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Stepping Stones or Dead Ends? The Effects of Paying Employers for Permanent Contracts



Stepping Stones or Dead Ends? The Effects of Paying Employers for Permanent Contracts

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PRELIMINARY & INCOMPLETE

Abstract

Many developed countries have limited-duration hiring subsidy programmes to help long-term unemployed find and retain employment. This article studies the effects of a Finnish programme which sought to induce transitions from subsidised to regular employment by paying firms a one-time reward for offering permanent contracts to subsidised workers. Using a difference-in-differences design with short-term unemployed hiring-subsidised workers as a control group, I find no evidence of improvement in long-term employment probability, but a short-run employment boost amounting to approximately 1.4 additional cumulative employment months per treated spell over four years, at a fiscal cost of around 8,000 euros per additional employment year. I also find evidence that the programme altered firms' hiring behaviour, with treated firms shifting their hiring more towards younger workers with stronger employment histories.

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1 Introduction

Crépon & van den Berg (2016) frame active labour market policies as opportunities, or treatments, to unemployed individuals who search for jobs in a labour market with informational (or search) frictions. Hiring subsidies¹ are one major form of active labour market policies. According to OECD (2025), private-sector recruitment incentives — the OECD labour market programme category closest hiring subsidies of the Finnish context — account for about one-sixth of spending on active programmes, excluding administration. To improve allocation and contain costs, many hiring subsidy programmes have limited duration and are targeted towards disadvantaged groups, such as the long-term unemployed. Although one key objective of these programmes is to help the target group transition to long-term employment, subsidised workers frequently return to unemployment once the subsidy is exhausted. This paper contributes to the literature on hiring subsidies by examining how their design affects long-term employment outcomes.

To improve labour market prospects of long-term unemployed, the Finnish government in 2019 implemented a policy programme to induce firms to offer permanent contracts to subsidised workers, a practice referred to here as regularisation. The programme paid out a fixed reward equivalent to approximately 1.5 months of labour costs for a firm which regularised a long-term unemployed subsidised employee at the beginning of, during, or at the end of the hiring subsidy spell. I study the effects of this programme using a difference-in-differences approach.

The design exploits the fact that the regularisation reward applied only to workers with sufficiently long unemployment histories, while hiring subsidies for short-term unemployed workers carried no such reward. This provides a natural control group — short-term unemployed hiring-subsidised workers — who faced the same broader labour market conditions over the same period but whose employers had no financial incentive to offer them permanent contracts. The analysis is based on population-level administrative register data covering

¹I follow Orszag & Snower (2003) and refer to these policies in general and the Finnish subsidy system in particular as hiring subsidies. The terms wage subsidy and employment subsidy are also used in the literature to refer to similar programmes.

all hiring subsidy spells in Finland over the study period, linked to employment records and background characteristics. Register data avoids the selective attrition and measurement error that affect survey-based studies of programme participation, and allows following all participants over the full four-year horizon without attrition.

The results indicate that while the programme did not increase the probability of being in employment over the longer term, it did have a short-run effect which appears as quite cost-effective measured in terms of programme costs relative to additional employment months. I also find evidence that firms switched their hiring patterns in response to the programme, hiring younger job-seekers with shorter unemployment histories and prior experience in the same firm. As the firms adjust their hiring patterns along observable dimensions, they may do so along unobservable dimensions as well, which means the positive employment effect may be biased upwards. This is also supported by the fact that, contrary to what one might expect, estimated effects are higher for continuation in any employment than for continuation in the same firm. Additionally the hiring response means that if such a regularisation reward were to be included as a permanent feature of a hiring subsidy system, it would shift the use of hiring subsidies towards more employable job-seekers (e.g. job-seekers with better employment histories), which may not be desirable.

The regularisation programme studied here can be seen as an active labour market policy (ALMP) measure seeking to enhance participants' long-term employment prospects. For a meta-analysis on ALMPs see Card, Kluve & Weber (2018). Targeted hiring subsidies are arguably the most prominent form of ALMP for the long-term unemployed, along with training. Although the regularisation reward is not a hiring subsidy, the policies' goals, approaches and target groups are very similar, which connects the present study to this broader literature. Hiring subsidies aim to reduce the barriers to entry these individuals face in the labor market. By providing financial incentives to employers, hiring subsidies lower the cost of hiring, encouraging firms to take a chance on individuals who have been out of work for an extended period.

Card, Kluve & Weber (2018) find that most studies on private-sector hiring subsidies

report positive long-term effects on employment, with considerable variation in estimated effects. Boockmann et al. (2012) study hiring subsidies for older workers in Germany and find positive employment effects consistently only for one subgroup, and Albanese et al. (2024) find no lasting effects for hiring subsidies targeted at low-educated unemployed youths in Belgium during the recovery from the Great Recession. More positive effects are found using Swedish data: Sianesi (2008) finds that effects diminish somewhat over time, while Sjögren & Vikström (2015) find persistent effects. For earlier research on hiring subsidies in Finland, Kangasharju (2007) and Korpela (2025) find only limited displacement effects on non-subsidised employment.

As the regularisation reward was conditional on contract type, the present study also connects to research on employment protection legislation. This legislation aims to provide job security and protect workers from arbitrary dismissal. Theoretically this can be expected to reduce both hiring and firing with an ambiguous effect on net employment, which broadly is also what is found in empirical research (Skedinger 2011).

In many European countries differential employment protection legislation concerning regular and temporary contracts has led to a two-tier segmentation referred to as dual labour markets. Stringent requirements for ending permanent contracts can make employers reluctant to offer these, preferring instead the flexibility provided by temporary contracts. A key question in dual labour markets is whether temporary contracts work as stepping stones towards more stable jobs, or whether they become dead-ends. (Bentolila et al. 2019)

Where temporary contracts do tend to become dead-ends, policy-makers face the question of how to incentivize firms to offer permanent positions. An obvious solution would be to narrow the regulatory gap between temporary and regular contracts, and while many countries have undertaken such reforms in recent years (Dolado 2017, Bentolila et al. 2019), the same political-economy reasons (Saint-Paul 2000) that led to dual labour markets in the first place may restrict the scope for such reforms. In this situation a possible solution is to grant subsidies to employers offering permanent contracts, which can be understood as compensation for higher costs of dismissal. In practice, however, this type of policy seems

rare, and the literature on regularisation rewards of the type studied here is scarce. Notable exceptions in this field of literature are Pérez et al. (2009), Ciani & de Blasio (2015), and Ciani et al. (2019).

Pérez et al. (2009) study the effects of hiring subsidies for firms offering temporary workers a permanent contract in Spain during 1997-2004. The subsidy consisted of a one-time payment to the hiring firm, amounting to approximately 3 months of labour costs on average. Eligibility for the subsidy programme varied over time and across worker type (age and gender) and region, and the authors use a triple-difference-in-differences approach to estimate the effects of the subsidy on labour market flows for different types of workers. It is only for middle-aged women that the authors find a positive, statistically significant effect on transitions to permanent contracts within the firm.

Ciani & de Blasio (2015) evaluate the effects of a temporary Italian subsidy programme designed to encourage firms to convert fixed-term contracts into permanent ones. The subsidy was available nationally, but funding constraints meant that applications were accepted only for a few days in 2008. The programme targeted women of all ages and men under 30. Using administrative data from the Veneto region and a difference-in-differences strategy using men over 30 as a control group, the authors find that the subsidy led to a substantial increase in contract conversions, with no evidence of substitution effects across time or worker types.

Ciani et al. (2019) study the effects of policies aimed to incentivize firms to offer permanent contracts to long-term unemployed workers in Italy. The authors use a triple-difference-in-differences approach, as the subsidy rate varied over time and across worker types (short-term vs. long-term unemployed). In 2014 the subsidy only concerned long-term unemployed and was significantly higher in Southern Italy, but in 2015 the subsidy was equalized between regions and worker types due to a national legislative change. The identifying variation in the subsidy was large, worth approximately 4.7 months of labour costs, although the subsidy was not a one-time payment but a payroll tax deduction for up to three years. The authors find that the programme increased the probability of finding a permanent job significantly,

by approximately 40%, although it should be noted that this only captures the short-term effect of the programme as the authors only use data for 2014-2015.

This paper contributes to the literature in several ways. While previous studies have evaluated incentive programmes aimed at encouraging the conversion of fixed-term contracts into permanent ones, the programme studied here is institutionally distinct in that it targets hiring-subsidised workers — a group already subject to public labour market intervention. This places the study at the intersection of two key literatures: the effectiveness of hiring subsidies and the design of active labour market policies.

First, the paper contributes evidence on the dynamics of employment effects. Most studies in the hiring subsidy literature focus on short- to medium-run outcomes, and evidence on longer horizons remains scarce. Following participants over four years from the beginning of the subsidy spell, I find that the short-run employment boost from the regularisation reward diminishes over time and does not translate into lasting gains in employment probability, qualifying the more optimistic short-run findings of Ciani et al. (2019) for a similar programme.

Second, the paper contributes to the small literature on firm responses to hiring subsidies. As Boockmann et al. (2012) note, hiring subsidies can only improve employment outcomes if they actually affect employers' hiring decisions. Unlike most prior work, I am able to examine not only worker-level outcomes but also whether the programme altered the composition of workers hired into subsidised employment. I find that it did: firms responded by hiring younger workers with stronger employment histories, raising the possibility that the employment gains partly reflect compositional changes rather than a genuine programme effect.

Finally, unlike most prior work on regularisation rewards, I am able to observe subsidy take-up at the level of individuals and firms, allowing for an analysis of implementation frictions. While such frictions are well-documented in social security and tax policy (Chetty et al. 2009, Ericson et al. 2025), evidence from the ALMP literature is more limited. Hamersma (2011) documents significant underusage of hiring subsidies in the US context,

and Behrens & Månsson (2023) find informational barriers as one reason why eligible firms do not claim hiring subsidies. I find that most employers continuing to employ eligible workers beyond the hiring subsidy spell do not collect the regularisation reward, which is indicative of the same frictions found in earlier literature.

2 The regularisation subsidy programme and institutional context

This section outlines the institutional context of the regularisation reward: the programme’s design and eligibility conditions, the hiring subsidy system on which it was layered, and the regulation of temporary and permanent contracts that determines the incentive environment the reward was intended to alter.

The regularisation programme. The regularisation programme² applied to a (large) subset of hiring subsidies. The regularisation reward was only paid to firms, and not to public or third-sector employers. The reward was paid upon regularisation of an employee who was eligible for the hiring subsidy based on unemployment history or a reduced capacity to work. The hiring subsidy rate of the target group was 40% for individuals with at least 12 months of unemployment in the preceding 14 months, and 50% for individuals with 24 months of unemployment in the preceding 28 months or a reduced capacity to work. The firm was eligible to receive the regularisation reward if the permanent job was full-time (at least 80% of normal hours in the industry), the permanent contract came into force during the hiring subsidy spell or immediately after it, and the worker was still working under the permanent contract when applying for the reward. The regularisation reward was 4,000 euros, which corresponds to approximately 1.5 months of labour costs in the target group.

The regularisation reward was paid for workers whose hiring subsidy spell had started between 1 September 2018 and 30 June 2019. The decree governing the regularisation reward was passed on 19 December 2018 and was in place from 1 January 2019 to 30 June 2019.

²Government decree 1168/2018.

This means that a firm which had regularised a worker between 1 September 2018 and 31 December 2018 was only able to apply for the reward from 1 January 2019 onwards (but a firm which had regularised a worker on 30 June 2019 could apply for the reward up to three months after this date). The stated aim of the reward programme was simply to promote employment.

