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ABSTRACT

The Unexpected Consequences of Job Search Monitoring: Disability Instead of Employment?*

This paper investigates how the implementation of Job Search Monitoring (JSM) programs over the last two decades could have impacted the rise of disability rates in OECD countries. To do so, we use an RDD design to study how a JSM program that was implemented in 2006 in Belgium could have played a role not only in the transition to employment and inactivity but also in the transition to disability. The RDD exploits the fact that the program was only targeted at long-term unemployed workers below the age of 50. Our results show that the JSM program has had a large impact on the transition rate from unemployment to disability and no impact on the transition rate to employment or inactivity. More precisely, individuals just below the age of 50 (the treatment group) are 1.43 percentage points (115%) more likely than individuals just above the age cut-off (the control group) to enter into disability during the next quarter. Looking at heterogeneous effects, we find that the effect is above all important for women and more particularly for single-women households. Overall, our study shows that JSM programs can have spillover effects on other social security branches, such as work disability. This is an important concern since it implies that JSM programs can push some individuals even further away from the labour market. Finally, our results show that the implementation of JSM could, constitute a viable explanation for the rise of the disability rate amongst unemployed workers.

JEL Classification: I13, J64

Keywords: disability, unemployment, Job Search Monitoring

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1. Introduction

Over the past decade, there has been growing concern about the increasing number of disability-benefit recipients in several OECD countries, including Germany, the United States, Australia and Japan. In Belgium, the disability rate has increased by more than 60% (from 5.5% to 8.9%) between 2004 and 2015, reaching an expenditure level equivalent to 1.6% of GDP (NIHDI 2014). This raises questions as to the reasons behind the rise in disability rates, especially since they have triggered a sharp increase in social security expenditures and generated a loss of productivity for the affected countries.

There has been an increasing number of studies focusing on the determinants of disability and the reasons behind increasing disability rates (Autor and Duggan 2003, Koning and Lindeboom 2015, Bratsberg et al. 2013). In this respect, a growing body of literature has emphasized that the frontiers between disability and other social security programs are often blurred and that transfers from one type of social support to another frequently occur (Autor and Duggan 2003, Stabuli 2011, Borghans et al. 2014). Reforms in one branch of social security thus raise the question of spillover effects on other social support programs. Figure 1 shows, for example, the evolution of the population registered in Belgium for unemployment and disability benefits between 1998 and 2016. We can observe that while the unemployed population has decreased over that period, the population of disabled individuals has largely increased.

[Figure 1]

Drawing on this context, this paper aims to explore how institutional changes in unemployment insurance affect the flow of entries into other types of social support, e.g. disability benefit[1]. Given that, in previous years, most OECD countries have set up different active labour market programs (ALMP) aiming at reducing unemployment duration, this paper focuses on the effect of one of these programs, i.e. a job search monitoring (henceforth JSM) program targeted at the long-term unemployed. In addition, several recent studies have shown that the transition rate from unemployment to disability has sharply increased since 2005 in OECD countries (OECD 2010) and

[1] In this article, we assign to all sickness absence of unemployed individuals the term "disability". This is because for an unemployed person, sickness absence is financed by the same institution from the first day of the sickness period. Any individual receiving sickness benefits is therefore considered as disabled in our analysis. In later sections, we make a distinction between short term disability and long term disability according to whether the disability lasts less or more than one year. More information about the Belgian disability system is provided in section 3.1.

more specifically in Belgium (De Brouwer and Tojerow 2019). In light of these facts, we aim to determine whether the implementation of job search monitoring for the unemployed increases the transition rate from unemployment to disability.

By doing so, we add to the existing literature by presenting evidence of interactions that occur between different branches of social security. Assessing the existence of interactions between unemployment and disability insurances is important for at least two reasons. First, to this day, no consensus has been reached regarding the causes of the rise in disability rates across many OECD countries. Although many studies have provided evidence that some links exist between labour market conditions and disability rates (Autor and Duggan 2003, Black et al. 2002), the mechanisms driving this relationship have not yet been clearly identified. Second, if substitution between social security programs does occur, it is crucial for policy makers to take these spillovers into account when designing policies (job search monitoring programs in the case of this paper) and assessing their overall costs and benefits.

To identify the effect of JSM programs on transition rates from unemployment to disability, we exploit the implementation of one of these programs in the mid-2000s in Belgium.[2] Aiming at improving the job search intensity of the long-term unemployed, this program made the entitlement to unemployment benefits conditional on "actively" searching for a job and providing evidence of sufficient job search efforts. In practice, after 13 months of unemployment, individuals became subject to periodic interviews during which their job search efforts were evaluated by the employment office. If these efforts were judged insufficient by the evaluators, or if there was evidence that the unemployed refused an acceptable job offer, sanctions imposed could go from a warning (in case of a first negative evaluation) to a permanent exclusion from unemployment benefits (in the case of three consecutive negative evaluations).

We take advantage of the fact that the program was implemented gradually by age groups, and hence only targeted the unemployed below the age of 50 between 2006 and 2013, to implement a regression discontinuity design. Identifying the difference between this treatment group and the unemployed over 50 (i.e. the control group), our results suggest that being subject to the monitoring scheme increases the quarterly transition rate to disability by 1,4 percentage points, which represents a 115% proportional increase in transitions to disability. This result is statistically significant and robust across various specifications and robustness tests. To

[2] The Job search monitoring program is fully described in section 3.

understand the mechanisms driving these results, we run various heterogeneity tests in order to see which groups were more particularly affected by the monitoring program. According to job search theory, individuals that are further from the labour market and individuals with more fragile health would be more incentivized to switch from employment to disability after the implementation of the program. We thus run heterogeneity tests by local unemployment rate, gender, household category, number of registered sick days in the past and the duration of the disability spell. We find that the effect is largely driven by women, for whom the increase is of 2.2 percentage points (a 195% proportional increase), while the effect for men is both negligible and insignificant. This effect is higher than previously found in the literature and is consistent with the observation that the recent increase in disability rates was especially pronounced for women. By further disentangling the effect by gender and household status, we find that amongst women, single-women households were particularly affected by the reform. Moreover, in line with job search theory, we find that the effect of JSM increases with the number of registered sick days in the past. These results represent a new addition to the literature and thus indicate the need for further research on the links between job search behaviour, health and household composition. When restricting our analysis to the group of women and establishing a distinction between short-term and long-term disability (i.e. disability spells lasting less or more than one year), we find a slightly higher proportional effect of JSM on short-term disability (+1.69 pp or a 199% proportional increase) than long-term disability (+0.53 pp or a 171% proportional increase). Finally, looking at heterogeneous effects across districts with different local unemployment rates, we find that the effect of JSM on disability increases with the local unemployment rate.

The outline of this paper is as follows. In the next section, we review the relevant scientific literature. In section 3, we briefly explain the Belgian institutional setting. We then describe the data used for conducting our estimations in Section 4. Section 5 presents our identification strategy and section 6 outlines the empirical findings. Finally, section 7 tests the robustness of these findings to several validity and falsification checks. The paper ends with a concluding discussion.

2. Literature Review

Over the past years, programs aiming at monitoring the unemployed's job search efforts were implemented in many OECD countries, which led to a growing interest in assessing the

effectiveness of these policies in bringing the unemployed back to work.[3] Overall, most studies focusing on JSM programs observe (i) a positive effect on the transition rate out of unemployment and (ii) a negative effect on post-unemployment earnings and job stability (Arni et al. 2013, Van den Berg and Vikström 2014, Petrongolo 2009).

In Switzerland, the effect of warnings and sanctions for not complying with unemployment eligibility criteria has been studied by Lalive et al. (2005) and Arni et al. (2013). Although both papers find that warnings and sanctions increase the job finding rate, Arni et al. (2013) find that they also reduce post-unemployment earnings (for both warnings and sanctions) and job stability (only for sanctions). A similar effect is observed with Swedish data by Van den Berg and Vikström (2014), who study the long-term consequences of imposing sanctions to unemployed workers for refusing acceptable job offers. The authors show that, despite their positive effect on the transition rate to work, sanctions force the unemployed workers to accept lower quality jobs (in terms of hourly wages, number of hours and occupation level), which decreases human capital accumulation and earnings in the long run. In the Netherlands, Van den Berg et al. (2004) estimate the effect of sanctions on the unemployed in 1994 throughout the city of Rotterdam, using a timing-of-events approach, while Lammers et al. (2013) analyse the effect of JSM and sanctions for the older unemployed (>57 years old) using a regression discontinuity design. Both studies find a substantial positive effect of sanctions on the transition rate to employment. However, these studies do not explore other post-unemployment outcomes such as job stability or earnings. In the UK, Petrongolo (2009) and Manning (2009) study the implementation of a large reform of the Jobseeker's Allowance program that took place in 1996 and strongly increased job search requirements for all jobseekers. Both papers find that the reform had a substantial negative effect on inflows to unemployment and a positive effect on unemployment exits. Nevertheless, the increased outflow from unemployment was not accompanied by the accession to stable jobs and, after one year, there is a negative effect of the reform on both annual earnings and the probability of having positive earnings. Finally, Cockx and Dejemeppe (2012) use a regression discontinuity design to study the effect of receiving a letter announcing the upcoming monitoring of job search efforts for the long-term unemployed under 30 years old in the Flemish part of Belgium. The authors do find a positive effect on the probability of finding a job but their point estimate is

[3] Both structural and reduced form approaches have been developed in this literature but, given that our paper is part of the second category, we will mainly review the papers that developed a reduced form approach. Note that we restrict our review to programs that impose sanctions and do not review the wider literature on activation programs. See for example Card et al. (2017) for a review on ALMP and Paserman (2008) and Van den Berg and Van der Klaaw (2013) on structural approach.

imprecisely estimated, only significant at a 6% significance level and does not apply to the entire Belgian population. More recently, Cockx et al. (2018) developed a structural job search model whose particularity is to integrate an imperfect job search monitoring technology. After calibrating their model with the same data used in Cockx and Dejemeppe (2012), they are able to estimate the behavioural effect of the monitoring program on job search intensity. The results show that the effect of monitoring on job search intensity is actually very close to zero.

Surprisingly, although a substantial amount of work has been dedicated to assessing the effect of job search monitoring and sanctions on unemployment duration and on the transition to employment, few have focused on the transition to other statuses, such as disability. Evaluations of job search monitoring schemes that do not take into account potential spillovers to other social support programs could, however, underestimate the true costs of these reforms. To our knowledge, the only studies that do look at the effects on non-employment outcomes are Lammers et al. (2013) and Petrongolo (2009).[4] Both find substantial evidence of transitions from unemployment to disability. Lammers et al. (2013), find that JSM for the unemployed aged between 57.5 and 59.5 increased the number of transitions from unemployment to disability by 4 (9) percentage points for men (women) within 24 months after the start of the unemployment spell. Petrongolo (2009) finds that starting an unemployment spell soon after the implementation of the JSA reform, compared to having started that spell 3 to 6 months before, increases the likelihood of receiving disability benefits by 2.5%-3% within 4 years. These studies thus indicate that the implementation of JSM could lead to an increase in the disability rate and hence obliterate the overall effect of JSM on employment.

Although these two articles focus on disability (Lammers et al. 2013, Petrongolo 2009), our article distinguishes itself by focusing on a JSM program that differs quite strongly in terms of the targeted population (namely with respect to age and unemployment duration), the potential sanctions that can be taken and the time horizon considered. More specifically, Lammers et al. (2013) focus on an older age population (57+) that is very close to retirement. Further, in the Dutch context that Lammers et al. (2013) investigate, the maximal sanction that can be taken is a 30-percentage reduction of the unemployment benefits during a period of 16 weeks. The Belgian context of our study differs substantially because the non-compliance of job search requirements

[4] In a slightly different context, Van den Berg et al. (2016) analyse the effect of vacancy referrals on unemployment duration by taking into account the endogenous probability of reporting sick to avoid being compelled to apply to the vacancy referral. Using a timing of event approach, the authors find that the probability of reporting sick sharply increases just after the notification of a vacancy.

may lead to permanent exclusion from the unemployment insurance. Petrongolo (2009) only looks at the outcome 2 to 4 years after the beginning of the unemployment spell. In contrast, we focus on direct transitions to another status, namely, disability. Third, unlike existing studies (Lammers et al. 2013, Petrongolo 2009), our research focuses on the population of the long-term unemployed aged around 50 years old. As a matter of fact, this age category is particularly interesting to study because job search monitoring schemes might affect these individuals specifically given that they are frequently considered to be those furthest away from the labour market (due to their age and the length of separation from the labour market). Coincidentally, the implementation timing of the procedure allows us to study these individuals for several years following the reform. Finally, in order to understand which populations are most affected by JSM, we explore the heterogeneous effect of the reform along several dimensions such as local labour market conditions, gender, household category, number of registered sick days in the past and the length of the disability spell.

To explain the impact of JSM on job search behaviour the literature has typically focused on elements like minimal compulsory job search effort in Mortensen (1986) and job search framework (Abbring et al. 2005, Arni et al. 2013, Boone et al. 2007, Van den Berg et al. 2004, Van den Berg and Vikström 2014). In these models, the introduction of such requirements fosters the job finding rate for two reasons. First, because job search intensity is measured imperfectly, JSM increases the job search efforts of all the unemployed. This is because job search effort is now used not only to find jobs but also to decrease the probability of being sanctioned[5]. Second, because the positive probability of being sanctioned decreases the present value of the flows of unemployment benefits, JSM and sanctions also decrease the reservation wage of the jobseeker. As a result, although the transition rate to employment is expected to rise, the effect on expected earnings and job quality is mitigated due to the negative effect on the reservation wage.

Among the authors who developed the theoretical framework discussed above, none have included disability in their analysis. However, the decision to apply for disability benefits must be treated distinctly mainly for two reasons. First, in many OECD countries, disability benefits are quite generous relative to unemployment benefits. For example, in Belgium long-term disability benefits amount to 65% of the last monthly wage for heads of household and remain stable over time. Second, disability applicants are subject to a screening process (whose strictness differs

[5] Note that Boone et al. (2009) distinguish an ex-ante effect (before being sanctioned) and an ex-post effect (after being sanctioned). They show in an experiment that both effects are significant but that the ex-ante effect is greater in magnitude.

across countries). Due to the fact that health is measured with some error, there is always a positive probability that an application is rejected. The inclusion of disability as a distinct alternative exit route from unemployment would constitute an interesting extension of the job search models developed earlier and is left for future research. However, based on the existing literature that has described the mechanisms behind the decision to apply for disability benefits (Autor and Duggan 2003, Low and Pistaferri 2015, Haller et al. 2018), we draw some insights about what such a model could predict.

We base our intuition on Low and Pistaferri (2015) who developed a life cycle model in which agents can be in three possible states: employment, unemployment or disabled. Every period, agents decide to stay in the labour market or to apply for disability benefits in the next period. This choice is made according to a maximization problem in which the decision depends on the employment status (employed or unemployed), individual productivity, health status and accumulated capital. These parameters influence the probability of being accepted in case of application to the disability program, which in turn affects the solution of the agent's decision problem. Based on this conceptual framework, we can easily draw some predictions about the effects of the introduction of a JSM program on the jobseekers' decision problem. The direct effect of introducing such program is to reduce the present value of unemployment, mainly through two channels, (1) increased job search effort and (2) uncertainty about the fact of being sanctioned. By shifting the present value of unemployment, the introduction of JSM therefore makes disability (and employment) more attractive for the jobseeker. Note that the magnitude of the effect interacts with parameters such as the health status, individual productivity and re-employment prospects. A natural hypothesis would be that unemployed workers with poor health and low re-employment prospects will be more induced to apply for disability benefits as a result of the introduction of the JSM program. These heterogeneous effects will be investigated in section 6.

