DISPLACEMENT EFFECTS OF ACTIVE LABOUR MARKET POLICIES

Jyväskylä University
School of Business and Economics

Master's Thesis

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ABSTRACT

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Abstract

This thesis examines the displacement effects of active labour market policies (ALMPs). Displacement effects are indirect side effects of ALMPs, which affect employment negatively via affecting other persons than the program participants. Thesis focuses on two types of side effects. First, ALMPs like subsidised employment programs can crowd out regular employment. Second, according to earlier literature ALMPs can also decrease employment probabilities of non-participant jobseekers. Consideration of these kind of side effects is vital if the goal is to evaluate net effects of ALMPs. Displacement effects of ALMPs is examined using modified Layard-Nickell model as theoretical framework, surveying earlier literature regarding the topic and finally, with empirical analysis.

Empirical analysis is implemented using yearly panel dataset of Finnish municipalities over the period 2008–2018. Data are analysed using static fixed effects model and dynamic generalized method of moments (GMM) model with instrumental variables (IVs). Fixed effects estimates show that subsidised employment and training measures are negatively associated with regular employment. The relationship of employment and ALMPs is simultaneous i.e. employment level can affect ALMP volumes and vice versa. Therefore, observed associations from fixed effects model suffer from endogeneity bias and it cannot be concluded whether observed negative associations are result of displacement effects or barely result of the simultaneous nature of ALMPs and employment. Endogeneity problem is tackled with IV method (GMM), but the GMM estimates are not valid due to invalid instruments.

Findings of the empirical analysis is comparable to earlier macroeconomic literature, which suggests that displacement effects associated with subsidised employment are significant. Firm level studies contradict with this finding since they suggest that wage subsidies are not associated with displacement effects. Therefore, further research of the topic is still needed in the future because both macroeconomic studies and firm level studies have certain limitations which could be improved.

Key words
active labour market policies, displacement effects, externalities, employment

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Tiivistelmä
Tutkielma käsittelee aktiivisen työvoimapolitiikan (ATP) syrjäytysvaikutuksia. Syrjäytysvaikutukset ovat ATP:n epäsuoria sivuvaikutuksia, jotka vaikuttavat työllisyyteen negatiivisesti muiden kuin ohjelmiin osallistuvien henkilöiden kautta. Tutkielmassa keskitytään kahden tyyppisiin sivuvaikutuksiin. Ensinnäkin ATP, kuten tukityöllistämisohjelmat, voi syrjäyttää vakinaista työllisyyttä. Lisäksi aiemman kirjallisuuden perusteella ATP voi myös vähentää ohjelmiin osallistumattomien työnhakijoiden työllistymistodennäköisyyttä. Sivuvaikutusten huomioiminen on tärkeää, jos halutaan arvioida ATP:n nettovaikutuksia. Tutkielmassa syrjäytysvaikutuksia tarkastellaan hyödyntäen muunneltua Layard-Nickell mallia teoriakehyksenä, tarkastellen aihetta käsittelevää aiempaa kirjallisuutta sekä lopuksi empiirisen analyysin avulla.

Empiirisessä analyysissä käytetään vuositason paneeliaineistoa suomalaisista kunnista vuosilta 2008–2018. Aineistoa analysoidaan staattisella kiinteiden vaikutusten mallilla ja dynaamisella yleistetyn momenttimenetelmän (GMM) mallilla hyödyntäen instrumenttimuuttujia. Kiinteiden vaikutusten mallin perusteella tukityöllistäminen ja koulutusohjelmat ovat negatiivisesti yhteydessä vakinaiseen työllisyyteen. Työllisyden ja ATP:n volyymien yhteys on kuitenkin simultaaninen eli työllisyyden taso voi vaikuttaa ATP:n volyymeihin ja päinvastoin. Näin ollen kiinteiden vaikutusten mallin tuloksiin liittyy endogeenisuusharhaa. Tämän vuoksi ei voida tehdä johtopäätöksiä siitä, kuvastaako havaitut negatiiviset yhteydet pelkästään muuttujien endogeenista suhdetta vai syrjäytysvaikutuksia. Endogeenisuusongelmaa ratkaistaan hyödyntämällä instrumenttimuuttujamenetelmää (GMM), mutta GMM mallin estimaatit eivät ole valideja epävalidien instrumenttien vuoksi.

Empiirisen analyysin tulokset ovat verrattavissa aiempiin makrotason tutkimuksiin, joiden perusteella tukityöllistämiseen liittyvät syrjäytysvaikutukset ovat mittavia. Yritystason tutkimukset ovat kuitenkin ristiriidassa näiden tulosten kanssa, koska niiden perusteella palkkatukeen ei liity syrjäytysvaikutuksia. Tulevaisuudessa tarvitaan vielä jatkotutkimusta aiheesta, koska sekä yritystason että makrotason tutkimuksissa on tiettyjä rajoituksia, joita voitaisiin parantaa tulevissa tutkimuksissa.

Asiasanat
aktiivinen työvoimapolitiikka, syrjäytysvaikutukset, ulkoisvaikutukset, työllisyys

Säilytyspaikka
Jyväskylän yliopiston kirjasto

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ACRONYMS

ALMP – Active labour market policy
CIA – Conditional independence assumption
ELY centre – Centre for Economic Development, Transport and the Environment
ESS – Employment Service Statistics
FES – Full employment schedule
GMM – Generalized method of moments
IV – Instrumental variable(s)
OLS – Ordinary least squares
RES – Regular employment schedule
SUTVA – Stable unit treatment value assumption
TE Office – Employment and Economic Development Office
TEM – Ministry Economic Affairs and Employment
VAR – Vector autoregression
VM – Ministry of Finance
WS – Wage-setting schedule

1 INTRODUCTION

Employment policies have been very topical theme in Finnish public discussion since last two governments have had very specific goals about how high they aim to increase the employment rate. Especially during 2020 and 2021 the evaluation of employment effects of different policies has risen in the centre of the political discussion. Traditionally active labour market policies (ALMPs) have been viewed as a tool to increase employment. ALMPs are implemented through different kind of programmes, e.g. wage subsidies, direct job creation and labour market training. The difference between active and passive policies is that passive policies aim to increase well-being of the unemployed but do not seek to improve their labour market performance (Cahuc, Carcillo, & Zylberberg, 2014, 900). For example, unemployment insurance is classified as such policy.

Empirical literature regarding employment effects of ALMPs on the participants of the programs (i.e. gross effects of ALMPs) is extensive (see e.g. Card, Kluve, & Weber, 2010; Card, Kluve, & Weber, 2018; Vooren, Haelermans, Groot, & Maassen van den Brink, 2019). However, ALMPs have also indirect effects (externalities) that affect also employment of non-participants (Brown & Koettl, 2015). It is important that these negative side effects are considered as well when evaluating the net effect of ALMPs.

This thesis focuses on studying adverse side effects of the ALMPs, especially substitution and direct displacement effects. The aim of the thesis is to study on what extent Finnish ALMPs are associated with displacement effects. *Direct displacement* effect refers to a situation where job generation of the ALMPs comes with the expense of jobs at other parts of the economy (Calmfors, 1994). For example, wage subsidies may cause displacement effects if competitiveness of subsidized firms rise above the competitiveness of other firms within the same industry. Due to this competitive disadvantage non-subsidized firms might need to let go employees to. *Substitution* effect occurs when program participants replaces directly a regular worker (Calmfors, 1994).

Substitution and direct displacement effects are indirect effects that affect the regular workers that are already employed. In addition to these effects a significant source of negative side effect is that ALMPs can decrease employment probabilities of non-participant jobseekers as well (Crépon, Duflo, Gurgand, Rathelot, & Zamora, 2013; Ferracci, Jolivet, & van den Berg, Gerard, 2014; Gautier, Muller, van der Klaauw, Rosholm, & Svarer, 2018).

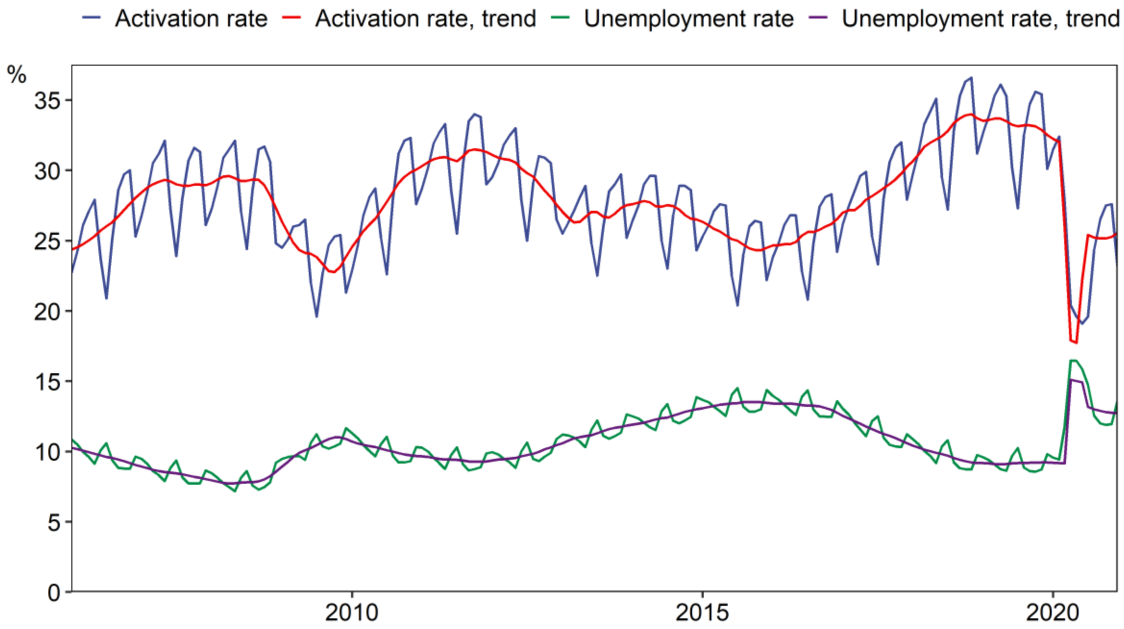
If these kind of indirect effects are not considered, evaluated employment effects are considerably larger compared to what the true net effect is. For example, according to Finnish Economic Policy Council's report (2021) the gross employment effect of Finnish activation model was roughly 5 000–12 000 persons. However, when displacement effects of the model is considered as well, the employment effect of the model is reduced to 2 000–4 900 persons. This means that the average of the estimated gross effect of the model is about 1.5 times larger than the estimated effect considering displacement effects. Another good example of the importance of displacement effects comes from Ministry of Finance's (VM) effectiveness evaluations of so called Nordic job search model. According to their estimations based on empirical literature, the gross employment effect of periodic interviews in the short run would be on average about 35 000 persons (VM, 2020). Again, when the displacement effects are considered as well the number reduces to about 12 000 persons. The gross effect is about 1.9 times larger than the employment effect where also displacement effects are considered.

Rest of the thesis is structured as follows. Section 2 describes recent developments in Finnish labour markets and how ALMPs are organised in Finland. Section 3 presents theoretical framework which can be used to analyse different effects of ALMPs. In the section 4 earlier literature regarding side effects of the ALMPs is surveyed. Section 5 describes methodology and the data for the empirical analysis. Section 6 presents the results of the empirical analysis. Finally, section 7 concludes the thesis.

2 INSTITUTIONAL BACKGROUND

2.1 Finnish labour market

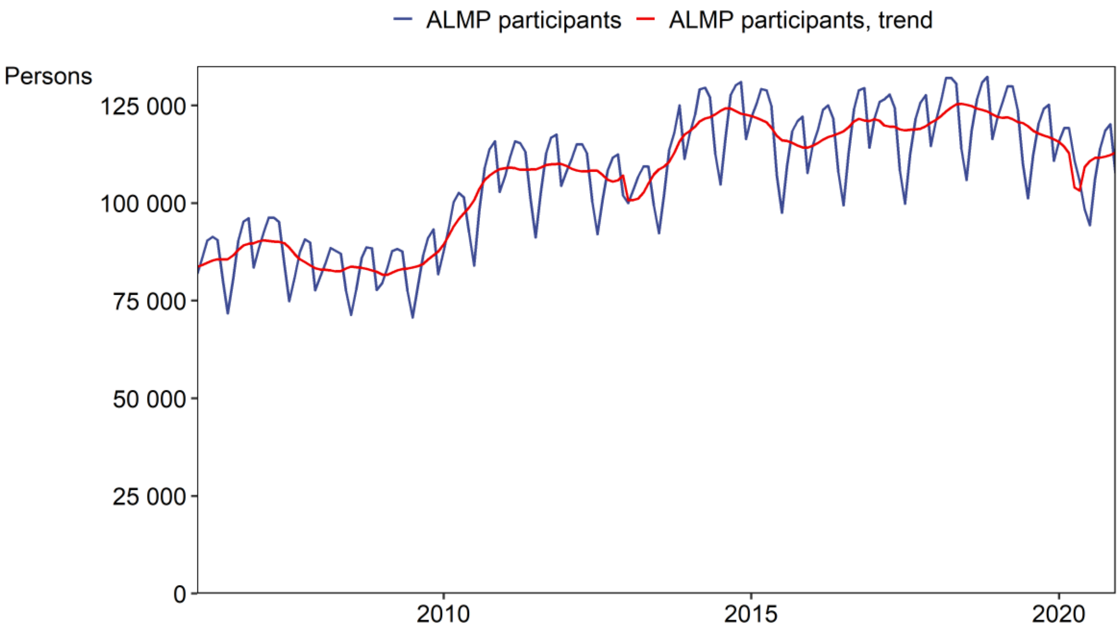
Figure 1 shows unemployment rate (ratio of unemployed to labour force) and activation rate (ratio of ALMP participants to sum of unemployed and ALMP participants i.e. total unemployment). Both measures have varied cyclically during the 2000's. Unemployment rate started rising in 2009 after the financial crisis, which was followed by stagnation period in 2010–2012. After 2012 the unemployment rate started rising again until 2015. Figure 1 shows that the activation rate has been decreasing when the unemployment rate has risen rapidly like in 2008–2009 and 2013–2015. It also illustrates the impact of corona pandemic on Finnish labour markets: unemployment (including layoffs) increased rapidly whereas activation rate plummeted in the beginning of 2020.



Source: Statistics Finland, Employment Service Statistics.

Figure 1. Activation and unemployment rates

In Finland, Ministry of Economic Affairs and Employment (TEM) produces Employment Service Statistics (ESS) concerning ALMP participation, for example regarding of number of ALMP participants in different programs. Statistics are gathered from customer data systems of Employment and Economic Development Offices (TE Offices) (TEM, 2020a). Statistics Finland publishes these statistics. Figure 2 shows development of the total number of ALMP participants. Between 2006 and 2020 there has been growing trend in the total number of ALMP participants. Especially after 2009 and after 2013 there were large expansions in the volume of ALMPs. Rapid expansion after 2009 seems to have been successful since the rise of the unemployment rate halted and activation rate started rising (figure 1). There was not same kind of phenomenon after 2013. Even though the number of ALMP participants was increased by over 15 thousand persons the rise of unemployment rate continued (figures 1 and 2).



Source: Statistics Finland, Employment Service Statistics.

Figure 2. Total number of ALMP participants

Figure 3 shows spending on active and passive labour market policy measures in Finland as a percentage of GDP. Overall, the spending share of the active measures has been stable. It increased rapidly in the beginning of 1990 when recession hit Finnish economy, but in the 2000s the spending share of active measures have not reacted as strongly. Spending on passive measures has more variation and economic cycles seem to affect it more than active measures. This highlights the problem of simultaneous causality when studying effect of labour market policies on employment or unemployment. It seems that spending on active and especially on passive measures increases when unemployment increases i.e. unemployment level affects the volume of labour market policy measures. Therefore, there needs to be proper identification strategy to identify the true effect of labour market policies on employment.

