Displacement effects of the Finnish hiring subsidy Evidence from a funding discontinuity

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Abstract

Finland offers a subsidy to employers for hiring individuals with low employment prospects. In 2015, changes to the subsidy's funding caused some regions to unexpectedly suspend new subsidy grants. I examine wages and employment in these regions using a differences-in-differences setting, with regions where the subsidy remained available serving as control areas. Among the population with low employment prospects, the temporary halt reduced the wage sum from subsidized employment by 37.2 million euros (−32%) over the following year and relative to the control areas. The similarly measured net decline in the total wage sum in this group was 32.7 million euros (−7.3%). Because the reductions in subsidized and total wage sums are very similar, the suspension of new subsidy grants appears to have been largely uncompensated by increases in unsubsidized employment. This suggests that any displacement effects from the subsidy, where the subsidized jobs would be replacing unsubsidized ones, are likely to be small.

1 Introduction

From 2006 to 2022, almost half a million Finns have been in jobs subsidized by a hiring subsidy scheme. The scheme aims to provide a stepping stone to regular employment for individuals that have difficulty finding a job otherwise. In 2015, a number of Finnish regions had almost exhausted their appropriations for the subsidy by mid-year, due to a combination of funding changes and technical issues.

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This paper examines the effects this reduction in availability had on regional populations of difficult-to-employ individuals. It also presents new evidence on the medium- and long-term effects of the subsidy on the participants, using accurate, high-frequency administrative data on subsidies, employment, wages and unemployment with full population coverage.

I examine the potential displacement effects within a large subpopulation of difficult-to-employ individuals, covering about a quarter of the regional working-age population. Using less affected regions as controls in a differences-in-differences setup, the reduction in subsidized jobs is estimated to have directly reduced the group's overall wages by 37.2 million euros (32%) over the following year. Wages from all sources combined are estimated to have subtracted by 32.7 million (7.3%). If under normal circumstances subsidized jobs were replacing other job opportunities, then halting new subsidy grants should correspondingly have been offset by employment gains elsewhere. Removing a source of displacement should thus have led to a smaller net reduction in aggregate employment and wages.

Hiring subsidies are generally estimated to be among the most effective active labour market policies (ALMPs). Reviews by Card, Kluve, and Weber (2017), Brown and Koettl (2015), Filges et al. (2015), Vooren et al. (2019), and Levy Yeyati, Montané, and Sartorio (2019) vary substantially in their approach, but all conclude that hiring subsidies in the private sector are likely to improve post-programme employment outcomes. Subsidized employment in the public sector is usually estimated to be ineffective in terms of later employment.

The reviews also highlight a number of concerns and limitations regarding ALMPs. Displacement effects are an important caveat: the employment gains observed for participants might be partially or fully gained at the expense of non-participants.

Displacement effects have been studied using a wide variety of approaches from both micro- and macro-level data. The results range from no discernible displacement to substantial displacement effects. Of studies concerning displacement due to hiring subsidies, Desiere and Cockx (2022), Blundell et al. (2004), and Puhani (2003) find no evidence of displacement, while Gautier et al. (2018), Calmfors et al. (2001), Pehkonen (1997), Skedinger (1995), and Dahlberg and Forslund (2005) find moderate to very large effects. For a Swedish subsidy scheme, Lombardi, Skans, and Vikström (2018) conclude that within-firm displacement effects depended on whether the subsidy was subject to approval by local employment offices, suggesting that careful screening may play an important part. Kangasharju (2007) examine the potential displacement effects of a previous version of the Finnish subsidy scheme at the firm level, and finds no evidence of displacement. Hirvonen (2021) attempts to replicate the analysis by Dahlberg and Forslund for Finland, but concludes the instruments used appear to be invalid in the Finnish institutional context.

The present study draws partial inspiration from a seminal block experiment by

Crépon et al. (2013). In their study, different blocks (regions) were randomly assigned different treatment rates; the treatment rate dictated the fraction within block that would be randomly assigned to a job placement assistance treatment. This allowed the authors to identify the displacement effects: if higher treatment rates correspond to lower employment rates among *non*-participants, this indicates displacement.

For identifying potential displacement effects, the paper proceeds as follows. The regionally reduced availability of the subsidy, driven by the changes to funding, is interpreted as an analogue of the exogenous, regionally varying treatment rate in the study by Crépon et al. The follow-up period spans from July 2015 to June 2016, using a control period from July 2014 to June 2015. The treatment and control areas are defined based on reports by regional officials of available funds in July 2015; roughly a million working-age Finns reside in the designated regions. For both areas, an at-risk group who are predicted unlikely to enter open market employment over the following 12 months is identified based on 44 observables. This set includes roughly a quarter of the regional working-age population. The group receives more than eighty percent of subsidized wages but only about two percent of unsubsidized wages over the next year.

Two difference-in-differences estimates are calculated for the at-risk group: employment from (a) subsidized jobs and from (b) all jobs, both measured in aggregate wages. Displacement usually means that some increase $+w$ in the (a) subsidized jobs translates to a smaller net increase $(1-d)w$ in (b) total employment, because some subsidized jobs were created at the expense of other jobs. If this displacement effect was substantial, one would expect that reducing subsidized jobs by $-w$ should correspondingly only reduce overall employment by a smaller net change of $(1-d)w$. In the present case, the reduction in wages from subsidized jobs, $\Delta w = €-213$ per month, was very close to the net reduction in wages from all jobs, $-€187$. This implies that the displacement effects related to subsidized jobs are probably small.

The setup is grounded in the following assumptions. First, it is assumed that without the change in funding, the employment rates across the two areas would have followed parallel trends. This assumption can be justified in terms of both the institutional basis for the depletion of funds (Section 2) and pre-trends (Section 8). Second, in terms of the effect measured, it is assumed that the displacement effects would be confined to this particular at-risk group and these regions. The areas considered are large, spanning about 95 000 square kilometer, and observable short-term rates of internal migration and cross-region commuting are small. Most subsidized jobs pay wages clearly below the median, and most of their professions are ranked at the low end of the ISCO skill level requirement classification. Put together, it appears reasonable to expect that the potential displacement effects would be most visible among jobseekers with lower than average short-term employment prospects.

This approach to displacement effects appears novel to the literature on hiring sub-

sidies. Previously, Forslund, Johansson, and Lindqvist (2004) has used a funding discontinuity to study the direct effects of hiring subsidies in Sweden. Their institutional context was, however, quite different: the availability of the subsidy varied at the level of small municipalities rather than regional labour markets. Forslund, Johansson, and Lindqvist interpret the variation in availability *within* labour markets and over time as the exogenous driver of selection into subsidized jobs, to identify effects on participants. In the present paper, the variation in availability *between* labour markets and over time is used to identify effects in the regional labour markets themselves.

In the second empirical part of the paper, the direct effects of the subsidy on participants are studied by matching by observables. The overall results are similar to those obtained through matching by Aho et al. (2018) and Asplund et al. (2018): subsidized jobs in the private sector improve later unsubsidized wages by roughly 4,000 euros per year, while public and third sector jobs appear to have no effects. While the share of private sector jobs decreased when the overall availability of the subsidy was reduced, the average per-sector effects stayed largely similar.

The depth of the available data allows this study to make a number of new contributions regarding the effects on participants. First, it appears that the participants are not more likely to become employed elsewhere after the subsidy period. Rather, the employers and employees in private sector jobs form a persistent bond, which may last up to ten years. Second, the matching results are not sensitive to significant variations in the matching method, to broadening the sample from which matches are picked, or how wages from later subsidized jobs are taken into account. Third, the subsidies appear widely dispersed across employers and industries in the private and public sectors, and few employers use them both intensively and repeatedly. Fourth, the net effect on public finances appears to be positive in the private sector, neutral in the public sector, and negative in the third sector, after accounting for taxes, transfers, and the subsidy itself.

The rest of the paper is organized as follows. Section 2 explains the institutional context for the subsidy and the depletion of funds in 2015. Section 3 covers data and definitions, while Section 4 documents how the populations with low employment prospects are identified. Overall descriptive evidence appears in Section 5. Section 6 is the first empirical part, presenting evidence on the displacement effects. The second empirical part, Section 7, examines the direct effects on the participants. Section 8 examines the sensitivity of the empirical results, and Section 9 concludes.

2 Institutional setting

The hiring subsidy[1] was originally introduced in Finland in 2006, replacing a number of earlier similar programs. The subsidy is paid for a limited time to an employer. To obtain the subsidy, the employer must apply for it and name a specific person they want to hire under the scheme before work can commence. The justification for the subsidy is that it should cover any additional training or induction costs for workers with low initial productivity. The goal is to allow workers to acquire sufficient experience and skills, enabling them to eventually move to regular, unsubsidized employment.

The employer-employee pair may meet in a number of ways. The available data suggests that more than 85% of subsidized jobs start by the pairs finding each other directly, rather than through assignments from public employment services or through subsidy-specific job ads.

In Finnish law, the hiring subsidy is called a wage subsidy (*palkkatuki*).[2] This paper follows the distinction by Brown and Koettl (2015), who distinguish between wage subsidies aimed at retaining existing employment and hiring subsidies aimed at creating new jobs. The Finnish hiring subsidy is characterised by a limited duration, narrow targeting at difficult-to-employ individuals, a strict approval process and limits on repeated use. The literature suggests all of these design features may improve the efficiency of such programs and reduce the magnitude of negative indirect effects, differentiating it from less constrained wage subsidies.

The subsidy applications are processed by 15 regional government offices (ELYs). In this paper, these authorities are referred to as regional authorities or simply the regions. The regional officials do not report to local polities; rather, they are supervised by the national Ministry of Economic Affairs and Employment.

A long list of individual criteria guide the application process. Broadly speaking, the law recognizes at least the following groups as potentially benefiting from the subsidy: persons without vocational or post-secondary education, immigrants, individuals with disabilities, the long-term unemployed, and persons below 25 or above 54 years of age. Satisfying such criteria may allow for longer subsidy durations in higher rates.

However, for the decision of whether the subsidy is granted at all, belonging to one of these groups is neither sufficient nor strictly necessary[3]. Rather, the officials should make a case-by-case judgement on whether the person has a significant shortage in skills

[1] The subsidy has seen many changes throughout its history: this section covers the broad strokes of the programme until 2022.

[2] In 2006, Finland also experimented with a temporary low-wage subsidy (*matalapalkkatuki*), which was a separate instrument. Huttunen, Pirttilä, and Uusitalo (2013) study the effects of the latter subsidy, and consider it effectively a payroll tax cut; this subsidy was available for any low-wage employees above the age of 54 regardless of when they were hired, and was not subject to a preapproval process. Huttunen, Pirttilä, and Uusitalo find the subsidy did not have employment effects.

[3] In 2022, the responsible ministry estimated that roughly 12% of subsidies were granted to persons who did not belong in any of the specific groups characterised above.

and whether the job in question might aid them to transition to unsubsidized work. The officials also consult a digital profiling tool which predicts a person's long-term unemployment risk from their characteristics.

The law distinguishes between subsidies to employers operating in competitive markets and other employers. The latter group refers to public and third sector employers insofar as they are not competing with private sector firms. Since the central government does not pay a subsidy to itself, public sector recipients are almost exclusively municipalities. Municipalities also have an additional incentive to hire unemployed persons into subsidized jobs, as they share some of the benefit costs for some of the long-term unemployed.[4]

Since 2015, the subsidy has been paid to employers as a percentage of subsidized wage receipts. Typically, the employer is compensated between 30% and 50% of gross wages and payroll taxes; in some circumstances, the subsidy rate can reach 100%. Before 2015, the typical subsidy was the size of a flat-rate unemployment assistance, or about 705 euros per month in 2019 levels, but could be larger depending on the circumstances. As wages in subsidized jobs tend to be low, the estimated average unit cost ended up staying largely similar from 2014 to 2015. Generally, the largest subsidy percentages apply for the most vulnerable groups outside the competitive market, primarily in the third sector.

The maximum per-individual duration of the subsidy period is typically one year, but can in some circumstances be extended to up to two years. If an employer is granted a shorter subsidy, they may later apply for an extension. After the maximum is reached, the worker must typically be unemployed for at least 10 months before a new subsidy spell can be granted.

The subsidized jobs must comply with the same regulations as other jobs, including wages, which in Finland are mostly covered by sectoral collective agreements. The subsidy alone neither prevents the employer from terminating a job contract nor justifies it. By default, a job contract is open-ended. While many subsidized jobs have a fixed termination date, a limited duration must be explicitly justified, and the subsidy alone is not a valid rationale.[5]

The system has gone through a number of revisions. A simultaneous reform in 2015 simplified the subsidy levels, adjusted the eligibility criteria for some groups, and streamlined some group-specific rules for the subsidy rates. The available evidence from later years suggests that the structural reforms did not have differential impacts across areas, and that they did not change the effects on participants. The observed changes in targeting are briefly discussed in subsection 8.2 and more thoroughly in appendix A.

