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

TYÖPAPEREITA / WORKING PAPERS

The Decentralization of Public Employment Services and Local Governments' Responses to Incentives*

337

Labore
TYÖN JA TALOUDEN TUTKIMUS
EST 1971

* A previous version of this paper has been circulated as Labour Institute for Economic Research Working Paper Nr. 332. We would like to thank Stefano Lombardi, Mika Kortelainen, Janne Tukiainen, Simo Mentula, Kari Hämäläinen, and various participants in the SOLE 2022 Annual Meeting and Helsinki GSE Public/Labor Economics PhD Workshop in December 2020 for their useful comments regarding this paper. We also thank the OP-Pohjola Foundation for the data cost funding.

** Labour Institute for Economic Research LABORE (Helsinki, Finland), ohto.kanninen@labore.fi
*** Labour Institute for Economic Research LABORE (Helsinki, Finland), hannu.karhunen@labore.fi
**** Department of Economics, Turku School of Economics, University of Turku. Rehtorinpellonkatu 3, 20500 Turku, Finland, jeremias.nieminen@utu.fi. Corresponding author.

ABSTRACT

We examine how the decentralization of public employment services (PES) affects the behavior and service provision of PES offices and the labor market outcomes of job seekers. We utilize a Finnish temporary reform during which PES were decentralized for specific target groups of job seekers in 23 treated municipalities and remained centralized for others. The form of the temporary reform presented the treated municipalities with the possibility of shifting costs to the central government. We estimate the 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 evidence consistent with municipalities being able to shift 10% of their unemployment benefit costs to the central government.

JEL Classification: H11, H75, J48, J64

Keywords:
public employment services
cost-shifting
fiscal federalism
decentralization

TIIVISTELMÄ

Tutkimme, miten työvoimapalveluiden alueellistaminen vaikuttaa työvoimapalveluiden toimintaan sekä työnhakijoiden työmarkkinatulemiin. Hyödynnämme tutkimuksessa vuosina 2017-2018 toteutettua väliaikaista alueellistamista (”Työvoima- ja yrityspalveluiden alueellinen kokeilu”), jossa työvoimapalvelut alueellistettiin tietyille kohderyhmille 23 kunnassa. Estimoimme reformin kausaalisia vaikutuksia käyttäen yksilötason erot eroissa (difference-in-differences) -menetelmää yhdessä kaltaistamisen kanssa. Emme löydä näyttöä positiivisista työmarkkinavaikutuksista, mutta tulokset viittaavat kuntien pystyneen siirtämään 10% työmarkkinatukimenoistaan valtion maksettavaksi.

Avainsanat:
työvoimapalvelut
fiskaalinen federalismi
alueellistaminen

1. Introduction

The provision of public employment services (PES) has been decentralized in countries such as Germany, Canada, and Denmark with the aim of increasing the efficiency of employment services (Mosley 2011; Mosley 2012). The fiscal federalism literature suggests that decentralization can in principle make public services more suited to local needs in the absence of interjurisdictional externalities (Oates 1972, 1999; Faguet 2004). In the case of PES, local authorities may have a better understanding of the local labor market and may thus be able to provide better services. However, it is also possible that local policy makers have ambitions other than improving the national level of employment. Given the opportunity, 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 (ALMPs) if increasing participation in these programs is beneficial for the local government, or it could lead to the lower mobility of job seekers if the aim of the municipalities is for the job seekers to be employed in their own jurisdictions. Although the effects of decentralization policies in other policy areas have been widely studied, evidence related to the economic costs and benefits of decentralized PES (Mergele & Weber 2020; Lundin & Skedinger 2006; Mörk et al. 2021; Boockmann et al. 2015) is scarce. Evidence of the effects of specific policies in different countries is needed to gauge the optimal level and type of PES decentralization.

In this paper, we provide quasi-experimental evidence of the effects of PES decentralization. We examine how employment office behavior and service provision change in a setting in which municipalities are given temporary authority to arrange employment services and where there exists the possibility of shifting some of the costs of unemployment to the central government through reductions in the specific penalty payments that municipalities must pay. Our main contribution is the measurement of the municipalities' cost-shifting behavior directly in a context in which the provision of PES is decentralized. In addition, as there is only one previous paper studying the causal effects of PES decentralization on labor market outcomes (see Mergele & Weber 2020), our paper is the second attempt in the literature to estimate the causal effects of PES decentralization reform on labor market outcomes.

Our main finding is that local governments shift costs to the central government by changing their service provision, with no detectable improvements in the labor market outcomes of participants. We are able to examine the cost-shifting behavior of the municipalities in detail because Finnish institutional arrangements allow us to study municipalities' policy responses more directly compared to Mergele and Weber (2020). We estimate that local governments succeed in shifting a significant amount of costs—approximately 10 million euros per year—to the central government during the temporary reform. A nationwide implementation of the policy change would transfer an annual expenditure of 42 million euros from local governments to the central government. This represents around 0.18% of the 23 billion euros that were collected annually as municipal taxes, or 10% of the penalty payments paid by municipalities.

To learn more about mechanisms, we find that the decentralized offices reduced the number of plans conducted in total and changed the plan composition, resulting in a higher number of activation plans, which are made primarily for the long-term unemployed. These behavior changes in service provision followed a decrease of 5 percentage points (17%) in 2018 regarding the probability of being registered as unemployed for more than 300 days in a year. Furthermore, our municipality-level estimates shows that decentralization reduced the number of individuals on the penalty list of long-term unemployment without increasing employment. To reiterate, municipalities had no real control over how much unemployment benefit costs they had to pay before the reform, but the decentralization reform gave them control of the type of plans and resulting ALMP placements. This opened an opportunity to shift costs by focusing on lowering registered long-term unemployment, for which municipalities pay penalties.

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. In addition, based on our results, we find no support for the claim that the decentralization of employment services would be effective in increasing the employment prospects of job seekers, at least in the short term. We find no evidence that PES decentralization would have affected participants' employment months per year, their annual labor earnings, or the annual mobility of job seekers. Our results also differ from and complement the negative employment effect estimated by Mergele and Weber (2020), because they looked at a different outcome: the job-finding rate. A nonexistent effect on labor mobility is consistent with the earlier results by Mergele and Weber (2020) and Lundin and Skedinger (2006), dampening possible concerns that the employment effort of the local authorities is skewed toward their own jurisdiction at the cost of worker mobility and national-level employment.

Our work touches on two separate strands of literature: one studying the effects of decentralization of central government functions[4] and the other focusing on employment services (see, e.g., Fougere et al. 2009) and ALMPs (see, e.g., Kluve 2010; Card et al. 2010; Card et al. 2018; Crepon & van den Berg 2016). We also expand the existing but scarce research on the decentralization of public employment services (PES) and cost-shifting.[5] In earlier research, Mergele and Weber (2020) and Lundin and Skedinger (2006) found support for the hypothesis that decentralized employment offices attempt to shift costs to the central government, and in a recent study, Mörk et al. (2021) have shown that local governments in Sweden may use temporary work programs to move individuals from social assistance to unemployment benefits, thereby shifting costs to the central government. To complement earlier findings, we can evaluate the amount of cost-shifting while explaining how local authorities change their behavior and procedures in practice.

This paper is organized as follows. The next section provides details on the institutional background and how the decentralization quasi-experiment was conducted. Section 3 introduces the data and the empirical strategy used. Section 4 presents estimation results and a discussion on the robustness and validity of our results. Section 5 is the conclusion.

[4] While literature on the effects of PES decentralization is scarce, the effects of the decentralization of government functions in other policy areas, such as education (see, e.g., Ahlin & Mörk (2008), Salinas et al. (2017) and Galiani et al. (2008)), environmental policy (see, e.g., Banzhav et al. (2012) and Lipscomb et al. (2017)), and public finance (see, e.g., Baicker et al. (2012)) have been widely studied. For a review of the fiscal federalism literature, see Martinez-Vazquez et al. (2016).

[5] Cost-shifting refers here to local governments attempting to shift costs to higher levels of government. In political economy, cost-shifting is often thought to be a problem in centralized systems in which common-pool problems are present (see, e.g., Weingast et al. (1981) and Besley & Coate (2003))—that is, local governments have incentives to increase their cost because these 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 (which is similar 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 the municipalities to shift costs to the central government.

2. Institutional background

2.1 Public employment services in Finland

PES are currently administered through ELY centers (Centre for Economic Development) in Finland. These 15 centers around Finland are controlled by the Finnish 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 PES to Finnish job seekers. The Finnish law on PES (FINLEX 916/2012) states that employment agencies should offer job placement services, advisory services, 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 toward them.

Finnish PES offices also monitor the job search process; for example, they provide statements that determine eligibility for unemployment benefits and conduct different types of plans for job seekers. In these plans, the PES office indicates 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, activation, and integration.

2.2 Employment plans and activation plans

According to the official guidance, employment plans should be conducted every three months and should include information about the job seeker's situation, goals, and possible limitations. In addition, the plan includes the 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 tasks before this deadline, they may face benefit sanctions. The frequency of employment plans can be changed: they have to be conducted every three months but can also be done more often. It has also been previously suggested that employment offices are not always able to conduct these plans as often as is required by the law (Valtakari et al. 2019).

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

Employment plans (and similar integration plans, which are geared toward recent immigrants) are conducted by the employment office, whereas activation plans are conducted cooperatively by employment offices and municipalities. However, this changed during the decentralization quasi-experiment described in the next subsection: all plans were conducted by the municipal offices during the temporary reform in treated municipalities. While employment and integration plans have similarities, activation plans differ from them. According to the official guidance, when an activation plan is conducted, the emphasis is on determining whether the individual has a need to participate in activation and rehabilitative services. In addition, the job seeker's ability to work is evaluated by the office. When making an activation plan, the employment office can consult public health care if needed. Employment plans, in turn, place emphasis on job-searching tasks, such as the need to complete job applications. Although an activation plan is required for rehabilitative work placement, employment plans can include obligations to participate in other types of ALMPs. For both types of plans, noncompliance with tasks can lead to benefit sanctions.

2.2 Temporary and partial decentralization

The temporary decentralization studied in this paper was called the Regional Pilot of Employment and Enterprise Services (in Finnish: *työvoima- ja yrityspalveluiden alueellinen kokeilu*).[6] The level of decentralization of PES refers here to the extent to which employment programs and services, including budgetary powers, are organized and managed at the subnational levels of government. This large-scale temporary decentralization was conducted between August 2017 and December 2018 with the aim of supporting employment, job creation, and entrepreneurship. In this paper, we focus on outcomes related to employment and service provision, as we have no data on entrepreneurship. During the temporary

[6]An earlier municipal-level analysis studied the effects of this and another Finnish pilot experiment conducted earlier (see Nieminen et al. 2021). However, the municipal-level analysis is not enough, because only a subset of job seekers in the treated municipalities participated in the 2017–2018 pilot, thus making it necessary to evaluate the effects using individual-level treatment and control groups. In addition, the effects on cost-shifting, employment and activation plans, or participation in different types of ALMP programs were not investigated by Nieminen et al. (2021).

decentralization, 23 treated municipalities in five areas assumed control of providing employment services for the specific target group of job seekers for 17 months. During the reform period, the treated municipalities were responsible for conducting employment and activation plans with job seekers and directing them to ALMP programs. Table 1 presents the responsibilities of the 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 on the map of Finland. All municipalities could apply for the pilot program, but in practice, they had to apply together, which is why treatment is clustered, as can be seen in Figure 1. In June 2016, 23 municipalities belonging to five areas were selected from 77 applicant municipalities by the Ministry of Economic Affairs and Employment of Finland. Thus, the municipalities were not randomly assigned to the program. According to an official statement, the selection of participating areas was made by evaluating the applicants based on the following criteria: the 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, and 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 aimed to choose areas from different parts of the country for participation. During the reform period, municipalities began to provide 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: decentralized and centralized.