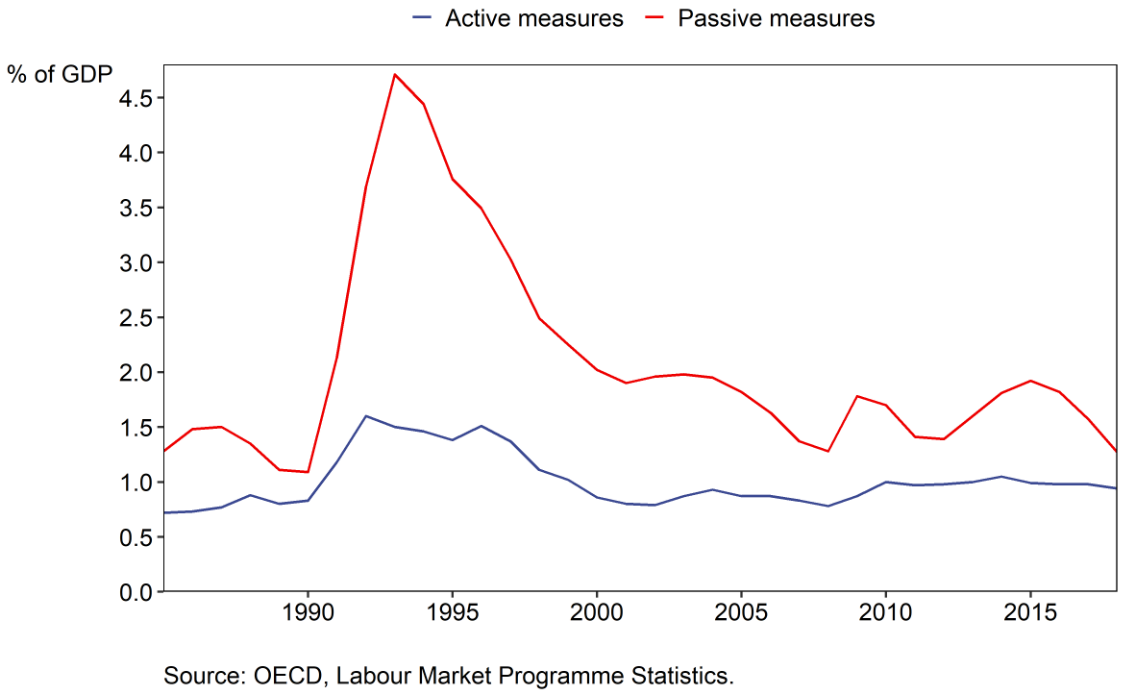


Figure 3. Spending on active and passive labour market policies

2.2 Active labour market policies in Finland

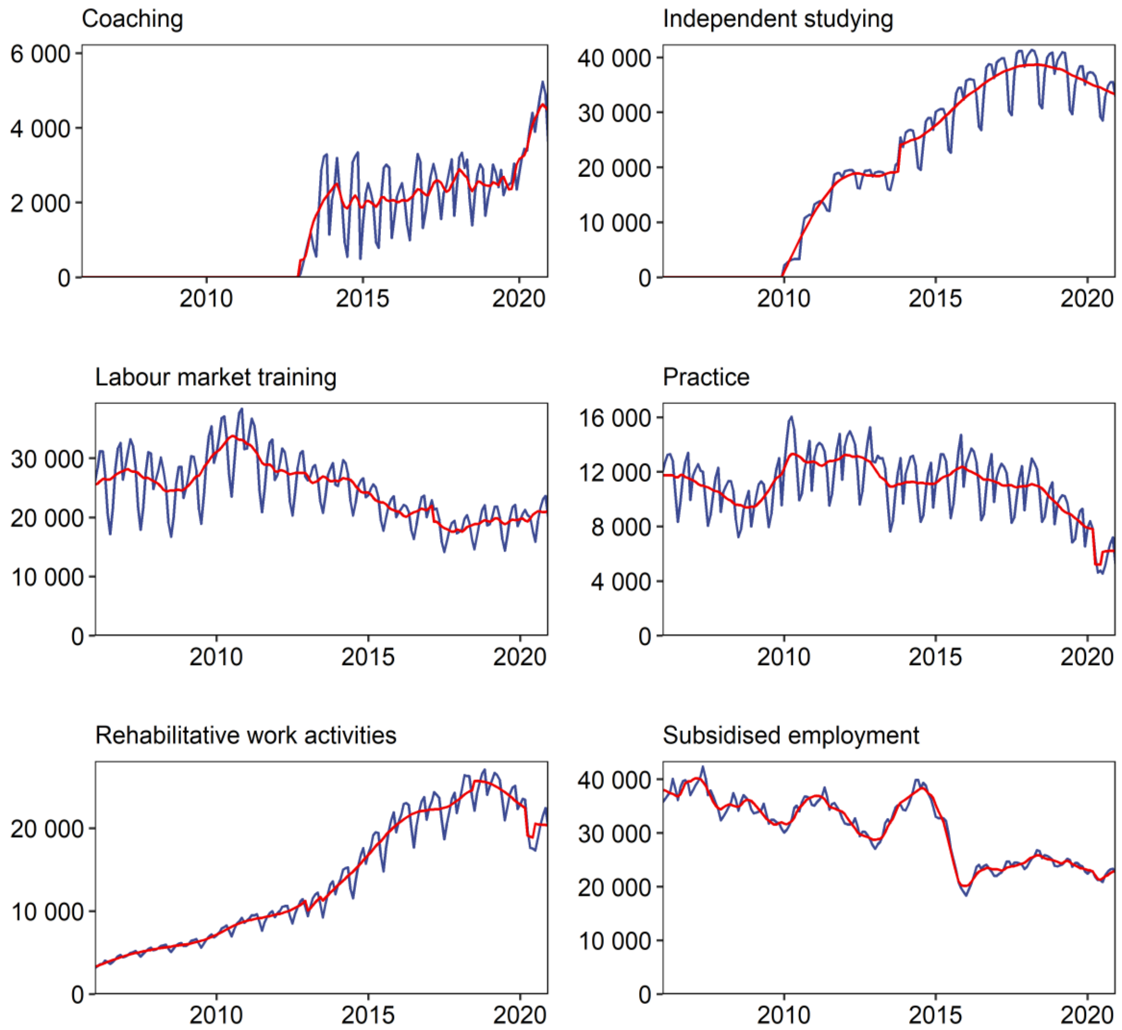
In Finland ALMPs are mostly organised by TE Offices which are coordinated by ELY centres (Centre for Economic Development, Transport and the Environment). ELY centres implement the central government's policies regarding e.g. employment, immigration and transportation. TEM oversees ALMPs by controlling ELY centres. At the moment there are 15 ELY centres.

Finnish employment services are centralized since it is central government's responsibility to arrange them. Employment services can be also decentralised (like in Denmark) if the local government such as municipality is in charge of arranging the services. Decentralization of the employment services has been in Finnish public discussion, mainly due to municipality experiments where municipalities had more responsibility at the organisation of employment services (Annala et al., 2019; Arnkil, Spangar, Jokinen, Tuusa, & Pitkänen, 2015).

TE Offices execute ALMPs at the operative level. In 2013 TE Offices were attached to regional government under ELY centres in an organisational reform. Organisational reform reduced the number of TE offices significantly (from 80 to 15).

According to Public Employment Services Act (Laki julkisesta työvoima- ja yrityspalvelusta 2012/916) central government uses public employment services to assure economic growth, high level of employment and well-being by promoting functioning of labour market and labour supply. Tasks of the TE offices are to provide job brokering services, information and advising services, different training programs, admit employment subsidies, to subsidise self-motivated studies of the unemployed and admit entrepreneurship start-up grants. TE Offices can produce the services by themselves or they can purchase services from different service providers. (Laki julkisesta työvoima- ja yrityspalvelusta 2012/916).

Overall, services provided by TE Offices can be categorised to two broad ALMP categories: subsidised employment and training measures. These services include for example wage subsidies, labour market training and rehabilitative work activities. Figure 4 shows how volumes of different services have developed. Next sections will describe ALMP programs more closely.



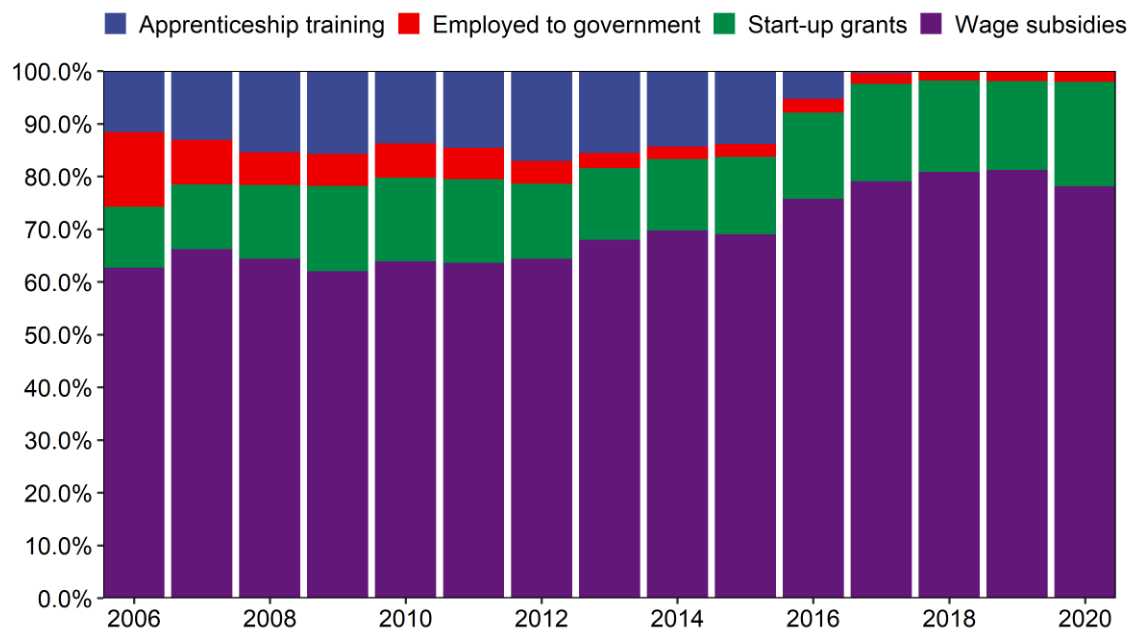
Notes: Blue lines are unadjusted values and red lines are trend values.
Practice includes work practice (2006-2014) and work trials (2013-2020).
Source: Statistics Finland, Employment Service Statistics.

Figure 4. Volumes of ALMP programs

2.2.1 Subsidised employment

Subsidised employment can be divided to two categories: direct subsidised employment programs (placements) and practice programs. Main difference between these two categories is that in practice programs there is not established employment relationship. Therefore, practice program participants do not receive wage for the practice.

Direct subsidised employment includes wage subsidies, employment at government agencies and institutes, start-up grants and apprenticeship training. Figure 5 shows average yearly participant shares of different direct subsidised employment programs. Wage subsidies have been the most important form of placements by far since its share has been around 60–80% of total number of direct subsidised employment during 2006–2020. Second largest program has been start-up grants, its share has been around 10–20%. Until 2015 job apprenticeship training was also major program, but after 2016 practically only few participants have been part of the program. The share of start-up grants has been steadily around 10–20% for last fifteen years. Employment at government agencies and institutes have not been notable program and its importance has decreased even more: its share has decreased from 5% to around 2%.



Source: Statistics Finland, Employment Service Statistics.

Figure 5. Participant shares of direct subsidised employment programs

During 2006–2014 the total number of direct subsidised employment was at high level: the number of participants was between 30 to 40 thousand (figure 4). During 2015 the number of participants started to fall drastically. This fall was due to decrease in offered wage subsidies and job apprenticeship training.

Wage subsidy is a direct subsidy granted to an employer to hire an unemployed. It can be granted either for apprenticeship training or for work performed on an employment contract. Eligibility for wage subsidy is assessed by TE Offices on a case-specific basis. (TEM, 2020b.) The length and size of wage subsidy varies depending on the employer and the length of the unemployment. The wage subsidy can be 30–50% of the salary costs and the subsidy can be granted for 6–24 months (TE palvelut, 2020a).

Start-up grants are granted for persons leaving paid work, studies or domestic work to become full-time entrepreneurs. This grant can be issued for both unemployed and non-unemployed persons (TEM, 2020b). Start-up grant can be granted if TE Office has investigated that entrepreneurship is viable option for the applicant. It can be granted for maximum of 12 months and the size of the grant is 33.66€/day for maximum of five days a week (TE palvelut, 2020b).

Regarding practice measures there have been two programs. Both programs have had similar goals: to familiarize participant with working life, to improve participants' skills and to provide possibility to experiment new professions. Until 2013 practice program was called work practice. Work practice participants could not receive wage from the practice, but they received labour market support or employment assistance subsidy if the participant were eligible for earnings related unemployment allowance from Kela. The long-term unemployed were also eligible to additional maintenance allowance. The maximum duration of work practice was 18 months. In the beginning of 2013 work trial program started to replace work practice. Work trial participants receive similar benefits as work practice participants. Maximum duration of work trial is 12 months, but it can be significantly shorter, even just one month.

Before approving the work trial TE Offices assess whether work trial could provide unfair advantage which could affect competition between firms. Work trial cannot be organised if TE Office evaluates that it distorts competition between firms. Same convention was used regarding work practice.

Number of participants in practice programs increased rapidly after financial crisis hit the Finnish economy (figure 4). During 2010–2013 the number of participants was around 13 000. After 2015 the significance of practice programs has been decreasing steadily. Before the corona pandemic hit Finland the number of practice program participants was around 8 thousand.

2.2.2 Training measures

Training measures include four type of programs: labour market training, coaching, independent studying, and rehabilitative work activities. Traditionally main form of training measures provided by TE Offices has been labour market training. The aim of labour market training is to complete a vocational qualification or special vocational qualification or part of it. Unemployed participating in labour market training receives same unemployment subsidies as other unemployed (TE palvelut, 2020c). Figure 4 shows that the number of participants in labour market training has been decreasing after 2011 whereas other training measures have become more popular.

TE Office provides also shorter trainings like job seeking training, career coaching and job coaching. Job seeking training and job coaching provide tools for the unemployed about practices on job seeking and how to succeed in job interviews. Career training aims to provide a new career or education path for the unemployed unsure of their career or education choices. Coaching programs started in 2013 and it has become more popular since, especially during 2020 probably as a result of corona pandemic. (TE palvelut, 2020d.)

It is also possible for the unemployed to enhance their skills by independent studying. Program participants receive labour market support from Kela. TE Offices do not provide the educational services for the participants but they evaluate whether the education improves applicant's professional skills and possibilities at the labour markets before approving the eligibility to receive labour market support while studying. Before the start of independent studying the applicant must also make employment plan with the TE Office. Independent studying program started in 2010 and its popularity has risen rapidly. During 2015–2020 number of participants in independent studying program has been around 30–40 thousand (figure 4).

Rehabilitative work activities are a program that is aligned to long term employed. The goal of the program is to enhance life management skills and improve overall functional capability of the unemployed. Therefore, rehabilitative work activities can be viewed as a social policy as well. The program is usually organised at municipalities or within different associations for example in the form of workshops. The number of participants in the program has been raising steadily from 2006 onwards and the number of participants has remained over 20 thousand after 2016 (figure 4).

2.3 Finnish labour market policies in an international perspective

Figure 6 shows the spending on ALMP as percentage of GDP for Nordic countries (excluding Iceland) and Germany for period of 1985–2018. In addition, it shows average of all OECD countries for the period of 2004–2018. Overall, there can be seen similar patterns between countries, but the magnitudes of the changes vary. The spending share increased significantly for all countries in the beginning of 1990s, when the downturn hit the global economy. Similar pattern can be seen after the financial crisis but at that time the response was not as homogenous as previously. This time the spending shares of Sweden and Denmark increased significantly more than spending shares of other countries. In addition, Norway, Germany, and the average for all OECD countries returned quickly to their previous spending levels or sank even below those levels.

Compared to other Nordic countries, Finland does not seem to use ALMPs as actively as Denmark or Sweden. Lately, the spending shares of Sweden and Finland have converged, but traditionally Sweden has been spending more on ALMPs. However, the spending share of Finland is significantly higher when compared to OECD average and Norway.

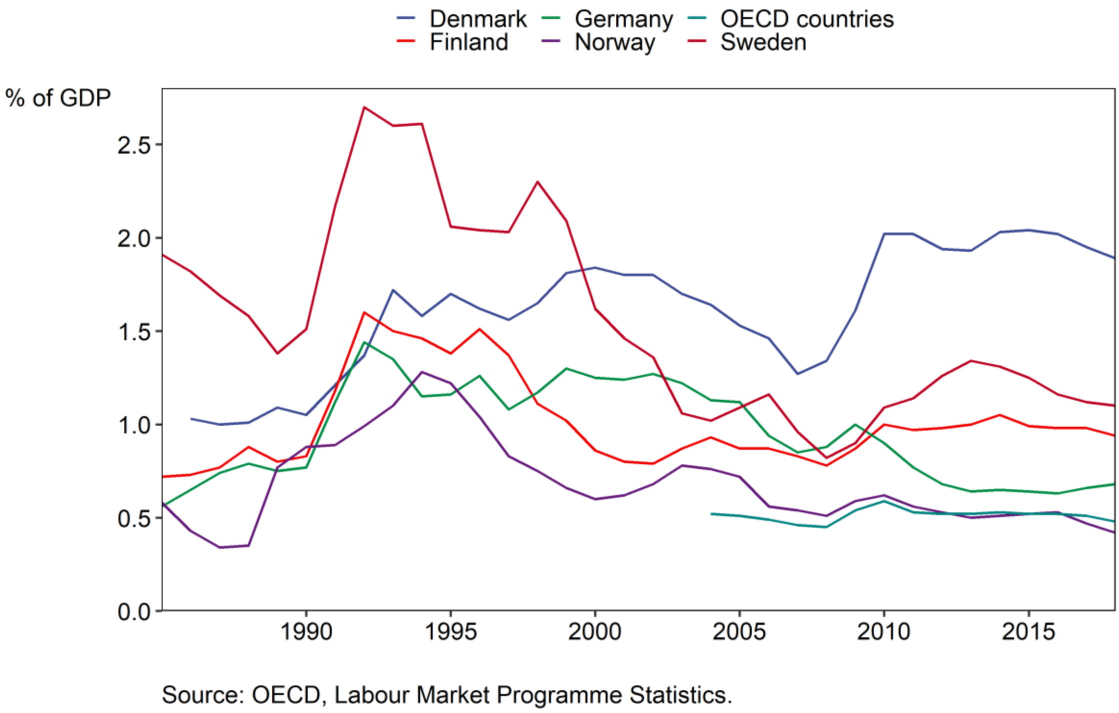


Figure 6. International comparison of spending on ALMPs

Figure 7 shows spending shares of passive labour market policies for same countries as figure 4. Development patterns of spending shares on passive measures are more homogenous than in the case of active measures. Magnitudes vary again between countries, but changes are more parallel. Overall, countries usually spend more on passive measures than active measures, except Norway. Finnish spending share on passive measures has been close to the top for the whole time period and in 2018 Finland's spending share was the highest. This may reflect different kind of policies compared to other countries, but it also reflects that Finnish economy did not recover from the downturn as well as other countries during 2010s.

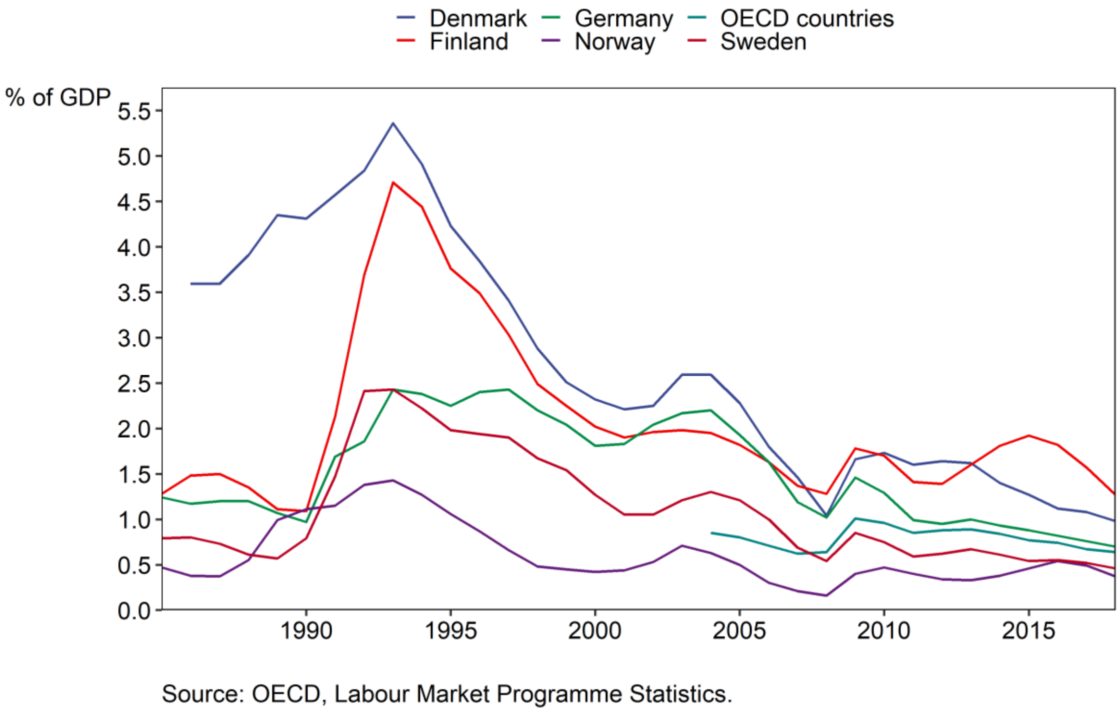


Figure 7. International comparison of spending on passive LMPs

3 THEORETICAL FRAMEWORK

3.1 Layard-Nickell model

Layard-Nickell model (Layard & Nickell, 1986) is one of the most used theoretical frameworks for macroeconomic labour market analysis. With slight modifications by Calmfors (1994) the same model can be used to analyse effects of ALMPs on (un)employment and real wages. Even though the models have seen time they are still topical. Similar theoretical framework could be laid out also using matching model (Cahuc & Le Barbanchon, 2010; Cahuc et al., 2014; Pissarides, 2000), but the main implications are same as from Calmfors' (1994) framework.