The hiring subsidy system. The regularisation subsidy programme is part of Finland's broader hiring subsidy system and was only available to workers already employed under a hiring-subsidised contract. Hiring subsidies are paid to employers who hire eligible jobseekers, and they typically cover a fixed percentage of labour costs for a limited duration. Legal provisions define the conditions for eligibility, maximum subsidy levels, and duration based on individual characteristics such as age, work history, or disability status. However, eligibility under the law does not guarantee receipt of a subsidy: decisions are made at the discretion of caseworkers at local employment offices, who assess whether a subsidy is justified in each case to improve the job-seeker's long-term employability. In practice, jobseekers usually first secure a tentative job offer, and only then can the employer apply for the subsidy. While reimbursement rates are almost always set at the legal maximum, actual subsidy durations tend to be shorter than the statutory limit.

Under the period analysed in this study, there were three categories of unemployed jobseekers who were eligible for a hiring subsidy, if the case worker assesses that hiring-subsidised employment will increase the person's employment prospects in the future.³ Individuals with less than 12 months of preceding unemployment were eligible for a hiring subsidy if the case-worker assesses that without a hiring subsidy the person's unemployment spell would be prolonged. The reimbursement rate for this group was 30% and maximum duration 6 months. Firms were not eligible to collect regularisation rewards for these employees, which I will call short-term unemployed and which will be used as the control group. The treatment group, i.e. types of employees for which the firm could collect the regularisation

³In addition to these three, there was a separate category for individuals with reduced work capacity due to disability or illness. Firms were eligible to collect regularisation rewards for these employees as well, but I exclude them from the sample because they are less comparable and represent only a small fraction of hiring-subsidised workers and regularisation rewards collected.

reward, consisted of long-term unemployed. These are either individuals with at least 12 months of unemployment in the preceding 14 months or individuals with at least 24 months of unemployment in the preceding 28 months. Maximum duration of the hiring subsidy for these groups was 12 months, with reimbursement rates being 40% and 50%, respectively.

Regulation of temporary and permanent contracts. As the programme aimed to increase transitions from temporary to permanent contracts through an incentive aimed at the employer, it is useful to understand how temporary and permanent contracts are regulated. Employment protection legislation (EPL) in Finland is moderately stringent by OECD standards. Permanent employees enjoy substantial protection against individual dismissal, while fixed-term contracts are governed by more flexible rules. This results in a two-tier structure that creates a pronounced gap in employment security between temporary and permanent workers.⁴ Additionally new permanent employees typically face a trial period of six months (maximum allowed in the law) during which the employer can terminate the contract without the need to provide specific reasons, unless the termination is discriminatory or otherwise unlawful.

The effects of the regularisation subsidy are potentially contingent on employment protection legislation, more specifically regulations concerning individual dismissals for temporary and regular workers. Several commentators of the draft proposal for the regularisation reward feared that employers would misuse the reward programme by regularizing the employee and collecting the reward, then laying her off during the trial period. The trial period of a regularised employee is at most 6 months (which is also the typical duration for hiring-subsidised employment) and in principle the requirements for dismissal during the trial period are quite general (e.g. the worker being "generally unsuitable for the job"); after the trial period the requirements become more stringent. Thus one potential outcome of the regularisation programme is an increase in the rate of dismissals during the trial period for subsidised workers.

⁴In OECD's Strictness of employment protection indicators for 2019 Finland's indicator for strictness in individual dismissals (regular contracts) is 2.37, whereas the indicator value for temporary contracts is 1.61; the corresponding OECD averages are 2.22 and 1.79, pointing to a smaller gap.

3 Data and methods

3.1 Data sources

The analysis in this paper is based on population-level administrative data covering firms, jobseekers, and employees in Finland. The data are drawn from several linked government registries, including those maintained by the public employment services, the national pension system and the tax authority. These sources provide detailed, individual-level information on hiring subsidy spells, regularisation rewards, employment histories, and a wide range of background characteristics. The data also include information on employers, allowing for matched worker–firm analysis. The main data sources are described below.

Hiring subsidy spells. Data on hiring subsidy spells are sourced from administrative registers maintained by the public employment offices. These records include unique identifiers for both the jobseeker and the employer, as well as the start and end dates of each hiring subsidy spell. The data also report the reimbursement rate and the stated grounds for eligibility—whether the subsidy is granted due to short-term or long-term unemployment, or on the basis of reduced work capacity.

Continuation in employment. The regularisation reward was conditional on the contract being permanent, but contract type is not directly observed in the data. Instead I evaluate the effects of the programme on continuation in employment, which is also arguably a more policy-relevant outcome than contract type. This is measured using an employer–employee-linked dataset on employment relationships for 2017–2023. The data include information on a person’s end-of-year employment relationship, longest employment relationship during the year, and in some cases other employment relationships as well. The dataset does not include additional information about the job itself, such as hours or wages.

Demographic information on the worker. The estimation data also include a large number of register-based covariates, such as age, gender, place of residence, education, work and unemployment history, and family status. These are used as control variables, and also as outcome variables when studying the effect of the regularisation reward on the composition

of the hiring-subsidised workforce.

Regularisation rewards paid out. Data on regularisation rewards paid out were obtained through a separate manual extract from the employment office administrative systems, rather than from the standard register datasets used elsewhere in the analysis. As this information was compiled specifically for this project and is not part of the routinely maintained research registers, it may be less standardised than the other administrative data sources and as such may be more susceptible to recording or linkage imperfections. The dataset includes an indicator for whether the hiring-subsidised employee was offered a permanent contract to begin with, or whether the employee was regularised when the firm applied for the regularisation reward.

Sample selection. The hiring subsidy spell and employment datasets are linked at the individual level. I exclude from the sample all hiring subsidy spells for which no corresponding employment record is found in the employment data. In principle all hiring subsidy spells should also be present in the employment dataset, but even register data is not error-free: there are cases where, according to the employment office records, a person worked in a firm as a hiring-subsidised employee, but this job does not appear in the employment dataset. More concretely, a hiring subsidy spell is retained only if corresponding employment is found for any of the first three months of the spell and for the end month of the spell.⁵ Approximately 13% of observations are excluded on this basis. These exclusions apply to both treatment and control groups and, assuming they reflect idiosyncratic registry errors, should not distort the analyses.

Maximum duration of hiring subsidy spells in the estimation sample should be 12 months. Despite this, approximately 7% of hiring subsidy spells (after the restriction described above) appear to continue beyond this threshold; these spells are excluded from the analysis.

⁵The reason why the selection is based on any of the first three months of the spell and not only the first month is that for spells beginning in 2019 the income registry has been used to define employment relationships, and all workers may not receive their first pay during the month they began working.

3.2 Empirical framework

Key estimation equations. The analysis relies on two complementary difference-in-differences specifications. To illustrate how treatment effects evolve over calendar time, I first estimate an event-study model with period-specific interactions:

$$Y_{i,t}^h = \psi_0 + \alpha I(g = T) + \sum_{k=0}^{\bar{t}} \beta_k I(t = k) + \sum_{k=0}^{\bar{t}} \gamma_k I(g = T) I(t = k) + \text{controls} + e_{i,t}, \quad (1)$$

where i indexes individuals, g indexes groups with $g = T$ denoting the treatment group, t is the starting month of the hiring subsidy spell, and $\bar{t}$ denotes the last period in the sample. $Y_{i,t}^h$ is the outcome evaluated h months after the beginning of the hiring subsidy spell. The coefficients of interest are γ_k , which trace the treatment–control difference relative to the omitted reference period. Results are reported using quarterly time intervals. Typically in this approach the last pre-treatment period is used as the reference period; here I use 2018:Q3 as the omitted reference period, for reasons discussed below under *Defining the treatment period*.

As a complementary summary measure, I estimate a pooled DiD specification:

$$\begin{aligned} Y_{i,t}^h = & \beta_0 + \beta_1 I(g = T) + \beta_2 I(t \in [2019:1, 2019:6]) \\ & + \gamma I(g = T \& t \in [2019:1, 2019:6]) + \text{controls} + \epsilon_{i,t}, \end{aligned} \quad (2)$$

where γ captures the average treatment effect of the regularisation reward in the January–June 2019 period, under the identifying assumptions of the DiD approach, which are discussed in the current context below.

Defining the treatment and control groups. The treatment group are hiring-subsidised workers with at least 12 months of unemployment during the preceding 14 months or at least 24 months of unemployment during the preceding 28 months (“long-term unemployed”) working for a for-profit private-sector employer (“firm”).

There are two potential non-treated control groups: long-term unemployed in the public and non-profit sector, and short-term unemployed (less than 12 months of unemployment

during the preceding 14 months) in firms. Of these two, the latter will be used as the control group in the analysis. This choice is based on two arguments. First, pre-trend analysis suggests threats to identification are larger using the former group (see Figure A1 in the Appendix). Second, institutional context suggests that risks of unobserved confounding shocks are greater along the private/non-private employer dimension compared to the long-term/short-term unemployed employee dimension. The macroeconomic environment was relatively stable during the sample period (2017-2019), which suggests at most small changes in unobserved long-term employability of short-term vs. long-term unemployed. As for the employer type, while private firms presumably hire and regularise hiring-subsidised workers to maximize profits in the prevailing market conditions, municipalities (where most non-private hiring-subsidised workers are) hire hiring-subsidised workers in part to shift costs to the central government (Nieminen et al. 2023) with fewer opportunities offered for continued employment. Moreover, the possibilities and incentives of the municipalities to shift costs using hiring subsidies vary across the sample period.

Defining the treatment period. Implementation of the programme creates some ambiguity concerning the treatment period, namely the treatment status of hiring subsidy relationships beginning in September-December 2018. While it is possible the programme may have had an effect on regularisation of hiring subsidy relationships beginning during the last months of 2018, certain institutional features suggest this effect is likely negligible. Details of the programme were announced for the first time in 26 October 2018, and marketing for the programme only began after the decree was passed in 19 December 2018. Descriptive statistics on take-up imply that a large share of employers were unaware of the programme even after it was passed. Additionally, there are bureaucratic lags: after receiving all proper documentation, it typically takes at least two weeks for the employment office to handle firms' subsidy applications. These institutional considerations inform the treatment of 2018:Q4 differently across the two estimation specifications. In the pooled DiD specification (Equation (2)), 2018:Q4 is assigned to the pre-treatment period, as the institutional evidence suggests firms were not effectively treated during this quarter. In the

event-study specification (Equation (1)), 2018:Q3 is used as the omitted reference period, leaving 2018:Q4 visible as a post-period coefficient so the reader can directly assess whether any pre-trend break is apparent at that point. Robustness checks that instead extend the treatment period to include 2018:Q4, or that drop 2018:Q4 spells from the sample entirely, are reported in Table A1.

Estimation sample. The estimation sample contains all hiring subsidy spells in the treatment and control groups beginning between 2017:1 and 2019:6. The beginning date is set following a reform of the hiring subsidy system implemented in 2017. The end date is chosen so that the treatment group’s treatment status does not change back to non-treated within the sample window. Additionally, from February 2020 onwards labour markets were struck by the COVID-19 pandemic; as the treatment and control groups are defined in terms of employment history, such a shock is likely to have had a strong effect on the composition of these groups, which motivates excluding spells beginning after June 2019.

Key outcomes. The primary research question concerning the effects of the regularisation programme is whether the programme succeeded in its aim of increasing long-term employment probabilities in the target group. The main analysis focuses on long-term effects measured at $h = 48$, but I also report results across a range of horizons ($h = 0, \dots, 48$) to assess the dynamics of programme impacts. In addition, two sets of mechanism outcomes are assessed: first, whether the programme led firms to alter the composition of hiring-subsidised hires (e.g. shifting toward younger workers or those with stronger employment histories); and second, whether regularised workers were subsequently dismissed during the trial period, which would indicate misuse of the reward.