[4]The government bill HE 207/2022 estimates that in 2022, the average savings to a municipality for interrupting a long unemployment spell for any reason varied between 4 400 and 6 100 euros. If the individual returns later to unemployment, the burden of their unemployment benefit costs is typically shifted back to parties other than the municipality.

[5]However, since 2017, it has been possible to hire a long-term unemployed person into a fixed-term job without further justification.

Until recently, a person could become individually entitled to a subsidized job. Their municipality would then be responsible for finding them this job. This individual mandate applied to persons who fulfill each of the following criteria: (a) they were above the age of 54, (b) they had exhausted the maximum duration of UI (100 weeks), and (c) they were not eligible to an extension of this maximum duration restricted to the oldest cohorts. This group of subsidies is separately discussed in appendix B. As these entitlements are very different from the regular subsidies, individuals predicted entitled to them are excluded when the direct effects are examined, although they are included in the sample when the displacement effects are analysed.

2.1 Funding

By mid-2015, a number of regions had mostly exhausted the funds available for the hiring subsidy. In other regions, subsidies continued to be available, albeit at lower frequencies than in the previous year. The situation was subject to two hearings in a parliamentary committee in the context of budget deliberations throughout the year. The relevant documents from these hearings, in addition to some supplementary source material, are listed in appendix C. Based on these documents, the events appear to have been driven by a combination of four primary factors.

I. Reduced flexibility in funding. In most years from 2006 to 2024, the hiring subsidy has been financed through two appropriations in the national budget: a general ALMP appropriation and a sub-appropriation under unemployment benefits.[6] In 2015 and 2016, the benefit channel was temporarily shut down.

The overall ALMP appropriation is allocated to regions using a formula based on regional measures of unemployment at the turn of the year, with a strong emphasis on long-term unemployment. Arguably, this formula approximates the *potential supply* of labour for the subsidy, i.e., the unemployed who need it the most. There are, however, two potential shortcomings. First, for individuals with very long unemployment durations, subsidized employment might not be a viable alternative, restricting *viable supply*, whether through stigma, human capital depreciation, or underlying health issues. Second, some regions with high unemployment might have fewer feasible employers willing to hire with the subsidy, constraining the *demand* side for labour.

In years before 2015 and again after 2017, this mismatch could be compensated for through the benefit appropriation, which is not earmarked to specific regions. When the temporary benefit channel was shut down, flexibility in allocating the funds between regions was significantly reduced. The government specifically cited the issues caused by

[6]The stated rationale for the benefit sub-appropriation is that most workers entering subsidized jobs are unemployed, and are assumed to have stayed unemployed without the subsidized job. The cost to the central government from one month of unemployment benefits and one month of a hiring subsidy are very similar.

inflexibility when the benefit channel was restored in 2017:[7] according to the government bills, between 71% and 92% of the regional ALMP funding had been committed by May–June in 2013–2015.

Usually, the regions have wide discretion on how to distribute the overall ALMP funds between programmes. In 2015, new earmarks covering roughly two fifths of overall funding were added to the regional appropriations. These earmarks constrained some funds to be only available for specific groups, such as the long-term unemployed. Such earmarks were reduced in later years.

II. An overall funding cut. In terms of overall ALMP funding, that funds available for the subsidy were reduced by roughly one sixth.

III. Explicit guidance from the ministry. According to the regional officials, the ministry explicitly encouraged the employment offices to actively grant subsidies to longer-term unemployed from the beginning of the year. The ministry's guidelines also explicitly state that questions such as the duration of the subsidy should be decided based on the individual's situation and needs, rather than budgetary concerns.[8]

IV. Technical bookkeeping problems and lags from previous years. After a subsidy application is granted, the subsidy is paid periodically based on actualized wages. The first payment may commonly be due six months after the job has started. Initially, the regional officials mark a part of the funds as committed based on a projection. The commitments are updated based on realized payments. This also implies that a part of the funds in early 2015 were being tied up by subsidies that had been granted in 2014.

In 2015, the subsidy system was changed from a system of mostly lump-sum subsidies to a system paying percentages of wages. While the mean unit cost per month did not end up changing much, the predictability did. The realized costs may sometimes be significantly lower or higher than projected due to unpaid absences, early terminations and overtime costs. More importantly, the regional officials stated in a joint statement that the centralized bookkeeping IT systems in use at the time were simply not up to the job, and that the ministry had implicitly admitted as much. Several documents from 2015 imply that the ministry and the regional officials had very different views on how much of the ALMP funds had been committed.

Reason I above explains why the budget cut had different effects in different regions. Factors III and IV together are important in explaining why the regions did not respond to the budget cuts by rationing the subsidies more evenly through the year. Driver IV also explains why the reduced availability came at least partly as a shock to the officials, and

[7]The channel was restored first on a temporary basis and later permanently; for additional information, see government bills HE 209/2016, HE 131/2018, and HE 155/2020. The National Audit Office (report 7/2020) also states that all the regional officials they interviewed agreed that the the transfer had improved the flexibility in the use of funds and allowed subsidized hirings to be funded more evenly throughout the year.

[8]See, for example, TEM/871/00.03.05.02/2015 p. 73.

why most potential employers and jobseekers probably did not anticipate the changes before the mid-year. First mentions in the largest daily newspaper archives about the availability of subsidy appear in May 2015. A number of other potential drivers, such as diverging trends in regional unemployment, employment or subsidy grants before mid-July 2015 are examined and excluded in the empirical section.

In October 2015, the parliament agreed to increase the ALMP appropriations for the ongoing year. However, this additional funding was only distributed to some of the regions. The reasons for choosing said regions is not entirely clear, although the appropriation document refers to the ministry's assessment of the extent of remaining funds. It is possible that this assessment was incomplete at the time due to the technical bookkeeping problems.

Additionally, the formula for the regional allocation of appropriations was changed in 2016. This change appears to have helped some regions, but not all, to accommodate potential backlog of subsidy demand that had piled up during 2015. For this analysis, only the regions where the funding remained similar to prior levels in 2016 are included.

For the purposes of this paper, six regions are examined further (presented on the map in Appendix D). The control area, labeled *not depleted* in figures and tables, consists of three regions in South-Eastern Finland which both (a) were not listed as having had their funds committed at 90% or more by the ministry by the end of the year, and (b) signaled less drastic reductions in the availability of subsidy through statements by regional officials by mid-year. The treatment area, labeled *depleted*, consists of three regions mostly in South-Western Finland which (a) stated they had partly or completely halted new subsidy grants by the mid-year, and (b) received only minor or zero new appropriations by the end of the year. The areas and their trends are further characterised in Section 5.

3 Data and definitions

The study combines individual-level full population register data on overall employment and wages, subsidized jobs, registered unemployment, taxes and transfers, and the population overall. Most of the periodical data is available at the accuracy of one day.

Population characteristics (1987–2020), most employment and wage data (1987–2018), pension, tax and transfer data (1987–2019) and data on employer finances (1987–2020) come from Statistics Finland's FOLK and FIRM modules.[9] Periods of registered unemployment and data on subsidized jobs[10] (1991–2022) are observed from the Ministry of Employment and Economic Affairs. More detailed data on unemployment assistance (2010–2021) and insurance (1999–2021), at the payment level, come from the Financial Security Authority and the Social Insurance Institution. More recent data on wage incomes, taxes and social security contributions (2019–2024) and received transfers (2021–2024) are obtained at the payment level from the Incomes Register.

Subsidized wages are calculated by comparing the subsidized periods to earning periods by employer-employee-date triplets; the exact matches cover roughly 98% of the durations. Subsidy amounts are calculated either based on the subsidy percentage and observed wages (since 2015) or an estimated subsidy amount by comparing the subsidy characteristics to legislation (before 2015).[11]

The duration of the current unemployment spell was commonly used as a control or matching variable. The spells were separately tracked for (a) registered unemployment and (b) benefit recipiency, as both have been used to determine target groups for the subsidy by the public employment services. Besides regular unemployment, such a spell also covers time in part-time unemployment, labour market training, and furloughs (suspended employment). A spell of registered unemployment ends when the person is not registered as an unemployed jobseeker for 30 days. Similarly, an unemployment benefit spell ends when the individual does not claim benefits for 30 days.

All monetary numbers are indexed to 2019 levels by the national wage index, unless otherwise noted.

[9] For this period, wages are observed at the annual level per employer-employee pair; daily wages are estimated by matching these wages with periods of employment.

[10] Data on subsidy applications was, unfortunately, not available for this study.

[11] As the percentage-based subsidies also cover employer-side social security contributions, which are not observed, these are approximated based on annual GDP statistics. Typically, these contributions do not vary much between employers and employees within a year.

4 Defining the at-risk group

Effects of the reduction in subsidy availability are examined within a broad set of individuals with low employment prospects. There is a three-fold justification for this choice. First, while the probability of entering a subsidized job clearly increases by time spent in unemployment, many individuals also start in such jobs after only a short period of unemployment. Second, the potential displacement effects might not be restricted to individuals who are registered as unemployed. Empirically, most transitions from non-employment to employment in Finland come from outside the labour force, rather than unemployment.

Finally, some restrictions on the at-risk group are necessary to identify the effects. The reduction in subsidized wages is large relative to the wages of persons with low employment prospects, but small relative to the aggregate wages of all working-age individuals. In the latter group, one could not reasonably expect to separate the signal from the noise.

Defining the at-risk group starts by collecting the resident population aged 18 to 63. This population is then split into groups with either high or low probability of having regular employment over the next year. The sample also includes persons who are presently in subsidized jobs. These individuals are included because the reduction in the availability of the subsidy also applied to extensions: that is, the depletion of funds also affected subsidized employers who applied for an extension to an ongoing subsidy.

The goal of the population split to capture a wide selection of individuals who are likely to encounter difficulties in getting employed in the regular labour market. The approach to modeling the probability of regular employment resembles the one used by Landais et al. (2021), using minimal assumptions and data-driven methods.

Regular employment is defined as a binary variable which takes value 1 if the person works in non-subsidized jobs or self-employment for at least half of the next twelve months, at a monthly wage of at least 970 euros in 2024 levels.[12] The probability of low employment (regular employment = 0) is predicted using an ensemble model, combining extreme gradient boosting and random forests.

The model was trained through repeated cross-validation on 2013 data.[13] The metric targeted was the area under the ROC curve. After iterative tuning of the hyperparameters, 44 baseline variables were selected as predictive out of 126 candidates.[14] The most important variables include wage earning days, wages, self-employment income and total

[12]The wage threshold is intended to exclude jobs with very few paid hours. Finland does not have a universal minimum wage, and the employment data does not have information on whether employment was full-time. The wage limit chosen corresponds to 1.4 times the flat-rate unemployment assistance, and is close to the wage threshold for UI eligibility.

[13]Different training years from 2011 to 2019 were tested separately, and the out-of-sample predictions are not sensitive to the choice of the training year.

[14]A variable was considered predictive if it ranked higher than white noise on the default variable importance metrics by the `caret`, `XGBoost` and `ranger` packages in R.

income over the last year and last 3 years, age, year and level of latest education, age of the youngest child, current and total past duration in unemployment, indebtedness, and time from last job. The accuracy of the model's prediction across years 2012–2020 is 0.90 (sensitivity 0.84, specificity 0.93).

As a baseline, the at-risk group is defined as having a predicted probability of regular employment of less than 0.2. This group constitutes about 25% of working-age individuals in the areas of interest. About 80% of all new subsidized jobs in the subsequent twelve months are entered by this group, which otherwise receives only about 2% of unsubsidized wages over the same period.

Separately, another model was trained through a similar procedure to predict subsidy recipiency over the following twelve months. As entering a subsidized job is a relatively rare event even when its funding is not restricted, the model assigns relatively low probabilities on average. Using a conventional threshold of 0.5, the specificity metric would be 0.99 but sensitivity of only 0.38. This subsidy prediction was used as one input to propensity score matching in Section 7.

The at-risk group changes significantly from year to year. Out of the risk group of July 2015, a quarter were not in the July 2014 set and a quarter were not in the July 2016 set. In most cases, this is due to increased predicted probability of employment; other reasons include being below the age of 18 or above the age of 63, or not being observed in the data. Appendix E presents further evidence on the compositional changes of the risk group.

5 Descriptive statistics

Table 1 collects characteristics of all participants in subsidized jobs between 2006 and 2022, and also presents a comparison of the same characteristics for the overall working-age population. The observables for subsidized jobs are broken down by sector, as the participants differ markedly by sector. Further characteristics are presented in appendix F. As expected, the table illustrates that on average the individuals entering subsidized jobs have low labour market attachment, especially in terms of their lifetime wages. Persons who enter subsidized jobs in the private sector tend to have stronger work histories than those who start subsidized employment in municipalities and the third sector.