The basis of basic Layard-Nickell model is illustrated in figure 8. Horizontal axis shows employment as a proportion of the labour force and vertical axis shows real wage level. The downward sloping employment schedule represents labour demand and shows how employment is negatively related to real wage. The relationship shows that if real wage rises the employment decreases. The upward sloping wage-setting schedule (WS) shows that employment is positively related to higher real wages. Higher employment creates pressure for higher wages since bargaining power of the employees increases. Equilibrium employment and real wage are achieved at the intersection of these curves (point A). The vertical line represents the full employment of the labour force (Full employment schedule, FES). The horizontal distance between equilibrium employment and full employment is the amount of involuntary unemployment. (Calmfors, 1994.)

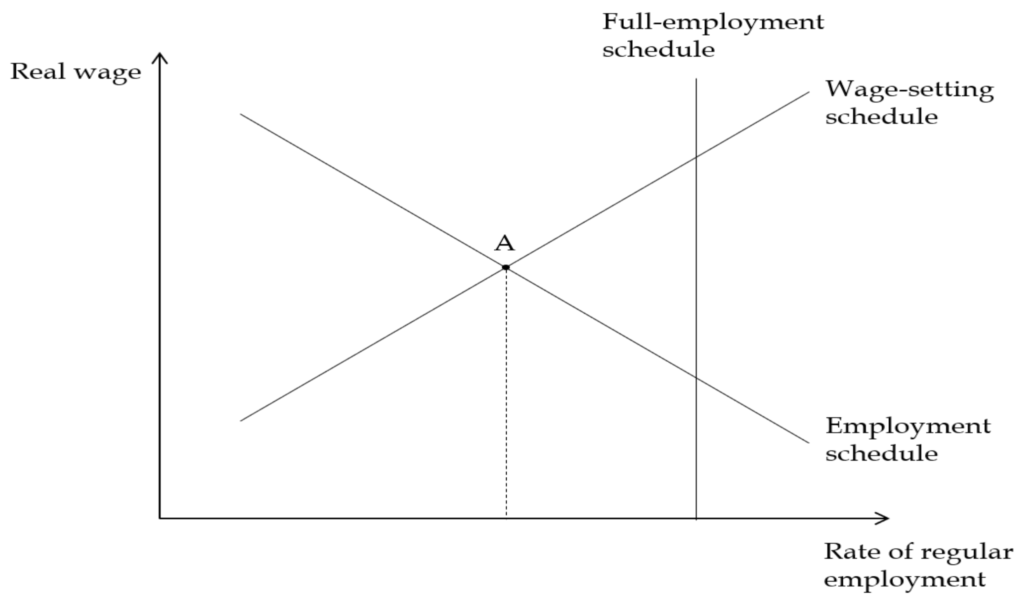


Figure 8. The Layard-Nickell model

In addition to Layard-Nickell model, the Beveridge curve can be used to analyse functioning of the labour markets. The Beveridge curve is shown in figure 9 and it illustrates negative relationship between unemployment rate and vacancy rate (the ratio of the number of vacant jobs to the labour force). It can be interpreted as measure of effectiveness of the matching process between the unemployed and vacancies. (Calmfors, 1994.) More efficient matching process provides more matches between job seekers and vacant job positions, given the number of vacancies and job seekers.

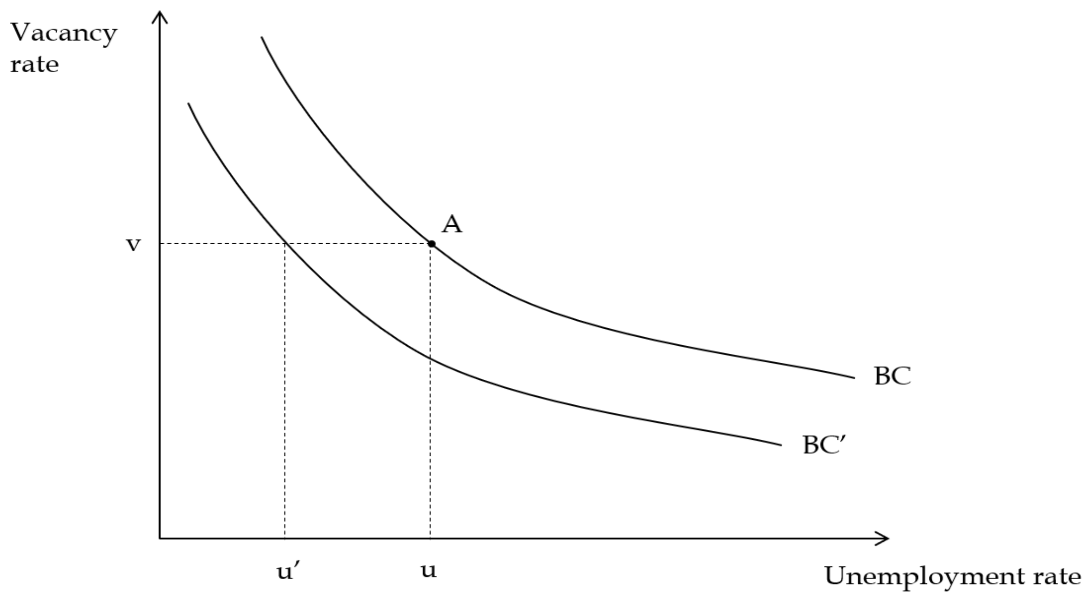
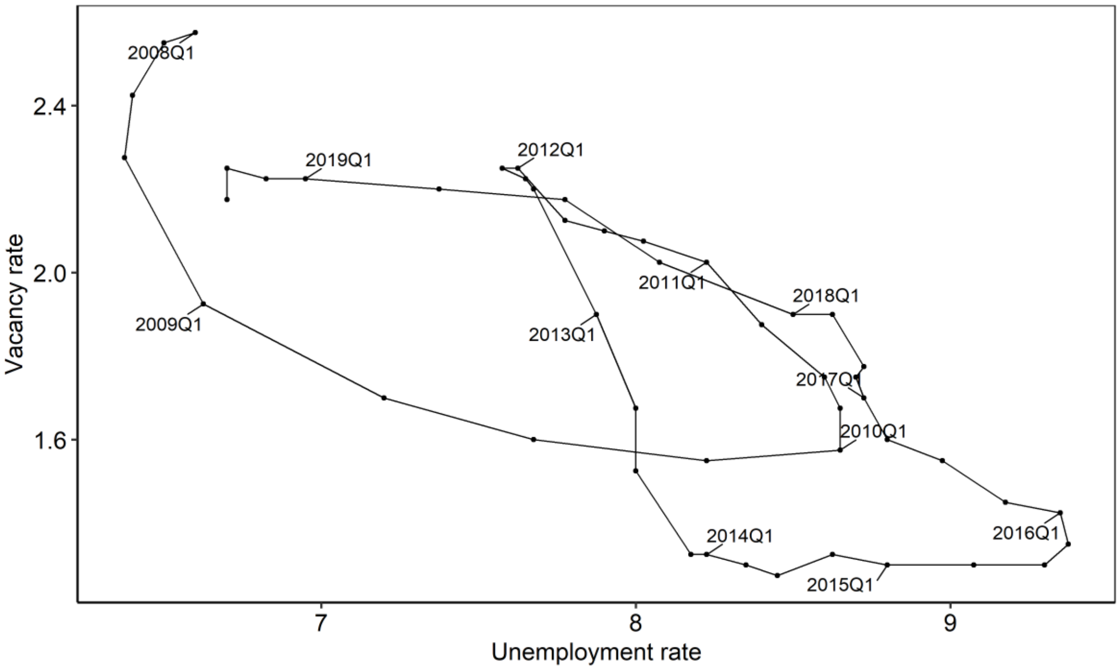


Figure 9. The Beveridge curve

The position of the Beveridge indicates the effectiveness of the matching process (Cahuc et al., 2014, 575). When the curve is closer to origin the process is more effective. In figure 9 curve BC′ reflects more effective matching process than curve BC since the unemployment rate is lower with the same vacancy rate. The movement along the curve reflects development of unemployment and vacancy rate when the effectiveness of the matching process remains constant.

Figure 10 shows Beveridge curve for Finland in 2008–2019. Figure shows that there have been several shifts in Beveridge curve between 2008 and 2019. After 2009 the effectiveness of matching process in Finnish labour market decreased since the curve shifted outwards from origin. Over the period 2010-2012Q2 there was movement along the curve: unemployment rate decreased, and the vacancy rate increased. In the beginning of 2012, the vacancy rate was at the same level as in the end of 2008, but the unemployment rate was significantly higher, which highlights that the curve had shifted outwards. After the first half of 2012 the curve shifted slightly towards the origin, but the unemployment level remained at a higher level than in 2008. During 2013 there was movement along the curve when vacancy rate decreased, and unemployment increased. After 2015 the Beveridge curve shifted outwards again. Overall, the efficiency of the matching process decreased during 2010s.



Source: Eurostat, Employment and Unemployment Statistics and Job Vacancy Statistics.

Figure 10. Beveridge curve in Finland

3.2 Modified Layard-Nickell model

The main modification from basic Layard-Nickell model is that participation in labour market programmes and regular employment need to be distinguished. Regular employment means employment excluding participation in programs and it is measured as a proportion of the labour force. This slightly modified version of the Layard-Nickell model is illustrated in figure 11. Now employment rate is replaced by regular employment rate on the horizontal axis. Vertical curve RR shows the proportion of the labour force that is not participating programs. Distance r between RR and full-employment schedule is the amount of participation in programs. Equilibrium levels of regular employment rate and real wage is determined by intersection of wage-setting and regular employment schedules (WS and RES). Distance u between the equilibrium and line RR is open unemployment. (Calmfors, 1994.)

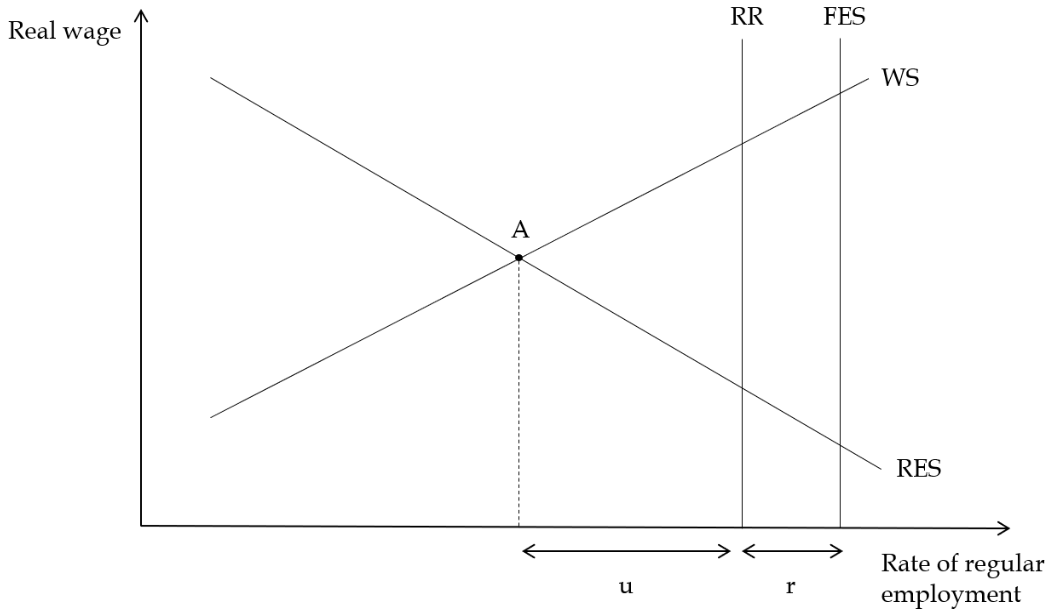


Figure 11. Modified Layard-Nickell model

The Beveridge curve needs to be modified similarly. Modified Beveridge curve is shown in figure 12. Now the number of job searchers without a regular job (sum of openly unemployed and program participants) is on the horizontal axis and vertical axis indicates the vacancy rate for regular jobs. There are two assumptions behind the illustration. First, vacant regular jobs can be filled either from the stock of openly unemployed or from the stock of program participants. Second, the matching process with respect to labour market programmes is simpler than in the regular job market. (Calmfors, 1994.)

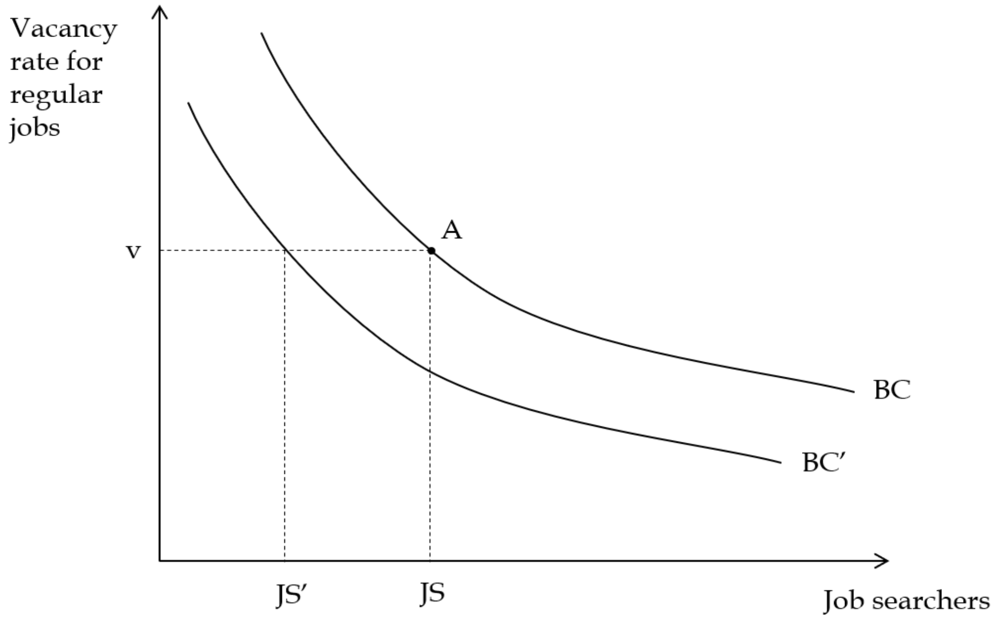


Figure 12. Modified Beveridge curve

This presented framework can be used to illustrate different effects of ALMPs. For example, an increase in the volume of participants in labour training or subsidised work programmes would lead to RR line shifting left shown in figure 13. The programme participation would increase by Δr and therefore the open unemployment would decrease the same amount as well (from u_0 to u_1), ceteris paribus. This is the gross effect of the expansion in programme volumes. However, ALMPs also affect incentives for wage setting, regular labour demand and labour supply. Indirect effects affect persons who are not participating in the programmes and these effects need to be accounted for to obtain the net effect of ALMP expansion. Next, different effects of ALMPs will be analysed. (Calmfors, 1994.)

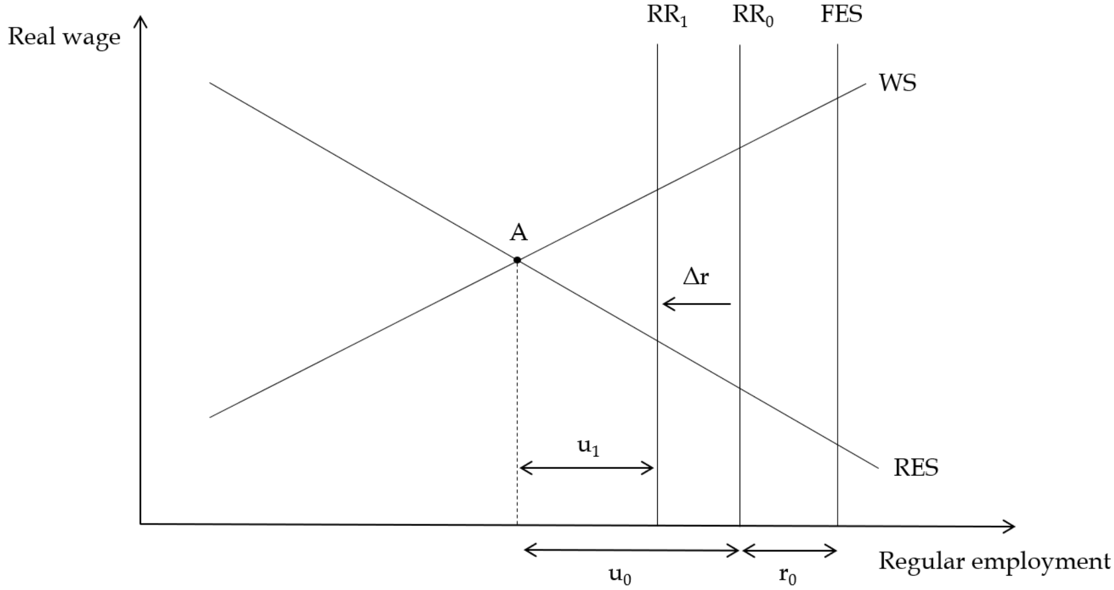


Figure 13. Effect of increase in program volumes

3.2.1 Effects on the matching process

ALMPs that can affect matching process are for example job-broking and job counselling. These activities aim to improve the efficiency of the matching process by three mechanisms. First, they could mould qualifications of job searchers to be more suitable for the labour market and that way reduce or eliminate mismatch between different submarkets for labour. Second, ALMPs can increase job searching activity of the unemployed. Third, ALMPs (for example rehabilitative work activity) can be a substitute for regular work experience and therefore reduce employer’s uncertainty of employability of an unemployed. (Calmfors, 1994.)

Improvement of the matching efficiency means that there are less job seekers with a given number of open jobs. It can be illustrated as a shift of the Beveridge curve towards origin like in figure 12. Improved matching process means that it is less costly to hire new employees since vacant jobs are filled quicker. Therefore, firms open more positions which leads to increase of labour demand i.e. rightward shift of the employment schedule (figure 14). In addition, improved matching efficiency weakens employers’ incentive to offer high wages to attract employees. This leads to wage-setting schedule shifting downwards and change of the equilibrium from A to B. Shifts of both schedules increase employment (from n to n’), but the effects on wages are opposite. The net effect on wages is unclear, it depends on magnitude of both effects. In this case the real wages decrease from w to w’ since wage-setting schedule shifts downwards more than regular employment schedule upwards. (Calmfors, 1994.)

