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Behavior and Effectiveness of Decentralized Employment Offices

Jeremias Nieminen*
Ohto Kanninen**
Hannu Karhunen***

Jeremias Nieminen, University of Turku*
Ohto Kanninen, Labour Institute for Economic Research**
Hannu Karhunen, Labour Institute for Economic Research***

Abstract

We study how decentralization of public employment services affects the labor market outcomes of job seekers and the behavior of employment offices. We utilize a Finnish temporary reform during which employment services were decentralized for specific target groups of job seekers in 23 treated municipalities and remained centralized for others. We estimate causal effects of the temporary reform using individual level difference-in-differences in a matched sample. We find no evidence of better labor market outcomes and find that municipalities are able to shift 14-17 % of their unemployment benefit costs to the central government..

JEL Classification: J08, H75, J48

Keywords: public employment services, fiscal federalism, decentralization

Tiivistelmä

Tutkimme yksilötason rekisteriaineistolla, miten julkisten työvoimapalveluiden alueellistaminen vaikuttaa sen kohteena olleiden työnhakijoiden työmarkkinatulemiin sekä itse työvoimapalvelun toimintaan, kuten työllisyys- ja aktivointisuunnitelmien määriin sekä ohjaukseen eri aktivointipalveluihin. Hyödynnämme tutkimusasetelmana Suomessa vuosina 2017-2018 järjestettyä työ- ja yrityspalveluiden alueellista kokeilua, ja tutkimme sen vaikutuksia yksilötasolla erot eroissa (difference-in-differences) menetelmällä. Tuloksiemme mukaan kokeilulla ei ollut lyhyellä aikavälillä vaikutuksia sen kohteena olleiden henkilöiden työkuukausiin, tuloihin tai liikkuvuuteen. Tuloksemme viittaavat myös siihen, että kunnat pystyivät siirtämään 14-17 % työmarkkinatuen kuntaosuuden kustannuksista valtiolle lisäämällä aktivointipalveluihin osallistumista.

Avainsanat: työvoimapalvelut, alueellistaminen, työllisyys

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00100 Helsinki

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1. Introduction

The provision of public employment services (PES) has been decentralized in many countries in the hope that it would increase the efficiency of employment services, since municipalities may have a better understanding of the local labor market and may be able to integrate employment services with other municipal services. The fiscal federalism literature suggests decentralization can in principle make public services more suited to local needs under certain conditions (Oates, 1972; 1999; Faguet, 2004). However, in the case of PES, it is also possible that local policy makers have other ambitions than improving the national level of employment: local governments may use their increased power simply to optimize their own budgets at the expense of the central government. This could result in job seekers being directed to less effective active labor market policies (ALMP) if those programs are otherwise beneficial for the local government, or in lower mobility of job seekers if the municipalities aim to get the job seekers employed in their own jurisdiction. Previous evidence on the costs and benefits of PES decentralization is very scarce and consists of two papers (Mergele & Weber 2020; Lundin & Skedinger, 2006). Deepening our understanding about the effects of employment service decentralization and relevant mechanisms is needed for more efficient policies.

In this paper, we provide quasi-experimental evidence on the effects of public employment service decentralization on job-seekers' labour market prospects and on the behavior of local governments. Crucially, we show how employment office behaviour and service provision change in a setting where municipalities are given the authority to arrange employment services, and where there exists a possibility to shift some of the costs of unemployment to the central government. Cost-shifting is possible through reductions in specific penalty payments. We study whether the local governments engage in cost-shifting behavior when they are given a direct channel to do so. Although some indirect evidence of cost-shifting behavior during PES decentralization has been found, the possibility to measure its magnitude directly is a novelty compared to earlier research on the topic. Our setting also allows us to study spillover effects to better understand whether resources get shifted between the non-target and target populations in the participating municipalities.

In the absence of random assignment of job seekers to centralized and decentralized regimes, our research design exploits a large-scale temporary reform during which employment services were decentralized for specific target groups of job seekers in 23 participating municipalities. The eligible individuals represented 10 percent of all job seekers in Finland in August 2017. A significant part of job seekers remained in the centralized system within and outside the participating municipalities, which makes it possible to evaluate the causal effects of the policy using quasi-experimental methods. We base our empirical approach on difference-in-differences combined with matching (see e.g. Heckman et al., 1997; Heckman et al., 1998; Blundell & Costa Dias, 2000) while using extensive administrative register data on job seekers. As a secondary approach, we conduct triple difference estimation separately for different areas.

We find no evidence that PES decentralization affects employment months per year, annual labor income or annual mobility of job seekers. Thus, this study does not find any support for the claim that the decentralization of employment services would be effective in increasing the employment prospects of job seekers in the short run. Our results also differ from and complement the negative employment effect estimated by Mergele and Weber (2020), since they looked at a different outcome, the job-finding rate. A non-existing effect on labour mobility is consistent with the earlier results by Mergele & Weber (2020) and Lundin & Skedinger (2006), dissipating possible concerns that the employment effort of the local authorities is skewed towards their own jurisdiction at the cost of worker mobility and national level employment.

We are able to study cost-shifting behavior of the municipalities in detail due to Finnish institutional arrangements. There is a clear incentive for municipalities to reduce measured long-term unemployment, for which they pay a monetary penalty. The decentralization of employment services gave them the possibility to reduce their penalties, for example through increasing ALMPs. Municipalities have to pay 50 (70) percent of the costs of unemployment benefits for everyone who has received a certain type of unemployment benefit for more than 300 (1000) days, but these penalty payments do not have to be paid if the job seeker participates in an ALMP. In addition, days in an ALMP do not count towards the 300 (1000) day cutoff. The central government bears the costs of unemployment benefits for job seekers with less than 300 unemployed days and pays for all ALMPs. We find a negative effect of 5 percentage points (17 %) in 2018 on the probability of being registered as unemployed for more than 300 days during a year, which is consistent with a cost-shifting strategy and also the announcements of local governments themselves as to their aims. Thus, decentralization reduced the number of individuals on the "penalty list" of long-term unemployment without increasing employment.

To learn more about what mechanisms at the local government level led to reductions in registered long-term unemployment and thus cost-shifting, we first estimate the effect of decentralization on the number and the type of employment and activation plans the local employment office initially conducts with the job seeker, as well as on ALMP placements later in the job seeking process. We find that decentralized offices reduced the number of plans conducted in total, and changed the plan composition, resulting in higher number of activation plans, which are plans that are made mainly for the long-term unemployed. This suggests that municipalities focused their efforts on the long-term unemployed, possibly at the expense of other job seekers.

When we focus on ALMPs, we see that decentralization increased the number of ALMP months by 27 percent during the first full year of the reform

(2018). While the size of the point estimate is quite similar across specifications, the estimate is not significant in most of the specifications due to lack of power. Our results suggest that the participating municipalities seem to have directed job seekers especially to rehabilitative work and wage subsidy programs: these two explain the increase in ALMP months, since point estimates on other ALMP types are negative or very small. Although statistically not significant, the point estimate on rehabilitative work in 2018 is very sizeable, indicating an increase of 34 %. This is consistent with the rise in activation plans: an activation plan is required for rehabilitative work placement.

Although increasing any type of ALMP placements would reduce the costs for municipalities through reductions in penalty payments for long-term unemployment, some ALMPs may be more beneficial or easier to expand at the local administrative level. Earlier research suggest that decentralization of employment services increase participation in those ALMPs that are organized by the municipality (Lundin & Skedinger 2006) and that are public employment schemes (Mergele & Weber 2020). Rehabilitative work schemes are organized and administered at the municipality level in Finland while its expenses are mainly covered by the central government. Furthermore, municipalities are possibly benefiting directly from rehabilitative work schemes as a customer receiving the services. Thus, our findings on rehabilitative work are also indicative of a cost-shifting strategy.

Our work touches upon two separate strands of literature: one studying the effects of decentralization of central government functions (see e.g. Oates, 1999; Faguet, 2004; Martinez-Vazquez et al., 2016) and other focusing on employment services (see e.g. Fougere et al. 2009) and active labor market policies (see e.g. Kluve, 2010; Card et al. 2010; Card et al. 2018; Crepon & van den Berg 2016). Despite the fact that employment services are decentralized in many developed countries, too little is understood about how decentralization affects the effectiveness of public employment services. Relevant empirical studies on this topic include Mergele and Weber (2020), Lundin and Skedinger

(2006) and Boockmann et al. (2015). Of these, Mergele and Weber (2020) is the only one estimating causal employment effects. Second, our finding of small or inexistent short-run employment effects of ALMPs are consistent with earlier findings in the massive ALMP literature (e.g. Kluve, 2010; Card et al. 2010; Card et al. 2018; Crepon & van den Berg 2016).

We expand the existing but scarce research on decentralization of public employment services and cost-shifting[1] by Mergele and Weber (2020) and Lundin and Skedinger (2006) who have found support for the hypothesis that decentralized employment offices try to shift costs to the central government. In particular, we are able to evaluate the amount of cost-shifting while explaining how local authorities change their behavior and procedures in practice. Our findings should provide important information to policy makers who plan to decentralize government services concerning possible unanticipated costs and how one might avoid the possibility of perverse incentives at the local administrative level. We estimate that local governments succeed in shifting a significant amount of costs, approximately 6.7–7.5 million euros per year to the central government during the temporary reform. If the policy change was implemented nationwide, it would potentially transfer 55–61 million euros of expenditures annually from local governments to the central government. This represents around 0.29-0.32 % of the 23 billion euros the were collected annually as municipal taxes, or 14-15 % of the 400 million euros of long-term unemployment penalties paid annually by the municipalities.

The paper is organized as follows. Next section provides details on institutional background and how decentralization quasi-experiment was

[1] Cost-shifting refers here to local governments trying to shift costs to higher levels of government. In political economy, cost-shifting is often thought to be a problem in centralized systems where common pool problems are present (see e.g. Weingast et al. (1981) and Besley & Coate (2003)), i.e. local governments have incentives to increase their cost since those costs are paid by the national budget. In some cases decentralization can mitigate these concerns, if the local governments are responsible for financing the services. In the case of the Finnish employment service decentralization (similarly to the German reform examined by Mergele et al. 2020), the costs of ALMP programs are paid by the central government, which makes it possible for municipalities to shift costs to the central government.

performed. Section 3 introduces data, and the empirical strategy used. and Section 4 presents estimation results and discussion on the robustness and validity of our results. Section 5 concludes.

2. Institutional background

2.1 Public employment services in Finland

Public employment services are currently administered through ELY centers (Centre for Economic Development) in Finland. These 15 centers around Finland are controlled by the Ministry of Employment and the Economy (TEM), and they execute the central government's employment, transportation and environmental policies. Hence, the central government is currently in charge of providing public employment services to Finnish job seekers. The Finnish law on public employment services (FINLEX 916/2012) states that the employment agencies should offer job placements services, advisory, and services to help job seekers accumulate human capital or become entrepreneurs. Employment agencies are also responsible for arranging active labor market services and directing job seekers to these services.

Finnish PES offices also monitor the job search process: for example, they give statements that determine eligibility for unemployment benefits, and they conduct different types of plans for job seekers. In these plans, the PES office marks what kinds of tasks – such as job applications, health checks, or service participation – the job seeker needs to complete. There are three different types of plans: employment plans, activation plans and integration plans. According to the official guidance, employment plans should be conducted every three months. These plans should include information about job seeker's situation, goals and possible limitations. In addition, the plan includes tasks the job seeker needs to complete; at least one such task is mandatory and has a deadline. If the job seeker is unable to complete the task before this deadline, he may face benefit sanctions.

Activation plans are conducted when rehabilitative work placement is considered, although an activation plan will not automatically lead to a placement in rehabilitative work: if an individual is fit for other services, he should not be directed to rehabilitative work. An activation plan should be conducted if an individual has been unemployed for a long time: over 180 days or 500 days depending on age. In addition, activation plans should be conducted for individuals who receive income support (the last resort social benefits) as opposed to unemployment benefits. Activation plans are updated every 3-24 months. It is, therefore, possible for the offices to change the frequency of making these plans if they want to do so.

Employment plans (and similar integration plans, which are aimed at recent immigrants) are conducted by the employment office, whereas activation plans are conducted co-operatively by employment offices and municipalities. During the decentralization quasi-experiment, described in the next subsection, this changed: all plans were conducted by the municipal offices during the temporary reform in treated municipalities.

2.2 Temporary and partial decentralization

The temporary decentralization[2] studied in this paper was called the Regional Pilot of Employment and Enterprise Services (in Finnish: *työvoima- ja yrityspalveluiden alueellinen kokeilu*).[3] This large-scale temporary decentralization was conducted between August 2017 and December 2018 with the aim of supporting employment, job creation, and entrepreneurship. During

[2] Decentralization of public employment services refers here to what extent employment programs and services – including budgetary powers - are organized and managed at the sub-national levels of government. For more broader literature on fiscal and governance decentralization, see the literature review by Martinez-Vazquez et al. (2016).

[3] We have earlier conducted a municipal-level analysis of this temporary reform and another Finnish decentralization Pilot (see Nieminen et al. 2020). The municipal level analysis is, however, not enough, since only a subset of job seekers in treated municipalities participated, thus making it necessary to evaluate the effects using individual level data. In addition, we did not investigate cost-shifting, plans, or participation in different ALMP types in the municipal-level analysis we conducted.

the temporary decentralization, 23 treated municipalities in five areas assumed control of providing employment services for the specific target group of job seekers for a period of 17 months. During the reform period, the treated municipalities were responsible for conducting employment and activation plans with the job seeker and for directing job seekers to ALMP programs. Table 1 presents the responsibilities of municipalities and centralized employment offices before and during the temporary reform.