At an annual level, only small fractions of the working-age population (1.4%) and wages (0.3%) are covered by the subsidy. However, over the long run the programme has been quite pervasive in terms of the participating population. Out of the working-age population in 2022, almost one in seven had worked in subsidized jobs at some point since 2006.

Table 2 compares the at-risk groups, defined in the previous section, across the two areas defined in the institutional section: the sets of treatment (subsidy funds depleted) and control (funds not exhausted) areas. While there are some minor differences in the regional populations, they appear stable across time. Appendix G presents additional statistics and detailed definitions.

Figures 1 and 2 illustrate the trends in employment and unemployment in the 2010's for the control and treatment regions. Non-employed persons are counted as having a wage of zero, and the resulting means are meant to measure both the probability and intensity of employment. Differences in nonsubsidized employment between the areas are small and stable for the risk group and its complement over the entire period. Subsidized employment shows some more volatility in the early 2010's, but again the differences are small between the regions for two years before the treatment event in the at-risk group. For the complement of the risk group, the trends actually cross just before the depletion of funds. This is assessed to be of minor significance, as these changes are small in absolute terms and most of the subsidized jobs go to the at-risk group.

There are some continued differences between the control and treatment regions some time after the end of the initial treatment period. The subsidy rates recovered slowly because the policy changes in 2015 were undone gradually. For example, the rules allowing for benefit appropriations to be flexibly used for subsidy grants were re-established in 2017.[15]

[15] On the other hand, the low regional availability of the subsidy was probably more anticipated in later years. Extending the follow-up over several years would also have increased the differences in labour market conditions across the pre- and post-treatment periods. For these reasons, the baseline treatment period was restricted conservatively to only one year.

Similarly, trends in registered unemployment[16] are very similar across the regions, especially in the vicinity of the depletion event. Appendix H demonstrates that trends for the risk group's share of the working age population, benefit recipiency and labour market training[17] are parallel over time, with the differences remaining stable. Appendix E describes the risk group's stability and persistence over time, showing how much the risk group changes from year to year. Appendix I covers how entries into subsidized jobs depend on time spent in unemployment, and how the depletion of funds interacted with the hazard rates.

Overall, the patterns of aggregate employment, unemployment and ALMP program participation seem surprisingly stable, given some of the events occurring in the region over the periods of interest. In 2014, Russia invaded and annexed the Crimean Peninsula, triggering sanctions and other economic responses. One would expect these events to affect employment rates in South-Eastern Finland in particular due to the region's reliance on trade and tourism flows with Russia. Turning to the western treatment areas with depleted funds, in both 2014 and 2015, Microsoft executed large-scale layoffs in these areas; the scale of these reductions was among the largest experienced in Finnish history. Despite these shocks, there is little evidence of *regionally* differential effects. This finding is consistent with the results by Alasalmi, Toikka, and Ritala (2024), who examine the effects of the full-scale invasion of Ukraine in 2022 by Russia on regional economies in Finland and find only minor regional differences in the economic responses.

[16] Trends in unemployment benefit recipiency rates are very similar.

[17] Labour market training is funded from the region's overall ALMP budget. Increases in LM training could simultaneously drain the funds available for the subsidy and increase employment rates. However, the trends of participation rates in training appear stable across the areas.

Table 1: Characteristics of subsidized individuals by sector, 2006–2022.

	Private sector	Municipality	Third sector	Working-age pop.
Subsidy spells, total	208 971	203 867	156 396	
Individuals, total	170 558	146 818	105 258	
Employers, total	79 095	1 129	10 470	
Subsidy duration, years	148 953	128 261	116 924	
Subsidy amount, total	1.56 G€	1.49 G€	1.61 G€	
Mean subsidy duration, y	0.71 (0.70)	0.63 (0.53)	0.75 (0.60)	
Mean subsidy	7.46 k€ (9.10 k€)	7.32 k€ (7.21 k€)	10.30 k€ (9.81 k€)	
Personal mandates	0.1%	10.3%	0.1%	
Part-time jobs	15.4%	16.8%	53.5%	9.9%*
Most common profession	Shop sales ass. (9.6%)	Child care w. (14.1%)	Shop sales ass. (8.9%)	Shop sales ass. (5.2%)
Subsidy percentage	32.0% (17.5%)	37.4% (14.6%)	57.8% (25.8%)	
Estimated wage, monthly	1.97 k€ (849 €)	1.76 k€ (436 €)	1.40 k€ (531 €)	3.15 k€ (2.04 k€)
Age	35.88 (12.78)	42.79 (13.52)	43.77 (11.36)	41.14 (13.29)
Past employment duration, y	5.69 (5.55)	5.69 (5.27)	5.19 (4.67)	10.53 (6.94)
Past wages, total	162 k€ (233 k€)	156 k€ (213 k€)	126 k€ (163 k€)	377 k€ (420 k€)
Recent employment duration, y	0.83 (0.88)	0.53 (0.70)	0.42 (0.64)	2.04 (1.21)
Recent wages, total	19.2 k€ (33.7 k€)	10.5 k€ (22.3 k€)	6.3 k€ (14.5 k€)	73.7 k€ (79.5 k€)
Registered unemployed	91.6%	91.7%	89.2%	12.0%
Collects UI	22.4%	17.5%	10.0%	4.9%
Current unemployment dur., y	0.87 (1.16)	1.35 (1.47)	1.45 (1.57)	1.19 (1.76)
Past unemployment duration, y	3.86 (3.47)	5.87 (3.72)	7.04 (3.57)	1.34 (2.44)
Years of education	12.56 (1.61)	12.53 (1.57)	12.53 (1.57)	13.41 (2.20)
Degree: vocational upper sec.	50.6%	47.5%	47.1%	33.7%
Most common educ. field	Engineering (18.5%)	Personal s. (11.0%)	Personal s. (10.7%)	Engineering (13.6%)
Foreign background	8.1%	8.4%	8.9%	6.3%
Female	38.6%	56.7%	56.0%	49.4%
Number of children	0.52 (0.95)	0.47 (0.95)	0.48 (0.94)	0.62 (1.02)
No previous job observed	7.0%	7.0%	5.4%	6.2%
Collects part-time benefits soon	5.6%	3.2%	33.4%	1.8%

The working-age population is a weighted sample, where one random (person, date t) pair was picked for each new subsidy started at date t. "Recently" refers to the previous 3 years. All data from register data, except *part-time work for working-age pop. from the labour force survey. The number in parentheses is the standard deviation, except for the categorical variables, where it is the share. Subsidy periods appearing in data less than 30 days apart for the same individual are interpreted as parts of the same subsidy spell. For other definitions, see appendix F.

Table 2: Characteristics of the at-risk group by region.

	Depleted, 2014	Depleted, 2015	Not depleted, 2014	Not depleted, 2015
Age	38.16 (15.83)	38.09 (15.72)	40.14 (16.09)	39.93 (15.97)
Income in previous year	11 200€ (10 000€)	11 300€ (9 900€)	11 700€ (10 200€)	11 700€ (9 890€)
Past wages, per year	7 000€ (10 500€)	6 870€ (10 300€)	7 660€ (11 300€)	7 490€ (11 100€)
Past employment, year/year	0.19 (0.26)	0.19 (0.25)	0.21 (0.27)	0.20 (0.27)
Employment over the last year	0.03 (0.10)	0.03 (0.10)	0.03 (0.11)	0.03 (0.10)
Recent employment, year/year	0.08 (0.17)	0.07 (0.17)	0.08 (0.18)	0.07 (0.17)
Recent wages, per year	2 810€ (8 940€)	2 630€ (7 240€)	2 800€ (10 200€)	2 720€ (8 640€)
Employment prediction	0.06 (0.05)	0.06 (0.05)	0.05 (0.05)	0.05 (0.05)
Years of education	11.31 (2.16)	11.34 (2.18)	11.23 (2.10)	11.26 (2.12)
Past subsidized jobs, years	0.37 (0.90)	0.38 (0.91)	0.44 (0.99)	0.44 (1.01)
Participated in subsidized jobs	27.4%	27.5%	30.1%	30.4%
Has children	23.4%	23.4%	21.0%	21.2%
Age of youngest child if any	14.32 (11.89)	14.25 (11.90)	15.25 (12.21)	15.10 (12.15)
Past unemployment, year/year	0.19 (0.22)	0.20 (0.22)	0.21 (0.23)	0.22 (0.23)
Recent LM training, days	15.79 (68.66)	16.47 (71.41)	15.80 (66.01)	16.25 (67.60)
Recent part-time benefit days	0.01 (0.25)	0.02 (0.36)	0.01 (0.20)	0.02 (0.28)
Recent unemployment ben., year/year	0.26 (0.34)	0.28 (0.35)	0.30 (0.36)	0.31 (0.37)
Recent reg. unemployed, year/year	52.7%	53.8%	55.3%	56.8%
Years from last subsidy if any	9.10 (6.86)	9.12 (7.19)	8.81 (6.84)	8.92 (7.16)
Female	49.0%	48.9%	48.1%	48.1%
Foreign background	8.3%	9.1%	9.1%	9.9%
In the risk group on previous year	75.6%	77.1%	76.7%	78.0%
N	175 100	174 826	141 381	141 384

Past wages, employment and unemployment are divided by the years since 1987 (1991 for unemployment) where the individual was 18 or older. Employment, wages and unemployoment over the previous 3 years are annualized; for example, if a person was employed for one year over three years, the value is 0.33. Standard deviations in parentheses. For further definitions, see appendix G

Figure 1: Subsidized and unsubsidized wages in control and treatment regions. The shaded vertical area is the treatment period, during which the funds available for the subsidy were depleted in the treatment regions. The at-risk group is redefined for July in each year. Non-employed persons are counted as having a wage of zero. The vertical scale is allowed to vary between panels to better highlight the differences, as the groups examined have very different employment and wage rates.

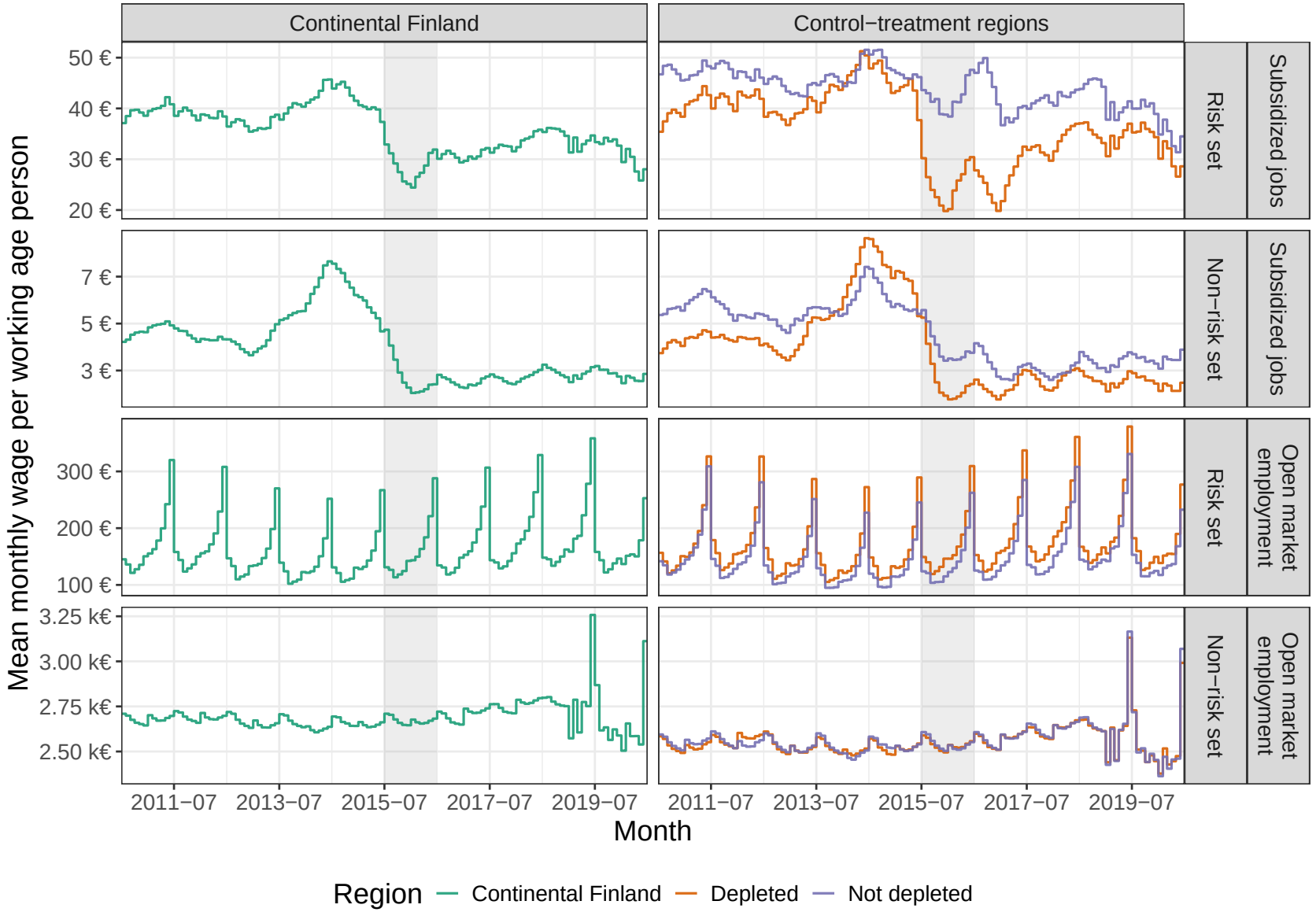


Figure 2: Registered unemployment in treatment and control regions.

