IZA Institute of Labor Economics

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DISCUSSION PAPER SERIES

IZA DP No. 17881

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**Tomi Kyyrä**
*VATT Institute for Economic Research, Helsinki and IZA*

**Jouko Verho**
*VATT Institute for Economic Research, Helsinki*

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**IZA – Institute of Labor Economics**

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

# ABSTRACT

## Do Financial Incentives for Training and Caseworker Meetings Enhance Re-Employment?*

In 2005, displaced workers in Finland with at least three years of work history were given the option to enroll in a Re-employment Program. Participants met with a caseworker at the beginning of their unemployment and drafted an employment plan. In return, they became eligible for higher benets for four weeks, as well as for the duration of individually targeted training programs specied in their plan. The program aimed to provide early counseling, encourage participation in labor market training, and improve matches between training programs and job seekers. Using a difference-in-differences approach, we show that the program increased caseworker meetings and participation in training programs but had no effect on unemployment duration in the short run or employment in the longer run. The eect on training participation was particularly strong for men, older workers and low-skilled workers, yet unemployment and employment effects were equally disappointing across all subgroups.

**JEL Classification:** C21, J64, J68

**Keywords:** unemployment benets, caseworkers, re-employment, active labor market policy, labor market training

**Corresponding author:**
Tomi Kyyrä
VATT Institute for Economic Research
Arkadiankatu 7
PO Box 1279
00101 Helsinki
Finland

E-mail: tomi.kyyra@vatt.fi

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# 1 Introduction

Many European countries continue to experience persistently high unemployment, highlighting the need for effective labor market policies. Active labor market programs (ALMPs)—including job-search assistance, labor market training programs, and subsidized employment—aim to improve the employment prospects of job seekers. While the extent of ALMP implementation varies across countries, these programs typically account for a significant share of public expenditures in developed economies.[1]

A large body of research has evaluated the effectiveness of these programs across various settings. Meta-analyses by Kluve (2010), Card et al. (2018) and Vooren et al. (2019) summarize findings from over 200 evaluation studies, concluding that ALMP participation has, on average, only a modest effect on employment.[2] Similarly, a review by Crépon & van den Berg (2016) concludes that ALMPs are generally less effective than have been expected. However, these reviews emphasize that while the average effects of ALMPs are small, certain programs show more positive effects for specific subpopulations, suggesting that better targeting could enhance the effectiveness of these policies.

In most countries, caseworkers assist job seekers with enrolling in ALMPs. Participation in programs is usually voluntary but can be mandatory for certain subgroups, such as unskilled workers or the long-term unemployed. Many job seekers appear to dislike ALMPs, leading to low participation rates and high dropout rates (Heckman et al. 2000, Behaghel et al. 2014). Even those who could benefit significantly may be reluctant to participate (LaLonde 2007). Moreover, the mere threat of mandatory ALMP participation, especially when combined with benefit sanctions for noncompliance, can increase job search efforts before the program begins (Black et al. 2003, van den Berg et al. 2009, Geerdsen 2006, Rosholm & Svarer 2008). Some countries with generous benefit systems, such as Denmark and the Netherlands, make extensive use of mandatory ALMPs and their associated threat effects (Kreiner & Svarer 2022).

The Finnish government took a markedly different approach by launching its Re-employment (RE) Program in July 2005. The program was voluntary and targeted displaced workers with at least three years of work history. Eligible workers who chose to enroll met with a caseworker either during their notice period or at the start of their unemployment spell. During this meeting, they drafted an employment plan specifying the ALMPs and other employment services they should use or apply for. This plan replaced the standard job-search plan with the similar content but which is typically drafted much later, often after several months of unemployment. Program participants became eligible for substantially higher unemployment insurance (UI) benefits—24% higher for a job seeker with median labor earnings—for up to four weeks, as well as for the duration of

[1] In 2018, total spending on ALMPs across OECD countries amounted to 0.5% of GDP (Le Barbanchon et al. 2024). In Nordic countries, spending was significantly higher, ranging from 1% to 2% of GDP.

[2] Studies on the effects of Finnish ALMPs include Tuomala (2011), which examines mandatory participation for the long-term unemployed, and Hämäläinen & Tuomala (2007), which analyzes vocational training for young job seekers. Neither study finds significant effects on employment.

individually targeted training programs outlined in their employment plan.

The RE program aimed to enhance re-employment by providing early counseling, encouraging participation in labor market training, and improving the match between training programs and job seekers. The program introduced a unique incentive scheme: to our knowledge, no other country offers significantly higher UI benefits for the unemployed who voluntary attend caseworker meetings and participate in individually targeted training programs.[3]

The program can enhance re-employment through several channels. First, participants receive job-search counseling much earlier than other unemployed individuals, which is expected to have a positive employment effect (e.g., Behaghel et al. 2014, Belot et al. 2019). Second, the program may reduce the underutilization of training programs and improve the matching of job seekers to appropriate training opportunities, both of which should enhance re-employment, particularly in the longer run. However, these effects depend on caseworkers' ability to assign individuals to the right programs at the right time. The effectiveness of caseworkers in this regard is uncertain. For example, Lechner & Smith (2007) find that caseworker allocation in Switzerland was no more better than random assignment to ALMPs.

Certain program features may also delay re-employment. The higher UI benefits for four weeks may reduce job search effort, albeit only temporarily. Additionally, if ALMPs were poorly targeted, the higher benefits for program participation could encourage job seekers—who might otherwise have found employment quickly—to enroll in training programs that do not benefit them, prolonging their unemployment spells. As a result, the overall employment effect of the RE program is a priori ambiguous and depends heavily on how effectively caseworkers assign job seekers to suitable programs.

We study the effects of the RE program, using rich data from multiple population-level administrative registers. Our analysis sample consists of workers aged 25–54 with at least three years of work history who became unemployed between 2002 and 2006 due to involuntary job loss. For identification, we exploit the rule that only workers laid off for economic reasons became eligible for the program starting in July 2005, whereas those whose fixed-term contract expired were ineligible. Using a difference-in-differences (DiD) approach, we compare unemployed workers who were laid off (treatment group) with those whose fixed-term contract expired (control group).

In the treatment group, program enrollment surged to 60% immediately after the reform and reached approximately 70% within a year. Due to incomplete take-up, we first estimate the effects of program eligibility (i.e., the intention-to-treat effects) on labor market outcomes. Additionally, we use group-specific changes in eligibility as an instrumental variable (IV) for program enrollment in order to estimate the effects of actual

[3] There are somewhat similar conditional cash transfer programs targeted at welfare benefit recipients. For example, Markussen & Røed (2016) study a comprehensive activation program combined with substantial cash transfers for hard-to-employ individuals in Norway, whereas Del Boca et al. (2021) examines a conditional cash transfer program for low-income families with dependent children in Italy. Both studies find that the programs increase employment and reduce poverty.

participation. To study underlying mechanisms, we also examine how the exit rate from UI benefits to employment changes when a job seeker receives higher UI benefits, meets with a caseworker, or participates in labor market training.

Our findings show that the RE program significantly increased caseworker meetings, with most enrollees meeting their caseworker already during their notice period. The program also raised the share of job seekers participating in training programs during their UI spell by four percentage points, from an initial level of 27%. Among program participants, training participation increased by nearly seven percentage points. These effects were particularly pronounced for men, older workers, and low-skilled workers. Descriptive results from the duration analysis suggest that interactions with caseworkers increased transitions into training programs while reducing exits to employment. However, we find no evidence of a change in the effect of training program participation on the exit rate to employment. This suggests that the RE program did not improve the matching of job seekers to appropriate training opportunities.

Despite the increase in caseworker meetings and labor market training participation, we find no statistically significant effects on unemployment duration, non-employment duration, or employment over a two-year period. These results hold across various subgroups. Thus, while the RE program successfully facilitated early access to employment services and encouraged labor market training, it ultimately failed to achieve its primary goal of enhancing re-employment.

Our study contributes to several branches of the ALMP literature. To our knowledge, we are the first to demonstrate that financial incentives can be effectively used to induce job seekers to interact with caseworkers and participate in labor market training. Additionally, we contribute to the recent literature on the effects of caseworker meetings. This literature has found that such meetings typically accelerate re-employment (e.g., Maibom et al. 2017, Michaelides & Mueser 2020). Some studies highlight substantial impact heterogeneity, finding significant effects only for the most productive caseworkers (Schiprowski 2020; Cederlöf et al. forthcoming). Schiprowski (2020) attributes this variation to personal counseling styles rather than to differences in ALMP assignments across caseworkers. Behncke et al. (2010) find that stricter caseworkers achieve better employment outcomes, while Huber et al. (2017) show that this heterogeneity is unrelated to differences in ALMP allocation.

These previous studies have focused on mandatory caseworker meetings held during unemployment spells. In contrast, we examine early, voluntary meetings typically conducted during the notice period, which were accompanied by a significant financial incentive. Only Homrighausen & Oberfichtner (2024) have studied voluntary pre-unemployment caseworker meetings, finding no effect for such meetings. Unlike meetings during unemployment, which serve not only to provide counseling but also to monitor job search efforts, pre-unemployment meetings lack this monitoring function and the associated threat of benefit sanctions. The null effect found by Homrighausen & Oberfichtner

(2024) may underscore the importance of the monitoring role,[4] potentially helping to explain the ineffectiveness of the Finnish RE program evaluated here.

A few studies have examined caseworker effectiveness in assigning job seekers to ALMPs. Lechner & Smith (2007) show that these assignments are, on average, suboptimal. Bolhaar et al. (2020) find that providing information on the effectiveness of different programs did not induce caseworkers to focus on promoting more effective programs. Similarly, Behncke et al. (2009) report that caseworkers largely ignored recommendations from a statistical targeting system designed to support optimal ALMP assignment. We contribute to this literature by showing that granting caseworkers the authority to use financial incentives to guide job seekers toward individually targeted training programs did not improve the match quality between participants and programs.

The rest of the paper proceeds as follows. The next section describes relevant institutions. Section 3 outlines the 2005 reform and research design. Section 4 describes data. Section 5 presents descriptive evidence, and Section 6 reports results for the program effects. Section 7 provides evidence on underlying mechanisms. Section 8 concludes.

# 2 Institutional setting

## 2.1 Unemployment benefits

Finland has a two-tier unemployment compensation system that provides earnings-related UI benefits for a limited period, followed by a less generous flat-rate unemployment assistance. To receive benefits, all recipients must be registered as unemployed job seekers with the Public Employment Service (PES), actively search for full-time work, and be ready and able to start working upon receiving a job offer.

Earnings-related UI benefits are paid by unemployment funds to eligible unemployed members with sufficient insured employment history.[5] New claimants face a seven-weekday waiting period before receiving benefits. However, individuals who voluntarily quit their job without an acceptable cause face an extended waiting period of one to three months.

During our study period, the maximum duration of UI benefits was 100 calendar weeks (500 weekdays), which is relatively long compared to other OECD countries. In addition, unemployed individuals aged 55 or older at the time of job loss, with at least 20 years of work history, are eligible for an extended benefit period that allows them to receive UI benefits until they reach old-age retirement. These older unemployed individuals are excluded from our analysis.

[4]In 2017, the frequency of caseworker meetings during unemployment spells was substantially increased in Finland. Using regional variation in the implementation of the new policy, Huuskonen (2023) finds that a higher frequency of meetings had a strong positive effect on ALMP participation and a modest positive impact on exits to employment. At least part of these effects can be attributed to intensified monitoring, as the reform also led to an increase in the benefit sanction rate.

[5]Specifically, a UI claimant must have worked for at least 43 weeks within the past 28 months (or at least 34 weeks within the past 24 months for previous UI recipients seeking to requalify) while being a member of an unemployment fund.

The UI benefit level is based on the claimant's average monthly earnings during the reference period used to establish eligibility. Unlike in most other countries, there is no cap on the benefit level; however, the replacement rate declines sharply as past earnings increase due to a kink in the benefit rule (see Figure 1 below). Between 2003 and 2016, workers with at least 20 years of work history who were laid off for economic reasons were eligible for somewhat higher UI benefits for up to 30 calendar weeks (150 weekdays).

The second-tier unemployment assistance is available to job seekers who either exhaust their UI benefits or do not meet the eligibility requirements for UI benefits. This assistance is means-tested but can be received indefinitely as long as the eligibility conditions are met. In 2006, unemployment assistance (excluding child supplements) amounted to 505 euros per month, approximately half of the average UI benefit at the time.

## 2.2 Caseworker meetings

Unemployed job seekers are generally expected to meet with a caseworker within one month of the beginning of their job search. This initial meeting serves to complete the job seeker's profile, explore available job and training opportunities, and assess the need for services. The required actions and job search goals are then documented in a job-search plan. If unemployment persists, a follow-up meeting should occur within five months from the start of the unemployment spell. During this meeting, the caseworker identifies any gaps in the job seeker's professional qualifications and evaluates addtional service needs. The job-search plan is also updated to more specifically outline the ALMPs and other services the job seeker should utilize or apply for. In subsequent meetings, the caseworker evaluates the job seeker's search efforts and compliance with the plan, updating it as needed. Failure to comply with the job-search plan may result in benefit sanctions, which can range from 30 to 90 weekdays of lost benefits. However, these sanctions are rarely enforced, possibly due to their severity.