Table 2: Characteristics of treated and control municipalities

	mean of all control municipalities	mean of control municipalities that applied but were not selected	mean of treated municipalities	t value, (treatment–control)	t value, treated – applied but not accepted
wage sum	237.3 million	738.5 million	611.8 million	1.9*	-0.17
wage sum per capita	11,892.8		13,986.6	3,24***	
share of urban population	0.592	0.730	0.794	4.32***	1.36
unemployment rate	0.0793	0.0868	0.0866	1.35	-0.03
share in subsidized employment	0.0071	0.00682	0.0056	-1.65*	-1.585
share in educational ALMPs	0.0079	0.00973	0.0105	3.19***	0.86
share in other ALMPs	0.0088	0.00719	0.0089	0.06	1.36
size of municipality (number of inhabitants)	15650.9	43373.9	41863.6	2.48**	-0.066

Notes. Source of the table: Own calculations using register data on the whole Finnish population.

As can be seen from Table 2, the treated municipalities differed significantly from the untreated ones in many ways, the most important being size. Although the difference-in-differences analysis does not require treatment and control areas to be similar, we also match on municipal-level covariates in our main specification. Though the matching does not succeed in balancing municipal-level variables in our individual-level treatment and control groups (see Appendix

F), the differences in means are relatively small in the matched treatment and control groups. Moreover, we can conduct the same difference-in-differences analysis using only controls from applicant but non-accepted municipalities, as applicant but non-accepted municipalities are no different from treated municipalities in regard to the observed characteristics (see Table 2). Using this alternative control group (see Appendix B, Figure B13) gives very similar results compared our main specification.

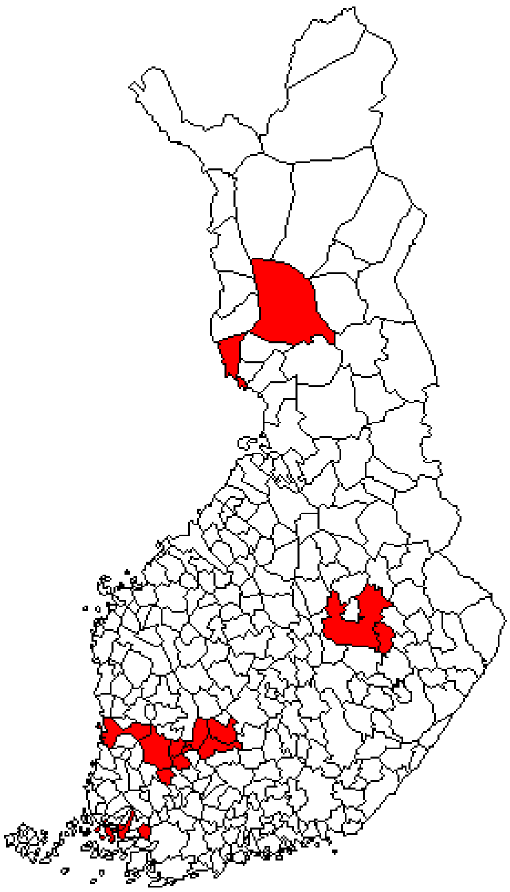


Figure 1: Participating municipalities

Notes. Source of the map data: Statistics Finland. Treated municipalities are in red. There were five treated areas: Pirkanmaa (10 municipalities), Varsinais-Suomi (4 municipalities), Pohjois-Savo (3 municipalities), Lappi (5 municipalities), and Pori (1 municipality). In Pirkanmaa, the treated group includes individuals who receive a basic unemployment allowance or labor market subsidy but do not receive the income-dependent unemployment benefit. In Varsinais-Suomi, job seekers under the age of 25 and job seekers who have been unemployed for more than 12 consecutive months are treated. In Pohjois-Savo and Lappi, the treatment group includes job seekers who have been unemployed for more than 12 consecutive months. In Pori, the treatment group includes 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.

Decentralized services were not similar in all areas, as municipalities exercised new decision-making power to offer different individualized services that were best suited to regional needs. Most notably, in Pirkanmaa (the largest treated area, consisting of 10 municipalities), each job seeker was assigned to an employment coach (OmaValmentaja), 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 conducted but, rather, to evaluate the average effects of PES decentralization. The target groups and the treatment details were not imposed by the central government but were offered by the participating areas as part of their application to the program.

2.3 Services and the cost burden of the central government versus 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—an unemployment benefit for individuals without extensive employment history—for more than 300 days, and 70% of the costs if the job seeker has received the labor market subsidy for more than 1,000 days (FINLEX 1290/2002). Municipalities, however, do not need to pay these costs when the job seeker participates in ALMPs. Additionally, the days on which an individual participates in activation do not count toward the 300-day or 1000-day cutoff. We refer to these individuals as the *penalty list*. An individual belongs to the penalty list at 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 factor that nullifies the unemployment days counter is working six months full-time. During the temporary decentralization, the treated municipalities could potentially decrease these penalty payments by increasing ALMP participation, as 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 that are not optimal for them. Alternatively, municipalities could save money by targeting individuals who are on the penalty list or who are about to cross the 300-day cutoff.

To study whether municipalities exploited employment services to do cost-shifting, we first estimate the effect on penalty payments at the municipality level. Second, we investigate the effect on long-term unemployment, proxying the probability of belonging to the penalty list. Third, we examine whether municipalities increase activation, and specifically placements in rehabilitative work, as it can be the most beneficial for the municipality and possibly the easiest way for local governments to increase ALMP participation. This is because it is a service that they usually provide directly, while other ALMPs must be procured from other service providers. These programs may not, however, be optimal for all job seekers. In fact, according to Finnish law, only job seekers who need rehabilitation should be directed toward these programs. However, the programs may also be valuable for municipalities for reasons other than 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 market price.

Cost-shifting, especially through the reduction of the cost burden that penalty payments cause for 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 actively lobbying for the permanent decentralization of employment services, suggesting that the prospect of being responsible for employment service provision seems alluring to the municipalities.

The amounts of the penalty payments made by the municipalities are publicly available at the municipality level. Thus, we calculate the effect on penalty payments at the municipality level, different from our other analyses, which are conducted using individual-level data. We do not use individual-level data in the penalty payment analysis, as identifying individuals on the penalty list is challenging as we do not know which of the three benefit types a job seeker receives and has received earlier. The type of unemployment benefit depends on unemployment fund membership status (can be observed imperfectly), unemployment duration, and whether the individuals fulfil requirements regarding working history (this is not easily observed). Although the municipal-level estimation is our preferred way of calculating the size of cost-shifting, we do individual-level calculations in the appendix, where we attempt to approximate the size of cost-shifting with individual-level data, with proxying being on the penalty list by having more than 300 unemployed days per year.

3. Data and methods

3.1 Data

3.1.1 Data sources

The individual-level administrative data sets utilized in this paper are obtained from Statistics Finland and TEM. We combine basic information about job seekers with their history of employment, earnings, and ALMP participation. The data modules used are FOLK basic, FOLK income, TEM job search, and TEM job seeker.[7]

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

3.1.2 Pre-matching treatment and control groups

The pre-matching treatment group is defined by the criteria that each treatment area set for job seekers to be eligible for treatment. These criteria are described in the notes for Figure 1 in Section 2.2. In addition, we limit the sample to individuals who were unemployed or participated in activation at the end of July 2017. Including individuals who become eligible later during the treatment period would make it more difficult to determine the control group and how the matching should be conducted. Moreover, calculating the yearly treatment effects

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

in such a setting would be problematic because different individuals would begin treatment in different months.

Table G1 in Online Appendix G shows the numbers of initially eligible and initially treated individuals in the five treatment areas. Eligibility predicts that an individual is treated, but not everyone who is eligible seems to receive the treatment initially.[8] We use all eligible individuals as our treatment group, although the results are similar if we calculate the instrumental variable (IV) estimates, instrumenting treatment status with eligibility (see Appendix Table B.11 in Online Appendix B for first-stage results and Table B.12 in Online Appendix B for IV estimates).

We omit from our sample the individuals living in the Pori area when the treatment begins, as we cannot reliably identify the initially eligible individuals in the Pori area owing to the complex eligibility criterion for individuals older than 25 years: 200 days receiving the labor market subsidy. We do not observe the number of days that the individual received this type of unemployment benefit—only the number of days that 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 correct individuals in Pori (see Online Appendix G). In addition, the Pori area comprised only one municipality, whereas the other treated areas consisted of a larger number of municipalities. The results do not change if we include Pori.

The pre-matching control group consists of all individuals living in untreated municipalities who were unemployed or participating in activation policies at the end of July 2017. The eligibility criteria vary between treated areas in a manner that does not allow us to further exclude individuals from the pre-matching control group. However, the eligibility criteria are included as matching variables. Ineligible job seekers inside treated municipalities are excluded from the sample.

3.2 Empirical strategy

Matching adjustments

We match eligible individuals in treated municipalities to job seekers in untreated municipalities to provide a control group for causal inference. Matching is used because

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

eligibility criteria varied between treated areas, thus making it impossible to simply compare eligible individuals in treated areas to individuals in control areas who meet similar criteria. Matching is conducted using both basic background characteristics (age, gender, and residence in an urban area) and variables related to individuals' employment and earnings history. Additionally, we match pre-treatment outcome variables in our main matching specification. 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 the three years before treatment to be able to test whether the pre-trends are parallel in the years before the matching period. We also conduct our analyses using a matching specification, wherein no pre-treatment outcome variables are used, as it has been noted that using pre-treatment outcomes in matching may increase bias 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. The balance of matching covariates before and after matching is shown in Appendix Table F1: with the exception of the municipal-level variables, the covariates are in balance after matching. Although the municipal-level covariates are not in balance, the means of the treatment and control groups are quite close to each other. The kernel densities of the propensity score before and after matching are presented in Appendix Figure F1. We also check robustness to other matching algorithms such as one-to-many PSM, entropy balancing, as well as coarsened exact matching (CEM), because propensity score matching has been criticized by, for example, King and Nielsen (2019), who propose that CEM should be favored over PSM. The results are qualitatively similar when these alternative matching adjustments are performed. The results from the alternative matching specifications can be found in Appendix B.

Difference-in-differences

Our main specification uses a standard, individual-level difference-in-differences (DiD) method to estimate the intention-to-treat effects.[9] This is done by estimating two-way fixed-effects regression models in the matched sample. To test the assumption that the pre-trends are parallel, we also calculate yearly treatment effects in the matched sample. In the main text, we

[9] We also calculate IV estimates, instrumenting the treatment*post dummy with an eligibility*post dummy, but we consider the intention-to-treat estimation our main specification.

show the results in which we estimate the treatment effects for each year and plot the coefficients on event study plots. This model can be written as

$$Y_{it} = \gamma_i + \lambda_t + \sum_{\substack{k=2006 \\ (k\neq 2016)}}^{2018} \theta_k D_{it}^k + \varepsilon_{it} \tag{1}$$

In the model (1), γ_i and λ_t are the individual and year fixed effects, respectively. The variables D_{it}^k are periodic treatment indicators—that is, interactions between the treatment (eligibility) and the year variable. Year 2016 is the reference period; hence, the treatment indicator for 2016 is omitted. The 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. We also estimate the basic DiD model, the results of which we show in the appendix. With individual and year fixed effects, the model can be written as

$$. \qquad Y_{it} = \gamma_i + \lambda_t + \delta(treat_i * post_t) + \varepsilon_{it} \tag{2}$$

In the model (2), 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—that is, 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 addition to requiring the parallel trends assumption to hold, the difference-in-difference strategy depends on the assumption that no simultaneous reforms that would affect the treatment and control groups were conducted during the observation period. Two reforms that were conducted simultaneously were the "periodic interviews" program introduced in 2017, and the "job seeker activation scheme" introduced in 2018. However, these are centralized reforms that would have impacted every job seeker in both the treated and control municipalities, and there is no reason to believe that it would affect these municipalities differently. Thus, they should not cause bias in our results.

