Journal of Economic Geography, 2023, 1–20
https://doi.org/10.1093/jeg/lbad027
Original Article

The decentralization of public employment services and local governments' responses to incentives

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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 use difference-in-differences, utilizing a Finnish temporary reform during which PES were decentralized for specific groups of job seekers in treated municipalities and remained centralized for others. The reform presented the treated municipalities with the possibility of shifting costs to the central government. 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.

Keywords: Public employment services, cost-shifting, fiscal federalism, decentralization

JEL classifications: H11, H75, J48, J64

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, 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 harbor ambitions other than improving the level of employment. Given the opportunity, local governments may use their increased power to simply 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. It could also lead to the lower labor mobility if the aim of the municipalities is to retain individuals 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 is scarce (see Mergele and Weber, 2020; Lundin and Skedinger, 2006; Mörk et al., 2021 and Boockmann et al., 2015 for relevant studies). Evidence of the effects of specific policies in different countries is needed to gauge the optimal level and type of PES decentralization.

In this article, we provide quasi-experimental evidence of the effects of PES decentralization in Finland. In the setting we study, the treated municipalities were given temporary authority to arrange

Received: 7 August 2022. **Editorial Decision:** 11 September 2023. **Accepted:** 27 September 2023

PES. The unintended consequence of this temporary reform was that the treated municipalities could shift part of the costs of the unemployment benefits granted to long-term unemployed job seekers to the central government. We study service provision for job seekers and later labor market outcomes, with a focus on possible cost-shifting behavior. Our main contribution is the measurement of the municipalities' cost-shifting behavior in detail. This article is also, to our knowledge, 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 with no improvement in labor market outcomes. Costs are shifted by transferring long-term unemployed individuals to services that suspend penalties that the municipalities would otherwise have to pay for each individual registered as long-term unemployed. These penalty payments constitute 50–70% of the individuals' unemployment benefits, which are otherwise paid by the central government.[1] 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.

Regarding the mechanisms, we measure PES behavior with the use of plans conducted for job seekers and ALMP placements. Employment offices conduct employment and activation plans periodically to guide the job seekers in the job search process, for example, through job search tasks or by directing job seekers to ALMPs. We find that the decentralized offices reduced the number of plans conducted in total and changed focus toward activation plans. Activation plans are made primarily for the long-term unemployed and are required for placements in certain types of ALMPs, such as rehabilitative work. Regarding ALMPs, our results suggest that the treated municipalities increase the use of ALMPs, and increased the share of rehabilitative work programs in particular. Finally, administratively registered long-term unemployment decreases by five percentage points (17%), which is directly responsible for reducing penalty payments. Our results indicate that main reason for this decrease is municipalities moving individuals from registered unemployment to ALMPs, such that they are not administratively considered to be long-term unemployed anymore.

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 effect on employment months per year, annual labor earnings or geographical mobility. Thus, our finding of a decrease of long-term unemployment appears to be simply a shift in administrative category and not due to real employment gains. Our results (PES behavior change, decrease in the registered long-term unemployment, no effect on employment) are consistent with a municipal focus on cost-shifting, not employment.

Our work touches on three separate strands of literature: the one studying the effects of decentralization of central government functions,[2] as well as the strands focusing on PES (see e.g., Fougere et al., 2009) and ALMPs (see, e.g., Card et al., 2010, 2018; Kluve, 2010; Crepon and van den Berg, 2016). In particular, we expand the existing but scarce research on the PES decentralization and cost-shifting.[3] 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. We examine municipal cost-shifting behavior more in detail compared with Mergele and Weber (2020), as they only focused on service provision while we also measure

[1] We describe this system in detail in the Subsection 2.3.

[2] 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 and Mörk, 2008; Galiani et al., 2008; Salinas and Sole-Olle, 2018), environmental policy (see e.g., Banzhaf and Chupp (2012) and Lipscomb and Mobarak (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. (2017).