It is important to note that these meetings are not always held. Caseworkers assess the need for individual guidance, and if a job seeker is deemed to have strong employment prospects, meetings may be considered unnecessary. Our data indicate that it is common for the meetings to be omitted within the study population.

## 2.3 Active labor market programs

The Finnish PES offers a wide range of ALMPs, similar to those available in other Nordic countries. The main programs include labor market training, job-search assistance, subsidized employment, and work practices. Some programs are specifically targeted at individuals with significant skill deficiencies. Labor market training is the most common ALMP among UI benefit recipients. Although organized by the PES, these training programs are often outsourced to educational institutions or private providers. Program content varies widely, ranging from short job-search training courses lasting a few days to vocational training programs that span several months. In our data, the median training program duration is three months.

Job seekers are generally free to apply for any available training programs, but admission is based on PES assessment. This means that even if a job-search plan recommends a specific type of training, the applicant is not guaranteed a spot. During our analysis period, the annual acceptance rate to training programs ranged between 53% and 60%.

Finally, it is worth noting that while unemployed individuals continue to receive benefits equivalent to their UI benefit level during participation in ALMPs, the time spent in these programs does not count against their maximum UI duration. This effectively extends the maximum benefit duration by the number of days spent in ALMPs, providing an additional incentive to participate.

# 3 The 2005 reform and research design

## 3.1 Re-employment Program

The Re-employment Program (*työllistymisohjelma*) was proposed by the government in April 2005 as part of a new employment act aimed at improving the re-employment prospects and social security of displaced workers.[6] The act came into effect on July 1, 2005. The RE program was available to all eligible workers whose unemployment began after this date. However, workers who were already receiving UI benefits at the time were also allowed to enroll, provided their job had been terminated between February 15 and June 30.

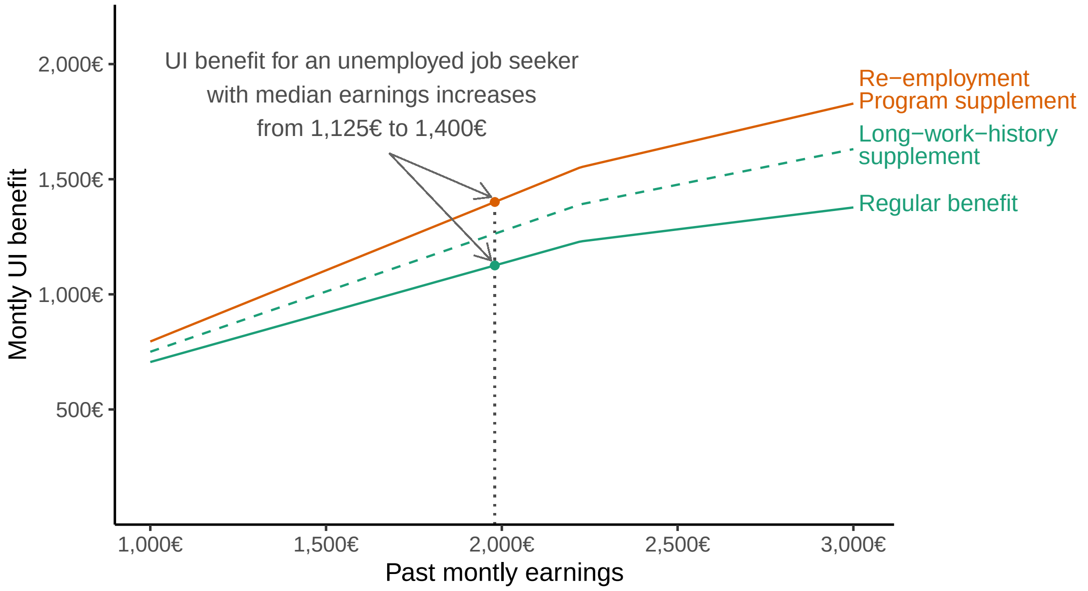
The program had strict eligibility criteria based on previous work history and the reason for job loss. The primary target group consisted of workers with at least three years of work history who were laid off for economic reasons. Additionally, workers whose fixed-term contracts had expired could qualify, but only if they had been employed by the same employer for at least 36 months within the past 42 months.

Participation in the program was voluntary for eligible workers. Those who enrolled were required to meet with a caseworker within 30 days of becoming unemployed or earlier during their notice period. During this meeting, an employment plan was drafted, outlining the ALMPs and other employment services the individual should engage with or apply for. The content of this employment plan closely resembles that of the standard job-search plan.

The program offered financial incentives for participation. Participants were entitled to higher UI benefits (i.e. a specific supplement paid on the top of the regular UI benefit) during active job search for up to four weeks (20 weekdays). Additionally, participants who took part in training programs outlined in their employment plans were eligible for

[6] The act also introduced obligations for employers to support re-employment during the layoff process. These obligations required employers to notify the PES of upcoming layoffs and the termination of fixed-term contracts, as well as to inform affected employees about their eligibility for the RE program.

Figure 1: Increased UI benefits for Re-employment Program participants and for displaced workers with over 20 years of work history compared to regular UI benefits




the higher benefits also for the duration of the training programs. The maximum duration for receiving higher benefits due to the RE program was 37 calendar weeks (185 weekdays).

Figure 1 illustrates that program participants were entitled to a significant benefit increase. With the median pre-unemployment earnings in our analysis sample, the regular UI benefit amounted to 1,125 euros per month, corresponding to a replacement rate of 57% (with a higher net replacement rate due to progressive income taxation). With the RE program supplement, the UI benefit increased to 1,400 euros per month, a 24% increase in benefit level compared to regular UI benefits, raising the replacement rate to 71%. The benefit increase due to the RE program was larger, both in absolute and relative terms, for unemployed individuals with higher past earnings. For instance, for past monthly earnings of 1,500 and 3,000 euros, participation in the RE program increased UI benefits by 20% and 33%, respectively.

For displaced workers eligible for the benefit supplement due to a work history of over 20 years, the RE program provided a smaller benefit increase, as it was not possible to receive both benefit supplements simultaneously. In our analysis sample, the long-work-history benefit supplements were primarily received by older individuals who were also eligible for the RE program. While we include these individuals in our main analysis, restricting the sample to unemployed job seekers with less than 20 years of work history does not alter our conclusions.

## 3.2 Research design

To identify the causal effects of program eligibility and participation, we exploit variation in eligibility status across workers who differ in work history and in the reason for job loss. Specifically, we adopt a difference-in-differences approach and compare laid-off workers with at least three years of work history (the treatment group affected by the reform) to temporary workers with less than 36 months of job tenure within the past 42 months (the unaffected control group) before and after the reform.[7]

We exclude temporary workers whose fixed-term contracts ended at firms where they had been employed for more than 36 months within the past 42 months. These individuals differ substantially from laid-off workers in terms of both program enrollment and re-employment prospects. Therefore, we treat them as a separate treatment group and conduct a distinct analysis for them as part of our robustness checks.

The key identifying assumption of the DiD design is that, in the absence of the reform, the expected outcomes for individuals in the treatment and control groups would have followed the same trend. Since our observation period begins several quarters before the law took effect (and before the reform became public knowledge), we can assess the validity of this common trends assumption by examining trends in outcome variables during the pre-reform period. If the outcomes moved in parallel for several months before the reform, it is reasonable to assume they would have continued to do so in the post-reform period in the absence of the reform. We provide evidence that this was indeed the case.

Additionally, we show that the unemployment inflow within the treatment group did not change significantly around the time of the reform. Such a change could have indicated anticipatory behavior by employers or eligible workers. Furthermore, we demonstrate that the sample composition of both the treatment and control groups remained relatively stable over time.

Crépon et al. (2013) and Cheung et al. (2025) find that targeted programs can negatively affect ineligible individuals if caseworkers reallocate limited resources away from them. However, this is unlikely to be a concern in our case. Since only a small share of all UI recipients were eligible for and enrolled in the RE program, it is unlikely that the program caused any displacement effects on the control group.

[7] Under the assumption that the effect of work history (overall work history for laid-off workers and job tenure in the last firm within the past 42 months for temporary workers) is smooth around the three-year threshold, the treatment effect could, in principle, be identified solely from post-reform data using the regression discontinuity design (RDD). However, this approach has three significant drawbacks in our setting. First, our work history variables are subject to some measurement error. Second, ineligible workers in the post-reform period with slightly less than three years of work history may have an incentive to take up a short-term job to qualify for the program at the beginning of their next unemployment spell. Third, there are relatively few observations around the eligibility thresholds. The first two issues could bias the estimated treatment effect in the RDD framework, as it relies on comparing outcomes between individuals just below and above the eligibility threshold. The third issue would further limit the reliability of the results due to a small sample size. Although the DiD estimator is more robust, it is not entirely immune to measurement error either, as misclassification of workers into the treatment or control group could introduce bias. However, in our case, misclassification does not appear to be a major concern, because fewer than 2% of control group members enrolled in the RE program during the post-reform period.

# 4 Data

## 4.1 Data sources

Our analysis is based on data constructed by linking several administrative registers that cover all Finnish residents. The primary data source is the benefit register of the Financial Supervisory Authority, which contains detailed information on earnings-related UI benefits paid by unemployment funds. These payment records provide information on the timing of compensated unemployment periods, the level and various components of the benefits (e.g., supplements for participation in the RE program or for long work history), and the justification for each payment (e.g., active job search or participation in labor market training).

To supplement the benefit records, we use registers from the Ministry of Employment and the Economy, which contain extensive background information on all registered job seekers at the PES. These registers also include data on active labor market programs, as well as individual-level information on applications and admissions to these programs.

Additionally, we obtain employment data from the registers of the Finnish Centre for Pensions. These records track job start and end dates, along with labor earnings, for the entire Finnish population over several decades, dating back to the introduction of pension laws. We use these records to measure the length of work histories, identify those who find new jobs, and measure longer-term employment outcomes.

## 4.2 Outcome variables

Unemployment spells are constructed using UI benefit payment records. Since some unemployed experience short breaks in their benefit payments, we merge consecutive spells that are less than 30 days apart to provide a more accurate representation of actual unemployment duration. Additionally, we use benefit records to identify job seekers who participated in training programs during their unemployment spells, as well as those who received benefit supplements due to enrollment in the RE program or having a work history of over 20 years.

In our analysis sample, 79% of job seekers transition directly from UI benefits to a new job. For the remaining 21%, there may be a relatively long gap between the end of their UI spell and the start of their next job, during which they may receive unemployment assistance or remain without benefits. To account for this, we also analyze non-employment spells, defined as the time between the end of the previous job and the start of the next job following UI receipt, provided that this period does not exceed two years. For individuals who did not find a new job within two years, non-employment duration is capped at two years.

The RE program may influence not only the duration of unemployment and non-employment but also long-term employment outcomes. For instance, it may help job seekers secure more stable employment. To capture potential longer-term effects, we also examine the total time spent in employment within the two-year period following the start of the unemployment spell.

### 4.3 Sample restrictions

We restrict our analysis to workers aged 25–54 who became unemployed between 2002 and 2006 due to either a layoff for economic reasons or the expiration of a fixed-term contract. We require that included individuals qualified for a new 100-week period of UI benefits, started collecting UI benefits within 60 days of job loss, and had at least three years of work history. We exclude workers over age 54 due to an early retirement scheme that allowed them to collect UI benefits until reaching old-age retirement.

For unemployed individuals whose fixed-term contract was expired, we further require that they did not work for the same employer for more than 36 months within the past 42 months. This restriction ensures that unemployed workers from temporary jobs included in the sample were not eligible for the RE program.

Additionally, we exclude members of the Teachers' Unemployment Fund. The vast majority of unemployed teachers are recent graduates who work as substitute teachers before securing a tenured position. In Finnish municipalities, there is an unconventional practice in which teachers on parental leave often return to work during holiday periods, resulting in their substitutes being taken off payroll for the summer months when no teaching occurs. This creates strong artificial seasonal variation in teachers' unemployment.