One regional (not municipal-level) program that could raise worries is a co-operation pilot that was arranged during the same time in three control regions, but without any additional funding or changes in legislation—that is, no changes in the actual responsibilities of the regions or municipalities. We can, however, drop the three affected control regions from the analysis, and doing this, we still obtain essentially the same results (see Appendix Figure B14). Regarding municipal-level reforms, we have had discussions with officials from both the

Ministry of Finance and TEM, and no one has raised any potential reforms that could have biased our results. The interventions that the participating municipalities may have conducted during the decentralization reform are part of our treatment and are not a source of bias. This is because if employment services were fully decentralized, we would expect different municipalities to offer different services and programs than the centralized PES offices.

4. Results

4.1 Cost-shifting behavior of local governments

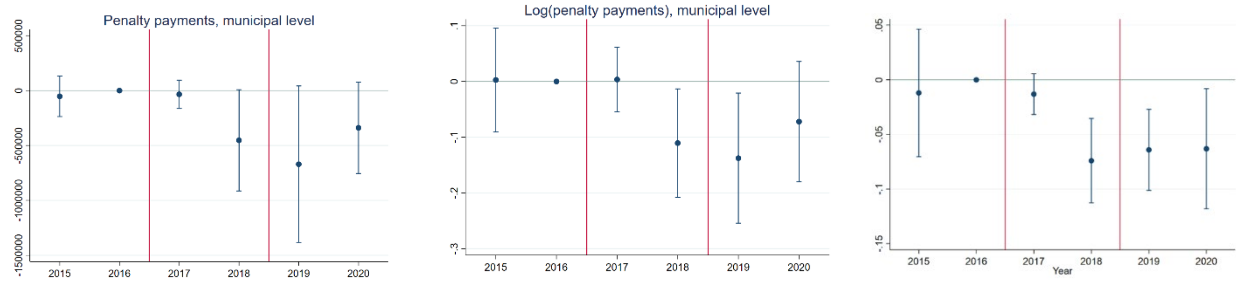
At the municipality level, there are data available on penalty payments paid by the municipalities. To estimate the extent of cost-shifting during the temporary reform, we estimate municipal-level difference-in-differences estimates, which are presented in Figure 2 below: Panel A shows the effects on penalty payments in euros, while Panel B shows the effect on the logarithm of penalty payments—that is, the effect in percentages. The municipal-level results suggest that municipalities were able to shift an average of 450,000 euros, or 10%, of their penalty payment costs to the central government (2018 estimate). As there were 23 participating municipalities, this means 23*450,000 = 10.4 million euros. If the reform were implemented nationwide, the estimate would suggest potential cost-shifting in the ballpark of 42 million euros. The decrease in penalty payments observed with municipality-level data is concurrent with a more than 5 percentage point decrease in the probability of long-term unemployment (Panel C of Figure 2) and an increase in ALMP participation (see Figure 4), which are estimated with individual-level data. It also seems that the penalty costs in 2019 remained smaller for the treated municipalities, which is probably due to ALMP participants' continuing participation in programs in which they had been placed during the temporary reform (see Figure 4).

Figure 2: Cost-shifting: municipal- and individual-level evidence

Panel A: Penalty payments (municipal-level data)

Panel B: Log(penalty payments) (municipal-level data)

Panel C: More than 300 unemployed days (individual-level data)



Notes. Panels A and B present municipal-level difference-in-differences estimates wherein the outcome variable is the penalty payments (in euros) and log penalty payments, respectively. Penalty payments in the current form (for all >300 days unemployed) have been collected since 2015. Panel C presents individual-level estimates wherein the outcome variable is having more than 300 days in registered unemployment during the year. All treated municipalities are included in the treatment group, and all untreated municipalities are included in the control group. The standard errors are clustered by panel id (municipality).

Unfortunately, our microdata does not have information whether an individual belongs to the penalty list. Thus, the municipality-level estimation of the amount of penalty payments is the only way to directly measure the effect on the number of people on the penalty list. However, it is possible to approximate the cost savings for municipalities based on the individual-level results regarding long-term unemployment. Although long-term unemployment is an imperfect proxy for belonging to the penalty list, the implied cost-shifting calculated using individual-level data is of the same magnitude as the municipality-level estimate. See Appendix E for the calculations in which we attempt to approximate cost-shifting using individual-level data.

4.3 Mechanisms of cost-shifting: Plans conducted and ALMP placement strategies

Next, we strive to understand the mechanism through which municipalities managed to reduce the number of individuals on the penalty list, consistent with a cost-shifting strategy, in the absence of any real employment gains. The 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, the ALMPs to which the unemployed are then directed. These two mechanisms are related because the plans are conducted before the actual placement begins. For example, an activation plan is always made when a rehabilitative work placement is considered, but it 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 for the employment offices (and here, municipalities) to change the frequency with which plans are conducted if they wish to do so.

We first look at the number of plans conducted by the employment office together with the job seeker. Additionally, we examine whether the treatment affected the types of plans that are conducted. We consider this a sort of first-stage analysis of the reform. If there are 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.

Figure 3 presents the estimation results for the number of activation plans and employment plans. In the first full reform year (2018), we observe an effect of 0.2 plans in the activation plans (72% increase relative to the control group mean) and an effect of -0.4 employment plans (33% decrease relative to the control group mean). Regarding the effect on all plans irrespective of type, 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 decrease of 13% (see Appendix B, Table B2, Column 3). A decrease in the number of 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; there is somewhat more flexibility than for employment plans, which should be updated every three months but can be updated even more frequently. 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 because an activation plan must be made when a job seeker is directed to a rehabilitative work program.

The treatment effect on employment plans is negative and significant in both the event study specification (Panel B of Figure 3) and in all other specifications (see Appendix Table B2 for basic DiD estimates and Appendix Table B5 for results with different matching algorithms). Similarly, the observed increase in activation plans is also significant in all specifications. The effect on activation plans is not visible before 2018, as can be seen Figure 3 showing yearly treatment effects. 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 decrease of around one-third in employment plans. This demonstrates that decentralization has a meaningful effect on PES. We have not included the effects on integration plans. There is no effect, as we have included only individuals who have lived in Finland every year during the observation period and who are consequently obliged to make an integration plan solely under rare circumstances. Integration plans are, nevertheless, included in the number of all plans per year.

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

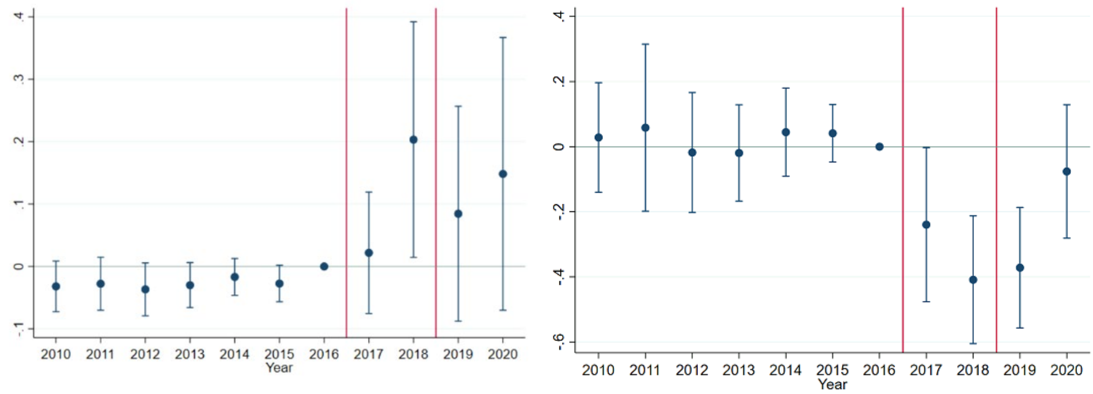


Figure 3: Activation and employment plans

Notes. The figure shows the 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. The reference period is 2016, and treatment begins in August 2017. The treatment group comprises the eligible individuals. The standard errors are clustered by municipality. The control group mean of activation plans in 2018 is 0.279 plans per year. The control group mean of employment plans is 1.222 plans per year. For outcome means in 2016, see Appendix Table B2, in which we present basic difference-in-differences estimates for plans.

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 occur. This is because there are significant differences in effectiveness between different types of ALMPs; for example, employing job seekers in the public sector has been shown to be less effective in regard to employment and displacement effects (see, e.g., Kluve 2010). A potential cost of the decentralization of employment services is that it may specifically encourage the use of less effective ALMPs if these are better for municipal finances (Mergele & Weber 2020). The results of previous studies examining the PES decentralization support this hypothesis. Lundin and Skedinger (2006) find that increasing municipalities' power in ALMP decisions made placements in ALMPs organized by municipalities more likely. Similarly, Mergele and Weber

(2020) find that decentralization increased participation in public employment schemes. In Finland, municipalities organize rehabilitative work programs, through which PES offices direct job seekers who need rehabilitation. During the decentralization reform, the treated municipalities could, however, decide who was fit to participate in these programs.

Panel A of Figure 4 shows a significant increase of about 0.4 activation months in 2018, the first full year of the temporary reform. Panel B of the same figure illustrates that this increase comes from an increase in rehabilitative work placements. For other ALMP types, the point estimates are negative or minimal (see Online Appendix B, Table B3). This could be interpreted as municipalities changing 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 rehabilitative work in 2018. The temporary reform started in August 2017; however, for 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 individual is engaged in rehabilitative work, they are no longer registered as unemployed.

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

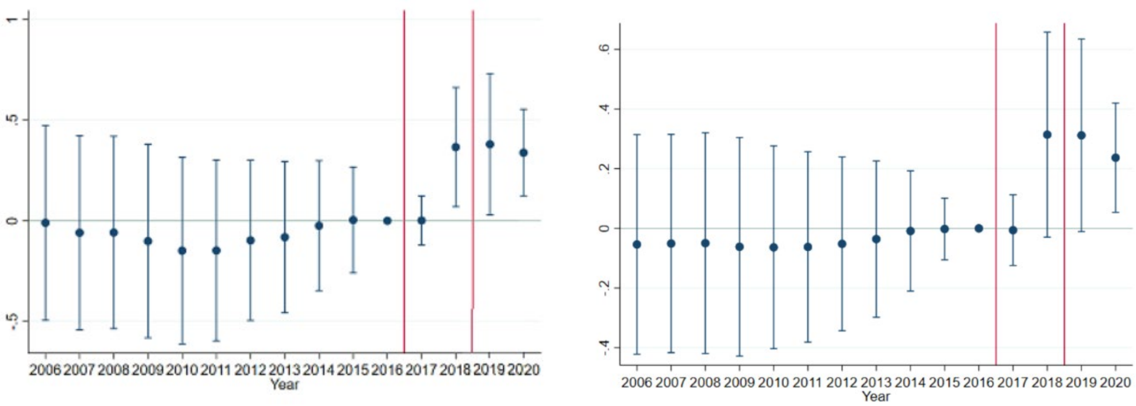


Figure 4: Months in all ALMPs and months in rehabilitative work

Notes. The figure shows the 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 of rehabilitative work per individual per year. The reference period is 2016, and the treatment begins in August 2017. The treatment group is the eligible individuals. The standard errors are clustered by municipality. The control group means in 2018 are 1.594 months in ALMPs and 1.012 months in rehabilitative work. For the outcome means in 2016, see Appendix Table B3, in which we present basic difference-in-differences estimates for ALMPs. Appendix Table B3 also shows the effects on ALMP types other than rehabilitative work.