Identifying assumptions. Evaluation of the causal effects of the regularisation subsidy exploits variation across time and worker types. In the first half of 2019 incentives changed for firms employing hiring-subsidised workers hired from the ranks of the long-term unemployed, with the regularisation subsidy incentivizing firms to offer these workers a permanent contract. Firms’ incentives regarding hiring-subsidised workers with shorter unemployment histories remained as they were. The identifying assumption is that, absent the programme,

long-term employment outcomes would have evolved similarly for the two groups.⁶

Under these assumptions, the DiD estimate captures the causal effect of the regularisation reward on employment outcomes. If the programme worked as intended — inducing firms to offer permanent contracts to workers they would otherwise have employed only temporarily — we would expect a positive effect on continuation in the same firm. A broader positive effect on any employment would suggest that permanent contracts acted as stepping stones, providing workers with greater job security and on-the-job experience that improves their longer-term labour market prospects. Finally, if the programme altered firms’ hiring behaviour, this would manifest as compositional changes in the observable characteristics of workers hired into subsidised employment in the treatment period.

As the evaluation of the programme’s effects will rely on variation across time and across worker types, other worker-type-specific shocks present a potential confounding factor and a threat to identification. The macroeconomic environment was quite favorable over the estimation period, with unemployment falling and a slight decrease in the long-term unemployed share over the years preceding the policy experiment in 2019 (see Figure A2 in the Appendix). This suggests that macroeconomic shocks are unlikely to pose a threat to identification.

This implementation could in principle allow disentangling two separate mechanisms of the programme. Because some hiring subsidy spells became eligible for the regularisation subsidy retroactively, the discontinuity in September 2018 could in principle be used to identify the effect of the programme when the firm is already employing a given worker (a worker-conditional effect). The number of regularisation rewards collected for spells starting in the last months of 2018 is, however, so low that any formal statistical testing of this

⁶The identifying assumption could be violated if the reward affected the labour market outcomes of the control group — for instance, through within-firm substitution in regularisation decisions or shifts in firms’ relative hiring behaviour. Several features of the setting limit the scope for such spillovers. First, the programme was small in scale: the reward was collected for only around 13 % of eligible spells, and 576 rewards in total represent a negligible share of hiring activity in the Finnish labour market. Second, within-firm spillovers require firms to employ both types of workers simultaneously, which was the case for only 8.5 % of firms in the pre-treatment period. Third, Figure 1 indicates that firms did not respond to the incentive by shifting their hiring toward the long-term unemployed (the treatment group) at the expense of the short-term unemployed (the control group), suggesting that the control group’s composition was not affected by the programme.

mechanism would be severely constrained by low power. It should also be noted that although the worker-conditional effect is theoretically interesting, the policy relevant effect is that which allows firms to adjust their hiring in response to the regularisation subsidy.

Covariates. To strengthen identification I use a large set of register-based covariates relating to the worker and the firm. Continuous covariates include age and age squared, dummy covariates include indicators for gender, non-native mother tongue, different measures of employment history (no employment history in the past 5 years, any employment in the same 1- and 5-digit level industry in the past 5 years, any employment in the same firm in the past 5 years) and fixed effects for region of residence, municipality type (urban, semi-urban, rural), duration of the preceding unemployment spell (0-99, 100-199, 200-499, 500-1,499 and more than 1,500 days), and firm industry (1-digit level).

4 Descriptive evidence

Figure 1 presents the monthly total of new hiring subsidy spells⁷ for firms, separately for long-term unemployed workers (treatment group, solid line) and short-term unemployed workers (control group, dashed line), over the period 2017–2021. The dotted line shows the ratio of treatment to control group spells on the right axis. The vertical dashed lines indicate the key dates of the programme.

If firms had responded to the regularisation reward by shifting their hiring toward long-term unemployed workers eligible for the reward, one would expect an increase in the treatment-to-control ratio during the treatment period. The ratio does not show such an increase; if anything it declines slightly around the start of the programme, and this decline persists after the programme ends, suggesting it reflects broader trends rather than a programme response. This indicates that firms did not respond to the incentive programme by directing their recruitment towards eligible job-seekers at the expense of the control group.

⁷The hiring subsidy scheme enables employers to extend temporary contracts of hiring subsidised workers, provided there is no gap between the two contracts. In this study consecutive spells are treated as a single spell; this is also in line with how the regularisation reward programme defined the starting dates of hiring subsidy spells.

Both series display pronounced seasonality, with peaks typically occurring at the turn of the year and around mid-year, and the seasonal patterns are broadly similar across the two groups.

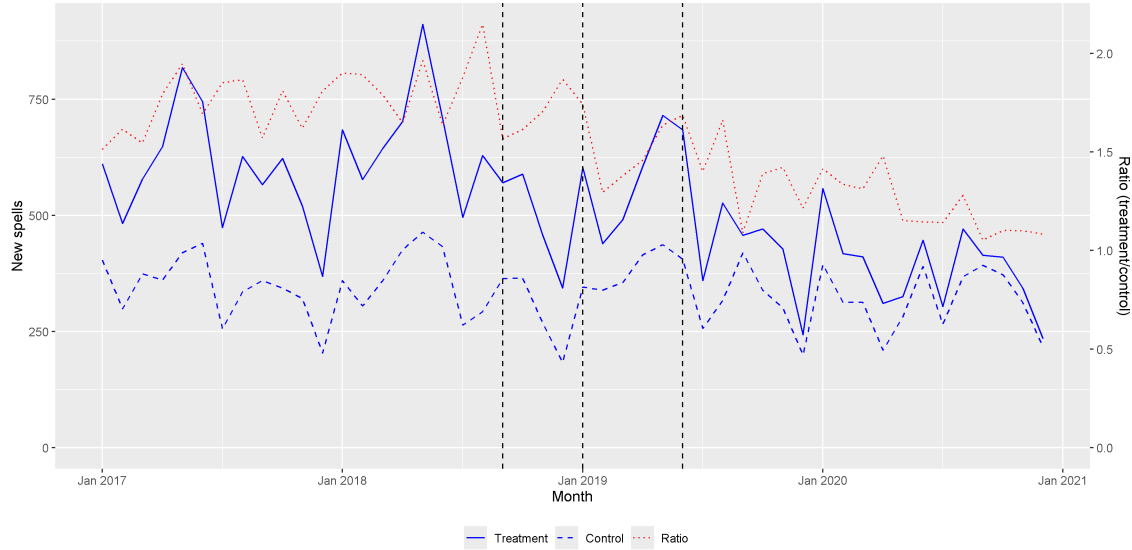


Figure 1: New hiring subsidy spells in firms, by employee type and month

Notes: New hiring subsidy spells in firms by employee type (short-term and long-term unemployed) and the ratio of new spells in these groups, by starting month of the hiring subsidy spell.

Figure 2 presents the number of regularisation rewards by month of start of the hiring subsidy spell between September 2018 and June 2019, separated by whether the reward was granted based on a job made permanent at the beginning of the hiring subsidy spell, or during or immediately after it. The figure also presents the ratio of rewards paid out with respect to new spells in the treatment group.

At the end of the program, the regularisation reward was collected for approximately 20% of new spells in the treatment group (this share being 12.9% for the whole period 2019:1 - 2019:6). Most commonly the contract was written as permanent at the beginning of the spell. Collection of regularisation rewards is lower for spells beginning in 2018. This partly reflects the fact that some of these spells ended before January 2019, when the reward could be collected, but imperfect take-up is also a possibility.

Linking the regularisation rewards to continuation in employment, I find that only 16% of spells in the target group beginning in the period 2019:1–2019:6 where the hiring-subsidised

worker continued working for the same firm 6 months after the end of the hiring subsidy spell are associated with a collected regularisation reward. In these cases it is possible that the employee’s contract was made permanent and the firm was unaware or unwilling to apply for the reward, pointing to underusage of the subsidy, or that the firm continued to employ the person on a temporary basis. This means that the estimated effects should be interpreted as intent-to-treat estimates reflecting a setting where most eligible firms did not collect the reward, and suggests that a permanent implementation with better employer awareness and lower application frictions could produce larger effects than those found here.

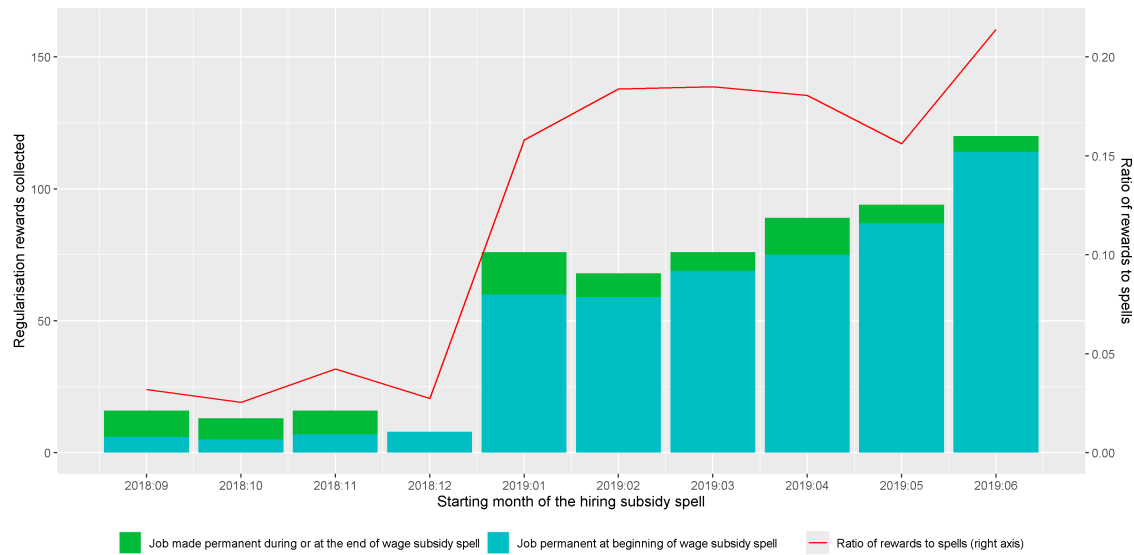


Figure 2: Regularisation rewards collected by employers, by month and reward type
Notes: Regularisation rewards collected by employers by beginning month of the hiring subsidy spell by reward type, and ratio of rewards to new spells in the treatment group, by month 2018:10–2019:6.

Table 1 presents means of employment outcomes and covariates by treatment status and period in the estimation sample, where the pre-treatment period covers hiring subsidy spells beginning in 2017:1–2018:12 and the post-treatment period covers spells beginning in 2019:1–2019:6. Employment outcomes are measured at 12, 24, 36 and 48 months after the beginning of the hiring subsidy spell (T+12, T+24, T+36 and T+48 respectively). As expected given the group definitions, individuals in the treatment group (long-term unemployed) have been out of work for substantially longer than those in the control group — around 649 days since previous employment compared to around 322 days in the pre-treatment period. Related

to this, they are on average older and less educated. Correspondingly, their longer-run employment outcomes are less favourable: employment probability at T+48 is around 69% in the treatment group compared to 83% in the control group in the pre-treatment period. Somewhat more surprisingly, focusing on continuation in the same firm after the hiring subsidy spell, the groups are considerably more similar: same-firm continuation at T+12 is around 0.54 in both groups in the pre-treatment period, with a difference of only one percentage point at T+48. This suggests that conditional on entering a hiring subsidy spell, short- and medium-run retention rates are broadly comparable across the two groups.

Turning to the treatment period, the main indication of possible selection effects is found in the share of workers with previous employment in the same firm, which almost doubles in the treatment group from 10% in the pre-treatment period to 18% in the treatment period, compared to a more modest increase from 9% to 12% in the control group. This suggests that firms hiring long-term unemployed workers under the programme increasingly drew on workers with prior attachment to the same firm.

5 Results

This section presents an analysis of the programme (described in section 2) using a difference-in-differences approach (described in section 3) on employment outcomes and selection.

Figure 3 presents descriptive evidence on employment outcomes of hiring-subsidised workers 48 months after the start of the hiring subsidy spell, by starting month and employee type. Panel A shows the probability of being in any employment 48 months after the start of the hiring subsidy spell, while Panel B reports cumulative months in employment over the same 48-month period. Outcomes are normalized to one in January 2018 and shown separately for the treatment group (long-term unemployed in firms) and the control group (short-term unemployed in firms).