3. Institutional Context and the Implementation of New Job Search Monitoring Schemes

The New Job Search Monitoring Scheme

Like in many OECD countries, a widespread reform was implemented in Belgium to the Unemployment Insurance system[6] in the 2000s, aiming at monitoring the job search efforts of the

[6] Appendix 1 (section 0) provides more details on the Belgium's Unemployment Insurance System.

unemployed. Before this reform, the unemployed had the obligation to be available for the labour market and had the choice to participate in regional employment support programs, but no monitoring of their actual search activity was in place. In 2004, the government announced that a Job search monitoring program would be set up for the unemployed below the age of 50 and considered as being long-term unemployed those collecting benefits for more than 13 months. Implemented gradually, the program first only applied to the unemployed below 30 years old, before being extended to those below 40 in July 2005 and to those below 50 in July 2006. Finally, in 2013, the original program limiting the monitoring procedure to the unemployed below 50 years old was extended to all entrants aged 50-54 years old and finally extended to all unemployed workers below the age of 60 in January 2015. The regression discontinuity design used in the empirical section takes advantage of this progressive implementation to capture the causal effect of this type of program on the unemployed.

[Figure 2]

In practice, after 13 months of unemployment, an unemployed worker receives a notification letter informing her about their obligations in terms of actively searching for a job.[7] This letter also states that the individual will be invited to present herself at the unemployment office 8 months later for an interview, during which her job search efforts will be evaluated[8]. Figure 2 describes the different potential outcomes after this first monitoring interview takes place. It can either be positive – in which case the individual will receive 8 months later a new notification letter inviting her for a new "first" interview 8 months later– or negative – in which case a second interview is scheduled 4 months later. No financial sanction is imposed for non-compliance during the first interview, but the unemployed person is invited to sign a contract in which she commits to taking a number of defined actions during the following 4 months, after which the respect of this contract is evaluated during a second interview. If the outcome of the second interview is positive, the procedure starts from the beginning and the individual is invited for a new first

[7] The unemployment spell before receiving the letter is only 7 months for people under 25.

[8] Note that during the first phases of the reform's implementation, the interviews could take place sooner for the stock of long-term unemployed workers, i.e. those who had been unemployed for over 13 months when the reform was introduced. In this case, the first interview could take place as early as 3 months after the notification (instead of 8), as long as the individual had reached 21 months (13 + 8) of unemployment on the date of the interview. Note also that it took almost a year (between July 2006 and June 2007) to send the first notification letters to the overall targeted population because the number of long-term unemployed workers under the age of 50 was so enormous when the reform was implemented. Hence, this element will need to be taken into account in our estimations as we can only be sure that the targeted individuals actually received a notification letter as of June 2007.

interview 12 months later. On the other hand, if the outcome of the second interview is negative, a financial sanction is imposed and a new, more extensive, contract is proposed by the case worker. The amount of the financial sanction for non-compliance depends on the position in the household. For single households and heads of household, the monthly benefits are reduced to the legal minimum income, which amounted in 2004 to 613 euros for single households and 817 euros for heads of households (SLPPES 2004). For cohabitants, the benefits are completely removed for a period of four months. If the unemployed refuses to sign the contract that she has been proposed, she is excluded from the UB scheme until she completes a full year of employment, which makes her eligible for UB again (ONEM 2004). The exclusion is immediate for cohabitants and after a six-month period of reduction to the legal minimum income for single and heads of households. A third and last interview is then organized 4 months later. Again, if the outcome of the third interview is positive, the procedure starts over from the beginning and the individual is invited to an interview 12 months later. In contrast, if the outcome of this last interview is negative, then the unemployed worker is excluded (immediately or after a six-month transitory period, depending on the household status as described above) from the UB scheme until she completes a full year of employment.

In comparison to other JSM programs that were implemented in European countries, the Belgian program that we study has some features that can be synthetized along three dimensions: (1) the timing of the interviews, (2) the magnitude of the sanctions and (3) the discretionary power of the case workers regarding the evaluation of job seekers. In short, compared to other countries, the Belgian JSM program that we investigate is characterized by long time intervals between the interviews, severe sanctions in case of negative evaluations and a very high discretionary power of the case workers in charge of the evaluations. In order to gain a more precise comparison between different programs that have been evaluated quite recently, Table 1 provides a short summary of their main features.

[Table 1]

The Disability Insurance System

In Belgium, any employed or unemployed worker satisfying some minimum amount of seniority and prior earnings[9] is insured against health shocks that affect her ability to work through the payment of disability benefits whose amount are a fraction of the last monthly earnings. As mentioned earlier, since this article focuses on the potential substitution between UI and DI when Job Search Monitoring schemes are implemented (Lammers et al. 2013, Koning and van Vuuren 2010, Borghans et al. 2014), the Disability Insurance system is explained in more detail in the following paragraphs.

The overall insurance system in case of disability[10] is publicly provided in Belgium at the federal level and depends on (i) the duration of disability and (ii) whether the individual is employed or unemployed[11]. During the first month (14 days) of sickness absence, employees (workers) remain fully paid by their employer[12]. This first stage is called the period of "guaranteed salary". After the period of guaranteed salary, all workers and employees become supported by their health insurance fund (the mutuality[13]). Unemployed individuals, on the other hand, have their unemployment benefits substituted from the first day of illness by their health insurance fund. During the first year of illness, individuals are considered short-term disabled and are covered by a program called "Primary incapacity". If the illness lasts more than a year, the individual enters the long-term disability program. Despite this different designation, both programs are financed by the National Institute for Health and Disability Insurance (NIHDI). In practice, the main

[9] Full-time workers and unemployed workers must have fulfilled a minimum of 180 working days (or active days of job search for the unemployed) during the last twelve months to have access to disability insurance. For part-time workers, the condition is to have worked at least 800 hours over the last 12 months.

[10] Note that disability insurance that we describe in this article differ from the Work Accident and Occupational Disease insurances, which are covered by other institutions.

[11] Note that the self-employed have a distinct sickness insurance program that we don't cover in this section.

[12] This rule differs for workers and employees who are on probation period. Workers on probation period and employees who have been employed for less than a month and are on probation period are not paid by their employer. Employees who have been employed for more than a month but are still on probation period are covered by their employer for the first 14 days of disability.

[13] In Belgium, although the health care system is publicly supported at the national level, the reimbursement of medical expenses and short-term disability benefits are paid by the public health insurance fund called "mutuality", which are funded by the National Institute for Health and Disability Insurance (NIHDI) and act as intermediaries between this Institute and the disabled. In short, to benefit from the Belgian medical coverage, individuals must register at a health insurance fund. The most important health insurance funds in Belgium are the socialist health insurance fund, the Christian health insurance fund, the liberal health insurance fund and the neutral health insurance fund. Beyond their reimbursement role, health insurance funds also have a duty to accompany, inform and defend their members.

differences between the short-term and long-term disability programs lie in (i) the way evaluation of the remaining ability to work is conducted and (ii) the estimation of the replacement rate.

Regarding the evaluation of the remaining ability to work, during a short-term disability period, individuals are examined by a doctor designated by their health insurance fund. To be recognized as "unable to work", applicants must fulfil three criteria. First, the applicant must have stopped all productive activity. Second, she must have stopped this productive activity as a direct consequence of a deterioration of health that is not directly linked to her professional activity[14]. And third, the applicant's ability to work must be reduced by at least 66% with respect to her previous occupation. Note that an important change occurs for the third criterion after 6 months of disability: the reduction in the ability to work is then evaluated with respect to any occupation that the worker could perform given her age, education and experience (instead of her previous occupation). Then, after one year of short-term disability, the sick/disabled worker enters the long-term disability program. In contrast to the short-term program, which is covered by the health insurance fund, the disability program is directly covered by the National Institute for Health and Disability Insurance (NIHDI). In practice, in order to be accepted into the long-term disability program, the applicant's doctor at her health insurance fund (who oversaw the applicant during the short-term period) submits the application to the NIHDI which can either directly approve the doctor's conclusions or run its own internal evaluation.

Regarding the level of the replacement rate, it differs between the short- and the long-term disability programs. During the short-term one, the replacement income amounts to 60% of the last wage payment received before becoming sick/disabled for employees and is equal to the unemployment benefits if the worker was unemployed before. In contrast, during the long-term disability period, the replacement income depends on the last wage payment received (for unemployed workers it is the last wage payment before unemployment) as well as the type and position of the disabled in the household. To be precise, this share is 65% for heads of households, 60% for single households and 40% for cohabitants, with defined floor and ceiling amounts. In

[14] This condition exists to establish a distinction between the short-term disability program and other programs such as the occupational injuries fund and the occupational diseases fund. In practice, there exists a list of diseases for each professional activity that gives access to the occupational diseases fund. As a remark, mental diseases, such as burn outs, and musculoskeletal diseases, such as back pain and tendinitis, are supported by the short-term disability fund.

2018, the minimum and maximum long-term disability benefits were respectively 1,487.4 euros and 2,417.0 euros per month[15].

4. The Data

Description of the Sample

In this paper, we take advantage of the Belgian *Labor Market Data Warehouse* (LMDW) of the national Crossroad for Social Security (CBSS) which aggregates administrative data from many governmental and social security institutions on all Belgian residents since 1998. To create our dataset, we first extract representative samples of the Belgian population for each year between 2003 and 2009 that contain, for each quarter, information on the individuals' employment history and socioeconomic status, as well as for each year, the nationality, year of birth, family status, position in the household, gender and district. We then restrict this sample of close to 1.000.000 individuals to the long-term unemployed, as they were the ones directly targeted by the JSM program. We finally narrow the sample to individuals aged between 46 and 53 years old. We choose this age-group since, between 2006 and 2013 the long-term unemployed aged 50+ were not subject to JSM whereas those under 50 were. This therefore allows us to compare a treatment group (the long-term unemployed aged 46, 47, 48 or 49) with a control group (the long-term unemployed aged 50, 51, 52 or 53). Note that we pick a relatively small age window (8 years) in order to study individuals who are as comparable as possible on the labour market (see section 7 for more details). Finally, although the JSM program started in July 2006, we restrict the period of analysis from June 2007 (2007 q3) to September 2009 (2009 q3)[16] .We decided on this restriction because the program was not applied to all the eligible unemployed workers at first but rather implemented gradually until June 2007. Starting our analysis after that date ensures that all the participant below the age of 50 and with more than 13 months of unemployment are subject to monitoring.

In that context, we should consider that an unemployed worker becomes subject to the monitoring procedure when she receives the first notification letter. Since we don't have information on whether (and when) these letters were sent, the treatment group instead consists

[15] These monthly benefits are calculated on the basis of a minimum allocation of 57,0€/day and a maximum allocation of 92,6€/day for 6 days a week (NIHDI 2018)

[16] This is done because our sample stops in December 2009 and we need to observe transitions into disability (and other states) to construct our outcome variable.

of individual who satisfy two conditions: (1) being under 50 years old and (2) being unemployed for at least 13 months[17]. By doing so, we make sure that all treated individuals are in some stage of the JSM program. The identification of the treatment group has two limitations. First, since we only observe age on an annual basis, some individuals who are about to turn 50 during a specific year will be considered as controls from the beginning of that year onwards, whereas they might be subject to JSM during the first months of that year (before their birthday). Another reason why some individuals in the control group might be treated at the age of 50 is that the monitoring does not end right after an individual turns 50. Indeed, if an individual entered the monitoring program before his/her 50th birthday then the monitoring would continue until the individual has obtained a positive interview even if that individual has turned 50 in the meantime. Second, we do not observe the stage of the monitoring procedure to which the individual is subject, i.e. if an interview has already occurred or not or if an evaluation was positive. We will address these two approximations in identifying the treatment group by implementing several sensitivity and placebo tests in the robustness checks section[18].

As a final note on the sample, because our dataset provides information for the end of each quarter, we do not have information on potential short-term transitions within a given quarter. This means that we are not able to observe whether an unemployed individual has experienced a short-term disability spell within a quarter. Since short-term spells are omitted, the number of transitions to disability will be underestimated, and our findings will present conservative estimates of the impact of job search monitoring on entries into disability.

Descriptive Statistics and Stylized Facts

Table 2 reports descriptive statistics, in the first column, for the unemployed individuals aged between 50 and 53 (i.e. the control group) and, in the second one, for those aged between 46 and 49 (i.e. the treated group). It provides information on the following individual characteristics for the third quarter of 2007 (2007 q3): gender, region, nationality, position in the household, length of unemployment and age in years. As we can see in Table 2, there are more heads of single-parent households among the long-term unemployed aged 46 to 49. Inversely, there are relatively more married couples without children among the oldest age category. A plausible explanation for this

[17] Since we only have data on a quarterly basis, we consider that the treated individuals must have an unemployment duration of at least 6 quarters (15-18 month or more), so that the analysis accounts for potential delays in sending the notification letters.
[18] See section 5 for precise explanations about the robustness analysis.

is that, people aged 46 to 49 more often have dependent children at home than those who are aged between 50 and 53 because the children of the eldest age category are, on average, more likely to already have reached the age to leave the household. Regarding nationality, little difference can be observed, although there appears to be an even larger majority of Belgian individuals among the group aged 50 to 53. Related to this, there are more individuals coming from EU countries or the rest of the world among the younger age category. The existence of differences in proportions between the two groups is not a threat to our identification strategy as long as it doesn't imply a discontinuity around 50. To verify the absence of discontinuity in these proportions at the age of 50, we have decomposed Table 2 by age groups (see Table A. 14 in annex). Hence, we do not observe a sudden jump for any proportion at the age of 50.

[Table 2]

Moving on to the main outcome variable, Panel A of Figure 3 shows the rate of entry from unemployment into disability by quarter from 2004 to 2009 for the treatment group (long-term unemployed aged 46-49 years old) and the control group (long-term unemployed aged 50-53 years old) respectively. We capture this transition rate by constructing a dummy variable equal to 1 when an individual enters disability during the next period (t+1). Figure 3 also displays two vertical lines that delimit the period during which the monitoring scheme was extended gradually to all the unemployed aged between 40 and 50 years old with more than 13 months of unemployment. Thanks to these lines, we can see that the transition rate suddenly increased during the implementation phase of the JSM program, but only for the treated individuals (those aged 46-49). Under the classical assumptions of a difference-in-difference model, one could argue that the increasing gap between the two lines corresponds to the treatment effect of the JSM program. However, since the two groups are different in terms of age, other factors influencing the transition rates from unemployment might have played a simultaneous role. This would correspond to a violation of the parallel trend assumption and bias the estimates of the difference in difference analysis. We therefore opt for a regression discontinuity design to identify the effect of JSM and verify that the descriptive evidence shown in Figure 3 corresponds to a causal effect of JSM on the exit rates into disability.

[Figure 3]

As a point of comparison, we consider two additional potential exits from unemployment after the implementation of the JSM program, i.e. exits towards employment and inactivity.[19] Panel B and C of Figure 3 show the transition rates from unemployment for these two additional outcomes, comparing again, in each case, the treatment group to the control one. In both cases, the two curves seem to diverge during the implementation of the monitoring program. The empirical section will allow us to see more precisely if this graphical evidence could be confirmed and if they differ from the ones observed for the entry into disability.

5. Methodology and Identification Strategy

In order to capture the effect of JSM on entries into disability, we exploit the fact that, between July 2006 and January 2013, the program was targeted only at the unemployed below the age of 50. We use this threshold for treatment (i.e. age) in a regression discontinuity design (RDD) framework, by comparing the entry rates into disability of the unemployed just below and above the cut-off of 50. The RDD method provides a visual and transparent way of estimating a treatment effect in a non-experimental setting and is well-suited to the reform we are analysing. In this context, the two main factors to consider are (i) the definition of the treatment variable and (ii) the estimation method that needs to be used.

Regarding the definition of the treatment variable, we consider that an unemployed worker becomes subject to the JSM procedure when she receives her first notification letter, i.e. after thirteen months of continuous unemployment. As explained in section 4 since we do not observe directly when a notification letter is sent to each individual, we rely on age and unemployment duration to identify a treated individual in our setting. The potential shortcomings of this approach will be addressed by performing a series of robustness checks in section 7.