We found that ALMP participation increased 0.4 months per individual during 2018, although the effect was not significant owing to a lack of power. The point estimate, although insignificant, is quite sizable because 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 of rehabilitative work is even larger, indicating a one-third increase in rehabilitative work participation, although the estimate is insignificant owing to a lack of power. At the same time, we found a decrease of 5 percentage points (17%) regarding the probability of having more than 300 days in registered unemployment per year.

Our finding suggesting an increase of 0.4 ALMP months per individual means 11,619 months in total in the treated area. If we assumed that the estimated increase fully targeted the long-term unemployed and that these individuals were moved to ALMP for the full year, this would then mean that 968 more individuals were moved to activation, representing 10% 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 that individuals were moved to activation for six months, the increase in ALMPs would explain all of the decrease observed in the number of long-term unemployed. Nevertheless, this calculation is very sensitive to assumptions regarding how long the new ALMP participants spent in ALMPs.

4.3 Other outcomes: regional mobility, employment, and earnings

Figure 5 shows the difference-in-differences estimates of the labor market outcomes in an event study figure. The figure shows that the decentralization of employment services had no effects on the number of months per year that the individuals worked in the short term. Similarly, we do not find any significant effects on annual labor income (Panel A), although the standard errors clustered by municipality are sizable. Basic difference-in-difference estimates (Appendix B, Table B4) also show that point estimates are close to zero in annual earnings (50 euros) and employment months (0.09 months). Observations from 2017 are omitted from the analysis shown in Appendix Table B4, because the treatment began late in the year in August 2017, although observations from 2017 are naturally included in the yearly event study plots shown in Figure 5.

We observe no significant pre-trends in employment months or earnings, although there is a slight, insignificant decrease 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 concern during the years of the temporary reform (2017–2018), as they were years of robust economic growth. It should also be noted that the clustered standard errors in the yearly figures vary, becoming visibly smaller in the post-treatment years compared to pre-treatment years. The reason for this is likely to be that everyone in both our treatment and control group is unemployed in July 2017; thus, there is probably little variation in employment-related outcomes in 2017. Because of this, it makes sense that the standard errors are the smallest in 2017 and then grow as we move further away from 2017. Owing to matching, the levels in the treatment and control groups are also similar, as shown for all main outcome variables in Appendix A, allowing for comparisons of the 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.

Appendix Table B1 presents the effects on labor market outcomes from a basic difference-in-differences model in a matched sample created using PSM, wherein the pre-treatment outcomes are included in addition to other individual and municipal-level characteristics. Observations from 2017 are omitted from the analysis, as the treatment began late in the year in August 2017, but they are naturally included in the yearly event study plots shown in Figure 5.

Panel C of Figure 5 reports estimates of long-term unemployment, defined as having more than 300 days in registered unemployment during the year. We find a significant 6 percentage point decrease in the probability of being long-term unemployed in 2018. 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 or entropy balancing. Results with alternative matching procedures can be found in Table B4 in Online Appendix B. No effect can be seen in 2017, which is again expected because the reform did not start until August 2017.

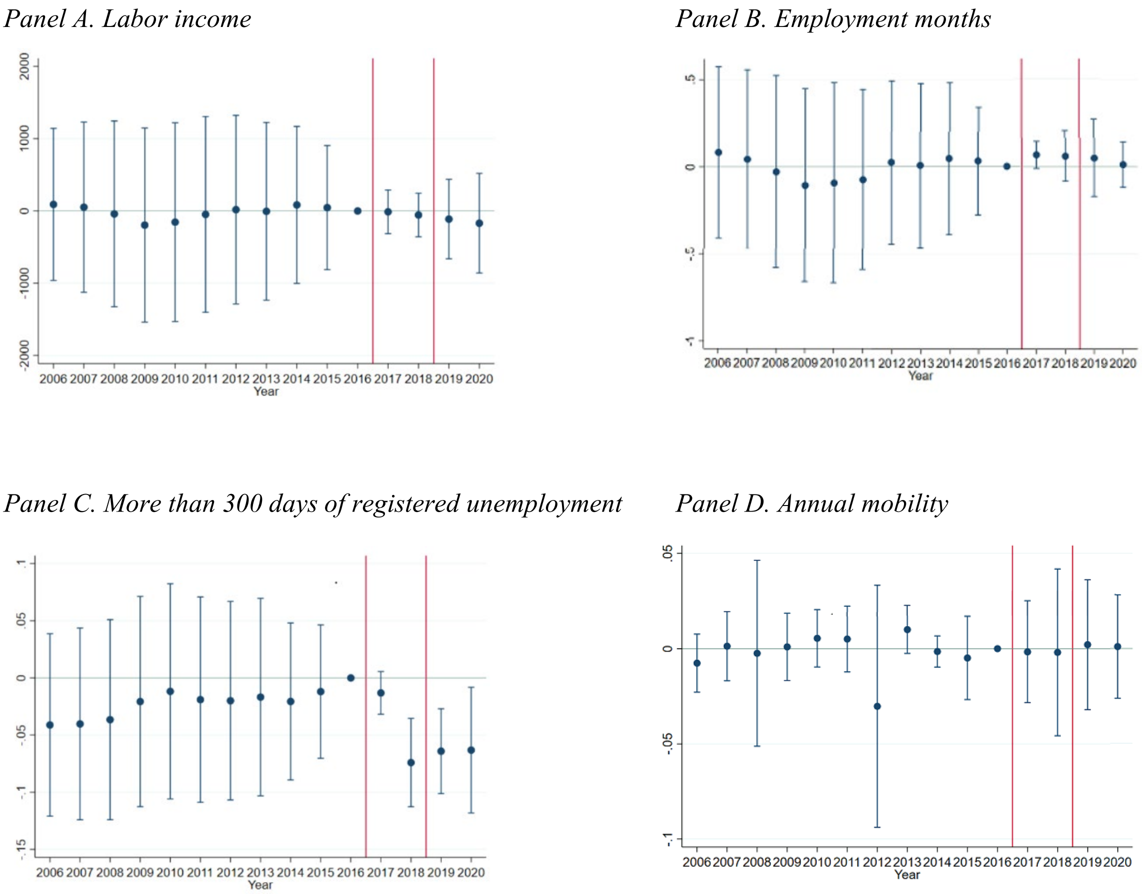


Figure 5: Labor market outcomes

Notes. The figure shows the yearly treatment effects. The reference period is 2016, and treatment begins in August 2017. The treatment group includes all eligible individuals. The standard errors are clustered by municipality. The control group means in 2018 are 4301 euros in labor income, 2.707 in employment months, 0.322 in probability of having more than 300 days of registered unemployment during the year, and 0.062 in annual mobility. For outcome means in 2016, see Appendix Table B3, in which we present basic difference-in-differences estimates for labor market outcomes.

Finally, we present the effects on annual mobility in Panel D of Figure 5. 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 are robust across specifications (see Appendix B for robustness specifications). As can be seen from Figure 5, there are no pre-trends. The finding of 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 the 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.

4.4 Robustness and spillovers

Doubly robust difference-in-differences

Our main results are robust to using the doubly robust difference-in-differences estimator (DR DID) proposed by Sant'Anna and Zhao (2020), whose method is an improved, more robust version of difference-in-differences combined with matching. We report the DR DID results for our main outcome variables in Appendix Table B10. The point estimates that the method provides are similar to the ones from our main specification, but standard errors are different because the R package DR DID 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-treatment period. Due to the large size of our data and the limited 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

Because the eligibility criteria varied between the treated areas, we cannot use the natural control group—that is, those individuals in control municipalities who fulfill the eligibility criteria. The main reason for using matching is that we can include the different eligibility criteria as predictors of treatment in matching. If we instead compared the treated individuals to all unemployed individuals in living control municipalities, the demographic differences between the treatment and control groups would be very large, as the eligible groups differed significantly from the average Finnish job seeker. Although we believe that matching is necessary in our context, we show a raw trend figure without any matching in Appendix Figure A.3. Figure A.3. suggests that pre-trends in labor market outcomes are not parallel before the start of treatment. However, in some of our most important variables—such as plans, ALMP participation, and long-term unemployment—the pre-trends are parallel even without matching, and the results regarding these variables seem to hold. We show these types of descriptive trend figures for three different cases: matching with pre-treatment outcomes (Figure A.1.), matching without pre-treatment outcomes (Figure A.2.), and no matching (Figure A.3.).

Our results are robust to changing the matching algorithm: our results are qualitatively similar when using CEM, one-to-many PSM, or entropy balancing. CEM requires us to use fewer variables in matching because 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. In particular, if municipal-level variables (e.g., municipal unemployment rate and population) are added, CEM is unable to find matches.

Standard errors

The standard errors are clustered at the 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 because 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, because the number of years is too small to be used as a clustering variable: we would have very few degrees of freedom in this case (see, e.g., Cameron et al. 2011). The results are, however, robust to two-way clustering (see Appendix tables B7–B9 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, the effects on rehabilitative work and ALMP months are significant at the 5% and 1% levels, respectively, if two-way clustering is used.

Placebo regressions

We assess the robustness of our results by running two types of placebo regressions, because a set of placebo regressions could uncover hidden weaknesses in our research setting and the matching procedure. First, we use regressions for which the placebo treatment is set at 2015, which is two years prior to the actual treatment. The placebo results in Table C1 in Online Appendix C show that there are no placebo effects, except for wage subsidies (1 of 17 outcomes). In the second set of placebo regressions (Online Appendix C, Table C2), we use the fake treatment group and the real treatment period. Except for training, no placebo effects are found.

Spillovers

Employment programs often affect nonparticipants through spillover effects (see, e.g., Crepon et al. 2013). Because not everyone in the treated municipalities was 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. Even if there were spillover effects, they would not affect the credibility of our DiD estimates, as we excluded ineligible individuals in participating municipalities.

The spillover analysis is conducted with similar matching and difference-in-differences analyses as our main specification, but non-eligible individuals living in treated municipalities are used as the treatment group. We estimate the spillover effects in two ways. First, we estimate the effects for initially ineligible individuals in treated municipalities. With this specification, one issue is that, depending on the area, up to 35% meet the eligibility criteria later during the pilot even if they were initially ineligible. This is especially the case for areas in which one of the eligibility criteria is the length of the unemployment spell. Second, we estimate spillover effects for those job seekers in January 2018 who entered unemployment during the November 2017–January 2018 period and who have not been unemployed previously during 2017. This fresh sample of unemployed job seekers cannot become eligible at any point during the pilot. This is because individuals could be transferred to the pilot up to September 30, 2018, meaning that individuals who become unemployed between November 2017 and January 2018 cannot accumulate more than 365 consecutive days of unemployment before September 30, 2018. The control group consists of similar matched individuals in control municipalities.

The estimates of the spillover effects, presented in Table 8, differ considerably, although for both groups, we find a negative effect on the aggregate number of plans. The signs of the point estimates are negative for both employment and activation plans. In other words, being in the non-eligible group in treatment municipalities lowered the amount of interaction with employment offices. Similarly, we see some evidence of a negative effect on rehabilitative work participation, or even ALMP participation in general, for ineligible groups in both specifications. However, despite the lowered amount of interaction with the offices and lower participation in rehabilitative work programs, we do not observe an accompanying decrease in employment or incomes in either specification. In fact, for ineligible new job seekers in the treatment municipalities (column 2), we observe a positive spillover effect on incomes. They appear to have benefited from the temporary reform.