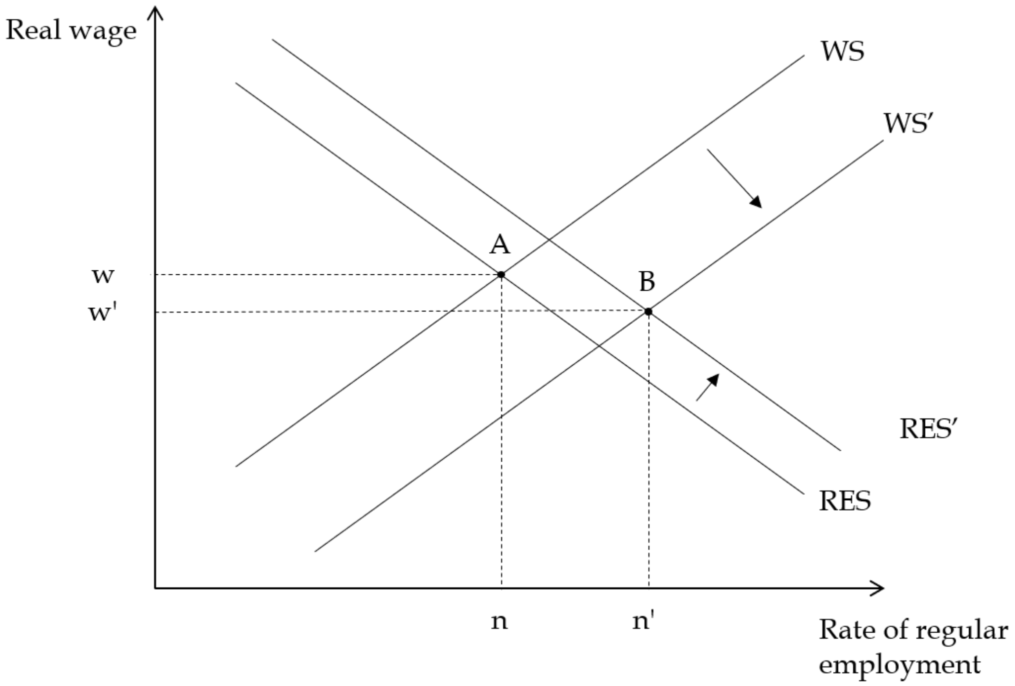


Figure 14. Effect of improved matching process

There are also ALMPs that can decrease the effectiveness of the matching process due to locking-in effects (Jespersen, Munch, & Skipper, 2008; Sianesi, 2008; Van Ours, 2004). Such programs are for example job training and job creation schemes. During or even before starting in the program participant's job search activity can decrease due to the participation (Jespersen et al., 2008; Van Ours, 2004). This locking-in effect affects employment level negatively the same way as improved matching efficiency affected positively. To calculate net effect of ALMPs on employment this negative adverse effect needs to be accounted as well.

3.2.2 Effects on the labour force and competition

High unemployment and poor economic situation may discourage job seekers so that they leave the labour force (Benati, 2001; Johansson, 2002). This is a significant risk especially when the unemployed are close to pension age or when the unemployment has prolonged for some time (Calmfors, 1994; Fuchs & Weber, 2017). Risk of older unemployed leaving the labour force is especially high in Finland since the unemployed close to the pension age can receive unemployment benefits for longer duration than normally (Kyyrä & Ollikainen, 2008). It seems that ALMPs are viable tool to mitigate negative effects on labour supply and have positive effect on labour force participation (Johansson, 2002).

If ALMPs create positive shock on labour supply, the competition for jobs is increased. Increased competition leads to lower real wages and higher number of regular employed since firms are incentivised to create more jobs with lower wages. However, the increase in the number of newly employed is smaller than the increase of the labour force. Therefore, the equilibrium in figure 15 shifts from

A to B. This leads to larger proportion of the labour force being openly unemployed or participating in programmes and equivalently smaller proportion of the labour force are regularly employed. Even though the regular employment rate is lower, the new situation is a positive development at the labour markets since more jobs has been created and larger share of the population is employed. (Calmfors, 1994.)

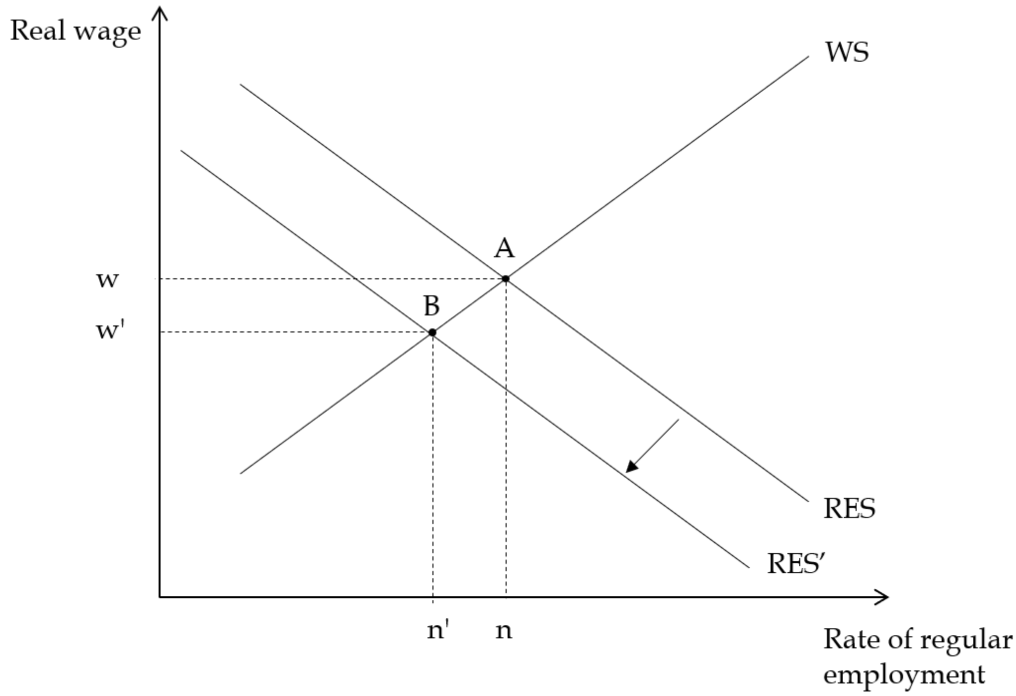


Figure 15. Effect of increase in labour supply

However, if the ALMPs are targeted at labour market outsiders (long-term unemployed, young persons, immigrants, etc.) there might be larger wage reducing competition effects. There will be similar positive effect on labour force but on top of that participation in the programmes may increase competitiveness of outsiders relative to insiders (Calmfors & Lang, 1995). Increased competitiveness leads to lower wages and higher regular employment as illustrated in figure 16 by the downward shift of wage-setting schedule. (Calmfors, 1994.)

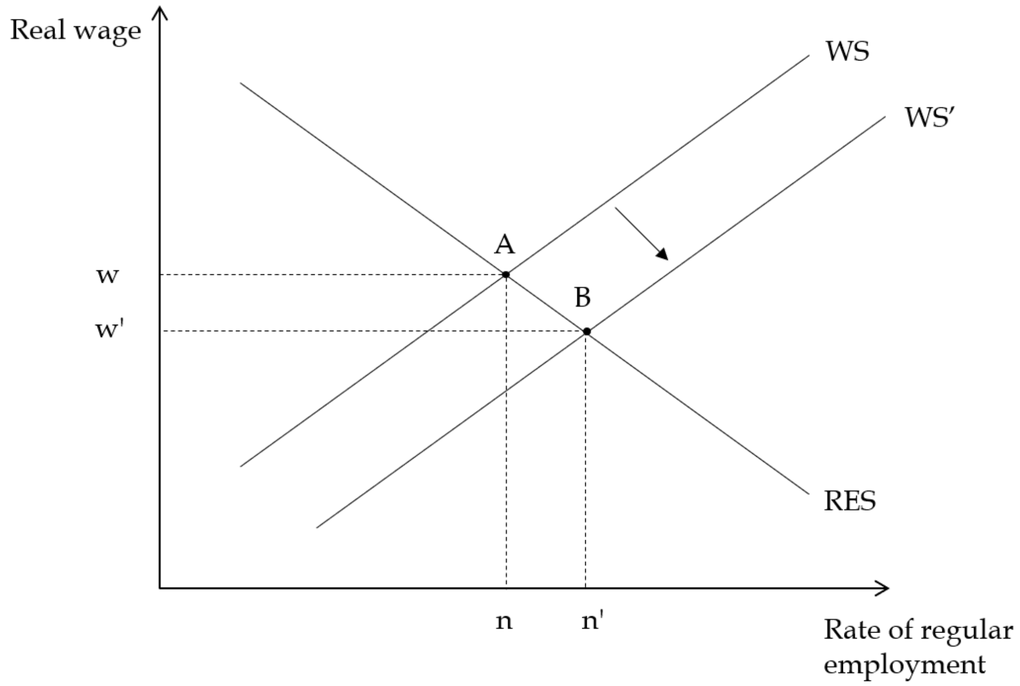


Figure 16. Effect of increased competitiveness

3.2.3 Effects on productivity

Unemployment may decrease the productivity of the unemployed via reducing the human capital of the unemployed (Edin & Gustavsson, 2008). One objective of the ALMPs is to counteract this possible phenomenon. Especially different labour market training or education programs aim to raise or maintain the productivity of the unemployed, but job creation schemes may have similar effect as well since it accumulates individual's job experience and knowledge. (Calmfors, 1994.)

Increased productivity of the unemployed shifts the employment schedule rightwards since their marginal productivity is increased (figure 17) (Calmfors, 1994). If everything else would stay constant, this would lead to higher employment. However, increased productivity might lead to increase in reservation wages as well. Therefore wage-setting schedule could shift leftwards as well, which would mitigate positive employment effects of productivity. The magnitude of the combined net effect depends on how much the wage-setting schedule shifts. If it shifts as much as employment schedule the combined net effect on regular employment is 0. (Calmfors, Forslund, & Hemström, 2002.) Figure 17 illustrates shifts of both schedules, and in this case the shift of the wage-setting schedule is not as large and therefore there is positive employment effect (n′′ > n).

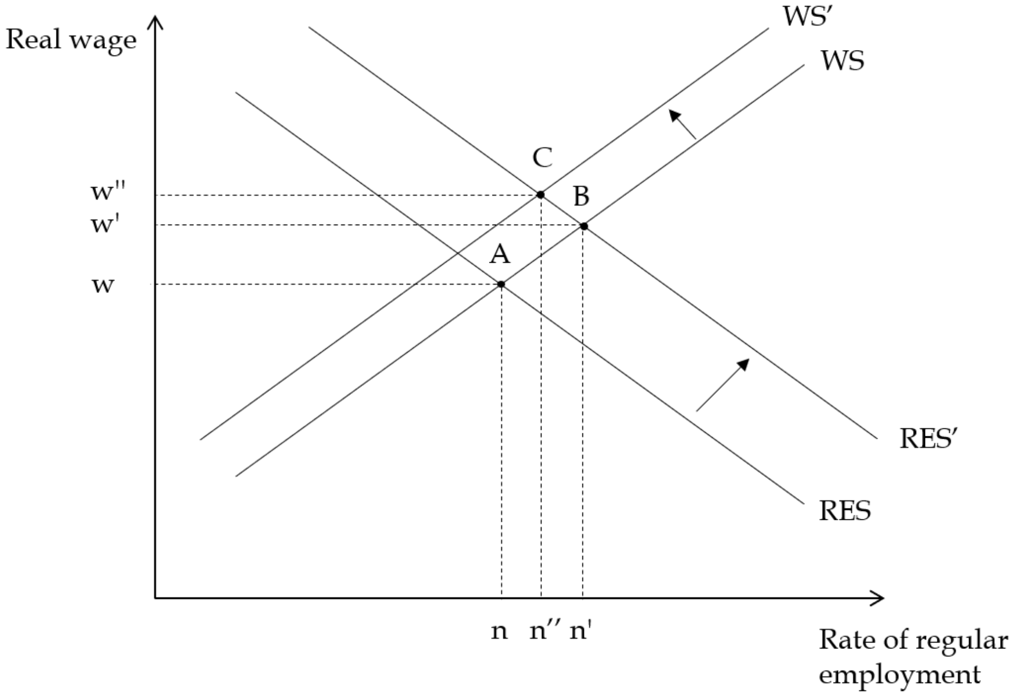


Figure 17. Productivity effects of ALMPs

3.2.4 Crowding out (displacement) effects

One significant negative effect ALMPs may have on employment is displacement effect (Dahlberg & Forslund, 2005). Displacement effect occurs if ALMP program causes program participants to crowd out regular employment (Calmfors, 1994). This kind of effect is especially related to subsidised employment. Employment subsidies create jobs, but those jobs may come with a cost of reduced regular employment elsewhere in the economy. If labour costs are subsidised for only proportion of firms, will those firms gain competitive advantage on other firms within the same industry. This leads to reduced labour demand in firms that do not receive subsidies. (Heinonen, Hämäläinen, Räisänen, Sihto, & Tuomala, 2004, 146.) This kind of direct displacement effect shifts employment schedule to the left and therefore, reduces both real wage and regular employment (figure 18) (Calmfors et al., 2002).

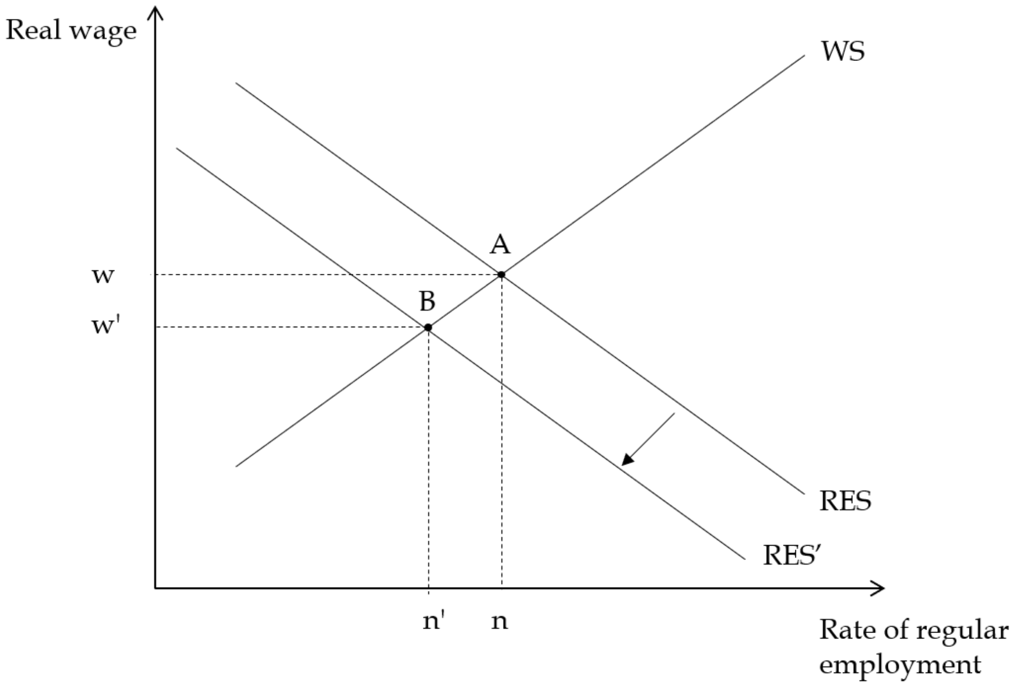


Figure 18. Direct displacement effect

In addition to direct displacement effects there might be indirect displacement effects as well. ALMPs often reduce welfare loss from being unemployed. Welfare loss could be reduced since participation in ALMPs may increase income compared to normal unemployment benefits or participation might improve future labour prospects. Reduced welfare loss from being unemployed increases wage pressure since bargaining position for trade unions and individuals improve. (Calmfors et al., 2002.) The state of being unemployed is not as undesirable since the welfare loss is not as large and therefore individuals are more willing to take the risk of becoming unemployed by negotiating higher wages. Indirect crowding out effect can be illustrated as an upward shift of wage-setting schedule which reduces employment and increases wages (figure 19) (Calmfors, 1994).

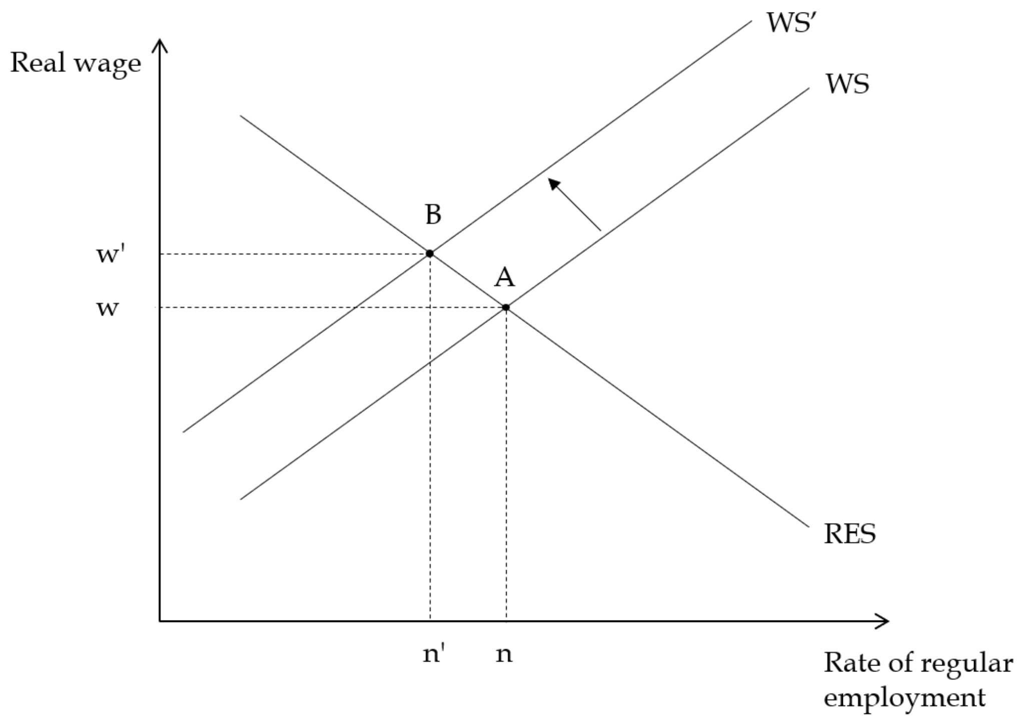


Figure 19. Indirect displacement effect

3.2.5 Substitution and deadweight effects

In addition to displacement effects, deadweight losses and substitution effects are also negative effects that ALMPs may cause. These effects are mainly related to subsidised work or job creation schemes. Deadweight loss in this case means that the programme participants would have been employed even without the program (Calmfors, 1994). Substitution effect means that the programme participants replace workers or applicants because of the subsidies (Calmfors, 1994; Heinonen et al., 2004, 145). Both deadweight loss and substitution effect can be mitigated by targeting outsiders of the labour market since they usually do not compete for the same jobs as insiders (Heinonen et al., 2004, 145-146).

The deadweight and substitution effects shift employment schedule leftwards in the case of employment subsidies (figure 20). This leads to reduction in regular employment and lower wages when the equilibrium moves from A to B. Usually these kinds of effects are related to subsidised work in private sector, but similar effects may arise regarding public-sector job creation schemes. The risk for similar effect is especially present when public-sector job creation schemes are financed by the central government, but the municipalities organise them. Then municipalities may restrain creation of ordinary jobs and exploit the subsidies from the central government. This is called fiscal displacement. (Calmfors, 1994.)