Regarding the estimation strategy, an issue that is particularly pronounced for RDD is the bias-variance trade-off implied by the choice of an optimal bandwidth and a functional form of the polynomial in the regression equation. In addition, we face another difficulty concerning age. Because age is measured on a yearly basis, we only have a few mass points on each side of the threshold. Since there is no consensus on the optimal estimation method in such cases, most papers generally rely on various specification tests (by changing the bandwidth and the functional form of the polynomial) to assess the robustness of their results. Our research found inspiration

[19] The transition rate into employment (inactivity) is defined as the proportion of the unemployed who switch from unemployment to employment (inactivity) during the considered year-quarter.

in the work of Kostol and Mogstad (2014), who systematically compare two estimates in the main results of a similar RDD framework. Their first estimates (the polynomial approach) are obtained by specifying a linear polynomial form and a bandwidth based on the method of Imbens and Kalyanaraman (2012). Their second estimates follow a first difference approach and are obtained by comparing the two mass points just above and below the cutoff value. The first method is advantageous as it estimates the polynomial form in a more robust way, but at the cost of a higher potential bias. The rationale behind the second method (which is advocated by Cattaneo et al. 2018) is as follows: if the individuals in the two mass points that are the closest to the cut-off are properly randomized between the treatment and the control groups, then the optimal estimation method consists in comparing only these two mass points. Comparing these two estimates (hereafter the polynomial approach and the FD approach) therefore provides a transparent way of assessing the robustness of the results.

Although we systematically present findings according to these approaches for the reasons given above, we consider the polynomial specification as more reliable. The main reason relates to the fact that our treatment variable, age, is only observable on an annual basis (see section4). As a consequence, some individuals who turned 50 during the year the JSM program was implemented are considered in our setting as being part of the control group, whereas they might be subject to JSM during the first months of that year (before their birthday). Extending the age window under consideration allows us to reduce the proportion of treated individuals that could potentially be wrongly included in the control group. Another reason is that the polynomial approach allows us to consider any potential relationship between age and the exit rates from unemployment. Like Kostol and Mogstad (2014), we present our findings with both the polynomial and the first-difference approach as a means of comparison. Hence, we estimate the following regression equation with a linear probability model for the period 2007q3-2009q2:

$$Y_{it} = \gamma + \delta_t + \beta \times \mathbf{1}[\mathrm{a_{it}} < \mathrm{c}] + \mathbf{1}[\mathrm{a_{it}} < \mathrm{c}] \times f_b(a_{it} - c) + \mathbf{1}[\mathrm{a_{it}} \geq \mathrm{c}] \times f_a(a_{it} - c) + \boldsymbol{\delta} \times \boldsymbol{X}_{it} + \varepsilon_{it} \qquad (1)$$

Where i indicates the individual, t is the year-quarter, Y_{it} is the dependent variable (dummy indicating whether or not the individual is disabled at the end of the next quarter), c is the cutoff age (c=50), γ is a constant, δ_t is a year-quarter dummy, $f_b(.)$ and $f_a(.)$ are polynomial functions (of degree one in the benchmark) whose parameters can be different below and above the age

cutoff, and $\mathbf{X}_{it}$ is a vector of individual characteristics[20]. The parameter of interest is therefore β, which captures the effect of being subject to JSM on Y_{it}. In our preferred specification, we chose a bandwidth of 4 years on each side of the cut-off (46-49 and 50-53 years old). Regarding the form of $f_b(.)$ and $f_a(.)$, in the polynomial approach, we choose a first order polynomial and use higher orders in the robustness section as advocated by Lee & Lemieux (2010). In contrast, the first-difference approach does not require to choose a polynomial order since the bandwidth only includes two mass points. Since earlier papers have generally focused either on exits from unemployment or entries into employment, we also include the effect of JSM on these outcomes, as a mean of comparison with these previous studies. In our benchmark results, the dependent variable will be (1) Exits from unemployment, (2) entries into disability, (3) entries into employment and (4) entries into inactivity.

An important assumption for the identification strategy to be valid is that there should be no other policy influencing the observed relationship between the score (here the age) and the outcome (here the transition rates to disability). In this context, we checked for other large-scale institutional changes that could have affected the treated and control groups differently. No trace of such reform was found. We only find minor policy adaptations differentiating between individuals aged under and over 50 but that are not directly related to the transitions from unemployment to disability. This assumption further justifies the choice of a small bandwidth to ensure that our results are not influenced by the existence of other policies.

6. Empirical Findings

Benchmark Findings

Before presenting the results of our estimations, we start with a visual analysis of the relationship between age and the transition rate into disability after the implementation of the JSM program. Figure 4 shows how this rate evolves with age, using a linear fit on both sides of the age threshold (the red vertical line) and a 95% prediction interval. This graph points quite clearly towards the existence of a discontinuity (a sudden drop) in the proportion of long-term unemployed individuals entering into disability at the age of 50. It therefore suggests that JSM has had an impact on the transition rate from unemployment to disability.

[20] This vector contains the following variables: gender, district, position in the household and nationality (see Table 2 for a more detailed description).

[Figure 4]

Table 3 presents the regression results for both the linear trend specification and the FD specification[21]. As discussed in the previous section, we use the polynomial specification to estimate our benchmark results but, following Kostol and Mogstad (2014), we systematically contrast them with the results of the first difference approach. To estimate the proportional effect of the treatment, we report for each regression the estimate of the transition rate to disability, just above the cutoff age, i.e. those with an age of 50. To do this, we run a similar regression from which we exclude all control variables and extract the constant[22]. Dividing the estimated treatment effect by the constant of that alternative regression provides an estimation of the proportional effect of being subject to a JSM program.

We observe that the JSM program is estimated to increase the exit rate from unemployment by 2.32 percentage points (a 48% proportional increase[23]) in the polynomial approach and by 2.41 pp. in the FD approach (a 43% proportional increase). However, this effect is mainly due to a higher inflow into disability rather than a higher inflow into employment. Indeed, we estimate that the JSM program increases the proportion of the long-term unemployed entering into disability by 1.43 percentage points (a 115% increase) in the linear trend approach and by 1.28 pp (a 88% increase) in the FD approach. In other words, while the estimated probability of being disabled in the next quarter for an individual who is not subject to the monitoring program is 1.24%, it is estimated at 2.66% for a similar individual who is subject to the monitoring program. Both effects are statistically significant at the 1% confidence level. Based on the linear trend specification, these findings suggest that the JSM reform caused the number of beneficiaries of disability insurance aged 46-49 to more than double after its implementation. Moreover, taking the ratio of the two effects (effect of entries into disability divided by the effect on exits from unemployment), we observe that 62%[24] of the exits from unemployment within the JSM program move towards disability.

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[21] As a robustness check, we estimated equation (1) without control variables. The results are displayed in Table A.15 and show almost no differences with the benchmark results. This is evidence that the small differences in terms of observable characteristics that we found in the descriptive analysis do not significantly influence our results.

[22] This is because the constant of that regression gives us the expected probability of being disabled in the next quarter, $P(\widehat{Y_{it}} = 1)$, for individuals just above the cutoff age, i.e. those at the age of 50. Strictly speaking, the constant and the treatment effect do not come from the same regression.

[23] 0,0232/0,0486=0,48

[24] 1.43/2.32=0.62.

[Table 3]

Regarding the effect of JSM on the transition rates to employment and inactivity we observe no significant coefficient in the linear trend approach and a very small and only slightly significant coefficient in the FD approach. Since the polynomial approach is our preferred one, the results suggest that the job search monitoring scheme has been inefficient in Belgium in moving the long-term unemployed back into employment. The fact that we observe a slightly significant effect in the FD approach will be tackled in section 7. We will see that our results regarding transitions to employment and inactivity are actually unstable.

If we compare these results to those obtained in the literature, we observe some differences and similarities. First, these results confirm the earlier conclusions of Lammers et al. (2013) and Petrongolo (2009) that imposing stricter access conditions for UB leads to a higher inflow of unemployed into disability. Lammers et al. (2013) find that the probability of being disabled after two years for men (women) aged 57.5-59.5 who are monitored from the start of their unemployment spell is 8.4 (11.7) percent, against 4.1 (2.5) for similar men (women) who are not monitored. Lammers et al. (2013), however, do not find any significant estimates for people that have been unemployed for more than 9 months Our results are therefore different from theirs in the sense that we obtain a significant effect on the transition rate to disability even for long term unemployed individuals. Second, the fact that we obtain no significant treatment effect on the rate of transition to employment echoes the conclusions of Cockx et al. (2018) but contrasts with most other papers in this literature (Arni et al. 2013, Manning 2008, Petrongolo 2009, Van den Berg 2004), who found a positive effect of monitoring and sanctions on the entry rate into employment. There are different possible explanations for this difference. First, the unemployment duration of the individuals in our sample is substantially higher compared to previous studies[25]. This can be an indication that JSM is less efficient for very long-term unemployed workers who possess very weak prospects on the labor market. This explanation is in line with Card et al. (2018), who find in a meta-analysis of the ALMP studies, that the average effect of threats and sanctions on the rate of return to employment is not significantly different from zero for the long term unemployed. Another explanation refers to the peculiar features of the Belgian JSM program. There are three main differences between the Belgian JSM program and other similar programs in other countries:

[25] In Arni et al. (2013), the median unemployment duration is 5 months, while unemployment duration is longer in Van den Berg et al. (2004), it is censored at 2.5 years. In Lammers et al. (2013), unemployment duration is censored at 25 months. In contrast, Table 2 shows that about 60% of our sample has an unemployment duration higher than 16 quarters (4 years).

first, Belgian officials conducted the interviews late and infrequently during the unemployment spell; second, the sanctions are more severe compared to most other countries and finally the evaluators within the Belgian system possess a high degree of discretionary power (Cockx et al. 2018).

Heterogeneous Effects

In this section, we analyse how the impact of JSM on entering into disability could vary with the local unemployment rates, gender, household status and the length of the disability spell. This heterogeneity analysis will draw insights on the theoretical mechanisms that we described in the literature review. Moreover, looking at the length of the disability spells is important to assess whether the JSM has had long term consequences on the affected individuals and thus draw some conclusions about the costs of the program in term of lost labour force.

Effects across Local Unemployment Rates

In order to draw policy recommendations, it is necessary to see whether the benchmark results vary across districts with different local unemployment rates. Although finding an effect of local unemployment rate on the treatment effect can occur for various reasons[26], answering this question could be interesting for policy makers who would like to set up monitoring programs in specific areas. So far, the literature has found mixed results on this point. Using a social experiment in the Netherlands, Van den Berg & Van der Klaaw (2006) have shown that the effectiveness of monitoring and counselling varies negatively with the business cycle and labour market prospects of the unemployed individuals. The reason is that unemployed individuals with good labour market prospects rely more on informal search channels. Monitoring the search of these individuals therefore only shifts job searching from informal to formal channels, leaving the job-finding rate unchanged. In contrast, Lammers et al. (2013) find no evidence that the effect of JSM on the job finding rate varies according to the educational level, which is a good proxy for individual labour market prospects. The impact of labour market prospects on the efficiency of monitoring may itself depend on how stringent entry into the disability program is. First, since individual labour market prospects are positively correlated with health, the effect described in

[26] It could be related to the local economic conditions or to differences in observed or unobserved characteristics for the unemployed across districts.

Van den Berg & Van der Klaaw (2006) may be counterbalanced by the fact that JSM will trigger a higher transition rate towards disability for individuals with low labour market prospects. Second, De Brouwer & Tojerow (2019) have shown that a high proportion of the geographic differences in the transition rate to disability remain unexplained even after controlling for a wide set of individual characteristics, which makes plausible the possibility that the stringency of the access to disability varies across districts and is especially low in high-unemployment districts. Therefore, JSM would have a higher effect on the transition rate to disability in this type of districts.

To explore the heterogeneity of our results according to the local unemployment rate, we divide our sample in four groups of a similar size. In order to construct them, local unemployment rates were computed for each district in our sample and divided into four quartiles[27]. While the national mean is 8.3 percent, the 1st, 2nd, 3rd and 4th quartile groups have unemployment rates of 6.2, 13.2, 16.3 and 18.5 percent, respectively.

[Table 4]

Table 4 shows the effect of JSM on entries into disability when estimating equation (1) separately for individuals living in the four types of districts using the linear trend specification. It shows that the effect of JSM on entries into disability varies between 0.81 pp. and 2.02 pp. depending on the local unemployment rate. It represents an increase of 86% in the entry rate for individuals who live in a district characterized by the lowest unemployment rate and an increase of 239% for the ones living in districts with the highest unemployment rates. This observation indicates that the long-term unemployed workers subject to monitoring and living in districts with relatively less employment opportunities are more likely to exit unemployment in favour of disability. These findings should be considered with care, however, since F-tests do not allow us to conclude the existence of significant differences between estimates.

Effects by Gender

Table 5 displays the estimates of heterogeneous effects by gender, using the linear trend specification and the first-difference approach. It shows that JSM has had a particularly strong impact on women. The estimated effect is of 2.22 percentage points for women when the linear

[27] The unemployment rate we use for each district is the mean unemployment rate in each district for the years 2007-2009. We used aggregated administrative data available online on the CBSS Datawarehouse website.

trend specification is used (nearly twice as large as the estimates we find when analysing both genders together) and is statistically significant at a 1% confidence level. The estimate found when using the first difference specification is only slightly lower, with an effect of 2.03 percentage points (also significant at the 1% confidence level). In contrast, none of these effects are significant for the population of men. The size of the effect in the linear trend specification corresponds to a 195% increase in the number of female beneficiaries of disability benefits aged 46 to 49 following the implementation of the reform. Interestingly, this result is in line with the fact that the increase of the disability rate in Belgium during the same period was mainly driven by an increase in women's disability rate (De Brouwer & Tojerow 2019). Although previous studies (Lammers et al. 2013, Borghans et al. 2014) have found that JSM schemes had a higher effect on transitions to employment and disability for women, no clear explanation has been identified yet[28]. At least two potential explanations for such gender differences could be put forward. First, women may have a lower employability than men, thus increasing the relative attractiveness of the disability insurance when a JSM scheme is implemented (Azmat et al. 2006). Second, if average health is lower for women than for men, implementing a JSM scheme could lead to a reduced cost of applying for the disability insurance relative to staying unemployed, thus leading to a higher transition rate from unemployment to disability for women. For example, according to the Belgian Health Survey, women are significantly more subject to depression than men (with a depression rate for 8.3% for women against 4.9% for men in 2013). Knowing that the recent increase in disability rates in Belgium was mainly driven by mental disorders, the differences in health between male and female unemployed workers could constitute an explanation for our result.

[Table 5]

Based on these striking differences across gender, we decided to further decompose our results by household type (i.e. single-, married- and cohabitant households' categories) to observe across each gender which category is more affected. Table 6 reports the results of these estimations. For men, regardless of the type of household, the results don't change and are not significant. For women, we can clearly see that the treatment effect is concentrated within the category of single-women households (+3.4 pp.) and to a lower extent for married-women households (+1.3 pp.). We conclude that the gender effect that we initially observed could be the result of a mix between gender and the household situation of each individual. This result could highlight the fact that

[28] Regarding the transition rate to disability, Lammers et al. (2013) obtain a treatment effect that is twice higher for women.

single households have much lower possibilities to smooth consumption when facing negative shocks in their income. It could therefore explain why the incentive to switch from unemployment to disability is much higher for single households that face the risk of losing part of their benefits due to the JSM program. Second, some of these households also have to care for children, which makes the cost of searching for jobs and working considerably higher.

[Table 6]

Effects by Health History:

From the theoretical discussion of section 2, we would expect that individuals with more fragile health would be more likely to enter into disability due to the monitoring program. First, individuals with more fragile health have less employment opportunities and a higher cost for job searches. Second, individuals with more fragile health obviously face a lower probability that their application to DI is rejected. To see whether this prediction is valid in our case, we need to construct a credible proxy for individual health. The best proxy for health would be constructed by using individual information on medical expenditures. Since we don't have access to such data, we instead use the number of sick days in the recent past as a proxy for present health. More precisely, we run separate regressions according to the number of sick days that each individual took between 2000 and 2005. Since we want to compare individuals that only differ according to the health variable, we consider only two groups of individuals. The first group had no registered sick days between 2000 and 2006, while the second group had between 1 and 100 registered sick days. Although there could remain differences between these two groups that are due to unobserved characteristics, we expect that these differences should be minimal when considering groups that do not strongly differ regarding their number of sick days in the past.

