UNIVERSITY OF TURKU

ABSTRACT

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Abstract

In this thesis, I investigate the effects of decentralized public employment services (PES) on employment and participation in active labor market policies. Decentralized provision of employment services could be more effective than central provision, since local offices could take into account local preferences and conditions (Oates, 1972). There is, however, a trade-off: municipalities may try to shift costs to the central government or to or direct job seekers to less effective activation programs (Mergele & Weber, 2020).

I analyze the effects of decentralized employment services utilizing two large-scale municipal employment trials conducted in Finland during 2012–2015 and 2017–2018. In the first trial, 61 municipalities arranged complimentary employment services to job seekers, while the centralized employment offices were still responsible for the basic services. In the second trial, 23 participating municipalities were given responsibility to arrange all employment services to certain target groups of job seekers, who can be identified from data. The second trial answers the question whether municipalities were better at arranging job services than the centralized offices, whereas the first trial is utilized to answer whether the additional decentralized services had employment effects.

In the analysis of the first trial the treated unit is the municipality, since the participants cannot be identified. In three specifications, I find an effect of 0.5 percentage points on employment, but the effect is not significant in three other specifications. No effects on unemployment are found in any specifications. This could mean that the trial increased employment of individuals outside the labour force.

The second trial either increased or had no effects on *extended unemployment*, depending on area. The probability of registered unemployment decreases in some areas, but this is offset by increased activation, resulting in no effect or an increase in extended unemployment. This means that the second trial mainly increased participation in ALMPs without increasing employment in the 1.5-year period for which data is available. In addition to this, I find evidence that municipalities may try to optimize their own budgets in expense of the central government: in Pirkanmaa area, I find a sizable, statistically significant increase in rehabilitative instruction. Additionally, I find a negative effect on sanctions in Pirkanmaa and Varsinais-Suomi. Both of these are channels municipalities may use to optimize their budgets. Overall, decentralization does not have the favourable employment effects

Key words	public employment service, decentralization, fiscal federalism, employment program
Further information	-

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DECENTRALIZED EMPLOYMENT SERVICES AND ACTIVE LABOR MARKET POLICY

Evidence from Finnish municipal employment trials

Master's Thesis
in Economics

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Acronyms

ALMP active labor market policy

ATT average treatment effect on the treated

DiD difference-in-differences

Kela The Finnish Social Insurance Institution

PES Public employment services

PSM propensity score matching

TEM Ministry of Employment and the Economy

Glossary

common agency model

principal-agent model with many principals

ELY-centre

Centre for Economic Development, Transport and the Environment

informational advantages

the local government having more information about local residents

local public good

a public good that is consumed locally

lock-in effect

decrease in labour mobility

Oates Decentralization Theorem

Theorem from Oates (1972) arguing that decentralization is beneficial if there are no externalities

TE-palvelut

Finnish employment offices

1 Introduction

Employment is important to the welfare of individuals in many ways: in addition to being crucial for financial well-being, employment is also linked to better health outcomes[1] and social relationships[2]. As the benefits of increased employment are sizeable, ways to get more people employed are highly sought after: in Finnish public debate, increasing the employment rate is often emphasized as one of the most important objectives of public policy. There are many ways in which the employment rate could be enhanced by policy, such as the design of unemployment benefit system, social safety net in general, or active labor market policy. In this thesis, I study how decentralized employment services affect employment and participation in active labor market policies. I answer this question by utilizing two Finnish experiments, called municipal employment trials, where municipalities were given responsibility to arrange public employment services.

Public employment services (PES) are agencies where the unemployed register as job seekers and employers register their open vacancies. The objective of PES is to make the matching process in job search more effective, i.e. to make job seekers and employers find each other faster and more easily. Most countries offer these services and they are an important part of job search for many. (Holzner and Watanabe, 2015).

The most obvious task of PES is to match job seekers to employers, but often, as in Finland, important tasks for PES include directing job seekers to active labor market policies (ALMP) as well as making employment and activation plans. Participation in these programs is mandatory for the job seeker; job seekers who decline to participate lose their unemployment

[1] see e.g. Ross and Mirowsky (1995)

[2] see e.g. Pohlan (2019)

benefits. (TE-palvelut, 2020a). Finnish employment agencies also give a statement (in Finnish: *työvoimapoliittinen lausunto*) about every job seeker to The Finnish Social Insurance Institution (Kela) or the individual's unemployment fund, indicating whether the individual qualifies for unemployment benefits (TE-palvelut, 2020b).

The effects of using PES in job search are not that well understood, but the services are hypothesized to reduce search frictions and lower wages (Holzner and Watanabe, 2015). Theoretically, the effects of PES have been analyzed by Pissarides (1979), who built a search model with both registered vacancies and random search. He finds that when the costs of using random search are decreased, job matching increases and steady-state unemployment decreases. When, in turn, the costs of using public employment services decrease, the effects are ambiguous. Therefore, the conclusion from Pissarides (1979) is that governments should encourage random search instead of using public employment services, if faster job finding is the objective.

Empirically, the evidence on the effects of PES is not very conclusive. Fougère et al. (2009) investigate whether public employment services affect search outcomes and find that public employment services are beneficial: they find that exits from unemployment increase when more jobs are contacted using PES. Regarding the effects on wages, Addison (2002) and Holzer (1988) find that job offers accepted through PES have lower wages. There is also evidence to the contrary: Weber and Mahringer (2008) as well as Osberg (1993) find that using PES has no effects on wages.

The provision of public employment services can be centralized or decentralized. Examples of decentralized employment service regimes are Canada, Denmark, Belgium and the Netherlands (Mosley, 2012). Many other countries have also experimented with or adopted decentralization in employment services, including Germany, Italy and Sweden (Mergele and Weber, 2020). According to Mosley (2012), in some countries with decentralized employment services the federal government is still very involved in providing employment services: in Belgium, for example, the central government uses activation interviews. In Finland, the central government is currently responsible for employment services. Decentralized employment services have, however, been experimented twice in Finland: the first trial was conducted in 2012–2015 and the second in 2017–2018. These two policy experiments are utilized in this thesis to investigate the effects of decentralized PES.

The basic research question of this thesis is: are decentralized employment offices better at arranging employment services compared to centralized employment agencies? This question can be answered especially by looking at the effects of the second trial, since in that trial the responsibility to arrange employment services was shifted to municipalities for some job seekers. The results from the first trial, in turn, can be used to assess whether complimentary municipal employment services have employment effects.

Evidence on the effects of PES decentralization is limited; the most credible study on this topic is the paper by Mergele and Weber (2020), who studied the decentralization of job offices in Germany. Other notable papers include Lundin and Skedinger (2006) and Boockmann et al. (2015), but they investigate somewhat different programs. Additionally, a short program duration of only three months is a problem in Lundin and Skedinger (2006).

Although the Finnish municipal employment trials are not completely similar to the German reform, and the used methods are different[3], this

[3]This thesis uses individual level data, while Mergele and Weber (2020) use job office level data in their main analyses. While both this thesis and their paper use a difference-in-differences strategy, the specifications and main outcome variables are different.

thesis can be thought of as a replication of the research by Mergele and Weber (2020) in Finnish context: the theoretical background and some of the outcomes studied here are similar to those in Mergele and Weber (2020). As Mergele and Weber (2020) was the first credible evidence on the effects of PES decentralization, this thesis answers an important question, which has not yet been fully answered: more empirical evidence is needed in order to make policy recommendations.

In Chapter 2, I present the Finnish institutional background and give descriptions of the municipal employment trials studied in this thesis. In the description of the trials, I also discuss the earlier evaluations of these trials. In Chapter 3, I discuss the theoretical reasons why decentralized employment services could have advantages over a centralized system or vice versa. The theory of decentralization is ambiguous about the effects: on one hand the municipalities know the local conditions and job seekers well and may care more about placing them in jobs, but on the other hand the objectives of local authorities may differ drastically from the ones of the central government (Mergele and Weber, 2020).

In Chapter 4, previous empirical research is reviewed. First, I present the earlier findings on employment service decentralization and then the empirical research on decentralization in general. After that, I discuss briefly the empirical literature on active labor market policies, since decentralization of PES is hypothesized to affect the types of ALMP where job seekers are directed. Chapter 5 describes the data and Chapter 6 introduces the matching and difference-in-differences methods used in this thesis.

In the empirical section of this thesis I conduct an empirical impact evaluation of two Finnish policy experiments: the municipal employment trial of 2012–2015 and the regional employment trial of 2017–2018. These programs were called trials, despite the lack of randomization. Because the municipalities participating in these programs were not chosen at random, selection bias likely occurs. That is, the municipalities that participated in the trials might be very different from those that did not, which makes evaluation harder. Additionally, the participants in the treated municipalities (in the 2017–2018 program) were for the most part those who had the worst employment prospects (i.e. the long-term unemployed). To overcome the selection bias, I use matching to create more balanced treatment and control groups and difference-in-differences to calculate the treatment effects.

The trials I study are different from each other: in the first trial, conducted between September 2012 and December 2015, municipalities arranged complimentary employment services, but the centralized employment office was still responsible for arranging the basic employment services. In the second trial, conducted between August 2017 and December 2018, some job seekers in treated municipalities were transferred from the centralized employment agencies to the municipality, making it possible to compare outcomes of the treated individuals to those of matched control individuals.

Because the trials are not similar, different outcomes are expected to be affected. For instance, ALMP participation is not expected to rise in the first trial, since direction to ALMP was not a responsibility of municipalities in the first trial. In the second trial, however, it was, so effects on activation are expected in the later trial. Because of this, effects on ALMP participation as a whole and participation in specific services are investigated in the analysis of the 2017–2018 trial. The results are discussed in relation to ALMP literature and predictions made based on the incentives of municipalities. Both trials had an objective of increasing employment. Hence, effects on employment are of interest in the context of both trials.

2 Institutional background

2.1 Finland

In Finland, public employment services are organized by the employment offices (TE-palvelut), which are coordinated by ELY centres (Centre for Economic Development, Transport and the Environment). There are 15 ELY centres in Finland and they are responsible for executing the central government's policies e.g. in areas of transportation, the environment, and employment[4]. Ministry of Employment and the Economy (TEM) controls ELY centres, so at the moment arranging employment services is a responsibility of the central government.

There are 15 employment agencies in Finland, one in every region. According to the law about public employment services in Finland (FINLEX 916/2012), the tasks of public employment agencies are to help the labor market work efficiently, promote employment and economic growth, and increase labor supply. According to the law, the employment agencies should offer job placement services, advising, services to increase human capital as well as services to help starting a business. Employment agencies are also responsible for arranging active labor market services and directing job seekers to these services. (FINLEX, 2012).

Active labor market policies play an important role in Finnish society: hundreds of millions of euros are spent every year on these policies. Finland spends more on active labor market policies than most countries: in 2014, Finland spent 1 percent of its GDP on these services (OECD, 2016). This results in approximately 4 percent of labor force participating in ALMP services in Finland (Crépon and van den Berg, 2016). Despite the generous

[4]TEM (2020). Url: `https://tem.fi/ely-keskukset`, last accessed: 6 May 2020

funding for ALMP, the funding for PES is not very high according to OECD (2016).

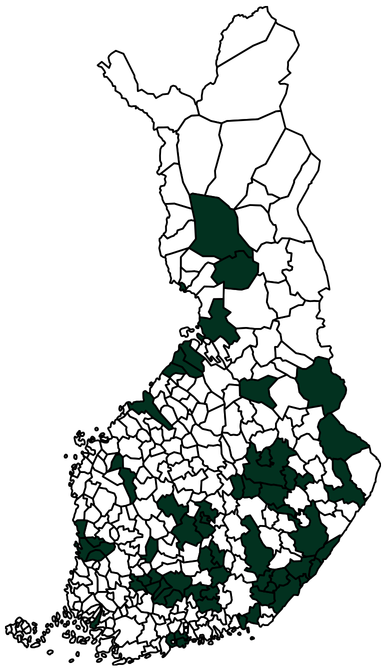
Following the lead of Canada, Belgium and others (Mosley, 2012), there has been discussion in Finland about making the municipalities take responsibility for arranging employment services. This change is believed by politicians and officials to both increase the activation rate and enhance the employment prospects of job seekers. The government even considers the decentralization of employment services as a tool to increase the employment rate. The interest in decentralizing employment services has led the Finnish government to conduct two large-scale experiments, which are utilized in this thesis. The details of the trials are discussed in the next subchapters.

2.2 2012–2015 municipal employment trial

In the first trial, conducted between September 2012 and December 2015, municipalities arranged complimentary employment services, but the responsibility to arrange the regular employment services (e.g. direction to activation) was still on the employment agencies. The municipalities offered a wide range of services that supported learning and co-operation with corporations. 61 municipalities participated in the first trial. (Arnkil et al., 2015). The participating municipalities are presented in Figure 1 and they are also listed in the Appendix C. The best cost estimate for the first trial is 30 million euros, which was the amount of funds granted for the trial (Arnkil et al., 2015).

The official report by Arnkil et al. (2015) did not attempt to analyze the causal effects of the trial. The effects of the first trial have only been analyzed by Nieminen et al. (2020). They used municipal-level data and studied effects of the trial using matching and difference-in-differences. They found no statistically significant effects on unemployment or activation rates. The treated municipalities were matched to untreated municipalities on propensity score, which was calculated on population, unemployment and number of people in activation. The matching was performed in the period before the trial started on a cross-section dataset.

Figure 1: Treated municipalities, 2012-2015 trial



Notes. Treated municipalities are colored. The map is created using open geodata from Statistics Finland and R packages `rgdal` and `sp`.

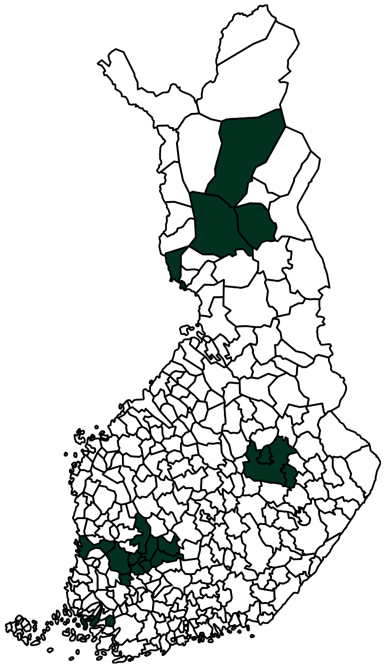
2.3 2017–2018 regional employment trial

The second program utilized in this thesis is the Regional Employment Trial (in Finnish: *Työvoima- ja yrityspalveluiden alueellinen kokeilu*). In this trial, there were 5 regions where 23 municipalities assumed the responsibility of arranging employment services (Annala et al., 2019). The difference to the 2012–2015 trial is that in the later trial there was actually a shift in responsibility: the municipalities started arranging all employment services to certain target groups, which can be identified from the data, unlike in the first trial. The target groups are presented in Table 1. Because of the shift in responsibility, the later trial is the most interesting of these two trials and hence will be the in the focus of this thesis. The participating municipalities are presented in Figure 2 and listed in Appendix C.

Table 1: Target groups by area, 17-18 trial

Area	Target group
Varsinais-Suomi	Job seekers under 25 and job seekers who have been unemployed over a year
Pirkanmaa	Job seekers on basic unemployment allowance
Pohjois-Savo	Unemployed over 12 months and have not been in activation services during that time
Lappi	Unemployed over 12 months
Pori	Those under 25 who have been unemployed over 6 months and those over 25 who have been on labor market allowance for over 200 days

Figure 2: Treated municipalities, 2017-2018 trial



Notes. Treated municipalities are colored. The map is created using open geodata from Statistics Finland and R packages `rgdal` and `sp`.

The effects of the 2017–2018 trial have not been analyzed on the individual level before (except an attempt in Varsinais-Suomi area) and hence this thesis is the first attempt to quantify these effects rigorously. Previously Arnkil et al. (2019) have studied the effects of the 2017–2018 regional program in Pirkanmaa area. They comparied matched control municipalities to treated Pirkanmaa area municipalities using municipal level data. They did not find effects on extended unemployment rate (meaning the sum of unemployed and those in activation services). The analysis, however, had some issues: first, the matching performed was not successful in creating treatment and control groups that would have had parallel trends in unemployment rate before treatment. Secondly, they ignored that similar programs were in place in some municipalities that were used as controls. For example, Turku was used as a control municipality for Tampere, while Turku was also participating in its own regional program.

Nieminen et al. (2020) have also studied the program using municipal level data and a similar approach as Arnkil et al. (2019) used in their evaluation of the Tampere Area trial. They found that the second trial increased activation ratio significantly, 5 percentage points, but the effect on unemployment was not significant.

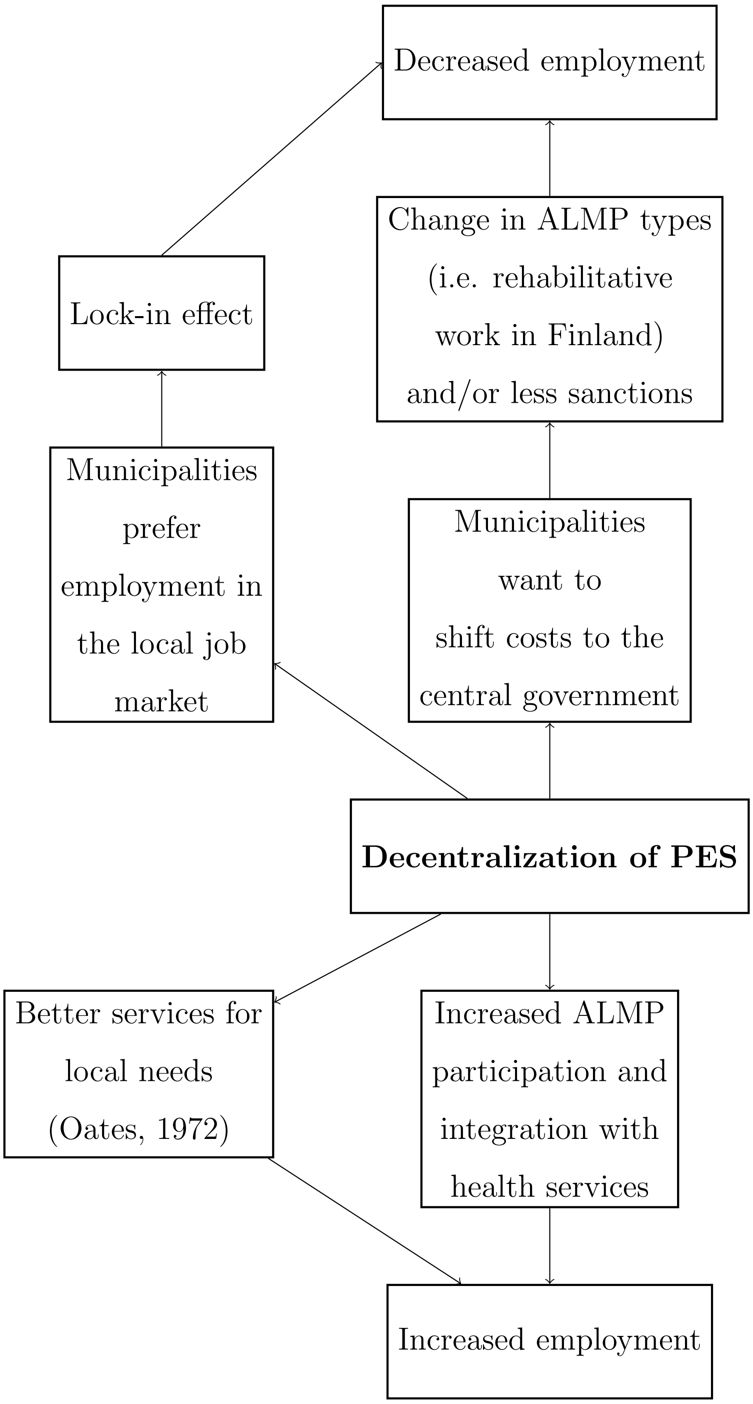
The only previous analysis using also individual level data is the report by Ylikännö et al. (2019). They did both a municipal level analysis as well as individual level analysis in Varsinais-Suomi area, but the individual level analysis is somewhat confused: they attempt to do difference-in-differences analysis using a logit specification, but they do not show pre-trends. Their finding was that unemployment prevalence was 4 percent higher in the treatment area after the start of treatment. They do not find statistically significant effects for the exits from unemployment or for the length of unemployment spells.

3 Theoretical background

The theoretical framework of this thesis is centered on the notion that while municipalities can be better at arranging employment services and may care more about placing job seekers in jobs, there is a trade-off caused by municipalities having different objectives than the central government: municipalities may e.g. try to shift costs to other levels of the government (Mergele and Weber, 2020). This is not the only trade-off in decentralization; issues to consider when assessing the benefits and costs of decentralizing employment services include the possible externalities or economies of scale (Oates, 1972), competition between areas (Tiebout, 1956), sharing of costs (Besley and Coate, 2003) as well as accountability (Tommasi and Weinschelbaum, 2007).

Because the Finnish municipal employment trials were similar to the German reform studied by Mergele and Weber (2020), the theoretical framework of this thesis is similar to the one they use. Mergele and Weber (2020) mentioned informational advantages as a main mechanism why decentralized employment offices could be more efficient than centralized offices. They mentioned lock-in-effect, change in ALMP types, and incentives of local politicians to change sanctioning policies in the hope of re-election as reasons why decentralization of employment services may have harmful effects. All of these mechanisms and related literature are discussed in this chapter. Additionally, I consider increased activation and increased integration of employment and health services as possible mechanisms affecting outcomes of participants. The key elements of the theoretical framework are illustrated in the Figure 3.