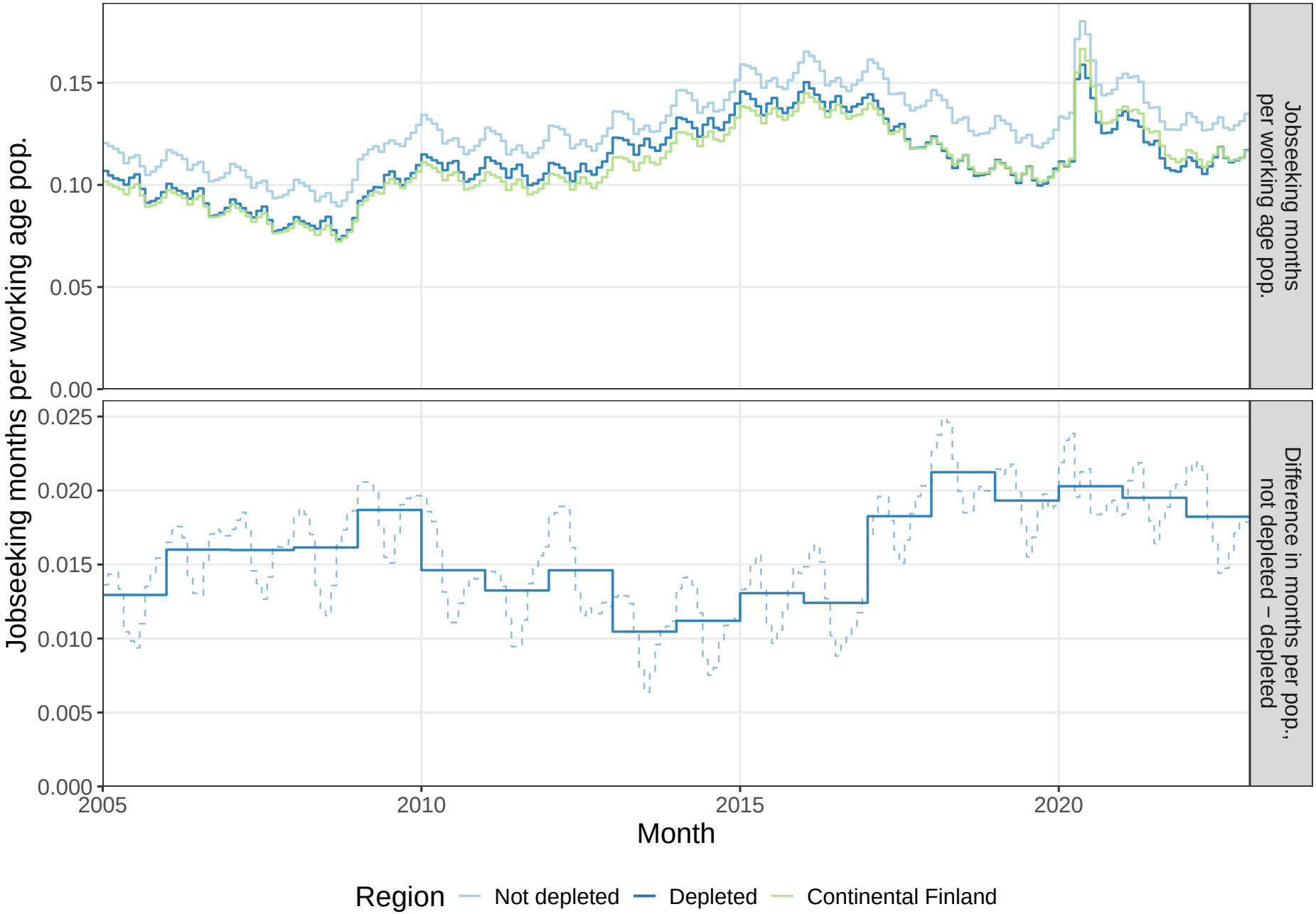
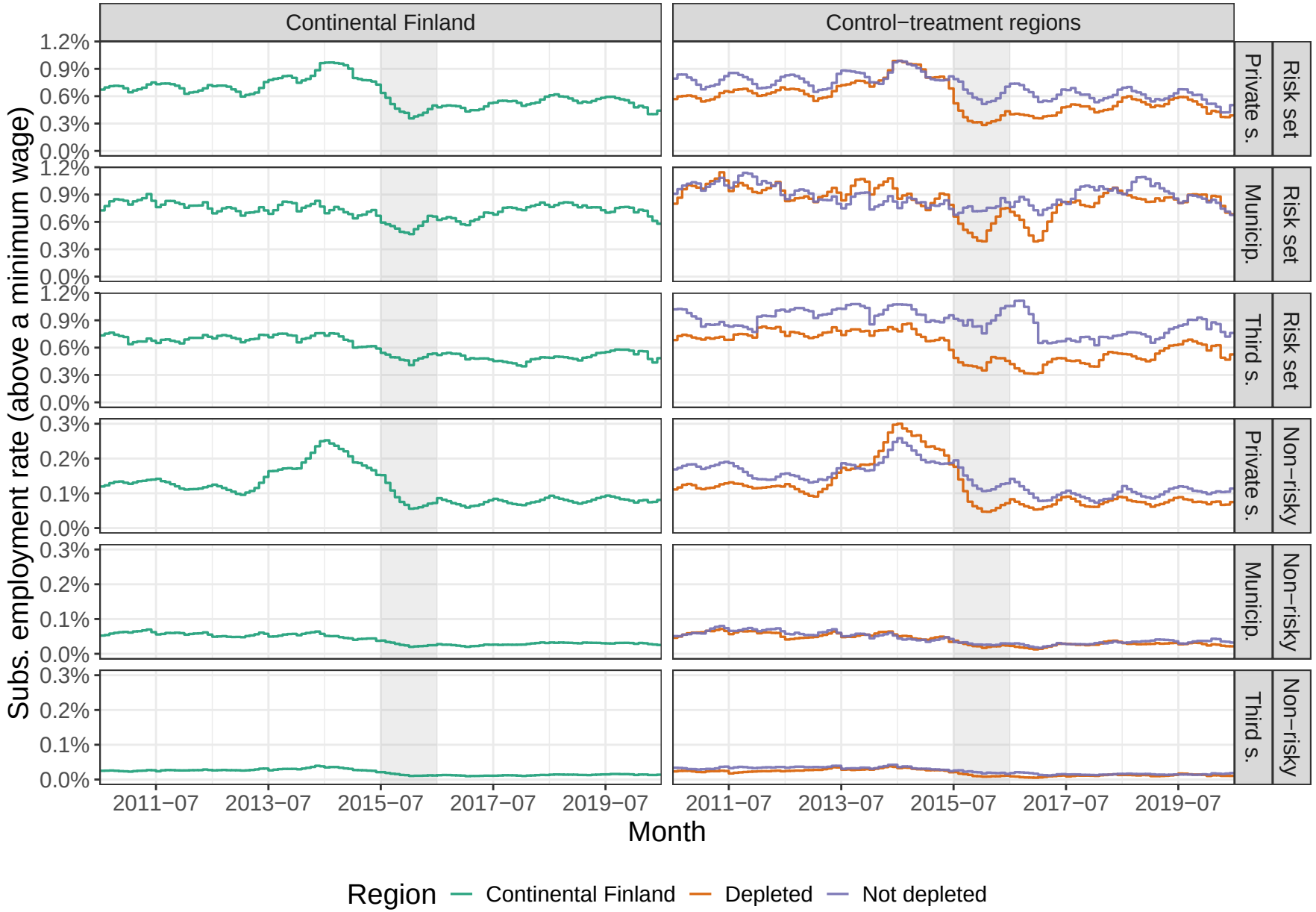


Figure 3: Subsidized jobs in different sectors in control and treatment regions. The shaded vertical area is the treatment period, during which the funds available for the subsidy were depleted in the treatment regions.



6 Results on the displacement effects

For examining the displacement effects, the sample is restricted to the at-risk group defined from data for July 2014 and July 2015 in the two areas defined in Section 2. The at-risk group consists of individuals with low short-term employment prospects, as defined in Section 4. Simple differences-in-differences specifications are fitted to this sample:

$$\begin{aligned} D_{i,t+1} = \omega + \alpha \cdot TreatmentTime_{i,t} + \gamma \cdot TreatmentArea_{i,t} + \\ \delta \cdot (TreatmentTime_{i,t} \times TreatmentArea_{i,t}) + x'_{i,t}\beta + \varepsilon_{i,t} \end{aligned} \quad (1)$$

where i indexes individuals and t the periods where the person is observed in the at-risk group (July 1st of 2014 or 2015). $TreatmentTime$ takes value of 1 for $t =$ July 2015, and $TreatmentArea$ takes value 1 for the regions where funds were depleted in July 2015. x_i is a vector of controls fixed at t. $D_{i,t+1}$ is the outcome over the observation period, the 12 months following t: subsidized wages or employment and overall wages or employment for the at-risk group.

Under the parallel trends assumption, the coefficient δ captures the gross effects of the treatment (the depletion of subsidy funds) on subsidized wages and overall wages. The estimates for this coefficient appears in table 3.

The difference of the two estimates is interpreted as the short-term regional displacement effect and deadweight loss within the risk group. The results for wages indicate the likely scope of displacement effects is relatively small at around 12%. In this setting, deadweight loss (subsidized employment replacing employment for non-participants) and displacement effects are identified jointly, as the sample includes both subsidized and unsubsidized individuals.

If one were to use days of employment as the measure, the scope of displacement would be much more substantial, at almost a third. However, the available data does not allow one to distinguish between part-time work and full-time work, and the measure of days worked may be a biased measure of full-time equivalent employment.[18] The estimated wages and other sources suggest that part-time work is common among the population examined. Thus, the effects on wages is assessed to be a more suitable measure of aggregate displacement effects.

The at-risk group also includes persons in ongoing subsidized jobs in July. The reason for their inclusion is that the funding depletion reduced the number and duration of both fresh subsidies and extensions of old ones. Tracking ongoing wages captures the joint effects of both of these channels in a simple and transparent manner.

Results when the at-risk group is constrained or expanded from the baseline, plus

[18]The data for subsidies does have a binary indicator for part-time work, but data from later years indicates the actual hours worked vary substantially even between part-time workers. The employment data for unsubsidized work does not have part-time work indicators over this period.

placebo tests for different periods, appear in 8.

Table 3: Estimation results for the displacement effects.

	Subsidized wages	Total wages	Subsidized employment	Total employment
With controls	-213.0 (36.0)	-187.0 (55.6)	-2.88 (0.21)	-1.86 (0.32)
Without controls	-215.0 (36.2)	-181.0 (56.7)	-2.89 (0.22)	-1.75 (0.35)
Baseline	668	2550	9.86	29.7
Observations	632 691	632 691	632 691	632 691

Estimates are for the coefficient δ in equation (1). Bootstrapped standard errors in parentheses. Wages are in 2019 euros over 12 months and include zeroes. Employment is in days worked above a wage threshold of 929 year 2019 euros per month. The baseline is the pre-treatment mean in the treatment regions. The controls include: employment prediction, age, total past employment, wages and employment in the last 1 and 3 years, overall income in the last year, number of underage children, gender, an indicator for foreign background, education in years, past subsidy duration, current duration of unemployment benefit spell, time from last job plus a dummy if no last job, past registered unemployment and unemployment benefit days, and an estimate of the inverse local labour market tightness, defined as open vacancies divided by jobseekers over the preceding 9 months, calculated over a flexibly coarsened grid of commuting areas and four-digit profession codes.

7 Results on the effects on participants

The stated goal of the hiring subsidy is to aid transitions to regular, unsubsidized employment. To examine the longer-term success of this goal on participants, it is necessary to follow individuals beyond the subsidy period.