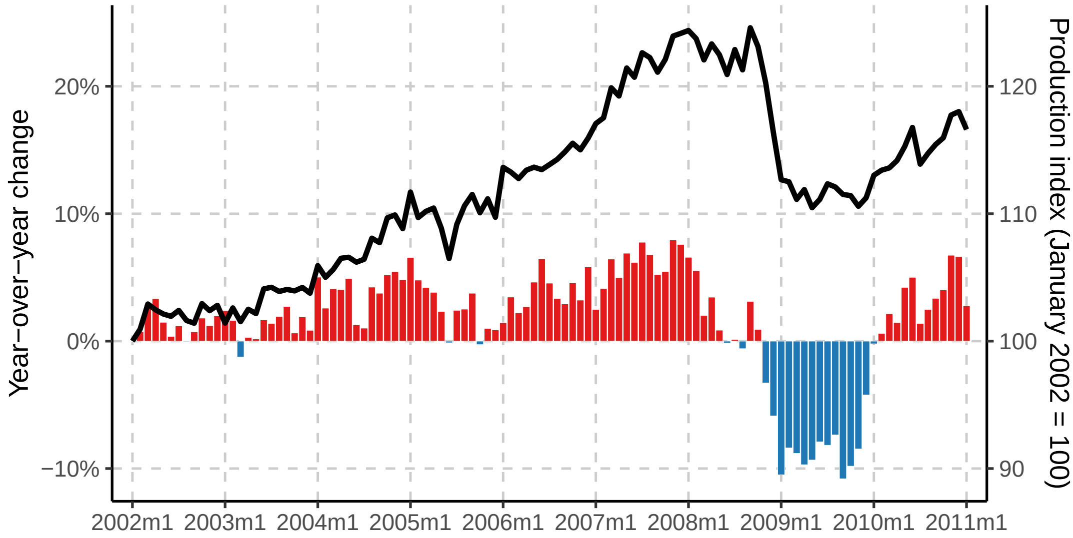
## 5 Descriptive evidence

### 5.1 Economic environment

Figure 2 illustrates that our sample members entered unemployment during a time of relatively smooth economic growth. From the beginning of 2002 to the end of 2006, the economy grew by 16%. Prior to the financial crisis, economic growth accelerated to 4% in 2006 and 5% in 2007. However, growth came to a halt in early 2008, and a sharp decline began at the end of that year. In 2009, the economy contracted by 9%, leading to a collapse in labor demand and placing unemployed job seekers in a difficult situation.

Although the drastic change in the economic environment should not pose a problem in the DiD analysis, this is not entirely the case here. The average UI duration in our treatment group is much longer than in the control group (39 weeks vs. 22 weeks). As a result, a larger proportion of workers in the treatment group who became unemployed in 2007 or early 2008 were affected by the financial crisis before they exited unemployment, compared to those in the control group who became unemployed at the same time. To address this issue, we limit our analysis to workers who became unemployed before 2007 and measure labor market outcomes over a two-year period only.

Figure 2: Production index and year-over-year change in production




*Notes:* The black line plots the seasonally-adjusted production volume index (right-hand scale), and the bars show year-over-year changes in production (left-hand scale). Both series are corrected for differences in monthly workdays. *Source:* Statistics Finland.

## 5.2 Unemployment inflow

Figure 3 shows that the overall trend in unemployment inflows remained stable and similar for both the treatment and control groups during the period 2002–2004. In the last two years, the inflow declined slightly more in the treatment group than in the control group,[8] likely due to improved economic conditions. While the robust economic outlook for 2006 and 2007 reduced layoffs, it had less impact on the termination of fixed-term contracts.

Another difference between the groups is that the control group exhibits much stronger seasonal variation in unemployment inflows, with noticeable spikes around the end of the year and early summer. As shown later, similar seasonal differences are also present in the unemployment duration and subsequent employment outcomes between the two groups, probably driven by occupational differences.

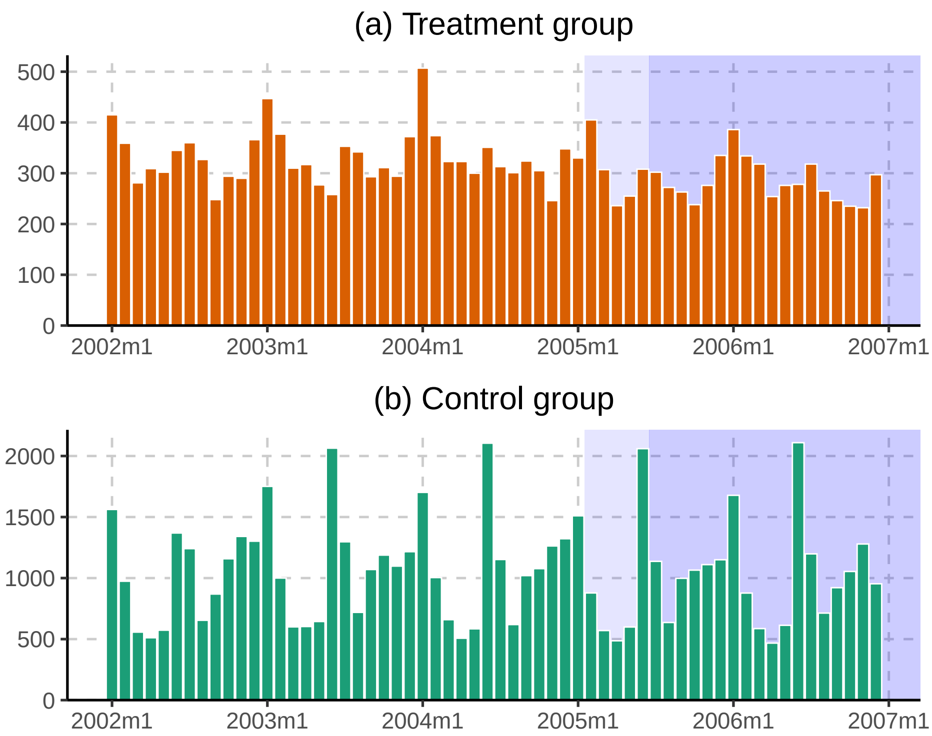
However, the most important observation is that there were no significant changes in the unemployment inflow of the treatment group around the time of the law change in July 2005, compared to the same months in other years. If such changes had occurred, they might have indicated anticipatory behavior among employers or eligible employees.

## 5.3 Program enrollment

In the post-reform period, approximately two-thirds of eligible individuals enrolled in the RE program (see Figure 4). Since the enrollment rate surged to around 60% by the end of 2005, it is unlikely that the program's gradual adoption by the PES and employers or

[8]In the treatment group, the inflow was 14% lower in 2006 than in 2004. In the control group. the inflow declined by 4% over the same period.

Figure 3: Monthly unemployment inflow




*Notes:* The graph plots the number of new UI spells each month. The dark blue area indicates the post-reform period starting from July 2005. UI spells that started in the light blue area (February to June, 2005) may or may not be affected by the reform from July 2005 onward, depending on the termination date of the previous job.

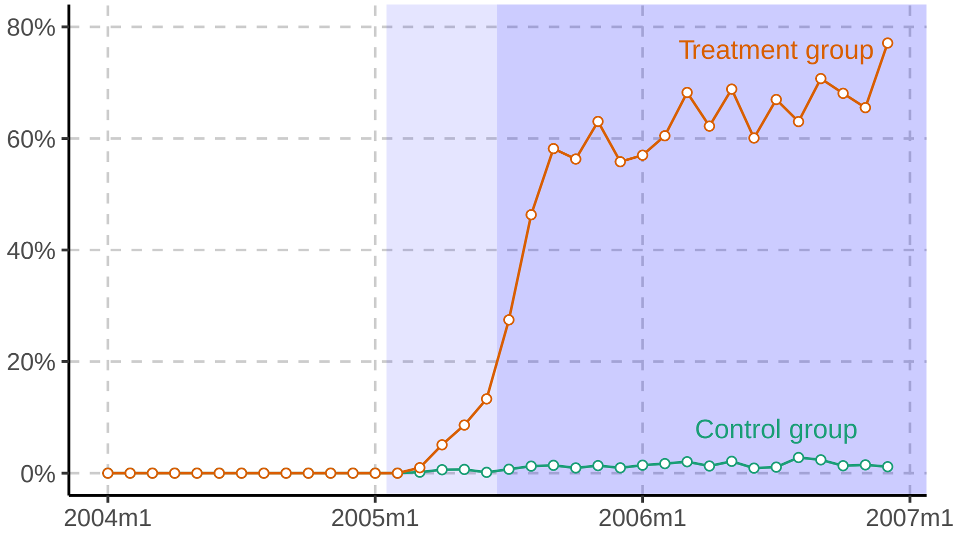
limited awareness among eligible workers at the outset posed significant issues.

Eligible workers who did not enroll experienced shorter unemployment spells (on average, 32 weeks compared to 41 weeks among enrolled workers) and spent less time in labor market training (8% of their UI days versus 23% for enrolled workers). If these individuals expected to find a new job quickly, they likely perceived the program as unnecessary and chose to forgo the financial benefits to avoid a fruitless meeting with a caseworker.

As expected based on the eligibility rules, some individuals in the treatment group who became unemployed between February and June 2005 (the light blue area) enrolled in the program. These individuals became eligible in July if they were still unemployed at that time, meaning that their eligibility status depends on the duration of their unemployment spell. And those who enrolled met with a caseworker later in their unemployment spell than other program participants. For these reasons, we typically exclude UI spells that began during this transition period from the analysis.

A small proportion (1.4%) of individuals in the control group enrolled in the RE program during the post-reform period, despite not being eligible based on their work history. This suggests that our measure of work history contains a minor degree of measurement error. Nevertheless, the classification of workers into the treatment and control groups remains highly accurate overall.

Figure 4: The Re-employment Program enrollment rate by the month of unemployment entry




*Notes:* The dark blue area indicates the post-reform period starting from July 2005. UI spells that started in the light blue area (February to June, 2005) may or may not be affected by the reform from July 2005 onward, depending on the termination date of the previous job.

## 5.4 Caseworker meetings

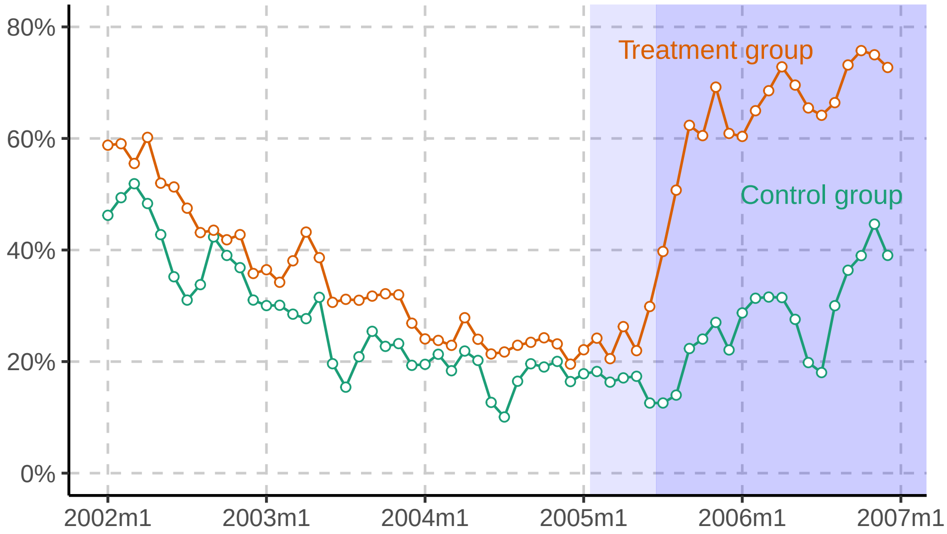
Before the reform took effect, one-fifth of UI recipients in both groups met with a caseworker either before their unemployment spell began or during their first unemployment month (Figure 5). Following the reform, this share rose to over 60% in the treatment group by September 2005. Early meetings with caseworkers also became more common in the control group during the post-reform period, particularly in late 2006. However, the gap in the early meetings between the groups remained steady at around 30 percentage points throughout the entire post-reform period. As such, the reform significantly accelerated the timing of the first meeting with caseworkers for the treatment group.

Figure 6 further illustrates the timing of caseworker meetings during unemployment spells in the treatment group. Before the reform, a significant share of long-term unemployed individuals did not meet with a caseworker, as 32% of job seekers had not drafted a job-search plan even after one year of unemployment. In the post-reform period, this share dropped to 12%.

In both periods, a small fraction of the treatment group began their unemployment spell in labor market training. The share of training program participants increases gradually with the duration of unemployment. Participation is particularly high beyond the 100th week of unemployment, at around 40%, since UI benefits cannot be collected beyond this point without being in labor market training at some point during the UI spell. Additionally, ALMPs are probably more actively offered to individuals who are at risk of exhausting their UI benefits.

Panel (b) of Figure 6 shows that the vast majority of RE program participants collected

Figure 5: The share of job seekers who drafted a job-search or employment plan during their first UI month or earlier by the month of unemployment entry




*Notes:* The dark blue area indicates the post-reform period starting from July 2005. UI spells that started in the light blue area (February to June, 2005) may or may not affected by the reform from July 2005 onward, depending on the termination date of the previous job.

higher UI benefits without enrolling in labor market training during the first four weeks of their UI spell (the dark blue area). This was only possible for those who had met with a caseworker before their unemployment spell began.