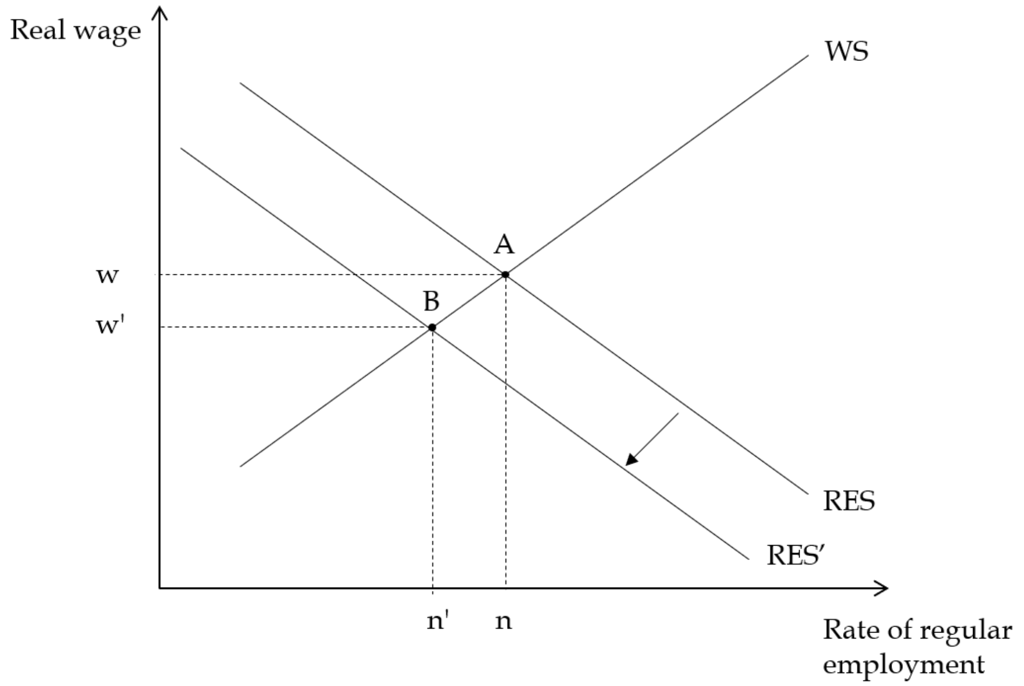


Figure 20. Deadweight and substitution effects

3.2.6 Net effect

Analysis above shows that ALMPs may have various effects on (un)employment. Effects may have positive or negative impact on employment, or the sign of the effect might be unclear. Table 1 summarizes expected signs of the effects. The net impact of ALMPs is the result of impact of all these effects or some of these effects.

Table 1. Expected effects of ALMPs

Effect	Real wage	Regular employment as a proportion of labour force	Regular employment as proportion of population	Labour force
Matching	?	+ (?)	+ (?)	0
Labour force	-	- (0)	+	+
Competition	-	+	+	0
Productivity	+ (0)	? (+)	? (+)	0
Direct displacement	-	-	-	0
Indirect displacement	+	-	-	0
Substitution and deadweight loss	-	-	-	0

Source: Calmfors (1994). Parentheses indicate possible but uncertain effects.

Table 1 shows that matching, competition, and productivity effects possibly lead to positive impact on regular employment. In addition, labour force effect may lead to higher regular employment when measured as proportion of population. Possible negative impacts come from deadweight losses and from substitution and displacement effects. The estimation of the size of different effects is an empirical question that can be addressed with multiple approaches.

Even though in theory it is possible to distinguish the differences of different effects it might not always be possible in empirical analysis. For example, in macroeconomic analysis it is hard to distinguish between substitution and displacement effects. If a programme participant displaces employed worker due to the employment subsidies (substitution effect) the observed effect is similar as in the case of displacement effect. There is one less regularly employed and one more employed whose employment is subsidised. The difference is that in the case of displacement effect the regular job is lost from another firm than the firm that hires the ALMP participant.

4 EMPIRICAL LITERATURE

Most of the microeconomic studies focus on investigating effects of ALMPs on the participants of different programs, referred as partial equilibrium effects or direct effects of the ALMPs. These kinds of studies are not usually able to consider indirect side effects of ALMPs since effects on persons outside the programme participants are not accounted for. When the indirect effects are also considered the studies examine general equilibrium effects of the ALMPs rather than partial equilibrium effects. Especially for policy implications it is crucial to consider general equilibrium effects since effectiveness evaluation or cost-benefit analysis may lead to completely different results when considering only partial equilibrium results (Blundell, Dias, & Meghir, 2003; Lise, Seitz, & Smith, 2004).

Whether a study can capture general or partial equilibrium effects is related to validity of causal identifying assumptions rather than methodologies used. There are two key assumptions regarding identification of the causal effects in microeconomic context: conditional independence assumption (CIA) and stable unit treatment value assumption (SUTVA) (Cahuc et al., 2014, 946-947). In the context of ALMPs CIA means that the control group of nonparticipants is comparable to treatment group of participants and SUTVA means that the program does not affect nonparticipants at all (Cahuc et al., 2014, 946). CIA can be fulfilled with randomisation in the case of controlled experiments or using other methods such as difference-in-difference and matching when using observational data. As analysed earlier, it is reasonable to assume that ALMPs do affect nonparticipants as well (Cahuc et al., 2014, 943-944; Calmfors, 1994). Therefore the strategy needs to be chosen such that SUTVA is not needed. If there is just an implicit assumption of absence of such effects, estimated effects will be biased (Cahuc et al., 2014, 947).

The side effects have been studied with three different approaches. First, the side effects have been studied using macroeconomic approach. These kinds of studies usually use regional data, for example from municipalities or provinces (e.g. Behrenz, Delander, & Månsson, 2012; Dahlberg & Forslund, 2005; Puhani, 2003). Macroeconomic studies have also been using aggregate time series data which have been analysed via vectoral autoregression (VAR) modelling, but these kind of studies provide merely suggestive results (e.g. Pehkonen, 1997; Skedinger, 1995). The idea of the analysis with regional data is to study whether number of different programme participants in the area affects regular employment negatively. If there is a negative effect on regular employment, then ALMPs have negative side effects. The main problem with the macroeconomic approach is that the employment may also have effect on volumes of ALMPs. This kind of simultaneous causality makes it hard to isolate the true causal effect of the ALMPs in macroeconomic level.

Second, the magnitude of the side effects has been estimated using surveys for the employers, programme participants and employment officers. In Sweden surveys have asked does respondent believe that the work performed by programme participants would have been done even without the program by someone else (substitution effect). Sometimes, if this question were answered in affirmative, there have been follow up question on whether the same person would have been employed for the job (deadweight loss). There are multiple challenges regarding the surveys. First, respondents may exaggerate or understate the importance of the ALMP for the hiring of the participant. Second, surveys are not able to estimate how ALMPs affect other firms and therefore they cannot be used to quantify the size of the displacement effect. (Calmfors et al., 2002, 32-33.)

Third, also microeconomic studies provide evidence of magnitudes of different side effects. These studies may use firm level data (e.g. Kangasharju, 2007; Lombardi, Skans, & Vikström, 2018) or individual level data (e.g. Blundell, Dias, Meghir, & van Reenen, 2004; Crépon et al., 2013; Gautier et al., 2018). The following sections examine different branches of the empirical literature in more detail, starting with macroeconomic literature.

4.1 Side effects of the active labour market policies

4.1.1 Macroeconomic studies

Dahlberg and Forslund (2005) studied direct displacement effects of subsidised employment and training measures on regular employment. They used panel data of 250 Swedish municipalities from 1987–1996. Their main identification strategy of the causal effects was based on using regional allocation of ALMP grants as an instrumental variable (IV). Most of the ALMP grants were distributed between counties based on number of job seekers (openly unemployed and ALMP participants) of previous fiscal year and service level of the employment services. Therefore, past unemployment and past total ALMP participants were used as instruments to identify variation in programme volumes that is independent from current employment levels. To avoid the simultaneity problems Dahlberg and Forslund (2005) also used municipal political majority as an instrument, used lagged ALMP participation in the estimations and constructed a proxy for municipality specific demand shocks. According to their generalized method of moments (GMM) estimation the magnitude of displacement effects from subsidised employment programmes was 65% but there were not any displacement effects from labour training measures.

Also Behrenz et al. (2012) examined direct displacement effects of Swedish ALMPs using similar model, identification strategy and data as Dahlberg and Forslund (2005). However, they used more recent data (panel of 285 municipalities from 1996–2006) and they focused on self-employment schemes. According to their GMM estimation results the self-employment schemes did not have any (statistically significant) displacement effects but the displacement effects from other programmes were 34%.

Puhani (2003) studied substitution and displacement effects using panel data of 49 Polish provinces from the period of 1992–1998. He analysed effects of expenditure on training programmes on outflows from unemployment into employment and effects of expenditure on all ALMPs (training, intervention works, direct job creation and loan schemes for self-employment) on outflows from employment into unemployment. The identification strategy was based on using lagged ALMP expenditures. According to Puhani (2003) the regional allocation of ALMP expenditures in Poland was determined by political process which should imply that the past ALMP expenditure would be exogenous variable i.e. it would not be determined by the flows between employment and unemployment. Models were estimated using both random and fixed effects estimators. According to the results training did not have any effects on outflows from unemployment to employment. The lack of effect on macroeconomic level may imply the presence of substitution effect since microeconomic evidence suggest that labour training has positive effect on employment probability of the programme participant (Card et al., 2018). Results did not show any robust evidence of displacement effects from other programs.

The studies regarding displacement effects of Finnish ALMPs are limited. However, Pehkonen (1997) studied displacement effects of job creation and training schemes in youth labour market in Finland using quarterly time series data from 1981Q1–1995Q2. Similarly, Skedinger (1995) studied the displacement effects in the youth labour market, but with Swedish data from 1970Q3–1991Q4. Both Pehkonen (1997) and Skedinger (1995) used similar VAR approaches in their analyses. The benefit of the VAR approach is that it avoids the simultaneity problem but at the same time obtained estimations are hard to interpret and it can provide merely indicative results rather than causal effects. Results of both studies suggest that job creation schemes in the youth labor market are associated with significant displacement effects.

Calmfors et al. (2002, 32-34) summarizes 11 Swedish survey studies regarding displacement effects. The results of the surveys vary significantly, but all studies suggest that displacement effects are related to different types of ALMPs. Overall, it seems that average displacement effect of a ALMP is higher if it is closer to regular labour market. The estimated average displacement effects of the recruitment subsidies, trainee replacement schemes, general employment subsidies and targeted employment subsidies are between 39 and 84% whereas estimated effects of work placement schemes and work experience schemes are around 15% (Calmfors et al., 2002, 34). Results of the surveys should be interpreted merely as suggestive evidence of the presence of displacement effects because of the possible biases discussed earlier.

Table 2 summarizes main findings from the macroeconomic empirical literature. Next section surveys the microeconomic literature.

Table 2. Summary of the macroeconomic literature

Author(s)	ALMP(s)	Data & method(s)	Main findings
Behrenz et al. (2012)	Self-employment schemes and other programs as a general variable.	Data: Panel of 285 Swedish municipalities from 1996–2006. Method: Difference GMM estimation, unemployment rate as external instrumental variable.	No direct displacement effects associated with self-employment schemes. Displacement effects from other programs were 34%.
Calmfors, et al. (2002, 32-34)	Subsidised employment programs (direct subsidised employment and practice programs).	Data: 11 Swedish survey studies, from 1989–1999. Method: Surveys	Surveys suggest that displacement effects from direct subsidised employment were substantial whereas displacement effects from practice programs were only modest.
Dahlberg & Forslund (2005)	Subsidised employment (relief work and practice) and training measures.	Data: Panel of 250 Swedish municipalities from 1987–1996. Method: Difference GMM estimation, unemployment rate as external instrumental variable.	Displacement effect from subsidized employment was 65%, but there were not any displacement effects associated with training measures.
Puhani (2003)	Training, subsidised employment (intervention works, direct job creation) and loan schemes for self-employment.	Data: Panel of 49 Polish provinces from 1992–1998. Methods: Fixed effects and random effects regressions.	Training programs did not have effect on outflows from unemployment to employment. Subsidised employment was not associated with displacement effects.
Pehkonen (1997)	Job creation schemes and training programs in youth labour market.	Data: Aggregate Finnish time-series data from 1981Q1–1995Q2. Method: VAR estimation.	Job creation schemes were associated with significant displacement effects.
Skedinger (1995)	Job creation schemes and training programs in youth labour market.	Data: Aggregate Swedish time-series data from 1970Q3–1991Q4. Method: VAR estimation.	Job creation schemes were associated with significant displacement effects.

4.1.2 Microeconomic studies

Blundell et al. (2004) studied effects of the New Deal for Young People labour market program in United Kingdom using individual level data. The program was aimed for 18–24-year-old persons that had been unemployed for at least 6 months. It consisted of 4-month job-search assistance stage and employment subsidies if the participant is still unemployed after the job-search assistance stage. Before national roll-out there was area specific pilot stage which enabled the matching adjusted difference-in-difference comparison within the pilot area based on age eligibility and between pilot area and other areas. Results showed that the program increased participants' transitions to employment by 5 percentage points. Wage subsidies was more influential program since only 1 percentage point of the 5 was due to job-search assistance stage. The effect of the program is the largest during the first quarter of the program and decreases after that which indicates that the effect might be smaller in the long run. Different specifications of control groups yielded quantitatively similar results which indicates that this program did not have displacement effects, or they were very modest.

Crépon et al. (2013) studied substitution effects of job placement assistance by using individual level data based on extensive randomized job seeker assistance experiment implemented in France. The randomization was implemented as a two-step design where in the first step proportion P of the job seekers (0%, 25%, 50%, 75% or 100%) was randomly assigned to a local employment area. In the second step a proportion P of the area's eligible job seekers was randomly selected to participate in the program. The participation was voluntary which led to rather low compliance rate: around one third of the selected chose to participate in the program. However, this design enabled the possibility of evaluation of the externalities of the program on the nonparticipants by comparing nonparticipants in the areas where some of the job seekers participants participated in the program to areas without participants.

Crépon et al. (2013) found that the job placement assistance program did have positive effect on the probability of employment of the participants in the short run (8 months after the program), but the effect came partly at the expense of nonparticipants. Results suggest that the program improved search effectiveness of the participants but reduced relative search effectiveness of nonparticipants. The substitution effect was present for men, especially in weak labor markets. When employment effects were examined for longer period (12 to 20 months after the program) there were no employment effects, which indicates that the program led only earlier employment of the participants.

Gautier et al. (2018) revisited Danish experiment of mandatory activation program and their aim was to investigate program's effects on the nonparticipants. Earlier the same experiment had been investigated e.g. by Graversen and Van Ours (2008), but their study was limited on employment effects on the participants of the program. In the experiment the participants of the mandatory program were selected randomly in two Danish counties. The program consisted of multiple phases which included 2-week job search assistance program in the beginning which were followed by regular meetings with caseworkers. If the participant was unemployed after 4 months the participant could be assigned to variety of different ALMPs for 3 months such as subsidised job or classroom training. Gautier et al. (2018) compared employment outcomes of the nonparticipants in experiment counties to employment outcomes of nonparticipants in other counties. Their results show that the program increased job-finding rate by 9% but one third of the effect was result of decrease in job-finding rate of the nonparticipants.

The literature regarding side effects of training measures is limited, but Ferracci et al. (2014) studied how participation rates of French training programs affected employment of both participants and nonparticipants. They used individual level quarterly data from 2002–2007. The identification strategy is based on an assumption that participation rate will not affect the employment outcomes between markets. Results of the analysis show that participation rate affected employment probabilities of the participants and nonparticipants significantly. Higher participation rates led to lower employment probabilities for an average participant. For nonparticipant, the relationship between employment probability and participation rate was convex. When participation rate went from 2% to 5.5% the effect on employment probability was negative but when participation rate increased over 5.5% the effect was positive. Negative employment effects of the higher participation in training programs might be due to locking-in, substitution and wage effects. Positive effects on employment of nonparticipants might be caused by increased labour demand of the firms.

Side effects of the employment subsidies have been studied using firm level data. Kangasharju (2007) investigated employment effects of wage subsidies using annual Finnish firm level data from 1995–2002. The analysis was based on the difference-in-difference estimator which compares the difference of changes of payroll (proxy for firm's employment) for subsidized and non-subsidized firms. To be able to obtain valid results the difference-in-difference method was adjusted with regression and matching methods since the firms receiving wage subsidies were not chosen randomly. According to the results the wage subsidy increased the payroll of a subsided firm around 9%. The average size of the wage subsidy was one third of the wage and the effect on payroll (9%) was over three times higher than the average proportion of the wage subsidies relative to the payroll (2.6%). Therefore, the wage subsidy truly stimulated the employment i.e., there were no deadweight losses. In addition, results showed that the wage subsidies did not influence non-subsidized firms within same industry or geographical area, which indicates that there were no displacement effects.

Lombardi et al. (2018) studied effects of targeted wage subsidies on firm performance using Swedish firm level data from 1998–2008. During the period there were two different wage subsidy schemes. Both schemes were targeted to long-term unemployed, but during 1998–2006 the wage subsidy needed to be approved by a caseworker of the employment office. From the beginning of 2007 such approval was not needed anymore. The analysis was based on comparison of firms receiving wage subsidies and non-subsidized firms. The comparison was adjusted by matching of the firm characteristics. According to the results employment in subsidised firms increased compared to the control group during the first scheme. During the second scheme there was not similar employment effects, the total employment of the subsidized firms was not higher compared to non-subsidized firms. Even though the total employment of the subsidized firms did not increase the number of workers receiving wage subsidies was higher during the second scheme. This implies that during the second scheme subsidised workers substituted non-subsidized workers.

Table 3 summarizises main findings from empirical microeconomic literature. Next section compares and discusses the findings of both microeconomic and macroeconomic literature.

Table 3. Summary of the microeconomic literature

Author(s)	ALMP(s)	Data & method(s)	Main findings
Blundell et al. (2004)	New Deal program for young people: job-search assistance and employment subsidies	Data: Individual level panel data from 1982–1999 from UK. Methods: Difference-in-differences with matching.	New Deal program increased transitions to employment of the participants by 5 percentage points. There were no signs of displacement effects associated with the program.
Crépon et al. (2013)	Job placement assistance program	Data: Individual level panel data from 2008–2010 from France. Method: OLS.	The program increased the probability of employment of the participants in the short run, but the effect came partly at the expense of non-participants i.e. the program caused negative externalities.
Gautier et al. (2018)	Mandatory activation program: job search assistance, subsidised jobs, classroom training and vocational training	Data: Individual level panel data from 2004–2006 from Denmark. Method: Difference-in-differences.	The program increased job-finding rate of the participants by 9% but about 1/3 of the effect was caused by decrease of job-finding rate of the non-participants.
Ferracci et al. (2014)	Training programs	Data: Individual level panel data from 2002–2007. Methods: Matching estimators.	Areal program participation rates affected employment probabilities of the participants and non-participants. However, the relationship between participation rate and employment probability of non-participants was convex, i.e. effect on non-participants was positive with higher participation rates and negative with middle level participation rates.