Appendix J examines a very simple instrumental variables approach that utilizes the natural experiment (the depletion of funds) to identify the effects of the subsidy on participants. The point estimates are close to the average effects from matching, but the setup suffers from severe lack of power and the results are extremely imprecise. The appendix also discussed several issues that arise when the outcome of interest has a much longer time horizon than in the previous section.

Due to the above limitations, the results on participants are estimated using matching by observables. This choice also improves the comparability of the estimates to prior research by Aho et al. (2018) and Asplund et al. (2018). The downside is that the results may be biased if individuals select into treatment by unobserved characteristics correlated with the outcomes.

To maintain some comparability with the DiD estimates in the previous section, the sample to be matched is again chosen as the at-risk group from July of each year and from the same areas as before. This choice provides some new contributions by itself. First, it allows the matching approach to also capture individuals who are otherwise very similar but happen to be in non-employment rather than unemployment shortly before entering the subsidy. This way, any persons who had only been unemployed for a short time when they started in a subsidized job can be included as treated units.

Second, by capturing a pre-treatment period for both the treated and matched control units, one may observe some potential lock-in effects before the subsidized job starts. While employment and wages of treated and control units are similar before a common *base date*, July 1st, they start diverging between this base date and the start of the subsidized job. A potential explanation is that after a provisional agreement upon a subsidized job with an employer, the jobseeker must wait for the application to be sent and processed. During the waiting period, they might decrease their search effort and thus be less likely to find open market employment compared to the matched units.

In this part of the paper, the treatment now refers to entering a subsidized job within a year from the base date, rather than the depletion of regional funds. The control group consists of matched units who do not enter a subsidized job over this period. Individuals who were already in ongoing subsidized jobs in July or in the year before are now excluded. Additionally, older individuals collecting unemployment insurance who are predicted to be entitled to a individually guaranteed subsidized job are excluded, since these guaranteed jobs are very different from the other subsidized jobs, as discussed in section 2. The breakdown of all excluded subsidy entries is provided in appendix K.

The matching is performed through 1-to-1 matching by propensity score, using a set of 25 observables. Figure 4 illustrates some of the covariate balance before restricting comparisons to the at-risk group, and before and after matching by observables. Some variables not used in matching are also included to illustrate how matching by the chosen covariates is reflected on the overall balance. Overall, the treatment and control groups appear well balanced after the matching.

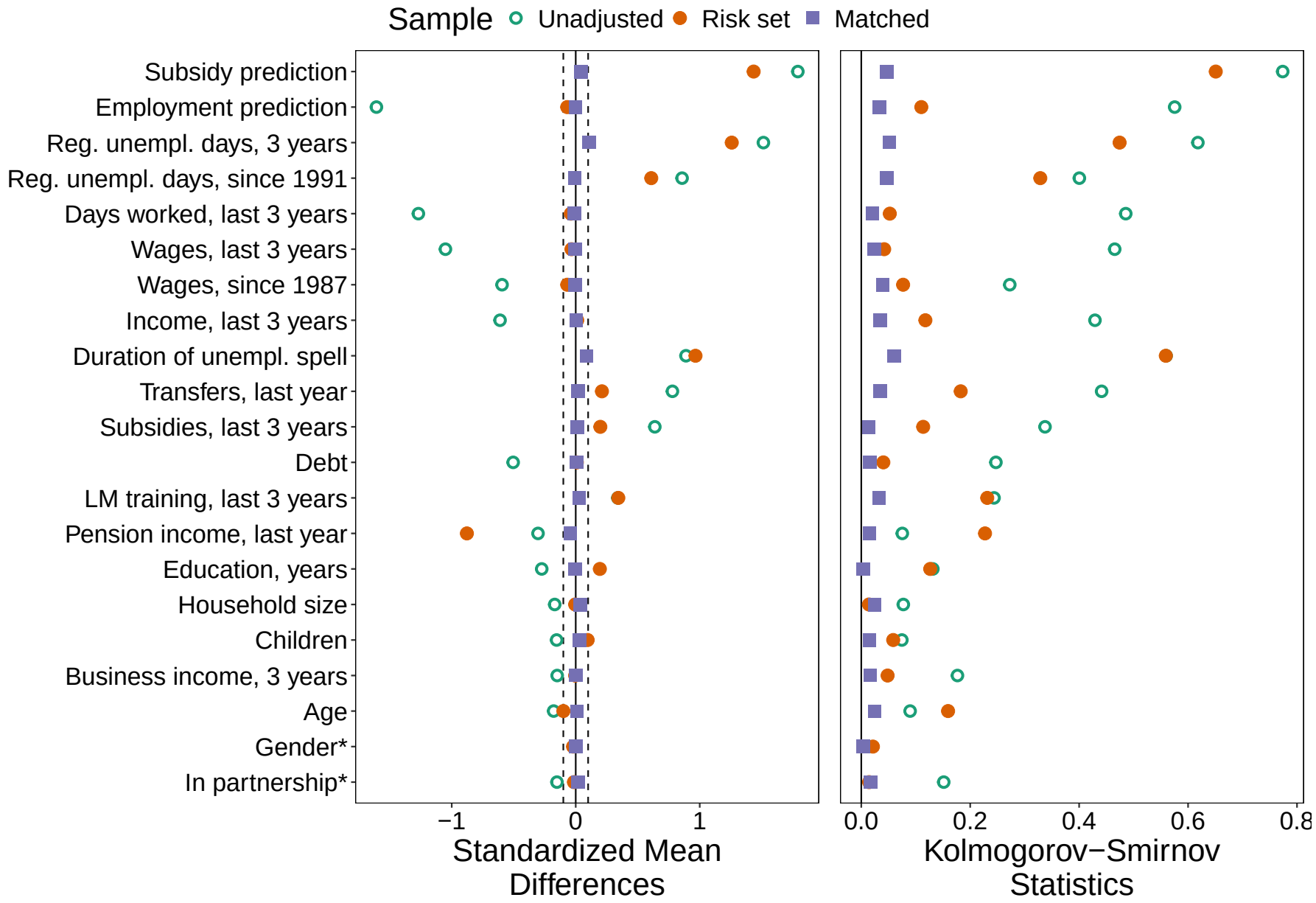
All the regressions in this section include the matching covariates plus some additional ones for precision.[19] The full list of both types of covariates used and additional measures of balance appear in appendix M.

Both coarsened exact matching and entropy balancing weights were tested as alternatives. These methods utilized the entire at-risk group appropriately weighted, but provided very similar results to 1-to-1 PS matching. Propensity score matching was used for two reasons: it improves the comparability of the results to earlier papers on the Finnish subsidy, and it makes detailed, long-term month-level follow-ups over various outcomes much easier to illustrate.

The matching is performed separately for private sector, public sector and third sector jobs, as the selection into these jobs differs significantly. Additionally, it is done separately for each of the years 2012-2017 and for the areas where subsidy funds were depleted/not depleted in 2015. In the regression analysis, most outcomes are measured for the fourth year (months 36–47) after the start of follow-up. As complementary evidence, monthly follow-ups of various outcomes are presented graphically. The main outcomes considered are (a) open market wages and entrepreneurial income and employment, (b) wages and employment including later subsidized jobs, and (c) the tax-transfer balance.

[19] The point estimates are not sensitive to their inclusion, so the results without covariates are omitted for brevity.

Figure 4: The pre- and post-matching balance in observables. The unadjusted difference is the difference between the working-age population and those entering subsidized jobs. The at-risk group is defined similarly to section 4.



7.1 Changes in the subsidy's effects when subsidy rate is reduced

Multiple channels might change the estimated efficiency of the subsidies when their availability is reduced. First, one would expect the officials will try to prioritize cases who benefit the most from the subsidy when availability is reduced. As a second example, there might be some applicants who have both a high likelihood of finding jobs elsewhere (i.e., deadweight loss) and high long-term employment rates and wages. When the subsidy is not easily available, such individuals are presumably the most likely to opt for their second-best alternative.

Table 4 lists the effects on participants, as estimated by OLS on samples matched by propensity score. There is some year-to-year, sector-by-sector and area-to-area variation in the estimated effects. The observed variation in the effects in 2015 does not appear extraordinary against this backdrop. Figure 5 complements with a graphical analysis of the monthly effects of the subsidy on employment, defined as simply the average treatment–control unit difference in overall wages.

While the changes of sector-specific effects do not appear to be related to the availability of the subsidy, the overall depletion of funds clearly altered the per-sector shares of subsidies. The private sector share of new subsidy spells fell by 11 percentage points in 2015, but recovered over the next year. Additionally, new subsidy spells were significantly shorter in 2015. The fact that the long-term effects appear roughly similar despite this reduction in duration suggests that shorter duration subsidies are as effective (and thus fiscally more efficient) as longer ones. This is consistent with earlier results by Sjögren and Vikström (2015) who concluded that the subsidizing rate is a more important determinant of its effects than its duration.

The treated individuals experience somewhat lower wages than the control units for some months *just prior* to the subsidy. This may be due to at least two different reasons: some systematic errors in the matching process, or some true negative lock-in effects. Negative lock-in refers to cases where the employer and the jobseeker have agreed upon a contract provisional to the subsidy, but must wait for the subsidy application to be processed before work can commence. The initial agreement might plausibly cause the jobseeker to reduce their search effort for other jobs.

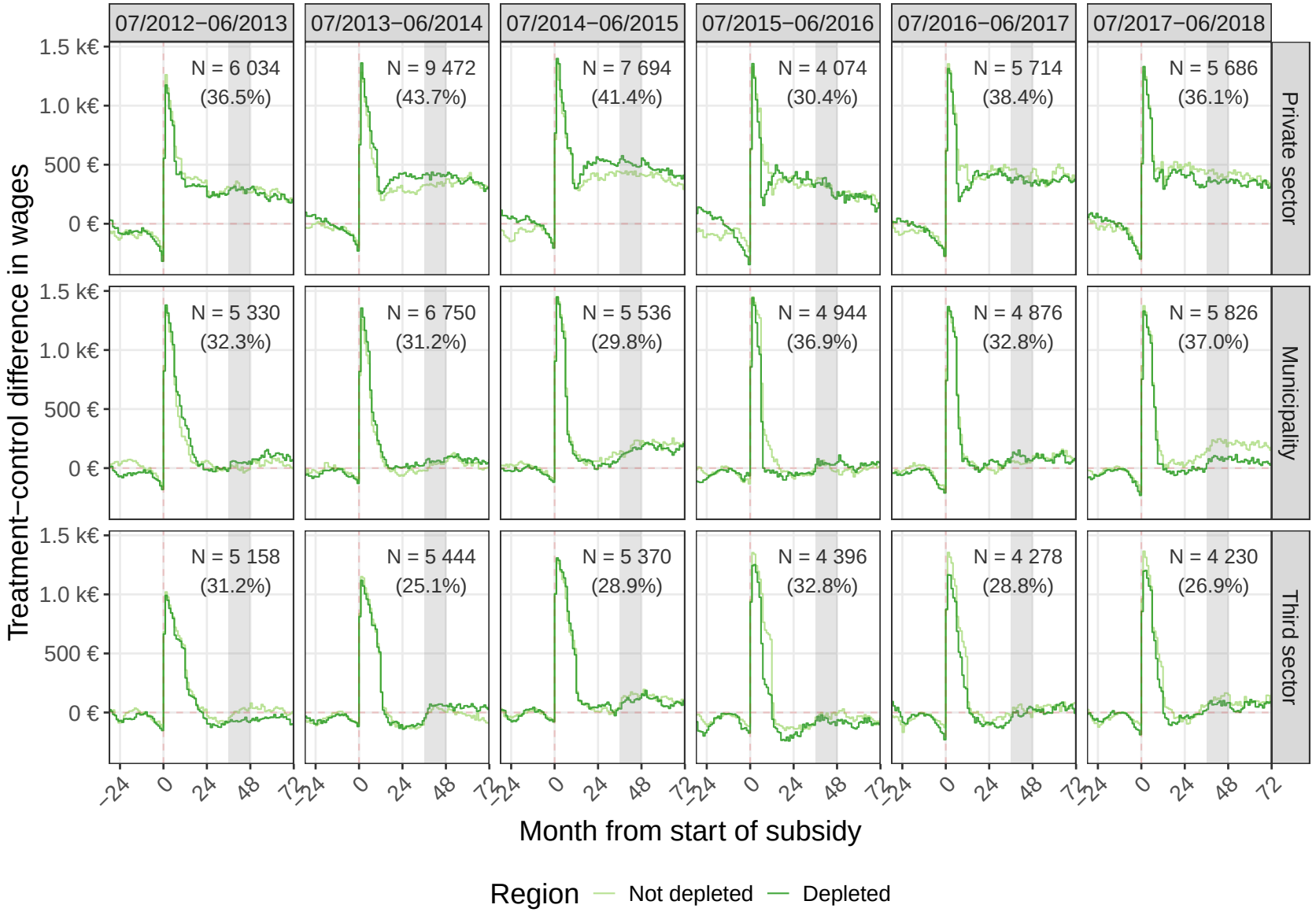
The overall findings of the subsidy's effects by sector are quite similar to results obtained by Aho et al. (2018) and Asplund et al. (2018). The differences between the approaches are examined in appendix N.