Table 1: Responsibilities before and during the temporary reform

Responsibility	Regular process	During the temporary decentralization in treated municipalities
Conducting employment plans and integration plans	Centralized employment office	Municipal employment office
Conducting activation plans	Centralized employment office together with the municipality	Municipal employment office
Directing job seekers to ALMPs	Centralized employment office	Municipal employment office, although selection decisions to labor force training were made by the centralized office
Official statements (e.g. benefit sanction statements)	Centralized employment office	Centralized employment office
Unemployment benefits	The central government, except for individuals on the penalty list for whom the municipality pays 50-70 % of the cost	The central government, except for individuals on the penalty list for whom the municipality pays 50-70 % of the cost
ALMP financing	The central government	The central government

Figure 1 illustrates the five pilot areas and municipalities. All municipalities could apply for the pilot program, but municipalities had in practice to apply in to pilot together, which is why treatment is clustered as can be seen from Figure 1. In June 2016, 23 municipalities belonging to 5 areas were selected from 77 applicant municipalities by the Ministry of Economic Affairs and Employment of Finland. Thus, municipalities were not randomly assigned to program. According to an official notice, the selection of participating areas was made by evaluating the applicants using the following criteria: what kind of services the applicants planned to conduct, how much the Pilot could potentially lower the aggregate unemployment costs for the whole public economy (central + local governments), how well the areas promised to follow the implementation of the Pilot, how committed the areas were to the implementation of the Pilot, how the areas planned to promote growth and entrepreneurship during the Pilot. The applicant areas had to provide information about these aspects in their application. In addition to the criteria described above, the Ministry of Economic Affairs and Employment also aimed to choose areas from different parts of the country to participate.

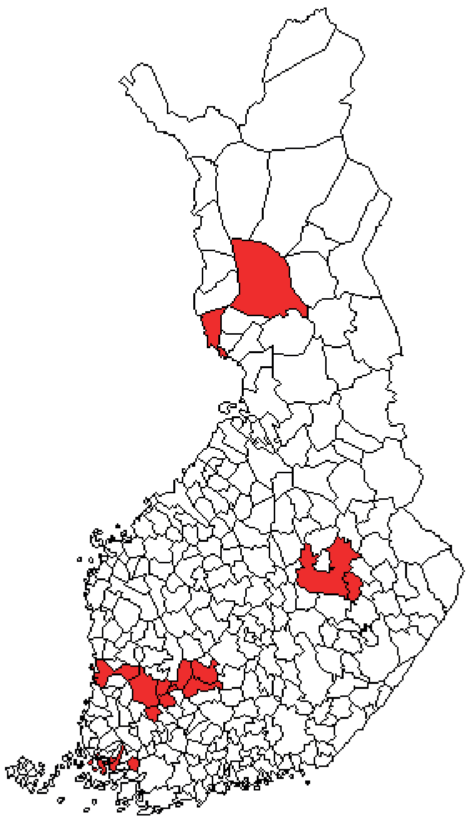

Figure 1: Participating municipalities.
Notes. Treated municipalities are in red.

Table 2 shows how the eligible groups varied by the five treatment areas. The pilot was mainly aimed at the long-term unemployed and those who do not receive income-dependent unemployment benefits (Annala et al. 2019). During the reform period, municipalities began providing all employment services for eligible job seekers within their jurisdiction, while the centralized PES office provided these same services for other job seekers. Hence, there were two types of employment offices in each treated area: the decentralized and centralized one.

Decentralized services were not similar in all areas as municipalities exercise new decision-making power to offer different individualized services best suitable to the regional needs. Most notably in the biggest treated area, Pirkanmaa (consisting of 10 municipalities), each job seeker was assigned an employment coach, who offered guidance to the job seeker (Arnkil et al. 2019). We are not aiming to study the effects of any single intervention the municipalities did, but instead to evaluate the average effects of decentralization.

Table 2: Characteristics of the treated areas

Treated area	Number of municipalities	Target groups
Pirkanmaa	10	Individuals who receive basic unemployment allowance or labor market subsidy (i.e. who do not receive the income-dependent unemployment benefit)
Varsinais-Suomi	4	Job seekers under the age of 25 and job seekers who have been unemployed for more than 12 months
Pohjois-Savo	3	Job seekers who have been unemployed for more than 12 months
Lappi	5	Job seekers who have been unemployed for more than 12 months
Pori	1	Job seekers under the age of 25 who have been unemployed for more than 200 days and job seekers under the age of 25 who have received the labor market subsidy for more than 200 days

Notes. The table presents target groups of job seekers in different areas.

Table 3 further shows the numbers of initially eligible and initially treated individuals in five treatment areas. Eligibility predicts that individual is treatment, but not everyone who is eligible seems to receive the treatment initially[4]. We use all eligible individuals as our treatment group instead of using only those who receive treatment, although the results are similar if we use treated individuals instead of eligible individuals.

We drop from our sample the individuals living in Pori area when the treatment begins, since we cannot reliably identify the initially eligible individuals in Pori area due to complex eligibility criterion for individuals older than 25 years: 200 days receiving the labor market subsidy. We do not observe how many days the individual received this type of unemployment benefit, but only how many days the individual has been unemployed. If we use unemployment days as a proxy for days receiving the labor market subsidy, the resulting eligible population does not seem to identify the right individuals (see Table 3: Pori). In addition, Pori area comprised of only one municipality, whereas the other treated areas consisted of a larger number of municipalities. The results do not change if eligible individuals (using the criterion in Table 3) from Pori are included in the estimation, or if we use individuals who actually receive treatment as our treatment group.

Table 3: Eligibility and participation

	Pirkanmaa		Varsinais-Suomi	
	Participant	Not participant	Participant	Not participant
Eligible	17 657	2 172	6 771	590
Non-eligible	224	7 579	244	8 972
	Pohjois-Savo and Lappi		Pori	
	Participant	Not participant	Participant	Non-participant
Eligible	4 424	598	678	1 855
Non-eligible	204	10 476	599	2 654

Notes. Eligible individuals are those who are unemployed in the last day of 7/2017 and fulfil the eligibility criteria in their area. Participants are the individuals who actually received treatment (i.e. decentralized services). Participation status is precisely observed. Eligibility is not precisely observed in the case of Pori and Pirkanmaa areas. In Pori, we cannot reliably identify the eligible individuals since we do not observe whether an individual receives the labor market subsidy or basic unemployment allowance. In Pirkanmaa, we can identify the eligible individuals much better, but not perfectly, since we have to use membership in an unemployment fund (necessary but not sufficient condition for receiving the income-dependent unemployment benefits) as a proxy for receiving the income-dependent unemployment benefit.

[4] Initially treated means here that an individual's employment office code is changed to the municipal office code in the last day of July 2017 (the temporary reform officially begins in the first day of August 2017).

2.3 Services and the cost burden of the central government vs. the municipality

Finnish municipalities have to pay 50% of the costs of unemployment benefits for each unemployed person who has received the labor market subsidy[5] for more than 300 days, and 70% of the costs if a job seeker has received the labor market subsidy for more than 1000 days (FINLEX 1290/2002). Municipalities, however, do not need to pay these costs when the job seeker participates in active labor market policies. Additionally, days during which an individual participates in activation do not count towards the 300 day or 1000 day cutoff. We refer to these individuals as the *penalty list*. An individual belongs to the penalty list in any given time, if they are registered as unemployed, receive the labor market subsidy, and have received it for more than 300 days. The only thing that nullifies the unemployment days counter is working 6 months full-time. During the temporary decentralization, the treated municipalities could potentially decrease these penalty payments by increasing ALMP participation, since ALMP costs are paid by the central government. For example, the local offices could aim to increase the number of ALMP participants as much as possible, which could result in some individuals participating in programs not optimal for them. Alternatively, municipalities could save money by targeting individuals who are on the penalty list or about to cross the 300 day cutoff.

The amounts of penalty payments paid by municipalities are publicly avaible on the municipality level, but we cannot calculate the exact amounts from individual level data, since we do not know which of the three unemployment benefit types a job seeker receives and has received earlier[6]. We proxy for being on the penalty list by having more than 300 unemployed days

[5] Labor market subsidy is an unemployment benefit for individuals who lack the employment history required to receive other types of unemployment benefits.

[6] The type of the unemployment benefit depends on unemployment fund membership status (can be observed imperfectly), unemployment duration, and whether or not the individuals has enough working history (this is not observed).

per year. We use a complementary variable, in which we also condition on being unemployed at the end of the year.

In order to study whether municipalities exploited employment services to do cost-shifting we 1) investigate whether municipalities reduced the number of individuals unemployed for more than 300 days per year, proxying the probability of belonging to the penalty list, and 2) examine whether municipalities increase activation, and specifically placements in rehabilitative work, since it can be the easiest way to increase ALMP participation, despite the fact that these programs may not be optimal for job seekers. In fact, according to Finnish law, only job seekers who need rehabilitation should be directed to these programs. These programs, however, might be valuable for municipalities also for other reasons than the reductions in penalty payments: in the absence of work schemes, the local governments would probably need to purchase some of the work hours (e.g. maintenance work) from the private market at the market price.

Cost-shifting, especially through reducing the cost burden that penalty payments cause to the municipality, was also a self-declared aim of some Finnish municipalities. For example, in an interview in Kuntalehti (2020), the director of employment services in the City of Tampere, emphasized that they were able to reduce their cost burden by 7 million euros during the temporary reform studied in this paper. Finnish municipalities have been very active in lobbing for permanent decentralization of employment services, suggesting that the prospect of being responsible for employment service provision seems alluring to the municipalities.

3. Data and methods

3.1 Data

3.1.1 Data sources

The individual level administrative datasets utilized in this paper are from Statistics Finland and the Finnish Ministry of Employment and the Economy (TEM). We combine basic information about job seekers to their history of employment, earnings, and ALMP participation. The used data modules are FOLK basic, FOLK income, TEM Job search and TEM Job seeker.[7]

FOLK basic module has annual information of all people living in Finland, i.e., more than 5 million yearly observations. From this data, we get basic covariates such as gender, age, place of residence, employment months per year, marital status, education, and some other demographic variables. Annual income and information about received and paid transfers originate from FOLK income module. We constrain our sample to individuals for whom we have data for every year, i.e., we only include individuals who have lived in Finland every year during 2006–2018. Doing this, we lose 2 808 of out 31 869 eligible individuals in the sample. We merge other needed variables to this yearly level, balanced panel dataset. The added variables are constructed using TEM modules and include information e.g., about plans conducted to job seekers, their ALMP participation, and whether the job seeker is a member of an unemployment fund.

[7] Data is available for research from Statistics Finland through remote access. The guidance for applying for the data access can be found here: https://www.stat.fi/tup/mikroaineistot/etakaytto_en.html.

3.1.2 Treatment and control groups

Before matching, our treatment and control groups consist of individuals who were unemployed or participated in activation at the end of July 2017. Of those, all eligible job seekers living in treated municipalities are included in the pre-matching treatment group, and individuals living in untreated municipalities are included in the control group. Ineligible job seekers inside treated municipalities are excluded from the sample in matched DiD estimations but naturally included when triple difference estimation is conducted.

Eligibility is defined differently in each treated area; see Table 2 for target groups. As can be seen from Table 3, eligibility predicts treatment, although not all eligible individuals are treated. Our pre-matching treatment group includes all initially eligible individuals, regardless of whether they were initially treated or treated at all. We could alternatively use those job seekers who actually receive treatment (i.e. are moved from the centralized to the decentralized system) as out treatment group, but since eligible and treated groups are almost the same, the results are very similar if such a definition is used.

In order to create the treatment and control groups, we need to know which individuals were unemployed or in activation at 7/2017, and which of those individuals were eligible to participate. To do this, we use TEM Job search data module, which has employment codes (i.e. unemployed, in activation, outside of labor force) for all job seekers. We keep those who are unemployed or in activation at the last day of July 2017 and create pre-matching treatment and control groups as described before. After that, we perform matching in order to construct the final treatment and control groups.

3.2 Empirical strategy

Matching adjustments

We match eligible individuals to non-eligible individuals to provide a control group for causal inference, since the 23 treatment municipalities and their residents can be expected to differ from non-treatment municipalities ex ante. Matching is conducted using basic background characteristics (age, gender, living in urban area) as well as variables related to individuals' employment history. Additionally, we match on pre-treatment outcome variables in our main matching specification. Using pre-treatment outcomes, especially lagged employment outcomes, are often used in labor market policy evaluations (see e.g. Dague et al. 2017). We only match on the outcomes of 3 years before treatment in order to be able to test whether the pre-trends are parallel in years before the matching period. We also conduct our analyses using a match where no pre-treatment outcome variables are used as it has been noted that using pre-treatment outcomes in matching may increase bias in when difference-in-differences with matching is used (Chabe-Ferret 2017).