Figure 3: Theoretical framework



3.1 Fiscal federalism

When designing public policies, governments have to decide at which level of governance (i.e. centralized or decentralized) they want to arrange public services or to provide public goods. The literature that addresses the question about the optimal level of government to provide public services is the fiscal federalism literature. The fiscal federalism theories can be divided to earlier and later generations: the earlier literature was mostly focused on the efficiency gains of decentralization, resulting from different preferences in different areas. (Martinez-Vazquez et al., 2017).

In the second generation fiscal federalism literature, there are other issues besides efficiency gains and losses that come into consideration. The newer fiscal federalism theories focus on the aspects of political economy: politicians are seen as selfish actors, who have their own objectives. This is in contrast with the first generation literature, where governments were viewed as well-minded actors. (Martinez-Vazquez et al., 2017). Weingast (2009) provides a good review of the second generation fiscal federalism theories.

3.1.1 Local preferences

One reason why municipalities could be better at arranging employment services is that municipalities can take local conditions and preferences better into account compared to a centralized employment agency. This argument about decentralization making it possible to differentiate policies according to the preferences in different areas was first introduced by Oates (1972), who studied decentralization in a setting where the central government would offer the same level of service to all areas, whereas decentralization would make it possible to provide different services to different areas.

The key result from Oates (1972) is called the Oates Decentralization Theorem. The theorem is still one of the most important results in the theory of decentralization. The Oates Theorem argues that decentralization is beneficial if different areas have different preferences and there are no externalities. In addition to these assumptions, the Oates theorem assumes that the central government would offer the same policy for all areas, meaning that the central government could not differentiate the services by area.

As was later argued in Oates (1999), the uniform provision assumption can be realistic, since decentralized governments have informational advantages: they know the residents in their area better than the central government, which makes them able to take the heterogeneous preferences into account. According to Besley and Coate (2003), the assumption about uniform provision in the centralized regime is, however, not very realistic: they argue that there are no theoretical or empirical reasons why the central government would not differentiate the policies by area. Also, the Oates Decentralization Theorem is only valid when there are no economies of scale in the production of the service: if there were, the centralized provision of the service could be preferred despite the welfare loss from not providing different outputs to different areas (Oates, 1972).

Oates (1972) argues that the central government should arrange public services that are likely to have big interjurisdictional externalities. Employment services are not likely to be this type of services, and therefore the Oates theorem suggests decentralized PES should be better than centralized PES. Additionally, Oates (1972) suggests that the optimal level of provision depends on how different the preferences are in different areas and how homogenous the areas are inside: if the districts are very different from each other and homogenous inside, decentralization is better. When it comes to

Finnish municipalities, the differences between large cities and rural Finland are big in terms of education level, population and age distribution[5]. Using Oates (1972), this would mean that decentralization in employment services could be beneficial.

3.1.2 Mobility of job seekers

Tiebout (1956) introduces an argument that individuals can move to an area where the provision of local public goods is what the individual wishes. This results in competition between districts. If employment services were decentralized, a job seeker could move to a municipality which has the best suited employment services for that individual, which would lead to greater social welfare. In case of employment services, the users of the service are unemployed, which may weaken the Tiebout's argument; the unemployed may not be willing to move to another municipality even if the services there were better.

Mobility of job seekers is related to the question of employment service decentralization also in other ways. Labour mobility is an outcome where the municipality may have different objectives than the central government: municipalities may want the job seekers to try finding a job in the local area, even if it means that the job search may take a longer time. The central government, however, could prefer that the job seeker finds a job faster, maybe in another area with shortages of labour. This mechanism is called *lock-in-effect* in the literature and it is mentioned as one of the possible disadvantages of employment service decentralization by Mergele

[5] For example, Helsinki has over 600 000 inhabitants while the median Finnish municipality has 6080 inhabitants (Kuntaliitto, 2019). In some municipalities over 40 percent of individuals are older than 65 years old, whereas in some other municipalities that share is much smaller (Yle, 2015).

and Weber (2020) and Lundin and Skedinger (2006). The lock-in-effect is usually thought to occur because getting job seekers employed in the same municipality increases the tax revenue of that municipality (Mergele and Weber, 2020).

Besides increasing the local tex revenue, municipalities may also have other reasons to prefer employment in their area. In Finland, especially young people move out of rural areas, which is good for the overall employment rate, but at the same time it is very bad for the municipalities with declining and aging populations. Therefore, the municipalities have incentives to try to stop this trend. Indeed, some municipalities have even offered money for young people to choose to go to school there or to move there: for example, in the municipality of Simo, high school students get free books and a laptop[6]. If municipalities want to decrease mobility, giving them a responsibility to arrange employment services could give them that possibility. This could then lead to worse job finding prospects for job seekers.

3.1.3 Political economy

Besley and Coate (2003) investigate theoretically whether provision of local public goods should be centralized or decentralized using political economy approach. They find that decentralization can be benefial even if the centralized regime could offer different services to different areas. Their approach, therefore, does not assume that the centralized regime would provide the same services in all areas – an assumption made in Oates (1972). Besley and Coate (2003) find that the effects of decentralization depend on spillovers and preferences of the citizens: with high spillovers, centralization can be better despite the political economy concerns. Their conclusion is similar

[6]Source: https://yle.fi/uutiset/3-11158844

to Oates (1972), but there is a difference: when in the earlier literature the main reason to decentralize was the assumption that the central government would offer same service in all areas, in Besley and Coate (2003) the main disadvantage of a centralized system is the conflict of interests between districts, when all costs are shared. Local politicians could make others pay for their spending.

While the conflicts of interest between areas in Besley and Coate (2003) arise in a centralized system with shared costs, it can be argued that local politicians may try to do similar *cost-shifting* in a decentralized system, especially in the case of employment service decentralization: Mergele and Weber (2020) argue based on Besley and Coate (2003) and Weingast et al. (1981) that decentralized employment offices may try to shift costs to the central government e.g. by changing ALMP types. This would lead to conflict of interest between the central government and the local governments: while the central government may want to mimimize the costs to the society as a whole, the local government may just want to minimize the costs they have to pay, not caring what costs have to be paid in the other levels of government. There is evidence that extensive cost-shifting often happens in welfare states with multiple levels of governance: Bonoli and Trein (2016) find that cost-shifting in social programs was rampant in both countries, Switzerland and Germany, that they studied.

Thus, if the possibility to shift cost to other levels of government was presented, it is likely that the municipalities would take advantage of it. For instance, if employment services were decentralized, municipalities could try to change the services such that costs for the municipality would be minimized. As was pointed out by Mergele and Weber (2020), this could mean e.g. changes in ALMP types or in sanctioning policies. Incentives of municipalities regarding ALMP types and sanctioning, in Finnish context, are discussed more in detail in the subchapters 3.3 and 3.4.

3.2 Principal-agent problem

Decentralization can also be thought in the principal-agent framework. There are reasons to believe municipalities are more informed about the local conditions, job market, and job seekers compared to the centralized employment agencies. This would naturally make them better at arranging employment services. The issue is, however, that municipalities may have different objectives than the central government, i.e. they may be more interested in the development of the local economy as opposed to the national economy. This situation is similar to the principal-agent problem[7], where in this case the central government is the principal and the municipality is the agent.

There have been attempts to compare decentralized and centralized provision of public goods using the principal-agent framework. It is, however, used to study a different trade-off than the one described above: Tommasi and Weinschelbaum (2007) use it to answer whether better accountability in a decentralized system makes decentralization desirable even in the presence of externalities. The idea behind their analysis is that decentralization may increase accountability by reducing problems citizens have in controlling the government, called *control problems*. They build a *common agency model* where there are M number of cities, and in all of them a local public good (or a policy) should be produced. In the model there are M goods denoted by $x = (x_1, x_2, ..., x_M)$ and $N = n_1 + n_2 + ... + n_M$ citizens. Each citizen has a type and the citizen of type i gets utility of b_{ii} from their own public good and externality of b_{ij} from other public goods. The citizens are considered

[7]see Sappington (1991)

to be principals and cities are considered to be agents in the model. In the model, a centralized regime is one where only one agent (a city) is hired and the decentralized system is one where many cities are hired.

Tommasi and Weinschelbaum (2007) analyze decentralization by comparing the efforts of the agents (cities) in three settings: the first best, the traditional second best problem and the harder agency problem with more principals. In the first-best case, the centralized provision is preferred: Tommasi and Weinschelbaum (2007) derive that too little effort (less than optimum) is produced as a result of decentralization, whereas in the centralized case the produced effort is at the optimum. In the second-best principal-agent case, which they call the *traditional principal agent probelm*, however, they find that the effort at the centralized regime is not at the optimum either. In the decentralized case, the effort is still smaller than in the centralized case, unless the externalities in consumption are zero.

When Tommasi and Weinschelbaum (2007) consider a harder agency problem with larger number of principals, the effort under the centralized regime is smaller than when there was just one principal. In this case, the effort is lowered by what they call *too many principals effect*. This means that having more principals makes the effort of centralized agents lower. In the decentralized case with many principals there is also the effect of many principals, but it is not as strong compared to the cenralized case. However, with decentralization there is the effect of externalities, which means that in order to compare the two regimes, the externality effect has to be compared to the difference in the many principal effects. By comparing these, Tommasi and Weinschelbaum (2007) get two results: 1) the trivial one, that decentralization is better if there are no externalities and 2) that for a fixed parameter δ, which measures control problems, there always exists a level of externalities, such that decentralization is preferable. This result by Tommasi and Weinschelbaum (2007) implies that decentralization can be preferred even if the preferences are homogeneous in cities, which means that the heterogeneity of preferences needed by the Oates Theorem is not needed to argue for decentralization in the model.

3.3 Incentives of municipalities

Mergele and Weber (2020) argue that local job offices may want to give less benefit sanctions in order to increase local politicians' re-election probabilities. This mechanism could be valid also in Finland, since municipal councils have elected politicians and they can probably influence how employment offices are run in the municipality. As Mergele and Weber (2020) noted, in the local level politicians influencing employment offices is much more a plausible scenario than it would be in the central government level.

There is also a reason specific to Finland why municipalities may want to give less sanctions. The Finnish system is organized in such a way that municipalities bear some costs of social benefits: for instance, municipalities pay 50 percent of costs in housing allowance and social assistance. This has effects on the incentives of municipalities, if they are in charge of employment services: if municipalities give less unemployment benefit sanctions to job seekers, they may have to pay less social assistance or housing allowance. In the empirical section, I study whether municipalities acted according to these incentives (i.e. reduced sanctions) or not. Analyzing the effect of sanctions is similar to Mergele and Weber (2020) who also looked at the effects of employment service decentralization on sanctions.

If the municipalities reduced sanctions, decentralization could increase unemployment, since giving less sanctions could make people less eager to seek work: sanctions are found to increase employment (Mergele and Weber, 2020). This would, however, depend on the actual employment effects of sanctions: if the municipalities were less strict in sanctioning without it affecting employment, this could even be seen as welfare improvement. This is because if it is possible to achieve the same (or higher) employment with less sanctions, giving less sanctions improves the welfare of those who were sanctioned without harming the welfare of others. This could be a possible scenario too, since there is evidence that benefit sanctions reduce the quality of employment: according to Arni et al. (2012), the effect of sanctions on employment durations and wages is negative.

Mergele and Weber (2020) consider ALMP types as a channel for local job offices to shift costs to the central government. In Finland, at least one channel of this kind exists: the rehabilitative work programs (in Finnish: *kuntouttava työtoiminta*). In Finland, municipalities have to pay a fine to the central government for every long-term unemployed person[8]. If, however, the person participates in a rehabilitative work program, the municipality does not have to pay the fine. Therefore, municipalities have clear incentives to increase the number of individuals in rehabilitative work programs. This can then lead to a sizeable increase in the costs the central government has to pay and may also lead to worse job-finding prospects for the unemployed, if these programs are less effective than other programs.

3.4 Activation services

One key mechanism through which decentralization of employment services could affect employment is via the activation services. Compared to centralized employment offices, municipalities could either direct job seekers to

[8]See, for example `https://yle.fi/uutiset/3-9236853`

different types of services than the centralized offices would or direct more job seekers to activation than the centralized offices would. In the empirical section, both of this mechanisms are studied. To see whether municipalities directed more job seekers to activation, I investigate the effects on the probability of participating in activation; to see whether municipalities changed the types of activation, I look at the effects of the trial on probabilities of participation in different types of activation policies.

Mergele and Weber (2020) mentioned different ALMP types as possible ways the local job offices can shift costs to the central government. As mentioned in the subchapter 3.3, in Finland the municipalities benefit financially from directing job seekers to rehabilitative work programs. Those programs are meant to be for individuals in need of rehabilitation, so any substantial increases in them indicate that municipalities are not acting in the best interests of job seekers or the central government, but instead are just optimizing the municipal expenses. Because of this, special weight should be given to the effects of the trials on the participation in rehabilitative work programs.

The activation services that municipalities offer can also be more integrated to other services: in the first trial, one key objective was to offer integrated health and employment services (Arnkil et al., 2015). If the integration with health services or the increased activation in general improve the labor market prospects of individuals, decentralization should result in better employment outcomes for participants in the long term. If employment increased through this mechanism, we would not expect to see the effects in a very short time period, because research has shown that if the ALMPs have positive effects, they are not immediate (Card et al., 2018). Because of this, the time period examined in this thesis may be too short to see these effects.

4 Empirical literature

4.1 Decentralization

The theory of fiscal federalism is ambiguous about the effects of decentralization of employment services, predicting there may be both adverse effects, such as cost shifting, but also favorable effects such as increased employment due to municipalities knowing the local conditions better. Therefore, empirical evidence is needed to address the magnitude of both of these mechanisms. The best way to address this question empirically would be by utilizing natural experiments, where countries change their employment service regimes. Finding credible evidence would, however, require variation in the degree of centralization within a country, similarly to the German reform in Mergele and Weber (2020) or the municipal employment trials studied in this thesis.

Decentralization of employment services has not been studied very extensively in the empirical fiscal federalism literature compared to other things, such as education, public finance or the environment (Mergele and Weber, 2020). Relevant studies include Mergele and Weber (2020), Lundin and Skedinger (2006) and Boockmann et al. (2015). One reason for the lack of empirical studies concerning decentralization of employment services is probably the lack of reforms that would have allowed the study of causal effects ot the reform: in such reforms, some areas would need to stay centralized, in order to provide a counterfactual for the decentralized areas.

The most credible empirical evidence on the effects of employment service decentralization is shown by Mergele and Weber (2020) who studied the German decentralization of job centers using job center level data. The reform they utilized left some job centers centralized while some were decentralized permanently. They compare job finding in decentralized areas to that in centralized areas using a difference-in-differences model with the stock of jobs and stock of job seekers as controls. The specification was derived from Cobb-Douglas form stock-flow matching function. Their analysis indicates negative employment effects: they find a decrease of 0.34 percentage points in job-finding.

Another mechanism Mergele and Weber (2020) study is whether the decentralization of job centers changed the types of activation measures where job seekers were directed. They find that job centers increased direction to job creation schemes by 0.29 percentage points. They interpret this as evidence suggesting that local governments exploited the system in order to shift costs to the central government.

Mergele and Weber (2020) also investigate whether the reform caused a lock-in-effect. This is done by estimating the elasticity of the flows to employment with respect to the vacancies in neighbouring areas. They found no lock-in-effect, and hence conclude that the negative effect on job-finding is not because of decreased labor mobility, but is due to general inefficiency of decentralized employment offices. The results from Mergele and Weber (2020) are very unfavourable to employment service decentralization: they find that decentralized offices are both worse at placing job seekers and are used by local governments to shift local costs to the national budget. Although they do not find a lock-in effect, the results are not very promising.

Lundin and Skedinger (2006) utilize a Swedish program that gave municipalities more responsibility in active labor policies. The Swedish pilot programme Lundin and Skedinger (2006) study is somewhat similar to the first Finnish municipal employment trial (2012-2015), so the study by Lundin and Skedinger (2006) is very relevant especially to the first trial. Similarly to Mergele and Weber (2020), Lundin and Skedinger (2006) do not find a lock-in-effect, but do find support to the cost-shifting concern: they have evidence that suggests municipalities tried to enhance their budgets in expense of the central government. They do not make claims about the employment effects of the program. The duration of the program short; it may have been, according to them, too short for the effects (e.g. the lock-in-effect) to appear.

Because the evidence on employment service decentralization is scarce, it is useful to look at studies that estimate the effects of decentralization in other areas. This can be especially useful when looking at services similar or linked to employment services, such as welfare administration. Boockmann et al. (2015) study the German welfare reform of 2005, where welfare administration was decentralized. Boockmann et al. (2015) find that the decentralization had negative employment effects for males. For females there was no effect. In services that are different from employment services, such as education, more positive effects have been found: Ahlin and Mörk (2008) study the Swedish school decentralization and find that more decentralized school regimes employ more teachers, meaning that they have a higher teacher-pupil ratio. Hence, decentralization increased the quality of the service in education.

In addition to possible employment effects, decentralization may have other effects: since especially the newer fiscal federalism theory emphasizes the political economy side of decentralization, empirical research on outcomes such as corruption is very central in order to understand decentralization. Fan et al. (2009) and Faguet (2004) study this mechanism empirically: Fan et al. (2009) find that there is more corruption in areas with large number of local public employees. This suggests that decentralization may not be desirable, if it means having more people employed by the local authority. Faguet (2004) studies decentralization in Bolivia and finds evidence against the corruption argument by Fan et al. (2009): to the contrary, decentralized areas seem to be more efficient than centralized ones, and the effect is more pronounced in poor areas.

Enikolopov and Zhuravskaya (2007) test empirically a theory that the optimal level of decentralization depends on the level of political decentralization in a country. They use data from 75 countries and find that stronger political parties make decentralization more beneficial. Additionally, they find that appointing local politicians instead of using elections does not improve the outcomes of decentralization.

In light of the empirical evidence by Mergele and Weber (2020), Lundin and Skedinger (2006) and Boockmann et al. (2015) as well as the previous evaluations of the Finnish municipal employment trials, it can be said that the previous empirical research does not support the view that employment service decentralization would increase employment: none of the studies have found significant increases in employment. Whether the effect is negative or zero, however, is not clear. Since the number of studies in this topic is very limited, and only Mergele and Weber (2020) is based on a setting that allows credible causal interpretations of the effects, more empirical research is needed in order to learn about the effects of PES decentralization.

4.2 Active labor market policy

Active labor market policy – the effectiveness of it as well as the types of services job seekers are directed by decentralized offices – is a key mechanism determining the success of PES decentralization. Active labor market policy is a topic that has been studied extensively, historically mainly by non-experimental studies but nowadays more and more randomized controlled trials are conducted in the topic (Card et al., 2018).

In the active labor market policy literature, policies are usually classified into the following categories: training, job search assistance, public sector employment and private sector incentives (Kluve, 2016). According to Kluve (2010) and (Kluve, 2016), public employment programs do not increase employment whereas incentives and wage subsidies have more positive effects. A summary of benefits and costs of different ALMP types by (Kluve, 2016) is presented in Table 2.

Table 2: Effectiveness of ALMP: Summary by Kluve (2016, 6.)

	Job search assistance	Training	Private incentives	Public employment
Cost	Low	Medium	High	High
Short term effect	+	−	+	+
Long tem effect	small (+/−)	large (+) or small (−)	+/−	zero/−
Displacement	medium	low	high	high

According to Crépon and van den Berg (2016), the evidence suggests that active labor market programmes do not have very big effects on employment probabilities: while effects may be positive, the gain from increased employment might be smaller than the costs. It is hard to know, since many evaluations just look at the effect on employment but do not conduct a cost-benefit analysis (Crépon and van den Berg, 2016).

Numerous other meta analyses have also been conducted on the topic. Card et al. (2010) use 199 programs in their analysis and find that public employment programs are less effective than job search assistance or training programs. They find that the long term impacts are usually better than the short term impacts: even programs with negative first-year results, the long term results look usually positive. They also find that different outcome variables give different results. Card et al. (2018) find in their meta-analysis that program impacts are close to zero in the short term and positive in the long term, i.e. 2 to 3 years after the program. Different kinds of programs have different impacts.

Even if active labor market policies had positive effects on the employment of those who participated, they may not have net effects on employment rate. This is because labor market programs may affect the economy as a whole, generating general equilibrium effects. This means that the program affects also the economic environment, not only outcomes of the program participants (Crépon and van den Berg, 2016). Regarding equilibrium effects, active labor market policies have been found to have large displacement effects by Crepon et al. (2013). They used a French double randomized controlled trial (RCT) with different treatment intensities to evaluate both the effect on participants and the displacement effect. They found that the programs had displacement effects resulting in no net gain in employment.

As can be seen from Table 2, which is from Kluve (2016), public employment creation programs have been found to be both ineffective and costly. Similar results are found also in Kluve (2010) and Card et al. (2010). Hence, if the decentralization increases this type of programs (e.g. rehabilitative work programs), the employment effects may be negative in light of the empirical evidence on ALMP effectiveness.

5 Data

5.1 Data description

I use individual level datasets from Statistics Finland (*FOLK* modules) and The Ministry of Economic Affairs and Employment (*TEM* modules). The datasets used in this research are:

- *FOLK perustieto*: basic information about individuals living in Finland, 1987-2018
- *FOLK jaksotieto (työttömyys, sijoitus, työsuhde)*: unemployment periods, work placement periods, employment periods, 1991-2017
- *FOLK jaksotieto (työvoimakoulutus)*: training periods, 2005-2017
- *TEM Työnhaku*: unemployment and activation periods, 1991-2018
- *TEM Työnhakija*: information about unemployed jobseekers, 1991-2018.