Continuation probabilities are consistently higher in the control group (84.0% versus 71.0% in January 2018), and a similar pattern is observed for cumulative employment months

Table 1: Descriptive statistics by group and period

	Control, Pre	Control, Post	Treatment, Pre	Treatment, Post
Panel A: Employment outcomes				
Same firm, T+12	0.53	0.54	0.54	0.58
Same firm, T+24	0.40	0.42	0.36	0.42
Same firm, T+36	0.32	0.33	0.30	0.33
Same firm, T+48	0.26	0.25	0.25	0.26
Any employment, T+12	0.79	0.77	0.70	0.73
Any employment, T+24	0.81	0.78	0.65	0.67
Any employment, T+36	0.82	0.82	0.67	0.69
Any employment, T+48	0.83	0.80	0.69	0.67
Cum. emp. months, same firm, T+12	9.28	9.45	9.24	9.75
Cum. emp. months, same firm, T+24	14.64	15.06	14.20	15.44
Cum. emp. months, same firm, T+36	18.88	19.46	18.15	19.84
Cum. emp. months, same firm, T+48	22.35	22.83	21.40	23.28
Cum. emp. months, any, T+12	10.93	11.05	10.25	10.75
Cum. emp. months, any, T+24	20.50	20.35	18.12	18.99
Cum. emp. months, any, T+36	30.25	29.98	26.04	27.10
Cum. emp. months, any, T+48	40.17	39.70	34.17	35.30
Panel B: Covariates				
Age	34.08	35.15	40.49	40.49
Female	0.40	0.39	0.36	0.35
Foreign-language	0.12	0.15	0.10	0.11
Region: Uusimaa	0.18	0.19	0.21	0.22
Only primary education	0.14	0.14	0.21	0.22
No previous employment in 5 years	0.14	0.12	0.23	0.21
Days since previous employment [†]	320.56	249.40	647.92	522.34
Any employment, same industry (1-digit)	0.41	0.42	0.39	0.37
Any employment, same industry (5-digit)	0.21	0.23	0.23	0.25
Any employment, same firm	0.12	0.14	0.17	0.19
Preceding job, same industry (1-digit) [†]	0.32	0.33	0.32	0.35
Preceding job, same industry (5-digit) [†]	0.15	0.17	0.16	0.21
Preceding job, same firm [†]	0.09	0.12	0.10	0.18
N	6393	1862	10434	2404

Notes: Means shown by treatment status and period; binary variables are shares. Employment outcomes refer to outcomes 12, 24, 36 and 48 months after the beginning of the hiring subsidy spell. Variables labelled “Any employment” indicate whether the worker had any employment in the given category within the 5-year period preceding the hiring subsidy spell. Variables labelled “Preceding job” indicate whether the worker’s most recent job before the hiring subsidy spell was in the given category; these are denoted by [†] and depict means for employees with previous employment in the 5-year period preceding the hiring subsidy spell.

(41.6 months versus 35.3 months), reflecting the more disadvantaged labour market position of the long-term unemployed. Importantly for identification, both series move closely together throughout the pre-treatment period without any systematic divergence, supporting the parallel trends assumption underlying the DiD design. In the treatment period, Panel A does not show any visible improvement in employment probability for the treatment group relative to the control group, suggesting no lasting effect on the extensive margin of employment. Panel B, however, shows the treatment group’s cumulative employment months tracking somewhat above the control group during the treatment period.

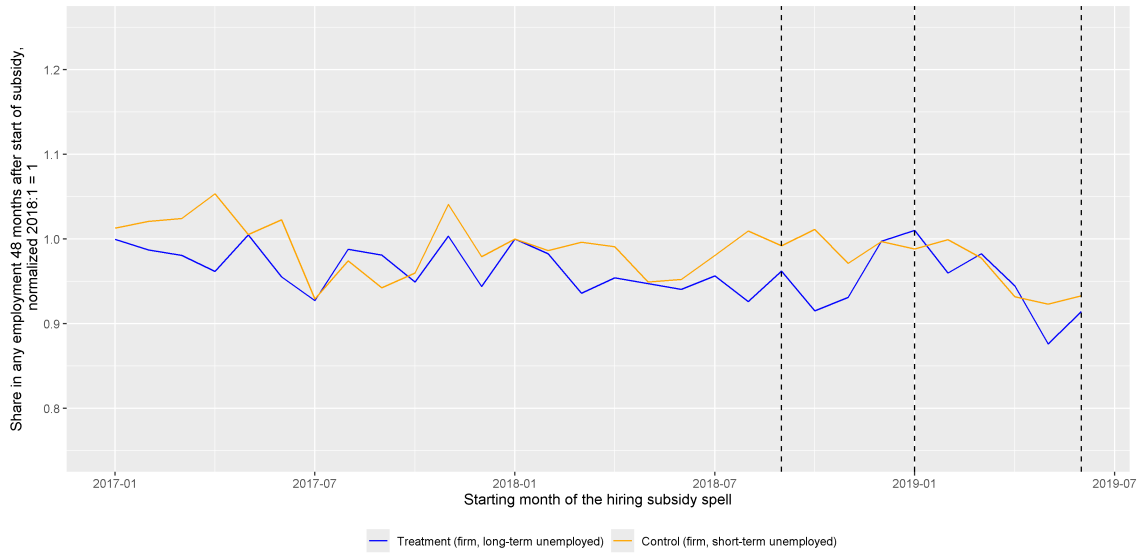
Figure A3 in the appendix presents the same figures for employment in the same firm. The overall patterns are similar — parallel pre-treatment trends, no long-run improvement in employment probability, and a modest treatment-period uptick in cumulative employment months — though the series are considerably more volatile, reflecting lower baseline levels.

5.1 Effects on continued employment in firm and in general

This section presents results for key outcomes of the programme: continued employment in the same firm and in any employment.

I begin by examining whether the identifying assumption of common pre-trends is plausible. Figure 4 presents event-study estimates based on Equation (1), aggregated to quarterly intervals to reduce noise relative to monthly estimation. The omitted reference period is 2018:Q3, the last quarter before the ambiguous treatment status of 2018:Q4 discussed in Section 2. Both outcomes — employment in the same firm and in any employment — are shown in each panel.

Panel A reports effects on probability of employment 48 months after the beginning of the hiring subsidy period. The pre-period coefficients for both outcomes fluctuate around zero without systematic trends, lending credibility to the parallel-trends assumption. In the post-period, point estimates for same-firm employment are positive and somewhat larger than those for any employment, which is consistent with the programme’s design: the reward directly incentivizes continuation in the same firm, so the effect on same-firm employment



(a) Employment probability 48 months after the start of the hiring subsidy spell



(b) Cumulative months in employment within 48 months of the hiring subsidy start

Figure 3: Employment outcomes of hiring-subsidised workers by starting month of the hiring subsidy spell

Notes: Panel A reports the probability of being employed 48 months after the start of the hiring subsidy spell, while Panel B reports cumulative months in employment over the same horizon. Outcomes are shown separately for the treatment group (long-term unemployed in firms) and the control group (short-term unemployed in firms) and are normalised to one in January 2018.

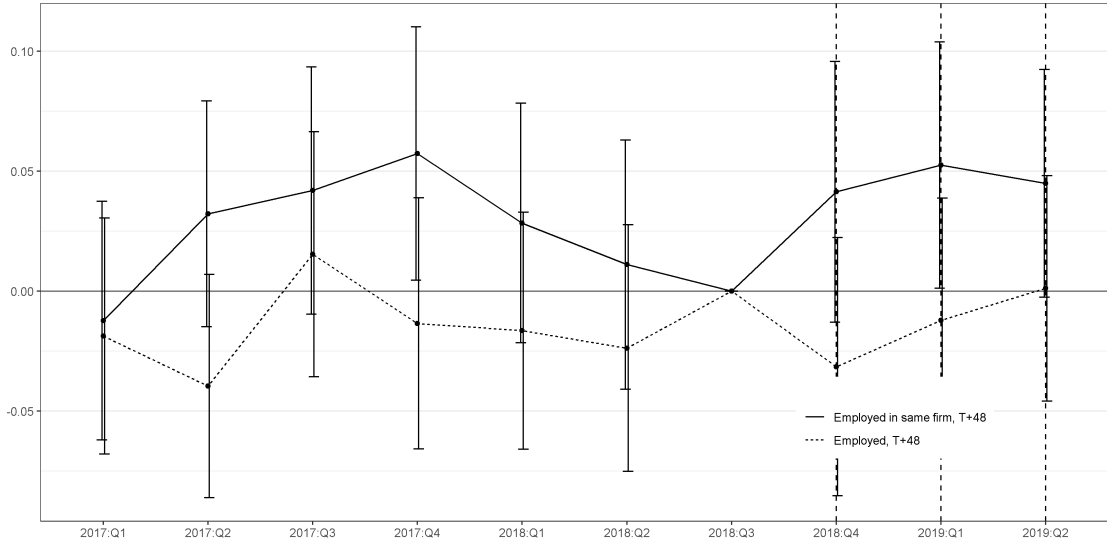
should be at least as large as on any employment. The estimates are, however, imprecise and not statistically significant.

Panel B reports effects on cumulative employment months over the same horizon. The pre-period pattern is again broadly flat for any employment, though the same-firm estimates show somewhat more variability, consistent with the noisier nature of same-firm outcomes noted earlier. In the post-period, both estimates are positive, pointing to positive employment effects.

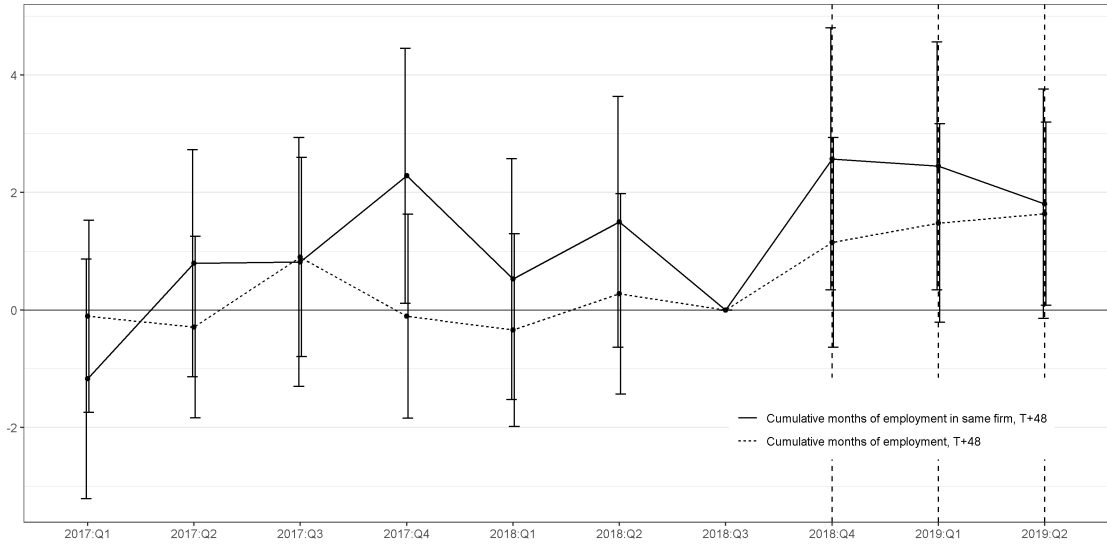
In the Appendix Figure A4 displays event-study estimates for outcomes at different horizons ($h = 12, 24, 36$ in addition to the baseline $h = 48$) and Figure A5 for specifications with and without demographic controls, both for any employment. Pre-treatment estimates are close to zero at all horizons and for both specifications, supporting the parallel trends assumption. In the post-treatment period, the employment probability panel of Figure A4 shows the fading of effects across horizons in event-study format: estimates are positive at $h = 12$ and $h = 24$ but diminish at longer horizons, corroborating the main finding that the short-run employment boost does not persist. For cumulative employment months, effects grow with the horizon as expected. Figure A5 shows that adding demographic controls leaves the estimates largely unchanged.

