSM on the probability of being on DI, in combination with having been through different steps of JSM. "DI before first (resp. second, third) interview" corresponds to being on DI without ever having had a first (resp. second, third) interview. Column (1) displays the predicted outcomes on the right (age = 49 + ) of the cutoff. Column (2) displays the ITT coefficients and Column (3) displays the LATE coefficients. Control variables included in the regressions are: year of unemployment entry; gender; household category; a dummy for Belgian nationality; region; education; contract type of the last job; last daily wage; length of unemployment; and cumulative worked days and DI days during the three years preceding unemployment entry. The dataset contains all unemployment spells that reached an unemployment duration of 15 months between July 2007 and December 2011 for individuals who have not been previously monitored. Robust standard errors are displayed in parentheses. *p<0.1; **p<0.05; ***p<0.01.

## A.8. Effect of JSM on additional outcomes

Table A7 displays the effect of JSM on a set of additional outcomes: (i) direct versus indirect transitions from UI to DI; (ii) jobseeking versus non-jobseeking UI; and (iii) salaried versus self-employment.

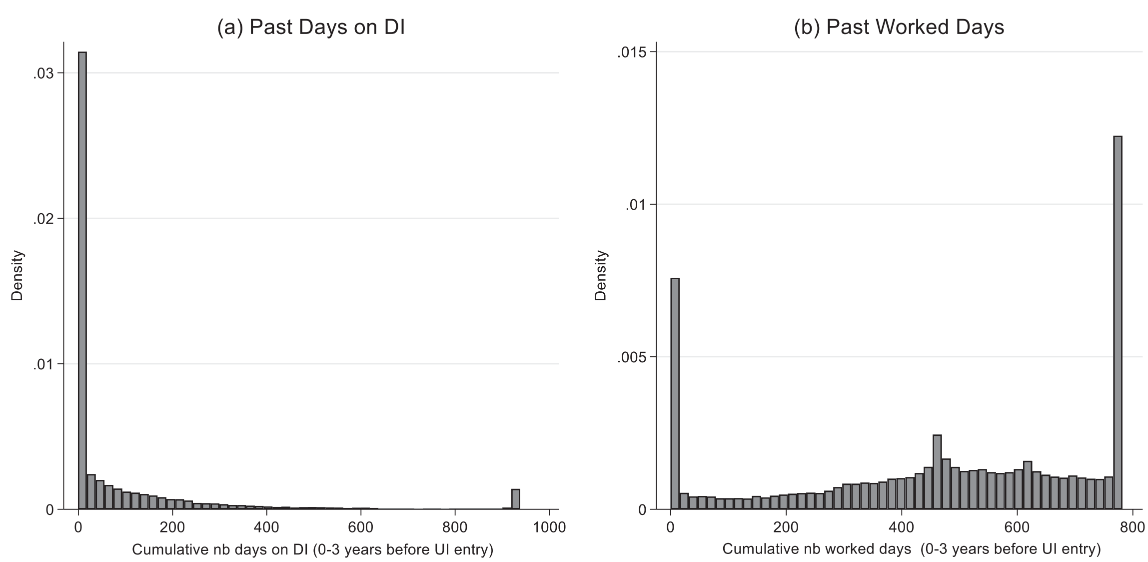


**Fig. A10.** Distribution of Past Days on DI and Past Worked Days. Notes: This figure displays the number of days spent on DI (Panel (a)) and number of days worked (Panel (b)) over the three years preceding entry into unemployment. Panel (a) shows that most individuals have<100 DI days before the start of their unemployment spell, but a small share of individuals have more than 900 past DI days. Panel (b) shows that a non-negligible share of individuals has not worked at all during the three years that preceded UI entry.