The *FOLK perustieto* module contains annual information about all people living in Finland. The *FOLK jaksotieto* modules contain all unemployment periods, activation periods as well as the employment status in the last week of every year. *TEM* datasets include monthly information about job seekers and their unemployment periods and activation periods. Not all types of activation periods are available in the *FOLK* activation periods, so data from *TEM Työnhaku* module is also used when determining which individuals are participating in activation in monthly basis.

The *TEM* modules contain information about job seekers and their unemployment and activation periods. Additionally, the important piece of information of who participated in the later trial is found in the *TEM Työnhakija* module: the module contains the code of the employment office where a job seeker is registered at, allowing me to identify the participants. This is because all five trial areas had their own trial code and the office codes for the participants were changed to these trial area codes. The *TEM* datasets have all data in the form of spells: they have start and end dates for e.g. unemployment periods, activation periods as well as the employment office code. Because the data is in the form of spells, it could be transformed to monthly or even daily data. I use bimonthly data (i.e. data points every two months) in the analysis of the 2017–2018 trial and yearly data in the analysis of the 2012–2015 trial.

The unemployment register data is not very clean: there are e.g. unemployment periods without the exit date despite the fact that there is a later unemployment spell with an exit date. This means that there may be two unemployment spells for the same individual, beginning in the same date, but only one of them has the exit date. The data is full of duplicates, so I clean the data by deleting unemployment spells that overlap, meaning that they have the same start or end date. Because some of the spells are correct and some clearly are not, the procedure I use to clean the unemployment spell data is the following:

- I delete job seekers who are not unemployed, but are laid off.
- I delete complete duplicates (same individual, same start and end dates).
- I delete spells with the same individual and start date, which don't have an end date (keeping the one with an end date). If there is no half-duplicate with an end date, the one without is kept.

- I delete spells where the end date is missing but the unemployment spell is not the last one for the individual.
- For a given end date, I keep only the spell with latest start date.
- For a given start date, I keep only the spell with the first end date.

Similar procedure is done with the activation period data, but the activation data is much cleaner than the unemployment period data, meaning that there are not many duplicates. Using the unemployment periods, I construct a dummy variable indicating whether a person is unemployed in a particular month. A person is considered to be unemployed in a particular month if they are unemployed in the last day of that month. I define being in activation similarly: those who are in activation services in the last day of a month are considered to be in activation in that month.

In the analysis of the 2012–2015 trial, I use yearly data. In the analysis, yearly employment and unemployment statuses are from the last month of every year. For the analysis of the 2017–2018 trial, in turn, I construct a dataset that has unemployment, activation, sanction and treatment statuses bimonthly (every two months) for years 2016–2018 and yearly covariates from the *FOLK* modules. I do the propensity score matching in a cross section dataset, obtained from this dataset by keeping only the data from July 2017.

5.2 Treatment and control groups

Treatment and control groups are defined differently in the evaluations of 2012–2015 and 2017–2018 trials. In the 2012–2015 trial the participants cannot be identified, so the unit of the analysis is the municipality: I investigate the municipal level effects using individual level data. The results can then be compared to Nieminen et al. (2020) who analyzed the trial using municipal level data. Nieminen et al. (2020) created treatment and control groups by matching treated municipalities to non-treated municipalities. In the analysis of the first trial, I use the same treated and control municipalities that were used by Nieminen et al. (2020). These are listed in the Appendix C.

In the 2017–2018 trial, however, participants can be identified, which makes it possible to create individual level treatment and control groups and investigate how their outcomes changed compared to similar individuals who did not participate. The treatment and control groups are created using propensity score matching. The matching is conducted in the month before the start of the trial; the method is described in the chapter 6.2. Additionally, I do specific restrictions to treatment and control groups before the matching procedure in order to achieve more similar groups. These are described here.

In Varsinais-Suomi, I utilize the 25-year-threshold in treatment assignment in two ways. First, I compare under 25-year-old job seekers in Varsinais-Suomi to under 25-year-old job seekers in other areas. Individuals who are treated in other areas are excluded from the control group. I consider these my main results in Varsinais-Suomi. In addition to those main results, I do an analysis inside Varsinais-Suomi: I then use all individuals in Varsinais-Suomi who were born in 1993 as a treatment group, and all individuals in Varsinais-Suomi who were born in 1991 as a control group. No matching is conducted in the second analysis.

In Pirkanmaa area, I compare participants in Pirkanmaa to matched controls elsewhere. I delete observations who are on income-dependent unemployment assistance, because all participants in Pirkanmaa area receive either basic unemployment allowance or labor market subsidy. I could alternatively have matched on that variable, but deleting the observations in this group gets rid of the issue more surely. Since the participation rate in Pirkanmaa area was over 60 percent and the treated and non-treated groups were very different, there is no possibility to do the analysis inside the area: even when trying to match on pre-treatment outcomes and dropping over 50 percent of observations in matching, the trends are not similar when the analysis is done inside Pirkanmaa area, i.e. the Pirkanmaa area participants are compared to Pirkanmaa area non-participants. Therefore, the control individuals have to be found from other areas.

In Pori, I compare job seekers under the of 25 in Pori to non-treated job seekers under the of 25 in other areas. The treated groups in Pori were those under 25 who who had been unemployed for over 6 months. Other groups were treated too, but restricting the treatment group to the job seekers under the age of 25 makes it easier to find a similar control group. In Lappi and Pohjois-Savo, I compare the participants to matched control individuals. I delete individuals who have been unemployed for over 60 months.

6 Methods

6.1 Evaluation problem

In order to evaluate the effectiveness of decentralized employment services, the causal effects of the municipal employment trials on the treated individuals need to be identified. This is called the average treatment effect on the treated (ATT) and using potential outcomes[9], it can be expressed as follows:

$$ATT = \mathbb{E}(Y_{1i}|D_i = 1) - \mathbb{E}(Y_{0i}|D_i = 1) \tag{6.1}$$

In the equation 6.1, Y_{1i} is the outcome if treatment is received and Y_{0i} is the outcome when treatment is not received. Clearly, the term $\mathbb{E}(Y_{0i}|D_i = 1)$ in the formula for ATT is not observed, since the outcome Y_{0i} is not observed for those who participate in the trial. If the means of the treated were compared to the non-treated ignoring the possibility of selection, it would yield:

$$\begin{aligned} &\mathbb{E}(Y_{1i}|D_i = 1) - \mathbb{E}(Y_{0i}|D_i = 0) \\ = {}&[\mathbb{E}(Y_{1i}|D_i = 1) - \mathbb{E}(Y_{0i}|D_i = 1)] + [\mathbb{E}(Y_{0i}|Y = 1) - \mathbb{E}(Y_{0i}|D_i = 0)] \end{aligned} \tag{6.2}$$

In the equation 6.2, in addition to ATT, the estimate would contain the term in the latter brackets, which is the selection bias. If there exist variables X such that if conditioned on X it holds that $\mathbb{E}(Y_0|Y = 1) = \mathbb{E}(Y_0|Y = 0)$, then the problem of selection bias has been overcome. This condition is called the conditional independence assumption: it means that if variables X are fixed, the groups are similar. If such variables were available, matching alone (or controlling for X in a regression) would be sufficient to tackle the selection problem (Angrist and Pischke, 2009). As no such variables are available, I use a combined matching and difference-in-differences method[10].

[9] see Angrist and Pischke (2009)

[10] Matching is only used in the analysis of the second trial (2017–2018).

Because the matching alone may not get rid of the selection bias, I use it only to create treatment and control groups that would satisfy the parallel trends assumption. This assumption means that the trends of treatment and control groups would be similar in the absence of treatment. Following the notation of Mummolo (2017), the parallel trends assumption can be expressed as

$$\mathbb{E}[Y_{0i}(1) - Y_{0i}(0)|D_i = 1] = \mathbb{E}[Y_{0i}(1) - Y_{0i}(0)|D_i = 0] \tag{6.3}$$

The assumption in (6.3) implies that

$$\mathbb{E}[Y_{0i}(1)|D_i = 0] = \mathbb{E}[Y_{0i}(1)|D_i = 1] - \mathbb{E}[Y_{0i}(0)|D_i = 1] + \mathbb{E}[Y_{0i}(0)|D_i = 0] \tag{6.4}$$

When (6.4) holds we see that the difference in the differences identifies the average treatment effect on the treated by the following:

$$\begin{aligned}
DiD = \{&\mathbb{E}(Y_i(1)|D_i = 1) - \mathbb{E}(Y_i(1)|D_i = 0)\}- \\
&\{\mathbb{E}(Y_i(0)|D_i = 1) - \mathbb{E}(Y(0)|D_i = 0)\} \\
&= \{\mathbb{E}(Y_{1i}(1)|D_i = 1) - \mathbb{E}(Y_{0i}(1)|D_i = 0)\}- \\
&\{\mathbb{E}(Y_{0i}(0)|D_i = 1) - \mathbb{E}(Y_{0i}(0)|D_i = 0)\} \\
&= [\mathbb{E}(Y_{1i}(1)|D_i = 1) - (\mathbb{E}[Y_{0i}(1)|D_i = 1] \\
&-\mathbb{E}[Y_{0i}(0)|D_i = 1] + \mathbb{E}[Y_{0i}(0)|D_i = 0])] \\
&-[\mathbb{E}(Y_{0i}(0)|D_i = 1) - \mathbb{E}(Y_{0i}(0)|D_i = 0)] \\
&= \mathbb{E}[Y_{1i}(1) - Y_{0i}(1)|D_i = 1]
\end{aligned} \tag{6.5}$$

6.2 Propensity score matching

Propensity score matching (PSM) is a widely used method in active labor market policy evaluations (Caliendo and Kopeinig, 2008). In PSM, participants are matched to non-participants using propensity scores, which are calculated using a logit model

$$\log \frac{P(T=1)}{1-P(T=1)} = X'\beta + \varepsilon_{it} \tag{6.6}$$

where covariates X' are variables that are thought to affect both treatment assignment and the outcome variable. After the calculation of propensity scores, individuals with similar propensity scores are matched to each other using nearest-neighbour matching or other matching technique. As propensity scores can be thought of as probabilities of being assigned to treatment, propensity score matching creates treatment and control groups with similar probabilities of treatment.

A central assumption in propensity score matching is the assumption of common support. Observations are in the area of common support if it is not completely determined by the covariates to which group (treatment or control) they belong. In other words, common support assumption means that there groups need to have overlap in their distributions of covariates used in matching. The assumption can be expressed as (Caliendo and Kopeinig, 2008):

$$0 < P(T=1|X) < 1 \tag{6.7}$$

Matching is often used together with difference-in-differences strategy when the parallel trends assumption is not satisfied (Chabé-Ferret, 2017). In the 2017–2018 municipal employment trial, the treated and non-treated individuals were very different in both background characteristics as well as in unemployment trends, which makes matching necessary in order to get similar trends before the treatment.

The selection of variables in matching is a difficult task, because it may impact the results. Matching can be done using pre-treatment values of the outcome variable or alternatively by using only confounding variables. Chabé-Ferret (2017) argue that pre-treatment outcomes should not be used in matching, and that matching on pre-treatment outcomes combined with difference-in-differences is a problematic strategy. According to Chabé-Ferret (2017) matching on pre-treatment outcomes may increase the bias of the difference-in-differences estimator.

Traditional argument in favor of matching on pre-treatment outcomes is the existence of an *Ashenfelter dip*, a drop in employment or earning before entering an active labor market program, found by Ashenfelter and Card (1985). Conditioniong on pre-treatment outcomes, it is possible to create treatment and control groups with parallel Ashenfelter dips. Additionally, matching on pre-treatment outcomes combined with difference-in-difference strategy gives similar results to RCTs, especially in the context of training programs. (Chabé-Ferret, 2017).

Propensity score matching may, however, increase imbalance according to King and Nielsen (2019). According to them, the problem with PSM arises from the fact that PSM tries to approximate a randomized trial. This creates what they call the *PSM paradox*. It means that the more balanced the data, the larger the biases caused by PSM. In light of their evidence, King and Nielsen (2019) argue that other matching methods, such as coarsened exact matching, should be used instead of PSM. Since PSM is only used to create the treatment and control groups, and difference-in-differences strategy is used to estimate the effects, the problems with PSM may not be a big threat to the validity of the research design in this thesis.

The variables I use in matching vary by treatment area, since all areas had different type of criteria when they selected participants. Below are the variables used in matching in different areas. The full balance tables after the matching procedure are presented in Appendix A.

The variables used in matching in **Lappi area** include: 1) the length of unemployment spell in July 2017, 2) the number of unemployment months in the last 12 months in July 2017, 3) unemployed for less than a year (not long-term unemployed), 4) education (levels 1–5), 5) indicator whether individual has participated in activation in the one-year period before the trial, 6) age and 7) gender.

In **Varsinais-Suomi area** the variables used in matching include: 1) the length of unemployment spell in July 2017, 2) basic level of education (categories 1–5), 3) indicator whether individual has participated in activation in the one-year period before the trial, 4) age, 5) gender, 6) living in an urban area, 7) size of family and 8) marital status (levels 1–4).

In **Pirkanmaa area** the variables used in matching include: 1) the length of unemployment spell in July 2017, 2) number of unemployment months in the last 12 months in July 2017 3) gender, 4) living in an urban area, 3) employed at the end of 2016, 4) employed at the end of 2015, 5) marital status (levels 1–4), 6) age, 7) size of family

In **Pori area** the variables used in matching include: 1) the length of unemployment spell in July 2017, 2) the number of unemployment months in the last 12 months in July 2017, 3) basic level of education (categories 1–5), 4) living in an urban area, 5) number of years employed during 2011–

2016 (levels 0–6), 6) employed in 2016, 7) employed in 2015, 8) age and 9) gender.

In **Pohjois-Savo area** the variables used in matching include: 1) the length of unemployment spell in July 2017, 2) unemployed for less than a year (not long-term unemployed), 3) basic level of education (categories 1-5), 4) indicator whether individual has participated in activation in the one-year period before the tria, 4) age, 5) gender, 6) living in an urban area, 7) size of family and 8) marital status.

6.3 Difference-in-differences

Difference-in-differences (DiD) is a popular quasi-experimental research design that allows the identification of causal effects if the *parallel trends assumption* holds. This assumption means that, in the absence of treatment, the trends of treatment and control groups would stay similar. If the parallel trends assumption holds, the difference-in-differences estimator identifies the average causal effect of treatment on individuals in the treatment group (ATT). In subchapter 6.1, I showed how the DiD-estimator identifies this effect.

The parallel trends assumption is untestable, but it can be evaluated by looking at the trends before treatment: if the pre-trends are parallel, it can be assumed that the trends would have continued to be similar in the absence of treatment. The pre-trends are parallel, if the treatment effects before the start of treatment are zero. In my analysis, I calculate periodic treatment effects and plot the coefficients. In addition to testing coefficients separately, the coefficients have to be tested simultaneously to see if the the hypothesis about all pre-treatment coefficients being zero is rejected or not.

The difference-in-differences identification strategy has also other assumptions: first, the treatment should not affect the control group. Additionally, no other reforms or interventions should have been implemented during the intervention that is studied. This assumption is satisfied, except for Pirkanmaa, where interviews for the unemployed were conducted just before the start of trial. This intervention caused sudden ends to unemployment spells in Pirkanmaa area, as the job seekers were directed to services. This makes it questionable whether the parallel trends assumption holds in the case of Pirkanmaa.

6.4 Estimated models

I use a difference-in-differences approach and calculate periodic treatment effects. In the first trial, I calculate yearly treatment effects and in the second trial the treatment effects are calculated bimonthly (i.e. every two months). I estimate the periodic difference-in-difference estimates using two-way fixed effects models with time and unit fixed effects. Depending on the specification, the unit fixed effect can be either municipality or individual fixed effect. Generally written, the model used in the empirical section is

$$Y_{it} = \gamma_i + \lambda_t + \sum_k \theta_k D_{it}^k + \varepsilon_{it} \tag{6.8}$$

where coefficients θ_k are the periodic treatment effects, γ_i is the unit (municipality or individual) fixed effect and λ_t is the time fixed effect. Variables D_{it}^k are interactions between the treatment and time variable, meaning that they equal 1 if individual i is treated and $t = k$. The treatment indicator for time period -1 is omitted, since the difference between treatment and control group in other periods is compared to the difference in the period -1.

The outcome variable Y_{it} is a dummy variable indicating e.g. unemployment, employment or sanctions.

I start the empirical section by analyzing the 2012–2015 trial, which will have a lesser focus in this thesis. In the analysis of the first trial, I calculate yearly treatment effects on the probability of employment and probability of unemployment. I estimate the model first with municipality fixed effects and then with individual fixed effects instead. Specifications with different controls are estimated in both cases. Standard errors are clustered at the municipal level in all specifications. The outcome variable Y_{it} is a dummy variable indicating unemployment or employment at the end of year t. The model used in the analysis can be written

$$Y_{it} = \gamma_i + \lambda_t + \sum_{k=2005}^{2016} \theta_k D_{it}^k + X'\beta + \varepsilon_{it} \tag{6.9}$$

where X' are the control variables. The years used in the analysis of the first trial are 2005 – 2016, as can be seen from the model equation (6.9).

After analyzing the 2012-2015 trial I move to the second trial, where I focus more on the effects on individual outcomes, since the treated individuals can be identified. In the analysis of the 2017–2018 trial I create treatment and control groups separately for all trial areas using the matching procedure described in previous sections. I then calculate the treatment effects separately for all 5 trial areas (Varsinais-Suomi, Pirkanmaa, Pohjois-Savo, Lappi and Pori). I calculate the effects bimonthly (observations every two months) due to large number of observations and lack of memory available in the system where the data is used[11]. In the analysis of the second trial the model (6.3) is in the form

[11]The data is used in Statistics Finland's FIONA remote computer service, which has limitations in how much memory a user can use.

$$Y_{it} = \gamma_i + \lambda_t + \sum_{k=-18}^{16} \theta_k D_{it}^k + \varepsilon_{it} \tag{6.10}$$

where coefficients θ_k are the periodic treatment effects. In the model, γ_i and λ_t are individual and time fixed effects, respectively. Variables D_{it}^k are interactions between the treatment and time variable, meaning that they equal 1 if individual i is treated and $t = k$. The treatment indicator for period -1 is omitted. Standard errors at the individual level, but I show the main results also with standard errors clustered by municipality. The outcome variable Y_{it} is a dummy variable indicating e.g. unemployment, extended unemployment[12], or participation in activation.

Since the outcome variable is dichotomous, the analysis could also be done using logit or probit models instead of linear probability model. These models are, however, harder two interpret than the OLS, hence the baseline method used is the OLS. In the analysis of the second trial, I estimate also logit model to see if it gives similar results. The logit estimations are conducted with the following specification:

$$log \frac{P(Y_{it} = 1)}{1 - P(Y_{it} = 1)} = treat_i + \lambda_t + \sum_{k=-7}^{4} \theta_k D_{it}^k + \varepsilon_{it} \tag{6.11}$$

I present the results for logit specification (model 6.11) by showing the coefficients θ_k from the model. These are the difference-in-differences in the logarithm of the odds, whereas in the OLS specification the difference-in-differences estimates were in the probability of the outcome. Difference in the logarithm of the odds is not as easy to interpret, but the logit estimates are useful to determine if there is an effect or not.

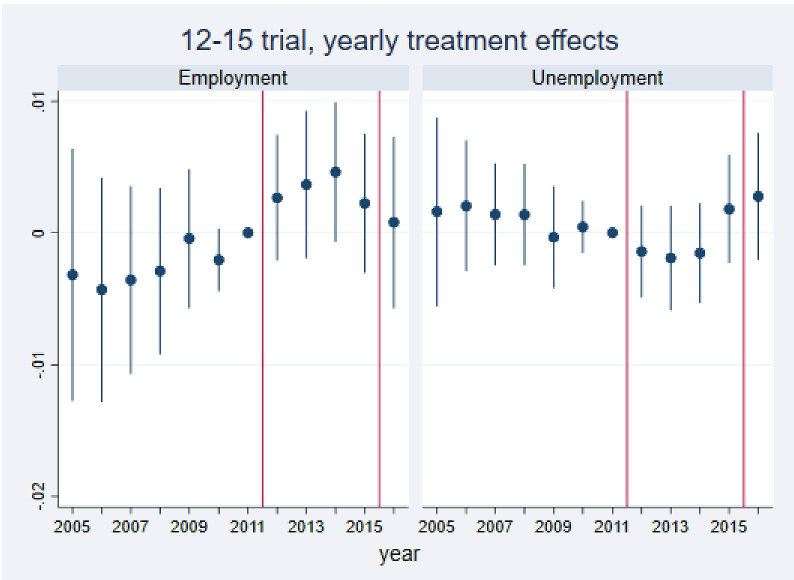
[12] Extended unemployment is defined here similarly to Arnkil et al. (2019). In this extended definition a person is still considered unemployed if they are in activation services.

7 Results

7.1 First trial

In the first trial the participants cannot be identified, so the treated units are municipalities. The aim of the 2012–2015 analysis is to extend the municipal level analyses in Nieminen et al. (2020) using individual level data. In the analysis, I first use data that has the working-age individuals for every year in the treated and control municipalities (see Appendix D) and estimate yearly treatment effects on employment and unemployment at the end of a year. First, the analysis is conducted using municipality and year fixed effects and no controls. The treatment effects from this specification are presented in Figure 4.