Next, Figure 5 turns to the pooled DiD specification (Equation (2)), which collapses the cohort dimension by pooling all starting cohorts into a single treatment effect estimate at each horizon h , varying h from 0 to 48 months after the start of the hiring subsidy spell.

Panel A shows that the estimated effects on employment probability grow during the first year, peak between 10 and 20 months, and attenuate after around two years, remaining small and positive but imprecisely estimated at longer horizons. The any-employment and same-firm series track each other closely, with the former being slightly higher for the first two years and slightly lower at the end of the estimation window. The close tracking of the two series suggests that on the one hand the programme did not displace employment for other employers, but on the other hand it did not work as a stepping stone by which the employee would go on to find additional employment opportunities because of the programme.



(a) Employment probability at the 48-month horizon



(b) Cumulative months in employment within 48 months

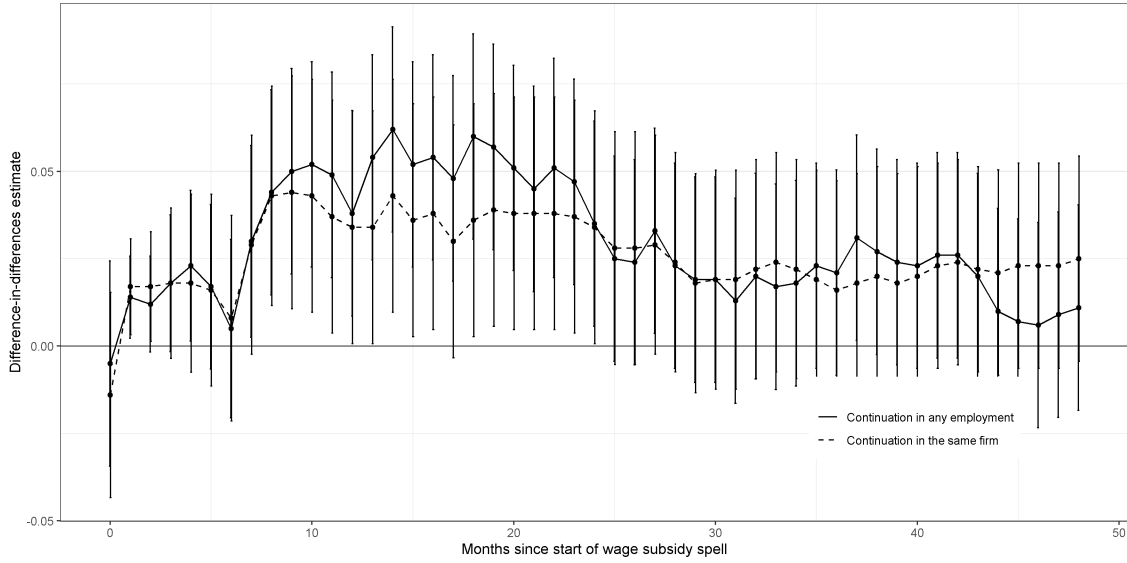
Figure 4: Quarterly event-study estimates of the effect of the regularisation reward on employment outcomes

Notes: Based on Equation (1). The treatment group consists of long-term unemployed hiring-subsidised workers in private-sector firms; the control group consists of short-term unemployed hiring-subsidised workers in private-sector firms. The estimation sample covers hiring subsidy spells beginning 2017:1–2019:6. The omitted reference period is 2018:Q3. Panel A reports effects on the probability of being employed at the 48-month horizon; Panel B reports effects on cumulative months in employment over the same horizon. Both panels show estimates for continuation in the same firm and for continuation in any employment; both series include demographic controls. Vertical bars denote 95% confidence intervals. Demographic controls include age and age squared, and indicators for gender, non-native mother tongue, region of residence, municipality type, educational level, employment history, and duration of the preceding unemployment spell.

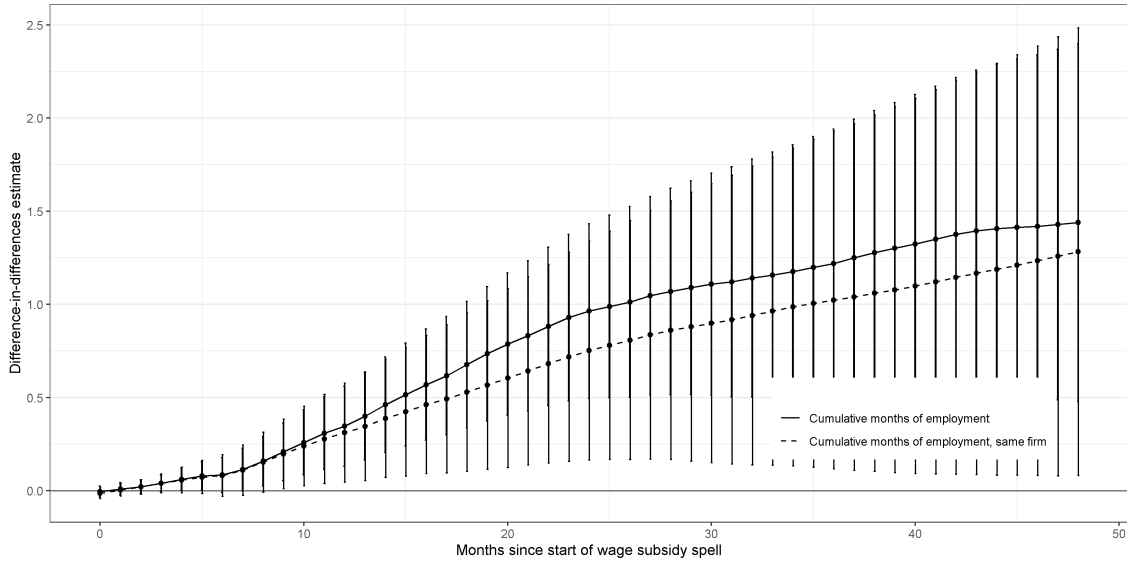
Panel B shows the cumulative employment months counterpart, where the same pattern holds: the any-employment and same-firm series are close throughout all horizons, again pointing to the absence of both displacement and stepping-stone effects. Figure A6 in the Appendix confirms this directly, with estimates for cumulative employment months worked for other employers being close to zero at all horizons. The slightly higher any-employment estimates are consistent with selection effects whereby the programme led employers to prioritize workers with stronger longer-term employability in general.

Finally, to summarize the pooled DiD estimates, Table 2 reports results from the 48-month horizon model under two specifications: a baseline model which only includes a treatment group dummy and year-month dummies, and a model with demographic control variables (see Table 1, Panel B; excluding variables concerning previous employment history which for some observations have missing values). The results confirm the earlier findings: the programme had no statistically significant effect on employment probability at the 48-month horizon (estimate: 0.011, s.e. 0.015), while the effect on cumulative employment months is positive and statistically significant (1.439 months, s.e. 0.481). The estimates attenuate slightly when demographic controls are added, consistent with the selection effects documented below, though it remains significant and of similar magnitude.

In the Appendix Table A1 assesses the robustness of the pooled DiD with respect to the treatment period status of 2018:Q4; the results change slightly at most. Table A2 tests for heterogeneity with respect to age and prior employment history in the same industry. The age split yields broadly similar estimates across subgroups. The industry history split reveals more pronounced heterogeneity: for cumulative employment months, the effect is concentrated among workers without prior same-industry experience (2.1 months), while the estimate for workers with same-industry experience is small and not statistically significant. This suggests the programme was particularly effective for workers seeking to establish themselves in a new field, while those already embedded in an industry may have been on a trajectory toward more permanent employment regardless of the reward. Table A3 assesses whether the programme resulted in an increase in the probability of a hiring-subsidised



(a) Employment probability at horizon $h = 0, \dots, 48$



(b) Cumulative employment months at horizon $h = 0, \dots, 48$

Figure 5: Difference-in-differences estimates of the effect of the regularisation reward for varying outcome horizons

Notes: Based on Equation (2). The treatment group consists of long-term unemployed hiring-subsidised workers in private-sector firms; the control group consists of short-term unemployed hiring-subsidised workers in private-sector firms. The estimation sample covers hiring subsidy spells beginning 2017:1–2019:6. Panel A reports effects on the probability of being employed at horizon h ; Panel B reports effects on cumulative months in employment up to horizon h . Both panels show estimates for continuation in the same firm and for continuation in any employment. Vertical bars denote 95% confidence intervals. Demographic controls include age and age squared, and indicators for gender, non-native mother tongue, region of residence, municipality type, educational level, employment history, and duration of the preceding unemployment spell.

Table 2: Treatment effects on employment outcomes at 48 months

	(1)	(2)
Panel A: Probability of employment		
Treatment effect	0.016 (0.015)	0.011 (0.015)
Num.obs	21093	21093
Panel B: Cumulative employment months		
Treatment effect	1.618*** (0.490)	1.439** (0.481)
Num.obs	21093	21093
Demographic controls		X

Notes: Entries are DiD estimates of the effect of the regularisation reward on employment outcomes at the 48-month horizon, based on Equation (2). The treatment group consists of long-term unemployed hiring-subsidised workers in private-sector firms; the control group consists of short-term unemployed hiring-subsidised workers in private-sector firms. The estimation sample covers hiring subsidy spells beginning 2017:1–2019:6. All columns include year-month-of-spell fixed effects. Column (1) additionally includes no further controls; column (2), the baseline specification, additionally includes demographic controls. Demographic controls include age and age squared, and indicators for gender, non-native mother tongue, region of residence, municipality type, educational level, employment history, and duration of the preceding unemployment spell. Standard errors in parentheses. ⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

worker returning to unemployment because of a termination during trial period, as some firms could also respond to the incentive by signing a permanent contract but terminating the contract during the trial period. The point estimates are positive but not statistically significant.

From a policy standpoint, the programme’s results appear mixed. While the programme does not seem to have improved employment in the long run and did not make hiring subsidies work any better as a stepping stone for employment for other employers, it did temporarily boost employment outcomes, and this increase in employment, taken at face value, seems quite cost-effective. The baseline estimate for the increase in cumulative employment months four years after the start of the hiring subsidy spell is 1.44 months, and as the target group included 2,414 hiring subsidy spells, this translates to 288 additional employment years created by the programme. The target group received 576 regularisation rewards at unit cost of 4,000 euros, with a total cost of approximately 2.3 million euros. Contrasting these two results gives a fiscal cost of an employment year of approximately 8,000 euros, which is quite low.⁸

⁸Most studies of active labour market policies do not directly report a cost-per-employment-year figure.

The regularisation reward changed firms’ incentives, and this change in incentives may have had an effect on which types of employers firms hire as hiring-subsidised workers in the first place. These possible selection effects are important for two reasons. First, indications of selection effects by observable characteristics raise the possibility of selection along unobservable dimensions, not controlled in the estimates presented in Table 2. Second, policymakers likely have preferences over the allocation of hiring subsidies between different types of unemployed job-seekers, and selection effects may point to possible unintended consequences in this regard.

5.2 Selection effects

Potential hiring-subsidised workers differ substantially: some can meet immediate demand for low-skilled labour, while others, with some on-the-job training, may provide a better long-term fit. A regularisation reward shifts employer incentives toward the latter type. To examine whether the reward programme changed hiring patterns across observable dimensions, I estimate simple difference-in-differences regressions with worker characteristics as outcome variables.⁹ The results are reported in Table 3. They indicate that, following the introduction of the reward, firms hired younger job-seekers, workers with stronger employment histories, and those with prior experience in the same firm. By contrast, I find no evidence of selection by gender, education, or region.

These selection effects nuance the interpretation of the programme’s employment impacts. First, they show that firms respond actively to incentives by altering which types of workers they take on, and not all relevant dimensions of selection may be observable.