We use one-to-one propensity score matching (PSM, see Caliendo & Kopeinig, 2008) as our matching algorithm. Balance of matching covariates before and after matching is shown in Appendix Table F1: most of the covariates are in balance after matching, except for municipal level variables. Kernel densities of the propensity score before and after matching is presented in Appendix Figure F1. We also check robustness to one-to-many PSM, and coarsened exact matching (CEM), since propensity score matching has been criticized by e.g. King & Nielsen (2019), who propose that CEM should be favored over PSM. The results are qualitatively similar when these alternative matching adjustments are performed. Results from alternative matching specifications can be found in Appendix B.

Difference-in-differences

Our main specification uses a standard difference-in-differences method to estimate the treatment effects. This is done by estimating two-way fixed effects regression models in the matched sample. The basic DiD model with individual and year fixed effects can be written as

$$Y_{it} = \gamma_i + \lambda_t + \delta(treat_i * post_t) + \varepsilon_{it}. \tag{1}$$

In the model (1), γ_i and λ_t are the individual and year fixed effects, respectively. The variable $treat_i$ is a dummy variable getting a value of 1 for individuals in the treated group, i.e. eligible individuals in treated municipalities. The variable $post_t$ is a dummy variable getting a value of 1 in the treatment period. The coefficient δ is the difference-in-differences estimate. In order to test the assumption that the pre-trends are parallel, we also calculate yearly treatment effects in the matched sample. This model can be written as

$$Y_{it} = \gamma_i + \lambda_t + \sum_{\substack{k=2006 \\ (k\neq 2016)}}^{2018} \theta_k D_{it}^k + \varepsilon_{it} \tag{2}$$

In the model (2), γ_i and λ_t are the individual and year fixed effects, respectively. The variables D_{it}^k are periodic treatment indicators, i.e. interactions between the treatment and the year variable. Year 2016 is the reference period, and hence the treatment indicator for 2016 is omitted. Standard errors are clustered at the municipality level. Coefficients θ_k are yearly treatment effects; they are difference-in-differences estimates calculated for each time period.

Triple difference estimation

Since the reform targeted only a subgroup of job seekers in each area, it allows us to conduct an additional analysis using triple differences (DDD) estimation in each area separately. We do not use any matching adjustments in the triple difference estimations. However, since the eligibility criteria differed slightly between the participating areas, no clear and unique control group exists for all areas. We thus need to first calculate the DDD estimates separately for each area with different eligibility criteria, and then calculate a weighted estimate from these 3 individual estimates, each of which uses a different control group, albeit the control groups may include same individuals. Calculation of the weighted estimate is done by weighting the individual estimates by the inverse of their squared standard error. Our DDD specification estimated separately in each of the 3 areas with different eligibility criteria can be written as

$$Y_{it} = \gamma_i + \lambda_t + \beta_1 post_t * eligibility_i + \beta_2 treatedmunicipality_{it} * eligibility_i + \beta_3 post_t * treatedmunicipality_{it} + \delta\, post_t * eligibility_i * treatedmunicipality_{it} + \varepsilon_{it} \qquad (3)$$

where γ_i denotes individual fixed effects, $eligibility_i$ denotes whether individuals fulfill the eligibility criteria and $treatedmunicipality_{it}$ denotes whether individual lives in a treated municipality at time period *t*. The triple difference estimate δ is the coefficient on the triple interaction term $post_t * eligibility_i * treatedmunicipality_{it}$. The triple difference results are estimated separately for 3 different areas: 1) Pirkanmaa, 2) Varsinais-Suomi, and 3) Lappi and Pohjois-Savo, since eligibility criteria were different in different areas. In addition to estimating DDD estimates for these areas separately, we also calculate weighted DDD estimates. Denoting the individual DDD estimates by $\widehat{\beta_i}$ and their standard errors by σ_i, the weighted estimates and standard errors are calculated like instructed in Dominguez-Islas & Rice (2018):

$$\hat{\delta}_{weighted} = \frac{\sum_{i=1}^{k} \frac{1}{\sigma_i^2} \widehat{\beta_i}}{\sum_{i=1}^{k} \frac{1}{\sigma_i^2}}$$

$$SE(\hat{\delta}_{weighted}) = \sqrt{\frac{1}{\sum_{i=1}^{k} \frac{1}{\sigma_i^2}}}$$

The weighted DDD estimation is not our main specification, since our weighted DDD estimates do not represent an average effect, but an effect weighted by the inverse of squared standard errors of the individual estimates. In some cases, this leads to the DDD estimates being different from the matched DiD estimates. For example, eligible individuals from the biggest treated area, Pirkanmaa, constitute 60 % of all eligible individuals – thus driving the DiD results – but for some outcomes the DDD estimate from Pirkanmaa gets a very low weight due to a larger standard error. Moreover, there are two additional concerns regarding the DDD estimates. First, our results show that they suffer from spillover effects.[8] Second, the pre-trends of our DDD estimates raise more concerns than those of our DiD estimates. The triple difference estimation results are reported in Appendix B.

4. Results

4.1 Labour market outcomes

Table 4 presents effects on labor market outcomes from our main specification, i.e., difference-in-differences estimates in a matched sample created using propensity score matching where pre-treatment outcomes are included in addition to other individual and municipal level characteristics. Observations from 2017 are dropped from the analysis, since the treatment began late in the year in August 2017, but naturally included in the yearly event study analysis.

Table 4, column 2 shows that decentralization of employment services had no effects on the number of months per year individuals worked in the short

[8] Our matched DiD estimates do not suffer from these types of spillover effects, since untreated individuals in the treated municipalities are not included in the matched sample.

term. Similarly, we do not find any significant effects on annual labour income (column 1), although the standard errors clustered by municipality are sizeable. The point estimates are small both in annual earnings (50 euros) and employment months (0.09 months).

Since our empirical strategy relies on parallel trends as the identifying assumption, we examine in Figure 2 the validity of our approach by estimating models that include leads and lags of the treatment indicator. We observe no significant pre-trends in employment months or earnings, although there is a slight insignificant drop in point estimates during the Great Recession years of 2008–2011 in the treatment group. Any specific bias during downturns that our research setup might suffer from, is not a worry during the years of the temporary reform (2017–2018), since they were years of robust economic growth. In the yearly figures it also has to be noted that the clustered standard errors vary, becoming visibly smaller in the post-treatment years compared to pre-treatment years. Due to matching, also levels in the treatment and control groups are similar as shown for all main outcome variables in Appendix A, allowing comparisons of estimated effects to the control group mean in the treatment year. Although it is not a perfect counterfactual for the treatment group, it is the best available comparison.

Columns 3 and 4 of Table 4 report estimates on long-term unemployment defined in two ways. First, long-term unemployment is defined as having more than 300 days in registered unemployment during the year (column 3), and second, being in addition officially registered as unemployed at the end of the year (column 4). We find a significant 5 percentage point decrease in the probability of being long-term unemployed. In relative terms, this means a 17 % reduction in the probability of long-term unemployment when compared to the control group mean in 2018. The size of the estimate is also robust to not using pre-treatment outcomes in matching, or to using CEM, although in those cases the estimate is only significant at 10 percent level. Results with alternative matching procedures can be found in

Appendix B (Table B1). Yearly treatment effects in Panel C of Figure 2 further show a negative effect of about 6 percentage points on the probability of long-term unemployment in 2018. No effect can be seen in 2017, which is again expected, since the reform did not start until the August of 2017.

Triple difference results can be found in Appendix tables B7. - B9. DDD estimates of the effect on long-term unemployment (Table B7.) are only significant in the case of the largest treatment area, Pirkanmaa, which consists of 10 municipalities. The weighted estimates are not significant, which is largely due to Pirkanmaa area getting a low weight in weighting due to larger standard errors compared to other areas. The DDD estimation strategy may also have other problems, such as non-parallel pre-trends.

Finally, we present effects on annual mobility in Column 5 of Table 4. The outcome variable we use to measure mobility is the probability of moving to another municipality during a year. We find no effects on mobility. Additionally, the point estimates are very close to zero, and robust across specifications (see Appendix B for robustness specifications). As can be seen from Figure 2, there are no pre-trends. Finding null effects in mobility is consistent with earlier research by Lundin and Skedinger (2006) and Mergele and Weber (2020) who found that PES decentralization did not cause regional lock-in of job seekers. Combined with earlier literature, these results suggest that decentralizing employment offices does not lead to decreased labor mobility, despite the fact that local governments have incentives to get job seekers employed in their own jurisdiction.

Table 4: Labor market outcomes

	(1) Labour income	(2) Employment months	(3) >300 days in registered unemploymen t per year	(4) >300 days in registered unemployment and unemployed at the end of a year	(5) Mobility
Treatment effect	51.25	0.0905	-0.0542**	-0.0558**	0.00128
	(639.6)	(0.259)	(0.0245)	(0.0237)	(0.0286)
Treatment group mean, 2016	1946.6	1.581	0.537	0.506	0.085
Control group mean, 2018	4301.0	2.707	0.322	0.312	0.062
N	697176	697176	697176	697176	697176
No. of individuals	58098	58098	58098	58098	58098
Individual FE	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes

Notes. Table presents difference-in-differences results in a matched sample created with one-to-one propensity score matching. Standard errors clustered by municipality are shown in parentheses. Significance levels: * p<0.1 ** p< 0.05, *** p< 0.01. Pre-treatment outcome variables are used in matching in addition to individual characteristics such as age, gender, and length of unemployment when the treatment begins. All matching variables as well as their balance before and after matching can be found in Appendix table F1. Pre-treatment period includes years 2006–2016 and post-period includes year 2018. Observations from 2017 are not included.

Panel A. Labor income *Panel B. Employment months*

Panel C. More than 300 unemployed days *Panel D. Annual mobility*

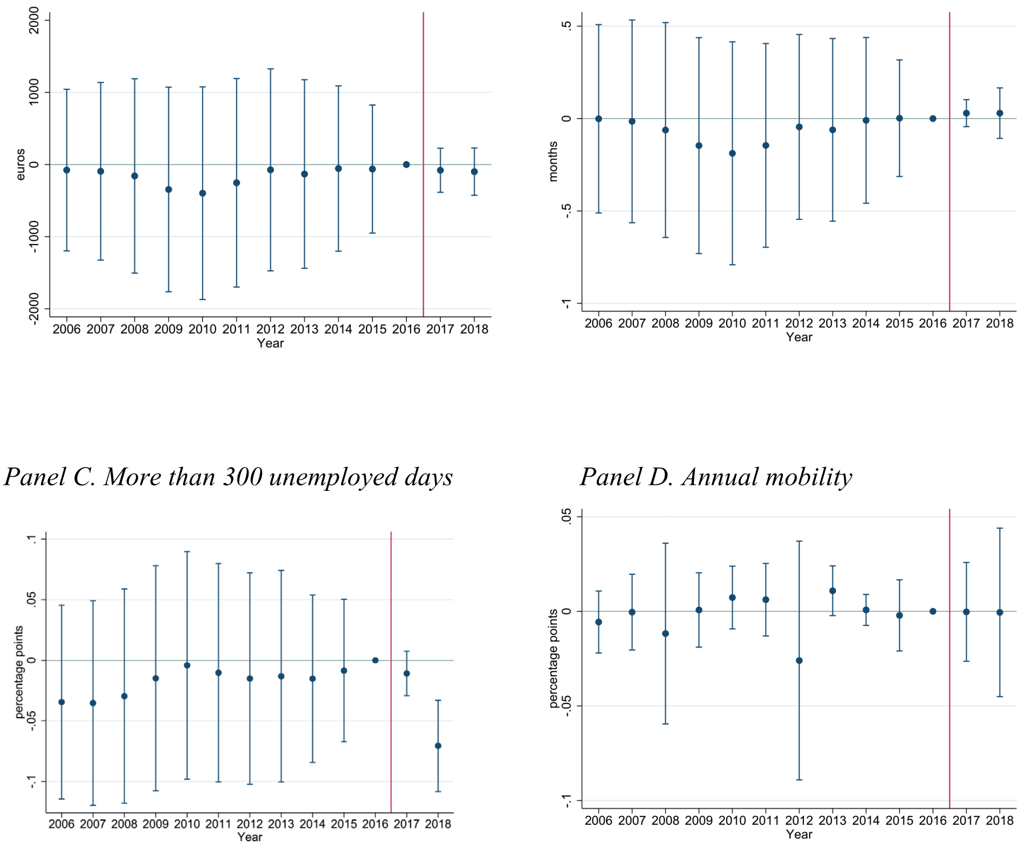

Figure 2: Labor market outcomes

Notes. Figure shows yearly treatment effects. 2016 is the reference period, treatment begins in August 2017. Treatment group includes all eligible individuals. Standard errors are clustered by municipality.

4.2 Size of cost-shifting

In this section, we calculate how much costs municipalities shifted to the central government via reductions in the penalty payments municipalities have to pay for each long-term unemployed individual. We also approximate how much the reform would cost to the central government, if implemented nationwide.

The size of cost-shifting through reductions in the penalty can be approximated using our estimation results. The average unemployment benefit is 703 euros per month. When a job seeker belongs to the penalty list, the municipality has to pay 50–70 percent of that cost. Since we find a reduction of approximately 5 percentage points (estimate in Table 4, Column 4) in the probability of having more than 300 unemployment days per year (a proxy for being on the penalty list), we calculate that if decentralization reform was implemented nationwide, this would result the size of the cost-shifting be approximately 55-61 million euros, amounting to approximately 0.3 % of annual municipal tax revenues.