Kangasharju (2007)	Wage subsidies	Data: Firm level data from Finland from, 1995–2002. Methods: Fixed effects, difference GMM, system GMM and difference-in-differences with matching.	Wage subsidies were not associated with deadweight losses or displacement effects.
Lombardi et al. (2018)	Wage subsidies	Data: Firm level data from Sweden from 1998–2008. Methods: Matching estimators and fixed effects.	When approval of the wage subsidy from employment office caseworker was needed the employment in subsidised firms increased compared to non-subsidised firms. When such approval was not needed anymore, total employment of the subsidised firms did not increase but the number subsidised workers increased which implies that subsidised workers substituted non-subsidised workers.

4.2 Discussion of the empirical literature

Earlier literature shows that different types of ALMPs certainly have significant side effects on agents outside of the programs. Summarizing, job search assistance programs introduce substitution effects between participants and other job seekers even though Blundell et al. (2004) did not find such effects when program was targeted to young people (Crépon et al., 2013; Gautier et al., 2018). Also training programmes seem to have substitution effects between participants and nonparticipants (Ferracci et al., 2014; Puhani, 2003). Private sector wage subsidies or self-employment schemes do not seem to have significant displacement or substitution effects (Behrenz et al., 2012; Kangasharju, 2007; Lombardi et al., 2018). However, macroeconomic studies suggest that employment subsidies (covering also public sector job creation) might have significant displacement and substitution effects (Dahlberg & Forslund, 2005; Pehkonen, 1997; Skedinger, 1995). Also survey studies imply that employment subsidies are associated with substitution effects (Calmfors et al., 2002).

Most of the microeconomic studies investigating effects on the nonparticipants show that the positive employment effects on participants come partly with the expense of nonparticipants (Crépon et al., 2013; Ferracci et al., 2014; Gautier et al., 2018). This implicates that SUTVA is not valid in studies regarding only employment effects of the participants and therefore the employment effects on nonparticipants should be also included, when evaluating the effectiveness of a program. In practice the examination of the side effects requires regional variation in implementation of the program that it is possible to compare employment outcomes of the nonparticipants which are affected and are not affected by the program. If the side effects are not included, the evaluation overestimates the effectiveness of the program.

Firm level studies implicate that private sector wage subsidies are efficient tool in general since they do not seem to create significant displacement or substitution effects (Kangasharju, 2007; Lombardi et al., 2018). However, Lombardi et al. (2018) found some signs of substitution effect after the applications for the subsidies were not approved by the caseworkers anymore. This indicates side effects of the employment subsidies could be mitigated by assessing the suitability of the subsidy applicants. Another key feature for effective wage subsidy program could be the targeting since subsidies examined by Lombardi et al. (2018) were targeted only to long-term unemployed. Similarly, Finnish subsidies were targeted mostly to the outsiders of the labour market (young unemployed and long-term unemployed) (Kangasharju, 2007). Survey results provide different kind of view on severity of side effects regarding wage subsidies but those are not nearly as convincing because of the possible biases introduced in the estimations (Calmfors et al., 2002).

Macroeconomic literature has estimated that subsidised employment is associated with large displacement effects (Dahlberg & Forslund, 2005; Pehkonen, 1997; Skedinger, 1995). However, these results are not comparable to the firm level studies for number of reasons. First, in macroeconomic studies also other measures of subsidised employment than merely private sector wage subsidies are included in estimations. Public job creation has had large role in subsidised employment especially in Sweden (Dahlberg & Forslund, 2005). Inclusion of public employment subsidies could be one of the reasons for such different results. This indicates also one of the problems regarding macroeconomic studies: they usually examine ALMPs as larger categories consisting of different programs even though some programs might have larger side effects than others.

Other problem regarding macroeconomic studies is that they suffer from problems (e.g. simultaneity) that challenges the causality of estimated effects. VAR approach does not suffer from simultaneity bias, but the results are not robust and studies use only aggregated time-series data (Pehkonen, 1997; Skedinger, 1995). Puhani (2003) deals with the simultaneity bias by using lagged values of spending on ALMPs but it is questionable whether this approach is enough to solve the problem. The dynamic panel data approach (difference GMM estimator (Arellano & Bond, 1991)) used by Behrenz et al. (2012) and Dahlberg and Forslund (2005) is in danger of simultaneity bias but they manage the problem by instrumenting and with the use of lagged variables. Dahlberg and Forslund (2005) also provide proof of robustness of the results by doing various robustness tests. However, recent literature has implicated that GMM estimators may not be reliable especially in the case of weak instruments and too large number of instruments (Bun & Sarafidis, 2015; Roodman, 2009b).

5 DATA AND METHODOLOGY

5.1 Identification strategy

The goal of this analysis is to attempt to analyse the effect of ALMPs on regular employment. The identification strategy, methodology and modelling follow Dahlberg and Forslund (2005), who implemented similar analysis in the Swedish context. Effects of ALMP participation on regular employment can be estimated by regressing municipal regular employment (n) on ALMP participation (P). As noted, there is high possibility that the relationship between ALMP and employment is simultaneous i.e., at some extent the volume of ALMPs affect employment level and the employment level affects the volume of ALMPs. As a solution for this problem the IV method is a possible solution to mitigate the effect of the endogeneity.

In addition to ALMP volumes there are also other factors that affect regular employment. Factors which affect regular employment and are correlated with ALMP volumes need to be controlled as well, so estimates are not biased by omitted variable bias. These factors are called control variables (X). The regression model for estimation of equilibrium regular employment can be written

(1) $n_{it}^{*} = \alpha + \beta P_{it} + \gamma X_{it} + \varepsilon_{it}$,

where n_{it}^{*} denotes equilibrium (regular) employment, i denotes municipalities, t denotes time (year), β is the effect of ALMP participation, γ is the effects of control variables and ε_{it} is the error term.

The model in equation (1) is static, but it is reasonable to assume that the adjustment of employment is sluggish. The sluggish adjustment might be due to hiring and firing costs, for example. This sluggishness can be taken into consideration by introducing dynamicity to the model. Following Dahlberg and Forslund (2005) the dynamic formulation of the model can be written

(2) $n_{it} = (1 - \lambda)(\alpha + \beta P_{it} + \gamma X_{it}) + \lambda n_{it-1} + \varepsilon_{it}$,

where n_{it} is actual employment at the period t and λ is an adjustment coefficient which measures how sluggishly actual employment changes to equilibrium employment. When λ is larger the sluggishness is stronger. Next sections describe the control variables and how the simultaneity problem is addressed via instrumenting and other methods.

5.1.1 Control variables

Local labour demand is important factor which influences the employment level of municipalities. Changes in local labour demand is controlled with a proxy measure of municipality specific labour demand shocks. The variable is constructed using information of how the employment is distributed across industries (letter level breakdown, in total 22 industries). The demand proxy variable is calculated similarly as Dahlberg and Forslund (2005) formulate it:

$$(3)\quad Demand\ proxy_{it} = \sum_{j=1}^{J} \frac{e_{ijt-1}}{e_{Njt-1}} * (e_{N-ijt} - e_{N-ijt-1})$$

where subscript N denotes the national level figures, subscript j denotes industry (j = 1, 2, …, 22) and e denotes employment. Subscript $-i$ in the second term denotes that employment figures in industry j for municipality i are excluded from the national level figures. The variable reflects how the absolute employment would have changed in municipality i between years t-1 and t if the employment development by industry would have been the same as in national level. The constructed proxy is application of Bartik instrument (Bartik, 1991) which is often called also shift-share instrument. This approach has been widely applied to different settings to control or instrument local demand shocks (see e.g. Autor & Duggan, 2003; Blanchard, Katz, Hall, & Eichengreen, 1992; Bound & Holzer, 2000; Broxterman & Larson, 2020; Notowidigdo, 2020; Saks, 2008).

Other factors affecting local labour demand are wage rate and the price of capital. There is not information available for the price of capital or wage rate, but wage rate can be proxied with average of annual labour income. In addition, demographic factors of municipalities might influence especially labour demand of the public sector. If large share of the municipality's population consists of the young and elderly, the municipality needs to provide more education and eldercare services. Therefore, also demographic characteristics of the municipality are included as control variables.

5.1.2 Simultaneity and instruments

In Finland, the total size of spending in ALMPs is mostly determined by employment grants (*työllisyysmäärärahat*). The total size of the employment grant is decided by the central government in the annual budgeting process. Central government also makes approximation of how the total amount is allocated to different types of ALMPs.

After the decisions by the central government, TEM decides how the grant will be distributed to ELY centres. There have been different criteria on how the grant is allocated to ELY centres, but the largest share of employment grant ("basic division", *perusjako*), has been distributed between ELY centres using the same criteria mostly. During 2016–2020 the basic division has been allocated across ELY areas depending on area's the number of unemployed jobseekers, unemployment rate (both including openly unemployed and ALMP participants) and number of unemployed that are difficult to employ during previous year (TEM, 2016; TEM, 2017; TEM, 2018; TEM, 2019; TEM, 2020c). According to TEM official (personal communication, October 23, 2020) in 2015 the criteria were otherwise the same, but number of unemployed that are difficult to employ was not included. During 2012–2014 the criteria were different, and the number of criteria was larger, but unemployment rate of previous year was one of the criteria considered. There is no information available from the years prior to 2012 about how the employment grants have been distributed, but it is reasonable to believe that the process has been similar.

This allocation process implies that regional ALMP spending is increasing in both past unemployment and past ALMP participation. Therefore, past unemployment and past ALMP participation are viable instruments for total ELY area spending on ALMPs. However, the data are from municipalities and there is not similar formal framework on how the ALMP grants are distributed within ELY areas to municipality level. Here it is assumed that similar relationship holds at the municipality level as well. If this critical assumption fails, there is clear problem with the validity of this instrument.

Another method to deal with the simultaneity is to use lagged values for the ALMP participation variable. The measures for ALMP participants are calculated as 12-month averages prior to measure of regular employment. I.e. regular employment is measured in month *m* (end of December each year). The number of ALMP participants is calculated as an average of months *m*-1 to *m*-12.

5.2 Methodology

The core of the identification strategy is formulated around the regional allocation of ALMP grants due to simultaneity problem. Therefore, the model needs to be estimated with IV methods. Suitable method is GMM estimator, which enables the possibility of using external and internal instruments and the use of lagged dependent variable which introduces dynamicity to the model. This would not be possible with simpler ordinary least squares (OLS) estimator since the usage of lagged dependent variable would provide biased estimates (Nickell, 1981).

5.2.1 Generalized methods of moments

In the OLS framework the estimation process can be described such that the estimator minimizes sum of squared errors. In other words, it can be described that the identification comes from the assumption that regressors are orthogonal to errors (Roodman, 2009a, 88). This means that the products of regressors and errors (moments) are set to 0. This is why this estimation method is called method of moments, and it can actually be shown that OLS is a special case of GMM (see e.g. Hayashi, 2000, chapters 1–2).

In the GMM framework there are two types of variables: regressors (denoted with x) and instruments (denoted with z). These categories can overlap: the variables which are included in both categories are exogenous or predetermined regressors and variables only in x are endogenous regressors (Hayashi, 2000, 199; Roodman, 2009a, 88). When considering GMM estimation the key principle is that estimated coefficient are chosen such that the moments of errors and instruments are equal to 0 (Hayashi, 2000, 204; Roodman, 2009a, 88). GMM is a generalization of method of moments since it allows the number of instruments (K) to be larger than the number of regressors (L) (Hayashi, 2000, 205). When the number of instruments is larger than the number of regressors, the model is overidentified. When K = L (i.e. model is exactly identified) the GMM estimator becomes "normal" IV estimator and when K = L and x = z the IV estimator reduces to OLS estimator (Hayashi, 2000, 206).

To present the basic principle of GMM estimator suppose there is linear model to be estimated

(4) $y_i = x_i'\beta + \varepsilon_i \; (i = 1, 2, \ldots, n)$,

(5) $E(g_i) = 0, where\ g_i = z_i * \varepsilon_i$

where x_i is L-dimensional vector of regressors, β is an L-dimensional coefficient vector, ε_i is the error term and z_i is K-dimensional vector (vector of instruments) where all K variables are orthogonal to the current error term as equation (5) states (orthogonality condition). In addition, let w_i be the unique and nonconstant elements of (y_i, x_i, z_i) which are jointly stationary and ergodic. (Hayashi, 2000, 198.)

The formulation of the orthogonality condition has important implication: in GMM framework the assumptions imposed on variables are less restrictive than in OLS framework. In OLS it is assumed that variables are exogenous (uncorrelated with past, present, and future error terms) whereas in GMM framework it is assumed that current and lagged values of the variables are uncorrelated only with current values of the error term.

Using equation (4) and w_i we can rewrite moments g_i as

(6) $g_i = g(w_i; \beta) = z_i(y_i - x_i'\beta)$.

In population level the orthogonality condition is $E[g(w_i; \beta)] = 0$ which empirically corresponds to sample average of $g(w_i; \beta)$ since the mean of the sample is the expected value (Hayashi, 2000, 204). With a value of $\tilde{\beta}$ for β the sample average can be written

(7) $g_n = \frac{1}{n}\sum_{i=1}^{n} g(w_i;\ \tilde{\beta})$.

Optimally one would like to estimate $\hat{\beta}$ such that $g_n(\hat{\beta})$ would equal exactly 0 but that is not possible when the model is overidentified (i.e. K>L). In this case the goal is to choose $\hat{\beta}$ such that $g_n(\hat{\beta})$ is as close to 0 as possible. The "closeness" of $g_n(\hat{\beta})$ to 0 is done by choosing $K\ x\ K$ dimensional moment weighting matrix *W*. The minimization problem to estimate coefficient β, denoted by $\hat{\beta}$ can be written

(8) $\hat{\beta}(W) = argmin\, J(\tilde{\beta}, W), where\, J(\tilde{\beta}, W) = ng_n(\tilde{\beta})' W g_n(\tilde{\beta}).$

Solution for this minimization problem is the GMM estimator and it can be written (Hayashi, 2000, 207):

(9)

$$\hat{\beta}(W) = (S_{zx}' W S_{zx})^{-1} S_{zx}' W s_{zy} = \left(\frac{1}{n}\left(\sum_{i=1}^{n} z_i x_i'\right) W \frac{1}{n}\left(\sum_{i=1}^{n} z_i x_i'\right)\right)^{-1} \frac{1}{n}\left(\sum_{i=1}^{n} z_i x_i'\right) W \frac{1}{n}\left(\sum_{i=1}^{n} z_i y_i\right).$$

The GMM estimator is efficient when the used weighting matrix is optimal weighting matrix $\widehat{W}_o = S^{-1} = \left(\frac{1}{n}\sum_{i=1}^{n} \hat{\varepsilon}_i^2\, z_i z_i'\right)^{-1}$, where $\hat{\varepsilon}_i = y_i - x_i\hat{\beta}$. To be able to obtain residuals for calculating the optimal weighting matrix the estimation process of efficient GMM estimation is implemented in two steps. In the first step estimate for coefficient β_1 is obtained with estimator $\hat{\beta}_1(\widehat{W})$ (see equation (9)) where weighting matrix is chosen arbitrarily, for example $\widehat{W} = S_{zz}^{-1} = \left(\frac{1}{n}\sum_{i=1}^{n} z_i z_i'\right)^{-1}$. The optimal weighting matrix $\widehat{W}_o$ can be calculated using residuals from the first step estimation. In the second step the model is estimated using optimal weighting matrix and thus the second step GMM estimator $\hat{\beta}_2(\widehat{W}_o)$ is efficient and also robust to heteroskedasticity. (Hayashi, 2000, 212–213; Roodman, 2009a, 94.)

When considering model in equation (2) the most suitable method of implementing GMM estimator would be difference GMM, also called Arellano-Bond estimator (Arellano & Bond, 1991; Holtz-Eakin, Newey, & Rosen, 1988). Difference GMM is designed for situations where data generating process may be dynamic, fixed individual effects may be arbitrarily distributed, some regressors can endogenous or predetermined and idiosyncratic disturbances may be heteroskedastic and serially correlated but uncorrelated across individuals (Roodman, 2009a, 99). It also allows the usage of both internal (lagged values of endogenous or predetermined regressors) and external instruments (Roodman, 2009a, 100).

Difference GMM is needed in this case since the adjustment process of (regular) employment is assumed to be sluggish. Therefore, lagged value of dependent variable needs to be added to the right-hand side of the model. However, the lagged dependent variable can create a problem called dynamic panel bias, which can be managed with difference GMM (Nickell, 1981; Roodman, 2009a, 101). Suppose that a municipality faces extensive negative shock to the employment in the middle of our sample period which is due to some factor that is not controlled in the model. If $\varepsilon_{it} = \mu_i + v_{it}$, where μ_i is the fixed effects component of the error term and v_{it} idiosyncratic component of the error term, the fixed effects component would be correlated with the lagged dependent variable in this case. Fortunately, the difference GMM can be used to remove the fixed effects component by transforming the data with first differences (Roodman, 2009a, 104).

5.2.2 Testing for model and instrument validity

Crucial assumptions regarding an instrument is that it is correlated with endogenous variable and it is exogenous or predetermined i.e. instruments are orthogonal to the error term (Hayashi, 2000, 200; Roodman, 2009a, 97). The validity of the instruments or whether the model is specified correctly can be tested using Hansen's test of overidentifying restrictions (also called J test) when the model is overidentified i.e. the number of instruments is larger than the number of regressors (Hansen, 1982; Roodman, 2009b, 141). The null hypothesis of the test is that the distribution of moments is centred around 0, i.e. instruments are jointly valid or the model is specified correctly (Roodman, 2009b, 141). However, the J test cannot be relied on solely. Large number of instruments often leads to implausibly high p-values which weakens the validity of the Hansen test (Andersen & Sørensen, 1996; Bowsher, 2002; Roodman, 2009b). Large number of instruments may also lead to overfitting of the endogenous variables which causes estimated coefficients to be biased, especially with a small sample size (Roodman, 2009b; Windmeijer, 2005).

There are two methods that can be used to restrict the number of instruments. First method is obvious: limit the maximum number of lags used as instruments rather than using all available lags. Second method is related to how the instrumental variable matrix Z is formulated. In the standard form of the matrix there is a column for every instrumental variable for every time period i.e. there is separate instruments for every time period (Roodman, 2009a, 107-108; Roodman, 2009b, 138). $Z_{standard}$ below illustrates the standard matrix structure where Δy_{it} is instrumented with all available lags in periods t = 3, 4, 5. First column is used to instrument Δy_{i3}, second and third for Δy_{i4} and three last columns for Δy_{i5}. Missing observations of lags are replaced with 0. Alternatively, the instrument sets for a period can be collapsed to one column, illustrated below as $Z_{collapsed}$ (Roodman, 2009a, 107-108; Roodman, 2009b, 148). In this case there is only one instrument for a time period. Note that collapsing the matrix also leads to smaller number of the moment conditions, since the number of conditions depend on the number of instruments (i.e. number of columns of the Z matrix).

$$Z_{standard} = \begin{bmatrix} 0 & 0 & 0 & 0 & 0 & 0 \\ y_{i1} & 0 & 0 & 0 & 0 & 0 \\ 0 & y_{i2} & y_{i1} & 0 & 0 & 0 \\ 0 & 0 & 0 & y_{i3} & y_{i2} & y_{i1} \end{bmatrix} \; or \; Z_{collapsed} = \begin{bmatrix} 0 & 0 & 0 \\ y_{i1} & 0 & 0 \\ y_{i2} & y_{i1} & 0 \\ y_{i3} & y_{i2} & y_{i3} \end{bmatrix}$$

Another test to determine whether the model is specified correctly is to apply Arellano-Bond autocorrelation test to the residuals in first differences (Arellano & Bond, 1991). The idea is to test whether first differences of the residuals are serially correlated of order 2 or higher. If first differences of the residuals $\Delta\varepsilon_{it} = v_{it} - v_{it-1}$ are serially correlated of order *l*+1 the residuals in levels are serially correlated of order *l*. This information reveals which lags can be used as instruments. If residuals $\varepsilon_{it} = v_{it}$ are serially correlated of order 1 (i.e. first differences of the residuals are serially correlated of order 2) then, for example, the second lag of the dependent variable y_{it-2} is endogenous to the v_{it-1} in the error term in first differences. In this case y_{it-2} would be invalid instrument and therefore only lagged values of *t*-3 and longer should be used as instruments. (Roodman, 2009a, 119.)