Table 4: Estimated effects of the subsidy on annual open market wages in the fourth year from the start of the base period.

Region	Period	Private sector	Municipality	Third sector
Not depleted	July 2012	3 704.2 (528.4)	-168.0 (414.3)	-199.6 (367.7)
Not depleted	July 2013	3 705.4 (420.2)	8.3 (359.7)	-93.6 (328.5)
Not depleted	July 2014	4 976.2 (513.5)	997.2 (450.9)	369.1 (382.0)
Not depleted	July 2015	4 492.8 (595.9)	-1 251.9 (1 195.1)	-1 430.2 (991.1)
Not depleted	July 2016	4 988.7 (534.3)	165.5 (468.7)	110.6 (424.4)
Not depleted	July 2017	4 816.7 (518.0)	1 341.2 (439.1)	257.9 (401.2)
Depleted	July 2012	4 241.0 (478.7)	271.0 (363.9)	-1 008.4 (834.3)
Depleted	July 2013	3 742.2 (786.1)	434.7 (376.8)	-15.8 (416.2)
Depleted	July 2014	6 706.9 (635.6)	652.9 (389.2)	923.6 (496.2)
Depleted	July 2015	5 284.2 (763.2)	660.0 (469.9)	-76.0 (497.6)
Depleted	July 2016	3 584.5 (480.1)	1 034.7 (426.6)	-381.4 (439.2)
Depleted	July 2017	3 107.6 (470.7)	535.8 (368.1)	644.4 (483.4)
Ref. wage	Averaged	10 746.8 (1211.6)	7055.5 (1027.3)	5798.1 (1087.9)

Standard errors clustered at the match level in parentheses. For control covariates used, see appendix M. The reference wage refers to average annual wages earned by control units in the fourth follow-up year.

Figure 5: The treatment–control difference in monthly mean wagesums, by subsidized sector, base year and region. Non-employed persons are counted with a wage of zero. The treatment-control pairs are identified from July of each year, but the zero offset is defined as the entry into subsidy for each pair separately. The shaded area is the fourth year from the base date, over which the effects are calculated in table 4. Percentages are for the share of participants in a given sector per year, summing to 100% by column.



7.2 Employment at the initial employer

The employment data identifies the person's employer by date. For the purposes of this paper, employment is categorized into three classes. Any subsidized jobs are simply denoted the *subsidized job*. For the treatment group, employment with the *subsidized employer* refers to unsubsidized employment, at any point in time, with the same employer as during the initial treatment. *Employment elsewhere* covers all other employment. *Wage contributions* from these different sources are calculated by dividing wages within an employment type by the size of the group, whether working or not.[20]

Figure 6 shows the monthly difference in treatment-control wage contributions by source for subsidies started in 2014. Because the control units do not have an initial "subsidized employer" by construction, the differences by employment class cannot be given a causal interpretation.

The observed patterns are, however, consistent with the notion that the subsidy's positive effects in the private sector are mainly due to persistent employment with the subsidized employer. Similarly, the patterns are consistent with little or no stepping-stone effects towards other jobs. These patterns might be due to, for example, human capital accumulation being largely employer-specific, or employers using the subsidy to decrease the costs of screening employees.

Additionally, it can be observed that some control units for the private sector subsidies do find employment elsewhere relatively quickly after the base date. There are at least two potential interpretations for this finding. First, it is possible these units do not reflect potential outcomes for the treated, i.e., the matching estimates suffer from some bias. Second, there may be some true deadweight loss, i.e., subsidies granted to participants who would have been able to find employment otherwise.

In many cases, the employment relationship with the initial employer is not a continuous one. On average, almost a half of those who have some unsubsidized employment with the initial employer are observed leaving and later again re-entering their employ. Additionally, the relationship is substantially weaker outside private sector employment. Together, these observations suggest that the persistence effect is not simply a byproduct of employment protection preventing termination of contracts. Appendix O presents additional figures for other years and long-term patterns of re-entry to the initial employer.

Since some control units do find employment elsewhere during the initial period, it is also possible to separately examine the contrast between these units and those entering subsidized jobs. This comparison is purely descriptive and must be interpreted with considerable caution: as noted above, these cases might simply reflect deficiencies in the matching process. For this analysis, the categories are slightly relabeled so that *first*

[20]For example, if 1/2 of the treatment group earned 2000 euros per month from the subsidized employer, and 1/6 earned 2500 from other employers, employment with the subsidized employer would contribute $\frac{2000}{2} = 1000$ and other employment $\frac{2500}{6} = 417$ euros.

employer refers jointly to the subsidized jobs and subsidized employers for the treated, and the first employer observed within a year from the base date for the matched control units. Only the treatment units that were matched 1-to-1 to control units that entered employment within the first year are included. This increases the observed similarity between unit pairs.[21]

Figure 7 illustrates the evolution of employment with the initial employer and other employers over time for subsidies starting in 2012. It appears, firstly, that while the wage contributions from the initial employer diminish over time, they can last for over ten years after the initial treatment. Second, the observed patterns would be consistent with the hypothesis that the subsidized employment might have very similar lock-in employment effects as employment anywhere for the individuals in question. Third, the control units find substantially more employment in other sectors over the long run. This might be because open market employment has much better stepping stone effects on the individuals, or because these particular units did not end up being well matched.

[21]However, it should be emphasized that the matching itself was based on propensity scores related to subsidized employment. The investigation of this subset is thus markedly different from the splitting of the treatments by sector, since the propensity scores were estimated separately for each treatment stratum, yilding distinct propensities for private, public and third sector subsidies.

Figure 6: The treatment–control difference in monthly wage contributions, by subsidized sector, employment type and region, for subsidized jobs started between July 2014 and June 2015. Non-employed persons are counted as having a wage of zero. The shaded area is the fourth year since entering the subsidy. The date of entering the subsidy determines offset 0 for both the treated units and their individual matched control unit.

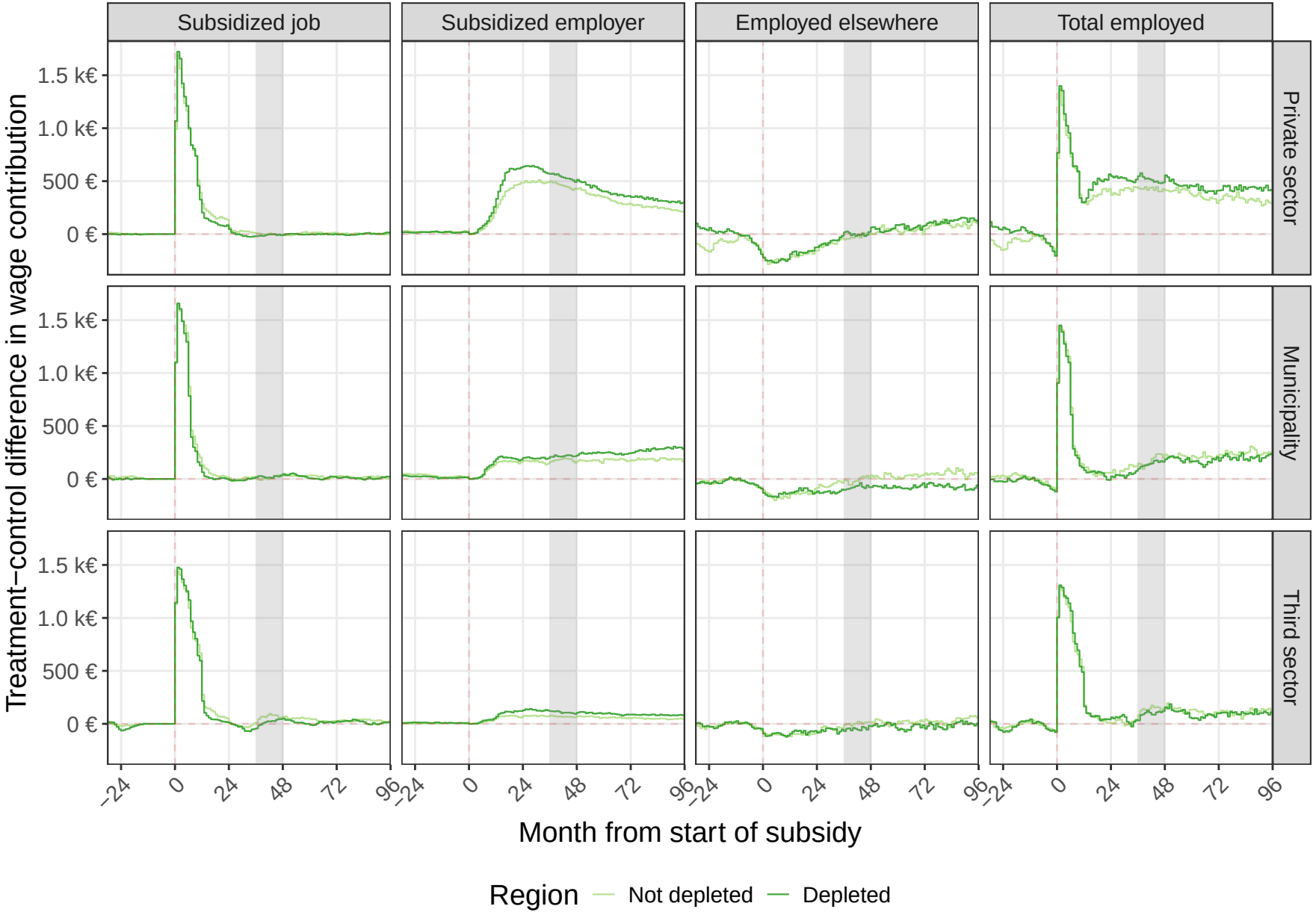
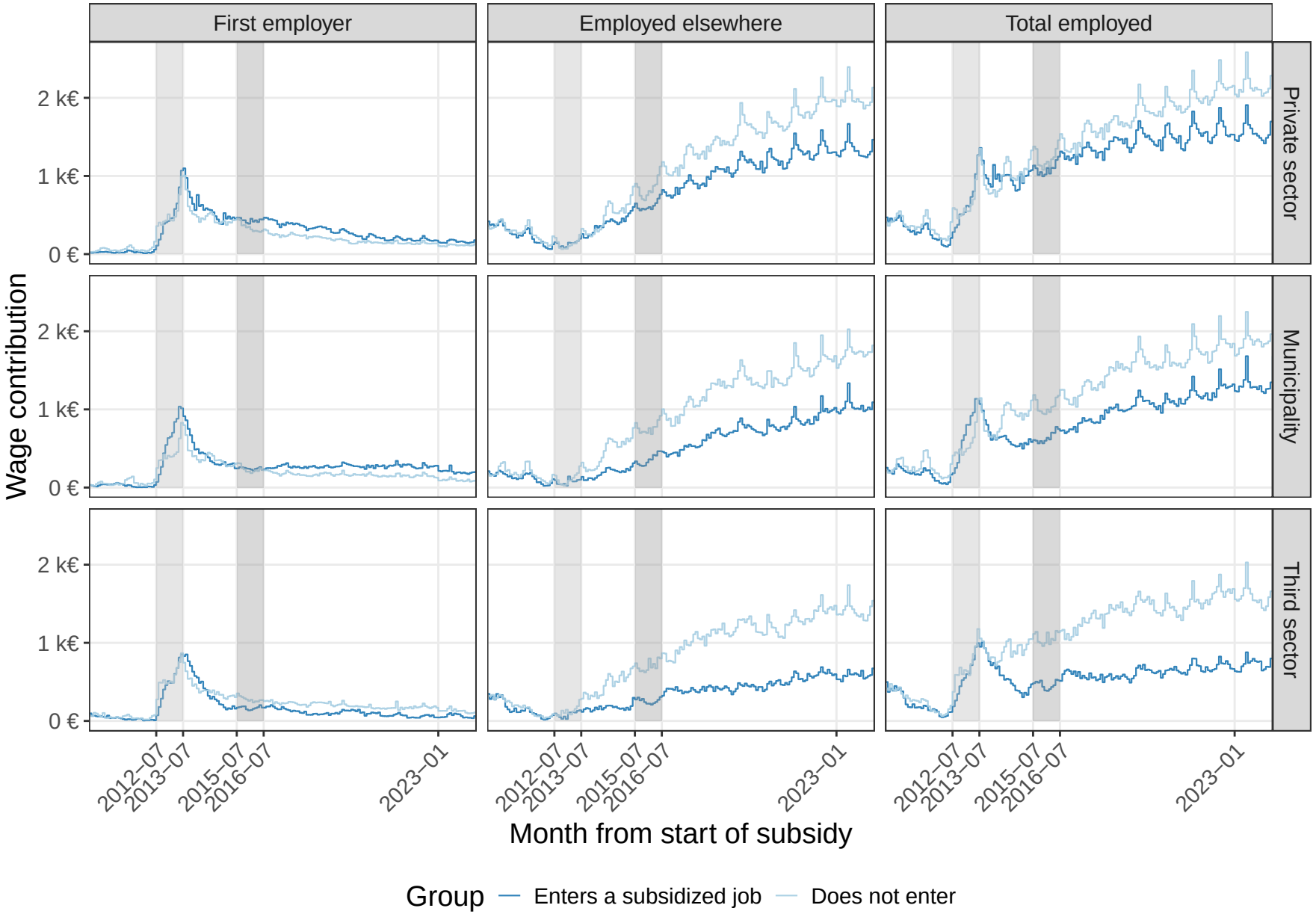


Figure 7: The treatment–control difference in monthly wage contributions, by subsidized sector, employment type and region, for a subset of subsidized jobs started between July 2012 and June 2013. Includes treatment-matched control pairs where the control unit found a job between July 2012 and June 2013. The shaded areas are the first year and the fourth year from the base date.