Table 5: Size of cost-shifting through reductions in penalty payments

	During the temporary reform	If reform implemented nationwide	
	in euros	in euros	% of municipal tax revenues
Assuming municipality pays 50 % of the unemployment cost for everyone who has more than 300 unemployment days	6.7 million	55 million	0.29 %
Assuming municipality pays 50 % of the costs for 70 % of the long-term unemployed, and 70 % of costs for 30 % of the unemployed	7.5 million	61 million	0.32 %

Notes. We assume that the 5-percentage point decrease (see Table 4) in the probability of long-term unemployment implies a (0.054/0.322) * 100 = 17 % decrease in the number of unemployed individuals (relative to the number of long-term unemployed in the control group). The number of long-term unemployed in the control group in 2018 is 0.322*29049 = 9353. The size of cost-shifting is calculated in the following way: estimated relative effect in 2018 * share of long-term unemployed in the control group in 2018 * number of individuals in the control group * 12 * monthly unemployment benefit * share of costs paid by municipality. For example, in the upper left corner we have: 0.17*9353*12*703*0.5 = 6.7 million. When calculating the cost for potential nationwide implementation, we use 76330 (2018 average) of as the number of long-term unemployed per month.

If we further assume that the number of individuals in the penalty payment list equal the number of individuals who have more than 300 unemployed days per year, and that these individuals would be in the list the whole year, the implied aggregate amount of penalty payments in the whole Finland would be approximately 322 million euros in 2018. This is less than the actual number of penalty payments paid in 2018, which is approximately 400 million euros. Comparing cost-shifting estimates to the implied aggregate amount (322 million), the effect is 17 %, i.e., the same as the point estimate on long-term unemployment in relative terms. Relative to actual number of penalty payments (400 million euros), the municipalities would be able to save 14 % of their penalty payment costs.

There are a number of reasons why the implied amount (322 million) differs from the actual amount of penalty payments. First, the determination of penalty payments is complex and therefore, not everyone who is in the penalty list has 300 unemployed days during the year – or vice versa. It is possible that an individual is on the penalty list even if they do not have 300 unemployed days during the year, since the individuals may have accumulated the 300 unemployed days during previous years. On the other hand, our proxy for being on the penalty list, which is the probability of having more than 300 days in registered unemployment, includes also individuals who receive other forms of unemployment benefits than the labor market subsidy, who thus do not belong to the penalty list.

The effect on penalty payments (actual amounts) can alternatively be calculated using municipality level data. In Appendix Figure E1, we present municipal level difference-in-difference results, where all treated municipalities are compared to all untreated municipalities. We find that on average, decentralization decreases these payments by 450 000 euros per municipality (DiD estimate), which amounts to around 10.3 million euros in all 23 treated municipalities combined. Thus, the municipal-level estimate (10.3 million in the treated area) implies higher cost-shifting than our individual level approximation (6-7 million in the treated area).

4.3 Mechanism

Next, we try to understand the mechanism through which municipalities managed to reduce the number of individuals in the penalty list, consistent with a cost-shifting strategy, in the absence of any real employment gains. Municipalities have two key policies that they can independently adjust and that also influence the cost. First, the type of plans they conduct and second, which ALMPs the unemployed are then directed to. These two mechanisms are related, since plans are conducted before the actual placement begins. For example, an activation plan is made always when a rehabilitative work placement is considered, but does not always lead to an actual placement. Although the law sets boundaries on how often plans have to be conducted, there is still room the employment offices (and here, municipalities) to change the frequency of conducting plans if they wish to do so.

4.3.1 Plans conducted

We first look at the number of plans conducted by the employment office together with the job seeker. Additionally, we look at whether the treatment affected the types of plans that are conducted. We consider this analysis as a sort of a first stage analysis of the reform. If something changes in the behavior of the employment offices, it is likely to show as a change in the number or type of plans. For example, more plans would mean that the offices either contacted job seekers more or were otherwise more efficient.

Table 6 presents estimation results for the number of plans. We estimate that all plans were reduced by approximately 0.2 per year compared to the control group mean of 1.5 in the treatment year, a drop of 13%. A decrease in the number plans could stem from adjustment issues to the reform or it could be because decentralization caused these plans to be conducted less frequently. As mentioned in the second chapter, there are some requirements set by the law regarding these plans, but there is nevertheless some room for the office to decide how often plans are made. This is especially the case with activation plans, which have to be updated every 3-24 months. Furthermore, we see that decentralized offices favored different types of plans compared to centralized offices: while decentralization increased activation plans, it decreased employment plans. This is consistent with cost-shifting behavior since an activation plan must be made when a job seeker is directed to a rehabilitative work program.

Treatment effect on employment plans is also negative and significant in all specifications. The increase in activation plans is significant in all specifications. The effect on activation plans is not visible before 2018, as can be seen from a yearly treatment Figure 3. The magnitudes of the effects on plans are quite sizable when compared to the control group mean ─ a near doubling in activation plans and a drop of around one third in employment plans. This demonstrates that decentralization has meaningful effect on public employment services. We have not included the effects on integration plans. There is no effect, since we have only included individuals who have lived in Finland every year during the observation period and are consequently only obliged to make an integration plan under rare circumstances. Integration plans are, nevertheless, included in the number of all plans per year.

Panel A. Activation plans *Panel B. Employment plans*

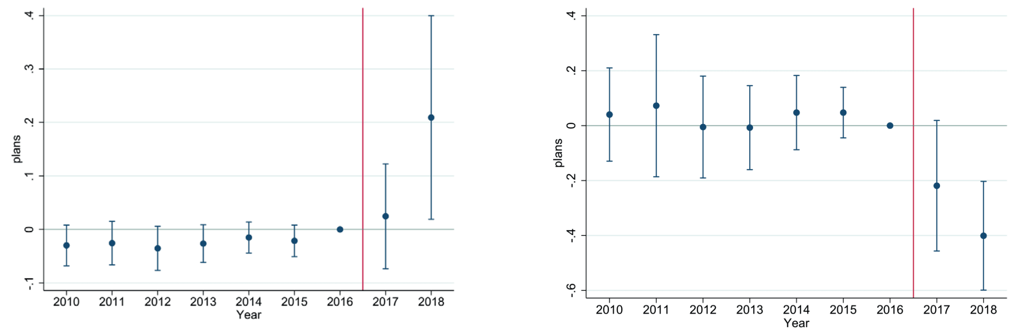

Figure 3: Activation and employment plans

Notes. Figure shows yearly treatment effects. In Panel A, the outcome variable is the number of activation plans conducted during the year. In Panel B, the outcome variable is the number of employment plans conducted during the year. 2016 is the reference period, treatment begins in August 2017. Treatment group is the eligible individuals. Standard errors are clustered by municipality.

Table 6: Plans conducted by the office

	(1) Activation plans	(2) Employment plans	(3) All plans
Treatment effect	0.231** (0.0968)	-0.429*** (0.101)	-0.198** (0.0792)
Treatment group mean, 2016	0.124	0.281	0.404
Control group mean, 2018	0.279	1.222	1.500
N	464784	464784	464784
No. of individuals	58098	58098	58098
Individual FE	yes	yes	yes
Year FE	yes	yes	yes

Notes. Table presents difference-in-differences results in a matched sample created with one-to-one propensity score matching. Standard errors clustered by municipality are shown in parentheses. Significance levels: * p<0.1 ** p< 0.05, *** p< 0.01. Pre-treatment outcome variables are used in matching in addition to individual characteristics such as age, gender, and length of unemployment when the treatment begins. All matching variables as well as their balance before and after matching can be found in Appendix table F1. Pre-treatment period includes years 2010–2016 and post-period includes year 2018. Observations from 2017 are not included. All plans includes not only activation and employment plans, but also integration plans. Since individuals are unemployed when the program starts in 7/2017, the number of plans increases substantially (but different amounts) in both groups in the post period, which explains why the negative effect on employment plans is bigger than the outcome mean in 2016.

4.3.2 ALMP placement strategies

The effectiveness of employment service decentralization depends crucially on what kinds of services and placements the decentralized offices offer to job seekers. ALMP placements are an important channel through which the potential effects of decentralization can take place. This is because there are big differences in effectiveness between different types of ALMPs: e.g., employing job seekers in the public sector jobs have been shown to be less effective in terms of having employment and displacement effects (see e.g., Kluve 2010). A potential cost of decentralization of employment services is, that it may encourage specifically the use of less effective ALMPs, if those are better for municipal finances (Mergele & Weber 2020).

There is support for this hypothesis from previous studies examining the effectiveness of PES decentralization. Lundin & Skedinger (2006) find that increasing municipalities' power in ALMP decisions made placements in ALMPs organized by municipalities more likely. Similarly, Mergele & Weber (2020) found that decentralization increased participation in public employment schemes. In Finland, municipalities organize rehabilitative work programs, where PES offices direct job seekers who need rehabilitation. During the decentralization reform, the treated municipalities could, however, decide themselves who was fit to participate in these programs.

Panel A of Figure 4 shows a significant increase of about 0.3 activation months in 2018, the first full year of the temporary reform. Panel B of the same figure illustrates that increase comes from an increase in rehabilitative work placements. Table 7 presents DiD estimates with one post-period (2018) and one pre-period (2006-2016). These results show sizable point estimates for all ALMPs and rehabilitative work, although the standard errors are bigger than in the yearly treatment effect estimation. For other ALMP types, point estimates are negative or very small. This could be interpreted such that municipalities changed the focus from other ALMPs to those organized by the municipality (rehabilitative work programs). This is consistent with the fact that we also found a positive effect on activation plans and on rehabilitative work in 2018. The temporary reform started in August 2017, but in all of these outcomes the effect begins consistently in 2018. This should be the case, because activation plans have to be conducted when an individual is directed to rehabilitative work, and when the individuals are in rehabilitative work, they are no longer registered as unemployed.

Panel A. All ALMPs *Panel B. Rehabilitative work*

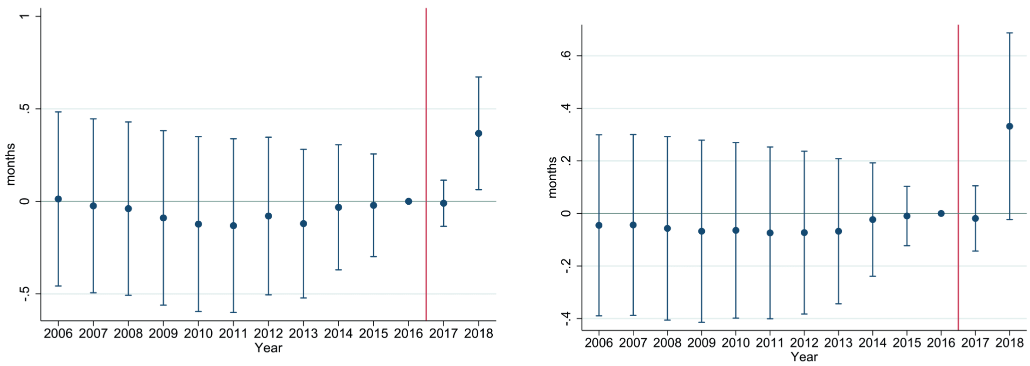

Figure 4: Months in all ALMPs and months in rehabilitative work

Notes. Figure shows yearly treatment effects. In Panel A, the outcome variable is the number of months in ALMPs per individual per year. In Panel B, the outcome variable is the number of months in rehabilitative work per individual per year. 2016 is the reference period, treatment begins in August 2017. Treatment group is the eligible individuals. Standard errors are clustered by municipality.

Table 7: Months in ALMPs

Panel A.	(1) All ALMPs	(2) Rehabilitative work	(3) Wage subsidies	(4) Wage subsidies, municipal sector
Treatment effect	0.426	0.341	0.0577	0.0458
	(0.325)	(0.301)	(0.0368)	(0.0402)
Treatment group mean, 2016	1.370	0.667	0.115	0.042
Control group mean, 2018	1.594	1.012	0.517	0.200
N	697176	697176	697176	697176
No. of individuals	58098	58098	58098	58098
Individual FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes

Panel B.	(5) Months in studying with unemployment benefit	(6) Months in coaching	(7) Months in labor force training	(8) Months in work trials
Treatment effect	0.0113	-0.0161	-0.0104	0.00310
	(0.0537)	(0.0107)	(0.0276)	(0.0147)
Treatment group mean, 2016	0.302	0.014	0.104	0.168
Control group mean, 2018	0.541	0.022	0.184	0.210
N	697176	697176	697176	464784
No. of individuals	58098	58098	58098	58098
Individual FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes

Notes. Table presents difference-in-differences results in a matched sample created with one-to-one propensity score matching. Standard errors clustered by municipality in parentheses. Significance levels: * p<0.1 ** p< 0.05, *** p< 0.01. Pre-treatment outcome variables are used in matching in addition to individual characteristics such as age, gender, and length of unemployment when the treatment begins. All matching variables as well as their balance before and after matching can be found in Appendix table F1. Pre-treatment period includes years 2006–2016 for outcomes (1)–(7), and years 2010–2016 for outcome (8). Post-period includes year 2018. Observations from 2017 are not included.

We found that ALMP participation increased 0.4 months per individual during 2018, albeit the effect was not significant due to lack of power. The point estimate, although insignificant, is quite sizable since the mean number of months in activation for our control group was 1.6 in 2018, indicating a one quarter increase in the number of ALMP months per year. The size of the point estimate on rehabilitative work is even larger, indicating a one third increase in rehabilitative work participation, although the estimate is insignificant due to lack of power. At the same time, we also found a decrease of 5 percentage points (17 %) on the probability of having more than 300 days in registered unemployment per year.