Figure 4: Treatment effects (2012–2015), specification 1

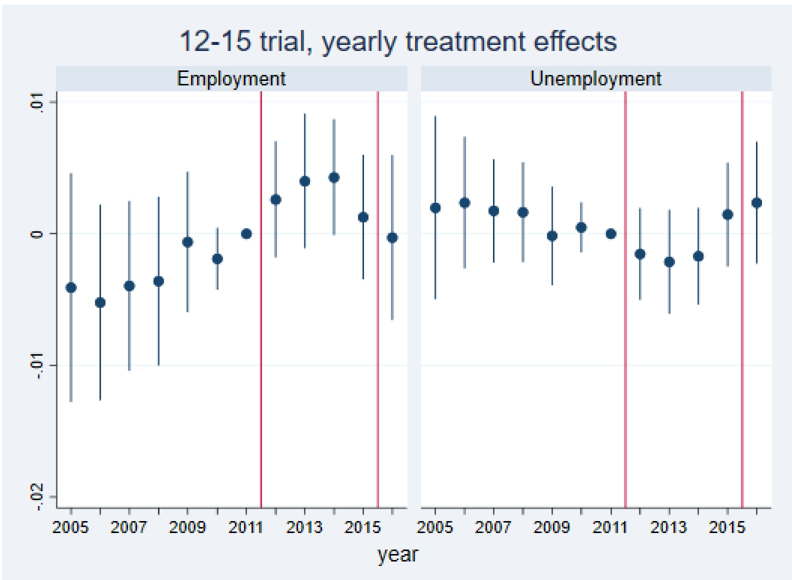


Notes. Figure plots regression coefficients from model (6.9). The coefficients are yearly treatment effects and the coefficient of period -1 (2011) is normalized to zero. Specification includes municipality and year fixed effects.

As can be seen from the Figure 4, there are no significant effects on the probabilities of unemployment or employment. However, the point estimates during the trial are positive for employment and there is also seems to be a dip in unemployment after the trial started. Similar phenomenon was found in the municipal-level analysis by Nieminen et al. (2020).

Individual level controls can be added to the model to see if they reduce the standard errors or change the estimates. Figure 5 presents the results from otherwise similar specification as earlier, but with controls for gender, age, living in an urban area and upper secondary education.

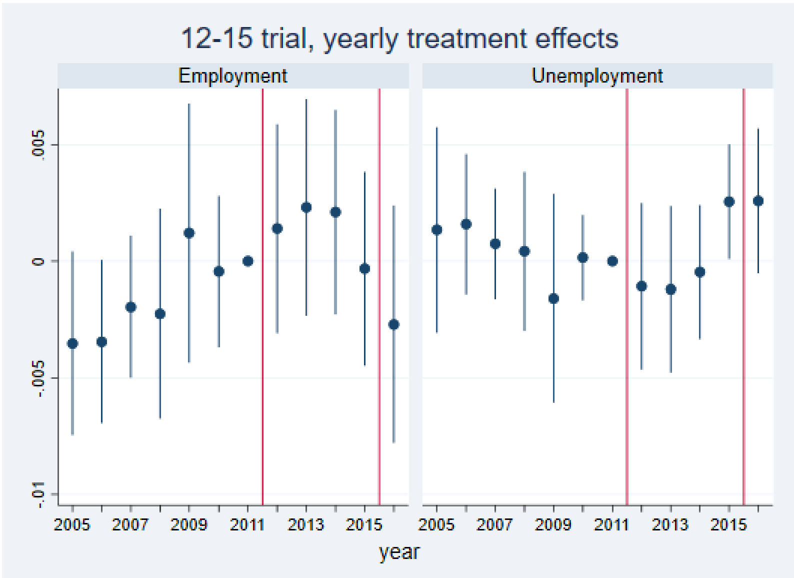
Figure 5: Treatment effects (2012–2015), specification 2



Notes. Figure plots regression coefficients from model (6.9). The coefficients are yearly treatment effects and the coefficient of period −1 (2011) is normalized to zero. Specification includes municipality and year fixed effects as well as individual level controls for gender, age, living in an urban area and upper secondary education.

When the controls are added (Figure 5), estimated effects are similar to the estimates without controls. The standard errors are somewhat smaller: in the specification without controls the standard errors of the post-treatment coefficients were between 0.0024 and 0.0028 and now they are between 0.0022 and 0.0026. Despite the reduction in standard errors, the point estimates insignificant. In Figure 6, further controls are added, including previous year's income, unemployment months and employment months.

Figure 6: Treatment effects (2012–2015), specification 3

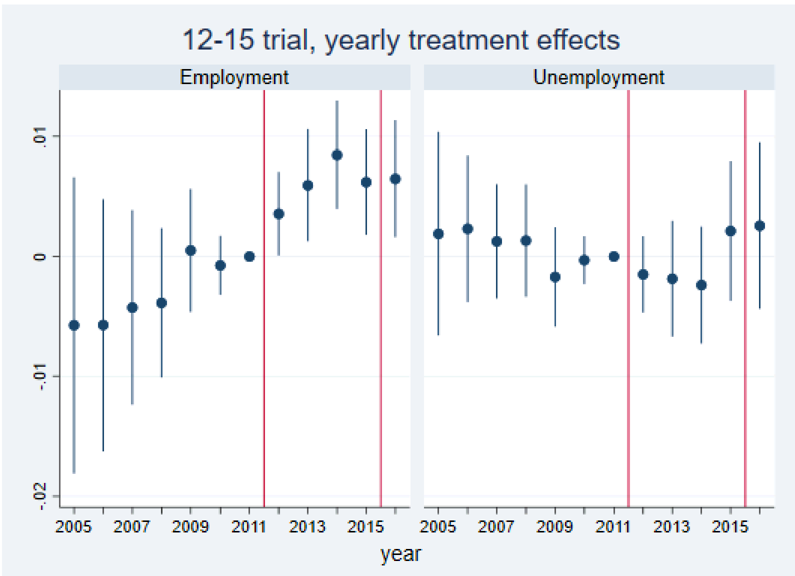


Notes. Figure plots regression coefficients from model (6.9). The coefficients are yearly treatment effects and the coefficient of period -1 (2011) is normalized to zero. Specification includes municipality and year fixed effects as well as individual level controls for gender, age, living in an urban area, upper secondary education, previous year's income, unemployment months and employment months.

In Figure 6, where also the lagged employment and income controls are included, we see that also the point estimates change. Still, however, the point estimates are positive for employment and negative for unemployment during the first three years after the trial. The pre-trends in employment do not look very good, since the point estimates appear to be increasing before the start of treatment. None of the pre-treatment coefficients are statistically different from zero, though.

I also want to do the analysis using individual fixed effects instead of municipality fixed effects. The problem with individual fixed effects is that the standard errors cannot be clustered at the municipality level if the same individual lives in different municipalities in different years. In order to make the municipalities have same individuals every year, I delete individuals who move to other municipality during this time. This procedure may impact the results, since those who don't move are probably different from those who move. This, however, affects both treated and control municipalities. Figure 7 presents yearly treatment effects from the individual fixed effects regressions without controls.

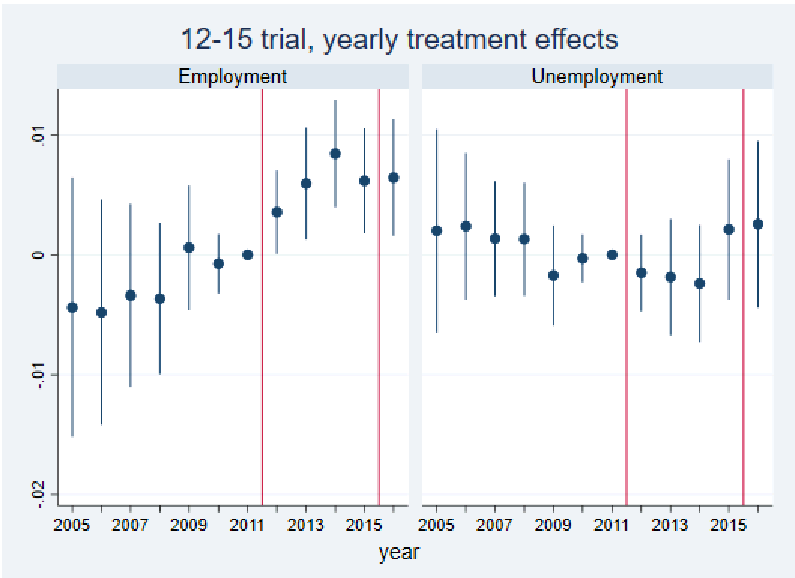
Figure 7: Treatment effects (2012–2015), specification 4



Notes. Figure plots regression coefficients from model (6.9). The coefficients are yearly treatment effects and the coefficient of period -1 (2011) is normalized to zero. Specification includes individual and year fixed effects.

As can be seen from Figure 7, a statistically significant effect of 0.005 on probability of employment is found. This means an increase in employment probability by 0.5 percentage points. In unemployment probability, we do not find statistically significant effects, though the decrease in point estimates is still present. Since the probability of employment increased but probability of unemployment did not decrease significantly, this result would imply that the probability of employment increased because the trial increased employment of those outside the labor force. This would be consistent with the findings of Lundin and Skedinger (2006) who found that municipal programs may target *outsiders* in the labor market.

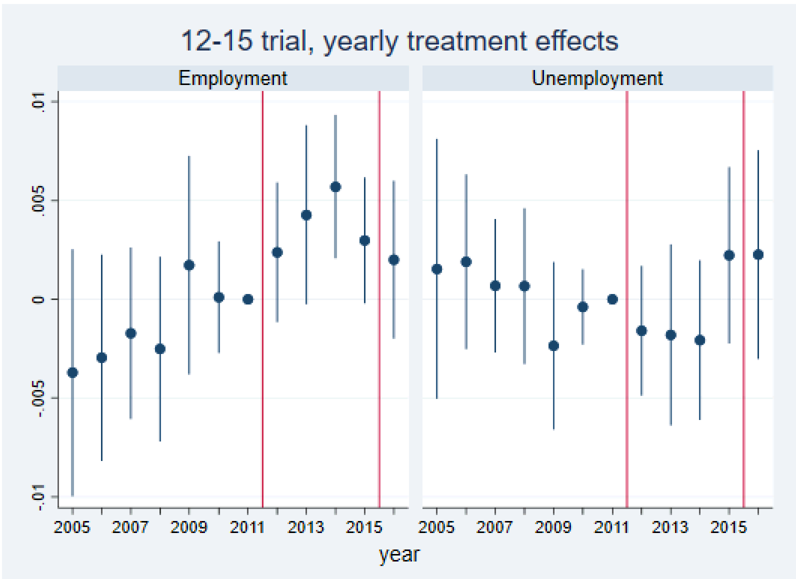
Figure 8: Treatment effects (2012–2015), specification 5



Notes. Figure plots regression coefficients from model (6.9). The coefficients are yearly treatment effects and the coefficient of period −1 (2011) is normalized to zero. Specification includes individual and year fixed effects as well as individual level controls for gender, age, living in an urban area and upper secondary education.

In Figure 8 above, gender, age, living in an urban area and upper secondary education are added as controls. With added controls, the effect on employment probability is still there and is significant. Overall, the added controls do not really change the results compared to not having any controls. Figure 9 below presents treatment effects from a specification where controls for previous year's income, unemployment months and employment months are added.

Figure 9: Treatment effects (2012–2015), specification 6



Notes. Figure plots regression coefficients from model (6.9). The coefficients are yearly treatment effects and the coefficient of period -1 (2011) is normalized to zero. Specification includes individual and year fixed effects as well as individual level controls for gender, age, living in an urban area, upper secondary education, previous year's income, unemployment months and employment months.

Now, the increase in employment during the trial is still around 0.5 percentage points, but the effect is only significant in 2014. The effect on employment is found in all specifications where individual fixed effects are used and is therefore robust to adding controls.

7.2 Second trial

In the evaluation of the second municipal employment trial, I study the effects on the probabilities of unemployment, extended unemployment, activation and sanctions. Additionally, I investigate how the trial affected the probability of participation in 5 different types of activation services: coaching, studying, work trials, rehabilitative work programs and labour force training. This chapter presents the results for 5 different trial areas (Varsinais-Suomi, Pirkanmaa, Pohjois-Savo, Lappi and Pori) in subchapters 7.2.1 – 7.2.5.

Since employment is only observed yearly and the latest information is from 2017, the main measure used to investigate employment effects is the 'extended unemployment' variable, which means both those who are registered as unemployed as well as those who are participating in activation services. In the figures, this variable is named 'Unemployed + Services'.

As mentioned in the Methods chapter, both OLS (fixed effects) and logit results are calculated. The OLS results are, however, the main analyses, while results from logit specifications are mainly presented as robustness checks to see if they give similar results. Therefore, logit results are not presented in all areas or outcome variables and are not discussed as thoroughly as the OLS results. Some logit results are presented in the Results chapter and some in the Appendix E.

7.2.1 Varsinais-Suomi area

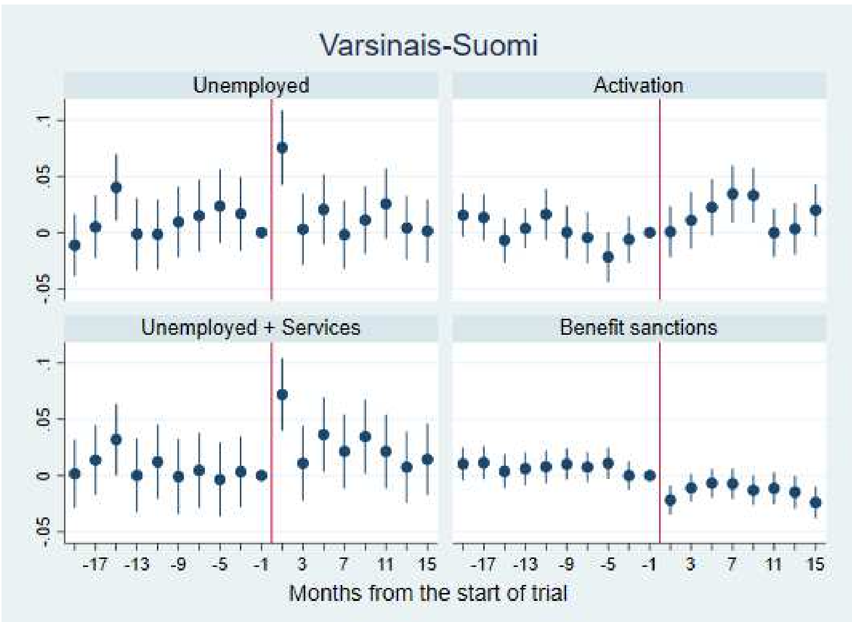
In Varsinais-Suomi area, the analysis is conducted in two ways: first, participants under the age of 25 in Varsinais-Suomi area are compared to matched non-treated, under 25-year-old individuals from other areas[13]. After this

[13] The control group is created using propensity score matching with variables listed in subsection 6.2. The balance tables after matching are presented in Appendix A.

analysis, outcomes of Varsinais-Suomi residents just under the 25-year threshold (born in 1993) are compared to individuals a little older than 25 (born in 1991), who live in Varsinais-Suomi. No matching is used in the second analysis.

The main results (participants in Varsinais-Suomi vs. matched controls) can be found in Figure 10. The figure presents bimonthly treatment effects (effects for 2-month periods) on unemployment, extended unemployment, participation in activation, and sanctions in Varsinais-Suomi area. The effects are coefficients θ_k from model (6.10). As noted earlier, the coefficient for period -1 is omitted, since it is the baseline period. Standard errors are clustered on the individual level. Coefficients of this main model can also be found in the regression tables in Appendix A.2.

Figure 10: Varsinais-Suomi, main results



Notes. Figure plots regression coefficients from model (6.10). The coefficients are monthly treatment effects and the coefficient of period -1 (July 2017) is normalized to zero.

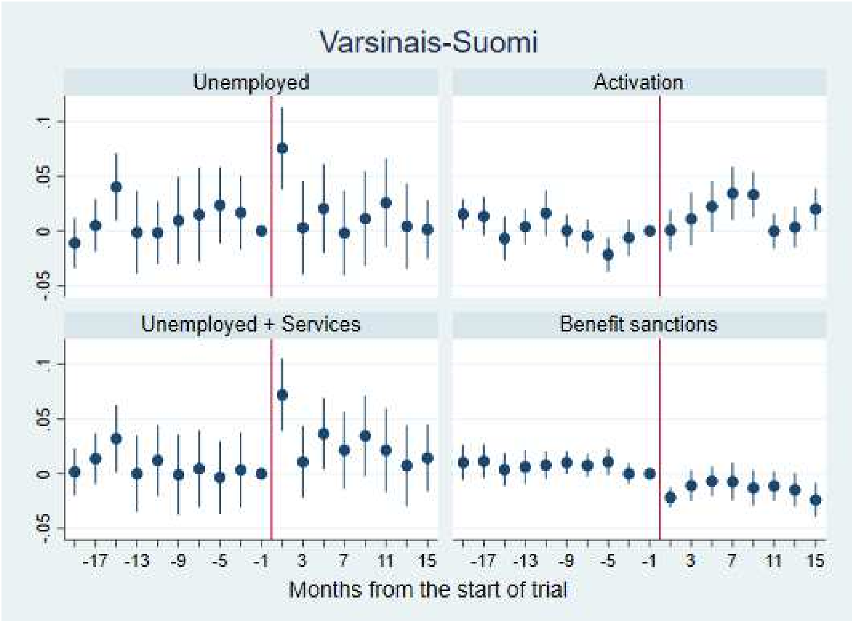
As can be seen from Figure 10, the trial did not have effects on the probability of unemployment or the probability of extended unemployment, except for the short term increase, lasting only one period. This increase could be e.g. because of probems in start of the trial or it could just be random. The pre-trends are otherwise zero, except for the coefficient 15 months before the start of the trial. During the trial, the treatment effects on unemployment do not differ from zero statistically significantly, meaning that no effect on unemployment is seen during the 15 months after the trial started.

Probability of ALMP participation rose briefly during the trial: the point estimates for periods 7 and 9 indicate an increase of 2.5 percentage points on the probability of activation. The effect vanishes when the trial ends. The most interesting result in Varsinais-Suomi area trial is the decrease in sanctions by almost one percentage point. The effect on sanctions persists and is still there when 15 months has passed since the trial. The reduced sanctions result is in line with the result Mergele and Weber (2020) found in the German reform. It is also in line with my theoretical predictions. The size of the result is still very surprising: 1 percentage point decrease in sanctions means over 10 percent decrease in relative terms.[14]

Since observations from the same municipality can be correlated, the standard errors may be wrong. To solve this problem, standard errors could be clustered at the municipal level instead of individual level. Figure 11 below presents the same results as before, but with standard errors clustered by municipality. As can be seen from Figure 11, clustering the standard errors on the municipal level doesn't change the results: effects on activation and sanctions are still significant.

[14]This is because the probability of being sanctioned is around 9 percent before treatment. If it decreases 1 percentage point, this is a change of $\frac{0.08-0.09}{0.09} * 100\% = -11\%$

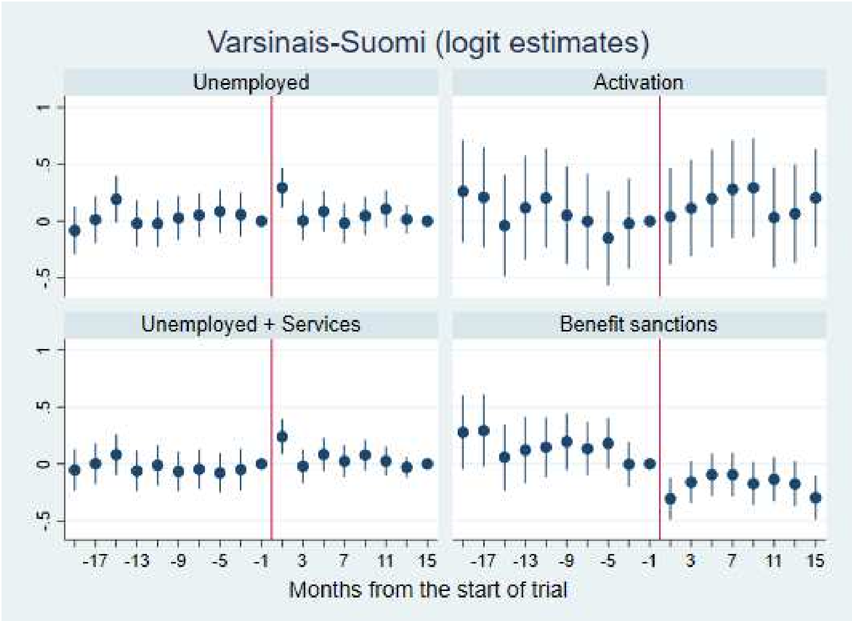
Figure 11: Varsinais-Suomi, clustering on municipality



Notes. Figure plots regression coefficients from model (6.10). The coefficients are monthly treatment effects and the coefficient of period -1 (July 2017) is normalized to zero.

As the outcome variables are dichotomous, the results from logistic DiD-specification (model 6.11) are also useful to address whether or not there are any effects. The results using logit specification can be found in Figure 12 below. When using logit specification, the results look quite similar: no effect on the probability of unemployment. In the logit specification, however, no effect on ALMP participation is found. The reduced sanctions result is also found with the logit specification. This means that the trial had a negative effect on the logarithm of the odds of being sanctioned.