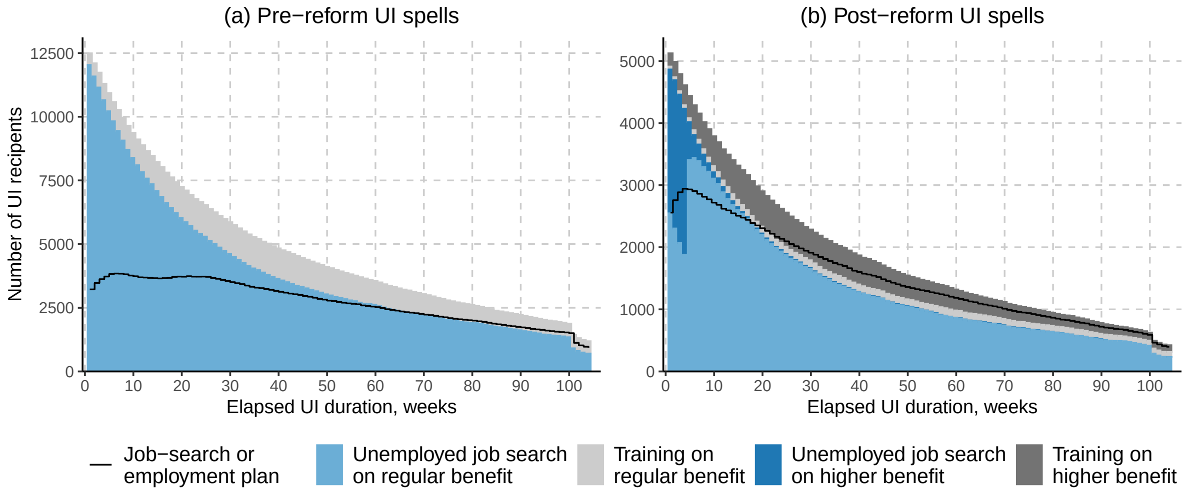
In the post-reform period, workers in the treatment group received higher benefits for most of their training days, suggesting they closely followed their employment plans and engaged in training programs outlined by caseworkers for them. Also, the overall time spent in labor market training during UI spells increased after the reform, though this change is not easily visible in Figure 6.

## 5.5 Sample statistics

Table 1 presents sample means by group and period, excluding the UI spells that started during the transition period between February 15 and June 31, 2015. While the treatment and control groups are similar in many respects, there are also distinct differences. Both groups have comparable average ages and levels of educational attainment. However, the treatment group consists of a higher proportion of men and exhibits some occupational differences. Specifically, industrial workers, and sales and commerce personnel are over-represented in the treatment group, whereas fewer than 3% worked in the healthcare and social work sector. Additionally, treatment group members spent less time on UI benefits in the two years preceding their current UI spell, have longer work histories, and remained in their previous jobs for a longer duration. Given these differences, it is unsurprising that their past labor earnings were higher, which also explains their higher UI benefits.

In the treatment group, the average UI benefit increased by 13% from the pre- to

Figure 6: The number of UI recipients in the treatment group as a function of elapsed UI duration and benefit type




*Notes:* The high of the bar equals the total number of UI recipients in the treatment group who were still unemployed at the beginning of a given UI week. Different colors shows the shares of those who received regular benefits or higher benefits based on the RE program during unemployed job search or during participation in training programs. The black line shows the number of those who had drafted a job-search or employment plant by given UI week. Panel (a) includes UI spells that begun before February 15, 2005, and panel (b) UI spells that begun in July 2005 or later.

post-reform period, compared to a 4% increase in the control group. The larger increase for the treatment group is primarily due to the supplements that were paid on the top of the regular UI benefits. In the post-reform period, 61% of the treatment group received higher benefits due to enrollment in the RE program, while the share of those receiving higher benefits based on their long work history rose from 20% to 31% (this supplement was introduced in 2003).

Workers who enrolled in the RE program did not differ notably from other eligible workers in the post-reform period (see columns 2 and 3). They were very similar in terms of average age, educational attainment, and occupational distribution, though they were slightly more likely to be female and had longer tenure in their previous job. On average, enrollees experienced UI and non-employment spells that were three weeks longer than those of all eligible workers in the post-reform period.

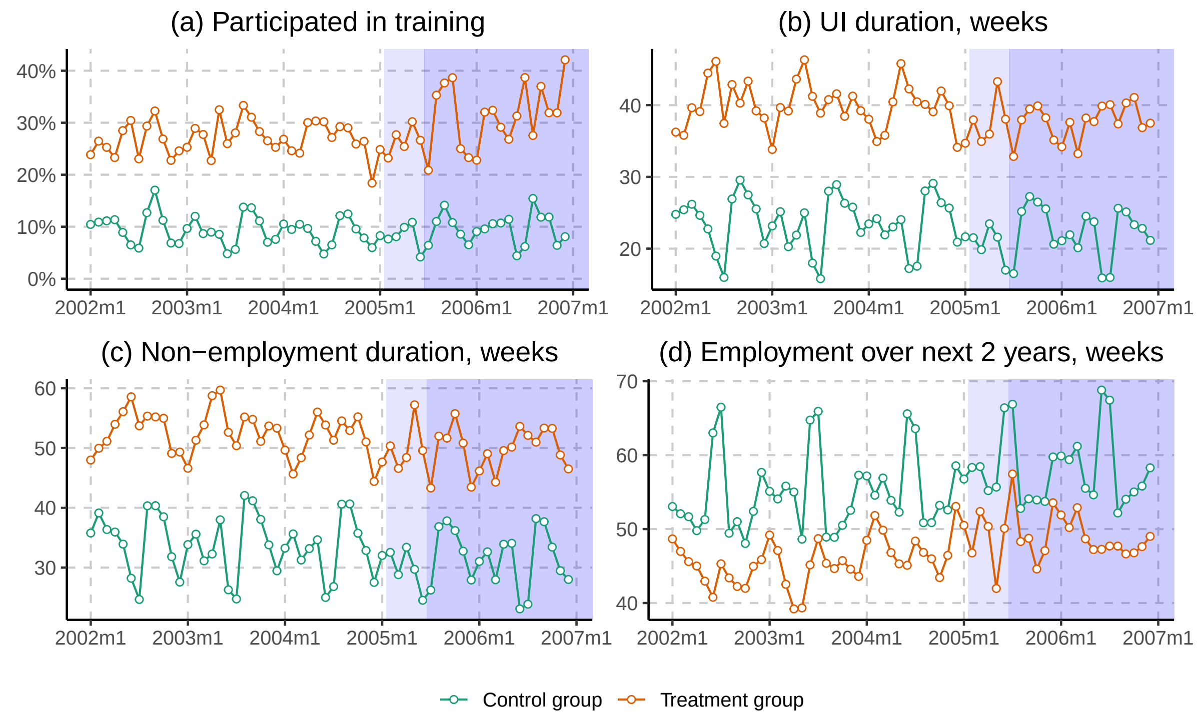
There are significant differences in UI duration and employment outcomes between the treatment and control groups. Individuals in the treatment group experienced substantially longer UI spells both before and after the reform—on average, about 40 weeks compared to 22 weeks in the control group. They also had lower re-employment rates, with about 10 percentage points fewer UI spells ending in a direct transition to a new job. In both groups, the average UI duration shortened slightly, and re-employment rates increased marginally in the post-reform period. Changes over time and differences in non-employment duration and subsequent employment weeks follow a similar pattern to UI duration, as labor market conditions improved slightly in the post-reform period.

Table 1: Sample means by group and period of unemployment entry

| | Treatment group | | | Control group | |
|---|---|---|---|---|---|
| | Pre-reform (1) | Post-reform (2) | Participants (3) | Pre-reform (4) | Post-reform (5) |
| *Sociodemographic factors* | | | | | |
| Age | 40.4 | 40.9 | 41.1 | 39.4 | 39.3 |
| Female, % | 39.4 | 42.2 | 46.8 | 53.3 | 55.4 |
| Immigrant, % | 4.3 | 4.6 | 4.2 | 3.4 | 3.8 |
| Education, % | | | | | |
| Compulsory | 27.3 | 25.4 | 23.4 | 26.0 | 24.5 |
| High school or vocational | 45.6 | 49.3 | 48.8 | 49.9 | 51.0 |
| Lower tertiary | 22.2 | 21.4 | 23.7 | 19.8 | 19.8 |
| Upper tertiary | 4.9 | 4.0 | 4.1 | 4.3 | 4.7 |
| Occupation, % | | | | | |
| Health and social work | 2.8 | 2.7 | 1.7 | 20.1 | 20.7 |
| Office work | 14.4 | 16.0 | 17.5 | 10.2 | 10.3 |
| Sales and commerce | 14.7 | 15.2 | 16.3 | 4.8 | 5.1 |
| Agriculture and forestry | 1.3 | 1.5 | 1.0 | 6.4 | 7.0 |
| Transport and logistics | 4.5 | 4.1 | 3.2 | 3.0 | 3.2 |
| Construction and mining | 9.6 | 7.8 | 3.4 | 16.6 | 13.9 |
| Industrial work | 32.0 | 32.4 | 36.7 | 16.5 | 16.8 |
| Services | 8.9 | 9.8 | 8.7 | 11.4 | 11.7 |
| *Labor market experience* | | | | | |
| Work history, years | 20.6 | 21.1 | 21.6 | 16.7 | 16.5 |
| UI weeks in past 2 years | 3.9 | 4.2 | 2.0 | 17.3 | 18.7 |
| Duration of previous job, weeks | 182.7 | 217.0 | 260.8 | 35.7 | 33.2 |
| Past labor earnings, €/month | 2,328 | 2,413 | 2,441 | 1,873 | 1,995 |
| *UI spells* | | | | | |
| UI benefit, €/month | 1,388 | 1,587 | 1,703 | 1,201 | 1,249 |
| Long-work-history supplement, % | 20.0 | 30.5 | 34.6 | 0.6 | 1.5 |
| RE program supplement, % | 0.0 | 60.6 | 100.0 | 0.0 | 1.4 |
| UI duration, weeks | 39.6 | 37.4 | 40.9 | 23.0 | 21.7 |
| Exit to a new job, % | 71.1 | 73.3 | 72.9 | 80.7 | 81.9 |
| *Labor market training* | | | | | |
| Participated, % | 26.8 | 31.0 | 40.6 | 8.8 | 8.9 |
| All training weeks | 9.1 | 10.4 | 14.1 | 2.5 | 2.5 |
| Training weeks on higher benefits | 0.0 | 7.6 | 12.5 | 0.0 | 0.1 |
| *Non-employment spells* | | | | | |
| Non-employment duration, weeks | 52.2 | 49.4 | 52.6 | 32.8 | 30.8 |
| Found a job within 2 years, % | 73.7 | 77.7 | 76.2 | 88.0 | 90.0 |
| *Subsequent employment* | | | | | |
| Employment weeks over next 1 year | 17.6 | 18.8 | 17.1 | 25.2 | 26.3 |
| Employment weeks over next 2 years | 45.9 | 49.4 | 46.8 | 56.1 | 59.1 |
| Employment weeks over next 3 years | 79.5 | 83.3 | 80.9 | 89.3 | 92.7 |
| Number of observations | 12,494 | 5,125 | 3,106 | 40,522 | 18,544 |

*Notes*: Spells started between February 15 and December 31, 2005 are excluded. Column 3 includes those who enrolled in the RE program during the post-reform period. Immigrant refers to individuals whose mother tongue is other than Finnish or Swedish. The past labor earnings are those that were used to compute the level of the regular UI benefit. The UI benefits are measured at the beginning of the UI spells, and they may include various supplements. Training weeks on higher benefits refers to training while receiving the RE program supplement.

Figure 7: Outcomes by group and month of unemployment entry




*Notes:* The dark blue area indicates the post-reform period starting from July 2005. Unemployment spells that started in the light blue area (February 15 to June 30, 2005) may or may not be affected by the reform from July 2005 onward, depending on the termination date of the previous job.

Participation in labor market training is more common among the treatment group, likely due to their longer unemployment spells. While training participation in the control group remained stable over time, it increased in the treatment group from 27% to 31%, with the average time spent in training rising by one week. Among those enrolled in the RE program, 41% participated in training. On average, they spent 14 weeks in training, with nearly all training weeks associated with higher UI benefits.

Figure 7 shows the evolution of the outcome variables over time by group. The graphs reveal some differences in seasonal variation between the groups. Individuals in the control group who became unemployed in June or July experienced relatively short UI and non-employment spells but had significantly more employment weeks over the following two years. In contrast, the treatment group did not exhibit similar seasonal patterns. This suggests that the control group includes workers whose fixed-term employment contracts do not cover the summer months. In the event-study analysis, we adjust for this group-specific seasonality. We also show that our results are robust to the exclusion of UI spells that began in the summer months. Despite the different seasonal patterns, overall trends in UI duration, non-employment, and subsequent employment were similar for both groups before the reform, supporting the parallel trend assumption of our DiD setting.

# 6 Results

## 6.1 Event-study estimates

We begin by estimating quarter-specific treatment effects using the event-study approach. Since event-study estimates can be sensitive to differential seasonal patterns, we remove group-specific seasonality from the outcome variables described above. Specifically, we first regress the outcome variable on calendar-month dummies separately for the treatment and control groups. We then extract the residuals, which capture deviations from the seasonal patterns represented by the calendar-month effects, and add the overall mean of the outcome variable to obtain the seasonally adjusted outcome variable. For the duration variables, we use logarithmic transformations to capture relative effects.

For a worker $i$ who enters unemployment at time $t$, we specify the following event-study DiD model:

$$\tilde{Y}_{it} = \alpha_t + X_{it}\beta + \theta Treat_{it} + \delta_t Treat_{it} + \varepsilon_{it}, \tag{1}$$

where $\tilde{Y}_{it}$ is the seasonally adjusted outcome variable, $\alpha_t$ are quarter-by-year fixed effects, $X_{it}$ is a vector of control variables,[9] $Treat_{it}$ is an indicator variable for belonging to the treatment group, and $\varepsilon_{it}$ is an error term.