[3] 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; Besley and 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 and Weber, 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.

the municipal cost structure (i.e., penalty payments) directly. To complement earlier findings, we evaluate the amount of cost-shifting while explaining how local authorities change their behavior and procedures in practice. Our results regarding labor market outcomes differ from and complement the negative employment effect estimated by Mergele and Weber (2020), since they looked at a different outcome: the job-finding rate. A nonexistent effect on labor mobility is consistent with the earlier results obtained by Mergele and Weber (2020) and Lundin and Skedinger (2006), dissipating 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.

This article 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 concludes.

2. Institutional background

2.1. PES 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

Public employment offices conduct three types of plans for the job seekers: employment plans, activation plans and integration plans. According to the official guidance, employment plans should be conducted every 3 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 3months 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 article was called the Regional Pilot of Employment and Enterprise Services (in Finnish: *työvoima- ja yrityspalveluiden alueellinen kokeilu*).[4] 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 article, we focus on outcomes related to employment and service provision, as we have no data on entrepreneurship. During the temporary 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.

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

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

[4] 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).

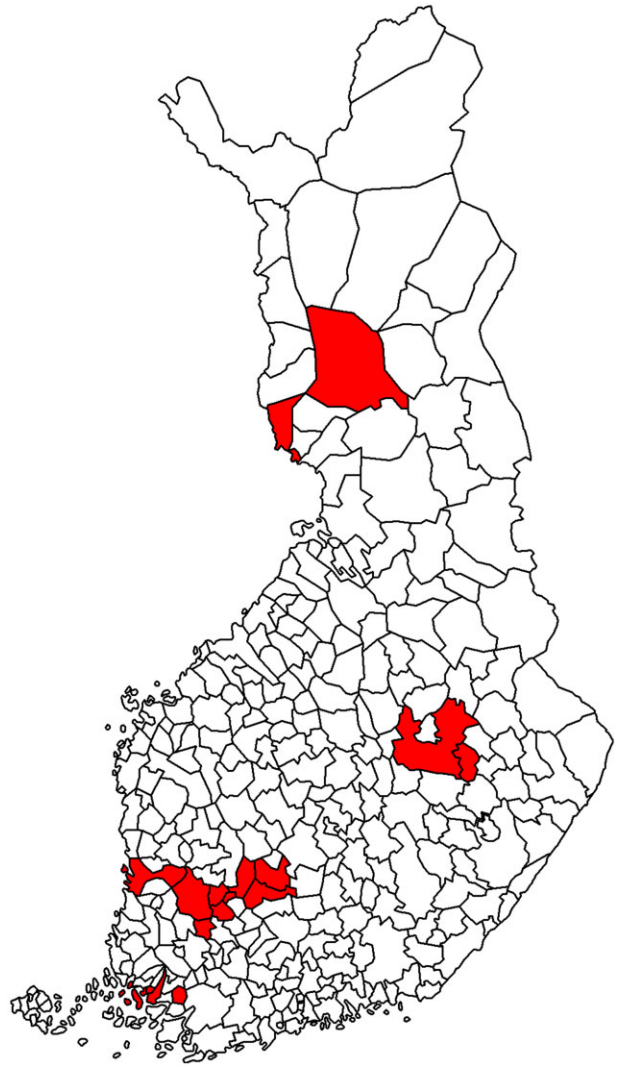

Figure 1. Participating municipalities.

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.

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.

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 (DiD) 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 Supplementary Appendix F), the

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)	15,650.9	43,373.9	41,863.6	2.48**	−0.066

Note: Source of the table: own calculations using register data on the whole Finnish population. Significance levels: * $t < 0.1$; ** $t < 0.05$; *** $t < 0.01$.

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

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 (Annala 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 1000 days (FINLEX 1290/2002). We refer to these unemployment benefit costs the municipality has to pay as 'penalty payments'. Municipalities 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.[5] Moreover, ALMP costs are mostly paid by the central government.

During the temporary decentralization, the treated municipalities could potentially decrease penalty payments by increasing ALMP participation of any type, regardless of whether those they believe it would help the job seekers employed. 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 article. 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

[5] The only factor that completely nullifies the unemployment days counter is working 6 months full-time.

type of unemployment benefit depends on unemployment fund membership status (can be observed imperfectly), unemployment duration and whether the individuals fulfill 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 Supplementary Appendix, where we strive 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 article 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.[6]

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 2808 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 in such a setting would be problematic because different individuals would begin treatment in different months.