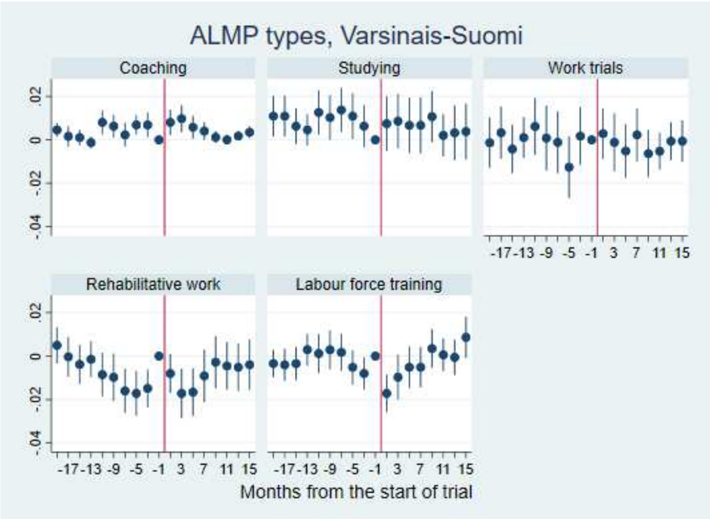
Figure 12: Varsinais-Suomi, logit estimates



Notes. Figure plots regression coefficients from model (6.11). The coefficients are monthly treatment effects and the coefficient of period −1 (July 2017) is normalized to zero.

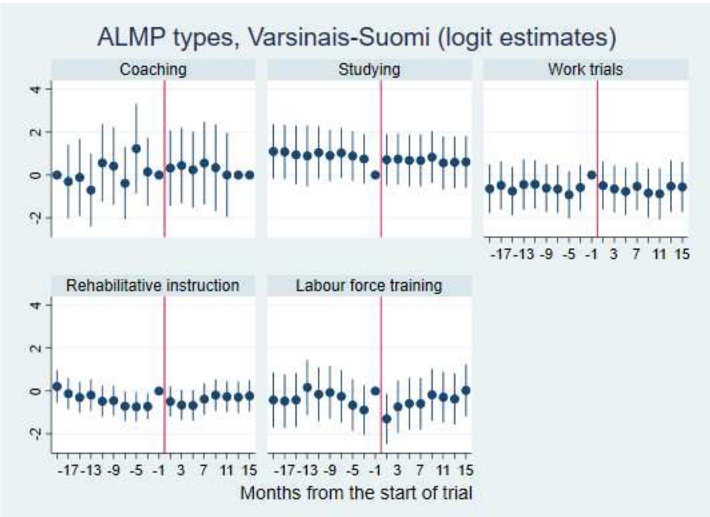
Figures 13 and 14 present the effects of the trial on probabilities of participating in different ALMP types. In Figure 15 presents the results from OLS specification and Figure 14 presents results from logit specification. As can be seen from Figure 13, the trial did not change the types of activation services provided to job seekers. The pre-trends are unfortunately not zero in all of the ALMP types; this may be because the number of individuals participating in specific ALMP programs is quite small and volatile. Figure 14 plots the logit esimates of the effects on ALMP types: no effects there either. Therefore, there is no evidence for cost-shifting through changing ALMP types in Varsinais-Suomi.

Figure 13: Varsinais-Suomi, ALMP types



Notes. Figure plots regression coefficients from model (6.10). The coefficients are monthly treatment effects and the coefficient of period −1 (July 2017) is normalized to zero.

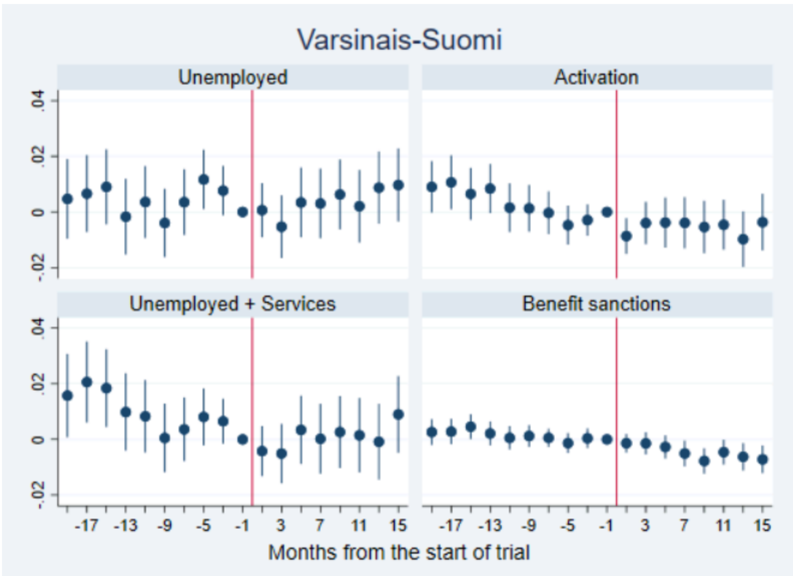
Figure 14: Varsinais-Suomi, ALMP types (logit)



Notes. Figure plots regression coefficients from model (6.10). The coefficients are monthly treatment effects and the coefficient of period −1 (July 2017) is normalized to zero.

In Varsinais-Suomi area the analysis can also be done inside the area, by comparing individuals in both sides of the 25-year-threshold in treatment assignment. The results from this setting are presented in Figure 15. The results are similar to the results above: no effect on the probability of unemployment or activation. The reduction in sanctions is found also in this setting, indicating robustness of the result. Finding no effect inside Varsinais-Suomi is consistent with the findings by Ylikännö et al. (2019).

Figure 15: Varsinais-Suomi, born in 1993 vs. 1991

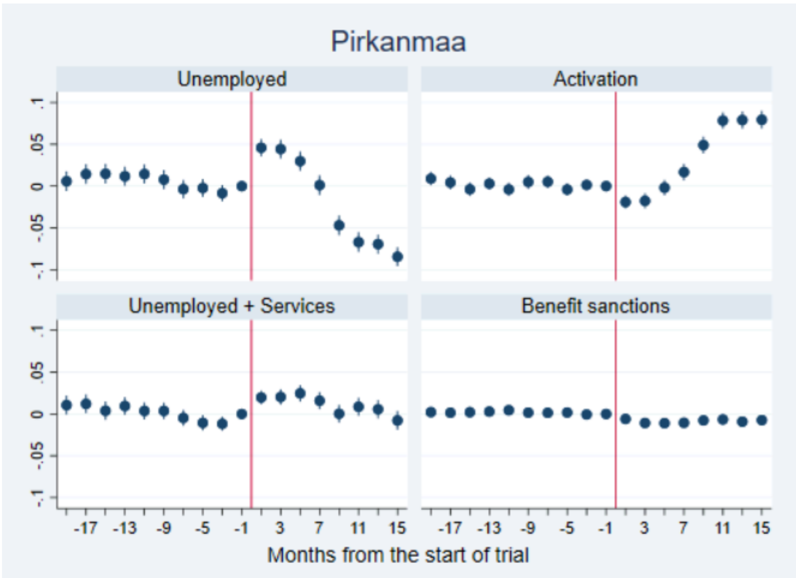


Notes. Figure plots regression coefficients from model (6.10). The coefficients are monthly treatment effects and the coefficient of period -1 (July 2017) is normalized to zero.

7.2.2 Pirkanmaa area

Pirkanmaa area trial is the most challenging of the 5 areas when it comes to the parallel trends assumption. Various matchings were tested, but there is always a drop in the probability of unemployment in the treatment group just before the start of the trial. Therefore, the pre-trends are not parallel in many of the outcome variables. I hypothesize that this is due to the interviews conducted for job seekers in Pirkanmaa at that time. If the pre-trends were coerced to be the same with matching (that is, using indicators for unemployment in each pre-treatment month in matching), the estimated effects would be similar to the results presented in this Chapter. I choose to present the results where this is not done, but instead the matching in done with variables presented before, in subsection 6.2. Figure 16 presents results in Pirkanmaa area.

Figure 16: Pirkanmaa, main results



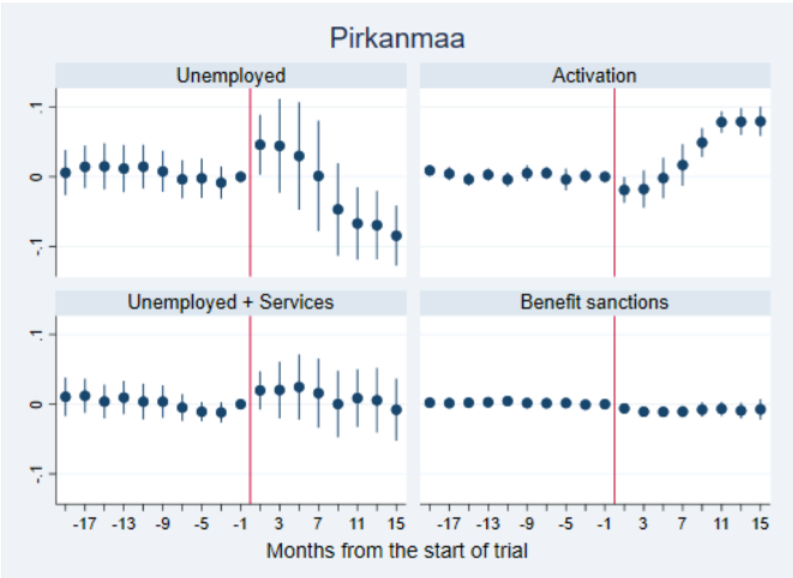
Notes. Figure plots regression coefficients from model (6.10). The coefficients are monthly treatment effects and the coefficient of period -1 (July 2017) is normalized to zero.

As can be seen from Figure 16, the trial briefly increases the probability of being registered as unemployed, but as the trial progresses, the probability of unemployment decreases clearly in the treatment group. This may explain why the Pirkanmaa area trial has received praise in the media. The decrease in registered unemployment does not, however, mean that employment would have increased: in fact, the trial first increases the probability of extended unemployment (Unemployed + Services) and in later months the treatment effects on extended unemployment are zero. The reason for the reduction in registered unemployment is that more people were directed to activation, which makes them not appear as unemployed in the register. The effect on activation is very sizable: an increase of over 7 percentage points in the probability of participating in activation.

The trial has also a sizable effect on the probability of receiving benefit sanctions: sanctions are reduced by 1 percentage points in Pirkanmaa area. The size of the result is similar to the result found in Varsinais-Suomi and it can be considered quite sizeable. The result on sanctions is presented in a single plot in Appendix E, since the results is hard to read from Figure 16 due to the scale on the vertical axis. As reducing sanctions can be one channel for the municipality to shift costs to the central government, this finding supports the hypothesis that municipalities would try to shift costs to the central government.

Results with standard errors clustered at the municipality level instead of individual level are presented in the Figure 17. When the standard errors are clustered by municipality, they increase visibly: in fact, the pre-trends look much better with the clustered errors. The effects on registered unemployment, activation and sanctions are all still significant, meaning that they are robust to the cluster correction of standard errors by municipality. The effect on extended unemployment is, however, not significant when standard errors are clustered at the municipality level. Hence, the results with clustered standard errors would imply that the Pirkanmaa trial did not affect employment during the 15-month period after the start of the trial. It did, however, decrease unemployment by increasing participation in ALMP services.

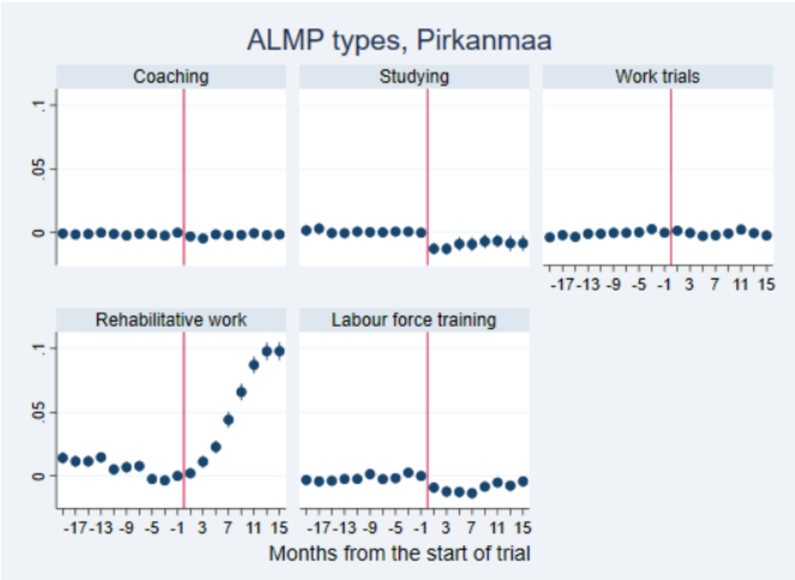
Figure 17: Pirkanmaa, clustered standard errors



Notes. Figure plots regression coefficients from model (6.10). The coefficients are monthly treatment effects and the coefficient of period -1 (July 2017) is normalized to zero.

Estimated effects on the ALMP types in Pirkanmaa area can be found in Figure 18. Clearly, there is a gradual increase in rehabilitative work programs and a sharp decrease in studying (in Finnish: *omaehtoinen opiskelu*). Additionally, there is a negative effect on the probability of participating in labour force training. Hence, we can conclude that the Pirkanmaa area trial increased rehabilitative work programs, which can be considered public employment schemes. These types of ALMP programs were found to be the least effective in empirical ALMP literature Kluve (2016) The decrease in studying and training is also worrisome, since training is one of the more effective types of ALMP according to Kluve (2016). In addition to the shift in ALMP types being possibly towards less effective programs, the increase in rehabilitative work programs in Pirkanmaa indicates that municipalities in Pirkanmaa area may have tried to shift costs to the central government.

Figure 18: Pirkanmaa, ALMP types

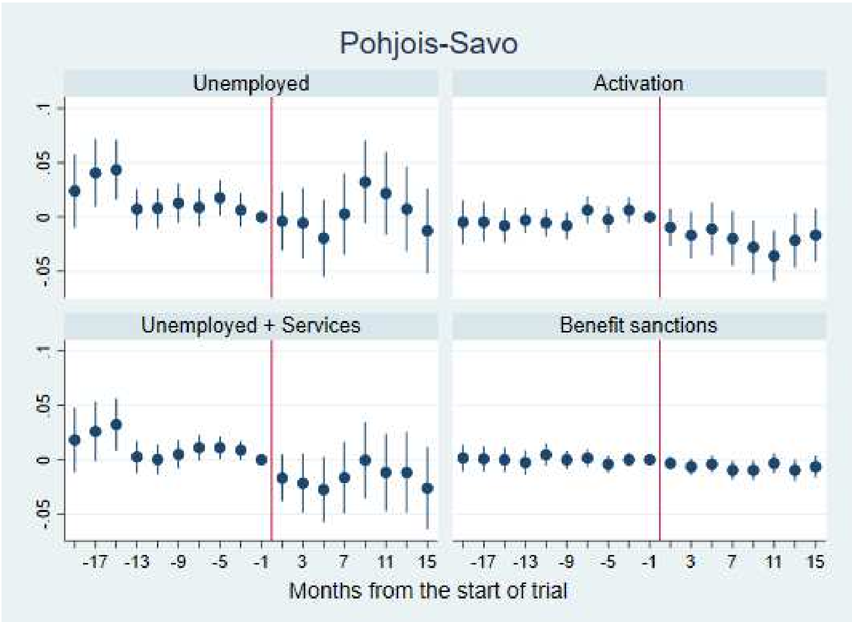


Notes. Figure plots regression coefficients from model (6.10). The coefficients are monthly treatment effects and the coefficient of period −1 (July 2017) is normalized to zero.

The key story from Pirkanmaa area trial is that activation increased substantially, but the activation was mainly in the form of rehabilitative work programs. Since studying and labour force training were reduced, they were probably replaced with rehabilitative work, i.e. some job seekers who could have otherwise been directed to studying or training, may have been directed to rehabilitative work programs during the trial. No employment effects were found in the 15 month period after the results, but such effects could occur in a longer time frame, if the increased ALMP participation helped job seekers.

7.2.3 Pohjois-Savo area

Figure 19: Pohjois-Savo, main results



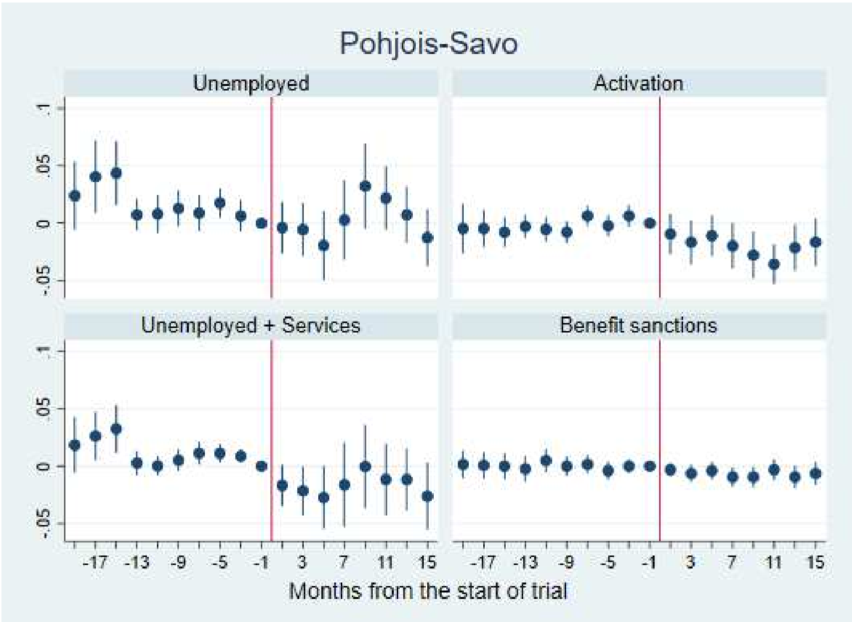
Notes. Figure plots regression coefficients from model (6.10). The coefficients are monthly treatment effects and the coefficient of period -1 (July 2017) is normalized to zero.

Figure 19 presents the main results in Pohjois-Savo area. In Pohjois-Savo, no effects are found except for the probability of activation, where a negative effect is found in periods 9 and 11. The negative effect on activation in Pohjois-Savo is probably attributed to replacing rehabilitative work programs with the municipalities' own work experience program[15]. The work experience program had, however, less participants than were previously in labour force training. Hence, the ALMP participation actually decreased in Pohjois-Savo, which is an unexpected result. The pre-trends look otherwise good, expect for the coefficients in periods -16 and -14 in unemployment

[15]see Annala et al. (2019)

and extended unemployment. Estimates with standard errors clustered at the municipality level are presented in Figure 20.

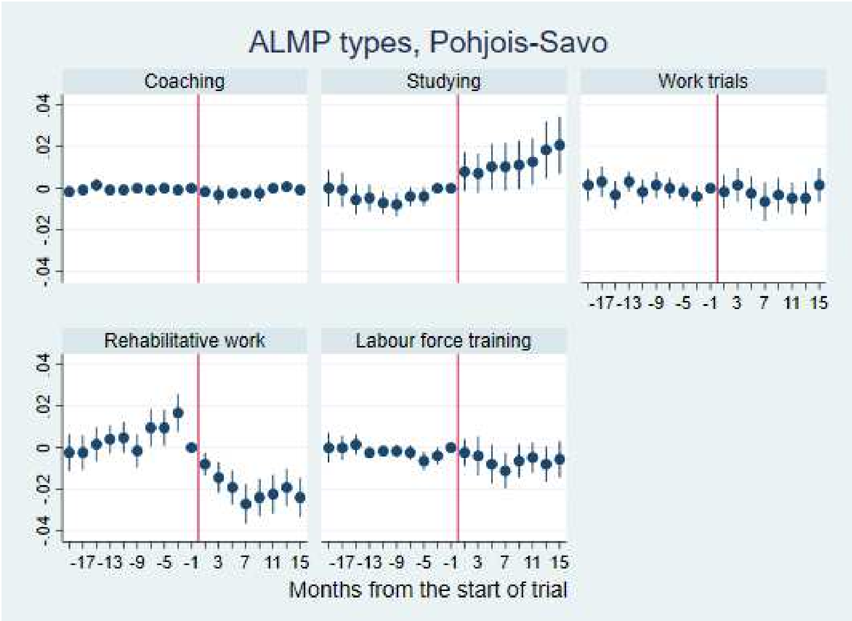
Figure 20: Pohjois-Savo, clustering by municipality



Notes. Figure plots regression coefficients from model (6.10). The coefficients are monthly treatment effects and the coefficient of period -1 (July 2017) is normalized to zero.

The effects on different ALMP types in Pohjois-Savo are plotted in Figure 21. The new work experience program in Pohjois-Savo was coded with same service code in the data as the rehabilitative work programs it replaced, but as can be seen from Figure 21 fewer participants participated in the program than used to participate in rehabilitative work programs. Regarding other ALMP types, studying increased statistically significantly. By looking at the effects on the ALMP types, Pohjois-Savo looks like an opposite of Pirkanmaa: in Pohjois-Savo rehabilitative work decreases and studying increases, whereas the opposite was true in the Pirkanmaa trial area.