5.3 Data

Dataset for the analysis is compiled from various statistical data sources of Statistics Finland. Number of ALMP participants in different programs and number of unemployed by municipality is obtained from ESS at monthly level. The total number of employed and the number of employed by industry in municipality level is obtained from population statistics which are available annually, measured in the end of the year. The average of annual income by municipality is obtained from income distribution statistics. Lastly, the annual demographic characteristics of the municipalities including population, population under 15 years-old, population aged 15-64 (working age population, *pop1564*) and population over 64 years-old are obtained from municipal key figures dataset, measured in the end of the year. The data was downloaded from Statistics Finland open access application programming interface (API) using *pxweb* R package (Magnusson, Kainu, Huovari & Lahti, 2019).

Variables are defined in table 4. All variables except *Income* are normalised using lagged working age population to make figures comparable between municipalities. Working age population is chosen as normalization variable instead of labour force since ALMPs can affect labour force participation (Johansson, 2002). Working age population is lagged by one year to avoid simultaneity problem since the number of ALMP participants might affect the number of working age population via migration. Income variable is also lagged one year for mitigation of the simultaneity problem.

There are two specifications for the *Subsidised employment* variable. First specification follows Dahlberg and Forslund (2005): both practice programs and other forms of subsidised employment are included under the same variable. However, practice programs and other subsidised employment programs are different in nature (see section 2.2.1) which is why the displacement effects of those programs might be different. Therefore, the second specification follows Dahlberg and Forslund (1999): practice programs are separated in their own variable (*Practice*) and the subsidised employment variable *Subsidised employment2* consists of programs where employment relationship is established.

Table 4. Definitions of the variables

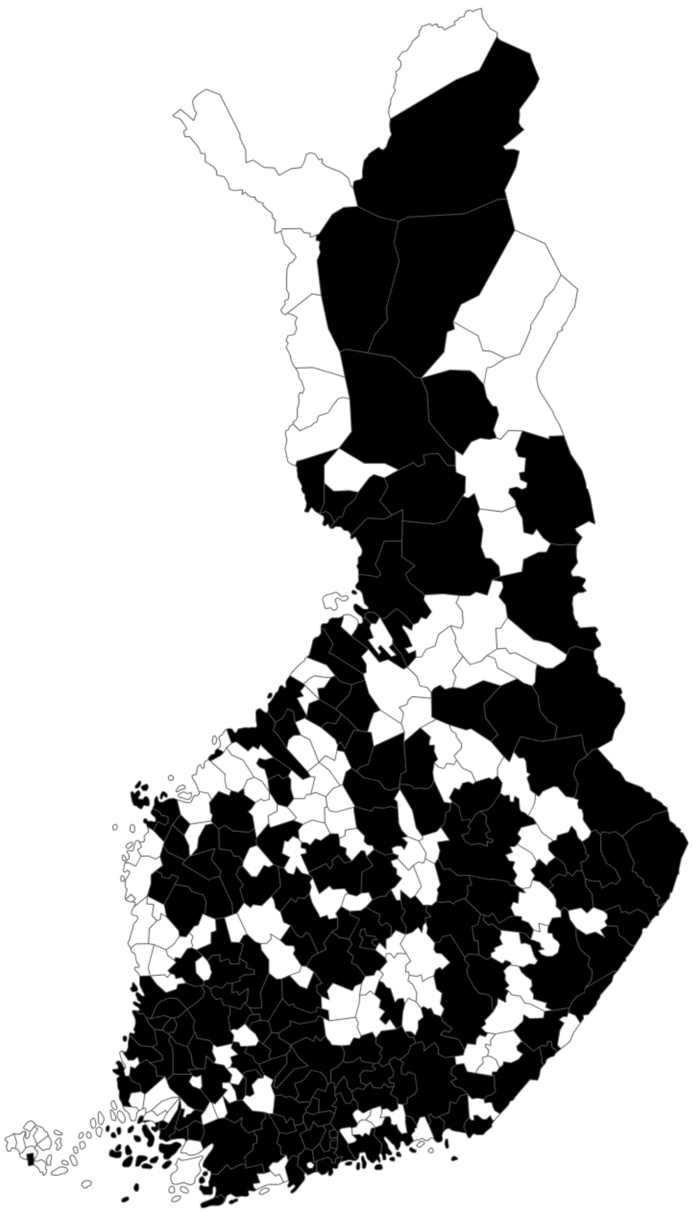
Variable	Definition
Dependent variable	
Regular employment (n)	Employed excluding subsidised employment that is classified as employed in the statistics: (Employed - wage subsidies - employed to government agencies and institutes – apprenticeship training – start-up grants)/pop1564
Explanatory variables of interest	
Subsidised employment	Average number of persons receiving wage subsidies, employed to government agencies and institutes, in apprenticeship training, receiving start-up grants and in work practice or in work trial/pop1564
Training	Average number of persons in labour market training, coaching, rehabilitative work activities and independent studying program/pop1564
Subsidised employment2	Average number of persons receiving wage subsidies, employed to government agencies and institutes, in apprenticeship training and receiving start-up grants/pop1564
Practice	Average number of persons in work practice or in work trial/pop1564
Control variables	
Demand	Labour demand proxy/pop1564
Income	Average of annual income in a municipality (euro)
Young	Fraction of municipality population aged 0-14
Old	Fraction of municipality population aged 65 or more
External instrumental variable	
Unemployment rate (u)	Average number of the unemployed/pop1564

Notes: pop1564 = Population aged 15–64 (working age population) of a municipality at period t-1.

The data for the number of employed by industry is available only from 2007 to 2018 which limits the time frame of datasets to 2008–2018 since the demand variable is constructed for period t using observations from t-1 (see equation (3)). Overall, the data are available for 310 municipalities (using 2020 regional division), but for large group of municipalities there are missing observations in programme participation due to confidentiality of the data. Observations are classified as confidential if there is less than 5 persons participating in a program. Municipalities with a missing observation in December for programs which are used to calculate regular employment were dropped from the dataset. In addition, municipalities with more than six missing monthly observations within a year for programs used to calculate subsidised employment or training variables were dropped. Final dataset used in the main analysis is balanced panel with 168 municipalities with a time frame of 2008–2018.

Six observations were chosen as a threshold to be able to smooth seasonal fluctuation of the programme participation and to avoid larger attrition of the sample. Only 103 municipalities do not have any missing monthly observations for the programme participation. From now on samples of the dataset without any restriction on monthly observations for programme participation are referred as unfiltered.

Attrition of municipalities is mainly related to the population of the municipality since smaller municipalities are more likely to have missing observations due to confidentiality. Therefore, missing observations and attrition of municipalities should not have any systematic relationship with displacement effects. In addition, the attrition of municipalities does not seem to be regionally systematic. Figure 20 shows municipalities within the sample in Finnish map. To assess whether construction of the dataset affects the obtained results, also other specifications of the datasets are considered in sensitivity analysis section.



Notes: Municipalities within the sample are colored.
Sources: Statistics Finland, geofi R package (Kainu, Lehtomäki, Parkkinen, Miettinen, Kantanen & Lahti, 2021).

Figure 21. Municipalities within the final dataset

Descriptive statistics for the dataset are shown in table 5. Descriptive statistics for balanced and unfiltered sample in addition to unbalanced and unfiltered sample are reported in appendix 1.

Table 5. Descriptive statistics

Variables	**Mean**	**Standard deviation**	**Min**	**Max**
		Dependent variable		
Regular employment	0.635	0.059	0.479	0.796
Explanatory variables of interest				
Subsidised employment	0.013	0.006	0.003	0.042
Training	0.018	0.009	0.002	0.056
Subsidised employment2	0.010	0.005	0.002	0.037
Practice	0.003	0.002	0.000	0.011
		Control variables		
Demand	-0.001	0.012	-0.040	0.049
Young	0.167	0.034	0.096	0.351
Old	0.220	0.053	0.077	0.384
Income	28 843	7 408	16 331	55 075
		External instrumental variable		
u	0.080	0.024	0.020	0.154

Notes: See table 4 for definitions of the variables. The number of municipalities is i = 168, the number of years is t = 11 and the number of observations is n = 1848.
Source: Statistics Finland, Employment Service Statistics, Population Statistics, Income Distribution Statistics and Municipal Key Figures Dataset.

6 RESULTS AND ANALYSIS

6.1 Static model

The starting point of the analysis is to estimate results in static framework using fixed effects estimator. Estimated model can be written

(9) $n_{it} = \alpha_t + \beta P_{it} + \gamma X_{it} + f_i + \varepsilon_{it}$,

where i denotes municipalities, t denotes time (year), α_t is a time dummy, n_{it} is regular employment, P_{it} is vector of ALMP program variables (*Subsidised employment*, *Subsidised employment2*, *Training* and *Practice*), X_{it} is vector of control variables (*Young*, *Old*, *Demand* and *Income*), f_i is the municipality specific effect, ε_{it} is the error term and β, γ are the estimated coefficients for program variables and control variables. Following Dahlberg and Forslund (2005), square root of the variable *Income* is used in the model.

Regression results for the fixed effects models are reported in table 6. Results show that regardless of how subsidised employment variable is specified, there is statistically significant negative relationship between regular employment and subsidised employment and training measures. When control variables are included in the model 4 the point estimate for *Practice* is not statistically significant which indicates that practice programs are not associated with displacement effects. Comparison of point estimates for subsidised employment between models 2 and 4 implicate similar conclusion since point estimate for subsidised employment less negative when the practice is included within the *Subsidised employment* variable.

The insignificance of *Practice* is interesting result since pairwise correlation between *Practice* and *Regular employment* is -0.52 and statistically significant at 1% level. At least this finding shows that it is important to control for other factors which also influence employment.

There are few indications that the endogenous nature of the employment and program variables influence the obtained estimates. First, the point estimates for subsidised employment in all specifications are below -1. In this framework, economic interpretation of the coefficients would be that subsidised employments crowds out over 100% of regular employment. In other words, for every subsidised job more than one regular job would be lost. This does not sound plausible. Second, with similar interpretation, point estimates indicate that training programs would crowd out 35% of regular employment. However, training measures are not expected to cause any displacement since participants are not supposed to be working. On the other hand, training measures might lock in job seekers which could lead to lower employment. Overall, results likely suffer from the endogeneity bias which is why the results should be interpreted only as associations and even then cautiously.

Table 6. Fixed effects estimation of static models

Dependent variable: regular employment (n)				
Variables	**(1)**	**(2)**	**(3)**	**(4)**
Subsidised employment	-1.637***	-1.061***		
	(0.190)	(0.143)		
Subsidised employment2			-1.621***	-1.146***
			(0.237)	(0.1971)
Training	-0.684***	-0.357***	-0.687***	-0.352***
	(0.137)	(0.108)	(0.136)	(0.107)
Practice			-1.691***	-0.719
			(0.580)	(0.527)
Young		0.3926***		0.400***
		(0.1344)		(0.133)
Old		0.473***		0.475***
		(0.077)		(0.076)
Demand		-0.027		-0.022
		(0.113)		(0.115)
Income		0.002***		0.002***
		(0.000)		(0.000)
Constant	0.670***	0.113*	0.670***	0.109*
	(0.004)	(0.058)	(0.004)	(0.058)
Observations	1 848	1 848	1 848	1 848
Adj. R-squared	0.660	0.706	0.660	0.706
Number of municipalities	168	168	168	168
Time dummies	Yes	Yes	Yes	Yes

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. See table 4 for definitions of the variables.

Regarding control variables, all point estimates are statistically significant except for *Demand*. *Young* and *Old* are positively associated with regular employment, like expected since municipalities organise education, eldercare and other services used by the young and the old. When shares of the young and the old are larger, intuitively a municipality would need to provide more of those services which would lead to higher regular employment.

Estimates show also that *Income* has statistically significant positive relationship with regular employment. However, the sign of the coefficient is the opposite than what would be expected if the variable would be interpreted as proxy for the wage rate. In that case the sign would be expected to be negative since higher wage rate would mean higher labour costs for the firms and therefore also labour demand should decrease. There are two possible explanations why the sign is not as expected. First, *Income* can also be interpreted as proxy for municipal tax base. Higher income would lead to more taxes for the municipality which would enable the municipality to produce more services which would lead to higher employment since the firms producing the services or the municipality itself would need to hire more employees. Alternatively, the estimates might only reflect simultaneous relationship of the regular employment and *Income* even though it was specified with lagged values.

6.2 Dynamic model

As a second step to analyse the relationship between ALMPs the model is also estimated with GMM estimator. GMM estimator enables the possibility of using instrumental variables as an attempt to mitigate the endogeneity problem in fixed effects estimations. In addition, use of GMM enables the possibility to add dynamics to the model in the form of lagged dependent variable. The dynamic model can be written

(10) $n_{it} = \alpha_t + \lambda n_{t-1} + \beta' P_{it} + \gamma' X_{it} + f_i + \varepsilon_{it}$,

where the notation is the same as in equation (9).

Results for the GMM estimations are reported in tables 7 and 8. Overall, the dynamic model was estimated with maximum number of lags for instruments from 4 to all available lags, but only models with maximum number of lags 4, 6 and 8 are reported since there were not significant differences between reported results and other specifications. Overall, results are not promising since they suggest that there is not any robust relationship between ALMPs and regular employment. This raises doubts about robustness of the results. Only model 2 in table 8 finds statistically significant relationship (at 5% level) between *Practice* and *Regular employment*. Point estimate for *Practice* indicates very significant crowding in but the magnitude of the point estimate is not sensible since the value is over 350%.

Another surprising result of the estimations is that for most of the specifications the lagged dependent variable is not statistically significant. The coefficient is statistically significant only when the maximum number of lags for instruments is 8 or higher. The insignificance of the dependent variable would imply that it is not important to control for dynamics i.e. employment would flexibly adjust to new equilibrium without any sluggishness.

Table 7. GMM estimations of the dynamic models 1

Dependent variable: regular employment			
	(1)	**(2)**	**(3)**
VARIABLES	Maximum number of lags: 6	Maximum number of lags: 4	Maximum number of lags: 8
Regular employment$_{t-1}$	0.197	0.174	0.418**
	(0.180)	(0.216)	(0.170)
Subsidised employment	0.096	0.110	-0.268
	(0.504)	(0.676)	(0.430)
Training	0.772	0.504	0.571
	(0.556)	(0.796)	(0.499)
Young	-0.140	-0.153	-0.113
	(0.918)	(1.069)	(0.751)
Old	0.486	0.581	0.188
	(0.311)	(0.378)	(0.276)
Demand	-0.240	-0.274	-0.091
	(0.221)	(0.317)	(0.143)
Income	-0.002	-0.003*	-0.003**
	(0.001)	(0.002)	(0.001)
Observations	1 512	1 512	1 512
Time dummies	Yes	Yes	Yes
Number of municipalities	168	168	168
Number of instruments	34	22	46
Arellando-Bond test, AR(2)	1.307	1.514	2.050
p-value of AR(2)	0.191	0.130	0.040
Hansen test	35.725	9.449	48.881
p-value of Hansen test	0.008	0.150	0.016

Notes: Reported estimates are from second step estimation. Robust standard errors in parentheses, errors have been corrected using Windmeijer (Windmeijer, 2005) finite sample correction. *** p<0.01, ** p<0.05, * p<0.1. The set of instruments includes regular employment (lags from 3 to maximum number of lags), Subsidised employment, Training, Demand, Income and Unemployed (lags from 1 to maximum number of lags).

Test statistics of the Hansen tests provide support to doubts about robustness since the null hypothesis of valid instruments is rejected in all models except when maximum of four lags for the instruments is used. This suggests that used instruments are not valid which might be due to violation of the orthogonality condition. It seems that when number of instruments (i.e. number of variables required to be orthogonal to error term) is smaller test fails to reject the null hypothesis but when the number of instruments increases the null hypothesis is always rejected.

Table 8. GMM estimations of the dynamic models 2

	Dependent variable: regular employment		
	(1)	**(2)**	**(3)**
VARIABLES	Maximum number of lags: 6	Maximum number of lags: 4	Maximum number of lags: 8
Regular employment$_{t-1}$	0.141	0.134	0.413**
	(0.175)	(0.208)	(0.172)
Subsidised employment2	-0.046	-0.416	-0.580
	(0.476)	(0.760)	(0.552)
Training	0.386	0.416	0.292
	(0.494)	(0.571)	(0.368)
Practice	2.526*	3.541**	2.178*
	(1.404)	(1.661)	(1.145)
Young	-0.577	-0.167	-0.534
	(0.735)	(0.956)	(0.563)
Old	0.661**	0.694*	0.342
	(0.304)	(0.354)	(0.268)
Demand	-0.334	-0.326	-0.172
	(0.210)	(0.277)	(0.128)
Income	-0.002	-0.003	-0.002**
	(0.001)	(0.002)	(0.001)
Observations	1 512	1 512	1 512
Time dummies	168	168	168
Number of municipalities	Yes	Yes	Yes
Number of instruments	40	26	54
Arellando-Bond test, AR(2)	1.166	1.460	1.546
p-value of AR(2)	0.243	0.144	0.122
Hansen test	38.511	8.901	62.484
p-value of Hansen test	0.022	0.446	0.005

Notes: Reported estimates are from second step estimation. Robust standard errors in parentheses, errors have been corrected using Windmeijer (2005) finite sample correction. *** p<0.01, ** p<0.05, * p<0.1. The set of instruments includes regular employment (lags from 3 to maximum number of lags), Subsidised employment2, Training, Practice, Demand, Income and Unemployed (lags from 1 to maximum number of lags).

According to Arellano Bond autocorrelation test results the error terms are not autocorrelated in most specifications. Only in model 3 in table 6 the null hypothesis of absence of second order serial correlation is rejected. This implies that the significant p-values of Hansen test should not be due to starting point (lag 1) of lagged instruments.