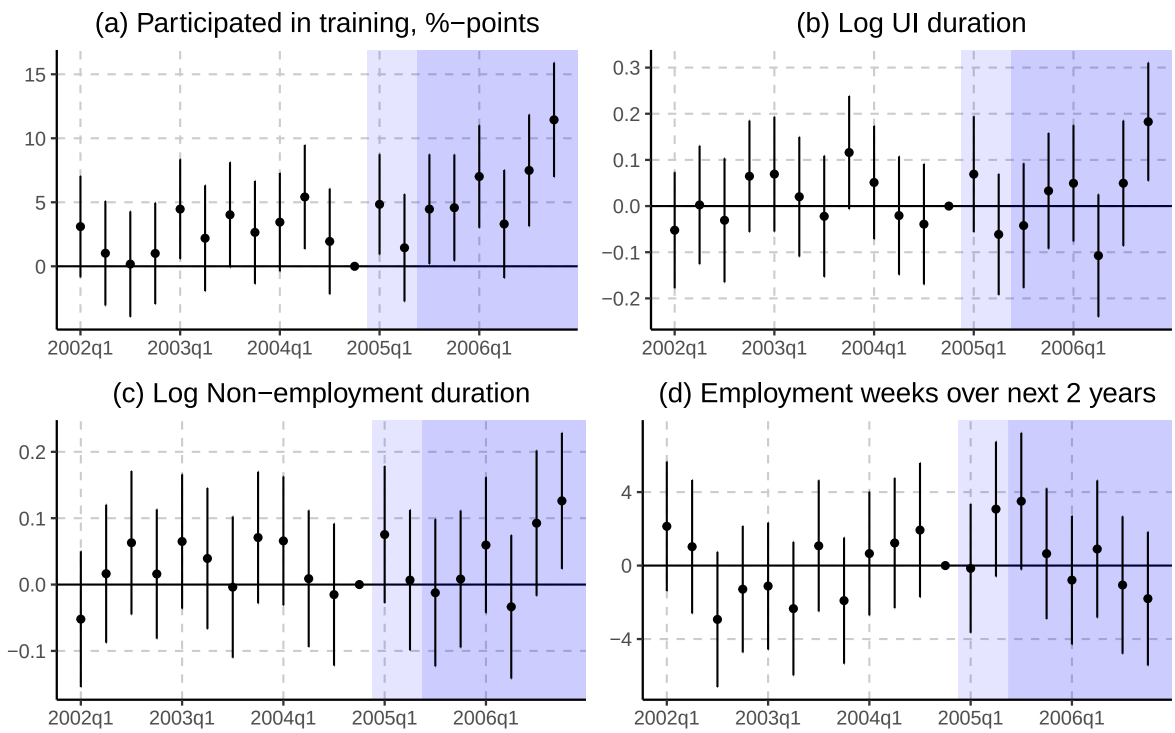
The key parameters of interest are $\delta_t$, which capture the effect of eligibility for the RE program (i.e., the intention-to-treat effect) on $\tilde{Y}_{it}$ over time. As a normalization, we set $\delta_{2004q4} = 0$, so that the program eligibility effects $\delta_t$ are measured relative to the last quarter of 2004, and $\theta$ captures the difference between the treatment and control group in that quarter. Although the reform took effect in July 2005, some limited effects may already be present in the first half of the year. This is because workers in the treatment group whose UI spells began between February 15 and June 30, 2005, became eligible for the program starting in July 2005, potentially leading to early effects before the official implementation.

The estimates of $\delta_t$ along with their 95% confidence intervals are plotted in Figure 8. The effects in the pre-reform period help detect potential pre-existing trends and anticipatory effects. If the parallel trends assumption holds and there was no anticipatory behavior, these effects should be close to zero. The pre-reform effects (in the white area) are generally small and not statistically different from zero at the 5% level, except in two cases in panel (a). These results align with the visual evidence in Figure 7, supporting the validity of the parallel trends assumption in our DiD approach.

In panel (a), all post-reform effects (in the dark blue area) are positive, and almost all are statistically significant at the 5% level, indicating that the RE program increased participation in training programs during UI spells among eligible workers. The average effect on the participation rate over the post-reform period is 6.2 percentage points. However,

[9] The control variables include gender, age, education level, mother tongue (Finnish, Swedish or other), occupation, living region, unemployment fund, the time spent on UI benefits in the past two years, work history, job tenure, and wage decile.

Figure 8: Event-study effects of program eligibility




*Notes:* The graph plots the estimates of $\delta_t$ and their 95% confidence intervals from the model outlined in equation (1). The dark blue area indicates the post-reform period starting from the third quarter of 2005. UI spells that started in the light blue area (the first and second quarter of 2005) may or may not be affected by the reform from July 2005 onward, depending on the termination date of the previous job. The effects are estimated from group-wise deseasonalized data.

this effect may be slightly overestimated, as the difference in participation rates between the treatment and control groups in the reference period—the last quarter of 2004—is unusually small due to an anomalous drop in the treatment group's participation rate in December 2004 (see panel (a) of Figure 7). This anomaly also explains the two significant pre-reform effects. If the third quarter of 2004 were used as the reference period, three out of six post-reform effects would remain statistically significant, the average effect over the post-reform period would be 4.3 percentage points, and none of the pre-reform effects would be statistically different from zero at the 5% level.

In panels (b), (c), and (d), the post-reform effects on unemployment and employment outcomes vary in sign, and most are not statistically different from zero. These findings do not support the hypothesis that the RE program shortens UI spells or improves subsequent employment outcomes.

## 6.2 Pooled-data estimates and impact heterogeneity

To increase statistical power, we estimate the following simplified DiD specification:

$$Y_{it} = \alpha_t + X_{it}\beta + \theta Treat_{it} + \delta \left(Treat_{it} \cdot Post_{it}\right) + \varepsilon_{it}, \tag{2}$$

where $Post_{it}$ is an indicator for spells that began after the reform. In this model, $\delta$ captures the average effect of program eligibility on $Y_{it}$ in the post-reform period.

In addition, we report IV estimates of the effect of actual participation in the RE program. These estimates are derived from the following model

$$Y_{it} = \lambda_t + X_{it}\psi + \pi Treat_{it} + \mu Enroll_{it} + \nu_{it}, \tag{3}$$

where $Enroll_{it}$ is an indicator for enrollment in the RE program, which we instrument with the interaction term $Treat_{it} \cdot Post_{it}$. The coefficient $\mu$ captures the average effect of program participation among participants. It equals to the ratio of the DiD estimate for $Y_{it}$—i.e., the estimated $\delta$ from the equation (2)—to the DiD estimate for $Enroll_{it}$.

When estimating these models, we exclude UI spells that began during the transition period between February 15 and June 31, 2005. The estimates of $\delta$ and $\mu$ are presented in Table 2 and visualized in Figure 9. In the first row of the table, we report estimates based on the full sample, followed by results for sub-samples categorized by sex, age, education, and pre-unemployment labor earnings. As a specification check, we also report placebo program eligibility effects, estimated using the pre-reform data only and assuming a placebo reform in July 2003. We do not report the first-stage estimates for the IV model, as Figure 4 clearly shows that our instrument is very strong.

All placebo effects in Table 2 are small and statistically insignificant at the 5% level, except for one estimate in column 16. These findings support the validity of the parallel trends assumption and indicate that our DiD approach remains appropriate when applied to various subgroups.

The results in columns 4 and 5 show that the reform encouraged participation in labor market training. Among the treatment group, the participation rate increased by 3.9 percentage points from a pre-reform level of 26.8%, representing a relative increase of 15%. For those who enrolled in the RE program, the participation rate rose by 6.7 percentage points. These effects were more pronounced for men, older workers, low-skill workers, and high-wage workers. However, these differences across groups should be interpreted with caution, as they are not statistically significant at conventional risk levels. That said, the findings suggest that the program particularly encouraged low-skill workers to update their skills, while having little impact on the participation rate of more educated individuals, despite their similar initial participation levels.

Older workers tend to remain unemployed for longer periods on average (42 weeks compared to 32 weeks for younger workers) and may be more likely to possess outdated skills. Notably, 29% of older workers have only completed compulsory education, compared to 15% of younger workers. These age-related differences in unemployment duration and educational attainment may explain why the reform resulted in an almost five percentage point increase in training participation among older workers.

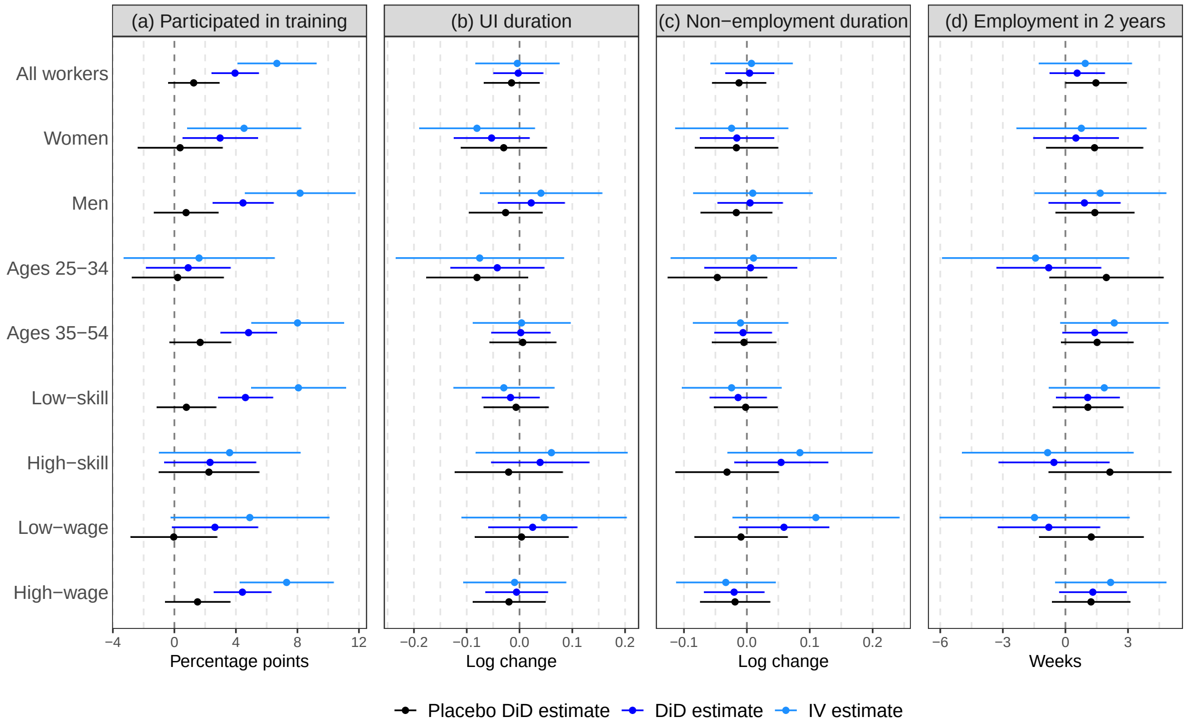
Men participate in labor market training programs less frequently than women. Before the reform, their participation rate was 9.3 percentage points lower than that of women.

Table 2: The effects of the RE program on training participation and re-employment outcomes