5. Discussion

We find that the temporary reform that decentralized PES in Finland did not achieve its goals regarding employment. This is somewhat surprising, as local governments can be expected to have region-specific information on the job market and the preferences of their constituencies, thereby allowing them to place job seekers more efficiently. On the one hand, the program gave a set of tools to shift costs to the central government, incentivizing behavior that would focus not only on employment but also on these cost-shifting behaviors. Such multi-objective optimization might have hampered employment outcomes. A setting with no possibility of cost-shifting could have redirected the focus fully to the preference of the local government, which is likely to be higher employment, and might consequently have yielded better employment outcomes. It should also be noted that our estimates on employment and earnings are not very precise; thus, there could be an effect that we are unable to detect owing to a lack of power.

Another aim of the reform was to decrease the costs of unemployment. We assume this rather vague aim includes the total costs of ALMPs and PES for the public sector, which ignores the possible cost-shifting between the regions and central government. 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 in all specifications) 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. The cost estimates end in 2014, and 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 this by the number of treated individuals (29,049), the total cost for the experiment amounts to approximately 1.4 million euros. If the sizable but statistically insignificant effect on rehabilitative work (0.341 months) is included, the cost estimate of the pilot increases to approximately 9.7 million euros, or to 7.9 million euros if we consider the spillover effect we found for initially ineligible individuals. Extrapolating these figures and taking the spillover effect into account, it would cost around 37 million euros annually if the reform were extended to all unemployed individuals across the country. These figures do not take into account the general implementation costs, which have been reported to have exceeded 10 million euros in the Pirkanmaa area alone during the temporary decentralization program.

A limitation of this study is that the selection of treated areas was not conducted randomly by the central government. Instead, each applicant area's application was evaluated based on certain criteria, including the quality of the planned implementation and the assessed effect on total government unemployment costs. If the central government had any success in selecting the areas with the most beneficial program effect on total costs for the whole public sector, it would decrease the total costs of the reform, meaning that our estimates give a lower bound of what the program would cost if extended to the whole country.

Because 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 the 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 the 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), although Figure D1 suggests that there is a small negative effect on benefit sanctions during the reform period.

Concerning ALMPs, we find some evidence suggesting that in addition to increasing placements in ALMPs, local governments choose a somewhat different policy mix from 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, although, owing to a 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 decrease in the nontarget population in the participating municipalities with regard to rehabilitative work ALMP, coupled with an insignificant decrease in all ALMPs; however, we observe no change in employment. The opposing changes in rehabilitative work months in the target and nontarget populations in the participating municipalities, together with no observed employment effects, challenge the effectiveness, at the margin, of this type of ALMP.

These results 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 two to three years after the programs. Naturally, different programs have heterogeneous effects in regard to timing and participant groups, but it has been shown that overall programs that focus on human capital accumulation (education and training) result in the most visible positive effect on employment over time. The effectiveness of public sector employment programs or wage subsidies is often found to be very low. As we are examining 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's information advantage is ignored.

When comparing our results to those of 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 divergent results stem from a different measure or an actual difference in outcomes. However, both results support the finding that local governments are unable to exploit their local understanding to promote employment better than the centralized government. We can complete the picture of how local government behavior is consistent through and through with the aim of cost-shifting, including self-proclaimed aims. This is done by targeting the long-term unemployed to reduce penalties that the municipalities must pay for every long-term unemployed individual.

The potential drawbacks for the external validity of these results are that the reform was temporary and targeted particular groups of individuals and the fact that the interventions differed by municipality. A longer or permanent reform would be likely to affect both municipality and job seeker behavior differently, and we are usually interested in steady-state effects, which might not have emerged in the shorter reform. Targeting particular groups could cause some bias, although those target groups were proposed by the municipalities themselves and would be likely to be a focus in a more expansive reform. Neither the targeting nor the temporary nature of the reform is likely to affect our main conclusions on cost-shifting, however. Finally, differing interventions by municipality are a feature of a decentralized system, not a source of bias.

6. Conclusion

Employment services have been decentralized in many countries, but evidence of the effects of this policy has been scarce. This study has complemented the literature by providing further evidence of how decentralization affects PES office behavior and the labor market outcomes of job seekers. Our results support the cost-shifting hypothesis made in the earlier literature and indicate no positive effects on labor market outcomes. Our results also shed light on how decentralization affected service provision more broadly. We find that municipalities preferred a 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, because municipalities have to penalties to the central government for each long-term unemployed person who fulfills certain criteria. 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 dramatic, as was the fall in employment plans. Thus, it seems that municipalities chose to target the planning efforts of those job seekers who occasion or are about to occasion penalty payments. Second, we observed an increase in activation, specifically in rehabilitative work. Third, we observed a decrease in the probability of long-term unemployment, indicating decreased penalty payments. We further contribute to the cost-shifting discussion by providing approximate calculations of the size of the cost-shifting that occurred during the Finnish temporary reform through reductions in the penalty payments that municipalities must make and calculate what the cost-shifting would amount to if the reform were implemented nationwide.

As we find null effects in employment and earnings, we find no clear benefits resulting from employment service decentralization. Thus, based on this study, the decentralization of PES in the given institutional context should not be expected to increase employment; however, more evidence is needed, as the literature is still sparse, and institutional details and incentives likely influence how this policy affects employment and PES office behavior. If policy makers want to implement PES decentralization reforms, this study suggests that the incentives of local governments should be designed carefully and that the cost-shifting possibilities should be minimized.

References

Ahlin, Å., and E. Mörk (2008). Effects of decentralization on school resources. *Economics of Education Review*. 27 (3), 276-284.

Alasalmi, J., N. Alimov, L. Ansala, H. Busk, V.-V. Huhtala., V. Kekäläinen, P. Keskinen, O.-P. Ruuskanen, and L. Vuori (2019). Työttömyyden laajat kustannukset yhteiskunnalle. Valtioneuvoston selvitys ja tutkimustoiminnan julkaisusarja, 16/2019.

Annala, M., L. Hokkanen, V. Laasonen, J. Pyykkönen, T. Ranta, and K. Sarkia (2019). Työvoima- ja yrityspalvelujen alueellisten kokeilujen toiminta- ja arviointitutkimus. Valtioneuvoston tutkimus- ja selvitystoiminnan julkaisusarja 1/2019.

Baicker, K., Clemens, J., Singhal, M. (2012) The rise of the states: U.S. fiscal decentralization in the postwar period. *Journal of Public Economics*, 96, 1079-1091.

Banzhaf, H.S. and B.A. Fiscal federalism and interjurisdictional externalities: New results and an application to us air pollution. *Journal of Public Economics*, 96, 449-464.

Besley, T. and S. Coate (2003). Centralized versus decentralized provision of local public goods: A political economy approach. *Journal of Public Economics* 87 (12), 2611–2637.

Blundell, R., and M. Costa Dias (2000): "Evaluation Methods for Non-Experimental Data," *Fiscal Studies*, 21(4), 427–468.

Boockmann, B., S. L. Thomsen, T. Walter, C. Göbel, and M. Huber (2015). Should Welfare Administration be Centralized or Decentralized? Evidence from a Policy Experiment. *German Economic Review* 16 (1), 13–42.

Caliendo, M. and S. Kopeinig (2008). Some Practical Guidance for the Implementation of Propensity Score Matching. *Journal of Economic Surveys* 22 (1), 31–72.

Cameron, A., B. Gelbach, and D. Miller (2011). Robust Inference with Multiway Clustering. J*ournal of Business & Economic Statistics* 29(29), 238–249.

Card, D., J. Kluve, and A. Weber (2010). Active labour market policy evaluations: A meta-analysis. *Economic Journal* 120 (548), 452–477.

Card, D., J. Kluve, and A. Weber (2018). What works? A meta-analysis of recent active labor market program evaluations. *Journal of the European Economic Association* 16 (3), 894–931.

Crepon, B., E. Duflo, M. Gurgand, R. Rathelot, and P. Zamora (2013). Do Labor Market Policies Have Displacement Effects? *Quarterly Journal of Economics* 128 (2), 531

Crepon, B. and G. J. van den Berg (2016). Active Labor Market Policies. *Annual Review of Economics* 8 (1), 521–546.

Chabe-Ferret, S. (2017). Should We Combine Difference In Differences with Conditioning on Pre-Treatment Outcomes? Working paper.

Dague, L., DeLeire, T., and Leininger, L. (2017): The Effect of Public Insurance Coverage for Childless Adults on Labor Supply. *American Economic Journal: Economic Policy* 2017, 9(2): 124–154.

Faguet, J. P. (2004). Does decentralization increase government responsiveness to local needs? Evidence from Bolivia. *Journal of Public Economics* 88 (3-4), 867–893.

Fougere, D., J. Pradel, and M. Roger (2009). Does the public employment service affect search effort and outcomes? *European Economic Review* 53 (7), 846–869.

Galiani, S., P. Gertler, Schargrodsky E. School decentralization: helping the good get better, but leaving the poor behind. *Journal of Public Economics*, 92, 2106-2120.

FINLEX 916/2012: Laki julkisesta työvoima- ja yrityspalvelusta.

FINLEX 1290/2002: Työttömyysturvalaki.

Greenberg, D. H., Michalopoulos, C., and Robins, P. K. (2003). A meta-analysis of government-sponsored training programs. *ILR Review*, 57(1), 31–53.

Heckman, J. J., H. Ichimura, and P. E. Todd (1997): "Matching as an Econometric Evaluation Estimator: Evidence from Evaluating a Job Training Programme," *The Review of Economic Studies*, 64(4), 605–654

Heckman, J. J., R. J. LaLonde, and J. A. Smith (1999): "The Economics and Econometrics of Active Labor Market Programs," in Handbook of Labor Economics, ed. 17 by O. C. Ashenfelter, and D. Card, vol. 3, chap. 31, pp. 1865–2097. Elsevier, North Holland.

Lipscomb, M., Mobarak A.M. Decentralization and pollution spillovers: evidence from the re-drawing of county borders in Brazil. *Review of Economic Studies*, 84, 464-502.

King, G. and R. Nielsen (2019). Why propensity scores should not be used for matching. *Poltical analysis 27* (4), 1–20.

Kluve, J. (2010). The effectiveness of European active labor market programs. *Labour Economics* 17 (6), 904–918.

Kuntalehti (2020). Kuntien maksamat työttömyydensakkomaksut nousukiidossa. https://kuntalehti.fi/uutiset/tyottomyyden-sakkomaksut-rajussa-nousukierteessa/

Lundin, M. and P. Skedinger (2006). Decentralisation of active labour market policy: The case of Swedish local employment service committees. *Journal of Public Economics* 90 (4-5), 775–798.

Martinez-Vazquez, J., S. Lago-Penas, and A. Sacchi (2017). the Impact of Fiscal Decentralization: a Survey. *Journal of Economic Surveys* 31 (4), 1095–1129.

Mergele, L. and M. Weber (2020). Public employment services under decentralization: Evidence from a natural experiment. *Journal of Public Economics* 182 (2), 104–113.

Mosley, H. (2011). Decentralization of Public Employment Services. Analytical Paper. The European Commission Mutual Learning Programme for Public Employment Services.

Mosley, H. (2012). Accountability in Decentralised Employment Service Regimes. OECD Local Economic and Employment Development (LEED) Papers 2012/10.

Mörk, E., L. Ottosson, U. Vikman (2021). To work or not to work? Effects of temporary public employment on future employment and benefits. IFAU Working Paper 2021:12.

Nieminen, J., O. Kanninen, H. Karhunen (2021). Mitä työllisyyden kuntakokeiluista voidaan oppia? *Yhteiskuntapolitiikka*, 1/2021.

Oates, W. (1972). *Fiscal federalism.* New York: Harcourt Brace Jovanovich.

Oates, W. (1999). An Essay on Fiscal Federalism. *Journal of Economic Literature* 37 (3), 1120–1149.