[Table 7]

The results of this exercise are shown in Table 7. Since estimates are very different across gender, we report our results for three groups: (1) men-women, (2) women and (3) men. Columns 1-4 of Table 7 indicate an important difference between the estimated effects regarding the group with 0 sick days and the group with 1-100 sick days when considering the polynomial approach. However, using the FD approach gives more mixed results for the group with 1-100 sick days. The results are much more robust when we disaggregate our results by gender. In line with the previous results, our results are only significant for the group of women. Within the group of women, the effect of JSM is 1.51 pp (a 165% proportional increase) for individuals with 0 sick

days and 4.65 pp. (a 404% proportional increase) for individuals with 1-100 sick days. This important difference between the two estimated effects therefore supports the theoretical prediction described above.

Effects by Length of Disability and Gender

Finally, we investigate whether the entries into disability persist in the long run for the unemployed subject to monitoring. More specifically, we distinguish between individuals who enter into a disability spell that lasts less than a year from those who enter a spell that lasts at least one year. From Columns 1-4 of Table 8, we observe that the treatment effect is highly significant for short-term disability spells and only weakly significant for longer disability spells. However, after distinguishing our results by gender, we observe that the effects are only significant for women, for both long and short-term disability spells in both the linear trend and first-difference approach. For the group of women, when looking at the linear trend specification, the proportional effect is only slightly higher for short-term disability spells (199%) than for the long-term spells (171%). This result indicates that the JSM program has increased transitions from unemployment to disability relatively homogeneously across durations of disability spells. This result suggests that the consequences of the JSM program last in the long run. This durable retreat from the labour market implies even lower re-employment perspectives for the affected workers and increased social security costs.

[Table 8]

7. Robustness Checks

The objective of this section is to verify that we identify the effect of the JSM program on entries into disability, by checking that our benchmark estimates are robust to different specifications and falsification tests. We first test the sensitivity of our estimates to the model specification (using various polynomial degrees) and the age window under consideration. We then test that excluding the individuals aged exactly 50 does not change our main results. Finally, we perform a series of placebo tests at other age cut-offs, for the short-term unemployed that are not subject to the JSM and for pre-existing discontinuities in transition rates at the age of 50.

Sensitivity Analysis: Model Specification and Bandwidth Choice

We first test the sensitivity of our results to (i) other model specifications and (ii) different bandwidths around the age cut-off. The sensitivity to misspecification is tested because, as outlined in Lee and Lemieux (2010) "*in the case of RD designs [...] misspecification of the functional form typically generates a bias in the treatment effect* τ". Sensitivity to the bandwidth choice is tested because the bandwidth should be chosen in a way that allows us to present results that are as informative (precise) as possible but, at the same time, that minimize the bias implied by misspecification of the functional form close to the cut-off. This echoes the typical "bias-variance trade-off" implied by the choice of bandwidth in discontinuity designs (Cattaneo et al. 2018) as explained in section 5. The benchmark estimates are based on a linear polynomial and a short age interval (46-53 years old) to study individuals that are as comparable as possible, besides the treatment effect, while being big enough to obtain statistically significant results. We explore the sensitivity of our findings to the use of different polynomial specifications (quadratic and cubic) than the linear one and to the use of different age intervals (40-59, 42-57, 44-55, 49-50) than the one used in the benchmark analysis (46-53). The results of these sensitivity tests are shown in Table 9. They indicate that the estimated increase in transitions from unemployment to disability following the implementation of the JSM scheme ranges from 1.14 to 1.6 percentage points across the different specifications. In line with our benchmark finding, they suggest that our main conclusion regarding the role of JSM in increasing the entry into disability is quite stable through various specifications and bandwidths.

[Table 9]

We perform the same sensitivity analysis for transitions from unemployment to employment and inactivity. The results of these sensitivity tests are presented in Appendix 2 (Table A.15 and Table A.16). In both cases, the effects do not seem robust across specifications reinforcing our findings that the reform didn't seem to have a significant effect on transitions from unemployment toward employment or inactivity.

Discontinuity Excluding the Unemployed Workers aged 50

In this section, we provide a robustness test in order to deal with the imprecision of the treatment variable discussed in Section 4. More precisely, the treatment effect could be downward biased because some individuals turn 50 in the current of the year and are therefore treated for part of the year, although they are considered as controls for the whole year. As a robustness check, we

therefore estimate equation (1) by dropping all individuals aged 50. We compare individuals aged 46-49 to those aged 51-54 in the linear trend specification and individuals aged 49 to those aged 51 in the FD specification. Table 10 shows the results of this analysis. We observe that all results are of a high magnitude and are almost all significant at the 1% level. Regarding transitions to disability, the point estimates are higher than the benchmark results (1.63 pp. instead of 1.43 pp. in the linear trend specification and 1.87 pp. instead of 1.28 pp. in the FD specification).

[Table 10]

The most important change is related to the effect of JSM on transitions rates to employment and inactivity. Contrary to our benchmark results, we now observe significant increases in both cases of 0.97 pp. and of 0.92 pp. respectively. There are two possible explanations to such changes in the results for transitions to employment and inactivity. First, it could be that wrongly assigning some individuals to the control group (while they are actually treated for a part of the year) induces a downward bias in estimates of the effect of JSM on exits to employment and inactivity. If this is true, our benchmark estimates are downwardly biased. Another explanation is that removing individuals who are aged exactly 50 leads to a comparison of two groups who are too far in terms of age, thus biasing the results upward. In this case, it is this sensitivity test that is biased upwardly. We are unfortunately not able at this stage to determine which one of these explanations causes the difference in the observed findings.

We conclude that these results should constitute an upper bound for the effect of JSM on the different types of exits. Considering only the linear trend specification, this implies that the effect of JSM on transition rates to disability lies between 1.43 and 1.63 pp. Regarding its effect on employment and inactivity, results are more difficult to interpret. The effect lies between a non-significant effect of 0.55 pp. and a significant effect of 0.97 pp. for employment and between a non-significant effect of 0.36 pp. and a significant effect of 0.92 pp for inactivity.

Next, in order to verify that the estimated effects found previously are indeed directly related to the effect of job search monitoring, we perform several placebo tests. Hence, we test that no other similar discontinuities can be found (i) when considering alternative age thresholds, (ii) for the short-term unemployed aged 46-53 and (iii) before the reform was implemented. The three following sections present the results of these placebo tests.

Placebo Test n°1: Discontinuity at other Age Cut-offs

In this section, we provide a placebo test in which we estimate the treatment effect on the dependent variables at alternative cut-offs. The idea behind this test is that the true cut-off should be the only one for which we observe a significant effect. Figure 5 shows the results of estimating the FD model for alternative age thresholds. Regarding disability, we observe significant effects at both 50 and 51 years old, while all other coefficients are not significant. As explained earlier, the fact that the FD model shows a significant coefficient for the age of 51 could be due to the fact that some unemployed who will turn 50 in a specific year are still subject to JSM for the remaining time before their birthday. As we can see, this effect disappears when we take 52 or 53 years old as the cut-off age. This is evidence that the linear trend approach is more reliable, since it decreases the proportion of individuals who are considered as controls while they are still subject to JSM.

[Figure 5]

Regarding transitions to employment and inactivity, we observe that many placebo age thresholds yield significant effects. These significant effects are probably due to the fact that transitions to employment and inactivity are more responsive to age than transitions to disability. Therefore, differences in age of only one year can lead to significant differences between these transition rates. The fact that the cut-off age of 50 does not show higher point estimates than the other placebo ages is evidence that the slightly positive effect we found in our benchmark results when using the first-difference approach should be associated with the fact of "getting older" instead of being interpreted as the effect of JSM.

Placebo Test n°2: Discontinuity for the Short-Term Unemployed

The results of the placebo test on short-term unemployed are displayed in Table 11 and illustrated graphically in Figure 6. As shown inTable 11, no significant treatment effect is found for the short-term unemployed for transitions to disability and inactivity. Regarding transitions to employment, we find a slightly significant placebo effect. This result could be due to the fact that turning 50 might constitute a small but significant signal of lesser productivity for employers, who become more reluctant to hire older unemployed workers. This is evidence that the small effect found in the benchmark results for the exit rate to employment when using the FD approach cannot be attributed to the causal effect of the JSM program. Note also that the very high transition rates to inactivity at the age of 58 are due to the fact that, during the period of analysis, unemployed workers could ask for an exemption of registration as a jobseeker at the age of 58,

which allowed them to keep their unemployment benefits without being considered as (and expected to) actively searching for a job.

[Table 11]

[Figure 6]

Placebo Test n°3: Pre-existing Discontinuities at Age 50

In this section, we provide a test for pre-existing discontinuities, i.e. discontinuities in the exit rates from unemployment at 50 years old before the effective implementation of the JSM program. In fact, although we didn't find any legislation applying to the long-term unemployed at 50 years old before July 2006, the fact of turning 50 years old may imply some changes in the behaviour of job searchers. One could for example stipulate that the mere fact of turning 50 has a direct psychological effect that influences the unemployed worker's probability of entering disability. Another explanation could be that turning 50 constitutes a negative signal for potential employers who discriminate against older workers.

[Table 12]

Table 12 shows the results of estimating equation (1) before the implementation of the JSM program (2004q2-2006q2) while Figure 7 illustrates the results graphically. We observe a slightly significant pre-reform effect for the entry rate into disability in the linear trend approach and a more significant effect in the FD approach. However, both estimates are much lower than those in the benchmark results. In Figure 7, we can see that there is a higher variability in the exit rate to disability just before the cut-off. Although the magnitude of the estimates is small, this could constitute a potential bias for our benchmark results.

[Figure 7]

To handle this potential threat, we implement a "Before-during-After RDD" approach proposed by Lalive (2008), which allows to take into account possible pre-existing discontinuities at the cut-off. More precisely, the idea is that, instead of estimating a discontinuity at the cut-off after the effective implementation of the reform, we estimate the change of that discontinuity between the "pre-reform" period and the "post-reform" period. The assumption is thus that the change in the discontinuity in the exit rates from unemployment is exclusively the result of the implementation of the JSM program. In our case, since there are mainly three important periods ("pre-reform",

"during-reform" and "post-reform"), we estimate the change in the discontinuity between the pre-reform period and (1) the "during-reform" period, (2) the "post-reform" period.

The equation that we estimate is the following:

$$Y_{it} = \gamma + \sum_{s=1}^{3} \mathbf{1}[t \in P_s] \times \{\beta_s \times \mathbf{1}[\mathrm{a_{it}} < \mathrm{c}] + \mathbf{1}[\mathrm{a_{it}} < \mathrm{c}] \times f_{bs}(a_{it} - c) + \mathbf{1}[\mathrm{a_{it}} \geq \mathrm{c}] \times f_{as}(a_{it} - c)\} + \boldsymbol{\delta} \times \boldsymbol{X}_{it} + \varepsilon_{it} \quad (2)$$

Where $P_1 = [2004q2 - 2006q2]$, $P_2 = [2006q3 - 2007q2]$ and $P_3 = [2007q3 - 2009q3]$. Using this interacted version of equation (1) allows us to estimate separate effects for each period s (Lalive 2008). The treatment effect is now β_3, which estimates the change in the discontinuity effect between the first period (2004q2-2006q2) and the third period (2007q3-2009q3). Moreover, the FD specification here corresponds to a standard difference-in-difference specification in which the treatment group is the group of unemployed aged 49, while the control group is the group of unemployed aged 50.

[Table 13]

Table 13 summarizes the results when the estimations are made using equation (2). Overall, the findings for transitions to disability are not too affected when this alternative specification is used. Relative to the effect of JSM on exits from unemployment, the estimated effect is even higher than in the benchmark results. Regarding entries into disability, the linear trend approach shows a slightly significant effect for the pre-treatment effect (as previously observed) and a strong effect on the transition rate to disability after the reform. The estimated coefficient is actually even higher in this alternative specification (+1,53 pp. or a 148% increase). Note that the difference of estimated coefficients between the linear trend and the FD approach is higher than for the benchmark results. Although both model clearly show evidence of a positive effect of the JSM program on transitions to disability, one should be aware that the point estimate of the magnitude of this effect lies somewhere between 0.92 pp. and 1.53 pp. (between 78% and 148%). Interestingly, the post-reform treatment effect on transitions to employment now becomes highly significant and of an important magnitude (+1,39% or a 77% increase). However, this result is not robust since it becomes non-significant when using the FD approach.

8. Conclusions

Over recent years, the growing disability rates in some developed countries, coupled with an over-representation of unemployed workers among the groups of new beneficiaries of disability insurance, have brought up questions as to the reasons behind these trends. Our paper investigates whether introducing a job search monitoring scheme for the unemployed generates a shift from unemployment to disability insurance, using an extensive administrative database that is representative of the Belgian population. In more general terms, our aim is to test whether individuals operate some sort of substitution between social support programs when changes are implemented in one of them. We do this following the observation that, in Belgium, the moment at which the share of unemployed individuals aged under 50 started to grow among the group of new disability beneficiaries corresponded with the introduction of a JSM scheme for those same individuals.

In turn, we exploit the fact that the JSM reform was implemented gradually by age, which allows us to replicate the conditions of a natural experiment and compare a treated group (i.e. the long-term unemployed under 50) with a control group (i.e. the long-term unemployed above 50). We use a regression discontinuity framework based on a linear trend approach to estimate the effect of JSM on entries into disability within an age window 46 to 53 years old, and also show the results using a first-difference approach as a means of comparison. Compared to previous studies in this field, our paper offers new insights to the impact of JSM programs on the labour market by focusing on direct transition rates to disability for prime-aged population and by breaking down the impact of the different stages of the monitoring process.

Our findings suggest that implementing a monitoring scheme of job search efforts induced an increase in proportions of entries into disability of 1,43 percentage points. In terms of magnitude, this means that the job search monitoring scheme led the number of beneficiaries of disability insurance to more than double (increase of 115%) after its implementation. To understand the mechanisms driving this effect, we ran several heterogeneity tests, in order to see which groups of individuals were more affected by the monitoring program. As described by job search theory, we would expect that individuals with lower employment prospects, lower economic resources and below average health would have more incentives to enter into disability due to the JSM program. We find evidence for such mechanisms. First, we found a higher point estimate for high unemployment districts, although this estimate is not significantly different from the other estimated effects. This observation could indicate that the unemployed living in a district with

relatively fewer employment opportunities are more likely to exit unemployment in favour of disability. Second, we find that the impact is nearly twice as large for female cohorts, with a local treatment effect of 2,22 percentage points, corresponding to an increase of 195% of the number of female disability insurance beneficiaries. Although this fact had been pointed out in some studies (Lammers et al., Borghans et al. 2014), the sharp difference between the estimates for men and women is new to the literature. Moreover, when further decomposing the effect by gender and household category, we find that almost all the effect that we initially observed is driven by single household women, for which the estimated effect is 3.39 pp (a 243% proportional increase). Single household women are a particular group since they are very exposed to negative income shocks (due to the absence of partner) and have much higher costs related to job search and balancing work with children. From a policy perspective, this result shows that taking the family situation of individuals into account should be an important factor in the design of JSM programs. In order to test whether individuals with lower average health were more affected by the program, we ran separate regressions according to the number of sick days each individual has had in the recent past. We indeed find that the effect of JSM increases with the number of past sick days. Finally, for the group of women, we find that the proportional effect of JSM is only slightly higher for short-term disability spells[29] (+1.69pp or a 199% increase) than for long-term spells[30] (0.53pp or a 171% increase), thus pointing towards a relatively homogeneous effect across durations of disability spells. The various validity tests performed yield similar conclusions, which indicate the reliability of these different results.