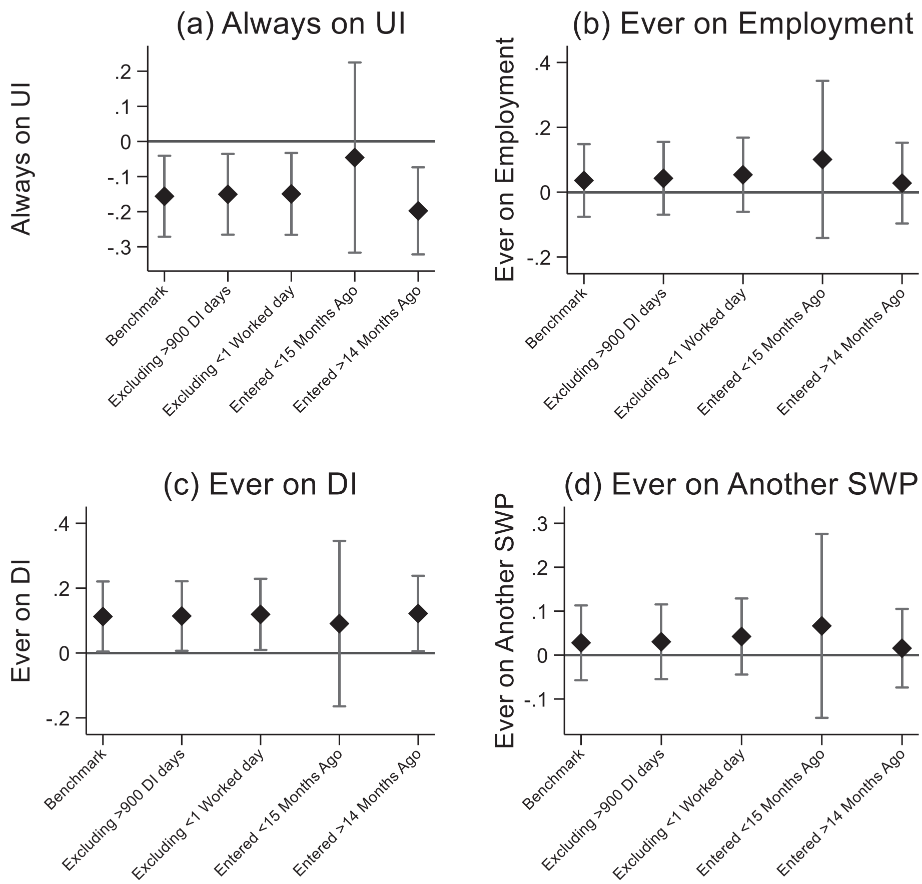


**Fig. A11.** Additional robustness tests. Notes: This figure compares the LATE coefficient of the benchmark results with the LATE coefficients obtained by excluding some groups of individuals. The bandwidth, kernel, and polynomial of the RD estimation are identical across all estimations.

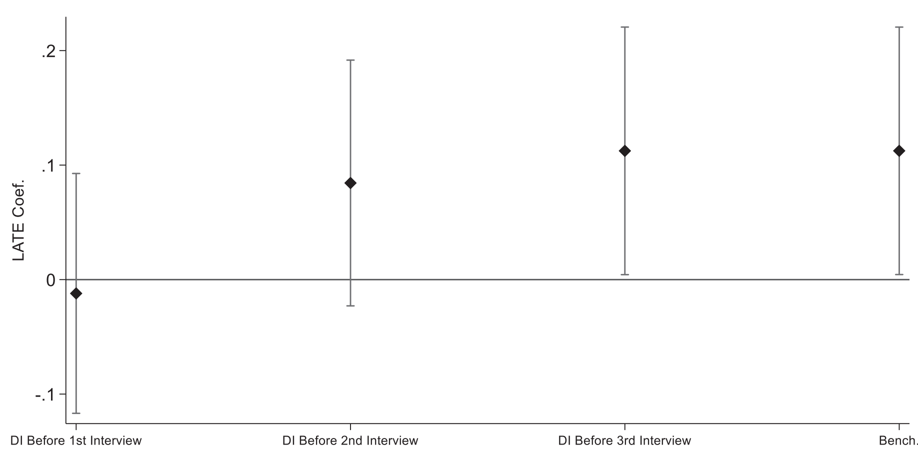


**Fig. A12.** DI effects at different stages of JSM. Notes: This figure displays the effects of JSM on the probability of being on DI, in combination with having been through different steps of JSM. "DI before first (resp. second, third) interview" corresponds to being on DI without ever having had a first (resp. second, third) interview. Our benchmark estimate corresponds to being in DI at any point in time within a three-year horizon (irrespective of whether it is before the first, second, or third interview).