Table G1 in Supplementary 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.[7] 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 Table B.11 in Supplementary Appendix B for first-stage results and Table B.12 in Supplementary 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 Supplementary 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

[6] 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.

[7] 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).

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

3.2.1. *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 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 DiD with matching is used (Chabe-Ferret, 2017).

We use one-to-one propensity score matching (PSM; see Caliendo and Kopeinig, 2008) as our matching algorithm. The balance of matching covariates before and after matching is shown in Supplementary 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 Supplementary 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 PSM 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 Supplementary Appendix B.

3.2.2. *DiD*

Our main specification uses a standard, individual-level DiD method to estimate the intention-to-treat effects.[8] This is done by estimating two-way fixed-effects regression models in the matched sample. See e.g., Heckman et al. (1997, 1999) and Blundell & Costa Dias (2000) for work on related conditional DiD estimators. 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 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

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

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 DiD estimates calculated for each time period. We also estimate the basic DiD model, the results of which we show in the Supplementary Appendix. With individual and year fixed effects, the model can be written as

$$Y_{it} = \gamma_i + \lambda_t + \delta(\text{treat}_i * \text{post}_t) + \varepsilon_{it}. \quad (2)$$

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

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 DiD estimate.

In addition to requiring the parallel trends assumption to hold, the DiD 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 Supplementary 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 first calculate municipal-level DiD estimates, which are presented in Figure 2. Panel A of Figure 2 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 five percentage point decrease in the probability of long-term unemployment (Figure 2C) 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).

Unfortunately, our microdata do 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 Supplementary Appendix E for the calculations in which we strive to approximate cost-shifting using individual-level data.

4.2. Mechanisms of cost-shifting: plans conducted and ALMP placement strategies

Next, we attempt 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

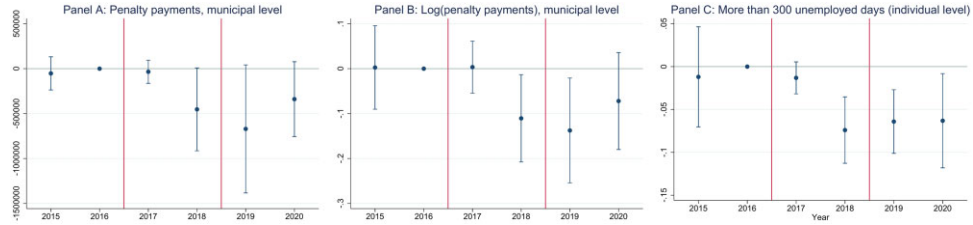

Figure 2. Cost-shifting: municipal- and individual-level evidence.

Notes: Panels A and B present municipal-level DiD 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).

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 with the control group mean of 1.5 in the treatment year, a decrease of 13% (see Supplementary 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 3 months but can be updated even more frequently. Furthermore, we see that decentralized offices favored different types of plans compared with 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 (Figure 3B) and in all other specifications (see Supplementary Appendix Table B2 for basic DiD estimates and Supplementary 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 in Figure 3 showing yearly treatment effects. The magnitudes of the effects on plans are quite sizable when compared with 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.