6.3 Sensitivity analysis

As a starting point of the sensitivity analysis the static model is applied to setting where it is not expected to observe notable displacement effects. For example, if there is not practically any subsidised employment in an industry there should not be any displacement effects either. There are two relatively large industries where the absolute number of subsidised employment program participants and fraction of program participants to total employment of an industry has been very low between 2008 and 2018: financial and insurance activities and transportation and storage. In financial and insurance activities industry the average number of subsidised employment (including practice programs) was 62 persons which is 0.14% of the average total employment in the industry. The average number of subsidised employment in transportation and storage industry was higher (500 persons) but it was only 0.37% of the average total employment in the industry.

Table 9 shows the estimation results for the fixed effect models, where dependent variables are total employment in financial and insurance activities industry and total employment in transportation and storage industry. When comparing table 9 to table 6 the difference is significant. All variables except *Old* are statistically insignificant at 5% level. However, when employment in transportation and storage industry is used as dependent variable (models 3 and 4) *Subsidised employment*, *Training*, *Demand* and *Income* are statistically significant at 10% level. Interpretation of these results is that even though the estimates in table 6 are probably affected by the endogeneity problem, the results are still somewhat robust since there is not statistically significant relationships between ALMPs and employment in industries where almost no program participants have been located.

To test whether choices in construction of the dataset affected the obtained results, the same models were also estimated with different specifications of the dataset. These samples are less restricted and therefore the samples have 649–1 188 observations more than the dataset used in earlier analysis. The results for fixed effects models with three alternative specifications of the dataset are reported in table 10. When comparing the results of the balanced and unfiltered sample (models 1 and 2) the estimates are close to estimates in table 6. Main difference between results is that in this case *Practice* is statistically significant at 10% level and point estimates for subsidised employment are closer to 0.

Table 9. Fixed effects estimation with employment in industries K and H as dependent variables

Variables	(1) Employment in industry K	(2) Employment in industry K	(3) Employment in industry H	(4) Employment in industry H
Subsidised employment	-0.010		-0.090*	
	(0.013)		(0.047)	
Subsidised employment2		-0.007		-0.081
		(0.016)		(0.062)
Training	0.002	0.002	-0.047*	-0.047*
	(0.008)	(0.008)	(0.027)	(0.027)
Practice		-0.013		-0.116
		(0.034)		(0.090)
Young	-0.000	-0.000	0.017	0.017
	(0.012)	(0.012)	(0.027)	(0.027)
Old	-0.018**	-0.018**	0.082***	0.083***
	(0.008)	(0.008)	(0.018)	(0.018)
Demand	0.006	0.006	0.045*	0.045*
	(0.008)	(0.008)	(0.025)	(0.025)
Income	-0.000	-0.000	0.0002*	0.0002*
	(0.000)	(0.000)	(0.0001)	(0.0001)
Constant	0.016***	0.015***	-0.003	-0.003
	(0.005)	(0.005)	(0.016)	(0.017)
Observations	1 848	1 848	1 848	1 848
Adj. R-squared	0.288	0.287	0.181	0.180
Number of municipalities	168	168	168	168
Time dummies	Yes	Yes	Yes	Yes

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Dependent variables are reported above the regression coefficients. Industries: H = Transportation and storage, K = Financial and insurance activities. See table 4 for definitions of the variables.

Models estimated with unbalanced samples (models 3–6 in table 10) have larger differences to estimates in table 6. First, using unbalanced samples *Practice* is statistically significant. The point estimates are quite close to balanced sample estimates, but the standard error is smaller. This suggests that there might be some displacement effects associated with practice programs. In addition, in both unbalanced sample specifications the point estimate of *Training* is more negative compared to balanced sample estimates. In unbalanced an unfiltered sample specification (models 5 and 6) the coefficient for *Young* is significantly lower compared to other models and it is not statistically significant.

GMM models were also estimated using different versions of dataset (not reported). The main difference to estimates reported in tables 7 and 8 was that with unbalanced samples and with balanced and filtered sample *Training* was often statistically significant when *Practice* was not included. Statistically significant point estimates for *Training* were between 0.75 and 2.05 which illustrates large differences between different lag structures. Otherwise, the estimates were close to results in tables 7 and 8 (including test statistics) which implies that unrobust results of GMM estimations are not caused by compilation of the main dataset.

Table 10. Fixed effects estimations with different data set specifications

	Dependent variable: regular employment (n)					
Variables	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Subsidised employment	-0.940***		-1.007***		-0.899***	
	(0.129)		(0.125)		(0.152)	
Subsidised employment2		-0.987***		-1.062***		-0.915***
		(0.160)		(0.151)		(0.187)
Training	-0.381***	-0.377***	-0.534***	-0.532***	-0.588***	-0.588***
	(0.091)	(0.091)	(0.097)	(0.097)	(0.098)	(0.098)
Practice		-0.750*		-0.792**		-0.791**
		(0.393)		(0.382)		(0.356)
Young	0.495***	0.496***	0.376**	0.381**	0.131	0.134
	(0.101)	(0.100)	(0.159)	(0.160)	(0.176)	(0.176)
Old	0.510***	0.511***	0.427***	0.426***	0.350***	0.351***
	(0.066)	(0.066)	(0.076)	(0.076)	(0.099)	(0.099)
Demand	-0.074	-0.073	-0.101	-0.099	-0.081	-0.081
	(0.108)	(0.110)	(0.113)	(0.114)	(0.112)	(0.113)
Income	0.002***	0.002***	0.002***	0.002***	0.002***	0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Constant	0.115**	0.113**	0.226***	0.224***	0.310***	0.307***
	(0.046)	(0.046)	(0.058)	(0.058)	(0.069)	(0.069)
Observations	2 497	2 497	2 676	2 676	3 036	3 036
Adj. R-squared	0.677	0.677	0.619	0.616	0.579	0.578
Number of municipalities	227	227	300	300	305	305
Time dummies	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. See table 4 for variable definitions. Models (1) and (2) use balanced sample without any restrictions on the number of monthly observations for the program variables (balanced and unfiltered sample). Models (3) and (4) use unbalanced sample, where the number of monthly observations for the program variables is at least six (unbalanced and filtered sample). Models (5) and (6) use unbalanced sample without any restrictions on the number of monthly observations for the program variables (unbalanced and unfiltered sample).

6.4 Discussion of the results

To summarize the results, the fixed effects model showed that subsidised employment and training programs have strong negative relationship with regular employment. Regarding practice programs negative relationship is not as robust since the coefficient is not significant with all specifications. Results indicate clearly that the lagged measures in fixed effects models are not enough to cope with the endogenous nature of the dependent variable and explanatory variables. Indications of the endogeneity are for example that point estimates are artificially high and negative relationship between training measures and regular employment is not expected if the endogeneity is not present. Therefore, it cannot be concluded whether the observed relationships are solely result of the endogeneity or is there pure crowding out from these programs.

Dahlberg and Forslund (1999) estimated similar static model with OLS and fixed effects which had similar results: subsidised employment (relief work in their case), training measures and practice programs all had statistically significant negative relationship with regular employment. Results of the static are also similar to Pehkonen (1997) and Skedinger (1995) who that job-creation schemes for the young were associated with displacement effects using VAR approach.

Regarding dynamic GMM model the results differ vastly from Dahlberg and Forslund (2005). Main problem with the estimates presented in tables 7 and 8 seem to be the validity of the instruments since in most cases the null hypothesis of the Hansen test is rejected. It seems that the lagged unemployment is not valid exogenous instrument in the Finnish ALMP context. There are multiple reasons why this might be the case.

First of all, as mentioned before, there have been numerous of different indicators which have influenced how the basic division of the employment grants have been devided between ELY areas. This means that the importance of the number of unemployed has been changing and there has been other factors that have had larger influence on how the grants have been divisoned. For example, in 2016–2018 there was three indicators (unemployment rate, number of unemployed and number of unemployed that are difficult to employ) whereas in 2012–2014 there was in total five indicators that were used used in the regional divison (TEM, 2016; TEM, 2017; TEM, 2018). In addition, there is no information prior 2012 on how the grants were divided between areas, so it is possible that number of unemployed or unemployment rate was not considered as all, even though that scenario is highly unlikely.

Second, only about 50% of the employment grants (basic divison) has been regionally divided using these indicators. This means that there have still been large share that have been allocated irrespective of the number of unemployed of the area. Also, if the government has decided to grant supplementary appropriation to the employment services during the fiscal year, the supplementary grant has been primarily allocated to ELY areas based on how much of the normal grants have not been used yet.

Third, some of the ALMPs have been have been financed through different grants than employment grants as well. For example, wage subsidies and start-up grants were financed only with employment grant in 2015–2016, but 2017 onwards these services were partly financed with unemployment benefit grant (*työttömyysetuusmääräraha*) which have been allocated between areas under different criteria (personal communication with a TEM official, October 23, 2020).

Fourth, and probably most importantly, the data used in the analysis is municipal level data whereas the grants have been divisioned to ELY centres. Therefore, it is possible that the number of ALMP participants within a municipality is not related to the number of unemployed in the municipality necessarily since it is possible that other factors have influenced on how the grants have been allocated within ELY areas. Unfortunately there is not information available on whether ELY centres have some formal rules about regional allocation of the received grants.

In addition to number of unemployed Dahlberg and Forslund (2005) used political majority of the municipal council as a external instrumental variable. The idea is that the political left is more likely to be supportive of ALMPs and therefore the use of ALMPs could be more extensive in municipalities with socialist majority in the council (Calmfors & Skedinger, 1995; Dahlberg & Forslund, 2005). This suggests that the decision making of the ALMPs have probably been more local in Sweden than in Finland. This could be one of the explanations why the number of unemployed in municipality was valid instrument in Swedish context but is not in the Finnish context. In Finland the employment services have been rather centralized until recent municipal experiments where municipalities have received more decision-making power on how the local ALMPs have been organised.

However, there are also arguments to be made whether the results of Dahlberg and Forslund (2005) should really be infered as causal relationships. First, is the number of unemployed truly valid instrument even in the Swedish context? Persons that are not employed are either unemployed or outside of the labour force. Therefore, at some extent, there is reverse relationship between the number of employed and the number of unemployed. Since a person outside of the labour force can become employed or unemployed the increase (decrease) of employment is not 1:1 to the decrease (increase) of unemployment, but at some extent the figures have same determinants i.e. are not fully independent of each other. The use of average of unemployment during 12-months prior to measurment of employment and use of regular employment as employment measure mitigate this problem at some extent, but still this is valid question to be raised.

Dahlberg and Forslund (2005) used Sargan test to test the validity of the instrument. The main difference between Sargan and Hansen tests are for Sargan test the standard errors are assumed to be homoskedastic. The use of Sargan test instead of Hansen is curious since usually Hansen is the standard test for two-step GMM since if the errors are not homoskedastic the Sargan statistic is not consistent (Roodman, 2009a, 97-98; Roodman, 2009b, 140). Sargan test statistic suggested that the used instruments are valid, but the use of Sargan instead of Hansen raises concerns on whether Hansen test would have suggested otherwise (Dahlberg & Forslund, 2005). Dahlberg and Forslund (2005) did not comment on why they chose Sargan test over Hansen test.

Fortunately, Dahlberg and Forslund (2005) did not rely solely on the Sargan test when infering the validity of the results. They also did similar but more extensive sensitivity analysis as in section 6.3, which showed that there were not drastic changes in results with different specifications. In addition, training measures did not have statistically significant relationship with regular employment as would be expected if the results reflect causal relationship rather than barely correlations. So, even though there are questions to be raised about the validity of the instruments, there are also undisputable evidence which supports the claim of the causal interpretation of the results.

7 CONCLUSIONS

Goal of the thesis was study displacement effects of ALMPs. Earlier empirical research shows that there are two types of negative externalities associated with ALMPs. First, ALMPs like job-search assistance and training programs increase employment probability of participants, but the increase comes partly in expense of the non-participant jobseekers (Crépon et al., 2013; Ferracci et al., 2014; Gautier et al., 2018). Blundell et al. (2004) did not find such effects regarding the New Deal program in United Kingdom, but overall, more recent studies have found significant negative externalities from ALMPs on non-participants (Crépon et al., 2013; Ferracci et al., 2014; Gautier et al., 2018). Therefore, effects on non-participants need to be considered as well to be able to evaluate the net effect of ALMPs on employment.

In addition to externalities on non-participant job-seekers, subsidised employment programs can also have impact on employed persons. This type of externality happens if employment subsidy program participants substitute regular employment in a firm or if the employment subsidies produce competitive advantage to a subsidised firm, which decreases employment in a non-subsidised firms. The findings of the studies regarding this type of displacement is contradictory.

Firm level studies suggest that subsidised employment is not associated with substitution or crowding out effects, even though there are some signs of substitution effects, when applications for wage subsidies were not monitored (Kangasharju, 2007; Lombardi et al., 2018). However, most of the macroeconomic studies and survey studies suggest that subsidised employment is associated with significant displacement effects (Calmfors et al., 2002, 32-34; Dahlberg & Forslund, 2005; Pehkonen, 1997; Skedinger, 1995). Main problem with the macroeconomic setting is that ALMP volumes and employment have simultaneous relationship, i.e. employment level might influence ALMP volumes and vice versa. This problem has been tried to mitigate by using IV (Dahlberg & Forslund, 2005) or VAR methods (Pehkonen, 1997; Skedinger, 1995), but still there are factors that raise questions about the robustness of the findings. Regarding survey studies there are multiple biases that might have impact on the results, which is why those findings can only be viewed as suggestive evidence (Calmfors et al., 2002, 32-34).

Findings of the empirical section of this thesis are comparable to earlier macroeconomic studies. Results showed that subsidised employment and training programs are negatively associated with regular employment. Relationship between regular employment and practice programs was not robust but there was indications that practice programs might also have negative association with regular employment. Negative relationship between training and regular employment indicates that estimates are affected by the endogenous nature of ALMPs and employment, which means that it cannot be concluded whether findings are due to displacement effects or endogeneity. GMM estimations did not yield to robust results due to invalid instruments.

Overall, based on the analysis of this thesis and earlier empirical studies the identification strategy of the displacement effects is far more convincing in firm level studies than in macroeconomic or survey studies. However, contradictory to findings of Kangasharju (2007) and Lombardi et al. (2018) theoretical framework suggests that subsidised employment is associated with displacement effects (Calmfors, 1994) which is why the matter needs to be studied more extensively before concluding how substantial the displacement effects are.

In Finnish contex wage subsidies are the most significant employment subsidy program, which is why the future research should begin with studying displacement effects of wage subsidies. Ideally, there would be randomised controlled trial similar to French job placement assistance experiment (Crépon et al., 2013). In the first stage of randomisation the intensity (proportion of jobseekers to be assignet to program) of wage subsidy programs would be randomised between areas, for example municipalities. Then eligible jobseekers would be randomly assigned to treatment and control groups, following the randomly drawn intensity in the first stage.

The problem with this kind of setting is how to find suitable working places for the treatment group, since the wage subsidy is paid to firms. Ideally participating firms would be chosen completely randomly, but realistically there should be for example an application process where firms apply to participate the program. Then firms and workers would be matched by characteristics of the participant (education, working experience, etc.) and firms (industry, type of the employee firm is seeking, etc.). This would produce a pool of suitable firms for a participant. As a last randomisation stage the firm where participant would be employed would be randomly drawn from that pool. This kind of extensive randomisation protocol would ensure that there should not be any selection bias regarding regions, participants and firms where participants would be employed. After randomisation as a empirical analysis it should be studied whether there is effect on firms not receiving subsidies and is there effect on the regular employment in firms receiving subsidies. Ideally similar randomisation experiment should also be implemented for other programs.

However, it is not really realistic that this kind of extensive randomised controlled trial would be implemented. More realistic approach for future research would be similar analysis as Kangasharju (2007) implemented. First, it would be interesting to study whether newer data would yield to similar the results. There is also room for improvement: Kangasharju (2007) used firm's total payroll as measure of firm's number of employees which might introduce measurement error since payroll can increase (decrease) even though number of personnel do not increase (decrease) for example due to pay raises. In addition, Kangasharju's (2007) measures variables annually which might influence the results since effect of the subsidised employment on regular employment might vary along the subsidised period. For example the substitution of regular employment might happen only in the beginning or the end of the subsidised period. Therefore the time interval should be more frequent (i.e. quarterly or monthly) to be able to capture possible varying magnitude of the effect Updated and improved version of Kangasharju's (2007) research could probably be implemented by compiling register data from TEM, Finnish tax authorities and Statistics Finland's business register. In that research setting the selection bias would be tackled with matching methods.

Future research should also focus on studying net employment effects of employment subsidies in the long run. International literature shows that private employment subsidies have positive employment effects on participants in the long run (Card et al., 2018). This might be due to that subsidy schemes help participants to gain working experience which might help the participants to find regular job in the future more likely compared to situation where they would not participate in the program. Crowded out workers are probably in a better situation to find a new regular job compared to vulnerable groups who are are targeted by the employment subsidy programs. Therefore the negative costs of displacement effects might not be as substantial in the long run.

This thesis has focused on studying what kind of different effects ALMPs have regarding employment. This kind of information can be applied for example when the goal is to evaluate net employment effects of a ALMP to make policy recommendations. At the same time, it should not be forgotten that ALMPs can also have other kind of positive effects on welfare of the unemployed via other channels. ALMPs are also associated with increased general well-being, health and mental health amongst the unemployed (Coutts, Stuckler, & Cann, 2014; Rose, 2019; Sage, 2015; Wang, Coutts, Burchell, Kamerāde, & Balderson, 2020). This means that policy makers should not view ALMPs only as employment policy tool but also as a tool of social policy to improve the well-being of the vulnerable groups of our society.

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APPENDIX 1

Table 11. Descriptive statistics for balanced and unfiltered dataset

Variable	Mean	Standard deviation	Min	Max
Dependent variable				
n	0.632	0.061	0.468	0.796
Explanatory variables of interest				
Subsidised employment	0.013	0.006	0.000	0.058
Training	0.017	0.009	0.002	0.056
Subsidised employment2	0.010	0.005	0.002	0.042
Practice	0.003	0.002	0.000	0.011
Control variables				
Demand	-0.002	0.012	-0.040	0.049
Young	0.166	0.038	0.083	0.351
Old	0.228	0.059	0.077	0.429
Income	28 008	7 672	13 975	55 075
External instrumental variable				
u	0.079	0.025	0.018	0.169

Notes: See table 4 for the definitions of the variables. The number of municipalities is i = 227, the number of years is t = 11 and the number of observations is n = 2497.

Table 12. Descriptive statistics for unbalanced and unfiltered dataset

Variable	Mean	Standard deviation	Min	Max
Dependent variable				
n	0.632	0.067	0.458	0.912
Explanatory variables of interest				
Subsidised employment	0.013	0.007	0.000	0.058
Training	0.017	0.009	0.000	0.056
Subsidised employment2	0.010	0.006	0.000	0.053
Practice	0.003	0.002	0.000	0.015
Control variables				
Demand	-0.002	0.012	-0.043	0.047
Young	0.162	0.040	0.011	0.351
Old	0.235	0.056	0.077	0.429
Income	27 465	8 098	13 975	77 135
External instrumental variable				
u	0.078	0.027	0.000	0.174

Notes: See table 4 for the definitions of the variables. The number of municipalities is i = 305. the number of years is t = 11 and the number of observations is n = 3036.