Salinas, P., and A. Sole-Olle (2018). Partial Fiscal Decentralization Reforms and Educational Outcomes: A Difference-in-Differences Analysis for Spain. *Journal of Urban Economics*, Vol. 107, 31-46.

Valtakari, M., R. Arnkil, J. Eskelinen, M. Kesä, M. Mayer, J. Nyman, T. Ålander (2019). Työttömien määräaikaishaastattelujen arviointi. altioneuvoston selvitysja tutkimustoiminnan julkaisusarja 2019:26.

Weingast, B. R., K.A. Shepsle, and C. Johnsen (1981) The Political Economy of Benefits and Costs: A Neoclassical Approach to Distributive Politics. *Journal of Political Economy* 89(4), 642–664.

Online Appendix to: “The Decentralization of Public Employment Services and Local Governments’ Responses to Incentives”

Jeremias Nieminen (UTU), Ohto Kanninen (Labore), Hannu Karhunen (Labore)

Online Appendix A. Trends in key variables

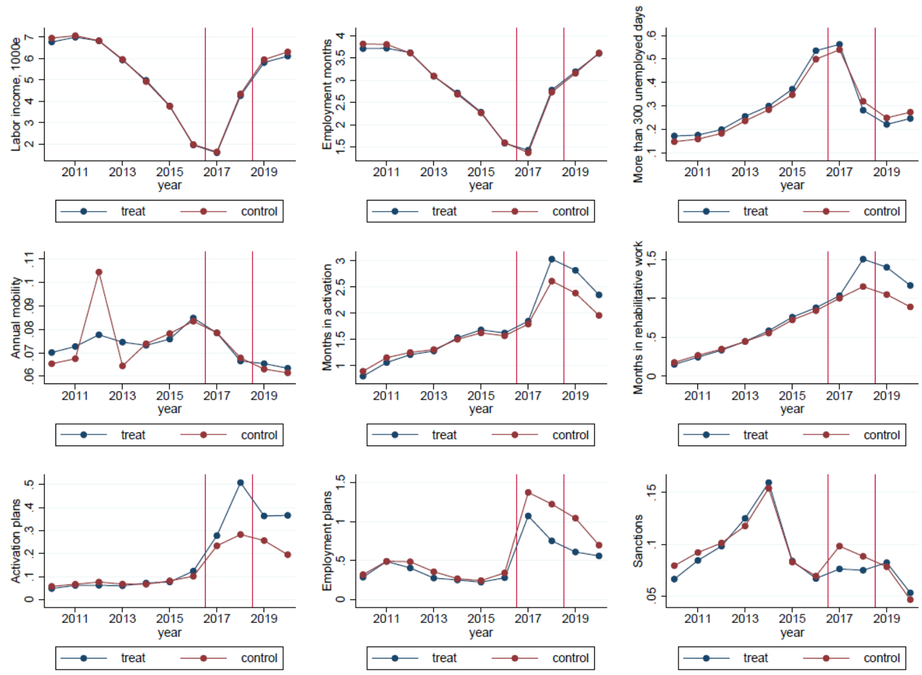


Figure A1. Trends in key variables

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.

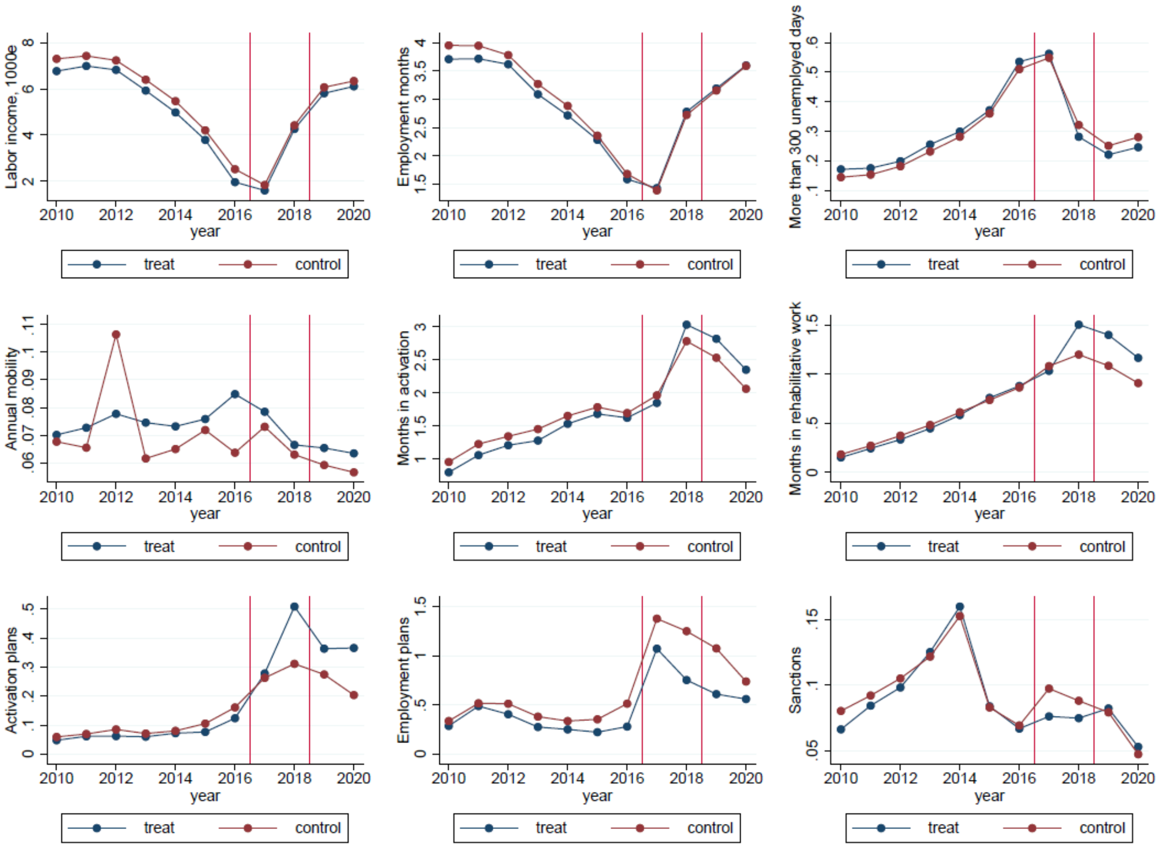


Figure A2. Trends in key variables (no pretreatment outcomes in matching)

Notes. Figure depicts the levels of our key outcomes in treatment and control groups when pre-treatment outcomes are not used in matching.

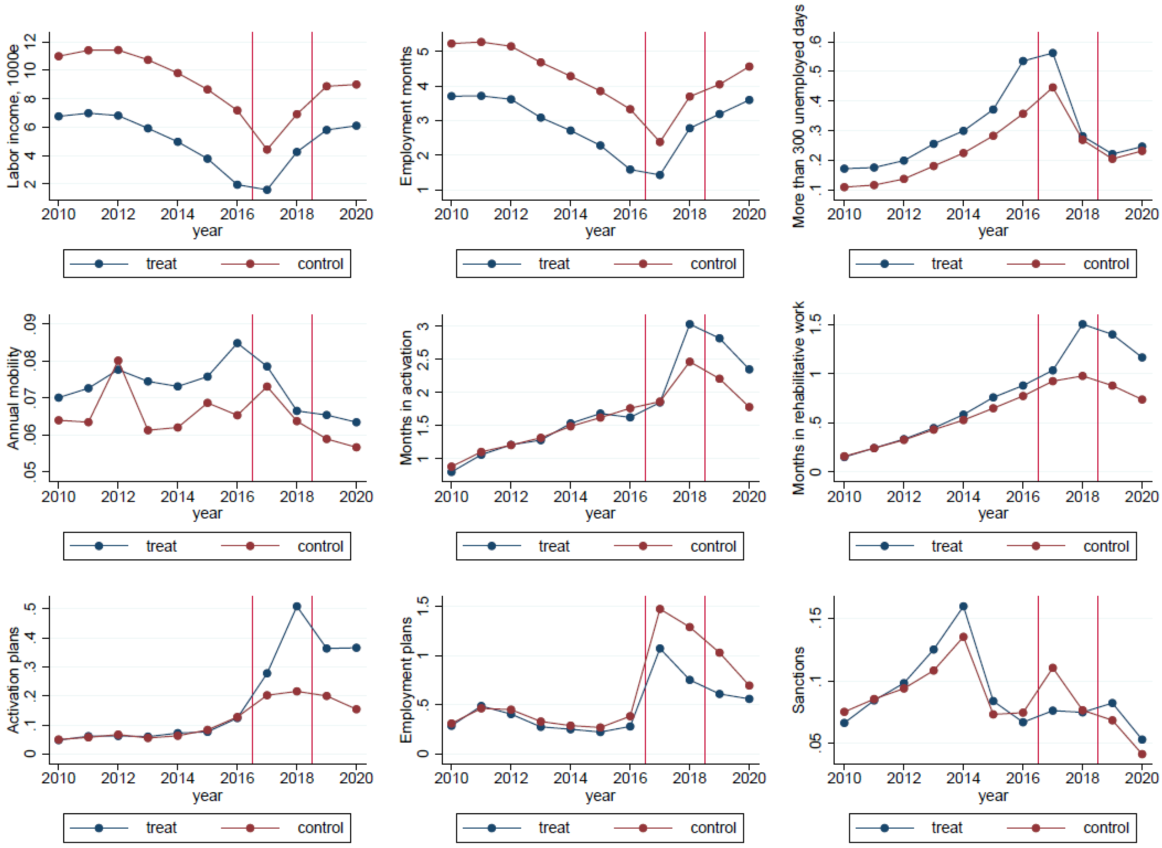


Figure A3. Trends in key variables (no matching)

Notes. Figure depicts the levels of our key outcomes in treatment and control groups when pre-treatment outcomes are not used in matching.

Online Appendix B. Alternative specifications

Basic DiD estimates

Table B1: Labor market outcomes

	(1) Labor income	(2) Employment months	(3) >300 days in registered unemployment per year	(4) Mobility
Treatment effect	51.25	0.0905	-0.0542**	0.00128
	(639.6)	(0.259)	(0.0245)	(0.0286)
Treatment group mean, 2016	1946.6	1.581	0.537	0.085
Control group mean, 2018	4301.0	2.707	0.322	0.062
N	697176	697176	697176	697176
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 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. Minimum detectable effect sizes, taking account clustering, are 0.994 months for employment months, 2731,98 for labor income, and 0.072 percentage points for annual mobility.

Table B2: Plans conducted by the office

	(1) Activation plans	(2) Employment plans	(3) All plans
Treatment effect	0.231**	-0.429***	-0.198**
	(0.0968)	(0.101)	(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 include 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.

Table B3: 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.