Overall, our results support the hypothesis that social support programs can be, to some extent, substitutable, thus reinforcing the argument that has already been made in past research (Burkhauser et al. 2014) that disability policy is part of a broader safety net (constituted namely of short-term and long-term disability insurance, unemployment insurance, social aid, early retirement and pensions) whose components cannot be considered independently. In other words, reforms in one branch of social security will often impact other branches. Such spillover effects should therefore be taken into account when implementing a reform in order to ensure that the policy will yield the targeted results (return to employment of the long-term unemployed in the case of this paper), instead of a mere shift from one social program to the other. Note that

[29] Spells lasting less than a year.

future research in this area could distinguish between different kinds of disability in order to assess whether some forms of disability are more subject to substitution than others.

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Table 1: Description of Other JSM Programs that Have been Studied in the Literature

Article	Country	Start of the program	Timing of interviews	Maximum sanction	Effect on transitions to employment
Arni et al. (2013)	Switzerland	At the start of the unemployment spell	Every month	100% reduction during 60 work days	Positive effect on exits to employment but negative long term effect on earnings.
Cockx and Dejemeppe (2012)	Belgium	After 13 months of unemployment	Every 4 to 16 months	Exclusion from Unemployment Insurance	Positive effect on exits to employment 8 month after reception of the notification letter.
Lammers et al. (2013)	Netherlands	At the start of the unemployment spell	Every 4-6 weeks	30 percentage point cut in the benefit level during 16 consecutive weeks	Positive effect on exits to employment but also on exits to disability.
Petrongolo (2009)	England	At the start of the unemployment spell	Every two weeks	Not defined	Negative effect on the probability of having positive earnings after 1 year and positive effect of transitions to disability.
Van den Berg & Vikström (2014)	Sweden	At the start of the unemployment spell	Not defined	Exclusion from unemployment until reemployment	Positive ex-post sanction effect on transitions to part time jobs and jobs with a lower occupational level.

Table 2: Descriptive Statistics of Unemployed by Age Group

	Control group (unemployed aged 50-53)	Treatment group (unemployed aged 46-49)
Women	58.9%	57.1%
Average Age	51.7	47.6
	(1.10)	(1.10)
Region:		
Brussels	14.2%	17.2%
Flanders	36.9%	26.4%
Wallonia	48.9%	56.4%
Nationality:		
Belgian	88.7%	87.7%
EU28	9.5%	9.8%
Other	1.8%	2.5%
Position in the Household:		
Child	4.2%	5.1%
Single with Children	12.9%	21.7%
Single without children	30.1%	28.9%
Married with children	27.0%	28.0%
Married without children	18.5%	8.2%
Cohabitant with children	1.7%	3.3%
Cohabitant without children	3.2%	2.6%
Other	2.4%	2.2%
Length of Unemployment Spell:		
6-10 quarters	18.4%	21.5%
11-15 quarters	21.2%	19.3%
16 + quarters	60.4%	59.2%
Transition rate into Disability	1.4%	3.3%
Number of Individuals	3,482	2,319

Notes: This table displays descriptive statistics for the year-quarter 2007q3 for the samples used in the paper. All percentages represent the share of individuals in the sample that present the considered characteristic. Other statistics are averages taken over all individuals in the sample. Standard deviations are in parentheses when relevant. To create the samples, we first extract representative samples of the Belgian population for each year between 2OO3 and 2009 and merge them. We then restrict this sample to the long-term unemployed (i.e. those unemployed for 6 quarters or more), as they were the ones directly targeted by the JSM program. We finally narrow the sample to the year-quarter during which the individuals are aged between 46 and 53 years old. The left column displays statistics for the group of long-term unemployed aged 50-53 (the control group) who are not subject to the JSM program while the right column displays statistics for the group of long-term unemployed aged 46-49 (the treatment group) who are subject to the JSM program. The variable "Region" is based on the region where the individual has her legal residence. The variable "Position in the Household" is based on information from the Belgian national register. The variable "Length of Unemployment Spell" breaks down the sample into three categories according to the number of quarters the individuals have been unemployed in 2007q3 (6-10 quarters, 11-15 quarters, or more than 16 quarters). The variable "Transition rate into Disability" displays the share of individuals who enter into either short or long-term disability between 2007q3 and 2007q4.

Table 3: The Effect of Job Search Monitoring on the Proportion of Entry into Disability, Employment and Inactivity

	Exits from unemployment		Disability		Employment		Inactivity	
VARIABLES	Linear trend	FD	Linear trend	FD	Linear trend	FD	Linear trend	FD
Treatment Effect	0.0232***	0.0241***	0.0142***	0.0128***	0.0054	0.0058*	0.0036	0.0054*
	(0.006)	(0.005)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)
Constant	0.0486***	0.0565***	0.0124***	0.0145***	0.0206***	0.0231***	0.0156***	0.0190***
	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)
Observations	43,394	9,660	43,394	9,660	43,394	9,660	43,394	9,660
Number of individuals	8,435	2,739	8,435	2,739	8,435	2,739	8,435	2,739

Notes: This table shows the estimated effect of the JSM program on the long-term unemployed between October 2007 (2007q3) and September 2009 (2009q3), captured by the transitions per quarter from unemployment to (1) Exits from unemployment, (2) disability, (3) employment and (4) inactivity after the reform was implemented. As discussed in section 5, we provide, for each type of transition, an estimation of the effect using a polynomial specification (liner trend) and a First-Difference specification (FD approach). The control variables included in these specifications are: gender (2 dummies), district (43 dummies), position in the household (8 dummies) and nationality (10 dummies). Standard errors are clustered at the individual level. To estimate the proportional effect of the treatment, we report for each regression the estimate of the transition rate to disability, employment or inactivity for individuals just above the cutoff age, i.e. those aged 50. To do this, we run a similar regression from which we exclude all control variables and extract the constant of it. Dividing the estimated treatment effect by the constant of that alternative regression provides an estimation of the proportional effect of being subject to the JSM program. This table shows that the JSM program has increased the exit rate from unemployment by 2.32 percentage points (a 48% proportional increase in the polynomial approach). However, most of this effect is due to an increase in the transition rate to disability (+ 1.42 pp. or a 115% proportional increase in the polynomial approach and a 1.28 pp, a 88% increase, in the FD approach) and not to a higher transition rate to employment. Indeed, regarding the effect of JSM on the transition rates to employment and inactivity we observe no significant coefficient in the linear trend approach and only a slightly significant coefficient in the FD approach. The sample used to perform these estimations is based on representative samples of the Belgian population for each year between 2003 and 2009. We restricted this sample to the long-term unemployed (i.e. those unemployed for 6 quarters or more), as they were the ones directly targeted by the JSM program. We finally narrowed the sample to individuals aged between 46 and 53 years old before conducting our estimations. *** p<0.01, ** p<0.05, * p<0.1

Table 4: Heterogeneous Treatment Effects Depending on Local Unemployment Rate

	1st Quartile		2nd Quartile		3rd Quartile		4rth Quartile	
VARIABLES	Linear trend	FD	Linear trend	FD	Linear trend	FD	Linear trend	FD
Treatment Effect	0.0139**	0.0177***	0.00812	0.0107*	0.0120	0.00579	0.0202***	0.0141***
	(0.00710)	(0.00624)	(0.00680)	(0.00614)	(0.00747)	(0.00691)	(0.00540)	(0.00489)
Constant	0.0161***	0.0180***	0.0130***	0.0154***	0.0133***	0.0148***	0.00844***	0.0105***
	(0.00264)	(0.00352)	(0.00261)	(0.00351)	(0.00320)	(0.00428)	(0.00190)	(0.00254)
Observations	13,063	2,705	9,911	2,353	7,140	1,527	13,280	3,075
Number of individuals	2847	832	1916	646	1310	431	2499	841

Notes: This table shows the estimated effect of the JSM program on the long-term unemployed by local unemployment rate between October 2007 (2007q3) and September 2009 (2009q3), captured by the transitions per quarter from unemployment to disability, after the reform was implemented. As discussed in section 5, we provide, for each type of transition, an estimation of the effect using a polynomial specification (liner trend) and a First-Difference specification (FD approach). The control variables included in these specifications are: gender (2 positions), district (43 positions), position in the household (8 positions) and nationality (10 positions). Standard errors are clustered at the individual level. To estimate the proportional effect of the treatment, we report for each regression the estimate of the transition rate to disability, employment or inactivity for individuals just above the cutoff age, i.e. those aged 50. To do this, we run a similar regression from which we exclude all control variables and extract the constant of it. Dividing the estimated treatment effect by the constant of that alternative regression provides an estimation of the proportional effect of being subject to the JSM program. This table shows that only the estimates in the first and the 4rth quartile of the local unemployment rate variable are significant. Although the point estimates are higher in the 4rth quartile group (the high unemployment districts), it is difficult to make any clear cut conclusion from it. The sample used to perform these estimations is based on representative samples of the Belgian population for each year between 2003 and 2009. We restricted this sample to the long-term unemployed (i.e. those unemployed for 6 quarters or more), as they were the ones directly targeted by the JSM program. We finally narrowed the sample to individuals aged between 46 and 53 years old before conducting our estimations. The unemployment rate we use for each district is a mean unemployment rate in the district for the years 2007-2009. We compute this using aggregate administrative data available online on the CBSS Datawarehouse website. The groups were constructed in order to keep the size of the groups as similar as possible. The 1st, 2nd, 3rd and 4th quartile groups have unemployment rates of respectively: 2.8-6.2 percent, 6.2-13.2 percent, 13.2-16.3 percent and 16.3-18.5 percent. *** p<0.01, ** p<0.05, * p<0.1

Table 5: Heterogeneous Treatment Effects by Gender

	Women		Men	
VARIABLES	Linear trend	FD	Linear trend	FD
Treatment effect	0.0222***	0.0203***	0.00437	0.00339
	(0.0048)	(0.0042)	(0.0044)	(0.0041)
Constant	0.0116***	0.0136***	0.0136***	0.0156***
	(0.0017)	(0.0022)	(0.0019)	(0.0026)
Observations	24,492	5,340	18,902	4,320
Number of individuals	4,812	1,532	3,623	1,207

Notes: This table shows the estimated effect of the JSM program by gender on the long-term unemployed between October 2007 (2007q3) and September 2009 (2009q3), captured by the transitions per quarter from unemployment to disability, after the reform was implemented. As discussed in section 5, we provide, for each type of transition, an estimation of the effect using a polynomial specification (liner trend) and a First-Difference specification (FD approach). The control variables included in these specifications are: district (43 dummies), position in the household (8 dummies) and nationality (10 dummies). Standard errors are clustered at the individual level. To estimate the proportional effect of the treatment, we report for each regression the estimate of the transition rate to disability, employment or inactivity for individuals just above the cutoff age, i.e. those aged 50. To do this, we run a similar regression from which we exclude all control variables and extract the constant of it. Dividing the estimated treatment effect by the constant of that alternative regression provides an estimation of the proportional effect of being subject to the JSM program. This table shows that the effect of JSM is large and significant for women whereas there appears to be no significant effect on men. The sample used to perform these estimations is based on representative samples of the Belgian population for each year between 2003 and 2009. We restricted this sample to the long-term unemployed (i.e. those unemployed for 6 quarters or more), as they were the ones directly targeted by the JSM program. We finally narrowed the sample to individuals aged between 46 and 53 years old before conducting our estimations. *** p<0.01, ** p<0.05, * p<0.1

Table 6: Heterogeneous Effects across Gender and Household Category

	Women						Men					
	Single Household		Married Household		Cohabitant		Single Household		Married Household		Cohabitant	
VARIABLES	Linear Trend	FD	Linear trend	FD	Linear trend	FD	Linear trend	FD	Linear trend	FD	Linear trend	FD
Treatment Effect	0.0339***	0.0227***	0.0128*	0.0173***	-0.00224	-0.00972	0.00993	0.00609	0.00233	0.000920	-0.0118	-0.00417
	(0.00761)	(0.00688)	(0.00704)	(0.00588)	(0.0162)	(0.0121)	(0.00698)	(0.00651)	(0.00704)	(0.00646)	(0.0153)	(0.0155)
Constant	0.0139***	0.0185***	0.0106***	0.00980***	0.00849	0.0126	0.0125***	0.0158***	0.0151***	0.0155***	0.0160*	0.0241*
	(0.00276)	(0.00369)	(0.00229)	(0.00282)	(0.00620)	(0.00896)	(0.00289)	(0.00408)	(0.00320)	(0.00413)	(0.00841)	(0.0123)
Observations	11,287	2,535	10,815	2,236	1,306	310	8,287	1,870	7,292	1,708	1,259	295
Number of individuals	2,234	741	2,122	640	259	82	1,597	514	1,406	478	254	92

Notes: This table shows the estimated effect of the JSM program by gender and household category on the long-term unemployed between October 2007 (2007q3) and September 2009 (2009q3), captured by the transitions per quarter from unemployment to disability, after the reform was implemented. As discussed in section 5, we provide, for each type of transition, an estimation of the effect using a polynomial specification (liner trend) and a First-Difference specification (FD approach). The control variables included in these specifications are: district (43 dummies), position in the household (8 dummies) and nationality (10 dummies). Standard errors are clustered at the individual level. To estimate the proportional effect of the treatment, we report for each regression the estimate of the transition rate to disability, employment or inactivity for individuals just above the cutoff age, i.e. those aged 50. To do this, we run a similar regression from which we exclude all control variables and extract the constant of it. Dividing the estimated treatment effect by the constant of that alternative regression provides an estimation of the proportional effect of being subject to the JSM program. This table shows that the effect of JSM is, again, only significant for women. When further decomposing the results by household category, we see that amongst women, the effect of JSM is significant for single households and married households to a lower extent. The sample used to perform these estimations is based on representative samples of the Belgian population for each year between 2003 and 2009. We restricted this sample to the long-term unemployed (i.e. those unemployed for 6 quarters or more), as they were the ones directly targeted by the JSM program. We finally narrowed the sample to individuals aged between 46 and 53 years old before conducting our estimations. *** p<0.01, ** p<0.05, * p<0.1

Table 7: Heterogeneous Effects by Past Health Historic:

	Men-Women				Women				Men			
	0 days	0 days	1-100 days	1-100 days	0 days	0 days	1-100 days	1-100 days	0 days	0 days	1-100 days	1-100 days
VARIABLES	Linear trend	FE	Linear trend	FE	Linear trend	FE	Linear trend	FE	Linear trend	FE	Linear trend	FE
Treatment effect	0.0084**	0.0089***	0.0262***	0.0136	0.0151***	0.0154***	0.0465***	0.0401***	0.0009	0.0008	0.001	-0.0192
	(0.0035)	(0.0030)	(0.0096)	(0.0091)	(0.00531)	(0.0045)	(0.0138)	(0.0129)	(0.0044)	(0.0042)	(0.0134)	(0.0148)
Constant	0.0102***	0.0115***	0.0155***	0.0200***	0.0091***	0.0105***	0.0115***	0.0126**	0.0116***	0.0126***	0.0215***	0.0310***
	(0.0013)	(0.0017)	(0.0037)	(0.0049)	(0.0018)	(0.0022)	(0.0041)	(0.0051)	(0.0020)	(0.0027)	(0.0069)	(0.0097)
Observations	30,756	6,883	6,734	1,527	16,949	3,721	4,025	866	13,807	3,162	2,709	661
Number of individuals	5,852	1,922	1,392	446	3,281	1,048	817	259	2,571	874	575	187

Notes: This table shows the estimated effect of the JSM program on the long-term unemployed by past number of sick days between October 2007 (2007q3) and September 2009 (2009q3), captured by the transitions per quarter from unemployment to disability, after the reform was implemented. As discussed in section 5, we provide, for each type of transition, an estimation of the effect using a polynomial specification (liner trend) and a First-Difference specification (FD approach). The control variables included in these specifications are: gender (2 positions), district (43 positions), position in the household (8 positions) and nationality (10 positions). Standard errors are clustered at the individual level. To estimate the proportional effect of the treatment, we report for each regression the estimate of the transition rate to disability, employment or inactivity for individuals just above the cutoff age, i.e. those aged 50. To do this, we run a similar regression from which we exclude all control variables and extract the constant of it. Dividing the estimated treatment effect by the constant of that alternative regression provides an estimation of the proportional effect of being subject to the JSM program. Past number of sick days refer to the number of registered sick days each individual has had during the years 2000-2005. This table shows that the effect of JSM is the highest for women with 1-100 past sick days. For that group, the polynomial approach shows an effect of 4.65 percentage points, which represents a 404% proportional increase in the transition rate to disability. The sample used to perform these estimations is based on representative samples of the Belgian population for each year between 2003 and 2009. We restricted this sample to the long-term unemployed (i.e. those unemployed for 6 quarters or more), as they were the ones directly targeted by the JSM program. We finally narrowed the sample to individuals aged between 46 and 53 years old before conducting our estimations. *** p<0.01, ** p<0.05, * p<0.1