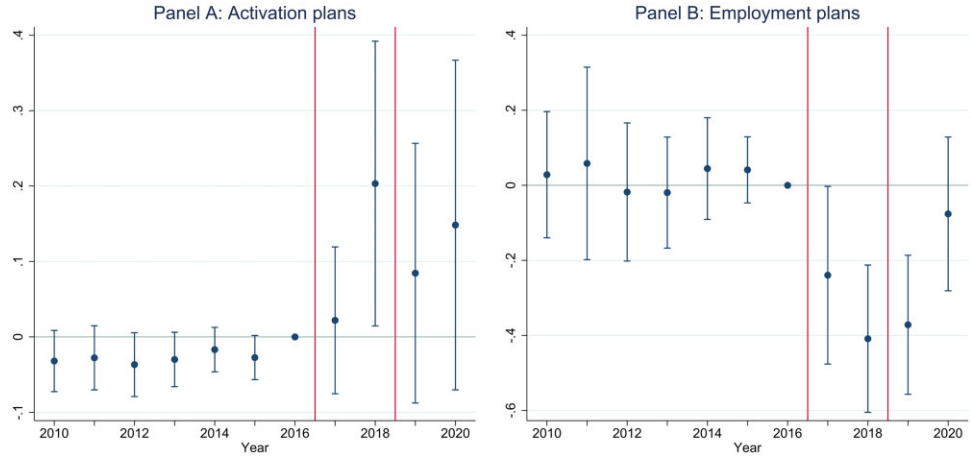

Figure 3. Activation and employment plans.

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 Supplementary Appendix Table B2, in which we present basic DiD estimates for plans.

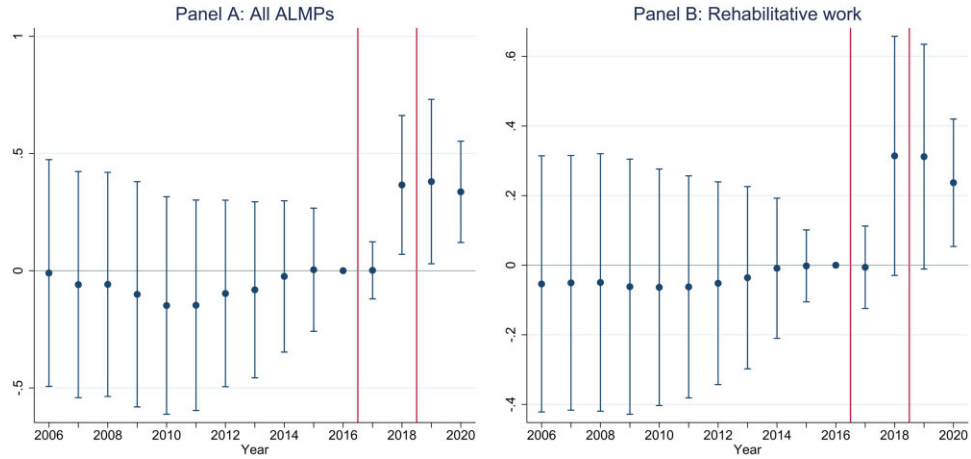

Figure 4. Months in all ALMPs and months in rehabilitative work.

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 Supplementary Appendix Table B3, in which we present basic DiD estimates for ALMPs. Supplementary Appendix Table B3 also shows the effects on ALMP types other than rehabilitative work.

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 and 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.

Figure 4A 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 Supplementary 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.

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 five percentage points (17%) regarding the probability of having more than 300 days in registered unemployment per year.

Our finding suggests 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 (9353) 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 6 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 DiD (event study) estimates of the effects on the labor market outcomes (earnings and employment) as well as the effects on the administrative measure of long-term unemployment and annual mobility. 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 DiD estimates (Supplementary Appendix B, Table B1) 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 Supplementary Appendix Table B1, 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 with 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