| | | Participated in training, %-points | | | | UI duration, log changes | | | | Non-employment duration, log changes | | | | Employment over 2 years, weeks | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | N (1) | Mean (3) | Placebo (4) | DiD (5) | IV (6) | Mean (7) | Placebo (8) | DiD (9) | IV (10) | Mean (11) | Placebo (12) | DiD (13) | IV (14) | Mean (15) | Placebo (16) | DiD (17) | IV (18) |
| All workers | 76,685 | 26.8% | 1.255 | 3.944*** | 6.662*** | 39.6 | -0.015 | -0.002 | -0.004 | 52.2 | -0.012 | 0.004 | 0.007 | 45.9 | 1.462* | 0.562 | 0.950 |
| | | | (0.846) | (0.779) | (1.305) | | (0.027) | (0.024) | (0.040) | | (0.022) | (0.020) | (0.033) | | (0.746) | (0.672) | (1.135) |
| Women | 38,959 | 32.5% | 0.371 | 2.972** | 4.529** | 43.6 | -0.030 | -0.053 | -0.081 | 58.4 | -0.017 | -0.016 | -0.024 | 41.4 | 1.396 | 0.501 | 0.764 |
| | | | (1.403) | (1.245) | (1.886) | | (0.041) | (0.036) | (0.056) | | (0.034) | (0.030) | (0.046) | | (1.182) | (1.042) | (1.588) |
| Men | 37,726 | 23.2% | 0.759 | 4.461*** | 8.173*** | 37.0 | -0.026 | 0.022 | 0.041 | 48.1 | -0.017 | 0.005 | 0.009 | 48.8 | 1.411 | 0.911 | 1.670 |
| | | | (1.066) | (1.008) | (1.829) | | (0.035) | (0.032) | (0.059) | | (0.029) | (0.026) | (0.048) | | (0.961) | (0.876) | (1.607) |
| Ages 25–34 | 24,258 | 22.1% | 0.214 | 0.896 | 1.605 | 32.2 | -0.081 | -0.042 | -0.075 | 46.8 | -0.047 | 0.006 | 0.011 | 51.9 | 1.962 | -0.802 | -1.435 |
| | | | (1.518) | (1.399) | (2.500) | | (0.049) | (0.045) | (0.081) | | (0.040) | (0.038) | (0.067) | | (1.393) | (1.276) | (2.283) |
| Ages 35–54 | 52,427 | 28.6% | 1.676* | 4.822*** | 8.007*** | 42.4 | 0.006 | 0.002 | 0.004 | 54.2 | -0.005 | -0.006 | -0.010 | 43.6 | 1.519* | 1.410* | 2.341* |
| | | | (1.018) | (0.931) | (1.530) | | (0.032) | (0.028) | (0.047) | | (0.026) | (0.023) | (0.039) | | (0.884) | (0.794) | (1.320) |
| Low-skill | 57,693 | 26.8% | 0.779 | 4.619*** | 8.063*** | 41.0 | -0.007 | -0.017 | -0.030 | 53.4 | -0.002 | -0.014 | -0.024 | 43.4 | 1.078 | 1.067 | 1.862 |
| | | | (0.982) | (0.907) | (1.568) | | (0.031) | (0.028) | (0.049) | | (0.026) | (0.023) | (0.040) | | (0.861) | (0.776) | (1.356) |
| High-skill | 18,992 | 26.8% | 2.244 | 2.319 | 3.591 | 35.8 | -0.021 | 0.039 | 0.060 | 48.9 | -0.032 | 0.054 | 0.084 | 52.5 | 2.133 | -0.551 | -0.853 |
| | | | (1.662) | (1.519) | (2.343) | | (0.052) | (0.047) | (0.073) | | (0.042) | (0.038) | (0.059) | | (1.499) | (1.353) | (2.095) |
| Low-wage | 38,227 | 24.8% | -0.042 | 2.635* | 4.904* | 39.5 | 0.004 | 0.025 | 0.046 | 53.2 | -0.009 | 0.059 | 0.110 | 43.6 | 1.239 | -0.797 | -1.483 |
| | | | (1.434) | (1.424) | (2.628) | | (0.045) | (0.043) | (0.080) | | (0.038) | (0.036) | (0.068) | | (1.275) | (1.247) | (2.319) |
| High-wage | 38,458 | 27.8% | 1.506 | 4.424*** | 7.297*** | 39.6 | -0.020 | -0.006 | -0.009 | 51.7 | -0.019 | -0.020 | -0.033 | 47.0 | 1.228 | 1.313 | 2.166 |
| | | | (1.078) | (0.949) | (1.554) | | (0.035) | (0.030) | (0.050) | | (0.028) | (0.024) | (0.040) | | (0.954) | (0.821) | (1.355) |

*Notes:* Spells started between February 15 and June 31, 2005 are excluded. N = number of observations. Mean = the average pre-reform outcome for the treatment group (percents in column 3, and weeks in columns 7, 11 an 15). DiD estimates are the estimates of $\delta$ from the model in equation (2) and IV estimates are the estimates of $\mu$ from the model in equation (3). Placebo estimates are DiD estimates obtained from the pre-reform data over the years 2002–2004 and assuming a placebo reform on July 1, 2003. High-skill workers include those with a Tertiary degree or higher. High-wage workers include those whose past labor earnings exceeds the sample median. All models include quarter-by-year time effects and controls for gender, age, education, occupation, mother tongue, living region, unemployment fund, the time spent on UI benefits in the past two years, work experience, job tenure, and wage decile. Standard errors clustered at the individual level are reported in the parentheses. Significance levels: *** 1%. ** 5% and * 10%.

Figure 9: The effects of the RE program on training participation and re-employment outcomes




*Notes:* DiD estimates are the estimates of $\delta$ from the model in equation (2) and IV estimates are the estimates of $\mu$ from the model in equation (3). Placebo estimates are DiD estimates obtained from the pre-reform data over the years 2002–2004 and assuming a placebo reform on July 1, 2003. Horizontal lines depict the 95% confidence intervals.

The larger impact of the RE program on men's participation rate may be partly due to their lower baseline participation level.

As discussed in Section 3.1, the RE program provides greater benefit increases for those with higher past wages. In the treatment group, the average potential benefit increase was 30% for high-wage workers and 21% for low-wage workers. A slightly stronger effect for high-wage workers suggests that financial incentives may influence job seekers' decisions to participate in labor market training programs.

The effects on UI and non-employment duration are generally small and not statistically significant at conventional risk levels. In columns 17 and 18, we find marginally significant effects on employment over the following two-year period for older workers. However, these effects are relatively small, corresponding to a 3–5% increase in employment. Moreover, the DiD estimate is close to the corresponding placebo effect, implying that the observed effect may be spurious and driven by mild pre-existing trend differences between older workers in the treatment and control groups.

Although the effects for subgroups are somewhat noisy, the estimates for all workers are reasonably precise. Based on the 95% confidence intervals of the program eligibility effects in columns 9 and 13, we can rule out reductions greater than 4.9% in UI duration and greater than 4.3% in non-employment duration due to the RE program among all eligible workers. Thus, if the program reduced UI or non-employment duration, these effects must be very small and quantitatively unimportant.

In summary, the results indicate that the RE program increased participation in labor market training across nearly all subgroups. However, it had no impact on the re-employment prospects of any group.

### 6.3 Robustness

Table 3 presents the results of robustness checks. The first row re-reports our baseline estimates from Table 2. The second row shows results from models excluding the control variables $X_{it}$. Since these estimates are close to the baseline results, our results are not sensitive to the inclusion of a large set of control variables. The fact that our results remain unaffected by observed changes in sample composition over time reduces the likelihood of significant changes in unobserved characteristics between the treatment and control groups. This further supports the validity of our research design.

As discussed in Section 2.1, workers with at least 20 years of work history who were laid off for economic reasons became eligible for higher UI benefits for up to 30 weeks in 2003. Since UI recipients could not receive both this long-work-history supplement and the RE program supplement simultaneously, the long-work-history supplement may have reduced financial incentives to enroll in the RE program and to participate in labor market training among those who did enroll. Moreover, because nearly all workers entitled to the long-work-history supplement belonged to the treatment group, its introduction in 2003 increased the average UI benefits in the treatment group but not in the control group.

To account for this, we replicate our analysis by restricting the sample to workers with less than 20 years of work history. Using this subsample, the DiD and IV estimates for the increase in labor market training participation are one-third smaller than the baseline estimates (columns 5 and 6). This suggests that the RE program had a particularly strong effect on participation rates among workers with long work histories, which may seem counterintuitive from a financial incentive perspective. In the post-reform period, 69% of treatment group members who received the long-work-history supplement also enrolled in the RE program, and 39% participated in labor market training. These relatively high participation rates may be due to longer unemployment spells in this subgroup, as their average UI duration is 48 weeks, suggesting a greater need for employment services. Thus, eligibility for the long-work-history supplement did not appear to have reduced enrollment in the RE program or participation in labor market training. Instead, workers with long employment histories actively engaged in these programs, possibly due to outdated skills and a heightened risk of long-term unemployment.

Also, differential seasonal patterns may influence the results. In the control group, unemployment inflows peaked in early summer, and UI spells that began during this period were generally much shorter than those starting at other times. This seasonality raises concerns about whether individuals in the control group who entered unemployment

Table 3: Robustness analysis

| | | Participated in training, %-points | | | | UI duration, log changes | | | | Non-employment duration, log changes | | | | Employment over 2 years, weeks | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | N (1) | Mean (3) | Placebo (4) | DiD (5) | IV (6) | Mean (7) | Placebo (8) | DiD (9) | IV (10) | Mean (11) | Placebo (12) | DiD (13) | IV (14) | Mean (15) | Placebo (16) | DiD (17) | IV (18) |
| Baseline sample with controls | 76,685 | 26.8% | 1.255 | 3.944*** | 6.662*** | 39.6 | -0.015 | -0.002 | -0.004 | 52.2 | -0.012 | 0.004 | 0.007 | 45.9 | 1.462* | 0.562 | 0.950 |
| | | | (0.846) | (0.779) | (1.305) | | (0.027) | (0.024) | (0.040) | | (0.022) | (0.020) | (0.033) | | (0.746) | (0.672) | (1.135) |
| Baseline sample without controls | 76,685 | 26.8% | 1.218 | 4.175*** | 7.039*** | 39.6 | -0.024 | -0.004 | -0.007 | 52.1 | -0.020 | 0.000 | 0.000 | 45.9 | 1.848** | 0.396 | 0.667 |
| | | | (0.865) | (0.799) | (1.333) | | (0.028) | (0.025) | (0.041) | | (0.023) | (0.020) | (0.034) | | (0.769) | (0.689) | (1.162) |
| Work history less than 20 years | 54,539 | 25.6% | 1.559 | 2.775*** | 4.774*** | 37.1 | -0.030 | -0.034 | -0.058 | 49.9 | -0.016 | -0.002 | -0.004 | 48.4 | 1.580* | 0.145 | 0.250 |
| | | | (1.058) | (0.983) | (1.681) | | (0.033) | (0.030) | (0.052) | | (0.027) | (0.025) | (0.043) | | (0.938) | (0.852) | (1.466) |
| June and July UI spells excluded | 60,135 | 26.7% | 0.847 | 4.097*** | 6.736*** | 39.3 | -0.044 | -0.015 | -0.025 | 51.9 | -0.012 | -0.004 | -0.007 | 46.0 | 0.818 | 0.303 | 0.498 |
| | | | (0.938) | (0.868) | (1.416) | | (0.030) | (0.027) | (0.044) | | (0.024) | (0.022) | (0.036) | | (0.819) | (0.738) | (1.214) |
| Temporary worker treatment group | 62,746 | 16.8% | -0.366 | 0.912 | 3.050 | 31.4 | -0.121* | -0.107** | -0.358** | 44.4 | -0.053 | -0.076** | -0.255* | 52.8 | 1.311 | 0.877 | 2.933 |
| | | | (1.754) | (1.256) | (4.175) | | (0.062) | (0.046) | (0.159) | | (0.051) | (0.039) | (0.132) | | (1.748) | (1.289) | (4.333) |

*Notes:* Spells started between February 15 and June 31, 2005 are excluded. N = number of observations. Mean = the average pre-reform outcome for the treatment group (percents in column 3, and weeks in columns 7, 11 an 15). DiD estimates are the estimates of $\delta$ from the model in equation (2) and IV estimates are the estimates of $\mu$ from the model in equation (3). Placebo estimates are DiD estimates obtained from the pre-reform data over the years 2002–2004 and assuming a placebo reform on July 1, 2003. Apart from the second model, all models include quarter-by-year time effects and controls for gender, age, education, occupation, mother tongue, living region, unemployment fund, the time spent on UI benefits in the past two years, work experience, job tenure, and wage decile. Standard errors clustered at the individual level are reported in the parentheses. Significance levels: *** 1%. ** 5% and * 10%.

in early summer are appropriate comparisons for the treatment group. To address this issue, we exclude all spells starting in June or July from the analysis. The resulting estimates remain very close to our baseline findings for all outcomes, indicating that seasonal differences do not affect our conclusions.

Lastly, we examine a distinct treatment group: temporary workers whose fixed-term contracts expired after they had worked at least 36 months within the past 42 months at the same firm. Although these workers were eligible for the RE program, we excluded them from the main analysis because they differ substantially from eligible workers who were laid off for economic reasons. Prior to the reform, eligible temporary workers experienced shorter UI spells (31 versus 40 weeks) and participated less frequently in labor market training programs (17% versus 27%) compared to eligible workers who were laid off. After the reform, only 31% of eligible temporary workers enrolled in the RE program, compared to 61% of the treatment group in our main analysis. Their lower enrollment rate may suggest a lower need for employment services, or it might reflect differences in the job termination process. Unlike layoffs—which typically involve discussions with labor union representatives and prior contact with the PES—the non-renewal of fixed-term contracts requires no employer action, possibly leaving temporary workers less informed about their eligibility for the RE program.

Table 3 presents findings for this treatment group, but the results are imprecise due to the relatively small sample (3,680 treated individuals, with 461 enrolling in the RE program). Notably, the RE program did not boost participation in training programs (columns 5 and 6) despite an initially low participation rate (column 2). In contrast, the effects on UI and non-employment durations are negative and statistically significant. The IV estimates indicate substantial reductions—30% in UI duration and 23% in non-employment duration—for those who enrolled in the program. Since training participation remained unchanged, these findings appear to suggest that temporary workers may have benefited significantly from early counseling and caseworker meetings. However, this interpretation should be approached with caution. The similarity of the placebo and DiD estimates (columns 8 and 9, and columns 12 and 13) implies that the estimates are likely spurious and driven by differential trends between the treatment and control groups. Indeed, adding a linear trend for the treatment group would kill all significant effects on UI and non-employment durations (not reported in the table).

# 7 Mechanisms

The overall null effect of the RE program on UI duration and employment may conceal the opposite effects of increased caseworker meetings, higher benefits, and greater participation in labor market training. To explore these underlying channels, we examine how the exit rate from UI benefits to employment changes when a job seeker begins to receive higher UI benefits, signs a job-search or employment plan, or participates in labor market training. This analysis is mainly descriptive, as the timings of these events during UI spells are not random and the 2005 reform does not provide exogenous variation to address these selection issues. However, we account for non-random selection into labor market training programs by employing the timing-of-event approach of Abbring & van den Berg (2003).