Table 8: Heterogeneous Treatment Effect across Length of the Disability Spell and Gender

	Men-Women				Women				Men			
	Short-term disability		Long-term disability		Short-term disability		Long-term disability		Short-term disability		Long-term disability	
VARIABLES	Linear trend	FD	Linear trend	FD	Linear trend	FD	Linear trend	FD	Linear trend	FD	Linear trend	FD
Treatment effect	0.0106***	0.0098***	0.0036*	0.0030*	0.0169***	0.0142***	0.0053**	0.0061***	0.0025	0.00404	0.0018	-0.0006
	(0.0027)	(0.0025)	(0.0019)	(0.0016)	(0.0041)	(0.0037)	(0.0026)	(0.0022)	(0.0035)	(0.0034)	(0.0026)	(0.0023)
Constant	0.00802***	0.0098***	0.0044***	0.0047***	0.0085***	0.0101***	0.0031***	0.0035***	0.0075***	0.0094***	0.0061***	0.0063***
	(0.0010)	(0.0014)	(0.0007)	(0.0010)	(0.0014)	(0.0019)	(0.0009)	(0.0011)	(0.0014)	(0.0020)	(0.0013)	(0.0017)
Observations	43,394	9,660	43,394	9,660	24,492	5,34	24,492	5,34	18,902	4,32	18,902	4,32
Number of individuals	8435	2739	8435	2739	4812	1532	4812	1532	3623	1207	3623	1207

Notes : This table shows the estimated effect of the JSM program on the long-term unemployed by past number of sick days between October 2007 (2007q3) and September 2009 (2009q3), captured by the transitions per quarter from unemployment to disability, after the reform was implemented. As discussed in section 5, we provide, for each type of transition, an estimation of the effect using a polynomial specification (liner trend) and a First-Difference specification (FD approach). The control variables included in these specifications are: gender (2 positions), district (43 positions), position in the household (8 positions) and nationality (10 positions). Standard errors are clustered at the individual level. To estimate the proportional effect of the treatment, we report for each regression the estimate of the transition rate to disability, employment or inactivity for individuals just above the cutoff age, i.e. those aged 50. To do this, we run a similar regression from which we exclude all control variables and extract the constant of it. Dividing the estimated treatment effect by the constant of that alternative regression provides an estimation of the proportional effect of being subject to the JSM program. This table shows that the effect of JSM is, again, only significant for women, and that it is three times larger for short-term disability spells than for entries into long-term disability. However, if we look instead at proportional effects, the effect of JSM on short term disability is 199% while the effect on long term disability is 171%. The sample used to perform these estimations is based on representative samples of the Belgian population for each year between 2003 and 2009. We restricted this sample to the long-term unemployed (i.e. those unemployed for 6 quarters or more), as they were the ones directly targeted by the JSM program. We finally narrowed the sample to individuals aged between 46 and 53 years old before conducting our estimations. *** p<0.01, ** p<0.05, * p<0.1

Table 9: Treatment Effect of the Monitoring Scheme for the Long-term Unemployed, using Various Age Windows and Polynomials (sensitivity test)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	Benchmark (10)	(11)	(12)	(13)
Age bandwidth	40-59	40-59	40-59	42-57	42-57	42-57	44-55	44-55	44-55	46-53	46-53	46-53	49-50
Polynomial order	Linear	Quadratic	Cubic	Linear	Quadratic	Cubic	Linear	Quadratic	Cubic	Linear	Quadratic	Cubic	FD
Treatment effect	0.0160***	0.0136***	0.0114**	0.0157***	0.0117***	0.0160**	0.0140***	0.0142***	0.0160*	0.0143***	0.0148**	0.0136	0.0128***
	(0.0019)	(0.0031)	(0.0051)	(0.0022)	(0.0037)	(0.0064)	(0.0026)	(0.0047)	(0.0092)	(0.0033)	(0.0071)	(0.0208)	(0.0030)
Constant	0.0129***	0.0124***	0.0129***	0.0127***	0.0126***	0.0133***	0.0125***	0.0129***	0.0137***	0.0124***	0.0137***	0.0145***	0.0145***
	(0.0008)	(0.0012)	(0.0014)	(0.0009)	(0.0013)	(0.0015)	(0.0010)	(0.0014)	(0.0016)	(0.0012)	(0.0016)	(0.0017)	(0.0017)
Observations	102,715	102,715	102,715	90,058	90,058	90,058	68,946	68,946	68,946	43,394	43,394	43,394	9660
Number of individuals	17465	17465	17465	15239	15239	15239	12393	12393	12393	8435	8435	8435	2739
AIC	-140000	-140000	-140000	-120000	-120000	-120000	-88520	-88516	-88513	-53671	-53669	-53667	-10312.26

Notes: This table shows the estimated effect of the JSM program on the long-term unemployed between October 2007 (2007q3) and September 2009 (2009q3) using different age bandwidths and specifications. This is done in order to check that our benchmark results are robust to different changes in specifications and bandwidths choices. The control variables included in these specifications are: gender (2 positions), district (43 positions), position in the household (8 positions) and nationality (10 positions). Standard errors are clustered at the individual level. To estimate the proportional effect of the treatment, we report for each regression the estimate of the transition rate to disability, employment or inactivity for individuals just above the cutoff age, i.e. those aged 50. To do this, we run a similar regression from which we exclude all control variables and extract the constant of it. Dividing the estimated treatment effect by the constant of that alternative regression provides an estimation of the proportional effect of being subject to the JSM program. With estimates ranging from 1,14 pp. to 1,67 pp, this table is in line with our benchmark findings (in column 10) regarding the role of JSM in increasing the probability of entry into disability. The sample used to perform these estimations is based on representative samples of the Belgian population for each year between 2003 and 2009. We restricted this sample to the long-term unemployed (i.e. those unemployed for 6 quarters or more), as they were the ones directly targeted by the JSM program. We then narrowed the sample to the individuals included in the age window analysed before conducting our estimations, i.e.: individuals aged between 40 and 59 years old in columns (1) to (3), individuals aged between 42 and 57 years old in columns (4) to (6), individuals aged between 44 and 55 years old in columns (7) to (9), individuals aged between 46 and 53 years old in columns (10) to (12) and individuals aged 49 to 50 in column (13). *** p<0.01, ** p<0.05, * p<0.1

Table 10: Treatment Effect Excluding Individuals aged 50

	Exit from unemployment		Disability		Employment		Inactivity	
	Linear trend	FD	Linear trend	FD	Linear trend	FD	Linear trend	FD
VARIABLES								
Treatment Effect	0.0355***	0.0486***	0.0163***	0.0187***	0.00971**	0.0145***	0.00954***	0.0155***
	(0.00595)	(0.00471)	(0.00337)	(0.00270)	(0.00383)	(0.00300)	(0.00336)	(0.00263)
Constant	0.0366***	0.0329***	0.0105***	0.00906***	0.0164***	0.0148***	0.00967***	0.00906***
	(0.00260)	(0.00226)	(0.00145)	(0.00120)	(0.00171)	(0.00153)	(0.00134)	(0.00120)
Observations	47,429	10,837	47,429	10,837	47,429	10,837	47,429	10,837
Number of individuals	9,580	3,622	9,580	3,622	9,580	3,622	9,580	3,622

Notes: This table shows the estimated effect of the JSM program on the long-term unemployed between October 2007 (2007q3) and September 2009 (2009q3), captured by the transitions per quarter from unemployment to (1) Exits from unemployment, (2) disability, (3) employment and (4) inactivity after the reform was implemented, but when individuals aged exactly 50 years old are excluded. from the estimations. As discussed in section 5, we provide, for each type of transition, an estimation of the effect using a polynomial specification (liner trend) and a First-Difference specification (FD approach). We thus re-estimate equation (1) for individuals aged 46-49 and 51-54 (in the linear trend specification) and for individuals aged 49 and 51 (in the first-difference specification), in order to keep the same number of years on each side of the cut off. This is done in order to check to sensitivity of our findings to the main limitation of our treatment variable, i.e. the fact that individuals observed as being aged 50 (and thus included in the control group) might spend a certain part of a given year aged 49 (and therefore be treated) depending on their date of birth. The control variables included in these specifications are: gender, district, position in the household and nationality. Standard errors are clustered at the individual level. The table shows that our benchmark findings only slightly underestimate the effect of JSM. The sample used to perform these estimations is based on representative samples of the Belgian population for each year between 2003 and 2009. We restricted this sample to the long-term unemployed (i.e. those unemployed for 6 quarters or more), as they were the ones directly targeted by the JSM program. We finally narrowed the sample to individuals aged between 46 and 53 years old and excluded individuals aged 50 before conducting our estimations. *** p<0.01, ** p<0.05, * p<0.1

Table 11: Treatment Effect of JSM on the Proportion of Entry into Disability, Employment and Inactivity of the Short-term Unemployed

	Disability		Employment		Inactivity	
	Linear trend	FD	Linear trend	FD	Linear trend	FD
Treatment effect	-0.000209	0.00398	0.0171*	0.0203**	0.00718	0.00711
	(0.0048)	(0.0045)	(0.0093)	(0.0084)	(0.0065)	(0.0060)
Constant	0.0296***	0.0309***	0.111***	0.111***	0.0513***	0.0529***
	(0.0026)	(0.0031)	(0.0053)	(0.0060)	(0.0035)	(0.0041)
Observations	23,315	6,059	23,315	6,059	23,315	6,059
Number of individuals	8037	2563	8037	2563	8037	2563

Note: This table shows the estimated effect of the JSM program on the short-term unemployed between October 2007 (2007q3) and September 2009 (2009q3), captured by the transitions per quarter from unemployment to (1) disability, (2) employment and (3) inactivity after the reform was implemented. This serves as a placebo test given that the short-term unemployed are not targeted by job search monitoring. The control variables included in these specifications are: gender, district, position in the household and nationality. Standard errors are clustered at the individual level. No significant treatment effect is found for the short-term unemployed for transitions to disability and inactivity. Regarding transitions to employment, however, we find a slightly significant placebo effect. The dataset used to perform these estimations is based on representative samples of the Belgian population for each year between 2003 and 2009. We restricted this sample of close to 1.000.000 individuals to the short-term unemployed (i.e. those unemployed for less than 6 quarters), as they are the ones we use as a placebo treated group. We finally narrowed the sample to individuals aged between 46 and 53 years old before conducting our estimations in order to provide a placebo test for our benchmark findings. Robust standard errors are presented in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 12: Test for Pre-existing Discontinuities at the age of 50

	Exits from unemployment		Disability		Employment		Inactivity	
VARIABLES	Linear Trend	FD	Linear Trend	FD	Linear Trend	FD	Linear Trend	FD
Treatment Effect	0.0039	0.0085**	0.0037*	0.0035**	-0.0037	0.0008	0.0039*	0.0042**
	(0.004)	(0.003)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)
Constant	0.0329***	0.0335***	0.0082***	0.0082***	0.0169***	0.0168***	0.0078***	0.0085***
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)
Observations	57,815	14,291	57,815	14,291	57,815	14,291	57,815	14,291
Number of individuals	9,095	3,404	9,095	3,404	9,095	3,404	9,095	3,404

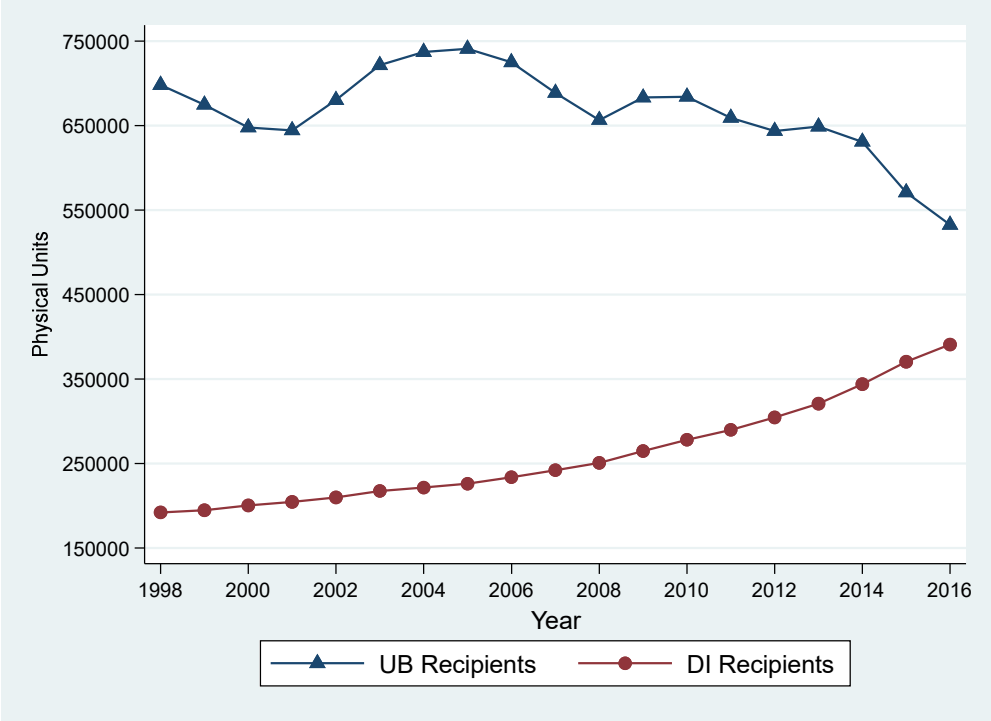
Note: This table shows the estimated effect of the JSM program on the long-term unemployed between April 2004 (2004q2) and June 2006 (2006q2), captured by the transitions per quarter from unemployment to (1) disability, (2) employment and (3) inactivity before the reform was implemented. This serves as a robustness check that no other pre-existing characteristic led to find significant treatment effects of the reform in our benchmark findings. As discussed in section 5, we provide, for each type of transition, an estimation of the effect using a polynomial specification (liner trend) and a First-Difference specification (FD approach). The control variables included in these specifications are: gender (2 positions), district (43 positions), position in the household (8 positions) and nationality (10 positions). Standard errors are clustered at the individual level. To estimate the proportional effect of the treatment, we report for each regression the estimate of the transition rate to disability, employment or inactivity for individuals just above the cutoff age, i.e. those aged 50. To do this, we run a similar regression from which we exclude all control variables and extract the constant of it. Dividing the estimated treatment effect by the constant of that alternative regression provides an estimation of the proportional effect of being subject to the JSM program. The table shows a slightly significant pre-reform effect for the entry rate into disability in the linear trend approach and a more significant effect in the FD approach. To handle this, we implement a "Before-during RDD" approach proposed by Lalive (2008), which allows to take into account possible pre-existing discontinuities at the cut-off. The results of this estimation can be seen in Table 11. The dataset used to perform these estimations is based on representative samples of the Belgian population for each year between 2003 and 2009. We restricted this sample of close to 1.000.000 individuals to the long-term unemployed (i.e. those unemployed for 6 quarters or more), as they were the ones directly targeted by the JSM program. We finally narrowed the sample to individuals aged between 46 and 53 years old before conducting our estimations. Robust standard errors are presented in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 13: Effect of JSM on Transitions to Disability, Employment and Inactivity when Potential Pre-existing Discontinuities are taken into account