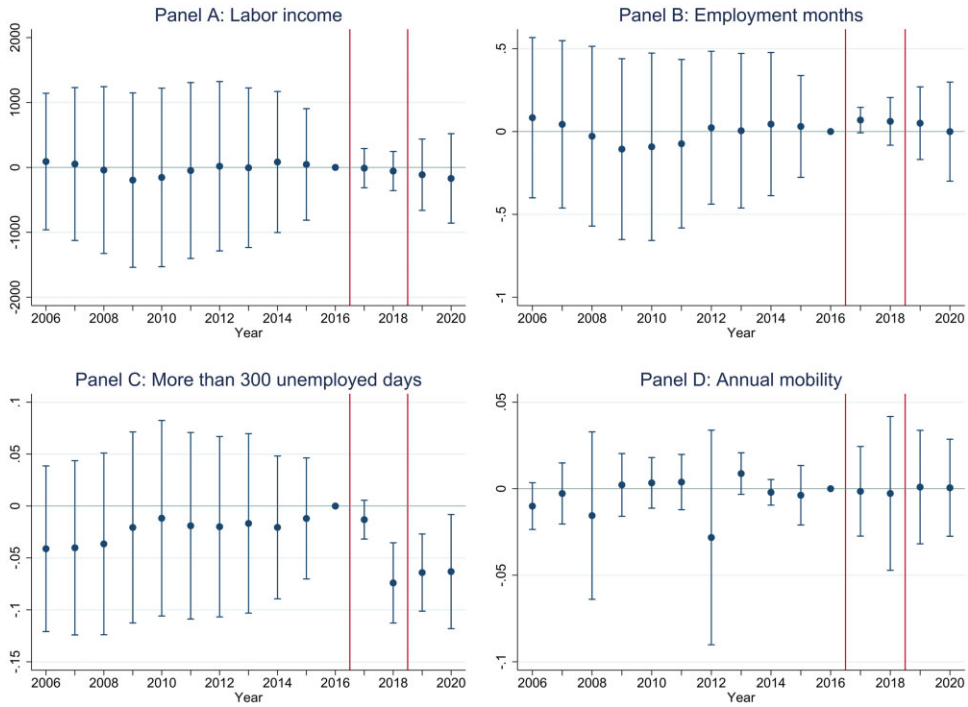

Figure 5. Labor market outcomes, measured long-term unemployment and mobility.

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 Supplementary Appendix Table B3, in which we present basic DiD estimates for labor market outcomes.

2017. Owing to matching, the levels in the treatment and control groups are also similar, as shown for all main outcome variables in Supplementary 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.

Supplementary Appendix Table B1 presents the effects on labor market outcomes from a basic DiD 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.

Figure 5C reports estimates of long-term unemployment, defined as having more than 300 days in registered unemployment during the year. We find a significant six 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 with 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 Supplementary Appendix B. No effect can be seen in 2017, which is again expected because the reform did not start until August 2017.

Finally, we present the effects on annual mobility in Figure 5D. 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 Supplementary 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

4.4.1. *Doubly robust DiD*

Our main results are robust to using the doubly robust DiD estimator (DR DID) proposed by Sant'Anna and Zhao (2020), whose method is an improved, more robust version of DiD combined with matching. We report the DR DID results for our main outcome variables in Supplementary 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.

4.4.2. *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 with 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 Supplementary 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.

4.4.3. *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 Supplementary 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.

4.4.4. *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 2 years prior to the actual treatment. The placebo results in Table C1 in Supplementary Appendix C show that there are no placebo effects, except for wage subsidies (1 of 17 outcomes). In the second set of placebo

regressions (Supplementary Appendix C, Table C2), we use the fake treatment group and the real treatment period. Except for training, no placebo effects are found.

4.4.5. *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 DiD analyses as our main specification, but noneligible 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 30 September 2018, meaning that individuals who become unemployed between November 2017 and January 2018 cannot accumulate more than 365 consecutive days of unemployment before 30 September 2018. The control group consists of similar matched individuals in control municipalities.

The estimates of the spillover effects, presented in Supplementary Appendix Table C3, 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 noneligible 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 5-year mean for 2010–2014 for a rough figure. First, wage subsidies cost around 9000 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 Supplementary 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 Supplementary 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 Supplementary Appendix Figure D1 and Supplementary 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 et al., 2003; Card et al., 2010, 2018; Kluve, 2010; Crepon and 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 2–3 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 (2020) 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 (an administrative measure), which is consistent with cost-shifting, because municipalities have to pay penalties to the central government for each long-term unemployed person who fulfills certain criteria. The reduction in this administrative measure of long-term unemployment does not mean that the individuals would have gotten employed—instead, as our estimations suggest—they have likely been placed in ALMPs, and especially, in rehabilitative work programs. Municipalities may, for example, target individuals who are registered as long-term unemployed or those that are about to be placed there. 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.

Acknowledgments

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 article. We also thank the OP-Pohjola Foundation for the data cost funding (grant number: 20220153). Previous versions of this article have been circulated as Labour Institute for Economic Research Working Paper Nr. 332 and Nr. 337.

Supplementary material

Supplementary data for this paper are available at *Journal of Economic Geography* online.

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