7.3 Effects on the tax-transfer balance

For each individual, the tax-transfer balance was calculated for each year except for 2020.[22] As a baseline, direct taxes and employee-side social security contributions paid represent revenues (positive values) for the public finances. Benefit transfers and the estimated cost of the subsidy count as costs (negative values).

Table 5 lists the effects on the tax-transfer balance by sector and base period in total euros (not annualized). These results suggest that in the private sector the subsidy has a varying but consistently positive balance already in the short term. In the third sector the subsidy appears to have a consistently negative balance, while municipalities lie between these two extremes.

The negative balance in the third sector is due to both higher unit costs of the subsidy and the absence of later employment gains. There are several interrelated potential explanations for this disparity. To avoid distortionary effects on composition, the law constraints the highest available subsidy rates and durations to the most vulnerable groups when they are hired outside the competitive market. Correspondingly, these vulnerable groups are overrepresented among the third sector participants compared to the other sectors, driving up the unit costs of the subsidies.

Regarding the absence of positive effects, there are at least two potential causes. First, it is possible that the individuals with very low employment prospects do not gain later employment benefits from *any* subsidized employment, which suggests other tools would probably be more suitable for assisting them. Alternatively, it could be that subsidized employment in the third sector is not helpful, regardless of participant selection, in which case the current participants might still benefit from the subsidy in other sectors. It is also possible that both statements are true.

A limitation of the directly observed tax-transfer balance is that the tax types included only account for roughly one third of all public revenues. Major types of indirect taxes include employer-side social contributions (payroll taxes) and taxes on consumption. As a complementary analysis, coarse estimates for these taxes were calculated using wage and income data. Payroll taxes were estimated by multiplying observed wages with an average multiplier, calculated annually from GDP statistics. Taxes on consumption were approximated using earlier Finnish research on the relationship between income ranks and indirect taxes; details appear in appendix P. The measurement error is likely to be small for payroll taxes, but may be considerable for taxes on consumption.

Figure 8 shows the treatment–control difference of the expanded fiscal balance at a monthly level for subsidies started between July 2012 and June 2013. Cumulative balance is plotted from the base date of July 2012. Appendix P plots the contributions of different taxes and transfers at a more detailed level.

[22] Data on transfers for year 2020 only included unemployment benefits, so this year has been omitted.

Incorporating the additional sources of tax revenues further increases the estimates of the long-run fiscal effects in the private sector. These broader estimates are slightly positive for subsidized employment in municipalities, and appear even worse for the third sector.

Three major limitations regarding the calculation remain. First, data on the consumption of public services is not available. Earlier research suggests that ALMPs may also have effects on secondary outcomes when they do not affect unemployment. For example, Davis and Heller (2020) concludes that supported jobs can reduce some types of crime even when not increasing employment. Wang et al. (2021) and Baekgaard et al. (2024) report mixed results regarding the effects of ALMPs on mental health, Wang et al. finding positive effects and Baekgaard et al. concluding the effects may also be negative. Since the participants in the third sector often belong to particularly vulnerable groups, it is possible that the welfare effects not captured in this paper might also be larger for them than for other groups.

Second, even a ten-year follow-up may be too short to consider long-term effects in a consistent way. In particular, pension contributions also accrue future pensions, which are primarily financed on a pay-as-you-go basis. The contribution side is fully included as a tax revenue, while pensions are only included insofar as people collect them over the follow-up period.

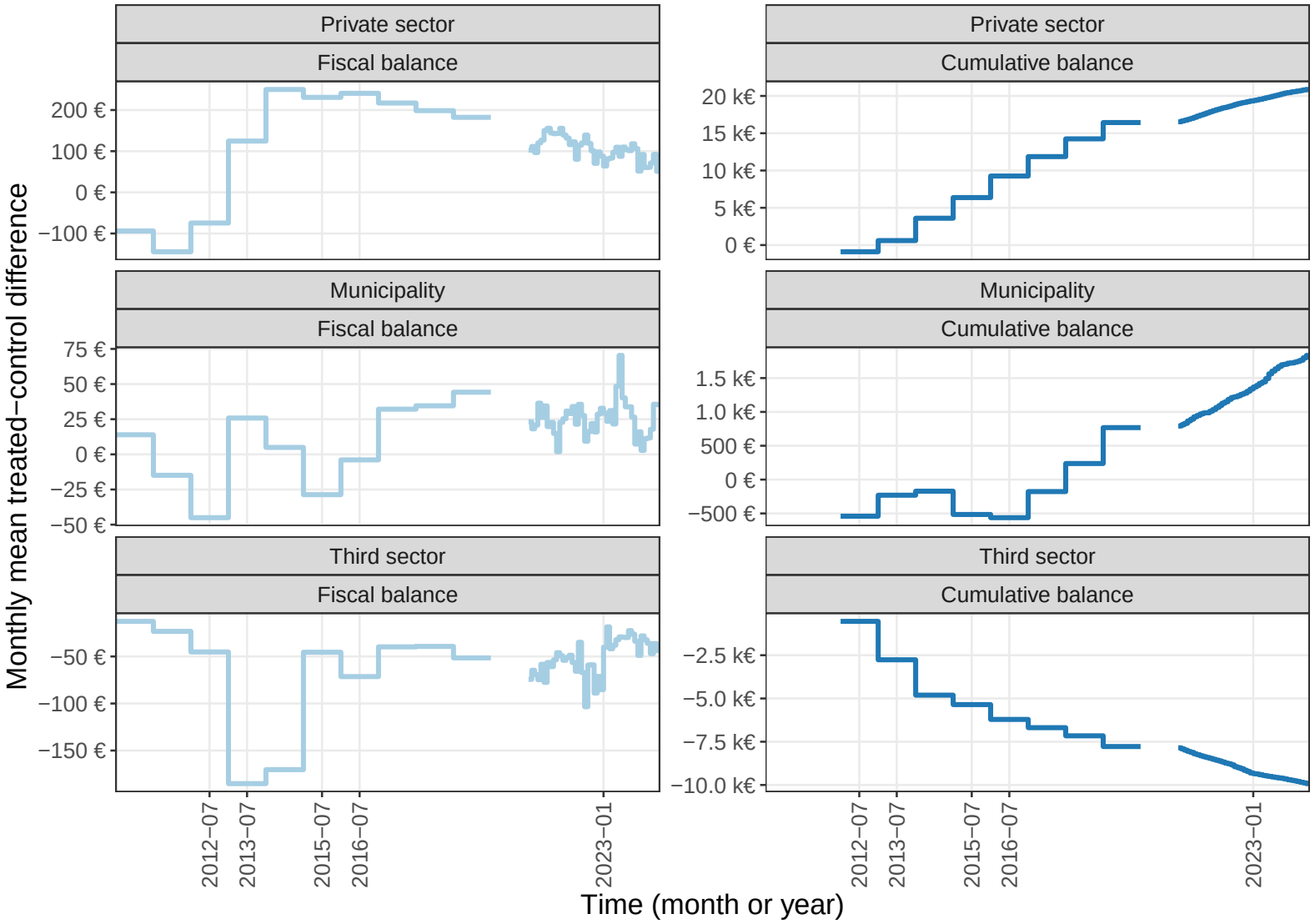
Finally, the costs for municipalities from subsidized jobs only include the subsidy from the central government. It is assumed that the unsubsidized part of the wage reflects the worker's productivity, and that the net gain of the subsidy to the municipality from work done is equal to the net cost. If this is not true, then the entire wage cost in municipalities should be understood as part of the public cost of the subsidy.

Table 5: Estimated effects on the balance of direct taxes, transfers and subsidies.

Base years	Sector	Three first years	Fourth year	N
2012-2013	Private sector	1 513.5 (243.6)	2 155.6 (121.0)	15,506
2012-2013	Municipality	-3 186.3 (220.7)	34.0 (114.6)	12,080
2012-2013	Third sector	-7 689.4 (270.1)	-137.6 (122.4)	10,602
2014-2015	Private sector	2 826.7 (271.2)	2 570.4 (137.7)	11,768
2014-2015	Municipality	-1 078.9 (237.1)	289.0 (128.9)	10,480
2014-2015	Third sector	-7 261.4 (313.4)	134.6 (144.0)	9,766

Includes income taxes, employee-side payroll taxes, received transfers and subsidy costs, but excludes effects on employer-side payroll taxes and indirect consumption taxes. Standard errors clustered at the match level in parentheses. For control covariates used, see appendix M.

Figure 8: The effect on public finances for subsidized employment started between July 2012 and June 2013, by sector. Includes direct taxes, taxes on consumption, payroll taxes, most received benefits and wage subsidy costs. Data until 2019 is at an annual level, translated to monthly amounts by dividing by 12; since 2021 it is at the payment level. Comprehensive data for year 2020 was unavailable and is omitted. Some smaller benefit types were not observed after 2020.



7.4 Employer-side patterns and competition

The distribution of the use of subsidy varies significantly by sector. Table 6 collects various summary measures of subsidy use by sector from 2006 to 2022. Generally, the typical intensity in the use of the subsidy is low in the private sector and in municipalities. While there are some firms that do receive a significant amount of the hiring subsidy relative to their overall wages, these are typically quite small and short-lived. The picture remains largely similar when examining different industries: while some industries do utilize the subsidies more than others, in most industries the ratio of subsidies to overall wages remains low, and even among more intensive users of the subsidy the intensity of use appears constrained. These findings are consistent with results by Toikka, Toivanen, and Valmari (2024), who also note that the Finnish hiring subsidies are dispersed across many employers.

The picture is very different within the third sector. The majority of the subsidies within this category are collected by organizations who finance a substantial fraction of their wage bill through the subsidy. This often means that a person is hired with a subsidy for a fixed-term, part-time job; once the individual's maximum duration on the subsidy is exhausted, they are replaced by a new subsidized employee.

As in the previous section, these patterns within the third sector may be partially driven by the institutional design of the subsidy itself. The design of the scheme has deliberately tried to avoid market distortions in the private sector. Some of the rules are less strict for employment in the third sector, insofar as these employers are understood to be operating outside the competitive open market.

Among the third sector employers most heavily reliant on the subsidies, when the availability of the subsidy was reduced in 2015, their total employment subtracted similarly. Outside of this group, most subsidies go to employers who only cover a small fraction of their wage bills with the subsidy. Thus, among the private and the public sector, any employer-level effects are also likely to be small and difficult to estimate. Some further descriptive evidence on employers is presented in appendix Q.

Table 6: Use of the subsidy by different employer types from 2006 to 2022.

Sector	Sector's share of subsidies	Sector's share of wages	Subsidy ratio threshold: lowest 50%	Threshold: 90%	Wage share of intensive users	Sector's aggregate subsidy ratio
Public	31.3% of subsidies	26.5% of wages	0.7%	1.6%	0.004%	0.4%
Third sector	32.9% of subsidies	3.9% of wages	36.8%	85.2%	9.6%	2.7%
Private	32.2% of subsidies	62.3% of wages	2.0%	22.4%	0.43%	0.2%
Unknown	1.5% of subsidies	2.1% of wages	1.9%	61.0%	0.49%	0.2%
Publicly owned	2.0% of subsidies	5.1% of wages	33.6%	43.4%	0.35%	0.1%

Sector for employers identified from Statistics Finland FOLK modules. Subsidy sums estimated by combining wage and subsidy spell data. *Subsidy ratios* refer to the ratio of hiring subsidies to wages, calculated at the employer level over the entire period. The ratio *thresholds* at 50% (or 90%) mean that 50% (90%) of subsidies within the sector were collected by employers who had a ratio lower than the threshold. Wage share of *intensive users* reports the total wage share within a sector for intensive employers, defined as having an overall subsidy-wage ratio of 5% or higher. Finally, the aggregate ratio of all subsidies to all wages within a sector is reported in the last column.