An exception is Cahuc et al. (2019), who study a temporary French hiring credit and report a cost per job created of approximately one quarter of labour costs or 700 euros per month (in 2009 euros), which is similar in magnitude to, if slightly higher than, the figure implied by the present study. Cahuc et al. also note that the cost per job from a permanent programme would be higher due to wage responses, as firms would partly pass on the subsidy to workers through higher wages rather than creating new jobs — a caveat that applies to the present setting as well, should the regularisation reward be made permanent. For cost-per-job estimates from other policy contexts see Chodorow-Reich (2019) and Saez et al. (2021), who report figures of around \$50,000 and \$60,000 respectively from US fiscal stimulus and payroll tax programmes.

⁹In addition to worker characteristics, I also tested whether the programme changed the duration of hiring subsidy spells. Spell duration is a potential margin of adjustment and would affect the cost-benefit analysis, but I find no evidence of such an effect.

This means that measured employment effects may conflate the impact of the programme with compositional changes in the workforce; this applies to any similar programme as well unless employer–employee matches are randomized, which is rarely policy-relevant. Second, the findings suggest that, even if this type of a regularisation programme raises conversions into permanent jobs among hiring-subsidised workers, it may simultaneously restrict access to hiring subsidies for less employable job-seekers. This raises a broader question about the purpose of hiring subsidies and their role in active labour market policy: are they primarily instruments for providing employment opportunities for disadvantaged job-seekers, or for creating long-term matches between job-seekers and employers?

Table 3: Selection on observables: DiD effects on baseline characteristics

	DiD estimate		Pre-treatment mean (control, pre)	N
Age	−1.056*	(0.424)	34.080	21093
Female	0.004	(0.017)	0.402	21093
Foreign-language	−0.011	(0.011)	0.125	21093
Region: Uusimaa	0.000	(0.014)	0.178	21093
Only primary education	0.006	(0.013)	0.138	21093
No previous employment in 5 years	0.008	(0.013)	0.140	21093
Days since previous employment	−54.065**	(16.630)	320.559	17116
Employment history in same industry (1-digit)	−0.033+	(0.017)	0.412	21093
Employment history in same industry (5-digit)	−0.001	(0.015)	0.210	21093
Employment history in same firm	0.005	(0.012)	0.120	21093
Previous employment in same industry (1-digit)	0.028	(0.018)	0.321	17116
Previous employment in same industry (5-digit)	0.030*	(0.014)	0.149	17116
Previous employment in same firm	0.045***	(0.012)	0.086	17116

Notes: Each row reports a DiD estimate of the effect of the regularisation reward on a worker characteristic measured at the start of the hiring subsidy spell, based on the interaction term in Equation (2) without demographic controls. The treatment group consists of long-term unemployed hiring-subsidised workers in private-sector firms; the control group consists of short-term unemployed hiring-subsidised workers in private-sector firms. The estimation sample covers hiring subsidy spells beginning 2017:1–2019:6. Pre-treatment mean refers to the control group mean in the pre-treatment period (2017:1–2018:12). Standard errors in parentheses. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

6 Conclusion

This paper studies the effects of a temporary Finnish labour market programme introduced in 2019, which paid firms a one-time regularisation reward equivalent to approximately 1.5 months of labour costs for offering permanent contracts to long-term unemployed hiring-subsidised workers. The programme was motivated by the observation that subsidised workers frequently return to unemployment after the subsidy is exhausted, and sought to address this by strengthening firms' incentives to retain subsidised workers on a permanent basis.

From a policy standpoint, the programme's results appear mixed. While the programme does not seem to have improved employment in the long run and did not make hiring subsidies work any better as a stepping stone for employment for other employers, it did temporarily boost employment outcomes, and this increase in employment, taken at face value, seems quite cost-effective. The baseline estimate for the increase in cumulative employment months four years after the start of the hiring subsidy spell is 1.44 months, and as the target group included 2,414 hiring subsidy spells, this translates to 288 additional employment years created by the programme. The target group received 576 regularisation rewards at unit cost of 4,000 euros, with a total cost of approximately 2.3 million euros. Contrasting these two results gives a fiscal cost of an additional year of employment of approximately 8,000 euros, which is quite low. However, the selection effects discussed in the next paragraph suggest that this estimate should be interpreted with caution, as compositional changes in the treatment group may have contributed to the measured employment gains.

The programme also appears to have altered employer behaviour in terms of which workers were hired into hiring-subsidised employment. Following the introduction of the reward, firms hired younger job-seekers with shorter unemployment histories and stronger prior attachment to the same firm, while no significant changes were found along dimensions such as gender, education, or region. This finding has two implications. First, as firms adjusted their hiring along observable dimensions, they may have done so along unobservable dimensions as well, which would introduce upward bias into the estimated employment effects. Second, the results suggest that making such a regularisation reward a permanent feature of a hiring

subsidy system would shift the allocation of subsidies toward more job-ready workers (those with better employment histories and stronger firm attachment). This would likely come at least partly at the expense of the most disadvantaged job-seekers, for whom hiring subsidies are primarily intended. The findings thus raise a broader question about the design of hiring subsidy programmes: whether they are best understood as instruments for providing employment opportunities, even temporary, to the hardest-to-employ, or for facilitating good long-term matches between firms and workers with reasonable employment prospects.

Several broader lessons can be drawn from this study. I do not observe contract type, but only 16% of workers who continued with the same firm six months after the subsidy spell had ended are associated with a collected reward. Partly this may reflect continued employment on a temporary contract, but it is likely that the reward had imperfect take-up. This points to the importance of implementation frictions even in relatively simple programmes. This echoes findings from the broader literature on take-up of social programmes, and suggests that informational barriers and administrative costs deserve attention in the design of active labour market policies. The results also qualify the more optimistic short-run findings of Ciani et al. (2019), who find large effects of a similar Italian programme on permanent contract conversions, but are limited to a one-year window. The present study, which follows participants over four years, finds that the short-run employment boost from regularisation rewards does not translate into lasting employment gains, tempering the case for such programmes as a tool for improving long-term labour market outcomes for the long-term unemployed.

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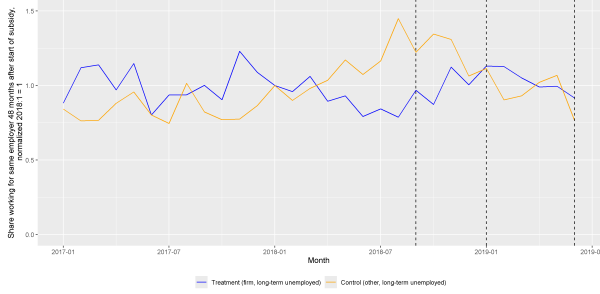
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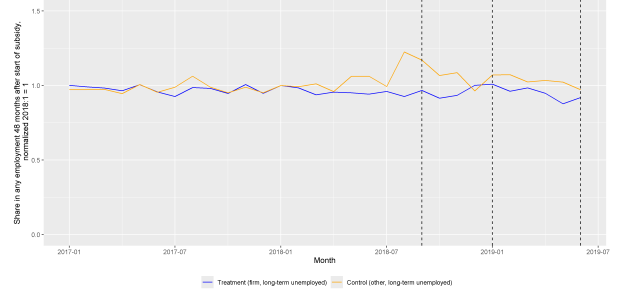
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A APPENDIX

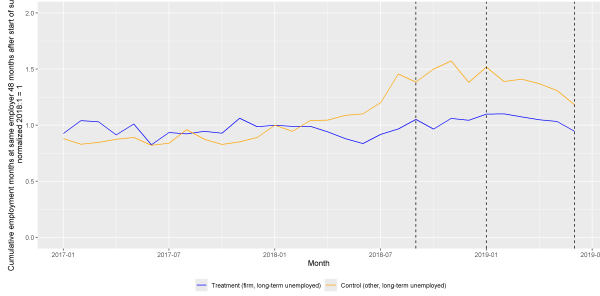
A.1 Additional figures and tables



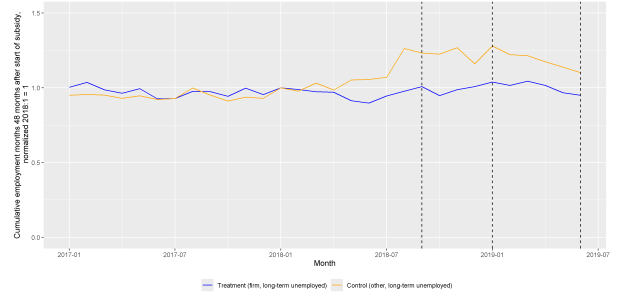
(a) Employment probability, same employer



(b) Employment probability, any employer



(c) Cumulative employment months, same employer



(d) Cumulative employment months, any employer

Figure A1: Normalised employment outcomes in the treatment group and an alternative control group

Notes: Normalised employment outcomes (2018:1=1) in the treatment group (long-term unemployed in firms) and for an alternative control group (long-term unemployed in the non-profit sector). Employment outcomes are measured 48 months after the beginning of the hiring-subsidy spell.