## 7.1 Timing-of-events model

To illustrate the selection issues, consider the following stylized version of the re-employment hazard (which abstracts from differences between the treatment and control groups, as well as the reform effect):

$$\theta_e(t|X, Plan, D_0, D_1, \upsilon_e) = \lambda_e(t) \exp\left\{X(t)\beta_e + \gamma_e Plan(t) + \eta_0 D_0\left(t\right) + \eta_1 D_1\left(t\right) + \upsilon_e\right\}, \tag{4}$$

where $t$ is the elapsed UI duration; $\lambda_e(t)$ is the baseline hazard, capturing duration dependence; $X(t)$ is a vector of control variables, some of which vary with elapsed UI duration or calendar time; $Plan(t)$ is a time-varying indicator that equals one if the job seeker has drafted a job-search or employment plan by time $t$; $D_0\left(t\right)$ is a time-varying indicator that equals one if the job seeker is participating in labor market training at time $t$; $D_1\left(t\right)$ is a time-varying indicator that equals one if the job seeker has completed a training program by time $t$; and $\upsilon_e$ is an unobserved heterogeneity term, capturing the effects of unmeasured characteristics, such as skills and search effort.

Participation in training programs is unlikely to be random. For example, job seekers with strong re-employment prospects (i.e., high values of $\upsilon_e$) may be less inclined to participate in training programs, whereas those who are generally more proactive may both find a job more quickly and be more likely to enroll in training. As a result, $D_0\left(t\right)$ and $D_1\left(t\right)$ are likely correlated with $\upsilon_e$. To address this selection problem, we also model transitions from unemployed job search to labor market training during the UI spell (i.e., the time when $D_0\left(t\right)$ switches from 0 to 1). Specifically, we specify the labor market training hazard as

$$\theta_d(t|X, Plan, \upsilon_p) = \lambda_d(t) \exp\left\{X(t)\beta_d + \gamma_d Plan(t) + \upsilon_d\right\}, \tag{5}$$

where $\upsilon_d$ is an unobserved heterogeneity term that can be correlated with $\upsilon_u$.

The bivariate duration model defined by equations (4) and (5) is known as the timing-of-events model. This model allows us to estimate the causal effect of labor market training on the re-employment hazard under relatively weak identifying assumptions. As shown by Abbring & van den Berg (2003), when we have single-spell data (i.e., one UI spell per individual) and time-invariant covariates $X$, the model is identified under the following assumptions: $(i)$ the hazard rates are of the mixed proportional hazard (MPH) form,[10] (*ii*) job seekers do not know the exact starting dates of their labor training periods in advance ("the no-anticipation assumption"), and (*iii*) the covariates $X$ are independent of the unobserved heterogeneity terms. Notably, identification of the model does not require parametric assumptions on the baseline hazard functions $\lambda_d(t)$ and $\lambda_d(t)$ or on the distribution of the unobservables $\upsilon_e$ and $\upsilon_d$. Furthermore, the identification does not require exclusion restrictions on the set of covariates $X$.

Although job seekers know the scheduled starting dates for training courses when they apply, they remain uncertain about the exact dates of their own participation because not every application results in enrollment. In fact, many job seekers submit multiple applications to different training programs, sometimes with overlapping schedules. For popular programs, some applications are rejected due to limited capacity, while less popular programs may be canceled because of too few applications. During our analysis period, the annual acceptance rate for training programs ranged between 53% and 60%. Moreover, applicants typically receive their acceptance notification only one or two weeks before the program begins. Given these factors, the timing of labor market training at the individual level is subject to considerable uncertainty, making the no-anticipation assumption plausible.

When time-varying covariates and multiple-spell data are available, as in our study, the assumptions regarding the MPH structure and the independence of observed and unobserved characteristics can be relaxed. Job seekers entering unemployment at different times experience different business cycle and seasonal conditions over any given UI duration, providing a robust source of variation to separate the impact of unobserved heterogeneity from other factors.[11] We take advantage of this variation by incorporating time-varying quarter-by-year fixed effects into $X(t)$. Furthermore, in our sample, 15% of individuals experienced at least two UI spells. The availability of time-varying covariates and multiple-spell data for some individuals provide additional variation so that our estimation results do not hinge critically on the assumptions of the MPH structure and covariate independence.

However, some caveats should be noted. First, the model accounts only for the potential endogeneity of the *timing* of training programs, while ignoring any additional endogeneity related to the *duration* of these programs. Second, we only model exits to employment—accounting for 78% of all exits—and treat UI spells ending for other reasons as right-censored. We have also estimated a more complex model that explicitly models both the duration of training programs and exits to inactivity. Since the results from this extended model are similar to those from the simpler specification, we report only the latter here; the results from the more complex model are available upon request.

[10] The MPH structure means that the hazard rate depends multiplicatively on the elapsed duration, observed covariates and unobserved heterogeneity: $\theta(t|X, \upsilon) = \lambda(t)\phi(X)\upsilon$.

[11] At time $t > 0$, the distribution of unobserved heterogeneity depends on the proportion of individuals who are still unemployed while individual hazards rates do not. It follows that time-varying covariates provide an implicit exclusion restriction, as their past values affect the current hazard rates only through the selection process (Gaure et al. 2007)

A third caveat concerns the variable $Plan(t)$, which is potentially endogenous. The timing-of-events approach cannot address this selection problem because it does not handle selection at the inflow stage, and a large share of job seekers met with a caseworker before their UI spell began (i.e., for many individuals, $Plan(0) = 1$). Consequently, we cannot interpret $\gamma_e$ and $\gamma_d$ as causal effects.

The stylized hazard functions presented above do not account for differences between the treatment and control groups or for changes following the 2005 reform. In practice, we estimate a DiD version of the timing-of-events model. Let $Treat$ be an indicator for the treatment group, and $Post$ an indicator for UI spells that began in the post-reform period. To allow the shape of the hazard functions to differ between groups, we specify distinct baseline hazards for the treatment and comparison groups. In particular, we include $Treat$ as an explanatory variable in both hazard rates, allowing its coefficient to vary with the elapsed UI duration. We also incorporate $Post$ and the interaction $Post \cdot Treat$ to capture group-specific shifts in the hazard rates following the reform. Furthermore, we allow the effects of $Plan(t)$, $D_0(t)$, and $D_1(t)$ to vary across groups and between pre- and post-reform periods. This is achieved by interacting these variables with $Treat$, $Post$, and $Post \cdot Treat$. Our primary interest lies in the coefficients of the triple-interaction terms, which capture the differential changes in the effects of $Plan(t)$, $D_0(t)$, and $D_1(t)$ following the reform in the treatment group compared to the control group.

Our estimation sample differs slightly from that used in previous analyses, as we exclude UI spells that began with labor market training (1.7% of UI spells). As before, we follow job seekers for a maximum of two years and censor ongoing UI spells at the two-year mark (3.7% of UI spells). For simplicity—and because multiple labor market training periods are rare—we model only the effect of the first training program, censoring UI spells at the onset of a potential second program (2.3% of UI spells).

We estimate the model using maximum likelihood, approximating both the baseline hazard functions and the unknown joint distribution of the unobserved heterogeneity terms in a non-parametric manner. For further details, see the Appendix.

## 7.2 Results

Table 4 reports estimates of the key parameters of the hazard functions. For comparison purposes, columns 1 and 2 present the results when the hazard functions are estimated separately, without unobserved heterogeneity. Columns 3 and 4 show the results from the timing-of-event model, which provides selectivity-corrected estimates of the effects of labor market training. Unless otherwise stated, the discussion below focuses on the timing-of-events estimates in columns 3 and 4.

The coefficient on $Post$ in the top row indicates that individuals who became unemployed in the post-reform period exited to employment at a higher rate than those who became unemployed earlier. This effect is the same for both the treatment and control groups, as the coefficient on $Post \cdot Treat$ is statistically insignificant. There is no significant change in the transition rate to labor market training programs for UI spells that began after the reform.

The models include controls for the past monthly labor earnings (decile dummies), which determine the level of the regular UI benefit, as well as time-varying indicators for benefit supplements based on participation in the RE program and a long work history of over 20 years. The coefficients on the time-varying benefit supplement indicators are reported in the table. The results suggest that higher benefits encourage participation in labor market training while discouraging exits to employment. The effect of the long-work-history supplement can be regarded as causal, as its variation is driven by the length of work history, the timing of unemployment entry, and reason for job loss (the supplement was introduced in 2003 and was available only to those laid off for economic reasons). However, the same does not hold for the RE program supplement, because not all eligible workers enrolled in the program during the post-reform period, even though the vast majority did. As a result, its estimated effect is likely subject to selection bias.

Also, the estimated effects of caseworker meetings, measured by the timing of job-search and employment plan drafting, are likely biased due to selection and should be interpreted with caution. Nevertheless, it is interesting to note that caseworker meetings increase transitions to labor market training while reducing exits to employment. The first effect is unsurprising, as the job-search and employment plans outline the training programs and other employment services that job seekers are expected to apply for. However, the reduction in exits to employment is less intuitive. One might expect these meetings to provide useful information about job opportunities, thereby increasing the re-employment hazard. Yet the opposite effect suggests that after meeting with a caseworker, job seekers may shift their focus from job searching to training programs. Alternatively, this effect could be driven by selection bias, as caseworkers are less likely to meet with job seekers they believe have strong re-employment prospects.

Before the reform, the effects of caseworker meetings on the hazard rates for labor market training and re-employment were similar for the treatment and control groups, as indicated by insignificant coefficients on the interaction $Plan(t) \cdot Treat$ in columns 3 and 4. While these effects remained stable over time in the control group, the effect on the labor market training hazard in the treatment group increased notably from 0.479 to 0.710 ($= 0.479 + 0.231$) following the reform. This finding suggests that higher benefits for the duration of the training programs specified in the employment plan for those who enrolled in the RE program encouraged training participation. Since transitions to labor market training did not generally increase in the treatment group after the reform (as indicated by the insignificant coefficient on $Post \cdot Treat$ in column 3), these results confirm that the increase in training participation in the treatment group documented in the previous sections was entirely driven by the combination of higher benefits and increased caseworker meetings among those enrolled in the RE program.

Labor market training programs have strong lock-in effects, reducing the re-employment

Table 4: Hazard model estimates

| | Separate hazard models without unobserved heterogeneity | | Timing-of-events model | |
|---|---|---|---|---|
| | Training hazard (1) | Re-employment hazard (2) | Training hazard (3) | Re-employment hazard (4) |
| Post | -0.075 | 0.188*** | -0.077 | 0.170*** |
| | (0.077) | (0.032) | (0.081) | (0.040) |
| Post × Treat | -0.145 | 0.042 | -0.150 | 0.056 |
| | (0.097) | (0.040) | (0.100) | (0.047) |
| *Effects of benefit supplements* | | | | |
| Re-employment Program ($t$) | 0.443*** | -0.165*** | 0.409*** | -0.144** |
| | (0.082) | (0.055) | (0.083) | (0.058) |
| Long work history ($t$) | 0.270*** | -0.251*** | 0.294*** | -0.251*** |
| | (0.047) | (0.026) | (0.048) | (0.030) |
| *Effects of caseworker meetings* | | | | |
| Plan($t$) | 0.460*** | -0.181*** | 0.479*** | -0.242*** |
| | (0.038) | (0.013) | (0.039) | (0.016) |
| Plan($t$) × Treat | -0.092* | 0.021 | -0.078 | 0.040 |
| | (0.055) | (0.026) | (0.057) | (0.031) |
| Plan($t$) × Post | 0.025 | -0.037* | 0.016 | -0.031 |
| | (0.066) | (0.022) | (0.067) | (0.027) |
| Plan($t$) × Treat × Post | 0.211* | 0.058 | 0.231** | 0.063 |
| | (0.111) | (0.051) | (0.116) | (0.060) |
| *Lock-in effects of training* | | | | |
| $D_0(t)$ | | -0.956*** | | -1.094*** |
| | | (0.039) | | (0.048) |
| $D_0(t)$ × Treat | | 0.477*** | | 0.642*** |
| | | (0.059) | | (0.067) |
| $D_0(t)$ × Post | | -0.000 | | -0.021 |
| | | (0.065) | | (0.072) |
| $D_0(t)$ × Treat × Post | | -0.011 | | -0.021 |
| | | (0.105) | | (0.116) |
| *Effects of completed training* | | | | |
| $D_1(t)$ | | 0.240*** | | 0.179*** |
| | | (0.033) | | (0.047) |
| $D_1(t)$ × Treat | | 0.107** | | 0.329*** |
| | | (0.053) | | (0.064) |
| $D_1(t)$ × Post | | -0.082 | | -0.093 |
| | | (0.057) | | (0.069) |
| $D_1(t)$ × Treat × Post | | 0.019 | | 0.012 |
| | | (0.088) | | (0.103) |

*Notes*: Spells started between February 15 and June 31, 2005 are excluded. The number of observations (i.e. UI spells) for all models is 75,404. Models in columns 1 and 2 do not contain unobserved heterogeneity terms. The timing-of-events model in columns 3 and 4 involves the discrete heterogeneity distribution with $3 \times 2$ points of support. The piecewise-constant baseline hazard for re-employment includes eleven duration intervals (6 x 1-month, 4 x 3-months, and an open-ended interval), and the baseline hazard for transitions to labor market training includes six duration intervals (5 x 3-months, and an open-ended interval). All hazards include time-varying quarter-by-year effects and controls for gender, age, education, occupation, mother tongue, living region, unemployment fund, the time spent on UI benefits in the past two years, work experience, job tenure, and wage decile. Standard errors are reported in the parentheses. Significance levels: *** 1%, ** 5%, and * 10%.

hazard by 67% in the control group ($=100 \cdot [1 - e^{-1.094}]$) and by 36% in the treatment group ($=100 \cdot [1 - e^{-1.094+0.642}]$). The smaller lock-in effect for the treatment group arises from their lower counterfactual re-employment hazard without training, while re-employment hazards during labor market training are roughly the same across both groups (see Appendix Figure A1). Completing a training program increases the re-employment hazard in both groups compared to the counterfactual scenario where the job seeker would have not participated in labor market training by time $t$. This post-training effect is 66% in the treatment group ($= 100 \cdot e^{0.179+0.329}$) and 20% in the control group ($= 100 \cdot e^{0.179}$). With a smaller lock-in effect and a stronger post-training effect, the treatment group benefits relatively more from labor market training programs than the control group.