### A.9. Aggregate effect of JSM on UI and DI transfers

Table A8 summarizes the number of notification letters that have been dispatched each year in Belgium between January 2007 and December 2011 to individuals aged 40–49. Table A9 displays the value of annual UI and DI transfers for individuals aged 40–49 between January 2007 and December 2011.

**Table A7**
Effects of JSM on additional outcomes.

| | (1) | (2) | (3) |
|---|---|---|---|
| | Mean (at age = 49 + ) | ITT | LATE |
| Direct transitions to DI | 0.151 | 0.051 *** | 0.096** |
| | | (0.014) | (0.044) |
| Indirect transitions to DI | 0.159 | 0.016 | 0.025 |
| | | (0.014) | (0.045) |
| Jobseeking UI | 0.403 | −0.056 *** | −0.103* |
| | | (0.018) | (0.058) |
| Non-jobseeking UI | 0.420 | 0.022 | 0.036 |
| | | (0.019) | (0.059) |
| Salaried employment | 0.525 | 0.029 | 0.043 |
| | | (0.019) | (0.058) |
| Self-employment | 0.067 | −0.013 | −0.022 |
| | | (0.009) | (0.029) |

Note: This table displays the effect of JSM on a set of additional outcomes: (i) direct versus indirect transitions from UI to DI; (ii) jobseeking versus non-jobseeking UI; and (iii) salaried versus self-employment. Control variables included in the regressions are: year of unemployment entry; gender; household category; a dummy for Belgian nationality; region; education; contract type of the last job; last daily wage; length of unemployment; and cumulative worked days and DI days in the three years preceding unemployment entry. The dataset contains all unemployment spells that reached an unemployment duration of 15 months between July 2007 and December 2011 for individuals who have not been previously monitored. Robust standard errors are displayed in parentheses. *p<0.1; **p<0.05; ***p<0.01.

**Table A8**
Number of notification letters sent 2007–2011.

| | (1) | (2) |
|---|---|---|
| Year | Number of notification letters | Number of notified individuals |
| 2007 | 51,713 | 49,865 |
| 2008 | 14,513 | 13,590 |
| 2009 | 16,035 | 15,129 |
| 2010 | 17,936 | 17,009 |
| 2011 | 16,152 | 15,350 |
| Total | 109,337 | 104,451 |

Notes: This table summarizes the number of notification letters that have been dispatched each year in Belgium between January 2007 and December 2011 to individuals aged 40–49. Column (1) displays the total number of letters registered by the federal UI agency. Column (2) displays the number of letters that have been sent for the first time in the unemployment spell (i.e., avoiding letters that have been sent for the second, third, etc. time during the same unemployment spell).

**Table A9**
Total UI and DI expenditures 2007–2012.

| | (1) | (2) |
|---|---|---|
| Year | Total UI expenditure | Total DI expenditure |
| 2007 | 1,355,813,925 € | 1,028,060,495 € |
| 2008 | 1,339,444,012 € | 1,124,627,890 € |
| 2009 | 1,618,543,127 € | 1,226,017,880 € |
| 2010 | 1,589,845,182 € | 1,310,787,758 € |
| 2011 | 1,509,002,723 € | 1,404,189,941 € |
| Total | 7,412,648,969 € | 6,093,683,964 € |

Notes: This table displays the value of annual UI and DI transfers for individuals aged 40–49 between January 2007 and December 2011. The data used here come from the CBSS Data Warehouse and contain all UI and DI transfers made in Belgium in a given year. All financial values are in 2013 euros.

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