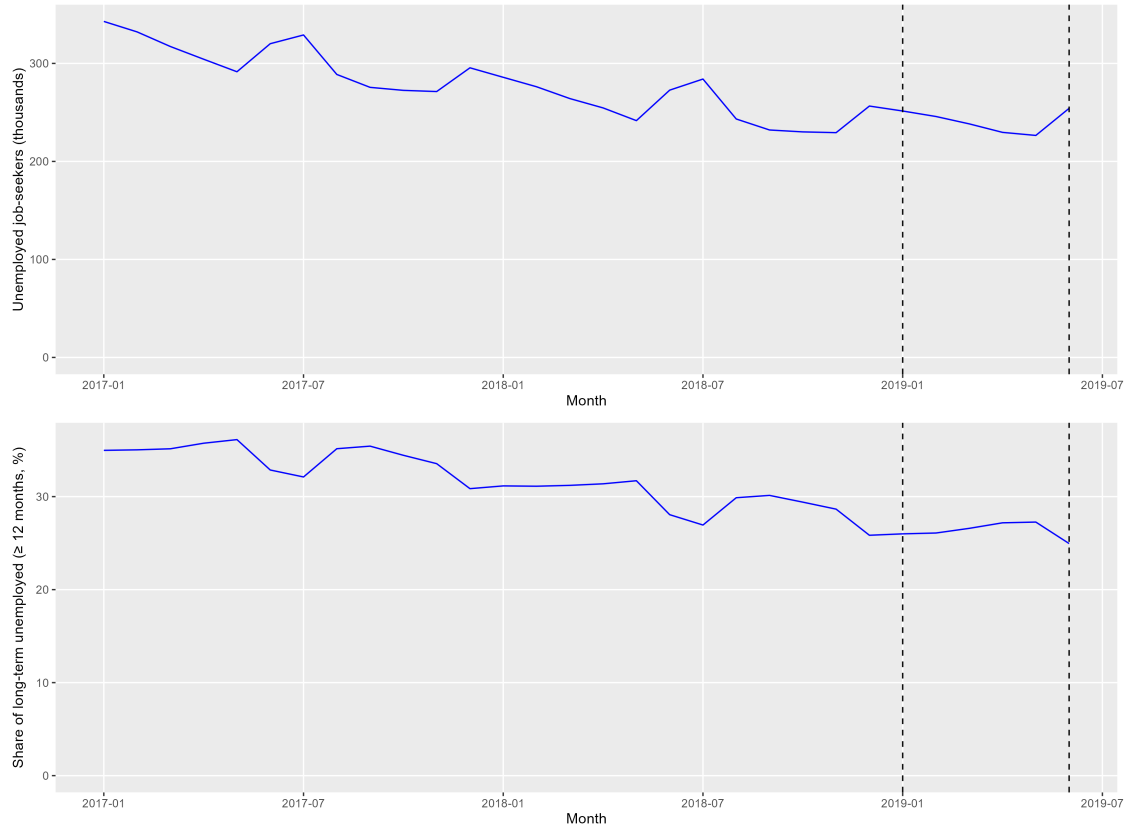
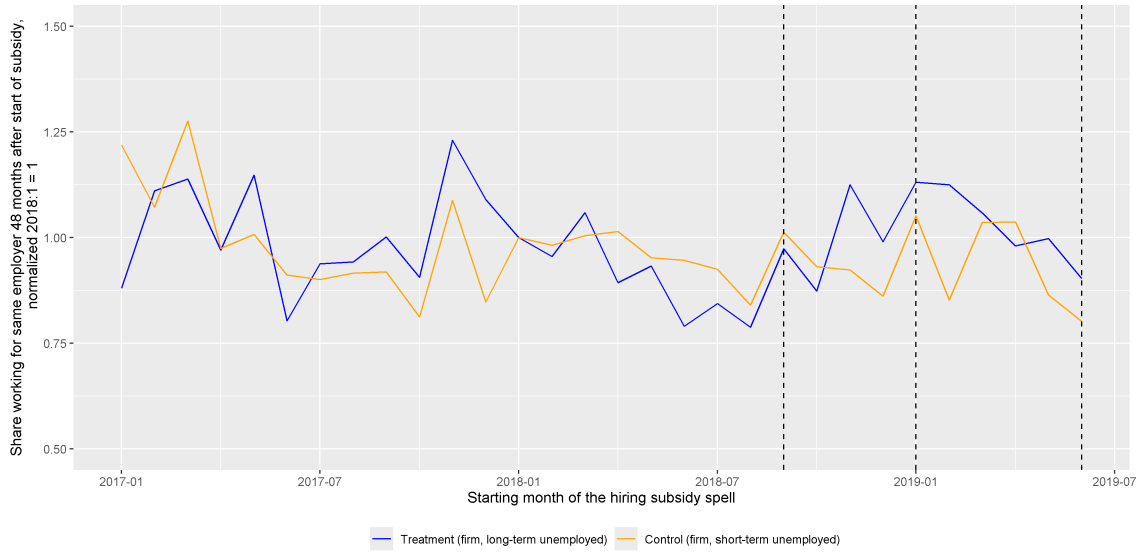
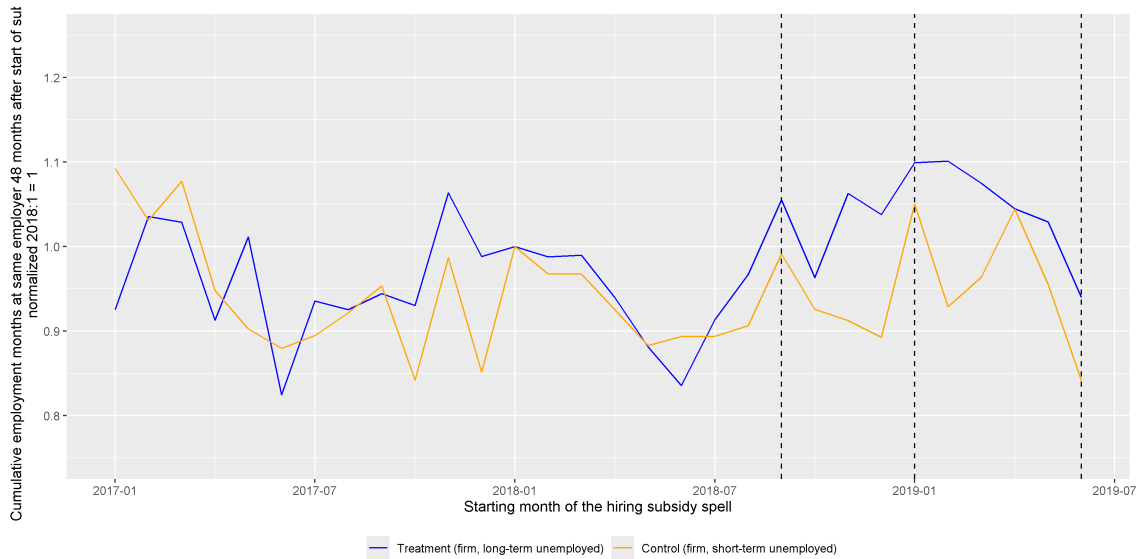


Figure A2: Unemployed job-seekers and share of long-term unemployed in Finland, 2017:1–2019:6

Notes: Number of unemployed job-seekers (upper panel) and share of long-term unemployed job-seekers (lower panel) in Finland, 2017:1–2019:6. Dashed vertical lines indicate the beginning of the treatment period (2019:1) and the end of the treatment period (2019:6). Source: Statistics Finland, Employment Service Statistics.



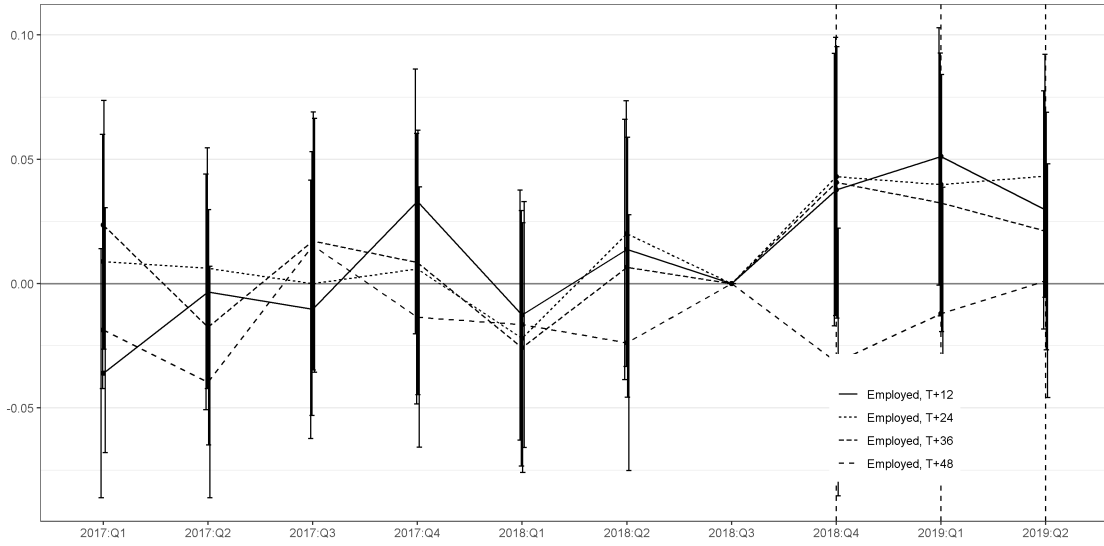
(a) Employment probability 48 months after the start of the hiring subsidy spell, same firm



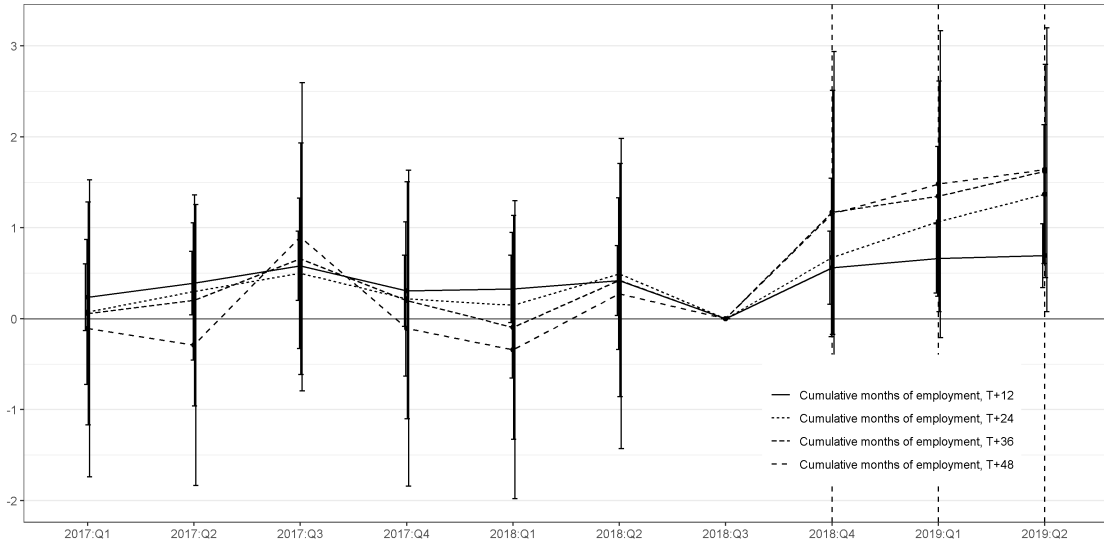
(b) Cumulative months in employment within 48 months of the hiring subsidy start, same firm

Figure A3: Firm-specific employment outcomes of hiring-subsidised workers by starting month of the hiring subsidy spell

Notes: Panel A reports the probability of being employed in the same firm 48 months after the start of the hiring subsidy spell, while Panel B reports cumulative months in employment in the same firm over the same horizon. Outcomes are shown separately for the treatment group (long-term unemployed in firms) and the control group (short-term unemployed in firms) and are normalised to one in January 2018.



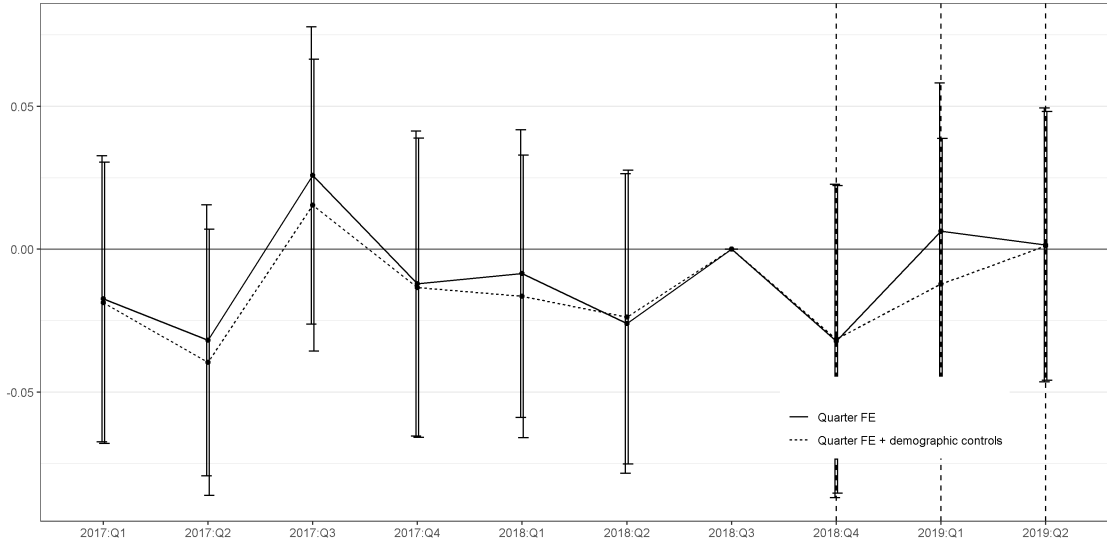
(a) Employment probability 12, 24, 36 and 48 months after the beginning of the hiring subsidy spell