Comparing the estimates in columns 2 and 4 reveals that correcting for selection bias slightly reduces the difference in lock-in effects but substantially increases the gap in post-training effects between the treatment and control groups. Specifically, the selectivity-corrected estimates suggest a more positive net effect of labor market training for the treatment group.

The effects of labor market training remained stable over time in both groups. An increase in the post-training effect for the treatment group relative to the control group after the reform could have been interpreted as evidence of improved matching between job seekers and training programs due to the RE program. However, the coefficient on $D_1(t) \cdot Treat \cdot Post$ in column 4 is close to zero, suggesting that caseworkers were not successful in directing RE program participants toward training programs that would have been particularly beneficial for them. A closer look at training program types does not reveal any changes in the distribution of the program types in the treatment group following the reform (see Appendix Table A1).

# 8 Conclusions

The Finnish RE program aimed to promote re-employment among displaced workers by advancing the timing of job-search assistance and encouraging participation in labor market training through financial incentives. Our findings show that the program substantially increased early engagement with employment services, as reflected in earlier and more frequent caseworker meetings, and led to a significant increase in training participation, particularly among men, older workers, and those with lower skill levels. Despite these behavioral changes, we find no evidence that the program improved labor market outcomes over a two-year horizon.

Our duration analysis implies that increased caseworker interactions directed job seekers toward training programs but did not increase direct transitions to employment. The reform did not affect the returns to training participation, indicating no improvement in matches between job seekers and training options. These findings point to a limited ability of caseworkers to use financial incentives to guide individuals toward more effective programs.

Taken together, our results underscore the importance of targeting and implementation quality in active labor market policies. Simply increasing caseworker interactions and training participation does not necessarily improve employment outcomes. Future policy efforts may benefit from greater investment in caseworker training and the development of more effective tools for matching job seekers with suitable job openings and training opportunities.

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# A Estimation of the timing-of-events model

The DiD version of the re-employment hazard is

$$\begin{aligned}\theta_e(t|\upsilon_e) = \lambda_e(t)\exp\{&X(t)\beta_e + \alpha_{e1}(t)Treat + \alpha_{e2}Post + \alpha_{e3}Post\cdot Treat\\&+\gamma_{e1}Plan(t) + \gamma_{e2}Plan(t)\cdot Treat + \gamma_{e3}Plan(t)\cdot Post + \gamma_{e3}Plan(t)\cdot Treat\cdot Post\\&+\eta_{01}D_0\left(t\right) + \eta_{02}D_0\left(t\right)\cdot Treat + \eta_{03}D_0\left(t\right)\cdot Post + \eta_{04}D_0\left(t\right)\cdot Treat\cdot Post\\&+\eta_{11}D_1\left(t\right) + \eta_{12}D_1\left(t\right)\cdot Treat + \eta_{13}D_1\left(t\right)\cdot Post + \eta_{14}D_1\left(t\right)\cdot Treat\cdot Post + \upsilon_e\},\end{aligned}$$

and the DiD version of the labor market training hazard is

$$\begin{aligned}\theta_d(t|\upsilon_p) = \lambda_d(t)\exp\{&X(t)\beta_d + \alpha_{d1}(t)Treat + \alpha_{d2}Post + \alpha_{d3}Post\cdot Treat + \gamma_{d1}Plan(t)\ ,\\&+\gamma_{d2}Plan(t)\cdot Treat + \gamma_{d3}Plan(t)\cdot Post + \gamma_{d4}Plan(t)\cdot Treat\cdot Post + \upsilon_d\}\,.\end{aligned}$$

For ease of exposition, we emphasize conditioning on the unobserved heterogeneity terms $\upsilon_e$ and $\upsilon_p$, as these must be integrated out of the likelihood function. The hazard functions are always conditional on the observed factors.

The contribution of an individual with $K$ UI spells to the likelihood function is given by

$$L = \iint \prod_{j=1}^{K} L_j\left(\upsilon_e, \upsilon_d\right) dG\left(\upsilon_e, \upsilon_d\right),$$

where $G$ is the joint distribution function of $\upsilon_e$ and $\upsilon_p$, and

$$L_j\left(\upsilon_e, \upsilon_d\right) = \theta_e(t_{ej}|\upsilon_e)^{C_{ej}}\theta_d(t_{dj}|\upsilon_p)^{C_{dj}}\exp\left\{-\int_0^{t_{ej}}\theta_e(u|\upsilon_e)du - \int_0^{t_{dj}}\theta_d(u|\upsilon_d)du\right\},$$

where $t_{ej}$ is the realized duration of the $j$th UI spell; $t_{dj}$ is the time until the start of a labor market training program during the $j$th UI spell; $C_{ej}$ is a non-censoring indicator equal to 1 if the job seeker exits to employment at time $t_{ej}$; and $C_{dj} = 1\{t_{dj} < t_{ej}\}$ is an indicator that equals 1 if the job seeker enters a labor market training program at time $t_{dj}$ before the termination of the UI spell. The unobserved heterogeneity terms $\upsilon_e$ and $\upsilon_p$ are assumed to be remain constant across different UI spells for the same individual.

The baseline hazards are specified as piecewise constant functions. Specifically, $\lambda_e(t)$ is defined over eleven duration intervals: six 1-month intervals, followed by four 3-month intervals, and a final open-ended interval. For $\lambda_d(t)$, six intervals are used: five 3-month intervals and one open-ended interval.

The coefficients on the treatment group indicator, $\alpha_{e1}(t)$ and $\alpha_{d1}(t)$, are allowed to vary across the same duration intervals as the respective baseline hazards. This parametrization effectively allows for different baseline hazards between the treatment and control groups.

To approximate the unknown joint distribution of $\upsilon_e$ and $\upsilon_p$, we adopt a nonparametric specification using a bivariate discrete distribution. We re-estimate the model by adding support points for the distribution $G$, employing a limited-memory modification of the BFGS quasi-Newton algorithm from R's Optim function. The optimal number of support points is selected based on the Akaike information criterion, helping to guard against both under- and over-parametrization. The resulting model specification features three distinct values for $\upsilon_e$ and two values for $\upsilon_p$.

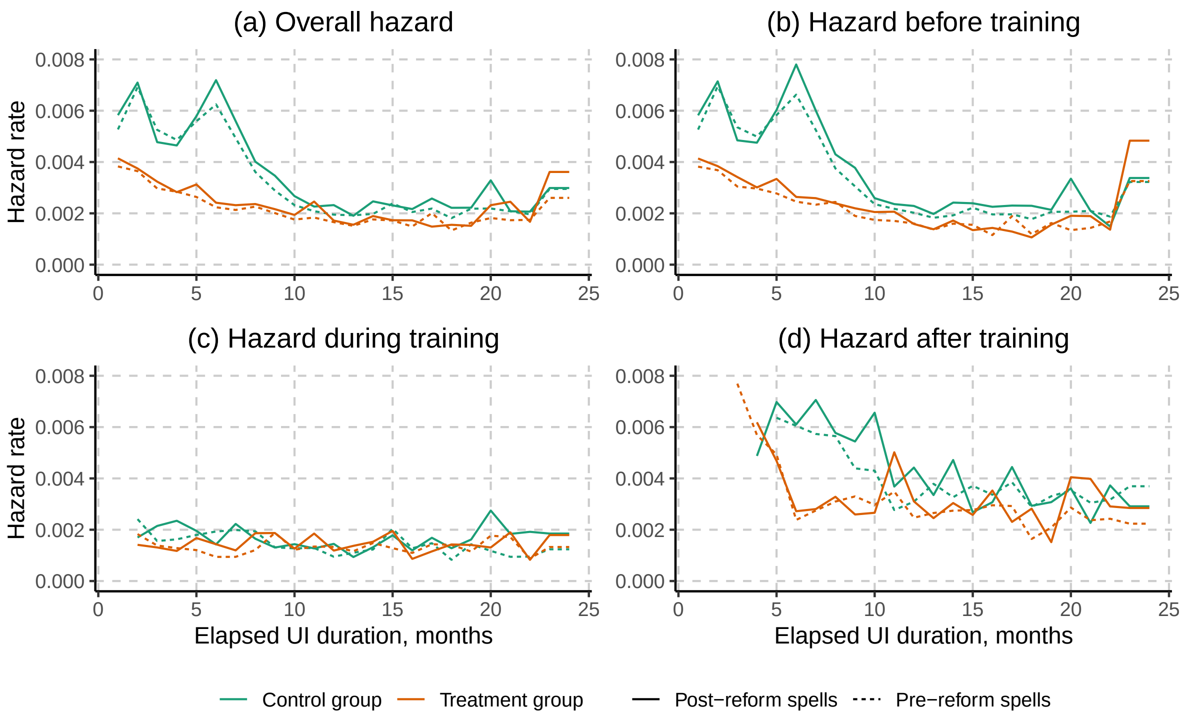
Monte Carlo studies of Gaure et al. (2007) and Lombardi et al. (2025) show that the timing-of-events approach effectively eliminates selection bias and yields accurate causal treatment effect estimates, especially when timing-varying covariates are available and when the number of support points for the heterogeneity distribution is chosen carefully using an information criterion.

Table A1: Drafted job-search and employment plans, and participation in training programs

| | Treatment group | | | Control group | |
|---|---|---|---|---|---|
| | Pre-reform | Post-reform | Participants | Pre-reform | Post-reform |
| *Job-search and employment plans* | | | | | |
| Time to the first plan, days | 75.5 | 27.3 | 12.7 | 57.3 | 54.6 |
| Plan by week 1, % | 29.4 | 56.7 | 75.8 | 21.9 | 22.5 |
| Plan by month 1, % | 34.8 | 64.7 | 85.4 | 26.1 | 27.3 |
| Plan by month 5, % | 46.4 | 73.0 | 90.3 | 33.6 | 36.4 |
| Without a plan, % | 45.3 | 22.9 | 7.8 | 61.9 | 58.8 |
| Number of plans | 0.8 | 1.3 | 1.6 | 0.5 | 0.6 |
| *Training programs* | | | | | |
| Number of programs | 0.5 | 0.6 | 0.7 | 0.2 | 0.2 |
| Program duration, days | 87.9 | 91.6 | 96.9 | 83.6 | 87.6 |
| *Program type, %* | | | | | |
| Basic training | 16.7 | 17.3 | 22.2 | 6.2 | 5.5 |
| Advanced training | 13.7 | 13.1 | 17.0 | 5.2 | 4.4 |
| Not specified | 11.5 | 10.7 | 12.6 | 3.4 | 3.1 |
| Code 'other' | 7.2 | 8.9 | 11.5 | 2.0 | 2.6 |
| *Program field, %* | | | | | |
| General (no field) | 18.1 | 20.0 | 25.0 | 5.7 | 5.7 |
| Professionals | 6.5 | 5.1 | 6.8 | 1.8 | 1.2 |
| Craft and related trades | 5.2 | 5.3 | 7.1 | 1.5 | 1.7 |
| Other fields | 10.6 | 12.8 | 16.4 | 3.9 | 4.2 |
| Number of observations | 12,494 | 5,125 | 3,106 | 40,522 | 18,544 |

*Notes*: Spells started between February 15 and June 31, 2005 are excluded. The third column includes those who enrolled in the RE program during the post-reform period. For job-search and employment plans that were drafted before the unemployment spell begun, a maximum of three months time difference is allowed, and time to the first plan is set to zero for these spells. The average duration of the training programs is calculated for participants. The program type basic includes general and basic education and re-training. The other program fields include the ISCO main categories other than professionals (2) and craft and related trades (7).

Figure A1: Monthly re-employment hazards




*Notes:* The graphs plot monthly exit rates from UI benefits to employment by group and the period of unemployment entry. In panel (a) all job seekers who are still receiving UI benefits are at risk of exiting to employment in a given month. In panels (b), (c) and (d), the risk set is also conditional on the past and current experiences of labor market training. The exit rates are plotted when the risk set includes at least 50 observations.