Our finding of an increase of ALMP months of 0.4 months per individual means 11,619 months in total in the treated area. If we assumed that the estimated increase was fully targeted at the long-term unemployed and these individuals were moved to ALMP for the full year, this would then mean 968 individuals more were moved to activation, representing 10 % of the number of the long-term unemployed (9,353) in the control group in 2018. This is 59 % of the decrease (17 %) we observed in long-term unemployment. By the same logic, if we assumed individuals were moved to activation for 6 months, the increase in ALMPs would explain all of the decrease in long-term unemployed. Nevertheless, this calculation is very sensitive to assumptions regarding how long the new ALMP participants spent in ALMPs.

4.4 Spillovers

Employment programs often affect non-participants through spillover effects (see e.g. Crepon et al. 2013). Since not everyone in the treated municipalities were transferred to the municipality during the Finnish decentralization program, it is possible to investigate whether there were any effects on those who remained in the centralized system inside treated municipalities. This is done by conducting similar matching and difference-in-differences analyses as in our main specification, but using non-eligible individuals as the treatment group, and excluding the eligible individuals in treated municipalities from the analysis.

Even if there were such effects, it would not affect the credibility of our DID estimates, since we have excluded ineligible individuals in participating municipalities. If there were spillovers, it would, however, affect the interpretation of our triple difference estimates. Estimates of spillover effects are presented in Appendix C (Table C1).

We find a significant spillover effect in the non-target population in the participating municipalities for rehabilitative work participation, which decreased for those who remained in the centralized system inside participating municipalities at around half the rate at which it increased for the decentralized (target) population, suggesting that a partial decentralization in the presence of cost-shifting opportunities causes municipalities to shift resources. We also find that, the total number of plans conducted decreased at around two thirds the rate of decrease for the target group. In total, there is a clear drop in the number of plans conducted in the participating municipalities during the temporary reform with no observable effect on employment.

4.5 Robustness and validity

Sant'Anna & Zhao (2020)

Our main results are robust to using doubly robust difference-in-differences estimator proposed by Sant'Anna & Zhao (2020). The method by Sant'Anna & Zhao (2020) is an improved, more robust version of difference-in-differences combined with matching. We report DRDID results for our main outcome variables in Appendix Table B10. The point estimates the method gives are similar to the ones from our main specification, but standard errors are different, since the R package DRDID does not allow us to calculate cluster-robust standard errors. The method allows for two time periods; we conducted the analysis using 2016 as the pre-treatment period and 2018 as the post-period. Due to the large size of our data and limits in computational power available, we did not use pre-treatment outcomes as covariates when estimating the model. Instead, we used important background characteristics, and the length of unemployment at the beginning of the reform when calculating the propensity score.

Matching procedure

The results are robust to changing the matching algorithm. The results are qualitatively similar when using coarsened exact matching (CEM)[9] one-to-many propensity score. CEM weighted DiD estimates are presented in columns 4 and 5 in the Appendix Tables B1-B3. Compared to the main specification, the point estimates are also similar if no pre-treatment outcome variables are used in matching or if matching is conducted with replacement (see columns 1-3 in Appendix Tables B1-B3). We have also run other robustness checks, which are not reported in this paper: for example, the results are robust to changing matching period from 3 to 6 years, i.e. using 6 years of pre-treatment outcomes in matching. Similarly, results hold if some variables are dropped from matching. Some of the results hold if no matching adjustment is performed at all, i.e., if we just compare the eligible individuals to other unemployed people. The demographic differences between treatment and control groups are, however, very large if no matching is conducted, since the eligible groups were very different from the average Finnish job seeker. If no matching was conducted, we would have pre-trends in some of the variables.

Standard errors

Standard errors are clustered at municipality level in all regressions. This is because it is reasonable to expect the observations from the same municipality to be correlated. Unfortunately, we have low power, since the SEs clustered at the municipal level are quite sizable in our case. Two-way clustering by municipality and year is not used in the main results, since the number of years (13) is too small to use it as a clustering variable: we would have very few

[9] CEM requires us to use fewer variables in matching, since it aims to find control individuals who have exactly the same covariate values. If we used all the same variables that we use in PSM, CEM would not be able to find matches for most of the individuals. Especially, if municipal-level variables (e.g. municipal unemployment rate, population) are added, CEM is not able to find matches.

degrees of freedom in that case (see e.g. Cameron et al. 2011). The results are, however, robust to two-way clustering (see Appendix tables B4-B6 for these results): in fact, some of our results become much more significant when two-way clustering by municipality and year is used. For example, effects on rehabilitative work and ALMP months are significant at five percent and one percent level, respectively, if two-way clustering is used.

Placebo regressions

We assess the robustness of our results by running placebo regressions, where a placebo treatment is set at 2015, two years prior to the actual treatment. A set of placebo regressions might uncover hidden weaknesses in our research setting and the matching procedure. Placebo results in Appendix Table C2 show that there are no placebo effects except for wage subsidies (1 of 17 outcomes).

5. Discussion

Many countries have recently decentralized public employment services (see e.g., Mosley 2012). One of the aims of the Finnish temporary reform to decentralize public employment services in 23 municipalities was to support the employment, job creation, entrepreneurship and growth in existing businesses. We find that the aims regarding employment were not achieved. The insignificant employment is the sum of least two possible causes. On the one hand, the program in Finland gave a set of tools to shift costs to the central government, incentivizing behavior that would not only focus on employment, but also on these cost-shifting behaviors. Such multi-objective optimization might hamper employment outcomes. On the other hand, local governments can be expected to have local information on the job market and the preferences of their constituencies, therefore allowing them to more efficiently place job seekers. The cost-shifting behavior and the information effect are likely to push employment in opposing directions. We cannot separate these effects empirically, but a setting with no possibility for cost-shifting would have redirected the focus fully on the preference of the local government, which is likely to be higher employment and might consequently have yielded better employment outcomes.

Another aim of the reform was to decrease the costs of unemployment to the public sector as a whole. We assume this rather vague aim includes the costs of ALMPs and PES as a whole. When looking at the public sector as a whole, we ignore cost-shifting. We focus here on the two ALMP types that appeared to show economically, albeit not statistically, significant changes: rehabilitative work and wage subsidies. For rehabilitative work, we estimate a point estimate of an increase of 0.34 months (not statistically significant) per year per individual. For wage subsidies the point estimate is an increase of 0.06 months per year per individual (again, not statistically significant). Using earlier calculations (Alasalmi et al. 2019) of the costs of ALMPs, in the absence of employment effects, we can make some rough estimates of the total cost of the change in PES behavior on total costs. The cost estimates end in 2014, we use a five-year mean for 2010 to 2014 for a rough figure. First, wage subsidies cost around 9,000 euros and 11,000 euros on average per year per individual in the municipal and private sectors, respectively. With an estimated effect of 0.06 months, using the average over the municipal and private sector numbers, the cost in the treatment municipalities is 50 euros per treated individual. When we multiply that by the number of treated individuals (29,049), the total cost for the experiment amounts to approximately 1.4 million euros. If the reform was extended to all 202,000 unemployed individuals across the country in 2018, it would have cost around 10 million euros annually. If the sizable but statistically insignificant effect on rehabilitative work ALMP is included, the costs estimate for nationwide implementation increases to 67 million euros. We do not have an estimate for implementation costs, but it has been reported that the implementation costs in Pirkanmaa area alone exceeded 10 million euros during the temporary decentralization program.

Since the reform changed the composition of ALMPs and reduced long-term unemployment, it is plausible that it had effects on benefits and transfers in general. In Appendix Table D2, we look at total transfers paid and received. Both figures are very close to zero and insignificant. We conclude that the total cost of the reform for the public sector was not significantly impacted by changes in transfers paid and received. Breaking down social benefits by type in Appendix Table D3, we observe that the estimates for income support and sickness benefit are positive, yet insignificant. The estimates for unemployment benefit and housing allowance are negative and insignificant. No long-lasting effects on benefit sanctions are observed either (see Appendix Figure D1 and Appendix Table D1).

Concerning ALMPs, we find some evidence suggesting that the local governments, in addition to increasing placements in ALMPs, choose a somewhat different policy mix to that of the central government in the presence of incentives. We find that the local governments favored wage subsidies and rehabilitative work programs over other ALMP types, albeit due to lack of power we cannot rule out these changes being zero in our main specification. However, we cannot distinguish whether this results from the incentives or preferences of the local governments. From what we observe, this changed ALMP mix does not increase employment months or earnings, suggesting that the ALMPs preferred by municipalities are not better than those favored by the centralized employment offices in this context. We also find a significant drop in the non-target population in the participating municipalities on rehabilitative work ALMP coupled with an insignificant drop in all ALMPs, yet we do not observe any change in employment. The opposing changes in rehabilitative work months in the target and non-target populations in the participating municipalities together with no observed employment effects challenge the effectiveness, at the margin, of this type of ALMP.

These findings are consistent with earlier findings in the massive ALMP literature (e.g. Greenberg 2003; Kluve, 2010; Card et al. 2010; Card et al. 2018;

Crepon & van den Berg 2016), which has found that the employment effects of ALMPs are often very small, especially in the short run, but that average impacts become more positive on average in two to three years after the programs. Naturally, different programs have heterogenous effects by timing and participant groups, but overall programs that focus on human capital accumulation (education and training) have shown to result the most visible positive effect in the employment over time. The effectiveness of public sector employment programs or wage subsidies are often found to be very low. As we are looking at a short-term effect, and the increased ALMPs were not in the field of education or training, we are not expecting to see an increase in employment, if the local government information advantage is ignored. As we are looking at a short-term effect, and the increased ALMPs were not in the field of education or training, we are not expecting to see an increase in employment, if the local government information advantage is ignored.

The presence of spillover effects in our setting do not mar the interpretation of our main estimates. We do not find any effects or spillover effects on employment. The spillover effects we find in public employment behavior simply show that there is some transfer of resources from the non-target population to the target population in the participating municipalities. However, spillover effects do largely invalidate the triple difference strategy in this setting, although the results are also reported here.

Comparing our results to similar studies by Mergele and Weber (2020) and Lundin and Skedinger (2006), similarities and some differences arise. Mergele and Weber study a permanent PES decentralization reform in Germany and find a negative effect on job-finding rate. We find no effect on aggregate employment. Unfortunately, we do not observe the job-finding rate and thus cannot be sure whether our differing results stems from a different measure or an actual difference in outcomes. However, both results support the finding that local governments are not able to exploit their local understanding to promote employment better than the centralized government. We are able to complete the picture of how local governments behavior is consistent through-and-through with an aim of cost-shifting, including self-proclaimed aims. This is done through targeting the long-term unemployed to reduce penalties that the municipalities have to pay for every long-term unemployed individual.

6. Conclusions

Employment services have been decentralized in many countries but the evidence on its effects has been very limited. This study has complemented the literature by providing further evidence on how decentralization affects labor market outcomes of job seekers and the behavior of employment offices. Our results indicate no effects on employment or earnings and support the cost-shifting hypothesis made in the earlier literature. Our results also shed light on how decentralization affected service provision more broadly. We find that municipalities preferred different mix of ALMPs and conducted different types of plans with job seekers.

Our evidence shows that municipalities were able to reduce registered long-term unemployment, which is consistent with cost-shifting, since municipalities have to pay penalty payments for each long-term unemployed person who fulfills certain criteria. The decrease in long-term unemployment is for a large part explained by increased in ALMP participation, although the estimate was not significant in most of our specifications due to lack of power. Furthermore, job seekers were placed in ALMPs that may provide public goods in the municipality, and hence are beneficial to municipal finances. This finding is consistent with earlier research on the topic. We further contribute to the cost-shifting discussion by providing approximative calculations on the size of cost-shifting that happened during the Finnish temporary reform through reductions in penalty payments municipalities have to pay, and also calculate what the cost-shifting would amount to, if the reform was implemented nationwide.

We were also able to look at the cost-shifting behavior as a process: first, we observed that municipalities strongly increased activation plans at the expense of other plans, while the aggregate number of plans was negatively affected. The rise in activation plans, which are conducted when a rehabilitative work placement is considered, was very dramatic, as was the fall in employment plans. Thus, it seems that municipalities chose to target the planning efforts on those job seekers who belong or are about to belong to the penalty list. Second, we observed the increase in activation, specifically in rehabilitative work. Third, we observed a decrease in the probability of long-term unemployment, indicating decreased penalty payments.

Since we find null effects in employment and earnings, we do not find any clear benefits resulting from employment service decentralization. Thus, based on this study, decentralization of PES should not be expected to increase employment, although more evidence is still needed as the literature is still very scarce, and institutional details and incentives likely influence how this policy affects employment and PES office behavior. If decentralization reform in employment services is implemented, this study suggests that the incentives of municipalities have to be designed carefully and the cost-shifting possibilities should be minimized.