	Exits from unemployment		Disability		Employment		Inactivity	
VARIABLES	Linear trend	FD	Linear trend	FD	Linear trend	FD	Linear trend	FD
Pre-period Effect	0.0040	0.0083**	0.0037*	0.0035**	-0.0036	0.0009	0.0038*	0.0039**
	(0.004)	(0.003)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)
During-period Effect	0.0155**	0.0091	0.0032	0.0014	0.0110**	0.0032	0.0014	0.0044
	(0.008)	(0.007)	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)
Post-period Effect	0.0352***	0.0163***	0.0153***	0.0092***	0.0139***	0.0053	0.0060*	0.0017
	(0.006)	(0.006)	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.003)
Constant	0.0391***	0.0443***	0.0103***	0.0115***	0.0179***	0.0196***	0.0109***	0.0132***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	119,767	28,248	119,767	28,248	119,767	28,248	119,767	28,248
Number of individuals	12,979	5,754	12,979	5,754	12,979	5,754	12,979	5,754

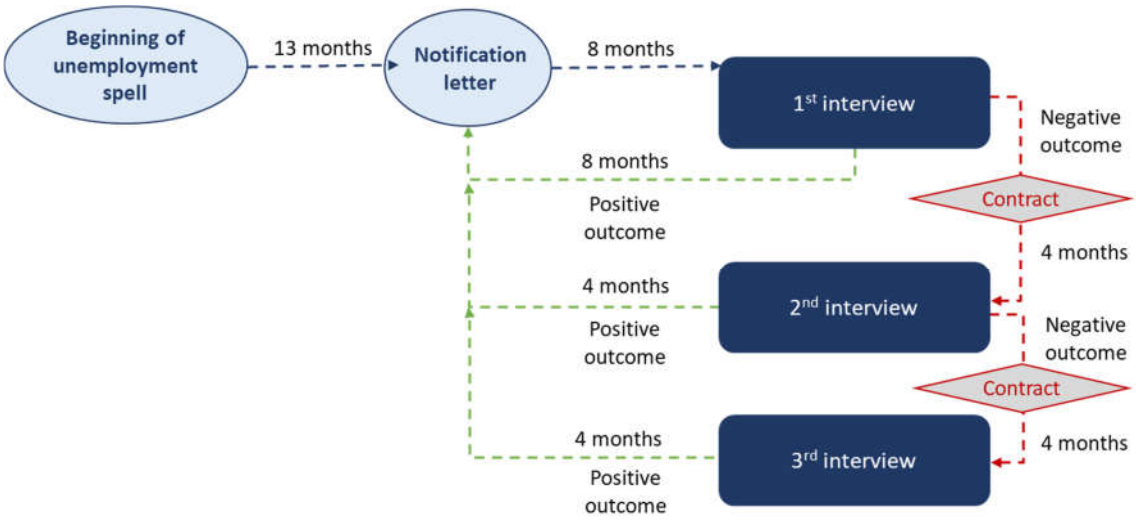
Note: This table shows the estimated effect of the JSM program on the long-term unemployed between April 2004 (2004q2) and September 2009 (2009q3), captured by the transitions per quarter from unemployment to (1) disability, (2) employment and (3) inactivity before the reform was implemented, when potential pre-existing discontinuities at the age of 50 are taken into account. This serves as a robustness check that no other pre-existing characteristic led to find significant treatment effects of the reform in our benchmark findings. The control variables included in these specifications are: gender, district, position in the household and nationality. Standard errors are clustered at the individual level. This table shows that our benchmark results do not significantly change when pre-existing discontinuities are taken into account. A significant exception however is that the Effect of JSM on the transition rate becomes very significant. The sample used to perform these estimations is based on representative samples of the Belgian population for each year between 2003 and 2009. We restricted this sample to the long-term unemployed (i.e. those unemployed for 6 quarters or more), as they were the ones directly targeted by the JSM program. We finally narrowed the sample to individuals aged between 46 and 53 years old before conducting our estimations. *** p<0.01, ** p<0.05, * p<0.1

Figure 1: Evolution of the Unemployed and Disabled Populations



Notes: This figure shows the evolution between 1998 and 2016 of the Belgian population that is registered as unemploymed or disabled. Sources: National Institute for Health and Disability Insurance (NIHDI) and ONEM.

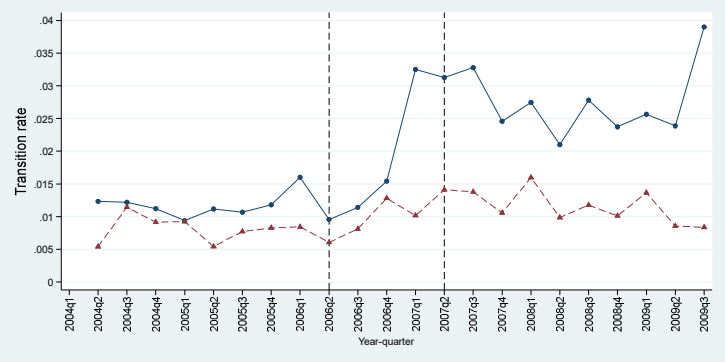
Figure 2: Description of the JSM program



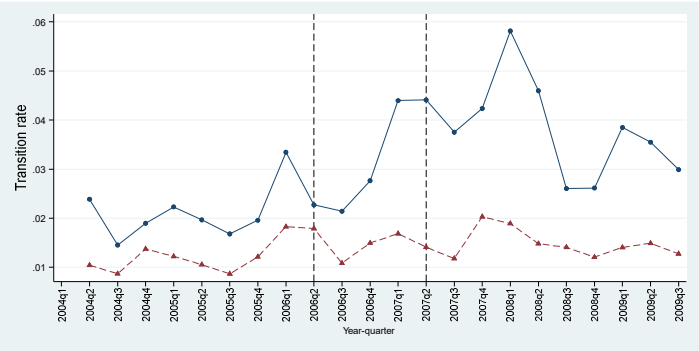
Notes: Figure 2 illustrates the process of the job search monitoring (JSM) program. When a worker is unemployed for 13 months, she receives a notification letter informing her that she will be invited for an interview 8 months later in order to evaluate her job search efforts of the past months. The first interview thus occurs after 21 months of unemployment. If the outcome of the first interview is positive (i.e. the caseworker deems that the worker's job search efforts were sufficient), the unemployed worker is invited for another interview 16 months later. 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 she commits to taking a number of defined actions regarding her search efforts, but no financial sanctions are imposed. The fulfilment of this contract is then evaluated 4 months later during the second interview. If the outcome of the second interview is positive, the worker is invited for another interview 12 months later. If the outcome is negative, then the worker is invited to sign a new, more extensive, contract and a financial sanction (ranging from a 4 months' reduction of benefit to the legal minimum income, to a 4 months' suppression of the unemployment allocation) is imposed. In this case, a third and last interview occurs 4 months later. If the outcome of this last interview is positive, the worker is invited for another interview 12 months later, and if it is negative, the worker is excluded from the unemployment scheme until she fulfils a full year of employment again.

Figure 3: Transition Rates to Disability, Employment and Inactivity for the long-term unemployed aged 46-53 years old

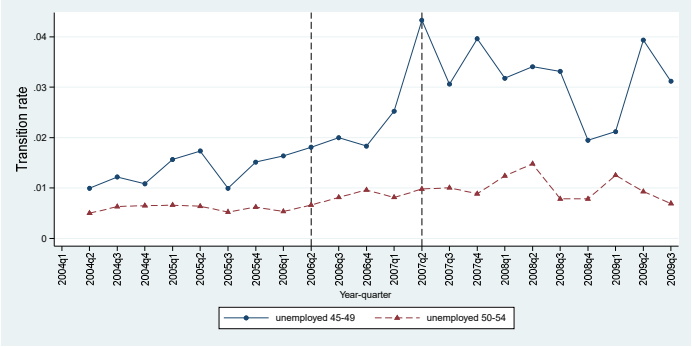
Panel A. Transition rate from Unemployment to Disability



Panel B. Transition rate from Unemployment to Employment



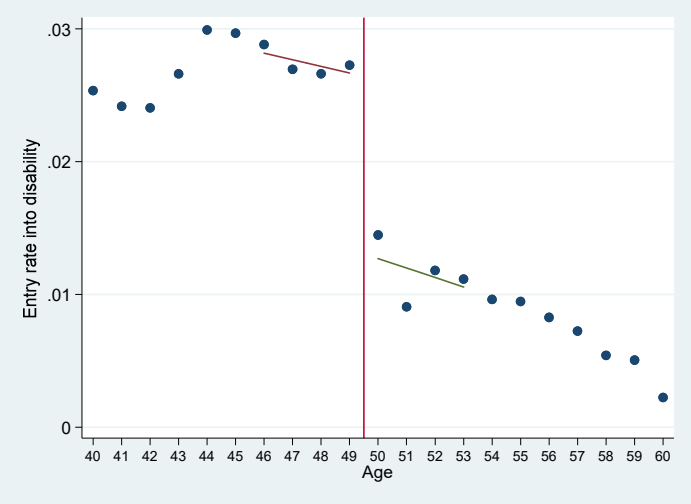
Panel C. Transition rate from Unemployment to Inactivity



Notes: This figure plots the evolution of the transition rate from unemployment to disability (Panel A), employment (Panel B) and inactivity (Panel C) for long-term unemployed (i.e. unemployed for 6 quarters or more) aged 46-49 (in blue) and aged 50-53 (in red) between 2004q2 and 2009q3. The transition rate is defined as the proportion of individuals who switch from unemployment to another status (disability in Panel A, employment in Panel B and inactivity in Panel C) during the considered year-quarter. The first vertical line corresponds to the year-quarter during which the first notification letters were sent to the group of long-term unemployed aged 40-49. The second vertical line correspond to the year quarter during which all long-term unemployed have received a notification letter and

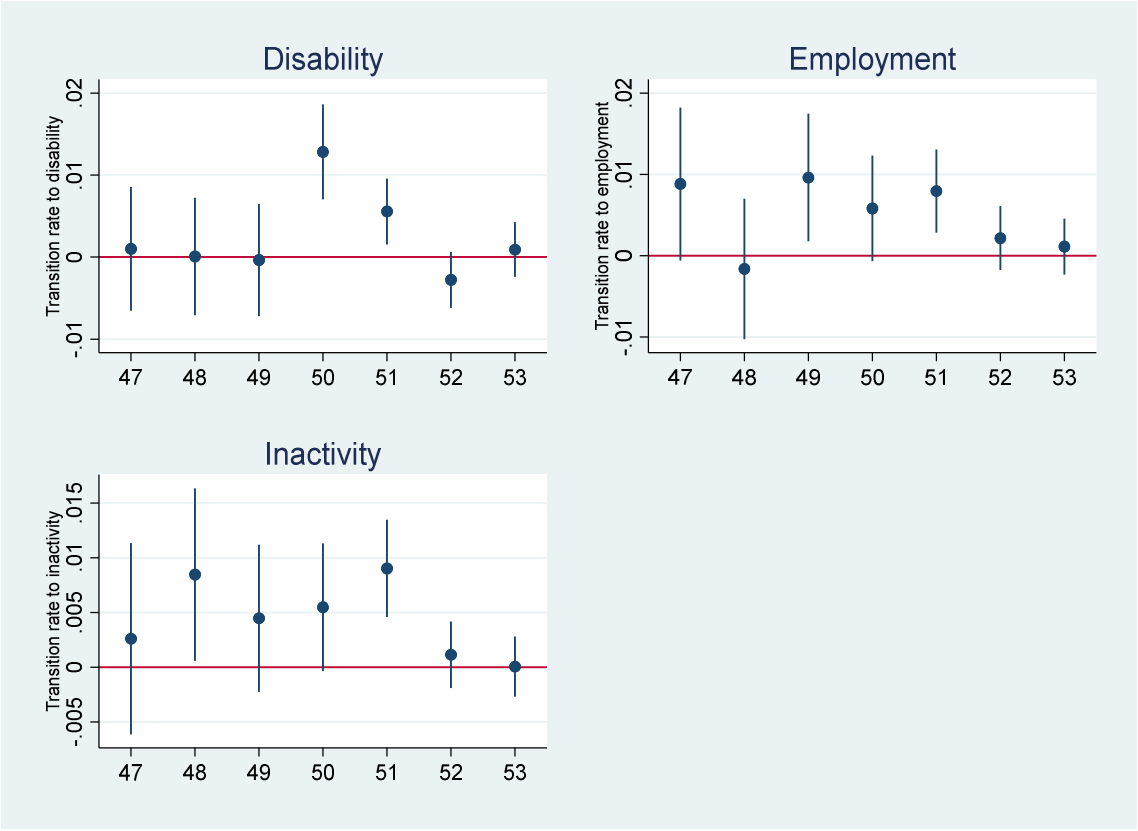
have therefore entered the JSM program. To create this dataset, we first extract samples representative of the Belgian population for each year between 2003 and 2009. We then restrict this sample of close to 1.000.000 individuals to the long-term unemployed (i.e. those unemployed for 6 quarters or more), as they were the ones directly targeted by the JSM program. We finally narrow the sample to individuals aged between 46 and 53 years old.

Figure 4: Discontinuity of the Entry Rate into Disability for Long-term Unemployed (post-reform)



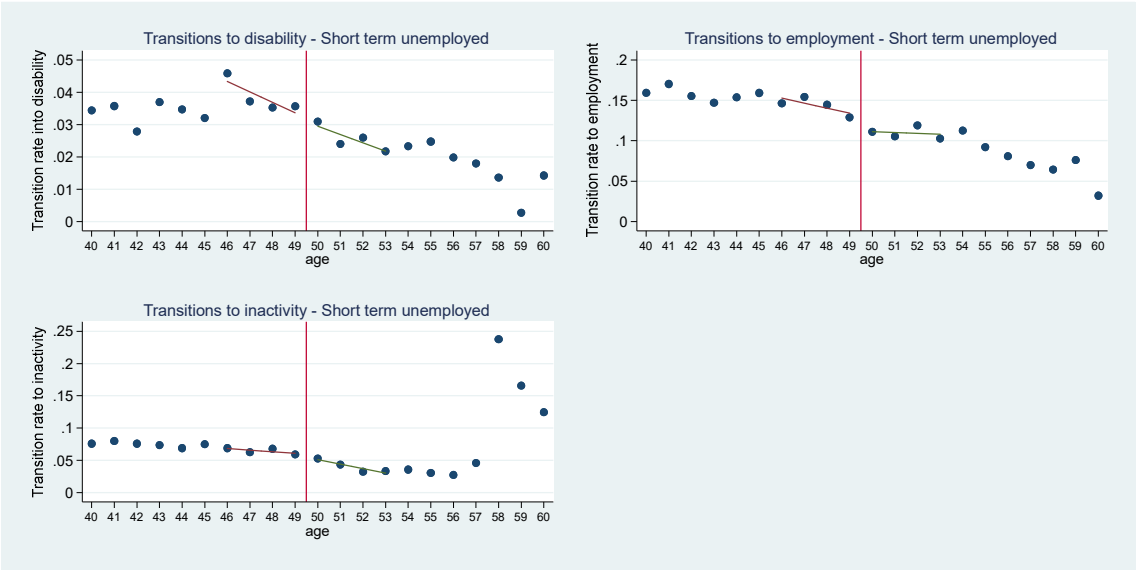
Notes: This figure plots the average proportion of entry into disability at every age between 40 and 60, for long term unemployed, during the period 2007q3 to 2009q3. It indicates that there exists a discontinuity in the proportion of entries into disability at the age of 50 (corresponding to the vertical red line on the graph). The lines each side of the age threshold are linear fits for the unemployed aged 46-49 (on the left of the age threshold) and 50-53 (on the right of the age threshold), with 95% prediction intervals. The dataset used for this plot is based on representative samples of the Belgian population for each year between 2003 and 2009. We restricted this sample of close to 1.000.000 individuals to the long-term unemployed (i.e. those unemployed for 6 quarters or more), as they were the ones directly targeted by the JSM program. We finally narrowed the sample to individuals aged between 46 and 53 years old before conducting our estimations.