Figure 21: Pohjois-Savo, ALMP types

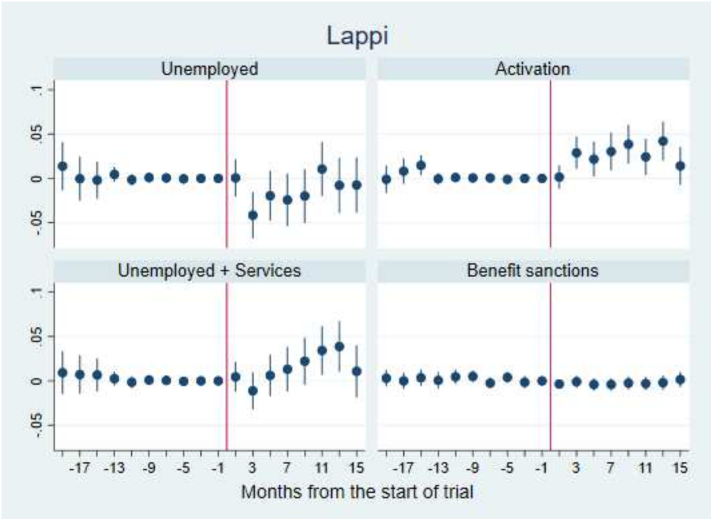


Notes. Figure plots regression coefficients from model (6.10). The coefficients are monthly treatment effects and the coefficient of period −1 (July 2017) is normalized to zero.

7.2.4 Lappi area

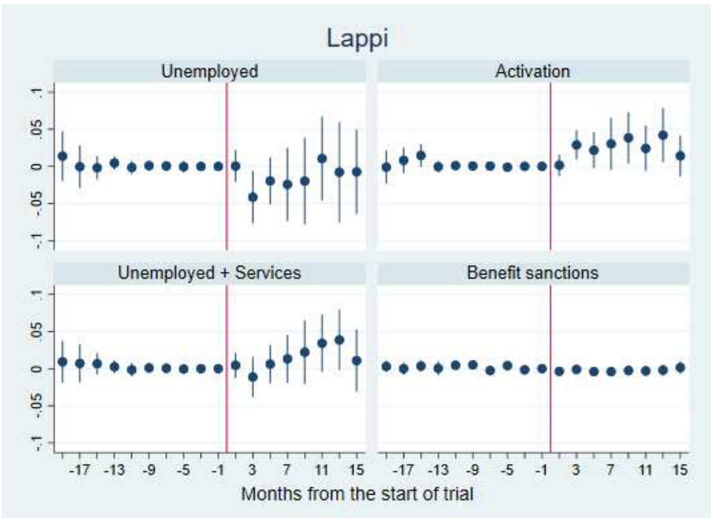
The main results in Lappi area are presented in Figure 22: short term decrease in registered unemployment (lasting one period), increase in extended unemployment and increase in activation by over 3 percentage points are found. The trial has no effect on sanctions. The increase in extended unemployment is driven by the increase in activation, as the registered unemployment does not rise. It will take longer time to see if the increased activation results in better employment prospects. Figure 23 presents the results with standard errors clustered at the municipality level. Clustering on municipality level makes the estimates largely insignificant, although some effects on probability of activation are still significant.

Figure 22: Lappi, main results



Notes. Figure plots regression coefficients from model (6.10). The coefficients are monthly treatment effects and the coefficient of period -1 (July 2017) is normalized to zero.

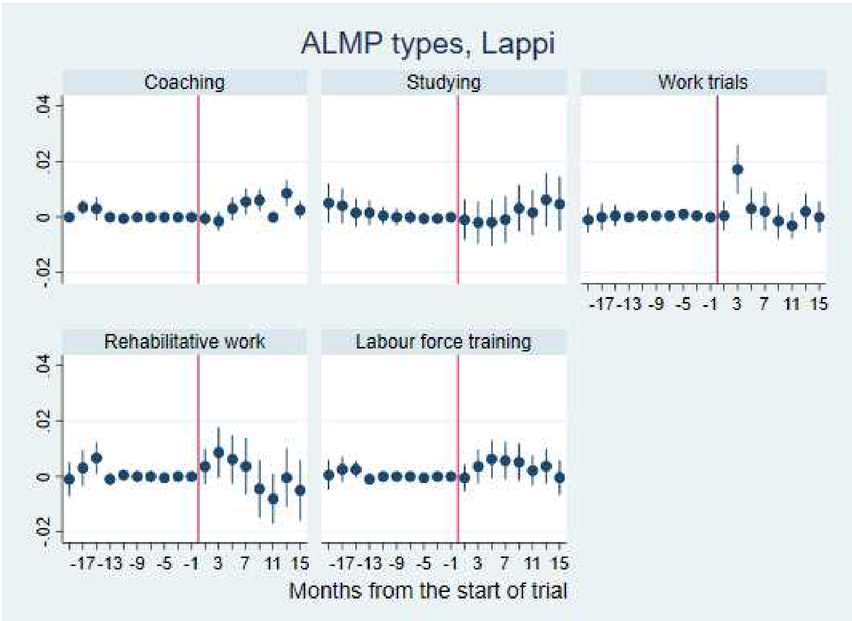
Figure 23: Lappi, clustering by municipality



Notes. Figure plots regression coefficients from model (6.10). The coefficients are monthly treatment effects and the coefficient of period -1 (July 2017) is normalized to zero.

The effects on ALMP types can be seen from Figure 24 below. There are no big changes in ALMP types in Lappi area during the trial, but the probability of participating in coaching increases statistically significantly, as does participation in work trials during one period.

Figure 24: Lappi, ALMP types

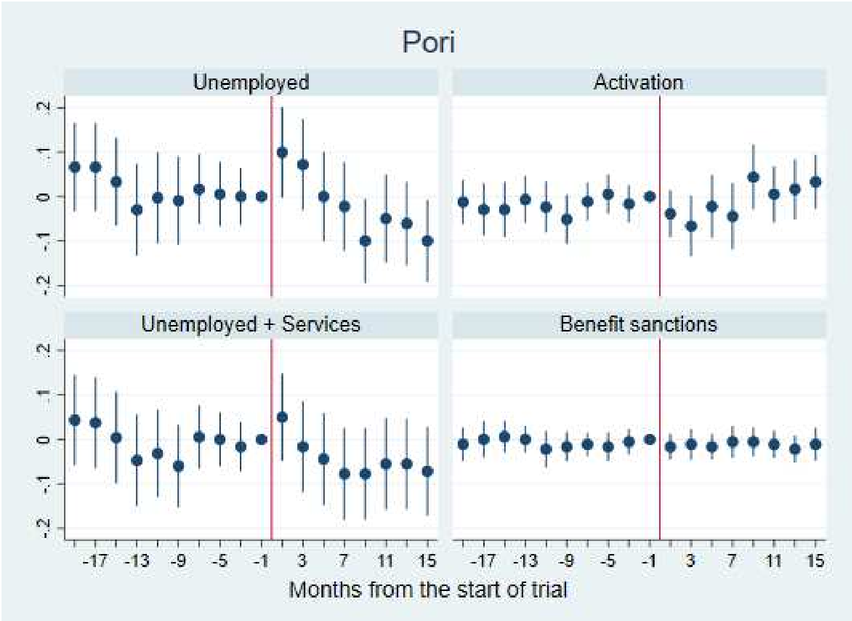


Notes. Figure plots regression coefficients from model (6.10). The coefficients are monthly treatment effects and the coefficient of period -1 (July 2017) is normalized to zero.

7.2.5 Pori area

In Pori trial area, there was just one treated municipality: the city of Pori. Since the treatment group consists of under 25-year-olds who have been unemployed over 6 months, the sample size is rather small: in the treatment group there are only 181 individuals, whereas in other areas there were thousands of participants, and even over ten thousand (in Pirkanmaa).

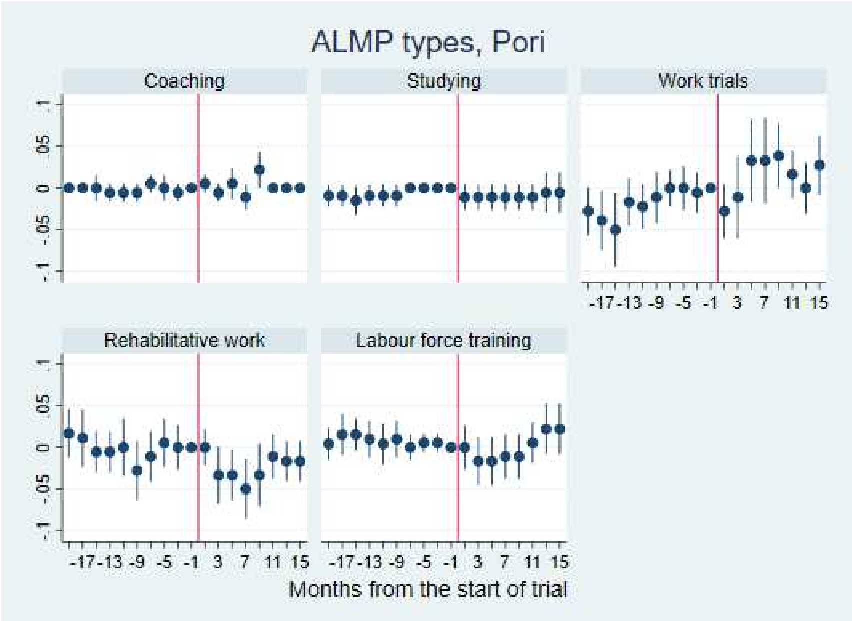
Figure 25: Pori, main results



Notes. Figure plots regression coefficients from model (6.10). The coefficients are monthly treatment effects and the coefficient of period -1 (July 2017) is normalized to zero.

Figure 25 shows main results in Pori area. The trial had no effects on extended unemployment, activation or sanctions. Registered unemployment decreased such that the coefficient for period 15 is negative and statistically significant. In Figure 26, effects on ALMP types in Pori are showed: rehabilitative work programs decreased, but no other effects are found.

Figure 26: Pori, ALMP types



Notes. Figure plots regression coefficients from model (6.10). The coefficients are monthly treatment effects and the coefficient of period -1 (July 2017) is normalized to zero.

7.3 Robustness and validity

7.3.1 Pre-trends

In the figures where treatment effects are presented, the validity of the parallel trends assumption can be evaluated by looking at the pre-treatment coefficients. If the assumption holds, the coefficients should not be statistically different from zero. This tests whether the difference of treatment and control groups has been constant before the treatment: since the coefficient for period -1 is omitted, the differences in other other periods are compared to the difference in period -1. If the difference between groups did not change in any period before the trial (as the identification assumption requires), the pre-treatment coefficients should be zero.

Judging from the Figures, the pre-trends seem to be parallel in the fist trial. In the second trial, however, the trends are not parallel in all areas or all outcome variables. The pre-trends look problematic especially in Pirkanmaa, where activation interviews were conducted just before the start of the municipal employment trial. Those interviews caused a dip in the probability of unemployment before the trial, since more unemployment spells were ended in Pirkanmaa at that time. Since the control individuals were from other areas than Pirkanmaa, they were not affected by these interviews, which caused a change in trends. This problem in parallel trends is not however present in the result concerning the reduction in sanctions: in that result, pre-trends look parallel.

The pre-treatment coefficients can also be tested jointly; this means testing simultaneously whether all the coefficients are zero using F-test. The results of joint significance test performed for pre-treatment coefficients in the 2012-2015 analyses are presented in Table 3. For the second trial, results of the F-test are listed in Table 4 for main outcomes. Table 3 indicates that all the pre-treatment coefficients in the analysis of the first trial are in fact zero. Hence, the parallel trends can be assumed to hold in the analysis of the 2012–2015 trial. In the second trial, pre-trends are not zero in all areas or outcome variables according to the F-test of joint significance.

Table 3: Pre-trends, joint significance (12–15)

Outcome	Specification	F value	p-value
Employment	1	1.34	0.25
Employment	2	1.42	0.21
Employment	3	1.22	0.30
Employment	4	0.72	0.64
Employment	5	0.81	0.56
Employment	6	0.70	0.65
Unemployment	1	0.34	0.92
Unemployment	2	0.33	0.92
Unemployment	3	0.39	0.89
Unemployment	4	0.49	0.82
Unemployment	5	0.48	0.82
Unemployment	6	0.49	0.82

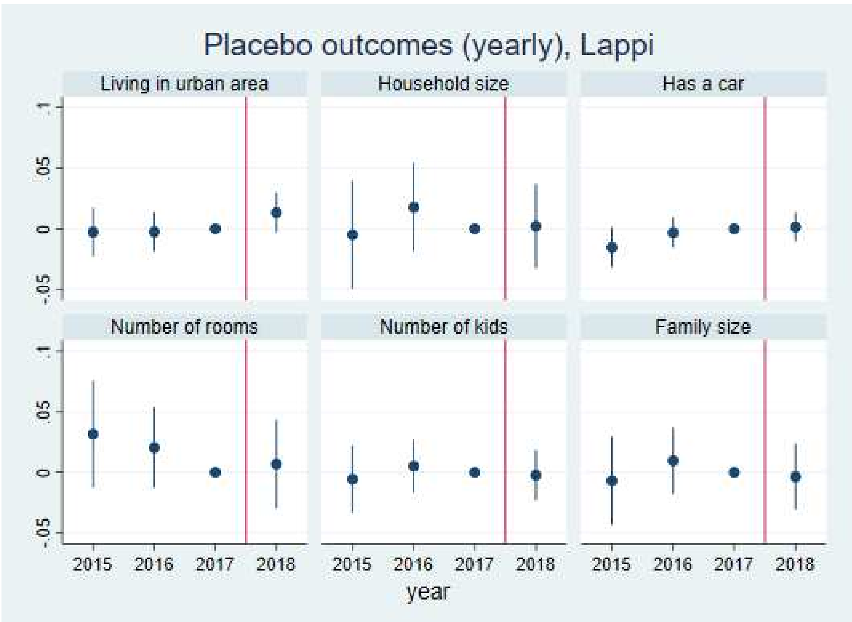
Table 4: Pre-trends, joint significance (17–18)

Area	Outcome	F value	p-value
Varsinais-Suomi	Unemployed	2.81	0.02
Varsinais-Suomi	Unemployed + Services	1.23	0.27
Varsinais-Suomi	Activation	2.00	0.04
Varsinais-Suomi	Sanctions	0.88	0.55
Pirkanmaa	Unemployed	2.65	0.004
Pirkanmaa	Unemployed + Services	3.21	< 0.001
Pirkanmaa	Activation	3.86	< 0.001
Pirkanmaa	Sanctions	0.75	0.66
Lappi	Unemployed	0.88	0.55
Lappi	Unemployed + Services	0.50	0.88
Lappi	Activation	1.31	0.23
Lappi	Sanctions	1.33	0.22
Pohjois-Savo	Unemployed	2.04	0.03
Pohjois-Savo	Unemployed + Services	1.65	0.10
Pohjois-Savo	Activation	1.29	0.24
Pohjois-Savo	Sanctions	0.51	0.87
Pori	Unemployed	0.39	0.94
Pori	Unemployed + Services	0.55	0.84
Pori	Activation	0.64	0.76
Pori	Sanctions	0.73	0.68

7.3.2 Placebo outcomes

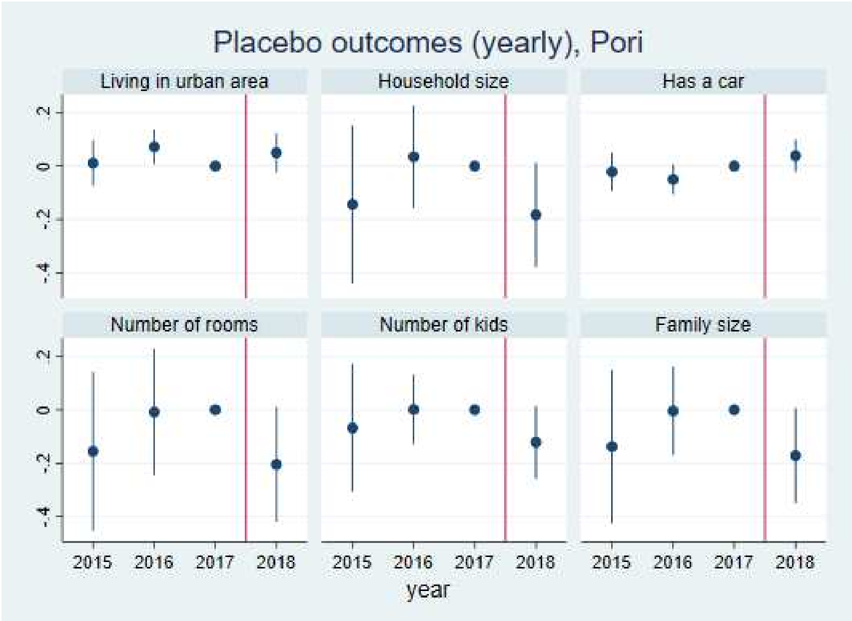
The validity of treatment and control groups can also be examined by looking at the effects on placebo outcomes. This means checking if the design produces an effect, even if there should not be one. I do the placebo outcomes check for all 5 trial areas in the 2017–2018 trial, using the same treatment and control groups (created using matching) as in the analysis. The placebo outcomes used are probability of living in an urban area, household size, having a car, number of rooms, number of kids and family size. As the basic variables are available yearly, I calculate yearly treatment effects on this placebo outcomes.

Figure 27: Placebo outcomes, Lappi



Notes. Figure plots regression coefficients from model (6.10). The coefficients are monthly treatment effects and the coefficient of period −1 (July 2017) is normalized to zero.

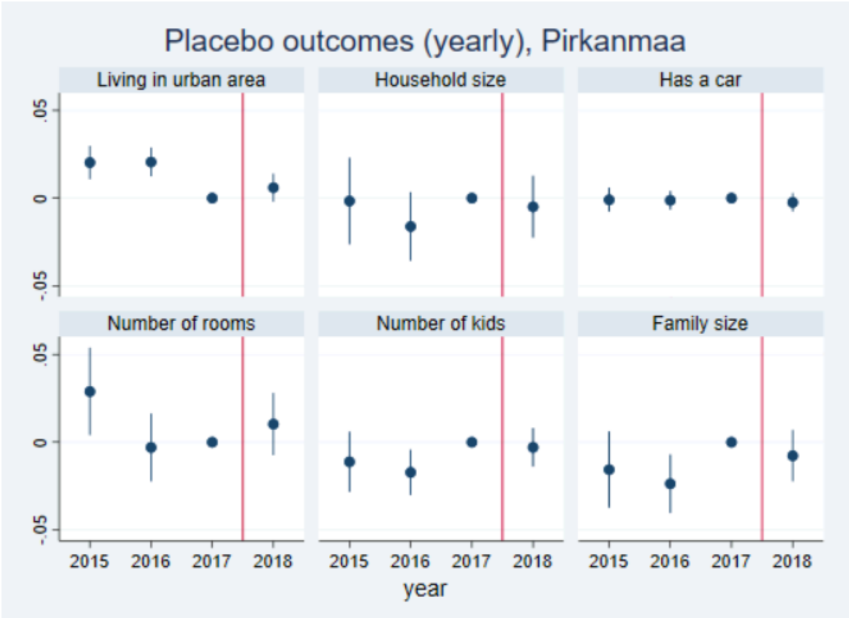
Figure 28: Placebo outcomes, Pori



Notes. Figure plots regression coefficients from model (6.10). The coefficients are monthly treatment effects and the coefficient of period -1 (year 2017) is normalized to zero.

As can be seen from Figures 27 and 28, the trial does not have any statistically significant effects on these placebo outcomes in Lappi or Pori areas: in the Lappi area, the point estimates are very close to zero, whereas in the Pori area some outcomes (family size, number of rooms, number of kids) are only barely insignificant. Fortunately, all effects are insignifcant, as they should be, since there are no reasons to believe the trial would affect these outcomes.

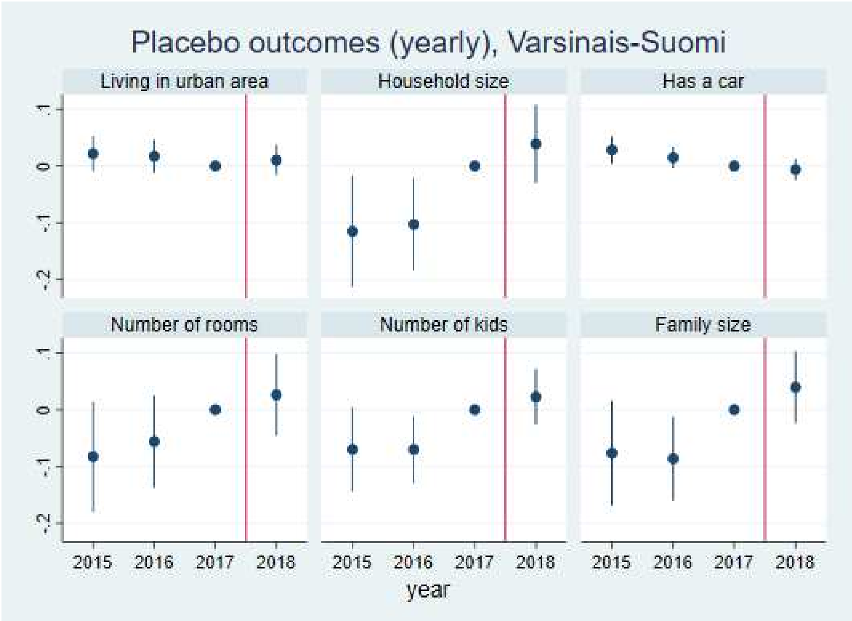
Figure 29: Placebo outcomes, Pirkanmaa



Notes. Figure plots regression coefficients from model (6.10). The coefficients are monthly treatment effects and the coefficient of period −1 (year 2017) is normalized to zero.