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Appendix A. Trends in key variables

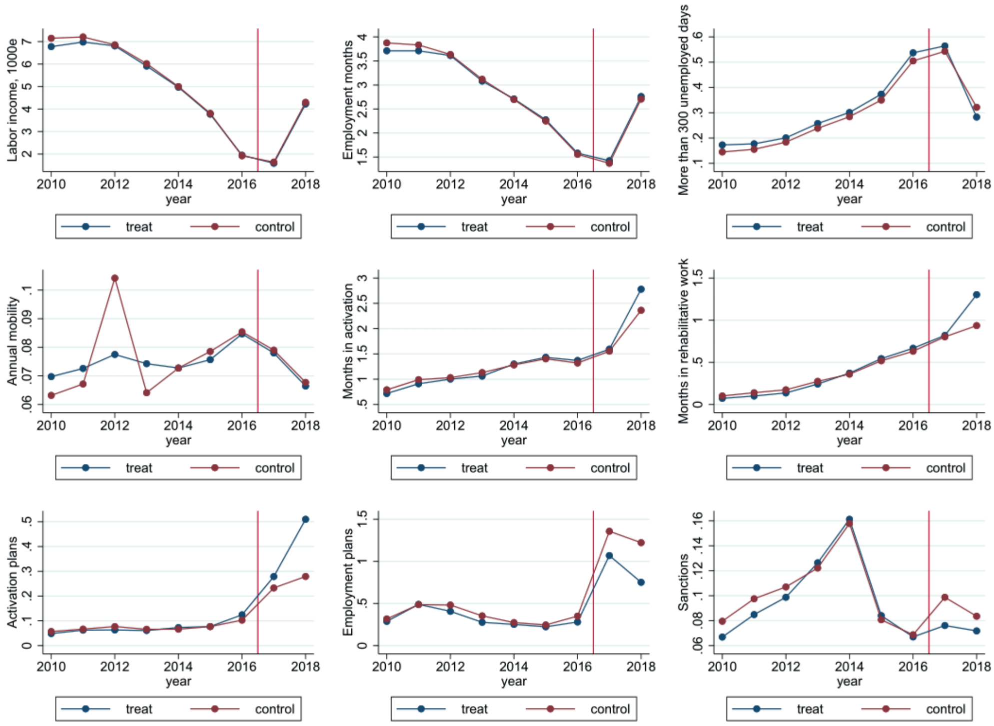


Figure A1. Trends in key variables

Notes. Figure depicts the levels of our key outcomes in matched treatment and control groups (main matching specification). Matching variables and their balance before and after marching can be found in Appendix Table F1.

Appendix B. Robustness

Alternative matching adjustments

In our main results, we used matched treatment and control groups created using one-to-one propensity score matching. In this section, we report DiD results on our main outcomes using different matching specifications, such as PSM without replacement, PSM excluding pre-treatment outcomes, and coarsened exact matching both with and without pretreatment outcomes in matching.

Table B1. Labour market outcomes, alternative matching procedures

	(1)	(2)	(3)	(4)	(5)
	DiD+PSM 1:1, excluding pre-treatment outcomes	DiD+PSM with replacement including pre-treatment outcomes	DiD+PSM with replacement excluding pre-treatment outcomes	DiD+CEM 1:m, excluding pre-treatment outcomes	DiD+CEM 1:m, including pre-treatment outcomes
Outcome: Annual labor income					
Treatment effect	403.8	72.54	449.4	344.7	621.1
	(620.5)	(625.7)	(610.6)	(624.1)	(1593.7)
Outcome: Employment months					
Treatment effect	0.216	0.126	0.251	0.0951	0.433
	(0.238)	(0.243)	(0.231)	(0.230)	(0.671)
Outcome: More than 300 days in registered unemployment					
Treatment effect	-0.0547**	-0.0503	-0.0528**	-0.0435*	-0.0326**
	(0.0250)	(0.0250)	(0.0295)	(0.0263)	(0.0143)
Outcome: More than 300 days in registered unemployment and unemployed at the end of the year					
Treatment effect	-0.0552**	-0.0520	-0.0531**	-0.0410	-0.0341**
	(0.0240)	(0.0240)	(0.0236)	(0.0251)	(0.0154)
Outcome:Annual mobility					
Treatment effect	0.00444	0.000933	0.00471	-0.00683	-0.0120
	(0.0287)	(0.0283)	(0.0286)	(0.0262)	(0.0241)
N	697200	610248	608688	2158416	131436
No of individuals	58100	50854	50724	179868	10953
Individual FE	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes

Notes. Standard errors clustered by municipality in parentheses. Significance levels: * p<0.1 ** p< 0.05, *** p< 0.01. Column (5) has a smaller number of observations because CEM discards a large number of individuals when pre-treatment outcomes are included in matching.

Table B2. Plans conducted by the office, alternative matching procedures

	(1)	(2)	(3)	(4)	(5)
	DiD+PSM 1:1, excluding pre-treatment outcomes	DiD+PSM with replacement including pre-treatment outcomes	DiD+PSM with replacement excluding pre-treatment outcomes	DiD+CEM 1:m, excluding pre-treatment outcomes	DiD+CEM 1:m, including pre-treatment outcomes
Outcome: All plans					
Treatment effect	-0.174**	-0.171**	-0.171**	-0.228**	-0.223***
	(0.0767)	(0.0757)	(0.0757)	(0.101)	(0.0799)
Outcome: Employment plans					
Treatment effect	-0.391***	-0.391***	-0.391***	-0.483***	-0.362***
	(0.100)	(0.0991)	(0.0991)	(0.134)	(0.0932)
Outcome: Activation plans					
Treatment effect	0.217**	0.219**	0.219**	0.256***	0.139**
	(0.0971)	(0.0964)	(0.0964)	(0.0952)	(0.0688)
N	464800	405792	405792	2158416	131436
No of individuals	58100	50724	50724	179868	10953
Individual FE	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes

Notes. Standard errors clustered by municipality in parentheses. Significance levels: * p<0.1 ** p< 0.05, *** p< 0.01. Column (5) has a smaller number of observations because CEM discards a large number of individuals when pre-treatment outcomes are included in matching.

Table B3. Active labor market policies, alternative matching procedures

	(1)	(2)	(3)	(4)	(5)
	DiD+PSM 1:1, excluding pre-treatment outcomes	DiD+PSM with replacement including pre-treatment outcomes	DiD+PSM with replacement excluding pre-treatment outcomes	DiD+CEM 1:m, excluding pre-treatment outcomes	DiD+CEM 1:m, including pre-treatment outcomes
Outcome: All ALMP months					
Treatment effect	0.310	0.420	0.241	0.322	0.338
	(0.343)	(0.342)	(0.324)	(0.319)	(0.241)
Outcome: Wage subsidies, months					
Treatment effect	0.0444	0.0436	0.0367	0.00793	0.0727
	(0.0383)	(0.0411)	(0.0391)	(0.0321)	(0.0658)
Outcome: Wage subsidies (municipality), months					
Treatment effect	0.0293	0.0309	0.0215	0.00590	0.0265
	(0.0396)	(0.0414)	(0.0408)	(0.0361)	(0.0470)
Outcome: Months in rehabilitative work					
Treatment effect	0.319	0.392	0.325	0.362	0.279
	(0.306)	(0.297)	(0.304)	(0.289)	(0.188)
Outcome: Months in studying with unemployment benefit					
Treatment effect	-0.0254	-0.00251	-0.0916**	-0.0236	0.0123
	(0.0562)	(0.0557)	(0.0442)	(0.0320)	(0.0290)
Outcome: Coaching					
Treatment effect	-0.0188*	-0.0156	-0.0205*	-0.0219*	-0.0121
	(0.0108)	(0.0105)	(0.0108)	(0.0113)	(0.0135)
Outcome: Training					
Treatment effect	-0.0104	-0.00324	-0.0115	-0.00829	0.0301
	(0.0276)	(0.0169)	(0.0315)	(0.0154)	(0.0244)
Outcome: Work trials					
Treatment effect	-0.000634	0.00454	-0.00736	-0.00405	-0.0443*
	(0.0153)	(0.0149)	(0.0202)	(0.0154)	(0.0257)
N	697200	610248	608688	2158416	131436
No of individuals	58100	50854	50724	179868	10953
Individual FE	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes

Notes. Standard errors clustered by municipality in parentheses. Significance levels: * p<0.1 ** p< 0.05, *** p< 0.01. Column (5) has a smaller number of observations because CEM discards a large number of individuals when pre-treatment outcomes are included in matching.

Main specification with twoway-clustered standard errors (clustered by municipality and year)

Table B4. Labor market outcomes, twoway clustered SEs

	(1) Labour income	(2) Employment months	(3) >300 days in registered unemploymen t per year	(4) >300 days in registered unemployment and unemployed at the end of a year	(5) Mobility
Treatment effect	51.25	0.0905	-0.0542***	-0.0558***	0.00128
	(131.0)	(0.0606)	(0.0107)	(0.00857)	(0.0129)
Treatment group mean, 2016	1946.6	1.581	0.537	0.506	0.085
Control group mean, 2018	4301.0	2.707	0.322	0.312	0.062
N	697176	697176	697176	697176	697176
No. of individuals	58098	58098	58098	58098	58098
Individual FE	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes

Notes. Standard errors clustered by municipality and year in parentheses. Number of clusters is only 12 since we have 12 years in the estimation sample. The specification is otherwise the same as in our main tables.

Table B5. Plans conducted by the office, twoway clustered SEs

	(1) Activation plans	(2) Employment plans	(3) All plans
Treatment effect	0.231***	-0.429***	-0.198***
	(0.0516)	(0.0485)	(0.0354)
Treatment group mean, 2016	0.124	0.281	0.404
Control group mean, 2018	0.279	1.222	1.500
N	464784	464784	464784
No. of individuals	58098	58098	58098
Individual FE	yes	yes	yes
Year FE	yes	yes	yes

Notes. Standard errors clustered by municipality and year in parentheses. Number of clusters is only 8 since we have 8 years in the estimation sample. The specification is otherwise the same as in our main tables.

Table B6. Months in ALMPs, twoway clustered SEs

Panel A.

	(1) All ALMPs	(2) Rehabilitative work	(3) Wage subsidies	(4) Wage subsidies in municipal sector
Treatment effect	0.426***	0.380**	0.0577***	0.0409**
	(0.136)	(0.126)	(0.0159)	(0.0160)
Treatment group mean, 2016	1.370	0.667	0.115	0.042
Control group mean, 2018	1.594	1.012	0.517	0.200
N	697176	697176	697176	464784
No. of individuals	58098	58098	58098	58098
Individual FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes

Panel B.

	(5) Months in studying with unemployment benefit	(6) Months in coaching	(7) Months in labor force training	(8) Months in work trials
Treatment effect	0.0113	-0.0161**	-0.0104	0.00310
	(0.0137)	(0.00525)	(0.00814)	(0.00540)
Treatment group mean, 2016	0.302	0.014	0.104	0.168
Control group mean, 2018	0.541	0.022	0.184	0.210
N	697176	697176	697176	464784
No. of individuals	58098	58098	58098	58098
Individual FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes

Notes. Standard errors clustered by municipality and year in parentheses. Number of clusters is only 12 since we have 12 years in the estimation sample. The specification is otherwise the same as in our main tables.

Triple difference (DDD) estimates

Table B7. presents triple difference estimation results. No matching adjustments have been conducted. Columns (1)-(3) present estimates areas with different eligibility criteria, and table (4) presents a weighted DDD estimate calculated as described in Section 3.2.

Table B7. Labour market outcomes (DDD)

	(1)	(2)	(3)	(4)
	DDD, Pirkanmaa area (10 municipalities)	DDD, Varsinais-Suomi (4 municipalities)	DDD, Pohjois-Savo & Lappi (3+5 municipalities)	DDD, weighted estimate
Annual labor income	-213.8	-12.70	204.7	5.544
Treatment effect	(766.6)	(211.0)	(504.3)	(188.6)
Employement months				
Treatment effect	-0.083	0.042	-0.295**	-0.072
	(0.204)	(0.099)	(0.140)	(0.075)
More than 300 unemployed days				
Treatment effect	-0.068***	0.0005	0.028**	-0.003
	(0.013)	(0.0048)	(0.011)	(0.004)
More than 300 unemployed days and unemployed at the end of the year				
Treatment effect	-0.064***	0.002	0.031***	0.001
	(0.014)	(0.004)	(0.009)	(0.004)
Annual mobility				
Treatment effect	-0.011	-0.001	-0.005	-0.004
	(0.008)	(0.006)	(0.006)	(0.004)
N	2877540	2751420	3268128	8897088
No of individuals in the sample	239795	229285	272344	741 424
No of treated individuals	17 728	6 789	4 430	28 947
Individual FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes

Notes. Columns (1)–(3) present triple difference estimation results from three different eligibility areas, each of which have a different amount of municipalities and treated individuals. Column (4) presents weighted DDD estimates, calculated by weighting the individual estimates by the inverse of their squared standard error.

Table B8. Plans conducted by the office (DDD)

	(1)	(2)	(3)	(4)
	DDD, Pirkanmaa area (10 municipalities)	DDD, Varsinais-Suomi (4 municipalities)	DDD, Pohjois-Savo & Lappi (3+5 municipalities)	DDD, weighted estimate
All plans				
Treatment effect	-0.237	-0.122***	-0.158	-0.131***
	(0.157)	(0.039)	(0.126)	(0.036)
Employment plans	-0.666***	-0.147***	-0.191**	-0.240***
Treatment effect	(0.075)	(0.037)	(0.087)	(0.031)
Activation plans	0.430***	0.026	0.034	0.039*
Treatment effect	(0.129)	(0.024)	(0.061)	(0.022)
N	2877540	2751420	3268128	8897088
No of individuals in the sample	239795	229285	272344	741 424
No of treated individuals	17 728	6 789	4 430	28 947
Individual FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes

Notes. Columns (1)–(3) present triple difference estimation results from three different eligibility areas, each of which have a different amount of municipalities and treated individuals. Column (4) presents weighted DDD estimates, calculated by weighting the individual estimates by the inverse of their squared standard error.