Figure 5: Discontinuities in Transitions to Disability, Employment and Inactivity between 46 and 53 (using the first-difference specification)



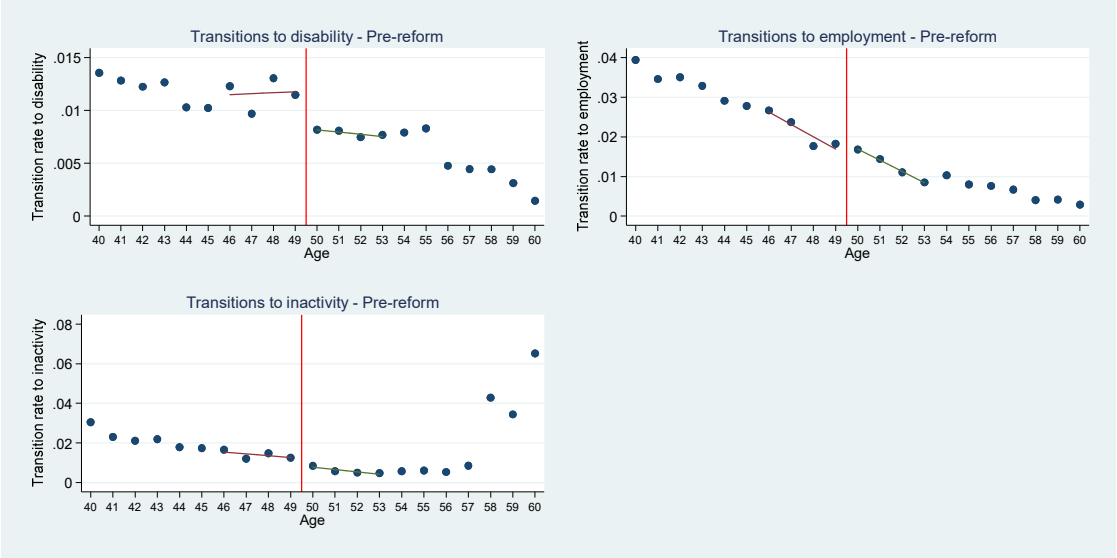
Notes: This figure shows the treatment effects that are found for JSM at each placebo age cut-off between 46 and 53 when the first-difference specification is used. The dataset used for these plots is based on representative samples of the Belgian population for each year between 2003 and 2009. We restricted this sample of close to 1.000.000 individuals to the long-term unemployed (i.e. those unemployed for 6 quarters or more), as they were the ones directly targeted by the JSM program. We finally narrowed the sample to individuals aged between 46 and 53 years old before conducting our estimations.

Figure 6: Test for discontinuities at the age of 50 in the group of short-term unemployed



Notes: These figures plot the average proportions of entry into disability at every age between 40 and 60, for short term unemployed, during the period 2007q3 to 2009q3. It indicates that not significant discontinuity in transitions to disability, employment or inactivity appear at the age cut-off of 50 for this group, thus supporting the assumption that JSM effectively had an effect on its targeted population. The dataset used for these plots is based on representative samples of the Belgian population for each year between 2003 and 2009. We restricted this sample of close to 1.000.000 individuals to the long-term unemployed (i.e. those unemployed for 6 quarters or more), as they were the ones directly targeted by the JSM program. We finally narrowed the sample to individuals aged between 46 and 53 years old before conducting our estimations.

Figure 7: Placebo Treatment Effect before the JSM reform was implemented



Notes: These figures plot the average proportions of entry into disability at every age between 40 and 60, for long term unemployed, during the period 2004q2 to 2006q2. It graphically shows the placebo effect of JSM before it was implemented. It can actually be seen that, for exits to disability, a small discontinuity already existed at the age of 50 before the reform was implemented. We thus perform an additional sensitivity test to determine how the benchmark estimations vary when this pre-existing discontinuity is taken into account. The dataset used for these plots is based on representative samples of the Belgian population for each year between 2003 and 2009. We restricted this sample of close to 1.000.000 individuals to the long-term unemployed (i.e. those unemployed for 6 quarters or more), as they were the ones directly targeted by the JSM program. We finally narrowed the sample to individuals aged between 46 and 53 years old before conducting our estimations.

9. Appendices

Appendix 1: The Belgian Unemployment Insurance System

During the period of analysis, the organisation and management of the Belgian Unemployment insurance was essentially the responsibility of the National Employment Office (ONEM). Regional employment services (Forem in Wallonia, Actiris in Brussels and VDAB in Flanders) complemented the National Employment Office, mainly by offering training and job search support activities to the unemployed. Note that, in 2014, an important reorganization of competences occurred in Belgium, shifting part of the ONEM's responsibilities to the Regions and German-speaking Community. The unemployment benefits are paid by the ONEM through the payment agencies, which correspond to the main trade unions in Belgium (CAPAC, CGSLB, CSC and FGTB). In Belgium, workers are entitled to UB in two instances: (i) after graduation and a waiting period of 12 months or (ii) after involuntary dismissal from a sufficiently long-lasting job. The UB payments, which are not limited in time, depend on the type of UB entitlement, the position in the household, previous earnings and the duration of unemployment. The replacement rate ranges between 40% and 60% of previous earnings (if any), with a floor and cap. A flat rate is thus applied for unemployed workers who do not have sufficient work experience to be eligible for income replacement.

Before 2004, the main obligation that unemployed workers had to fulfil was to be available for the labour market, i.e. they had the obligation to accept when offered a "suitable job". They also had the choice to participate in regional employment support programs if they wished to, but no obligation was imposed in this respect. However, there was no systematic verification of the intensity of the unemployed individuals'' search efforts. Through a royal decree enforced in July 2004, the Belgian Government implemented a Job search monitoring program. At this stage, the scheme was only targeted at individuals who had been long-term unemployed (i.e. for over 13 months) and were aged under 50. It was implemented gradually between 2004 and 2006, starting with the youngest long-term unemployed and progressively extended to the eldest. Hence, in 2004, only the unemployed aged below 30 were monitored. They were followed in 2005 by those aged under 40 years old and, finally, in 2006, the scheme started to apply to all long-term unemployed individuals aged under 50. Then, in 2012, a new royal decree extended the scheme to individuals aged 50 to 54 years old (as of 2013) and 55 to 59 years old (as of 2015).

Appendix 2: Tables:

Table A. 14: Descriptive Statistics by Age

Age	46	47	48	49	50	51	52	53
Gender								
Women	54%	54%	55%	54%	55%	56%	57%	58%
Men	46%	46%	45%	46%	45%	44%	43%	42%
Region								
Brussels	17%	18%	16%	15%	14%	14%	13%	12%
Flander	34%	35%	36%	36%	36%	38%	40%	42%
Wallonia	49%	48%	48%	49%	49%	48%	47%	45%
Nationality								
Belgium	88%	88%	88%	89%	89%	89%	89%	89%
EU28	9%	9%	8%	8%	9%	9%	9%	9%
Other	3%	3%	3%	3%	2%	2%	2%	2%
Household status								
Child	5%	6%	5%	4%	4%	4%	3%	4%
Cohabitant with children	5%	5%	4%	4%	3%	3%	2%	2%
Cohabitant without children	3%	3%	3%	4%	4%	4%	4%	4%
Married with children	29%	30%	30%	28%	28%	28%	27%	25%
Married without children	7%	6%	8%	11%	13%	16%	18%	21%
Other	2%	2%	2%	2%	2%	2%	2%	2%
Single with children	28%	28%	29%	30%	29%	30%	30%	31%
Single without children	21%	20%	19%	17%	16%	14%	13%	11%
Unemployment duration								
6-10 quarters	28%	25%	24%	23%	21%	21%	19%	16%
11-15 quarters	15%	16%	16%	17%	17%	16%	18%	17%
16 + quarters	57%	58%	60%	61%	62%	63%	63%	66%

Notes: This table displays descriptive statistics for the year-quarter 2007q3 by age for the samples used in the paper. All percentages represent the share of individuals in the sample that present the considered characteristic. Other statistics are averages taken over all individuals in the sample. To create the samples, we first extract representative samples of the Belgian population for each year between 2003 and 2009. We then restricted this sample to the long-term unemployed (i.e. those unemployed for 6 quarters or more), as they were the ones directly targeted by the JSM program. We finally narrow the sample to the year-quarter during which the individuals are aged between 46 and 53 years old. The variable "Region" is based on the region where the individual has her legal residence. The variable "Position in the Household" is based on information from the Belgian national register.

Table A.15: Benchmark Results Without Control Variables:

	Exit from unemployment		Disability		Employment		Inactivity	
VARIABLES	Linear Trend	FD	Linear Trend	FD	Linear Trend	FD	Linear Trend	FD
Treatment Effect	0.0232***	0.0250***	0.0138***	0.0128***	0.0058	0.0066**	0.0036	0.0057*
	(0.006)	(0.005)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)
Constant	0.0486***	0.0565***	0.0124***	0.0145***	0.0206***	0.0231***	0.0156***	0.0190***
	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)
Observations	43,394	9,660	43,394	9,660	43,394	9,660	43,394	9,660
Number of individuals	8,435	2,739	8,435	2,739	8,435	2,739	8,435	2,739

Notes: This table shows the estimated effect of the JSM program on the long-term unemployed between October 2007 (2007q3) and September 2009 (2009q3), captured by the transitions per quarter from unemployment to (1) Exits from unemployment, (2) disability, (3) employment and (4) inactivity after the reform was implemented. As discussed in section 5, we provide, for each type of transition, an estimation of the effect using a polynomial specification (linear trend) and a First-Difference specification (FD approach). There are no control variables included in these regressions. Standard errors are clustered at the individual level. This table shows that the fact of running the same regression than in the benchmark results without including control variables does not lead to very different results. The sample used to perform these estimations is based on representative samples of the Belgian population for each year between 2003 and 2009. We restricted this sample to the long-term unemployed (i.e. those unemployed for 6 quarters or more), as they were the ones directly targeted by the JSM program. We finally narrowed the sample to individuals aged between 46 and 53 years old before conducting our estimations. *** p<0.01, ** p<0.05, * p<0.1

Table A.16: Sensitivity Test for Transitions from Long-term Unemployment to Employment

VARIABLES	40-59	40-59	40-59	42-57	42-57	42-57	44-55	44-55	44-55	46-53	46-53	46-53	49-50
Polynomial order	Linear	Quadratic	Cubic	Linear	Quadratic	Cubic	Linear	Quadratic	Cubic	Linear	Quadratic	Cubic	/
Treatment effect	0.0152***	0.00679*	-0.00534	0.0133***	0.00237	-0.00223	0.00890***	0.00102	-0.000329	0.00547	-0.000379	-0.0407*	0.00575*
	(0.00232)	(0.00372)	(0.00587)	(0.00256)	(0.00430)	(0.00726)	(0.00296)	(0.00533)	(0.0106)	(0.00377)	(0.00794)	(0.0246)	(0.00330)
Constant	0.0182***	0.0201***	0.0217***	0.0184***	0.0208***	0.0222***	0.0193***	0.0216***	0.0227***	0.0206***	0.0227***	0.0231***	0.0231***
	(0.000936)	(0.00143)	(0.00179)	(0.00104)	(0.00157)	(0.00190)	(0.00123)	(0.00177)	(0.00202)	(0.00157)	(0.00201)	(0.00211)	(0.00211)
Observations	102,715	102,715	102,715	90,058	90,058	90,058	68,946	68,946	68,946	43,394	43,394	43,394	9660
Number of individuals	17465	17465	17465	15239	15239	15239	12393	12393	12393	8435	8435	8435	2739
AIC	-100000	-100000	-100000	-91626	-91638	-91637	-68312	-68315	-68313	-40857	-40857	-40858	-8.049.796

Notes: This table shows the estimated effect of the JSM program on the transition rate to employment for the long-term unemployed between October 2007 (2007q3) and September 2009 (2009q3) using different age bandwidths and specifications. This is done in order to check that our benchmark results are robust to different changes in specifications and bandwidths choices. The control variables included in these specifications are: gender (2 positions), district (43 positions), position in the household (8 positions) and nationality (10 positions). Standard errors are clustered at the individual level. To estimate the proportional effect of the treatment, we report for each regression the estimate of the transition rate to employment for individuals just above the cutoff age, i.e. those aged 50. To do this, we run a similar regression from which we exclude all control variables and extract the constant of it. Dividing the estimated treatment effect by the constant of that alternative regression provides an estimation of the proportional effect of being subject to the JSM program. These results show quite unstable results when we change the bandwidth and the order of the polynomial. However, the results are very low and insignificants in most cases. The sample used to perform these estimations is based on representative samples of the Belgian population for each year between 2003 and 2009. We restricted this sample to the long-term unemployed (i.e. those unemployed for 6 quarters or more), as they were the ones directly targeted by the JSM program. We then narrowed the sample to the individuals included in the age window analysed before conducting our estimations, i.e.: individuals aged between 40 and 59 years old in columns (1) to (3), individuals aged between 42 and 57 years old in columns (4) to (6), individuals aged between 44 and 55 years old in columns (7) to (9), individuals aged between 46 and 53 years old in columns (10) to (12) and individuals aged 49 to 50 in column (13). *** p<0.01, ** p<0.05, * p<0.1

Table A.17: Sensitivity Test for Transitions from Long-term Unemployment to Inactivity

VARIABLES	40-59	40-59	40-59	42-57	42-57	42-57	44-55	44-55	44-55	46-53	46-53	46-53	49-50
Polynomial order	Linear	Quadratic	Cubic	Linear	Quadratic	Cubic	Linear	Quadratic	Cubic	Linear	Quadratic	Cubic	/
Treatment effect	0.0374***	-0.0057*	-0.0095*	0.0164***	-0.0010	-0.0025	0.0092***	-0.0011	-0.0005	0.0036	-0.0006	0.0158	0.00546*
	(0.0022)	(0.0034)	(0.0054)	(0.0023)	(0.0038)	(0.0066)	(0.0026)	(0.0048)	(0.0096)	(0.0034)	(0.0072)	(0.0219)	(0.00297)
Constant	-0.0120***	0.0289***	0.0180***	0.0102***	0.0170***	0.0168***	0.0134***	0.0167***	0.0181***	0.0154***	0.0181***	0.0186***	0.0186***
	(0.0011)	(0.0014)	(0.0017)	(0.0009)	(0.0014)	(0.0017)	(0.0011)	(0.0016)	(0.0018)	(0.0014)	(0.0018)	(0.0019)	(0.00190)
Observations	102,715	102,715	102,715	90,058	90,058	90,058	68,946	68,946	68,946	43,394	43,394	43,394	9660
Number of individuals	17465	17465	17465	15239	15239	15239	12393	12393	12393	8435	8435	8435	2739
AIC	-83317	-84535	-84659	-110000	-110000	-110000	-85693	-85705	-85704	-52308	-52311	-52309	-9964.697

Notes: This table shows the estimated effect of the JSM program on the transition rate to inactivity for the long-term unemployed between October 2007 (2007q3) and September 2009 (2009q3) using different age bandwidths and specifications. This is done in order to check that our benchmark results are robust to different changes in specifications and bandwidths choices. The control variables included in these specifications are: gender (2 positions), district (43 positions), position in the household (8 positions) and nationality (10 positions). Standard errors are clustered at the individual level. To estimate the proportional effect of the treatment, we report for each regression the estimate of the transition rate to inactivity for individuals just above the cutoff age, i.e. those aged 50. To do this, we run a similar regression from which we exclude all control variables and extract the constant of it. Dividing the estimated treatment effect by the constant of that alternative regression provides an estimation of the proportional effect of being subject to the JSM program. These results show quite unstable results when we change the bandwidth and the order of the polynomial. However, the results are very low and insignificants in most cases. The sample used to perform these estimations is based on representative samples of the Belgian population for each year between 2003 and 2009. We restricted this sample to the long-term unemployed (i.e. those unemployed for 6 quarters or more), as they were the ones directly targeted by the JSM program. We then narrowed the sample to the individuals included in the age window analysed before conducting our estimations, i.e.: individuals aged between 40 and 59 years old in columns (1) to (3), individuals aged between 42 and 57 years old in columns (4) to (6), individuals aged between 44 and 55 years old in columns (7) to (9), individuals aged between 46 and 53 years old in columns (10) to (12) and individuals aged 49 to 50 in column (13). *** p<0.01, ** p<0.05, * p<0.1