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 B4. Labor market outcomes, alternative matching procedures

	(1) DiD+PSM 1:1, excluding pre-treatment outcomes	(2) DiD+PSM with replacement including pre-treatment outcomes	(3) DiD+PSM with replacement excluding pre-treatment outcomes	(4) DiD+CEM 1:m, excluding pre-treatment outcomes	(5) DiD+CEM 1:m, including pre-treatment outcomes	(6) Entropy balancing
Outcome: Annual labor income						
Treatment effect	403.8	72.54	449.4	344.7	621.1	-34.83
	(620.5)	(625.7)	(610.6)	(624.1)	(1593.7)	(620.6)
Outcome: Employment months						
Treatment effect	0.216	0.126	0.251	0.0951	0.433	0.0860
	(0.238)	(0.243)	(0.231)	(0.230)	(0.671)	(0.231)
Outcome: More than 300 days in registered unemployment						
Treatment effect	-0.0547**	-0.0503**	-0.0528**	-0.0435*	-0.0326**	-0.0494*
	(0.0250)	(0.0250)	(0.0295)	(0.0263)	(0.0143)	(0.0261)
Outcome:Annual mobility						
Treatment effect	0.00444	0.000933	0.00471	-0.00683	-0.0120	-0.00349
	(0.0287)	(0.0283)	(0.0286)	(0.0262)	(0.0241)	(0.0253)
N	697200	610248	608688	2158416	131436	3069288
No of individuals	58100	50854	50724	179868	10953	255774
Individual FE	yes	yes	yes	yes	yes	yes
Year FE	yes	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 B5. Plans conducted by the office, alternative matching procedures

	(1) DiD+PSM 1:1, excluding pre-treatment outcomes	(2) DiD+PSM with replacement including pre-treatment outcomes	(3) DiD+PSM with replacement excluding pre-treatment outcomes	(4) DiD+CEM 1:m, excluding pre-treatment outcomes	(5) DiD+CEM 1:m, including pre-treatment outcomes	(6) Entropy balancing
Outcome: All plans						
Treatment effect	-0.174**	-0.171**	-0.171**	-0.228**	-0.223***	-0.281***
	(0.0767)	(0.0757)	(0.0757)	(0.101)	(0.0799)	(0.107)
Outcome: Employment plans						
Treatment effect	-0.391***	-0.391***	-0.391***	-0.483***	-0.362***	-0.530***
	(0.100)	(0.0991)	(0.0991)	(0.134)	(0.0932)	(0.142)
Outcome: Activation plans						
Treatment effect	0.217**	0.219**	0.219**	0.256***	0.139**	0.250**
	(0.0971)	(0.0964)	(0.0964)	(0.0952)	(0.0688)	(0.0977)
N	464800	405792	405792	1438944	87624	2046192
No of individuals	58100	50724	50724	179868	10953	255774
Individual FE	yes	yes	yes	yes	yes	yes
Year FE	yes	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 B6. Active labor market policies, alternative matching procedures

	(1) DiD+PSM 1:1, excluding pre-treatment outcomes	(2) DiD+PSM with replacement including pre-treatment outcomes	(3) DiD+PSM with replacement excluding pre-treatment outcomes	(4) DiD+CEM 1:m, excluding pre-treatment outcomes	(5) DiD+CEM 1:m, including pre-treatment outcomes	(6) Entropy balancing
Outcome: All ALMP months						
Treatment effect	0.310	0.420	0.241	0.322	0.338	0.385
	(0.343)	(0.342)	(0.324)	(0.319)	(0.241)	(0.345)
Outcome: Wage subsidies, months						
Treatment effect	0.0444	0.0436	0.0367	0.00793	0.0727	0.0129
	(0.0383)	(0.0411)	(0.0391)	(0.0321)	(0.0658)	(0.0325)
Outcome: Wage subsidies (municipality), months						
Treatment effect	0.0293	0.0309	0.0215	0.00590	0.0265	-0.000791
	(0.0396)	(0.0414)	(0.0408)	(0.0361)	(0.0470)	(0.0364)
Outcome: Months in rehabilitative work						
Treatment effect	0.319	0.392	0.325	0.362	0.279	0.354
	(0.306)	(0.297)	(0.304)	(0.289)	(0.188)	(0.297)
Outcome: Months in studying with unemployment benefit						
Treatment effect	-0.0254	-0.00251	-0.0916**	-0.0236	0.0123	0.0291
	(0.0562)	(0.0557)	(0.0442)	(0.0320)	(0.0290)	(0.0516)
Outcome: Coaching						
Treatment effect	-0.0188*	-0.0156	-0.0205*	-0.0219*	-0.0121	-0.0165
	(0.0108)	(0.0105)	(0.0108)	(0.0113)	(0.0135)	(0.0108)
Outcome: Training						
Treatment effect	-0.0104	-0.00324	-0.0115	-0.00829	0.0301	0.00934
	(0.0276)	(0.0169)	(0.0315)	(0.0154)	(0.0244)	(0.0138)
Outcome: Work trials						
Treatment effect	-0.000634	0.00454	-0.00736	-0.00405	-0.0443*	-0.00491
	(0.0153)	(0.0149)	(0.0202)	(0.0154)	(0.0257)	(0.0156)
N	697200	610248	608688	2158416	131436	3069288
No of individuals	58100	50854	50724	179868	10953	255774
Individual FE	yes	yes	yes	yes	yes	yes
Year FE	yes	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 B7. Labor market outcomes, twoway clustered SEs

	(1) Labor income	(2) Employment months	(3) >300 days in registered unemployment per year	(5) Mobility
Treatment effect	51.25	0.0905	-0.0542***	0.00128
	(131.0)	(0.0606)	(0.0107)	(0.0129)
Treatment group mean, 2016	1946.6	1.581	0.537	0.085
Control group mean, 2018	4301.0	2.707	0.322	0.062
N	697176	697176	697176	697176
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 8 since we have 8 years in the estimation sample. The specification is otherwise the same as in our main tables.

Table B8. 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 B9. Months in ALMPs, two-way 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.

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
Outcome: *Annual labor earnings*	353.2168***	58.3991	6.048	< 0.001
Outcome: *Annual mobility*	-0.0168***	0.0024	-7.0028	< 0.001

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

Instrumental variables results

Table B11. First stage

First stage:

	(1) Treatment status
Eligibility status	0.904***
	(0.00747)
F statistic for weak identification	14645.21
N	697176
Individual FE	yes
Year FE	yes

Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01"

Table B12. IV estimates

Page 1/2

Income	
Treatment effect	56.71
	(708.0)
Employed months	
Treatment effect	0.100
	(0.287)
More than 300 unemployed days over 300d	
Treatment effect	-0.0600**
	(0.0276)
All ALMP	
Treatment effect	0.472
	(0.378)
Subsidized employment	
Treatment effect	0.0639
	(0.0405)
Subsidized municipal employment	
Treatment effect	0.0453
	(0.0442)
Rehabilitative work	
Treatment effect	0.420
	(0.333)
Studying	
Treatment effect	0.0125
	(0.0595)
Mobility	
Treatment effect	0.00141
	(0.0317)
Sanctions	
Treatment effect	-0.0104
	(0.00897)
Housing allowance	
Treatment effect	-55.23
	(71.84)
Income support	
Treatment effect	62.12

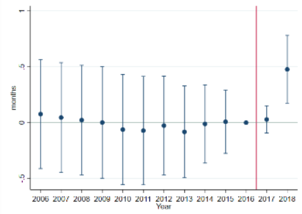
	(52.80)

Page 2/2

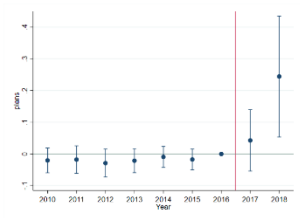
All plans	
Treatment effect	-0.219**
	(0.0875)
Employment plans	
Treatment effect	-0.475***
	(0.115)
Activation plans	
Treatment effect	0.256**
	(0.109)
Coaching	
Treatment effect	-0.0179
	(0.0120)
Training	
Treatment effect	-0.0115
	(0.0227)
Work trials	
Treatment effect	0.00343
	(0.0163)
N	697176
Individual FE	yes
Year FE	yes

Including only municipalities that applied to the Pilot

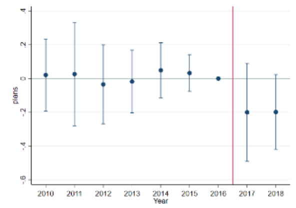
Figure B13: Event study estimates where the control group is matched individuals in non-accepted applicant municipalities



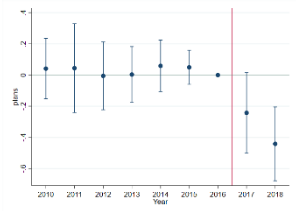
All ALMPs



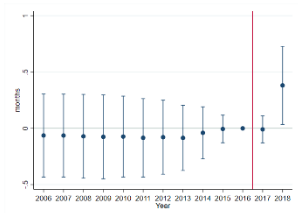
Activation plans



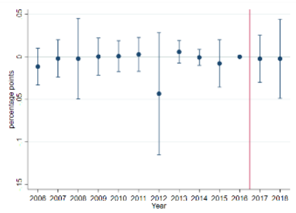
All plans



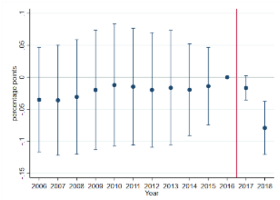
Employment plans



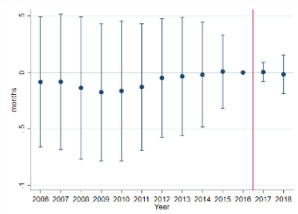
Rehabilitative work



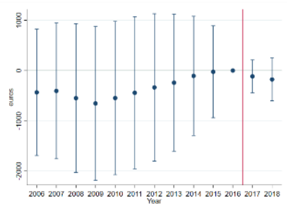
Mobility



Long-term unemployed



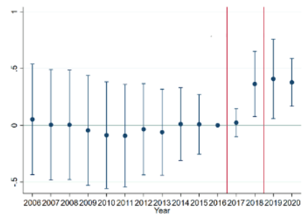
Employment months



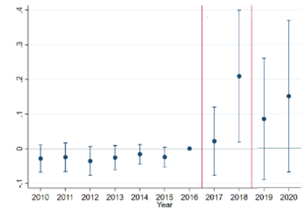
Annual earnings

Dropping those control municipalities that participated in the regional-level co-operation pilot program that was organized partially at the same time with the decentralization pilot

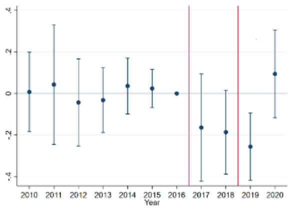
Figure B14: Event study estimates where individuals from Keski-Suomi, Pohjois-Karjala and Keski-Pohjanmaa are dropped from the control group



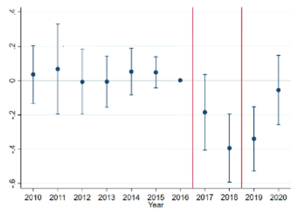
All ALMPs



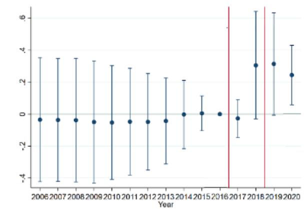
Activation plans



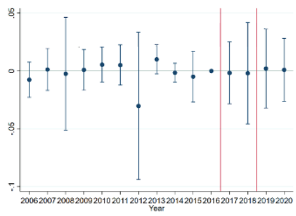
All plans



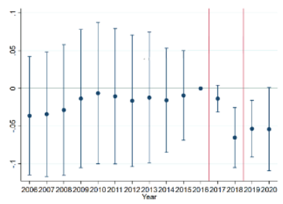
Employment plans



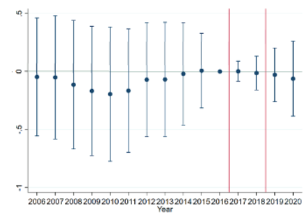
Rehabilitative work



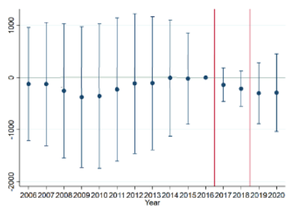
Mobility



Long-term unemployed



Employment months



Annual earnings

Online Appendix C. Placebo and spillover estimates

Table C1 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. Placebo difference-in-differences (placebo treatment year: 2015)

	With 2015		Without 2015	
	(1)	(2)	(3)	(4)
	PSM, including pretreatment outcomes in matching	PSM, excluding pretreatment outcomes in matching	PSM, including pretreatment outcomes in matching	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.