In Pirkanmaa area (Figure 29), there are no placebo effects (meaning effects after the start of treatment with parallel pre-trends), but the pre-trends are not parallel in some of the placebo outcomes, i.e. in urban area and or in number of rooms. This is not ideal and may indicate that the treatment and control groups used in Pirkanmaa area evaluation are not valid. This kind of judgement should, however, not be based on this test only.

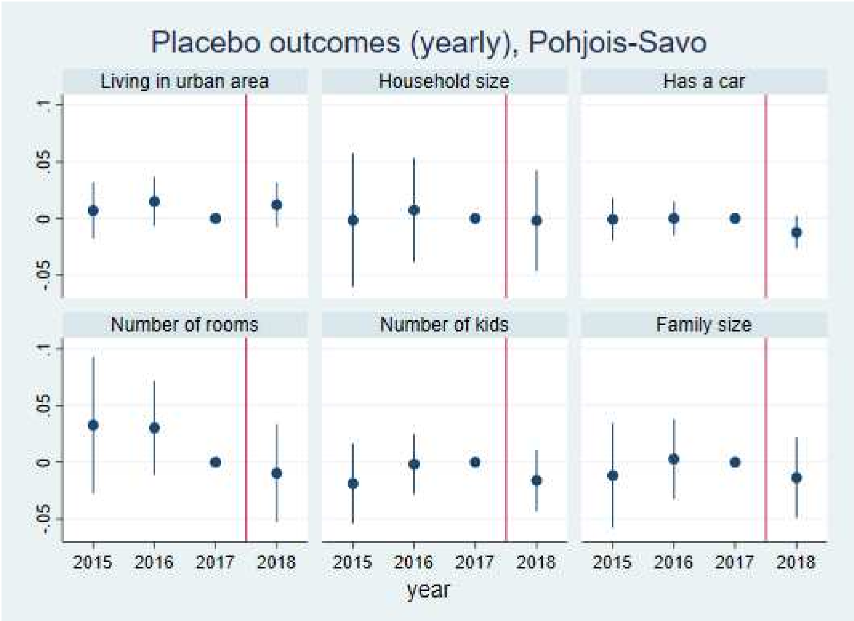
Figure 30: Placebo outcomes, Varsinais-Suomi



Notes. Figure plots regression coefficients from model (6.10). The coefficients are monthly treatment effects and the coefficient of period -1 (year 2017) is normalized to zero.

In Varsinais-Suomi area (Figure 30), the placebo outcome tests give results similar to the results from Pirkanmaa: in some outcomes, the trends are not parallel. At the start of treatment, however, no effect is found as expected. All of the variables with non-parallel pre-trends are related to the household size (household size, family size, number of kids). The treatment and control groups seem to have different trends in this type of variables. The dissimilarity of the pre-trends in placebo outcomes in Varsinais-Suomi area raises concerns about the validity of the treatment and control groups.

Figure 31: Placebo outcomes, Pohjois-Savo



Notes. Figure plots regression coefficients from model (6.10). The coefficients are monthly treatment effects and the coefficient of period −1 (year 2017) is normalized to zero.

In Pohjois-Savo, the effects on placebo outcomes (Figure 31) are not significant and the point estimates are very close to zero. Thus, when effects on placebo outcomes have been investigated in all 5 areas, it can be concluded that in 3 areas (Lappi, Pori, Pohjois-Savo) the placebo outcome tests support the validity of the design, whereas in two areas (Varsinais-Suomi and Pirkanmaa), it gives less promising results. Possible issues with the validity of the treament and control groups in both Pirkanmaa and Varsinais-Suomi have, therefore, been found both in the joint significance test for pre-trends as well as in this placebo outcome check.

7.4 Internal and external validity

Since the pre-trends are parallel in the analyses of the first trial (2012–2015), it seems that parallel trends can be assumed. Hence, the results of the first trial seem to be internally valid. That is, the method should identify the average causal effect of the treatment. Regarding external validity, the results from specifications 1–3 are better, since they include all working-age individuals in treated and control municipalities. This means that the estimated effects could be interpreted as effects on net employment. In the specifications 4–6, movers were removed in order to be able to cluster standard errors by municipality in an individual level fixed effects model. The removal of those who have moved may weaken the external validity of the results.

The second trial is more problematic. First, matching was used, which may increase the bias of the DiD-estimator (Chabé-Ferret, 2017). Further, the use of PSM as a matching method has been discredited strongly by King and Nielsen (2019). Secondly, the pre-trends are not parallel in some outcome variables in some areas; this is the case especially in Pirkanmaa area, where the activation interviews were arranged just before the start of the municipal employment trial. The pre-trends in the result regarding the reduction of sanctions in Pirkanmaa are, however, parallel. In other areas, pre-trends are parallel in most outcome variables.

As a robustness check, effects on placebo outcomes were investigated in all trial areas in the second trial. Testing placebo outcomes yielded no effects in Pori, Pohjois-Savo or Lappi, indicating that the treatment and control groups in those areas are valid. In Pirkanmaa and Varsinais-Suomi areas, the trends were not pre-trends were not parallel in some placebo outcome variables. There were, however, no placebo *effects*, meaning instances where the trends would have been similar before but not after the trial. Although the internal validity of the results can be questioned, the causal interpration of the results is in my view possible, at least in some areas and outcome variables.

The estimated effects are average treatment effects on the treated. Hence, they have to be interpreted as effects on the treated population (e.g. long-term unemployed in Pohjois-Savo and Lappi, or under 25-year-olds in Varsinais-Suomi). Effects for these groups may not be generalizable to other groups. Hence, the results may not reflect what the effects would be if everyone was moved to a decentralized system. There are also additional concerns: since this was an experiment, municipalities may not have all the resources they would if this was a permanent reform. Recruiting employees and the development of services may for instance have been inadequate. Because of these concerns, this evidence does not rule out that decentralization of employment services could have favourable effects.

8 Conclusions

The results from the two trials are different, as were the trials themselves: in the first trial an effect on employment was found, whereas the results indicate that the second trial did not have employment effects. This can be interpreted such that decentralization of employment services (the second trial) does not increase employment, but additional employment services provided by municipalities (the first trial), may have employment effects.

The results of the 2017–2018 trial are similar to Mergele and Weber (2020) in that the decentralization seems to have negative employment effects (rise in extended unemployment) at least in some areas. The results are similar to many active labor market policy evaluations: especially the short term effects in some areas are negative (a rise in unemployment), but the long-term point estimates are closer to zero.

In some areas the second trial shifted the focus of activation to different services: e.g. probability of participation in rehabibilitative work programs increased substantially – 10 percentage points – in Pirkanmaa during the trial. This has been already noted earlier by others, and it has been critized since rehabilitative work programs should by law only be for those who are in need of rehabilitation. The increase in rehabilititive work programs in Pirkanmaa can also be seen as evidence on the cost-shifting municipalities may do if they are given the responsibility to arrange employment services.

The effects on benefit sanctions were studied because of the hypothesis that municipalities would optimize their expenses and therefore decrease benefit sanctions. This would be due to the fact that municipalities have to pay 50 percent of income support and housing allowance. Decrease in sanctions was found in two areas, Pirkanmaa and Varsinais-Suomi, which are the biggest trial areas in terms of population. The reduction in sanctions may very well be a mechanism lowering employment, since reduced sanctions lowers the incentives of individuals to seek employment.

If the decentralization reform is implemented, this study suggests that the incentives of municipalities have to be designed in such a way that there are no incentives to give less sanctions. Earlier research by Lundin and Skedinger (2006) as well as Mergele and Weber (2020) also suggests that municipalities may try to do cost-shifting, so the government should not implement this reform without first designing the system carefully so that the cost-shifting opportunities are minimized. The incentive structure could be designed in such a way that municipalities would have incentives to get job seekers employed. There could still, however, be a danger that municipalities would be less strict in sanctioning than employment offices, due to the objectives of local politicians.

Only looking at employment effects of the trials has been criticised by some: authors of the Pirkanmaa municipal level analysis (Arnkil et al., 2019) argue in Kuntalehti (2019) that the trials may have had impacts on the *paths* that some long term job seekers are. According to them, it is not even possible to get these people employed right away but instead these services can improve their quality of life and may in very long-term yield better employment outcomes. This claim may very well be true, but since the possible full decentralization reform is planned to affect everyone, it is not very convincing to rationalize not seeing any employment effects by using this argument.

Decentralization may be a plausible way to reform employment services, but in the light of previous research and the results of this study, it would be very optimistic to expect huge employment effects. Therefore, the reform should not be thought as a tool to increase employment.

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A Appendix: Matching tables

Table A.1: Varsinais-Suomi, after matching

Variable	Treated	Control	t value	p value
Length of unemployment	3.718	3.730	-0.07	0.85
Education.2	0.019	0.014	1.06	0.29
Education.3	0.603	0.605	-0.10	0.92
Education.4	0.277	0.296	-1.20	0.23
Education.5	0.079	0.069	1.10	0.27
Activation, last 12 months	0.299	0.305	-0.41	0.69
Age	21.43	21.42	0.18	0.85
Gender.2 (woman)	0.48	0.48	0.00	1.00
Urban area	0.95	0.96	-0.33	0.75
Size of family	1.92	1.92	0.14	0.89
Marital status.2	0.048	0.045	0.48	0.63
Marital status.3	0.005	0.002	1.39	0.17
Marital status.4	0.0006	0.0006	0.00	1.00
Number of observations	1746	1746		

Notes. Length of unemployment means the number of consecutive unemployment months when the trial started. Education variable is from *TEM Työnhakija* module and has the following categories: 1 = folk school, 2 = middle school, 3 = comprehensive school, 4 = high school, 5 = other. Since everyone is under 25 years old in this matching, nobody should have education levels 1 or 2 (folk schools or middle schools have not existed in their lifetimes). Hence, the data has some errors (individuals in Education.1 or Education.2 should be in some other category). Marital status has the following categories: 1 = not married, 2 = married or in registered union, 3 = divorced, 4 = widow.

Table A.2: Pirkanmaa, after matching

Variable	Treated	Control	t value	p value
Length of unemployment	12.192	12.436	-1.54	0.12
Unemployment months, last 12 months	8.681	8.656	0.54	0.59
Gender.2 (woman)	0.447	0.446	0.12	0.90
Urban area	0.933	0.932	0.14	0.89
Employed in 2015	0.101	0.102	-0.36	0.72
Employed in 2016	0.078	0.082	-1.26	0.21
Marital status.2	0.246	0.235	2.20	0.03
Marital status.3	0.139	0.137	0.33	0.74
Marital status.4	0.0070	0.0054	1.68	0.09
Age	37.46	37.54	-0.53	0.60
Size of family	2.03	1.98	3.28	0.001
Number of observations	13 876	13 876		

Notes. Length of unemployment means the number of consecutive unemployment months when the trial started. Marital status has the following categories: 1 = not married, 2 = married or in registered union, 3 = divorced, 4 = widow.

Table A.3: Pohjois-Savo, after matching

Variable	Treated	Control	t value	p value
Length of unemployment	24.87	25.67	-1.38	0.17
Unemployed under 12 months	0.116	0.109	0.51	0.613
Education.2	0.128	0.26	0.18	0.86
Education.3	0.304	0.322	-0.99	0.32
Education.4	0.322	0.306	0.86	0.39
Education.5	0.030	0.032	-0.23	0.82
Activation, last 12 months	0.072	0.074	-0.15	0.88
Age	50.4	50.5	-0.28	0.78
Gender.2 (woman)	0.494	0.496	-0.12	0.91
Urban area	0.84	0.87	-1.92	0.06
Size of family	2.07	1.97	2.07	0.04
Marital status.2	0.40	0.41	-0.32	0.75
Marital status.3	0.21	0.21	0.15	0.88
Marital status.4	0.023	0.025	-0.39	0.70
Number of observations	1254	1254		

Notes. Length of unemployment means the number of consecutive unemployment months when the trial started. Education variable is from *TEM Työnhakija* module and has the following categories: 1 = folk school, 2 = middle school, 3 = comprehensive school, 4 = high school, 5 = other. Marital status has the following categories: 1 = not married, 2 = married or in registered union, 3 = divorced, 4 = widow.

Table A.4: Lappi, after matching

Variable	Treated	Control	t value	p value
Length of unemployment	24.95	24.85	0.27	0.79
Unemployment months, last 12 months	11.913	11.913	-0.00	1.00
Unemployed under 12 months	0.02	0.02	-0.00	1.00
Education.2	0.096	0.095	0.11	0.91
Education.3	0.48	0.48	0.00	1.00
Education.4	0.198	0.201	-0.24	0.81
Education.5	0.03	0.03	0.76	0.45
Activation, last 12 months	0.05	0.04	1.69	0.09
Age	50.6	50.7	-0.21	0.83
Gender.2 (woman)	0.41	0.41	0.16	0.87
Number of observations	1974	1974		

Notes. Length of unemployment means the number of consecutive unemployment months when the trial started. Education variable is from *TEM Työnhakija* module and has the following categories: 1 = folk school, 2 = middle school, 3 = comprehensive school, 4 = high school, 5 = other.

Table A.5: Pori, after matching

Variable	Treated	Control	t value	p value
Length of unemployment	7.99	8.08	-0.15	0.88
Unemployment months in 12 months	9.34	9.36	-0.07	0.94
Education.2	0.01	0.006	0.58	0.56
Education.3	0.67	0.73	-1.15	0.25
Education.4	0.22	0.20	0.51	0.61
Education.5	0.07	0.05	0.88	0.38
Work years.1	0.19	0.18	0.40	0.69
Work years.2	0.07	0.03	1.74	0.08
Work years.3	0.027	0.027	0.00	1.00
Work years.4	0	0	.	.
Work years.5	0	0	.	.
Work years. 6	0	0	.	.
Employed in 2016.4	0.09	0.08	0.38	0.70
Employed in 2015.5	0.12	0.10	0.50	0.62
Age	21.66	21.60	0.44	0.66
Gender.2 (woman)	0.47	0.45	0.42	0.67
Number of observations	181	181		

Notes. Length of unemployment means the number of consecutive unemployment months when the trial started. Education variable is from *TEM Työnhakija* module and has the following categories: 1 = folk school, 2 = middle school, 3 = comprehensive school, 4 = high school, 5 = other. Work years variable indicates how many years an individual has been employed between 2011 and 2016.

B Appendix: Regression tables

Table B.6: Regression table (employment), 12-15

	Employment					
	(a)	(b)	(c)	(d)	(e)	(f)
Treatment * 2005	-0.00319	-0.00409	-0.00352	-0.00574	-0.00436	-0.00372
	(0.0048)	(0.0044)	(0.0020)	(0.0062)	(0.0054)	(0.0032)
Treatment * 2006	-0.00432	-0.00523	-0.00345*	-0.00573	-0.00478	-0.00296
	(0.0043)	(0.0037)	(0.0018)	(0.0053)	(0.0047)	(0.0026)
Treatment * 2007	-0.00358	-0.00396	-0.00195	-0.00426	-0.00337	-0.00172
	(0.0036)	(0.0033)	(0.0015)	(0.0041)	(0.0038)	(0.0022)
Treatment * 2008	-0.00291	-0.00361	-0.00225	0.00387	-0.00363	-0.00252
	(0.0032)	(0.0032)	(0.0023)	(0.0031)	(0.0032)	(0.0024)
Treatment * 2009	-0.00043	-0.00062	0.00121	0.00050	0.00060	0.00173
	(0.0027)	(0.0027)	(0.0028)	(0.0026)	(0.0026)	(0.0028)
Treatment * 2010	-0.00206	-0.00190	-0.00044	0.00074	-0.00074	0.00010
	(0.0012)	(0.0012)	(0.0016)	(0.0012)	(0.0012)	(0.0014)
Treatment * 2011	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)
Treatment * 2012	0.00266	0.00260	0.00140	0.00357**	0.00356**	0.00238
	(0.0024)	(0.0022)	(0.0023)	(0.0018)	(0.0018)	(0.0018)
Treatment * 2013	0.00366	0.00401	0.00231	0.00594**	0.00595**	0.00428*
	(0.0028)	(0.0026)	(0.0023)	(0.0024)	(0.0024)	(0.0023)
Treatment * 2014	0.00462*	0.00430*	0.00211	0.00848***	0.00845***	0.00570***
	(0.0027)	(0.0022)	(0.0022)	(0.0023)	(0.0023)	(0.0018)
Treatment * 2015	0.00223	0.00127	-0.00032	0.00621***	0.00618***	0.00299**
	(0.0027)	(0.0024)	(0.0021)	(0.0022)	(0.0022)	(0.0016)
Treatment * 2016	0.00079	-0.00028	-0.00270	0.00650**	0.00645**	0.00201
	(0.0033)	(0.0032)	(0.0026)	(0.0025)	(0.0025)	(0.0020)
Basic controls		X	X		X	X
Further controls			X			X
Municipality FE	X	X	X			
Individual FE				X	X	X
Year FE	X	X	X	X	X	X
R^2	0.003	0.158	0.634	0.014	0.107	0.620
N	17.3 million	17.3 million	17.3 million	9.4 million	9.4 million	9.4 million

Notes. Standard errors clustered by municipality in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B.7: Regression table (unemployment), 12-15

	Unemployment					
	(a)	(b)	(c)	(d)	(e)	(f)
Treatment * 2005	0.00161	0.00198	0.00135	0.00190	0.00202	0.00153
	(0.0036)	(0.0035)	(0.0022)	(0.0043)	(0.0043)	(0.0033)
Treatment * 2006	0.00203	0.00236	0.00159	0.00232	0.00239	0.00190
	(0.0025)	(0.0025)	(0.0015)	(0.0031)	(0.0031)	(0.0022)
Treatment * 2007	0.00139	0.00175	0.00075	0.00127	0.00135	0.00068
	(0.0019)	(0.0020)	(0.0012)	(0.0024)	(0.0024)	(0.0017)
Treatment * 2008	0.00137	0.00164	0.00043	0.00133	0.00131	0.00067
	(0.0019)	(0.0019)	(0.0017)	(0.0024)	(0.0024)	(0.0020)
Treatment * 2009	-0.00034	-0.00015	-0.00159	-0.00171	-0.00172	-0.00236
	(0.0020)	(0.0018)	(0.0023)	(0.0021)	(0.0021)	(0.0021)
Treatment * 2010	0.00044	0.00049	0.00015	-0.00030	-0.00030	-0.00039
	(0.0009)	(0.0009)	(0.0009)	(0.0010)	(0.0010)	(0.0010)
Treatment * 2011	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)
Treatment * 2012	-0.00142	-0.00153	-0.00108	-0.00150	-0.00150	-0.00160
	(0.0018)	(0.0018)	(0.0018)	(0.0016)	(0.0016)	(0.0017)
Treatment * 2013	-0.00193	-0.00213	-0.00120	-0.00185	-0.00185	-0.00181
	(0.0020)	(0.0020)	(0.0018)	(0.0024)	(0.0024)	(0.0023)
Treatment * 2014	-0.00154	-0.00171	-0.00047	-0.00239	-0.00239	-0.00207
	(0.0019)	(0.0019)	(0.0014)	(0.0025)	(0.0025)	(0.0020)
Treatment * 2015	0.00180	0.00147	0.00256**	0.00213	0.00212	0.00223
	(0.0021)	(0.0020)	(0.0012)	(0.0029)	(0.009)	(0.0022)
Treatment * 2016	0.00276	0.00236	0.00259	0.00257	0.00256	0.00226
	(0.0024)	(0.0023)	(0.0016)	(0.0035)	(0.0035)	(0.0027)
Basic controls		X	X		X	X
Further controls			X			X
Municipality FE	X	X	X			
Individual FE				X	X	X
Year FE	X	X	X	X	X	X
R^2	0.002	0.018	0.233	0.0008	0.009	0.043
N	17.3 million	17.3 million	17.3 million	9.4 million	9.4 million	9.4 million

Notes. Standard errors clustered by municipality in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, * p < 0.1

Table B.8: Regression table, 17-18 (Varsinais-Suomi)

	(1) Unemployment	(2) Activation	(3) Unemployed + Services	(4) Sanctions
Treatment * 1/2016	-0.01129	0.01527	0.00164	0.01028
	(0.0140)	(0.0099)	(0.0154)	(0.0075)
Treatment * 3/2016	0.00494	0.01340	0.01367	0.01142
	(0.0143)	(0.0105)	(0.0158)	(0.0076)
Treatment * 5/2016	0.04023***	-0.00693	0.03211**	0.00378
	(0.0150)	(0.0101)	(0.0162)	(0.0077)
Treatment * 7/2016	-0.00134	0.00374	0.00004	0.00614
	(0.0163)	(0.0091)	(0.0168)	(0.0074)
Treatment * 9/2016	-0.00160	0.01610	0.01216	0.00782
	(0.0158)	(0.0116)	(0.0169)	(0.0074)
Treatment * 11/2016	0.00934	0.00019	-0.00094	0.01014
	(0.0161)	(0.0121)	(0.0170)	(0.0071)
Treatment * 1/2017	0.01489	-0.00458	0.00458	0.00745
	(0.0164)	(0.0116)	(0.0169)	(0.0068)
Treatment * 3/2017	0.02348	-0.02176	-0.00344	0.01088
	(0.0168)	(0.0114)	(0.0168)	(0.0071)
Treatment * 5/2017	0.01661	-0.00630	0.00344	0.00000
	(0.0168)	(0.0105)	(0.0160)	(0.0066)
Treatment * 7/2017	0	0	0	0
	(.)	(.)	(.)	(.)
Treatment * 9/2017	0.07560***	0.00057	0.07216***	-0.02176***
	(0.0169)	(0.0115)	(0.0165)	(0.0066)
Treatment * 11/2017	0.00286	0.01088	0.01088	-0.01088
	(0.0169)	(0.0130)	(0.0169)	(0.0064)
Treatment * 1/2018	0.02049	0.02234*	0.03648**	-0.00677
	(0.01687)	(0.0129)	(0.0169)	(0.0065)
Treatment * 3/2018	-0.00197	0.03439***	0.02147	-0.00734*
	(0.0154)	(0.0129)	(0.0169)	(0.0069)
Treatment * 5/2018	0.01121	0.03319***	0.03461**	-0.01307
	(0.0154)	(0.0125)	(0.0169)	(0.0068)
Treatment * 7/2018	0.02568	-0.00020	0.02142	-0.01133**
	(0.0160)	(0.0110)	(0.0168)	(0.0073)
Treatment * 9/2018	0.00419	0.00329	0.00744	-0.01477***
	(0.0145)	(0.0116)	(0.0162)	(0.0075)
Treatment * 11/2018	0.00129	0.01995	0.01431	-0.02395***
	(0.0143)	(0.0119)	(0.0162)	(0.0073)
Individual FE	X	X	X	X
Month FE	X	X	X	X
R^2	0.135	0.015	0.103	0.007
N	62 442	62 442	62 442	62 442