8 Sensitivity

8.1 Placebo tests for the displacement effects

Three placebo tests were performed for the difference-in-differences analyses in Section 6. The placebo tests use the same geographical regions, outcomes and risk group definitions as the main estimates, but for different year-pairs where the funding of the subsidy did not change. As before, each at-risk group is separately constructed for July 1st of a given year, and wages are then examined for 12 months starting from this base date.

Besides this measure of parallel trends, an additional concern is the sensitivity of the analysis to how the risk group is constrained. The threshold used for the employment prediction was chosen so that it would include at least 80% of subsidized workers in all years. A higher threshold would cover a larger share of subsidies and might also identify more of the displacement effects if they are more widely dispered in the labour market. The tradeoff in increasing the threshold is that a much smaller share of the sample is affected by the subsidy, making small effects more difficult to identify in practice.

Table 7 presents the results from both the placebo tests and from varying the at-risk group (through the employment prediction threshold). Generally, the estimates of subsidized wages are close to zero for the placebo tests, and similar to the baseline for the alternative thresholds. The estimated effects on total wages are somewhat more variable in absolute terms, but remain reasonably close to zero (for the placebo tests) or to the baseline estimates (for varying the risk group).

Table 7: Estimation results for displacement effects, placebo tests and different at-risk groups.

Analysis	Subsidized wages	Total wages	Observation
Baseline	-213.00 (36.00)	-187.00 (55.60)	632, 691
Placebo, 2011-2012	2.19 (17.50)	-23.10 (72.30)	625, 052
Placebo, 2012-2013	22.20 (16.50)	76.30 (44.20)	618, 569
Placebo, 2013-2014	-24.00 (29.50)	-19.30 (49.40)	627, 194
Threshold 0.15	-214.00 (39.30)	-200.00 (53.20)	573, 823
Threshold 0.30	-211.00 (32.30)	-167.00 (67.60)	716, 061

Estimates are for the coefficient δ in equation (1). Bootstrapped standard errors in parentheses. Wages are in 2019 euros over 12 months. Estimated with the same control covariates as in section 6.

8.2 Other simultaneous changes

The changes to funding in 2015 coincided with a number of structural changes to the subsidy. As noted in the institutional section, these changes mainly aimed at simplifying and streamlining the system. In terms of observable characteristics of the participants,

it appears that the targeting changed slightly in both 2015 and 2016, compared to both 2013 or 2014. (A subsequent, smaller reform was implemented in 2017.)

Most of the observed changes in participants' characteristics over time, such as in their age, prior lifetime wages, recent employment, and gender, appear relatively minor, and appear to be similar across areas. Table 8 summarizes the changes using the mean predictions for open market employment and subsidized employment as summary measures. The maximum observed change across year-pairs and sectors was 3 percentage points. Out of all variable-year pair-sector triplets examined, in approximately 5% of the cases the change differed across regions by a statistically significant magnitude. These changes are listed in detail in appendix A. Finally, the results from matching also suggest that the structural changes did not change the subsidy's effects on participants in profound ways. Thus, it appears safe to interpret the estimates from 6 as estimating the effects of partially shutting down the availability of the subsidy, rather than a joint effect of this reduction and a structural reform of the subsidy.

Table 8: Changes in the characteristics of subsidy participants from 2013 to 2015 and 2016.

Prediction	Sector	Latter year	Value in base year, not depleted	Value in base year, depleted	Value in latter year, not depleted	Value in latter year, depleted	Diff-in-diff estimate
Employment	Municipality	2015	0.07 (0.07)	0.07 (0.07)	0.05 (0.06)	0.05 (0.06)	-0.06 (0.04)
		2016			0.05 (0.06)	0.05 (0.06)	-0.02 (0.04)
	Private	2015	0.11 (0.09)	0.13 (0.10)	0.09 (0.09)	0.09 (0.08)	-0.14 (0.06)
		2016			0.08 (0.08)	0.09 (0.09)	-0.05 (0.05)
	Third	2015	0.05 (0.06)	0.05 (0.07)	0.04 (0.05)	0.04 (0.05)	-0.07 (0.04)
		2016			0.04 (0.05)	0.04 (0.05)	-0.07 (0.04)
Subsidy	Municipality	2015	0.26 (0.18)	0.26 (0.18)	0.24 (0.14)	0.25 (0.14)	0.06 (0.05)
		2016			0.26 (0.15)	0.26 (0.15)	0.00 (0.05)
	Private	2015	0.19 (0.17)	0.18 (0.17)	0.20 (0.15)	0.19 (0.15)	0.04 (0.05)
		2016			0.22 (0.17)	0.21 (0.16)	0.01 (0.05)
	Third	2015	0.28 (0.19)	0.28 (0.18)	0.25 (0.15)	0.27 (0.15)	0.13 (0.06)
		2016			0.28 (0.17)	0.29 (0.17)	0.07 (0.06)

Predictions were made from July 1st data for open market or subsidized employment over the next 12 months, with a model trained on 2013 data and with standard deviations listed in parentheses. The difference-in-differences estimates correspond to the coefficient δ in 1 for demeaned and standardized prediction variables, with bootstrapped standard errors in parentheses.

8.3 Biases from later subsidized jobs

Some control units matched to the treatment participants in Section 7 enter subsidized jobs later on. By the end of the fourth follow-up year, 18%–25% of the control units had entered a subsidized job.[23] This relatively high participation rate is partially explained by the fact that units are matched by their propensity to treatment, and many of the examined individuals remain prone to enter subsidized jobs for long periods.

While the treated units can also re-enter a different subsidized job later on, this is not common. This effect is at least partly a mechanical consequence of the duration limits. After a subsidized job, an individual must typically requalify for the subsidy through a new lengthy period of unemployment.

There are two implications: first, insofar as the subsidy improves employment rates, it will also improve rates among the control units. If such cases are common, the estimated effects from matching are the effect of getting a subsidized job *earlier rather than later*, as noted by Sianesi (2004).

Another aspect of this question is related to the choice of the outcome measure. Consider an individual who is in a subsidized job during the follow-up. This mechanically but temporarily increases their unrestricted wages and decreases their open market wages, for the duration of the subsidy. Suppose the person is a matched control unit; in this case, neither measure is an accurate reflection of the unsubsidized employment the treatment participants would receive without a treatment or with a later treatment. The simplest way to tackle this question is to calculate both measures and see whether they differ substantially, which is done in table 9. The estimated effects change only slightly, especially for the private sector, suggesting that the choice of measure is not important. Appendix O further demonstrates that incorporating entrepreneurial and property income from self-employment in the income measure does not substantially change the estimates either.

Table 9: Estimated effects of subsidies in the fourth year from a base year, pooled sample for base years 2012–2017.

Sector	Open market wages	Wages including subsidized jobs	N
Private sector	4 458.4 (183.8)	4 461.9 (190.1)	38,674
Municipality	365.3 (148.2)	419.2 (150.0)	33,262
Third sector	-133.1 (161.1)	-255.0 (194.4)	28,876

Does not include income from self-employment. Each person entering a subsidized job in a given sector between July in base year t and June of year $t+1$ was matched separately to a non-participant who did not enter between this initial follow-up. For control covariates used, see appendix M.

[23] The lower percentage is for the control units matched to treated units in the private sector, and the higher percentage for the other sectors.

9 Discussion

This paper has presented new evidence on both the indirect and the direct evidence of the Finnish hiring subsidy scheme. According to the difference-in-differences estimates calculated among large difficult-to-employ populations, the depletion of subsidy funds in 2015 reduced subsidized wages by 37.2 million euros (8.3% of the baseline wage sum, s.e. 1.4%) relative to total wages, and total wages by about 32.7% million (7.3%, s.e. 2.2%). If the hiring subsidies were replacing job opportunities among this group to a substantial extent, one would have expected the reduction in total wages to be much smaller.

As with any estimates on displacement, a number of assumptions limit the scope of the estimates. First, it was assumed that the displacement would occur over a limited time scale: both subsidized and total wages were only followed for twelve months. Second, the displacement was assumed to be confined to regional labour markets, albeit in this case the geographical areas are very large. Third, the effects were only examined among a group of working-age individuals with low employment prospects. Thus, it is not possible to rule out effects on employees with higher productivity through direct substitution of workers, redistributing tasks from higher-wage workers to subsidized workers, or through general equilibrium.

Turning to the direct effects of the subsidies on participants, the estimates based on matching suggest that private sector subsidies improve later annual wages in unsubsidized jobs by about $4,500$ euros (+42%) on average. In contrast, subsidies to the public sector and the third sector do not yield such benefits. Both types of findings are consistent with prior research from both Finland and other countries.

The reduction in the availability of funding does not appear to have changed the effects of the subsidy on participants. While there is some year-to-year variation in the effects, this variation was not different across regions with very different availability of the subsidy, and it also occurs over years where the funding and structure of the subsidy were unchanged. As the depletion of funds reduced both the number and the duration of new subsidy spells, this would be consistent with even relatively short subsidy spells being effective in the private sector.

In the private sector, many of the worker-subsidized employer pairs form a persistent relationship: while the strength of this bond diminishes over time, some workers stay and even repeatedly return to the same employer for up to ten years. The treatment–control difference in employment elsewhere appears, in contrast, to be close to zero. While this decomposition of employment pattern cannot be given a causal interpretation, it is consistent with most of the employment benefits stemming from the bond between the worker and the subsidized firm, rather than stepping-stone effects. Such a pattern might be explained by asymmetric information, where employers end up using the subsidized job to reduce the costs of employee screening, or by the human capital accumulation in

subsidized jobs being largely firm-specific.

The cumulative effect of private sector hiring subsidies on the balance of direct taxes, transfers and subsidy costs is estimated to be roughly +4000 to +5000 euros by the end of the fourth year since entering the subsidy. Incorporating rough estimates of effects on consumption taxes and employer-side payroll taxes further improves this estimate.

Including these additional items into the balance brings the net effect of public sector subsidies to close to zero in the medium-term. This result is somewhat surprising, as the subsidies in this sector did not have significant effects on employment. However, the estimates suggest that in the public sector, most of the subsidized workers would have continued to live on benefits, had the subsidized job not been available. Since the monthly subsidy cost is on average close to the monthly unemployment benefit cost, the subsidy only increases fiscal burden for a residual group: those jobseekers who would have quickly left the benefit system if the subsidized job was not available. Even a modest increase in overall taxes paid, due to increased income and consumption during the subsidized period, is sufficient to offset this difference.

An important assumption necessary for this result is that the unsubsidized part of the wage paid by municipalities reflects the worker's productivity and gains to the public sector. If the primary reason for municipalities for hiring these individuals is something other than their work product, such as their welfare or labour market situation, then the entire wage paid by municipalities should be included in the public costs. In this latter case, the effects on the tax-transfer balance would be similar for the third sector and the public sector.

The subsidies are widely dispersed among employers in the private and public sectors. Most employers only utilize the subsidy sporadically, for a small fraction of their entire workforce, and the use of subsidies is not strongly concentrated in specific industries.

All the above results on the participants appear much less positive for work done in the third sector. The estimated effects on employment are close to zero or even negative, despite the unit costs of the subsidy being higher than in the private and public sectors. A significant share of the subsidized employers in the third sector also finance a large fraction of their long-run wage expenditure with the subsidy. Due to the individual-specific duration limits, this leads to high staff turnover, as new subsidy-eligible workers are hired to replace outgoing ones after their subsidy was exhausted. However, the available of evidence is insufficient to conclude whether the weak effects are driven by negative selection or by the nature of the work itself: on average, the participants in the third sector have much weaker employment prospects to start with than those in other sectors.

The paper has estimated effects on employment, wages, taxes and transfers. The subsidy may also have other effects on the welfare of participants or on their use of public services. The current estimates imply, however, that in the third sector such effects would have to be exceptionally large to justify the subsidy. Even if subsidies to the third sector

turned out to be welfare-improving despite these costs, the current implementation would probably be suboptimal for two reasons. First, the present approval process emphasizes the assessment of the employment gains from the subsidized job; if the job primarily serves other purposes, then the process should strive to ensure that those goals are being met. Second, the subsidy provides only limited-duration jobs, which is likely to increase onboarding costs and decrease the welfare benefits to participants.

Overall, the hiring subsidy appears to be working as intended in the private sector. The subsidy is improving employment in the open market among a group that is otherwise difficult to employ, and the scope of displacement effects appears small. This result is likely due to the relatively strict eligibility criteria, limited durations and screening of applicants. In the third sector, there is no evidence of positive employment effects, and the program also has high unit costs. The public sector lies between the two extremes: it appears to have little long-term positive effects, but the estimated net cost of the program is also low.

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