Table B9. Active labor market policies (DDD)

	(1) DDD, Pirkanmaa area (10 municipalities)	(2) DDD, Varsinais-Suomi area (4 municipalities)	(3) DDD, Pohjois-Savo & Lappi areas (3+5 municipalities)	(4) DDD, weighted estimate
All ALMP months				
Treatment effect	0.675***	0.032	-0.163	0.020
	(0.245)	(0.051)	(0.108)	(0.045)
Wage subsidies, months				
Treatment effect	-0.119***	0.042	-0.009	-0.030
	(0.029)	(0.028)	(0.053)	(0.019)
Wage subsidies (municipality), months				
Treatment effect	-0.079***	0.126***	-0.002	0.018
	(0.024)	(0.025)	(0.037)	(0.016)
Months in rehabilitative work				
Treatment effect	0.821***	0.039	-0.076	0.023
	(0.240)	(0.043)	(0.066)	(0.036)
Months in studying with unemployment benefit				
Treatment effect	0.070**	0.078	-0.067**	0.007
	(0.031)	(0.052)	(0.028)	(0.019)
Coaching				
Treatment effect	-0.017***	-0.008*	-0.012	-0.013***
	(0.004)	(0.0046)	(0.012)	(0.003)
Training				
Treatment effect	-0.053**	-0.042***	-0.041*	-0.044***
	(0.026)	(0.013)	(0.023)	(0.010)
Work trials				
Treatment effect	0.001	-0.061***	-0.010	-0.008
	(0.012)	0.012	(0.012)	(0.008)
N	2877540	2751420	3268128	8897088
No of individuals in the sample	239795	229285	272344	741 424
No of treated individuals	17 728	6 789	4 430	28 947
Individual FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes

Notes. Columns (1)–(3) present triple difference estimation results from three different eligibility areas, each of which have a different amount of municipalities and treated individuals. Column (4) presents weighted DDD estimates, calculated by weighting the individual estimates by the inverse of their squared standard error.

Sant'Anna & Zhao (2020): Further improved locally efficient double robust DID

Table B10. presents DR DID estimation results for our most important outcome variables (employment months, long-term unemployment, and ALMP months).

Table B10. Estimates using Sant'Anna & Zhao (2020)

	ATT	Standard error	t value	p-value
Outcome: *more than 300 days in registered unemployment*	-0.0619***	0.0039	-15.7	< 0.001
Outcome: *ALMP months*	0.3378***	0.0275	12.3	< 0.001
Outcome: *Activation plans*	0.2521***	0.0064	39.6	< 0.001
Outcome: *Employment months*	0.0818***	0.0293	2.79	0.0052

Notes. Outcome regression estimation method is weighted least squares. Propensity score estimation method is inverse probability tilting. Standard analytical DR DID standard error.

Appendix C. Spillover and placebo effects

Table C1 presents DiD estimates for ineligible individuals in treated municipalities. We find no spillovers except for rehabilitative work participation, which decreases for ineligible individuals in treated municipalities during the temporary reform. This does not affect the credibility of our main DiD estimates, since ineligible individuals in treated municipalities are not included in DiD estimations. However, this spillover effect on rehabilitative work does affect the interpretation of the DDD estimate on rehabilitative work.

Table C2 presents placebo DiD results. We have conducted similar matching procedure as in our main results, but for individuals who were unemployed in July 2015 (actual treatment started in July 2017). We find no placebo effects except for wage subsidies (1 of 17 outcomes).

Table C1. Spillover effects

	(1)	(2)
	Using pretreatment outcomes in matching	No pretreatment outcomes in matching
Annual income from employment		
Treatment effect	167.9 (1160.6)	401.1 (1160.2)
Number of months employed		
Treatment effect	0.100 (0.360)	0.154 (0.353)
Probability of more than 300 days in registered unemployment		
Treatment effect	-0.0124 (0.0114)	-0.0119 (0.0119)
Mobility		
Treatment effect	0.00701 (0.0222)	0.00528 (0.0219)
Months in rehabilitative work		
Treatment effect	-0.169** (0.0810)	-0.164** (0.0795)
Months in studying		
Treatment effect	0.0333 (0.0420)	0.00995 (0.0392)
Number of sanctions received		
Treatment effect	-0.00155 (0.00391)	0.00161 (0.00384)
Housing allowance		
Treatment effect	-47.46 (86.86)	-21.54 (85.83)
Income support		
Treatment effect	-45.98 (40.59)	-30.44 (41.38)
All plans conducted		
Treatment effect	-0.141** (0.0685)	-0.113* (0.0674)
Employment plans conducted		
Treatment effect	-0.106 (0.0767)	-0.0787 (0.0764)
Activation plans conducted		
Treatment effect	-0.0356* (0.0196)	-0.0346* (0.0200)
Months in coaching		
Treatment effect	-0.00313 (0.00963)	-0.00960 (0.00974)
Months in training		
Treatment effect	0.0232 (0.0204)	0.0289 (0.0231)
Months in work trials		
Treatment effect	0.0108 (0.0229)	-0.00113 (0.0219)
Wage subsidies, months		
Treatment effect	0.049 (0.033)	0.0293 (0.0347)
All ALMP months		
Treatment effect	-0.0552 (0.112)	-0.0986 (0.111)
N	603000	603024
No of individuals	50250	50252
Individual FE	yes	yes
Year FE	yes	yes

Notes. Table reports the treatment effects on non-treated individuals in the treated municipalities. Matching is performed similarly to the analyses in the main text. Standard errors clustered by municipality in parentheses. Number of observations is different (smaller) for certain outcome variables (all plans, activation plans, employment plans, work trials) due to smaller number of years data available.

Table C2. Placebo difference-in-differences (placebo treatment year: 2015)

	With 2015		Without 2015	
	(1) PSM, including pretreatment outcomes in matching	(2) PSM, excluding pretreatment outcomes in matching	(3) PSM, including pretreatment outcomes in matching	(4) PSM, excluding pretreatment outcomes in matching
Income				
Treatment effect	-21.84 (835.1)	-74.21 (841.4)	-114.0 (752.8)	-113.8 (763.4)
Employment months				
Treatment effect	0.0534 (0.293)	0.0639 (0.295)	0.0436 (0.275)	0.0745 (0.278)
Probability of having more than 300 unemployment days per year				
Treatment effect	0.00859 (0.0212)	0.00851 (0.0215)	0.0153 (0.0145)	0.0175 (0.0150)
Rehabilitative work				
Treatment effect	0.0460 (0.154)	0.0250 (0.156)	0.0381 (0.155)	0.0140 (0.156)
Studying				
Treatment effect	-0.00413 (0.0343)	-0.00543 (0.0346)	-0.00957 (0.0463)	-0.00675 (0.0466)
Probability of moving to another municipality				
Treatment effect	0.0000995 (0.0185)	0.00333 (0.0186)	0.000835 (0.0324)	0.00336 (0.0324)
Number of sanctions received				
Treatment effect	0.00766 (0.00513)	0.00754 (0.00510)	0.00136 (0.00698)	0.00220 (0.00667)
Housing allowance				
Treatment effect	34.11 (75.31)	38.17 (77.02)	34.90 (76.73)	41.90 (78.13)
Income support				
Treatment effect	20.03 (47.89)	-13.04 (47.22)	13.77 (40.79)	-16.60 (41.08)
All plans				
Treatment effect	-0.127 (0.0831)	-0.0646 (0.0824)	-0.102 (0.0836)	-0.0407 (0.0824)
Employment plans				
Treatment effect	-0.0966 (0.0694)	-0.0384 (0.0689)	-0.0668 (0.0657)	-0.0115 (0.0644)
Activation plans				
Treatment effect	-0.0303 (0.0200)	-0.0262 (0.0199)	-0.0352 (0.0276)	-0.0290 (0.0272)
Coaching				
Treatment effect	-0.00487 (0.0178)	-0.00711 (0.0177)	-0.00719 (0.0180)	-0.0111 (0.0179)
Training				
Treatment effect	-0.00729 (0.0171)	-0.00893 (0.0166)	0.00782 (0.0197)	0.000999 (0.0207)
Work trials				
Treatment effect	-0.00650 (0.0221)	0.000134 (0.0229)	0.0112 (0.0309)	0.0174 (0.0313)
ALMP participation				
Treatment effect	-0.0117 (0.185)	-0.0317 (0.184)	-0.0493 (0.177)	-0.0775 (0.177)
Wage subsidies				
Treatment effect	-0.0364 (0.0441)	-0.0366 (0.0443)	-0.0912*** (0.034)	-0.0933*** (0.035)
N	687742	687742	625220	625220
No of individuals	62522	62522	62522	62522
Individual FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes

Notes. Standard errors in clustered by municipality in parentheses. Data is from period 2006 – 2016 for most outcome variables and from 2010 – 2016 for plans and work trials.

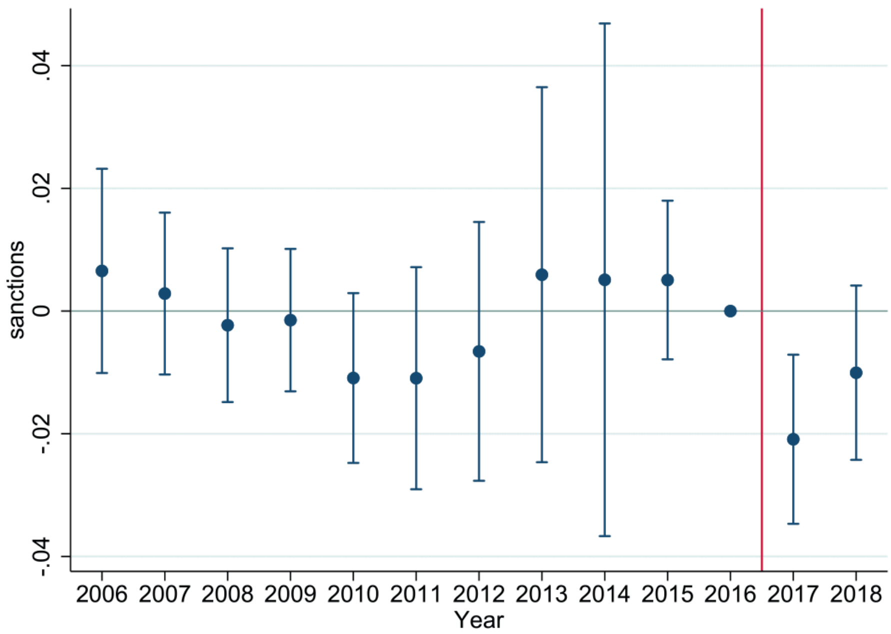
Appendix D: Additional outcomes

Benefit sanctions and social benefit use

Mergele & Weber (2020) hypothesized that decentralized employment offices could be less strict in monitoring job seekers due to their incentives, although they did not find any evidence supporting that hypothesis. Although municipalities were not directly responsible for sanctioning during the Finnish temporary reform, it could still be possible for them to affect sanctioning e.g. through changing how employment and activation plans are conducted or changing the types of ALMP programs available to job seekers.

We find that decentralization initially reduced the number of sanctions, despite the fact that sanctions were officially still determined at the central level during the temporary decentralization. There could be many reasons for this finding: for instance, it could be a mechanical effect resulting from different composition of active labor market policies offered to job seekers, or it could be due to administrative problems at the start of the temporary reform. If we believed that there was a reason for municipalities to reduce sanctioning, the results could also be interpreted as municipal employment offices being able to reduce sanctioning through changing their behavior even if they are not directly responsible for sanctioning. The policy relevance of this result, if interpreted in that way, is direct: the central government may not be able to easily mitigate any specific downside employment service decentralization may have.

Figure D1. New benefit sanction statements



Notes. Figures show yearly treatment effects. Treatment group is the eligible individuals. Treatment began in August 2017. Treatment group includes all eligible individuals. Standard errors are clustered by municipality. Matching period includes years 2014-2016. New sanctions are determined by sanction statements, and the sanctions can vary in length.

Table D1. Benefit sanctions

	(1) DiD (PSM 1:1 with pretreatment outcomes)	(2) DiD (PSM 1:1 without pretreat outcomes)	(3) weighted DDD
Outcome: Sanctions			
Treatment effect	-0.00944	-0.0129	-0.0179***
	(0.00811)	(0.00808)	(0.00363)
Treatment group mean, 2016	0.067	0.067	0.065
Control group mean, 2018	0.084	0.084	0.075
N	697176	697200	8897088
No. of individuals	58098	58100	741424
Individual FE	yes	yes	yes
Year FE	yes	yes	yes

Notes. Standard errors clustered by municipality in parentheses. Columns (1) and (2) present conditional difference-in-differences estimates. In column (1), pre-treatment outcome variables are used in matching, whereas in column (2) we use only individual characteristics such as age, gender, and length of unemployment when entering the trial. Column (3) presents triple difference estimates. Number of observations for the weighted DDD estimates (columns 3 and 6) is the sum of the number of observations of 3 separate DDD estimations for different area. Pre-treatment mean indicates the mean value of the outcome variable in 2016 for the treatment group.