Notes. Standard errors clustered by individual in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, * p < 0.1

Table B.9: Regression table, 17-18 (Pirkanmaa)

	(1) Unemployment	(2) Activation	(3) Unemployed + Services	(4) Sanctions
Treatment * 1/2017	0.00580	0.00901	0.01061*	0.00223
	(0.0060)	(0.0041)	(0.0057)	(0.0025)
Treatment * 3/2017	0.01433**	0.00435	0.01207**	0.00156
	(0.0060)	(0.0043)	(0.0057)	(0.0025)
Treatment * 5/2016	0.01482**	-0.00378	0.00562	0.00214
	(0.0060)	(0.0041)	(0.0056)	0.0024)
Treatment * 7/2016	0.01173**	0.00297	0.00950*	0.00285
	(0.0058)	(0.0038)	(0.0054)	(0.0025)
Treatment * 9/2016	0.01448**	-0.00395	0.00365	0.00452*
	(0.0059)	(0.0041)	(0.0052)	(0.0024)
Treatment * 11/2016	0.00777	0.00498	0.00370	0.00159
	(0.0058)	(0.0042)	(0.0051)	(0.0023)
Treatment * 1/2017	-0.00368	0.00519	-0.00476	0.00137
	(0.0056)	(0.0038)	(0.0049)	(0.0022)
Treatment * 3/2017	-0.00223	-0.00404	-0.01067**	0.00173
	(0.0055)	(0.0037)	(0.0047)	(0.0022)
Treatment * 5/2017	-0.00850	0.00137	-0.01175***	-0.00065
	(0.0049)	(0.0030)	(0.0042)	(0.0021)
Treatment * 7/2017	0	0	0	0
	(.)	(.)	(.)	(.)
Treatment * 9/2017	0.04591***	-0.01903***	0.01975***	-0.00598***
	(0.0053)	(0.0040)	(0.0042)	(0.0019)
Treatment * 11/2017	0.04432***	-0.01766***	0.02032***	-0.01074***
	(0.0058)	(0.0046)	(0.0047)	(0.0021)
Treatment * 1/2018	0.02979***	-0.00191	0.02464***	-0.01090***
	(0.0059)	(0.0047)	(0.0050)	(0.0021)
Treatment * 3/2018	0.00113	0.01680***	0.01591***	-0.01053***
	(0.0060)	(0.0050)	(0.0052)	(0.0022)
Treatment * 5/2018	-0.04693***	0.04908***	0.00028	-0.00770***
	(0.0060)	(0.0051)	(0.0055)	(0.0023)
Treatment * 7/2018	-0.06701***	0.07841***	0.00864	-0.00662***
	(0.0060)	(0.0050)	(0.0055)	(0.0024)
Treatment * 9/2018	-0.06943***	0.07903***	0.00564	-0.00921***
	(0.0059)	(0.0053)	(0.0057)	(0.0025)
Treatment * 11/2018	-0.08451***	0.07941***	-0.00784	-0.00740***
	(0.0058)	(0.0054)	(0.0057)	(0.0025)
Individual FE	X	X	X	X
Month FE	X	X	X	X
R^2	0.082	0.025	0.042	0.004
N	497 166	497 166	497 166	497 166

Notes. Standard errors clustered by individual in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, * p < 0.1

Table B.10: Regression table, 17-18 (Lappi)

	(1) Unemployment	(2) Activation	(3) Unemployed + Services	(4) Sanctions
Treatment * 1/2017	0.01378	-0.00101	0.00922	0.00304
	(0.0138)	(0.0080)	(0.0123)	(0.0048)
Treatment * 3/2017	-0.00043	0.00812	0.00717	0.00000
	(0.0127)	(0.0074)	(0.0111)	(0.0046)
Treatment * 5/2016	-0.00199	0.01471**	0.00665	0.00355
	(0.0107)	(0.0059)	(0.0096)	(0.0048)
Treatment * 7/2016	0.00454	-0.00048	0.00255	0.00051
	(0.0042)	(0.0032)	(0.0041)	(0.0049)
Treatment * 9/2016	-0.00154	0.00105	-0.00151	0.00456
	(0.0034)	(0.0026)	(0.0033)	(0.0041)
Treatment * 11/2016	0.00099	0.00054	0.00103	0.00507
	(0.0029)	(0.0022)	(0.0029)	(0.0034)
Treatment * 1/2017	0.00051	0.00051	0.00051	-0.00253
	(0.0026)	(0.0021)	(0.0025)	(0.00324)
Treatment * 3/2017	-0.00051	-0.00101	-0.00051	0.00405
	(0.0029)	(0.0020)	(0.0027)	(0.0032)
Treatment * 5/2017	0.00000	-0.00000	0.00000	-0.00152
	(0.0024)	(0.0012)	(0.0023)	(0.0034)
Treatment * 7/2017	0	0	0	0
	(.)	(.)	(.)	(.)
Treatment * 9/2017	0.00051	0.00152	0.00456	-0.00355
	(0.0108)	(0.0069)	(0.0086)	(0.0027)
Treatment * 11/2017	-0.04154***	0.02888***	-0.01114	-0.00101
	(0.0133)	(0.0093)	(0.0106)	(0.0034)
Treatment * 1/2018	-0.01960	0.02168**	0.00615	-0.00405
	(0.0143)	(0.0100)	(0.0118)	(0.0035)
Treatment * 3/2018	-0.02433	0.03036***	0.01316	-0.00405
	(0.0151)	(0.0109)	(0.0127)	(0.0035)
Treatment * 5/2018	-0.01988	0.03849***	0.02219	-0.00252
	(0.0155)	(0.0111)	(0.0136)	(0.0038)
Treatment * 7/2018	0.01056	0.02423**	0.03431**	-0.00303
	(0.0155)	(0.0104)	(0.0140)	(0.0038)
Treatment * 9/2018	-0.00787	0.04204***	0.03877***	-0.00200
	(0.0159)	(0.0112)	(0.0146)	(0.0042)
Treatment * 11/2018	-0.00742	0.01412	0.01079	0.00155
	(0.0159)	(0.0109)	(0.0151)	(0.0044)
Individual FE	X	X	X	X
Month FE	X	X	X	X
R^2	0.186	0.034	0.114	0.005
N	71 028	71 028	71 028	71 028

Notes. Standard errors clustered by individual in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, * p < 0.1

Table B.11: Regression table, 17-18 (Pori)

	(1) Unemployment	(2) Activation	(3) Unemployed + Services	(4) Sanctions
Treatment * 1/2017	0.06657	-0.01199	0.04358	-0.01072
	(0.0506)	(0.0256)	(0.0517)	(0.0136)
Treatment * 3/2017	0.06664	-0.02900	0.03769	0.00027
	(0.0504)	(0.0299)	(0.0523)	(0.0148)
Treatment * 5/2016	0.03336	-0.02912	0.00429	0.00592
	(0.0503)	(0.0316)	(0.0524)	(0.0123)
Treatment * 7/2016	-0.02951	-0.00659	-0.04710	0.00027
	(0.0529)	(0.0270)	(0.0525)	(0.0096)
Treatment * 9/2016	-0.00269	-0.02341	-0.03158	-0.02214
	(0.0521)	(0.0294)	(0.0500)	(0.0158)
Treatment * 11/2016	-0.00895	-0.05116*	-0.06000	-0.01655
	(0.0504)	(0.0278)	(0.0474)	(0.0125)
Treatment * 1/2017	0.01657	-0.01105	0.00552	-0.01105
	(0.0400)	(0.0218)	(0.0361)	(0.0096)
Treatment * 3/2017	0.00552	0.00552	-0.00000	-0.01657
	(0.0367)	(0.0224)	(0.0308)	(0.0124)
Treatment * 5/2017	-0.00000	-0.01657	-0.01657	-0.00552
	(0.0323)	(0.0212)	(0.0277)	(0.0078)
Treatment * 7/2017	0	0	0	0
	(.)	(.)	(.)	(.)
Treatment * 9/2017	0.09945*	-0.03867	0.04972	-0.01657
	(0.0516)	(0.0268)	(0.0499)	(0.0096)
Treatment * 11/2017	0.07182	-0.06630*	-0.01657	-0.01105
	(0.0522)	(0.0350)	(0.0522)	(0.0111)
Treatment * 1/2018	-0.00000	-0.02210	-0.04420	-0.01657
	(0.0513)	(0.0357)	(0.0527)	(0.0096)
Treatment * 3/2018	-0.02210	-0.04420	-0.07735	-0.00552
	(0.0506)	(0.0381)	(0.0526)	(0.0111)
Treatment * 5/2018	-0.09945	0.04420	-0.07735	-0.00552
	(0.0480)	(0.0372)	(0.0523)	(0.0096)
Treatment * 7/2018	-0.04972	0.00552	-0.05525	-0.01105
	(0.0503)	(0.0320)	(0.0525)	(0.0078)
Treatment * 9/2018	-0.06077	0.01657	-0.05525	-0.02210
	(0.0481)	(0.0341)	(0.0520)	(0.0096)
Treatment * 11/2018	-0.09945**	0.03315*	-0.07182	-0.01105
	(0.0469)	(0.0309)	(0.0509)	(0.0110)
Individual FE	X	X	X	X
Month FE	X	X	X	X
R^2	0.211	0.024	0.163	0.004
N	6 504	6 504	6 504	6 504

Notes. Standard errors clustered by individual in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, * p < 0.1

Table B.12: Regression table, 17-18 (Pohjois-Savo)

	(1) Unemployment	(2) Activation	(3) Unemployed + Services	(4) Sanctions
Treatment * 1/2017	0.02386	-0.00476	0.01830	0.00156
	(0.0172)	(0.0103)	(0.0151)	(0.0062)
Treatment * 3/2017	0.04050**	-0.00472	0.02620	0.00076
	(0.0160)	(0.0093)	(0.0140)	(0.0059)
Treatment * 5/2016	0.04352***	-0.00787	0.03247***	-0.00005
	(0.0140)	(0.0081)	(0.0122)	(0.0058)
Treatment * 7/2016	0.00728	-0.00301	0.00268	-0.00244
	(0.0094)	(0.0061)	(0.0076)	(0.0058)
Treatment * 9/2016	0.00808	-0.00542	0.00026	0.00477
	(0.0093)	(0.0063)	(0.0070)	(0.0052)
Treatment * 11/2016	0.01287	-0.00782	0.00505	-0.00003
	(0.0093)	(0.0064)	(0.0067)	(0.0042)
Treatment * 1/2017	0.00877	0.00638	0.01116*	0.00159
	(0.0091)	(0.0065)	(0.0061)	(0.0042)
Treatment * 3/2017	0.01754**	-0.00239	0.01116**	-0.00399
	(0.0084)	(0.0063)	(0.0052)	(0.0040)
Treatment * 5/2017	0.00638	0.00638	0.00877**	-0.00000
	(0.0079)	(0.0061)	(0.0044)	(0.0032)
Treatment * 7/2017	0	0	0	0
	(.)	(.)	(.)	(.)
Treatment * 9/2017	-0.00399	-0.00957	-0.01675	-0.00319
	(0.0137)	(0.0087)	(0.0110)	(0.0030)
Treatment * 11/2017	-0.00558	-0.01675	-0.02153	-0.00638
	(0.0166)	(0.0110)	(0.0138)	(0.0039)
Treatment * 1/2018	-0.01952	-0.01106	-0.02739*	-0.00398
	(0.0181)	(0.0123)	(0.0154)	(0.0040)
Treatment * 3/2018	0.00273	-0.01983	-0.01629	-0.00956
	(0.0190)	(0.0127)	(0.0168)	(0.0044)
Treatment * 5/2018	0.03219*	-0.02781**	-0.00040	-0.00956
	(0.0195)	(0.0127)	(0.0179)	(0.0046)
Treatment * 7/2018	0.02182	-0.03582***	-0.01160	-0.00318
	(0.0195)	(0.0118)	(0.0182)	(0.0046)
Treatment * 9/2018	0.00735	-0.02143*	-0.01168	-0.00956
	(0.0199)	(0.0127)	(0.0189)	(0.0050)
Treatment * 11/2018	-0.01264	-0.01665	-0.02609	-0.00636
	(0.0200)	(0.0124)	(0.0193)	(0.0052)
Individual FE	X	X	X	X
Month FE	X	X	X	X
R^2	0.147	0.021	0.117	0.008
N	45 096	45 096	45 096	45 096

Notes. Standard errors clustered by individual in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

C Appendix: Participating municipalities

C.1 First trial (2012-2015)

Akaa, Espoo, Hämeenlinna, Hamina, Hartola, Hattula, Heinola, Helsinki, Hollola, Imatra, Jämsä, Janakkala, Joensuu, Jyväskylä, Kajaani, Kemi, Keuruu, Kokkola, Kotka, Kuhmo, Kuopio, Lahti, Lappeenranta, Lemi, Leppävirta, Lieksa, Luumäki, Merikarvia, Mikkeli, Muurame, Orimattila, Oulu, Outokumpu, Parikkala, Pomarkku, Pori, Pyhäjoki, Raahe, Ranua, Rautalampi, Rautjärvi, Rovaniemi, Ruokolahti, Savitaipale, Savonlinna, Seinäjoki, Siikajoki, Siilinjärvi, Suonenjoki, Sysmä, Taipalsaari, Tampere, Turku, Tuusniemi, Ulvila, Urjala, Valkeakoski, Vantaa, Varkaus

C.2 Second trial (2017-2018)

Kangasala, Kemi, Kemijärvi, Kuopio, Lempäälä, Naantali, Nokia, Orivesi, Paimio, Pirkkala, Pori, Punkalaidun, Raisio, Rovaniemi, Sastamala, Siilinjärvi, Sodankylä, Tampere, Tornio, Turku, Tuusniemi, Vesilahti, Ylöjärvi

D Appendix: 12–15 trial, treatment and control groups

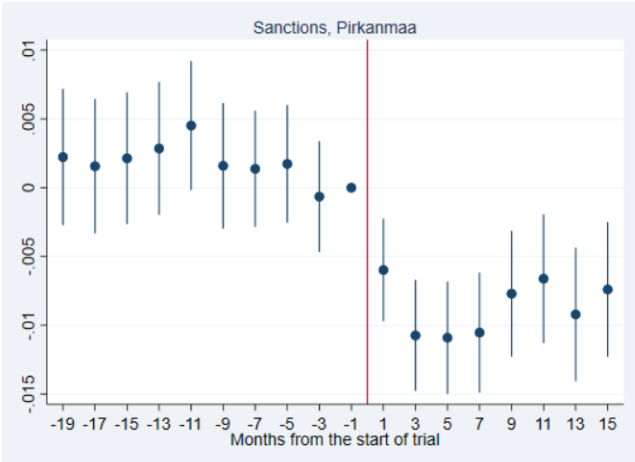
In the analysis of the 12–15 trial, where the unit of analysis is the municipality, treatment and control groups have to be created in the municipality level. Matching on unemployment rate, population and number of people in activation, Nieminen et al. (2020) find 51 control municipalities for the 2012-2015 trial municipalities. 8 of the biggest treated municipalities were discarded, leaving 51 municipalities to both the treatment and the control group.

Treatment group: Akaa, Espoo, Hämeenlinna, Hamina, Hartola, Hattula, Heinola, Hollola, Imatra, Jämsä, Janakkala, Kajaani, Kemi, Keuruu, Kokkola, Kotka, Kuhmo, Kuopio, Lappeenranta, Lemi, Leppävirta, Lieksa, Luumäki, Merikarvia, Mikkeli, Muurame, Orimattila, Outokumpu, Parikkala, Pomarkku, Pori, Pyhäjoki, Raahe, Ranua, Rautalampi, Rautjärvi, Rovaniemi, Ruokolahti, Savitaipale, Savonlinna, Seinäjoki, Siikajoki, Siilinjärvi, Suonenjoki, Sysmä, Taipalsaari, Tuusniemi, Ulvila, Urjala, Valkeakoski, Varkaus

Control group: Äänekoski, Enontekiö, Forssa, Haapavesi, Huittinen, Hyvinkää, Ii, Iisalmi, Jämijärvi, Kangasniemi, Kankaanpää, Kannus, Kärsämäki, Kaskinen, Kemijärvi, Kempele, Kinnula, Kitee, Kittilä, Konnevesi, Kontiolahti, Kouvola, Kuusamo, Kyyjärvi, Laitila, Loimaa, Loviisa, Mänttä-Vilppula, Muhos, Naantali, Nokia, Nurmes, Pieksämäki, Porvoo, Pyhäntä, Rantasalmi, Rauma, Riihimäki, Ristijärvi, Saarijärvi, Salla, Salo, Sastamala, Suomussalmi, Tohmajärvi, Toivakka, Tornio, Utsjoki, Vaala, Vaasa, Veteli

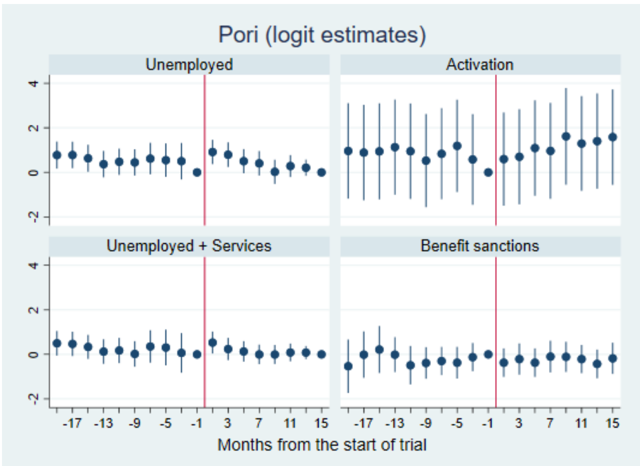
E Appendix: Additional Figures

Sanctions, Pirkanmaa



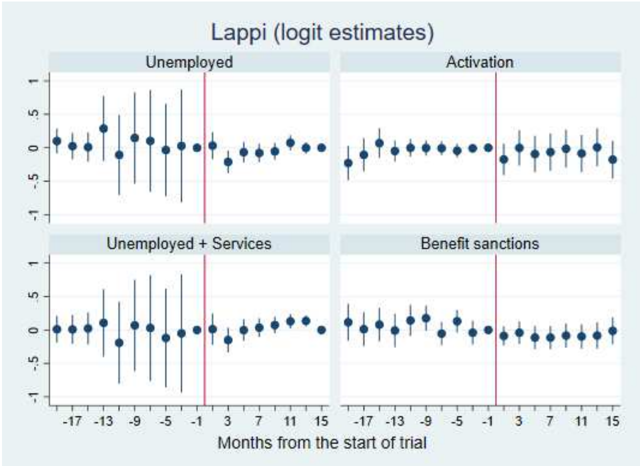
Notes. Figure plots regression coefficients from model (6.10). The coefficients are bimonthly treatment effects and the coefficient of period −1 (July 2017) is normalized to zero.

Logit results, Pori



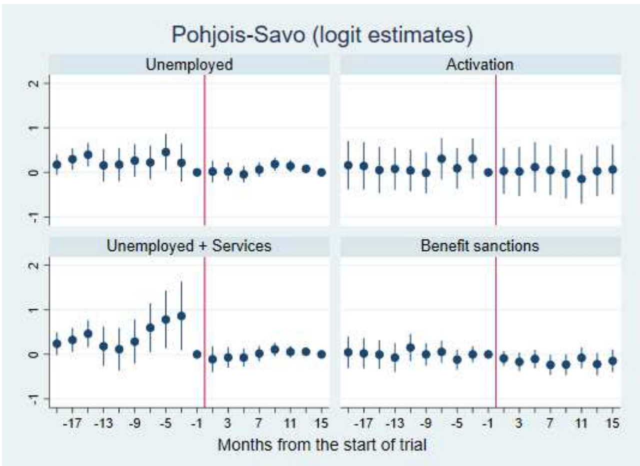
Notes. Figure plots regression coefficients from model (6.11). The coefficients are bimonthly treatment effects and the coefficient of period −1 (July 2017) is normalized to zero.

Logit results, Lappi



Notes. Figure plots regression coefficients from model (6.11). The coefficients are bimonthly treatment effects and the coefficient of period −1 (July 2017) is normalized to zero.

Logit results, Pohjois-Savo



Notes. Figure plots regression coefficients from model (6.11). The coefficients are bimonthly treatment effects and the coefficient of period −1 (July 2017) is normalized to zero.