Table C2: Placebo estimates with fake treatment group

Page 1/2

Income	
Treatment effect	-392.7
	(695.4)
Employed months	
Treatment effect	-0.0697
	(0.276)
Unemployed more than 300 days during year	
Treatment effect	0.0150
	(0.0194)
All ALMPs	
Treatment effect	0.0506
	(0.138)
Subsidized employment	
Treatment effect	0.0410
	(0.0470)
Subsidized municipal employment	
Treatment effect	0.0106
	(0.0376)
Rehabilitative work	
Treatment effect	0.0227
	(0.142)
Studying	
Treatment effect	0.0247
	(0.0318)
Mobility	
Treatment effect	-0.0126
	(0.0299)
Sanctions	
Treatment effect	-0.00103
	(0.00718)
Housing allowance	
Treatment effect	108.9
	(114.3)

Income support	
Treatment effect	24.57
	(72.32)
Page 2/2	
All plans	
Treatment effect	0.132
	(0.141)
Employment plans	
Treatment effect	0.130
	(0.182)
Activation plans	
Treatment effect	0.00315
	(0.0653)
Coaching	
Treatment effect	0.00191
	(0.00818)
Training	
Treatment effect	-0.0464*
	(0.0262)
Work trials	
Treatment effect	0.00706
	(0.0273)
N	362352
Individual FE	yes
Year FE	yes

Table C3: Spillover effects for two different populations

	(1) Spillover effects for initially ineligible individuals who are job seekers when the Pilot begins	(2) Spillover effects for non-eligible job seekers in January 2018 who entered unemployment between 11/2017-1/2018 without previous unemployment or ALMP months in 2017
Panel A: Labor market outcomes		
Outcome: Annual income from employment		
Treatment effect	167.9 (1160.6)	1341.0* (750.8)
Outcome: Number of months employed		
Treatment effect	0.100 (0.360)	0.568 (0.377)
Outcome: Probability of more than 300 days in registered unemployment		
Treatment effect	-0.0124 (0.0114)	-0.0107 (0.0128)
Outcome: Annual mobility		
Treatment effect	0.00701 (0.0222)	0.00721 (0.0366)
Panel B: ALMP types		
Months in rehabilitative work		
Treatment effect	-0.169** (0.0810)	-0.0307** (0.0148)
Months in studying		
Treatment effect	0.0333 (0.0420)	-0.0523 (0.0347)
Months in coaching		
Treatment effect	-0.00313 (0.00963)	-0.0112* (0.00614)
Months in training		
Treatment effect	0.0232 (0.0204)	0.0247 (0.0272)
Months in work trials		
Treatment effect	0.0108 (0.0229)	-0.0308* (0.0176)
Wage subsidies, months		
Treatment effect	0.0544* (0.0316)	-0.000243 (0.0286)
All ALMP months		
Treatment effect	-0.0421 (0.109)	-0.116** (0.0551)
Panel C: Plans conducted		
Number of all plans conducted		
Treatment effect	-0.141** (0.0685)	-0.147* (0.0847)
Number of employment plans conducted		
Treatment effect	-0.106 (0.0767)	-0.139 (0.0860)
Number of activation plans conducted		
Treatment effect	-0.0356* (0.0196)	-0.00847 (0.00986)
Panel D: Other outcomes		
Number of sanctions received		
Treatment effect	-0.00155 (0.00391)	0.0192 (0.0120)
Housing allowance		
Treatment effect	-47.46 (86.86)	-82.87 (82.12)
Income support		
Treatment effect	-45.98 (40.59)	1.04 (81.66)
N	603000	70464
No of individuals	50250	5872
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.

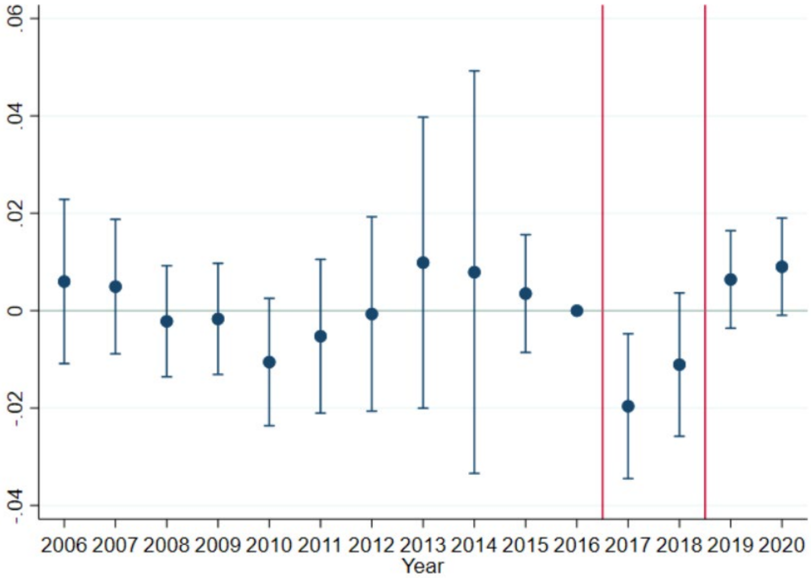
Online Appendix D: Additional outcomes

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)
Outcome: Sanctions		
Treatment effect	-0.00944	-0.0129
	(0.00811)	(0.00808)
Treatment group mean, 2016	0.067	0.067
Control group mean, 2018	0.084	0.084
N	697176	697200
No. of individuals	58098	58100
Individual FE	yes	yes
Year FE	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 program.

Table D2. Transfers paid and received

	(1) DiD (PSM 1:1 with pretreatment outcomes)	(2) DiD (PSM 1:1 without pretreatment outcomes)
Panel A: All transfers paid (incl. taxes)		
Treatment effect	13.26	94.67
	(145.7)	(141.9)
Treatment group mean, 2016	1937.4	1937.4
Control group mean, 2018	2059.6	2059.6
Panel B: All transfers received		
Treatment effect	-35.19	-11.94
	(164.7)	(177.1)
Treatment group mean, 2016	11852.7	11852.7
Control group mean, 2018	11139.2	11139.2
N	697176	697200
No. of individuals	58098	58100
Individual FE	yes	yes
Year FE	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 program.

Table D3. Social benefit types

	(1)	(2)
	DiD (PSM 1:1 with pretreatment outcomes)	DiD (PSM 1:1 without pretreatment outcomes)
Panel A: Housing allowance		
Treatment effect	-49.91	-25.77
	(65.14)	(63.33)
Treatment group mean, 2016	1773.3	1773.3
Control group mean, 2018	1849.7	1849.7
Panel B: Income support		
Treatment effect	56.13	66.69
	(47.48)	(51.24)
Treatment group mean, 2016	1207.5	1207.5
Control group mean, 2018	1182.4	1182.4
Panel C: Unemployment benefits		
Treatment effect	-115.3	-207.9**
	(100.7)	(105.6)
Treatment group mean, 2016	7353.6	7353.6
Control group mean, 2018	5727.0	5727.0
Panel D: Sickness benefit		
Treatment effect	22.30	16.22
	(17.34)	(16.18)
Treatment group mean, 2016	210.41	210.41
Control group mean, 2018	298.80	298.80
N	697176	697200
No. of individuals	58098	58100
Individual FE	yes	yes
Year FE	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 program.

Online Appendix E: Approximating the size of cost-shifting using individual level data

In the main text, we presented the actual effect on penalty payments using municipal level data. In this section, we try to approximate cost-shifting using individual level data. To do this, we proxy belonging to the penalty list with long term unemployment (having more than 300 days in unemployment during a year).

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 5, Column 3) in the probability of having more than 300 unemployment days per year (a proxy for being on the penalty list), we calculate that if the amount of cost-shifting in treated areas would amount to 6.7-7.5 million, depending on assumptions made[10]

There are a number of reasons why the amount calculated with individuals level data (around 7 million) differs from the municipal-level estimate of cost-shifting. 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. Using municipality level data, we find that on average, decentralization decreases these payments by 450 000 euros per municipality, or 10 % in relative terms, 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).

[10] 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, when calculating the lower limit for cost-shifting, we have: 0.17*9353*12*703*0.5 = 6.7 million.

Online Appendix F: Matching tables and figures

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

Page 1/3

	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 labor income 2014	0.0536	0.04116	0.000	0.0536	0.05587	0.229
Missing value for labor income 2015	0.03869	0.03053	0.000	0.03869	0.04059	0.242
Missing value for labor 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.

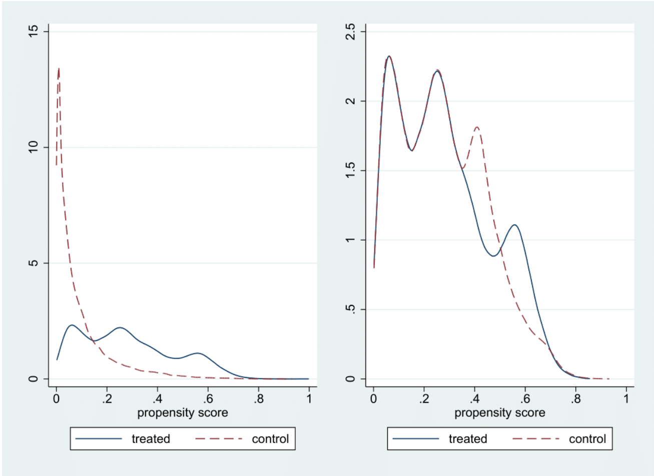


Figure F1. Propensity score density before and after matching (main specification)

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.

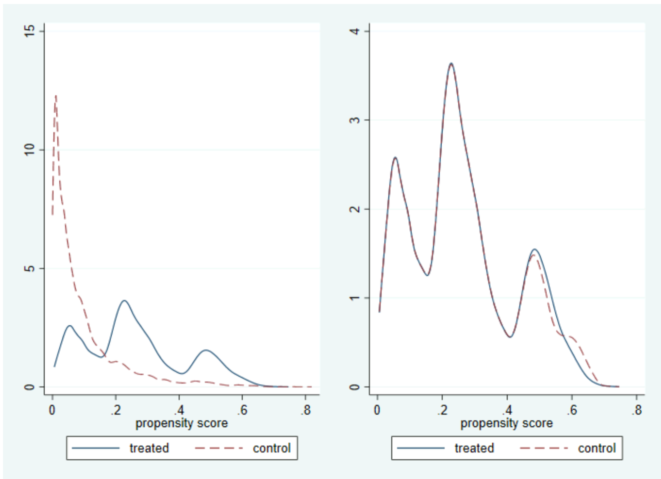


Figure F2. Propensity score density before and after matching (no pretreatment outcomes in matching)

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.

Online Appendix G: Eligibility and participation

Table G1: 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.

Työn ja talouden tutkimus Labore
on vuonna 1971 perustettu itsenäinen ja voittoa tavoittelematon kansantalouden asiantuntijayksikkö. Laitoksessa tehdään taloustieteellistä tutkimusta ja laaditaan suhdanne-ennusteita. Lisäksi laitoksen tutkijat toimivatulkopuolisissa asiantuntijatehtävissä sekä osallistuvat aktiivisesti julkiseen talouspoliittiseen keskusteluun. Laboren toiminnan tavoitteena on tarjota tutkimustietoa yhteiskunnallisen keskustelun sekä päätöksenteon tueksi.

Laboressa tehtävän tutkimustyön painopiste on tilastollisiin aineistoihin perustuvassa empiirisessä tutkimuksessa. Sen taustalla on vahva teoreettinen näkemys ja tieteellisten menetelmien asiantuntemus.

Työn ja talouden tutkimus Labore
Arkadiankatu 7 (Economicum)
00100 Helsinki
Puh. +358 40 940 1940
labore.fi

ISBN 978-952-209-199-4
ISSN 1795-1801 (pdf)