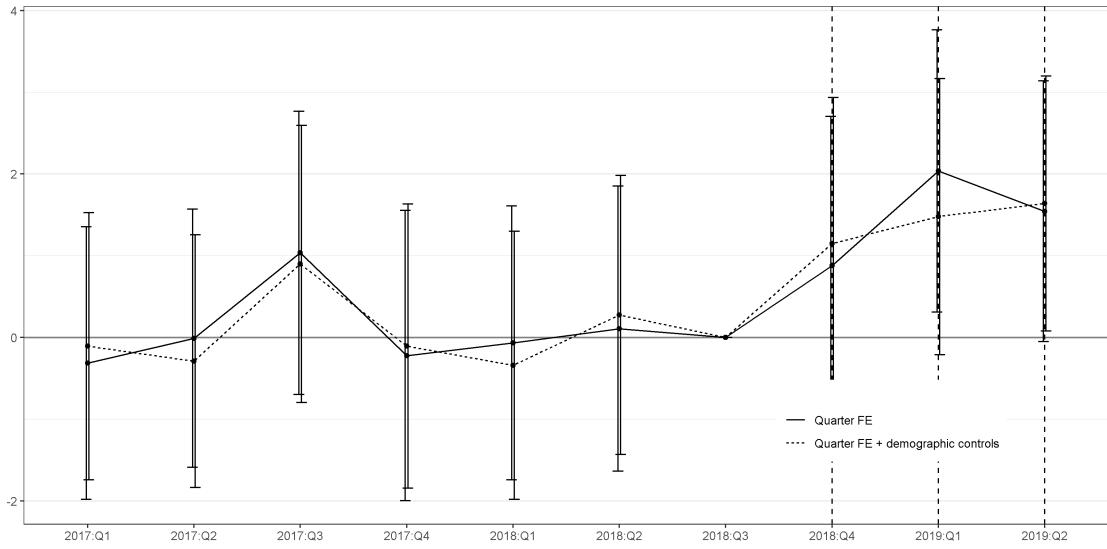
(b) Cumulative months in employment 12, 24, 36 and 48 months after the beginning of the hiring subsidy spell

Figure A4: Quarterly event-study estimates of the effect of the regularisation reward on employment outcomes, varying outcome horizons

Notes: Based on Equation (1). The treatment group consists of long-term unemployed hiring-subsidised workers in private-sector firms; the control group consists of short-term unemployed hiring-subsidised workers in private-sector firms. The estimation sample covers hiring subsidy spells beginning 2017:1–2019:6. The omitted reference period is 2018:Q3. Panel A reports effects on the probability of being employed at 12, 24, 36 and 48 month horizons; Panel B reports effects on cumulative months in employment over the same horizons. Vertical bars denote 95% confidence intervals. Demographic controls include age and age squared, and indicators for gender, non-native mother tongue, region of residence, municipality type, educational level, employment history, and duration of the preceding unemployment spell.



(a) Employment probability 48 months after the beginning of the hiring subsidy spell



(b) Cumulative months in employment 48 months after the beginning of the hiring subsidy spell

Figure A5: Quarterly event-study estimates of the effect of the regularisation reward on employment outcomes, with and without demographic controls

Notes: Based on Equation (1). The treatment group consists of long-term unemployed hiring-subsidised workers in private-sector firms; the control group consists of short-term unemployed hiring-subsidised workers in private-sector firms. The estimation sample covers hiring subsidy spells beginning 2017:1–2019:6. The omitted reference period is 2018:Q3. Panel A reports effects on the probability of being employed at the 48-month horizon; Panel B reports effects on cumulative months in employment over the same horizon. Each panel shows estimates for a specification with quarter fixed effects only and a specification additionally including demographic controls. Vertical bars denote 95% confidence intervals. Demographic controls include age and age squared, and indicators for gender, non-native mother tongue, region of residence, municipality type, educational level, employment history, and duration of the preceding unemployment spell.

Table A1: Robustness of treatment effects on employment outcomes at 48 months with respect to the treatment status of 2018:Q4

	(1)	(2)	(3)
Panel A: Probability of employment			
Treatment effect	0.011 (0.015)	0.009 (0.015)	0.003 (0.013)
Panel B: Cumulative employment months			
Treatment effect	1.439** (0.481)	1.516** (0.486)	1.477*** (0.416)
Num.obs	21093	19494	21093
Demographic controls	X	X	X
Treatment of 2018:Q4	Pre-treatment period	Dropped	Treatment period

Notes: Entries are DiD estimates of the effect of the regularisation reward on employment outcomes at the 48-month horizon, based on Equation (2). The treatment group consists of long-term unemployed hiring-subsidised workers in private-sector firms; the control group consists of short-term unemployed hiring-subsidised workers in private-sector firms. The estimation sample covers hiring subsidy spells beginning 2017:1–2019:6. Column (1) assigns 2018:Q4 spells to the pre-treatment period (baseline); column (2) drops 2018:Q4 spells from the estimation sample; column (3) assigns 2018:Q4 spells to the treatment period. All columns include year-month-of-spell fixed effects and demographic controls. Demographic controls include age and age squared, and indicators for gender, non-native mother tongue, region of residence, municipality type, educational level, employment history, and duration of the preceding unemployment spell. Standard errors in parentheses. ⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A2: Heterogeneity in treatment effects on employment outcomes at 48 months

	baseline	age		emp. history in same industry (1-digit)	
		young	old	no	yes
	(1)	(2)	(3)	(4)	(5)
Panel A: Probability of employment					
Treatment effect	0.011 (0.015)	−0.010 (0.024)	0.031 (0.019)	0.037+ (0.019)	−0.025 (0.023)
Panel B: Cumulative employment months					
Treatment effect	1.439** (0.481)	1.747** (0.639)	1.219** (0.757)	2.136*** (0.629)	0.373 (0.750)
Num.obs	21093	11599	9494	12697	8396
Demographic controls	X	X	X	X	X

Notes: Entries are DiD estimates of the effect of the regularisation reward on employment outcomes at the 48-month horizon, based on Equation (2). The treatment group consists of long-term unemployed hiring-subsidised workers in private-sector firms; the control group consists of short-term unemployed hiring-subsidised workers in private-sector firms. The estimation sample covers hiring subsidy spells beginning 2017:1–2019:6. Column (1) is the baseline full-sample estimate; columns (2)–(3) split the sample by age (young: under 40; old: 40 and above); columns (4)–(5) split by whether the worker has prior employment history in the same 1-digit industry. All columns include year-month-of-spell fixed effects and demographic controls; age and age squared are excluded from columns (2)–(3), and employment history indicators are excluded from columns (4)–(5). Demographic controls include age and age squared, and indicators for gender, non-native mother tongue, region of residence, municipality type, educational level, employment history, and duration of the preceding unemployment spell. Standard errors in parentheses. ⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

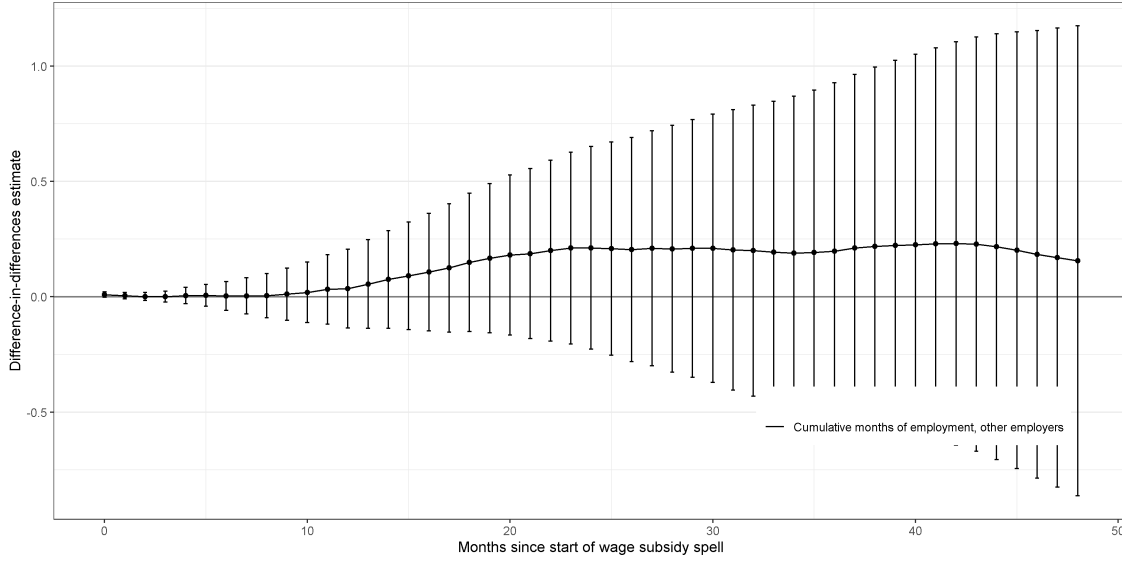


Figure A6: Difference-in-differences estimates: cumulative employment months with other employers, varying outcome horizons

Notes: Based on Equation (2). The treatment group consists of long-term unemployed hiring-subsidised workers in private-sector firms; the control group consists of short-term unemployed hiring-subsidised workers in private-sector firms. The estimation sample covers hiring subsidy spells beginning 2017:1–2019:6. The figure reports effects on cumulative months of employment with employers other than the initial hiring-subsidy employer. Vertical bars denote 95% confidence intervals. Demographic controls include age and age squared, and indicators for gender, non-native mother tongue, region of residence, municipality type, educational level, employment history, and duration of the preceding unemployment spell.

A.2 Termination during trial period

The regularisation reward was granted based on a permanent job contract between the firm and the hiring-subsidised employee. Like temporary contracts, permanent contracts can and typically do include a trial period during which the employer can terminate the contract without the need to provide specific reasons, unless the termination is discriminatory or otherwise unlawful. The unemployment registry data includes, for each new unemployment spell, end date of the previous employment spell and a classification as to why the previous unemployment spell had ended. Linking the hiring subsidy spells to possible later unemployment spells allows testing whether the programme also resulted in an increase of trial period terminations.

I test this using two different specifications for linking the hiring subsidy spells with later unemployment spells. First I use an exact match, where trial period termination is observed if the end date of the hiring subsidy spell exactly coincides with an end date of a previous

employment spell in a new unemployment spell, where the previous employment spell has ended due to termination during the trial period. The second specification allows for some measurement error in the dates by looking at new unemployment spells where the previous employment spell ended due to termination in the trial period 12 months after the beginning of the hiring subsidy spell. Baseline probabilities of trial period termination are 1.8% and 2.6% for these two specifications.

Positive effects on trial period termination could indicate misuse of the programme, where a firm only collects the reward with no intention of employing the worker in the longer run. On the other hand, if the programme works as intended and nudges some firms to offer permanent contracts from the beginning, some of these matches will turn out not be in the firm's interests and will result in a termination during the trial period - in the counterfactual the firm might simply exhaust the temporary contract and not offer continuation.

Table A3 presents results of this estimation. Although the point estimates are quite large in relative terms, about 25% relative to baseline, they are not precise enough to rule out a zero effect. The point estimates for the employee returning to unemployment because a temporary contract has ended (not shown here) are similar in magnitude with opposite sign.

Table A3: Treatment effects on trial period termination

	(1)	(2)
Panel A: Termination at end date of hiring subsidy spell		
Treatment effect	0.005 (0.006)	0.004 (0.006)
Panel B: Termination during 12 months following beginning of hiring subsidy spell		
Treatment effect	0.005 (0.007)	0.005 (0.007)
Num.obs	21093	21093
Demographic controls		X

Notes: Entries are DiD estimates of the effect of the regularisation reward on the probability of returning to unemployment due to termination during the trial period, based on Equation (2). The treatment group consists of long-term unemployed hiring-subsidised workers in private-sector firms; the control group consists of short-term unemployed hiring-subsidised workers in private-sector firms. The estimation sample covers hiring subsidy spells beginning 2017:1–2019:6. Panel A defines termination as occurring when the end date of the hiring subsidy spell coincides exactly with the end date of a subsequent employment spell ending due to trial period termination; Panel B uses a broader definition based on any trial period termination within 12 months of the beginning of the hiring subsidy spell. All columns include year-month-of-spell fixed effects. Column (1) includes no further controls; column (2) additionally includes demographic controls. Demographic controls include age and age squared, and indicators for gender, non-native mother tongue, region of residence, municipality type, educational level, employment history, and duration of the preceding unemployment spell. Standard errors in parentheses. ⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.