Table D2. Transfers paid and received

	(1)	(2)	(3)
	DiD (PSM 1:1 with pretreatment outcomes)	DiD (PSM 1:1 without pretreatment outcomes)	weighted DDD
Panel A: All transfers paid (incl. taxes)			
Treatment effect	13.26	94.67	-19.61
	(145.7)	(141.9)	(66.80)
Treatment group mean, 2016	1937.4	1937.4	1962.48
Control group mean, 2018	2059.6	2059.6	2273.31
Panel B: All transfers received			
Treatment effect	-35.19	-11.94	-91.96
	(164.7)	(177.1)	(125.7)
Treatment group mean, 2016	11852.7	11852.7	12075.4
Control group mean, 2018	11139.2	11139.2	11411.0
N	697176	697200	8897088
No. of individuals	58098	58100	741424
Individual FE	yes	yes	yes
Year FE	yes	yes	yes

Notes. Standard errors clustered by municipality in parentheses. Columns (1) and (2) present conditional difference-in-differences estimates. In column (1), pre-treatment outcome variables are used in matching, whereas in column (2) we use only individual characteristics such as age, gender, and length of unemployment when entering the trial. Column (3) presents triple difference estimates. Number of observations for the weighted DDD estimates (columns 3 and 6) is the sum of the number of observations of 3 separate DDD estimations for different area.

Table D3. Social benefit types

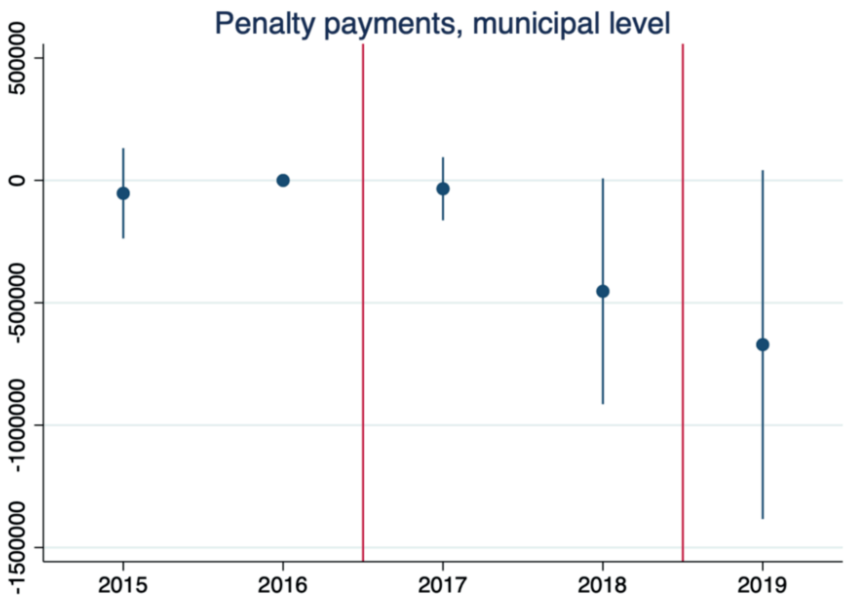
	(1) DiD (PSM 1:1 with pretreatment outcomes)	(2) DiD (PSM 1:1 without pretreatment outcomes)	(3) weighted DDD
Panel A: Housing allowance			
Treatment effect	-49.91	-25.77	-21.23
	(65.14)	(63.33)	(19.43)
Treatment group mean, 2016	1773.3	1773.3	1801.5
Control group mean, 2018	1849.7	1849.7	1720.9
Panel B: Income support			
Treatment effect	56.13	66.69	10.36
	(47.48)	(51.24)	(14.38)
Treatment group mean, 2016	1207.5	1207.5	1206.8
Control group mean, 2018	1182.4	1182.4	1142.3
Panel C: Unemployment benefits			
Treatment effect	-115.3	-207.9**	12.77
	(100.7)	(105.6)	(45.54)
Treatment group mean, 2016	7353.6	7353.6	7554.3
Control group mean, 2018	5727.0	5727.0	6765.0
Panel D: Sickness benefit			
Treatment effect	22.30	16.22	-14.47
	(17.34)	(16.18)	(14.04)
Treatment group mean, 2016	210.41	210.41	212.97
Control group mean, 2018	298.80	298.80	381.90
N	697176	697200	8897088
No. of individuals	58098	58100	741424
Individual FE	yes	yes	yes
Year FE	yes	yes	yes

Notes. Standard errors clustered by municipality in parentheses. Columns (1) and (2) present conditional difference-in-differences estimates. In column (1), pre-treatment outcome variables are used in matching, whereas in column (2) we use only individual characteristics such as age, gender, and length of unemployment when entering the trial. Column (3) presents triple difference estimates. Number of observations for the weighted DDD estimates (columns 3 and 6) is the sum of the number of observations of 3 separate DDD estimations for different area.

Appendix E: Penalty payments, municipal level

On municipality level, there is data available on penalty payments paid by municipalities while in individual level we could not directly observe belonging to the penalty list. Figure E1. presents municipal level difference-in-difference estimates. The municipal level results suggest that municipalities were able to shift on average 450 000 euros of costs to the central government through reductions in penalty payments (2018 estimate). Since there were 23 participating municipalities, this means 23*450000 = 10,4 million euros. This is bigger than the individual level calculations which suggested that the cost-shifting would amount to 6-7 million euros. It also seems that the penalty costs in 2019 stayed smaller for the treated municipalities, probably due to ALMP participants continuing participation in programs where they had been placed during the temporary reform.

Figure E1. Penalty payments



Notes. Municipal level difference-in-differences. All treated municipalities are included in the treatment group, and all untreated municipalities are included in the control group. Standard errors are clustered by panel id (municipality).

Appendix F: Matching tables and figures

Table F1. Balance table before and after PSM (main specification)

	Before matching			After matching		
	treated	control	p-value	treated	control	p-value
Length (days) of current employment code (unemployment/ALMP)	565.84	393.7	0.000	565.85	570.88	0.312
Length (days) of registered unemployment (0 if not in reg. unemployment, but in e.g. ALMP)	540.57	373.08	0.000	540.58	545.88	0.294
Has been unemployed over 12 months consecutively	0.54406	0.31736	0.000	0.54408	0.54067	0.410
Age	40.462	42.534	0.000	40.462	40.224	0.044
Age squared	1837.6	2002.2	0.000	1837-6	1824.2	0.170
Completed upper secondary school (i.e. academic track high school)	0.28427	0.27195	0.000	0.28424	0.27967	0.220
Gender.2 (woman)	0.44165	0.48167	0.000	0.44167	0.44452	0.488
Living in an urban area	0.91807	0.85414	0.000	0.91807	0.91745	0.786
Married	0.22888	0.29867	0.000	0.22885	0.23223	0.334
Language.2 (Swedish)	0.00737	0.02984	0.000	0.00737	0.00785	0.504
Language.3 (other than Finnish or Swedish)	0.07522	0.0742	0.536	0.07518	0.07191	0.131
Birth country.2 (other than Finland)	0.08423	0.08474	0.770	0.0842	0.07935	0.033
1 or 2 children	0.14895	0.17045	0.000	0.14896	0.15161	0.371
More than 2 children	0.14244	0.1749	0.000	0.14241	0.14279	0.896
Education category 2	0.0033	0.00487	0.000	0.0033	0.00351	0.669
Education category 3	0.05618	0.06893	0.000	0.05618	0.05642	0.900
Education category 4	0.06878	0.07692	0.000	0.06875	0.06623	0.227
Education category 5	0.05852	0.06835	0.000	0.05852	0.05429	0.027
Education category 6	0.00565	0.0053	0.449	0.00565	0.00496	0.253
Education category 9	0.25246	0.24755	0.068	0.25247	0.25447	0.580
Number of months employed in 2014	2.7104	4.2803	0.000	2.7105	2.696	0.690
Number of months employed in 2015	2.2761	3.8449	0.000	2.2761	2.2501	0.430
Number of months employed in 2016	1.5818	3.3201	0.000	1.5815	1.5574	0.383
Income from employment 2014	4971.6	9801.9	0.000	4971.8	5001.4	0.753
Income from employment 2015	3773.5	8641.0	0.000	3773.6	3810.6	0.646

Income from employment 2016	1947.5	7161.7	0.000	1946.6	1921.2	0.577
Housing allowance 2014	1302.8	965.45	0.000	1302.8	1244.2	0.000
Housing allowance 2015	1538.6	1158.8	0.000	1538.6	1477	0.000
Housing allowance 2016	1773.3	1331.9	0.000	1773.4	1709.3	0.000
Missing value for labour income 2014	0.0536	0.04116	0.000	0.0536	0.05587	0.229
Missing value for labour income 2015	0.03869	0.03053	0.000	0.03869	0.04059	0.242
Missing value for labour income 2016	0.03194	0.02469	0.000	0.03195	0.03343	0.316
Months in rehabilitative work 2014	0.37215	0.35926	0.203	0.37216	0.35998	0.357
Months in rehabilitative work 2015	0.5442	0.47779	0.000	0.54422	0.5184	0.114
Months in rehabilitative work 2016	0.66726	0.60338	0.000	0.66729	0.63179	0.057
Moving to another municipality 2014	0.07274	0.06199	0.000	0.07274	0.0727	0.987
Moving to another municipality 2015	0.07566	0.06869	0.000	0.07567	0.07852	0.197
Moving to another municipality 2016	0.08468	0.06544	0.000	0.08468	0.08541	0.755
Income support 2014	1115.8	947.61	0.000	1115.8	1088.8	0.165
Income support 2015	1160.8	970.58	0.000	1160.8	1139.1	0.263
Income support 2016	1207.5	979.51	0.000	1207.5	1194	0.492
Number of sanctions 2014	0.16124	0.13608	0.000	0.16124	0.15787	0.535
Number of sanctions 2015	0.08406	0.07345	0.000	0.08406	0.08073	0.241
Number of sanctions 2016	0.06695	0.07487	0.000	0.06696	0.06868	0.463
Months in studying 2014	0.25704	0.30153	0.000	0.25705	0.25429	0.836
Months in studying 2015	0.29735	0.34884	0.000	0.29736	0.29202	0.707
Months in studying 2016	0.30182	0.36229	0.000	0.30183	0.29223	0.511
Number of all plans 2016	0.40434	0.51257	0.000	0.40435	0.4521	0.000
Number of all plans 2015	0.30062	0.35429	0.000	0.30063	0.32215	0.000
Number of all plans 2014	0.32468	0.35302	0.000	0.32469	0.34029	0.002
Months in training 2016	0.10448	0.12045	0.003	0.10448	0.10252	0.778
Months in training 2015	0.12065	0.13798	0.004	0.12066	0.12527	0.546
Months in training 2014	0.17594	0.15982	0.015	0.17594	0.17629	0.971
Months in work trials 2016	0.16771	0.16839	0.892	0.16772	0.16737	0.959
Months in work trials 2015	0.17704	0.1502	0.000	0.17705	0.17612	0.892
Months in work trials 2014	0.1715	0.13917	0.000	0.1715	0.17347	0.772
Months in coaching 2016	0.01398	0.01799	0.000	0.01356	0.01425	0.570
Months in coaching 2015	0.02255	0.01793	0.000	0.02214	0.02251	0.801

Months in coaching 2014	0.03831	0.02279	0.000	0.038	0.03494	0.135
Not receiving income-dependent unemployment allowance	0.91133	0.66522	0.000	0.91132	0.90819	0.188
In activation services	0.09115	0.07818	0.000	0.09116	0.08964	0.524
Size of municipality	1.5e+05	1.6e+05	0.000	1.5e+05	1.4e+04	0.000
Municipal unemployment rate	0.10198	0.09247	0.000	0.10198	0.10184	0.330
Share in activation services	0.00833	0.00764	0.000	0.00833	000814	0.000
Share in educational ALMPs	0.0159	0.01338	0.000	0.0159	0.0157	0.000
Share in supported employment	0.00592	0.00674	0.000	0.00592	0.00588	0.001

Notes. Table includes variables used in Match 1. Municipality level variables have the same values for all individuals who live in the same municipality. Share in activation services means the ratio of individuals participating in certain active labor market policies (e.g. rehabilitative work, coaching, work trials) out of the working-age population. Individuals participating in education related ALMPs (e.g. labor force training, studying on the unemployment benefit) are not included there, but instead in the share in educational services variable.

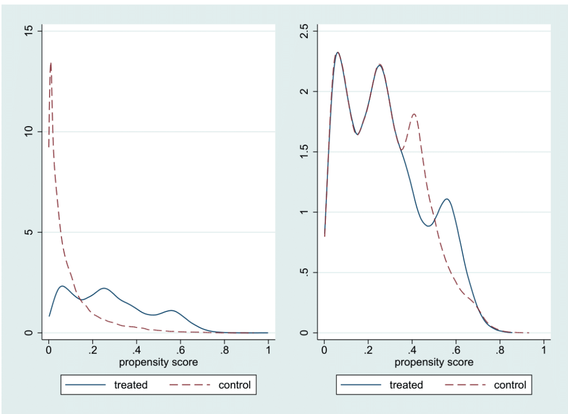


Figure F1. Propensity score density before and after matching (main specification)

Notes. Figure presents kernel density plots of propensity score before and after matching, respectively. We include outcomes from 3 pre-treatment years in matching. All matching variables and their balance can be seen in Table F1.